Spillovers without Social Interactions in Urban Sanitation*

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Abstract

We run a randomized controlled trial coupled with lab-in-the-field social network experiments in urban Dakar. Decision spillovers and health externalities play a large role in determining uptake of sanitation technology, with decision spillovers being largest among households that don't receive significant subsidies. There is no evidence that social mechanisms such as social pressure, learning from others, or reciprocity explain the spillovers. We do find evidence of a fourth, non-social, mechanism impacting decisions: increasing returns to scale. As more neighbors adopt the sanitary technology, it becomes more worthwhile for other households to adopt as well.

Keywords: social networks, sanitation, spillovers, reciprocity.
JEL Classification: O10, Q56, R11.

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1 Introduction

Understanding how to increase adoption of new technologies is crucial for sustaining economic growth in developing countries. This is particularly true for sanitation goods, which have significant health externalities. Policies relying on social mechanisms such as learning from others or reciprocity to generate a multiplier effect in adoption appear promising in an environment with limited budgets for subsidies.

Social-network-based interventions such as Community Led Total Sanitation (CLTS) have been implemented extensively in rural areas, and existing research finds evidence of social multiplier effects in sanitation decisions (Guiteras et al. 2015). Very little is known about the size of social multiplier effects in urban areas which are more socially heterogeneous, transient, and anonymous. Duflo et al. (2012) discuss the importance of understanding differences between urban and rural areas for interventions, especially given the rapid migration and urbanization of developing countries.

Due in part to this rapid urbanization, centralized sewer systems are not available in many neighborhoods of urban Dakar, the setting we study in this paper. As a result, households often rely on toilets connected to latrine pits. When these pits fill (approximately every 6 months), the waste needs to be removed and disposed of, and unsanitary techniques are common. There are two main technologies for emptying (desludging) a pit. The less sanitary but less expensive option is a manual desludging. In this case, a person enters the pit with a shovel and a bucket to take out the sludge and then dumps it nearby, usually on the street in front of the house, where it remains for weeks. The more sanitary and more expensive option is a mechanized desludging. In this case, a vacuum truck pumps the sludge out of the pit. One latrine pit typically contains enough waste to fill the truck's tank, and the truck then transports the sludge to dump at a treatment center.

We implement a randomized controlled trial paired with lab-in-the-field social network experiments and social network data to test the mechanisms driving household sanitation decisions in urban Dakar. We randomly offer households subsidies for a mechanized desludging, varying the saturation of these subsidies across clusters. In line with previous work (Bates et al. 2012, Guiteras et al. 2015), households that are offered subsidies are more likely to adopt. Additionally, households living near a higher share of highly subsidized households are also more likely to adopt, even if they themselves were not offered a subsidy. We call this a *decision spillover*, when one household's adoption decision affects the decision of others. For each additional household receiving the high subsidy in a neighborhood, its neighbors are 1.1 percentage points more likely to adopt. Moving from a neighborhood in which only twenty percent of households are offered a large subsidy to a neighborhood in which fifty percent of households are offered the high subsidy has the same impact on adoption as actually being offered the large subsidy. Importantly, this effect is strongest among households offered a lower subsidy or no subsidy at all.

We also find evidence of *health externalities*; the impact that one household's sanitation decision has on the health of others. Improved sanitation decreases incidence of diarrhea. Every additional high subsidy offered near a household leads to an (imprecisely estimated) 6% decrease in the share of sick household members.[1]

Decision spillovers have been found in the adoption of health and sanitation goods in rural environments (Dupas 2014, Gautam 2018, Guiteras et al. 2015). These studies leave open the questions both of whether such spillovers will exist in an urban setting and, if so, the mechanism causing these spillovers. We explore social or inter-personal mechanisms underlying decision spillovers including social pressure, learning from others, and reciprocity. Social pressure can be used to effectively increase adoption of sanitary behavior in CLTS programs commonly practiced in rural villages (Pickering et al. 2015). Households learn about new technologies from each other, leading to increased adoption (Beaman et al. 2020, Conley & Udry 2010, Dupas 2014). Reciprocity plays an important role in rural areas in maintaining property rights and avoiding theft

[1] Kresch et al. (2020) review the evidence on both decision spillovers and health externalities in sanitation, distinguishing between evidence from rural and urban settings, and also focusing on nonlinearities and thresholds in health externalities.

(Schechter 2007). Even with detailed social network data and interventions designed to measure each of these social mechanisms, we do not find evidence of socially-based decision spillovers.

While such inter-personal mechanisms have been found to be important in rural areas, our study takes place in urban Dakar, a metropolitan area with a population of 2.5 million and high mobility. Urban households may be more susceptible to social effects from work peers or family members scattered across the large city. In addition, neighborhood norms may change through a non-social process.

Decision spillovers may also be driven by increasing returns to scale or decreasing costs with scale. Sanitation investments may exhibit increasing returns to scale, such that as a neighborhood becomes cleaner, the marginal benefits to a household of improving their own sanitation increases (Andrés et al. 2017, Fuller et al. 2016, Oswald et al. 2017). Investment could also lead to decreasing costs, if one household's adoption decreases costs faced by its neighbors, for example by increasing the availability and accessibility of the technology in the neighborhood. In either of these cases an intervention which leads to increased adoption may lead to a virtuous cycle, regardless of the social interactions taking place between the households. Fuller et al. (2016) posit that there exists a lower threshold critical mass below which a few households with improved sanitation has no effect (with Andrés et al. (2017) calculating that in rural India this threshold is 30%). There may also be an upper threshold above which transmission is interrupted.[2] We provide evidence consistent with an increase in demand–even among unsubsidized households–in neighborhoods where more large subsidies have been offered.

The fact that sanitation adoption decisions spread across urban areas suggests that harnessing decision spillovers to improve sanitation could help

[2]This is similar to non-convexities in returns to vaccination: below a low level of vaccination coverage only the immunized are protected, and above a high threshold there is herd immunity. See also Lerva (2020) who studies farmers' pesticide adoption and distinguishes between non-social spillovers by which lowering pests in one plot also helps control the pest population in a neighbor's plot, and social spillovers by which neighbors share information about the pesticide.

spread better health practices at lower costs. Particularly when only a small percentage of households are subsidized, increasing the number of subsidies in a neighborhood increases adoption by both subsidized and unsubsidized households. The null impacts of social network effects combined with the importance of decision spillovers suggests that the recent movement toward community based sanitation initiatives may be less successful in urban areas than more traditional targeted subsidy programs.

In Section 2, we discuss the setting of urban Dakar in general, and desludging and sanitation more specifically. In Section 3, we discuss the basic underlying experimental design which allows us to measure decision spillovers and the health externalities they cause. Section 4 describes the data, while Section 5 describes the estimation strategy we employ to measure decision spillovers and health externalities. We show evidence of decision spillovers in Section 6. Given that these decision spillovers exist, Section 7 describes the experimental design, estimation strategy, and results for exploring the different mechanisms which could be leading to these decision spillovers. Section 8 concludes.

2 Background

While sanitation issues have been widely studied in rural contexts, urban communities face different, but equally complex, sanitation-related problems. Latrine or toilet ownership is common in urban areas, but the disposal of latrine waste can be problematic. Improper removal and disposal of waste are common and lead to important health repercussions (Mara et al. 2010).

We study the issue of latrine waste and disposal in the context of urban Dakar, Senegal. Almost two million people in Dakar use latrines which are not connected to sewage systems. The latrine pits fill up approximately once every six months and then need to be desludged (emptied), for continued use. When the pit is full, households have two options: manual or mechanized desludging. In a manual desludging, a person enters the pit with a shovel and a bucket and dumps the sludge in the courtyard or in the street in front of the house. This can be conducted by a family member (30% of the time in our baseline survey) or the household can hire a *baay pell* specializing in manual desludgings (26% of the time). In a mechanized desludging, households hire a truck driver to pump the sludge out of their pit and transport it to dump at a treatment center (chosen by 44% of our respondents).

Manual desludgings are significantly less expensive and less sanitary. The average price of a mechanized desludging is $50. In contrast, family members usually do manual desludgings at no charge while baay pells charge an average of $29 for a manual desludging. Manual desludgers dump the sludge in front of the house, in the street, in the household's courtyard, or in a nearby vacant lot (34%, 27%, 27%, and 7% of the time respectively).

Since most streets are not paved, after a manual desludging the sludge is put in a shallow pit built up from sand, often against the wall outside the compound. The liquid from the desludging will evaporate or absorb into the sand in a few days. Over time, people may try to cover the open pit with sand. When it rains, some of the material will 'wash' away, but without proper drainage it won't necessarily travel far. The solid material might remain visible for up to a month.

Mechanized desludging by a vacuum truck is more expensive but more sanitary. A vacuum truck can typically only service one client per trip, because one latrine fills the truck's tank. After servicing a household, the trucker must drive to one of the three treatment centers in Dakar to dump the sludge before servicing another household. This limits economies of scale in terms of the costs of desludging.

We asked respondents from households that had never purchased a mechanized desludging why they had not done so. The most common response was that the price was too high (62%). Another 26% were concerned that their house would not be accessible by trucks due to narrow roads. A further 6% were concerned that vacuum trucks leave sludge at the bottom of a pit and preferred to hire a human with a shovel who can get everything out.

Residents of urban areas move more frequently than those in rural areas and often do not know their neighbors well. Our sample consists only of households that make their own decisions about how to desludge their pit. This means it includes homeowners, as well as those renters in charge of their desludging. This implies that our survey respondents are probably less transient than the modal resident of urban Dakar, and thus potentially more swayed by social pressure and peer effects from their neighbors.

In our sample which, if anything, oversamples more stable households, 50% of respondents have moved into their residence in the past ten years and 11% in the past two years. Although these households have lived in the same dwelling for years, they are not always familiar with their neighbors. We asked each respondent about eleven nearby households, usually those living around a square block. Respondents report they are not 'aware' of almost 40% of their neighbors' identities. They do claim to talk about sanitation with 13% of their neighbors, almost a quarter of the neighbors of whom they are aware.

We did not ask respondents their beliefs about the health effects of manual desludgings because we were wary of activating experimenter demand effects in take-up decisions. However, residents report finding the effects of manual desludging to be unpleasant given the smell, dirt, and bugs it attracts. Given the multitude of public health announcements on the radio and television, residents are aware that manual desludgings are not merely a messy inconvenience, but that they also have negative health effects.

3 Basic Experimental Design

To test whether we can harness decision spillovers to increase the take-up of mechanized desludgings in an urban setting, we implemented a randomized experiment offering subsidies to households for a mechanized desludging service. We worked with 4920 households in 410 clusters in Dakar, the capital of Senegal.

We mapped the city of Dakar excluding areas connected to the sewage network, military barracks, parkland, and flood-prone areas.[3] We placed 410 equally spaced grid points across the remaining areas and used the residence closest to each grid point as our starting point. Coming out the door of the

[3]Flood-prone areas often get free emergency desludgings from the government or NGOs.

first house, the teams would turn right, mapping households on both sides of the street, and turning right at every corner. If one circuit of the block was not enough to identify 25 households, the team would return to the original household and spiral out to take the second right instead of the first right. Throughout the paper we will use the word 'cluster' to refer to households which originated from the same starting grid point.

We approached those 25 mapped households in a pre-specified random order until we found 12 that had a functioning pit for which the household made the desludging decision (e.g., not a renter if the owner was the one in charge of desludging decisions), and who consented to respond to our survey. In each cluster we interviewed 12 households. We offered the subsidized mechanized desludging service to ten randomly chosen 'treatment' households. We additionally interviewed another two 'spillover' households, to whom we did not offer the subsidized desludging service, to help measure decision spillovers and health externalities.

We offered treatment households up to two discounted mechanized desludgings over a period of 12 months if they signed up in advance. We randomly offered half of the households a low subsidy leading to a price of \$48 and the other half a high subsidy leading to a price of \$34.[4] In the baseline in our sample, the average price for a mechanized desludging was \$50 and the average cost of a manual desludging not conducted by a family member was \$29. This implies that our low subsidy was close to the current going rate for a mechanized desludging, whereas our high subsidy was a substantial discount close to the going rate for a manual desludging. A translation of the script which the enumerators used to introduce the desludging service can be found in Appendix A.

To measure spillovers we did two things. First, in every cluster we inter-

[4] We organized a call-in center to which customers could call and truckers were invited to bid on jobs via text message. The trucker who bid the lowest price won the job. We guaranteed prices for our subsidized households, and paid the difference between the subsidized price and the winning bid ourselves. Other households throughout Dakar calling the call center paid the winning bid. Deutschmann et al. (2020) and Houde et al. (2020) analyze the call-in center and the auctions.

viewed two ‘spillover’ households that were not offered the subsidized mechanized desludging. Second, at the cluster level we randomized the number (saturation) of households offered the high subsidy. Conditional on that saturation, the subsidy level was randomized at the household level. The previous literature (Dupas 2014, Guiteras et al. 2015) suggests that the size of spillover impacts can vary with the intensity with which the cluster is treated. The modal cluster of ten treated households had five households offered a high subsidy and five a low subsidy. But, the number of high subsidies in a cluster ranged from one to nine, with fewer clusters at the extremes. The exact distribution can be found in Appendix B. Baird et al. (2018) call designs such as ours ‘random saturation,’ first randomizing the share of treated households in a cluster, then randomizing which households are treated.[5]

The experiment was also designed to measure the effects of nudges such as deposits and earmarked savings accounts, as analyzed in Lipscomb & Schechter (2018). All households received a \$6 payment for their participation in the survey. A randomly chosen 88% of the treatment group was required to leave this as a deposit if they signed up for the subsidized desludging service (but could access the money immediately if they did not). Their deposit went towards the first subsidized desludging, or was returned at the end of the year if unused. The remaining 12% of the treatment group was not required to leave a deposit and could sign up as purely cheap talk. Lipscomb & Schechter (2018) show that behavioral nudges such as deposit requirements and earmarked accounts do not increase adoption. They show that more traditional interventions such as subsidies do have a large impact. In this current paper, we focus on the decision spillovers and health externalities that the subsidies lead to.

After all baseline surveys in a cluster were completed, we returned and gave stickers to the households that signed up for the desludging service. Households could post these stickers outside their house both as a signal for the desludger, and as a signal to their peers, that they signed up for the sanitary

[5]Unfortunately we designed our experiment before their analysis of the optimal design. The fact that we have more clusters with even splits of five high and five low subsidies, and fewer clusters at the extremes, means that our power to detect how these spillovers vary with intensity of treatment is not as high as it could be.

mechanized desludging. The sticker itself as well as photos of the sticker on house entry-ways are shown in Appendix C.

4 Data

We conducted two-tiered baseline data collection. The first survey, collected in September and October of 2013, determined eligibility and collected demographic data. The second survey, collected between February and May 2014, asked desludging-related questions and offered the intervention to the treatment households. The respondent to the second survey was someone in the household who helped make the desludging decision (henceforth called the 'decider'). Because of the time between the two surveys, there was a reasonable amount of attrition. Of the original 4920 households (4100 treatment and 820 spillover) in the first survey, we found 4521 households (3757 treatment and 764 spillover) for the second baseline survey. We conducted one single endline survey a year later, from March to May 2015, in which we re-interviewed 4101 of the original households (3404 treatment and 696 spillover).

We look at attrition from the first baseline survey to the later surveys, and from the second baseline survey (which included the intervention) to the endline survey in Appendix Table G-1. Spillover households are less likely to attrit than households offered subsidies. While the magnitude is relatively small (spillover households are 3 p.p. more likely to appear in the endline data) it is significant. There is no difference in attrition across clusters in which different numbers of households received high subsidies. In Table 1, we test balance across the randomized treatment groups. Baseline characteristics appear to be well-balanced with respect to both the individually-randomized subsidy level and the cluster-randomized saturation of high subsidies.

We use random variation in the number of households in a cluster (or the number in the nearest five households to a household) offered a high subsidy to measure decision spillovers and health externalities. The average distance between any two households in the same cluster (including the household itself) is 50 meters (s.d. 16). For some analysis we focus on the five households

Table 1: Randomized Treatment Balance

	Treatments				# of high subsidy hhds in cluster				Obs.
	Mean (SD)	Coefficient (SE)		p-value	Mean (SD)	Coefficient (SE)		p-value	
	(1) Low Subsidy (LS)	(2) High Subsidy (HS)	(3) Spillover (SO)	(4) HS= SO= 0	(5) 4-6 High Subsidies (HS46)	(6) 1-3 High Subsidies (HS13)	(7) 7-9 High Subsidies (HS79)	(8) HS13= HS79= 0	(9) Total Obs.
Respondent male	0.658	0.025	-0.014	0.101	0.670	-0.020	0.003	0.646	4,520
	(0.475)	(0.017)	(0.022)		(0.470)	(0.023)	(0.020)		
Respondent age	49.84	-0.450	-1.183**	0.096	49.34	0.634	-0.557	0.362	4,467
	(13.08)	(0.445)	(0.552)		(13.31)	(0.641)	(0.661)		
Respondent years of education	5.669	0.074	-0.226	0.463	5.768	-0.471	-0.027	0.401	4,508
	(5.537)	(0.189)	(0.242)		(5.624)	(0.351)	(0.382)		
Household size	10.34	-0.097	-0.034	0.888	10.26	0.322	-0.357	0.289	4,464
	(5.870)	(0.200)	(0.238)		(5.813)	(0.339)	(0.343)		
Wealth index	-0.008	0.014	-0.045	0.691	0.014	-0.112	0.015	0.524	4,464
	(1.636)	(0.054)	(0.069)		(1.586)	(0.105)	(0.094)		
Own their house	0.775	0.007	0.008	0.872	0.773	0.014	0.019	0.578	4,464
	(0.418)	(0.015)	(0.019)		(0.419)	(0.019)	(0.022)		
House has two stories	0.245	0.002	-0.002	0.976	0.255	-0.043	-0.025	0.191	4,464
	(0.430)	(0.015)	(0.018)		(0.436)	(0.026)	(0.023)		
Number of rooms in house	6.490	-0.041	0.038	0.838	6.514	-0.121	-0.212	0.503	4,464
	(3.284)	(0.117)	(0.141)		(3.333)	(0.198)	(0.193)		
Courtyard looks clean	0.747	0.007	0.011	0.835	0.764	-0.054**	0.004	0.114	4,464
	(0.435)	(0.016)	(0.020)		(0.425)	(0.027)	(0.025)		
Used mechanized in year before bl	0.293	0.005	-0.008	0.778	0.298	-0.029	-0.014	0.642	4,521
	(0.455)	(0.015)	(0.018)		(0.457)	(0.033)	(0.031)		
Used manual in year before bl	0.367	0.006	0.032	0.337	0.373	0.017	0.005	0.833	4,521
	(0.482)	(0.016)	(0.022)		(0.484)	(0.029)	(0.029)		
p-value of joint F-test		0.914	0.604			0.523	0.823		

Note: The sample includes all households that responded to the second baseline survey. All variables are measured in the baseline (bl) and respondent characteristics refer to the respondent in the second baseline survey. Columns (1) and (5) show the mean and standard deviation of observations with a low subsidy and observations in a cluster with 4-6 high subsidy hhds, respectively. Columns (2) and (3) show the coefficients on high subsidy and spillover in a regression including grid-point level fixed effects. Columns (6) and (7) show the coefficients on clusters with 1-3 and 7-9 high subsidy hhds in a regression with no fixed effects. Standard errors clustered at the grid-point level in parentheses in columns (2), (3), (6), and (7): * p<0.10, ** p<0.05, *** p<0.01. Columns (4) and (8) show the *p*-values for tests of whether the coefficients in columns (2)-(3) or columns (6)-(7) equal one another and equal 0. The last row shows the *p*-value for a joint test of all individual tests in the preceding rows.

nearest to a respondent's household (including the household itself), with average distance of 23 meters (s.d. 10).[6] For comparison, the World Health Organization (2020) recommends a distance of 30 meters from the house to un-improved latrines.

[6]The households in the same cluster are located close enough to one another that we expect there to be spillovers. The 5th, 50th, and 95th percentile distances between households in the same cluster are 11, 50, and 115 meters. Households in different clusters are far enough away that we do not expect such spillovers to be present. The median distance between a household in one cluster and the *closest* household to it in a different cluster is 200 meters (with the 5th percentile being 115 meters).

5 Basic Estimation Strategy

In sections 5 and 6, we lay out the estimation strategy and results measuring decision spillovers and health externalities. In section 7, we lay out the estimation strategy and results exploring the mechanism behind these decision spillovers. But, before jumping into externalities and spillovers, we first explore direct effects of the subsidies on the households that were offered them.

We analyze the impact of the high subsidy treatment on outcome, y_{iga}, for household i living near grid-point g in arrondissement a. The four outcomes we look at are: signing up for the subsidized mechanized desludging in the baseline, purchasing a subsidized mechanized desludging in the year after the baseline, purchasing any mechanized desludging in the year after the baseline (either from us or on the open market), and purchasing or using any manual desludging in the year after the baseline (an action we hope the interventions discourage). The first two outcomes come from the second baseline survey and administrative data, so the sample is the full sample. The last two outcomes were measured in the endline survey, with slightly smaller sample sizes.

We run the following OLS regression:

$$y_{iga} = \alpha + H'_{iga}\beta_1 + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{1}$$

We cluster standard errors at the grid-point cluster level and include arrondissement fixed effects ψ_a (of which there are four). The sample includes all treated households and the coefficient of interest is β_1 on the indicator for whether the household was offered a high subsidy (H_{iga}). We include other household level controls measured in the baseline in X_{iga}. These include sociodemographics such as sex, age, education, household size, and a wealth index. We include characteristics of the house which would affect the desludging decision, such as whether they own their house, whether the house has two stories, the number of rooms in the house, and whether the enumerator thought the courtyard looked clean in the baseline survey. Finally, in accord with McKenzie (2012), we control for pre-intervention outcomes: having had a mechanized desludging in the year before baseline and having had a manual desludging in the year before the baseline. We also control for the other random interventions discussed in Section 7.1.[7] We also run the regression using controls as chosen by the post-double-selection LASSO procedure elucidated in Belloni et al. (2014) and as programmed by Ahrens et al. (2020). Appendix D lists the variables from which the controls are chosen, and the controls selected in each regression.

5.1 Decision Spillovers

We look at whether one household choosing a more sanitary technology (for example due to being randomly offered a high subsidy to do so) makes it more likely that their neighbors also choose a more sanitary technology. We look for these decision spillovers by exploring whether the number of high subsidies offered in a cluster ($\overline{H}_{ga}$) has an effect on rates of mechanized desludging in that cluster, conditional on the household's own subsidy level.

$$y_{iga} = \alpha + H'_{iga}\beta_1 + \overline{H}'_{ga}\beta_2(+S'_{iga}\beta_3) + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{2}$$

We run the above regression on all treated households, on just the two spillover households in each cluster, and on all households, while additionally controlling for being a spillover household (S_{iga}). The coefficient of interest is β_2, the effect of the number of high subsidies in the cluster.

A different, less direct strategy for studying decision spillovers is to look at the characteristics of the desludger hired. If households contract with a new provider we might think the intervention is convincing households to newly try mechanized desludgings. We turn to an alternative-specific conditional logit with four alternative outcomes: the household needed no desludging between baseline and endline, the household used a manual desludging between baseline

[7] These include whether the household was required to leave a deposit to sign up, whether the household lives in a cluster where the subsidy levels offered to neighbors were made public, whether the household is a 'second five' household in a cluster where the second five treatment households were told which of the first five treatment households signed up, and whether the household is a 'second five' household in a cluster where the second five treatment households were told how many of the first five households signed up.

and endline, the household used a mechanized desludging between baseline where it is the first time the household has hired that desludger, and the household used a mechanized desludging where it is not the first time hiring that specific desludger. We model the household's utility from each desludging alternative k in the endline (period 2):

$$U_{igak2} = \beta_1 y_{igak1} + \beta'_{2k} H_{iga} + \beta'_{3k} \overline{H}_{ga} + \beta'_{4k} S_{iga} + \epsilon_{igak} \tag{3}$$

where y_{igak1} is the most recent alternative they chose in the baseline. We also analyze the means through which the respondent found the desludger, such as going to the desludger's garage, calling their phone number, or other options.

5.2 Health Externalities

We also look at whether one household choosing a more sanitary technology makes it more likely that their neighbors have better health outcomes. We look for these health externalities by running a similar regression to that in equation (2) but in this case the outcome y is a measure of the prevalence of diarrhea in a household in the endline survey. As a falsification test, we use the alternative outcome of cough prevalence, which should not be greatly affected by sanitation. Because health externalities may be more localized, we additionally estimate a version in which we measure the effect the number of households in the nearest five (including the household itself) that were offered the high subsidy. The control variables additionally include the pre-intervention baseline levels of both diarrhea and cough.

Our preferred estimation strategy is the OLS specification measuring the effects of subsidy saturation on health outcomes. As an alternative, we also look in Appendix G at the effect of sanitation choices on health outcomes, instrumenting sanitation choices with the randomized subsidies. This is the IV specification:

$$y_{iga} = \alpha + H'_{iga}\beta_1 + \overline{Desl}'_{ga}\beta_2 + \overline{Mech}'_{ga}\beta_3 + S'_{iga}\beta_4 + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{4}$$

We measure the impact on health outcomes of the number of households in a cluster that had a mechanized desludging ($\overline{Mech}_{ga}$) between the baseline and endline, conditional on the number of households in a cluster that had any desludging ($\overline{Desl}_{ga}$). We instrument for $\overline{Mech}_{ga}$ with the number of households in a cluster that were offered the high subsidy ($\overline{H}_{ga}$). Because these are relatively weak instruments, these are not our preferred estimates.

6 Basic Results

We measure whether receiving a high subsidy influences a household's decision to take-up the more sanitary technology; whether there are decision spillovers leading households' high subsidies to influence their neighbors' take-up; and whether these decision spillovers lead to health externalities.

Table 2: Impact of Subsidy on Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
High subsidy	0.191***	0.081***	0.031**	-0.034**
	(0.015)	(0.009)	(0.013)	(0.014)
N	3654	3654	3308	3308
R^2	0.105	0.083	0.302	0.268
Mean of outcome variable	0.400	0.075	0.315	0.316

Note: The sample includes all treatment households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) signed up for the subsidized mechanized desludging, (2) purchased the subsidized mechanized desludging, (3) purchased any mechanized desludging between the baseline and endline, and (4) had any manual desludging between the baseline and endline. The outcome in column (1) comes from the baseline survey, in (2) from the administrative data, and in (3)-(4) from the endline survey. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (deposit required, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level.

In Table 2 we use equation (1) to estimate the effects of own high subsidy on technology adoption. The corresponding similar LASSO results are shown in Appendix Table G-2. In line with existing evidence (Bates et al. 2012), offering households subsidized preventive health products increases take-up. Those who received the high subsidy are 19 p.p. more likely to sign up, 8 p.p. more likely to purchase a subsidized mechanized desludging through us, 3 p.p. more likely to purchase a mechanized desludging overall, and 3 p.p. less likely to purchase or use a manual desludging.

6.1 Decision Spillovers

To explore decision spillovers visually, we bin clusters into those with one to three high subsidies, four to six high subsidies, and seven to nine high subsidies. Figure 1 shows that as more households in a cluster are offered the high subsidy, there are fewer households that use manual desludging and more households that purchase a mechanized desludging. The results align with Guiteras et al. (2015) who find that spillovers increase most when going from low to medium shares of subsidies. We show effects separately for households offered a high subsidy, low subsidy, and no subsidy (spillover). The effect is strongest among the low subsidy and spillover households. Neighbors' subsidy levels have very little impact on those households which were themselves offered a high subsidy.[8]

We explore these decision spillovers in regression form by estimating equation (2) in Table 3. The corresponding similar LASSO results are shown in Appendix Table G-3. Panel A contains just the treated households, panel B just the spillover households, and panel C pools both groups. The evidence is consistent that when more households in a cluster are offered the high subsidy, households in that cluster become more likely to purchase a mechanized desludging and less likely to use a manual desludging. As the number of high

[8]This result differs from Guiteras et al. (2015) who find that neighbors' subsidies have a strong impact on all households. The difference may be because we study subsidization of sanitary desludgings, something that a household will need no matter what; while they study subsidization of hygienic latrines, which households do not necessarily need to purchase. In our case, at the high subsidy level the sanitary option is almost the same price as the less sanitary option, meaning that households which are offered a high subsidy should need little extra motivation to purchase the sanitary option.

subsidy households in the cluster increases, the household substitutes from manual toward mechanized desludging.

Table 3: Decision Spillovers in Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
Panel A: Treated Households				
# of high subsidy hhds in cluster	-0.007	-0.001	0.009*	-0.013**
	(0.007)	(0.003)	(0.005)	(0.005)
N	3654	3654	3308	3308
R^2	0.105	0.083	0.303	0.270
Mean of outcome variable	0.400	0.075	0.315	0.316
Panel B: Spillover Households				
# of high subsidy hhds in cluster			0.021**	0.002
			(0.009)	(0.011)
N			676	676
R^2			0.396	0.341
Mean of outcome variable			0.322	0.377
Panel C: All Households				
# of high subsidy hhds in cluster			0.011**	-0.011**
			(0.005)	(0.005)
N			3984	3984
R^2			0.315	0.280
Mean of outcome variable			0.316	0.326

Note: Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) signed up for the subsidized mechanized desludging, (2) purchased the subsidized mechanized desludging, (3) purchased any mechanized desludging between the baseline and endline, and (4) had any manual desludging between the baseline and endline. The outcome in column (1) comes from the baseline survey, in (2) from the administrative data, and in (3)-(4) from the endline survey. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), and fixed effects at the arrondissement level. Additional controls in panel A and C include randomized intervention indicators (high subsidy, deposit required, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster). An additional control in Panel C is a spillover household indicator.

We next look at whether decision spillovers lead households to hire a desludger with whom they haven't worked previously. We show the results from equation (3) in Table 4. The top row shows that households tend to choose the same alternative as they have in the past. The subsequent rows show how the randomized treatments affect the probability of use of each of the alternatives. High subsidy households are more likely to use a mechanized desludging with a new provider. This makes sense, since a condition of using our subsidized service is that they must use the provider we assign them.

Perhaps more interesting is the decision spillover. Living in a cluster with more high subsidy neighbors decreases the likelihood that a household uses a manual desludging and increases the likelihood that a household uses a mechanized desludging service provider, and specifically a provider that they have not used in the past. This suggests that having a large number of neighbors with high subsidies encourages households to try something new and find a new provider for their more sanitary mechanized desludging.

We run another version of equation (3) focusing on how households find their desludging provider, with results presented in Appendix Table G-4. The first two alternatives (needing no desludging and using a manual desludging) are the same. The remaining three alternatives involve the household i) finding the desludger at their garage or parking site, ii) finding them by flagging them down, calling the number seen on a truck, or by referral, and iii) finding them by calling our service or the more general call center.[9] Appendix Table G-4 shows that high subsidy households are more likely to purchase a mechanized desludging by calling our service. Spillover households are more likely to purchase a desludging by any means other than by calling our service (for which they were not eligible). In clusters with a higher number of high subsidy households, households are less likely to get a manual desludging. Households in these clusters are more likely to get a mechanized desludging where they found the desludger by calling their phone number,[10] waiting by the road, or being referred by someone. This suggests that when trucks are more present in a cluster, households are more likely to use them.

6.2 Health Externalities

Fewtrell et al. (2005) and Wolf et al. (2014) conduct systematic reviews of

[9]We would ideally like to distinguish between neighborhood-effects which are more social (like referrals) and those which are more mechanical (like seeing a truck in the neighborhood and flagging it down). Unfortunately, these sub-categories become quite small. And, the survey does not distinguish between referrals from neighbors and those from friends living elsewhere so referrals don't necessarily imply social neighborhood-level effects.

[10]Many trucks have their phone number painted on them, so customers can call after seeing the truck working nearby or passing on the road.

Table 4: First Encounter with Mechanized Desludger?

	(1)
	First Time Desludger Hired
Alternative-Specific Variable	
Alternative chosen in bl	0.622***
	(0.051)
Manual (26%)	
High subsidy	-0.126
	(0.108)
Spillover household	0.340*
	(0.202)
# of high subsidy hhds in cluster	-0.089**
	(0.038)
Mechanized - first (18%)	
High subsidy	0.223*
	(0.121)
Spillover household	0.254
	(0.207)
# of high subsidy hhds in cluster	0.077**
	(0.038)
Mechanized - not first (14%)	
High subsidy	-0.127
	(0.137)
Spillover household	0.402*
	(0.242)
# of high subsidy hhds in cluster	0.002
	(0.048)
N of Obs.	15936
N of Cases	3984

Note: The sample includes all households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Conditional logit where the outcome variable is the desludging choice between baseline and endline. The base alternative is having no desludging between baseline and endline. The other alternatives are (1) having a manual desludging between baseline and endline, (2) having a mechanized desludging with a new desludger between the baseline and endline, and (3) having a mechanized desludging with a desludger used in the past between the baseline and endline. The outcome in column (1) comes from from the endline survey. Controls (measured in baseline), all interacted with the different alternatives, include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (deposit required, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level.

the effects of improved water, hygiene, and sanitation on diarrheal diseases, and note that a limitation is the dearth of randomized controlled trials around improved sanitation. The size of the effect of improved sanitation is similar in the two reviews, at 32% and 38%, and also similar in size to what we will find below. Wolf et al. (2014) note that the largest effects come from sewer-related interventions.

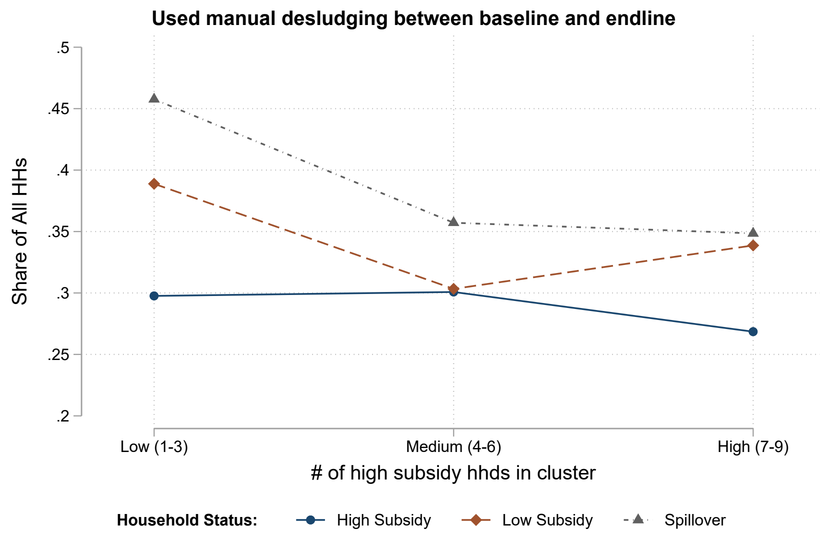
Improvements in health appear to be due to improvement in neighborhood-level sanitation rather than due to improvements in sanitation in the particular household in which the respondent resides. For example, Andrés et al. (2017) finds a household's own improved sanitation leads to a decrease in diarrhea of 10%, while improved sanitation at the community level leads to an additional benefit of 37%. Barreto et al. (2007) finds a decrease in diarrhea of 22% entirely due to neighborhood sewer coverage; none of this improvement is due to the sanitation choices of the household itself.

While the direct goal of the interventions was to increase the use of mechanized and decrease the use of manual desludgings, the indirect goal is to improve health outcomes. In the baseline and endline surveys, we asked whether each household member had one or more episodes of diarrhea in the past week. As a falsification test, we also asked about episodes of cough which should not be affected by local sanitation choices. The systematic review by Wolf et al. (2014) shows evidence that treatment effects on diarrhea are constant across ages. We use both the number and share of all household members, of any age, who experienced an incident of diarrhea or cough in the past week as our outcomes of interest. The fact that we only asked about health in the past week, while the desludging intervention took place over the course of a year, leads the regressions to be under-powered.

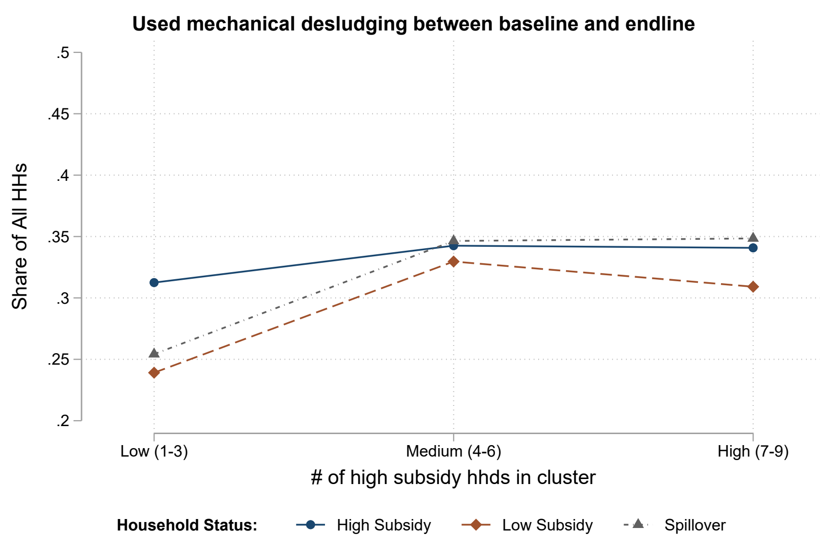
Table 5 shows the effects of the subsidy saturation level on health outcomes. Additional high subsidy households in the cluster decrease the number of people in the household who had diarrhea in the past week by .016 and the share of people in the household who had diarrhea in the past week by .002. These coefficients are not significant at conventional levels but have p-values below 0.20. When focusing on the nearest 5 households (including the house-

Figure 1: Decision Spillovers in Desludging Decisions

(a)



(b)



Note: The top figure shows the share of households that had any manual desludging between the baseline and endline. The bottom figure shows the share of households that purchased any mechanical desludging between the baseline and endline. Households are grouped by whether they were randomly assigned to a high subsidy offer, a low subsidy offer, or spillover (no subsidy offer). Clusters are grouped into those with few (1-3), several (4-6), or many (7-9) high subsidies.

Table 5: Impact on Health Outcomes - OLS

	Had Diarrhea				Had Cough			
	(1) Number	(2) Share	(3) Number	(4) Share	(5) Number	(6) Share	(7) Number	(8) Share
High subsidy	-0.007	0.002	0.017	0.003	-0.006	0.001	0.012	0.001
	(0.038)	(0.004)	(0.041)	(0.004)	(0.051)	(0.005)	(0.053)	(0.005)
# of high subsidy hhds in cluster	-0.016	-0.002			0.002	0.001		
	(0.012)	(0.001)			(0.018)	(0.002)		
# of high subsidy hhds in nearest 5			-0.039**	-0.003*			-0.015	0.001
			(0.018)	(0.002)			(0.022)	(0.002)
# of hhd members with diarrhea in last week in bl	0.139***		0.140***		0.068**		0.068**	
	(0.033)		(0.033)		(0.034)		(0.034)	
# of hhd members with cough in last week in bl	0.016		0.016		0.074***		0.074***	
	(0.016)		(0.016)		(0.017)		(0.017)	
Share of hhd members with diarrhea in last week in bl		0.079***		0.080***		0.056**		0.056**
		(0.019)		(0.019)		(0.025)		(0.025)
Share of hhd members with cough in last week in bl		0.027**		0.026**		0.088***		0.088***
		(0.012)		(0.012)		(0.017)		(0.017)
N	3986	3986	3986	3986	3986	3986	3986	3986
R^2	0.116	0.038	0.116	0.038	0.087	0.036	0.087	0.036
Mean of outcome variable	0.502	0.048	0.502	0.048	0.694	0.070	0.694	0.070

Note: The sample includes all households. Standard errors clustered at the grid-point level in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Outcome variables are # or share of hhd members who had diarrhea or cough in the past week in endline. The outcomes in all columns are from the endline survey. In columns (3), (4), (7), and (8), # of high subsidy hhds in nearest 5 includes the household itself. The table shows controls for either number (odd columns) or share (even columns) of hhd members sick with diarrhea and cough in the past week in the baseline. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size at baseline, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (deposit required, spillover household, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level. Odd columns additionally control for household size in the endline.

hold itself), every additional high subsidy leads to a decrease of .039 in the number or .003 in the share of household members with diarrhea. These effects are both significant at traditional levels. A household whose five nearest sampled neighbors all received a high subsidy, would see a 39% decrease in the number of sick household members, and a 29% decrease in the share of sick household members. The magnitudes are in line with those of the previous literature cited above.

Interestingly, while a household's own subsidy level impacts its own likelihood of adopting, the household's own subsidy level (and hence the household's own sanitation decision) does not impact its own health outcomes conditional on controlling for the neighborhood-level subsidies. Mirroring the results in Barreto et al. (2007), it takes a neighborhood-level change to impact health outcomes. The falsification tests using incidence of cough as an outcome show an effect size close to zero, as we would have expected. In Appendix G Table G-6, we show the IV results from equation (4), instrumenting for the number of mechanized desludgings in a cluster with the number of high subsidy households in the cluster. Because this instrument is relatively weak, this is not our preferred specification. The results are qualitatively similar to those from the OLS regressions.

7 Disentangling Mechanisms

In the previous section we showed both decision spillovers and health externalities in urban sanitation. Next we explore potential mechanisms behind them. We consider social mechanisms including social pressure, learning-from-others, coordination, and reciprocity; and non-social mechanisms including decreasing costs or increasing returns to sanitation investments.

7.1 Experimental Design - Mechanisms

We conducted three randomized interventions to disentangle the mechanisms. First, we randomized whether the households knew the subsidy level offered to each of their neighbors. Second, for some households in some clusters, before the respondents made their own decision we randomly informed them either how many or which of their neighbors had signed up for the subsidized mechanized desludging service. Finally, we randomly chose some clusters to participate in an incentivized non-anonymous dictator game.[11] The breakdown of the randomizations can be found in Appendix B.

The *'public-price'* treatment varies social pressure by randomizing whether households are told the subsidy level of their neighbors. We hypothesized that households which received a high subsidy and whose neighbors were told about this might face stronger pressure to purchase the sanitation service than households who had received the high subsidy secretly. In the half of the clusters that we call *'public-price'*, all treated households were given a sheet of paper listing the names of the ten treated households in their cluster and the subsidy level offered to each one. In the other half of the clusters, treated households were given a sheet listing the names of the ten households in their cluster, and were told that on average half of the households were offered a high subsidy and half were offered a low subsidy. Examples of the two sheets can be found in Appendix E.

The *'public-how-many/who'* treatments vary learning-from-others and coordination by randomizing whether households are told how many or which of their neighbors signed up for the subsidized desludging. In half of the clusters, we randomly split the ten treatment households into a group of five which enumerators interviewed first. After that, the second half were interviewed and were informed about the choices of the first group. In half of those clusters (*'public-how-many'*), the second five respondents were told the number of households in the first five that adopted. In the other half of those clusters (*'public-who'*), the second five respondents were told the names of the first five households that adopted. In the control clusters, households were surveyed in the order that minimized logistical difficulty. For those clusters, we did still randomly split the households into two groups ex-ante, though this split was

[11] The dictator games were not a treatment. We randomized participation because we did not have enough funding for all households to participate.

not used to determine the order of interviews. We use this split to label all respondents as either being in the *'first five'* or *'second five'*.[12][13]

Finally, in a quarter of the clusters we ran an incentivized economic experiment measuring reciprocity. Respondents answer questions about their real-world network links and also choose how much money to give to each other in a directed dictator game. The game was conducted at the end of the baseline (pre-intervention) and endline. Respondents did not know anything about the intervention when they played at baseline. Every respondent was given 1200 CFA (approximately $2) to divide between himself and the other 11 households in his cluster. Money sent to other households was doubled, while money kept for himself was not. The experimental script can be found in Appendix F. Play in the baseline measures pre-intervention altruism, while play in the endline can additionally express reciprocity towards those who adopted mechanized desludging. In other settings, participants have been found to express reciprocity for actions taken outside of the experiment with their giving in an experiment (Ligon & Schechter 2012).

In the clusters which participated in the experiment, we took advantage of the fact that we had to return to hand out winnings and also conducted a mini-survey. In that survey, we asked the respondent what subsidy level he remembered each of the other households getting and whether, according to his memory, each of the other households signed up for the desludging.

After ruling out social mechanisms, we consider the non-social mechanisms of decreasing costs and increasing returns. To do this, we check for differential changes in reported prices in neighborhoods with many versus few subsidies.

7.2 Estimation Strategy - Mechanisms

Before analyzing the sub-randomizations varying information availability, we first look for suggestive evidence of social decision spillovers using the social

[12] Thus 'first five' households in 'public-how-many' and 'public-who' clusters were interviewed first, while 'first five' households in the other clusters were interviewed in any order.

[13] Unfortunately, the original survey programming of the 'public-how-many' intervention left the number of households that had signed up blank. This affected the first 24% of 'public-how-many' households before we fixed the programming.

network data. Table 3 showed that an increase in the number of high subsidy households in a cluster makes households more likely to choose a mechanized desludging. Here we ask whether this effect is stronger when the high subsidy households are those with whom the respondent is more closely linked in the social network.

To study these effects we add on to equation (2) a measure of the number of surveyed households in a cluster with which the household shares a particular relationship N^r_{iga} (e.g., the number of households out of 11 with whom he drinks tea) and the number of *high subsidy* households in the cluster with whom he shares that relationship NH^r_{iga}. The network relationships we focus on include: which households they are aware of, which they drink tea with, which they talk about sanitation with, and which they would pick to lead a health information campaign. We estimate

$$y_{iga} = \alpha + H'_{iga}\beta_1 + \overline{H}'_{ga}\beta_2 + S'_{iga}\beta_3 + X'_{iga}\gamma + N^{r'}_{iga}\beta_4 + NH^{r'}_{iga}\beta_5 + \psi_a + \epsilon_{iga} \quad (5)$$

separately for each of the social network relationships listed above, in addition to wealthy households and households in the nearest five geographically (both including themselves). This will give us suggestive evidence as to whether social interactions matter, and as to whether the effects depend on geography.

We can also use the three additional randomized interventions which made some form of information public to disentangle the mechanisms. We create an indicator for clusters where the subsidy levels offered to each household were made public (P^p_{ga}), standing for public price. We also create indicators for whether the household is a second-five household in a public-how-many cluster (P^n_{iga}) or is a second-five household in a public-who cluster (P^w_{iga}). In what follows, we discuss estimation of each of the different mechanisms.

7.2.1 Social Pressure

If a high subsidy household's neighbors know the subsidy level offered, then the high subsidy household may face *social pressure* to adopt due to the health externalities that neighbors know will arise. To test whether high subsidy households are more likely to adopt when their neighbors were told about their high subsidy level, we add an interaction term between living in a public-price cluster and receiving the high subsidy. We run the following regression:

$$y_{iga} = \alpha + H'_{iga}\beta_1 + P^{p'}_{ga}\beta_2 + H_{iga}P^{p'}_{ga}\beta_3 + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{6}$$

If our hypothesis holds, we would expect the coefficient on the interaction term, β_3, to be positive as those who received a high subsidy and whose neighbors knew about it would be more likely to adopt.

7.2.2 Learning and Coordination

We also examine *learning from others and coordination*. The effect of telling the second five households visited about the choices made by the first five visited should depend on the content of the information they are given. When a household member is told that more households in his cluster signed up, this may make him more likely to sign up if he learns about the high value of mechanized desludging. Alternatively, the household may be more likely to sign up due to coordination; if there are increasing returns to adoption then the benefit of signing up for a mechanized desludging increases as more neighbors also sign up for it. Note that signing up for the subsidized mechanized desludging during the survey is a noisy signal of whether the household will actually adopt. The extent to which respondents react to the information depends on how likely they think their neighbors who signed up are to follow through.

We run a regression on all second-five households in clusters where information was and was not given. We add a control for the number of households in the first five that signed up in that cluster, N^s_{ga}, and the interaction of that with being in a public-how-many or public-who cluster. We run the following regression:

$$y_{iga} = \alpha + H'_{iga}\beta_1 + N^{s'}_{ga}\beta_2 + P^{n'}_{ga}\beta_3 + N^s_{ga}P^{n'}_{ga}\beta_4 + P^{w'}_{ga}\beta_5 + N^s_{ga}P^{w'}_{ga}\beta_6 + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{7}$$

We expect β_4 and β_6 to both be positive if learning that more households in one's cluster signed up increases the household's likelihood of signing up. Because the number of households in the first five that sign up is endogenous, we show an additional specification in which we instrument for the number of households in the first five that signed up (N^s_{ga}) with the number in the first five that had a high subsidy and the number that had to leave a deposit. We instrument for the interaction between the number that signed up in the first five and the randomized cluster type ($N^s_{ga}P^n_{ga}$ and $N^s_{ga}P^w_{ga}$) with those two instruments interacted with the cluster type.[14]

If we see no impact in the above-mentioned regressions, it may be because it doesn't matter how many households in the first five signed up, but rather which specific households signed up. We next focus in on the effects of being told that households with different network characteristics have signed up. We exclude the public-how-many clusters because it should not matter which households in the first five sign up when the respondent is only told how many but not which households signed up. We add controls (and the interaction of the controls with being in a public-who cluster) for the number of households in the first five that signed up and have different traits, N^{st}_{iga}. For example, this variable might be the number of households in the first five with whom the respondent drinks tea that signed up. We also control for the number of first-five households that have that trait, regardless of whether or not they signed up, N^t_{iga}. We run the following regression on all second-five households in clusters other than the public-how-many clusters:

$$\begin{aligned} y_{iga} = \alpha &+ H'_{iga}\beta_1 + N^{s'}_{iga}\beta_2 + N^{t'}_{iga}\beta_3 + N^{st'}_{iga}\beta_4 \\ &+ P^{w'}_{ga}\beta_5 + N^t_{iga}P^{w'}_{ga}\beta_6 + N^s_{iga}P^{w'}_{ga}\beta_7 + N^{st}_{iga}P^{w'}_{ga}\beta_8 + X_{iga}\gamma + \psi_a + \epsilon_{iga}. \end{aligned} \tag{8}$$

[14] Earlier in the paper we instrument for the number of households that purchased a mechanized desludging with the number that were offered the high subsidy. Here we instrument for the number that signed up with the number that were offered the high subsidy and the number that had to leave a deposit. We add the second instrument here because the deposit requirement did affect signing up, but did not end up affecting actual usage. See Lipscomb & Schechter (2018) for more discussion of the deposit requirement.

We expect β_8 to be positive if respondents are more likely to sign up when they learn that somebody in their social network has also signed up. We also show results instrumenting for the number of households of a certain type adopting with the number of those households that were offered a high subsidy and the number that had to leave a deposit. Our preferred specification is the OLS estimation due to weak instruments in the IV.

7.2.3 Reciprocity

Households may also adopt in anticipation of *reciprocity* from their neighbors, expecting that their neighbors will thank them in some way for providing a public good. The outcome of interest is the amount sent by person i to person j in the endline game (or the relationship between person i and j in the endline survey): A_{ijga2}. We control for individual giver i fixed effects to account for unobserved differences in altruism across individuals. We control for the amount sent from i to j in the baseline A_{ijga1} to control for the altruism i feels towards j specifically. The dyadic regression looks as follows:

$$A_{ijga2} = \alpha + A'_{ijga1}\gamma + X'_{jga}\beta + \psi_i + \epsilon_{ijga}. \tag{9}$$

Variables X_{jga} include household j's subsidy level, whether household j lives in a public-price cluster, and whether household j signed up for the subsidized desludging or purchased a manual desludging between baseline and endline, and the double and triple interactions between those variables. Both treatment and spillover households participated in the games and so all are included in the regressions.

Finding a negative coefficient on the receiver's purchase of a manual desludging when offered a high subsidy in a public-price cluster would imply that givers were punishing receivers who chose not to improve local sanitation when the cost to themselves was low. That coefficient may be approximately zero for one of two reasons. On the one hand when playing in the endline, the players may not remember which of their neighbors were offered a high subsidy and which signed up. On the other hand they may remember that information, but not feel it is important enough to change their giving behavior.

To distinguish between these two explanations, we check whether respondents are aware of their neighbors' subsidy levels and sign-up decisions weeks and months after the baseline survey. In the clusters participating in the incentivized experiment, we returned a few weeks after the baseline survey to give the participants their winnings. We took advantage of that opportunity to also ask them what price they thought we offered to each of the other treatment households in their cluster, and whether they thought the household had signed up for the subsidized desludging. We also asked the same questions to all households in the endline survey a year later.

Using this data we can directly test whether the information treatments increase knowledge a few weeks and a year after the intervention. Define C_{ijga} as household i correctly knowing the treatment or decision of household j. We then estimate the following dyadic regression equation:

$$C_{ijga} = \alpha + P^{p'}_{ga}\beta_1 + P^{w'}_{iga}\beta_2 + P^{w'}_{jga}\beta_3 + P^{w}_{iga}P^{w}_{jga}\beta_4 + X'_{iga}\gamma + \psi_a + \epsilon_{ijga}. \quad (10)$$

Here P^{w}_{iga} is still an indicator for household i being a second-five household in a pubic-who cluster and P^{w}_{jga} is an indicator for household j being a first-five household in a public-who cluster. When both P^{w}_{iga} and P^{w}_{jga} equal 1, this would mean that giver i had been given information about the sign-up decision made by household j. Included in the controls is an indicator for whether the same household member responded to both the original survey and the follow-up survey, as well as a control for how many weeks elapsed between the two surveys.

7.2.4 Increasing Returns and Decreasing Costs

The final mechanisms we consider, *increasing returns and decreasing costs* to improved sanitation within a cluster, are not social. We did not design an experiment to tease out this mechanism, so our exploration of this mechanism is more speculative. Our intervention could have affected the price of a mechanized desludging on the open market. On the one hand, our intervention could lead to an increase in price if it increases households' willingness to pay for a mechanized desludging and leads to a move up the supply curve. On the other hand, our intervention could lead to a decrease in price if it caused trucks to be present in a neighborhood more often, decreasing trucker costs or increasing trucker awareness of the neighborhood. There is a limit to how much the truckers' costs could be decreased, since trucks can rarely service two households on the same trip. Testing for effects on price allows us to confirm this insight.

We look at prices paid in the endline by either spillover households only, or both spillover households and low-subsidy households. This regression is limited to those households which purchased a mechanized desludging between the baseline and endline since those are the households who reported a price. We exclude the high subsidy households since the price they paid is often the highly subsidized price we offered them. We run a regression similar to equation (2) in which the outcome is the price paid for a mechanized desludging in the endline and we control for the price that household paid for a mechanized desludging in the year before the baseline (as well as an indicator for whether the household did not have a mechanized desludging before the baseline) and either the number of high subsidy households in the cluster or the number of high subsidy households in the nearest five.

7.3 Results - Mechanisms

We first explore whether decision spillovers are stronger when they come from households with whom the respondent is linked. In Table 6 we estimate equation (5). We see that it is the overall number of high subsidy households in the cluster that determines whether a household purchases a mechanized or manual desludging. An additional nearby household with a high subsidy increases the probability that a household will purchase a mechanized desludging by 1.1 pp –that is an effect of approximately 3.4% at the mean. It does not matter if the high subsidy households were known by the respondent, friends with the respondent, or respected by the respondent. It also doesn't matter if the high subsidies are concentrated among residents in the nearest five or not.

7.3.1 Social Pressure

Next we explore social pressure by looking at whether high subsidy households are more likely to adopt when living in a cluster where their subsidy level was public information. We estimate equation (6) in Table 7. We do not find any evidence of social pressure playing a significant role. The coefficient on the interaction term in the signed up regression is far from significant and of the wrong sign. As publicizing the subsidy level has no impact on signing up initially, it is unlikely that it would have impacts on adoption measures later in time. The coefficient on the interaction term is only significant in the regression of having a manual desludging, and is of the opposite sign as would be expected. Overall we do not find evidence that social pressure plays a role in encouraging sanitation decision spillovers.

Why might social pressure not work in this setting? According to the set-up of Bursztyn & Jensen (2017), it may either be because in an urban setting a household's reference group does not consist of its neighbors or, it may be because households do not have a strong belief about what is socially desirable behavior in this urban setting.[15] In our data even closer connections show minimal effects, although it may be that we have not identified the right connections. Informal discussions during the design phase of this project made clear that manual desludging is not considered socially desirable.

7.3.2 Learning and Coordination

Next we explore learning from others and coordination. There is limited evidence that informing households how many of their neighbors have signed up increases either signing up or purchasing. In the odd columns in Table 8 using OLS, we see that in areas where more households in the first five sign up and their neighbors are told about this, a household is more likely to sign

[15] A third reason might be because the action is not easily observable, but in our setting choosing a manual desludging and leaving the sludge in the street nearby is easily observable.

up for the subsidized mechanized desludging and more likely to use it. This is not true for being in a public-who cluster[16] and that result does not continue to hold in the IV regressions in the even columns. None of the results carry over to actual adoption decisions. Overall, this is relatively weak evidence of coordination or learning from others. The absence of an effect of having more neighbors sign-up is not in contradiction with our earlier results showing decision spillovers. Signing up is purely cheap talk and may be interpreted as such by neighbors, while actually purchasing a mechanized desludging is a costly investment.

If respondents were learning from others rather than coordinating, we would expect a household's relationship with the others which chose to sign up to be important. For example, if a household that is seen as a local leader signs up, this should be more impactful than if a household whose members are not known by the respondent signs up. In Table 9, we look at estimation of equation (8). In the table we show heterogeneity with respect to six network traits.[17] As before, the odd columns of Table 9 show OLS results and the even columns show IV results. The OLS are our preferred results given that the IV regressions suffer from weak instruments, with F-statistics ranging from 1.5 to 8 depending on the specific regression.[18] There are few statistically significant coefficients across the table. Overall, these tables suggest that learning from others (at least from residential neighbors) does not affect sign-up decisions or other outcomes in these urban areas of Dakar, while coordination has very small or no impact.

[16]It may be that the mystery of knowing that many households signed up but not knowing exactly which encourages take up, or that households attribute signing up to neighbors that they most trust when not provided with the information.

[17]To be consistent in our definitions throughout the paper, the closest five households are calculated over the full 12 households in the cluster and include the household itself.

[18]Lee et al. (2020) show that even the commonly used benchmark of 10 for F-statistics is often too low and can lead to a downward bias in the standard errors.

Table 6: Decision Spillovers and Social Networks in Desludging Decisions

(1) # of high subsidy hhds in cluster that ...	(1) Any Mechanized Desludging	(2) Manual Desludging
# of high subsidy hhds in cluster	0.011*	-0.000
	(0.007)	(0.007)
(1) ... you are aware of (bl)	0.000	-0.017**
	(0.007)	(0.008)
# of high subsidy hhds in cluster	0.011**	-0.007
	(0.005)	(0.006)
(1) ... you drink tea with (bl)	-0.001	-0.013
	(0.009)	(0.009)
# of high subsidy hhds in cluster	0.011**	-0.009
	(0.005)	(0.005)
(1) ... you would pick as health leader (bl)	0.003	-0.018
	(0.012)	(0.013)
# of high subsidy hhds in cluster	0.011**	-0.011**
	(0.005)	(0.005)
(1) ... you talked about sanitation with (bl)	0.005	0.002
	(0.013)	(0.012)
# of high subsidy hhds in cluster	0.014***	-0.016***
	(0.005)	(0.005)
(1) ... are wealthy (bl)	-0.010	0.016*
	(0.009)	(0.009)
# of high subsidy hhds in cluster	0.013**	-0.012**
	(0.005)	(0.006)
(1) ... are in nearest 5	-0.004	0.004
	(0.008)	(0.008)
N	3984	3984
Mean of outcome variable	0.316	0.326

Note: Each pair of rows shows estimates of β_2 and β_5 from a different variant of equation (5). The sample includes all households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) purchased any mechanized desludging between the baseline and endline, and (2) had any manual desludging between the baseline and endline. The outcomes in all columns come from the endline survey. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (high subsidy, deposit required, spillover household, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level. Each regression also controls for the # of hhds in the cluster with the relation in question (e.g., # of hhds in cluster with which they drink tea).

Table 7: Impact of Social Pressure on Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
High subsidy	0.206***	0.085***	0.029	-0.065***
	(0.020)	(0.013)	(0.019)	(0.019)
Public price cluster	-0.005	-0.007	-0.011	-0.018
	(0.022)	(0.008)	(0.019)	(0.021)
High subsidy × Public-price cluster	-0.030	-0.007	0.004	0.062**
	(0.030)	(0.018)	(0.026)	(0.028)
N	3654	3654	3308	3308
R^2	0.105	0.083	0.302	0.269
Mean of outcome variable	0.400	0.075	0.315	0.316

Note: The sample includes all treatment households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) signed up for the subsidized mechanized desludging, (2) purchased the subsidized mechanized desludging, (3) purchased any mechanized desludging between the baseline and endline, and (4) had any manual desludging between the baseline and endline. The outcome in column (1) comes from the baseline survey, in (2) from the administrative data, and in (3)-(4) from the endline survey. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (deposit required, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level.

Table 8: Impact of Learning and Coordination on Desludging Decisions

	Signed Up		Subsidized Desludging		Any Mechanized Desludging		Manual Desludging	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
High subsidy	0.195***	0.187***	0.086***	0.085***	0.036*	0.033	-0.031	-0.033
	(0.021)	(0.023)	(0.013)	(0.014)	(0.020)	(0.021)	(0.021)	(0.021)
Public-how-many cluster	-0.069	-0.378	-0.040	-0.058	-0.027	-0.121	0.022	-0.008
	(0.052)	(0.280)	(0.030)	(0.133)	(0.042)	(0.178)	(0.042)	(0.173)
Public-who cluster	0.002	0.136	0.002	-0.052	-0.052	-0.167	0.016	0.020
	(0.045)	(0.153)	(0.021)	(0.096)	(0.033)	(0.151)	(0.045)	(0.152)
# signed up in first 5	0.042***	0.040	0.009	-0.067	0.025*	0.008	-0.020*	-0.056
	(0.012)	(0.066)	(0.007)	(0.042)	(0.013)	(0.062)	(0.011)	(0.059)
# signed up in first 5 × Public-how-many cluster	0.062**	0.231	0.035**	0.041	0.005	0.057	0.001	0.015
	(0.026)	(0.150)	(0.017)	(0.071)	(0.021)	(0.096)	(0.020)	(0.094)
# signed up in first 5 × Public-who cluster	0.019	-0.068	0.002	0.020	0.017	0.087	-0.007	-0.018
	(0.021)	(0.086)	(0.012)	(0.055)	(0.019)	(0.086)	(0.020)	(0.088)
N	1711	1711	1711	1711	1545	1545	1545	1545
R^2	0.121	0.077	0.095	-0.001	0.318	0.339	0.292	0.288
Mean of outcome variable	0.400	0.400	0.079	0.079	0.328	0.328	0.295	0.295
First stage Cragg-Donald F-statistic		10.985		10.985		9.489		9.489

Note: The sample includes all second-five households except those in early public-how-many clusters for which there was a programming glitch in the survey. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1/2) signed up for the subsidized mechanized desludging, (3/4) purchased the subsidized mechanized desludging, (5/6) purchased any mechanized desludging between the baseline and endline, and (7/8) had any manual desludging between the baseline and endline. The outcome in columns (1/2) comes from the baseline survey, in (3/4) from the administrative data, and in (5)-(8) from the endline survey. Three endogenous variables (A) # signed up in 1st 5, (B) # signed up in 1st 5 × public-how-many cluster, and (C) # signed up in 1st 5 × public-who cluster are instrumented by 6 variables. (i) # high subsidy in 1st 5, (ii) # deposit required in 1st 5, (iii) # high subsidy in 1st 5 × public-how-many cluster, (iv) # deposit required in 1st 5 × public-how-many cluster, (v) # high subsidy in 1st 5 × public-who cluster, and (vi) # deposit required in 1st 5 × public-who cluster. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (deposit required and public-price cluster), and fixed effects at the arrondissement level.

Table 9: Impact of Learning and Coordination on Desludging Decisions

# signed up in 1st 5 that ...	Signed Up		Subsidized Desludging		Any Mechanized Desludging		Manual Desludging	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
you are aware of × Public-who cluster	0.016	0.171	-0.000	0.002	-0.014	-0.111	-0.012	0.040
	(0.050)	(0.217)	(0.023)	(0.112)	(0.037)	(0.164)	(0.043)	(0.184)
you drink tea with × Public-who cluster	0.039	-0.508	-0.040	-0.220	0.007	0.037	0.013	-0.239
	(0.054)	(0.423)	(0.025)	(0.177)	(0.040)	(0.245)	(0.044)	(0.288)
you would pick as health leader × Public-who cluster	0.052	0.163	0.001	0.022	0.015	0.109	-0.136**	-0.152
	(0.082)	(0.448)	(0.040)	(0.206)	(0.065)	(0.351)	(0.068)	(0.429)
you talked about sanitation with × Public-who cluster	-0.017	0.003	0.040	-0.096	-0.044	-0.234	0.046	-0.238
	(0.072)	(0.321)	(0.034)	(0.211)	(0.062)	(0.265)	(0.057)	(0.282)
are wealthy × Public-who cluster	0.060	0.017	-0.044	0.036	-0.024	-0.178	-0.050	-0.036
	(0.058)	(0.217)	(0.029)	(0.130)	(0.057)	(0.189)	(0.050)	(0.198)
are in nearest 5 × Public-who cluster	-0.096*	0.099	-0.052*	-0.089	-0.004	0.131	-0.052	-0.101
	(0.052)	(0.250)	(0.027)	(0.145)	(0.045)	(0.238)	(0.044)	(0.238)
N	1360	1360	1360	1360	1228	1228	1228	1228
Mean of outcome variable	0.390	0.390	0.074	0.074	0.332	0.332	0.296	0.296

Note: Each row shows estimates of β_8 from a different variant of equation (8). The sample includes all second-five households except those in public-how-many clusters. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1/2) signed up for the subsidized mechanized desludging, (3/4) purchased the subsidized mechanized desludging, (5/6) purchased any mechanized desludging between the baseline and endline, and (7/8) had any manual desludging between the baseline and endline. The outcome in columns (1/2) comes from the baseline survey, in (3/4) from the administrative data, and in (5)-(8) from the endline survey. Four endogenous variables (A) # signed up in 1st 5, (B) # signed up in 1st 5 × public-who cluster, (C) # signed up in 1st 5 with certain trait, and (D) # signed up in 1st 5 with certain trait × public-who cluster. These are instrumented by 8 variables (i) # high subsidy in 1st 5, (ii) # deposit required in 1st 5, (iii) # high subsidy in 1st 5 × public-who cluster, (iv) # deposit required in 1st 5 × public-who cluster, (v) # high subsidy in 1st 5 with certain trait, (vi) # deposit required in 1st 5 with certain trait, (vii) # high subsidy in 1st 5 with certain trait × public-who cluster, and (viii) # deposit required in 1st 5 with certain trait × public-who cluster. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (high subsidy, deposit required, public-price cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level. Additional controls in this table include: # in 1st 5 with certain trait, # in 1st 5 that signed up with certain trait, # in 1st 5 with certain trait × public-who cluster.

7.3.3 Reciprocity

We hypothesized that another reason that households might adopt mechanized desludging would be an attempt to curry favor with their neighbors. We test this by looking at whether we can find evidence that generosity towards a neighbor increases after that neighbor signs up or decreases after they purchase a manual desludging. We also test whether this reciprocal generosity is greater when the recipient signed up despite being offered a low subsidy in the public neighborhoods.

We show results from estimating equation (9) in Table 10. Columns (1) and (3) look at whether signing up affects experimental giving, as well as the differential effect of signing up with a low versus a high subsidy level. Columns (2) and (4) explore the differential effects of purchasing a manual desludging at different subsidy levels. The first row shows that the amount an individual sends to a partner in the endline is highly correlated with how much he sent to that same partner in the baseline. This reassures us that the players understood the game.

We see little evidence of reciprocity in response to neighbors' desludging decisions, either signing up for our subsidized service or having purchased a manual desludging in the year between the baseline and the endline more generally.[19] In the first two columns, none of the coefficients on receiver characteristics are significant. In the last two columns, some coefficients are significant but they do not tell a consistent story. If anything, recipients who were unlucky enough to be offered a low subsidy and who live in clusters where this information was made public are subsequently sent less money by their neighbors, rather than more. The fact that individuals do not punish neighbors for bad desludging behavior suggests a limited role for reciprocity.

Similarly, in results not shown here, we run the same regression but instead look at the real world social network outcomes of being aware of a neighbor, drinking tea with a neighbor, and lending money to a neighbor. While we do see evidence that people are more likely to be aware of and drink tea

[19]The results are quite similar if the outcome is an indicator for giving anything to that recipient instead of the amount given to the recipient.

Table 10: Evidence of Reciprocity

	Amount given to receiver in el experiment			
	(1)	(2)	(3)	(4)
Amt. given to receiver in bl experiment	0.19***	0.21***	0.19***	0.21***
	(0.03)	(0.03)	(0.03)	(0.03)
Receiver characteristics:				
High subsidy	3.09	4.44	4.47	6.22
	(3.04)	(3.50)	(2.84)	(3.77)
Low subsidy	0.53	3.65	8.05**	9.99**
	(2.62)	(3.67)	(3.27)	(4.00)
High subsidy × Signed up	0.19		1.19	
	(2.91)		(3.68)	
Low subsidy × Signed up	0.34		-4.34	
	(2.61)		(3.52)	
Man. desl. btw bl and el		3.62		2.05
		(5.39)		(4.03)
High subsidy × Man. desl.		-0.44		1.28
		(6.17)		(5.86)
Low subsidy × Man. desl.		-3.64		-0.05
		(5.96)		(4.95)
High subsidy × Public-price cluster			-2.94	-3.07
			(5.90)	(6.85)
Low subsidy × Public-price cluster			-14.79***	-12.73*
			(4.97)	(7.07)
High subsidy × Signed up × Public-price cluster			-1.47	
			(5.77)	
Low subsidy × Signed up × Public-price cluster			9.49*	
			(5.17)	
Man. desl. × Public-price cluster				2.58
				(10.57)
High subsidy × Man. desl. × Public-price cluster				-3.83
				(11.98)
Low subsidy × Man. desl. × Public-price cluster				-5.39
				(11.60)
N	11025	9424	11025	9424
R^2	0.04	0.04	0.04	0.04
Mean of outcome variable	79.51	80.90	79.51	80.90

Note: The sample includes dyads for all pairs of households in all clusters participating in the incentivized experiment. Fixed effects at the giver level. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variable is "the amount given to that receiving partner in endline experiment."

with households that sign up for the desludging, this may just be because the type of household that signs up is more integrated in social networks. Real-world social network linkages are not affected by what type of desludging the household ended up using and the impact of signing up is not differentially affected by low versus high subsidy levels.

There are two possible reasons for this lack of evidence of reciprocity. It could be that neighbors know each other's prices and sign-up status, but that this doesn't invoke feelings of reciprocity. Or, it could be that respondents do not find this information to be important and so they quickly forget it. Appendix Table G-7 estimates equation (10) and shows that the public-price intervention did successfully increase knowledge regarding the price offered to neighbors. Column (1) shows that household members are 7 pp more likely to correctly remember their neighbor's subsidy level two weeks later and column (3) shows an impact of 1 pp a year later. But, low levels of knowledge regarding neighbors' prices suggests the price offer was not very salient or memorable and thus did not trigger feelings of reciprocity.

7.3.4 Increasing Returns to Sanitation and Decreasing Costs

We find evidence of significant decision spillovers and yet we found no evidence of social mechanisms. As a result, we explore increasing returns and decreasing costs as a non-social explanation for the decision spillovers we observe. We check whether the intervention has an effect on prices paid by households purchasing mechanized desludgings on the open market.

In Appendix Table G-5, we find that the price paid for a mechanized desludging by spillover households is higher when more of their neighbors were offered a high subsidy. The price of a mechanized desludging increases by about 3% for each household in the cluster that is offered a high subsidy. The effect is not significant for low subsidy households, but of course they have been offered a subsidized price so we don't expect them to be purchasing their desludging on the open market at a higher price as often. This effect on price is more evidence that increased demand by subsidized households led to increased demand by the spillover households who paid, and were willing to pay, a higher price.

8 Conclusion

We show that decision spillovers and health externalities in sanitation can be large. Unsubsidized households and households receiving low subsidies use improved sanitation at greater rates when more households in their cluster are offered a high subsidy. This increase in mechanized desludgings leads to a health externality of decreased diarrhea incidence. The impact of offering an additional high subsidy in a neighborhood tapers off as more households receive high subsidies, suggesting that decision spillovers may be most important in areas where the use of a technology is less common.

When we explore social mechanisms behind this result, our results are mostly negative. We find no evidence for social effects: neither social pressure, nor reciprocity, nor learning from others. Most of the literature finding rich and strong social effects in sanitation decisions was conducted in rural areas. In our urban setting, what matters is the number of households in a cluster that were offered the high subsidy, not which specific households were offered the subsidy. While we do not find evidence of social effects, it is worth remembering that in an urban setting, social effects may be more likely between colleagues from work or religious organizations rather than between residential neighbors. Also, our intervention may change neighborhood cleanliness norms without social interactions.

We provide evidence consistent with the hypothesis that the decision spillovers we find are due to increasing returns from investment in sanitation. As a neighborhood becomes cleaner, it becomes more worthwhile for additional households to make more sanitary decisions themselves. We leave it to future research to design interventions more explicitly geared towards exploring non-social sources of decision spillovers.

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For Online Publication

A Script Explaining Subsidized Desludging Service in Surveys

The below script is translated from Wolof to English. It appeared on the portable devices used by the enumerators with the different wordings automated by treatment group.

Today, we are going to offer you a subscription to a mechanized desludging service. Mechanized desludging, it's very important, for you, your family, and your neighbors. When you use a truck to desludge your pit, the truck takes all of the filth from the pit, takes it far away from the house, so that you're sure that your house and the area around it is all very clean, and your children and other children in the neighborhood will not play in that filth.

The subscription to the service that we're offering is very useful: it will help you plan for when you will need to desludge your pit, and it is thanks to the subscription that we will be able to subsidize the cost of a desludging over time, and it will enable you to have access to a quality desludging.

If you agree to sign up, when you need a desludging, you will call ZZ, identify yourself as a subscriber, and say that you need a desludging. We'll then find a truck to desludge your pit within about 2 to 3 hours of the call. The desludging service covers one trip by one truck, getting about 8 m^3 from the pit, without 'curage'.

Of the twelve houses near you that we chose to participate in the research, ten will be offered a subsidized mechanized desludging. There are small subsidies and large subsidies, and of those 10 households, each household has a 50% chance of being offered a large subsidy. The other two households will not be offered the chance to subscribe to the desludging service.

[*For private-price treatment clusters.*] We randomly selected each household's subsidy level. We are leaving you with a piece of paper that lists the names of all households near you which were offered a subsidy, but the subsidy level offered to each household will not be told to the other households.

[*For public-price treatment clusters.*] We randomly selected each household's subsidy level. We are leaving you with a piece of paper that lists the name and subsidy level of each household living near you.

[*Enumerator: Pause, give the list to the respondent, and read it aloud with him.*]

You can use the subsidy to desludge your pit twice within the next 9 months. [*Note: Later changed to 12 months.*] If you need more than two desludgings within that period, these additional desludgings will not be subsidized. Also, if you do not desludge your pit twice during this period, you will not be able to use the subsidy after those 9 [*Note: Later 12*] months.

In a few weeks, we will come back to the households that decide to sign up for the service to put a sticker on their door signaling that the house signed up.

The undiscounted price of a desludging is 25000 CFA. Your discount is: [discval]. So, you will pay [25000 - discval] for each of your first 2 desludgings over the next 9 months.

[*For second five households in public-how-many clusters.*] We have already asked [List of First 5 Offered] whether or not they want to sign up, and [Number of Signed Up in First 5] of them have decided to sign up.

[*For second five households in public-who clusters.*] We have already asked [List of First 5 Offered] whether or not they want to sign up, and [List of Signed Up in First 5] have all decided to sign up.

[*For deposit treatment households.*] If you would like to sign up for the subsidized mechanized desludging service, you will have to leave a deposit of 3000 CFA. We will take this 3000 CFA from your participation fee, so you will not have to give us any money out of pocket if you sign up. Would you like to sign up?

[*For non-deposit treatment households.*] Would you like to sign up for the subsidized mechanized desludging service? You do not have to pay anything now.

[*Enumerator records whether the respondent signed up. The rest of the* *script containing details on how they can use the service is only read to people who sign up.*]

B Layout of Randomization

At the cluster level, the 410 clusters are split across 12 types.

- Public-price, Public-how-many, No experiment (39 clusters)
- Public-price, Public-how-many, Experiment (13 clusters)
- Public-price, Public-who, No experiment (38 clusters)
- Public-price, Public-who, Experiment (13 clusters)
- Public-price, Private-signup, No experiment (78 clusters)
- Public-price, Private-signup, Experiment (25 clusters)
- Private-price, Public-how-many, No experiment (40 clusters)
- Private-price, Public-how-many, Experiment (12 clusters)
- Private-price, Public-who, No Experiment (37 clusters)
- Private-price, Public-who, Experiment (13 clusters)
- Private-price, Private-signup, No experiment (77 clusters)
- Private-price, Private-signup, Experiment (25 clusters)

At the household level, we have 4 different types of households:

- High discount, no deposit (255 households).
- Low discount, no deposit (254 households).
- High discount, deposit (1792 households).
- Low discount, deposit (1799 households).

At the cluster level we randomized the number of households, out of ten, which were offered the high subsidy.

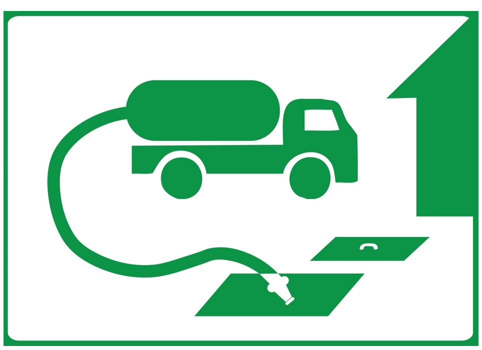
- One high subsidy household (8 clusters)
- Two high subsidy households (8 clusters)
- Three high subsidy households (43 clusters)
- Four high subsidy households (98 clusters)

- Five high subsidy households (106 clusters)
- Six high subsidy households (81 clusters)
- Seven high subsidy households (50 clusters)
- Eight high subsidy households (8 clusters)
- Nine high subsidy households (8 clusters)

At the cluster level we randomized the number of households, out of ten, which were required to leave a deposit if they wanted to sign up for the subsidized mechanized desludging service.

- Six households must pay deposit (16 clusters)
- Seven households must pay deposit (41 clusters)
- Eight households must pay deposit (67 clusters)
- Nine households must pay deposit (188 clusters)
- Ten households must pay deposit (98 clusters)

C Sticker Given to Households which Signed Up

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D Variables Offered to and Selected by LASSO

In the appendix we show that our main results are robust to choosing the controls using post-double selection lasso. This method was designed by Belloni et al. (2014) and is useful when estimating the causal impact of one or more variables. Choosing control variables using post-double-selection lasso reduces error and increases statistical power. The 'double' comes from the fact that covariates are included which predict the outcome, and covariates are included which predict the control of interest.

We consider 131 variables measured in the baseline which could potentially be included as controls in the regressions. We list them here in general categories. (These categories are irrelevant for the estimation, but may be helpful when considering the variables.) We replace missing observations with the sample mean and also include indicator variables for observations that are missing a value for each variable. In theory this could imply 131 additional indicator variables though in practice it only leads to 37 additional variables since not all variables have missing observations or the same households have missing observations for multiple variables. All variables are standardized.

- Head and respondent characteristics (7 variables) - head male, head education level, respondent male, respondent education level, respondent married, respondent age, respondent is the head.

- Family characteristics (12 variables) - household size, number of households in compound, anyone outside of compound regularly uses latrine, number of children age 0-5, number of children age 0-14, number of adults, number of female adults, number of household members with jobs, number of household members earning a pension, number of people outside the household providing money, Muslim, primary language is Wolof.

- Residence characteristics (22 variables) - own their residence, years lived in residence, lived in residence more than ten years, expect to move out within five years, expect to stay at least ten more years, don't expect to move out in the future, residence has two stories, number of stories in the residence, number of rooms in the residence, has electricity, courtyard looks clean, road is wide enough for a truck, road is not sandy, number of functioning pits, pit in the compound (as opposed to in the street), animals were seen by the enumerator in the compound, animals were seen by the enumerator outside of the compound, household rents out rooms in the compound to others, courtyard has flooded in last year, floor is made of tile, roof is made of slab, household is only in the second baseline survey (implying they moved in recently).

- Assets (25 variables) - owns a cell phone, owns a radio, owns a television, owns a computer, owns a bicycle, owns a motorcycle, owns a car, owns a fan, owns an air conditioner, owns a refrigerator, owns a gas oven, owns a washing machine, owns a microwave, owns a generator, household asset index, number of animals owned, number of cows owned, number of sheep owned, number of goats owned, number of pigs owned, number of chickens owned, number of other productive animals owned, owns land other than where household lives, has a water meter, wealth index.

- Finance (9 variables) - owns jewelry, value of jewelry owned, household is wealthy, respondent has any account, respondent has a savings account, household in a tontine, total monthly tontine contributions, household expects tontine payout within two months, respondent has heard of Wari mobile money.

- Desludging history (20 variables) - desludge at least once a year, desludging frequency in dry season, desludging frequency in rainy season, desludging frequency if less than once a year, current pit ever desludged, current pit desludged more than once, any pit ever desludged, ever used manual desludging, ever used mechanized desludging, ever used both types of desludging, never desludged, desludged in last year, mechanized desludging in last year, manual desludging in last year, last desludging due to rain, last desludging done within two days of when the need was noticed, months since last desludging, months since last manual desludging done by a family member, months since last manual desludging done by a baay pelle, months since last mechanized desludging.

- Social networks (5 variables) - number of households in the cluster they are aware of, number they drink tea with, number they would pick as a health leader, number they talked about sanitation with, number that are wealthy.

- Preferences (11 variables) - trust people in the neighborhood, people in the neighborhood would take advantage, time preferences today, time preferences in a month, consistent time preferences, hyperbolic time preferences, patient now and impatient later, use savings for big expenses like a desludging, prefer to pay at once with a discount rather than at a higher price in installments, positive reciprocity, negative reciprocity.

- Health (8 variables) - number of household members with diarrhea in the last week, share of household members with diarrhea, number of children 0-14 with diarrhea, number of children 0-5 with diarrhea, number of household members with cough in the last week, share of household members with cough, number of children 0-14 with cough, number of children 0-5 with cough.

- Survey characteristics (7 variables) - enumerator reports no problems with interview, enumerator reports responses seemed reliable, supervisor accompanied enumerator, survey conducted in Wolof language, date of survey, months between baseline and endline survey, household is in endline survey.

- Randomized treatments (6 variables) - high subsidy, deposit required, public-price cluster, second 5 hhd in public-how-many cluster, second 5 hhd in public-who cluster, spillover household.

Appendix Table G-2 shows the LASSO results mirroring the OLS results we show in Table 2, while Appendix Table G-3 shows the LASSO results mirroring the OLS results we show in Table 3. Results are qualitatively similar when using OLS and when using LASSO.

There are two LASSO regressions with the outcome measuring whether the respondent signed up for the subsidized mechanized desludging. The control variables which were selected when the outcome variable is ‘signed up’ are very similar across the two. The controls that were consistently chosen in column (1) of Table G-2 and in column (1) of Panel A of Table G-3 are: respondent education level, courtyard looks clean, road is wide enough for a truck, roof is made of slab, wealth index, respondent has any account, deposit required, and whether the value for months since the most recent mechanized desludging is missing.[20] High subsidy was selected in the regression in which it was included but was not the variable of interest.

There are also two LASSO regressions with the outcome measuring whether the respondent used the subsidized mechanized desludging. The control variables which were selected in both regressions when the outcome variable is ‘subsidized desludging’ (column (2) of the tables) are respondent education level, residence has two stories, number of rooms in the residence, road is wide enough for a truck, roof is made of slab, household asset index, respondent has any account, mechanized desludging in the last year, household is in endline survey, and indicators for whether some variables were missing (missing has a water meter, missing desludge at least once a year, missing months since most recent mechanized desludging, and missing months between baseline and endline surveys). In Table G-3, high subsidy and missing survey conducted in Wolof were also selected as covariates.

There are four LASSO regressions with the outcome measuring whether the respondent used a mechanized desludging between the baseline and the endline. This outcome additionally has Panels B and C of Table G-3 which are

[20]It may seem strange that the indicator for whether the months since the most recent mechanized desludging is missing was selected. This variable only differs from the variable measuring whether the household has ever used a mechanized desludging in 34 cases. Both variables were supplied as candidates for the LASSO estimation. In those 34 cases, the respondent answered that they had used a mechanized desludging at some point in the past but then answered ‘don’t know’ for the months since the last mechanized desludging.

conducted on the sub-sample of spillover households only and the full sample. The control variables which were selected in all regressions when the outcome variable is ‘any mechanized desludging’ (column (3) of the tables) are the household asset index, ever used a mechanized desludging, and mechanized desludging in the last year. In addition, the control variables which were selected in all regressions except panel B of Table G-3 (the spillover subsample panel) are residence has two stories, number of floors in the residence, number of rooms in the residence, floor is made of tile, roof is made of slab, wealth index, months since last mechanized desludging, and indicators for whether some variables were missing (missing desludging frequency in rainy season and missing months since most recent mechanized desludging). High subsidy was selected in the two regressions in which it was included but wasn’t the variable of interest. Spillover household was selected in the one regression for which it was an option because it included both treated and spillover households, in column (3) of panel C of Table G-3.

Finally, there are four LASSO regressions with the outcome measuring whether the respondent used a manual desludging between the baseline and the endline. The control variables which were selected in all regressions when the outcome variable is ‘manual desludging’ (column (4) of the tables) are roof is made of slab, ever used a manual desludging, manual desludging in last year, and a missing indicator for desludge at least once a year. In addition, the control variables which were selected in all regressions except panel B of Table G-3 (the spillover subsample panel) are household size, road is wide enough for a truck, owns a refrigerator, household asset index, any pit ever desludged, mechanized desludging in last year, and indicators for whether some variables were missing (missing desludging frequency in dry season, missing months since most recent manual desludging done by a family member, and missing months since most recent mechanized desludging). High subsidy was selected in the two regressions in which it was included but wasn’t the variable of interest. Spillover household was selected in the one regression for which it was an option because it included both treated and spillover households, in column (4) of panel C of Table G-3.

E Sheets Given to Treatment Households

E.1 Private-Price Clusters

[cluster]

Subsidies for Desludging Subscriptions

Recipients of subsidy:
• [participant 1] • [participant 2] • [participant 3] • [participant 4] • [participant 5] • [participant 6] • [participant 7] • [participant 8] • [participant 9] • [participant 10]

The subsidies for the desludging service were assigned to each household listed above by a random draw. Each household had one chance out of two to be offered a large subsidy, and one chance out of two to be offered a small subsidy.

A household can only access the subsidy if the household signs up for our mechanized desludging service.

Community Sanitation 2014

E.2 Public-Price Clusters

[cluster]

Subsidies for Desludging Subscriptions

Recipients of a large subsidy
Must pay 17.000 for a desludging:

- [high subsidy participant 1]
- [high subsidy participant 2]
- [high subsidy participant 3]
- [high subsidy participant 4]
- [high subsidy participant 5]
- ...

Recipients of a small subsidy
Must pay 24.000 for a desludging:

- [low subsidy participant 1]
- [low subsidy participant 2]
- [low subsidy participant 3]
- [low subsidy participant 4]
- [low subsidy participant 5]
- ...

A household can only access the subsidy if the household signs up for our mechanized desludging service.

Community Sanitation
2014

F Script Explaining Experiment

The below script is translated from Wolof to English.

Introduction to the game

- Now I am going to explain to you the rules of the game which is part of the study.
- For this game, real money will be used.
- You will make all decisions yourself autonomously during the game.
- We will first review several examples before starting the real game.
- Understand that the money that you take from this game will be entirely given to you at the end.
- In this way, when we have finished the game with your neighbors participating in the study, your winnings will be sent to you by Wari [mobile money].
- Your participation in the game does not require any bets or fees on your part.
- The money used in this game is part of the budget of the study. If you would like to withdraw from the game before the end, any money you have already won will be given to you.

Instructions

- This same game will be reproduced in the other 11 households in your neighborhood participating in the study.
- At the beginning of the game, you will have 1200 CFA.
- Of this amount, you will decide how much you want to distribute to each of your 11 neighbors listed and how much you want to keep for yourself.

- What you distribute to your neighbors can range from 0 to 1200 CFA in steps of 100.
- All that you give to your neighbors will be multiplied by two.
- The amount that you decide to keep for yourself will be given to you, but will not be multiplied by two.

Terms of the game

- Your 11 neighbors participating in the study will play one by one this game and will distribute their 1200 CFA between their 11 neighbors and themselves.
- Nobody will know how you have distributed your 1200 CFA and we will not tell you how your neighbors have used their 1200 CFA.
- You alone will know how much you have given to your neighbors and how much you kept for yourself.
- When all your neighbors have finished distributing their 1200 CFA, the game will end, and we will send you your winnings by Wari. I remind you that what you keep for yourself will not be multiplied by two, but what you give to your neighbors and what they give to you will be multiplied by two.
- We will combine all your winnings and give them to you as one lump sum.

Examples

Before we begin, let us practice with two examples. After these examples, we will ask you to tell us how you want to distribute the 1200 CFA allocated to you between your 11 neighbors and yourself.

Let's talk through this example. You start with 1200 CFA. Let's say you decide to give 100 to each of the other households and keep 100 for yourself. How much will you earn?

[*If the person gets the answer wrong the device shows: Enumerator, please work through this example with the respondent.*]

Remember, in this example you start with 1200 CFA. You decide to give 100 to each of the other households and keep 100 for yourself. How much will each of the other households earn?

[*If the person gets the answer wrong the device shows: Enumerator, please work through this example with the respondent.*]

Let's talk through another example. You start with 1200 CFA. Let's say you decide to give 500 to Ahmadou, 500 to Cheikh, keep 200 for yourself, and give nothing to the other nine households. How much will you earn?

[*If the person gets the answer wrong the device shows: Enumerator, please work through this example with the respondent.*]

Remember, in this example you start with 1200 CFA. You decide to give 500 to Ahmadou, 500 to Cheikh, keep 200 for yourself, and give nothing to the other nine households. How much will Ahmadou earn?

[*If the person gets the answer wrong the device shows: Enumerator, please work through this example with the respondent.*]

Remember, in this example you start with 1200 CFA. You decide to give 500 to Ahmadou, 500 to Cheikh, keep 200 for yourself, and give nothing to the other nine households. How much will the other households earn?

[*If the person gets the answer wrong the device shows: Enumerator, please work through this example with the respondent.*]

Now, I am going to show you the list of your neighbors that we selected. I will take note of how much you want to give to each of your neighbors and how much you want to keep for yourself, so that the total of what you give and what you keep comes to 1200.

[*The enumerator then listed aloud the name of each eligible household head.*]

How much would you like to keep for yourself?

How much would you like to give to [Neighbor X's] household? [*This was asked eleven times, once for each neighboring household.*]

[*If the amounts did not sum to 1200.*] The respondent must use exactly 1200 CFA, no more and no less. Ask him to readjust his answers so the total comes to 1200.

The game is over, thank you. We will return within the next two weeks to give you your winnings.

G Appendix Tables

Table G-1: Survey Attrition

	Treatments				# of high subsidy hhds in cluster				Obs.
	Mean (SD)	Coefficient (SE)		p-value	Mean (SD)	Coefficient (SE)		p-value	
	(1) Low Subsidy (LS)	(2) High Subsidy (HS)	(3) Spillover (SO)	(4) HS= SO= 0	(5) 4-6 High Subsidies (HS46)	(6) 1-3 High Subsidies (HS13)	(7) 7-9 High Subsidies (HS79)	(8) HS13= HS79= 0	(9) Total Obs.
Panel A: Hhds that responded to both baseline surveys									
Responded to endline survey	0.895	0.018	0.019	0.175	0.903	0.004	0.008	0.813	4,521
	(0.306)	(0.011)	(0.012)		(0.296)	(0.013)	(0.013)		
Panel B: Hhds that responded to first baseline survey									
Responded to 2nd baseline survey	0.910	0.010	0.024**	0.096	0.917	0.008	0.008	0.697	4,916
	(0.286)	(0.009)	(0.011)		(0.276)	(0.015)	(0.011)		
Responded to endline survey	0.815	0.024	0.035**	0.041	0.828	0.011	0.015	0.625	4,916
	(0.388)	(0.013)	(0.015)		(0.377)	(0.019)	(0.017)		

Note: Panel A uses the sample of households that responded to both baseline surveys. Panel B uses the sample of households that responded to the first baseline survey. Columns (1) and (5) show the mean and standard deviation of observations with a low subsidy and observations in a cluster with 4-6 high subsidy hhds, respectively. Columns (2) and (3) show the coefficients on high subsidy and spillover in a regression including grid-point level fixed effects. Columns (6) and (7) show the coefficients on clusters with 1-3 and 7-9 high subsidy hhds in a regression with no fixed effects. Standard errors clustered at the grid-point level in parentheses in columns (2), (3), (6), and (7): * p<0.10, ** p<0.05, *** p<0.01. Columns (4) and (8) show the p-values for tests of whether the coefficients in columns (2)-(3) or columns (6)-(7) equal one another and equal 0.

Table G-2: Impact of Subsidy on Desludging Decisions - LASSO

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
High subsidy	0.191***	0.080***	0.028**	-0.028**
	(0.015)	(0.009)	(0.013)	(0.013)
N	3757	3757	3395	3395
Mean of outcome variable	0.398	0.074	0.315	0.317

Note: The sample includes all treatment households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) signed up for the subsidized mechanized desludging, (2) purchased the subsidized mechanized desludging, (3) purchased any mechanized desludging between the baseline and endline, and (4) had any manual desludging between the baseline and endline. The outcome in column (1) comes from the baseline survey, in (2) from the administrative data, and in (3)-(4) from the endline survey. Controls (measured in baseline) are chosen using post-double-selection lasso. Fixed effects at the arrondissement level. This is the same regression as in Table 2 but using post-double-selection LASSO to choose the control variables.

Table G-3: Decision Spillovers in Desludging Decisions - LASSO

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
Panel A: Treated Households				
# of high subsidy hhds in cluster	-0.010	-0.002	0.009*	-0.011**
	(0.007)	(0.003)	(0.005)	(0.005)
N	3757	3757	3395	3395
Mean of outcome variable	0.398	0.074	0.315	0.317
Panel B: Spillover Households				
# of high subsidy hhds in cluster			0.021**	-0.001
			(0.008)	(0.011)
N			696	696
Mean of outcome variable			0.325	0.378
Panel C: All Households				
# of high subsidy hhds in cluster			0.011**	-0.010**
			(0.004)	(0.005)
N			4091	4091
Mean of outcome variable			0.317	0.328

Note: Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are (1) signed up for the subsidized mechanized desludging, (2) purchased the subsidized mechanized desludging, (3) purchased any mechanized desludging between the baseline and endline, and (4) had any manual desludging between the baseline and endline. The outcome in column (1) comes from the baseline survey, in (2) from the administrative data, and in (3)-(4) from the endline survey. Controls (measured in baseline) are chosen using post-double-selection lasso. Fixed effects at the arrondissement level. This is the same regression as in Table 3 but using post-double-selection LASSO to choose the control variables.

Table G-4: Method of Finding Desludging Provider

	(1) How Desludger Found
Alternative-Specific Variable	
Alternative chosen in bl	0.731***
	(0.056)
Manual (26%)	
High subsidy	-0.139
	(0.109)
Spillover household	0.351*
	(0.205)
# of high subsidy hhds in cluster	-0.092**
	(0.038)
Mechanized - garage (5%)	
High subsidy	-0.221
	(0.194)
Spillover household	0.573*
	(0.334)
# of high subsidy hhds in cluster	-0.014
	(0.052)
Mechanized - call, flag, or referral (20%)	
High subsidy	-0.168
	(0.123)
Spillover household	0.447**
	(0.214)
# of high subsidy hhds in cluster	0.081**
	(0.038)
Mechanized - other (6%)	
High subsidy	0.902***
	(0.167)
Spillover household	-0.937**
	(0.403)
# of high subsidy hhds in cluster	0.015
	(0.053)
N of Obs.	19920
N of Cases	3984

Note: The sample includes all hhds. Std errors clustered at grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Alternative-specific conditional logit. Outcome is desludging choice between bl and el from el survey. Base alternative is no desludging btwn bl and el. Other alternatives are (1) manual desludging btwn bl and el, or mechanized desludging btwn bl and el and finding desludger (2) at garage or parking site, (3) calling trucker (5%), flagging down truck (2%), or referral (13%), and (4) calling the call-in center with or without a subsidy (5%), calling the ministry (0.2%), and don't know (1%). Controls (measured in bl), interacted with alternatives, include: respondent sex, age, and educ, hhd size, wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before bl and manual desludging in year before bl), other randomized intervention indicators (deposit required, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and arrondissement fixed effects.

Table G-5: Impact of Intervention on Price of Mechanized Desludging

	Mechanized price between bl and el			
	(1)	(2)	(3)	(4)
	LS + SO	SO	LS + SO	SO
# of high subsidy hhds in cluster	0.473	1.500**		
	(0.354)	(0.690)		
# of high subsidy hhds in nearest 5			0.429	1.722*
			(0.518)	(0.984)
Spillover household	-3.186		-3.011	
	(2.011)		(2.013)	
Mechanized price in bl	0.474***	0.437***	0.476***	0.445***
	(0.064)	(0.127)	(0.064)	(0.126)
N	664	206	664	206
Mean of outcome variable	47.13	47.08	47.13	47.08

Note: The sample in columns (1) and (3) includes all low subsidy and spillover households who purchased a mechanized desludging between the baseline and endline. The sample in columns (2) and (4) includes only spillover households who purchased a mechanized desludging between the baseline and endline. Standard errors clustered at the grid-point level in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The outcome is the price paid for a mechanized desludging (in dollars) between the baseline and endline. All regressions control for the price paid for a mechanical desludging in the baseline, and an indicator for if that variable is missing. Other controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, and pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline) and fixed effects at the arrondissement level. Additional controls in odd columns include other randomized intervention indicators (deposit required, spillover household, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster).

Table G-6: Health Externalities - IV

	Had Diarrhea				Had Cough			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number	Share	Number	Share	Number	Share	Number	Share
High subsidy	-0.005	0.002	0.009	0.002	-0.005	0.001	0.009	0.001
	(0.038)	(0.004)	(0.041)	(0.004)	(0.051)	(0.005)	(0.051)	(0.005)
# of hhds in cluster that had any desl. since bl	0.078*	0.009*			0.009	-0.002		
	(0.047)	(0.005)			(0.065)	(0.007)		
# of hhds in cluster that had mech. desl. since bl	-0.116	-0.014			0.012	0.006		
	(0.090)	(0.009)			(0.124)	(0.013)		
# of nearest 5 hhds that had any desl. since bl			0.330**	0.025*			0.136	-0.001
			(0.166)	(0.014)			(0.162)	(0.017)
# of nearest 5 hhds that had mech. desl. since bl			-0.549*	-0.041			-0.218	0.007
			(0.314)	(0.027)			(0.312)	(0.032)
# of hhd members with diarrhea in last week in bl	0.138***		0.126***		0.069**		0.063*	
	(0.033)		(0.033)		(0.034)		(0.034)	
# of hhd members with cough in last week in bl	0.016		0.020		0.074***		0.075***	
	(0.016)		(0.016)		(0.017)		(0.017)	
Share of hhd members with diarrhea in last week in bl		0.076***		0.071***		0.057**		0.057**
		(0.019)		(0.020)		(0.025)		(0.025)
Share of hhd members with cough in last week in bl		0.026**		0.028**		0.089***		0.088***
		(0.012)		(0.013)		(0.017)		(0.017)
N	3986	3986	3986	3986	3986	3986	3986	3986
Mean of outcome variable	0.502	0.048	0.502	0.048	0.694	0.070	0.694	0.070
First stage F-statistic	7.563	7.603	8.163	8.217	7.563	7.603	8.163	8.217

Note: The sample includes all households. Standard errors clustered at the grid-point level in parentheses: * p<0.10, ** p<0.05, *** p<0.01. Outcome variables are # or share of hhd members who had diarrhea or cough in the past week in endline. The outcome in all columns comes from the endline survey. In columns (1), (2), (5), and (6), endogenous # of hhds in cluster purchasing mechanized desludging between baseline and endline is instrumented by the # of high subsidy hhds in cluster. In columns (3), (4), (7), and (8), endogenous # of the nearest 5 hhds purchasing mechanized desludging between baseline and endline is instrumented by # of high subsidy hhds in nearest 5 (where nearest 5 includes the household itself). The table shows controls for either number (odd columns) or share (even columns) of hhd members sick with diarrhea and cough in the past week in the baseline. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size at baseline, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline, manual desludging in year before baseline), other randomized intervention indicators (deposit required, spillover household, public-price cluster, second 5 hhd in public-how-many cluster, and second 5 hhd in public-who cluster), and fixed effects at the arrondissement level. Odd columns additionally control for household size in the endline. The bottom row shows the F-statistic of the instrument in the first stage regression.

Table G-7: Ability to Remember Neighbors' Subsidy Levels and Decisions

	Baseline		Endline		
	(1) Subsidy	(2) Signed Up	(3) Subsidy	(4) Signed Up	(5) Used Subs Desl
Public-price cluster	0.073***	0.009*	0.007***	-0.001	0.001
	(0.017)	(0.005)	(0.002)	(0.002)	(0.001)
Public-who cluster × 2nd 5 hh	0.041	0.011	-0.004**	0.005	0.001
	(0.027)	(0.009)	(0.002)	(0.005)	(0.003)
Public-who cluster × 1st 5 nghbr	0.020	0.006	0.002	-0.004	-0.001
	(0.017)	(0.007)	(0.003)	(0.003)	(0.002)
Public-who cluster × 2nd 5 hh × 1st 5 nghbr	-0.019	0.002	-0.001	0.006	0.000
	(0.018)	(0.009)	(0.003)	(0.005)	(0.004)
Same respondent in bl intervention and bl payment survey	0.067***	0.019***			
	(0.018)	(0.005)			
Weeks between bl intervention and bl payment survey	-0.007**	-0.003***			
	(0.003)	(0.001)			
Same respondent in bl intervention and el survey			0.003	0.009***	0.003***
			(0.002)	(0.002)	(0.001)
Weeks between bl intervention and el survey			-0.001***	0.000	-0.001**
			(0.000)	(0.001)	(0.000)
N	7981	7978	30519	30507	30251
R^2	0.079	0.017	0.006	0.003	0.001
Mean of outcome variable	0.038	0.015	0.005	0.016	0.005

Note: The sample in columns (1)-(2) includes dyads for all pairs of treatment households in all clusters participating in the incentivized experiment. The sample in columns (3)-(5) includes dyads for all pairs of treated households in all clusters. Standard errors clustered at the grid-point level in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Outcome variables in columns (1)-(2) are whether, in the final baseline survey to give out experimental winnings, the respondent correctly knew (1) his neighbor's subsidy level, and (2) whether his neighbor signed up for the subsidized desludging. Outcome variables in columns (3)-(5) are whether, in the endline survey, the respondent correctly knew (3) his neighbor's subsidy level, (4) whether his neighbor signed up for the subsidized desludging, and (5) whether his neighbor purchased a subsidized mechanized desludging. Controls (measured in baseline) in all regressions include: respondent sex, age, and education, hhd size, a wealth index, own house, two-story house, rooms in house, courtyard looks clean, pre-intervention outcomes (mechanized desludging in year before baseline and manual desludging in year before baseline), other randomized intervention indicators (high subsidy, deposit required, and second 5 hhd in public-how-many cluster), and fixed effects at the arrondissement level.