Spillovers without Social Interactions in Urban Sanitation*

Joshua W. Deutschmann
University of Chicago

Molly Lipscomb
University of Virginia

Laura Schechter
UW-Madison

Jessica Zhu
Precision Development

October 12, 2022

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 the sanitation technology, with decision spillovers being largest among households that don't receive significant subsidies. There is no evidence that the spillovers are explained by social forces in general, nor that they are explained by specific social mechanisms such as learning from others, social pressure, or reciprocity. We do find evidence of a fourth, non-social, mechanism impacting decisions: increasing health benefits. 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.

*Deutschmann: Development Innovation Lab, University of Chicago (email: `jdeutschmann@uchicago.edu`); Lipscomb: Departments of Economics and Public Policy, University of Virginia (email: `ml4db@virginia.edu`); Schechter: Departments of Agricultural and Applied Economics and Economics, UW-Madison (email: `lschechter@wisc.edu`); Zhu: Precision Development (email: `s.jessica.zhu@gmail.com`). We thank Madelyn Bagby, Shoshana Griffith, Ahmadou Kandji, Sarah Nehrling, Cheikh Samb, and Heidi Schramm for excellent research assistance. We are grateful to the Bill and Melinda Gates Foundation for funding this project and research. Thanks to Mbaye Mbeguere and the members of the Senegal Office of Sanitation as well as Water and Sanitation for Africa for their collaboration. Data collection was approved by Innovation for Poverty Action's IRB with protocol 574, Notre Dame University's IRB with protocol 12-038, Senegal's IRB with protocol 227, and University of Virginia's IRB with protocol 2012-0272. This RCT was pre-registered in the AEA RCT Registry: AEARCTR-0000344 (https://doi.org/10.1257/rct.344-3.0).

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. 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, Lipscomb & Schechter 2018), households that are offered subsidies are 2.9 percentage points more likely to adopt. Lipscomb & Schechter (2018), discussed in more detail in Section 3, use data from the same experiment to show that households that receive higher subsidies are more likely to purchase a mechanized desludging. They find the impact of receiving a subsidy to be significantly larger than the impact of paying a deposit or receiving a mobile money account and receiving regular text message nudges. Lipscomb & Schechter (2018) focus only on the direct impact of receiving the high subsidy, and look at neither decision spillovers nor health externalities.

This paper shows that 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. 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, 2019). These studies leave open the questions both of whether such spillovers exist in an urban setting and, if so, the mechanism causing these spillovers. Households learn about new technologies from each other, leading to increased adoption (Beaman, Ben Yishay, Magruder & Mobarak 2021, Conley & Udry 2010, Dupas 2014). Social pressure and shame can be used to effectively increase adoption of sanitary behavior, for example in CLTS programs commonly practiced in rural villages (Cameron et al. 2019, Guiteras et al. 2019, 2016, Pattanayak et al. 2009, Pickering et al. 2015). Reciprocity plays an important role in rural areas in maintaining property rights, avoiding theft, and improving sanitation (Schechter 2007, Stopnitzky 2017).

Cai et al. (2015) use the most similar experimental design to ours to measure social mechanisms behind network effects in the adoption of crop insurance in rural China. Similar to them, we use social network data combined with randomized subsidies to test for social mechanisms more generally. In addition, both papers use mini-experiments to explore specific social or inter-personal mechanisms underlying decision spillovers including learning from others, social pressure, and reciprocity. Cai et al. (2015) find that naturally occurring network effects are driven by sharing of information about the insurance product, but individuals also respond to information about others' adoption if the experimenters share that information.

Even with detailed social network data collected to test for general social spillovers, and interventions designed to measure each social mechanism more directly, we do not find evi-

[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 non-linearities and thresholds in health externalities.

dence 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. Beaman, Keleher, Magruder & Trachtman (2021) and Dupas et al. (2022) show that urban households are much less aware of their neighbors and so measuring relative poverty using peer rankings, which has worked in rural settings, doesn't work as well in urban settings. Urban households may be more susceptible to social effects from work peers or family members scattered across the city. Evidence from Herrera-Yagüe et al. (2015) suggests that urban social networks do not tend to be based on geography, but instead based on social determinants such as hobbies and careers. In addition, neighborhood norms may change through a non-social process.

Decision spillovers may be driven by non-social mechanisms such as increasing health benefits or decreasing search costs.[2] Sanitation investments may exhibit increasing health benefits, 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).[3] Andrés et al. (2017) and Fuller et al. (2016) posit that there exists a lower threshold critical mass below which a few households with improved sanitation has no effect. There may also be an upper threshold above which transmission is interrupted.[4] Investment could also lead to decreasing search costs, if one household's adoption decreases search 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

[2] What we are calling 'increasing health benefits' have also been called technical complementarities (Guiteras et al. 2015) and epidemiological complementarities (Guiteras et al. 2019).

[3] The epidemiology literature suggests that community-level sanitation choices have as large or even larger effects on health outcomes as the individual household's choice (Andrés et al. 2017, Fuller et al. 2016, Jung, Hum, Lou & Cheng 2017, Oswald et al. 2017). The literature also presents significant evidence of increasing returns in the public benefit to improved community sanitation (Andrés et al. 2017, Cameron et al. 2021, Jung, Lou & Cheng 2017, Wolf et al. 2019). There is some, albeit less, evidence regarding the relationship between the private benefits to adopting and community sanitation coverage. Andrés et al. (2017) shows evidence that the private returns to own improved sanitation increase with the community's adoption rate. Based on the evidence in that paper, Radin, Wong, McManus, Sinha, Jeuland, Larbi, Tuffuor, Biscoff & Whittington (2020) and Radin, Jeuland, Wang & Whittington (2020) assume this relationship for the purposes of their cost-benefit analysis. The only evidence we know of the opposite, decreasing health benefits, comes from Ben Yishay et al. (2017) who find a negative multiplier effect from latrine subsidies. The authors posit that the health impacts of owning a latrine decrease in neighbor's latrine ownership due to non-linearities in the health production function or due to the fact that neighbors commonly share latrines with one another.

[4] 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.

place between the households. 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 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 effects combined with the importance of decision spillovers suggests that the movement toward community-based sanitation initiatives may be less successful in urban areas than 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 dug in the 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 typically doesn't 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 serve 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, over-samples 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 only 'aware' of 55% of their neighbors' identities. They claim to talk about sanitation with 12% of their neighbors, over one-fifth 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 4,920 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.[5] 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 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. The subsidized mechanized desludging service was offered on the second visit to the household and was described as being offered by Innovations for Poverty Action (IPA). IPA would likely not have been known in these communities. We organized a call-in center to which customers could call and truckers were invited to bid on jobs via text

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

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.[6] Thus, while the subsidies are expected to affect demand, they are not necessarily expected to affect supply.[7]

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 interviewed 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.[8]

The experiment was also designed to measure the effects of nudges such as deposits and earmarked savings accounts, and compare their effect sizes with the effect of subsidies, 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.

[6]Other households throughout Dakar calling the call center paid the winning bid. Deutschmann et al. (2020) and Houde et al. (2022) analyze the call-in center and the auctions.

[7]Our other work focused on the supply side of the mechanized desludging industry gives evidence that mechanized desludging providers are colluding and operating below full capacity (Deutschmann et al. 2023, Houde et al. 2022). Thus we expect there to be room for the truckers to accommodate this increase in demand.

[8]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.

Lipscomb & Schechter (2018) show that behavioral nudges such as deposit requirements and earmarked mobile money accounts with different features and regular text message reminders neither increase nor decrease adoption. They show that the subsidy does have a large impact on the subsidized household itself. The current paper looks at the same subsidy program and explores whether subsidies received by one household cause decision spillovers and health externalities for other households living in the same neighborhood. The current paper also explores the social network data and additional interventions described below which attempt to tease out the mechanisms behind these spillovers.

Households that were offered a subsidized desludging were told that if they signed up for the desludging we would come back to put a sticker up on their house indicating their status as a household that signed up. We did not explain the stickers to the 'spillover' households. After all baseline surveys in a cluster were completed, we returned and gave stickers to the households that had signed up. Households could post these stickers outside their house as a signal for the desludger, and for their peers, that they had signed up for the sanitary mechanized desludging. In the endline survey, we asked respondents if they noticed the sticker on each of their 11 neighbors' houses. Over half (55%) of respondents who signed up for the service and over a quarter (30%) of those who did not sign up claim to have noticed a sticker on at least one of their neighbor's houses. Respondents are five times as likely to 'notice' a sticker on the house of a neighboring household that signed up (and so should actually have a sticker). 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 (Deutschmann et al. 2022). 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 4,920 households (4,100 treatment and 820 spillover) in the first survey, we found 4,521 households (3,757 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 4,101 of the original households (3,404 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. Households did not know their treatment status until the second baseline so attrition after that point is quite important. Attrition after the first baseline survey is also important because every cluster in the first baseline survey included two spillover and ten treated households. Of those ten treated households, it was pre-determined how many (and which) would receive the high subsidy. So, attrition between the first and second baseline surveys could lead to there being slightly fewer households with different subsidy levels (though when we measure impacts of the number of high subsidy households, we use the original random assignment ignoring attrition).

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 four 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 (excluding the household itself) is 55 meters (s.d. 32). For some analysis we focus on the four households nearest to a respondent's household. The average distance between two households in the nearest four from one another (excluding the household itself) is 28 meters (s.d. 17).[9] For comparison, the World Health Organization (2020) recommends a distance of 30 meters from the house to unimproved latrines.

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.

[9] 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 199 meters (with the 5th percentile being 117 meters).

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.

5.1 Decision Spillovers

We analyze the impact of different treatments 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 second baseline, purchasing a subsidized mechanized desludging in the year after the second baseline, purchasing any mechanized desludging in the year after the second baseline (either from us or on the open market), and purchasing or using any manual desludging in the year after the second baseline (an action we hope the interventions discourage). The first two outcomes come from the second baseline survey and administrative data. The last two outcomes were measured in the endline survey, with slightly smaller sample sizes. The first outcome, signing up, refers to the initial agreement in the second baseline survey (which may or may not have required the household to leave a deposit depending on their treatment group). Many households sign up for the subsidized desludging and then don't end up actually purchasing it. They may have intended to follow through but not ended up doing so due to liquidity constraints or time inconsistent preferences. Or they may never have intended to purchase the mechanized desludging and signing up may have been cheap talk.

We know from Lipscomb & Schechter (2018) that households which receive the high subsidy are more likely to adopt a mechanized desludging. We explore whether one household choosing a more sanitary technology (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 control for an indicator for whether the household was offered a high subsidy (H_{iga}) as in Lipscomb & Schechter (2018), and we control for the number of other households offered a high subsidy in the cluster ($\overline{H}_{iga}$, which excludes the household itself). We run the following linear probability model, though results are similar if we instead estimate a logit.

$$y_{iga} = \alpha + \beta_1 H_{iga} + \beta_2 \overline{H}_{iga} \ (+\beta_3 S_{iga}) \ + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \tag{1}$$

We run the above regression on the ten treated households in each cluster, on the two spillover households in each cluster, and on all twelve households. When we run the regression on all households, we add the part of equation (1) in parentheses, 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.

We cluster standard errors at the grid-point cluster level and include arrondissement fixed effects ψ_a (of which there are four). We include other household level controls measured in the baseline in X_{iga} as chosen by the post-double-selection LASSO procedure elucidated in Belloni et al. (2014). Appendix D lists the variables from which the controls are chosen.

For this and all other regressions, we compute the ex post minimum detectable effect (MDE) size for our key coefficient of interest (Bloom 1995, Haushofer & Shapiro 2016). This is the effect that would have been detectable with 80% power at the 5% significance level and is calculated as MDE $= 2.8 \times SE(\hat{\beta})$. These are shown in Appendix Table G-2 and help us distinguish between true null results and under-powered statistical tests. In discussing the results of each table, we compare them to the estimated minimum detectable effect size.

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 base alternative), the household used a manual desludging between baseline 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):

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

where y_{igak1} is the alternative they had most recently chosen at the time of the baseline. We assume that the elements of the error term are independent and distributed Type 1 extreme-value. This implies independence from irrelevant alternatives (IIA) - a household's relative probability of choosing between two alternatives is independent of other alternatives in the choice set. 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 (1) 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 also estimate a version in which we measure the effect of the number of households in the nearest four that were offered the high subsidy. The control variables include the pre-intervention baseline levels of both diarrhea and cough. Additional controls are chosen by post-double-selection LASSO (see Appendix D).

6 Basic Results

We first measure whether there are decision spillovers leading households' high subsidies to influence their neighbors' take-up. Then we measure whether there are health externalities, with households' high subsidies influencing their neighbors' health outcomes.

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 more households that purchase a mechanized desludging and fewer households that use manual 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.[10]

We explore these decision spillovers in regression form by estimating equation (1) in Table 2. Panel A of Table 2 shows a similar regression to that in Table 2 of Lipscomb & Schechter (2018). The differences are that i) in the current paper we additionally control for the number of high subsidy households in the cluster, ii) in the current paper we analyze the additional outcome of purchasing a manual desludging, and iii) that the other covariates in the current paper are chosen by LASSO. The results here are thus quite similar to those in Lipscomb & Schechter (2018). Here we see that households that 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.[11]

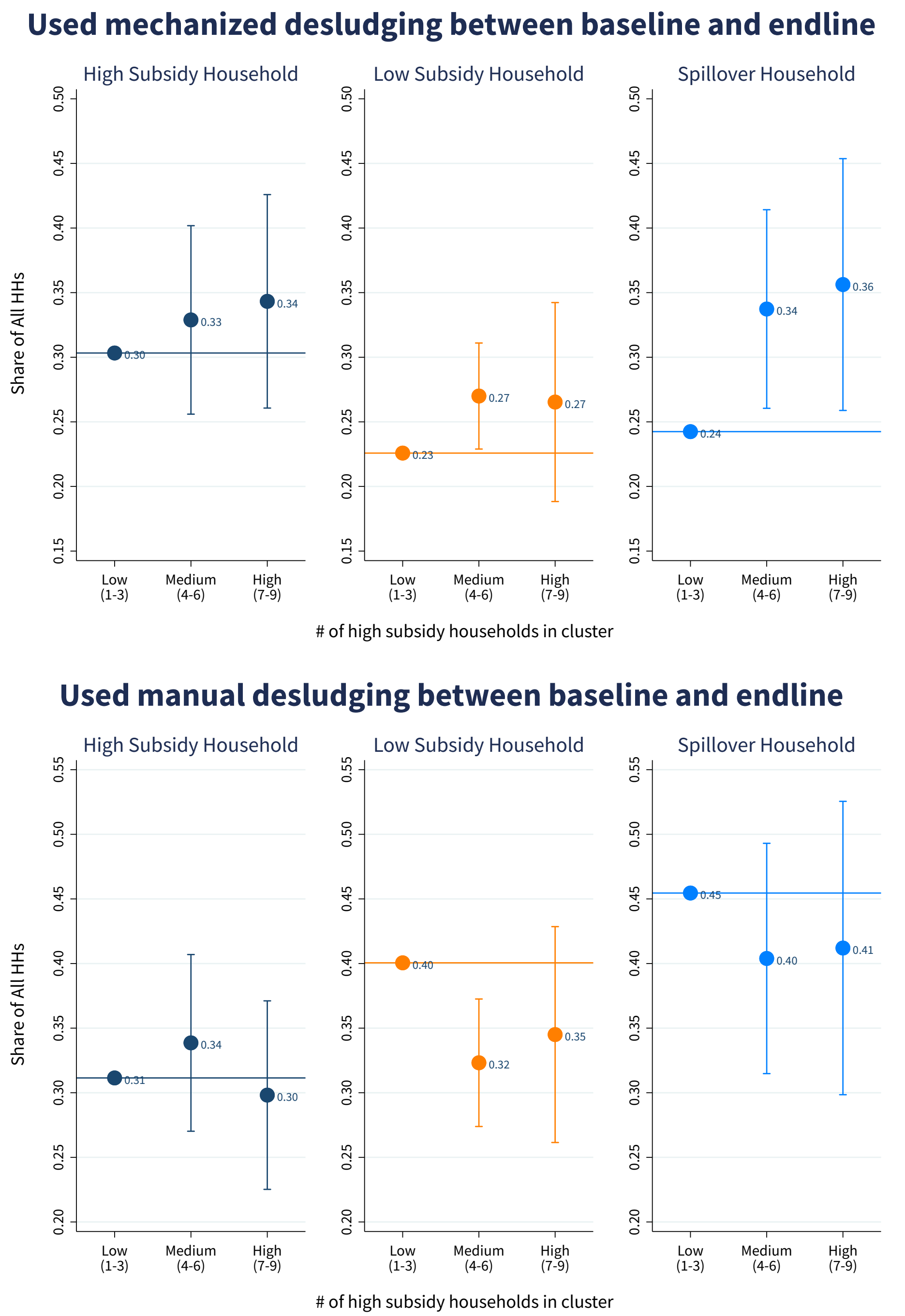
In addition to Panel A which contains just the treated households, we show panel B which includes just the spillover households, and panel C which pools both groups. The evidence is consistent with decision spillovers; when more households in a cluster are offered the high subsidy, households in that cluster become more likely to purchase a mechanized desludging and less likely to use a manual desludging. As the number of high subsidy households in the cluster increases, the household substitutes from manual toward mechanized desludging. The magnitude of the effect implies that every additional high subsidy allocated to a neighbor increases the household's probability of getting a mechanized desludging and decreases their probability of getting a manual desludging by 1 to 3 percentage points. Given that the effect of a household's own high subsidy is 3 percentage points, this implies that the effect sizes are the same for i) being offered a high subsidy and ii) having three neighbors be offered high subsidies. The ex-post MDEs shown in Appendix Table G-2 suggest that we are well-powered to detect decision spillovers of these magnitudes.

We next look at whether decision spillovers lead households to hire a desludger with whom they haven't worked previously. We show the odds ratios resulting from equation (2) in Table

[10]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.

[11]The effect sizes for those first three outcomes, with slightly different control variables, in Lipscomb & Schechter (2018) are 20 p.p., 8 p.p., and 3 p.p.

Figure 1: Decision Spillovers in Desludging Decisions



Note: Figure displays the estimated coefficient plus the mean for that type of household in clusters that had 1-3 high subsidy households. 95% confidence intervals are included. The regressions are post-double-selection lasso separately for each type of household (high subsidy/low subsidy/spillover). The control variables of interest are being in clusters with a medium or high number of high subsidies, with the excluded control variable being in a cluster with a low number of high subsidies. The top figure shows the share of households that purchased any mechanized desludging between the baseline and endline, while the bottom figure shows the share of households that had any manual desludging.

Table 2: Decision Spillovers in Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
Panel A: Treated Households				
Own high subsidy	0.191***	0.080***	0.029**	-0.028**
	(0.015)	(0.009)	(0.013)	(0.013)
# other high subsidies in cluster	-0.010	-0.002	0.009*	-0.012**
	(0.007)	(0.003)	(0.005)	(0.005)
N	3757	3757	3395	3395
Outcome mean, comparison group	0.299	0.033	0.295	0.336
Panel B: Spillover Households				
# other high subsidies in cluster			0.020**	0.000
			(0.008)	(0.011)
N			696	696
Outcome mean, spillover households			0.325	0.378
Panel C: All Households				
Own high subsidy			0.029**	-0.028**
			(0.013)	(0.013)
# other high subsidies in cluster			0.011**	-0.010**
			(0.004)	(0.005)
N			4091	4091
Outcome mean, comparison group			0.304	0.348

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 at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. Additional control in Panel C is a spillover household indicator. In panel A, the outcome mean is shown for low-subsidy households; in panel B, for spillover households; and in panel C, for low subsidy and spillover households.

3. 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 (as evidenced by the odds ratio being less than 1) 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 (2) focusing on how households find their desludging provider, with the resulting odds ratios presented in Appendix Table G-3. The first two

Table 3: First Encounter with Mechanized Desludger?

	(1) First Time Desludger Hired
Alternative-Specific Variable	
Alternative chosen in bl	1.513***
	(0.096)
Manual (26%)	
Own high subsidy	0.860
	(0.094)
# other high subsidies in cluster	0.920**
	(0.033)
Mechanized - first (18%)	
Own high subsidy	1.376***
	(0.152)
# other high subsidies in cluster	1.088**
	(0.041)
Mechanized - not first (14%)	
Own high subsidy	0.888
	(0.121)
# other high subsidies in cluster	1.028
	(0.049)
N of Obs.	16364
N of Cases	4091

Note: The sample includes all households. Standard errors clustered at the grid-point level in parentheses. Conditional logit estimation presenting odds ratios and testing significance with respect to 1: * p<0.10, ** p<0.05, *** p<0.01. Odds ratios greater than 1 imply a positive and odds ratios less than 1 a negative relationship. Outcome is the desludging choice between baseline and endline. The base alternative is having no desludging (42% of observations). The other alternatives are (1) having a manual desludging, (2) having a mechanized desludging with a new desludger, and (3) having a mechanized desludging with a desludger used in the past. The outcome comes from the endline survey. Controls (measured in baseline) selected by post-double-selection LASSO for inclusion in Table 2 (Panel C, columns 3 and 4) and arrondissement fixed effects, all interacted with the different alternatives.

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.[12] Appendix Table G-3 shows that high subsidy households are more likely to purchase a mechanized desludging by calling our service. In clusters with a higher number of high

[12]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 are quite small. And, the survey does not distinguish between referrals from neighbors and those from friends living elsewhere so referrals do not necessarily imply social neighborhood-level effects.

subsidy households, households are less likely to get a manual desludging (as evidenced by the odds ratio less than 1). Households in these clusters are more likely to get a mechanized desludging where they found the desludger by calling their phone number,[13] 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 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 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 we study was to increase the use of mechanized and decrease the use of manual desludgings, the indirect goal was 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.

[13]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: Impact on Health Outcomes

	Had Diarrhea				Had Cough			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number	Share	Number	Share	Number	Share	Number	Share
Own high subsidy	-0.005	0.000	-0.004	0.000	0.013	0.002	0.013	0.002
	(0.033)	(0.004)	(0.033)	(0.004)	(0.043)	(0.005)	(0.043)	(0.005)
# other high subsidies in cluster	-0.016	-0.002			0.005	0.001		
	(0.011)	(0.001)			(0.016)	(0.002)		
# of high subsidies in nearest 4			-0.028*	-0.003			-0.007	0.001
			(0.016)	(0.002)			(0.021)	(0.002)
N	4092	4092	4092	4092	4092	4092	4092	4092
Outcome mean, comparison group	0.491	0.049	0.491	0.049	0.685	0.070	0.685	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. Regressions include controls for either number (odd columns) or share (even columns) of hhd members sick with diarrhea and cough in the past week at baseline and a spillover control. Odd columns additionally control for household size in the endline. Other controls (measured at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for low-subsidy and spillover households.

Table 4 shows the effects of the subsidy saturation level on health outcomes. Each 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 four households (including the household itself), every additional high subsidy leads to a decrease of .028 in the number or .003 in the share of household members with diarrhea, with the first effect being significant at traditional levels. A household whose four nearest sampled neighbors all received a high subsidy, would see a 23% ((0.028*4)/0.491) decrease in the number of sick household members, and a 24% decrease in the share of sick household members ((0.003*4)/0.049). The magnitudes are in line with those of the previous literature cited above. Table G-2 shows that our ex-post MDEs for these outcomes are of comparable magnitudes to yield estimates that are similarly in line with that literature.

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. Mirroring the results in Barreto et al. (2007), it takes a neighborhood-level change to impact health outcomes.[14] The falsification tests using incidence of cough as an outcome show an effect size close to zero, as we would have expected.

7 Disentangling Mechanisms

In the previous section we showed both decision spillovers and health externalities in urban sanitation. Next we conduct an omnibus test to explore whether these effects are due to social mechanisms more generally. We use data on households' social network connections in combination with the subsidies randomized at the household level to test for social effects in the decision spillovers. The disadvantages of the omnibus test are two-fold. On the one hand, if it fails to reveal that the spillovers are social, we might be concerned that it does so because social network connections are mismeasured and the test is under-powered. On the other hand, if it does reveal that the spillovers are social, we would like to understand the underlying social mechanism. For these reasons, we also conducted mini-experiments that should have greater power to explore specific social mechanism. The specific social mechanisms we

[14] To test for increasing health benefits, we regress diarrhea incidence on own desludging choice, nearest four neighbors' desludging choices, and the interaction between the two. If there were increasing health benefits, we would expect the coefficient on the interaction to be negative – a household's own mechanized desludging leads to a larger decrease in diarrhea if more neighbors have also adopted a mechanized desludging. In results not shown here, the coefficients on the interaction term are not significant at traditional levels, but they are negative as predicted. Due to issues of power and the endogeneity of desludging choices, these results should be taken with a grain of salt.

consider include learning-from-others, coordination, social pressure, and reciprocity. We also explore non-social mechanisms of decreasing search costs and increasing health benefits from sanitation investments made by neighbors.

7.1 Experimental Design - Mechanisms

We conducted three randomized mini-experiments to disentangle the social mechanisms. First, 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. Second, for all households in some randomly chosen clusters, before the respondents made their own decision we informed them the subsidy level offered to each of their neighbors. Finally, we randomly chose some clusters to participate in an incentivized non-anonymous dictator game.[15] The breakdown of the randomization can be found in Appendix B.

Parts of our design are similar to Cai et al. (2015) who study crop insurance adaptation in rural China. They randomize features including the price charged to the farmer, the information given to the farmer about how insurance works, and the information given to the farmer about how many or which other farmers in the village adopted. They also complement the randomized interventions with social network data. Differences in our settings include the facts that i) we are looking at an urban setting, ii) the technology we study involves health externalities, iii) adoption of the technology we study is visible to neighbors, iv) experimental households can sign up for the subsidized mechanized desludging (and this sign up information is what we can share) but may or may not actually purchase it when their pit fills, v) we have data on pure spillover households which were not involved in the randomized experiment, and vi) we have data on whether individuals buy unsubsidized mechanized desludgings on the open market outside of our experiment. Cai et al. (2015) find that information about how insurance functions flows naturally through the social network but information about who purchases it does not. Informing individuals how many or which of their peers adopted affects the farmer's own adoption. Differences between their results and ours may be due to one or many of the differences in setting listed above.

The *'public-how-many/who'* treatments vary learning-from-others and coordination by randomizing whether households are told how many of their neighbors signed up for the subsidized desludging, which of their neighbors signed up for the subsidized desludging, or nothing at all. In a quarter of the clusters (*'public-how-many'*), we randomly split the ten treatment households into a group of five which enumerators interviewed first (the first five).

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

After that, the second half (the second five) were interviewed and were informed about the number of households in the first five that signed up. In another quarter of the clusters (*'public-who'*), the second five respondents were told both how many and which specific households signed up from the first five. The final half of the clusters were considered control clusters and respondents were not told anything about their neighbors' sign-up decisions. For those clusters, we did still randomly split the households into two groups ex-ante, though this split was 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.'*[16][17]

The *'public-price'* treatment varies social pressure by randomizing whether households are told the subsidy level of their neighbors. 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 which were the control, 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. There is no group in which respondents are not told anything about actual or expected subsidies in the cluster, so we can not measure the effect of knowing that some of your neighbors were offered a subsidy. We measure the differential effect of whether or not the respondent is told specifically which neighbors were offered a subsidy.

Finally, in a quarter of the clusters we ran an incentivized economic experiment measuring reciprocity. Respondents chose 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 1,200 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 households that adopted mechanized desludging. In other settings, participants have been found to express reciprocity for actions taken outside of the experiment with their giving inside the 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,

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

[17] 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.

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.

7.2 Estimation Strategy and Results - Mechanisms

In this section we show the estimation strategy and results for social mechanisms in general and then for each of the different mechanisms separately. We begin in Section 7.2.1 with an omnibus test of social mechanisms using our social network data. The results of this omnibus test suggest that the decision spillovers we found above are not due to social mechanisms. Still, we test for evidence of specific social mechanisms. Sections 7.2.2, 7.2.3, and 7.2.4 explore and then reject the impacts of learning and coordination, social pressure, and reciprocity respectively. Finally, we explore and find suggestive evidence of non-social increasing health benefits and decreasing search costs from neighbors' adoption in Section 7.2.5.

7.2.1 Omnibus Test of Social Mechanism

We start by looking for evidence of social decision spillovers using the social network data. We saw in Table 2 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 strongly linked in the social network.

Social spillovers can arise from individuals communicating with their neighbors, asking about each other's experiences, pressuring each other to coordinate on new social norms, and emphasizing the health benefits. This would suggest that if a household that a respondent knows or with which they talk about sanitation adopts (perhaps encouraged by randomly receiving a high subsidy), the effect would be different than if a household with which the respondent is less strongly connected adopts (Conley & Udry 2010). Guiteras et al. (2019) find this to be the case in rural Bangladesh; subsidies given to individuals who people rely on for advice about new technologies lead to larger spillovers, while subsidies to leaders actually lead to smaller spillovers.

To study these effects, we add on to equation (1) 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 which 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. As noted above in Section 2, respondents are 'aware' of 55% of their neighbors. A small fraction of respondents report knowing either zero (3%) or all eleven (5%) neighbors. Tea drinking is a common social activity, typically involving a long time spent together talking, and could be a particularly useful indicator of a stronger social tie within a neighborhood. Respondents report drinking tea with 29% of their neighbors. Sanitation is a topic of conversation within the networks we measure, with respondents claiming to talk about sanitation with 12% of their neighbors.

We estimate

$$y_{iga} = \alpha + \beta_1 H_{iga} + \beta_2 \overline{H}_{iga} + \beta_3 S_{iga} + X'_{iga}\gamma + \beta_4 N^r_{iga} + \beta_5 NH^r_{iga} + \psi_a + \epsilon_{iga} \tag{3}$$

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

Table 5 shows the results. 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. The high subsidies do not have a greater impact if they were given to households that were known by the respondent, drank tea with the respondent, or were considered a source of health and sanitation-related information. It also doesn't matter if the high subsidies are concentrated among residents in the nearest four or not. As Table G-2 shows, our ex-post MDEs are of comparable magnitude to (though slightly larger than) the size of the general decision spillover, suggesting power to detect an effect size of 2-3 percentage points per additional high-subsidy household with a particular network tie. This suggests that if decision spillovers were indeed operating through the networks we measure, we would have power to detect a relatively small effect.

This might be perceived as evidence against any social channel behind the spillovers. But, this non-result could also be due to poorly measured network data. Individuals may not be very good at saying who their contacts are. Thus, we also conducted mini-experiments which do not rely on the social network data to look at specific social channels. We discuss this evidence in the following subsections.

7.2.2 Learning and Coordination

We also examine *learning from others and coordination*, measuring the effect of telling the second five households visited about the choices made by the first five. When a respondent is told that more households in his cluster signed up, this may make him more likely to

Table 5: Decision Spillovers and Social Networks in Desludging Decisions

(1) # of high subsidy hhds in cluster that ...	(1) Any Mechanized Desludging	(2) Manual Desludging
# other high subsidies in cluster	0.013**	-0.001
	(0.006)	(0.006)
(1) ... you are aware of (bl)	-0.003	-0.015**
	(0.007)	(0.008)
# other high subsidies in cluster	0.011**	-0.006
	(0.005)	(0.005)
(1) ... you drink tea with (bl)	-0.000	-0.013
	(0.009)	(0.009)
# other high subsidies in cluster	0.011**	-0.007
	(0.005)	(0.005)
(1) ... you would pick as health leader (bl)	0.005	-0.016
	(0.011)	(0.012)
# other high subsidies in cluster	0.011**	-0.010**
	(0.005)	(0.005)
(1) ... you talked about sanitation with (bl)	0.004	0.003
	(0.012)	(0.012)
# other high subsidies in cluster	0.013***	-0.013**
	(0.005)	(0.005)
(1) ... are wealthy (bl)	-0.008	0.012
	(0.009)	(0.009)
# other high subsidies in cluster	0.013**	-0.010*
	(0.005)	(0.005)
(1) ... are in nearest 4	-0.005	0.001
	(0.007)	(0.008)
N	4091	4091
Outcome mean, comparison group	0.304	0.348

Note: Each pair of rows shows estimates of β_2 and β_5 from a different variant of equation (3). 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. All regressions control for a household's own subsidy status and the # of hhds in the cluster with the relation in question (e.g., # of hhds in cluster with which they drink tea). Controls (measured at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for low-subsidy and spillover households.

sign up if he uses that to infer that mechanized desludging is of high value. Alternatively, a respondent may infer a change in the social norm involving a coordinated move to a new, more sanitary equilibrium, and this may make him more likely to sign up. The intention behind this mini-experiment is similar to the public commitment to adopt sanitary latrine maintenance in Bakhtiar et al. (2021), which does increase adoption in a rural setting. The households make non-binding commitments in front of one another and Bakhtiar et al. (2021)

call this a 'simple verbal coordination device.'

The mechanisms of learning and coordination discussed here differ from the non-social increasing health benefits we discuss in Section 7.2.5. In this mini-experiment, households learn either how many households or which households signed up for the mechanized desludging. Signing up during the survey is a noisy (cheap talk) signal of whether the household will actually adopt, and signing up involves no direct health benefits. In addition, social coordination can occur in the face of constant or even decreasing health benefits, while the non-social increasing health benefits effect depends on the benefits being increasing.

We alter our original regression specification in equation (1) to 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 a second-five household in a public-how-many (P^n_{iga}) or public-who (P^w_{iga}) cluster. We run the following regression on all second-five households:

$$y_{iga} = \alpha + \beta_1 H_{iga} + \beta_2 N^s_{ga} + \beta_3 P^n_{ga} + \beta_4 N^s_{ga} P^n_{ga} + \beta_5 P^w_{ga} + \beta_6 N^s_{ga} P^w_{ga} + X'_{iga}\gamma + \psi_a + \epsilon_{iga}. \quad (4)$$

The interactions between the number of households in the first five that signed up and the randomized public-who and public-how-many cluster indicators are conditionally random after controlling for the number of households in the first five that signed up. 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.

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 which 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:

$$\begin{aligned} y_{iga} = \alpha &+ \beta_1 H_{iga} + \beta_2 N^s_{iga} + \beta_3 N^t_{iga} + \beta_4 N^{st}_{iga} \\ &+ \beta_5 P^w_{ga} + \beta_6 N^t_{iga} P^w_{ga} + \beta_7 N^s_{iga} P^w_{ga} + \beta_8 N^{st}_{iga} P^w_{ga} + X_{iga}\gamma + \psi_a + \epsilon_{iga}. \quad (5) \end{aligned}$$

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.

Table 6: Impact of Learning and Coordination on Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
Own high subsidy	0.200***	0.082***	0.028	-0.037*
	(0.021)	(0.013)	(0.019)	(0.020)
Public-how-many cluster	-0.056	-0.042	-0.014	0.015
	(0.051)	(0.028)	(0.039)	(0.041)
Public-who cluster	-0.009	-0.002	-0.024	0.044
	(0.044)	(0.020)	(0.035)	(0.045)
# signed up in first 5	0.040***	0.008	0.022*	-0.022*
	(0.012)	(0.006)	(0.012)	(0.011)
# signed up in first 5 × Public-how-many cluster	0.051**	0.035**	0.004	0.011
	(0.025)	(0.016)	(0.019)	(0.020)
# signed up in first 5 × Public-who cluster	0.023	0.005	0.009	-0.020
	(0.021)	(0.012)	(0.018)	(0.020)
N	1762	1762	1585	1585
Outcome mean, comparison group	0.293	0.036	0.304	0.328

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) 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 at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for second-five low-subsidy households that are not in a public-how-many or public-who cluster.

There is limited evidence that informing households how many of their neighbors have signed up increases either signing up or purchasing. In Table 6 we estimate equation (4). We see that in public-how-many clusters where more households in the first five sign up, second-five households are also more likely to sign up for the subsidized mechanized desludging and more likely to use it. This is not true for being in a public-who cluster.[18] This positive effect does not carry over to actual adoption decisions. Households must sign up in order to use the subsidized desludging, but anyone can purchase an unsubsidized mechanized desludging on the open market. While we see a reasonably large effect of the information on signing up, this translates into a smaller effect on purchase of the subsidized desludging, and no significant effect on the purchase of any mechanized desludging. As Table G-2 shows, our ex-post MDEs for the effect of the interaction (between the number signing up and the

[18]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.

public-how-many cluster indicator) on purchases of any mechanized or manual desludging are relatively large, between 5 and 6 percentage points, suggesting we do not have a lot of power to estimate the effects in columns (3) and (4). Overall, this is relatively weak evidence against coordination or learning from others.

Table 7: Impact of Learning and Coordination Within Networks on Desludging Decisions

# signed up in 1st 5 that ...	Signed Up (1)	Subsidized Desludging (2)	Any Mechanized Desludging (3)	Manual Desludging (4)
you are aware of × Public-who cluster	0.040	0.030	0.023	-0.026
	(0.048)	(0.024)	(0.036)	(0.039)
you drink tea with × Public-who cluster	0.038	-0.049**	-0.007	0.032
	(0.052)	(0.024)	(0.040)	(0.044)
you would pick as health leader × Public-who cluster	0.069	-0.015	0.022	-0.112
	(0.080)	(0.039)	(0.065)	(0.070)
you talked about sanitation with × Public-who cluster	-0.013	0.028	-0.036	0.040
	(0.073)	(0.032)	(0.056)	(0.057)
are wealthy × Public-who cluster	0.033	-0.040	-0.019	-0.028
	(0.057)	(0.030)	(0.055)	(0.052)
are in nearest 4 × Public-who cluster	-0.084	-0.048*	0.008	-0.060
	(0.052)	(0.028)	(0.043)	(0.045)
N	1397	1397	1258	1258
Outcome mean, comparison group	0.293	0.036	0.304	0.328

Note: Each row shows estimates of β_8 from a different variant of equation (5). 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) 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 at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. Additional controls include: own high subsidy, public-who cluster, # in 1st 5 that signed up, # in 1st 5 with certain trait, # in 1st 5 that signed up × public-who cluster, # in 1st 5 with certain trait × public-who cluster, # in 1st 5 that signed up with certain trait. The outcome mean is shown for second-five low-subsidy households that are not in a public-how-many or public-who cluster.

In Table 7, we look at estimation of equation (5) exploring the importance of a household's relationship with the other households that chose to sign up in pubic-who clusters. In the table, we show heterogeneity with respect to six network traits. 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 urban Dakar, while coordination has very small or no impact. However, we caution the reader that these results are where we suffer most from a lack of statistical power. Table G-2 shows that our ex-post MDEs exceed 10 percentage points, which would imply we could only detect a rather large learning effect within these networks.

7.2.3 Social Pressure

Social pressure and shaming are key elements of many commonly used policies aimed at increasing the take-up of improved sanitation. Community Led Total Sanitation (CLTS) explicitly relies on shaming, in some cases even charging children with the responsibility of blowing whistles at and yelling at older people when they see them participating in open defecation (Kar & Chambers 2008). The CLTS methods have been tried in urban environments to varying success (for example Swachh Bharat Mission in India (Sharma et al. 2022)). We test the impact of social pressure in an urban environment as an important component of peer effects which could be used to increase technology adoption. Since households can view each other's desludging choice and would prefer their neighbors use mechanized rather than manual desludgings, households which received a high subsidy and whose neighbors were told about this might face stronger *social pressure* to purchase the sanitation service than households that had received the high subsidy in secret.

We test the extent to which social pressure based on neighbor's knowledge of people's high subsidies affects take-up by randomizing whether individuals are told the value of their neighbors' subsidies. If this test were to find evidence of social pressure's effect on high subsidy households, then we might additionally imagine that as more individuals in a neighborhood switched to using mechanized desludgings, the remaining holdouts (e.g., spillover and low subsidy households) using manual desludgings would feel more social pressure from those using the more sanitary technique to switch to the more sanitary method as well. If we do not find evidence of social pressure's impact on high subsidy households (which we can directly randomize), then it seems unlikely that social pressure is affecting the choices of low subsidy and spillover households.

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 (P^p_{ga}, standing for public price) and receiving the high subsidy. We run the following regression:

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

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

We estimate equation (6) in Table 8 and do not find any evidence of social pressure playing a significant role. The coefficient on the interaction term in column (1) is far from significant and of the wrong sign. As publicizing the subsidy level has no impact on sign up, it is unlikely that it would have impacts on actual adoption. 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, although Appendix Table G-2 shows that this test may be under-powered.

Table 8: Impact of Social Pressure on Desludging Decisions

	(1) Signed Up	(2) Subsidized Desludging	(3) Any Mechanized Desludging	(4) Manual Desludging
Own high subsidy	0.201***	0.085***	0.016	-0.056***
	(0.020)	(0.013)	(0.018)	(0.018)
Public price cluster	-0.008	-0.007	-0.024	-0.015
	(0.022)	(0.008)	(0.018)	(0.019)
High subsidy × Public-price cluster	-0.019	-0.010	0.024	0.056**
	(0.030)	(0.018)	(0.026)	(0.027)
N	3757	3757	3395	3395
Outcome mean, comparison group	0.306	0.038	0.309	0.327

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 at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for low-subsidy households that are not in a public-price cluster.

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.[19] Informal discussions during the design phase of this project made clear that manual desludging is not considered socially desirable. Thus, we suspect that social pressure's lack of impact is due to the fact that neighbors are not the right reference group.

7.2.4 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

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

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 specification for the dyadic regression is as follows:

$$A_{ijga2} = \alpha + \gamma A_{ijga1} + X'_{jga}\beta + \psi_i + \epsilon_{ijga}. \tag{7}$$

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.

If households adopt mechanized desludging in an attempt to curry favor with their neighbors, generosity towards a neighbor would increase after that neighbor signed up or would decrease after that neighbor purchased a manual desludging. Reciprocal generosity would also be greater when the recipient signed up or purchased despite being offered a low subsidy in the public-price clusters.

We test this in Table 9. 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.[20] 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 with households that sign up for the desludging, this may

[20] 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 9: 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
Outcome mean, comparison group	78.09	79.24	78.09	79.24

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. The outcome mean is shown for spillover receiver households.

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.

We may not find any effect of signing up on the amount received 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 one year earlier. On the other hand, they may remember that information but not feel it is important enough to affect 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:

$$C_{ijga} = \alpha + \beta_1 P^p_{ga} + \beta_2 P^w_{iga} + \beta_3 P^w_{jga} + \beta_4 P^w_{iga} P^w_{jga} + X'_{iga}\gamma + \psi_a + \epsilon_{ijga}. \qquad (8)$$

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. If both P^w_{iga} and P^w_{jga} equal 1, then giver i was given information about the sign-up decision made by recipient 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.

Appendix Table G-4 estimates equation (8) and shows that the public-price intervention did successfully increase knowledge regarding the price offered to neighbors. Column (1) shows that household members are 8 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.

7.2.5 Increasing Health Benefits and Decreasing Search Costs

The final mechanisms we consider, *increasing health benefits* and *decreasing search costs*, are not social. As can be seen in the pre-registration,[21] when we designed the intervention we had only considered social mechanisms. After running the experiment and conducting the analysis, we found evidence of decision spillovers that were not caused by pre-specified social

[21] Pre-registration information for AEARCTR-0000344 found here: https://doi.org/10.1257/rct.344-3.0).

mechanisms. But, because hypotheses related to non-social mechanisms were developed after data collection was complete, we did not conduct any mini-experiments designed specifically to test them, and thus our exploration of the non-social mechanisms are more speculative.

The epidemiological literature described in Section 1 suggests that increasing health benefits are likely.[22] Investments in sanitation may yield increasing health benefits (also known as technical complementarities (Guiteras et al. 2015) or epidemiological complementarities (Guiteras et al. 2019)) whereby as a neighborhood becomes cleaner, the marginal benefits to a household's investment in sanitation increases (Andrés et al. 2017, Fuller et al. 2016, Oswald et al. 2017). There is much evidence that health outcomes are more greatly affected by sanitation of the community as a whole, rather than by the individual household's level of sanitation (Andrés et al. 2017, Fuller et al. 2016, Jung, Hum, Lou & Cheng 2017, Oswald et al. 2017). There is also significant evidence that the marginal public benefit to community-level sanitation increases as sanitation improves (Andrés et al. 2017, Cameron et al. 2021, Jung, Lou & Cheng 2017, Wolf et al. 2019). Finally, there is evidence of increasing health benefits (or epidemiologocial complementarities). The private returns to own improved sanitation increase with the community's adoption rate (Andrés et al. 2017, Radin, Jeuland, Wang & Whittington 2020, Radin, Wong, McManus, Sinha, Jeuland, Larbi, Tuffuor, Biscoff & Whittington 2020).

Another non-social mechanism is decreasing search costs. Truckers may be more present in neighborhoods with more high subsidies, making it easier for households to flag down trucks or take note of the phone number printed on the truck and thus decreasing neighbors' search costs. Appendix Table G-3 discussed earlier showed that, in clusters with more high subsidies, individuals are more likely to find a desludger by calling their phone number (which is painted on the truck and can be seen as it drives by), waiting by the road, or being referred. This suggests a potential decrease in search costs.

Both increasing health benefits and decreasing customer search costs would increase demand, and potentially increase prices, in clusters with more high subsidies. Any increase in price would temper the size of decision spillovers. On the other hand, a third potential non-social mechanism explaining the decision spillover is that our intervention decreased the price of a mechanized desludging on the open market in clusters with more high subsidies due to increasing returns to the producer. This might happen if mechanized desludgers were

[22]Increasing health benefits differ from the coordination mechanism discussed in Section 7.2.2. Coordination occurs ex-ante (before adoption), while increasing health benefits arise ex-post. When we look for evidence of 'coordination,' we look at whether the decision of some households to sign up for the subsidized mechanized desludging affects other households' technology choice. When we look at 'increasing health benefits,' we look at whether households' purchases of mechanized desludgings affect other households' technology choice.

in those neighborhoods more frequently and so trucks did not need to use as much gas to arrive at those houses. We do not believe there to be increasing returns to the producer within clusters. This is both because i) a truck can only take one household's sludge at a time and would have to drive to the treatment center to dump it before returning to service a second household, and ii) households in the same cluster do not tend to use the same mechanized desludger for subsidized desludgings purchased through our service and for desludgings purchased on the open market (with the caveat that we have a lot of missing data on the desludging company used for purchases on the open market). We do not expect our intervention to lead to significant changes in cluster-level supply.[23]

Testing for effects of the number of high subsidies on unsubsidized prices allows us to confirm that there are not increasing returns to the producer. In Appendix Table G-5, 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 which 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 (1) 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 four.

We do not find any evidence that our intervention led to a decrease in market prices. If anything, we find that the price paid for a mechanized desludging by spillover households is marginally higher when more of their neighbors were offered a high subsidy, although the effects are not statistically significant. The coefficient suggests that the price of a mechanized desludging increases by about \$0.73, or 1.6% relative to the mean of \$45.17, for each household in the nearest four in the cluster that is offered a high subsidy. The effect is similarly 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. In a separate analysis, not shown here, we additionally see no evidence that desludgers' winning bids for subsidized desludgings are higher when more households in the cluster are offered the high subsidy. Because the household pays the subsidized price, while we pay the winning desludger his bid, this is more evidence that the subsidies do not lead to changes on the supply side. The positive coefficients in Table G-5

[23]Our other work suggests that mechanized desludging providers collude, and that there is significant excess capacity (Deutschmann et al. 2023, Houde et al. 2022).

are in accord with our hypothesis that a large saturation of high subsidies increased demand by spillover households that were willing to pay, if anything, a higher price, and not that there were changes on the supply side.

Differentiating prices paid by earlier versus later purchasers could potentially further help distinguish between increasing returns for producers (which would lead to falling prices) and increasing demand due to increasing health benefits (which would lead to rising prices). We know the exact timing of subsidized mechanized desludging purchases made from our administrative data, and approximate timing of mechanized desludging purchases on the open market from recall data in our survey. For the self-reported data, the bunching of observations at 0 (this month), 3, 6, and 12 months ago suggests that this will not give us very reliable information on the exact timing of purchases. We have evaluated the data using multiple approaches but find no evidence of systematic changes in prices within a cluster over time.

We hypothesize that if our intervention had social effects, then there might be spillover effects on water disposal more generally. If there are only epidemiological effects, then we only expect a spillover effect on sludge removal. We asked enumerators to note whether there was gray water (from washing dishes and bathing) disposed of in the courtyard and analyze that outcome in results not shown here. One's own high subsidy decreases both the likelihood of using a manual desludging and the likelihood of there being gray water found in the courtyard, perhaps because the subsidy causes households to decide to be more sanitary with their disposal of all types of water. But, the number of high subsidies in the cluster only impacts desludging decisions, and does not significantly impact the presence of gray water. This gives weak suggestive evidence against social spillovers in favor of epidemiological spillovers due to increasing health benefits.

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 learning from others, nor social pressure, nor reciprocity. 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 health benefits from neighbors' 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.

References

Andrés, L. A., Briceño, B., Chase, C. & Echenique, J. A. (2017), 'Sanitation and externalities: Evidence from early childhood health in rural India', *Journal of Water, Sanitation and Hygiene for Development* **7**(2), 272–289.

Baird, S., Bohren, A., McIntosh, C. & Özler, B. (2018), 'Optimal design of experiments in the presence of interference', *Review of Economics and Statistics* **100**(5), 844–860.

Bakhtiar, M. M., Guiteras, R. P., Levinsohn, J. & Mobarak, A. M. (2021), Social and financial incentives for overcoming a collective action problem. Unpublished Manuscript.

Barreto, M. L., Genser, B., Strina, A., Teixeira, M. G., Assis, A. M. O., Rego, R. F., Teles, C. A., Prado, M. S., Matos, S. M. A., Santos, D. N., dos Santos, L. A. & Cairncross, S. (2007), 'Effect of city-wide sanitation programme on reduction in rate of childhood diarrhoea in northeast Brazil: Assessment by two cohort studies', *The Lancet* **370**, 1622–1628.

Bates, M. A., Glennerster, R., Gumede, K. & Duflo, E. (2012), 'The price is wrong', *Field Actions Science Reports* **6**(4).

Beaman, L., Ben Yishay, A., Magruder, J. & Mobarak, A. M. (2021), 'Can network theory-based targeting increase technology adoption?', *American Economic Review* **111**(6), 1918–43.

Beaman, L., Keleher, N., Magruder, J. & Trachtman, C. (2021), 'Urban networks and targeting: Evidence from Liberia', *AEA Papers and Proceedings* **111**, 572–76.

Belloni, A., Chernozhukov, V. & Hansen, C. (2014), 'Inference on treatment effects after selection among high-dimensional controls', *Review of Economic Studies* **81**(2), 608–650.

Ben Yishay, A., Fraker, A., Guiteras, R., Palloni, G., Shah, N. B., Shirrell, S. & Wang, P. (2017), 'Microcredit and willingness to pay for environmental quality: Evidence from a randomized-controlled trial of finance for sanitation in rural Cambodia', *Journal of Environmental Economics and Management* **86**, 121–140.

Bloom, H. S. (1995), 'Minimum detectable effects: A simple say to report the statistical power of experimental designs', *Evaluation Review* **19**(5), 547–556.

Bursztyn, L. & Jensen, R. (2017), 'Social image and economic behavior in the field: Identifying, understanding, and shaping social pressure', *Annual Review of Economics* **9**, 131–153.

Cai, J., de Janvry, A. & Sadoulet, E. (2015), 'Social networks and the decision to insure', *American Economic Journal: Applied Economics* **7**(2), 81–108.

Cameron, L., Gertler, P., Shah, M., Alzua, M. L., Martinez, S. & Patil, S. (2021), The dirty business of eliminating open defecation: Evidence from field experiments in India, Indonesia, Mali and Tanzania. Unpublished Manuscript.

Cameron, L., Olivia, S. & Shah, M. (2019), 'Scaling up sanitation: Evidence from an RCT in Indonesia', *Journal of Development Economics* **138**, 1–16.

Conley, T. G. & Udry, C. R. (2010), 'Learning about a new technology: Pineapple in Ghana', *American Economic Review* **100**(1), 35–69.

Deutschmann, J. W., Gars, J., Griffith, S., Houde, J.-F., Johnson, T., Lipscomb, M., Mbeguere, M., Nehrling, S., Schechter, L. & Zhu, S. J. (2020), Using market mechanisms to increase the take-up of improved sanitation. Unpublished Manuscript.

Deutschmann, J. W., Gars, J., Houde, J.-F., Lipscomb, M. & Schechter, L. (2023), 'Privatization of public goods: Evidence from the sanitation sector in Senegal', *Journal of Development Economics* **160**.

Deutschmann, J. W., Lipscomb, M., Schechter, L. & Zhu, S. J. (2022), 'Replication data for: Spillovers without social interactions in urban sanitation', `https://doi.org/10.3886/EICPSR181101V1`.

Duflo, E., Galiani, S. & Mobarak, M. (2012), Improving access to urban services for the poor: Open issues and a framework for a future research agenda. J-PAL Urban Services Review Paper.

Dupas, P. (2014), 'Short-run subsidies and long-run adoption of new health products: Evidence from a field experiment', *Econometrica* **82**(1), 197–228.

Dupas, P., Fafchamps, M. & Houeix, D. (2022), Measuring relative poverty through peer rankings: Evidence from Côte d'Ivoire. Unpublished Manuscript.

Fewtrell, L., Kaufmann, R. B., Kay, D., Enanoria, W., Haller, L. & Colford Jr, J. M. (2005), 'Water, sanitation, and hygiene interventions to reduce diarrhoea in less developed countries: A systematic review and meta-analysis', *The Lancet Infectious Diseases* **5**(1), 42–52.

Fuller, J. A., Villamor, E., Cevallos, W., Trostle, J. & Eisenberg, J. N. (2016), 'I get height with a little help from my friends: Herd protection from sanitation on child growth in rural Ecuador', *International Journal of Epidemiology* **45**(2), 460–469.

Gautam, S. (2018), Household (under) adoption of sanitation: Importance of externalities and borrowing constraints. Unpublished Manuscript.

Guiteras, R., Levinsohn, J. & Mobarak, A. M. (2015), 'Encouraging sanitation investment in the developing world: A cluster-randomized trial', *Science* **348**(6237), 903–906.

Guiteras, R., Levinsohn, J. & Mobarak, A. M. (2019), Demand estimation with strategic complementarities: Sanitation in Bangladesh. Unpublished Manuscript.

Guiteras, R. P., Levine, D. I., Luby, S. P., Polley, T. H., e Jannat, K. K. & Unicomb, L. (2016), 'Disgust, shame, and soapy water: Tests of novel interventions to promote safe water and hygiene', *Journal of the Association of Environmental and Resource Economists* **3**(2), 321–359.

Haushofer, J. & Shapiro, J. (2016), 'The short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya', *The Quarterly Journal of Economics* **131**(4), 1973–2042.

Herrera-Yagüe, C., Schneider, C. M., Couronné, T., Smoreda, Z., Benito, R. M., Zufiria, P. J. & González, M. C. (2015), 'The anatomy of urban social networks and its implications in the searchability problem', *Scientific Reports* **5**.

Houde, J.-F., Johnson, T., Lipscomb, M. & Schechter, L. (2022), Imperfect competition and sanitation: Evidence from randomized auctions in Senegal. Unpublished Manuscript.

Jung, Y. T., Hum, R. J., Lou, W. & Cheng, Y.-L. (2017), 'Effects of neighbourhood and household sanitation conditions on diarrhea morbidity: Systematic review and meta-analysis', *PLOS ONE* .

Jung, Y. T., Lou, W. & Cheng, Y.-L. (2017), 'Exposure-response relationship of neighbourhood sanitation and children's diarrhoea', *Tropical Medicine and International Health* **22**(7), 857–865.

Kar, K. & Chambers, R. (2008), *Handbook on Community-Led Total Sanitation*, Institute of Development Studies, Brighton.

Kresch, E. P., Lipscomb, M. & Schechter, L. (2020), 'Externalities and spillovers from sanitation and waste management in urban and rural neighborhoods', *Applied Economic Perspectives and Policy* **42**(3), 395–420.

Lerva, B. (2020), Quantifying externalities in technology adoption: Experimental evidence from Ugandan farmers. Unpublished Manuscript.

Ligon, E. & Schechter, L. (2012), 'Motives for sharing in social networks', *Journal of Development Economics* **99**(1), 13–26.

Lipscomb, M. & Schechter, L. (2018), 'Subsidies versus mental accounting nudges: Harnessing mobile payment systems to improve sanitation', *Journal of Development Economics* **135**, 235–254.

Mara, D., Lane, J., Scott, B. & Trouba, D. (2010), 'Sanitation and health', *PLoS Medicine* **7**(11).

Oswald, W. E., Stewart, A. E., Kramer, M. R., Endeshaw, T., Zerihun, M., Melak, B., Sata, E., Gessese, D., Teferi, T., Tadesse, Z., Guadie, B., King, J. D., Emerson, P. M., Callahan, E. K., Flanders, D., Moe, C. L. & Clasen, T. F. (2017), 'Active trachoma and community use of sanitation, Ethiopia', *Bulletin of the World Health Organization* **95**(4), 250–260.

Pattanayak, S. K., Yang, J.-C., Dickinson, K. L., Poulos, C., Patil, S. R., Mallick, R. K., Blitstein, J. L. & Praharaj, P. (2009), 'Shame or subsidy revisited: Social mobilization for sanitation in Orissa, India', *Bulletin of the World Health Organization* **87**(8), 580–587.

Pickering, A. J., Djebbari, H., Lopez, C. & Alzua, M. L. (2015), 'Effect of a community-led sanitation intervention on child diarrhoea and child growth in rural Mali: A cluster-randomised controlled trial', *The Lancet* **3**(11), 701–711.

Radin, M., Jeuland, M., Wang, H. & Whittington, D. (2020), 'Benefit-cost analysis of Community-Led Total Sanitation: Incorporating results from recent evaluations', *Journal of Benefit-Cost Analysis* **11**(3), 380–417.

Radin, M., Wong, B., McManus, C., Sinha, S., Jeuland, M., Larbi, E., Tuffuor, B., Biscoff, N. K. & Whittington, D. (2020), 'Benefits and costs of rural sanitation interventions in Ghana', *Journal of Water, Sanitation and Hygiene for Development* **10**(4), 724–743.

Schechter, L. (2007), 'Theft, gift-giving, and trustworthiness: Honesty is its own reward in rural Paraguay', *American Economic Review* **97**(5), 1560–1582.

Sharma, A., Gupta, S. & Pandey, N. K. (2022), 'Analysis of sanitation scheme in rural and urban areas under Swachh Bharat Abhiyan (SBM): A comparative study', *Regional Economic Development Research* **3**(1), 73–88.

Stopnitzky, Y. (2017), 'No toilet no bride? Intrahousehold bargaining in male-skewed marriage markets in India', *Journal of Development Economics* **127**, 269–282.

Wolf, J., Johnston, R., Hunter, P. R., Gordon, B., Medlicott, K. & Prüss-Ustün, A. (2019), 'A Faecal Contamination Index for interpreting heterogeneous diarrhoea impacts of water, sanitation and hygiene interventions and overall, regional and country estimates of community sanitation coverage with a focus on low- and middle-income countries', *International Journal of Hygiene and Environmental Health* **222**(2), 270–282.

Wolf, J., Prüss-Ustün, A., Cumming, O., Bartram, J., Bonjour, S., Cairncross, S., Clasen, T., Colford Jr, J. M., Curtis, V., De France, J., Fewtrell, L., Freeman, M. C., Gordon, B., Hunter, P. R., Jeandron, A., Johnston, R. B., Mäusezahl, D., Mathers, C., Neira, M. & Higgins, J. P. T. (2014), 'Assessing the impact of drinking water and sanitation on diarrhoeal disease in low- and middle-income settings: Systematic review and meta-regression', *Tropical Medicine and International Health* **19**(8), 928–942.

World Health Organization (2020), *Fact sheet 3.2: Open-air defecation*, Geneva, Switzerland. `https://www.who.int/water_sanitation_health/emergencies/fs3_2/en/`.

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).

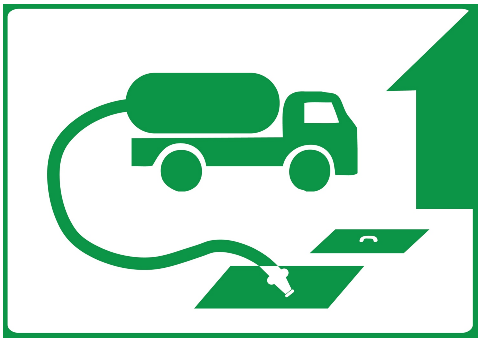
At the cluster level we randomized the number of households, out of ten, which were offered the high subsidy.

- 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



D Variables used for post-double-selection LASSO

In this appendix, we describe the variables used for the post-double-selection lasso methods (Belloni et al. 2014) we use throughout the paper. 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.

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: Ex-post Minimum Detectable Effect Sizes (MDEs)

		(1) Used Any Mechanized Desludging	(2) Used Manual Desludging	(3) # of hhd members who had diarrhea	(4) Share of hhd members who had diarrhea
Panel A: Reference points					
Baseline data	Mean	0.292	0.376	0.952	0.096
	SD	(0.455)	(0.484)	(1.489)	(0.155)
Panel B: Ex-post power calculations - Minimum detectable effect sizes (MDE)					
Table 2	(treated households) # of high subsidy hhds in cluster	0.013	0.014		
	(spillover households) # of high subsidy hhds in cluster	0.023	0.030		
	(all households) # of high subsidy hhds in cluster	0.012	0.013		
Table 4	# of high subsidy hhds in cluster			0.030	0.003
	# of high subsidy hhds in nearest 4			0.044	0.005
Table 5	# of high subsidy hhds in cluster that you are aware of	0.020	0.021		
	# of high subsidy hhds in cluster that you drink tea with	0.025	0.024		
	# of high subsidy hhds in cluster that you would pick as health leader	0.031	0.034		
	# of high subsidy hhds in cluster that you talked about sanitation with	0.034	0.034		
	# of high subsidy hhds in cluster that are wealthy	0.025	0.025		
	# of high subsidy hhds in cluster that are in nearest 4	0.020	0.022		
Table 6	# signed up in 1st 5 × Public-how-many cluster	0.054	0.056		
	# signed up in 1st 5 × Public-who cluster	0.052	0.056		
Table 7	# signed up in 1st 5 that you are aware of × Public-who cluster	0.102	0.109		
	# signed up in 1st 5 that you drink tea with × Public-who cluster	0.112	0.124		
	# signed up in 1st 5 that you would pick as health leader × Public-who cluster	0.181	0.196		
	# signed up in 1st 5 that you talked about sanitation with × Public-who cluster	0.157	0.161		
	# signed up in 1st 5 that are wealthy × Public-who cluster	0.153	0.146		
	# signed up in 1st 5 that are in nearest 4 × Public-who cluster	0.121	0.125		
Table 8	High subsidy x Public-price cluster	0.072	0.075		

Note: This table shows the ex-post minimum detectable effect sizes (MDE) for main outcomes and main controls. Outcome variables are (1) purchased any mechanized desludging between the baseline and endline, (2) had any manual desludging between the baseline and endline, (3) number of household members who had diarrhea in the past week in endline, and (4) share of household members who had diarrhea in the past week in endline. Panel A shows the mean and standard deviation of outcomes in the baseline. Panel B reports the minimum detectable effect sizes, calculated ex-post using a significance level of 0.05 and power of 80%.

Table G-3: Method of Finding Desludging Provider - Odds Ratios

	(1) How Desludger Found
Alternative-Specific Variable	
Alternative chosen in bl	1.758***
	(0.144)
Manual (26%)	
Own high subsidy	0.855
	(0.093)
# other high subsidies in cluster	0.918**
	(0.033)
Mechanized - garage (5%)	
Own high subsidy	0.821
	(0.142)
# other high subsidies in cluster	1.006
	(0.052)
Mechanized - call, flag, or referral (20%)	
Own high subsidy	0.936
	(0.112)
# other high subsidies in cluster	1.104**
	(0.043)
Mechanized - other (6%)	
Own high subsidy	2.584***
	(0.402)
# other high subsidies in cluster	1.026
	(0.055)
N of Obs.	20455
N of Cases	4091

Note: The sample includes all hhds. Standard errors clustered at the grid-point level in parentheses. Conditional logit estimation presenting odds ratios and testing significance with respect to 1: * p<0.10, ** p<0.05, *** p<0.01. Odds ratios greater than 1 imply a positive and odds ratios less than 1 a negative relationship. Outcome is the desludging choice between baseline and endline. The base alternative is having no desludging (42% of observations). The other alternatives are (1) having a manual desludging, or having a mechanized desludging and finding the desludger (2) at a 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 baseline) selected by post-double-selection LASSO for inclusion in Table 2 (Panel C, columns 3 and 4) and arrondissement fixed effects, all interacted with the different alternatives.

Table G-4: Ability to Remember Neighbors' Subsidy Levels and Decisions

	Baseline		Endline		
	(1)	(2)	(3)	(4)	(5)
	Subsidy	Signed Up	Subsidy	Signed Up	Used Subs Desl
Public-price cluster	0.076***	0.010**	0.006***	-0.002	0.001
	(0.018)	(0.005)	(0.002)	(0.002)	(0.001)
Public-who cluster × 2nd 5 hh	0.039	0.012	-0.004**	0.004	0.000
	(0.027)	(0.010)	(0.002)	(0.005)	(0.003)
Public-who cluster × 1st 5 nghbr	0.017	0.004	0.002	-0.004	-0.000
	(0.017)	(0.007)	(0.003)	(0.003)	(0.002)
Public-who cluster × 2nd 5 hh × 1st 5 nghbr	-0.016	0.004	-0.002	0.005	-0.000
	(0.017)	(0.009)	(0.003)	(0.005)	(0.004)
Same respondent in bl intervention and bl payment survey	0.066***	0.018***			
	(0.018)	(0.005)			
Weeks between bl intervention and bl payment survey	-0.008**	-0.003***			
	(0.003)	(0.001)			
Same respondent in bl intervention and el survey			0.002	0.007***	0.003**
			(0.002)	(0.002)	(0.001)
Weeks between bl intervention and el survey			-0.001	-0.001	-0.001*
			(0.001)	(0.001)	(0.001)
N	8187	8184	31311	31291	31034
Outcome mean, comparison group	0.002	0.010	0.002	0.017	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 at baseline) are chosen using post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for households that are not in a public price cluster.

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
# other high subsidies in cluster	0.459	0.274		
	(0.351)	(0.654)		
# of high subsidies in nearest 4			0.186	0.728
			(0.473)	(0.809)
N	682	213	682	213
Outcome mean, comparison group	45.05	45.17	45.05	45.17

Note: The sample in columns (1) and (3) includes all low subsidy and spillover households that purchased a mechanized desludging between the baseline and endline. The sample in columns (2) and (4) includes only spillover households that 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 include the baseline mechanized desludging price reported, a control for whether the baseline mechanized price is missing, a spillover dummy in columns (1) and (3), and additional controls are selected by post-double-selection LASSO. Fixed effects at the arrondissement level. The outcome mean is shown for all low subsidy and spillover households in columns (1) and (3), and for all spillover households in columns (2) and (4).