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Measuring Land Rental Market Participation in Smallholder Agriculture

Can Survey Design Innovations Improve Land Market Participation Statistics?

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ABSTRACT

The emergence of rural land rental markets in Sub-Saharan Africa is recognized as a key component of the region's ongoing economic transformation. However, the evidence base on land market participation relies on survey-derived measures, which do not always cohere when compared and triangulated, suggesting the possibility of non-trivial measurement error. We report the results of a priming and list experiments designed to shed light on a persistent mystery in rural household survey data from Africa: why there are so many fewer self-reported landlords (renters-out) than tenants (renters-in)? Our design addresses two hypotheses using experimental data from Ethiopia. First, rented-out and rented-in land may be systematically underreported because enumerators and respondents are typically primed to emphasize parcels that are actively managed/cultivated by the household. Second, rented or sharecropped-out land may be systematically underreported because of respondents' reluctance to acknowledge an activity for which public disclosure may have negative repercussions. We address the first hypothesis with a priming experiment by exposing a random subset of respondents to a nudge that explicitly reminded them to fully account for all land, including rented/sharecropped-in and rented/sharecropped-out. We address the second hypothesis with a double-list experiment, designed to elicit true rates of land renting and sharecropping-out. We find that nudging induces about 4 percentage points increase (or 13% in relative terms) in the share of households participating in renting in or sharecropping-in practices but has negligible effects on reported rates of renting and sharecropping-out. Interestingly, our list experiment indicates much higher revealed rates of renting-out (14-15%) than is reflected in the nominal parcel-roster responses (3%). The magnitude of the latter finding fully explains the apparent difference in renting in versus renting-out rates derived from the regular parcel roster responses. These results indicate that efforts to document land market participation rate and associated impacts must overcome large systematic reporting biases.

***Keywords*:** land rental markets; social desirability bias; double list experiment; household survey design

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1. Introduction

The empirical literature on land rental market development in sub-Saharan Africa has documented rapid growth in land market participation rates over the last two decades in many parts of the continent, reflecting increasing land pressures, expanding market opportunities for agriculture, and evolving tenurial institutions (Holden et al., 2010; Deininger et al., 2017; Jayne et al., 2021; Abay et al., 2021). Previous studies indicate that these rental transactions facilitate land transfers from less productive households to more productive households, leading to efficiency gains, as well as from land-abundant households to land-constrained households, resulting in equity gains (e.g., Jin and Jayne 2013; Chamberlin and Ricker-Gilbert, 2016; Wineman and Liverpool-Tasie, 2017; Ricker-Gilbert and Chamberlin, 2018; Holden and Tilahun, 2021). However, such conclusions about the nature and impacts of land rental market participation depend critically upon the fidelity of observational data, in particular the degree to which survey responses are fully observed and/or accurate measures of true participation (Ricker-Gilbert et al., 2019; Jayne et al., 2021; 2022).

Yet there are suggestive indications that survey-derived participation rates may be problematic. In particular, one persistent mystery is why so many more tenants are observed than landlords in nationally representative surveys in Africa. For example, using nationally representative data from six countries (Ethiopia, Malawi, Niger, Nigeria, Tanzania and Uganda), Deininger et al. (2017) show that the number of households reporting renting-out land (i.e., as landlords) are equivalent to only 2% and 25% of the number of households reporting renting in land (i.e., as tenants), depending upon the country. Also using nationally representative data for Malawi and Zambia, Chamberlin and Ricker-Gilbert (2016) find that there are 44% - 62% fewer self-reported landlords than tenants in Malawi, and 30% - 83% fewer self-reported landlords than tenants in Zambia (with specific values depending upon the year of comparison). Abay et al. (2021) find similar patterns using the nationally representative Living Standard Measurement Surveys (LSMS) for Ethiopia, Malawi and Tanzania. These patterns hold both at household and parcel levels and are not reconciled when accounting for the total amount of land transacted. Such discrepancies suggest that we are not fully observing the landlord side of the market, which implies that our conclusions about efficiency and welfare effects may be fundamentally biased.

What might account for such reporting discrepancies? There are several logical possibilities for many more indicators of land rental participation in sub-Saharan Africa are heavily skewed toward tenancy. First, it is possible that landlords rent-out their land to multiple tenants, such that while the numbers of landlords and tenants may differ, the total amount of land rented is fully observed. However, this appears unlikely: the authors of the studies cited above note that this persistent mismatch cannot be reconciled on the basis of land area (i.e., the total amount of land reported as rented-out is similarly lower than the total amount of land rented in). Second, it is possible that landlords are systematically unobserved in survey sampling frames, or they are less likely to participate in household surveys. This would be the case if outsiders (e.g., urban-based landlords) were renting to local households who were more likely to turn up in samples. This is certainly plausible, given the prevalence of urban-based investors in much of the region, for example, as part of the expansion of medium-scale farms (Jayne et al., 2022). However, there is no direct evidence for this.[1] Qualitative studies of land institutions in Africa have emphasized outsiders as land seekers, rather than as absent landlords; descriptive narratives emphasize how outsiders acquire land, with these processes sometimes framed as evidence of how wealthy or influential actors (like urban elites) take advantage of emerging land markets to displace poor people via distress sales and/or outright land grabbing by elites working with traditional authorities (Chitonge et al., 2017; Chimhowu, 2019).

This leaves two other possible explanations for the asymmetry in observed numbers of landlords and tenants. First, it is possible that renting-out land is systematically under-reported in household surveys which emphasize productive agricultural activities like crop cultivation. Both enumerators and respondents may fail to list all rented-out plots if they understand the emphasis of data collection to be on farming activities of the household. This is plausible given that there are not many "specialized" surveys on land market participation in Africa and most statistics associated with land market participation come from agriculture modules included as part of multipurpose household surveys. The main purpose of these surveys is often not land market participation and these long surveys sometimes suffer from fatigue and recall bias that could lead to under-reporting of parcels/plots in general and rented-out/-in parcels and plots in particular (e.g., Beegle et al., 2012; De Weerdt et al., 2020; Ambler et al., 2021; Abay et al., 2022; Abay et al.,

[1] Not only is there no evidence of this in the literature, but no anecdotal evidence that we are aware of. Note that the few existing references to "absentee landlords" in African contexts (e.g., Morapedi, 2020; Bret, 1973) refer to cases of land being held by non-resident owners, but not being actively used (i.e., "idle land"), and make no reference to renting or leasing out such land to tenants. The only study we are aware of that explicitly seeks to profile the whole suite of land market participants is a fairly localized study of rental markets in four districts of Malawi (Ricker-Gilbert et al., 2022). While they did find evidence of non-local tenants (where non-local was defined on the basis of being from a different village, rather than the place of usual residence) they found no such evidence for non-local landlords.

2023; Jeong et al., 2023). In contexts where smallholder farmers manage fragmented and multiple small parcels/plots, listing all parcels entail cognitive burden on respondents, which in turn can encourage under-reporting of portfolio of land and parcels to minimize the burden. Besides distorting descriptive statistics, such under-reporting rates are likely to behave systematically and hence distort statistical inferences, including those related to the impact of land market participation and associated implications on technical and allocative inefficiency (e.g., Deininger et al., 2008; Holden et al., 2009; Jin and Jayne, 2013; Chamberlin and Ricker-Gilbert 2016; Restuccia and Santaeulalia-Llopis, 2017; Chen et al., 2023).[2]

A second possibility is that many respondents may be reluctant to disclose renting or sharecropping-out land in interview settings. This may be the case, for example, if land rental is explicitly or implicitly disallowed or discouraged under local customary land norms, or if it could be perceived as a potential signal to customary authorities that land is not being used productively by the landlord and may thus provoke fears of reallocation by traditional authorities or clan members (Ghebru and Lambrecht, 2017; Hall et al., 2017; Honig, 2017; Stickler et al., 2018). This is particularly relevant in contexts like Ethiopia where land redistribution practices usually target unused land by landholders. Despite some differences across regions, unused or abandoned land is subject to redistribution to landless youth in Ethiopia. Similarly, land formerly held by deceased members of the community is subject to redistribution, through inheritance claims by family members or through reallocation by local authorities to other community members. These practices are likely to trigger systematically different reporting rates across tenants and landlords. Despite these potentially distortionary practices and policies, we know very little about their implications on households' reporting behavior and hence land market participation statistics. A systematic empirical exploration of these latter two possibilities (under-reporting because of cognitive burden or intentional motives) is the primary focus of this paper.

Understanding the underlying sources of the apparent underreporting of land market participation rates is important because it has implications for how we evaluate and quantify the impacts of rental market development, including how we interpret the results of prior empirical studies on the role and potential of land rental markets. This is particularly crucial as these

[2] More broadly, these limitations in data associated with land rental markets can distort our understanding of the role of land rental markets in economic development and structural transformation of rural economies (Deininger and Jin, 2005; Jin and Deininger, 2009).

underlying factors are likely to generate differential responses across different types of households. For example, if our observation of rental participation is systematically biased toward wealthier or poorer households, then microeconomic models of the drivers and benefits of land rental market participation will be inconsistent, with potentially large consequences for the analytical conclusions drawn from such studies. Only by fully observing (or understanding how and why we fail to fully observe) the participation in rental markets by both tenants and landlords can we robustly inform relevant land policies.

In this paper, we describe a novel set of survey experiments, implemented on a large random sample of Ethiopian smallholder farmers, designed to address two of the hypothesized sources of reporting bias outlined above. We use a priming (nudge) experiment, in which a random subset of survey respondents are explicitly reminded to fully account for all land (including rented/sharecropped-in and rented/ sharecropped-out parcels) before reporting their cultivated land portfolio, to evaluate the impacts of such prompting on the measured incidence of renting/sharecropping-out and renting/sharecropping-in. These households were primed to first list rented/sharecropped-in and rented/sharecropped-out parcels when they start the land use module. We also employ a list experiment to elicit true rates of renting-out or sharecropping-out, under the assumption that this is a potentially sensitive question that many farmers are reluctant to respond directly to because of fears of reallocation. We employ both of these experiments in a cross-randomized design that allows us to examine their effects independently as well as jointly.

We find that nudging has only negligible effects on reported rates of renting-out and sharecropping-out but induces large and statistically significant increases in the reported rate of renting-in and sharecropping-in parcels. Specifically, priming (nudging) increased the reported share of households participating in land markets as tenants by 4 percentage points (13% of the revealed participation rate). At parcel level, it leads to a 2 percentage points (15%) increase in reporting rates of rented-in and sharecropped-in parcels. This implies that 15% of rented-in or sharecropped-in parcels will not be observed when the nudge is omitted, amounting to a non-trivial share of market participation on the tenant side. These results are interesting in that they not only respond to our motivating questions around the sensitivity of reported rates of renting-out, but also generate some surprising and unexpected insights about reported rates of renting-in. The results from the list experiment, on the other hand, indicate much higher revealed rates of renting-out (14% to 15%) than the reported rate of 3% derived from parcel roster responses, i.e., the standard way of identifying landlords in survey data. This finding explains the difference in renting-in and renting-out rates derived from parcel roster responses in our data.

Our findings have important implications for improving land market participation statistics and informing relevant designs to address sensitive questions. The results imply that survey-derived land rental rates reported elsewhere for the region may be underestimated by non-trivial amounts. Our study makes several unique contributions to different strands of literature. First, despite the widely acknowledged imbalance between reported rates of renting in and out in the literature on African land markets, and some speculation about underlying reasons (e.g., Jayne et al., 2021, 2022) we know of no prior work that has empirically evaluated potential explanations. Secondly, our contribution to the methodological literature on rural household survey design in developing country contexts is also unique. Although prior studies have evaluated alternative framing and prompting cues on outcomes of interest, such as food security outcomes (Friedman et al., 2017; Conforti et al. 2018; Abate et al., 2022), child labor (Jouvin, 2023), violence (Gulesci et al., 2021), and sexual behavior (Chuang et al., 2021), we are not aware of survey design experiments related to land-related outcomes, including rental market participation.[3] Third, our results contribute to the literature on the effectiveness of nudges (e.g., DellaVigna and Linos, 2022; Congiu and Moscati, 2022) and list experiments (e.g., Karlan and Zinman, 2012; Chuang et al., 2021; Agüero and Frisancho, 2021; De Cao and Lutz, 2018; Bulte and Lensink, 2019; Aksoy et al., 2022). Our findings demonstrate that simple nudge embedded in survey designs can meaningfully improve reporting rates in land market participation while the list experiment findings hint the need for specialized survey designs to understand true level of land market participation in similar contexts. While there have been many prior evaluations of list experiments for eliciting answers to sensitive questions, to our knowledge there have been no prior applications to addressing land use or land market participation decisions.[4] Our work thus explores the

[3] Other important avenues of methodological research on survey design, although less directly relevant to our study, have addressed sensitivity of various reported outcomes to recall period (Nicola and Giné, 2014; Arthi et al., 2018; Wollburg et al., 2021), survey length and to gender/household position of respondent (e.g., Ambler et al. 2021) and choice of respondent (Kilic et al., 2021). See De Weerdt et al. (2020) and Abay et al. (2023) for a recent review of survey design experiments in developing country contexts.
[4] Studies employing this method have largely focused on behaviors such as condom use, sexual orientation, and sexual activity (e.g., Chong et al., 2020; Chuang et al., 2021; Aksoy et al., 2022), intimate partner violence (e.g., Joseph et al., 2017; Agüero and Frisancho, 2021) and female genital cutting (e.g., De Cao and Lutz, 2018).

relevance of such methodological questions on data elicitation strategies for a previously unexamined thematic domain of importance in development economics.

The rest of the paper is organized as follows. In Section 2, we describe our experimental design and implementation, followed by a description of the survey instrument and sample in Section 3. We present and discuss our results in detail in Section 4. Section 5 offers concluding remarks and recommendations for improving survey design, data quality and inferences associated with land rental market statistics.

2. Experimental design

Our design tests the above-mentioned hypotheses related to potential sources of bias in reporting on different categories of land rental market participation. The first hypothesis is that rented land is systematically underreported because agricultural land use modules are often included in large household surveys that take several hours, leading to enumerator and respondent fatigue. Furthermore, smallholder farmers managing several fragmented parcels may face significant cognitive burden to recount and recall all parcels managed and/or owned by the household, especially when the cultivation and survey timing entail significant gaps. In these contexts, enumerators and respondents may be forced to prioritize and emphasize parcels that are owned and actively managed (e.g., cultivated) by the household at the expense of rented/sharecropped-in and rented/sharecropped-out parcels. To test this hypothesis, we introduce a simple survey experiment that nudges (primes) respondents at the beginning of the land use module to be "comprehensive" and "exhaustive" in their reporting of parcels they own and manage (including parcels that are rented in (out) and sharecropped in (out)). We explicitly prime respondents with the following instructions, which vary across the randomly assigned "control" and "treatment" groups:

(i) Control group households: For this group of farmers enumerators were instructed as follows: *Enumerator*: *"Draw a simple map of the parcels of agricultural land **Owned** by or **Managed** by members of the household in either season (Meher or Belg) of 2022-2023 (2014-2015 Ethiopian calendar). Then number the parcels and complete the following module for each parcel.*"

(ii) Treatment group households: For this group of farmers enumerators were instructed as follows: *Enumerator*: "*Draw a simple map of the parcels of agricultural land **Owned** by*

> *or **Managed** by members of the household in either season (Meher or Belg) of 2022-2023 (2014-2015 Ethiopian calendar). Then number the parcels and complete the following module for each parcel. Please start with parcels that are **rented-out** or **rented-in**, then those **sharecropped-out** and **sharecropped-in**, and finally those that are owned and managed by members of the household*."

As part of the nudge, we probe the respondents in the treatment group asking if there are additional rented in (out) or sharecropped in (out) parcels at the end of the land use module (i.e., *Enumerator*: Ask the following question at the end of the parcel roster: "are you sure there are no more rented in (out) or sharecropped in (out) parcels?"). We hypothesize that the explicit nudge/priming in the treatment group can help respondents recall and list all parcels owned or managed by the household. This can reduce under-reporting of renting-in/out or sharecropping-in/out, especially if respondents suffer from recall bias.

The second hypothesis we address is that rented or sharecropped-out land is systematically underreported because of respondents' reluctance to acknowledge renting- and/or sharecropping-out land for which public disclosure may have negative repercussions (e.g., if extra-legal). To test the second hypothesis, we introduced a double list experiment technique commonly used to accurately capture responses to sensitive questions that are subject to social desirability bias, shame, or fear (e.g., Jouvin, 2023, Tadesse et al., 2020; Cullen, 2020). List experiments, also referred to as item count or unmatched count techniques, hide individual responses to sensitive question/item by bundling them with answers to non-sensitive questions/items. The questions/items we use in the list experiment are presented in Table 1 and the sensitive items are those related to land rented-out and sharecropped-out.

Our list of the non-sensitive items follow the standard guidance on selecting such items to reduce the variance of the list experiment, which includes: seeking a balance between the length of the lists with privacy protection (too short items may not offer sufficient privacy while too long list may be taxing to respondents); inclusion of both low-prevalence and high-prevalence items to avoid "floor" ("no" to everything) or ceiling ("yes" to everything) responses; designing negative correlation for items within a list, and positive correlation between the lists in a double list design (see Blair and Imai, 2012 and Glynn, 2013).

Table 1: List experiment design

	Group A	Group B
List experiment 1	Terracing prevents soil erosion. The price of maize is always higher than the price of teff. My family is smaller than the average family.	Terracing prevents soil erosion. The price of maize is always higher than the price of teff. **I have rented-out land in the last *Meher* season.** My family is smaller than the average family.
List experiment 2	Extension is a men's business and women shouldn't attend. Rainfall in this area is sometimes not enough for a good harvest. **I have rented-out land in the last *Meher* season.** Life is less expensive now, compared with last year	Extension is a men's business and women shouldn't attend. Rainfall in this area is sometimes not enough for a good harvest. Life is less expensive now, compared with last year
List experiment 3	Education is useful for improving livelihood. Men and women are born equal. More rainfall is good for production.	Education is useful for improving livelihood. Men and women are born equal. **I have sharecropped-out land in the last *Meher* season.** More rainfall is good for production.
List experiment 4	Drinking too much alcohol is not good for health. Farming is more tiresome than animal keeping. **I have sharecropped-out land in the last *Meher* season.** Farming is easy work.	Drinking too much alcohol is not good for health Farming is more tiresome than animal keeping. Farming is easy work.

As shown in Table 1, we first randomly assigned our sample households in two different study groups – Group A and Group B – and implemented a double-list experiment that allow each study group to serve sequentially as the treatment and then control group or vice versa. In the double list experiment set-up, respondents participate in two list experiments with different control items but the same sensitive item (Glynn, 2013). The advantage of the double-list design is that the two list experiments provide a difference-in-mean estimator that can be averaged, and the combined estimate has approximately half the variability (variance) relative to the single list experiment (Droitcour et al., 1991; Glynn, 2013). In our set-up, households in Group A served as a control group for list experiments 1 and 3 and as a treatment group for list experiments 2 and 4. On the other hand, households in Group B served as a treatment group for list experiments 1 and

3 and as a control group for list experiments 2 and 4. In the case of control, households are presented with only non-sensitive items and asked to indicate to how many of the items they agree with (not asked to say whether they agree to each of the items). The same non-sensitive items are presented in the case of the treatment, with the addition of the sensitive item of interest and again asked to indicate to how many of the items they agree with. Assuming that the two groups have comparable responses to non-sensitive items (given the random assignment), the estimated prevalence rate of the sensitive item is then calculated as the difference in mean responses across the treatment and control groups.

Regarding actual implementation of the experiment, enumerators were provided with a script they are required to read in its entirety. The instruction presented to control (treatment) group are as follows: *"Please have these 3 (4) beans and hold them in your right hand behind your back. I am going to read 3 (4) statements about various topics. If the statement I read is true, and applies to you, transfer a bean from your right hand to your left hand behind your back. If the statement is not true, do not transfer a bean. After I read you all the 3 (4) statements, you will show me your left hand and the number of beans you transferred. Please do not answer "agree" or "disagree" when I read the statements to you since I do not want to know which statement(s) is true for you; I am only interested on the number of statement(s) that are true for you."* The experiments were administered in the same order, irrespective of the respondent group.

The two survey experiments were cross-randomized to allow the examination of their effects independently as well as jointly. In both cases, the randomization of households were stratified at the *kebele* (village) level, the lowest administrative unit in Ethiopia. In other words, within each *kebele*, sample households were randomly allocated to treatment and control groups to generate study groups (arms) with similar characteristics including on community land governance regime.

3. Data and descriptive statistics

We use data from the Ethiopian Agricultural Commercialization Clusters (ACC) survey conducted by the International Food Policy Research Institute (IFPRI) for the Ethiopian Agricultural Transformation Agency (ATA) in 2019 and 2023. In 2019, the ACC (baseline) survey covered 5311 sample households in 154 *woredas* (districts) and 355 *kebeles* in the four main agriculturally important regions (Amhara, Oromia, SNNP, and Tigray). The sample households were selected following a three-stage sampling procedure. First, the *woredas* (districts) were stratified into Agricultural Commodity Clusters (ACC) defined by the ATA and five sample *woredas* (districts) were randomly selected from each ACC. Second, two *kebeles* were randomly selected from each district to be part of the survey. Finally, 15 farm households were randomly selected from each sample *kebele* based on the household lists maintained by local administrations. In addition, about 20% of the sample was selected from neighboring districts outside the ACCs, using the same three-stage sampling. In 2023, we were able to revisit 3904 households despite the security situation in the country (i.e., we were not able to revisit all sample households in Tigray and sizable number of households in Amhara and Oromia) and that constitute the final sample we use in this study. [5]

The survey is a multipurpose household survey that covers a wide range of topics including household demographics, housing and assets, land ownership and use, crop inputs and labor use, crop production, storage and utilization, livestock ownership, sources of non-farm incomes, saving and credit, food and non-food consumption expenditures, and experience-based food (in)security measures, among others. The two survey experiments were implemented in the 2023 round data collection using a computer-assisted personal interview (CAPI) platform, which enabled us to introduce the two experimental variations in data collection within the survey instrument itself following the random assignment of households, which was implemented on Stata and preloaded onto the survey form.

As shown in Table 2, most respondents in our sample are married (87%) and live in a household headed by male (89%). The average household has 6 members and is headed by a person 51 years of age and with three years of schooling. The average farm size in our sample amounts to about 1.6 ha, while the average size of parcels owned and managed by sample households amounts to about 0.57 ha. Table 2 also shows the descriptive statistics by household's treatment status for the nudge experiment. The results from the pairwise t-test show that the two study groups are similar in terms of their household and location characteristics, indicating that the random assignment generated comparable treatment and control groups. Results of descriptive statistics by household's treatment status for the list experiment also show similar pattern. Again, the results from the pairwise t-test show that households assigned to Group A and Group B are statistically

[5] Despite the significant attrition because of the security situation, we note that almost all the attrition was triggered by village or *kebele* level inaccessibility rather than household level factors and variation. As we have stratified the random assignment at *kebele* level, we anticipate that the attrition rates are likely to be similar across control and treatment groups.

similar in terms of their basic household and location characteristics, indicating that the random assignment generated comparable treatment and control groups. We note that the observable characteristics reported in Table 2 are based on the pre-intervention (2019) survey. (Table A1 in the Appendix report similar balance table using the 2023 survey).

Table 2: Socioeconomic characteristics by treatment and control groups (round= 2023)

	Nudge				List experiment		
	Total	Treated	Control	Pairwise t-test (p-value)	Group A	Group B	Pairwise t-test (p-value)
Household size	5.89	5.86	5.92	0.36	5.90	5.88	0.78
	(2.23)	(2.24)	(2.22)		(2.21)	(2.25)	
Gender of Household head (=1 if female)	0.13	0.13	0.13	0.67	0.14	0.13	0.37
	(0.34)	(0.34)	(0.34)		(0.34)	(0.33)	
Marital status (=1 if currently married)	0.86	0.86	0.85	0.34	0.85	0.87	0.15
	(0.35)	(0.34)	(0.35)		(0.36)	(0.34)	
Age household head	50.25	50.18	50.33	0.72	50.46	50.04	0.30
	(12.80)	(12.84)	(12.77)		(12.61)	(13.00)	
Years of schooling of head	3.08	3.11	3.04	0.52	3.01	3.14	0.28
	(3.65)	(3.66)	(3.64)		(3.60)	(3.70)	
Land size (owned)	1.31	1.30	1.32	0.52	1.35	1.33	0.55
	(1.07)	(1.07)	(1.06)		(1.19)	(1.15)	
Avg. parcel size	0.48	0.49	0.48	0.31	0.51	0.51	1.00
	(0.35)	(0.36)	(0.35)		(0.43)	(0.43)	
Total number of parcels owned	2.76	2.72	2.80	0.19	2.75	2.79	0.56
	(1.96)	(1.96)	(1.96)		(1.97)	(2.01)	
Distance of *kebele* from paved road	10.34	10.23	10.44	0.65	10.30	10.37	0.88
	(14.11)	(14.06)	(14.15)		(14.09)	(14.12)	
Kebele access to electricity	0.51	0.51	0.51	0.81	0.51	0.51	0.97
	(0.50)	(0.50)	(0.50)		(0.50)	(0.50)	
Number of markets in the *kebele*	0.57	0.57	0.57	0.93	0.57	0.57	0.93
	(0.70)	(0.70)	(0.70)		(0.69)	(0.70)	
Region==Amahara	0.29	0.29	0.30	0.80	0.29	0.30	0.62
	(0.46)	(0.46)	(0.46)		(0.45)	(0.46)	
Region==Oromia	0.41	0.41	0.41	0.56	0.42	0.40	0.43
	(0.49)	(0.49)	(0.49)		(0.49)	(0.49)	
Region==SNNP	0.29	0.29	0.30	0.70	0.29	0.30	0.72
	(0.46)	(0.45)	(0.46)		(0.45)	(0.46)	
Number of households	3904	1986	1918		1964	1940	

Notes: Standard deviations in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4. Empirical estimation strategy

To identify the impact of the nudge, we exploit the random assignment of farm households (and respondents) into treatment and control groups. Although the random assignment facilitates identification of average treatment effects using simple mean differences, the availability of rich set of observable household characteristics as well as baseline data enables estimation of more structured and more powered multivariate regressions and differences-in-differences or fixed effects models. Although the treatment assignment was implemented at the household level, we can estimate the impact of the nudge both at parcel as well as household level. Thus, we start by estimating a simple linear ordinary least square (OLS) equation of the following form, which we implement at the parcel level:

$$Y_{hp} = \alpha_0 + \alpha_1 Nudge_{hp} + \alpha_2 X_{hp} + \epsilon_{hp} \qquad (1)$$

Where Y_{hp} stands for our measure of land market participation indicator associated with each household h and parcel p. These indicators stand for binary outcomes capturing renting in (out) and sharecropping-in (out) rates. For each household Y_{hp} assumes a value of 1 if parcel p is rented in (out) and sharecropped in (out) and 0 otherwise. α_0 is a constant term. $Nudge_{hp}$ is a binary indicator assuming a value of 1 for those households randomly assigned to receive the nudge and 0 for those households who were administered the survey without the nudge. X_{hp} captures observable characteristics of households and parcels as well as geographic dummies. We control only those characteristics that are unlikely to be affected by the nudge itself. ϵ_{hp} contains other unobservable factors that may explain land market participation rates. α_1 is our main parameter of interest, which captures the impact of exposure to the nudge on reporting of land market participation rates. Estimating equation (1) at the household level entails aggregating parcel-level land market participation rates into household level indicators. For this purpose, the outcome variable in equation (1) assumes a value of 1 for those households renting in (out) and sharecropping-in (out) one or more parcel(s) and 0 otherwise.

Given the availability of baseline (pre-intervention) data, we can also extend the empirical specification in equation (1) and estimate the following difference-in-differences or fixed effects model controlling for household fixed effects:

$$Y_{hpt} = \alpha_h + \beta_1 Round_t + \beta_2 Nudge_{hp} * Round_t + X_{hpt} + \varepsilon_{hpt} \qquad (2)$$

where all terms except α_h and $Round_t$ are as defined in equation (1). α_h stands for household fixed effects that capture all time-invariant differences across households. $Round_t$ stands for a binary indicator of survey round, assuming a value of 1 for the follow-up (midline) survey involving the intervention and 0 for the pre-intervention (baseline) survey. This variable $Round_t$ and hence β_1 captures aggregate changes in land market participation rates, including those driven by covariate shocks to the demand and supply side of local land markets. $Nudge_{hp}$ is defined as time-invariant treatment and hence assumes a value of 1 for those households exposed to the priming experiment and 0 otherwise. X_{hpt} capture additional time-varying characteristics of households and parcels. ε_{hpt} contains other unobservable factors that drive land market participation rates. β_2 is the difference-in-difference estimator that captures the impact of exposure to the nudge.

We note that while the expression in equation (1) exploits the cross-sectional random variation in exposure to the nudge, the difference-in-difference estimate in equation (2) combines cross-sectional and temporal variation in exposure to the nudge. Thus, comparing the two coefficients (α_1 *and* β_2) serves to probe the robustness of our results. With the objective of probing the robustness of our results, we also estimate these empirical specifications using parcel level and household-level outcomes and for each production season (*Meher* and *Belg*).[6] As the empirical specifications in equation (1) and (2) are implemented at parcel level and households own multiple parcels, this can generate correlation in errors terms associated with the same household but different parcels. Thus, we cluster standard errors at household level.

For quantifying the land market participation rates using the list experiment, we apply the following assumptions and procedures. Given the successful randomization we demonstrated in the previous section, we assume that there is no "design effect" and hence the two groups are expected to generate comparable responses to non-sensitive items.[7] Following the standard practice, we also assume that the response for each sensitive item is truthful. With these assumptions, we estimate the prevalence rate of the sensitive item (in our case renting and

[6] *Meher* refers to the primary rainy season in Ethiopia, which covers May to September, while the *Belg* (short rains) season covers February to April, which is mostly important in the south and southwestern parts of the country.

[7] We formally test this following Blair and Imai (2012) and hence by estimating the joint probabilities of all possible combination of responses to nonsensitive and sensitive items. These probabilities are reported in Table A3-A4. Almost all these probabilities are positive and hence plausible.

sharecropping-out) using a simple difference-in-means estimator (e.g., Holbrook and Krosnick, 2010; Imai, 2011; Blair and Imai, 2012; Blair et al., 2014; Tsai, 2019). To operationalize this, let us introduce the following notations: let T_h be the indicator variable assuming value of 1 if the household was assigned the long list (including the sensitive items on renting and sharecropping-out practices) and 0 otherwise. Similarly, let S_h be respondent's potential answer to the sensitive item on land rental market participation and R_h be respondent's affirmative responses to the non-sensitive items (which implies that the total number of affirmative answers can be expressed as $Y_h = S_h + R_h$). Using these notations, the land rental market participation (renting and sharecropping-out) rates can be computed using the following difference-in-means expression:

$$P(S_h = 1) = \frac{\sum_{h=1}^{n} T_h Y_h}{\sum_{h=1}^{n} T_h} - \frac{\sum_{h=1}^{n} (1-T_h) Y_h}{\sum_{h=1}^{n} (1-T_h)} \tag{3}$$

The expression in equation (3) serves to estimate prevalence rates from the single list experiment. To compute the difference-in-means estimate for the double list experiment, we follow previous practices (Droitcour et al., 1991; Glynn, 2013; Tsai, 2019) and compute an arithmetic mean of the two difference-in-means computed above as shown below.

$$P(S_h = 1) = \frac{\left[\left\{\frac{\sum_{h=1}^{n} T_h Y_h^A}{\sum_{h=1}^{n} T_h} - \frac{\sum_{h=1}^{n} (1-T_h) Y_h^A}{\sum_{h=1}^{n} (1-T_h)}\right\} + \left\{\frac{\sum_{h=1}^{n} (1-T_h) Y_h^B}{\sum_{h=1}^{n} T_h} - \frac{\sum_{h=1}^{n} T_h Y_h^B}{\sum_{h=1}^{n} (1-T_h)}\right\}\right]}{2} \tag{4}$$

The difference-in-means estimate outlined in equation (3)-(4) can be estimated using regression framework of the following type:

$$Y_h = \gamma + \delta T_h + \mu_h \tag{5}$$

Where Y_h stands for the total count of affirmative responses to all items. The difference-in-means estimator δ approximates the prevalence rate of land rental market participation, while γ offers the average affirmative responses for those respondents exposed to the nonsensitive items. μ_h captures unobservable factors that may affect our outcome variable of interest. We note that the expression in equation (5) can be extended to allow differential response rates across households (by interacting the treatment indicator with observable characteristics of respondents) as well as to capture potential differential response rates across the two groups used for the double list experiment.

5. Empirical results and discussions

Having established the validity of our randomization for the two experiments using the balancing tests reported in Table 2, we now present the empirical results on the effects of the nudge and list experiments on land market participation statistics. Looking at the descriptive results, sharecropping is the dominant form of rental transaction in our sample, accounting for roughly 65-75% of rental transactions in both years and reference seasons (Table 3 and Table A5). Although there is only a four-year gap between the survey waves, we find a reasonably strong growth in rental rates across waves, particularly for sharecropped land. For example, total rental participation rates in the *Meher* season grew from 12% to 14% for tenants (renting in or Sharecropping-in land) and from 8% to 11% for landlords (renting-out or sharecropping-out land). This amounts to annual growth rates of 4% and 9%, a very rapid increase (Table 3). While rapid growth rates in rental markets have been documented for other settings in SSA (e.g., Chamberlin and Ricker-Gilbert 2016; Deininger et al., 2017), they have not been hitherto described for Ethiopia. Land market participation rates are slightly higher in the *Meher* season than the *Belg* season (see Table A5, Panel A in the Appendix for the results based on the *Belg* production season).

Table 3: Trends in land market participation rates (*Meher season*)

Variable	Parcel level		Household level	
	2019	2023	2019	2023
Rented-out	0.01	0.01	0.02	0.03
	(0.13)	(0.12)	(0.15)	(0.18)
Sharecropped-out	0.07	0.10	0.15	0.20
	(0.43)	(0.46)	(0.36)	(0.40)
Rented-out +	0.08	0.11	0.17	0.22
sharecropped-out	(0.45)	(0.48)	(0.38)	(0.42)
Rented in	0.03	0.03	0.09	0.09
	(0.28)	(0.23)	(0.28)	(0.28)
Sharecropped-in	0.08	0.11	0.20	0.24
	(0.39)	(0.43)	(0.40)	(0.43)
Rented-in +	0.12	0.14	0.26	0.30
Sharecropped-in	(0.47)	(0.47)	(0.44)	(0.46)
No. observations	13423	14120	3296	3904

5.1 Results from nudge experiment

Using the baseline data from 2019, we first test whether the treated and control group households had statistically comparable reporting rates on land market participation before our nudge intervention and the results show no significant differences, again confirming the validity of our randomization. Coming to the 2023 round, we observe some nuanced findings: while we observe significant differences in renting-in and sharecropping-in rates, renting-out and sharecropping-out rates are statistically similar across the treated and control group households. This suggests that any systematic underreporting of renting or sharecropping-out behavior is not primarily due to a framing bias around data collection on actively farmed plots (under which we might expect lower reporting of rented-out plots). In other words, the nudge is likely ineffective in triggering meaningfully higher rates of renting or sharecropping-out. However, we do find that the nudge has a large impact on the reported rates of renting-in and sharecropping-in rates. For example, among the treated group respondents we find that 15% of parcels were rented-in or sharecropped-in for the 2023 *Meher* season, compared with 13% in the control respondents. If we take the larger value as the true rate of renting in, then this 2 percentage point difference indicates that 15% of rented/sharecropped-in parcels are not observed when the nudge is omitted (Table 4, Panel A). This is a non-trivial share of rental market participants. The similarity of the *Meher* and *Belg* results reinforces the robustness of our finding. This is a striking finding and suggests that other survey-based estimates of rental market participation rates in Ethiopia (and perhaps elsewhere in SSA) may be systematically underestimated on the tenancy side.[8]

[8] In the Appendix (Table A2) we show that this is not driven by reduction in the number of parcels owned and managed by the household. Rather, much of these results are driven by additional parcels rented in or sharecropped in which in the absence of the nudge would have remained unreported.

Table 4: Parcel and household level land market participation rates across treatment (nudge) and control households, by round (*Meher season*)

Variable	2019 Nudge	2019 Control	2019 Pairwise t-test (P-value)	2023 Nudge	2023 Control	2023 Pairwise t-test (P-value)
Panel A: Parcel level land market participation rates						
Rented-out	0.01	0.01	0.26	0.01	0.01	0.73
	(0.10)	(0.16)		(0.13)	(0.11)	
Sharecropped-out	0.07	0.07	0.64	0.10	0.10	0.45
	(0.42)	(0.43)		(0.46)	(0.46)	
Rented-out + sharecropped-out	0.08	0.08	0.44	0.11	0.11	0.53
	(0.43)	(0.46)		(0.48)	(0.49)	
Rented in	0.03	0.03	0.54	0.04	0.03	0.04^{**}
	(0.30)	(0.25)		(0.24)	(0.22)	
Sharecropped in	0.09	0.08	0.63	0.12	0.10	0.02^{**}
	(0.40)	(0.39)		(0.44)	(0.41)	
Rented in + sharecropped in	0.12	0.12	0.96	0.15	0.13	0.00^{***}
	(0.48)	(0.45)		(0.49)	(0.45)	
Number of parcels	6705	6718		7239	6881	14120
Panel B: Household level land market participation rates						
Rented-out	0.02	0.03	0.25	0.03	0.03	0.91
	(0.14)	(0.16)		(0.18)	(0.18)	
Sharecropped-out	0.16	0.15	0.66	0.20	0.20	0.73
	(0.36)	(0.36)		(0.40)	(0.40)	
Rented-out + sharecropped-out	0.17	0.17	0.95	0.22	0.22	0.88
	(0.38)	(0.38)		(0.41)	(0.42)	
Rented-in	0.08	0.09	0.18	0.10	0.07	0.01^{***}
	(0.27)	(0.29)		(0.30)	(0.26)	
Sharecropped-in	0.20	0.20	0.82	0.25	0.23	0.10^{*}
	(0.40)	(0.40)		(0.44)	(0.42)	
Rented-in + Sharecropped-in	0.26	0.27	0.61	0.32	0.28	0.01^{***}
	(0.44)	(0.44)		(0.47)	(0.45)	
No. of households	1644	1652		1986	1918	

Note: Standard deviations, clustered at household level, are given in parentheses. *Meher* refers to the primary rainy season in Ethiopia, occurring from June to September. * p<0.10, ** p<0.05, *** p<0.01.

In Panel B of Table 4, we aggregate the parcel level participation rates into household level participation rates. These participation rates show the clear mismatch between tenants and landlords. For example, only 2% of households report renting-out one or more of their parcels while about 9% of households report renting-in one or more parcels of land. These rates are similar across 2019 and 2023. The discrepancy between sharecropping-in and out rates is relatively smaller than those between renting in and out rates, partly because sharecropping practices are much more popular in Ethiopia. Similar to the parcel level results, we find that households in the control and treatment group exhibit statistically indistinguishable land market participation rates in 2019. However, in 2023 we clearly see that the nudge has significantly increased the share of households reporting renting-in and sharecropping-in one or more parcels of land. For example, households who received the nudge reported 3 percentage points higher probability of participating in land rental markets by renting-in land than the control group, which translates to about 43% increase in the share of households participating in land rental markets. The effect of the nudge on the share of households participating in sharecropping practices is relatively smaller. Combining the effects on renting-in and sharecropping-in practices, we find that the nudge increased the share of households participating in renting-in and sharecropping-in practices by about 4 percentage point (about 14 percentage change in the share of households participating in land markets). Again, consistent with the parcel level results, we find that the nudge was ineffective in improving reporting behaviors associated with renting-out and sharecropping-out practices, which highlights the limits of the nudge to uncover potentially deliberate underreporting of these practices.

In Table 5 we report the estimation results associated with equation (1), which formally and parametrically estimates the impact of exposure to the nudge.[9] Panel A provides parcel level results for renting-in or sharecropping-in rates while Panel B reports corresponding results at the household level. Panel C and D provides parcel and household level impacts of the nudge on renting-out and sharecropping-out. Odd columns report unconditional regression results while even columns report conditional regression results controlling for a list of household and parcel characteristics (those listed in Table 2). The results in the first column show that the nudge increases reporting of renting-in by about 1 percentage point, which is about 28% increase in the share of parcels rented-in. The results associated with sharecropping rates show that the nudge increased the share of sharecropped-in parcels by about 2 percentage points, which translates to about 17% increase. Focusing on the household level impacts, Panel B shows that the nudge increased the share of households participating in land markets (mainly through renting-in and sharecropping-in land) by about 4 percentage points or 13%. The results in Panel C and D of Table 5 show that the nudge is ineffective in increasing renting or sharecropping-out rates.

[9] We report similar results in the Appendix for the *Belg* season (see Table A5-A6).

Table 5: OLS estimates on the impact of nudge on land market participation (*Meher* season)

	Rented in		**Sharecropped in**		**Rented in or sharecropped in**	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parcel level						
Nudge	0.008**	0.008**	0.017**	0.017**	0.025***	0.024***
	(0.004)	(0.003)	(0.007)	(0.007)	(0.008)	(0.007)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.029	0.029	0.101	0.101	0.130	0.130
No. observations	14120	14120	14120	14120	14120	14120
Panel B: Household level						
Nudge	0.023***	0.023***	0.023*	0.021	0.039***	0.037***
	(0.009)	(0.009)	(0.014)	(0.013)	(0.015)	(0.014)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.074	0.074	0.231	0.231	0.284	0.284
No. observations	3904	3904	3904	3904	3904	3904
	Rented-out		**Sharecropped-out**		**Rented-out or sharecropped-out**	
Panel C: Parcel level						
Nudge	0.001	0.001	-0.006	-0.005	-0.005	-0.004
	(0.002)	(0.002)	(0.008)	(0.007)	(0.008)	(0.007)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.011	0.011	0.102	0.102	0.112	0.112
No. observations	14120	14120	14120	14120	14120	14120
Panel D: Household level						
Nudge	-0.001	-0.000	-0.004	-0.005	-0.002	-0.002
	(0.006)	(0.006)	(0.013)	(0.012)	(0.013)	(0.013)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.033	0.033	0.200	0.200	0.223	0.223
No. observations	3904	3904	3904	3904	3904	3904

Notes: Standard errors, clustered at household level, are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We also estimate more structured and powered difference-in-differences using the 2019 data as baseline and the 2023 data as post-treatment (Table 6). The results from this alternative approach are consistent with those in Table 5 and suggest that the magnitude of our estimates of treatment effects are not sensitive to other observed and unobserved controls. The impacts in Panel A of Table 6 show that the nudge increases reporting of land market participation, mainly through renting-in, by 2 percentage points while this increases to 4 percentage points when we consider household level participation rates in Panel B. On the other hand, Panel C and D show that the nudge is not associated with increased reporting rates for renting-out and sharecropping-out. This is perhaps not surprising given that under-reporting rates associated with renting-out and sharecropping-out may be driven by strategic and intentional motives. The results in Table 5 and

6 focus on the main (*meher*) season. We report similar results for the *Belg* season in Table A6 and A7 in the Appendix.

Table 6: Difference-in-Differences (fixed effects) estimation on the impact of nudge on probability of land market participation (meher season)

	Rented in		**Sharecropped in**		**Rented in or sharecropped in**	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parcel level results						
Nudge#Round	0.011**	0.011**	0.009	0.009	0.020**	0.020***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.008)	(0.008)
Round	-0.002*	-0.001	0.003**	0.004***	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable (baseline)	0.034	0.034	0.083	0.083	0.117	0.117
No. observations	27543	27543	27543	27543	27543	27543
Panel B: Household level results						
Nudge#Round	0.037***	0.038***	0.017	0.017	0.040**	0.040**
	(0.011)	(0.011)	(0.016)	(0.016)	(0.017)	(0.017)
Round	-0.007***	-0.005***	0.003	0.004	-0.002	0.000
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes
Mean of dep. variable (baseline)	0.092	0.092	0.204	0.204	0.269	0.269
No. observations	7200	7200	7200	7200	7200	7200
	Rented-out		**Sharecropped-out**		**Rented-out or sharecropped-out**	
Panel C: Parcel level results						
Nudge#Round	0.002	0.002	-0.004	-0.005	-0.002	-0.003
	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.008)
Round	0.001	0.000	0.007***	0.005***	0.008***	0.006***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable (baseline)	0.009	0.009	0.073	0.073	0.082	0.082
No. observations	27543	27543	27543	27543	27543	27543
Panel D: Household level results						
Nudge#Round	0.006	0.006	-0.011	-0.011	-0.002	-0.003
	(0.007)	(0.007)	(0.015)	(0.015)	(0.016)	(0.015)
Round	0.002	0.001	0.011***	0.007***	0.011***	0.008***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes
Mean of dep. variable (baseline)	0.025	0.025	0.150	0.150	0.169	0.169
No. observations	7200	7200	7200	7200	7200	7200

Notes: Standard errors, clustered at household level, are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Results from list experiment

We next report results from the list experiment, summarized in Tables 7 and 8. Our basic result is striking: the list experiment indicates much higher rates of renting or sharecropping-out land than is indicated in the parcel roster. For example, based on the results of exposure to List 2 in Table 7, 18% of the sample households actually rented-out land (for cash). Similarly, the results of exposure to List 4 in Table 7 indicate that 17% of the sample sharecrops-out land. These implied renting-out rates are relatively lower for lists 1 and 3, which is likely simply due to sub-sample variation. These renting-out and sharecropping-out rates are significantly higher than those reported in previous studies for Ethiopia (e.g., Deininger et al., 2017; Gebru et al., 2019; Abay et al., 2021).

Table 7: Descriptive results of the list experiment

Variable	No. Obs.	Total Mean/(SD)	No. Obs.	**Group B** Mean/(SD)	No. Obs.	**Group A** Mean/(SD)	**Pairwise t-test** differences
Panel A: Mean affirmative responses with and without renting-out							
List 1	3758	1.49	1870	1.55	1888	1.43	0.11***
		(0.67)		(0.72)		(0.61)	
List 2	3734	1.65	1856	1.56	1878	1.75	0.18***
		(0.89)		(0.81)		(0.95)	
Panel B: Mean affirmative responses with and without sharecropping-out							
List 3	3844	2.22	1908	2.27	1936	2.16	0.11***
		(0.85)		(0.89)		(0.82)	
List 4	3761	1.71	1869	1.63	1892	1.79	0.17***
		(0.74)		(0.66)		(0.80)	

Notes: Standard deviations are given in parentheses. The sensitive question on renting/sharecropping-out is in Group B in Lists 1 and 3, and in Group A in Lists 2 and 4. Tests of pairwise differences in the last column are based on differences calculated as mean differences in affirmative responses between those containing the sensitive item and nonsensitive items. For list 1 and 3 this stands for Group B minus Group A while these reverses for List 2 and 4. * p<0.10, ** p<0.05, *** p<0.01.

We also parametrically estimate the probability of answering the sensitive item (renting-out and sharecropping-out) affirmatively using equation (4). One way to synthesize the results from the double list experiment is to use both sets of treatment indicators in a linear difference in means estimator (e.g., Imai et al., 2015; Tsai et al., 2029). Results from this estimator, captured by $\hat{\delta}$ in Table 8, effectively show combined estimates of the true renting-out rates, averaged across the different list treatments. The resulting estimate of the share of the sample renting-out for cash is 15%. This is significantly higher than the renting-out rates derived from parcel roster responses (which is 3% for the *Meher* season as shown in Table 4). This reported renting-out rate fully explains the apparent differences in the nominal rates of renting-out and renting-in rates from the parcel roster results (Table 4). The corresponding estimate for sharecropping-out rate stands at 14%, which is not far from the rate derived from the parcel roster (Table 4). The fact that the imputed rate of renting-out for cash is meaningfully different from the reported rate (a 12-percentage point difference) suggests that the perceived downsides of disclosing cash rental activities may be relatively larger than for sharecropping-out. This suggests that the observed differences in tenancy versus landlord transactions observed elsewhere (e.g., Jin and Jayne, 2013, Chamberlin and Ricker-Gilbert, 2016; Deininger et al., 2017; Abay et al., 2021) may be entirely driven by households' self-censorship.

Table 8: Linear difference-in-means estimates of land market participation (double list experiment)

	(1) Rented-out (List 1 and 2)	(2) Sharecropped-out (List 3 and 4)
$\hat{\delta}$	0.149***	0.141***
	(0.017)	(0.015)
$\widehat{\gamma_A}$	1.430***	2.184***
	(0.014)	0.019
$\widehat{\gamma_B}$	1.562***	1.626***
	(0.019)	(0.015)
No. observations	3720	3756

Notes: These linear difference-in-means results are estimated using equation (4), implemented using the Stata package kict (Tsai, 2019). Standard errors are given in parentheses. The estimates in the first column report results associated with the double list experiment containing List 1 and 2. $\hat{\delta}$ captures probability of affirmative response to the sensitive item associated with renting-out or sharecropping-out land. $\widehat{\gamma_A}$ and $\widehat{\gamma_B}$ capture mean affirmative responses to the nonsensitive items in each group. * p<0.10, ** p<0.05, *** p<0.01.

The results in Table 7 and 8 provide average rates across different types of respondents who may have varying level of motive to report land market participation and associated land market transactions. We explore potential heterogeneities on reporting as results of the listing experiment by splitting the sample across quintiles of farm size and gender of the household head and estimate equation (4) on a split sample. Table 9 provides estimates of renting-out and sharecropping-out rates for farmers with varying level of farm size. As expected, those farmers with larger farm size are more likely to participate in renting-out and sharecropping-out a portion of their land. Among those within the fourth and fifth quintile 18% to 19% of them are participating in land rental markets by renting-out land while the corresponding rate among those in the first quintile amount to 9% (Table 9, Panel A). Panel B of Table 9 show similar rates for sharecropping-out land. Below each panel, we also report renting-out and sharecropping-out rates derived from the self-reported parcel roster. This can facilitate comparison between the two values to evaluate whether the discrepancies between the two estimates vary across households with varying farm size. The relatively higher renting-out or sharecropping-out rates are consistent with the notion that land market participation in many parts of Africa entail transfer of land from land-abundant to landless households (e.g., Jin and Jayne, 2013; Deininger et al., 2009; Ghebru and Holden 2019; Chamberlin and Ricker-Gilbert, 2016). Interestingly, comparing the renting-out rates computed using the list experiment and the self-reported rates from the parcel roster suggests that farmers with varying farm size may exhibit differential underreporting rates, suggesting that such patterns may generate nonclassical measurement error with important inferential consequences. For example, farmers in the first quintile underreport the "true" renting out rate (elicited through the list experiment) by more than 80% while this goes down to about 70% for those farmers in the last quintile.

Table 9: Linear difference-in-means estimates of land market participation, by land size quintiles

	Q1	Q2	Q3	Q4	Q5
Panel A: Rented-out					
$\hat{\delta}$	0.090**	0.131***	0.143***	0.178***	0.201***
	(0.039)	(0.035)	(0.038)	(0.038)	(0.042)
Self-reported rate	0.016	0.021	0.030	0.034	0.066
No. observations	738	766	728	746	742
Panel B: Sharecropped-out					
$\hat{\delta}$	0.077**	0.130***	0.174***	0.115***	0.207***
	(0.034)	(0.032)	(0.035)	(0.033)	(0.034)
Self-reported rate	0.077	0.182	0.188	0.228	0.338
No. observations	745	774	734	755	748

Notes: These linear difference-in-means results are estimated using equation (4), implemented using the Stata package kict (Tsai, 2019). $\hat{\delta}$ captures probability of affirmative response to the sensitive item associated with renting-out or sharecropping-out land. The self-reported rate come from parcel roster and associated land market participation rates reported by households. * p<0.10, ** p<0.05, *** p<0.01.

6. Conclusions

Given the importance of rural land rental markets in Sub-Saharan Africa (SSA), critical attention to the fidelity of land market participation data is warranted. Our study was motivated specifically by the persistent imbalance in reported rates of renting in versus renting-out farmland in rural household survey data from SSA, wherein reported rates of renting-in (i.e., being a tenant) are consistently higher than reported rates of renting-out (i.e., being a landlord) in the same sample. We set out to inquire to what extent these imbalances may be the result of either (a) systematic omissions of rented-out plots in survey plot rosters due to failure to emphasize the importance of including all plots (and not just actively cultivated/managed parcels), or (b) systematic reluctance to disclose renting-out plots in socio-institutional settings where respondents may perceive potential adverse consequences to disclosing such practice. We address these questions with a randomized nudge (for case (a)) and a double list experiment (for case (b)).

Our results indicate that rates of land market participation by landlords (i.e., the share of respondents reporting renting-out or sharecropping-out land) was not sensitive to nudges, suggesting that unintentional omission of renting-out land was not an underlying reason for the imbalance. However, our list experiment results indicate much higher revealed rates of market participation by landlords than the rates derived from parcel roster responses, the standard way of defining rates of renting-out farmland in rural land market studies. For example, revealed renting out rates are 15% while the corresponding rate coming from the self-reported parcel rosters amount only 3%. This result fully explains the difference in the nominal rates of tenancy and landlords, as derived from parcel roster responses. Our results suggest that the tenant/landlord discrepancy widely observed elsewhere in SSA (e.g., Jin and Jayne 2013, Chamberlin and Ricker-Gilbert 2016; Deininger et al. 2017; Abay et al. 2021) may be entirely attributed to systematic reluctance to fully disclose renting-out behavior.

Another interesting finding is that the nudge experiment, while having no significant effect on reported rates of renting-out, had a large and statistically significant effect on reported rates of renting-in and sharecropping-in. More specifically, priming (nudging) increased the reported share of households participating in land markets as tenants by 4 percentage points (13% increase). At parcel level, it leads to a 2 percentage points (15%) increase in reporting rates of rented-in and sharecropped-in parcels. This implies that 15% of rented-in or sharecropped-in parcels will not be observed when the nudge is omitted, amounting to a non-trivial share of land market participation on the tenant side. These results suggest that the literature on rural land rental market development in SSA, which has emphasized the strong recent growth of such markets, has likely *underestimated* the actual levels of rental market participation by tenants for the time periods observed. This result has relevance for our understanding of both the current extent of land market development in the region as well as efforts to measure its impacts.

While our results are strongly indicative of more widespread measurement issues in survey-derived land rental participation rates, it is important to note that these results come from a single country. Replication of these methods in other contexts would be useful to establish the external validity of our analytical conclusions.

This caveat notwithstanding, our results do suggest that policies related to land market development should be cautious in interpreting empirical assessments of participation rates, and estimates of impacts, because such estimates are likely based on partial view of rental market participants. If the probability of not being observed as a landlord (or tenant) is not random as we show in some of our results, then estimates of the costs and benefits of rental market participation may be biased. Future empirical work will hopefully clarify to what extent this may be the case.

One implication of our work that seems very clear is the value of building nudges for rented in/out land into survey questionnaires just prior to entering parcel-roster modules. Further experimental work that evaluates alternative nudge designs would be useful. Addressing the issue of respondent reluctance to disclose renting-out activity, however, may require novel elicitation techniques that can attenuate the perceived negative consequences of disclosing renting-out land. Moreover, there may be some utility in more emphatic reminders that may help to address confidentiality concerns. Testing such data collection and survey design innovations would be another area that merits further experimental work. Including a measure of rural land ownership in urban household surveys can also shade light on the extent of absentee landlords. Ultimately, a proper land registry or cadaster with owners and tenants would provide a clear picture of land market participation rates and allow a comprehensive analysis of its welfare impacts.

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Appendix: Supplementary results

Table A1: Socioeconomic characteristics by treatment groups (round= 2019)

		Nudge			List experiment		
	Total	**Treated**	**Control**	**Pairwise t-test**	**Group A**	**Group B**	**Pairwise t-test**
Household size	5.86	5.82	5.90	0.29	5.91	5.81	0.20
	(2.22)	(2.19)	(2.25)		(2.17)	(2.27)	
Gender of Household head (=1 if female)	0.11	0.11	0.11	0.86	0.12	0.10	0.05*
	(0.31)	(0.31)	(0.31)		(0.32)	(0.30)	
Marital status (=1 if married)	0.89	0.89	0.88	0.66	0.88	0.90	0.10
	(0.32)	(0.31)	(0.32)		(0.33)	(0.30)	
Age household head	48.25	48.26	48.23	0.93	48.40	48.10	0.51
	(13.08)	(13.05)	(13.12)		(12.75)	(13.41)	
Years of schooling of head	2.70	2.70	2.70	0.95	2.69	2.71	0.88
	(3.45)	(3.39)	(3.50)		(3.42)	(3.48)	
Land size (owned)	1.37	1.38	1.36	0.61	1.42	1.39	0.53
	(1.08)	(1.07)	(1.09)		(1.24)	(1.16)	
Average parcel size	0.46	0.47	0.45	0.13	0.48	0.49	0.69
	(0.34)	(0.34)	(0.34)		(0.39)	(0.40)	
Total number of parcels owned	3.24	3.24	3.23	0.95	3.31	3.28	0.78
	(2.32)	(2.30)	(2.34)		(2.51)	(2.51)	
Distance of kebele from paved road	7.84	7.95	7.72	0.67	7.76	7.91	0.77
	(13.94)	(14.04)	(13.84)		(13.84)	(14.04)	
Kebele has access to electricity	0.46	0.45	0.46	0.69	0.45	0.46	0.85
	(0.50)	(0.50)	(0.50)		(0.50)	(0.50)	
Number of markets in the kebele	0.45	0.45	0.45	0.91	0.46	0.45	0.69
	(0.60)	(0.60)	(0.61)		(0.61)	(0.60)	
Region==Amhara	0.30	0.30	0.30	0.98	0.30	0.30	0.62
	(0.46)	(0.46)	(0.46)		(0.46)	(0.46)	
Region==Oromia	0.37	0.37	0.37	0.83	0.37	0.37	0.85
	(0.48)	(0.48)	(0.48)		(0.48)	(0.48)	
Region==SNNP	0.33	0.33	0.33	0.81	0.33	0.33	0.77
	(0.47)	(0.47)	(0.47)		(0.47)	(0.47)	
Number of households	3296	1644	1652	3296	1645	1651	3296

Note: Standard deviations are given in parenthesis. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A2: Number of parcels reported by treatment and control groups

Variable	2019 Full sample	Treated Mean/(SD)	Control Mean/(SD)	Pairwise t-test (P-value)	2023 Full sample	Treated Mean/(SD)	Control Mean/(SD)	Pairwise t-test (P-value)
Total number of parcels owned	3.24	3.24	3.23	0.95	2.76	2.72	2.80	0.19
	(2.32)	(2.30)	(2.34)		(1.96)	(1.96)	(1.96)	
No. parcels rented-out (*Meher*)	0.01	0.01	0.02	0.18	0.01	0.01	0.01	0.92
	(0.21)	(0.12)	(0.27)		(0.14)	(0.15)	(0.14)	
No. parcels sharecropped-out (*Meher*)	0.17	0.16	0.17	0.62	0.20	0.20	0.21	0.50
	(0.75)	(0.74)	(0.77)		(0.76)	(0.77)	(0.76)	
No. parcels rented-out + sharecropped-out (*Meher*)	0.19	0.17	0.20	0.33	0.23	0.22	0.24	0.40
	(0.80)	(0.76)	(0.84)		(0.81)	(0.81)	(0.82)	
No. parcels rented in (*Meher*)	0.05	0.04	0.06	0.09*	0.05	0.05	0.05	0.83
	(0.37)	(0.33)	(0.40)		(0.33)	(0.31)	(0.35)	
No. parcels Sharecropped-in (*Meher*)	0.14	0.15	0.13	0.36	0.18	0.20	0.17	0.19
	(0.61)	(0.62)	(0.59)		(0.68)	(0.71)	(0.65)	
No. parcels rented in + Sharecropped-in (*Meher*)	0.21	0.20	0.21	0.83	0.25	0.27	0.24	0.23
	(0.77)	(0.76)	(0.77)		(0.80)	(0.82)	(0.78)	
No. parcels rented-out (*Belg*)	0.01	0.01	0.01	0.26	0.01	0.01	0.01	0.75
	(0.20)	(0.11)	(0.26)		(0.12)	(0.13)	(0.11)	
No. parcels sharecropped-out (*Belg*)	0.11	0.11	0.11	0.98	0.14	0.14	0.14	0.97
	(0.63)	(0.64)	(0.62)		(0.65)	(0.66)	(0.63)	
No. parcels rented-out + sharecropped-out (*Belg*)	0.13	0.12	0.13	0.75	0.15	0.15	0.15	0.99
	(0.67)	(0.66)	(0.67)		(0.67)	(0.68)	(0.65)	
No. parcels rented in (*Belg*)	0.03	0.02	0.05	0.04**	0.03	0.03	0.03	0.50
	(0.29)	(0.23)	(0.34)		(0.28)	(0.26)	(0.30)	
No. parcels Sharecropped-in (*Belg*)	0.09	0.10	0.08	0.16	0.09	0.10	0.08	0.33
	(0.50)	(0.52)	(0.48)		(0.46)	(0.49)	(0.43)	
No. parcels rented in + Sharecropped-in (*Belg*)	0.14	0.14	0.14	0.89	0.13	0.14	0.11	0.11
	(0.64)	(0.63)	(0.65)		(0.57)	(0.61)	(0.53)	

Notes: Standard deviations in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3: Design effect list experiments 1 and 2 (rented-out)

	List Experiment 1				List Experiment 2			
	Coef	Robust SE	z	P>z	Coef	Robust SE	z	P>z
Pr(R=0,S=1)	0.001	0.002	0.518	0.698	0.008	0.008	0.975	0.835
Pr(R=0,S=0)	0.003	0.001	2.453	0.993	0.058	0.005	10.757	1.000
Pr(R=1,S=1)	0.054	0.016	3.369	1.000	0.075	0.016	4.583	1.000
Pr(R=1,S=0)	0.561	0.012	48.527	1.000	0.374	0.013	29.187	1.000
Pr(R=2,S=1)	0.042	0.009	4.808	1.000	0.043	0.012	3.515	1.000
Pr(R=2,S=0)	0.283	0.013	21.569	1.000	0.300	0.015	20.471	1.000
Pr(R=3,S=1)	0.016	0.003	5.522	1.000	0.058	0.005	10.757	1.000
Pr(R=3,S=0)	0.040	0.006	6.637	1.000	0.085	0.010	8.691	1.000
Test for design effects (with GMS)								
Ha: Pr<0	K	Lambda	P>Lambda	#P>Lambda	K	Lambda	P>Lambda	#P>Lambda
Pr(R ,S=0)	0	0.000	1.000	1.000	0	0.000	1.000	1.000
Pr(R ,S=1)	0	0.000	1.000	1.000	0	0.000	1.000	1.000

Table A4: Design effect list experiments 3 and 4 (sharecropped-out)

	List Experiment 3				List Experiment 4			
	Coef	Robust SE	z	P>z	Coef	Robust SE	z	P>z
Pr(R=0,S=1)	0.002	0.001	1.401	0.919	-0.005	0.003	-1.804	0.036
Pr(R=0,S=0)	0.001	0.001	1.415	0.921	0.011	0.002	4.496	1.000
Pr(R=1,S=1)	0.033	0.014	2.418	0.992	0.066	0.016	4.090	1.000
Pr(R=1,S=0)	0.223	0.010	23.072	1.000	0.390	0.011	34.312	1.000
Pr(R=2,S=1)	0.009	0.016	0.552	0.710	0.079	0.011	7.158	1.000
Pr(R=2,S=0)	0.310	0.015	20.572	1.000	0.366	0.014	25.391	1.000
Pr(R=3,S=1)	0.069	0.006	11.860	1.000	0.028	0.004	7.384	1.000
Pr(R=3,S=0)	0.353	0.013	27.941	1.000	0.066	0.008	8.486	1.000
Test for design effects (with GMS)								
Ha: Pr<0	K	Lambda	P>Lambda	#P>Lambda	K	Lambda	P>Lambda	#P>Lambda
Pr(R ,S=0)	0	0.000	1.000	1.000	0	0.000	1.000	1.000
Pr(R ,S=1)	0	0.000	1.000	1.000	1	3.254	0.036	0.071

Note: # Bonferroni-adjusted p-values.

Table A5: Parcel and household level land market participation rates across treatment and control households, by round (*Belg season*)

	2019				2023			
Variable	**Full sample**	**Treated group**	**Control group**	**Pairwise t-test** (P-value)	**Full sample**	**Treated Group**	**Control group**	**Pairwise t-test** (P-value)
Panel A: Parcel level land market participation rates								
Rented-out	0.01	0.01	0.01	0.56	0.01	0.01	0.01	0.23
	(0.12)	(0.09)	(0.14)		(0.10)	(0.11)	(0.09)	
Sharecropped-out	0.05	0.05	0.05	0.82	0.06	0.06	0.06	0.81
	(0.36)	(0.36)	(0.35)		(0.37)	(0.38)	(0.36)	
Rented-out + sharecropped-out	0.05	0.05	0.05	0.97	0.07	0.07	0.07	0.59
	(0.38)	(0.38)	(0.38)		(0.39)	(0.40)	(0.38)	
Rented in	0.02	0.02	0.02	0.53	0.02	0.02	0.02	0.01**
	(0.25)	(0.27)	(0.22)		(0.20)	(0.21)	(0.19)	
Sharecropped-in	0.06	0.06	0.05	0.31	0.06	0.06	0.05	0.05**
	(0.34)	(0.34)	(0.33)		(0.32)	(0.34)	(0.30)	
Rented in + sharecropped in	0.08	0.08	0.08	0.65	0.08	0.09	0.07	0.00***
	(0.42)	(0.43)	(0.40)		(0.38)	(0.40)	(0.35)	
Panel B: Household level land market participation rates								
Rented-out	0.02	0.02	0.02	0.52	0.02	0.03	0.02	0.45
	(0.13)	(0.12)	(0.14)		(0.15)	(0.16)	(0.15)	
Sharecropped-out	0.10	0.10	0.10	0.46	0.13	0.13	0.12	0.61
	(0.30)	(0.30)	(0.29)		(0.33)	(0.33)	(0.33)	
Rented-out + Sharecropped-out	0.11	0.11	0.11	0.70	0.14	0.15	0.14	0.36
	(0.32)	(0.32)	(0.31)		(0.35)	(0.36)	(0.35)	
Rented in	0.06	0.06	0.07	0.26	0.05	0.06	0.04	0.00***
	(0.24)	(0.23)	(0.25)		(0.22)	(0.24)	(0.20)	
Sharecropped-in	0.14	0.14	0.13	0.45	0.13	0.14	0.12	0.02**
	(0.35)	(0.35)	(0.34)		(0.34)	(0.35)	(0.32)	
Rented in + Sharecropped-in	0.18	0.19	0.18	0.91	0.17	0.19	0.15	0.00***
	(0.39)	(0.39)	(0.39)		(0.38)	(0.39)	(0.36)	
Observations (parcels).	13423	6705	6718		14120	7239	6881	
No. of households	3296	1644	1652		3904	1986	1918	

Note: Standard deviations, clustered at household level, are given in parentheses. *Meher* refers to the primary rainy season in Ethiopia, occurring from June to September. The *belg* (short rains) season occurs from February to May. * p<0.10, ** p<0.05, *** p<0.01.

Table A6: OLS estimates on the impact of nudge on land market participation (*Belg*)

	Rented in		Sharecropped in		Rented in or sharecropped in	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parcel level						
Nudge	0.008**	0.008**	0.011**	0.011**	0.019***	0.019***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.006)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.016	0.016	0.051	0.051	0.068	0.068
No. observations	14120	14120	14120	14120	14120	14120
Panel B: Household level						
Nudge	0.022***	0.022***	0.025**	0.024**	0.042***	0.042***
	(0.007)	(0.007)	(0.011)	(0.010)	(0.012)	(0.012)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.040	0.040	0.119	0.119	0.150	0.150
No. observations	3904	3904	3904	3904	3904	3904
	Rented-out		**Sharecropped-out**		**Rented-out or sharecropped-out**	
Panel C: Parcel level						
Nudge	0.002	0.002	0.002	0.002	0.004	0.004
	(0.002)	(0.002)	(0.006)	(0.006)	(0.007)	(0.006)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.007	0.007	0.059	0.059	0.066	0.066
No. observations	14120	14120	14120	14120	14120	14120
Panel D: Household level						
Nudge	0.004	0.004	0.005	0.006	0.010	0.011
	(0.005)	(0.005)	(0.011)	(0.010)	(0.011)	(0.011)
Household and parcel controls	No	Yes	No	Yes	No	Yes
Mean of dep. variable (control)	0.022	0.022	0.123	0.123	0.139	0.139
No. observations	3904	3904	3904	3904	3904	3904

Note: standard errors, clustered at household level, are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Difference-in-Differences (fixed effects) estimation on the impact of nudge on probability of land market participation (*Belg*)

	Rented in		**Sharecropped in**		**Rented in or sharecropped in**	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parcel level results						
Nudge#Round	0.011^{**}	0.011^{**}	0.000	0.000	0.011	0.011
	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)	(0.008)
Round	-0.002^{***}	-0.002^{***}	-0.000	0.000	-0.003^{**}	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable (baseline)	0.025	0.025	0.054	0.054	0.079	0.079
No. observations	27543	27543	27543	27543	27543	27543
Panel B: Household level results						
Nudge#Round	0.027^{***}	0.028^{***}	0.007	0.007	0.034^{**}	0.034^{**}
	(0.010)	(0.010)	(0.015)	(0.015)	(0.016)	(0.016)
Round	-0.007^{***}	-0.006^{***}	-0.004	-0.003	-0.009^{***}	-0.008^{***}
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes
Mean of dep. variable (baseline)	0.067	0.067	0.135	0.135	0.184	0.184
No. observations	7200	7200	7200	7200	7200	7200
	Rented-out		**Sharecropped-out**		**Rented-out or sharecropped-out**	
Panel C: Parcel level results						
Nudge#Round	0.003	0.003	-0.001	-0.001	0.002	0.001
	(0.002)	(0.002)	(0.008)	(0.008)	(0.008)	(0.008)
Round	0.000	-0.000	0.004^{***}	0.003^{*}	0.004^{***}	0.003
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable (baseline)	0.007	0.007	0.046	0.046	0.053	0.053
No. observations	27543	27543	27543	27543	27543	27543
Panel D: Household level results						
Nudge#Round	0.008	0.008	0.002	0.001	0.011	0.011
	(0.006)	(0.006)	(0.014)	(0.014)	(0.015)	(0.015)
Round	0.001	0.000	0.007^{***}	0.004	0.008^{***}	0.005^{*}
	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
Household/parcel controls	No	Yes	No	Yes	No	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes
Mean of dep. variable (baseline)	0.019	0.019	0.096	0.096	0.111	0.111
No. observations	7200	7200	7200	7200	7200	7200

Notes: standard errors, clustered at household level, are given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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