

The Price of Empowerment: Experimental Evidence on the Demand for Land Titles and Female Co-Titling in Urban Tanzania

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August 2013[§]

Abstract

While multiple studies have shown that a woman's control over land is positively associated with bargaining power outcomes, few have succeeded in highlighting successful methods for increasing this control. We report on a policy experiment in an unplanned settlement in Dar es Salaam, Tanzania, that provided access to formal land titles to informal settlers at randomized prices, with additional price discounts conditional on designating a woman as owner or co-owner of the land in question. Results show that the household's are highly responsive to price incentives, as households offered a small conditional discount are roughly 30% more likely to co-title their land. Despite these large differences, households offered conditional discounts are just as likely to purchase a formal land title as those offered general discounts. We discuss the implications of these results for the expected bargaining power impacts of the intervention.

Keywords: Land tenure, Tanzania, Gender, Intra-household bargaining

JEL classification: P14, Q15, D10, J16

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[§]We gratefully acknowledge funding from the Department for International Development, UK government, through the International Growth Centre (IGC) (<http://www.theigc.org/>), specifically via the IGC Research Award "Economic Impact of Urban Property Rights and Tanzania: The Role of Infrastructure" led by the Department of International Development, University of Oxford.

1 Introduction

Land tenure formalization is frequently recommended as a policy tool for developing country governments to spur savings and investment by the poor, and extend access to credit by unlocking collateral. But these benefits are offset by concerns that formalization will undermine traditional land rights, particularly among women and the poor. Rather than a simple increase in tenure security, collateralizability, or transferrability, formalization must also be seen as a shift to a government-administered, individual freehold land tenure system, from a starting point of a decentralized system of communal or family-based rights, which often make complex and subtle distinctions between rights of sale and rights of use for various purposes.

Across sub-Saharan Africa, one stubbornly persistent sign of gender inequality is the low rate of land ownership among women. The Food and Agriculture Organization's (FAO) Gender and Land Rights database reveals the dire position most women face in this regard: of the countries surveyed women made up, on average, no more than 20% of all landowners.¹

Meanwhile, as large-scale titling programmes continue to rise in popularity, researchers have primarily been concerned with identifying the aggregate effects these schemes have on households. However, significantly less attention has been given to the *intra*household effects of land titling and similar property rights interventions, particularly their impact on women's asset ownership and marital bargaining power. This is a growing concern, as land titling programmes often produce female inclusion rates which are lower than the desired level, despite many schemes making joint-titling between husband and wife a requirement (Deere and León 2001; Payne, Durand-Lasserve, and Rakodi 2007). If women are not being included as owners during the titling process, it is possible that these programmes might at best maintain an already unfavourable status quo and at worst strip women of the informal, customary claims to land that they might already enjoy.

This paper presents preliminary results from a unique experiment in the unplanned settlements of Dar es Salaam, a context in which formal land titles have only been available for a short time and self-reported *de facto* female ownership is quite low.² The main goal of the experiment was to drastically reduce the cost of obtaining a land title for a randomly-chosen group of land-owning households. However, the experiment also had a second level of randomization, where a lottery system was used to introduce further random variation in the price that households faced to buy a land title, with some households being assigned discounts which were made conditional on including a woman as owner on the title application.

¹Authors' calculations. The FAO's database only covers a select group of countries, with data ranging from the early 1990s to the early 2000s.

²Only 13% of dual-headed households in our sample report a woman as being an owner of their land, with less than 50% reporting that a woman must be consulted in the event of sale, transfer or rental.

Using data on which households went on to purchase a land title and whether or not they included a woman in the application, we show that not only do vouchers have a positive impact on purchase of land titles, but households receiving conditional subsidies are just as likely to purchase as those receiving unconditional subsidies, indicating that conditionality does not depress demand. We go on to show that, for those households purchasing a land title, receiving a conditional subsidy substantially and significantly increases the probability that a woman’s name is included on the title. The overall result is that offering conditional discounts will increase, in net terms, the number of women listed as landowners. While these results are encouraging, the fact that households are so easily nudged into co-titling³ raises concerns that they might not be treating the decision as if it has had significant implications for household bargaining power. To investigate this further, we investigate whether voucher assignments are more or less effective in households with higher levels of ex-ante bargaining power, as measured using baseline household characteristics.

To our knowledge, this is the first research to introduce randomized variation in women’s access to property. It shows that not only are these interventions relatively easy to design and implement, but that they can have substantial effects on women’s legal claims to ownership. This paper is structured as follows: in Section 2, we discuss the motivation for such an experiment by drawing on existing evidence for gender and bargaining power impacts of property rights and land titling interventions. This section also covers the Tanzanian context, where recently-introduced land tenure reforms have created an opportunity for the intrahousehold status quo to change substantially. In Section 3, we discuss the experiment in more detail, specifically the conditionality of the vouchers, balance, and household characteristics at baseline. Section 4 covers the main results on demand for title, co-titling, and discusses the implications for effectiveness of gender conditionality. Finally, we conclude the paper with Section 5.

2 Background and motivation

2.1 Titling, land ownership and bargaining power outcomes

While evidence of the impact of formal joint-titling on women’s outcomes is limited, there are several studies which associate improvements in women’s property rights with other desirable outcomes such as measures of female empowerment, child health, education and women’s welfare, all of which are associated with increases in bargaining power. For example, self-reported ownership of land is positively correlated with child health status and various measures of empowerment in Nepal (Allendorf 2007) and with expenditure on ‘gendered’ goods in both China and Ghana (Wang 2011; Doss 2005). Inheritance rights, in

³For the remainder of the paper, we will use ‘co-titling’ to indicate any situation where a woman is included on a land title.

particular, appear to matter: Peterman (2011) shows that women in rural Tanzania who enjoy improvements in inheritance rights are more likely to enter the labour market and earn higher wages. Telalagic (2012) shows that women from villages practicing matrilineal descent, whose improved inheritance rights result in a better outside-option, are less likely to utilise domestic labour as a source of bargaining power. Both Roy (2008) and Deininger, Goyal, and Nagarajan (2010) have found a positive impact stemming from India’s Hindu Succession Act, which extended inheritance rights to women, on outcomes such as female education and self-reported autonomy.

There is also growing evidence that formal land titling itself can be advantageous to women, irrespective of their state of ownership. Using data from a Peruvian titling programme with a distinct focus on joint-titling, Field (2003) demonstrated a link between title acquisition and subsequent reduction in household fertility. Galiani and Schargrodsky (2010) show that titling in Buenos Aires resulted in a reduction in household size and higher levels of child education. Preliminary evidence from Rwanda has also shown that titling programmes can be successful at increasing perceived female ownership and the recording of inheritance rights (Ayalew Ali and Goldstein 2011).

While it is clear that land titling has the capacity to improve the lot of women in developing countries, most studies are unable to distinguish the overall impact of titling from the *additional* impact of joint-titling (what we will call co-titling in this paper). This distinction might seem less crucial in contexts where land titling is compulsory, but in the face of large costs for formalisation governments are often resorting to demand-driven approaches (Payne et al. 2007). In these settings, if households see co-titling as a cost, then policymakers might find that convincing households to purchase property titles and getting them to co-title are conflicting goals. If making co-titling a requirement depresses a household’s demand for a title, we should be concerned with identifying the ‘price of empowerment’, the subsidy required to offset that reduction in demand.

2.2 Female land ownership in urban Tanzania

To get a sense of the current state of women’s *de jure* ownership in Tanzania, we return to the 1999 Land Act, which has previously been hailed as being one of the first pieces of land legislation to explicitly recognise the rights of women as landowners (Sundet 2005). The Land Act also established two forms of urban land tenure, a short term lease known as a residential license, and a longer-term title known as a certificate of right of occupancy (CRO). The Land Act has several provisions relating directly to both the default ownership status and the rights of spouses. The default ownership state for spouses is known as occupancy-in-common, which provides each spouse with an equal share which can be sold (conditional on the agreement of the other shareholder) or left as inheritance. Legally, this implies that each ‘occupier’ or owner has substantial control over the land: one occupier will be unable to sell, rent or mortgage the property without

consent of the other.

This would suggest that women actually have significant legal control over land, yet the Land Act suggests that spouses of landowners might not be considered to be occupiers-in-common if they are not listed as an owner on a CRO:

“Where a spouse obtains land under a right of occupancy for the co-occupation and use of both spouses or where there is more than one wife, there shall be a presumption that, unless a provision in the certificate of occupancy or certificate of customary occupancy clearly states that one spouse is taking the right of occupancy in his or her name only . . . the spouses will hold the land as occupiers in common and, unless the Presumption is rebutted in the manner stated in this subsection, the Registrar shall register the spouses as joint occupiers accordingly.”

The law appears to be ambiguous enough to allow landowners to register land in their name only, excluding their spouses as legal occupiers. In practice this appears to be quite common: Table 1 shows the estimated gender breakdown of ownership of land, first from the Kinondoni property registrar, then for the subset of households which elected to purchase a residential license. In both columns, the majority (72-75%) of the listed owners are male, a result in line with estimates by Tanzanian scholars (Kironde 2006).⁴ It then appears that being included on a being included on a CRO application is a crucial, if difficult step to cementing legal ownership.

The Land Act does contain a few provisions which are intended to provided spouses with some basic rights over land, irrespective of their inclusion in formal documents. Subsequent provisions, listed in Table 10 in the Appendix, allow for spouses who have invested in the land to be considered co-occupiers by default. There are also provisions which stipulate that owners who decide to sell or mortgage the land must first obtain the consent of their spouses. It is unknown whether or not these particular provisions are actually enforced or upheld during conflicts over ownership.

While the *de jure* state of women’s land rights seems particularly precarious, what of women’s *de facto* ownership of land? Figure 2 gives a sense of the state of *de facto* ownership: it is constructed using baseline data from the experimental intervention, which is discussed in more detail in the following section. Households in two unplanned settlements in Dar es Salaam were asked a series of questions about the *de facto* ownership of land, including the rights of household members over the sale, rental and transfer of land, as well as who would be include in a CRO application if one was made. The results, which are restricted to dual-headed households, suggest that women have limited *de facto*

⁴The Kinondoni Municipality does not record the gender of the landowner. To estimate the proportion of male landowners, household data from the Tanzanian Land Rights Survey was used to identify a series of forenames which were unique to each gender (approximately 90% of all names from the roster were gender-unique). These ‘gendered’ names were then merged with the property register, of which 77% of all owner names could be identified using a gender-specific name.

Table 1: Gender distribution of property registration

	Property register	RL Database
Male owner listed	31,493 (75%)	13,545 (72%)
Female owner listed	10,329 (25%)	5,147 (28%)
Total	41,822 (100%)	18,792 (100%)

Note: Author’s calculations based on land registry maintained by Kinondoni Municipality.

Table 2: Female land ownership in Dar es Salaam

Variable	Mean	(Std. Dev.)	Min.	Max.	N
One of default owners is female	0.132	(0.339)	0	1	606
Woman has rights over land sale	0.449	(0.498)	0	1	602
Woman has rights over transfer	0.437	(0.496)	0	1	602
Woman has rights over rental	0.42	(0.494)	0	1	602
Household would include woman on CRO	0.253	(0.435)	0	1	600

Notes: data are from Tanzanian Land Rights survey. Sample restricted to dual-headed households in treatment blocks.

rights over land: roughly 13% of households report that a woman is one of the “default” owners of the land. Despite this, women fare better in ‘use’ rights, with just over 40% of households reporting that at least one woman in the household must agree before the land can be sold, transferred or rented out.⁵ Finally, when households were asked who would be included on a CRO, only 25% indicated that a woman would be included. While this is certainly better than the de facto state, it reinforces concerns that titling may help cement a status quo in which women are excluded from owning land.

3 The experiment and baseline data collection

The setting for the main experiment is Kinondoni, one of the three municipalities constituting Dar es Salaam. We focus on two adjacent communities: Mburahati Barafu and Kigogo Kati are unplanned, informal settlements with markedly low levels of access to infrastructure and public utilities, even by the relatively low benchmark set by other communities in the municipality. Basic parcel characteristics from the municipal database are presented in table 3 alongside average characteristics for all of Kinondoni. Both of these *mitaa* also appear to have noticeably lower levels of female land ownership: investigating the gender breakdown of land ownership in the Kinondoni land registry reveals that Barafu and Kati have female ownership rates of 17% and 22% respectively, compared to the municipal average of 25%.

⁵To avoid priming, households were not asked directly about female ownership. Instead, they were asked to list all members of the household that were default owners, must be consulted before a sale, or would be included on a CRO.

The main purpose of the experiment was to induce households in both communities to purchase certificates of right of occupancy (CROs), in order to subsequently study their impact. This involved several levels of randomization:

1. **Cadastral survey and repayment programme:** blocks of land parcels were identified and randomly selected into treatment and control groups. All parcels in treatment blocks were subject to cadastral surveying, with residents given the option to repay the heavily-subsidized cost (100,000 TSh) in exchange for a land title, drastically bringing down the cost of a CRO for residents.
2. **Random price variation within treatment blocks:** households within treatment blocks were randomly allocated vouchers redeemable for different levels of discount on the final price of a CRO.
3. **Random voucher conditionality:** roughly half of these vouchers were made conditional, where households were only allowed use them if a female household member was included as an owner on the CRO application.

Next, we will discuss these interventions in more detail, including the timing of their introduction in both communities.

3.1 Main intervention and voucher distribution

In the summer of 2010, prior to the intervention, the University of Oxford conducted a complete census of land parcels in Barafu and Kati, known as the Tanzanian Land Rights Survey (TLRS). Households were identified using records and maps from the Kinondoni Municipality, which had created a listing of all households in the area to assist with the creation of the land registry. Using this listing, parcel-owning households were identified and interviewed,⁶ resulting in detailed data on household and parcel characteristics.

Following this survey, a ward-level meeting was held by a local NGO, the Women's Advancement Trust (WAT), to explain the overall intervention and process of selection into treatment and control blocks. Using a town plan recently drawn up as a prerequisite for CRO distribution, we then divided land parcels into 'blocks' (contiguous groups of parcels), randomly assigning half of these into treatment and control groups.⁷ All parcels in treatment blocks were subject to a cadastral survey and owning households were invited to participate in the programme to obtain a land title, which required them to repay the cost of 100,000 TSh over roughly a six month period.

The second and third dimensions of the intervention were cross-cutting and randomized at the individual parcel level within treatment blocks. After treatment parcels were selected, owners were to be given up to two types of discounts on the price of a CRO,

⁶The survey team was agnostic about *which* household member was the actual member. The only condition for interview was that one of the household members was considered to be the owner.

⁷For Barafu, the total number of blocks was 10, for Kati it was 15.

Table 3: Summary Statistics on Parcel Characteristics

	Kinondoni Municipality	Kigogo Kati	Mburahati Barafu
Formal employment	49.9%	44.6%	44.3%
Size and Value of Property			
Area in square meters	439	264	247
Property value in '000 TSh.	12,562	9,939	8,910
Land rent in TSh.	3,679	2,125	1,907
Accessibility to the Property			
No access	1.3%	1.1%	1.1%
Foot path	55.2%	71.3%	82.0%
Feeder road	36.4%	19.8%	16.2%
Main road	5.5%	6.6%	0.6%
Highway	1.6%	1.1%	0.0%
Access to Public Utilities			
Piped water (incl. public)	22.7%	22.0%	5.6%
Electricity connection	46.1%	38.6%	35.1%
Waste removal services			
Burn/buried on plot	35.4%	25.4%	55.7%
Gutter/river/street	20.0%	49.6%	35.4%
Collected by priv. company	40.8%	24.4%	8.4%
Collected by municipality	3.8%	0.7%	0.5%
Number of properties	65,535	1,474	990

Source: Authors' calculations based on the land registry maintained by Kinondoni Municipality.

both redeemable at WAT's office. The first type was an unconditional voucher, a simple discount on the 100,000 TSh price. The second was a conditional voucher, which could only be applied if one of the names registered on the CRO application form was a female household member. These conditions were carefully explained in Swahili on each type of voucher. If households elected to use a conditional voucher, names were checked at the time of application to ensure compliance with the requirements. Vouchers were assigned to a parcel, rather than to a particular owner, so as to remain impartial to the identity of the actual owner within the household and to prevent vouchers from being exchanged between households. Examples of both types of vouchers can be found in Figure 4 in the appendix.

Vouchers could take on values ranging from zero to 80,000 TSh, in iterations of 20,000, so households could face subsidies between 0% and 80% of the total cost of a CRO. This variation will be crucial for our ability to estimate the price-elasticities of demand for both unconditional and conditional 'prices' of CROs. As shown in Table 4, every feasible combination of vouchers was given equal weighting in the randomization.⁸

While there were ex-ante concerns that a randomized top-down voucher allocation

⁸The net price of a title was restricted to be strictly greater than zero, so any voucher combination which would violate this restriction was excluded from the randomization.

Table 4: Intended general and gender-specific discount distributions

General Discount	Conditional Discount					Total
	0	20k	40k	60k	80k	
0	6.7%	6.7%	6.7%	6.7%	6.7%	33.3%
20k	6.7%	6.7%	6.7%	6.7%	.	26.7%
40k	6.7%	6.7%	6.7%	.	.	20.0%
60k	6.7%	6.7%	.	.	.	13.3%
80k	6.7%	6.7%
Total	33.3%	26.7%	20.0%	13.3%	6.7%	100%

The baseline price was TSh. 100,000 for a CRO, per parcel, regardless of size or other characteristics.

Each cell shows the intended bivariate distribution of assignment to each combination of general and gender-specific discounts. Blank cells were not used to avoid offering a negative net price.

might be perceived as unfair by participants, block-level public lotteries were deemed to be too impractical and problematic for ensuring balance and compliance. To balance these two concerns, we performed the voucher randomization in the following manner for each block:

1. We randomly drew a distribution of general/conditional voucher pairs, repeating the draw 100 times.
2. Balance was then tested for each draw using a vector of observable parcel-level characteristics and the three draws that were the most balanced (defined by *average* t-stat values) were kept.
3. These three outcomes were then presented to residents at the block-level information sessions. Each attendee was made aware of the three possible distributions, each labeled with a designated number. One of the attendees was selected by the rest to draw a number out of a hat, each number corresponding to a voucher distribution outcome. Whichever number was chosen determined the draw that would be used for the voucher distribution.

Thus we were able to maintain control over the broad aspects of the randomization while still allowing residents some perceived agency in choosing the outcome. Following the voucher distribution, households were free to sign up with WAT and begin repayment.

Both the block and the parcel-level randomizations in Barafu and Kati were performed at different times and thus represent independent draws. Due to delays in the government provision of the maps necessary to identify treatment and control households, the programme was first introduced in Barafu in late 2010, but not in Kigogo Kati until approximately a year later. In Barafu, block-level information and voucher sessions were held in late October, 2010, with participating landowners paying their net price to WAT between November and the summer of 2011. Following repayment, landowners in Barafu have been filling out and turning in CRO applications, to then be checked and sent on

to the local government by WAT. In Kigogo Kati, the voucher sessions were held in early November, 2011, with repayment continuing until the summer of 2012. Due to excessive flooding in Kati, overall participation and take up has been significantly lower than in Barafu. The data presented in this paper comprises the latest take up and application data available from the project.

3.2 Balance and summary statistics

Table 5 shows summary statistics for a select group of baseline characteristics, as well as a series of balance tests. To test whether there is a significant correlation between assigned voucher values and baseline characteristics, we estimate the following specification for each characteristic using ordinary least squares:

$$x_i = \alpha + \gamma_1 v_G + \gamma_2 v_C + \gamma_3 kati + \varepsilon_i \quad (1)$$

Where x_i is the characteristic of interest, v_G is the general voucher value, v_C is the conditional voucher value, expressed in thousands of shillings and $kati$ is a dummy equal to one if the household is part of the Kigogo Kati randomization. While it is more common to test the bivariate relationship between baseline characteristics and a single treatment, this method most-closely approximates the specification we will be using in the next section. Furthermore, as general and conditional voucher values were drawn as part of a joint distribution, it is more appropriate to test for the partial correlation between each voucher value while holding the other constant.

In Table 5, column (1) shows the mean and standard deviation for each baseline characteristic. These include the year the parcel was acquired, whether or not it is currently being rented out, whether it was inherited, if the parcel has electricity access, whether there has been recent investment in the parcel and the log of the parcel size in square meters. Household characteristics include whether the household is Muslim, monthly income and total assets, the household’s average schooling and size, and whether the household live in the parcel. While these are the characteristics we will be using as controls in the next section, we might also be interested in whether the intervention is balanced along a range of measures of female empowerment. These include whether the household is a single-female headed household, whether a woman in the household has any use rights, whether or not there is a default female owner, if the household would hypothetically include the woman on a CRO, and the percentage of total household income contributed by the female household head.

Columns (2) and (3) show estimates of γ_1 and γ_2 , respectively. Column (4) displays the point estimate of a bivariate regression of the baseline characteristic on the net price faced by the household ($100 - v_G - v_C$). In general, there is good balance across the range of baseline characteristics. There are a few significant differences: households with a higher likelihood of having access to electricity had higher general and conditional voucher values,

inherited parcels were assigned slightly lower voucher values. There is also a slight lack of balance between household size, parcel size, the female household head's share of income and general voucher values. On the whole, these differences are small, but do imply that these characteristics should be used as control in the main specification. In the next section, we will include most of these baseline characteristics as controls.

Table 5: Summary statistics and balance

	Mean/SD (1)	General (2)	Conditional (3)	Price (4)
Year parcel was acquired	1992.487 (13.505)	-.009 (0.017)	-.024 (0.019)	0.018 (0.015)
Parcel is rented out	0.388 (0.512)	-.001 (0.0007)	-.0006 (0.0007)	0.0008 (0.0006)
Parcel was inherited	0.113 (0.332)	-.0004 (0.0005)	-.0008 (0.0004)*	0.0006 (0.0004)
Electricity access?	0.398 (0.514)	0.001 (0.0007)**	0.001 (0.0007)**	-.001 (0.0006)**
Recent investment in parcel	0.214 (0.43)	0.0004 (0.0006)	0.0007 (0.0005)	-.0005 (0.0005)
Muslim hh	0.569 (0.522)	-.0004 (0.0007)	-.0004 (0.0007)	0.0004 (0.0006)
Monthly income (tsh '000)	387.497 (686.831)	-.915 (0.957)	-.817 (0.744)	0.86 (0.71)
Log(total assets (tsh '000))	7.518 (1.238)	-.003 (0.002)	0.0005 (0.002)	0.0008 (0.001)
Average schooling of hh	12.219 (2.895)	-.002 (0.004)	-.002 (0.004)	0.002 (0.003)
Household size	5.044 (2.711)	0.007 (0.004)*	0.003 (0.003)	-.005 (0.003)
Log(Parcel Area m^2)	5.115 (0.579)	-.002 (0.0008)**	-.0008 (0.0008)	0.001 (0.0007)
HH lives on parcel	0.794 (0.425)	0.0004 (0.0006)	0.0005 (0.0006)	-.0004 (0.0005)
Single female-headed household	0.189 (0.413)	-.0003 (0.0006)	-.0007 (0.0005)	0.0005 (0.0005)
Woman has rights over sale	0.582 (0.593)	-.0004 (0.0008)	-.0002 (0.0008)	0.0003 (0.0007)
De facto female owner	0.266 (0.464)	-.0005 (0.0007)	-.0006 (0.0006)	0.0005 (0.0005)
Would hypothetically cotitle	0.355 (0.507)	-.0004 (0.0007)	0.00005 (0.0007)	0.0001 (0.0006)
Women's share of hh income	0.307 (0.546)	-.001 (0.0008)	-.001 (0.0007)*	0.001 (0.0006)*
Obs	1148	1148	1148	1148

Column (1) displays the mean and standard deviation for each variable. Columns (2)-(3) display the mean and standard error of β_2 and β_3 from the linear regression of each variable $var = \beta_1 + \beta_2 * G + \beta_3 * C$, where G and C are the general and conditional voucher values. Column (4) shows the results of a single bivariate regression of each variable on the overall price households faced, net of all vouchers. Voucher values are measured in ('000 TSh). Robust standard errors
*($p < 0.10$),** ($p < 0.05$),*** ($p < 0.01$)

4 Results

4.1 Demand results

To test the relationship between randomized voucher values and the subsequent purchase of CROs, we estimate a linear probability model of the form:

$$T_i = \alpha + \beta_G v_G + \beta_C v_C + \mathbf{x}_i \delta + \varepsilon_i \quad (2)$$

In this equation, v_G and v_C are the levels of general and conditional vouchers which household i has been allocated, expressed in thousands of Tanzanian shillings. For all demand estimates, we restrict the effect of voucher values to be linear. Table 11 in Appendix A displays the results from a series of tests which suggest that this linear restriction is reasonable for take-up. The matrix \mathbf{x}_i indicates household and parcel-level characteristics from the baseline survey, which will be included in some specifications. The outcome measure, T_i , is a dichotomous variable, equal to one if the household has fully paid for a CRO through the programme.

Table 13 displays the results from the estimation of equation (2), first for the two communities Barafu and Kati separately, then pooling both together. In each case, the sample is restricted to households in treatment blocks.⁹ Column (1) shows the results from estimating equation (2) without baseline controls. In Column (2), the sample is restricted to households with baseline data and in column (3) we include baseline controls. This process is repeated for Kigogo Kati in columns (4), (5) and (6).

In Barafu, general vouchers have a large, positive effect on take-up of CROs, with a point coefficient of 0.00471, indicating that each TSh 20,000 of subsidy results in an increase in the predicted probability of take-up by 9.4 percentage points. This effect increases slightly when baseline controls are included, but not substantially so. While it is still significant, the effect of general vouchers is much smaller in Kigogo Kati, where each TSh 20,000 reduction in the price of a CRO leads to roughly a three percentage point increase in the probability of take-up. In this specification, the constant can be interpreted as the take-up rate for households receiving no vouchers of either type. The results indicate that not only do general vouchers appear to be less effective in Kati, but overall adoption rates as well.

Across both neighbourhoods, households appear to be equally responsive to conditional vouchers. At the bottom of Table 13, “Test 1” reports the p-value from the linear test of $\beta_G = \beta_C$, revealing that we can comfortably accept the null that these two coefficients are equal across all specifications.¹⁰ The results here strongly suggest that households in both communities treat conditional vouchers as ‘cash’: that is, they do not

⁹Households in control blocks were excluded from purchasing through the NGO, and local records suggest that none have gone on to purchase CROs through the municipal government.

¹⁰The failure to reject the null is not driven by imprecision, as the coefficients displayed here are precisely estimated.

Table 6: Effect of voucher distribution on CRO adoption

	Barafu			Kati			Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
General voucher value (tsh '000)	0.00471*** (0.00110)	0.00420*** (0.00117)	0.00501*** (0.00119)	0.00152** (0.000650)	0.00161** (0.000692)	0.00153** (0.000667)	0.00278*** (0.000611)	0.00496*** (0.00118)
Conditional voucher value (tsh '000)	0.00480*** (0.000941)	0.00511*** (0.000957)	0.00541*** (0.000944)	0.00174*** (0.000651)	0.00166** (0.000691)	0.00167** (0.000673)	0.00320*** (0.000561)	0.00533*** (0.000941)
General \times Kati								-0.00341** (0.00135)
Conditional \times Kati								-0.00360*** (0.00116)
Kati dummy							-0.460*** (0.0279)	-0.273*** (0.0639)
Constant	0.349*** (0.0538)	0.375*** (0.0559)	0.346*** (0.0559)	0.0754** (0.0297)	0.0754** (0.0315)	0.0710** (0.0307)	0.461*** (0.0367)	0.346*** (0.0556)
Baseline controls	No	No	Yes	No	No	Yes	Yes	Yes
Test 1: $\beta_G = \beta_C$	0.929	0.386	0.703	0.752	0.947	0.849	0.482	0.723
Test 2: $\beta_G + \beta_{G \times K} = \beta_C + \beta_{C \times K}$	0.0576	0.0621	0.104	0.0130	0.0132	0.0702	0.283	0.801
R^2	461	422	422	684	612	612	1034	1034
Obs								

Notes: Linear probability model. Dependent variable = 1 if household has fully paid for a CRO. The first three columns show results using the Barafu sample with column (1) using no unreported controls. Column (2) restricts the sample to households with non-missing controls. Column (3) includes baseline controls. The three columns for Kati sample follow this same pattern. Columns (7) and (8) pool both samples together, with and without Kati dummy interactions.

Test 1 displays the p-value from a linear test of the hypothesis that the general and conditional voucher coefficients are equal. Test 2 tests the same hypothesis in the fully-interacted model. Robust standard errors $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Figure 1: Voucher values and take-up rates, by mtaa

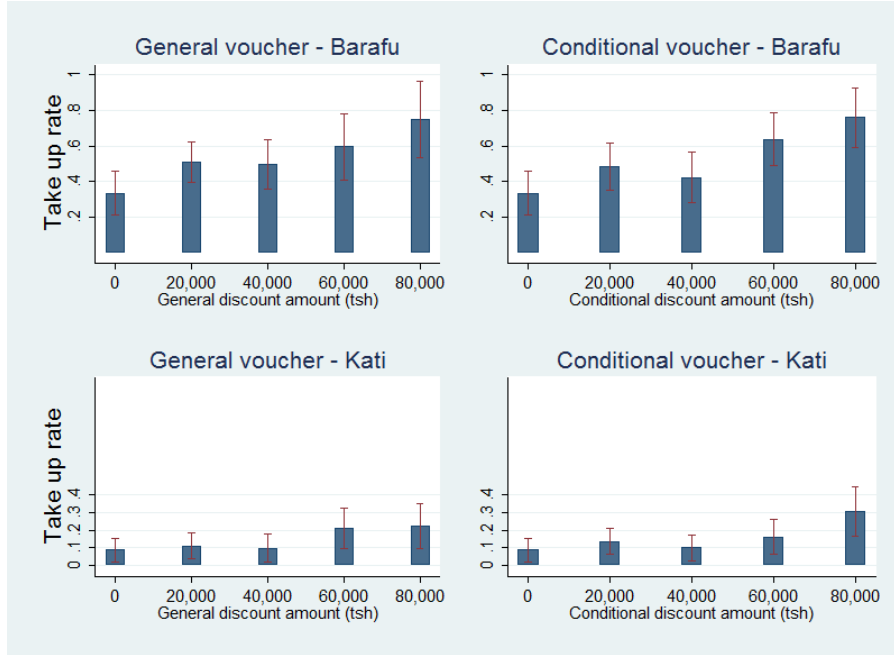


Figure shows estimates of take-up probability, conditioning on general conditional voucher values. Red bars indicates 95% confidence interval.

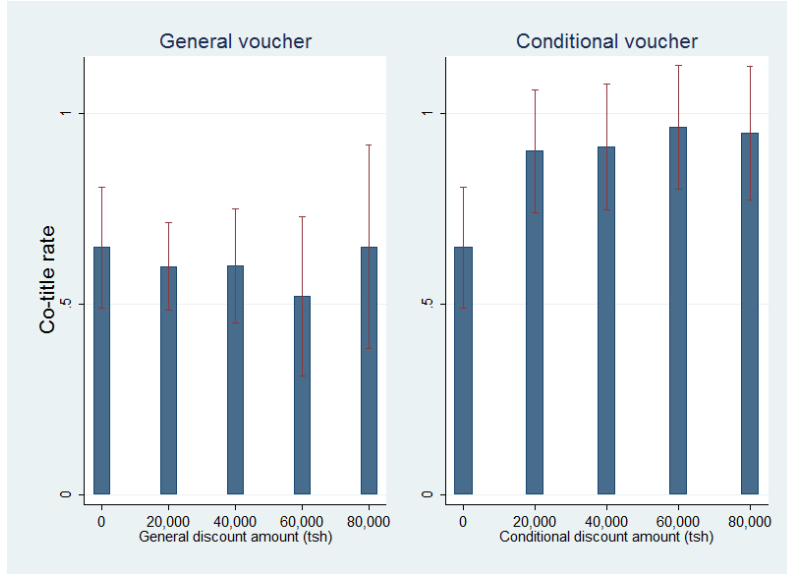
appear to be any demand effects of imposing conditionality. This implies that, on average, gender conditionality can be imposed without excluding households averse to co-titling. We will discuss the implications for bargaining power effects shortly. Figure 1 displays estimated take-up levels for each voucher type for Barafu and Kati separately. While the pattern of take-up across each value value differs slightly between general and conditional vouchers, they do not appear to be significantly different.

Columns (7) and (8) of Table 13 pool the data from both communities, reporting average voucher effects across both *mitaa* while controlling different level effects with a dummy for Kigogo Kati. Again, general and conditional effects appear to be almost identical, with lower overall take-up in Kati. Interacting the voucher values with the Kati dummy reinforces the result that demand response is lower in Kati, but that households in both locations do not discern between general and conditional vouchers.

4.2 Co-titling results

While the results in the previous subsection encouragingly suggest that applying conditionality does not deter households from purchasing land titles, it is not yet clear that this conditionality actually leads to an increase in co-titling. Households might be indifferent to listing women as owners or might have all planned to co-title irrespective of any conditionality.

Figure 2: Voucher values and female co-title rates



Note: Figure shows estimates of co-titling (conditional on submission of an application probability), conditioning on general/conditional voucher values. Red bars indicates 95% confidence interval. Sample is pooled across both mitaa.

To investigate whether households respond to price incentives by co-titling, we rely on data from the household’s CRO application, where women from the household were identified and recorded. Define $cotitle_i$ as a binary outcome equal to one if the household has included *any* woman from the household on the CRO application, *conditional* on the household having chosen to purchase a CRO. We then wish to re-estimate (2), using this variable as our outcome of interest:

$$cotitle_i = \alpha + \beta_G v_G + \beta_C v_C + \mathbf{x}_i \delta + \varepsilon_i \quad (3)$$

Ideally, equation (3) should be estimated over the full sample of households who have chosen to purchase a CRO. However, to date, application data is not available for approximately 30% of households who have taken up. While we will proceed as if the determinants of application data being observable are random, it is possible that non-random selection could lead to bias of our estimates. We investigate this further in Appendix A, where we use a basic sample selection model to suggest that selection is not resulting in any significant bias of our estimates of β_G and β_C .

Table 7 shows the results from estimating (3) for Barafu and Kati separately in columns (1) and (2), then as a pooled sample, all with baseline control included. For Barafu, while the general voucher only has a small, insignificant negative effect on co-titling, the conditional voucher has a large, positive significant impact, with each 10,000 TSh subsidy resulting in an increase in the predicted probability that a woman is included

Table 7: Effect of voucher distribution on female co-titling, conditional on CRO application

	Barafu	Kati	Pooled	
	(1)	(2)	(3)	(4)
General voucher	-0.00103 (0.00156)	0.000544 (0.00316)	-0.000828 (0.00122)	
Conditional voucher	0.00348*** (0.00126)	0.00724** (0.00283)	0.00372*** (0.00106)	
General = 20				-0.0250 (0.0657)
General = 40				-0.0491 (0.0773)
General = 60				-0.144 (0.101)
General = 80				0.0559 (0.114)
Conditional = 20				0.288*** (0.0697)
Conditional = 40				0.297*** (0.0763)
Conditional = 60				0.324*** (0.0807)
Conditional = 80				0.304*** (0.0962)
Constant	0.748*** (0.0807)	0.540*** (0.167)	0.733*** (0.0649)	0.645*** (0.0749)
Baseline controls	Yes	Yes	Yes	Yes
R^2	0.130	0.176	0.126	0.171
Obs	211	53	264	264

Notes: Linear probability model. Dependent variable = 1 if household included a woman on their CRO application, conditional on having paid for a CRO. Sample is restricted to households with application data. First three columns use linear measures of voucher values. Last column introduces dummy for each voucher value. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Net effect of voucher distribution on co-titling

	Barafu	Kati	Pooled	
	(1)	(2)	(3)	(4)
	All	All	All	Dual-headed
General voucher	0.00243** (0.00116)	0.000416 (0.000467)	0.00109** (0.000509)	0.00158** (0.000698)
Conditional voucher	0.00498*** (0.000981)	0.000318 (0.000410)	0.00228*** (0.000489)	0.00288*** (0.000649)
Constant	0.214*** (0.0530)	0.0428** (0.0208)	0.326*** (0.0319)	0.324*** (0.0424)
Baseline controls	Yes	Yes	Yes	Yes
Test: $\beta_G = \beta_C$	0.0219	0.835	0.0227	0.0652
R^2	0.0849	0.0185	0.214	0.231
Obs	422	615	1037	603

Notes: Linear probability model. Dependent variable = 1 if household has fully paid for a CRO and submitted an application with a woman listed on it. First two columns restrict the sample to Barafu and Kati, respectively. Column (3) pools both samples. Column (4) restricts sample to dual-headed households. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

by 3.5 percentage points. The results are even stronger in Kigogo Kati, where each 10,000 TSh of conditional subsidy increases the probability of co-titling by 7.2 percentage points. Column (3) shows the results, pooling both samples together, and in column (4) we depart from the usual linear specification and introduce the individual voucher values as dummies. Households which receive *any* conditional voucher are 29-30 percentage points more likely to co-title than those that receive no voucher (the omitted category). This effect is persistent and statistically indistinguishable across all voucher values, indicating that households are effectively nudged into co-titling by conditional vouchers. This is illustrated in Figure 2, where co-titling rates are graphed against voucher values, indicating that households receiving any conditional voucher are highly likely to include a woman as an owner on the CRO application. In contrast, general vouchers do not appear to have any significant effect on co-titling. As households receiving conditional vouchers are no less likely to purchase a CRO, but are almost certain to co-title, this suggests that imposing conditionality can only increase the total number of women on land titles. To test this, we define an unconditional co-titling outcome, equal to one if the household purchases a CRO, submits an application and includes a woman as an owner on the application, and equal to zero otherwise. We then repeat the standard specification with this “net co-titling” outcome to see if, in aggregate, conditional vouchers are more successful at moving households into a co-titled state. Columns (1) and (2) of Table 8 display the results for Barafu and Kati separately, with column (3) showing the pooled

result. Column (4) shows the pooled result, but with the sample restricted to dual-headed households, where we might expect bargaining effects to be at play. Below the main results, ‘Test’ reports a linear test of the null hypothesis that the two vouchers have equal effects. Across all the columns, general vouchers have a positive, strong and significant effect in all but the Kati specification.¹¹ General vouchers have a positive effect because they induce households to purchase CROs, many of whom go on to co-title even without conditional incentives. Again, in all columns but (2), conditional vouchers have a strong positive effect, large enough that we are able to reject the null hypothesis of $\beta_G = \beta_C$ in three out of the four columns.

So it seems that, while the basic intervention was itself successful on improving the status quo, imposing conditionality can take it even further. However, while policymakers might see this as an easy, simple way to get women on land titles, the results might make us question the subsequent impact of getting women onto land titles.

4.3 Discussion and heterogenous effects

Reconsider the linear probability model (2) used to estimate the demand results in the previous subsection:

$$T_i = \alpha + \beta_G v_G + \beta_C v_C + \mathbf{x}_i \delta + \varepsilon_i$$

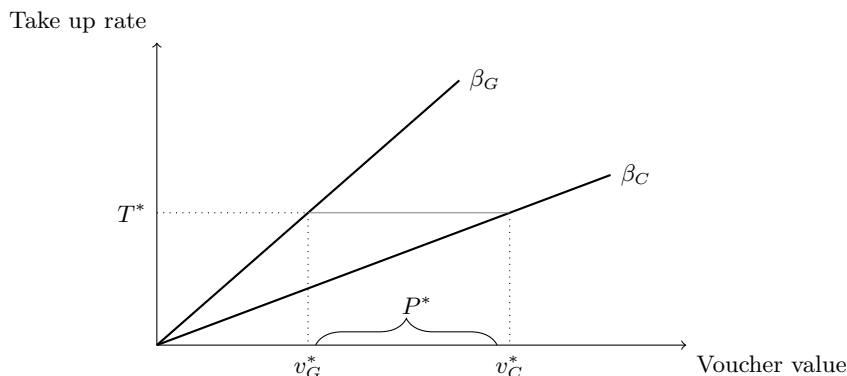
In a very simple world where households receive some disutility from co-titling, we would expect estimates of β_G and β_C to differ. Thus, to ensure a given level of titling, households with conditional vouchers would have to receive higher subsidies. This is the “price of empowerment”, the amount that would be need to transferred to households to offset the decline in demand caused by conditionality. Figure 3 illustrates this relationship: for a desired level of CRO take-up T^* and linear demand effects of general and conditional vouchers β_G and β_C , the extra discount needed to offset the conditionality of the vouchers is given by $v_C^* - v_G^* = P^*$. This price is crucial for policymakers weighing the benefits of co-titling against the extra costs associated with the reduction in demand.

The results from the randomized voucher intervention have shown us that, given our estimates of β_G and β_C are indistinguishable, the cost of conditionality P^* is effectively zero: in the context of this intervention, small price incentives are sufficient to overcome any resistance to co-titling. This is encouraging from a simple policy perspective, as it seems particularly easy to nudge women onto land titles.

However, the fact that households are so easily nudged into including women suggests that either co-titling does not result in any substantial shifts in bargaining power or that households do not believe that it will. To better understand whether or not households

¹¹In Kigogo Kati, due to extremely low take-up, only 6% of households purchase a CRO, fill out an application, and include a woman as an owner, suggesting that there may just not be enough observations in this category to provide the necessary precision to reject the null.

Figure 3: The price of “empowerment”



are behaving as if co-titling will have substantial bargaining power effects, we can explore heterogeneity in take-up and co-titling, using baseline characteristics that might proxy for women’s ex-ante bargaining power. This also allows us to investigate whether or not conditional vouchers are more successful at inducing certain types of households to co-title.

Table 9 displays the results from re-estimating the three specifications used before (CRO take-up, conditional co-titling and net co-titling) with the sample restricted to dual-headed households, to focus on households where bargaining power is likely to be a concern. We consider two dummy variables which might proxy for women’s current bargaining power: whether or not a woman is considered a default owner of the property, and the share of total household income the female household-head provides. Column (1) shows the aggregate result for take-up and column (2) displays the same specification, but with interactions between the default owner dummy and both voucher values. The results indicate that properties where women are already considered co-owners are significantly less likely to adopt CROs, but are not significantly more or less responsive to voucher allocations, nor do they treat general or conditional voucher values differently. However, the picture changes when we observe conditional co-titling outcomes in column (3), where households with de facto female ownership are substantially more likely to co-title, but are not responsive to conditional vouchers. While conditional vouchers appear to still have a strong positive effect on households without default female ownership, a linear test cannot reject the hypothesis the the two vouchers have an equivalent impact for households with default ownership (Test 2 under column three). Column (4) displays the unconditional, net co-titling outcomes, indicating no substantial differences between households with de facto ownership in either average outcomes nor responsiveness to vouchers.

Columns (5), (6) and (7) repeat this exercise, interacting the head’s share of total household income with voucher values. Households in which women provide a greater share of household income are slightly less likely to purchase a CRO, although this effect is not significant at the 10% level. There is also no concrete evidence that these households

respond differently to either voucher. However, column (6) indicates households where women provide greater shares of income are significantly more likely to co-title, conditional on purchasing a CRO and are less responsive to gender vouchers.

What are we to make of these results? Households where women appear to have less bargaining power are, overall, more likely to buy CROs and seem no less responsive to general or conditional price incentives. Conditional on purchasing a CRO, these households appear less likely to co-title, but are more responsive to conditional vouchers. Under the assumption that co-titling leads to a substantial shift in bargaining power, it is puzzling that male-dominated households appear happy to sign up women when asked to. Again, this suggests that households are not acting as if they expect co-titling to lead to substantive changes in the status quo. However, if households are short-sighted and these choices will eventually have implications for bargaining power, then these results are promising: households which have high female-bargaining power appear to be co-titling by default, where male-dominated households are successfully induced to co-title by conditional vouchers, with no observable reduction in demand.

5 Conclusion

In this paper, we presented preliminary results from a land titling experiment in Dar es Salaam, Tanzania, where we use targeted subsidies to induce random variation in the price that land-owning households faced when purchasing a land title. In addition, we also created randomized variation in the conditionality of these subsidies, requiring some households to include a woman on the land title application in order to apply the discount.

Our results strongly suggest that, on average, both general and conditional subsidies have identical impacts on CRO adoption, revealing that households are not deterred by conditionality. Conditional on purchasing a CRO, households which were allocated a conditional voucher were much more likely to include a woman on their title application. These two results, taken together, indicate that small price incentives are an effective means of encouraging *de jure* empowerment of women in the implementation of land titling schemes. However, it remains to be seen whether or not these strictly legal improvements in women’s land ownership will result in actual *de facto* improvements in the lives of urban landowners, in particular for the lives of women. The fact that the “price of empowerment” appears to be very low is troubling, as households might be co-titling under the belief that *de jure* improvements in women’s land ownership will not translate into real changes in women’s household bargaining power. Future iterations of this research will take advantage of follow-up data to determine whether or not co-titling results in any palpable changes in women’s welfare.

Table 9: CRO adoption and co-titling, interaction effects - dual-headed households

	Default female owner			Women's share of income			Hypothetically cotitle			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Take up	Take up	Co-titling	Net women	Take up	Co-titling	Net women	Take up	Co-titling	Net women
General voucher	0.00363*** (0.000827)	0.00325*** (0.000898)	-0.000964 (0.00202)	0.00138* (0.000745)	0.00386*** (0.00106)	0.000522 (0.00213)	0.00180** (0.000888)	0.00374*** (0.000920)	0.000986 (0.00227)	0.00201*** (0.000767)
Conditional voucher	0.00386*** (0.000742)	0.00349*** (0.000815)	0.00488*** (0.00156)	0.00289*** (0.000719)	0.00334*** (0.000953)	0.00724*** (0.00194)	0.00331*** (0.000820)	0.00412*** (0.000872)	0.00654*** (0.00180)	0.00327*** (0.000759)
X		-0.217** (0.0988)	0.475*** (0.176)	-0.0637 (0.0932)	-0.136 (0.138)	0.820** (0.335)	0.0253 (0.116)	-0.0377 (0.0991)	0.589*** (0.167)	0.0675 (0.0882)
General $\times X$		0.00288 (0.00230)	-0.00378 (0.00352)	0.00171 (0.00214)	0.00171 (0.00333)	-0.0156** (0.00626)	-0.00123 (0.00217)	0.000182 (0.00205)	-0.00870** (0.00403)	-0.00121 (0.00173)
Conditional $\times X$		0.00267 (0.00189)	-0.00938** (0.00418)	-0.000116 (0.00171)	0.00449 (0.00274)	-0.0116** (0.00498)	-0.0000545 (0.00245)	-0.000679 (0.00174)	-0.00879*** (0.00277)	-0.00133 (0.00158)
Constant	0.436*** (0.0493)	0.466*** (0.0526)	0.680*** (0.108)	0.332*** (0.0454)	0.431*** (0.0613)	0.545*** (0.128)	0.295*** (0.0506)	0.436*** (0.0545)	0.572*** (0.123)	0.296*** (0.0464)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test 1: $\beta_G = \beta_C$	0.780	0.782	0.0000439	0.0451	0.596	0.0000212	0.0818	0.680	0.000345	0.120
Test 2: $\beta_G + \beta_{G \times X} = \beta_C + \beta_{C \times X}$		0.992	0.936	0.878	0.449	0.00295	0.161	0.773	0.0359	0.432
R^2	0.279	0.283	0.190	0.233	0.275	0.275	0.244	0.281	0.218	0.228
Obs	603	603	166	603	519	143	519	597	163	597

Notes: Linear probability model. Dependent variable = 1 if household has fully paid for a CRO and submitted an application with a woman listed on it. First two columns restrict the sample to Barafu and Kati, respectively. Column (3) pools both samples. Column (4) restricts sample to dual-headed households. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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A Extra figures and tables

Table 10: Tanzania’s Land Act of 1999 - provisions relating to spouses

161.-(1)	Where a spouse obtains land under a right of occupancy for the co-occupation, and use of both spouses or where there is more than one wife, there shall be a presumption that, unless a provision in the certificate of occupancy or certificate of customary occupancy clearly states that one spouse is taking the right of occupancy in his or her name only or that the spouses are taking the land as occupiers in common, the spouses will hold the land as occupiers in common and, unless the Presumption is rebutted in the manner stated in this subsection, the Registrar shall register the spouses spouses as joint occupiers accordingly.
161.-(2)	Where land held for a right of occupancy is held in the name of one spouse only but the other spouse or spouses contribute by their labour to the productivity, upkeep and improvement of the land, that spouse or those spouses shall be deemed by virtue of that labour to have acquired an interest in that land in the nature of an occupancy in common of that land with the spouse in whose name the certificate of occupancy or customary certificate of occupancy has been registered.
161.-(3)	<p>Where a spouse who holds Act No. 5 land or a dwelling house for a right of occupancy in his or her name alone undertakes a disposition of that land or of 1971 dwelling house, then-</p> <ol style="list-style-type: none"> 1. Where that disposition is a mortgage, the lender shall be under a duty to make inquiries of the borrower has or as the case may be, have consented to that mortgage accordance with the provisions of section 59 of the Law of Marriage Act, 1971 2. Where that disposition is an assignment or a transfer of land, the assignee or transferee shall be under a duty to Make inquiries of the assignor Or transferor as to whether the spouse or spouses have consented to that assignment or transfer in accordance with section 59 of the Law Of Marriage <p>and where the aforesaid spouse undertaking the disposition deliberately misleads the lender or, as the case may be, the assignee or transferee as to the answers to the inquiries made in accordance with Paragraphs (a) and (b), the disposition shall be voidable at the option of the spouse or spouses who have not consented to the disposition.</p>

Figure 4: Example vouchers, general and conditional

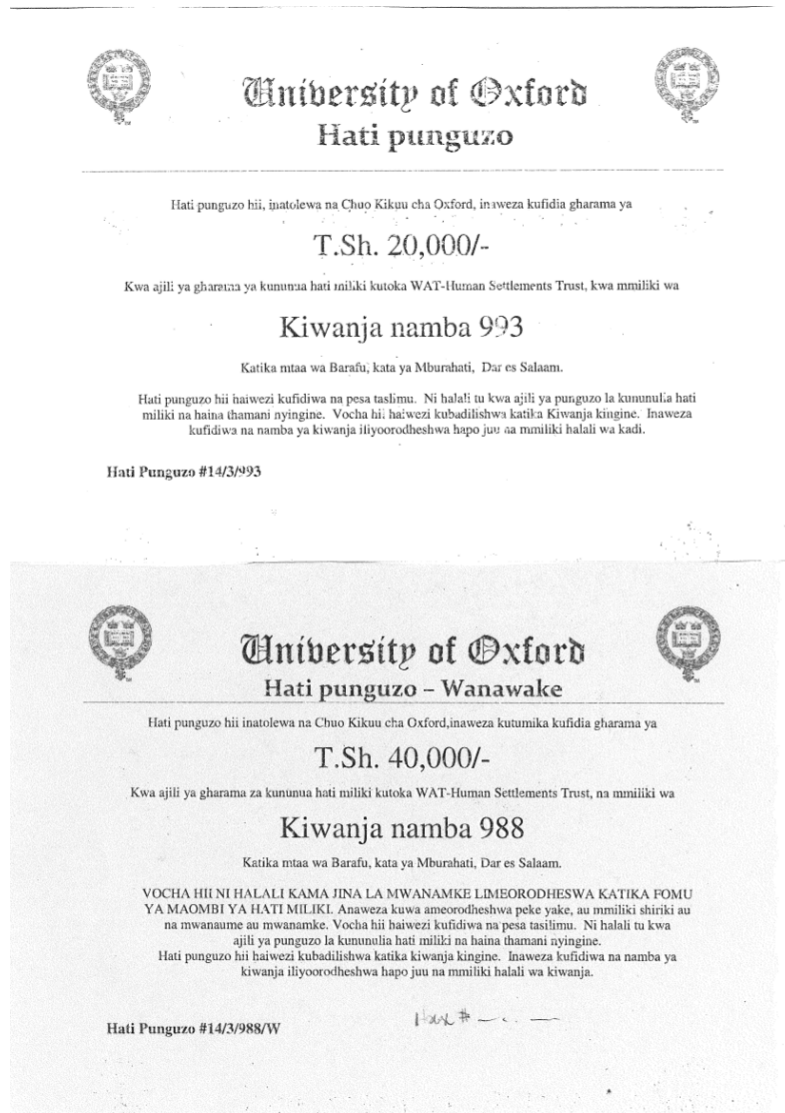


Figure shows two examples of vouchers which households might have received, indicating the conditionality (“wanawake” is Swahili for “woman”), the amount the voucher was worth, as well as the parcel number for which the voucher would apply (*kiwanja namba*).

Table 11: Test of linearity assumption of voucher impacts

	20	40	60	80
General voucher				
20	.	.3231502	.7376661	.4800987
40	.3231502	.	.337561	.7324069
60	.7376661	.337561	.	.5441553
80	.4800987	.7324069	.5441553	.
Conditional voucher				
20	.	.3042746	.7128511	.8541609
40	.3042746	.	.2297262	.1226721
60	.7128511	.2297262	.	.6533703
80	.8541609	.1226721	.6533703	.

Results taken from regression of take up on a dummy for each general and conditional voucher value. Each cell contains the p-value from a test of linearity between two coefficients. For example, cell (20,40) displays the results from the test of $2 * \beta_{20} = \beta_{40}$

sectionSelection model for applications Several regressions in this paper rely on CRO application data which is only observable for a subset of CRO-purchasing households who have submitted an application. Consider two equations, the co-titling specification from Subsection 4.2 and a selection equation, where A_i is a binary variable equal to one if the household has submitted an application (conditional on purchasing a CRO):

$$cotitle_i = \alpha + \beta_G v_G + \beta_C v_C + \mathbf{x}_i \delta + \varepsilon_i \quad (4)$$

$$A_i = \gamma + \mathbf{z} \beta_z + v_i \quad (5)$$

The vector \mathbf{z} comprises observable household characteristics which affect the probability a household submits an application and ordinarily contains all the covariates included in (4). If the error terms of these two equations are uncorrelated, then the determinants of selection are random (conditional on the covariates in (4)). However, if the unobserved determinants of selection are correlated with the unobserved determinants of co-titling, $cov(\varepsilon_i, v_i) \neq 0$, then estimates of β_G and β_C will be subject to sample selectivity bias. For example, if male-dominated households are less likely to turn in an application, conditional on purchase, and male-dominated households are less likely to co-title, then coefficient estimates in the co-titling equation are likely to be biased.

To account for this bias, we use a standard Heckman selection model, in which we first estimate equation (5) using a probit, then use the predicted values to construct the estimated inverse mills ratio $\frac{\phi(\mathbf{z} \hat{\beta}_z)}{\Phi(\mathbf{z} \hat{\beta}_z)}$. If equation (4) is subject to sample selection bias, inclusion of the IMR should correct for it (Heckman 1979). However, in practice, if \mathbf{z} only comprises observable characteristics already included equation (4), then the inverse mills ratio will be strongly collinear with the pre-existing covariates in the outcome equation.

For robust identification, we require an observable characteristic which can be included in the selection equation, but reasonably be excluded from the outcome equation. In this case, our ‘instrument’ of choice is a dummy equal to one if the household resides on the owned parcel. The NGO tasked with managing the repayment programme found it substantially more difficult to reach and follow up with households living away from their parcels, as these households were often located outside of the neighbourhood. Unsurprisingly, households living off of their owned parcel were much less likely to purchase a land title or submit an application. However, conditional on the purchase decision, we argue that the household’s residence status can reasonable be excluded from the *co-titling* equation, as there is no reason to believe that households living on their owned parcel will be more or less likely to include a woman as a landowner.

Table 12 displays the results from four separate specifications. Column (1) displays the results from the main OLS specification, estimating the probability of co-titling on the voucher values and a vector of baseline controls. Column (2) uses the Heckman 2-step method of correcting for sample selection bias.¹² Estimated coefficients from the outcome equation are shown in the top half of the table and those from the first-stage selection estimation are shown in the bottom half (baseline controls are not reported). In the selection specification, the estimated coefficients on the voucher dummies are broadly similar to those in the OLS specification. Indeed, the estimated coefficient of the inverse mills ratio is not statistically significant, indicating that selection is not biasing the OLS results. To test for differences using a different function form, columns (3) and (4) display the results from a probit estimation of the outcome equation and a probit model with Heckman sample selection.¹³ Again, the results are very similar across the two specifications, and a test of independence between the two fails to reject the null of no difference.

¹²The two-stage procedure is more robust to violation of the assumption of bivariate normal error terms.

¹³This procedure is described in Section 17.4.3 in (Wooldridge 2002)

Table 12: Effect of voucher distribution on co-titling, sample selection specification

	(1) OLS	(2) Selection	(3) Probit	(4) Selection Probit
Main Equation				
General voucher	-0.000824 (0.00122)	-0.00115 (0.00152)	-0.00244 (0.00432)	-0.00328 (0.00438)
Conditional voucher	0.00371*** (0.00106)	0.00429*** (0.00146)	0.0181*** (0.00539)	0.0177*** (0.00477)
Constant	0.727*** (0.0644)	0.896*** (0.135)	0.579** (0.242)	0.767*** (0.241)
Pr(Application submitted)				
General voucher		-0.000943 (0.00369)		-0.000500 (0.00369)
Conditional voucher		-0.00331 (0.00339)		-0.00303 (0.00342)
HH lives on parcel		0.386* (0.204)		0.459** (0.193)
Constant		0.608** (0.281)		0.525* (0.277)
λ		-0.435 (0.337)		
χ^2 (test of indep)				1.87
Prob > χ^2				0.1711
Baseline controls	Yes	Yes	Yes	Yes
R^2	0.125		0.146	
Obs	264	368	264	368

Notes: Dependent variable = 1 if household includes a woman on CRO application. Column (1) shows results from linear probability model. Column (2) uses Heckman 2-step to account for the selection of submitting an application. All controls are included in outcome and selection equation but not reported. A dummy for whether the household resides on the owner parcel is excluded from the second-stage equation. Column (3) shows the raw coefficients from a probit model. Column (4) shows the results from a selection probit with the same exclusion restriction. Main equation shows results from second stage and Pr(application) shows results from first stage.

Robust standard errors * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Effect of voucher distribution on CRO adoption, female-headed households

	Barafu			Kati		
	(1)	(2)	(3)	(4)	(5)	(6)
General voucher (tsh '000)	0.00542*** (0.00130)	0.00547*** (0.00130)	0.00604*** (0.00132)	0.00197** (0.000801)	0.00197** (0.000801)	0.00203*** (0.000782)
Conditional voucher (tsh '000)	0.00602*** (0.00102)	0.00618*** (0.00103)	0.00631*** (0.00102)	0.00221*** (0.000776)	0.00221*** (0.000776)	0.00230*** (0.000762)
General \times female-headed	-0.00607** (0.00296)	-0.00611** (0.00296)	-0.00526* (0.00295)	-0.00183 (0.00153)	-0.00183 (0.00153)	-0.00252* (0.00141)
Conditional \times female-headed	-0.00566** (0.00280)	-0.00582** (0.00280)	-0.00504* (0.00271)	-0.00313* (0.00161)	-0.00313* (0.00161)	-0.00367** (0.00157)
Female (only) headed hh	0.344** (0.147)	0.353** (0.147)	0.317** (0.144)	0.0745 (0.0780)	0.0745 (0.0780)	0.104 (0.0727)
Baseline controls	No	No	Yes	No	No	Yes
Test 1: $\beta_G + \beta_{G \times F} = 0$	0.810	0.810	0.771	0.910	0.910	0.678
Test 2: $\beta_G + \beta_{C \times F} = 0$	0.889	0.889	0.612	0.519	0.519	0.318
R^2	0.0738	0.0769	0.115	0.0221	0.0221	0.0791
Obs	423	421	421	608	608	608

Notes: Linear probability model. Dependent variable = 1 if household has fully paid for a CRO. The first three columns show results using the Barafu sample with column (1) using no unreported controls. Column (2) restricts the sample to households with non-missing controls. Column (3) includes baseline controls. The three columns for Kati sample follow this same pattern. Test 1 displays the p-value from a linear test of the hypothesis that the general voucher effect for female-headed households is zero and Test 2 tests the same hypothesis for conditional vouchers. Robust standard errors * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$