

# The Price of Empowerment: Experimental Evidence on Land Titling in Tanzania

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## Abstract

We report on a randomized field experiment using price incentives to address both economic and gender inequality in land tenure formalization. During the 1990s and 2000s, nearly two dozen African countries proposed de jure land reforms extending access to formal, freehold land tenure to millions of poor households. Many of these reforms stalled. Titled land remains the de facto preserve of wealthy households and, within households, men. Beginning in 2010, we tested whether price instruments alone can generate greater inclusion by offering formal titles to residents of a low-income, unplanned settlement in Dar es Salaam at a range of subsidized prices, as well as additional price incentives to include women as owners or co-owners of household land. Estimated price elasticities of demand confirm that prices – rather than other implementation failures or features of the titling regime – are a key obstacle to broader inclusion in the land registry, and that some degree of pro-poor price discrimination is justified even from a narrow budgetary perspective. In terms of gender inequality, we find that even small price incentives for female co-titling achieve almost complete gender parity in land ownership with no reduction in demand.

**JEL Codes:** J16, K11, O12, O18, Q15

**Keywords:** land titling, formalization, gender, field experiment, Tanzania

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# 1 Introduction

Economic historians have long pointed to the emergence of formal, transferrable, and collateralizable property rights as an important catalyst to economic development (North and Weingast 1989; Besley and Ghatak 2010). Since the 1990s, responding partially to the popular work of De Soto et al. (1989), a growing empirical literature in development economics has examined the impact of government programs to formalize the informal land rights of poor households in rural villages and urban slums in the developing world. Experimental and quasi-experimental impact evaluations of several major land-titling initiatives have shown significant positive effects not only on savings and investment, but also labor supply, attitudes toward the market, and fertility choices (Field 2003; Di Tella, Galiani, and Schargrotsky 2007; Galiani and Schargrotsky 2010). Most recently, the U.N. High Level Panel on post-2015 development goals recommended setting explicit targets for “the share of women and men, communities and businesses with secure rights to land, property, and other assets.”

But outside of a handful of extensively researched cases in Latin America, similar efforts to formalize land tenure – particularly in sub-Saharan Africa – have had zero effect, inasmuch as reform projects have failed or been abandoned before ever being implemented. From 1990 to 2003, twenty-three African countries proposed new legislation to reform land administration, most of which took small steps in the direction of individual, freehold tenure (Alden Wily 2003). A decade later, implementation of most of these reforms has stalled. Virtually none of the countries that proposed new land legislation in the 1990s have managed to implement a land administration system providing formal tenure security to any sizable share of poor land owners (Deininger, Ali, Holden, and Zevenbergen 2008). In Tanzania, the focus of this study, a sweeping 1999 reform of the country’s land laws created a legal pathway for rural and urban households to acquire formal tenure rights. But as of 2011, the U.S. Agency for International Development noted that formal land titles “have not yet moved beyond pilot projects” (USAID 2011), and in December 2013 the World Bank approved a new loan to the Tanzanian government to attempt, yet again, to implement the land administration system envisioned in the 1990s land reforms (World Bank 2013).

There is a strong gender component to this phenomenon. Many of the 1990s land laws in Africa were explicitly designed to mitigate gender inequality. Yet where formalization of land rights has begun, there are signs that the process may reinforce exclusive male control of land. Female inclusion rates have been disappointingly low in early titling programs in the region (Deere and León 2001; Payne, Durand-Lasserve, and Rakodi 2007). As a complex system of overlapping customary or religious rights to land is replaced with a centralized government system of individual freehold tenure in the sole name of the person listed on a formal title, women may lose what little bargaining power they have over land if the title lists only a man. As is, women constitute on average approximately 30%

of land owners in all countries in sub-Saharan Africa for which survey data is available (Doss et al. 2013), and this ownership deficit is considered a major contributing factor to women’s overall economic disadvantage (Goldstein and Udry 2008).

This paper asks two broad questions. First, on the supply side, why has the Tanzanian government failed to implement land titling laws in urban areas, and how should it price titles going forward? We test whether demand for titles is simply too low to justify the high fixed costs of systematic demarcation, or alternatively, if the government is ‘leaving money on the table’ by failing to formalize unplanned urban settlements. Second, on the demand side, why do households fail to record women’s ownership claims on household land, and how can they be incentivized to do so? We show that men are by default treated as the sole legal owners of household land, and hypothesize they will require strong economic incentives to relinquish exclusive ownership. This suggests any attempt to encourage or require female co-titling will reduce overall demand for land tenure formalization, which we test.

We present results from a field experiment in unplanned settlements of Dar es Salaam beginning in 2010, a context in which formal land titles are theoretically available to all residents but extremely rare in practice, and in which self-reported female ownership of land is quite low.<sup>1</sup> All households owning land in the treatment area had the opportunity to buy a formal land title at a base price of approximately USD \$64.<sup>2</sup> Households were then randomly assigned two vouchers that could be redeemed for a discount on this base price. The first voucher was a general, unrestricted price discount. The second voucher provided an additional discount, over and above the general voucher, conditional on the household including a woman as owner on the title application.

Household level analysis of general price-elasticity of demand for titles lead to three key findings. First, given the high fixed costs of land formalization in Tanzania – comprised of cadastral survey costs and lengthy bureaucratic procedures – titling will not generate positive net revenue for the Tanzanian government, even after including future tax revenues and even in neighborhoods significantly more affluent than the study area. Second, if these fixed costs of surveying and red-tape could be lowered, price elasticities imply that the optimal sale price for land titles by a revenue-maximizing monopolist would be considerably lower than current levels. Third, elasticities also imply that the government would maximize revenue through price discrimination, offering lower prices to poorer households. This is true independent of the equity benefits of such pricing schemes.

Stepping back, we draw two broad conclusions about the fiscal viability of urban land

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<sup>1</sup>Only 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.

<sup>2</sup>For reference, the median household income in the sample was approximately USD \$200/month. Throughout the paper we use an exchange rate of 1,565 Tanzanian shillings per U.S. dollar. This was the prevailing rate as of January 1st, 2012, approximately mid-way through the window in which households purchased titles.

titling in Tanzania. First, from a positive political economy perspective, inserting the demand elasticities from our field experiment into a model of the Tanzanian government as a revenue maximizer – a modeling decision based on explicit statements by Ministry of Lands officials – appears to explain the failure to implement the 1999 land laws throughout most of Dar es Salaam, and particularly in low-income areas. Second, any route to a fiscally sustainable land titling program in Tanzania appears to require a dramatic reduction in the costs of formalization. International experience suggests this is feasible (World Bank 2007; Ali, Deininger, and Goldstein 2011).

Turning to the gender dimension, the striking feature of our results is the contrast between (a) the rarity of female titling outside of the project, as well as the strong link between female titling and development outcomes in other research, and (b) the relative ease with which we are able to motivate households to give women access to formal co-ownership of household land. 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 aggregate, the number of women listed as landowners. While these results are encouraging, the fact that households are so easily nudged into co-titling<sup>3</sup> raises concerns that they might not be treating the decision as if it has significant implications for household bargaining power. To investigate this further, we test whether voucher assignments are more or less effective in households where women have 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.

The rest of the 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. 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, with effects of gender conditionality on co-titling in Section 5. Section 6 concludes.

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

## 2 Background and context

### 2.1 From expropriation to taxation

In theory, a state with stable monopoly power on the use of force should restrict expropriation to maximize economic output and tax revenue (Besley and Ghatak 2010). Or as Olson (1993) memorably observed, “in a world of roving banditry there is little or no incentive to produce or accumulate anything that may be stolen.” But a rational bandit with firm control over a fixed population will instead opt to provide peaceful order, “thereby obtaining more in tax theft than he could in migratory plunder.”

Land titling programs present developing country governments with this basic tradeoff between expropriation and taxation. The 1999 Tanzanian land law that we examine here can be seen as a commitment to forego expropriation of land in unplanned, informal settlements, in exchange for the ability to levee property taxes on that land.

Ideally, a benevolent social planner would consider the diverse economic benefits from the formalization of property rights, including the impacts on savings and investment, credit market access, labor supply, the efficiency of land allocation, and so on. A less benevolent, or less forward-looking policymaker might maximize fiscal revenues from property formalization while ignoring these broader social benefits. In the following sections, we explore the ability of such a pessimistic, revenue-maximization model to explain the Tanzanian government’s land titling policies to date.

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Tanzania’s land tenure formalization program was directly shaped by the work of Hernando de Soto, who emphasized that formalization would unlock the “dead capital” of the informal sector, providing a catalyst to economic development and, in turn, generate tax revenue for the state (Sundet 2006). Former Tanzanian president Benjamin Mkapa (1995-2005) invited De Soto to Tanzania to help establish a “Property and Business Formalisation Programme”, known by its Swahili acronym, MKURABITA. The core aim of the program was to impart formal land titles and business registration to the poor that are freely tradable and usable as collateral for formal credit (Sundet 2006).

While MKURABITA remains active, the roll-out of land titles to the poor has been extremely limited. Even in urban areas, the proportion of land covered by a formal title that is transferrable and usable as collateral in the formal credit market is less than 15%, and the share of actual parcels covered is considerably smaller.

So why has the Tanzanian government failed to make the pivot from expropriation to taxation in the management of land? Explanations can be divided into two types: (i) the costs of formalization, including the lost benefits of potential expropriation and the direct cost of cadastral surveying, exceed the fiscal benefits to the state in increased tax revenue, or (ii) states with weak implementation capacity or fiscal space to make large

upfront outlays fail to make investments in formalization that would yield clearly positive fiscal returns.

To distinguish these explanations requires data on both the costs and benefits of formalization. We begin by sketching the costs associated with formalization; the experimental results below will shed light on the benefits, defined narrowly in terms of state revenue from the sale of land titles and collection of property taxes.

For the Tanzanian government, by far the largest single cost of land tenure formalization is cadastral surveying. Crucially, cadastral surveying exhibits strong scale economies. These economies are due in part to simple geometry: beacons placed at the corners of one parcel can double as markers for adjacent parcels.<sup>4</sup> In addition, the administrative processes associated with cadastral surveying – everything from consulting with local community leaders to filing papers with the relevant sub-ward, ward, municipality, city council, and ministerial officials – can be done en masse with considerable cost savings.

Interviews with multiple surveying companies in Dar es Salaam produced cost estimates for surveying a single parcel ranging from approximately \$600 at the very low end to upwards of \$3,000 – depending on the size, location, and other complicating factors related to local government administration.

The Tanzanian Ministry of Lands estimates systematic demarcation at scale costs approximately 150,000 to 250,000 per parcel (\$96 to \$160), though the basis for these estimates is somewhat opaque. For the purposes of this randomized field experiment, the World Bank – in collaboration with the Ministry of Lands, the Dar es Salaam City Council, and the Kinondoni Municipality – contracted a private surveying company to produce a cadastral survey of the roughly 1,100 parcels in the treatment area. In addition, because of the large number of parcels affected, the project was also required to contract a certified town planner to produce a detailed map of future, purely hypothetical infrastructure investments in the area (including the boundaries of roads if paved, installation of electric street lights, public water pumps, etc.) that was approved by the Kinondoni Municipal Council. The combined cost of town planning and surveying for the project was considerably below the Ministry’s estimates, at roughly 70,000 shillings per parcel (approximately \$45).

Assessing the fiscal sustainability of systematic land demarcation is slightly more difficult than testing whether the government can sell titles to 100% of demarcated parcels at a price in excess of 70,000 shillings (or 50% above 140,000 shillings, and so on). First, we explore the possibility for price discrimination on the basis of household wealth, which combines progressivity with additional revenue generation. Second, the government’s fiscal calculus must also factor in not just the immediate revenue from the sale of titles, but the discounted present value of future property tax receipts on titled land. We return

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<sup>4</sup>In the simple case of rectangular parcels on a grid, surveying  $n$  parcels requires  $(\sqrt{n} + 1)^2/n$  beacons. Obviously demarcating a single parcel requires four beacons. As  $n$  increases, the number of beacons required per parcel approaches one.

to both of these issues in Section 4.

From a policy perspective, it is important to emphasize that the cost of titling in Tanzania are extremely high, and perhaps artificially so. Similar programs of systematic land demarcation in other settings, including the Rwanda, India (Andra Pradesh), and the Kyrgyz Republic, have achieved costs on the order of a few dollars per parcel (World Bank 2007; Ali, Deininger, and Goldstein 2011). These dramatic cost reductions have generally been achieved by abandoning cadastral surveying in favor of aerial photography and satellite imagery. But bureaucratic red tape are also a significant part of the fixed costs of surveying in Tanzania.

## 2.2 Intra-household bargaining power outcomes

The second question we pose about the grand De Soto-inspired formalization project in Tanzania is whether it will serve to enshrine – or even exacerbate – patriarchal norms underlying customary land tenure. We begin by reviewing the existing evidence relating land tenure formalization to shifts intra-household bargaining power.

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 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. Recent work by Doss, Kim, Njuki, Hillenbrand, and Miruka (2014) reveals that, in Tanzania, women who report joint-ownership of land are more involved in household decision-making.

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 program 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. Evidence from Rwanda has also shown that titling programs can be successful at increasing perceived female ownership and the recording of

inheritance rights (Ali, Deininger, and Goldstein 2011).

Although 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 formalization 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.

### 2.3 Female land ownership in urban Tanzania

One of the main aims of this experiment is to investigate whether Tanzanian households can actually be induced to co-title their land as part of the formalization process. While there are both theoretical arguments and some empirical evidence suggesting that co-titling actually improves women’s ownership claims and bargaining power, we must first investigate whether, ex-ante, we would expect co-titling to make a difference in the Tanzanian setting. There are several states of the world to consider: the default position of female ownership rights under informality, both in a *de jure* sense and in a *de facto* sense, and how this position changes as households adopt formal titles with only a male spouse as the owner or as a jointly-owned title. We will consider each of these states in turn:

1. **Informality:** Under informality, women’s *de facto* ownership remains unclear. The 1999 Land Act was hailed as being one of the first pieces of land legislation to explicitly recognize the rights of women as landowners (Sundet 2005) and contains several provisions granting ownership rights to women who co-reside with their husbands. However, the language and framing of the Land Act supposes that formalization has taken place and titles have been granted, so it is uncertain whether or not women can actually take advantage of these ownership rights under informality. The state of women’s *de jure* ownership claims prior to formalization appears to be weak at best. Table 1 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 included in a CRO application if one was made. The results, which are restricted to dual-headed households, suggest that women have limited *de facto* rights over land: roughly 13% of households report that a woman is one

of the “default” owners of the land. Women fare a little (but not much) 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.<sup>5</sup>

2. **Titled with male ownership:** The Land Act becomes more salient when formalization has taken place and titles have been issued in the male spouse’s name only, granting women ownership status when they invest in or maintain the land in question as well as giving them rights to block the sale or mortgage of land. However, the Land Act conflicts with older, more established pieces of legislation like the 1971 Law of Marriage act which stipulates that property assigned solely to one spouse cannot be claimed by the other later on.<sup>6</sup>, so it remains unclear whether or not women’s rights are actually binding in a *de jure* sense. Although the move from informal to formal sole male ownership does not necessarily weaken women’s claims to lands (and might, under some circumstances, improve it), evidence to date implies that households tend to cement the status quo during formalization: in the Kinondoni property registrar approximately 70-75% of all land registered with a residential license is done so with a single male name.<sup>7</sup> Similarly, when households in our study sampled were asked who would be included on a full title if they applied for one, only 25% mentioned a women as one of the owners (Table 1).
3. **Titled with co-ownership:** Things become more clear when women are granted co-ownership of the land as part of the titling process. Here the Land Act is quite explicit: co-owners (or occupiers-in-common as they are known) have veto powers over all forms of land dispensation<sup>8</sup>, and because the women has been named as an owner of the property, there is no longer any conflict with the Law of Marriage Act. What is less clear is whether or not co-titling improves the *de facto* state of women’s ownership relative to that of male-titling or informality, a question this experiment ultimately aims to answer.

### 3 Experimental design and 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. Both of these *mitaa* also appear to have noticeably lower

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<sup>5</sup>To 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.

<sup>6</sup>Section 191(2) of the 1999 Land Act and section 58 of the (1971) Law of Marriage Act.

<sup>7</sup>Authors’ calculations using data from the Kinondoni municipal data.

<sup>8</sup>Section 159(6) of the 1999 Land Act.

Table 1: 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.

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%.

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, redeemable only 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, resulting in detailed data on household and parcel characteristics.

Table 2: 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.

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.<sup>9</sup> 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, 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.

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 2, every feasible combination of vouchers was given equal weighting in the randomization.<sup>10</sup>

<sup>9</sup>For Barafu, the total number of blocks was 10, for Kati it was 15.

<sup>10</sup>The 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.

While there were ex-ante concerns that a randomized top-down voucher allocation 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 3 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:

$$x_i = \alpha_0 + \alpha_G v_{Gi} + \alpha_C v_{Ci} + \varepsilon_i \quad (1)$$

where  $x_i$  is the characteristic of interest,  $v_G$  is the general voucher value, and  $v_C$  is the conditional voucher value, expressed in thousands of shillings. We repeat the same exercise replacing the individual voucher values ( $v_G$  and  $v_C$ ) with the net price,  $p$ . 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 3, 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 (a possible proxy for female bargaining power), 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  $\alpha_G$  and  $\alpha_C$ , 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 3: 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 household	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)
Total assets, Log(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)
Parcel Area, Log( $m^2$ )	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  $\alpha_G$  and  $\alpha_C$  from the linear regression of each variable  $var = \alpha_0 + \alpha_G v_{Gi} + \alpha_C * v_{Ci}$ , where  $v_{Gi}$  and  $v_{Ci}$  are the general and conditional voucher values for each parcel  $i$ . 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 Household-level analysis: Pricing and pro-poor targeting

In this section we estimate the demand curve for land titles, exploiting the random variation in prices induced by the voucher experiment. We begin by presenting the econometric estimates of the price- and wealth-elasticity of demand, and then use these econometric estimates to address a range of policy questions.

### 4.1 Price and income elasticities of demand

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

$$q_i = \beta_0 + \beta_p p_i + \beta_x \mathbf{x}_i + \beta_{px}(p_i \times \mathbf{x}_i) + \varepsilon_i \quad (2)$$

In this equation, the dependent variable  $q_i$  is a binary indicator of whether household  $i$  purchased and fully paid for a CRO. The key parameter of interest is the coefficient on  $p_i$ , the randomly assigned price of a title expressed in thousands of Tanzanian shillings, net of all voucher discounts. For all demand estimates, we restrict the effect of voucher values to be linear, which appears to be a reasonable approximation of the underlying data.<sup>11</sup> The vector  $\mathbf{x}_i$  indicates household and parcel-level characteristics from the baseline survey, which will be included in some specifications.

Equation (2) implicitly assumes that general vouchers and conditional vouchers (which require a female co-signatory on the title) can be treated interchangeably in calculating net price offers. The experiment is designed to test this assumption, which is the focus of Section 5. For now we pool all the voucher values to maximize the precision of our estimates of the general price elasticity of demand.

Turning to the results in Table 4, it is reassuring to see the demand curve is significantly downward sloping. Column (1) shows the results from estimating equation (2) without baseline controls. An increase in price of 10,000 shillings reduces the probability of buying a title by 3%, significant at the 1% level. This coefficient is essentially unchanged by the inclusion of socioeconomic controls. Predicted take-up rates are shown for each price level in Figure 1. With no voucher discounts and a maximum price of 100,000 shillings, predicted take-up at mean values of the socio-economic controls is just under 20%. This rises to over 30% at a price of 60,000 shillings and nearly 45% at a price of 20,000 shillings.

Land titles appear to be a normal good, but price sensitivity does not vary much by income. Column (1) of Table 4 shows that an increase of assets or income by one standard deviation increases take-up by 2% and less than 1% respectively, though only the asset coefficient is statistically significant. The interaction between price and either income or

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<sup>11</sup>This can be seen in Figure 3. More formally, table 7 in the appendix displays the results from a series of tests which fail to reject linearity.

Figure 1: Experimental price variation and demand for land titles

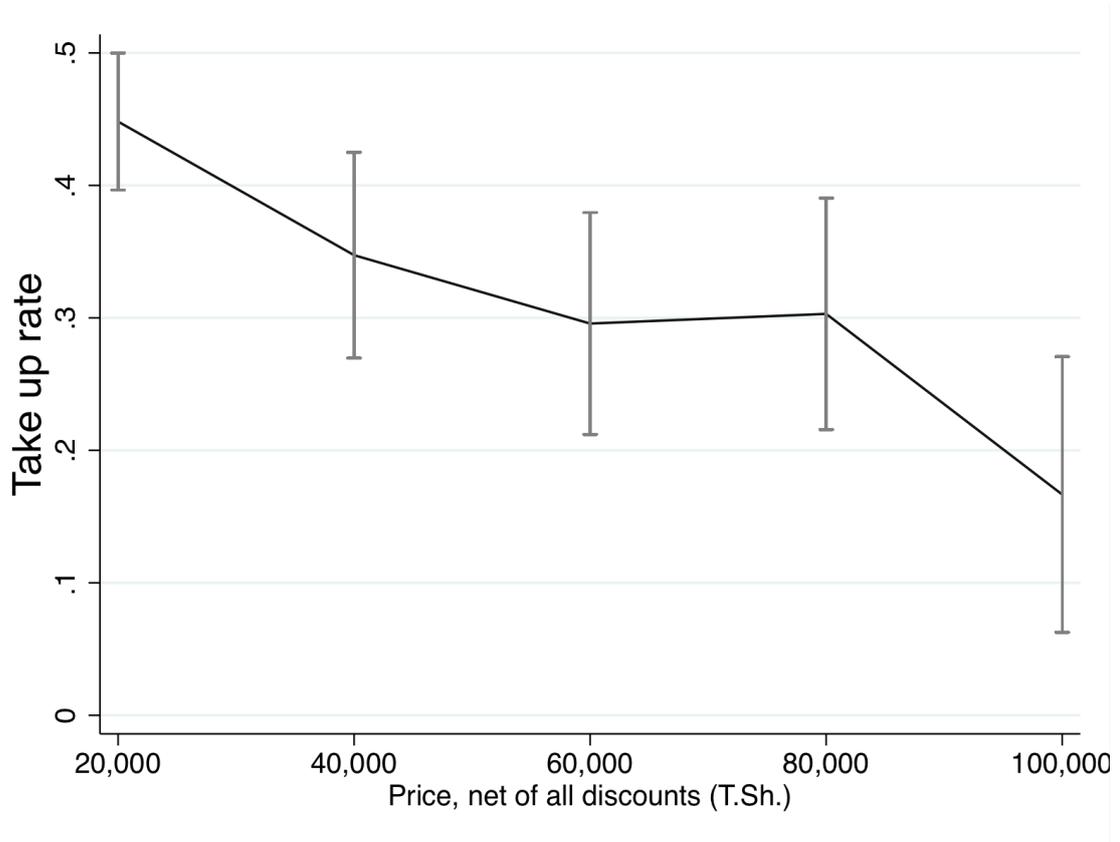


Figure shows estimates of take-up probability, conditioning on price net of all discounts. Bars indicate 95% confidence intervals.

assets is entirely insignificant, with point estimates close to zero. In column (4) we replace income and assets with a combined measure of household socioeconomic status, based on the first principle component of five variables: income, assets, average schooling of the adults in the household, household size, and parcel area. This proxy for socioeconomic status shows no significant effect on take-up and the coefficient on its interaction with price is almost precisely zero. This result, combined with the linearity tests in Appendix A suggest that a linear model with additively separable price and income terms provides a reasonably good approximation of take-up.

## 4.2 Discussion: price discrimination and pro-poor targeting

We now apply our elasticity estimates to a very simple model of a government with a monopoly on the issuance of land titles, which chooses the price of land titles to maximize revenues from sales and property taxes. This somewhat pessimistic framework ignores governments' responsibility to make investments in public goods which may have a long run economic payoff beyond short-term revenues through fees and taxes. Nevertheless, the model's assumptions allow us to provide a positive analysis of the Tanzanian government's titling policies, and to use these demand elasticity estimates to weight the prospects for land titling in urban Tanzania:

Consider the supply decision faced by the Tanzanian Ministry of Lands. We posit that the government is primarily concerned with the direct fiscal revenue from selling titles, and secondly with the increase in future property tax revenues from formalization.

As a monopoly supplier of land titles, the Tanzanian government has the power to set prices and, potentially, engage in significant price discrimination. Based on conversations with Ministry of Lands officials, the government's objective function appears to be well approximated by profit maximization from the sale of land titles and property tax collection.<sup>12</sup> Officials insist that land formalization can only proceed where "full cost recovery" is foreseeable. Note that the implications of profit maximization may be observationally equivalent to a model where titling policy is designed to maximize opportunities for rent extraction by government officials, town planners, and land surveyors.

The costs of large-scale land titling are largely fixed costs. Once the Ministry has decided to systematically demarcate a certain ward or sub-ward, the marginal cost of titling an individual parcel of land quickly approaches zero, driven partially by the simple geometry of doing cadastral surveys of contiguous parcels. The large upfront costs of designing a new town plan with allowances for future roads, parks, and other infrastructure, and of passing the plan through national, regional, municipal, and local political bodies is invariant to the number of properties titled. Thus, conditional on deciding to supply land titles in a given neighborhood, profit maximization is well approximated by revenue

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<sup>12</sup>We are deliberately vague with the term "government." In recent years, some aspects of land formalization in urban areas have been devolved from the Ministry to municipalities. Thus, our model with a unitary decision-maker is a simplification of the actual political economy at work.

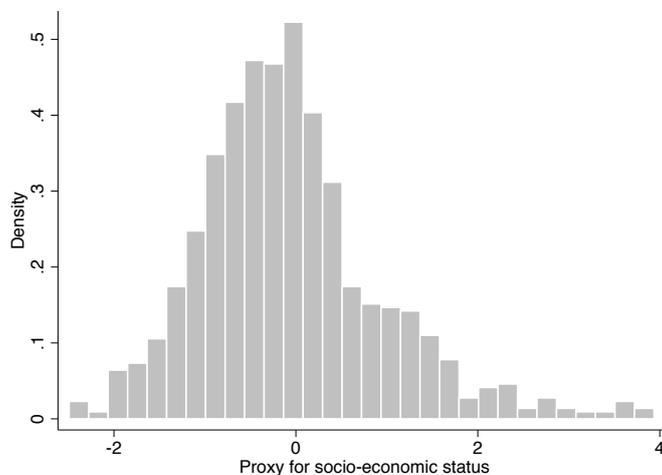
Table 4: Price and CRO adoption

	(1)	(2)	(3)	(4)	(5)	(6)
Price (tsh)	-0.00301*** (0.000503)	-0.00301*** (0.000503)	-0.00300*** (0.000503)	-0.00303*** (0.000502)	-0.00304*** (0.000503)	-0.00303*** (0.000504)
Price × Assets		-0.0000986 (0.000582)				
Price × Income			0.000483 (0.000597)			
Price × Log(Area)				0.000961* (0.000492)		
Price × SES						0.000330 (0.000469)
HH monthly income (std)	0.00759 (0.0111)	0.00770 (0.0111)	-0.0156 (0.0285)	0.00736 (0.0109)		
HH asset stock(std)	0.0279** (0.0140)	0.0329 (0.0288)	0.0273* (0.0140)	0.0281** (0.0140)		
Log(Area)	0.00545 (0.0131)	0.00537 (0.0131)	0.00559 (0.0131)	-0.0405 (0.0275)		
SES Index					0.0356*** (0.0117)	0.0191 (0.0251)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.282	0.282	0.283	0.285	0.281	0.282
Obs	1034	1034	1034	1034	1034	1034

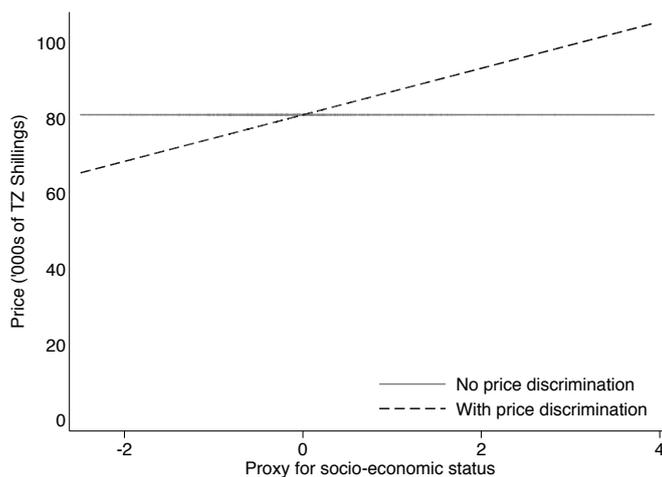
**Notes:** Linear probability model. Dependent variable = 1 if household has fully paid.  
for a CRO. Robust standard errors \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Figure 2: Optimal price and take-up rates with and without price discrimination

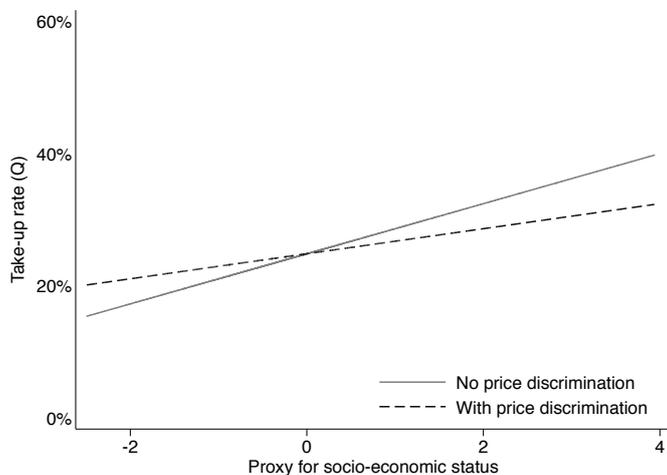
(a) Distribution of socio-economic status



(b) Monopolist's optimal prices by socio-economic status



(c) Predicted take-up at optimal prices



In all three graphs, the horizontal axis measures the socio-economic status of households. Panel (a) shows the distribution of this socio-economic status proxy. Panel (b) shows the optimal price for a revenue-maximizing monopoly supplier of land titles, with (dashed) and without (solid) price discrimination based on socio-economic status. Given the optimal price at each level of socio-economic status, panel (c) shows the predicted level of demand.

maximization, and hinges primarily on the shape of the demand curve. The Ministry’s decision to enter a neighborhood in the first place will hinge on the size of fixed costs relative to demand, which we consider at the end of this section.

As a benchmark, assume the Ministry cannot price discriminate, and faces the demand curve presented in Table 4 in the previous section; we consider the case with price discrimination based on socio-economic status below. The Ministry’s problem is then:

$$\begin{aligned} \max_p R &= p \times Q(p) \\ &= p \times N \times (\beta_0 + \beta_p p_i + \beta_x \mathbf{x}_i) \end{aligned} \quad (3)$$

where price ( $p$ ) is the decision variable, the maximand is revenue ( $R$ ), the total quantity of titles sold is denoted by  $Q$ , and  $N$  is the number of parcels in our treatment area. The notation in the second line refers to the coefficients from equation (2), but we have dropped the final term because the estimate of  $\beta_{px}$  in Table 4 is indistinguishable from zero. Maximizing (3) with respect to price yields the Ministry’s optimal price, assuming no price discrimination:

$$p^* = -\frac{1}{2} \frac{\beta_0 + \beta_x \bar{\mathbf{x}}}{\beta_p} \quad (4)$$

where  $\bar{\mathbf{x}}$  denotes the sample average. According to Table 4,  $\hat{\beta}_0 = 0.5$  and  $\hat{\beta}_p = -0.003$ . This implies a value of  $p^*$  in equation (4) of approximately 80,000 shillings (\$51). At this price, the estimates in Table 4 suggest that take-up would be approximately 25% overall, and would be considerably lower for poorer households relative to richer ones.

Now consider the case with price discrimination. Lower willingness to pay among poorer households implies that a progressive pricing scheme – in which poorer households pay less – will generate more revenue.<sup>13</sup>

The solution with price discrimination is shown by the dashed black lines in Figure 2: the optimal price in panel (b) is progressive, beginning at about 67,000 shillings for the poorest households and rising to over 100,000 shillings for the richest (compared to the flat, solid gray line at 80,000 shillings without price discrimination). This price schedule restores some balance in the socioeconomic distribution of households demanding land titles. Turning from optimal price to predicted take-up, the pattern of demand with a fixed price ranges from around 16% for the poorest 1% to roughly 40% for the richest 1% as shown by the solid gray line in panel (c). Price discrimination flattens this slope somewhat so that it ranges from 21% to 32%.<sup>14</sup>

<sup>13</sup>To reflect this,  $\mathbf{x}_i$  replaces  $\bar{\mathbf{x}}$  in the numerator of equation (4), thus the optimal price with price discrimination is

$$p^* = -\frac{1}{2} \frac{\beta_0 + \beta_x \mathbf{x}_i}{\beta_p}$$

. This is the formula used to calculate the prices shown in Figure 2 with price discrimination.

<sup>14</sup>Note that our discussion here ignores a second, potentially important source of government revenue affected by land titling: property tax revenue. In the appendix, we explore the implications of including future tax revenue in the Ministry’s problem and show that under fairly conservative assumptions – i.e.,

Lastly, we return to the issue of fixed costs and the Ministry’s initial supply decision. Having calculated optimal prices and demand at those prices, we can assess whether, given the high fixed costs of doing systematic demarcation and overcoming bureaucratic red tape, a profit-maximizing Ministry should supply titles to our study area in the first place.

The answer is clearly ‘no’. As noted above, the project expended roughly 70,000 shillings per parcel. The optimal price without price discrimination was about 80,000 shillings, but with an average take-up rate of just 25%. The calculations are not radically different with price discrimination. The average revenue per parcel over the whole sample under price discrimination is just over 21,000 shillings. Even if we look only at the parcels with wealth levels four standard deviations above the mean in our sample, the average revenue is under 40,000 shillings. In short, there is no sign of any way to make land titling financially viable for a revenue-maximizing government supplier without dramatically reducing the costs of demarcation.

To summarize, well-identified estimates of the price-elasticity of demand for land titles allow us to answer three questions within the context of a simple monopoly supplier model.

1. Are titles too expensive? Clearly the answer is “yes” for many Tanzanians, but the price may also be too high from the government’s point of view. Even if the Tanzanian government remains committed to “full cost recovery” in the sale of land titles, our results suggest the optimal price from a monopolist’s perspective is approximately 84,000 shillings – a small fraction of the price currently faced by individual Dar es Salaam residents seeking land titles, and significantly less than the Ministry of Lands charges in large-scale demarcation efforts.
2. Should pricing be more progressive? From a strict revenue maximization perspective, our results show the government’s optimal policy involves price discrimination, charging higher prices to wealthier land owners. But the scope for revenue-maximizing price discrimination is limited. The optimal price for the poorest land owners is only about a third lower than the optimal price for the richest land owners in our study area.<sup>15</sup>
3. Is the current approach to titling fiscally sustainable? The previous questions ignore the large fixed costs of systematic demarcation. Acknowledging these fixed costs, our results suggest the Tanzanian government would make a net loss on the sale of land titles in our study area, even if charging an optimal monopolist’s price. Furthermore, even in much more affluent areas – shifting the wealth distribution

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assumptions that would tend to tilt the solution away from pro-poor price discrimination – our main conclusion here holds.

<sup>15</sup>For the sake of establishing an analytical result, we have employed the best available measures of socio-economic status, including monthly incomes and household assets. When translating these results into policy advice, a more practical system of progressive pricing might be restricted to targeting on observable parcel and housing characteristics.

in our sample up by two or more standard deviations – issuing land titles with the current cost structure would still yield net losses to government coffers.

Collectively, these results suggest the Tanzanian government is overcharging for land titles, even from a perspective of narrow financial self-interest, *conditional* on choosing to pursue land titling at all. But we interpret the third result, in particular, as evidence that a sustainable titling program will require cheaper technologies for demarcation and a reduction in the bureaucratic obstacles which drive up costs. These high fixed costs, rather than demand constraints or low confidence in the value of titles as currently issued, appear to be the main obstacle to wider land formalization.

## 5 Intra-household analysis: Incentives for gender equity

So far we have treated households as unitary decisionmakers, deciding whether or not to buy a land title in response to a randomized price offer. We now turn to intrahousehold issues and test (a) whether price discounts that are made conditional on female co-titling are less effective as stimulating demand for titles, and (b) whether these conditional price discounts significantly raise the co-titling rate.

### 5.1 Demand results

We begin by repeating the specification in equation (2), but decomposing the price variable  $p_i$  into its general and conditional voucher components,  $g_i$  and  $c_i$ .

$$q_i = \beta_0 + \beta_G v_{Gi} + \beta_C v_{Ci} + \beta_x \mathbf{x} + \varepsilon_i \quad (5)$$

In this equation,  $v_{Gi}$  and  $v_{Ci}$  are the levels of general and conditional vouchers which household  $i$  has been allocated, expressed in thousands of Tanzanian shillings, such that  $p_i = 100,000 - v_{Gi} - v_{Ci}$ .

The main hypothesis to be tested here is whether households place less value on conditional vouchers, i.e., whether  $\beta_g = \beta_c$ .

The results of estimating equation (5) are presented in columns (1) and (2) of Table 5. Because the variables are expressed as discounts rather than prices, we anticipate positive coefficients on these variables. As before, the sample is restricted to households in treatment blocks with baseline data available.<sup>16</sup> Columns (1) and (2) show the results from estimating equation (5) first without, then with baseline controls respectively.

Households appear to be equally responsive to conditional vouchers. At the bottom of Table 5, “Test 1” reports the p-value from the linear test of  $\beta_G = \beta_C$ , revealing that we can

<sup>16</sup>Households 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. Results do not differ when households without baseline are included.

Table 5: Effect of voucher distribution on CRO adoption and co-titling

	Take-up		Co-titling		Net women	
	(1)	(2)	(3)	(4)	(5)	(6)
General voucher value (tsh '000)	0.00257*** (0.000614)	0.00278*** (0.000611)	-0.00127 (0.00114)	-0.000782 (0.00123)	0.00103** (0.000513)	0.00111** (0.000508)
Conditional voucher value (tsh '000)	0.00309*** (0.000572)	0.00320*** (0.000561)	0.00341*** (0.00102)	0.00364*** (0.00107)	0.00212*** (0.000489)	0.00217*** (0.000485)
Constant	0.476*** (0.0370)	0.461*** (0.0367)	0.750*** (0.0607)	0.735*** (0.0654)	0.331*** (0.0320)	0.330*** (0.0318)
Baseline controls	No	Yes	No	Yes	No	Yes
Test: $\beta_G = \beta_C$	0.387	0.482	0.00000398	0.0000223	0.0355	0.0424
$R^2$	0.256	0.283	0.105	0.126	0.208	0.220
Obs	1034	1034	261	261	1034	1034

**Notes:** Linear probability model. Columns 1-2: Dependent variable = 1 if household has fully paid for a CRO. Columns 3-4: dep var = 1 if household has included a woman as co-owner on CRO application, conditional on taking up and filling out application filling one out.

Columns 5-6: dep var = 1 if household has included woman on CRO application, zero if any other action taken. Test displays the p-value from a linear test of the hypothesis that general conditional voucher coefficients are equal. Robust standard errors

\* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$

Figure 3: Voucher values and take-up rates

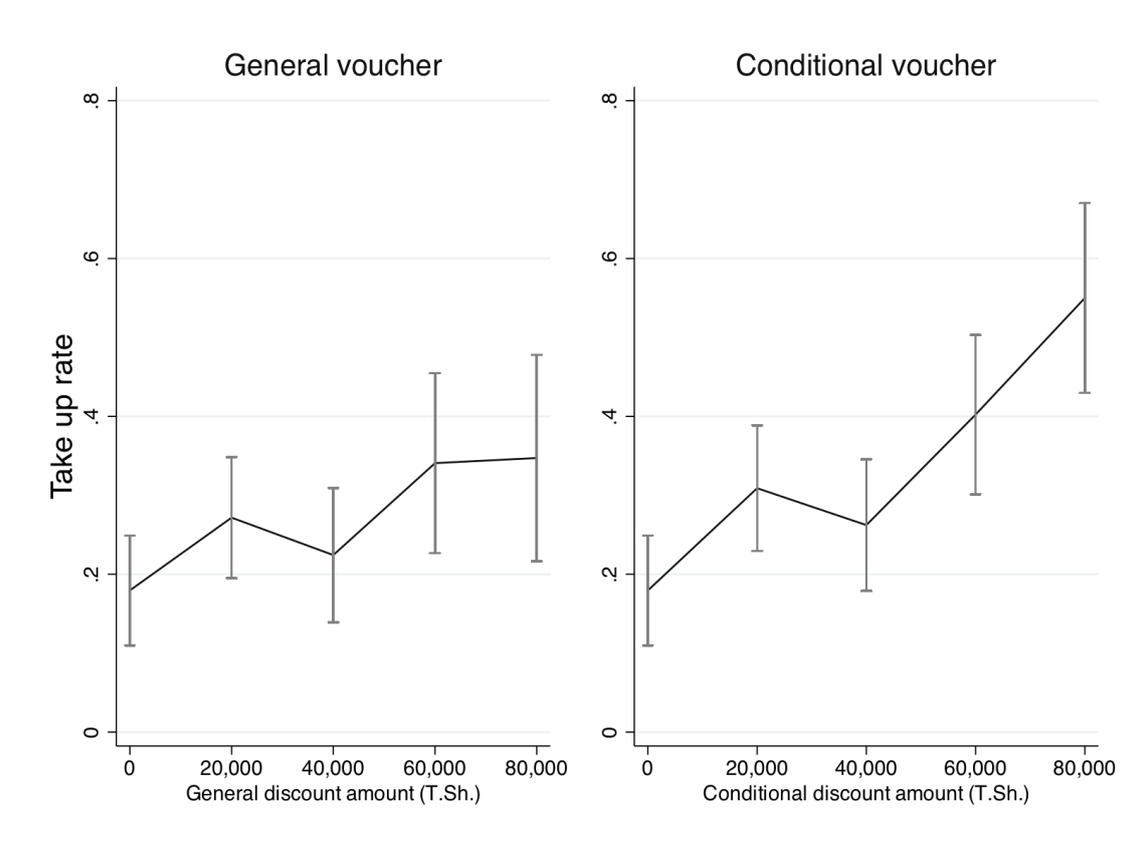


Figure shows estimates of take-up probability, conditioning on general conditional voucher values. Bars indicate 95% confidence intervals. (Note that while the x-axis in Figure 1 measures price net of voucher discounts, the x-axis here in Figure 3 measures the size of the discount.)

comfortably accept the null that these two coefficients are equal across all specifications.<sup>17</sup> The results here strongly suggest that households treat conditional vouchers as ‘cash’: that is, they do not 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 3 displays estimated take-up levels for each voucher type. While the pattern of take-up across each value differs slightly between general and conditional vouchers, they do not appear to be significantly different.

## 5.2 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

<sup>17</sup>The failure to reject the null is not driven by imprecision, as the coefficients displayed here are precisely estimated.

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.

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 (5), using this variable as our outcome of interest:

$$cotitle_i = \gamma_0 + \gamma_G v_{Gi} + \gamma_C v_{Ci} + \gamma_x \mathbf{X} + \varepsilon_i \quad (6)$$

Ideally, equation (6) should be estimated over the full sample of households who have chosen to purchase a CRO. However, to date approximately 30% of households who have finished payment on a CRO have yet to finish filling out an application, thus our analysis of application decisions covers a smaller number of households.<sup>18</sup>

Columns (3) and (4) of Table 5 show the results from estimating (6). The general voucher has no detectable effect on co-tilting, while the conditional voucher has a positive and highly significant impact. The magnitude of this effect is fairly large: each 10,000 TSh subsidy results in an increase in the predicted probability that a woman is included by 3.4 percentage points. When baseline controls are included, this point estimate increases slightly to 3.6.

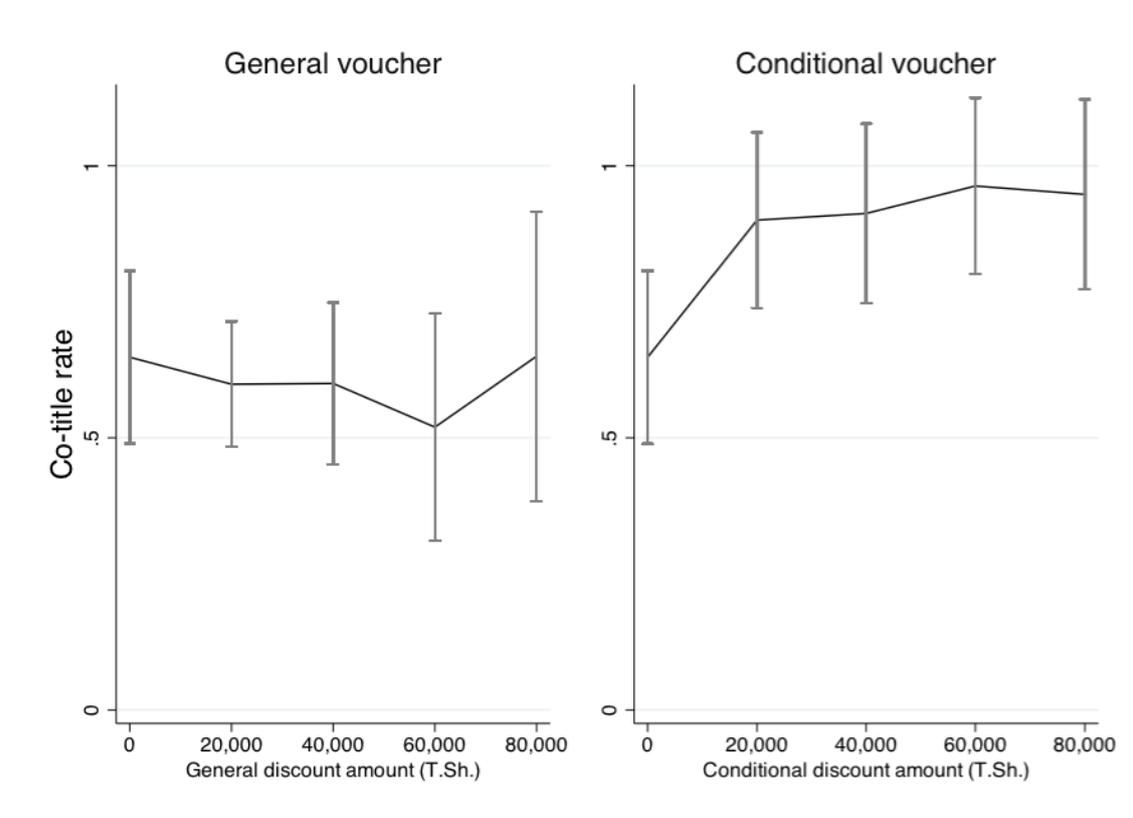
An interesting and important feature of the results in Table 5 is the size of the constant term in columns (3) and (4). In the absence of any voucher discount, nearly 75% of buyers list a woman as co-owner on title application, although this high ‘control’ co-titling rate is in part a result of our linear specification.<sup>19</sup> This is compared to just 13% of households who report a woman as one of the owners of the parcel during the baseline survey. While not identified experimentally, this suggests a large, across the board increase in the demand for female co-titling as a result of the intervention. As noted above, the intervention was coordinated by a non-governmental organization strongly committed to promoting female empowerment, an organization founded and directed by a prominent female Tanzanian politician. The female ownership rights guaranteed under the 1999 Land Act are a core theme of the NGO’s public activities. Thus it is no surprise that female co-titling was high at all voucher values. While this is no threat to the experiment per se, in that all households received this marketing pitch in favor of female ownership, it may place an upper bound on the size of the impact of conditional vouchers that we

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<sup>18</sup>While we will proceed as if the determinants of application choices being observable are random, it is possible that non-random selection of households choosing to fill out an application could lead to bias. Re-estimating (6) using a basic sample selection model does not change the main results here (tables available on request).

<sup>19</sup>Nonlinear specifications, such as including each voucher amount separately as an indicator variable, result in a ‘control’ co-titling rate closer to 60-64% This lower rate is observed in Figure 4

Figure 4: 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. Bars indicate 95% confidence intervals. In a nonlinear specification where an indicator variable for each voucher value is included, for each positive conditional voucher value separately we can reject the null of no differential effect from no subsidy ( $\gamma_C = 0$ ) or from the same general voucher value ( $\gamma_C = \gamma_G$ ).

can detect. It is hard to raise female co-titling too much when starting from such a high floor.

There is also some evidence that a linear specification (which was not rejected for the demand equation) may be too restrictive here. This is illustrated in Figure 4, where co-titling rates are graphed against voucher values. 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 even very small conditional vouchers.

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 (5) and (6) of Table 5 display the results. Again, below the main results, ‘Test’ reports a linear test of the null hypothesis that the two vouchers have equal effects. The null is rejected at the 5% level. While general vouchers have a small positive effect on the total number women listed on titles (operating through the general increase in titling), conditional vouchers raise the number of women listed on titles by roughly twice as much.

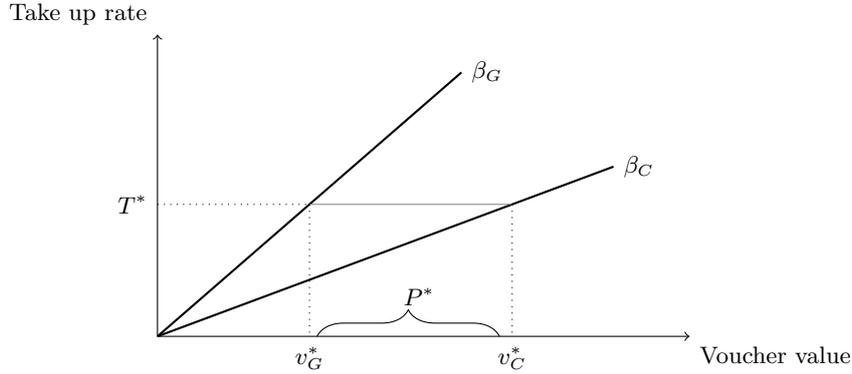
In summary, while the basic intervention was itself successful on improving the status quo, imposing conditionality dramatically increases female titling. Furthermore, this expansion in female access appears to come at no cost in terms of reduced demand for titling overall.

### 5.3 Discussion and heterogenous effects

Reconsider the linear probability model (5) used to estimate the demand results in the previous subsection, equation (5).

In a very simple world where households receive some disutility from co-titling, we would expect estimates of  $\beta_G$  and  $\beta_C$  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 be transferred to households to offset the decline in demand caused by conditionality. Figure 5 illustrates this relationship: for a desired level of CRO take-up  $T^*$  and linear demand effects of general and conditional vouchers  $\beta_g$  and  $\beta_c$ , 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.

Figure 5: The price of “empowerment”



The results from the randomized voucher intervention have shown us that, given our estimates of  $\beta_G$  and  $\beta_C$  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 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 6 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 households with at least one adult man and woman (henceforth, 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.

In sum, we find that even small price incentives lead to nearly uniform co-titling, with no reduction in demand for titles. One interpretation of this result is that men are easily persuaded to sacrifice long-term bargaining power over household land in exchange for short-term cost savings. An alternative explanation is that co-titling actually has no such long term explanations. We have showed that households where women have stronger bargaining power are more likely to co-title, suggesting that this decision is taken seriously. We see this as suggestive, but far from conclusive, evidence that the stakes from co-titling are real. Ultimately, the resolution to that question will depend on the results of longer-term follow-up to this randomized titling intervention.

## 6 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 to these general price discounts, we reported impacts on overall demand for titling and female co-titling from conditional vouchers that required households to include a woman on the land title application in order to apply the full discount.

Willingness to pay for land titles is, on average, between \$40 and \$50. This is high as a proportion of owners' incomes, but low relative to the unit costs of cadastral surveying required to produce legal titles. In short, demand is not sufficient to allow the Ministry of Lands to sell titles at a profit in low-income, unplanned settlements in Dar es Salaam. However, once the Ministry incurs the large fixed costs of cadastral survey work for a given neighborhood, our results suggest the Ministry would increase revenues by engaging in price discrimination to offer lower prices to low-income households.

Turning to the gender dimension of land titling, 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 the requirement of co-titling women. Con-

Table 6: 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		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)		0.00374*** (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)		0.00412*** (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)		-0.0377 (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)		0.000182 (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)		-0.000679 (0.00174)		-0.00879*** (0.00277)		-0.00133 (0.00158)
Constant	0.436*** (0.0493)		0.466*** (0.0526)		0.680*** (0.108)		0.333*** (0.0454)		0.431*** (0.0613)		0.545*** (0.128)		0.295*** (0.0506)		0.436*** (0.0545)		0.572*** (0.123)		0.436*** (0.0545)		0.572*** (0.123)		0.296*** (0.0464)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	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		0.680		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		0.773		0.0359		0.432
$R^2$	0.279		0.283		0.190		0.233		0.275		0.275		0.244		0.281		0.218		0.281		0.218		0.228
Obs	603		603		166		603		519		143		519		597		163		597		163		597

**Notes:** All columns show a separate linear probability model. For each section of the table (columns 2-4, 5-7, 8-10),  $X$  is defined by the column group heading.

In columns labeled "take-up", the dependent variable = 1 if household has fully paid for a CRO. In "co-titling" columns, the dependent variable = 1 if a woman is listed as owner or co-owner on a CRO application. In "net women" columns, dependent variable measures the total number of women in the household listed on a title, combining take-up and co-titling effects. Robust standard errors. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

ditional 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" for women in Dar es Salaam appears to be very low raises new concerns. For instance, 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 rounds of this research project will take advantage of follow-up data to determine whether or not co-titling results in any palpable changes in women's welfare over a longer time horizon.

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## A Extra figures and tables

The appendix includes additional material on two topics: a test for linearity of our main specification, and an extension of the model in Section 4 to test whether our conclusions are robust to the inclusion of future property tax revenues in the Ministry’s optimal price calculations.

First, we test the linearity of the relationship between price and the probability of take-up, i.e., the specification from equation (2). To do so we test the null that the effect of, say, a 40,000 shilling discount is twice as large as the effect of a 20,000 shilling voucher, and similarly for all other pairwise combinations of vouchers. The p-values from these tests are shown in Table 7. As seen, we fail to reject in every case.

Table 7: Test of linearity assumption of voucher impacts

	20	40	60	80
General voucher				
20	NA	0.32	0.74	0.48
40	0.32	NA	0.34	0.73
60	0.74	0.34	NA	0.54
80	0.48	0.73	0.54	NA
Conditional voucher				
20	NA	0.30	0.71	0.85
40	0.30	NA	0.23	0.12
60	0.71	0.23	NA	0.65
80	0.85	0.12	0.66	NA

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  $2 * \beta_{20} = \beta_{40}$ .

Second, we return to the Ministry’s optimal pricing calculations. The Ministry’s problem as depicted in equation (3) ignored future revenues which the government would receive as a consequence of land titling through increased property taxes.

In principle, all land owners in Dar es Salaam are liable for property taxes assessed on a per-square-meter basis, regardless of whether or not they hold a formal title. But tax compliance is low, and owners with title have greater incentive to pay their taxes if they hope to be able to use that title for any administrative transaction (collateral, sale, etc.).

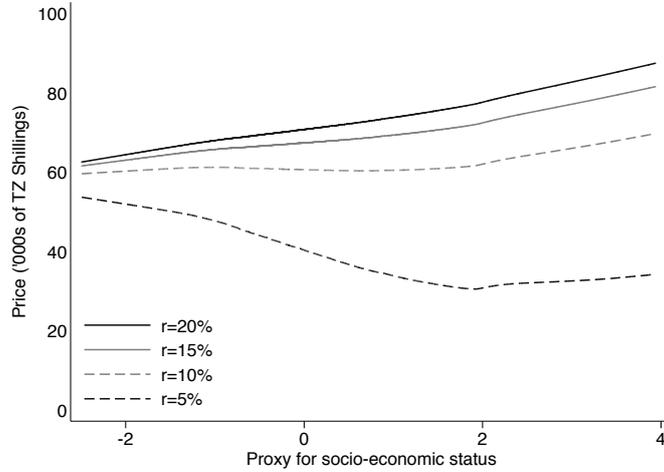
In general, including tax revenue in the Ministry’s problem will reduce the incentive to price discriminate in favor of the poor – as poor households tend to live on smaller parcels which generate less tax revenue. To take an extreme assumption, suppose tax compliance goes from zero to 100% upon formalization of tenure. Let  $\tau$  denote the annual tax bill for an average parcel, about 4,000 shillings (\$2.55) in our data, and let  $r$  denote the annual interest rate the Ministry uses in present value calculations. The Ministry’s problem is then:

$$\max_p R = \left[ p + \frac{\tau}{r} \right] \times N \times (\beta_0 + \beta_p p_i + \beta_x \mathbf{x}_i)$$

which yields

$$p^* = -\frac{1}{2} \left[ \frac{\beta_0 + \beta_x \mathbf{x}_i}{\beta_p} + \frac{\tau_i}{r} \right]$$

Figure 6: Optimal price including tax implications at various discount rates



Compare to Figure 2b. The horizontal axis measures the socio-economic status of households. All lines show the optimal price for a revenue-maximizing, price-discriminating monopoly supplier of land titles. Each line uses a different discount rate, shifting the weight placed on future property tax revenues. Note the price schedules are not linear because taxes are calculated on land area, which is imperfectly correlated with our socio-economic status index.

where  $\tau_i$  is a function of parcel area. Note that these calculations take the tax regime as fixed; the Ministry might instead choose to set both the price of land titling and the property tax rate simultaneously. As we could not experimentally vary tax rates, we cannot estimate their impact on take-up.

In contemplating price discrimination, the Ministry must now weigh two countervailing issues. On the one hand, lower willingness to pay among poorer households favors a progressive pricing scheme. On the other hand, poorer households tend to generate less tax revenue. The balance between the progressive and regressive tendencies of price discrimination hinges on the discount rate applied, as shown in Figure 6. A higher interest rate will lead to more progressive pricing, as the Ministry will prioritize the immediate gains from attracting more poor buyers over the long-term tax revenue of attracting more affluent buyers. The tipping point between an absolutely regressive and progressive pricing scheme is at an interest rate of approximately 10% per annum, where the optimal price line is roughly flat in Figure 6.

The actual cost of capital to the Tanzanian government is arguably quite high. As of late 2011, the nominal interest rate for seven-year Treasury bonds issued in Tanzanian shillings was 15%. At this rate, the optimal price remains quite progressive, but somewhat less so than the results in the main text. The price rises from around 60,000 shillings at the low end up to approximately 80,000 for the richest households. Thus, despite assuming that tax compliance will jump from zero to one-hundred percent upon titling, our main conclusion about the progressivity of optimal pricing holds.