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Falling Off the Map: The Impact of Formalizing (Some) Informal Settlements in Tanzania

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Abstract

When the Tanzanian government formalized over 200,000 informal land claims by granting leasehold titles to residents of unplanned settlements in Dar es Salaam in 2004, a few neighborhoods in the initial plan were excluded due to missing satellite photos. We examine the impact of this low-cost, large-scale titling intervention a decade later in a regression discontinuity design using new survey data collected on either side of the arbitrary boundary created by the missing photos. We find significant, positive effects on housing investment, and indicative but not statistically robust increases in tenure security and reductions in land sales. There is no evidence that titles improved access to credit markets.

JEL classification: J16, K11, O12, O18, Q15
Keywords: land titling, formalization, natural experiment, Tanzania

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1 Introduction

Africa is urbanizing much more rapidly than the rest of the world, at a faster pace than its income growth levels would predict (Henderson et al., 2013). Despite the fact that new land administration systems were launched in nearly two-dozen African countries over the two past two decades, most of these systems have proven to be too costly and impractical to implement with any success (Alden Wily, 2003; Deininger et al., 2008). As a result of sustained informal growth, unchecked by competent land administration, over 60% of the urban African population lives in informal settlements, or slums (UN-HABITAT, 2010). These trends pose a basic policy challenge to many African governments: how to provide housing, public services, and basic governance to the swelling populations of peri-urban slums.

At the same time, the economics literature suggests that bringing secure, formal property rights to urban landowners would bring about a number of substantial welfare benefits, some of which have been documented empirically (Field, 2003; Galiani and Schargrodsky, 2010). These benefits include the often-cited "de Soto" hypothesis, the argument that extending property rights to the poor makes land more easily collateralizable and lowers the barriers to accessing formal credit (De Soto, 2000). Other explanations include the possibility that formal property rights make land markets more fluid and ease the costs of transferability, as well as creating investment incentives for landowners through reducing the risk of expropriation (Besley, 1995).

In this paper we examine the effects of the Tanzanian government's ambitious attempt to deal with the informality problem and reap some of the benefits of titling: a large-scale effort launched in 2005 to bring millions of urban residents onto the formal land registry for the first time by issuing land titles known as "residential licenses" that were sold quite cheaply (, without the costs and delays of cadastral surveying), identified the rightful owner of the land, provided a guarantee against government expropriation for a fixed term (initially five years), but were not transferrable and, hence, could not be foreclosed upon by banks.

To identify the effects of this policy, we exploit a unique natural experiment. The land registry was drawn on the basis of satellite images of Dar es Salaam. In a few isolated parts of the city, satellite images were unavailable for technical reasons. Administrative units which were scheduled to be included, but were covered by missing maps, were omitted from the registry in 2005, and thus remained ineligible for residential licenses almost a decade later when our data was collected in 2014. We exploit the sharp geographic cut-off between eligible and ineligible administrative areas to estimate the effect of land titling in a geographic regression discontinuity design.[1] In terms of methodology, our paper follows a growing tradition of using spatial regression discontinuity designs to evaluate public policies,

[1]To address the possibility that results are driven by other, unrelated effects of belonging to a specific administrative unit, we exploit an addition quirk to our natural experiment: a large section of our control sample was reassigned to the same administrative unit as the treatment area subsequent to the initial roll-out, but the land remained ineligible for titling, allowing us to isolate the pure effect of eligibility.

initially dominated by work on outcomes in the United States and other rich countries (Card and Krueger, 2000; Black, 1999), but has since become more popular with research in development settings (Dell, 2010; Magruder, 2013; Ali et al., 2014).

We find evidence of modest effects of the residential license program on land investment as proxied by construction and improvements to existing structures. There is also weak evidence that residential licenses improved the (real and perceived) tenure security of parcel owners, although might have also slowed down the land market, although these results are not robust to every specification. Outside of these results, we find little compelling evidence that, nearly ten years after the introduction of the program, that there have been any substantive impacts on most of the broad range of outcomes we consider, including access to credit.

This paper is, to our knowledge, unique in applying a quasi-experimental design to test the impact of property rights in an urban, African context. Most existing studies on the impact of property rights in an African context (Besley, 1995; Goldstein and Udry, 2008) have focused on rural agriculture, and on the reduced-form effect of property rights on investment. Methodologically, these findings rely on variation in self-reported tenure security, or political connections that affect perceived security, rather than specific policy levers – an exception being Ali et al. (2014).

Our paper is most similar to previous work on natural experiments in urban land titling from Latin America, which find effects of land titling on labor supply in Peru (Field, 2007) and housing investment in Peru and Argentina (Galiani and Schargrodsky, 2010; Field, 2005). But there are reasons to expect important differences between our study context and those documented in the Latin American literature. In both the Peruvian and Argentine natural experiments, residents faced extremely heightened risks of expropriation and/or theft. In Argentina, squatters in the sample were subject to an active legal effort in the courts to evict them by the putative owners of the land. Reluctance to make sunk investments in housing without secure title in this context may be unsurprising but not representative. In the Peruvian case, the peri-urban slums grew rapidly through an influx of internally displaced persons during the country's civil war, and law and order broke down in many communities, necessitating a large element of 'guard labor' which secure property rights appear to have reduced. Anecdotally, the legitimate risk of expropriation in peri-urban Tanzania appears considerably lower given that unplanned settlements have expanded primarily through economic migration and high urban fertility (rather than civil warfare), and there are no large-scale private claimants to the land in question.

The rest of the paper is organized as follows. The following section introduces the natural experiment and briefly reviews the legal, political, and historical context around land tenure formalization in urban Tanzania. Section 3 presents a bespoke household survey conducted by the authors, targeting the area of the natural experiment, and discusses our econometric specification and the outcomes we will consider. Section 4 discusses the results and Section 5 concludes.

2 Background and context of natural experiment

2.1 Tanzanian land reform and the residential license program

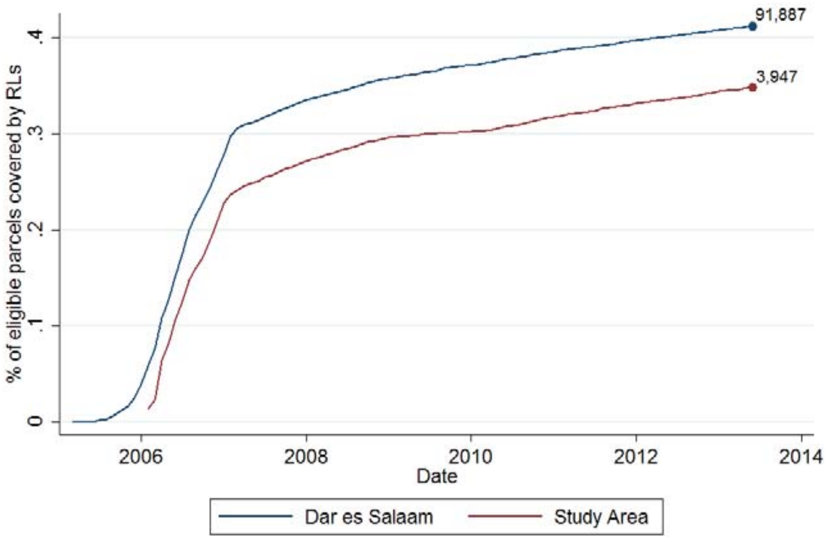
The Government of Tanzania was openly hostile towards informal settlement in its cities for most of its post-independence history, occasionally going as far as to forceable relocate residents to rural *Ujaama* villages (Kironde, 2006). This began to change in the early 1990s as Tanzania, along with a number of other sub-Saharan countries, began to consider comprehensive land reform in order to better incorporate both urban/informal and rural/customary tenure into a modern system of land administration (Alden Wily, 2003; Sundet, 2005). The resulting legislation, known as the 1999 Land Act, not only introduced a new system for individualized land tenure intended for informal landowners but also established a comprehensive framework for land compensation, sales and mortgage.

While the Land Act took significant steps to recognize the existing rights of informal landowners, more practically it introduced two new forms of formal land tenure for residents to acquire. The first is known as a Certificate of Right of Occupancy (CRO), a 33-99 year leasehold of urban land, currently the closest any individual landowner can come to holding a full freehold title in Tanzania. CROs, which were designed to be fully transferable and intended to be used on the mortgage market, are seen as the ultimate goal of formalization by the Tanzanian government. However, to date there has been very little progress in extending CRO access to informal residents, aside from a handful of medium-scale registration exercises in Dar es Salaam carried out by NGOs or the government. High fees, complex perquisites including a cadastral survey and back-pay for all outstanding property taxes, long wait times and substantial bureaucracy have prevented most landowners from acquiring titles.

To create a stepping-stone to full tenure, the 1999 Land Act also introduced a second, intermediate form of formal tenure known as a residential license (RL), which will be the focus of this paper. In contrast to CROs, residential licences are cheap and easy to obtain: an average license costs between 10-20,000 tsh, and the only perquisite required is for the parcel to already be covered in the Municipal land registry (more on this below). However, the stated benefits of RLs are less attractive than CROs: initially, a residential license must be renewed ever two years, alter to be extended to five. They are not legally transferrable, although the purchaser of an unregistered parcel can, in theory, apply for a new one. Residential licenses were also not designed to be used as collateral in the local credit market, and initial reports on their use for loan-access indicated that most financial institutions were hesitant to extend loans based on a title which only lasted two years (Shemdoe, 2012).

Following an extension of RL durations from two to five years, many microfinance institutions and some small banks appeared to reverse their policies, not only allowing residential licenses to be accepted as a form of collateral but also, on occasion, allowing borrowers to

Figure 1: Residential license take-up over time



The map above displays the cumulative percentage of parcels in the land registry covered by a residential license (i) city-wide and (ii) in our study area. Figures in the top right indicate the final # of parcels registered in each region.

access higher loan amounts.[2] Anecdotal reports on the use of RLs for collateral are reinforced by a rise in their registration as collateral with Dar es Salaam's municipal governments over the past few years, with records showing that approximately 2% of all registered RLs.[3] However, even though there is clear evidence that RLs are being used as collateral, this is not enough evidence to suggest that they actually facilitate the access of credit, as they could easily be displacing other forms of collateral, such as purchase agreements, or shifting households from informal to formal borrowing.

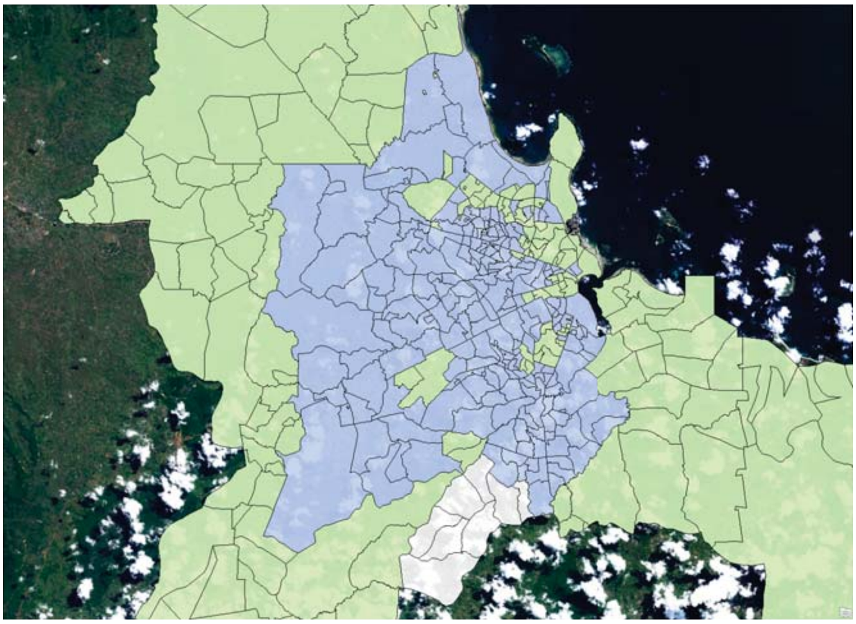
The government's efforts to develop the formal tenure system around these two forms of title comprised two separate efforts: the responsibility of divining a path towards complete the long term goal of registering all land with a CRO fell to the Property and Business Formalisation and Registration Programme (MKURABITA), using a model developed by Hernando de Soto's Institute for Liberty and Democracy (ILD). Concurrently, the Ministry of Lands took on the intermediate step of getting all land provisionally registered under the residential license system, albeit with input from MKURABITA Kironde (2006).

To create a land registry against which residential licenses could be issued, the Ministry of

[2]Based on discussions between the authors and loan officers from ACCESS Bank, AKIBA, PRIDE and NMB Bank

[3]To date, the Kinondoni and Temeke municipalities report over 1,000 RLs registered as collateral. Most landowners, however, do not appear to be following official procedure, so this count is likely to be an underestimate

Figure 2: Phase One of the Residential License Programe



The map above displays the entire DSM city boundary, broken down by (subward). Areas designated to be included in Phase I of the residential license program are highlighted in blue. The area designated to be included in Phase 1 which was excluded due to lost maps is highlighted in white.

Lands embarked on the first phase of an ambitious land registration program in 2004. Using a combination of satellite imagery and aerial photography, land officers visually identified land parcels in informal settlements, then followed up on the ground to establish ownership and map out parcel boundaries. This first phase covered approximately 260,000 parcels of land, extending into what was, at the time, the periphery of the city. Residential licenses were first made available the following year and to date approximately 40% of informal landowners covered by the program have acquired to them. While demand surged as they were initially made available, current trends in take-up suggest that full coverage will likely never be achieved. Figure 1 displays residential license take-up over time both across the entire city and within our study area.

2.2 A natural experiment created by lost maps

The Ministry of Lands planned to roll out the residential license program in two phases, the first covering all urban informal settlements, with the second covering all peri-urban land on the Dar es Salaam periphery. Figure 2 shows the total area designated as Phase I in 2004. Before the Ministry could establish a land registry for these areas, it first needed detailed satellite imagery to provide land officers with an accurate map of existing settlements.

The Ministry sought to acquire updated satellite maps from QuickBird, a high resolution satellite, through an intermediate Tanzanian GIS firm. However, while the Ministry requested satellite imagery for the entire Phase I area, the order was returned incomplete with several grid maps missing, predominantly those on the periphery.[4] Rather than pay for the acquisition of more maps, the Ministry of Lands opted instead to wait until a series of scheduled flights took place to take aerial photographs of the Phase II areas. However, these flights never took place due to inclement and overly cloudy weather, and eventually funding for Phase II fell through. As a result of the lack of satellite imagery, the Ministry was unable to create a land registry for several subwards in southern Dar es Salaam which already had been selected for Phase I of the project. Thus even though these subwards met the same criteria for selection into Phase I of the residential license program, they faced the bad luck of being left off the map at the wrong moment.

Without a place the municipal land registry, the residents of two subwards in the subdistrict of Charambe (Machinjioni and Machimatitu) were unable to acquire residential licenses from the local municipal government, where residents of the three adjacent subwards (Nsasa A, Nsasa Ba and Mianzini) were made eligible. As the Ministry of Lands has continued to issue residential licenses using the existing registry, but has been unable to update it due to lack of funds, the 'untreated' settlements continue to be excluded from the program nearly ten years following its introduction.

This misfortune affords us an interesting natural experiment: while the modest success of the residential license program's coverage would allow us to compare long term outcomes between parcel-owners which took up RLs to those who did not, this naive specification would ultimately confound unobservable characteristics which predict take-up with a treatment effect of RLs. The lack of satellite imagery creates a plausibly-exogenous source of variation in RL access, allowing us to compare households in excluded areas (control subwards) with households which ultimately received RL access (treatment subwards).

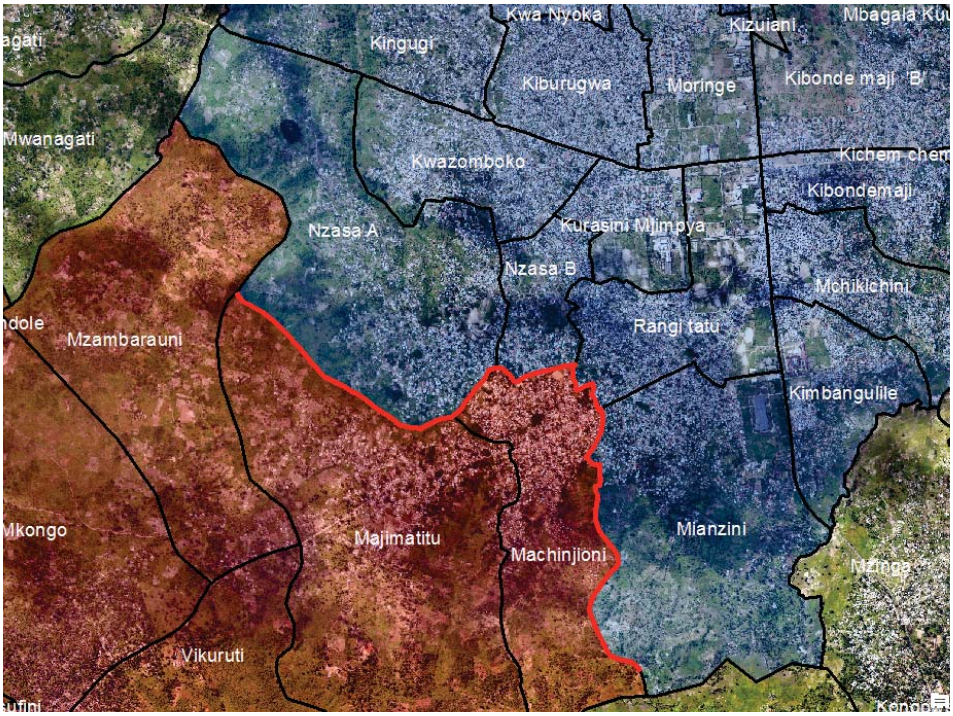
If we solely compared the average parcel of land in a treatment subward to the average parcel in a in control subward we would be left with two threats to identification. The first is the possibility that the Ministry's inability to treat several subwards was not the result of a 'random' difficulty of obtaining a map, but instead a decision based on unobservable characteristics at the subward level which also predict the outcomes of interest.[5] The second is that, even if the exclusion of the control subwards is plausible exogenous, parcels in excluded subwards are, on average, different. For example, the average parcel in Machinjioni is several kilometers further away from the city center than the average parcel in Nsasa A, an observable difference which is likely to drive land prices and a host of other outcomes which might also affect our outcomes of interest, confounding any impacts we observe via the residential license treatment.

To account for both of these concerns, in addition to relying on a natural experiment, we will use a regression discontinuity design (RDD) to compare parcels of land which run

[4] Based on conversations with land officers from the Ministry of Lands.

[5] Land officers at the Ministry of Lands are adamant that the exclusion of these areas was never intentional.

Figure 3: Boundary of the land registration exercise



The map above displays 2002 satellite maps and administrative boundary for subwards included in the Ministry of Lands registration program (blue) and those which were excluded due to missing satellite imagery (red). The discontinuity we will be exploiting is highlighted in red.

along the border between treatment and control areas. Figure 3 highlights the treatment and control areas as well as the administrative boundaries at the time of the intervention. While some of these boundaries have shifted since the creation of the land registry in 2004, the assignment of intent-to-treat (whether the household has been included in the land registry) has remained static since the program was implemented.

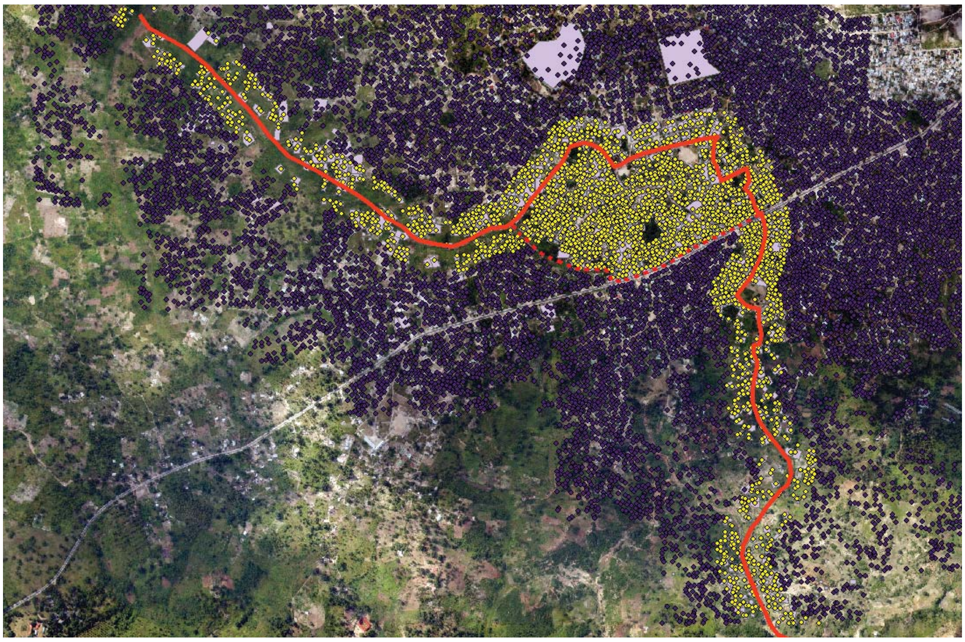
Spatial discontinuity approaches to RD design have grown more common over recent years. An identification strategy based around a spatial RDD will require additional assumptions, which we will discuss in the next section.

3 Data collection and RDD specification

3.1 Data collection and sampling

To take advantage of the discontinuity in the availability of residential licenses, we conducted a large-scale survey of all land parcels within 100 meters of the 2004 boundary between

Figure 4: Location of sampled structures



The map above displays all visible structures sampled using 2014 satellite imagery. All structures within 100m of the RL project boundary (solid red line) are sampled, as well as all structures on the 'control' side of the boundary which were later re-assigned to a treatment district (re-assigned dots are indicated by the area below the project boundary bordered below by a *dashed* red lined. Sampled structures are highlighted in yellow -non sampled structures are in purple.

treatment and control wards. While all parcels established in 2004 are clearly listed in the Temeke land registry, no such list of land ownership exists on the control side. To create a comparable sampling frame on both sides of the boundary, we constructed a frame based on observed structures using satellite data. Using satellite images taken in both 2004 and 2014, a GIS firm highlighted every visible structure in both periods with a 'structure dot.' We then undertook a full census of all dots within 100m of RL program boundary.

In addition to this, we sampled all dots which were beyond the 100m threshold on the control side of the boundary, but had, in 2011, been re-assigned to a treatment subward. These are parcels which had been part of Machinjioni when the residential license program was introduced, so were excluded, but then were subsequently re-assigned to Nsasa A and B, so were administered by these sub-ward governments from 2011 onwards. The next section will cover how this sub-sample will be used as a robustness check on our main results. Figure 4 shows the location of all sampled dots.

To reconstruct the state of land ownership in 2004, we approached the census as follows: enumerators approached a sampled structure and then ascertained what other nearby structures were part of the same parcel of land, as of 2014. Then, the enumerators discussed with the owner, neighbors and local leaders as to the state of that land in 2004 (whether it had been part of another nearby parcel) and recorded that link. Thus, parcels observed in 2014, even if they had split since the intervention, could later be linked back to their 2004 state, contingent to a lack of error in recall.

For each sampled parcel (a collection of sampled structure dots), the enumerator attempted an interview with the owning household (whether or not they lived on a given parcel), asking detailed questions about that household, the structure of the plot, etc. For a sub-sample of households which could not be reached, a shorter questionnaire was used to record observable characteristics of the parcel (number of buildings, upgrading, etc).

As a result, we have detailed parcel and owning-household data from which we will draw our set of outcome measures of interest. For clarity, we will divide these into four broad categories and describe our reasoning for choosing them:

(1) Parcel investment and construction

As formal property rights have both the potential to reduce both expropriation risk and lead to a rise in land prices, we expect a positive impact on the probability a household decides to invest in a parcel (Besley, 1995), in line with empirical evidence from several other studies (Field, 2005; Galiani and Schargrodsky, 2010). We would also expect investment incentives to be created by increased transferability. In this paper we rely on several different measures of investment: the number of structures built upon the parcel as of 2014, the total number of square meters of construction on the parcel, a dummy for whether or not the household made any investment in the parcel in the past year, and a categorical variable for the amount of parcel investment made.

(2) Credit access

Formal property rights have often been championed as a means to 're-ravel' the credit market, either directly by allowing households to utilize their land as collateral to access loans or indirectly by allowing landowners to signal their credit-worthiness (through ease of identification, etc) (De Soto, 2000; Dower and Potamites, 2012). We thus expect, given the anecdotal and descriptive evidence that residential licenses are being used as collateral, that eligibility will have expanded credit access to the treatment group. We propose four different credit-related measures: whether or not the owning household has applied for a formal loan in the past 12 months (loan demand), whether or not anyone from the household has successfully received a loan from a formal financial institution, the probability that the parcel owner would expect to receive a formal loan if they applied, and the (unconditional) expected loan amount they would receive.

(3) Tenure (in)security and land disputes

Table 1: Summary statistics for outcomes of interest

Variable	Obs	Mean	Std. Dev.	Min	Max
# of 2014 structures	2231	1.191	.563	1	8
M^2 of completed construction	1851	2.152	1.201	0	4
Any investment in past year?	1628	.147	.355	0	1
Tsh investment in past year	1851	.247	.762	0	4
Applied for loan?	1041	.058	.233	0	1
Obtained formal loan?	1016	.034	.182	0	1
Could you obtain a loan?	1039	.315	.368	0	1
Subjective loan amount	1054	.974	1.424	0	4
Perceived Expropriation risk	1039	.425	.364	0	1
Dispute in past 12 months?	1843	.068	.252	0	1
Parcel has been sold	1851	.259	.438	0	1
Log(value of sale price)	1323	13.222	2.032	2.708	19.925
Parcel is rented out	1836	.374	.484	0	1
Absentee landlord	1844	.26	.439	0	1

Tenure security is one of the most salient concerns over land in Tanzania. In a separate survey, we found that Dar es Salaam residents are not only fearful of expropriation, but also have a (stated) belief that both residential licenses and CROs reduce expropriation risk. We hypothesize here that residential license reduce overall expropriation risk in the long run by clarifying ownership. Similarly, while land registration exercises might exacerbate land disputes in the short run (as ownership is determined and boundaries are set), in the long run it should reduce disputes. While we do not measure expropriation risk directly, we elicited each owner's perceived expropriation risk by asking them to estimate the probability they would lose their land within the next five years. To measure changes in land disputes, the questionnaire also recorded information on whether or not there had been any disputes over the ownership or boundary of the parcel in the last twelve months.

(4) Land market activity

We hypothesize that RLs encourage rental-market activity, as they reduce a potential landlord's risk that tenants or other parties will assert ownership claims. However, while reduced uncertainty about legitimate ownership is hypothesized to be conducive to sales-market activity (Besley, 1995), frictions in the registration process may actually drive a wedge between the value of land to those who owned it at the time of RL receipt and prospective buyers (Jacoby and Minten, 2007), consequently, the effect of RLs on land sales is a priori ambiguous. Our measures of land market activity include a dummy variable equal to one if the parcel has been purchased since the introduction of the RL program, the log value of the parcel's sale price in current prices, a measure for whether the parcel is being rented out and whether the landlord is living on the parcel.

Summary statistics for each of these outcomes measures is listed in Table 1. Following (Kling et al., 2007), for or our main analysis we standardize the variables in each category[6] calculate mean effects

3.2 Estimation framework and identification

Our analysis in this paper follows a pre-analysis plan (PAP) which we registered with the EGAP (Experiments in Governance and Politics) website[7] at the point when data collection had been concluded, but before we had been capable of combining outcome data with knowledge of treatment eligibility or take-up. For the purpose of deciding how to construct our mean-effects we did have had access to an incomplete sample and set of outcome variables for cleaning purposes, stripped of all information that would identify a household or parcel as treatment or control. In this section we will highlight any elements of the analysis which were not specified in the PAP, which we have considered later on.

The empirical specification employed in this analysis is a regression discontinuity design. To make use of quasi-experimental assignment of sub-wards to treatment, while allowing for the possibility that potential outcomes vary smoothly across space, we treat distance to the 2004 sub-ward border as a running variable. This is a fuzzy regression discontinuity design in the sense that there is one-sided noncompliance: while no parcels on the control side of the boundary could have been treated, not all parcels on the treatment side of the parcel opted to apply for residential licenses. We present results from two types of RD estimators, as in Abdulkadiroğlu, Angrist, and Pathak (2014).

Our non-parametric specification implements a locally linear regression, with bandwidth choice following the data-driven procedure of Imbens and Kalyanaraman (2012, henceforth IK). As IK propose alternative criteria for the selection of bandwidth in a fuzzy discontinuity case, we use that protocol for selecting a distinct bandwidth appropriate to the Wald estimate when estimating treatment effects on those actually receiving RLs in the vicinity of the boundary.

As specified in our PAP, the baseline parametric specification employs a third-order polynomial in distance-to-the boundary, estimated separately on either side of the boundary. However since the time of our deciding on this specification, there have been some compelling arguments made against the use of higher-order polynomials in spatial RDD frameworks, most notably by Gelman and Imbens (2014).[8] To address these concerns, following Lee and Lemieux (2010), in addition to this primary specification we will explore robustness to alternative polynomial specifications, including linear and quadratic.

[6]The method proposed by (Kling et al., 2007) is to standardize variables by subtracting from the mean of the untreated subward, and dividing by the control-side standard deviation.

[7]The PAP is available at the following URL: http://egap.org/wp-content/uploads/2013/06/Final-PAP-for-Tanzania-RD-20140509.pdf

[8]Specifically, Gelman and Imbens show that results using higher-order polynomials are especially sensitive to the chosen order and also increase Type 1 error rates.

Our parametric specification therefore estimates an equation of the following form:

$$y_i = \alpha + \gamma T_i + f_0(D_i) + f_1(D_i) \times T_i + \varepsilon_i, \quad (1)$$

where y_i is a given outcome of interest for sampled parcel i. The variable D_i, the parcel's distance to the RL program boundary, is our 'running' or 'forcing' variable which determines assignment to treatment. T_i is the treatment assignment, taking on a value of one if the parcel is on the side of the boundary which was subject to land registration and RL eligibility. In this specification, the coefficient γ thus represents the intent-to-treat (ITT) effect of RL eligibility.[9] For our function of distance, we will use several polynomials of different order. For example, a cubic specification would take the form $f_T(D) = \delta_{T1}D + \delta_{T2}D^2 + \delta_{T3}D^3$, where T subscripts indicate that coefficients are allowed to vary by side of the border.

Identification of the ITT effect of RL eligibility in this quasi-experimental setting requires several assumptions. The first is that there can be no manipulation of the running variable close to the boundary. Households owning land in one subward could not get themselves re-assigned to the neighboring subward in anticipation of the treatment.[10] There is no evidence that any such manipulation took place: interviews with several local sub-ward leaders indicate that many households in control area petitioned to be included in the land registration exercise to no avail. Furthermore, it is worth emphasizing that while the reason the treatment sub-wards received RL eligibility due the availability of satellite imagery, the treatment was enforced at at an administrative boundary which has been established many years before. This removes any concerns that the boundaries of the RL program were drawn to reflect conditions on the ground, rather than what were fairly arbitrary sub-ward boundaries. In Figure 6 in the appendix, we present a test of manipulation of the running variable as described in McCrary (2008) which reveals no evidence of manipulation.

The fact that the sub-ward boundaries determined eligibility for RL access does raise another concern for identification. While the reason some sub-wards received eligibility was effectively random, there still might be systematic sub-ward level differences due to imbalance of treatment or due to complementary investments in treatment wards. While most sub-ward level differences are geographic and thus vary continuously over space, some might be *administrative*, changing discontinuously at the boundary. For example, if households living on the RL-eligible side of the boundary were given special treatment by their sub-ward government (such as access to credit), our estimates of γ would end up capturing administrative-level differences rather than solely the ITT effect of RL eligibility.

To test for this, we will rely on the 're-assigned' area we mentioned previously: the area of land in the control subward Machinjioni which was re-assigned to a treatment subward in

[9]In the PAP, we also specify that in addition to the ITT effect we will also estimate measuring the effect of RLs on parcels for which they are purchased - for each outcome of interest - using a two-stage least squares or Wald estimator. Due to issues linking administrative data on RL access to our sample, these results are not available here, but will be in future versions of the paper.

[10]Once the maps for the land registry were drawn, no additions were allowed. Any manipulation would have to happen during the process of the land registry creation.

2011. If administrative-level effects are driving our results, they should be less of a factor when we compare parcels which are now under the same administrative jurisdiction. Thus we will re-run our main specification (1), restricting the sample to all parcels on the treatment side, but only re-assigned parcels on the control side.

As with all models of treatments effects, identification of γ rests on a traditional Stable Unit Treatment Value assumption (SUTVA), which requires that there be no geographic spillovers from either becoming eligible to receive an RL or actually receiving one. In practice, this assumption might be violated: for example, if residing proximate to other households with property rights make expropriation more likely, then the estimated treatment effect will be upward biased.

Finally, will report results with controls for geographic sub-strata Even if all baseline parcel characteristics are balanced,we may gain efficiency by controlling for this observable heterogeneity. In particular, we seek to limit comparisons to treatment and control parcels within areas of similar construction density at baseline. In the PAP, we indicated we would divide the boundary line into 4 segments of equal length, then including fixed effects for each segment. Parcels are assigned to a given segment (or sub-strata) based on the segment of the boundary line to which they are closest. In practice, we found there to be greater efficiency gains by including up to 30 sub-strata, without any palpable effect on the results. Given the under-powered nature of our tests, we feel that this is a reasonable departure from the PAP.

In the next section we will present the results from our estimation of 1 over four main categories of outcome measures.

3.3 Weighting, units of observation and non-response

As mentioned above, our unit of sampling during data collection were parcels as observed in 2014. However the size and structure of 2014 parcels is itself a potential outcome of treatment, as the availability of residential license (or the presence of a land register) may have either hampered or encouraged land sales and division on the treatment side of the boundary. In the PAP, we specified that the natural unit of analysis would be parcels as they existed in 2004. Using information on which parts of each parcel were acquired or sold in the interim, it is possible for us to reconstruct a basic picture of parcel structure in 2004.

In the PAP, we specified that under ideal conditions, we would weight the unit observation (2014 parcel i) - by the proportion of 2004 parcels it comprises, to estimate effects on a 2004 parcel basis. However this would be an inappropriate choice if either (i) treatment affects the remembered number and size of 2004 parcels or (ii) pre-determined and exogenous parcel sizes are imbalanced at the 2004 baseline.

As per the PAP, we decided that we would first test for the presence a real or pseudo imbalance in 2004 parcel size using the same specification noted above. The results, displayed in Table 2 below, hint at a moderate treatment effect of approximately 10%, although the difference is only significant in the linear case. In the PAP we specified that we would thus proceed by using weights proportional to 2014 parcel size in order to estimate treatment effects on a per-square-meter basis. In practice, this gives a large amount of weight to a select group of significantly larger parcels (the largest parcel in our sample is, for example, 30 times the size of the median parcel). Thus, we will present our main results as unweighted, but display weighted results in the robustness section.

Finally, there were two forms of non-response we encountered during the survey which might lead to bias in our estimates. The first is, due to a unexpectedly large share of absentee landlords (owners who lived elsewhere in the city and could not be contacted), a subset of parcels were covered by a 'short' questionnaire, which covered basic parcel-level attributes (visible structures, recent investment or sale) which could be determined from conversations with neighbours or local leaders. For non-negligible proportion of parcels, even less data could be collected due to lack of information on the ground, leaving us with only characteristics observed from satellite imagery.

To account for what could be non-random response, we implement a trimming procedure as described in Lee (2009) and adapted for a RD framework in Lindo et al. (2010) in which we trim the treatment arm with the higher response rate to make the treatment group and control group comparable. We use the estimated impact of treatment on response to determine the share of the higher-reporting arm which should be trimmed. To create an upper bound we trim the proportion of treatment (or control) parcels with the lowest reported outcomes (e.g. those with the lowest level of reported investment) and to create a lower bound we trim the proportion of parcels with the highest reported outcomes.

Given a number of different possible drivers of non-response, such as unavailability of respondents due to lack of economic activity or absentee landlordism, we will take an agnostic approach to the drivers of selection and instead trim based on the response rate for each variable separately. Trimming as described in Lee (2009) requires two assumptions: that assignment to treatment is random and that it has a monotonic effect on response rates. Even though, in practice, we find little significant evidence of treatment effects on response rates, we provide trimmed estimates for our main outcome indices in Table 11 in the appendix.

4 Results: parcel and household level outcomes

4.1 Balance

As with a randomized experiment, the quasi-experiment in our regression discontinuity design should produce balance in terms of exogenous variables. In other words, running our preferred RD specification on variables that were pre-determined or otherwise unaffected by the land registration program should yield estimates of zero 'impact'.

Table 2: Balance table and first-stage regression for fuzzy RD

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Balance test: # of structures in 2004					
Full sample	2322	-0.02	0.1	0.2*	-0.010
		(0.05)	(0.08)	(0.1)	(0.07)
Re-assigned control parcels	1249	-0.10	0.2*	0.3**	0.1
		(0.09)	(0.1)	(0.2)	(0.1)
Balance test: Log(2004 parcel size M^2)	2250	0.1***	0.09	0.1	0.1
		(0.04)	(0.06)	(0.08)	(0.06)
First stage: parcel is covered by a RL					
Full sample	2068	0.2***	0.2***	0.3***	0.2***
		(0.03)	(0.04)	(0.06)	(0.04)
Re-assigned control parcels	1544	0.3***	0.2***	0.3***	0.2***
		(0.04)	(0.06)	(0.08)	(0.04)

Each coefficient in the table reports a separate treatment effect estimate; column headings describe alternative specifications. The first two rows report balance tests where the dependent variable is the only exogenous variable that can be retrospectively observed prior to treatment using satellite imagery: the number of 2004 structures per parcel. The last two rows report the first stage regression, where the dependent variable is an indicator of whether a residential license (RL) was ever issued for the parcel. All equations include location fixed effects, corresponding to where the parcel is located along the boundary line (as opposed to distance from the boundary). Robust standard errors are listed in parentheses.

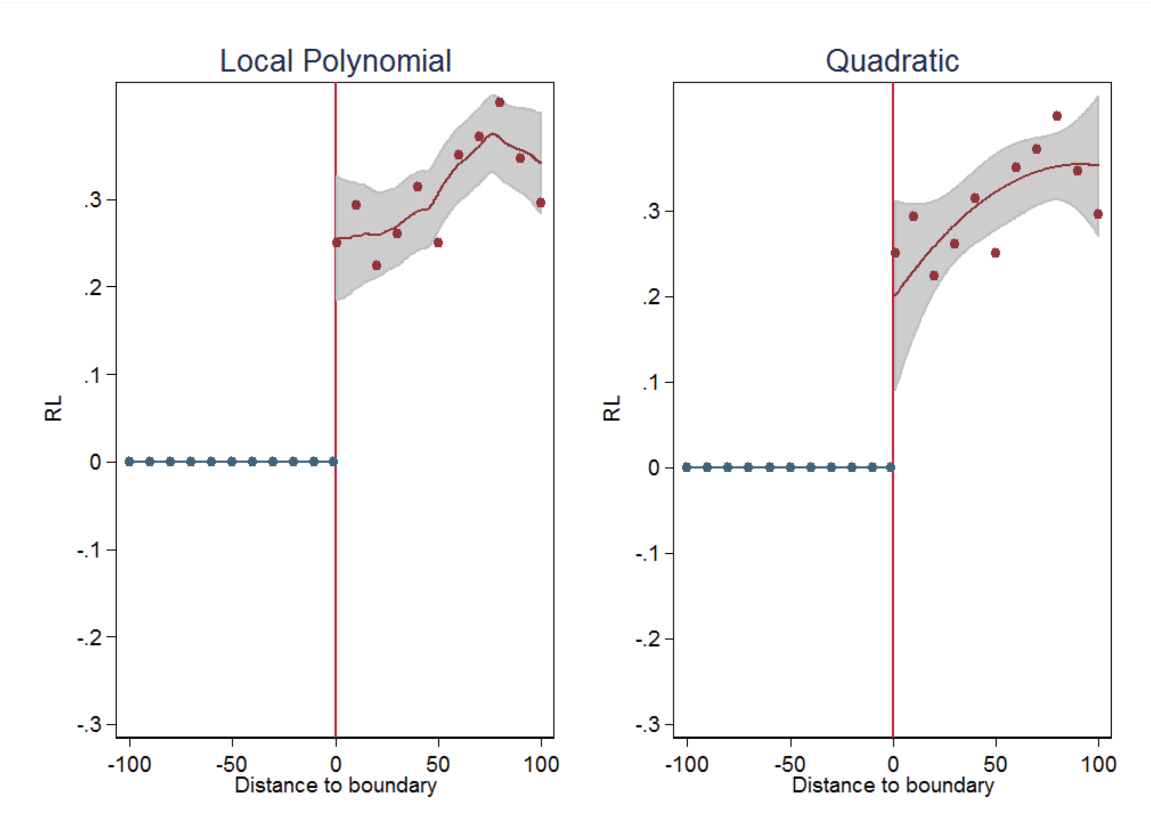
We have information on very few variables that could not plausibly have been affected by the policy treatment. This is mostly due to the fact that we lack baseline survey data. The only variables that we can reliably observe prior to treatment are those that can be measured from satellite imagery. We use the baseline satellite imagery to measure the number of structures per parcel circa 2004.

Results for this balance test in the first rows of Table 2 are somewhat reassuring, but not entirely so. The difference in number of structures per parcel is small in all specifications, and spread fairly evenly around zero. However, various specifications imply that these small differences in baseline structure density are marginally statistically significant, though they differ on whether treatment or control parcels started with more structures.

We also construct another baseline measure – of parcel *size* circa 2004 – based on retrospective interviews with residents in 2014. Experience in the field suggests these reports are potentially quite unreliable relative to the baseline satellite imagery. Nevertheless, we report the results in Table 2. Across most specifications, differences in reported baseline parcel size are small and insignificant, though a roughly 10% difference is significant in the linear specification.

To address any potential imbalance, we control for the number of 2004 structures – our most reliable baseline measure – in some variants of the parcel-level treatment effect regressions below.

Figure 5: RL take-up and distance to the boundary line



Note: negative distance indicates control side of boundary.

4.2 Take-up

We have reliable data on residential license take-up from the Temeke municipal registry. This allows us to examine the 'first stage' result: does selection into the municipal registry actually predict take-up of a residential license? For the treatment side, we use parcels as defined in the registry to estimate RL take-up as this gives us the most precise information on relative take-up.[11] Due to difficulties in directly linking registry information with household and parcel data on the ground, while we estimate the first stage in this section, we will not use it to instrument for RL take-up in the paper, instead relying on estimating ITT effects.

The results, available both in Table 2 and in Figure 5, suggest that treated parcels at the boundary were significantly more likely to take-up a residential license, although at a slightly lower rate that the average: roughly 20-30% of parcels at the boundary obtained an RL at some point since 2004.

[11]The results are not significantly different if we attempt to infer RL take-up decisions by overlaying the government registry on our own estimates of parcel boundaries, but this gives us a more precise estimate.

4.3 Impacts

To preemptively summarize our main results: we find some statistically and economically significant evidence of positive treatment effects from residential licensing on housing investment, but no such effects along the other main outcomes analyzed: credit market access, tenure security, or land market activity. In the robustness section, we will discuss some results which are conditional on different specifications.

We present our main results in terms of intention-to-treat (ITT) effects. In other words, we measure the impact of *eligibility* for a residential license on a range of outcomes specified in our publicly registered pre-analysis plan. Bear in mind when interpreting these results that eligibility raised take-up by roughly 20-30%. Thus the magnitude of the effect of treatment on the treated (TOT) would be roughly 3-5 times larger than the ITT effects reported in Tables 3 and 4.

We present two separate versions of all the main results: Table 3 uses the full sample, whereas Table 4 restricts attention to the areas on both the treatment and control side which were – at the time of treatment in 2004 – part of the same administrative ward. To recall the discussion above, these latter estimates reduce the possibility that the treatment discontinuity is colinear with a relevant break in administrative governance. It also restricts our sample to a more homogenous set of parcels along various other dimensions. Graphical RD results for this subgroup (without controls) are shown in Figures 7a through 8b in the appendix, with negative distance values indicating the control side of the boundary.

We report results on fourteen distinct outcomes. As discussed above, to minimize the risk of spurious results due to multiple comparisons, we group our outcome variables into "mean effects" indices following Kling et al. (2007). Our four indices are investment, credit market access, tenure security, and land market activity. In each case, variables are defined so that higher values suggest confirmation of our hypotheses; individual variables are standardized using the mean and standard deviation on the control side; indices are calculated as the average for a given parcel of the variables in each category; and the indices themselves are standardized so that effects can be interpreted in terms of standard deviations.

As seen in Tables 3 and 4, results across the majority of outcomes and outcome indices are modest in magnitude but statistically insignificant. The notable exception arises in the case of land investment.

Using the full sample, we find positive point estimates on the investment index in all specifications – including linear, quadratic, and cubic controls, as well as the non-parametric analysis, though the result is marginally significant only in the cubic specification. Magnitudes range from a 0.06 to 0.2 standard deviation increase in investment activity.

Turning to the sub-sample with a more homogenous set of treatment and control parcels in 4, results are more robust. There is fairly strong evidence of a positive impact of residential licensing on investment with a magnitude of 0.2 to 0.3 standard deviations. Looking at the individual components of the index, this appears to be driven by a combination of (a) square meters of completed construction, and (b) self-reported investment measured in Tanzanian shillings over the previous year.

No other index outcomes are either significant or robust in the sense that they take a consistent sign, suggesting that the RL exercise had no effect on these outcomes. We will discuss this in more detail in the next two subsections.

4.4 Heterogeneous effects

In our pre-analysis plan, we registered our intent to explore two types of heterogeneous effects. The motivation for examining heterogeneous effects and the relevant dimension of heterogeneity differs between outcome variables related to housing investment and those related to household welfare.

First, for housing investment outcomes, the two strata of interest will be defined by baseline 2004 construction. There is large heterogeneity within our sample area in terms of the density of housing construction at baseline. Casual visual inspection of a map of our sample area circa 2004 shows that areas near the geographic mid-point of the boundary line were already well-established, high-density residential neighborhoods at baseline, while areas at either extreme (the northwest and southeast limits of the boundary line) were very low density, and have built up considerably over the past decade. We hypothesized, inter alia, that parcels without any structures as of 2004 would experience larger treatment effects in terms of new housing construction.

Results in Table 5 fail to provide any significant corroboration for this hypothesis. Rather than finding larger investment effects on parcels with no structure at baseline, we find roughly equal effects in both groups, almost all of which are insignificantly different from zero due to the loss of sample size in the sub-group analysis.

Second, for household welfare outcomes, the two strata of interest are defined by whether or not the current (2014) owner has owned the parcel since prior to treatment, i.e. since 2004. Our interest here is in the mechanism underlying any of our main effects. Clearly, any observed treatment effect on household outcome measures might arise due to an effect on a fixed set of households, or land transactions that attract households with different socio-economic characteristics (i.e., "gentrification" or its inverse).

In practice, this second set of hypotheses is somewhat mute, given the lack of evidence for significant effects on household outcomes in the main results. Furthermore, in the sub-sample analysis, we find no evidence that overall small and insignificant effects on household outcomes are masking significant effects in either sub-sample (new owners or continuous owners).

Table 3: Impact estimates: intent-to-treat effects (ITT)

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	2231	0.1	0.2	0.2	0.06
		(0.08)	(0.1)	(0.2)	(0.1)
# of 2014 structures	2231	0.1***	0.1*	0.2**	0.09
		(0.04)	(0.07)	(0.08)	(0.06)
M^2 of completed construction	1851	0.1	0.1	0.04	0.06
		(0.1)	(0.2)	(0.2)	(0.2)
Any investment in past year?	1628	-0.01	0.04	0.03	-0.001
		(0.03)	(0.05)	(0.07)	(0.05)
Tsh investment in past year	1851	0.010	0.06	0.006	-0.05
		(0.07)	(0.1)	(0.1)	(0.1)
Credit - mean index	1054	0.05	0.06	-0.2	0.007
		(0.1)	(0.2)	(0.2)	(0.1)
Applied for loan?	1041	0.03	-0.007	-0.05	-0.004
		(0.03)	(0.04)	(0.04)	(0.03)
Obtained formal loan?	1016	0.04	0.01	-0.03	-0.001
		(0.02)	(0.03)	(0.04)	(0.03)
Could you obtain a loan?	1039	-0.08*	-0.03	-0.06	-0.04
		(0.04)	(0.06)	(0.08)	(0.06)
Subjective loan amount	1054	0.02	0.1	0.04	0.06
		(0.2)	(0.3)	(0.3)	(0.2)
Tenure insecurity - mean index	1845	-0.09	0.06	0.02	-0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Perceived Expropriation risk	1039	-0.05	0.03	-0.02	-0.05
		(0.04)	(0.07)	(0.09)	(0.06)
Dispute in past 12 months?	1843	-0.006	0.03	0.05	0.02
		(0.02)	(0.04)	(0.06)	(0.04)
Land market activity - mean index	1851	0.005	-0.08	-0.08	-0.08
		(0.09)	(0.1)	(0.2)	(0.1)
Parcel has been sold	1851	-0.05	-0.08	-0.1	-0.1*
		(0.04)	(0.06)	(0.07)	(0.06)
Log(value of sale price)	1323	0.06	0.5	0.7*	0.6*
		(0.2)	(0.3)	(0.4)	(0.3)
Parcel is rented out	1836	-0.007	-0.02	-0.09	-0.05
		(0.04)	(0.06)	(0.09)	(0.06)
Absentee landlord	1844	0.08*	0.01	0.1	0.03
		(0.04)	(0.06)	(0.09)	(0.06)

Dependent variables are listed in the first column; column headings describe alternative specifications. Each coefficient reports a separate treatment efffect estimate. Mean indices are the average of the standardized variables listed below. All equations control for the number of structures per parcel in 2004 and include location fixed effects, corresponding to where the parcel is located along the boundary line (as opposed to distance from the boundary). Robust standard errors are listed in parentheses.

Table 4: Impact estimates: intent-to-treat effects (ITT) – sub-sample in reassigned area

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	1707	0.2**	0.3**	0.2	0.2
		(0.09)	(0.1)	(0.2)	(0.1)
# of 2014 structures	1707	0.10*	0.10	0.1	0.08
		(0.05)	(0.07)	(0.09)	(0.07)
M^2 of completed construction	1388	0.4***	0.4**	0.2	0.09
		(0.1)	(0.2)	(0.3)	(0.2)
Any investment in past year?	1210	0.005	0.07	0.03	0.03
		(0.04)	(0.06)	(0.08)	(0.06)
Tsh investment in past year	1388	0.1	0.2**	0.1	0.2**
		(0.07)	(0.10)	(0.1)	(0.09)
Credit - mean index	757	-0.1	-0.1	-0.5	-0.3
		(0.2)	(0.3)	(0.3)	(0.3)
Applied for loan?	748	0.03	-0.010	-0.10	-0.09
		(0.04)	(0.06)	(0.07)	(0.07)
Obtained formal loan?	730	0.007	-0.02	-0.10	-0.10
		(0.04)	(0.06)	(0.07)	(0.07)
Could you obtain a loan?	746	-0.2***	-0.1*	-0.2*	-0.10
		(0.06)	(0.08)	(0.1)	(0.09)
Subjective loan amount	757	-0.2	-0.10	-0.05	0.2
		(0.2)	(0.3)	(0.4)	(0.3)
Tenure insecurity - mean index	1382	-0.1	0.01	-0.2	-0.06
		(0.1)	(0.2)	(0.2)	(0.2)
Perceived Expropriation risk	746	-0.1**	-0.05	-0.1	-0.1
		(0.05)	(0.08)	(0.10)	(0.08)
Dispute in past 12 months?	1380	-0.004	0.03	0.03	0.04
		(0.03)	(0.04)	(0.06)	(0.05)
Land market activity - mean index	1388	-0.1	-0.3	-0.2	-0.009
		(0.1)	(0.2)	(0.2)	(0.2)
Parcel has been sold	1388	-0.06	-0.1	-0.1	0.07
		(0.04)	(0.06)	(0.08)	(0.07)
Log(value of sale price)	964	-0.5	-0.04	0.2	0.3
		(0.3)	(0.4)	(0.6)	(0.6)
Parcel is rented out	1374	0.001	-0.02	-0.1	-0.10
		(0.06)	(0.08)	(0.1)	(0.09)
Absentee landlord	1381	0.03	-0.07	0.06	-0.001
		(0.05)	(0.08)	(0.1)	(0.08)

See notes from Table 3. Estimates are identical to Table 3, but limited on the control side to the sub-sample of parcels that originally belonged to the same administrative unit as the treatment area and were subsequently reassigned after the treatment.

Table 5: Heterogeneous treatment effects: intent-to-treat effects (ITT)

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index					
if structure present	1286	0.02	0.1	0.05	0.02
		(0.09)	(0.1)	(0.2)	(0.1)
if no structure	945	0.1	0.2	0.3	0.1
		(0.1)	(0.2)	(0.2)	(0.2)
if owner has changed	859	0.05	0.09	0.2	0.2
		(0.1)	(0.2)	(0.3)	(0.2)
if same owner	1372	0.1	0.2	0.1	-0.006
		(0.09)	(0.1)	(0.2)	(0.1)
Credit access - mean index					
if structure present	534	0.05	0.3	-0.02	0.1
		(0.2)	(0.2)	(0.3)	(0.2)
if no structure	520	-0.03	-0.1	-0.5	-0.10
		(0.2)	(0.2)	(0.3)	(0.2)
if owner has changed	339	-0.3*	-0.4*	-0.6**	-0.2
		(0.2)	(0.2)	(0.3)	(0.2)
if same owner	715	0.2	0.2	-0.06	0.09
		(0.2)	(0.2)	(0.3)	(0.2)
Tenure insecurity - mean index					
if structure present	1022	-0.06	-0.02	-0.07	-0.02
		(0.1)	(0.2)	(0.2)	(0.2)
if no structure	823	-0.07	0.1	0.1	-0.1
		(0.1)	(0.2)	(0.3)	(0.2)
if owner has changed	479	-0.1	0.4	0.3	0.02
		(0.2)	(0.3)	(0.3)	(0.3)
if same owner	1366	-0.09	-0.08	-0.1	-0.06
		(0.10)	(0.1)	(0.2)	(0.1)
Land market activity - mean index					
if structure present	1025	-0.10	-0.1	-0.3	-0.06
		(0.1)	(0.2)	(0.2)	(0.2)
if no structure	826	0.06	-0.07	0.1	-0.1
		(0.1)	(0.2)	(0.3)	(0.2)
if owner has changed	479	-0.01	0.09	0.4	0.06
		(0.1)	(0.2)	(0.3)	(0.2)
if same owner	1372	0.07	-0.02	-0.1	-0.010
		(0.10)	(0.1)	(0.2)	(0.1)

See notes from Table 3.

4.5 Robustness and alternate specifications

In this section, we explore the robustness of our results to several adjustments to the main specification. First, as mentioned in Section 3.3, due to weak, but suggestive signs of parcel imbalance at baseline, we will present results as weighted by 2014 parcel size, as specified in the PAP.[12]

The second robustness check is to take advantage of one feature of treatment: there are a handful of parcels on the treatment side of the boundary which did not exist at the time of the 2004 registration, but have subsequently been formed post-treatment. A priori, as these parcels could not have been registered or received residential licenses, we expect treatment effects for these parcels to be zero. Thus, we repeat the main specification (both weighted and unweighted), dropping these parcels from the analysis to estimate treatment effects over only treatment-eligible land. While this adjustment was not specified in the PAP, we feel it is a reasonable robustness check, as these non-eligible parcels might bias estimates of the ITT towards zero. We refer to these parcels as 'late arrivals' for the remainder of the paper.

Tables 6 and 7 display these robustness checks. These checks reveal that the positive effect sizes generated for investment outcomes are generally robust, although still rarely significant, with effect sizes ranging from 0.06 to 0.4 standard deviations. Across other indices there are few consistent results, although it is worth noting that estimates for the tenure mean index are consistently negative (indicating an improvement in tenure security) and robustly significant in weighted specification which drops late arrivals. These results seem to be driven primarily by a reduction in the number of reported parcel disputes, as evidenced in Tables 8 and 9 in the appendix. While no significant effect of treatment on the land market index is ever identified, there does appear to be a robust negative effect on the probability of land sale in specifications which drop-late comers. Although this result should be interpreted with caution, this suggests that residential licences might reduce land sales by adding an extra layer of bureaucratic procedure to the process of buying and selling land.

5 Conclusion

In this paper we exploited a natural experiment in the provision of low-cost, short term leasehold titles in an urban Tanzanian setting in order to explore the subsequent long term impacts on landowners who were eligible to receive them. What we find is modest evidence that the residential license program has an impact on parcel investment. Given that we estimate intent-to-treat effects, the effect sizes generated are substantial given the relatively low rate of take-up in the population close to boundary of the program (between 20 and 30%). These results seem to be robust to a number of different specification choices, but unfortunately often do not reach conventional levels of statistical significance.

[12] To avoid placing too much weight on parcels at the extreme end of the distribution, we drop all parcels larger than the 99th percentile and above, although we did not specify this in the PAP.

The residential license program does not appear to have generated any consistent impacts along the other three dimensions we consider: credit, land market activity or tenure insecurity, although there is some suggestive evidence that the latter two were reduced by the program. In particular, we find no robust evidence that residential licenses either increased or decreased the probability that an owning-household would access formal credit, undermining both the hypothesis and anecdotal reports that residential licenses provided a new form of collateral for landowners.

The long term effects of these limited forms of property rights are not insignificant, but the results do suggest that while the low cost of these limited forms of leaseholds do allow for greater coverage, they may be hampered by the fairly short length of tenure they can guarantee. This might explain why the residential license program eventually failed to take off, and why the benefits often associated with land formalization do not appear to be fully emerging in this setting.

Table 6: ITT estimates, robustness

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	2231	0.1	0.2	0.2	0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Weighted by parcel size	2201	0.08	0.2	0.2	0.08
		(0.09)	(0.1)	(0.2)	(0.1)
Dropping late arrivals	1946	0.1	0.3*	0.3	0.04
		(0.1)	(0.2)	(0.2)	(0.1)
Weighting and dropping late arrivals	1918	0.1	0.3	0.3	0.06
		(0.1)	(0.2)	(0.3)	(0.2)
Credit - mean index	1054	0.05	0.06	-0.2	0.007
		(0.1)	(0.2)	(0.2)	(0.1)
Weighted by parcel size	1043	-0.07	-0.06	-0.3	-0.1
		(0.1)	(0.2)	(0.2)	(0.2)
Dropping late arrivals	861	0.2	0.4	-0.04	0.2
		(0.2)	(0.3)	(0.4)	(0.2)
Weighting and dropping late arrivals	852	-0.02	0.2	-0.05	-0.1
		(0.2)	(0.3)	(0.4)	(0.2)
Tenure insecurity - mean index	1845	-0.09	0.06	0.02	-0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Weighted by parcel size	1817	-0.2*	-0.02	-0.08	-0.1
		(0.09)	(0.1)	(0.2)	(0.1)
Dropping late arrivals	1596	-0.2**	-0.2	-0.3	-0.2*
		(0.1)	(0.2)	(0.2)	(0.1)
Weighting and dropping late arrivals	1570	-0.3***	-0.3**	-0.4*	-0.3**
		(0.10)	(0.1)	(0.2)	(0.1)
Land market activity	1851	0.005	-0.08	-0.08	-0.08
		(0.09)	(0.1)	(0.2)	(0.1)
Weighted by parcel size	1823	0.10	0.01	-0.1	-0.06
		(0.10)	(0.1)	(0.2)	(0.1)
Dropping late arrivals	1602	-0.08	-0.2	-0.3	-0.1
		(0.1)	(0.2)	(0.3)	(0.1)
Weighting and dropping late arrivals	1576	-0.0004	-0.1	-0.3	-0.1
		(0.1)	(0.2)	(0.3)	(0.2)

Dependent variables and specification type are listed in the first column; column headings describe alternative specifications. Each coefficient reports a separate treatment effect estimate, with subsequent rows reporting the results from specifications listed in first column. All equations control for the number of structures per parcel in 2004 and include location fixed effects, corresponding to where the parcel is located along the boundary line (as opposed to distance from the boundary). Robust standard errors are listed in parentheses.

Table 7: ITT estimates, robustness – sub-sample in reassigned area

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	1707	0.2**	0.3**	0.2	0.2
		(0.09)	(0.1)	(0.2)	(0.1)
Weighted by parcel size	1681	0.1	0.3*	0.3	0.2
		(0.1)	(0.2)	(0.2)	(0.2)
Dropping late arrivals	1422	0.2*	0.4**	0.3	0.2
		(0.1)	(0.2)	(0.2)	(0.1)
Weighting and dropping late arrivals	1398	0.2	0.3*	0.3	0.2
		(0.1)	(0.2)	(0.3)	(0.2)
Credit - mean index	757	-0.1	-0.1	-0.5	-0.3
		(0.2)	(0.3)	(0.3)	(0.3)
Weighted by parcel size	748	-0.4*	-0.4	-0.7*	-0.4
		(0.2)	(0.3)	(0.4)	(0.4)
Dropping late arrivals	564	0.1	0.4	-0.2	-0.1
		(0.2)	(0.4)	(0.5)	(0.4)
Weighting and dropping late arrivals	557	-0.2	0.03	-0.3	-0.4
		(0.2)	(0.3)	(0.5)	(0.4)
Tenure insecurity - mean index	1382	-0.1	0.01	-0.2	-0.06
		(0.1)	(0.2)	(0.2)	(0.2)
Weighted by parcel size	1357	-0.2	-0.08	-0.3	-0.2
		(0.1)	(0.2)	(0.2)	(0.2)
Dropping late arrivals	1133	-0.2	-0.2	-0.4	-0.2
		(0.1)	(0.2)	(0.3)	(0.2)
Weighting and dropping late arrivals	1110	-0.2**	-0.3*	-0.5**	-0.4*
		(0.1)	(0.2)	(0.2)	(0.2)
Land market activity	1388	-0.1	-0.3	-0.2	-0.009
		(0.1)	(0.2)	(0.2)	(0.2)
Weighted by parcel size	1363	-0.01	-0.1	-0.2	-0.02
		(0.1)	(0.2)	(0.2)	(0.2)
Dropping late arrivals	1139	-0.2	-0.3	-0.4	-0.07
		(0.1)	(0.2)	(0.3)	(0.2)
Weighting and dropping late arrivals	1116	-0.06	-0.2	-0.3	-0.07
		(0.2)	(0.2)	(0.3)	(0.2)

See notes from Table 6. Estimates are identical to Table 6, but limited on the control side to the sub-sample of parcels that originally belonged to the same administrative unit as the treatment area and were subsequently reassigned after the treatment.

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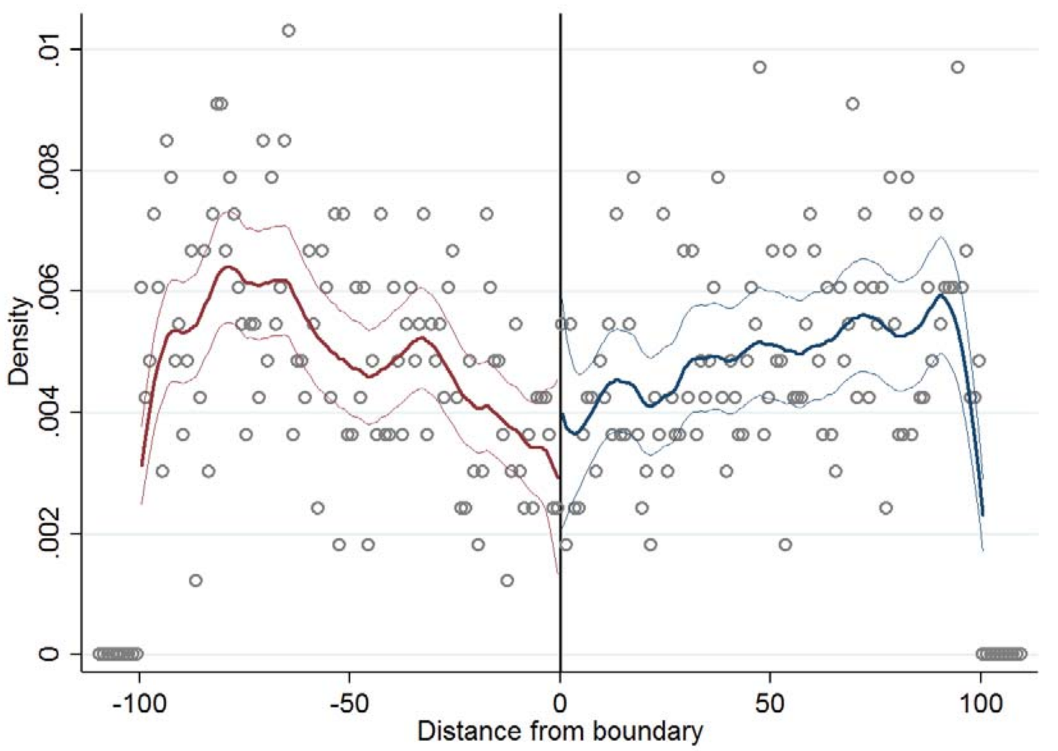
Appendix: Extra figures and tables

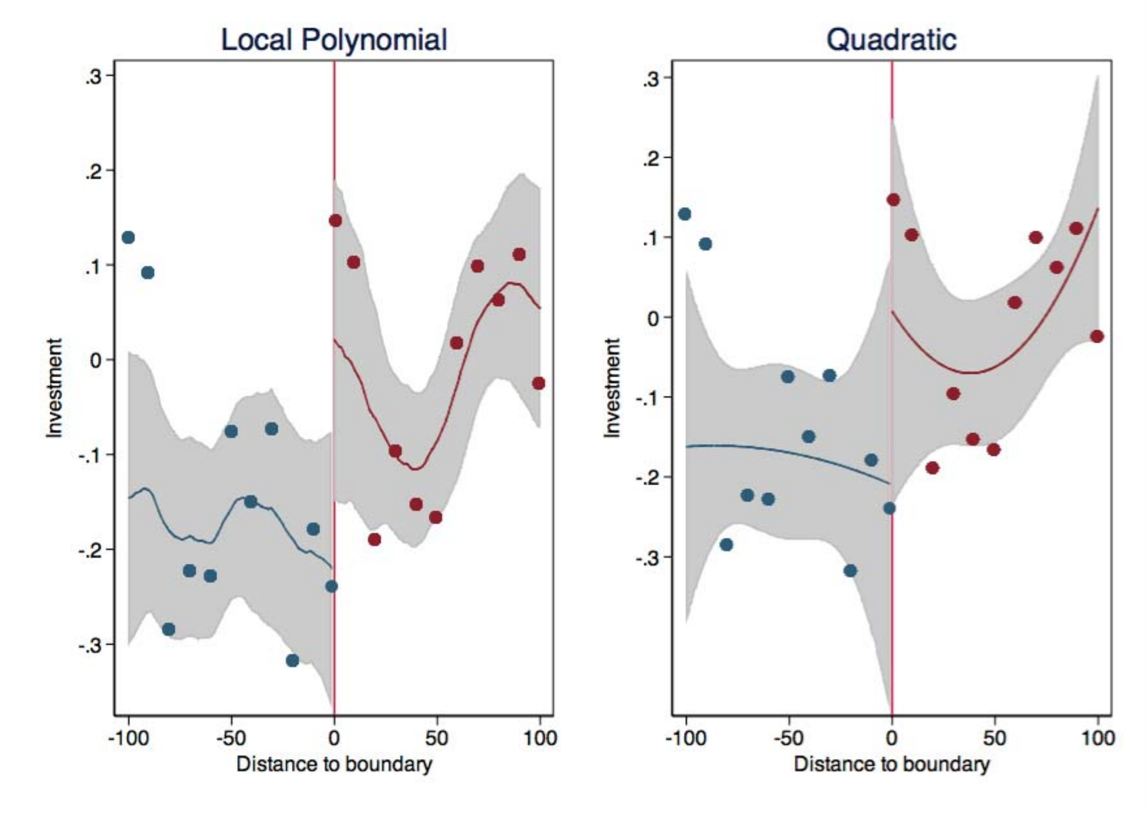
Figure 6: McCrary test of manipulation of the running variable



The above figure displays the results from a density test for manipulation as described in McCrary (2008). Negative values of distance indicate parcels on the control side of the boundary.

Figure 7: Mean-effects indexes and distance to the boundary line

(a) Investment



(b) Credit

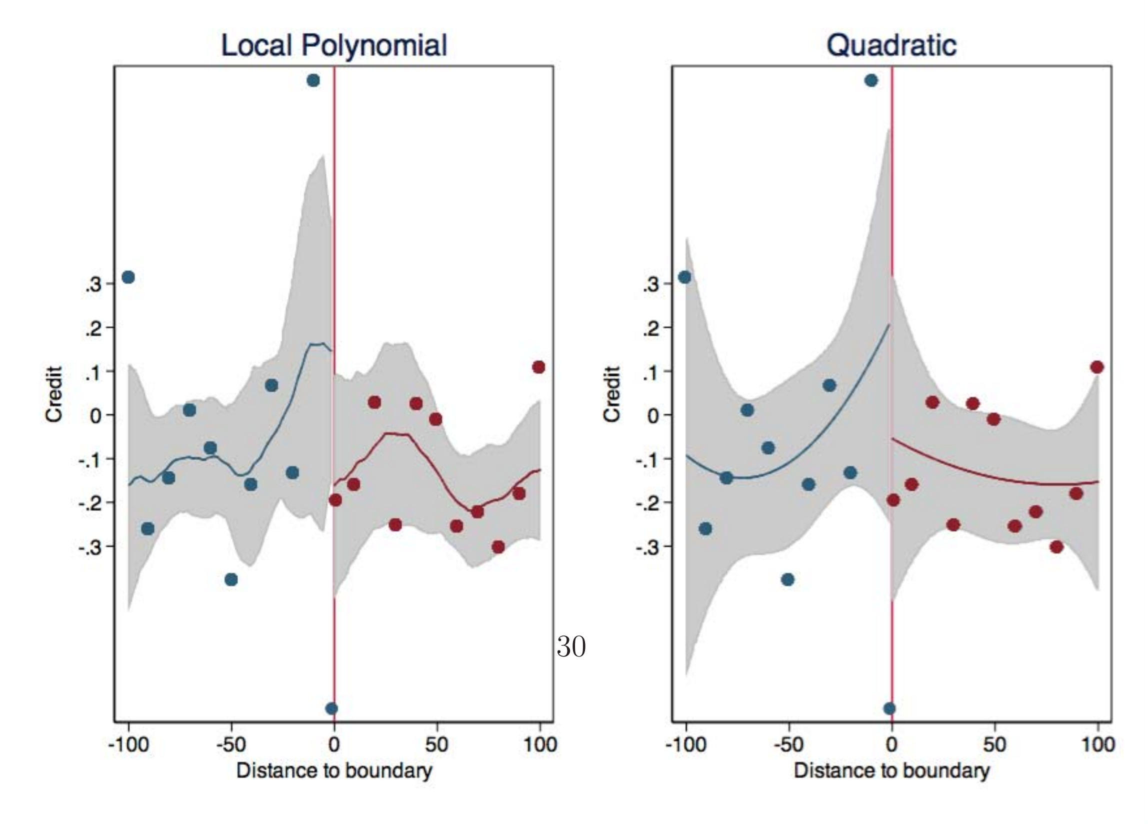
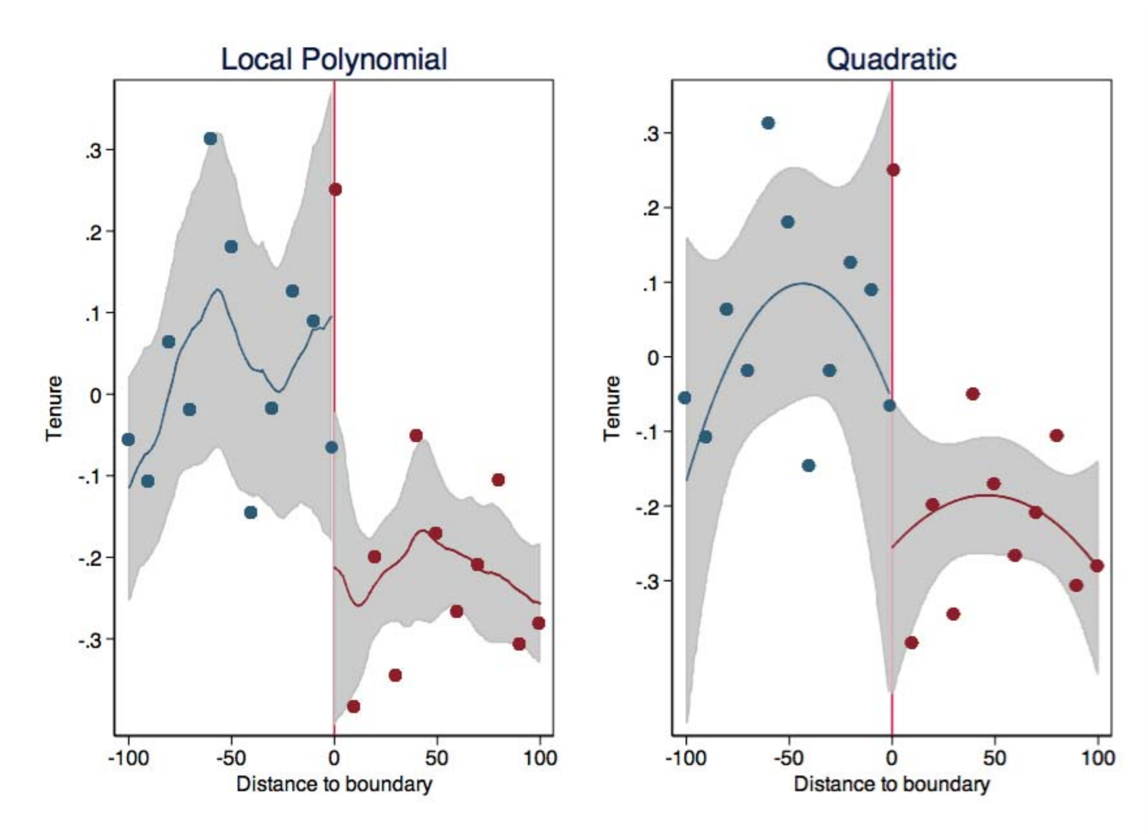


Figure 8: Mean-effects indexes and distance to the boundary line

(a) Tenure security



(b) Land market outcomes

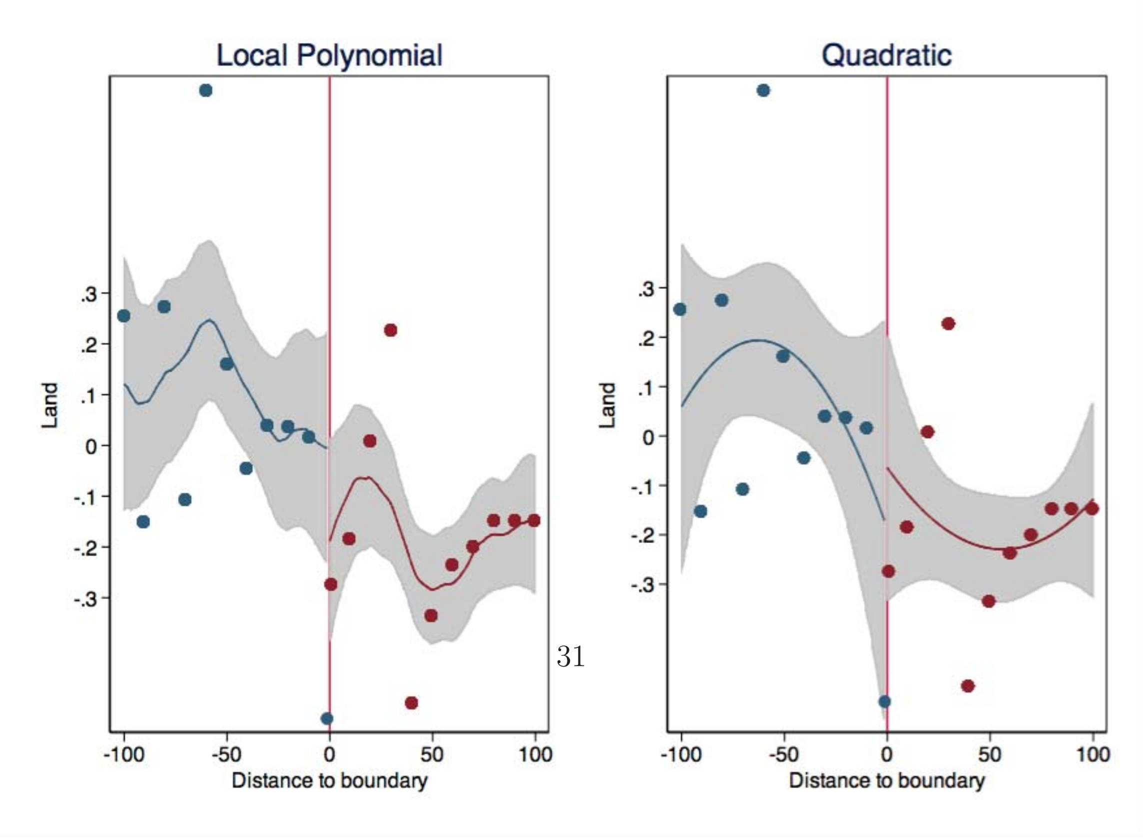


Table 8: ITT estimates, dropping untreated parcels in treatment areas

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	1946	0.1	0.3*	0.3	0.04
		(0.1)	(0.2)	(0.2)	(0.1)
# of 2014 structures	1946	0.2**	0.2	0.3**	0.10
		(0.06)	(0.1)	(0.1)	(0.08)
M^2 of completed construction	1602	0.3	0.5**	0.6	0.3
		(0.2)	(0.3)	(0.4)	(0.2)
Any investment in past year?	1407	-0.03	0.06	-0.004	-0.02
		(0.04)	(0.07)	(0.08)	(0.05)
Tsh investment in past year	1602	-0.01	0.08	-0.03	-0.1
		(0.09)	(0.1)	(0.2)	(0.1)
Credit - mean index	861	0.2	0.4	-0.04	0.2
		(0.2)	(0.3)	(0.4)	(0.2)
Applied for loan?	850	0.07	0.04	-0.05	0.04
		(0.05)	(0.06)	(0.08)	(0.05)
Obtained formal loan?	826	0.07*	0.06	-0.01	0.04
		(0.04)	(0.06)	(0.07)	(0.04)
Could you obtain a loan?	848	-0.06	0.05	-0.04	-0.01
		(0.06)	(0.10)	(0.1)	(0.07)
Subjective loan amount	861	-0.04	0.6	0.7	0.02
		(0.2)	(0.4)	(0.6)	(0.3)
Tenure insecurity - mean index	1596	-0.2**	-0.2	-0.3	-0.2*
		(0.1)	(0.2)	(0.2)	(0.1)
Perceived Expropriation risk	848	-0.09	0.007	-0.07	-0.09
		(0.06)	(0.10)	(0.1)	(0.07)
Dispute in past 12 months?	1595	-0.05*	-0.08**	-0.07	-0.04
		(0.03)	(0.04)	(0.06)	(0.03)
Land market activity - mean index	1602	-0.08	-0.2	-0.3	-0.1
		(0.1)	(0.2)	(0.3)	(0.1)
Parcel has been sold	1602	-0.1**	-0.2***	-0.2**	-0.2***
		(0.05)	(0.07)	(0.08)	(0.06)
Log(value of sale price)	1132	0.002	0.5	0.8	0.8**
		(0.3)	(0.4)	(0.6)	(0.4)
Parcel is rented out	1589	-0.04	-0.07	-0.2*	-0.04
		(0.06)	(0.10)	(0.1)	(0.07)
Absentee landlord	1595	0.1*	0.08	0.1	0.06
		(0.06)	(0.09)	(0.1)	(0.07)

Dependent variables are listed in the first column; column headings describe alternative specifications. Each coefficient reports a separate treatment efffect estimate. Mean indices are the average of the standardized variables listed below. All equations control for the number of structures per parcel in 2004 and include location fixed effects, corresponding to where the parcel is located along the boundary line (as opposed to distance from the boundary). Robust standard errors are listed in parentheses.

Table 9: ITT estimates, dropping untreated parcels in treatment areas – sub-sample in reassigned area

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Investment - mean index	1422	0.2*	0.4**	0.3	0.2
		(0.1)	(0.2)	(0.2)	(0.1)
# of 2014 structures	1422	0.1**	0.1	0.2	0.08
		(0.06)	(0.1)	(0.2)	(0.09)
M^2 of completed construction	1139	0.4**	0.7**	0.5	0.3
		(0.2)	(0.3)	(0.4)	(0.2)
Any investment in past year?	989	-0.02	0.09	-0.001	0.01
		(0.05)	(0.07)	(0.09)	(0.06)
Tsh investment in past year	1139	0.07	0.2*	0.05	0.1
		(0.09)	(0.1)	(0.2)	(0.1)
Credit - mean index	564	0.1	0.4	-0.2	-0.1
		(0.2)	(0.4)	(0.5)	(0.4)
Applied for loan?	557	0.07	0.05	-0.08	-0.04
		(0.06)	(0.08)	(0.10)	(0.08)
Obtained formal loan?	540	0.05	0.05	-0.06	-0.06
		(0.05)	(0.07)	(0.09)	(0.08)
Could you obtain a loan?	555	-0.1	-0.006	-0.1	-0.07
		(0.07)	(0.1)	(0.1)	(0.10)
Subjective loan amount	564	-0.1	0.5	0.7	0.2
		(0.3)	(0.5)	(0.7)	(0.4)
Tenure insecurity - mean index	1133	-0.2	-0.2	-0.4	-0.2
		(0.1)	(0.2)	(0.3)	(0.2)
Perceived Expropriation risk	555	-0.09	-0.01	-0.1	-0.2
		(0.07)	(0.1)	(0.1)	(0.09)
Dispute in past 12 months?	1132	-0.04	-0.07	-0.07	-0.02
		(0.03)	(0.04)	(0.06)	(0.04)
Land market activity - mean index	1139	-0.2	-0.3	-0.4	-0.07
		(0.1)	(0.2)	(0.3)	(0.2)
Parcel has been sold	1139	-0.08*	-0.2**	-0.2**	-0.04
		(0.05)	(0.07)	(0.09)	(0.07)
Log(value of sale price)	773	-0.6	-0.03	0.2	0.4
		(0.4)	(0.5)	(0.7)	(0.6)
Parcel is rented out	1127	-0.03	-0.07	-0.2*	-0.09
		(0.07)	(0.1)	(0.1)	(0.10)
Absentee landlord	1132	0.05	-0.005	0.1	0.03
		(0.06)	(0.10)	(0.1)	(0.09)

See notes from Table 3. Estimates are identical to Table 3, but limited on the control side to the sub-sample of parcels that originally belonged to the same administrative unit as the treatment area and were subsequently reassigned after the treatment.

Table 10: Estimates of non-random response and trimmed results

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Response rate for investment index	2231	0	0	0	0
		(.)	(.)	(.)	(.)
Lower bound estimate	2231	0.1	0.2	0.2	0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Untrimmed estimate	2231	0.1	0.2	0.2	0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Upper bound estimate	2231	0.1	0.2	0.2	0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Response rate for credit index	2231	0.02	0.004	0.1	-0.003
		(0.04)	(0.06)	(0.08)	(0.06)
Lower bound estimate	1053	-0.1	0.09	-0.2	0.007
		(0.1)	(0.2)	(0.2)	(0.1)
Untrimmed estimate	1054	0.05	0.06	-0.2	0.007
		(0.1)	(0.2)	(0.2)	(0.1)
Upper bound estimate	1052	0.07	0.06	-0.2	0.007
		(0.1)	(0.2)	(0.2)	(0.1)
Response rate for tenure index	2231	0.03	-0.03	0.06	-0.04
		(0.03)	(0.04)	(0.06)	(0.04)
Lower bound estimate	1801	-0.2***	0.03	-0.3**	-0.1
		(0.07)	(0.1)	(0.1)	(0.1)
Untrimmed estimate	1845	-0.09	0.06	0.02	-0.06
		(0.08)	(0.1)	(0.2)	(0.1)
Upper bound estimate	1800	-0.08	0.2*	0.05	0.1
		(0.08)	(0.1)	(0.2)	(0.10)
Response rate for land index	2231	0.03	-0.01	0.09*	-0.03
		(0.03)	(0.04)	(0.05)	(0.04)
Lower bound estimate	1820	-0.04	-0.1	-0.3*	-0.2
		(0.09)	(0.1)	(0.2)	(0.1)
Untrimmed estimate	1851	0.005	-0.08	-0.08	-0.08
		(0.09)	(0.1)	(0.2)	(0.1)
Upper bound estimate	1819	0.02	-0.04	0.06	-0.07
		(0.09)	(0.1)	(0.2)	(0.1)

Dependent variables are listed in the first column; column headings describe alternative specifications. First row in each section is a regression of the probability of respone for each index. Subsequent rows show lower, untrimmed and upper bounds following method outlined in Lee (2009) Each coefficient reports a separate treatment efffect estimate. Robust standard errors are listed in parentheses.

Table 11: Estimates of non-random response and trimmed results – sub-sample in reassigned area

	Observations	(1) Linear	(2) Quadratic	(3) Cubic	(4) Non-parametric
Response rate for investment index	1707	0	0	0	0
		(.)	(.)	(.)	(.)
Lower bound estimate	1707	0.2**	0.3**	0.2	0.2
		(0.09)	(0.1)	(0.2)	(0.1)
Untrimmed estimate	1707	0.2**	0.3**	0.2	0.2
		(0.09)	(0.1)	(0.2)	(0.1)
Upper bound estimate	1707	0.2**	0.3**	0.2	0.2
		(0.09)	(0.1)	(0.2)	(0.1)
Response rate for credit index	1707	0.05	0.008	0.2*	0.08
		(0.05)	(0.07)	(0.10)	(0.08)
Lower bound estimate	724	-0.5***	-0.08	-0.7***	-0.5*
		(0.1)	(0.3)	(0.3)	(0.3)
Untrimmed estimate	757	-0.1	-0.1	-0.5	-0.3
		(0.2)	(0.3)	(0.3)	(0.3)
Upper bound estimate	725	-0.08	-0.1	-0.4	-0.3
		(0.2)	(0.3)	(0.3)	(0.3)
Response rate for tenure index	1707	0.02	-0.07	0.03	-0.02
		(0.04)	(0.05)	(0.07)	(0.06)
Lower bound estimate	1369	-0.2**	-0.05	-0.4**	-0.09
		(0.09)	(0.2)	(0.2)	(0.2)
Untrimmed estimate	1382	-0.1	0.01	-0.2	-0.06
		(0.1)	(0.2)	(0.2)	(0.2)
Upper bound estimate	1368	-0.1	0.2**	-0.1	0.01
		(0.1)	(0.1)	(0.2)	(0.2)
Response rate for land index	1707	0.03	-0.04	0.06	-0.008
		(0.04)	(0.05)	(0.07)	(0.06)
Lower bound estimate	1383	-0.2	-0.3*	-0.4**	-0.07
		(0.1)	(0.2)	(0.2)	(0.2)
Untrimmed estimate	1388	-0.1	-0.3	-0.2	-0.009
		(0.1)	(0.2)	(0.2)	(0.2)
Upper bound estimate	1382	-0.08	-0.2	-0.1	0.05
		(0.1)	(0.2)	(0.2)	(0.2)

Dependent variables are listed in the first column; column headings describe alternative specifications. First row in each section is a regression of the probability of respone for each index. Subsequent rows show lower, untrimmed and upper bounds following method outlined in Lee (2009) Each coefficient reports a separate treatment efffect estimate. Robust standard errors are listed in parentheses.