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# Using Registry Data to Assess Gender-Differentiated Land and Credit Market Effects of Urban Land Policy Reform

## Evidence from Lesotho

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## Abstract

Since 2010, Lesotho has implemented legal and institutional changes to allow female land ownership, established a new land agency, reduced the cost of registering land, and carried out systematic urban land titling. Analysis using administrative data shows that these reforms triggered discontinuous and sustained changes in quality of service delivery, female land ownership, and registered land sales and mortgage volume. Land and credit market activation is, however, exclusively due to policy reforms. While (subsidized) systematic land registration allows women to access documented land rights, these effects may not be sustained without further regulatory change, highlighting the importance of reducing fees and streamlining processes to improve urban land and financial market functioning as a key precondition for Africa's expected wave of urbanization translating into productive cities and jobs.


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# Using Registry Data to Assess Gender-Differentiated Land and Credit Market Effects of Urban Land Policy Reform: Evidence from Lesotho*

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## 1. Introduction

With experts predicting that its urban population will more than triple by 2050 (Collier 2017), Sub-Saharan Africa is now the region with the fastest rate of urbanization globally (Henderson and Turner 2020). Several issues, including lower per capita income that in other regions when they experienced urbanization boosts (Jedwab et al. 2017); a weaker link between urbanization and economic growth in the hinterland due to globalization (Jedwab and Vollrath 2015); and a scope for resource rents fueling real estate bubbles (Gollin et al. 2016) pose unique challenges for the region. The way in which these will be solved will affect African cities' footprint and competitiveness for decades (Venables 2017).

Well-functioning land markets are critical to help make this transition by allowing transfers of land to its best use, facilitating credit access for investment in durable housing stock and providing the information needed for property taxation to incentivize efficient service delivery and planning to increase land values. As documented land rights are a necessary condition for land market operation, many programs focused on subsidized systematic titling without clear understanding of the regulatory and institutional preconditions to ensure such titles can be transferred at affordable cost and prevent reversion to informality over time. In fact, while studies document that titling programs were either ineffective (Jacoby and Minten 2007) or unsustainable due to an inappropriate policy environment (Gutierrez and Molina 2020), we know of no study -in Africa or anywhere else- of the impact of urban land policy reform and its relationship to titling.

To help address this gap, this paper assesses the impact of systematic titling as well as legal, regulatory and institutional reforms implemented under the Lesotho Land Administration Reform Project (LARP) relying on a range of data sources. Administrative data point towards clear discrete changes starting with adoption of regulatory reforms and sustained since in terms of (i) a reduction of the time needed to register transfers or mortgages; (ii) an increase in the share of newly registered plots owned (jointly or exclusively) by women matched by a decline of those owned exclusively by men; (iii) an upward shift and acceleration of the number of registered transfers and mortgages; and (iv) expansion of registry coverage resulting through a burst of project-supported systematic plot registrations from 2011 to 2013 and a more gradual growth of the number of plots registered sporadically.

Yet, a traditional DID approach on a matched household sample rejects the hypothesis of no significant effect on land markets or credit access. To probe if this reflects reality or may be attributed to measurement error or methodological issues such as the inability of a DID approach to identify effects of policy change that affect treatment and control areas equally, we use administrative data to implement an approach using block fixed effects that also allows identifying the effects of policy change separately from those of systematic registration. We implement this by dividing the entire project area into 1,932 blocks of 250*250m each and aggregating georeferenced registry and cadaster data at block level for each of the 20

years (2000-2019 inclusive) of interest. The data are then used for panel econometric analysis of program impacts -on owners' gender for newly registered plots and the number of registered land- and credit-market transactions- with block- and year- fixed effects.

Such analysis yields three key results: First, the likelihood of a newly registered plot having a female co-owner increased by 55 percentage points, one-third of which due to policy change and two-thirds to systematic registration. Second, the share of registered parcels sold or pledged as mortgage more than doubled, an effect that can be attributed to policy-induced reductions in the cost of first time (40%) and subsequent (60%) sporadic registration. Third, systematic registration had no impact on credit access, in line with what is found in the literature evaluating systematic registration programs (Agyei-Holmes *et al.* 2020; Field and Torero 2006; Galiani and Schargrodsky 2010). Substantive results are robust to restricting the sample to the areas chosen as treatment and control for the DID specification, suggesting that the lack of measured impact in the DID analysis is not merely due to selection issues. It confirms that in addition to going beyond traditional approaches allowing to measure to what extent policy changes were implemented and to identify their impact, registry data allow to avoid measurement error that has biased estimated program effects towards zero even for econometric models that are properly specified, a notion that is confirmed by comparing registry to survey data for the same households.

In light of the significant and quantitatively large impacts of regulatory reform, it is puzzling that, beyond elimination of the Ministerial consent requirement, implementation of reforms remained limited: contrary to Rwanda which moved from the 137$^{th}$ to the top rank in less than a decade, Lesotho still stands at 114$^{th}$ and registering a land transfer is estimated to cost 8.5% of property value. In addition to posing challenges for sustaining the (gender) gains made under the program, this also generates rents the elimination of which may be perceived to be politically too costly by policy makers who long relied on land as a means to reinforce clientelist ties. Our results suggest that regulatory reforms that reduce registration cost are key for improved urban land markets in Africa by catalyzing land and credit market participation from registered plots and, via sporadic registration, drawing in previously unregistered ones. Supporting such reform by highlighting and expanding associated benefits, harnessing sub-national competition, and offering the prospect of results-based approaches to implement, could significantly enhance the prospects for Africa's coming wave of urbanization to contribute to a virtuous cycle rather than a vicious one.

The paper contributes to several strands of literature: First, expected credit effects from systematic land registration as popularized by de Soto (2000) have become the standard justification for titling programs in the developing world although evidence of such effects is scant (Fenske 2011; Lawry *et al.* 2016). This led scholars and practitioners to increasingly discount credit effects from land titling as a myth the permeation, which will at best lead to misallocation of scarce public resources (Easterly 2008) and at worst to serious

and adverse distributional effects (Bromley 2009). Distinguishing policy or regulatory reform from titling and showing that these have very different land market activation effects allows us to reconcile these views. We document robust land market effects that can be unambiguously attributed to regulatory reform which, by reducing transaction costs, increased the number of registered transfers and mortgages for parcels that had already been registered or were registered sporadically in response to this policy change. This makes our paper the first to document robust credit market effects from regulatory and institutional reform. Our finding that (subsidized) systematic titling had a negative net impact on land market activity suggests that land market activation is best achieved via policy reform.

Second, while a large literature suggests that women having formal land rights enhances their well-being by increasing investment (Ali *et al.* 2014), labor force participation (Newman *et al.* 2015) and bargaining power (Menon *et al.* 2014), less is known on what is needed for women to have their land rights documented and participate in markets in the first place. Results suggest that legal change can help increase female land ownership, its full potential will be realized if combined with other measures. While in our case this was performed by systematic registration, financial incentives or nudges to include women on title documents (Ali *et al.* 2016) may be equally or more effective once regulatory reforms have been enacted.

Third, compared to an upsurge in the use of administrative data including for evaluating large programs (Muralidharan *et al.* 2016) or assessing tax compliance (Pomeranz 2015), the advantages of registry data in terms of frequency, granularity, reliability and zero marginal cost to evaluate land programs have not been fully realized with studies mainly remaining at a descriptive level (Ali *et al.* 2017; Holden and Tilahun 2020). We show that econometric analysis of spatially aggregated registry data can help identify impacts of policy reform that elude a traditional DID approach, in addition to remedying some of the shortcomings of household survey data. Establishing systems to provide such data routinely in land programs could thus enhance accountability; improve tracking of outcomes and understanding of the channels for impacts to materialize; and provide insights long after interventions have wound down.

The rest of the paper is structured as follows. Section 2 provides conceptual framing of the general challenges of evaluating land registration projects as well as the specific characteristics of the project to be considered here. Section 3 discusses descriptives based on administrative data and describes in detail the empirical methodology for evaluating the impact of this intervention using both household survey and administrative data. Section 4 discusses econometric evidence from analyzing household survey and administrative data, compares results from these, and draws out methodological and conceptual conclusions. Section 5 concludes by highlighting some implications for future research.

## 2. Background and program characteristics

A growing body of literature argues that the extent to which Africa's urbanization will contribute to growth of productivity and welfare will depend on the functioning of urban land markets. While efforts to improve their functioning often focused on systematic titling, this is a necessary but not sufficient condition. Without policy reform titling may not be sustained whereas in a more conducive policy environment, people will acquire title by themselves. Failure to address regulatory and institutional factors underpinning high barriers to land registration could thus potentially lock cities in a vicious cycle of urban informality. As it combined legal and institutional reforms -including major advances on women's rights- the program to be analyzed here allows us to separately analyze the impact of these components.

### 2.1 Conceptual issues and link to the literature

Having long lagged other regions in terms of urbanization, Sub-Saharan Africa is now undergoing a process of rapid urbanization (Henderson and Turner 2020) that is predicted to more than triple its urban population by 2050 (Collier 2017). The pace and nature of this process together with much lower levels of per capita income than in other regions at similar levels of urbanization (Henderson and Kriticos 2018) imply that Africa's urbanization poses enormous challenges. In light of the durable nature of any investments, the way in which cities are built will determine the potential and nature of economic growth in Africa for decades. Successful urbanization would allow penetration of global markets (Venables 2017) to create productive jobs (Chauvin *et al.* 2017). Failure, on the other hand could create unproductive and non-competitive cities where large parts of the population are stuck with low-quality buildings and poor service provision (Bird *et al.* 2017) together with attempts at private provision of key public goods by the affluent leading to residential segregation and social polarization.

Impersonal land markets and the institutional infrastructure to support them (Arrunada 2012) are critical to building vibrant and productive cities in several respects (Lall 2017). First, they are needed to bring land to its best use rather than having it locked up in slums or uses that, while consistent with political objectives, may have hugely negative welfare impacts.[1] Second, by facilitating use of land as collateral (Besley and Ghatak 2010), land markets can facilitate credit access to build durable housing stock that can support dense settlement to lower the cost of service delivery (including for transport) and cities' carbon footprint. Third, property registries and cadasters provide the data needed for property taxation to encourage efficient land use and generate local revenue, thereby encouraging a virtuous cycle where tax collection, service provision, and higher property values reinforce each other and in doing so have the potential to strengthen

[1] For example, in Nairobi, overcoming frictions to land reallocation would yield gains amounting to about thirty times annual rental payments per slumdweller even after all relevant parties have been compensated (Henderson *et al.* 2020).

the social contract (Weigel 2020), build state capacity (Besley and Persson 2014), and provide services in a proactive forward-looking way.

Documented land rights are a necessary condition for land markets. The fact that most of Africa's residents lack such rights has long provided the justification for so-called titling programs that mobilize external support to systematically issue title documents, often for free or at a cost (and speed) that would not be possible with the established system. In rural areas, award of formal land rights reduced the need for owners to spend resources on protecting claims (Goldstein *et al.* 2018) and improved investment (Melesse and Bulte 2015), especially by women (Ali *et al.* 2014; Deininger *et al.* 2008) for whom enforcing informal claims had been more difficult (Kumar and Quisumbing 2015; Melesse *et al.* 2018; Muchomba 2017). In line with land markets' potential to improve efficiency of land use (Restuccia and Santaeulalia-Llopis 2015), reduce misallocation (Chen *et al.* 2017), and facilitate inter-sectoral reallocation of labor (Chen 2017), documenting rights increased rural out-migration and productivity in Mexico (de Janvry et al. 2015). Yet, evidence of credit effects has been rare (Lawry *et al.* 2016), with only one study finding that rural titling increased credit market access for the better off (Carter and Olinto 2003) and evidence of reductions in transaction cost through digitization increasing urban credit access (Deininger and Goyal 2012).[2]

While efforts at urban land tilting in Africa have been less common, evidence from elsewhere shows that such programs yielded benefits but generally failed to improve market functioning. Instead titling increased formal labor market participation in Peru (Field 2007) and increased human capital investment (Galiani and Schargrodsky 2004), political attitudes (Di Tella *et al.* 2007) and risk aversion (Aragón *et al.* 2020) in Argentina. Yet, (peri-) urban titling had effects on credit access neither in Argentina (Galiani and Schargrodsky 2010) nor in Peru (Field and Torero 2006) or Ghana (Agyei-Holmes *et al.* 2020). More importantly from a policy perspective even where titles were successfully issued, high cost of registering subsequent transactions caused reversals to informality in Jamaica (Barnes and Griffith-Charles 2007), the Philippines (Maurer and Iyer 2008), Argentina (Galiani and Schargrodsky 2016) and Peru (Gutierrez and Molina 2020). The case of Peru, where elimination of a low-cost registry for residents of recently formalized low-income neighborhood was abandoned in response to pressure by notaries, provides a natural experiment to illustrate the importance of prices. This shows that increases in registration costs were accompanied by a drop of more than 8% in the probability of registration (Gutierrez and Molina 2020).

Potentially limited sustainability and land market impacts of titling activities suggest that expected impacts of titling may be constrained by the regulatory and institutional environment that defines how transfers of rights -across generation in the same household or with others- can be registered. At about 8% of land value,

[2] Policy makers in Tanzania placed enormous hopes in non-transferable documents as a means to improve credit access in rural (Stein *et al.* 2016) and urban areas (Kironde 2006). These were bound to be disappointed (Kusiluka and Chiwambo 2019).

the cost of land registration in Africa are higher than in any other region.[3] Many countries' systems seem stuck in a vicious cycle where outdated procedures -often dating back to colonial times- and limited use of information technology (IT) increase costs of service delivery to a point where many potential beneficiaries can no longer afford getting their rights -or changes in their rights- registered. Limited interoperability then often further reduces titles' value and banks' ability to use them as trustworthy evidence of actual rights that can be accepted as collateral.[4]

Breaking this cycle and improving land market functioning will require regulatory and institutional reforms to make registration more affordable. This would eliminate barriers to formalizing transfers (or mortgages) for parcels that had already been registered while also encouraging owners to register parcels via existing channels, a process often referred to as sporadic registration. While the importance of regulatory reforms is widely acknowledged and has led to inclusion of policy reform components in most titling programs, the lack of rigorous assessment of either outcomes or impact of such reform is an important knowledge gap; in fact, we know of no quantitative analysis assessing the impacts of regulatory reforms or its interaction with systematic title issuance.

### 2.2 Program description and evaluation strategy

To help close this gap, we use the US$ 20.5 million Land Administration Reform Project (LARP) that was part of the Millennium Challenge Corporation's (MCC) 2008 to 2013 Lesotho Compact. Project activities focused on first time systematic land registration to create a basis for land markets and legal, regulatory and institutional reform to improve the functioning of urban land and financial markets.[5]

The systematic land registration/titling component -which accounted for most of LARP spending- aimed to issue 90-year leases essentially free of charge to some 55,000 land plots from informal settlements in urban and peri-urban areas of Maseru, a city of some 300,000 inhabitants. The remainder of program resources was devoted to a subcomponent supporting legal and institutional reforms to enhance effectiveness and efficiency of land service delivery to reduce the cost of registering land transfers and mortgages and to improve women's access to land rights through passage of a new Land Act and associated regulations as well as establishment of a new Land Administration Authority (LAA) in 2010.

[3] Data from the registering property section of the World Bank's 'Doing Business' indicators shows that registering a land transfer in Africa (a lower bound for first time registration) still costs close to 8% of land value, a figure that changed somewhat since 2005 (Deininger *et al.* 2020a).
[4] Applicant characteristics such as having a stable income as well as the benefit stream from the proposed use of borrowed funds will be the main factors affecting whether or not they will receive a loan. However, if there is a non-zero risk of default, a lender's willingness to lend funds to applications will be affected by $pV-f$ where $V$ is the value of land owned, $f$ a fixed foreclosure cost $f$, and $p$ the likelihood of finding a buyer.
[5] To avoid confusion between first and second registration, we refer to first registration (either sporadic or systematic) as 'titling' or 'issuance of a title/lease'. The subject(s) of this transaction are referred to as 'owners' although technically they are leaseholder(s).

A historic achievement by the Land Act was that, for the first time, it recognized married spouses' right to own land, eliminating earlier prohibitions on married women holding land title (Shale 2019).[6] This is similar in scope to the demise of coverture in the United States in the 1850s, an intervention shown to have affected human capital accumulation (Geddes *et al.* 2012), employment choice, financial sector development and levels of industrialization (Hazan *et al.* 2019).

To improve access to documented land rights and functioning of land and mortgage markets, procedures were streamlined to remove barriers to registering land rights and reduce transaction costs of registration. Regulatory reforms also simplified planning standards, eliminated the need for Ministerial consent prior to lease issuance, and established the LAA as a one-stop shop to speed up registration.[7] Quality of registry data was to be improved by creating a digital register that would link textual to spatial records and facilitate maintenance through interoperability with other registries so banks could be confident in using land as collateral that could actually be repossessed in case of default.

The initial project design aimed to evaluate program impacts based on a randomized roll-out of systematic registration activities. Contamination of the control group made it impossible to adhere to this strategy and resulted in its replacement with a DID strategy in 2013. To this end, four of Maseru Municipality's Councils (MMCs), spatial units elsewhere referred to as wards in Maseru's Northeast where systematic regularization activities had not yet started when this defect was discovered, were selected for impact evaluation. Their boundaries and location relative to Maseru city are illustrated in figure 1. Wards MMC 1, 2, and 3, then next in line for implementation, were designated as treatment and ward MMC 27 as control.

This design raises several concerns. First, as the wards chosen for impact evaluation are located at the fringe of Maseru in areas with lower settlement densities, the extent to which they are representative of the intervention area as a whole is doubtful. Second, controls are more distant than treatment areas from the CBD throughout, limiting comparability. Finally, as treatment and control are contiguous, information spillovers may occur. The administrative data discussed below allow us to check if such concerns are justified.

## 3. Data and analytical approach

Administrative data from before and after regulatory reforms suggests that the 2010 reforms triggered discrete and sustained changes in (i) the quality of service defined as time needed to register a transaction; (ii) the share of plots registered in the name of females either individually or jointly; and (iii) the number

[6] This applies to land held under civil or customary law. Prior to passage of the Act, women were considered as perpetual minors who could contract or own property only via assistance from a guardian, i.e., their father, husband or their husband's heir.
[7] In addition to the 2010 Land and LAA Acts, laws and regulations were passed to regulate systematic land tenure regularization, planning, surveying, registry operation and dispute resolution.

of registered transfers and mortgages. We describe the DID strategy used to evaluate the impact of titling and, as policy changes that affect everybody equally will not be identified by such an approach, complement it with spatial aggregation of administrative as the basis for before-after regressions using block fixed effects to provide *inter alia* estimates of reform impacts on the likelihood of sporadically or systematically registered plots being subject to a registered sale or mortgage transaction.

### 3.1 Evidence from administrative data

To obtain a complete set of registry and cadaster data covering the period of 1981 to 2019 inclusive for all of Maseru as a basis for assessing program impacts, paper records for all registered transactions (including transfers, and mortgages) for the period before LAA migrated to a functional digital platform were digitized. Although such data lack information on owners' socio-economic characteristics or the presence, size or characteristics of structures, they include information on gender of registered owner(s) as well as start and end date as well as type of each transaction, allowing us to compute actual time required for completing a registration as an intermediate outcome. [8]

Table 1 presents descriptive statistics using these data for the entire 39-year period (1981-2019) for which data are available in column 1 and three sub-periods, namely (i) the 1981-2009 pre-reform period (column 2); (ii) the 2010-2013 implementation period following passage of the Land Act and establishment of LAA (column 3); and (iii) the 2014-19 post-program period of routine LAA operation (column 4). Wherever possible, figures are disaggregated by subject of registration to assess the reform's gender focus.

The rate of annual titles issued increased from 560 before reform to 12,450 during implementation before leveling off at 4,020 in the post-implementation period. While all registrations were sporadic before 2010, 87% and 39% followed a systematic approach during and after implementation, respectively, bringing the total number of systematically registered plots to 52,700 or 58% of the total. [9] Figure 2 shows that the number of sporadically issued leases increased at a slow pace before 2010 and accelerated thereafter. Project-supported systematic registration was piloted in 2011 and 2012, rolled out at large scale in 2013, and dropped to essentially zero after 2014.

We also note a monotonic increase (from 137 in the pre- to 480 in the post-implementation period) in the annual number of registered mortgages; an increase in the share of mortgages issued for plots jointly held or registered exclusively to women from 2% to 41% and 18% to 23% with an average loan size of about US$ 41,000 in the post-implementation period; and a decrease in the loans issued to plots registered

[8] Contrary to the World Bank's 'Doing Business' indicators that refer to a hypothetical transaction and thus constitute a lower bound if procedures are followed efficiently, these figures refer to actual processes.

[9] Table 1 shows that the share of commercial as well as residential land decreased from 15% and 83% in the pre-reform period to 6% and 79% in the post-reform era, a shift largely accounted for by an increase from 4% to 15% in the share of newly registered 'other' or mixed use land.

exclusively to men from 73% to 29% with a somewhat higher mean loan size of US$ 54,000 in the post-implementation period. While the share of mortgages issued to companies remained more or less constant at 7%-8% of the total, the value of such loans was well above US$ 100,000.

To assess the impact of policy reform, we use annual data for the 20-year period (2000-2019 inclusive) that covers 10 years each before and after the intervention and present graphical evidence using nonparametric regressions on key outcomes as well as program characteristics. Figures 3 and 4 display results for the number of days required to register a mortgage or transfer, suggesting that the 2010 reform coincides with a clear break. The number of days to process the average mortgage declined from some 250 a decade before the reform (including 98 days on average to obtain consent, a requirement that was eliminated by the reform) to less than 50 days in the immediate post-reform period. With a reduction from 500 to some 250 days, reductions in the time required to register land transfers were less pronounced, though still significant.

To create spatial units that can serve as the basis for impact evaluation with block fixed effects, we define a total of 1,932 blocks each 250*250m in size and aggregate annual values of relevant variables as basis for panel econometric analysis. The principle is illustrated in figure 5 where grid cells indicate block boundaries and registered plots are colored depending on whether they were registered sporadically before (purple) or after (brown) the 2010 reform or systematically through LARP after 2010 (blue). Plots not registered in the name of a private individual including public spaces are white. This is overlaid with 2016 building footprints extracted from high resolution satellite imagery using machine learning.[10]

Non-parametric regressions using plot and block level data on the share of newly registered parcels by gender support the notion of a discrete break in the reform year 2010, that is often followed by a change in slope. Figure 6 (plot level) suggests that eliminating restrictions on female land ownership significantly affected registered landowners' gender:[11] the share of leases issued to males individually -which stood at 60%-70% before the reform- dropped to about 15% thereafter. The share of new leases issued jointly in the name of men and women increased from about 10% before reforms to more than 50% thereafter where it stabilized for the next 5-7 years. A less pronounced, though still statistically significant shift occurred for leases issued in the name of women only which increased from 29% before to 33% after reform.

Policy-induced changes extend beyond registered lessees' gender to formal land and mortgage market activity. Figure 7 shows the number of registered transfers shifting upwards discontinuously with reform and accelerated thereafter to about 0.4 per block. Transfers of plots registered (individually or jointly) in

[10] Hand labeling of structures in 2013 and 2016 to provide the basis for the regressions reported in table 2 was undertaken only for treatment and control areas. Support from Ecopia, led by Brandon Palin in generating these footprints, is acknowledged.
[11] We focus on natural persons only, noting that, as indicated in table 1, companies' share of registered leases dropped from 7% before and 1% during implementation of the systematic registration campaign before increasing to 14% thereafter.

the name of women grew faster post-reform than those registered to males. While increases in the number of registered mortgages (figure 8) were less pronounced, levelling off at 0.18 per block, the number of mortgages on jointly owned plots displayed a rapid increase.

Beyond exploring shifts in registration or changes in the properties of registered plots due to policy reform, administrative data also allow assessing the extent to which concerns about the evaluation design mentioned earlier may be relevant. Table 2 tabulates such data for the 2000-2019 period together with number and area of buildings in 2016 at block level for all of Maseru (column 1), and separately for areas identified as treatment (col. 3) or control (col. 4) and the rest of the city (col. 2). With a size of 62,500 $m^2$ of which 44% was privately held and registered in 2019, the average block includes about 28 registered plots of some 1,000 $m^2$ each titled using a sporadic (11) or systematic (17) process.

Beyond confirming trends noted earlier such as increases in the number of plots registered both sporadically and systematically and registered transfers and mortgages, table 2 shows pre-program differences between treatment (MMC 1-3) and control (MMC 27) that are relevant in two respects. First, a higher number of plots registered sporadically before the program in treatment than in control areas (6.23 vs. 0.33 in 2000 and 9.23 vs. 1.22 in 2010) suggests limited comparability between the two. Also, the number of sporadic registrations pre-program in wards MMC 1-3 (6.23 in 2000 and 9.23 in 2010) exceeds that in the remainder of Maseru (2.98 in 2000 and 5.73 in 2010), pointing towards limited representativeness of these areas for the intervention.[12] While interpreted imagery is available only for 2016 (i.e., post-intervention), it supports this argument by pointing towards a much higher number of structures or built-up area in the wards assigned to the treatment group (75 vs. 31 structures and 7,045 $m^2$ vs. 2,132 $m^2$ of built up area, respectively).

### 3.2 DID approach using household survey data and structures extracted from satellite imagery

To provide data for a DID impact evaluation in the four wards (MMC 1-3 and 27) selected as treatment and control as described above, a baseline survey on a sample of 1,904 households was conducted in 2013 as described in more detail by Maredia et al. (2016), followed by an end-line survey in 2019, almost 6 years after the baseline. Both surveys used a household and female instrument and the end-line survey instrument was identical to that used in the baseline except for minor modifications to improve response rates, capture changes in household composition and landownership, and provide detail on participation in the systematic registration program.

Baseline survey data as reported in data appendix table 1 indeed point towards significant differences in observed attributes between the groups. Propensity score matching (Rosenbaum and Rubin 1983)

[12] Levels of systematic registration were similar between treatment and the rest of the intervention area and with an average of 0.5 plot registered systematically in the control, data suggest that contamination remained limited.

based on pre-intervention characteristics is thus used to improve comparability between the two groups. Once households with missing data (including attrition at follow-up) or those with formal certificates at baseline are dropped, we are left with 1,215 valid observations (775 and 440 from treatment and control, respectively). Using kernel-based matching eliminates 58 off-support treated households, resulting in a matched sample of 1,157 (717 treatment and 440 control) and also reduces the mean (median) percentage bias from 17.6 (15.5) to 3.8 (3.2), creating balance between treatment and control in almost all variables (appendix table 1).[13]

A difference-in-differences (DID) approach on the matched sample would then allow assessing LARP impacts on outcome variables such as female access to land rights and land as well as investment and credit-market participation. Letting *i* denote households and *t* time, the equation to be estimated is

$$Y_{it} = \alpha_i + \delta_1 T_i * P_t + \delta_2 T_i * P_t * FH + \lambda P_t + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{it} \quad (1)$$

where $T$ is the treatment dummy ($T$=1 for treatment group, $T$=0 for control group), $P$ is a time dummy ($P$=1 for post treatment period, $P$=0 for the pre-treatment period) to control for common time-trend, $\alpha$ captures observed and unobserved time invariant individual heterogeneity, $FH$ is a dummy for female head, $\boldsymbol{X}$ is a vector of time varying control variables (including land size and annual per capita expenditure), $\delta_1$ is the key parameter of interest measuring the average effect of the land tenure regularization program and $\delta_2$ captures heterogeneity effect by gender of the head of the household. Outcome variables $Y_{it}$ focus on participation in land markets and land-related investment as well as credit market access.

Aerial photography from before (2012) and after (2016) the intervention allows extracting of finished or unfinished structures at two points in time as an 'objective' measure of investment that may suffer less from bias than household survey responses.[14] Appendix figure 3 provides some examples for such changes. We use a main road separating treatment and control in the area of interest (figure 9) to implement a spatial discontinuity design along this road by estimating (1) with area under finished or unfinished structures as outcome variable for different bandwidths. This is a plausible design if, before the project, households either side were similar in observed and unobserved characteristics.

[13] Estimated mean and median bias are lowest for kernel-based vs. to one-to-one or two-nearest neighbor matching, an impression confirmed by visual inspection. Standardized percentage biases, defined as the percentage difference of sample means in treatment and control groups (unmatched or matched) as a percentage of the square root of the average of the sample variances in the treatment and control groups (Rosenbaum and Rubin, 1983), for different covariates as displayed in appendix figures 1 and 2 illustrate that, compared to being widely spread for the unmatched sample, biases are concentrated around zero in the matched sample.

[14] Imagery was available for 2012 (at 20 cm resolution) and 2016 (at 35 cm resolution) to guide program implementation and census mapping, respectively. Finished and unfinished structure were hand digitized. Unfortunately, the resolution proved insufficient to robustly discern changes in characteristics such as presence of fences, walkways, and complexity or type of roofs that could be made out from drone imagery using machine learning (Gevaert *et al.* 2020).

### 3.3 Before-after approach with block fixed effects

While the above design will provide robust evidence on the impact of land titling, effects of policy changes that affect treatment and control areas equally are not identified. To separately analyze these impacts and those of new systematic or sporadic titling on newly registered owners' gender and the share of plots for which transfer in land or mortgage markets were registered, we use annual data at block level for 2000-2019 as described above. Block and year fixed effects are used to control for time invariant local conditions and time variant common shocks. If included as an independent variable, the lagged stock of sporadically registered/titled plots is instrumented by its second lag.

Formally, for each block *i* and year *t*, we have the number of plots (denoted by $r_{it}$) that entered the registry for the first time in *t*, separately for systematic ($r_{it}^{v}$) and sporadic ($r_{it}^{p}$) modalities of registration/titling. We also have information on the stock of registered plots at *t*, denoted by $R_{it}$ overall and by $R_{it}^{v}$ and $R_{it}^{p}$ for systematic and sporadic modalities, respectively. For both stock and flow, we know if plots entering the registry at *t* are in the name of females alone ($r_{it}^{F}$ *or* $R_{it}^{F}$), males alone ($r_{it}^{M}$ or $R_{it}^{M}$) or males and females jointly ($r_{it}^{J}$ or $R_{it}^{J}$). Letting $P$ be an indicator variable for the post-2010 regime when the Land Act was effective and LAA had been established, the basic equation to be estimated is

$$Y_{it} = \alpha_i + \beta_1 r_{it} + \beta_2 r_{it} * P + \lambda_t + \varepsilon_{it} \quad (2)$$

where $Y_{it}$ is either $r_{it}^{F} - r_{it}^{M}$ or $r_{it}^{J} - r_{it}^{M}$, i.e., the difference between the number of plots registered in the name of females individually or jointly and that of men or $r_{it}^{J} - r_{it}^{F}$, the difference between the number of plots registered in the name of females jointly and individually; $\alpha$ is a block fixed effect, $\lambda$ is a time fixed effect, and ε is the error term. Our interest in (2) is in $\beta_2$, the point estimate of the program effect as the total impact of LARP-induced changes.

Conceptually, estimated program impact comprises three elements, namely the effects of (i) a decrease in the cost or complexity of first-time sporadic registration; (ii) a reduction of the transaction cost of registering transfers and mortgages; and (iii) systematic titling of plots in target areas through a process of awareness raising and elimination of cost for title issuance. As the number of plots registered/titled sporadically and systematically before and after policy reform is observed and $r_{it} = r_{it}^{p} + r_{it}^{v}$, it is possible to isolate impacts of policy reform from those of systematic titling by estimating

$$Y_{it} = \alpha_i + \gamma_1 r_{it}^{p} + \gamma_2 r_{it}^{p} * P + \gamma_3 r_{it}^{v} + \lambda_t + \varepsilon_{it} \quad (2')$$

where variables are as above, $\gamma_1$ is the estimated direct effect of the increase in sporadic registrations (pre-reform), $\gamma_2$ the estimated impact of the 'pure' policy change on sporadically registered plots, and $\gamma_3$ the estimated impact of policy changes plus systematic titling. The estimated impact of systematic titling over

and above the policy change on women's (individual or joint) access to registered land can be computed as $\gamma_3 - (\gamma_1 + \gamma_2)$ and its statistical significance be tested using standard tests.

A key project assumption was that, without program-induced policy reform, high cost (and poor quality) of registration pre-reform might have limited participation in land or financial markets. To assess impacts in this dimension, we use the above framework but replace flows ($r_{it}$) with (lagged) stocks ($R_{it-1}$). Potential endogeneity of sporadic registration arising from the fact that individuals may register a plot to immediately transact it or take out a mortgage on it are addressed by instrumenting $R^p_{it-1}$ with the second lag $R^p_{it-2}$. Denoting instrumented variables by $\hat{R}^p_{it-1}$ and distinguishing stocks of plots registered/titled sporadically before and after the reform as well as systematically,[15] we estimate

$$Y_{it} = \alpha_i + \theta_1 \hat{R}^{p,pre}_{it-1} + \theta_2 \hat{R}^{p,post}_{it-1} + \theta_3 R^{v,post}_{it-1} + \lambda_t + \varepsilon_{it} \quad (3)$$

where $Y_{it}$ is the number of formally registered plots in block $i$ that had either a transfer or a mortgage registered in $t$ and $\theta_1, \theta_2$ and $\theta_3$ are coefficients to be estimated. Specifically, $\theta_1$ is the likelihood of being transferred or mortgaged for plots that had been registered sporadically before 2010; $\theta_2$ denotes the combined impact of reform-induced reduction of barriers to first-time (sporadic) registration and the reduction in transaction costs of subsequent registration brought about by policy reform on the likelihood of registering a transfer or mortgage on a property; and $\theta_3$ denotes the combined effects of policy reform and elimination of first-time registration cost on the likelihood of registering a transfer or mortgage on a property. The net impacts of reducing barriers to first time sporadic registration or of systematic registration can then be expressed as $(\theta_2 - \theta_1)$ and $(\theta_3 - \theta_2)$, respectively.

## 4. Econometric results

Estimates of titling impacts using the DID design show insignificant impacts on market participation or investment. By contrast, analysis of administrative data indicate significant positive effects, overall and for systematic and sporadic titling separately, on the gender of registered right holders as well as policy reform effects on the share of titled plots being subject to a registered land or mortgage. Restricting the sample to the areas chosen as treatment and control for the DID specification affects estimated coefficients' magnitude but not significance and comparing registry to survey data for the same household points towards measurement error as a key factor that biases estimated program effects towards zero. We use this to assess distributional effects and discuss why, despite large impacts, urban land policy reform or titling more generally is rarely observed in practice.

[15] Unit prices for registered transactions of US$15/m$^2$ and US$12/m$^2$ in the pre-and post-implementation period and US$17/m$^2$ during implementation are difficult to interpret without additional information on property characteristics.

### 4.1 Results from the DID approach

Estimated program effects based on estimation of (1) are reported in table 3 panels A and B with columns 1, 3, 5, and 7 without and columns 2, 4, 6, and 8 with heterogeneity of gender effects. Regressions are weighted by the matching weight and all standard errors are clustered at village (survey cluster) level.

Results in panel A suggest no statistically significant effect on the share of land/property managed or controlled by women except for female headed households where the point estimate of LARP effects is about 11 percentage points compared to a control mean at baseline of 35.1 percent. Estimated program effects on knowledge about women's inheritance rights and LAA existence remain largely insignificant (except about the 4 percentage points increase in the former for male-headed househols and about the marginally significant 10 percentage points increase in the later for female-headed households), implying weak impact on female empowerment and awareness of institutional reforms. With less than 5% of the sampled households from the control area expressing concern of becoming involved in land-related conflict at baseline, perceived tenure security seems to have been high even without the program so that an estimated program effect of about 5 percentage points, significant at 5%, seem non-negligible.

Although land market activation was a key project objective, the data presented in panel B suggest no significant impact on land purchase market activity. This holds irrespectively of whether we use responses to retrospective questions about land transactions for the last 3 (i.e., 2010-2013 and 2016-2019) or the last 7 years (i.e., 2007-2013 and 2013-2019) for baseline and endline, respectively. Finally, panel C suggests that estimated program effects on credit market access, proxied by whether a secured home loan was taken, the household applied for a formal loan or had a credit card or bank account are all insignificant (except a marginally significant negative effect on loan application in col. 3). The only coefficient that is precisely estimated (significant at 1%) suggests a treatment-induced 20% decrease in the likelihood of land-related investment, consistent with the notion of investment having been made to stake out claims in anticipation of formal registration.

Data on area under complete or incomplete structures from satellite imagery as described above allows estimating investment effects in a more narrow band along the road separating treatment from control area. Table 4 displays results from using built-up area for all (cols. 1-3) or finished (cols. 4-6) structures for 40,684 structures in the entire treatment and control areas (cols. 1 and 4) or 16,236 and 8,578 in the 1,000 m (cols. 2 and 5) and 500 m (cols. 3 and 6) bands on both sides of the road, respectively. The estimated coefficient on the relevant parameter is negative throughout but loses significance for the 500 m band (and the 1,000 m band for finished structures). As mean built-up area per plot at baseline in the control was about 50%-60% of the level in the treatment group, investment in built-up area seems to be driven more by urban expansion and catch-up than by access to documented land rights. We thus reject the hypothesis of positive

program impacts on investment over and above a significant and positive trend (with an estimated increase of about 21 $m^2$ or about 30% of the pre-project mean for the narrowest band).

### 4.2 Results from administrative data

To see whether insignificant results are attributable to lack of program success or methodological and data limitations, we use administrative data. Results from estimating (2) as reported in columns 1, 3, and 5 of table 5 illustrate that, before policy reform, registration of new leases was strongly biased against women with point estimates for $\beta_1$ in regressions for female vs. male (-0.25), joint vs. male (-0.48) and joint vs. female (-0.23) registration indicating size of female disadvantage. The combined actions taken as part of the program reversed this pattern. Point estimates for $\beta_1 + \beta_2$, the joint effect of policy reform and systematic registration are all positive and large with coefficients of 0.18 for the female vs. male and 0.37 for joint vs. male individual as well as 0.18 for joint vs. female rights. While the likelihood of a plot being registered in the name of a woman individually did not change (as an estimated marginal gain of 0.18 was outweighed by a commensurate increase in joint registration), the project is estimated to have changed the marginal likelihood of a newly registered plot to be held (registered) in the name of women - individually or jointly- by 0.55, a meaningful and rather big effect.

Table 5 cols. 2, 4, and 6 provides results from (2') to separate the net effect of policy change $(\gamma_1 + \gamma_2)$ from that of systematic registration $\left(\gamma_3 - (\gamma_1 + \gamma_2)\right)$. It suggests that 35% and 29% of changes in female and joint vs. male individual registrations and 22% of the change in joint vs. female individual registration can be attributed to policy changes and the remainder to systematic first-time registration. The importance of first-time registration to enforce policy changes on female land ownership is consistent with the notion that changes in women's legal rights to land alone may not become fully effective -and may even result in effects opposite to what was intended (Anderson and Genicot 2015)- unless they are disseminated in a way that makes them socially acceptable (Bhalotra *et al.* 2018). Systematic registration increased the share of jointly registered land plots by reducing the share of those registered individually by either men or women.

Results from estimating (3) for the number registered mortgages at block level are in table 6 panel A with estimates for all registered plots in column 1 and those owned exclusively by men, women, or jointly in columns 2, 3, and 4, respectively. In contrast to results from the DID specifications using household survey data, estimated coefficients for sporadic and systematic registration are precisely estimated with point estimates suggesting significant impacts.

Focusing on sporadic registration first, we note that $\theta_2$, the estimated marginal effect for plots sporadically registered after 2010 is, with 0.0273 well above $\theta_1$ (0.0160), the effect for plots sporadically registered before 2010 which is also the upper bound of the 'pure' effect of reducing the transaction costs of mortgage

registration. A point estimate of 0.011 for $(\theta_2 - \theta_1)$ implies that barriers to first-time registration that, in pre-reform period, prevented collateralizable plots from entering the registry accounts for some 40% of the reform effect with the remainder due to reductions in regular registration cost. Gender differences between the estimate of $\theta_2$ - $\theta_1$ for plots registered individually by males (0.02; see col. 2), females (-0.0065; see col. 3) and jointly (insignificantly different from zero; see col. 4) are pronounced. Follow-up research to explore if these can be attributed to differences in plot characteristics or if, despite changes in legal provisions regarding registration of land rights, mortgage and financial markets remain biased against females.

For systematically registered plots, the point estimate for $\theta_3$ is positive and significant overall (0.0016) as well as for plots held only by females or jointly (see cols. 3 and 4). Yet, the size of the estimated overall coefficient is an order of magnitude smaller than that for $\theta_1$ (0.016), implying an estimate for $\theta_3$ - $\theta_1$, the marginal suitability for mortgaging plots registered systematically, of -0.0144. This means the net marginal effect of systematic registration was negative, presumably as systematic registration focused on parcels with attributes that make them less mortgageable. Exploring to what extent observable owner attributes (type of job, income, assets, etc.) or plot characteristics (size, distance to CBD, neighborhood public goods) can explain such differences is an area for future research that could provide interesting insights into the operation and evolution of Lesotho's mortgage markets.

Results from (3) for the number of registered transfers of all (registered) plots in table 7 panel A, using the same format as in table 6, point towards significant land market activation effects throughout. The marginal transfer registration effect for plots that were sporadically registered after 2010 is, with 0.041 almost double that for plots sporadically registered before reform (0.022), suggesting that barriers to first-time registration prevented marketable plots from entering the registry before reform. Similarly, the point estimate of 0.0039 for systematically registered plots is an order of magnitude below that on post-2010 sporadic registration, pointing towards marketability of systematically titled plots well below that for plots registered sporadically before (a point estimate $\theta_3$ - $\theta_1$ of -0.0185 for) or after 2010 (a point estimate of $\theta_3$ - $\theta_2$ of -0.0371).

As was observed for mortgages, the estimated marginal impact of eliminating barriers to (sporadic) first registration $(\theta_2)$ is significantly larger than that of reducing the transaction costs of registering transfers $(\theta_1)$ for all parcels (col. 1) and those transferred by male owners (col. 2). While systematic registration had a significant and positive marginal impact on the frequency of registered transfers, the estimated coefficient $(\theta_3)$ is below that of $\theta_1$ except for plots transferred by exclusively male owners. Systematically registered plots thus seem much less transferable than those registered before 2010, more than outweighing positive effects from policy reform.

### 4.3 Robustness check and incidence of measurement error

To explore if limited representativeness and comparability of the areas chosen as treatment and control might explain differences in results between the DID approach and the block panel based on administrative data, we re-estimate (2) and (3) only for the 455 blocks in wards MMC 1, 2, 3 and 27 chosen for LARP impact evaluation. Results from these regressions are in panel B of tables 5, 6, and 7. While differences in the size of some coefficient estimates between restricted and full samples indeed imply that the wards chosen to evaluate LARP impact are not a good representation of Maseru overall, virtually all coefficient estimates for the restricted sample are still highly significant statistically, though in some cases slightly smaller intervention effects. It suggests that sample selection alone cannot explain the lack of significant results in the DID specification.

Several differences emerge. First, as to gender of registered owners (table 5 panel B) we find that (sporadic) registration of land plots in distant wards was more gender-biased than in the full sample; the point estimate for $\beta_1$ is -0.50 in the restricted and -0.26 in the overall sample. Gender effects are broadly comparable to those in the unrestricted sample in terms of the likelihood of joint vs. male (0.34 vs. 0.37), female vs. male (0.23 vs. 0.18), and joint vs. female (0.10 vs. 0.18) registration. Registration modalities display marked gender differences with sporadic registration favoring female sole owners (0.47) over joint ones (-0.35) and the opposite for systematic registration (-0.25 for female vs. male and 0.48 for joint vs. female).

While the elimination of barriers to first-time registration ($\theta_2$) was estimated to have been more important than reduction of the transaction costs for registering mortgages ($\theta_1$) in the total sample, the opposite is true for the restricted sample (table 6); estimated coefficients on plots (sporadically) registered pre-2010 are larger than for those registered post-2010, in line with the notion that plots in outlying wards are less suitable as collateral. For registered land transfers (table 7), $\theta_2 - \theta_1$ is not significantly different from zero except for plots transferred by sole female owners. Point estimate for systematically registered plots (0.0014 for mortgages and 0.0039 for land transfers) are of comparable magnitude to those for the entire sample.

We test if measurement error may bias results from the DID approach towards zero by comparing (accurate) registry data to responses obtained in the survey for the same households.[16] Spatially matching sampled plots for the household survey with the cadastral map (table 8) suggests that in 26% of cases information is reported incorrectly. Type I errors (survey respondents claiming to have lease document although nothing is present in the registry) and type II errors (respondents indicating lack of a legal land document in their household while a lease is registered in the registry) occur with approximately equal frequency. Both types

[16] The proper way to eliminate discrepancies -and understand reasons for them- is to pre-populate the household survey with relevant information from administrative data. As digitization of registry data was carried out in parallel to household survey implementation, this was not possible here..

of errors are lowest for plots without a legal document (17%), followed by those with documents issued to men and women jointly (30%), individually to women (35%), and individually to men (54%). This is consistent with the notion that information on nature of land ownership documents held or owners' identity may not be fully shared among household members (Ashraf 2009) with men generally more secretive about their documents and more likely to falsely claim having a document.[17]

As mortgages and transfers are rare events, the sample sizes commonly used in household surveys may make it difficult to arrive at precise estimates, possibly magnifying the effect of measurement error. Of the plots registered by our survey households, 60 had a mortgage registered (with the entry dating from 2013 in 32 and from after 2013 in 28 cases). But only for 22 of these (37% of the total) did household survey respondents report existence of a mortgage or having taken out a bank loan. Routine availability of administrative data can thus help reduce measurement error by cross-checking survey information against registry entries and by stratifying household survey samples, allowing to combine data and make inferences on distributional aspects, heterogeneity of impacts, and the extent to which land or credit market activity triggered processes such as gentrification that are impossible with administrative data on their own.

### 4.4 Interpreting the results

If, as suggested by the above figures, policy reform lowered barriers to titling, one would expect to see differences in observable characteristics between plots registered in pre- and the post-reform periods. To test this, we regress the type of titling (systematic/sporadic before or after the policy change) on geographic plot attributes in particular distance to CBD and size. Results in appendix table 2 suggest that plots that entered the registry sporadically before reform were larger and closer to the CBD while focus thereafter shifted towards smaller plots (a coefficient much larger in absolute terms than for systematic adjudication).

Concerns about negative distributional impacts as a result of biased access to information (Jansen and Roquas 1998) or a fixed cost element for registering transfers or mortgages (Carter and Olinto 2003) have long been raised in the literature. Although available data limit our ability to explore this, assuming that smaller plots more distant from the CBD are less valuable allows us to test the link between these proxies for plot value and land or mortgage market participation by interacting right hand side variables in equation (3) with parcels' distance to the CBD, average parcel size at block level, or the interaction of the two. Results, as reported in appendix table 3, suggest that, if anything, smaller and more distant plots are more likely to be transferred and, to a lesser extent, mortgaged.

[17] While more detailed analysis (e.g., by linking errors to survey respondents' identity) is beyond the scope of this paper, the role of measurement error is consistent with what was found in studies such as Deininger *et al.* (2020b).

A puzzling observation in light of the significant impact of reducing transaction cost documented above is that reform-induced reductions in time required for registration were not matched by commensurate drops in registration cost. In fact, with registration fees of 8.2% of plot value,[18] rents seem to persist and reform remains incomplete. Failure to eliminate such rents before starting systematic registration is likely to have reduced LARP impact by (i) pushing individuals who -had it been properly priced- would have opted for sporadic registration to demand systematic services; and (ii) reducing the likelihood that, without further reform, gender benefits of systematic registration documented above will be sustained as newly registered owners may be unable to afford the fees required for registering subsequent transactions and thus revert to informality. A tighter focus on regulatory and institutional reform could thus possibly have enhanced the program's cost-effectiveness and sustainability, including by complementing systematic registration with targeted subsidies or nudges to improve gender equity along the lines suggested by Ali *et al.* (2016).

One explanation for policy-makers' limited support for land titling is that many may see political benefits from embedding security of land rights in clientelist relationships that depend on a credible threat of withdrawal. Land reforms that, instead of giving secure rights and encourage investment, established rights that could be easily withdrawn and put political intermediaries in control of access to markets and services,[19] illustrate a willingness to tolerate significant economic losses for political gain. The extent to which land rights were formalized (Christensen and Garfias 2020), land was valued (Sánchez-Talanquer 2020), and the ability to provide social services (Fergusson *et al.* 2020) have been affected by political considerations. The attenuation of clientelist ties caused by urban land titling reduced local incumbents' vote share in Mexico (Larreguy *et al.* 2018).[20] While further study is warranted, economic and political considerations thus seem to favor a focus on regulatory and institutional reform which would benefit all land owners rather than just a narrow set of the population; demonstrably enhance activity in land and financial markets; and be sustainable rather than short-lived.

## 5. Conclusion

Although land titling programs are often seen as a way to improve land and financial market functioning and thus to contribute to urban structural transformation, we know of no study that would document credit market impacts of urban land titling. Employing administrative rather than household survey data to identify impacts of titling and regulatory as well as institutional reforms separately suggests that policy reform, but

[18] See the 2020 'Doing Business' figures at https://www.doingbusiness.org/en/data/exploretopics/registering-property.
[19] Despite the associated productivity losses, rural reforms awarded insecure and often non-transferable rights in cases such as Zimbabwe (Deininger *et al.* 2004), Kenya (Hassan and Klaus 2020), Colombia (Albertus 2019), Peru (Albertus and Popescu 2020), Mexico (Albertus *et al.* 2016), and the Philippines (Adamopoulos and Restuccia 2020). Clientelist relationships were often reinforced by establishing vast bureaucracies dedicated to provide services to beneficiaries.
[20] Fear of similarly negative consequences may explain why, rather than focus on urban areas with high land values and potential impacts, titling efforts supported by donors and IFIs were mostly directed to rural aeras with more limited potential and scope for sustained impact.

not systematic titling, improves land and mortgage market activity. While policy reform and systematic titling increase women's access to documented land rights, the gender benefits from (subsidized) systematic titling are unlikely to be sustained unless titling is accompanied by regulatory and institutional reform to reduce the cost of subsequent registration.

**Table 1: Descriptive statistics based on administrative data**

| | Total | 1981-2009 | 2010-2013 | 2014-2019 |
|---|---|---|---|---|
| **First time registration** | | | | |
| No. of leases issued | 90,251 | 16,308 | 49,803 | 24,140 |
| per year | 2,314 | 562 | 12,451 | 4,023 |
| Plot area ($m^2$) | 1,211 | 1,452 | 1,062 | 1,393 |
| Sporadic | 37,548 | 16,308 | 6,437 | 14,803 |
| Systematic | 52,703 | - | 43,366 | 9,337 |
| Sporadic/year | 963 | 562 | 1,609 | 2,467 |
| Systematic/year | 1,351 | - | 10,842 | 1,556 |
| *Approach and land use type* | | | | |
| Sporadic registration | 0.416 | 1.000 | 0.129 | 0.613 |
| Systematic registration | 0.584 | 0.000 | 0.871 | 0.387 |
| Residential land | 0.834 | 0.818 | 0.863 | 0.786 |
| Commercial land | 0.062 | 0.141 | 0.038 | 0.057 |
| Agricultural land | 0.004 | 0.004 | 0.001 | 0.012 |
| Other Use | 0.099 | 0.038 | 0.098 | 0.145 |
| *Subject of registration* | | | | |
| Ownership male/female joint | 0.402 | 0.023 | 0.512 | 0.431 |
| of which systematic reg. | 0.756 | 0.000 | 0.886 | 0.463 |
| Ownership by females only | 0.295 | 0.230 | 0.326 | 0.276 |
| of which systematic reg. | 0.651 | 0.000 | 0.888 | 0.436 |
| Ownership by males only | 0.247 | 0.676 | 0.150 | 0.158 |
| of which systematic reg. | 0.339 | 0.000 | 0.817 | 0.377 |
| Ownership by companies or others | 0.056 | 0.072 | 0.012 | 0.136 |
| of which systematic reg. | 0.081 | 0.000 | 0.390 | 0.054 |
| **Transfers** | | | | |
| No. of transfers | 10,848 | 2,578 | 1,576 | 6,694 |
| per year | 278 | 89 | 394 | 1,116 |
| Transferred by male owners | 0.387 | 0.619 | 0.413 | 0.291 |
| Transferred by female owners | 0.304 | 0.230 | 0.298 | 0.333 |
| Transferred by males/female jointly | 0.263 | 0.028 | 0.260 | 0.354 |
| Transferred by companies | 0.047 | 0.123 | 0.030 | 0.022 |
| Days to get consent | 100 | 340 | 111 | 32 |
| Days to complete registration | 124 | 162 | 127 | 108 |
| Days since approval of consent | 80 | 80 | 78 | 80 |
| Purchase price (US$) [a] | 11,088 | 13,785 | 14,688 | 9,273 |
| Plot area ($m^2$) | 1,166 | 1,502 | 1,146 | 1,046 |
| Price (US$/ $m^2$) | 13 | 15 | 17 | 12 |
| **Mortgages** | | | | |
| No. of registered mortgages | 8,101 | 3,981 | 1,240 | 2,880 |
| per year | 208 | 137 | 310 | 480 |
| Mortgage value (US $) | 51,628 | 43,514 | 76,954 | 51,938 |
| Plots reg. to male owners | 0.538 | 0.726 | 0.521 | 0.292 |
| mortgage value (US $) | 43,834 | 35,137 | 69,274 | 53,753 |
| Plots reg. to female owners | 0.198 | 0.176 | 0.203 | 0.225 |
| mortgage value (US $) | 36,311 | 26,874 | 48,508 | 41,617 |
| Plots reg. by males & female jointly | 0.185 | 0.016 | 0.192 | 0.411 |
| mortgage value (US $) | 41,104 | 32,748 | 44,619 | 40,838 |
| Plots reg. by companies | 0.079 | 0.083 | 0.084 | 0.073 |
| mortgage value (US $) | 169,153 | 158,281 | 263,473 | 139,489 |
| Days for registration process | 78 | 137 | 29 | 19 |
| Days to get ministerial consent | 98 | 98 | | |

*Source:* LAA, land administration database and digitized records, 2020.

[a]Note that the number of observations for purchase price and perice per unit of land slightly different due to missing plot area, and hence unit price cannot be computed from the mean values.

**Table 2: Descriptive statistics at block level**

| | Total | Outside TC | Treatment (T) | Control (C) |
|---|---|---|---|---|
| **Sporadic registration** | | | | |
| Number of plots | | | | |
| 2000 | 3.20 | 2.98 | 6.23 | 0.33 |
| 2010 | 5.76 | 5.73 | 9.23 | 1.22 |
| 2015 | 8.53 | 8.55 | 12.45 | 3.13 |
| 2019 | 10.87 | 10.87 | 14.26 | 6.30 |
| Area of registered plots $m^2$ | | | | |
| 2000 | 4,516 | 4,415 | 7,743 | 746 |
| 2010 | 6,915 | 6,939 | 10,424 | 2,045 |
| 2015 | 9,730 | 9,782 | 13,513 | 4,331 |
| 2019 | 11,883 | 11,833 | 15,273 | 7,604 |
| **Systematically registration** | | | | |
| No. of plots | | | | |
| 2015 | 16.43 | 18.65 | 18.75 | 0.49 |
| 2019 | 16.64 | 18.89 | 18.98 | 0.51 |
| Area of registered plots $m^2$ | | | | |
| 2015 | 15,349 | 17,361 | 17,712 | 554 |
| 2019 | 15,554 | 17,597 | 17,928 | 565 |
| **No. of registered transfers** | | | | |
| 2000 | 0.03 | 0.03 | 0.05 | 0.00 |
| 2010 | 0.06 | 0.06 | 0.09 | 0.00 |
| 2015 | 0.34 | 0.36 | 0.40 | 0.20 |
| 2019 | 0.34 | 0.34 | 0.36 | 0.32 |
| **No. of registered mortgages** | | | | |
| 2000 | 0.01 | 0.01 | 0.01 | 0.00 |
| 2010 | 0.08 | 0.08 | 0.09 | 0.03 |
| 2015 | 0.19 | 0.19 | 0.32 | 0.07 |
| 2019 | 0.16 | 0.16 | 0.17 | 0.15 |
| **Structures (based on 2016 imagery)** | | | | |
| No. of buildings | 56 | 56 | 75 | 31 |
| Built-up area ($m^2$) | 4,989 | 5,003 | 7,045 | 2,132 |
| Number of blocks | 1,932 | 1,373 | 321 | 238 |
| Block area $m^2$ | 62,500 | 62,500 | 62,500 | 62,500 |

*Source:* LAA, land administration database and digitized records, 2020

**Table 3: Estimated effects on legal awareness and land market participation (using kernel-based matched sample)**

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Women's land management, legal knowledge and perceived tenure security** | | | | | | | | |
| | **Female manages plot** | | **Women can inherit** | | **Knows LAA** | | **Worries over conflict** | |
| T *post-LARP ($\delta_1$) | 0.039 | -0.011 | 0.024 | 0.038** | 0.081* | 0.057 | -0.052** | -0.037 |
| | (0.045) | (0.049) | (0.018) | (0.023) | (0.063) | (0.066) | (0.031) | (0.036) |
| T *post*fem. head ($\delta_2$) | | 0.114** | | -0.031** | | 0.051 | | -0.033 |
| | | (0.059) | | (0.018) | | (0.059) | | (0.043) |
| Observations | 2,311 | 2,311 | 2,301 | 2,301 | 2,308 | 2,308 | 2,311 | 2,311 |
| R-squared | 0.809 | 0.852 | 0.497 | 0.499 | 0.640 | 0.641 | 0.499 | 0.502 |
| Baseline control mean | 0.351 | | 0.979 | | 0.385 | | 0.045 | |
| Tests: pvalue | | | | | | | | |
| $\delta_1 + \delta_2 = 0$ | | 0.009 | | 0.562 | | 0.0446 | | 0.022 |
| **Panel B: Investment and land market** | | | | | | | | |
| | **Purchased land (3yrs)** | | **Purchased land (7yrs)** | | **Rented out** | | **Made investment[a]** | |
| T *post-LARP ($\delta_1$) | 0.011 | 0.008 | 0.007 | 0.009 | -0.002 | 0.000 | -0.203*** | -0.205*** |
| | (0.024) | (0.024) | (0.040) | (0.045) | (0.004) | (0.004) | (0.083) | (0.090) |
| T *post*fem. head ($\delta_2$) | | 0.005 | | -0.005 | | -0.006* | | 0.004 |
| | | (0.027) | | (0.045) | | (0.004) | | (0.057) |
| Observations | 2,311 | 2,311 | 2,311 | 2,311 | 2,311 | 2,311 | 2,311 | 2,311 |
| R-squared | 0.524 | 0.525 | 0.543 | 0.543 | 0.952 | 0.952 | 0.555 | 0.555 |
| Baseline control mean | 0.0341 | | 0.0932 | | 0.014 | | 0.376 | |
| Tests: pvalue | | | | | | | | |
| $\delta_1 + \delta_2 = 0$ | | 0.634 | | 0.902 | | 0.131 | | 0.005 |
| **Panel C: Credit market access** | | | | | | | | |
| | **Had sec. home loan** | | **Applied for any loans** | | **Has bank account** | | **Head has credit card** | |
| T *post-LARP ($\delta_1$) | 0.011 | 0.021 | -0.063* | -0.065 | -0.023 | -0.002 | 0.045 | 0.055 |
| | (0.015) | (0.019) | (0.049) | (0.059) | (0.086) | (0.087) | (0.064) | (0.073) |
| T *post*fem. head ($\delta_2$) | | -0.022 | | 0.003 | | -0.047 | | -0.022 |
| | | (0.018) | | (0.055) | | (0.049) | | (0.050) |
| Observations | 2,311 | 2,311 | 2,244 | 2,244 | 2,246 | 2,246 | 2,240 | 2,240 |
| R-squared | 0.501 | 0.502 | 0.593 | 0.594 | 0.685 | 0.686 | 0.533 | 0.533 |
| Baseline control mean | 0.009 | | 0.0760 | | 0.575 | | 0.0808 | |
| Tests: pvalue | | | | | | | | |
| $\delta_1 + \delta_2 = 0$ | | 0.958 | | 0.127 | | 0.466 | | 0.476 |

*Notes:* The dependent variable is the share land held by females in (co-)ownership weighted by plot size (col. 1), indicator variables for knowledge (col. 2), participation in rental markets (col. 3) and the household having been involved in land conflict or made any investment (cols. 4 and 5). Regressions are weighted by matched controls' weight and only matched households are included. Year and household fixed effects as well as time varying household controls (age of head of the household, land size and annual per capita expenditure) included throughout. Robust standard errors clustered at the level of survey clusters reported in parentheses. Stars are based on p-values from wild bootstrap clustering (Roodman *et al.* 2018) using Rademacher weights and 1,000 replications.*** significant at 1%; significant at 5%; * significant at 10%.

[a] Investments include constructions of new buildings/houses, repairs rehabilitation, improvement or of buildings, adding facilities for water supply, landline phone service, electricity, sewage, drainage, and toilets.

**Table 4: Estimated effects on built up structures from hand-labelled pre- and post-intervention satellite imagery**

| | All structures in square meters | | | Finished structures in square meters | | |
|---|---|---|---|---|---|---|
| | **All** | **1000m band** | **500m band** | **All** | **1000m band** | **500m band** |
| Treatment*Post | -4.905*** | -3.081** | -2.674 | -5.085*** | -1.293 | -0.0620 |
| | (0.953) | (1.300) | (1.869) | (0.983) | (1.362) | (1.958) |
| Post | 18.93*** | 17.96*** | 20.61*** | 21.38*** | 20.30*** | 21.69*** |
| | (0.830) | (0.977) | (1.421) | (0.856) | (1.023) | (1.489) |
| Constant | 108.2*** | 97.91*** | 94.64*** | 101.5*** | 89.78*** | 86.84*** |
| | (0.289) | (0.456) | (0.653) | (0.298) | (0.477) | (0.684) |
| Observations | 40,684 | 16,236 | 8,578 | 40,684 | 16,236 | 8,578 |
| R-squared | 0.0650 | 0.0730 | 0.0909 | 0.0796 | 0.0939 | 0.105 |
| Mean dep. Var. | 115.8 | 106.0 | 104.2 | 110.2 | 99.57 | 97.67 |
| Mean dep. var. control 2013 | 68.59 | 71.51 | 76.06 | 61.34 | 64.80 | 70.44 |
| Mean dep. var. treated 2013 | 134.8 | 133.2 | 126.1 | 130.6 | 128.1 | 120.4 |

*Source:* Own computation based on LAA's land administration database and digitized records as well as hand-digitized plot size from 2013 and 2016 aerial photography as described in the text.

**Table 5: Estimation results for newly registered land**

| | Female – Male | | Joint - Male | | Joint - Female | |
|---|---|---|---|---|---|---|
| **Panel A: Entire sample** | | | | | | |
| No. of registered plots ($\beta_1$) | -0.256*** | | -0.482*** | | -0.226*** | |
| | (0.00878) | | (0.00950) | | (0.0108) | |
| No. of registered plots | 0.439*** | | 0.848*** | | 0.410*** | |
| * post-2010 ($\beta_2$) | (0.00889) | | (0.00962) | | (0.0109) | |
| No. of sporadically | | -0.270*** | | -0.512*** | | -0.243*** |
| registered plots ($\gamma_1$) | | (0.00871) | | (0.00913) | | (0.0107) |
| No. of sporadically | | 0.338*** | | 0.625*** | | 0.287*** |
| registered plots * post 2010 ($\gamma_2$) | | (0.00957) | | (0.0100) | | (0.0118) |
| No. of systematically | | 0.195*** | | 0.392*** | | 0.198*** |
| registered plots ($\gamma_3$) | | (0.00145) | | (0.00151) | | (0.00178) |
| Constant | 0.00660 | 0.00813 | -0.00511 | -0.00172 | -0.0117 | -0.00985 |
| | (0.0229) | (0.0227) | (0.0248) | (0.0238) | (0.0281) | (0.0279) |
| No. of obs (250*250 m blocks) | 38,640 | 38,640 | 38,640 | 38,640 | 38,640 | 38,640 |
| C | 0.448 | 0.458 | 0.736 | 0.758 | 0.355 | 0.367 |
| Size & sig. of linear combinations | | | | | | |
| $\beta_1+\beta_2$ | 0.183*** | | 0.367*** | | 0.183*** | |
| | (0.0014) | | (0.0015) | | (0.0017) | |
| $\gamma_1+\gamma_2$ | | 0.0684*** | | 0.112*** | | 0.0438*** |
| | | (0.0045) | | (0.0047) | | (0.0055) |
| $\gamma_3$ - ($\gamma_1+\gamma_2$) | | 0.126*** | | 0.280*** | | 0.154*** |
| | | (0.0047) | | (0.0049) | | (0.0058) |
| **Panel B: Restricted sample** | | | | | | |
| No. of registered plots ($\beta_1$) | -0.497*** | | -0.629*** | | -0.132*** | |
| | (0.0239) | | (0.0222) | | (0.0300) | |
| No. of registered plots | 0.731*** | | 0.965*** | | 0.234*** | |
| * post-2010 ($\beta_2$) | (0.0240) | | (0.0224) | | (0.0302) | |
| No. of sporadically | | -0.473*** | | -0.653*** | | -0.180*** |
| registered plots ($\gamma_1$) | | (0.0233) | | (0.0217) | | (0.0282) |
| No. of sporadically | | 0.938*** | | 0.772*** | | -0.166*** |
| registered plots * post 2010 ($\gamma_2$) | | (0.0254) | | (0.0237) | | (0.0308) |
| No. of systematically | | 0.217*** | | 0.352*** | | 0.135*** |
| registered plots ($\gamma_3$) | | (0.00293) | | (0.00273) | | (0.00355) |
| Constant | -0.00252 | -0.00546 | -0.0132 | -0.0105 | -0.0107 | -0.00500 |
| | (0.0435) | (0.0425) | (0.0406) | (0.0396) | (0.0547) | (0.0515) |
| No. of obs (250*250 m blocks) | 9,100 | 9,100 | 9,100 | 9,100 | 9,100 | 9,100 |
| R-squared | 0.516 | 0.539 | 0.727 | 0.740 | 0.138 | 0.238 |
| Size & sig. of linear combinations | | | | | | |
| $\beta_1+\beta_2$ | 0.234*** | | 0.335*** | | 0.101*** | |
| | (0.0029) | | (0.0027) | | (0.0036) | |
| $\gamma_1+\gamma_2$ | | 0.465*** | | 0.119*** | | -0.346*** |
| | | (0.0114) | | (0.0106) | | (0.0138) |
| $\gamma_3$ - ($\gamma_1+\gamma_2$) | | -0.249*** | | 0.232*** | | 0.481*** |
| | | (0.0118) | | (0.0110) | | (0.0144) |

Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.010

**Table 6: Estimation results for number of mortgages by gender of owner, 2000-2019**

| | Total | Male | Female | Joint |
|---|---|---|---|---|
| **Panel A: Entire sample** | | | | |
| Spor. reg. plots pre-2010 ($\theta_1$) | 0.0160*** | 0.0088*** | 0.0206*** | 0.0297*** |
| | (0.0010) | (0.0010) | (0.0012) | (0.0038) |
| Spor. reg. plots post-2010 ($\theta_2$) | 0.0273*** | 0.0301*** | 0.0141*** | 0.0283*** |
| | (0.0008) | (0.0015) | (0.0009) | (0.0007) |
| Syst. reg. plots post 2010 ($\theta_3$) | 0.0016*** | 0.0009 | 0.0007** | 0.0019*** |
| | (0.0003) | (0.0009) | (0.0003) | (0.0002) |
| Constant | -0.0380*** | -0.0120** | -0.0118*** | -0.0002 |
| | (0.0091) | (0.0056) | (0.0037) | (0.0041) |
| No. of obs (blocks) | 38,600 | 38,600 | 38,600 | 38,600 |
| Linear combinations | | | | |
| $\theta_2$ - $\theta_1$ | 0.0113*** | 0.0213*** | -0.0066*** | -0.0015 |
| | (0.0014) | (0.0020) | (0.0017) | (0.0041) |
| $\theta_3$ - $\theta_2$ | -0.0257*** | -0.0292*** | -0.0133*** | -0.0264*** |
| | (0.0008) | (0.0017) | (0.0009) | (0.0007) |
| $\theta_3$ - $\theta_1$ | -0.0144*** | -0.0079*** | -0.0199*** | -0.0279*** |
| | (0.0011) | (0.0013) | (0.0012) | (0.0038) |
| **Panel B: Restricted sample** | | | | |
| Spor. reg. plots pre-2010 ($\theta_1$) | 0.0267*** | 0.0161*** | 0.0067* | 0.0325*** |
| | (0.0033) | (0.0032) | (0.0039) | (0.0102) |
| Spor. reg. plots post-2010 ($\theta_2$) | 0.0092*** | 0.0119** | 0.0106*** | 0.0150*** |
| | (0.0022) | (0.0050) | (0.0015) | (0.0025) |
| Syst. reg. plots post 2010 ($\theta_3$) | 0.0014*** | -0.0010 | 0.0010 | 0.0029*** |
| | (0.0006) | (0.0022) | (0.0006) | (0.0006) |
| Constant | -0.1100*** | -0.0452*** | -0.0051 | -0.0001 |
| | (0.0219) | (0.0153) | (0.0084) | (0.0083) |
| No. of obs (blocks) | 9,100 | 9,100 | 9,100 | 9,100 |
| Linear combinations | | | | |
| $\theta_2$ - $\theta_1$ | -0.0175*** | -0.0041 | 0.0038 | -0.0174 |
| | (0.0045) | (0.0063) | (0.0045) | (0.0108) |
| $\theta_3$ - $\theta_2$ | -0.0077*** | -0.0129** | -0.0096*** | -0.0122*** |
| | (0.0023) | (0.0056) | (0.0017) | (0.0025) |
| $\theta_3$ - $\theta_1$ | -0.0253*** | -0.0171*** | -0.0057 | -0.0296*** |
| | (0.0033) | (0.0038) | (0.0039) | (0.0102) |

*Note*: Standard errors in parentheses: * p<0.10, ** p<0.05, *** p<0.010

**Table 7: Estimation results for number of transfers by gender of transferor, 2000-2019**

| | Total | Male | Female | Joint |
|---|---|---|---|---|
| **Panel A: Entire sample** | | | | |
| Spor. reg. plots pre-2010 ($\theta_1$) | 0.0223*** | 0.0103*** | 0.0250*** | 0.0712*** |
| | (0.0014) | (0.0014) | (0.0020) | (0.0056) |
| Spor. reg. plots post-2010 ($\theta_2$) | 0.0410*** | 0.0400*** | 0.0270*** | 0.0318*** |
| | (0.0010) | (0.0020) | (0.0015) | (0.0011) |
| Syst. reg. plots post 2010 ($\theta_3$) | 0.0039*** | 0.0086*** | 0.0026*** | 0.0024*** |
| | (0.0004) | (0.0013) | (0.0005) | (0.0003) |
| Constant | -0.0388*** | -0.0040 | -0.0086 | -0.0004 |
| | (0.0123) | (0.0076) | (0.0063) | (0.0061) |
| No. of obs (blocks) | 38,600 | 38,600 | 38,600 | 38,600 |
| Linear combinations | | | | |
| $\theta_2$ - $\theta_1$ | 0.0187*** | 0.0297*** | 0.002 | -0.0393*** |
| | (0.0020) | (0.0027) | (0.0028) | (0.0060) |
| $\theta_3$ - $\theta_2$ | -0.0371*** | -0.0314*** | -0.0245*** | -0.0294*** |
| | (0.0010) | (0.0024) | (0.0015) | (0.0011) |
| $\theta_3$ - $\theta_1$ | -0.0185*** | -0.0017 | -0.0225*** | -0.0687*** |
| | (0.0014) | (0.0018) | (0.0021) | (0.0056) |
| **Panel B: Restricted sample** | | | | |
| Spor. reg. plots pre-2010 ($\theta_1$) | 0.0306*** | 0.0102*** | 0.0180** | 0.0367*** |
| | (0.0045) | (0.0037) | (0.0074) | (0.0124) |
| Spor. reg. plots post-2010 ($\theta_2$) | 0.0263*** | 0.0122** | 0.0228*** | 0.0129*** |
| | (0.0030) | (0.0060) | (0.0029) | (0.0030) |
| Syst. reg. plots post 2010 ($\theta_3$) | 0.0039*** | 0.0071*** | 0.0051*** | 0.0018*** |
| | (0.0008) | (0.0026) | (0.0012) | (0.0007) |
| Constant | -0.1033*** | -0.0132 | -0.0105 | -0.0001 |
| | (0.0298) | (0.0181) | (0.0161) | (0.0100) |
| No. of obs (blocks) | 9,100 | 9,100 | 9,100 | 9,100 |
| Linear combinations | | | | |
| $\theta_2$ - $\theta_1$ | -0.0043 | 0.0019 | 0.0048 | -0.0239* |
| | (0.0061) | (0.0075) | (0.0085) | (0.0131) |
| $\theta_3$ - $\theta_2$ | -0.0225*** | -0.005 | -0.0177*** | -0.0111*** |
| | (0.0031) | (0.0067) | (0.0031) | (0.0031) |
| $\theta_3$ - $\theta_1$ | -0.0268*** | -0.0031 | -0.0129* | -0.0349*** |
| | (0.0045) | (0.0044) | (0.0075) | (0.0123) |

*Note*: Standard errors in parentheses: * p<0.10, ** p<0.05, *** p<0.010

**Table 8: Comparing registry-based with survey information**

| Registry\Survey | Male indiv. | Female indiv. | Joint | No title | Total | % Type 2 errors |
|---|---|---|---|---|---|---|
| Male indiv. | 79 | 27 | 30 | 49 | 185 | 57.30 |
| Female indiv. | 15 | 190 | 16 | 56 | 277 | 31.41 |
| Joint | 23 | 28 | 227 | 47 | 325 | 30.15 |
| No title | 45 | 63 | 46 | 762 | 916 | 16.81 |
| Total | 162 | 308 | 319 | 914 | 1,703 | |
| % Type 1 errors | 51.23 | 38.31 | 28.84 | 16.63 | | |

*Note:* For explanations, see text.

**Figure 1: Location of treatment (MMC 1, 2, and 3) and Control Wards in Maseru City**

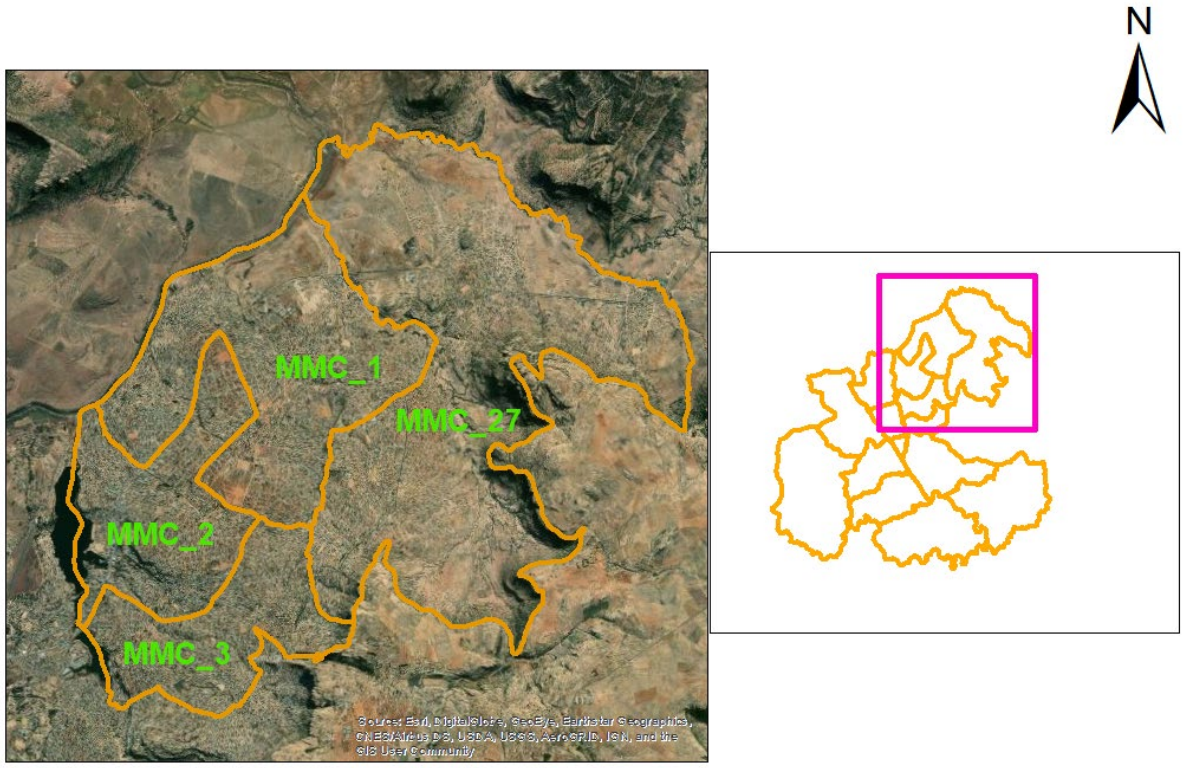


**Figure 2: Cumulative number of registered parcels by year**

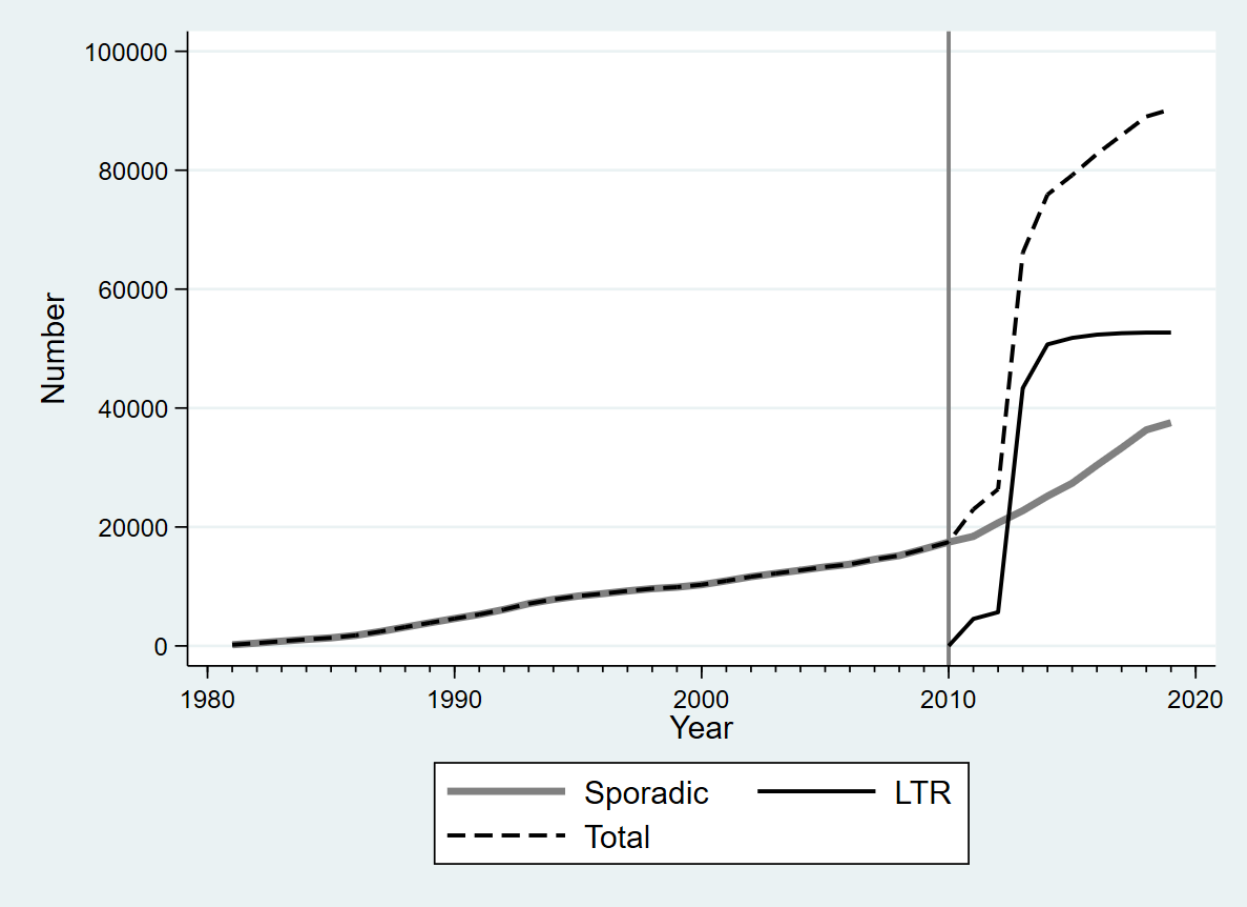
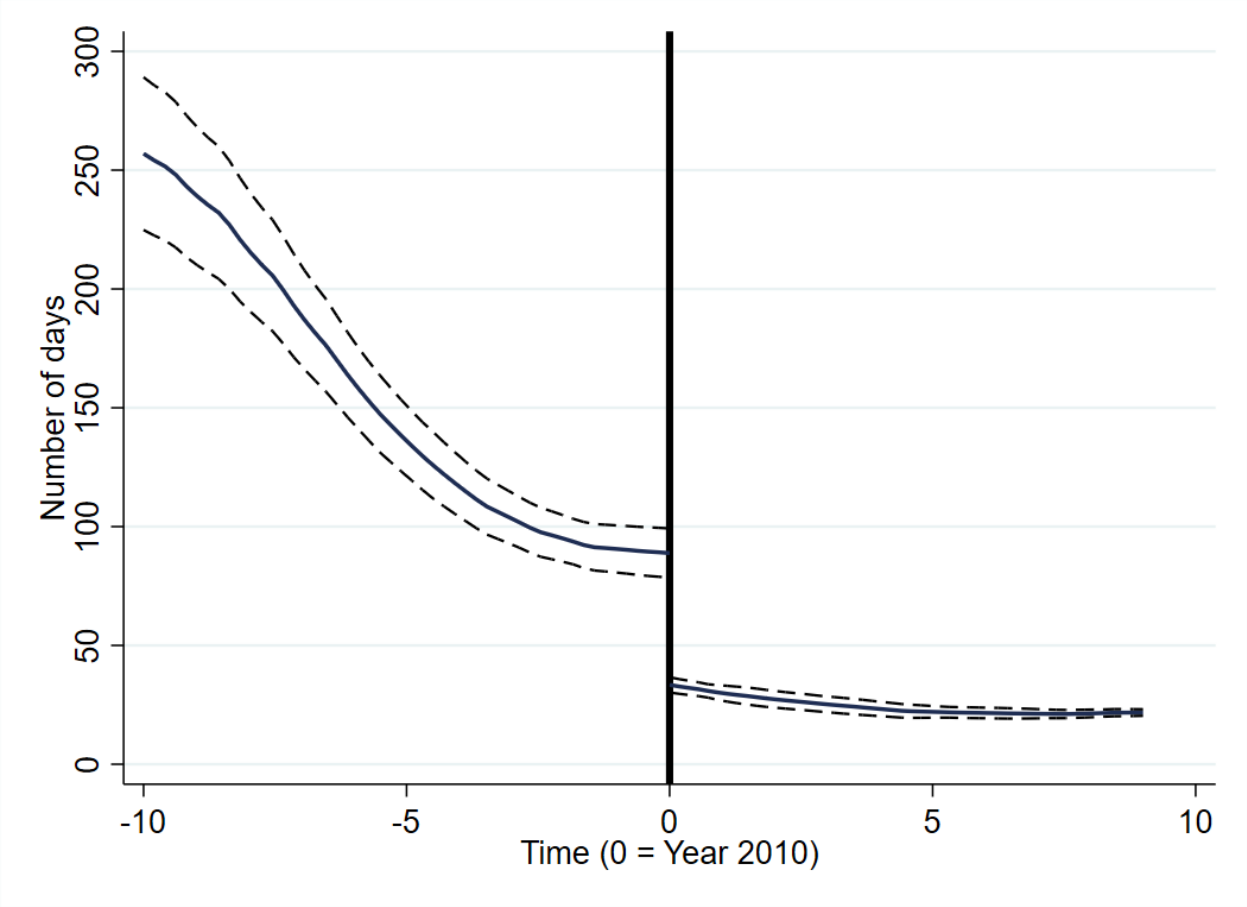
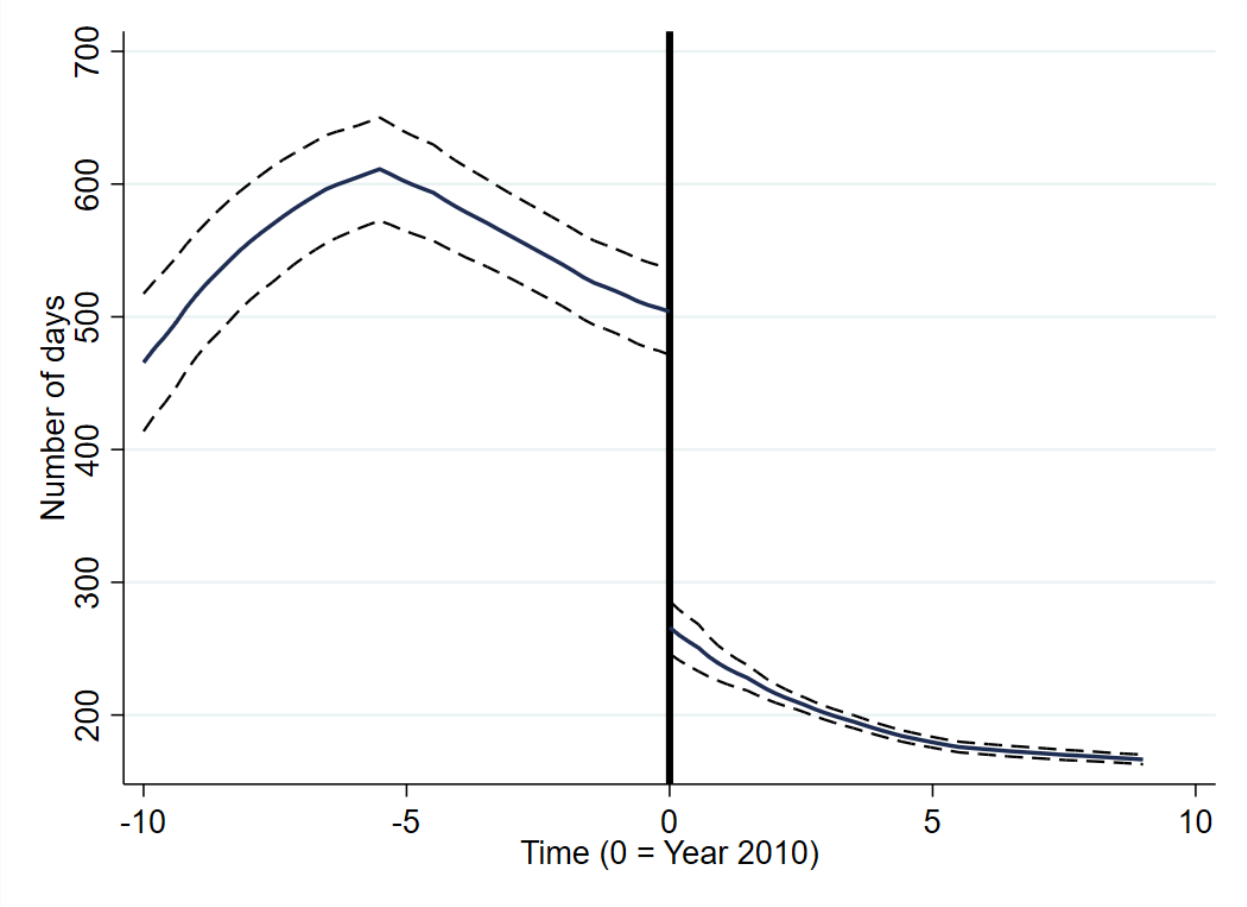


**Figure 3: Duration of mortgage registration by year**




*Note:* Duration is from start of registration to end. Dashed lines indicate confidence intervals.

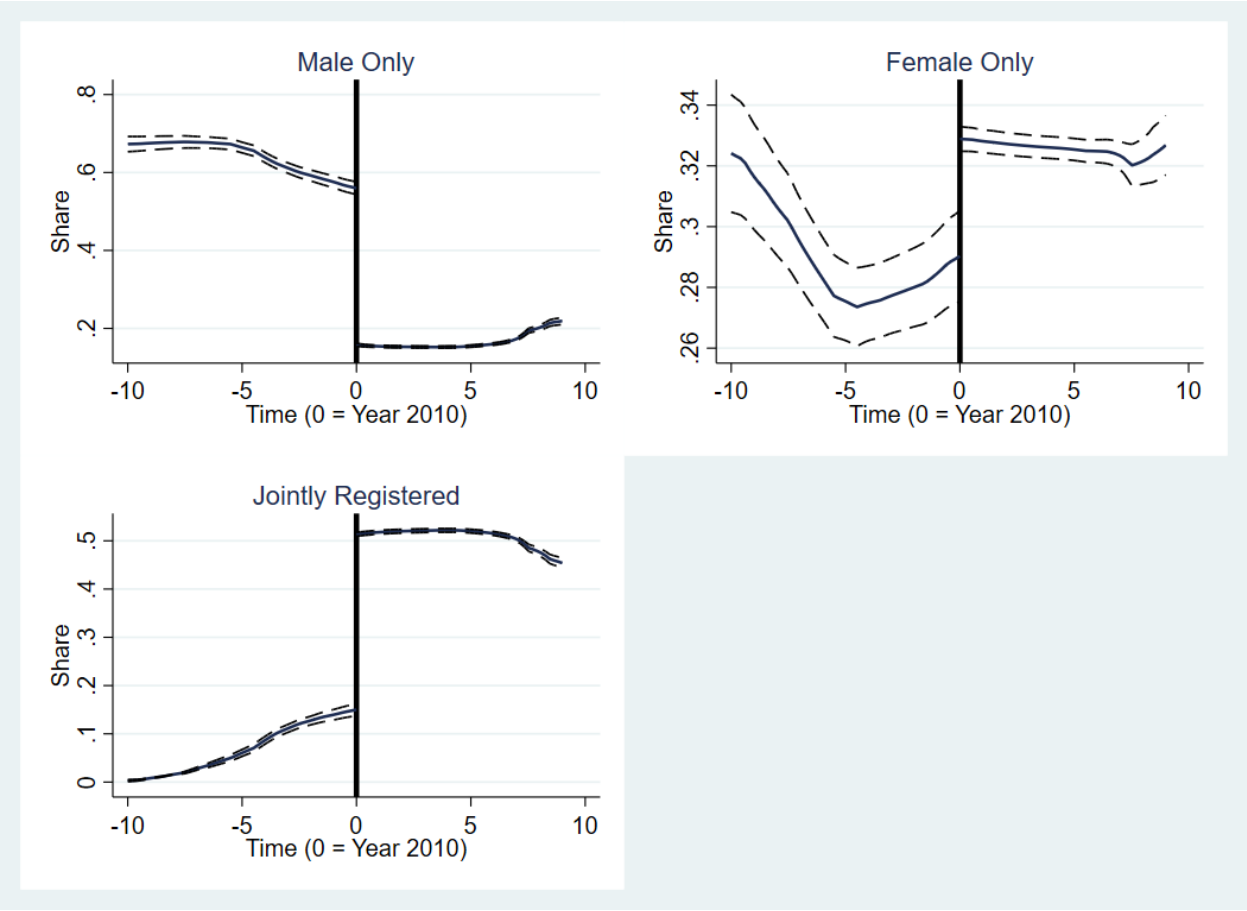
**Figure 4: Duration of registering a transfer before and after reform**




*Note:* Dashed lines represent confidence intervals.

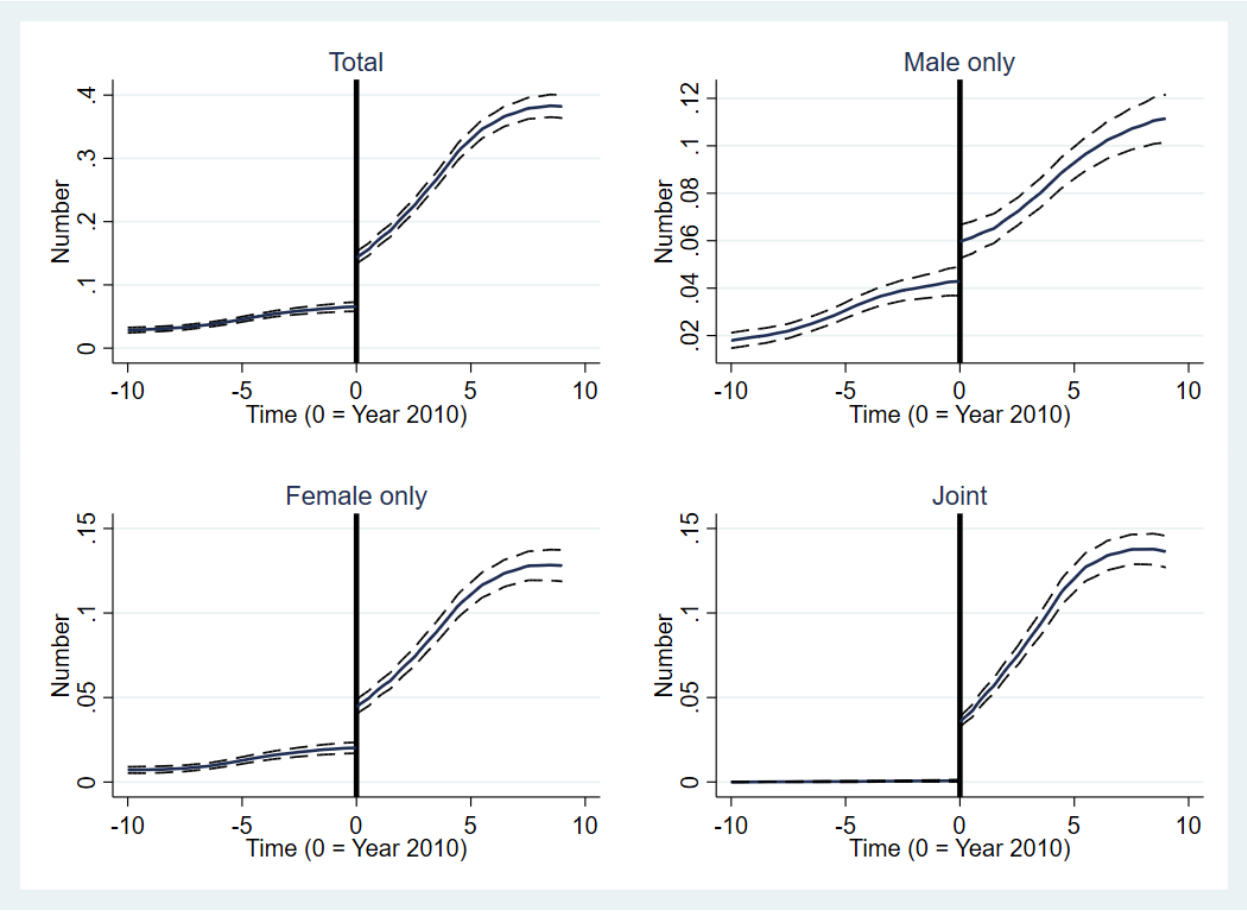
**Figure 5: Illustration of blocks used for impact evaluation based on administrative data**

**Figure 6: Share of first-time registered parcels by males and females individually and jointly before and after reform**




*Note:* Dashed lines denote confidence intervals.

**Figure 7: Number of registered transfers by type of registered owner (block level)**




*Note:* Dashed lines denote confidence intervals.

**Figure 8: Number of registered mortgages by type of registered owner (block level)**

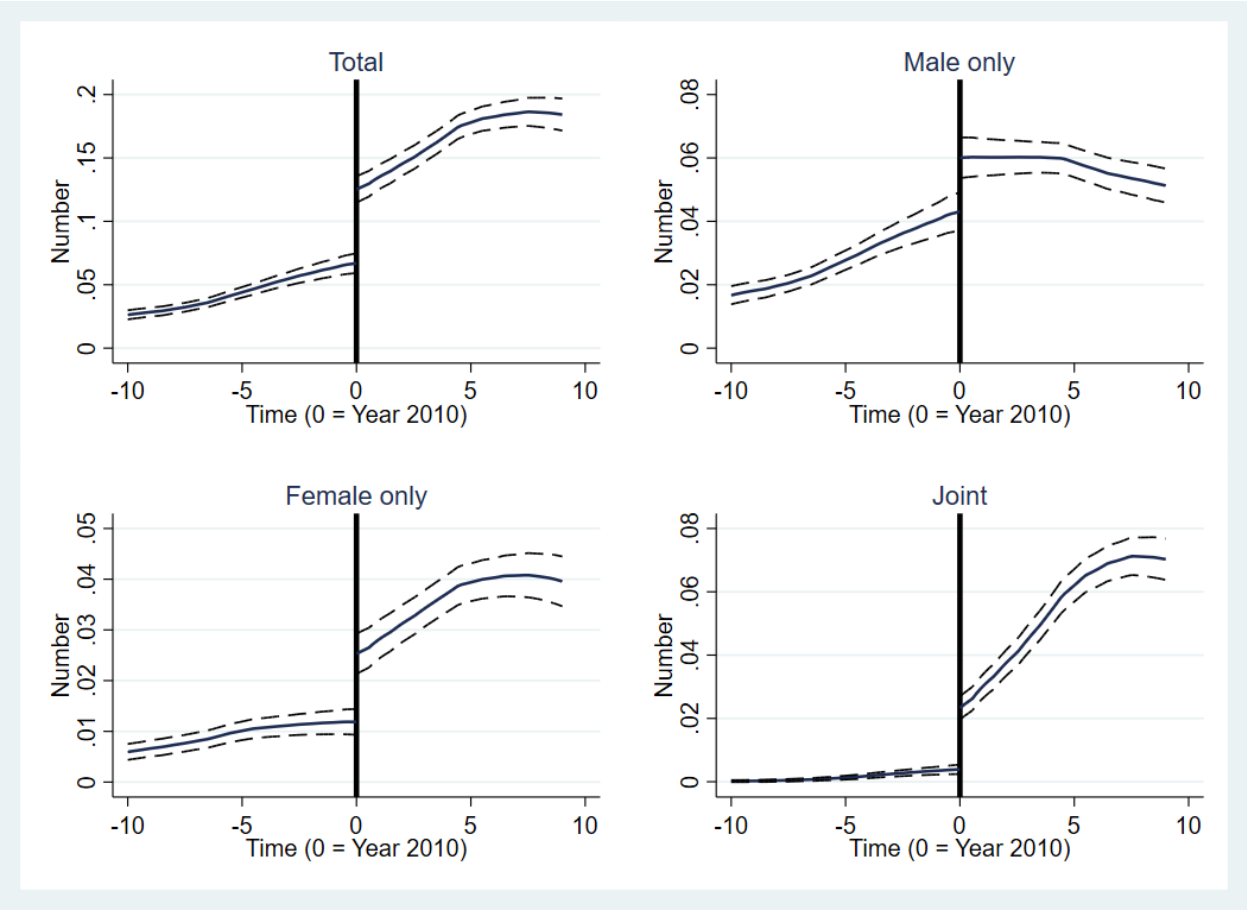
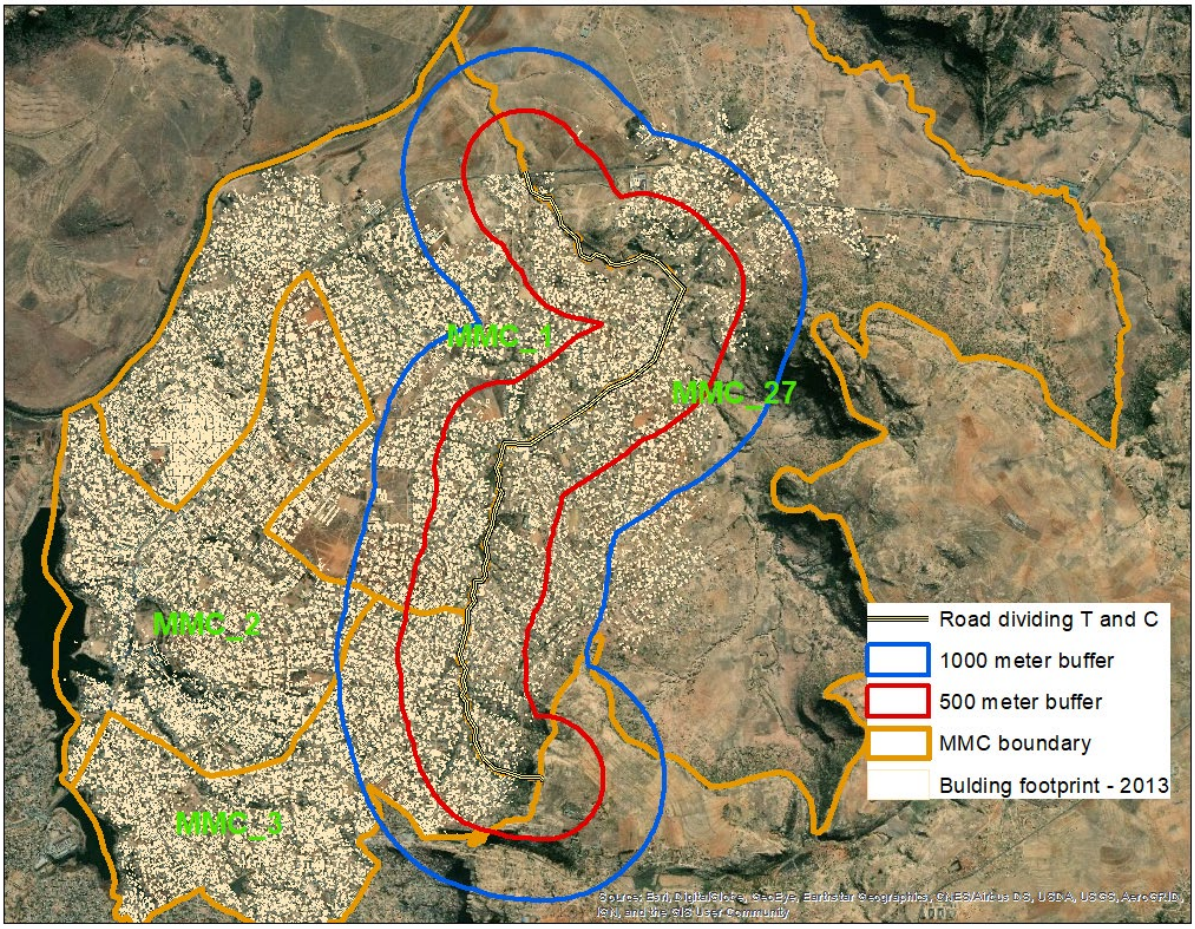


**Figure 9: Geographic discontinuity design used for satellite imagery interpretation**



**Appendix table 1: Descriptive statistics for matched and unmatched baseline household survey sample (kernel matching)**

| | | Unmatched | | | | Matched | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Label | Treatment | Control | p-value | Sig. | Treatment | Control | p-value | Sig. |
| Female head | dfemhead | 0.449 | 0.372 | 0.008 | *** | 0.439 | 0.421 | 0.486 | |
| Household size | hhsize | 4.192 | 4.300 | 0.365 | | 4.252 | 4.123 | 0.212 | |
| Share of boys (age <= 14) | share_boy | 0.095 | 0.120 | 0.004 | *** | 0.099 | 0.108 | 0.283 | |
| Share of girls (age <= 14) | share_girl | 0.094 | 0.115 | 0.013 | ** | 0.097 | 0.094 | 0.661 | |
| Share of male adults (age 15-65) | share_madu | 0.332 | 0.323 | 0.564 | | 0.330 | 0.335 | 0.688 | |
| Share female adults (age 15-65 | share_fadu | 0.393 | 0.347 | 0.001 | *** | 0.396 | 0.390 | 0.660 | |
| Share of male olds (age > 65) | share_mold | 0.034 | 0.033 | 0.911 | | 0.031 | 0.034 | 0.628 | |
| Head's age | hage | 52.406 | 49.679 | 0.001 | *** | 52.227 | 50.988 | 0.086 | * |
| Any plot-level investment | d_invest | 0.343 | 0.373 | 0.260 | | 0.345 | 0.324 | 0.395 | |
| Female managed plots | owner_ft | 0.416 | 0.351 | 0.024 | ** | 0.409 | 0.411 | 0.946 | |
| Main plot w. paved road access | dpvdroad | 0.106 | 0.009 | 0.000 | *** | 0.035 | 0.026 | 0.325 | |
| Main plot with tap water access | dprvtap | 0.829 | 0.538 | 0.000 | *** | 0.814 | 0.817 | 0.911 | |
| Main plot has electricity | dnoelect | 0.197 | 0.239 | 0.057 | * | 0.199 | 0.206 | 0.737 | |
| Main plot has flush toilet | dflusht | 0.136 | 0.059 | 0.000 | *** | 0.130 | 0.108 | 0.190 | |
| Main plot has pit toilet | dpit_vip | 0.792 | 0.838 | 0.035 | ** | 0.796 | 0.829 | 0.088 | * |
| Main plot w. indoor bath/shower | dindorbath | 0.860 | 0.891 | 0.088 | * | 0.864 | 0.838 | 0.144 | |
| Knows about land leases | dknowllse | 0.557 | 0.453 | 0.000 | *** | 0.550 | 0.590 | 0.118 | |
| Lease and more valued than w/o | dpaymorelse | 0.762 | 0.831 | 0.004 | *** | 0.756 | 0.756 | 0.993 | |
| More willing to sell land w. lease | dslemorelse | 0.709 | 0.731 | 0.411 | | 0.710 | 0.730 | 0.390 | |
| Heard about 2010 Land Act | dknowlact | 0.179 | 0.105 | 0.000 | *** | 0.179 | 0.162 | 0.405 | |
| Likelier to improve land w. lease | dlikeinvlsenewl | 0.721 | 0.830 | 0.000 | *** | 0.741 | 0.724 | 0.490 | |
| Annual expenditure (USD) | exp | 4,834 | 3,557 | 0.001 | *** | 4,512 | 4,350 | 0.547 | |
| Would use mortgage to improve | dlctln3 | 0.284 | 0.218 | 0.011 | ** | 0.282 | 0.284 | 0.925 | |
| Would use mortgage to buy land | dlctln2 | 0.107 | 0.088 | 0.276 | | 0.100 | 0.104 | 0.819 | |
| Land area in square meter | gpsarea | 1,297 | 1,566 | 0.010 | *** | 1,306 | 1,278 | 0.690 | |

*Source:* Own computation from MSU 2013 baseline survey.

Note: The standardized mean and median covariate bias before matching were 17.6 and 15.5 percent, respectively. The kernel matching technique reduces the mean and median percentage bias to 3.8 and 3.2, respectively. One-to-one and 2-nearest neighbor matching strategies reduce the mean and median percentage bias (4.9 and 4.1; and 5.6 and 4.3, respectively), suggesting that the kernel-based matching performs slightly better than the latter two approaches.

**Appendix table 2: Registration and distance to CBD - linear regression with block fixed effects**

| | Sporadic adjudication | | | Systematic |
|---|---|---|---|---|
| | **Total** | **Pre-reform** | **Post-reform** | **Adjudication** |
| Distance from CBD in m (log) | -0.236*** | -0.186*** | 0.0194 | 0.167*** |
| | (0.0612) | (0.0495) | (0.0503) | (0.0583) |
| Plot area in $m^2$ (log) | 0.0182*** | 0.0366*** | -0.0268*** | -0.00977*** |
| | (0.00229) | (0.00185) | (0.00189) | (0.00218) |
| N | 79,051 | 79,051 | 79,051 | 79,051 |
| $R^2$ | 0.00108 | 0.00541 | 0.00265 | 0.000395 |

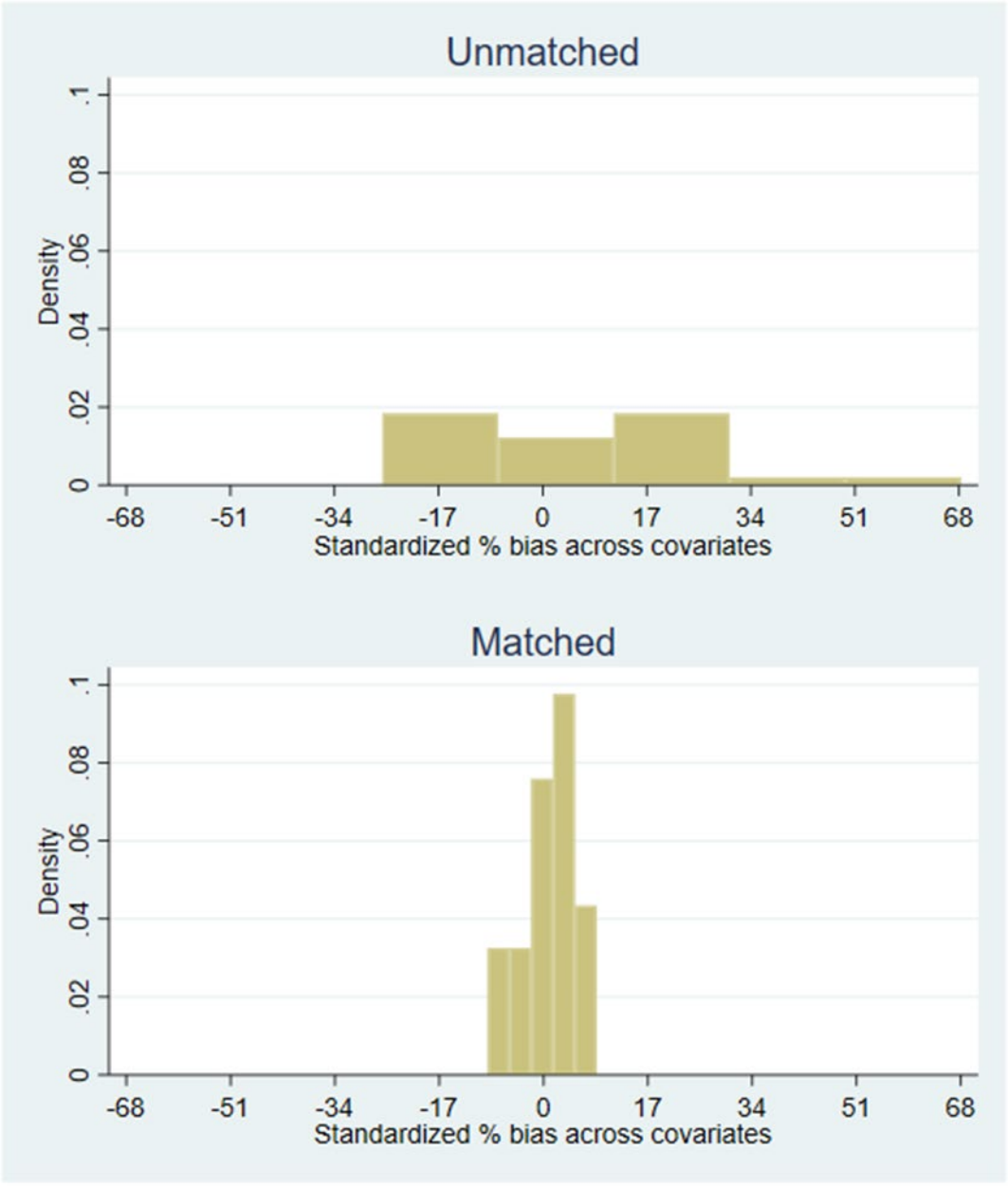
*Note:* Block fixed effects and constant included throughout. Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.010

**Appendix table 3: Results for number of transfers & mortgages with CBD distance (CBDD) interaction, 2000-2019**

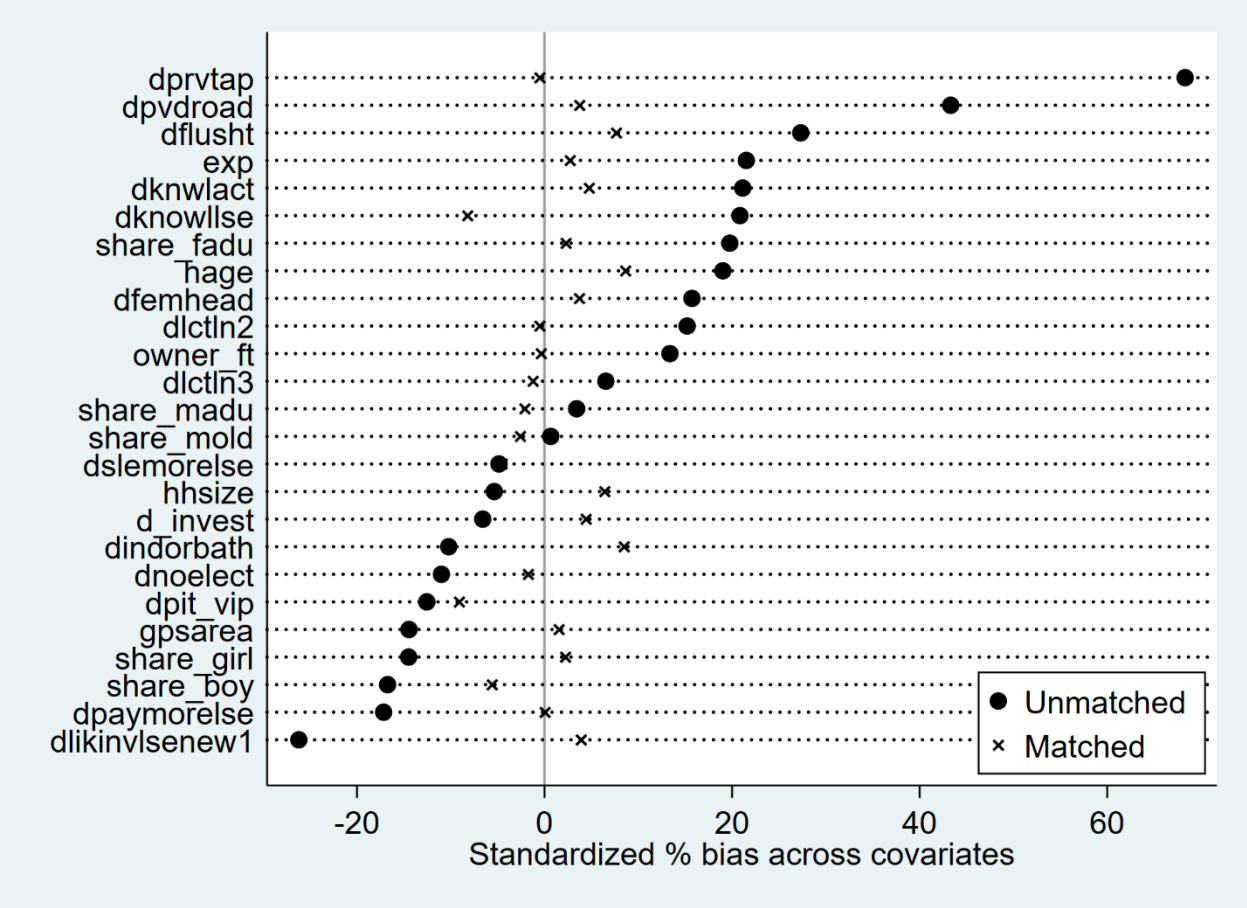
| | Mortgages | | | Transfers | | |
|---|---|---|---|---|---|---|
| | **Dist. to CBD** | **Plot size** | **Dist*plot size** | **Dist. to CBD** | **Plot size** | **Dist*plot size** |
| **Panel A: Registered mortgages** | | | | | | |
| Spor. reg. plots pre-2010 ($\theta_1$) | 0.0167*** | 0.0178*** | 0.0167*** | 0.0096* | 0.0272*** | 0.0233*** |
| | (0.0033) | (0.0017) | (0.0018) | (0.0055) | (0.0024) | (0.0025) |
| Spor. reg. plots post-2010 ($\theta_2$) | 0.0145*** | 0.0299*** | 0.0283*** | -0.0007 | 0.0419*** | 0.0376*** |
| | (0.0025) | (0.0011) | (0.0011) | (0.0019) | (0.0015) | (0.0015) |
| CBDD*Spor. reg. plots pre-2010 | 0.0001 | -0.0328 | -0.0019 | -0.0010 | -0.0798*** | -0.0007 |
| | (0.0005) | (0.0209) | (0.0037) | (0.0008) | (0.0281) | (0.0050) |
| CBDD*Spor. reg. plots post-2010 | 0.0017*** | -0.0320*** | -0.0012 | 0.0027*** | -0.0193 | 0.0054*** |
| | (0.0003) | (0.0108) | (0.0013) | (0.0009) | (0.0146) | (0.0017) |
| Syst. reg. plots post 2010 ($\theta_3$) | 0.0004 | -0.0000 | 0.0003 | 0.0017*** | 0.0059*** | 0.0044*** |
| | (0.0006) | (0.0006) | (0.0004) | (0.0004) | (0.0008) | (0.0005) |
| CBDD*Syst. reg. plots post 2010 | 0.0002** | 0.0182*** | 0.0029*** | 0.0015*** | -0.0278*** | -0.0009 |
| | (0.0001) | (0.0069) | (0.0007) | (0.0001) | (0.0094) | (0.0009) |
| No. of obs (250*250 m blocks) | 38,600 | 38,600 | 38,600 | 38,640 | 38,640 | 38,640 |

*Note:* Block fixed effects and constant included throughout. Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.010

**Appendix figure 1: Kernel-based matching density distribution of standardized percentage bias across covariates**

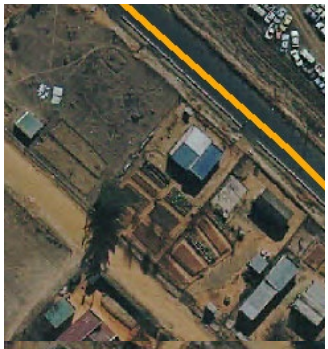


**Appendix figure 2: Standardized percentage bias across covariates (reported in appendix table 1) before and after kernel-based matching**




*Note:* See appendix table 1 for variable labels.

**Appendix figure 3: Sample land use changes between 2013 and 2016 using high resolution aerial photography**

| 2013 | 2016 | 2013 | 2016 |
|---|---|---|---|
|  |  |  |  |
|  |  |  |  |
|  |  |  |  |
|  |  |  |  |

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