

# Land- and credit-market effects of urban land titling: Evidence from Lesotho\*

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*Abstract:* While land titling is advocated to improve land and financial market functioning in developing country cities, evidence of credit market effects is limited. We use 20 years of registry data from Lesotho to assess impacts of an urban titling program overall and by component (titling vs. policy/institutional reforms) on land and mortgage markets by gender over time. In the longer term, both components increased land and mortgage markets activity, though at different rates. In the short term, titling did not affect mortgage markets, but policy reforms increased registered parcels' likelihood of being mortgaged by reducing registration costs. Titling catalyzed translation of earlier family law changes to strengthen women's property rights (that had no independent effect) into social and economic empowerment. Detailed analysis of impact channels and time profiles for titling programs' components can provide policy-relevant insight and is relatively easy using parcel-level registry data.

*JEL Classification:* G21, H41, J16, K11, O18, Q15

*Keywords:* Urban land policy reform, systematic land registration, formal land market, credit market, gender, Lesotho

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## 1. Introduction

Africa is now the region with the fastest rate of urbanization globally (Henderson & Turner 2020) with an urban population expected to triple by 2040 (Collier 2017). While urbanization can bring benefits whereby skill-based human capital externalities from input sharing, labor market pooling, and knowledge exchange (Rosenthal & Strange 2020) help generate productive jobs and boost competitiveness (Venables 2017), it imposes costs from congestion, contagious disease, and crime (Duranton & Puga 2020). Land titles that allow use of land as collateral to build high structures (Henderson *et al.* 2021) and make it easier to foster mobility through public infrastructure (Brandily & Rauch 2020) can support sustainable urbanization in many ways as evidenced by strong links between greater cadastral coverage and economic growth (d'Arcy *et al.* 2021) at country level. Evidence on credit effects from titling—long the main argument for such interventions (de Soto 2000)—at micro-level remains, however, scant.

One reason for this may be that the ability to use title as collateral depends on institutional features such as presence of a public registry that provides complete and authoritative information on property ownership. If titling programs are implemented in settings where this is not the case and fail to address regulatory or institutional constraints impeding effective registry operation, credit effects from titling will be contingent on eliminating such barriers. To test this hypothesis, we analyze effects of the Lesotho Land Administration Reform Project (LARP) that combined legal and regulatory reforms and institutional restructuring (used interchangeably with ‘policy reforms’ below) with systematic titling (or first registration of rights) in select areas of the city over the 2011-13 period. Parcel-level administrative data (aggregated to small blocks) that includes information on owners’ gender for 2000-2019 allow us to separate short- from longer-term effects and explore the gender dimension of program impacts.

Our outcome variables are the flow of parcels registered for the first time as well as the number of parcels subject to a registered sale or mortgage, separately for parcels with and without female co-owner. Identification makes use of two factors and our ability to identify whether a parcel was registered by the titling program (often referred to as systematic registration process) or independently from it (under a sporadic process). First, program-supported policy reforms improved functioning of an existing registry by reducing the cost of registering new sales or mortgages and their impact will materialize via the stock of parcels already registered. Second, as the systematic titling process is associated with a presumably more gender sensitive process than that used for sporadic registration and targets parcels or owners likely to differ systematically from those that already registered through the sporadic process, gender distribution of rights and likelihood of being sold or mortgaged are expected to differ depending on whether parcels entered the

registry through systematic titling or sporadic self-registration. Block- and year-fixed effects and time varying data on built area and population density are included as control variables throughout.

Complete registry data from 1981-2019, digitized for this purpose, provide the basis for our analysis. We divide the city into 1,932 blocks of 250m x 250m each and aggregate the number of parcels registered for the first time under the project's systematic process or independently from it (what we refer to as 'sporadic' registration) as well as the number of registered sales and mortgages involving a land parcel at block level. Panel regression analysis for 2000-2019 (using some 40,000 block-year-level observations) is then used relying on two identification strategies. For short-term effects, we rely on the fact that not all parts of Maseru were eligible for titling (and to a lesser extent the phased roll-out) and interact the lagged stock of registered parcels with a reform dummy to obtain a lower bound estimate of the catalytic effects of policy reforms. To obtain an estimate of longer-term effects on functioning of land and mortgage markets, we use stocks of parcels registered under the titling program or independently of it and an interaction with the policy reform dummy with the latter.

We find that impacts differ between program components and evolve over time. A key effect of titling was to increase levels of registration and women's ownership in the short term. Regulatory reform and institutional restructuring then improved the functioning of markets, especially those for mortgages, an effect that materialized quickly for parcels that had already been registered sporadically but that took longer time to materialize for parcels that had only just been formalized via the systematic land registration program. As a result, in the longer-term stocks of systematically and sporadically registered parcels had significant and positive effects on increasing sales and mortgages, but the magnitude of respective elasticities remained much larger for parcels that had been registered sporadically. We note that this is partly due to systematic differences in parcel characteristics that affected parcels' marketability and thus their suitability as collateral for loans. Interestingly, we also find that the 2006 legal provisions that allowed married women to independently own assets alone did not translate into higher levels of female land ownership and titling increased female land ownership well above what was achieved by regulatory reforms.

Our findings contribute to three strands of literature. First, a large body of literature assessing short-term impacts of titling finds no impact of new title issuance in the short term. Our results are consistent with this but show that (i) regulatory or institutional changes that may be initiated as part of titling programs may, by affecting the cost of selling or mortgaging parcels registered independently from titling, result in credit market effects even in the short term; and (ii) the size and significance of credit market effects increase over time. This suggests that considering the effects of program components separately and expanding the time horizon for analysis can provide policy relevant insight. As the cost of policy and institutional reforms

is generally well below that of titling, better understanding of the impacts of each component separately and their interactions and the determinants of such impacts can have potentially far-reaching implications for the design and sequencing of titling programs.

Second, most studies of titling programs in developing countries rely predominantly on household survey data that create challenges for identification of policy effects and, as land transactions are rare events and information on land rights or encumbrance to them through mortgages may not be shared among household members, may suffer from reporting error. Our analysis shows that administrative data allow to overcome these shortcomings and more appropriate analysis of the time profile of titling benefits. As such data can be generated at very low cost by any land titling program, making them, with proper data protection, routinely available for analysis including—as a sample frame for follow-up household surveys—is easy and can offer an opportunity to significantly broaden the knowledge base, build capacity, and inform policy.

Finally, we contribute to the literature on women’s property rights to land in two ways. On the one hand, we go beyond the literature that has documented clear social empowerment effects from providing women with documented land rights by showing that, if a functioning registry is in place, empowerment will extend to the economic sphere and allow greater access to land and financial markets. On the other hand, while the literature documents the far-reaching impacts of legally empowering women in the long term (Hazan *et al.* 2019), there is also evidence that, in the short term, changes in land or family law to the same end may have limited (Linkow 2019) or even adverse (Anderson & Genicot 2015) effects. By showing that far-reaching legal changes to married women’s property ownership by the Legal Capacity of Married Persons Act (LCMPA) did not affect their registered co-ownership of real estate but that more specific legislation such as the presumption of joint ownership for all newly registered property by married couples and procedures to documenting such rights via titling greatly increased its impact in this respect, our findings suggest that legal provisions in favor of women are a necessary but not a sufficient condition for actual empowerment. Global comparisons of women’s rights based on legal analysis might thus usefully be complemented with indicators on outcomes that can be easily obtained from digital administrative data.

The rest of the paper is organized as follows. Section two places the study within the literature on the multi-faceted impacts of secure property rights to land and describes legal and administrative reforms and initial registration activities supported by LARP. Section three discusses data and descriptive evidence and provides equations for estimating short- and longer-term effects. Results for the different outcome variables, including those related to gender, are discussed in section four, and section five concludes and offers implications for policy and research.

## 2. Background and Program Characteristics

Although land titles that can help provide access to mortgage lending to construct urban floorspace will be important to make expected increases in Africa’s urbanization productive, most empirical studies find little evidence of such effects. We note that this may be due to a focus on the short-term and a focus on titling but not registry operation and discuss how evidence from Lesotho can overcome both these shortcomings.

### 2.1 Context and relation to the literature

A key tenet in the development literature has been that issuance of formal titles allows land to be used as collateral for credit, converting an unregistered ‘dead asset’ into ‘live capital’ (de Soto 2000). However, although significant resources were spent on land titling in developing countries, there is little evidence on positive impact even in urban areas where the potential benefits from accessing credit will be higher and the transaction cost of doing so lower than in rural settings. In urban Peru, titling increased formal labor market participation (Field 2007), human capital investment (Galiani & Schargrodsky 2004), and political participation (Di Tella *et al.* 2007) but had no effect on credit (Field & Torero 2006). Credit effects also failed to materialize in urban land titling programs in Argentina (Galiani & Schargrodsky 2010), Ghana (Agyei-Holmes *et al.* 2020) and Benin (Fabbri & Dari-Mattiacci 2021).

While many of the above studies focused on short-term impacts, two factors could explain the widespread absence of credit effects from titling in the literature. One is that programs overwhelmingly focused on informal settlements where land markets may be thin and scope for using land as collateral more limited. The other is that continued high cost of registering any changes after (subsidized) first registration would result in prohibitive cost of registering mortgages even if banks were willing to accept land titled under such programs as collateral. High registration fees are the main reason why, even in a well-known program in Argentina, a decade or more after the original program had ended, almost 80% of cases where ownership had changed reverted to informality (Galiani & Schargrodsky 2016).<sup>1</sup>

The importance of affordable fees to sustain the benefits from systematic land registration over time is supported by evidence from Peru where, in response to political pressure from notaries, a low-cost registry created specifically for transactions involving systematically titled properties was closed, triggering a significant return to informality by beneficiaries from this program who no longer could afford high registration fees (Gutierrez & Molina 2020). The transaction costs of processing loan applications are also important for lenders: elimination of the need for office visits and payment of informal fees through digital

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<sup>1</sup> In the case reported by Galiani and Schargrodsky (2016), registering an inter-family transaction cost 27% of property value or more than 7 times average household income while registering a within-family transfer such as an inheritance or divorce cost about 20% of property value and 5 times average monthly household income. More circumstantial evidence of high fees leading to ‘deformalization’ comes from Jamaica (Barnes & Griffith-Charles 2007), the Philippines (Maurer & Iyer 2008), and—for agricultural land—Rwanda (Ali *et al.* 2021).

registry access in the course of registry computerization increased the volume of mortgage lending in urban India (Deininger & Goyal 2012).

As the cost of registering rights and accessing registry information depends on regulations and institutions, policy and institutional reforms can ensure that any transactions involving newly registered rights can be registered at low cost, thereby eliminating potential barriers to sustainability and credit impact. In Lesotho's case, LARP-supported reforms comprised three laws and associated implementing regulations.

First, while not the primary focus of our study, the 2006 LCMPA provided the basis for LARP. Before this law, married women were considered minors who were technically property of their husbands, a status that prevented them from owning property or land independently. Parallel to the elimination of coverture in the US in the 1850s that was credited with higher levels of girls' education (Geddes *et al.* 2012) and a deepening of financial markets (Hazan *et al.* 2019), the LCMPA removed the minority status of married women and the powers a husband previously had over the person and the property of his wife.<sup>2</sup>

Second, legal reform involved adoption of the 2010 Land Act and the 2010 Land Administration Authority (LAA) Act and associated regulations, most of which became effective in 2011. The Land Act aimed to (i) reduce barriers to registering land rights by simplifying procedures, including elimination of the need for Ministerial consent to register land transactions; (ii) reinforce the rights women had received after the passage of the LCMPA through a presumption that any property acquired in marriage is jointly owned by both spouses; and (iii) regulate systematic land titling including by exercising the joint ownership presumption.

These laws provided the basis for institutional reform,<sup>3</sup> a main element of which was the establishment of the LAA as a one-stop shop to register rights to land and obtain reliable information on rights or encumbrances for any land parcel. A fully digital register and cadaster, designed to be interoperable with systems by other agencies (e.g., municipalities' building registers and valuation rolls) was also established to among others reduce maintenance costs. While these regulatory changes will impact all landowners, the nature of the associated effects will vary depending on whether land rights had been registered before the program or were eligible for first registration under LARP's systematic land titling component.

Building on these above legal reforms, a program of land titling (systematic first registration of rights) was rolled out over the 2011-2013 period in poor informal settlements corresponding to Maseru's expansion

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<sup>2</sup> Until this act was passed, women were considered to be perpetual minors who could contract for or own property only with the assistance of a guardian, i.e., their father, husband, or husband's heir. Restrictions removed by the LCMPA related to ability to enter a contract; suing or being sued; registering immovable property in the woman's name; acting as an executrix of a deceased's estate or a director of a company; and binding herself as surety. See <http://www.osall.org.za/docs/2011/03/Lesotho-Legal-Capacity-of-Married-Persons-Act-9-of-2006.pdf> for a copy of the Act.

<sup>3</sup> A total of 18 regulations pertaining to systematic land titling, planning, surveying, registry operation, and dispute resolution had been drafted and most had been adopted by 2013.

area. Figure 1 illustrates the intervention area and the sequencing of the roll-out, by displaying 722 blocks not eligible for the systematic registration (in white) together with 1,210 blocks eligible for this intervention comprising 150 blocks (light grey) covered in a 2011 pilot; 29 (dark grey) covered in 2012, and 1,031 (black) blocks treated during the 2013 full roll-out.

To assess the impact of these intervention, we digitized paper records from registry books for transactions registered before LAA migrated to a functional digital platform, thereby obtaining data on the universe of registered rights and transactions that occurred in the 1981-2019 period in all of Maseru.<sup>4</sup> In particular, we have data on the location, size, the right holders' gender for all parcels which were registered for the first time, transferred via sale, or encumbered via a mortgage as well as the year of first time registration and, for sales or mortgage registrations, the start and end date of registration process.

To create geographical units for analysis, we divide the city into close to 2,000 square blocks of 250m x 250m each and aggregate dependent and independent variables used in the analysis at this level. Appendix Figure A1 illustrates the principle: dashed lines show block boundaries and parcels are assigned colors depending on whether they were registered sporadically before (purple) or after (brown) the 2010 legal changes or systematically after 2010 (blue).<sup>5</sup> Spaces for which no rights are registered remain white. For illustrative purposes, 2016 building footprints, derived from high-resolution satellite imagery using machine learning are also displayed.

We use these data to examine four questions. First, at the outcome level, we examine if policy reforms reduced the cost of registering land rights as intended. While program impacts on the number of registered rights or women's land ownership could still materialize, program impacts on activity in land and financial markets would be implausible without evidence of reforms having reduced registration cost. We use the actual time required to complete registration of sales or mortgages every month, to measure this and use regression discontinuity plots to display changes in registration time before and after the reform.

Second, we assess the program's short-term effects on the number of parcels registered for the first time as well as sales and mortgages registered, overall and with females as a sole or co-owner. To do so, we use block-level panel regressions for the period 2007-14 on a matched sample and rely on differences between eligible and non-eligible blocks as well as the program's phased roll-out for identification. A placebo test (for 2003-10) is used to rule out other unobserved factors causing observed effects. Moreover, as estimated effects are comprised of the two program components, i.e., regulatory and institutional reforms and systematic titling, we also explore the impact of reforms in interaction with the stock of registered parcels.

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4 Such administrative data avoid potentially large sampling and non-sampling errors from trying to obtain reliable information on land transactions via household surveys or mortgages based on actual transactions rather than expert opinions about "typical" transactions.

5 Parcels that cross block boundaries are assigned to the block containing the parcel centroid.

Third, we go beyond the short term to identify longer-term program impacts on functioning of land sales and mortgage markets via the elasticity of such transactions with respect to the stock of registered parcels. Differentiating between parcels that were registered systematically via the program and those registered sporadically outside the program and interacting sporadic parcels with a post-reform dummy allows us to compare between the two types of parcels and recover an estimate of the effect of policy reforms.

Finally, although evidence provided is less rigorous, we ask if women's legal empowerment through the 2006 LCMPA was on its own a sufficient condition to increase women's asset ownership or whether learning, subsequent regulatory reform or free first registration using gender sensitive procedures and targeted dissemination added to it. We do so by comparing the share of newly registered parcels with at least one female owner in different time periods using (as in the other regressions) block fixed effects to control for time invariant locational characteristics.

## 2.2 Data and descriptive statistics

For panel econometric analysis, we use 2000-2019 as analysis period, a window covering approximately equal number of years before and after the 2010 legal and institutional changes. We aggregate the annual number of registered parcels and sales or mortgage transactions by gender for the 250m x 250m blocks as discussed above. In the absence of time series data on built volume, we use time-varying information on built area as time varying control variable.<sup>6</sup> To obtain it, public data from the World Settlement Footprint Evolution (WSFE) annual time series on built area for the period 1985–2015 at 30x30m resolution was complemented with a layer on built area in 2019 at 10m resolution made available by the WSFE team.<sup>7</sup> Overlaying with the registered parcel layer provides an estimate of built area on registered and non-registered parcels for each block.

Table 1 summarizes transaction-level variables for the 39-year period (1981–2019) for which data is available (column 1) and separately the 1981–2010 pre-reform period, split into pre-and post-2000 (columns 2 and 3); the 2011–14 LARP period following passage of the Land Act and establishment of LAA (column 4); and the 2015–19 post-program period of routine LAA operation (column 5). Detailed statistics by process of registration and gender are presented in Appendix Table A1.

The number of parcels registered annually increased from 522 in the 2000-10 period to 9,574 in the 2011-14 period, when systematic registration under LARP was active, before they levelled off at 1,581 in the 2015–19 post-LARP period. While all registrations were sporadic before 2011, about 86 percent of initial

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6 Esch *et al.* (2022) describe the first global dataset providing cross sectional information on built volume at a 90m resolution but time series data on this variable are not available.

7 Annual time series data from 1985 to 2015 is available from <https://doi.org/10.6084/m9.figshare.10048412.v1>; data at 10m resolution for 2019 was kindly made available by M. Marconcini and missing annual values (2016–18) interpolated using a cubic spline.



registrations in 2011-14 were carried out through the systematic approach. Together with pending registrations that were cleared after LARP formally ended, this brings the total number of systematically registered plots in Maseru city to 34,422—59 percent of the total. Appendix Figure A2 plots the total number of newly registered parcels by modality and time. We note a monotonic increase in systematic registration, with the pace increasing somewhat after 2010. The phases of piloting in 2011 and initial rollout in 2012, full roll-out in 2013, and post-rollout clean-up thereafter are clearly distinguishable.

Descriptive statistics also point to changes in outcome variables. Sales and mortgages registered annually increased from 96 and 94 in 2000–10 to 871 and 400 after program completion in 2015–19, respectively. The share of newly registered parcels that had at least one female co-owner went up from 23 percent in 1981–99 and 40 percent in 2000–10 to about 85 percent in 2011–14 and 80 percent in 2015–19. Increases were less pronounced in the share of built-up area overall (from 46.1 to 49.8 percent) and on registered land (from 6.8 to 22.3 percent) between 2000–10 and 2015–19.

As a first check on the extent to which the 2010 regulatory and institutional reforms may have affected the transaction cost of registering sales or mortgages, we use the number of days between lodging an application for registering a sale or mortgage and actual registration of each transaction.<sup>8</sup> Figure 2 provides regression discontinuity (RD) plots of this variable for property transfers (panel A) and mortgages (panel B) by month with January 2011 as the cutoff. In each case, we use a second-order global polynomial for the entire 2001-2019 period as well as a local linear approach for time windows covering 40 and 20 months on either side of the cutoff.

In both cases, a discrete break and slope change that coincide with the implementation of legal and regulatory reforms in 2011 are visible. The shift is more pronounced for registering mortgages that do not require surveying, something that can be attributed to two factors. On the one hand, sales that do not involve the transfer of an undivided parcel require the hiring of surveyor to conduct the physical partition in the field, an activity that was not streamlined via new regulation. Also, while LARP-supported regulations eliminated the need for Ministerial consent that had often taken 80–100 days for both sales and mortgages, the effect was proportionately larger for the latter.

Table 2 presents block-level information on pre-program values for outcome and control variables for the entire sample (col. 2) and the matched sample based on observed block level characteristics before policy changes as discussed in the next section (col. 3), which is then separated into treatment (col. 4) and control (col. 5) blocks with and without LARP-supported systematic registration activity. T-tests for equality of means between treatment and control reported in col. 6 suggest that such equality cannot be rejected at a

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<sup>8</sup> As fee receipts to measure monetary cost of registering are not available, we focus on the time required to complete registration instead.

99 percent confidence level for any of the variables listed and at the 90 percent level for only one variable (number of registered transfers in 2000). For the matched sample, Figure 5 graphs the number of registered parcels vs. time separately for treatment and control blocks. The value of the test statistic for parallel trends before the reform is 1.8, well below the critical value for rejection at any conventional level of significance.

### 3. Short-term effects of the intervention

While we find significant effects of the program on the number of registered parcels with women owners, effects on mortgage markets are insignificant, in line with the literature. Separating program effects suggests that, via the stock of registered parcels, policy reforms have an immediate positive effect on mortgage and sales markets while titling has a negative effect.

#### 3.1 Overall program effects

In a way that replicates the type of analysis in most studies of titling programs, we use a panel of blocks for 2007–14 to analyze short-term effects of systematic land registration on the number of newly registered plots with or without a female co-owner and the number of registered transfers or mortgages with or without a female co-owner for the matched sample. Indexing blocks by  $i$  and time by  $t$ , the equation to be estimated is

$$Y_{it} = \alpha_i + \beta_1 T_{it} + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (1)$$

where  $Y_{it}$  is the relevant outcome variable as discussed above;  $T_{it}$  is a zero-one indicator denoting whether or not block  $i$  had been eligible for the systematic land titling program at  $t$ ;  $X_{it}$  is a vector of time-varying covariates including built-up area, population,<sup>9</sup> and the interaction between the two;  $\alpha_i$  and  $\lambda_t$  are vectors of block and time fixed effects; and  $\varepsilon_{it}$  is an error term. The coefficient of interest,  $\beta_1$ , is the difference-in-differences (DID) estimate of the short-term impact of LARP.

A central assumption for identification is that control and treatment blocks are comparable in relevant characteristics. Appendix Table A2 suggests this was not the case; in line with LARP’s focus on informal areas, treatment areas were more densely populated and built but had fewer sporadically registered parcels and fewer registered sales and mortgages than controls. To address this, we generate a more comparable sample via propensity score matching using pre-reform values for the share of built area, population density, distance to the Central Business District (CBD), and flows of initial registrations, transfers, and mortgages to construct the propensity score. Using 3-nearest-neighbor matching with replacement<sup>10</sup> to trim

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9 We use population data for 2000–19 from the Center for International Earth Science Information Network (CIESIN) gridded population of the world (version 4) data set with a 1 km spatial and 5-year temporal resolution is used to interpolate annual levels of block-level population density that are included as a control throughout.

10 Alternative matching models (one-to-one and 5-nearest-neighbors) were considered but 3-nearest-neighbors performed better in terms of the standardized reduction in bias between the matched and unmatched samples.

observations from outside the common support leaves us with a total of 1,514 observations—1,073 in the treated and 441 in the control group.

Table 3 panel A reports estimated short-term effects of the program on the number of parcels registered for the first time (cols. 1&2); registered sales (cols. 3&4), and mortgages (cols. 5&6). Three findings are relevant. First, the program resulted in a large increase of registered parcels (coefficient of 9.5), especially those held jointly by women (8.2), well beyond pre-project means of 0.22 and 0.08 for registered parcels overall and with female co-ownership.

Second, the program more than doubled the number of registered sales, especially by women (coefficients of 0.083 and 0.083 compared to pre-project means of 0.054 and 0.02, respectively). Descriptive evidence is inconsistent with fears of such sales being an indication of women abrogating their rights in favor of male kin as observed in the context of reforms to inheritance rights in some South Asian contexts or of distress sales that may be associated with significant land concentration. We run a separate regression for purchases rather than sales (results not reported) and find that the estimated coefficient in this regression is about equal in size and significance to that for sales, lending further evidence to the notion that this result is not driven by forced sales.<sup>11</sup> Finally, we find no significant effects on the number of registered mortgages, in line with much of the literature on credit effects from land titling programs.

Results from a placebo test based on data spanning the 2003-2010 instead of 2007-14 and a pseudo program in 2006 are presented in panel B of table 3. Estimates for  $\beta_1$  are not significantly different from zero throughout and it is important to note that the absolute magnitude of the coefficients of the placebo treatment are an order of magnitude smaller than the coefficients estimated for the actual treatment indicator. These results tend to suggest the reliability of our estimates of the impact of the titling program.

### 3.2 Testing for policy reform effects

The program effect ( $\beta_1$ ) as estimated above comprises the effect of regulatory and institutional reforms as well as systematic registration. Regulatory and institutional reforms will affect outcomes via the stock of registered parcels and through new sporadic registrations. While economy-wide effects of these reforms are absorbed by time dummies, interacting a policy reforms dummy with the stock of registered parcels allows capturing one of the elements through which these reforms affect outcomes of interest.<sup>12</sup> Formally, letting  $P$  be an indicator for the post-2011 period when the Land Act became effective and LAA was established, we estimate

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<sup>11</sup> As information on landowners' IDs was not provided, we are unable to ascertain whether the program was associated with land concentration. Regression results are not reported but available from the authors upon request.

<sup>12</sup> If the impact of policy changes via new registration is positive, this would thus allow us to obtain an upper bound for the impact of systematic registration.

$$Y_{it} = \alpha_i + \beta_1 T_{it} + \beta_2 \mathcal{R}_{it-1} + \beta_3 P \times \mathcal{R}_{it-1} + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (2)$$

where, in addition to the variables defined above,  $\mathcal{R}_{it-1}$  is the lagged stock of registered parcels. To deal with potential reverse causality between the stock of registered parcels and registered mortgages or sales, (2) is estimated using an instrumental variable (IV) fixed effects estimator with the two-year lag of the stock of registered parcels used as an instrument.

Results from estimating (2) as reported in Table 4 point to three main findings. First, estimated coefficients on systematic titling (14.66 overall and 12.71 for female co-ownership) are larger than those in Table 3 due to negative effects of the lagged stock of registered parcels (coefficient of -0.431 overall and -0.376 for female co-ownership) that are reinforced by policy related effects (coefficients of -0.106 and -0.089, respectively). In other words, the number of registered parcels converges across blocks and such convergence is reinforced by policy-induced changes.

Second, for mortgages and sales, policy reforms improved (female) owners' ability to sell or mortgage land to which they have registered co-ownership rights. In fact, the coefficient on  $\beta_2$  is not statistically significant for mortgages overall (at 5%) and for sales and mortgages with a female co-owner. In the context of Lesotho, policy reforms were thus instrumental to translate higher levels of land registration into greater sales and mortgage market activity. Interestingly, after adjusting for policy and the pre-existing stock of registered parcels, the estimated coefficients of systematic titling on land sales and mortgages are negative and significant throughout. Estimates of longer-term sales and credit market effects will be needed to determine whether this is due to a learning effect or reflects consistently lower levels of marketability of the parcels targeted by systematic titling during the period of observation.

Finally, to appreciate the magnitudes involved, estimated elasticities of the stock of registered parcels and its policy interaction at the mean of relevant variables are displayed in the bottom part of Table 4. We note that, to the extent that it is possible to extrapolate from these, doubling the stock of parcels would be estimated to more than double registered sales and increase mortgages by 69% with rates slightly lower (elasticities of 59% and 41%, respectively) for parcels co-owned by females. Between one third of the increase in registered sales and more than two thirds of the increase in mortgages are policy induced. Larger estimated effects on mortgage registration can likely be attributed to the fact that registering a mortgage does not require a survey and that surveying was not affected by policy and institutional reforms as noted above.

#### 4. Longer-term effects

For longer-term market effects, evidence point towards positive effects from titling and reform with the latter particularly pronounced for mortgage markets. Expanding the earlier gender analysis also suggests that the possibility of married women gaining asset ownership established by the 2006 LCMPA was a necessary but not sufficient condition for higher levels of (registered) female land ownership compared to no significant effect of this act on its own, both policy reforms and titling significantly increased levels of female land ownership.

##### 4.1 Longer-term effects of systematic registration and policy changes

Longer-term effects on land and financial markets could differ from those observed in the short term for several reasons, including time needed by individuals or institutions to adjust to the changed regulatory and institutional environment or for recipients of documented rights to learn about the opportunities associated with such rights. To assess program impacts, separately for titling and policy reforms, we assume that (i) land sales and mortgage market participations are a function of the lagged stock of registered parcels; (ii) the likelihood of being subject to a registered (sale or mortgage) transaction may differ between parcels registered systematically (by the program) and those registered sporadically (outside the program) due to systematic unobserved differences in parcel- or owner-characteristics; and (iii) regulatory and institutional reforms affect land sales and mortgage markets functioning by changing associated registration cost.

Formally, let  $\mathcal{R}_{it}$  as well as  $\mathcal{R}^p_{it}$ ,  $\mathcal{R}^y_{it}$  denote the stock of total, sporadic, and systematically registered parcels in  $t$ . Using lagged values for the stock of registered parcels, we estimate

$$Y_{it} = \alpha_i + \theta_1 \mathcal{R}^p_{it-1} + \theta_2 \mathcal{R}^p_{it-1} \times P + \theta_3 \mathcal{R}^y_{it-1} + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (3)$$

where  $Y_{it}$  denotes the number of registered transfers or mortgages;  $P$  is an indicator variable for  $t > 2011$  (i.e., registration happened after 2011);  $X_{it}$  are time-varying covariates;  $\alpha_i$  are block fixed effects;  $\lambda_t$  are time fixed effects; and  $\varepsilon_{it}$  is the error term. Potential endogeneity of sporadic registration due to the possibility of parcels being registered to facilitate a transfer or mortgage transaction are addressed by instrumenting  $\mathcal{R}^p_{it-1}$  with its second lag  $\mathcal{R}^p_{it-2}$ . To interpret coefficients, note that  $\theta_1$  denotes the estimated impact of sporadic registration in the absence of policy reforms;  $\theta_3$  the estimated impact of systematic registration which, as all titling happened after 2011, includes the policy reforms effect; and  $\theta_1 + \theta_2$  the estimated impact of sporadic registration post-2011 so that  $\theta_2$  provides an estimate of the impact realized via policy reforms.

Results from estimating (3) for registered land sales and mortgages overall (cols. 1 and 3) and for female co-owners separately (cols. 2 and 4) are reported in Table 5. All coefficients are positive and highly

significant. This means that increases in the stock of registered parcels—irrespective of whether they were due to systematic or sporadic registration—resulted in higher levels of land sales and mortgage market activity. Moreover, regulatory and institutional reforms independently expanded the scope for transacting registered parcels in formal land or mortgage markets. To facilitate interpretation, we report elasticities and their statistical significance evaluated at the means of covariates in the bottom part of Table 5.

Two types of comparison are of interest. First, we can compare the elasticity of sporadic registration conditional on policy reforms ( $\theta_2$ ) to that of systematic registration on its own ( $\theta_3$ ). Doing so suggests that in the case at hand, the catalytic effect of policy reforms is relatively more important than titling for operation of mortgage markets while the opposite is true for sales markets. In both cases, policy reforms are comparatively more important for women's participation: with elasticities of 0.080 for sales and 0.258 for mortgage registration (0.165 and 0.273 for parcels (co-)owned by women in sales and mortgage markets), policy-induced effects are important on both counts.

Second, the elasticity of the stock of systematically registered parcels ( $\theta_3$ ) vs. those sporadically registered ones ( $\theta_1 + \theta_2$ ) post-reforms suggests that the parcels targeted for first registration in informal settlements at the city's periphery were less marketable (elasticity of 0.156 vs. 1.09 overall and 0.191 vs. 0.658 for female co-ownership) or mortgageable (elasticity of 0.071 vs. 0.882 overall and 0.156 vs. 0.667) than those registered sporadically.

One of the reasons for differences in estimated effects between registration modalities may be attributable to systematic unobserved differences between parcels (or their owners) who entered the registry through systematic as compared to sporadic registration. To check if such differences, which might make certain parcels less marketable or attractive as collateral, may be present, we regress, for registered parcels, a dummy indicating whether they were registered via a systematic or sporadic process and, for the latter, whether first registration happened before, during, or after the LARP-supported systematic registration program was operational, against distance to the central business district (CBD) and parcel area.

Results, displayed in Appendix Table A3, suggest that, overall, sporadically registered parcels are larger and located more closely to the CBD than those registered systematically. These differences are most pronounced for parcels registered in the pre-reform period (1981-2009) and much weaker in the period when systematic titling was conducted. Parcels newly registered post-2015 are indistinguishable from those included in the systematic titling program in observable attributes.

#### **4.2 Was legal empowerment a sufficient condition for women's economic participation?**

Given strong evidence in support of gender effects of first registration and policy reforms in the above regressions, it is of interest to explore the extent to which co-ownership by women upon first registration

of land rights can be attributed to (i) broad legal changes in the form of the 2006 LCMPA that predated the reforms analyzed here; (ii) the legal and regulatory reforms supported by LARP (e.g., mandatory joint registration of conjugal property); or (iii) procedural changes and awareness raising activities associated with LARP-supported systematic first registration.

To do so, we let  $Y_{it}$  denote the number of parcels with at least one female co-owner newly registered in  $t$  (i.e., a flow rather than a stock variable as in (3)) and  $G$  an indicator for the LCMPA having been in effect by its own (i.e.,  $G=1$  for  $2006 \leq t < 2011$ ). Letting  $r_{it}^p$  and  $r_{it}^y$  denote the flow of parcels in block  $i$  that enter the registry in year  $t$  via either a sporadic or systematic process so that  $r_{it} = r_{it}^p + r_{it}^y$  is the total number of parcels registered for the first time in  $t$ , we estimate

$$Y_{it} = \alpha_i + \gamma_1 r_{it}^p + \gamma_0 r_{it}^p \times G + \gamma_2 r_{it}^p \times P + \gamma_3 r_{it}^y + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (4)$$

where other variables are as above. Coefficients of interest are estimated impacts of (i) the LCMPA alone without further regulatory reform ( $\gamma_0$ ); (ii) sporadic registration without regulatory reform ( $\gamma_1$ ); sporadic registration with the 2006 and 2010 legal and regulatory reforms ( $\gamma_2$ ) and (iii) systematic registration that builds on the 2006 and 2010 legal and policy reforms ( $\gamma_3$ ).

A surprising result from these regressions, reported in Table 6, is the lack of statistical significance of  $\gamma_0$ . It suggests that either women did not become aware of the legal changes until 2010 or that, in the absence of sector-specific follow-up, the LCMPA alone failed to increase the share of parcels registered first time with female (co-)owners from the base level of 0.283 ( $\gamma_1$ ). The statistically significant point estimates of 0.232 for  $\gamma_2$  and 0.866 for  $\gamma_3$  suggest that policy reforms almost doubled the share of parcels registered for the first registrations with at least one female owner and that systematic registration increased the marginal probability of female ownership by a further 35.1% ( $0.866 - (0.283 + 0.232)$ ), most likely via awareness raising and standardized gender-sensitive procedures applied in the course of systematic titling.

## 5. Implications for research and policy

We use a long time series of detailed registry records to analyze the short and the longer-term effects on the number of parcels registered for the first time as well as registered land sales and mortgages overall and with at least one registered female owner of a program that combined urban titling with policy reforms (i.e., legal and regulatory changes as well as institutional restructuring). Substantively, three findings stand out.

In the short term, we find significant program effects on the number of registered parcels overall and those with at least one female owner and (less clear-cut) land sales but no effect on mortgages, mirroring results from the literature. Disaggregation suggests insignificant credit effects comprise a positive impact of policy changes (in interaction with the stock of registered parcels) together with a negative effect of first

registration, consistent with the notion that it is unrealistic to expect land that was just registered to quickly become mortgageable.

An interesting aspect of our analysis is that it allows us to explore longer-term effects that materialized up to almost a decade after key legal reforms were enacted. Both first registration and policy reforms made a significant contribution to improved land and financial market functioning. With elasticities of 0.258 overall and 0.273 for parcels with female co-owners, policy reforms seem to have had a larger impact on mortgaging of already registered parcels than titling (elasticities of 0.071 and 0.156) whereas titling seems, with elasticities of 0.156 and 0.191 vs. 0.079 and 0.165, have more impact on land sales markets than those arising from policy reforms.

Both policy reforms and systematic registration were instrumental to ensure effectiveness of legal provisions (through the 2006 LCMPA) that granted married women ownership rights to land and other assets. Our analysis shows that, while the law on its own had no effect, regulatory and institutional reforms increased the share of women's ownership of parcels registered for the first time by 82% (0.232/0.283) and the systematic registration program added another 68% (0.351/0.515). This suggests that laws empowering women alone will not be enough to rapidly expand female land ownership.

From a policy perspective, the above results imply that (i) expecting land titling to improve land and financial markets performance without policy reforms may be unrealistic; (ii) registering rights to parcels far from the center in informal settlements without addressing barriers to first registration of more centrally located ones as was, for example, done in Argentina (Galiani & Schargrotsky 2010), may forgo large economic benefits; and (iii) if there is a fixed cost in registry operation that requires a minimum threshold of transactions to be viable, an exclusive focus of titling on poor and informal households may undermine longer-term sustainability and a more universal approach with cost recovery may be advisable. This, in fact, is an area for future research.

Methodologically, we have shown that administrative registry data can be used to derive meaningful insights not only on the effectiveness of regulatory reform and institutional restructuring but also on the impact of land registration programs and the channels through which such impacts materialize. As these data are available routinely in any land registry, it is surprising that no more use has been made of them in this respect and options to expand their use (including earmarking of project resources to digitize pre-intervention records or use them to build local analytical capacity) could be explored to redress this and ascertain the extent to which the type and magnitude of effects identified here are more broadly representative. Three areas are promising for further research, namely (i) link to rental market; (ii) combine with time series data on building height; and (iii) use administrative data as a sample frame.



**Table 1: Descriptive Statistics Based on Administrative data**

	<b>Total</b>	<b>1981–99</b>	<b>2000–10</b>	<b>2011–14</b>	<b>2015–19</b>
No. of leases issued	58,582	6,641	5,739	38,295	7,907
per year	1,502	350	522	9,574	1,581
of which owned solely by males <sup>a</sup>	0.264	0.766	0.603	0.142	0.208
Sporadic registrations per year	619	350	522	1,304	1,313
Systematic registrations per year	883			8,270	269
Share of residential property	0.892	0.853	0.926	0.888	0.921
No. of transfers per year	210	51	96	456	871
No. of mortgages per year	168	117	94	322	400

Source: LAA land administration database from 2011 and digitization of paper records, 1981–2011.

<sup>a</sup> Gender relates to the 94.4 percent of properties registered in the name of natural persons rather than legal entities.

<sup>b</sup> Share of built-up area is at the block level, and data for the period 1981–99 is not available.

**Table 2: Descriptive Statistics at Block Level**

	Year	Total	Matched Sample for Short-Term Effects			t-test
			Total	Treatment	Control	
No. of parcels registered	1990	1.35	1.262	1.244	1.280	
	2000	3.20	3.015	3.184	2.846	
	2010	5.76	4.834	4.856	4.813	
No. of registered transfers	1990	0.040	0.025	0.025	0.024	
	2000	0.030	0.017	0.022	0.011	*
	2010	0.060	0.051	0.049	0.053	
No. of registered mortgages	1990	0.097	0.090	0.090	0.090	
	2000	0.011	0.006	0.008	0.004	
	2010	0.076	0.057	0.042	0.072	
Share of total area built	1990	0.382	0.440	0.445	0.434	
	2000	0.445	0.511	0.521	0.501	
	2010	0.476	0.539	0.550	0.527	
Population density (indiv./km <sup>2</sup> )	1990	954	1,055	971	1,138	
	2000	1,356	1,499	1,459	1,539	
	2010	1,570	1,731	1,691	1,770	
Number of blocks		1,932	1,514	1,073	441	

*Sources:* LAA land administration database from 2011 and digitization of paper records, 1981–2011 for parcels, transfers, and mortgages registered. WSFE for built area; and gridded population of the world v4 (CIESIN) for population density.

**Table 3: Short-term Effects of Systematic Registration, Basic Regression and Placebo Test**

	Parcels reg. first-time		Registered sales		Registered mortgages	
	Total	♀ own	Total	♀ own	Total	♀ own
<b>Panel A: Regression</b>						
Titling dummy	9.538*** (0.350)	8.283*** (0.318)	0.081*** (0.030)	0.083*** (0.019)	0.028 (0.020)	0.020 (0.014)
R-squared	0.449	0.454	0.042	0.048	0.018	0.022
Mean dep. var.	0.220	0.083	0.054	0.020	0.043	0.007
SD dep. var.	0.667	0.424	0.256	0.152	0.223	0.083
<b>Panel B: Placebo</b>						
Pseudo titling dummy	-0.019 (0.046)	-0.002 (0.026)	0.011 (0.015)	0.008 (0.006)	-0.018 (0.013)	-0.000 (0.004)
R-squared	0.014	0.016	0.004	0.005	0.007	0.000
Mean dep. var.	0.159	0.052	0.036	0.009	0.022	0.006
SD dep. var.	0.532	0.260	0.204	0.095	0.156	0.080

*Note:* Regressions in panel A are for 2007–2014 with systematic first-time registrations in 2011–14; placebo tests in panel B are for 2003–10 with a pseud-systematic registration four years earlier; dependent variables are as indicated in the table title. Population density, share of built-up area of the block, block and year fixed effects, and a constant are included throughout; the coefficients are not reported. The number of observations (i.e., 250m x 250m blocks) is 12,112 (1,514 for 8 years) in both panels A and B. Mean and standard deviation for the dependent variable are for the pre-program period. Standard errors are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.010.

**Table 4: Test for Short-term Policy Effects: IV regression**

	Parcels reg. first time		Registered sales		Registered mortgages	
	Total	♀ own	Total	♀ own	Total	♀ own
<b>Coefficients</b>						
Titling dummy	14.6580*** (0.275)	12.7108*** (0.237)	-0.1007*** (0.024)	-0.0417** (0.018)	-0.0611*** (0.018)	-0.0291** (0.012)
Stock of registered parcels (lagged)	-0.4314*** (0.027)	-0.3760*** (0.023)	0.0123*** (0.002)	-0.0011 (0.003)	0.0030* (0.002)	0.0008 (0.002)
Reform # stock of registered parcels	-0.1062*** (0.016)	-0.0890*** (0.014)	0.0069*** (0.001)	0.0161*** (0.003)	0.0064*** (0.001)	0.0051** (0.002)
<b>Elasticities</b>						
Total effect (policy=1)	-3.0665*** (0.1253)	-3.6534*** (0.1487)	1.0318*** (0.1018)	0.5897*** (0.0648)	0.6871*** (0.1055)	0.4139*** (0.0767)
Direct effect	-2.4607*** (0.1529)	-2.9543*** (0.1815)	0.6614*** (0.1242)	-0.0423 (0.1335)	0.2175* (0.1288)	0.0555 (0.1581)
Interaction with policy	-0.6058*** (0.0912)	-0.6991*** (0.1082)	0.3704*** (0.0741)	0.6320*** (0.1201)	0.4695*** (0.0768)	0.3583** (0.1422)
Mean dep. var.	0.148	0.045	0.034	0.009	0.027	0.007
SD dep. Var	0.535	0.259	0.204	0.098	0.173	0.083

*Note:* Units of observation are 250m x 250m blocks in the 2007–2014 period (with systematic first-time registration phased in over 2011–2014) and ‘reform’ denoting regulatory and institutional reforms, i.e., the Land Act and establishment of LAA that were effective from 2011. The stock of registered parcels is the lagged cumulative number of parcels registered in the name of a women alone or jointly and its interaction with the policy dummy. Controls include population density, block and year fixed effects, and a constant are included throughout; the coefficients are not reported. The number of observations throughout is 12,112 (1,514 blocks per year for 8 years). Mean and standard deviation for the dependent variable are for the pre-program period. Standard errors are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.010.

**Table 5: Longer-term Effects on Number of Registered Transfers, Mortgages, and Built-up Area**

	No. of registered sales		No. of registered mortgages	
	Total	♀ owner	Total	♀ owner
<b>Coefficients</b>				
Systematically, reg. parcels $\theta_3$	0.0049*** (0.000)	0.0040*** (0.000)	0.0012*** (0.000)	0.0015*** (0.000)
Sporadically, reg. parcels $\theta_1$	0.0275*** (0.002)	0.0235*** (0.003)	0.0104*** (0.001)	0.0091*** (0.002)
Spor. reg. parcels # post-2011, $\theta_2$	0.0022** (0.001)	0.0078*** (0.003)	0.0043*** (0.001)	0.0063*** (0.002)
<b>Elasticities</b>				
Syst. reg. parcels ( $\theta_3$ )	0.1559*** (0.0123)	0.1910*** (0.0162)	0.0708*** (0.0155)	0.1558*** (0.0221)
Spor. reg. parcels post- reform ( $\theta_1 + \theta_2$ )	1.0926*** (0.0369)	0.6580*** (0.0242)	0.8821*** (0.0417)	0.6665*** (0.0309)
Spor. reg. parcels pre- reform ( $\theta_1$ )	1.0133*** (0.0575)	0.4933*** (0.0616)	0.6239*** (0.0650)	0.3939*** (0.0787)
Spor. reg. parcels # post-2011, $\theta_2$	0.0793** (0.0390)	0.1647*** (0.0529)	0.2582*** (0.0441)	0.2726*** (0.0675)
Mean dep. var.	0.0375	0.0115	0.0278	0.0059
SD dep. Var	0.2118	0.1141	0.1777	0.0775

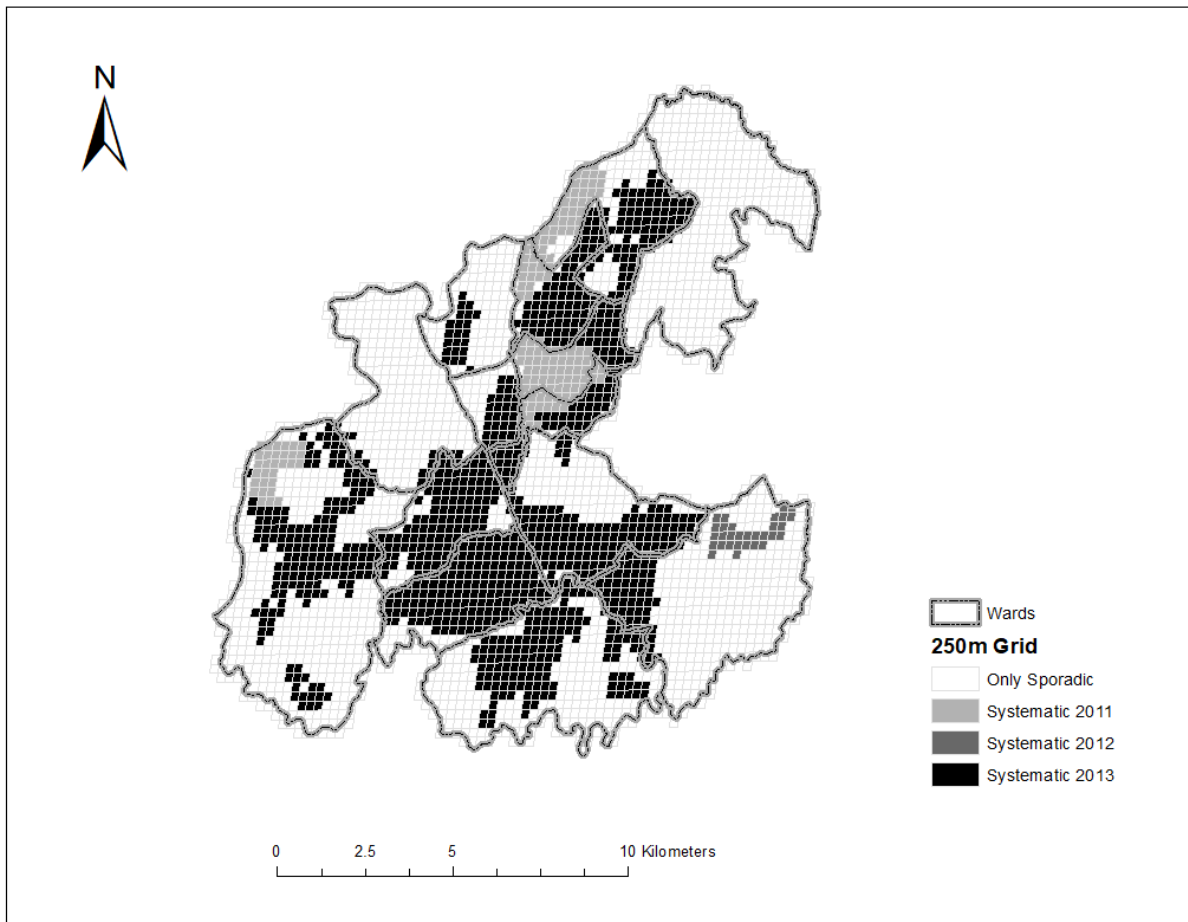
Note: All regressions are at for 250m x 250m blocks for 2000–19. The dependent variable is the number of registered sales or mortgages, overall and with female co-owner at block level. Dependent variables (systematically and sporadically registered parcels) refer to the lagged cumulative number of parcels systematically or sporadically registered and the latter instrumented by its two-period lagged value as discussed in the text. Population density, share of built-up area of the block, block and year fixed effects, and a constant are included throughout; the coefficients are not reported. The number of observations is 30,280 (1,514 blocks annually for 20 years) throughout. Mean and standard deviation for the dependent variable are for the pre-program period. Elasticities are calculated at mean values. Standard errors are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.010.

**Table 6: Longer-term Effects on Number of Parcels Registered Initially with One or More Female Co-owners**

	Naïve Regression	With Policy Reforms	
		2006 only	2006 and 2011
Sporadically reg. parcels # post-2006 & pre-2011, ( $\gamma_0$ )			0.100 (0.063)
Sporadically reg. parcels ( $\gamma_1$ )	0.490*** (0.064)	0.288*** (0.027)	0.283*** (0.026)
Sporadically reg. parcels # post-2011, ( $\gamma_2$ )			0.232*** (0.080)
Systematically reg. parcels ( $\gamma_3$ )	0.865*** (0.004)	0.866*** (0.004)	0.866*** (0.004)
Sporadically reg. parcels # post-2006 ( $\gamma_4$ )		0.215*** (0.070)	
R-squared	0.972	0.973	0.973

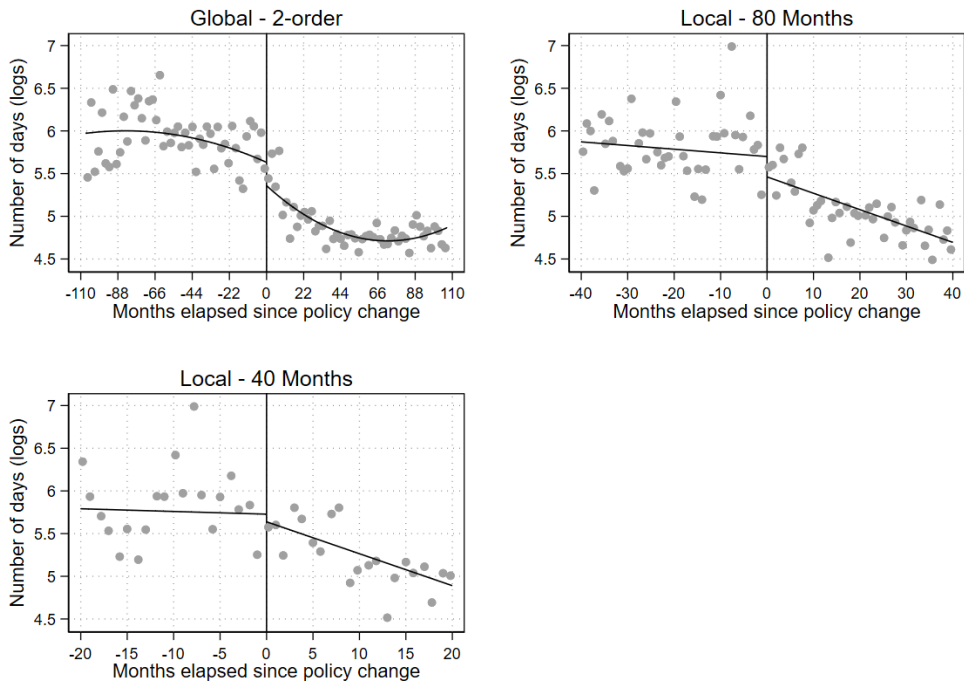
*Note:* All regressions are for 30,280 (1,514 blocks over 20 years) 250m x 250m blocks for 2000–19. The dependent variable is the number of parcels registered for the first time with a female co-owner in the relevant year. Population density, share of built-up area, block and year fixed effects, and a constant are included throughout with coefficients not reported. Standard errors are in parentheses. Standard errors are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.010.

Figure 1: Location of Blocks for Sporadic and Systematic Registration



**Figure2: Discontinuity of Mean Monthly Time Required for Registering Sales or Mortgages**

**Panel A: Land sales**



**Panel B: Mortgages**

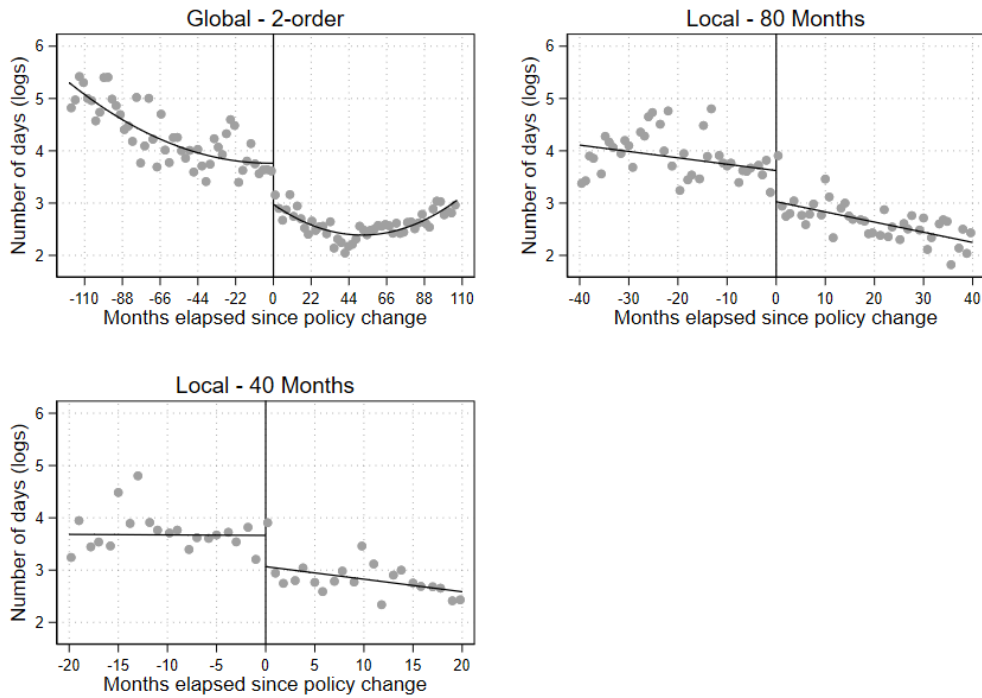
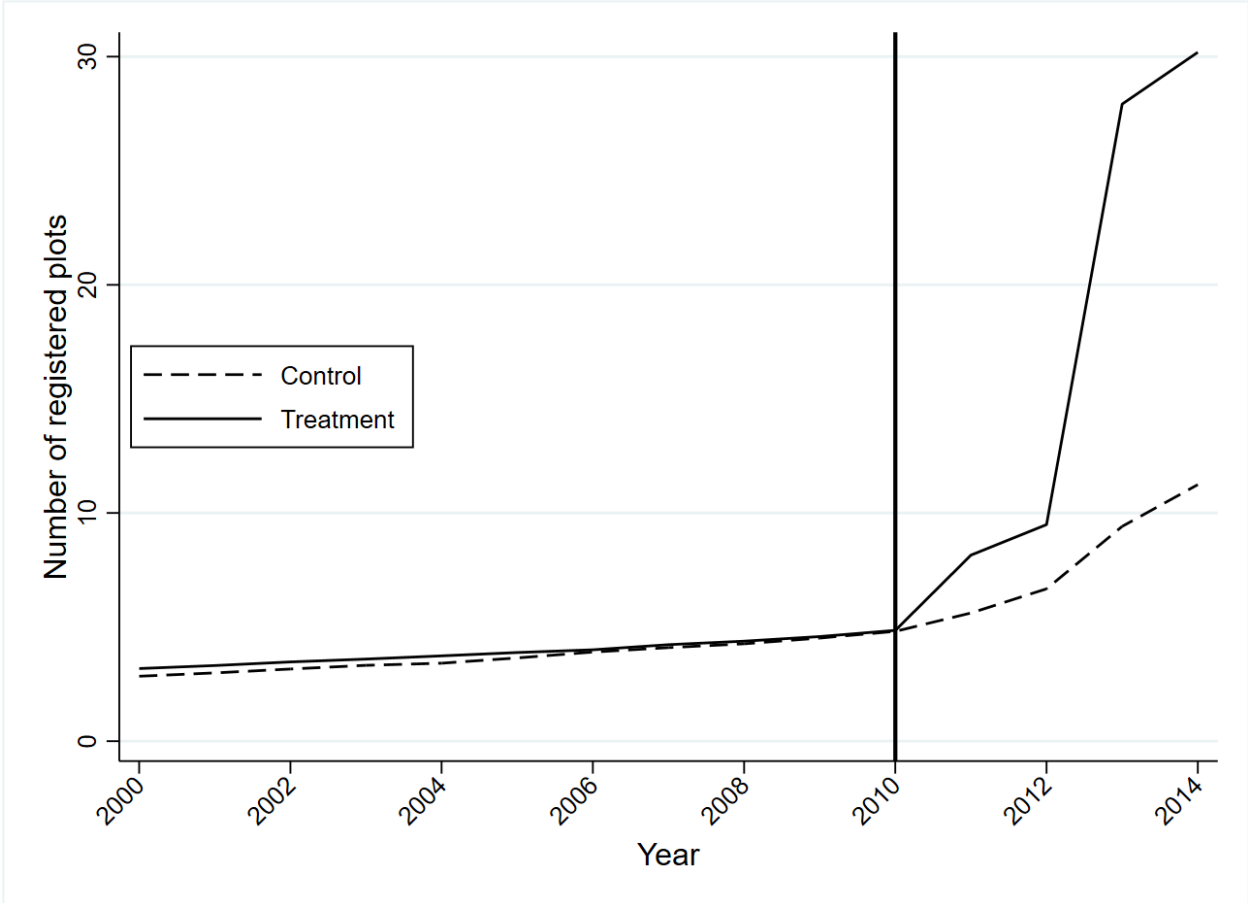




Figure 3: Initial Registrations by Year, Treatment and Control, 2000–14



Note: Sample is matched. The F-statistic for the test of the hypothesis of no parallel trends pre-reform is 1.80 with a p value of 0.180.

## Appendix: Supplementary Tables and Figures

**Table A1: Descriptive Statistics Based on Administrative Data, 1981–2019**

	Total	1981–99	2000–10	2011–14	2015–19
<b>Initial registration</b>					
No. of leases issued	58,582	6,641	5,739	38,295	7,907
per year	1,502	350	522	9,574	1,581
Plot area (m <sup>2</sup> )	1,060	1,494	993	975	1,173
Sporadic	24,160	6,641	5,739	5,217	6,563
Systematic	34,422			33,078	1,344
Sporadic/year	619	350	522	1,304	1,313
Systematic/year	883			8,270	269
<i>Approach and land use type</i>					
Sporadic registration	0.412	1.000	1.000	0.136	0.830
Systematic registration	0.588	0.000	0.000	0.864	0.170
Residential land	0.892	0.853	0.926	0.888	0.921
Commercial land	0.046	0.115	0.054	0.035	0.034
Agricultural land	0.002	0.001	0.001	0.001	0.013
Other Use	0.059	0.030	0.019	0.076	0.032
<i>Subject of registration</i>					
Ownership male/female joint	0.399	0.002	0.088	0.522	0.365
of which systematic reg.	0.777	0.000	0.000	0.875	0.235
Ownership by females only	0.303	0.215	0.291	0.324	0.282
of which systematic reg.	0.633	0.000	0.000	0.874	0.174
Ownership by males only	0.251	0.709	0.577	0.140	0.170
of which systematic reg.	0.322	0.000	0.000	0.847	0.151
Ownership by companies or others	0.046	0.074	0.044	0.014	0.182
of which systematic reg.	0.101	0.000	0.000	0.362	0.051
<b>Transfers</b>					
No. of transfers	8,204	972	1,054	1,823	4,355
per year	210	51	96	456	871
Transferred by male owners	0.384	0.614	0.663	0.341	0.283
Transferred by female owners	0.309	0.180	0.279	0.325	0.338
Transferred by males/female jointly	0.263	0.002	0.010	0.314	0.361
Transferred by companies	0.044	0.204	0.047	0.019	0.018
Days to get consent	93	190	382	63	28
Days to complete registration	120	160	146	121	104
Days since approval of consent	77	85	74	73	77
Purchase price (US\$) <sup>a</sup>	11,394	13,335	15,388	12,231	9,701
Plot area (m <sup>2</sup> )	1,005	1,204	1,385	920	907
Price (US\$/ m <sup>2</sup> )	15	15	17	15	14
<b>Mortgages</b>					
No. of registered mortgages	6,542	2,227	1,030	1,287	1,998
per year	168	117	94	322	400
Mortgage value (US \$)	51,573	37,372	61,504	71,221	49,620
Plots reg. to male owners	0.529	0.732	0.680	0.429	0.296
mortgage value (US \$)	42,626	31,279	44,819	65,802	49,338
Plots reg. to female owners	0.209	0.175	0.209	0.230	0.234
mortgage value (US \$)	37,447	21,333	40,828	51,075	40,520
Plots reg. by males & female jointly	0.183	0.009	0.042	0.262	0.395
mortgage value (US \$)	41,917	19,975	38,449	45,454	41,130
Plots reg. by companies	0.078	0.085	0.070	0.078	0.075
mortgage value (US \$)	175,531	131,375	279,546	250,753	131,479
Days for registration process	69	136	79	22	20
Days to get ministerial consent	93	99	78		

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

<sup>a</sup>Note that because the number of observations for purchase price and price per unit of land differ slightly due to missing plot area, unit prices cannot be computed from the mean values.

**Table A2: Descriptive Statistics at Block Level: Full Sample**

	1990	2000	2010	2015	2019
<b>Panel A: Total</b>					
Parcels reg. sporadically #	1.3	3.0	6.0	9.0	11.0
Area parcels reg. sporadic m <sup>2</sup>	2,223	4,516	6,915	9,730	11,883
Parcels reg. syst. #				16	17
Area reg. syst. m <sup>2</sup>				15,349	15,554
No. reg. sales annually	0.04	0.03	0.06	0.34	0.34
No. reg. mortgages annually	0.10	0.01	0.08	0.19	0.16
Share of built-up reg. area	NA	0.05	0.08	0.24	0.20
Population density indiv/km <sup>2</sup>	954	1,356	1,570	1,722	1,847
<b>Panel B: Treatment (systematic &amp; sporadic)</b>					
Parcels reg. sporadically #	1.2	3.0	5.0	7.0	9.0
Area parcels reg. sporadic m <sup>2</sup>	1,896	4,202	5,854	7,896	9,448
Parcels reg. syst. #				25	25
Area reg. syst. m <sup>2</sup>				23,358	23,637
No. reg. sales annually	0.03	0.02	0.05	0.33	0.30
No. reg. mortgages annually	0.09	0.01	0.04	0.19	0.12
Share of built-up reg. area	NA	0.05	0.08	0.30	0.24
Population density indiv/km <sup>2</sup>	1,042	1,562	1,809	1,984	2,129
<b>Panel C: Control (sporadic only)</b>					
Parcels reg. sporadically #	1.6	3.0	8.0	11.0	14.0
Area parcels reg. sporadic m <sup>2</sup>	2,771	5,041	8,694	12,803	15,965
Parcels reg. syst. #			-	2	2
Area reg. syst. m <sup>2</sup>				1,926	2,009
No. reg. sales annually	0.06	0.04	0.08	0.36	0.40
No. reg. mortgages annually	0.11	0.02	0.14	0.20	0.24
Share of built-up reg. area	NA	0.06	0.09	0.14	0.12
Population density indiv/km <sup>2</sup>	794	1,011	1,170	1,282	1,375

Sources: LAA land administration database from 2011 and digitization of paper records, 1981–2011 for parcels, transfers, and mortgages registered. WSFE for built area; and Gridded Population of the World v4 (CIESIN) for population density.

**Table A3: Registration Modality and Parcel Characteristics**

	<b>Systematic Total</b>	<b>Sporadic Total</b>	<b>1981–2009</b>	<b>Sporadic 2010–14</b>	<b>2015–19</b>
Ln CBD distance (m)	0.137* (0.0733)	-0.216*** (0.0771)	-0.179*** (0.0665)	-0.0896* (0.0512)	0.132** (0.0516)
Ln parcel area (m <sup>2</sup> )	-0.0138*** (0.00297)	0.0251*** (0.00312)	0.0503*** (0.00269)	-0.00183 (0.00208)	-0.0347*** (0.00209)
Mean dep. var.	0.604	0.397	0.209	0.0832	0.104

*Note:* Regression is at parcel level for the 53,432 parcels that passed quality control procedures for inclusion in the cadaster; the dependent variable is a dummy of whether the parcel was first registered by a systematic or a sporadic process. Block fixed effects and constant are included throughout but not reported. Standard errors are in parentheses.

Figure A1: Illustration of Blocks Used to Evaluate Impact

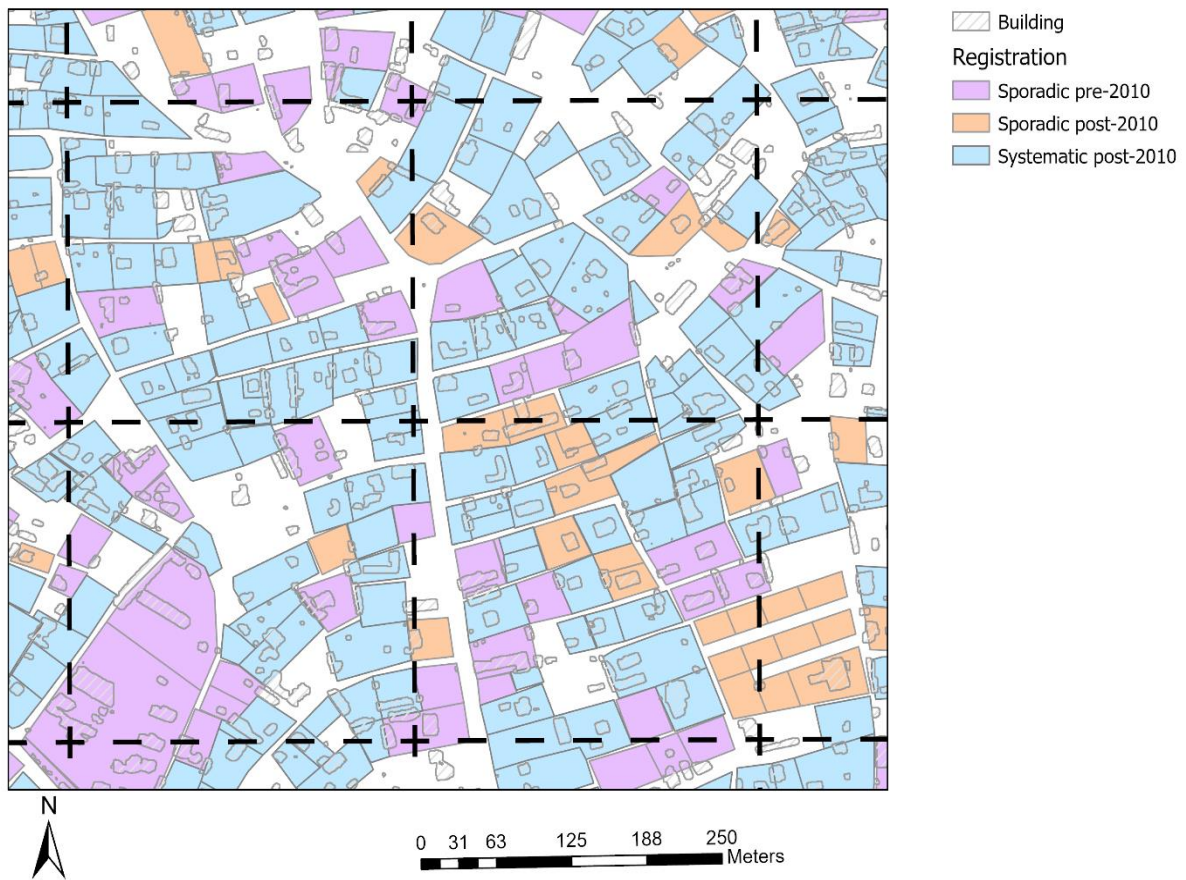
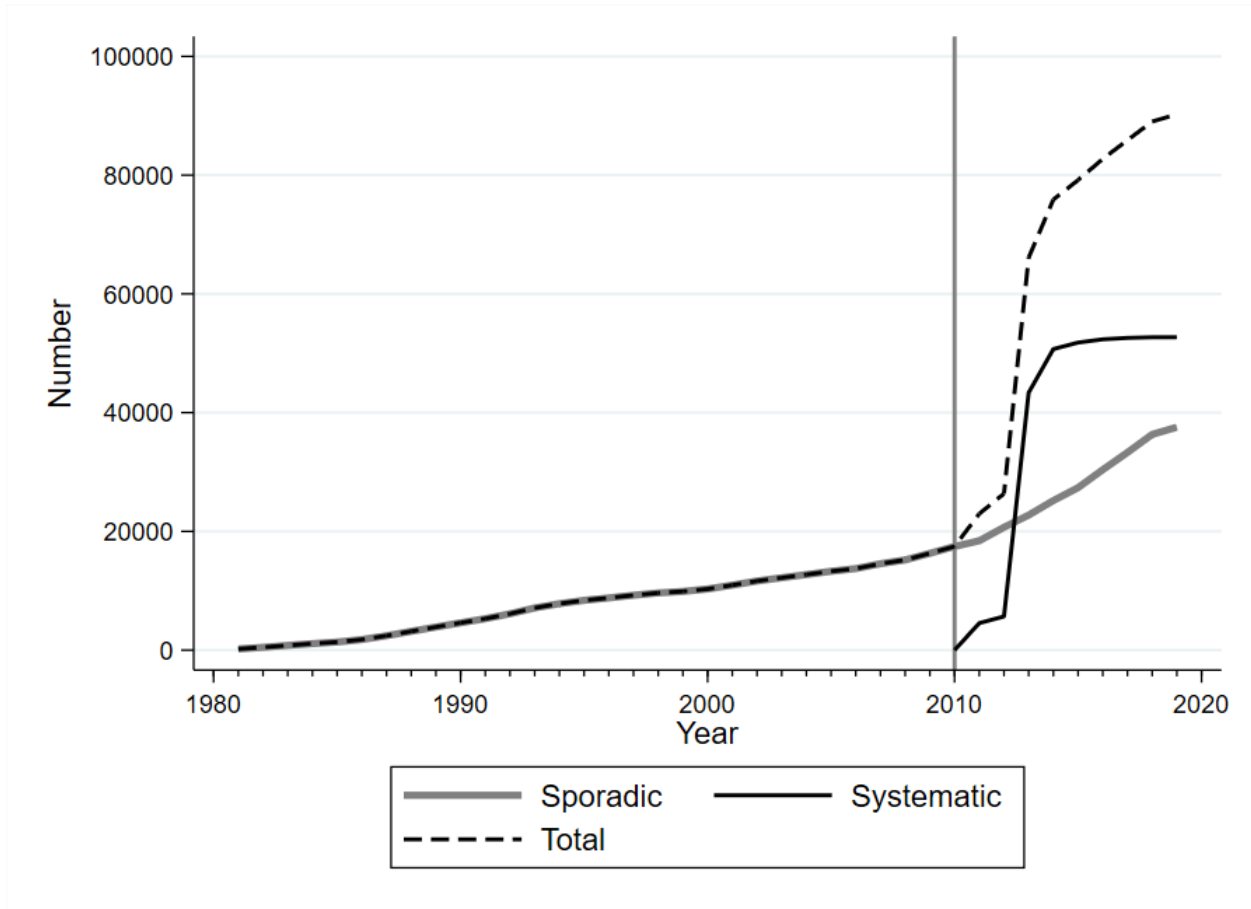


Figure A2: Cumulative Number of Registered Parcels by Year



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