

How Urban Land Titling and Registry Reform Affect Land and Credit Markets

Evidence from Lesotho

Klaus Deininger

Daniel Ayalew Ali



WORLD BANK GROUP

Development Economics

Development Research Group

May 2022

Abstract

Using spatial fixed effects and time-varying controls, this paper draws on complete registry data for 1981–2019, supplemented by satellite imagery, to analyze impacts of urban land titling for some 40,000 grid cells in Lesotho. Beyond confirming the short-term impacts on female co-ownership and investment, previously reported, the paper documents medium-term impacts on land sale and mortgage market activity and women's participation in these markets. Although titling was instrumental in ensuring the

effectiveness of an earlier legal reform that allowed women to be co-owners of land, the credit and land market effects are due not to titling but to changes in policy to reduce the transaction cost of registering land that took effect just before titling started. Downward shifts in the time required to register transactions support this interpretation. The paper concludes by discussing what the evidence implies for design and evaluation of property registration programs.

This paper is a product of the Development Research Group, Development Economics. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at <http://www.worldbank.org/prwp>. The authors may be contacted at kdeininger@worldbank.org and dali1@worldbank.org.

The Policy Research Working Paper Series disseminates the findings of work in progress to encourage the exchange of ideas about development issues. An objective of the series is to get the findings out quickly, even if the presentations are less than fully polished. The papers carry the names of the authors and should be cited accordingly. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

How Urban Land Titling and Registry Reform Affect Land and Credit Markets: Evidence from Lesotho*

Klaus Deininger and Daniel Ayalew Ali

The World Bank, 1818 H Street NW, Washington, DC

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

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

* We thank the Lesotho Land Administration Authority (LAA), especially Nts'ebo Putsoa, for supporting the project, administrative data access and insightful discussion and Innocent Pangapanga-Phiri for support in leading and supervising the administrative data collection. Special thanks are due to Thea Hilhorst and Jennifer Lisher for the overall support of the project, valuable comments and discussions. We also would like to thank the World Bank Lesotho country team particularly Yoichiro Ishihara and Jana El-Horr for useful comments and suggestions. Funding support from the US Millennium Challenge Corporation (MCC) is gratefully acknowledged. The views presented in this paper are those of the authors and do not represent those of the World Bank or its member countries.

1. Introduction

Secure and transferable land rights have long been considered a precondition for economic development: They incentivize land-related investment; facilitate operation of land rental and sales markets so that land can be used most effectively to support structural transformation; and allow land to be used as collateral for credit, deepening financial markets: secure and registered property rights that stimulate investment in urban density are essential to harness benefits from urban agglomeration and capture the associated increases in property values. Having property rights documented also makes it easier for vulnerable groups like women to enforce their rights against competing claims, amplifying their voice and risk-taking and reducing the need for wasteful spending on disputes.

Although land titling—or more specifically the subsidized systematic first registration of rights in land—has long been advocated to increase credit access by transforming dead assets into ‘live capital’ (de Soto 2000), evidence of such effects is still minimal. One reason, we explain, may be that for many countries property registries have been a low priority. To understand why, note that properly designed¹ titling programs will increase registry coverage only. Insofar as this clarifies assignment (e.g., by helping to resolve current disputes) and simplifies enforcement of rights (e.g., by providing owners with a written document that can be presented in court), titling is likely to increase tenure security and land-related investment. If done in a gender-sensitive way and under a legal framework of gender equality, it will also empower women socially.

Titling will, however, stimulate the operation of land markets and allow land to be used as collateral for credit only if a registry provides authoritative information on property rights freely and allows transfers or encumbrances of rights to be registered or pledged land to be foreclosed in case of default without incurring prohibitive transaction costs. In situations where unaffordable registration caused limited coverage with registered rights (and thus demand for titling) in the first place, land titling will be sustainable or improve land and credit market operations only if preceded or accompanied by legal, regulatory, and institutional reforms (‘policy’ reforms) to reduce such costs. The volume of registered land transactions can indicate whether such reforms are effective and whether titling programs achieved their social and economic objectives sustainably.

To illustrate how these principles can inform empirical analysis, we use the Lesotho Land Administration Reform Project (LARP) in Maseru in 2010–13, supported by the Millennium Challenge Corporation. The goal of LARP was to improve registry operation through policy reforms and increase coverage of registered land rights through systematic first registration of land rights. We use registry data from 1981–2019,

¹ *Proper design* means that accepted rules on how to prioritize between competing claims are in place, and that potential claimants are sufficiently aware that they can protect themselves from abuse by powerful or well-connected individuals.

digitized for this purpose, divide the city into 1,932 blocks each 250m x 250m, and for regression analysis focus on close to 40,000 block-year-level observations in 2000-2019.

Information on the stock of parcels registered, the flow of those registered for the first time either through a project-supported systematic process or sporadically based on owner applications, and the number of registered land sales and mortgages are aggregated by block to obtain a panel of annual data for 2000 through 2019. Registry data is complemented by annual spatial data on built area and population density as control variables. The amount of built area registered is used as an additional outcome variable to proxy for land-attached investment.

We use these data to estimate short- and medium-term effects of LARP using two independent panel econometric specifications with block fixed effects and time-varying controls. The program's roll-out over time and space is used to identify short-term effects. To separate medium-term impacts of policy change from those of initial registration, we rely on the fact that policies affected registry operation and sporadic registration of land rights before, during, and after LARP-supported initial registration. We thus use the stock of registered rights, by modality of registration and interacted as needed with an indicator for the policy regime (pre- or post-reform), to estimate the impacts of policy changes and the contribution of systematic initial registration to outcome variables of interest. Three results stand out:

First, in line with the literature, we find that LARP's main short-term impacts derive from systematic first registration of rights. A time-varying indicator variable for blocks with active first registration points to significant program-induced increases in the number of registered parcels, female co-ownership of land, built registered area, and registered sales—but not mortgages. Interacting a policy reform indicator with initial values suggests that policy changes had significant impact throughout; however, estimated marginal effects are smaller than those of the systematic registration program.

Second, beyond the immediate impact of systematic first registration of rights, policy reform had significant impact on the number of registered mortgages and land sales. These credit and land market activation effects are particularly pronounced for female co-owners. Evidence of policy-induced transaction cost reductions supports the plausibility of this result that provides a significant addition to the literature. Our ability to document a policy-induced drop in the cost of registering land transactions supports this argument by providing a plausible channel for such impacts to materialize.

Third, the time gap between enactment of different gender-related laws and regulations allows us to test whether greater female access to land rights can be attributed to the landmark 2006 Legal Capacity of Married Persons Act (LCMPA), which, for the first time in Lesotho's history, allows married women to hold and register land rights in their names and provides supportive regulations. We find that the LCMPA

alone had no impact on female co-ownership of land until the 2010 Land Act made co-ownership the default option for common-law marriages, an awareness campaign had been launched, and procedures for systematic initial registration were designed to put such rights into practice.

This study contributes to several research strands: First, although there is ample evidence of land and credit linkages at the macro level, to the best of our knowledge ours is the first study to document the credit- and land-market activation effects of land titling at the parcel level. Historically, the type of land rights and land ownership structure affected the evolution of cooperative credit institutions (Suesse & Wolf 2020), financial system operation (Rajan & Ramcharan 2011), real estate cycles (Rajan & Ramcharan 2015), and long-term development (Devijlder & Schoors 2020).² On U.S. Indian reservations, credit access has been identified as the main channel that supported greater development on parcels with fully, compared to only partially transferable property rights (Dippel et al. 2020). Establishment of property registries and adoption of secure transaction laws by Indian bands has also been linked to greater economic development (Dippel et al. 2021). In India, registry computerization that improved registry access has been shown to expand urban mortgage lending at the level of registry offices (Deininger & Goyal 2012).

We add to such macro-studies by using administrative data to provide micro evidence on the impacts of systematic first registration separately from those of policy and institutional reform. While each of these interventions has significant impact, differences in target variables and the time profile with which effects materialize make them highly complementary: while systematic first registration expands registry coverage, female co-ownership, and investment in the short term, the thrust of policy reform is to translate greater coverage into market activity and credit access, especially for female co-owners. and, in a hypothetical scenario without policy reform, systematic initial registration would actually reduce the number of registered transactions.

A second strand of literature relate to the design and targeting of titling programs. Studies suggest that demand for an urban title is price-elastic (Balan et al. 2020) and informal urban dwellers prefer regular small payments or saving schemes to large up-front payments (Ali et al. 2016). Whether they are due to inefficient registry operation (Nkurunziza 2015), fees far above the cost of service delivery (Manara & Regan 2021), or interest group pressure (Gutierrez & Molina 2020), high fees for titling or other barriers to registry access are likely to reduce demand for titles; coverage of registered rights; and the public sector's ability to provide local public goods by planning land use and capturing continued and historically large increases in urban land values (Knoll et al. 2017).

² Chaudhary and Swamy (2020) describe the impacts of foreclosure policies and interest rate ceilings in India, as Zegarra (2017) does for Peru.

Our finding that even modest reductions in the cost of accessing the registry increase mortgage market activity and women's credit access suggests that the extent to which registration costs determine financial sector deepening and female empowerment may have been underappreciated, at least in urban contexts. It implies that, without a well-functioning property registry that has reasonably complete urban coverage, the benefits from expanding into informal and rural areas that have traditionally been the focus of land titling may be few and difficult to sustain (Galiani & Schargrotsky 2016). Policy and institutional reforms to reduce the cost of gender-sensitive urban property registration may thus not only constitute a precondition for expanding registry coverage into peri-urban and rural areas but also generate significant direct benefits. If improving how land and financial markets function can translate into higher property values, part of which can be captured through property taxation to finance local public goods, in principle such reforms could be self-sustaining and strengthen accountability and the social contract (Weigel 2020).

A final area on which we provide insight relates to the contribution of land rights to women's social and economic empowerment. Registering land in women's names, as sole or joint owners, has proved to increase their ownership of assets and enhance their bargaining power, thus affecting consumption choices, investment, productivity, and participation in local land or labor markets in countries as diverse as China (Wang 2014), Vietnam (Menon et al. 2017), India (Agarwal 1994), Ethiopia (Deininger et al. 2008), Rwanda (Ali et al. 2014), Malawi (Deininger et al. 2021), and Peru (Field 2007). Though analyses of the economic impacts of U.S. legal reforms in the 1850s that allowed women to own property, such as Hazan et al. (2019), have found far-reaching long term impacts, they do not discuss the mechanisms underpinning such changes.

In the context examined here, similarly far-reaching legal provisions had no independent impact in the short term but did increase female co-ownership of land once the necessary legal and procedural changes had taken effect. Policy reforms that made it easier to access the registry then helped women translate co-ownership rights into participation in land and financial markets. Our findings complement the studies of family law reform (Deininger et al. 2013; Harari 2019) and are consistent with evidence that, wherever awareness and understanding of legal changes may be limited or countervailing social norms predominate, the short-term impact of sweeping reforms may, without more detailed guidance, be limited (Linkow 2019) or even negative (Anderson & Genicot 2015). Moreover, even if legal mandates have provided them with registered rights, high registry access fees may prevent female co-owners from translating co-ownership rights into economic benefits.

In what follows, section 2 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 3 discusses data and descriptive evidence and provides equations for

estimating short- and medium-term effects. Results for the different outcome variables, including those related to gender, are discussed in section 4, and section 5 concludes and offers implications for policy and research.

2. Background and Program Characteristics

We review the conceptual justification for securing land rights in terms of their impacts on investment, female empowerment, and the operations of land and financial markets. Noting that the literature provides ample evidence of investment and gender effects but hardly any examples of credit market impact, we discuss possible substantive and methodological reasons for this difference and briefly characterize related activity by the Lesotho LARP, the program we are evaluating.

2.1 Conceptual Issues and Links to the Literature

Secure and transferable land rights have long been considered a precondition for economic development because they incentivize land-related investment; facilitate operation of land rental and sales markets to bring land to its most effective support for structural transformation; and allow land to be used as collateral for credit. Secure land rights enable rights holders to benefit from investment and empower women by making it easier to defend land rights against challenges. In Benin, randomized titling increased investment, especially in female-managed agricultural plots (Goldstein et al. 2018) and reduced deforestation (Wren-Lewis et al. 2020). Greater tenure security increased investment in Ethiopia (Deininger & Jin 2006), Cameroon (Kazianga & Masters 2006), and in a large number of other countries (Fenske 2011).³ Documentation of indigenous rights in Brazil during the 1982–2016 period found a major drop in deforestation (Baragwanath & Bayi 2020), as was also found for Colombia (Vélez et al. 2020) and in the short term for Peru (Blackman et al. 2017). Registering rights increased investment even if, as in Canada, the rights were not fully transferable (Aragón & Kessler 2020).

Having land rights documented enables members of weaker groups, like women, to ward off challenges, which reduces the risk of land loss and the need to spend resources on dispute resolution. Well-designed interventions can vastly increase women’s land ownership, as in Rwanda, where after reform in 2011–13, more than 85 percent of women had land rights documented either individually or jointly—a level that has since been largely maintained (Ali et al. 2021). Registering female rights in land has reduced the need for female guarding labor (Field 2007); increased investment, especially by female-headed households (Ali et al. 2014); enhanced women’s participation in markets for land (Holden et al. 2011) and labor (Newman et al. 2015); and improved their status, income, and investment in the welfare of future generations (Menon

³ If rights are uncertain and cannot be secured otherwise, visible investments may be undertaken to claim rights as in Burkina Faso (Brasselle et al. 2002) or Côte d'Ivoire (Djezou 2016) where such preventive investment is widespread among migrants (Bros et al. 2019).

et al. 2017). Positive impacts on women emerged even when land sales were prohibited, as in Ethiopia (Muchomba 2017; Melesse et al. 2018) and Madagascar (Widman 2014); and the benefits have been reinforced by family law reforms (Kumar & Quisumbing 2015).

Although the expectation that systematic land registration facilitates access to credit has been a standard justification for titling efforts in the developing world, there is little support for this hypothesis; a literature review by Lawry et al. (2016) found no evidence of credit effects in rural areas. Studies have been unable to find credit effects even where they were expected or where titling had been shown to affect investment and attitudes, as in Benin (Fabbri & Dari-Mattiacci 2021); or where patterns of labor allocation had changed, as in Ghana (Agyei-Holmes et al. 2020) and Peru (Field & Torero 2006); or where, as in Argentina, registration affected investment (Galiani & Schargrotsky 2004), values (Di Tella et al. 2007), and labor market outcomes (Galiani & Schargrotsky 2010).

The failure to find credit effects can partly be attributed to the fact that many titling programs were set up for informal settlements or rural rather than urban areas where land is less marketable. Moreover, use of land as collateral requires that registries be established (Dippel et al. 2021) and charge reasonable fees (Gutierrez & Molina 2020). As this is often not the case where titling is needed, regulatory reforms are often combined with titling programs to improve the quality and efficiency of registry services.⁴ However, the impact of such reforms, which may take time to materialize, has rarely been thoroughly analyzed, partly because doing so without administrative data is almost impossible.

2.2 Land Tenure Interventions in Lesotho

Lesotho is a landlocked kingdom entirely surrounded by South Africa; its area is about 30,000 km². It has a population of 2.1 million and per capita GDP of about US\$800, and in the next decade urbanization is expected to rise to 30 percent. Land in rural areas is held under customary tenure, but there is a registry for urban land. Women had very few rights until, in a reform similar in scope to the demise of coverture in the US in the 1850s (Geddes et al. 2012), the 2006 LCMPA removed the minority status of married women and the powers a husband had previously had over the person and the property of his wife.⁵

Building on the LCMPA, the US\$20.5 million LARP focused on three areas. First, it supported drafting and adoption of the 2010 Land Act and the 2010 Land Administration Authority (LAA) Act and associated regulations. The Land Act aimed to (1) reduce barriers to registering land rights by simplifying procedures, as by eliminating the need for Ministerial consent to registering land transactions; (2) regulate systematic

⁴ Such reforms normally support adoption of modern digital technology, streamlined workflows, and data interoperability.

⁵ 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.

land titling; and (3) reinforce women's rights through a presumption that any property acquired in marriage is jointly owned by both spouses.

The LAA Act established the LAA as a one-stop shop for land registration that would provide reliable information on land ownership; expedite registration and transfer of leases; reduce registration fees; and deliver services efficiently. A fully digital register and cadaster, designed to be fully interoperable with systems held by other agencies, such as municipalities' building registers, was established. By 2013, 18 regulations to regulate systematic land titling, planning, surveying, registry operation, and dispute resolution had been drafted and most had been adopted.

A program of systematic initial registration was designed to ensure the security of land tenure by issuing legal documents to 55,000 property owners in largely informal settlements in Maseru, a city with 300,000 inhabitants. The program was piloted in 2011 with the intent of covering all intervention areas in 2012, but contractual issues delayed the roll-out to 2013.

3. Data, Descriptive Evidence, and Approach

To assess how policy reform and systematic initial registration of land rights in 2011–13 affected parcels registered, women's co-ownership, the functioning of land and credit markets, and investment linked to land, we divided the area of interest into almost 2,000 250m x 250m blocks that serve as the basic units of analysis. Digitized administrative data for registered transactions and gridded information on built area was then aggregated to block level. Descriptive data show that 2010 policy reforms coincide with a downward shift in transaction costs; an uptick in registered sales and mortgage transactions; and a massive increase in female co-ownership. We discuss econometric strategies to examine short- and medium-term impacts and the differing contribution of 2006 and 2010 legal reforms on gender outcomes.

3.1 Data

To obtain a complete set of textual and spatial data before and after the intervention, we digitized paper records from registry books for all transactions registered before LAA migrated to a functional digital platform. This provides data on the number of parcels for which rights were registered for the first time, the gender of registered rights holders, and registered sales and mortgage transactions, with attendant details for all of Maseru from 1981 through 2019.

While these data lack information on parcel attributes or the socio-economic characteristics of owners other than gender, they cover the universe of registered transactions, thus avoiding the potentially large errors, sampling and non-sampling, that would be likely in household surveys because land transfers are rare and information on land rights may not be shared with all household members. The data also include the actual

start and end dates for more than 98 percent of registered sales and mortgages based on actual transactions rather than expert opinions about “typical” transactions.

To create spatial units as a basis for impact evaluation, we divide the area of Maseru Municipal Corporation (MMC) into a grid of 250m x 250m blocks and aggregate spatial variables for each block. Figure A1 in the Appendix⁶ illustrates the principle: block boundaries are represented by a dashed line 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). Spaces the rights to which are not registered in the name of a private individual or legal entity remain white and 2016 building footprints, derived from high-resolution satellite imagery using machine learning, are displayed for illustrative purposes.⁷

Figure 1 maps the MMC area: the 722 blocks not covered by systematic initial registration are colored white and the 1,210 blocks covered by systematic first registration are colored based on whether they entered LARP through the 2011 pilot (light grey; 150 blocks); the initial roll-out in 2012 (dark grey; 29 blocks) or the full roll-out in 2013 (black; 1,031 blocks). Relevant outcomes from the registry (e.g., number of registered parcels, land sales and mortgage transactions) are then aggregated to the block level on an annual basis so that blocks serve as the basis of econometric analysis.

We complement that data with gridded information on built area and population on each block to control for time-varying attributes. 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 10 m resolution made available by the WSFE team.⁸ In the absence of time series 3D data on built volume, we also use this variable as a proxy for land-related investment in durable structures by overlaying it with the registered parcel layer to provide an estimate of built area on registered and non-registered parcels for each block. Population data for 2000–19 is from the Center for International Earth Science Information Network (CIESIN) gridded population of the world (version 4) data set at a 1 km spatial and 5-year temporal resolution. It is used similarly throughout to generate annual values of block population density to serve as a control.

3.2 Descriptive Statistics

Table 1 summarizes transaction-level variables for the 39-year period (1981–2019) for which data is available (column 1) and the 1981–2009 pre-reform period split into pre-and post-2000 (columns 2 and 3);

⁶ Figures and tables whose number is preceded by an A are all in the Appendix.

⁷ Parcels that cross block boundaries are assigned to the block containing the parcel centroid.

⁸ 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. Results in Tables 3 and 4 are based on pre-2015 values and thus robust. Differences in resolution and the methodology used to compute built area between the 1985–2015 series and the 2019 values reduce the consistency of the time series so that results in table 5 should be considered only illustrative.

the 2010–14 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 Table A1.

Parcels registered per year surged from 485 in 2000–09 to 7,838 in 2010–14 during the LARP before they levelled off at 1,581 in 2015–19. While all registrations were sporadic before 2010, about 85 percent of initial registrations in 2011–13 used 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. 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 93 sales and 97 mortgages in 2000–09 to 871 and 400 after program completion in 2015–19. The share of newly registered parcels that had at least one female co-owner went up from 23 percent in 1981–99 and 37 percent in 2000–09 to about 85 percent in 2010–14 and 80 percent in 2015–19. Increases were less pronounced in the share of built-up area overall (from 46.0 to 49.8 percent) and on registered land (from 6.6 to 22.3 percent) between 2000–09 and 2015–19.

To check if the 2010 regulatory and institutional reforms affected the transaction cost of registering sales or mortgages given the absence of information on fee receipts, we rely on data on the number of days between lodging an application for registering a sale or mortgage and actual registration of each transaction. Results from non-parametric regressions using this variable for the 20-year period centered on 2010 are in Figure 2. In both cases, a discrete break in the time needed for processing that coincides with legal and regulatory reforms in 2010 is visible. Impacts are most pronounced for registering mortgages that do not require survey work; eliminating the need for Ministerial consent, which had often taken 80–100 days, had a relatively large effect. Sales, which may require surveys, still take about 4 months—it appears that there is scope to improve processing efficiency.

Non-parametric regressions for the number of registered transfers and mortgages at block level (Figure 3) also reveal both an uptick in 2010 and a change in slope that is particularly marked for transfers. The share of registered parcels with at least one female co-owner (see Figure 4) changed only slightly after passage of the LCMPA in 2006 compared to the jump in 2010, which suggests that policy changes for land administration may have been needed to ensure the LCMPA had actual impact—a notion we formally test below.

A central assumption for identification is that control and treatment blocks are comparable in relevant characteristics, but Table A2 suggests this was not the case. In line with LARP 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 replacement⁹ to trim 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—that we use to estimate short- and long-term effects of the intervention.

Table 2 shows block-level information on pre-program values for outcome and control variables for the entire sample (col. 2) and the matched sample (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 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.

3.3 Econometric Approach

The intervention we are evaluating, LARP, had two components. Systematic initial registration was limited to certain blocks, phased in over 2011–13, and expected to have impact in the short term. Policy reforms became effective when the 2010 Land and LAA Acts came into force and affected the entire city, though the impact may have materialized with some lag. Sporadic on-demand registration of parcels and sale and mortgage registrations occurred before, during, and after this period.

Indexing blocks by i and time (2000–14) by t , the basic equation for estimating short-term effects of the systematic land registration program is

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

where Y_{it} is the outcome variable of interest (i.e., number of registered transfers or mortgages or of newly registered plots with or without at least one female owner) of block i at year t ; SyR_{it} is a zero-one indicator

⁹ 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.

denoting whether or not in t , block i had entered the systematic registration program; X_{it} are time-varying covariates including population, built-up area and the interaction between the two; α_i are block fixed effects; λ_t are time fixed effects; ε_{it} is an error term; and β_1 , the coefficient of interest, is the difference in differences (DID) estimate of the short-term impact of systematic registration under LARP.

To interpret the results from this regression, the fact that legal, regulatory, and institutional changes affected all parcels in the city implies that their impact, if any, will be subsumed by the λ_s . If policy change has a positive effect, β_1 will thus provide a lower bound of the full impact of LARP, and if it has a negative effect, it will provide an upper bound. Although policy effects cannot be identified separately from the time dummies, we can check for potential policy effects by including interactions between the policy dummy and the pre-reform stock of registered parcels (\bar{R}_{ib}) and the pre-reform (baseline) values of the outcome variable in question (\bar{Y}_{ib}). Significant coefficients for the former would imply that policy reform made it possible to capitalize on parcels already registered, and for the latter would point to policy reform facilitating convergence or divergence independent of efforts at systematic registration.

Formally, letting P be an indicator for the post-2010 period when the Land Act became effective and LAA was established, we estimate

$$Y_{it} = \alpha_i + \beta_1 SyR_{it} + \beta_2 P \times \bar{R}_{ib} + \beta_3 P \times \bar{Y}_{ib} + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (1)$$

where, in addition to the variables defined above, \bar{R}_{ib} is the stock of registered parcels pre-reform, and \bar{Y}_{ib} denotes initial conditions for Y_{it} , i.e., either the stock of registered parcels with a female co-owner, the share of built-up area in 2009, or the 5-year average of registered transfers or mortgages in 2005–09. The new coefficients β_2 and β_3 thus indicate whether policy reforms affected outcomes conditional on the pre-reform stock of registered parcels or on initial conditions. If yes, more detailed analysis to identify the impacts of policy reforms over a longer period may be warranted.

Investigating long-term effects using the 2000–19 period adds two dimensions to this analysis.

In the first place, it allows us to test whether the impact of systematic registrations supported by LARP is different from that of sporadic registrations conducted independently before, after, or during the LARP roll-out. To do so, we use the lagged stock of parcels registered by systematic and by sporadic processes where the lagged stock of sporadically registered parcels is instrumented by its second lag to allay concerns about endogeneity. As policy changes took effect in 2010, we assume that the effect of parcels registered sporadically after 2010 equals that of sporadic parcels before 2010 plus a policy shifter. By comparison, systematic initial registration under LARP only happened post-reform.

Formally, let r_{it}^p and r_{it}^y be the flow of parcels in block i that enter the registry in year t through both sporadic and systematic processes, so that $r_{it} = r_{it}^p + r_{it}^y$ is the total flow of parcels registered in t . Letting R_{it} as well as R_{it}^p , R_{it}^y denote the stock of total, sporadic, and systematically registered parcels in t and using lagged values for the stock of registered parcels allows us to estimate

$$Y_{it} = \alpha_i + \theta_1 R_{it-1}^p + \theta_2 R_{it-1}^p \times P + \theta_3 R_{it-1}^y + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (2)$$

where Y_{it} is either the share of built area, as a proxy for investment, or the number of registered transfers or mortgages; P is a post-2010 dummy; X_{it} are time-varying covariates; α_i are block fixed effects; λ_t are time fixed effects; and ε_{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 R_{it-1}^p with its second lag R_{it-2}^p .

To interpret coefficients of interest, note that all systematic registrations happened after 2010. On the plausible assumption that the 2010 policy changes equally affect sporadically and systematically registered parcels, θ_3 is the estimate of the impact of systematic registration plus policy reform; and θ_1 is the estimated impact of sporadic registration without policy reform. With $\theta_1 + \theta_2$ as the estimated impact of sporadic registration post-2010 and θ_2 as the estimated effect of policy reforms, the hypothetical effect of systematic initial registration without policy changes is thus given by $\theta_3 - \theta_2$.

The second opportunity is that the data for the entire period enables us to test the extent to which changes in the distribution of registered rights by gender can be attributed to legal reforms supported by the 2006 LCMPA or the 2010 Land and LAA Acts. In this context, it will also be possible to assess the contribution of policy relative to systematic initial registration in any changes in women's access to registered land rights. To assess whether changes in the gender composition of ownership rights in registered parcels can be attributed to the LCMPA alone or the LCMPA plus policy changes in land registration, we let G be an indicator that the LCMPA in effect is on its own (i.e., $G=1$ for $2006 \leq t < 2010$). Recalling that $r_{it} = r_{it}^p + r_{it}^y$ —the flow of parcels registered for i in t is the sum of sporadically and systematically registered parcels—and letting r_{it}^M denote the number of parcels with only male owners newly registered in t , so that $r_{it}^F = r_{it} - r_{it}^M$ is the number of parcels registered in the same period that had at least one female co-owner and defining Y_{it} as $r_{it}^F - r_{it}^M$, we estimate

$$Y_{it} = \alpha_i + \gamma_1 r_{it}^p + \gamma_0 r_{it}^p \times G + \gamma_2 r_{it}^p \times P + \gamma_3 r_{it}^y + \delta X_{it} + \lambda_t + \varepsilon_{it} \quad (3)$$

where other variables are as in equation (2). Thus, γ_1 , γ_0 , γ_2 and γ_3 denote the estimated impact of (a) sporadic registration without policy reform; (b) the LCMPA alone; (c) the 2006 and 2010 legal and policy reforms together; and (d) systematic registration together with the 2006 and 2010 legal and policy reforms.

Equivalent to equation (2), the hypothetical net effect of systematic registration without policy changes on Y_{it} is $\gamma_3 - \gamma_2$.

4. Estimation Results

The program did have significant impacts, but their time profile and the outcomes affected differed between components: In the short term, for systematic registration, expansion of registry coverage, female access to registered rights, and investment dominate. In the medium term, policy effects on the volume of registered sales and mortgages and on the participation of women as co-owners materialized, complementing short-term impacts by translating social into economic empowerment and ensuring sustainability: Specific regulations and procedural safeguards via systematic registration were necessary to transform the right of married women to own property granted to them by law in 2006 into registered ownership rights.

4.1 Short-term Effects

Table 3 panel A reports the estimated short-term effects, based on equation (1), of systematic initial registration for 2000–14 at block level for the matched sample. We distinguish three groups of dependent variables: (a) the total number of parcels registered for the first time (col. 1) and the number with female co-owners (col. 2); (b) the total number of registered sales (col. 3) and the number with female co-owners (col. 4); (c) the total numbers of mortgages (col. 5) and the numbers with female co-owners (col. 6); and (d) built-up area as a proxy for investment in registered parcels (col. 7). Systematic initial registration led to marked increases in the number of parcels registered both without and with a female co-owner and the number of registered sales and the extent of built-up areas on registered land.

Columns 1 and 2 suggest that, compared to pre-project means of 0.16 of total parcels registered annually and 0.05 with a female co-owner up to 2010, systematic registration triggered increases of 9.2 in the former and 8.0 in the latter. While the number of registered parcels is a program output, gender of registered owners is a variable likely to be affected by project activities. The program appears to have had significant short-term impacts on the total number of registered sales, up by 2.3 times the pre-program mean, and of female co-owners, up by 8.2 times. While the estimated short-term impact of initial registration on mortgage market activity is generally insignificant, it is at least marginally significant for mortgages registered with female co-owners—which seems to have almost tripled from its (small) pre-reform mean: With a coefficient of 0.142—2.6 times the pre-program mean—the estimated impact on registered area built is thus significant and economically meaningful.

While balance in pre-program values of the relevant variables and the associated inability to reject parallel pre-program trends allays fears that these results were spurious, we complement the calculation with a placebo test: we posit pseudo policy changes occurring 6 years before their actual date, i.e., systematic

initial registration in 2005–07 rather than 2011–13. Point estimates for β_1 in these regressions (see Table 3 panel B for all outcome variables) are insignificant throughout, with one exception: the number of parcels registered for the first time with female co-owners, a variable where any impacts of the 2006 LCMPA would be captured by our pseudo-reforms. Yet the point estimate of 0.046 for β_1 in the placebo regression is an order of magnitude smaller than in the main regression (8.006).

In interpreting the regression results, recall that β_1 comprises the combined effect of systematic registration and policy changes in 2010 (2006 and 2010 for gender). Although policy effects cannot be directly identified, reductions in policy-induced transaction costs could work through the stock of parcels already registered (by making it easier to transact) or through sporadic registration of unregistered parcels independent of the program. Adding interactions between a policy dummy with the pre-reform stock of registered parcels and with the value of the dependent variable can provide a first indication of potential policy impacts.

At the bottom of Table 4 we report marginal effects of policy interactions, at the mean value of relevant variables, are reported. Most estimated coefficients on policy interactions are significant, suggesting that even in the short-term policy changes affected outcomes of interest. However, neither the significance nor the magnitude of coefficients estimated for β_1 is fundamentally affected. In line with program logic that viewed systematic registration as a key element, cols. 1 and 2 show that, while there was limited convergence, systematic initial registration drove increases in parcels registered and female co-owners.

Policy changes made a significant contribution to short-term increases in the number of sales registered (policy effect of 0.033 vs. 0.081 for the systematic registration dummy) and mortgages, where the systematic registration dummy is insignificant and the marginal effect of policy is 0.036. Results for sales and mortgages registered by a female co-owner (cols. 4 and 6) show one minor difference from the full sample: The systematic initial registration dummy in the mortgage regression is marginally significant, a finding consistent with the notion that, in the systematic registration process, owners receive information on how to use land documents. Registered built area increased for blocks where values of this variable were already high. Yet the estimated elasticity of policy (0.008) is an order of magnitude smaller than the coefficient on the initial registration dummy, a finding consistent with investment being labor-intensive rather than based on credit or a mortgage.

4.2 Medium-term Effects on Land and Financial Market Activity

To assess the importance of policy in the medium term, Table 5 reports results for the entire 20-year period from estimating equation (2) for total registered land sales and mortgages (cols. 1 and 3) and separately for female co-owners (cols. 2 and 4) and built-up area on registered parcels (col. 5). Lagged stocks of

sporadically registered parcels (instrumented by their second lag) and their interaction with a policy reform dummy and also with the stock of systematically registered parcels are entered separately as independent variables. Elasticities of variables at the mean and their significance are reported at the bottom of the table.

Throughout, the number of parcels that entered the registry via systematic initial registration is statistically significant. The magnitudes of the coefficients vary, however, between the number of registered land sales and built area on the one hand and mortgages and land sales involving female co-owners on the other. It is estimated that policy changes affected all the outcome variables considered except registered sales. The number of parcels registered before 2010 is estimated to have had no impact on the access of female co-owners to mortgages or their ability to sell land. This could mean that, until the change in the law making joint ownership a default and adoption of processes to safeguard women's rights (as reflected in θ_2), supported by a campaign to raise awareness in order to increase the number of parcels registered with women as co-owners (as reflected in θ_3), the 2006 changes in the law itself did not empower women economically.

Elasticities computed at the mean of relevant variables can illustrate the relative magnitudes involved. Registration of sales and mortgages including female co-owners was driven entirely by policy changes; estimated elasticities were 0.49 for sales and 0.56 for mortgages, compared to elasticities of systematic initial registration of 0.29 and 0.26. In line with the magnitude of the reduction in policy-induced transaction costs and the lower level post-reform of such costs for mortgages compared to sales (see Figure 2), policy changes are estimated to have contributed to a more significant increase in the number of total registered mortgages (elasticity increased from 0.52 to 0.74) but no significant impact on sales. Consistent with the notion that activity in land and credit markets that requires interaction with the registry will be more sensitive to transaction costs, the elasticity of systematically compared to sporadically registered parcels with respect to sales and mortgages is about one-third and one-fifth that of sporadically registered ones—but about 2.5 times that for land-related investment, for which the impact of policy changes, though statistically significant, is negligible.

A plausible explanation for these differences is that parcels registered systematically (or their owners) differ from those registered sporadically in ways that make them less marketable or less attractive as collateral but do increase the marginal return on investment. Table A3 details the results of regressing dummies for systematic and sporadic registration (for the latter, for periods before, during, and after systematic registration was launched) against distances to the CBD and parcel area. There is clear evidence that, with sporadically registered parcels generally larger and located closer to the CBD, parcels subject to systematic initial registration are more likely to be at the urban fringe. Qualitative differences between the two modalities of initial registration are more pronounced pre-reform; after 2015 they disappear.

Finally, the point estimate of the hypothetical effect of systematic initial registration without policy changes, $\theta_3 - \theta_2$, is positive for the number of registered sales and built registered area. The latter is consistent with investment incentives through tenure security rather than credit. Negative estimates of $\theta_3 - \theta_2$ for registered mortgages and female co-owner involvement illustrate the importance of policy reforms to translate social empowerment of women into economic benefits. To the extent that registering sales transactions can be taken as representing total transfers, including non-monetary intra-family ones, policy reforms would also be instrumental in sustaining gains in registry coverage and female co-ownership made via systematic initial registration.

4.3 Differentiating Gender Effects in the Medium Term

Data for the 20-year period also allows us to explore whether the positive short-term effects of providing women with registered rights are sustained over time and to assess the extent to which the major shift in female access to documented land rights can be attributed to (a) the 2006 legal reform; (b) institutional changes implemented in 2010; and (c) systematic or sporadic registrations.

Results from estimating equation (3) with the number of annually registered parcels overall (Table 6, panel A) and disaggregated by registration modality (panel B) as independent variables for a naïve specification with initial registration only but no indicator of legal reforms (col. 1); a specification that includes only an indicator for the 2006 legal reform; and a specification with indicator variables for legal and institutional reforms in 2006 and 2010. The proper specification (panel B, col. 3) illustrates that sporadic registration before 2006 was heavily biased against female co-ownership of land, as indicated by the negative and highly significant point estimate for γ_1 of -0.338 . The 2006 reform that granted women the right to register rights to land in their own name had no impact on their actual access to documented land rights—the estimate for γ_4 (the post-2006 and pre-2010 dummy) is insignificant. Instead, the 2010 change in the laws to make joint ownership the default for common-law marriages plus institutional reforms to streamline implementation together made the 2006 reforms effective: With an estimate of 0.674 for γ_2 , $\gamma_1 + \gamma_2 = 0.336$, sporadic initial registration switched from being biased against women up to 2010 (even with the LCMPA) to favoring female co-ownership thereafter. Policy changes thus had a marked impact even without systematic registration.

In fact, systematic initial registration increased women's co-ownership of land well beyond what was achieved through policy. The point estimate for γ_3 , the estimate of systematic registration after the policy change, is virtually indistinguishable from that of the policy effect γ_2 (0.73), so that $\gamma_3 - \gamma_2$ is insignificantly different from zero. In other words, the entire effect of sporadic registration in empowering women can be attributed to post-2010 policy changes. The pre-reform gender bias of sporadic registration implies that since the reform, systematic processes are still more gender-friendly than sporadic ones. While

exploring potential causes of bias will require household data, our data can help in investigating whether, since LARP ended, the bias had eased in locations that had been exposed to systematic registration earlier. Table A4 shows this was not the case; the estimated coefficient on (NASpP # SyD) is insignificant.

In addition to providing substantive insights, Table 6 illustrates the danger that improper specification—either by lumping sporadic and systematic processes together or by omitting relevant policy reforms—will lead to faulty conclusions. Panel A col. 3 shows that, even if policy reforms are properly specified, aggregating systematic and sporadic initial-registration modes results in biased coefficients; in our case, the estimated impacts of policy reform are biased upward and those of registration downward, resulting in a highly significant negative point estimate of the coefficient on initial registration. The fact that omission of the 2010 reforms would bias estimated effects of 2006 reforms upward, mistakenly attributing the impact of the former to the latter, reinforces the need for specifications to be informed by implementation details.

5. Implications for Research and Policy

Administrative data allows us to analyze the short- and medium-term effects of a program in Lesotho that combined systematic initial registration of rights (titling) with reforms of regulatory and institutional policies. Based on descriptive evidence that a structural break in the cost of registration coincided with the reforms, as was expected given much of the literature, we were able to document that titling had significant social empowerment effects on female co-ownership and investment. We add to the literature by showing that policy reforms affected operation of land sales and mortgage markets, especially in the medium term and for female co-owners, thus ensuring registry sustainability and translating social into economic empowerment. Beyond the focus on all property in a city rather than just informal settlements or rural areas, a major difference from previous land titling studies is our use of administrative data for a relatively long period both before and after project interventions and changes in policy. Using administrative data as a frame for household surveys to explore the underlying distributional and growth dynamics could be of great interest.

The finding that beyond the short-term effects of systematic initial registration, policy reforms associated with urban titling can activate land sales and mortgage markets raises two questions about design of programs in this area:

In most contexts the potential for land and associated mortgage market activity and, if registration is easier, sporadic registration, is higher in cities than in the informal settlements or rural areas that have traditionally been the focus of titling programs. Supporting policy and institutional reforms to make it easier to register property in cities could launch a nationwide strategy while at the same time catalyzing expansion of registry coverage, activation of mortgage markets, and the realization of indirect benefits in terms of land use

planning and harnessing the potential for property tax collection—thus setting in motion a virtuous cycle of rising land prices and better and more effective provision of local public goods that can enhance accountability.

Because registration fees are a central policy variable, it will be of interest to characterize conceptually and assess empirically what fee levels would maximize the direct and indirect benefits of coverage and operation of property registries in settings with heterogeneous agents. Exploring these issues in more detail will be important for follow-up research that could have considerable policy relevance.

Table 1: Descriptive Statistics Based on Administrative data

	Total	1981–99	2000–09	2010–14	2015–19
No. of leases issued	58,582	6,641	4,845	39,189	7,907
per year	1,502	350	485	7,838	1,581
of which owned solely by males ^a	0.263	0.766	0.632	0.149	0.208
Sporadic registrations per year	619	350	485	1,222	1,313
Systematic registrations per year	883			6,616	269
Share of residential property	0.892	0.853	0.924	0.889	0.921
No. of transfers per year	210	51	93	390	871
No. of mortgages per year	168	117	87	289	400
Share of area built ^b	0.475	N/A	0.460	0.484	0.498
Share of registered area built ^b	0.128	N/A	0.066	0.155	0.223

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

^a Gender relates to the 94.4 percent of properties registered in the name of natural persons rather than legal entities.

^b 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	
Share of registered area built	1990	0.052	0.049	0.051	0.047	
	2000	0.085	0.080	0.082	0.078	
	2010	0.076	0.057	0.042	0.072	
Population density (indiv./km ²)	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		Reg. sales		Reg. mortgages		Built area
	Total	♀ Co-own	Total	♀ Co-own	Total	♀ Co-own	Registered
Panel A: Regression							
Syst. dummy (SyD)	9.191*** (0.340)	8.006*** (0.310)	0.083*** (0.028)	0.090*** (0.019)	0.021 (0.018)	0.022* (0.013)	0.142*** (0.008)
R-squared	0.472	0.477	0.054	0.058	0.031	0.027	0.639
Mean dep. var.	0.163	0.052	0.038	0.012	0.030	0.007	0.067
SD dep. var.	0.600	0.293	0.221	0.117	0.183	0.084	0.092
Panel B: Placebo							
Pseudo syst. dummy	0.015 (0.027)	0.046*** (0.013)	-0.007 (0.014)	-0.009 (0.008)	-0.003 (0.010)	0.005 (0.003)	0.000 (0.001)
R-squared	0.014	0.011	0.013	0.011	0.006	0.001	0.288
Mean dep. var.	0.157	0.049	0.036	0.011	0.028	0.007	0.065
SD dep. var.	0.593	0.282	0.214	0.113	0.176	0.081	0.091

Note: Regressions in panel A are for 2000–14 with systematic first-time registrations in 2011–13; placebo tests in panel B are for 2000–08 with a pseud-systematic registration six 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 22,710 (1,514 for 15 years) in panel A and 13,626 (1,514 for 9 years) in panel 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

	Parcels reg. first time		Registered sales		Reg. mortgages		Built area
	Total	♀ Co-own	Total	♀ Co-own	Total	♀ Co-own	Registered
Syst. dummy (SyD)	9.188*** (0.333)	8.004*** (0.306)	0.081*** (0.028)	0.087*** (0.018)	0.023 (0.018)	0.022* (0.013)	0.142*** (0.008)
Policy # regist. 2009	-0.086*** (0.017)	-0.046* (0.025)	0.011*** (0.003)	0.023*** (0.004)	0.009*** (0.002)	0.006** (0.003)	0.000 (0.001)
Policy # initial value		-0.042 (0.064)	-0.386*** (0.106)	-0.544*** (0.093)	-0.201* (0.115)	-0.091 (0.151)	0.091** (0.037)
R-squared	0.474	0.478	0.061	0.067	0.039	0.028	0.642
ME: policy	-0.390*** (0.076)	-0.260*** (0.073)	0.033*** (0.010)	0.017*** (0.004)	0.036*** (0.008)	0.007** (0.003)	0.008*** (0.002)
Mean dep. var.	0.163	0.052	0.038	0.012	0.030	0.007	0.067
SD dep. var.	0.600	0.293	0.221	0.117	0.183	0.084	0.092

Note: All regressions are at 250m x 250m blocks in years 2000–14 with systematic first-time registration phased in over 2011–13. Initial value is the pre-program (2009) number of parcels with female co-owners (col. 2); the mean annual number of registered sales and mortgages with female co-owners (cols. 3 (4) and 5 (6)); and the built-up area overlapping with registered parcels in col. 7. Controls include population density, block and year fixed effects, a constant, and, for regressions reported in cols. 1–4, the block's share of built-up area throughout. The number of observations throughout is 22,710 (1,514 annually for 15 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: Medium-term Effects on Number of Registered Transfers, Mortgages, and Built-up Area

	No. of registered sales		No. of registered mortgages		Built area Registered
	Total	♀ Co-owner	Total	♀ Co-owner	
LCSystP, θ_3	0.0049*** (0.001)	0.0041*** (0.001)	0.0012*** (0.000)	0.0015*** (0.000)	0.0051*** (0.000)
LCSpP, θ_1	0.0292*** (0.004)	0.0130 (0.012)	0.0103*** (0.003)	0.0075 (0.005)	0.0041*** (0.000)
LCSpP # post-2010, θ_2	0.0007 (0.002)	0.0178* (0.009)	0.0044*** (0.001)	0.0078** (0.004)	0.0005** (0.000)
$\theta_3 - \theta_2$	0.0042*** (0.0018)	-0.0266*** (0.0044)	-0.0032*** (0.0013)	-0.0138*** (0.0024)	0.0047*** (0.0002)
Elasticities					
LCSystP	0.2869*** (0.0423)	0.2943*** (0.0417)	0.1457*** (0.0508)	0.2551*** (0.0629)	0.358*** (0.0067)
LCSpP, θ_1	0.7945*** (0.1170)	0.205 (0.1835)	0.5214*** (0.1331)	0.2773 (0.1872)	0.1439*** (0.0169)
LCSpP, ($\theta_1 + \theta_2$)	0.8134*** (0.0930)	0.4862*** (0.0679)	0.7417*** (0.1009)	0.5631*** (0.0867)	0.1606*** (0.0131)
Mean dep. var.	0.0383	0.0118	0.0299	0.0068	0.0666
SD dep. Var	0.2208	0.1169	0.1826	0.0838	0.0925

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, or registered area built at block level. LCSystP denotes the lagged cumulative number of parcels systematically registered and LCSRP those sporadically registered, with LCSpP instrumented by its two-period lagged value as described 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. Standard errors are in parentheses. Mean and standard deviation for the dependent variable are for the pre-program period. 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 relevant mean values, i.e., for sporadic registration post-2010 means and for systematic post-2011 means restricted to blocks covered by systematic registration are considered. Standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.010.

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

	Naïve Regression	With Policy Reforms	
		2006 only	2006 and 2010
Panel A: Total registered parcels			
No. registered parcels (NARP)	0.692*** (0.011)	-0.238*** (0.057)	-0.284*** (0.055)
NARP post-2006		0.935*** (0.059)	
NARP post-2006 & pre-2010			-0.004 (0.071)
NARP post-2010			0.986*** (0.058)
R-squared	0.871	0.878	0.885
Panel B: Systematic and sporadic separately			
Systematic parcels (NSyst.P), γ_3	0.733*** (0.009)	0.734*** (0.009)	0.734*** (0.009)
Sporadic parcels (NASpP), γ_1	0.260*** (0.070)	-0.314*** (0.052)	-0.338*** (0.051)
NASpP # post-2006, γ_0		0.610*** (0.079)	
NASpP # post-2006 & pre-2010, γ_4			-0.001 (0.069)
NASpP # post-2010, γ_2			0.674*** (0.087)
$\gamma_3 - \gamma_2$			0.060 0.089
R-squared	0.893	0.896	0.899

Note: All regressions are for 250m x 250m blocks for 2000–19. The dependent variable is the number of parcels registered for the first time in a given year at block level with a female co-owner. LCSystP denotes the lagged cumulative number of parcels systematically registered and LCSRP the number sporadically registered, with LCSpP instrumented by its lagged value as described in the text. Throughout, population density, share of built-up area of the block, block and year fixed effects, and a constant are included, with coefficients not reported. Standard errors are in parentheses. A two-period lag of the number of parcels registered is used as an instrument. The number of observations throughout is 30,280 (1,514 blocks over 20 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.

Figure 1: Location of Blocks for Sporadic and Systematic Registration

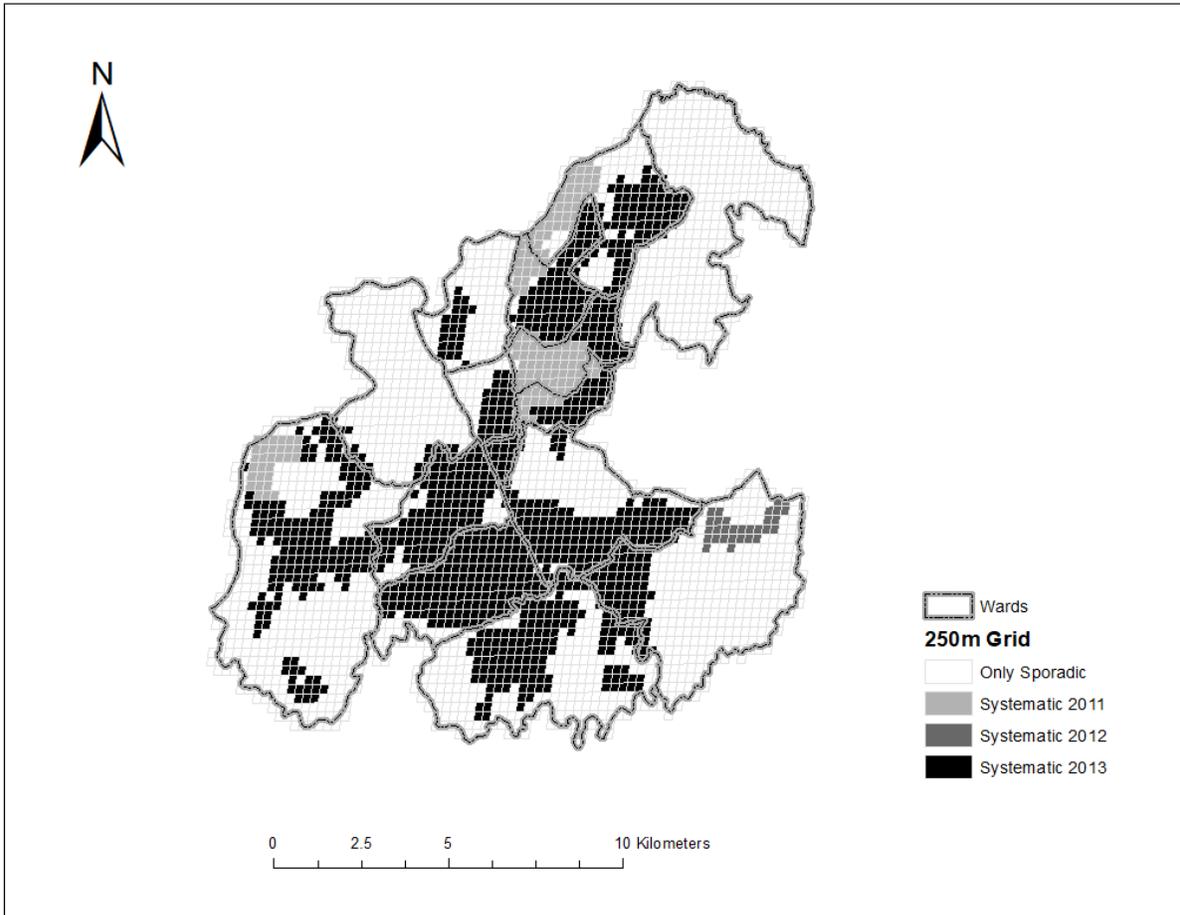
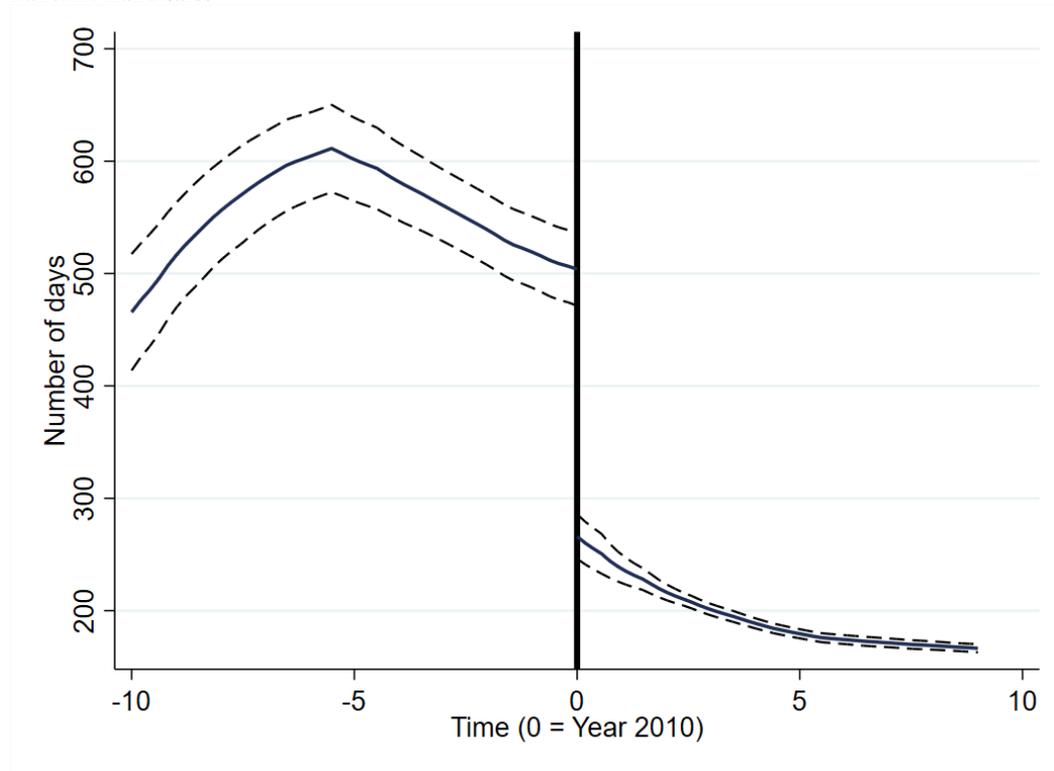
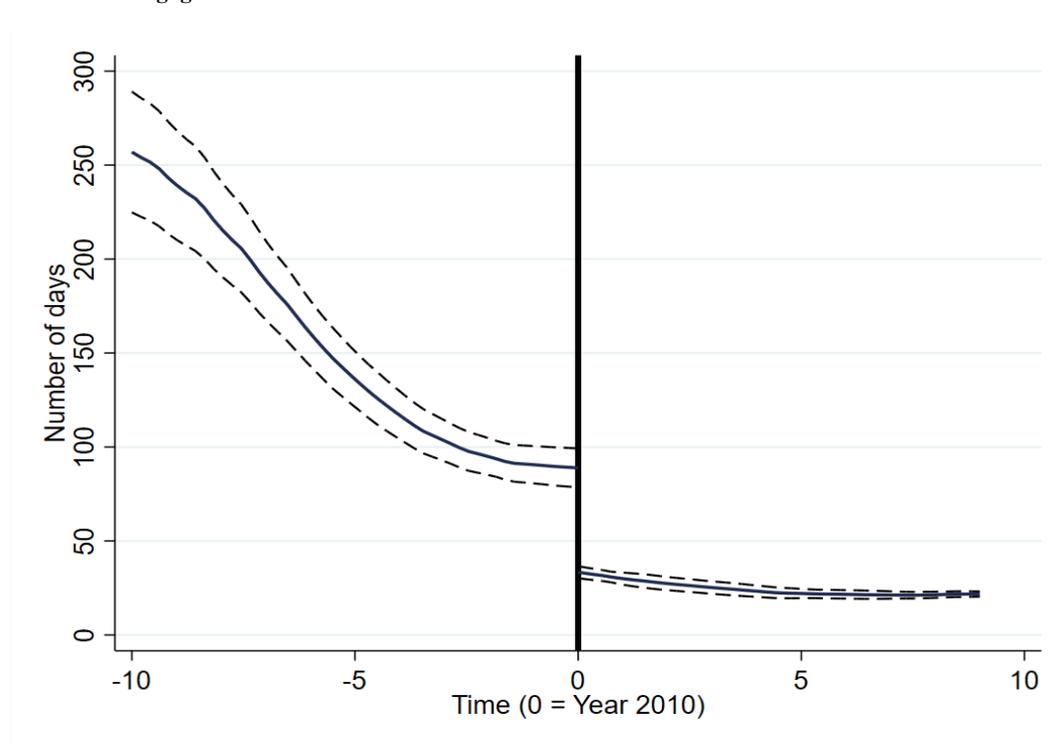


Figure 2: Non-parametric Regressions of Time Required for Registering Sales or Mortgages

Panel A: Land sales



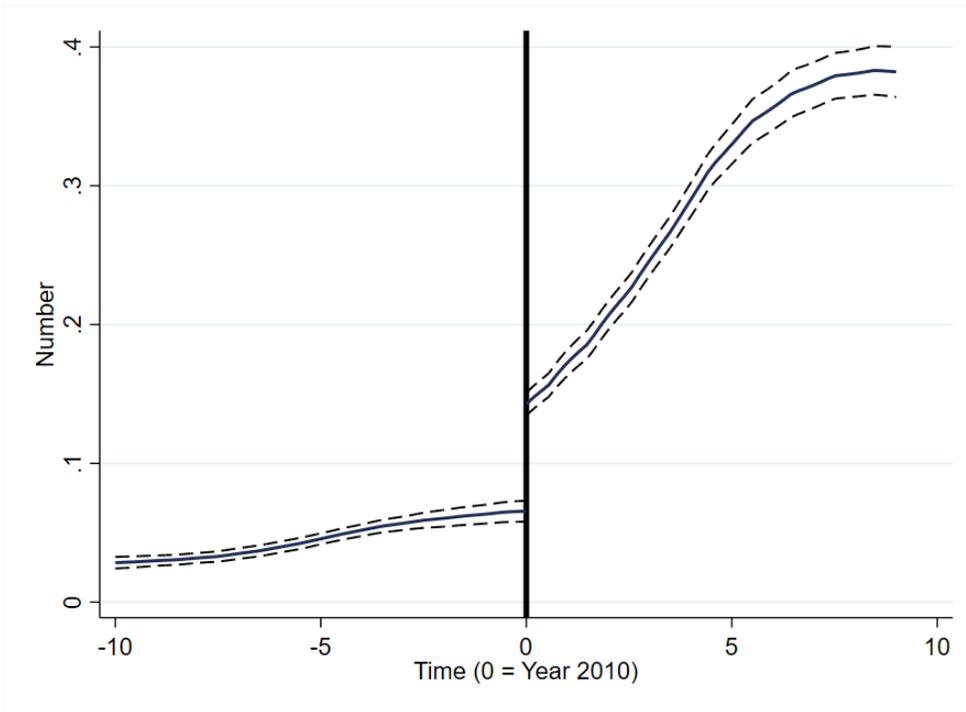
Panel B: Mortgages



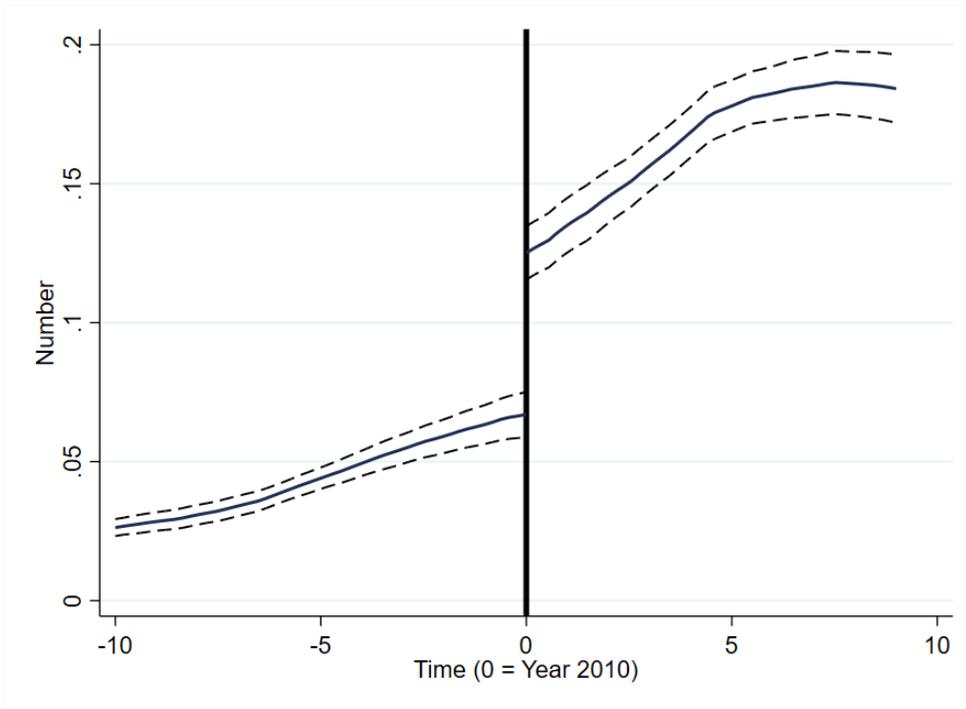
Note: Dashed lines represent confidence intervals.

Figure 3: Non-parametric Regressions of Registered Sales and Mortgages

Panel A: Sales

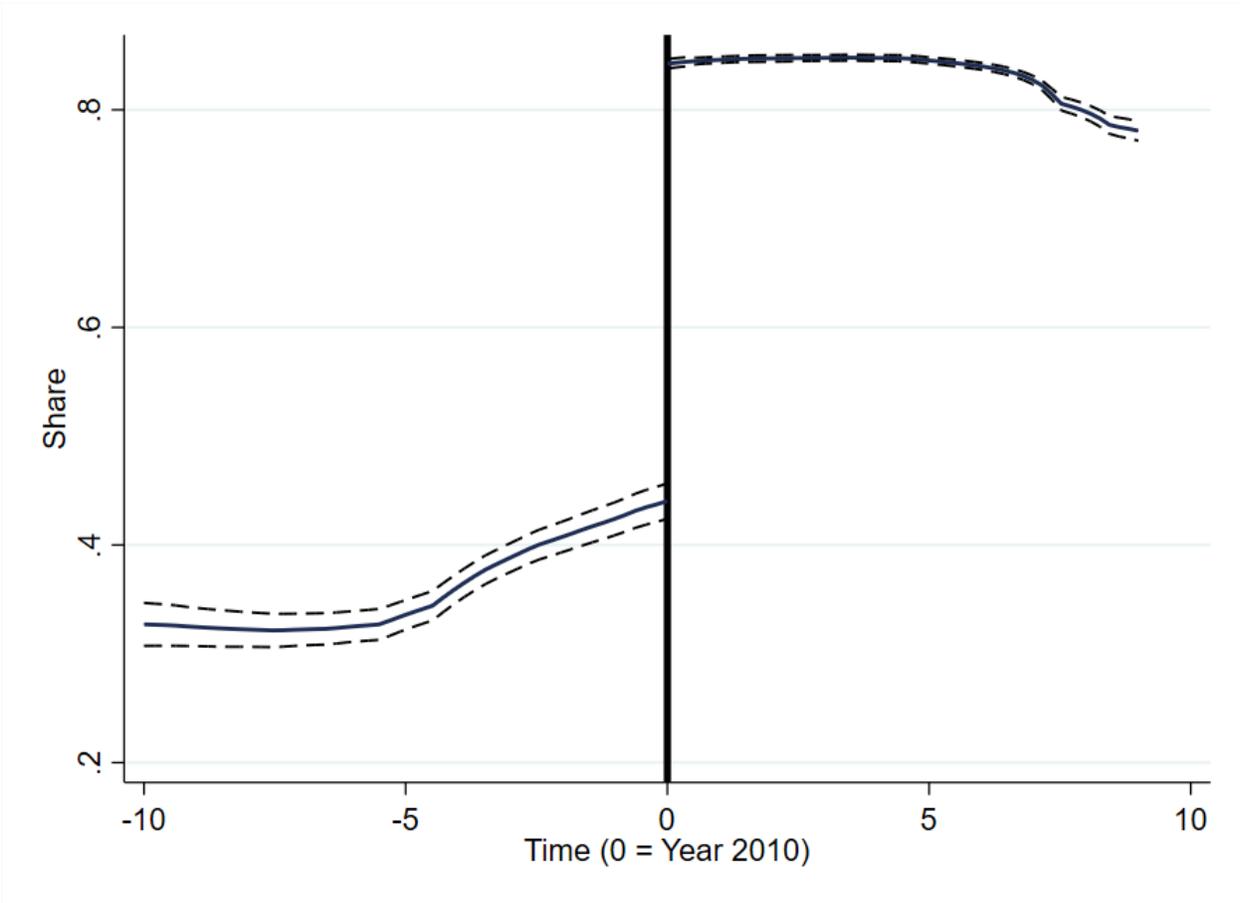


Panel B: Mortgages



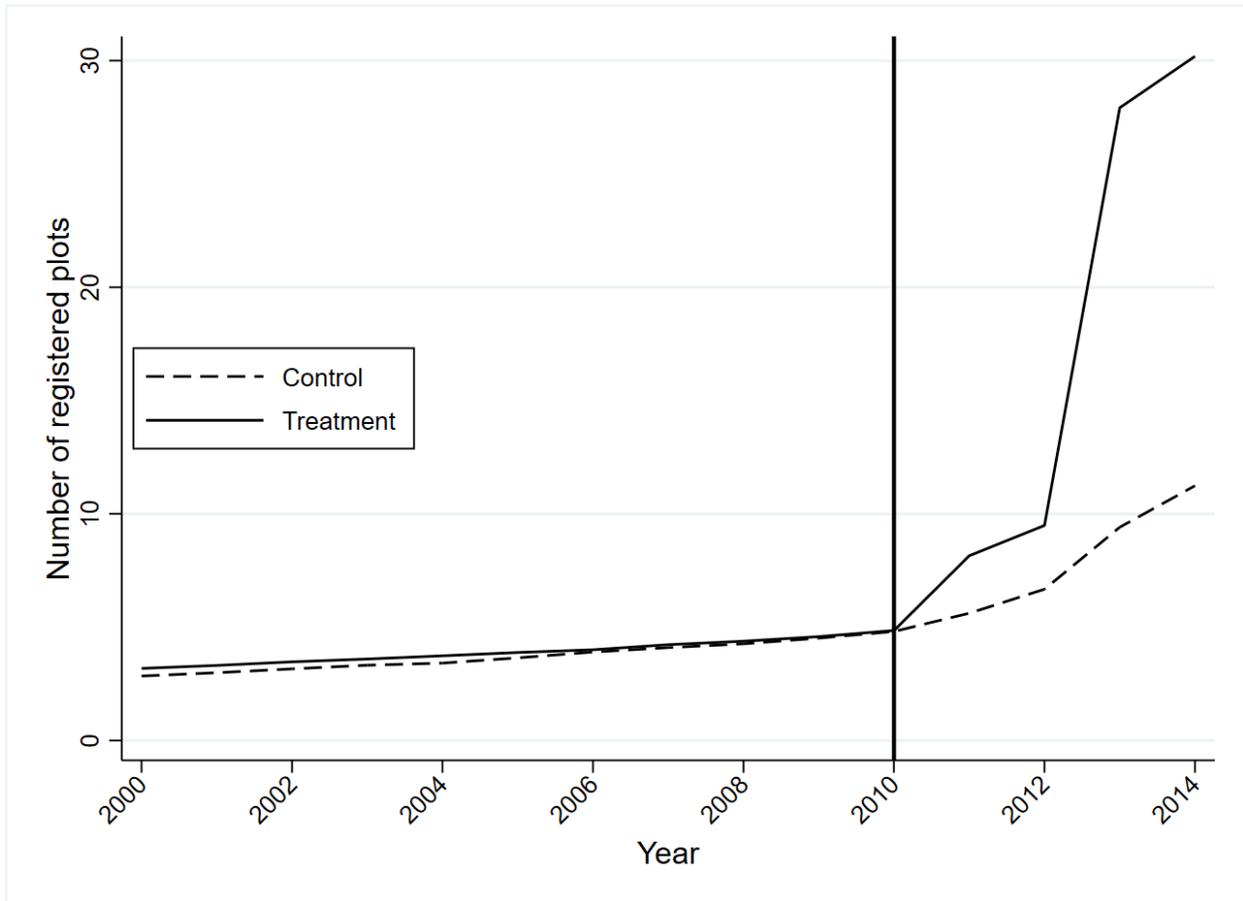
Note: Dashed lines denote confidence intervals.

Figure 4: Non-parametric Regression of Initial Registrations with One or More Female Co-owners, Percent



Note: Dashed lines denote confidence intervals.

Figure 5: 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

Tables

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

	Total	1981–99	2000–09	2010–14	2015–19
Initial registration					
No. of leases issued	58,582	6,641	4,845	39,189	7,907
per year	1,502	350	485	7,838	1,581
Plot area (m ²)	1,060	1,494	1,025	972	1,173
Sporadic	24,160	6,641	4,845	6,111	6,563
Systematic	34,422			33,078	1,344
Sporadic/year	619	350	485	1,222	1,313
Systematic/year	883			6,616	269
<i>Approach and land use type</i>					
Sporadic registration	0.412	1.000	1.000	0.156	0.830
Systematic registration	0.588	0.000	0.000	0.844	0.170
Residential land	0.892	0.853	0.924	0.889	0.921
Commercial land	0.046	0.115	0.055	0.035	0.034
Agricultural land	0.002	0.001	0.001	0.001	0.013
Other Use	0.059	0.030	0.020	0.075	0.032
<i>Subject of registration</i>					
Ownership male/female joint	0.399	0.002	0.063	0.515	0.365
of which systematic reg.	0.777	0.000	0.000	0.866	0.235
Ownership by females only	0.303	0.215	0.287	0.324	0.282
of which systematic reg.	0.633	0.000	0.000	0.855	0.174
Ownership by males only	0.251	0.709	0.602	0.147	0.170
of which systematic reg.	0.322	0.000	0.000	0.789	0.151
Ownership by companies or others	0.046	0.074	0.048	0.014	0.182
of which systematic reg.	0.101	0.000	0.000	0.362	0.051
Transfers					
No. of transfers	8,204	972	927	1,950	4,355
per year	210	51	93	390	871
Transferred by male owners	0.384	0.614	0.662	0.363	0.283
Transferred by female owners	0.309	0.180	0.282	0.321	0.338
Transferred by males/female jointly	0.263	0.002	0.008	0.296	0.361
Transferred by companies	0.044	0.204	0.049	0.021	0.018
Days to get consent	93	190	395	79	28
Days to complete registration	120	160	148	121	104
Days since approval of consent	77	85	73	74	77
Purchase price (US\$) ^a	11,394	13,335	15,497	12,386	9,701
Plot area (m ²)	1,005	1,204	1,411	938	907
Price (US\$/m ²)	15	15	17	16	14
Mortgages					
No. of registered mortgages	6,542	2,227	872	1,445	1,998
per year	168	117	87	289	400
Mortgage value (US \$)	51,573	37,372	59,329	71,471	49,620
Plots reg. to male owners	0.529	0.732	0.672	0.461	0.296
mortgage value (US \$)	42,626	31,279	43,427	63,503	49,338
Plots reg. to female owners	0.209	0.175	0.222	0.220	0.234
mortgage value (US \$)	37,447	21,333	40,483	50,632	40,520
Plots reg. by males & female jointly	0.183	0.009	0.032	0.245	0.395
mortgage value (US \$)	41,917	19,975	39,425	45,082	41,130
Plots reg. by companies	0.078	0.085	0.073	0.075	0.075
mortgage value (US \$)	175,531	131,375	255,818	266,687	131,479
Days for registration process	69	136	86	24	20
Days to get ministerial consent	94	99	79		

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

^aNote 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
Panel A: Total					
Parcels reg. sporadically #	1.3	3.0	6.0	9.0	11.0
Area parcels reg. sporadic m ²	2,223	4,516	6,915	9,730	11,883
Parcels reg. syst. #				16	17
Area reg. syst. m ²				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 ²	954	1,356	1,570	1,722	1,847
Panel B: Treatment (systematic & sporadic)					
Parcels reg. sporadically #	1.2	3.0	5.0	7.0	9.0
Area parcels reg. sporadic m ²	1,896	4,202	5,854	7,896	9,448
Parcels reg. syst. #				25	25
Area reg. syst. m ²				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 ²	1,042	1,562	1,809	1,984	2,129
Panel C: Control (sporadic only)					
Parcels reg. sporadically #	1.6	3.0	8.0	11.0	14.0
Area parcels reg. sporadic m ²	2,771	5,041	8,694	12,803	15,965
Parcels reg. syst. #			-	2	2
Area reg. syst. m ²				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 ²	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

	Systematic Total	Sporadic Total	1981–2009	Sporadic 2010–14	2015–19
CBD distance (m)	0.137* (0.0733)	-0.216*** (0.0771)	-0.161** (0.0650)	-0.108** (0.0544)	0.132** (0.0516)
Parcel area (m ²)	-0.0138*** (0.00297)	0.0251*** (0.00312)	0.0515*** (0.00263)	-0.00304 (0.00220)	-0.0347*** (0.00209)
Mean dep. var.	0.604	0.397	0.194	0.0986	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. Standard errors are in parentheses; * p<0.10, ** p<0.05, *** p<0.010

Table A4: Effect of Reform on Gender of Holders of Registered Land: Female Co-owner or Male Only

	(1)	(2)	(3)	(4)	(5)
No. of sporadic parcels (NASpP)	0.221*** (0.080)	-0.340*** (0.042)	-0.340*** (0.042)	-0.316*** (0.052)	-0.339*** (0.051)
No. of systematic parcels (NASystP)	0.732*** (0.009)	0.734*** (0.009)	0.734*** (0.009)	0.733*** (0.009)	0.734*** (0.009)
NASpP # SyD	0.168 (0.129)	0.072 (0.141)	0.072 (0.141)	0.121 (0.134)	0.072 (0.141)
NASpP # post-2010		0.657*** (0.097)	0.657*** (0.097)		0.656*** (0.101)
NASpP # post-2006				0.584*** (0.088)	
NASpP # post-2006 & pre-2010					-0.001 (0.069)
No. of obs. (250*250 m blocks)	30,280	30,280	30,280	30,280	30,280
R-squared	0.894	0.899	0.899	0.896	0.899

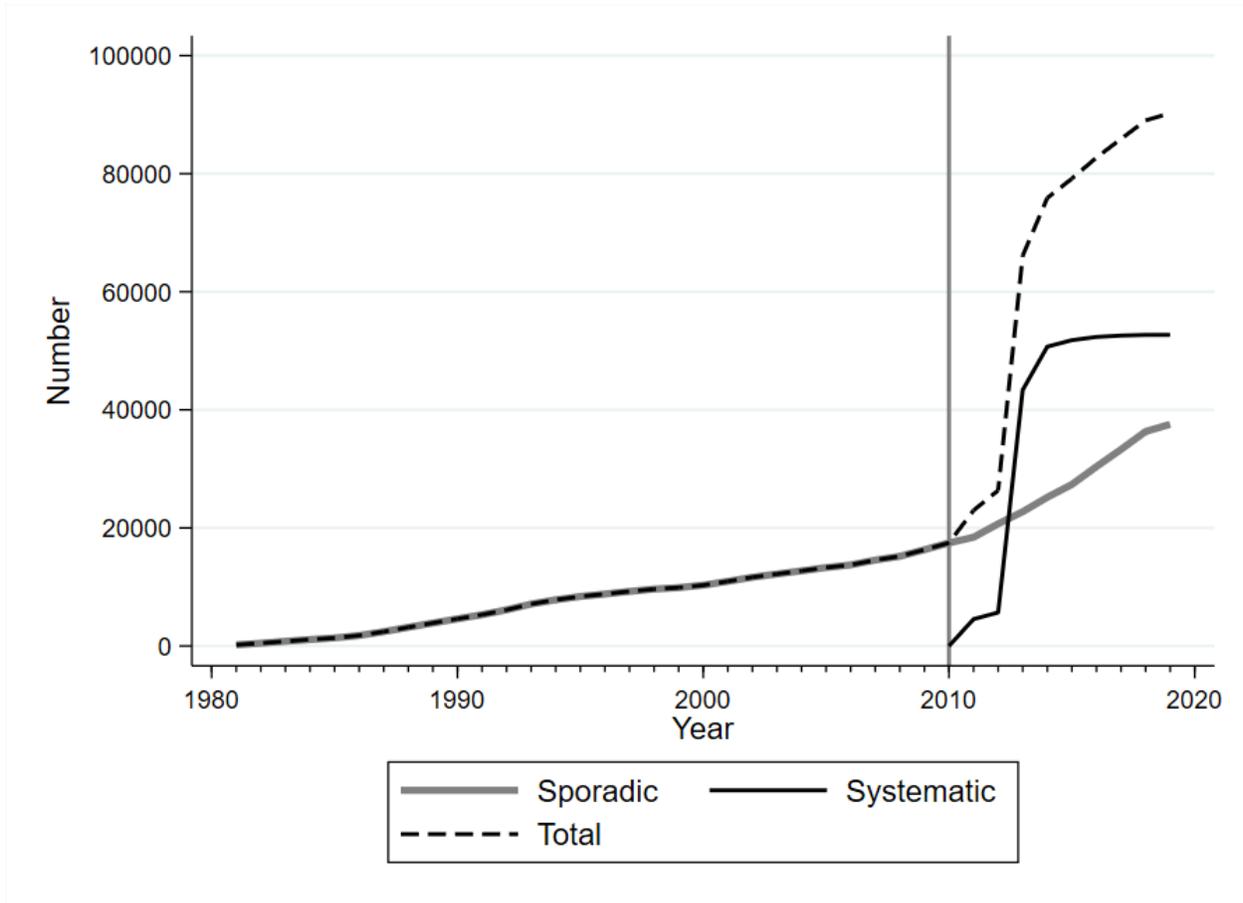
Note: All regressions are for 250m x 250m blocks in 2000–19. The dependent variable is the difference between the number of parcels registered in year t with a female co-owner and those with male owners only. NASpP and NASystP are the number of parcels initially registered, whether sporadically or systematically, in year t . SyD is a block-level dummy for systematic initial registration conducted as in Tables 3 and 4. Population density, share of built-up area of the block, block and year fixed effects and a constant are included throughout; coefficients are not reported. The number of observations is 30,280 (1,514 blocks annually for 20 years) throughout. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Figures

Figure A1: Illustration of Blocks Used to Evaluate Impact



Figure A2: Cumulative Number of Registered Parcels by Year



References

- Agarwal, B. 1994. *A Field of One's Own: Gender and Land Rights in South Asia*. New York and Melbourne: Cambridge University Press.
- Agyei-Holmes, A., N. Buehren, M. Goldstein, R. Osei, I. Osei-Akoto, and C. Udry. 2020. *The Effects of Land Title Registration on Tenure Security, Investment and the Allocation of Productive Resources*. Washington, DC: World Bank.
- Ali, D.A., M. Collin, K. Deininger, A. Dercon, J. Sandefur, and A. Zeitlin. 2016. "Small Price Incentives Increase Women's Access to Land Titles in Tanzania." *Journal of Development Economics* 123, 107–22.
- Ali, D.A., K. Deininger, and M. Goldstein. 2014. "Environmental and Gender Impacts of Land Tenure Regularization in Africa: Pilot Evidence from Rwanda." *Journal of Development Economics* 110: 262–75.
- Ali, D.A., K. Deininger, G. Mahofa, and R. Nyakulama. 2021. "Sustaining Land Registration Venefits by Addressing the Challenges of Reversion to Informality in Rwanda." *Land Use Policy* 110, 104317.
- Anderson, S., and G. Genicot. 2015. "Suicide and Property Rights in India." *Journal of Development Economics* 114, 64–78.
- Aragón, F. M., and A. S. Kessler. 2020. "Property Rights on First Nations Reserve Land." *Canadian Journal of Economics* 53: 460–95.
- Balan, P., A. Bergeron, G. Tourek, and J. Weigel. 2020. *Land Formalization in Weak States: Experimental Evidence from Urban Property Titling in the D.R. Congo*. Cambridge, MA: Harvard University. *Is this a book published by the Press , or a paper distributed by a department?*
- Baragwanath, K., and E. Bayi. 2020. "Collective Property Rights Reduce Deforestation in the Brazilian Amazon." *Proceedings of the National Academy of Sciences* 117: 20495.
- Blackman, A., I. Corral, E. S. Lima, and G. P. Asner. 2017. "Titling Indigenous Communities Protects Forests in the Peruvian Amazon." *Proceedings of the National Academy of Sciences* 114: 4123.
- Brasselle, A.S., F. Gaspart, and J. P. Platteau. 2002. "Land Tenure Security and Investment Incentives: Puzzling Evidence from Burkina Faso." *Journal of Development Economics* 67: 373–418.
- Bros, C., Desdoigts, A., Kouadio, H., 2019. Land Tenure Insecurity as an Investment Incentive: The Case of Migrant Cocoa Farmers and Settlers in Ivory Coast. *Journal of African Economies* 28, 147–75.
- Chaudhary, L., and A. V. Swamy. 2020. "A Policy of Credit Disruption: The Punjab Land Alienation Act of 1900." *Economic History Review* 73, :134–58.
- Deininger, K., D. A. Ali, S. Holden, and J. Zevenbergen. 2008. "Rural Land Certification in Ethiopia: Process, Initial Impact, and Implications for Other African Countries." *World Development* 36: 1786–1812.
- Deininger, K., and A. Goyal, 2012. "Going Digital: Credit Effects of Land Registry Computerization in India." *Journal of Development Economics* 99: 236–43/
- Deininger, K., A. Goyal, and H. K. Nagarajan. 2013. "Women's Inheritance R Fights and Intergenerational Transmission of Resources in India." *Journal of Human Resources* 48: 114–41.
- Deininger, K., and S. Jin. 2006. "Tenure Security and Land-Related Investment: Evidence from Ethiopia." *European Economic Review* 50: 1245–77.
- Deininger, K., Xia, F., Kilic, T., Moylan, H., 2021. Investment impacts of gendered land rights in customary tenure systems: Substantive and methodological insights from Malawi. *World Development* 147, 105654.
- de Soto, H., 2000. *The Mystery of Capital: Why Capitalism Triumphs in the West and Fails Everywhere Else*. New York: Basic Books.

- Devijlder, N., and K. Schoors. 2020. "Land Rights, Local Financial Development and Industrial Activity: Evidence from Flanders (Nineteenth-Early Twentieth Century)." *Cliometrica* 14: 507–50.
- Di Tella, R., Galiani, S., Schargrodsy, E., 2007. The Formation of Beliefs: Evidence from the Allocation of Land Titles to Squatters. *Quarterly Journal of Economics* 122, 209–41.
- Dippel, C., D. Feir, B. Leonard, and M. Roark. 2021. "Secured Transactions Laws and Economic Development on American Indian Reservations." *AEA Papers & Proceedings* 111: 248–52.
- Dippel, C., D. Frye, and B. Leonard. 2020. "Property Rights without Transfer Rights: A Study of Indian Land Allotment." NBER Working Papers 27479. Cambridge, MA: National Bureau of Economic Research.
- Djezou, W., 2016. "Land Tenure Security and Deforestation: A Case Study of Forest Land Conversion to Perennial Crops in Côte d'Ivoire." *Economics Bulletin* 36: 173–86.
- Fabbri, M., and G. Dari-Mattiacci. 2021. "The Virtuous Cycle of Property." *The Review of Economics and Statistics* 103: 413–27.
- Fenske, J. 2011. "Land Tenure and Investment Incentives: Evidence from West Africa." *Journal of Development Economics* 95: 137–56.
- Field, E. 2007. "Entitled to Work: Urban Property Rights and Labor Supply in Peru." *Quarterly Journal of Economics* 122: 1561–1602.
- Field, E., and M. Torero. 2006. *Do Property Titles Increase Credit Access Among the Urban Poor? Evidence from a Nationwide Titling Program*, edited by M. Torero. *[[Publisher?]]*
- Galiani, S., and E. Schargrodsy, 2004. "Effects of Land Titling on Child Health." *Economics and Human Biology* 2:353–72.
- . 2010. "Property Rights for the Poor: Effects of Land Titling." *Journal of Public Economics* 94: 700–29.
- . 2016. "The Deregularization of Land Titles." *Man & the Economy* 3: 169–88.
- Geddes, R., D. Lueck, and S. Tennyson. 2012. "Human Capital Accumulation and the Expansion of Women's Economic Rights." *Journal of Law and Economics* 55: 839–67.
- Goldstein, M., K. Hounbedji, F. Kondylis, M. O'Sullivan, and H. Selod. 2018. "Formalization without Certification? Experimental Evidence on Property Rights and Investment." *Journal of Development Economics* 132: 57–74.
- Gutierrez, I. A., and O. Molina. 2020. "Reverting to Informality: Unregistered Property Transactions and the Erosion of the Titling Reform in Peru." *Economic Development & Cultural Change* 69: 317–34.
- Harari, M. 2019. "Women's Inheritance Rights and Bargaining Power: Evidence from Kenya." *Economic Development and Cultural Change* 68: 189–238.
- Hazan, M., D. Weiss, and H. Zoabi. 2019. "Women's Liberation as a Financial Innovation." *Journal of Finance* 74: 2915–56.
- Holden, S. T., K. Deininger, and H. Ghebru. 2011. "Tenure Insecurity, Gender, Low-cost Land Certification and Land Rental Market Participation in Ethiopia." *Journal of Development Studies* 47: 31–47.
- Kazianga, H., and W.A. Masters. 2006. "Property Rights, Production Technology, and Deforestation: Cocoa in Cameroon." *Agricultural Economics* 35: 19–26.
- Knoll, K., M. Schularick, and T. Steger. 2017. "No Price Like Home: Global House Prices, 1870–2012." *American Economic Review* 107: 331–53.
- Kumar, N., and A. R. Quisumbing. 2015. "Policy Reform toward Gender Equality in Ethiopia: Little by Little the Egg Begins to Walk." *World Development* 67: 406–23.

- Lawry, S., C. Samii, R. Hall, A. Leopold, D. Hornby, and F. Mtero. 2016. "The Impact of Land Property Rights Interventions on Investment and Agricultural Productivity in Developing Countries: A Systematic Review." *Journal of Development Effectiveness*, **[[volume number?]]** 1–21.
- Manara, M., and T. Regan. 2021. "Eliciting Demand for a Publicly Provided Good from Community Leaders: Evidence from an RCT with Title Deeds in Urban Tanzania." University of Oxford, Oxford.
- Melesse, M.B., A. Dabissa, and E. Bulte. 2018. "Joint Land Certification Programmes and Women's Empowerment: Evidence from Ethiopia." *Journal of Development Studies* 54: 1756–74.
- Menon, N., Y. van der Meulen Rodgers, and A. R. Kennedy. 2017. "Land Reform and Welfare in Vietnam: Why Gender of the Land-Rights Holder Matters." *Journal of International Development* 29: 454–72.
- Muchomba, F.M., 2017. "Women's Land Tenure Security and Household Human Capital: Evidence from Ethiopia's Land Certification." *World Development* 98: 310–24.
- Newman, C., F. Tarp, and K. van den Broeck. 2015. Property Rights and Productivity: The Case of Joint Land Titling in Vietnam." *Land Economics* 91, 91–105.
- Nkurunziza, N. 2015. "Implementing and Sustaining Land Tenure Regularization in Rwanda." In *How Innovations in Land Administration Reform Improve on 'Doing Business'*, edited by T. Hilhorst and F. Meunier, pp. 10–20. Washington DC: World Bank.
- Rajan, R., and R. Ramcharan. 2011. "Land and Credit: A Study of the Political Economy of Banking in the United States in the Early 20th Century." *The Journal of Finance* 66: 1895–1931.
- . 2015. "The Anatomy of a Credit Crisis: The Boom and Bust in Farm Land Prices in the United States in the 1920s." *American Economic Review* 105: 1439–77.
- Suesse, M., and N. Wolf. 2020. "Rural Transformation, Inequality, and the Origins of Microfinance." *Journal of Development Economics* 143: 102429.
- Vélez, M. A., J. Robalino, J. C. Cardenas, A. Paz, and E. Pacay. 2020. Is Collective Titling Enough to Protect Forests? Evidence from Afro-descendant Communities in the Colombian Pacific Region." *World Development* 128: 104837
- Wang, S.-Y. 2014. "Property Rights and Intra-household Bargaining." *Journal of Development Economics* 107; 192–201.
- Weigel, J. L., 2022. "The Participation Dividend of Taxation: How Citizens in Congo Engage More with the State When it Tries to Tax Them." *The Quarterly Journal of Economics* 135: 1849–1903.
- Widman, M. 2014. "Land Tenure Insecurity and Formalizing Land Rights in Madagascar: A Gender Perspective on the Certification Program." *Feminist Economics* 20: 130–54.
- Wren-Lewis, L., L. Becerra-Valbuena, and K. Hounbedji. 2020. "Formalizing Land Rights Can Reduce Forest Loss: Experimental Evidence from Benin." *Science Advances* 6: eabb6914.
- Zegarra, L. F., 2017. "Usury Laws and Private Credit in Lima, Peru. Evidence from Notarized Records." *Explorations in Economic History* 65: 68–93.