A Field of Her Own: Property Rights and Women's Agency in Myanmar*

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Can financial incentives lead households to register land in women's names, thereby providing them with formal property rights? Can formal property ownership improve women's economic outcomes and change decision-making dynamics within the household? To investigate these questions, we take advantage of a bank lending policy in Myanmar that motivated households with land holdings above ten acres to title the surplus land in their wives' names. We surveyed 5,068 men and women in Myanmar about land ownership, economic activity, and gendered decision-making. We find that the financial incentives provided by the bank lending policy led to increases in women's formal property ownership, but these exogenously assigned rights did not manifest broadly into greater economic empowerment or decision-making power for recipients. We provide suggestive evidence that local cultural norms are a significant constraint for women to achieve empowerment through formal land rights.

Keywords: land rights, women's empowerment, lending policy, ten acres, Myanmar, regression discontinuity

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This study was conducted in compliance with local laws and standards; ethics approval and permission to conduct data collection were obtained in advance from the Government of Myanmar. Replication files are available in the JOP Data Archive on Dataverse (https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/YJWRAB). The empirical analysis has been successfully replicated by the JOP replication analyst. Supplementary material for this article is available in the appendix in the online edition. This research was supported in part by an NICHD training grant to the Population Studies Center at the University of Michigan (T32HD007339). Financial support for data collection was provided by King Philanthropies and Landesa Rural Development Institute.

A critical driver of gender inequality is women's limited access to land, especially in low-income countries: 61% of working women are employed in agriculture in the least developed nations, but women hold only 18% of global agricultural land (ILO 2019; FAO 2022). Assessing the relationship between women's property rights and economic and political power is fundamental to understanding gender inequality (Moghadam 1990). In this paper, we explore whether expanding women's *de jure* (written, legal) ownership of land, a critical economic, political, and social asset, shapes women's power within the household. We study a "natural experiment," created by a bank policy in Myanmar, that provided financial incentives to subdivide land and obtain additional formal land certificates in a woman's name. Our analysis asks two questions. First, can such incentives increase formal property ownership among women? Second, will formal ownership improve economic outcomes as well as shift power dynamics within and beyond the household?

We find strong evidence that these incentives led households to transfer formal *de jure* property rights to women: households affected by the policy were 15 percentage points more likely to have at least one land title (known as a Form 7) with the wife's name on it.[1] These households also reported a higher number of land-collateralized loans. Despite this change in women's debt levels, we find no evidence, on average, that *de jure* property rights for women were linked to differences in economic or social decision-making in the household, and our

[1] We focus our analysis on the household members with primary decision-making power. This is most often a married couple, who for brevity we refer to as husband and wife.

results are robust to multiple tests. However, after separating the sample geographically by local gender norms, we observe measurable effects on decision-making outcomes *only* in areas where norms and elite attitudes were more favorable towards women's land ownership and economic participation. This suggests that a supportive cultural and political environment is a necessary condition for property transfers to meaningfully affect gender dynamics.

This research expands on theories of women's economic empowerment that model changes in power within the household as a bargaining game (e.g., Browning and Chiappori 1998), where the allocation of assets within the household shapes bargaining power and decision-making (Iversen and Rosenbluth 2006). Building on this seminal work, our theory emphasizes an individual's legal ownership of land as potentially offering a fundamental shift in power dynamics within the household. We trace a possible theory of change that flows from women's formal property ownership, to credit access, to increases in economic returns/productivity, and finally to greater agency in household decision-making. Each of these steps is subject to important scope conditions, including consideration of social norms (Carnegie et al. 2020).

According to the Myanmar Poverty and Living Conditions Survey from 2015, women comprised only 20% of formal landholders in Myanmar, though customary rights vary (Lambrecht et al. 2023). Formal written documentation of property rights matters greatly, as evidenced by the fact that nearly 80% of respondents in our survey said that having their name on a written document is what makes the land "theirs." This makes Myanmar an ideal setting in which to study the impact of formal land transfers, relative to places with more limited documentation in sub-Saharan Africa (e.g., Huntington and Shenoy 2021). On the other hand, norms or enforcement mechanisms may be more important in determining whether formal property rights lead to changes in power within the household (Brulé 2020;

Carnegie et al. 2020), and on this dimension Myanmar is considered by most observers to perform relatively poorly. Myanmar ranked in the bottom third of the UNDP's 2021 Gender Inequality Index and the bottom sixth of the World Economic Forum's 2023 Global Gender Gap Index. This is an important consideration for contextualizing our results more broadly.

In 2013, the Myanmar Agricultural Development Bank (MADB), the country's dominant rural lender, rationed its loans by capping the amount of land eligible for agricultural lending at ten acres. Households owning more than ten acres of land were incentivized to break apart plots and formally register the surplus land in the name of another household member—often the wife of the current rights holder—in order to access additional bank credit. Employing a regression discontinuity (RD) design, we compare households with land holdings just below ten acres (who had no incentive to transfer land rights) to those just above this threshold. Our data comes from a household survey conducted between November 2019 and January 2020, during which we interviewed husbands and wives in 2,534 households across 138 village tracts of the Ayeyarwaddy region in southern Myanmar. We collected information from both partners in each household on a wide range of demographic and economic outcomes, as well as household decision-making processes, perceptions of agency, social norms, and—through a demand elicitation exercise—women's willingness to pay for monetary autonomy.

We highlight two key findings with implications for public policy and future research. First, as predicted, the bank lending policy leads to an increase in formal property rights for women and subsequent change to their economic position and financial engagement in the household (specifically in the form of loans taken out in women's names). Second, despite these changes in *de jure* power, women's earnings and decision-making ability within the household do not change. Further analysis suggests that such changes are only likely to

occur in places where local norms are supportive of women's land ownership and decision-making roles. Future work should therefore focus on designing policies that both encourage inclusive property rights and seek changes to intra-household power dynamics.

Our work contributes to the literatures on the formalization of property rights, the empowerment of women, and the intersection of public and private power (Cheema et al. 2023). An extensive literature in economics emphasizes the importance of formal *de jure* property rights for investment and labor market decisions (De Soto 2001; Field 2007; Galiani and Schargrodsky 2010). In most patriarchal societies, ownership and control of immovable property such as land and houses is restricted to men (Agarwal 1995), and spouses are often unable to have their names included on household property and land titles (Brown 2003; Savath, Fletschner and Santos 2015). Our setting provides a unique opportunity to study the consequences of assigning property rights specifically to women.

If women obtain formal property, it could lead to empowerment, defined as the ability to make decisions about one's own life (Akter et al. 2017), with implications both within and outside the household (Meinzen-Dick et al. 2019). These benefits may follow directly from the economic returns to formal property rights (Agarwal 1997), or from non-economic changes that nevertheless shape the position of women in the household (Panda and Agarwal 2005; La Ferrara and Milazzo 2017; Harari 2019) and their subsequent ability to exercise political preferences (Cheema et al. 2023). Previous studies on women's land rights have focused on changes in land tenure laws, registration of informally held land, legal consultation on land registration, and encouragement of joint titling programs. Many of these find positive effects on measures of market engagement or empowerment (Ali, Deininger and Goldstein 2014; Brulé and Gaikwad 2021; Menon, Van der Meulen Rodgers and Kennedy

2017; Wiig 2013; Holden, Deininger and Ghebru 2011), but others document neutral or even deleterious consequences (Bhalotra, Brulé and Roy 2020; Roy 2015).[2] In contrast, we examine a setting in which households have a financial incentive to title land in women's names, without a change in the overall legal system and without conferring new property rights to the household as a whole. Such voluntary changes could theoretically improve women's empowerment more than mandated legal changes—which could engender backlash—but as our findings demonstrate, there is a limit to the impact that can be expected from this type of voluntary property transfer.

PROPERTY RIGHTS AND RURAL FINANCE

All land in Myanmar is owned by the state, but the central government allocates long-term usage rights—which can be exchanged, sold, and mortgaged—and therefore are akin to private property rights.[3] We refer to these usage rights as "property rights" for simplicity.

The two documents that confer the strongest property rights are the Land Grant for urban settings and the Form 7 for agricultural settings (such as the Ayeyarwaddy region, where our data was collected). The Form 7 is often referred to as a land-use rights certificate

[2] Studies have also examined other economic interventions targeted at women, including cash transfers, microfinance initiatives, financial access, and business training programs (Duflo 2003; Almas et al. 2018; Ashraf et al. 2020; Field et al. 2021).

[3] In some cases, these usage rights can be rescinded by the local authorities for eminent domain or when the lease is violated by the user (Rhoads 2018).

(LURC) to signify that it provides tenure security (Mark 2016). While it is formally limited to agricultural work, non-farm activities often take place on Form 7 land. In a recent survey of businesses with Form 7s, only 4% listed their primary sector as agriculture, with the rest listing manufacturing, retail trade, and services (Malesky, Dulay and Peltovuori 2020).[4]

Myanmar's colonial history, as well as intermittent conflict between the state and armed groups in certain regions, has led to the uneven proliferation of documented property rights throughout the country. In addition to Land Grants and Form 7s, households may hold other land documents (such as Forms 105, 106, 15, or 39) that do not have the same exchange or mortgage privileges and cannot be considered a secure title (UN Habitat 2019). It is also common in Myanmar for families to hold Form 7s with the names of previous owners, together with an informal contract acknowledging the change of ownership. This informal contract, however, is not officially recognized as a tenure right by government authorities; formal transfer of the Form 7 is required to secure these rights (Mark 2016).

Following several decades of military rule, Myanmar capped a gradual transition to democratic governance when the National League for Democracy (NLD) won the 2015 parliamentary elections under the leadership of Nobel Laureate Aung San Suu Kyi. In January 2016, the NLD-controlled parliament approved a new National Land Use Policy following a public consultation process that, while far from comprehensively fair and inclusive, included domestic businesses, foreign investors, non-governmental organizations, and regional/ethnic

[4] According to the 1953 Land Nationalization Act, Article 39, to change a parcel from "agricultural" to "nonagricultural" land, one must get permission from the State/Region Peace and Development Council.

political groups. The main goal of the new Land Use Policy was to harmonize existing land laws and guide the development of new land. The policy clarified the legal rules for obtaining Form 7 rights, registration of those rights in a cadastral map, and utilization of rights for exchange and mortgage (Mark and Belton 2020).

As a result of legislative attempts to address legal barriers to women's land access and co-ownership of spousal property, there was no formal prohibition on women's sole or joint registration of a Form 7 in the 2016 National Land Policy (Mark 2016). In practice, however, joint registration was limited and few women received documented rights to agricultural land; 80% of farmland certificates (Form 7s) had only a man's name listed by 2016 (Lambrecht et al. 2023; Namati 2016). In many cases, cultural norms about a woman's role in Myanmar society limited equal access to formal property rights and, ultimately, agricultural decision-making (Carnegie et al. 2020; Akter et al. 2017). In our survey, 13% of respondents from landed families expressed the view that only men's names should be included on land documents (since men are the primary decision-makers), and a similar fraction of respondents expressed the view that having two names on a land certificate would lead to conflicts between couples. These attitudes were also shared by local leaders, as 22% of officials in our survey expressed the view that women should never make decisions about household plots and only 50% believed that a woman's name should be included on land documents.

The Myanmar Agricultural Development Bank

While private banks do exist in Myanmar, the dominant source of agricultural lending across the country is the Myanmar Agricultural Development Bank (MADB), which accounted for

between 60 to 90% of bank lending in rural parts of the country in 2013 (Win 2013). The MADB is a government-owned entity, which is required by a 1997 statute to return 75% of its profits back to state coffers. In 2020 (prior to the 2021 military coup), the MADB operated 206 branches throughout the country and served over two million customers.

To obtain an MADB loan for a given plot, a potential recipient needed to present evidence of a Form 7, verification of a savings account at the MADB, proof of participation in a lending group,[5] and approval by a village loan screening committee (Aung, Nguyen and Sparrow 2019). Prior to the 2021 coup, the average interest rate on MADB agricultural loans was about 0.71% per month, or approximately 8.5% per annum (Luna-Martinez and Anantavrasilpa 2014). By comparison, the benchmark market interest rate in Myanmar from 2011 to 2019 was 10%.[6] Strict lending criteria, including the Form 7 requirement, ensured very high MADB repayment rates, so while the MADB provided well over 60% of total rural credit in the country, they accounted for less than 35% of outstanding loans.

Loan sizes increased incrementally with plot size, but only up to a limit. Loan recipients received 100,000 Myanmar Kyat (MMK)—about 100 USD—per acre for paddy production (and 20,000 MMK for other crops), up to a maximum of ten acres. Any plot larger than this was still only entitled to the maximum loan of one million MMK, and additional loans required a separate Form 7 in the name of a different party. Aung, Nguyen and Sparrow (2019) verify the immediate effect of this policy, showing that farms just above the ten-acre threshold received loans that were 18–24 USD per acre less than for those just below the

[5] This condition was no longer necessary at the time of our survey.

[6] https://tradingeconomics.com/myanmar/interest-rate

threshold—up to a 25% reduction in loan size. The authors do not test for differences in plot size, though they do find slightly higher household income growth above the threshold. Critically, they find no difference in the agricultural yield or income from the sale of rice on either side of the threshold (Aung, Nguyen and Sparrow 2019).

The potential for increased access to credit created a clear financial incentive for families with household plot sizes greater than ten acres to divide the land within the household, register the additional land with a new Form 7, and apply for an additional loan under the new household member's name. According to our discussions in Myanmar and anecdotal conversations, many men decided to put land in their wives' names for this purpose, as it was logistically simple and guaranteed that the asset would remain in the immediate family.

CONCEPTUAL FRAMEWORK

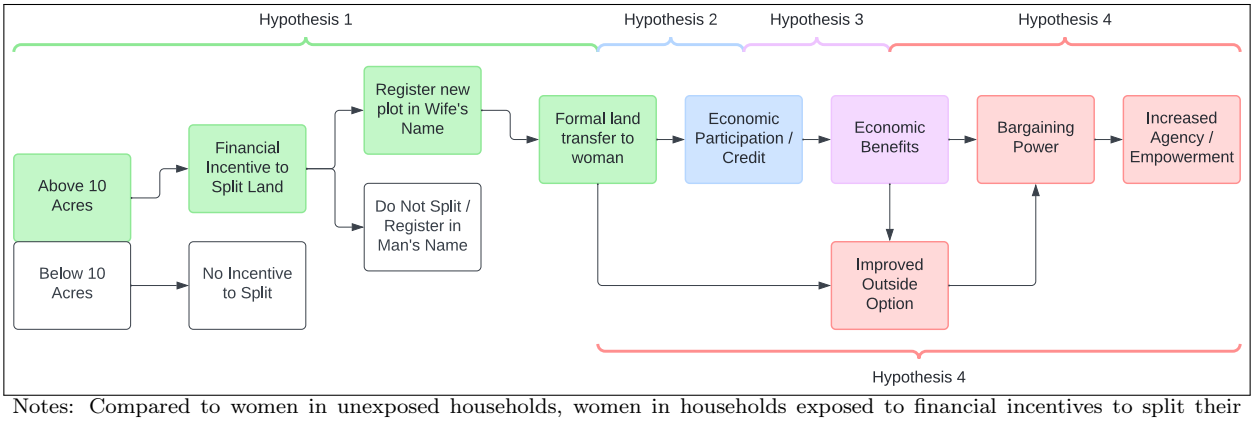
Many empirical studies have verified that household decision-making does not follow a "unitary" model, in which a single decision-maker maximizes the utility of all household members (Rangel and Thomas 2019; Chiappori et al. 2022; Lechene, Pendakur and Wolf 2022). Current theoretical frameworks seek to model intra-household power dynamics and their implications for economic and political outcomes (Browning and Chiappori 1998; Chiappori 1992; Iversen and Rosenbluth 2006), a key insight of which is that an individual's position within the household and their resultant ability to make decisions is in part dependent on their "outside option," i.e., their best alternative to remaining in the household. Recent work has shown that giving women control over their finances enhances their outside option, lead-

ing to changes in household bargaining, and ultimately, their ability to influence household decisions (Field et al. 2021; Meinzen-Dick et al. 2019; Deere and Doss 2006).

Changes to the outside option—and consequently changes to women's bargaining power—can be influenced in many ways, and in our context, we focus on the acquisition of state-provided, written, *de jure* property rights. In particular, we highlight two important features of *de jure* rights that might strongly affect women's empowerment. First, *de jure* rights may lead to higher levels of investment and create new economic opportunities for rights holders (Field 2007). Second, because *de jure* rights signify an interaction between an individual and the state, their transfer could lead to more interactions with state officials and a greater presence in public spaces that enhances the impact of any economic benefits.

We define empowerment as the ability to make decisions about one's own life (Akter et al. 2017; Kabeer 1999), our primary measure of which is based on self-reported accounts of women's involvement in various dimensions of household decision-making. Scholars of women's empowerment have recently emphasized the importance of "critical consciousness" of an individual who moves from being an object to a subject (with agency) who takes power (Donald et al. 2020). Previous work has found that households in which both partners agree on the woman's decision-making power experience significantly better development outcomes, compared to households where the woman is only allowed to exercise decision-making in a circumscribed way (Ambler et al. 2021; Bussolo, Sarma and Williams 2021). We operationalize this concept in a secondary measure of women's empowerment that compares women's and men's responses on the same dimension of decision-making (The construction of both measures is described in more detail in Appendix A).

Figure 1: *Conceptual Framework*



Notes: Compared to women in unexposed households, women in households exposed to financial incentives to split their land will report: more property rights in their name (*Hypothesis 1*); higher levels of economic participation, including more loans in their name (*Hypothesis 2*); increased economic benefits, including agricultural income (*Hypothesis 3*); higher levels of empowerment, including more participation in household decision-making (*Hypothesis 4*). Hypotheses 1 and 2 represent primary outcomes, while Hypotheses 3 and 4 represent secondary, downstream outcomes.

Figure 1 illustrates our theory of change, for which we now discuss the scope conditions that each hypothesis depends upon. We begin with the initial argument that the bank policy will induce men to register land in their wives' names (*Hypothesis 1*), requiring the basic assumption that current landowners are willing to make a large asset transfer within the household, which may not be the case (see Mani 2020). It also assumes, should this asset transfer take place, that it will be made to a woman. While we have heard anecdotally from colleagues in Myanmar that this is common, and—in the case of married couples—it seems that an asset transfer between spouses carries the least risk, there will undoubtedly be some households that choose beneficiaries that are men (e.g., sons or brothers).

Next, this transfer of *de jure* property rights should lead to more economic participation for women—as measured by the number of land-collateralized loans registered in their name (*Hypothesis 2*)—but note that this depends on how well the banking system functions, and whether titled land is an effective collateral for agricultural loans (e.g., Do and Iyer 2008 find no increase in bank lending following formal land titling).

These property rights should then also lead to economic benefits for the holder, which we define as agricultural income generated from a given plot, and any non-agricultural revenue generated by the landholder (*Hypothesis 3*). However, gaining access to credit would affect income only if credit constraints are the main barrier to agricultural productivity, and if women are successfully able to deploy this additional capital. Such a change may not happen if there are other constraints to generating economic returns (e.g. knowledge or technology), or if there are barriers to women's economic participation unaffected by property rights. For example, de Mel, McKenzie and Woodruff (2009) find that providing credit has a large effect on enterprises owned by men but no effect on those owned by women.

If property rights create economic benefits for women, we also expect an increase in bargaining power within the household, leading to empowerment (*Hypothesis 4*). However, even in the absence of functioning credit markets or the existence of non-financial barriers to women's economic activity, it is possible that formal land rights affect women's empowerment directly—by increasing the credibility of the threat of leaving the marriage. Because legal ownership of a valuable asset confers more leverage and financial independence, we may expect women who own land to be unconditionally more involved in household decision-making. This means that Hypothesis 4 can hold even if Hypotheses 2 and/or 3 do not.[7]

Finally, social norms may play an important role in determining the relationship between obtaining rights and the outcomes we predict above. For example, even if husbands want to transfer land, disapproving local officials could prevent them from doing so. Where transfers

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[7] A fifth hypothesis, concerning the effect of women's land ownership on political knowledge, attitudes, and participation, is described and tested in Appendix F.

are made, social norms may also limit the extent to which women can take advantage of the economic benefits or changes to their outside option. Our data indicates a prevalence of gender-biased views among land officials in Myanmar, which could make ownership changes difficult, and the relatively low levels of gender equality in the country suggest an environment in which property rights may not always lead to economic or social gain for women.

DATA AND MEASUREMENT

Our main source of data is a household survey conducted in the Ayeyarwaddy region of Myanmar from November 2019–January 2020.[8] The survey was conducted across 138 randomly selected village tracts in 14 out of 26 townships in the region. The sample represents rural households in a largely Burmese-speaking region of Myanmar, where women historically have had little control over land. Townships were selected as potential sites based on the availability of vacant land for a planned land-to-the-landless program.[9]

We surveyed 2,534 households across the 138 village tracts. Since we were interested in co-registration and other intra-household dynamics, our sample was restricted to households where both husband and wife were available; single-headed households were excluded. The survey team was directed to survey at least two households with land holdings greater than ten acres in each village tract, and our sampling frame also included a local leader—either the Village Tract Administrator (who oversees land issues), the Village Leader, or an influ-

[8] See Appendix C for a detailed ethics statement, which guided our survey research.

[9] Following the military coup of February 2021, the status of this program is uncertain.

ential elder—and their spouse. The majority of survey questions were asked separately to both partners, enabling a comparison of their responses and assessment of each partner's independent knowledge of household activities.

Our main estimation sample consists of 1,657 households, in which both the man and the woman report non-zero ownership of land (landless households are excluded from our analysis). The median land holding reported in our estimation sample was 8 acres, and the vast majority (88%) of households report possession of at least one Form 7, which represents formal legal title to agricultural land. Women's formal claims to land assets are very low in this setting: only 9.4% of households have any Form 7 that includes the wife's name, according to women's responses (men report this figure at 8.9%), and only 5.8% of households report any plot of land with the wife's name exclusively on the Form 7. Our main outcome variables measure access to loans, patterns of agricultural and non-agricultural economic activities, and women's empowerment. See Appendix A for details regarding the calculation of these variables and full summary statistics.

EMPIRICAL STRATEGY

Simply comparing outcomes across households in which women have formally recognized property rights, and those in which they do not, is likely to yield biased estimates. For instance, if only the most economically productive or otherwise empowered women manage to obtain formal titles to land, then our estimated association will overestimate the effects of formal land titling. On the other hand, if other family members are willing to allow land to be titled in a woman's name only if she is otherwise disempowered within the household,

then this comparison would underestimate the strength of the relationship. What we need for valid identification is a factor that leads to a greater probability of formal titling in a woman's name, but that is not correlated with the woman's own characteristics. Such a factor is provided by the discontinuity built into the MADB's lending policy.

As described earlier, households owning more than ten acres of land have a clear financial incentive to divide their land into multiple plots and register these under the name of a different member of the household (which may be the wife), to obtain additional loans from the MADB. As stated in Hypothesis 1, we therefore expect households with land holdings greater than ten acres to report a larger number of plots, a larger number of Form 7s, and a larger number of Form 7s registered in a woman's name. We can also examine whether outcomes related to economic activities and women's empowerment are systematically different in households with land holdings above ten acres (Hypotheses 2-4). However, since households with more than ten acres of land are also likely to be different than those with less than ten acres on other confounding dimensions—and these differences are likely to increase as we move further from the ten-acre threshold in either direction—we cannot trust broad comparisons between large and small landholders.

To this end, we examine the impact of financial incentives using a regression discontinuity (RD) design to determine whether there is a sudden sharp increase in the land-holding structure for households that are just above the ten-acre threshold relative to those that are just below. Specifically, we run the following regression specification:

$$Y_h = \alpha + \beta AboveThreshold_h + f(Landholding_h) + \epsilon_h \quad (1)$$

where Y_h is an outcome measure for household h, $Landholding_h$ (the "running variable") is the total land owned by household h (in acres) and $AboveThreshold_h$ is an indicator that equals one if the household owns more than ten acres of land. $f()$ is a function that controls for any continuous relationship between total land holdings and our outcomes of interest, so that we are only identifying effects that vary discontinuously at the same threshold as the bank lending policy. As has been argued in recent work, RD analyses should primarily focus on points close to the discontinuity (Stommes, Aronow and Savje 2021), so we restrict our sample to a narrow bandwidth of land holdings around the ten-acre discontinuity and use a local linear polynomial for $f()$.[10] To calculate optimal bandwidths, we use the algorithm from Calonico, Cattaneo and Titiunik (2014), which optimizes the trade-off between greater precision obtained from a larger bandwidth (that retains more observations for estimation) and greater bias generated by observations further away from the discontinuity.[11]

In Appendix D, we present several tests of the validity of our RD strategy. First, we verify that other covariates do not show discontinuities at the ten-acre threshold. Second, we assess the smoothness of the "running variable" at the threshold and conclude that the observed bunching does not reflect strategic changes to land holding in response to the bank policy. Third, we test whether we have sufficient statistical power to identify significant effects. To ensure that under-powered estimates, where they exist, are not responsible for null results,

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[10] Our coefficient estimates are also bias-corrected using a quadratic polynomial (unless otherwise noted), as described in Calonico, Cattaneo and Titiunik (2014).

[11] Note that the optimal bandwidth is not constant across outcomes due to differences in the distribution of each variable.

we implement a randomization inference (RI) approach as described in Cattaneo, Frandsen and Titiunik (2015), in which inference is based on the *sharp* null hypothesis that each individual treatment effect is zero. Compared to the standard hypothesis that the *average* effect is zero, the sharp null is easier to reject and thus provides a useful robustness check in RD models with relatively few observations. In the tables below, RI p-values are reported beneath each regression result in addition to the RD robust standard errors.

REGRESSION RESULTS

We present results of the RD analysis below, examining each of our hypotheses in turn. We discuss how the MADB's financial incentives to transfer property affect measures of land rights, economic outcomes, and women's empowerment.[12]

Land Rights

Our results provide empirical support for Hypothesis 1. As expected, having more than ten acres of land results in a significantly higher number of plots, consistent with the hypothesis that the MADB lending policy incentivizes plot divisions within the household (Table 1,

[12] All results in the main text are estimated from women's responses. Estimates derived from men's responses are generally larger in magnitude (and noisier), though the two are rarely statistically different from one another. Table E.1 presents robust, bias-corrected RD estimates for men's responses, as well as less conservative, traditional RD estimates.

column 1). Households above the ten-acre threshold also have more Form 7s, which are required for these plots to be used as collateral for MADB loans (Table 1, column 2).

Table 1: *Financial Incentives Lead to Greater Formal Property Rights for Women*

	(1) Number of Plots with nonmissing plot size	(2) Total num. of Form 7s in HH	(3) Household has at least 1 Form 7 in Wife's name	(4) HH has at least 1 Form 7 in Wife's name (exclusive)
Above 10 Acres	0.962***	0.751**	0.153*	0.150*
	(0.283)	(0.365)	(0.087)	(0.085)
RI p-Value	0.000	0.000	0.000	0.000
Control Mean	1.446	1.983	0.069	0.055
Observations	404	353	428	427
Bandwidth	2.133	2.402	2.625	2.599

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's responses. Robust standard errors are shown in parentheses, and the dependent variable for each specification is indicated in the column header. "RI p-Value" indicates alternative p-values derived from the randomization inference procedure. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure.

Most importantly, households above the threshold are 15 percentage points more likely to have at least one Form 7 with the wife's name on it. Since the Form 7 is the single legally recognized document of land ownership, this result indicates a formal transfer of property rights. In other words, women above the ten-acre threshold are more likely to have legally documented land use rights. Almost all of this increase is driven by Form 7s with the woman's name registered exclusively (signifying sole control), as would be required for the lending policy (Table 1, columns 3 and 4). The magnitude of these coefficients implies that households above the threshold own one additional plot on average, and that only one-fifth of the additional Form 7s are registered in the woman's name (Table 1, columns 2 and 3).

These results are also presented graphically in Figure E.1, where we show binned outcome values on both sides of the ten-acre threshold, with a quadratic relationship fit separately to either side. The graphs show a clear upward jump to the right of the ten-acre threshold for all measures of land holdings and property rights.

Economic Outcomes

Our analysis also supports Hypothesis 2. We find that women in households directly above the ten-acre threshold report a higher number of land-collateralized loans in their name, compared to those in households below the threshold (Table 2, column 1); this result is corroborated by men's reports (Table E.1, column 5). Both men and women also report a higher number of loans in the husband's name (Table 2, column 2; and Table E.1, column 6), but this should be expected given that the lending policy does not uniquely encourage transfers to women. Combining the coefficients on men's and women's loans indicates that households above the ten-acre threshold have one additional loan on average, consistent with the Table 1 finding of one additional plot for these same households. Thus, while the lending policy does incentivize land transfers and increased borrowing, only about a fifth of these transfers benefit women directly. Again, this is consistent with our finding from Table 1 that only a fifth of Form 7s are being registered in a woman's name.

Despite increased access to formal sector loans, we do not find any increases in economic revenue, as would have been predicted by Hypothesis 3. We examine the total revenue generated from plots with the wife's name on the associated Form 7, as well as the total revenue generated from all household plots (Table 2, columns 3 and 5). We find no significant

differences between households with more than ten acres of land and those with less. On the other hand, we do find a significant decline in total non-agricultural revenue earned by the wife, suggesting that access to land-collateralized loans (which are reserved for agricultural use) may lead to greater focus on the agricultural sector, at the expense of other types of economic activity. Figure E.2 presents lending and income results graphically.

Table 2: *Financial Incentives Affect Access to Credit But Do Not Increase Revenues*

	(1) Number of Loans in Wife's Name (Land Collateralized)	(2) Number of Loans in Husband's Name (Land Collateralized)	(3) Log Total Agricultural Revenue from Plots with Wife's Name on Form 7	(4) Log Total non-Agricultural Income from Wife	(5) Log Total Agricultural Revenue from all Plots
Above 10 Acres	0.170*	0.830***	-0.610	-2.244*	1.660
	(0.097)	(0.270)	(4.560)	(1.254)	(1.218)
RI p-Value	0.000	0.000	0.394	0.000	0.040
Control Mean	0.032	0.814	10.494	2.253	11.626
Observations	404	404	78	434	574
Bandwidth	2.188	2.163	6.161	2.945	3.362

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's responses. Robust standard errors are shown in parentheses, and the dependent variable for each specification is indicated in the column header. See Table 1 notes for full details.

Women's Empowerment

The results thus far confirm that the MADB lending policy incentivized a transfer of property rights and increased borrowing by women. Though we do not see effects on downstream economic outcomes, we now examine whether formal property rights and credit access lead directly to higher levels of women's decision-making within the household. Overall, we do not find any significant effects on these measures, falsifying Hypothesis 4. Table 3 shows the RD results for all measures of women's agency in agricultural decisions. Women in

households with more than ten acres of land measure only 0.1 standard deviations higher on the decision-making index, a gap which is statistically insignificant. We also find no significant increases in women's agency as reported by their husbands, and a negative but also insignificant change in the probability of women "taking power" within the household.

Table 3: *Financial Incentives are Unrelated to Women's Agency in Agricultural Decisions*

	(1) Index of Women's Agency in Agricultural Decisions, Wife's Response	(2) Index of Women's Agency in Agricultural Decisions, Husband's Response	(3) Agricultural Decisions index: Wife takes power	(4) Agricultural Decisions index: Husband gives power	(5) Agricultural Decisions index: Wife and husband agree
Above 10 Acres	0.099	0.444	-0.503	0.212	0.564
	(0.177)	(0.430)	(0.343)	(0.224)	(0.527)
RI p-Value	0.268	0.038	0.000	0.000	0.000
Control Mean	0.077	0.424	0.636	0.069	0.330
Observations	580	403	271	267	271
Bandwidth	3.659	2.333	1.983	1.848	1.987

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's (column 1) and men's (column 2) responses. (Columns 3–5 combine men's and women's responses.) Robust standard errors are shown in parentheses, and the dependent variable for each regression is indicated in the column header. See Table 1 notes for full details.

Similarly, we find no significant effects on women's agency regarding expenditure decisions (Table E.2), which is not surprising given the lack of effects on agricultural decision-making, where we expected effects to be more likely ex-ante. The graphical representation of all empowerment outcomes can be seen in Figures E.3 and E.4. Consistent with these null results, we also do not find any impact of formal property rights on the demand for autonomy, as measured by our demand elicitation exercise (Appendix B), nor do we find any effects on the political knowledge or views held by women (Appendix F).

Robustness Checks

We conduct three robustness checks on the interpretation of all primary results outlined above. First, we show that results are not being driven by differences between rounders and non-rounders near the discontinuity. Second, we show that our findings are unique to the policy-relevant threshold of ten acres, and are not replicated at other nearby cutoffs. Finally, we discuss issues of power and the implications of the randomization inference tests.

As discussed in Appendix D, we observe differences between individuals that report exactly ten acres of land and those that report precise amounts (Specifically, the former come from significantly larger households and are significantly less educated than the latter.). To the extent that we believe at least some of those who report exactly ten acres are *rounding down*, and therefore incorrectly assigned to the control group, we may be concerned that education and household size are confounding treatment effect estimation. As a simple check against this concern, we report all regression results from an alternative specification that includes both household size and secondary education rate as control variables (Table E.3). These results are very similar to our baseline estimates, providing reasonable assurance that our findings are not driven by differences between rounders and precise reporters.

Another potential objection to our analysis is that families with large plots may simply be more likely to subdivide. Such a tendency would call into question the unique incentive caused by the MADB policy and the validity of the counterfactual assumption we make about households directly below the ten-acre threshold. In Figures E.5 and E.6, we test the validity of the ten-acre cut-off for both property rights and loan outcome variables by re-running the RD specification at alternative cut-offs. The resulting coefficient plots demonstrate that the

effects presented in the main analysis (Tables 1 and 2) are most prominent at the ten-acre threshold. With the exception of marginally positive effects on the number of plots at the 5 and 7-acre cutoffs, there are no other significant effects on outcomes related to property rights for all integer cutoffs between 5 and 15 acres. We also document positive effects on men's borrowing at the 5 and 12-acre cutoffs, but the only significantly positive coefficient on women's borrowing is observed at ten acres. Thus, while the main RD results for two outcomes are replicated at cutoff values other than ten acres, these do not include any outcomes relating to women specifically. There may be some unrelated reason for the jump in men's borrowing at 5 acres, but the MADB policy mechanism we have identified—and its effect on intra-household property transfers—appears to be unique to the ten-acre cutoff. In Figures E.6 through E.8, we reproduce the same plots for all downstream economic and women's agency outcomes as well, showing similar null effects for most alternative cut-offs above and below the policy-relevant ten-acre threshold.

Finally, one might also be concerned that the null results reported for downstream outcomes may simply be the result of underpowered inference due to small sample size. The power calculations shown in Table D.2 indicate that we should expect about 50% of regressions to be sufficiently powered to detect effect sizes of at least 0.8 standard deviations for women's responses (at 80% power). Ex-post, comparing results from the randomization inference (RI) tests with standard inference, we find agreement in 9 out of the 13 women's outcomes presented in Tables 1 through 3—greater than the 50% prediction. In the four cases of disagreement, we fail to reject the standard null hypothesis while rejecting the sharp null of the RI test, which is an indication that regressions are not sufficiently powered to detect average treatment effects (since the sharp null is easier to reject). While this may be

a concern for the final three outcomes related to women's agency in agricultural decisions (Table 3, columns 3–5), we observe a precise zero effect in the primary decision-making index (Table 3, column 1). Though we cannot rule out the possibility that any one of our null results is underpowered, we take this as evidence that the majority of results are precisely estimated, and that the conclusions of our analysis are not biased by small sample issues.

Heterogeneity Analysis

As discussed in our conceptual framework, the fact that we observe an increase in women's land ownership—but no corresponding change in empowerment/agency—may be a product of the cultural environment in which these changes take place. In other words, if the prevailing social norms in our sample area are not favorable to progressive views on gender roles, then *de jure* tenure rights may not have *de facto* significance, and could explain the failure to validate Hypotheses 3 and 4.

To investigate this empirically, we re-run the regressions in Table 3 on separate subsamples of the data, split into regions of "low" and "high" social norms as measured by two separate survey questions administered to the local leader: (1) "Do you think it is right for women to make decisions about their household's farmland," and (2) "Do you think it is your responsibility to support more women in your village (village tract) to acquire land tenure?" Leaders who selected "Always" or "Sometimes" in response to question (1) are coded as having "high beliefs," while those who selected "Rarely" or "Never" are coded as having "low beliefs." Similarly, leaders who responded "Yes" to question (2) are assigned to a "high practices" category, while those who responded "No" are assigned to "low practices."

Table 4 reports the heterogeneous effects on women's agency for areas with low and high levels of these beliefs and practices.[13] As predicted, the full sample results obscure important differences across cultural contexts. In villages where the local leader holds "high beliefs," we see that women in households above the ten-acre threshold score significantly higher on the agricultural decisions index relative to those just below. We also observe a positive effect in villages where the local leader maintains "high practices." On the other hand, in villages reflecting low levels of either beliefs or practices, we document the same null effects as observed across the full sample. This heterogeneous effect is not observed for the remaining four outcomes, however, as these are noisier measurements of agency.

Although sample size constraints prevent us from exploring the question fully, these results strongly suggest the importance of local norms in determining how *de jure* land transfers shape intra-household power dynamics. In areas where prevailing norms cut against women's participation in agricultural decisions (Low Beliefs), or areas in which leaders do not personally support growth in women's land tenure (Low Practices), legal ownership of land through the Form 7 has no teeth. Within areas in which local norms are conducive to women's decision-making and/or local leaders support pathways to women's land ownership, the Form 7 appears to confer more *de facto* power to its holder.

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[13] The "beliefs" and "practices" measures have a low correlation coefficient of 0.136, indicating that the questions capture two distinct dimensions of local norms. This helps explain the difference in magnitude observed between the two heterogeneous effects in Table 4.

Table 4: *The Effect of Financial Incentives on Women's Agency in Agricultural Decisions Depends on Local Norms*

	(1) Index of Women's Agency in Agricultural Decisions, Wife's Response	(2) Index of Women's Agency in Agricultural Decisions, Husband's Response	(3) Agricultural Decisions index: Woman takes power	(4) Agricultural Decisions index: Husband gives power	(5) Agricultural Decisions index: Wife and husband agree
Panel A: Beliefs					
Low Leader Beliefs	-0.118	0.827	-0.709***	0.408	0.260
	(0.178)	(1.187)	(0.166)	(0.576)	(0.584)
Control Mean	0.003	0.586	0.449	0.118	0.275
Observations	184	133	93	91	93
Bandwidth	3.659	2.333	3.659	3.659	3.659
High Leader Beliefs	0.802***	1.011	-0.452	-0.046	1.728
	(0.222)	(0.660)	(0.751)	(0.047)	(1.647)
Control Mean	0.102	0.392	0.719	0.042	0.347
Observations	338	235	157	155	157
Bandwidth	3.659	2.333	3.659	3.659	3.659
Panel B: Practices					
Low Leader Practices	-0.199	1.633	-0.596	0.001	1.021
	(0.272)	(1.654)	(0.722)	(0.003)	(1.033)
Control Mean	0.042	0.971	0.649	0.135	0.243
Observations	101	72	45	45	45
Bandwidth	3.659	2.333	3.659	3.659	3.659
High Leader Practices	0.482*	0.852	-0.574	0.257	0.927
	(0.286)	(0.761)	(0.499)	(0.441)	(1.019)
Control Mean	0.069	0.315	0.615	0.050	0.342
Observations	459	319	215	211	215
Bandwidth	3.659	2.333	3.659	3.659	3.659

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients are derived from the same regressions presented in Table 3, but estimated for separate sub-samples of the data. See text for a description of the criteria used to split the sample into "high" and "low" categories. Households from 16 (6) village tracts are dropped from the Panel A (B) sample, as their respective leader did not provide an answer to the first (second) question.

CONCLUSION

In this paper, we use the (unintended) consequence of a bank lending policy as a means to examine the effects—on economic and social outcomes—of an exogenous transfer of formal

property rights to women, contributing to our understanding of how they gain power within the household. Many anti-poverty programs transfer assets or income to women (e.g., micro-finance programs often target women borrowers) and either explicitly or implicitly attempt to confer a more prominent role in decision-making. Our research explores what happens when women instead gain access to *de jure* power as the result of a government policy, moving beyond externally financed, donor-driven programs that may not be sustainable.

We show that the bank incentive does induce households to give women legal control over land, often the family's most important asset. However, *de jure* control over real assets does not lead to increases in women's economic activity, decision-making power, or autonomy—at least not over relatively short time periods. As has been documented in other work, cultural barriers to women's participation in household decision-making may constrain the impact of property transfers (Carnegie et al. 2020; Akter et al. 2017), and we see suggestive evidence of this in our data. After splitting the sample into regions of "high" and "low" gender norms, we observe positive effects on women's decision-making only in "high" areas. That we see evidence of heterogeneity at all, even within our own limited sample, demonstrates the importance of cultural context in studying the gender impacts of property rights. We would be interested to see our methods replicated in other parts of the world, where existing institutions may be more supportive of women's agency within the household.

Our findings suggest that efforts to change power dynamics in the household must move beyond simple economic interventions. Even transferring legal control of large assets directly to women is insufficient in our context. Consequently, programs that focus exclusively on legal or economic empowerment overlook the entrenched politics that limit a woman's role in decision-making. In order to help evaluate the trade-offs that both men and women face

when power is redistributed within the household, future research should explore the costs and benefits of changing the political and social dynamics of land ownership.

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Appendix

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A Measurement of Outcome Variables

Respondent Characteristics

Our main estimation sample consists of 1,657 households, in which both the man and the woman report non-zero ownership of land.[1] The average age of our survey respondents was 48 for women and 51 for men, and households consisted of 4.6 members on average.[2] Women are less educated than men: 17% of men had completed secondary school compared to only 11% of women (Table A.1, panel A). The median land holding reported in our estimation sample was 8 acres, the mean was 12 acres, the 5th percentile was 2 acres and the 95th percentile was 35 acres of land. Only 12% of households owned more than two plots. The distribution of land holdings reported by women is nearly identical to the distribution of land holdings reported by men (see histogram in Figure A.1). Note that there is a tendency to report land holdings in multiples of five, since the histogram shows distinct spikes in the frequency of reporting 5, 10, 15 or 20 acres. We address the inferential implications of this heaping in Appendix D below.

The majority of survey questions were asked separately to both partners, enabling a comparison of their responses and assessment of each partner's independent knowledge of household activities. The survey included modules on the household's land holdings and associated land rights, agricultural activities (crop patterns, input choices, revenues), other economic activities within the household (including engagement in non-farm enterprises), legal literacy, and knowledge of land rights. Several modules that implemented measurements of women's empowerment were administered only to women, with strict confidentiality protocols put in place (see Appendix C).

Formal Land Rights and Economic Outcomes

In our sample, a very high proportion (88%) of households report possession of at least one Form 7. Interestingly, more than 30% of plots with at least one Form 7 are reported to have multiple Form 7s associated with them. The most common reasons stated for such multiplicity are boundary differences between the land holder and the land authority (40%), acquisition of different parts of the plot from different owners or at different times (39%), and for the purpose of applying for multiple loans (8%).

We observe significant gender differences between men and women regarding knowledge of the household's land documentation. To the question of how many Form 7s are associated with a given plot, men answer "don't know" on about 19% of a household's plots, compared

[1] We surveyed six households in which men did not report land holding size while women did, and eight households in which women did not report land holding size while men did. These are excluded from our analysis.

[2] Because the research design is focused on owners of relatively large agricultural plots, the mean age in our analysis sample is higher than the national average.

to nearly 23% for women—a difference that is statistically significant.[3] As a result, women report 2.16 total Form 7s in the household compared to the 2.23 reported by men.

To measure the extent of women's formal land rights in our data, we use the following two variables: an indicator for whether the wife has her name on any Form 7 for any household plot (this could be co-registered with the husband or someone else), and whether the wife has only her name listed on the Form 7 for an entire plot (this reflects the woman's sole *legal* control over that plot). Women's formal claims to land assets are very low in this setting. Only 9.4% of households have any Form 7 with the wife's name attached, according to women's responses (men report this figure at 8.9%), and only 5.8% of households report any plot of land with the wife's name exclusively on the Form 7 (Table A.1, panel B).

In our analysis, we focus on indicators of economic participation for which the literature predicts improvement associated with an increase in formal access to land. These include indicators of financial access (the number of land-collateralized loans taken out by women and men separately) and indicators of economic activity and success (agricultural income generated by a given plot, non-agricultural revenue earned by the wife, and total household agricultural revenue). Overall, very few women report having taken out land-collateralized loans; the average number of such loans for women is 0.06 compared to 0.74 for men. In addition, only 21% of men and 19% of women report being engaged in any non-agricultural activities, and the revenue generated from such activities is only about 5% of the revenue from agricultural activities (Table A.2, panel A).

Measuring Women's Empowerment

We use a range of indicators to capture women's empowerment, since there is no universally accepted measure of this concept. First, we compute an index of household decision-making for agricultural activities and expenditures. We asked questions about women's involvement in decisions such as hiring agricultural labor, livestock raising, gardening, choosing crops, buying/selling/renting land, and whether decisions were made by the wife alone, jointly with another person, or wholly by another person. We code the wife as being involved in decision-making in a given domain if she reports making decisions alone or jointly with someone else (see Table A.3 for summary statistics of each component). We sum up all these components and construct a standardized index for agricultural decision-making.[4]

Based on their self-reports, we find that women are rarely involved in decisions about land transactions or livestock raising, but are more involved in decisions about gardening or hiring labor (Table A.3). Interestingly, the index of women's agricultural decision-making as reported by men is much higher than the index as reported by women (Table A.2, panel B).

Similarly, we construct a second index of household decision-making based on women's involvement in decisions relating to child care, health care, cooking, education, children's expenditure, food consumption, religious expenditure, and fertility. Based on self-reports,

[3] Here we assess statistical significance via a simple t-test comparing the mean responses of women versus men.

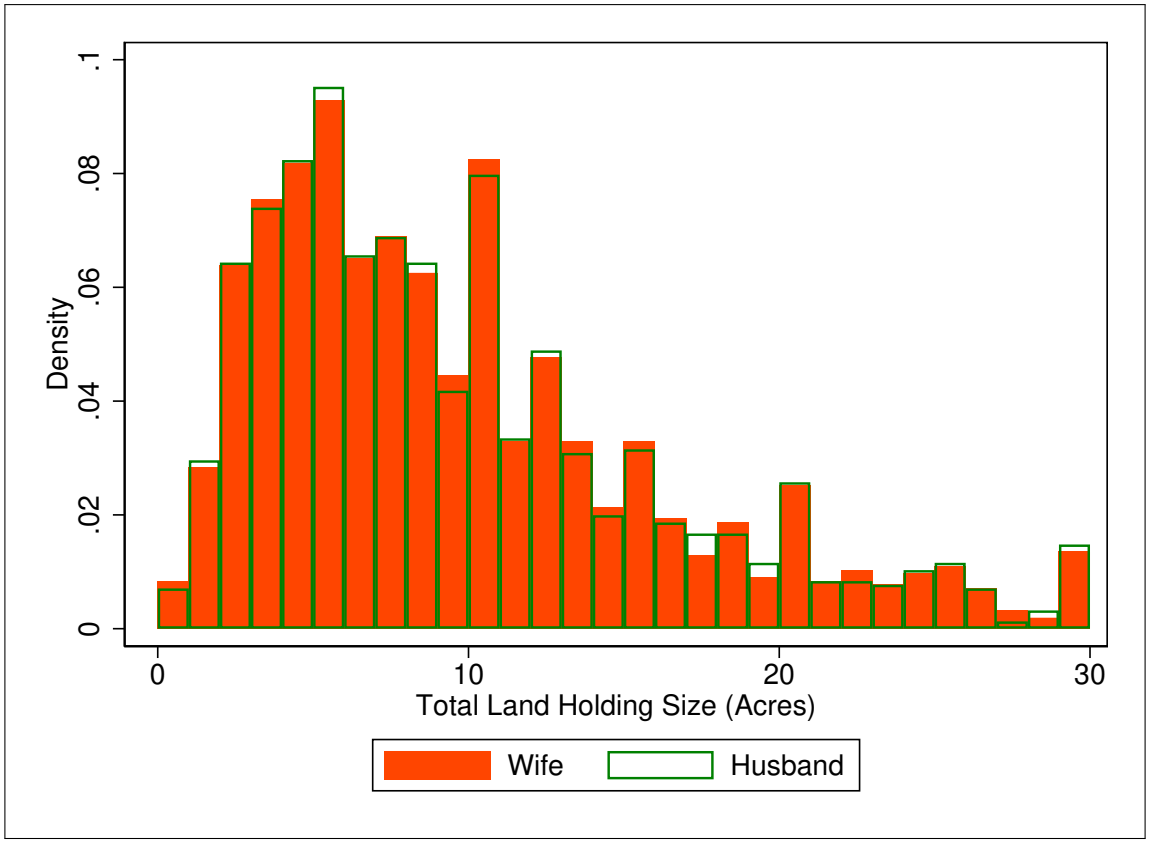
[4] The index is constructed as a standard z-score, obtained by subtracting the sample mean from each respondent's raw component sum and dividing by the sample standard deviation.

women are more involved in these decisions relative to the agricultural sphere—except for fertility decisions, where only 17% of women report being involved (Table A.3). Unlike before, where men reported higher levels of women's participation in *agricultural* decision-making, measures of women's involvement in *expenditure* decisions are considerably lower in men's reports relative to women's reports (Table A.2, panel B).

As previously discussed, in addition to having agency over household decisions, an important dimension of empowerment relates to how that agency is exercised: is power taken by the individual, or are they permitted to exercise this power by someone else? We attempt to measure this concept by categorizing women who report higher levels of decision-making authority than their partners report about them as "power-takers," and women who report less decision-making authority than their partners report about them as "power-receivers"(Donald et al. 2020). If *both* partners report that the woman is involved in decision-making, we categorize these as domains in which the husband and wife agree, while domains in which both partners report no involvement by the woman are not included in this measure. We find that, on average, both husband and wife report *no* involvement by the wife in four out of five components of agricultural decision-making, while households report power-taking behavior in two out of eight components of expenditure decision-making (Table A.2, panel B).

Finally, we conducted an elicitation exercise designed to measure women's demand for autonomy by asking how much money they would be willing to give up to retain sole control of a specific amount of money or land. This methodology is based on the idea that women with very little within-household autonomy would be willing to give up larger amounts to retain control (see details in Appendix B). Interestingly, despite the patriarchal nature of Myanmar society and limited levels of empowerment measured by the decision-making indices, we find that a sizeable fraction of women in our survey do not have a preference for sole control. However, since this measure was gathered post-treatment, after households may have already redistributed property rights, it may simply reflect satisfaction with current allocations.

Figure A.1: *Distribution of Land Holdings as Reported by Husbands and Wives*



Notes: This histogram shows the distribution of land holding assessments as reported by husbands and wives in our survey; we restrict the sample to those reporting at least 2 acres and no more than 30 acres of land. The high degree of overlap between men's and women's reports shows that there was not much discrepancy in the size of land holdings as reported by the two household heads. We also observe high probabilities of reporting land holdings in multiples of five, as shown by the spike in frequencies at 5, 10, 15 and 20 acre values.

Table A.1: *Summary Statistics on Demographics and Women's Property Rights*

	Female	Male	Total
Panel A: Demographics			
Age of Respondent	48.437	50.810	49.623
	(10.422)	(10.332)	(10.443)
Respondent has Less than Primary Education	0.047	0.040	0.043
	(0.211)	(0.196)	(0.204)
Respondent has at least Primary but Less than Secondary Education	0.785	0.722	0.753
	(0.411)	(0.448)	(0.431)
Respondent has at least Secondary Education	0.111	0.172	0.142
	(0.315)	(0.377)	(0.349)
Number of Household Members	4.564	4.563	4.563
	(1.642)	(1.642)	(1.642)
Panel B: Formal Property Rights			
Number of plots with nonmissing plot size	1.551	1.555	1.553
	(0.889)	(0.885)	(0.887)
Total number of Form 7's in hh	2.156	2.232	2.195
	(1.685)	(1.821)	(1.756)
Household has at least 1 Form 7 in Wife's name	0.094	0.089	0.092
	(0.292)	(0.285)	(0.288)
HH has at least 1 Form 7 in Wife's name (exclusive)	0.057	0.059	0.058
	(0.233)	(0.235)	(0.234)

Notes: This table shows sample means for each variable separately by gender, as well as a combined figure; standard deviations are reported in parentheses. In all figures, the sample is restricted to those who report non-zero land holdings.

Table A.2: *Summary Statistics on Economic Outcomes and Women's Empowerment Measures*

	Total	Wife	Husband
Panel A: Economic Outcomes			
Number of Loans in Wife's Name (Land Collateralized)	0.058 (0.252)	0.063 (0.265)	0.053 (0.239)
Number of Loans in Husband's Name (Land Collateralized)	0.738 (0.708)	0.710 (0.693)	0.765 (0.723)
Total Agricultural Revenue from Plots with Female Name on Form 7 (1000s MMK)	3689.738 (5103.612)	3747.073 (5352.020)	3632.402 (4856.312)
Total Paddy Revenue from Plots with Female Name on Form 7 (1000s MMK)	3070.249 (5173.931)	3083.982 (5400.424)	3056.517 (4951.652)
Total non-Agricultural Income from Wife (1000s MMK)	65.698 (349.708)	69.068 (366.594)	62.329 (332.041)
Total Agricultural Revenue from all Plots (1000s MMK)	2888.101 (7672.391)	2727.815 (4701.593)	3048.388 (9778.037)
Total Agricultural Revenue from all Paddy Plots (1000s MMK)	2061.691 (7534.968)	1910.314 (4443.200)	2213.069 (9684.915)
Panel B: Women's Agency			
Index of Female Agency in Agricultural Decisions (Standardized Sum)	0.197 (0.967)	0.106 (0.659)	0.287 (1.192)
Index of Female Agency in Expenditure Decisions (Standardized Sum)	-0.022 (0.999)	0.125 (0.787)	-0.168 (1.156)
Agriculture decisions index: Woman takes power	0.651 (0.857)		
Agriculture decisions index: Husband gives power	0.031 (0.202)		
Agriculture decisions index: Wife and husband agree	0.300 (0.646)		
Expenditure decisions index: Woman takes power	2.312 (1.842)		
Expenditure decisions index: Husband gives power	0.406 (0.872)		
Expenditure decisions index: Wife and husband agree	1.203 (1.362)		

Notes: This table shows sample means for each variable separately by gender, as well as a combined figure; standard deviations are reported in parentheses. In all figures, the sample is restricted to those who report non-zero land holdings.

Table A.3: *Components of Decision-Making Indices*

	Female	Male	Total
Panel A: Agricultural Decisions Index			
Wife Involved in Decisions About Selling/Renting/Buying Land	0.0754 (0.264)	0.261 (0.439)	0.168 (0.374)
Wife Involved in Decisions About Livestock Raising	0.0221 (0.147)	0.273 (0.446)	0.148 (0.355)
Wife Involved in Decisions About Gardening	0.646 (0.478)	0.462 (0.499)	0.554 (0.497)
Wife Involved in Decisions About Hiring Agricultural Labor	0.480 (0.500)	0.426 (0.495)	0.453 (0.498)
Wife Involved in Decisions About Choosing Crops	0.263 (0.441)	0.378 (0.485)	0.320 (0.467)
Panel B: Expenditure Decisions Index			
Wife Involved in Decisions About Child Care	0.403 (0.491)	0.457 (0.498)	0.430 (0.495)
Wife Involved in Decisions About Healthcare	0.731 (0.444)	0.616 (0.486)	0.674 (0.469)
Wife Involved in Decisions About Cooking	0.966 (0.182)	0.484 (0.500)	0.725 (0.447)
Wife Involved in Decisions About Expenditures for Education	0.529 (0.499)	0.536 (0.499)	0.533 (0.499)
Wife Involved in Decisions About other Expenditures for Children	0.558 (0.497)	0.557 (0.497)	0.557 (0.497)
Wife Involved in Decisions About Expenditures for Food Consumption	0.906 (0.292)	0.711 (0.453)	0.808 (0.394)
Wife Involved in Decisions About Religious Expenditures	0.861 (0.346)	0.732 (0.443)	0.797 (0.402)
Wife Involved in Decisions About Fertility	0.164 (0.370)	0.291 (0.454)	0.227 (0.419)

Notes: This table shows sample means for each individual component of the *Index of Women's Agency* in both *Agricultural* and *Expenditure Decisions* for men and women separately, as well as a combined figure; standard deviations are reported in parentheses. In all figures, the sample is restricted to those who report non-zero land holdings.

B Behavioral Measures of Empowerment

As part of the household survey, we collected information from a behavioral exercise designed to capture an important aspect of women's economic empowerment. Building on the work of Almas et al. (2018), we elicited the amount (price) that women would be willing to pay in order to control a small cash transfer, following the intuition that women's willingness to pay more to control additional resources decreases when their control of existing resources is greater. The exercise asked women to choose between keeping a certain sum of money for themselves (e.g., 2750 MMK) versus giving a larger sum to their spouse (e.g., 3000 MMK). This choice was repeated with different monetary amounts, until we arrived at the amount for which the woman was indifferent between keeping the smaller sum and giving away the larger sum. For instance, if a woman opts to keep 2750 MMK for herself (as opposed to 3000 MMK for her spouse), but does not prefer to keep 2500 MMK, we infer that her willingness to pay for sole control is between 250 and 500 MMK. Such an elicitation is based on the well known Becker-DeGroot-Marschak (BDM) demand elicitation method.

We find that a sizeable fraction of the women in our survey do not have a preference for sole control; 10% of women would choose to hand over the entire amount of 3000 MMK to their spouse rather than keep it for themselves and 30% of women would choose to do the same even when offered sole control over 3250 MMK (indicating a negative willingness-to-pay for autonomy). These unexpected responses do not stem from a misunderstanding of the questions asked. We repeated the entire BDM elicitation mechanism with choices over land assets rather than cash, and obtained a similar pattern of results. In fact, the correlation between the willingness-to-pay (WTP) measures for cash and for land is a statistically significant 0.62. Interestingly, the correlation of these WTP measures with the self-reported measures of women's decision making is extremely low (between -0.01 and -0.05). Finally, as seen in Table B.1 below, we note that the demand elicitation outcomes seem to be unaffected by the bank incentives examined in this paper.

Table B.1: *Financial Incentives are Unrelated to Women's Demand for Autonomy*

	(1) Amount Willing to Accept for Autonomy in Cash Game (Min)	(2) Amount Willing to Accept for Autonomy in Cash Game (Max)	(3) Amount Willing to Accept for Autonomy in Land Game (Min)	(4) Amount Willing to Accept for Autonomy in Land Game (Max)
Above 10 Acres	-270.988	-188.380	-0.589	-0.571
	(439.167)	(430.670)	(0.746)	(0.736)
RI p-Value	0.066	0.184	0.070	0.062
Control Mean	2417.918	2585.788	4.462	4.692
Observations	431	565	574	575
Bandwidth	2.741	3.035	3.363	3.429

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's responses. Robust standard errors are shown in parentheses, and the dependent variable for each specification is indicated in the column header. Alternative p-values derived from the randomization inference procedure are displayed directly beneath standard errors. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations used for the RD estimate, i.e., the number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure.

C Research Ethics

Research ethics are a critical part of any study involving human subjects. Investigators must carefully consider trade-offs between the potential costs and/or harms to research participants and the benefits that can be generated by the findings. Our research team, in collaboration with our implementation partners—Innovations for Poverty Action (IPA) and Landesa, a land rights NGO with long established connections to Myanmar government and civil society—took several steps to ensure that the research was conducted ethically.

Research team members consulted with and received feedback on all project stages (including research design, survey creation, and data collection) from their respective universities, local partners, and funding organizations. IRB approval was obtained from affiliated universities as well as IPA's own internal process, and permission for survey work was obtained from the Government of Myanmar.[5] Participants were compensated for their time in accordance with local and international standards for this type of survey research. Perhaps most critically, for survey questions that were understood to be sensitive, including questions about the dynamics of the relationship between husbands and wives, multi-part ethics protocols were followed. First, enumerators received special training on how to ask sensitive questions, which were administered privately so that women would be assured of the confidentiality of their responses. During survey administration, respondents heard the questions read aloud over headphones and input their responses directly into a tablet. Finally, strict reporting mechanisms were put in place to manage any adverse events.

[5] Survey protocols are listed in Ayeyarwaddy regional government decision No. 16 at the cabinet meeting No. (35/2019). Protocols used for this study will be made available in the interest of research transparency, and on condition that such use does not jeopardize ongoing work or create risk for any participant.

D Validity of the RD Estimation Strategy

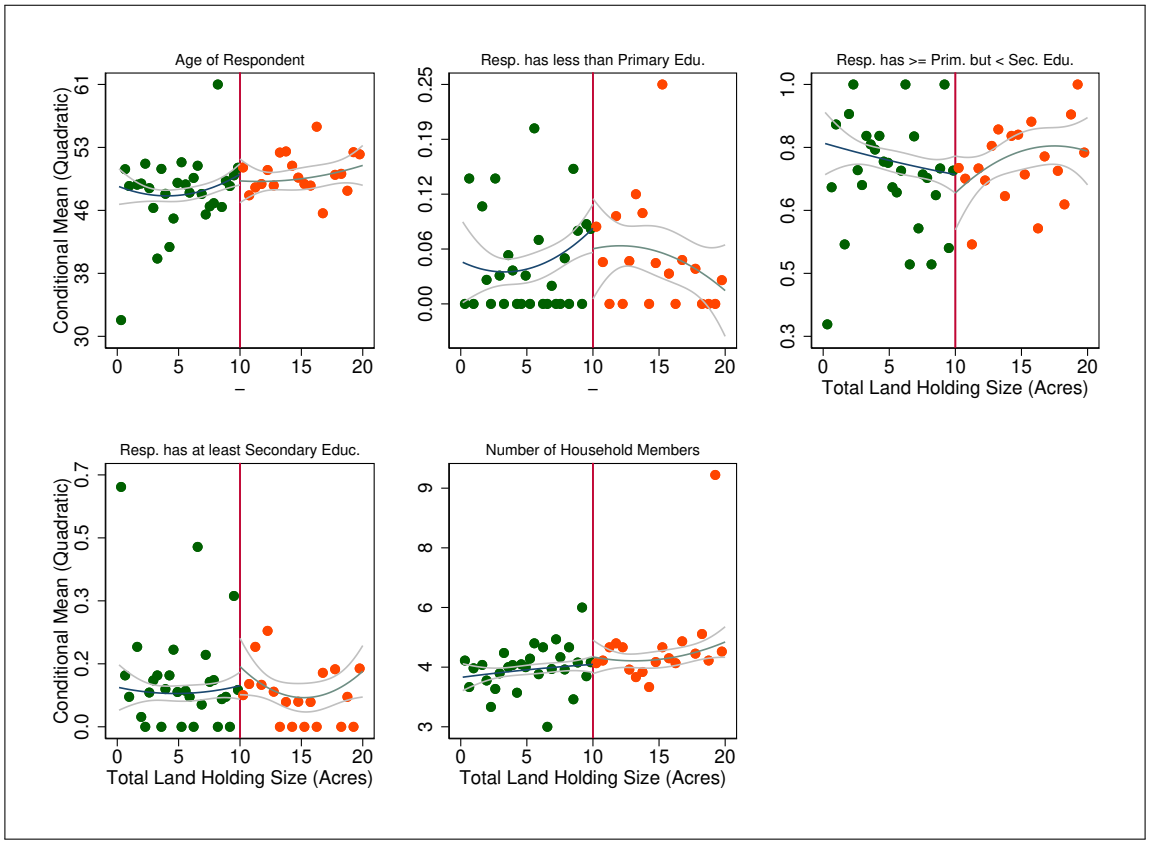
In order to make the claim that RD effect estimates are driven by behavioral responses to the MADB lending policy, we need to verify that other important factors do not show discontinuities at the ten-acre threshold. Figure D.1 illustrates how characteristics such as age, education, and household size change with land holdings. Note that these characteristics vary continuously at the ten-acre threshold, validating the assumption that our comparison is not confounded by differences in household characteristics on either side of the cutoff.

We also examine whether the "running variable" (land holding size) is smoothly distributed at the ten-acre threshold. We would be concerned if households strategically obtained (or sold) land in order to exceed (or fall below) the loan policy cutoff, but we believe this to be unlikely in our context, as it would be illogical for families to do so (indeed the MADB policy guarantees there is no financial incentive in such behavior). Following the norms in the literature, we present a McCrary density test in Figure D.2, and while the graph indicates a discontinuity in land holding at the ten-acre threshold, we do not interpret this as evidence of sorting. Rather, we believe it results from a natural bunching of self-reported land holdings at multiples of five: we can see in Figure D.2 that a similar discontinuity in land holding size distribution is visible at 5, 15, and 20 acre cutoffs as well, even though none of these are relevant to bank lending policies.

Next, since the incentive to subdivide an individual plot is only active for plots strictly larger than ten acres, households reporting exactly ten acres of land are considered to be below the RD threshold in our analysis. This may cause measurement error if some households are assigned to the "control group" (below the RD cutoff)—even though their true land holding is above ten acres—simply because of reporting a rounded number. If the decision to report a rounded number was made randomly by each respondent, then the likelihood of being incorrectly assigned to the control group would be uncorrelated with outcomes, and this misreporting would not be a confounding factor. In Table D.1, we check this condition by comparing respondents who report owning exactly ten acres (comprising both rounders and non-rounders) with respondents who report owning between 9 and 10 or 10 and 11 acres of land (i.e. comprising only non-rounders). Among the baseline characteristics previously reported in Figure D.1, we observe balance in three out of five variables, but find that non-rounders are significantly more likely to have a high school education and come from smaller households. We therefore conduct robustness tests for our main regression results by including controls for education and household size.

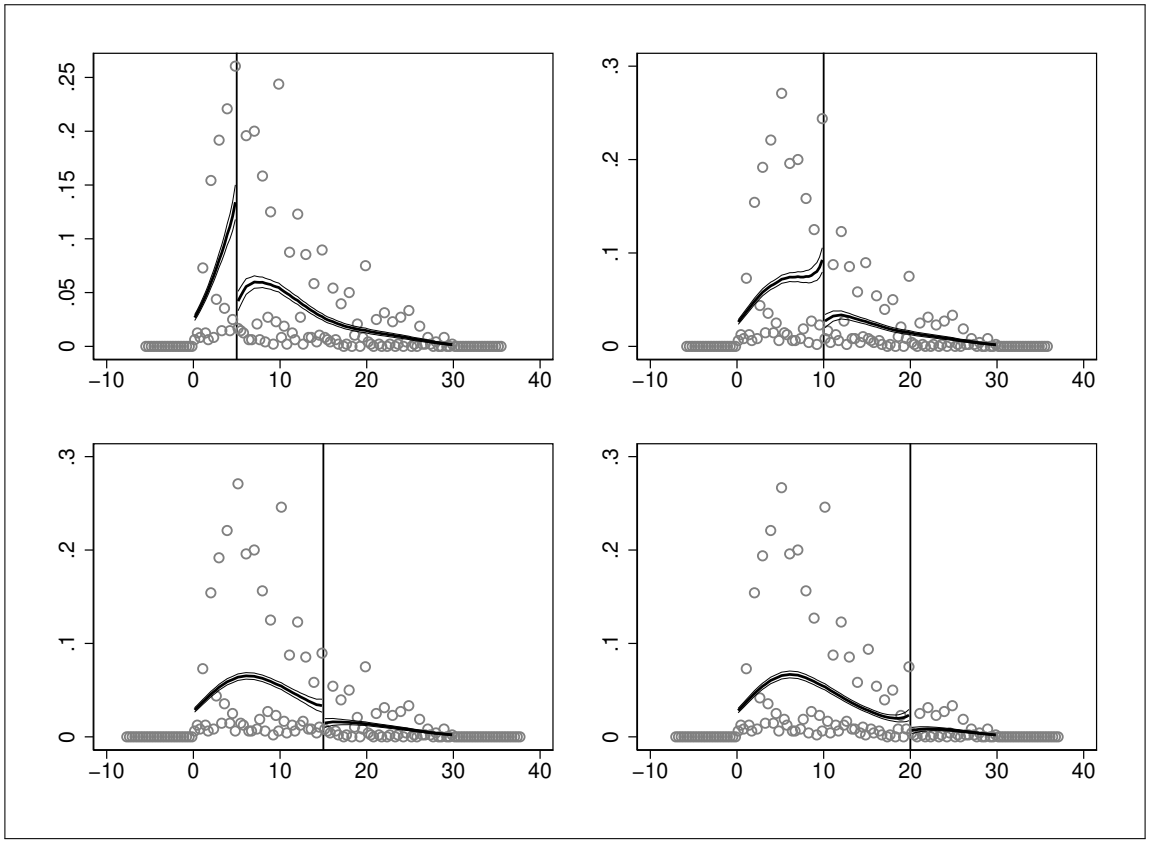
Finally, we need to confirm that our methods are sufficiently powered to identify true effects in the data and to avoid Type-II estimation errors. Following Stommes, Aronow and Savje (2021), we conduct power calculations to determine the number of observations necessary within the RD optimal bandwidth to estimate a minimum detectable effect of a given size. We find that our sample size is sufficient to detect effect sizes of 0.8 standard deviations at 80% power for 46% of our outcome variables for women respondents and 56% for men. This proportion increases to nearly 80% for women respondents after decreasing power to 60% or increasing the effect size to 1 standard deviation (see Table D.2).

Figure D.1: *Household Characteristics Above and Below the Ten-Acre Threshold*



Notes: In each figure, the conditional mean of the indicated dependent variable (based on women's responses) is plotted for bins of fixed width in the running variable (Total Land Holding Size). The horizontal red line indicates the RD cutoff at ten acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 90% confidence intervals for the best-fit lines are also indicated in gray. These figures show no evidence of discontinuities in relevant household characteristics.

Figure D.2: *Density of Land Holdings as Reported by Wives*



Notes: These figures represent McCrary style density tests for manipulation in the running variable. We plot the density of reported land holding size for women, and test whether the distribution exhibits a discontinuity at a specific cutoff. We see that the distribution exhibits statistically significant discontinuities at the 5-acre, 10-acre, 15-acre and 20-acre integer values. The estimated size of the discontinuities and associated standard errors are, respectively, -1.250 (0.136), -1.288 (0.162), -0.803 (0.203) and -1.284 (0.293). The fractions of households that report owning exactly 5, 10, 15 or 20 acres of land are 8.0%, 7.5%, 2.7% and 2.3%, respectively. We interpret these graphs as evidence that reporting of land values is bunched at integer values rather than evidence of intentional sorting.

Table D.1: *Comparison of Rounders vs. Non-Rounders*

	Non-Rounders	Rounders	Diff.
Age of Respondent	51.90	52.08	-0.183
			(1.646)
Respondent has Less than Primary Education	0.03	0.06	-0.028
			(0.034)
Respondent has at least Primary but Less than Secondary Education	0.71	0.74	-0.035
			(0.065)
Respondent has at least Secondary Education	0.24	0.12	0.126**
			(0.051)
Number of Household Members	4.09	4.65	-0.567**
			(0.229)
Observations	58	225	

Notes: *** p<0.01, ** p<0.05, * p<0.10. This table compares mean values of select characteristics between "rounders" and "non-rounders". "Rounders" are individuals that report owning exactly ten acres of land, and "non-rounders" are those that report owning between 9 and 10 or 10 and 11 (exclusive) acres of land. Standard Errors are reported in parentheses.

Table D.2: *Power Calculations*

Wives' Responses

	Power		
Effect Size	60%	80%	95%
0.1	0.00	0.00	0.00
0.2	0.00	0.00	0.00
0.5	0.12	0.00	0.00
0.8	0.77	0.46	0.08
1.0	0.77	0.77	0.42

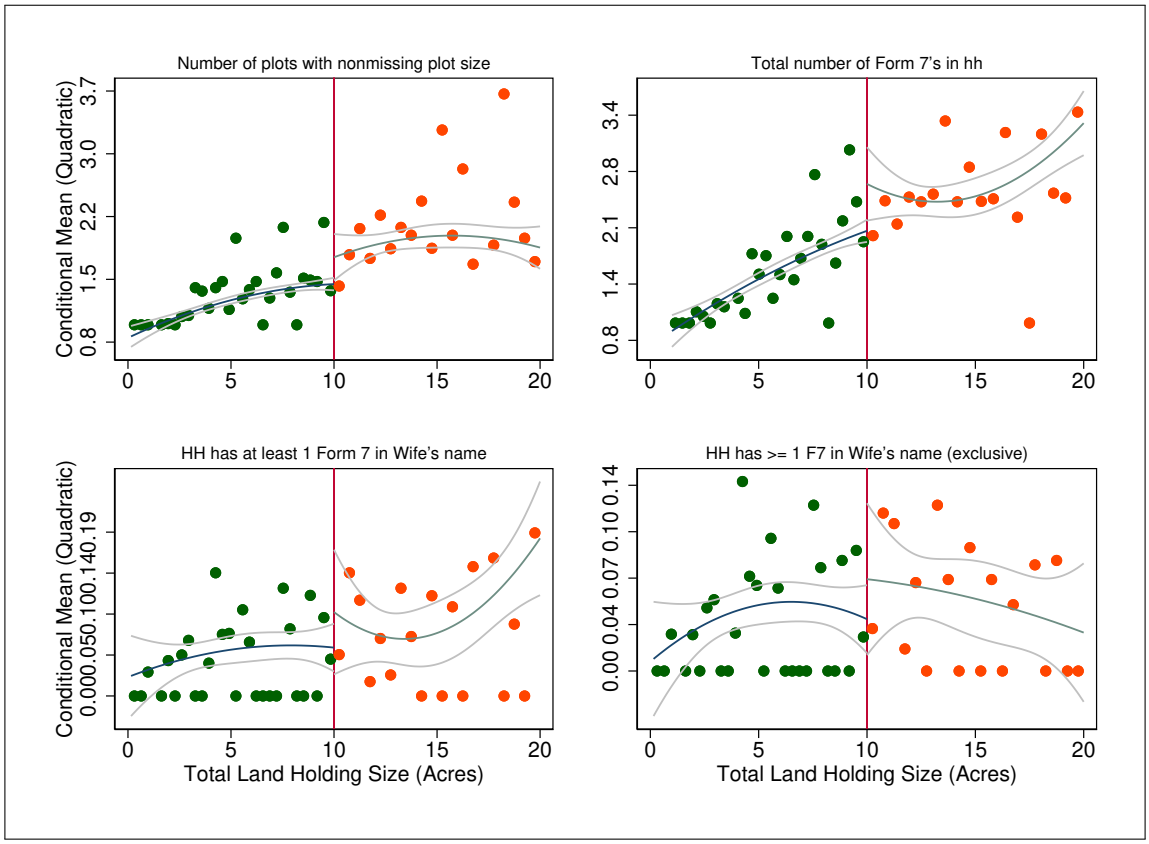
Husband's Responses

	Power		
Effect Size	60%	80%	95%
0.1	0.00	0.00	0.00
0.2	0.00	0.00	0.00
0.5	0.19	0.13	0.06
0.8	0.69	0.56	0.19
1.0	0.69	0.69	0.44

Notes: For any given effect size and power level, these tables indicate the proportion of outcome variables for which our effective sample size (as determined by the optimal bandwidth procedure in Calonico, Cattaneo and Titiunik 2014) is sufficient. Results are presented separately for Women's and Men's responses.

E Additional Figures and Tables

Figure E.1: *The Effect of Financial Incentives on Formal Property Rights*



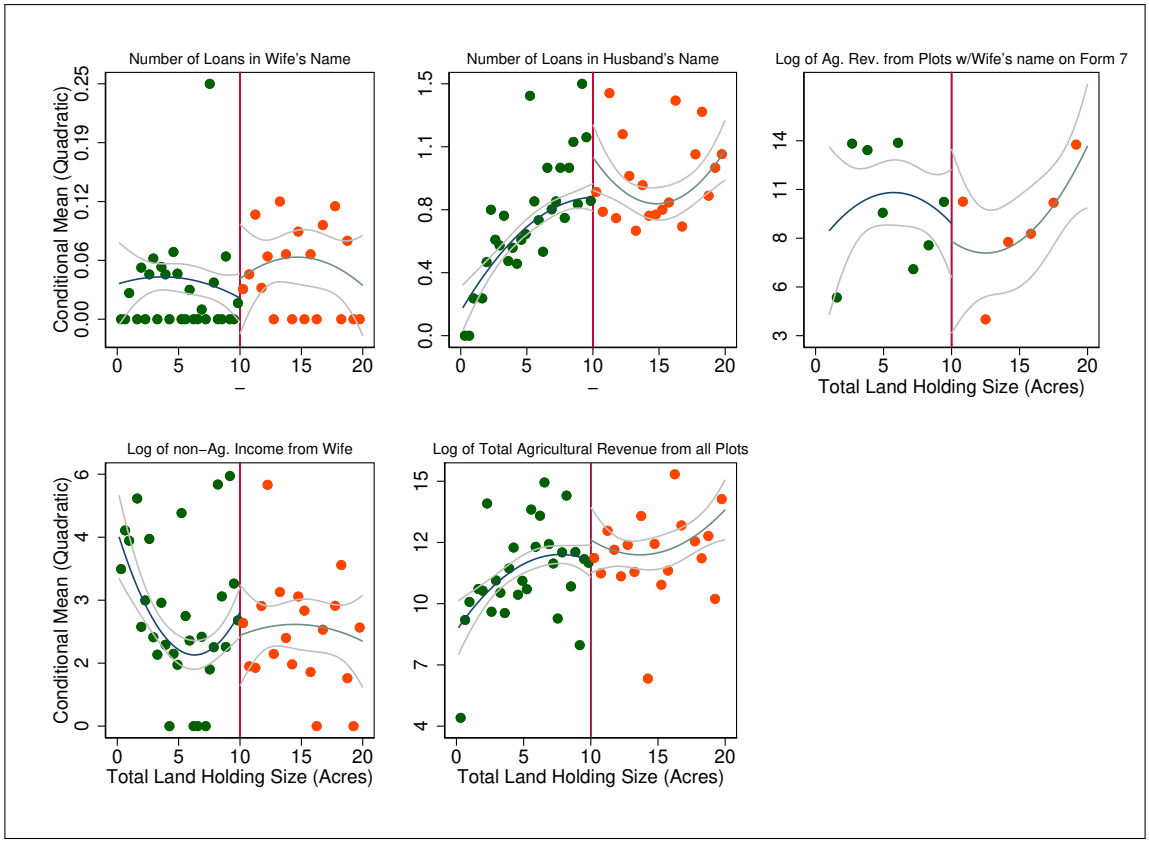
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Land Holding Size). The horizontal red line indicates the RD cutoff at ten acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 90% confidence intervals for the best fit lines are also indicated in gray. All plots are based on women's responses only.

Table E.1: *RD Results: Husband's Responses*

	Property Rights				Economic Outcomes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number of Plots with nonmissing plot size	Total num. of Form 7s in HH	Household has at least 1 Form 7 in Wife's name	HH has at least 1 Form 7 in Wife's name (exclusive))	Number of Loans in Wife's Name (Land Collateralized)	Number of Loans in Husband's Name (Land Collateralized)	Log Total Agricultural Revenue from Plots with Wife's Name on Form 7	Log Total non-Agricultural Income from Wife	Log Total Agricultural Revenue from all Plots
Conventional	0.847***	-0.078	0.207*	0.210*	0.330	0.197	-2.188	-2.084**	-0.669
	(0.238)	(0.288)	(0.110)	(0.111)	(0.225)	(0.235)	(3.268)	(0.954)	(1.230)
Bias-corrected	1.350***	0.050	0.282**	0.281**	0.536**	0.642***	2.349	-1.221	-0.438
	(0.238)	(0.288)	(0.110)	(0.111)	(0.225)	(0.235)	(3.268)	(0.954)	(1.230)
Robust	1.350***	0.050	0.282	0.281	0.536	0.642*	2.349	-1.221	-0.438
	(0.331)	(0.436)	(0.234)	(0.235)	(0.472)	(0.343)	(4.186)	(1.281)	(1.792)
RI p-Value	0.000	0.688	0.000	0.000	0.000	0.052	0.186	0.000	0.222
Control Mean	1.438	2.032	0.048	0.045	0.051	0.888	12.197	2.235	12.338
Observations	404	353	428	427	404	404	78	434	574
Bandwidth	2.133	2.402	2.625	2.599	2.188	2.163	6.161	2.945	3.362

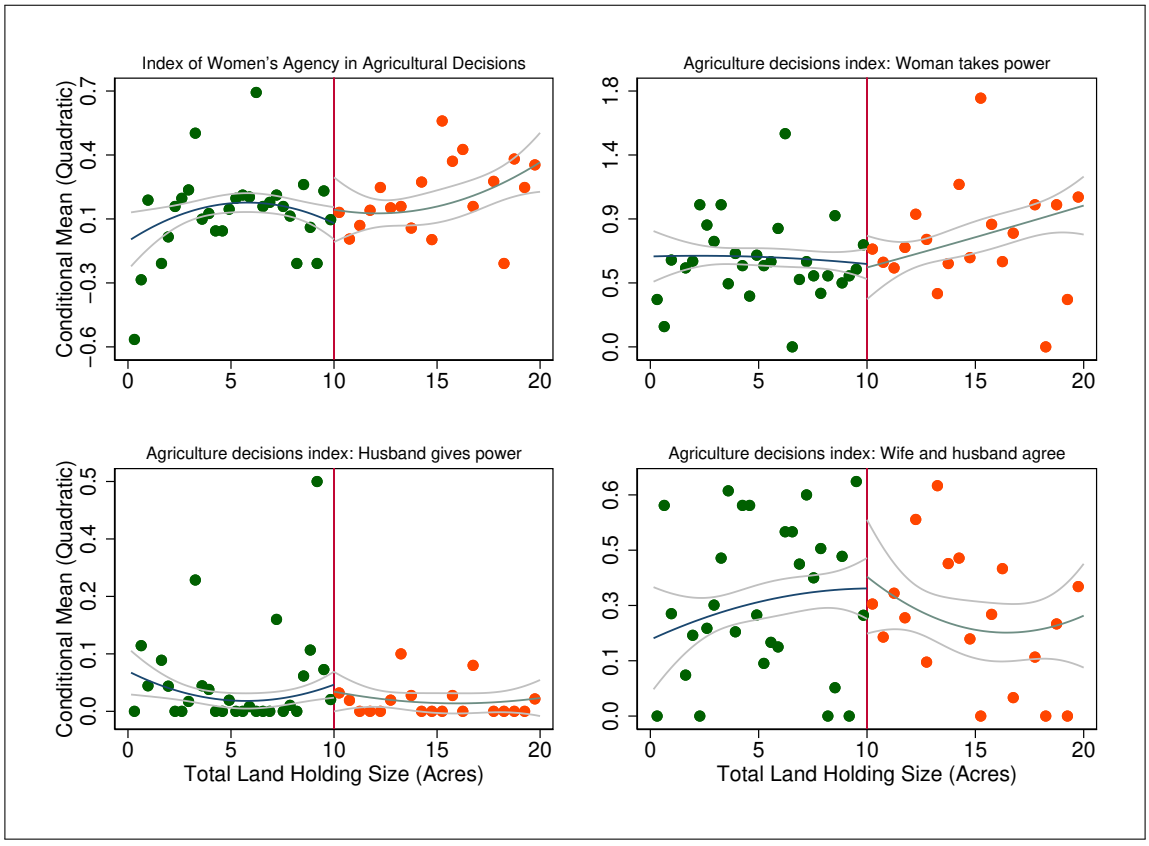
Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the RD effect estimates—with a cutoff defined at ten acres—for men's responses. Since men's responses provide noisier measures than women's responses, we present less conservative RD estimates in addition to the robust estimates reported in all other tables in the paper, i.e., estimate procedure (iii) as described below. The following estimates are reported: (i) conventional RD estimates with conventional variance estimator; (ii) bias-corrected RD estimates with conventional variance estimator; (iii) bias-corrected RD estimates with robust variance estimator. (See Calonico, Cattaneo and Titiunik 2014 for more details.) Standard errors are shown in parentheses, and alternative p-values derived from the randomization inference procedure are displayed beneath the results. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations used for the RD estimate, i.e., the number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure.

Figure E.2: *The Effect of Financial Incentives on Loan and Economic Outcomes*



Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Land Holding Size). The horizontal red line indicates the RD cutoff at ten acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 90% confidence intervals for the best-fit lines are also indicated in gray. All plots are based on women's responses only.

Figure E.3: *The Effect of Financial Incentives on Women's Agency: Agricultural Decisions*



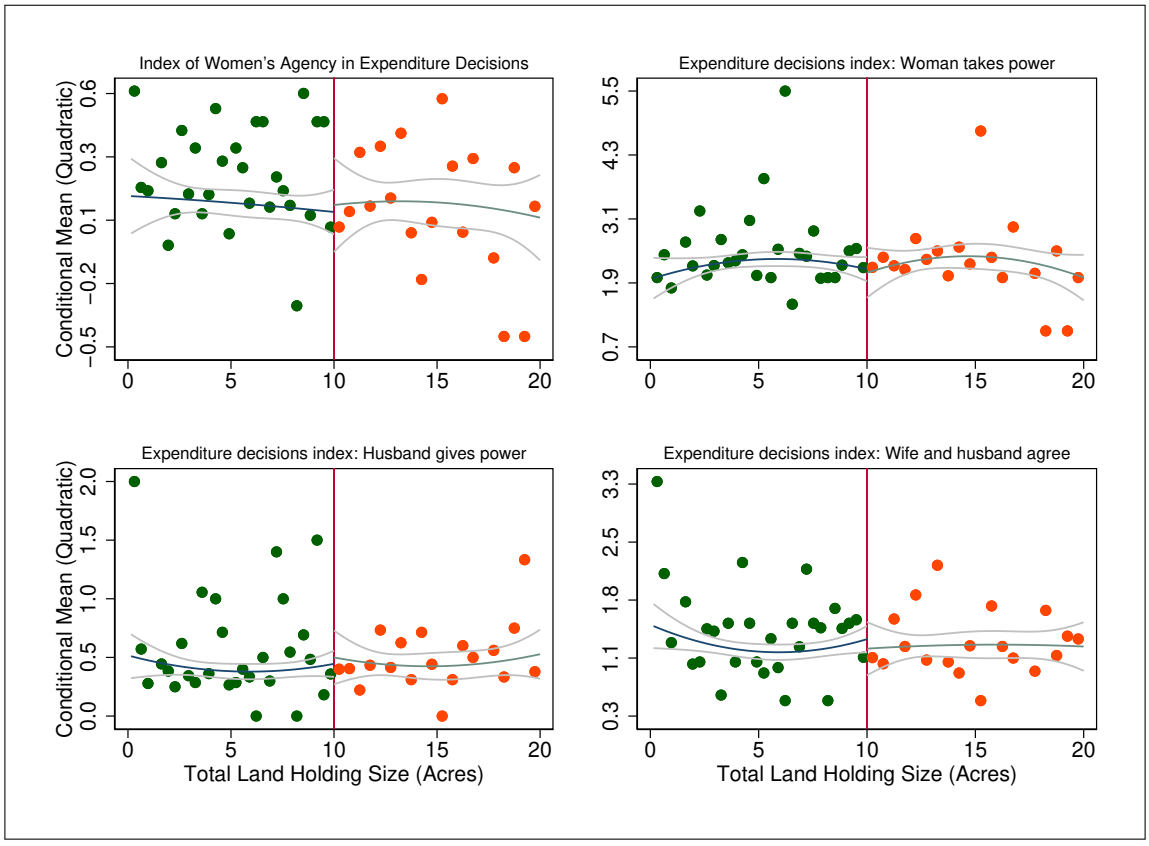
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Land Holding Size). The horizontal red line indicates the RD cutoff at ten acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 90% confidence intervals for the best-fit lines are also indicated in gray. The aggregate index for agricultural decisions presented in the first panel is based on the women's responses only. The remaining power indices are defined from a combination of men's and women's responses.

Table E.2: *Financial Incentives are Unrelated to Women's Agency in Expenditure Decisions*

	(1) Index of Women's Agency in Expenditure Decisions, Wife's Response	(2) Index of Women's Agency in Expenditure Decisions, Husband's Response	(3) Expenditure Decisions index: Wife takes power	(4) Expenditure Decisions index: Husband gives power	(5) Expenditure Decisions index: Wife and husband agree
Above 10 Acres	-0.056	0.390	-0.109	0.830	0.290
	(0.298)	(0.404)	(0.563)	(0.521)	(0.569)
RI p-Value	0.902	0.040	0.838	0.000	0.248
Control Mean	0.103	-0.117	2.236	0.415	1.282
Observations	431	396	569	270	403
Bandwidth	2.731	2.153	3.170	1.890	2.074

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women (column 1) and men (column 2) responses. (Columns 3–5 are derived from a combination of men's and women's responses.) Robust standard errors are shown in parentheses, and the dependent variable for each specification is indicated in the column header. Alternative p-values derived from the randomization inference procedure are displayed directly beneath standard errors. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations used for the RD estimate, i.e., the number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure.

Figure E.4: *The Effect of Financial Incentives on Women's Agency: Expenditure Decisions*



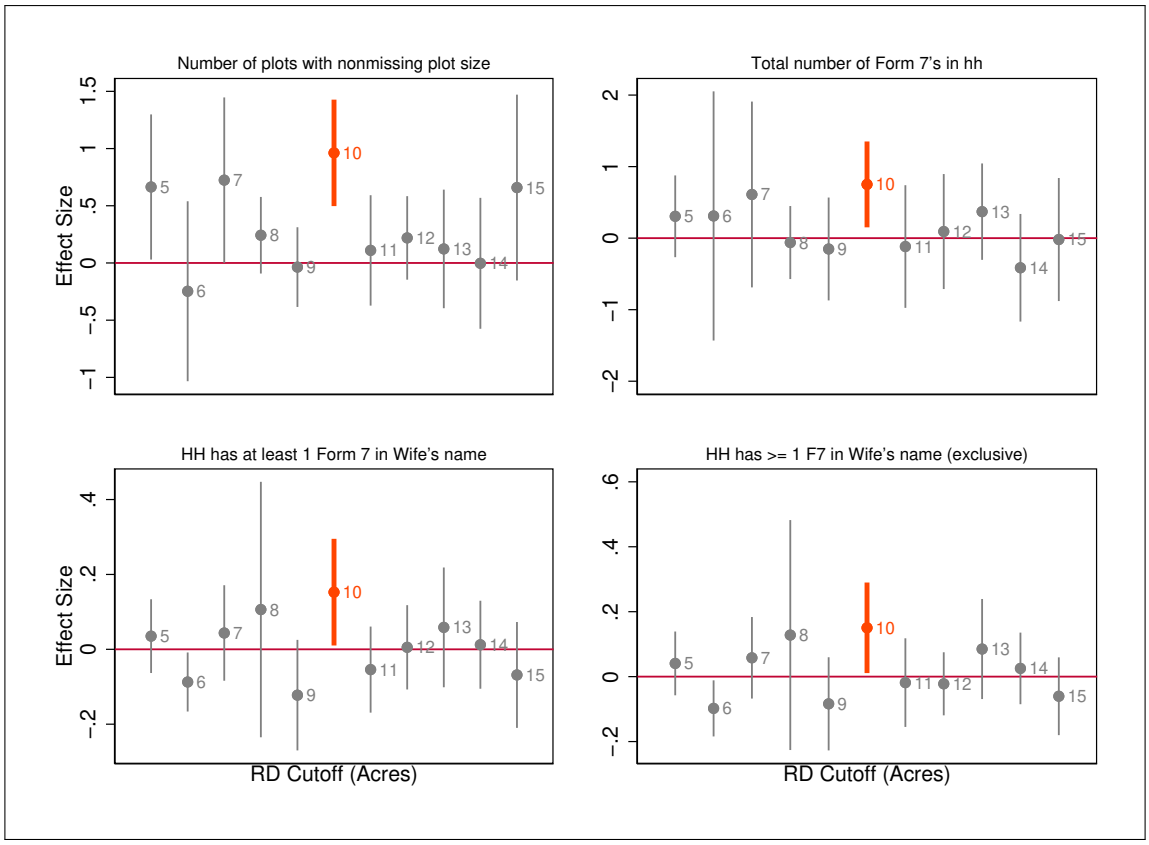
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Land Holding Size). The horizontal red line indicates the RD cutoff at ten acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 90% confidence intervals for the best-fit lines are also indicated in gray. The aggregate index for expenditure decisions presented in the first panel is based on the women's responses only. The remaining power indices are defined from a combination of men's and women's responses.

Table E.3: *Robustness Check: Controlling for Education and Household Size*

	Property Rights				Economic Outcomes					Agency in Ag. Decisions				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Above 10 Acres	1.010***	0.940***	0.140*	0.136*	0.168*	0.860***	-1.590	-2.394*	1.665	0.084	0.443	-0.487	0.198	0.479
	(0.287)	(0.355)	(0.084)	(0.081)	(0.094)	(0.279)	(4.447)	(1.228)	(1.215)	(0.175)	(0.434)	(0.345)	(0.221)	(0.508)
Control Mean	1.446	1.983	0.069	0.055	0.032	0.814	10.494	2.253	11.626	0.077	0.424	0.636	0.069	0.330
Observations	404	353	428	427	404	404	78	434	574	580	403	271	267	271
Bandwidth	2.133	2.402	2.625	2.599	2.188	2.163	6.161	2.945	3.362	3.659	2.333	1.983	1.848	1.987

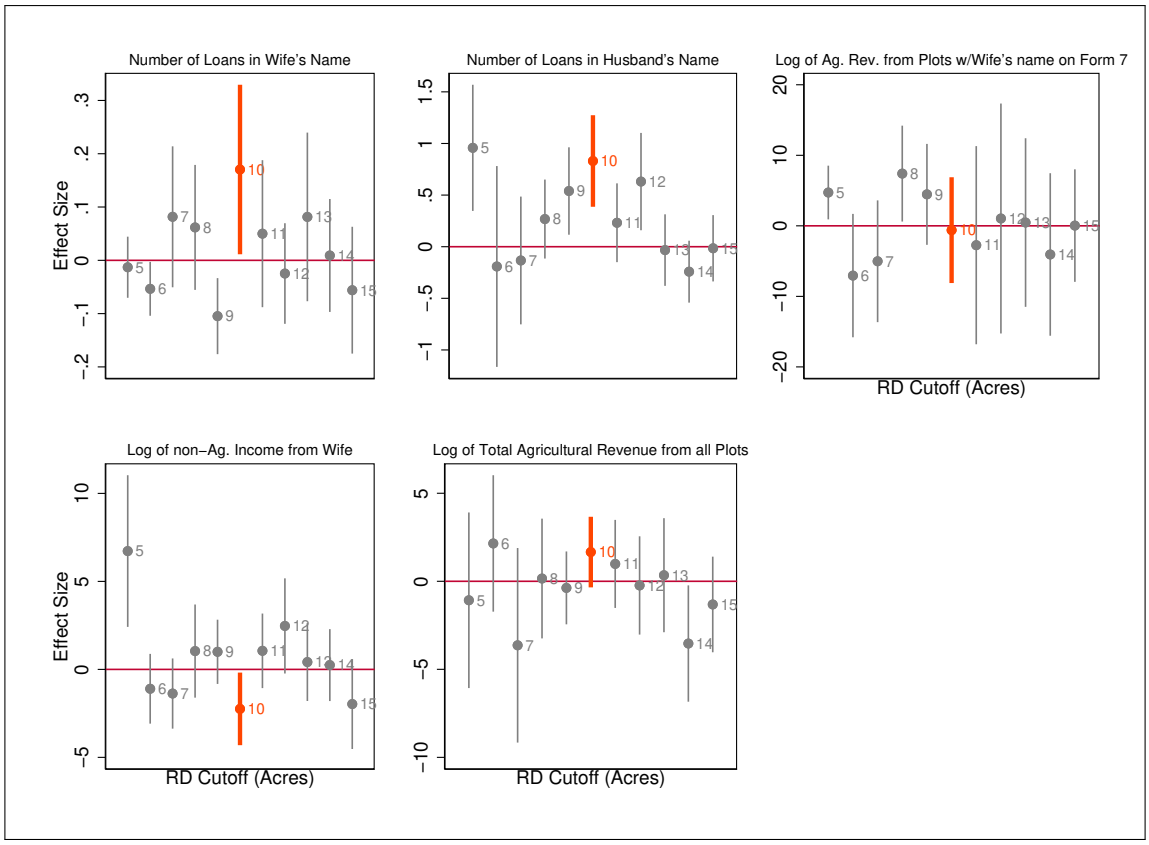
Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's responses (note that column 11 is actually based on men's responses, while Columns 12–14 are derived from a combination of men's and women's responses.) All regressions include controls for secondary education rate and household size. Robust standard errors are shown in parentheses beneath each regression coefficient. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations used for the RD estimate, i.e., the number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure. The order of regression models follows that of the paper's main tables, with each number indicating a different dependent variable, as follows: (1) Number of Plots with nonmissing plot size; (2) Total num. of Form 7s in HH; (3) Household has at least 1 Form 7 in Wife's name (joint); (4) HH has at least 1 Form 7 in Wife's name (excl. and no other man); (5) Number of Loans in Wife's Name (Land Collateralized); (6) Number of Loans in Husband's Name (Land Collateralized); (7) Log Total Agricultural Revenue from Plots with Wife's Name on Form 7; (8) Log Total non-Agricultural Income from Wife; (9) Log Total Agricultural Revenue from all Plots; (10) Index of Women's Agency in Agricultural Decisions, Wife's Responses; (11) Index of Women's Agency in Agricultural Decisions, Husband's Responses; (12) Agricultural Decisions index: Wife takes power; (13) Agricultural Decisions index: Husband gives power; (14) Agricultural Decisions index: Wife and husband agree.

Figure E.5: *Coefficient Plots for Land Outcomes*



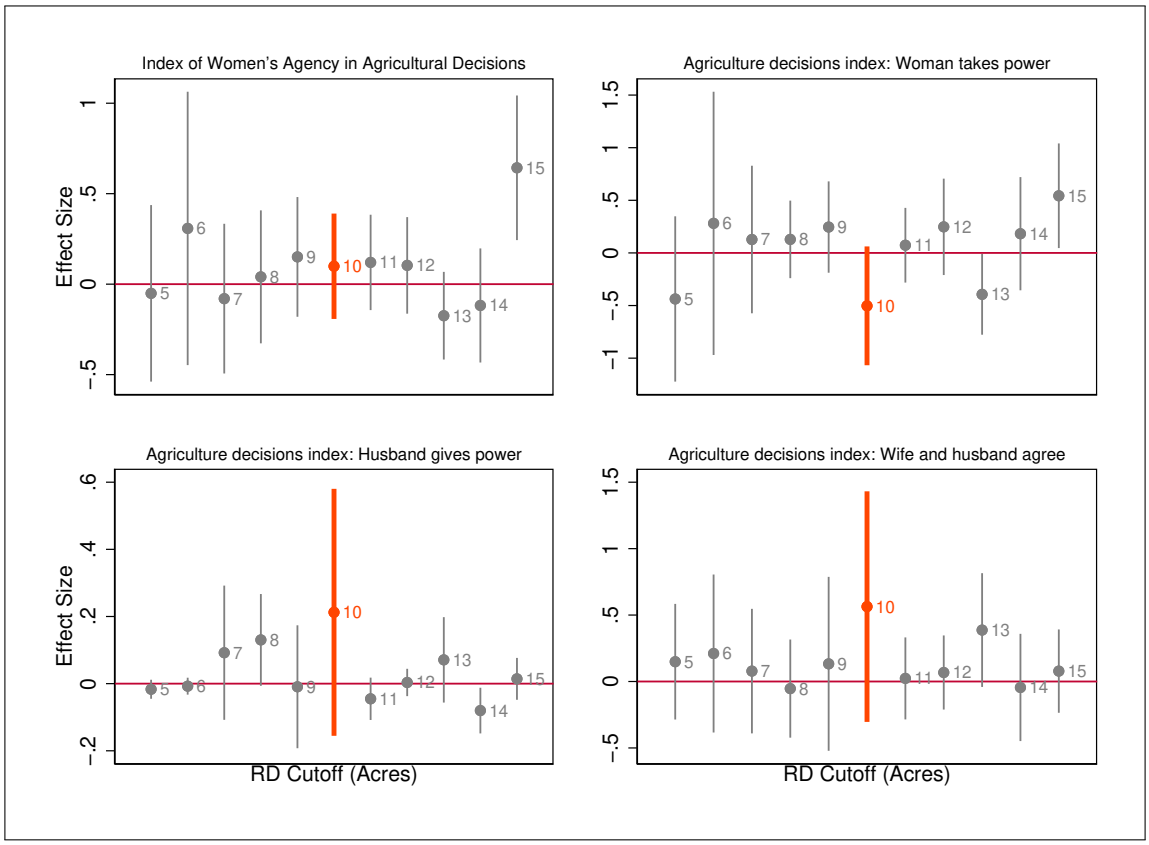
Notes: Each figure plots the RD Effect for the indicated outcome (β_{RD} from Equation 1) using 11 different cut-off points in the running variable (Total Land Holding Size), ranging from 5 acres to 15 acres. For each different cut-off value, the point estimate for β_{RD} is plotted along with the 90% confidence interval. All results are based on women's responses only.

Figure E.6: *Coefficient Plots for Loan and Economic Outcomes*



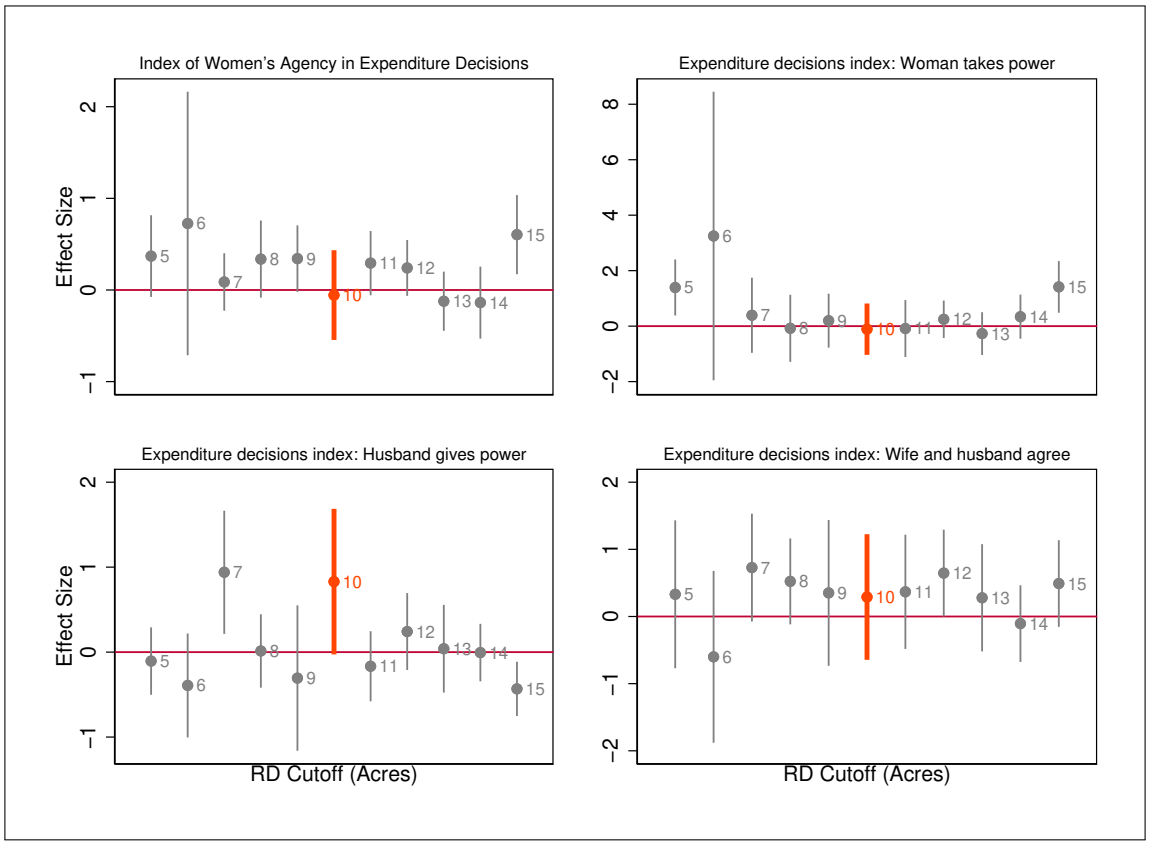
Notes: Each figure plots the RD Effect for the indicated outcome (β_{RD} from Equation 1) using 11 different cut-off points in the running variable (Total Land Holding Size), ranging from 5 acres to 15 acres. For each different cut-off value, the point estimate for β_{RD} is plotted along with the 90% confidence interval. All results are based on women's responses only.

Figure E.7: *Coefficient Plots for Agency Outcomes: Agricultural Decisions*



Notes: Each figure plots the RD Effect for the indicated outcome (β_{RD} from Equation 1) using 11 different cut-off points in the running variable (Total Land Holding Size), ranging from 5 acres to 15 acres. For each different cut-off value, the point estimate for β_{RD} is plotted along with the 90% confidence interval. The aggregate index for expenditure decisions presented in the first panel is based on the women's responses only. The remaining power indices are defined from a combination of men's and women's responses.

Figure E.8: *Coefficient Plots for Agency Outcomes: Expenditure Decisions*



Notes: Each figure plots the RD Effect for the indicated outcome (β_{RD} from Equation 1) using 11 different cut-off points in the running variable (Total Land Holding Size), ranging from 5 acres to 15 acres. For each different cut-off value, the point estimate for β_{RD} is plotted along with the 90% confidence interval. The aggregate index for expenditure decisions presented in the first panel is based on the women's responses only. The remaining power indices are defined from a combination of men's and women's responses.

F Analysis of Political Outcomes

The existing literature on democratization and political participation finds that historically, greater land equality is related to increased demands for democracy (Ansell and Samuels 2010; Albertus 2015). To examine whether formal property rights can influence the political participation of women, we explore five basic measures of political knowledge and behavior: (1)–(2) whether respondents could name political leaders in the Ayerarwaddy regional government (the Chief Minister and the Village Tract Administrator), (3) whether they were planning to vote in the upcoming national election, (4) whether they thought democratic processes were preferable to other forms of government, and (5) their general satisfaction with democracy in Myanmar.[6] Overall, we find little to no effects. While we do not observe any evidence that women in households above the ten-acre threshold are more politically aware or have different democratic preferences, we note that men in such households are more likely to know the name of the village tract administrator (VTA). One possible explanation of this result is that men spend time interacting with local officials, including the VTA, while transferring land titles to their wives. The fact that we observe this effect for men—and not women—suggests that the administrative process of land registration is handled primarily by the benefactor rather than the beneficiary of the transfer. Given that we do not find strong effects of *de jure* property rights transfers on other economic or empowerment outcomes, this set of largely null results for women's political engagement is not unexpected.

[6] The latter question elicited responses based on an integer scale ranging from 1 ("Very Satisfied") to 4 ("Not at all satisfied").

Table F.1: *Financial Incentives are Unrelated to Political Outcomes*

	(1) Name CM of Ayeyarwaddy correctly	(2) Name VTA correctly	(3) Plan to vote in upcoming National election	(4) Dem. always preferable to any other gov.	(5) Satisfaction with democracy in Myanmar
Panel A: Wife	0.004	0.010	-0.119	0.163	-0.023
	(0.063)	(0.073)	(0.091)	(0.119)	(0.143)
RI p-Value	0.496	0.872	0.000	0.022	0.478
Control Mean	0.058	0.943	0.977	0.442	1.849
Observations	583	568	569	565	672
Bandwidth	3.795	3.116	3.163	3.027	4.240
Panel B: Husband	-0.138*	0.060***	-0.132	-0.078	0.311
	(0.083)	(0.021)	(0.148)	(0.254)	(0.333)
RI p-Value	0.256	0.000	0.000	0.406	0.302
Control Mean	0.142	0.964	0.992	0.614	1.678
Observations	583	568	569	565	672
Bandwidth	3.795	3.116	3.163	3.027	4.240

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at ten acres—for women's and men's responses separately. Robust standard errors are shown in parentheses, and the dependent variable for each specification is indicated in the column header. Alternative p-values derived from the randomization inference procedure are displayed directly beneath standard errors. "Control Mean" is defined as the average of the dependent variable for observations between the lower limit of the RD bandwidth and RD cutoff, while "Observations" indicates the total number of observations used for the RD estimate, i.e., the number of observations that fall within the chosen bandwidth. "Bandwidth" reports the size of the RD bandwidth (in acres), as calculated by the CCT optimal bandwidth procedure.

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