

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 more formal property rights? Can formal property ownership improve women's economic outcomes and change political dynamics within the household? To investigate these questions, we take advantage of a bank lending policy in Myanmar that motivates men in 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-holding, economic activity, and gendered decision-making. We find that financial incentives provided by bank lending policies led to increases in women's formal property ownership, but these exogenously assigned rights did not manifest into greater economic empowerment or decision making power for recipients.

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1. Introduction

Women are economically and socially disadvantaged in many parts of the world. A critical driver of gender inequality is women’s limited access to land and other assets: 61% of working women are employed in agriculture in least developed states (ILO 2019), but women hold only 18% of global agricultural land (FAO 2022). Recognizing these concerns, the U.S. government spent \$48 billion on aid programs aiming to improve women’s empowerment in 2017-18 (OECD 2020), yet policymakers’ knowledge about both the effectiveness and unintended consequences of such policies is still limited. In this paper, we explore what happens in the presence of financial incentives to subdivide household land and obtain additional formal land certificates in the name of a female household member. Will such incentives serve to provide access to formal property ownership for women, thereby improving economic outcomes as well as shifting power dynamics within the household? Assessing the relationship between women’s property rights and economic and political power is fundamental to understanding gender inequality (Moghadam 1990). However, a systematic review of the literature finds that most empirical studies of land and women’s empowerment are limited by small sample sizes and lack of credible counterfactuals (Meinzen-Dick et al. 2019) .

Our study focuses on a national banking policy to explore the empirical consequences of access to formal property rights for women in Myanmar, a country with considerable economic gaps between men and women. Myanmar’s agricultural census of 2003 found that

women comprised only 15% of landholders. In 2020, 76% of men participated in the labor force compared with 46% of women (World Bank 2020), and 19% of women in Myanmar had experienced intimate partner violence (WHO 2021). These power differentials have direct implications for local and national politics: in 2016, Myanmar citizens elected local village tract officials for the first time, and 95% of successful candidates were men. At the same time, the Myanmar state has relatively high levels of property tax collection, and most citizens recognize the importance of written documentation of property rights (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 a much more likely case for formal property rights to have an impact, compared to places with more limited documentation such as sub-Saharan Africa (e.g., Huntington and Shenoy (2021)).

We take advantage of a quasi-experiment created by a policy of the Myanmar Agricultural Development Bank (MADB), the country’s dominant rural lender, which capped the amount of land eligible for agricultural lending at ten acres. Families with plot holdings larger than ten acres now had an incentive to break apart the plot and formally register the surplus land in the name of a different household member, most often their female spouse, in order to access additional bank credit. Based on the existing literature, we theorize that this financial incentive could increase women’s formal property ownership and their access to bank credit; in turn, these factors could increase their economic returns or productivity and also confer greater agency in household decision making. However, each of these steps is subject to important scope conditions that include consideration of social norms and intra-family bargaining dynamics (see Section 2).

To test our hypotheses, we use a regression discontinuity design (RDD) to compare households with plot sizes just below 10 acres (who have no incentive to divide the plot), to those just above this threshold. Our data comes from a survey we conducted in November 2019 through January 2020. We surveyed male and female partners in 2,534 households across 128 villages of Ayeyarwaddy region in Myanmar. We collected information from both partners in each household on a wide range of demographic and economic factors, as well as on household decision-making processes, perceptions of agency, social norms, and women’s willingness to pay for monetary autonomy.

We find strong evidence for a financial incentive to transfer formal *de jure* property rights to women: households that had more than ten acres reported a significantly larger number of individual plots in the household and were 13 percentage points more likely to have at least one land title (known as a Form 7) with the female household head’s name on it. Households with more than 10 acres also reported a higher number of land-collateralized loans. Despite these important improvements, we find that property rights were not linked to differences in economic or social decision-making in the household, nor did property rights lead to changes in political knowledge or attitudes for women.

Our findings that *de jure* rights do not lead to empowerment have several implications. First, supplying formal property rights to women does not necessarily change their bargaining position within the household, perhaps because long-standing social norms do not change in response to these formal rights. Future work should focus on designing policies that both encourage inclusive property rights and seek to shift changes in intra-household power dynamics. Second, exploratory analysis of the impact of incentives to formalize women’s

property rights on political participation also show no effect. Shifting a patriarchal equilibrium in property rights and politics requires more than financial incentives.

Our work contributes to the literatures on property rights and the empowerment of women. An extensive literature in economics emphasizes the importance of formal property rights for investment and labor market decisions (De Soto 2001; Field 2007; Galiani and Schargrodsky 2010). Most of this literature focuses on formalizing property rights at the level of the household without consideration of women’s rights specifically. In most patriarchal societies, restrictions on ownership and control of immovable property, such as land and houses, is restricted to men (Agarwal 1995). Many studies have documented the inability of spouses to have their names included on household property and land titles (Brown 2003; Savath, Fletschner and Santos 2015). As a result, in many parts of the world, women have not been able to use household property to seek individual loans or start businesses that might provide them with economic autonomy. Our setting provides a unique opportunity to study the consequences of assigning property rights specifically to women.

Formal property rights may also affect women’s empowerment, which is defined as the ability to take decisions about one’s own life. These benefits may follow directly from the economic returns to formal property rights (Agarwal 1997), or from non-economic changes, including changes in the allocation of time, social status, perceptions of self-efficacy, reductions in vulnerability to violence, or changes in decision-making within the household (Panda and Agarwal 2005; La Ferrara and Milazzo 2017; Harari 2019). In contrast to these prior studies, we examine a situation where households have a financial incentive to title land in women’s names, without a change in the overall legal system or conferring new property

rights to the household as a whole. Such voluntary changes may improve women’s empowerment more than a mandated legal change that may engender backlash. For example, Roy (2015) finds that improving women’s land inheritance rights does not necessarily lead to more property rights for women in India, and may even lead to female foeticide (Bhalotra, Brulé and Roy 2020). Prior studies have focused on economic initiatives targeted towards increasing women’s empowerment. These include cash transfers, microfinance initiatives, financial access and business training programs (Duflo 2003; Almas et al. 2018; Ashraf et al. 2020; Field et al. 2021). Many such initiatives have focused on NGO and other donor-led programs, where the role of external actors can make it hard to assess the durability of shifts in gender inequality (Milazzo and Goldstein 2019).

A second contribution of our paper to the literature on women’s empowerment lies in making progress on data and methodology. In many cases, systematic data on economic indicators, including sources of income and ownership of housing and land, are usually collected at the level of the household. This excludes important within-household variation in power, a key factor in producing structural inequality (Doss et al. 2015). We interview both men and women within each household, and construct several different measures of women’s empowerment (see Donald et al. (2020)).

Empirically estimating the relationship between women’s property rights and economic or social outcomes is complicated by the fact that underlying household characteristics (structure, norms, ideology) could shape both whether women have formal property rights and what their outcomes look like. A different challenge is reverse causality, namely that changes in economic, political or social outcomes could, in turn, lead to changes in formal property

rights. For instance, women entrepreneurs may be more likely to pursue property rights to better insulate their operations from expropriation, rather than property rights leading to entrepreneurship, or political empowerment could lead to changes in property rights (Brulé 2020). The benefit of the MADB’s loan policy, which we describe in more detail in Section 3, is that it provides an exogenous incentive for allocating formal land titles to women, which enables us to overcome these issues of omitted variables bias and reverse causality.

The rest of the paper is structured as follows: Section 2 sets out a brief conceptual framework and Section 3 describes the context in Myanmar. Section 4 describes our data, Section 5 lays out the empirical strategy, Section 6 documents our empirical results, and Section 7 concludes.

2. Conceptual Framework

Many prior studies have verified that household decision-making does not follow a “unitary” model, where a single decision-maker maximizes the utility of all household members.¹ In fact, the distribution of power within the household (often proxied by patterns of land ownership) is an important determinant of household consumption decisions. Studies have also found that women’s plots tend to have lower levels of agricultural investment (family labor, fallowing etc), indicative of their lower bargaining power within the household.²

¹ See Chiappori (1992) and Browning and Chiappori (1998) for seminal theoretical models of intra-household decision making.

² Udry (1996) is one of the earliest studies to document such within-household inefficiencies. See also the more recent study by Rangel and Thomas (2019) and other studies cited therein.

Based on these frameworks and other literature, we briefly hypothesize the likely effects of financial incentives for registering land in a woman’s name; Table 1 lists the observable implications which we aim to test with our data. Note that each of these hypotheses would hold only under specific conditions.

Table 1: Theoretical Framework

	Women in households exposed to financial incentives to split their land will report...
Hypothesis 1	...more property rights in their name...
Hypothesis 2	...higher levels of economic participation, including more loans in their name...
Hypothesis 3	...increased economic benefits, including agricultural income and revenue...
Hypothesis 4	...higher levels of empowerment, including more participation in household decision-making ...
Hypothesis 5	...a greater likelihood of political participation...
	...compared to women in unexposed households.

We expect that financial incentives should lead to households subdividing their land and registering it in the female household member’s name (Hypothesis 1). Note that this requires that male members are willing to change the bargaining power within the household in return for economic gains, which may not happen in contexts, where socio-cultural norms about male dominance are particularly strong (see Mani (2020)). Our finding that substantial numbers of land officials hold gender-biased views (see section 3.1) means that such formal titling in favor of women may not happen even when households want it to. Similarly, obtaining formal rights to land may not necessarily lead to greater lending; this would depend 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).

Gaining access to credit would change agricultural or non-agricultural income (Hypothesis 3) only if lack of credit was 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 improving economic activities (e.g. knowledge or technology) or if there are barriers to women’s economic participation. For example, de Mel, McKenzie and Woodruff (2009) find that increasing credit has a large effect on enterprises owned by men and no effect on those owned by women. Finally, note that formal land rights may affect women’s empowerment (Hypothesis 4) not only by increasing their economic returns, but also directly by increasing women’s legal knowledge, confidence or within-household bargaining prowess. This means that Hypothesis 4 can hold even if Hypothesis 3 does not. A similar logic holds for political participation (Hypothesis 5) in that formal land rights could change political behavior even if economic outcomes are not affected.

3. Property Rights and Bank Lending in Myanmar

3.1 Property Rights in Myanmar

All land in Myanmar is owned by the state. The central government allocates long-term usage rights, which can be exchanged, sold, and mortgaged, and therefore are akin to private property rights. Throughout this paper we refer to these usage rights as “property rights” for

simplicity’s sake. These usage rights can be rescinded by the local authorities for eminent domain or when the lease is violated by the user, but such cases are relatively rare (Rhoads 2018).

The documents that provide the strongest property rights are the *Land Grant* for urban settings and *Form 7* for agricultural settings (e.g., the Ayeryawaddy region, where our data was collected). Form 7 is often referred to as a land-use rights certificate (LURC) to denote that it provides tenure security (Mark 2016). While the LURC 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).³

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 LURCs, households also hold other land documents (such as Forms 105, 106, 15, or 39) that do not have the same exchange or mortgage privileges as a Land Grant or Form 7 and cannot be considered a secure title (UN Habitat 2019).⁴ It is also common in Myanmar for families to hold Form 7s with the names of previous owners, together with an informal contract that stipulates that the current holder bought the land from the previous owner. This informal contract, however, is not officially

³ 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.

⁴ Form 105 is a prerequisite to acquiring a Form 7, Form 106 is a legal documentation of the land’s history, Form 15 is a sublet of primarily agricultural land, and Form 39 allows the transfer of agricultural land to other uses, which can eventually be upgraded to a Land Grant.

recognized as a tenure right by government authorities. Formal transfer of the Form 7 is required to secure the land plot (Mark 2016).

After several decades of military rule, Myanmar began to transition to democracy in the twenty-first century. After boycotting the 2010 national election, the National League for Democracy led by Nobel Laureate Aung San Suu Kyi won the 2015 parliamentary elections. In January 2016, the Myanmar Parliament, under the control of the National League for Democracy, approved a new National Land Use Policy⁵ following an extensive public consultation process that included domestic businesses, foreign investors, non-government organizations, and regional and ethnic 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 the rights for exchange and mortgage. Importantly for our project, the policy provided assurance of equitable land access for smallholders and landless people, with consideration of customary tenure and gender equality (Mark and Belton 2020).

Improving co-ownership and easing access of women to Form 7 documentation were specific goals of the legislative drafters. Consequently, there is no legal prohibition on women's property registration or joint registration on a Form 7 in the 2016 National Land Policy. In practice, however, joint registration is limited and few women have documented rights to agricultural land. According to very limited data on farmland certification in Myanmar, 80% of farmland certificates have only a man's name listed (Namati 2016). In many cases,

⁵ <http://extwprlegs1.fao.org/docs/pdf/mya152783.pdf>

cultural norms about women’s roles in Myanmar society limit equal access to formal property rights. This cultural norm can be visually observed in the small space available for the listing of names on the LURC. Without extremely small penmanship, it is difficult to display the names of both a husband and wife on the certificate. In our survey, 13% of respondents from landed families expressed the view that only men’s names should be on the land certificates, since men were the main decision makers for the household, and a similar fraction of respondents also expressed the view that having two names on a land certificate would lead to conflicts between the couple. These attitudes were shared by land officials as well, constituting a further barrier towards formal land ownership by women: 22% of land 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 in the land document.

3.2 Rural Bank Lending in Myanmar

While small private banks, micro-lenders, and other actors do exist, the dominant source of agricultural lending in the country is the Myanmar Agricultural Development Bank (MADB), which accounts for between 60 to 90% of bank lending in rural parts of the country (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. Currently, MADB has 206 branches throughout the country and over two million customers.

To obtain an MADB loan, a potential recipient must present evidence of a Form 7 for the plot where they wish to deploy the money, verification of a savings account at MADB,

participation in a lending group, and approval by a village loan screening committee regarding the viability of the project (Aung, Nguyen and Sparrow 2019). The average interest rate on MADB agricultural loans is about 0.71% per month or approximately 8.5% per annum (Luna-Martinez and Anantavrasilpa 2014). Strict lending criteria, including Form 7 possession, have ensured very high repayment rates for MADB. The bank accounts for less than 35% of total outstanding rural loans in the country, despite providing well over 60% of the total lending.

Loan sizes increase incrementally with plot size. Loan recipients receive 100,000 Kyat (about USD 100) per acre for paddy production (and 20,000 Kyat for other crops), up to a maximum of ten acres, and any plot above ten acres in size is still only entitled to the maximum loan of 1 million Kyat. Additional loan amounts require a separate Form 7 in the name of a different party. Aung, Nguyen and Sparrow (2019) verify the immediate effect of the policy, showing that farms just above the ten acre threshold receive loan sizes that are USD 18-24 per acre less than for those just below the threshold—up to a 25% reduction in loan size. However, they find no difference in the agricultural yield or income from the sale of rice on either side of the threshold.

The potential for increased access to credit creates 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 Form 7, and apply for a new loan under the new household member's name. According to our own discussions in Myanmar and anecdotal conversations, most families decide to put the land in the female spouse's name for this purpose.

4. Data and Measurement

4.1 Household Survey

Our main source of data is a household survey conducted in the Ayeyarwaddy region of Myanmar from November 2019–January 2020.⁶ The survey was conducted across 128 randomly selected village tracts in 14 out of 26 townships in this region. The sample represents rural households in a largely Burmese speaking region of Myanmar, where historically women 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 in the region.⁷

Our budget allowed us to survey 2,534 households across these village tracts. Since we are interested in co-registration and other intra-household dynamics, our sample was restricted to households that had both a male and female head; single-headed households were excluded from our survey. Around one-third of survey respondents were landless households that were potential beneficiaries of the land-to-the-landless program, but as they did not yet possess land (and land registration was therefore not applicable to them), these households are excluded from our analysis.

The average age of our survey respondents was 48 for women and 51 for men, and households consisted of 4.6 members on average. Women are less educated than men: 17% of men had completed secondary school compared to only 11% of women (See Table A.1, panel A).

⁶ Please see Appendix C for a detailed ethics statement, which guided our survey research.

⁷ Following the military coup of February 2021, the status of this program is uncertain.

The survey team was directed to survey at least two households with land ownership greater than ten acres in each village tract. Our main estimation sample consists of 1,657 households, in which both the man and the woman report non-zero ownership of land.⁸ The median landholding 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 own more than two plots. The distribution of landholdings reported by women was extremely similar to the distribution of landholdings 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 our statistical analysis below.

The majority of survey questions were asked separately to both partners, enabling a comparison of their responses and assessment of each partner’s individual 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 in place (see Appendix C).

⁸ There were six households where men did not report landholding size while women did, and eight households where women did not report landholding size while men did. These are excluded from our analysis.

4.2 Land Distribution and Formal Land Rights

In our sample, a very high proportion (88%) of households report possession of at least one Form 7, which, as explained above, is the formal legal title to agricultural land. Interestingly, more than 30% of plots with at least one Form 7 are reported to have multiple Form 7s associated with a single plot. The most common reasons stated for such multiplicity are boundary differences between the landholder 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 existence of formal documentation of the household's land. 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 to nearly 23% for women - a difference that is statistically significant. As a result, women respondents report 2.16 total Form 7 certificates in the household compared to 2.23 for men. In particular, women report 31.6% of plots with multiple Form 7s, while men report 34.4%, a difference that is also statistically significant.⁹

To measure women's formal land rights in our data, we use the following two indicators: a dummy variable for whether the female household head has her name on any Form 7 for any household plot (this could be co-registered with the male household head), and a dummy for whether the female head of household has only her name listed on the Form 7 for an entire plot (i.e., this variable reflects sole control over that plot for the female household head).

⁹ Here we assess statistical significance via a simple t-test comparing the responses of women versus men.

Women’s formal claims to household land assets are very low in this setting. Only 9.4% of households have any Form 7 with the female household head’s name on it, according to female respondents (men report this figure as 8.9%), and only 5.8% of households report any plot of land with the female head of household’s name exclusively on that plot’s Form 7 (Table A.1, panel B).

4.3 Economic Outcomes

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 by the female and male heads of household) and indicators of economic activity and success (agricultural income generated by a given plot, engagement in a non-agricultural enterprise, and the revenue from such enterprises). Very few women report having taken out land-collateralized loans compared to men; the average number of such loans for women is 0.06 compared to 0.74 for men. At the same time, in terms of economic activity, only 21% of men and 19% of women report being engaged in any non-agricultural activities, and the revenue from such activities is only about 5% of the revenue from agricultural activities (Table A.2, panel A).

4.4 Measuring Women’s Empowerment

We use a range of indicators for women’s empowerment, since there is no single universally accepted measure for this concept. First, we compute an index of household decision-making

for agricultural activities and related expenditures. We asked questions about women’s involvement in agricultural decisions such as hiring agricultural labor, livestock raising, gardening, choosing crops, buying/selling/renting land, and whether decisions were made by the female head alone, jointly with another person, or wholly by another person. We code the woman as being involved in decision-making in a given domain if she makes decisions in that domain alone or jointly with someone else (see Table A.3 for summary statistics of each individual component). We sum all these components and construct a standardized index for agricultural decision-making (z-score obtained by subtracting the mean and dividing by the standard deviation).

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

In a similar manner, we construct a second index of household decision-making based on questions about 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, women are more involved in these decisions relative to the agricultural sphere—with the exception of fertility decisions, where only 17% of women report being involved (Table A.3). Unlike before, where men reported higher female participation in *agricultural* decision-making, measures of women’s involvement in *expenditure* decisions are considerably lower from men’s reports relative to women’s reports (Table A.2, panel B).

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). A valuable dimension of empowerment, then, is not only whether a women reports that she has the ability to control economic and social aspects of her life, but also whether she takes that power for herself as opposed to being permitted to exercise it in a circumscribed way. Previous work has found that households in which both partners agree on the woman’s decision-making power experience significantly better outcomes in terms of family planning decisions, children’s health outcomes, and domestic violence (Donald et al. 2020). We operationalize this 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.” 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 female involvement are not included in this measure. We find that, on average, both husband and wife report that the woman has *no* involvement 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 also conducted an elicitation exercise designed to measure women’s demand for autonomy by asking how much money they would be willing to give up in order to retain sole control of a specific amount. 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 the limited 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.

5. Empirical Strategy

5.1 Identification Using the Bank Lending Policy

To examine the effect of formal land rights on women’s economic outcomes or empowerment, we cannot simply compare these outcomes across households in which the female head has formally registered property rights and those in which she does not. Such a comparison would yield biased estimates due to selection effects and/or the endogeneity of formal land rights. If it is the case that 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 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 in the MADB’s lending policies.

As described in Section 3, households that have more than 10 acres of land have an incentive to divide their land into multiple plots for the purpose of obtaining additional loan

amounts, and registering those newly created plots under the name of a different member of the household (which is often the female household head). As stated in Hypothesis 1 above, we therefore expect households with land holdings greater than 10 acres to report a larger number of plots, a larger number of Form 7s, and a larger number of Form 7s registered with a woman’s name. We can also examine whether outcomes related to economic activities, women’s empowerment, and political participation are systematically different in households with land holdings above ten acres (hypotheses 2-5). However, since households with more than 10 acres of land are also likely to be different than those with less than 10 acres on other confounding dimensions—and these differences are likely to increase as we move further from the 10-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 design (RDD) to examine whether there is a sudden sharp increase in the landholding 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 landholding is above ten acres. $f()$ is a function that controls for any continuous relationship between total landholding size and our outcomes of interest, so that

we are identifying only effects that vary discontinuously at the same threshold as our bank lending policy. As has been argued in recent work, such RDD analyses should primarily focus on points close to the discontinuity (Stommes, Aronow and Savje 2021). We therefore restrict our sample to a narrow bandwidth of landholdings around the 10-acre discontinuity, and use a local linear polynomial for $f()$. 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 and hence more degrees of freedom for estimation) and greater bias generated by observations further away from the discontinuity (note that the optimal bandwidth is not constant across outcomes due to differences in the distribution of each variable).

5.2 Validity of the RDD Estimation Strategy

Before we examine the effects of these financial incentives, we verify that other important factors do not vary discontinuously at the 10-acre threshold. Figure A.2 shows how household characteristics such as the age/education of the household head and the number of household members change with land holdings. Note that these characteristics vary continuously at the 10-acre threshold, lending support to the assumption that our comparison is not confounded by differences in household characteristics on either side of the threshold.

We also examine whether the “running variable” (household land holdings) is smoothly distributed at the 10-acre threshold. We would be concerned if households strategically obtained land in order to exceed the loan policy cutoff, but we believe this to be unlikely in the Myanmar context, as land markets do not function very smoothly and land transactions

are relatively rare events. It would also be quite illogical for families to strategically purchase land to cross the threshold, as the MADB policy guarantees there is no financial incentive to do so. Following the norms in the literature, we present a McCrary Density test in Figure A.3, and while the graph indicates a discontinuity of landholding size at the 10-acre threshold, we do not interpret this as evidence of sorting. Rather, we believe it results from a natural bunching of self-reported landholding sizes at multiples of 5: we can see in Figure A.3 that the same discontinuity in landholding size distribution is visible at 5, 15 and 20 acre cutoffs as well, even though none of these are relevant to bank lending policies.

Since the incentive to sub-divide an individual plot is only active for plots strictly larger than 10 acres, households reporting exactly 10 acres of land are considered to be below the RDD threshold in our analysis. This may cause measurement error if some households are assigned to the “control group” (below the RDD cutoff), even though their true landholding is above 10 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 A.4, we compare respondents who report owning exactly 10 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 A.2, 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 there-

fore conduct robustness tests for our main results by including controls for education and household size (see Section 6.4).

Finally, we sought 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 conducted power calculations to determine the number of observations necessary within the optimal bandwidth to detect a minimum detectable effect of given size. We find that our sample size is sufficient to detect effects of 0.8 standard deviations at 80% power for 46% of our outcome variables among female respondents and 56% among male respondents. This proportion increases to nearly 80% for female respondents after decreasing power to 60% or increasing the effect size to 1 standard deviation (see Table A.5 for full summary tables by power and effect size). As a secondary check for under-powered results, 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 results below, RI p-values are reported beneath each regression result in addition to the RD robust standard errors.

6. The Impact of Financial Incentives

6.1 Financial Incentives and Women’s Land Rights

Our results provide empirical support for Hypothesis 1. As expected, we find that having more than 10 acres of land results in a significantly larger number of individual plots in the household, consistent with the hypothesis that the MADB lending policy incentivizes plot divisions (Table 2, column 1). Households above the 10-acre threshold also have a higher number of Form 7s, which are required if these plots are to be used as collateral for MADB loans (column 2).

Most importantly, households above the threshold are 13 percentage points more likely to have at least one Form 7 with the female household head’s name on it. Almost all of this increase is driven by Form 7s with the woman’s name registered exclusively, as would be required for the lending policy (Table 2, columns 3 and 4). Since we asked all questions of men as well as women, we can also observe any differences between male and female reports, though here we find general agreement between sexes (as men above the cutoff also report more Form 7s with a woman’s name; Table A.6). Though the point estimates from men’s responses are larger in magnitude than those from women’s responses, the two are not statistically different from one another.

These results are also presented graphically in Figure A.4, where we show binned outcome values on both sides of the 10-acre threshold (with a quadratic relationship fit separately to

Table 2: *Financial Incentives Lead to Greater Formal Property Rights for Women*

	(1)	(2)	(3)	(4)
	Number of Plots with nonmissing plot size	Total num. of Form 7s in HH	Household has at least 1 Form 7 in Female HoH's name (joint)	HH has at least 1 Form 7 in Female HoH's name (excl. and no other male)
Female Responses	0.8458*** (0.2389)	0.6309** (0.2956)	0.1305* (0.0735)	0.1316* (0.0711)
RI p-Value	0.0000	0.0000	0.0000	0.0000
Control Mean	1.4464	1.9834	0.0690	0.0554
Observations	404	353	428	427
Bandwidth	2.1334	2.4016	2.6251	2.5994

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female 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.

either side) for female responses only. The graphs show a clear upward jump to the right of the 10-acre threshold for all measures of land holdings and property rights.

6.2 Financial Incentives and Economic Outcomes

We now explore whether the data support Hypothesis 2, that the financial incentives to split land will result in higher levels of financial access for women. We find that women in households directly above the 10-acre threshold report a higher number of land-collateralized loans, compared to those in households below the threshold (Table 3, column 1); this result is corroborated by men’s reports (Table A.6, column 5). Surprisingly, both men and women also report a higher number of loans in the male household head’s name, though only the

women’s reports yield a statistically significant estimate (Table 3, column 2; and Table A.6, column 6).

Despite this higher access to formal sector loans, we do not find any increases in economic revenue, as predicted by Hypothesis 3. We examine total revenue generated from plots with the female household head’s name on the associated Form 7, as well as total revenue generated from all household plots (Table 3, columns 3 and 5). We find no significant differences for households with more than 10 acres of land compared to those with less. On the other hand, we do find a significant decline in total non-agricultural revenues earned by the female household head, 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. The graphical representation of these loan and income variables can be seen in Figure A.5.

6.3 Financial Incentives and Women’s Empowerment

Thus far, we have shown that an exogenous financial incentive induces couples to transfer property rights to the woman’s name, which in turn leads her to take out more formal loans. These results demonstrate the validity of our research design, confirming the underlying assumption that the MADB lending policy is actually incentivizing a transfer of property rights and increase in female borrowing. 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 female decision-making within the household. Overall, we do

Table 3: *Financial Incentives are Associated with Access to Credit But Do Not Increase Revenues*

	(1)	(2)	(3)	(4)	(5)
	Number of Loans in Female HoH's Name (Land Collateralized)	Number of Loans in Male HoH's Name (Land Collateralized)	Log Total Agricultural Revenue from Plots with Female Name on Form 7	Log Total non- Agricultural Income from Female HoH	Log Total Agricultural Revenue from all Plots
Female Responses	0.1457* (0.0787)	0.7117*** (0.2323)	-1.6713 (3.6186)	-1.8947* (1.0777)	1.2947 (1.0414)
RI p-Value	0.0000	0.0000	0.3940	0.0000	0.0400
Control Mean	0.0321	0.8143	10.4941	2.2528	11.6264
Observations	404	404	78	434	574
Bandwidth	2.1881	2.1633	6.1611	2.9453	3.3622

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female 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.

not find any significant effects on these measures, falsifying Hypothesis 4. Table 4 shows the RD results for several measures of women’s agency in agricultural decisions. Women in households with more than 10 acres of land report only a 0.07 standard deviation increase in their decision making index, which is statistically insignificant. We also find no significant increases in women’s agency as reported by their male partners, and a negative but also insignificant change in the probability of women “taking power” within the household.

Similarly, we find no significant effects on women’s agency with regard to expenditure decisions (Table A.7). This is not surprising given the lack of any effect on agricultural decision making, where we expected effects to be more likely ex-ante. The graphical representation of these empowerment outcomes can be seen in Figures A.6 and A.7. Consistent with these null results, we also do not find any impact of formal property rights on the demand for

Table 4: *Financial Incentives are Unrelated to Women’s Agency in Agricultural Decisions*

	(1) Index of Female Agency in Agricultural Decisions, Female Response	(2) Index of Female Agency in Agricultural Decisions, Male Response	(3) Agricultural Decisions index: Woman takes power	(4) Agricultural Decisions index: Husband gives power	(5) Agricultural Decisions index: Wife and husband agree
Female/Male	0.0738 (0.1459)	0.3176 (0.3598)	-0.4667 (0.3134)	0.1930 (0.2094)	0.5216 (0.4941)
RI p-Value	0.2680	0.0380	0.0000	0.0000	0.0000
Control Mean	0.0767	0.4239	0.6359	0.0686	0.3301
Observations	580	403	271	267	271
Bandwidth	3.6592	2.3327	1.9831	1.8476	1.9875

Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female (column 1) and male (column 2) responses. (Columns 3–5 are derived from a combination of male and female 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.

autonomy, as measured by our demand elicitation survey (see Appendix B). Neither do we find any effects on the political knowledge or views expressed by women (see Appendix D).

6.4 Robustness Checks

We conduct three robustness checks on the interpretation of our primary results outlined above. First, we show that our results are not being driven by differences between rounders and non-rounders near the discontinuity (see Section 5.2). Second, we show that our findings are unique to the policy-relevant threshold of 10-acres, and are not replicated at other nearby cutoffs. Finally, we discuss issues of power and the implications of the randomization inference tests.

As reported in Table A.4 and discussed in Section 5.2, individuals in our sample that report exactly 10 acres of land (rounders) come from significantly larger households and are significantly less educated than those reporting slightly more or less than 10 acres (non-rounders). To the extent that we believe at least some of those who report 10 acres are rounding and therefore incorrectly assigned to the control group, we may thus 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 A.8). These results are very similar to our baseline estimates, making us confident 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 sub-divide. 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 10-acre threshold. In Figures A.8 and A.9, we test the validity of the 10-acre cut-off for both property rights and loan outcome variables (see Sections 6.1 and 6.2) by re-running the RDD specification at alternative cut-offs. The resulting coefficient plots clearly demonstrate that, for both male and female respondents, the results presented in the main analysis are only statistically significant at the 10-acre threshold. For a range of other thresholds above and below 10 acres, there is no observable effect on land registration and borrowing outcomes. In Figures A.9 through A.11, we produce the same plots for all downstream economic and female agency outcomes as well, showing similar null effects for most alternative cut-offs above and below the policy-relevant 10-acre threshold.

Finally, one might also be concerned that the null results reported for downstream outcomes may simply be the result of under-powered inference due to a relatively small sample size. As mentioned earlier, the power calculations shown in Table A.5 indicate that we should expect about 50 percent of regressions to be sufficiently powered to detect effect sizes of at least 0.8 standard deviations for female responses (at 80 percent power). Ex-post, comparing p-values from the randomization inference tests with standard inference, we find agreement¹⁰ in 9 out of the 13 female outcomes presented in Tables 2 through 4—greater than the 50 percent prediction. In the four cases of disagreement, we fail to reject the standard null hypothesis while rejecting the sharp null of the randomization inference test, which is an indication that regressions are not sufficiently powered to detect average treatment effects (since the sharp null is easier to reject in smaller samples). While this may be a concern for the final three outcomes related to female agency in agricultural decisions (Table 4, columns 3–5), we observe a precise zero effect in the primary decision making index (Table 4, column 1). Though we cannot rule out the possibility that any one of our null results is under-powered, we take the above as evidence that the majority of results are precisely estimated, and that the conclusions of our analysis are not biased by small sample issues.

7. Conclusions and Future Research

This paper uses an unintended consequence of a bank lending policy to examine the effects—on various economic and social outcomes—of an exogenous transfer of formal property rights

¹⁰ Agreement simply means that the result of the standard hypothesis test is the same as the RI hypothesis test (based on the less conservative sharp null).

to women, contributing to our understanding of how women gain economic and social power. Many anti-poverty programs aim to transfer assets or income streams to women (e.g., micro-finance programs often target women) and either explicitly or implicitly attempt to confer a more powerful 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 are typically not sustainable.

Our results show that households strongly respond to financial incentives to give women legal control over land, which is often a household's most important asset. However, despite what might be read as a large transfer of power within the household, *de jure* control over real assets is not sufficient to lead to a generalized increase in women's economic activities, decision making power, or autonomy. While it is important to note that null results may have different interpretations, we find evidence of pre-existing cultural constraints that suggest our findings are not a statistical artifact. Survey data from men in our sample and male local leaders, who were actually charged with regulating land, confirms the prior strength of these norms in Myanmar.

In sum, our findings suggest that efforts to change power dynamics in the household must move beyond simple economic interventions. Even giving women legal control of large assets is insufficient in our context. Consequently, programs that focus exclusively on economic empowerment overlook the entrenched politics that reinforce women's limited role in economic decision-making. Future research should explore the costs and benefits of changing political dynamics in order to correctly address the trade-offs that both men and women face when power is redistributed within the household.

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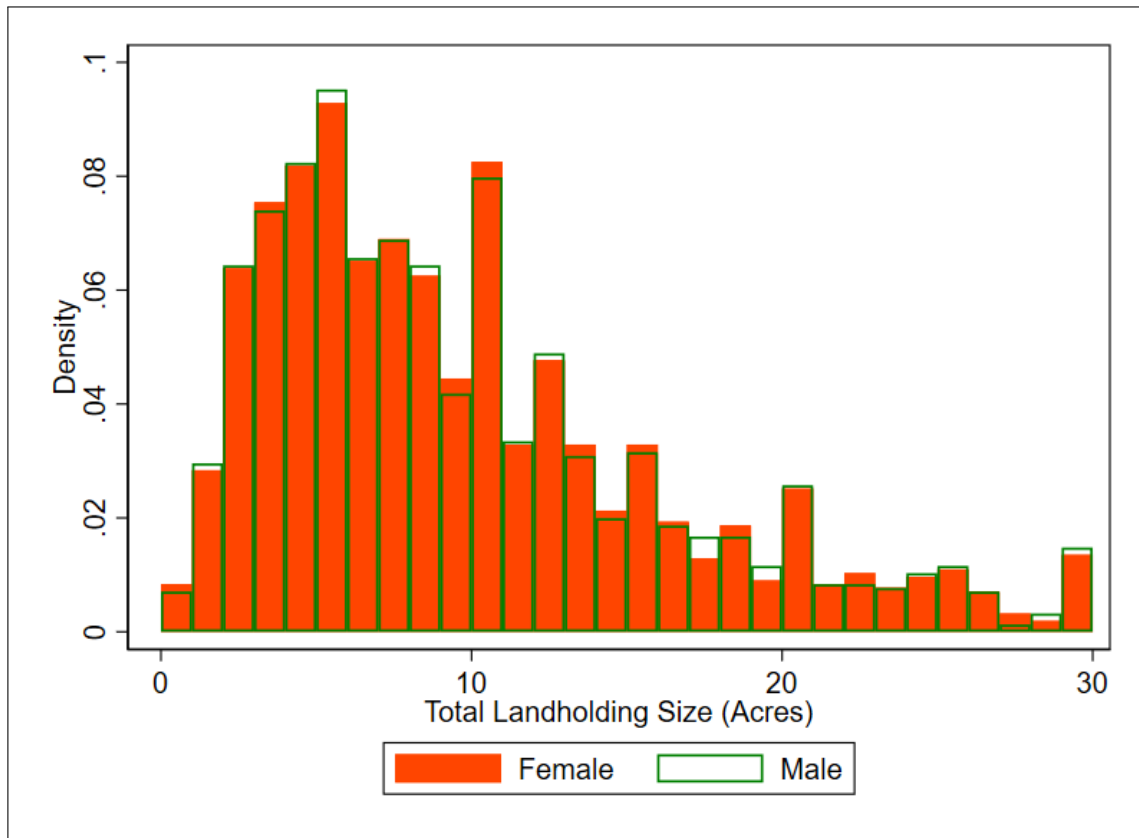
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A. Appendix A: Additional Figures and Tables

Figure A.1: *Distribution of Land Holdings as Reported by Male and Female Heads of Household*



Notes: This histogram shows the distribution of landholdings as reported by male and female heads of household 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 male and female reports shows that there was not much discrepancy in the size of landholdings as reported by the two household heads. We also observe high probabilities of reporting landholdings 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	48.44 (10.42)	50.81 (10.33)	49.62 (10.44)
Respondent has Less than Primary Education	0.0467 (0.211)	0.0401 (0.196)	0.0434 (0.204)
Respondent has at least Primary but Less than Secondary Education	0.785 (0.411)	0.722 (0.448)	0.753 (0.431)
Respondent has at least Secondary Education	0.111 (0.315)	0.172 (0.377)	0.142 (0.349)
Number of Household Members	4.564 (1.642)	4.563 (1.642)	4.563 (1.642)
Panel B: Formal Property Rights			
Number of plots with nonmissing plot size	1.543 (0.893)	1.549 (0.888)	1.546 (0.891)
Total number of Form 7's in hh	2.156 (1.685)	2.232 (1.821)	2.195 (1.756)
Household has at least 1 Form 7 in Female HoH's name (joint)	0.0940 (0.292)	0.0892 (0.285)	0.0916 (0.288)
HH has at least 1 Form 7 in Female HoH's name (excl. and no other male)	0.0575 (0.233)	0.0586 (0.235)	0.0580 (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 landholdings.

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

	Total	Female	Male
Panel A: Economic Outcomes			
Number of Loans in Female HoH's Name (Land Collateralized)	0.0580 (0.252)	0.0634 (0.265)	0.0527 (0.239)
Number of Loans in Male HoH'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)	3689.7 (5103.6)	3747.1 (5352.0)	3632.4 (4856.3)
Total Paddy Revenue from Plots with Female Name on Form 7 (1000s MMK)	3070.2 (5173.9)	3084.0 (5400.4)	3056.5 (4951.7)
Total non-Agricultural Income from Female HoH (1000s MMK)	65.70 (349.7)	69.07 (366.6)	62.33 (332.0)
Total Agricultural Revenue from all Plots (1000s MMK)	2888.1 (7672.4)	2727.8 (4701.6)	3048.4 (9778.0)
Total Agricultural Revenue from all Paddy Plots (1000s MMK)	2061.7 (7535.0)	1910.3 (4443.2)	2213.1 (9684.9)
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)	-0.0215 (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	0.0311 (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)		

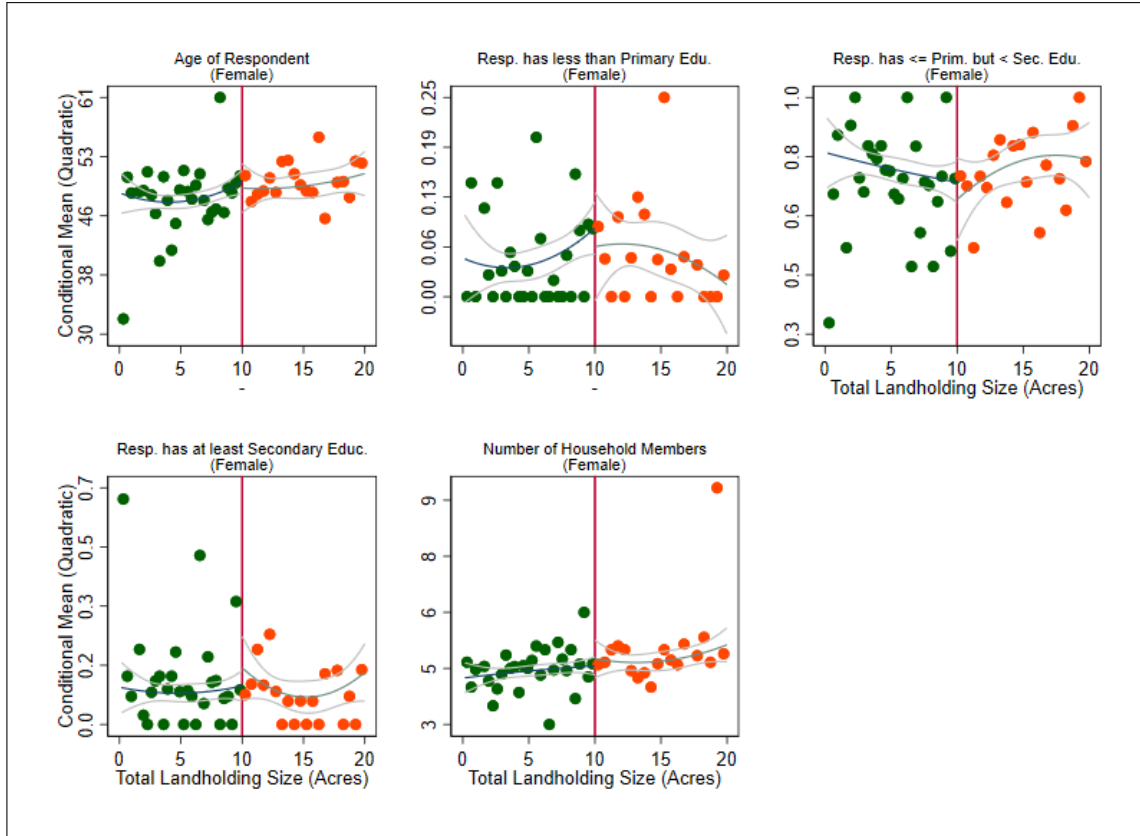
Notes: This table shows sample means for each variable separately by gender, as well as a combined figure; standard deviations are reported in parentheses. In all figures, the sample is restricted to those who report non-zero landholdings.

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

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

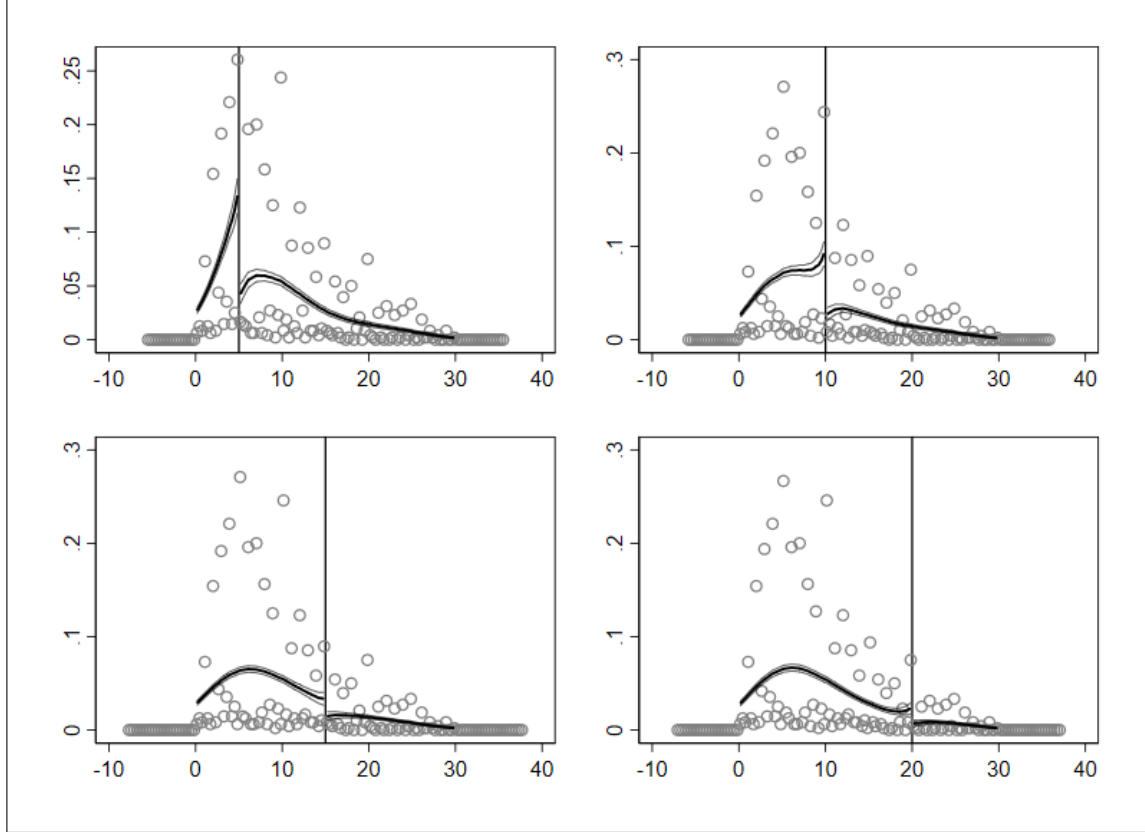
Notes: This table shows sample means for each individual component of the *Index of Female Agency* in both *Agricultural* and *Expenditure Decisions* for men and women separately, as well as a combined figure; standard deviations are reported in parentheses. In all figures, the sample is restricted to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

Figure A.2: *Household Characteristics Above and Below the 10-acre Threshold*



Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Landholding Size). The horizontal red line indicates the RD cutoff at 10 acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 95% confidence intervals for the best fit lines are also indicated in gray. These figures show no evidence of discontinuities in relevant household characteristics.

Figure A.3: *Density of Landholdings as Reported by Female Heads of Household*



Notes: These figures represent McCrary style density tests for manipulation in the running variable. We plot the density of reported landholding size for female respondents, 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 A.4: *Comparison of Rounders vs. Non-Rounders*

	Non-Rounders	Rounders	Diff.
Age of Respondent	51.90	52.08	-0.1834 (1.6457)
Respondent has Less than Primary Education	0.03	0.06	-0.0277 (0.0341)
Respondent has at least Primary but Less than Secondary Education	0.71	0.74	-0.0353 (0.0652)
Respondent has at least Secondary Education	0.24	0.12	0.1258** (0.0509)
Number of Household Members	4.09	4.65	-0.5671** (0.2293)
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 10 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 A.5: *Power Calculations***Female Responses**

Effect Size	Power		
	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

Male Responses

Effect Size	Power		
	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

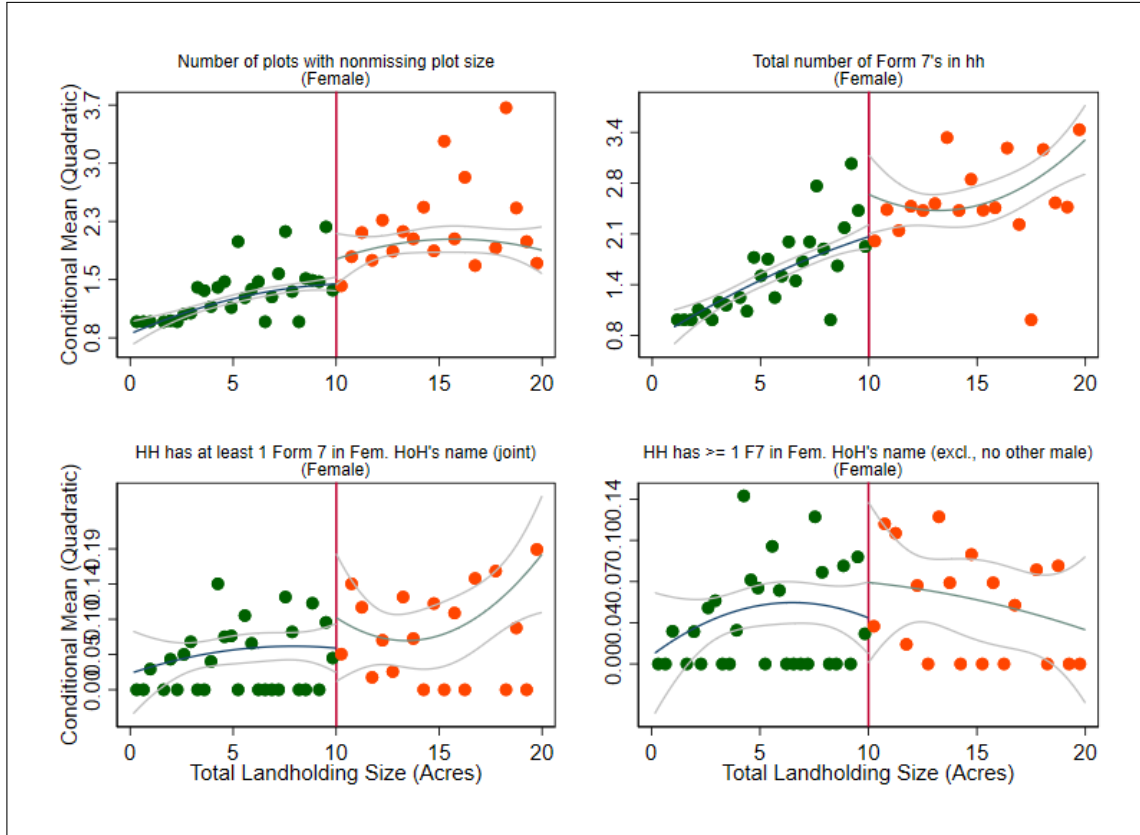
Notes: For any given effect size and power level, these tables indicate the proportion of outcome variables for which our effective sample size (as determined by the optimal bandwidth procedure in (Calonico, Cattaneo and Titiunik 2014)) is sufficient. Results are presented separately for Female and Male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

Table A.6: RDD Results: Male 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 Female HoH's name (joint)	HH has at least 1 Form 7 in Female HoH's name (excl. and no other male)	Number of Loans in Female HoH's Name (Land Collateralized)	Number of Loans in Male HoH's Name (Land Collateralized)	Log Total Agricultural Revenue from Plots with Female Name on Form 7	Total non-Agricultural Income from Female HoH	Log Total Agricultural Revenue from all Plots
Male Responses	0.8467*** (0.2384)	-0.0777 (0.2877)	0.2068* (0.1101)	0.2097* (0.1109)	0.3298 (0.2246)	0.1968 (0.2352)	-2.1879 (3.2683)	-2.0835** (0.9539)	-0.6688 (1.2296)
RI p-Value	0.0000	0.6880	0.0000	0.0000	0.0000	0.0520	0.1860	0.0000	0.2220
Control Mean	1.4384	2.0316	0.0484	0.0453	0.0507	0.8877	12.1966	2.2347	12.3376
Observations	404	353	428	427	404	404	78	434	574
Bandwidth	2.1334	2.4016	2.6251	2.5994	2.1881	2.1633	6.1611	2.9453	3.3622

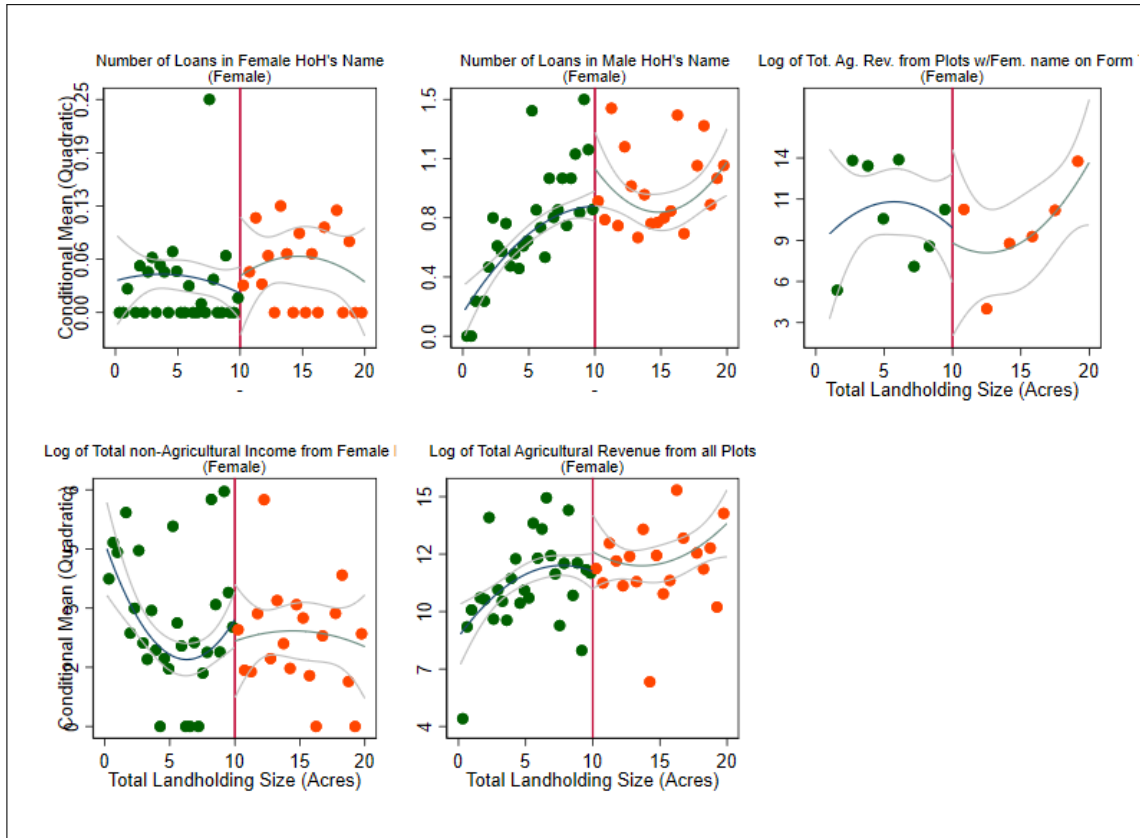
Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for male responses. Robust standard errors are shown in parentheses, and alternative p-values derived from the randomization inference procedure are displayed directly beneath these. “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 A.4: *The Effect of Financial Incentives on Formal Property Rights*



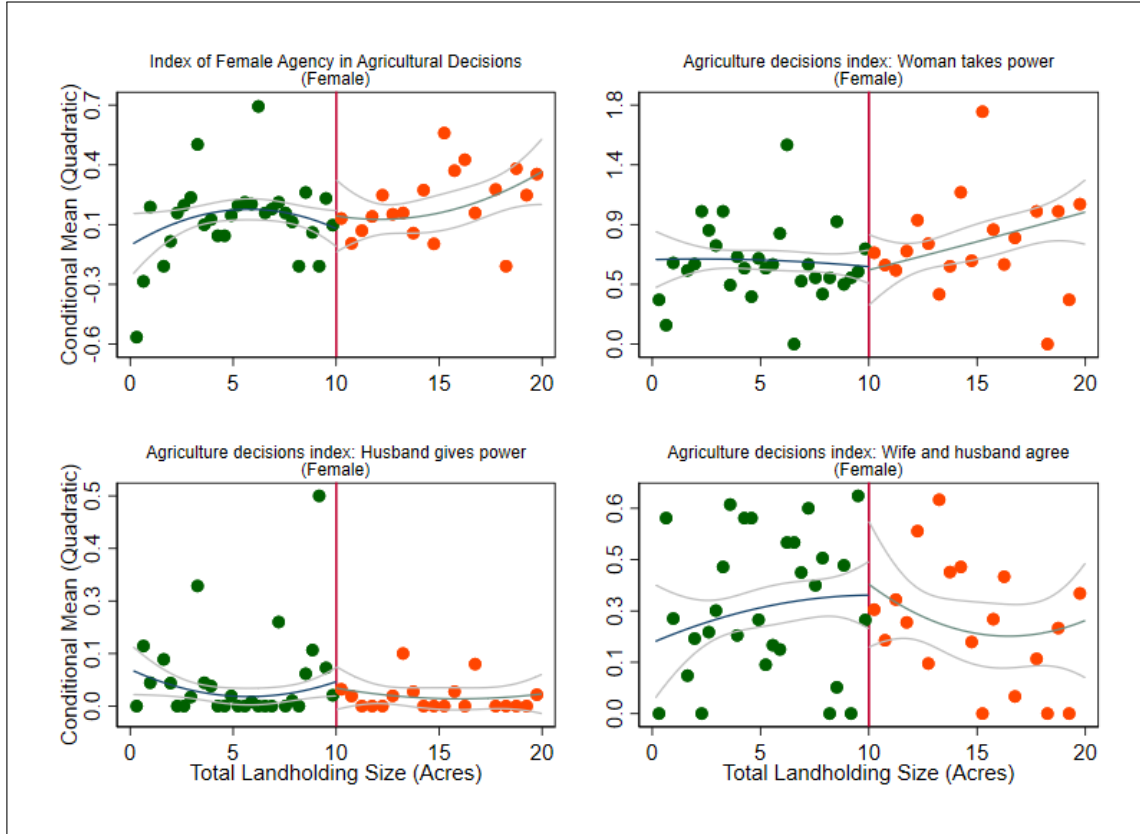
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Landholding Size). The horizontal red line indicates the RD cutoff at 10 acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 95% confidence intervals for the best fit lines are also indicated in gray. All plots are based on female responses only.

Figure A.5: *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 Landholding Size). The horizontal red line indicates the RD cutoff at 10 acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 95% confidence intervals for the best fit lines are also indicated in gray. All plots are based on female responses only.

Figure A.6: *The Effect of Financial Incentives on Female Agency: Agricultural Decisions*



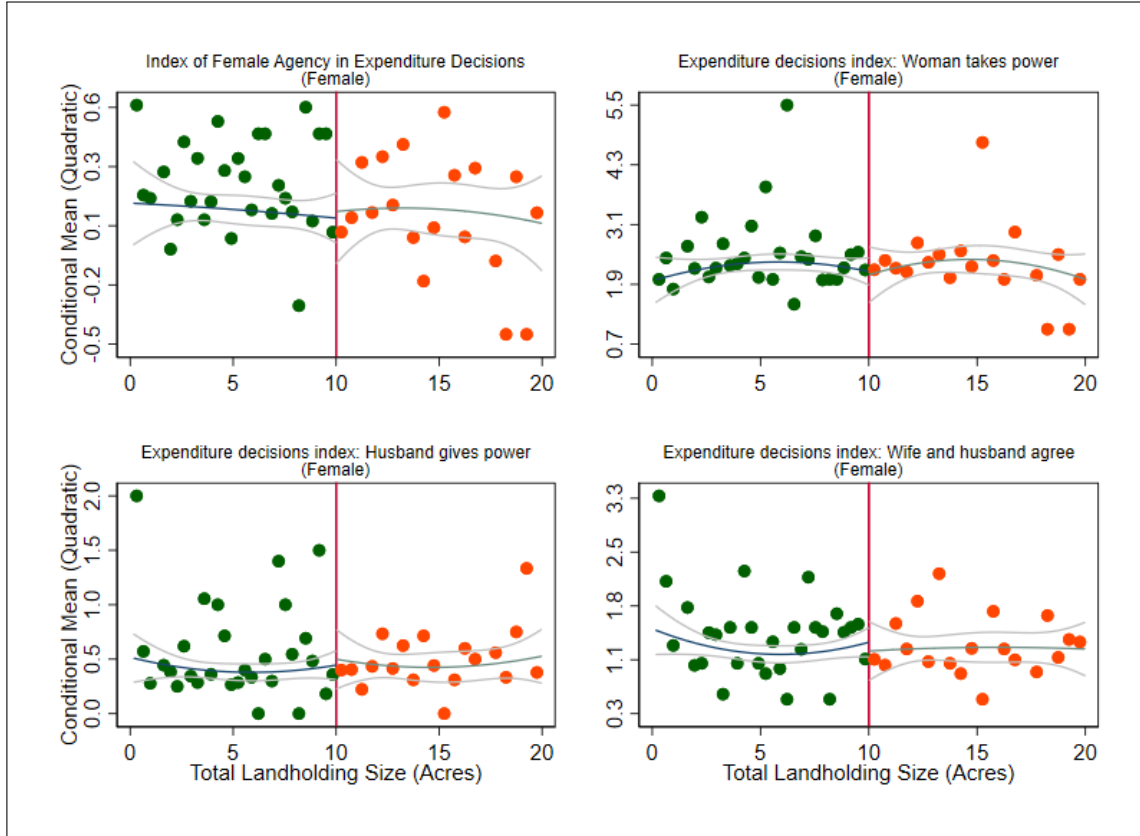
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Landholding Size). The horizontal red line indicates the RD cutoff at 10 acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 95% 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 female responses only. The remaining power indices are defined from a combination of male and female responses.

Table A.7: *Financial Incentives are Unrelated to Women’s Agency in Expenditure Decisions*

	(1) Index of Female Agency in Expenditure Decisions, Female Response	(2) Index of Female Agency in Expenditure Decisions, Male Response	(3) Expenditure Decisions index: Woman takes power	(4) Expenditure Decisions index: Husband gives power	(5) Expenditure Decisions index: Wife and husband agree
Female/Male	-0.0078 (0.2422)	0.3183 (0.3278)	-0.0396 (0.4589)	0.7709 (0.4852)	0.2195 (0.4694)
RI p-Value	0.9020	0.0400	0.8380	0.0000	0.2480
Control Mean	0.1026	-0.1174	2.2359	0.4146	1.2821
Observations	431	396	569	270	403
Bandwidth	2.7312	2.1529	3.1697	1.8896	2.0743

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female (column 1) and male (column 2) responses. (Columns 3–5 are derived from a combination of male and female 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 A.7: *The Effect of Financial Incentives on Female Agency:
Expenditure Decisions*



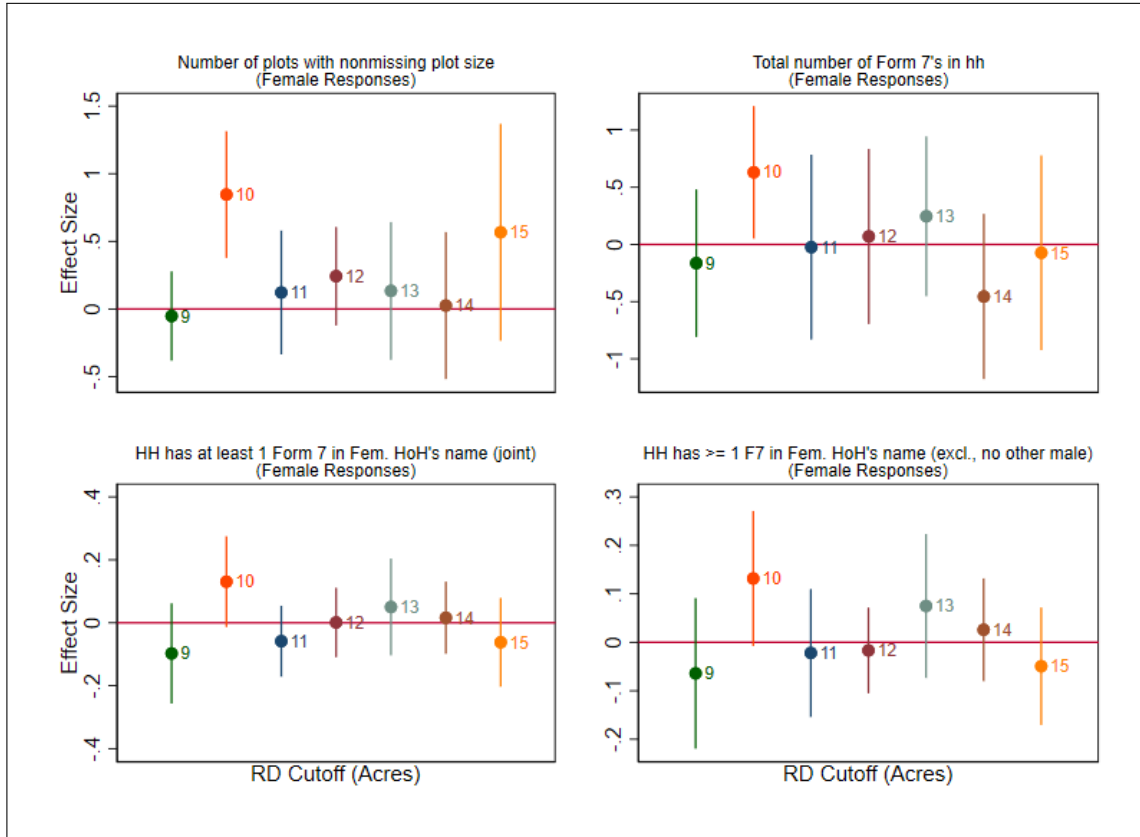
Notes: In each figure, the conditional mean of the indicated dependent variable is plotted for bins of fixed width in the running variable (Total Landholding Size). The horizontal red line indicates the RD cutoff at 10 acres, and separate quadratic lines are fit below the cutoff (between 0 and 10 acres) and above the cutoff (between 10 and 20 acres); 95% 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 female responses only. The remaining power indices are defined from a combination of male and female responses.

Table A.8: 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)
Female Responses	0.8972*** (0.2470)	0.8029*** (0.2911)	0.1232* (0.0717)	0.1232* (0.0681)	0.1438* (0.0765)	0.7522*** (0.2444)	-2.4622 (3.5743)	-1.9436* (1.0336)	1.3109 (1.0452)	0.0649 (0.1444)	0.3242 (0.3647)	-0.4563 (0.3146)	0.1799 (0.2061)	0.4478 (0.4753)
Control Mean	1.4464	1.9834	0.0690	0.0554	0.0321	0.8143	10.4941	2.2528	11.6264	0.0767	0.4239	0.6359	0.0686	0.3301
Observations	404	353	428	427	404	404	78	434	574	580	403	271	267	271
Bandwidth	2.1334	2.4016	2.6251	2.5994	2.1881	2.1633	6.1611	2.9453	3.3622	3.6592	2.3327	1.9831	1.8476	1.9875

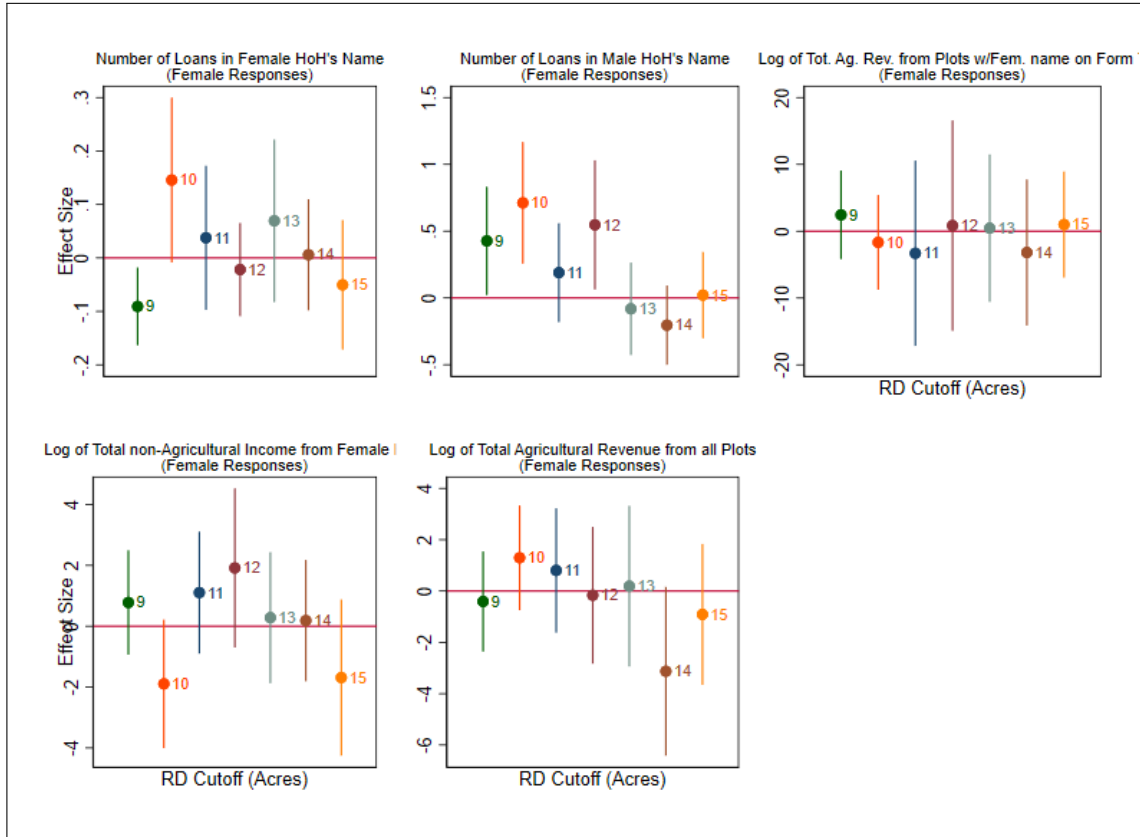
Notes: *** p<0.01, ** p<0.05, * p<0.10. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female responses (note that column 11 is actually based on male responses, while Columns 12–14 are derived from a combination of male and female responses.) All regressions 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 Female HoH’s name (joint); (4) HH has at least 1 Form 7 in Female HoH’s name (excl. and no other male); (5) Number of Loans in Female HoH’s Name (Land Collateralized); (6) Number of Loans in Male HoH’s Name (Land Collateralized); (7) Log Total Agricultural Revenue from Plots with Female Name on Form 7; (8) Log Total non-Agricultural Income from Female HoH; (9) Log Total Agricultural Revenue from all Plots; (10) Index of Female Agency in Agricultural Decisions, Female Response; (11) Index of Female Agency in Agricultural Decisions, Male Response; (12) Agricultural Decisions index: Woman takes power; (13) Agricultural Decisions index: Husband gives power; (14) Agricultural Decisions index: Wife and husband agree.

Figure A.8: *Coefficient Plots for Land Outcomes*



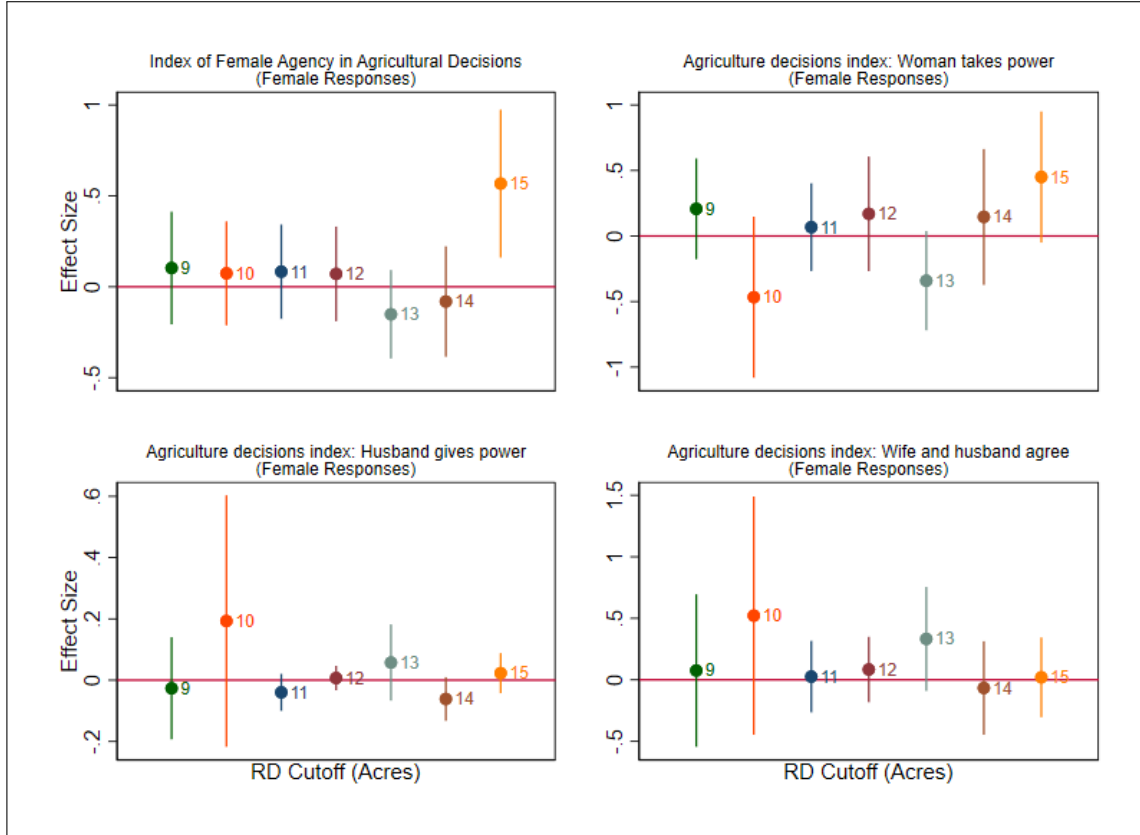
Notes: Each figure plots the RD Effect for the indicated outcome (β_{RDD} from Equation 1) Using 7 different cut-off points in the running variable (Total Landholding Size), ranging from 9 acres to 15 acres. For each different cut-off value, the point estimate for β_{RDD} is plotted along with the 95% confidence interval. All results are based on female responses only.

Figure A.9: *Coefficient Plots for Loan and Economic Outcomes*



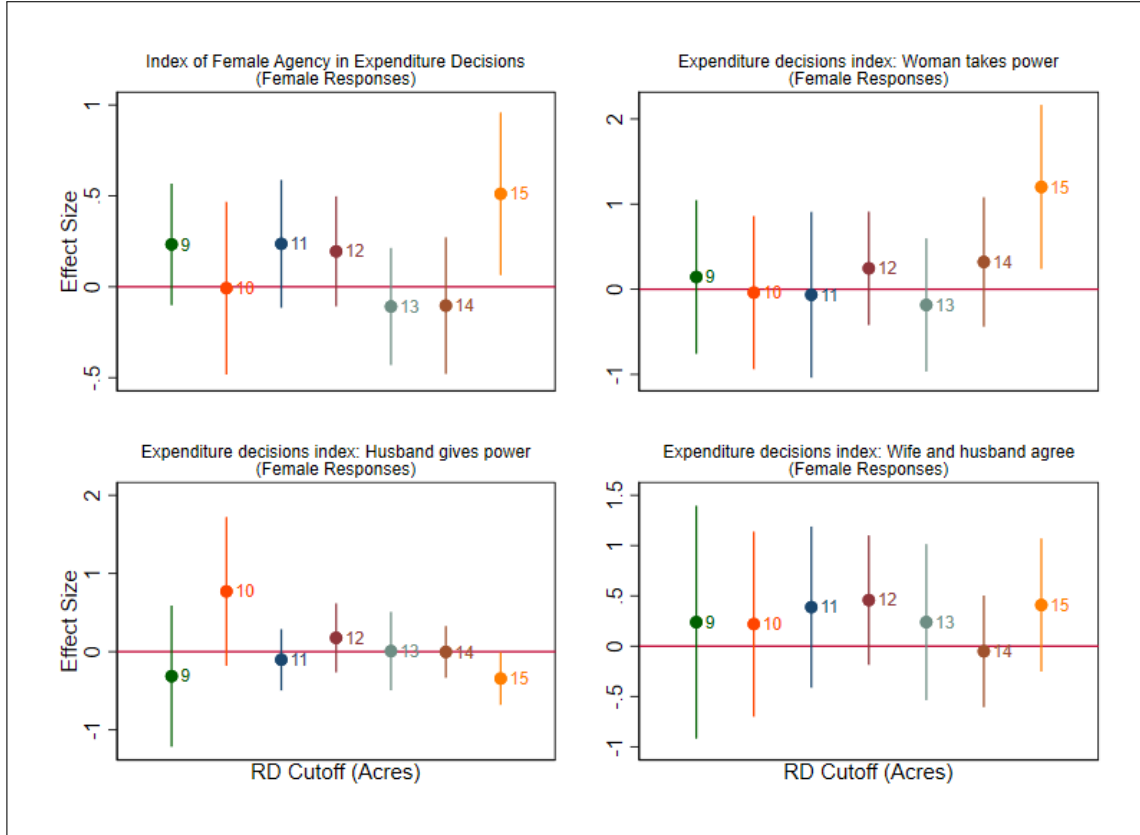
Notes: Each figure plots the RD Effect for the indicated outcome (β_{RDD} from Equation 1) Using 7 different cut-off points in the running variable (Total Landholding Size), ranging from 9 acres to 15 acres. For each different cut-off value, the point estimate for β_{RDD} is plotted along with the 95% confidence interval. All results are based on female responses only.

Figure A.10: *Coefficient Plots for Agency Outcomes: Agricultural Decisions*



Notes: Each figure plots the RD Effect for the indicated outcome (β_{RDD} from Equation 1) Using 7 different cut-off points in the running variable (Total Landholding Size), ranging from 9 acres to 15 acres. For each different cut-off value, the point estimate for β_{RDD} is plotted along with the 95% confidence interval. The aggregate index for expenditure decisions presented in the first panel is based on the female responses only. The remaining power indices are defined from a combination of male and female responses.

Figure A.11: *Coefficient Plots for Agency Outcomes: Expenditure Decisions*



Notes: Each figure plots the RD Effect for the indicated outcome (β_{RDD} from Equation 1) Using 7 different cut-off points in the running variable (Total Landholding Size), ranging from 9 acres to 15 acres. For each different cut-off value, the point estimate for β_{RDD} is plotted along with the 95% confidence interval. The aggregate index for expenditure decisions presented in the first panel is based on the female responses only. The remaining power indices are defined from a combination of male and female responses.

B. Appendix B: Behavioral Measures of Empowerment

As part of the household survey, we collected information from a behavioral intervention designed to capture an 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. Our game asked women to choose between keeping a certain sum of money for themselves (e.g., 2750 Kyat) versus giving a larger sum to their spouse (e.g., 3000 Kyat). 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 opted to keep 2750 Kyat for herself (as opposed to 3000 Kyat for her spouse), but did not prefer keeping 2500 Kyat, we infer that her willingness to pay for sole control is between 250 and 500 Kyat. Such an elicitation is based on the well-known Becker-Deegroot-Marschak demand elicitation mechanism.

We find that a sizeable fraction of the women in our survey do not have a preference for sole control, e.g., 10% of women would choose to hand over the entire amount of 3000 Kyat 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 Kyat (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 involvement is extremely low (between -0.01 and -0.05). Finally, as seen in Table B.1 below, we also note that the behavioral game outcomes seem to be unaffected by the transferal of property rights.

Table B.1: *Financial Incentives are Unrelated to Behavioral Game Outcomes*

	(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)
Female Responses	-270.7475 (354.0966)	-199.7051 (352.7226)	-0.4737 (0.6128)	-0.4627 (0.6044)
RI p-Value	0.0660	0.1840	0.0700	0.0620
Control Mean	2417.9181	2585.7881	4.4625	4.6918
Observations	431	565	574	575
Bandwidth	2.7408	3.0347	3.3626	3.4292

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female 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. Appendix C: Research Ethics

Research ethics are a critical part of any study involving human subjects. In all aspects of the research process, investigators must carefully consider trade-offs between the potential costs and/or harms to research participants versus the benefits that can be generated by the findings. The research team, in collaboration with all implementation partners—including the local organizations Innovations for Poverty Action (IPA) Myanmar 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.

The research team consulted with and received feedback on all stages of this project from their respective universities, local partners, and funding organizations, including research design, survey creation, and project implementation. IRB approval was obtained from affiliated universities as well as IPA Myanmar, and permission for survey work was also obtained from the Government of Myanmar.¹¹ 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 might be understood to be sensitive, including questions about the dynamics of the relationship between husbands and wives, multi-part protocols were followed. 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. Respondents heard the questions read aloud over headphones and input

¹¹ Protocols listed in Ayeyarwaddy regional government decision No. 16 at the cabinet meeting No. (35/2019). Protocols and data used for this study will be made available in the interest of research transparency in accordance with the assessment of research stakeholders, including implementing partners, that such data does not jeopardize ongoing activities or create risks for any participant.

their responses directly into the tablet. Finally, reporting mechanisms were put in place to manage any adverse events or challenges that may have arisen.

D. Appendix D: Analysis of Political Outcomes

The existing literature on democratization and political participation finds that historically, more equal land rights are related to increases in demands for democracy (Ansell and Samuels 2010; Albertus 2015). To examine whether formal property rights can improve the political participation of women (Hypothesis 5), we explore five basic measures of political knowledge and behavior: whether participants could name political leaders in the Ayerarwaddy regional government (the Chief Minister and the Village Tract Administrator), whether they were planning to vote in the upcoming national election, whether they thought democratic processes were preferable to other forms of government, and their overall satisfaction with democracy in Myanmar. Overall, we find little to no effects. While we do not observe any evidence that women in households above the 10-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 these men may have spent time interacting with local officials, including the VTA, while transferring land titles into their wives' names. Given that we do not find 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.

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

	(1)	(2)	(3)	(4)	(5)
	Name CM of Ayeyarwaddy correctly	Name VTA correctly	Plan to vote in upcoming National election	Dem. always preferable to any other gov.	Satisfaction with democracy in Myanmar
Panel A: Female	0.0166 (0.0527)	0.0046 (0.0594)	-0.1018 (0.0769)	0.1362 (0.1000)	-0.0393 (0.1179)
RI p-Value	0.4960	0.8720	0.0000	0.0220	0.4780
Control Mean	0.0582	0.9434	0.9769	0.4419	1.8489
Observations	583	568	569	565	672
Bandwidth	3.7951	3.1160	3.1634	3.0272	4.2398
Panel B: Male	-0.0413 (0.0706)	0.0421*** (0.0154)	-0.0538 (0.0615)	0.0454 (0.1369)	0.0750 (0.1750)
RI p-Value	0.2560	0.0000	0.0000	0.4060	0.3020
Control Mean	0.1418	0.9637	0.9922	0.6136	1.6779
Observations	583	568	569	565	672
Bandwidth	3.7951	3.1160	3.1634	3.0272	4.2398

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Coefficients represent the robust RD effect estimates—with a cutoff defined at 10 acres—for female and male 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.