A Replication Plan For

"Property rights for the poor: Effects of land titling"
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**Introduction**

Galiani and Schargrodsky's "Property rights for the poor: Effects of land titling," exploits a unique natural experiment wherein squatters living on land in the County of Quilmes (Buenos Aires Province, Argentina), were awarded titles to the land they occupied in an effectively random allocation by the Argentine government. The authors use several statistical techniques to estimate the effect of land titling among this community of squatters on housing investment, household structure, access to credit, labor earnings, fertility decisions, household size, and educational outcomes among offspring of the household head(s).

Treatment households were compared to a counterfactual group who maintained only "usufructuary" land rights over the properties they occupied, and received no land titles. Treatment households were found to increase capital housing investment in walls, roofing, surface area, concrete sidewalks, and overall appearance, as well as have fewer children, house fewer extended family members, and increase the years of secondary and post-secondary education among older children. Land titling was found to have no impact on access to formal credit, while affecting only a very modest positive increase in informal credit engagement, and had no impact on household income, investment in household durable goods, or employment activities.

This empirical analysis presents a compelling option for replication for a number of reasons. The most significant motivation is that Galiani and Schargrodsky's (henceforth, GS) findings seem to suggest that among the poorest of the poor, land titling does not lead to an immediate increase in access to, and the use of, formal credit mechanisms. Their findings cast doubt on the common assumption that land titling improves the condition of the poor through the mechanism of increased access to credit and that the poor will collateralize newly titled land and borrow against its value (De Soto 2000). As the authors point out, this is a particularly prescient concern for the millions of people in developing countries with feeble property rights (Deninger 2003; Banerjee and Duflo 2006). For this reason alone, a replication of GS would be an important step in examining the validity of a highly policy-influential publication.

This study also suggests that below a certain threshold of poverty (particularly among the landless poor who pay no rents), the mere formalization of land titles may be a necessary but insufficient condition to begin engaging in formal credit. Indeed, Lemanski (2011) finds that low-income homeowners in South Africa are usually reluctant to use their "(typically-primary) asset [housing] as collateral security for credit," suggesting that the introduction of property ownership alone does little to influence collateralization of assets for access to credit markets. These findings may even present a compelling argument for the needed addition of alternative inputs, such as self-help groups (Deninger and Liu 2009) to encourage engagement with formal credit markets among the very poor.

GS is also a compelling study to replicate for a number of other reasons. First, the estimation strategy is relatively simple in design, allowing for an easy interpretation of findings. Second, there are also a rich and complex number of robustness techniques employed to strengthen the validity of the study's findings in the face of various potential sources of bias. This allows the replication team to reexamine a number of interesting procedures. Finally, there is ample room left to examine various impact heterogeneities. In particular, impacts among the treatment group seem to be incompletely understood in terms of heterogeneity among female-only headed households, joint female-headed households and women in general. As explained later in this proposal, impact heterogeneity is not fully explored in the existing study, and a more robust analysis would add much greater clarity to the theory of change, particularly with regard to fertility decisions and educational outcomes.

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The replication team recognizes that only limited contact with the original authors should be necessary during the process of reanalysis and should be even further limited when testing alternative analysis techniques and theories of change. We fully intend to share the results of our analysis with GS before making findings public. We also have a strong interest in maintaining our already congenial working relationship with the original authors who have thus far been extremely amenable to the replication of their work. Approaching a replication from this vantage point allows for a more-constructive dialogue around the particularities of the study. A smooth process also stands to benefit the practice of replication and the understanding of the effects of land titling on the poor for the sake of better international development policy recommendation.

**Replication Questions**

1. Does the allocation of land titles have a different impact on female-headed households, or on females in general (particularly with respect to fertility decisions, educational outcomes, and demographic structure)?

2. Does the allocation of land titles have similarly gender-heterogeneous impacts on asset poverty (as measured by a wealth index)?

3. Is the analysis of GS robust to alternative specifications and alternative measurement techniques?

4. Will a broader analysis of indicators (if available) support a consistent story regarding the *mechanisms* by which titling affects recipient households?

**Replication Plan**

The particulars of this replication proposal can best be understood in terms of the three phases of replication described by Brown, Cameron and Wood (2014): First, through a *pure replication*, we will attempt to validate the findings of GS by recreating the estimation techniques used to arrive at the published results. Next, we will attempt to strengthen the robustness of the findings by engaging in alternative *measurement and estimation* strategies and address any errors or alternative processes we uncovered during our pure replication. Finally, we will attempt to better understand some of the causal mechanisms behind the findings originally presented by GS by investigating impact heterogeneity and alternative mechanisms in a *theory of change analysis*.

*Pure Replication*

In a sense, a pure replication of the identification strategy and estimation techniques used by GS should be the most straightforward. A pure replication will seek to methodically and systematically recreate and document the statistical methods used to arrive at the reported outcomes. We outline GS's processes here to demonstrate our intent to replicate each of the steps performed by the original author in arriving at their published conclusions.

*Identification Strategy*

GS exploit a natural experiment to estimate the effect of land titling among a community of squatters on several outcomes of interest. As the authors discuss in section 2 of their paper, they establish treatment and control groups as follows. Both the treatment and control are composed of squatters that, starting in 1981, occupied tracts of land that were (unbeknownst to the occupants) each privately owned by 13 separate owner entities (single or multiple people). In 1984, a constitutionally passed expropriation law (No. 10.239) provided for 1) compensation to the previous owners of the land and 2) transfer of title to the squatters, conditional upon certain

actions taken by the occupants. While 8 previous owners accepted the government offer of compensation (leading to distribution of the first round of titles in 1989), 5 chose to pursue their demands for higher compensation through legal channels. After one lawsuit covering one owner was settled, a second group of squatter households received legal titles in 1998. These households are regarded as "late-treatment" households. Based on these facts, the authors seek to establish the treatment group as composed of two arms: the squatters living on the land owned by the first group of 8 owners are regarded as early-treatment households, and the squatters who received titles in 1998 are regarded as late-treatment households. The control group is composed of the squatters living on the land owned by the second group of 5 owners, minus the late-treatment households.

The authors address several potential concerns with this process. First, they explain that the land selection by the squatters could in no way have been based on a priori knowledge of the original owners' intent to sell their parcels, since a) the squatters mistakenly thought that the occupied land was state-owned, and b) subsequent events, such as the passage of the expropriation law had not yet occurred. Second, they explore potential correlation between the squatters and the quality of the land. Suggesting that more powerful squatters could chose better quality land. They test differences in parcels using observable parcel characteristics and find no statistically significant differences between the treatment and control groups' parcels. They also show that, while there are differences between the owners of the parcels in the two groups in terms of number of owners of any one parcel, and familial ties of owners of any one parcel, the possibility that the contesting owners were motivated by their ownership of better quality of land suggests that the squatters in the control group could be sitting on higher quality land (as perceived by the contesting owners).

The authors then test time-invariant demographic characteristics of the original squatters (between treatment and control) seeking to confirm that exogenous factors led to the creation of the treatment and control group, and that the two groups are similar in every respect except the legal transfer of title. They find no statistically significant differences between means for the original squatters in terms of gender, nationality, parents' nationality, and parents' education. They do find a statistically significant though small difference in the average parcel size, but the magnitude of this difference is deemed negligible.

After establishing the treatment and control groups, the authors use an ordinary least squares (OLS) regression model to estimate the effect of a household possessing legal title over their land on outcomes of interest. The model also contains an error term as well as household and parcel characteristics used as controls for variation among the households. To control for potential correlation between the error term and the control variables, the authors cluster the standard errors at either the parcel-block level or the household level, depending on the outcome of interest. To control for any differences between those that were offered and accepted legal title and those that were offered and did not accept, the authors alternatively report results using a predicted value of whether a household possesses legal land title. This predicted value is estimated using a binary variable equal to 1 if the household was offered legal title.

The authors also address potential problems associated with attrition. First, they exclude households that arrived after the 1986 deadline by which owners had to transfer title or proceed with legal action. However, the authors then note the potential for attrition problems. They show that there is no statistically significant difference between the percentage of households in the control group that arrived before the offer of compensation in 1986 and the percentage of households in the late-treatment group that arrived before 1986. They then compare the estimated coefficients using the late-treatment group with the coefficients using the early-treatment group, concluding that the concern of attrition is assuaged.

In order to further insure against attrition concerns, the authors also employ propensity-score matching (PSM) in order to group similar treatment and control households (as defined by their propensity scores) within preset intervals. The matching process, based on the assumption that no unobservable characteristics can explain the differences between treatment and control households, employs observable parcel-level characteristics in a logistic survival model, revealing that there is a statistically significant and positive relationship between the distance from the parcel to a polluted or floodable waterway and the likelihood of remaining on the land. Using this relationship and a set of household characteristics, the authors match households across treatment and control groups and within preset intervals. After estimating the effect on the outcome of interest within each interval of treatment and control households, the authors weight each interval calculation using the share of treated households out of all treated households, and calculate an average effect using these interval estimates and the corresponding weights.

The pure replication will recreate all of these steps paying particular attention to the coding used to arrive at each outcome along the way. Should any inconsistent reporting or coding errors arise from this process, they will be taken into consideration before proceeding with further measurement and estimation analysis, and theory of change analysis.

**Measurement and Estimation Analysis**

As outlined above, GS use a number of techniques to test the robustness of their findings. As such, offering and deciding upon alternative strategies for robustness testing is challenging without first conducting a pure replication of the techniques used, and establishing what errors might exist. However, a number of potential avenues to test and improve the robustness of their findings are available.

*Alternative Matching Techniques*

We will employ alternative matching techniques to test the sensitivity of the initial findings. In their paper, GS used the stratification method to estimate the propensity score. They then divided the treatment and control groups into matching blocks defined by fixed percentiles, bounded by the 10th and 90th percentiles of the estimated propensity score. While GS did employ alternative matching methods in their earlier work (see Galiani and Schargrodsky 2004), we see no evidence that any method beyond stratification was used in the subject analysis. Owing to this, we will introduce alternative intervals and additional matching approaches, possibly including nearest neighbors and kernel matching, into our replication study.

*Principal Components Analysis*

An additional test of the robustness of findings would be to construct an asset index using Principal Components Analysis (PCA). The original paper by GS finds that among treatment households there is an increase in various elements of housing investment including increased quality of wall materials, quality roofing, greater constructed surface area, an increase in concrete sidewalks, and a general increase in a constructed "housing quality" index. Meanwhile, consumption of household durable assets (in the text, refrigerator with freezer, refrigerator without freezer, washing machine, TV, cellular phone) is not found to be greater among the treatment group. The use of PCA allows us to determine if the original findings are robust to other forms of measurement. In this case, we hope to determine whether findings of increased investment in the home are robust to a combined index of household investment variables and durable goods. Through this analysis we hope to provide a more complete picture of the patterns of household investment between treatment and control households. We might also be likely to

find that households shift their investment from durable assets to improvements to the home through this additional analysis.

Household asset indices can be a useful way to measure long-term wealth and human welfare outcomes while avoiding shocks and vulnerabilities in labor markets and seasonal employment (Moser and Felton 2007). PCA may also utilize an index of durable household assets and other forms of human capital such as education and housing characteristics as a proxy for wellbeing.

This method assigns weights to items in a basket of household assets based upon the distribution of these assets throughout a sample (Córdova 2008). These asset variables are combined to construct one latent variable, or factor score, for further analysis. For example, by aggregating binary asset ownership variables from panel data between 1978-2004, Moser and Felton (2007) were able to assign weighted values to tangible items based upon the frequency of item ownership (and changes over time) among families in a peri-urban slum in Guayaquil, Ecuador. A similar opportunity exists in this sample from Argentina. PCA accentuates the differences across households that may otherwise seem socioeconomically similar, providing powerful evidence of how households allocate their resources, and giving a clearer picture of how families within and between income groups employ different strategies to insure themselves against shocks and income variability (Moser and Felton 2007; Filmer and Pritchett 2001).

However, we are cognizant of the risks in over-interpreting findings from this form of analysis. When using PCA Moser and Felton caution that the model is based on the assumption that observed variables are positively correlated, that the index is difficult to interpret across levels of observation, and that its use requires considerable knowledge of the different variables being used (Moser and Felton 2007). Previous studies successfully using PCA note that between 10 and 30 variables from different asset categories (housing, durables, productive capital) should be used to estimate wealth differences (McKenzie 2003; Schellenberg et al. 2003) and to avoid "clumping" by category (Vyas and Kumaranayake 2006). We will take these cautions into consideration when constructing our own index. Vyas and Kumaranayake also note 4 important steps when using PCA to create an asset-based wealth index that we intend to follow: 1) asset variables should be deliberately selected according to assumptions about household wealth and socioeconomic circumstance; 2) categorical variables should be converted to binary variables, and those with limited observations or too many missing values should be removed; 3) factor scores (or weightings) for each of the components should be evaluated carefully based on knowledge of the study setting; and 4) households should be grouped into quintiles (if possible) for further analysis. By fully considering the caveats of using PCA, we will be more certain to draw well-informed conclusions from whatever findings might result.

**Theory of Change Analysis**

Perhaps the most interesting potential for replication lies in further investigating the theory of change postulated by GS. First, we intend to provide a visual map of the theory of change as it currently exists in GS. We then plan to test alternative causal pathways that might inform alternative theories of change that we interpret from GS's original work.

One of the more compelling (and surprising) findings is that households in the first treatment group (receiving land titles in 1989) had significantly fewer children than in untitled households (a substantial difference of 0.95 fewer children per household). Unfortunately, the authors' analysis seems to end here with a conclusion that land titles encourage investment in housing rather than the addition of more children to the family as a means of insurance against future shocks. Though this is a rational conclusion, it is open to certain criticisms. The analysis of

these data would benefit immensely if these findings were further unpacked to examine gender heterogeneities or other mediating effects.

The authors note that a female family member was responsible for decision-making in 95% of treatment households either solely or jointly. However, the analysis does not disentangle the heterogeneity of impact among households by gender of the household head, or by individual vs. shared household decision making. To more fully investigate the potential causal pathways behind the effect of land titling on fertility it seems important to disaggregate effects between households where decision-making was shared jointly by both genders and those where it was not. Depending on sufficient statistical power, further disaggregation could also take place across four types of households: joint households where the female is the head of household; joint households where the male is the head of the household; male-only headed households (currently reported as 5% of homes in the sample); and female-only headed households.

We hypothesize that female-only headed households may have driven the observed reduction in fertility, suggesting that when women are given title to their property they may be more inclined to have fewer children only in the absence of shared family planning decisions. Alternatively, we also plan to test the hypothesis that shared decision-making was a sufficient condition to influence the observed reduction in fertility (as evidenced in Feld 2003). GS did not seem to explore either of these possibilities, leaving room for further exploration of the theory of change in a replication.

In order to investigate these hypotheses, we will regress the outcome of interest (fertility) on a treatment dummy variable (0, 1) equal to 1 if the household has received the land title, a vector of household characteristics, and an interaction term – a product of the binary treatment variable times the type of household (female only; joint, female head; joint, male head; and male only).[1] The results should reveal whether impacts are heterogeneous among the various types of household heads.

We also plan to use this technique to better understand some of the other outcomes reported by GS. In particular, GS find that secondary and post secondary educational outcomes are improved for treatment households. Following our previous hypotheses, we plan to test whether these outcomes differ between female- versus male-headed households (or joint- vs. single parent-headed households). Arends-Kuenning and Duryea (2006) use LSMS data across Latin America to find stark differences between educational outcomes among children of single (female) parent homes vs. dual parent homes in which mothers are the heads of household. They posit "targeting benefits to children in single-mother families would reach more children at risk of poor school outcomes than targeting children in [merely] female-headed households" (263). Based on these findings, it is possible that very different educational outcomes might exist not only between female- versus male-headed households, but also between single female headed households and dual-parented, female or male headed households. Currently, GS's findings on educational outcomes merely control for gender, but do not examine heterogeneity of impact regarding gender (GS, 710). Furthermore, since GS find that treatment families with lower total numbers of children tend to experience better educational outcomes, it stands to reason that positive outcomes in education, like fertility, may be driven by gender-related influences.

An earlier publication by Galiani and Schargrodsky (2004) highlights the effects of land titling on child health using the 2003 data from the same population and natural experiment. These findings suggest that teenage girls experience less childbirth in treatment households (at a rate of 7.9% self reported pregnancy, versus 20.8% self reported pregnancy in the control

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[1] Keeping in mind that one will be dropped by Stata because of collinearity.

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households), though findings were not disaggregated by the gender of the household head.[2] In their earlier work GS do control for the relationship of the child to household head, and for the gender of the household head (Galiani and Schargrodsky 2004, 359) so we are confident that sufficient data exist to determine gender of the main head of household. However, we are not fully confident that enough female-only heads of household exist in the sample in order to have sufficient power to detect the effect size. In order to determine minimum detectable effect sizes for each of the heterogeneities we outline in the section above, we will provide ex ante power calculations, and again after our analysis is complete to show the level of power we have achieved.

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[2] In this previous publication, the authors utilize the same statistical techniques to account for non-compliance problems by instrumenting property rights with the offer to gain property rights, and estimating the effects of land titling by two-stages least squares regression analysis. The authors also use only the data from 2003 (since this study was published before the 2007 round of data collection in the 2010 study), and eliminate observations that moved into the area between the first and second allocation of land titles. However, whether the non-contiguous San Martin neighborhood was included in this analysis is unclear.

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