Fertility Responses to Land Titling: The Roles of Ownership Security and the Distribution of Household Assets

ERICA FIELD[†]
Harvard University

This paper examines the link between intra-household allocation of ownership rights and fertility using data from a nation-wide titling program in Peru. A stated objective of the program was to improve gender inequality of property ownership by including female names on land titles. I use data from the target population of urban poor to study whether improvements in ownership equality were associated with changes in household decision-making and fertility behavior.

I find that women in program regions are 50% more likely to appear as owners on property documents and 30% more likely to participate in household decision-making. My estimates indicate that land titling is also associated with a significant and sharp reduction in annual births among program beneficiaries of 21% in the year prior to the survey, and a 19% reduction in birth rates two years prior to the survey among households titled early in the program. Meanwhile, annual birth rates corresponding to children two years and older exhibit no significant differences according to whether the household resides in an early program neighborhood and is eligible for participation, consistent with the hypothesis that the program is responsible for the trend.

In addition to changes in female ownership, three other channels of impact are examined: the effect of titling on household labor force participation, wealth, and tenure-security related demand for children. Instrumental variables estimates provide evidence that increases in female bargaining power are at least partially responsible for the fertility decline associated with titling.

1. INTRODUCTION

In his recent book, Amartya Sen (2001) argues that women's empowerment, including female education, job opportunities and property rights, are key instruments for reducing fertility rates. There is now extensive statistical evidence linking women's education and labor force participation to demographic transition in many countries. However, there is far less empirical evidence on the influence of property rights on fertility. Land titling programs that incorporate gender equality of ownership as an explicit objective provide a unique opportunity to empirically evaluate this relationship.

This paper studies the link between land ownership and fertility among urban squatter households in Peru. I use data from a nation-wide urban land titling program that

[†] I am grateful to Daniel Andal<u>u</u>z in the COFOPRI office for providing the survey data. I also thank Anne Case, Hank Farber, Jeff Kling, David Lam, Murray Leibbrandt, Paul Shultz, and John Strauss, in addition to Princeton public finance workshop and IUSSP conference participants for comments and suggestions.

distributed over 1.6 million property titles over a five-year period to examine whether changes in ownership rights both at the household level and across individuals within households influenced fertility behavior within the target population of urban poor. Variation across individuals within households stems from the political objectives of program implementation. While traditionally land registration in Peru has been heavily biased in favor of male ownership, a key complimentary objective of the titling program was to equalize the gender distribution of real estate assets. To accomplish this, the program guidelines instructed that, among common-law and legally married household heads, both spouses' names appear on government-issued property documents. As a result of the titling program, 77% of new titles in married households included female names, as opposed to the old rate of 49%, providing a natural experiment in redistribution of household assets across male and female members.

The importance of women's ownership rights for childbearing depends on whether the pooled income hypothesis applies to fertility decisions in the Peruvian setting. There is some evidence from existing empirical work that female bargaining power may be an important factor affecting family size (See Schultz (1990), Rao and Greene (1993), and Rasul (2001)), although the magnitude and even direction of influence varies across studies. This analysis offers two principal advantages over previous work. First, land rights granted through a titling program arguably provide a cleaner source of variation in relative bargaining power than traditional measures, such as female earned and unearned income. In contrast to income, household participation in the titling program is convincingly exogenous to relative prices and past behavior. For instance, unlike changes in labor market opportunities, giving women rights to land affects their fallback position but not their ability to make contributions within the relationship. While individual property shares are generally correlated with past labor supply and other determinants of ownership rights, changes in individual property claims resulting from the Peruvian program are clearly independent of past behavior. The unexpected nature of the program also guarantees that property granted through the titling program is exogenous to marital choice, unlike assets controlled by individuals at the time of household formation.

Second, since legal claims to property are not as easily affected by family redistribution rules, land ownership may have a particularly strong influence on relative bargaining power of individuals within a household. While income can be readily transferred across individual members, residential ownership rights are necessarily "pooled" by all members until the point of household dissolution. Furthermore, property ownership is intimately related to an individual's ability to fulfill subsistence needs outside the family. For both reasons, reallocation of ownership rights has a unique potential to effect a significant and persevering change in divorce threat points and associated bargaining positions of individuals in a household.

While increasing women's agency by granting property rights presents one potential pathway through which land reform can alter the household demand for children, household-level increases in security of property may also have more general influences on child-bearing decisions. Three channels through which changes in tenure security could alter the time costs, direct costs, or benefits of child-rearing are examined in this paper. First, land titling induces a wealth effect, either through increased value of real estate or greater access to credit. Strengthening property rights also has the potential to alter the productive value of children, either through changes in the level of home versus market production or by reducing the role of children in securing informal tenure rights or providing old-age subsistence. Finally, increases in tenure security have the potential to change the value of adult household members' time at home and hence the opportunity cost of child-rearing.

To identify the effect of the titling program on fertility, I use data taken midway through the titling program to compare households that have been reached by the program to households that have not been reached. To control for potential unobserved heterogeneity between early and late program neighborhoods, I also make use of a control group of non-beneficiary households residing in both early and late neighborhoods. Difference-in-difference estimates indicate a substantial reduction in annual birth rates associated with urban land titling. In particular, residing in a program neighborhood reached prior to 1999 is associated with a 21% lower probability of birth in 1999. Similarly, neighborhoods titled prior to 1998 exhibit a 19% lower birth rate in 1998 relative to households titled afterwards.

Importantly, regression estimates indicate that all of this difference is concentrated among households that receive property titles in female names. Because female ownership rights granted through the program may not be exogenous to female bargaining power, to explore the role of female ownership on fertility I make use of survey data on household decision-making. I find that both program participation and female ownership is also associated with a significant shift in the degree to which women report participating in household decisions. Instrumenting female bargaining power with program participation and baseline characteristics of the household related to the likelihood of joint ownership and its impact on bargaining power suggests that granting women full equality in decision-making can account for the bulk of the observed decline in fertility in the years after the program.

To explore the importance of competing channels of influence, I exploit survey data on reported tenure security, property values, household labor force hours and the location of household entrepreneurial activity. Results from instrumental variables estimates support the first-round estimates, indicating that greater female agency in household decision-making exerts substantial negative influence on the desired number of offspring. Meanwhile, changes in the value of adult time at home, the organization of household productive activity and pure wealth effects appear to have little influence on child-bearing behavior.

2. CONCEPTUAL FRAMEWORK

The empirical literature on the microeconomic determinants of fertility is enormous. Decisions regarding the intended number and the timing of offspring have long been recognized to be part of households' economic decisions, interrelated with decisions concerning household consumption, savings and labor supply. Household production fertility models (Becker (1965), Willis (1973), Cain and Dooley (1976), Schultz (1980), Moffitt (1984), Hotz and Miller (1988), Eckstein and Wolpin (1989)) provide a framework to analyze this integrated decision problem in which children are viewed as durable consumption commodities, and parents make decisions about children as they do other consumer goods — maximizing utility subject to prices and a budget constraint. Becker and Lewis (1973) offer an alternative model in which the level of household expenditure on children, or child "quality", is also an explicit choice variable.

The abovementioned neoclassical models of fertility make the simplifying assumption that the household maximizes a single welfare function, treating the household as if either all members share the same preferences or there is a dominant decision maker (Becker (1965), (1981)). The unitary assumption additionally implies that households pool income from all members such that only aggregate household resources determine outcomes including fertility. In contrast, bargaining models predict that outcomes depend on the distribution of resources and preferences across household members. The cooperative bargaining models of McElroy and Horney (1981) and Manser and Brown (1979) assume that household resource allocation is determined according to the Nash bargaining solution, such that increases in assets owned by an individual increase that person's relative bargaining position and lead to changes in household allocations towards that individual's preferences. A rapidly expanding empirical literature has found much evidence against the pooled income hypothesis, supporting the notion that household decisions are better modeled as the outcome of a bargaining process (Schultz (1990), Thomas (1990), Gray (1998), Chiappori (1988, 1992, 1997)).

Both unitary and collective household models imply that changes in the cost and benefits of having children and in the income level of the household have the potential to affect fertility decisions. In addition, the collective model implies that changes in the bargaining position of individual household members may exert an independent influence on family size. The property titling program in Peru could conceivably inflict changes on any one of these parameters. First, if land titles increase property values or reduce credit constraints, households experience a direct income effect from the program, which has ambiguous influence on fertility. If children are a normal good, household demand for children will fall. Meanwhile, in the Becker-Lewis model, reduced incentives to send children to work will decrease the opportunity cost of child schooling, which has the potential to shift the household's optimal decision towards producing higher quality but fewer children.

In addition to the income effect, granting property titles may alter the opportunity cost of raising children. Household production fertility models imply that higher wages for children or adult men tend to increase fertility, while better job market opportunities for women tend to reduce fertility (Rosenzweig and Evenson (1977), Willis (1973), Layard and Mincer (1985)). Among urban squatter households in Peru, Field (2002) finds evidence that the increase in tenure security arising from property formalization reduced the need for households to keep adult members at home to protect the residence, thereby encouraging labor force hours in the outside market and discouraging market work at home. Both effects imply that receiving a formal property title lowers the opportunity cost of adult labor outside the home, and hence raised the opportunity cost of having children in urban squatter settlements.

Property titling also involves a potential shift in the benefits of offspring. Several authors have emphasized that children often provide direct production value to the household, an aspect that has received particular attention in the case of farm families (Rosenzweig and Evenson (1977), Rosenzweig (1980)). In the urban setting, improvements in security of property have been associated with the reorganization of market work from inside to outside the home, an effect which could also lower the value of children as a source of unpaid family labor. In addition, changes in the productive value of children may accompany property titling in settings in which children serve to increase informal claims to land or decrease the probability of expropriation of illegally held property. For instance, informal tenure systems might allocate land on the basis of need, exerting a positive influence on household fertility. An additional productive role of offspring in many settings is to provide old-age insurance to parents through intergenerational reciprocity, particularly in developing countries where social security systems are absent or incomplete. The shift to private property ownership in place of usufructuary rights (which may decline with age) has the potential to reduce parents' reliance on intergenerational transfers as a means of old-age insurance, providing an additional source of reduction in the benefits of children. Furthermore, the elderly may be able to use property to bargain for better care and support from extended families or other community members, rendering them less dependent on the survival and altruism of offspring.

Finally, because names of specific individuals are included on property documents, the titling program constitutes a person-specific transfer program. Hence, in households in which female members are given legal rights to the land, land titling programs have the potential to change the relative bargaining position of women. If the collective model of household decision-making applies to fertility choices in urban Peru and if household members differ in their desired number of children, changes in relative bargaining positions may have an independent influence on childbearing.[1]

In particular, I consider a standard Nash cooperative bargaining model in which a married couple can either remain married or divorce and live singly. There is a convex utility possibility set S containing all utility distributions (U_1,U_2) that could be achieved if they remain married. The utility of person i if he or she divorces is given by V_i. The assumption of potential gains to marriage implies that there are utility distributions (U_1,U_2) in S that strictly dominate the utility distribution (V_1,V_2). As in McElroy and Horney (1981) and Manser and Brown (1979), the outcome in marriage is assumed to be the symmetric Nash bargaining solution where the threat point is divorce. According to Nash bargaining theory, the outcome in this household will be the utility distribution (U^*_1,U^*_2) that maximizes $(U_1\text{-}V_1)*(U_2\text{-}V_2)$ on the utility possibility set S. This gives rise to a system of demand equations that depend on prices, individual income shares, and extra-marital environmental parameters (α) affecting the utility from being divorced (McElroy, 1990):[2]

[1] A significant amount of evidence suggests that males and females differ in this respect, including Rasul (2001), Mason and Taj (1987), and Pritchett (1994).
[2] Extra-marital environmental parameters are defined by McElroy (1990) as "parameters that shift the threat point but do not affect prices and non-wage income faced by married individuals." Examples include sex

$$q_i = h_i(p, I_f, I_m; \alpha_f, \alpha_m)$$

In the basic divorce threat model, the outcome in a marriage is completely determined by the utility possibility set and by the position of the threat point, (V_1, V_2). Hence, changing who owns property when the couple is together will have no effect on the distribution of utilities if there is no change in ownership rules in the event of a divorce. Meanwhile, changes in the extra-marital environmental parameters that affect the utility of being single will affect the distribution of utility within the household and hence may change household decisions including fertility. There is little doubt that legal claims to property alter the utility of being single, as ownership rights are largely a parameter describing the legal structure within which divorce occurs. Hence, even if property values do not rise with formal ownership, by changing the legal ownership rights of female household members, the Peruvian titling program alters women's utility of being divorced and therefore the threat point of female household members. If female members have on average lower desired fertility, changes in the threat point will lead to a reduction in birth rates.

3. EMPIRICAL STRATEGY

3.1 Project background[3]

This research examines the Peruvian government's recent series of legal, administrative and regulatory reforms aimed at promoting a formal property market in urban squatter settlements. In 1996, under the auspices of the public agency, COFOPRI (Committee for the Formalization of Private Property), and *Decree 424: Law for the Formalization of Informal Properties*, the Peruvian government embarked on an innovative nationwide program whose goal was "rapid conversion of informal property into securely delineated land holdings by the issuing and registering of property titles" (World Bank, 1992). Implementation involved area-wide titling, in which project teams entered one neighbourhood at a time, moving contiguously within cities until all informal settlements had been reached (World Bank, 1998). While the old process of acquiring a property title was prohibitively slow and expensive, the new process was virtually free and extremely rapid (see Field (2004) for an overview). Eligibility for program participation required title claimants to verify pre-1995 residency on eligible public properties, generally using informal title documents from local registries, post-dated mail, utilities bills or signed sales documents. As a result of the reforms, roughly 80% of the country's eligible residents became nationally registered property owners, affecting approximately 6.3 million individuals.[4]

---

ratios of potential mates, legal rules surrounding marriage and divorce, and systematic differences in non-wage income between married and non-married states.

[3] This section borrows heavily from Field (2002).

[4] By December 2002, 1.64 million lots had been formalized and 1.21 million titles granted, the vast majority of which took place between 1998 and 2000.

3.2 Data

The data employed in this analysis come from a March 2000 survey of 2750 households randomly sampled from the program target population in eight cities of Peru.[5] The survey instrument contains a wide variety of information on household and individual characteristics. These data comprise 4433 women between the ages of 14 and 50, 42% of which are married or partnered, 8.3% are separated, divorced or widowed, and the remaining 49.8% are single.[6] Unfortunately, children and mothers cannot be precisely matched in the data, so I use a basic algorithm to match each child to a mother in the household based on information on age, relationship to head and family structure, detailed in Appendix A. The matched records are used to construct an individual-level data set comprised of all fertile-aged women in the sample containing the number of children of each age born to each woman.

An alternative approach is to estimate fertility using household-level data on the number of children and the number of fertile-aged women in the household. Household-level estimates are reported in Appendix B and Appendix C. Because measurement error is a potential concern in the individual-level analysis, it is worth noting that the pattern of results does not differ greatly from the household level-analysis.

3.3 Difference-in-difference identification strategy

The survey was taken mid-way through program implementation, so the data capture a random sample of eligible title recipients in both treated and yet-to-be treated neighborhoods. In particular, 63% of households in my data reside in neighborhoods in which the program has yet to enter. My identification strategy makes use of the staggered program timing by comparing birth outcomes of women in treated neighborhoods to birth outcomes of women in neighborhoods in which the program has not yet entered. Neighborhoods are classified as having been reached by the time of the survey if more than one household in the cluster reports owning a COFOPRI title.[7] Since the households in the treated neighborhoods may or may not actually have received a government title by the time of the survey, I employ an intent-to-treat (ITT) analysis.

Although target areas for wide scale economic development programs are never randomly selected, these data have the advantage that all sample members live in areas that will eventually be targeted for program intervention, increasing confidence in the comparability of treated and untreated households. Furthermore, the universal nature of the treatment and the participation rules of the program generally rule out concern over

[5] The sample was stratified on city, with cluster units of ten households randomly sampled at the neighborhood level within cities.

[6] Here, "partnered" is defined as unmarried co-habitation, or a non-spouse "conyuge".

[7] Reasons that households may be excluded include: the household cannot prove residence prior to 1995; the household belongs to a cooperative association; the residence lies on an archeological site, flood plane, mining site or private property; and ambiguous or disputed ownership claims. Unfortunately, none of the above information is collected in the survey. According to anecdotal evidence from program administrators, disputed claims within families or between neighbors are the most common reason that title distribution is delayed for an untitled household in a treated neighborhood (Carlos Gandolfo, personal interview, Lima, August 9, 2000).

individual selection bias that could arise even if program placement were random. Nonetheless, there is still potential for program *timing* bias, in which areas selected for early program participation are different from the rest, which could lead to a biased estimate of program effect.

The influence of non-random city timing is easily resolved by including city fixed effects in the regression estimates.[8] A more complicated source of program timing bias concerns the order in which project teams entered neighborhoods *within* cities. While the available information on program timing suggests that program timing was largely exogenous to the economic environment of neighborhoods, without precise knowledge of the formula for neighborhood timing I cannot safely assume random assignment to treatment nor accurately specify a selection on observables model. Hence, cautious quasi-experimental analysis calls for an estimation strategy that is robust to potential selection on unobservables.

As a further means of controlling for unobservable heterogeneity between early and late program neighborhoods, I make use of a control group of non-beneficiary households. In particular, among the eligible households, roughly 60% already have a registered property title (non-squatters), so receive relatively few benefits from program intervention.[9] In this manner, I identify the program effect on fertility with a quasi-experimental difference-in-difference estimator in which households that lack a title prior to intervention form the treatment group and households that already posses a title prior to the program serve as the control group. The simple idea underlying this distinction is that the tenure security effect of titling disproportionately (or solely) benefits households with weak ex ante property claims, for whom the demand for tenure security is high.[10] The staggered introduction of the titling program across neighborhoods provides the exogenous change that only affects the economic environment of the treatment group. Hence, the difference in the number of offspring of squatters before and after the acquisition of a property title can be compared to the difference among women in households that already possessed a property title at the time of intervention. While possible endogeneity in neighborhood program timing raises questions about identification from simple comparisons between women in early and late neighborhoods, the difference-in-differences estimator eliminates any additive area fixed effects.

[8] The only information on the ordering of cities comes from a vague statement in the World Bank Project Report (#18359), which specifies that the order was designated in advance according to "ease of entry." As far as neighborhood program timing, there appears to have been no specific algorithm in the program guidelines. The COFOPRI office claim only that order was subject to "geographical situation, feasibility to become regularized, dwellers' requests, existing legal and technical documents, and linkages with other institutions involved in the existing obstacles" (Yi Yang, 1999).

[9] See Field (2002) for a detailed description of the titling procedure and definition of the control group.

[10] There were several ways a household might have obtained a property title in the era before the recent titling effort. First, there was always the lengthy and costly option of following the official bureaucratic process for obtaining and registering a municipal property title. Second, there were a handful of past isolated attempts at property reform in which interim titling agencies were set up by municipal governments in an effort to incorporate some proportion of informal residents (De Soto, 1986). Finally, on a number of occasions, mayoral and presidential candidates were known to distribute property titles in an effort to win voter support prior to an election (Yi Yang, 1999).

The fertility outcomes I explore are annual births before and after program intervention, identified as mothers who have a child less than one year old at the end of a one-year interval.[11] The difference-in-difference estimator for births in year *t* is obtained from the following equation:

$$Birth_{it}=\beta_0+\beta_{1t}(program)+\beta_{2t}(squatter)+\beta_{3t}(program*squatter)+\beta_{4t}'A_{it}+\beta_{5t}'X_i+e_{it} \quad (1)$$

Here, *program* is an indicator of whether the titling program has entered individual *i's* neighborhood, and *squatter* is an indicator of whether individual *i* lives in a household that had a property title before the program entered. The estimated coefficient on the interaction between being a squatter and residing in a program neighborhood, β_3, represents the marginal change in the probability of having a child for squatters in program neighborhoods relative to squatters in non-program neighborhoods. Finally, A_{it} is a vector of time-varying individual characteristics that includes age group and birth parity in year *t*, and X_i is a vector of time-invariant individual and household characteristics.[12] By including age as a right-hand side variable I approximate the program effect on age-specific fertility rates.

Parity is a critical parameter to include in any empirical model of fertility to account for differences in the marginal cost of child rearing. Since families with no children face a higher marginal cost of child bearing than do families with children, behavioral responses to the program should vary with family size. To account for immediate reductions in fecundity due to postpartum amenorrhea, I also include an indicator of whether or not the woman gave birth in year *t-1*.

Time-invariant individual characteristics relevant to fertility outcomes and included in X_i are marital status, including whether the woman is married or has a partner, whether the spouse or partner resides in the household, literacy, and education level at the time of the survey; relationship to household head (whether woman is spouse, child, or grandchild of head, other family member, or non-family member), whether the woman was born in another province and this interacted with whether the woman moved to the city to marry. From previous work, female education and literacy are potential determinants of both fertility and bargaining power, as they correspond to desired number of children, fertility control, and fallback position (Argawal, 1997; DasGupta, 1995). The last two measures account for differences in external social support networks and access to community resources, which are potential determinants of intra-family bargaining power (Argawal, 1997).

---

[11] In studying annual birth rates, I am not directly estimating the program impact on the total fertility rate (TFR). Reduction in annual birth rates is a combination of delayed and reduced fertility. With a few assumptions about time trends of age-specific fertility, the TFR can be backed out of the age-specific annual birth rates

[12] In the household-level estimates, the following equation is run:

$$Babies_h = \beta_0+\beta_1(program)+\beta_2(squatter)+\beta_3(program*squatter)+ \beta_4'A_h +\beta_4'X_h+e_h$$

Here, A_h is a vector of the household number of women in different fertile age groups, and X_h includes the same demographic characteristics as X_i , but discrete individual characteristics are here grouped according to the number of fertile-aged women in the households with each characteristic (i.e. education levels, marital status, relationship to head, birth parity).

Because there are large differences in the regional composition of treated and non-treated neighborhoods, I control for city-wide differences in fertility patterns by including dummy indicators of the city of residence. The following set of household characteristics is also included in X_i, largely to account for differences in baseline family income: education level of the household head, the highest hourly wage among male wage-earners in the household and a dummy indicator of whether the household head is male. The labor income of fertile-age women is excluded on account of possible endogeneity resulting from a decline in wage rates associated with child-rearing. In contrast, male wage income is not likely to respond to the presence of children. Although the data contain little information on non-labor income, as a proxy for non-labor income I include the size of the household property as well as the number of years the household has lived in the neighborhood. For poor urban households, housing and land are likely to constitute the bulk of assets. To control for potential differences in the availability of family planning interventions, I include an indicator of whether any household in the cluster is using a family planning clinic.[13] Summary data on all variables used in the analysis are provided in Table 1. As mentioned in the previous section, the most important thing to note in these data is that baseline characteristics are remarkably similar across women in program and non-program neighborhoods. Controlling for city-wide fixed effects accounts for the existing differences in residential tenure and education.

From the survey data I examine the program effect on whether a woman gives birth by estimating equation (1) separately for each year t prior to the survey, where the comparison groups are households reached by the program before and after the start of year $t-1$.[14] Because most treated households in the data participated in the program one to three years prior to the survey, the analysis of fertility responses to the program is necessarily limited to studying birth rates in the three years prior to the survey.[15] However, the ability to examine fertility prior to 1996 from inferred birth histories enables a useful control experiment. In particular, since the program can have no effect on pre-1996 fertility behavior, I can test for the absence of a program effect on annual birth rates between 1994 and 1995 observed from the number of four- and five-year-olds reported in the data.[16]

[13] The specific survey question is: “Have you or any member of the household participated in or benefited from family planning campaigns?” The existence of family planning programs does not appear to be related to the timing of program intervention.

[14] Only the year and not the month that the program reached a neighborhood is available from the survey data. Year $t-1$ is chosen as the relevant cut-off point for program influence on births in year t since birth outcomes within the year reflect fertility decisions made at least nine months beforehand. Furthermore, since I define births according to age of the child at the time of the survey, births in year t actually refer to births during the twelve-month interval between March $_t$ and March $_{t+1}$. In other words, I assume that in order for the titling program to influence the birth of a child during March 1999 and March 2000, the program must have reached the neighborhood before 1999.

[15] The survey was conducted in March 2000, so the set of treated neighborhoods includes neighborhoods reached between 1995 and 2000, the bulk of which were reached between 1998 and 2000.

[16] In these estimates, I exclude women residing in the 50 households titled prior to 1995 during the pilot titling project conducted by the Institute of Liberty and Democracy 1992-1994.

It is important to keep in mind throughout the analysis that the set of "program" households varies with birth year *t* since recently titled households were not necessarily treated at the time of earlier fertility decisions. Hence, in measuring the program effect on birth rates during the year before the survey (or the observed number of children less than one year old), households residing in neighborhoods in which the program entered in 2000 are included in the control group of non-program households. Likewise, in studying the program effect on birth rates between 1998 and 1999 (measured by the observed number of one-year-old children), households in program neighborhoods reached in both 1999 and 2000 are included in the control group.[17]

4. RESULTS

4.1 Annual births

I begin the analysis by exploring general fertility trends among women in the four categories of households in the sample. Table 2 presents age-specific annual birth rates in 1999 and 1994 without accounting for population differences in relevant household and individual fertility determinants. The 1999 birth rates are plotted against age in Figure 1 to illustrate the trend in birth profiles by age. Here we observe a clear pattern of variation in birth profiles across squatters and non-squatters in program and non-program regions. In particular, while the age-specific fertility patterns of non-program households follow the same general pattern, the fertility of squatter households falls significantly below that of non-squatter households in program neighborhoods. In contrast, annual birth rates prior to the program (in 1994) look remarkably similar across squatter and non-squatter households. Figure 2 plots birth rates in 1994 corresponding to women in the four categories of households. This discontinuity over time revealed by the comparison between 1994 and 1999 suggests a shift in birth patterns that takes place between 1995 and 1999, and corresponds only to beneficiaries in early program neighborhoods.

In comparing age-specific births in 1994 inferred from the number of 5-year-olds in the household with births in 1999 inferred from the number of newborns, it is worth noting that the birth rates are significantly higher for the older cohort of women and the implied birth timing is also shifted downwards by 2-4 years. Rather than reflect a dramatic sample-wide time trend in fertility, these differences illustrate that the sample of women aged 15-50 cannot be perfectly compared to the sample of women aged 20-55 in the data set given that the age distribution of women in the selected households does not reflect a random sample of women from the population. In particular, the households eligible for program participation must have at least four years of residential tenure. Hence, there are a disproportionate number of "new" families absent from the data, which are likely to include a large number of newborn children.[18] This limits the usefulness of older cohorts as a comparison group. In addition, sample characteristics

[17] With respect to the control experiment, the distinction between early and late treatment is irrelevant in the analysis of birth rates prior to the start of the program.

[18] In other words, there is a higher rate of migration out of the sample among younger members. Because of this inherent aging of the sample population, if we observed the same families four years later we would observe an even lower number of newborns relative to five-year-olds.

such as the population age distribution, provided in Appendix D, are important to keep in mind in extrapolating the results of this analysis to other populations.

Both Table 2 and Figure 1 also reveal that the bulk of the observed fertility difference corresponds to women between the ages of 25 and 35. Relatively young and relatively older women in squatter households in program neighborhoods have roughly the same annual birth rates as their counterparts in non-program regions. Table 3 presents fertility patterns according to birth parity and program participation. Here we see that, with respect to recent births (1998-1999), both squatter and titled households in program regions have a higher proportion of childlessness than non-program households. However, this contrast is significantly greater among untitled households. Within the population of squatter households, women in program neighborhoods begin having children at one-third the rate of women in non-program neighborhoods. In general, zero-parity is by far the sharpest determinant of total fertility rates within a population. For example, the numbers in Table 3 correspond to a total fertility rate (TFR) post-program that is only slightly less than one-third of the TFR among squatters before the program. Not only are women in program households less likely to start a family, but the numbers in Table 3 also indicate that the program affects higher order birth parities, with women in program neighborhoods much less likely to proceed beyond two children. In fact, in 1999 *no* women in squatter households proceed beyond three children!

Table 4 presents the estimated marginal probabilities from probit estimates of annual births 1993-1999 of the subpopulation of program beneficiaries only, or women living in households that did not posses a title prior to the program. In order to estimate the program effect on births over the three years before the survey, in columns (1) - (3) the sample is divided into households reached by the program prior to 1999, prior to 1998 and prior to 1997. Columns (4) – (6) make use of the same geographic division in order to compare fertility patterns three years beforehand. The results demonstrate that birth rates over the past three years follow a strikingly consistent pattern with respect to program intervention, while birth rates in the three years prior to the program exhibit no variation according to the same division of early and late program neighborhoods. Birth rates in both 1999 and 1998 are 0.009 and 0.007 percentage points lower among women in program neighborhoods, and the differences are statistically significant. This corresponds to a 23% and 19% decline in annual birth rates in those years. In addition, the point estimate on the estimated coefficient for program households in the very early stages of the program (prior to 1997) is negative and has a similar magnitude (0.007), although the standard error on this point estimate is too large to draw conclusions. While these magnitudes are large, they are comparable to the effect of changes in tax incentives on annual birth rates found in other studies, such as Milligan (2001).

Although Table 1 reveals that the sample of squatter households is observationally similar in early and late program neighborhoods, to account for possible unobservable differences between households according to program timing, the next set of estimates makes use of non-beneficiaries as a control group. These results are presented in Table 5. The estimates in all six columns correspond to equation (1), where the estimated program effect on fertility is captured by the interaction between being a squatter and participating in the program. Here we see that the observed fertility effect on births in 1998 and 1999 is concentrated entirely among titling program beneficiaries (squatters). The first column reveals that squatters in early program neighborhoods have a 0.012 percentage point lower annual birth rate in 1999 than squatter households in late program neighborhoods, while in 1998 that difference is 0.007 percentage points. Both point estimates are statistically significant.

The regressions corresponding to columns 2 and 5 include a binary indicator of whether the household actually received a title through the program. Results from these estimates confirm that the intent-to-treat effect is indeed concentrated among beneficiaries who were titled by the start of the birth interval. For households that possessed a title before the program there is no difference in the likelihood of giving birth across program and non-program neighborhoods, as indicated by the estimated coefficient on the program variable. Hence, it does not appear that differences in annual birth rates among squatters corresponding to program timing are a result of unobserved heterogeneity across early and late program neighborhoods.

As a further robustness check against the endogeneity of program timing, an alternative difference-in-difference estimator makes explicit use of women in older cohorts as a control group. Since the bulk of the program effect is observed among women between the ages of 25 and 29, I estimate the change over time in this age-specific birth rate between squatter and non-squatter households in program neighborhoods. Using birth history information for women currently between the ages of 30 and 34 inferred from existing five-year-old children in the household, I look at the likelihood that women in program neighborhoods ages 25-29 gave birth in 1999 compared to women from the same age group in 1994. The same comparison is also made for 1998 relative to 1993, among women aged 26-30 relative to women aged 31-35. The estimated probit model is analogous to equation (1):

$$Birth_i=\beta_0+\beta_1(squatter)+\beta_2(early\ cohort)+\beta_3(squatter*early\ cohort)+\beta_4'A_i+\beta_5'X_i+e_i \quad (2)$$

Results from these estimates, presented in Table 6, show the same pattern as the estimates using late program neighborhoods as a control group. The estimates indicate that the relationship between fertility and program participation is robust to using alternative control groups both inside and outside the program neighborhoods. However, the results depend on the absence of unrelated cohort effects on fertility that differ across squatters and non-squatters.

4.2 Intra-household allocation of property

The second component of the analysis attempts to gauge whether the observed program effect on fertility patterns is related to changes in the level of female property ownership accomplished by the titling program. As discussed in section 2, one of the primary goals and accomplishments of the COFOPRI titling program was to increase the number of female names on formal property documents. Such a shift in relative ownership rights has the potential to increase female bargaining power, which could cause birth rates to fall if women's desired number of offspring is on average lower than men's.[19]

As a first step, I estimate the magnitude of the redistribution that resulted from these program guidelines. In the survey, all households with titles were asked which individuals' names appear on the property document.[20] With this information I construct a dummy variable indicating whether the title belongs to any female member of the household. Table 7 reports the fraction of households in program and non-program neighborhoods in which property documents are recorded in female names. Here we observe that 77% of couples that received a property title through COFOPRI share ownership rights to the residence. Subtracting the difference among non-beneficiaries in rates of female ownership across early and late program neighborhoods indicates that the program increased the rate of female ownership by 54 percentage points.[21]

It is important to keep in mind that the analysis is limited to examining the relationship between changes in *legal* ownership rights and fertility outcomes. Hence, in table 7, the fact that no squatter households in non-program areas have female owners reflects the fact that survey data on ownership rights was restricted to formal legal ownership, and none of these households have titles by the time of the survey. Because traditionally recognized informal ownership rights within households are unobservable, so is the change in recognized ownership rights within the family.

4.3 Female ownership and fertility

Having established a first-stage effect of the titling program on the inter-household allocation of property, I proceed to look for evidence of an associated change in female bargaining power that may influence fertility decisions. Since not all couples that receive a title through the program are granted joint ownership, I can examine differences in the program effect across households with different ownership patterns by augmenting the Table 6 regressions with the indicator of female ownership. The coefficient estimates on the interaction terms in the following equation allow me to observe whether the difference in annual births is indeed concentrated among households with joint ownership:

$$Birth_i = \beta_0+\beta_1\ (program)+\beta_2\ (squatter)+\beta_3\ (program*squatter)+\beta_4\ (title*squatter)+\beta_5\ (female\ name\ on\ title)+\beta_6\ (female\ name\ on\ title*program) +\beta_7\ (female\ name\ on\ title*squatter)+ \beta_8'A_i+\beta_9'X_i+e_i \quad (3)$$

Equation (3) is the same regression as (2) but includes dummy indicators for whether or not the household actually received a property title prior to the potential birth year and whether or not a woman's name appeared on this property document.[22] It is

[19] The survey data contain no information regarding desired number of offspring.
[20] Respondents could report any number of names, and survey-takers were asked to verify these reports with information from the actual title whenever possible.
[21] Controlling for city fixed effects reveals that the difference between early and late neighborhoods among non-beneficiaries is driven entirely by differences across cities. In particular, Lima has a much greater gender equality of ownership than other cities in Peru, and was also the first city to be titled.
[22] As opposed to the indicator of whether or not any woman in the household is an owner, another possibility is to look only at whether a fertile-aged woman is herself owner of the property. I opt to

important to note from equation (3) that, in contrast to the intent to treat estimates obtained from equations (1) and (2), to explore the nature of the program effect among households with female ownership rights, I distinguish households that actually received a title prior to the potential birth year from households residing in the neighborhood and eligible for a property title but that were not titled by 1999. An intent-to-treat approach that incorporates information on female ownership would be possible only with data on the allocation of property rights within untitled households.

In equation (3) the variable *title* corresponding to β_4 serves to control for the size of the total household wealth transfer involved in the titling process independent of which family members have rights to that wealth. Meanwhile, the coefficient on female ownership, β_3, indicates whether the influence of titling depends on the gender of the owner. As mentioned earlier, if the unitary model applies to household decisions over fertility, the program effect should be independent of which family members receive the transfer. However, here as in a most contexts, female property ownership is likely to be endogenously linked to female bargaining power, such that the above OLS estimator is biased. The COFOPRI program is useful in overcoming such endogeneity concerns only insofar as the titling program conferred joint ownership independent of household demand for equality. Clearly, in order to obtain an unbiased estimate of the effect of female ownership using variation in the nature of ownership rights granted through the program, title claims must be exogenous to relative bargaining power within households, a problematic assumption without information on the process by which names were determined when program guidelines were not followed.

Interacting the female ownership indicator with survey data on property values provides an additional source of variation in intra-household property reallocation associated with the titling program. In particular, receiving ownership rights over relatively valuable land should constitute a larger wealth transfer to women and hence likely corresponds to an even greater increase in the divorce threat point.[23] To account for differences in property values, I make use of data from a survey question in which respondents were asked to estimate the selling price of their property. As an additional indicator of property value, I utilize information regarding the perceived tenure security of the residence, or the stated likelihood that the government or another resident will evict current residents. Hence, I distinguish between women who gain strong legal rights to land versus those that gain only weak rights to the land. Finally, I make use of data on loan transactions of the household in which the property title was used as collateral. By redistributing property to women and by increasing credit access among title-holders, the

concentrate on the broader measure of female ownership rights primarily because the bulk of property owners are already out of or in later stages of the fertility cycle. Hence, the majority of women in the margin of influence are daughters or daughters-in-law of the household head. However, increasing the bargaining power of the spouse relative to the household head has the potential to increase fertility incentives of daughters or granddaughters living in the household. For instance, a woman may be more likely to transfer ownership rights explicitly to her daughter or daughter-in-law. Furthermore, the head and spouse may have considerable influence on the fertility of other family members.

[23] However, if relative ownership shares between men and women determine bargaining power, there is no observable variation.

COFOPRI program increased the number and amount of loans over which women in the household have a legal claim. If women have a legal claim to property that can be used to secure a loan, they should also have greater legal claim to financial resources acquired with the use of a property title and, hence, more bargaining power over household resources. Because I am using data on selling prices and tenure security for which retrospective data is unavailable, this component of the analysis is limited to studying the impact on births in 1999 only. The following regression incorporates property values into the above difference-in-difference estimate:

$$\begin{aligned} Birth_i = {} & \beta_0 + \ldots + \beta_8(property\ value) + \beta_9(value*squatter)\,) + \beta_{10}(value*program) \\ & + \beta_{11}(value*program*squatter) + \beta_{12}\,(title*\ value*squatter) \\ & + \beta_{13}(female\ name\ on\ title*\ value*squatter) + \beta_{14}'A_i + \beta_{15}'X_i + e_i \end{aligned} \tag{4}$$

The interaction between property value and the indicator of whether property documents are in female names, β_{13}, captures variation in the magnitude of the reallocation of assets that resulted from the program.

Table 8 presents the probit results from (3) and (4). The first column controls only for whether any woman's name is included on the document, while the remaining columns include additional controls for property values, amount of formal credit secured with property documents and tenure security. The estimates reveal strong differences in fertility outcomes between households with and without female owners. In particular, the estimate in column (1) demonstrates that households with female owners account for the entire program effect on fertility. Columns (3) and (5) indicate that the estimated effect falls with the value of property, as measured by the amount of loans acquired with collateral and the estimated selling price of the residence. In other words, the effect of female ownership on fertility appears to be larger for women who acquire ownership rights to more valuable lots. Both of these variables also exert an independent negative influence on fertility, suggesting a negative baseline relationship between household wealth and demand for children.

With respect to tenure security, the effect of female ownership on fertility is positive and significant. This result contradicts the original hypothesis that tenure security is simply another indicator of property value. While security of property is a critical component of real estate value, the discussion of section 2 revealed that ownership security is also itself an independent mediator potentially affecting child-bearing decisions. One plausible explanation for the decreasing role of female ownership with tenure security is that security narrows the gender differential in desired quantity of children. As discussed earlier, if the productive value of children declines with tenure security, so will household-level demand for offspring. This also implies that male and female desired fertility are likely to converge as tenure security rises. Hence, the negative correlation between eviction likelihood and female ownership could reflect the fact that – when tenure security is high enough – male demand for children approaches female demand.

4.4 Female ownership and household decision-making

For many reasons, legal rights over household resources may not automatically translate into improvements in female bargaining power. For instance, legal transparency may be such that women are not adequately aware of their increased rights. Furthermore, traditional patterns of ownership may remain a predominant influence on intra-household bargaining behavior if redistribution is not accompanied by social legitimacy, or "extra-household" bargaining power (Argawal, 1997). In this section I explore with a probit analysis whether female property ownership is associated with changes in the decision-making power of the household head. If a key mechanism to reducing fertility is granting women more agency in household decisions, then the importance of female ownership rights should be reflected in their participation in household decisions. Two caveats apply in making this link. As noted by Argawal (1997), "more fundamentally, relative bargaining power is revealed in whose interests prevail in the decisions made," rather than in who participates. Furthermore, participation in decisions about leisure may not be reflective of household decisions regarding fertility.

To gauge whether there is a first-stage effect of the program on bargaining power, I make use of a direct measure of household equality in decision-making based on a survey question in which the spouse of the household head was asked to describe how the family makes a decision about how to spend leisure time together. As a proxy for female bargaining power, I distinguish between households in which decisions are made by the household head alone versus those in which household members decides jointly.[24] This dummy variable is used as a dependent variable in probit equations analogous to (3) and (4). The sample is necessarily restricted to households in which the head is partnered or married and the partner or spouse of the head is residing in the household. As in the previous set of estimates, I also compare households with different levels of property wealth and tenure security to examine whether the magnitude of the reallocation is correlated with who makes decisions in the household. In addition to the set of demographic controls included in the fertility estimates, X_i includes three variables that may directly influence intra-household bargaining shares: age difference between household head and spouse, and whether the property was inherited from the family of either the head or the spouse.

From these estimates, reported in Table 10, it appears that redistributing property to women significantly affects the likelihood that they participate in household decisions, further evidence of changes in bargaining power associated with legal claims to property. As reported in the first column, female participation in household decision-making appears to depend heavily on female ownership rights in joint households. In particular, women are 14 percentage points more likely to participate in household decisions about how to spend free time if property is recorded in their name. The magnitude and significance of this coefficient estimate persists in all of the Table 10 regressions.

[24] The exact survey question is: When your family has the opportunity to go out, how do you come to a decision about where to go? The response options are: (1) The household head knows what to do and decides; (2) We discuss the alternatives and the household head decides; (3) We discuss the alternatives and postpone the decision if we cannot agree; (4) We discuss the alternatives and decide jointly.

Meanwhile, in columns (3) and (5), interactions between female ownership and both credit access and property values are small and insignificant.

In contrast, although the role of female ownership appears to be unrelated to the value of the residence or the amount of credit securitized with property, the interaction between tenure security and female ownership is strongly negative and significant. Conditional on having ownership rights, women's influence in household decisions is strongly increasing in the tenure security of the residence. This result is consistent with female bargaining power rising with the value of the property given to them. It is also consistent with the previous interpretation of the negative coefficient on the interaction between tenure security and female ownership in Table 8. The fact that higher tenure security gives rise to greater participation of women in decision-making suggests that bargaining power is indeed increasing in tenure security as one would expect. Hence, it is reasonable to assume that the decreasing role of female ownership on fertility decisions with tenure security does not reflect a negative relationship between bargaining power and tenure security, which would be difficult to explain. Instead, it is reasonable to conclude that, as tenure security rises, the preferences of men approach those of the women, so their increased role in decision-making has less influence on fertility outcomes.

4.5 Instrumental variables estimates

While the initial evidence on female ownership is compelling, it is impossible to draw strong conclusions about the observed changes in relative ownership rights and fertility outcomes without the unrealistic assumption that document names were randomly assigned. Since this is unlikely to be the case, I instead make use of the available information on household decision-making to account for presumed changes in female bargaining power associated with ownership rights granted through the program. If the survey question on decision-making adequately captures variation across households in relative bargaining power and if bargaining power is the only pathway through which relative ownership rights influence fertility, then directly estimating the effect of bargaining power on fertility patterns will be sufficient to capture the impact of changes in ownership rights. As before, the primary empirical equation of interest is:

$$B_{i,t} = \alpha + \theta_{i,t}\beta_3 + X_{i,t}\beta_6 + \varepsilon_{it} \tag{7}$$

where θ is a measure of female bargaining power and the dependent variables of interest are once again whether a woman gave birth in 1999 and in 1998. Given that the effect of θ, is likely to be correlated with the error term in the above equation, OLS techniques may produce biased estimates. For example, both household decision-making and underlying fertility preferences are likely to be associated with social conservatism. To control for such biases I use an instrumental variables model. Here the endogenous variable is a four-level index of female participation in household decision-making which I instrument with program participation interacted with baseline characteristics of the household, maintaining the ITT framework.[25] In particular, I interact the program with an indicator of whether the household possessed a title prior to intervention in addition to the age difference between spouses and the year the program entered the neighborhood. Inspection of the data reveal that year in which the program entered the neighborhood is a relatively good predictor of joint ownership among program households since it appears that adherence to the program guidelines increased over time. In contrast, age differences between spouses is likely to have a negative affect on whether female names were included if household preferences were influential in determining this outcome. However, so long as some fraction of program households were assigned joint ownership independent of preferences for equality, the relationship between bargaining power and age difference of spouses should be less steep for squatter households in program areas relative to non-participating households. Here, the idea is that joint ownership conferred on households in which female bargaining power is ex-ante low induces a greater change in relative bargaining power than does granting ownership to women in relatively equal households.

As discussed in Section 2, there are several pathways through which increases in ownership rights might influence fertility. The four principal mechanisms are: income effects from increased property values or access to credit; reduction in the productive value of children related to either home production, informal tenure security or old-age subsistence; increases in the opportunity cost of having children arising from increased incentives for adults to work outside the home (either through an increase in household work hours outside the home or a shift in market work from inside to outside the home); and increased female bargaining power in fertility decisions arising from changes in the intra-household allocation of ownership rights. Recall the household demand function for child quantity in the Nash bargaining model:

$$q_i = h_i(p, I_f, I_m; \alpha_f, \alpha_m)$$

In order to identify an IV model using program participation as an instrument for the effect of bargaining power on fertility, the exclusion restriction requires that the relationship between land titling and fertility is fully mediated by changes in bargaining power, such that relative ownership rights are the *only* pathway through which the titling program affects fertility. This exclusion restriction becomes much more plausible once I consider the interactions between spousal age difference and program timing, baseline characteristics which have a clear relationship with changes in bargaining power but not other fertility-related behaviors.

However, since the exclusion restriction may not hold, I also estimate an IV model with multiple endogenous variables to capture the multiple mediators affecting prices, income shares, and divorce threat points. In particular, I examine the influence on birth outcomes of the following household effects of receiving a title: reported selling price of the residence; a four-level index of reported tenure security; whether or not the residence is used as a source of economic activity; total weekly household labor force

[25] The binary indicator of female participation in household decision-making used in the previous section is constructed from the same survey question.

hours; and the index of female participation in decision-making. The reduced form demand function for children obtained from the above demand shifters is thus:

$$q_i = \psi(S_h, H_h, P_h, \pi_h; \theta_h, X_i)$$

where i represents each woman aged 15-50 living in household h, S is an index measure of tenure security, θ is female bargaining power, π is whether or not the household engages in home production, H is weekly household labor force hours, P is change in real estate value, and X is a vector of individual-level covariates predicted to influence fertility. To estimate the change in the selling price of the residence associated with receiving a property title, I regress log selling price on the following observable characteristics of the residence: property size, age of residence, number of rooms, number of floors, floor material, roof material, whether the residence had plumbing, electricity, and an indoor bathroom and binary indicators of whether or not the residence had a property title prior to the program and whether or not the property was titled through the program. Predicted log selling price prior to the program is then calculated by setting the program title indicator to zero. The log difference between the predicted and actual values approximates the change in home value related to receiving a property title.

Since the titling program affects multiple aspects of behavior related to fertility, using the program as a single instrument is insufficient to capture all of the induced behavioral changes that constitute the program effect of land titling on birth outcomes. Instead, the first stage of the multiple IV model requires estimating five equations of the following form, where the Zs represent the instruments used to identify the first-stage equations:

$$S_i = \alpha_0 + Z_i\gamma + X_i\beta + \upsilon_i$$
$$\theta_i = \alpha_0 + Z_i^{'}\gamma + X_i\beta + \nu_i$$
$$P_i = \alpha_0 + Z_i^{''}\gamma + X_i\beta + \eta_i$$
$$\pi_i = \alpha_0 + Z_i^{'''}\gamma + X_i\beta + \varsigma_i$$
$$H_i = \alpha_0 + Z_i^{''''}\gamma + X_i\beta + \mu_i$$

For the model to be identified, there must be at least five instruments. To create multiple instrumental variables I exploit variation in the responses of particular subpopulations to the program by using interactions between the program and particular baseline characteristics of households. Aside from a dummy indicator of program participation, the variables in Z include program participation interacted with the total number of adults in the household, residential tenure, age difference between household head and spouse, the year in which the neighborhood was reached by the program, and whether or not the property was acquired by invasion.[26] In these estimates, variation in the program effect according to household size predicts changes in household labor force

[26] Here, the baseline characteristics serve as covariates in both equations and the interactions between the program variable and the baseline characteristics serve as instruments. “Invasion” refers to the act of taking over residences that have already been inhabited as opposed to land that has never been settled for residential purposes.

hours; variation according to the age difference between spouses predicts the change in female decision-making power; variation according to whether property was acquired by invasion predicts the location of entrepreneurial activities inside versus outside of the home; variation in residential tenure predicts the change in tenure security; and the residual effect of the program on beneficiaries picks up the wealth effects from increased property values and credit access. Justification and evidence for these relationships is found in Field (2005), who shows that there is significant variation in the magnitude of the program effect on labor supply according to the number of working-age household members, and in the program response to market work at home according to whether property was acquired by invasion. Similarly, residential tenure is a strong predictor of the change in tenure security that results from receiving a title since households with strong community ties have high levels of de facto tenure security, so experience less of a change in eviction likelihood.

Although in general obtaining rich enough variation in program effects can be difficult with several endogenous variables and only one program, the set of instruments chosen in this analysis includes well-defined pathways separately affecting each endogenous variable. For instance, it is reasonable to assume that the change in bargaining power achieved by the program operated only through the redistribution of titles to women and was arguably unaffected by the change in tenure security, property value, or work behavior, controlling for the interaction between price changes and security changes and gender of the owner. Similarly, while changes in property values and tenure security are highly correlated, the indicators of the household experience allow me to identify households that experienced one but not the other. In the intent-to-treat model, variation according to residential tenure allows me to reliably distinguish changes in tenure security from changes in home value as long as the tenure security value of community membership in not transferable to new owners (Lanjouw and Levy, 2001). In this case, long residential tenure implies a small change in tenure security but is unrelated to the change in property value. Finally, the variation in program effect according to number of adults appears to be unique to household labor supply, as in the concentration among invading households unique to the change in home production.

For the IV estimates to be valid, the following assumptions must hold. First, the order in which different household types receive titles must be independent of other factors affecting fertility. Second interacting the program indicator with demographic characteristics requires that variation in program effects across subpopulations be exogenous to fertility decisions, or that the effect of the endogenous variables on fertility be the same across different levels of each instrument. In the first-stage regressions, all baseline variables are insignificant. Third, identification depends on satisfying the exclusion restriction, which states that the only channel through which the instruments affect the outcome is through the pathways explicitly included as endogenous variables in the model. I test the validity of the instruments by conducting a test of overidentifying restrictions to determine whether there is association between the instruments and the error term in the second-stage equation.[27] The corresponding Sargan test statistic provides some indication that the instruments are valid.

Finally, in the case of multiple endogenous variables, there must be sufficient variation in the effects of the individual instruments on the set of endogenous variables. To examine this, I use the diagnostic measure suggested by Shea (1997) to gauge the relevance of instrumental variables in models with multiple endogenous variables, in which the standard R-squared is compared with Shea's partial R-squared in each of the first-stage regressions.[28] Results from this test indicate that the set of instruments reliably predicts the outcome of interest and also reliably distinguishes the prediction of one outcome from the prediction of another outcome. Furthermore, visual inspection of the coefficient estimates on the instruments across different endogenous variables from the first-stage regression results reveals substantial variation.

Results from the probit IV estimates using the single and full set of instruments are reported in Table 11, where columns (1) and (2) correspond to births in 1999, and columns (3) and (4) correspond to births in 1998. Both IV models show comparable results on the role of bargaining power across both years. In all four probits, the coefficient estimates on female decision-making are large and significant, whereas the other endogenous variables are consistently small and insignificant. This suggests that changes in female agency are indeed related to the observed change in birth rates immediately following program intervention. Only in column 4, which corresponds to births in 1998, do we see evidence that changes in the productive value of children may exert separate negative influences on household fertility decisions, consistent with the probit estimates in Tables 8 and 10 and the corresponding information inferred about ownership rights, decision-making equality, and tenure security.

Meanwhile, the income effect of changes in property values is positive and insignificant, as are the effects of changes in household work hours and home production arising from the change in home security provision.[29] The absence of an income effect is not surprising, as the implied behavioral change is ambiguous and empirical results from other settings are mixed. While there may be an implicit positive income effect on fertility, households may also be substituting consumption away from children towards

---

[27] Sargan's test statistic is given by: $s = \Delta\hat{u}'\left[\sum_{i-1}^{N} W_i'(\Delta\hat{u}_i)(\Delta\hat{u}_i)W_i\right]^{-1} W'(\Delta\hat{u}) \overset{asy}{\sim} \chi^2_{p-K-1}$

[28] Shea's partial R^2 can be computed as follows (simplification provided in Godfrey (1999)):

$$R_p^2 = \frac{\mathrm{var}(b_{1(LS)}) * \sum_{i=1}^{n}(y_i - x_i' b_{IV})^2}{\mathrm{var}(b_{1(IV)}) * \sum_{i=1}^{n}(y_i - x_i' b_{LS})^2}$$

[29] Including the amount of formal credit in the set of endogenous variables in place of the property value indicator has no effect on the magnitude or significance of any potential pathway. The estimates are also robust to replacing the index measures of household-decision making and tenure security (for instance, replacing the endogenous variables with binary indicators of whether property is considered "very secure" and whether the household head is reported to make all decisions).

consumption in housing infrastructure, accounting for the absence of an income effect. The insignificance of changes in adult labor force hours also makes sense given that previous estimates have found that male and not female labor force participation accounts for the bulk of the increase in work hours among untitled households (Field (2005)). As with labor force hours, the opportunity cost of raising children is not likely to change much with the relocation of market work from inside to outside the home if it is primarily adult men who are no longer working at home. Furthermore, the change in the productive value of children related to home business activity is likely to be small since there is no corresponding change in the rate of self-employment, and therefore neither in the value of children as unpaid labor in family business activities.

5. CONCLUSIONS

This paper presented new evidence on the relationship between property rights and fertility in developing countries. There are two major findings. First, the responsiveness of fertility to titling programs in urban settings is estimated to be large. My estimates indicate up to a 22 per cent reduction in fertility for squatter families who received a property title through the government program in Peru. Furthermore, IV estimates indicate that the change in desired number of children results from two distinct sources. First, there appears to be a significant reduction in fertility associated with the redistribution of household property assets to women involved in government titling programs such as COFOPRI that make an explicit effort to include both male and female names on property documents. This evidence supports the hypothesis that female bargaining power, particularly as it derives from the ownership of land assets, matters for family fertility decisions in developing countries. To this extent, the results also provide evidence against the pooled income hypothesis for fertility decisions: households in which property titles are distributed to both male and female heads of the household experience nearly twice the reduction in the probability of having a child. In addition, changes in tenure security may exert an independent negative influence on desired number of offspring, controlling for changes in property values and reductions in credit constraints that accompany titling. If increasing household tenure security brings about a reduction in the productive value of children, either parents are less dependent on offspring for old-age subsistence, or children serve less of a role in securing informal ownership rights or claims to community resources. Both findings suggest that recent efforts to emphasize gender equality of ownership in the implementation of land titling programs could have unexpected influence on demographic transition.

1 References

[1] Agarwal, Bina (1994). Gender and command over property: A critical gap in economic analysis and policy in South Asia." *World Development* 22(10):1455-1478.

[2] _________ (1997). Bargaining and gender relations: Within and beyond the household. *Feminist Economics* 3 (1).

[3] Alderman, Harold, Pierre-Andre Chiappori, Lawrence Haddad, John Hoddinott and Ravi Kanbur (1995). Unitary versus collective models of the household: Is it time to shift the burden of proof?" *The World Bank Research Observer* February, 1995:1-21.

[4] Becker, Gary S. (1960). An economic analysis of fertility. In Demographic and Economic Change in Developed Countries (Universities-National Bureau Committee for Economic Research, ed.), Princeton: Princeton University Press.

[5] Becker, Gary S. and H. G. Lewis (1973). On the interaction between the quantity and quality of children. *Journal of Political Economy* 81:279-88.

[6] Becker, Gary S. (1974). A theory of social interactions. *Journal of Political Economy* 82: 1063-1091.

[7] Becker, Gary S. (1965). A theory of the allocation of time. *Economic Journal*, September: 493–517.

[8] Cain, Glen and Martin D. Dooley (1976). Estimation of a model of labor supply, fertility, and wages of married women. *The Journal of Political Economy* August, 1976: 179-199.

[9] Chiappori, Pierre-Andre (1997). Introducing household production in collective models of labor supply. *Journal of Political Economy* 105: 191-209

[10] Chiappori, Pierre-Andre (1992). Traditional versus collective models of household behavior: What can data tell us? In Understanding how resources are allocated within households. *IFPRI Policy Briefs* 8. Washington, D.C.: International Food Policy Research Institute, World Bank: 39-40.

[11] Chiappori, Pierre-Andre (1988). Rational household labor supply. *Econometrica* 56:63-89.

[12] Davies, J.B. and J. Zhang (1995). Gender bias and investments in children and bequests. *International Economic Review*, 36: 795-818.

[13] de Janvry, Alain, Marcel Fafchamps, Mohamed Raki, and Elisabeth Sadoulet (1991). Peasant household behavior with missing markets: Some paradoxes explained. *Economic Journal* 101: 1400-1417.

[14] Doss, Cheryl R (1996a). Testing among models of intra-household resource allocation. *World Development* 24(10): 1597-1609.

[15] Doss, Cheryl R. (1996b). Women's bargaining power in household economic decisions: Evidence from Ghana. Staff Paper P96-1. Department of Applied Economics, University of Minnesota.

[16] Eckstein, Zvi and Kenneth Wolpin (1989). Dynamic labor force participation of married women and endogenous work experience. *Review of Economic Studies* 56 (July): 375-390.

[17] Fabella, Raul Villasenor. *Economics of Scale in the Household Production Model and Intra-family Allocation of Resources.* Doctoral dissertation submitted to the Department of Economics, Yale University, 1982.
[18] Field, Erica and Maximo Torero (2003). Do property titles increase credit access among the urban poor? Evidence from Peru. Mimeo, Princeton University, September 2002.
[19] Field, Erica (2002). Entitled to work: Urban property rights and labor supply in Peru. Princeton University Research Program in Development Studies Working Paper #220.
[20] Folbre, Nancy (1986). Hearts and spades: Paradigms of household economics. *World Development* 14 (2): 245-255.
[21] Godfrey, L. G. (1999). Instrument relevance in multivariate linear models. *Review of Economics & Statistics* 81(3): 550-552.
[22] Haddad, Lawrence, John Hoddinot and Harold Alderman (1994). Intra-household resource allocation. Policy Research Working Paper 1255. Washington: The World Bank.
[23] Horney, Mary Jean and Marjorie McElroy (1988). The household allocation problem: Empirical results from a bargaining model. *Research in Population Economics* 6: 15-38.
[24] Hotz, V Joseph & Miller, Robert A. (1988). An empirical analysis of life cycle fertility and female labor supply. *Econometrica* 56(1): 91-118.
[25] Lanjouw, Jean and Phillip Levy (2001). Untitled:
[26] Lastarria-Cornhiel S. (1996) Impact of Privatization on Gender and Property Rights in Africa. (Paper presented at the International Conference of Land Tenure and Administration, Orlando, Florida, November 12-14, 1996)
[27] Leuthold, Jane H. (1968). An empirical study of formula income transfers and the work decision of the poor. *The Journal of Human Resources* 3(3): 312-323
[28] Lundberg, Shelly and Robert A. Pollak (1993). Separate spheres bargaining and the marriage market. *Journal of Political Economy* 101(6): 988-1010.
[29] Lundberg, Shelly and Robert A. Pollak (1966). Bargaining and distribution in marriage. *Journal of Economic Perspectives* 10: 139-158.
[30] Lundberg, Shelly (1988). Labor supply of husbands and wives: A simultaneous equations approach. *Review of Economics and Statistics* 70 (2): 224-235.
[31] Manser, Marilyn and Murray Brown (1980). Marriage and household decision-making: A bargaining analysis. *International Economic Review* 21: 31-44.
[32] Manser, Marilyn and Murray Brown (1979). Bargaining analyses of household decisions. In *Women in the Labor Market.* Edited by Cynthia B. Lloyd, Emily S. Andrews and Curtis L. Gilroy. New York: Columbia University Press: 3-26.
[33] Mason K. O. and A. M. Taj (1987). Differences between women's and men's reproductive goals in developing countries. *Population and Development Review* 13:6-38.
[34] McElroy, Marjorie B. (1990). The empirical content of Nash-bargained household behavior. *The Journal of Human Resources* 25: 559-583.

[35] McElroy, Marjorie B. and Mary Jean Horney (1981). Nash-bargained household decisions: Toward a generalization of the theory of demand. *International Economic Review* 22(2):333-350.
[36] Milligan, Kevin (2001). Subsidizing the stork: New evidence on tax incentives and fertility. University of British Columbia, Department of Economics Discussion Paper: 01/21.
[37] Moe, Karine S. (1998). Fertility, time use, and economic development. *Review of Economic Dynamics* 1(3): 699-718.
[38] Moffitt, Robert (1974). Life cycle profiles of fertility, labor supply, and wages of married women. *Review of Economic Studies* 60 (1984): 263-278.
[39] Nussbaum, Martha C. (1995). Introduction. In *Women, Culture and Development: A Study of Human Capabilitie*s. Edited by Martha C. Nussbaum and Jonathan Glover. Oxford: Clarendon Press: 1-34.
[40] Pritchett, Lance H. (1994) Desired fertility and the impact of population policies. *Population and Development Review* 20:1-55.
[41] Rao, V.J. and Greene, M. (1993). Marital instability, intra-household bargaining, and their implication for fertility in Brazil. Population Research Center, University of Chicago. Mimeo.
[42] Rasul, Imran (2001). Household bargaining over fertility: Theory and evidence from Malaysia. London School of Economics. Mimeo.
[43] Rosenzweig, Mark and Robert Evenson (1977). Fertility, schooling, and the economic contribution of children in rural India: An econometric analysis. *Econometrica* 45(5):1065-79
[44] Rosenzweig, Mark (1980). Neo-classical theory and the optimizing peasant: An econometric analysis of market family labor supply in a developing country. *Quarterly Journal of Economic*s, February: 31-55.
[45] Schultz, T, Paul (1990). Testing the neo-classical model of family labor supply and fertility. *The Journal of Human Resources* 25: 600-634.
[46] Schultz, T. Paul (1980). An economic interpretation of the decline in fertility in a rapidly developing country. In Population and Economic Change in Developing Countries, (ed.) R.A. Easterlin, Chicago: University of Chicago Press.
[47] Sen, Amartya (2001). Development As Freedom, Alfred A. Knopf, Inc., New York.
[48] Shea, John (1997). Instrument relevance in multivariate linear models: A simple measure. *Review of Economics and Statistics*. 79:348-352.
[49] Thomas, Duncan (1990). Intra-household resource allocation: An inferential approach. *The Journal of Human Resources* 25: 635-663.
[50] Udry, Christopher (1996). Gender, agricultural production, and the theory of the household. *Journal of Political Economy* 104 (5): 1010-1046.
[51] Willis, Robert (1973). A new approach to the economic theory of fertility behavior. *Journal of Political Economy*, March-April: S14--S64.

Table 1: Summary Statistics, women ages 15-50

	Titled			*Untitled*		
	No Program	Program	$\lvert t_D \rvert$	No Program	Program	$\lvert t_D \rvert$
N:	*2375*	*944*		*610*	*428*	
Characteristics of Household						
Number of members	6.59	6.29	1.66	6.10	5.96	0.63
Sex of household head	0.22	0.24	0.68	0.24	0.25	0.30
Monthly wage income	1429.6	1188.3	1.89	1088.9	1059.4	0.19
Total monthly consumption	612.0	653.2	1.29	583.7	591.6	0.28
Whether HH savings	0.09	0.07	0.84	0.05	0.05	0.09
HH extreme poor	0.31	0.28	1.13	0.31	0.30	0.16
Estimated selling price of residence	16139	16108	0.24	16328	16083	0.79
Years of residence	17.1	19.6	2.62	15.9	16.2	0.17
Whether home business	0.25	0.26	0.50	0.23	0.19	0.94
Family planning clinic in neighborhood?	0.80	0.76	0.63	0.80	0.75	0.60
Characteristics of Individual						
HH head	0.06	0.07	1.65	0.08	0.09	0.42
Spouse of HH head	0.30	0.33	1.22	0.35	0.34	0.32
Child of HH head	0.58	0.54	1.85	0.54	0.49	1.32
Other family member	0.06	0.06	0.18	0.03	0.08	2.66
Age	30.0	30.5	1.56	30.3	29.4	2.10
Married	0.26	0.28	0.88	0.23	0.21	0.43
Partnered	0.18	0.18	0.27	0.22	0.24	0.70
Partner present in household*	0.74	0.76	1.13	0.76	0.74	0.80
Labor force participant	0.39	0.45	2.56	0.42	0.42	0.07
Literate	0.97	0.98	1.80	0.97	0.97	0.11
Highest grade primary school	0.20	0.17	1.33	0.23	0.22	0.43
Highest grade high school	0.55	0.60	2.24	0.56	0.62	1.66
Highest grade college	0.12	0.12	0.24	0.11	0.08	1.36
Born in province	0.63	0.63	0.28	0.67	0.62	1.19
Whether migrated for marriage	0.07	0.03	3.40	0.05	0.03	1.26
Participate in family planning clinic?	0.25	0.26	0.44	0.29	0.36	0.60
Offspring						
Child age 0	0.037	0.042	0.61	0.038	0.026	1.08
Child age 1	0.045	0.043	0.20	0.054	0.040	1.11
Child age 2	0.055	0.063	0.70	0.057	0.058	0.07
Child age 3	0.052	0.061	1.13	0.050	0.056	1.06
Child age 4	0.064	0.066	0.19	0.051	0.075	1.45
Child age 5	0.062	0.066	0.34	0.069	0.077	0.47
Child age 6	0.070	0.088	1.57	0.082	0.089	0.36
Total number of children	1.55	1.57	0.34	1.52	1.44	0.96
No children	0.45	0.43	1.13	0.45	0.44	0.56

*Means include married and partnered women only.

Table 2. Age-specific birth rates by program participation and tenure status

1999	titled, no program	titled, program	untitled, no program	untitled, program
Age group				
16-20	0.0334	0.0368	0.0248	0.0297
21-25	0.0617	0.0443	0.0550	0.0615
26-30	0.0720	0.0959	0.0889	0.0267
31-35	0.0429	0.0656	0.0435	0.0182
36-40	0.0310	0.0254	0.0189	0.0000
41-45	0.0021	0.0000	0.0089	0.0147

1994	titled, no program	titled, program	untitled, no program	untitled, program
Age group *(in 1994)*				
16-20	0.0321	0.0190	0.0642	0.0932
21-25	0.1286	0.1575	0.1444	0.1333
26-30	0.1557	0.1639	0.1304	0.1818
31-35	0.0942	0.0678	0.0769	0.0667
36-40	0.0733	0.0561	0.0333	0.0606
41-45	0.0160	0.0141	0.0233	0.0182

Table 3. Parity-specific birth rates by program participation and tenure status

		Titled		Untitled	
		no program	program	no program	program
	parity				
1999	0	0.033	0.020	0.032	0.011
	1	0.083	0.119	0.062	0.078
	2	0.037	0.072	0.036	0.060
	3	0.024	0.090	0.073	0.000
	parity				
1994	0	0.041	0.054	0.063	0.055
	1	0.119	0.136	0.103	0.148
	2	0.102	0.093	0.174	0.163
	3	0.079	0.048	0.043	0.094

Table 4: Probit estimate of whether woman gave birth within year

	(1)	(2)	(3)	(4)	(5)	(6)
Year	**1999**	**1998**	**1997**	**1995**	**1994**	**1993**
(Age of child)	*0-1*	*1-2*	*2-3*	*4-5*	*5-6*	*6-7*
Titling program 1995-1998 †	-0.009			0.003		
	(2.21)*			(0.80)		
Titling program 1994-1997††		-0.007			0.003	
		(4.69)**			(0.24)	
Titling program 1993-1996†††			-0.007			0.003
			(0.40)			(0.34)
N:	*953*	*960*	*906*	*884*	*849*	*817*

†Households titled prior to 1995 excluded. ††Households titled prior to 1994 excluded. †††Households titled prior to 1993 excluded.

Notes: Marginal effects from a probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is the dummy indicator of whether a woman gave birth in the indicated year. In each estimate, sample includes all women who were age 15-50 in the given year of birth and living in households without property titles before the program. All estimates control for city dummies along with the following demographic characteristics: 5-year age group in birth year, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, and whether family planning clinic in neighborhood.

Table 5: Probit estimate of whether woman gave birth within year

	(1)	(2)	(3)	(4)	(5)	(6)
Year	**1999**	**1999**	**1994**	**1998**	**1998**	**1994**
(Age of child)	*0-1*	*0-1*	*5-6*	*1-2*	*1-2*	*5-6*
squatter	0.005	0.001	-0.000	-0.002	-0.001	-0.002
	(0.81)	(0.08)	(0.01)	(0.30)	(0.16)	(0.26)
titling program 1995-1999	0.000	0.006	0.003			
	(0.00)	(1.01)	(0.36)			
(titling program 1995-1999)*squatter	-0.012	0.004	0.001			
	(2.20)**	(0.33)	(0.05)			
received title from program 1995-1999		-0.019				
		(2.26)*				
titling program 1995-1998				0.003	0.004	-0.001
				(0.76)	(0.80)	(0.12)
(titling program 1995-1998)*squatter				-0.007	0.012	0.005
				(2.74)*	(3.08)**	(0.63)
received title from program 1995-1998					-0.017	
					(6.42)**	
N:	*4384*	*4384*	*4384*	*4120*	*4121*	*4122*

Notes: Marginal effects from a probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is the dummy indicator of whether a woman gave birth in the indicated year. In each estimate, sample includes all women who were age 15-50 in the given year of birth. Households titled prior to 1995 excluded. All estimates control for city dummies along with the following demographic characteristics: 5-year age group in birth year, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, and whether family planning clinic in neighborhood.

Table 6: Probit estimate: whether woman gave birth between ages 25 and 30, program neighborhoods only

	(1)	(2)
Birth year	*1999/1994*	*1998/1993*
Young cohort	-0.003	0.004
	(0.41)	(0.10)
Untitled (program beneficiary)	0.000	-0.020
	(0.00)	(0.67)
Untitled*young cohort	-0.052	-0.047
	(2.79)*	(2.23)*
N:	*400*	*215*

Notes: Marginal effects from a probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is the dummy indicator of whether a woman gave birth between ages of 25 and 30. In column 1, sample includes all women who were age 25-30 in the given year of birth and living in neighborhoods which the titling program reached before 1999. Column 2 includes all women who were age 25-30 in the given year of birth and living in neighborhoods which the titling program reached before 1998. All estimates control for city dummies along with the following demographic characteristics: age in birth year, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, and whether family planning clinic in neighborhood.

Table 7: Whether female name on property document, difference-in-difference estimates

	No program *(N=1215)*	Program *(N=638)*	Difference	Difference-in-difference
Pre-program titled *(N=1402)*	0.417	0.646	0.229	
	(0.022)	(0.039)	(0.045)	
Pre-program squatter *(N=451)*	0.000	0.773	0.773	0.544
	(0.000)	(0.047)	(0.047)	(0.054)

* Standard errors in parentheses.

Table 8: OLS estimates, effect of ownership rights on whether had a child in 1999

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Program*squatter	0.013	0.009	0.020	-0.002	0.000	-0.042	-0.040
	(0.84)	(0.59)	(1.01)	(0.13)	(0.02)	(8.26)**	(6.64)**
Titled*squatter	-0.017	-0.019	-0.016	-0.019	-0.013	0.647	0.363
	(1.79)	(1.75)	(1.13)	(1.47)	(0.87)	(3.86)**	(2.38)*
Female name on property title* program*squatter	-0.014		-0.013		-0.019		-0.018
	(2.29)*		(1.32)		(4.08)**		(2.17)*
Estimated selling price of residence* program*squatter		-0.000	-0.000				
		(1.69)	(1.75)				
Estimated selling price * female name on property title			-0.000				
			(2.23)*				
Amount of formal credit *program* squatter				-0.052	-0.051		
				(3.87)**	(3.87)**		
Amount of formal credit * female name on property title					-0.043		
					(2.78)**		
Tenure security level*program* squatter						-0.062	-0.060
						(11.30)**	(9.32)**
Tenure security level * female name on property title							0.037
							(2.97)**

** p<0.01; * p<0.05; ^ p<0.10 two tailed

Notes: Marginal effects from a probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is whether woman gave birth in past year. Sample includes all women age 15-50. All estimates control for city dummies, all relevant intermediate interactions, and the following demographic characteristics: 5-year age group, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, education level of head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, family planning clinic in neighborhood.

Table 9a: Whether household head makes family decisions, difference-in-difference estimates

	No program *(N=1178)*	Program *(N=606)*	Difference	Difference-in-difference
Pre-program titled *(N=1353)*	0.187 (0.016)	0.158 (0.020)	-0.029 (0.031)	
Pre-program squatter *(N=431)*	0.226 (0.034)	0.131 (0.032)	-0.095 (0.045)	-0.066 (0.049)

* Standard errors in parentheses.

Table 9b: Whether household head makes family decisions, titled households only

	Male name only on title *(N=1178)*	Female name on title *(N=606)*	Difference	Difference-in-difference
Pre-program titled *(N=1353)*	0.158	0.200	0.042	
Pre-program squatter *(N=431)*	0.211	0.096	-0.115	-0.157 (0.100)

* Standard errors in parentheses.

Table 10: OLS estimates, effect of ownership rights on whether household head makes decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Program*squatter	-0.003	0.094	0.082	0.023	0.016	0.268	0.250
	(0.04)	(1.89)	(1.27)	(0.36)	(0.22)	(0.62)	(0.53)
Titled*squatter		-0.164	-0.071	-0.124	0.035	-0.066	0.919
		(6.89)**	(0.66)	(2.21)*	(0.43)	(0.34)	(.)
Female name on property title* program*squatter	-0.139		-0.159		-0.169		-0.283
	(3.77)**		(1.79)		(4.80)**		(6.02)**
Estimated selling price of residence* program*squatter		-0.000	-0.000				
		(2.43)*	(2.42)*				
Estimated selling price * female name on property title			0.000				
			(0.70)				
Amount of formal credit *program* squatter				-0.000	-0.000		
				(0.69)	(0.69)		
Amount of formal credit * female name on property title					-0.000		
					(1.10)		
Tenure security level*program* squatter						0.098	0.093
						(0.70)	(0.67)
Tenure security level * female name on property title							-0.142
							(6.12)**

** p<0.01; * p<0.05; ^ p<0.10 two tailed

Notes: Marginal effects from a probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is whether woman gave birth in past year. Sample includes all women age 15-50. All estimates control for city dummies, all relevant intermediate interactions, and the following demographic characteristics: 5-year age group, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, education level of head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, family planning clinic in neighborhood, age difference between head and partner, and whether property was inherited from head's family.

Table 11: Probit IV estimates, Whether birth in 1999 and 1998

	1999		1998	
	(1)	(2)	(3)	(4)
Decision-making power of head	-0.061 (2.32)*	-0.064 (2.10)*	-0.051 (1.63)	-0.046 (2.74)**
Tenure security level		0.051 (1.66)		0.062 (2.56)*
Total weekly labor force hours		0.000 (0.27)		0.000 (0.39)
Whether residence used as a source of economic activity		0.074 (0.68)		-0.014 (0.15)
Change in selling price of residence		0.025 (0.69)		0.003 (0.09)

** p<0.01; * p<0.05; ^ p<0.10 two tailed

Notes: Marginal effects from an instrumental variables probit estimate are reported, t-statistics are in parentheses. Robust standard errors account for sample clusters and strata. Dependent variable is whether a woman gave birth during a one-year interval. Standard errors are in parentheses.

Sample includes all women age 15-50 living in households in which household head has a legal or common-law spouse residing in the household. All estimates control for city dummies along with the following demographic characteristics: 5-year age group, birth parity, marital status, whether partner present in household, education level, literacy, whether household head, spouse of head, daughter of head or other family member, sex of household head, education of head, whether born in province, whether migrated to marry, property size, residential tenure, highest monthly wage of non-fertile-aged female household member, and whether family planning clinic in neighborhood.

Appendix A: Procedure to match mothers to children

For each household member I observe only age and relationship to the household head. Hence, for children born to the household head or spouse, it is straightforward to match mother and child, and the only source of potential measurement error is if children are stepchildren of the female spouse. However, in extended family settings, it is arguably equally relevant to model parity as a function of the total number of existing children in the households.

When household members are recorded as grandchildren of the household head, the mother can be identified only if there is only one fertile-aged daughter or in-law residing in the household. However, in the majority of multiple-family data records, household members are listed in family clusters ordered by personal identification numbers. For roughly 80% of households with extended families, household members are grouped according to sub-families residing in the household, such that individual family sizes can be directly observed by looking at the ordering of personal identification numbers. For the other 20%, household members are listed according to relationship to household head and ordered by age. When there is more than one female candidate, grandchildren are matched to the oldest female child who is married or partnered and whose personal identification number is above the child's. If more than one candidate exists, the oldest daughter under age 40 at birth is chosen. If no coupled daughter exists, the child is matched to the oldest uncoupled daughter. Children of other family members were assigned to the oldest partnered female other family member.

Once individual children are matched to mothers, the associated total number of children corresponding to each woman, and hence information on birth parity, is readily available.

Appendix B. Annual Births per Household, Year by Year*

[Reference group is women age 25-30] (N=2435)	OLS (+city dums and interactions***) Babies 0-1 Year	OLS (+city dums and interactions***) Babies 1-2 Year	OLS (+city dums and interactions***) Babies 2-3 Year	OLS (+city dums and interactions***) Babies 3-4 Year	OLS (+city dums and interactions***) Babies 4-5 Year
Program area	0.020	-0.078	0.005	-0.053	-0.021
	(0.014)	(0.037)	(0.063)	(0.060)	(0.041)
Pre-program squatter	0.008	0.093	-0.037	0.201	-0.126
	(0.015)	(0.086)	(0.080)	(0.116)	(0.040)
Program*squatter *(program effect)*	**-0.052**	**-0.071**	**-0.064**	**0.033**	**-0.017**
	(0.023)	(0.029)	(0.034)	(0.034)	(0.032)
Number marrried women 15-55	0.082				
	(0.021)				
Number partnered (non-married) women 15-55	0.104				
	(0.021)				
Number women partner/spouse away 15-55	0.063				
	(0.020)				
Number single women 15-55	0.029				
	(0.019)				
Number women age 15-20	-0.013				
	(0.020)				
Number women age 20-25	-0.005				
	(0.018)				
Number women age 30-35	-0.015				
	(0.018)				
Number women age 35-40	-0.045				
	(0.017)				
Number women age 40-45	-0.067				
	(0.019)				
Number women age 45-55	-0.097				
	(0.021)				
Number finished elementary school	-0.007				
	(0.012)				
Number women finished high school	-0.010				
	(0.014)				
Number women attended college	0.008				
	(0.020)				
Number babies under 5 (excluding dependent variable)	-0.074				
	(0.013)				
Number kids age 5-9	-0.010				
	(0.008)				

* The dependent variable is total number of HH members age 1. Standard errors are in parentheses. Robust standard errors account for sample clustering and stratification. Ineligible (residential tenure post-1995) and recently titled HHs (past 2 years) are excluded.

** Demographic controls include: sex, age and grade level of HH head, presence of grandparents, number of children age 10-14, 3-level poverty index; lotsize, # of rooms, # floors, #bedrooms, electricity, indoor plumbing, indoor bathroom, straw roof, dirt floor, and age of dwelling; and whether neighborhood has municipal services, adequate infrastructure, water supply, public security, public services, private guards, and four road quality dummies.

*** Interactions include squat*region and enter*region.

Appendix C. Annual Births per Household, June 1996-June 1997 (Program year>1996 only)*

(N=2472) *[Reference group is women age 25-30]*	OLS	OLS (+*demographic***)	OLS (+*city dums and interactions****)	IV : level of tenure security (*squatters only*)	IV: change in tenure security (*squatters only)*	IV: whether titled (*squatters only*)
Program area	0.013	0.007	-0.068			
	(0.013)	(0.014)	(0.054)			
Pre-program squatter	-0.040	-0.043	0.190			
	(0.014)	(0.015)	(0.11)			
Program*squatter *(program effect)*	0.020	0.029	0.026	0.089	0.065	0.063
	(0.025)	(0.025)	(0.033)	(0.046)	(0.033)	(0.031)
Number marrried women 15-55	0.096					
	(0.023)					
Number partnered (non-married) women 15-55	0.117					
	(0.025)					
Number women partner/spouse away 15-55	0.085					
	(0.024)					
Number single women 15-55	0.028					
	(0.016)					
Number women age 15-20	-0.008					
	(0.016)					
Number women age 20-25	-0.004					
	(0.018)					
Number women age 30-35	-0.011					
	(0.020)					
Number women age 35-40	-0.028					
	(0.019)					
Number women age 40-45	-0.084					
	(0.020)					
Number women age 45-55	-0.088					
	(0.022)					
Number finished elementary school	-0.017					
	(0.012)					
Number women finished high school	-0.009					
	(0.016)					
Number women attended college	-0.031					
	(0.015)					
Number babies age 4	-0.030					
	(0.019)					
Number kids age 5-9	0.006					
	(0.008)					

* The dependent variable is total number of HH members age 3. Standard errors are in parentheses. Robust standard errors account for sample clustering and stratification. Ineligible (residential tenure post-1995) HHs are excluded.

** Demographic controls include: sex, age and grade level of HH head, presence of grandparents, number of children age 10-14, 3-level poverty index; lotsize, # of rooms, # floors, #bedrooms, electricity, indoor plumbing, indoor bathroom, straw roof, dirt floor, first owner, bought dwelling, and age of dwelling; and whether neighborhood has municipal services, adequate infrastructure, water supply, public security, public services, private guards, and four road quality dummies.

*** Interactions include squat*region and enter*region.

Appendix D. Age structure of sub-populations

	Titled		*Untitled*	
	No Program	Program	No Program	Program
Age group				
16-20	23.52	20.47	20.44	24.69
21-25	17.67	17.03	18.41	15.89
26-30	15.49	15.73	15.20	18.34
31-35	10.65	13.15	11.66	13.45
36-40	12.22	12.72	15.37	11.00
41-45	10.12	11.53	10.14	8.07
46-50	10.34	9.38	8.78	8.56