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Property Rights and Gender Bias: Evidence from Land Reform in West Bengal†

By Sonia Bhalotra, Abhishek Chakravarty, Dilip Mookherjee, and Francisco J. Pino*

We examine intra-household gender-differentiated effects of property rights securitisation following West Bengal's tenancy registration program, using two independently gathered datasets. In both samples, higher program implementation increased male child survival rates in families without a firstborn son, but not in those that already have a firstborn male child. We argue this reflects intensified son preference as land rights improve, ostensibly to ensure a male heir to inherit land. Consistent with this, girls with firstborn brothers also experience increased survival, but not girls with firstborn sisters. The gender bias manifests both in infant mortality rates and the sex ratio at birth. (*JEL* D13, I12, J16, O15, O17, P14, Q15)

Secure property rights are considered a cornerstone of economic development. Land rights are particularly important in developing countries where large fractions of the population are dependent upon agriculture. During 1955–2000 a billion people and nearly as many hectares were affected by land reform (Lipton 2009). Previous research demonstrates the importance of land security in increasing agricultural productivity, facilitating access to credit, reducing poverty, and cross-household asset inequality (Besley and Burgess 2000, Besley 1995, Besley and Ghatak 2010, Besley et al. 2016, Goldstein and Udry 2008, Hornbeck 2010, Bardhan and Mookherjee 2011, Bardhan et al. 2014). Effects of land reform on intra-household gender inequality, such as the problem of "missing women" in China and India, however, have not been examined, though other dimensions of this problem have been studied by various authors (Almond, Edlund, and Milligan

*Bhalotra: Department of Economics, University of Essex, Wivenhoe Park, Colchester CO4 3SQ, United Kingdom (email: srbhal@essex.ac.uk); Chakravarty: Department of Economics, The University of Manchester, Oxford Road, Manchester M13 9PL, United Kingdom (email: abhishek.chakravarty@manchester.ac.uk); Mookherjee: Department of Economics, Boston University, 270 Bay State Road, Boston MA 02215 (email: dilipm@bu.edu); Pino: Department of Economics, School of Economics and Business, University of Chile, Diagonal Paraguay 257, Santiago, Región Metropolitana, Chile (email: fjpino@fen.uchile.cl). Esther Duflo was coeditor for this article. We are grateful to S. Anukriti, Maitreesh Ghatak, Giacomo de Giorgi, Tarun Jain, Stephan Litschig, Pushkar Maitra, Giovanni Mastrobuoni, Patrick Nolen, Imran Rasul, Debraj Ray, Sanchari Roy, Alessandro Tarozzi, and participants at several conferences and seminars for their comments. Funding from the MacArthur Foundation Inequality Network, NSF grant 0418434, Fondecyt Iniciacion grant 11150304, and the Centre for Social Conflict and Cohesion Studies (CONICYT/FONDAP/15130009) is greatly acknowledged. All errors remain our own.
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2013; Anderson and Ray 2010; Sen 2003; Bhalotra 2010; Bhalotra and Cochrane 2010; Chakravarty 2010; Anukriti and Chakravarty forthcoming; Rose 1999).

In this paper, we explore the hypothesis that land reform may exacerbate an underlying preference for sons and thereby increase gender inequality, in societies where land rights are heritable and primarily inherited by sons (Abrevaya 2009, Bhalotra and Cochrane 2010). Gender differentiated preferences among parents could conceivably result from a combination of motives: wealth effects that raise survival chances differentially between boys and girls, and inheritance patterns that differ between male and female children. There is some evidence of gender differentiated wealth effects in the literature, which tends to show a bias in favor of females (e.g. see Rose 1999, Maccini and Yang 2009), but little evidence of an inheritance effect which might favor boys. A common pattern in patrilineal societies is that daughters take their bequest at marriage as dowry and marry some distance from their natal home (Guner 1999, Rosenzweig and Wolpin 1985), while sons tend to co-reside with parents, work on the land, and subsequently inherit it. Botticini and Siow (2003) postulate that a rationale for the origin and persistence of these arrangements is that they incentivize sons to work on the father's land, contributing to wealth creation as well as old-age security. Primogeniture, or the practice that the first son has first command over ancestral land, makes the first son particularly important. Hence, it is plausible that land reform, which awards land rights to the landless and small landowners, besides raising land values via productivity improvements, would enhance the inheritance motive for ensuring a male heir. This would be compounded if son preference arose also partly owing to a greater role played by male children in cultivating land owned by the household.

The inheritance-cum-child labor motive would therefore generate a higher effect of the land reform on survival chances of male children born in families without a first son, compared to those with a first son. One would expect the corresponding wealth effects to be ordered the opposite way, since a first son if anything would be associated with higher household wealth. Hence, a higher effect of the land reform on survival of male children in families without a first son compared to those with a first son, would indicate that the inheritance-cum-child labor motive dominates the wealth effect; the difference between these two effects provides a lower bound to the magnitude of the former motive.

We exploit variation in land rights created by Operation Barga, a flagship tenancy reform in the Indian state of West Bengal, that previous research has shown to have increased agricultural productivity and farm incomes significantly (Banerjee, Gertler, and Ghatak 2002; Bardhan and Mookherjee 2011). We find evidence consistent with the coexistence of a male biased inheritance effect and female-biased wealth effect: higher program implementation rates significantly raised survival chances of male children in families without a first son, relative to those with a first son. The converse was observed in families with a first son: survival chances of subsequent daughters rose, and those of subsequent sons were unaffected. As the male biased inheritance motive does not operate among families with a first son, we interpret the latter finding as reflecting wealth effects associated with the reform, which benefited female rather than male children, consistent with the findings of previous literature (e.g., Anukriti 2018, Bhalotra and Cochrane 2010, Maccini and Yang

2009). The effects are pronounced among Hindu families (whose inheritance practices are known to be more male biased than non-Hindu families), and among landless and small landowning households.

We find no evidence of corresponding effects of the land reform on gender or survival chances of firstborn children, consistent with the hypothesis that the gender of the firstborn was effectively random. The differential reform effects on survival of later born children across families depending on the gender of the firstborn therefore provide compelling evidence in support of our hypothesis, by thus controlling for possible community-specific and household-specific confounding factors. Nevertheless we confirm the results are robust with respect to controls for pre-reform trends, mother or household fixed effects, birth year and birth order fixed effects, mother's age at birth, district-specific linear time trends and district-year measures of rice productivity and infrastructure. Further, we obtain similar results in separate investigations utilizing two independently gathered datasets varying in sample coverage, questionnaires, and measures of land reform.

The land reform program involved registration of tenant farmers in West Bengal which endowed them with heritable tenurial security and capped landlord shares. It was initiated by a Left Front government elected in 1977. It is estimated that 2–3 million sharecropper tenants were registered (half to two-thirds of all tenants) by the mid-1990s, after which registration plateaued (Bardhan and Mookherjee 2010). We merge district-year data on the sharecropper registration rate between 1977–1991 used in Banerjee, Gertler, and Ghatak (2002) with the year and district of birth of children in the National Family Health Survey (NFHS), which collects detailed household characteristics and fertility histories based on questionnaires administered to a large sample of women. We combine this with other district-year level statistics put out by the Government of West Bengal for infrastructure, and rice productivity data from ICRISAT.

The NFHS data allows us to separately examine effects of the land reform on sex ratios at birth, infant mortality rates, and fertility. Our main regression uses the infant mortality rate before age 1 year as the dependent variable, reductions in which correspond to increased survival chances. We find that passing a 50 percent registration rate (corresponding roughly to the median of the distribution of implementation rates) was associated with a mortality rate reduction of 6.4 percentage points for boys at birth order 2 or above in Hindu families without first sons, but not in families with first sons. Conversely, there was a 6 percentage point reduction in infant mortality rates of girls (at birth order 2 and above) in Hindu families with first sons, and no such reduction in those without first sons. The effects are both statistically and quantitatively significant (the pre-reform mean mortality rate was 10.7 percentage points). They are robust to our controls, including lagged district-level sex ratios at birth (which proxy for access to ultrasound facilities).[1] The corresponding estimates for non-Hindu families are smaller and statistically insignificant, though this may also reflect lower precision of estimates due to smaller sample sizes.

[1] The results inclusive of controls for lagged district sex ratios are available upon request.

Among Hindu families, we also find above-median registration rates had no impact on sex ratios for firstborn children. Among later born children, however, it raised the proportion of boys born by 4.5 percentage points; an effect statistically significant at the 5 percent level and large compared to a pre-reform ratio of 49.3 percent. The effect is present regardless of the gender of the firstborn child, unlike in the infant mortality results, where wealth effects from first sons appear to favor Hindu girls. The corresponding effects are smaller and insignificant for non-Hindu families. As there was largely no access to ultrasound facilities in most of rural West Bengal until the mid-1990s (Bhalotra and Cochrane 2010), we interpret the effects on sex ratios at birth as underreporting of births of children that survived for very short durations. As for effects of land reform on fertility, we find that above-median registration had no impact on the likelihood that second-born children had a younger sibling in Hindu households, irrespective of the gender of the firstborn child. Hence, the differential effects observed for infant mortality among boys and girls by the gender of the firstborn cannot be attributed to larger household size (e.g., which may strain household resources per child).[2]

These results are corroborated in a second dataset, a village-household panel survey (VHPS) conducted and used by Bardhan and Mookherjee (2010), Bardhan and Mookherjee (2011), and Bardhan et al. (2014) to study impacts of the land reform on farm productivity and land inequality. This includes data on proportion of cultivable land registered under Operation Barga at the village rather than at the district level. This provides a more accurate measure of land reform implementation compared to the district-level data used with the NFHS exercise, for two reasons: it was collected directly from local land records offices, and relies on land area estimates rather than proportion of sharecroppers registered. The VHPS also includes data on landholdings at the household level, enabling us to separately estimate land reform effects on child survival within households owning varying amounts of land prior to the reform.

However, in this dataset we only observe number and ages of surviving children in the survey year (2004), rather than separate data for births and infant mortality. We cannot therefore disentangle effects on sex ratios at birth, infant mortality and subsequent fertility as was possible with the NFHS data. The dependent variable accordingly is the likelihood of birth of a child of either gender in a given year (of birth order 2 or above) that survived until 2004, which is regressed on extent of land reform implemented in the village, interacted with gender of the firstborn child (assumed to be the oldest surviving child). Above-median land area registered in a village in a given district-year led to a 4.9 percentage point greater effect on chances of a surviving boy being born following a first child who was a girl, compared with families where the first child was a boy, consistent with the male-biased inheritance motive. This estimate is significant at the 1 percent level, and robust to controls for household fixed effects, land owned prior to the reform, land titles received under a parallel land reform program, and district-year fixed effects. We find no evidence of wealth effects favoring girls born in first-son families in this dataset, but this is

[2] However, a differential effect on fertility was observed when the registration rate crossed 25 percent, among Hindu and non-Hindu families alike.

potentially explained by the worsening of the post-reform sex ratio captured in the NFHS results, that may counteract gains in female infant survival in these families. The differential effects in later born male child survival are significant (at the 5 percent level) among Hindu families, but not among non-Hindu families, as in the NFHS data. The effects are concentrated among landless households and among small landowners (owning between 1.25 and 2.5 acres of cultivable land). They were plausibly the largest beneficiaries of the program: the landless owing to gaining secure and heritable cultivation rights to leased land, and the small landowners owing to rising land values.

The results therefore provide compelling evidence of a significant male-biased inheritance motive favoring survival of higher birth order male children in families without a first son. This motive co-exists with wealth effects that favor survival of higher birth order female children in families with a first son. The contrasting nature of the effects on infant mortality by the gender of the firstborn makes it difficult to infer aggregate impact of the land reforms on gender imbalance in mortality rates in the population as a whole. However, the NFHS results indicate that crossing the median registration rate led to a significant 5 percentage point worsening of the sex ratio at birth for Hindu families, and a 3.8 point effect for all families, irrespective of the gender of the firstborn. This suggests that the West Bengal land reform worsened gender imbalance overall, while also raising productivity and incomes, lowering inequality between households, raising education among low-caste children (Deininger, Jin, and Yadav 2011), and lowering fertility (see Table 6).

A related paper (Almond, Li, and Zhang 2013) analyses the Chinese land reform during the late 1970s and finds child gender ratios became more male-biased after land reform. The Chinese reform differed from the West Bengal reform by retaining state control over allocation of land, whereby intergenerational transfer of land within households was not assured. Moreover, men and women had equal rights in state redistribution of land. Hence, the inheritance mechanism that we focus on in this paper is unlikely to have operated in a similar way in the Chinese context. Almond et al. argues a different set of channels operated in China: income gains from land reform in China raised both the desire to have sons and the feasibility of fulfilling this desire (for instance, by making it easier for them to afford travel to provincial capitals for abortions).

The rest of the paper is organized as follows. Section I provides a background discussion of Operation Barga in West Bengal and prevailing son preference norms in India. Section II sets out a theoretical framework to structure and interpret the empirical analysis. Section III describes the data, Section IV outlines the empirical methodology, and Section V presents the empirical results. Section VI concludes.

I. Background

A. *Historical Context*

Upon national independence in 1947, the Indian central government initiated three main types of land reforms to address large historical inequalities in land distribution. These were abolition of intermediaries, new tenancy laws to protect against eviction and extraction of excessive rental crop shares by landlords, and land ceilings to limit the amount of land held by any one household with the aim of vesting and redistributing surplus land to small farmers. Implementation of the reforms was left to individual state governments. However, barring intermediary abolition in nearly all states, landlords were able to subvert the remaining reform measures by way of preemptive tenant evictions and parceling land to relatives to avoid state confiscation of above-ceiling holdings (Appu 1996). Variation in state-level reform implementation and legislation over time has been used in previous studies to empirically estimate land reform impacts on poverty, equity, and human capital (Besley and Burgess 2000; Ghatak and Roy 2007; Ghosh 2007; Deininger, Jin, and Yadav 2011). West Bengal's land reform was an unusual success amidst myriad failures, and a number of influential studies have analysed its economic impacts (Banerjee, Gertler, and Ghatak 2002; Bardhan and Mookherjee 2011; Bardhan, Mookherjee, and Kumar 2012; Bardhan et al. 2014).

Reforms in the state of West Bengal were spurred by the outcome of the 1977 state assembly election, following a Maoist land-based movement in late 1960s. The Left Front coalition won an absolute majority, which it retained until 2011. This new government created a three-tier system of local governments called panchayats, which for the first time would be democratically elected. These tiers, in descending order of size of jurisdiction, were district, block, and finally the gram panchayat that operated at the village level with a jurisdiction of 10–15 hamlets (mouzas). Many national development programs as well as aspects of new state welfare initiatives such as Operation Barga were then decentralized to gram panchayats, who were responsible for selecting local eligible beneficiaries and lobbying the upper tiers of the new system for funds (Bardhan and Mookherjee 2011).

B. *Operation Barga and the Green Revolution*

West Bengal, along with Kerala, was an exceptional state in terms of the effort and success with which the state government pursued land reforms. Registration protected sharecroppers from eviction by landlords, giving them permanent, tenancy rights and capping the share of the crop payable as rent to landlords to 25 percent. The tenancy rights could be used as collateral for loans and could be passed on to their heirs. By 1981 over 1 million sharecropper tenants were registered, and almost 1.5 million were registered by 1990 (Lieten 1992). Estimates of the fraction of sharecroppers registered in the state range from 45 percent (Bardhan and Mookherjee 2011) to 65 percent (Banerjee, Gertler, and Ghatak 2002), to as high as 80 percent (Lieten 1992).

Besides Operation Barga, the state also aimed to vest land held by households above the stipulated ceiling of 12.5 acres and redistribute it to the landless and small landowners in small plots (or pattas). Most vesting of land had already taken place by 1978, so the Left Front government's main role was in redistributing this land. Appu (1996) estimates that 6.72 percent of state operated area was distributed by 1992; several times the national average of 1.34 percent. However, this land was redistributed in small plots (less than half an acre, on average, in the sample of farms in Bardhan and Mookherjee 2011) and was of low quality for cultivation as landlords would only part with their lowest quality above-ceiling holdings. Hence, unlike tenant registration, land redistribution had virtually no impact on agricultural productivity (Bardhan and Mookherjee 2011), while lowering the incidence of landlessness (Bardhan et al. 2014).

There were other government initiatives launched in the state at the same time, including decentralization, local infrastructure investment, and programs aimed at boosting agricultural productivity and reducing poverty. Alongside Operation Barga, the state government also distributed minikits containing high-yield variety (HYV) seeds, fertilizers, and insecticides to farmers throughout the state via gram panchayats. Land reform in combination with minikit distribution led to a substantial increase in agricultural yields in West Bengal over the 1980s, transforming the state into one of the best agricultural performers in the country and leading this period to be called West Bengal's Green Revolution. This period is also associated with significant declines in poverty and growth in rural employment. Banerjee, Gertler, and Ghatak (2002) attributed the increase in yields to land reform, citing decreased Marshall-Mill sharecropping distortions from increased tenancy security. Bardhan and Mookherjee (2011), however, shows that while decreased inefficiencies played a role in increasing yields, it was largely minikit distribution that was responsible for the agricultural growth in this period. Other programs administered in the 1980s with gram panchayats targeting local beneficiaries include the Integrated Rural Development Programme that provided subsidized credit, and employment initiatives such as the Food for Work program, the National Rural Employment Programme, and the National Rural Employment Guarantee Programme.

C. *Son Preference*

The majority Hindu community in India traditionally exhibits greater son preference than other religious communities, as evidenced by conditional sex ratios in the population and empirical evidence on child mortality and education that reflect childhood parental investments (Bhalotra and Zamora 2010, Bhalotra and Cochrane 2010, Bhalotra 2010). The literature in this regard has focused on Hindu-Muslim differences, as other religious communities make up a very small part of the population.

While no definitive explanation has been agreed upon for the differing degrees of son preference between the Indian Hindu and Muslim communities, existing arguments such as the Dyson-Moore hypothesis base them in marital institutions and inheritance practices. In North India including West Bengal, Hindu marriage is exogamous for women, who leave their natal family village to marry into families in villages much further away to avoid marrying a possible relative. The distance from natal family after marriage reduces Hindu women's bargaining power and also their claim to natal family land, which is seen as bringing no reciprocal benefit and lost to the family when daughters inherit. Sons, on the other hand, care for parents and natal family members in their old age by remaining with the natal family and working the family land, eventually inheriting it upon the death of the family patriarchs. Cultural taboos against Hindu women sharing public spaces with men and working agricultural land also often prevent them from claiming and cultivating land (Agarwal 2003). The bridal dowry practice also often entails loss or mortgage of family land at the time of a daughter's marriage. With regard to Operation Barga specifically, Gupta (2002) finds, from interviews of 870 households in two West Bengal districts, that 99 percent of households reported dowry being a serious concern, and that mortgaging barga land to meet dowry payments was a common practice. She also finds that dowry was largely a Hindu practice, but that the custom has penetrated younger generations of Muslims.

Under the *Mitakshara* Hindu doctrine followed in North India, women have no claim to joint family property, whereas men are entitled at birth to a share of such family property held by their fathers, paternal grandfathers, and paternal great-grandfathers. In South India, close-kin marriages are more prevalent for Hindu women, allowing them to inherit a greater share of ancestral land as they reside close enough to participate in cultivation on natal family land after marriage. These marital institutions have been used to explain more favorable female-male sex ratios in South India compared to North India (Chakraborty and Kim 2010). In West Bengal, the *Dayabhaga* Hindu system of inheritance is followed, where the concept of joint family property is absent, and all of a Hindu male's property is subject to equal claims by his widow, sons, and daughters upon his intestate death (Lingat 1973). While this appears more gender-equal than the *Mitakshara* system in theory, in practice Hindu women nearly always relinquish their inheritance claims to their brothers and sons so as to avoid social exclusion, intimidation, and losing the family safety net in times of financial crisis (Agarwal 2003). Hindu upper caste women also do not physically work agricultural land due to prevailing social norms. Lower caste women have higher workforce participation rates in agriculture as wage laborers, but still female employment rates in agriculture in the state have been persistently low. Hindu women therefore are very much financially dependent on their male kin, leading them to give up their rights to family land to avoid losing that support. These unequal gender norms governing labor market participation have also been argued to contribute to son preference, as they increase the household returns to having sons relative to daughters (Rosenzweig and Schultz 1982).

Muslim communities follow inheritance practices based in the Shariat, which guarantees women at least half as much inheritance as their closest male counterpart inheritors. Consanguineous marriage is also practiced to keep all ancestral property within the family, allowing Muslim women to remain close to their natal families after marriage and inherit more family property, in practice, similar to Hindu women in South India. Marital dowry is also less prevalent among Muslims, and abortion, sex selective or otherwise, is strictly forbidden under the Shariat. The effect of these institutions arguably reduces parental neglect of Muslim female children compared to Hindu female children in many parts of the country, including West Bengal, despite the fact that the Muslim minority population experience higher levels of poverty nationwide than the Hindu majority and Muslim female labor force participation in West Bengal is even lower than that of Hindu women (Nasir and Kalla 2006, Chakraborty and Chakraborty 2010).

II. Data and Descriptive Statistics

We use two independently gathered household survey datasets, both representative of the state of West Bengal: the National Family Health Survey (NFHS) focusing on fertility and child health, and a village-household panel survey (VHPS) conducted to gather data on land reform and its partial and general equilibrium effects on farm productivity and land distribution (Bardhan and Mookherjee 2011). We use them in two separate empirical analyses, so as to take advantage of the unique features of each dataset.

The strengths of the NFHS data are that it records the entire birth history of all women aged 13 or 15 to 49 at the time of the survey, allowing us to identify the exact date of birth and death for children. Moreover, we have fertility histories for biological mothers, so we can identify the birth order and sex of every child, allowing us to construct an indicator for the sex of the firstborn child. Nevertheless, there is a possibility of underreporting of births of some children that died very soon after birth (at home). For this reason, we shall also examine effects of the reform on the sex ratio at birth (among reported births). As mentioned previously, the possibility of sex-selective abortion was low during the period being studied owing to the lack of availability of ultrasound scan facilities in rural West Bengal until the mid-1990s. Hence, unbalanced sex ratios at birth are likely to reflect underreporting of births of children that survived a very short period.

The weaknesses of the NFHS data are twofold. First, we do not have access to reliable data on land reform implementation in all the villages represented in the NFHS data. Hence, we use the district-level share of tenant farmers registered (from Banerjee, Gertler, and Ghatak 2002) as a measure of land reform implementation. Second, we do not have data on land owned by each household, so cannot examine heterogeneity of impacts across different land classes. In all of the analysis, the dependent variables are at the individual level. Since the treatment is at the district level, we account for the non-independence of the errors within the treatment unit.

These problems do not arise in the VHPS, which covers a different sample of villages and households, and includes data on household demographics and land details, as well as land reform at the village level. The household-level data includes family histories and land ownership since 1967. The questionnaire elicited information from the head about all members residing in the household in 2004, including the year they were born or joined the household. It reports the births of all children in the household, but only for those that survived till 2004. We therefore have a compound measure of birth and survival. For approximately two-thirds of the households in the sample, a consistent history of household landholdings and demographics could be constructed (we call this the "restricted sample;" details are in Bardhan et al. 2014). For the rest a consistent history could be constructed under specific assumptions on the nature of recall errors. While we report only results from the restricted sample, we verify that the results do not differ qualitatively in the full sample.

Information on land reform implemented in each of the 89 surveyed villages between 1968–1998 was collected from Block Land Records Offices. The strength of the land reform data is that it is at the village level rather than the district level. Moreover, it was compiled firsthand from official land records rather than in aggregated form from indirect sources (the authenticity of which in West Bengal has often been questioned (see, e.g., Boyce 1987)). Data quality aside, the share of cultivable village land registered is likely to be a better measure than the share of tenants

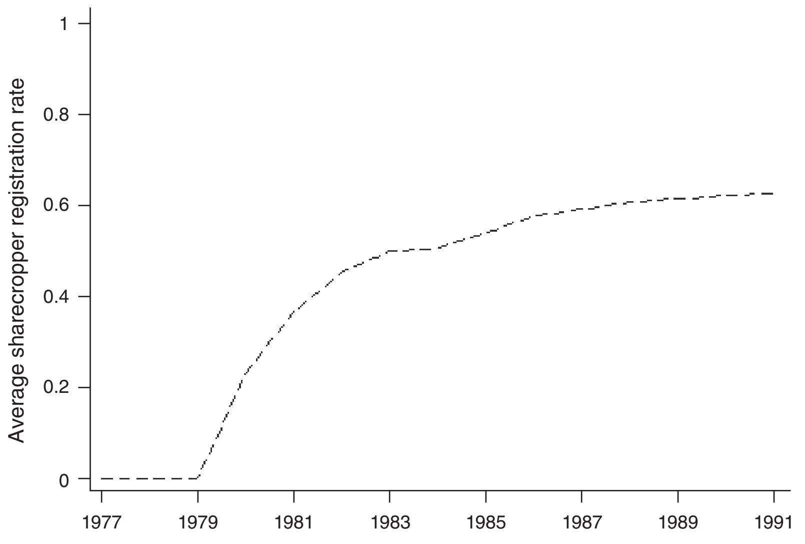

Figure 1. Cumulative Share of Tenants Registered by Year

Note: The figure shows the average rate of completed sharecropper registration across the 14 West Bengal districts in the Banerjee et. al (2002) data during 1975–1991.

registered because it overcomes the concern with the latter that it may provide a misleading measure of the intensity of the program if the potential number of tenants is small, but most of them are registered.

On the other hand, the VHPS data has the drawback that it comes from a survey conducted in 2004, where the demographic module includes birth years of all members residing in the household in that year. This enables us to measure children born during a past year who survived until 2004, i.e., the joint outcome of birth in some year t prior to 2004, and survival of this child until the year 2004. Children who were born but did not survive until 2004 are not reported. So we cannot separately estimate land reform effects on fertility and infant mortality.

A. *Descriptive Statistics*

We pool the 1992–1993 and 1998–1999 waves of the NFHS as these rounds contain a district identifier for every household. The data are transposed to create identifiers for the district and year of birth of every child, and then merged with district-level sharecropper registration rates for the 14 districts for which the data is available (from Banerjee, Gertler, and Ghatak 2002).

Figure 1 shows the evolution of the tenant registration rate over time. There is no positive registration recorded in the data prior to 1978, although registration of tenants had begun under the previous government. Sharecropper registration occurred most rapidly up until about 1983, after which the pace slowed considerably. Our analysis is confined to births during 1978–1991, as we do not have information on district-level programs other than land reform after this year.

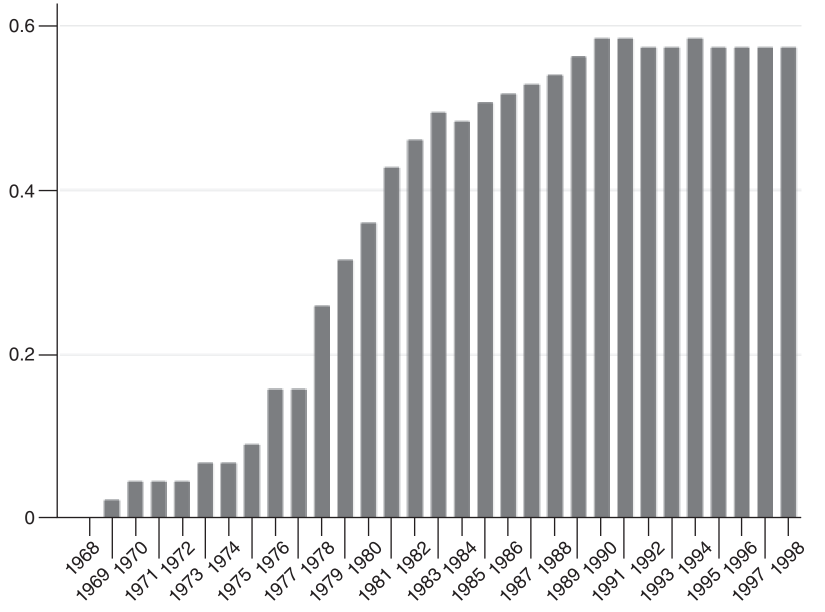

Figure 2. Proportion of Villages above the Median Share of Land Registration

Notes: The figure shows the proportion of villages above the median share of land registration across the 89 villages from the VHPS dataset during the years 1968–1998. The percent of cultivable land registered declines after 1985 as registration slowed during this period, while the amount of cultivable land increased on average.

Figure 2 shows the alternative measure of land reform (proportion of villages above the median share of land registration) from the VHPS data. It shows there was some reform prior to 1977, but the pace picked up between 1978 and 1985, slowed down between 1985–1989, and plateaued thereafter. The overall time pattern is very similar to Figure 1. In the regressions we will use the period 1978–1998; the exact choice of end year does not really matter as there was very little additional reforms being implemented during the 1990s.

Panels A and B of Table 1 provide descriptive statistics pertaining to characteristics of children in the NFHS sample born during 1967–1993 and their mothers. Neonatal and infant mortality rates were 6.4 percent and 9.4 percent, respectively. The probability that a child is male was 51.1 percent, and the probability of the child having a younger sibling was 71.8 percent. Sixty-eight percent of mothers resided in rural areas; the average age at which they give birth was 19.03 years. The average years of education of mothers in the sample is 3.42 and they have an average of 3.39 births. Seventy-five percent of mothers are Hindu.

We obtained district-level data on yields and area under cultivation of rice in West Bengal from the ICRISAT Village Dynamics in South Asia (VDSA) database to construct measures of annual district rice productivity in thousands of tonnes of output per one thousand hectares for the years 1977–1990. We also collected district time series information from the annual Economic Survey reports of the West Bengal government to control for the effects of other programs and infrastructure. Specifically, we gathered information on the number of medical institutions

Table 1—DHS Summary Statistics

Variables	Mean (1)	SD (2)	Min (3)	Max (4)	Observations (5)
Panel A. Mother characteristics: 1967–1993					
Years of education	3.416	4.297	0	18	6,443
Age at birth	19.034	3.532	5	40	6,468
Total births	3.386	2.010	1	11	6,468
Hindu	0.750	—	0	1	6,468
Rural	0.680	—	0	1	6,468
Panel B. Child outcomes: 1967–1993					
Infant death	0.094	—	0	1	20,148
Neonatal death	0.064	—	0	1	20,148
Male child	0.511	—	0	1	20,148
Has younger sibling	0.718	—	0	1	20,148
Panel C. District productivity and programs: 1977–1990					
Rice productivity	1.473	0.434	0.720	2.595	196
Patta area per capita	6.518	4.937	0.321	17.986	196
Surfaced roads per capita	0.208	0.067	0.115	0.392	196
Medical institutions per capita	0.056	0.016	0.033	0.115	196

Notes: Panel A shows mother characteristics and panel B shows child outcomes for cohorts born during 1967–1993. Panel C shows productivity and program statistics in the 14 districts with sharecropper registration data for years 1977–1990, which are the years for which they enter as controls in the regressions. Neonatal death takes value 1 if the child dies aged 0–1 months, and infant death takes value 1 if the child dies aged 0–12 months.

per capita, kilometers of surfaced roads per capita, and hectares of patta land distributed per capita. Descriptive statistics for the district-year varying controls are in panel C of Table 1.

Table 2 provides descriptive statistics from the VHPS data, for the period 1978–1998 used in the regressions. Eighty percent households were Hindu; 25 percent had immigrated into the village since 1967. Half were landless, 16 percent were marginal landowners owning less than 1.25 acres of cultivable land, 9 percent were small owners owning between 1.25 and 2.5 acres, while 25 percent owned more than 2.5 acres. Average land owned was 2.23 acres. Panel B shows the average likelihood of a male and female child being born in any given year between 1978–1998 and surviving until 2004 was 6.0 percent and 6.4 percent, respectively. Panel C reports relevant village-level characteristics: the mean proportion of village land registered (across different village-years) was 5.1 percent. To make results comparable to those from the NFHS sample, in the regressions we measure the extent of reform activity as a dummy that takes the value of 1 if the cumulative percentage of village cultivable land registered under sharecropping (barga) is above the median (computed at the village-year level).

In the VHPS data we can control for the land redistribution component of the program, which involved awarding titles to small plots (pattas) to farmers. Approximately 15 percent of surveyed households had received patta land by 1998. However, as discussed in Bardhan and Mookherjee (2011) and Bardhan et al. (2014), the patta program did not raise farm productivity appreciably because the distributed plots were small and of poor quality, and were not eligible to be used as collateral for subsidized credit. In contrast, plots registered under barga (the tenancy reform) were of a much larger size (1.5 acres on average) and could be used as collateral for loans

Table 2—VHPS Summary Statistics

	Mean	SD	Min	Max	Observations
Panel A. Household characteristics (1978–1998)					
Hindu	0.807	—	0	1	1,946
Immigrant	0.252	—	0	1	1,946
Landless in 1977	0.501	—	0	1	1,946
Marginal in 1977	0.163	—	0	1	1,946
Small in 1977	0.090	—	0	1	1,946
Large in 1977	0.246	—	0	1	1,946
Household size	5.454	2.030	1	22	1,946
Boys	0.756	0.915	0	7	1,946
Girls	0.812	0.957	0	6	1,946
Panel B. Household-year characteristics (1978–1998)					
Boy birth and survival	0.060	—	0	1	24,696
Girl birth and survival	0.064	—	0	1	24,696
Agricultural land (acres)	2.237	3.568	0	36	24,696
Panel C. Village-year characteristics (1978–1998)					
Percent land registered	0.051	0.106	0.000	0.516	1,825
log(rice productivity)	0.491	0.330	−0.440	1.119	1,825

Notes: All sources are listed in the text. In panel C, percent land registered has been winsorized at the 98.5th percentile due to two villages that exhibit abnormally high land registration in various years.

from state financial institutions, yielding greater positive impacts on rice productivity. Hence, we focus on Operation Barga rather than the land distribution program.

III. Model and Predicted Effects of Land Reform

Under Operation Barga, agricultural tenants benefited directly in two respects, increased land security and a greater share of agricultural output. At the same time, the reform reduced land rights and rents of landlords. These comprise the direct partial equilibrium (PE) effects. The reform also generated a number of general equilibrium (GE) effects. Reduced profitability of leasing out land induced large landowners to sell some of their landholdings to smaller landowners, lowering land inequality (Bardhan et al. 2014). In addition, there were positive effects on land productivity across *all* farms, both owner cultivated and tenanted (Bardhan and Mookherjee 2011), owing partly to induced investments in minor irrigation which lowered water prices in the village (Bardhan, Mookherjee, and Kumar 2012).

Our hypothesis is that there were two kinds of impacts of the reform on the value placed by (predominantly Hindu) families on children: wealth effects benefiting children of both genders, possibly differing across gender, and a property inheritance effect favoring boys in families without a prior son. Both effects vary with birth order and gender of elder children. The following model describes these disparate effects and helps generate testable predictions.

Let β denote the measure of land reform (LR) implemented in a given village. The resulting wealth of household j is

$$W_j(\beta) = \theta(\beta)\left[l_j + \beta t(l_j)\right], \tag{1}$$

where θ denotes land value that is rising in β owing to the GE productivity enhancing effects of the reform. Land owned by the household is denoted by l_j and the change in land rights owing to the reform is given by $\beta t(l_j)$, where $t(l)$ is a decreasing function, satisfying $t(0) > 0$, positive over an interval $(0, l^*)$ and negative if $l > l^*$. This captures both the direct PE effect and the GE effect of the reform through land markets: positive for the landless leasing in land, decreasing in land owned (reflecting negative correlation between land leased in and land owned), and negative for large landowners who own more than l^* and lease out land.

The resulting impact on the value placed by household j on child i of birth order two or above is

$$(2) \qquad v_{ij} = \left[a + (\delta_1 + \delta_2 f_j)(1 - m_i) + \left\{\delta_3 f_j + \pi(1 - f_j)\right\} m_i\right] W_j(\beta).$$

The first term on the right-hand side of (2) is a common wealth effect: for each unit increase in wealth $a \geq 0$ is an increased value on children of both genders; m_i is a dummy variable for male gender of the child in question, while f_j is a dummy for male gender of the firstborn child. The term $(\delta_1 + \delta_2 f_j)$ represents the supplemental wealth effect for a female child, which depends on the gender of the firstborn: δ_1 represents the gender bias in the wealth effect in a family without a first son, while $\delta_1 + \delta_2$ in a family with a first son. The sign of δ_1 is ambiguous, while we expect δ_2 to be positive (based on previous findings in the literature; e.g., see Anukriti 2018, Bhalotra and Cochrane 2010); δ_3 is a corresponding wealth effect on the value of a male child, which is nonnegative.

The parameter $\pi \geq 0$ represents the property inheritance effect, which is biased in favor of boys, and operates only in families without a first son. The difference between the LR effect on male child survival in families without and with a first son equals $(\pi - \delta_3) W_j'(\beta)$. Since δ_3 is nonnegative, this difference provides a lower bound to the size of the male-biased inheritance effect $\pi W_j'(\beta)$.

How do the predicted effects vary with the land owned by the household? Notice that

$$(3) \qquad \frac{\partial W_j}{\partial \beta} = \theta'(\beta)\, l_j + \left\{\beta\theta'(\beta) + \theta(\beta)\right\} t(l_j).$$

The first term on the RHS (which reflects the GE effect of LR on land productivity) increases in land owned l_j. The second term (which includes both the direct PE effect as well as a GE productivity effect) is proportional to the effect of the reform on land rights/rents, which is decreasing in l_j. Hence, the net effect could be non-monotone in land owned. For the landless we have $l_j = 0$, and the first term drops out; as $t(0) > 0$, we expect a positive effect resulting from the access gained by landless households leasing in land to more secure and lucrative tenurial terms. The predicted effect continues to be positive for a range of marginal and small landowners with $l_j < l^*$. For those owning land in excess of l^* ,we have $t(l_j) < 0$, and the second term is negative, offsetting the positive GE productivity effect represented by the first term. Hence, the expected sign for large landowners is ambiguous.

In the NFHS dataset, we observe infant mortality of children born, but lack data on landholdings of each household, so we cannot examine how the predicted effects

TABLE 3—PREDICTED EFFECTS: LAND REFORM ON CHILD SURVIVAL

	Male child	Female child
First son = 0		
Model effect:	$a+\pi$	$a+\delta_1$
Predicted effect:	$(\gamma_1+\gamma_3)$	(γ_1)
NFHS estimate:	$-[\eta+\rho_2]$	$-[\eta]$
VHPS estimate:	$\{\phi_1^M\}$	$\{\phi_1^F\}$
First son = 1		
Model effect	$a+\delta_3$	$a+\delta_1+\delta_2$
Predicted effect:	$(\gamma_1+\gamma_2+\gamma_3+\gamma_4)$	$(\gamma_1+\gamma_2)$
NFHS estimate:	$-[\eta+\rho_1+\rho_2+\rho_3]$	$-[\eta+\rho_1]$
VHPS estimate:	$\{\phi_1^M+\phi_2^M\}$	$\{\phi_1^F+\phi_2^F\}$

Note: The table shows the predicted effects of land reform on child survival and the corresponding empirical model parameters identified by (10) for infant mortality using the NFHS data (square brackets) and by (12) for birth-cum-survival using the VHPS data (curly brackets) by gender of the child, and the gender of the child's firstborn sibling.

vary with landholdings. We observe households' district of residence rather than their village of residence, and have a measure of land reform at the district level. Hence, the predicted infant mortality (IM) of child i in family j in district k in year t can be expressed as follows:

$$(4)\qquad IM_{ijkt} = \gamma_0 - \gamma_1\beta_{kt} - \gamma_2\beta_{kt}\times f_j - \gamma_3\beta_{kt}\times m_i - \gamma_4\beta_{kt}\times m_i\times f_j - \gamma_5 f_j - \gamma_6 m_i - \gamma_7 f_j\times m_i.$$

The LR effect is measured for a household with average landholding $\theta(\beta)\left[E\{l_j\}+\beta E\{t(l_j)\}\right]$, where E denotes an expectation operator with respect to l_j. Table 3 displays the combination of these γ coefficients to the relevant model parameters within parentheses at the bottom of each cell. The LR effect may be nonlinear in β so we shall proxy it by indicators for crossing different thresholds or different quartiles of the distribution of LR across district-years. The corresponding regression specification is provided in the next subsection.

To the extent that births of children that survived very short periods were underreported, we can use the NFHS data to examine the effects of the land reform on sex ratio at birth (from the reported births). This is an alternative way of testing effects on infant mortality among very young children. The expression for the predicted effects is slightly different from (4) as the dependent variable is M_{jkt}, an indicator for male gender of a child born to a given mother j in district k for given year t, and child gender indicators are dropped from the right-hand side:

$$(5)\qquad M_{jvt} = \gamma_0 - \gamma_1\beta_{vt} - \gamma_2\beta_{vt}\times f_j - \gamma_5 f_j.$$

We also examine LR effects on fertility, by estimating the likelihood that a given child exposed to the reforms has a younger sibling, and allowing this to vary with gender of the firstborn child. The expression for predicted land reform effects on this outcome is similar to (4), but contains only the f_j terms. Details of the corresponding regression specifications are provided in the next subsection.

In the VHPS dataset we do not directly observe infant mortality of children that were born. Instead, we observe number of surviving children (in 2004) that were born in a given year within the LR implementation phase 1978–1998. In other words, we observe outcomes of the joint event of birth and survival, rather than survival conditional on birth. This incorporates non-reporting of children that did not survive. In this dataset, we observe the land owned by each household, so we can both control for and interact landholdings with variables in the regression. With too many interactions, the regression becomes difficult to interpret. So predicted LR effects for BS_{ij} (the joint event of birth and survival of child i in household j in village v in year t) can be expressed separately for female (F) and male (M) children as follows:

$$BS^F_{ijvt} = -\gamma_0 + \gamma_1 \beta_{vt} + \gamma_2 \beta_{vt} \times f_j + \gamma_5 f_j, \tag{6}$$

$$BS^M_{ijvt} = (\gamma_6 - \gamma_0) + (\gamma_1 + \gamma_3)\beta_{vt} + (\gamma_2 + \gamma_4)\beta_{vt} \times f_j + (\gamma_5 + \gamma_7) f_j, \tag{7}$$

as well as separately for each landownership category l (landless, marginal, small, or large) of the household in 1977:

$$BS^F_{ijlvt} = -\gamma_{0l} + \gamma_{1l} \beta_{vt} + \gamma_{2l} \beta_{vt} \times f_j + \gamma_{5l} f_j, \tag{8}$$

$$BS^M_{ijlvt} = (\gamma_{6l} - \gamma_{0l}) + (\gamma_{1l} + \gamma_{3l})\beta_{vt} + (\gamma_{2l} + \gamma_{4l})\beta_{vt} \times f_j + (\gamma_{5l} + \gamma_{7l}) f_j. \tag{9}$$

A. *Empirical Specification*

NFHS Households.—We estimate the equations above for infant mortality, the probability of a male birth, and fertility-stopping in the NFHS data, using OLS on the sample of children of birth order two or higher born during 1978–1991.[3] We carry out separate estimations for Hindu and non-Hindu children to account for the different institutional practices between communities described earlier. As the indicator of reform varies at the district level, and there are only 14 districts, the standard errors are wild cluster-bootstrapped (Cameron, Gelbach, and Miller 2008), using the procedure in Busso, Gregory, and Kline (2013).[4] For the infant mortality outcome, we estimate the predicted effects of LR in (4) using the following specification:

$$\begin{aligned} IM_{ijkt} = {} & \tau + \rho_1\, R50_{k,t-1} \times \mathit{firstson}_j \times \mathit{male}_i + \rho_2\, R50_{k,t-1} \times \mathit{male}_i \\ & + \rho_3\, R50_{k,t-1} \times \mathit{firstson}_j + \psi_1\, \mathit{firstson}_j + \psi_2\, \mathit{male}_i \\ & + \psi_3\, \mathit{firstson}_j \times \mathit{male}_i + \eta R50_{k,t-1} + \lambda X_{ijkt} + \zeta_k + \nu_t + \epsilon_{ijkt}, \end{aligned} \tag{10}$$

[3] We verify that land reform did not affect the mortality of firstborn children; see Table A.1. We also check for consistency of estimates by including firstborn children in the sample and coding the firstborn son indicator as zero for these firstborns, and by restricting the sample to the first two children only. The results do not change, and are available from the authors upon request.

[4] We also estimate a specification with an AR1 process for the standard errors, and the results are largely unchanged; see Appendix Table A.2.

where IM_{ijkt} is a dummy variable taking value 1 if child i of mother or household j, born in district k in year t died aged 0–12 months, and 0 otherwise; $R50_{kt-1}$ is an LR indicator that takes the value 1 if sharecropper registration rate in district k reaches at least 50 percent, respectively in the year preceding the childs birth year t, and 0 otherwise. The omitted category of children consists of girls with firstborn sisters born in districts where registration was less than 50 percent in the year preceding birth, or "untreated" by land reform. We chose this threshold rate based on estimates from a more flexible specification, and by the fact that 50 percent registration roughly coincides with the median registration rate in the child-level distribution of registration rates in the estimation sample (which was 48.5 percent).[5] Note that we would only expect linearity in the registration rate if all districts had the same tenancy rates at baseline, which was not the case. The variable $firstson_j$ indicates households with a firstborn son and $male_i$ indicates that the index child is male. We exclude firstborn children from the sample, but also verify that the reforms did not affect mortality among firstborns. The estimated coefficients in (10) capture LR impacts by child gender and gender of the firstborn child. Table 3 relates these coefficients (in square brackets) to the predicted LR effects in the model, yielding $(\rho_1 + \rho_3)$ as a lower bound estimate of π.

Since all districts in West Bengal experienced tenant registration and the variation is only in rates of progression, we also report results from estimating (10) including children born in bordering districts in the neighboring state of Bihar as a control group, as these children are never exposed to land reform. There are effectively four dimensions across which we exploit differences to achieve identification, which are district, year of birth, child gender, and the gender of the firstborn child in the household. The impacts are identified independently of child birth year and district fixed effects captured in dummy variables ν_t and ζ_k. We test robustness to include district-specific linear trends in child birth year to control for district specific unobservable trends that may be simultaneously correlated with sharecropper registration rates and infant mortality risk. The covariate vector X_{ijkt} includes indicators for child birth order, household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms in the age of the mother at the birth of the child.[6] So as to allow for individual selection into program uptake or fertility, we also estimate the specification with mother fixed effects. Mother fixed effects absorb district fixed effects since mothers typically do not migrate between births.

Productivity was increasing in West Bengal in the period studied, partly owing to the land reform, which generates the GE effect of the land reform explained in the previous section. To gain some insight into the magnitude of the PE effect, we examine the effects of controlling for increased agricultural yields. Specifically, we

[5] We tested for significant effects of cumulative sharecropper registration rates in 10 percent increments, and we tested for a quadratic in registration rates. These results are available from the authors upon request.

[6] To control for possible confounding effects of the spread of fetal sex determination technology such as ultrasound across West Bengal and all of India in the 1980s, we also test our results for robustness to the inclusion of the lagged district-level sex ratio at birth, calculated from the NFHS data as proxy for access to such technology—an approach used previously in the literature (Hu and Schlosser 2016). The results are almost completely unchanged, and available upon request.

estimate specifications including the log of district productivity of rice in the year prior to the child's birth as a regressor, interacted with indicators for the sex of the firstborn child and the sex of the second or higher order index child. Rice is the major crop in West Bengal, accounting for more than 70 percent of gross cropped area during 1971–1991, according to state government economic reviews, but we also controlled for yield of all other cereals.

To further control for any confounding effects of public health improvements, infrastructure development, and the other arm of the land reform, we include controls for the logarithm of medical institutions per capita, kilometers of surfaced road per capita, and hectares of patta land distributed per capita in the district in the year preceding the child's birth, and their interactions with index child gender and the gender of the firstborn child.

We then investigate the predicted impacts of LR on the sex ratio at birth as expressed in (6). We define an outcome variable taking value 1 if child i is male, and 0 otherwise. The regressor of interest, as before, is median registration indicator $R50_{kt-1}$ interacted with the indicator for a firstborn son $firstson_j$. We first test our assumption that the sex of first births is quasi-random and unaffected by the reforms. We then estimate the equation for second and higher order births to test whether sex at birth is modified by land reform in the same direction as sex after birth (via infant mortality). The sex of a birth is, of course, conditional upon fertility. We assess any selection bias by estimating fertility responses to tenancy reform, which is also of interest in its own right.

Finally, to investigate whether tenancy reform influenced fertility, we estimate an equation with the dependent variable an indicator taking value 1 if index child i has a younger sibling, and 0 otherwise. Given evidence that fertility-stopping behavior at any time is sensitive to the sex composition of preceding children, and evidence that the sex of the firstborn is quasi-random, we interact the median registration rate indicator $R50_{kt-1}$ with the firstborn son indicator $firstson_j$. In fact, we find fertility responses at below-median levels of registration, so we include a further $R25_{kt-1}$ indicator taking value 1 if registration in district k was at least 25 percent in the year preceding the child's birth, and its corresponding interaction with the firstborn son indicator. We estimated these specifications sequentially for separate samples of children by birth order, so as to identify the margin at which households alter childbearing in response to land reform. We found no impact of land reform on fertility-stopping after the first birth (see Appendix Table A.3) and also no impacts on stopping after the third birth (available upon request). We therefore present estimates for stopping after the second birth, which is plausibly the relevant margin.

Test for Targeting of Sharecropper Registration.—If the rate of tenant registration was correlated with pre-reform trends in the outcome variables, the estimated impacts of registration on the outcomes may be spurious. For instance, registration may have progressed more rapidly in districts where male infant mortality was already declining faster than female infant mortality (and more so in households with firstborn daughters). To investigate this, we use pre-reform data on the outcomes. Since registration is a continuous variable, we discretize it by assigning districts as treated or not depending on whether they had achieved above- or below-median levels of registration by 1985. We chose 1985 because registration occurred most rapidly up until 1985 (see Figure 1). We use a sample of children of birth order 2 or higher born before the program, during 1958–1977. We then regress the outcomes of interest on "treated" interacted with a linear time trend. A significant coefficient on this interaction term will reveal whether district pre-program trends in the outcomes were correlated with a district becoming a "treated" (or high intensity reform) district in the future. Since the main equations are estimated with first-son interactions, the stricter test of pre-trends includes this interaction. The estimated equation for infant mortality for instance is

$$
\begin{aligned}
(11) \quad IM_{ijkt} = {} & \tau + \kappa_1 treated_k \times trend_t \times firstson_j \times male_i \\
& + \text{three-way interactions} + \text{two-way interactions} + maineffects \\
& + \lambda X_{ijkt} + \zeta_k + \nu_t + \epsilon_{ijkt},
\end{aligned}
$$

where IM_{ijkt} is the infant mortality outcome for child i of mother or household j, born in district k in year t. The variable *treated* is the indicator for above-median district registration in 1985, *trend* is a linear time trend for the pre-reform years 1958–1977, and we include all three and two-way interactions and main effects though these are not displayed. The covariates included in X_{ijkt} are the same as in (10), except that controls for other district programs and infrastructure are not included here as they are not available for the pre-reform years. We estimate analogous equations for the other outcomes, fertility, and the sex ratio at birth.

VHPS Data.—As noted earlier, the VHPS data do not contain full birth histories or exact dates of death, so we are unable to directly identify either infant mortality or fertility. Instead, we model as outcomes the probability of a surviving girl, and a surviving boy being observed in 2004 (the last round of the village survey) in response to land registered under Operation Barga during 1982–1995. We estimate the predicted LR effects in (6) and (7) using the following specification:

$$
(12) \qquad BS^s_{ijvt} = \tau + \phi^s_1 LR_{vt} + \phi^s_2 LR_{vt} \times firstson_j + \varsigma X_{ijvt} + \nu_t + \zeta_j + \epsilon_{ijvt},
$$

where $s \in \{F, M\}$. The variable BS^s_{ijvt} takes the value 1 if a surviving child i is born in household j in village v in year t, and 0 otherwise. Specifically by child gender, the outcome variable takes value 1 when the surviving child i born in year t is a boy (girl), and value 0 if there is no birth in year t, or if there is a surviving birth in year t that is a girl (boy). The dummy variable LR_{vt} takes the value 1 when the cumulative percentage of village cultivable land registered under sharecropping falls above the median percentage of village land registered in year t in the district-year distribution.[7] As with the NFHS data, we interact the above-median land reform indicator

[7] Results are robust to replacing land reform in year t by land reform in years $t-1$ or $t-2$.

with an indicator for the first child in the household being male, $firstson_j$. We set $firstson_j$ equal to 1 if the oldest observed surviving child in the household is male, and 0 otherwise. The estimates of ϕ_1^s and $\phi_1^s + \phi_2^s$ therefore identify the predicted LR effects for boys $(s = M)$ and girls $(s = F)$ without and with firstborn male siblings, respectively, and are shown in curly brackets with the corresponding predicted effects they identify in Table 3. The model predictions in (8) and (9) are identified by estimating (12) separately by household landholding category for each child gender. The terms ν_t and ζ_j are year and household fixed effects, respectively, and ϵ_{ijvt} is an idiosyncratic error term. Household fixed effects absorb village fixed effects, since mothers typically do not migrate between births, and account for potentially correlated regional heterogeneity and household level selection.

The regressors X_{ijvt} include (lagged) land owned by the household, an above-ceiling indicator (whether it owned more land than permitted by the land ceiling), and patta land distribution. We define landowning classes, household land holdings, and the land ceiling indicator using pre-reform reported household landholdings in 1977 to avoid endogenous sample selection on landholdings that may change due to the reform. Finally, we examine how results are affected upon controlling for district-year fixed effects, which control flexibly for any relevant time-varying unobservables at the district level, including the GE effect of the reform.

IV. Empirical Results from the NFHS Data

A. *Results for Infant Mortality*

Figure 3, panels A–C show event study graphs of land reform impacts on infant mortality for Hindu children of different birth orders and gender, across years varying in distance from the achievement of the median registration rate. These effects are produced by estimating (10) after replacing the above-median registration indicator $R50_{k,t-1}$ with indicators for years before and years after median registration in district k, with the year that median registration is reached as the omitted category. All the controls barring those for other programs and rice productivity are included in the regressions, as well as district-specific linear time trends. Among firstborn children in Figure 3, panel A, we see a decline in mortality rates for both boys and girls, with no significant gender difference. Among higher birth order children with a firstborn sister in Figure 3, panel B, mortality rates for boys drop while those of girls rise. When the first child is a son, the mortality rate of boys drops slightly in Figure 3, panel C, but more sharply for girls.

Table 4 reports the regression estimates from (10) for infant mortality with the full set of controls.[8] Columns 1–3 provide the results for the entire sample, and columns 4–6 and 7–9 show the results for the subsamples of Hindu and non-Hindu families, respectively. For each of these samples, the first column shows estimates conditional on district fixed effects, the next column adds lagged controls for the log of district rice productivity, and the final column further adds a district-specific linear time

[8] We supress the j, v, and k subscripts on the regressors for simplicity of exposition.

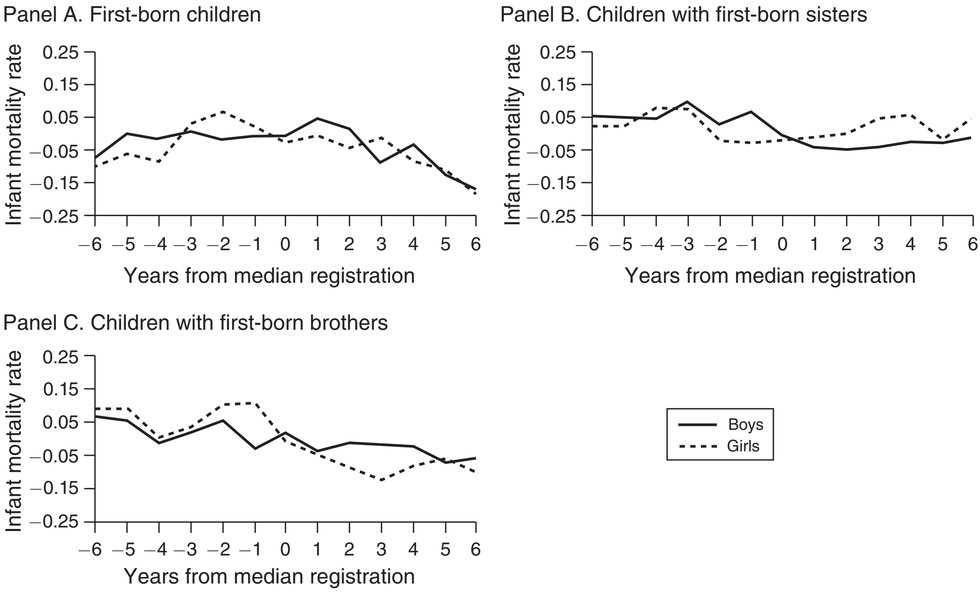

Figure 3. Infant Mortality of Hindu Children

Notes: The figure shows coefficient estimates from an annual event study of years before and after a district achieves median sharecropper registration. The covariates in the specification are the same as in (10).

trend. The estimated marginal effects are shown by the gender of the index child and of the firstborn child at the bottom of each column.[9] Like the regression coefficients, these are reported in comparison to the omitted group of girls with firstborn sisters who are unexposed to land reform. We calculate the statistical significance of the marginal effects using robust standard errors clustered at the district level, which likely overestimate significance levels due to the small number of clusters. However, the statistical significance levels of the interaction terms in the coefficient estimates are an accurate indication of differing reform impacts by the gender of the index child and the firstborn child, as these are calculated using the wild cluster bootstrap.

In the pooled sample, we find a statistically significant decline of 3.9–4.4 percentage points in infant mortality for boys in households without firstborn sons in columns 1–3, following above-median tenant registration, indicated by the coefficient on $R50_{k,t-1} \times male$. There are no such perceptible declines for girls without firstborn brothers, as the coefficient estimate on $R50_{k,t-1}$ is close to 0 and statistically insignificant. The post-reform decline in infant mortality for girls with firstborn brothers, indicated by the coefficient estimate for $R50_{k,t-1} \times firstson$, is also insignificant, but appears larger at 3.4–3.5 percentage points. The estimated coefficient for $R50_{k,t-1} \times firstson \times male$ suggests that the mortality decline for boys with firstborn brothers is smaller than that for girls with firstborn brothers, but is not statistically significant.

[9] Online Appendix Table A.4 reports corresponding results with cubic and quartic productivity controls. The estimates and marginal effects turn out to be robust to these controls, and are in fact often larger and more strongly significant for both the Hindu and non-Hindu samples.

Table 4—NFHS: Infant Mortality

	Infant death								
	All children			Hindu children			Non-Hindu children		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$R50_{t-1} \times firstson \times male$	0.050	0.067	0.066	0.068	0.093	0.093	0.016	0.016	0.019
	(0.033)	(0.041)	(0.041)	(0.038)	(0.049)	(0.051)	(0.044)	(0.056)	(0.053)
$R50_{t-1} \times male$	−0.044	−0.039	−0.039	−0.056	−0.063	−0.064	−0.018	0.011	0.008
	(0.020)	(0.023)	(0.022)	(0.022)	(0.030)	(0.029)	(0.039)	(0.048)	(0.048)
$R50_{t-1} \times firstson$	−0.034	−0.035	−0.035	−0.066	−0.060	−0.059	0.022	0.006	0.008
	(0.022)	(0.029)	(0.029)	(0.030)	(0.033)	(0.031)	(0.038)	(0.048)	(0.050)
$R50_{t-1}$	0.002	−0.003	−0.016	0.027	0.020	0.015	−0.054	−0.054	−0.082
	(0.016)	(0.019)	(0.019)	(0.016)	(0.016)	(0.019)	(0.032)	(0.038)	(0.046)
$firstson \times male$	−0.014	−0.002	−0.004	0.040	0.088	0.081	−0.102	−0.144	−0.134
	(0.143)	(0.162)	(0.163)	(0.172)	(0.194)	(0.199)	(0.256)	(0.268)	(0.258)
male	−0.170	−0.139	−0.137	−0.157	−0.149	−0.148	−0.177	−0.140	−0.136
	(0.152)	(0.135)	(0.139)	(0.141)	(0.126)	(0.130)	(0.265)	(0.271)	(0.258)
firstson	0.001	0.010	0.008	−0.069	−0.045	−0.047	0.109	0.100	0.091
	(0.097)	(0.102)	(0.106)	(0.149)	(0.139)	(0.139)	(0.268)	(0.281)	(0.271)
District fixed effects	X	X	X	X	X	X	X	X	X
District productivity		X	X		X	X		X	X
District-year trend			X			X			X
ME: Boys, first-born brother	−0.026	−0.011	−0.024	−0.027	−0.009	−0.016	−0.034	−0.021	−0.047
ME: Girls, first-born brother	−0.032	−0.039	−0.051	−0.039	−0.039	−0.045	−0.032	−0.047	−0.073
ME: Boys, first-born sister	−0.042	−0.043	−0.055	−0.028	−0.042	−0.049	−0.072	−0.043	−0.073
ME: Girls, first-born sister	0.002	−0.003	−0.016	0.027	0.020	0.015	−0.054	−0.054	−0.082
Observations	8,367	8,367	8,367	5,448	5,448	5,448	2,919	2,919	2,919
Pre-reform y mean	0.098	0.098	0.098	0.107	0.107	0.107	0.074	0.074	0.074
Cohorts	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91
Districts	14	14	14	14	14	14	14	14	14

Notes: NFHS data. y refers to the dependent variable. ME refers to marginal effect. Wild cluster bootstrapped standard errors are in parentheses. Samples include children of birth order two or higher. All specifications also include birth year fixed effects, birth order fixed effects, year of interview fixed effects, indicators for household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms of the mother's age at which the child is born. Lagged district covariates include logs of patta land area distributed, number of medical institutions, and kilometers of surfaced road per capita, and their two-way and three-way interactions with the male child and the first-born son indicators.

We find sharper evidence of gender-differentiated reform effects in the Hindu sample of households in columns 4–6. We confirm the results expected from the event study graphs: a statistically significant reduction of 6.0–6.6 percentage points in mortality rates among girls in Hindu families with a first son, and of 5.6–6.4 percentage points among later sons in Hindu families without a first son. Both these results are consistent with our model predictions, and are precisely estimated as significantly different from the effects for other firstborn and index child combinations (later sons with first son, and later girls without a first son). In fact, for these other combinations, we fail to find a persistently significant effect. The estimated mortality declines for girls with firstborn brothers, and boys without firstborn brothers are robust to the successive inclusion of district fixed effects, rice productivity controls, and district-specific linear time trends, and are significantly larger and more precisely estimated than those for the pooled sample in columns 1–3.

The richest specification in column 6 yields a lower bound estimate for the male-biased inheritance effect π of 3.4 percentage points among Hindu households; a large effect compared to the pre-reform infant mortality rate of 10.7 percentage points, but statistically insignificant. Among non-Hindu families in columns 7–9, we estimate sizable reductions in mortality rates for all combinations in the columns 7–9, albeit imprecisely. Hence, the patterns appear to be dissimilar between Hindu and non-Hindu families: in the latter, higher birth order children appear to experience mortality reductions even when the firstborn has the same gender. However, owing to the small size of the non-Hindu sample, we do not find statistical significance in these differences.

A specification that incorporates mother fixed effects in Appendix Table A.5 also produces broadly similar estimates to those in this table. The coefficient estimates for the productivity controls are reported in online Appendix Table A.6. Online Appendix Table A.7 shows that these results are driven largely by children of birth order 3 or higher.

Every district in West Bengal experienced land reform, so the preceding results capture impacts of varying progression of tenancy reform across districts. We tested robustness to have a strict control group in which no tenants were registered, by introducing into the sample all districts of the neighboring state of Bihar that are contiguous to West Bengal. The controls are as before (except for district-level infrastructure and healthcare measures, which are unavailable for Bihar), and include district-specific trends. These results are shown in Online Appendix Table A.8; the estimates are essentially unchanged. Notably, our estimates in Table 4 also remain essentially unaltered if controls for district rice productivity are dropped. This suggests that PE rather than GE effects were the primary source of intensified preference for boys in Hindu households with a firstborn daughter, barring imprecision in the productivity measures.

B. *Results for Sex Ratio at Birth*

We now show impacts of land reform on sex ratio at birth in Table 5. The estimates are reported conditional on district fixed effects, district rice productivity controls, and district-level linear time trends, and are robust to the inclusion of all of these.[10] In column 1, we find no impact of land reforms on the probability of the firstborn child being male. The same is true for the Hindu and non-Hindu subsamples of firstborn children in columns 4 and 7. Column 2, however, shows that for higher order births in the pooled sample, there is a statistically significant increase of 3.8 percentage points in male births following above-median sharecropper registration. The magnitude of this effect in column 3 is unaffected by the gender of the firstborn child. Columns 5 and 6 show that the male-biased reform impacts on the sex ratio are driven by Hindu families, consistent with the previous literature. The impact of reform on the probability of higher order births being male rises to a statistically significant 4.5 percentage points among Hindus in column 5, and a larger 5.1 percentage points in column 6, when the firstborn child is a girl. These effects are large, compared to the pre-reform mean of 49.3 percentage points. We find no such evidence of increased male bias in child sex ratios in non-Hindu families following

[10] We verify that the reform did not affect the sex ratio at birth among firstborns, and also show that the sex ratio results are robust to the inclusion of cubic and quartic productivity controls in Appendix Table A.9. Coefficient estimates for the productivity controls are reported in Appendix Table A.10.

Table 5—NFHS: Sex Ratio at Birth

	Child is male								
	All children			Hindu children			Non-Hindu children		
	Birth order 1 (1)	Birth order > 1 (2)	Birth order > 1 (3)	Birth order 1 (4)	Birth order > 1 (5)	Birth order > 1 (6)	Birth order 1 (7)	Birth order > 1 (8)	Birth order > 1 (9)
$R50_{t-1} \times$ *firstson*	—	—	−0.012	—	—	−0.012	—	—	−0.005
			(0.022)			(0.024)			(0.047)
$R50_{t-1}$	−0.019	0.034	0.040	−0.071	0.045	0.051	0.109	0.031	0.033
	(0.036)	(0.020)	(0.027)	(0.050)	(0.022)	(0.028)	(0.061)	(0.032)	(0.042)
firstson	—	−0.007	0.091	—	−0.009	0.188	—	−0.006	−0.017
		(0.008)	(0.089)		(0.010)	(0.117)		(0.017)	(0.223)
District fixed effects	X	X	X	X	X	X	X	X	X
District productivity	X	X	X	X	X	X	X	X	X
District-year trend	X	X	X	X	X	X	X	X	X
Observations	3,248	8,367	8,367	2,323	5,448	5,448	925	2,919	2,919
Pre-reform y mean	0.449	0.494	0.494	0.433	0.493	0.493	0.488	0.498	0.498
Cohorts	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91
Districts	14	14	14	14	14	14	14	14	14

Notes: NFHS data. y refers to the dependent variable. Wild cluster bootstrapped standard errors are in parentheses. All specifications also include birth year fixed effects, birth order fixed effects, year of interview fixed effects, indicators for household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms of the mother's age at which the child is born. Lagged district covariates include logs of rice yield, patta land area distributed, number of medical institutions, and kilometers of surfaced road per capita and their corresponding interactions with the male child and the first-born son indicators.

land reform in columns 8 and 9, though again this may owe to the imprecision of the estimates associated with the smaller size of the non-Hindu sample.

C. *Results for Son-Biased Fertility Stopping*

Table 6 shows the estimated effects of reform on the probability of a child of birth order 2 having a younger sibling. Again, columns 1–3 show results from the pooled sample, and results for the Hindu and Non-Hindu subsamples are in columns 4–6 and 7–9, respectively. Hence, fertility effects and associated intra-household resource effects cannot account for the observed patterns on infant mortality. The estimates are stable across specifications with successively richer controls.[11]

There are two relevant patterns. First, there is little evidence of son-biased fertility stopping in response to above-median land reform, as the $R50_{kt-1} \times$ *firstson* coefficient estimates are statistically insignificant in all the columns, indicating that our results for infant mortality and the sex ratio are minimally influenced by fertility responses to reform.[12]

[11] Appendix Table A.11 shows that these results are robust to the inclusion of cubic and quartic productivity controls, and that there are no son-biased fertility stopping effects at birth order 1. Appendix Table A.12 shows that the results are also robust to the inclusion of bordering control districts in Bihar. Coefficient estimates of productivity are reported in Appendix Table A.13. Appendix Table A.14 shows these are driven largely by children of birth order 2.

[12] Including the 25 percent registration indicator $R25_{k,t-1}$ and its corresponding interaction terms alongside the $R50_{kt-1}$ terms in (10) changes none of the infant mortality and sex ratio results, with the latter terms for above-median registration still attracting all the large, statistically significant coefficient estimates.

TABLE 6—NFHS: SON-BIASED FERTILITY STOPPING

	Child has a younger sibling								
	All children			Hindu children			Non-Hindu children		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$R50_{t-1} \times$ *firstson*	−0.007	0.009	0.012	−0.011	0.003	0.008	0.011	0.020	0.019
	(0.046)	(0.044)	(0.045)	(0.051)	(0.053)	(0.049)	(0.056)	(0.051)	(0.050)
$R25_{t-1} \times$ *firstson*	−0.118	−0.114	−0.116	−0.106	−0.104	−0.108	−0.179	−0.179	−0.182
	(0.051)	(0.052)	(0.054)	(0.051)	(0.048)	(0.051)	(0.075)	(0.082)	(0.084)
$R50_{t-1}$	−0.048	−0.048	−0.048	−0.035	−0.035	−0.038	−0.087	−0.087	−0.096
	(0.027)	(0.027)	(0.030)	(0.033)	(0.033)	(0.039)	(0.038)	(0.037)	(0.050)
$R25_{t-1}$	0.022	−0.001	−0.020	0.016	−0.008	−0.058	0.010	0.000	0.036
	(0.040)	(0.041)	(0.056)	(0.057)	(0.065)	(0.079)	(0.062)	(0.061)	(0.082)
firstson	−0.315	−0.223	−0.201	−0.436	−0.343	−0.347	0.108	0.150	0.205
	(0.210)	(0.193)	(0.193)	(0.265)	(0.247)	(0.262)	(0.131)	(0.114)	(0.129)
District fixed effets	X	X	X	X	X	X	X	X	X
District productivity		X	X		X	X		X	X
District-year trend			X			X			X
Observations	2,686	2,686	2,686	1,919	1,919	1,919	767	767	767
Pre-reform y mean	0.839	0.839	0.839	0.808	0.808	0.808	0.952	0.952	0.952
Cohorts	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91	1978–91
Districts	14	14	14	14	14	14	14	14	14

Notes: NFHS data. y refers to the dependent variable. Wild cluster bootstrapped standard errors are in parentheses. The sample in every column is children of birth order two only. All specifications include birth year fixed effects, year of interview fixed effects, indicators for household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms of the mother's age at which the child is born. Lagged district covariates include logs of patta land area distributed, number of medical institutions, and kilometers of surfaced road per capita and their corresponding interactions with the male child and the first-born son indicators.

Second, the tendency is for land reform to lower the probability of transition to a third birth at 25 percent tenant registration among Hindu families with firstborn sons and all non-Hindu families, but not in Hindu families with firstborn daughters. Among Hindus with a first son, the probability of a third birth declines by a statistically significant 10.8 percentage points (13.4 percent of the mean pre-reform probability) once district registration exceeds 25 percent (and there is no further reduction at 50 percent coverage). There are no perceptible effects on fertility stopping after the second birth if the first child is a daughter, consistent with these families continuing fertility to achieve a son. This ties in with a previous literature showing that fertility stopping rules are sensitive to the sex of previous births, with families tending to continue fertility till they have achieved the desired sex composition of births (e.g., Rosenblum 2013). First-son families are smaller at baseline because of underlying son-biased fertility stopping. Among non-Hindus, we see no evidence that land reform leads to changes in the sex ratio at birth or after, but we see similar son-biased fertility stopping behavior. This is consistent with previous research which shows that Muslim households (which dominate the non-Hindu sample) exhibit a preference for sons by continuing fertility to achieve them. In fact non-Hindus exhibit a greater decline in fertility, consistent with their higher baseline levels of fertility, and this is irrespective of the gender of the first child. At 25 percent coverage, the decline is, as for Hindus, restricted to first-son families, and as large as 18.2 percentage points (19 percent of the mean).

Table 7—NFHS: Test of Targeted Registration, Infant Mortality

	Infant death								
	All children			Hindu children			Non-Hindu children		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treated × *trend*	−0.002	0.002	0.005	0.000	0.004	0.005	−0.009	−0.006	−0.003
	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.010)	(0.008)	(0.009)
treated × *trend* × *male*	—	0.007	−0.006	—	−0.008	−0.008	—	−0.006	−0.001
		(0.008)	(0.008)		(0.009)	(0.011)		(0.009)	(0.009)
treated × *trend* × *firstson* × *male*	—	—	−0.003	—	—	−0.001	—	—	−0.007
			(0.006)			(0.006)			(0.007)
District fixed effects	X	X	X	X	X	X	X	X	X
Observations	3,389	3,389	3,389	2,428	2,428	2,428	961	961	961
Cohorts	1958–77	1958–77	1958–77	1958–77	1958–77	1958–77	1958–77	1958–77	1958–77
Districts	14	14	14	14	14	14	14	14	14

Notes: NFHS data. Wild cluster bootstrapped standard errors are in parentheses. Samples include children of birth order two or higher. All specifications also include the female child and first-born son indicators and their three-way and two-way interactions with the trend and treatment indicator, birth year fixed effects, indicators for household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms of the mother's age at which the child is born.

Once coverage reaches 50 percent, there is further fertility decline of 9.6 percentage points.[13]

D. *Test for Targeting of Sharecropper Registration*

Estimates of (11) for infant mortality are presented in Table 7 and estimates of the same equation for the probability of a younger sibling and the probability of a male birth are in Table 8. We find no statistically significant correlations in any of the three samples of children between the pre-reform trend in infant mortality and the intensity of registration in the district in 1985, by either the gender of the child or the firstborn sibling. The coefficients are all also nearly identical to zero.

V. Results from VHPS Data

We now present results from estimating (12) using the VHPS dataset. As explained previously, as we do not observe full birth histories or exact dates of child deaths in these data, we instead model as outcomes the probability of birth of a female or male child that survived till 2004 in response to land registered under Operation Barga. On the other hand, the data includes details on landholdings at the household level, allowing us to examine land reform effects for different land classes. We use pre-reform landholdings in 1977, and classify households into four categories: landless, marginal, small, and large. The land reform measure is different from NFHS, an indicator for the village crossing a threshold corresponding to the median of the village-year distribution of proportion of cultivable land that was registered.

[13] These results are robust to the inclusion of children born in border districts in the neighboring state of Bihar; see Appendix Table A.7.

Table 8—NFHS: Test of Targeted Registration, Male Births and Fertility

	All children		Hindu children		Non-Hindu children	
	Male child (1)	Younger sibling (2)	Male child (3)	Younger sibling (4)	Male child (5)	Younger sibling (6)
treated × *trend*	0.001	0.001	0.001	0.001	0.002	0.002
	(0.005)	(0.004)	(0.005)	(0.005)	(0.008)	(0.004)
treated × *trend* × *firstson*	—	−0.009	—	−0.012	—	0.003
		(0.007)		(0.008)		(0.008)
District fixed effects	X	X	X	X	X	X
Observations	3,389	1,369	2,428	1,015	961	354
Cohorts	1958–77	1958–77	1958–77	1958–77	1958–77	1958–77
Districts	14	14	14	14	14	14

Notes: NFHS data. Wild cluster bootstrapped standard errors are in parentheses. Samples for the sex ratio regressions include children of birth order two or higher, and of birth order two for the fertility regressions. The specifications for the probability of having a younger sibling also include the first-born son indicator and its interaction with the trend and treatment indicator, birth year fixed effects, indicators for household religion and caste, whether the household is rural, mother's educational attainment, and linear and quadratic terms of the mother's age at which the child is born.

Table 9 presents results for the full sample of households for the period 1978–1998, for non-firstborn female and male children, respectively. In each case, we first provide the results for the entire sample, and then the following four columns present the corresponding results for the four land categories. In columns 1–5, we see no significant impacts of the land reform on birth of surviving girls, irrespective of the gender of the firstborn, for the entire sample as well as for each land category. In contrast, column 6 shows significant positive effects of land reform on the birth of surviving boys when the firstborn was a girl, driven largely by landless households and small landowning households in columns 7 and 9, respectively. These effects vanish when the firstborn is a boy. The lower bound estimate of male-biased inheritance effect π implied by these estimates in column 6 is of the order of 4.9 percentage points, statistically significant at the 1 percent level. These results are consistent with those we find in the NFHS data, i.e., an effect of the land reform crossing the median threshold on the sex ratio at birth, as well as male infant survival probability differences by gender of the firstborn child. The reform impact on the sex ratio unambiguously improved the probability of male births in the NFHS data, but the positive reform impact on infant survival probability for later born male children only manifested when the firstborn sibling was a girl. The results for the probability of observing a surviving male child in the VHPS data mirror these patterns.

We find that the positive effect of reform on the probability of observing surviving boys was driven mainly by landless and small landowning households. The effects were larger for the latter group, who were less numerous than the former. This is consistent with our theoretical expectations, wherein the effect for the landless is driven principally by the PE effect, while that for the small landowners is driven by a combination of GE and PE effects. The estimated effects for marginal landowners in column 8 is quantitatively close to those for the landless, but statistically

Table 9—VHPS: Pooled Sample, by Gender of First Child

	Female surviving child					Male surviving child				
	All	Landless	Marginal	Small	Large	All	Landless	Marginal	Small	Large
Land category:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agricultural land	−0.003	—	−0.006	0.007	−0.004	−0.001	—	−0.002	0.005	−0.000
	(0.002)		(0.007)	(0.008)	(0.002)	(0.001)		(0.007)	(0.008)	(0.002)
LR	−0.000	−0.003	0.016	−0.036	0.005	0.051	0.044	0.046	0.101	0.021
	(0.016)	(0.025)	(0.029)	(0.053)	(0.023)	(0.016)	(0.025)	(0.030)	(0.049)	(0.031)
firstson	0.003	0.007	−0.026	−0.043	0.029	−0.211	−0.228	−0.193	−0.125	−0.230
	(0.011)	(0.019)	(0.026)	(0.052)	(0.018)	(0.015)	(0.023)	(0.032)	(0.049)	(0.028)
LR × *firstson*	0.007	0.011	0.004	0.026	−0.002	−0.049	−0.052	−0.048	−0.113	−0.005
	(0.016)	(0.024)	(0.034)	(0.053)	(0.023)	(0.017)	(0.025)	(0.030)	(0.051)	(0.032)
Observations	24,696	10,213	4,155	2,497	7,789	24,696	10,213	4,155	2,497	7,789
Households	1,946	974	317	173	480	1,946	974	317	173	480

Notes: Village panel survey data 1978–1998, children of birth order two or above. Robust standard errors are clustered by village in parentheses. *LR* indicates above-median registration of the cumulative share of village cultivable land by district-year. Controls include a land ceiling indicator, year and household fixed effects, and cumulative village land distributed. Household land ownership category is defined by landholdings in 1977.

insignificant. The LR effects are smaller for large landowners in column 10 than for small landowners, possibly because of an adverse PE effect which neutralized a weak GE effect.

Table 10 shows results from estimating (12) on the Hindu subsample of households. Again, we find no LR impacts on the probability of observing a surviving girl child across households in any landholding category in columns 1–5. However, in column 6, we find a positive, statistically significant increase of 3.7 percentage points in the probability of observing a male surviving child in a Hindu household without a first son. This effect vanishes for corresponding families with a first son. The implied estimate of the male-biased inheritance effect is 4.0 percentage points, statistically significant at the 5 percent level. While the estimates in the subsamples further split by landholding category in columns 7–10 are largely statistically insignificant due to a smaller number of observations than in Table 9, the coefficient estimates in column 8 again indicate that the effects are driven by landless and small landowners. The estimate of the inheritance effect is significant at the 5 percent level for the landless.

Table 11 shows corresponding results for the non-Hindu subsample. We find a statistically significant increase of 10.6 percentage points in the probability of observing a surviving male child among households without a first son in column 6, but no such effects for girls in columns 1–5. This is consistent with the evidence for son-biased fertility stopping among non-Hindus in Table 6. This effect appears to be partially offset if the firstborn child is male, as in the pooled sample and in the Hindu subsample. However, the interaction term of the firstborn son indicator with the above-median reform indicator is statistically insignificant, preventing us from stating this with much confidence. The samples divided further by land category across columns 7–10 do not yield meaningful results, possibly due to the small number of observations in each regression.

Finally, Table 12 shows how results for the full sample are affected when we control for district-year fixed effects, which absorb any district-specific shocks to

TABLE 10—VHPS: HINDU SAMPLE, BY GENDER OF FIRST CHILD

	Female surviving child					Male surviving child				
	All	Landless	Marginal	Small	Large	All	Landless	Marginal	Small	Large
Land category:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agricultural land	−0.002	—	−0.011	0.009	−0.003	−0.002	—	−0.003	−0.002	−0.000
	(0.002)		(0.008)	(0.009)	(0.002)	(0.002)		(0.008)	(0.007)	(0.002)
LR	0.001	0.016	0.020	−0.060	−0.011	0.037	0.038	0.037	0.082	−0.007
	(0.019)	(0.025)	(0.039)	(0.067)	(0.025)	(0.017)	(0.029)	(0.030)	(0.059)	(0.022)
firstson	−0.006	0.002	−0.010	−0.061	0.003	−0.225	−0.235	−0.213	−0.171	−0.231
	(0.012)	(0.016)	(0.031)	(0.056)	(0.024)	(0.014)	(0.021)	(0.035)	(0.050)	(0.026)
LR × *firstson*	0.002	−0.008	−0.001	0.046	0.007	−0.040	−0.051	−0.050	−0.097	0.019
	(0.018)	(0.025)	(0.041)	(0.065)	(0.024)	(0.016)	(0.029)	(0.031)	(0.060)	(0.025)
Observations	20,274	8,473	3,288	2,148	6,323	20,274	8,473	3,288	2,148	6,323
Households	1,571	808	239	144	378	1,571	808	239	144	378

Notes: Village panel survey data 1978–1998, children of birth order two or above. Robust standard errors clustered by village are in parentheses. *LR* indicates above-median registration of the cumulative share of village cultivable land by district-year. Controls include a land ceiling indicator, year and household fixed effects, and cumulative village land distributed. Household land ownership category is defined by landholdings in 1977.

TABLE 11—VHPS: NON-HINDU SAMPLE, BY GENDER OF FIRST CHILD

	Female surviving child					Male surviving child				
	All	Landless	Marginal	Small	Large	All	Landless	Marginal	Small	Large
Land category:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agricultural land	−0.008	—	−0.005	−0.033	−0.008	0.005	—	−0.001	0.065	0.001
	(0.003)		(0.019)	(0.029)	(0.003)	(0.004)		(0.022)	(0.013)	(0.004)
LR	−0.005	−0.041	−0.014	0.006	0.018	0.106	0.085	0.069	0.146	0.180
	(0.026)	(0.068)	(0.045)	(0.069)	(0.062)	(0.042)	(0.064)	(0.055)	(0.146)	(0.119)
firstson	0.029	0.046	−0.066	0.095	0.076	−0.159	−0.175	−0.170	−0.031	−0.216
	(0.027)	(0.087)	(0.045)	(0.129)	(0.034)	(0.041)	(0.108)	(0.048)	(0.198)	(0.045)
LR × *firstson*	0.025	0.055	0.035	−0.010	0.017	−0.076	−0.080	−0.042	−0.129	−0.138
	(0.029)	(0.062)	(0.070)	(0.072)	(0.054)	(0.045)	(0.064)	(0.053)	(0.168)	(0.116)
Observations	4,422	1,740	867	349	1,466	4,422	1,740	867	349	1,466
Households	375	166	78	29	102	375	166	78	29	102

Notes: Village panel survey data 1978–1998, children of birth order two or above. Robust standard errors clustered by village are in parentheses. *LR* indicates above-median registration of the cumulative share of village cultivable land by district-year. Controls include a land ceiling indicator, year and household fixed effects, and cumulative village land distributed. Household land ownership category is defined by landholdings in 1977.

rice productivity or other factors that could otherwise affect child survival.[14] The addition of these fixed effects does very little to change the estimated coefficients from those in Table 9.

[14] We do not use estimates of farm productivity used in Bardhan and Mookherjee (2011) because this would have unduly restricted the number of years of data used in the regression (as farm productivity estimates are available only between 1982 and 1996, for between three and four years for each village).

Table 12—VHPS: Pooled Sample, by Gender of First Child, with District-Year Fixed Effects

	Female surviving child					Male surviving child				
	All	Landless	Marginal	Small	Large	All	Landless	Marginal	Small	Large
Land category:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agricultural land	−0.003	—	−0.009	0.001	−0.003	−0.000	—	0.004	0.006	0.000
	(0.002)		(0.008)	(0.008)	(0.002)	(0.001)		(0.009)	(0.008)	(0.002)
LR	−0.004	−0.008	0.020	0.013	0.021	0.060	0.058	0.036	0.073	0.056
	(0.017)	(0.027)	(0.036)	(0.069)	(0.028)	(0.017)	(0.025)	(0.031)	(0.049)	(0.036)
firstson	0.004	0.007	−0.023	0.007	0.034	−0.213	−0.225	−0.206	−0.134	−0.215
	(0.012)	(0.020)	(0.029)	(0.058)	(0.019)	(0.016)	(0.023)	(0.034)	(0.057)	(0.027)
LR × *firstson*	0.005	0.018	0.002	−0.062	−0.014	−0.046	−0.053	−0.042	−0.098	−0.018
	(0.016)	(0.027)	(0.036)	(0.066)	(0.027)	(0.017)	(0.028)	(0.033)	(0.049)	(0.034)
Observations	24,696	10,213	4,155	2,497	7,789	24,696	10,213	4,155	2,497	7,789
Households	1,946	974	317	173	480	1,946	974	317	173	480

Notes: Village panel survey data 1978–1998, children of birth order two or above. Robust standard errors clustered by village are in parentheses. *LR* indicates above-median registration of the cumulative share of village cultivable land by district-year. Controls include a land ceiling indicator, year, household and year × district fixed effects, and cumulative village land distributed. Household land ownership category is defined by landholdings in 1977.

VI. Conclusions

We find that increased property rights security exacerbates gender discrimination in Hindu families, with parents manipulating sex ratios at birth and after birth until the age of one, so as to increase the chances of survival of at least one son to inherit the family property. Land reform is also associated with greater son-biased fertility-stopping, widening sibship size differences between first-son and first-daughter families. This is in contrast to evidence from other settings that land reform alters existing gender-unequal institutions in favor of women, for instance, tenure regularization is argued to have significantly improve women's tenurial and inheritance claims to land in Rwanda (Ali, Deininger, and Goldstein 2014), and joint spousal titling increased women's intra-household bargaining power in Peru (Wiig 2013). Male-biased inheritance law in India appears to have resulted in very different outcomes.

The pattern of our results increases confidence that our findings are driven by male-biased inheritance effects, rather than by wealth effects of land reform. We find that land reform has opposite effects in families with first-sons versus first-daughters. In particular, if as in the China study of Almond, Li, and Zhang (2013), wealth effects of the reform raised the demand for sons, we would not expect to see a smaller increase in male child survival in families with firstborn sons. An alternative explanation of land reform strengthening the desire to have sons is that it raises the returns to labor, and males are more likely to be employed as farm labor. Using detailed farm-level data gathered alongside the West Bengal village survey data, we estimated whether the ratio of male to female labor was modified by land reform and find no evidence that it was.[15] We cannot, however, rule out the possibility that a greater share of males among farm labor at baseline drives some of the identified

[15] These results are available upon request.

effects of land reform on son preference. Even if this was the case, this channel is closely related to our preferred explanation based on male-biased inheritance patterns. As elucidated in the introduction, the labor supply of sons on family farms and their inheritance rights are closely tied: patrilocality involves married sons co-residing with or living very close to their parents, while married daughters marry some distance away from the natal home, so that it is primarily sons who work on family land and subsequently inherit it.

Our results on fertility-stopping further increase confidence that we are capturing male-biased inheritance effects of reform and not wealth effects. Almond, Li, and Zhang (2013) finds a small positive effect of land reform on fertility (after controlling for the negative effects of the One Child Policy), which is consistent with income effects being a dominant mechanism in China because income tends to raise fertility in low-income settings (Currie and Schwandt 2014; Vogl 2013, Bhalotra and Rocha 2013). In contrast, we find a negative effect of land reform on fertility. In the non-Hindu sample, where the sex ratio of births appears not to be manipulated by parents, we find across-the-board reductions in fertility after land reform. In the Hindu sample, fertility reduction is restricted to families that have a first-son, while first-daughter families continue fertility to achieve a son.

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