Land Reform and Sex Selection in China

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China's land reform in 1978–84 unleashed rapid growth in farm output and household income. In new data on reform timing in 914 counties, we find an immediate trend break in the fraction of male children following the reform. Among second births that followed a firstborn girl, sex ratios increased from 1.1 to 1.3 boys per girl in the 4 years following reform. Larger increases are found among families with more education. The land reform estimate is robust to controlling for the county-level rollout of the One Child Policy. Overall, we estimate land reform accounted for about 1 million missing girls.

I. Introduction

Economic development has narrowed gender gaps over the past quarter century, including those in educational attainment, life expectancy, and labor force participation (World Bank 2012). Nevertheless, perhaps the

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FIG. 1.—Sex ratios, GDP, and fertility in China: 1970–2000. *A*, Sex ratio and GDP. *B*, Fertility. Color version available as an online enhancement.

starkest manifestation of gender inequality—the "missing women" phenomenon—can persist with development (Das Gupta et al. 2003; Duflo 2012). Figure 1*A* shows its evolution in China. Despite rapid GDP growth since 1980, the sex ratio at birth increased from 1.06 in 1978 to 1.20 in 2000. In 2010, the sex ratio remained 1.19, or about 500,000 more male births per year than the biological norm of around 1.05 males per female.[1] Previous research on this perverse effect of economic development has focused on two mechanisms: reductions in the cost of sex selection (e.g., ultrasound diffusion in Chen, Li, and Meng [2013]) and reduced fertility (e.g., Jayachandran 2017). Surprisingly, China's sex ratios started to rise in 1980, when costs of sex selection were high and, moreover, when fertility was relatively flat (fig. 1*B*).[2] Why?

We propose a new factor affecting sex selection: 1978–84 land reform in rural China (then home to 86 percent of the Chinese population). Introduction of the Household Responsibility System unraveled collectivized agriculture and marked a critical first step toward a market-based economy. The reform granted land usership rights to individual households on a long-term basis, while land ownership remained with the col-

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[1] Data on GDP per capita (current US dollars) are from the World Bank. Sex ratios are aggregated from microdata of the 1982, 1990, and 2000 censuses.

[2] Data on the total fertility rate are from Cai (2008).

lective. It is well documented that land reform spurred remarkable growth in agricultural output (McMillan, Whalley, and Zhu 1989; Lin 1992) and lifted hundreds of millions of rural households out of poverty (World Bank 2000).[3] The main empirical challenge is to disentangle the effect of land reform from that of the One Child Policy (OCP), which started around the same time.

We analyze new data from primary sources, including county records on land reform adoption in 914 counties, covering half of the rural population, and merge these with the 1990 population census microdata. Previous research focused on the rollout to 28 provinces (Lin 1992). We also collect previously unanalyzed data on the county-by-county rollout of the OCP during 1979–83. There is substantial variation in the timing of both land reform and the OCP at the county level, which enables us to evaluate both policies and capture their interactions. Our focus on the local rollout of signature national policies parallels recent work on US counties (e.g., Hoynes, Schanzenbach, and Almond 2016; Isen, Rossin-Slater, and Walker 2017).

Our empirical strategy is based on a demographic regularity: the sex ratio of the first child is biologically normal but becomes abnormally male biased at higher birth orders, especially among Chinese families with no previous son (Zeng et al. 1993). We find a clear increase in the fraction male for second children in families without a firstborn son. Prior to land reform, we do not see trends in this sex ratio. Nor do we see substantial increases in sex ratios following land reform for either the firstborn child or the second child if the first child was male. These raw patterns are replicated in our regression estimates that remove unrestricted county by year fixed effects. We find a precisely estimated trend break of 0.7 percentage points per year in the fraction of males following a first girl after land reform, which accumulates to a 2.8 percentage point increase 4 years after reform (isomorphic to the raw sex ratio rising from 1.1 before reform to 1.3 after reform).

We provide empirical evidence on potential confounders. These must mimic the rollout of land reform by county and differentially affect families with the first child being a daughter versus a first son. From our systematic review of reform policies, candidate confounders do not adhere to this precisely prescribed pattern with one potential exception: the OCP. We find robust results for land reform. Estimates on the land reform effect are undiminished when the county-level OCP rollout is controlled for. Second, land reform increased sex ratios before the introduction of the rural OCP. We also find that the number of second births is not

[3] Using time-series data, McMillan et al. (1989) suggest that over three-quarters of the productivity increase in 1978–84 could be attributed to land reform. Using provincial reform rollout, Lin (1992) finds that the reform accounts for half of realized output growth.

affected by land reform conditional on the OCP, reducing concerns about potential changes in the composition of families caused by land reform.

There is no shortage of explanations for why land reform may have increased sex selection. A priori, we view two mechanisms as particularly plausible: increased income (wage and/or nonlabor income of parents) and a greater productivity benefit of sons. We formalize these mechanisms in online appendix A.[4] Empirically, we find more consistency with the income mechanism. The sex selection response was concentrated among parents with more education and in counties with higher income growth after reform. Within the subset of "productive son" models, those that reward a second son would predict sex selection for first births as well as for second births with an elder brother. However, these predictions are not supported by the empirical evidence. Moreover, in areas/crops in which males are more productive, productive son models predict a larger sex selection response to land reform. Using previously unanalyzed occupation and industry codes from the 1982 census, we do not find this predicted heterogeneity. We discuss five additional hypotheses and likewise do not find supporting evidence. That said, we are more circumspect in interpreting empirical evidence on mechanisms than our reduced form because our mechanism measures are relatively crude.

Finally, how did land reform increase sex selection? Parents might prefer to conceal sex selection behaviors, and as such, detecting them is "forensic economics" (Zitzewitz 2012). As in previous studies, we unfortunately do not have a direct measure of sex selection. But sex ratios have a natural benchmark (1.05), and ultrasound technology was introduced in China's provincial capitals during the 1970s, permitting noninvasive prenatal sex determination. Combining data on ultrasound machine diffusion to provincial capitals collected by Chen et al. (2013) with the 1980 rail network data provided by Matthew Turner, we find larger increases in sex ratios in rural counties with railroad connections to provincial capitals where ultrasound machines were available. We also find increased sex selection due to land reform in places where ultrasound was unavailable, possibly through greater postnatal selection. A common feature of sex selection in both contexts is that parents find it costly.

The role of land reform in sex selection has gone unrecognized for more than 30 years, we suspect, because county-level data on land reform timing were unavailable. Effects of China's economic watershed are interesting per se and suggest a perverse effect of economic development. The persistence of sex selection among Asians in the West (Dubuc and Coleman

[4] We consider the response of sex selection and fertility to either increased income or the increased opportunity cost of child rearing (wage) from land reform. A son increases parental utility but may also increase wages or income, i.e., a productivity channel.

2007; Almond and Edlund 2008) underscores that factors beyond parochial ones like the OCP or physical brawn help account for "missing girls."

II. Background

A. *The Post-Mao Land Reform*

From 1956 to 1977, China was a planned economy. In agriculture, prices were centrally controlled and fixed, trading in the market was prohibited, and production was organized collectively in production teams, making it difficult to monitor and reward individual effort. Unsurprisingly, China's grain output per capita stagnated during this period (Zweig 1987).

In 1978, 2 years after the death of Mao Zedong, the Chinese government initiated some fledgling, "top-down" reforms in an attempt to increase productivity. Party leaders reached consensus on three national interventions: raising the long-depressed state procurement prices for major crops, reducing grain procurement quotas, and opening interregional trade (Perkins 1988; Lin 1992).[5] More structural changes to agricultural production were considered too radical by Mao's designated successor, Hua Guofeng.

The more substantive agricultural reform of 1978–84 thus came about as a grassroots movement that broke with official policy. At the end of 1978, a small number of production teams in Anhui Province experimented with contracting land and assigning output quotas to individual households. A year later, these teams harvested yields far larger than other teams (Lin 1992). As the movement spread, increased agricultural output softened official resistance. The party's prohibition was relaxed in 1979 by allowing exceptions for poor regions. When Hua Guofeng was replaced by Zhao Ziyang in 1980 and Hu Yaobang in 1981, the reform gained acceptance and was rolled out more quickly. In January 1982, Central Document no. 1 officially announced that "the Household Responsibility System (HRS) is the production responsibility system of the socialist economy."

In essence, land reform allowed collectives to allocate an equal share of land to each individual; households could make input decisions and receive all residual income from land after meeting procurement obligations to the state (Perkins 1988). Land reform created variation across time and space (Lin 1992), which we will explore in the empirical analysis below.

[5] Decentralized price and market reforms did not come until 1985, when the central government announced that mandatory procurement quotas in agriculture were no longer permitted (Sicular 1988).

B. The One Child Policy in Rural Areas

China started the One Child Policy in 1979, and financial penalties were introduced to enforce it. While a strict one-child rule has been applied in urban areas since 1979, a second child was allowed in rural areas until 1984. Only a third or higher-parity child in rural areas was punished in the 1979–83 period (Banister 1987).[6] Fertility control was decentralized to local governments, and resistance from parents led to a gradual implementation of the policy (Scharping 2003). Overall in this period, penalties for above-quota births in rural areas were relatively mild. More substantial fines were imposed only later (Ebenstein 2010).

Fertility was higher following 1979 than commonly believed. As shown by figure 1*B*, well before the OCP, the total fertility rate (TFR) fell by nearly half from 1970 to 1977. It "bottomed out" at about 2.5 children around 1979, where it remained throughout the early OCP period until 1987. The macro trend suggests that the OCP played at most a modest role in affecting fertility during our study period.

III. Data

A. County-Level Reform Rollout

Data on county rollout of land reform and the OCP come from county gazetteers. Gazetteers are compiled by local historians to record local history and draw on materials in local archives. They were not used in evaluating local officials and, prima facie, are less susceptible to misreporting. To empirically gauge the quality of gazetteer data, we compare economic statistics in gazetteers to the commonly used statistics in yearbooks, on which cadre evaluation was based. Appendix Section B.1.1 reports results using gross production of grain as an example. First, we document substantial agreement between the two data sources. Second, we show that the gazetteer measures respond more to rainfall and soil quality than yearbook data. Third, we apply Benford's law in a manner suggested by Varian (1972) to detect fake data, where falsified digits tend to be made up uniformly. To summarize, we find that gazetteer data are similar to yearbook data but appear more accurate when they disagree.

We conducted a comprehensive review of all county gazetteers published to date. Our primary analysis sample includes the 914 counties (half of China's rural counties) that record precise timing of both land reform and the OCP. These reporting counties are very similar to other counties (app. table A.5).

[6] The rural OCP was changed in 1984 to allow a second child following a first girl, the so-called "1.5 Child" Policy (Greenhalgh 1986), which is unlikely a confounder to land reform because it came after land reform was completed. We discuss the 1.5 Child Policy in app. Sec. C.4.

The start of land reform is defined by the year when collectively owned land was first contracted to individual households in a few villages for each county; it usually took 2–3 years for it to spread to the entire county. We find in appendix Section B.1.2 that the pattern in the county-by-county reform rollout is consistent with both government policies discussed in Section I and the existing literature on the reform rollout by province.

We define the beginning of the OCP as the year when the county government issued the first policy document to enforce penalties for above-quota, third births. The county rollout data correspond to local implementation: we find that fertility reduction following the county policy indeed occurred at the third parity and above (app. table A.4). We also find that fertility responses to the policy rollout scale up to replicate national fertility trends.

In figure 2, the solid line represents the fraction of counties that started land reform between 1978 and 1984 and the dashed line shows the rollout of the OCP, both scaled by the vertical axis on the left. Despite similar timing in aggregate, land reform and the OCP show substantial differences in their county-level rollout. Land reform came earlier than the OCP in 27 percent of counties, in 25 percent they coincided, and in 48 percent the OCP came earlier (app. fig. A.3). At the county level, the correlation between HRS timing and OCP timing is −.005.

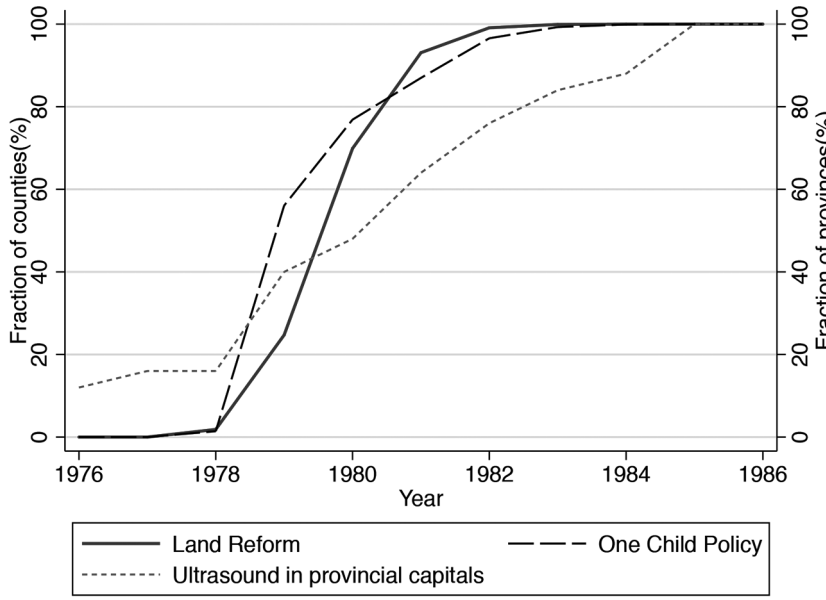

Fig. 2.—Reform rollout. Color version available as an online enhancement.

To confirm land reform's previously documented effect on agricultural productivity, we combine two variables in the gazetteers to calculate grain output per capita: annual gross production of grain divided by annual population by county. There are 415 counties that report both the reform timing and complete year-by-year grain production and population from the 1970s to the mid-1980s.[7] Because county statistics have been released systematically in yearbooks only since the 1980s in China, gazetteer data from the 1970s are particularly valuable in assessing the timing of income increases as they relate to land reform.

B. *Diffusion of Ultrasound Technology to Provincial Capitals*

Data on the diffusion of ultrasound are provided by Chen et al. (2013), also collected from county gazetteers. Just 4 percent of rural counties had ultrasound machines by 1982, when the rollout of land reform was nearly completed. Although prenatal sex determination was all but unavailable locally, it was accessible in certain provincial capitals. The first ultrasound machine arrived in Xi'an in Shaanxi Province in 1965. Other provincial capitals started to acquire their first machines in the late 1970s. In figure 2, the dotted line shows the rollout of ultrasound machines to the 30 provincial capitals (on the secondary vertical axis). During the rollout of land reform, one option for pregnant women (especially for those in rail-connected rural counties) was to travel to the provincial capital to ascertain fetal sex. In Section VII, we examine to what extent sex selection induced by land reform operates through ultrasound access in provincial capitals.

C. *Sex Ratios from Census Microdata*

To consider child sex ratios during the reform period, we use the 1 percent sample of the 1990 census microdata in 914 counties. We focus on individuals born in 1974–86, who were aged 4–16 in 1990.[8] Our main sample includes second children in all families with at least two children.[9] In our study period, 92 percent of women over age 35 had at least two children.

Because the Chinese census does not explicitly query one's birth order and sex of siblings, we use information on one's relationship to the

[7] Appendix table A.5 also shows that these counties with grain data are comparable to other counties.

[8] The National Bureau of Statistics reported that the child underreporting rate was 0.7 percent. While low, child underreporting is more common under age 4 in the census year (Zhang and Zhao 2006). Therefore, we focus on children older than 4. We also check the robustness of estimates by including children under age 4.

[9] We also check the robustness of estimates using all births at the second parity and above.

household head to identify the household head's children and order these children using their month and year of birth.[10] To verify that this order is complete, we require that the number of children linked to the household head is equal to the number of surviving births reported.[11] Even for the oldest cohort of second children in our sample, we observe the firstborn child in their family because he or she was still too young to leave home in rural China. Indeed, when we compare the distribution of birth year of first children in the 1990 census to that in the 1988 National Fertility Survey, in which parents report complete fertility histories, the two distributions are nearly identical (see app. fig. A.4).

An advantage of analyzing the 1990 census is that internal migration was under strict control and was not relaxed until the 1990s (Wang 2005). Although the Chinese census does not query county of birth, one's county of residence in 1990 maps very closely to county of birth. The migration rate is 0.63 percent among families with children in our analysis sample.[12] We exclude these migrants in our analysis.

Summary statistics are reported in appendix table A.6. The fraction of males among first births is stable at 0.51 before and after land reform. Among second births following a firstborn boy, the fraction of males remains about 0.5 before and after the reform. The most striking change is observed among second births following a firstborn girl: the fraction of males increases from 0.53 before land reform to 0.56 after the reform. Similarly among all births at the second parity and above, the fraction of males following no previous son increased by 4 percentage points after the reform, while the fraction is stable following at least one previous son.

IV. Empirical Strategy

A. *Event Studies*

We start by describing grain output and sex ratios before and after land reform (no regression adjustment). The relatively flat trend of grain output per capita prior to reform (before time 0) in figure 3 confirms sluggish productivity growth under the collective system. One year after the reform started, it turns sharply to an upward-sloping trend. We estimate the change in slope in appendix table A.15 (col. 1 of panel A) and find a

[10] Twins and triplets (2.3 percent of all births) are not analyzed because birth order is more difficult to identify and interpret.

[11] In our sample, 81 percent of women report the number of surviving births equal to the number of children observed in their family in the census. We also confirmed the robustness of estimates to including families that report a number of surviving children different from that observed in the family.

[12] In the 1990 census, a migrant respondent is defined as not residing in the same county in 1985.

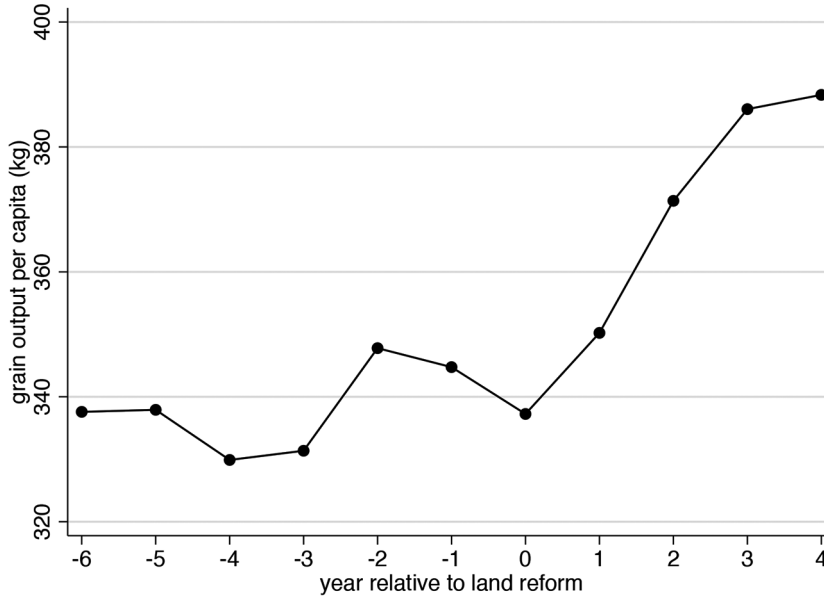

Fig. 3.—Event study of grain output per capita

3.8 percent increase in grain output per capita per year following reform. The change in slope in agricultural productivity is consistent with the gradual spread of the HRS within a county (recalling that our measure of land reform "turns on" when the first villages adopted the HRS).[13]

Figure 4*A* presents sex ratios of first births by year of birth relative to the start of land reform. The sex ratios are stable at the biologically normal rate of 1.05 before and after the reform, suggesting the absence of sex selection among first births.

Figure 4*B* shows sex ratios of second children in families with a first girl and those with a first boy before and after land reform. Importantly, there are no preexisting trends of second children's sex ratios in either families with a first boy or families with a first girl.[14] Moreover, among families with a first boy, little change in sex ratios of second births is observed after reform. In stark contrast, sex ratios following a first girl show sharp increases right after the reform, from 1.1 to 1.3 6 years after reform. The slope change in the sex ratio trend following a first girl is consistent with the timing of the slope change of grain output in figure 3: both departures were immediate but took several years to manifest fully.

[13] It is also possible that households gradually respond to the incentives under the HRS.

[14] Following a first boy, girls are slightly more common than the biological norm, as noted by Chen et al. (2015), who argue that girls were adopted by families with sons.

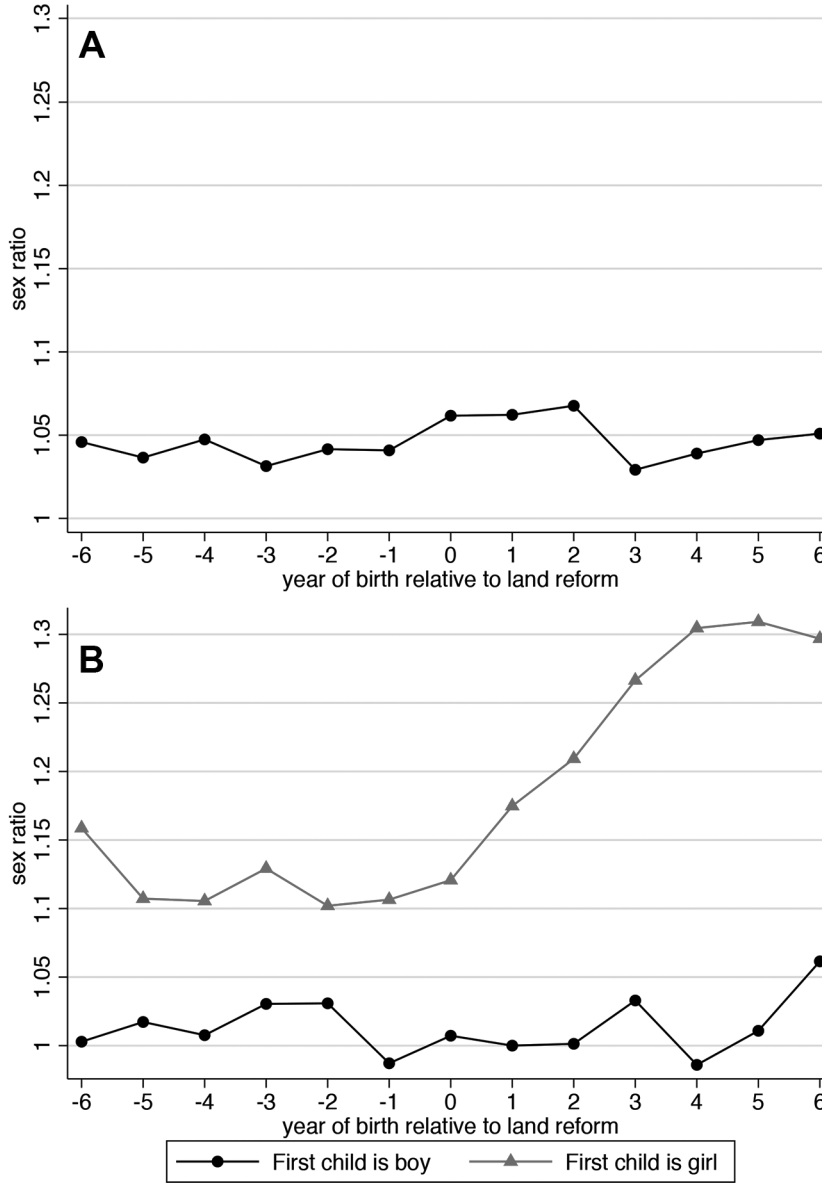

FIG. 4.—Event study of sex ratios. *A*, First children. *B*, Second children. Color version available as an online enhancement.

B. *Econometric Specification*

To assess the postreform slope change in sex ratios following a first girl, we estimate a trend break model:

$$y_{ijt} = \alpha + \beta_1 E_{jt} \times \text{GirlFirst}_{ijt} + \beta_2 \text{Reform}_{jt} \times E_{jt} \times \text{GirlFirst}_{ijt} + \beta_3 \text{GirlFirst}_{ijt} + \gamma_{jt} + \epsilon_{ijt}, \quad (1)$$

where i is family, j county of birth, and t year of birth; y_{ijt} is a dummy variable equal to one if the second child is male in family i; and GirlFirst_{ijt} is a dummy variable equal to one if the first birth is a girl. Event time E_{jt} is defined as birth year minus the reform year and is interacted with GirlFirst_{ijt}. The variable β_1 measures the average prereform trend in the fraction of males among second births following a first girl. Reform_{jt} is a dummy variable equal to one if one is born after the reform and is interacted with $E_{jt} \times \text{GirlFirst}_{ijt}$. The coefficient of interest, β_2, measures the average postreform slope change in the trend of the fraction of males following a first girl. The increase in the fraction of males k years after reform is $\beta_2 \times k$. All regressions include GirlFirst_{ijt}, as well as county-by-year fixed effects γ_{jt} to absorb time-varying county characteristics. The term γ_{jt} also absorbs event time E_{jt} and the reform indicator Reform_{jt} main effects. Standard errors are clustered by county.

In addition to the linear trend break model, we also estimate the average reform effect over the entire postreform period in our sample by

$$y_{ijt} = \alpha + \beta_4 \text{Reform}_{jt} \times \text{GirlFirst}_{ijt} + \beta_5 \text{GirlFirst}_{ijt} + \gamma_{jt} + \epsilon_{ijt}, \quad (2)$$

where β_4 measures the average postreform increase in the fraction of males in families with a first girl, compared to families with a first boy.

Our key identifying assumption is that without land reform the trends in the fraction of male second births would be the same in counties that started earlier and those that started later. The absence of preexisting trends in figure 4*B* supports the assumption. The remaining concern would be about other policies that closely follow the county-level reform rollout and have had differential impacts on the sex of the second child depending on the sex of the first one. The most likely candidate is the OCP. In the following empirical exercise, we disentangle the effect of land reform from the OCP.

For second births following a firstborn boy to be a valid control group, the sex of the firstborn child should not be affected by land reform.[15] Figure 4*A* shows that the sex ratio of the firstborns is at the biological level of 1.05. Finally, our analysis sample of second births should not be selected on the basis of our identifying variable. We therefore examine whether

[15] As predicted by the sex selection model described in Sec. VI.

land reform affects fertility at the second parity differentially by the sex of the first child below.

V. Main Results

A. *Land Reform and Sex Ratios*

Table 1 reports trend break estimates in panel A and average effect estimates in panel B. Regression-adjusted estimates yield the same results as the raw data displayed in figure 4.

We first examine whether the sex of the first child is affected by land reforms. In column 1, the estimated trend break (land reform × event time) in panel A and the estimated average effect (land reform) in

TABLE 1
LAND REFORM AND MALE BIRTHS

		Male Second Child		
	Male First Child: All (1)	All (2)	High-Education Mothers (3)	Low-Education Mothers (4)
	A. Trend Break			
Land reform × event time	−.001			
	(.001)			
Land reform × event time × girl first		.007***	.010***	.004
		(.002)	(.003)	(.004)
Observations	325,633	241,547	125,601	115,943
R^2	.006	.051	.085	.097
	B. Average Effect			
Land reform	.001			
	(.005)			
Land reform × girl first		.030***	.038***	.022***
		(.004)	(.007)	(.007)
Observations	325,633	241,547	125,601	115,946
R^2	.006	.051	.085	.097
County-by-year fixed effects		Yes	Yes	Yes
County fixed effects and year of birth fixed effects	Yes			
County-specific linear trends	Yes			

NOTE.—High-education mothers completed elementary school. Trend break regressions on first children control for event time and on second children event time × girl first. All regressions on second children also control for girl first. Robust standard errors clustered at the county level are reported in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

panel B are quite small and not statistically significant. Beyond the absence of sex selection for the first births following land reform, these findings also suggest that a biological channel through nutritional improvement is less likely.[16]

In contrast, the probability of the second child being male increases with land reform if the first child is a girl. For second births in column 2, the trend break (land reform × event time × girl first) in panel A and the average effect (land reform × girl first) in panel B are both precisely estimated. The estimated trend break is a 0.7 percentage point increase per year in the fraction of males following a first girl after land reform. For example, four years after the reform, 2.8 (0.7 × 4) percentage points more parents with a first daughter engaged in successful sex selection of their second child than would have occurred had the prereform sex ratio trend (flat) persisted. Again, this is highly consistent with the unadjusted event study in figure 4*B*. Correspondingly, we find in panel B that the average effect of land reform in the postreform period is 3 percentage points.[17]

If land reform affects sex selection through the income mechanism, we would expect parents with a larger income increase after land reform to be more likely to have male second births. In the absence of an income measure from the census, we use parental education as a proxy; previous research finds that education contributed to higher agricultural productivity and profits after land reform (Yang and An 2002). Indeed, we find a large and highly significant trend break among second births whose mothers have more education in column 3: a 1 percentage point increase per year after the reform. Among less educated mothers, the estimated trend break is smaller and not statistically significant in column 4. Similarly for the average reform effect, mothers with high education engaged in more sex selection after the reform.[18] The heterogeneous response to land reform by parental education suggests that increased parental income is a plausible mechanism through which land reform increased sex ratios.[19]

[16] In particular, improved nutrition after land reform would disproportionately reduce mortality among "fragile males" (Kraemer 2000) but does not appear to be a major mechanism through which land reform affected sex ratios.

[17] Appendix table A.7 shows that estimates are robust to including children under age 4. Appendix table A.8 shows that estimates are robust to including families that report a number of surviving children different from that observed in the family.

[18] Similarly, app. table A.9 shows that children of high-education fathers were more likely to be sons after land reform.

[19] High-education parents have slightly weaker stated son preferences than low-education parents; see app. table A.10 using the China In-Depth Fertility Survey, phase I in 1985 and phase II in 1987. Greater sex selection among high-education parents after land reform cannot readily be explained by differences in preferences. In addition to income, education could affect sex selection through an information channel (better information-seeking on selection methods among the better educated). Access to sex selection technology conditional on income and the information set might be the third channel.

Our estimates are robust to including all births at the second parity or above. We compare all two+ births following sister(s) versus those following at least one son before and after land reform in appendix table A.11. The estimated effect is slightly larger for both the trend break (0.8 percentage points) and the average effect (3.8 percentage points) models than that from second births, consistent with greater sex selection at three+ parity following no previous son seen in cross-sectional analyses (Zeng et al. 1993).

To estimate the contribution of land reform to overall sex ratios, we calculate a weighted average effect across different parities using our estimates above. The weighted average effect of land reform on overall sex ratios is 3.8 percent.[20] Given that the overall sex ratios in our sample increased by 6.6 percent (from 1.06 in 1978 to 1.13 in 1986), land reform contributed to about 58 percent of the increase in rural sex ratios in this period.

B. Land Reform versus the OCP on Sex Ratios

Differences in start dates by county allow us to disentangle the effect of land reform from that of the OCP (fig. 2), which would not be possible in province-level or national data. In this subsection, we focus on whether the county-level rollout of the OCP confounds the effect of land reform on sex ratios.

First, we run a "horse race" between land reform and the OCP in table 2. For the trend break model in column 1, we include two triple interaction terms as independent variables, land reform × event time × girl first and OCP × event time × girl first, where the OCP indicator is equal to one if one is born after the OCP was introduced. We find that arrival of the OCP does not generate a trend break as the coefficient estimate is small and insignificant. In contrast, the estimated trend break after land reform is robust to controlling for the OCP effect. In column 2, the estimated average effect of land reform is very close to that in table 1. Again, we fail to find an average effect of the OCP on sex ratios. These findings suggest that the county-level rollout of the OCP does not confound the effect of land reform.[21]

Second, we also stratify the sample according to the timing of the two policies. We estimate the average reform effect rather than a trend break because of the short pre- or postreform period in these exercises. Esti-

[20] The estimated average reform effect among two+ births, a 3.8 percentage point increase, implies a 16.5 percent increase in sex ratios from the prereform level. The weighted average effect is calculated as follows: $0 \times 0.44 + 0 \times 0.56 \times 0.59 + 0.165 \times 0.56 \times 0.41 = 0.038$.

[21] Nor did the 1.5 Child Policy introduced after 1984 confound the effect of land reform (see app. table A.12). We do find that the 1.5 Child Policy could be an additional factor contributing to the increase in sex ratios since the late 1980s.

TABLE 2
LAND REFORM VERSUS THE ONE CHILD POLICY

	Male Trend Break: All (1)	Male Average Effect				
		All (2)	Pre-OCP (3)	Post-OCP (4)	Pre-HRS (5)	Post-HRS (6)
Land reform × event time × girl first	.008**					
	(.003)					
OCP × event time × girl first	−.001					
	(.003)					
Land reform × girl first		.033***	.025**	.039***		
		(.008)	(.013)	(.010)		
OCP × girl first		−.004			−.009	.004
		(.008)			(.010)	(.013)
Observations	241,547	241,547	113,174	128,373	117,991	123,556
R^2	.051	.051	.054	.048	.053	.048
County-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.—Robust standard errors clustered at the county level are reported in parentheses.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

mates in columns 3 and 4 suggest that land reform has a positive and significant effect on the probability of a second child being male following a first girl for both pre- and post-OCP adoption periods. Thus, land reform has an independent effect.[22] Interestingly, when stratifying the sample by the timing of land reform in columns 5 and 6, we find the OCP has no effect in both the pre- and postreform periods. These findings further support that it is land reform rather than the OCP that affects sex selection among second births that follow an elder sister.

C. *Fertility*

In this section, we examine whether land reform affects fertility and thus poses a sample selection issue. Overall, we find land reform has little effect on fertility, while the OCP has only a modest effect (in the expected direction). In column 1 of table 3, we estimate the effect of land reform versus the OCP on overall fertility: the total number of births by county and year. The estimate of land reform is very small and insignificant.

Fertility decreased by 3 percent after the OCP, and this estimated effect from the county-level rollout can be scaled up to replicate the na-

[22] In app. table A.13, we test the interactive effect of land reform and the OCP by including their interaction term. We do not find a statistically significant interactive effect, although our confidence intervals are relatively wide and include some large positive interactive effects.

TABLE 3
FERTILITY: NUMBER OF BIRTHS BY COUNTY AND YEAR

	Ln(Number of Births)		
	All Births	Second Births	
	(1)	(2)	(3)
Land reform	.008	−.023	
	(.015)	(.024)	
OCP	−.030**	−.022	
	(.015)	(.027)	
Land reform × girl first			−.027
			(.036)
OCP × girl first			.173***
			(.038)
Observations	11,864	11,649	22,532
R^2	.939	.833	.900
County fixed effects and year fixed effects	Yes	Yes	
County-specific linear trends	Yes	Yes	
County-by-year fixed effects			Yes

NOTE.—Robust standard errors clustered at the county level are reported in parentheses.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

tional fertility trend in figure 1*B*: both are modest.[23] For second births in column 2, neither land reform nor the OCP had a statistically significant effect. The absence of an OCP effect is consistent with the rural policy: a second child was allowed and financial penalties were on three+ parity children in the early to mid-1980s. Indeed, we find in appendix table A.4 that the decline in overall fertility following the OCP in column 1 is mainly from fertility reduction at the third parity and above.

Turning to the fertility margin most relevant to our identification, we examine whether land reform had differential fertility effects depending on the sex of the first child. To implement this, we change the unit of observation in column 3 to county × year × sex of the first child. The estimate of land reform × girl first is small and insignificant, suggesting that land reform does not affect the number of second births differentially for first-girl families and first-boy families. Therefore, selected second births may not be a major issue for our main results on land reform.

[23] In app. table A.3, we estimate the change in the national TFR after 1979. The decrease is 5 percent from the pre-1979 average TFR. Furthermore, using province-by-year TFR, we find a smaller decrease of 3 percent. These point estimates are similar to our estimate using the county-level OCP rollout here, while estimates are less precise using national or provincial data. Fertility effects of the OCP in the early to mid-1980s were small because fines were mild for relatively rich families and were not effectively collected from relatively poor families (Scharping 2003). The level of fines increased significantly since the late 1980s along with more strict enforcement.

In contrast, the OCP has differential fertility effects depending on the sex of the first child. The positive and statistically significant estimate of OCP × girl first indicates that families with a first girl were 17.3 percentage points less likely to stop having a second child than those with a first boy after the OCP. A resulting concern is that the change in composition by the first child's sex as a response to the OCP could affect the estimated land reform effect in table 2. In appendix table A.14, we use an inverse probability weighting approach to test the robustness of our estimates.[24] We find that reweighted estimates are similar to estimates in table 2.

To summarize, we do not find evidence that fertility responses would compromise our findings regarding land reform and sex selection. Furthermore, the modest fertility effect we find for the rural OCP (which maps to the national fertility trend and in this respect is difficult to dispute) would imply a minuscule effect of the rural OCP on sex selection through the fertility channel.[25] The minuscule predicted effect on sex selection is confirmed in table 2.

VI. Why Did Land Reform Increase Sex Selection?

It is well known that land reform boosted agricultural output and income. Here we discuss economic motivations behind sex selection as induced by land reform. Empirically, we find the most consistency with an income mechanism.

A. *The Income Mechanism*

We formalize a stylized sex selection model in appendix A that allows sex selection and fertility to respond to both income and the opportunity cost of child rearing (wage). We assume that having a son provides utility to parents (Edlund 1999), and a second son provides no benefit beyond

[24] In the sample of second children, we first predict the effect of the OCP on the probability of a second child in a first-boy family. Because fewer first-boy families are in the sample of second births as a result of the OCP, we use the predicted probability to assign larger weights to first-boy families where the OCP effects are larger. We then run weighted regressions analogous to cols. 1 and 2 of table 2.

[25] The 3 percent reduction in overall fertility comes from a 12 percent fertility reduction among three+ births (see app. table A.4); 67 percent of families had more than three+ children prior to land reform. When the number of children decreases from three to two, the probability of having a son naturally decreases by 14 percent. Not all those with a reduced likelihood of having a son naturally will sex-select. The 1990 rural sex ratio of 1.14 implies that roughly 2.3 percent of all parents selected a male successfully, or about 4.6 percent of those parents pregnant with girls. Scaling that 1990 incidence up (arbitrarily) by an order of magnitude, assume that one-third of parents would sex-select in response to a reduced natural probability of having a son. This would imply that sex selection increased by $(14\% \times 12\% \times 67\%)/3$, or 0.4 percent as a result of OCP-induced fertility reduction.

that of a daughter. To support this assumption, appendix figure A.1 reports the stated preference among rural parents from the China In-Depth Fertility Survey (phase I in 1985 and phase II in 1987): they indeed desire just one son.[26] We also assume that selecting a son is costly. Travel costs for accessing ultrasound in provincial capitals were nontrivial for rural parents (who were very poor on average at this time).[27] Additionally, sex selection might impose a psychic cost.[28]

In the model, parents maximize utility over having a second child, having a son, and consumption. They decide among (1) having a second child with sex selection, (2) having a second child without sex selection, and (3) stopping childbearing following one child. We allow for potentially competing wage and income effects in the decision, as land reform may have affected both. The model first delivers unambiguous predictions of sex selection for neither the first child nor the second child following a firstborn boy, which are supported by results presented in both panels of figure 4 and table 1. More importantly, it predicts greater sex selection of the second child following a firstborn girl in response to income or wage increases. This prediction is analogous to that for consumption "goods" without close substitutes that tend to be normal (Black et al. 2013). In contrast, the wage has an ambiguous effect on the decision to have a second child in the model, depending on the magnitudes of income versus substitution effects (app. Sec. A.2).

Our main empirical finding that land reform affects sex selection for first-girl families is consistent with predictions from the model. The income mechanism could also explain more sex selection among high-education parents after land reform. Furthermore, in appendix Section D.1, we find corresponding heterogeneity in sex selection by the magnitude of postreform income growth. The positive trend break in the fraction of males was concentrated in counties with fast output growth after reform, while no trend break is found where grain output had little growth.

In sum, our findings are consistent with the most basic price theoretic framework (and intuition) where sex selection is costly.

[26] Among rural parents, 79 percent want a son next when they have not already had one, which drops to 31 percent for parents who already have a son (the other 49 percent want a daughter and 20 percent have no preference).

[27] We collected information on train ticket prices and hotel costs in large cities in the 1970s. Two nights at a hotel cost, on average, 9 RMB. Depending on the mileage, train tickets within a province could cost from 0.5 to 4 RMB. Average rural income in 1980 was 191 RMB per person per year (Ravallion and Chen 2007). If one lived within a 2-hour train ride (around 70–80 kilometers) to the provincial capital, one such trip could cost about 6 percent of one's annual income. And multiple trips might be necessary to achieve (and confirm) a son.

[28] The psychological costs could be larger for postnatal sex selection.

B. *The "Productive Son" Mechanism*

To consider the possibility that higher productivity and wages are expected from male children, we extend our model so that sons increase wages (app. Sec. A.5.3).[29] Clearly, this increases the benefit to having two sons. As wage increases, parents can be more likely to select both the first child's sex and the second child's sex following a firstborn boy, neither of which is observed in the data. It is also generally not true from the productive son model that parents are more likely to select the sex of the second child after a firstborn girl unless additional assumptions are imposed.

A distinct prediction of the productive son mechanism is more sex selection when the return to male labor is higher. To evaluate this prediction, we compare land reform's effect on sex ratios in counties more suitable for growing male-labor-intensive crops versus those more suitable for female-labor-intensive crops in appendix Section D.2. In the 1982 census microdata, cotton was the most female-labor-intensive crop, while fruit had been most male-labor-intensive.[30] To assess gender-specific income, we use crop suitability indices based on agroclimate conditions from the Food and Agriculture Organization Global Agro-Ecological Zones database. We first ascertain that these indices do predict cropping patterns in China (app. table A.16). We then estimate the interactive effect of the suitability index of each crop with land reform $\times$ girl first on the fraction of males. Estimates of these interactions are all very small and statistically insignificant (app. table A.17). There is no pronounced heterogeneity by gendered wages as the productive son mechanism would predict.

C. *Other Potential Mechanisms*

In appendix Section D.3, we examine five additional hypotheses: (i) if sons received more land than daughters, this would reward sex selection; (ii) if land reform destroyed the "collective pension system," parents may have been forced to rely more on their sons for old-age support; (iii) if land reform weakened rural health care provision, girls may have suffered; (iv) if land reform weakened the authority of village administration, monitoring and enforcement of any prohibitions on sex selection may have languished; (v) if land reform loosened travel restrictions, it could be easier for peasants to travel to access ultrasound. We do not find these hypotheses consistent with the empirical evidence. That said,

[29] Qian (2008) found that increases in female-specific income, as captured by the relative price increase of tea following tea price reform, increased the survival rate of girls.

[30] Thirty-five percent of workers who grew cotton were male, and 69 percent of workers who grew fruit were male.

our evidence on these mechanisms is more suggestive than dispositive and invites additional data collection and analysis.

VII. How Did Land Reform Increase Sex Selection?

Was sex-selective abortion feasible for rural Chinese in the early 1980s? Land reform generally preceded the arrival of ultrasound machines in rural China, but ultrasound had become increasingly available in provincial capitals since the 1970s.[31] Because railroads were the main means of long-distance transportation at that time, we consider whether a county was connected by railroad to provincial capitals where ultrasound machines were available.

Using a digitized national map of railroad networks in 1980, we define railroad access by whether a railroad line passed through a rural county.[32] Every county on a railroad line was connected to the capital city of the same province; 36 percent of rural counties had railroad access. Access to ultrasound is defined as 1 if a county was connected by railroad to the provincial capital that had ultrasound machines available after land reform. For counties that are assigned 0, either they had no rail connection or ultrasound machines were not yet available in the (rail-connected) provincial capital (or both).

Results reported in table 4 confirm the role of ultrasound access in urban hospitals in sex selection. In column 1, the triple interaction term land reform × girl first × railroad to provincial capital that had ultrasound has a positive coefficient statistically significant at the 10 percent level, suggesting a larger land reform effect if parents could take the train from their home county to the provincial capital to access ultrasound.[33] In column 2, we consider the effect of ultrasound access including all travel options to provincial capitals. We interact land reform × girl first with a binary variable indicating whether ultrasound was available in the provincial capital, which measures the total effect that comes via ultrasound in the province. Comparing estimates in columns 1 and 2, we find that the majority of travel to access ultrasound occurred via rail-

[31] In our basic model in app. Sec. A.2, the costs of sex selection via ultrasound access include travel costs to provincial capitals and psychological costs. In app. Sec. A.5.1, we extend the basic model to include son-rearing costs that yield similar predictions.

[32] The railroad data are generously provided by Matthew Turner and digitized from SinoMaps Press (https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/FAZJE4; Baum-Snow et al. 2017).

[33] A potential concern is that railroad access might also help peasants connect to a larger input or output market or have more exposure to the urban environment. To isolate the effect of ultrasound access from that of rail access, in app. table A.19 we include an additional interaction term land reform × girl first × railroad to provincial capital. The estimate of this newly included interaction term is small and insignificant, suggesting that in the absence of ultrasound technology in the provincial capital, rail access per se does not increase the fraction of males following land reform.

TABLE 4
ULTRASOUND ACCESS IN PROVINCIAL CAPITALS

	Male	
	(1)	(2)
Land reform × girl first × railroad to provincial capital that had ultrasound	.024*	
	(.012)	
Land reform × girl first × provincial capital had ultrasound		.012**
		(.005)
Land reform × girl first	.026***	.032***
	(.005)	(.005)
Observations	241,547	241,547
R^2	.051	.051
County-by-year fixed effects	Yes	Yes

NOTE.—Col. 1 also controls for girl first and girl first × railroad to provincial capital that had ultrasound. County-by-year fixed effects absorb the double interaction of land reform × railroad to provincial capital with ultrasound. Col. 2 likewise controls for girl first and girl first × provincial capital had ultrasound. The double interaction of land reform × provincial capital had ultrasound in col. 2 is absorbed by the county-by-year fixed effects. Robust standard errors clustered at the county level are reported in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

road.[34] Finally, in column 2, the estimate of the double interaction term land reform × girl first suggests a nontrivial and precisely estimated reform effect on sex ratios in places and years without access to ultrasound.[35]

In addition to sex-selective abortion, postnatal sex selection could also be at play for our results because our census data pertain to surviving children.[36] In appendix Section E, we conduct additional analysis on postnatal mortality using the 1992 UNICEF Chinese Children Survey that reports childhood deaths. We find suggestive evidence that the mortality rate among male second births following a firstborn girl decreased after the reform and that female mortality increased. Parents seem to have allocated some of the land reform bounty to boys, which also contributes to the increase in the sex ratio of surviving children.

[34] In our sample, 28 percent of births occurred in counties and years that had a railroad to provincial capitals that had ultrasound. In col. 1, the increase in sex ratios through this channel after land reform is 0.0067 (0.024 × 0.28), which is 20.5 percent of the total effect: 0.0067/(0.0067 + 0.026). In col. 2, 79 percent of births occurred in counties and years in which ultrasound was available in the provincial capital. Therefore, the share through ultrasound including all travel options is 22.9 percent: (0.012 × 0.79)/(0.032 + 0.012 × 0.79), only slightly higher than that through railroads.

[35] In app. table A.20, we stratify the sample by a county's distance to the provincial capital. We find that the effect through ultrasound is strongest for counties within 70 kilometers of the provincial capital.

[36] Although postnatal selection renders a costly ultrasound unnecessary, the psychological costs of postnatal selection could be large and thereby generate predictions regarding an income effect similar to those of ultrasound. Moreover, including son-rearing costs also leads to similar predictions.

VIII. Conclusion and Discussion

Policy makers in Asia have attempted to address high sex ratios by prohibiting prenatal sex determination. China began restricting the use of ultrasound for sex determination as early as 1986, and India issued a similar prohibition in 1994.[37] As the persistence of high sex ratios attests, prenatal sex determination technology is difficult to regulate and continues to improve.[38] It is unclear whether bans provide much of a practical obstacle. In our analysis, sex selection increased despite prenatal sex determination being unavailable locally: poor farmers were willing to bear substantial travel costs. Likewise, fertility did not fall substantially and still sex selection increased.

We find that land reform contributed to about half of the increase in rural sex ratios from the late 1970s to the mid-1980s, when sex selection began in earnest in China. Land reform's historical role in fomenting sex selection has been overlooked by researchers, we suspect, because it was introduced at the same time as the One Child Policy, about which priors are strong and county-level data heretofore unavailable. We estimate that land reform led to about 1 million missing girls (between 0.8 and 1.2 million, 95 percent confidence interval) in this period. In later periods, land reform may continue to contribute as the trend break clearly persists in figure 1*A*.

Our findings add to the documented cultural preference for sons (Das Gupta 1987; Das Gupta et al. 2003) by showing that this preference may interact with household income: we are unaware of existing sex ratio research estimating household income elasticities. From a basic price theory perspective, such estimates are overdue. Son preference implies boys and girls are imperfect substitutes. Like other costly consumption "goods" without a close substitute, is having a son normal? Depending on the estimate of how much land reform increased income, income elasticities of sex ratios range from 0.080 to 0.181 (app. Sec. F). Using 0.080 and assuming a linear relationship between income and sex ratios, one would project that the sex ratio in 2000 would be about 1.2, similar to the actual sex ratio in the 2000 census as shown in figure 1*A*.[39]

We also benchmark our range of land reform elasticities against two other readily available "back-of-the-envelope" estimates. First, we consider the cross-country income elasticity in four Asian economies where there is son preference—mainland China, India, South Korea, and Taiwan—in

[37] Six US states adopted bans on sex-selective abortion in 2012–14 (Rebouché 2015).

[38] "Sex determination could turn into an entirely at-home exercise with home testing kits" (Rebouché 2015, 576). Morain, Greene, and Mello (2013) describe a "new era" in noninvasive prenatal testing.

[39] Rural income has increased by 220 percent from 1980 to 2000 (Ravallion and Chen 2007).

appendix Section G. For the 1975–95 period, we find that a 1 percent increase in GDP per capita is correlated with a 0.089 percent increase in sex ratios. The analysis of cross-country data, while it includes country and year fixed effects, is mainly descriptive given the potential for confounding. At a minimum, the cross-country approach does not cast doubt on a positive income elasticity. Second, we also consider a public policy designed to increase incomes in rural China. Meng (2013) finds that the antipoverty program of 1994–2000 increased rural income by 38 percent. In appendix Section G we see that this increased the overall sex ratios by 3.1 percent, which implies an income elasticity of about 0.082. Both estimates are similar to the lower sex ratio elasticity estimate of 0.080.

As incomes continue to rise and the technology of sex selection disseminates and improves, we might expect elevated sex ratios to increase or at least persist, including among Asians in the West (Almond, Edlund, and Milligan 2013; Almond and Sun 2017). On the other hand, ambitious efforts at "triggering normative change within the society as a whole" may be feasible and have already achieved some success in Korea (Chung and Das Gupta 2007, 777).[40] Given practical challenges to enforcement, banning sex selection may ultimately be more effective in the normative message sent to parents about gender preference.

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