LAND REFORM AND SEX SELECTION IN CHINA*

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Abstract

Following the death of Mao in 1976, agrarian decision-making shifted from the collective to the individual hosuehold. This watershed institutional reform enabled remarkable growth in agricultural output and unprecedented reductions in poverty. We consider whether China's excess in male births may have responded to rural land reform. In newly-available data from over 1,000 counties, we find that a second child following a daughter was 5.5 percent more likely to be a boy after land reform, doubling the prevailing rate of sex selection. Larger increases in sex ratios are found in families with more education and in counties with larger output gains from the reform. Proximately, sex selection was achieved in part through prenatal ultrasounds obtained in provincial capitals and decreased mortality of male children after the reform. The One Child Policy is frequently blamed for increased sex ratios during the early 1980s. Land reform's effect is robust to controlling for the county-level rollout of the One Child Policy. We find suggestive evidence of an interactive effect that increased sex selection.

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1 Introduction

Economic development has helped narrow key gender gaps over the past quarter century, including those in educational attainment, life expectancy, and labor force participation [World Development Report 2012]. On the other hand, perhaps the starkest manifestation of gender inequality – the "missing women" phenomenon – can persist with development, particularly if development reduces the cost of sex selection [Duflo, 2012]. Figure 1 shows the case in China. Despite the rapid growth of GDP per capita since 1980, the sex ratio at birth has increased from 1.06 in 1979 to 1.20 in 2000. In 2010, the sex ratio at birth remains 1.19, or about 500,000 more male births per year than the biological norm of around 1.05 per female.

In this paper, we consider the effect of a fundamental institutional reform in rural China on sex ratios. How did the change from a collective system to an individual household-based farming system affect de-selection of girls? The introduction of the "Household Responsibility System" during 1978-84 unraveled collectivized agriculture and marked a critical first step toward a market-oriented Chinese economy. The reform partially shifted land property rights by granting land user-ship rights to individual households. Land ownership remained with the collective. Land was contracted to households for 3-5 years initially and for longer terms later. Individual households could make their own input decisions and receive all income from the land after meeting the tax and quota sales obligations [Perkins, 1988]. The remarkable growth in agricultural output spurred by the reform has been well documented [McMillan et al., 1989; Lin, 1992]. Land reform is further recognized for its achievement in lifting *hundreds of millions* of rural households out of poverty [World Bank, 2000]. We analyze new data on the rollout of the 1978-84 land reform in China to over 1,000 counties; previous work has focused on variation across 28 Chinese provinces [Lin, 1992].

By evaluating the effect of land reform on sex selection, this paper directly speaks to two prevalent beliefs about sex selection. First, China's One Child Policy (OCP) is routinely blamed for increased sex ratios. By reducing the number of random draws of child sex, the chance that parents obtain a son naturally is lowered, who then turn to sex selection, e.g. Ebenstein [2010]. Coverage of the recently-announced OCP relaxation regularly invokes the Policy's role in "missing girls" [*Xinhua News Agency* (the official press agency of China), Nov. 2013; *USA Today,* Nov. 2013].[1] While intuitive, this argument ignores the historic decline in fertility just prior to the OCP's introduction in 1979. Although China's fertility rate fell dramatically during the 1970s, sex ratios did not increase (Figure 1). Once the OCP was introduced in 1979, fertility rates were comparatively flat (Appendix figures 1A & 1B), which limits the scope for OCP-regulated fertility to explain the aggregate sex ratio trends. We explore whether the effect of land reform on "missing girls" is confounded by the OCP, as both reforms proliferated 1978-84 in rural China. Second, OCP aside, previous findings on the perverse effect of development have usually focussed on particular factors that reduce the cost of sex selection (e.g. prenatal ultrasound). In this

[1] http://news.xinhuanet.com/english/china/2013-11/15/c_132891920.htm
http://www.usatoday.com/story/news/world/2013/11/15/china-one-child-policy/3570593/

respect, increases in sex selection with "development" are not altogether surprising. By contrast, non-cost dimensions of economic development are generally thought to reduce sex selection, e.g. Jensen and Oster [2009].

Using the 1990 population Census microdata, we see a striking increase in the fraction male following land reform in families without a firstborn son (see event study in Figure 2B). Prior to land reform (year 0 and before), we do not see trends in the sex ratio. Nor do we see substantial increases in sex ratios following land reform for the firstborn child (Figure 2A) or the second child if first child was male (Figure 2B, lower line). These raw patterns are replicated in a triple-difference regression framework.[2,3] Specifications that account for a full set of county-by-year fixed effects deliver the same basic finding: following a first daughter, the second child is 5.5 percent more likely to be a boy following land reform. This translates into a 11 percent increase in the county-by-year sex ratio of the second child, or a doubling of the sex selection rate following a first-born daughter. Any potential confounder needs to mimic land reform rollout by county *and* differentially affect families with a first daughter.

As is well known, the OCP was introduced during the late 1970s and early 1980s, i.e., the same period as land reform. We collect the most comprehensive data on the initial introduction dates of the OCP at the county level between 1978 and 1985. We find that land reform's effect is robust to controlling for the county-level rollout of the OCP.[4] Furthermore, holding the OCP environment constant (by looking either before or after the OCP introduction in a county), we find heterogenous land reform effects by the OCP. Specifically, we find a larger land reform effect after OCP introduction. In contrast, when the land reform environment is held constant, we fail to find an effect of OCP on sex ratios either before or after land reform. These findings suggest that it was land reform, not the OCP, that increased sex ratios in the rural areas during the early 1980s (home to 86% of China's population at the time). The subsequent "1.5 Child" Policy arrived 3-5 years after the OCP (Figure 3a); controlling for it does not affect our estimates for land reform. Likewise, ultrasound diffusion would not confound the effect of land reform because it was unavailable in rural counties until the mid-1980s (Figure 4).

Fertility responses are of independent interest and might lead to endogenous sample selection and bias in our sample of the second births. We find a small positive response in the total number of births to land reform. However, on the margin that affects sample selection – the decision to have a second child and the birth interval between the first and second child – land reform had little effect. In contrast, we estimate a consistent, precisely-estimated, but *modest* fertility decline in response to our 1978-85 OCP county-level rollout measure, i.e. during the era of relatively stable national (and rural) fertility.

Finally, we consider economic and proximate mechanisms for the reduced form effect of land reform.

[2] We compare the sex of the second child born before and after the reform between families with a first girl and those with a first boy, using families with a first boy as our control group based on a previously-documented demographic regularity: the sex ratio of the first child is biologically normal, but it becomes abnormally male-biased at higher birth orders, especially among families with no previous son [Zeng et al. 1993].

[3] Standard errors are clustered at the county level.

[4] Land reform accentuated sex selection following a firstborn daughter that preceded both land reform *and* the OCP. The upper line in Figure 2B shows that second parity sex ratio following a firstborn daughter was abnormally high (around 1.15) seven years before land reform, and remained steady until land reform was adopted (whereupon it doubled).

Enhanced male productivity could spur sex selection, either through higher earnings of the father or so as to secure the future productivity increase of sons. Likewise, if sons received more land than daughters, this could induce sex selection. Our evidence is inconsistent with either a productivity or "direct remuneration" mechanism. Instead, we find the income mechanism (increased rural incomes following the reform) more plausible. The sex selection response was highly concentrated in: i) counties that experienced larger income gains from the reform, and; ii) families with more education. 53% of mothers who sex selected in response to land reform (the "compliers") had at least a high school education, despite making up just 4% of mothers having a second child.[5,6]

Turning to proximate mechanisms, parents might prefer to conceal sex selection behaviors, and as such detecting them an exercise in "forensic economics" [Zitzewitz, 2012]. Some rural parents may have determined sex prenatally by traveling to provincial capitals, where ultrasound technology was introduced in the mid-1970s.[7] We estimate that ultrasound access in provincial capitals and reduced male mortality after birth accounted for roughly half of the sex ratio increase that followed land reform.

The remainder of the paper is organized as follows. We summarize the background of land reform and the One Child Policy in Section 2 and preferences over the the sex composition of children in Section 3. The identification strategy follows in Section 4 and data in Section 5. Our main results are presented in Section 6. Section 7 considers economic mechanisms (why sex selection responded) and Section 8 proximate mechanisms (how). Section 9 concludes.

2 Background

2.1 The post-Mao land reform

Under collectivization implemented during the 1950s, workers received daily fixed work points and were paid at the end of the agricultural year [Lin, 1988]. The incentive to work was low and agricultural productivity was stagnant. From 1956 to 1977, there was virtually no change in grain output per capita [Zweig, 1987].

Following the death of Mao Zedong and the end of the Cultural Revolution, a small number of production teams in Anhui Province experimented with contracting land and assigning output quotas to individual households in late 1978 [Lin, 1987; Yang, 1996]. As the movement spread, communes were dismantled and the farm fields were contracted to households for individual cultivation for 3-5 years during 1978-83 (the lease was extended to 15 years nationally in 1984).[8] The land has continued to be owned by the collective. But the basic decision-making unit was shifted from the collective farm to

[5] See Section 4.4.4 of Angrist & Pischke [2009] on estimating average complier characteristics.

[6] Yang and An [2002] find education improved the uses of household-supplied inputs and contributed to higher agricultural profits under land reform. See also Section 7.1 and Appendix Table 2.

[7] Using data on ultrasound machine diffusion by county from Chen, Li, and Meng [2013] and 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 at the time of land reform (see Section 8.1).

[8] It was further extended to 30 years in 1993.

individual households, who could make their own input decisions and receive all the residual income from the land after meeting the tax and quota sales obligations to the state [Perkins, 1988; Sicular, 1991]. Individuals of a former production team were entitled to use of an equal share of the land on a *per capita* basis [Kung and Liu, 1997]. A household received an additional plot for a newborn and lost one when a member passed away [Oi, 1999].

The initial response of the Central Committee of the Chinese Communist Party (CCP) to the new Household Responsibility System (HRS) was unfavorable. "Regulations on the Management of Rural People's Commune" passed by the CCP in the November of 1978 clearly stipulated that contracting to individual households was not permitted. But increased agricultural output quickly softened official resistance. The Party's prohibition was relaxed in September 1979 by allowing exceptions to households living in areas that were peripheral, distant, mountainous, and isolated due to transportation difficulties.[9] In September 1980, Central Document No.75 issued by the Central Committee further allowed poor and remote areas and production units heavily dependent on state subsidies to contract land and output quotas to households. By August 1981, the Central Committee's position on household farming was liberalized in a mission statement sent to fifteen provinces: "contracting to households is not only a means of relieving poverty but also a way of enhancing productivity; and it hasn't changed the production relations of the collective economy".[10] In January 1982, Central Document No.1 officially announced that "the HRS is the production responsibility system of the socialist economy", which first showed the CCP's willingness to popularize the HRS.

2.2 Variation in the county-level reform timing

The rapid rollout of the HRS is shown by the solid line in Figure 3A (See Section 5.1 for data description), which shows the fraction of counties that had introduced the HRS. Under two percent of counties pioneered reform in 1978. The vast majority reformed between 1979 and 1981, with the peak of 45 percent adopting in 1980. By 1984, all counties had adopted the HRS.

Before considering the effect of land reform, we explore what drove reform timing. The institutional history suggests two primary drivers: drought and poverty prior to reform. A severe drought led to large declines in agricultural production, which in turn provided the local government incentive to reform.[11] The negative production shock changed the cost-benefit calculation such that political risk-taking became more worthwhile: contracting land to individual households was not officially permitted in earlier years. Poor and remote counties were among the first permitted to adopt the HRS by the central government as a means to reduce national poverty rates.

The existing literature on HRS adoption at the province level provides three additional insights [Lin,

[9] *Agriculture Yearbook of China 1980*, 1981, Beijing, Agricultural Press.

[10] People's Daily, August 4th, 1981.

[11] Bai and Kung [2011] provide indirect evidence using province level data. They find that provinces that suffered more in the 1959-61 Famine started land reform earlier when struck by bad weather. The interpretation is that the Famine undermined local beliefs that collective farming could effectively cope with negative weather shocks.

1987; Yang, 1996; Chung, 2000]. First, the diffusion of HRS was faster where reduction in monitoring cost was higher and thus productivity gains larger. Using size of production team to measure monitoring cost, previous studies show mixed results.[12] The second hypothesis is that provinces that suffered more from the 1959-61 Famine reformed earlier because they were more disenchanted with collective farming [Yang 1996; Bai and Kung 2011]. Lastly, Yang [1996] argues that provinces further from Beijing had more freedom to initiate reform earlier.

We first test the correlation between reform timing and its potential time-invariant determinants (measured prior to the reform). At the county level, poverty is captured by grain output per capita in 1977 that are collected from county gazetteers. Remoteness is measured by distance to provincial capital using a GIS map of the 1982 Census. Size of production team is proxied by the density of the labor force (aged 16-60) in 1977.[13] Famine intensity is measured by the average birth cohort size in 1953-1957 divided by the average cohort size in 1959-1961 using the 1982 Census.[14] We also calculate the distance to Beijing to proxy for discretion in local policy-making. Table 1A shows that counties that were initially poor, had larger production teams in 1977 and higher famine intensity in 1959-1961, and were located further from the central government adopted reform earlier, consistent with previous studies using provincial variation. The correlation between reform timing and the baseline sex ratio at birth in 1975-77 (from 1982 Census) is not statistically significant. This suggests that the underlying tendency to sex select (and its predictors) at the county level are uncorrelated with land reform timing. In the multivariate regression, controlling for grain output per capita in 1977 forces us to drop two thirds of the sample due to lack of data (we still have an order of magnitude more sample than previous studies). We omit grain output in the last column of Table 1A and find robust results for labor force density and famine intensity. The final note is on explanatory power. The R^2 is 0.095 when all initial controls are included. In a simple test on how much county fixed effects alone predict reform timing, we find that the increase in R^2 by adding county FE is very close to 0.095, suggesting our time-invariant observables may indeed capture the static predictors of reform timing.

Next, we test whether drought led to land reform by matching the county-level data on reform timing with county-by-year data on precipitation.[15] Land reform is an irreversible event, implying that drought prior to reform might affect the decision to reform, but drought after would not. Thus, we assign zero before reform, one to the first year of reform, and missing values after. In addition, the Chinese Academy of Agricultural Sciences [1984] suggests that the growth of rice, the No.1 grain in China by output, largely depends on rainfall at the beginning of the growing season, usually in March or April. In Table 1B, column 1 shows no correlation between the first year of reform and drought defined by average monthly precipitation in the whole growing season (March to September) in the reform year

[12] Lin [1987] finds that provinces with larger production teams reformed earlier, while Chung [2000] has the opposite finding.

[13] Density is calculated by population size aged 16-60 years in 1977 divided by area at the county level using 1982 Census.

[14] Meng et al. [2009] use a similar measure of famine intensity using the 1990 Census. See also Dyson (1991) on fertility response as a famine metric in South Asia.

[15] See Data Appendix.

and the year preceding.[16] From columns 2 to 5, we measure drought by monthly precipitation from March to June separately. As expected, droughts in March and April of the reform year and one year prior have a strong and precisely estimated effect of hastening reform.

Given the observed differences across counties in reform timing as well as possibly unobserved differences, we use two variants to our triple-difference framework (see section 4.1). First, we control for the time-varying droughts in March-April and time-invariant determinants of reform timing interacted with time fixed effects (see equation (1)). Second, we implement a more demanding test: controlling for the full set of county by year-of-birth interactions (see equation (2), our preferred specification).

2.3 Land reform and grain output

Land reform rewarded individual effort more than collective farming. McMillan et al. [1989] used national, time-series data and suggest that over three-quarters of the productivity increase 1978-84 could be attributed to the incentive effects of the HRS. Using the reform rollout by province, Lin [1992] has a similar finding that the reform accounts for half of the output growth. Official statistics show that the rural poverty rate declined from 30 percent in 1978 to 5 percent in 1998 [World Bank, 2000].

Unfortunately, we do not observe household income in the Census microdata, nor is income data available from other sources for this period. Nevertheless, we provide the first quantitative evidence on the output gain from the 1978-84 land reform at the county level. We use grain production by county and year from the 1970s to the mid-1980s that we entered from hard-copy county gazetteers. Records on grain output in the 1970s are particularly scarce because in general county-level statistics have only been released systematically since the 1980s in China. These data are also arguably reliable because they were originally from local official archives (Xue, 2010).[17] There are 400 counties that report both the reform timing and the complete year-by-year grain production from 1974 to 1984. Data on other crops, especially cash crops, are rarely reported in the county gazetteers, nor are they available from any other data sources for the 1970s. Therefore, our analysis below presumably yields a conservative estimate of the overall output gain.

We plot grain output per capita by year relative to land reform in Appendix Figure 2. Time 0 indicates the first year of reform. The trend prior to land reform is relatively flat, consistent with the literature that agricultural productivity growth under the collectivized system was sluggish. There is a jump of grain output one year after the first reform year, suggesting that the first impacted harvest was one year after the reform. Additional detail on magnitudes is provided below (Section 7.1).

[16] The month of reform is not recorded consistently. In data on reform year, a drought in the growing season is likely to affect reform at the second half of the current year or in the next year.

[17] Because the purpose of compiling county gazetteers is to accurately record local history rather than to report to the upper level government, local historians in the county gazetteer office have relatively little incentive to manipulate the grain output data.

2.4 One Child Policy and subsequent "1.5 Child" Policy

One Child Policy

The One Child Policy (OCP) was introduced over the same period as land reform. Prior to the OCP, the government had started a series of birth-planning propaganda campaigns in 1971 (Scharping, 2003). These campaigns focused on promoting "later, longer, and fewer", which referred to later marriage (minimum marriage age was 23 for women and 25 for men in rural areas), longer birth spacing (three to four years) and fewer children. A two-child norm was widely promoted. A popular slogan was: "One isn't too few, two are just fine, three are too much". During the Cultural Revolution, the government relied on ideological education and campaigns, which coincided with a large drop in average fertility. The total fertility rate decreased from almost 6 in 1970 to a little less than 3 in 1979, a nearly 50% decline (See Appendix Figure 1A from Cai (2008)). When economic reform started in 1978, the government set a population target of 1.2 billion in 2000 to maintain desired economic growth rates. Scientists hired by the government agued successfully that the population target could not be achieved under a two-child policy (Scharping, 2003).

In January of 1979, the OCP was officially announced. Departing from the propaganda campaign of the 1970s, the 1979 policy introduced a new system of financial incentives for birth control. The initial policy permitted one child in urban areas (home to approximately 14% of the Chinese population). Urban parents who gave birth to two children would suffer economic sanctions. Rural parents who had a third child were punished [Banister, 1987]. But introduction of the OCP between 1979 and 1982 did set explicit incentives for the second child in the rural areas. From our county-level OCP rollout data (see Section 5.1), 56% of counties introduced the OCP in 1979, and 97% had OCP by 1982.

Fertility was higher following the OCP's introduction than commonly believed. Nationally, the post-1979 total fertility rate (TFR) was fairly stable around 2.5 children per woman until 1988 (Appendix Figure 1A). We separate rural from urban TFR trends using the 10% sample of the 1988 national two-per-thousand Population Sampling Survey on Fertility and Contraceptives (Appendix Figure 1B). The rural TFR fell by nearly half from 1970 to 1977, and it "bottomed out" around 3 children, where it remained until 1986, the year the youngest cohort in our analysis sample were born. These trends are noteworthy given a common belief that the OCP had led to a large fertility decline in the 1980s (compared to fertility in the 1970s). Furthermore, fertility in rural areas remained steady and well *above* replacement levels during the HRS and OCP rollout period.

"1.5 Child" Policy

In 1984, the stated OCP was relaxed by national "Document 7" to allow second child permits to families with a first girl, the so-called "1.5 Child" Policy [Greenhalgh, 1986; Scharping, 2003].[18] Guangdong and Hainan are the only two provinces that started the 1.5 Child Policy prior to the national policy, in 1981-82 [Scharping, 2003]. By the time the 1.5 Child Policy was implemented in 1984, all counties had

[18]The stated policy was tightened to allow only a few types of rural families to have the second child in 1982, but we do not see any county governments revising their policies on this margin 1982-1984 in the county gazetteers.

the HRS for at least one or two years (see Figure 3A). Our main potential confounder is thus the earlier One Child Policy. Indeed, when we control for the 1.5 Child Policy in Appendix Table 1, we find quite similar results for land reform.

3 Preferences for sex composition of children

Son preference in China has been well documented. Below, we cite three lines of evidence suggesting that if there are two children, a sex mix is most preferred, followed by two sons. Two girls are least preferred.

First, interviews conducted by demographers suggest that for rural parents, the vast majority report preferring two children if there were no fertility restriction, with "one son, one daughter" (Chu, 2001; Greenhalgh et al. 1994). Moreover, most rural women think that "having two sons is not perfect but acceptable". In Chu (2001)'s interviews, "rural women whose first child is a son usually take no measure to guarantee the sex of the second one, while those with a first girl would take steps to ensure the second is a son". These studies suggest that 1) son preference is non-monotonic; 2) preference for diversity could lead to sex selection.

Second, we discuss reasons why parents might prefer a sex mixture to all sons. Suppose parents prefer and can have two children. First, raising a son is more costly than raising a daughter, especially when it comes to marriage. In rural China, parents have to prepare a house and wedding for their son's marriage, while marrying a daughter may cost parents nothing (Chu, 2001). Second, there is disutility of having more than one son. While parents of one son can anticipate to live with him, two sons bring friction and uncertainty on whom to rely in their old age (Greenhalgh et al. 1994). Moreover, two sons might fight for splitting family wealth when they get married. Third, it may be the case that a daughter is beneficial in raising a son (Chen, Ebenstein, Edlund, Li, 2012).

Third, we consider the sex of children in the 1990 Census microdata. Following a first son, girls are actually slightly more common than biologically normal: Figure 2B shows that the sex ratio of the second children is consistently below the 1.05 norm when first child is son, a feature previously noted by Chen, Ebenstein, Edlund, Li (2012). That said, the pro-son bias after a daughter is stronger than the pro-daughter bias after a son. Nevertheless, a mixture seems preferred to two boys.

If sex mix is most preferred, the cheapest way to attain that *ex ante* is to not sex select with the first child, and sex select as necessary for the second child. And indeed, sex ratios are normal for the first child. Were one to sex select on the first child, one still bears a roughly even chance of having to sex select again with the second child to achieve a mix. This suggests that although childbearing and sex selection is a sequential "game", the action is hypothesized to be on the second child. This assumes that the decision to have the second child is unaffected by land reform, which we also provide evidence for below.

4 Empirical Strategy

4.1 Econometric Specification

We use the arrival of land reform by county as a natural experiment. We start the analysis with basic comparisons of sex ratios before and after the reform (i.e. without regression adjustment) in event study figures.

To estimate the effect of exposure to land reform on the probability of second child being male, our main estimation framework is a triple-difference. The first double differences are among birth cohorts born before and after the reform and between counties that reformed earlier and those that reformed later. The third difference is between families with a first girl and those with a first boy. To account for potentially confounding differences across countries related to reform timing, we adopt two approaches. Equation (1) represents the first approach:

$$\begin{aligned} Boy^2_{ijt} = {} & \alpha + \beta_1 Reform_{jt} + \beta_2 Girl^1_{ijt} + \beta_3 Reform_{jt} * Girl^1_{ijt} \\ & + \gamma_j + \delta_t + \phi_j * t + D'_{jt}\theta_t + D'_{jt-1}\lambda_{t-1} + \sum_{t=1975}^{1986} (X'_j * T_t)\rho_t + \varepsilon_{ijt} \end{aligned} \quad (1)$$

where the subscript i denotes the individual, j the county of birth, and t the year of birth. The superscript denotes birth order: 1 for the first child, and 2 the second. The dependent variable, Boy_{ijt}, is a binary outcome that is equal to 1 if the second child is a boy and 0 otherwise. The land reform indicator $Reform_{jt}$ is equal to 1 if the child was born one year after reform and 0 otherwise, which is determined by one's year of birth and county of birth. $Girl^1_{ijt}$ is an indicator that is equal to 1 if the first child is a girl and 0 otherwise. We interact the reform indicator with sex of the first child to get the key regressor, $Reform_{jt} * Girl^1_{ijt}$. The coefficient of interest is β_3. Standard errors are clustered at the county level.

To remove possible confounding differences among birth cohorts and between reform starters and followers, a comprehensive set of controls are included in the estimation. County fixed effects γ_j and year of birth effects δ_t absorb the effects of time invariant county characteristics and birth cohort effects. County specific linear trends, $\phi_j * t$, account for county characteristics that change smoothly over time and that are correlated with the reform timing. Furthermore, we account for time-varying effects of county characteristics that are found to drive the reform timing: droughts in March and April of the current year are denoted by D'_{jt}, and droughts of previous year are denoted by D'_{jt-1}. The time-invariant determinants of the reform timing, X'_j, including labor force density in 1977, famine intensity in 1959-61 and distance to Beijing, are interacted with time fixed effects from 1975 to 1986, with 1974 omitted.

Second, a more demanding approach enabled by the "first daughter" experiment is to control for county-by-year fixed effects to absorb *all* time-varying county characteristics:

$$Boy_{ijt}^{2} = \alpha + \beta_2 Girl_{ijt}^{1} + \beta_3 Reform_{jt} * Girl_{ijt}^{1} + \gamma_{jt} + \epsilon_{ijt} \tag{2}$$

where γ_{jt} denotes the county-by-year fixed effects. The coefficient β_1 of the reform indicator $Reform_{jt}$ is no longer identified. Comparing β_3 from estimating equation (1) and (2) helps to infer whether time-varying county features omitted in equation (1) would bias the impact estimate. We will see that estimates without regression adjustment are quite similar to regression-adjusted estimates from estimating either (1) or (2).

We use specification (2), our preferred approach, through most of the analysis below.

4.2 Identification

The coefficient of interest, β_3, measures the effect of land reform on whether the second child is male in families with a first girl relative to that in families with a first boy. Two identifying assumptions underpin this triple-difference strategy:

1. The second births in families with a first boy provide the appropriate counterfactual.
2. There are no unobserved changes coincident with land reform by county and year that have differential effects on the sex of the second child depending on the sex of the first child.

The validity of the first assumption requires that the sex of the first child is not endogenous to the reform and the absence of pre-existing trends in the sex ratio of the second child in families with a girl versus those with a first boy. As noted in the Introduction, Zeng et al. [1993] documented that the sex ratio of the first births is biologically normal. That is, we have an observable metric of the exogeneity of the first-born child's sex in it's proximity to normal sex ratio of 1.05 – we don't think first-born *sons* are selectively aborted, which could offset deselection of girls and thereby yield a normal sex ratio on net. To be cautious, we also directly test whether the reform affected the sex of the first child and fail to find an effect. We also provide transparent evidence that there are no pre-existing trends in the sex ratio of the second births.

China experienced many dramatic changes in the late 1970s and early 1980s. Concurrent reforms by county might call into question the second identifying assumption. To confound the effects of land reform, other reforms should both follow the timing of land reform adoption by county *and* have had differential impacts on the sex of the second child depending on the sex of the first one. To incorporate such potential confounders, we have conducted a comprehensive reading of reform policies from the late 1970s to the mid-1980s. At first pass, two historic reforms might appear to pose confounding threats. First, price reform and market reform (aspects of the broader rural economic reform) might also lead to a stronger desires for sons. However, these were introduced in the same year nationwide: the increases in procurement prices and in bonuses for above-quota production occurred in 1979 [Sicular, 1991]; reductions in the planning of agricultural production and in the restrictions on interregional trade were also universal state interventions [Lin, 1992]. The effect of these sweeping reforms are absorbed by year of birth effects δ_t. Second, using the second child following a first boy as our control group, we can difference out any effect of reforms that arrived at the same time as land reform, but whose effect would not depend on the sex of the first child.

The initial introduction of the OCP in 1978-1984 stands out as the most likely confounder for our triple difference approach. Previous studies at the provincial level find that higher fines under the OCP led to higher sex ratios, especially at higher birth orders with no older brothers [Ebenstein, 2010]. For our purposes, it is the timing of OCP introduction by county in 1978-1984, the same period when HRS was introduced, that poses a threat to our identification of the land reform effect. We therefore have compiled the most detailed data on the timing of OCP implementation by county, i.e. finer geographic resolution than previous studies using policy variations at the provincial level, e.g. Ebenstein (2010). Using data on the county-level timing of both land reform and the OCP, we can disentangle which reform is the more important driving force in increased sex ratios in the 1980s.

Conceptually, one might be concerned about the gender-specific revision of the OCP to the 1.5 Child Policy: only parents who had a girl first were allowed to have a second child under the latter policy. However, the 1.5 Child Policy did not start nationally until 1984 (except for Guangdong and Hainan provinces), i.e. after introduction of the HRS in 1978-1984. Because the 1.5 Child Policy did not coincide with the introduction of HRS, it is unlikely to confound our analysis of the land reform effect (see Figure 3A and Appendix Table 1).

A final note is on the introduction of ultrasound machines which increased sex ratios, especially following a first girl [Chen, Li and Meng, 2013]. Ultrasound machines did not arrive in rural areas until the mid-1980s, i.e. after the rollout of land reform. As a result, the county-level rollout of ultrasound machines would not confound our findings on land reform, when these birth cohorts were around age 5. Nevertheless, earlier introduction of ultrasound technology in provincial capitals could help shed light on how parents sex selected. In Section 8.1, we further investigate the role of ultrasound machines in provincial capitals below using data from Chen, Li and Meng [2013].

5 Data

5.1 Local reforms and ultrasound access

Our main data source for the county-level rollout design is the post-1949 county gazetteers that document local events and statistics about geography, politics, the economy and culture from 1949 to the 1980s. We conducted a comprehensive survey of all county gazetteers that have been published to date, covering 1835 counties. We compiled and digitized data on the county-level rollout of land reform and the OCP from these hard-copy county gazetteers. These records are originally from official sources, e.g., historical archives and policy documents of county governments (Xue, 2010).

Land reform rollout (county-level)

We identified information on the year the HRS was introduced by county for 1242 counties, representing two-thirds of all counties that have ever published gazetteers.[19] Specifically, we use the reported year when collectively owned land was first contracted to individual households in a few villages for each county; it usually took 2-3 years to spread the HRS to the whole county. Because land reform occurred in rural areas, our sample includes locations that were rural counties at the time of the reform.[20]

One Child Policy rollout (county-level)

For the OCP, we compiled data on the year the county government issued the first policy document to enforce rewards for the single child and penalties for above-quota, third births. There are 990 counties that report the timing of both land reform and the OCP.

In Figure 3A, the short-dotted line shows the fraction of counties that had introduced the OCP between 1978 and 1986, while the solid line represents HRS timing, both scaled by the Y-axis on the left. Despite similar timing in 1978-1984 in aggregate, land reform and the OCP show substantial difference in the county-level timing between 1978 and 1982. The county-level difference is visible in Figure 3B, showing the distribution of the difference between land reform start year and the OCP start year. Land reform came earlier than the OCP in 27% of counties, 25% in the same year, and in 48% the OCP came earlier. The correlation between HRS timing and OCP timing at the county level is -0.005. By 1982 when the OCP supposedly became restrictive on the second child in the rural areas, 99% of counties had already introduced the HRS.

1.5 Child Policy rollout (province-level)

The 1.5 Child Policy was announced as a national policy in 1984. County-level information on the Policy was rarely recorded. Instead, we obtained the rollout timing by province from two sources: 1) the chapter on birth planning policies in provincial gazetteers; 2) Sharping (2003) chapter 6.4.

Five provinces (Xinjiang, Yunnan, Ningxia, Qinghai and Shanghai) did not implemented the 1.5 Child Policy in the 1980s.[21] We plot the provincial rollout among the other 24 provinces in 1978-1986 with the long-dotted line in Figure 3A, scaled by the Y-axis on the right. By 1981 when Guangdong province started the 1.5 Child Policy, more than 90% of counties had completed land reform. By 1984 when the 1.5 Child Policy started to spread nationwide, all counties had already had the HRS for at least one or two years. To confound our results, the 1.5 Child Policy have to have had to particularly affect sex selection among three and four year olds (see also Appendix Table 1).

[19] The other one-third of counties either do not report the timing of HRS adoption or report it as "the late 1970s" or "the early 1980s", i.e. too vague to implement our identification strategy.

[20] City districts are defined and excluded by using the county code in the 1982 Census and the official definition.

[21] In the 1980s, Xinjiang, Yunnan, Ningxia and Qinghai issued second child permits to the entire rural population, and Shanghai did not revise the OCP to the 1.5 Child Policy (Scharping, 2003).

Ultrasound technology adoption (county and province level)

Because ultrasound diffusion increased sex ratios in China (Chen, Li, and Meng, 2013), we might be concerned that land reform is capturing the effect of ultrasound. We match our data on HRS rollout with the rollout of ultrasound technology by county (provided by Chen, Li and Meng [2013]) and show this is not the case. In Figure 4, the short-dotted line shows the fraction of counties that introduced ultrasound machines between 1978 and 1990. As noted above, the vast majority of counties acquired ultrasound machines after 1984. By 1982 when HRS was introduced in more than 99% counties, only 4% had ultrasound machines. During the rollout of land reform, there was little change in the local cost of sex selection through the introduction of ultrasound machines.

Although ultrasound technology was unavailable in the rural areas during land reform, it was introduced in provincial capitals as early as the 1960s. The first ultrasound machine arrived in Xi'an in Shaanxi province in 1965. Other provincial capitals started to acquire their first machine since the mid-1970s, which made prenatal sex determination possible. In Figure 4, the long-dotted line shows the rollout of ultrasound machines in provincial capitals, mostly between 1978 and 1984.[22] So during the rollout of land reform, one option for pregnant women was to travel to the provincial capital to ascertain fetal sex. In Section 8, we examine further whether and to what extent sex selection induced by land reform seemed to operate through ultrasound access in provincial capitals.

5.2 Microdata

To consider sex ratios, we use the 1 percent sample of the 1990 Census microdata.[23] Our analysis focuses on rural areas which were defined as counties in the 1982 Census, the definition closest to the time of land reform. Census data in China do not report county of birth, which forces us to use county of residence in 1990 to match the Census data with the county-level data on reform timing. There are 1065 counties (58 percent of all) that are matched with data on reform timing and county controls. Concerns about endogenous migration are circumscribed because internal migration had been under strict control under *Hukou* system until after the land reform we consider was completed; the first *Hukou* relaxation was in 1985 [Wang, 2005]. (Migration rates are described further later in this subsection.)

In the 1990 Census microdata, we focus on cohorts born 1974-1986, who were surviving children in 1990. One concern of studying survivors is that the income increase following land reform would make male fetuses less fragile and thereby increase the male survival rate at birth [Kraemer, 2000]. This biological mechanism could also explain an increase in sex ratios after land reform, but it is distinct from sex selection choices made by parents. If land reform indeed made male fetus more likely to survive, we should observe an increase in sex ratios at other birth orders. In next section, we examine this implication at the 1st and the 2nd birth order.

[22] Interestingly, the rollout of ultrasound machines in non-capital cities was later, i.e. similar to the rollout to rural counties.

[23] Available at: https://international.ipums.org/international/index.shtml

Implementing our research design requires information on one's birth order and the sex of previous children, which are not explicitly queried in the Census data. We use information on the relationship to the household head to identify his/her children and order them using their month and year of birth. To verify 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 by their mother.[24] Our analysis sample includes second births born 1974-86.

A natural concern about imposing the sample restriction is whether families with an older first child living outside the household in 1990 are excluded (by the restriction that the number of surviving children equal the number of observed children). The oldest second child in the sample was age 16 in 1990. Using the average birth interval of 3 years, the oldest first child would be around 19, who were usually too young to leave their parents' home. Nevertheless, we test how large the sample bias would be by comparing the birth year distribution of the first child (who are matched to our second child) in the 1990 Census and the 10% sample of the 1988 national two-per-thousand Population Sampling Survey on Fertility and Contraceptives, the latter of which does not suffer from a sample selection problem as it reports year of birth, birth order, and sex of every birth. If we have excluded a substantial number of families with an older first child away, we would expect more older cohorts (precisely, first births before 1974) in the 1988 Fertility Survey compared to that in the 1990 Census. In Appendix Figure 3, the birth year distributions of first children before 1974 in these two dataset are nearly identical, reducing concerns about sample selection.

We impose two additional sample restrictions. First, we exclude families with multiple births, where birth order is more difficult to identify and interpret. Second, for the sub-analysis by parental education, we consider only children in two-parent families.

A reason for excluding children born 1987 and later is to reduce the possibility of under-reporting. Parents may underreport above-quota births following the introduction of the One Child Policy. Based on follow-up surveys conducted right after the Census in 1990, the National Bureau of Statistics reports that the underreporting rate is 0.7%. The rate is very low, but it is more common that children aged 0-4 in the Census year are underreported (Zhang and Zhao, 2006). Therefore, we focus on children born prior to 1987.[25]

In our sample of births, one is defined as a migrant if he/she did not reside in the same county in 1985, which is reported in the Census. The migration rate among individuals born in 1974-84 is 0.63 percent. Throughout our analysis, we use the 99.37 percent born 1974-84 who resided in the same county in 1985 and all births (irrespective of relocation since 1985) in 1985-86.

Summary statistics of the full sample and the two-parent sample are reported in Table 2. Roughly half the child sample was "exposed" to land reform. About 10% of their parents completed high school, with substantially higher completion rates among fathers.

[24]In our sample of counties matched with the land reform data, 87% of mothers report the number of surviving births that is equal to the number of children linked in the census.

[25]We checked the robustness of our results by including children born 1987-1990. Results are very similar to those in our main sample.

6 Main results

6.1 Land reform and sex ratios: event study figures

We begin by plotting the sex ratio of the first child by birth timing relative to the year of reform in Figure 2A (raw/unadjusted figure). The sex ratio is very stable at the biologically normal rate of 1.05 before and after the reform, supporting our use of families with a first boy as the control group. Land reform did not precipitate more sex selection for the first child, which might have been expected if sons (plural) were strongly preferred and their cost alone was an overriding deterrent. Moreover, the stable sex ratio among the first born children also addresses concern that more male fetus survived after the land reform due to their reduced frailty.

Figure 2B shows our primary result: sex ratios of the second child for families with a first girl before and after land reform. For comparison, we plot families with a first boy separately (neither line is regression adjusted). Among these comparison families, little change in the (second child) sex ratio is observed in the pre- and post-reform periods. More importantly, there are no pre-existing trends for either families with a first boy or those with a first girl. Among the pre-reform cohorts, the sex ratio of the second child in families with a first girl is persistently higher than that in families with a first boy. The steady 10 percentage points gap suggests son preference as a culture, that is, parents with no previous son manifest a stronger desire for a subsequent son (and have some means of achieving it). Starting from one year after the reform, the sex ratio in families with a first girl increases dramatically, from around 1.15 to the peak of 1.3 six years after the reform. The sharp contrast between these two groups in the pre- and post reform periods suggests that land reform is the driving force behind rising sex ratios.

6.2 Land reform and sex ratios: regression estimates

Estimating equation (1) in column 1 and 2 of Table 3 yields the same estimates as the raw data displayed in Figure 2A. In column, the estimate of land reform on the sex of first child is economically very small (a 0.6 percent increase relative to sample mean) and not statistically significant.

Column 2 presents the estimate for the effect of land reform on the second child being male, with the full set of control variables listed in equation (1). We find an increase in the probability of being male of 2.9 percentage points among families with a first girl relative to families with a first boy, statistically significant at the 1 percent level.[26] The effect is sizable in magnitude, around 5.5 percent relative to the sample mean for all second births. Land reform's effect is slightly larger than the baseline level of son preference, as captured by the effect of having a first girl, which is an increase of 2.7 percentage points.

We implement a more demanding comparison by controlling for county-by-year fixed effects, i.e.

[26] We also estimated the trend break model suggested by the change in slope in Figure 2B. The probability of being male increases by 0.5 percentage points per year after the reform. Over 6 years, the increase is 3 percentage points, consistent with our estimate of the shift in level captured by equations (1) and (2).

equation (2). Notably, we get exactly the same point estimate and standard errors for reform interacted with the first child being a girl. This suggests that none of the omitted time-varying county characteristics in equation (1) affect our estimate of interest. For all subsequent estimations below, we use the preferred specification in equation (2).

Han Chinese (90% of population) are known to have stronger and more consistent son preference than ethnic minorities. We would therefore expect sex ratio impacts to be concentrated among the Han. In column 4, we find a 3.3 percentage points increase in the probability of being male among Han families with a first girl relative to Han families with a first boy (using column 2 specification). This suggests a larger effect of land reform on sex ratios among Han Chinese.

To translate the effect of land reform on male births to the effect on sex ratios, we estimate equation (2) on the sex ratio of all second births aggregated by county and birth year. In column 5, the sex ratio in families with a first girl increases by 0.14 following the reform, a precisely estimated increase of 11 percent that matches the magnitude in the (unadjusted) Figure 2B.

6.3 The One Child Policy and sex ratios

We present three sets of results to distinguish the effect of land reform from that of the OCP and its later revision (the 1.5 Child Policy in the mid-1980s).

In the first test, the data we digitized on the county-level rollout of land reform and the OCP permits a horse race between these two reforms. We focus on rural counties, home to 86% of China's population at the time, and we use the sample of 990 counties that report the timing of both land reform and the OCP.[27] We assign treatment status to the OCP as 1 for individuals born one year after the OCP or later and 0 otherwise. We estimate the effects of both land reform and the OCP using equation (2). In Table 4, the first three columns report the results for all births in our sample. Column 1 shows a similar estimate in this subsample as in column 3 of Table 3. In column 2, we find that the second birth in families with a first girl is 2.4 percentage more likely to be male after the introduction of OCP, which is precisely estimated. Thus, at first blush it appears that "phase 1" of the OCP increased sex ratios. This initial finding is consistent with the common argument that the OCP increased sex ratios (which has likewise not accounted for land reform). However, when we take the additional step of controlling for both land reform and the OCP in column 3, the estimate for land reform is robust while estimates for OCP become much smaller and statistically insignificant. Indeed, the point estimate on the OCP by first girl interaction term falls by an order of magnitude.

The OCP applies to Han Chinese, not to ethnic minorities (see, e.g. Li, Yi, and Zhang, 2011). One might be concerned that columns 1-3 average over Han and (otherwise dissimilar) ethnic minorities. In column 4-6, we repeat the column 1-3 specifications in the subsample of Han Chinese. When both land reform and the OCP are included in column 6, a larger land reform effect is found among Han: 3.9 percentage points compared to 3.3 in column 3. Again, we fail to find an effect of the OCP on sex ratios

[27]Sex ratios in rural and urban areas were similar during the early 1980s and increased by comparable amounts 1978-84.

among Han Chinese.

In the second test, we stratify the sample by holding the OCP environment constant. We define the subsamples according to whether the OCP was not yet in effect in the county or already in effect. If land reform indeed increased the sex ratios, we should observe land reform effects on both subsamples. Column 1 of Table 5 consider children born before the introduction of the OCP in their county. Even with relatively mild fertility restriction, the second birth in families with a first girl is 2.1 percentage more likely to be male following land reform, significant at the 10 percent level. After the OCP is introduced in column 2, the land reform effect is 3.9 percentage points and is more precisely estimated (significant at the 1 percent level). These findings suggest heterogeneous effects of land reform depending on fertility restrictions.

To test whether the OCP has an effect on sex ratios holding the land reform environment constant, we do a similar exercise in two subsamples in which land reform is either not in effect or in effect for everyone. In column 3, individuals were all born before the introduction of land reform. The estimated effect of the OCP is not statistically significant and has a perverse sign (reduces sex selection). Column 4 includes individuals born after land reform. The OCP estimate is again not statistically significant. Note that the point estimates for the OCP in columns 3-4 are much smaller than those on land reform in columns 1-2. We fail to find an effect of OCP on sex ratios either before or after land reform.

Finally, we consider whether our land reform estimates are altered by allowing for the rollout of the 1.5 Child Policy by province. Appendix Table 1 reports the results. As one would expect, the gender-specific 1.5 Child Policy is indeed being captured: the probability of being male among second births following a first girl increased. When land reform, the OCP, and the 1.5 Child Policy are all included, the estimated effect of land reform, (3 percentage points) is similar to that in column 3 of Table 4 without controlling for the 1.5 Child Policy. We also find that the probability of the second child being male increased by 2.2 percentage points after the 1.5 Child Policy, consistent with the letter of this policy. Nevertheless, as suggested by the timing shown in Figure 3A, land reform's effect on sex ratios does not appear confounded by the later revision of the OCP.

6.4 Fertility responses to land reform and the One Child Policy

Fertility responses are of independent interest, and could also complicate interpretation of the sex selection results. First, if land reform increases the desire to have more than one child, our sample of second births would be endogenously selected (see, e.g. McCrary and Royer, 2011). Another concern is about the timing of the second child. After the reform, parents might want to have the second child sooner in order to receive another plot of land earlier, which would generate selection on birth year.

We first test the effect of land reform on fertility. In Table 6A, the number of births by county and year increased by 2 percent due to land reform, while it is decreased by 2 percent by the OCP. We take the former as suggesting that having children is a normal good [Becker, 1960].[28] The effect of the

[28] See Section 7.

OCP in reducing fertility is small, consistent with Appendix Figures 1A & 1B showing that the major national fertility decline occurred prior to the OCP. The small fertility effect of the OCP also helps to explain our null finding that the increased sex ratios were not caused by the initial introduction of the OCP.

On the margin of having a second child, it is not obvious *a priori* how land reform would affect the decision. Parents may desire more children to secure more land, but the rule of land distribution only applied for authorized births after the OCP was introduced. As a reward for compliance with the OCP, a single child received double plots of land, while as a punishment for non-compliance, above-quota births either did not receive land, or in some cases their parents' land allotment was revoked (various issues of county gazetteers). There are 73% of counties in our sample that introduced the OCP prior to or the same year as land reform, where land distribution favored the first (and single) child. To test whether land reform affected the decision to have a second child, we focus on couples during peak conception likelihood for a second child. We assign treatment status based on the year of birth of the first child and the average 3-year birth interval we find in the Census. We assume that two years after the first birth, parents made the decision whether to have a second. Suppose land reform came in year 0; the first group of parents whose decision was affected were those who had the first child in year -2. Thus, we assign 1 to the first child born 2 years prior to land reform or later and 0 otherwise.

Empirically, we find that the decision to have a second child is affected by the OCP but not land reform. In column (1)-(3) of Table 6B, controlling for the OCP, the effect of land reform on having a second child is very small and statistically insignificant, reducing concerns about endogenous sample selection. Moreover, if the "1.5 Child Policy" (which conditions on sex of first born) coincided with land reform, we would have observed a larger likelihood in having the second following a first girl with land reform. Our finding here further discounts the "1.5 Child Policy" as a confounder. In stark contrast to the sex ratio results, the effect on having a second child all loads onto the OCP and is statistically significant at the 1% level.

Regarding the timing of fertility (conditional on having a second child), we test whether land reform shortened the birth interval between the first and second child. We assign treatment status according to year of birth of the second child. From column (4) to (6), there is little change in the birth interval induced by land reform when both reforms are controlled for. Overall, we do not find evidence that fertility responses would confound our findings, along with evidence that the OCP had a quite modest (although statistically significant) fertility effect.

7 Economic Mechanisms

Why did land reform increase sex selection? A common feature of land reform in other settings is that sons inherit land. This is unlikely to explain the increased sex selection we find because China's reform did not privatize land ownership. Intergenerational transfer was (and remains) impossible. *A priori*, two remaining mechanisms are most plausible:

1. Increases in household **income** following the reform increase the demand for a son or make a son more affordable. Just as children may be a normal good [Becker, 1960], so too may having a son. In consumer theory, goods with few close substitutes tend to be normal (e.g. Black et al. [forthcoming]). In cultures with a strong son preference, a daughter is a poor substitute for a son, so achieving a son may be expected to be a normal good. Moreover, sex selection and raising a son become more affordable as income increases.

2. If males have greater **productivity** in agricultural production, land reform could increase male earnings disproportionately. There are two distinct channels through which this could increase sex ratios: i) fathers' higher earnings induced more sex selection, or; ii) parents selected sons in order to obtain the disproportionate income increase ten or more years in the future, once the son became old enough to start working.

Empirically, having a second son became more common following land reform, but only after a first daughter. The economic mechanism should account for why having sons (plural) did not increase.

7.1 Income mechanism

As noted above, land reform's best documented effects in the existing literature are its positive impacts on agricultural output and income. To test for the income mechanism, we would like to compare the sex of the second child in households with larger income gains and those with smaller gains after land reform. Unfortunately, no household-level income data are available from the 1970s to the early 1980s in China. Alternatively, we test two related predictions: 1) better educated parents who possibly gained more from land reform might sex select more; 2) higher sex ratios are observed in counties that gained more economically from the reform.

We first examine whether sex selection behavior following the reform differs by parental education. In column 1 of Table 7, we find that mothers with higher education levels were more likely to have a boy after the reform. The largest effect is found among mothers with a high school education, who are 7.4 percentage points more likely to have a son relative to those with no formal schooling. Similar to the calculation on the likelihood being a complier (Section 4.4.4 of Angrist and Pischke, 2009), we calculate the fraction of sex selectors following land reform by maternal education. We first estimate the benchmark effect of land reform on sex in the subsample of mothers with no formal schooling to be 0.016 (statistically significant at the 1 percent level). Among mothers who sex select due to land reform, 53% of them had a high school education, 27% a middle school education, and 20% a primary school education or no schooling (versus 4%, 13%, and 31% in mothers with a second child). In column 2, the education gradient among fathers is most apparent at the level of high school education, and the magnitude is smaller than that of mothers. When we control for both parents' education levels in column 3, estimates for mothers' education are robust, especially for high school education, while estimates for fathers' education are no longer statistically significant.

Better educated parents might capture larger income increases from land reform, which in turn spur more sex selection. Education improved the uses of household-supplied inputs and contributed to higher agricultural profits under the HRS (Yang and An, 2002). In Appendix Table 2, we find that counties with larger fraction of educated workers indeed have larger increase in grain output following reform. Furthermore, our education findings are consistent with a “first mover" advantage in sex selection, whereby high status parents would respond more strongly with selection because they are less susceptible to the marriage market consequence of imbalanced sex ratio (given hypergamy, women “marrying up", [Edlund, 1999]). The challenge lower status families might face in finding a wife for their son might temper their sex selection behavior.

Alternatively, parental education is likely to operate through non-income channels. For example, better educated mothers may have had more information on sex selection technologies and thus had greater effective access. We find this channel less plausible based on our findings on proximate mechanisms in Section 8. During the time period in our study, only 14% of sex selection was achieved through access to sex determination technologies (and subsequent sex-selective abortion), where information might have played a role. Among other proximate mechanisms, 33% of girls were missing due to excess postnatal female mortality. It is difficult to imagine that superior information lead educated mothers to de-select girls postnatally, particularly methods such as infanticide, abandonment, etc.

Next, using grain output data at the county level, we test the income hypothesis between counties that benefited more from reform and those that benefited less. In Panel A of Appendix Table 3, we report the estimated effect of land reform on grain output per capita in our grain sub-sample. In column 1, on average, HRS adoption increases grain output by 2.6 percent at the 10 percent significance level.[29] We stratify the sample by the change in grain output before and after reform. Column 2 shows a precisely estimated output increase of 9.2 percent in counties above the median change in grain output at the 1 percent significance level, while column 3 shows a 3.9 percent decrease at the 10 percent significance level in counties below the median. Only counties above the median experienced an increase in grain output after the reform. In the subsample of counties with grain (and land reform) data, we present the estimated effect of land reform on the second child being male in Panel B. In column 1, the magnitude of the increase in the probability of being male is 1.6 percentage points, smaller than that in our full sample. This indicates that we might underestimate the effect on male births using this grain-matched subsample. Column 2 shows an increase in probability male of 2.5 percentage points for counties above the median of the change in grain output, much larger than the overall effect in column 1. In contrast, the estimate in column 3 for counties below the median is very small in magnitude and not statistically different from zero.[30]

[29]The magnitude is smaller than the effect size found using provincial level data by Lin (1992). The outcome measure in Lin (1992) is the value of agriculture output, while ours uses only grain output thereby excluding changes in the price of grain (from price reform), as well as changes in cash crop production and price of cash crops. The effect size based on grain production and our more finely-focussed identification strategy presumably captures the lower bound of income change induced by the reform.

[30]If parents thought sex selection was “bad” but wanted to do it anyways, they might increase their practice during the disorder right after land reform. If this alternative channel dominated, we would expect the same increase in sex ratios

To summarize, our evidence on the heterogeneous treatment effects of land reform – more sex selection among better educated parents and in counties with larger income gains – is consistent with the income mechanism.

7.2 Productivity mechanism

Qian [2008] found that increases in female-specific income, as captured by the relative price increase of tea following post-Mao price reform, increased the survival rate of girls. If either higher paternal income or demand for sons' future labor were the primary force to sex select following land reform, we would expect more skewed post-reform sex ratios where the agricultural production was more male intensive.

We use two approaches to capture gender-specific productivity at the county level. First, we ascertain which crops were more or less male-labor intensive using the occupation and industry codes in the 1982 Census microdata. Overall, agricultural labor was fairly evenly divided between men and women. In Appendix Table 4 (Panel B), the county-level mean of male agricultural labor is 0.52 with a standard deviation of 0.026 across counties. It is so largely because grain production, which employed 95% of agricultural labor, was fairly gender neutral. Nevertheless, there is substantial variation in the county-level mean of males growing cash crops across counties (mean 0.52 and standard deviation of 0.23). Our first approach is to use the fraction of men growing cash crops by county to proxy for demand for male labor at the time of the reform. Among the main cash crops, cotton was the most female labor intensive: 35% of workers who grew cotton were male. Fruit appears to have been most male labor intensive: 69% of workers who grew fruit are male.

A potential concern is that crop choices might change after the reform when households could make their own production decisions. To provide a relatively exogenous measure for gender specific income, our second approach uses crop suitability indices based on agro-climate conditions from the FAO Global Agro-Ecological Zones (GAEZ) 2012 database. FAO calculated an estimate of the potential yield of each crop and crop suitability in each 0.5-degree-by-0.5-degree grid cell, given an assumed level of crop management and input use.[31] We aggregate the crop suitability indices to the county level. We focus on three sets of crops: 1) cotton, a female intensive crop; 2) fruits including citrus and banana, male intensive crops; 3) grain including wheat and wetland rice, the gender neutral crops. Our second approach is to compare the land reform effect on sex between "cotton friendly" counties and "fruit friendly" counties.

In Table 8, we attempt to isolate male income. Column 1 reports the coefficient on the interaction of land reform, the first child being a girl, and the fraction of male workers growing cash crops by county. It is statistically insignificant and economically very small: an increase of 0.01 percentage points, that is, a 10 percent increase in the fraction of male workers leads to a 0.1 percent increase in the probability of

regardless of changes in grain output.

[31] The crop suitability indices are based on intermediate input level. Water supply is rain-fed. Each index scales from 1 to 7, the higher the more suitable. Scale 1 indicates water, not suitable or very marginal, 2 for marginal, 3 for moderate, 4 for medium, 5 for good, 6 for high, and 7 for very high.

second child being male. This estimate increases to 0.02 percentage points in column 2 when we control for the interaction term with the fraction of male workers growing grain, but again it is not statistically significant. The estimate is fairly precise (standard error of .0002). In column 3, we compare the reform effect between counties more suitable for female-intensive crop and those more suitable for male-intensive crops, while suitability of gender-neutral crops is controlled for. None of these estimates are statistically significant. One index of a male-intensive crop, citrus, has a positive sign. However, the index of the female-intensive crop, cotton, also has a positive sign. Thus, we do not see much heterogeneity according to gendered agricultural earnings (compared to heterogeneity by maternal education or grain output).

Overall, neither gender-specific income nor demand for future gender-specific labor appears to be a plausible mechanism for the sex selection effect. Alternatively, evidence in this subsection is consistent with an increase in total household income.

7.3 Other economic mechanisms

This subsection examines another four possible channels through which land reform might affect sex ratios. None of these mechanisms is supported by our empirical evidence.

1. Was land distribution male biased?

Men and women had equal rights in land distribution. However, absent central oversight of women's land rights after marriage, there is anecdotal evidence that local rules might favor males. For example, when a daughter married out of her village, her plot of land was taken back by the village; getting a new plot in the village she married might not be automatic (Bossen, 2002). If women in fact received less land because of expropriation at marriage, it is perhaps less surprising to observe rising sex ratios following a reform that so directly favors males. If expropriation was common practice across China, we would expect that on average families with more males would have more land *within the village*, the administrative unit where land allocation and reallocation (due to household demographic changes) were implemented.

Unfortunately, we do not observe land holdings in the 1990 Census data. We test whether men had more land in two rural household surveys in the 1980s: the 1989 Chinese Health and Nutrition Survey (CHNS) that covers nine provinces and the 1986-89 Rural Fixed Point Survey that is nationally representative.[32] Using the CHNS 1989 wave in Panel A of Appendix Table 5, we find that, within village, having more male members has a very small effect on size of land farmed by the household (a 50% increase in the fraction of males increases household land size by 0.1 *mu*, or a 3% increase compared to the sample mean), which is not statistically significant. Furthermore, we test whether possible land reallocation in a 4-year window favored families with an increase in the fraction of adult

[32]The Chinese Household Income Project Survey (CHIPS) 1988 also has information on household land size and gender composition. We do not use CHIPS 1988 because the smallest administrative unit is county, and therefore we cannot conduct the analysis within village.

males (if daughters "marry out") using the 1986-89 Rural Fixed Point Survey. From the household-level fixed effect estimator in Panel B, we find no evidence that changes in household land size are correlated with changes in the fraction of male labor.

One might argue that parents *feared* losing the land of a daughter, despite the lack of empirical evidence to support the expectation. We do not think it is plausible because of the short duration of land leases when the HRS was introduced. As documented in various county gazetteers, the initial reform granted a 3-5 year lease to individual households. In 1984, the central government officially extended the lease to 15 years. If parents had any expectation on the land rights of their children, it would not be beyond 15 years, when their children would still be too young to get married.

2. Extension of land lease in 1984

The subsequent extension of land leases to 15 years in 1984 might have substantially changed families' expectation of future income. If families waited until the extension to respond with sex selection, we should observe a large increase in sex ratios in 1984. We plot sex ratios of the second child by year of birth in Appendix Figure 4. There is no obvious change in the slope of the sex ratio following a first girl; the first increase in sex ratios occurred a few years before 1984. In Appendix Table 6, we interact the indicator of born 1984-86 with the girl first dummy to capture the effect of the land lease extension in 1984. The estimate is an increase of 1.3 percentage points, but it is not statistically significant. Moreover, including this interaction causes little change to the estimate of the (larger) land reform effect (0.032, consistent with Table 4 results).

3. Increase in demand for old age support

Another interpretation is that land reform destroyed the financial basis of the "state pension system". Its destruction then forced parents to rely on sons (instead of the collective or state) for old age support. If demand for sons were driven by collapse of collective support, we would expect that initially poor families, or families that gained less from the reform, were more in need of financial support from sons, and thus were more likely to select sex. Because we do not have a income or wealth measure prior to reform, we cannot test this hypothesis at the household level. At the county level, our findings in Section 7.1 show the opposite: counties that experienced more output gains have a substantially larger increase in sex ratios after the reform. Furthermore, in Appendix Table 7, we present evidence on heterogeneous effects by initial economic conditions at the county level. Similarly, initially-rich counties also had more boys born after the reform. An increase in demand for old age support can not be easily reconciled with these findings.

4. Collapse of rural medical system

The rural medical system of Mao's era also came to its end after the reform. A resulting concern is that parents might respond to the negative healthcare shock differently for boys and for girls. If the cutoffs in health care supply had any effect on child survival, it would be the opposite to the effect of income growth. Although we cannot directly separate these two offsetting channels, we can test the net effect of the reform on infant health outcomes in the UNICEF 1992 Chinese Children Survey (no health indicators in the census data). The survey covers 522,371 households from 1088 counties in 29 provinces.

In Appendix Table 8, Panel A reports estimates for all births. We find that neonatal mortality decreased by 0.4 percentage points and postneonatal mortality decreased by 0.3 percentage points, and birth weight increased by 34 grams (statistically significant at the 10% level, but a 1% effect relative to sample mean). These findings indicate that the impact of the change in health care supply, if any, would not offset the health benefits of land reform. To compare the effects on health outcomes with our main estimates on sex ratio, we focus on the second births in Panel B. Using the sample of all second births, there is little evidence that the effects of land reform on health outcomes differ by the sex of the first child.

We do not find evidence that the large increases in sex ratios coincided with a major deterioration in childhood health caused by compromised rural healthcare. Again, the large improvement in birth outcomes is consistent with increased income and reduced poverty improving health.

8 Proximate Mechanisms

How did land reform increase sex selection? Small deviations from normal sex ratios (around 1.05) occur "naturally" due to biology, e.g. Norberg [2004]; Almond & Edlund [2007]. Large increases in population sex ratios are generally accepted as behavioral, i.e. they reflect discriminatory decisions made in response to knowledge of offspring sex [Duflo, 2012]. Sex selection behavior includes sex-selective abortion, infanticide, adoption, and differential investment, including neglect and abandonment. Parents might prefer to conceal such behaviors, and as such detecting them a sleuthing exercise in "forensic economics" [Zitzeqitz, 2012]. In general, direct observation of such behaviors is impossible.[33] Compounding matters, we only observe the sex of children in census microdata, not at birth, making it more difficult to distinguish prenatal versus postnatal behaviors. A convenient feature of our study from a forensic perspective is that the sex ratio has both ordinal and cardinal properties: ratios substantially above 1.05 were presumably achieved through a combination of these responsive behaviors. Below we provide indirect evidence related to two proximate mechanisms: sex-selective abortion following prenatal ultrasound and postnatal mortality. Their analysis and the omission of other mechanisms below is dictated by the data available for this time period.

[33] A possible exception is Gu and Li [1996], who observed the sex of aborted fetuses in southern *Zhejiang* province, finding more female fetuses were aborted following a female live birth.

8.1 Ultrasound availability in provincial capitals

Was sex-selective abortion possible? Land reform generally preceded the arrival of ultrasound machines in rural China, while ultrasound was largely available in provincial capitals from the late 1970s (Figure 4). We consider rail access as it was the main means of long-distance transportation at that time.

Using a digitized national map of railroad networks in 1980 (generously provided by Matthew Turner),[34] we define railroad access by whether a railroad line passed through a rural county. Every county on a railroad line was connected to the capital city of the same province. 36% of counties had railroad access. We assign access to ultrasound technology as 1 if a county was connected by railroad to the provincial capital that had ultrasound machines available one year after land reform or earlier, and 0 otherwise. Counties that are assigned 0 either had no railroad passing through or they had railroad linked to the provincial capital but ultrasound machines were not available there yet, or both.

In column 1 of Table 9, the land reform effect on sex is 2 percentage points higher if parents could take the train from their home county to the provincial capital to access ultrasound machines. When we compare the estimate of land reform, 0.025, to our main estimate 0.029 in column 3 of Table 3, prenatal sex determination through our measure of rail access to ultrasound could explain 14% of the increase in sex ratios induced by land reform.

A potential concern is that railroad access might also help peasants to connect to a larger input/output market and hence increase their income, another interpretation of the results in column 1. To isolate the effect of access to ultrasound in provincial capitals from other channels, we include the interaction of land reform, girl first ,and railroad to province capital in column 2. Absent ultrasound technology in the provincial capital, rail access does not seem to increase sex ratios following land reform. The effect of access to ultrasound technology is larger (.023) once the railroad access is accounted for, suggesting that the main channel railroad access contributed to higher sex ratios is through access to ultrasound technology.

8.2 Reduced male mortality after birth

The UNICEF 1992 Chinese Children Survey allows us to consider postnatal mortality. The Survey will miss female infanticide to the extent that their live births were not reported in the Survey. Following a first daughter, we do not find an effect of land reform on the overall mortality of second births 1977-1986 (column 1, Appendix Table 9). However, this masks heterogeneity by gender of the second child. Male mortality decreased 2.3 percentage points in column 2, while the estimate for female mortality after land reform is positive but not significant in column 3. These findings suggest that male children benefited from the increased household income after the reform, but female children did not.

Using these point estimates and the roughly 3% baseline mortality rate, a back-of-the-envelope calculation indicates that the reduction in male mortality induced by land reform would increase the sex ratio from 1.05 to 1.073. The sex ratio in our main sample increased from 1.06 prior to land reform

[34]Digitized from SinoMaps Press (1982) and used in Baum-Snow, Brandt, Henderson, Turner and Zhang (2012).

to 1.13 after the reform. Therefore, roughly 33% of girls were missing due to postnatal reduced male mortality after the reform.

In sum, we find that sex-selective abortion via "provincial" ultrasound and excess female mortality accounted for 47% of the increase in sex ratios following land reform. This suggests that remaining selection methods, e.g. infanticide, abandonment, prenatal sex determination by other technologies or locations, etc., might account for a little more than half of the sex ratio imbalance.

9 Discussion

We find that the post-Mao land reform increased the number of missing girls by more than 1.24 million over its first six years. In so doing so, we challenge two core beliefs about sex selection.

First, the argument that the One Child Policy (OCP) raised sex ratios is plausible *a priori*: fewer parents can have a son by chance if families are small. But fertility rates were cut in half during the 1970s (Appendix Figure 1A & 1B), i.e. prior to the introduction of OCP incentives and penalties. This historic fertility decline was not reflected by an increase in sex ratios (Figure 1). Furthermore, we collect the most comprehensive county-level dataset to date and find that while the OCP did reduce fertility in rural counties (home to 86% of China's population at the time), its impact was very small. Whatever modest impact it appears to have on sex selection is eliminated once land reform is accounted for. Coverage of the recently-announced OCP relaxation regularly invokes the Policy's role in "missing girls" [*Xinhua News Agency* (the official press agency of China), Nov. 2013; *USA Today,* Nov. 2013].[35] To the extent that the introduction of the rural OCP is taken as evidence for this connection, our findings suggest otherwise. Indeed, fertility in Hong Kong and Taiwan is well below replacement levels in the absence of a OCP, so the opportunity to have a son by chance may not change appreciably even if the OCP is relaxed or eliminated.

Second, it is commonly argued that development will help eliminate gender disparities [World Development Report 2012]. While previous work has shown that lowering the cost of sex selection can increase sex selection, this usually refers to a narrow facet of development: diffusion of prenatal sex determination technologies. Indeed, policy-makers in Asia have considered restricting access to such technologies as a solution to high sex ratios. India started to ban ultrasound in prenatal sex determination as early as 1994 and China issued a similar law in 2003. But prenatal sex determination technology continues to evolve and may be increasingly difficult to regulate.[36] While banning its use may send an important message, it is unclear whether it will provide much of a practical obstacle. In our analysis, sex selection increased even when ultrasound access did not. Our findings suggest that given a cultural preference for sons [Almond, Edlund, & Milligan, 2013], development more generally may not eliminate "missing girls", and therefore the phenomenon is more intractable than realized.

[35] http://news.xinhuanet.com/english/china/2013-11/15/c_132891920.htm
http://www.usatoday.com/story/news/world/2013/11/15/china-one-child-policy/3570593/

[36] For example, see Devaney et al. [2011] on recent advances in non-invasive fetal sex determination.

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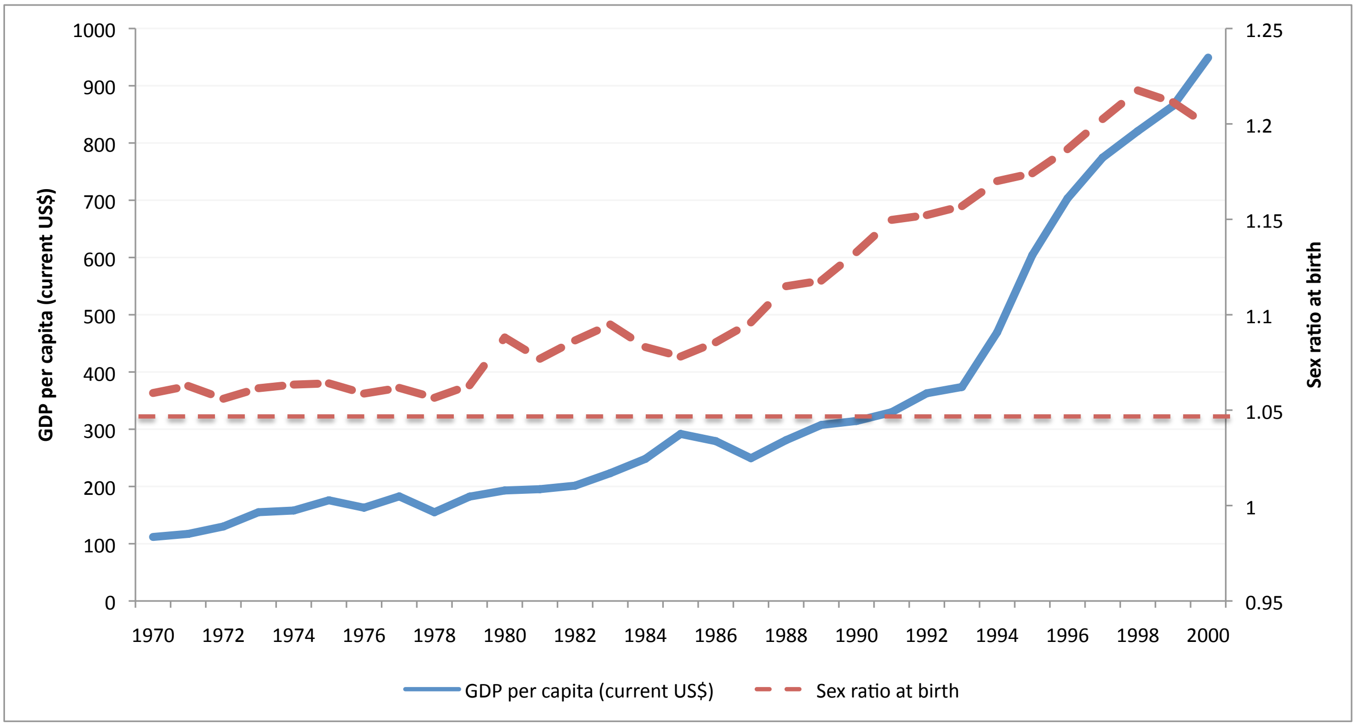
Figure 1: GDP per capita and sex ratio at birth in China: 1970-2000



Notes: 1) Data on GDP per capita (current US$) are from World Bank; 2) Data on sex ratios at birth in 1970-1981 are from the 1% sample of the 1982 Census, 1982-1989 data are from the 1% sample of the 1990 Census, and 1990-2000 data are from the 1% sample of the 2000 Census. 3) The horizontal line is at sex ratio of 1.05, the biologically normal rate.

Figure 2A: Sex ratio of the first child

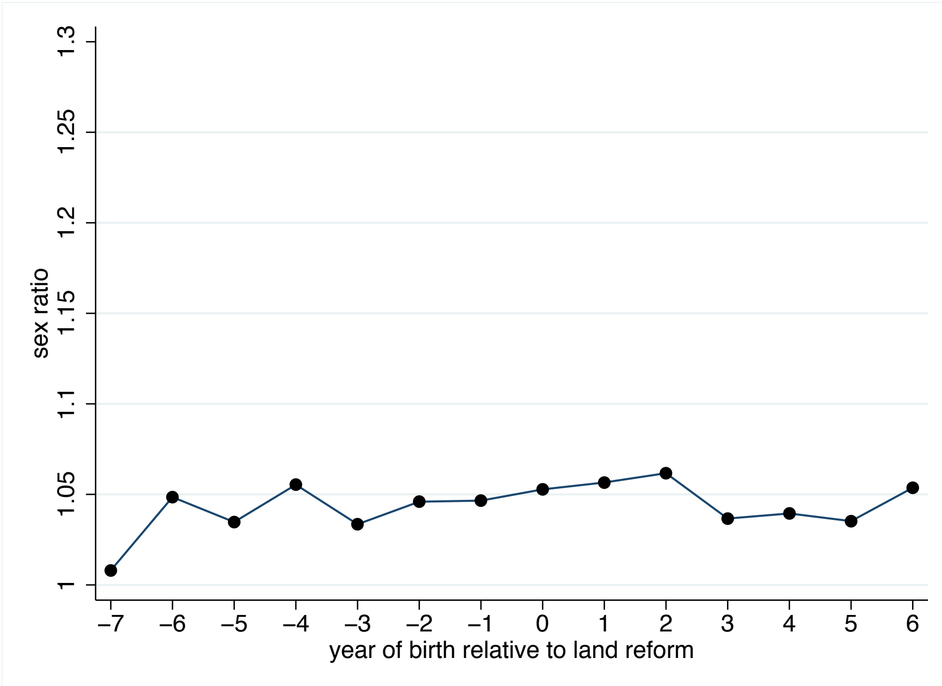


Figure 2B: Sex ratio of the second child

Note: Figure 2A and 2B are unadjusted figures, plotting sex ratios by the year of birth relative to land reform.

Figure 3A: County-level rollout of land reform and the One Child Policy, and Provincial rollout of the 1.5 Child Policy

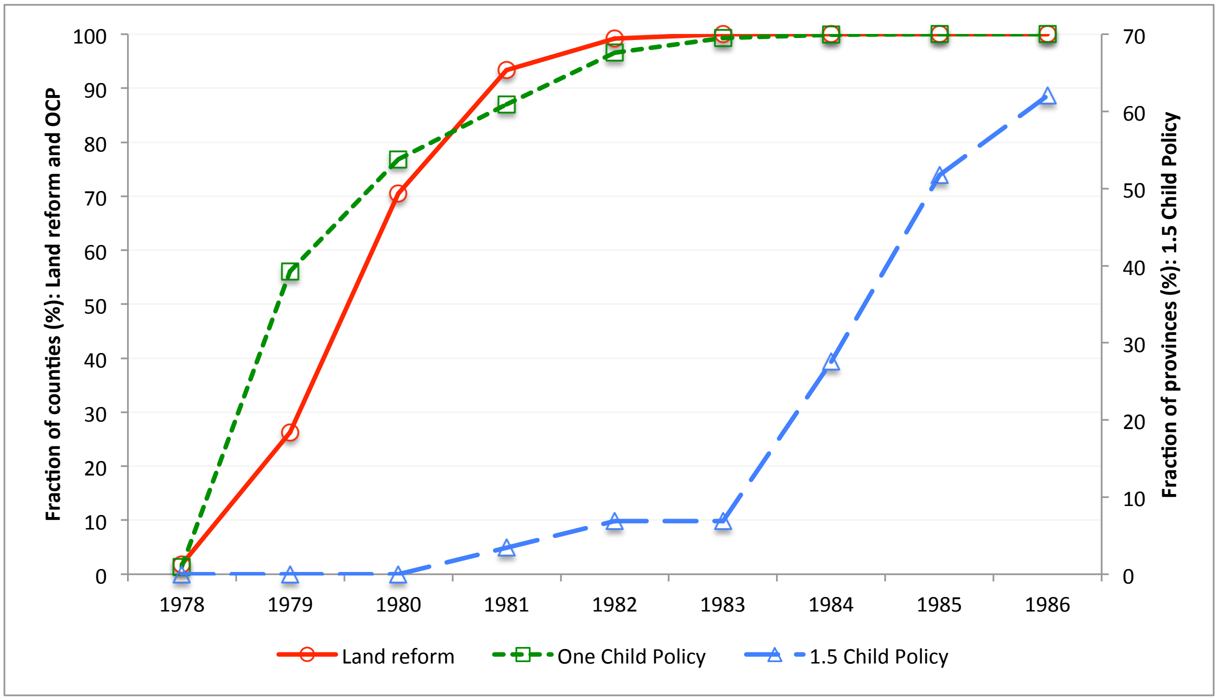
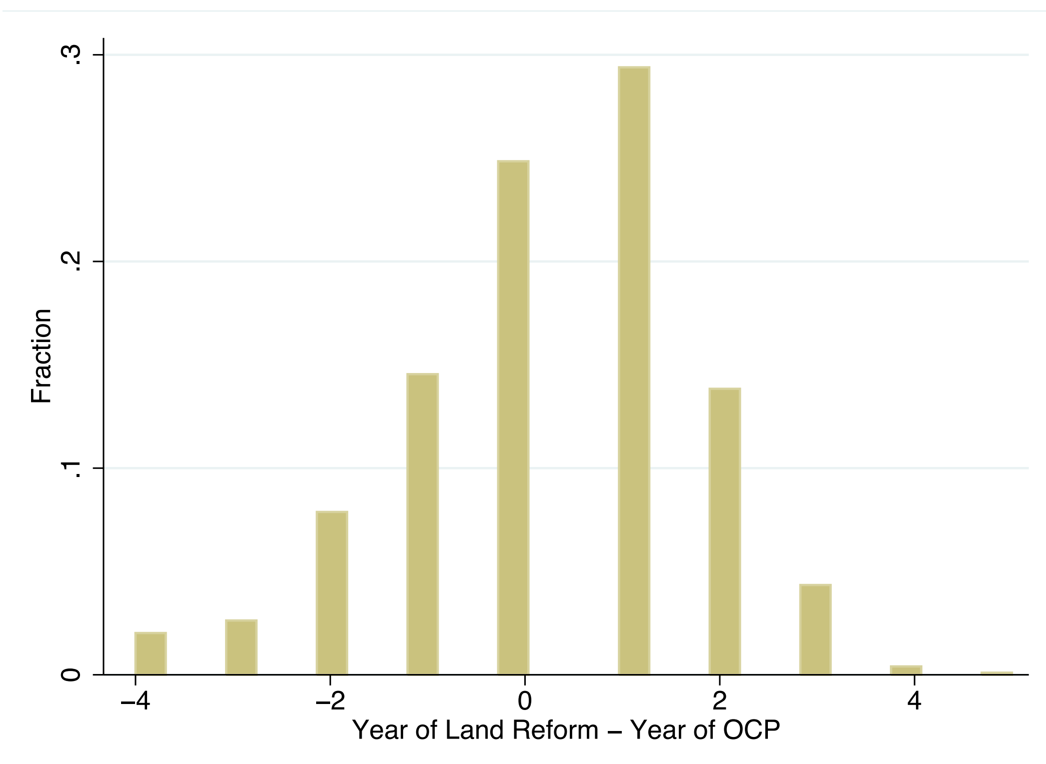


Figure 3B: Difference between land reform start year and the OCP start year



Note: Figure 3B shows the distribution of the difference between land reform start year and the OCP start year.

Figure 4: Rollout of land reform and ultrasound technology

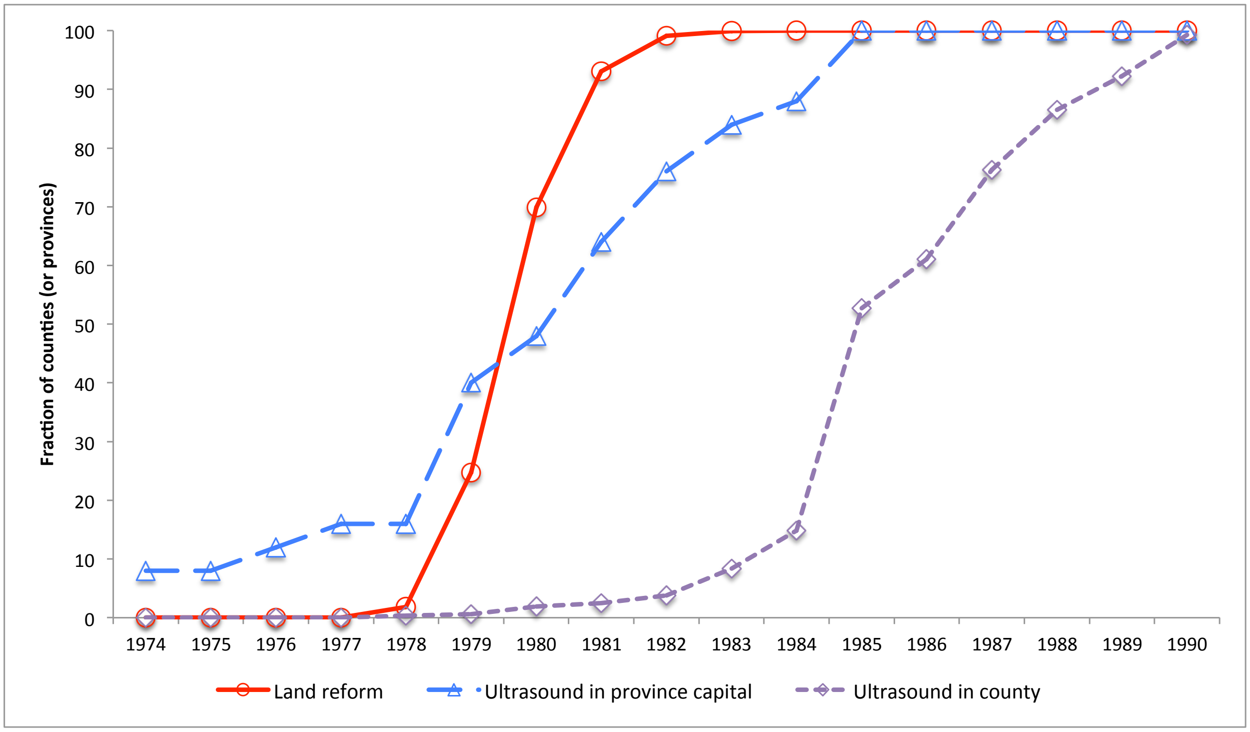


Table 1A: Time-invariant determinants of reform timing

	Dependent variable: first year of land reform (1978-1984)				
	Univariate			Multivariate	
		Obs	R-squared		
ln (grain output per capita 1976)	0.250**	481	0.011	0.400***	
	[0.121]			[0.126]	
ln (distance to province capital)	0.075**	1,201	0.003	-0.003	-0.039
	[0.036]			[0.061]	[0.039]
ln (labor force density 1976)	-0.147***	1,117	0.044	-0.172***	-0.149***
	[0.022]			[0.045]	[0.028]
ln (famine intensity 1959-1961)	-0.494***	1,189	0.033	-0.291**	-0.349***
	[0.081]			[0.144]	[0.089]
ln (distance to beijing)	-0.074*	1,201	0.003	-0.127	-0.134***
	[0.038]			[0.078]	[0.041]
ln (sex ratio at birth 1975-77)	-0.135	1,193	0.001	-0.198	-0.235
	[0.144]			[0.214]	[0.145]
Observations				438	1,114
R-squared				0.096	0.072

Notes: The dependent variable is the first year of land reform, which varies from 1978 to 1984. For univariate analysis, each estimate is from a separate regression. Multivariate regressions include all independent variables. Data on grain output per capita in 1976 are collected from county gazetteers: only 438 counties report this information. Distance to Beijing and distance to province capital city are in kilometers and are obtained from a GIS map of 1982 Census. Labor force density in 1976 is calculated by population size aged 16-60 in 1976 divided by area. Using the 1982 Census, we measure the 1959-61 famine intensity by the average cohort size born in 1953-1957 divided by the average cohort size born in 1959-1961. Sex ratios at birth for birth cohorts 1975-77 are from the 1982 Census. Robust standard errors are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 1B: Droughts (time-variant) and reform timing

	Dependent variable=1 for the first year of reform, 0 before reform and missing after the first year				
	(1)	(2)	(3)	(4)	(5)
	March-September	March	April	May	June
Drought in year t	-0.011	-0.021***	-0.037***	-0.006	-0.004
	[0.009]	[0.008]	[0.008]	[0.008]	[0.009]
Drought in year t-1	0.001	-0.026***	-0.027***	0.004	-0.009
	[0.008]	[0.007]	[0.008]	[0.008]	[0.008]
County FE	X	X	X	X	X
Year FE	X	X	X	X	X
County linear trend	X	X	X	X	X
Observations	7,306	7,306	7,306	7,306	7,306
R-squared	0.768	0.769	0.769	0.768	0.768

Notes: The dependent variable is 1 for the first year of reform, 0 prior to the reform, and missing value after the first year. Drought is a dummy variable which is equal to 1 if the average monthly precipitation is below the bottom 20th percentile in the precipitation distribution during 1957-1984 and 0 otherwise. We include two drought indicators, one in the current year and another the year before. In the first column we measure drought using monthly average precipitation from March to September. Each of the other column headings presents the single month in which drought is measured. All regressions include county fixed effects, year effects and county linear trends. The sample includes 1194 counties and the time span is from 1975 to 1984. Robust standard errors are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 2: Summary Statistics

	Births between 1974 and 1986			
	Full sample		Two-parent sample	
	first child	second child	first child	second child
Boy	0.511	0.523	0.511	0.523
Girl first		0.507		0.507
Exposed to land reform	0.541	0.511	0.545	0.519
Mother No formal schooling			0.468	0.522
Mother Primary school			0.260	0.308
Mother Middel school			0.197	0.133
Mother High school			0.075	0.037
Father No formal schooling			0.327	0.324
Father Primary school			0.169	0.268
Father Middle school			0.343	0.294
Father High school			0.160	0.114
Observations	371762	279069	349351	260529

Table 3: Land reform and sex ratio

	Male=1				Sex ratio (county-year)
	(1)	(2)	(3)	(4)	(5)
	First child	Second child			Second child
				Han only	
Land reform*Girl first		0.029***	0.029***	0.033***	0.138***
		[0.004]	[0.004]	[0.004]	[0.044]
Land reform	0.003	-0.010*			
	[0.004]	[0.006]			
Girl first		0.027***	0.027***	0.028***	0.144***
		[0.003]	[0.003]	[0.003]	[0.030]
County FE	X	X			
YOB FE	X	X			
Initial control*YOB FE	X	X			
Spring drought in t and t-1	X	X			
County-specific linear trends	X	X			
County * YOB FE			X	X	X
Dependent variable mean	0.511	0.523	0.523	0.524	1.27
Observations	371762	279069	298755	267570	24,255
R-squared	0.006	0.011	0.052	0.053	0.552

Notes: Column (1) reports estimate for the effect of exposure to land reform on the probability of first child being male; column (2) and (3) for the effect on second child being male for all second births, column (4) for the effect on second child being male for Han Chinese only. Column (5) reports results on sex ratio of all second births by county, birth year and sex of the first child. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing and initial controls. Regressions in column (3)-(5) include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4: Land reform versus the One Child Policy

	Male=1					
	All			Han		
	(1)	(2)	(3)	(4)	(5)	(6)
Land reform*Girl first	0.030***		0.033***	0.035***		0.039***
	[0.005]		[0.008]	[0.005]		[0.008]
OCP*Girl first		0.024***	-0.004		0.027***	-0.005
		[0.004]	[0.008]		[0.005]	[0.008]
Girl first	0.024***	0.027***	0.025***	0.025***	0.028***	0.026***
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]
County*YOB FE	X	X	X	X	X	X
Observations	241547	241547	241547	199423	199423	199423
R-squared	0.051	0.051	0.051	0.052	0.052	0.052

Notes: The sample in column (1)-(3) includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP, and the sample in column (4)-(5) includes all second births of Han ethnicity. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5 : Holding the OCP constant and holding land reform constant

	male=1			
	Holding OCP constant		Holding land reform constant	
	(1)	(2)	(3)	(4)
	OCP is not in effect	OCP is in effect	HRS is not in effect	HRS is in effect
Land reform*Girl first	0.021*	0.039***		
	[0.013]	[0.010]		
OCP*Girl first			-0.009	0.008
			[0.010]	[0.013]
Girl first	0.025***	0.016*	0.025***	0.047***
	[0.004]	[0.010]	[0.004]	[0.012]
County*YOB FE	X	X	X	X
Observations	104374	120226	109089	115511
R-squared	0.054	0.048	0.054	0.048

Notes: Column (1) includes individuals born before the OCP came in. Column (2) includes individuals born after the OCP came in. Column (3) includes individuals born before the land reform came in. Column (4) includes individuals born after the land reform came in. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6A: Fertility response (1) - number of births

	Number of births by county and year		
	(1)	(2)	(3)
Land reform	2.333**		2.277**
	[1.101]		[1.104]
OCP		-2.824**	-2.783**
		[1.101]	[1.097]
Dependent variable mean		90	
Observations	11137	11137	11137
R-squared	0.948	0.948	0.949

Notes: The sample is at the county-birth year level, including birth cohorts between 1974 and 1986 in counties that are matched with data on timing of land reform and the OCP. All regressions include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6B: Fertility response (2) - decision to have a second child and birth interval

	Have second child=1			Birth interval between 1st and 2nd		
	(1)	(2)	(3)	(4)	(5)	(6)
Land reform*Girl first	0.026***		-0.009	-0.030*		0.018
	[0.004]		[0.009]	[0.016]		[0.027]
OCP*Girl first		0.039***	0.046***		-0.043***	-0.057**
		[0.004]	[0.009]		[0.016]	[0.028]
Girl first	0.040***	0.030***	0.031***	-0.178***	-0.171***	-0.172***
	[0.004]	[0.003]	[0.003]	[0.011]	[0.011]	[0.011]
County*YOB FE	X	X	X	X	X	X
mean of dependent variable		0.82			2.9	
Observations	298770	298770	298770	224600	224600	224600
R-squared	0.37	0.37	0.37	0.135	0.135	0.135

Notes: The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and the OCP. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 7: Treatment effect heterogeneity, by parental education

	Dependent variable: Male=1		
	(1)	(2)	(3)
Land reform*Girl first*Mother High school	0.074***		0.065***
	[0.024]		[0.025]
Land reform*Girl first*Mother Middle school	0.026*		0.024*
	[0.013]		[0.014]
Land reform*Girl first*Mother Primary school	0.005		0.005
	[0.009]		[0.010]
Land reform*Girl first*Father High school		0.035**	0.019
		[0.016]	[0.017]
Land reform*Girl first*Father Middle school		0.008	-0.001
		[0.012]	[0.012]
Land reform*Girl first*Father Primary school		0.003	0
		[0.011]	[0.011]
Land reform*Girl first	0.019***	0.019**	0.017*
	[0.006]	[0.009]	[0.009]
Observations	279065	279065	279065
R-squared	0.055	0.055	0.055

Note: Land reform*Parental eduation, Girl first*Parental education and Parental education are also controlled for.
This table reports estimate for the effect of exposure to land reform on the probability of second child being male by parental education. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on land reform timing. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 8: Treatment effect heterogeneity, by the fraction of male workers or crop suitability

	Male=1		
	(1)	(2)	(3)
A. % Male workers by county in the 1982 Census (Appendix Table 1)			
Land reform*Girl first*% Male growing cash crop	0.0001	0.0002	
	[0.0002]	[0.0002]	
Land reform*Girl first*% Male growing grain		-0.0004	
		[0.0004]	
B. Average crop suitability index by county from FAO GAEZ			
Land reform*Girl first*Cotton suitability index			0.007
			[0.005]
Land reform*Girl first*Citrus suitability index			0.008
			[0.011]
Land reform*Girl first*Banana suitability index			-0.005
			[0.011]
Land reform*Girl first*Wheat suitability index			0.003
			[0.006]
Land reform*Girl first*Wetland Rice suitability index			-0.011
			[0.014]
Observations	271772	271263	295482
R-squared	0.052	0.052	0.052

Notes: The fraction of male workers growing cash crop or grain by county is constructed using occupation and industry codes in the 1982 Census microdata (see also Appendix Table 1). Average crop suitability index by county is aggregated using data from the FAO GAEZ Data Portal version 3.0 (2012 May). The suitability index (for intermediate input level rain-fed) is from 1 to 7, the higher the more suitable. The sample includes individuals born between 1974 and 1986 in counties that are matched with the county-level data on reform timing. Regressions in column 1 and 2 also include fraction of male*land reform, fraction of male*girl first, and girl first*land reform. Regression in column 3 also includes each crop index*land reform, each crop index*girl first, and girl first*land reform. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 9: Railroad access to province capital cities that had ultrasound machines

	male=1	
Land reform*Girl first*Railroad to province capital where ultrasound came in 1 year after land reform or earlier	0.020*	0.023*
	[0.010]	[0.013]
Land reform*Girl first*Railroad to province capital		-0.005
		[0.011]
Land reform*Girl first	0.025***	0.026***
	[0.004]	[0.005]
Observations	298755	298755
R-squared	0.052	0.052

Notes: In column (1), Girl first, Land reform*Railroad to province capital that had ultrasound and Girl first*Railroad to province capital that had ultrasound are also controlled for. In column (2), additionally, Land reform*Railroad to province capital and Girl first*Railroad to province capital are also controlled for. The sample includes counties that are matched with county-level data on land reform. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

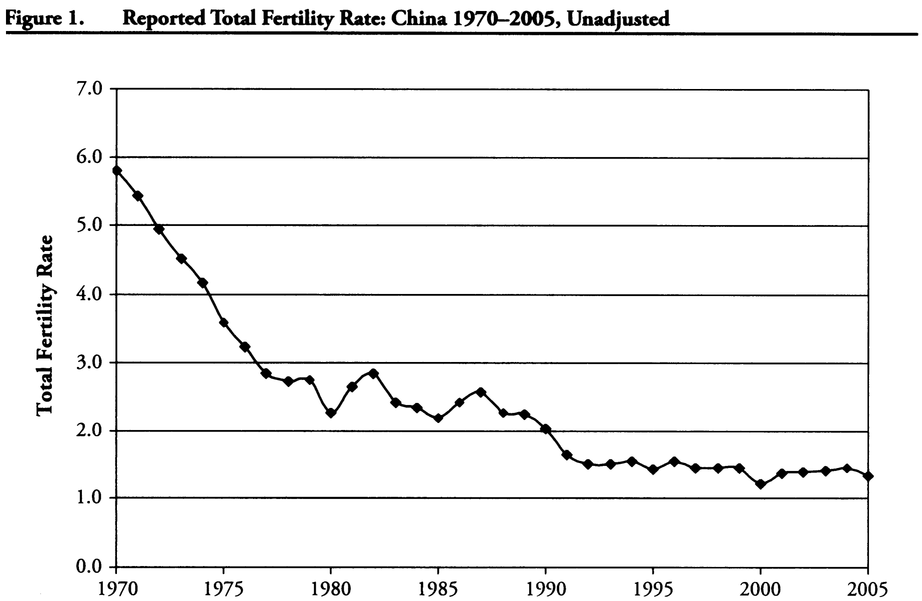
Appendix

Data Appendix: Precipitation Data

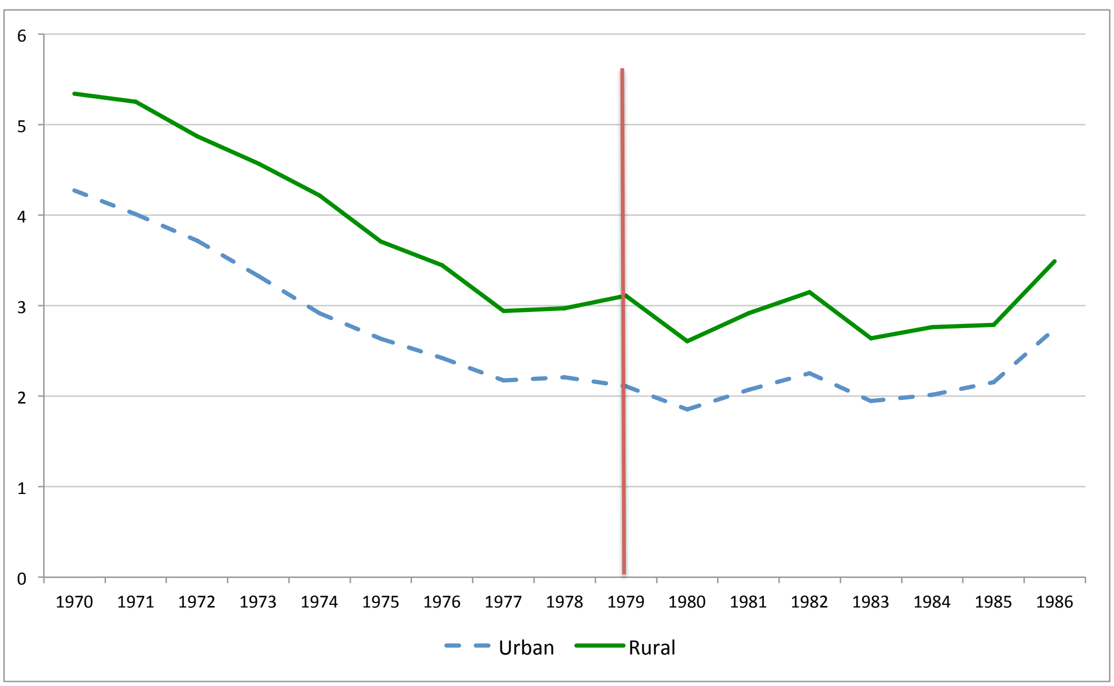
We use the Global Surface Summary of Day data produced by the National Climate Data Center (NCDC). Throughout China, daily data on the total precipitation amount (to 0.01 inches) are available from 225 weather stations from 1956 to 1964 and 536 stations from 1973 to 1984. In each year, we assign each county in the 1982 Census the precipitation data from the nearest weather station using longitude and latitude. Because the number of weather stations increases overtime, a county might be assigned different stations in different years, with relatively closer stations in more recent years.

To construct the measure of drought in March, for example, we first generate the distribution of total precipitation in March from all years during 1956-1964 and 1973-1984 for each county. We then define drought in March as a binary variable that is equal to 1 if the monthly precipitation is below the bottom 20 percentile of the distribution for each county in each year and 0 otherwise. For drought in the whole growing season, we calculate the average monthly precipitation from March to September and use its distribution to define drought.

Appendix Figure 1A: Total Fertility Rate, 1970-2005 (Cai, 2008)

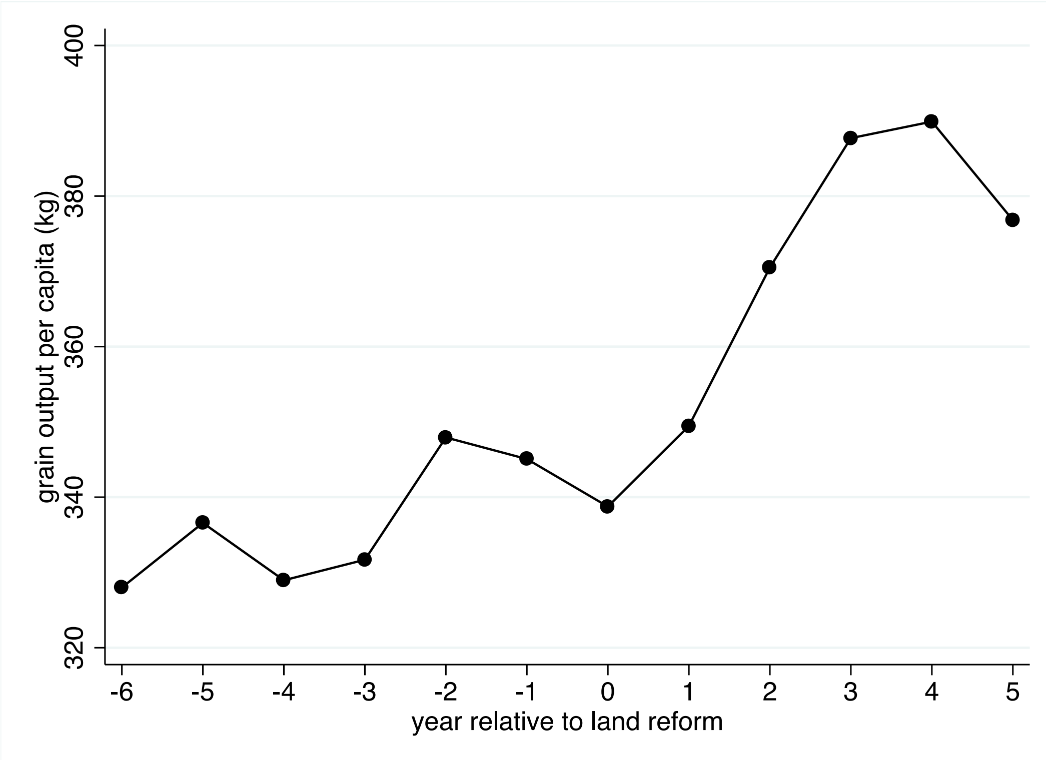


Appendix Figure 1B: Total Fertility Rate by Rural/Urabn, 1970-1986



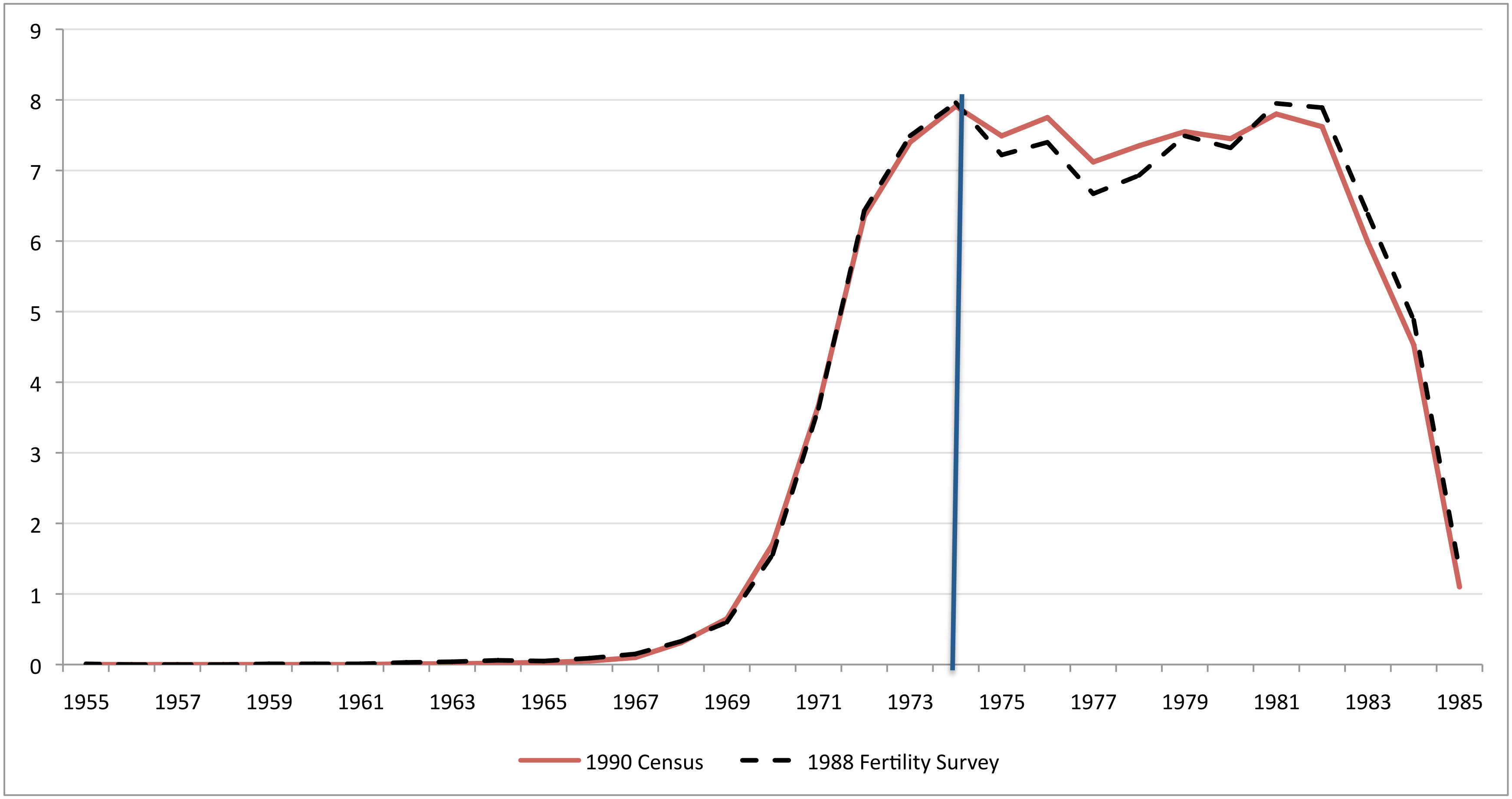
Note: Appendix Figure 1B is plotted by the authors using data from the 10% sample of the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives. The vertical line is at year 1979.

Appendix Figure 2: Grain output per capita



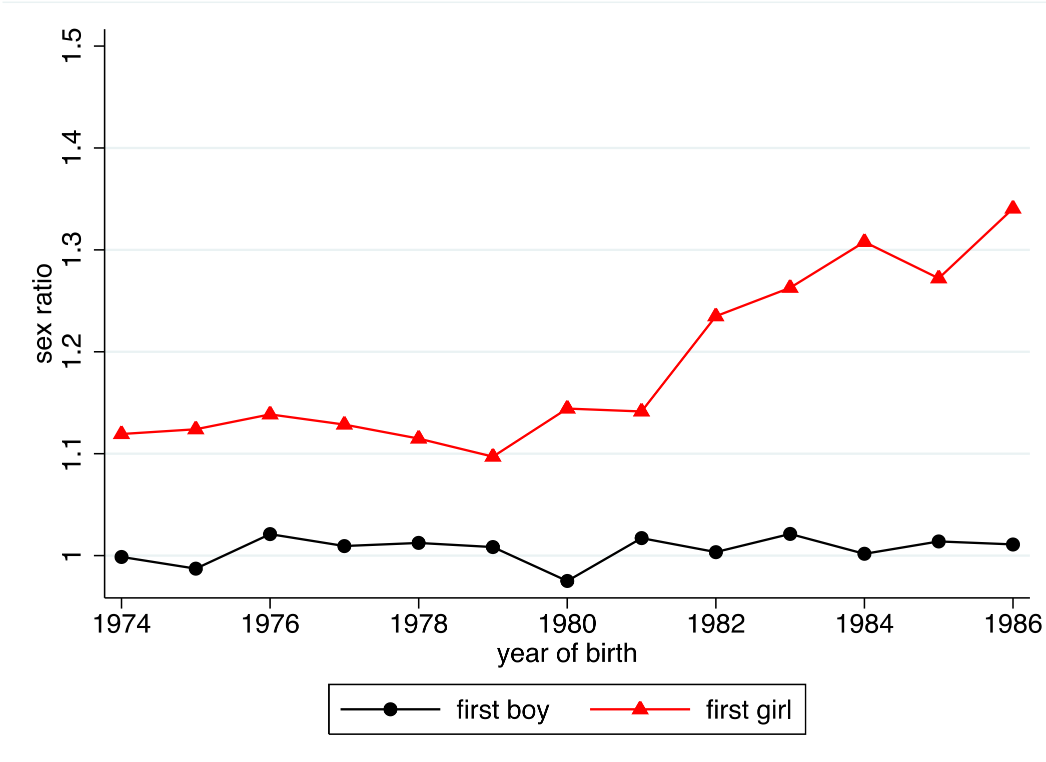
Note: The sample includes 400 counties that we have data on both land reform timing and grain output per capita from the 1970s to 1980s.

Appendix Figure 3: Frequency of birth year distribution of the first child



Note: The solid line is from the 1% sample of the 1990 Census. The dotted line is from the 10% sample of the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives.

Appendix Figure 4: Sex ratio of the second child, by year of birth



Appendix Table 1: Land reform, the OCP and the 1.5 Child Policy

	Male=1
Land reform*Girl first	0.030***
	[0.008]
OCP*Girl first	-0.006
	[0.008]
1.5 Child Policy*Girl first	0.022***
	[0.008]
Girl first	0.025***
	[0.003]
Observations	241547
R-squared	0.051

Notes: 1.5 Child Policy is assigned 1 if one was born after the 1.5 Child Policy started in the province of birth and 0 otherwise. The sample includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 2: Grain output by the fraction of educated workers

	ln(grain output per capita)		
Land reform*% High school	0.008*		
	[0.004]		
Land reform*% Middle school		0.004**	
		[0.002]	
Land reform*% Primary school			0.001
			[0.001]
Land reform	0.022	-0.009	0.034
	[0.035]	[0.043]	[0.061]
Observations	2,093	2,093	2,093
R-squared	0.906	0.906	0.906

Notes: Estimation in this table uses the sample of counties that are above the median of productivity change. All regressions control for county fixed effects, year effects, county-specific linear time trends, determinants of reform timing interacted with time fixed effects and droughts in March and April in year t and t-1. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 3: Treatment effect heterogeneity, by changes in grain output

	Sample: 400 counties		
	(1)	(2)	(3)
	Full sample	Change in grain output above median	Change in grain output below median
	Panel A: ln(grain output per capita)		
Land reform	0.026*	0.092***	-0.039*
	[0.015]	[0.019]	[0.021]
Observations	4,188	2,093	2,095
R-squared	0.874	0.905	0.818
	Panel B: Male=1		
Land reform*Girl first	0.016**	0.025**	0.005
	[0.008]	[0.010]	[0.012]
Girl first	0.029***	0.030***	0.027***
	[0.005]	[0.006]	[0.008]
Dependent variable mean	0.521	0.524	0.519
Observations	93183	53243	39940
R-squared	0.053	0.047	0.062

Notes: Estimation in this table uses the sample of 400 counties that report grain data. Panel A reports reports estimates of land reform on log grain output per capita by county and year (1974-1984), and panel B reports estimates of land reform on second child being male at the individual level. Column (1) reports the estimate using the full sample, column (2) a subsample of counties above median of the change in grain output in capita after the reform, and column (3) a subsamle of counties below median of the change in grain output. Regressions in Panel A control for county fixed effects, year effects, county-specific linear time trends, determinants of reform timing interacted with time fixed effects and droughts in March and April in the current year and the preceding year. Regressions in Panel B include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 4: County-level mean of male workers by crop in the 1982 Census

	Obs	Mean	Std. Dev.
A. county-leve mean of agricultural workers for each crop (all counties)			
Grain	1065	0.945	0.136
Cash Crops	1065	0.050	0.132
Cotton	1065	0.033	0.126
Fruit	1065	0.002	0.011
B. county-level mean of male workers for each crop (counties that grow some particular crop)			
All Crops	1065	0.519	0.026
Grain	1062	0.545	0.098
Cash crops	935	0.515	0.227
Cotton	232	0.348	0.236
Fruit	407	0.692	0.331

Notes: This table shows the summay statistics of county-level mean in the 1982 Census microdata. These counties can be matched with the county-level data on reform timing and the 1990 Census. The sample of individuals is restricted to agricultural workers. We use the unharmonized codes for occupation (OCC) and industry (IND) in the 1982 Census from IPUMS International to identify the crop an agricultural worker grows, e.g. fruit=1 if OCC==614&IND==14. We then obtain the county-level mean and report the mean and standard deviation across counties.

Appendix Table 5: Land size and gender

	Total amount of cultivated land for household (mu=1/6 acre)
	A. Chinese Health and Nutrition Survey 1989
% Male members	0.002
	[0.004]
Village FE	X
dependent variable mean	3.1
Observations	2495
R-squared	0.438
	B. Rural Fixed Point Survey 1986-1989 (Household-level Panel Data)
% Male labor	0.002
	[0.002]
dependent variable mean	7.6
Observations	9,762
No. of households	2,460
R-squared	0.000

Note: in Panel A, village fixed effects are controlled for. In Panel B, we report household fixed effect estimator using household-level panel data from 1986 to 1989.

Appendix Table 6: Extension of land lease in 1984

	Male=1
Land reform*Girl first	0.032***
	[0.008]
OCP*Girl first	-0.004
	[0.008]
1{Born in 1984-1986}*Girl first	0.012
	[0.008]
Girl first	0.025***
	[0.003]
Observations	241547
R-squared	0.051

Notes: 1{Born in 1984-1986} is assigned 1 if one was born in 1984-1986 and 0 otherwise. The sample includes all second births between 1974 and 1986 in counties that are matched with the county-level data on timing of land reform and OCP. All regressions include county-by-birth year fixed effects.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 7: Heterogeneity by grain output in 1977

	Dependent variable: Male=1		
	(1)	(2)	(3)
	Full sample	Grain ouput in 1977 above median	Grain ouput in 1977 below median
Land reform*Girl first	0.020***	0.024**	0.017
	[0.007]	[0.009]	[0.011]
Girl first	0.025***	0.026***	0.024***
	[0.005]	[0.006]	[0.007]
Observations	105536	55883	49653
R-squared	0.052	0.05	0.055

Notes: Column 1 reports estimate for the effect of exposure to land reform on the probability of second child being male in the full sample; column 2 for the effect in counties above the median of grain output in 1977; column 3 for the effect in counties below the median. The sample includes individuals born between 1974 and 1986 in 400 counties that are matched with the county-level data on reform timing and grain output. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 8: Land reform and infant health (UNICEF 1992 Chinese Children Survey)

	UNICEF 1992 Chinese Children Survey		
	Neonatal mortality	Post-neonatal mortality	Birth weight
Panel A: All births			
Land reform	-0.004***	-0.002**	16.402*
	[0.001]	[0.001]	[9.307]
Observations	114881	114881	31783
R-squared	0.089	0.091	0.344
Panel B: Second births			
Land reform*Girl first	0.002	-0.004	-27.561
	[0.003]	[0.002]	[33.245]
Observations	33976	33976	9349
R-squared	0.187	0.202	0.457

Notes: Using the UNICEF 1992 Chinese Children Survey, we report estimated effects of land reform on infant health outcomes. Panel A includes all births, and Panel B for the second births. Regressions in Panel A include county fixed effects, year of birth effects, county-specific linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the preceding year. Regressions in Panel B include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Appendix Table 9: Land reform and child mortality (UNICEF 1992 Chinese Children Survey)

	Died in 1977-1986=1 (UNICEF 1992 Chinese Children Survey)		
	(1)	(2)	(3)
	All	Male	Female
Land reform*Girl first	-0.002	-0.023**	0.015
	[0.007]	[0.011]	[0.011]
OCP*Girl first	0.000	0.013	-0.018
	[0.007]	[0.012]	[0.011]
Girl first	0.001	0.002	0.006
	[0.004]	[0.006]	[0.007]
dependent variable mean	0.028	0.028	0.027
Observations	33976	18014	15962
R-squared	0.2	0.3	0.349

Notes: Using the UNICEF 1992 Chinese Children Survey, we report estimated effects of land reform on child mortality of second births in 1977-1986. Column (1) reports the estimate for all second births, column (2) for male births and column (3) for female births. All regressions include county-by-birth year fixed effects. Robust standard errors clustered at the county level are reported in brackets.

* significant at 10% level; ** significant at 5% level; *** significant at 1% level.