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ABSTRACT

Following the death of Mao in 1976, abandonment of collective farming lifted millions from poverty and heralded sweeping pro-market policies. How did China's excess in male births respond to rural land reform? In newly-available data from over 1,000 counties, 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. Mothers with higher levels of education were substantially more likely to select sons than were less educated mothers. The One Child Policy was implemented over the same time period and is frequently blamed for increased sex ratios during the early 1980s. Our results point to China's watershed economic liberalization as a more likely culprit.

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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 reevaluate 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]. In the current debate about relaxing or eliminating the OCP, its role in “missing girls" is frequently invoked [CNN, July 2012; NPR, April 2013; *New York Times*, May 2013].[1] While intuitive, this argument ignores the historic decline in fertility just prior to the OCP's introduction in 1979; fertility rates were comparatively steady from 1978-84. We explore whether the OCP's purported effect on “missing girls" is confounded by land reform, 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 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]. Here, we consider a fundamental economic liberalization: how did the “world's largest anti-poverty program" [McMillan, 2002] affect de-selection of girls?

To evaluate these questions, we analyze new data on the rollout of land reform to over 1,000 counties; previous work has focused on variation across 28 provinces [Lin, 1992]. The “Household Responsibility System” unraveled collectivized agriculture and marked a critical first step toward a market-oriented Chinese economy. While land usership rights were shifted from the collective to individual households, land ownership remained with the collective. Land was contracted to households for 3-5 years. 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].

Using the 1990 population Census, we see a striking increase in the fraction male following land

[1] http://globalpublicsquare.blogs.cnn.com/2012/07/09/could-chinas-one-child-policy-change/
http://www.npr.org/2013/04/23/176326713/for-chinese-women-marriage-depends-on-right-bride-price
http://www.nytimes.com/2013/05/22/opinion/chinas-brutal-one-child-policy.html

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 or for the firstborn child (Figure 2A) or the second child if first child was male (Figure 2B, lower line). These patterns are replicated in a triple-difference regression framework.[2,3] Specifications that account for county-specific time trends and time-varying effects of county characteristics that primarily drive reform timing likewise 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 12 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. Our results are also robust to including a full set of county-by-year interactions. 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. 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 aggregate sex ratio trends. Ebenstein [2010] found that higher sex ratios were associated with higher fines under the OCP at the province level. 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 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). Likewise, ultrasound diffusion would not confound the effect of land reform because it was unavailable in rural counties until the mid-1980s. Some rural parents, however, may have determined sex prenatally by traveling to provincial capitals, where ultrasound technology was introduced in the mid-1970s.[4]

The sex selection response was highly concentrated in 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] To the extent that parents not only wanted sons but wanted these sons to marry, high-education families may have been less worried about the marriage market consequences of imbalanced sex ratios following land reform, and thereby had a (temporary) "first mover" advantage [Edlund, 1999].[6] This finding complements evidence that sex selection persists among some Asian immigrants to the West [Dubuc and Coleman

[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]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 counties with railroad connections to provincial capitals, where ultrasound machines were available at the time of land reform.

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

[6]In a dynamic model, land reform could increase the equilibrium level of sex selection if it increased uncertainty by reducing the intergenerational transmission in status [Edlund, 1999].

2007; Almond and Edlund 2008; Abrevaya 2009], who likewise may not suffer marriage market consequences of sex imbalance. We also examine possible fertility responses to land reform that might lead to endogenous sample selection. We find a small positive response in the total number of births. However, on the margins that affect sample selection (the decision to have a second child and birth interval between the first and second child), land reform had little effect.

Having isolated what we believe the causal effect of land reform on sex ratios, we consider – more speculatively – the potential mechanism through which land reform operated. In particular, we investigate four of the more obvious "mechanical" explanations and fail to find supportive evidence. First, land reform did not change land ownership, meaning that sons did not inherit land. Second, we show that land distribution did not depend on gender, suggesting that it was not demand for land usership that led to sex selection. Third, we cast doubt on increases in paternal income and demand for sons' future labor as explanations. Fourth, if sex selection reflected demand for old age support, we would expect that poorer parents would rely more on sons. However, we find the opposite: responses in sex selection were larger in relatively wealthy regions. Finally, we find substantial reductions in postneonatal mortality and increases in birth weight for births after the reform. Were land reform's effect confounded by a collapse of the rural medical system, birth outcomes should have deteriorated.Instead, we gravitate to the best-established consequence of land reform in China: it increased rural incomes. 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]). That said, evidence for this proposed explanation is more suggestive than dispositive, and clearly rests on a daughter being perceived as a poor substitute for a son.

We introduce the background of land reform and the One Child Policy in Section 2. Section 3 discusses preferences over the sex composition of children. The identification strategy is described in Section 4 and data in Section 5. Results are presented in Section 6 and potential mechanisms in Section 7. Section 8 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, which suffered from a severe drought, 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).[7] The land has continued to be owned by the collective. But the basic decision-making unit was shifted from the collective farm to 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.[8] 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".[9] 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.[10] The negative production shock changed the cost-benefit calculation such that

[7] It was extended to 30 years in 1993.

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

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

[10] Bai and Kung [2011] provide indirect evidence using province level data. They find that provinces that suffered

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, 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.[11] 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.[12] 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.[13] 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

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.

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

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

[13] 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.

timing with county-by-year data on precipitation.[14] 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 and the year preceding.[15] 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.

In all regressions on the effect of land reform, we control for the time-varying droughts in March and April in the current and prior year, as well as time-invariant determinants of reform timing by county interacted with time fixed effects.[16] This allows for characteristics that are correlated with reform timing to have their own idiosyncratic time effects, i.e. more flexibility than interacting characteristics with a linear time trend as in Acemoglu, Autor, & Lyle [2004].

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

[14] See Data Appendix.

[15] 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.

[16] It turns out that omitting controls for time-varying drought does not affect parameters of interest.

[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.

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 Figure 5. 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 an obvious 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 6.6).

2.4 The One Child Policy (OCP)

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 <u>not</u> set explicit incentives for the second child in the rural areas. 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 gazeteers. The stated policy was relaxed in 1984 to issue the second child permit to families with a first girl, the so-called "1.5 Child Policy" [Greenhalgh, 1986; Scharping, 2003].[18] From our county-level OCP rollout data (see Section 5), 51% of counties introduced the OCP in 1979.

[18]Guangdong province introduced the "1.5 Child Policy" in 1982 [Scharping, 2003]. In our data of land reform timing, all counties except for one in Guangdong province had introduced HRS by the end of 1981.

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.

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 strategy is a triple-difference approach. 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:

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

where the subscript i denotes the individual, j the county of birth, and t the year of birth. The superscript denotes birth order, 1 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.

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).

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 right 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.

Concurrent changes 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. We have conducted a comprehensive reading of reform policies from the late 1970s to the mid-1980s. Two historic reforms might at first 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 OCP 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]. OCP rules have been modified over time from 1979 to the present. For our purpose, the potential threat comes from the *initial introduction* of the OCP by county between 1978 and 1985 that might overlap with the county-level rollout of land reform. We therefore have collected the most detailed data on the timing of OCP implementation by county, i.e. more detailed than previous studies using policy variations at the provincial level. Using data on the county-level timing of both land reform and the OCP, we directly test which reform is the more important driving force in increased sex ratios in the 1980s.

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. Nevertheless, earlier introduction of ultrasound technology in provincial capitals could help shed light on how parents sex selected. In Section 6.5, we further investigate the role of ultrasound machines in provincial capitals below using data from Chen, Li and Meng [2013].

5 Data

5.1 County-level data on reform rollout

The post-1949 county gazetteers document local events and statistics about geography, politics, the economy and culture from 1949 to the 1980s. We conducted a comprehensive reading of all county gazetteers that have been published to date, covering 1835 counties in China. 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

We located 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

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 dotted line shows the fraction of counties that had introduced the OCP between 1978 and 1985, while the solid line represents HRS timing. Despite similar timing 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. All counties had already had the HRS for at least one or two years when the "1.5 Child Policy" was introduced in 1984. Therefore, the latter (and perhaps better known) revisions of the OCP would not confound our results. We therefore focus on separating the effect of land reform from that of the *initial* introduction of the OCP to rural counties (that did not apply directly to second births).

Ultrasound technology adoption

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 long-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

[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.

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.

Prior to ultrasound technology becaming available in the rural areas, 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. The short-dotted line in Figure 4 shows the rollout of ultrasound machines in provincial capitals, mostly between 1978 and 1984.[21] So during the rollout of land reform, one option for pregnant women was to travel to the provincial capital to ascertain fetal sex. Below, we examine further whether 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.[22] 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.)

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. 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

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

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

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 2, 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 underreporting. 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.[23]

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.

6 Results

6.1 Land reform and sex ratios: graphical presentation

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.

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-

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

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: main estimates

When we estimate equation (1), we find the same estimates as those displayed in Figure 2A (unadjusted). In column 1 of Table 3, 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. 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.

In column 3, we implement a more demanding comparison by controlling for county-by-year fixed effects (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. The R-squared increases from 0.011 in column 2 to 0.052 in column 3. For all subsequent estimations below, we use the main approach in equation (1). To translate the effect of land reform on male births to the effect on sex ratios, we also estimate equation (1) on the sex ratio of second births aggregated by county and birth year. In column 4, the sex ratio in families with a first girl increases by 0.15 following the reform, a precisely estimated increase of 12 percent that matches the magnitude in the (unadjusted) Figure 2B.

Just as in the event study figures, we do not find an increase in second sons following land reform. This is consistent with the preference for sex mix described above. We do find a small decreases in second sons (-0.01, significant at 10% level) in column 2. While smaller and less robust than our primary result, how might this decrease in sons be achieved? We think it is unlikely that sons were selectively aborted. Nor is there any anecdotal evidence of selective abortion of males until a female was achieved. Furthermore, prior to land reform the children that followed sons were, if anything, disproportionately female (below the biological normal sex ratio of 1.05, see Figure

2B). Thus, there was little/no scope for reducing the selective abortion of females following sons in the wake of land reform. Instead, *adopting* a second daughter is more likely [Chen, Ebenstein, Edlund and Li, 2012]. First, since lineage is traced through males, adopting a daughter may be less aversive than adopting a son. Second, Chen, Ebenstein, Edlund and Li (2012) find that the number of adopted girls increased significantly since 1979, while the number of adopted boys remained nearly constant from the 1970s to the 1990s. Third, most of children adopted at parity two are girls in families with a previous boy.

Putting these pieces together (and still taking the -.010 coefficient in column 2 at face value), our findings have several implications. First, observing fewer two-son families after reform is consistent with the absence of sex selection for the first child. Second, the fear of having two girls is substantially larger than that of having two boys. Finally, the net increase in sons (through abortion of females or other non-adoption means) following a first girl would be 2 percentage points (.029 − .010). Two percentage points is also how much the fraction male increased in absolute terms following female and land reform: one third of the .029 DDD increase in sons following girls may have been "offset" by the increased supply of girls to other families. But this would not imply that the 2 percentage points absolute increase in fraction male following a girl as achieved by fully 1 percentage point of the *parents* giving up a daughter for adoption, as on average it will take giving up more than one second daughter for adoption to give birth to a second son (among parents not practicing sex-selective abortion). From the perspective of child welfare, we do not think that it is appropriate to conclude that the net effect of .02 captures land reform's effect on girls. After all, adopted girls are treated much more harshly on average than non-adopted girls (Chen, Ebenstein, Edlund and Li, 2012). Instead, we allow that some of land reform's effect of increased "sex mix" of children may have been achieved via both sex selective abortion and an expanded adoption market for girls.

6.3 The One Child Policy and Sex Ratios

The newly-available data on the county-level rollout of land reform and OCP permits a horse race between these two reforms. We focus on rural counties, home to 86% of China's population at the time.[24] We assign treatment status to the OCP as 1 for individuals born one year after the OCP or later and 0 otherwise. In Table 4A, the first three columns report the results using our main strategy in equation (1). Column 1 shows similar estimates for land reform in the subsample with OCP data as in the full sample. 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

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

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. In column (4)-(6), we repeat this horse race controlling for the full set of county-by-year fixed effects. Again, results are very robust indicating that it was land reform, not the OCP, that increased sex ratios in the 1980s.

Although the OCP does not confound the effect of land reform, one might expect that the land reform effect would be larger when the third birth was fined and first birth rewarded under the OCP. We examine the possible interactive effect by testing how the land reform effect changes after the OCP arrived. The OCP enforcement at the time of land reform is defined as 1 if the OCP came earlier than land reform or in the same year. In Table 4B, there is suggestive (but not overwhelming) evidence that more sex selection follows land reform if the OCP was in place. However, the estimate is small in magnitude and statistically insignificant. Below, we will show that the OCP does have a robust, if modest, impact on fertility.

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 5A, 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]. The effect of the OCP on reducing fertility is small, consistent with Appendix Figures 1A & 1B showing that the 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 5B, 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 rules out 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. However, the net effect of OCP on having the second child is economically small, -0.004 (-0.027+0.046*0.5) relative to sample mean of 0.82, consistent with the absence of fines for the second when OCP was first introduced.

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.

6.5 Ultrasound technology in provincial capitals

How was sex selection accomplished prior to ultrasounds' arrival in rural counties? We show in Figure 4 that land reform generally preceded the arrival of ultrasound machines in the rural areas, while such technology was largely available in provincial capitals from the late 1970s. Rail was the main means of long-distance transportation at that time.

Using a digitized national map of railroad networks in 1980 (provided by Matthew Turner),[25] 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

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

both.

In Table 6, 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, 0.024, to our main estimate 0.029 in Table 3, prenatal sex determination through our measure of rail access to ultrasound could explain one-sixth of the increase in sex ratios induced by land reform. There is also anecdotal evidence that other sex determination technologies, e.g. amniocentesis and chorionic villus sampling (CVS), were available in provincial capitals since the late 1970s, which could further accentuate the contribution of railroad access to the increase in sex ratios following land reform. Below, we will show that highly educated mothers, who might have been more likely to take a train to obtain a prenatal ultrasound in the city, sex selected the most in response to land reform.

6.6 Response Heterogeneity

We now consider whether the sex selection response to reform was fairly uniform or varied by parental education and the income change induced by land reform.

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 7.5 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.

Better educated parents might capture larger income increases from land reform, which in turn spur more sex selection. Yang and An [2002] found that education improved the uses of household-supplied inputs and contributed to higher agricultural profits under the HRS. In Appendix Table 2, we also find that counties with larger fraction of educated workers indeed have larger increase in grain output following reform. 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. Our findings on travel by train to provincial capitals to access ultrasound machines provides a possible interpretation: better educated mothers might know better where the ultrasound technology was available. 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.

Unfortunately, no household-level income data are available from the 1970s to the early 1980s in China. Thus, we focus our income analysis at the county level. In Panel A of Table 8, 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.[26] 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, 1.3 percentage points, is smaller than that in our full sample, and it is also less precisely estimated. This indicates that we might underestimate the effect on male births using this grain-matched subsample. Column 2 shows a precisely estimated increase in probability male of 2.7 percentage points for counties above the median of the change in grain output, which doubles 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, and has negative sign.[27]

In this subsample, we also observe a 2.9 percentage point decrease in sons following a son. Demand for girl adoptees apparently goes up more than that in the main sample in Table 3. The extra supply of girls is likely from families that were giving up a girl following a daughter in the absence of land reform, as measured by the coefficient 0.27 for first child being a girl. After land reform, the 2.7 percentage points could be achieved more through abandonment than other forms of selection, e.g. abortion.

7 Mechanisms

We have found that land reform caused the sex ratio imbalance to widen substantially. This section attempts to shed light on the primary mechanism(s). Increases in household income following

[26] 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.

[27] 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 regardless of changes in grain output.

the reform is likely an important one. 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, sons and sex selection become more affordable as income increases. Our evidence on the heterogeneous treatment effects, more sex selection in counties with larger income gains and among better educated parents, supports the income channel. As noted above, land reform's best documented effects to date are its positive impacts on agricultural output and income.

We consider alternative channels that might also account for our findings. One hypothesis (more common in other settings of property rights reform) is that sons inherit land. It is easily ruled out in our study because the reform did not privatize land ownership and therefore intergenerational transfer was impossible. We examine another four possible channels below:

1. Gender bias in land distribution;
2. Increase in the demand for male labor or male income;
3. Increase in demand for old age support;
4. Collapse of the rural medical system.

The latter three channels were hypothesized by Sen [1990]. None of four hypotheses 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.[28] Using the CHNS 1989 wave in Panel A of Table 9, we find

[28] 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.

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 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. Higher paternal income and demand for sons' labor

The switch from collectivized farming to household farming enabled households to make their own input decisions, including labor division within household. It is commonly argued that men have greater physical strength on average in agricultural production, so 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. 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 production 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 1 (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.[29] Data are aggregated 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 10, 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 0.02 percentage points, that is, a 10 percent increase in the fraction of male workers leads to a 0.2 percent increase in the probability of second child being male. The estimate is fairly precise (standard error of .0002). This estimate is unchanged in column 2 when we control for the interaction term with the fraction of male workers growing grain. 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 (*cf* heterogeneity by maternal education or grain output).

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

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 6.6 show the opposite: counties that experienced more

[29] 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.

output gains have a substantially larger increase in sex ratios after the reform. Furthermore, in Appendix Table 3, 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 in 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 1992 Chinese Children Survey.

In Appendix Table 4, Panel A reports estimates for all births. We find that postneonatal mortality decreased by 0.3 percentage points (37.5% relative to sample mean), and birth weight increased by 34 grams (1% 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 to D. 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. Looking at second boys and girls separately in Panel C and D, we find that postneonatal mortality decreased by 0.6 percentage points among boys at the 10 percent significance level, while the effect on girls is the opposite and imprecisely estimated. While these results may indicate that boys had better access to health care in the postneonatal period, the effect size is too small to generate our findings on the sex ratio. Suppose the sex ratio is otherwise biologically normal, then six fewer deaths per thousand boys under age 1 would increase the sex ratio from 1.05 to 1.051, which explains a negligible part of the 12% increase in sex ratio we found in Table 3.

Overall, 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 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. In the current debate about relaxing or eliminating the OCP, its role in "missing girls" is frequently invoked [CNN, July 2012; NPR, April 2013; New York Times, May 2013].[30] 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.[31] 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.

[30]http://globalpublicsquare.blogs.cnn.com/2012/07/09/could-chinas-one-child-policy-change/
http://www.npr.org/2013/04/23/176326713/for-chinese-women-marriage-depends-on-right-bride-price
http://www.nytimes.com/2013/05/22/opinion/chinas-brutal-one-child-policy.html

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

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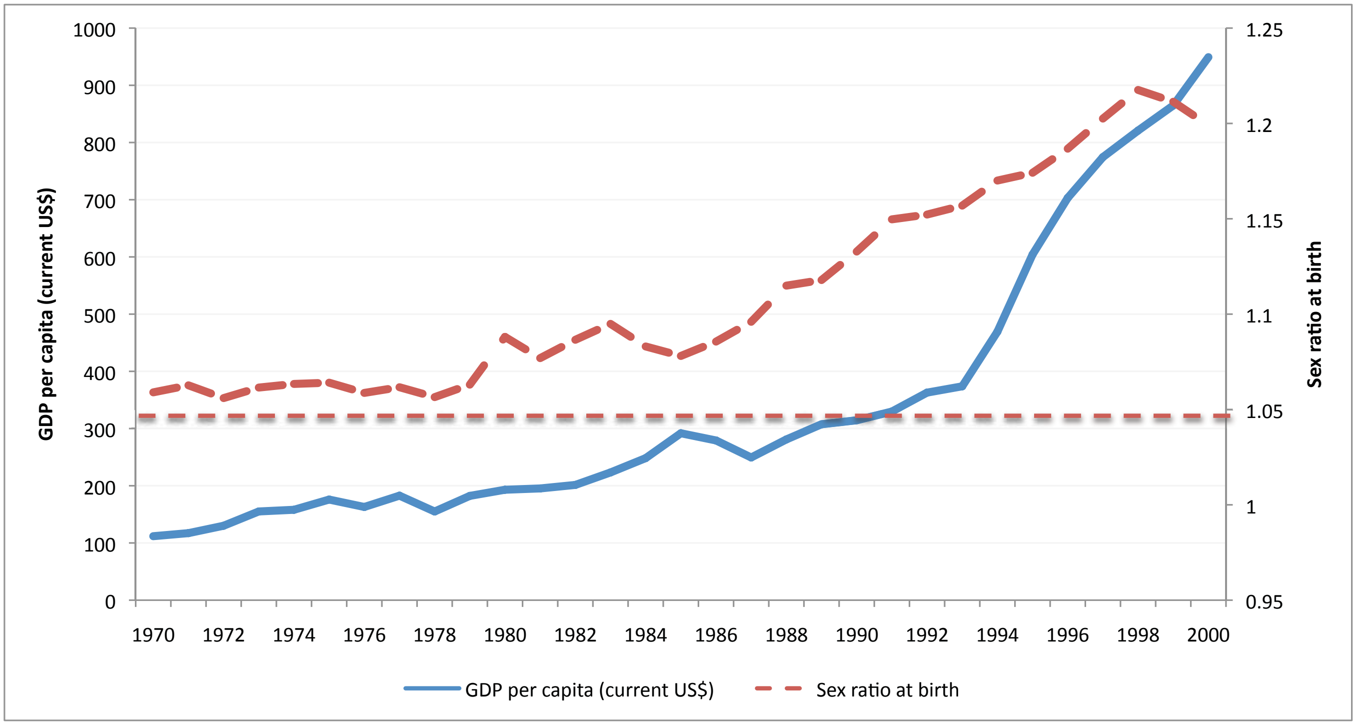
Figure 1: GDP per capita and sex ratio at birth in China: 1970-2000



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

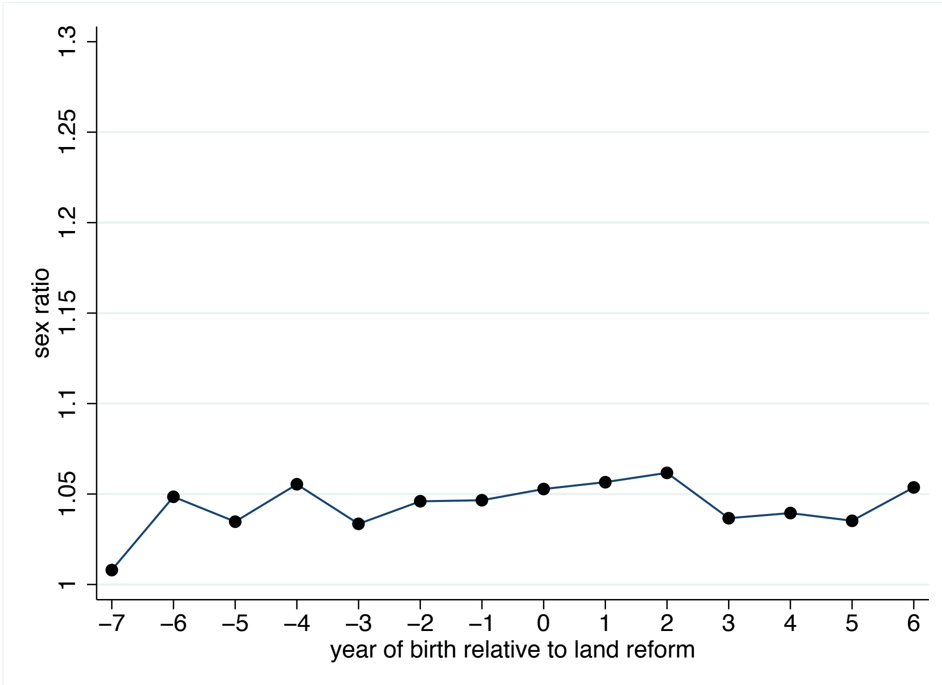
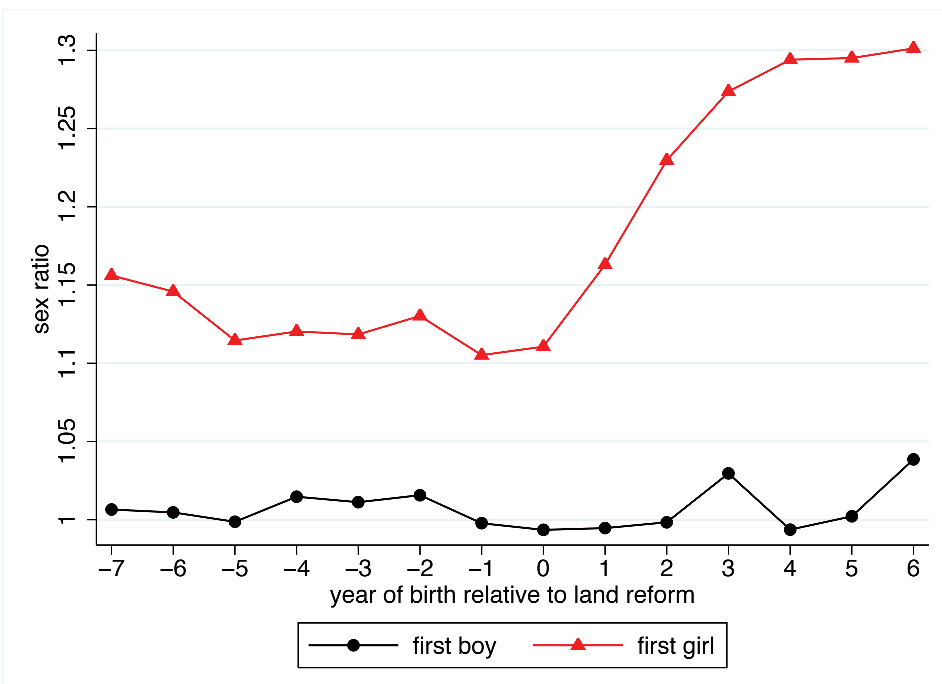


Figure 2B: Sex ratio of the second child



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

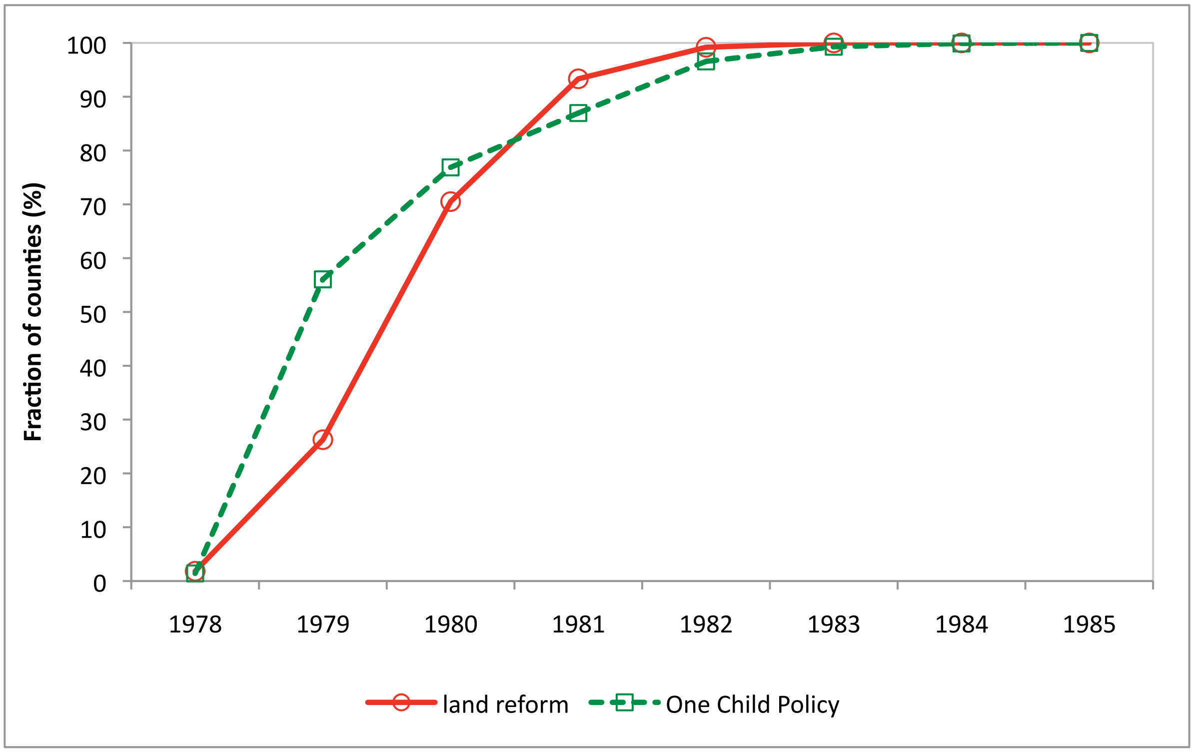
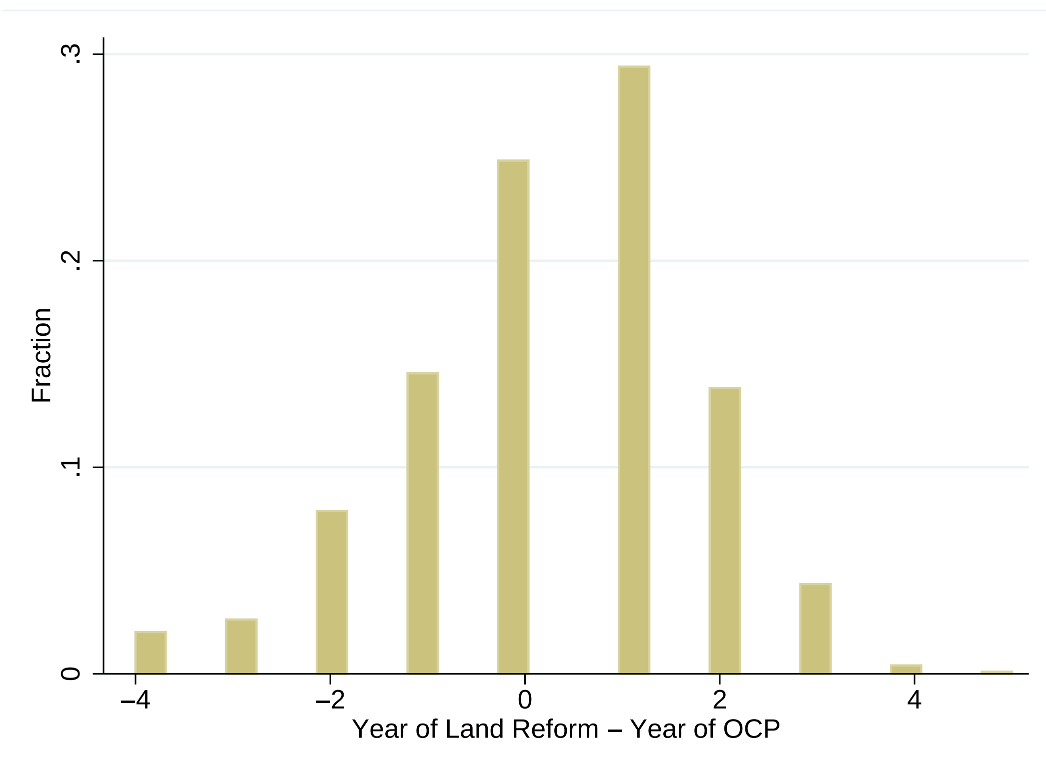


Figure 3B: Difference between land reform start year and the OCP start year



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

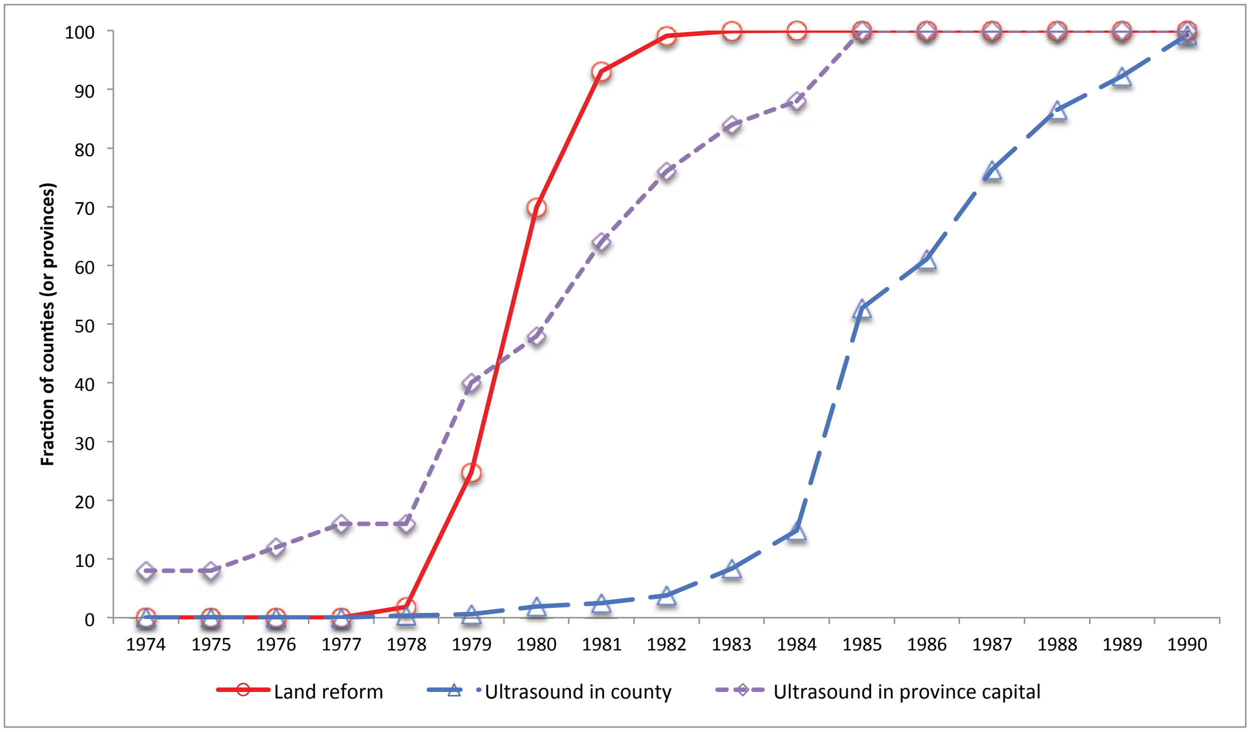
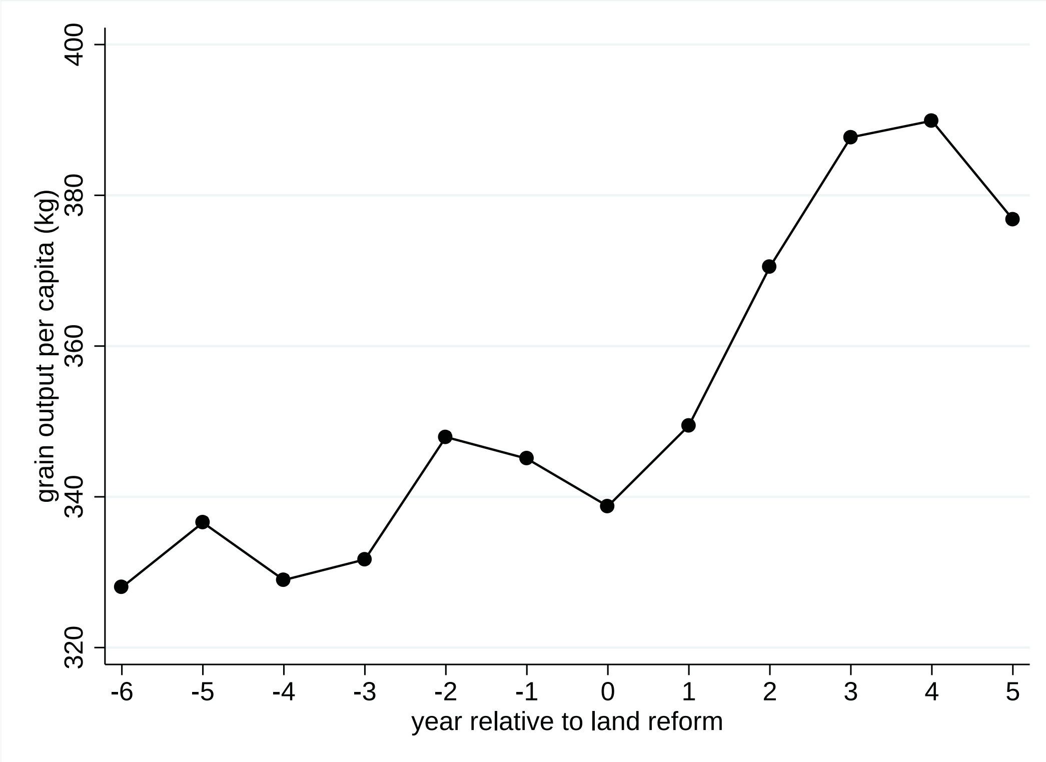


Figure 5: Grain output per capita



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.

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
	(1)	(2)	(3)	(4)
	First child	Second child		Second child
Land reform*Girl first		0.029***	0.029***	0.151***
		[0.004]	[0.004]	[0.030]
Land reform	0.003	-0.010*		-0.021
	[0.004]	[0.006]		[0.044]
Girl first		0.027***	0.027***	0.136***
		[0.003]	[0.003]	[0.020]
County FE	X	X		X
YOB FE	X	X		X
Initial control*YOB FE	X	X		X
Spring drought in t and t-1	X	X		X
County-specific linear trends	X	X		X
County * YOB FE			X	
Dependent variable mean	0.511	0.523	0.523	1.27
Observations	371762	279069	298755	24,255
R-squared	0.006	0.011	0.052	0.131

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; column (4) for sex ratio of 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 (1), (2) and (4) 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. Regression in column (3) includes county-by-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 4A: Land reform versus the One Child Policy

	male=1					
	Our main specification			County-by-year FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Land reform*Girl first	0.030***		0.031***	0.030***		0.033***
	[0.005]		[0.008]	[0.005]		[0.008]
Land reform	-0.012*		-0.013*			
	[0.007]		[0.007]			
OCP*Girl first		0.024***	-0.002		0.024***	-0.004
		[0.004]	[0.008]		[0.004]	[0.008]
OCP		-0.017***	-0.004			
		[0.006]	[0.007]			
Girl first	0.025***	0.028***	0.025***	0.024***	0.027***	0.025***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Observations	224600	224600	224600	241547	241547	241547
R-squared	0.011	0.011	0.011	0.051	0.051	0.051

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 OCP. Regressions using our main specification 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

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

Table 4B: Land reform effect interacted with the One Child Policy

	male=1
Land reform*Girl first*OCP	0.006
	[0.010]
Land reform*Girl first	0.025***
	[0.009]
Land reform	0.007
	[0.010]
Girl first	0.027***
	[0.006]
Observations	224600
R-squared	0.011

Notes: OCP is assigned 1 if the OCP came earlier than land reform or in the same year and 0 otherwise. HRS*OCP and Girl first*OCP are also controlled for. 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. The regression includes 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 5A: 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 5B: 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	-0.026*		0.021
	[0.004]		[0.008]	[0.015]		[0.026]
Land reform	-0.016***		0.001	0.057**		0.032
	[0.005]		[0.006]	[0.025]		[0.028]
OCP*Girl first		0.038***	0.046***		-0.039**	-0.056**
		[0.004]	[0.009]		[0.015]	[0.026]
OCP		-0.023***	-0.027***		0.004	0.014
		[0.004]	[0.006]		[0.024]	[0.028]
Girl first	0.040***	0.030***	0.031***	-0.181***	-0.174***	-0.175***
	[0.004]	[0.003]	[0.003]	[0.011]	[0.011]	[0.011]
mean of dependent variable		0.82			2.9	
Observations	298310	298310	298310	224600	224600	224600
R-squared	0.343	0.343	0.343	0.087	0.087	0.087

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 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 6: Railroad access to province capital cities that had ultrasound machines

	male=1
Land reform*Girl first*Railroad to province capital that had ultrasound 1 year after land reform or earlier	0.020**
	[0.010]
Land reform*Girl first	0.024***
	[0.005]
Observations	279069
R-squared	0.011

Notes: Land reform and Girl first are also controlled for. The sample includes counties that are matched with county-level data on land reform. The regression includes county fixed effects, year of birth effects, county-specific linear trends, and initial county controls interacted with birth year 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	0.075***		0.062**
	[0.024]		[0.025]
Land reform*Girl first*Mother Middle school	0.031**		0.026*
	[0.013]		[0.014]
Land reform*Girl first*Mother Primary school	0.01		0.008
	[0.009]		[0.010]
Land reform*Girl first*Father High school		0.044***	0.027
		[0.016]	[0.017]
Land reform*Girl first*Father Middle school		0.015	0.005
		[0.011]	[0.012]
Land reform*Girl first*Father Primary school		0.005	0.001
		[0.010]	[0.011]
Land reform*Girl first	0.017***	0.016*	0.014
	[0.006]	[0.009]	[0.009]
Observations	260529	260529	260529
R-squared	0.007	0.007	0.007

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 reform timing and initial controls. 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 8: 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	0.013*	0.027***	-0.004
	[0.007]	[0.009]	[0.010]
Land reform	-0.015	-0.029***	0.003
	[0.010]	[0.013]	[0.017]
Girl first	0.029***	0.027***	0.033***
	[0.005]	[0.007]	[0.008]
Dependent variable mean	0.521	0.524	0.519
Observations	93335	53243	40092
R-squared	0.011	0.011	0.013

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 before and after the reform, and column (3) a subsamle of counties below median. 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 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 9: 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.

Table 10: 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	0.0002	0.0002	
	[0.0002]	[0.0002]	
Land reform*Girl first*% Male growing grain		-0.0005	
		[0.0005]	
B. Average crop suitability index by county from FAO GAEZ			
Land reform*Girl first*Cotton suitability index			0.005
			[0.005]
Land reform*Girl first*Citrus suitability index			0.011
			[0.011]
Land reform*Girl first*Banana suitability index			-0.002
			[0.011]
Land reform*Girl first*Wheat suitability index			0.006
			[0.006]
Land reform*Girl first*Wetland Rice suitability index			-0.014
			[0.014]
Observations	256605	256096	278522
R-squared	0.011	0.011	0.012

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 and initial controls. 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 girl first, land reform, 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.

Data Appendix

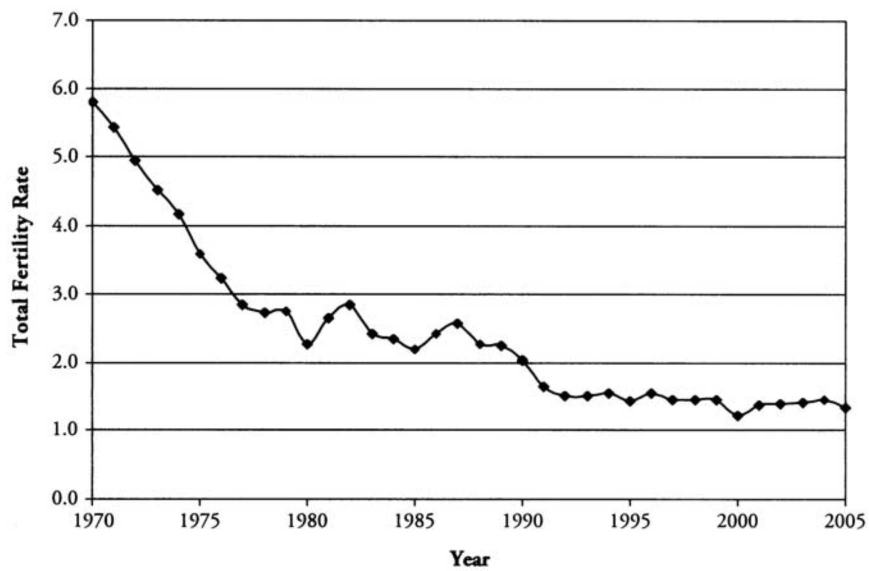
Precipitation Data

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)

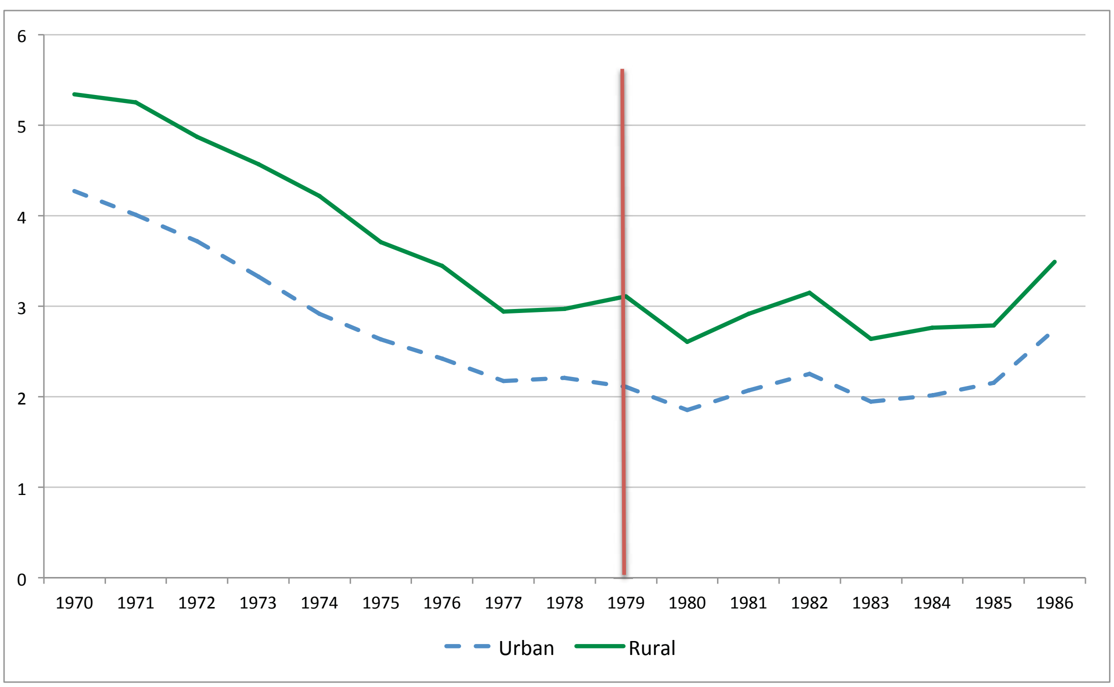
Figure 1. Reported Total Fertility Rate: China 1970–2005, Unadjusted



Sources: Guo (2004); NBS (1995–2006); Yao (1995).

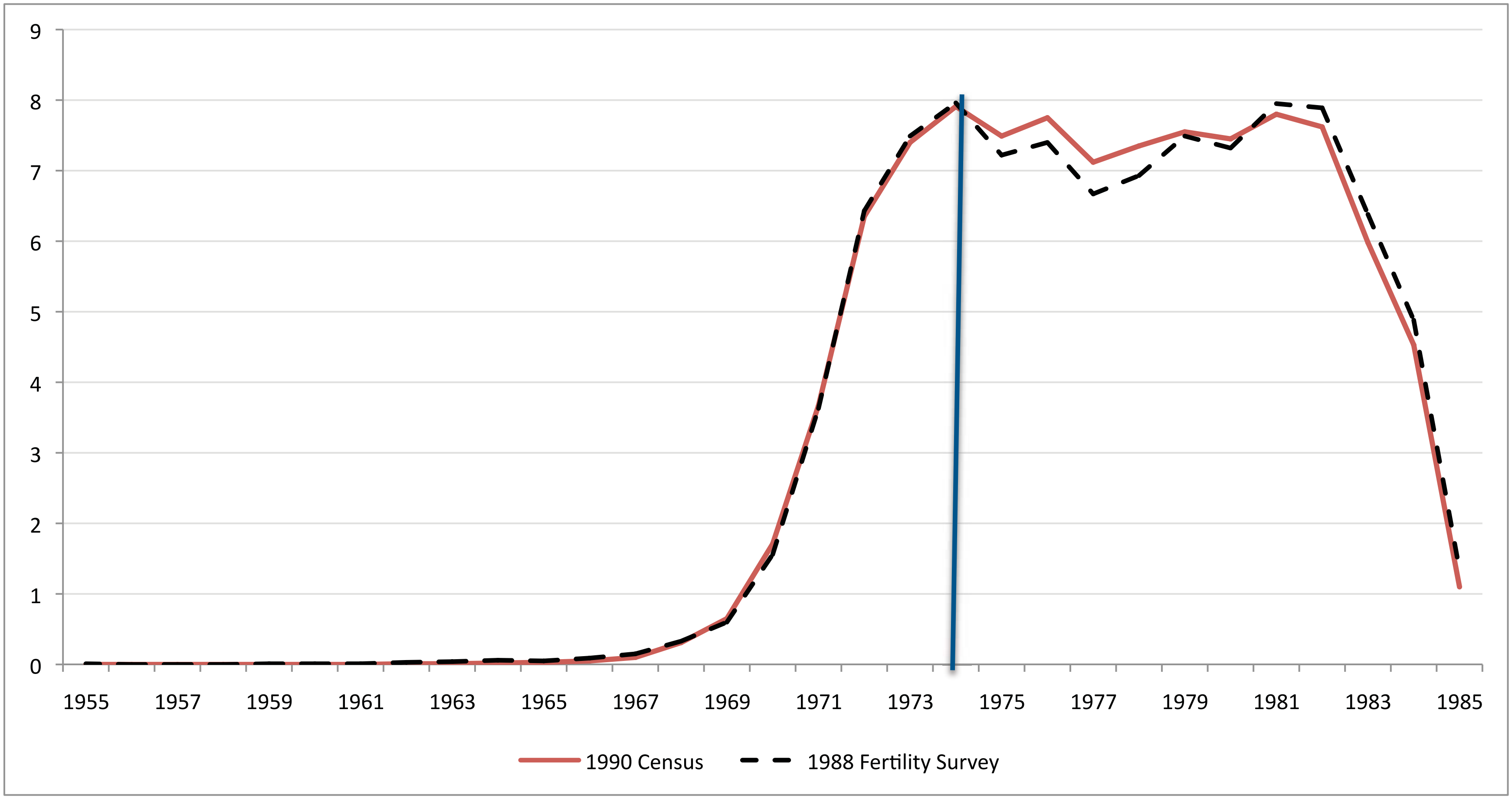
Notes: Data for 1970–1992 are from Yao's (1995) compilation: 1970–1981 data are based on the 1982 National One-per-thousand Population Sampling Survey on Fertility; 1982–1987 data are based on the 1988 National Two-per-thousand Population Sampling Survey on Fertility and Contraceptives; 1988–1992 data are based on the 1992 Fertility Sampling Survey in China; 1993 data are from Guo (2004), which is based on the 1997 National Survey on Fertility and Reproductive Health; 1994–2005 are from *China Population Statistical Yearbook* (NBS 1995–2006).

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: 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 Table 1: 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 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: 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	0.017**	0.022**	0.011
	[0.007]	[0.009]	[0.011]
Land reform	0.002	-0.01	0.017
	[0.009]	[0.012]	[0.015]
Girl first	0.027***	0.028***	0.025***
	[0.005]	[0.006]	[0.007]
Observations	99024	52633	46162
R-squared	0.011	0.011	0.011

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 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.

Appendix Table 4: Land reform and infant health

	Neonatal mortality	Post-neonatal mortality	Birth weight
	A: All births		
Land reform	-0.0001	-0.003**	33.969**
	[0.002]	[0.001]	[15.320]
Observations	107934	107934	28876
R-squared	0.015	0.019	0.158
	B: Second births		
Land reform*Girl first	0.002	-0.002	10.811
	[0.004]	[0.003]	[26.845]
Observations	31892	31892	8598
R-squared	0.029	0.03	0.231
	C: Second boys		
Land reform*Girl first	-0.002	-0.006*	41.645
	[0.006]	[0.003]	[39.824]
Observations	16911	16911	4665
R-squared	0.047	0.053	0.277
	D: Second girls		
Land reform*Girl first	0.005	0.002	-45.777
	[0.005]	[0.004]	[42.152]
Observations	14981	14981	3933
R-squared	0.06	0.062	0.296

Notes: This table reports estimate for the effect of exposure to land reform on infant health outcomes. Panel A includes all births; Panel B for the second births; Panel C for boys among second birth; Panel D for girls among second births. 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.