Income and Sex Selection:
A cautionary tale of land reform and sex ratios in China

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Abstract

This paper examines the effect of income growth induced by the 1978-84 land reform on the sex ratio imbalance in China. Using variation in the reform timing by county and the well-documented fact that the sex ratio of the first child is biologically normal, we compare the sex of the second child between families with a first girl and those with a first boy before and after the reform. We find that, following a first daughter, the second child is 5.5 percent more likely to be a boy after the reform. Moreover, better educated parents are more likely to respond in sex selection. Our findings indicate that a 1 percent increase in income leads to a 1.6 percent increase in sex ratio of the second birth.

1 Introduction

A large literature in economics details the wide-ranging benefits of economic growth. This work extends to positive health impacts of income growth, including studies following the striking "Preston Curve" that shows gains in life expectancy associated with income increase [Preston, 1975], or recent design-based micro-econometrics studies, e.g. Gertler [2004] and Amarante et al. [2011]. Furthermore, there may be complementarity to gender equality and income growth. Over the past quarter century, economic development has closed gender gaps in many dimensions, including educational attainment, life expectancy, labor force participation, etc. [World Development Report 2012].

A dissenting literature cautions that income growth may have some seemingly perverse effects. A number of studies suggest negative relationship between income and health [Ruhm, 2000, Dehejia and Lleras-Muney, 2004, Snyder and Evans, 2006]. Duflo [2011] observes that, the most stark area gender inequality persists along economic development is the "missing women" phenomenon. Figure 1 shows the case in China. While the GNP per capita increases dramatically between 1980 and 2000, sex ratio at birth also rises from 1.08 in 1980 to 1.2 in 2000. In the 2005 mini Census, sex ratio at birth stays over 1.2 [Li, 2007]. Whether this co-movement of income and sex ratio should be interpreted as causality or merely correlation remains an open question.

We document that income growth causally results in more sex selection. In particular, following a first daughter, the second child is 5.5 percent more likely to be a boy following the 1978-84 land reform in China. This translates to a 12 percent increase in the county-by-year sex ratio of the second child. Our finding adds to recent work finding that sex selection persists and is even furthered by other dimensions of economic development, e.g. technological diffusion (ultrasound rollout by Chen et al. [2010]), or immigration to more economically developed countries [Dubuc and Coleman 2007; Abrevaya 2009; Almond and Edlund 2008; Almond et al. 2009]. If the prior is that higher income, along with better social security system, would eliminate sex selection, finding highly biased sex ratio among Asian immigrants is striking. On the other hand, showing that economic development does not fully stop sex selection is only part of the story. Without observing the counterfactual of sex selection among immigrants, this literature does not offer further insight on how sex choices change as income grows.

In this paper, we use the post-Mao land reform as the source of exogenous increase in household income. The reform replaced the collectivized agriculture with the Household Responsibility System (HRS), under which collectively owned land was contracted to individual households who can receive all income from the land after meeting the tax and quota sales obligations to the state [Perkins, 1988]. By partially shifting the property rights to households,[1] the HRS provided an incentive scheme that tightly linked effort to reward. The reform had led to remarkable agricultural

[1]Because ownership of land remains collective, this reform should be distinguished from land titling reforms that fully shift the property rights.

growth between 1978 and 1984 [McMillan et al., 1989; Lin, 1992]. It has been widely recognized for its achievements in lifting hundreds of millions of rural households out of poverty [World Bank, 2000].

We exploit the county-level rollout of the reform between 1978 and 1984 using unique data on the reform timing for more than 1000 counties, whereas most previous work is based on the variation across 30 provinces. Our research design compares the sex of the second child between families with a first girl and those with a first boy before and after the reform. First of all, one's exposure to the reform depends on one's year of birth and county of birth. Our estimation accounts for county time trends and any timing-varying effects of county characteristics that primarily drive the reform timing. Moreover, the third difference by the sex of the first child is based on a well-documented demographical pattern (Zeng et al. [1993]). While the sex ratio of the first child is biologically normal, it becomes abnormally male-biased at higher birth orders, especially among families with no previous son. Thus, we focus on the second child in families with a first girl, where parents have a stronger desire for sons. The second child in families with a first boy is an ideal control group, because little human intervention is involved with the sex of the first child.

We can distinguish income growth from institutional changes for interpreting the effect of land reform on sex ratio imbalance. Following the reform, the increase in household demand for manual labor and the increase in demand for old age support might also motivate stronger desire for sons (Sen [1990]). We find that the effect of land reform on male births does not vary by gender-specific demand for labor or increase in gender-specific income, which contrasts with the argument on increase in demand for male labor. Moreover, the effect of land reform on sex ratio is larger where productivity gains are greater. The heterogeneous effect casts doubt on increase in demand for sons to secure old age, because the collapse of rural safety net was universal once the collectivized farming was replaced, regardless of post-reform productivity.

We find substantial heterogeneity in response according to maternal education. In particular, we find a 12 percent larger effect on the likelihood of second child being male for mothers with a high school education. Again, it is consistent with the interpretation that higher earnings (of better educated parents) lead to more sex selection. This finding also concords with the theoretical literature on marital matching – high status males are least likely to bear cost of high sex ratios [Edlund, 1999].

Our findings highlight an alternative explanation for the secular increase in sex ratios between the late 1970s and mid 1980s in China. This was the time when One Child Policy (OCP) was introduced. It is commonly argued that, when parents have fewer chances to have a male naturally under the fertility restriction, we would expect more sex selection.[2] Land reform occurred at the same time as the implementation of the initial OCP regulations. Controlling for the most detailed data compiled on OCP rollout between 1978 and 1984 by county, we find that the effect of land

[2]Ebenstein [2010] provides provincial level evidence that the level of fines for above-quota births is positively associated with sex ratio.

reform on sex ratio is unchanged.

Finally, we find that while total fertility responded positively to land form, the decision to have a second child is not affected by land reform. OCP decreases total fertility and the likelihood to have a second child, though parents with a first daughter would still want a second one under OCP. Intuitively, the margin of fertility affected by each reform is different. While the decrease in total fertility under OCP is likely coming from fewer second births, the increase in total fertility following land reform would be most likely induced by the land distribution rule that only authorized births (mostly the single child) got land. This implies less incentive to have the second one who would not benefit from the reform. On the margin to have a second child, we indeed find no effect of land reform, reducing concerns about endogenous fertility relevant for our identification.

We introduce the background of land reform in Section 2. Section 3 provides an analytical framework on land reform, income growth and sex ratio. We discuss data and identification strategy in Section 4. Main results are presented in Section 5. Results on heterogeneous effects are reported in Section 6. Section 7 presents results of robustness checks. Section 8 offers a brief discussion.

2 Background

2.1 The post-Mao land reform

China had pursued collective agriculture for more than two decades since the mid-1950s. Under this system, workers generally received daily fixed work points and got 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 had been virtually no change in grain output per capita [Zweig, 1987]. Following the death of Mao Zedong and the end of the Cultural Revolution, a decollectivization reform was initiated in 1978. Only except for land ownership, collective system was abandoned. Collectively owned land was contracted to individual households for up to 15 years.[3] Consequently, 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]. The general rule of land distribution was based on family size. A family got an additional plot for a new born and lost one when a member passed away [Oi, 1999].[4]

Essentially, economic incentives that reward individual effort were introduced to replace the ideology based system. The reform has been documented to have greatly improved agricultural productivity between 1978 and 1984. McMillan et al. [1989] use aggregate data and suggest

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

[4]After the introduction of the OCP in 1979, land distribution for new borns was restricted to authorized births only (various issues of county gazetteers).

that over three-quarters of the productivity increase during this period could be attributed to the incentive effects of the HRS. Using the reform rollout at the provincial level, Lin [1992] has similar finding that the reform accounts for half of the output growth. The reform is also widely recognized for its achievements in lifting hundreds of millions of rural households out of poverty [World Bank, 2000].[5]

2.2 Evolution of reform policies

The HRS was initially worked out by farmers without approval from the central government [Lin, 1987]. At the end of 1978, a small number of production teams in Anhui Province, which suffered from a severe drought in that year, start to experiment with contracting land and output quotas to individual households [Lin, 1987; Yang, 1996]. The initial position of the Central Committee of the Chinese Communist Party (CCP) on household farming was in a strong opposition. The document "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.

The prohibition was relaxed in September 1979 by allowing a few exceptions to households living in areas that were peripheral, distant, mountainous, and isolated due to transportation difficulties.[6] In September 1980, the 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 quota to households. By August 1981, the Central Committee's position on household farming became more liberalized in a mission sent to fifteen provinces "contracting to households is not only a means of relieving poverty but also ways of enhancing productivity; and it hasn't changed the production relations of the collective economy".[7] In January 1982, the Central Document No.1 officially announced that "the HRS is the production responsibility system of the socialist economy", which first showed the CCP's commitment to popularize HRS.

2.3 Variation in the county-level reform timing

Data on the timing of land reform at the county level were previously undiscovered. We have collected new data on the year HRS was introduced by county from 1242 county gazetteers in which local history from 1949 to the 1980s was recorded. Specifically, it is the year when collectively owned land was first contracted to individual households in a few villages of each county; and it usually took 2-3 years to spread the HRS to the whole county. These counties represent two-thirds of all counties that have ever published gazetteers.[8] Because land reform occurred in rural areas,

[5]Official estimates indicate that rural poverty rate declined from 30 percent in 1978 to 5 percent in 1998 [World Bank, 2000].

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

[7]People's Daily, August 4th, 1981

[8]The other one-third of counties do not report the timing of HRS adoption, or report it as "the late 1970s" or "the early 1980s", which are not useful for our identification strategy.

our sample includes locations that were rural counties at the time of the reform.[9] In Figure 2, we plot the fraction of counties that had introduced HRS between 1978 and 1984. Only 1.85 percent of counties were the reform pioneers in 1978. The vast majority reformed between 1979 and 1981, with the peak of 45 percent in 1980. By 1984, all counties had adopted HRS.

To identify the effect of land reform using the variation in the reform timing, the first priority is to understand what the driving forces were towards an earlier reform. The evolution of the reform policies suggests two primary sources of the variation: drought and poverty prior to the reform. A severe drought led to large declines in agricultural production, which in turn provided the local government incentive to reform.[10] Particularly, the negative production shock spurred taking the political risk as reform starters, since contracting land to individual households were not permitted in earlier years. Poor and remote counties were among the first permitted to adopt HRS by the central government as a means to reduce national poverty rates.

Moreover, 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 is higher and thus productivity gains are larger. Using size of production team to measure monitoring cost, previous studies show mixed findings.[11] The second hypothesis is that provinces that suffered more from the 1959-61 Famine reformed earlier, because the more painful the lesson from the Famine, the less likely the province to favor collective farming. Supportive evidence is provided by Yang [1996] and Bai and Kung [2011]. Lastly, Yang [1996] argues that provinces further from Beijing had more freedom to initiate earlier reform.

We first test whether drought had led to land reform by matching the county-level data on reform timing with county-by-year data on precipitation.[12] 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 one to the first year of reform, zero before the first year and missing values after. In addition, Chinese Academy of Agricultural Sciences [1984] suggests that, the growth of rice, the number one grain output in China, largely depends on rainfall at the beginning of the growing season, usually in March or April. In Table 1A, 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 before.[13] From column 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 before have strong and precisely estimated

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

[10]Bai and Kung [2011] provide indirect evidence using province level data. They find that provinces that suffered more in the 1959-61 Famine started land reform earlier when struck by bad weather. The interpretation is that the Famine experience undermined the local belief 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]See Data Appendix.

[13]Because the first month of reform are mostly not documented, a drought in the growing season is likely to affect reform at the second half of the current year or next year.

effect on motivating an earlier reform.

Next, to test the correlation between reform timing and other time-invariant county characteristics as discussed above, we have collected and constructed county-level measures prior to the reform from various sources. Poverty is captured by grain output per capital in 1977 that are collected from county gazetteers, and remoteness is measured by distance to province capital using 1982 Census. Size of production team is proxied by the density of labor force (aged 16-60) in 1977.[14] 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.[15] We also calculate the distance to Beijing to capture freedom in local policy-making. In Table 1B, consistent with previous studies using provincial variation, counties that were initially poor, that had larger production teams in 1977 and higher famine intensity in 1959-1961, and further from the central government decided to reform earlier. Controlling for grain output per capita in 1977 forces us to drop two thirds of the sample due to lack of data. To get close to the full sample with data on reform timing and explore the robustness, we omit grain output in the last column and find robust results for labor force density and famine intensity, though it decreases the standard error of distance to Beijing that becomes statistically significant.[16]

In all regressions on the effect of land reform, we control for the time-variant droughts in March and April in current year and the year before, as well as time-invariant determinants of reform timing by county interacted with time fixed effects.

3 Analytical framework

The effect of income growth on sex selection is theoretically ambiguous. Higher earning parents might rely less on sons for financial support and possibly have more savings in old age. On the other hand, sons might be normal good. Edlund [1999] suggest that, if parents not only want sons but married sons, parents from higher socioeconomic status would worry less about marriage market consequences of imbalanced sex ratio. Moreover, the costly sex selection becomes more affordable as income increases.

To attribute the effect of land reform on sex ratio to the effect of income growth, it is important to distinguish the income channel from other mechanisms through which land reform might affect sex selection. Sen [1990] hypothesizes that, the rising sex ratio following the reform could be induced by three institutional changes: the collapse of collective medical system, increase in household demand for manual labor and increase in demand for sons as old age support. If the cutbacks in health care supply had a pro-male effect, the effect would not depend on birth order or

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

[15]Meng et al. [2009] use a similar measure of famine intensity using the 1990 Census.

[16]Grain output per capita in 1977 is negatively correlated with distance to Beijing, and positively correlated with labor force density in 1977.

the sex of the previous child. The first institutional change is therefore less relevant for interpreting our findings, while the other two changes could potentially confound the income channel.

Previously, household production was strictly restricted. Under the collective system, every woman or man was assigned a job upon 16 years old by the production team, where 20-30 families worked together in common plots [Perkins, 1988]. After land was contracted to individual households, the immediate household demand for manual labor led to demand for sons. Obviously, this argument assumes that in general men have physical strength in agricultural production and receive higher reward under the new system.

The decollectivization also destroyed the safety net for the rural elderly. Since agriculture was collectivized in 1956, the Chinese Communist Party had committed to guarantee the elderly economic security by public welfare funds without relying on families. The law required each collective to support all member who could not work and who had no kin on housing, medical care, food, burial, etc. [Davis-Friedmann, 1983]. Following land reform, the collective no longer had welfare funds to support the elderly, while no similar welfare system was built in the 1980s. Sons became crucial for old age support, particularly for parents who had only daughters.

Although the data do not allow us to directly test each mechanism, evidence on heterogeneous effects of land reform on sex ratio is helpful to rule out implausible ones. We first test the argument on increase in old age support by testing heterogeneity by productivity gains after reform. Once land was contracted to individual households, the financial basis of the collective support was taken away, even if households were not more productive under the HRS. If increase in demand for sons to secure old age is the main motivation, we would expect a homogeneous effect of land reform on sex ratio regardless of changes in productivity.

We can also distinguish increase in total household income from demand for gender-specific labor or increase in gender-specific income. If demand for male labor and larger increase in male income contribute to the rising sex ratio, we would expect a larger effect of land reform where the agricultural production is more male labor intensive. To relate our evidence to the existing literature [Qian, 2008], we instead measure demand for female labor and female income by the fraction of women growing cash crops by county. Our approach does not rely on an assumption that a certain cash crop is gender specific. In the 1982 Census, 57 percent of workers growing cotton are women, 54 percent for vegetable, 42 percent for tea and 38 percent for fruit.[17] The variation in the fraction of female workers for all cash crops comes from the composition of cash crop mix and how female labor intensive each crop is at the local level.[18]

Finally, we break the sample by parental education. While there was no return to human capital in collectivized farming, Yang and An [2002] find that education improves the uses of household-

[17]In Qian [2008], one key assumption is that tea is picked mainly by women in China, which contrasts with the facts that less than half of tea pickers are women, and the fraction is just 4 percentage points higher than that of fruit and far below that of the other two cash crops.

[18]See summary statistics for the composition of cash crop mix and the fraction of women growing each cash crop by county in Appendix Table 1.

supplied inputs and contributes to higher agricultural profit under the HRS. If the reform affects sex selection through income, we would expect that better educated parents who earned more in household production are more likely to have sons after the reform.

4 Data and Empirical Strategy

4.1 Data

The microdata we use is the publicly released 1 percent sample of the 1990 Census. Our analysis focuses on rural areas which were defined as counties in the 1982 Census, the 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 minimal, because internal migration had been under strict control under *Hukou* system until its first relaxation in 1985 [Wang, 2005].

Our research design needs information on one's birth order and sex of previous children, which are not directly reported in the census data. The only question to identify one's position within a household is his/her relationship to the household head. Therefore, we focus on children of the household head. Our main analysis sample of second births is constructed as follows. First, we require that the number of children linked to the househead is equal to the number of survived births reported by their mother (spouse of household head or household head herself). Families where older children were always in 1990 are excluded. Second, we exclude families with multiple births, where birth order is more difficult to identify. Third, using information on each child's year and month of birth, we define one's birth order as well as the sex of the previous child. Finally, we restrict the sample of second births to children born in 1974 and 1986. The oldest cohort were 16 in 1990, who were unlikely to be away from home of parents, and were also more likely to be matched with the first child. For the analysis by parental education, we further restrict the sample to children in two-parent families.

An important reason for excluding children born 1987-1990 is to reduce the possibility of under-reporting. Parents may under report above-quota births since the introduction of One Child Policy. The underreporting rate in the 1990 Census is very low, only 0.06%, according to the National Bureau of Statistics (Zhang and Zhao, 2006). However, underreporting children aged 0-4 in the census year is a more common problem (Zhang and Zhao, 2006). Therefore, we use a sample of children born prior to 1987 that are closer to the actual population for the age group.

In our sample of births, one is defined as 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 as low as 0.63 percent. Thoughout our analysis, we use the 99.37 percent born in 1974-84 who resided in the same county in 1985 and all births in 1985-86.

Summary statistics of the full sample and the two-parents sample are reported in Table 3.

4.2 Empirical Strategy

We estimate the effect of exposure to land reform on the probability of second child being male using 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. Consider the estimation equation:

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

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 reform indicator $Post_{jt}$ 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 regressor of interest, $Post_{jt} * Girl^1_{ijt}$.

To tease out possibly 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 last 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.

The coefficient of interest, β_3, measures the effect of land reform on second child being male in families with a first girl relative to that in families with a first boy. Two identifying assumptions are crucial in this triple-difference strategy. The second births in families with no previous son provide the right counterfactual. Moreover, 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 change in the sex of the second child among the pre-reform cohorts does not differ by the sex of the first one. As noted in the introduction, Zeng et al. [1993] documents that the sex ratio of the first births is biologically normal. To be cautious, we directly test whether the reform affected the sex of the first child and fail to find an effect. We also provide transparent evidence of no pre-existing trends in the sex ratio of the second births for both families with a previous son and those without.

For the second identifying assumption, the potential threats come from other reforms during the same period. To confound the effects of land reform, other reforms should have followed the timing of land reform closely at the county level and have had differential impacts on the sex of the second child for families with a previous son and those without. We have conducted a comprehensive reading on reform policies from the late 1970s to the early 1980s and grouped them into three categories. The first type includes reforms that might lead to stronger desires for sons among parents with a first girl, but that were introduced in the same year nationwide. Typical examples are the price reform and market reform that came with land reform as a whole package of rural economic reforms. The increases in procurement prices and in bonuses for above-quota productions were in 1979 [Sicular, 1991], and the reductions in the planning of agricultural production and in the restrictions in interregional trade were also universal state interventions [Lin, 1992]. Any effects of exposure to reforms in this category are accounted for by controlling for year of birth effects δ_t.

Another type of reforms were introduced at different time during the same period, but their effect on the sex of the second child (if any) does not depend on the sex of the first one. As discussed in Section 3, the crisis in health care supply following land reform belongs to this category. We difference out possibly time-invariant effect of these reforms by controlling for the sex of the first child. Moreover, we provide evidence below that the sex ratio of the second child in families with a first boy remains constant before and after the reform, suggesting no sign for an effect of this type of reforms.

The introduction of One Child Policy stands out as the most likely confounder. Previous studies at the provincial level find that higher fines under OCP lead to higher sex ratio, especially at higher birth orders with no older brothers [Ebenstein, 2010]. OCP rules varied over time from 1979 to the present. For our purpose, the potential threat comes from the initial enforcement of OCP by county between 1978 and 1985 that might overlap with the county-level rollout of land reform. We have collected the most detailed data on the timing of OCP implementation by county to directly disentangle the effect of land reform from that of OCP.

5 Main results

5.1 Graphical presentation

We begin by plotting the sex ratio of first child by birth year relative to reform year, in Figure 3A. 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 control group.

Figure 3B shows sex ratio of the second child for families with a first girl and those with a first boy separately. Once again, little change in the sex ratio for families with a first boy in the pre- and post- reform periods demonstrates the validity of our research design. Among the pre-reform cohorts, while no pre-existing trend is observed for both groups, the sex ratio of second child in families with a first girl is constantly higher than that in families with a first boy by more than 10 percentage points. This illustrates son preference as a culture, that is, parents with no previous son always have stronger desire for a son. 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 in six years after the reform, while the ratio in families with a first boy remains nearly constant. The sharp contrast between these two groups in the pre- and post reform periods supports land reform as a driving force of the rising sex ratio.

5.2 Estimates

Column 1 of Table 3 echoes the message in Figure 3A. 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 statistically insignificant.

Column 2 presents the estimate for the effect of land reform on second child being male at the individual level. We find an increase in the probability of being male by 2.9 percentage points among families with a first girl relative to families with a first boy, and it is 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. Interestingly, it is slightly larger than the effect of son preference as a culture, as captured by the indicator of having a first girl, which is an increase of 2.7 percentage points. To translate the effect of land reform on male births to the effect on sex ratio, we also estimate equation (1) on sex ratio of second births by county and birth year. In column 3, sex ratio in families with a first girl increases by 0.15 following the reform, a precisely estimated increase of 12 percent and consistent with the visual impression from Figure 3B.

6 Mechanisms

Having found more missing girls following land reform, now we turn to three sets of heterogeneity tests that help to distinguish the income channel from other potential pathways.

A. Alternative hypothesis 1: increase in demand for old age support

As discussed in Section 3, if the rising sex ratio is induced by the increase in demand for old age support, we would not expect that the effect of land reform on sex ratio varies by post-reform changes in productivity. On the other hand, if income growth following reform is the main mechanism, we would expect a larger effect on sex ratio where productivity gains are larger.

We have collected unique data on grain production by county and year from the 1970s to the mid- 1980s from county gazetteers.[19] There are 400 counties that report both the reform timing and the complete year-by-year grain production from 1974 to 1984. In Panel A of Table 4, we first report the estimated effect of land reform on grain output per capita. In column 1, on average, HRS adoption increases grain output by 2.6 percent at the 10 percent significant level. We break the sample by change in grain output before and after reform. Column 2 shows a precisely estimated increase of 9.2 percent in counties above median of change in grain output at the 1 percent significant level, while column 3 shows a 3.9 percent decrease at the 10 percent significant level in counties below the median. Only counties above the median experienced an increase in grain output after the reform.

Using this subsample of counties, we present the estimated effect of land reform on 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 subsample. Nevertheless, this sample provides the unique opportunity to explore the treatment heterogeneity by changes in productivity. Column 2 shows a precisely estimated increase 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.

The heterogeneous effects of land reform by productivity gains suggest that the collapse of collective support for the elderly, which does not depend on changes in productivity, is unlikely to play the main role in interpreting our findings.

B. Alternative hypothesis 2: increase in demand for male labor

If the increase in sex ratio is mainly caused by demand for male labor or increase in male income, we would expect that the increase is smaller where the agricultural production is more female labor intensive. In Table 5, however, we fail to find evidence of heterogeneity by demand for female labor or change in female income, as measured by the county-level fraction of female workers growing cash crops. The estimate for the interacted effect of post-reform, first being a girl and

[19]Records on grain output in the 1970s are particularly valuable because county-level statistics have been released systematically only since the 1980s in China. They are also arguably reliable because these data were originally from local official archives [Xue, 2010].

the fraction of female workers is economically very small, a decrease of -0.02 percentage points, implying that a 10 percent increase in the fraction of female workers leads to a 0.2 percent decline in the probability of second child being male. It is also statistically insignificant. This estimate provides two important insights. The demand for labor after the reform is not gender specific. Moreover, higher female income does not lead to less sex selection.

C. Heterogeneity by parental education

Our evidence is most consistent with the hypothesis that increase in total household income following the reform led to higher sex ratio. To further explore the income channel, we test whether better educated parents respond to land reform differently compared to less educated ones. In Panel A of Table 6, we find that mothers with higher education levels are more likely to have a boy after the reform, and the difference increases with education level. All the estimates are statistically significant at the 1 percent level. The largest effect is found among mothers with a high school education, who are 6.5 percentage points more likely to have a son. Similarly results for fathers are presented in Panel B, though the differences between better educated fathers and less educated ones are smaller than those for mothers.

In Appendix Table 2, we find that counties with larger fraction of educated workers have larger increase in grain output following reform. This implies that better educated parents would be less liquidity constrained. Again, the income channel is supported by the heterogeneous effects of land reform on sex ratio by parental education.

These findings may also indicate that higher earning mothers had better access to sex selection technology, particularly prenatal sex determination using Ultrasound, which was very costly at that time. While the large scale diffusion of Ultrasound to counties started in 1985, only around 20 percent of cities and counties had introduced it by 1984 when land reform had completed (Chen et al. [2011]). Provincial gazetteers record that Ultrasound and other selection technologies (e.g. Amniocentesis) were only available in a few large cities in the late 1970s and early 1980s. Furthermore, our findings are consistent with the prediction in Edlund [1999] that high status parents would respond first to select because they are less concerned about the marriage market consequence of imbalanced sex ratio.

7 Robustness

7.1 One Child Policy

One Child Policy was officially announced by the central government in January of 1979. Distinct from the birth control policies that had relied on press campaign and propaganda since the early 1970s, the 1979 policy introduced a new system of financial incentives for birth control for the first time. Under the initial policy, parents who sign the one-child pledge would get rewards, while

those who give birth to three or more would suffer economic sanctions [Banister, 1987].[20] At the local level, the implementation of these rules was in a highly diverse manner [Scharping, 2003].

We have compiled new data on the year the county government issued the first policy document to enforce rewards for the single child and penalties for above-quota births, also from county gazetteers. In Figure 4, we plot the distribution of the timing of land reform and OCP from 856 counties that report the timing of both reforms. There are 56 percent of counties that enforced OCP in 1979, while 1 percent started as early as 1978 and the other 43 percent implemented between 1980 and 1985. Introduced during the same period, land reform and OCP show substantial difference in their timing at the county level, which allows us to separate the effect of land reform from that of OCP.

We assign treatment status to OCP as 1 for individuals born one year after OCP or later and 0 otherwise. In Panel A of Table 7, both land reform and OCP have little effect on the sex of the first child. In Panel B, column 1 shows similar estimates for land reform 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. When we control for both land reform and OCP in column 3, the estimate for land reform is very robust, while estimates for OCP become much smaller and statistically insignificant. These results suggest that the effect of land reform is not confounded by OCP. Moreover, they cast doubt on a common argument that OCP leads to higher sex ratio, which, however, could be confounded by land reform.

7.2 Fertility response

Land reform may increase the desire to have more children because the land distribution is based on family size, while OCP would decrease fertility. We first estimate the effect of land reform and OCP on total fertility. Consistent with expectations, Panel A of Table 8 shows that the number of births by county and year is increased by 2 percent by land reform, and it is decreased by 2 percent by OCP.

If the increased fertility after land reform concentrates in the second birth, our sample would be a selected one. However, the rule of land distribution only applies to authorized birth (the single child), implying that the margin of fertility affected by land reform would be most likely from zero to one. On the other hand, the effect of OCP would be on a different margin, that is, fewer second births. To test whether land reform affects the decision to have a second child, we use the year of birth of the first child and the stable birth interval to assign treatment status. Specifically, since 3-year is the common interval between the first and second child at that time, we assign 1 to the first child born 2 years before land reform or later and 0 otherwise. In Panel B, controlling for OCP, the effect of land reform on having a second child is very small and statistically insignificant.

[20]The initial policy did not set a rule for the second child. The policy was tightened to allow only a few types of families to have the second child in 1982, and it was relaxed in 1984 to issue the second child permit to a broader range of families (Greenhalgh, 1986; Scharping, 2003).

Interestingly, the effect on having a second child all loads to OCP, that is, OCP increases the probability of having a second child, but parents with a first girl are still more likely to have a second one after OCP.

Overall, these findings suggest that, on the margin of fertility response most relevant to our identification, we do not find an effect of land reform.

8 Discussion

We find that, following a first daughter, the second child is 5.5 percent more likely to be a boy following the 1978-84 land reform in China. This translates to a 12 percent increase in the county-by-year sex ratio of the second child. Evidence on heterogeneity is most consistent with the income channel: higher income following reform led to more sex selection.

Our results can infer the effect size of income growth on sex ratio. In the subsample where post-reform productivity gains are positive, grain output per capita increases by 9.2 percent following reform. The response in the county-by-year sex ratio of second birth is a 15 percent increase. These estimates suggest that a 1 percent increase in grain output per capita leads to a 1.6 percent increase in sex ratio of the second birth. To compare with the effect size in Figure 1, we estimate the correlation between GDP per capita and sex ratio using provincial level data between 1980 and 2000. In Table 9, a 1 percent increase in GDP per capita is associated with a 4 percent increase in sex ratio, controlling for province fixed effects and year fixed effects. Clearly, panel data analysis overestimates the effect of income growth on sex ratio.

The highly male-biased sex ratio could be achieved through various forms of sex selection, e.g. sex-selective abortion, abandonment, neglect, infanticide, etc. For the time period studied in our paper (the late 1970s and 1980s), infanticide was unlikely [Zeng et al. 1993]. It was banned by the 1950 and again 1980 Marriage Law. Among abandoned girls, some of them were adopted. Johansson and Nygren [1991] show that the adoption rates among live births were between 1 to 2.5 percent from the 1970s to 1987, and on average girls were 75 percent of all adopted children. This suggest the adoption rates among live girls between 1.5 to 3.75 percent, which indicates a 2.25 increase during this period. Using the 1990 Census, we find that the overall sex ratio (regardless of birth order) increased by 7.5 percentage points following land reform, suggesting 7.5 percent more girls missing after reform. Among them, 30 percent (2.25/7.5) might be adopted by demostic families.

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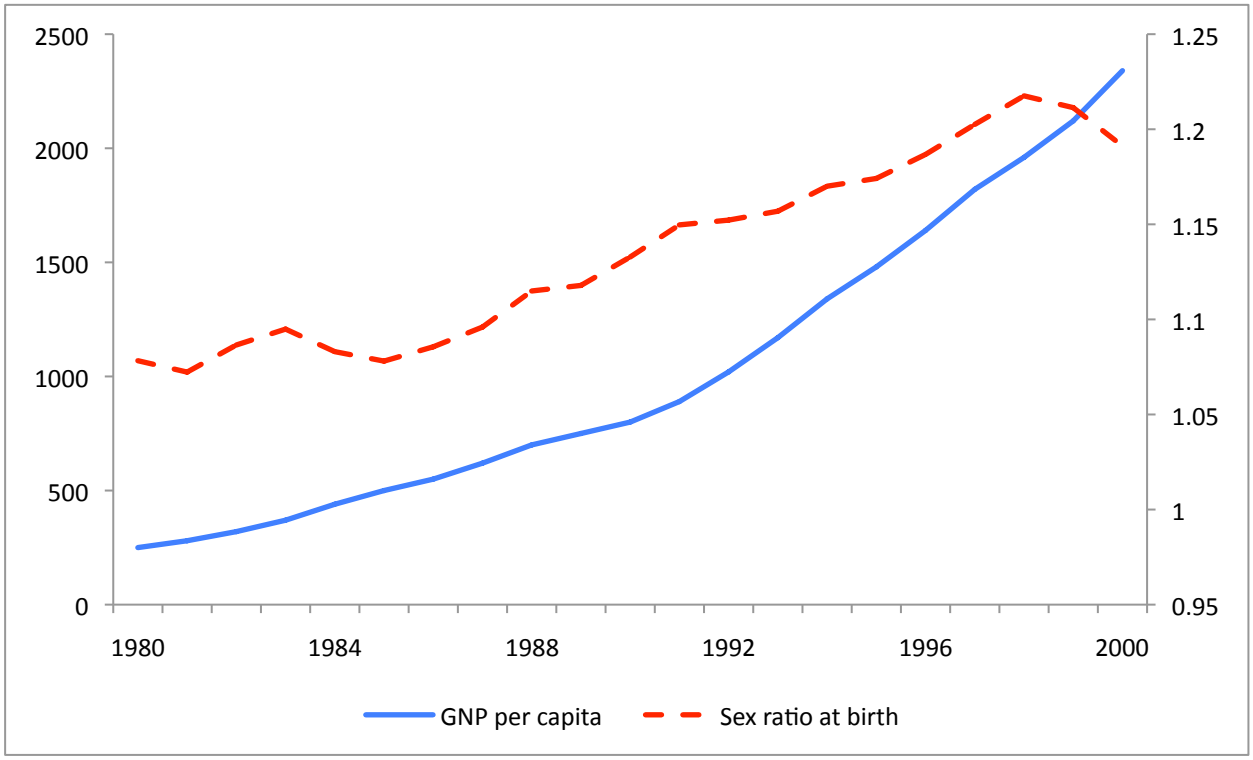
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Figure 1: Income and sex ratio at birth in China: 1982-2000



Notes: 1) Data on GNP per captia are from World Bank; 2) Data on sex ratios at birth from 1980 to 1990 are from 1% sample of 1990 Census, and sex ratios at birth from 1991 to 2000 are from 1% sample of 2000 Census.

Figure 2: Land reform timing: 1978-1984

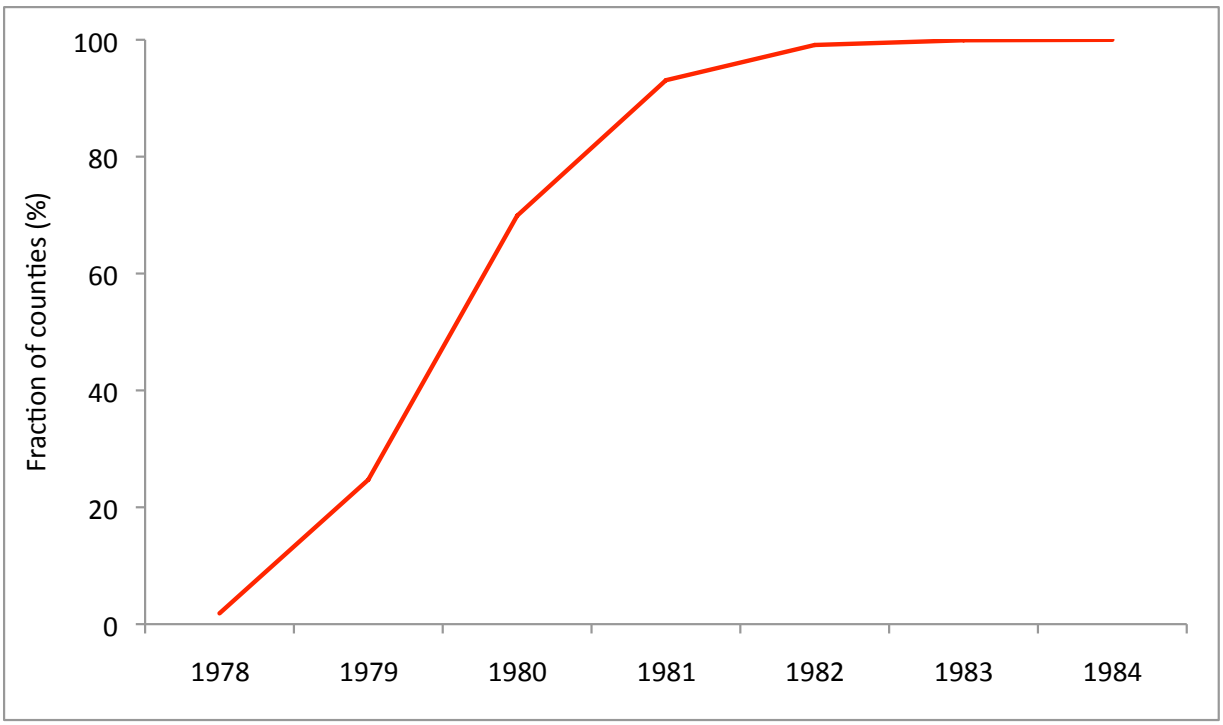


Figure 3A: Sex ratio of the first child

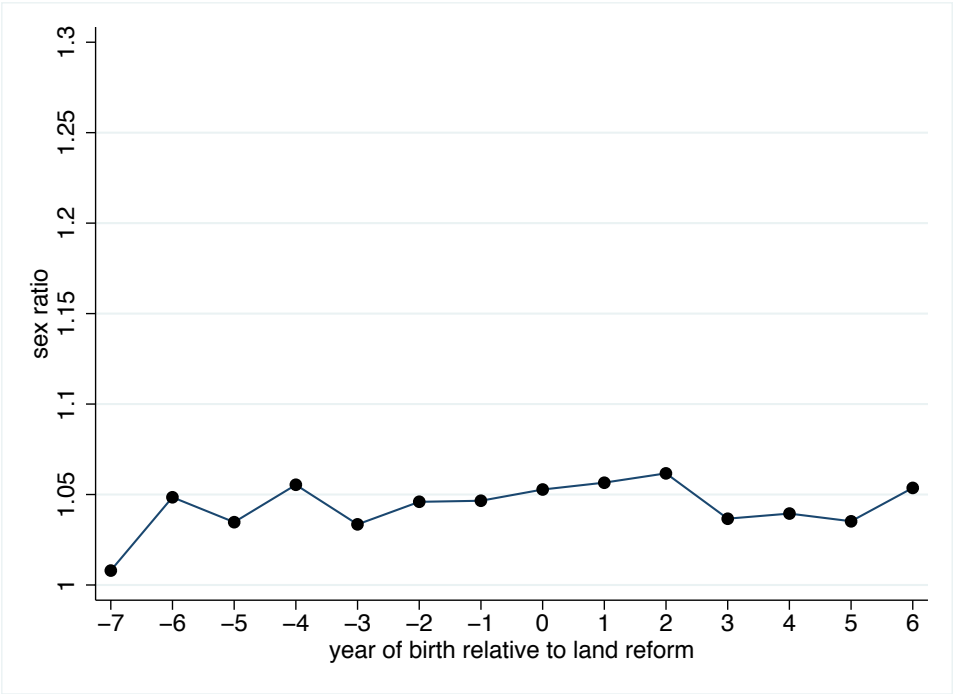


Figure 3B: Sex ratio of the second child



Figure 4: Timing of land reform and One Child Policy

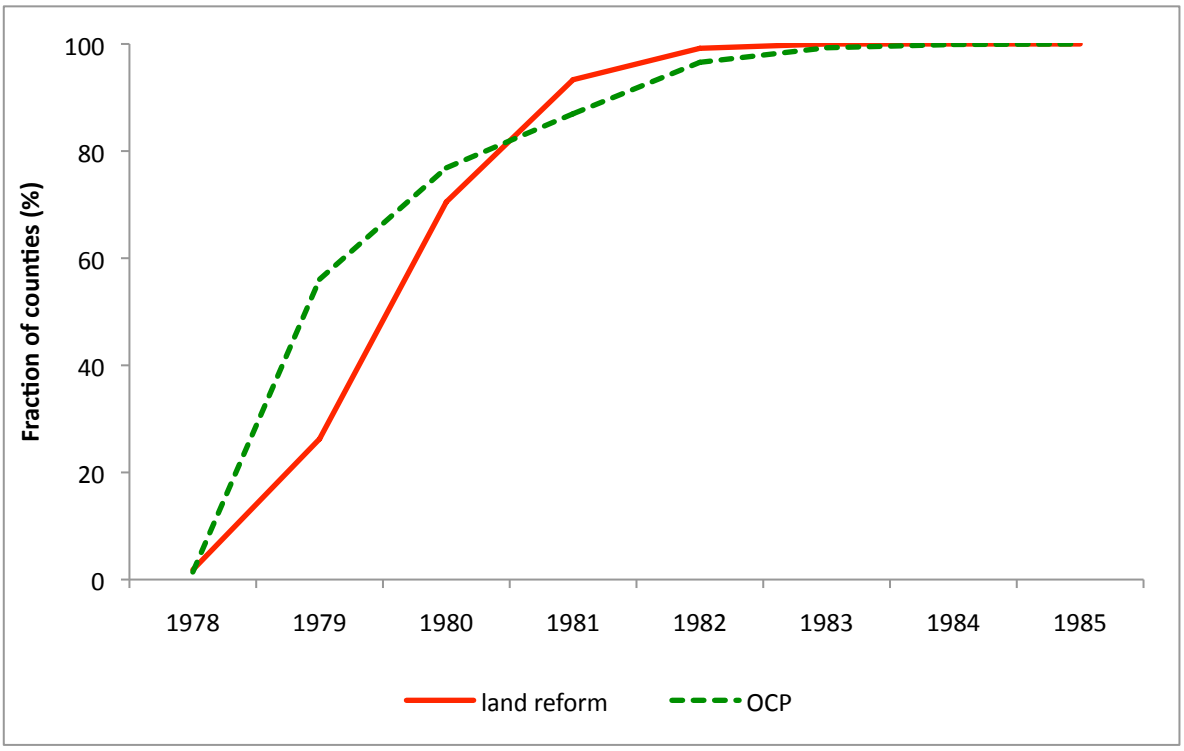


Table 1A: Drought (time-variant) and reform timing

	Dependent variable: first year of land reform (1978-1984)				
	(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 the first year of land reform, which is equal to 1 at the year reform started, 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 bottom 20th percentile in the precipitation distribution during 1957-1984 and 0 otherwise (precipitation data between 1965-1972 are unavailable). 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 in the whole period between March and September, while 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.

Table 1B: 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	0.399***	
	[0.121]			[0.127]	
ln (distance to province capital)	0.075**	1,201	0.003	0.004	-0.037
	[0.036]			[0.061]	[0.039]
ln (labor force density 1976)	-0.147***	1,117	0.044	-0.173***	-0.150***
	[0.022]			[0.046]	[0.028]
ln (famine intensity 1959-1961)	-0.494***	1,189	0.033	-0.289**	-0.342***
	[0.081]			[0.144]	[0.089]
ln (distance to beijing)	-0.074*	1,201	0.003	-0.123	-0.130***
	[0.038]			[0.078]	[0.041]
Observations				438	1,114
R-squared				0.095	0.070

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 regression includes all dependent variables. Data on grain output per capita in 1976 are collected from county gazetteers, and only 438 counties report this information. Distance to beijing and distance to province capital city are in kilometers and are obtained from GIS map of 1982 Census. Labor force density in 1976 is calculated by population size aged 16-60 in 1976 divided by area. Using 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. Robust standard errors are reported in brackets.

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 Primary school			0.607	0.515
Mother Middel school			0.272	0.170
Mother High school			0.075	0.037
Father Primary school			0.833	0.790
Father Middle school			0.504	0.408
Father High school			0.160	0.114
Observations	371762	279069	349351	260529

Table 3: Land reform, male birth and sex ratio

	Male=1		Sex ratio
	(1)	(2)	(3)
	First child	Second child	Second child
Post	0.003	-0.010*	-0.021
	[0.004]	[0.006]	[0.044]
Girl first		0.027***	0.136***
		[0.003]	[0.020]
Post*Girl first		0.029***	0.151***
		[0.004]	[0.030]
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 linear trends	X	X	X
Dependent variable mean	0.511	0.523	1.27
Observations	371762	279069	24,255
R-squared	0.006	0.011	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 for the effect on second child being male; column 3 for sex ratio of second births by county and year. 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 linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in

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

Table 4: Treatment 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)		
Post	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		
Post*Girl first	0.013*	0.027***	-0.004
	[0.007]	[0.009]	[0.010]
Post	-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 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%; ** significant at 5%; *** significant at 1%.

Table 5: Treatment heterogeneity, by the fraction of women growing cash crops

	Dependent variable: Male=1
Post*Girl first*% Women	-0.0002
	[0.0002]
Post*Girl first	0.039***
	[0.011]
Post	-0.02
	[0.013]
Girl first	0.02**
	[0.008]
Observations	256605
R-squared	0.011

Notes: Post*% Women and Girl first*% Women 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 the county-level fraction of women growing cash crops. 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. The regression includes county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

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

Table 6: Treatment heterogeneity, by parental education

	Dependent variable: Male=1		
	(1)	(2)	(3)
A. Mother			
Post*Girl first*High school	0.065***		
	[0.024]		
Post*Girl first*Middle school		0.035***	
		[0.011]	
Post*Girl first*Primary school			0.020**
			[0.008]
Post*Girl first	0.025***	0.021***	0.017***
	[0.004]	[0.004]	[0.006]
Observations	260529	260529	260529
R-squared	0.012	0.012	0.012
B. Father			
Post*Girl first*High school	0.033**		
	[0.014]		
Post*Girl first*Middle school		0.016*	
		[0.009]	
Post*Girl first*Primary school			0.014
			[0.010]
Post*Girl first	0.025***	0.020***	0.016*
	[0.004]	[0.005]	[0.009]
Observations	260529	260529	260529
R-squared	0.012	0.012	0.012

Note: Post*Parental eduation, Girs 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 linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

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

Table 7: Land reform, OCP and sex ratio

	Male=1		
	(1)	(2)	(3)
	Panel A: First Child		
Land reform	0.005		0.005
	[0.005]		[0.005]
OCP		-0.003	-0.002
		[0.005]	[0.005]
Observations	298310	298310	298310
R-squared	0.006	0.006	0.006
	Panel B: Second Child		
Land reform*Girl first	0.030***		0.031***
	[0.005]		[0.008]
Land reform	-0.012*		-0.013*
	[0.007]		[0.007]
OCP*Girl first		0.024***	-0.002
		[0.004]	[0.008]
OCP		-0.017***	-0.004
		[0.006]	[0.007]
Girl first	0.025***	0.028***	0.025***
	[0.003]	[0.003]	[0.003]
Observations	224600	224600	224600
R-squared	0.011	0.011	0.011

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. All regressions include county fixed effects, year of birth effects, county linear trends, initial county controls interacted with birth year effects and droughts in March and April of the current year and the year before. Robust standard errors clustered at the county level are reported in brackets.

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

Table 8: Land reform, OCP and fertility

	A. County-by-year number of birth		
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
	B. Have second child=1		
Land reform*Girl first	0.026***		-0.009
	[0.004]		[0.008]
Land reform	-0.016***		0.001
	[0.005]		[0.006]
OCP*Girl first		0.038***	0.046***
		[0.004]	[0.009]
OCP		-0.023***	-0.027***
		[0.004]	[0.006]
Girl first	0.040***	0.030***	0.031***
	[0.004]	[0.003]	[0.003]
Dependent variable mean			
Observations	298310	298310	298310
R-squared	0.343	0.343	0.343

Table 9: Income and sex ratio

	sex ratio	
ln(GDP per capita)	0.046***	0.042**
	[0.003]	[0.020]
Province FE	X	X
Year FE		X
dependent variable mean	1.12	
Observations	609	609
R-squared	0.582	0.598

Notes: This table reports estimate for correlation between GDP per capita and sex ratio by province and year. The data cover 29 provinces from 1980 to 2000.

Appendix Table 1: Female workers by crop in the 1982 Census

	full sample			counties where fraction of female workers growing cash crop > median			counties where fraction of female workers growing cash crop < median		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
A. fraction of agricultural workers for each crop (all counties)									
Cash Crop	1065	0.050	0.132	546	0.060	0.145	519	0.039	0.117
Cotton	1065	0.033	0.126	546	0.044	0.137	519	0.021	0.112
Vegetable	1065	0.011	0.043	546	0.011	0.048	519	0.011	0.037
Tea	1065	0.004	0.015	546	0.003	0.016	519	0.005	0.014
Fruit	1065	0.002	0.011	546	0.002	0.014	519	0.002	0.008
B. fraction of female workers for each crop (counties that grow some particular crop)									
Cash crop	935	0.485	0.227	416	0.669	0.143	519	0.337	0.165
Cotton	232	0.652	0.236	156	0.706	0.211	76	0.540	0.247
Vegetable	810	0.519	0.263	381	0.641	0.218	429	0.410	0.252
Tea	570	0.357	0.320	251	0.485	0.335	319	0.257	0.269
Fruit	407	0.308	0.331	163	0.420	0.360	244	0.233	0.287

Notes: the sample includes counties that are matched with the county-level data on reform timing, the 1990 and 1982 Census.

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

	ln(grain output per capita)		
Post*% High school	0.008*		
	[0.004]		
Post*% Middle school		0.004**	
		[0.002]	
Post*% Primary school			0.001
			[0.001]
Post	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 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%; ** significant at 5%; *** significant at 1%.