LAND REFORM AND SEX SELECTION IN CHINA
Online Appendices

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A Sex Selection Model

A.1 Chinese parents prefer just one son (not two)

In the China In-depth Fertility Survey (Phase I in 1985 and Phase II 1987)[1], parents are asked their sex preference for the next child, along with complete fertility histories. Figure A.1 shows that among rural parents, 79% want a son next when they have not already had one, which drops to 31% for families who already have a son.

Therefore, in the model, we assume that having a son brings utility to parents, and further that a second son provides no benefit beyond that from a daughter (we relax this assumption in Section A.5.2).

A.2 Model setup

We assume parents decide among: 1) having a second child with sex selection, 2) having a second child without sex selection, and 3) stopping childbearing following one child. We allow for potentially competing wage and income effects in the decision, as land reform may have affected both. We take the firstborn's sex as exogenous because first parity sex ratios are normal and there is no evidence that boys are de-selected, nor does the first parity sex ratio change after land reform, see Figure 4a. Thus our discussion on sex selection (Section

[1] Data source: `http://opr.princeton.edu/archive/cidfs/`. These surveys were conducted by the State Statistical Bureau of China in collaboration with the International Statistical Institute, covering 67,187 households in nine provinces: Hebei, Shaanxi, Shanghai, Gansu, Guangdong, Guizhou, Liaoning, Shandong and Beijing.

A.1) focuses on two-child families.[2] (In Section A.3.2, we allow parents to pick the first child's sex and show that in two-child families, sex selection would wait until the second child.)

Following a first child, parents maximize:

$$W(\texttt{second}, \texttt{son}, C) = N * \texttt{second} + M * \texttt{son} + V(C) \tag{1}$$

where N is the utility gain of having a second child ($\texttt{second} = 1$). We assume a unitary household and additive separability of son preference as in Edlund (1999).[3] $\texttt{son} = 1$ if either first or second child is male, or both, and $M>0$. V is concave in consumption C. Parents maximize $W(.)$ subject to the budget constraint:

$$Y + wT = (p_n + wh) * \texttt{second} + p_u * \texttt{select} + C, \tag{2}$$

where $\texttt{select} = 1$ if parents use sex selection to have a son. We assume that spending p_u (e.g., the price of ultrasound) guarantees parents a son.[4] p_n is the fixed cost of having a second child, w wage, h the time spent on childcare, and thus wh lost wages from time spent child-rearing. The price of other consumption goods is normalized as 1. We ignore the costs and benefits of first child since he/she is taken as given.[5] Thus p_n, wh, and N are the marginal costs and benefit of the second child.[6]

A.3 Sex selection

A.3.1 Second child & sex selection

After a firstborn daughter (denoted $\bar{f}$), parents who have a second child but do not sex select expect utility:

[2]Figure 2A also suggests that sex selection is absent in families with exactly one child, as it would raise the overall first parity sex ratio above 1.05.

[3]Land was distributed on a *per capita* basis, i.e., equally to men and women. Because neither household bargaining nor interactions between consumption, birth order, and child gender are required to explain our empirical results, rudimentary (1) is our point of departure. Further, if "custom or strong social traditions give all the power to one person (usually the husband) in the household", the unitary model may be more appropriate (Browning et al., 2014)

[4]Transit may be a substantial part of cost p_u: accessing an ultrasound machine during this time period would generally require train tickets to a provincial capital. Hotels there were geared to SOE workers, who received lower room prices. Urban hospitals were likewise expensive to farmers. Finally, p_u also includes psychological costs of sex selection.

[5]We assume parents have at least one child. When we allow sex selection of the first child in Subsection A.3.2, we allow an additional p_u to be spent on the first child.

[6]We impose no assumption about their magnitude relative to those for the first child, e.g., whether there are economies to scale in childrearing. Y and T in (2) represent income and time available after that expended on the firstborn.

$$W^*(1, \widetilde{son}, C|\bar{f}) = \beta(N + \ M + V(C^*)) + (1-\beta)(N + V(C^{**}))$$

$$= N + \beta M + V(Y + (T-h)w - p_n) \tag{3}$$

where β is the biological likelihood of a male, $\widetilde{son} = 1$ if the second child is male, and $Pr(\widetilde{son}) = \beta \sim .51$. Equation (3) uses additive separability and our assumption that girls and unselected boys cost the same, so $C^* = C^{**}$.

Parents of a firstborn daughter will sex select the second child if:

$$W^*(1, 1, C|\bar{f}) - W^*(1, \widetilde{son}, C|\bar{f}) \equiv \Delta^{ultra|\bar{f}}_{chance|\bar{f}} > 0 \tag{4}$$

$$= (1-\beta)M + V[Y + (T-h)w - p_n - p_u] - V[Y + (T-h)w - p_n] > 0 \tag{5}$$

At higher incomes, (5) is more likely to hold. Furthermore,

$$\frac{\delta \Delta^{ultra|\bar{f}}_{chance|\bar{f}}}{\delta Y} > 0, \ \frac{\delta \Delta^{ultra|\bar{f}}_{chance|\bar{f}}}{\delta w} > 0.$$

Sex selection increases in non-labor income and wages, both of which may have increased from land reform, conditional on there being a second child. Thus, selecting a son is a normal good. These predictions are also consistent with compliant sub-population being disproportionately higher-education families, to the extent they tend to have a higher Y and w. Turning to parents with a firstborn son:

$$W(1, 1, C|\bar{m}) - W(1, \texttt{son}(\widetilde{son}), C|\bar{m}) \equiv \Delta^{ultra|\bar{m}}_{chance|\bar{m}} < 0. \tag{6}$$

Abusing notation, $\texttt{son}(\widetilde{son})|\bar{m} = 1$, so sex selection incurs cost p_u with zero benefit.[7] (6) says parents would not sex select at second parity if they already have a son, consistent with our empirical findings.

A.3.2 First child & sex selection

Taking the first child as given simplifies analysis of the second child. We justified taking the first child's sex as exogenous above on empirical grounds. This assumption can also be justified as a prediction of an extended model for two-child families where sex selection can occur earlier.

[7] $\texttt{son}|\bar{m} = 1$ regardless of the second child's sex. In contrast, $\texttt{son}(\widetilde{son})|\bar{f} = 1$ if $\widetilde{son} = 1$ and 0 if $\widetilde{son} = 0$.

If we allow parents to select either the first child or the second child's sex (or both), conditional on having two children, they would never sex select the first child. Absent sex selection, fm and mf and mm yield identical consumption levels and utility (and higher utility than ff). Families who have a firstborn son by chance do not have to pay p_u. Families who have a firstborn daughter still have the option of paying p_u for second child. Thus, looking prospectively at the first two children, parents can be assured of a son with expected selection cost of $(1-\beta)*p_u$. Parents who sex select the first child also guarantee themselves a son but with a selection cost of p_u. Under no scenario would a 2-child family sex select the first child as it requires an additional expected cost of $\beta * p_u$ for no benefit. Note, p_u need not to be entirely the ultrasound cost, but also the cost of (train) travel to provincial capital and monetized psychological cost of sex-selective abortion(s). Therefore, in two-child families, the sex ratio of the first child is predicted to be $\beta/(1-\beta)$, as observed.

Families with a single child are excluded from our main analysis sample. For completeness, we note that in the subsample of 1-child families, land reform could, depending on model parameters, lead families to switch from having 1 unselected child to 1 sex-selected child, or switching from two unselected children to 1 selected child.[8] In contrast, (5) for two-child families is unambiguously increased by $\uparrow Y$ or $\uparrow w$.

A.4 Fertility

Childrearing costs are plausibly relevant and can "choke off" a positive fertility response (`second`=1) due to increased Y or w:

$$\Delta_{stop|\bar{f}}^{chance|\bar{f}} \equiv W(1, \widetilde{son}, C|\bar{f}) - W(0, 0, C|\bar{f})$$
$$= N + \beta M + V(Y + (T-h)w - p_n) - V(Y + Tw) \tag{7}$$

[8] Utility from stopping with one child whom you sex select $= W(0, \texttt{son}, C) = M + V[Y + wT - p_u]$. Considering prospectively having two unselected children:

$$W(1,\ \widetilde{son}\&\widetilde{son},\ C) = N + (2\beta - \beta^2)M + V[Y + wT - wh - p_n].$$

$$\triangle_{2\ chances}^{stop|ultra} = (1 - 2\beta + \beta^2)M - N + V[Y + wT - p_u] - V[Y + wT - wh - p_n]$$

has an ambiguous sign. The child portion of net utility depends roughly on $M \gtreqless 4N$. The consumption side of net benefit depends on $p_u \gtreqless hw + p_n$. Likewise $\frac{\delta \Delta_{2chances}^{stop|ultra}}{\delta Y} \gtreqless 0$ is ambiguous, as is: $\frac{\delta \Delta_{2chances}^{stop|ultra}}{\delta w} \gtreqless 0$.

has an ambiguous sign. For the comparative statics:

$$\frac{\delta\Delta^{chance|\bar{f}}_{stop|\bar{f}}}{\delta Y} > 0, \ \delta\frac{\Delta^{chance|\bar{f}}_{stop|\bar{f}}}{\delta w} \gtreqless 0$$

That fertility effect of wage increase is ambiguous in sign reflects competing substitution and income effects of wage increase. Which force wins[9] depends on:

$$T[V'(Y + (T-h)w - p_n) - V'(Y + Tw)] - hV'(Y + (T-h)w - p_n) \gtreqless 0 \tag{8}$$

where we interpret $T[V'(Y + (T-h)w - p_n) - V'(Y + Tw)] > 0$ as an income effect and $-hV'(Y + (T-h)w - p_n) < 0$ as a substitution effect. Equation (10) can be rewritten as:

$$\frac{V'(Y + (T-h)w - p_n)}{V'(Y + Tw)} \gtreqless \frac{T}{T-h} \tag{9}$$

where the inequality's direction depends on magnitudes p_n, h, $\frac{T}{T-h}$ and $V''(Y+Tw)\Big|_{-}$.[10,11,12]

Fertility after a firstborn son depends on:

$$\begin{aligned}\Delta^{chance|\bar{m}}_{stop|\bar{m}} &= N + M + V(Y + (T-h)w - p_n) - M - V(Y + Tw) \\ &= N + V(Y + (T-h)w - p_n) - V(Y + Tw) \gtreqless 0 \end{aligned} \tag{10}$$

Again, the direction of fertility effect is ambiguous and less likely to be positive for a small N.[13] Because $\frac{\delta\Delta^{chance|\bar{f}}_{stop|\bar{f}}}{\delta Y} = \frac{\delta\Delta^{chance|\bar{m}}_{stop|\bar{m}}}{\delta Y}$ and $\frac{\delta\Delta^{chance|\bar{f}}_{stop|\bar{f}}}{\delta w} = \frac{\delta\Delta^{chance|\bar{m}}_{stop|\bar{m}}}{\delta w}$, the tension between income and substitution effects is balanced at the same p_n, h, $\frac{T}{T-h}$ and $V''(T+w)\Big|_{-}$ as given by (9) regardless of firstborn sex.

[9]The total derivative of (7) $\frac{\delta}{\delta Y}\Delta Y + \frac{\delta}{\delta w}\Delta w$ is positive if $\frac{V'(Y+(T-h)w-p_n)}{V'(Y+Tw)} > \frac{\Delta Y - T\Delta w}{\Delta Y + (T-h)\Delta w}$: likewise ambiguous, depending also on magnitudes of ΔY v. Δw.

[10]Between sex selecting the second child and stopping childbearing, $\Delta^{ultra|\bar{f}}_{stop|\bar{f}} \equiv W(1,1,C|\bar{f}) - W(0,0,C|\bar{f}) = N + M + V(Y + (T-h)w - p_n - p_u) - V(Y + Tw)$. $\frac{\delta\Delta^{ultra|\bar{f}}_{stop|\bar{f}}}{\delta Y} > 0$. $\delta\frac{\Delta^{ultra|\bar{f}}_{stop|\bar{f}}}{\delta w} = (T-h)[V'(Y + (T-h)w - p_n - p_u)] - TV'(Y + Tw)] \gtreqless 0$. Compared to equation (7), because wage increases make sex selection more affordable, parents are more responsive in sex selection over stopping than taking a chance over stopping.

[11]Unsuccessfully, we attempted to find of a way to separate income and substitution effects. Grandparents residing in the household might help drive h towards 0, but they likely affect other model parameters, including M, income, etc. Therefore, we do not think grandparents provide traction. Empirically, we did not find striking heterogeneity in land reform effects by co-residence of grandparents. (In the cross-section, households with grandparents are more likely to have a second child.)

[12]Similarly, when we compare sex selecting the second child versus stopping childbearing following a firstborn girl, predictions are ambiguous, and the inequality's direction further depends on p_u.

[13]When we compare sex selecting the second child versus stopping childbearing following a firstborn boy, again, predictions are ambiguous, and the inequality's direction further depends on p_u.

A.5 Extensions

A.5.1 Son-rearing cost

If raising a son costs more "out of pocket" than a daughter (e.g., marriage costs, wherein parents of the groom pay for the wedding), the budget constraint (2) can be extended to allow an additional cost p_m:

$$Y + wT = (p_n + wh) * \texttt{second} + p_u * \texttt{select} + p_m * (\texttt{\#sons}) + C \tag{11}$$

Nevertheless, qualitative predictions on sex selection under (11) remain the same as in Section A.1 and A.2 (where $p_m = 0$). The fertility effect of wage increase is ambiguous in sign, and magnitudes of income and substitution effects further depend on p_m.

A.5.2 Utility benefit from a second son

We can also relax the assumption of (1) that a second son provides no direct utility benefit beyond that from a daughter. Following a first child, parents maximize:

$$W(\texttt{second}, \texttt{\#sons}, C) = N * \texttt{second} + \psi(\texttt{\#sons}) + V(C) \tag{12}$$

where $\psi(.)$ is concave and increasing in the number of sons.

Two predictions under (12) deviate from those under (1). First, following a firstborn son, the utility gain from a second son introduces a tendency to sex select the second child, depending on the magnitude of the utility gain versus the cost of selection; increased income or wages continues to increases sex selection of the second child, as in Section A.3.1. Second, if we allow sex selection to occur at the first parity, parents considering $\psi(\texttt{\#sons})$ have a tendency to sex select the first child. As income or wage increases, parents are more likely to sex select the first child. This tendency is absent when a second son provides no benefit beyond that from a daughter, as in equation (1).

One interpretation of Figures 2A and 2B is that concavity of $\psi(.)$ is pronounced and reasonably approximated by $M * \texttt{son}$. This is also consistent with discussion of son preference in Section 3.

A.5.3 Productive sons

We extend (2) to allow sons to increase wage income:

$$Y + wT + z(\texttt{\#sons}, w) = (p_n + wh) * \texttt{second} + p_u * (\texttt{\#selected}) + C, \tag{13}$$

where $z'_{\texttt{\#sons}}(\texttt{\#sons}, w) > 0$. The functional form of z (including z'_w) might depend on the particular mechanism considered: productivity benefit of sons versus pro-male land distribution versus old-age support from sons.[14] Regardless, (13) introduces a benefit to having a second son beyond N, which either a girl or a boy could procure, and thereby a tendency to select the first of two children and to select the second child after a firstborn boy.[15] A necessary condition for this to occur is $z(2, w) - z(1, w) > p_u$. That is, if the marginal product of the second son exceeds the cost of sex selection, parents may select the first child's sex and the second child's sex following a firstborn boy (prior to land reform).[16] *Cf.* Section A.3.2, where sex selecting first child is never optimal. As the wage increases, under certain parameters, parents are predicted to be more likely to select the first child's sex and the second child's sex following a firstborn boy.

That said, additional assumptions can eliminate these wayward tendencies in the "productive son" model. Specifically (and intuitively), parents do not sex select the first child and the second child following a firstborn boy if the cost of sex selection exceeds or equals the marginal product of a second son:

$$z(2, w) - z(1, w) <= p_u, \tag{14}$$

In Figure 2A and 2B, sex ratios are normal among first births and second births following a firstborn boy prior to land reform, nor do they change with land reform. So we need (14) to hold in order to "discipline" the productive son model. Further, to generate more selection on the second child's sex after a firstborn girl as income or wage increase, the following should also hold:

$$V'[Y+(T-h)w+z(1)-P_n-P_u] > \frac{1}{2}V'[Y+(T-h)w-p_n]+\frac{1}{2}V'[Y+(T-h)w+z(1)-p_n]. \tag{15}$$

In sum, "productive sons" *can* explain Figure 2A and 2B under the "right" assumptions. Where the "productive sons" model falters is on $z'_w(1, w)$. To the extent we think that $z'_w(1, w)|male\ crop > z'_w(1, w)|female\ crop$, this implies more sex selection of the second child $|\bar{f}$ when the (son) wage increases. But we do not see more sex selection in areas more suitable for male labor intensive crops ("fruits" areas, see Section 7.2), where land reform may have favored male labor. Productive son models will generally predict more sons when

[14] Collapse of old age support could be modeled as (13) with $\Delta Y < 0$.

[15] If we set M=0 (a daughter and son provide the identical utility benefit N), (13) could explain "on its own" sex selection $|\bar{f}$ following land reform.

[16] We ignore discounting of son's productivity benefit, which would make $z(2, w) - z(1, w) > p_u$ more likely to hold.

the male wage increases (also intuitive).

For comparison, and to recap, our basic model in (1) and (2) has unambiguous predictions of zero selection of sons for the first child and the second child following a firstborn boy, and more sex selection on the second child following a firstborn girl as a response to income or wage increases. The maleness of income does not enter into (1) and (2), nor its predictions, as observed empirically. However, our measures of gendered income, while improved by leveraging previously ignored 1982 occupation and industry microdata, remain crude so we may fail to detect the true income heterogeneity.

A.6 Special case: $N = p_n = p_m = h = 0$

We describe this special case of (1) and (2) because it is simplest and consistent with our major empirical findings regarding land reform and sex selection.

Land reform has an unambiguously positive effect on sex selection. This prediction is true regardless of whether land reform increase non-labor income, wages, or both. Fertility is not predicted to change with land reform. The increase in sex selection is accounted for by parents with a firstborn daughter who switch from having a son at parity two by chance to sex selection. Parents with a firstborn son are indifferent among all "natural" options, and this indifference does not change when Y or w increases.

On the other hand, not only are N, p_n, p_m and h possibly non-zero in reality, but the special case cannot rationalize families where the only child is a daughter.

B Additional Data Description

B.1 Why Gazetteer data should be believed

B.1.1 Data are not used for cadre evaluation

In Figure 3 of the main text, we use the annual gross production of grain and population in 1974-89 from county Gazetteers to measure grain output per capita. During the second half of this period, an alternative source of grain production data exists: county yearbooks published in 1980, 1985 and 1987, which are commonly used by researchers.[17] These two sources were collected for different uses by different governments. Gazetteers are compiled by local historians to record local history and be kept locally; data in yearbooks are reported to the upper level government and are used in evaluating local officials. Grain output data from two sources are highly correlated (correlation=0.79), but they are not identical. In 82% of county-year observations, we find data discrepancy. Below we conduct two empirical tests to understand which series is more reliable when they disagree:

1. We compare both data to a third and independent benchmark. Previous literature documents that crop suitability and rainfall are predictive of agricultural output. We predict both sources of grain output by crop suitability and rainfall data from non-government sources. The county-level crop suitability indices based on agro-climate conditions are from the FAO Global Agro-Ecological Zones database. Rainfall data are from weather stations in China provided by the US's National Climate Data Center.

   In Table A.1, column (1)-(2) show that crop suitability indices and rainfall are more predictive of grain output data from Gazetteers, compared to data from yearbooks. In column (3)-(6), we focus on a subsample of counties where the two sources of data show discrepancies. When the discrepancies are non-trivial in column (5)-(6), Gazetteer data are more consistent with the benchmark.

2. We apply Benford's law to both data. Benford's law states that in many naturally occurring collections of numbers, the leading significant digit is likely to be small. In Figure A.2, Gazetteer data are more consistent with the Law.

B.1.2 Land reform rollout is consistent with government policies and the existing literature

Our key identifiying assumption is that *trends* would be the same in the absence of land reform. In the main text, Figure 4a and 4b show no trends prior to the reform. Moreover,

[17] County yearbooks were not published before 1980.

in equation (1) and (2), any time variant county characteristics that are correlated with the reform rollout are absorbed by county-by-year fixed effects. Nevertheless, to test how consistent the county-level reform rollout is with government policies and the existing literature, here we investigate variation in the rollout data.

As discussed in Section 2.1 of the main text, poor counties were among the first permitted by the central government to adopt the HRS. In addition, the existing literature has examined three other hypotheses in the province-level reform rollout (Lin, 1987; Yang, 1996; Chung, 2000). First, the diffusion of HRS was faster where the reduction in monitoring cost was higher and thus productivity gains larger. Second, provinces that suffered more from the 1959-61 Famine reformed earlier because they were more disenchanted with collective farming. Lastly, provinces farther from Beijing had more freedom to initiate reform earlier.

In Table A.2, we test the correlation between reform timing and potential determinants prior to the reform. At the county level, poverty is captured by grain output per capita in 1977. The size of production team is proxied by the density of the labor force (aged 16-60) in 1977. 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.[18] We also calculate the distance to Beijing to proxy for discretion in local policy-making. We find that counties that were initially poorer, had larger production teams and higher famine intensity adopted the HRS earlier, consistent with previous studies using provincial data.

Note that the correlation between reform timing and the baseline sex ratio at birth in 1975-77 is very small and not statistically significant. If the sex ratio increases from 1.07 to 1.17, the point estimate implies that land reform would be earlier by 4.6 days, small compared to our unit of analysis: year. Therefore, the underlying tendency of sex selection at the county level is uncorrelated with the reform rollout. A final note is on explanatory power. The R^2 is 0.096 when all initial controls are included. In a simple test on how much county fixed effects alone predict reform status by county and year, we find that the R^2 from including county FE alone is very close to 0.096, suggesting these pre-reform county characteristics may indeed capture the predictors of reform timing.

B.1.3 Fertility responses to the OCP rollout are consistent with national trends and the rural policy

The OCP fertility effect we estimate from the county rollout "scales up" to replicate national fertility trends: both are modest. The national fertility trend in Cai (2008) (cited in Figure 1b of the main text) suggests that the change in Total Fertility Rate (TFR) following the national introduction of OCP in 1979 was very small. In column 1 of Table A.3, we estimate

[18]See Dyson (1991) on fertility response as a famine metric in South Asia.

the size of change in the national rural TFR level following 1979. The decrease is 5 percent (0.187/3.5). Using province-level rural TFR in column 2, we find a smaller decrease of 3 percent (0.118/3.5). These point estimates are similar to our estimate using the county-level OCP rollout data in the main text (3 percent decrease in the number of births). The consistency in effect sizes suggests that the county-level OCP rollout data correspond well to the local policy enforcement. Note that estimates using the national policy in 1979 are statistically insignificant. In contrast, using county-level rollout in the paper gives us more precise estimates.

Moreover, we find that the margin of fertility response to the OCP rollout is consistent with the rural policy that fines are charged for the third child or above. In the main text, we find that the OCP had little effect on the number of second children. In column (1) of Table A.4, we find that the number of 3+ births decreased by 12.2 percent following the OCP, statistically significant at the 1 percent level. In column (2), we find less fertility reduction in families with no previous son, compared to those who already had one.

On land reform, column (1) shows suggestive evidence that the reform might have increased the number of 3+ births (marginally significant at the 10 percent level), but this effect does not differ by whether the family has a son in column (2), suggesting no change in the composition of first-girl families versus first-boy families that would affect estimates using all 2+ births in Table A.11. Nevertheless, the sample of second children appears to be the "cleanest" (no land reform effect on fertility) and therefore is used as our main sample in the main text.

B.2 Census microdata: the sample of families is representative

We analyze families where the number of children linked to the household head is equal to the number of surviving births reported in the 1990 Census. A natural question would be: did we exclude families with an older first child living outside the household in 1990?

To assess, we compare the birth year distribution of the first child (who are matched to our second child) in the 1990 Census to that in the 10% sample of the 1988 national two-per-thousand Population Sampling Survey on Fertility and Contraceptives, the latter of which does not suffer from a sample selection problem because parents report every child's birth year, birth order, and sex. If we have had excluded a substantial number of families with an absent older first child, we would expect more older cohorts (i.e., first births before 1974) in the 1988 Fertility Survey compared to that in the 1990 Census. In Figure A.4, the birth year distributions of first children in these two dataset are nearly identical, particularly before 1974, reducing concerns about missing firstborn children.

C Robustness of main results

C.1 Including children under age 4 in 1990

In the main text, we focus on children born 1974-86 in the 1990 Census because child underreporting is more common under age 4 in the census year (Zhang and Zhao, 2006). Here we examine the robustness of our estimates to including children under age 4. Table A.7 shows that the trend break estimate is the same as that in Table 1, and that the average effect is larger when more post-reform cohorts are included.

C.2 Including families that report a different number of surviving children than that observed in the family

In the main text, we use information on one's relationship to the household head to identify the household head's children and order these children 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. 81% of women report the number of surviving births equal to the number of children observed in their family in the census. Here we check the robustness of estimates by including families that report a different number of surviving children than that observed in the family. Table A.8 replicates column (1) and (2) of Table 2 in the main text and shows that these estimates are similar to estimates in Table 2.

C.3 All 2+ births

In the main text, we focus on second children in all families that have at least two children. Table A.11 show similar findings among all births at the second parity or above. The estimated trend break in the fraction of males among all 2+ births following no previous son is 0.8 percentage points, slightly larger than the estimate on the second births (0.7) in the main text. This is consistent with the observation of more sex selection at 3+ parity following no previous son (Zeng et al., 1993). The estimated average reform effect is a 3.8 percentage points increase in the fraction of males in families with no previous son, also slightly larger than the estimate of 3 percentage points among second births. All estimates are statistically significant at the 1 percent level and are robust to including the OCP.

C.4 Including the 1.5 Child Policy

We find evidence in the main text that the county-level rollout of the One Child Policy in 1979-83 does not confound the effect of land reform. After 1984, stricter enforcement of the rural OCP was introduced as the so-called "1.5 Child" Policy, which imposed fines for second children born after a boy (Greenhalgh, 1986; Scharping, 2003). By 1984, land reform had been completed in all counties. Therefore, the 1.5 Child Policy is not a confounder to the county-level rollout of the land reform. Here our questions is: does the 1.5 Child Policy explain some of the effect on sex ratios in the later period, 1984-86, in our sample?

In the province-level rollout data on the 1.5 Child Policy (county-level information unavailable), 8 provinces introduced the policy by 1984, 15 by 1985, and 23 by 1990.[19] In Table A.12, we include land reform, the OCP and the 1.5 Child Policy. First of all, the estimated effect of land reform is very similar to that in column (2) of Table 2 in the main text. And again, the OCP does not affect the fraction of males. We also find that the gender-specific 1.5 Child Policy increased sex ratios: the fraction of males following a first girl increased by 2.2 percentage points after the 1.5 Child Policy. These findings suggest that our estimates on land reform are robust, and the 1.5 Child Policy is an additional factor that contributes to increases in sex ratios since the mid-1980s.

C.5 Inverse probability weighting

We find in Table 3 of the main text that, following the OCP, families with a firstborn boy are more likely to stop having a second child than families with a firstborn girl. This finding raises a concern about the estimates on the OCP effect in Table 2: does the change in composition in families with a firstborn girl *versus* those with a firstborn boy after the OCP affect the estimated (zero) effect of the OCP on fraction of males among second births?

Here we use an inverse probability weighting approach to test the robustness of estimates in Table 2. In the sample of second children, we first predict the effect of the OCP on the probability a second child in a first boy family. Because fewer first boy families are in the sample of second births due to the OCP, we use the predicted probability to assign larger weights to first boy families where the OCP effects are larger. We then run weighted regressions analogous to column (1) and (2) of Table 2. In Table A.14, we find that weighted estimates are very similar to unweighted ones from Table 2.

[19] The policy rollout data are from two sources: 1) the chapter on birth planning policies in provincial gazetteers; 2) Scharping (2003) chapter 6.4.

D Additional analysis of mechanisms

D.1 Income mechanism: heterogeneity in reform effect by income growth

The income mechanism discussed in the main text predicts that larger wage or income gains from land reform would lead to more sex selection. We present in the main text that high education parents sex select more after land reform. Here we examine the heterogeneity by county-level income gains in the 415 counties where county-by-year grain data are available. Did sex ratios increase more in counties where land reform led to higher income growth?

Panel A of Table A.15 reports the estimated trend break in grain output after land reform. Column (1) reports the estimate in the full sample, a 3.5 percent increase in grain output per capita per year after the reform, statistically significant at the 5 percent level. In column (2) and (3), while the trend break is as large as 10 percent in counties that experienced larger growth, it is much smaller and statistically insignificant in other counties. In Panel B, we compare the trend break in the fraction of males following a first girl in counties with larger *versus* smaller growth in grain output, from estimating equation (1) in the main text. We find a large trend break in the fraction of males in counties where grain output grew rapidly after the reform, a 1.1 percentage points increase per year, statistically significant at the 5 percent level. In contrast, no trend break in sex ratios is found in counties where grain output had little growth. These findings are consistent with the income mechanism.

D.2 The "productive son" mechanism

To test the "productive son" mechanism, we compare land reform's effect on sex ratios in counties which are more suitable for growing male-labor intensive crops *versus* those more suitable for growing female-labor intensive crops.

First, which crops were more male-labor intensive? Using the "unharmonized" occupation and industry codes in the 1982 Census microdata, we find that, cotton was the most female-labor intensive: only 35% of workers who grew cotton were male. Fruit appears to have been most male-labor intensive: 69% of workers who grew fruit were male.

We then measure gender specific income using crop suitability indices based on agro-climate conditions from the FAO Global Agro-Ecological Zones (GAEZ) 2012 database. FAO estimated the potential yield of each crop and crop suitability in each 0.5-degree-by-0.5-degree grid cell, given an assumed level of crop management and input use.[20] We

[20] The crop suitability indices are based on intermediate input level. Water supply is rain-fed. Each index ranges from 1 to 7, the higher the more suitable. Scale 1 indicates water, not suitable or very marginal, 2

aggregate the crop suitability indices to the county level. We focus on three sets of crops: 1) cotton, the most female-labor intensive crop; 2) grain (wheat and wetland rice) and tea, the mildly male-labor intensive crops; 3) fruits including citrus and banana, the most male-labor intensive crops. In Table A.16, we test the first-order effect: do these crop suitability measures predict actual cropping patterns in China? We find that wetland rice and wheat suitability indices are positively correlated with sown area of grain and grain output in 1980, statistically significant at the 1% level. Similar results are found for the cotton suitability index.[21]

In Table A.17, we interact each crop suitability index with Land reform*Girl first. None of these estimates are statistically significant. The index for cotton, the most female-labor intensive crop, has an opposite sign as the "productive son" mechanism would predict. Thus, despite using improved and previously-unanalyzed census microdata on industry and occupation of parents, we do not observe pronounced heterogeneity by gendered wages.

D.3 Analysis of other potential mechanisms

In addition to the income mechanism and "productive son" mechanism discusses in the main text, here we investigate five other potential mechanisms.

1. Was land distribution male biased?

In the model of Subsection A.5.2, additional land from sons operates similarly to a productivity benefit of sons, which increase wages or income in equation (13). As with the "productive son" mechanism, our model predicts that parents have a tendency to select the first child's sex as well as the second child's sex after a firstborn son, which is not observed in Figure 4a and 4b in the main text.

Moreover, we test directly if land distribution was on average pro-male in rural China. Specifically, we examine whether families that "lost" female labor suffered in land reallocation. Households report their land holding every year in a nationally representative household-level panel data from the Rural Fixed Point Survey, conducted by the Ministry of Agriculture since 1986. We use household data for 1986-89 and a household fixed-effect specification in Table A.18. The correlation between the change in the fraction of female labor and the change in family land size is very small and not statistically significant, and the sign goes the opposite way as the hypothesis would predict.

for marginal, 3 for moderate, 4 for medium, 5 for good, 6 for high, and 7 for very high.

[21] Sown area and output data on fruits and tea in the early 1980s at the county-level are not available.

2. Changes in old age support

If land reform destroyed the financial basis of the "collective pension system" and forced parents to rely on their sons for old age support, this could be captured as introducing wage benefits from sons to the model as well as a drop in (pension) income of parents in Appendix Section A. In Subsection A.5.2, we consider these changes in (13), including that sons' earnings might serve as a buffer. Again, these wage benefits from sons introduce a tendency to sex select the first child and the second child following a firstborn son, which again is not detected in Figure 4a and 4b.

Cohabitation with exactly one son can be "allowed" by a sufficiently concave $z(\#\texttt{sons}, w)$ in the number of sons in the model, which tempers the incentive to select a second son. That said, the elderly in rural areas often lived with sons even during the collective period; we conceive of changes in collective support as more related to income Y. (See also discussion on the "productive son" mechanism in Appendix Section D.2.)

3. Compromised rural healthcare

Compromised health care following the collapse of the collective system could harm child health. Income growth, on the other hand, would tend to improve child health. Although we cannot directly separate these two opposing channels, we can test the net effect of the reform on infant mortality in the UNICEF 1992 Chinese Children Survey (health indicators are unavailable in the census data).[22]

In column (1) of Table A.21, we find that infant mortality among all births decreased by 0.5 percentage points after land reform, statistically significant at the 5 percent level. The effect size is a 20% reduction from the pre-reform infant mortality rate. This finding suggests that the impact of the deterioration in health care, if any, would not offset the health benefits of income growth from land reform. We also find in column (2) that the effect of land reform on infant mortality among second births does not differ by the sex of the first child, which reduces the concern of changes in composition for our estimates in the main text.

There is no evidence that the large increases in sex ratios after land reform coincided with a major deterioration in infant health. Again, the large (and previously undocumented) improvement in infant mortality is consistent with income improving health.

4. Reduced oversight of sex selection

If decollectivization weakened the authority of village administration, it may have become more difficult for village leaders to monitor and enforce its goals and policies. This channel

[22] The survey covers 522,371 households from 1088 counties in 29 provinces.

requires that the collective was thwarting sex selection *before* land reform, in the 1970s. This was not the case. Only in 1986 did the Ministry of Health and the State Family Planning Commission issue its first ban against prenatal sex screening: "Notice on Forbidding Prenatal Sex Determination" (Chu, 2001). In 1994, the State Council of China codified the ban into law in "Document No. 25" (Article 32 in Law on Maternal and Infant Health Care of the People's Republic of China in 1994).

During the 1970s, when ultrasound technology became available in a few provincial capitals, Rigdon (1996) describes a sanguine attitude among medical professionals toward sex selection. A 1975 article written by the Gynecology and Obstetrics Department of an Anshan Hospital reported 29 of 30 abortions being performed after fetal sex predictions were female used language that does not suggest the doctors thought this was wrong or unusual: "If the predicted fetal sex is not in agreement with the parents' wishes and artificial abortion is performed..." (Tietung Hospital of Anshan Iron and Steel, 1975)".[23] Anecdotally, we also spoke to nine doctors who worked in village clinics in the 1970s and the 1980s. At that time, induced abortion, sterilization and insertion of IUD for women with "too many" births were important parts of their job. However, they were not told to monitor sex selection.

In summary, stemming sex selection was neither a policy nor a professional goal before or during land reform. Therefore any weakening of village administration is unlikely to foment sex selection.

5. Travel restrictions

Institutionally, introduction of the HRS did not free peasants to travel. In China, all travel required official permission from local governments until the late 1980s, when the government started to issue national identification cards to citizens (de Brauw and Giles, 2017). Throughout our study period of 1974-86, and in the absence of individual IDs, a travel permission letter was required for peasants to buy train tickets, check in to urban hotels, and go to urban hospitals.

Empirically, changes in travel restrictions alone cannot explain why more sex selection occurred among high education parents and in counties with higher output growth. Furthermore, some of the sex selection induced by land reform was through postnatal mortality (Appendix Section E), for which travel is presumably less relevant.

[23] Available at URL `http://124.205.33.103:81/ch/reader/view_abstract.aspx?file_no=1975-2-117&year_id=1975&quarter_id=2&flag=1`

E Additional analysis of selection methods

In addition to sex-selective abortion, postnatal sex selection is another possible way to achieve higher sex ratios of surviving children in the 1990 Census. In Table A.22, we examine child mortality among second children born in 1977-86 by gender. We find that mortality rate of male second births following a firstborn girl decreased by 1.8 percentage points after land reform, while the female mortality rate increased by 1.5 percentage points, both statistically significant at the 10 percent level. These estimates provide suggestive evidence that parents seem to have allocated more of the land reform bounty to boys. Following a firstborn girl, the increase in the male survival rate and the excess female mortality among second children would also increase sex ratios of surviving children after land reform.

F Back-of-the-envelope calculation on income elasticities of sex ratios

While the existing estimates of the land reform's effect on income focus on grain output or crop output (Lin (1992), Huang and Rozelle (1996) and our estimate), a more comprehensive measure of the total income change including all possible income sources is required for the calculation of the income elasticity. In the first wave of the Rural Fixed Point Survey conducted by the Ministry of Agriculture in 1986, household microdata show that 51% of rural income was from grain crops, 12% from cash crops and 37% from non-crop income, including animal husbandry, fishery, forestry and non-farm work. Therefore, in this exercise, we consider all income sources and assume that the increase in income by source due to land reform was proportional to the aggregate output growth by source. Table 1 in Lin (1992) shows that in aggregate from 1978-84, cotton grew 3.7 times faster than grain annually, and non-crop output grew on average 3 times faster than grain annually.

We use our estimates together with estimates from Lin (1992) and Huang and Rozelle (1996) to calculate a range of income changes due to land reform. First, our Table A.15 shows a 3.5% increase in grain output per year following reform, suggesting a 21% total increase during 1978-84 (similar to the estimate in Lin (1992)). We calculate that cash crops grew by 77.7% (21%*3.7) and non-crop output grew by 63% (21%*3) due to land reform. We can then calculate that land reform led to 43% increase in rural income (0.51*0.21+0.12*0.777+0.37*0.63). Second, we also use the estimate in Huang and Rozelle (1996) to provide an income estimate. They find that land reform accounts for 35.6% of the growth in rice yields during 1978-84. Applying this number to the total grain growth of 29% during 1978-84 in Lin (1992)'s Table 1, we calculate that grain output increased by 10% due to the HRS (0.356*0.29). We then calculate that rural income increased by 21% following the HRS (0.51*0.1+0.12*0.1*3.7+0.37*0.1*3).

We then use the range of income estimates to provide a range of income elasticities of sex ratios: 0.088 (3.8/43) to 0.181 (3.8/21).

G New evidence on income and sex ratios in other contexts

We present additional evidence on income and sex ratios in both cross-country data in Asia where there is son preference and microdata with a more recent policy change in China.

G.1 Cross-country evidence in China, India, South Korea and Taiwan

In national trends, DasGupta et al. (2009) show that sex ratios of children under age 5 in China and India grew monotonically from 1980 to 2005. During the same period, both countries also had high GDP growth rates. Relatively less commonly known, when South Korea and Taiwan's economies cooled off in the 1990s, their sex ratios also leveled off.

Using country-by-year data in these four countries in 1975-1995, column 1 of Table A.23 shows that GDP per capita and sex ratios are positively correlated. In column 2, the GDP estimate remains robust after controlling for total fertility rate, consistent with our findings on land reform *versus* OCP in the paper. The magnitude of the cross-country income elasticity, 0.089, is similar to the lower number of 0.088 from land reform.

The analysis using the cross-country data is mainly descriptive given potential omitted variable bias. Therefore, we use Chinese microdata to assess a public policy in the 1990s which also induced income increases within the country below.

G.2 Anti-poverty program in rural China in the 1990s

Meng (2013) finds that the anti-poverty program in 1994-2000 increased rural income by 38%.[24] Combining data on the county-level program status from Meng (2013) and the 2000 population census, we examine how this program affected rural sex ratios in 1990-1999. We use a similar specification as equation (2) in the main text:

[24]Poor counties that were treated by the program received credit assistance, budgetary grants for investment and public employment projects (Meng, 2013).

$$y_{ijt} = \alpha + \beta_1 \texttt{NP94}_{jt} * \texttt{GirlFirst}_{ijt} + \beta_2 \texttt{GirlFirst} + \gamma_{jt} + \epsilon_{ijt} \tag{16}$$

where i is family, j county of birth, and t year of birth. y_{ijt} is 1 if the second child is male in family i. The program indicator $\texttt{NP94}_{jt}$ is equal to 1 in treated counties (defined as poor counties by the program) after 1994. β_1 measures the overall post-program increase in the fraction of male second births in families with a firstborn girl.

Table A.24 shows the estimates. We find a 2.4 percentage point increase in the fraction of males among second births following a first girl after the program, or a 11.4% increase in sex ratios from the pre-program level (1.67). Our estimate suggests that the anti-poverty program in the 1990s increased the overall sex ratios by 3.1%. Using the estimate of 38% income growth from Meng (2013), the income elasticity of sex ratios from the anti-poverty program is about 0.082 (3.1/38), close to the lower number of 0.088 from land reform.

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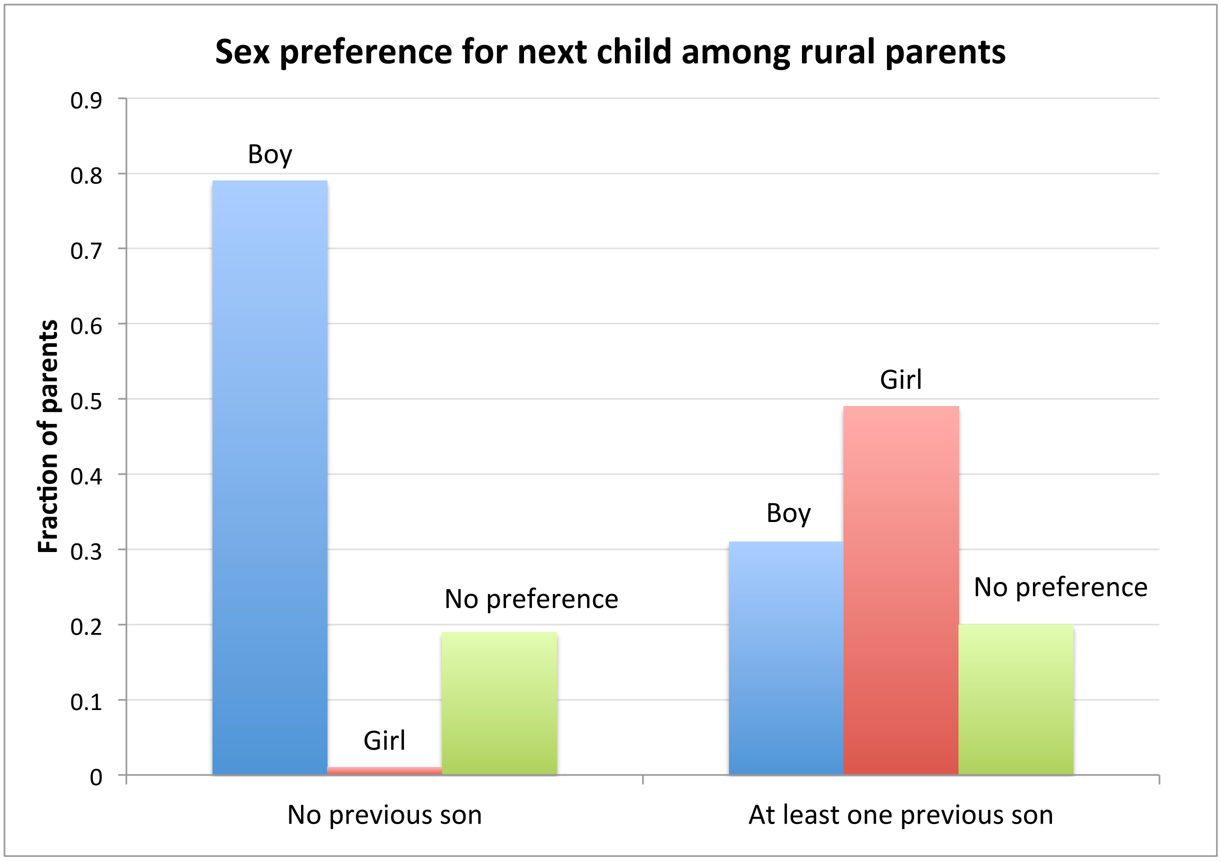
Figure A.1: Sex preference for next child among rural parents



Notes: Data source is the China In-depth Fertility Survey (Phase I in 1985 and Phase II 1987).

Figure A.2: Apply Benford's Law to grain output data

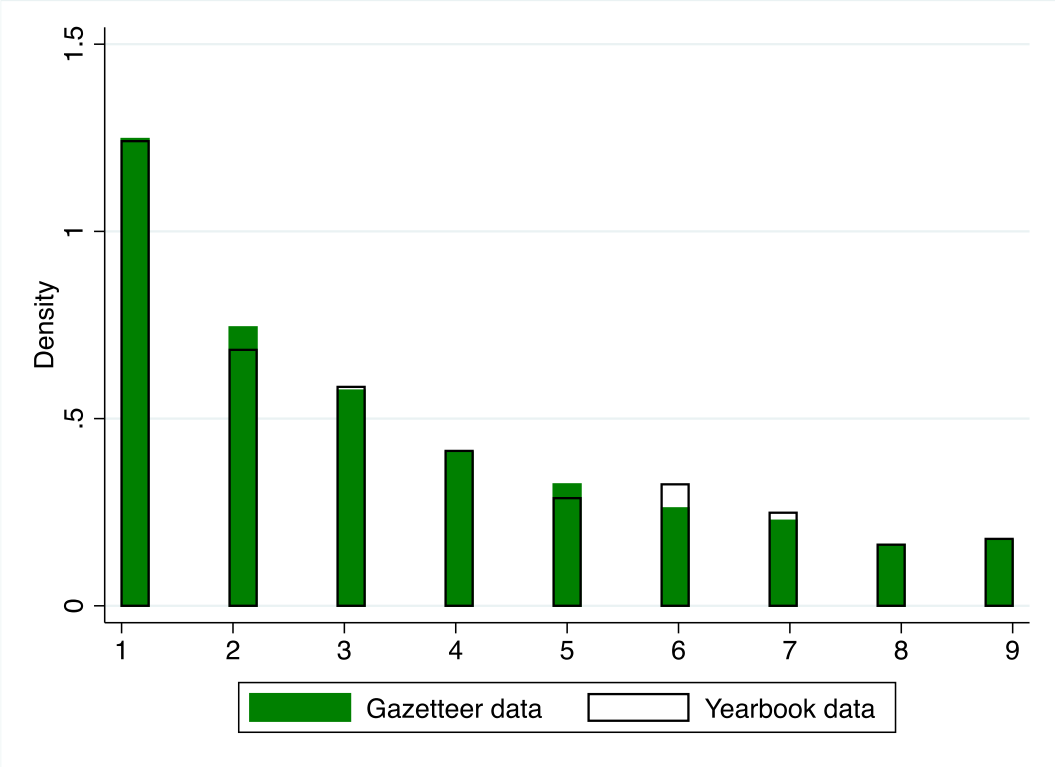


Figure A.3: Difference between land reform start year and the OCP start year

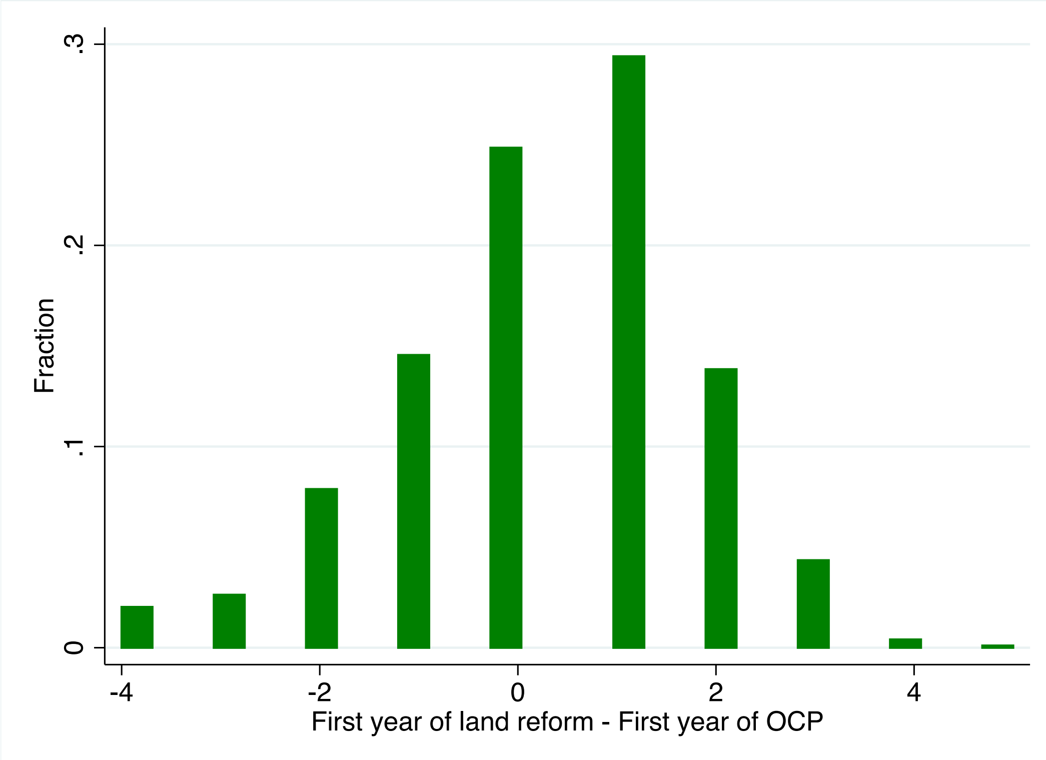


Figure A.4: Distribution of birth year among first children

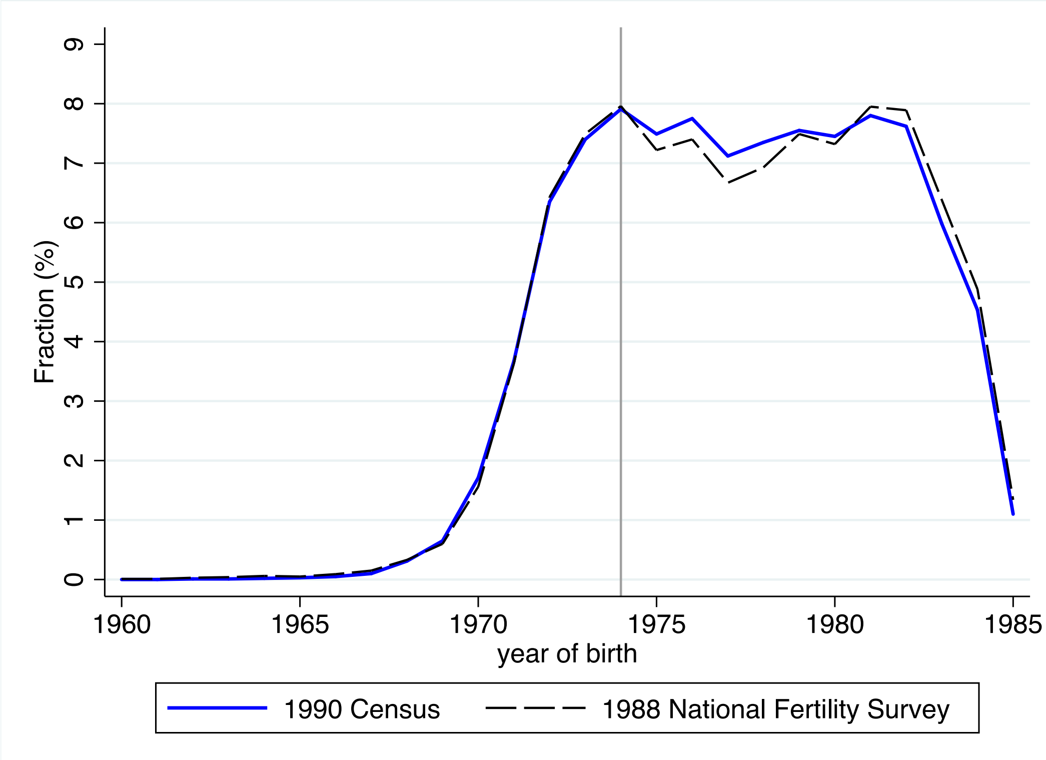


Table A.1: Predict grain output by crop suitability and rainfall

	Dependent variable: ln(gross production of grain)					
	All counties		Counties with data discrepancies<1 percent		Counties with data discrepancies>1 percent	
	Gazatteer (1)	Yearbook (2)	Gazatteer (3)	Yearbook (4)	Gazatteer (5)	Yearbook (6)
Wetland rice suitability	0.856***	0.800***	0.640*	0.642*	0.796***	0.720***
	(0.085)	(0.152)	(0.347)	(0.347)	(0.092)	(0.197)
Wheat suitability	0.313***	0.239***	0.508***	0.510***	0.282***	0.185***
	(0.023)	(0.036)	(0.090)	(0.091)	(0.026)	(0.046)
Rainfall	0.014***	0.013***	0.021***	0.021***	0.014***	0.013***
	0.001	0.001	0.003	(0.003)	(0.001)	(0.002)
Observations	1,860	1,860	235	235	1,289	1,289
R^2	0.225	0.120	0.302	0.301	0.219	0.095

Notes: The county-level crop suitability indices based on agro-climate conditions are from the FAO Global Agro-Ecological Zones database. Rainfall data are from weather stations in China provided by the US's National Climate Data Center. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.2: Determinants of reform rollout

	Year of land reform (1978-84)	
	Univariate regressions	Multivariate regression
	(1)	(2)
ln(Grain output per capita 1976)	0.250**	0.401***
	(0.121)	(0.124)
ln(Labor force density 1976)	-0.147***	-0.171***
	(0.022)	(0.043)
ln(Famine intensity 1959-1961)	-0.494***	-0.291**
	(0.081)	(0.145)
ln(Distance to Beijing)	-0.074*	-0.127
	(0.038)	(0.077)
Sex ratio at birth 1975-77	-0.135	-0.197
	(0.144)	(0.211)
R^2		0.096

Notes: The dependent variable is the first year of land reform, which varies from 1978 to 1984. Data on grain output per capita in 1976 are collected from county gazetteers. 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. Distance to Beijing is in kilometers and is obtained from a GIS map of 1982 Census. 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 A.3: The OCP and Total Fertility Rate in rural China

	TFR (rural)	
	National	Province-by-year
	(1)	(2)
Post 1979	-0.187	-0.118
	(0.464)	(0.095)
Pre-1979 dependent variable mean	3.5	3.5
Observations	13	377
R^2	0.335	0.848

Notes: Column (2) controls for province FE and province specific linear trends.

Table A.4: Fertility: number of 3+ births

	ln (number of births)	
	(1)	(2)
OCP	-0.122***	
	(0.037)	
Land reform	0.069*	
	(0.037)	
OCP*No previous son		0.531***
		(0.069)
Land reform*No previous son		0.017
		(0.068)
Observations	11,138	20,241
R^2	0.857	0.848

Notes: Column (1) controls for county fixed effects, year fixed effects and county specific linear trends. Column (2) controls for the indicator of no previous son and county-by-year fixed effects. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.5: Sample of counties

	Number of counties	Number of children	Fraction of males born 1974-86	Mothers who completed primary school
All rural counties in 1990 Census	1873	2.36	0.517	55%
Main sample: report rollout of both land reform and OCP	914	2.37	0.518	54%
Only for analyzing grain output: report land reform rollout and grain production	415	2.42	0.519	51%

Table A.6: Summary statistics

	Pre- Land erform		Post- Land reform	
	Mean	Obs	Mean	Obs
First births				
Male	0.510	148153	0.512	177480
Second births				
Male following a firstborn girl	0.528	59065	0.559	63410
Male following a firstborn boy	0.503	58926	0.503	60146
All 2+ births				
Male following no previous son	0.535	81605	0.577	90237
Male following previous son(s)	0.503	127262	0.505	120542

Table A.7: Second children born 1974-1990

	Male	
	Trend break	Average effect
	(1)	(2)
Land reform*Event time*Girl first	0.007***	
	(0.002)	
Land reform*Girl first		0.044***
		(0.004)
Observations	328,716	328,716
R^2	0.052	0.052
County-by-year FE	Y	Y

Notes: Trend break regressions also include Event time*No previous son. All regressions also control for the indicator of Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.8: Including families that report a different number of surviving children than that observed in the family

	Male	
	Trend break	Average effect
	(1)	(2)
Land reform*Event time*Girl first	0.009***	
	(0.003)	
OCP*Event time*Girl first	0.001	
	(0.003)	
Land reform*Girl first		0.027***
		(0.007)
OCP*Girl first		-0.008
		(0.007)
Observations	298,239	298,239
R^2	0.042	0.042
County-by-year FE	Y	Y

Notes: Trend break regression also includes Event time*No previous son. All regressions also control for the indicator of Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.9: Land reform and male births, by fathers' education

	Male			
	(1)	(2)	(3)	(4)
	Father completed primary school	Father didn't complete primary school	Father completed middle school	Father didn't complete middle school
Land reform*Girl first	0.031***	0.017	0.035***	0.024***
	(0.005)	(0.012)	(0.008)	(0.006)
Observations	178,205	47,250	91,309	134,146
R^2	0.066	0.206	0.118	0.087
County-by-year FE	Y	Y	Y	Y

Notes: Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.10: Sex preference for next child among rural parents, by parental education

	Mother completed primary school	Mother has no schooling	Differences in Means
Fraction want a son next with no previous son	0.785 (0.007)	0.818 (0.010)	-0.033*** [0.012]
Observations	3765	1597	
	Father completed middle school	**Father didn't complete middle school**	**Differences in Means**
Fraction want a son next with no previous son	0.786 (0.007)	0.807 (0.009)	-0.021* [0.011]
Observations	3234	2087	

Notes: Data source is the China In-depth Fertility Survey (Phase I in 1985 and Phase II 1987). * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.11: Fraction of males among all 2+ births

	Male			
	Trend break	Average effect	Trend break	Average effect
	(1)	(2)	(3)	(4)
Land reform*Event time*No previous son	0.008***		0.009***	
	(0.002)		(0.003)	
Land reform*No previous son		0.038***		0.037***
		(0.004)		(0.006)
OCP*Event time*No previous son			-0.002	
			(0.003)	
OCP*No previous son				0.004
				(0.006)
Observations	419,646	419,646	419,646	419,646
R^2	0.033	0.032	0.032	0.032
County-by-year FE	Y	Y	Y	Y

Notes: Trend break regressions also include Event time*No previous son. All regressions also control for the indicator of no previous son and birth order fixed effects. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.12: Land reform, the OCP and the 1.5 Child Policy

	Male
Land reform*Girl first	0.030***
	(0.008)
OCP*Girl first	-0.006
	(0.008)
1.5 Child Policy*Girl first	0.022***
	(0.008)
Observations	241,547
R^2	0.051
County-by-year FE	Y

Notes: The regression also control for Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.13: Interaction of Land reform and the One Child Policy

	(1) Male
Land reform*Girl first	0.025**
	(0.013)
OCP*Girl first	-0.009
	(0.010)
Land reform*OCP*Girl first	0.013
	(0.016)
Observations	241,547
R^2	0.051
County-by-year FE	Y

Notes: Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.14: Inverse probability weighting

	Male			
	Unweighted		Weighted	
	Trend break	Average effect	Trend break	Average effect
	(1)	(2)	(3)	(4)
Land reform*Event time*Girl first	0.0083**		0.0081**	
	(0.0033)		(0.0033)	
OCP*Event time*Girl first	-0.0014		-0.0014	
	(0.0035)		(0.0035)	
Land reform*Girl first		0.0332***		0.0333***
		(0.0079)		(0.0079)
OCP*Girl first		-0.0038		-0.0043
		(0.0078)		(0.0077)
Observations	241,544	241,544	241,526	241,526
R^2	0.051	0.051	0.051	0.051
County-by-year FE	Y	Y	Y	Y

Notes: The regression also control for Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.15: Heterogeneity in trend break in grain output and fraction of males

	All	Counties that had large trend break in grain output	Counties that had small trend break in grain output
	(1)	(2)	(3)
A: Dependent variable: ln(Grain output per capita)			
Land reform*Event time	0.035**	0.100***	0.023
	(0.014)	(0.021)	(0.021)
Observations	4,550	2,290	2,260
R^2	0.876	0.900	0.838
B: Dependent variable: Male			
Land reform*Event time*Girl first	0.006*	0.011**	-0.001
	(0.004)	(0.005)	(0.005)
Observations	81,317	46,314	35,003
R^2	0.052	0.047	0.058

Notes: Panel A controls for county specific trends by event time, county fixed effects and year fixed effects. Panel B controls for Event time*Girl first, Girl first and county-by-year fixed effects. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.16: Crop suitability indices and cropping outcomes

	ln(Sown area)	ln(Gross production)
	(1)	(2)
Grain		
Wetland rice suitability index	0.527***	1.174***
	(0.098)	(0.117)
Wheat suitability index	0.381***	0.302***
	(0.024)	(0.030)
Observations	1,816	1,816
R^2	0.165	0.114
Cotton		
Cotton suitability index	2.670***	4.380***
	(0.099)	(0.150)
Observations	1,821	1,821
R^2	0.340	0.344

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

Table A.17: Does the reform effect on fraction of males differ by crop suitability?

	Male
Land reform*Girl first*Cotton suitability index	0.007
	(0.007)
Land reform*Girl first*Citrus suitability index	0.004
	(0.014)
Land reform*Girl first*Banana suitability index	-0.012
	(0.015)
Land reform*Girl first*Tea suitability index	0.003
	(0.008)
Land reform*Girl first*Wheat suitability index	0.003
	(0.008)
Land reform*Girl first*Wetland rice suitability index	-0.010
	(0.018)
Observations	238,784
R^2	0.051
County-by-year FE	Y

Notes: Land reform*Girl first, Girl first*crop suitabilities, and Girl first are also controlled for. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.18: Household land size and gender

	Size of cultivated land (unit: mu=1/6 acre)
Fraction of female labor	-0.236
	(0.164)
Dependent variable mean	7.6
Observations	9,762
R^2	0.949
Household FE	Y
Year FE	Y

Notes: The household panel data cover 2,460 households in 217 villages of 29 provinces in China from 1986 to 1989. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.19: Robustness test on railroad access to province capitals that had ultrasound machines

	Male
Land reform*Girl first *Railroad to provincial capital that had ultrasound	0.031**
	(0.015)
Land reform*Girl first *Railroad to provincial capital	-0.009
	(0.012)
Land reform*Girl first	0.027***
	(0.006)
Observations	241,547
R^2	0.051
County-by-year FE	Y

Notes: Girl first, Girl first*Railroad to provincial capital with ultrasound, and Girl first*Railroad to provincial capital are also controlled for. County-by-year fixed effects absorbed the double interaction terms: Land reform*Railroad to provincial capital with ultrasound, and Land reform*Railroad to provincial capital. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.20: Ultrasound access in provincial capitals by distance

	Male	
	(1)	(2)
	Distance<70km	Distance>70km
Land reform*Girl first* Provincial capital has ultrasound	0.032**	0.009
	(0.016)	(0.006)
Land reform*Girl first	0.046***	0.030***
	(0.014)	(0.005)
Observations	27,427	214,120
R^2	0.053	0.051
County-by-year FE	Y	Y

Notes: Girl first and Girl first*Provincial capital with ultrasound are also controlled for. County-by-year fixed effects absorbed the double interaction term: Land reform*Provincial capital with ultrasound. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.21: Infant mortality in the UNICEF Chinese Children Survey (1992)

	Infant mortality	
	All births	Second births
	(1)	(2)
Land reform	-0.005**	
	(0.002)	
Land reform*Girl first		-0.002
		(0.004)
Observations	114,881	33,976
R^2	0.023	0.126
County FE and YOB FE	Y	
County-specific linear trends	Y	
County-by-year FE		Y

Notes: Column (2) also controls for Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.22: Child mortality in the UNICEF Chinese Children Survey (1992)

	Child mortality		
	Second births		
	All	Male	Female
	(1)	(2)	(3)
Land reform*Girl first	-0.001	-0.018*	0.015*
	(0.006)	(0.009)	(0.009)
Observations	33,976	18,022	15,954
R^2	0.131	0.208	0.244
County-by-year FE	Y	Y	Y

Notes: All regressions also control for Girl first and OCP*Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table A.23: Sex ratio and GDP in Mainland China, Taiwan, India and South Korea

	ln(Sex ratio)	
	(1)	(2)
ln(GDP per capita)	0.083**	0.089**
	(0.039)	(0.037)
ln(Total Fertility Rate)		-0.036
		(0.034)
Observations	79	79
R^2	0.614	0.618
Country FE	Y	Y
Year FE	Y	Y
County specific linear trends	Y	Y

Notes: 1) Data on GDP per capita (current US$) and total fertility rate in China, India and Korea are from the World Bank, and data in Taiwan are from Taiwan Statistical Bureau. 2) Data on sex ratio in China and India are aggregated from census microdata, and Indian census data are from IPUMS. Korean data are from Korean Statistical Information Office and are unavailable before 1980. Taiwan's data are from Taiwan Statistical Bureau.

Table A.24: Anti-poverty program and fraction of males

	Male
Anti-poverty program*Girl first	0.024**
	(0.010)
Observations	251,874
R^2	0.108
County-by-year FE	Y

Notes: Regression also controls for Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.