ABSTRACT

Title of dissertation: EMPIRICAL ESSAYS ON DEVELOPMENT ECONOMICS IN CHINA

Lingsheng Meng, Doctor of Philosophy, 2010

Dissertation directed by: Professor Mark Duggan
Department of Economics

My dissertation is composed of three essays on Development Economics in China. The first chapter, co-authored with Hongbin Li, evaluates the impact of the 8-7 Plan, the second wave of China's poverty alleviation program, on rural income growth at the county level over the period 1994-2000. Since program participation was largely determined by whether a county's pre-program income fell below a given poverty line, a regression discontinuity approach is employed to estimate causal effects of the program. Using a panel dataset, we find that the 8-7 Plan resulted in a gain of about a 26 percent increase in rural income for the counties which were treated. Our empirical results also suggest an important role for initial endowments in the path towards economic development.

The second chapter examines the question: How much of the increase in the sex ratio (males to females) at birth since the early 1980s in China is due to increased prenatal sex selection? I answer this question by exploiting the differential introduction of diagnostic ultrasound throughout China during the 1980s, which significantly reduced the cost of prenatal sex selection. The improved local access

to ultrasound technology is found to have resulted in a substantial increase in the sex ratio at birth. Furthermore, this effect was driven solely by a rise in the sex ratio of higher order births, especially following births of daughters. I estimate that the local access to ultrasound increased the fraction of males by 1.3 percentage points for second births and by 2.4 percentage points for third and higher order births. Using the annual birth rate at the county level as a proxy for local enforcement of the One Child Policy, the effects of ultrasound are found to be stronger for individuals under tighter fertility control. These findings suggest that the current trend in skewed sex ratios in China is significantly influenced by prenatal sex selection. Several robustness checks indicate that these results are not driven by preexisting differential trends.

The third chapter, co-authored with Douglas Almond and Hongbin Li, explores the gender bias in parental investment in children's health in China. Where the fraction of male births is abnormally high, heterogeneity in son preference would suggest that parents of sons may have a stronger son preference than parents of daughters. Child sex may have become a stronger signal of parental sex preferences over time as the cost of sex selection has declined and sex ratios at birth have increased. In this chapter, we consider whether ultrasound diffusion changed the pattern of early childhood investments in girls versus boys. If parental investments (like sex ratios) respond to parental sex preferences, postnatal investments in girls should increase with the diffusion of ultrasound and increased prenatal sex selection. In contrast, the prediction for investments prior to birth is ambiguous. For pregnancies carried to term, ultrasound revealed sex as much as six months prior to delivery, enabling

gender discrimination in *in utero* investments. In contrast, sex selective abortions would tend to increase *in utero* investments in girls through preference sorting. We evaluate these competing predictions using microdata on investments in children using the 1992 Chinese Children Survey. We find no effect of ultrasound access on the gender difference in postnatal investments. In contrast, we find early neonatal mortality of girls increased relative to boys with ultrasound access. As neonatal mortality tends to reflect pregnancy conditions, we infer that prenatal investments for girls carried to term may have fallen relative to boys once fetal sex was revealed.

EMPIRICAL ESSAYS ON DEVELOPMENT ECONOMICS IN CHINA

by

Lingsheng Meng

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Advisory Committee:
Professor Mark Duggan, Chair
Professor John Ham
Professor Melissa Kearney
Professor Jeanne Lafortune
Professor Anna Alberini

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Dedication

To the memory of my grandmother.

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Chapter 1

Evaluating China's Poverty Alleviation Program: A Regression Discontinuity Approach

1.1 Introduction

Evidence from cross-country studies suggests that sustained economic growth has typically been poverty-reducing (Ravallion and Chen, 1997; Dollar and Kraay, 2002). However, broad-based growth is not always the panacea for curing poverty (Morduch, 2000). Due perhaps to inferior initial conditions, the impoverished people residing in certain regions are unable to fully share the gains from aggregate high growth (Ravallion and Jalan, 1999). As a response to concerns about the lagging poor, public efforts have been taken in many countries in the form of poor area development programs to fight poverty and reduce inequality. Despite the theoretical underpinnings of the strategies pursued, whether these programs worked as intended is mainly an empirical issue.

This paper evaluates the large-scale poverty alleviation program instituted by the Chinese government in 1994. Known as the 8-7 Plan, it aspired to promote local economic development through targeted public investments. The program is impressive in terms of scale and its public outlay. In a bid to lift the majority of the remaining 80 million poor out of poverty by 2000, the program covered a total of

592 counties, or 28 percent of all county-level administrative units in China. Over the course of its seven-year operation, the program cost RMB 1240 billion (USD 14.9 billion equivalent), an annual 5-7 percent of China's central government expenditures. Three distinctive features of this program also deserve special emphasis. First, the program assignment was based on an indicator-based targeting scheme, which relies on an (or a set of) identifiable characteristic(s) to identify the poor. Second, the interventions, which were administered at the county level, intended to promote permanent income growth by supporting productive investment rather than subsidizing consumption. Third, the Chinese central government delegated the implementation of the program to local governments. Moreover, the central government resorted to its political personnel control to motivate local officials to promote economic development.

The 8-7 plan is pre-dated by the first wave of China's poverty program, which is very similar in content but smaller in scale. A modest literature evaluating the first wave interventions suggests that the program was successful in raising income and consumption of people living in the poor counties (Jalan and Ravallion, 1998; Rozelle et al., 1998; Park et al., 2002). As an aside, Park et al. (2002) also examines the performance of the 8-7 Plan after its first full year of implementation. It finds that the program increased per capita income growth by 0.91 percent during the period 1992-1995. Albeit an interim evaluation, it is the only study that we are aware of that assesses the effectiveness of the 8-7 Plan. Nevertheless, given the fact that the new wave of program did not come into effect until 1994, an estimate of the program impact of a longer term is needed to assess its overall efficacy.

Estimating program effectiveness is often complicated by the nonrandomness of program placement (Ravallion, 2008). This problem is especially relevant when public interventions are targeted based on certain average individual characteristics. In our context, if the treated counties benefited more from the aggregate economic growth than the untreated counties due to geographic differences, a "naive" comparison of gain incomes can lead to an underestimation of the program effect. On the contrary, upward bias could rise when political connections were important. The program effect could also be overstated if those selected into the program had experienced adverse shocks prior to eligibility, relative to those that were not selected. To deal with the endogeneity problem, we implement a regression discontinuity approach that is facilitated by the program's discrete assignment rule. In particular, program placement was largely determined by whether a county's pre-program per capita income fell below certain poverty lines. As such, causal impacts of the program can be gauged by comparing counties clustering just below the dividing line to counties just above.

We make use of the discontinuity of program assignment at the eligibility threshold to construct an instrumental variable for actual treatment status. Employing a panel data set of roughly 1700 Chinese counties over 20 years, our two-stage least squares results show that the 8-7 Plan had a large positive impact on rural income growth. We estimate that the program increased 1994-2000 per capita income gains by about 26 percent (0.7 standard deviation). By contrast, the estimates obtained from a conventional difference-in-differences approach suggest that the program was associated with only a 15 percent increase in per capita income, which understate the positive program impact by over 40 percent. The overall direction of the bias in the "conventional" estimates indirectly reveals the significant role of initial conditions in subsequent economic development.

To explore whether the program has a persistent effect, we use the change in log income per capita 1994-2003 as the outcome variable that measures income growth in a longer time horizon. It turns out that the estimates of the program effect in a "longer-run" are slightly smaller in magnitude, and also less precisely estimated for subsamples close to the threshold. This finding indicates that the program might have had only sizable short-run effects that started to decay just a few years after its implementation.

Importantly, the estimates of the program effects from our design are robust across a variety of specifications. They are insensitive to the inclusion of various control variables, and different functional form choices. Moreover, the estimates are also insensitive to the exclusion of observations far away from the cutoff point. Finally, we show that our results are unlikely to be driven by discontinuities in pre-program county characteristics or endogenous sorting around the threshold.

The remainder of the paper proceeds as follows. Section 2 provides some background information on poverty in China and the poverty alleviation program. Section 3 illustrates our empirical strategy and Section 4 describes the data used. Section 5 reports the empirical results and Section 6 concludes.

1.2 Background

1.2.1 Poverty in China

In China poverty is closely linked to geography (Ravallion and Jalan, 1999). There is a marked difference between rural and urban areas, with existing evidence pointing towards poverty being mainly a rural phenomenon (World Bank, 2000). Moreover, there are greater differences in the incidence of rural poverty between the prosperous coastal areas and the less developed and most poverty-prone inland areas. Figure 1.1 shows the geographic distribution of the incidence of rural poverty in 1991 at the provincial level. A province's shading indicates its rural poverty incidence, in which darker shading represents higher level of incidence. The graph clearly shows the geographic concentration of poverty in inland areas.

Official statistics indicate that the poor population has been declining during the 1990s. Figure 1.2 plots trends from 1990-2000 in the entire nation's official poverty headcount. Poverty reduction was impressive over the period, with both the number of poor population and the incidence of poverty experiencing a dramatic decline. The population identified as poor fell from 85 million in 1990 to 32 million in 2000. As a share of the rural population, the poverty headcount fell from 9.5 percent in 1990 to 3.4 percent in 2000.

1.2.2 Poverty Alleviation Program in China

In response to concerns about the lagging poor, the Chinese central government launched an ambitious anti-poverty program in the mid-1980's. An interministeral

Leading Group for Poverty Reduction (henthforth the Leading Group) was founded in 1986 to supervise and coordinate the implementation of the entire program. Two features of the new program distinguish itself from China's prior poverty reduction efforts. First, program assignment was based on a system of county-level targeting. In recognition of the remarkably uneven distribution of poverty across the country, the planner decided to use a targeting device to disburse limited funds to areas of the greatest need. Second, the newly adopted measures introduced a new emphasis on promoting economic development. Unlike prior welfare and relief programs, the transfer was not intended for direct consumption. Rather, resources were primarily channeled toward economic development and revenue-generating activities, which we elaborate below.

A priority ranking of counties is based on a statistic known as the rural net income per capita.[1] Each year, every county-level statistical agency randomly chooses around 100 rural households as the survey sample. The sample households are told to keep records of their revenues and expenditures in either nominal or real terms. Finally, those data are collected and aggregated, based on which the statistic is computed. This measure is one of the most important official poverty statistics that the Chinese government often relies on in welfare assessment in rural areas and related policy making (Park and Wang, 2001).

In 1986, the Leading Group initially identified 258 counties as National Poor Counties according to a mixed set of poverty lines. In general, counties with ru-

[1]This approach is known as an indicator-based targeting, or statistical targeting, which relies on certain key indicators to administer interventions (Besley and Kanbur, 1993).

ral net income per capita below 150 yuan were designated as poor. For political considerations, the poverty line was raised to 200 for minority counties and 300 for "revolutionary base" counties. However in practice, the explicit criteria for designation were not strictly enforced.[2]

Although the first-round designation had captured a sizeable fraction of the poor population, there had been considerable criticism raised against the approach for program placement. The goal of poverty alleviation was heavily compromised by politics (Park et al., 2002). In some provinces, the inclusion of politically favored counties even crowded out counties below the mandated poverty line (World Bank, 2000). Further, the validity of the poverty lines used for designation was questioned by researchers. Partially in response to these criticisms, the Leading group adopted a renewed poverty line, according to which major revision was made to its list of National Poor Counties in 1993. In principle, the standard for choosing poor counties was rural net income per capita below 400 yuan in 1992. However, faced with the pressure from previously designated counties, the central government decided to raised the poverty line to 700 for the counties label as "poor" before 1993.[3] The new adjustment raised the number of National Poor Counties to 592, almost a third of all counties in China. The red shaded regions in Figure 1.3 show the designated counties, clustering mostly in inland and mountainous areas. Compared with the

[2]Another 73 counties were added in the three years that followed. For a detailed description of the first-round designation, see Park et al. (2002).

[3]It is worth pointing out here that the 1994 designation employed the 1992 data to assign programs and the poverty lines were not made public until the data collecting was completed. This setting makes any precise sorting around the eligibility cutoff unlikely.

previous wave of designation, the new list certainly did a better job covering the poorest population. The government estimates suggest that over 72 percent of the rural poor were residing in the newly designated counties. This second wave of designating poor counties in 1993 and subsequent development assistance are also known as the 8-7 Plan[4].

The package of interventions in the 8-7 Plan was comprised of three major components. First, the targeted counties received credit assistance. This subsidized loan program is the largest of the poverty programs. The government has invested about RMB 27.8 billion (1994 price) in the subsidized loan program during this 7 year period. Each year, the funds for loans were disbursed by the People's Bank of China (the central bank) to the provincial Agricultural Banks of China (ABC's), which in turn made allocations to their lower level branches, following the scheme set up by the Leading Group. The final decisions to lend were made jointly by county-level offices of the Leading Group and ABC. The subsidized annual interest rate was 2.88 percent. Prior to 1996, the bulk of the loans were channeled to rural enterprises rather than rural households. After that, the priority of allocation switched back toward households.

Second, the Ministry of Finance provided budgetary grants for investment in poor counties. The funds, primarily managed through the fiscal system, totaled around RMB 12.5 billion (1994 price) over the years. Upon approval by the Leading Group, the Ministry of Finance made allocations to provincial departments of

[4]The program was called "8-7 Plan" because its main objective was to lift the majority of the remaining 80 million poor above the government's poverty line within 7 years.

finance. Provincial departments of finance then transmitted the funds to county finance bureaus, which administered the final disbursements in cooperation with the respective sectoral county government departments. The majority of the funds came in the form of Poor Area Development Funds, which were used for productive construction projects. Other budgetary grants were provided through earmarked grants for basic education, health care and so forth.

Third, public employed projects (Food-for-Work) were established in the designated areas. The program sought to raise the long-term development capacity of poor areas by supporting the construction of basic infrastructure, such as roads, drinking water systems, and land improvement. Meanwhile, it also expected to provide short-term assistance to the poor by creating more jobs. The central government distributed coupons to relevant local planning commissions, which used them to pay for the physical inputs and labor done under the program. In general, the central and provincial governments made decisions on the types of investment. County government selected sites and village committees were responsible for the allocation of project investment and labor contribution.

Apart from its huge spending, the central government also tried to meet its poverty reduction goals by making use of its unique personnel control system.[5] In a meeting called by the central government in 1996, its stated clearly how it would reward or punish local government officials according to their performance regarding poverty alleviation. In particular, local officials were to be demoted had they failed

[5]For a review of China's personnel control system and empirical evidence on its incentive role in local economic development, see Li and Zhou (2005).

to accomplish certain poverty reduction objectives set by the central government. The implementation of the entire project was supervised by the Leading Group. All counties designated as poor got treated with roughly the same intensity. In the analysis that follows, we assume homogeneous treatment intensity and try to estimate the collective impact of the whole bundle of interventions received by the Poor Counties.

1.3 Empirical Strategy

In this section, we discuss the econometric models employed to estimate the program impact. Absent a random assignment, one could assess the program effect under a difference-in-differences (DD) framework, i.e. to see whether the income increases more among National Poor Counties than among undesignated counties,

$$\Delta \log y_i = \log y_i^{00} - \log y_i^{94} = \alpha + \beta \cdot \mathtt{NP94}_i + \epsilon_i^{00} - \epsilon_i^{94} \tag{1.1}$$

where $\Delta \log y_i$ is the change in log rural net income per capita in county i from 1994 to 2000; $\mathtt{NP94}_i$ is a binary indicator equal to one if county i was designated as poor in 1994; and ϵ_i^t represents unobserved county-level factors of county i in year t. β is the parameter of interest, which measures the program impact.

Consistent estimation of β using least squares requires $E[\mathtt{NP94}_i \cdot (\epsilon_i^{00} - \epsilon_i^{94})] = 0$. If the omitted transitory factors that affect income growth are at the same time correlated with National Poor County status, then the DD approach will yield an inconsistent estimate of the true parameter. Poor and non-poor counties differ dramatically in terms of local "initial conditions" such as geography. If geography, for instance, has different effects on income in different years, it cannot be treated as a fixed effect and is not removed by first-differencing. In particular, non-poor counties mostly lie in coastal and less hilly areas, whereas poor counties tend to be concentrated in remote upland areas. If geographic disparities are important, non-poor counties certainly can benefit more from the fast-growing economy as a whole than poor counties. In this sense, a simple comparison of gain incomes in poor and non-poor counties can lead to an underestimation of the program effect.[6]

The DD estimator could also exaggerate the program's effectiveness. Being designated as poor is partially driven by the transitory shock ϵ_i^{92}. Suppose there is some transitory shock ϵ_i^t that is serially correlated. If the shock lingers on for at least two years but wears off almost completely in twelve years, a subsequent rise in y_i^{t+8} among the treated is expected even in the absence of a true program effect.[7] In addition, estimation of β is confounded by unobserved factors such as the change in central leaders' preferences toward certain local leaders/areas, which simultaneously affects the selection of poor counties and local income growth (Park et al., 2002). All the problems described above render the DD estimator unattractive for our purpose.

By tying the decision of designation to certain poverty lines, the assignment rule for the poverty programs in China creates a discrete relationship between a county's pre-program performance (the criteria variable) and its probability of being

[6] In a very similar argument, Ravallion and Jalan (1999) points out that "geographic externalities" is a particularly significant source of bias in conventional evaluations of poor-area programs.

[7] This problem is analogous to that observed in the analysis of training programs, in which participants are selected into treatment after experiencing a decline in pre-program earnings (Ashenfelter, 1978).

treated (aka. propensity score). This provides an excellent opportunity to estimate the causal effects of the poverty alleviation program with a Regression Discontinuity (RD) design (Hahn et al., 2001). The simplest version of a RD design is known as the Sharp Design, when there is a perfect relationship between the treatment status and the selection variable (Lee, 2008). However in many cases, this relationship might be imperfect when the observed selection criterion is not strictly followed. The administrator may rely on other factors to assign treatment, but these additional variables might be unobserved by the researcher. In this case the assignment to treatment depends on the selection variable in a stochastic manner. This setup corresponds with a Fuzzy Design.

It is useful to conceptualize the RD approach in an instrumental variable (IV) framework in case of a Fuzzy Design (Angrist and Lavy, 1999; van der Klaauw, 2002). In essence, we use the initial eligibility of the program, $\texttt{Eligible}_i = 1(y_i^{92} < \bar{y})$, as an instrument for actual National Poor County status, $\texttt{NP94}_i$. Here $1(\cdot)$ is an indicator function which equals unity when the embraced statement is true; $\bar{y}$ is the publicized threshold of National Poor County (NP94) eligibility. The parametric first-stage and reduced-form equations are

$$\texttt{NP94}_i = \pi_0 + \pi_1 \cdot \texttt{Eligible}_i + f(y_i^{92}) + u_i \tag{1.2}$$

and

$$\Delta \log y_i = \gamma_0 + \gamma_1 \cdot \texttt{Eligible}_i + g(y_i^{92}) + v_i \tag{1.3}$$

where $f(\cdot)$ and $g(\cdot)$ are smooth funtions of the selection variable. π_1 captures the discontinuous change in the propensity score at the cutoff; γ_1 measures the relative difference in the outcome variable for counties above and below the cutoff. In an exactly identified case like ours, the IV estimator, β_{IV}, is simply the ratio γ_1/π_1. The above specification assumes that the impacts of the program are homogeneous. And as the detailed information on the implementation of individual counties is not available, we also have to assume that the program intensity is the same for every county. The key assumption for identification here is that there is no discontinuity in counterfactual outcomes at the critical cutoff. In practice, we model $f(\cdot)$ and $g(\cdot)$ as lower-order polynomials and simple Two-Stage Least Squares (2SLS) implicitly assumes that both functions are approximated by polynomials of the same order. Since two different types of counties face two distinct cutoffs as described in the background section, we also include county-type fix-effects in our empirical model.

1.4 Data

In this paper, we use a socio-economic dataset on 1947 Chinese counties in 30 provinces over 20 years (1981-1995) collected by the Ministry of Agriculture. County is the unit of observation. The data set contains both economic variables and information on population and geography for each county. It is worth noting that the rural net income per capita, the variable used for program assignment and welfare assessment, is observed in this data set. We supplement the data set with economic variables for the years of 2000 and 2003 from various issues of the Yearbooks of Agricultural Development Bank of China. Price deflators at the province level are obtained from the China Compendium of Statistics 1949-2004. Due to missing information on the county-level income per capita for a few counties in certain years, our major sample has approximately 1700 observations.

Descriptive statistics of the key variables are provided in Table 1.1. To facilitate the comparison of baseline characteristics between the poor counties and non-poor counties, we also provide descriptive statistics by Poor County status in Table 1.2.

About 28% of the counties were designated as National Poor Counties in 1994. Note that the 1992 rural net income per capita (current price) were used to assign the program. Further, 1994 was the first full year of treatment and the program was phased out after 2000. During this period, the average growth rate in per capita income is 0.52 log point.

The statistics show that the National Poor Counties are indeed much poorer than the Non-Poor Counties before the program was introduced. In 1994, average per capita income of the Non-Poor Counties was 84% higher than that of the National Poor Counties. In 2002, however, average per capita income of the Non-Poor Counties was only 51% higher than that of the National Poor Counties. Comparison of Changes in log income per capita by treatment status suggests that income for the Poor Counties was growing faster than the Non-Poor Counties during the period when the program was effective. However, we cannot simply interpret the difference in growth rate as the causal effect of the program, because the treatment status was not randomly assigned.

1.5 Results

1.5.1 Main Results on Income Growth

In this subsection, we systematically test whether the poverty alleviation program has a positive impact on rural income. Unless otherwise noted, we use the Eicker-White robust standard errors for inference.

As a useful benchmark for our analysis, we first present simple difference-in-differences estimates of the program effect in Table 1.3. The dependent variable is the the change in log income per capita 1994-2000. Column 1 gives the unadjusted correlation between the dependent variable and the designation status. The coefficient on the treatment dummy is positive and statistically significant at the one percent level. It suggests that the program is helping the poor counties: over the decade, counties with designation had about a 18 percent relative gain in rural net income per capita. To see if the NP94 indicator has picked up any cross-county variation along other dimensions, column 2 introduces into the regression a set of pre-program county-level variables, namely the population, sown area, grain production per capita and industrial income per capita in 1994. After the adjustment for covariates, the magnitude of the estimated program effect decreases slightly but still remains statistically significant. Column 3 further includes province fixed-effects in the regression, allowing only with-in province comparisons of counties. This exercise leads to only marginal change in the size of the estimate. The implied program effect is around 12 percent gain in per capita income.

As discussed earlier, the first differencing approach is unable to control for time-varying factors that affect both designation status and income. To address this concern, we next implement the instrumental variable approach described in section 3 to estimate the program effects. We use the indicator of initial eligibility to instrument for the actual designation status in 1994.

Prior to presenting the formal regression results, we first provide some graphical evidence. Figure 1.4 plots the first-stage relation between a county's 1992 per capita income relative to the cutoff and its probability of being treated. Each data point (solid square) represents the fraction of counties designated as poor within 10 yuan intervals of the 1992 income per capita relative to cutoff. The figure present dramatic evidence that there is indeed a stark change in the probability of being treated around the cutoff. Visually the probability of treatment is approximately 0.60 higher among the initially eligible counties.

Figure 1.5 shows a scattered plot of each county's change in log income per capita, the outcome of interest, against its income per capita relative to the eligibility cutoff in 1992, the running variable. The continuous line is the predicted outcome from a regression that includes a third order polynomial in the running variable, and a dummy for observations above the cutoff. The dashed lines are pointwise 95 percent confidence intervals from the regression. There is a clearly discernible gap in the predicted outcome at the threshold. Compare the counties which were barely eligible (for example, the income relative to cutoff is -5 yuan), with counties which were barely ineligible (for example, the income relative to cutoff is 5 yuan). With a valid regression discontinuity design, the two groups should on average be similar in every respect except having different propensity scores. Therefore the break of the fitted line at the cutoff is an estimate of parameter π_1 in equation (2) (without any adjustment of covariates), equal to roughly 0.1.

Table 1.4 (Panel A) reports the results from estimating equation (2), the first-stage relation between actual treatment, the dependent variable, and initial eligibility. Column (1) shows the results from our most parsimonious specification, where eligibility is the only regressor other than provincial and county-type dummies. The coefficient on eligibility is highly significant and large in magnitude, and the R^2 exceeds 0.6. Consistent with the graphical evidence, the eligible counties, i.e., those below the official poverty line, have a significantly higher probability of being designated. The size of this estimate remains substantial (greater than 0.45) when a cubic of the running variable and other controls are included (columns 2-4). In summary, these results indicate that a 1992 per capita income above the official poverty line is a strong predictor of the actual treatment status.

Panel B contains the results from estimating equation (3), the reduced-form model. We regress change in log income on the indicator for program eligibility. Column (1)-(4) give the results of regressions using the same specifications as in Panel A. The eligibility coefficients are always positive and significant at least at the 1-percent level. Moreover, the fit of the regressions is considerably good. From a model with the richest set of controls in column (4), the estimated impact of eligibility is 12 percent, equivalent to a 0.3 standard deviation of the change in log income.

Panel C presents the instrumental variable results for a variety of specifications. These estimates are just the ratio of the reduced-form coefficients (in Panel

A) and first-stage estimates (in Panel B). The estimate in column (1) suggests a large effect when the selection variable is not controlled for. As column (2) includes the running variable, the NP94 coefficient shrinks by approximately a third. Adding a cubic of the running variable further decreases the size of the estimate, as shown in column (3). The coefficient is large in magnitude and significant at the 1-percent level, and it implies that in 1994-2000, designated counties had about a 28 percent increase in income per capita (a 0.7 standard deviation). Further, this estimate is insensitive to including other county-level covariates (column 4). Overall, the IV estimates for the program effect are always significant and relatively stable across specifications.

For our parametric estimates to be credible, identification of the intercept shift at the threshold should not rely solely on particular functional forms, or on data points in the extreme ends of the distribution. To address this concern, we add higher order polynomials and limit our sample within increasingly narrow intervals around the cutoff. Table 1.5 reports the results from these exercises. For the sake of comparison, column (1) reproduces the results from column (4), Panel C in Table 1.4. In the next column, a quartic term is included and the entire sample is used for estimation. The parameter estimate is very similar to that from the previous specification. In the following two columns, we focus on counties within 1000 yuan of their respective cutoffs and experiment with different functional forms. Our estimates appear robust to these changes. Columns (5)-(6) repeat the exercise with a "±500 yuan" window. Again, the estimated effects are statistically significant and comparable in magnitude. Broadly speaking, our estimates are largely insensitive to the choice of functional forms and samples.

To see if the program has a lasting effect on income growth, we also use the change in log income per capita 1994-2003 as the outcome variable. Remember that the program under consideration did not phase out until 2000. Instead of looking at the program's impact immediately after its implementation, we are now allowing for a three-year lag when constructing the growth measure. If the program does have a persistent effect, estimates using this new outcome variable should be at least as large as the estimates from the previous analysis for a shorter period. Table 1.6 repeats the exercise as in Table 1.5, this time with the longer-run growth measure as the outcome of interest. Overall, the 2SLS produces positive and relatively stable estimates (around 20-30 percent) for different specifications. However, compared to results from the last table, the estimated program impacts on the 94-03 income growth are slightly smaller in magnitude and less robust to changes in specification. In particular, the "long-run" estimates are insignificant for the subsample of counties with 500 yuan around the eligibility cutoff. These results, taken in conjunction with the "short-run" estimates from the previous table, indicate that the 8-7 Plan might have merely created a spurt in income that began to decay after only three years.

1.5.2 Specification Checks

For our empirical strategy to be valid, National Poor Counties and Non-National Poor Counties around the eligibility threshold should have similar pre-program trends in income growth. For a first robustness check, we use the change in log income per capita 1992-1994 as the outcome of interest (recall that the program was not introduced until 1994). In the absence of any differential trends between the treated and the untreated close to the cutoff, the expectation is that the 2SLS estimates using the Eligibility as the instrument for actual treatment should yield estimates that are statistically insignificant from zero. The IV estimates of the program effects on the pre-program income growth are reported in Table 1.7. The structure of the table is similar to Tables 1.5 and 1.6, which displays estimates from various specifications using different functional forms and samples within increasingly narrow intervals near the cutoff point. Overall, the 2SLS estimates are very smaller in magnitude none of them are statistically insignificant at conventional levels, which reveals no differential trends around the cutoff.

The identifying assumption underlying the RD design is that before treatment, only counties' propensity scores change discretely at the critical cutoffs. All other county-level factors, observable or unobservable, should evolve smoothly with the running variable, especially at the cutoffs. As a partial test of this assumption, we can check for discontinuities in the predetermined county characteristics that might confound our estimates. We systematically test the smoothness of a set of pre-determined county-level covariates through the eligibility cutoff. The results are shown in Figure 1.6, which plot average county characteristics against the running variable. For each variable, we also plot the predicted outcome from regrssion using the eligibility indicator, a quartic in 1992 income per capita relative to the threshold as explanatory variables. Generally, the figures indicate that none of the discontinuity estimates are statistically distinguishable from zero, suggesting no clear breaks around the cutoff.

The continuity assumption could also be violated in the presence of precise sorting around the eligibility threshold (McCrary, 2007). According to the institutional setup, manipulation of this sort should not be expected. In 1994, the administrator used 1992 data to allocate treatment and the exact poverty lines were not released until after the data were collected. Figure 1.7 presents a histogram of the number of counties at each of the 50 bins of 1992 rural income per capita, suggesting no evidence of nonrandom sorting across the cutoffs.

1.6 Conclusion

This study employs a panel data set to examine the performance of 8-7 Plan, a poverty alleviation program introduced in the early 1990's by the Chinese government. The program's placement rule causes a discontinuity in a county's probability of treatment as a function of its pre-program per capita income. This feature is exploited to identify the causal effects of the program on the change in the county's per capita income. Based on a regression discontinuity approach, our analysis shows that the program had a significant positive effect on income growth. This finding is robust to different model specifications. According to our estimates, the program increased per capita income gains by around 26 percent in the 1994-2000 period. This estimated program effect is much larger than that obtained from a conventional difference-in-differences method, which indirectly reveals the important role of initial endowments in the path towards economic development.

To examine whether the program has a lasting effect in promoting income growth, we also estimate the program's impact on the change in log rural per capita income during 1994-2003. It turns out that the estimates of the program effect in this longer time horizon are slightly smaller in size, and also less significant for a number of model specifications than the "short-run" estimates. This finding suggests that the 8-7 Plan might only have generated a spurt in rural income that began to decay shortly after the program was phased out.

Constrained by data availability, our assessment of the 8-7 Plan focuses on average income growth instead of poverty reduction. The use of county-level data also prevents us from depicting the program's distributional implications. For a more comprehensive evaluation of the success of the 8-7 Plan, more research is needed to look at these dimensions of the program effect.

Chapter 2

Prenatal Sex Selection and Missing Girls in China: Evidence from the Diffusion of Diagnostic Ultrasound

2.1 Introduction

China has a significant population sex imbalance in favor of males. It has been estimated that tens of millions of Chinese women are "missing" (Coale, 1991; Sen, 1990, 1992). Sen (1992) suggested that a substantial excess female mortality due to sex discrimination was responsible for the huge deficit of females in China. Since then, the issue of "missing women" has received great attention from researchers and policy makers alike. The problem undoubtedly needs to be addressed because of its profound ethical implications for women's welfare (Croll, 2000). In addition, concerns have also been raised about the adverse social consequences of the huge surplus of men (Ebenstein and Jennings, 2008; Edlund et al., 2007).

The recent years have seen a worsening of the "missing women" problem in China as the ratio of male to female births continued to rise significantly. As reported in the censuses, the sex ratio[1] of the population age 0, which was 107.6 in 1982, has reached 117.8 in 2000. The rapid rise in the sex ratio at birth coincided with the introduction of the One Child Policy, and the increased access to fetal sex determination technologies, chiefly diagnostic ultrasound. Previous research sug-

[1]Throughout the paper, **sex ratio** is defined as the number of males per 100 females.

gests that the rising sex ratio at birth in China was mainly a result of the escalating incidence of sex-selective abortion (Banister, 2004; Chu, 2001; Miller, 2001).[2]

Existing empirical analysis based on large samples has generally attempted to infer indirect evidence of sex-selective abortion in China from sex ratio differentials by birth order, and by sex composition of older sibling(s). Specifically, the previous literature finds that the sex ratio at birth is close to the natural rate (roughly 106) for first births, but increases steeply with parity following lower order female births (Zeng et al., 1993; Das Gupta, 2005; Ebenstein, forthcoming). This empirical regularity recovered from both fertility surveys and censuses is certainly consistent with the hypothesis that sex selection is responsible for the shortage of girls.[3] However, no previous research has presented direct evidence of sex-selective abortion or quantified its impact on the sex ratio at birth in China.

This paper aims to provide more convincing evidence on the importance of sex-selective abortion on the sex ratio at birth in China. A major obstacle to empirical research in this line is that sex-selective abortion depends on both the demand for sons by parent and the supply of the technology that facilitates the practice. In this paper, I construct a unique data set that tracks the differential diffusion of

[2]Oster (2005) argued that the high prevalence of Hepatitis B can explain up to 75% of the female deficit in China. Lin and Luoh (2008), using a huge medical data set from Taiwan, showed that the virus can raise the probability of having a son only very slightly. In a recent study, Oster et al. (2008) claimed that there is no link between Hepatitis B status and the sex ratio at birth in China.

[3]A similar pattern of male-biased sex ratio is found among US-born children of Chinese and Asian-Indian parents by Almond and Edlund (2008) and Abrevaya (forthcoming).

diagnostic ultrasound in China, thus obtaining variation in the access to prenatal sex determination technology that is plausibly orthogonal to demand factors. This data set identifies the year in which ultrasound machines were introduced into each of the roughly 1,500 counties from many issues of Local Gazetteer, a form of Chinese local history. This data set is then matched with a comprehensive microdata set that contains more than 500,000 live births in China from 1975 to 1992, a time of rapid expansion in ultrasound access. The birth records in the microdata allow an accurate measure of sex ratio *at birth* that is necessary for the analysis of *prenatal* sex selection.[4]

To estimate the effect of access to selective abortion on the sex ratio at birth in China, I use a difference-in-differences (DD) approach that exploits plausibly exogenous variation in the timing of ultrasound adoption across counties. During the period under study, induced abortion was legal, and abortion services were provided in public health facilities throughout China in the same way as other medical procedures. Therefore, the access to sex-selective abortion depended crucially on the availability of the ultrasound capable of prenatal sex determination. Because ultrasound technology entered different Chinese counties at different times, women who became pregnant after the introduction of ultrasound should have better knowledge of the fetal sex than those who became pregnant prior to the introduction of ultrasound. As such, the effects of better access to sex-selective abortion can

[4]The use of sex ratio of the cohort age 0 based upon population census, for example, runs the risk of misclassifying neonatal sex selection as prenatal sex selection, thus an overestimation of the latter.

be estimated by comparing changes in sex ratio at birth for counties that adopted ultrasound relative to those that had not yet adopted it.

I find that the adoption of ultrasound technology has significant effects on the sex ratio at birth in China. My most credible estimates imply that on average, the introduction of ultrasound increases the probability of a male birth by 1.3 percentage points for second order births, and by 2.4 percentage points for third or higher order births in a county. I also find that, if there were no previous sons, having local access to ultrasound raises the male probability by 4.8 percentage points for second births, and by 6.8 percentage points for third births. I interpret these findings as showing clear evidence of prenatal sex selection in China during the period under consideration. All results are robust to various controls, including mother characteristics and pregnancy characteristics.

The expansion of ultrasound coincided with a period when the Chinese population was under strict fertility regulations, commonly known as the One Child Policy. Earlier work suggests that the One Child Policy has been an important contributing factor for the high sex ratio in China (Ebenstein, forthcoming; Edlund et al., 2007). In addition, the enforcement of the policy is known to be highly localized and varying over time (Greenhalgh, 1986; Short and Zhai, 1998; Scharping, 2003; Gu et al., 2007). To account for potential correlation between ultrasound adoption and the temporal and regional variation in the implementation of the One Child Policy, I use the yearly birth rate at the county level as proxy for the intensity of population controls.[5] My main results are not affected by the inclusion of birth rate

[5]Birth rate is expected to be lower under tighter enforcement of the One Child Policy.

as an additional control variable. Consistent with previous studies, I find that the sex ratios are more skewed toward males in areas with more stringent fertility policies. Furthermore, results from the specification that allows the effects of ultrasound to vary with the degree of policy enforcement imply that the observed increase in sex ratio at birth in China since the 1980s was mainly a result of the interaction of the One Child Policy and the diffusion of ultrasound technology.

A number of robustness checks support the key identifying assumption that the timing of ultrasound's entry is uncorrelated with differential trends in sex ratios. First, I include the interaction of linear time trends with county pre-treatment variables to account for possible differential trends across counties. I find that my basic results are not affected. Second, I show that the change in sex ratio at birth is not correlated with future ultrasound access. Furthermore, the event study analysis that allows for differential effects before, during, and after the adoption of ultrasound also confirms that there are no preexisting differential trends in the sex ratio at birth in counties with and without ultrasound, and that the increase in male fraction among high order births is closely aligned with the introduction of ultrasound.

My final set of results addresses heterogeneity in the effects of ultrasound on birth gender. I find that the estimates of the average effect of ultrasound are driven mainly by individuals from the three largest ethnic groups in China, while the effects on smaller ethnic groups are small and statistically insignificant. I also find that the introduction of ultrasound results in a more skewed sex ratio for second and higher order births in urban areas relative to rural areas. Finally, my results also suggest that prenatal sex selection in China is more prevalent among the less-educated population.

This study contributes to efforts to understand the exact cause of the sex imbalance in China, which has the largest population in the world. To the best of my knowledge, my analysis is the first to show that the rise of the sex ratio at birth since the early 1980s was caused, in part, by prenatal sex selection that was made possible by the diffusion of diagnostic ultrasound across China. Moreover, by showing that prenatal sex selection tended to be more prevalent under tougher fertility control, this paper also is in accordance with the previous studies that stress the important role of the One Child Policy in the rise in sex ratio in China (Ebenstein, forthcoming; Edlund et al., 2007).

Somewhat further afield, the findings in this paper may also be relevant for studies of the costs and benefits of technological advances in medical care (see, for example, Newhouse, 1992; Cutler and McClellan, 2001; Cutler, 2007). Previous studies have examined the monetary cost of medical innovation. For the first time, this paper provides an example of how the introduction of a new technology can lead to biased sex ratios that can have substantial social costs when the technology is misused for purposes other than health care.

The remainder of the paper is organized as follows. Section 2 provides background on the motivation for sex selection in China and how this selection may be achieved through sex-selective abortion. Section 3 describes the empirical strategy. Section 4 discusses the data and presents some descriptive statistics. Section 5 reports the empirical results. Section 6 concludes the paper.

2.2 Background

2.2.1 The Motivation Behind Sex Selection in China

China has a long history of son preference. The concept of male superiority is part of the Confucian values that are deeply rooted in Chinese culture. This tradition stresses the importance of continuing the family line through male offspring, and thereby reinforcing male dominance within a household. These values shaped marriage patterns and family structures that were strictly patriarchal.

In a patriarchal family, sons are also valued more than daughters for economic reasons.[6] Daughters are usually lost to their natal family after marriage. Sons, however, normally stay with their parents and are expected to provide labor for the farm or family business. Parents also have to depend on their sons for old-age support, because of the absence of well-functioning social insurance programs as in industrialized countries.

Son preference has profoundly shaped the childbearing and rearing behaviors in China. During the time when average fertility was high, parents rarely stopped having more children before the desired number of sons were reached.[7] However, the national family planning program in China, enacted in 1979 and commonly known as the One Child Policy, changed the picture dramatically. In essence, the stringent birth control policy placed a legal limit on the family size, which prevented

[6] Qian (2008) showed that a rise in the value of female labor improves the survival rates for girls in China.

[7] Gender specific stopping rule (Dahl and Moretti, 2008) *per se* is different from sex selection, and will not cause the imbalance in sex ratio itself.

individuals from having multiple births to ensure the birth of a son (or sons). The opportunity cost of having another child of unwanted sex increased substantially. As a consequence, son preference manifests itself through sex-selection practices, which can be performed either prenatally or postnatally. Female infanticide, neglect of baby girls, and preferential allocation of key household resources to sons fall into the category of postnatal sex-selection strategies. Prenatal sex selection, however, was made possible only recently by new technologies.

2.2.2 Abortion in China

Prenatal sex selection is usually done in the form of sex-selective abortion (Edlund, 1999).[8] The practice of sex-selective abortion requires that two conditions be met. First, the technology must be available to reveal the sex of the fetus. Second, women need to have access to abortion facilities following the decision to terminate a pregnancy. In this subsection, I briefly describe abortion law (policies) and practices in China.

In contemporary China, abortion was legalized in 1953. Until 1957, however, legal abortions were permitted only when continuation of the pregnancy was medically undesirable, when the spacing of childbirth was too close, or when a woman had difficulty in breastfeeding the previous child. Even in such cases, abortions were not allowed without the certification from a physician and the approval of the couples' work units. In 1957, legal access to abortion was made easier as part of the China's early efforts to curb its population growth. In principle, abortions were

[8] Sex selection prior to conception, such as sperm sorting, does not require induced abortions.

available only upon request to any married woman within ten weeks of conception, and an abortion could only be performed once a year. In 1979, the One Child Policy was announced. Abortion became an essential component of this birth control campaign, and restrictions on abortion were relaxed accordingly. All reference to abortion was omitted from the Criminal Code of China of 1979 (Savage, 1988; Rigdon, 1996). The number of abortions grew rapidly after the introduction of the One Child Policy. In 1983, a national campaign was launched that included mandatory abortion for pregnancies exceeding the quotas stipulated by the Policy (Simon, 1988).

In China, abortion services were provided in government health facilities in the same way that other health services are provided during this time. Early abortions are usually performed by a qualified medical personnel using vacuum aspiration. Second-trimester abortions are performed in a hospital by a physician. Abortions are free, and the woman undergoing the procedure would also be rewarded by paid leave of up to 30 days.[9] Although most abortions are carried out during early pregnancy, the government allows abortions to be done up to six months of gestation (Hepburn and Simon, 2007). During the period under study, abortion was a frequent form of birth control in areas where access to contraception was limited and failure rates were high. Under the One Child Policy, abortions of above-quota pregnancies were encouraged, if not forced, by the Chinese government. In addition, the Chinese population at large is permissive toward abortion, thanks to the lack of a strong

[9]Therefore those who had abortions, except the unmarried women, would have an incentive to report them.

religious tradition (Rigdon, 1996). Therefore, the feasibility of prenatal sex selection hinged crucially on the access to prenatal sex determination technologies.

2.2.3 Ultrasound and Prenatal Sex Selection

While there exist a variety of reliable diagnostic procedures for fetal sex determination, ultrasound examination is used most frequently in China because it is the least expensive and most easily accessible method.[10] Ultrasound-B machines were originally designed for diagnostic purposes such as monitoring fetal development and checking intrauterine device placement. Ultrasound examination is also capable of prenatal sex identification, based on direct visualization of the external genitalia of the developing fetus. The accuracy of the technique is substantially improved from 15 to 16 weeks of gestation onwards.[11] With the recent development of high-resolution ultrasound equipment, and with the advent of transvaginal sonography (TVS), a diagnosis (although relatively inaccurate) can be made even as early as 11 weeks (Whitlow et al., 1999; Efrat et al., 1999). Most, if not all, of the obstetric ultrasound scans in China in the study period were by transabdominal sonography (TAS), and the lower-resolution equipment hindered accurate fetal sex determination in early pregnancy. The diagnostic procedure of ultrasound scan is painless and safe, with results immediately available at the time of visit. More importantly, the service is relatively inexpensive and readily affordable by any ordinary household.

[10] Other modern methods of prenatal sex determination include chorionic villous sampling, amniocentesis, hematological tests, and so on.

[11] For a review of the medical literature on this subject, see, for example, Mielke et al. (1998).

In China, it has been the most prevalent form of prenatal sex determination since its introduction.

In 1979, China was able to manufacture its very first ultrasound-B machine. Since the early 1980s, large numbers of imported and Chinese-made ultrasound machines have been introduced into the market. By 1987, the number of ultrasound-B machines being used in hospitals and clinics was estimated to exceed 13,000, or roughly six machines per county. According to official records, the number of imported ultrasound machines peaked in the late 1980s; over 2,000 state-of-the-art color ultrasound machines were imported in 1989 alone. It was also estimated that in the early 1990s, China had the capacity to produce over 10,000 machines annually, the equivalent of four additional machines per year for each county. By the mid-1990s, all county hospitals and clinics, as well as most township clinics and family planning service stations, were equipped with ultrasound devices that could be used for prenatal sex identification (Chu, 2001).

The popularization of ultrasound has made sex selection easier. Concurrent with the rapid growth of access to ultrasound, China witnessed an unprecedented rise in the sex ratio at birth during the 1980s (Chu, 2001). In 1989, having realized the potential disastrous consequences of the abuse of this technology, the Chinese government outlawed fetal sex determination for non-medical purposes and legislated substantial penalties for physicians performing such tests. The government regulations, however, proved ineffectual in practice. The misuse of ultrasound was often hard to police, and doctors continued to do so as a favor to relatives, friends, or people who paid bribes (Zeng et al., 1993). In addition, the problem was made worse by the incentive structure under the One Child Policy. After all, the local officials who were pressed to meet the birth planning targets that emphasized solely the number of births would rather turn a blind eye at the use of sex-selective abortions than pay the consequences of missing their targets.

Evidence suggests that China's sex ratio at birth has been increasing with the use of abortion, as shown in Figure 2.1. The upper line is the time series of the sex ratio at birth at the national level from 1978 to 1990, and the lower line is the time series of the abortion ratio[12] at the national level. While the co-movement of the two variables is consistent with an effect of sex-selective abortion on the sex ratio at birth, the extent to which these abortions were performed for sex-selective purposes remains unclear. Figure 2.2 plots the abortion ratio versus sex ratio at birth based on data of pregnancies aggregated into pregnancy-year $\times$ pregnancy-order cells. It shows that for all the fetuses conceived in the same year and of the same pregnancy order, the higher the abortion ratio, the higher the sex ratio of live births. This finding provides clear evidence of sex-selective abortions during this period. Notably, the positive correlation is driven mostly by second and higher-order pregnancies. The abortion ratio and the sex ratio at birth of the first pregnancies, however, are both stable over the years (clustered in the lower left corner of the panel).

[12] Abortion ratio is defined as the proportion of pregnancies ending in abortion.

2.3 Empirical Approach

I exploit variation in the year that ultrasound was introduced in each county to estimate the effect of access to sex-selective abortion on the probability of having a male birth. The time of conception and the county of residence jointly determine the mother's exposure to ultrasound technology.

To investigate the differential impacts of ultrasound technology on the probability of the birth being a male child across birth orders, I first estimate the following linear probability model using the sample of all births:

$$\begin{aligned} \mathsf{Boy}_{ict} &= \beta_1(\mathsf{1st} \times \mathsf{ultrasound}_{ct}) + \beta_2(\mathsf{2nd} \times \mathsf{ultrasound}_{ct}) + \beta_3(\mathsf{3rd}^+ \times \mathsf{ultrasound}_{ct}) \\ &+ \beta_4\, \mathsf{2nd} + \beta_5\, \mathsf{3rd}^+ + X_{ict}\gamma + \mu_c + \nu_t + \epsilon_{ict} \qquad (2.1) \end{aligned}$$

Here i indexes individual birth, c indexes county, and t indexes year. The dependent variable Boy_{ict} is a binary variable which equals one if the birth is male. $\mathsf{1st}$, $\mathsf{2nd}$ and $\mathsf{3rd}^+$ are indicator variables for the first birth, the second birth, and the third or higher parity birth. Positive β_4 and β_5 values imply that the offspring in higher birth orders are more likely to be male. $\mathsf{ultrasound}_{ct}$ is a dummy variable indicating whether ultrasound technology has been introduced into county c in year t, when the mother became pregnant. If the incentive for sex selection grows with family size, one would expect ultrasound to have a more pronounced effect on higher order births. To test this hypothesis, I interact the ultrasound variable with birth order indicators, allowing for differential effects of ultrasound by birth order.[13] X_{ict} is a

[13]It is possible that people with really strong preference for sons would travel to neighboring counties to get ultrasound scanning for sex selection. This spillover effect may lead to an underes-

vector of controls for ethnicity, maternal education, maternal age, and its square term, gestational age and information on prenatal care that might affect the likelihood of a male birth. μ_c is a vector of county of birth dummies and ν_t a vector of year of conception dummies.

It should be recognized that the county by year variation in local ultrasound access is certainly not random. The more urbanized areas tended to adopt ultrasound earlier.[14] The underlying factors that drove the introduction of ultrasound could lead to spurious estimates if those same county characteristics are associated with differential trends in sex ratios. To account for possible differences in trends that might be correlated with the timing of ultrasound adoption, I include the interaction between "pre-treatment" county characteristics, Z_c^{80}, with a linear time trend, and also the triple interaction terms between county variables, linear time trend, and birth order indicators. Specifically, using the sample for counties that adopted ultrasound technology after 1980, I estimate the following model:

$$\begin{aligned}
\mathsf{Boy}_{ict} &= \beta_1(\mathsf{1st} \times \mathsf{ultrasound}_{ct}) + \beta_2(\mathsf{2nd} \times \mathsf{ultrasound}_{ct}) + \beta_3(\mathsf{3rd}^+ \times \mathsf{ultrasound}_{ct}) \\
&+ (Z_c^{80} \times t)\,\theta_1 + (Z_c^{80} \times t \times \mathsf{2nd})\,\theta_2 + (Z_c^{80} \times t \times \mathsf{3rd}^+)\,\theta_3 \\
&+ \beta_4\,\mathsf{2nd} + \beta_5\,\mathsf{3rd}^+ + X_{ict}\gamma + \mu_c + \nu_t + \epsilon_{ict} \qquad (2.2)
\end{aligned}$$

My second strategy explores whether the effect of ultrasound varies with sex composition of previous sibling(s). Using samples restricted to second or third

timation of the true effect of the local access to ultrasound.

[14]In results not shown, I find that the technology was first introduced to counties with large population and a higher level of electricity consumption per capita.

births, I run regressions of the following form:

$$\begin{aligned}\mathsf{Boy}_{ict} &= \pi_1\,\mathsf{noboy} + \pi_2\,\mathsf{ultrasound}_{ct} + \pi_3\,(\mathsf{noboy} \times \mathsf{ultrasound}_{ct}) \\ &+ X_{ict}\gamma + \mu_c + \nu_t + \epsilon_{ict} \qquad (2.3)\end{aligned}$$

where noboy is an indicator that equals one if the mother had no son(s). A positive π_1 indicates that mothers with daughters are more likely to give birth to sons. Moreover, if those families with no older boy(s) are more interested in sex selection, one would expect the technology to have a larger effect on births without male elder sibling(s). Therefore I hypothesize that $\pi_3 > 0$. A same set of control variables are included as in Equation (1).

The above specification gives no sense of the dynamic effect of ultrasound adoption on the sex ratio at birth, that is, how fast local sex ratios would respond to the adoption of ultrasound, and whether the effect accelerates, stabilizes, or fades as time passes. Moreover, the examination of the pattern of anticipatory effects provides a useful specification check. If the rise in sex ratios was truly driven by the availability of ultrasound, one should not observe any growth in sex ratios before the introduction of ultrasound. To fully investigate the dynamics described above, I augment the statistical model with leads and lags of the implied ultrasound adoption, thereby allowing for anticipatory effects and lagged effects of ultrasound. Moreover, I interact the leads and lags of ultrasound with birth order indicators to allow these effects to vary with birth orders. Specifically, I fit the model of the form:

$$\begin{aligned}\mathsf{Boy}_{ict} &= \phi\,\mathsf{2nd}^{+} + \sum_{\tau=-2}^{5} \delta_\tau D_{ct}^{\tau}\,\mathsf{1st} + \sum_{\tau=-2}^{5} \lambda_\tau D_{ct}^{\tau}\,\mathsf{2nd}^{+} \\ &+ X_{ict}\gamma + \mu_c + \nu_t + \epsilon_{ict} \qquad (2.4)\end{aligned}$$

where D^{τ}_{ct} is an indicator variable for the number of years before or after the county's adoption of ultrasound. τ indexes the time relative to ultrasound adoption. Accordingly, I let $D^{\tau}_{ct} = 1$ if, in year t, ultrasound device has been introduced into county c for τ years (or, for a negative τ, county c adopted ultrasound $-\tau$ years later).[15] In Equation (4), the dummy variables, D^{τ}_{ct}, $\tau = -2, -1, 0, 1, \cdots, 5$ jointly represent the event of ultrasound adoption in county c.[16] In particular, δ_{τ} (or λ_{τ}) is the parameter giving the effect of local access to ultrasound on the fraction of male birth in the county τ years following its adoption of ultrasound for first (or second and higher order) births. The coefficients are measured relative to the omitted coefficient ($\tau < -2$). In addition, the event study model includes a birth order indicator, a vector of time-varying characteristics of the birth, and unrestricted fixed effects for county and year.

2.4 Data Sources and Descriptive Statistics

This paper makes use of two primary data sets. The first is a microdata set from the Chinese Children Survey, which was conducted by the National Bureau of Statistics of China in June 1992. Funding and support for this project was provided by the United Nations Children's Fund, the Ministry of Education of China, the Ministry of Health of China, and the All-China Women's Federation. The original purpose of the survey was to study child welfare in China. This is a large and representative sample of 560,000 households and two million individuals (including

[15] Alternatively, $D^{\tau}_{ct} = 1$ if county c adopted ultrasound in year $\tau - k$.

[16] D^{5}_{ct} is equal to one if in every year beginning with the fifth year adopting ultrasound.

children, their parents, and other family members) throughout China.

What makes this survey well suited for my analysis is the pregnancy history form for all women who have ever been pregnant since 1976. The qualified respondents were asked questions regarding each pregnancy. Each pregnancy record contains information on the pregnancy order, approximate time of conception, use of prenatal care, gestation length, and its final outcome (miscarriage, induced abortion, live birth, and others). For live births, gender and date of birth are also recorded. The mother identifier ensures accurate matching of a given mother's births and their respective parities, which is extremely useful for my analysis. One of the key variables for identifying *in utero* ultrasound "exposure" is the year of conception. The data provide the year of conception and the exact date of birth of each child. For about 1% of the sample, the reported year of birth is either earlier or two years later than the reported year of conception. In this case, I use the reported gestation length and year of birth to infer the conception year to minimize measurement error.[17]

My analysis is confined to the sample of children born in and after 1975. The main sample contains about 600,000 pregnancies and 500,000 live births. The summary statistics are described in Table 2.1. About 13% of the pregnancies were

[17]Unlike administrative data, self-reported are potentially inaccurate when respondents have difficulty recalling events from their past, in my case their pregnancy histories. We expect that the measurement error to have only minimal effect on the main results, because the reported measure used to construct my key explanatory variable, the local access to ultrasound, is the year of conception.

terminated in induced abortion. Around 84% of the pregnancies resulted in births. Abount 53% of the births are boys. The implied sex ratio at birth is 113, well above the biological norm of 105 boys per 100 girls. To illustrate the increasing trend of sex ratios during the period under consideration, Figure 2.3, based on my sample of births, provides a plot of the time series of sex ratios at birth broken down by birth order.[18] During the late 1970s, there is no observable difference in sex ratios across birth order. Since the early 1980s, sex ratios for different birth orders begin to diverge. As is evident from the figure, there is a systematic ordering of the sex ratios across parities, with more elevated sex ratios among higher order births. The sex ratios at birth at parity two and above experienced a steady climb during the 1980s, while for first births the sex ratio was relatively stable over time and closer to the biological norm. Figure 2.4 provides time-series plots of the sex ratios for second and third births, respectively, conditional upon sex composition of previous child(ren). Panel (a) of Figure 2.4 gives sex ratios for second and third births with no older male sibling(s). Panel (b) shows sex ratios for second and third births with older male sibling(s). In both panels, sex ratios for first births are included for comparison. There appears to be a striking difference between sex ratios of birth with or without older brothers. It clearly shows that the sex imbalance at birth during this period comes almost entirely from higher order births following daughters. Throughout the time period shown, the sex ratio of births with older male sibling(s) is stable over time and not significantly different from the sex ratio

[18]Sex ratios for years before 1978 are not reported because the number of observations for higher order births is below 500.

of first births.

The second data set documents the timing of county-level ultrasound adoption. This information is collected by combing many volumes of Local Gazetteer. The Chinese Government has a long tradition, lasting over one thousand years, of publishing issues of Local Gazetteer from time to time to record the development in a certain locality, typically a province, a city, or a county. Local Gazetteer is a copious official publication that embraces all types of information concerning history, economy, administration, culture, development, and so on. As such, it is often regarded as the authoritative encyclopedia of various particular locations in China.

In the early 1980s, the age-old tradition was revived when a new collection of Local Gazetteers were published to reflect the dramatic social changes that had taken place since the last major revision in the 1920s. Each local government set up its own Local Gazetteer Compilation Committee and performed a systematic review of its jurisdiction in a host of areas. A volume of Local Gazetteer was published as the final product of this bureaucratic effort. The new Local Gazetteers usually do not have a uniform framework in general, but most contain a chapter on public health issues. In this chapter, the time of the introduction of ultrasound machines was often recorded as a remarkable achievement in the public health sector for many counties.

The geographic distribution of counties with ultrasound over time is illustrated by a series of maps of China (see Figure 2.5), where the counties that had the ultrasound device between 1980 and 1995 are represented by areas shaded in dark blue, compared to areas where ultrasound was not yet available, which are denoted with light blue shading.[19] It appears that the technology expansion did not follow any clear geographic pattern (for example, from the coast to interior areas). Figure 2.6 tabulates the cumulative percentage of counties that had adopted ultrasound in each year in my data set. A few counties started to have ultrasound machines as early as 1965 (not shown in the figure). The coverage increased relatively slowly during the 1970s. Since the early 1980s, the expansion accelerated. In 1985 alone, over 500 counties adopted ultrasound, and the fraction of counties with ultrasound devices more than doubled. Virtually all the counties had their own ultrasound equipment by the end of the 1980s. This tabulation indicates that my microdata span a period of rapid diffusion of ultrasound technology.

Figure 2.7 plots the sex ratios at birth classified by birth orders, and by local access to ultrasound. Each set of two bars shows the sex ratios for first births, second births, and third and higher order births. For each birth order group, I compare the sex ratios of births with local access to ultrasound to the sex ratios of those who had no access to ultrasound in their counties of birth.[20] I use the delta method to derive standard errors of sample sex ratios, based on which 95% confidence intervals are calculated (represented by error bars around the sample mean). The underlying sample size is provided at the bottom of each bar. The figure demonstrates that the sex ratio at birth increases with birth order, with sex ratio slightly higher than the biological norm among first births. For first births, the sex ratio varies only

[19] Grey shaded areas are counties for which the information on ultrasound adoption is unavailable.

[20] A live birth is classified as having local access to ultrasound if ultrasound-B machines had been introduced into the county when the mother was pregnant.

slightly with local ultrasound access. The sex ratio at birth at parity one is 107.2 when ultrasound is unavailable, compared to 108.7 when local access to ultrasound is available. However, the increase in sex ratios for second and higher order births is evident after the adoption of ultrasound: the average sex ratio increases from 113.2 to 121.2 for second births, and from 118.6 to 132.4 for third and higher order births. In addition, the data strongly refute the null hypothesis that the difference in sex ratios of second births (or third and higher order births) is zero. I interpret these findings as preliminary evidence of prenatal sex selection among higher order births when parents have local access to fetal sex determination technology, although formal regression analysis is needed to account for potential omitted variable biases.

Finally, I supplement the above two data sets with control variables from other sources. A list of 1980 county characteristics is taken from the Chinese Compendium of Economic Statistics by County. These data are employed to measure "pre-treatment" county variables for use as potential determinants of the date of ultrasound adoption after 1980. In particular, I construct log population, farmland area per capita, sown area per capita, grain and meat production per capita, agricultural machinery power per capita, fertilizer use per capita, and power consumption per capita in 1980 for each county.

2.5 Results

The reported estimates in this section are from linear probability regressions, where the independent variable is an indicator variable that equals one if the birth is male. The linear probability models are useful for my purpose since the fitted probabilities are very close to 50%. Probit estimation produces nearly identical results. Standard errors are adjusted for serial correlation by clustering at the county level.

2.5.1 Sex Ratio at Birth by Parity

I first explore how the probability of the birth being male differs across birth parity. I start by including only the birth order indicators (with first births being the omitted category) as the explanatory variables in the first column of Table 2.2. In the next column, I add in a set of mother- and pregnancy-specific covariates that could potentially affect the likelihood of a male birth. The mother level controls are the mother's ethnicity (*Han* vs. ethnic minorities), education, a quadratic in maternal age at conception. Pregnancy characteristics include gestation length and indicators for the timing of initial prenatal care visits. Additional controls, including the year (of conception) effects and county fixed effects, are added sequentially in Columns (3)-(4). The coefficients of the birth order indicators should be interpreted as the difference in male probability of higher order births from first births. The estimates of the birth order effects are positive and statistically significant at the 1% level, and neither the point estimates nor the standard errors are much affected by the inclusion of additional controls. In my preferred specification that has the richest set of controls (the last column of Table 2.2), the estimates imply that second births are 2.1 percentage points more likely to be male than first births, while third and higher order births are 4.1 percentage points more likely to be male than first order births. My results so far are consistent with the empirical regularity found in censuses and other fertility surveys that the sex ratio in China tends to increase with birth order. I interpret the male bias at higher parity in my birth data as suggestive evidence of prenatal sex selection, whereas the next subsection explores the question more directly.

2.5.2 The Effect of Ultrasound Availability on Male Probability by Birth Order

In this subsection, I estimate the effect of ultrasound on the probability of a male birth, allowing this effect to vary by birth parity. Since I only have information on ultrasound for a subset of the administrative units in China, the sample size is reduced accordingly in the following analysis. Results are reported from the estimation of Equation (1).

In the first column of Table 2.3, I present the results from the most parsimonious specification with only the birth order indicators, the interactions between ultrasound availability, and a full set of birth order indicators as independent variables. The coefficient on the interaction ultrasound and first-birth indicator is positive but very small in size and not statistically different from zero, which implies that having access to ultrasound is not associated with any significant change in sex ratio of first births. The coefficients of the interactions between ultrasound and higher birth order indicators, however, are both positive and highly significant. The estimates suggest that after the introduction of ultrasound, the probability of male births increased by 2.0 percentage points for second births, and by 3.0 percentage points for third and higher order births. It is also worth noting that a qualitatively similar pattern of birth order main effects is found even in the absence of local access to ultrasound, albeit with a smaller magnitude. This implies that prenatal sex selection was not impossible, but certainly more costly, before ultrasound was introduced into the mother's county of residence.

In Column (2), results are reported for a regression specification that controls for observed individual heterogeneity. A similar set of individual control variables are added as in the previous table. The estimates of birth order indicators, as well as their interactions with ultrasound, are largely insensitive to the inclusion of the set of individual covariates. The results also suggest an imprecise zero impact of ultrasound on male probability of first births. The next column adds county fixed effects in the regression model, which eliminates potential bias in the previous estimates that are attributable to time-invariant omitted factors that vary across counties. Again, the estimates are very robust to this adjustment. In the remaining column in Table 2.3, I further include unrestricted year fixed effects to the preceding specification. The year effects absorb aggregate shocks to sex ratio at birth that might be correlated with ultrasound adoption. This exercise leads to only minimal decreases in the coefficients on the interaction between ultrasound and second and higher birth order indicators. The estimates suggest that the local access of ultrasound increased the fraction of boys by 1.3 percentage points for second births, and by 2.4 percentage points for third and higher order births. Both of the coefficients are significant at the 1% level. Moreover, the estimated effect of ultrasound on gender of first births is close to zero and not statistically significant at the 5% level.

Overall, my estimates suggest that the introduction of ultrasound has a positive effect on the male birth likelihood among second births, and a more pronounced positive effect on third and higher order births. The effect of ultrasound on sex of first births is small and statistically insignificant.

2.5.3 The Effect of Ultrasound on Male Probability Conditional on Sex of Previous Child(ren)

In this subsection, I examine whether the impact of ultrasound on sex ratio varies with the sex composition of previous child(ren) using the specification described in Equation (3).

Table 2.4 presents the results for the sample of second births. In Column (1), the regression includes an indicator that equals one if the first birth is female, the ultrasound availability, and their interaction term as explanatory variables. The regression shows that the second birth's gender depends on the firstborn's gender. When the first child is a girl, the second birth is 3.5 percentage points more likely to be a boy. More importantly, the coefficient of the interaction term between ultrasound and firstborn girl indicator is positive and highly significant. The point estimate implies that if there was no previous son, having local access to ultrasound raises the male probability by 4.7 percentage points among second births. The main effect of ultrasound is negative but insignificant at conventional levels. To probe the robustness of these results, I gradually add in a set of individual control variables, county fixed effects, and year effects in Columns (2)-(4). The coefficients on the previous gender indicator and its interaction with ultrasound are extremely robust to the covariate adjustments. Interestingly, in the last specification, the main effect of ultrasound is found to be *negative* and significant at the 10% level, although with a relatively small magnitude.

Table 2.5 displays the results for the sample of third births. Analogous to the second births analysis, I now investigate how ultrasound affects the sex ratio of third births conditional on the sex composition of previous children. The estimates show that in general, the third birth is more likely to be a boy after two previous girls. For mothers with two daughters born first, the probability that the third birth will be male is roughly 8.3 percentage points higher compared with mothers who already had at least one son. Adjusting for observable characteristics does not alter the estimated effects substantially. For all specifications, I do not find a consistent effect of ultrasound on the male probability of children at parity three if the mothers already had at least one son. However, the interaction effect between ultrasound and two girls is positive and statistically significant. The point estimate indicate that for mothers who already had two girls, local ultrasound access increases the male probability by approximately 6.8 percentage points for third births.

In summary, the results presented in Tables 2.4 and 2.5 show that the son-biased sex ratios at birth at higher parity seem to be solely due to the biased sex ratio of birth following daughters. It shows that the local access to ultrasound raises the male probability of subsequent births without older brothers. These findings indicate that the male-biased sex ratio of births at higher parities was largely due to prenatal sex selection motivated by son preference.

2.5.4 The Role of One Child Policy

This subsection investigates the role of China's One Child Policy in determining the sex ratio at birth. The existing literature suggests that the One Child Policy has been an important contributing factor for the high sex ratio in China. Moreover, the enforcement of the policy was highly localized and varied over time. One obvious concern about my key identifying assumption is that the timing of the introduction of ultrasound to Chinese counties may pick up the temporal and spatial variation in the implementation of the One Child Policy. In the absence of a direct measure of the local enforcement of the policy, I use the birth rate as a proxy for the overall intensity of population controls at the county level in each year after the One Child Policy was introduced.

In the analysis that follows, variation in local family planning policy enforcement is measured as the county-level birth rate during the year when the mother became pregnant. Birth rate is defined as the number of births divided by the number of women aged 15-49. The variable is constructed so that it is smaller under tighter enforcement in county c in year t. For meaningful comparison of estimates across specifications, the birth rate is normalized as mean 0.

I start by estimating Equation (1), including the birth rate as an additional covariate to control the county-by-year variation in the implementation of the One

Child Policy. Furthermore, the birth rate is interacted with the local access to ultrasound to examine how the effects of ultrasound differ for loose and strict enforcement of fertility control. The results are shown in Table 2.6.

Column (1) of the table displays the results from estimating my main equation using births after 1979, when the One Child Policy was introduced. The estimated coefficients are very similar to the results from using the whole sample, because the effects of ultrasound are identified almost entirely from the rapid diffusion of the technology during the 1980s.

In Column (2), I add the interaction terms of birth rate with birth order indicators into the regression. The estimated effects of ultrasound are essentially unchanged, which implies that my baseline model is not simply capturing a spurious correlation that is confounded by local birth control policies. The coefficients of the interactions between birth rate and higher order indicators are negative and significant, suggesting that the higher order births are more likely to be male in counties with more stringent fertility policies.

In the last column of the table, I further include the regression triple interactions of birth rate, ultrasound, and birth order indicators, allowing the effects of ultrasound to vary with the intensity of local policy implementation. The coefficients on the interactions between birth rate and higher birth order indicators are negative but no longer significant. However, the coefficients on the triple interactions of birth rate, ultrasound, and birth order indicators are negative and significant at least at the 10% level. This implies that local ultrasound access has large positive effects on male probability for higher order births, more so when enforcement of the One Child

Policy is stricter. In other words, the results sgguest that the observed increase in sex ratio at birth in China was largely driven by the interaction of the One Child Policy and the access to ultrasound technology.

To more clearly see the effects of ultrasound on child gender with strong, medium, and weak enforcement of the One Child Policy, I calculate the ultrasound effects at 25th percentile, median, and 75th percentile values of the birth rate. To find the ultrasound effects at 25th percentile of the birth rate, for example, I re-run the regression, in which I replace the birth rate with [birth rate $- P_{25}(birth\, rate)$]. This yields, as the new coefficients on ultrasound indicators, the estimated effects of ultrasound at 25th percentile of the birth rate, along with their standard errors. The same approach is applied to obtain the effects of ultrasound at median and 75th percentile value of the birth rate. The estimation results are presented in Table 2.7.

I begin by examining the effects of ultrasound access under strict enforcement of the One Child Policy. Column (1) of the table presents the estimated impact of ultrasound on child gender at 25th percentile of the birth rate. At this level, I estimate a 1.5 percentage points increase in the probability of being male for second births and a 2.6 percentage points increase for third (and above) births as a result of ultrasound adoption. Both of the estimates are significant at the 1% level. In Column (2), I examine the effects of ultrasound under a medium level of enforcement. For the median birth rate, having ultrasound increased the probability of being male for second births by 0.9 percentage point, and increased the probability of being male for third (and above) births by 2.0 percentage points. The last column presents the estimates of the effects of ultrasound under weak enforcement of the

Policy. The estimated effects of access to ultrasound around 75th percentile of the birth rate are quantitatively smaller and not statistically significant at conventional levels. Overall, these results suggest that the observed effect of ultrasound on child gender is predominantly a result of prenatal sex selection in areas under tougher enforcement of birth control when the One Child Policy was effective.

2.5.5 Robustness

In this subsection, I perform several checks on the robustness of the above results, which are presented in Table 2.8.

One major empirical concern of my difference-in-differences results is the possibility that trends in some county-level variables are driving both the introduction of ultrasound and the change in sex ratios. I first use the method described in Equation (2) to account for possible differential trends in the outcome by observable county factors. Specifically, I include in the regression the interaction between county "pre-treatment" characteristics and a linear time trend, together with the triple interaction terms between county variables, linear trend, and birth order indicators. The available Chinese county-level information was unavailable until 1980, which means I have to exclude all counties that adopted ultrasound before 1980. For comparison, Column (1) in Table 2.6 displays the results from re-estimating equation (1) using observations from counties that adopted ultrasound after 1980. Column (2) contains the results from estimating Equation (2), which shows that controlling for 1980 county characteristics interacted with a linear time trend does not change the main results in any appreciable way.

There remains a concern that the differential trends might be correlated with ultrasound access in a way that linear trends based on initial values of observable county variables do not capture. To test for the possibility of preexisting trends more directly, I include an indicator for getting ultrasound *next* year into the regression. If the unobservables that affect both ultrasound access and sex ratio evolve gradually over time, one should see change in sex ratios that "anticipate" getting ultrasound in the future. For the sake of comparison, Column (3) simply reproduces the results from Column (4) in Table 2.3. In the next column, an indicator for getting ultrasound next year, and its interactions with birth order indicators, are added to the previous specification. Compared with those in the previous column, the results indicate that the effect of having ultrasound this year is qualitatively unchanged. More importantly, getting ultrasound one year later does not predict changes in the probability of having a male birth. As shown in the same column, the coefficients on "ultrasound next year" and its interactions with birth order indicators are small in magnitude and statistically insignificant. Overall, the results suggest little evidence of preexisting trends or "anticipation" effects of access to ultrasound.

2.5.6 Event Study Analysis

Figure 2.8 plots the estimated effects (with 95% confidence intervals around the point estimates) of the introduction of ultrasound on fraction of male birth from estimating Equation (4). This model allows for differential effects before, during, and after the adoption of ultrasound. Panel (a) depicts $\hat{\delta}_\tau$'s, the coefficients of the interactions of the event year dummy variables and the dummy variable for first births. To eliminate potential compositional bias, I restrict the sample to a balanced panel of births for all eight event years to two years before adoption and five years after, which means that I include only counties that adopt ultrasound between 1976 and 1986. Panel (a) shows the results for first births graphically, suggesting no significant impact of having ultrasound on the gender of first births prior to, during, and after the adoption of ultrasound. Panel (b) plots $\hat{\lambda}_\tau$'s, the coefficients of the interactions of the event year dummy variables and the dummy variable for second (and above) births. The coefficients on the interactions of second (and above) birth dummy and pre-adoption dummies are close to zero and are not statistically significant at conventional levels. A Wald test of the hypothesis that λ_{-2} and λ_{-1} are jointly zero is not rejected by the data (p-value $= 0.5697$), showing an absence of a pre-trend in male fraction anticipating ultrasound adoption. I view this finding as further evidence of the validity of my identification strategy that exploits the sharp timing of the county-by-county rollout of ultrasound technology. The estimates show no effect in the year of adoption, which makes sense as it takes time for the county's residents to realize the local access to ultrasound and accumulate necessary knowledge before making use of the technology for sex selection purposes. In the next year after adoption, the probability of being male for second (and above) births increases substantially by 2.0 percentage points, after which the effect of ultrasound continues to grow gradually over the subsequent three years. The increment appears to flatten out after four years, with a permanently higher probability of around 4.0

percentage points in the affected counties.

2.5.7 Heterogeneity

The main results of this paper suggest that local access to ultrasound has significantly positive effects on the probability of a birth being male for second and higher order births. However, these results provide no information on how the impacts differ across different types of mothers. The effect of local access to ultrasound on birth gender is plausibly heterogeneous for several reasons: (1) the incentive for sex selection can differ across subpopulations due to different levels of son preference and different types of the One Child Policy they face; and (2) the knowledge and ability to make use of sex-selection technologies may differ across groups. In Table 2.9, I investigate whether there is a systematic gradient in the magnitude of the estimated effects by ethnicity, residency (urban vs. rural), mother's education, and household income. Each row of the table presents estimates of the effects of ultrasound on the male probability for different subgroups by estimating the model described in Equation (1).

I start by examining whether there are differential effects for different ethnic groups. The one-child-per-couple policy was initially applied only to the majority *Han* Chinese population. Ethic minorities were largely exempted from the one-child rule, however ethnic groups with a population larger than 10 million, namely *Zhuang* and *Manchu*, were treated on the same footing as the *Han* (Li and Zhang, 2007). Columns (1) and (2) show the effects of ultrasound separately by ethnicity. For the three largest ethnic groups (with a population of 10 million and above), *Han*, *Zhuang*, and *Manchu*, which faced a more stringent fertility control policy, the estimated coefficients on the interaction of ultrasound with second birth indicator and the interaction of ultrasound with third (and higher) birth indicator are positive and highly significant, with magnitudes qualitatively similar to those in the full sample. For mothers from smaller ethnic groups (with a population less than 10 million), the point estimates are quantitatively small and statistically indistinguishable from zero, showing no consistent evidence of prenatal sex selection for this subpopulation. This finding echoes the earlier research that emphasizes the role of the One Child Policy in explaining China's sex imbalance (Ebenstein, forthcoming). However, this interpretation should be treated with caution as I am unable to rule out the alternative explanation that people from smaller ethnic groups exhibit a relatively weaker preference towards sons.

I next examine whether the effects of ultrasound differ between urban and rural population. This type of heterogeneity could arise if urban households have stronger incentives for sex selection than rural households due to the urban-rural difference in fertility policies. Previous studies indicate that the One Child Policy was more stringent in urban areas than in rural areas (e.g., Zhang and Spencer, 1992). While the one-child rule was strictly imposed on urban couples, rural couples with a daughter in some provinces were allowed to have a second birth after 1984 (Greenlaugh, 1986). Moreover, the penalties for above-quota births were much harsher in urban areas than in rural areas (Banister, 1987). Heterogeneity could also arise if the urban residents have better access to health care than rural residents. If a better health care system could lower the cost of prenatal sex selection, one should observe a more pronounced effect of ultrasound for the urban population. There are other reasons, however, that rural families are more likely to engage in sex selection. Male children are desired in rural areas for provision of labor and old-age support in the absence of a well-functioning social security system. Yet, in urban China, son preference is less prevalent, thanks to a better social security system in cities. In Columns (3) and (4), I break the entire sample down by residency type to examine the rural-urban disparities in sex selection. Since registration status is unobserved in the data, I have to use mother's occupation, i.e., peasant vs. non-peasant, to proxy for rural vs. urban residency. The results indicate that the introduction of ultrasound leads to a more skewed sex ratio among second and higher order births in urban areas relative to rural areas. Specifically, the local ultrasound access increases the male probability by 2.2 percentage points for second births in urban areas, which is more than double the estimate for rural areas (1.0 percentage point). The urban-rural difference in the effects for third and higher order births is also sizable (3.8 vs. 2.1 percentage points). Interestingly, the availability of ultrasound reduces the probability of having a boy at parity one by 1.0 percentage point. The estimate is statistically significant at the 5% level. This result suggests that some parents in rural areas may have selectively aborted their first *male* fetuses to ensure the birth of a girl at parity one. For rural households that also have a preference for a large family size, this type of behavior is consistent with the incentive structure created by the rural fertility policy, as rural couples in some areas were allowed a second birth if the first birth was a girl. The evidence provided so far supports the claim that the desire for sex selection in China was largely shaped by its fertility policy (Ebenstein, forthcoming).

Another issue that I address in this subsection is whether the effect of ultrasound varies with the education level of the women. There are reasons to believe that a woman's education may affect her sex-selection behavior. First, women with higher educational attainment may have better knowledge regarding the practice of sex-selective abortion. On the other hand, the improvement in female education would enhance women's status, thereby reducing the preference for sons. Moreover, the relative improvement in women's earning capacity may also decrease the discrimination against girls (Qian, 2008). Previous empirical studies on other countries also suggest that son preference is negatively correlated with mother's education level (e.g., Chung and Das Gupta, 2007). In Columns (5) and (6), I divide the sample into two subsamples by mother's education level and re-estimate the model. The dividing point is the median educational level, six years of schooling, which is equivalent to being a primary school graduate. The results suggest that the introduction of ultrasound leads to more prenatal sex selection among less-educated mothers. In particular, local access to ultrasound increases the male probability by 2.4 percentage points for second births of less-educated mothers, while the effect is 0.8 percentage point for mothers with more education (6 years and above) and is not precisely estimated. A similar pattern is found for third and higher order births: the point estimate for the less-educated sample is almost 2.7 times the estimate for the better-educated sample (4.0 vs. 1.5 percentage points).

Finally, I investigate how the effects vary by income levels. There are several possible reasons for the income gradient in the effects of ultrasound on child gender. Richer households are less financially constrained; therefore, they have better access to and better knowledge of prenatal sex-selection technologies. By contrast, there is empirical evidence showing that the bias against girls may be stronger for poorer households (Burgess and Zhuang, 2000). In the last two columns, I stratify the sample by household income. The dividing point is 3340 yuan, the median household income in 1992.[21] According to the split sample analysis, prenatal sex selection at parity two appears to be more prevalent for households with lower levels of income, while selection among third (and higher-order) birth is concentrated in richer households. The mixed results presented here are merely descriptive and should not be over-interpreted, since household income is correlated with many household characteristics that may affect the desire for sex selection.

2.6 Conclusion

This paper addresses the question of whether the worsening of the sex ratio at birth since the early 1980s in China is due to increased prenatal sex selection. It does this by using both time and cross-section variation in local access to prenatal sex determination caused by the differential introduction of diagnostic ultrasound into Chinese counties during the 1980s. My empirical analysis uses a unique data set to demonstrate that the increased local access to ultrasound technology led to a dramatic increase in the sex ratio at birth. The observed effect of ultrasound

[21] Household income is measured *ex post* and thus potentially endogenous.

was driven entirely by an increase in sex ratios of higher order births. The analysis also shows that the local access to ultrasound has more pronounced effects on the probability of male birth for higher order births if there are no male older siblings. These findings, taken together, leave little doubt that the primary explanation for China's rising sex ratio at birth since the 1980s is sex-selective abortion, the cost of which was reduced markedly by the access to ultrasound.

The expansion of ultrasound in China coincided with a period when the One Child Policy was in effect. Absent a direct measure of the local enforcement of the policy, I use birth rate to proxy for the overall intensity of population controls at the county level in each year, and find that the sex ratios at birth were higher when enforcement of the One Child Policy was tougher. More importantly, I also show that the impact of One Child Policy on sex ratio at birth for this period was primarily a result of prenatal sex selection.

Chapter 3

Son Preference and Early Childhood Investments in China

3.1 Introduction

Does knowledge of fetal gender during pregnancy alter parental investments? Previous studies have documented differential treatment of baby girls in South and East Asia (Das Gupta, 1987; Basu, 1989; Burgess and Zhuang, 2000; Pande, 2003; Borooah, 2004; Mishra et al., 2004; Park and Rukumnuaykit, 2004). To this, we add a natural experiment in parents' knowledge of the sex of their child during pregnancy provided by the roll-out of ultrasound technology across the counties of China (Meng, 2009). Changes in investments that affect female fetuses are of concern not only because they can affect health and mortality, but also because they can impair the later-life health and human capital of females. With prenatal sex determination, gender biased investments could occur at an earlier and potentially more-sensitive stage of development. Thus, our research question engages both the son preference and "fetal origins" literatures.[1]

Observationally, deliberate sex selection by parents may obscure the effect of knowing fetal gender on early childhood investments. The diffusion of ultrasound technology across China during the 1980s allowed parents to observe and exercise

[1]See Lhila and Simon (2008) for an early contribution in this vein that focused on Asian immigrants to the US.

son preference through sex-selective abortion: local access to ultrasound strongly predicts increased male-to-female ratios at birth (Meng, 2009). If some Chinese parents had a stronger son preference than others, those choosing to abort females might have a stronger son preference than those who delivered daughters despite newfound access to ultrasound. Thus, Meng's result suggests that with increased sex ratios, parents were increasingly sorted according to son preference. In this respect, parents of girls may have benefited from increased investments.

To disentangle the effect of access to prenatal sex determination technology from parents' preference for sons, we use data on the year in which ultrasound machines were introduced into each of the roughly 1,500 counties from issues of the *Local Gazetteers*. This data set is then matched with a comprehensive microdata set that contains more than 500,000 live births in China from 1975 to 1992, a time of rapid expansion in ultrasound access. Using a difference-in-differences approach, we compare outcomes of females versus males before and after the introduction of ultrasound. Furthermore, our data allow us restrict comparisons to those within the family by including maternal fixed effects, which can address biases arising from differences across families, e.g. in fertility behavior and son preference. In our richest specification, the effects of ultrasound availability are identified using variation in ultrasound between children in the same families, after controlling flexibly for year fixed effects, county fixed effects, county time trends, and observed characteristics of the mother.

We find that postnatal investments do not seem to change as a result of preference-sorting induced by the availability of ultrasound. Nevertheless, we estimate a sizable increase in female neonatal mortality relative to male neonatal mortality following ultrasound availability. No significant effects are found for post-neonatal mortality measures, which implies that the effect of the availability of ultrasound on child health are concentrated soon after birth. Overall, these mortality results suggest that parents withheld investment in female fetuses relative to males after prenatal sex determination became available.

The remainder of the paper is organized as follows. Section 2 provides necessary background about son preference and the diffusion of diagnostic ultrasound in China. Section 3 discusses the ways in which the knowledge of fetal gender may affect parental investment in children. Section 4 describes the empirical strategy. Section 5 discusses the data and presents some descriptive statistics. Section 6 reports the empirical results. Section 7 concludes the paper.

3.2 Background

3.2.1 Son Preference and Gender Bias in China

China has a long history of son preference (see, e.g., Banister 1987 or Edlund 1999). The concept of male superiority is part of the Confucian values that are deeply rooted in Chinese culture. This tradition stresses the importance of continuing the family line through male offspring, and thereby reinforcing male dominance within a household. These values shaped marriage patterns and family structures that were strictly patriarchal.

In a patriarchal family, sons may be more valued more than daughters for economic reasons. Daughters are usually lost to their natal family after marriage. Sons, however, normally stay with their parents and are expected to provide labor for the farm or family business. Parents also have to depend on their sons for old-age support.

The strongest evidence of gender bias in China has been an abnormally high sex ratio (number of males per 100 females) at birth. It has been estimated that tens of millions of Chinese women are "missing" (Coale, 1991; Sen, 1990, 1992). Sen (1992) suggested that a substantial excess female mortality was responsible for the huge deficit of females in China. The recent years have seen a worsening of the "missing women" problem in China as the ratio of male to female births continued to rise significantly. Recent studies suggested that the primary explanation for China's risng sex ratio at birth since the 1980s is sex-selective abortion (Ebenstein, forthcoming; Meng, 2009). The previous literature has also documented discrimination against *surviving* girls. For example, there is empirical evidence which shows that girls have lower school enrollment rate relative to boys (Brown and Park, 2002; Gong et al., 2005). More recently, there are a number of studies that document gender bias against girls in intra-household allocation (Burgess and Zhuang, 2001; Park and Rukumnuaykit, 2004).

3.2.2 Ultrasound and Prenatal Sex Determination

While there exist a variety of reliable diagnostic procedures for fetal sex determination, ultrasound examination is used most frequently in China because it is the least expensive and most easily accessible method.[2] Ultrasound-B machines were originally designed for diagnostic purposes such as monitoring fetal development and checking intrauterine device placement. Ultrasound examination is also capable of prenatal sex identification, based on direct visualization of the external genitalia of the developing fetus. The accuracy of the technique is substantially improved from 15 to 16 weeks of gestation onwards.[3] With the recent development of high-resolution ultrasound equipment, and with the advent of transvaginal sonography (TVS), a diagnosis (although relatively inaccurate) can be made even as early as 11 weeks (Whitlow et al., 1999; Efrat et al., 1999). Most, if not all, of the obstetric ultrasound scans in China in the study period were by transabdominal sonography (TAS), and the lower-resolution equipment hindered accurate fetal sex determination in early pregnancy. The diagnostic procedure of ultrasound scan is painless and safe, with results immediately available at the time of visit. More importantly, the service is relatively inexpensive and readily affordable by any ordinary household. In China, it has been the most prevalent form of prenatal sex determination since its introduction.

In 1979, China was able to manufacture its very first ultrasound-B machine. Since the early 1980s, large numbers of imported and Chinese-made ultrasound machines have been introduced into the market. By 1987, the number of ultrasound-B machines being used in hospitals and clinics was estimated to exceed 13,000,

[2] Other modern methods of prenatal sex determination include chorionic villous sampling, amniocentesis, hematological tests, and so on.

[3] For a review of the medical literature on this subject, see, for example, Mielke et al. (1998).

or roughly six machines per county. According to official records, the number of imported ultrasound machines peaked in the late 1980s; over 2,000 state-of-the-art color ultrasound machines were imported in 1989 alone. It was also estimated that in the early 1990s, China had the capacity to produce over 10,000 machines annually, the equivalent of four additional machines per year for each county. By the mid-1990s, all county hospitals and clinics, as well as most township clinics and family planning service stations, were equipped with ultrasound devices that could be used for prenatal sex identification.

The popularization of ultrasound has made sex selection easier. Concurrent with the rapid growth of access to ultrasound, China witnessed an unprecedented rise in the sex ratio at birth during the 1980s (Chu, 2001). In 1989, having realized the potential disastrous consequences of the abuse of this technology, the Chinese government outlawed fetal sex determination for non-medical purposes and legislated substantial penalties for physicians performing such tests. The government regulations, however, proved ineffectual in practice. The misuse of ultrasound was often hard to police, and doctors continued to do so as a favor to relatives, friends, or people who paid bribes (Zeng et al., 1993). In addition, the problem was made worse by the incentive structure under the One Child Policy. After all, the local officials who were pressed to meet the birth planning targets that emphasized solely the number of births would rather turn a blind eye at the use of sex-selective abortions than pay the consequences of missing their targets.

3.3 Hypothesized Effects of Knowing the Fetal Gender

First, consider the case of postnatal investment. Apparently before diagnostic ultrasound was available, parents would know the fetal sex prior to making postnatal investment decisions. However, the availability of ultrasound makes prenatal sex selection feasible. If there is heterogeneity in son preference across families, the increases in the sex ratio at birth after the introduction of ultrasound would suggest that following ultrasound availability, girls are born to parents with weaker son preference, relative to parents of girls prior to ultrasound. Therefore we hypothesize that postnatal investments in girls would increase relative to boys following ultrasound availability.

Second, consider the case of prenatal investment. One hypothesized effect of ultrasound access is to cause a reduction in prenatal investments in girls relative to boys. Before ultrasound was available, child gender was presumably unknown until delivery, which would tend to equalize prenatal investment in girls versus boys. However, after ultrasound was available, parents would have better knowledge of the fetuses. For those parents whose abortion costs outweigh the distaste for having a girl, they would decide to carry the baby to term. However if these parents still favor boys, knowing the fetal gender in advance could induce them to invest differently. Specifically, they might withhold prenatal investment in female fetuses. Meanwhile parents would tend to increase investment when they know the fetus is male with higher certainty. On the other hand, for the same reason as in the postnatal case, increased preference sorting with ultrasound access would tend to increase prenatal investments in girls relative to boys. After all, the prediction for prenatal investments in girls relative to boys following ultrasound availability is *ambiguous*.

3.4 Empirical Approach

Under a difference-in-differences framework, we estimate the difference in the impact of the introduction of diagnostic ultrasound on birth outcomes and parental investment measures for female children relative to male children. Specifically, the estimating equation is:

$$\begin{aligned} y_{ijct} &= \beta_1\,\mathsf{girl}_{ijct} + \beta_2\,\mathsf{ultrasound}_{ct} + \beta_3\,(\mathsf{girl}_{ijct} \times \mathsf{ultrasound}_{ct}) \\ &+ X_{ijct}\gamma + \mu_c + \nu_t + \mu_c \times t + \epsilon_{ijct} \end{aligned} \tag{3.1}$$

Here i indexes individual birth, j indexes mother, c indexes county, and t indexes year. y_{ijct} is the outcome of interest. girl_{ijct} is a binary variable which takes the value one if the child is female. $\mathsf{ultrasound}_{ct}$ is a dummy variable indicating whether ultrasound technology has been introduced into county c in year t, when the mother became pregnant. Any gender difference in outcome in the absence of diagnostic ultrasound is captured by β_1. β_2 measure the change in outcome for boys after the introduction of ultrasound. Our key parameter is β_3, the coefficient on the interaction between girl_{ijct} and $\mathsf{ultrasound}_{ct}$. It measures the difference between girls and boys in the change in outcome following the introduction of ultrasound. X_{ijct} is a vector of individual- and mother-specific controls for ethnicity, maternal education, maternal age at birth, and its square term. μ_c is a vector of county of birth indicators and ν_t a vector of year of conception indicators. $\mu_c \times t$ is the county specific linear time trends.

One possible threat to identification is that mothers with potentially worse (or better) birth outcome could be induced to bear children after the introduction of diagnostic ultrasound, which is an improvement of the local health facility, and at the same time can coincide with other improvement of medical technologies within that area. Suppose the error term ϵ_{ijct} consist of a mother-specific component α_{jc}. Let $\epsilon_{ijct} = \alpha_{jc} + u_{ijct}$. In estimating Equation (1), selection bias could arise if any of the mother-specific unobservables are correlated with the availability of ultrasound. An arguably better way to avoid this possible compositional bias is to estimate a model with mother fixed effects. By differencing out the with-mother means, the portion of bias that is due to unobserved mother (or household) characteristics that are constant across the siblings is eliminated. This second model is given by:

$$\begin{aligned} \Delta y_{ijct} &= \beta_1 \, \Delta \mathsf{girl}_{ijct} + \beta_2 \, \Delta \mathsf{ultrasound}_{ct} + \beta_3 \, \Delta(\mathsf{girl}_{ijct} \times \mathsf{ultrasound}_{ct}) \\ &+ \Delta X_{ijct} + \Delta \nu_t + \Delta(\mu_c \times t) + \Delta u_{ijct} \end{aligned} \tag{3.2}$$

where Δ differences across siblings. All variables are now of the form $\Delta x_{ijct} = x_{ijct} - \bar{x}_{jc}$ where $\bar{x}_{jc}$ is the with-mother mean of x_{ijct}. In this specification, only within-mother variation are used for identification and children without any siblings will be dropped out of the sample. Now β_3 is identified by siblings of the opposite gender and "straddle" the introduction of ultrasound. Comparing the within-mother estimates from Equation (2) to the between-mother estimates of Equation (1) (using the sibling sample) gives and idea of the potential selection bias present in the former.

3.5 Data Sources and Descriptive Statistics

This paper makes use of two primary data sets. The first data set documents the timing of county-level ultrasound adoption. This information is collected by examining many volumes of Local Gazetteer. The Chinese Government has a long tradition, lasting over one thousand years, of publishing issues of Local Gazetteer from time to time to record the development in a certain locality, typically a province, a city, or a county. Local Gazetteer is a copious official publication that embraces all types of information concerning history, economy, administration, culture, development, and so on. As such, it is often regarded as the authoritative encyclopedia of various particular locations in China.

In the early 1980s, the age-old tradition was revived when a new collection of Local Gazetteers were published to reflect the dramatic social changes that had taken place since the last major revision in the 1920s. Each local government set up its own Local Gazetteer Compilation Committee and performed a systematic review of its jurisdiction in a host of areas. A volume of Local Gazetteer was published as the final product of this bureaucratic effort. The new Local Gazetteers usually do not have a uniform framework in general, but most contain a chapter on public health issues. In this chapter, the time of the introduction of ultrasound machines was often recorded as a remarkable achievement in the public health sector for many counties.

The geographic distribution of counties with ultrasound over time is illustrated by a series of maps of China (see Figure 3.1), where the counties that had the ultrasound device between 1980 and 1995 are represented by areas shaded in dark blue, compared to areas where ultrasound was not yet available, which are denoted with light blue shading.[4] It appears that the technology expansion did not follow any clear geographic pattern (for example, from the coast to interior areas). Figure 3.2 tabulates the cumulative percentage of counties that had adopted ultrasound in each year in our data set. A few counties started to have ultrasound machines as early as 1965 (not shown in the figure). The coverage increased relatively slowly during the 1970s. Since the early 1980s, the expansion accelerated. In 1985 alone, over 500 counties adopted ultrasound, and the fraction of counties with ultrasound devices more than doubled. Virtually all the counties had their own ultrasound equipment by the end of the 1980s. This tabulation indicates that our microdata span a period of rapid diffusion of ultrasound technology.

The second data set is a microdata set from the Chinese Children Survey, which was conducted by the National Bureau of Statistics of China in June 1992. Funding and support for this project was provided by the United Nations Children's Fund, the Ministry of Education of China, the Ministry of Health of China, and the All-China Women's Federation. The original purpose of the survey was to study child welfare in China. This is a large and representative sample of 560,000 households and two million individuals (including children, their parents, and other family members) throughout China.

What makes this survey well suited for our analysis is the pregnancy history form for all women who have ever been pregnant since 1976. The qualified re-

[4]Grey shaded areas are counties for which the information on ultrasound adoption is unavailable.

spondents were asked questions regarding each pregnancy. Each pregnancy record contains information on the pregnancy order, approximate time of conception, use of prenatal care, gestation length, and its final outcome (miscarriage, induced abortion, live birth, and others). For live births, gender and date of birth are also recorded. The mother identifier ensures accurate matching of a given mother's births and their respective parities. One of the key variables for identifying *in utero* ultrasound "exposure" is the year of conception. The data provide the year of conception and the exact date of birth of each child. For about 1% of the sample, the reported year of birth is either earlier or two years later than the reported year of conception. In this case, we use the reported gestation length and year of birth to infer the conception year to minimize measurement error.

Our analysis is confined to the sample of children born in and after 1975. The main sample with non-missing ultrasound information contains nearly 300,000 live births. The summary statistics are described in Table 3.1, Panel A. The top row of the panel shows that about 47% of the births are boys. The implied sex ratio at birth is 113, well above the biological norm of 105 boys per 100 girls. The next rwo shows that for around 36% of the births in the sample, ultrasound machines have already been introduced into the county when the mother became pregnant. It is unfortunate that prenatal investment of individuals is not observed in our data. Instead, only information on the early-life mortality is available. Neonatal mortality, which is infant death within 28 days of life, is usually linked to the health environment during pregnancy (Grossman and Jacobowitz, 1981). It is therefore useful to use the neonatal mortality as an outcome variable that may capture the impacts on child survival through prenatal investment on which we do not have data. The next set of rows presents infant mortalities. Each of the infant mortality varaibles takes on the value one if a birth dies in a certain period of time and 0 otherwise. The infant mortality rate is about 7 deaths within 28 days of birth (aka. neontal infant mortality) per 1,000 births. Roughly 80% of the neonatal infant deaths occur within 7 days of birth and around 40% within 24 hours. In addition, the post-neontal mortality rate (infant death within 28 days to 1 year) is around 3 deaths per 1,000 neontal surviors.

Panel B of Table 3.1 presents summary information for the subsample with postnatal parental investment variables. Because questions on parental inputs are asked only for children born after June 1987, the sample size is reduced accordingly. The vaccination variable is an indicator variable for whether the child received any of the standard childhood vaccines for BCG, IPV, DPTa and measles.[5] The vaccination rate in China is fairly low, ranges from 16% - 25% depending on the type of vaccine. In contrast, over 97% of children were ever breastfed. This suggests that breastfeeding was widespread in China, compared to childhood vaccination. The mean duration of breastfeeding is 14.9 months. Lastly, around 85% of the children were being taken care of by their mothers.

Table 3.2 presents the summary statistics for the sibling subsample. This subsample contains live births with at least one other sibling identified within the cohorts 1975 to 1992. The means and standard deviations for most of the observable

[5]BCG, is a vaccine for Tuberculosis (TB); IPV helps prevent Poliomyelitis (Polio) and DPTa protects against diphteria, pertussis and tetanus.

characteristics of the sibling sample are very similar to those from the entire sample. The only exception is that there is a lower percentage of the siblings “exposed” to ultrasound. This is possibly because the children born in the later part of our sampling window are more likely to be “exposed” to ultrasound, and are at the same time less likely to have siblings due to the tightening One Child Policy under way.

3.6 Results

Table 3.3 shows the effect of ultrasound on infant mortality. The subsequent tables presenting results with different outcome variables have a similar structure. The first column displays the coefficients on the female indicator, ultrasound indicator and their interaction term. These results are from the linear model for whether an infant died in a certain period of time regressed on the female indicator, ultrasound indicator and their interaction, plus controls for birth order indicators, ethnicity, mother’s age and age squared, mother’s education, county fixed effects, year fixed effects and county-specific linear time trends. The second column shows the same regression, but for the subsample of births with at least one other sibling identified. In the third column, the coefficients displayed correspond to the regression model that now includes mother fixed effects. County fixed effects are dropped accordingly. For all regressions in our study, standard errors are adjusted for serial correlation by clustering at the county level.

The top panel in Table 3.3 shows the results from estimating equation (1) using infant mortality within 24 hours as the dependent variable. For all specifications, the coefficients on the female indicator are always small in magnitude and statistically indistinguishable from zero, suggesting no systematic difference in infant mortality within 24 hours between boys and girls before ultrasound is available. The full sample results suggest that the availability of ultrasound has a positive relationship with the probability of death within 24 hours for male infants. When we use only the sibling sample in column (2), this relationship more than doubles and is also highly significant. The positive association between ultrasound availability and infant death is somewhat surprising since the introduction of diagnostic sound is normally considered an improvement in the technology of prenatal care. One plausible explanation would be that the availability of better medical technology might have induced more disadvantaged women to bear children that are more likely to die young, and if this compositional effect dominates the health benefits of ultrasound, one could observe an increase in the overall infant mortality rate. In column (3), after adding mother fixed effect in the sibling sample, the coefficient on ultrasound becomes negative and is no longer significant at conventional levels. The mother fixed effect estimate implies that the introduction of ultrasound may in fact improve the health of newborn male infants, and the between-estimator of ultrasound might be biased due to selection. For the interaction term, both the full sample and sibling sample using OLS produce positive and statistically significant coefficients with the similar magnitude. However, after including mother fixed effects, the estimate almost doubles, suggesting an impact of 0.19 percentage point increase in the one-day mortality for girls relative to boys with the introduction of ultrasound.

The bottom panel in Table 3.3 presents the results for infant mortality within 7 days of life. Once again, the coefficients on the female indicator from all three specifications are small in magnitude and statistically indistinguishable from zero, which indicates no systematic difference in infant mortality within 7 days between boys and girls before ultrasound. Interestingly, the same contrast in results across specification arises when we compare the coefficients on ultrasound using the entire sample and the sibling sample with and without mother fixed effects. The full sample results suggest that the availability of ultrasound is associated with a 0.14 percentage point increase in the probability of death within 7 days for male infants. When we use only the sibling sample, this relationship more than doubles. However, after including mother fixed effect in the sibling sample, the coefficient on ultrasound becomes negative and is significant at the 1% level, suggesting a 0.28 percentage point decrease in one-week mortality for boys after the introduction of ultrasound. A similar pattern for the coefficients on the interaction term is also found for one-week mortality as for the one-day mortality. Both the full sample and sibling sample using OLS give positive and statistically significant coefficients with the similar magnitude and adding mother fixed effects doubles the estimates. The implied effect of ultrasound is a 0.25 percentage point increase in the one-week mortality for girls relative to boys.

The top panel in Table 3.4 displays the results from estimating equation (1) using infant mortality within 28 days as the outcome variable. Recall that roughly 80% of the neonatal infant deaths occur within 7 days of life. Not surprisingly, the results are very similar to the 7-days results, which suggest that the introduction of ultrasound has a positive impact on the neonatal mortality for girls relative to boys.

The bottom panel in Table 3.4 shows the results for infant mortality within 28 days to 1 year as the outcome variable, conditional on survival until 28 days of age. For all three specifications, the coefficients on the girl indicator are always small in magnitude and statistically insignificant, indicating no systematic difference in post-neonatal mortality between boys and girls before ultrasound. The least-squares results without mother fixed effect indicate a positive relationship between ultrasound availability and infant death within 28 days to 1 year. The point estimate increases substantially from using only the sibling sample. Once again, the estimate becomes negative, although no longer significant. Unlike the case with neonatal mortalities, the coefficients on the interaction term between ultrasound and girl indicators are small in size and statistically insignificant. Furthermore, the point estimate gravitates towards zero after including mother fixed effects. Our results suggest that ultrasound access has no further effect on the difference in survival between boys and girls once the child survived the neonatal stage.

The infant mortality results presented thus far suggest that the availability of ultrasound has a disproportionate impact on the probability of death soon after birth for girls relative to boys. The estimates imply that about 70% of the effect of ultrasound on the girl-boy difference in neonatal mortality is due to increase in the difference in infant death within 24 hours of birth. By contrast, ultrasound does not seem to have any effect on relative mortality if the child has survived the first month of life. Taken together, these results show that the effect of ultrasound on relative mortality of girls is concentrated soon after birth, which suggest that the knowledge of fetal gender might cause parents to withhold prenatal investment in female fetuses, which decreases the probability of survival of girls relative to boys during the neonatal period.

Tables 3.5 and 3.6 explore the relationship between ultrasound availability and gender imbalance in childhood vaccination. Specifically, an indicator variable for whether the child is vaccinated is regressed on the gender indicator, ultrasound indicator and their interaction term. The same sets of control variables are included as in the previous analysis of infant mortality. We observe vaccination receipt information for four types of vaccines, namely vaccines against BCG, IPV, DTPa and measles.

The top panel in Table 3.5 shows the estimated effects of ultrasound on the probability of receiving vaccine type 1, i.e. the vaccine against BCG. The cross-section results using either the entire sample or the sibling sample reveal no significant gender difference in vaccination before ultrasound. However the sibling fixed effects analysis in column (3) indicates a non-intuitive female advantage in receipt of vaccination and the point estimate is significant at the 10% level. For all three specifications, the coefficients on the ultrasound indicator and the girl-ultrasound interaction are always statistically insignificant, which provide little evidence of any impact of ultrasound on receiving vaccination type 1 for both genders.

The bottom panel in Table 3.5 shows the analysis for vaccine type 2, i.e. the vaccine against IPV. Interestingly, the coefficients on the girl indicator are uniformly positive in all three columns and statistically significant at least at the 10% level, which indicates a clear female advantage in receipt of vaccination type 2. The least-squares estimates for the ultrasound effect are small and statistically insignificant. The mother fixed effect estimate of ultrasound, however, is negative and significant at the 10% level, which indicate that the introduction of ultrasound is associated with a 1.7 percentage points decrease in receipt of IPV vaccination for boys. For all three specifications, the coefficients on the girl-ultrasound interaction are never statistically significant at conventional levels, which provide little evidence of any effect of ultrasound on receiving vaccination type 2 for girls relative to boys.

The top panel in Table 3.6 shows the results for vaccine type 3, i.e. the vaccine against DTPa. We find some evidence of female advantage of getting this particular type of vaccine. The coefficient from full-sample on the female indicator is positive and significant at the 10% level. The coefficient remains virtually unchanged once we use the sibling sample with or without mother fixed effects, although the effect is less precisely measured. For all three specifications, the coefficients on the ultrasound indicator and the girl-ultrasound interaction are always statistically insignificant, which reveals little evidence of the effect of ultrasound on receiving vaccination type 3 for both genders.

The bottom panel in Table 3.6 shows the results for vaccine type 4, i.e. the vaccine against measles. We find a substantial female advantage of getting this particular type of vaccine. The estimate of the gender difference barely changes across specifications and is always statistically significant. Once again, we find no substantial difference in receipt of vaccine type 4 for boys after the introduction of ultrasound. Moreover, estimates of the interaction effect of gender and ultrasound from different specification are not suggestive of any effect of ultrasound on receiving vaccination type 4 for girls relative to boys.

Table 3.7 shows the effect of ultrasound on postnatal investments. The top panel presents the analysis of breastfeeding. The outcome of interest is an indicator for whether the child has ever been breastfed by the mother. The estimates for the female indicator from all the specifications reveal no significant difference in the rate of breastfeeding across genders. The introduction of ultrasound does not seem to have a significant effect on the probability of breastfeeding for boys. The least-squares estimates for the interaction effect are small and statistically insignificant. The mother fixed effect estimate of the interaction effect, however, is negative and significant at the 1% level, and its magnitude implies that the introduction of ultrasound is associated with a 1.3 percentage points decrease in breastfeeding for girls relative to boys.

The next panel in Table 3.7 reports the results for the duration of breastfeeding. The dependent variable is the duration of breastfeeding in months. The cross-section results suggest that breastfeeding duration are longer for boys before ultrasound and this relationship holds even after adding in mother fixed effect. The within-mother estimate indicates that before ultrasound daughters receive 0.7 month less of breastfeeding relative to sons. The between-mother estimates using the whole sample and the sibling sample indicate that the introduction of ultrasound is associated with a roughly 0.2 month increase in the breastfeeding duration for male children. Including mother fixed effect hardly changes the size of the coefficient, although it is measured less precisely. Moreover, none of the estimates of the interaction effect of gender and ultrasound from different specification is suggestive of any impact of ultrasound on the length of breastfeeding for girls relative to boys.

Finally, the bottom panel in Table 3.7 displays the results for whether the child was taken care of by his or her mother. We implicitly assumes that the mother has the natural comparative advantage of taking care of her children and the child should be better off being taken care of by his or her mother. The estimates for the female indicator from all the specifications reveal no significant difference in the probability of being taken care of by mother across genders. The introduction of ultrasound does not seem to have a significant effect on the probability of being taken care of by mother for boys. Moreover, none of the estimates of the interaction effect of gender and ultrasound from different specification is suggestive of any impact of ultrasound on the probability of being taken care of by mother for girls relative to boys.

3.7 Conclusion

This paper addresses the question of whether parental investment decisions change when they are able to know child gender during pregnancy in China. It does this by using both time and cross-section variation in local access to prenatal sex determination caused by the differential introduction of diagnostic ultrasound into Chinese counties during the 1980s. Furthermore, we include maternal fixed effects to control for unobserved time-invariant characteristics of the mother. We find little evidence of any change in postnatal investments as a result of preference-sorting caused by the access to ultrasound. Nevertheless, we estimate a sizable increase in female neonatal mortality relative to male neonatal mortality after ultrasound was introduced. Further, our empirical analysis reveals no significant effects for post-neonatal mortality measures, which implies that the effect of the availability of ultrasound on child health are concentrated soon after birth. Taken together, our results for infant mortality measure indicate that parent withheld investment in female fetuses relative to males after ultrasound became available.

Table 1.1: Summary statistics

Variables	Observations	Mean	Standard Deviation	Min	Max
NP94	1946	0.28	0.45	0	1
1992 rural net income per capita (1992 price)	1946	646.80	292.18	119	2482
1994 rural net income per capita (1994 price)	1796	1021.10	498.12	221	3866
2000 rural net income per capita (1994 price)	1906	1717.67	820.77	280	5777
2003 rural net income per capita (1994 price)	1910	1920.69	948.10	361	6732
Change in log income per capita 94-00	1758	0.52	0.40	-1.12	2.06
Change in log income per capita 94-03	1761	0.64	0.39	-1.01	2.29
National Poor County before 1994	1946	0.16	0.36	0	1
1994 population	1806	469219	330401	7229	2079895
1994 sown area (10,000 Mu)	1797	99.00	71.19	1	438
1994 grain production per capita (Kg)	1790	424.24	198.68	2.21	2029.54
1994 fiscal expenditure per capita	1698	213.36	244.91	0.68	5546.32

Notes:
The major data set is collected by the Ministry of Agriculture.
NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994.

Table 1.2: County-level covariates by Poor County status

Variables	Observations	Mean	Standard Deviation
National Poor Counties			
1992 rural net income per capita (1992 price)	549	409.36	122.81
1994 rural net income per capita (1994 price)	491	634.20	212.46
2000 rural net income per capita (1994 price)	539	1259.65	568.3
2003 rural net income per capita (1994 price)	539	1402.92	594.1
Change in log income per capita 94-00	481	0.65	0.46
Change in log income per capita 94-03	481	0.78	0.42
Population in 1994	502	367316	278826
1994 sown area per (Mu)	498	83.58	59.31
1994 Grain production per capita (Kg)	498	342.63	137.82
1994 Industrial income per capita	463	187.72	153.73
Non-National Poor Counties			
1992 rural net income per capita (1992 price)	1397	740.12	286.59
1994 rural net income per capita (1994 price)	1305	1166.66	497.02
2000 rural net income per capita (1994 price)	1367	1898.26	834.77
2003 rural net income per capita (1994 price)	1371	2124.25	983.35
Change in log income per capita 94-00	1277	0.48	0.37
Change in log income per capita 94-03	1280	0.59	0.37
Population in 1994	1304	508448	340278
1994 sown area per capita (Mu)	1299	104.91	74.43
1994 Grain production per (Kg)	1292	455.70	209.36
1994 Industrial income per capita	1235	222.98	270.74

Notes:
The major data set is collected by the Ministry of Agriculture.

Table 1.3: Difference-in-differences results for 1994-2000 change in log income per capita

	Dependent variable: change in log income per capita 94-00		
	(1)	(2)	(3)
NP94	0.18***	0.12***	0.15***
	(0.02)	(0.03)	(0.03)
Controls			
County-level factors	N	Y	Y
Provincial dummies	N	N	Y
R^2	0.04	0.06	0.15
Sample size	1758	1655	1655

Notes: NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994. County-level controls include the population, sown area, grain production per capita and industrial income per capita in 1994. All regressions control for county-type dummies. Huber-White standard errors are in parentheses. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 1.4: First-stage, reduced-form, and 2SLS results for 94-00 change in log income per capita, using eligibility for NP94 as an instrument

	(1)	(2)	(3)	(4)
Panel A: 1st stage (Dependent variable : NP94)				
Eligible	0.64***	0.56***	0.45***	0.46***
	(0.03)	(0.04)	(0.04)	(0.05)
R^2	0.62	0.64	0.66	0.67
Panel B: reduced-form (Dependent variable: Change in log income per capita 94-00)				
Eligible	0.34***	0.20***	0.12***	0.12***
	(0.03)	(0.03)	(0.04)	(0.04)
R^2	0.17	0.24	0.25	0.25
Panel C: 2SLS (Dependent variable: Change in log income per capita 94-00)				
NP94	0.53***	0.35***	0.28***	0.26***
	(0.06)	(0.06)	(0.09)	(0.09)
Notes on all panels				
Running variable	N	Y	Y	Y
Cubic in running variable	N	N	Y	Y
Other county-level controls	N	N	N	Y
Sample size	1758	1758	1758	1655

Notes: NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that equals one if the county's 1994 rural net income per capita was below the poverty line. County-level controls include the population, sown area, grain production per capita and industrial income per capita in 1994. All regressions control for county-type and provincial dummies. Huber-White standard errors are in parentheses. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 1.5: 2SLS results for 94-00 change in log income per capita, using eligibility for NP94 as an instrument

	Full sample		±1000 yuan		±500 yuan	
	(1)	(2)	(3)	(4)	(5)	(6)
NP94	0.26*** (0.09)	0.30*** (0.10)	0.33*** (0.11)	0.42** (0.18)	0.33* (0.20)	0.40* (0.23)
Polynomial terms	3	4	3	4	3	4
Sample size	1655		1618		1400	

Notes: The dependent variable is the 94-00 change in log income per capita. NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that equals one if the county's 1994 rural net income per capita was below the poverty line. County-level controls include the population, sown area, grain production per capita and industrial income per capita in 1994. All regressions control for county-type and provincial dummies. Huber-White standard errors are in parentheses. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 1.6: 2SLS results for 94-03 change in log income per capita, using eligibility for NP94 as an instrument

	Full sample		±1000 yuan		±500 yuan	
	(1)	(2)	(3)	(4)	(5)	(6)
NP94	0.22** (0.08)	0.23** (0.09)	0.29*** (0.10)	0.31* (0.16)	0.18 (0.17)	0.21 (0.20)
Polynomial terms	3	4	3	4	3	4
Sample size	1657		1619		1401	

Notes: The dependent variable is the 94-03 change in log income per capita. NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that equals one if the county's 1994 rural net income per capita was below the poverty line. County-level controls include the population, sown area, grain production per capita and industrial income per capita in 1994. All regressions control for county-type and provincial dummies. Huber-White standard errors are in parentheses. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 1.7: 2SLS results for 92-94 change in log income per capita, using eligibility for NP94 as an instrument

	Full sample		±1000 yuan		±500 yuan	
	(1)	(2)	(3)	(4)	(5)	(6)
NP94	-0.010 (0.041)	-0.016 (0.10)	-0.011 (0.048)	0.036 (0.079)	0.081 (0.091)	0.053 (0.097)
Polynomial terms	3	4	3	4	3	4
Sample size	1655		1618		1400	

Notes: The dependent variable is the 92-94 change in log income per capita. NP94 is an indicator variable that equals to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that equals one if the county's 1994 rural net income per capita was below the poverty line. County-level controls include the population, sown area, grain production per capita and industrial income per capita in 1994. All regressions control for county-type and provincial dummies. Huber-White standard errors are in parentheses. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.1: Summary statistics

Variables	Observations	Mean	Standard Deviation
Panel A: Pregnancies			
Birth	614408	0.84	0.37
Abortion	614408	0.13	0.33
Miscarriage	614408	0.03	0.16
Still birth	614408	0.004	0.060
Panel B: Births			
Male child	512201	0.53	0.50
First birth	512201	0.60	0.49
Second birth	512201	0.29	0.45
Third (or higher order) birth	512201	0.12	0.32
Urban	514389	0.30	0.46
Han	514389	0.87	0.34
Maternal education	514389	5.86	4.22
Maternal age (at conception)	514389	26.34	7.33
Household income (in 1992)	501333	3816.35	3221.12
Gestation (months)	514389	9.28	0.56
No prenatal care	514389	0.47	0.50
First trimester initial visit	514389	0.18	0.39
Second trimester initial visit	514389	0.16	0.37
Third trimester initial visit	514389	0.16	0.37
Ultrasound (available in county)	299935	0.41	0.49

Notes:
Panel A contains pregnancies that started between January 1974 and June 1992.
Panel B contains births occurred between January 1975 and June 1992.
Both samples are from the Chinese Children Survey.
Information on ultrasound access is collected by the author.

Table 2.2 : Effect of birth order on male probability: linear probability model results

	Dependent var.: child is male			
	(1)	(2)	(3)	(4)
Second birth	0.022***	0.024***	0.023***	0.021***
	(0.002)	(0.002)	(0.002)	(0.002)
Third (or higher order) birth	0.039***	0.044***	0.044***	0.041***
	(0.003)	(0.003)	(0.003)	(0.003)
Individual controls	No	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes
Year (of conception) fixed effects	No	No	No	Yes
Observations	512207	512207	512207	512207
R-squared	0.0008	0.0011	0.0032	0.0035

Notes: Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.3 : Effect of birth order, ultrasound availability, and their interactions on male probability: linear probability model results

	Dependent var.: child is male			
	(1)	(2)	(3)	(4)
Second birth	0.013***	0.015***	0.014***	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)
Third (or higher order) birth	0.025***	0.030***	0.030***	0.027***
	(0.004)	(0.004)	(0.004)	(0.004)
First birth × Ultrasound	0.003	0.000	0.003	-0.007*
	(0.003)	(0.003)	(0.003)	(0.004)
Second birth × Ultrasound	0.020***	0.017***	0.019***	0.013***
	(0.003)	(0.004)	(0.004)	(0.004)
Third (or higher order) birth × Ultrasound	0.030***	0.028***	0.030***	0.024***
	(0.006)	(0.006)	(0.006)	(0.006)
Individual controls	No	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes
Year (of conception) fixed effects	No	No	No	Yes
Observations	298615	298615	298615	298615
R-squared	0.0010	0.0014	0.0033	0.0036

Notes: Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.4 : Effect of ultrasound, sex composition of the first child, and their interactions on male probability: linear probability model results from the second births

	Dependent var.: child is male			
	(1)	(2)	(3)	(4)
First child is female	0.035***	0.035***	0.035***	0.035***
	(0.006)	(0.006)	(0.006)	(0.006)
Ultrasound	-0.004	-0.009*	-0.003	-0.015*
	(0.005)	(0.005)	(0.006)	(0.008)
First child is female × Ultrasound	0.047***	0.047***	0.048***	0.048***
	(0.008)	(0.008)	(0.008)	(0.008)
Individual controls	No	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes
Year (of conception) fixed effects	No	No	No	Yes
Observations	86351	86351	86351	86351
R-squared	0.0041	0.0048	0.0108	0.0113

Notes: Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.5 : Effect of ultrasound, sex composition of the first two children, and their interactions on male probability: linear probability model results from the third births

	Dependent var.: child is male			
	(1)	(2)	(3)	(4)
First two children are both female	0.083***	0.081***	0.083***	0.083***
	(0.011)	(0.011)	(0.011)	(0.011)
Ultrasound	0.003	-0.001	0.004	-0.012
	(0.008)	(0.008)	(0.009)	(0.013)
First two children are both female × Ultrasound	0.065***	0.065***	0.069***	0.068***
	(0.014)	(0.014)	(0.014)	(0.014)
Individual controls	No	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes
Year (of conception) fixed effects	No	No	No	Yes
Observations	26958	26958	26958	26958
R-squared	0.0143	0.0165	0.0310	0.0315

Notes: Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.6 : The effect of ultrasound availability, and its interactions with local One Child Policy enforcement on male probability: linear probability model results

	Dependent var.: child is male		
	(1)	(2)	(3)
First birth × Ultrasound	-0.008*	-0.006	-0.006
	(0.004)	(0.004)	(0.004)
Second birth × Ultrasound	0.011**	0.010**	0.008
	(0.005)	(0.005)	(0.005)
Third (or higher order) birth × Ultrasound	0.022***	0.020***	0.019***
	(0.007)	(0.007)	(0.007)
Birth rate × First birth		0.032	0.021
		(0.045)	(0.052)
Birth rate × Second birth		-0.097*	-0.010
		(0.056)	(0.069)
Birth rate × Third (or higher order) birth		-0.149**	-0.051
		(0.074)	(0.093)
Birth rate × Ultrasound × First birth			0.011
			(0.072)
Birth rate × Ultrasound × Second birth			-0.197**
			(0.094)
Birth rate × Ultrasound × Third (or higher order) birth			-0.211*
			0.0037
Individual controls	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Year (of conception) fixed effects	Yes	Yes	Yes
Sample	After 1979	After 1979	After 1979
Observations	257528	257508	257508
R-squared	0.0037	0.0037	0.0037

Notes: Birth rate is calculated as the number of births divided by the number of women aged 15-49 in the county during the year when the mother became pregnant. For meaningful comparison of estimates across columns, birth rate is demeaned using the sample average. Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.7 : The estimated effect of ultrasound availability on male probability under strong, medium and weak implementation of the One Child Policy

	Dependent var.: child is male		
Enforcement of One Child Policy:	Strong (1)	Medium (2)	Weak (3)
First birth × Ultrasound	-0.006 (0.005)	-0.006 (0.004)	-0.005 (0.005)
Second birth × Ultrasound	0.015*** (0.005)	0.009* (0.005)	0.002 (0.006)
Third (or higher order) birth × Ultrasound	0.026*** (0.008)	0.020*** (0.007)	0.013 (0.009)
Evaluation at	25^{th} percentile of the birth rate	median of the birth rate	75^{th} percentile of the birth rate

Notes: Birth rate is calculated as the number of births divided by the number of women aged 15-49 in the county during the year when the mother became pregnant. Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.8 : Robustness of the effect of birth orders, ultrasound availability, and their interactions on male probability: linear probability model results

	Dependent var.: child is male			
	(1)	(2)	(3)	(4)
First birth × Ultrasound	-0.008*	-0.008*	-0.007*	-0.008*
	(0.004)	(0.005)	(0.004)	(0.005)
Second birth × Ultrasound	0.011**	0.012**	0.013***	0.011**
	(0.005)	(0.005)	(0.004)	(0.005)
Third (or higher order) birth × Ultrasound	0.024***	0.024***	0.024***	0.023***
	(0.007)	(0.007)	(0.006)	(0.007)
Ultrasound **next year**				-0.004
				(0.006)
Second birth × Ultrasound **next year**				-0.001
				(0.009)
Third (or higher order) birth × Ultrasound **next year**				0.003
				(0.011)
Individual controls	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Year (of conception) fixed effects	Yes	Yes	Yes	Yes
Sample	After 1980	After 1980	Whole sample	Whole sample
1980 county variables × Linear time × Birth orders	No	Yes	No	No
Observations	278393	266962	298615	298615
R-squared	0.0036	0.0037	0.0036	0.0036

Notes: "Ultrasound **next year**" is an indicator for whether the county gets ultrasound next year to test for pre-trends. Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 2.9 : Heterogeneity in Effects of ultrasound availability on male probability: OLS results

	Dependent var.: child is male							
By groups	Ethnicity		Residency		Maternal education		Household income	
	Han, *Zhuang* & *Manchu* (1)	Smaller ethnic groups (2)	Urban (3)	Rural (4)	< median (5)	≥ median (6)	< median (7)	≥ median (8)
First birth × Ultrasound	-0.007	-0.011	0.002	-0.010**	-0.008	-0.006	-0.008	-0.008
	(0.004)	(0.013)	(0.008)	(0.005)	(0.008)	(0.005)	(0.005)	(0.006)
Second birth × Ultrasound	0.014***	0.006	0.022**	0.010**	0.024**	0.008	0.017***	0.009
	(0.005)	(0.015)	(0.010)	(0.005)	(0.010)	(0.005)	(0.006)	(0.007)
Third (or higher order) birth × Ultrasound	0.027***	0.008	0.038**	0.021***	0.040***	0.015*	0.021**	0.027***
	(0.007)	(0.017)	(0.016)	(0.007)	(0.011)	(0.008)	(0.010)	(0.008)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year (of conception) fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	263991	34624	83205	215410	89035	209580	145604	153011
R-squared	0.0040	0.0111	0.0066	0.0044	0.0066	0.0040	0.0055	0.0042

Notes: Individual controls include mother's ethnicity, education, maternal age at conception and its squared term, gestation length and indicators for the timing of initial prenatal care visits. County fixed effects are separate indicator variables for each county. Year fixed effects are indicators that allow for unrestricted differences in year-to-year changes. Reported in parentheses are standard errors clustered by county. *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 3.1: Summary statistics (samples with ultrasound information)

Variables	Observations	Mean	Standard Deviation
Panel A			
Subsample with mortality measures			
Girl	284728	0.468	0.499
Ultrasound	284728	0.360	0.480
Death within 24 hours of birth	284728	0.003	0.056
Death within 7 days of birth	284728	0.006	0.075
Death within 28 days of birth	284728	0.007	0.084
Death within 28 days to 1 year	282711	0.003	0.055
Panel B			
Subsample with postnatal measures			
Girl	93967	0.458	0.498
Ultrasound	93967	0.361	0.480
Vaccine 1 (BCG)	93601	0.159	0.366
Vaccine 2 (IPV)	93600	0.157	0.363
Vaccine 3 (DTPa)	93600	0.192	0.394
Vaccine 4 (Measles)	93600	0.254	0.435
Breastfeeding	93968	0.971	0.167
Duration of breastfeeding (months)	91263	14.842	7.843
Taken care of by mother	93967	0.846	0.361

Notes:
The samples are taken from the Chinese Children Survey.
Information on ultrasound access is collected by the authors.

Table 3.2: Summary statistics (sibling sample with ultrasound information)

Variables	Observations	Mean	Standard Deviation
Panel A			
Subsample with mortality measures			
Girl	243482	0.468	0.499
Ultrasound	243482	0.307	0.461
Death within 24 hours of birth	243482	0.003	0.059
Death within 7 days of birth	243482	0.006	0.080
Death within 28 days of birth	243482	0.008	0.089
Death within 28 days to 1 year	241557	0.003	0.058
Panel B			
Subsample with postnatal measures			
Girl	80121	0.458	0.498
Ultrasound	80121	0.308	0.462
Vaccine 1 (BCG)	79926	0.163	0.369
Vaccine 2 (IPV)	79926	0.159	0.365
Vaccine 3 (DTPa)	79926	0.195	0.396
Vaccine 4 (Measles)	79926	0.257	0.437
Breastfeeding	80122	0.971	0.168
Duration of breastfeeding (months)	77806	14.852	7.868
Taken care of by mother	80121	0.846	0.361

Notes:
The samples are taken from the Chinese Children Survey.
Information on ultrasound access is collected by the authors.

Table 3.3: The effect of ultrasound on mortality: with and without mother fixed effects

	Full sample	Sibling sample No mother FE	Sibling sample With mother FE
	(1)	(2)	(3)
		Death within 24 hours of birth	
Girl	0.0002	0.0002	0.0002
	(0.0003)	(0.0003)	(0.0003)
Ultrasound	0.0007**	0.0016***	-0.0009
	(0.0003)	(0.0004)	(0.0006)
Ultrasound × Girl	0.0009**	0.0010*	0.0019***
	(0.0004)	(0.0005)	(0.0007)
Observations	284254	243132	243132
R-squared	0.0033	0.0037	0.0033
		Death within 7 days of birth	
Girl	-0.0002	-0.0002	-0.0003
	(0.0003)	(0.0004)	(0.0005)
Ultrasound	0.0014***	0.0031***	-0.0028***
	(0.0004)	(0.0006)	(0.0009)
Ultrasound × Girl	0.0014**	0.0013*	0.0025***
	(0.0006)	(0.0007)	(0.0010)
Observations	284254	243132	243132
R-squared	0.0035	0.0043	0.0038
County fixed effects	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes
County time trends	Yes	Yes	Yes

Notes: Individual controls include birth order indicators, mother's ethnicity, education, maternal age at conception and its squared term. Standard errors clustered at the county level are reported in parentheses;
*denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 3.4: The effect of ultrasound on mortality: with and without mother fixed effects (continued)

	Full sample	Sibling sample No mother FE	Sibling sample With mother FE
	(1)	(2)	(3)
		Death within 28 days of birth	
Girl	-0.0002	-0.0001	-0.0002
	(0.0004)	(0.0004)	(0.0005)
Ultrasound	0.0020***	0.0042***	-0.0033***
	(0.0005)	(0.0006)	(0.0010)
Ultrasound × Girl	0.0013**	0.0011	0.0026**
	(0.0007)	(0.0008)	(0.0011)
Observations	284254	243132	243132
R-squared	0.0037	0.0047	0.0041
		Death within 28 days to 1 year†	
Girl	-0.0001	-0.0001	-0.0001
	(0.0003)	(0.0003)	(0.0003)
Ultrasound	0.0008**	0.0018***	-0.0007
	(0.0003)	(0.0004)	(0.0006)
Ultrasound × Girl	0.0006	0.0005	-0.0001
	(0.0005)	(0.0005)	(0.0007)
Observations	282240	241210	241210
R-squared	0.0035	0.0041	0.0036
County fixed effects	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes
County time trends	Yes	Yes	Yes

Notes: Individual controls include birth order indicators, mother's ethnicity, education, maternal age at conception and its squared term. Standard errors clustered at the county level are reported in parentheses;
*denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level; analysis of infant death within 28 days to 1 year are conducted for neonatal survivors.

Table 3.5: The effect of ultrasound on vaccination: with and without mother fixed effects

	Full sample	Sibling sample No mother FE	Sibling sample With mother FE
	(1)	(2)	(3)
		Vaccine 1	
Girl	0.0032	0.0030	0.0068*
	(0.0027)	(0.0028)	(0.0041)
Ultrasound	0.0009	-0.0002	-0.0132
	(0.0041)	(0.0046)	(0.0099)
Ultrasound × Girl	0.0017	0.0007	0.0079
	(0.0045)	(0.0048)	(0.0084)
Observations	93454	79816	79816
R-squared	0.1769	0.1812	0.0276
		Vaccine 2	
Girl	0.0061**	0.0057**	0.0082*
	(0.0027)	(0.0029)	(0.0044)
Ultrasound	0.0023	-0.0009	-0.0165*
	(0.0043)	(0.0049)	(0.0097)
Ultrasound × Girl	-0.0040	-0.0002	0.0030
	(0.0046)	(0.0051)	(0.0083)
Observations	93453	79816	79816
R-squared	0.1261	0.1288	0.0373
County fixed effects	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes
County time trends	Yes	Yes	Yes

Notes: Individual controls include birth order indicators, mother's ethnicity, education, maternal age at conception and its squared term. Standard errors clustered at the county level are reported in parentheses;
*denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 3.6: The effect of ultrasound on vaccination: with and without mother fixed effects (continued)

	Full sample (1)	Sibling sample No mother FE (2)	Sibling sample With mother FE (3)
		Vaccine 3	
Girl	0.0050*	0.0044	0.0044
	(0.0028)	(0.0030)	(0.0047)
Ultrasound	0.0005	-0.0025	-0.0123
	(0.0045)	(0.0053)	(0.0099)
Ultrasound × Girl	0.0010	0.0040	0.0065
	(0.0049)	(0.0057)	(0.0091)
Observations	93453	79816	79816
R-squared	0.1488	0.1518	0.0416
		Vaccine 4	
Girl	0.0116***	0.0111***	0.0114**
	(0.0032)	(0.0034)	(0.0051)
Ultrasound	0.0039	0.0046	-0.0026
	(0.0046)	(0.0053)	(0.0107)
Ultrasound × Girl	-0.0087	-0.0065	-0.0132
	(0.0054)	(0.0062)	(0.0102)
Observations	93453	79816	79816
R-squared	0.1531	0.1527	0.0803
County fixed effects	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes
County time trends	Yes	Yes	Yes

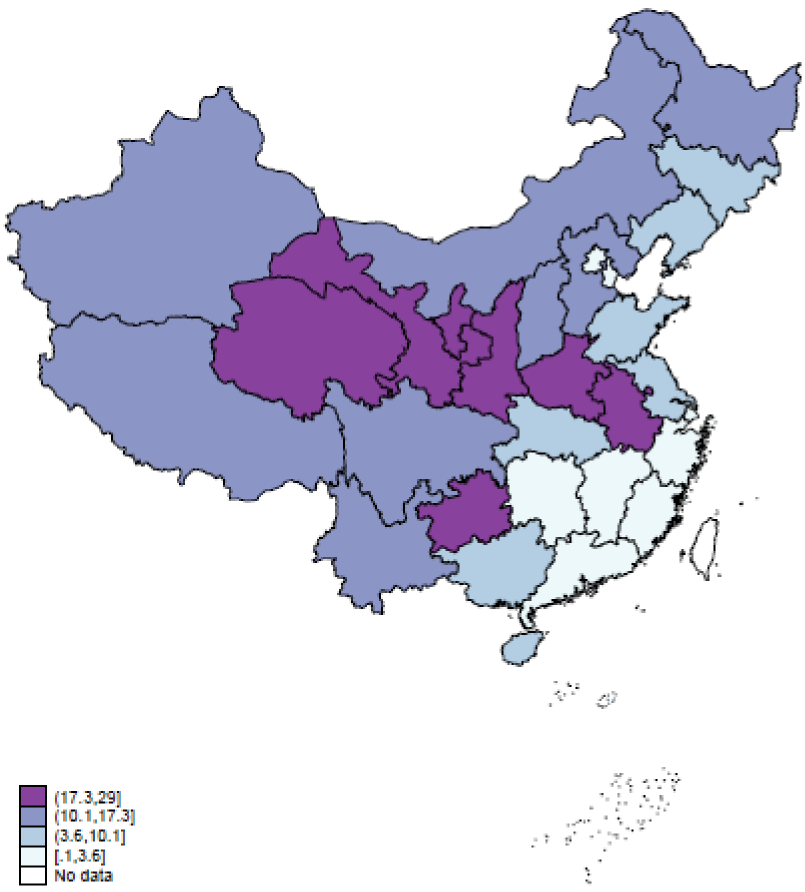
Notes: Individual controls include birth order indicators, mother's ethnicity, education, maternal age at conception and its squared term. Standard errors clustered at the county level are reported in parentheses;
*denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 3.7: The effect of ultrasound on care: with and without mother fixed effects

	Full sample	Sibling sample No mother FE	Sibling sample With mother FE
	(1)	(2)	(3)
		Breastfeeding	
Girl	-0.0020	-0.0019	-0.0020
	(0.0014)	(0.0014)	(0.0022)
Ultrasound	0.0028	0.0019	0.0020
	(0.0017)	(0.0020)	(0.0043)
Ultrasound × Girl	-0.0027	-0.0027	-0.0125***
	(0.0023)	(0.0026)	(0.0043)
Observations	93831	80025	80025
R-squared	0.0384	0.0407	0.0128
		Duration of breastfeeding	
Girl	-0.6988***	-0.6698***	-0.5756***
	(0.0606)	(0.0632)	(0.0969)
Ultrasound	0.1983***	0.2286***	0.2616
	(0.0751)	(0.0848)	(0.1818)
Ultrasound × Girl	0.0447	0.0042	-0.0925
	(0.0977)	(0.1129)	(0.1867)
Observations	91136	77715	77715
R-squared	0.3440	0.3458	0.3124
		Taken care of by mother	
Girl	-0.0032	-0.0028	-0.0054
	(0.0024)	(0.0025)	(0.0041)
Ultrasound	-0.0001	0.0022	-0.0072
	(0.0034)	(0.0040)	(0.0078)
Ultrasound × Girl	0.0017	0.0010	0.0049
	(0.0042)	(0.0051)	(0.0079)
Observations	93830	80024	80024
R-squared	0.1403	0.1428	0.0288
County fixed effects	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes
County time trends	Yes	Yes	Yes

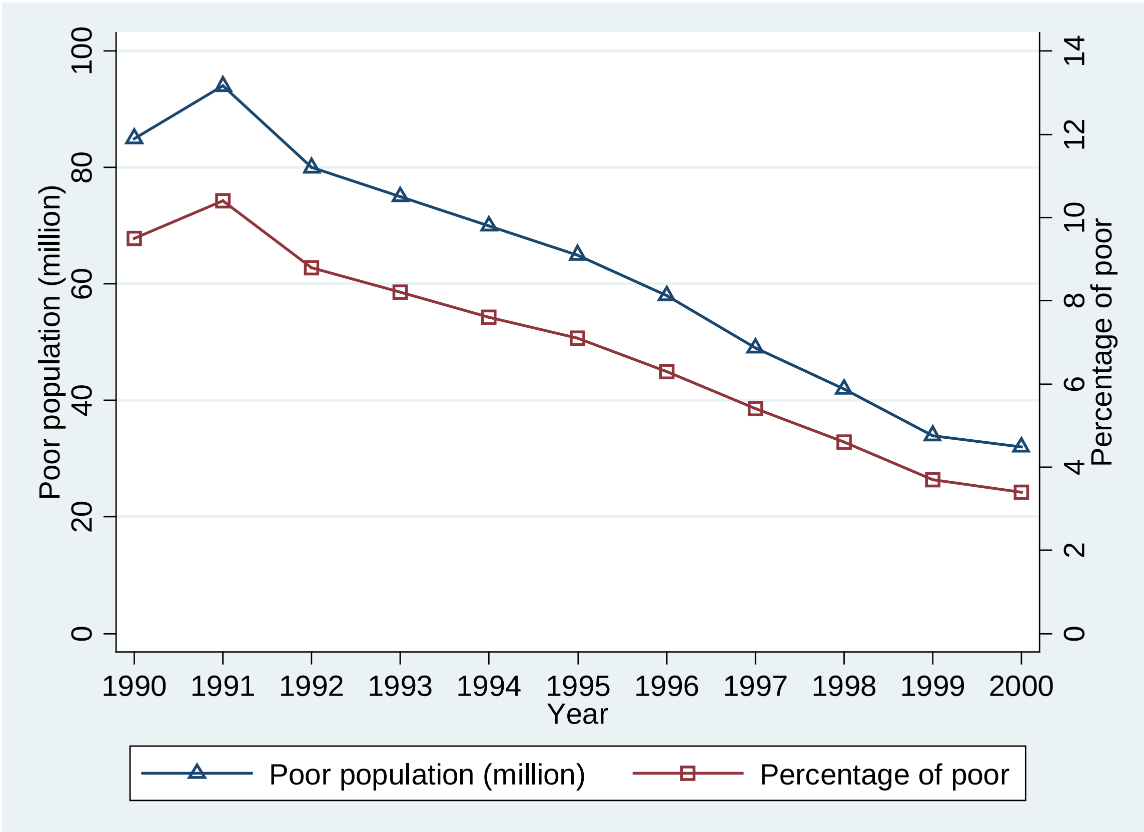
Notes: Individual controls include birth order indicators, mother's ethnicity, education, maternal age at conception and its squared term. Standard errors clustered at the county level are reported in parentheses; *denotes statistical significance at the 10% level, ** at the 5% level, *** at the 1% level.

Figure 1.1: Incidence of rural poverty in 1991



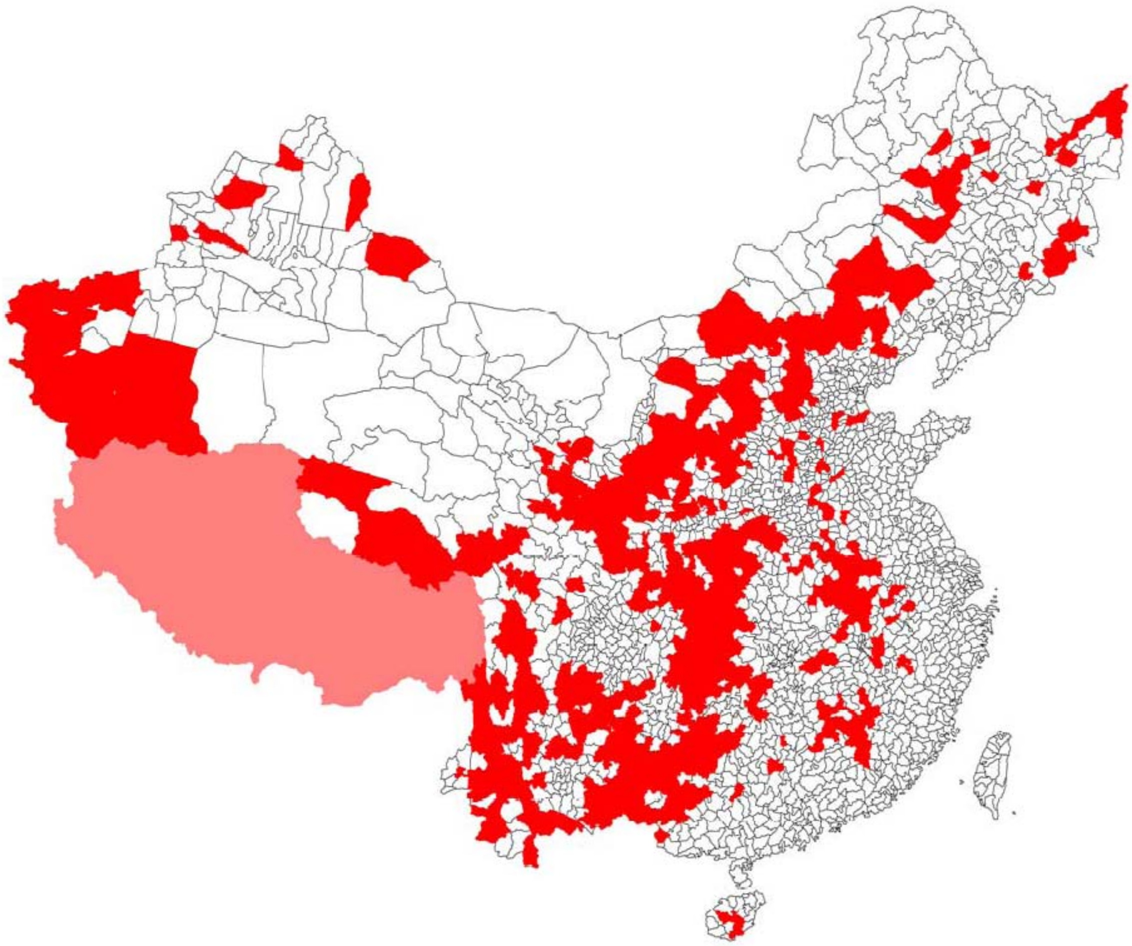
Source: World Bank (2000)
Notes: Incidence of rural poverty is defined as the number of poor as a percentage of rural population.

Figure 1.2: National trends in China's official poverty headcount, 1990-2000



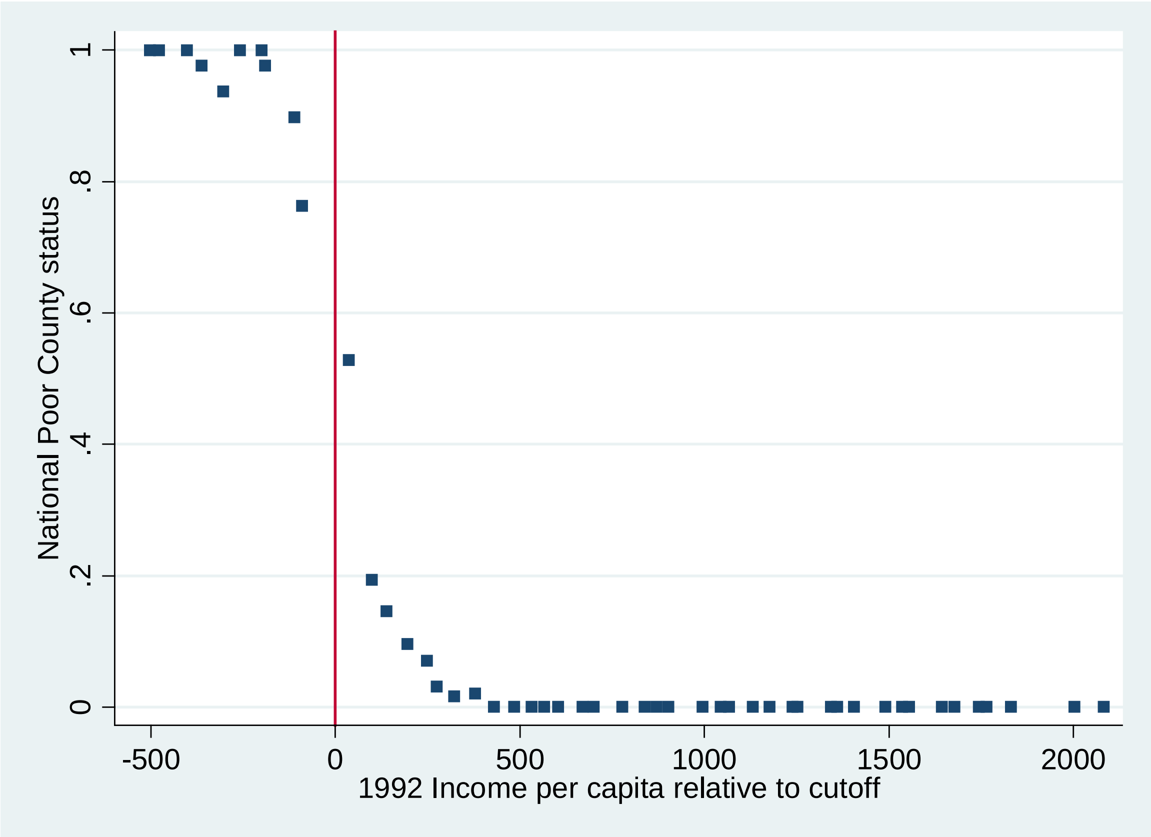
Source: Park and Wang (2001)

Figure 1.3: Map of National Poor Counties (designated in1994)



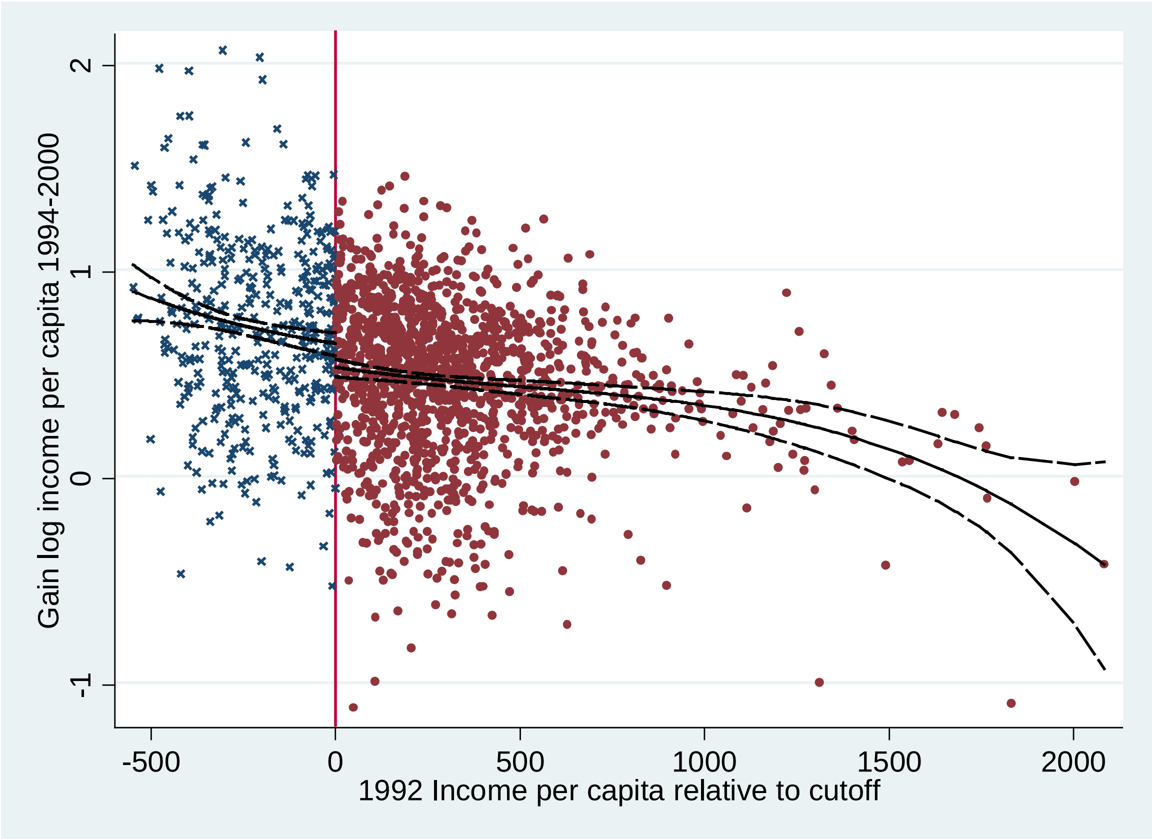
Source: Heilig et al. (2006)
Notes: The National Poor Counties designated in 1994 are denoted by the red shaded regions.

Figure 1.4: Program placement



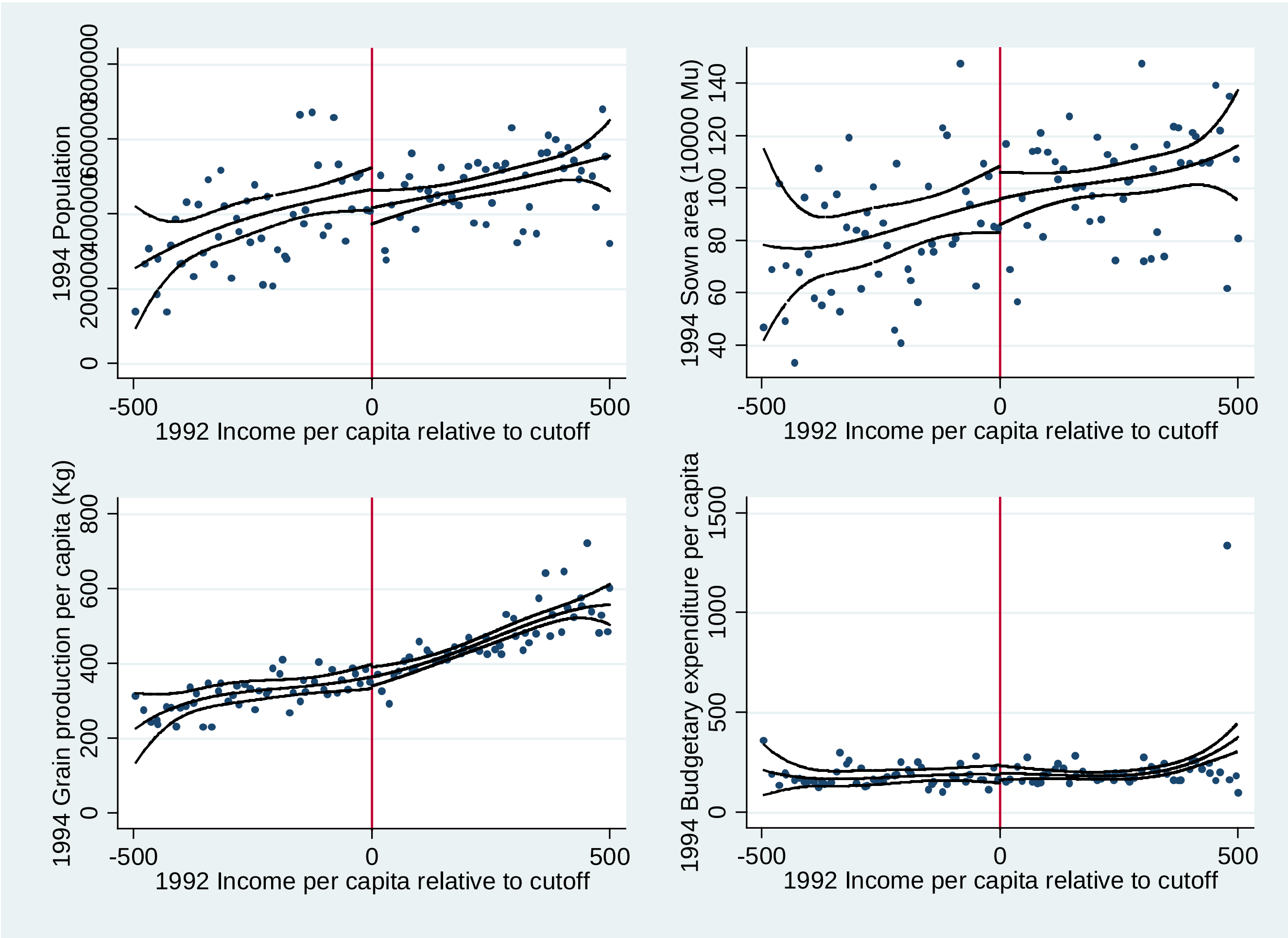
Notes: Each solid square represents the fraction of counties designated as poor within 10 yuan intervals of the 1992 income per capita relative to cutoff.

Figure 1.5: Change in log income per capita by 1992 income per capita relative to cutoff



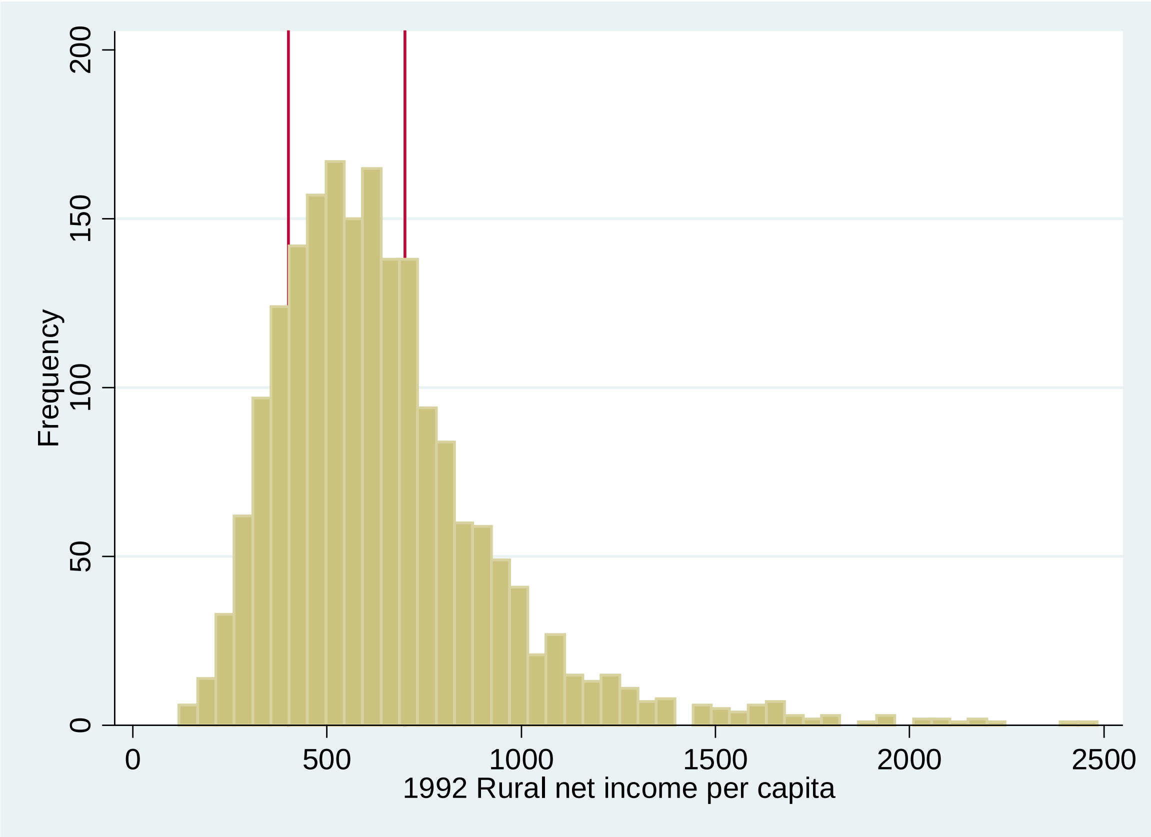
Notes: The continuous line is the predicted outcome from a regression that includes a third order polynomial in the running variable, and a dummy for observations above the cutoff. The dashed lines are pointwise 95 percent confidence intervals from the regression.

Figure 1.6: Similarity of counties' pre-treatment characteristics around the eligibility cutoff



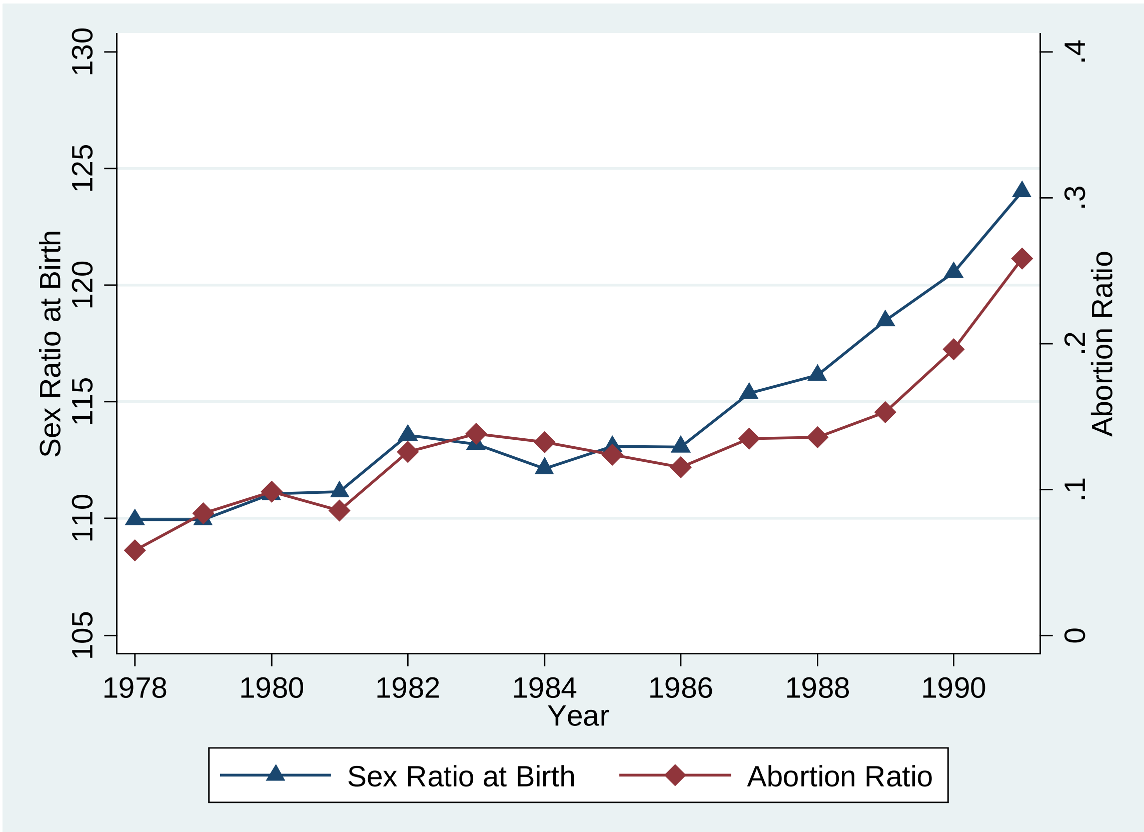
Notes: Panels refer to (from top left to bottom right) the following county attributes: population, sown area grain production per capita and budgetary expenditure per capita for the year of 1994. The continuous line represents the predicted value from a fourth order polynomial in the running variable , and a dummy for counties above the cutoff. The dashed lines are pointwise 95 percent confidence intervals from the regression.

Figure 1.7: Histogram of the 1992 rural net income per capita



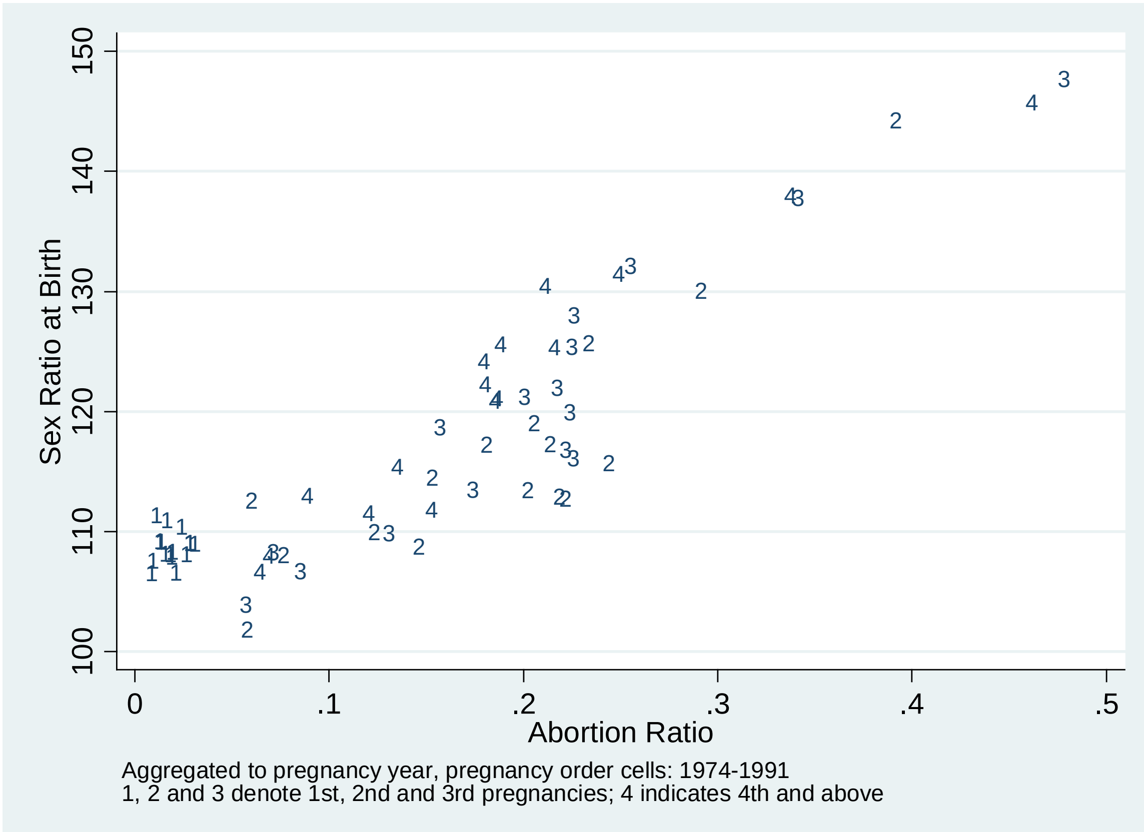
Notes: The red lines indicate the poverty lines used for 1994 designation.

Figure 2.1: Sex ratio at birth and abortion ratio by year



Source: Chinese Children Survey, June 1992
Notes: Sex ratio at birth is defined as the number of male births per 100 female births. Abortion ratio is defined as the proportion of pregnancies ending in abortion.

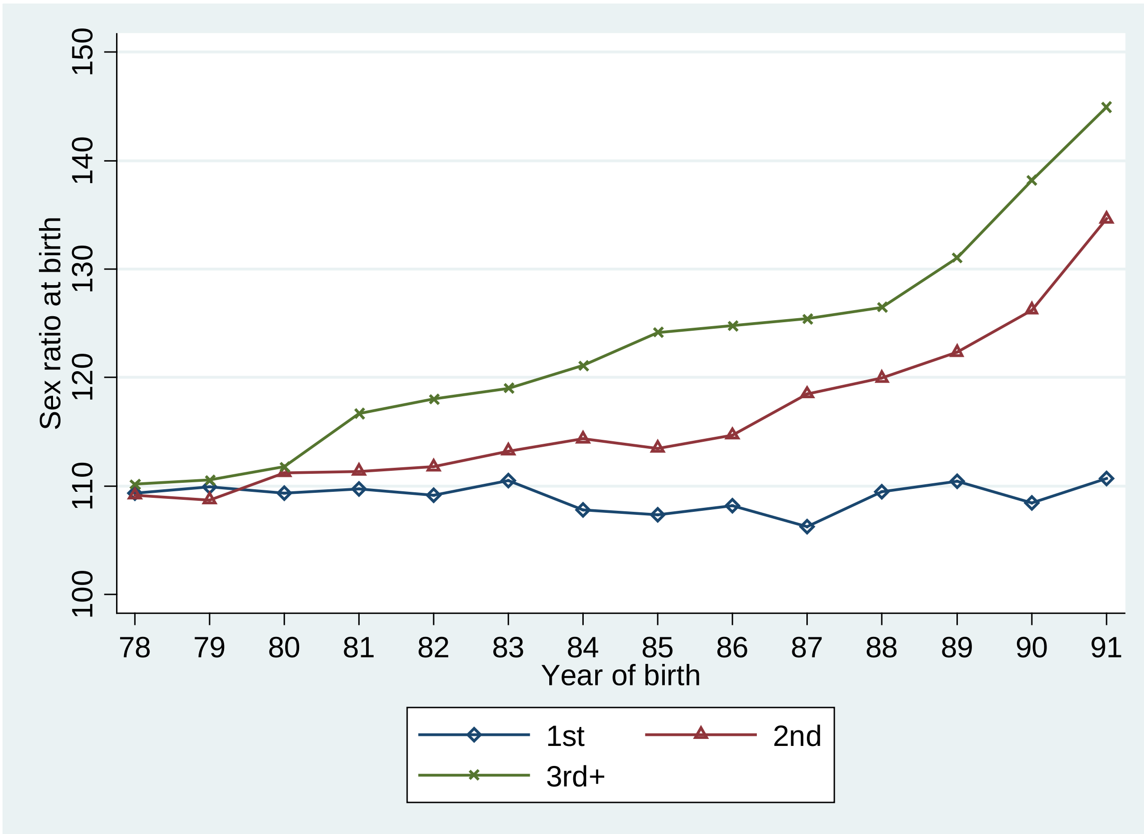
Figure 2.2: Sex ratio at birth and abortion ratio by pregnancy year and pregnancy order



Source: Chinese Children Survey, June 1992

Notes: Sex ratio at birth is defined as the number of male births per 100 female births. Abortion ratio is defined as the proportion of pregnancies ending in abortion.

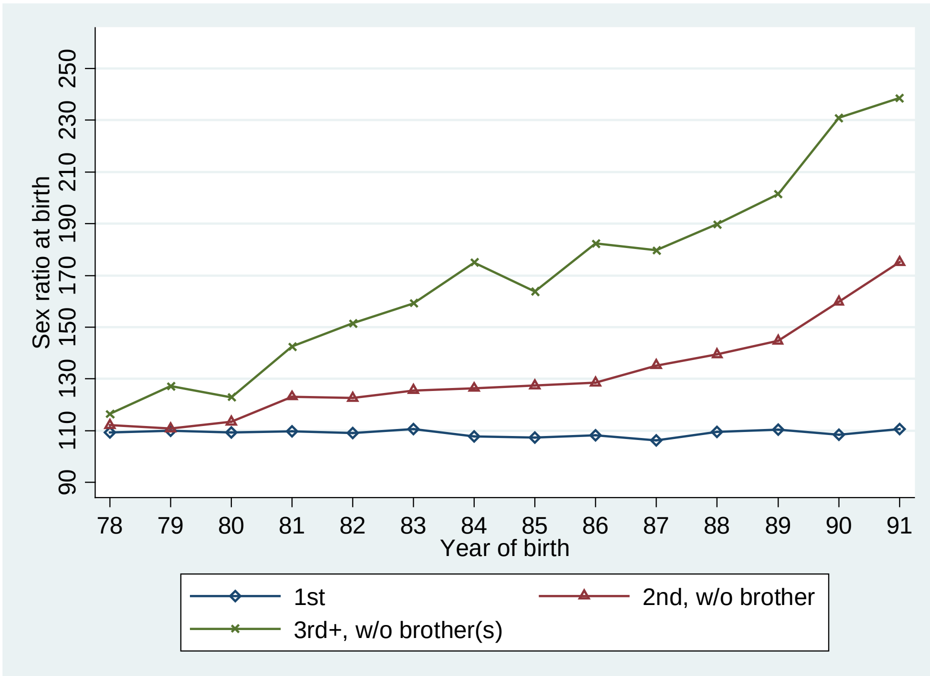
Figure 2.3: Sex ratio at birth by parity and over time



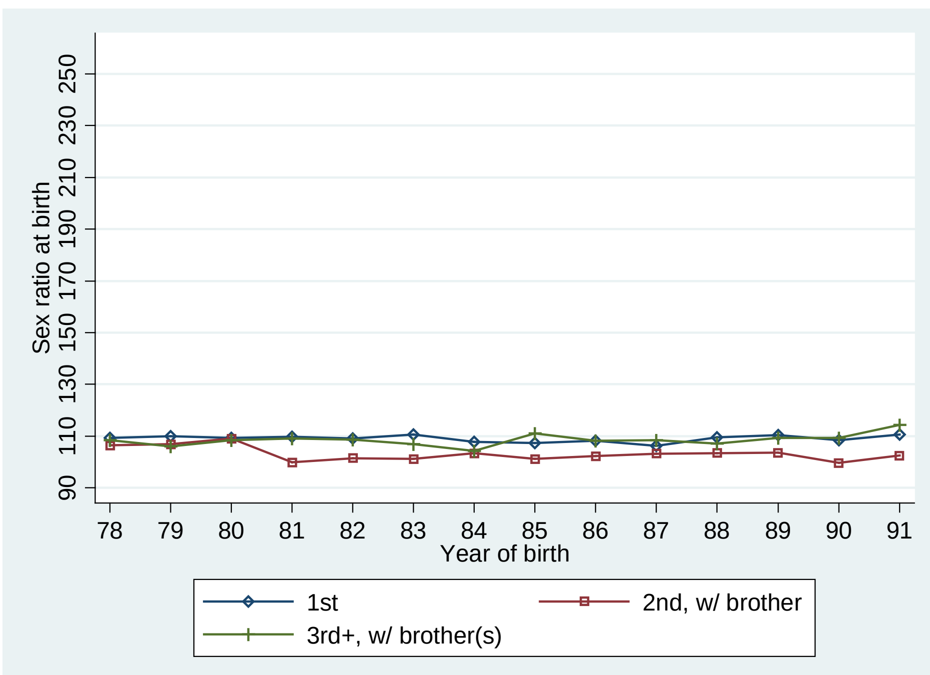
Source: Chinese Children Survey, June 1992
Notes: Sex ratio at birth is defined as the number of male births per 100 female births.

Figure 2.4: Sex ratio at birth by parity and sex of older sibling(s) over time

(a) First birth and higher order birth without brother(s)

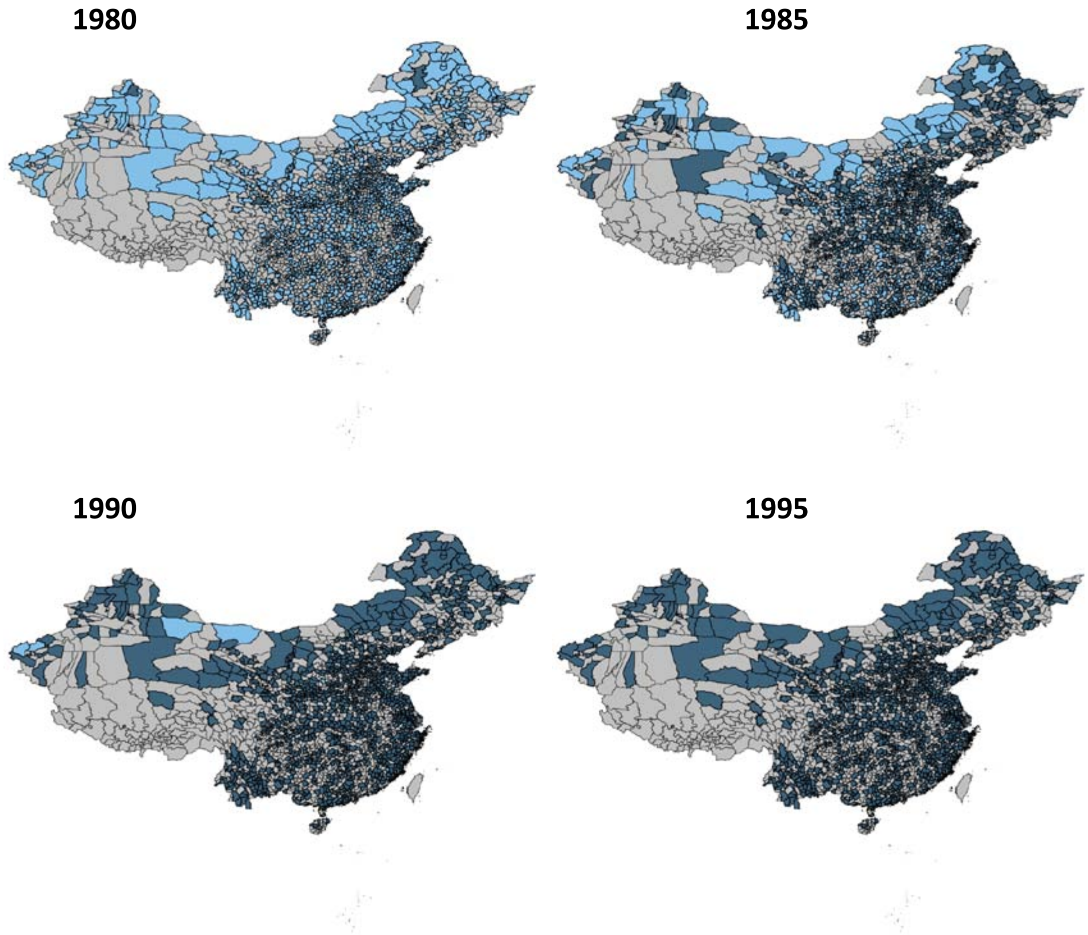


(b) First birth and higher order birth with brother(s)



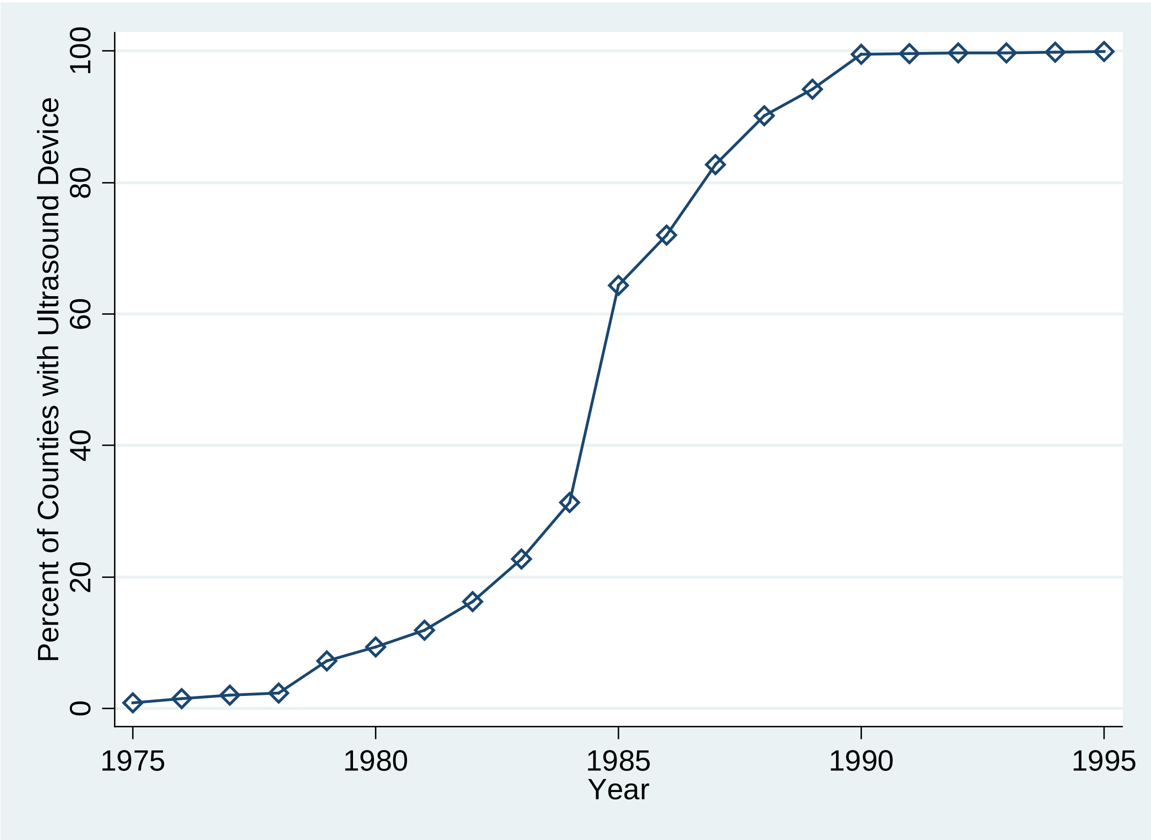
Source: Chinese Children Survey, June 1992
Notes: Sex ratio at birth is defined as the number of male births per 100 female births.

Figure 2.5: The spread of ultrasound technology across Chinese counties



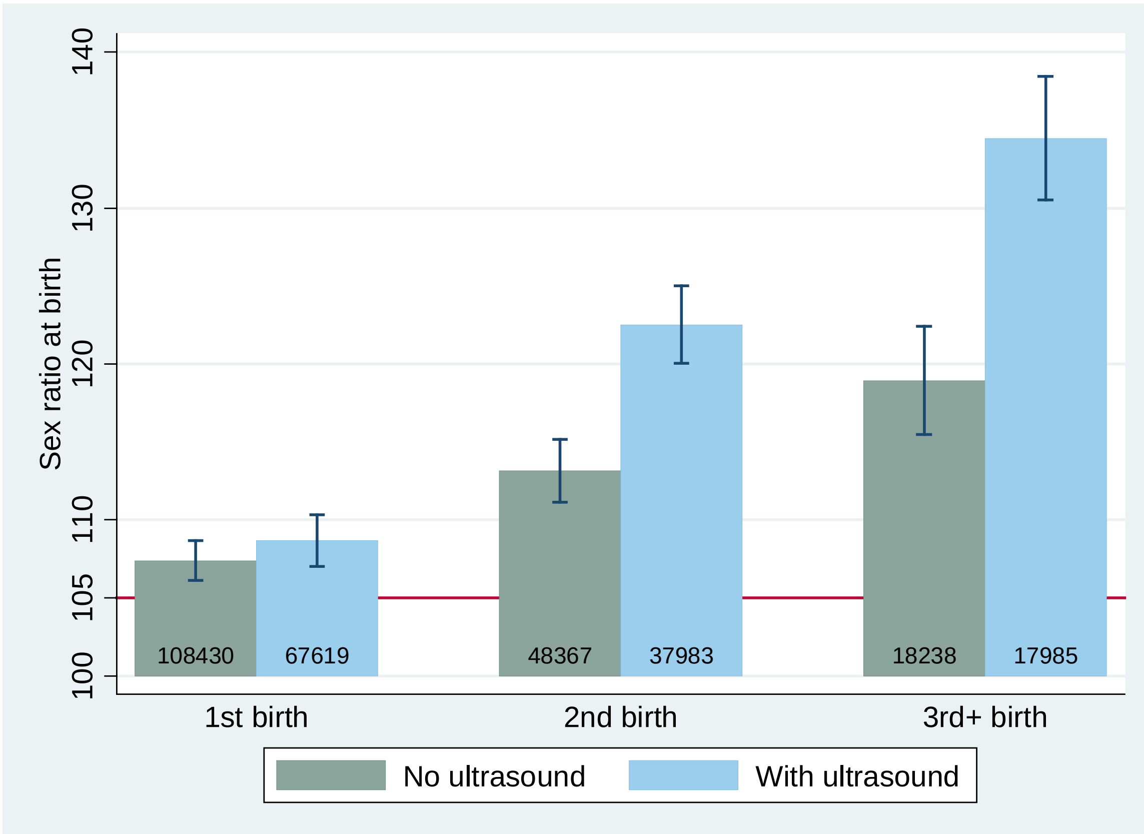
Notes: Tabulations of the author's own dataset on ultrasound introduction at the county level. The shading corresponds to the availability of ultrasound, where dark blue areas represent counties that had ultrasound; light blue areas corresponds to counties without ultrasound and grey areas are counties for which the information on ultrasound adoption is not available.

Figure 2.6: Percent of Chinese counties with ultrasound, 1975-1995



Notes: Tabulations of the author's own dataset.

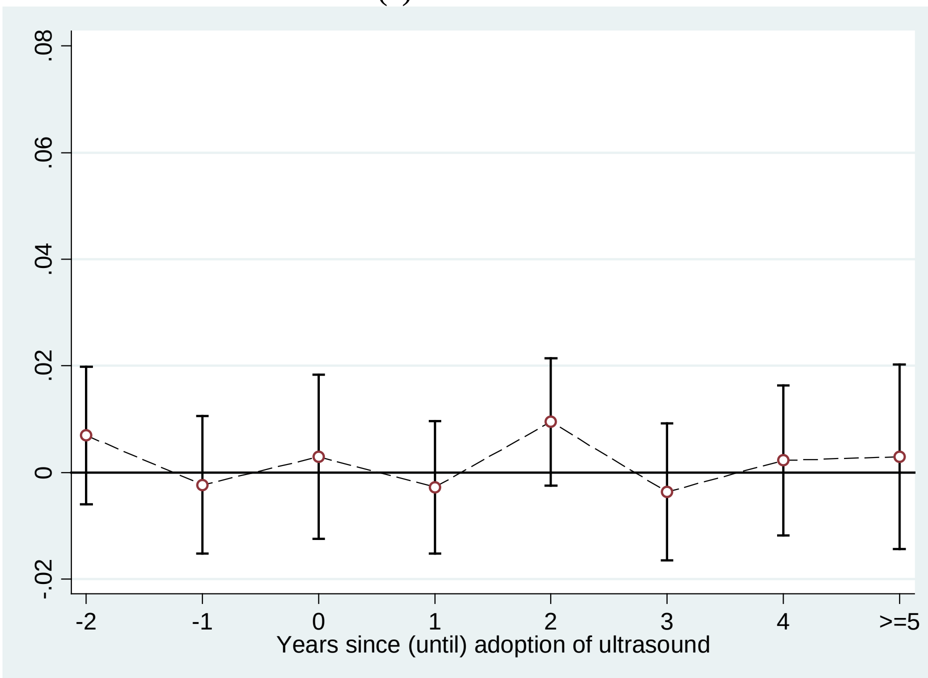
Figure 2.7: Sex ratio at birth by parity and by availability of ultrasound



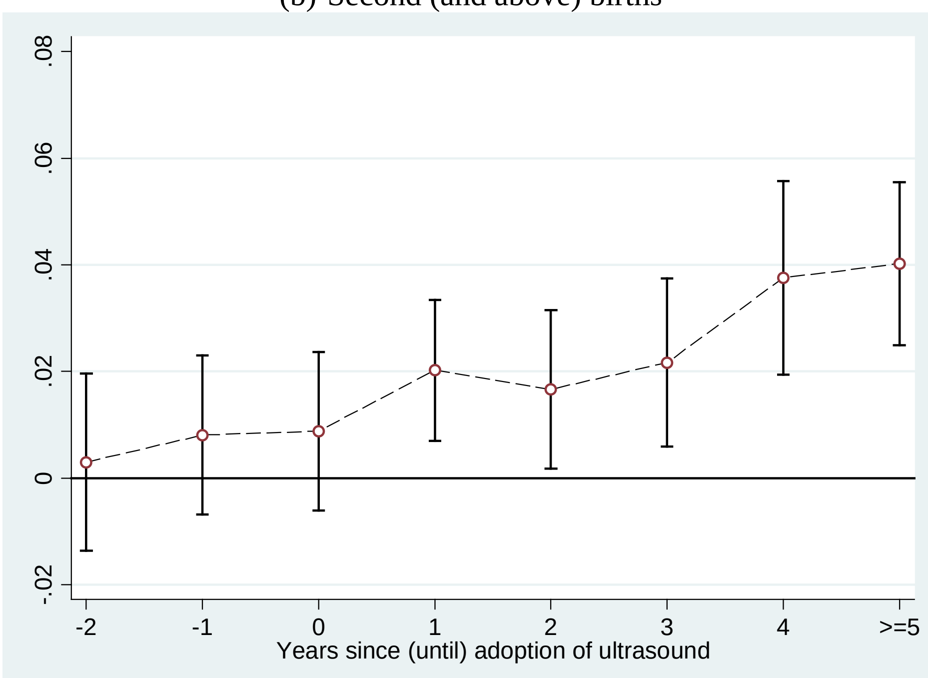
Source: Chinese Children Survey, June 1992
Notes: Sex ratio at birth is defined as the number of male births per 100 female births. Error bars represent 95% CI around the sample mean. The underlying population size is given at the bottom of each bar.

Figure 2.8: Estimates from event study analysis

(a) First births

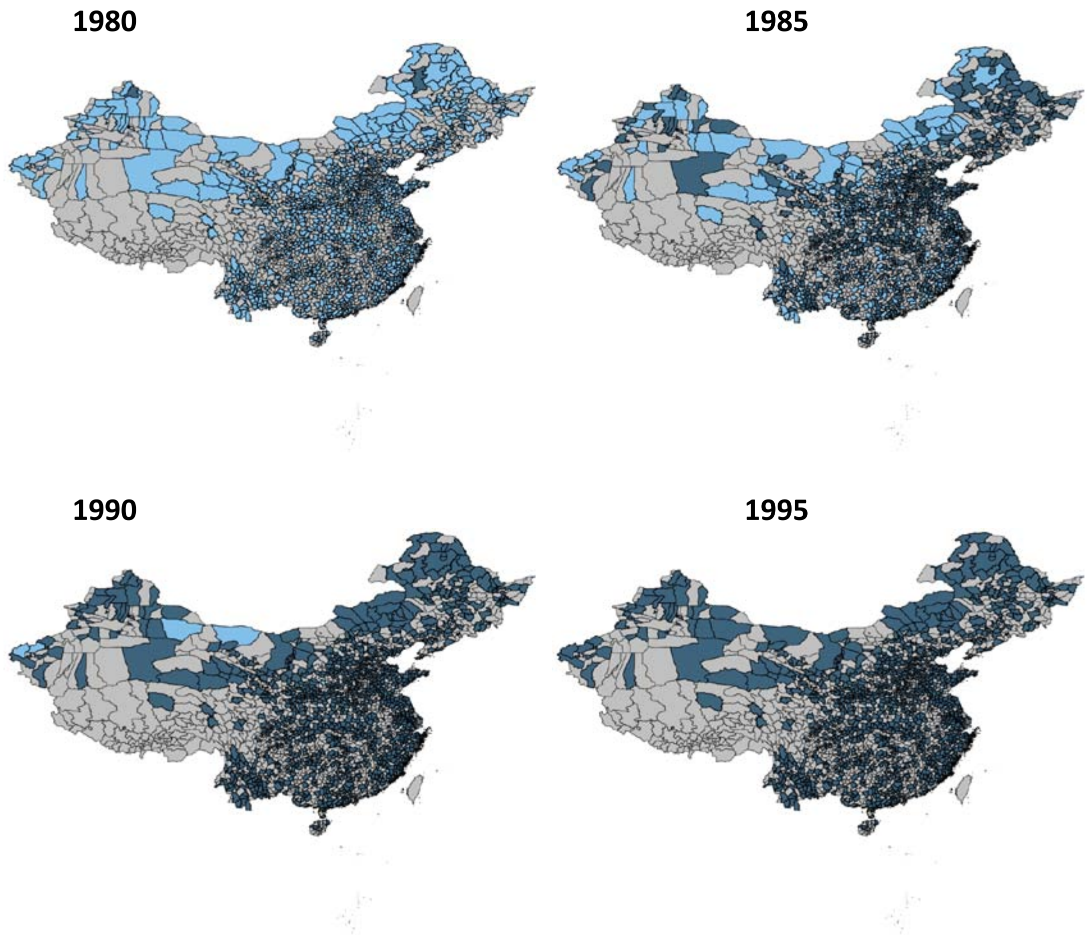


(b) Second (and above) births



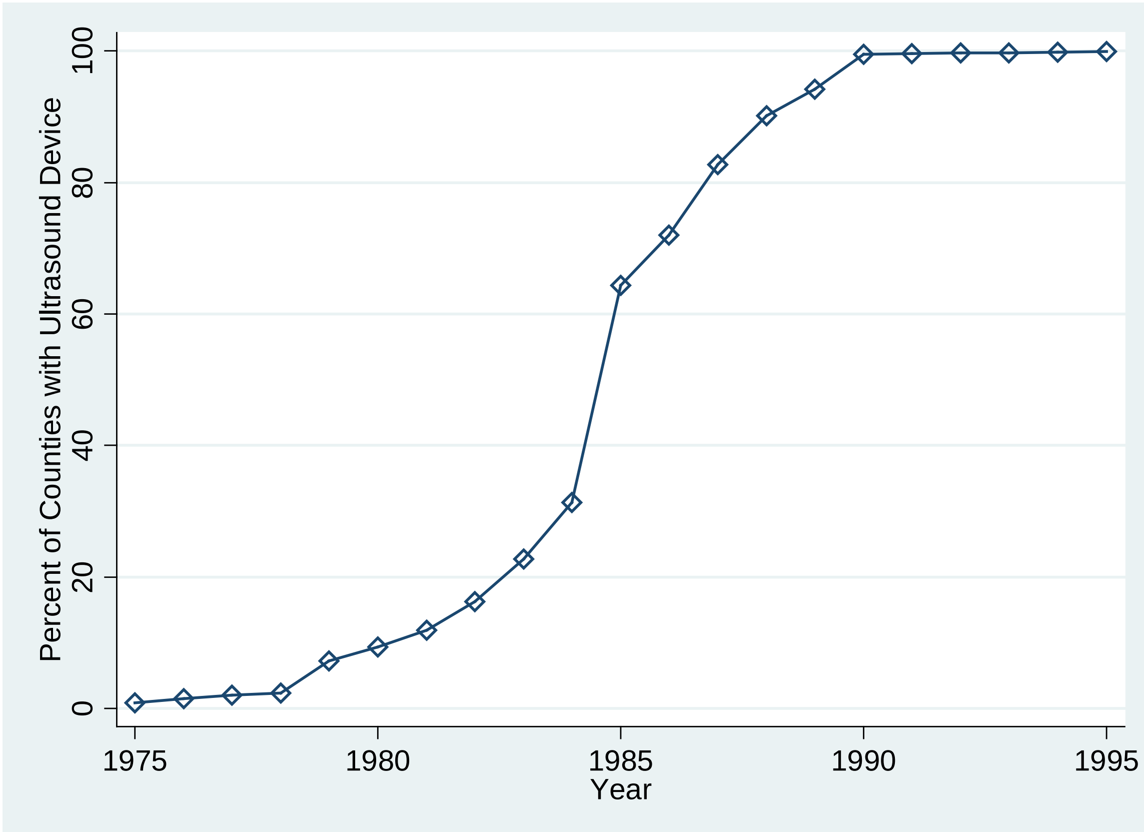
Notes: Each figure plots coefficients from an event-study analysis that allows for effects before, during and after the adoption of ultrasound. The full specification is described in Equation (5). Panel (a) plots $\hat{\delta}_\tau$'s, the coefficients of the interaction terms of the event year dummy variables and the dummy variable for first births. Panel (b) plots $\hat{\lambda}_\tau$'s, the coefficients of the interaction terms of the event year dummy variables and the dummy variable for second (and above) births.

Figure 3.1: The spread of ultrasound technology across Chinese counties



Notes: Tabulations of the authors' own dataset on ultrasound introduction at the county level. The shading corresponds to the availability of ultrasound, where dark blue areas represent counties that had ultrasound; light blue areas corresponds to counties without ultrasound and grey areas are counties for which the information on ultrasound adoption is not available

Figure 3.2: Percent of Chinese counties with ultrasound, 1975-1995



Notes: Tabulations of the authors' own dataset.

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