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JEL: I15, I38, P26, Q15, Q18

Keywords: land reform, birth weight, early-life health, agricultural productivity, China

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1. Introduction

Poorly defined land property rights—characterized by insecurity, inefficiency, and inequity—are widely recognized as significant barriers to economic growth and national development (Banerjee et al., 2002; Besley and Ghatak, 2010). Consequently, many scholars and social activists advocate for clear and secure land rights as essential measures to combat poverty (De Soto, 2003; Deininger, 2003). A wealth of empirical research demonstrates that land reforms, particularly those that enhance property rights through titling and formalization, yield numerous positive outcomes. These reforms spur agricultural investment (Banerjee et al., 2002; Fenske, 2011; Hornbeck, 2010), ease financial constraints (Besley et al., 2012), improve land allocation (Chari et al., 2021), and bolster economic growth (Besley and Burgess, 2000; Galiani and Schargrodsky, 2010). However, there remains limited evidence on how strengthening individuals' land rights affects subsequent generations.

This paper fills this gap by exploring the impact of land reform on health outcomes for newborns. Emphasizing the early-life health of the next generation is of utmost importance, as health endowments fundamentally shape both later-life health and socioeconomic trajectories (Almond and Currie, 2011; Barker, 1998; Behrman and Rosenzweig, 2004; Royer, 2009). Assessing the impact of the reform on neonatal health is therefore essential for understanding its long-term implications and its potential to sustainably uplift target populations from cycles of generational poverty.

In this paper, we leverage the staggered rollout of China's prominent Household Responsibility System (HRS) reform to investigate the causal effect of prenatal exposure to improved land property rights on birth weight, a critical measure of birth outcomes. Before this reform, rural laborers were organized into production teams and farmers' rewards were only loosely connected to their actual work, leading to diminished incentives and widespread free-riding. This disconnect caused agricultural growth rates to lag far behind population expansion (Lin, 1988), leaving farmers with unstable food entitlements. The HRS reform allocated collectively owned farmland to individual households under contracts of up to 15 years, granting them exclusive land use and residual claim rights while retaining collective land ownership. This strategic shift in property rights addressed the incentive problem, resulting in increased labor productivity and higher rural household incomes (Lin, 1992).

In theory, exposure to the HRS reform during pregnancy could improve birth outcomes by increasing household income, which might lead to better maternal nutrition, greater investments in prenatal care, and improved access to medical services. Additionally, the reform granted households more autonomy in production decisions, potentially allowing pregnant women to reduce their workload or take maternity leave more easily, further contributing to positive birth outcomes. However, there is also a potential downside: the HRS reform may have adversely affected fetal development through a "substitution effect." As the reform increased labor productivity, it raised the value of a woman's time, thereby increasing the opportunity costs of fetal care (Miller and Urdinola, 2010). If pregnant women redirected their time from fetal care to productive activities, birth outcomes might have worsened. These possibilities suggest that the impact of reform exposure on birth outcomes is ambiguous, warranting comprehensive empirical investigation.

For this purpose, we utilize data from the 1985 and 1987 waves of the China In-Depth Fertility Survey (IDFS), which, to the best of our knowledge, is the earliest micro-level fertility dataset available in China. This dataset contains detailed information on birth histories and outcomes for eligible ever-married women aged 15 to 49. We combine this data with information on the timing of HRS reform for each county, which we manually extracted from various county gazetteers. By leveraging the staggered rollout of the HRS reform across counties, we employ a difference-in-differences approach to identify the causal effect of prenatal reform exposure on birth weight.

We restrict our sample to mothers who had at least two children during the study period, allowing us to directly compare the birth weights of siblings with different prenatal exposure to the HRS reform. This sibling-based approach has been widely used in studies evaluating the impacts of early-life events on human capital accumulation (Currie, 2009). A key advantage of this method is its ability to flexibly control for unobserved household or maternal time-invariant confounders that may affect birth weight. Equally important, this identification strategy helps address potential shifts in the composition of expectant mothers post-reform. For example, households with healthier laborers, particularly parents, might experience greater income growth following the reform, which could increase their fertility desires. If healthier parents were more likely to have children after the reform, the observed improvement in birth weight could be attributed to genetic predispositions, rather than the effect of prenatal exposure to the HRS reform.

Using this methodological framework, our analyses reveal that the HRS reform significantly improved the birth weight of the children exposed to it *in utero*. According to our preferred specifications, the average birth weight increased by approximately 55 grams for those with prenatal exposure to the reform. These findings suggest that the positive effects of increased household income during gestation due to the HRS reform outweighed any potential substitution effects. Consistent with this interpretation, our results show that the reform's impact on birth weight is more pronounced in counties with a higher share of female-labor-intensive crops, indicating that increased productivity post-land reform did not necessarily elevate the opportunity costs of women's time or crowd out the amount of prenatal care received by mothers. Furthermore, the effects of the HRS reform are more salient for children with fewer older siblings and those from counties with better land endowments—regions that likely have experienced a more substantial increase in resources due to the reform.

To provide more direct evidence of the income effect, we collect provincial-level data to examine the impact of the HRS reform on *per capita* grain output and rural net income. Our findings demonstrate that full adoption of HRS at the province level led to substantial increases in resources for rural households. The income effects could operate through multiple channels and we further differentiate the roles of nutrition intake and healthcare access in affecting birth weight. Surprisingly, our results show that children exposed to the HRS reform in utero were significantly *less* likely to receive prenatal checks and institutional deliveries. We interpret these results as evidence that the HRS reform may have contributed to the collapse of the rural cooperative medical system, which reduced healthcare accessibility for rural households—an issue largely overlooked in previous literature (Chen et al., 1993; Wagstaff et al., 2009). Moreover, our findings reveal a significant positive association between the HRS adoption rate and *per capita* rural food consumption at the province level, indicating that nutrition intake indeed increased in rural areas following the HRS rollout. We also explore the possibility of the maternity leave mechanism, considering that the HRS reform granted households greater autonomy in production decisions, potentially allowing pregnant women to take leave from agricultural work more easily. While this mechanism is plausible, our analysis suggests it is less likely to be the dominant factor behind our results. Taken together, these results suggest that improved nutrition intake is likely the primary mechanism behind the observed increase in birth weight.

We conduct a battery of robustness checks to validate our results. First, we investigate the potential selection bias stemming from endogenous fertility responses after the HRS rollout. We analyze changes in fertility behaviors at both the county and individual levels, and find no evidence of selective fertility following the reform's implementation. Second, we perform a placebo test using an urban sample, which was not directly affected by the HRS reform. This test yields no discernible effects, further supporting the credibility of our main findings. Third, to address potential estimation bias arising from the staggered timing of the HRS reform across counties, we employ the heterogeneity-robust estimator as suggested by Sun and Abraham (2021) to examine whether our main results hold under this alternative method. Additionally, we assess the sensitivity of our findings by selecting alternative sample periods and using randomly assigned placebo years for HRS adoption in our estimations. These analyses consistently corroborate our initial results, reinforcing the robustness of our conclusions.

Our paper contributes to the literature on the effects of land reforms on poverty reduction. While extensive literature has focused on government interventions that formally recognize farmers' land rights, the scope of this focus has been somewhat limited. Specifically, scholars have examined how such interventions influence agricultural investments (Banerjee et al., 2002; Galiani and Schargrodsky, 2010; Hornbeck, 2010) and land reallocation (Chari et al., 2021). Additionally, some research has explored associated labor market outcomes, particularly labor reallocation (de Janvry et al., 2015). Yet, there remains a lack of evidence regarding the impact of these reforms on early-life endowments for the next generation, leaving a crucial gap in understanding whether these reforms can break the cycle of intergenerational poverty. Our study complements the existing literature by examining the effect of China's HRS reform on birth outcomes. We provide rigorous evidence of a significant income effect resulting from the reform, which leads to notable improvements in birth weight.

This study also enriches the rapidly expanding literature examining the impacts of early-life shocks, particularly those occurring during gestation. The theory of fetal origins posits that prenatal conditions, as captured by birth endowments, can significantly influence an individual's later-life social and economic trajectory (Almond and Currie, 2011; Barker, 1995; Behrman and Rosenzweig, 2004; Black et al., 2007). Numerous studies have tested this hypothesis, primarily focused on transient shocks induced by environmental changes, weather patterns, or commodity price fluctuations.[6] Our study adds to this body of work by offering quasi-experimental evidence from the HRS reform, a permanent productivity shock resulting from a major institutional change. A recent study by Xu (2021) demonstrated that early-life exposure to the HRS reform significantly improved individuals' adult health, education, and labor

[6] Commonly studied early-life shocks include environmental regulations (Flynn and Marcus, 2023; Isen et al., 2017), weather shocks (Adhvaryu et al., 2018; Maccini and Yang, 2009; Shah and Steinberg, 2017), commodity price fluctuations (Adhvaryu et al., 2019; Miller and Urdinola, 2010), and nutritional and health interventions (Brown et al., 2019; Field et al., 2009).

market outcomes. While that study suggested that increased income during gestation due to the HRS reform may be a key driver of these long-term effects, it did not directly examine the birth endowment mechanism due to data limitations. The detailed birth history data from the IDFS allow us to complement the previous work by providing new evidence that the HRS reform can generate long-term positive effects by improving birth endowments. The granularity of our data, combined with county-specific variation in the reform rollout, adds further precision and credibility to our findings.

This paper proceeds as follows. The next section provides the background of HRS reform. Section 3 details the data sources and descriptive statistics. Section 4 outlines the empirical strategy and presents the main results. Section 5 explores the underlying mechanisms and heterogeneity, and the final section concludes.

2. The HRS reform

Prior to the HRS reform, China's agricultural production was organized under people's commune system, which was a three-layer hierarchy including commune, brigade, and production team, with the production team as the fundamental production unit. Under this system, all resources, including land, production tools and draft animals, were collectively owned (Lin, 1992; Xu, 2021). Farmers were obliged to work in production teams and were accredited with work points based on their working hours, rather than their actual labor input, as it was difficult and costly to monitor each member's labor efforts (Zweig, 1987).

This remuneration system in collective agricultural operation dampened farmers' enthusiasm for agricultural production and generated widespread problems of idle labor and free-riding among the production teams. Unsurprisingly, agricultural growth in China stagnated during this period (Lin, 1988). Some anecdotes suggest that the production teams relied heavily on national support for agricultural production and livelihoods, but this support was still insufficient to meet the subsistence level for farmers (Cui et al., 2020). Official data revealed that in 1978, the rural extreme poverty rate in China stood at 30.7 percent of the total rural population, with about 250 million people living in extreme poverty according to the rural poverty line at that time (China Agriculture Yearbook, 1981).[7]

The transformation began with the implementation of the Household Responsibility System (HRS) in 1978. Unlike many other land reforms enacted by governments, the HRS reform was first piloted by a small group of farmers in Xiaogang, a small village in Anhui Province. This trial was induced by a severe drought in 1978 that worsened the existing food scarcity (Xu, 2021). The farmers took a daring step and secretly allocated collective land, agricultural equipment, and output quotas to individual households, unbeknownst to the Central government. Despite the political risk, these pioneers insisted on delineating production responsibilities. A year later, agricultural yields in Xiaogang far surpassed those of the surrounding production teams. This success encouraged the county government to acknowledge this new system and relay Xiaogang's practices to Wan Li, the first secretary of the Anhui Provincial Party Committee at that time, who expressed approval of their innovative approach. In 1980, the Central Committee of the CCP issued an official document formally recognizing the HRS, prompting its rapid implementation across counties. By the end of 1984, the HRS system had been adopted by 98 percent of production teams and 97 percent of rural households in China (China Agriculture Yearbook, 1985).

The HRS reform fundamentally transformed the farming institution from the collective model to an individual-based system, inevitably leading to the collapse of the people's communes. The establishment of clearer and more secure land property rights due to the HRS reform had a profound impact on labor productivity and agricultural production. Under the HRS, previously collectively-owned farmland was distributed to individual households based on either household size or the number of laborers within

[7] In 1978, China set its initial rural poverty line at 100 *yuan* per person per year. This threshold equaled approximately 63.41 US dollars per person per year (1 US $ = 1.5771 *yuan* in 1978), and did not meet the international standard of 1 dollar per day.

the household, granting them the autonomy to make agricultural production decisions. Furthermore, households were granted secure tenure for an initial period of 15 years. While land ownership remained with the collective, households were given land use rights and residual claim rights over the output after fulfilling procurement obligations (Perkins, 1988). This property rights framework incentivized farmers to work hard on their contracted land, thereby producing a boom in agricultural production. Lin's (1990) seminal work revealed that the annual agricultural growth rate between 1978 and 1984 surpassed the growth rate observed from 1952 to 1978 by a factor of 2.7, and the implementation of HRS accounted for approximately half of the output growth during 1978-1984. In terms of grain, point estimates presented in Table A1 indicate a significant positive correlation between the HRS adoption rate and *per capita* grain output.

The HRS reform not only led to an agricultural boom, but also had positive effects on rural poverty alleviation. Despite the rural poverty line doubling, the extremely poor population declined significantly, from 250 million in 1978 to 128 million in 1984 (China Statistical Abstract, 2018). As for rural household income and food consumption, *per capita* rural net income surged from 134 *yuan* in 1978 to 355 *yuan* in 1984, while *per capita* food consumption expenditure among rural households doubled during the same period, rising from 78.6 *yuan* to 161.5 *yuan* (China Agricultural Yearbook, 1985). Given the substantial improvement in household income and nutritional intake during the HRS reform period, it would be natural to wonder how the HRS reform affected individuals' health, particularly for those exposed to the HRS early in life. The staggered rollout of the HRS reform across counties combined with the unique birth history data provide a unique opportunity to examine the effect of prenatal exposure to the HRS reform on birth outcomes.

3. Data and summary statistics

3.1 Birth weight

We utilize data from the 1985 and 1987 waves of the China In-Depth Fertility Survey (IDFS). The survey was conducted by the State Statistical Bureau of China, with technical support from the International Statistical Institute Research Center (ISIRC). This survey employed a stratified, multi-stage, clustered random sampling strategy to select the sampled localities: the first wave included two provinces (Hebei and Shanxi) and one municipality (Shanghai), while the second wave selected five provinces (Liaoning, Shandong, Guangdong, Guizhou, and Gansu) along with one municipality (Beijing). Figure 1 depicts the geographical distribution of all sampled counties, which encompasses a total of 174 counties across the eight localities.[8] Although the IDFS covered a limited number of provinces, it was considered geographically and demographically representative, encompassing both inland and coastal regions. The first and second waves of the survey represented populations of 93 million and 236 million, respectively (State Statistical Bureau of China, 1986, 1989). This wide coverage of various counties enables us to utilize the variations in the timing of the HRS rollout in our study.

The IDFS data was collected at three levels: community, household, and individual. For the individual survey, 13,307 and 39,210 ever-married women aged 15-49 years at the survey date were selected for the first and second waves, respectively. Based on the standard World Fertility Survey, the individual questionnaire covered six main topics for each woman: individual and family characteristics, marriage history, detailed birth and pregnancy history, knowledge and use of contraceptives, fertility preferences, and background information on the husband. Previous studies have rated the quality of the IDFS data as excellent, with response rates ranging from 95% to 100% in each of the

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[8] Due to a data confidentiality agreement, we do not have access to the IDFS data for Guangdong province.

sampled localities (International Institute of Statistics, 1991; Zhang, 1990).

A key advantage of the individual-level survey is its detailed record of pregnancy and birth histories for all eligible ever-married women. In this paper, we focus on birth weight as the key measure of birth outcome since it has been uniformly and consistently collected by IDFS. It is not only a widely recognized metric of neonatal health but also a strong predictor of adult outcomes, as demonstrated by various studies (Almond and Currie, 2011; Behrman and Rosenzweig, 2004; Royer, 2009). Other measures such as height or Apgar score are not available in the IDFS data.

In addition to birth weight, the birth history module also contains information on various demographic characteristics and early-life investment for all live children. These variables include each child's age (month and year of birth), gender, prenatal checks, place of delivery, whether delivered by a professional attendant, and breastfeeding duration. We determine a child's exposure to the HRS reform *in utero* by using the exact birth date and gestation period. Additionally, we obtain information regarding each live child's birth order and the number of older siblings, based on their birth dates.

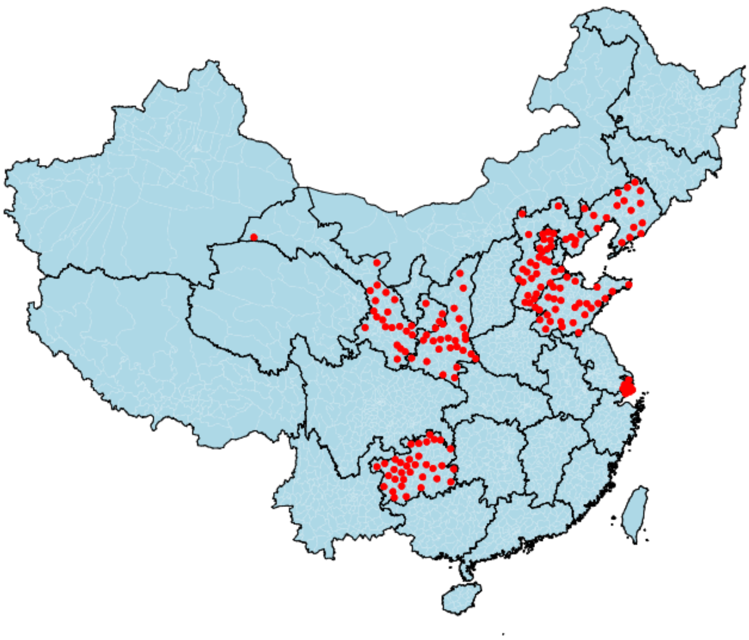

Figure 1. Geographical Distribution of IDFS Sample Counties

Notes: The red dots in the figure represent sample counties.

We establish our analysis sample through several steps. First, we retain only data for live births, since birth weight information is only available for surviving children. Second, to mitigate potential compositional changes among pregnant mothers, we restrict the sample to those who experienced pregnancies both before and after the introduction of the HRS reform so as to draw sibling comparisons. We further limit the sample to children living in rural areas at the time of the survey, since the HRS reform was exclusively implemented in rural regions.[9] Nonetheless, for placebo tests, we also utilize data from urban individuals. Finally, to account for potential confounding effects from other events, we narrow the sample to live births conceived within a window of seven years before or after the implementation of the HRS reform.[10] After applying these sample restrictions, our final dataset comprises 13,520 live births from 4,628 mothers.

3.2 The rollout of HRS and OCP across counties

While we have already restricted the sample to a relatively short event period, this restriction may not fully rule out the potential confounding effects of other policy changes that occurred during the HRS reform period. One noticeable reform concurrent with the HRS reform is the One Child Policy (OCP), which aimed to impose financial penalties for above-quota births (Banister, 1987). While implementation of the OCP in rural areas was milder than in urban regions, it can affect rural households' fertility behavior, which may lead to changes in the characteristics of births and potentially bias our estimates.

To address the potential confounding effect of OCP, we collect information on both the county rollout of the HRS reform and the OCP from county gazetteers. These

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[9] Because the IDFS does not ask the place of birth for each child and only records each mother's residence at the time of the survey, we classify births as rural according to the mother's residence. This approach is reasonable and should not affect our results, as rural-to-urban migration was strictly controlled under China's household registration system before the 1990s (Kinnan et al., 2018).

[10] We conduct sensitivity tests using alternative event periods restrictions, and the results presented in Figure A6 indicate that our main results are robust to the choice of different event periods.

gazetteers were compiled by local historians to record local history and were not used for political purposes. Therefore, they are considered reliable sources of information on the timing of various reforms in China (Almond et al., 2019).

In our study, we follow existing literature and define the initiation of the HRS reform as the year when collectively owned land was first contracted to individual households in each county. In a similar vein, the beginning of the OCP is marked as the year when the county policy document first mentioned the enforcement of explicit penalties for above-quota births. Figure A1 illustrates the annual progress of each reform by displaying the proportion of counties that had adopted either the HRS or the OCP. As depicted, the HRS reform was implemented between 1978 and 1984 in the sample counties, while the introduction of the OCP occurred from 1977 to 1983. While both reforms were expanded rapidly, the OCP had an earlier starting year and a faster rollout compared to the HRS reform. This timing pattern aligns with the observations made by Almond et al. (2019), indicating that there is adequate variation to distinguish the impacts of the two reforms.

3.3 Other variables

The HRS reform may have increased the opportunity costs of fetal care due to increased labor productivity, potentially altering maternal decisions about fetal care and thereby influencing birth outcomes. To explore the significance of this substitution effect, we collect information on the cotton area and yield one year before the HRS reform from the *Compilation of Cotton Statistics in China 1949-2000*. The substitution effect hypothesizes that since women hold comparative advantage in cotton production, the greater the significance of cotton production in a county, the higher mothers' opportunity costs will be (Liang et al., 2020; Xue, 2021). In addition, we obtain data on rural population and cultivated land from the *Summary of Rural Economic Statistics by*

County in China 1980 to calculate the *per capita* land endowment in each county.[11]

As discussed in the previous section, the rollout of the HRS reform could have been affected by the weather. The climatic variables are also strong proxies for agricultural productivity, labor market opportunities, income, and nutrition in rural settings—factors crucial to infant birth outcomes (Jayachandran, 2006; Kaur, 2019; Maccini and Dean, 2009; Shah and Steinberg, 2017). Therefore, we consider time-variant climatic variables in our analyses. Data on rainfall and temperature are drawn from the ERA5-Land Dataset, which is accessible at the Copernicus Climate Change Service (C3S) Climate Data Store (CDS). This dataset, available at a refined spatial resolution of 0.1° by 0.1°, covers the period from January 1950 to the present. We merge this annual climatic data with our county samples by averaging over grid points within each county.

Beyond OCP, another noticeable event during the HRS reform period is the rapid development of township and village enterprises (TVEs), which could also potentially influence rural household incomes and alter female labor market participation (Brandt et al., 2017). To mitigate the potential confounding effects of TVEs, we collect data on the number of TVEs from 1978 to 1987 at the province level, sourced from the *Statistics of Township Enterprises in China*, compiled by the Township Enterprise Bureau of the Ministry of Agriculture.[12]

In addition, considering that counties with different features could have different trends in birth outcomes, we incorporate additional variables to account for trends related to county characteristics. Data on average altitude and slope are derived from the ASTER GDEM data, which provides a global digital elevation model (DEM) at a

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[11] *Per capita* cultivated land before the land reform (such as *per capita* cultivated land in 1978) would be a more appropriate proxy for land endowment, but the earliest available data on cultivated land at the county level that we could obtain is from 1980. While using data from 1978 would have been ideal, it is reasonable to work with the 1980 data given that land endowment is unlikely to have changed rapidly during the initial years of the HRS reform.

[12] Since county-level data on the number of TVEs is only available for about 30 percent of counties during our study period, we construct this control variable for each province and year using data from the Statistics of Township Enterprises in China.

spatial resolution of 1 arc second (approximately 30-meter). Data on each county's population and *per capita* rural net income in 1978 are obtained from the *Compilation of Provincial Statistics*, which is compiled by the Statistical Bureau of each province.

3.4 Summary statistics

Table 1 presents summary statistics for live births in our sample. The outcome variables of interest are the birth weight and an indicator for low birth weight (defined as below 2,500g following the World Health Organization guidelines). Panel A of Table 1 shows that the mean birth weight is approximately 3,225 grams, and about 3.4 percent of births in our sample are classified as low birth weight.[13] In terms of early-life health investment, Panel A reveals that about 17.8 percent of children were delivered in a formal sector (such as a hospital or clinic), while only 6.4 percent of children had an attended delivery (delivered with the help of a doctor, nurse or midwife). Additionally, about 14 percent of children had prenatal checks. The average breastfeeding duration for all children was about 20 months.

Panel B reports the summary statistics for an array of county characteristics. The average maximum and minimum temperature for all birth years are 23.69 degrees Celsius and -1.57 degrees Celsius, respectively. The logarithm of annual rainfall is about 6.98. Roughly 26.5 percent of sample counties had cultivated cotton one year before the implementation of HRS. The average population in 1978 was about 0.42 million and the rural income level was very low then, with an average *per capita* rural

[13] It is worth noting that the reported incidence of low birth weight (LBW) in our study is lower than in other studies, which may be due to differences in geographical coverage and sample selection criteria. First, the IDFS data only covered six provinces and two municipalities (Beijing and Shanghai), which are, on average, more economically developed than other regions in China. Second, the use of mother fixed effects implies that only mothers who gave birth to at least two children during the sample period were included. This criterion led to the selection of mothers who typically had at least two births during our study period. Table A2 compares the characteristics of these selected mothers with those who did not meet this criterion. We find that selected mothers were generally younger, more educated, and from households with higher reported incomes compared to unselected mothers. Additionally, there could be measurement errors in self-reported birth weights due to recall bias and inaccuracies. However, we consider this concern minimal in our analysis, as most of our estimates are statistically significant at conventional levels. We thank an anonymous reviewer for highlighting this point.

net income of about 97 *yuan*.

Table 1. Summary statistics

VARIABLES	Mean	SD	N
Panel A: Birth outcomes			
Birth weight (grams)	3,225	570.4	13,520
Low birth weight (<2500 grams)	0.034	0.181	13,520
Birth order	2.781	1.686	13,520
Child's gender (1=male, 0=female)	0.493	0.500	13,520
Prenatal check (1=yes, 0=no)	0.139	0.346	10,293
Institutional delivery (1=yes, 0=no)	0.178	0.383	13,520
Attended delivery (1=yes, 0=no)	0.064	0.245	13,520
Breastfeeding duration (months)	20.46	12.16	13,520
Panel B. County characteristics			
Maximum temperature (deg. Celsius)	23.69	2.797	13,520
Minimum temperature (deg. Celsius)	-1.571	5.679	13,520
Rainfall (mm, logs)	6.978	0.544	13,520
Altitude (m, logs)	6.249	1.560	13,520
Slope (degree)	15.13	6.189	13,520
Distance to Beijing (km, logs)	7.021	0.890	13,520
Whether planted cotton in year t-1	0.265	0.442	13,520
Cotton land in year t-1 (ten thousand *Mu*)	2.181	5.621	13,520
Cotton yield in year t-1 (Jin/*Mu*)	11.26	27.12	13,121
Population in 1978 (ten thousand)	41.90	23.56	13,520
Rural net income *per capita* in 1978 (*Yuan*)	96.82	67.98	13,520

Notes: The table presents summary statistics for our main analysis sample. Information on prenatal checks was only collected in the 1987 wave of IDFS.

4. The effect of the HRS reform on birth weight

4.1 Empirical strategy

We leverage variations in HRS reform timing across counties and compare siblings born to the same mother, with siblings experiencing prenatal exposure to the HRS as the treatment group and siblings without such exposure as the control group. The estimation equation is the following:

$$Y_{imct} = \alpha + \beta HRS_{ct} + \gamma X_{imct} + \delta_m + \nu_t + \gamma_s + \varepsilon_{imct} \tag{1}$$

Here, Y_{imct} is the birth weight for child *i*, born to mother m, living in county c, with pregnancy start year *t*. The key explanatory variable, HRS_{ct}, is a binary variable that equals one if the HRS reform in county c was implemented before or during the pregnancy start year t.[14] X_{imct} is a vector of covariates including the child's gender and birth order, a dummy that equals one if the OCP initiated before or during the pregnancy-start year, as well as weather factors that could potentially affect both the HRS timing and the child's birth outcomes (Shah and Steinberg, 2017). Specifically, the weather variables encompass the minimum and maximum monthly temperatures, and annual volume of rainfall during the conception year (Chacón-Montalván et al., 2021).

Several sets of fixed effects are also included in the regression models. The first is mother fixed effects, δ_m, which absorb all time-invariant household and mother-specific determinants of birth outcomes. For instance, more educated mothers are better managers of health issues (Bharadwaj et al., 2020). The fixed effects at the mother level not only control for the mother's education and all other time-invariant characteristics, but can also avoid potential effects of compositional changes among mothers. The second set of fixed effects we include are pregnancy-start year fixed effects, ν_t, which account for any time-specific birth weight shocks that are common to the same cohort, such as other concurrent rural economic reforms that were implemented across the country within the same year. In certain specifications, we further incorporate pregnancy-start month fixed effects, γ_s, to address seasonal variations in unobservable factors correlated with birth outcomes. For instance, seasonal changes in the concentration of agrichemicals in water, due to crop growth cycles, can influence fetal health (Brainerd and Menon, 2014; Mateus et al., 2023). In all regression models, the standard errors, ε_{imct}, are clustered by county to account for within-county correlation over time.

[14] We define the treatment status of each child based on the year when the pregnancy began rather than the year of birth because a child's birth weight is largely determined by *in utero* conditions.

As discussed earlier, the timing of the HRS reform was not fully exogenous. For example, to minimize the political risk, an official document issued by the Central Committee of the CCP in 1980 emphasized that only remote and poor regions should be allowed to experiment with the HRS reform.[15] In addition, regions stricken by bad weather exhibited a more urgent need for reform and were also early adopters (Lin, 1987). If these characteristics are associated with differential trends in the outcome variables, the estimated coefficient of β could be biased. To examine which factors are predictors for the timing of the HRS reform, we regress the HRS initiation year on a range of pre-reform county characteristics. The results presented in Table A3 confirm that counties that had lower pre-reform *per capita* rural net income appear to have implemented the HRS reform earlier. The estimated coefficient for distance to Beijing is also consistent with the literature, showing that regions more distant from the capital tended to adopt the HRS reform earlier. However, when the province fixed effect is controlled, the coefficient for distance to Beijing becomes statistically insignificant. Additionally, the reform appears to have been adopted later in counties at higher altitudes when all pre-reform variables are included, regardless of whether the province fixed effects are included or not. Nevertheless, the pre-reform characteristics altogether only account for 37 percent of the variation in reform timing, suggesting that there are considerable idiosyncratic differences that can be exploited for identification purposes. To further address endogeneity concerns, we follow Hoynes et al. (2016) and control for the interactions between these pre-reform county characteristics and linear cohort trends in the regressions.

[15] In September 1980, the Central Committee of the CCP convened a symposium with the provincial first secretaries of the Party. The primary agenda item was to discuss how to strengthen and improve the agricultural production responsibility system. The meeting minutes, titled *Further Strengthening and Improving the Agricultural Production Responsibility System,* were issued after the symposium. They acknowledged and affirmed the diverse forms of agricultural production responsibility systems which had been established in various localities. Moreover, the minutes proposed that agricultural production could be contracted to households in remote mountainous areas and impoverished regions. More details are available at: http://www.scio.gov.cn/m/zhzc/6/2/Document/1011401/1011401.htm

The difference-in-differences approach relies on the parallel trend assumption that the difference in birth outcomes between siblings who were exposed to the HRS reform during gestation and those who were not would remain constant over time in the absence of the reform. In our context, the parallel trend assumption could be violated if higher birth-order children enjoy better birth outcomes in nature due to uterine advantages (for example, lower levels of intrauterine estrogen) or if the birth weight of children of different birth orders demonstrates different development trajectories due to intra-household resource allocations (Behrman, 1988). In additional analysis, we conduct an event study on the impact of the HRS reform to test the pre-trends. The empirical model is specified as follows:

$$Y_{imct} = \alpha + \sum_{k=-7}^{k=7} \beta_k \, HRS_{c,t_0+k} + \gamma X_{imct} + \delta_m + \nu_t + \gamma_s + \varepsilon_{imct} \quad (2)$$

HRS_{c,t_0+k} denotes a set of dummy variables indicating the years relative to the HRS initiation year t_0 in county c. Specifically, it equals one for children conceived k years following the adoption of HRS. We treat those conceived one year before the HRS reform (i.e., $k = -1$) as the reference category that is omitted in the model. Hence, β_k captures the differential effects between these two groups. All other variables are defined consistently with those in Equation 1.

4.2 Main results

Table 2 presents the estimated results of Equation 1, where the dependent variable is birth weight. In all columns, we control for mother fixed effects and pregnancy start-year fixed effects. In Columns 1 through 5, we gradually include additional controls: the child's demographic characteristics (gender and birth order), an indicator of the One Child Policy (OCP) reform, climatic variables (temperature and rainfall), and the number of Township and Village Enterprises (TVEs) *per capita*. Across all specifications, we observe consistently positive effects of prenatal exposure to the HRS reform on birth weight. Specifically, the estimate in Column 5 suggests that for siblings born to the same mother, prenatal exposure to HRS leads to a roughly 55-gram increase in birth weight, corresponding to 1.7 percent of the sample mean. This relationship remains robust even when controlling for pre-reform county characteristics interacted with linear cohort trends, as shown in Column 6. Although the inclusion of these trends reduces some variation and slightly lowers the point estimate, the coefficient remains statistically significant at the 5% level.[16]

Table 2. Effects of HRS Reform on Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)
HRS	61.80***	53.84**	53.91**	54.72***	54.57***	38.77**
	(21.96)	(20.85)	(20.88)	(20.68)	(20.58)	(18.57)
Gender		144.5***	144.4***	144.6***	144.6***	142.9***
		(10.39)	(10.39)	(10.37)	(10.39)	(10.39)
Birth order		-11.00	-10.99	-11.23	-11.21	-7.243
		(9.58)	(9.58)	(9.570)	(9.579)	(9.474)
OCP			1.913	3.030	3.114	4.855
			(14.49)	(14.40)	(14.30)	(14.10)
Mean of dep. var.	3225	3225	3225	3225	3225	3225
PS month FE	No	Yes	Yes	Yes	Yes	Yes
Weather controls	No	No	No	Yes	Yes	Yes
TVE controls	No	No	No	No	Yes	Yes
Linear trends	No	No	No	No	No	Yes
R^2	0.687	0.697	0.697	0.697	0.697	0.698
N	13,520	13,520	13,520	13,520	13,520	13,520

Notes: All regressions control for mother fixed effects and pregnancy start-year fixed effects. Weather controls include minimum and maximum temperature, and rainfall (in logs). TVE controls represent the number of TVEs *per capita*, calculated by dividing the annual number of TVEs by the rural population in 1978. Column 6 controls for pregnancy start year linear trends interacted with pre-reform county characteristics (log of population in 1978, log of rural net income *per capita* in 1978, log of average slope, log of average altitude of the county, and log of distance to Beijing). Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

The magnitude of these estimated effects is not only economically meaningful but

[16] We thank a reviewer for highlighting the broader changes brought about by the HRS reform, including the collapse of the people's communes. Given that the people's communes were structured to sustain a collective farming system with centralized production and allocation decisions, and the HRS reform aimed to shift farming from this collective model to an individual-based system, the collapse of the people's communes emerged as a natural outcome of the HRS reform. Therefore, we interpret our findings as capturing the overall impact of these transformations.

also aligns with findings from previous studies, underscoring the robustness and credibility of our results. Almond and Mazumder (2011), for instance, estimate a reduction of approximately 40 grams in birth weight for Arab infants due to Ramadan exposure during the initial two months of pregnancy. This reduction, corresponding to about 1.2% of the mean birth weight for Arab newborns, is fairly close to the effects documented in our study. Additionally, Flynn and Marcus (2023) estimate an 8-gram increase in average birth weight associated with reduced surface water pollution as a result of the Clean Water Act grants. Through sibling comparisons, Camacho (2008) documents an 8.7-gram reduction in birth weight for babies born to mothers exposed to landmine explosions during pregnancy. Our estimated effects are significantly larger in magnitude compared with these estimates. This discrepancy can be attributed to the distinct nature and intensity of the shocks being studied; the pollution shocks analyzed by Flynn and Marcus (2023) and Camacho (2008) are comparatively milder than the substantial socio-economic alterations induced by the land reform in our context.

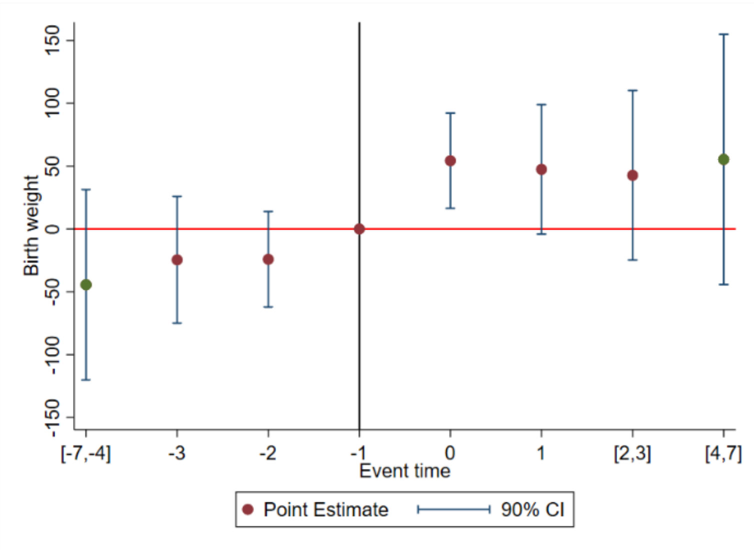

Figure 2. Event-Study Estimates for Impact of HRS on Birth Weight

Notes: This figure shows the event-study estimates from Equation 2. The model includes mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects, as well as extended controls (gender and birth order of each child, OCP, minimum and maximum temperature, log of rainfall, the number of TVEs *per capita*). The green endpoints on the left and right ends represent the average effects for periods 4-7 years before, and 4-7 years after the HRS rollout, respectively. Standard errors are clustered at the county level.

Figure 2 presents the event-study estimates for the impact of HRS on birth weight, using the model specified in Equation 2. This illustration yields several important observations. First, it confirms a notable increase in birth weight for children who experienced prenatal exposure to the HRS. Specifically, those conceived during the reform year experienced a sharp increase in birth weight, an effect that remains consistent and stable among subsequent cohorts. Second, the pre-reform trends are parallel, with the level difference in effect size prior to the reform being approximately zero. Finally, the effect on birth weight manifests concurrent with the implementation of the reform. These results help validate the difference-in-differences strategy we employ, lending further credibility to our previous findings.

Additionally, we follow the WHO guidelines and construct a dummy for the low birth weight that equals one if the birth weight is below 2,500 grams. We use this dummy as the dependent variable and replicate the previous analysis. The estimated coefficients reported in Table A4 do show a consistently negative impact across models, suggesting a 1 percentage point reduction in low birth weight. However, the estimates are not statistically significant. The event-study estimates depicted in Figure A2 also reveal a negative but statistically insignificant effect on the incidence of low birth weight. In addition to the underrepresentation of low birth weight infants in our sample, the non-uniform impact of the HRS reform across different socioeconomic groups may also contribute to the absence of a significant overall effect on LBW incidence. The heterogeneity analysis based on maternal education presented in Table A5 confirms our hypothesis: the HRS reform significantly reduced the probability of low birth weight among more educated mothers (Columns 3-4), while the impact was statistically insignificant among less educated mothers (Columns 1-2), despite a consistently negative coefficient.

4.3 Robustness checks

The results so far show that the average birth weight of siblings exposed to HRS *in* *utero* is significantly higher than that of siblings who were not exposed. In this section, we perform several robustness checks to address potential factors that could confound the estimated effects or the interpretations.

Endogenous fertility responses. Although we try to alleviate concerns about potential compositional changes in reproductive mothers by controlling mother fixed effects, our results may be still subject to selection bias as a result of changes in fertility desires after the HRS rollout, even for the same mother. Specifically, if the reform led to an increase or decrease in births, and these marginal children were either healthier or less healthy, our estimates could be biased upwards or downwards. Additionally, there might be potential shifts in the gender composition of newborns because males are expected to gain higher productivity and wages than females (Almond et al., 2019).

To address this concern, we first utilize data from China's 1990 Census one-percent sample to examine whether there are significant changes in fertility behaviors and desires following the HRS reform. We limit the analysis to the same set of counties used in our main regressions and calculate the number of births by county and year. Additionally, we calculate the sex ratio at birth as another dimension of potential fertility shifts. The results are reported in Table A6. Columns 1-2 display the estimated effects of HRS on birth cohort size, which are small in size and statistically indistinguishable from zero, suggesting that endogenous fertility behavior following the HRS rollout was unlikely to significantly bias our estimates.[17] Columns 3-4 detail the effects of HRS on the sex ratio at birth, where we observed a slight decrease in female births relative to male births following the HRS reform. However, this finding would not affect our main conclusions, as we have controlled for the gender of each birth in our analyses.

Furthermore, recognizing that county-level analysis may not fully capture micro-

[17] Figure A3 depicts the event-study estimates for the impact of HRS reform on birth cohort size, showing a stable pattern with no significant effects across the post-treatment periods. This aligns with results presented in Table A6 and supports our notion that endogenous fertility behavior is not a major concern in our study context.

level confounders, we construct a mother-year panel using our IDFS sample to examine whether the probability of pregnancy or giving birth changed significantly for specific mothers after the HRS implementation. Consistent with our primary analysis, we applied a time window restricted to seven years before and after the HRS implementation, with the dependent variable being a dummy indicating whether a mother had a pregnancy or gave birth in a given year. Reassuringly, the estimated results presented in Table A7 indicate no significant fertility response to the HRS reform, supporting the robustness of our initial findings.[18]

Placebo test using an urban sample. Given that the HRS reform was exclusively implemented in rural areas, we restrict our sample to mothers residing in rural areas for the main analysis. In this exercise, we conduct placebo tests using the urban sample to corroborate our estimation method. Since individuals born in urban areas were not directly impacted by the reform, we anticipate a lack of similar effects as observed in the rural sample. If, however, our previous findings are driven by county-specific trends or shocks, the birth weight of the urban children would also be affected. As shown in Figure A5, the estimated effects for the "placebo" urban sample are generally negligible and statistically insignificant. This supports the notion that our initial findings are attributable to the land reform shock, which uniquely affects households in rural areas.

Alternative estimators. We examine whether our main results in Table 2 are robust to alternative estimators using the method of Sun and Abraham (2021), which accounts for the potential contamination of heterogeneous treatment effects across periods and groups when adopting two-way fixed effect models. The estimated results shown in Table A8 are significantly positive and remain stable in magnitude across all specifications. In addition, the estimators under Sun and Abraham (2021)'s method are comparable to those presented in Table 2. This mitigates concerns regarding potential estimation bias that may arise due to the staggered rollout of the HRS reform.

[18] Figure A4 displays the event-study estimates of maternal fertility behavior, indicating no significant changes or trends in birth or pregnancy rates across all post-treatment periods.

Sensitivity to alternative event periods. In our primary analysis, we restrict the sample to a window of cohorts born 7 years before and after the reform. This restriction is meant to allow sufficient statistical power while minimizing potential confounding effects from parallel policies. Here, we assess the sensitivity of our findings to alternative event periods. Figure A6 plots the point estimates and confidence intervals obtained from different sample frames. Reassuringly, we observe that the estimated results are consistent across different event periods. The estimated effects are closely aligned both in terms of magnitude and significance level, underscoring the robustness of our initial findings.

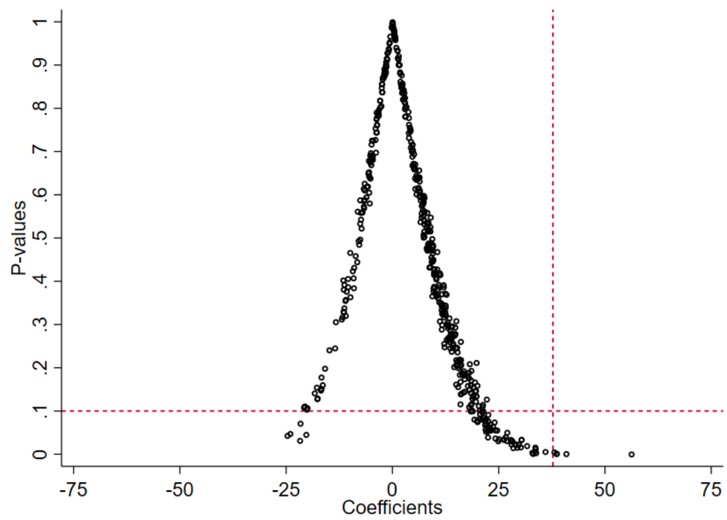

Figure 3. Placebo Test Using Randomly Assigned HRS Rollout Year

Notes: This figure depicts the estimated coefficients and corresponding p-values from estimating the effect of the "placebo" year of the HRS reform on birth weight. We randomly select a year from the period spanning 1970 to 1986 as the "placebo" reform year for each county and repeat this random sampling procedure 500 times. The vertical red dashed line represents the true estimated coefficient in Column 5 of Table 2. Standard errors are clustered at the county level.

Placebo test using randomly assigned HRS implementation year. To further ensure that our main results are not driven by unobserved confounders, we conduct a placebo test by randomly assigning an HRS reform year to each county. Specifically, we randomly select a "placebo" year from 1970 to 1986 as the HRS implementation year for each county, and repeat this random sampling procedure 500 times. The estimation specification employed for this placebo test is identical to that used in

Column 5 of Table 2. The results are visualized in Figure 3, which displays a scatterplot illustrating that the majority of the estimated coefficients are concentrated around the zero value and significantly skewed to the left of the true estimated coefficient. Furthermore, most of the p-values are greater than 0.1, indicating that most of the estimated coefficients obtained under the "placebo" years are statistically insignificant. This placebo test further alleviates the concerns regarding unobserved confounders and strengthens the robustness of our main results.

5. Mechanism and heterogeneity analysis

After establishing the causal effect of prenatal exposure to HRS on birth weight, we now explore the mechanisms through which HRS exposure impacts birth weight. First, the substitution effect: the HRS reform might reduce fetal investment as the value of maternal time increases, raising the opportunity costs of fetal care (Miller and Urdinola, 2010).[19] Second, the income effect: the implementation of the HRS led to substantial increases in grain output and household income. This increase could have two significant effects on birth weight: improved nutritional intake for pregnant women, leading to better birth outcomes (Bharadwaj et al., 2020), and increased demand for prenatal healthcare. Third, the maternity leave mechanism: by granting individual households greater autonomy in agricultural production decisions, the HRS reform might provide women with more flexibility to take leave from agricultural work during pregnancy, potentially improving birth outcomes. In this section, we present evidence suggesting that any potential adverse effects on birth weight due to the substitution effect are offset by the positive income effect, with improved nutrition intake likely being the primary mechanism behind our results.

[19] It is plausible that the substitution effect might also operate through the quantity–quality (Q–Q) tradeoff (Becker and Lewis, 1973). Specifically, if households' fertility increased as a result of the HRS reform, it could lead to decreased resources allocated to each pregnancy, thereby reducing the birth weight for those born after the reform. However, the results in Tables A6 and A7 indicate no significant changes in fertility behavior following the HRS rollout, suggesting that the Q-Q tradeoff is unlikely to be a primary mechanism behind our results. We thank an anonymous reviewer for raising this point.

5.1 Substitution effect

An extensive body of literature has demonstrated that productivity and income shocks can influence the opportunity cost of human capital investment (Miller and Urdinola, 2010; Shah and Steinberg, 2017; Xu and Adhvaryu, 2024). While the HRS reform incentivized individual households to increase agricultural productivity, it also likely increased the opportunity cost of fetal care for women, particularly in regions where women had a comparative advantage in agricultural practices. To test this substitution effect, we compare the impact of the HRS reform in counties well-suited for growing cotton to that in counties less suitable. Specifically, we interact the HRS treatment variable with three measures of county cotton production in the year prior to the HRS reform in our main regressions: a dummy variable indicating whether cotton was cultivated in the county, the average cotton yield, and the proportion of land allocated to cotton cultivation. We focus on cotton because it is a significant crop in China, for which women have distinct comparative advantages over men, particularly in tasks like cotton picking and spinning (Liang et al., 2020). Consequently, in counties where cotton played a more critical economic role, the value of women's time would have increased disproportionately following the HRS reform. If the substitution effect were substantial and led to a reduced allocation of time to prenatal care, we would expect a smaller positive impact of the HRS reform on birth weight in cotton-producing counties.

Contrary to the increased opportunity costs hypothesis, which anticipates less pronounced effects in regions where women have a comparative advantage in production, the results from Columns 1-3 of Table 3 suggest otherwise. The effects of the HRS reform on birth weight are more pronounced for those born in counties that cultivated cotton, achieved higher cotton yields, and allocated more land for cotton cultivation. The stronger effects in these cotton counties likely stem from the positive impacts of increased income and improved relative economic status for women in these areas. This is consistent with previous research indicating that women's income and economic status play a critical role in shaping children's human capital (Akee et al., 2010; Qian, 2008). As the value of women's labor in cotton production increases, their intra-household decision-making power may also be enhanced, potentially increasing family support and resource allocation during pregnancy, thereby improving newborns' health outcomes (Chiappori and Mazzocco, 2017; Doepke and Tertilt, 2018; Majlesi, 2016; Qian, 2008).

Table 3. Mechanism Analysis – Substitution vs. Income Effect

	(1)	(2)	(3)	(4)	(5)
HRS	35.94**	63.35***	51.28***	83.35***	28.09
	(17.85)	(21.58)	(19.30)	(22.70)	(22.33)
HRS × Cotton	47.48***				
	(18.10)				
HRS × Cotton Yield		6.461***			
		(2.301)			
HRS × Cotton Area			2.986***		
			(1.146)		
HRS × Older Siblings				-31.54***	
				(7.987)	
HRS × Land					45.46***
					(14.92)
Mean of dep. var.	3,225	3,225	3,225	3,225	3,225
R^2	0.698	0.699	0.698	0.698	0.698
N	13,520	13,121	13,520	13,520	13,413

Notes: All regressions control for mother fixed effects, pregnancy start-year fixed effects, pregnancy start-month fixed effects and covariates. The covariates include gender and birth order of each birth, an indicator of OCP, maximum and minimum temperature, log of rainfall and the number of TVEs *per capita*. Robust standard errors clustered at the county level are in parenthesis (***p < 0.01, **p < 0.05, *p < 0.1).

5.2 Income effect – Nutritional intake

Since household or individual income data during each pregnancy is not available in the IDFS dataset, we conduct heterogeneity analyses using several proxies for household resources to shed light on the income mechanism. First, we use the number of older siblings for each child as heterogeneity variable and interact it with the HRS treatment indicator. The rationale is that with the reform-induced increase in household resources, the more children there are in a family, the fewer reform-induced resources are available per child. Therefore, if increased income significantly drives improvements in birth outcomes, we would expect the effects to be more substantial for newborns with fewer older siblings. The estimated results presented in Column 4 of Table 3 align with this hypothesis: the coefficient of the interaction term is significantly negative. This indicates that the impact of the HRS reform on birth weight is more pronounced for newborns with fewer older siblings, who are more likely to experience greater resource increases.

Furthermore, if the income mechanism is dominant, we should anticipate the effect of HRS on birth weight to be more pronounced for counties with more arable land. This hypothesis stems from the fact that counties with higher *per capita* arable land would witness greater improvements in output and income following the HRS reform. To test this, we collect county-level data on *per capita* cultivated land area in 1980 and interact it with the HRS treatment variable. Reassuringly, the estimated coefficient on the interaction term in column 5 of Table 3 supports the significance of the income mechanism: the greater the *per capita* cultivated land in a county, the stronger the effects of the HRS reform on birth weight.

To provide more direct evidence of the income effect, we also collect provincial-level data on *per capita* grain output and rural net income from provincial statistical yearbooks to examine how these variables were affected by the HRS adoption rate. The estimated results, presented in Table A1, demonstrate a significant positive association between the HRS adoption rate and household resources. Specifically, full adoption of the HRS in a province leads to a significant increase of 19.4-21.7 percent in *per capita* grain output (Columns 1-2), and 20.7-24.5 percent in *per capita* rural net income (Columns 3-4). Moreover, the results in Columns 5-6 reveal a significant positive relationship between HRS adoption and *per capita* rural food consumption, indicating that nutrition intake indeed increased in rural areas following the HRS rollout. Taken together, these findings support the notion that the income effect, particularly increased nutrition intake, was likely the dominant mechanism through which the HRS influenced birth weight.

5.3 Income effect - Access to healthcare

The literature indicates that access to healthcare during pregnancy is critical to shaping birth outcomes (Almond et al., 2010; Guarin et al., 2021). While an increase in income due to HRS could improve prenatal nutrition intake, it could also provide pregnant women with better access to prenatal care. To distinguish these channels, we utilize IDFS data on prenatal checks and delivery behaviors for each pregnancy to investigate the impact of the HRS reform on healthcare accessibility. The results are presented in Table 4. Surprisingly, the estimates in Columns 1-2 indicate a significant *decline* in the probability of prenatal checks for children exposed to the HRS reform *in utero*.[20] The estimated effects of the HRS reform on the utilization of healthcare services during delivery are presented in Columns 3-6. The findings suggest that while the probability of attended deliveries is not significantly affected, there is a significant decrease in the likelihood of institutional deliveries after the HRS rollout. As such, the observed birth weight improvements is unlikely to be attributed to better access to medical services, indicating that improved nutrition intake is more likely the primary mechanism behind our main findings.

We interpret these findings as indicative of an adverse effect of the HRS reform on the rural cooperative medical system (CMS). Initially, the CMS was organized under the collective economy framework, which allocated a portion of collective revenue for a health and welfare fund. This fund was used to recruit local healthcare workers (commonly known as "barefoot doctors") and to deliver basic healthcare services (Chen et al., 1993). Therefore, the function of the CMS in rural areas heavily relied on financial support from the collective farming system. However, the HRS reform shifted

[20] Figure A7 illustrates the event-study estimates of the effects of the HRS reform on the probability of prenatal checks. There is a notable decline in the probability of prenatal checks for children conceived during the HRS adoption year. However, the estimates become less precise in subsequent periods, which is likely due to the smaller number of observations in these later periods.

the framework by granting individual households residual claim rights to agricultural outputs, incentivizing them to increase agricultural productivity (Perkins, 1988). This shift not only undermined the collective's ability to finance healthcare through communal funds but also diverted healthcare workers' time from public health duties to private agricultural activities. Consequently, participation in the CMS sharply declined, dropping from 90 percent in rural villages to just 10 percent during the 1980s (Chen et al., 1993; Wagstaff et al., 2009).

Table 4. Effects of the HRS Reform on Access to Healthcare

	Prenatal Check		Attended Delivery		Institutional Delivery	
	(1)	(2)	(3)	(4)	(5)	(6)
HRS	-0.045***	-0.041***	-0.015	-0.008	-0.032***	-0.029***
	(0.016)	(0.015)	(0.012)	(0.012)	(0.011)	(0.010)
Mean of dep. var.	0.139	0.139	0.178	0.178	0.064	0.064
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.765	0.765	0.806	0.806	0.691	0.692
N	10,293	10,293	13,522	13,522	13,524	13,524

Notes: The dependent variable in Columns 1-2 is an indicator for ever having a prenatal check, the dependent variable in Columns 3-4 is whether the birth had attended delivery (doctor, nurse, midwife), the dependent variable in Columns 5-6 is whether the birth was delivered in a formal institution (hospital or clinic). The prenatal check data was only collected in the 1987 wave of IDFS. All regressions control for mother fixed effects, pregnancy start-year fixed effects, and pregnancy start-month fixed effects. Even columns additionally control for covariates, including gender and birth order of each birth, an indicator of OCP, maximum and minimum temperature, rainfall (in logs), and the number of TVEs *per capita*. Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

The above findings also speak to existing literature that highlights the exacerbated health inequalities in China following the transition to a more market-oriented economy in 1978. Zhang and Kanbur (2005) documented a widened health gap, particularly evident in infant mortality rates between urban and rural areas. Liu et al. (1999) reported a decline in healthcare utilization among rural residents from the mid-1980s to 1993. However, to the best of our knowledge, no studies have specifically examined the underlying causes for the breakdown of China's rural cooperative medical system in the post-Mao period. Our paper contributes to this body of literature by providing quantitative evidence that the HRS reform yielded short-term adverse effects on both prenatal and delivery healthcare utilization among rural women.

5.4 Maternity leave hypothesis

During the era of the people's commune system, the production decisions were highly centralized and pregnant women were not easily excused from collective agricultural production. In contrast, the HRS reform afforded households more autonomy in production decisions, potentially enabling pregnant women to reduce their workload or take leave from agricultural work more easily. This flexibility serves as a kind of maternal leave mechanism, which is shown to have positive impacts on birth outcomes (Rossin, 2011).

Table 5. Mechanism Analysis - Maternity Leave Hypothesis

	(1)	(2)	(3)	(4)
HRS	49.78**	49.83**	50.86**	50.62**
	(24.44)	(24.48)	(24.24)	(24.13)
HRS × Number of female laborers	3.027	3.038	2.903	2.972
	(10.76)	(10.76)	(10.79)	(10.79)
Mean of dep. var.	3225	3225	3225	3225
OCP	No	Yes	Yes	Yes
Climate controls	No	No	Yes	Yes
TVE controls	No	No	No	Yes
R^2	0.697	0.697	0.697	0.697
N	13,520	13,520	13,520	13,520

Notes: The number of female laborers within a household is calculated as the total number of working-age female adults aged 15 to 64. All regressions control for mother fixed effects, pregnancy start-year fixed effects, pregnancy start-month fixed effects, gender and birth order of each birth. Column 2 further incorporates an indicator of OCP, column 3 further includes climate variables (minimum and maximum temperature, log of rainfall), and column 4 further controls for the number of TVEs *per capita*. Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

To investigate the potential role of increased maternity leave in explaining our main findings, we conduct a heterogeneity analysis that focuses on the presence of potential substitute female laborers within the household. The rationale is that if there are other working-age women within the household, it may be more feasible for the pregnant mother to take leave from agricultural work during pregnancy, potentially amplifying the reform's impact on birth outcomes. Specifically, we calculate the number of female laborers aged 15-64 within each mother's household and interact this variable with the HRS treatment indicator in our main regression models. The estimated results are reported in Table 5. While the coefficients of this interaction term are positive across all specifications, indicating more pronounced effects in households with more potential substitute laborers for the pregnant mother, these coefficients are not statistically significant. Hence, these findings are considered suggestive and align with the maternity leave hypothesis but do not provide definitive evidence.

5.5 Additional heterogeneity analyses

The HRS reform could potentially reshape households' gender preferences. For example, households may have stronger preferences for sons over daughters due to the belief that male labor productivity would disproportionally improve after the HRS reform (Almond et al., 2019). If this were the case, households might allocate more resources to male fetuses once the gender is revealed, leading to more pronounced effects of the reform on the birth weight among male children.[21] We explore this possibility by interacting the treatment variable with a gender dummy. However, as indicated in Column 1 of Table 6, the analysis reveals no significant disparity in the average birth weight between male and female children who were exposed to HRS *in utero*. This finding points towards the absence of a notable gender bias in the allocation of intra-household resources, at least during the fetal stage. This is likely attributable to the limited capacity of rural households to determine the gender of a child before birth.

Previous studies have also highlighted the positive relationship between education and agricultural productivity following the HRS reform (Yang and An, 2002). If this holds, the effects of the HRS reform could be more pronounced for children born to

[21] There has been a significant increase in the availability of ultrasound technology in provincial capitals since the 1970s. Almond et al. (2019) provide evidence that the effect of the HRS reform on sex ratio is more pronounced among households that have access to ultrasound, suggesting that gender discrimination is possible if they get access to ultrasound technology.

higher educated mothers. To investigate this heterogeneity, we interact the treatment variable with two education measures: the mother's years of schooling and an indicator for higher education. The estimated coefficients for the interaction terms in Columns 2 and 3 are both significantly positive, implying that the effects of the HRS reform are more pronounced for children born to more educated mothers. More educated women likely experienced greater productivity enhancements due to the reform, which in turn strengthened their economic status and increased family support during pregnancy. These results are consistent with the more substantial effects observed for infants born in counties with better cotton cultivation endowments, as discussed in Section 5.1. Furthermore, these findings further challenge the opportunity cost mechanism, suggesting that increased productivity post-land reform does not necessarily elevate the opportunity costs of women's time or crowd out the amount of prenatal care received by mothers.

Table 6. Additional Heterogeneity Analyses

	(1)	(2)	(3)
HRS	45.90**	34.72*	38.51*
	(19.85)	(19.71)	(20.00)
HRS × Male	17.16		
	(15.66)		
HRS × Years of Schooling		4.828***	
		(1.847)	
HRS × Higher Education			31.59**
			(15.09)
Mean of dep. var.	3,225	3,225	3,225
R^2	0.698	0.698	0.698
N	13,520	13,520	13,520

Notes: All regressions control for mother fixed effects, pregnancy start-year fixed effects, pregnancy start-month fixed effects and covariates. The covariates include gender and birth order of each birth, an indicator of OCP, maximum and minimum temperature, log of rainfall, and the number of TVEs *per capita*. Robust standard errors clustered at the county level are in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

5.6 Effects on post-natal investment

In this subsection, we take advantage of the unique breastfeeding data from IDFS to further explore whether post-natal parental investment responds to children's initial health endowments at birth. There are competing theories to consider. On one hand, an improved health endowment might yield a higher rate of return on post-natal investments, given that the initial health endowments are a key input for future human capital development (Bharadwaj et al., 2018; Cunha and Heckman, 2007). Under this framework, mothers might increase breastfeeding durations for children exposed to the reform during gestation. Conversely, if parents prioritize equity among siblings, they might reduce breastfeeding duration for those with superior health endowments.

The results reported in Table A9 show that the coefficients for the HRS treatment variable are consistently positive and statistically significant across all specifications, indicating that siblings exposed to the HRS reform *in utero* are breastfed for longer durations. When accounting for age-in-month fixed effects and linear trends adjusted for pre-reform county characteristics, we observe that siblings exposed to HRS *in utero* experience an increase in breastfeeding duration of approximately 4.8 months. The magnitude of this impact is sizeable, corresponding to roughly 23.7 percent of the sample's mean duration. The lack of a pre-trend and the discrete jump in breastfeeding duration concurrent with the HRS rollout, as depicted in Figure A8, further corroborate the validity of these results.

These findings are consistent with Adhvaryu and Nyshadham (2016), pointing to a complementary relationship between post-natal investment and initial health endowments. Furthermore, the extended duration of breastfeeding may also indicate that mothers have greater flexibility in managing their labor commitments following the HRS rollout. This aligns with our discussion around the maternity leave mechanism in Section 5.4, where we note that in households with more substitutable female adult laborers, the impact of HRS reform on birth weights of infants tend to be greater. Additionally, extended breastfeeding duration is more feasible for mothers in households experiencing greater income growth (Jayachandran and Kuziemko, 2011), consistent with the income effects that we explored in Section 5.2.

6. Conclusions

This paper investigates the impact of prenatal exposure to China's HRS reform on birth weight outcomes, utilizing variation in HRS rollout timing across counties. To address potential biases from compositional changes in pregnant mothers, we compare siblings born to the same mother, with those exposed to HRS *in utero* as the treatment group. Our findings reveal a significant increase in average birth weight among treated siblings, indicating that the income effect outweighs any substitution effect. Further analysis suggests that these improvements in birth weight are likely driven by enhanced nutritional intake during pregnancy. Additionally, the effect of prenatal exposure to HRS is more pronounced for children born to more educated mothers, with no significant differences across gender.

Our estimates indicate substantial benefits both in the short and long term. There is a consensus in the literature that even modest increases in birth weight can significantly reduce infant mortality rates. For example, Almond et al. (2005) report that a 100-gram increase in birth weight reduces infant mortality by 2.22 deaths per 1,000 live births within the first year. Based on our findings, the HRS reform, which increased average birth weight by about 55 grams, would translate to a reduction of 1.22 deaths per 1,000 live births. Supporting this, Royer (2009) finds that a 100-gram increase in birth weight leads to a 3.3 percent decrease in the one-year mortality rate, implying that the HRS reform could reduce the infant mortality rate within the first year by approximately 1.8 percent.

Moreover, previous studies suggest that increased birth weight leads to persistent health improvements, consistent with the fetal origins hypothesis (Barker, 1995). For instance, Royer (2009) examines the long-term health effects of increased birth weight using twin data from California birth records between 1960 and 1982 and finds that a 100-gram increase in birth weight results in a 10% reduction in hypertension and a 4.5% reduction in pregnancy complications. Applying these findings to our context, the HRS reform would generate substantial long-term health benefits, reducing the incidence of hypertension and pregnancy complications by 5.5% and 2.5%, respectively.

These findings have important implications for anti-poverty policies in the developing world. In recent decades, programs and interventions focused on improving land property rights have gained popularity as effective means of poverty reduction. While extensive literature has examined the impacts of such initiatives on agricultural production, household income, and economic growth, less is known about their effects on subsequent generations. Our study, using the successful case of the HRS reform, highlights the positive effects of improved land property rights on individuals' health endowments at birth. This is crucial for a comprehensive understanding of such programs' roles in poverty reduction and for identifying effective strategies to break the cycle of poverty from birth.

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Appendix

Table A1. Effects on *per capita* Grain Output, Rural Net Income, and Rural Food Consumption

	Log (*per capita* Grain Output)		Log (*per capita* Rural Net Income)		Log (*per capita* Rural Food Consumption)	
	(1)	(2)	(3)	(4)	(5)	(6)
HRS Adoption Rate	0.217***	0.194**	0.207***	0.245***	0.071**	0.082**
	(0.069)	(0.081)	(0.061)	(0.057)	(0.030)	(0.037)
Mean of dep. var.	6.042	6.042	5.427	5.427	4.741	4.741
Provincial linear trends	No	Yes	No	Yes	No	Yes
R^2	0.931	0.938	0.968	0.974	0.963	0.969
N	190	190	190	190	190	190

Notes: The data is limited to the years 1978 to 1984. All regressions control for province and year fixed effects. Even columns further control for linear trends interacted with baseline provincial characteristics (whether a coastal province, log of the total value of agricultural output, log of the total agricultural population, log of the total power of agricultural machinery, log of total sown area, the share of the agricultural sector, the share of the industry sector and the share of the service sector in 1978). Robust standard errors clustered at the province level appear in parentheses (***p<0.01, **p<0.05, *p<0.1).

Table A2. Comparisons of the characteristics of selected and unselected mothers

	Selected	Unselected	Difference
	(1)	(2)	(3)=(1)-(2)
Age at the survey	33.37	38.42	-5.051***
	(4.797)	(4.395)	(0.140)
Age at first pregnancy	21.81	20.47	1.344***
	(2.898)	(2.665)	(0.087)
Husband's age at the survey	35.96	41.42	-5.457***
	(5.549)	(5.329)	(0.164)
Years of schooling	2.892	1.998	0.894***
	(4.054)	(3.366)	(0.116)
Marital status	0.992	0.990	0.003
	(0.088)	(0.100)	(0.003)
Household income	908.3	769.4	138.9***
	(1732)	(828.6)	(46.49)
Per capita household income	176.9	125.0	51.83***
	(312.8)	(133.1)	(8.333)
N	4,628	1,491	

Notes: The sample is restricted to rural samples. Standard deviations (columns 1 and 2) and standard errors (column 3) are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A3. Determinants of the Timing of HRS Rollout

	HRS Start Year						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log Population	0.093					0.076	0.026
	(0.144)					(0.151)	(0.147)
log Income p.c.		0.356**				0.274***	0.316**
		(0.149)				(0.077)	(0.094)
log Slope			-0.166			-0.172	-0.303
			(0.122)			(0.165)	(0.201)
log Altitude				-0.065		0.194**	0.256***
				(0.043)		(0.072)	(0.060)
log Dist. to BJ					-0.586***	-0.683***	-0.055
					(0.085)	(0.081)	(0.181)
Province FE	No	No	No	No	No	No	Yes
R^2	0.003	0.037	0.011	0.013	0.260	0.307	0.377
N	178	178	178	178	178	178	178

Notes: The dependent variable in this table is the start year of the HRS reform in each county. Robust standard errors clustered at the province level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A4. Effects of HRS Reform on Low Birth Weight

	(1)	(2)	(3)	(4)	(5)	(6)
HRS	-0.009	-0.009	-0.009	-0.010	-0.010	-0.009
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Gender		-0.014***	-0.014***	-0.014***	-0.014***	-0.014***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Birth order		0.005	0.005	0.005	0.005	0.004
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
OCP			-0.007	-0.007	-0.007	-0.007
			(0.005)	(0.005)	(0.005)	(0.005)
Mean of dep. var.	0.034	0.034	0.034	0.034	0.034	0.034
PS month FE	No	Yes	Yes	Yes	Yes	Yes
Weather controls	No	No	No	Yes	Yes	Yes
TVE controls	No	No	No	No	Yes	Yes
Linear trends	No	No	No	No	No	Yes
R^2	0.535	0.536	0.536	0.536	0.536	0.537
N	13,520	13,520	13,520	13,520	13,520	13,520

Notes: All regressions control for mother fixed effects and pregnancy start-year fixed effects. Weather controls include minimum and maximum temperature, and rainfall (in logs). TVE controls represent the number of TVEs *per capita*, calculated by dividing the annual number of TVEs by the rural population in 1978. Column 6 controls for pregnancy start year linear trends interacted with pre-reform county characteristics (log of population in 1978, log of rural net income *per capita* in 1978, log of average slope, log of average altitude of the county, and log of distance to Beijing). Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A5. Heterogeneous Effects on Low Birth Weight by Maternal Education

	Less educated mothers		More educated mothers	
	(1)	(2)	(3)	(4)
HRS	-0.007	-0.007	-0.021*	-0.020*
	(0.009)	(0.009)	(0.012)	(0.012)
Mean of dep. var.	0.039	0.039	0.025	0.025
Weather controls	No	Yes	No	Yes
TVE controls	No	Yes	No	Yes
R^2	0.562	0.562	0.470	0.470
N	8,589	8,589	4,931	4,931

Notes: All regressions control for mother fixed effects, pregnancy start-year fixed effects, pregnancy start-month fixed effects, gender and birth order of each birth, and an indicator of OCP. Even columns further incorporate weather and TVE controls, including the maximum and minimum temperature, rainfall in logs, and the number of TVEs *per capita*. Robust standard errors clustered at the county level are in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A6. Effects of HRS on Fertility Selection: County-level Analysis

	Log(Birth cohort size)		Sex ratio at birth	
	(1)	(2)	(3)	(4)
HRS	-0.007	-0.015	-0.023	-0.020
	(0.025)	(0.025)	(0.032)	(0.032)
OCP	0.033	0.035	-0.004	-0.006
	(0.025)	(0.025)	(0.033)	(0.033)
Mean of dep. var.	4.246	4.246	0.969	0.969
County-specific linear trends	No	Yes	No	Yes
R^2	0.895	0.906	0.107	0.110
N	2,391	2,391	2,390	2,390

Notes: Data used in this table is from 1990 Census data. Birth cohort size is calculated by county and birth year, while sex ratio at birth is calculated as the number of females per male. All regressions control for county fixed effects, birth year fixed effects, and weather controls (including minimum and maximum temperature, and log of rainfall). Columns 2 and 4 further control for birth year linear trends interacted with pre-reform county characteristics. Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A7. Effects of HRS on Fertility Selection: Mother-level Analysis

	Whether to give birth		Whether to experience pregnancy	
	(1)	(2)	(3)	(4)
HRS	-0.006	-0.007	0.000	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)
OCP	-0.004	-0.003	0.009	0.009
	(0.006)	(0.006)	(0.006)	(0.006)
Mean of dep. var.	0.139	0.139	0.160	0.160
County-specific linear trends	No	Yes	No	Yes
R^2	0.047	0.048	0.057	0.059
N	273,115	273,115	264,502	264,502

Notes: Data used in this table is from 1985 and 1987 IDFS data. All regressions control for mother fixed effects, birth (pregnancy) year fixed effects, an indicator of OCP, weather controls (including minimum and maximum temperature, and log of rainfall) and the number of TVEs *per capita*. Columns 2 and 4 further controls for birth (pregnancy) year linear trends interacted with pre-reform county characteristics. Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A8. Alternative estimators considering treatment effects heterogeneity

	(1)	(2)	(3)	(4)	(5)
HRS	46.80**	38.72**	41.06**	40.94**	40.26**
	(18.96)	(17.91)	(18.28)	(18.25)	(18.28)
Mean of dep. var.	3225	3225	3225	3225	3225
Individual controls	No	Yes	Yes	Yes	Yes
OCP	No	Yes	Yes	Yes	Yes
Weather controls	No	No	Yes	Yes	Yes
TVE controls	No	No	No	Yes	Yes
Linear Trends	No	No	No	No	Yes
N	11,861	11,861	11,861	11,861	11,861

Notes: This table presents the estimated coefficients of Sun and Abraham (2021)'s method which uses the last-treated units as the control group. The analysis sample is restricted to newborns born before the last-treated period (1984). All regressions control for mother fixed effects, pregnancy-start year fixed effects and pregnancy-start month fixed effects. Individual controls include the gender and birth order of each child. Weather controls include minimum and maximum temperature, and log of rainfall. TVE control denotes the number of TVEs *per capita*. Column 5 additionally controls for pregnancy-start year linear trends interacted with pre-reform county characteristics (log of population in 1978, log of rural net income *per capita* in 1978, log of average slope, log of average altitude of the county, and log of distance to Beijing). Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A9. Effects of HRS Reform on Breastfeeding Duration

	(1)	(2)	(3)	(4)
HRS	5.579***	4.434***	5.160***	4.840***
	(0.680)	(0.567)	(0.505)	(0.514)
Mean of dep. var.	20.42	20.41	20.41	20.41
Covariates	No	Yes	Yes	Yes
Age-in-month FE	No	No	Yes	Yes
Linear Trends	No	No	No	Yes
R^2	0.542	0.587	0.618	0.619
N	13,545	13,526	13,523	13,523

Notes: The dependent variable is the breastfeeding duration measured in months and is winsorized at the 99th percentile to mitigate extreme values. All regressions control for mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects. The covariates include the gender and birth order of each child, an indicator of OCP, minimum and maximum temperature, log of rainfall and the number of TVEs *per capita*. Column 4 controls for pregnancy start-year linear trends interacted with pre-reform county characteristics (log of population in 1978, log of rural net income *per capita* in 1978, log of average slope, log of average altitude of the county, and log of distance to Beijing). Robust standard errors clustered at the county level appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

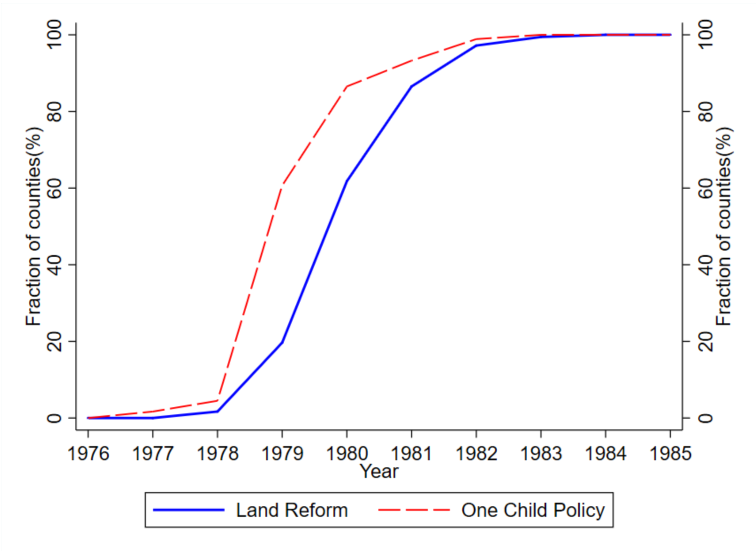


Figure A1. The Rollout of HRS and OCP Across Years

Notes: The blue and red lines represent the proportion of counties that have adopted the HRS and OCP, respectively.

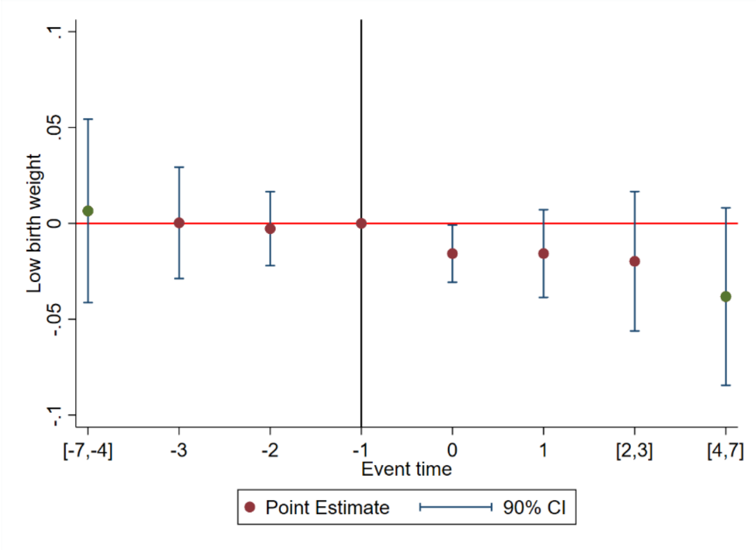


Figure A2. Event-Study Estimates on the Probability of Low Birth Weight

Notes: This figure shows the event-study estimates for the low birth weight indicator, using the model specified in Equation 2. The model includes mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects, as well as extended controls (gender and birth order of each birth, an indicator of OCP, minimum and maximum temperature, log of rainfall, and the number of TVEs *per capita*). The green endpoints on the left and right sides represent the average effects for periods 4-7 years before, and 4-7 years after the HRS rollout, respectively. Standard errors are clustered at the county level.

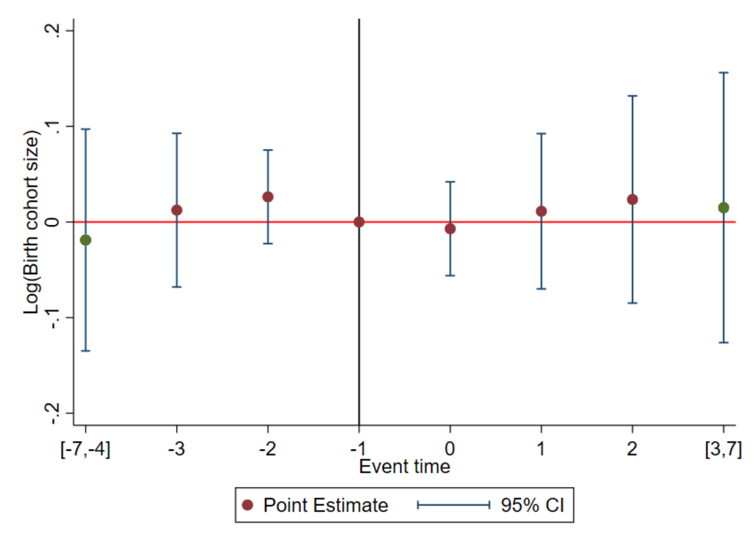
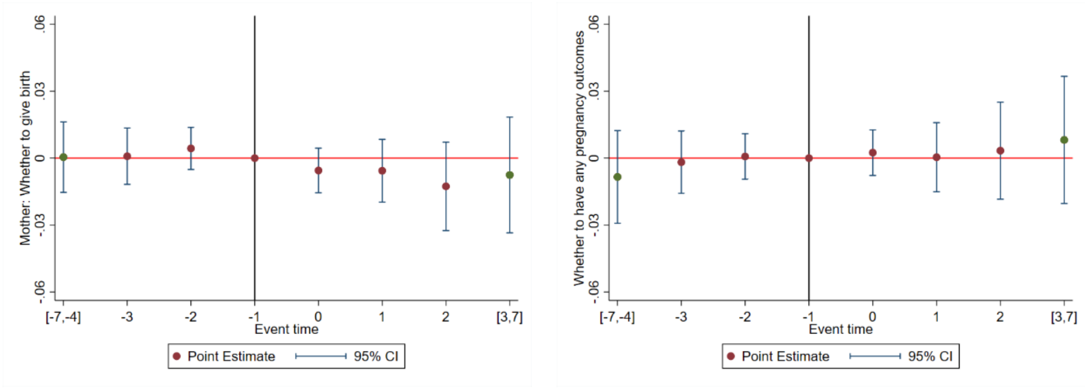


Figure A3. Event Studies on Fertility Selection: County-level Analysis

Notes: Data used in this figure is from China's 1990 Census data. Birth cohort size is calculated by county and birth year. The model controls for county fixed effects, birth year fixed effects, maximum and minimum temperature and log of rainfall, as well as birth year linear trends interacted with pre-reform county characteristics. The green endpoints on the left and right sides represent the average effects for periods 4-7 years before, and 3-7 years after the HRS rollout, respectively. 95% confidence intervals are displayed around each estimate. Standard errors are clustered at the county level (***p < 0.01, **p < 0.05, *p < 0.1).

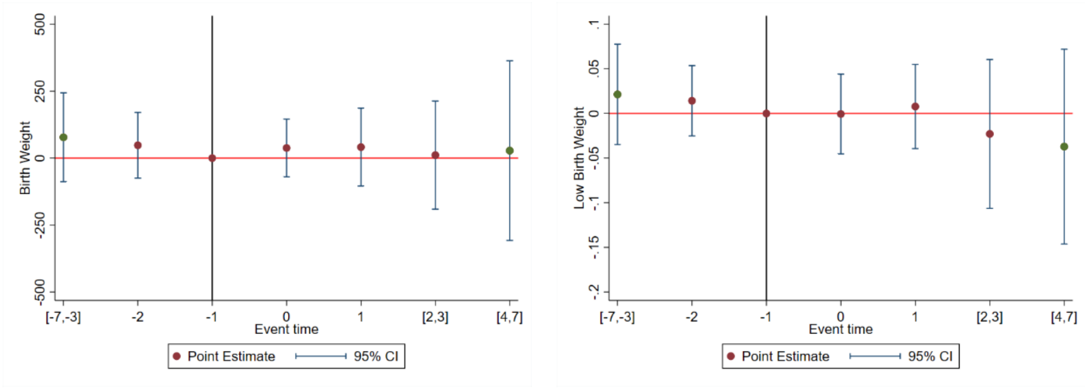


a. Whether to give birth

b. Whether to experience pregnancy

Figure A4. Event Studies on Fertility Selection: Mother-level Analysis

Notes: Data used in this figure is from 1985 and 1987 IDFS data. All regressions control for mother fixed effects, birth (pregnancy) year fixed effects, an indicator of OCP, minimum and maximum temperature, log of rainfall, the number of TVEs *per capita*, and birth (pregnancy) year linear trends interacted with pre-reform county characteristics. The green endpoints on the left and right sides represent the average effects for periods 4-7 years before, and 3-7 years after the HRS rollout, respectively. 95% confidence intervals are displayed around each estimate. Standard errors are clustered at the county level (***p < 0.01, **p < 0.05, *p < 0.1).



a. Birth Weight b. Low Birth Weight

Figure A5. Event Studies Using Urban Samples

Notes: Data used in this figure is from the urban sample of 1985 and 1987 IDFS data. The left and right figures show the event-study estimates for birth weight and low birth weight, respectively. All regressions control for mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects, as well as extended controls (gender and birth order of each birth, an indicator of OCP, minimum and maximum temperature, log of rainfall and the number of TVEs *per capita*). The green endpoints on the left and right sides represent the average effects for periods 3-7 years before, and 4-7 years after the HRS rollout, respectively. Standard errors are clustered at the county level.

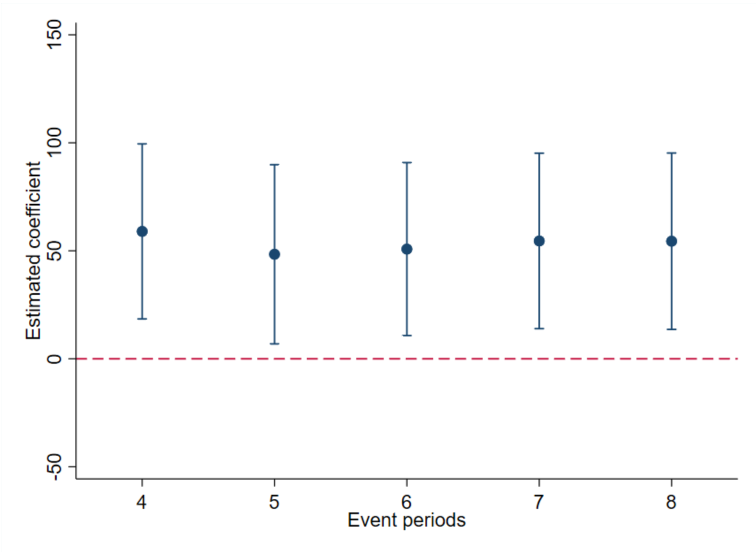


Figure A6. Sensitivity to the Selection of Event Periods

Notes: This figure depicts the average treatment effect of prenatal exposure to the HRS reform on birth weight under the different choices of time windows. The values on the x-axis represent the number of event periods before and after the reform. All regressions control for mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects, as well as extended controls (gender and birth order of each birth, an indicator of OCP, minimum and maximum temperature, log of rainfall and the number of TVEs *per capita*). 95% confidence intervals are displayed around each estimate. Standard errors are clustered at the county level.

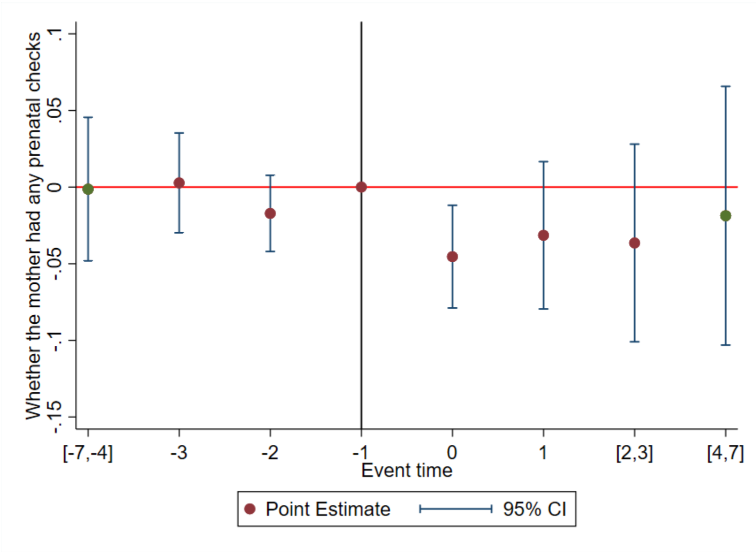


Figure A7. Event-Study Estimates on the Probability of Prenatal Checks

Notes: This figure shows the event-study estimates for prenatal checks, using the model specified in Equation 2. The model includes mother fixed effects, pregnancy start year fixed effects, pregnancy start month fixed effects, as well as controls for gender and birth order of each child, OCP, maximum and minimum temperature, rainfall (in logs), and the number of TVEs *per capita*. 95% confidence intervals are displayed around each estimate. The green endpoints on the left and right sides represent the average effects for periods 4-7 years before, and 4-7 years after the HRS rollout, respectively. 95% confidence intervals are displayed around each estimate. Standard errors are clustered at the county level.

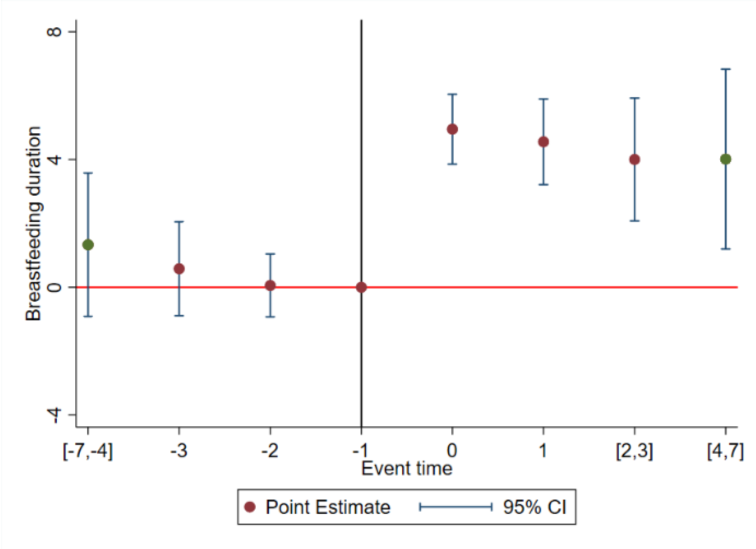


Figure A8. Event-Study Estimates for Impact of HRS on Breastfeeding Duration

Notes: This figure shows the event-study estimates from Equation 2. The model includes mother fixed effects, pregnancy start-year fixed effects and pregnancy start-month fixed effects, as well as extended controls (gender, birth order, OCP, minimum and maximum temperature, log of rainfall, the number of TVEs *per capita*). The breastfeeding duration is winsorized at the 99th percentile. 95% confidence intervals are displayed around each estimate. The green endpoints on the left and right sides represent the average effects for periods 4-7 years before, and 4-7 years after the HRS rollout, respectively. 95% confidence intervals are displayed around each estimate. Standard errors are clustered at the county level.