Stepping Down from Life-long Posts: Layoffs, Fertility, and Educational Attainment in Urban China*

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Abstract

We analyze how layoffs affect child quantity and quality, by exploiting the downsizing of China's state-owned enterprises between 1995 and 2004. It induced a layoff of more than 47 million workers who had previously held permanent jobs. Difference-in-differences estimates indicate that layoffs increased birth rates by around 3.6‰, driven mainly by layoffs of female workers. This occurs on two margins: 1) the extensive margin: women's selection into earlier motherhood; and 2) the intensive margin: the increased births of subsequent children. We also find that the layoffs reduced children's educational attainment, driven by two mechanisms: 1) negative parental selection: women with lower socioeconomic status are more likely to increase fertility during layoffs, compared with others; and 2) reduced educational investments during schooling. This evidence suggests that negative income shocks affect both the size and quality of the involved cohorts.

Keywords: state-owned enterprises, layoffs, fertility, educational outcomes
JEL codes: I20, J13, J64, P21

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1 Introduction

This paper explores the impact of layoffs on fertility and educational attainment of children born during the layoffs in the context of a quantity and quality trade-off. Our starting point is the growing evidence that negative income shocks affect fertility (e.g., Huttunen and Kellokumpu, 2016). Yet, starting from the influential articles by Becker (1960), Becker and Lewis (1973), and Willis (1973) who consistently highlight households' demand for child quality,[1] the quantity and quality trade-off has been a theoretical framework proven powerful for understanding fertility. Recognition of the trade-off implies that ignoring the quality changes would miss important dimensions of fertility adjustments induced by layoffs, and consequently, we examine the demographic consequence of layoffs from a point of view of quantity and quality combined in the paper.

We proceed in three steps to address the issue. We first examine the relationship between layoffs and fertility. This is an empirical question when the relationship is viewed through the seminal article on fertility choice by Becker (1965), who highlights the opposing income and substitution effects. However, the endogeneity of individuals' labor market outcomes empirically poses a key challenge for establishing causality. We analyze the question in the context of China's state-owned enterprises (SOEs) reform characterized by exogenous massive layoffs. Second, we further decompose the effects of fertility into the extensive and intensive margins, generating additional insights into the adjustments of fertility induced by layoffs. Third, we examine how layoffs affect child quality and mechanisms involved, motivated by a growing literature that highlights the importance of endowment at birth and early-life investment for educational attainment (e.g., Dahl and Lochner, 2012; Chevalier and Marie, 2017; Figlio, Karbownik, Roth, Wasserman et al., 2019).

In particular, the fertility and education effects of layoffs are estimated by exploiting the downsizing of China's SOEs starting from 1995, which represents an exceptional opportunity for exploring these effects and adjustments at margins. During the period of the downsizing, more than 47 million SOE workers originally holding life-long posts were laid off (Hughes, 1998).[2] One-fifth of Chinese households were negatively affected by the retrenchments, and some of the highest unemployment rates were observed among women of fertile age. The reform, consequently, created an extremely negative shock of unemployment for SOE workers and their families, which is rare, large-scale, and less selective in history, and facilitates our research design discussed below.

To frame our empirical analysis, we build a simple model that clarifies the conditions under which layoffs induce an increase in child quantity with a cost of quality decline. The subsequent analysis tests the prediction. Our identification strategy exploits regional layoff variation generated by the predetermined intensity of SOE employment in 1995, the initial year of the reform. The intensity reasonably captures the exogenous shock of layoffs induced by the reform, as documented below. Our sample consists of households with fertile women between 1990 and 2004, exposed to the reform between 1995 and 2004, and their detailed fertility outcomes are recorded in the 2005 mini-census of population. The comprehensive data allow a difference-in-differences (DID) assessment of whether the fertility of urban households residing in cities with a higher initial intensity of SOE employment increased more after 1995 than those in cities with a lower initial intensity. To corroborate the results, we exploit cities' distances to the nearest coal mines to implement an instrumental variable (IV) estimation. Our construction of IV leverages the fact that China's early

[1] For example, they use the quantity and quality trade-off to explain, among other things, why birth rates could decline when income increases even though children are not an inferior good.

[2] China Labour and Social Security Yearbook (Beijing: Ministry of Labour and Social Security, 2002).

state-owned sectors prioritized heavy industries relying crucially on coals as energy or production inputs. We conduct a series of tests to ensure that the distances do not impact birth rates through other channels.

We find robust evidence that layoffs induced by the reform increased birth rates, driven mainly by the layoffs of females (not males, instead). According to our preferred specification, 24 more babies were born for every 1000 fertile women residing in cities with full SOE employment in 1995 per year after the reform, relative to its counterpart with zero employment of SOE workers in 1995. We find similar results by using either a two-period double difference or a multi-period event study that allows for differential trends across regions. These results remain robust to including geographic variables interacted with post dummies, various variables on fertility policies, socioeconomic variables, demographic variables, and a rich set of fixed effects.

Furthermore, we find that layoffs increased women's selection into earlier motherhood and the births of subsequent children, suggesting that both tempo and quantum effects contribute to the increase in fertility following the reform. This finding generates additional insights into fertility choices affected by unemployment. Specifically, we associate the number of newly married women with the extensive margin as it largely reflects the subsequently added number of first birth in a family;[3] we also associate the births of subsequent children with the intensive margin as it speaks to the "out-of-plan" births given that the one-child policy (OCP) in China allows only one child free of penalties for urban households.[4]

We also find that layoffs reduced child quality as measured by schooling years for children born during the layoffs by using a framework of a cohort DID. The reduction is driven mainly by two mechanisms. First, as documented, mothers with lower socioeconomic status (SES) are more likely to increase fertility than their counterparts with higher status, consistent with negative selection into motherhood to the extent that mothers' layoffs decreased the quality composition of children at birth if higher-education mothers tend to have higher-quality children. Second, we assess whether educational inputs are lower for children born during the reform period and find that a household with a laid-off mother spent around 100 RMB (19.2%) less on after-school education in 2001. This finding suggests reduced education investment. Finally, we find a moderate increase in premarital births but no impact on the sex ratio of newborns caused by the layoffs.

We subject these results to a variety of placebo tests and robustness tests. First, we conduct placebo regressions that test the effects of initial variation in employment intensity generated by different types of non-SOE firms that do not experience similar layoffs over the period. In doing so, we calculate the employment ratio of exporting firms (EF) in 1995 and that of foreign-invested firms (FIF) in the same way as we compute the initial intensity of SOE employment. Using the intensity of FIF and EF employment, we repeat the main specification and find most estimates become insignificant with small magnitudes, corroborating the causal interpretation of our main results. Additionally, we address one by one the potential concerns on negative weights, migration, underreporting of the newborns due to the OCP, and age structure impacts, and our results remain robust. Finally, we construct two alternative measures of layoffs: 1) the change in the ratio of the SOE employment over the entire period of 1995-2004, and 2) a Bartik-style change in the SOE share

[3] According to the 2000 population census, more than 60% of newly formed families had their first child in the subsequent 1.5 years, and the number increased to 80% by the third year of their marriage. This finding means that most Chinese couples have their first birth shortly after their marriages over the period 1990-2004.

[4] The population control policy is well known as the one-child policy (OCP), and the rule was implemented stringently for urban families. Extra fertility would induce various punishments. See Zhang (2017) and Huang, Lei and Sun (2021) for a detailed discussion on the evolution and the implementation of the OCP in China.

weighted by initial industry composition. Our results are robust to alternative measures in both cases.

We make three main contributions to the literature. First, we use a large-scale and less-selective layoff to explore the causal link between layoffs and fertility. Previous studies have either used small economic shocks, such as plant closures, or unemployment during economic recessions (Dehejia and Lleras-Muney, 2004; Del Bono, Weber and Winter-Ebmer, 2012; Bono, Weber and Winter-Ebmer, 2015; Huttunen and Kellokumpu, 2016). These shocks may be short-lived and cyclical, making unemployment a moderate shock. By contrast, we examine a more substantial shock of layoffs induced by a reform that removed workers from life-time posts and induced large labor supply adjustments. These adjustments, including quitting the labor market,[5] may have reduced the time value of women substantially, and additional children thus become more attractive. Our findings also relate to the literature that examines fertility decomposed into margins. Previous studies distinguishing between the intensive and extensive fertility margins include Aaronson, Lange and Mazumder (2014), Baudin, De La Croix and Gobbi (2015), and Momota (2016). Our analysis adds to the growing literature by examining the timing of women's selection into marriage and motherhood, generating new insights into fertility adjustments.[6]

Second, our findings contribute to the literature by explaining a negative relationship between income and fertility that is particularly puzzling if a child is not an inferior good (e.g., Schultz, 1985). Theoretical discussions of the relationship, starting from Becker (1960), emphasize the demand for child quality and opportunity costs of parents' time. We contribute to the literature by providing the first piece of evidence that a decline in women's opportunity cost of time driven by layoffs resulted in increased fertility with a decrease in child quality. This evidence stands in sharp contrast to a vast literature that finds that layoffs reduce the demand for child quantity without a discussion on simultaneous changes in quality choice (e.g., Lindo, 2010; Sobotka, Skirbekk and Philipov, 2011; Del Bono, Weber and Winter-Ebmer, 2012; Huttunen and Kellokumpu, 2016; Clark and Lepinteur, 2020; Matysiak, Sobotka and Vignoli, 2021).

Third, this paper relates to the literature on the trade-off between child quantity and quality, a crucial ingredient of unified growth models that explain the transition from Malthusian stagnation to modern growth (Galor, 2005). We contribute to the literature by assessing the effect of an exogenous change in incomes induced by layoffs on the joint choices of quantity and quality of children. By contrast, previous studies, most of which use twin births as an experiment, empirically examine the impact of an exogenous increase in fertility on child quality implied by the Becker-Lewis quantity-quality model (Rosenzweig and Wolpin, 1980; Hanushek, 1992; Caceres, 2004; Angrist, Lavy and Schlosser, 2005; Black, Devereux and Salvanes, 2005; Li, Zhang and Zhu, 2008; Rosenzweig and Zhang, 2009; Mogstad and Wiswall, 2016; Qin, Zhuang and Yang, 2017). Finally, this paper also relates to the literature that examines the consequences of the restructuring of China's state-owned sector. Although the profound impact of the SOE restructuring has prompted numerous studies analyzing its economic and efficiency consequences, such as precautionary savings and productivity gains (Berkowitz, Ma and Nishioka, 2017; Hsieh and Song, 2015; He, Huang, Liu and Zhu, 2018),

[5]According to Feng, Hu and Moffitt (2017), the labor force participation rate in China substantially declined over the period, from 80.3% in 1995 to 74.2% in 2004. See Feng, Hu and Moffitt (2017) for a detailed discussion.

[6]Theoretically, childlessness as the extensive margin of fertility is also an important option for fertility choice, as analyzed by Baudin, De La Croix and Gobbi (2015). Yet, the childlessness rate is extremely low in China, because the continuity of the family line is one of the most important social norms in the Chinese society, leaving birth timing an important dimension to affect fertility. According to the 2005 population survey, the childlessness rate is less than 1% for women reaching 49 years old.

less well understood is whether and how the transformation impacts the quantity and quality of children over a period characterized by unprecedented layoffs in urban China.

The structure of the paper is as follows. A theoretical framework and institutional background are given in Section 2. Empirical strategy and data description are introduced in Section 3. We discuss the results on the effect of layoffs on fertility in Section 4. Results on decomposing the fertility-enhancing effects into margins are reported and discussed in Section 5. Effects of layoffs on child quality are analyzed in Section 6. Robustness checks and additional findings are reported in Section 7. Section 8 concludes the study.

2 Theoretical Framework and Institutional Background

2.1 The theoretical adjustments of quantity and quality

In this section, we present a simple illustrative quantity-quality trade-off model following the seminal work by Becker (1960). To simplify the analysis, we consider layoffs as equivalent to wages, allowing us to model the fertility impact of layoffs by directly formulating wages. Considering a representative household that maximizes utility subject to a budget constraint and the technology of human capital production, the choice problem for households is as follows:

$$\begin{aligned} \max_{c,n,q,h} \quad & \alpha_c \log c + \alpha_n \log n + \alpha_q \log q \\ \text{s. t.} \quad & c + \beta_0 n + hn \leq w(1-\beta_1 n) \\ & q = f(h), \end{aligned} \tag{2.1}$$

where c is consumption, n is the number of children, q is the average quality of children, β_0 and β_1 are goods and time costs for producing and rearing children, respectively. The input for child quality is denoted by h and $f(h)$ is the production function of child quality.

The optimization issue yields the first-order conditions:

$$\begin{aligned} \frac{hf'(h)}{f(h)} &= \frac{\alpha_n}{\alpha_q} \frac{\frac{h}{w}}{\frac{\beta_0}{w} + \beta_1 + \frac{h}{w}} \\ n^* &= \frac{\alpha_n}{\alpha_c + \alpha_n} \frac{1}{\frac{\beta_0}{w} + \beta_1 + \frac{h^*}{w}} \end{aligned} \tag{2.2}$$

Solving the optimization issue requires additional assumptions on the properties of the human capital production function, $f(h)$, as discussed below. Here we follow Becker and Tomes (1976) and let

$$f(h) = d_0 + d_1 h, d_0 > 0, d_1 > 0 \tag{2.3}$$

The solution to h^* and n^* is

$$\begin{aligned} h^* &= \frac{\frac{\alpha_q}{\alpha_n}(\beta_0 + \beta_1 w) - \frac{d_0}{d_1}}{1 - \frac{\alpha_q}{\alpha_n}} \\ n^* &= \frac{\frac{\alpha_n - \alpha_q}{\alpha_c + \alpha_n}}{\beta_1 + \frac{\beta_0}{w} - \frac{d_0}{w d_1}} \end{aligned} \tag{2.4}$$

Taking derivatives with respect to household's wage, w, gives

$$\frac{\partial h^*}{\partial w} = \frac{\frac{\alpha_q}{\alpha_n}}{1-\frac{\alpha_q}{\alpha_n}}\beta_1$$
$$\frac{\partial n^*}{\partial w} = \frac{(\beta_0 - \frac{d_0}{d_1})(\alpha_n - \alpha_q)}{w^2(\alpha_c + \alpha_n)(\beta_1 + \frac{\beta_0}{w} - \frac{d_0}{wd_1})^2} \tag{2.5}$$

As can be seen from the expressions, as long as $0 < \alpha_q < \alpha_n$ and $\frac{d_0}{d_1} > \beta_0$, we have

$$\frac{\partial h^*}{\partial w} > 0$$
$$\frac{\partial n^*}{\partial w} < 0$$

.

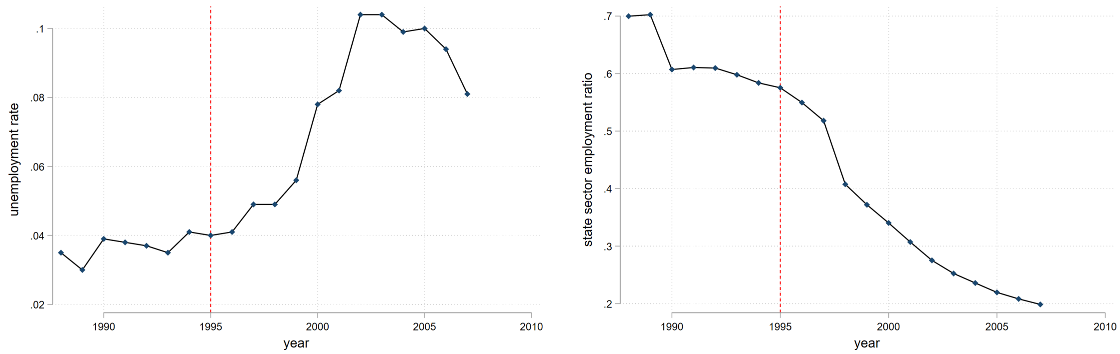
Below we further discuss the model. As can be seen from the model, a negative income shock (i.e., induced by layoffs) can result in the quantity of children to increase and the quality to decrease, and this result requires three conditions in general. First, let $\eta = \frac{hf'(h)}{f(h)}$, we need that $\eta' > 0$, that is, the human capital produced increases rapidly with the increases in the input for child quality. This is purely a property of human capital production functions which are not uncommon in the present literature. Examples of the production functions from influential work include Becker and Tomes (1976) and De La Croix and Doepke (2003).

Second, the preference for the number of children is higher than that for the quality in households' choice of fertility, $\alpha_q < \alpha_n$. This assumption makes quality relatively less expensive for people earning a higher wage, resulting in a trade-off between the quantity and quality of children. When birth rates are compared across countries, it is clear that developing countries tend to have substantially higher ones than developed countries, and this fact implies that households from the developing world, including the urban China over 1995-2004, are very likely to have a strong preference for quantity relative to the quality of children.

Third, the goods cost of children (β_0) is smaller relative to $\frac{d_0}{d_1}$, that is, $\frac{d_0}{d_1} > \beta_0$. Becker and Tomes (1976) interpret d_0 as endowment at birth (or "innate ability") and d_1 is the marginal productivity of human capital production. If ability transmits over generations (i.e., through genes) for a cohort, then the quality endowment at birth may correlate highly to the quality composition of mothers. This motivates our subsequent analysis of negative parental selection as a mechanism for understanding deteriorated quality of children born during the reform (see Section 6.2.1).

2.2 Institutional background

The state-owned sector plays an important role in China's economy until today. For minimizing social instability and politically reducing the pain induced by China's economic reform starting in 1978, the state-owned sector continued to host a high share of urban employment before 1995. This is a strategic result of the central government encouraging the rise of the non-state sector without downsizing the state sector. Yet, this strategy inevitably caused excess employment in the state-owned sector. As competition from the more efficient non-state sector continued to increase, the



(a) Unemployment rates

(b) State sector employment rates

Figure 1: Unemployment rates and state sector employment ratio

Source: Feng, Hu and Moffitt (2017).

financial condition of SOEs continued to deteriorate, and default loans flowing to the state-owned sector increased sharply. As a result of the default loans, financial support made by central and local governments caused chronic high inflation (Brandt and Zhu, 2000). By 1994, financially subsidizing SOEs to support employment was difficult to sustain (Zhu, 2012).

In 1995, China began its SOE reform by allowing bankruptcy and privatization of SOEs (Cao, Qian and Weingast, 1999; Feng, Hu and Moffitt, 2017). An immediate consequence of the reform that reduced governments' commitment to stable employment is the closure and privatization of many SOEs that accommodated huge employment. Not surprisingly, tens of millions of workers were laid off from the state-owned sector, as these enterprises went bankrupt, restructured, or privatized. The lay-offs were known as xiagang in Chinese which literally translates into English as "to step down from one's post." Prior to the reform, given that these workers enjoyed the benefits of an "iron rice bowl", and absolute job security along with social benefits (such as healthcare and pensions) provided by the state, the reform created an extremely negative shock of incomes for urban households.

The 15th Congress of the Chinese Communist Party held in 1997 substantially accelerated the reform (Lau, 1999). The Congress formally sanctioned the private sector as an important "component" of China's "socialist market economy" and legalized the co-existence of the two components. Since 1997, China saw an accelerated shrinking of the state-owned sector. Additionally, as an important part of the reform, a large number of large and medium SOEs exited the market permanently in the way of merger and bankruptcy. For the small ones, many of them were privatized or went bankrupt under the policy slogan "grasping the large and letting go of the small", which means selling small enterprises and keeping big and strategically important enterprises as state-owned (Hsieh and Song, 2015).

By 2001, more than 35 million workers who originally held secure, lifetime jobs were laid off from SOEs (World Bank, 2007). According to China's Labor Statistical Yearbooks, the total layoffs amount to 47.87 million over the period from 1995 to 2004. This number means that almost one-fifth of Chinese households were affected by the SOE retrenchments. Figure 1 displays the dramatic increase in unemployment rates and the rapid decline in employment ratio for the state-owned sector over the period, suggesting a mass layoff induced by the reform. The reform of employment guarantees for SOE workers thus represents a rare, large-scale, and less-selective layoff shock in

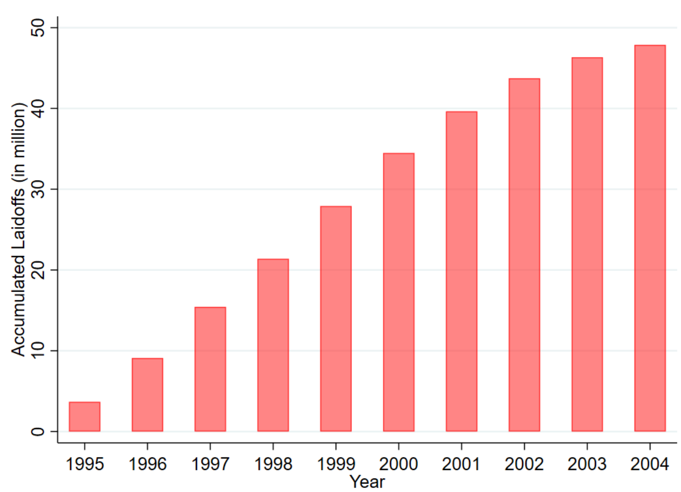


Figure 2: Evolution of accumulated layoffs, 1995-2004

Notes: Figure 2 displays the annual evolution of accumulated layoffs between 1995 and 2004. Data are from China's Labor Statistical Yearbooks (1996, 1997, 1998, 2005).

history.

By dismantling life-long employment, the reform also altered China's employment system fundamentally. Indeed, before that, the employment of the state-owned sector is described figuratively as "iron rice bowl". Thus, the reform is described accordingly as "smashing the iron rice bowl." As market-based (not planning-based) employment became increasingly dominant, labor force participation rates declined rapidly in China. As calculated by Feng, Hu and Moffitt (2017), the rates declined considerably from 80% in 1995 to 74% in 2004, and more of them are women. This evidence suggests that many laid-off workers, especially women, quit the labor markets.

2.3 Quantifying the extent of SOE layoffs

The restructuring of the state-owned sector induced one of the largest layoff waves in Chinese history. Figure 2 displays the annual evolution of accumulated layoffs of SOE workers from 1995 to 2004.[7] Two patterns stand out. First, more than 1.5 million workers were laid off from the state-owned sector each year, reflecting that the state-owned sector dramatically downsized throughout the period. Second, by the end of the period, the layoffs accumulated to 47.867 million, reflecting a spike of unemployment induced by the reform. [8]

The evolution of accumulated and new layoffs hides important variations both across cities and within cities over time that we exploit to estimate the effects of layoffs on fertility. The shaded area in Figure 3 represents the interquartile range for prefectures' SOE employment ratio, and the line within it plots the mean SOE employment ratio over the period.[9] Cities initially differed tremendously in SOE employment, as reflected in the interquartile range that is relatively wide in 1995, the earliest year when China's official statistics systematically recorded the city-level employment

[7] China's Labor Statistical Yearbooks did not record information on layoffs until 1995.

[8] City-level information on the employment of SOEs prior to 1995 is not available.

[9] Given that regional employment ratios for SOE are not available in 1996 and 1997, the two years are excluded in plotting the interquartile range for prefectures' SOE employment ratio.

of SOEs. Yet, over time the shaded area becomes increasingly narrow, highlighting the sharp downsizing of SOEs. In magnitude, the interquartile range declined from 7%-24% in 1995 to 2%-7% in 2004.

Regions varied in their exposures to the layoff shocks. Regions with an initially higher proportion of workers employed in the state-owned sector suffered from a larger exposure to job destruction. For example, the northeastern rust belt, where SOEs were most concentrated, is one of the hardest-hit regions during the reform. This fact implies that the initial intensity of SOE employment in a region largely reflects local exposure to job reductions induced by the reform. Hence, we use the SOE employment ratio in 1995 to measure the extent of a city's exposure to layoffs induced by the reform. The layoff ratio from 1995 to year t is constructed by using the following equation:

$$Layoff\ ratio_{c,t} = \frac{(SOE\ emp_{c,1995} - SOE\ emp_{c,t})}{Urban\ pop_{c,1995}}, \tag{2.6}$$

where $Layoff\ Ratio_{c,t}$ is the layoff ratio in city c during 1995 to year t (year t ranges from 1998 to 2004);[10] $SOE\ emp_{c,1995}$ and $SOE\ emp_{c,t}$ are total SOE employment in 1995 and year t, respectively; $Urban\ pop_{c,1995}$ denotes the urban working-age population in 1995. The main data used for constructing the measure is an economic census of all industrial firms with independent accounting (around 510,000 firms) in the Annual Survey of Industrial Firms (ASIF) in 1995. It covers universal industrial firms operating at the time, including SOEs.

We use the initial intensity of the SOE employment in 1995 as a proxy for local exposure to layoffs induced by the reform, given that regions with higher intensity of SOE employment in 1995 will see a larger reduction in the employment of SOE over the period from 1995 to year t. With the measure in hand, the exogeneity of reform-induced unemployment for a region is that the initial intensity represents an exogenous local reduction in the SOE employment in the subsequent years, if all cities end up with a similar intensity of SOE employment regardless of the initial intensity. Several reasons support the exogeneity. First, the low remaining variation in the SOE employment ratio by 2004 implies little room for reform discretion in the reduction in the SOE employment. Indeed, the interquartile range for the SOE employment ratio was only 1.6%-6.6% at the end of the period. The average SOE employment ratio declined from 18% to 5% between 1995 and 2004, but equally remarkable was the decline in the standard deviation across cities from 15% to 5%. Second, the partial correlation between the reduction in the SOE employment ratio over the 1995-2004 period and the 1995 ratio of SOE employment is as high as 0.80. This result means that a city's change in the ratio of SOE employment from 1995 to 2004 (and other subsequent years) is well explained by its initial ratio.

To further test the exogeneity of reform-induced unemployment, we regress the layoff ratio between 1995 and year t (year t ranges from 1998 to 2004) computed from Equation 2.6 on the initial SOE employment ratio in 1995 at the city level. These regressions also include province fixed effects and a set of geographic variables as controls, including distance to coast in logarithm, ruggedness, latitude, longitude, and urban working-age population in 1995. As shown in Table A in the Appendix, SOE employment ratios in 1995 strongly predict the reduction in the ratios of SOE employment during the post-reform periods. Specifically, column (7) suggests that increasing the SOE employment ratio from 0 to 1 in 1995 results in a higher layoff ratio of 0.897 during 1995-2004.

[10] Annual Survey of Industrial Firms (ASIF) only contains a small fraction of SOEs in 1996 and 1997, therefore, we cannot determine the city-level employment of SOEs in these two years.

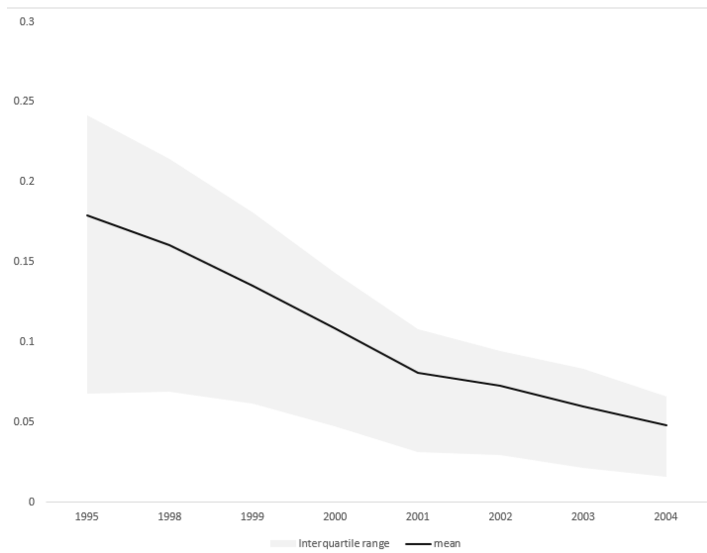


Figure 3: Evolution of SOE employment ratio, 1995-2004

Notes: The solid lines show the mean SOE employment ratio across cities. The shaded area shows the interquartile range. Data are from the 1995 Economic Census and the 1998-2004 ASIF.

Figure 4 presents seven plots with fitted lines and reference lines. These plots explicitly show the two-way relationship between the initial level in SOE employment ratio (on the horizontal axis) and the changes in SOE employment ratio between 1995 and year t (on the vertical axis).[11] Two observations can be made from the plots. First, in either one of the plots, the relationship between the initial employment in 1995 and the changes in the SOE employment ratio is almost one-to-one, as reflected in the close fit with the red 45-degree lines. Second, in a comparison of these plots, the fitted lines become increasingly closer to the red reference lines. This result suggests that the SOE employment ratio tends to be increasingly even across cities as the SOE reform continued to roll out.

Thus, a city's relative reduction in the SOE employment in a post-period is well explained by its initial SOE ratio, and the explanatory power increases over time, suggesting little room for local discretion in the reform-induced layoffs. The initial SOE employment ratio is, therefore, a plausibly exogenous measure for the layoff shock generated by the reform that provides an excellent experiment that we use to establish causality below.

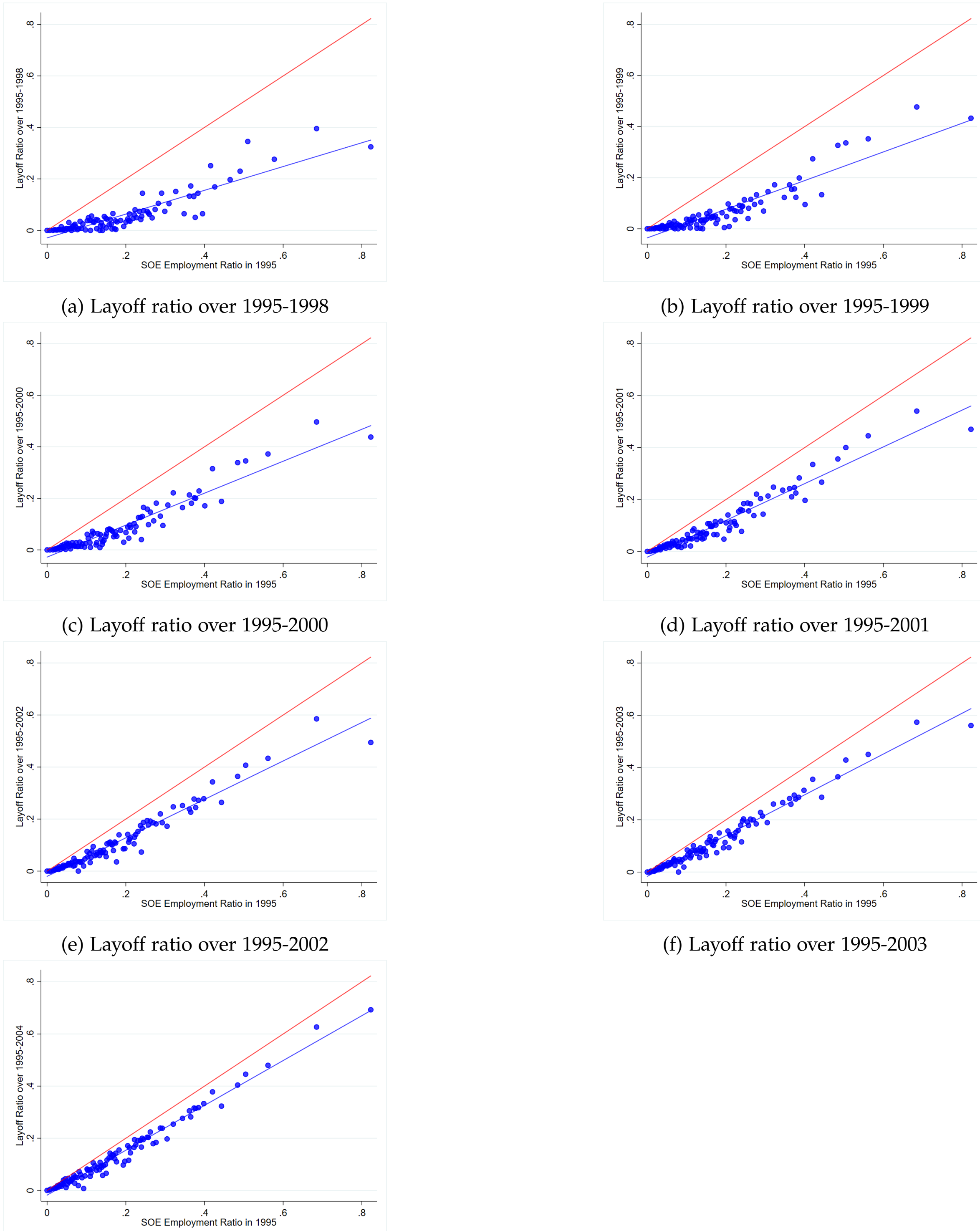
3 Empirical Strategy and Data

3.1 Empirical strategy

To identify the effect of reform-induced layoffs on fertility, we estimate the following empirical model:

$$\begin{aligned} Birth\ rate_{c,t} = \beta_0 + \beta_1 SOE\ ratio_{c,1995} \times Post_t + \beta_2 X_c^{geo} \times Post_t + \\ \beta_3 X_{p,t}^{pol} + \beta_4 X_{c,1994}^{soc} \times Post_t + \beta_5 X_{c,t}^{dem} + \mu_c + \lambda_t + \varepsilon_{c,t}, \end{aligned} \tag{3.1}$$

[11] In Appendix A, we report these results used for plotting the figure.



(a) Layoff ratio over 1995-1998

(b) Layoff ratio over 1995-1999

(c) Layoff ratio over 1995-2000

(d) Layoff ratio over 1995-2001

(e) Layoff ratio over 1995-2002

(f) Layoff ratio over 1995-2003

(g) Layoff ratio over 1995-2004

Figure 4: Layoff ratio by initial employment level

Note: The solid blue line is the regression line of layoff ratio on initial employment of SOE; the red line is the 45-degree line (for comparison).

where c, t, and p refer to city, year, and province, respectively. The outcome variable, $Birth\ rate_{c,t}$ is the number of births per 1000 urban women of childbearing age (aged 15 to 49 years) for city c in year t.[12] We consider the period 1990-2004, which covers the majority of the periods of SOE reform and includes several years earlier as a comparison. $SOE\ ratio_{c,1995}$ denotes the SOE employment ratio in 1995, the starting year of the reform, and is defined as the ratio of the total SOE employment over the urban working-age population (aged 16 to 59) in city c in 1995. As we have verified in Section 2.3, a higher SOE employment ratio in 1995 predicts a larger layoff ratio in post-1995 periods, suggesting that the measure captures the extent to which the urban part of a city is exposed to the layoff shock induced by the reform. Considering that typical households make a birth decision around 10 months before a baby is born, we lag one year to define $Post_t$ as a dummy indicating years starting in 1996 (from 1996 to 2004).

An empirical concern is that inherent differences between higher and lower SOE-employment areas may have caused some city characteristics to change differently after 1995, even in the absence of the reform. To address the concern, most specifications control for post-interacted measures of geographic variables ($X_c^{geo} \times Post_t$), and the geographic variables include cities' distance to coast (in logarithm), ruggedness, latitude, and longitude. Specifically, controlling for the distance to coast allows for the impact of coast proximity to change after the reform, as cities closer to the coast are more likely to benefit from China's WTO accession in 2001 by better access to world markets. Terrain ruggedness could be related to the distribution of SOEs because some SOEs were constructed in the rugged regions of inner provinces (in Northwestern and Southwestern China) for national defense considerations (Naughton, 1988). Finally, controlling separately for longitude and latitude allows for spatial patterns in economic changes that may be correlated with a household's decision to have a baby.

Our specifications also control for other city characteristics flexibly that may predict differential fertility responses. The first set of controls covers fertility policy variables ($X_{p,t}^{pol}$), including fines for extra births, bonuses, and premium to one-child families. These time-variant variables at the province level are used to address the concern on differential implementations of the OCP across regions. Second, we control for the interactions of city-level socioeconomic variables ($X_{c,1994}^{soc}$) with the post-reform dummy, $Post_t$, including average nighttime lights in 1994 and the number of newly married couples in 1994. Nighttime lights capture the economic development levels and population density. Controlling for the interactions of pre-reform socioeconomic variables allows for differential fertility changes associated with initial socioeconomic conditions. Third, we control for city-level time-variant demographic variables ($X_{c,t}^{dem}$), including the number of women of childbearing age and their average education. We control for time-invariant average nighttime lights and the number of newly married couples in 1994 (interacted with the post dummy) because they might react to the reform, whereas the number of women of childbearing age and their average education are predetermined. Our results are similar if we control for time-variant average nighttime lights and the number of newly married couples instead of their interactions with the post dummy.

We also include a full set of city fixed effects (μ_c) and year fixed effects (λ_t) in all specifications. $\varepsilon_{c,t}$ is the error term. Standard errors are clustered at the city level. We restrict the sample to cities dominated by the Han population (at least 50% of the local population)[13] given that minority cities differ substantially from Han cities in the implementation of the OCP and various economic and

[12]According to the 20% sample of the 2005 population survey, 99.9% of women give birth during 15 to 49 years old. See Section 3.2 for a discussion.

[13]Our results are robust when we improve the threshold to be 60%, 70%, 80%, or 90%.

social environments, and more than 91.51% of the population is ethnic Han in the 2005 population survey. Our sample accounts for around 89.2% of Chinese cities. All regressions are weighted by the urban population of a city in 2000.[14]

3.2 Data and variables

The data for our main analysis come from two sources. The first is the Population (mini-) Census of China, which has been conducted every five years since 1990 by China's National Bureau of Statistics (NBS). We use the 2005 mini-census, of which we have access to a 20% sample. These data are well suited for constructing our outcome variables on birth rate, marriage rate, and sex ratio, among many others. Specifically, the birth rate is defined as the number of births per 1000 women of childbearing age (aged 15 to 49 years). We select an upper age limit of 49 under the assumption that fertility is essentially complete by 49 for urban females. This assumption is motivated by the fact that less than 0.2% of urban females gave birth older than 49 according to the data from the 2005 population census of China, as shown in Figure 5. Similarly, the marriage rate is defined as the number of new couples per 1000 women of childbearing age.

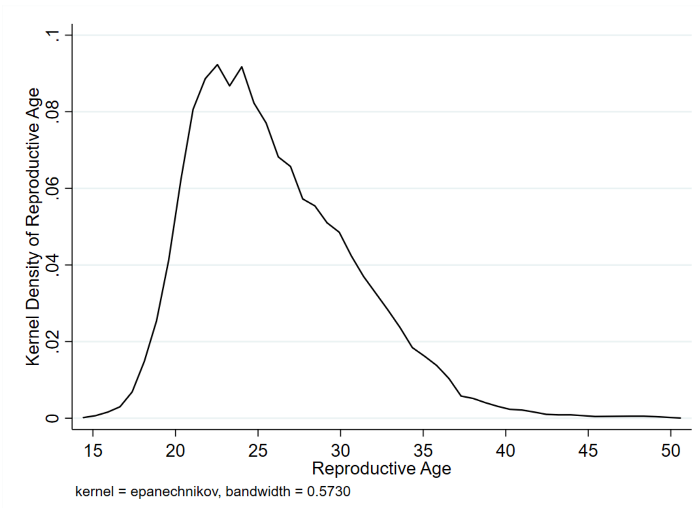


Figure 5: Distribution (kernel density) of reproductive age, 2005

Note: This graph displays the distribution (kernel density) of reproductive age on the basis of data from the mini-census of China in 2005.

Another source of data is the ASIF. The ASIF is an all-China representative firm survey run by NBS starting in 1995. In 1995, China conducted an economic census of all industrial firms (around 510,000 firms) in ASIF. However, the released data in 1996 and 1997 only include a tiny fraction of the firms. From 1998, the NBS changed the coverage of the annual survey and covered non-state firms with sales exceeding 5 million RMB and all state-owned firms (Brandt, Van Biesebroeck and Zhang, 2014).[15] These firms contribute to the majority of China's industrial sales, employment, export, and value-added. The ASIF collects detailed firm information on location, industry affiliation, ownership, capital structure, assets and debts, various dimensions of outputs, such as sales value, profits, and various dimensions of inputs such as employment and wages (Brandt, Van Biesebroeck

[14] Our results are also robust when we do not put weight in the regressions.

[15] At the exchange rate of 8.27 RMB per USD (between January 1997 and July 2005), this is equivalent to approximately 600,000 USD.

and Zhang, 2014). The ASIF data are used to calculate matrices in the national income account (e.g., GDP) and major statistics published in China's Statistical Yearbooks. We use the 1995 ASIF data to calculate the predetermined SOE employment ratio as our key independent variable, which is defined as the ratio of the total SOE employment over the urban working-age population for each city. The predetermined ratio of SOE employment, as documented in Section 2.3, shows a strong prediction of the intensity of layoffs induced by the reform across regions.

We combine various sources of data to construct control variables. The first set of controls is time-invariant geographic variables, including distance to coast (in logarithm), ruggedness, latitude, and longitude. We compute the distance from the centroid of a city to the coast in GIS software. City-level average terrain ruggedness is computed on the basis of the data from Nunn and Puga (2012). We also use GIS to obtain the latitude, and longitude of each city. The second set of controls is time-variant province-level variables that measure fertility policy, including fines for extra births, bonuses to one-child families, and whether one-child families have a premium (such as advantages in the college entrance examination). They are from Ebenstein (2010), who combines various sources of information to impute these variables at the province level. These variables are available for download at the author's website.[16] Third, for city-level socioeconomic variables, we consider average nighttime lights in 1994 and the number of newly married couples in 1994, and have the two variables interacted with the post-reform dummy ($Post_t$). Nighttime lights are from the Defense Meteorological Satellite Program's Operational Lines System. The number of new-married couples is from the 20% sample of the 2005 mini-census of population. The fourth set of controls includes the number of women of childbearing age and their average education. These demographic variables are computed using the 20% sample of the 2005 mini-census of population.

Finally, we collect information on coal mines from China Coal Industry Yearbook (1995) to construct our IV–the distance to the nearest coal mine interacted with the post-reform dummy ($Post_t$). Given that small coal mines are less likely to affect the distribution of SOEs, and they are usually adjacent to major coal mines, we only use coal mines with an annual production above 100,000 tons to construct the instrument.[17] A total of 174 of them satisfy the criteria. The summary statistics of these variables are reported in Table B.1 in Appendix B.1.

4 Effect of layoffs on fertility: results

4.1 Basic results

Table 1 displays city-level estimates from Equation 3.1. The first column shows the result from a regression including geographic controls, city fixed effects, and year dummies. The second column displays the result from adding policy variables as additional controls. In column (3), we further control for variables on socioeconomic factors and demographic factors according to our preferred specification. In the last column, we report the result from a regression including province×year fixed effects, which can help absorb omitted time-variant factors associated with local birth rates within a province.

Across the columns, the coefficients on $SOE\ ratio_{c,1995} \times Post$ remain positive and statistically significant. This result suggests that cities featured with a higher ratio of SOE workers in 1995

[16] http://pluto.mscc.huji.ac.il/ebenstein/

[17] Our results are robust when we improve the threshold to 500,000 tons.

and thus more vulnerable to the massive layoffs experienced a larger increase in birth rates in the following years. Using our preferred specification used in column (3), our estimates imply that increasing a city's SOE employment ratio from 0 to 1 in 1995 enhances birth rates by around 24 babies per 1000 women of childbearing age in a year over the post-reform period (1996-2004), or equivalently, around 60.8% (23.98/39.46) increase in birth rates compared to the average birth rates during 1990 and 2004. Considering the average SOE employment ratio in 1995 is 15%, a back-of-the-envelope calculation indicates that the layoffs increased birth rates by around 3.6‰(23.98‰ ×15%) on average per year during the post-reform periods.

Table 1: SOE Reform and Birth Rate

Dependent Var.:	Birth Rate (‰)			
	(1)	(2)	(3)	(4)
SOE ratio$_{c,1995}$ ×Post	21.71***	22.00***	23.98***	12.49**
	(4.193)	(4.162)	(4.409)	(4.890)
Geographic controls ×Post	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes	Yes
Time-variant demographic controls	No	No	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Province×Year FEs	No	No	No	Yes
Observations	4455	4455	4455	4455
R^2	0.551	0.552	0.558	0.608
Mean dependent var.	39.46	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

4.2 Event study: pre-trends and treatment effects

A potential concern with our identification strategy is that other events that occurred during the period of our study may confound our DID results. To address the concern rigorously, we employ a standard event study approach to investigate how the trends of birth rates change over time in regions with differential SOE employment ratios in the initial year. The specification is as below:

$$\begin{aligned} Birth\ rate_{c,t} = \beta_0 + \beta_{1t} \sum_{t=1990}^{2004} SOE\ ratio_{c,1995} \times Year_t + \beta_{2t} \sum_{t=1990}^{2004} X_c^{geo} \times Year_t + \\ \beta_3 X_{p,t}^{pol} + \beta_{4t} \sum_{t=1990}^{2004} X_{c,1994}^{soc} \times Year_t + \beta_5 X_{c,t}^{dem} + \mu_c + \lambda_t + \varepsilon_{c,t}, \end{aligned} \tag{4.1}$$

where $Year_t$ is a dummy variable indicating a specific year. Here we set 1995 as the reference year and thus omit it in our regressions. β_{1t} measures the effects of the SOE employment ratio in 1995 on birth rates in year t. In this specification, the time-invariant geographic and socioeconomic controls are interacted with year dummies. Other variables are the same as those in Equation 3.1.

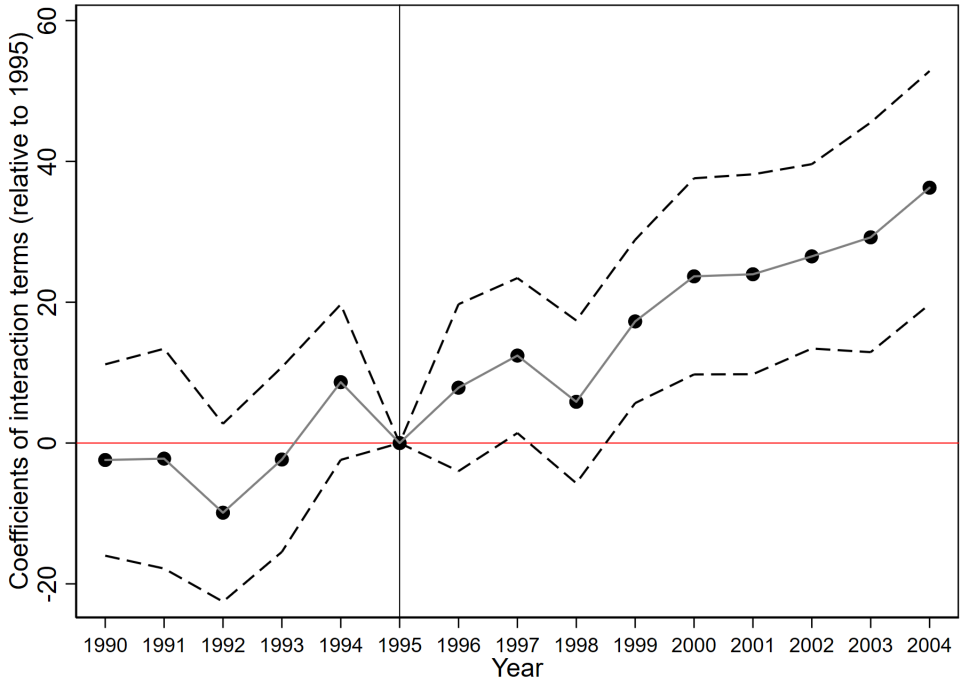


Figure 6: Common trend test, results based on column (3) of Table C.1

Considering the possible time lag between job loss and conception, new births could appear one or two years after the massive layoffs. Table C.1 displays the estimated coefficients over time, and the coefficients of column (3) are plotted in Figure 6. Throughout the period from 1990 to 1994, these coefficients are not statistically different from that of 1995, as expected. However, these coefficients become positively significant since 1997 and become increasingly larger in magnitude over time. These growing effects on birth rates appear consistent with the fact that the number of laid-off workers accumulated along with the SOE reform. Indeed, China's economic reform, including the SOE reform, is characterized by a progressive schedule instead of a shock therapy (Naughton, 1988); thus, the reform effect on fertility tends to grow over time.

4.3 Instrumental variable estimation

A primary concern for identification is the presence of omitted observables or unobservables that are correlated with both fertility and layoffs. For example, the rapid growth of China's exports, which may correlate with the initial intensity of SOE employment in 1995, may have an effect on local birth rates through employment if local export expansion induces a change in the labor market demand. To further isolate exogenous variation in the change in the reform-induced SOE layoffs from other local factors, we construct our IV on the basis of the distance to the nearest coal mine.

Our instrument exploits the fact that coal mines played a role in determining the distribution and size of SOEs in China. China's early state-owned sectors prioritized heavy industries, which rely crucially on coal as production inputs. In the "156 Program"–an unprecedented technology transfer wherein China "received the most frontier technology" from the Soviet Union in 1950s (Heblich, Seror, Xu and Zylberberg, 2019), more than 90% were heavy industries, and 25 of them are the

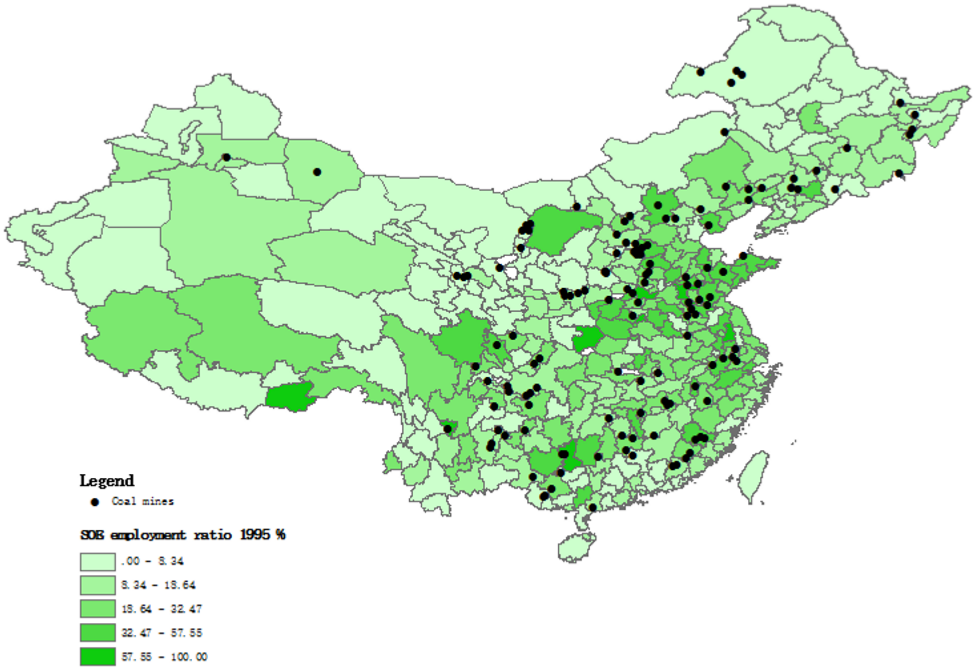


Figure 7: SOE employment ratio in 1995 and coal mines in China

Note: The black points are coal mines with an annual production of more than 100,000 tons in 1994. Data are from China Coal Industry Yearbook (1995).

constructions of coal mines. This program laid the foundation for the development of Chinese SOEs, and the impacts lasted until the early 1990s (Heblich, Seror, Xu and Zylberberg, 2019). Zhi, Ding and Liang (2016) find that the distribution of SOEs in the 1990s was highly correlated with the distribution of early SOEs, which was largely affected by the location of coal mines. In 1978, SOEs produced 85.9% of the total coal outputs, and the percentage was still as high as 76.1% in 2001 (Wright, 2006).

Another reason for making distances to the nearest coal mine relevant for the initial ratio of SOE employment in 1995 is the strategy adopted in the reform. The reform particularly bankrupted SOEs with low efficiency and/or high pollution, and the problems of excess employment and pollution were severe in these enterprises concentrated in the old industrial centers relying on coal that were developed under the Soviet model (Cao, Qian and Weingast, 1999). Indeed, the northeastern rust belt of China, as a cluster of heavy industries of that time, is one of the regions hit hardest by the reform, as discussed by Lee (2000). Consequently, a high correlation can be expected between the employment ratio of SOEs and their distance to the nearest coal mine.

Figure 7 displays the distribution of coal mines and regional SOE employment ratios in 1995 across China. Figure 8 shows the scatter plot of the distances to the nearest coal mines and the initial SOE employment ratio in 1995. As shown, cities adjacent to coal mines tend to have a higher SOE employment ratio in 1995, consistent with our observation that the distance to the nearest coal mine for a city is highly correlated to its employment ratio of SOE in the initial year.

In practice, we interact the distances to the nearest coal mine with the post dummy ($Post_t$) to instrument the key interaction of our interest, $SOE\ ratio_{c,1995} \times Post_t$. We use the following equation

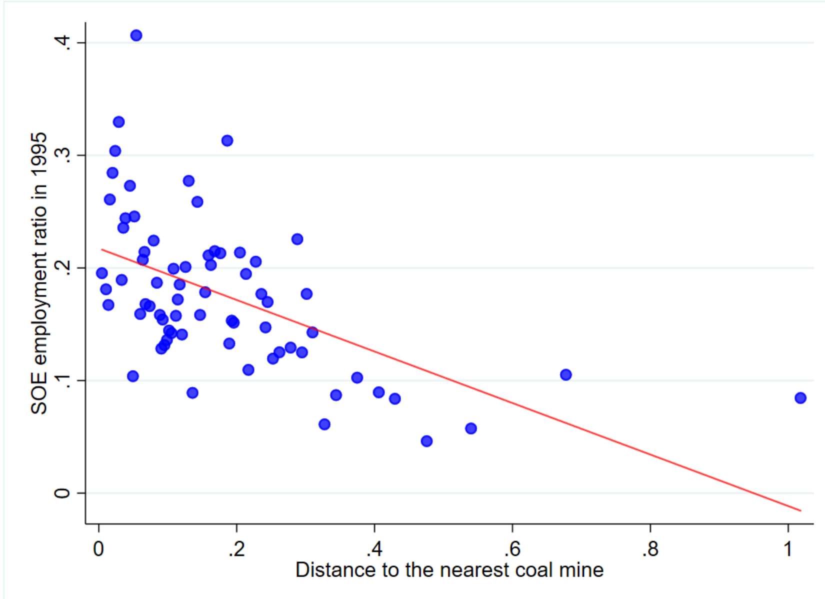


Figure 8: Distance to the nearest coal mine and SOE employment ratio in 1995

to estimate the first-stage effects of these distances on SOE employment ratios:

$$SOE\ ratio_{c,1995} \times Post_t = \beta_0 + \delta_1 Distance_c \times Post_t + \beta_2 X_c^{geo} \times Post_t + \beta_3 X_{p,t}^{pol} + \beta_4 X_{c,1994}^{soc} \times Post_t + \beta_5 X_{c,t}^{dem} + \mu_c + \lambda_t + \varepsilon_{c,t}. \tag{4.2}$$

Table 2, Panel B, presents the "first-stage" results from Equation 4.2. As estimated, a one-thousand-kilometer increase in the distance to the nearest coal mine is associated with around 0.43 decline in the ratio of SOE employment,[18] according to the estimate in column (3) of Panel B, Table 2. Throughout columns, these estimations produce KP-F statistics larger than 27, suggesting a strong predictive power of the distance to the nearest coal mine on the initial ratio of SOE employment. Next, we estimate the second-stage equation of the following form:

$$Birth\ rate_{c,t} = \beta_0 + \beta_1 \widehat{SOE\ ratio_{c,1995} \times Post_t} + +\beta_2 X_c^{geo} \times Post_t + \beta_3 X_{p,t}^{pol} + \beta_4 X_{c,1994}^{soc} \times Post_t + \beta_5 X_{c,t}^{dem} + \mu_c + \lambda_t + \varepsilon_{c,t}, \tag{4.3}$$

where $\widehat{SOE\ ratio_{c,1995} \times Post_t}$ is estimated by using Equation 4.2.

Table 2, Panel A, presents the second-stage regression results from instrumenting the initial ratio of SOE employment. The estimates are robust to adding different sets of controls. Overall, these second-stage coefficients further confirm our previous findings: cities relying more on state-owned sectors witnessed a larger increase in birth rates during the SOEs restructuring period. The magnitude of IV estimates is around 1.34 times as large as that of OLS estimates, according to the estimates in column (3). Over the period, relative to other SOEs, coal-related SOEs laid off a larger number of workers due to their inefficiency and pollution problem. Hence, one possible explanation for the larger IV estimates is the local average treatment effect.

A potential threat to our identification strategy is that distances to the nearest coal mine might

[18] or around 43% decline in the percentage of SOE employment,

Table 2: SOE Reform and Birth Rate: IV Estimation

Panel A: Second-stage estimates	Birth Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}\times$Post	33.47***	36.22***	32.02***
	(11.216)	(10.864)	(9.852)
Panel B: First-stage estimates	**SOE ratio$_{c,1995}\times$Post**		
	(1)	(2)	(3)
Distance$_c\times$Post	-0.39***	-0.42***	-0.43***
	(0.074)	(0.072)	(0.065)
Geographic controls $\times$Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls$\times$Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
KP F-statistic	27.9	34.2	43.6
R^2	0.797	0.798	0.822
Second-stage mean dependent var.	39.46	39.46	39.46

Notes: IV is the distance to the nearest coal mine interacted with the post-reform dummy. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

impact birth rates through channels other than SOE reform. We conduct several exercises to check against this possibility. First, we investigate the relationship between distances to the nearest coal mine and birth rates during the pre-reform period (1990-1995). Table D.1 demonstrates no significant correlation between the two variables. After adding all the baseline controls in column (3), the magnitude of the coefficient becomes negligible. Second, we regress a set of geographic and socioeconomic variables in 1995 on the distance to the nearest coal mine. As displayed in Table D.2, most of the estimates show no statistically significant correlation with the distance. Third, we add our instrument in Equation 3.1 to compare the coefficient on our instrument (Distance$_c\times$Post) and the coefficient on our main explanatory variable (SOE ratio$_{c,1995}\times$Post). Column (1) of Table D.3 suggests that our instrument negatively predicts birth rates. However, as shown in column (2), the predictive power of the instrument becomes much weaker when the interaction of the SOE employment ratio and the post dummy is controlled. This pattern remains consistent as we add more controls. These results imply that our instrument may only predict the birth rate through the SOE employment ratio and its effect is absorbed when the main explanatory variable is controlled.

Recall that we only consider cities dominated by Han population (cities with at least 50% Han population) in our baseline estimation 3.1. We find that SOE ratio$_{c,1995}\times$Post does not predict birth rates in cities with a minority rate higher than 50%. This might be because cities dominated by minorities are different from Han cities in terms of the implementation of the OCP and other economic and social environments. Here, we examine the role of our instrument in affecting birth rates in both types of cities. As shown in Table D.4, our instrument predicts SOE ratio$_{c,1995}\times$Post in both regions.

It also directly predicts birth rates in regions where SOE ratio$_{c,1995}\times$Post predicts birth rates (minority rate lower than 50%), but it does not predict birth rates in regions where minority rate is higher than 50%. This evidence supports the validity of the instrument. Suppose another channel through which our instrument affects birth rates for regions with a minority rate higher than 50%, a negative correlation can be expected between the instrument and birth rates even when SOE ratio$_{c,1995}\times$Post plays no role in birth rates. However, column (4) of Table D.4 demonstrates an insignificant positive relationship (instead of a negative relationship), dismissing the conjecture.

Overall, these four exercises bolster our confidence that the exclusion restrictions of this IV should be satisfied. Distance to the nearest coal mine provides an attractive source of variation to investigate how the massive layoffs affect birth rates during the SOE reform period.

4.4 The effects of layoffs by gender

4.4.1 Individual-level analysis

We further explore whether the effects are driven by laid-off women or their husbands using individual-level data. China Health and Nutrition Survey (CHNS) contains information on respondents' jobs across waves, which can be used to determine whether a worker was laid off during a certain period. We consider the first six waves of the survey (1989, 1991, 1993, 1997, 2000, 2004) to make the sample almost consistent with our baseline estimation. Regressions are performed on the basis of the following equation:

$$New\ Birth_{w,t\ to\ t+1} = \beta_0 + \beta_1 Laidoff_{w,t} + \beta_2 Laidoff_{h,t} + \beta_3 X_{w,t} + \beta_4 X_{h,t} + \mu_w + \lambda_t + \varepsilon_{w,t}, \quad (4.4)$$

where $New\ Birth_{w,t\ to\ t+1}$ is the number of new births for woman w, during survey year t to $t+1$, we consider a one-year lag to allow the time of pregnancy; $Laidoff_{w,t}$ is a dummy indicating whether a woman previously worked in a SOE, but not during the year of the survey (year t);[19] $Laidoff_{h,t}$ is a dummy indicating whether the woman's husband previously worked in an SOE, but not during the year of the survey; $X_{w,t}$ is a set of control variables, including dummies indicating whether the woman was working in other types of firms in year t and whether she gave birth prior to the survey year, and age fixed effects. $X_{h,t}$ indicates whether the husband was working in other types of firms, and husbands' age fixed effects; μ_w is the woman fixed effects to account for individual-level time-invariant characteristics that might affect the probability of being laid off; λ_t are survey year fixed effects. Standard errors are clustered at the individual level of women.

Only urban Han women are included because the OCP is different for minorities and rural residents. Individuals who only participated in one wave of the survey are dropped, because we need at least two waves of the information to identify whether a worker was laid off. Here, we mainly consider married women of childbearing age (aged 15 to 49 years) at the year of the survey. We also report results for all women of childbearing age in the first two columns of Table 3.[20] In columns (1) and (2), we conduct regressions by excluding husbands' information and find that being laid off is positively associated with more births for women. In columns (3) and (4), after controlling for husbands' information, women are still more inclined to give birth after job loss. By contrast,

[19] No other information can distinguish those being laid off and those who voluntarily resigned from an SOE. In light of the massive layoffs during that period and the welfare of SOEs, the probability of resignation should be small.

[20] And control for their marital status.

for husbands, dismissal from SOEs could reduce the probability of childbearing, but this result is not robust after controlling for more variables in our preferred specification in column (4).Thus, our results suggest that women's layoffs substantially increase their possibility to have a baby but probably not for men.

Table 3: The Effects of Layoffs by Gender

Dependent Var.:	Number of New Births			
	All Women (15yo-49yo)		Married Women (15yo-49yo)	
	(1)	(2)	(3)	(4)
Woman Laid off	0.045***	0.046***	0.046***	0.059***
	(0.013)	(0.012)	(0.013)	(0.012)
Husband Laid off			-0.044**	-0.021
			(0.021)	(0.016)
Controls	No	Yes	No	Yes
Female's age FEs	Yes	Yes	Yes	Yes
Husband's age FEs			Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Female FEs	Yes	Yes	Yes	Yes
Observations	6333	6333	3509	3509
R^2	0.436	0.624	0.677	0.768
Mean dependent var.	0.042	0.042	0.070	0.070

Notes: Data are from CHNS (1989, 1991, 1993, 1997, 2000, 2004). Outcome variable is the number of children born in year t and $t+1$. In columns (1) to (2), we include all women aged 15 to 49 years. In columns (3) to (4), we only consider married women aged 15 to 49 years. Woman Laid off is a dummy indicating the woman's status was laid off at the year of the survey. We consider only urban Han women. Standard errors are clustered at the individual level. * p<0.1; ** p<0.05; *** p<0.01.

4.4.2 City-level analysis

To find the differential effects of laid-off women and laid-off men, we should determine the SOE employment ratio by gender. The economic census in 1995 cannot distinguish female employment and male employment in SOEs. However, the 1% population census in 1990 contains information on individuals' jobs, but we do not know whether they are employed in SOEs or other types of firms. Combining the two datasets, we further explore evidence regarding the fertility effects of layoffs by gender.

First, using the 1% population census in 1990, we compute the urban employment ratio for females in industrial firms of each city, which is the total urban employment for females in industrial firms over working-age population of urban females. Females in agricultural sectors or service sectors were much less vulnerable to the massive layoff caused by the SOE reform. We divide all cities into two groups on the basis of whether the female employment ratio in industrial firms is above-median. The underlying assumption is, cities with an above-median female employment ratio in industrial firms in 1990 were likely to lay off more females during the SOE reform period. As a result, we should observe a stronger fertility effect in those cities. Table 4 suggests that cities with an above-median female employment ratio in industrial firms in 1990 experienced a higher increase in birth rates after the SOE reform, compared to their below-median counterparts. The coefficients in column (6) and column (3) are 30.51 and 17.29 respectively. A t-test for the difference between the two coefficients suggests that the former is statistically larger than the latter (p-value is 0.000).[21]

However, if male employment ratio is strongly correlated with female employment ratio, then the above results could still be a combined effect of laid-off females and males. In order to further distinguish the different effects by gender, we compute the male employment ratio in industrial firms of each city in 1990, which is the total male employment in industrial firms over male working-age population. To perform regressions, both the female employment ratio in industrial firms and the male employment ratio in industrial firms are interacted with the post-reform dummy and we use them to replace the interaction between the SOE employment ratio and the post-reform dummy in Equation 3.1. By doing so, we can roughly capture the effects of laid-off females and the effects of laid-off males, although they are an imperfect measure because some industrial firms were not SOEs in 1990. The results are displayed in Table 5. In cities with a higher initial female employment ratio in industrial firms, the SOE reform led to significantly higher birth rates. However, cities with a higher initial male employment ratio in industrial firms experienced insignificantly lower birth rates after the reform. These results are consistent with our conjecture that the improvement in fertility during the post-reform period is more likely to be driven by laid-off females, instead of males.

Table 4: Differential Effects by Female Employment Ratio in Industrial Firms in 1990

Dependent Var.:	Birth Rate (‰)					
Female Employment Ratio:	Ratio below Median			Ratio above Median		
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}$ $\times$Post	17.29***	17.75***	17.29***	25.22***	25.51***	30.51***
	(6.102)	(6.029)	(6.032)	(5.354)	(5.411)	(5.900)
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Policy controls	No	Yes	Yes	No	Yes	Yes
Socioeconomic controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2235	2235	2235	2220	2220	2220
R^2	0.542	0.546	0.550	0.541	0.542	0.550
Mean dependent var.	41.72	41.72	41.72	37.18	37.18	37.18

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). We divide the sample into two groups according to whether the employment ratio of females in industrial firms is below or above the median in 1990. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

5 Decomposing Fertility into Margins

5.1 Selection into marriage and earlier childbearing

Female labor force participation tends to be negatively correlated with marriage rates, especially in East Asian countries (Lee, Jang and Sarkar, 2008). This phenomenon is a consequence of opportu-

[21] The t-tests also suggest that the coefficient in column (5) is statistically larger than that in column (2), and the coefficient in column (4) is statistically larger than that in column (1).

Table 5: Differential Effects by Female Employment Ratio and Male Employment Ratio

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
Female Emp ratio$_{c,1990}$ × Post	30.08***	30.92***	27.65***
	(8.440)	(8.390)	(8.200)
Male Emp ratio$_{c,1990}$ × Post	-7.88	-8.16	-9.17
	(7.875)	(7.809)	(7.602)
Geographic controls × Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls × Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4215	4215	4215
R^2	0.551	0.552	0.556
Mean dependent var.	39.09	39.09	39.09

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). Female Emp ratio is the ratio of female employment in industrial firms in 1990. Male Emp ratio is the ratio of male employment in industrial firms in 1990. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

nity cost and social norms. Before the reform, women's labor force participation in China had been as high as 73% in 1995 (World Bank, 2017), which represents one of the highest female labor force participation rates in Asia-Pacific (MacPhail, 2017). If layoff shocks cause a withdrawal of females from labor markets, then opportunity costs of starting a marriage and giving birth would decline, which may encourage females' selection into earlier marriage and childbearing as a result.

Moreover, Chinese households share a strong cultural norm of having at least one child. The popular fertility view dating back to the 1990s is that continuing the family line is imperative and seen as a part of the traditional virtue of filial piety. Additionally, childbirth in China usually occurs in the early years of marriage as observed in many other countries. According to the data from a 20% sample of the 2005 population survey, 93.6% of urban couples have at least a child (mothers born after 1940). From 1990 to 2004, around 65.1% of the urban children were born in the first two years of marriage, and around 82.4% were born during the first five years. These facts imply that the timing of marriage is an important determinant of the arrival of the first birth in China. Therefore, we associate the number of newly married women with the extensive margin of fertility.

Table 6: Selection into Marriage

Dependent Var.:	Marriage Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}$ $\times$Post	12.69***	12.49***	11.57***
	(2.335)	(2.303)	(2.421)
Geographic controls $\times$Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls$\times$Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.334	0.337	0.341
Mean dependent var.	31.48	31.48	31.48

Notes: Marriage rate is the number of new couples per 1000 fertile women in the urban area. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

We examine whether the layoffs resulted in females' selection into earlier marriages that largely reflects the subsequently added number of first birth in a family. To test the conjecture, we estimate Equation 3.1 by replacing the birth rate with the marriage rate. Table 6 displays the results. In columns (1)-(3), these estimates suggest that cities more vulnerable to the massive layoff experienced a higher increase in marriage rates. Specifically, moving the SOE employment ratio from 0 to 1 leads to an additional presence of around 11.57 couples in 1000 fertile females every year during the post-reform period. This magnitude is not negligible given the large population in urban China. Overall, these results suggest that females' selection into marriage and motherhood contributes to the increase in birth rates.

To examine how households respond to layoffs, we estimate Equation 3.1 and replace birth rates with the proportion of women giving first birth in an age group. Figure 9 shows that the point estimates remain positive for most ages below 31 and become close to zero after that. It suggests that SOE reform mainly predicted the fertility decisions of those aged below 31. To examine the effect of SOE reform on selection into childbearing by women of different age groups, we replace birth rates with the proportion of women giving first birth earlier than certain ages to re-estimate Equation 3.1. These effects are analogous to a cumulative distribution function. if Figure 9 represents a probability density function. As shown in Figure 10, the effect of SOE reform becomes more pronounced as we add more women from age 15 to age 31. However, it declines as we consider more women aged above 31. This pattern suggests that SOE reform encouraged women's selection into earlier childbearing.

5.2 Policy enforcement: OCP punishments were less of a threat

As an important part of the public sector, China's SOEs are obligated to enforce government policies, including the well-known OCP. Relative to residents who work in non-SOEs, the imple-

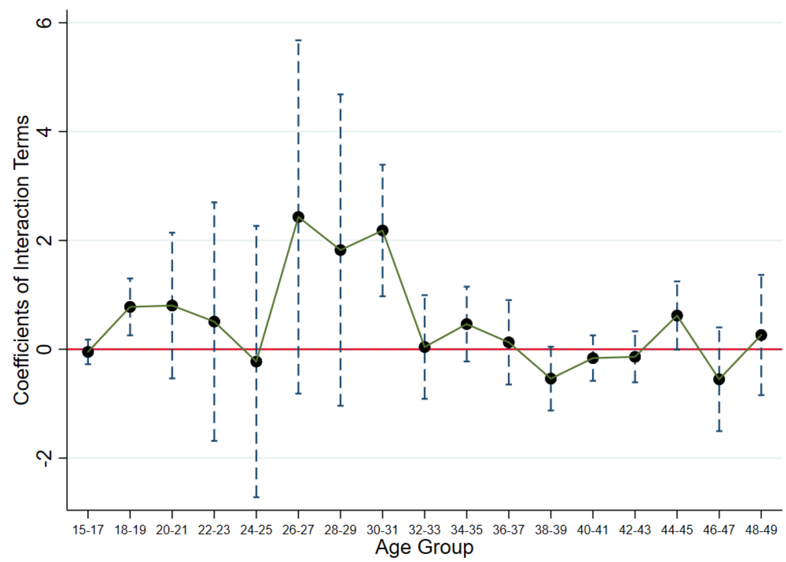


Figure 9: SOE reform and females' selection into earlier childbearing by age cell
Notes: The outcome variable is the percent of females choosing to give birth to their first child in each age group (the denominator is the number of females in that group). See Table D.5 for regression results producing the plot.

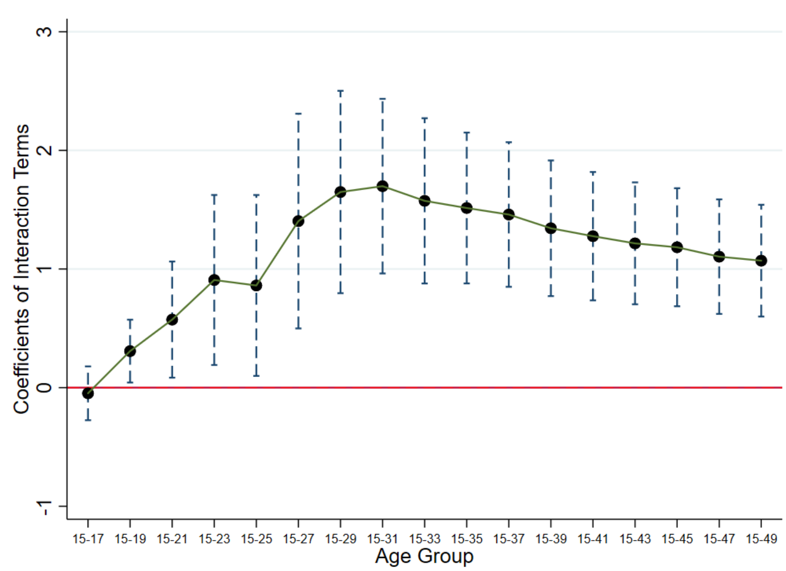


Figure 10: SOE reform and females' selection into earlier childbearing by accumulated age cell
Notes: The outcome variable is the percent of females choosing to give birth to their first child in each age group (the denominator is the number of females in that group). See Table D.6 for regression results producing the plot.

mentation of OCP for SOEs workers is more stringent for several reasons. First, the managers of SOEs have a stronger incentive to enforce the policy for promotion (Cheng, Ma, Qi and Xu, 2021), whereas the promotion in private firms is less likely to be affected by such consideration. Second, for SOE workers, various realistic threats, such as wage and/or bonus reduction, demotion, and even dismissal would be the potential results of punishment if they violate such policies (Bulte, Heerink and Zhang, 2011). Moreover, without the permission from their working units, obtaining a birth permit was difficult for SOE workers,[22] which is a requirement for legal fertility (Currier, 2008).

After being laid off from SOEs, laid-off workers, in essence, faced lower costs for having subsequent children, which would lead to a penalty for urban households under OCP. Indeed, career concerns were alleviated for those workers, although they still had to pay fines for extra births. We disentangle the effects of layoff shock and policy enforcement by linking layoffs to the rates of the births of subsequent children. Specifically, the estimated effect of the SOEs layoffs on the birth rate of the first child, which is not a violation of OCP, reflects the lower bound of the fertility effect of layoffs. By comparison, the estimated effect of the SOE layoffs on the birth rate of children with higher birth order reflects actually a combined effect of layoff shock and policy enforcement.

The first three columns of Table 7 display results from using birth rates of the first child as the outcome variable. These results suggest that SOE reform led to an increase in the births of the first child, driven mainly by layoff shock. These results lend extra support to our previous findings that layoffs increased fertility. Columns (4)-(6) display results from using birth rates of subsequent children as the outcome variables. These results suggest that birth rates of subsequent children also increased significantly after 1995 in regions with more layoffs. Actually, the coefficients on birth rates of subsequent children are higher than its mean value, whereas the coefficients on birth rates of the first child are around half of their mean values.

Table 7: First Child and Subsequent Children

Dependent Var.:	Birth Rate (‰)					
	First Child			Subsequent Children		
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}$ × Post	15.94***	15.94***	16.61***	5.77***	6.05***	7.37***
	(2.926)	(2.923)	(3.154)	(1.940)	(1.935)	(1.988)
Geographic controls × Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes	No	Yes	Yes
1994 socioeconomic controls × Post	No	No	Yes	No	No	Yes
Time-variant demographic controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4455	4455	4455	4455	4455	4455
R^2	0.420	0.421	0.425	0.632	0.636	0.644
Mean dependent var.	33.75	33.75	33.75	5.71	5.71	5.71

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

[22] The permit, which varies across regions, is connected to the medical services, allowance and benefits, employment of the mother, and the household registration of the newborn child. Waiving all of these benefits is too costly, especially the registration for the baby; therefore, most couples would manage to obtain the permit in the middle of the pregnancy. The permit is also known as a "birth service certificate" or "family planning service permit" in China.

6 Effect of Layoffs on Child Quality

Our model in Section 2 highlights the joint choice of quantity and quality of children affected by income shocks. The previous section has provided evidence that layoffs increased fertility. In this section, we empirically analyze the impact of layoffs on child quality measured by the competed schooling years, and further examine the mechanisms involved.

6.1 Long-term effect on educational attainment

To estimate the effect of layoffs on child quality, we use data from the China Health and Retirement Longitudinal Study (CHARLS) conducted in 2018. To ensure children in our example are old enough to complete education, we restrict children only to those born during 1980-2000 and thus these children were at least 18 years old in 2018 when the CHARLS was conducted.

We define the SOE children (refer to cohorts who were affected by the SOE reform) as those who were less than 5 years old during the reform period (1995-2004). This definition follows a large body of literature that finds that intervening in the zero-to-five age period was more successful at improving educational outcomes than intervention of the school-age (Campbell, Ramey, Pungello, Sparling and Miller-Johnson, 2002; Doyle, Harmon, Heckman and Tremblay, 2009).[23] Given that the SOE reform started in 1995 and the children of our interest were at least 18 years old in 2018, the SOE children are those born during 1990-2000, and children born during 1980-1990 are free of the shock at the zero-to-five age period.

We adopt a cohort DID framework to evaluate the causal effects of layoffs on educational attainment. To obtain an exogenous assignment of treatment and control groups, we use the pre-reform SOE and non-SOE status of parental work affiliation following Kong, Osberg and Zhou (2019). Thus, our treatment group consists of households with at least one parent working in the SOE sector in 1995. The empirical model is as follows:

$$Schooling\ year_{i,p,c} = \beta_0 + \beta_1 SOE_{i,p,c} \times Post_c + \beta_2 SOE_{i,p,c} + \beta_3 Control_{i,p,c} + \mu_p + \lambda_c + \varepsilon_{i,p,c}, \quad (6.1)$$

where i, p, and c refer to individual, city, and cohort (year), respectively. The outcome variable, $Schooling\ year_{i,p,c}$ is the schooling years for individual i, cohort c, and birth city p. $SOE_{i,p,c}$ is a treated group indicator. $Post_c$ is a dummy variable indicating whether cohort c is less than 5 years old in 1995 (i.e., $Post_c = 1$ for children born during 1990-2000), the initial year of the reform. μ_p and λ_c are city and cohort fixed effects, and $Control_{i,p,c}$ is a vector of individual-level controls.

We report the results from estimating Equation 6.1 in Table 8, where estimates of the variable of interest ($SOE \times Post$) are displayed for various specifications. Our estimates use data consisting of children born during 1980-2000. Among them, children born during 1990-2000 experienced the exposure of SOE reform at zero-to-five age period, the key period for improving educational outcomes. Column (1) presents the basic result, and column (2) additionally includes a set of control variables. In column (3), we further control for city fixed effects. Throughout columns, the coefficients of our interest, ($SOE \times Post$), are negative and significant. These results are consistent with the conjecture that the SOE children tend to have lower educational attainment, relative to others, suggesting that the reform-induced layoffs lowered child quality as measured by the schooling years.

[23] Duncan, Kalil, Mogstad and Rege (2022) summarize studies on the effects of investments in early childhood.

One potential concern is whether the children born during 1980-2000 are old enough to complete education. Indeed, when the survey was conducted in 2018, the youngest cohort was only 18 years old. To address the potential concern, we restrict the sample only to children born before 1998 to ensure they were at least 20 years old when surveyed. We use the sample to repeat the estimate of column (2), which is our preferred specification, and report the results in column (4). Overall, the estimates in column (4) are robust to column (2).

Columns (5)-(6) report results from estimating heterogeneous effects by SES. We divide the sample into groups of higher SES and lower SES. Higher SES (lower SES) group consists of children with at least one parent having high-school and above education (middle-school and below education). The estimate in column (6) is larger in magnitude, relative to column (5) (confirmed by a t-test). These results imply that the effect of the reform-induced layoffs is more pronounced for children with lower SES, compared with those with higher SES.

Table 8: The Effects of Layoffs on Educational Attainment

Dependent Var.:	Schooling Years					
	All Sample				Higher SES vs. Lower SES	
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ×Post	-0.799*	-0.941**	-0.779*	-0.920***	-0.475	-1.271**
	(0.439)	(0.428)	(0.444)	(0.459)	(0.583)	(0.553)
SOE	1.273***	1.278***	0.742***	1.277***	0.728*	2.001***
	(0.327)	(0.310)	(0.251)	(0.310)	(0.415)	(0.437)
Controls	No	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	No	No	Yes	No	No	No
Observations	1291	1257	1250	1207	772	485
R^2	0.054	0.214	0.386	0.389	0.172	0.225
Mean dependent var.	12.470	12.452	12.455	12.488	13.111	11.403

Notes: Data are from CHARLS 2018. Outcome variable is the schooling years of children born during 1980-2000. We divide the children in the sample into two groups; the Higher SES (lower SES) group consists of children with at least one parent having high school and above education (middle school and below education). Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

6.2 Mechanisms on deteriorated child quality

To further understand the estimated impact of layoffs on child quality, we examine the mechanism through which the reform-induced layoffs lowered child quality measured by schooling years. The potential mechanisms empirically emphasized below include: 1) the negative parental selection and 2) reduced educational investments.

6.2.1 Negative parental selection

The effects of layoffs on fertility may differ across women with differential SES. If the fertility effects of layoffs are more pronounced for women with lower SES and low SES women are more likely to have a child with lower quality at birth, then the layoffs are expected to lower child quality through a mechanism on negative parental selection.

To test the mechanism, we divide mothers into two groups according to their education (middles school and below education vs. high school and above education), and re-estimate 3.1 for each group. Table 9 reports the results. Throughout columns, the reform-induced layoffs increased birth rates for women of childbearing age with different educational attainment. Specifically, the magnitude is larger for women with middle school education or below. The estimated coefficient in column (2) is around 3.5 times larger compared with the coefficient in column (4) (the p-value of a t-test for the difference is 0.000), suggesting that the fertility effect is relatively larger for women with lower education. This result is probably because women with a high school education or above are less likely to be laid off and relatively easier to be re-employed after being laid off from SOEs,[24] or because they have a lower probability to give birth. By contrast, for primary school workers and middle school workers, the job markets are substantially tight and job vacancies in the private sector are scarcer, implying a dim career prospect after being laid off. As discussed by Lee (2000), a fair number of workers with lower education even exited the labor market.

These results suggest that the fertility impact of layoffs is more pronounced for women with lower educational attainment (lower SES). Consequently, layoffs may have lowered the quality composition of children at birth, which is crucial for educational attainment, if lower SES women are more likely to have a child with lower quality at birth.

Table 9: Heterogeneous Fertility Effects by Mothers' Educational Attainment

Dependent Var.:	Birth Rate (‰)			
Mother's Edu:	<=Middle School		>=High School	
	(1)	(2)	(3)	(4))
SOE ratio$_{c,1995}$ $\times$Post	32.62***	35.99***	10.55***	10.21***
	(6.429)	(6.430)	(3.608)	(3.912)
Geographic controls $\times$Post	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	No	Yes
1994 socioeconomic controls$\times$Post	No	Yes	No	Yes
Time-variant demographic controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Observations	4455	4455	4455	4455
R^2	0.451	0.464	0.254	0.256
Mean dependent var.	52.39	52.39	27.22	27.22

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years) with different educational attainment each year from 1990 to 2004. We divide mothers into two groups according to their education. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

6.2.2 Reduced educational investment

Educational inputs could be lower for children born during negative income shocks, which is not beneficial for educational outcomes. Using CHNS, Liu and Zhao (2014) find that parental job loss from SOEs is negatively correlated with children's daily nutrient intake and health outcomes. CHNS does not contain information on educational input. As a result, we use the China Urban

[24] According to China Labor Statistical Yearbook (1998), among the laid-off female employees in 1997, 62.4% hold a middle school degree or below and 37.7% hold a high school degree or above.

Labor Survey (CULS), conducted in 2001, the only CULS data available during the period we study (1990-2004) to examine the mechanism on educational investment. The CULS data includes variables on being retrenched and expenditure on education (in school and after school), which we use as the outcome variables.

In-school expenditure includes tuition fees and textbooks, whereas after-school expenditure mainly consists of personal tutoring and extracurricular classes. We control for several sets of fixed effects to account for children's, parents', and cities' characteristics in Table 10. The estimates in columns (1) and (2) suggest no evidence of a correlation between dismissal from SOEs and educational expenditure in school. This result is probably because most of the children in China attend public schools with relatively homogenous fees. However, columns (3) and (4) show that maternal job loss is associated with lower educational investment after school, which could widen the gap between normal children and children with a laid-off mother.

Although the average in-school educational expenditure is higher than after-school expenditure, the magnitude of the reduction in after-school expenditure caused by the layoff is economically important. A child with a laid-off mother spent around 100 RMB less on average for after-school education in 2001, or around 19.2% less compared to the mean value of after-school expenditure. Overall, these results suggest that layoff reduced educational inputs, which is not beneficial for educational attainment.

Table 10: Suggestive Evidence on Educational Investment

Dependent Var.:	Expenditure in Education			
	In School		After School	
	(1)	(2)	(3)	(4)
Mother laid off	-49.881	68.613	-283.247***	-100.154**
	(252.854)	(250.962)	(58.807)	(34.789)
Father laid off	-31.540	33.819	-209.215	-77.720
	(191.788)	(267.644)	(151.036)	(99.564)
Child's sex FEs	Yes	Yes	Yes	Yes
Child's edu FEs	Yes	Yes	Yes	Yes
Mother's age FEs	No	Yes	No	Yes
Father's age FEs	No	Yes	No	Yes
Mother's edu FEs	No	Yes	No	Yes
Father's edu FEs	No	Yes	No	Yes
City FEs	Yes	Yes	Yes	Yes
Observations	629	629	634	634
R^2	0.117	0.301	0.037	0.207
Mean dependent var.	1391.943	1391.943	519.980	519.980

Notes: Data are from CULS (2001). The outcome variable is the expenditure on education last year. We only consider families with a child attending kindergarten, primary school, or junior high school. Mother (father) laid off is a dummy indicating whether the mother (father) was laid off. Standard errors are clustered at the city level. Edu indicates years of education. * p<0.1; ** p<0.05; *** p<0.01.

7 Robustness Analysis and Additional Findings

7.1 Placebo tests

The Asian financial crisis, which broke out in 1997, coincided with the SOE reform in timing. It worsened SOEs' performance (He, Huang, Liu and Zhu, 2018), and might be an accelerator of the SOE reform. However, in addition to SOEs, exporting firms (EF) and foreign-invested firms (FIF) were also affected by the financial crisis. According to China Statistical Yearbook (1998), the growth rate of exports decreased from 21% in 1997 to 0.58% in 1998, and that of FDI decreased from 8.4% in 1997 to 0.07% in 1998. If our results are driven by the Asian financial crisis, then a more pronounced effect of the employment ratio of EF or FIF in 1995 should be expected on birth rates.

To address the concern, we calculate the employment ratio of EF in 1995 and that of FIF in the same way as we compute the SOE employment ratio.[25] Then we use the employment ratio of EF and that of FIF to replace the SOE employment ratio, separately, to estimate Equation 3.1. The results of the regressions are displayed in Table 11. Based on our preferred specification (column 3 and column 6), those two employment ratios are basically uncorrelated with the change of birth rates. It suggests that SOE reform, rather than the Asian financial crisis, played a major role in the change of birth rates during this period.

Table 11: Placebo Tests: Employment Ratios of Exporting Firms and Foreign-invested Firms

Dependent Var.:	Birth Rate (‰)					
	(1)	(2)	(3)	(4)	(5)	(6)
Export Firm Emp Ratio1995*Post	3.91	4.19	0.02			
	(3.830)	(3.812)	(4.146)			
Foreign Firm Emp Ratio1995*Post				5.39	5.68*	2.23
				(3.354)	(3.365)	(3.547)
Geographic controls ×Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes	No	No	Yes
Time-variant demographic controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4455	4455	4455	4455	4455	4455
R^2	0.541	0.542	0.547	0.542	0.542	0.547
Mean dependent var.	39.46	39.46	39.46	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). $Export firmempratio1995$ is the ratio of total employment of EF (over the urban working-age population) in 1995. $Foreign firmempratio1995$ is the ratio of total employment of FIF (over the urban working-age population) in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

[25] Total employment of EF in 1995 over the urban working-age population in 1995 and total employment of FIF in 1995 over the urban working-age population in 1995. EF are those with positive exports. Data are from ASIF.

7.2 Negative weights

Our baseline estimation is a DID with multiple groups and time periods, controlling for two-way fixed effects. De Chaisemartin and d'Haultfoeuille (2020) point out that this method actually estimates weighted sums of the treatment effect in each group and period, and the weights could be negative. As a consequence, we could obtain a negative coefficient in those regressions while the treatment effect is positive in every group and time period. To test this possibility, we first compute the weights implied in the decomposition of the DID estimator.[26] It estimates a weighted sum of 2610 ATTs (average treatment on the treated).[27] A total of 981 ATTs receive a positive weight, and 1629 ATTs receive a negative weight. It suggests that negative weights could be a concern in our previous analysis. Second, we obtain the ratio of the absolute value of the expectation of our DID estimator to the standard deviation of the weights, which can be used to assess the robustness of the DID estimator. It is 11.36, not close to 0, indicating that negative weights are less of a threat to our DID estimator.

Furthermore, we use the new estimator (DID_M) proposed by De Chaisemartin and d'Haultfoeuille (2020) to address the negative weights issue.[28] As shown in Table D.8, the coefficients remain positive and their magnitude is similar to our baseline estimates, suggesting that the negative weights do not lead to serious bias in our previous results. The new estimates are less precise and have increased standard errors for three reasons (see more detailed discussion in Chareyron, Goffette-Nagot and Letrouit (2020)). First, it uses data aggregated at the level of prefecture $\times$ pair of years. Second, some prefectures are not taken into account in the estimation because of the more demanding requirement of the new method. Third, the DID estimation with two-way fixed effects obtains the lowest variance estimates, based on the Gauss-Markov theorem.

7.3 Misreporting and migration

Another potential concern is the underreporting of newborns due to the OCP, which could confound our estimates if regions with fewer SOEs hide more babies after the reform. However, this phenomenon is only present for extremely young kids (Zeng, Tu, Gu, Xu, Li and Li, 1993). Moreover, considering that hukou (a system of household registration used in China) is a requirement for school attendance that usually starts at age 6 in urban China, estimates based on a sample of children who are at least 6 years old can help address the issue of underreporting of the newborns. Hence, we run regressions using the 2010 census sample that consists of children who were at least 6 years old at the time of the survey. Specifically, we perform the same regression as Equation 3.1 but estimate all the demographic variables (such as birth rate, number of fertile women, etc.) on the basis of the 2010 sample census. The results are reported in the first 3 columns of Table D.7. One can observe similar empirical results as our baseline estimation.

Migration is another concern that could bias our results. Yet, under China's hukou system, the migration barrier is substantial over the period we study, making migration difficult (Démurger, 2015). Indeed, according to our 2005 mini-census, 90.3% of the urban population live in their hukou registered counties (no information on birthplace). Moreover, according to the 2010 census, 86.2% of the urban population live in their hukou registered counties and 73.3% live in their birth counties.

[26] Using the *twowayfeweights* Stata package.

[27] We have a large amount of ATTs because our treatment variable is continuous.

[28] We use the *did_multiplegt* Stata package to run the regressions.

This evidence suggests that the majority of the population do not migrate.

To address the migration concern, we take two strategies in general. First, regressions throughout the paper are run with data constructed using hukou registered place (instead of the place of residence). Second, we use only a sample of local residents (people living in their birthplaces) in the 2010 sample census as a robustness check.[29] Column (4) to (6) of Table D.7 present the results. These results are reasonably close to our previous estimation and demonstrate that our findings are less likely to be driven by migrants.

7.4 Additional controls on age structure and non-SOE employment ratio

There could be other factors driving our estimation. First, age structure, as an omitted variable, might play a role if a large number of women entered childbearing ages around 1995 in regions with more SOEs. To mitigate the concern, we control for fractions of women in six age groups (15-19, 20-24, 25-29, 30-34, 35-39, 40-44). As shown in Table D.11, the estimates are similar to our previous results. Second, the SOE employment ratio could be related to the non-SOE employment ratio, if private firms struggle to survive in regions dominated by SOEs, or developed regions have more firms including both SOEs and non-SOEs. To alleviate the concern that non-SOEs could impact our results, we control for the non-SOE employment ratio[30] in 1995 to test the robustness of our results. Table D.12 suggests that the coefficients of the SOE employment ratio are reasonably close to our baseline estimation. However, we also find that the non-SOE employment ratio basically has no predictive power on birth rates, which demonstrates that our results are driven by SOE reform, instead of the employment ratio of other types of firms.

7.5 Alternative measures

As we have verified, the SOE employment ratio in 1995 strongly predicts a change in the layoff ratio in later periods. However, it is not a direct measure of layoff. We propose an alternative measure, which is the layoff ratio based on the change in SOEs' total employment between 1995 and 2004 as the following equation.

$$Layoff\ Ratio_c = \frac{(SOEemp_{c,1995} - SOEemp_{c,2004})}{Urban\ Working\ age\ Pop_{c,1995}}, \tag{7.1}$$

where $LayoffRatio_c$ is the layoff ratio in city c during 1995 to 2004; $SOEemp_{c,1995}$, $SOEemp_{c,2004}$ are total SOE employment in 1995 and 2004, separately; $Urban\ Working\ age\ Pop_{c,1995}$ is urban working-age population in 1995.

A potential issue of this measure is that it is also affected by SOE employment in 2004, which is endogenous. Consequently, we construct a Bartik index as another measure. In doing so, we first compute the share of SOE workers in each industry (two-digit, 40 industries) in 1995 for each city on the basis of ASIF. Second, we compute the change in SOE employment share in each industry from 1995 to 2004 at the national level. The Bartik index is computed as below:

[29] The 2005 mini-census does not contain information on birthplace.

[30] Total employment of non-SOEs in 1995 over the urban working-age population in 1995.

$$Bartik_c = \sum_{i=1}^{n} SOE\ emp\ share_{i,c,1995} \times \triangle SOE\ emp\ share_i,$$

where $SOE\ emp\ share_{i,c,1995}$ is SOEs' employment share in industry i, city c in 1995. $\triangle SOE\ emp\ share_{i,t}$ is the change in SOEs' employment share in industry i between 1995 and 2004.

For these two alternative measures, the results are reported in Table D.9 and Table D.10, respectively. Both of them show positively significant coefficients. Specifically, Table D.9 demonstrates very similar estimates to our baseline regressions in terms of magnitude and direction, thereby strengthening the validity of our previous findings.

7.6 Births to unmarried women and sex ratio of the newborn

Massive layoffs could lead to worse economic conditions and social instability, which are involved with more births to unmarried women (Kearney and Levine, 2012). In order to test this possibility, we compute the births to unmarried women. The results for the premarital birth rate are reported in the first three columns of Table 12. As expected, the premarital birth rate is higher after the reform in regions with a high SOE employment ratio in 1995. Premarital births account for around 16% (3.82/23.98) of the increase in the birth rate after the reform based on our preferred specification in column 3.

Sex selection is an important issue in the birth rate in China (Almond, Li and Zhang, 2019). On the one hand, sex selection could be more common prior to the reform when OCP policies are better implemented in SOEs. People probably are more inclined to select a male baby when they only plan to have one child. On the other hand, sex selection might occur more frequently after the reform if households use extra birth to have a son. We investigate the relationship between the SOE reform and the sex ratio of the newborn with an agnostic view. The estimates are presented in columns (4) to (6) of Table 12. The coefficients are very close to zero across specifications, which indicates basically no impact caused by the reform.

Table 12: Effect on Premarital Birth Rates and Sex Ratio

Dependent Var.:	Premarital Birth Rate			Fraction of Male Babies		
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}$ $\times$Post	3.20***	3.14***	3.82***	-0.01	-0.01	-0.01
	(0.874)	(0.891)	(0.941)	(0.029)	(0.029)	(0.031)
Geographic controls $\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes	No	Yes	Yes
1994 socioeconomic controls$\times$Post	No	No	Yes	No	No	Yes
Time-variant demographic controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4448	4448	4448	4448	4448	4448
R^2	0.385	0.386	0.391	0.093	0.094	0.095
Mean dependent var.	3.71	3.71	3.71	0.52	0.52	0.52

Notes: Premarital birth rate is the number of births to unmarried women per 1000 fertile women in the urban area. The fraction of males measures the sex ratio of the newborn. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

8 Final Remarks

The relationship between layoffs and fertility has received substantial attention in the literature. However, establishing causality is challenging, because households' choice of fertility may relate closely to unobservable characteristics that are determinants of unemployment risk. The restructuring of SOEs starting from the mid-1990s provides a unique opportunity for exploring the causality. The restructuring caused a rare, large-scale, and less-selective shock of unemployment. Taking advantage of the restructuring as a natural experiment, we investigate how layoffs affect fertility along the intensive and extensive margins. To estimate the effects, we exploit the predetermined initial ratio of SOE employment that captures the intensity of unemployment induced by the reform.

The DID estimation suggests that layoffs induced by the reform increased birth rates by around 3.6‰, and the results are more likely to be driven by retrenched females, instead of males. These results remain robust under IV estimates and various robustness tests and receive further support from placebo tests. Further analysis suggests that the fertility-enhancing effects arise mainly from two margins: first, at the extensive margin, females selected into earlier marriage and motherhood. Second, at the intensive margin, we find an increase in the births of subsequent children, driven at least in part by the declining cost of violating the OCP after being laid off from the SOE sector.

Following the theoretical framework of quantity and quality trade-off, we also examine how layoffs affect child quality and the mechanisms involved, motivated by a growing literature that highlights households' demand for child quality and the importance of endowment at birth and early-life investment for educational attainment. We find that layoffs lowered child quality, and the deteriorated quality of child is driven mainly by two mechanisms involving negative selection into motherhood and reduced educational inputs during the layoff.

These results indicate that layoffs affect both the quantity and quality of children in a family. Previous studies looking at either quantity or quality can only provide a partial picture on the effect of an unemployment or income shock, and thus our findings highlight the importance of examining the joint choice of quantity and quality following a layoff. Indeed, negative income shocks affect not only the size but also the composition of the involved cohorts in urban China.

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Appendices for Online Publication Only

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A Results for Validation Test

Table A.1: Validation Test

Dependent Var.:	Layoff Ratio (%)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years:	1995-1998	1995-1999	1995-2000	1995-2001	1995-2002	1995-2003	1995-2004
SOE ratio$_{c,1995}$	0.564***	0.652***	0.713***	0.770***	0.788***	0.822***	0.897***
	(0.069)	(0.058)	(0.064)	(0.066)	(0.060)	(0.043)	(0.022)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	310	334	336	336	338	338	338
R^2	0.723	0.782	0.818	0.862	0.887	0.923	0.948
Mean dep. var.	0.060	0.067	0.085	0.106	0.113	0.124	0.136

Notes: OLS regressions at the city level. Layoff ratio during 1995 and 1998 is measured by: (SOE employment in 1995-SOE employment in 1998)/urban working-age population in 1995. SOE ratio 1995 is the ratio of SOE employment (over the urban working-age population in each city) in 1995. Controls are distance to coast in logarithm, ruggedness, latitude, longitude and urban working-age population in 1995. Robust standard errors are in parenthesis. * p<0.1; ** p<0.05; *** p<0.01.

Table A.1 reports the results from regressing the reduction in SOE employment ratio between 1995 and year t on the initial SOE employment ratio in 1995 and controls at the city level. The results confirm the positive relationship between the initial SOE employment ratio in 1995 and the subsequent change in layoff ratio in year t relative to 1995.

From columns (1) to (7), we report the estimates year by year in Table A.1. Throughout columns, the positive significant coefficients strongly suggest that the initial intensity of SOE employment is a powerful predictor of the layoff induced by SOE reform. Second, the estimated coefficients grew over time, suggesting that the unemployment consequence of the SOE reform tends to accumulate throughout the period from 1995 to 2004. By 2004, the layoff ratio increased to 89.7% relative to 1995, according to the estimates in column (7).

The strong positive correlation suggests that the initial share of SOE employment is highly correlated with the shock intensity of reform-induced layoffs over the reform period at the level of the prefecture. This finding is consistent with our assumption that regions with a higher initial share of SOE employment were hit harder during the restructuring of SOEs. Thus, a city's relative reduction in the SOE employment in a post-year is well explained by its initial SOE ratio, and the explanatory power increases over time. Hence, there was little room for local discretion in the unemployment induced by the reform, and the initial ratio of SOE employment is a relatively exogenous measure for the layoff shock induced by the SOE reform.

B Summary Statistics

B.1 Summary Statistics

Table B.1: Summary Statistics

	High SOE Employment Ratio			Low SOE Employment Ratio		
Variables	Count	Mean	SD	Count	Mean	SD
Birth Rate	2235	37.35	15.02	2220	41.58	17.93
Marriage Rate	2235	31.60	11.75	2220	31.36	11.46
Birth Rate (first child)	2235	33.04	12.68	2220	34.46	13.39
Birth Rate (extra children)	2235	4.31	5.62	2220	7.12	8.76
SOE Employment Ratio	2235	0.29	0.14	2220	0.08	0.04
Dist to coast (in logarithm)	2235	12.47	1.14	2220	12.64	1.62
Ruggedness	2235	2.17	2.13	2220	2.53	1.71
Latitude	2235	32.93	5.25	2220	33.97	8.12
Longitude	2235	114.77	6.68	2220	112.22	9.35
Premium	2235	0.92	0.27	2220	0.93	0.25
Fine	2235	2.24	1.15	2220	2.32	1.24
Bonus	2235	927.72	359.51	2220	1142.13	541.93
Average Night Lights	2235	3.39	3.92	2220	3.46	6.62
Number of New Couples	2235	6825.50	4589.43	2220	12891.89	17250.17
Number of Fertile Female	2235	215195.53	144070.71	2220	448272.75	827323.86
Mean Edu of Fertile Female	2235	3.65	0.25	2220	3.60	0.29
Dist to the Nearest Coal Mine	2235	0.13	0.09	2220	0.18	0.13

Notes: We divide the sample into two groups according to whether the SOE employment ratio in 1995 is above median. Average night lights, number of newly married couples, and distance to the nearest coal mine are in 1994. Distance to coast, ruggedness, latitude, and longitude are time-invariant. The unit of distance to the nearest coal mine is 1000 km.

C Common Trend Test

Table C.1: Common Trend Test

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}$ × Year1990	-2.41	-2.44	-4.70
	(6.904)	(6.969)	(7.545)
SOE ratio$_{c,1995}$ × Year1991	-2.21	-2.24	-4.55
	(7.931)	(8.006)	(8.481)
SOE ratio$_{c,1995}$ × Year1992	-9.90	-9.92	-11.32*
	(6.419)	(6.511)	(6.771)
SOE ratio$_{c,1995}$ × Year1993	-2.34	-2.17	-3.00
	(6.666)	(6.747)	(7.157)
SOE ratio$_{c,1995}$ × Year1994	8.65	8.52	7.24
	(5.610)	(5.610)	(5.889)
SOE ratio$_{c,1995}$ × Year1996	7.86	7.86	8.79
	(6.023)	(6.024)	(6.446)
SOE ratio$_{c,1995}$ × Year1997	12.43**	12.63**	13.54**
	(5.591)	(5.571)	(5.932)
SOE ratio$_{c,1995}$ × Year1998	5.86	6.16	4.72
	(5.871)	(5.865)	(6.077)
SOE ratio$_{c,1995}$ × Year1999	17.27***	17.62***	18.86***
	(5.892)	(5.882)	(6.219)
SOE ratio$_{c,1995}$ × Year2000	23.68***	24.02***	24.60***
	(7.078)	(7.079)	(7.485)
SOE ratio$_{c,1995}$ × Year2001	23.97***	24.31***	23.78***
	(7.212)	(7.212)	(7.532)
SOE ratio$_{c,1995}$ × Year2002	26.52***	26.86***	28.16***
	(6.654)	(6.663)	(7.069)
SOE ratio$_{c,1995}$ × Year2003	29.22***	29.56***	31.86***
	(8.285)	(8.284)	(8.721)
SOE ratio$_{c,1995}$ × Year2004	36.27***	36.61***	37.84***
	(8.416)	(8.414)	(8.501)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.567	0.568	0.577
Mean dependent var.	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. *Year*$_t$ are dummies indicating the specific year. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights and the number of newly married couples. Demographic controls include the number of fertile women and their average education. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

D Exclusion Restrictions and Robustness Analysis

D.1 IV: Exclusion Restrictions

Table D.1: Distance to the Nearest Coal Mine and Pre-reform Birth Rate

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
Distance	7.25	7.25	-0.78
	(8.226)	(8.231)	(6.638)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes
Observations	1782	1782	1782
R^2	0.347	0.348	0.447
Mean dependent var.	46.96	46.96	46.96

Notes: Birth rate is the number of new births per 1000 fertile women. Distance is the distance to the nearest coal mine. The sample only includes the years from 1990 to 1995. We restrict cities to those with at least 50% Han population. All results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.2: Correlation with Other Variables

Dependent Var.:	Dist to Coast	Ruggedness	Latitude	Longitude	Premium	Fine
	(1)	(2)	(3)	(4)	(5)	(6)
Distance	-1.40**	0.51	0.23	0.54	-0.00	0.00
	(0.635)	(1.137)	(1.347)	(1.950)	(0.000)	(0.000)
Observations	293	293	293	293	293	293
R^2	0.745	0.517	0.968	0.941	1.000	1.000
Mean dep. var.	12.56	2.39	33.45	113.50	0.93	2.23
Dependent Var.:	Bonus	Night Light	New Couples	Fertile Women	Average Edu	
	(1)	(2)	(3)	(4)	(5)	
Distance	0.00	-2.21	-18643.14	-581972.95	-0.35	
	(0.000)	(3.642)	(14731.729)	(380501.281)	(0.225)	
Observations	293	293	293	293	293	
R^2	1.000	0.406	0.394	0.353	0.297	
Mean dep. var.	969.37	3.93	10592.59	330698.70	3.57	

Notes: Dependent variables include the full set of our controls in 1995. Distance is a prefecture's distance to the nearest coal mine. Province fixed effects are included. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.3: Distance to Coal Mine vs SOE Employment Ratio

Dependent Var.:	Birth Rate (‰)					
	(1)	(2)	(3)	(4)	(5)	(6)
Distance×Post	-13.09***	-4.98	-15.32***	-6.54	-13.77***	-3.80
	(4.547)	(4.500)	(4.615)	(4.584)	(4.382)	(4.461)
SOE ratio$_{c,1995}$×Post		20.73***		20.77***		23.17***
		(4.362)		(4.290)		(4.643)
Geographic controls ×Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	No	No	Yes	Yes	Yes	Yes
1994 socioeconomic controls×Post	No	No	No	No	Yes	Yes
Time-variant demographic controls	No	No	No	No	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4455	4455	4455	4455	4455	4455
R^2	0.542	0.551	0.544	0.552	0.549	0.558
Mean dependent var.	39.46	39.46	39.46	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women (age 15-49). Distance is the distance to the nearest coal mine. *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.4: The Role of Distance to the Nearest Coal Mine in Different Regions

	Minority<50%		Minority>50%	
Dependent Var.:	SOE ratio×Post (1)	Birth Rate (‰) (2)	SOE ratio×Post (3)	Birth Rate (‰) (4)
Distance×Post	-0.43***	-13.77***	-0.11**	6.08
	(0.065)	(4.382)	(0.052)	(8.582)
Geographic controls ×Post	Yes	Yes	Yes	Yes
Time-variant policy controls	Yes	Yes	Yes	Yes
1994 socioeconomic controls×Post	Yes	Yes	Yes	Yes
Time-variant demographic controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Observations	4455	4455	570	570
R^2	0.822	0.549	0.885	0.341
Mean dependent var.	0.11	39.46	0.07	54.46

Notes: SOE ratio is the SOE employment ratio in 1995. The birth rate is the number of new births per 1000 fertile women (age 15-49). Distance is a prefecture's distance to the nearest coal mine. From column (1) to (2), we only keep cities with minority rates lower than 50%. From columns (3) to (4), we only keep cities with minority rates higher than 50%. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

D.2 Results for Figures D.5 and D.6

Table D.5: Effects of Female's Decision into Earlier Childbearing by Age Cells

Dependent Var.:	Percent of Female Giving Birth to First Child (%)					
Female's Age:	15-17	18-19	20-21	22-23	24-25	26-27
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}\times$Post	-0.05	0.78***	0.80	0.51	-0.23	2.43
	(0.115)	(0.266)	(0.680)	(1.114)	(1.267)	(1.649)
Female's Age:	28-29	30-31	32-33	34-35	36-37	38-39
	(7)	(8)	(9)	(10)	(11)	(12)
SOE ratio$_{c,1995}\times$Post	1.82	2.18***	0.04	0.46	0.13	-0.54*
	(1.454)	(0.614)	(0.484)	(0.349)	(0.394)	(0.297)
Female's Age:	40-41	42-43	44-45	46-47	48-49	
	(13)	(14)	(15)	(16)	(17)	
SOE ratio$_{c,1995}\times$Post	-0.16	-0.14	0.62*	-0.55	0.26	
	(0.212)	(0.238)	(0.319)	(0.484)	(0.561)	
Geographic controls $\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	Yes	Yes	Yes	Yes	Yes	Yes
1994 socioeconomic controls$\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4448	4448	4448	4448	4448	4448

Notes: The outcome variable is the percentage of females choosing to give birth to their first child in each age group (the denominator is the number of females in that group).

Table D.6: Accumulated Effects of Female's Decision into Earlier Childbearing by Age Cells

Dependent Var.:	Percent of Female Giving Birth to First Child (%)					
Female's Age:	15-17	15-19	15-21	15-23	15-25	15-27
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}\times$Post	-0.05	0.31**	0.57**	0.91**	0.86**	1.40***
	(0.115)	(0.135)	(0.249)	(0.364)	(0.387)	(0.460)
Female's Age:	15-29	15-31	15-33	15-35	15-37	15-39
	(7)	(8)	(9)	(10)	(11)	(12)
SOE ratio$_{c,1995}\times$Post	1.65***	1.70***	1.57***	1.51***	1.46***	1.34***
	(0.433)	(0.374)	(0.354)	(0.323)	(0.310)	(0.290)
Female's Age:	15-41	15-43	15-45	15-47	15-49	
	(13)	(14)	(15)	(16)	(17)	
SOE ratio$_{c,1995}\times$Post	1.28***	1.22***	1.18***	1.10***	1.07***	
	(0.275)	(0.261)	(0.253)	(0.245)	(0.239)	
Geographic controls $\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	Yes	Yes	Yes	Yes	Yes	Yes
1994 socioeconomic controls$\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4448	4448	4448	4448	4448	4448

Notes: The outcome variable is the percentage of females choosing to give birth to their first child in each age group (the denominator is the number of females in that group).

D.3 Robustness Analysis

Table D.7: Estimates Using 2010 Sample Census

Dependent Var.:	Birth Rate (‰)					
	Full Sample			Local Residents		
	(1)	(2)	(3)	(4)	(5)	(6)
SOE ratio$_{c,1995}$ $\times$Post	14.36**	15.12**	29.78***	22.74***	23.09***	25.28***
	(6.404)	(6.366)	(8.711)	(5.249)	(5.213)	(5.739)
Geographic controls $\times$Post	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes	No	Yes	Yes
1994 socioeconomic controls$\times$Post	No	No	Yes	No	No	Yes
Time-variant demographic controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4455	4455	4455	4451	4451	4451
R^2	0.551	0.552	0.584	0.580	0.581	0.588
Mean dependent var.	45.37	45.37	45.37	48.51	48.51	48.51

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). From columns (1) to (3), we include all urban population. From columns (4) to (6), we only consider local residents (migrants are dropped). *SOE ratio*$_{c,1995}$ is the ratio of the SOE employment in 1995. It is standardized. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.8: DID$_M$ *Estimator*

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}$ $\times$Post	20.29*	20.29*	17.68
	(11.55)	(12.04)	(15.09)
Geographic controls $\times$Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls$\times$Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Mean dependent var.	39.46	39.46	39.46

Notes: The estimators are computed using the did_multiplegt Stata package. The birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level (on the basis of 100 bootstrap replications). * p<0.1; ** p<0.05; *** p<0.01.

Table D.9: Layoff Ratio and Birth Rate

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
Layoff Ratio×Post	21.43***	21.81***	22.85***
	(4.742)	(4.709)	(4.955)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.549	0.550	0.555
Mean dependent var.	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). Layoff Ratio is measured by: (SOE employment in 1995-SOE employment in 2004)/urban working-age population in 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.10: Bartik Index

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
Bartik Index×Post	2.16***	2.12***	2.46***
	(0.599)	(0.600)	(0.620)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.546	0.546	0.553
Mean dependent var.	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.11: Age Structure

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}$ ×Post	17.23***	17.54***	18.88***
	(3.729)	(3.692)	(3.902)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Age structure	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.575	0.575	0.580
Mean dependent var.	39.46	39.46	39.46

Notes: We control for variables on age structure across the three columns, including the fraction of females aged 15-19, 20-24, 25-29, 30-34, 35-39, and 40-44 separately. The birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.

Table D.12: Non-SOEs Employment Ratio

Dependent Var.:	Birth Rate (‰)		
	(1)	(2)	(3)
SOE ratio$_{c,1995}$ ×Post	20.23***	20.26***	22.97***
	(4.183)	(4.122)	(4.299)
Non-SOE ratio$_{c,1995}$ ×Post	1.20	1.42	1.00
	(1.523)	(1.562)	(1.616)
Geographic controls ×Post	Yes	Yes	Yes
Time-variant policy controls	No	Yes	Yes
1994 socioeconomic controls×Post	No	No	Yes
Time-variant demographic controls	No	No	Yes
Year FEs	Yes	Yes	Yes
City FEs	Yes	Yes	Yes
Observations	4455	4455	4455
R^2	0.551	0.552	0.558
Mean dependent var.	39.46	39.46	39.46

Notes: Birth rate is the number of new births per 1000 fertile women (aged 15 to 49 years). *SOE ratio*$_{c,1995}$ is the ratio of SOE employment in 1995. *NonSOE ratio*$_{c,1995}$ is the ratio of non-SOE employment in 1995. Post is a dummy indicating years after 1995. Geographic controls include distance to coast, ruggedness, latitude, and longitude. Policy controls include fines for extra births, bonuses, and premium to one-child families. Socioeconomic controls include average nighttime lights, number of newly married couples, number of fertile women, and their average education. We only keep cities with at least 50% Han population. The results are weighted by urban population. Standard errors are clustered at the city level. * p<0.1; ** p<0.05; *** p<0.01.