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Estimating the Effect of the One-Child Policy on the Sex Ratio Imbalance in China: Identification Based on the Difference-in-Differences

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Abstract In China, the male-biased sex ratio has increased significantly. Because the one-child policy applies only to the Han Chinese but not to minorities, this unique affirmative policy allows us to identify the causal effect of the one-child policy on the increase in sex ratios by using a difference-in-differences (DD) estimator. Using the 1990 census, we find that the strict enforcement of the one-child policy led to 4.4 extra boys per 100 girls in the 1980s, accounting for about 94% of the total increase in sex ratios during this period. The robust tests indicate that the estimated policy effect is not likely confounded by other omitted policy shocks or socioeconomic changes. Moreover, we conduct the DD estimation using both the 2000 census and the 2005 mini-census. Our estimates suggest that the one-child policy resulted in about 7.0 extra boys per 100 girls for the 1991–2005 birth cohorts. The effect of the one-child policy accounts for about 57% and 54% of the total increases in sex ratios for the 1991–2000 and 2001–2005 birth cohorts, respectively.

Keywords One-child policy · Sex ratio imbalance · Difference-in-differences estimator

Introduction

According to three recent waves of population censuses in China, the sex ratio at birth has drastically increased from 108.5 in 1982 to 113.8 in 1990 and

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119.9 in 2000, deviating far from the biologically stable range of 103 to 107 (NBS 2002).[1] Because of the far-reaching consequences of the persisting sex ratio imbalance on both the marriage market and the labor market (Angrist 2002; Becker 1991; Chiappori et al. 2002; Rao 1993), within the household (Porter 2007, 2008; Wei and Zhang 2008), and even on crime (Dreze and Khera 2000; Edlund et al. 2007; Hudson and Boer 2002),[2] the problem of the male-biased sex ratio in China has drawn increasing attention from demographers, economists, and other social scientists (e.g., Banister 2004; Coale and Banister 1994; Hesketh and Xing 2006; Qian 2008; Tuljapurkar et al. 1995; Zeng et al. 1993).

However, the cause for the increase in the sex ratio has been a subject of heated debate. Oster (2005) raised the possibility that a biological factor, hepatitis B, accounts for about 70% of the “missing women” in China, an explanation that Das Gupta (2005) questioned. Lin and Luoh (2008) showed direct micro evidence that hepatitis B does not affect the sex ratio in Taiwan. Oster and Chen (2008) also conceded that hepatitis B does not explain male-biased sex ratios in China. Although the biological factor of hepatitis B seems to have been excluded from this “missing women” story originally uncovered by Sen (1990), the debate on the cause for the increase in sex ratios has not totally resolved. Many economists and demographers have argued that the one-child policy has been the major reason for the increase in sex ratios in China (Das Gupta 2005; Ebenstein 2010; Li 2002; Zeng et al. 1993). Guilmoto (2009) surmised that the increase in the sex ratio in Asia may be triggered by the progress in gender-selection technology. Qian (2008) claimed that a substantial percentage of the increase in the sex ratio is attributable to the gender wage gap.

In this article, we argue that the increase in the sex ratio in China has been the result of a combination of son preference, the progress of gender-selection technology, and a decrease in fertility induced by the one-child policy. Son preference is the cultural background; gender selection, such as gender-selective abortion, is a tool through which a desired gender can be pursued; and the decrease in fertility induced by the one-child policy leads to increasing sex ratios. Given son preference and gender-selection technology, the frequency or intensity of active gender selection is different at different fertility levels. The increase in sex ratios is a manifestation of the decline in fertility caused by the one-child policy. Because of the combination of lower fertility and gender selection, the impact of any factor on the sex ratio is higher.[3]

Previous research has tried to identify the causal relationship between the one-child policy and the increase in the sex ratio by exploiting spatial and temporal variations in the implementation of the one-child policy. For example, Ebenstein (2010) examined data on the regional and temporal variation in fines at the provincial level for unauthorized births and found that higher fine regimes are associated with higher sex ratios but lower fertility. The spatial and temporal

[1] The sex ratio is defined as the number of males per 100 females in the reference population.

[2] It has also been suggested that the endogenized sex ratio by the son preference may systematically degenerate girls to be born in low-status families (Edlund 1999).

[3] The interaction between sex preference, fertility, and sex ratio has long been recognized in the literature (Becker 1991; Ben-Porath and Welch 1976).

variations of the one-child policy, however, may be endogenous to gender selection and sex ratios in empirical analysis.

In this article, we avoid this problem by presenting a difference-in-differences (DD) estimator based on an exogenous differential treatment between the Han and minorities under the one-child policy. In the quasi-experimental design of the DD estimator, both the ethnicity-specific heterogeneity and the effects of socioeconomic development have been eliminated. Therefore, the DD estimate is very clean, and we are able to rigorously identify the causal effect and obtain the quantitative effect of the one-child policy on the sex ratio imbalance in China.

China's one-child policy stipulates that each couple is allowed only one child. Couples are given birth quotas, and they are penalized for "above-quota" births. However, a unique feature of the one-child policy is that minority women are generally allowed to have at least two children, whereas the Han can have only one child.[4] The differential application of the one-child policy across different ethnic groups has been embodied in various regulations (Banister 2004; Hardee-Cleaveland and Banister 1988; Park and Han 1990; Scharping 2003). In addition, the motivation of the differential application of the one-child policy across ethnic groups has been evidently exogenously imposed. Therefore, the consistently differential treatment of the Han and minorities under the one-child policy provides a precious and unique opportunity to identify its causal effect on the sex ratio imbalance in China.[5]

Using the Chinese population census of 1990, we estimate the treatment effect on the probability of being a boy to be as large as 1.01 percentage points for the 1980–1990 birth cohorts. This means that the strict enforcement of the one-child policy causally increased the sex ratio by 4.4, which accounts for about 94% of the increase in sex ratios during this period. Our robust analysis indicates that the estimated treatment effect is not likely confounded by other omitted policy shocks or socioeconomic changes. We further conduct the DD estimation using both the 2000 census and 2005 mini-census. Our DD estimates suggest that the one-child policy increased the sex ratio by about 7.0 for the birth cohort of 1991–2005. The effect of the one-child policy accounts for about 57% and 54% of the increase in sex ratios for the 1991–2000 and 2001–2005 birth cohorts, respectively.

In the following sections, we describe the research background, specify our empirical strategy, introduce the data set, report our estimates of the effect of the one-child policy on sex ratios, and offer our conclusions based on the analysis.

[4] Second births were strictly forbidden at the early stage of the implementation of the one-child policy after 1979. However, Central Document 7, issued in early 1984 by the Party Central Committee, allows rural couples to have a second child if the first was a girl (Peng 1996).

[5] Two facts greatly substantiate the validity of our quasi-experimental design of the DD estimator. First, there is clear evidence that the sex ratios for minorities and the Han were very close prior to the enactment of the one-child policy but diverged significantly afterward. Second, there is little difference between Han and minorities in changes of family structure, mother's age at first birth, parental characteristics, and parental labor market behaviors during the post-treatment period. Li and Zhang (2009) constructed a similar DD estimator based on the differential treatment across ethnic groups to test the external effects of fertility behavior. Li and Zhang (2007) also exploited the differential treatments of the one-child policy between the Han and minorities to construct an instrumental variable to identify the effect of (endogenous) population growth on economic growth.

Background

The Increase in Sex Ratio in China

Although scattered historical statistics indicate that China has had a traditionally male-biased sex ratio, the sex ratio at birth was relatively stable and was only marginally higher than western countries from the foundation of the People's Republic of China in 1949 to the beginning of the one-child policy in 1979, except the 1956–1958 birth cohort, those born just prior to the Great Famine. Based on four modern waves of the Chinese population census after 1949 and two large-scale in-depth fertility surveys, Coale and Banister (1994) systematically documented sex ratios for the birth cohorts from 1936 to 1989 and convincingly demonstrated that the reported sex ratio at birth was very close to 106–107 throughout the 1960s and 1970s.[6] Figure 1 depicts the time series of sex ratios by birth cohort from 1949 to 1990 projected by the Chinese population census in 1990.[7] It confirms that the sex ratio in the pre–policy change period before 1979 was significantly lower than that in the post–policy change period. In fact, the mean value of the sex ratio for the birth cohorts from 1949 to 1978 is 106.6. Figure 1 also shows that the sex ratio increased almost monotonically after 1979 and attained an unprecedented level of 113.8 in 1990.

It has been claimed that a substantial percentage of the increase in the sex ratio is due to the gender wage gap (Qian 2008). Although the correlation between the relative female income to the total household income and the outcomes for boys and girls, including female survival rates, has been extensively investigated in the literature (Duflo 2003; Foster and Rosenzweig 1999; Rosenzweig and Schultz 1982; Thomas 1994), the magnitude of the time-series change of the gender-specific earning gap does not match the magnitude of the increase in sex ratios in China during the past three decades (Cai et al. 2008; Rozelle et al. 2002). Based on an empirical analysis on the gender earning gap in China's rural areas, Rozelle et al. (2002) concluded that "no evidence was found to suggest that the economic reform policies and market competition had led to any measurable increase or decrease in gender wage discrimination." Zhang et al. (2008) also showed that the gender earning gap has been rather stable during the past two decades in urban China, though it has experienced a marginal increase.[8] Thus, the gender-specific earning gap could account for only a small portion of the increase in sex ratios in China.

We believe that the increase in the sex ratio has been a result of a combination of son preference, the progress of gender-selection technology, and low fertility rates regulated by the one-child policy. First, son preference is a cultural background. There is no doubt that the male-biased sex ratio in China is associated with son preference, which is traditionally rooted in Chinese society. However, the traditional son preference alone cannot explain the recent increase

[6] The four modern population censuses used in Coale and Banister (1994) were conducted in 1953, 1964, 1982, and 1990, and the two in-depth fertility surveys were carried out in 1982 and 1988.

[7] This figure is very similar to Coale and Banister's (1994) Figure 2 except that their cohort analysis was conducted in a five-year moving average form to smooth out irregular disturbances.

[8] Descriptive statistics from various sources show that the ratio of female wages to male wages hovered around 80% throughout the 1980s and 1990s, which is comparable to that in the United States during the same period (Blau and Kahn 1997; Gustafsson and Li 2000; Meng 1998).

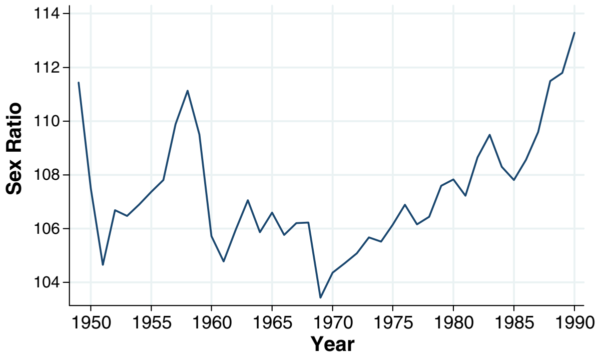

Fig. 1 Sex ratios by birth cohort. Calculations are based on the 1990 Chinese population census (1% sample)

in sex ratios because, as a preference, it is relatively stable. Second, gender-selection technology, such as gender-selective abortion, is a necessary tool to change the sex ratio. Without the progress of gender-selection technology, gender selection would be infeasible, and thus the sex ratio would not be unbalanced. For example, during the 1970s, the total fertility rate dropped from near 6 to 2.3 under the later-longer-fewer policy. However, there was not much change in sex ratios during this period. The absence of gender-selective abortion seems a plausible explanation. Furthermore, regardless of which factors are the ultimate causes of the high sex ratio, they all have to involve gender-selection technology and induced abortion.

Finally, the decrease in fertility induced by the one-child policy can itself lead to the rise in the sex ratio for the given son preference and gender-selection technology. Assuming that "at least one boy" is the preferred gender composition of children, parents can achieve their target by manipulating fertility without resorting to gender-selection technologies. For example, if the total fertility rate is higher than 5, as in the pre–policy change period from 1949 to 1979, the probability of failing to achieve the preferred gender composition target is less than 2.5%. On the other hand, a substantial percentage of parents are more likely to practice gender selection when fertility is compressed to 1 or 2 by the one-child policy, and the probability of failing to have "at least one boy" increases to 50% or 25%, respectively.

Given that both the Han Chinese and minority groups are similarly exposed to a change in gender-selection technology, the difference in the change in sex ratios between the two groups is thus ascribed to the difference in the change of fertility induced by the differential application of the one-child policy by ethnicity. In addition, the sex ratios among the Han and minorities were very similar before the one-child policy. Therefore, our DD estimate identifies the causal effect of the one-child policy on sex ratios conditional on son preference and gender-selection technology.

The One-Child Policy

After the termination of the Cultural Revolution in 1976, the Chinese leadership was shocked by the fact that, while grain production in 1977 stagnated at its 1955 level, the overall population had increased by more than 50% from 614 million in 1955 to 949 million in 1977. Thus, the fear of a Malthusian catastrophe compelled the

Communist leaders to enact a radical birth-planning policy, the one-child policy, which began in the late 1970s and is possibly the largest social experiment in human history. After its promulgation, however, the implementation of the one-child policy has exhibited three distinctive features that may have brought problems to the estimation of its demographic, social, and economic consequences.

First, instruments employed in the implementation of the one-child policy have exhibited great diversity. They range from mild methods—such as economic incentives and fines, propaganda and educational work, the effective distribution of contraceptives, and commitment and subscription of the One-Child Certification—to extreme sanctions—such as mandatory IUD insertions, the refusal of supplies of water and electricity, the unroofing of peasant families' homes for violation, and enforced abortion for above-quota pregnancies (Scharping 2003). In practice, rather than a single policy, the one-child policy appears to be a set of policies. The fine for above-quota births is just one piece in the set of instruments.

Second, the one-child policy has been implemented using variable levels of rigidity and flexibility across different regions and different years. Although the enforcement of the one-child policy began as early as 1979, the official Population and Family Planning Law was enacted as late as 2002. The one-child policy has been implemented through various executive regulations and rules. Thus, both the central and local governments are given substantial discretionary power to adjust the policy strictness in response to demographic, social, and economic changes. For example, the release of the Central Document 7 in 1984 eased the strictness of the one-child policy to a certain extent and explicitly specified exceptional conditions under which two children were allowed: based on local conditions, families in certain areas were allowed to have a second child if the first one was a girl. However, the central government late noted the laxity and a "loss of control" in the Document, and called for strict adherence to population targets. Finally, it was replaced in 1986 by Central Document 13, which is more stringent than the previous stipulation (Peng 1996; Scharping 2003).

Finally, and most importantly, the spatial and temporal variations may be endogenous to sex ratios. These variations may reflect the unobserved interaction between regional heterogeneities (e.g., son preference and socioeconomic development). The unobserved interaction may also affect gender selection and sex ratios. Based on panel data from the China Health and Nutrition Survey, Short and Zhai (1998) suggested that variations in enforcement of the one-child policy have been based on local demographic, social, and economic conditions. Li and Zhang (2008) found that the level of the fine increases with the community wealth level and the local government's birth-control incentives but decreases with the local government's revenue incentives. In particular, the spatial and temporal variations in the one-child policy have been documented as being affected by the fertility rate. As discussed earlier, the central government explicitly tightened the one-child policy in 1986 in response to high fertility, which was a result of Central Document 7 of 1984. Since fertility affects both the sex ratio and government policy, every factor that affects fertility would likely affect the sex ratio and the strictness of the one-child policy simultaneously.

In summary, the instrument-, time-, and region-varying natures of the implementation of the one-child policy make the measurement and the identification of the causal effect of the policy on the sex ratio imbalance extremely difficult, especially at the national level. However, there is a general rule that minority women were normally allowed to have at least two children until the end of the 1980s. This rule is clear-cut and uniform across regions and years. The differential application across ethnic groups has been embodied in all documents of birth-planning policies and has even been officially written into the Law on Regional National Autonomy (Peng 1996). Therefore, the differential application of the one-child policy between the Han and minorities serves as a precious and unique quasi-experiment to identify the causality between the one-child policy and the increase in sex ratios in China.

Empirical Strategy

We are interested in estimating the effect of the one-child policy on the sex ratio. However, sex ratios cannot be defined at the individual level. Therefore, we will first estimate the policy effect on the probability of being a boy at the individual level. We then translate it into the policy effect on the sex ratio at the population level by a nonlinear transformation. As discussed in previous sections, the differential application of the one-child policy between the Han and minorities serves as a quasi-experiment to identify the causal effect on the sex ratio imbalance. Since the one-child policy has been applied only to the Han, the Han Chinese serve as the treatment group and ethnic minorities are the comparison group. Let S_i be a child's gender indicator, equaling 1 if the child is a boy and 0 if the child is a girl. In addition, let H and T be the ethnicity and birth cohort indicators, respectively; $H=1$ for a Han child, and $T=1$ if the child was born in the post–policy change period (after 1979). Therefore, we have four groups: Han Chinese born before 1979, Han Chinese born after 1979, minorities born before 1979, and minorities born after 1979. The average probabilities of being a boy for the four groups can be denoted as shown in the following table:

	Han	Minority
Born Before 1979	$E(S_i \vert H=1,T=0)$	$E(S_i \vert H=0,T=0)$
Born After 1979	$E(S_i \vert H=1,T=1)$	$E(S_i \vert H=0,T=1)$

We use the following DD framework to control for systematic differences across both ethnic groups and birth cohorts. Differencing the mean value of S_i across birth cohorts and ethnic groups gives

$$\begin{aligned} DD = {} & [E(S_i|H=1,T=1) - E(S_i|H=1,T=0)] \\ & - [E(S_i|H=0,T=1) - E(S_i|H=0,T=1)]. \end{aligned} \tag{1}$$

In this equation, our estimated DD captures the causal effect of the one-child policy on the sex ratio imbalance. Specifically, the time-invariant and ethnicity-specific factors are eliminated in the two differences of $E(S_i|H=1,T=1) - E(S_i|H=1,T=0)$ and $E(S_i|H=0, T=1) - E(S_i|H=0,T=1)$, respectively. Then, in the second step, any changes not resulting from the implementation of the one-child policy, while common to both the Han and minorities, are eliminated in the difference of $\Delta E(S_i|H=1) - \Delta E(S_i|H=0)$. In other words, our DD estimate nets out the effect of socioeconomic development and reflects only the effect of the one-child policy.

In practice, the following regression-adjusted DD model is used to identify the effect of the one-child policy on the probability of being a boy:

$$S_i = \alpha_0 + \alpha_1 H_i + \alpha_2 T_i + \alpha_3 H_i \times T_i + \varepsilon_i, \tag{2}$$

where the two dummy variables, H_i and T_i, pick up the ethnicity and time effects, respectively. For example, the effect of socioeconomic development on the probability of being a boy is reflected by α_2. The coefficient of our interest is on the interactive term of $H_i \times T_i$, which captures the causal effect of the one-child policy on the gender of a Han child who was born after 1979. In fact, α_3 is identical to our DD in Eq. 1. To see it clearly, we plug the dummy variables into Eq. 2, and then the average probabilities of being a boy for the four groups are as follows:

	Han	Minority	Difference
Before 1979	$\alpha_0 + \alpha_1$	α_0	α_1
After 1979	$\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3$	$\alpha_0 + \alpha_2$	$\alpha_1 + \alpha_3$
Difference	$\alpha_2 + \alpha_3$	α_2	α_3

Thus, the policy effect on the probability of being a boy is $[(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3) - (\alpha_0 + \alpha_1)] - [(\alpha_0 + \alpha_2) - \alpha_0] = \alpha_3$.

Using the estimates in Eq. 2, we are able to derive the policy effect on sex ratios. Specifically, the ratios of males over females for the four groups are as follows:

	Han	Minority
Before 1979	$(\alpha_0 + \alpha_1) / [1 - (\alpha_0 + \alpha_1)]$	$\alpha_0 / (1 - \alpha_0)$
After 1979	$(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3) / [1 - (\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3)]$	$(\alpha_0 + \alpha_2) / [1 - (\alpha_0 + \alpha_2)]$

Thus, the policy effect on the sex ratio (*PESR*) can be calculated as

$$\begin{aligned} PESR = 100 \times \{ &\{(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3) / [1 - (\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3)] - (\alpha_0 + \alpha_1) \\ &/ [1 - (\alpha_0 + \alpha_1)]\} - \{(\alpha_0 + \alpha_2) / [1 - (\alpha_0 + \alpha_2)] - \alpha_0 / (1 - \alpha_0)\}\}. \end{aligned} \tag{3}$$

We multiply 100 on the right side of Eq. 3 because the sex ratio is defined as the number of males per 100 females in the population. Furthermore, we are able to statistically test the policy effect on sex ratios by using the estimates in Eq. 2. The null hypothesis is that there is no policy effect, that is, $PESR = 0$. The hypothesis can be tested by using a Wald-type nonlinear test, which is based on the delta method. Finally, we can figure out the percentage of the increase in sex ratios that is due to the one-child policy (*POCP*) for the Han Chinese as

$$\begin{aligned} POCP = PESR / \{ &(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3) / [1 - (\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3)] - (\alpha_0 + \alpha_1) \\ &/ [1 - (\alpha_0 + \alpha_1)]\}, \end{aligned} \tag{4}$$

where $\{(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3) / [1 - (\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3)] - (\alpha_0 + \alpha_1) / [1 - (\alpha_0 + \alpha_1)]\}$ is the total increase in sex ratios for the Han Chinese, and *PESR* is the part attributed to the one-child policy.

Using the regression framework, we can control for other demographic, geographic, and socioeconomic characteristics,

$$S_i = \alpha_0 + \alpha_1 H_i + \alpha_2 T_i + \alpha_3 H_i \times T_i + \mathbf{X}_i \beta + \varepsilon_i, \tag{5}$$

where $\mathbf{X}_i$ is a vector of control variables.[9] Our empirical results change little when we include $\mathbf{X}_i$ as shown in the next section.

The key identifying assumption of the DD estimator is that the coefficient on the interaction term of $H_i \times T_i$ in Eq. 2 should be zero in the absence of the one-child policy (Angrist and Krueger 1999). In other words, minorities should be a suitable comparison group for the Han in identifying the treatment effect of the one-child policy on the sex ratio imbalance in China (Meyer 1995). The validity of our DD estimator will be extensively discussed here and will be systematically tested in subsequent sections.

First, the possibility of an endogenously differential implementation of the one-child policy between the Han and minorities can be safely excluded.[10] As evident from early documents on the motivation of the one-child policy, the decision of the nontreatment of the one-child policy on minorities was driven by pure political considerations rather than by different fertility rates or sex ratios across ethnic groups (Greenhalgh 2003). Hence, the threats of political economy and selection problems to the validity of a DD estimator discussed in Meyer (1995) can be ignored in our estimation.

Second, Fig. 2 depicts sex ratios for the Han and minorities by birth year, illustrating the comparability of sex ratios across ethnic groups. We observe that the time-series pattern of sex ratios of minorities closely followed that of the Han Chinese during the pre–policy change period from 1949 to the late 1970s. This situation substantially favors our empirical design because the comparison group of minorities has a distribution of outcomes that is very similar to that of the treatment group of the Han Chinese during the pre–policy change period (Meyer 1995).

Third, a common identifying assumption in a quasi-experimental design is the absence of omitted interactions. In other words, no omitted variables changed the outcomes between treatment and comparison groups in different ways during the post–policy change period (Meyer 1995). Fortunately, several features of our empirical design facilitate robust tests of the validity of this assumption in our DD estimates. We test and discuss the robustness of our DD estimates extensively in later sections.

Data Description

The 1% sample of the 1990 Chinese population census is employed to implement the DD estimation. There are several reasons for using the data from the 1990 census. First, to evaluate the effect of the one-child policy on sex ratios at the

[9] Since the dependent variable is a dummy variable, a logit model would seem to be a natural choice. However, a linear probability model facilitates the interpretation of our DD estimates, as shown above. The major results in our article are confirmed when we use a logit model.

[10] Besley and Case (2000) addressed the endogeneity in the implementation of policies in natural experiment studies.

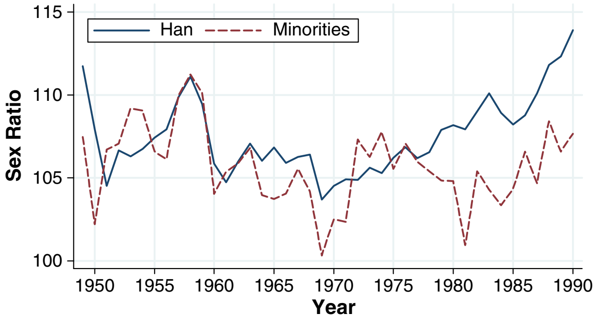

Fig. 2 Sex ratios for the Han and minorities. Calculations are based on the 1990 Chinese population census (1% sample)

national level, the advantage of using census data is self-evident compared with other survey data, which were derived at the provincial level or even at the county level. Second, in contrast to the 1982 population census, the 1990 census allows an appropriately lagged length of time for the evaluation of the policy effect: 10 years had already passed after the enactment of the one-child policy in 1979. Third, before the 1990s, the household registration (*Hukou*) system in China was still strictly regulated, and thus there was little household mobility. Hence, the registration type (agricultural vs. nonagricultural Hukou) and other geographic information were very likely to remain unchanged.

Our empirical analysis is conducted mainly on two subsamples drawn from the 1990 census data set. Sample 1 includes all children born after 1972 with ethnicity, gender, age, registration type, and geographic information.[11] Sample 2 is restricted to Sample 1 children satisfying the following conditions: (1) they are sons or daughters of household heads; (2) there is complete information about their mothers, fathers, and siblings; and (3) their mothers are between 20 and 38 years old.[12]

Table 1 gives variable definitions and summary statistics for the two subsamples. *Gender*, *Han*, *rural*, and *treat* are four dummy variables indicating the sex, ethnicity, household registration type, and birth cohort of the child surveyed. If the child is a Han boy with the agricultural Hukou and was born after 1979, then the values of these four variables are all equal to 1. In fact, there is another variable describing the household's location type (i.e., city, town, or village) in the 1990 census. However, since the registration type, Hukou, and the location type are highly correlated ($r = .97$), only the household registration type is used to indicate the geographic characteristic of the child. Another reason for using the registration type is that the differential application of the one-child policy for households is based on registration type and not on location type.

The educational levels of parents are classified into four categories: illiterate or semiliterate, primary school, junior high school, and senior high school or higher

[11] The reason that the birth year is truncated at 1972 is that children born in 1973 were just 17 years old in the census year of 1990. Since 18 is the legal minimum age for full-time work in China, most children younger than 18 are still economically dependent on and living with their parents.

[12] Because the census contains no information about children no longer living at home, excluding those households with children living outside home will result in a biased sample. Following Angrist and Evans (1998), we restrict mother's age to less than or equal to 38 to mitigate the sample-selection problem. Because the minimum age for marriage as prescribed by the Chinese Marriage Law is 20, the age cutoff is 17 for the eldest children of these women, and most of these children are still living with their parents.

Table 1 Variable definition and summary statistics

Variables	Definitions	Sample 1 Mean	Sample 1 SD	Sample 2 Mean	Sample 2 SD
Gender	1 = male; 0 = female	0.5196	0.4996	0.5235	0.4994
Han	1 = Han; 0 = national minorities	0.9030	0.2960	0.9094	0.2871
Rural	1 = agricultural Hukou; 0 = nonagricultural Hukou	0.8409	0.3658	0.8625	0.3443
Treat	1 = born after 1979; 0 = otherwise	0.6055	0.4888	0.8053	0.3960
Mother Illiterate	1 = illiterate or semiliterate; 0 = otherwise			0.2327	0.4225
Mother Primary	1 = primary school; 0 = otherwise			0.4224	0.4934
Mother Junior	1 = junior middle school; 0 = otherwise			0.2529	0.4347
Mother Senior or Higher	1 = senior middle school or higher education levels; 0 = otherwise			0.0920	0.2890
Father Illiterate	1 = illiterate or semiliterate; 0 = otherwise			0.0618	0.2407
Father Primary	1 = primary school; 0 = otherwise			0.3550	0.4785
Father Junior	1 = junior middle school; 0 = otherwise			0.4084	0.4915
Father Senior or Higher	1 = senior middle school or higher education levels; 0 = otherwise			0.1748	0.3798
Family Size	The number of siblings plus 1 when the child surveyed			2.2647	1.0131
Number of Observations		3,856,489		1,880,322	

Notes: Sample 1 includes all children aged 0 to 17 in the Chinese population census in 1990 (1% sample) for whom there is information on age, gender, registration type, and geographic location. Sample 2 restricts Sample 1 to children of the household head for whom there is complete information of mother, father, and siblings, and whose mothers are aged 20 to 38.

education. Finally, the family size variable denotes the number of siblings of the child surveyed plus one, which amounts to the number of children of the household head.

Empirical Results

In this section, we systematically examine the effect of the one-child policy on the increase in sex ratios in the 1980s. We carry out the DD estimation of Eq. 5 without and with the control vector of geographic indicators. Before presenting our DD estimates, Fig. 2 plots sex ratios for the Han and minorities by birth cohort. While the sex ratio shows no systematic difference across ethnic groups before 1979, it is significantly and consistently higher for the Han Chinese throughout the 1980s. In fact, the sex ratio gap across ethnic groups was, on average, 0.73 (with a standard deviation of 2.08) in the pre–policy change period from 1950 to 1979, and rose to 4.75 (with a standard deviation of 1.51) in the post–policy change period.

Using Sample 1, Table 2 reports the DD estimates of the treatment effect of the one-child policy on the gender of the child by birth cohort. We report the mean value of gender for cohorts born in the pre–policy change period in row 1, and the mean

Table 2 Difference-in-differences (DD) estimates of the effect of the one-child policy on the probability of being a boy

Birth Cohort	Han (1)	Minorities (2)	Differences: Han-Minorities (3)		DD Estimates (4)		DD Estimates (controlling for province) (5)		DD Estimates (controlling for province and rural) (6)	
Pre–Policy Change Period (N = 1,521,563)										
1973–1979	0.5153	0.5149	0.0004	(0.0014)						
Post–Policy Change Period (N = 2,334,926)										
1980	0.5197	0.5117	0.0080*	(0.0038)	0.0076†	(0.0046)	0.0075†	(0.0046)	0.0075†	(0.0046)
1981	0.5191	0.5023	0.0167**	(0.0037)	0.0163**	(0.0032)	0.0160**	(0.0032)	0.0160**	(0.0032)
1982	0.5216	0.5131	0.0084*	(0.0035)	0.0080	(0.0064)	0.0076	(0.0064)	0.0075	(0.0064)
1983	0.5240	0.5105	0.0135**	(0.0037)	0.0131**	(0.0041)	0.0127**	(0.0041)	0.0127**	(0.0041)
1984	0.5213	0.5082	0.0131**	(0.0036)	0.0127**	(0.0042)	0.0124**	(0.0041)	0.0124**	(0.0041)
1985	0.5198	0.5106	0.0092**	(0.0036)	0.0087*	(0.0039)	0.0086*	(0.0039)	0.0086*	(0.0039)
1986	0.5210	0.5159	0.0051	(0.0035)	0.0046	(0.0039)	0.0042	(0.0039)	0.0043	(0.0039)
1987	0.5241	0.5114	0.0127**	(0.0034)	0.0122*	(0.0047)	0.0118*	(0.0048)	0.0119*	(0.0048)
1988	0.5279	0.5202	0.0077*	(0.0036)	0.0072	(0.0047)	0.0066	(0.0046)	0.0066	(0.0046)
1989	0.5290	0.5159	0.0131**	(0.0035)	0.0127**	(0.0039)	0.0119**	(0.0041)	0.0119**	(0.0041)
1990	0.5325	0.5185	0.0141**	(0.0051)	0.0136*	(0.0063)	0.0127†	(0.0064)	0.0125†	(0.0064)
1980–1990	0.5234	0.5124	0.0110**	(0.0011)	0.0106**	(0.0023)	0.0101**	(0.0022)	0.0101**	(0.0022)

Notes: Standard errors are in parentheses. The dependent variable is a dummy variable indicating the gender of the child; it equals 1 if the child is a boy, and 0 otherwise. The data set used is Sample 1, with a total of 3,856,489 observations (see Table 1).

$^{\dagger}p < .10$; $^{*}p < .05$; $^{**}p < .01$

value of gender for each cohort born in the post–policy change period in the rows below. In the last row, we report the average treatment effect of the one-child policy during the entire post–policy change period. Columns 1 and 2 report the mean value of gender for Han and minorities, respectively, and column 3 reports the difference between the two groups. Column 4 reports the difference-in-differences (Eq. 1), which equals column 3 minus the mean difference in gender during the pre–policy change period.

Consistent with Fig. 2, Table 2 shows little difference between Han and minority groups in gender during the whole pre–policy change period. The magnitude of the difference is close to zero and is statistically insignificant. In contrast, the difference is as large as 0.011 for the entire post–policy change period and is statistically significant at 1%. It means that the sex ratio for the Han is, on average, 4.7 higher than that for minorities during the entire post–policy change period.[13] Moreover, the difference is consistently positive and mostly significant for each cohort born in the post–policy change period. Finally, column 4 shows that all the DD estimates for all birth cohorts are positive and most are significant at conventional levels. The DD estimate for the entire post-treatment period, presented in the last row of column 4, is 0.0106 and is statistically significant at the 1% level.

Columns 5 and 6 report the DD estimates by including the provincial and rural indicators as control variables. Interestingly, we find that, after controlling for provincial and rural indicators, the DD estimates are quite close to the estimates without controlling for them (column 4). It suggests that geographic characteristics are not important determinants of the change in the Han-minority sex ratio gap. The last row of columns 5 and 6 shows that the average treatment effect on the probability of being a boy is 0.0101. It implies that the one-child policy has increased the sex ratio by 4.4, and 93.62% of the rise in sex ratios for the Han Chinese throughout the 1980s can be accounted for by employing Eqs. 3 and 4. Using a Wald-type nonlinear test, as discussed earlier, we find that the policy effect on sex ratios is statistically significant at the 1% level.[14]

The Effect of the One-Child Policy by Birth Order

In this section, we outline results from the DD estimation by birth order. Based on Sample 2 with complete information on siblings, columns labeled DD (1) in the first panel of Table 3 report our DD estimates by birth order without other control variables. We find that the magnitude of the DD estimate on the first-birth parity is very small and is statistically insignificant. However, the DD estimate of the one-child policy is tripled for the second-birth parity and becomes marginally significant at the 10% level. Moreover, the DD estimate on the third- and higher-birth parities is as large as 0.0259, which is statistically significant at the 1% level. Columns labeled

[13] The difference in sex ratios between the Han and minorities in the post–policy change period is given by $100 \times [0.5234/(1-0.5234) - 0.5124/(1-0.5124)]$, which equals 4.7 (note that 0.5234 and 0.5124 are from the last row of columns 1 and 2 in Table 2).

[14] We conducted the DD estimation on the rural and urban subsamples, respectively, and found that the DD estimates were statistically significant only for the rural subsamples. For a detailed discussion, see Li et al. (2010).

Table 3 Difference-in-differences (DD) estimates of the effect of the one-child policy on the probability of being a boy by birth order, family size, and gender composition of elder siblings

	Birth Order = 1				Birth Order = 2				Birth Order >2			
	DD (1)		DD (2)		DD (1)		DD (2)		DD (1)		DD (2)	
By Birth Order	0.0034	(0.0043)	0.0033	(0.0039)	0.0095	(0.0061)	0.0100[†]	(0.0061)	0.0259**	(0.0071)	0.0254**	(0.0078)
By Family Size and Birth Order												
Family size = 1	0.0106	(0.0213)	0.0117	(0.0205)								
Family size = 2	−0.0013	(0.0082)	0.0023	(0.0126)	0.0331**	(0.0113)	0.0310**	(0.0106)				
Family size >2	−0.0091	(0.0100)	−0.0087	(0.0100)	−0.0233*	(0.0134)	−0.0232[†]	(0.0136)	0.0259**	(0.0071)	0.0254**	(0.0078)
By Birth Order and Gender Composition of Elder Sibling(s)												
One boy					−0.0105	(0.0086)	−0.0103	(0.0085)				
One girl					0.0277*	(0.0118)	0.0279**	(0.0086)				
Two boys									−0.0264	(0.0245)	−0.0177	(0.0241)
One girl, one boy									0.0141	(0.0130)	0.0144	(0.0126)
Two girls									0.0376*	(0.0179)	0.0335[†]	(0.0182)

Notes: Standard errors are in parentheses. The dependent variable is a dummy variable indicating the gender of the child; it equals 1 if the child is a boy, and 0 otherwise. The data set used is Sample 2, with a total of 1,880,322 observations (see Table 1). DD (1) refers to the DD estimates without control variables, and DD (2) refers to DD estimates controlling for rural, province, and mother's and father's education level.

$^{\dagger}p<.10$; $*p<.05$; $**p<.01$

DD (2) in the first panel of Table 3 report DD estimates by birth order with other control variables. The control variables are rural, mother's and father's education level, and provincial indicators. We find that the DD estimates by birth parity are robust to the inclusion of these control variables, and the DD estimate on the second-birth parity even becomes statistically significant. Therefore, the estimated effect of the one-child policy in Table 2 is mainly reflected at the second- and higher-birth parities.

The variation pattern of our DD estimates across different birth parities is consistent with the one-child policy explanation of the increase in sex ratios. With the regulated birth quota of the one-child policy, the distortion of the sex ratio could be focused on the second- and higher-birth parities. The reason is that the required payment for the above-quota birth acts like a screener. Those parents risking the sanction to have a second or third birth should have stronger son preference on average than those who had only one child, and they are more likely to practice gender-selective abortion. Furthermore, the imposition of the above-quota birth sanction has indeed increased the probability of choosing the gender-selection technology and practicing gender-selective abortion on the second- and higher-birth parity even in a homogenous case.

The second panel of Table 3 reports the DD estimates by both family size and birth order. It is interesting to note that the DD estimates exhibit a significant variation across family size and birth order. The DD estimate is positive and statistically significant for the second-birth parity, while it is insignificantly negative for the first-birth parity in two-child families. If a family has more than two children, the DD estimates are negative for the first two lower parities and positive for other, higher parities. We further carry out the DD estimation by both birth order and gender composition of elder siblings in the third panel of Table 3. We find that the DD estimate is significant only for those children born in the second parity with an elder sister and in higher-birth parities with two elder sisters.

Caution is needed in interpreting the DD estimates in Table 3 because fertility (family size) is a choice variable. Therefore, we should look at the variation pattern of the DD estimates by birth order, family size, and gender composition of elder siblings rather than the DD estimate for a certain group of children. Summarizing from Table 3, we conclude that parents subject to the one-child policy are more likely to practice gender selection at the second- or higher-birth parities, especially when the first child is a girl or when children at low birth parities are all girls.

Robust Analysis

The DD Estimates for Other Outcome Variables

Although the previous analysis has identified the one-child policy as the main culprit for the increase in sex ratios in China, we need to ensure that our DD estimates are not mainly picking up the effect of other omitted policy shocks or socioeconomic changes. The key identification assumption of the DD estimator is that there should be no other policy shocks or changes in socioeconomic variables during the post-treatment period that have affected the gender-selection behavior of the Han and minorities differently. Although we do not know of any such unobservable shocks or changes *a priori*, the validity of our DD identification can be justified as follows. If the DD estimate on the sex ratio is due to something other than the one-child policy (e.g., economic reform, which might have affected the Han and minorities differently), the impact should also manifest on other aspects. Thus, we also provide the DD estimate for other outcome variables covering family structure, mother's age at first birth, parental education, and labor market behaviors. To validate our DD identification, we expect the DD estimates for these variables to be zero.

Table 4 reports the DD estimates for other outcome variables. It is reassuring to find that all DD estimates are statistically insignificant except for mother's education. Considering the huge sample size, we conclude that there is no difference in changes between the Han and minorities in terms of family structure, mother's age at first birth, father's education, and parental labor market behaviors. As for mother's education, the literature suggests that the increase of mother's education, relative to father's, enhances mother's bargaining power within the household and thus leads to a weakened preference for boys (Behrman and Rosenzweig 2002; Thomas 1994).

Table 4 Difference-in-differences (DD) estimates of the effect of the one-child policy on other outcome variables

Dependent Variables	DD, Total Population		DD, Rural Residents	
Family Structure				
Number aged 64+ in the household	−0.0066	(0.0059)	−0.0082	(0.0065)
Father is the household head	0.0018	(0.0026)	0.0010	(0.0017)
Mother's Age at the First Birth	0.0084	(0.0898)	−0.0259	(0.0876)
Father				
Education level	0.0580	(0.0360)	0.0519	(0.0343)
Senior school	0.0146	(0.0158)	0.0090	(0.0146)
College and higher	−0.0007	(0.0028)	−0.0017	(0.0013)
Working	−0.0013†	(0.0007)	−0.0009	(0.0006)
On-farm working			−0.0072	(0.0052)
Off-farm working			0.0065	(0.0052)
Unable to work	0.0006	(0.0004)	0.0004	(0.0002)
Mother				
Education level	0.1004*	(0.0450)	0.1030*	(0.0412)
Senior school	0.0141	(0.0131)	0.0085	(0.0088)
College and higher	−0.0017	(0.0026)	0.0001	(0.0003)
Working	−0.0082	(0.0058)	−0.0074	(0.0046)
On-farm working			−0.0048	(0.0048)
Off-farm working			−0.0026	(0.0024)
Unable to work	−0.0001	(0.0001)	−0.0001	(0.0001)
N	1,880,322		1,621,821	

Notes: Standard errors are in parentheses. Province is controlled for in all regressions. The data set used is Sample 2 (see Table 1).

†$p < .10$; *$p < .05$

Therefore, the fact that mother's education increases more for the Han than for minorities would bias downward our DD estimates of the effect, if any, of the one-child policy. In other words, the DD estimate of the one-child policy on the sex ratio would have been larger without the increased female education of the Han relative to minorities.

The Geographic Pattern of the DD Estimates by Provinces and Autonomous Regions

In China, the percentage of minorities living in the five autonomous regions to the whole population of minorities is higher than 40%, and these autonomous regions are geographically located at the western and southern parts of China. There had been unbalanced socioeconomic development across provinces and autonomous regions after the economic reform of the late 1970s. During the same time, the increase in sex ratios in China is suggested to have been correlated with socioeconomic development (Qian 2008). Therefore, the benefit of estimating the treatment effect at the provincial or autonomous regional level is that the Han and minorities should be more homogenously affected by the economic reform within the same area, and thus the interfering effect of socioeconomic development can be held constant or eliminated.

Table 5 reports our DD estimates by province and autonomous region in the upper panel, and by each autonomous region in the lower panel. Columns 1 and 2 show that people living in provinces constitute a dominant share of 91.15% of the total population, where 93.59% are Han. Only 8.85% of the Chinese population lives in the five autonomous regions, and almost 44% of the residents living there are minorities. Column 3 reports the DD estimates for the provinces and autonomous regions. We find that both DD estimates are positive and significant

Table 5 Difference-in-differences (DD) estimates of the effect of the one-child policy on the probability of being a boy: Provinces versus autonomous regions

	% of the Total Population (1)	% Han (2)	DD Estimates (3)		DD Estimates With Rural Dummy Variable (4)	
Provinces	91.15	93.59	0.0070**	(0.0026)	0.0070**	(0.0026)
Autonomous Regions	8.85	56.33	0.0226**	(0.0067)	0.0223**	(0.0065)
Inner-Mongolia	2.17	77.14	0.0098	(0.0087)	0.0102	(0.0087)
Guangxi Zhuang	4.47	58.68	0.0245**	(0.0050)	0.0241**	(0.0050)
Tibet	0.25	0.35	−0.0409	(0.1857)	−0.0499	(0.1881)
Ningxia Hui	0.42	62.73	0.0358*	(0.0168)	0.0367*	(0.0170)
Xinjiang Uyghur	1.53	27.62	0.0183†	(0.0093)	0.0185*	(0.0094)

Notes: Standard errors are in parentheses. The dependent variable is a dummy variable indicating the gender of the child; it equals 1 if the child is a boy, and 0 otherwise. The data set used is Sample 1, with a total of 3,856,489 observations (see Table 1). The first panel reports the DD estimates for provinces and autonomous regions, and the second panel reports the DD estimates for each of the five autonomous regions.

$^{\dagger}p<.10$; $^{*}p<.05$; $^{**}p<.01$

at the 1% level. Furthermore, column 4 shows that these two DD estimates are robust to the inclusion of the rural indicator. The lower panel of Table 5 shows that the estimated treatment effects are positive for all the autonomous regions except Tibet. The reason for the insignificant DD estimate for Tibet may be that, as shown in column 2, only 0.35% of the children in Tibet are Han. Thus, essentially, most people in Tibet have been exempted from the one-child policy. The negative DD estimate for Tibet may actually reflect a weakening son preference with socioeconomic development. To sum up, the geographic pattern in Table 5 further validates our quasi-experiment design.[15]

Our robust analysis indicates that the DD estimates of the effect of the one-child policy on sex ratios are unlikely to pick up the effects of other policy shocks or socioeconomic changes that may differentially affect the gender-selection behavior of the Han and minorities.

DD Estimates Using the 2000 Census and the 2005 Mini-Census

In this section, we present the DD estimates using the 2000 census and the 2005 mini-census. The sex ratio at birth in China increased dramatically during 1990–2005. Although the DD method requires birth cohorts to be near the implementation year of the one-child policy, exploring the policy effect in the 1990s onward is interesting. As in the foregoing analysis, here we treat the 1973–1979 birth cohorts in the 1990 census as the group born prior to the one-child policy. In the 2000 census, we use the 1991–2000 birth cohorts as the group born in the 1990s. Similarly, we use the 2001–2005 birth cohorts in the 2005 mini-census.

Table 6 reports the estimation results using the 2000 census and the 2005 mini-census. Column 1 shows that compared with the birth cohort before 1980, the one-child policy increased the probability of being a boy by as large as 1.52 percentage points in the 1990s. Using Eq. 3, we calculate that the policy effect on sex ratios is about 6.98, which is statistically significant at the 1% level. Using Eq. 4, we find that the effect of the one-child policy accounts for 57.12% of the total increase in sex ratios during the 1990s. As shown in columns 2 and 3, the policy effect is robust to the inclusion of provincial and rural dummy variables. Similarly, column 4 shows that the one-child policy increases the probability of being a boy by 1.58 percentage points. This implies that the one-child policy increased the sex ratio by 7.01 in 2001–2005, accounting for 53.59% of the total increase in sex ratios during the same period.

Table 7 summarizes the effects of the one-child policy on sex ratios across the 1990 census, the 2000 census, and the 2005 mini-census. Column 1 reports the actual sex ratios at different periods. Column 2 shows the total increases in sex ratios for two adjacent periods. The estimated effects of the one-child policy on the sex ratio in different periods are reported in column 3. Note that the effect of the one-child policy on sex ratios increased by 2.58 from the 1980s to the 1990s (i.e., 6.98–4.40), then

[15] We also analyzed the time pattern of the DD estimates by birth year (Li et al. 2010). We found that the time pattern of our DD estimates is highly consistent with the implementation stages of the one-child policy in the 1980s.

Table 6 Difference-in-differences (DD) estimates of the effect of the one-child policy on the probability of being a boy: 2000 census and 2005 mini-census

	2000 Census			2005 Mini-Census		
	(1)	(2)	(3)	(4)	(5)	(6)
Han	0.0004	−0.0004	−0.0001	0.0004	0.0004	0.0002
	(0.0014)	(0.0015)	(0.0015)	(0.0014)	(0.0016)	(0.0016)
Treat	0.0119**	0.0134**	0.0137**	0.0131*	0.0134†	0.0138†
	(0.0018)	(0.0018)	(0.0018)	(0.0037)	(0.0038)	(0.0038)
DD	0.0152**	0.0135**	0.0137**	0.0158**	0.0143**	0.0140**
	(0.0019)	(0.0019)	(0.0019)	(0.0040)	(0.0041)	(0.0041)
Constant	0.5149**	0.5169**	0.5137**	0.5149**	0.5214**	0.5234**
	(0.0013)	(0.0040)	(0.0040)	(0.0013)	(0.0052)	(0.0052)
Rural Indicator	No	No	Yes	No	No	Yes
Province Indicators	No	Yes	Yes	No	Yes	Yes
N	3,033,321	3,033,321	3,033,321	1,656,094	1,656,094	1,656,094

Notes: Standard errors are in parentheses. The dependent variable is a dummy variable indicating the gender of the child; it equals 1 if the child is a boy, and 0 otherwise.

†$p < .10$; *$p < .05$; **$p < .01$

leveled off in 2001–2005 (i.e., 7.01–6.98). The increase in the policy effect on sex ratios from the 1990s onward may be attributable to the 1.5-child policy, which was more prevalent after 1990.[16] Zeng (2007, 2009) analyzed the structural impact and the implicit psychological effect induced by the 1.5-child policy on sex ratios. For example, the 1.5-child policy suggests that the government implicitly agrees that a girl's value is half that of a boy. Such implicit psychological effects, together with the traditional strong son preference, lead a rural resident whose first child is a girl to conduct prenatal gender selection to have at least one boy.[17]

Column 4 of Table 7 shows that the percentage of increase in sex ratios that is accounted for by the one-child policy substantially decreased from the 1980s to the 1990s. Although the one-child policy accounted for a dominant share (94%) of the increase in sex ratios in the 1980s, it explained only 57% and 54% of the increases in 1990–2000 and 2001–2005, respectively. This implies that the continuing increase in sex ratios from the 1990s onward is mainly caused by other factors, such as socioeconomic development, rather than by the one-child policy.

As discussed earlier, given son preference and gender-selection technology, the frequency or intensity of gender selection is different at different fertility levels; thus, the sex ratio also becomes different. Specifically, *ceteris paribus*, the sex ratio

[16] The 1.5-child policy refers to the policy that allows rural families with a first-birth girl to have a second birth.

[17] Guo (2005) and Gu et al. (2008) analyzed fertility rates and sex ratios across different policy regimes. It would be interesting to explore the causal effects of the 1-, 1.5-, and 2-child policies on sex ratios among the Han Chinese. However, these policies among the Han Chinese are suspected to be endogenously imposed. In contrast, the differential treatment of the policy across ethnic groups is evidently exogenously imposed.

Table 7 The effects of the one-child policy on the sex ratio across the 1990 census, 2000 census, and the 2005 mini-census

Period	Actual Sex Ratio (1)	Total Increase in Sex Ratio (2)	One-Child Policy Effect (3)	% Explained by the One-Child Policy (4)
1973–1979 (1990 census)	106.31			
1980–1990 (1990 census)	111.01	4.70	4.40	93.62
1991–2000 (2000 census)	118.53	12.22	6.98	57.12
2001–2005 (2005 mini-census)	119.39	13.08	7.01	53.59

Notes: Column 1 reports actual sex ratios at different periods; column 2 calculates the total increases in the sex ratio in the periods 1980–1990, 1991–2000, and 2001–2005, compared with the period 1973–1979; column 3 calculates the effect of the one-child policy on sex ratio by using Eq. 3; column 4 calculates the percentage of the total increase in sex ratio accounted for by the one-child policy (column 3 / column 2).

increases as fertility decreases. Of course, lower fertility with desired gender (boys) in the population can be realized only by resorting to gender selection at an earlier time or at a more intensified pace. Following this logic, the steady increase in sex ratios after 1990 was a result of the steady decrease in fertility during the same period. The total fertility rate decreased from 2.46 in 1990 to 1.80 in 2000. Therefore, any factor that led to a decrease in fertility also drove up sex ratios. The one-child policy had played a major role in decreasing fertility in the 1980s; hence, it was the dominant driver of the increase in the sex ratio during that period. The importance of the one-child policy in the decline of fertility rates from the 1990s onward may have lessened, and correspondingly, its importance in increasing sex ratios may have likewise decreased. The rapid economic growth in the 1990s, for example, increased the private monetary and opportunity cost of childbearing. During the same time, the emphasis on human capital pushed parents to invest more in their children's education. Therefore, socioeconomic development decreased fertility in the 1990s. Low fertility, strong son preference, and the availability of modern gender-selection technology jointly led parents to practice gender selection.

Conclusions

Exploiting a unique feature of China's one-child policy (i.e., it was applied only to the Han Chinese), we constructed a difference-in-differences estimator to identify the causal relationship between the one-child policy and the recent increase in sex ratios in China. Based on the 1990 Chinese population census, the estimated effect of the one-child policy on the probability of being a boy was as large as 1.01 percentage points in the 1980s. This implies that the strict enforcement of the one-child policy causally increased the sex ratio by 4.4, accounting for about 94% of the increase in sex ratios throughout the 1980s. Further exploration reveals that the policy effect is driven mainly by second- and higher-birth parities and by rural residents. Moreover, several robust tests indicate that the DD estimates are not likely confounded by other policy shocks or socioeconomic changes. We also conducted the DD estimation by using both the 2000 census and the 2005 mini-census. The estimates imply that the one-child policy increased the sex ratio by about 7.0 for the 1991–2005 birth cohorts, accounting for about 57% and 54% of the increases in sex ratios for the 1991–2000 and 2001–2005 birth cohorts, respectively.

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