Long-Term and Multi-Generational Impacts of Skilled Birth Attendance

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Abstract

This paper examines the long-term and multi-generational benefits of skilled birth attendance (SBA), which involves having a trained midwife or doctor present at delivery to safely perform normal deliveries using aseptic techniques and provide first-line emergency obstetric care. Using data on the county-by-county rollout of SBA in China from the 1930s to the 1970s, our research first demonstrates that the SBA reform substantially reduced neonatal mortality. We then show that exposure to skilled delivery during birth leads to a 1.5% increase in adult income. Moreover, we discovered that the benefits of exposure to SBA in previous generations extend to subsequent offspring. Children with at least one parent who experienced SBA have a 2.6% higher monthly income in adulthood than those whose parents did not have access to SBA. This effect is more pronounced when the mother, rather than the father, was exposed to SBA. We also present evidence of several underlying mechanisms, including improved physical and mental health, better educational outcomes, and enhanced cognitive abilities. Our findings indicate that having skilled health professionals attend childbirths can result in significant long-term and multi-generational benefits.

Keywords: skilled birth attendance, adult earnings, human capital, health, public goods
JEL classification: H51, H75, I15, I18, J24, N35, O12, O15

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1 Introduction

Worldwide, nearly 3.4 million infants and 600,000 women aged 15 to 49 die every year due to poor hygiene or harmful practices during delivery (WHO, 2023). For every woman and newborn who dies, many more suffer from serious conditions that impact them for the rest of their lives. While most pregnancies and births are uneventful, all deliveries involve some level of risk (WHO, 2003). A significant number of the complications that arise around the time of childbirth are neither predictable nor preventable.[1] In response, the World Health Organization (WHO) has advocated skilled birth attendance (SBA) – having a trained midwife or doctor present during childbirth to perform safe, aseptic deliveries and provide first-line emergency obstetric care – as the most critical factor in reducing maternal and neonatal mortality, and as a key strategy for promoting the long-term well-being of children (WHO, 1999). While existing research confirms that SBA significantly lowers maternal and neonatal mortality rates, few studies have explored its long-term benefits for children born under such care, particularly in developing countries (Anderson et al., 2020; Lazuka, 2023; Ahsan et al., 2021). Furthermore, the economic, epidemiological, and child development literature suggests that the benefits of SBA could extend across generations. However, little is known about whether and to what extent safer birth practices lead to improved adult outcomes in future generations (East et al., 2023; Currie, 2009).

By examining the county-by-county rollout of SBA throughout China from the 1930s to the 1970s as a quasi-natural experiment, the present study offers a unique opportunity to enhance our understanding of the long-term and multi-generational benefits of safe birth practices. When a county implements SBA practices, local doctors and midwives receive training in midwifery skills to manage normal labor with an emphasis on clean delivery. They also refer mothers and babies for interventions that exceed their capabilities or the resources available in their settings.[2]

[1] While conditions such as hypertensive disorders significantly heighten the risk of childbirth complications, it is important to recognize that every delivery carries some level of risk. According to the WHO, delivery complications can affect any woman, regardless of health or socioeconomic status. Even healthy women with high socioeconomic status face a 15% chance of complications during delivery (WHO, 1999).

[2] When a county adopts SBA practices, local doctors and midwives receive standardized training in midwifery skills based on WHO guidelines and official directives from the MOH. The SBA practices are identical across different counties. In developed counties and many urban areas within them, SBA typically occurs in health facilities such as hospitals and

There are several reasons why skilled delivery at birth could have an impact on early-life health and lasting consequences on adult outcomes. First, traditional birth attendants (TBAs), who lack formal midwifery training, often use unhygienic practices, such as cutting the umbilical cord with non-sterile instruments or applying contaminated materials to the umbilical stump. These practices frequently result in severe neonatal bacterial infections, including sepsis and tetanus. Both medical and economic research highlight that early-life exposure to disease and injury, particularly during the neonatal period, can lead to enduring negative outcomes (Finch, 2010; Currie and Almond, 2011). Second, skilled birth attendants who are proficient in midwifery skills can accurately diagnose at-risk newborns (e.g., preterm or low birth weight) and immediately refer them to specialists for timely and appropriate treatment. Research emphasizes that such timely and effective care is critical for improving the long-term health outcomes of at-risk newborns (Charpak et al., 2017).[3]

For this study, we hand-collected a county-level dataset of the years in which each county initiated SBA from over 3,000 book-length local gazetteers. We combined this dataset with data from China's 2005 One-Percent Population Survey, the China Family Panel Studies (CFPS), and the China Health and Nutrition Survey (CHNS) to estimate the program's long-term and intergenerational effects on a broad range of outcomes.

The substantial variation in the timing of deliveries assisted by skilled birth attendants at the county level allowed us to use a cohort difference-in-differences (DID) design to estimate the long-term impacts of the program.[4] First, we focused on individuals directly exposed to the rollout of SBA: babies born between 1935 and 1965, whom we referred to as the "first generation." To determine policy exposure for the first generation, we used county and year of birth data. We then compared exposed children in the same counties to older, untreated cohorts. In addition, we compared children born in

tertiary referral centers, where doctors receive standardized training in midwifery skills. However, in rural and remote areas with limited access to health facilities, midwives are often the primary providers, trained and equipped to manage normal deliveries and facilitate referrals for mothers and babies requiring higher levels of care.

[3]Third, childbirths attended by skilled health professionals can potentially reduce newborn stress by providing specialized and maternal care during the critical immediate postnatal period. Previous studies suggest that early-life exposure to stress hormones can permanently affect brain function, with long-term consequences for mental health and well-being (Danese and McEwen, 2012; Persson and Rossin-Slater, 2018). Fourth, skilled health professionals with advanced midwifery expertise could perform newborn screenings to enable early detection and treatment of diseases, potentially preventing the progression of certain conditions (Bhai and Mitchell, 2024).

[4]Once a county implements skilled care at delivery, it remains in place.

different years for early and later-treated counties.

Using the staggered DID strategy and individual-level census data, we demonstrated that the SBA reform substantially reduced neonatal mortality. Besides, we discovered that children delivered with the assistance of skilled health personnel (a midwife or doctor) earned approximately 1.5% more in adulthood than individuals who were not treated, which is equivalent to approximately 0.4 years of additional schooling.[5] The changes are due primarily to hourly wage increases rather than hours worked and do not vary by gender. After estimating the overall increase in adult income, we examined the respective roles of health and schooling, utilizing the wealth of information found in the census data and household surveys. This analysis is inspired by theoretical works that have identified mechanisms through which good health in early childhood can unlock lifetime benefits (see, e.g., Currie and Almond 2011; Heckman et al. 2013; Almond et al. 2018; García et al. 2020). We found that children born with the help of skilled attendants demonstrate substantial improvements in physical and mental health, increases in years of schooling, and higher math and verbal test scores. These findings align with the related literature on health-related income gains (Bleakley, 2007; Pitt et al., 2012; Baird et al., 2016).

We then moved on to investigate whether the benefits of SBA affected the next generation by examining adult outcomes among the first generation's offspring, whom we referred to as the "second generation." As most counties had implemented SBA by the end of 1970s, nearly all individuals in the second generation were born with the assistance of skilled health personnel. For the second generation, we link parents' county and year of birth to measures of their SBA exposure. Our findings revealed that children in the second generation, with at least one parent who was exposed to SBA, have a 2.6% higher monthly income in adulthood than those whose parents were not exposed to SBA. This effect is more pronounced when the mother, rather than the father, was exposed to SBA.

The income increase is driven by both higher hourly wages and more hours worked, with a larger increase observed among female offspring than male offspring. We also documented improved physical and mental health, increased years of schooling, and better cognitive skills among offspring of the first

[5]For this comparison, we used Wang (2013) results, who found that an additional year of schooling in China raises an individual's income by approximately 5.3%.

generation who were born with SBA.

We have presented compelling evidence to uphold the central parallel-trend assumption in our cohort DID strategy. Analyzing the impact of a policy across different generations is complex due to the time gap between the first generation's treatment and the subsequent generation's outcomes. Our identification assumption requires that within cohorts born in treated counties around the policy implementation period there are no other economic or health shocks that vary by cohort and county and could also influence the health outcomes of the first or subsequent generations. In the event study, we demonstrate that the estimates of "placebo" exposure in the first generation (i.e., cohorts who were born before their county introduced SBA) are very small and statistically insignificant. Similarly, the estimates of such placebo exposure in the second generation (i.e., cohorts whose parents were both born before the implementation of SBA in their county) are negligible and not statistically significant. This lack of impact of SBA on non-treated cohorts supports the parallel trend of our DID strategy. We demonstrate that our main findings are not driven by changes in fertility or contemporaneous historical events. In addition, our main results are robust to a wide range of alternative specifications, to accounting for multiple hypothesis testing, and to using alternative income measures. Moreover, with staggered reform adoption, the two-way fixed-effects estimator may suffer from a negative weighting problem that leads to biased estimates in the presence of heterogeneous treatment effects over time or across groups. We performed several robustness analyses to confirm that our estimates were not contaminated by these concerns (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020).

Furthermore, we conducted back-of-the-envelope calculations to quantify the induced benefits estimated above. The cumulative income gain over just 2 years of adulthood for the second generation (216 RMB or 30 USD per person) exceeds the expected cost of SBA per child (179 RMB or 24.5 USD per live birth).[6]

Given the levels of population, health, medical technology, and wealth involved in the practice of SBA in China, our results are directly relevant to ongoing policy debates regarding the merits of

[6]Due to the unavailability of archival records for SBA costs before 1949, our calculations focus on the second generation.

SBA in developing countries. In many developing nations, the majority of births are attended by TBAs, who have no formal education or training. Experts have claimed that replacing TBAs with skilled birth attendants would substantially reduce infant and maternal mortality (Thompson et al., 2011; Anderson et al., 2020; Kotsadam et al., 2017). This claim is based primarily on historical studies conducted in Sweden and the United States, with little empirical evidence from developing countries to support it and justify the promotion of SBA in nations of the Global South. Moreover, no credible evidence has been presented to examine the impacts of SBA across different generations. Using a variety of survey and administrative data, we demonstrate that SBA could generate substantial long-term and multi-generational benefits. The infant mortality rates and per capita incomes in China – the largest developing country – during the period of the rollout of SBA are comparable to those of the Global South countries today. Therefore, our results and methods contribute credible evidence to this important policy issue.

Our findings contribute to the literature on the impacts of SBA. While existing studies confirm that SBA significantly reduces maternal and neonatal mortality rates, few have examined its long-term benefits for children born under such care, particularly in developing countries (Anderson et al., 2020; Lazuka, 2023; Ahsan et al., 2021; Renard, 2022; McMichael, 2023). Moreover, little is known about whether and to what extent safer birth practices improve adult outcomes in future generations (Miller and Wherry, 2019; Currie, 2009). To the best of our knowledge, this is one of the first studies to explore the long-term income and multi-generational benefits of SBA.

Our results also add to the literature measuring the long-term effectiveness of public health programs. Previous studies have examined the effectiveness of piped water (Devoto et al., 2012; Li and Xiao, 2023), hookworm eradication (Baird et al., 2016), iodine supplements (Field et al., 2009), and vaccination (Bloom et al., 2012). Our results indicate that public investments in SBA have persistent long-term impacts on both the treated generation and future generations. By quantifying these effects, we established that benefit–cost ratios based only on outcomes directly experienced by cohorts who were immediately affected by the implementation of SBA underestimate the program's overall efficacy. More broadly, our analyses suggest that even "long-term" studies of early-life interventions may fail to

capture the full extent of benefits conferred.

The results of this paper also expand the literature on the long-term effects of early-life conditions. A subset of this literature has focused on demonstrating the impact of negative or traumatic experiences, such as stress (Aizer, 2011; Currie and Rossin-Slater, 2013), diseases (Nelson, 2010), and famines (Chen and Zhou, 2007). More recent studies have focused on estimating gains from exposure to early childhood interventions, such as infant care programs (Bhalotra et al., 2017; Butikofer et al., 2015; Daysal et al., 2022), psychological interventions (Gertler et al., 2013), and health insurance coverage (Aizer, 2007; Cohodes et al., 2016; Miller and Wherry, 2019; Turner, 2015).Our examination of variations in the county-by-county rollout of SBA revealed that skilled delivery at birth significantly improves work earnings, physical and mental health, and educational attainment for both children and their offspring.

The rest of this study is structured as follows. First, we present background information on the rollout of SBA in China in Section 2. In Section 3, we present details on the data used in this study. In Section 4, we introduce the main empirical specification, which is a cohort DID model. Section 6 presents the main results of the DID model, along with several robustness checks. Section 7 presents a back-of-the-envelope benefit–cost calculation of SBA and concludes our study.

2 Background

SBA was first introduced in the early twentieth century in a small number of cities, primarily in eastern China, by missionary doctors from Western countries such as the United Kingdom and the United States. With the support of the nationalist government, these missionary doctors not only provided safe delivery during childbirth, but also trained local medical professionals in Western midwifery skills to increase the accessibility of SBA in these cities. However, due to the Second Sino-Japanese War, SBA remained largely limited to the cities in which missionary doctors were located. In the vast rural areas and other cities, births were generally attended by TBAs, who had no formal education or training. These attendants often relied on harmful practices and poor hygiene during delivery, leading to

maternal and neonatal mortality rates as high as 30% to 50%.

After World War II, both the nationalist Chinese government and the subsequent government of the People's Republic of China, after gauging public opinion and consulting with medical experts, prioritized SBA in order to address high maternal and neonatal mortality rates. After observing significantly lower maternal and infant mortality rates in cities with SBA, the Ministry of Health (MOH) expanded this program nationwide. By the end of the 1970s, over 98.1% of counties and 99.6% of the Chinese population were covered by SBA.

The nationwide rollout of SBA exhibited four features that facilitated our empirical analysis. First, although designed and initiated by the central government, the implementation of safe birth practices was carried out locally. Therefore, the significant variations in the timing of the introduction of SBA at the county level allowed us to utilize a cohort DID model to capture its long-term impacts.[7] In particular, children in later-treated counties were compared to children in counties that initiated the program in earlier years. This comparison of early- and later-treated children addressed concerns over both selection into treatment and changes in cohort-specific earning patterns.

Second, when a county introduced SBA practices, local doctors and midwives received standardized training in midwifery skills based on WHO guidelines and official directives from the MOH. This training enabled them to manage normal labor with careful attention to clean delivery, recognize the onset of complications, perform essential treatments, and refer mothers and babies for interventions that exceeded their capabilities or the resources in their particular settings. The SBA practices were identical across the various counties. Consequently, our analysis of children born with the assistance of trained healthcare personnel is informative of the overall effect of SBA and sheds light on countries and regions that have not yet implemented the program.

Third, as SBA was offered free of charge by the local government for all live births, compliance rates were very high after a county initiated the program. In most counties, the proportion of births attended by a skilled professional (midwife or doctor) exceeded 90% within six years of the policy's initiation. Therefore, all children born after a county implemented SBA comprised the treatment

[7] Once a county introduces SBA it remains in effect.

group. Within the same counties, exposed children could be compared against older untreated cohorts. This age–cohort comparison is in line with the related literature (Duflo, 2001; Hoynes et al., 2016; Hoehn-Velasco, 2021) and enables the inclusion of county fixed effects that remove time-invariant county characteristics.

Fourth, most counties in our data implemented SBA before the 1970s. As the key dependent variable, income, comes from the 2005 Population Survey, we have a sufficient post-implementation period to estimate the long-term and inter-generational impacts of the program. In addition, a child's birth date and *hukou* county of registration approximate the treatment assignment for skilled delivery at birth in our empirical analysis.[8] The unique *hukou* system serves as a domestic passport system with nonimmigrant visas only.[9] An individual's *hukou* is ascribed at birth based on their mother's *hukou* status. People can apply for temporary permits to live and work in other counties, but are only entitled to social welfare and various public services in their *hukou* county of registration.[10] Only small groups are permitted to permanently change their *hukou* counties of registration under the following conditions: recruitment by a state-owned enterprise (*zhaogong*), enrollment in an institution of higher education (*zhaosheng*), and promotion to senior government official jobs (*zhaogan*) (Yang and Zhou, 1999). These groups comprised a very small percentage (between 1% and 5%) of the whole population by 2005, thereby mitigating concerns about selection bias (Chan and Zhang, 1999; Grey, 2008; Sun, 2021). As this nationwide restriction on migration was still in place as at 2005, our estimation serves as a conservative lower-bound estimate of SBA's effect on adult earnings: the small percentage of people who had permanently changed their *hukou* county of registration by the 2005 Population Survey through the channels mentioned above are likely to be wealthier and better educated than their peers, thus attenuating our estimations toward zero (Chen et al., 2020; Sun, 2021; Zhao, 1997; Heckman,

[8]The ideal data would provide details about a child's birth county. As such data do not exist, we gauged the treatment assignment for safe birth practices on the basis of a child's birth date and *hukou* county of registration.

[9]The *hukou* system was first implemented in cities in 1951 and was promulgated as a permanent nationwide system in 1958 (Chan and Zhang, 1999; Sun, 2021).

[10]The *hukou* system makes no provision of permanent residency that allows individuals to live, work, and have access to social welfare and public services in another county without a permanent change in their *hukou* county of registration. An individual who chose to migrate without going through legal channels would not be permitted access to resources in the destination area. Denial of food, housing, education, and any other social services rendered illegal migration impossible to maintain (Grey, 2008).

2005).

3 Data

In this section, we present the main features of the datasets used in this study. First, we gathered information from over 3,000 local gazetteers (described in more detail in the next subsection) to construct a unique, county-level dataset of the years in which each county implemented SBA practices. Second, we matched this county-level information to individual-level population censuses and family surveys. The main outcome variable, adult earnings, comes from the 2005 Population Survey.

County-Level Rollout of SBA

Data on the county rollout of SBA were obtained from county gazetteers. Often considered the county's "encyclopedia," gazetteers are book-length volumes compiled by local historians to record each county's major events and draw upon material in local archives. County gazetteers are not used in evaluating local officials and are therefore less susceptible to misreporting. A number of studies have empirically gauged and endorsed the quality of gazetteer data (Almond et al., 2019; Chen et al., 2020; Chen and Lan, 2020).[11] Local governments typically form task forces to write and periodically update their gazetteers as an important source of local history and pride. However, not all historical information is recorded in local gazetteers; for example, one piece of information may be found in the gazetteers of some counties but not in others. We conducted a comprehensive review of all 3,153 gazetteers published in 2,868 counties.[12] Our primary analysis sample included the 1,721 counties that recorded the precise timing of the introduction of SBA.[13] Appendix Figure A.1. presents an example

[11] Several studies (Almond et al., 2019; Chen et al., 2020; Chen and Lan, 2020) have compared economic statistics in county gazetteers, such as gross production of grain, to the commonly used statistics in yearbooks, on which cadre evaluations are based. They document substantial agreement between the two data sources. Furthermore, Benford's Law, as suggested by Varian (1972), was applied to detect fake data in gazetteers, where falsified digits tended to be made up uniformly.

[12] The number of gazetteers exceeds the number of counties for two reasons. First, a county may have multiple gazetteers on different topics. Second, some counties may produce various gazetteers across different years. For example, a county may have written one gazetteer during the 1990s and another in the 2000s.

[13] We excluded 28 counties that were merged before 2005. In those scenarios, we could not uniquely link counties in which the gazetteers were compiled to those in the 2005 Population Survey.

of how the timing of SBA practices is recorded in local gazetteers, using information from the Sanhe County Gazetteer in Hebei Province (Commission of Sanhe Gazetteer 1997, p. 713).

We also collected other county-level statistics from local gazetteers to complement our analysis. Appendix Table C.1. presents the summary statistics of these socioeconomic variables from the various gazetteers. Columns 1–3 present the characteristics of the full sample of 2,868 counties in China, while Columns 4–6 present a comparison with the 1,721 baseline counties for which we have precise data on the timing of SBA implementation. The characteristics of these 1,721 counties are comparable to those of the full sample, with no statistically significant differences in the mean values of county characteristics between the two groups. This suggests that the 1,721 counties with data on SBA timing are representative of the broader set of 2,868 counties.

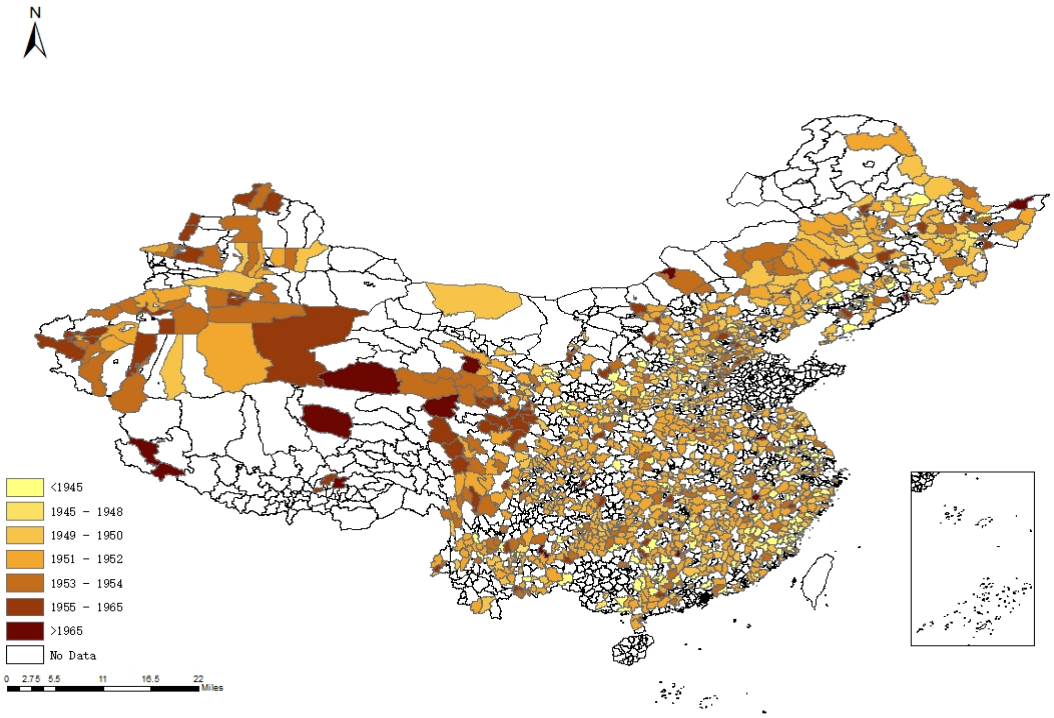


FIGURE 1
Counties and their year of entry into treatment, 1930-1979

Notes: Figure 1 plots county-by-county rollout over time. The county rollout data for the SBA adoption are from county gazetteers.

There may be concerns about sample selection bias due to missing values for the starting year of

SBA in the other 1,147 counties. We examined the pattern of missing values in Panel A of Appendix Table C.3. After adding province fixed effects, the reporting counties had demographic and socioeconomic characteristics similar to the non-reporting counties. Therefore, missing values in gazetteers are not likely to threaten our empirical results in the baseline specification with province-by-cohort fixed effects.[14] Another concern is the exogeneity of the timing of SBA implementation. If the timing is not randomly selected, early- and later-treated counties could exhibit systematic differences. We tested whether county demographic and economic characteristics predict adoption timing, and our results are presented in Panel B of Table C.3. After adding province fixed effects, all county-level characteristics were unrelated to the rollout of SBA, suggesting that exposure to SBA is plausibly exogenous, conditional upon the baseline fixed effects in our main specifications.

Figure 1 presents the regional variation in the rollout of SBA, in which the redder a county, the earlier it implemented SBA. Considerable variation can be observed in the timing of implementation among both developed and poorer counties. More importantly, as indicated in Figure 2, the timing of treatment in the implementing counties varies over the entire period under consideration. To identify the long-term and intergenerational effects of the program, our empirical strategy – discussed in the next section – relies on this variation in the timing of treatment initiation.

2005 Population Survey

We merged the county rollout data with the 1% sample from China's 2005 Population Survey to evaluate how SBA affected the adult earnings of exposed children and the earnings of their offspring. The 2005 Population Survey is suitable for this study for four reasons. First, it recorded self-reported personal monthly income (measured in hundreds of RMB), which we used as the main outcome variable in the DID specification. Earlier or later censuses do not include personal income data. The 2005 Population Survey also reported hours worked in the previous week as an additional measure of income. We calculated hourly wages for census respondents by dividing the monthly income by the number of hours worked. To limit the influence of potential outliers in the income data, we winsorized 0.5%

[14] In addition to province-by-cohort fixed effects, the baseline model includes county and birth year fixed effects.

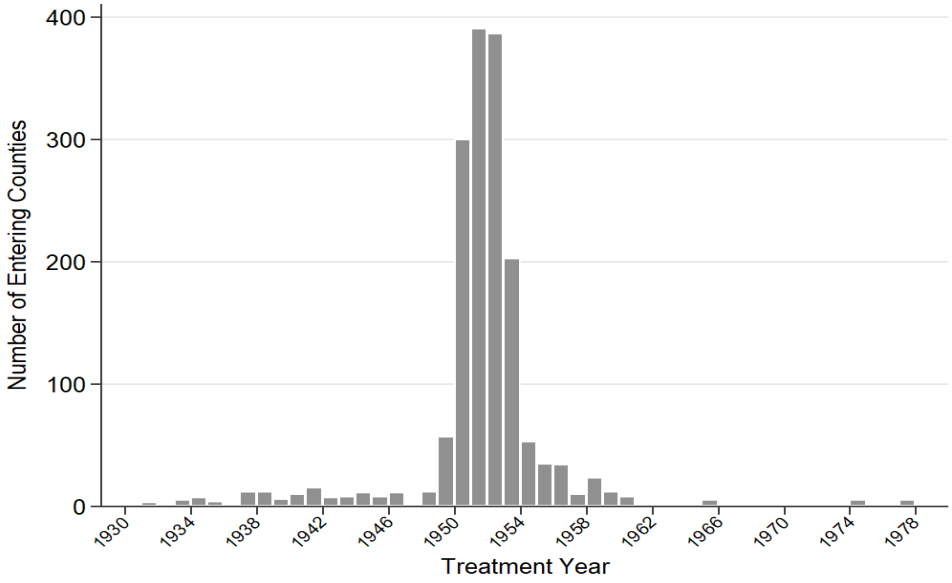


FIGURE 2

Number of counties by their year of entry into treatment, 1-year bins, 1930-1979

Notes: Figure 2 shows the number of counties entering the SBA program in different years. The county rollout data for SBA adoption are from county gazetteers.

from each tail of the income distribution for the baseline sample.[15] In our baseline analysis of the first generation, we focused on cohorts born between 1935 and 1965, who were directly exposed to the rollout of SBA. For the baseline analysis of the second generation, we examined cohorts born between 1970 and 1986, who were at least 18 years old and had, in most cases, completed their compulsory education by 2005. Furthermore, most individuals in the second generation were not exposed to the negative shocks that occurred in the early years of the establishment of the People's Republic of China, such as the Great Famine and the Cultural Revolution.[16]

Second, the 2005 Population Survey gathered data on year of birth and *hukou* county of registration. As mentioned in the background section, a child's birth date and *hukou* county of registration closely approximate the treatment assignment for SBA. Specifically, Appendix Figure B.2. presents

[15]For the 0.5% winsorization, we replaced values above the 99.5th percentile by the value at the 99.5th percentile and values below the 0.5th percentile by the value at the 0.5th percentile.

[16]The early years of the People's Republic of China (1949-1976) were marked by a variety of catastrophic events, including the Great Leap Forward (1958–1962), the Great Chinese Famine (1959–1961), and the Cultural Revolution (1966–1976). A large body of literature has discussed the negative, long-term impacts of these historical events (e.g., Chen et al., 2020; Meng et al., 2015; Meng and Qian, 2009; Chen and Zhou, 2007; Deng and Treiman, 1997; Meng and Zhao, 2017). In the baseline analysis, we excluded pre-1970 cohorts that were exposed to these significant negative shocks.

exposure to SBA by birth cohort, utilizing the baseline sample for the 1935–1965 cohorts.[17] As the timing of SBA varied across counties, substantial variations in exposure to the program can be observed, both within and between cohorts. This finding, combined with the large sample sizes (e.g., over 5,000 census respondents in each birth cohort) allowed us to use the discrete nature of the roll-out of SBA with significant statistical power. Descriptive statistics for the linked sample are presented in Table 1. As indicated in Panel B, the average share of cohorts exposed to SBA was 0.525.

TABLE 1
Summary statistics from the 2005 Census

Variables	(1) Obs.	(2) Mean	(3) Std. Dev.
Panel A. Income variables			
Monthly income	258,267	474.4	528.3
Hourly wage	251,342	2.688	3.063
Hours worked	258,267	42.79	15.08
Panel B. Regressor of interests			
Fractions of cohorts exposed to SBA	398,943	0.525	0.499
Panel C. Control variables			
Age	398,943	55.15	7.096
Male	398,943	0.513	0.500
Share of ethnic minorities	398,943	0.101	0.301
Rural status	398,943	0.711	0.453

Third, the 2005 Population Survey gathered data on the relationship between each household member and the head of the household. This relationship variable allowed us to link parents' records to their children's adult outcomes.

Fourth, the 2005 Census provides several measures of educational outcomes that we could use to test the role of schooling in improving adult incomes. We coded our main education variable, *years of education*, based on the highest level of education an individual received and whether they completed

[17] As it is not possible to define a clear-cut starting date of the program, we assume that it was initiated at the beginning of the year and count the reform year as one year of exposure for the exposed cohorts. To check the robustness of our analyses, we constructed two alternative measures for the exposure to SBA, assuming that the program was introduced either in the middle or at the end of the reform year, so that the reform year is counted as 0.5 year of exposure or not counted for early-life exposure. The results, reported in Appendix Table E.2., are similar to the main results, suggesting that measurement error is unlikely to affect our results.

each tier of schooling. We assumed that an individual received six years of education if they graduated from primary school. We coded higher-level schooling years in a similar fashion. We also utilized dummy variables indicating whether an individual completed primary (elementary) school, middle (junior high) school, high school, or college education as alternative measures of educational attainment. In addition to the income measures and educational outcomes, we utilized several individual-level demographic and socioeconomic characteristics from the 2005 Population Survey, including gender, age, ethnicity, and rural status, as control variables in our main DID specifications.

China Health and Nutrition Survey

We merged the county rollout data with the CHNS data to evaluate how SBA affected the adult earnings of exposed children and the earnings of their offspring. The CHNS is a large-scale, nationally representative, longitudinal survey conducted by the Carolina Population Center at the University of North Carolina at Chapel Hill and the Chinese Center for Disease Control and Prevention. It covers 9 of the 31 provinces and autonomous regions in China. These nine sample provinces cover approximately 45% of China's total population and vary widely in terms of geography, economic development, public resources, and health indicators. Each wave of the survey collects comprehensive information on the demographic attributes, health measures, and economic activities of adults and children.

The CHNS 1989–2011 provides numerous measures of physical and mental health that we utilized to explore additional mechanisms through which exposure to SBA could lead to improved adult earnings. Our first measure of physical health was the self-assessed general health status, which was rated on a 5-point scale (poor, fair, good, very good, or excellent).[18] We also constructed a binary health indicator that takes the value of 1 if the adult reports being in excellent or very good health and 0 otherwise. Our second measure of health was an indicator of whether a person had visited a doctor within the past two weeks due to physical discomfort. In addition, we aggregated these physical health measures to create a physical health index in order to address issues related to multiple hypothesis testing. Consistent with previous research (Hoynes et al., 2016; Boudreaux et al., 2016), the physical health

[18]The scale is arranged in ascending order, with poor equal to 1 and excellent equal to 5.

index was constructed as the average value across the standardized z-scores of each measure of physical health.

For mental health, the first two measures were indicators of whether an individual had experienced feelings of distress or hopelessness in the past four weeks. Similarly, the third and fourth measures indicate whether a person had experienced feelings of restlessness or worthlessness over the past four weeks. We also aggregated these four measures of mental health to create a mental health index, which was calculated as the average value across the standardized z-scores of each measure to address multiple hypotheses.

Consistent with the 2005 Population Survey, we also collected individual-level demographic and socioeconomic characteristics from the CHNS 1989–2011 as control variables in our main DID specifications, including gender, age, ethnicity, and rural status (Panel B of Appendix Table C.2.).

China Family Panel Studies (CFPS)

In addition to the CHNS 1989–2011, we utilized cognitive outcomes from the CFPS to further explore the potential mechanisms behind improvements in income. The CFPS is an ongoing longitudinal survey conducted by the Social Science Survey Institute at Peking University. It covers 25 of the 31 provinces and autonomous regions in China. We utilized the baseline 2010 wave (CFPS-2010) in our main empirical analysis.

To assess cognitive abilities, we utilized scores on the math and verbal exams designed and administered in the CFPS-2010. The math exam assesses knowledge of primary and secondary math and consists of 24 questions.[19] The verbal test is based on 34 questions and measures an individual's ability to spell Chinese characters correctly. The highest possible score for the verbal test is 34 points.[20] In addition, we constructed standardized z-scores for these two cognitive measures (calculated by subtracting the average test scores and dividing by the standard deviation).

[19] These questions are ranked in ascending order of difficulty. Each correct answer is worth one point. The total possible score for the math exam is 24 points.

[20] Similar to the math test, questions in the verbal test are arranged in increasing order of difficulty. One point is awarded for each correct answer and zero points are awarded for questions that are skipped or answered incorrectly.

4 Empirical Methodology

Difference-in-Differences Specification

Equation 1 below presents our baseline staggered DID model for the first generation, which compares individuals exposed to SBA to those born earlier and contrasts children across each birth year for early- and later-treated counties:

$$Y_{icb} = \alpha + \beta SBA_{cb} + \tau X_{icb} + \gamma_c + \mu_b + \delta_{pb} + \epsilon_{icb}, \tag{1}$$

In the equation, Y_{icb} is the natural logarithm of monthly income for individual i in *hukou* county of registration c and birth year b, and SBA_{cb} is a dummy variable indicating whether an individual was born with the assistance of a skilled healthcare professional (it is a function only of a child's birth year b and county c). For example, if a county adopted SBA in 1951, cohorts born in that county in and after 1951 were considered exposed and assigned the value 1, while those born before 1951 were regarded as non-exposed and assigned the value 0.

The estimates of the coefficient β reflect the effect of SBA. With almost 100% compliance rates, the estimated effects come closely approximate the population average treatment effects of SBA. In addition, X_{icb} includes a range of individual characteristics, such as gender, age, ethnicity, and rural status. County fixed effects, γ_c, account for time-invariant location-specific factors that could influence adult earnings. The birth year fixed effects, μ_b, control for secular changes that are consistent across all counties in a particular year. Local, time-varying shocks to adult earnings that impact all individuals are absorbed by province-by-birth-year fixed effects, δ_{pb}.[21] The term ϵ_{icb} represents the regression error, with all standard errors clustered at the county-year level throughout the analysis, allowing for arbitrary correlation in error terms for a specific county-year pair.[22]

[21] It should be noted that the fixed effect for the timing of implementation, θ_t, is absorbed by the county fixed effect. This is because the timing of the adoption of SBA does not vary within county.

[22] In our robustness checks in Appendix Table E.3., we clustered standard errors at the county level. The results reveal that the effect of SBA remains virtually the same when clustering standard errors at the county-year level.

To determine whether the benefits associated with the first generation's exposure to SBA had any spillover effects on their offspring, we adjusted the analysis for the second generation by replacing the first-generation outcomes Y_{icb} with those of their children. Specifically, we replaced SBA_{cb} with a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional were classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional were classified as non-exposed (assigned a value of 0).

The advantage of the specification in Equation 1 lies in the creation of two control groups for SBA_{cb}. The first control group includes cohorts born before the implementation of SBA practices in their *hukou* county of registration. The second control group was created due to the staggered adoption of SBA; children born with the assistance of a skilled attendant in early-treated counties are compared with older, non-exposed children in later-treated counties. The timing of treatment is unrelated to county demographic characteristics after controlling for province-by-birth-year fixed effects, as indicated in Panel B of Appendix Table C.3., These later-treated individuals help to account for unobservable characteristics that may affect selection into treatment and are not eliminated by county fixed effects.

Internal Validity

The key requirement for identification in our DID strategy is the parallel-trend assumption. This assumption states that in the absence of SBA, the average income of individuals exposed to SBA would have followed the same trend as those who were not exposed. While the counterfactual is inherently unobservable, we conducted a series of robustness tests to validate the parallel-trend assumption. Figures 4 and 6 present the results of an event study we conducted. This study aimed to examine the correlation between treatment status and adult outcomes for the first generation, as well as the connection between parents' treatment status and their offspring's adult outcomes for the second generation. Specifically, we employ a set of dummy variables indicating the timing of exposure to SBA. We discovered that the estimates of placebo exposure (i.e., cohorts in the first generation born before

the initial implementation of SBA in their *hukou* county of registration) are very small and statistically insignificant. This lack of impact of SBA on non-treated cohorts supports the parallel trend of our DID strategy. In order to address concerns about differential cohort trends more effectively, we hypothetically assumed that SBA had been implemented 1, 2, 3, 4, or 5 years earlier than the actual implementation year in each county and then replicated the main regressions in Appendix Table E.10.. Consistently, we found no effect of SBA on adult income for both the first and second generations in these placebo settings, providing further support for the staggered DID approach.

We demonstrate that our main results are robust to varying sample selections. For the first generation, we utilized the 1935–1965 cohorts in the baseline estimation. Since a few countries implemented the SBA program prior to 1945, there is a concern that our findings might be influenced by potential confounding factors related to World War II. To address this issue, we recalculated the baseline model using the 1945–1965 cohorts. The results, presented in Column 6 of Table E.1., reveal that the coefficients related to SBA remain consistent with the baseline specifications and are statistically significant. This reaffirms that the impact of SBA is unaffected by adjustments for World War II effects. As additional robustness checks, we reran the baseline model using data from the 1935–1958, 1935–1960, 1940–1958, 1940–1960, and 1940–1965 cohorts. The results are presented in Columns 1–5 of Appendix Table E.1. The effect of SBA remains consistently robust.

For the second generation, we excluded cohorts born before 1976 in the baseline estimation. These cohorts experienced catastrophic events during their early childhood in the early years of the People's Republic of China, such as the Great Chinese Famine (1959–1961) and the Cultural Revolution (1966–1976). Numerous studies have discussed the negative long-term impacts of these events on various socioeconomic outcomes (Chen et al., 2020; Meng et al., 2015; Meng and Qian, 2009; Chen and Zhou, 2007; Deng and Treiman, 1997; Meng and Zhao, 2017). As a robustness check, we re-ran the baseline model using data from the 1970–1987, 1970–1985, 1970–1983, 1965–1983, 1965–1985, and 1960–1985 cohorts. The results are presented in Appendix Table F.1. Consistent with previous studies, these historic events had a negative impact on the earnings of the affected cohorts, resulting in somewhat smaller estimated effects. Despite the significant negative shocks, the estimated effects

of SBA remain significant, suggesting that our results are robust even when including the pre-1976 cohorts.

In addition, there may be concerns that our estimates are biased by other concurrent policies and factors that coincide with the rollout of SBA by county and only affect children born after SBA was first introduced in the county. Through our systematic review of contemporaneous historical policies, we found that no candidate confounders adhere to these precisely described patterns. Nonetheless, to further address concerns regarding concurrent policies, we controlled for the potential impacts of the compulsory education system and the one-child policy on the second generation, which could increase overall lifetime earnings in China. The results are presented in Table F.3. in the Appendix. The estimated coefficients on SBA remain consistent with those in the baseline specifications and are statistically significant, thus confirming the robustness of our estimates. Additional details can be found in Sections E and F of the Appendix.

Furthermore, in Appendix Tables E.8. and Table F.5., in the Appendix, we demonstrate that the estimates for the long-term and intergenerational effects of SBA are robust to accounting for heterogeneity in the treatment effects over time or across groups.[23] In Table E.3., our estimates are also robust to various empirical specifications that relax the classical DID assumption and allow for differential growth trajectories among different provinces.

Finally, we conducted the randomization inference procedure, as suggested by Bertrand et al. (2004), and the results are presented in Appendix Figure E.1. Here, we randomly assigned a year of SBA implementation to each county and estimated the DID specification as a placebo treatment effect. This procedure was repeated 1,000 times to form a distribution of placebo treatment effects. The point estimates obtained in the main regressions were significant compared to the distribution of placebo effects, thus corroborating the validity of the DID approach.[24]

[23] For this robustness check, we used the methods proposed by De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2021).

[24] Please see Section E in the Appendix for more detail.

5 Short-Term Impacts of Skilled Birth Assistance on Neonatal Mortality

Prior to the implementation of SBA, neonatal mortality rates were alarmingly high, as all births were attended exclusively by TBAs. These attendants often relied on harmful practices and lacked proper hygiene during delivery, contributing to maternal and neonatal mortality rates as high as 30% to 40%. Following the introduction of SBA, the proportion of births attended by skilled birth attendants steadily increased, as illustrated in Appendix Figure B.1.

Table 2 presents the estimated immediate effects of SBA on county-level neonatal mortality rates, derived from annual data recorded in local gazetteers. Different control variables were used in each DID specification. The specification in Column 1 controls for county, birth year, and province-by-birth-year fixed effects, indicating that the SBA reform reduced neonatal mortality rates by 9.8% (48% of the pretreatment mean). As county characteristics may affect neonatal mortality, our preferred specification in Column 2 adds controls for county population size, gross domestic product (GDP) per capita, and urbanization rate. Including these additional variables lowers the estimated effect to a 9.1% reduction in neonatal mortality, which remains statistically significant.

We made several adjustments to the base specifications to ensure the robustness of these results to county-time effects, which may be related to the timing of SBA practices. In particular, our specifications in Columns 3–6 build on the model in Column 2 by incorporating linear cohort trends interacted with county characteristics from Table C.3. These characteristics include population size, share of industrial employment, share of ethnic minorities, and average years of schooling. The estimates obtained are very similar to those presented in Columns 1–2, which enhances our confidence in our baseline model.

With lower neonatal mortality rates, birth cohort sizes are likely to have increased following the implementation of SBA. To investigate this, we examined whether the introduction of SBA is associated with larger birth cohorts, as observed in the 2005 Population Survey. Specifically, we modified Equation 1 to use county-level birth cohort sizes as the dependent variable. The results, presented in Columns 1–2 of Appendix E.4., indicate that the estimated coefficients on SBA are approximately 0.2

TABLE 2

	Neonatal mortality rates					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
SBA	0.098***	0.091***	0.089***	0.088***	0.090**	0.089**
	(0.045)	(0.045)	(0.049)	(0.046)	(0.056)	(0.056)
Observations	11,871	11,871	11,871	11,871	11,871	11,871
R-squared	0.816	0.841	0.815	0.751	0.781	0.793
Baseline FE	YES	YES	YES	YES	YES	YES
Controls		YES	YES	YES	YES	YES
Log(County population in 1935) * Trend			YES	YES	YES	YES
Share of industrial employment in 1936 * Trend				YES	YES	YES
Share of ethnic minorities * Trend					YES	YES
Average years of schooling * Trend						YES

Notes: This table shows the regression results for Equation 1. Data are a linked sample of county-level neonatal mortality rates with the county-by-county rollout of SBA. The dependent variable in each column is the neonatal mortality rates in county c and birth year b. SBA is a function only of a child's birth year b and *hukou* registration county c. The main specification in each column includes birth year, county, province-by-birth-year fixed effects. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

and statistically significant. This suggests that birth cohort sizes increased by approximately 20% after the introduction of SBA.

Event Study: Differential Effects Across Birth Cohorts

To ensure the robustness of these findings, we conducted an event study to assess the validity of the parallel-trend assumption of the DID approach. This assumption states that trends in neonatal mortality rates and birth cohort sizes across different counties would have been similar in the absence of exposure to SBA. Empirically, we replaced the main measure (SBA_{cb}) in Equation 1 with a set of dummy variables for a child's birth year relative to the year in which SBA was implemented in their *hukou* county of registration. These variables were measured in one-year bins (e.g., -7-, -6, -5, -4, and so on). Relative to the baseline model, this flexible specification allowed us to capture the differential effects across all windows of exposure, including the placebo exposure of children born before the introduction of SBA.

Panels A and B in Figure 3 present the estimates of the timing dummies with 95% confidence intervals for neonatal mortality rates and birth cohort sizes, respectively. These estimates utilize the same

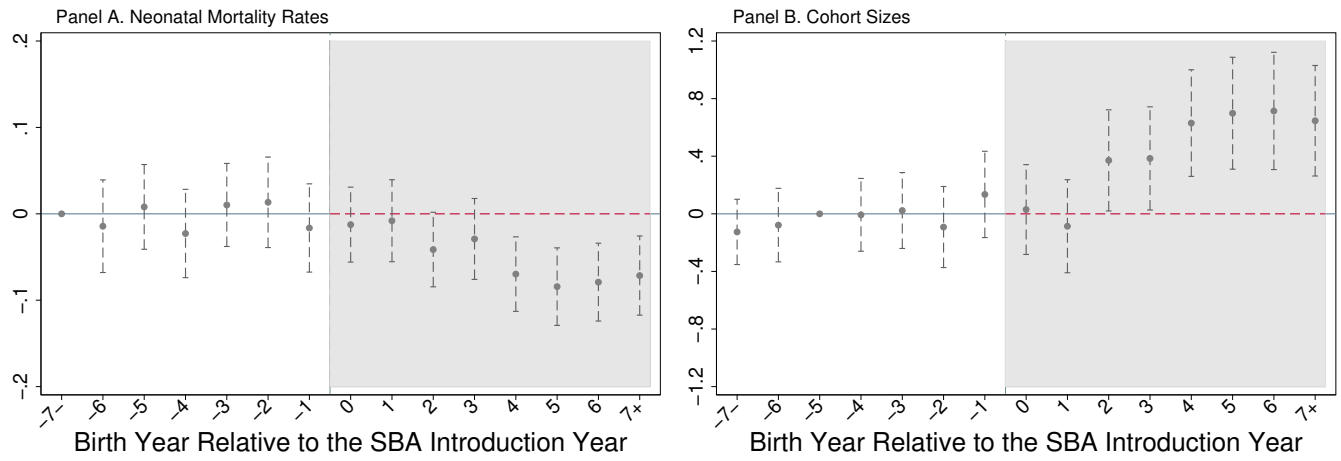


FIGURE 3

The short-run effect of SBA on neonatal mortality rates and cohort sizes

Notes: Figure 3 plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of categorical measures of different birth years relative to the introduction year of SBA in the county. On the x-axis, the event time "-7-" represents children born at least seven years before SBA was introduced in the county. Other event times, such as "1" and "2," indicate children born one or two years after SBA was implemented in the county. The model estimated uses the same set of preferred county, birth year, and province-by-birth-year fixed effects as in Eq. 1. Standard errors are clustered at the county level.

set of preferred county, birth year, province-by-birth-year fixed effects, and controls as in Column 2 of Table 2. The reported coefficients capture the effects of SBA on neonatal mortality rates and birth cohort sizes with varying exposures to the program. On the x-axis, the event time "-7-" represents children born at least seven years before SBA was introduced in the county. Other event times, such as "1" and "2," indicate children born one or two years after SBA was implemented in the county. Consistent with the results in Table 2, Figure 3 highlights a systematic and persistent decrease in neonatal mortality rates and a corresponding increase in cohort sizes for cohorts exposed to SBA. The estimates of placebo exposure (e.g., -7-, -6, -5, and so on) are small and statistically insignificant. This zero or negligible impact on children not exposed to SBA because the program arrived too late supports the validity of our DID specification. In addition, as indicated in the event-study figure, the county-year-level birth cohort size remained elevated, but leveled off a few years after implementation, suggesting that the event study did not merely capture a general growth trend in birth cohort size or neonatal mortality rates.

Overall, we anticipate that exposure to SBA could enhance an individual's earnings as an adult. However, these same factors could also result in compositional changes. For example, if the introduction of SBA leads to increased fertility among low-income families, this may have a negative impact on the composition of birth cohorts and subsequently bias our estimates downward. We addressed this potential issue by examining whether the SBA reform influenced fertility rates among the parents of the cohorts being studied. The 2005 Population Survey provides data on the total number of children born to each female respondent. We modified Equation 1 to use the number of children born at the individual level as the dependent variable. The results are presented in Columns 3–4 of Appendix Table E.4. It can be observed that the estimated coefficients on SBA are small and insignificant, suggesting that the SBA reform had no effect on fertility.

According to the fetal origins hypothesis, the increase in child survival rates following the introduction of SBA, coupled with minimal fertility selection, suggests that our long-term estimates of improvements in adult income, educational attainment, and health outcomes among children are likely conservative. Prior to the rollout of SBA, only the most resilient infants survived.

6 Long-Term and Multi-Generational Effects of Skilled Birth Attendance

6.1 First Generation Income Estimates

This section assesses the long-term impacts of SBA on adult earnings. Table 3 presents estimates of the increases in adult incomes driven by SBA, using different control variables in each DID specification. The specification in Column 1 controls for county, birth year, and province-by-birth-year fixed effects, indicating that SBA exposure leads to a 1.7% increase in adult earnings. As individual characteristics may affect adult outcomes, our preferred specification in Column 2 includes gender, age, ethnicity, and rural status as additional control variables.[25] The addition of these individual characteristics decreases

[25]Given that the age variable is calculated by subtracting the birth year from the 2005 Census year, it is collinear and absorbed by the birth year fixed effects.

the estimates to a 1.5% income gain, and the effect remains statistically significant.

We made make several adjustments to the base specifications to ensure the robustness of these results to county-time effects, which may be related to the timing of SBA practices. In particular, our specifications in Columns 3–6 build on the model in Column 2 by adding linear cohort trends interacted with county characteristics in Table C.3., including population size, share of industrial employment, share of ethnic minorities, and average years of schooling. The estimates obtained are very similar to those shown in Columns 1-2, which enhances our confidence in our baseline model.

TABLE 3
Effects of skilled birth attendance on adult income (first generation)

	log(Monthly income)					
	(1)	(2)	(3)	(4)	(5)	(6)
SBA	0.017**	0.015**	0.012**	0.012**	0.013**	0.012**
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	258,267	258,267	223,051	223,051	223,012	223,051
R-squared	0.385	0.507	0.452	0.452	0.452	0.452
Baseline FE	YES	YES	YES	YES	YES	YES
Controls		YES	YES	YES	YES	YES
Log(County population in 1935) * Trend			YES	YES	YES	YES
Share of industrial employment in 1936 * Trend				YES	YES	YES
Share of ethnic minorities * Trend					YES	YES
Average years of schooling * Trend						YES

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. The dependent variable in each column is the natural logarithm of monthly income for individual i. SBA is a function only of a child's birth year b and *hukou* registration county c. The baseline specification in Column 1 includes birth year, county, and province-by-birth-year fixed effects. Column 2 adds individual characteristics on the basis of Column 1, including age, gender, ethnicity, and rural status. Columns 3, 4, 5, 6, and 7 build on the specification in Column 2 by adding the natural logarithm of the county population in 1935, share of industrial employement in 1936, share of ethnic minorities, and average years of schooling interacted with linear time trend. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix Table D.1. also presents the heterogeneous effects of SBA on adult earnings by gender and level of development (more-developed eastern provinces versus less-developed non-eastern provinces).[26] In Columns 1-2, we observe that children from eastern provinces benefited more from SBA than their peers from non-eastern provinces. The higher estimates for exposure to SBA in more developed provinces are justifiable, as these provinces provide better job prospects for individuals to

[26] Eastern provinces were the original focus of Chinese economic liberalization in the 1980s. Thus, these provinces have higher levels of economic growth, foreign direct investments, and personal incomes.

maximize their earning potential. Columns 3-4 indicate that the effects did not vary by gender.[27]

Building on the estimates of adult incomes, we conducted an event study to assess the validity of the parallel-trend assumption of the DID approach. This assumption states that trends in adult outcomes across different counties would have been similar in the absence of exposure to SBA. Figure 4 presents the estimates of the timing dummies with 95% confidence intervals for log value of monthly income. We used the same set of preferred county, birth year, province-by-birth-year fixed effects, and controls as in Column 2 of Table 3. Consistent with our results in Table 3, Figure 4 highlights a systematic and persistent increase in adult earnings for those exposed to SBA. The estimates of placebo exposure (e.g., -7-, -6, -5, and so on) are small and statistically insignificant, corroborating the validity of our DID specification.

To provide context for the long-term effects of SBA, our estimates of the earnings improvement from SBA exposure are comparable to those found in a recent study on early childhood exposure to county-level health departments (CHDs) in rural America (Hoehn-Velasco, 2021). Specifically, Hoehn-Velasco (2021) determined that CHD operations before the age of five can increase men's later-life earnings by 2%–5%. In addition, the estimated income gains are similar to those seen in studies on school lunch reform (Lundborg et al., 2022). Lundborg et al. (2022) found that Swedish pupils who participated in the free school lunch program throughout their primary school years had 3% higher lifetime incomes than their peers who did not participate. However, the benefit of SBA is lower than the estimated positive long-term effects of early childhood intervention programs, such as deworming treatments, malaria eradication, and the use of food stamps (Baird et al., 2016; Bleakley, 2007, 2010; Hoynes et al., 2016). For example, malaria eradication has been associated with a 50% increase in later-life income (Bleakley, 2010). In the current study, lower estimates for exposure to SBA are expected, as these intervention programs targeted highly selected and disadvantaged groups, where the scope for long-term income improvements is likely greater from the outset.

[27] Our estimates of the earnings improvement for males are not statistically different from those estimated for females (*p-value=0.926*).

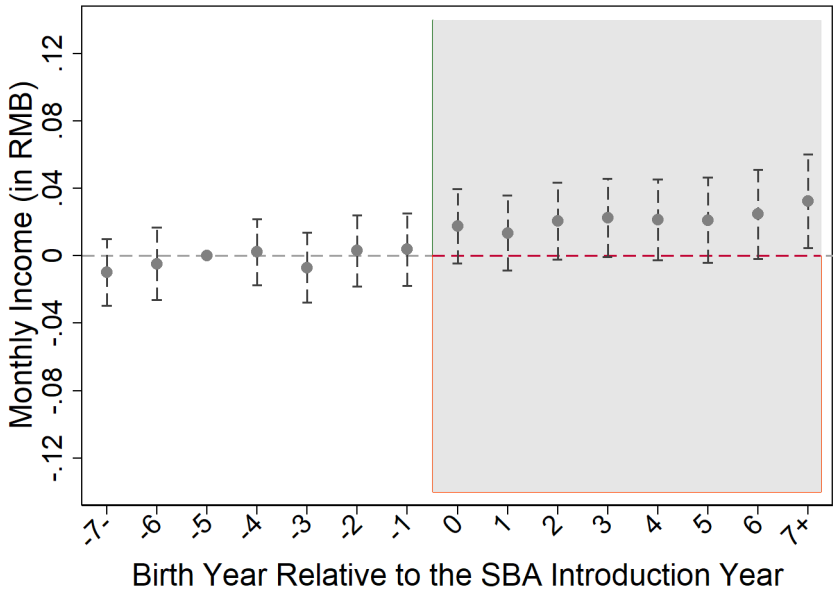


FIGURE 4
Effect of SBA on adult income in the first generation

Notes: Figure 4 plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of categorical measures of different birth years relative to the introduction year of SBA in the county. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 to 1965. On the x-axis, the event time "-7-" represents children born at least seven years before SBA was introduced in the county. Other event times, such as "1" and "2," indicate children born one or two years after SBA was implemented in the county. The model estimated uses the same set of preferred county, birth year, province-by-birth-year fixed effects and individual characteristics as in Column 2 of Table 3. Standard errors are clustered at the county level.

Decomposition of the Increase in Adult Income

After estimating the overall impact of SBA on adult earnings, we decomposed the effect by estimating Equation 1 on the natural logarithm of hourly wage and, separately, on the natural logarithm of hours worked in the past week, using the preferred specification in Column 2 of Table 3. The estimates are presented in Appendix Table D.2.. As can be observed, Column 1 replicates the regression in Column 2 of Table 3 as benchmark estimates. Column 2 examines the effects of SBA on hourly wages and finds similar results. Comparing the estimates of β in Columns 1 and 2, we observe that the increase in adult earnings results primarily from the increase in marginal productivity. Meanwhile, Column 3 examines the effect of SBA on hours worked in the past week. As presented in the table, the coefficient of β is only 0.003 and not statistically significant. These findings suggest that income improvements result

primarily from the higher marginal productivity of individuals, rather than a trade-off between leisure and time spent in the labor force.

As higher-income individuals generally have better access to pension and medical insurance, we examined the long-term impacts of SBA on ownership of these benefits, using our preferred specification in Column 2 of Table 3. The estimates, as presented in Appendix Table J.1., indicate that children born with the assistance of a skilled birth attendant are more likely to have access to pension and medical insurance in adulthood. To ensure the robustness of these results, we conducted an event study for pension and medical insurance in Panels A and B of Appendix Figure J.1., respectively. Consistent with initial expectations, Appendix Figure J.1. highlights a systematic and persistent increase in access to pension and medical insurance for children exposed to SBA. Moreover, the estimates of placebo exposure for children before the introduction of SBA in the county are small and statistically insignificant, confirming the validity of our DID estimates.

Robustness Tests

In Section E of the Appendix, we discuss the robustness of the main income results and address several confounding factors that may contribute to the positive link between the introduction of SBA and an increase in adult income. We also performed additional robustness tests to strengthen a causal interpretation of our results. Our main results are robust to different sample choices (Appendix Table E.1.), to the use of alternative measures for SBA exposure (Appendix Table E.2.), and to various specifications that allow differential growth trajectories for different provinces and cluster standard errors at the county-year level (Appendix Table E.3.). Moreover, the effect on income is not driven by changes in fertility (Appendix Table E.4.), contemporaneous historical events (Appendix Table E.5.), or income outliers, such as top- and low-earners (Appendix Table E.6.). Furthermore, the effect of SBA on adult income was robust after controlling for linear and quadratic county-specific trends (Appendix Table E.7.), upon accounting for recent concerns that staggered DID estimates may be biased in the presence of heterogeneity in treatment effects over time or across groups (Appendix Table E.8.), and accounting for multiple hypothesis testing (Appendix Table E.9.). Finally, the point estimates obtained in

the main regressions are significant compared with the distribution of placebo effects obtained in the randomization inference procedure (Appendix Figure E.1.).

6.2 First Generation Health and Educational Outcomes

After estimating the income improvements driven by SBA, we then examined the health and educational outcomes. Specifically, we considered the effects of SBA on the physical and mental health, educational attainment, and cognitive development of the children exposed to it. While these are interesting outcomes in their own right, we also evaluated them as potential mediators through which SBA could influence adult income.

Physical Health

TABLE 4
The effect of SBA on physical health (first generation)

	Self-reported health status			
	5-point scale	Dummy	Doctor visit	Physical health index
	(1)	(2)	(3)	(4)
SBA	0.346**	0.109**	-0.131***	0.479***
	(0.149)	(0.050)	(0.039)	(0.113)
$\bar{Y}$ of control group	4.627	0.915	0.101	0.461
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	3,641	3,641	3,641	3,641
R-squared	0.229	0.204	0.200	0.192

Notes: This table shows the regression results for Equation (1). Data are a linked sample of the CHNS 1989-2011 with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. The dependent variable in Column 1 is a 5-point scale of self-reported health status. The dependent variable in Column 2 is a binary health indicator that takes the value of 1 if the adult reports being in excellent or very good health and 0 otherwise. The dependent variable in Column 3 is a binary health indicator that takes the value of 1 if the adult has had a doctor's visit due to physical discomfort within the past two weeks and 0 otherwise. The dependent variable in Column 4 is a summary index calculated as the average of standardized z-scores for the three measures of physical health in Columns 1-3. SBA is a function only of a child's birth year b and *hukou* registration county c. The specification in each column includes birth year, county, province-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level;** significant at the 5 percent level; * significant at the 10 percent level.

Access to skilled delivery at birth may improve children's long-term physical health and development by reducing the risk of postpartum infections, such as sepsis, and ensuring proper management

of delivery complications. For the first generation, we accessed this potential channel using the sample of cohorts born between 1935 and 1965 from the CHNS 1989–2011. Specifically, we modified Equation 1 to use the self-assessed general health status rated on a 5-point scale as the dependent variable. Column 1 of Table 4 presents the effect of SBA on general health status in adulthood, using the same set of preferred control variables as in Column 2 of Table 3. In line with our expectations, the introduction of SBA improves self-assessed health status.

Next, we repeated the analysis using a dummy variable in Column 2 of Table 4, indicating whether an individual is in excellent or very good health. The results indicate that SBA increases the chances of being in good health by 10.9 percentage points. In Column 3 of Table 4, we replicated the analysis using an indicator of whether an individual had visited a doctor due to physical discomfort within the past two weeks. Consistent with the estimates in Columns 1 and 2, SBA reduces the probability of doctor's visits due to physical discomfort by 13.1 percentage points. These findings provide evidence that there is an improvement in the overall health of individuals born with the assistance of a skilled attendant. These estimates remain very similar when accounting for multiple hypothesis testing with FDR-corrected p-values (Panel A in Appendix Table I.1.).

To further ensure the robustness of the results, we conducted an event study for the physical health index in Panel A of Figure 5. Consistent with initial expectations, it was found that SBA exposure leads to physical health improvements. The estimates of placebo exposure are small and statistically insignificant, corroborating the validity of our DID specification. Our estimate of the long-term health improvement of exposure to SBA is qualitatively similar to the corresponding estimates for Medicaid (Boudreaux et al., 2016) and the food stamp program (Hoynes et al., 2016). In addition, these findings align with previous studies (Pitt et al., 2012; Baird et al., 2016; Bhalotra et al., 2017; Hoehn-Velasco, 2021), suggesting that enhancements in health during early childhood lead to improved physical well-being in adulthood.

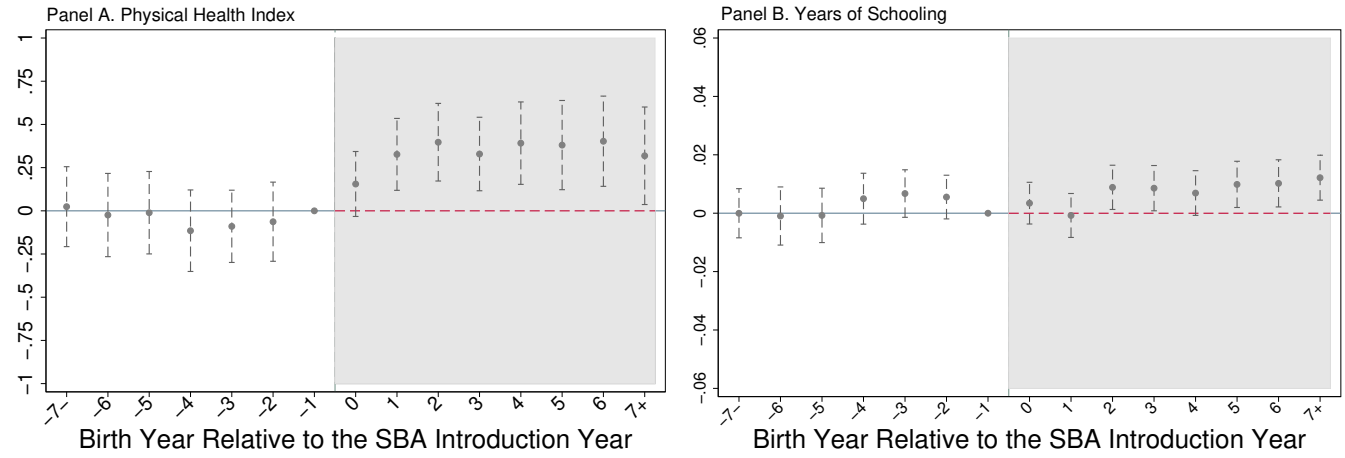


FIGURE 5
Event Study Plots for Health and Education Outcomes (first generation)

Notes: Figure 5 plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of categorical measures of different birth years relative to the introduction year of SBA in the county. Data are a linked sample. On the x-axis, the event time "-7-" represents children born at least seven years before SBA was introduced in the county. Other event times, such as "1" and "2," indicate children born one or two years after SBA was implemented in the county. The model estimated uses the same set of preferred county, birth year, province-by-birth-year fixed effects and individual characteristics as in Table 3. Standard errors are clustered at the county level.

Mental Health

In addition to the benefits mentioned above, exposure to SBA could reduce mental distress and improve mental well-being by providing children with a higher quality of life and improved productivity. To analyze this connection, we estimated the long-term effects of SBA on four measures of mental health using Equation 1. The results are presented in Table 5. The estimates indicate significant improvement in psychological well-being in adulthood for individuals exposed to SBA. Specifically, as shown in Columns 1–2, access to SBA, compared to its absence, is associated with a 13.7 percentage point reduction in the probability of experiencing feelings of worthlessness and a decrease of 15.9 percentage points in the likelihood of experiencing feelings of distress. Similarly, in Columns 3–4, the estimates suggest that SBA reduces the probability of feeling restless by 8.6 percentage points and decreases feelings of hopelessness by 5.4 percentage points.

In addition, in Column 5, we demonstrate that exposure to SBA reduces the mental health index

TABLE 5
The effect of SBA on mental health (first generation)

	(1)	(2)	(3)	(4)	(5)
Variables	Worthless	Distress	Restless	Hopeless	Mental health index
SBA	-0.137**	-0.159***	-0.086**	-0.054***	-0.767***
	(0.051)	(0.049)	(0.017)	(0.013)	(0.154)
$\bar{Y}$ of control group	0.048	0.026	0.031	0.039	-0.107
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	3,641	3,641	3,641	3,641	3,641
R-squared	0.093	0.089	0.068	0.054	0.120

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the CHNS 1989-2011 with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in Column 1 is an indicator that takes the value of 1 if a person has had upset feelings in the past four weeks and 0 otherwise. The dependent variables in Columns 2-4 are indicators that take the value of 1 if a person has experienced feelings of hopelessness, restlessness, or worthlessness in the past four weeks, respectively. The dependent variable in Column 5 is a summary index calculated as the average of standardized z-scores for the four measures of mental health in Column 1-4. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

by 0.767 standard deviations. The effect of SBA on mental health remains significant even after accounting for multiple hypotheses using FDR-corrected p-values (Panel B in Table I.1.). These findings confirm the results of related studies (Adhvaryu et al., 2019; Persson and Rossin-Slater, 2018) which suggest that better early-life health decreases the probability of mental distress and enhances mental well-being in adulthood.

Educational Outcomes

Children who experience SBA are more likely to become healthier, attend school regularly, and achieve higher levels of education. This increase in the quantity of education is associated with higher earnings in adulthood, as presented in Table 3. To explore the potential connection between SBA and schooling, we examined the sample of children born between 1935 and 1965 from the 2005 Population Survey and estimated the effects of SBA on literacy and years of education using Equation 1. In addition, we used dummy variables indicating whether an individual completed elementary (primary) school, middle (junior high) school, or high school as alternative measures of educational attainment. The first

column of Table 8 presents the results for years of schooling, indicating that exposure to SBA increased education by approximately 0.07 years. Based on previous estimates of returns to schooling, this 0.07-year increase in schooling corresponds to an increase of approximately 0.4% in adult incomes.[28] We reached a similar conclusion after controlling for schooling in a regression on the effect of SBA on income, as evidenced by a 10% decrease in the coefficient (Table H.1.). To ensure the robustness of the results, we present event-study estimates of the effect of SBA on years of education in Panel B of Figure 5. As expected, exposure to SBA enhances years of schooling. The pattern in the figure reveals no signs of pre-trends; moreover, the point estimates of the placebo exposure are close to zero and insignificant.

TABLE 6
The effect of SBA on educational attainment (first generation)

Variables	Years education	Primary	Junior high	Senior high	College
	(1)	(2)	(3)	(4)	(5)
SBA	0.07**	0.024**	0.022**	0.020*	0.025***
	(0.028)	(0.005)	(0.010)	(0.011)	(0.010)
$\bar{Y}$ of control group	5.416	0.467	0.281	0.193	0.012
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	315,069	315,069	315,069	315,069	315,069
R-squared	0.336	0.272	0.217	0.295	0.092

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in Column 1 is years of education. The dependent variables in Columns 2-5 are dummy variables indicating whether an individual completed primary (elementary) school, middle (junior high) school, high school, or college education. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Columns 2 and 3 present the results using the completion of primary and middle school, respectively. It can be observed that exposure to SBA increases the likelihood of primary and junior high school graduation by 2.4 and 2.2 percentage points, respectively. The effect on schooling remains significant even after adjusting for multiple hypothesis testing using FDR-corrected p-values (Panel A in Appendix Table I.2.). The educational benefit of SBA is substantial and comparable to estimates

[28] For this calculation, we used results from Wang (2013), who concluded that an additional year of schooling in China raises income by approximately 5.3%. For the current study's sample of children born between 1935 and 1965 from the 2005 Population Survey, the income increase is 0.07 x 5.3, which is approximately 0.4%.

from previous studies on early childhood exposure to health insurance (Huang and Liu, 2023; Miller and Wherry, 2019) and iodine supplements (Deng and Lindeboom, 2022; Field et al., 2009). In addition, the improvements in schooling are comparable to those estimated for the establishment of new maternity wards in Sweden (Lazuka, 2023) and access to smallpox vaccines (Lazuka and Jensen, 2024).

Cognitive Outcomes

Furthermore, as schoolwork is an energy-intensive activity, children who are healthier are more likely to perform better in their studies. This improvement in the quality of education could also lead to higher earnings in adulthood. To investigate this potential connection, we examined the sample of 1935–1965 cohorts from the CFPS-2010 and estimated the effects of SBA on two cognitive measures, math and verbal test scores, based on Equation 1. The first two columns of Appendix Table G.1. present the results for verbal scores, indicating no statistically significant change in verbal test scores after the introduction of SBA. By contrast, Column 3 indicates a significant improvement in math scores following exposure to SBA. The introduction of SBA is associated with an increase of 1.62 percentage points in math test scores, which is significant at the 5% level. Similarly, Column 4 indicates a 0.26 increase in the standard deviation in math scores after the implementation of SBA.[29] This pattern is consistent with previous studies on cognitive outcomes, which find stronger and more significant effects on math test scores (Chen et al., 2022; Harris and Sass, 2009). Specifically, the increases in verbal and math test scores are comparable to those estimated in recent studies on early childhood exposure to health insurance (Huang and Liu, 2023) and tap water (Chen et al., 2022). The effects of SBA on the identified cognitive measures are robust after accounting for multiple hypothesis testing using FDR-corrected p-values, as indicated in Panel B of Appendix Table I.2.

[29] Here, a one standard deviation increase in math score is equivalent to answering 4.5 more questions correctly on the math test. As such, a child with exposure to SBA would answer around 1.1 more questions correctly than a child with no exposure.

6.3 Second Generation Income Estimates

Building on our analysis of income improvements among the first generation directly exposed to the rollout of SBA, we used the same model in Equation 1 to assess whether and how the benefits associated with first-generation exposure to SBA had any spillover effects on the income of their offspring. Table 7 presents the results relating to income. The specification in Column 1 controls for county, birth year, and province-by-birth-year fixed effects, indicating that a mother's exposure to SBA leads to a 2.3% increase in their offspring's adult earnings. As individual characteristics may affect adult outcomes, our preferred specification in Column 2 includes gender, age, ethnicity, and rural status as additional control variables.[30] The addition of these individual characteristics increases the estimates to a 2.6% income gain, and the effect remains statistically significant. The higher estimates for increases in adult earnings in the second generation are justifiable. Many individuals from the first generation experienced significant hardships during their early childhood due to catastrophic events in the early years of the People's Republic of China, such as the Great Chinese Famine (1959-1961) and the Cultural Revolution (1966-1976). Numerous studies have discussed the negative long-term impacts of these events on various socioeconomic outcomes (Chen et al., 2020; Meng et al., 2015; Meng and Qian, 2009; Chen and Zhou, 2007; Deng and Treiman, 1997; Meng and Zhao, 2017). Moreover, most individuals in the first generation were employed in state-owned enterprises, where wages tended to be lower.

Appendix Table D.3. presents the heterogeneous effects of SBA on adult earnings in the second generation. As indicated in Columns 1–2, the income increase is more pronounced when the mother, rather than the father, was exposed to SBA. In Columns 3-4, we show that children from eastern provinces benefited more from SBA than their peers in non-eastern provinces, Furthermore, Columns 5-6 reveal that the effects are more pronounced among female than male offspring. These results suggest that SBA not only increased overall adult income levels, but also contributed to reducing social inequality.

We also conducted an event study to assess the validity of the parallel-trend assumption of our DID

[30] Given that the age variable is calculated by subtracting the birth year from the 2005 Census year, it is collinear and absorbed by the birth year fixed effects.

TABLE 7
Effects of SBA on adult income (second generation)

	log(Monthly income)					
	(1)	(2)	(3)	(4)	(5)	(6)
SBA	0.023***	0.026***	0.025***	0.026***	0.024***	0.023***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	97,423	97,423	97,423	97,423	97,423	97,423
R-squared	0.442	0.482	0.483	0.485	0.488	0.442
Baseline FE	YES	YES	YES	YES	YES	YES
Controls		YES	YES	YES	YES	YES
Log(County population in 1964) * Trend			YES	YES	YES	YES
Share of urban population in 1964 * Trend				YES	YES	YES
Share of ethnic minorities in 1964 * Trend					YES	YES
Average years of schooling * Trend						YES

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. The dependent variable in each column is the natural logarithm of monthly income for individual i. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year *b* and county *c*. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The baseline specification in Column 1 includes birth year, county, and province-by-birth-year fixed effects. Column 2 adds individual characteristics on the basis of Column 1, including age, gender, ethnicity, and rural status. Columns 3, 4, 5, and 6 build on the specification in Column 2 by adding the natural logarithm of the county population in 1964, share of urban population in 1964, the natural logarithm of grain output in 1965, share of ethnic minorities in 1964, and average years of schooling interacted with linear time trend. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

approach. Empirically, we replaced the main measure (SBA_{mcb}) in Equation 1 with a set of dummy variables representing the parents' birth year relative to the introduction year of SBA in their *hukou* county of registration, measured in 1-year bins (e.g., -5, -4, and so on). Relative to the baseline model, this flexible specification allowed us to capture the differential effects across all windows of exposure, including placebo exposure for children whose parents were both born before the implementation of SBA in their county.

Figure 6 presents the estimates of the timing dummies with 95% confidence intervals for the log value of monthly income. This analysis used the same set of preferred county, birth year, province-by-birth-year fixed effects, and controls as in Column 2 of Table 7. The coefficients reported reflect the spillover effects of SBA on the adult income of the second generation, varying based on their parents' exposure to the program. On the x-axis, the event time "-5-" represents cases in which both the mother and father were born at least five years before the introduction of SBA in their county.

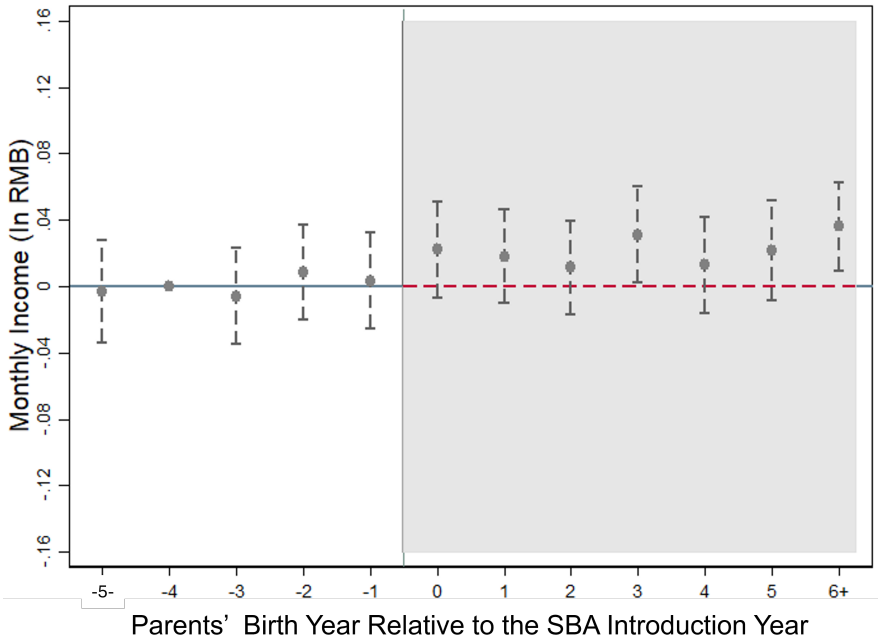


FIGURE 6
Effect of SBA on adult income in the second generation

Notes: Figure 6 plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of categorical measures of the parents' birth year relative to the introduction year of SBA in their *hukou* registration county, measured in 1-year bins (e.g., -5, -4, and so on). Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and includes adults who were born between 1976 and 1986. On the x-axis, the event time "-5-" represents cases where both the mother and father were born at least five years before SBA was introduced in their county. Other event times, such as "1" and "2," indicate at least one parent was born one or two years after the implementation of SBA in their county. The model estimated uses the same set of preferred county, birth year, province-by-birth-year fixed effects and individual characteristics as in Table 7. Standard errors are clustered at the county level.

Other event times, such as "1" and "2," indicate that at least one parent was born one or two years after the implementation of SBA in their county. Consistent with our results in Table 7, Figure 6 highlights a systematic and sustained increase in adult earnings for the second generation, where at least one parent was exposed to SBA. The estimates of placebo exposure (e.g., -5, and so on) are small and statistically insignificant. This negligible impact on children whose parents were not exposed to SBA, due to the program's later implementation, supports the validity of our DID specification.

Our estimates of the earnings improvement in the second generation are of similar magnitude to those estimated in a recent study on school lunch reform (Lundborg et al., 2022). Lundborg et al. (2022) found that Swedish pupils who participated in the free school lunch program throughout their primary school years had 3% higher lifetime incomes than their peers who did not participate. In

addition, the estimated income gains are qualitatively similar to the estimates on additional medical care for at-risk newborns (Lazuka, 2023). Lazuka (2023) found that the establishment of new maternity wards in Sweden significantly increased the proportion of hospital births, resulting in a 4.3% rise in labor income for individuals born in hospitals compared to those born at home.

Decomposition of the Increase in Adult Income

After estimating the overall impact on adult earnings in the second generation, we decomposed the effect by estimating Equation 1 using the natural logarithm of hourly wage and, separately, the natural logarithm of hours worked in the past week. We used the preferred specification in Column 2 of Table 7. The estimates are presented in Appendix Table D.4.. Column 1 replicates the regression in Column 2 of Table 7 as benchmark estimates. Column 2 examines the spillover effects of SBA on hourly wages and yields similar results. In Column 3, we analyze the spillover effect of SBA on hours worked in the past week, revealing a statistically significant coefficient for β of 0.013. These findings suggest that income improvements stem partly from higher marginal productivity and partly from an increase in hours worked.

Robustness Tests

In Section F of the Appendix, we discuss the robustness of the main income results and address several confounding factors that may contribute to the positive link between the spillover effects of SBA and increased adult income in the second generation. Our main results are robust to different sample choices (Appendix Table F.1.) and to various specifications that account for differential growth trajectories in different provinces and cluster standard errors at the county level (Appendix Table F.2.). In addition, the effect on income is not influenced by contemporaneous historical events (Appendix Table F.3.) or income outliers, such as top- and low-earners (Appendix Table F.4.). Moreover, the spillover effect of SBA on adult income remains robust upon accounting for recent concerns about potential bias in staggered DID estimates due to the presence of heterogeneity in the treatment effects over time or across groups (Appendix Table F.5.), and accounting for multiple hypotheses testing (Appendix Ta-

ble E.9.). Finally, the point estimates from the main regressions are significant when compared to the distribution of placebo effects obtained through the randomization inference procedure (Appendix Figure F.1.).

6.4 Second Generation Health and Education Outcomes

After estimating the income improvements in the second generation driven by SBA, we went on to address health and educational outcomes. Specifically, we considered the effects of SBA on the second generation's physical and mental health, educational attainment, and cognitive development.

Physical Health

The higher incomes of the first generation exposed to SBA allow these parents to afford quality healthcare, nutritious food, and safe living environments, all of which contribute to a healthier upbringing. In addition, healthier parents may engage in positive health behaviors and pass on beneficial genetic traits to their children. For the second generation, we assessed this potential connection using a sample of cohorts born between 1976 and 1986 from the CHNS 1989–2011. The estimates are presented in Table 8. Column 1 indicates the effect of SBA on the general health status of the second generation in adulthood, using the same set of preferred control variables as in Column 2 of Table 7. As expected, children with at least one parent exposed to SBA reported better self-assessed health status. The estimates in Column 2 indicate that having at least one parent born with access to SBA increases the likelihood of offspring being in good health by 8.7 percentage points. Similarly, Column 3 reveals that parental exposure to SBA reduces the probability of doctor visits due to physical discomfort in their children by 11.7 percentage points. These results provide strong evidence of the spillover effects of SBA on overall health improvements in the second generation.

To ensure the robustness of these results, we conducted an event study for the physical health index, presented in Panel A of Figure 7. As expected, parental exposure to SBA led to significant physical health improvements in their children. The estimates for placebo exposure are small and statistically insignificant, further supporting the validity of our DID specification. Our findings on the spillover

effects of SBA on health improvements in the second generation are consistent with those reported for vaccination (Lazuka and Jensen, 2024) and the food stamp program (Hoynes et al., 2016). Moreover, these results align with research (East et al., 2023) which demonstrates that a mother's in utero exposure to Medicaid significantly increases her children's birthweight and reduces the incidence of very low birthweight.

TABLE 8
The effect of SBA on physical health (second generation)

	Self-reported health status			
	5-point scale	Dummy	Doctor visit	Physical health index
	(1)	(2)	(3)	(4)
SBA	0.241**	0.087**	-0.117***	0.305***
	(0.109)	(0.036)	(0.025)	(0.125)
$\bar{Y}$ of control group	4.665	0.932	0.061	0.067
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	4,138	4,138	4,138	4,138
R-squared	0.211	0.240	0.208	0.219

Notes: This table shows the regression results for Equation 1.. Data are a linked sample of the CHNS 1989-2011 with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. The dependent variable in Column 1 is a 5-point scale of self-reported health status. The dependent variable in Column 2 is a binary health indicator that takes the value of 1 if the adult reports being in excellent or very good health and 0 otherwise. The dependent variable in Column 3 is a binary health indicator that takes the value of 1 if the adult has had a doctor's visit due to physical discomfort within the past two weeks and 0 otherwise. The dependent variable in Column 4 is a summary index calculated as the average of standardized z-scores for the three measures of physical health in Columns 1-3. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The specification in each column includes birth year, county, province-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Mental Health

Parents who experience SBA tend to enjoy better health, which often enables them to engage more actively with their children, fostering a sense of security and emotional well-being. Moreover, financial stability can help reduce household stress, creating a nurturing environment in which children feel safe and supported. Together, these factors lay a strong foundation for positive mental health outcomes in children. To analyze this connection, we estimated the spillover effects of SBA on four measures of

second-generation mental health using Equation 1. The results are presented in Table 9. As indicated in Columns 1–2, having at least one parent with access to SBA is associated with a 9.5 percentage point reduction in the likelihood of experiencing feelings of worthlessness and a 8.9 percentage point decrease in the probability of experiencing feelings of distress. Similarly, Columns 3–4 reveal that parents' exposure to SBA reduces the likelihood of their children feeling restless by 12.7 percentage points and lowers the probability of experiencing feelings of hopelessness by 6.2 percentage points. In addition, Column 5 demonstrates that parents' access to SBA reduces the mental health index of their children by 0.413 standard deviations.

Overall, these estimates indicate significant improvements in the psychological well-being of children in adulthood when at least one parent has access to SBA. These findings align with related studies (Adhvaryu et al., 2019; Persson and Rossin-Slater, 2018), which suggest that better early-life health of parents decreases the likelihood of mental distress and enhances the mental well-being of their offspring.

TABLE 9
The effect of SBA on mental health (second generation)

	(1)	(2)	(3)	(4)	(5)
Variables	Worthless	Distress	Restless	Hopeless	Mental health index
SBA	-0.095***	-0.089**	-0.127**	-0.062***	-0.413***
	(0.049)	(0.051)	(0.047)	(0.021)	(0.131)
$\bar{Y}$ of control group	0.048	0.026	0.031	0.039	-0.107
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	4,138	4,138	4,138	4,138	4,138
R-squared	0.250	0.261	0.193	0.249	0.265

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the CHNS 1989-2011 with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in Column 1 is an indicator that takes the value of 1 if a person has had upset feelings in the past four weeks and 0 otherwise. The dependent variables in Columns 2-4 are indicators that take the value of 1 if a person has experienced feelings of hopelessness, restlessness, or worthlessness in the past four weeks, respectively. The dependent variable in Column 5 is a summary index calculated as the average of standardized z-scores for the four measures of mental health in Column 1-4. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Educational Outcomes

Children who benefit from the spillover effects of SBA are more likely to attend school consistently and attain higher levels of education. This increase in education contributes to greater earnings in adulthood. To investigate the potential link between parents' exposure to SBA and educational outcomes in the second generation, we analyzed data from the 2005 Population Survey relating to children born between 1976 and 1986, with the results presented in Table 10. The first column of Table 10 indicates that having at least one parent exposed to SBA is associated with an increase of approximately 0.07 years in the children's education. Based on prior estimates of returns to schooling, this 0.07 year increase corresponds to an approximate 0.95% rise in adult income.[31] This conclusion remains consistent when we account for schooling in a regression model estimating the effect of SBA exposure on second-generation income. In this model, the coefficient of SBA decreases by 26% compared to the coefficient of SBA in baseline model (see Table H.2.), suggesting that education plays a significant mediating role.

To confirm the robustness of these findings, event-study estimates of the spillover effects of parental SBA exposure on second-generation educational attainment are presented in Panel B of Figure 7. As expected, the results indicate that children with at least one parent exposed to SBA report higher years of schooling. Furthermore, the event-study analysis reveals no evidence of pre-trends. The point estimates for placebo exposure are close to zero and statistically insignificant.

Columns 2–4 present results for the completion of primary school, middle school, and high school, respectively. While no significant effect was observed for primary school completion, parents' exposure to SBA increased the likelihood of middle school and high school graduation by 2.0 and 0.8 percentage points, respectively.

The spillover effect of SBA on educational attainment is substantial and aligns with estimates from prior studies on early childhood exposure to health insurance (Huang and Liu, 2023; Miller and Wherry, 2019) and iodine supplementation (Deng and Lindeboom, 2022; Field et al., 2009). Moreover,

[31] This calculation is based on Wang (2013), which estimates that an additional year of schooling in China increases income by about 5.3%. For our sample, the income effect is calculated as 0.18 x 5.3%, or approximately 0.95%.

TABLE 10
The effect of SBA on educational attainment (second generation)

Variables	Years education (1)	Primary (2)	Junior high (3)	Senior high (4)	College (5)
SBA	0.18**	0.01	0.02**	0.008**	0.002
	(0.088)	(0.05)	(0.003)	(0.003)	(0.010)
$\bar{Y}$ of control group	9.010	0.936	0.768	0.250	0.030
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	125,960	125,960	125,960	125,960	125,960
R-squared	0.491	0.272	0.239	0.312	0.113

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1976 to 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in Column 1 is the years of education. The dependent variables in Columns 2-5 are dummy variables indicating whether an individual completed primary (elementary) school, middle (junior high) school, high school, or college education. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

the magnitude of the observed improvements in schooling is comparable to the effects of school lunch programs in Sweden (Lundborg et al., 2022) and access to safe drinking water in China (Zhang and Xu, 2016).

Cognitive Outcomes

Since schoolwork is an energy-intensive activity, healthier children are more likely to excel academically. This improvement in educational outcomes can, in turn, lead to higher earnings in adulthood. To examine this connection, we analyzed the 1976–1986 cohorts from the CFPS-2010 and estimated the impact of parents' exposure to SBA on their children's math and verbal test scores, using Equation 1. The first two columns of Appendix Table G.2. present the results for verbal scores, indicating no statistically significant changes among children with at least one parent exposed to SBA. However, Column 3 reveals a significant improvement in math scores among the second generation. Specifically, children with at least one parent who was exposed to SBA exhibit an increase of 1.081 percentage points in math test scores compared to children whose parents were not exposed to SBA – a result significant at the 5% level. Similarly, Column 4 indicates an increase of 0.13 standard deviations in

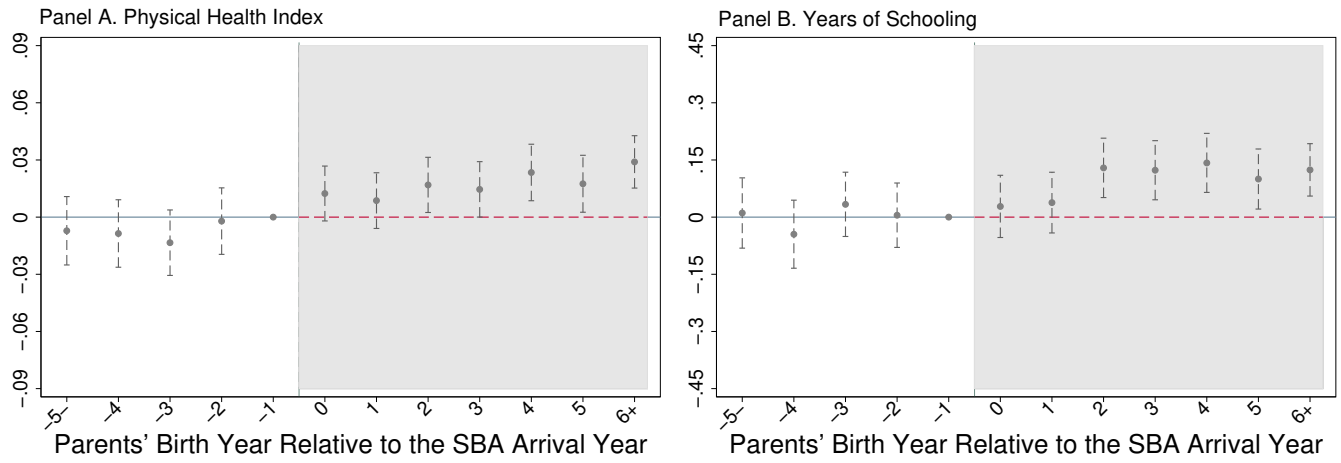


FIGURE 7
Event Study Plots for Health and Education Outcomes (second generation)

Notes: Figure 7 plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of dummy variables representing the parents' birth year relative to the introduction year of SBA in their county. On the event time "-5-" represents cases where both the mother and father were born at least five years before SBA was introduced in their county. Other event times, such as "1" and "2," indicate at least one parent was born one or two years after the implementation of SBA in their county. The model estimated uses the same set of preferred county, birth year, and province-by-birth-year fixed effects as in Eq. 1. Standard errors are clustered at the county level.

math scores.[32] The spillover effects of parental exposure to SBA on these cognitive measures in their children remain robust after adjusting for multiple hypothesis testing using FDR-corrected p-values, as demonstrated in Panel B of Appendix Table I.2. These cognitive gains are qualitatively similar to the corresponding estimates for the Deferred Action for Childhood Arrivals (DACA) program (Kuka et al., 2020), the Matlab Maternal and Child Health and Family Planning (MCH-FP) program implemented in Bangladesh (Barham, 2012), sustained cognitive activity (Brown et al., 2022), and the Institute of Nutrition of Central America and Panama (INCAP) early-life intervention program conducted in Guatemala (Maluccio et al., 2009).

[32] An increase of one standard deviation in math scores corresponds to answering approximately 4.5 additional questions correctly on the math test. Consequently, children with at least one parent exposed to SBA would answer about 1.3 more questions correctly than their peers with no parental exposure to SBA.

7 Discussions and Conclusions

The WHO has advocated skilled birth attendance (SBA), by a midwife or doctor, as the most critical factor in reducing maternal and neonatal mortality rates, as well as a key strategy for promoting the long-term well-being of children (WHO, 1999). While existing research confirms that SBA significantly lowers maternal and neonatal mortality rates, few studies have explored the long-term benefits for children born under such care, particularly in developing countries (Anderson et al., 2020; Lazuka, 2023). Furthermore, although economic, epidemiological, and child development literature suggests that the benefits of SBA could extend across generations, little is known about whether and to what extent safer birth practices lead to improved adult outcomes in future generations (Miller and Wherry, 2019).

The county-by-county rollout of SBA throughout China from the 1930s to the 1970s provides a unique opportunity to examine how SBA improves the adult earnings of an individual and of their offspring. To conduct this analysis, we digitized a novel county-level dataset on the precise timing of SBA from over 3,000 book-length local gazetteers, matching it with the 2005 Population Survey and individual-level household surveys.

We then employed a cohort DID specification, in which our identifying assumption was that exposure to SBA is plausibly exogenous, conditional on the baseline fixed effects in our main specifications. A number of specification checks support this assumption. We demonstrated that the SBA reform substantially reduced neonatal mortality, and that the exposure to SBA increases adult income by 1.5%. In addition, we found evidence that the benefits associated with treated generations' exposure extend to subsequent offspring. Children with at least one parent exposed to SBA experience a 2.6% increase in monthly income in adulthood compared to those whose parents were not exposed to SBA. This increase is driven by both higher hourly wages and more hours worked, with the effect being more pronounced among female than male offspring. Our main results are robust to a wide range of alternative specifications. In addition to estimating the overall increase in adult earnings, we investigated the potential mechanisms underlying the documented long-term and multi-generational benefits of

SBA. By exploring rich information in the data from the 2005 Population Survey, the CFPS-2010, and the CHNS (1989–2011) we found that the main productivity effect of SBA operates through improved adult physical and mental health, and better educational attainment and cognitive skills. This finding aligns with the literature on health-related income gains (Pitt et al., 2012; Baird et al., 2016).

SBA has a high internal rate of return. We calculated the returns on SBA by following the methodology of Goodman-Bacon (2021), who examined the long-term effects of childhood Medicaid eligibility on adult health and economic outcomes. Due to the unavailability of archival records for SBA costs before 1949, our calculations focus on the second generation. Specifically, with a median annual income of 4,128 RMB for the 1976–1986 cohorts in the 2005 Population Survey, our DID estimates suggest that exposure to SBA increases adult earnings by approximately 2.6%, equivalent to 108 RMB annually.[33] Considering that the average cost of SBA was approximately 179 RMB per child in 2005, the cumulative income gain over just 2 years of adulthood would more than offset the expected cost of SBA at childbirth.[34] Given that the total financing cost of the program per child is 179 RMB and given its long-term benefits for health outcomes and years of schooling, a conservative estimate of the long-term annual return on this investment would be approximately 60.3% (=108/179). These back-of-the-envelope calculations imply that the implementation of SBA is extremely cost-beneficial. If we consider the cost savings related to improved physical and mental health, as well as the long-term and multi-generational benefits of improved educational attainment and cognitive skills, the true returns of SBA would be much higher

Our results are directly relevant to ongoing policy debates regarding the merits of SBA in developing countries. In many developing nations, the majority of births are attended by TBAs, who have no formal education or training. Experts have claimed that replacing TBAs with skilled birth attendants would substantially reduce infant and maternal mortality rates (Thompson et al., 2011). This claim is based primarily on historical studies conducted in Sweden and the United States, with little empiri-

[33] Specifically, we estimated the annual benefit of SBA by multiplying the median income of the 1976-1986 birth cohorts in 2005 by the estimated marginal effect of SBA on income.

[34] In principle, the costs of the SBA program from 1976–1986 should be inflated to their value as at 2005 for accurate cost calculations. However, a review of historical records reveals that the growth rate of SBA costs per child closely tracked the inflation rate. As a result, the 2005 SBA expenditure reasonably approximates the inflated costs of the program during the 1976–1986 period.

cal evidence from developing countries to support it or justify the promotion of SBA in countries of the Global South. Moreover, no credible evidence has been presented to examine the impacts of SBA across multiple generations. Using a variety of survey and administrative data, we have demonstrated that SBA could generate substantial long-term and multi-generational benefits. The infant mortality rates and per capital incomes in China – the world's largest developing country – during the period of SBA rollout are comparable to those of the Global South countries today. Our results and methods therefore contribute credible evidence to this important policy issue.

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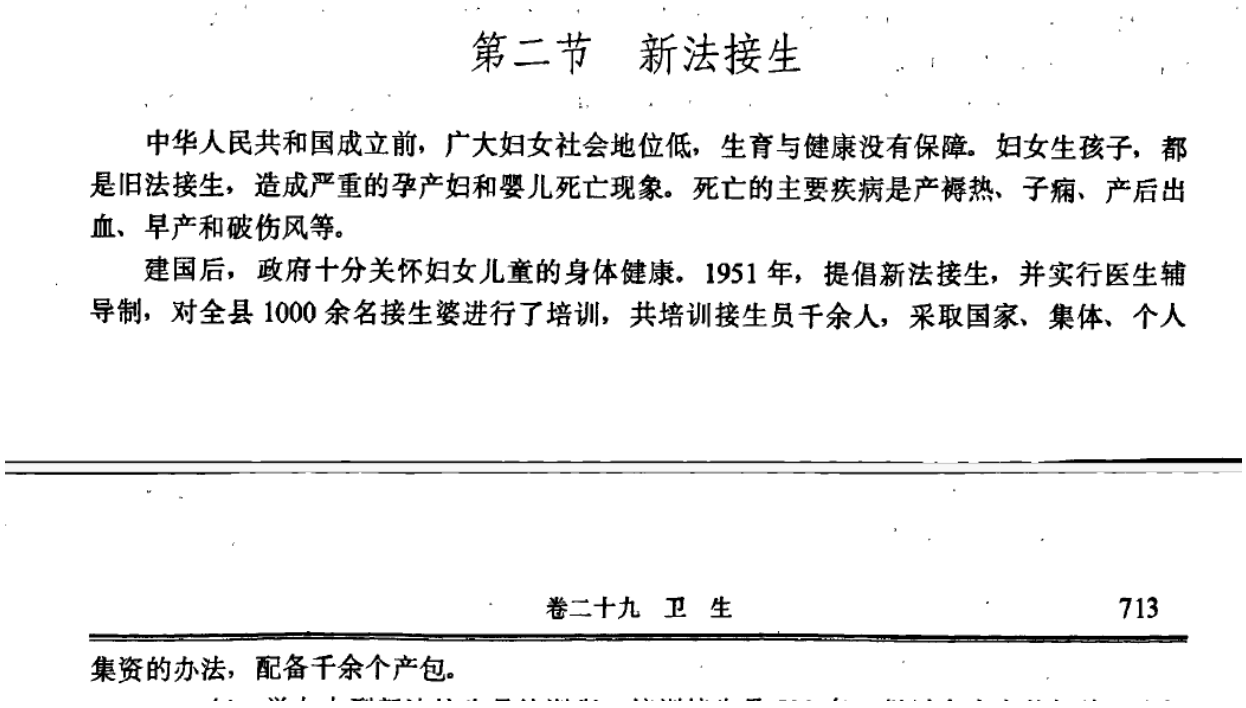
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Appendices

Appendix A: Examples of the records on SBA from local gazetteers



第二节　新法接生

中华人民共和国成立前，广大妇女社会地位低，生育与健康没有保障。妇女生孩子，都是旧法接生，造成严重的孕产妇和婴儿死亡现象。死亡的主要疾病是产褥热、子痫、产后出血、早产和破伤风等。

建国后，政府十分关怀妇女儿童的身体健康。1951 年，提倡新法接生，并实行医生辅导制，对全县 1000 余名接生婆进行了培训，共培训接生员千余人，采取国家、集体、个人

卷二十九　卫　生　713

集资的办法，配备千余个产包。

FIGURE A.1.
Examples of the records on SBA from local gazetteers

Notes: Figure A.1. shows the information on the introduction of SBA from the local gazetteers of Sanhe County in Hebei province, which documented that "In 1951, SBA was implemented in Sanhe county."

Appendix B: Fraction of cohorts exposed to SBA

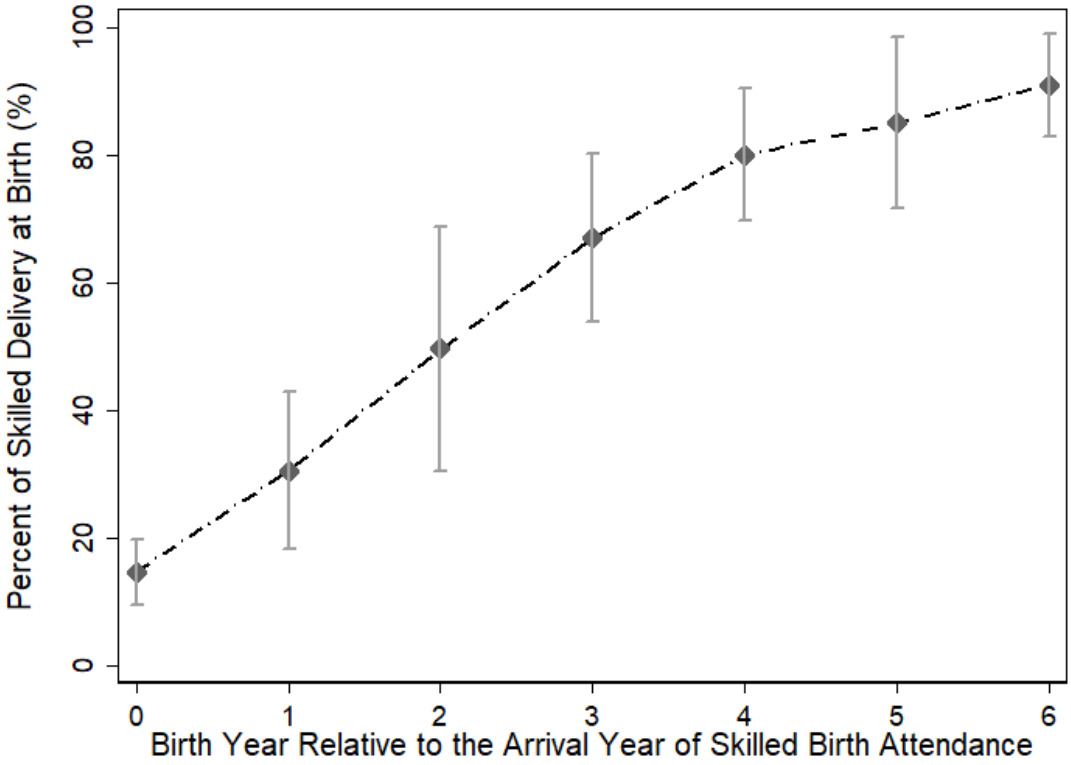


FIGURE B.1.
Fraction of cohorts exposed to SBA.

Notes: Figure B.1. plots fractions of skilled delivery at birth from local gazetteers. Error bars represent ±1 standard deviation from the mean

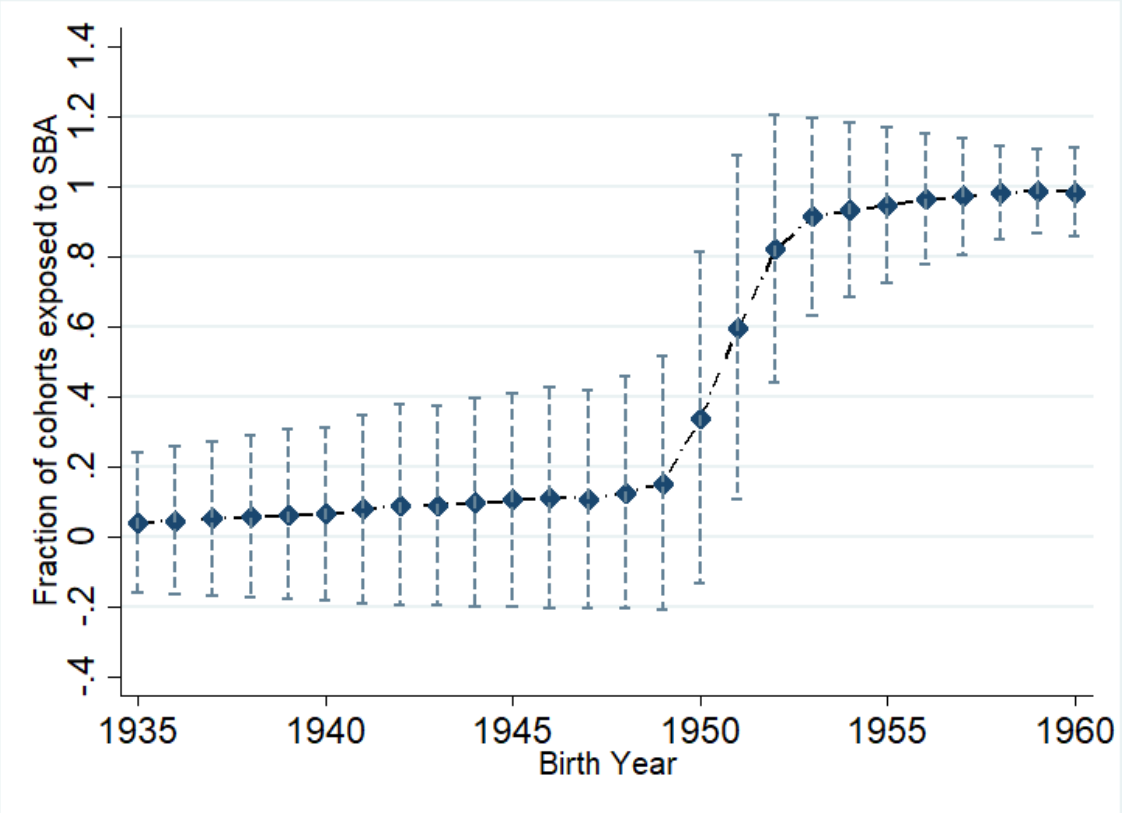


FIGURE B.2.
Fraction of cohorts exposed to SBA, by birth cohort.

Notes: Figure B.2. plots the 1935-1965 birth cohorts from the 2005 Census (individual-level observations). Error bars represent ±1 standard deviation from the mean

Appendix C: Summary statistics

TABLE C.1.
Summary statistics of county characteristics from local gazetteers

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Baseline sample		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Industrial employment share in 1936	2,868	0.092	0.042	1,712	0.093	0.037
County population in 1935 (Log)	2,868	4.230	0.318	1,712	4.511	0.365
County population in 1964(Log)	2,868	12.360	0.856	1,721	12.380	0.783
Share of urban population in 1964	2,868	10.030	11.710	1,721	9.356	10.430
Grain output in 1965(Log)	2,868	11.170	0.870	1,721	11.200	0.857
Share of ethnic minorities	2,868	0.158	0.295	1,721	0.138	0.276
Average years of education	2,868	8.460	2.038	1,721	8.433	1.789

TABLE C.2.
Summary statistics of health and education outcomes

	Obs.	Mean	Std.Dev.
Panel A. 2005 Census			
Years education	115,069	9.369	3.216
Primary school graduation	115,069	0.958	0.204
Junior high graduation	115,069	0.812	0.391
Senior high graduation	115,069	0.269	0.443
College graduation	115,069	0.033	0.178
Panel B. CHNS			
Gender	3,385	0.510	0.500
Age	3,385	12.399	3.160
Share of ethnic minorities	3,385	0.241	0.428
Rural status	3,385	0.805	0.396
Self-reported health status	3,385	4.641	0.644
Health status dummy	3,385	0.962	0.191
Doctor visit	3,385	0.086	0.281
Physical health index	3,385	-0.007	1.005
Upset	3,385	0.089	0.285
Hopeless	3,385	0.044	0.205
Restless	3,385	0.055	0.227
Worthless	3,385	0.035	0.184
Mental health index	3,385	-0.000	0.993
Panel C. CFPS			
Verbal test score	2,142	23.071	8.307
Verbal test z score	2,142	-0.001	0.993
Math test score	2,142	13.680	5.898
Math test score z score	2,142	-0.001	0.991
Gender	2,142	0.493	0.500
Age	2,142	24.735	5.538
Share of ethnic minorities	2,142	0.099	0.299

TABLE C.3.
Determinants of the rollout of SBA from local gazetteers

Variables	(1) 1936 Industrial Employment Share	(2) Pop 1935 (Log)	(3) Pop 1964 (Log)	(4) Share of Urban Pop 1964	(5) Grain Output 1965 (Log)	(6) Share of Ethnic Minorities	(7) Average Years Education
Panel A. Whether the missing values are random							
Whether policy time is missing (without province FE)							
	0.072*	0.045**	0.009	-0.001**	0.009	0.000	-0.070**
	(0.043)	(0.021)	(0.009)	(0.001)	(0.010)	(0.004)	(0.028)
Observations	2,165	2,083	2,785	2,769	2,491	2,800	2,800
R-squared	0.000	0.000	0.000	0.001	0.000	0.000	0.002
Whether policy time is missing (with province FE)							
	0.001	0.004	-0.002	-0.001	0.008	0.006	0.037
	(0.008)	(0.015)	(0.012)	(0.001)	(0.012)	(0.005)	(0.040)
Observations	2,165	2,083	2,782	2,766	2,488	2,795	2,795
R-squared	0.067	0.051	0.088	0.087	0.095	0.099	0.099
Panel B. The timing of policy implementation is random							
Policy time (without province FE)							
	0.162**	0.041	-0.114	-0.050**	-0.301	0.442***	2.010**
	(0.24)	(0.31)	(0.290)	(0.024)	(0.256)	(0.162)	(0.811)
Observations	1,621	1,713	1,798	1,798	1,729	1,739	1,739
R-squared	0.001	0.004	0.000	0.006	0.002	0.014	0.007
Policy time (with province FE)							
	0.121	0.172	-0.236	-0.024	-0.348	0.144	-0.928
	(0.327)	(0.256)	(0.339)	(0.023)	(0.267)	(0.221)	(1.221)
Observations	1,621	1,713	1,798	1,798	1,729	1,739	1,739
R-squared	0.048	0.051	0.084	0.084	0.097	0.093	0.093

Notes: *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix D: Heterogeneous effects of SBA on adult earnings

TABLE D.1.
Heterogeneous income effects of SBA (first generation)

	log(Monthly income)			
	(1)	(2)	(3)	(4)
VARIABLES	Eastern	Non-Eastern	Female	Male
SBA	0.019***	0.010**	0.013**	0.014**
	(0.005)	(0.004)	(0.004)	(0.003)
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	157,243	101,024	143,583	145,519
R-squared	0.351	0.404	0.435	0.433

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 to 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. All models include birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE D.2.
Decomposition of the long-term income effects of SBA (first generation)

Variables	log(Monthly income)	log(Hourly wage)	log(Hours worked)
	(1)	(2)	(3)
SBA	0.015**	0.009***	0.003
	(0.005)	(0.003)	(0.006)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Observations	258,267	251,342	258,267
R-squared	0.507	0.497	0.252

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. The dependent variable of Column 1 is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The dependent variable of Column 2 is the natural logarithm of hourly wage and the dependent variable of Column 3 is the natural logarithm of hours worked last week. SBA is a function only of a child's birth year b and *hukou* registration county c. All models include birth year, county, and province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE D.3.
Heterogeneous income effects of SBA (second generation)

	log(Monthly income)					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Mother Exposed	Father Exposed	Eastern	Non-Eastern	Female	Male
SBA	0.028***	0.022**	0.031***	0.020**	0.029**	0.021**
	(0.062)	(0.071)	(0.073)	(0.074)	(0.050)	(0.029)
Baseline FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Observations	56,211	57,804	46,121	46,308	43,583	45,519
R-squared	0.517	0.404	0.415	0.440	0.354	0.343

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. All models include birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE D.4.
Decomposition of SBA effect on adult income (second generation)

Variables	log(Monthly income) (1)	log(Hourly wage) (2)	log(Hours worked) (3)
SBA	0.026***	0.012***	0.013**
	(0.006)	(0.004)	(0.005)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Observations	97,423	95,197	97,423
R-squared	0.483	0.492	0.229

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. The dependent variable of Column 1 is the natural logarithm of monthly income. The dependent variable of Column 2 is the natural logarithm of hourly wage and the dependent variable of Column 3 is the natural logarithm of hours worked last week. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). All models include birth year, county, and province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix E: Robustness analyses (first generation)

Sample Adjustments

In Appendix Table E.1., we show that our main results are robust to different sample choices. In Columns 1-2, the estimated effects of SBA are almost identical to our baseline specification when we use the sample of children born between 1935 and 1958 and those born between 1935 and 1960 instead. As additional robustness check, we reran the baseline model with the 1940-1958, 1940-1960, 1940-1965, and 1946-1965 cohorts. Columns 3-6 in Table E.1. present the results, respectively. The estimated effects of the SBA remain significant, suggesting that our results are robust to different cohort selections.

There may be a concern that top- and low-earners could lead to biased estimates. Thus, we winsorize the tails of income distribution from the baseline sample in Table E.6. to control for the influence of potential outliers. In Column 1, the one percent tails are winsorized from the sample.[35] The effects of SBA are quite similar to the baseline. Columns 2-5 additionally winsorize the two, three, four, and five percent tails. The effects of skilled delivery at birth on earnings remain highly similar. These processes serve as additional checks of the earnings results, thus ensuring that our findings are not driven by outliers.

Alternative Measures for SBA Exposure

In Appendix Table E.2., we construct two alternative measures for the exposure to SBA, assuming that the program has been introduced in the middle (Column 2) or at the end of the year (Column 3). In Column 2, the SBA arrival year is counted as 1/2 year of exposure. In Column 3, the SBA arrival year is not counted for exposure. The results are quite similar to the main results, thus confirming that measurement errors are not likely to affect our results.

Sensitivity Checks with Alternative Specifications

In Appendix Table E.3., we perform several sensitivity checks of our main results. In Column 1, we replace province-by-cohort fixed effects in our baseline specification with province-specific time trends to allow differential growth trajectories for different provinces. This modification to the baseline DID specification does not appreciably change our main results. In Column 2, we clustered standard errors at the county-year level instead, allowing arbitrary correlations in error terms for a given county-year

[35] For the 1% winsorization, we replaced values above the 99th percentile by the value at the 99th percentile and values below the 1st percentile by the value at the 1st percentile.

pair. The results indicate that the effect of SBA remains highly similar. Column 3 includes both modifications from Columns 1-2, and as can be seen, the effect of SBA remains highly robust.

Heterogeneous Treatment Effects

There are some concerns that the two-way fixed-effect estimates in staggered DID designs may be biased in the presence of heterogeneous treatment effects over time or across groups. We check the robustness of our results by using the methods proposed by De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2021). Table E.8. presents the results of these additional robustness checks, confirming that the estimates of SBA are robust to accounting for treatment heterogeneity.

Multiple Hypotheses Testing

Estimates of SBA that falsely appear significant in multiple hypotheses testing are another potential issue with our income analyses. Thus, to address this concern regarding multiple inferences, we applied the false discovery rate (FDR) correction proposed by Benjamini and Yekutieli (2001) and reported FDR-adjusted p-values in Table E.9. in the Appendix. The results are robust to accounting for multiple hypotheses testing.

Randomization Inference Procedure

As an additional robustness check, we conduct the randomization inference procedure, as suggested by Bertrand et al. (2004). First, we randomly assign a year of the implementation of SBA to each county, while maintaining the distribution of SBA events over time. Next, we estimate the DID specification to derive the corresponding placebo treatment effect. We repeat this process 1000 times to generate a distribution of placebo treatment effects, against which we compare the treatment effect observed in the actual treatment assignment. This allowed us to obtain p-values and tests of statistical significance. In Appendix Figure E.1., we present the probability density function of placebo treatment effects for adult earnings. In particular, we show that the point estimates obtained in the main regressions are clearly significant compared with the distribution of placebo effects, thus confirming the validity of the DID approach.

TABLE E.1.

Robustness checks with sample adjustments (first generation)

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	[1935,1958]	[1935, 1960]	[1940, 1958]	[1940, 1960]	[1940, 1965]	[1946, 1965]
SBA	0.015**	0.015**	0.013**	0.014**	0.014**	0.011**
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
Baseline FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Observations	228,786	258,267	196,535	244,953	363,186	174,657
R-squared	0.508	0.499	0.505	0.505	0.494	0.481

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. SBA is a function only of a child's birth year b and *hukou* registration county c. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.2.

Robustness checks of SBA across different introduction times in a year (first generation)

	(1)	(2)	(3)
	The beginning of the year	The middle of the year	The end of the year
SBA	0.015**	0.013**	0.009*
	(0.006)	(0.005)	(0.006)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Observations	258,267	258,267	258,267
R-squared	0.507	0.524	0.481

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county-year level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.3.

Robustness checks with different specifications (2005 Census, first generation)

	log(Monthly income)		
	(1)	(2)	(3)
SBA	0.015**	0.013*	0.015**
	(0.006)	(0.007)	(0.007)
Observations	258,267	258,267	258,267
R-squared	0.508	0.507	0.508
Province-specific linear cohort	YES	NO	YES
Cluster at county-year level	NO	YES	YES

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level in Column 1 and county-year level in Columns 2 and 3. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.4.
The effect of SBA on birth cohort sizes and fertility

	(1)	(2)	(3)	(4)
	Birth cohort sizes	log(Birth cohort sizes)	Children born, altogether	log(Children born, altogether)
SBA	0.203**	0.021**	-0.007	-0.043
	(0.090)	(0.009)	(0.026)	(0.028)
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	41,871	41,871	228,786	228,786
R-squared	0.851	0.751	0.169	0.142

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in Columns 1-2 are the birth cohort sizes at the county-year level. The dependent variable in Columns 3-4 are the total number of children born to each female respondent. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.5.

Robustness checks addressing contemporaneous historical events (first generation)

	log(Monthly income)		
	(1)	(2)	(3)
SBA	0.014**	0.016**	0.014**
	(0.007)	(0.006)	(0.005)
Great Famine	YES	NO	YES
People's communalization	NO	YES	YES
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Observations	258,267	258,267	258,267
R-squared	0.564	0.453	0.490

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, and province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.6.

Robustness checks with additional winsorization of adult income (first generation)

	(1)	(2)	(3)	(4)	(5)
Variables	1% Tail	2% Tail	3% Tail	4% Tail	5% Tail
SBA	0.014**	0.014**	0.013**	0.013**	0.012**
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	258,267	258,267	258,267	258,267	258,267
R-squared	0.517	0.520	0.525	0.526	0.526

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.7.

Effects of SBA on adult income: Specifications with county-specific trends (first generation)

	log(Monthly income)		
	(1)	(2)	(3)
SBA	0.015**	0.010**	0.010**
	(0.006)	(0.004)	(0.004)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Linear trends	NO	YES	NO
Quadratic trends	NO	NO	YES
Observations	258,267	258,267	258,267
R-squared	0.508	0.512	0.524

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.8.
Heterogeneity in the treatment effect (first generation)

	(1) Baseline	(2) 1% Tail	(3) 3% Tail
Panel A. DiD estimator proposed by de Chaisemartin and D'Haultfoeuille (2021)			
SBA	0.160***	0.157***	0.145***
	(0.048)	(0.047)	(0.045)
Observations	89,112	89,112	89,112
Panel B. DiD estimator proposed by Callaway and Sant'Anna (2021)			
SBA	0.015***	0.014***	0.014***
	(0.006)	(0.006)	(0.006)
Observations	46,155	46,155	46,155

Notes: Panels A and B implement recent estimators that address problems with two-way fixed effects difference-in-differences regressions in setting with staggered delivery of the treatment. Specifically, Panel A follows De Chaisemartin and d'Haultfoeuille (2020), while Panel B follows Callaway and Sant'Anna (2021). Standard errors are clustered at the county level in parentheses. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.9.

Adjusting p-values for multiple hypotheses testing on adult income (first generation)

	(1)	(2)	(3)
	log(Monthly income)	log(Hourly wage)	log(Hours worked)
SBA	0.015**	0.009***	0.003
	(0.006)	(0.004)	(0.006)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Unadjusted p-values	0.011	0.001	0.171
FDR-adjusted p-values	0.013	0.001	0.165

Notes: The table shows original and FDR corrected p-values. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in Column 1 is the natural logarithm of monthly income. The dependent variable in Column 2 is the natural logarithm of hourly wage and the dependent variable in Column 3 is the natural logarithm of hours worked last week. All models include birth year, county, and province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE E.10.

Placebo Exposure Tests: Assume earlier introduction of SBA (first generation)

	(1)	(2)	(3)	(4)	(5)
	1 years earlier	2 years earlier	3 years earlier	4 years earlier	5 years earlier
SBA	-0.011	-0.007	-0.020	-0.014	-0.008
	(0.008)	(0.008)	(0.018)	(0.008)	(0.008)
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	258,267	258,267	258,267	258,267	258,267
R-squared	0.464	0.427	0.472	0.465	0.431

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

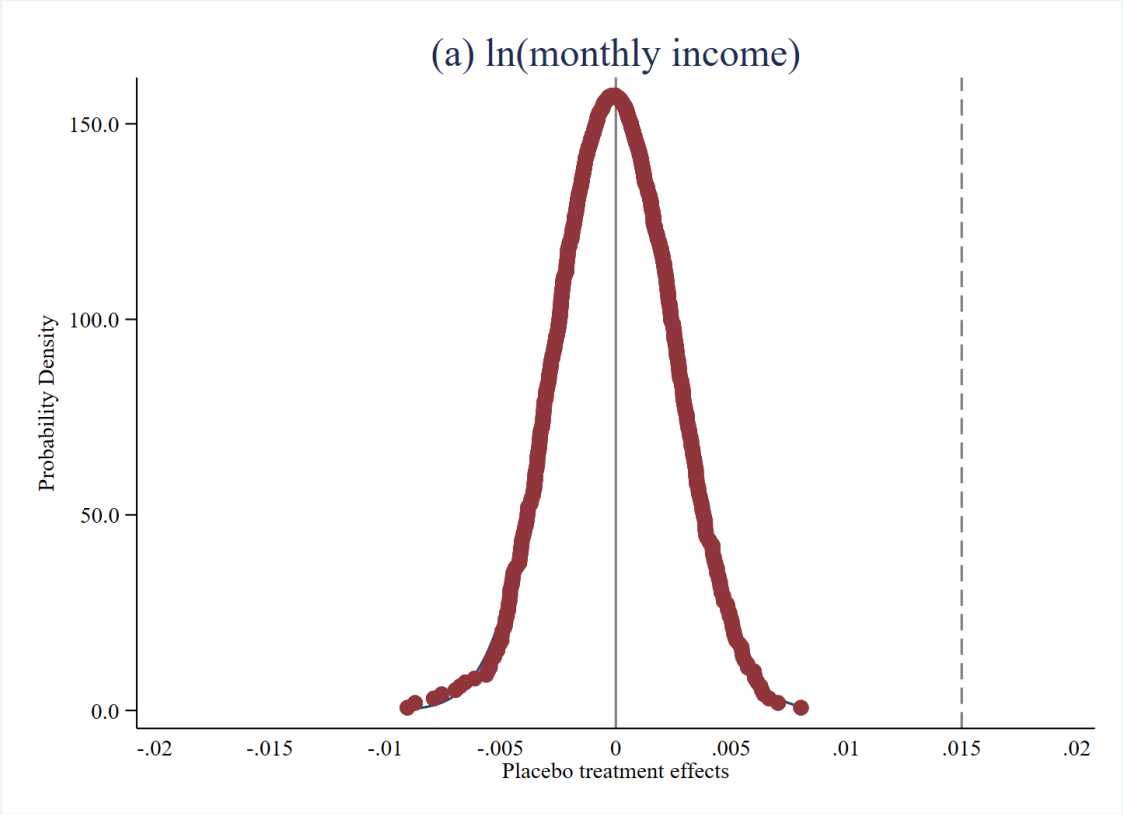


FIGURE E.1.
Random inference distribution (first generation)

Appendix F: Robustness analyses (second generation)

Contemporaneous Historical Events

Another concern would be about other policies that closely follow the county-level rollout of SBA and may have influenced the later-life earnings of children whose parents were exposed to SBA. From our systematic review of contemporaneous historical events, no candidate policies adhered to these precisely prescribed patterns. Nevertheless, to further alleviate concerns over concurrent policies, we reported the results after controlling for the possible influences of the compulsory education system and the One-Child Policy (OCP), which could raise the overall lifetime earnings of children in China.

The Compulsory Education System

China's Compulsory Education Law (CEL) was passed and implemented in 1986. Under the CEL, all Chinese children are mandated to receive nine years of free education, generally starting at six years of age. Prior studies have shown that education plays an important role in determining labor market performance, with better-educated individuals generally receiving higher earnings. Following this line of logic, the implementation of the compulsory education system could have raised overall educational attainment and lifetime earnings in China, thus upwardly biasing our estimates. As the timing of the CEL adoption does not vary within provinces (Du et al., 2021; Fang et al., 2012; Ma, 2019), the effect of compulsory education is absorbed by province-by-birth-year fixed effects in our baseline specification. To further control for the influence of the CEL, we substituted province-by-birth-year fixed effects with province-specific time trends. Then, we follow Du et al. (2021), who studied the impact of education on gender role attitudes, to exploit exogenous temporal and geographical variation in the enforcement of the CEL. As the central government recognized that not all provinces would have sufficient resources to enforce the law immediately, the provinces were initially allowed to have different effective dates to implement the law. Most provinces implemented the law in 1986 and 1987, while some provinces, such as Gansu, Guangxi, Hainan, and Tibet, only implemented the law in the early 1990s (Du et al., 2021). For the current study, we collected province-level information on the timing of the CEL. Panel A of Table F.3. presents the results after considering the impact of the CEL. As can be seen, the estimated coefficient of the CEL is positive and statistically significant, indicating that the CEL helped raise adult incomes. Meanwhile, the estimated coefficients on SBA are somewhat larger than those in Table 7 and remain statistically significant, thus confirming the robustness of our estimates.

The One-Child Policy (OCP)

The Chinese government imposed the OCP in 1979 to curb the growth of the population, which, at that time, was reaching 972 million people.[36] This nationwide fertility policy was firmly enforced

[36]The policy most strictly applied to Han Chinese but not to ethnic minorities around China, although there were

for around 32 years until 2011. With tight fertility control, parents were likely to devote more time and financial resources to childcare and educational investment for their only child, consequently raising their later-life earnings. For the current study, our empirical strategy to control for the influence of the OCP follows that of Ebenstein (2010), who studied the impact of the OCP on higher ratios of males to females.[37] Specifically, we approximate the variation in enforcement of the policy with the average policy fine for excess fertility.[38] Panel B of Table F.3. reports the results after controlling for the effects of the OCP. The estimated coefficient of SBA is almost identical to the main specification in Table 7. The last panel of Table F.3. shows the results of simultaneously controlling for all the confounding factors considered above, and as can be seen, the effect of SBA remains highly robust.

exceptions for rural farmers and certain situations. Rural parents are allowed to have a second child if the first is a daughter. Likewise, families with a handicapped child are entitled to another birth.

[37] We collected province-year-level information on the average monetary penalty (i.e., OCP policy fine) rate for unauthorized births from 1979 to 2000 to formally differentiate the effects of SBA and the OCP. The policy fine is formulated in multiples of annual income (Ebenstein, 2010; Wei and Zhang, 2011).

[38] Given that there is no within-province variation in fines levied by unauthorized births, we substituted province-by-birth-year fixed effects with province-specific time trends for this robustness check.

TABLE F.1.

Robustness checks with sample adjustments (second generation)

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	[1970,1987]	[1970, 1985]	[1970, 1983]	[1965, 1983]	[1965, 1985]	[1960, 1985]
SBA	0.020**	0.016**	0.016**	0.015**	0.015**	0.015**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Baseline FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Observations	97,423	82,721	68,064	73,311	87,967	99,105
R-squared	0.483	0.484	0.490	0.489	0.484	0.483

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year *b* and county *c*. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE F.2.

Robustness checks with different specifications (2005 Census, second generation)

	log(Monthly income)		
	(1)	(2)	(3)
SBA	0.026***	0.027***	0.026***
	(0.006)	(0.006)	(0.006)
Observations	97,423	97,423	97,423
R-squared	0.483	0.482	0.483
Province-specific linear cohort	YES	NO	YES
Cluster at county level	NO	YES	YES

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in each column is the natural logarithm of monthly income. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level in Column 1 and county-year level in Columns 2 and 3. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE F.3.

Robustness checks addressing contemporaneous historical events (second generation)

	log(Monthly income)		
	(1)	(2)	(3)
SBA	0.027***	0.028***	0.027***
	(0.006)	(0.006)	(0.006)
One Child Policy	-0.016		-0.012
	(0.010)		(0.010)
Compulsory Education		0.141***	0.136***
		(0.035)	(0.035)
Baseline FE	YES	YES	YES
Controls	YES	YES	YES
Observations	97,423	97,423	97,423
R-squared	0.482	0.481	0.482

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year *b* and county *c*. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, and province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE F.4.

Robustness checks with additional winsorization of adult income (second generation)

	(1)	(2)	(3)	(4)	(5)
Variables	1% Tail	2% Tail	3% Tail	4% Tail	5% Tail
SBA	0.026***	0.027***	0.027***	0.026***	0.026***
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Baseline FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Observations	97,423	97,423	97,423	97,423	97,423
R-squared	0.490	0.491	0.496	0.495	0.495

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year *b* and county *c*. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variable in each column is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE F.5.
Heterogeneity in the treatment effect (second generation)

	(1) Baseline	(2) 1% Tail	(3) 3% Tail
Panel A. DiD estimator proposed by de Chaisemartin and D'Haultfoeuille (2021)			
SBA	0.158**	0.152**	0.145**
	(0.060)	(0.058)	(0.061)
Observations	1,331	1,331	1,331
Panel B. DiD estimator proposed by Callaway and Sant'Anna (2021)			
SBA	0.155**	0.142**	0.137**
	(0.061)	(0.061)	(0.061)
Observations	1,331	1,331	1,331

Notes: Panels A and B implement recent estimators that address problems with two-way fixed effects difference-in-differences regressions in setting with staggered delivery of the treatment. Specifically, Panel A follows De Chaisemartin and d'Haultfoeuille (2020), while Panel B follows Callaway and Sant'Anna (2021). Standard errors are clustered at the county level in parentheses. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

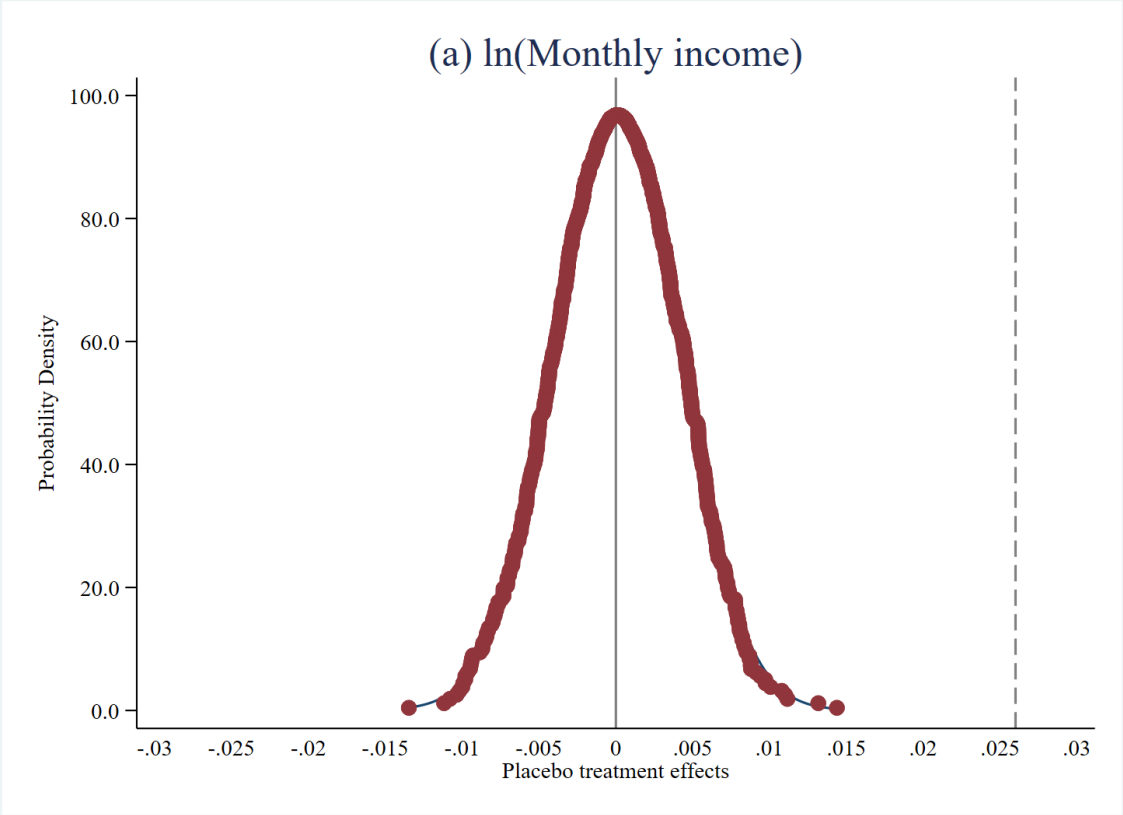


FIGURE F.1.
Random inference distribution (second generation)

Appendix G: Mechanisms (continued)

TABLE G.1.
The effect of SBA on cognitive outcomes (first generation)

Variables	Verbal test score		Math test score	
	Score	z-score	Score	z-score
	(1)	(2)	(3)	(4)
SBA	1.471	0.318	1.618**	0.261**
	(1.141)	(0.136)	(0.825)	(0.148)
$\bar{Y}$ of control group	20.873	0.121	11.219	-0.081
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	2,142	2,142	2,142	2,142
R-squared	0.647	0.571	0.793	0.740

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the CFPS-2010 with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variables in Columns 1 and 3 are math and verbal test scores, respectively. The dependent variables in Columns 2 and 4 is a summary index calculated as the average of standardized z-scores for math and verbal test scores. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE G.2.

The effect of SBA on cognitive outcomes (second generation)

Variables	Verbal test score		Math test score	
	Score	z-score	Score	z-score
	(1)	(2)	(3)	(4)
SBA	1.017	0.128	1.081**	0.126**
	(0.941)	(0.090)	(0.412)	(0.047)
$\bar{Y}$ of control group	21.256	0.211	13.129	-0.018
Baseline FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Observations	2,219	2,219	2,219	2,219
R-squared	0.574	0.517	0.639	0.714

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the CFPS-2010 with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year *b* and county *c*. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). The dependent variables in Columns 1 and 3 are math and verbal test scores, respectively. The dependent variables in Columns 2 and 4 is a summary index calculated as the average of standardized z-scores for math and verbal test scores. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix H: Mediation analyses

TABLE H.1.
Mediation analysis: The effect of SBA on long-term income (first generation)

	log(Monthly income)	
	(1)	(2)
SBA	0.015**	0.014**
	(0.006)	(0.006)
Years education		0.040***
		(0.001)
Baseline FE	YES	YES
Controls	YES	YES
Observations	258,267	258,267
R-squared	0.508	0.525

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. The dependent variable is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. SBA is a function only of a child's birth year b and *hukou* registration county c. All models include birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE H.2.

Mediation analysis: The effect of SBA on long-term income (second generation)

	log(Monthly income)	
	(1)	(2)
SBA	0.026***	0.014**
	(0.006)	(0.006)
Years education		0.046***
		(0.001)
Baseline FE	YES	YES
Controls	YES	YES
Observations	97,423	97,423
R-squared	0.508	0.525

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1976 and 1986. The dependent variable is the natural logarithm of monthly income for individual i in *hukou* registration county c and born in year b. SBA is a dummy variable indicating the parents' treatment status, which depends only on each parent's birth year b and county c. Children with at least one parent born with the assistance of a skilled healthcare professional are classified as exposed (assigned a value of 1), while those with neither parent attended by a skilled birth professional are classified as non-exposed (assigned a value of 0). All models include birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix I: Multiple hypotheses testing

TABLE I.1.

Adjusting p-values for multiple hypothesis testing of mechanism analysis (first generation)

	(1)	(2)	(3)	(4)	(5)
Panel A. Physical health					
	Self-reported health status				
	5-point scale	Dummy	Doctor visit	Physical health index	
SBA	0.346**	0.109*	-0.131***	0.479***	
	(0.149)	(0.050)	(0.039)	(0.113)	
Unadjusted p-values	0.016	0.026	0.001	0.000	
FDR-adjusted p-values	0.021	0.026	0.001	0.000	
Panel B. Mental health					
	Worthless	Distress	Restless	Hopeless	Mental health index
SBA	-0.137**	-0.159***	-0.086**	-0.054***	-0.767***
	(0.051)	(0.049)	(0.017)	(0.013)	(0.154)
Unadjusted p-values	0.002	0.010	0.019	0.003	0.000
FDR-adjusted p-values	0.005	0.013	0.019	0.005	0.000

Notes: The table shows original and FDR corrected p-values. Data in Panel A and B are a linked sample of the CHNS 1989-2011 with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year *b* and *hukou* registration county *c*. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

TABLE I.2.

Adjusting p-values for multiple hypothesis testing of mechanism analysis (first generation)

	(1)	(2)	(3)	(4)	(5)
Panel A. Educational attainment					
	Years education	Primary	Junior high	Senior high	College
SBA	0.07**	0.024**	0.022**	0.020*	0.025***
	(0.028)	(0.005)	(0.010)	(0.011)	(0.010)
Unadjusted p-values	0.029	0.602	0.728	0.066	0.014
FDR-adjusted p-values	0.072	0.728	0.728	0.110	0.072
Panel B. Cognition					
	Verbal test score			Math test score	
	Score	z-score		Score	z-score
SBA	1.471	0.318		1.618**	0.261**
	(1.141)	(0.136)		(0.825)	(0.148)
Unadjusted p-values	0.270	0.147		0.037	0.045
FDR-adjusted p-values	0.270	0.196		0.091	0.091

Notes: The table shows original and FDR corrected p-values. Data in Panel A are a linked sample of the 2005 Census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. Data in Panel B are a linked sample of the CFPS-2010 with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Appendix J: Pension and Medical Insurance

TABLE J.1.
The effect of SBA on access to pension and medical insurance (first generation)

Variables	With Pension	With Medical Insurance
	(1)	(2)
SBA	0.007***	0.004*
	(0.002)	(0.003)
Baseline FE	YES	YES
Controls	YES	YES
Observations	398,914	363,888
R-squared	0.548	0.529

Notes: This table shows the regression results for Equation 1. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 and 1965. SBA is a function only of a child's birth year b and *hukou* registration county c. The dependent variable in Column 1 is a dummy variable indicating whether an individual has access to pension for retirement fund. The dependent variable in Column 2 is a dummy variable indicating whether an individual has access to medical insurance. The main specification in each column includes birth year, county, province-by-birth-year fixed effects, as well as individual characteristics including gender, age, ethnicity, and rural status. Standard errors are clustered at the county level. *** Significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

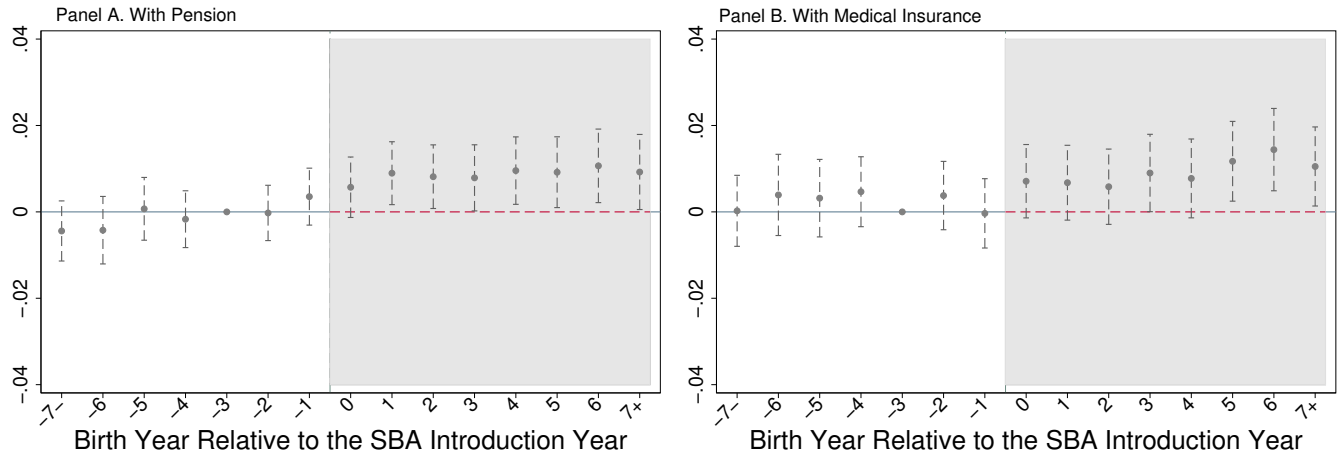


FIGURE J.1.
Effect of SBA on access to pension and medical insurance in the first generation

Notes: Figure J.1. plots the coefficients and the 95% confidence intervals for the main specification, where the key independent variables are a set of categorical measures of different birth years relative to the introduction year of SBA in the county. Data are a linked sample of the 2005 census with the county-by-county rollout of SBA and include adults who were born between 1935 to 1965. On the x-axis, the event time "-7-" represents children born at least seven years before SBA was introduced in the county. Other event times, such as "1" and "2," indicate children born one or two years after SBA was implemented in the county. The model estimated uses the same set of preferred county, birth year, province-by-birth-year fixed effects and individual characteristics as in Table 3. Standard errors are clustered at the county level.