The One-Child Policy in China and its Intergenerational Effects on Health*

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Abstract

This paper studies the intergenerational spillover effects of the One-Child Policy on the health of the subsequent generation. Using a reduced-form regression discontinuity design centered on the policy's implementation, we isolate the local average treatment effect among urban Han Chinese families. We find that children born to policy-affected mothers experience significant improvements in both physical and mental health. Our analysis identifies three primary transmission channels: improved maternal health and socioeconomic status, increased household investment, and better parenting practices. These results demonstrate that family planning policies can have profound, positive health impacts across generations.

Keywords: One-Child Policy, Health, Spillover Effects, Family Planning
JEL-Codes: I10, I15, J13, J18

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1 Introduction

The One-Child Policy is a family planning policy implemented nationwide in China from late 1979 to 2016 to control the country's rapid population growth. The policy strictly limited the number of children in each family to a minimum, except for those from ethnic minorities or living in rural areas. More than three decades of implementing the policy significantly changed the social dynamics and family structures in China (Settles et al., 2012; Zhang, 2017). Low fertility rates and reduced family size have led to population aging and increasing pressures on elderly care (Bai and Lei, 2020; Nie and Zhao, 2023). However, findings indicate that reducing sibling size would prompt increased investment in children, subsequently leading to improved education and health outcomes (e.g. Cáceres-Delpiano, 2006; Lee, 2008; Rosenzweig and Zhang, 2009; Zhong, 2017). If the parental generation receives significant investment and wealth as a result of family planning policies, we expect them to allocate more resources to their offspring. Furthermore, improved health outcomes of parents can be passed on to the next generation, resulting in better health observed among their children (Emanuel et al., 1992; Eriksson et al., 2005; Strauss and Thomas, 2007).

While existing literature primarily examines the impact of sibling and family size, the direct intergenerational consequences of policy enforcement on health remain underexplored. Our paper addresses this gap by investigating health outcomes for children whose parents were subject to the One-Child Policy. By focusing on the urban Han Chinese population, where enforcement was most stringent, we provide a clearer understanding of how family planning policies reverberate across generations.

We leverage data from the China Family Panel Studies (CFPS), a nationally representative biennial longitudinal survey conducted by the Institute of Social Science Survey (ISSS) at Peking University since 2010. The CFPS covers both economic and non-economic aspects of the Chinese population, providing rich data on economic activities, education, family dynamics, and health (Xie and Hu, 2014). We follow Anderson (2008) to construct a weighted summary *health index* comprising the absence of illness, no hospitalization, self-rated health, and interviewer-rated health as our main physical health outcome. We also develop a separate mental health indicator – *distress* – based on the Kessler Psychological Distress Scale (K6) and the Center for Epidemiologic Studies Depression Scale (CES-D).[1] We employ a reduced form regression discontinuity design (RDD), exploiting the policy cut-off in 1980Q1 that creates a discontinuity in the number of single-kid families,[2] thereby precisely isolating the policy's local average treatment effect (LATE) on the next generation's health outcomes. We show that our design passes multiple checks around the core RDD assumptions. Given the significantly smaller sample

[1] Detailed description of the data and our selection of outcome variables are presented in Section 3.
[2] We discuss the rationale for selecting the 1980Q1 policy cut-off in Section 2.

size for father data and numerous evidence showing that urban Han mothers benefited from the demographic pattern created by the One-Child Policy (Fong, 2002; Veeck et al., 2003; Zhang, 2019), we focus primarily on the effects of the policy from the mothers' side.

We begin by examining the policy's effects on the first generation – mothers (Section 5.1). We find that the policy significantly changes mothers' sibling composition, improves their health, and enhances their socioeconomic status in adulthood, consistent with long-term gains in human capital and economic outcomes among affected mothers (Hu and Yang, 2025; Zhang, 2019). In particular, policy-affected mothers are 7.9 percentage points more likely to be single kids, and less likely to have been born into a pre-existing sibling group. Their *health index* significantly increases by 18% of a standard deviation, although we do not find any effects on mental health. In addition, they have lower fertility, higher college participation, higher income, and a greater likelihood of holding leadership positions. These benefits may carry over to the next generation, improving child health through better maternal health, greater economic resources, and increased parental investments in nutrition, healthcare, and the home environment.

We then empirically investigate the intergenerational health effects of the policy on the second generation – children (Section 5.2). Our baseline results show that children born to policy-affected mothers exhibit significant improvements in both physical and mental health. Specifically, their *health index* improves by about 15% of a standard deviation. They also experience lower levels of mental distress, with the probability of having distress reduced by 9.3 percentage points.[3] We conduct a wide range of sensitivity checks to show that our baseline estimates remain robust. Our results, thus, provide evidence for the intergenerational transmission of health and household characteristics across generations. A lower fertility rate, which is transmitted from grandparents to parents (Kolk, 2014; Murphy and Knudsen, 2002), leads to increased human capital investment per child, supporting our narratives on child health investment. Additionally, mothers affected by the policy exhibit better health outcomes, which can be passed on to their children (Emanuel et al., 1992; Eriksson et al., 2005).

Finally, in Section 5.3, we empirically investigate other potential channels for the intergenerational health effects of the policy. First, greater investment in children's health, as family size becomes smaller, can explain better health in children. This is consistent with the quantity-quality trade-off formulated by Becker (1960), which illustrates a negative correlation between family size and the resources allocated to each child. Second, parenting practices and parent-child interactions show that policy-affected parents tend to be "authoritative", characterized by high responsiveness combined with appropriate

[3]We find similar but less statistically efficient results when examining children whose fathers were born after 1980 and affected by the policy. Given the smaller sample size and narrower bandwidth in the fathers' dataset, we interpret these results with caution.

supervision (Baumrind et al., 2010; Doepke and Zilibotti, 2017). The balance of warmth, support, and supervision from parents fosters a home environment that promotes children's development and improves their mental health. This aligns with literature on parental demands (Lo et al., 2020; Soysa and Weiss, 2014; Wong et al., 2019) and parental responsiveness (Davidov and Grusec, 2006; Miller-Slough et al., 2018), especially in the Chinese context, where children are the only child (Liu et al., 2010; Lu and Chang, 2013).

Related Literature. Our research question is relevant to several strands of literature. First, our paper relates to the theoretical basis for the quantity-quality trade-off in children. This framework was first theorized by Becker (1960), who treated children as a consumption good, requiring a family to decide not only on the number of children but also on the corresponding expenditure allocated to them. This trade-off occurs because parents have to spread their time and resources more thinly as the number of children increases (Hanushek, 1992). The literature presents mixed empirical evidence on the quantity-quality trade-off. Numerous studies have found a negative association between family size and investment in children (Chen, 2020; Li et al., 2008; Ponzo and Scoppa, 2022; Rosenzweig and Zhang, 2009). However, several others have observed no evidence of the quantity-quality trade-off (Angrist et al., 2010; Diaz and Fiel, 2021) or even a positive relationship (Gomes, 1984; Lao and Lin, 2022; Qian, 2009).

In terms of health, several studies provide evidence of a negative association between family size and health. Liu (2014) and Zhong (2017) investigate the One-Child Policy in China and find negative impacts of family size on child height. A similar study conducted by Liang and Gibson (2018) also shows that each additional sibling reduces a child's nutrient intake by about 10% to 20% of the recommended level. Nevertheless, Lordan and Frijters (2013) utilizing data from the Young Lives Project (YLP) in Peru find that family size is negatively associated with child height for unplanned births but positively associated for planned births. Datar (2017) investigates the relationship between family size and obesity in the US and provides evidence that children with siblings have lower BMI and are less likely to be obese, thanks to healthier diets. Regarding subjective well-being, several studies find that being an only child negatively impacts self-reported psychological health (Wu, 2014; Zeng et al., 2020). Cameron et al. (2013), leveraging the One-Child Policy in China, provide evidence that these "little emperors" are less trustworthy and more pessimistic. However, Liu et al. (2010) and Rao et al. (2024) compare single-kid and multiple-kid families and show that single kids reported lower psychological distress and fewer mental health problems, attributed to higher parental responsiveness.

Our paper, however, extends beyond the conventional quantity-quality trade-off, aiming to explore this trade-off at the national scale. The policy is estimated to account for

at least 30% of the increase in aggregate savings, as fertility restrictions diminished anticipated old-age support, motivating parents to save more and allocate greater resources toward their children's education (Choukhmane et al., 2023). The One-Child Policy's first generation, particularly in urban areas, benefited from concentrated investments in education and health (Zhang, 2019), resulting in higher educational attainment and improved health, especially for women (Fong, 2002; Huang et al., 2016a; Rao et al., 2024). Additionally, the policy led to shifts in social perceptions on traditional patriarchy (Shi, 2017), promoted female empowerment (Huang et al., 2021) and strengthened parent-child relationships (Short et al., 2001). All of these factors can affect the second generation who are not directly affected by the policy.

In addition, our paper contributes to the literature on the intergenerational transmission of health and household characteristics across generations. Studies consistently demonstrate modest yet persistent effects in the transmission of parents' fertility patterns to their children (Kolk, 2014). Another body of work focuses on intergenerational health transmission, grounded in the fetal origins hypothesis (Barker, 1990), which emphasizes the lasting influence of early life (in-utero) conditions on later health and economic outcomes (Emanuel et al., 1992; Eriksson et al., 2005). Numerous studies also measure intergenerational health association, in which children inherit parents' health capital, with an estimated persistence of about 0.2 in the United Kingdom (Bencsik et al., 2023), 0.3 in the United States (Halliday et al., 2020) and between 14 and 38% in Denmark (Andersen, 2021).

Our primary contribution, therefore, is to provide new causal evidence for the intergenerational effects of family size and family planning policies on subsequent generations. We leverage data from the China Family Panel Studies (CFPS), a nationally representative and one of the most comprehensive social panel surveys conducted in China (Xie and Hu, 2014), ensuring high reliability and coverage. We examine the spillover effects on both the physical and mental health of children whose parents were affected by the policy. We also explore several mechanisms from the CFPS data to explain our results, thereby contributing to the literature on quantity-quality trade-offs and intergenerational effects. In addition, we further address the question of parenting behavior and parent-child relationships in modern Chinese families.

The paper is structured as follows: Section 2 provides historical background and introduces the One-Child Policy in China; Section 3 describes the data and sample; Section 4 illustrates empirical strategy and identification assumptions; Section 5 presents the main findings of the paper, including the policy effects on the first-generation (mothers), intergenerational health effects on children, and the intra-household mechanisms that help explain our results; Section 6 concludes.

2 Background

Since beginning its industrialization in 1949, China has experienced substantial population growth, with the belief that this growth would contribute to the nation's development (Zhu, 2012). However, persistent poverty and high fertility rates have raised concerns about overpopulation (De Silva and Tenreyro, 2017). From 1970 onwards, citizens were encouraged to delay their marriages. In the early 1970s, the state introduced a series of birth planning policies. In 1978, the authorities began encouraging one-child families, and in early 1979 they announced their intention to implement this policy, which later became national policy. Introduced in 1979, the One-Child Policy was a strict family planning policy designed to curb the country's rapid population growth (Wang et al., 2016), and it was formally written into the constitution in 1982. The policy primarily affected the Han Chinese, who make up 92% of the population (Huang et al., 2016b). From late 1979 onwards, in principle, a couple was allowed only one child, except in some rural ethnic minority areas such as Xinjiang, Yunnan, Ningxia, and Qinghai.

The evolution of the One-Child Policy had several phases (Greenhalgh, 2008; Scharping, 2013). It was first announced in the second half of 1978 as "Best is one, at most two; eliminate third births". In December 1979, the National Population and Family Planning Commission announced the policy as "Best is one". From February 1980, this quickly changed to the "One for all" policy. Over time, however, various exceptions were made, and the policy was revised in early 1989 to "One child with exceptions for rural couples with only a daughter".

In 2016, China officially relaxed its One-Child Policy, marking a significant shift in its approach to population control. The policy change, which had allowed families to have two children since 2015 with certain modifications, reflected mounting concerns about the policy's adverse demographic and socio-economic impacts. The relaxation aimed to address issues such as an aging population, a shrinking workforce, and gender imbalances. Nevertheless, many urban families still choose to have only one child due to financial and social pressures, even after the policy relaxation (Qian and Jin, 2024). The long-term effects of this policy change on family dynamics, economic stability and population health remain subjects of ongoing policy debate.

Social consequences of the One-Child Policy (OCP). The One-Child Policy in China has led to a significant decline in the fertility rate, which was already decreasing due to earlier family planning campaigns in the 1970s (Feng et al., 2014). The average family size fell from 4.8 in the early 1970s to 3.1 by 2010 (Aird, 1983; Census Office of the State Council, 2020). Single-child families became prevalent, particularly in urban areas, where approximately 80 percent of families consisted of three members by the end of the 20th century (Tu, 2016). Other direct outcomes included a skewed male-to-female ratio

and higher fertility rates in rural areas than in urban areas, which disadvantaged rural families economically (Ebenstein, 2010). Despite criticism, the policy often benefited urban daughters, who received more family resources and achieved higher educational attainment and empowerment (Huang et al., 2016a, 2021). Long-term impacts include accelerated population aging, increased pressure on elderly care, and the rise of "empty nest" families in urban areas (Bai and Lei, 2020; Nie and Zhao, 2023; Zhu and Walker, 2021).

Policy effects on the first generation. Those subjected to the One-Child Policy as the first generation experienced notable benefits, particularly in urban areas. Families could focus their resources on their single child's education and health (Zhang, 2019). This focus led to higher educational attainment among females and improved overall health outcomes (Huang et al., 2016a; Rao et al., 2024). Women born under the One-Child Policy achieved higher levels of education, and stricter early-life fertility restrictions increased female empowerment, as evidenced by an increase in female-headed households (Huang et al., 2021).

The policy also brought about qualitative changes in family dynamics. These changes included simplified family structures, reduced patriarchy in families with only daughters, and greater individual choice regarding family living arrangements and childbearing (Shi, 2017). Additionally, increased parental involvement in childcare has improved parent-child interactions (Short et al., 2001). Consequently, this generation enjoys higher income levels and reduced overall stress. The persistence of intergenerational income in urban China highlights the policy's lasting economic impact (Yi, 2016).

Policy cut-off in this paper. We examine the introduction of the policy during the 1979-1980 period to identify the policy cut-off for our study. Before the policy was officially implemented in China, the government had imposed restrictions on the number of children a couple could have. From 1977 to 1978, urban and rural couples were required to limit their family size to two children (Hardee-Cleaveland and Banister, 1988). Several intentions for the universal One-Child Policy were introduced in early 1979, and the "Best is One" policy was officially announced in December 1979. The "One for all" policy was quickly adopted in February 1980, clearly stating that every couple could have only one child. Later, on September 25, 1980, the Chinese Communist Party's Central Committee issued a public letter urging all party members and the Communist Youth League to adhere to the One-Child Policy. This date is often mentioned as the policy's "official" start date (Scharping, 2013). The revised 1980 Marriage Law, ratified during the Third Session of the National People's Congress on September 10, 1980, also explicitly mandated birth control for all couples (Hardee-Cleaveland and Banister, 1988; Hare-Mustin, 1982; Santana Cooney et al., 1991). Strict fines for violating the One-Child

Policy were imposed nationwide in January 1980 (Santana Cooney et al., 1991). During this period, abortions were required in several provinces, even in the second and third trimesters of pregnancy. The number of induced abortions increased sharply in 1979 and rose even higher in subsequent years (Hardee-Cleaveland and Banister, 1988).

We expect to see a sharp increase in the proportion of single-child births in the first quarter of 1980, according to our data structure, which we will discuss later. Although official announcements and legislation related to the One-Child Policy and birth control were issued in September 1980, the policy was strictly enforced beginning in 1980 with rigorous measures such as mandatory abortions and birth control practices. Additionally, beginning in 1980, penalties were imposed on women who had a second child without official permission (Hardee-Cleaveland and Banister, 1988), confirming that it was difficult for Han mothers living in urban areas to have another child. We later verify this cut-off date by illustrating the discontinuity in the ratio of individuals with no siblings at our predicted cut-off point (the first quarter of 1980) within our dataset.

3 Data and Descriptive Statistics

3.1 Data

We use the China Family Panel Studies (CFPS) at the Institute of Social Science Survey (ISSS) at Peking University, China.[4] The CFPS is a nationally representative, biennial, longitudinal survey of Chinese families and individuals that began in 2010. For our analysis, the CFPS dataset has several key features. First, it allows us to identify the intra-household relationships, which are necessary for our empirical strategy that uses parents' birth information. Second, in the first wave (2010), adults were asked to provide information about their parents and their siblings, providing valuable data for studying sibling composition and policy effects. Third, with six waves up to 2020, the CFPS provides national-level information on family dynamics and health outcomes for our study.

Health index. To measure children's physical health status, we follow Anderson (2008) to generate a summary *health index* [5], which is a weighted mean of the following standardized components:

[4]The data are from the China Family Panel Studies (CFPS), funded by the 985 Program of Peking University and carried out by the Institute of Social Science Survey of Peking University.

[5]Summary index tests can reduce measurement error arising from idiosyncratic noise in individual outcome measures by aggregating information across several related outcomes into a single composite index. We first recode each variable so that a positive value always indicates a better health outcome. We then standardise each variable and calculate the weighted average. The weight is generated by the inverse of the covariance matrix of the standardised outcomes to maximize the amount of information captured by the index (Anderson, 2008).

1. Whether a child was ever sick last month: CFPS defines sickness as a condition in which a child experiences physical discomfort and requires treatment (medicines or other treatments). We recode this variable (*"No Sick"*) so that higher values indicate a better health status, meaning the child did not experience illness last month.
2. Whether a child was ever hospitalized last month: CFPS defines hospitalization as a child staying overnight in a hospital for at least one night due to disease or accidental injury. This variable is also recoded so that a value of 1 denotes better health ("*No Hospital*"). This ensures directional consistency across component outcomes of our *health index*.
3. Children's self-rated health: The survey asked respondents to rate their health status on a scale from 1 to 5, where 1 indicates healthy and 5 indicates very unhealthy. Only those aged 10 and above answered this question. We recode this into a binary variable with 1 for healthy and 0 for all other ratings.
4. Interviewer-observed child health: The interviewer recorded their assessment of the presented respondent's health, choosing from 1 (worst) to 7 (best). We also recode this variable as a binary variable, with 1 for an observed health rating of 4 or higher and 0 otherwise.

Similarly, we construct a parental *health index* based on the following indicators: no discomfort, no chronic disease, no hospitalization, self-rated health, and observed health.[6]

Distress. We incorporate two psychological scales – the Kessler Psychological Distress Scale (K6) (Kessler et al., 2002) and the Center for Epidemiologic Studies Depression Scale (CES-D) (Radloff, 1977) – to measure children's and parents' mental health, as different scales are available in different waves. A detailed description of these scales can be found in Appendix A. We construct a binary variable called "distress", which is coded as 1 if a child shows signs of mental distress, i.e. if the K6 score is greater than or equal to 5 (Prochaska et al., 2012), the CES-D8 score is greater than or equal to 7, or the CES-D20 score is greater than or equal to 16 (Bi et al., 2023). In Appendix A, we also analyze comparable items between the K6 and CES-D scales.

Mechanisms. We examine the mechanisms through which the effects of the policy on the first generation (parents) could be passed on to the second generation (their children). First, we examine the policy effects on parents' health, number of siblings, fertility choices and socioeconomic status.[7] Second, we examine family-level mechanisms, including

[6]Information on *sickness* is not available in the parents' dataset. However, data on *discomfort* and *chronic disease* are available.

[7]See Section 5.1.

income and expenditure, with a focus on expenditure on children. These measures reflect the resources allocated to support children's well-being and shed light on the broader context in which children are raised and develop. Lastly, we examine the interactions between parents and children, which can affect children's mental health. The quality and nature of these interactions are crucial in determining children's emotional and psychological well-being. Through studying these mechanisms, we aim to understand how policy effects are transmitted across generations.[8]

3.2 Sample selection and summary statistics

We compile data from six waves of the China Family Panel Studies (CFPS) based on information from child questionnaires. Then, we create a repeated cross-sectional dataset and merge it with data on parents and families from the corresponding waves. We first focus on a sample of parents born before or after five years from the policy cut-off in 1980. We then drop observations missing key demographic characteristics, such as children's age, gender, and rural or urban residence; the birth information of both parents; and family size (about 1% are dropped at this stage). Next, we split the dataset into two: one containing information about mothers and the other containing information about fathers. For each dataset, we exclude observations from provinces with fewer than 50 mothers or fathers (five provinces for both). Next, due to the policy focus, we restrict the sample to Han ethnicity (87.11% of mothers and 88.01% of fathers) and to urban parents (51.56% of mothers and 53.79% of fathers after restricting to Han ethnicity).

These steps ensure that we have complete information on the birth years and months of urban Han parents. Due to the limited number of observations resulting from the specific focus on urban Han parents, we base our analysis on quarterly birth data for these parents to ensure sufficient statistical power. Then, we exclude those with missing information on children's health outcomes at each wave. Table 1 presents summary statistics for our sample, focusing on children and mothers. Most of the statistically significant differences between the control and treatment groups are related to age. This is due to the nature of the policy, which makes those born later, i.e. younger, more likely to be treated.

We further demonstrate the validity of the policy cut-off date in our data. Figure 1a shows the proportion of Han urban adults born within four years (16 quarters) of the 1980Q1 policy cut-off who have no siblings. As mentioned previously, adult sibling information is only available in the first wave, which is from 2010. We use the 2010 adult data with the sampling weights provided by the CFPS to ensure national representativeness. The graph shows the simple overall national average of the no-sibling ratio in each quarter. It presents an upward general trend in the proportion of adults without siblings immedi-

[8] See Section 5.3.

Table 1. Descriptive statistics

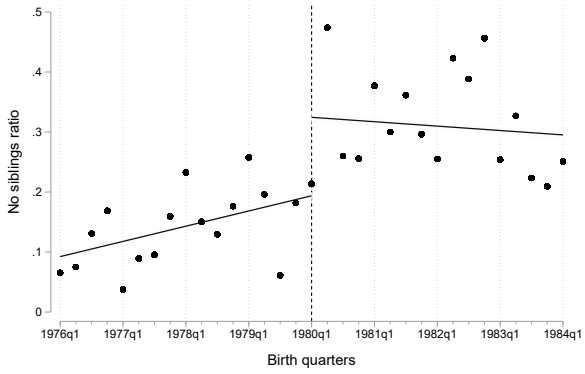
	All observations				Within 13-quarter bandwidth			
	All	Control	Treated	Diff	All	Control	Treated	Diff
A. Health Outcomes								
Health Index	0.02	0.05	-0.01	0.06***	0.01	0.01	0.00	0.01
	(0.40)	(0.47)	(0.31)	(10.05)	(0.39)	(0.42)	(0.36)	(1.07)
No sickness last month	0.72	0.77	0.68	0.09***	0.73	0.74	0.73	0.01
	(0.45)	(0.42)	(0.47)	(12.54)	(0.44)	(0.44)	(0.45)	(1.03)
Not hospitalized last month	0.78	0.83	0.74	0.09***	0.79	0.80	0.79	0.01
	(0.41)	(0.38)	(0.44)	(13.53)	(0.40)	(0.40)	(0.41)	(1.12)
Self-rated health (healthy = 1)	0.44	0.45	0.40	0.05**	0.39	0.39	0.38	0.01
	(0.50)	(0.50)	(0.49)	(3.26)	(0.49)	(0.49)	(0.49)	(0.31)
Interviewer-observed health (≥ 4 on 1-7 scale)	0.98	0.98	0.98	0.00	0.98	0.98	0.97	0.01
	(0.14)	(0.14)	(0.15)	(1.02)	(0.15)	(0.13)	(0.16)	(1.37)
Distress indicator based on K6 and CESD	0.18	0.19	0.16	0.03*	0.17	0.19	0.14	0.04**
	(0.39)	(0.39)	(0.37)	(2.41)	(0.38)	(0.39)	(0.35)	(2.67)
B. Demographics								
Child's age	7.90	10.22	5.88	4.34***	8.21	9.23	7.29	1.93***
	(4.53)	(4.05)	(3.93)	(68.83)	(4.23)	(4.16)	(4.08)	(17.42)
Child's gender (female = 1)	0.47	0.46	0.47	-0.01	0.48	0.47	0.49	-0.03*
	(0.50)	(0.50)	(0.50)	(-1.29)	(0.50)	(0.50)	(0.50)	(-1.98)
Child's birthyear	2007	2003	2010	-6.44***	2006	2005	2008	-2.67***
	(5.32)	(4.40)	(4.09)	(-95.57)	(4.03)	(3.90)	(3.72)	(-25.99)
Mother's age	34.89	39.48	30.75	8.73***	34.87	36.35	33.51	2.84***
	(6.23)	(4.59)	(4.33)	(121.98)	(3.62)	(3.33)	(3.33)	(31.30)
Mother's birthyear	1980	1974	1985	-10.83***	1980	1978	1981	-3.52***
	(6.70)	(4.01)	(3.92)	(-172.76)	(2.03)	(1.01)	(1.03)	(-128.21)
Father's age	36.83	41.01	33.18	7.83***	36.90	38.19	35.71	2.47***
	(6.38)	(5.15)	(4.94)	(91.87)	(4.51)	(4.38)	(4.30)	(20.03)
Father's birthyear	1978	1973	1983	-9.88***	1978	1976	1979	-3.24***
	(6.80)	(4.74)	(4.64)	(-127.61)	(3.57)	(3.30)	(3.07)	(-36.29)
Family size	5.08	4.62	5.48	-0.86***	5.06	4.94	5.17	-0.23***
	(1.91)	(1.62)	(2.04)	(-29.71)	(1.92)	(1.91)	(1.93)	(-4.48)
C. Mother's characteristics								
Health Index	0.02	0.03	0.02	0.01	0.04	0.03	0.05	-0.02
	(0.42)	(0.46)	(0.39)	(1.25)	(0.39)	(0.41)	(0.37)	(-1.64)
Distress indicator based on K6 and CESD	0.18	0.19	0.16	0.03*	0.17	0.19	0.14	0.04**
	(0.39)	(0.39)	(0.37)	(2.41)	(0.38)	(0.39)	(0.35)	(2.67)
No siblings	0.08	0.05	0.12	-0.07***	0.10	0.07	0.13	-0.06***
	(0.26)	(0.21)	(0.32)	(-12.30)	(0.30)	(0.26)	(0.34)	(-6.33)
Number of children	1.75	1.79	1.72	0.07***	1.76	1.76	1.77	-0.01
	(0.79)	(0.82)	(0.75)	(5.91)	(0.87)	(0.90)	(0.85)	(-0.48)
College+	0.17	0.14	0.20	-0.07***	0.21	0.18	0.24	-0.06***
	(0.38)	(0.34)	(0.40)	(-11.26)	(0.41)	(0.38)	(0.43)	(-5.48)
Ln(Yearly Income)	5.44	5.17	5.72	-0.54***	5.46	5.23	5.69	-0.46**
	(4.86)	(4.78)	(4.94)	(-6.41)	(4.93)	(4.90)	(4.95)	(-3.14)
Social Position (Leading Cadres)	0.01	0.01	0.01	0.01***	0.01	0.01	0.01	0.00
	(0.09)	(0.11)	(0.07)	(4.08)	(0.11)	(0.11)	(0.10)	(0.21)
D. Grandparents' characteristics								
Grandfather's age	61.42	66.10	54.33	11.77***	59.30	61.59	56.88	4.71***
	(9.35)	(8.05)	(6.19)	(83.01)	(6.17)	(6.15)	(5.19)	(26.37)
Grandmother's age	62.71	66.71	56.53	10.18***	60.93	62.64	59.14	3.50***
	(8.71)	(7.71)	(6.19)	(73.52)	(6.04)	(6.13)	(5.40)	(19.18)
Literacy (grandfather)	0.75	0.70	0.82	-0.12***	0.80	0.76	0.84	-0.08***
	(0.43)	(0.46)	(0.38)	(-14.82)	(0.40)	(0.43)	(0.36)	(-6.67)
Literacy (grandmother)	0.55	0.48	0.65	-0.17***	0.60	0.54	0.66	-0.13***
	(0.50)	(0.50)	(0.48)	(-17.67)	(0.49)	(0.50)	(0.47)	(-8.28)
Unemployment (grandfather)	0.14	0.15	0.13	0.03***	0.12	0.13	0.11	0.02*
	(0.35)	(0.36)	(0.33)	(3.87)	(0.32)	(0.34)	(0.31)	(2.08)
Unemployment (grandmother)	0.24	0.25	0.22	0.03***	0.22	0.23	0.20	0.03*
	(0.43)	(0.44)	(0.42)	(3.52)	(0.41)	(0.42)	(0.40)	(2.27)
Either of grandparents is communist	0.11	0.15	0.07	0.08***	0.14	0.14	0.14	0.00
	(0.31)	(0.36)	(0.26)	(16.23)	(0.35)	(0.35)	(0.35)	(0.15)
Observations	16105	7514	8591	16105	5533	2635	2898	5533

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses. "All" means the whole sample, "Treated" means mothers have no siblings, and "Control" means mothers have siblings. "Diff" shows the mean difference between treated and control groups. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best).

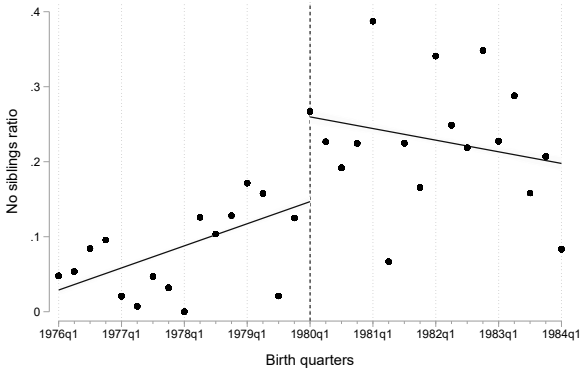
ately following the policy, and suggests a small jump in this ratio in 1980Q1 among Han urban residents. Looking at females and males separately in Figures 1b and 1c, we see a clear small jump for both genders in 1980Q1. The no-sibling ratio for females starts very low before the policy and remains fairly stable afterwards, showing an upward trend. For males, the no-sibling ratio is stable before the policy and increases afterwards. As mentioned previously, there were policy exceptions, such as allowing a second child if the first was a daughter. This can partly explain the different patterns observed between men and women.

Figure 1. Proportion of adults without siblings born within 4 years of policy cut-off

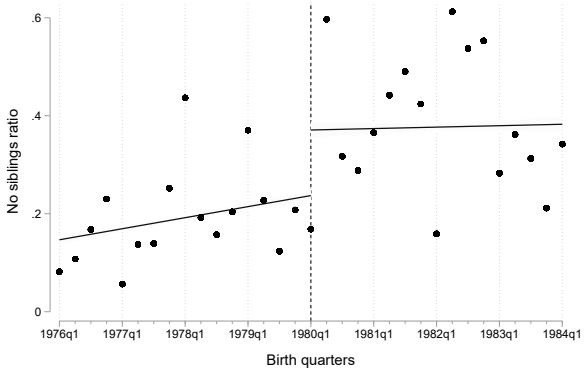
(a) Overall



(b) Female



(c) Male



Notes: Figure (a) shows the proportion of Han urban adults nationally (using sampling weights provided by the CFPS) born within 16 quarters (4 years) of the policy cut-off with no siblings, while Figures (b) and (c) show the no siblings ratio for females and males, respectively. The policy cut-off is 1980Q1. We use the 2010 CFPS adult data as this is the only wave that provides information on adults' siblings.

4 Empirical Strategy

4.1 Main specifications

To explore the spillovers of the policy, we employ a regression discontinuity in time (RDiT) with the number of quarters between mothers' date of birth and policy date as the running variable. In an RDiT, the running variable plays a crucial role in determining the treatment status when a discontinuity occurs at a specific cut-off point. In our context, this is the first quarter of 1980. However, all other covariates should exhibit smoothness at the cut-off.

We rely on a reduced-form regression discontinuity design (RDD) rather than a fuzzy RDD for several reasons. First, our goal is to identify the effects of exposure to a policy-induced discontinuity at the cut-off, rather than to estimate a local average treatment effect (LATE) of a specific endogenous choice such as family size. In our setting, the One-Child Policy leads to gradual changes in the family environment, with sibling composition as the most direct and observable manifestation. We choose not to combine the RD with an instrumental variables (IV) approach because it is challenging to separate the effects of sibling size from those of sibling gender. As shown in Figure 1, we can only see a small jump in the no-sibling ratio because not all families adhered to the policy. During its implementation, wealthier urban couples were willing to pay fines to have a second child (Burgess and Zhuang, 2002; Li et al., 2008). Moreover, literature has shown that the policy might also have an impact beyond parents' siblings composition. For example, the policy is estimated to explain at least 30% of the rise in aggregate savings, as fertility restrictions reduced expected old-age support, prompting parents to save more and invest more in their children's education (Choukhmane et al., 2023). At the household level, the policy led to significant qualitative changes in family dynamics, including diminishing patriarchal norms in daughter-only households, and greater individual freedom in decisions about living arrangements and childbearing (Shi, 2017). However, these changes at the national level (such as aggregate savings) or at the household level (such as preferences and norms) will not be captured by the RD, because individuals born before the cut-off were most likely exposed to the same changes.

In our specifications, we employ a triangular kernel weighting function in all regressions, with the weight assigned to each observation decreasing as the distance from the cut-off increases. In addition, we estimate the effects from mothers and fathers separately.

For bandwidth selection, Table 2 shows the data-driven optimal bandwidths for our two main outcomes – *Health Index* and *Distress* – following Calonico et al. (2020). We select the bandwidth equal to the average of optimal bandwidths generated for these outcomes within the parents' gender. In particular, we choose a bandwidth of 13 quarters when examining the effects from the mothers' side (maternal effects) and a bandwidth

of 10 quarters when examining the effects from the fathers' side (paternal effects). Unless specified otherwise, all analyses in our paper will use a 13-quarter bandwidth for maternal effects and a 10-quarter bandwidth for paternal effects. Specifications with other bandwidth choices will be considered in our robustness checks.

Table 2. Optimal bandwidths for RDD

	Mother's information	Father's information
	(1)	(2)
Health Index	15.635	9.843
Distress	9.237	10.128

Notes: This table shows the mean square error optimal bandwidths of main outcomes following Calonico et al. (2020) – CCT bandwidths with a local linear polynomial. Standard errors are clustered by mothers' or fathers' birth years. Column (1) presents the optimal bandwidth for each outcome using mothers' information, while column (2) presents the optimal bandwidth for each outcome using fathers' information. The optimal bandwidth using the mother's information is rounded up to 13 quarters, and the optimal bandwidth using the father's information is around 10 quarters.

The regression measuring the direct effect of the policy on the first-generation's (or parents') outcomes takes the following form:

$$Y_i^P = \alpha^P + \beta^P Policy_i + f(quarter_i^P) + \gamma^P \boldsymbol{X}_i^{GP} + \theta^P \boldsymbol{Age}_i^P + \lambda^P Province_i^P \times Birthyear_i^P + \tau_t^P + \nu_i^P \quad (1)$$

where Y_i^P denotes a parent's outcomes; $Policy_i$ is a dummy variable equal to 1 if the mother's/father's date of birth is from 1980Q1. $f(quarter_i^P)$ is the RD polynomials controlling for the distance from the cut-off in quarters. $\boldsymbol{X}_i^{GP}$ contains pre-determined demographic and social characteristics of grandparents, including their age, literacy, employment status and whether either of them is a member of the communist party.[9] $\boldsymbol{Age}_i^P$ is a non-linear control for the parent's age, including age and age^2. We also control for the parents' province-birthyear fixed effects. τ_t^P is interview year fixed effects. Standard errors are clustered by parents' year of birth. The causal effect of the policy on parents' outcomes is β^P. However, our main focus is to examine the spill-over effects of the policy on the second generation: children. The regression estimating the intergenerational effects on children's health outcomes takes the form of:

$$Y_i^C = \alpha^C + \beta^C Policy_i + f(quarter_i^P) + \gamma^C \boldsymbol{X}_i^{GP} + \theta^C \boldsymbol{Age}_i^P + \delta^C \boldsymbol{X}_i^C + \lambda^C Province_i^P \times Birthyear_i^P + \zeta^C Province_i^C \times Birthyear_i^C + \tau_t^C + \nu_i^C \quad (2)$$

[9]Party members were urged to "take the lead" in the One-Child Policy campaign. In September 1980, the Central Committee of the Chinese Communist Party issued an "Open Letter" to all Party and Youth League members, asking them to lead the way in implementing the policy (Committee, 1984; White, 1990).

where Y_i^C denotes a child's health outcomes. In addition to the pre-determined characteristics of the grandparents $\boldsymbol{X}_i^{GP}$ and the parent's non-linear age control $\boldsymbol{Age}_i^P$, we also control for the characteristics of children $\boldsymbol{X}_i^C$, in particular, age and gender, and children's province-birthyear fixed effects.

In our RD design, $f(quarter_i^P)$ are RD polynomials controlling for the distance from the cut-off in quarters. We use a local linear RD polynomial in the baseline specifications (Gelman and Imbens, 2019). The local linear polynomial has a function as $f(quarter_i^P) = \eta quarter_i^P + \sigma Policy_i \times quarter_i^P$. The interaction term between the treatment and running variables allows for different functions on either side of the cut-off. In robustness testing, we estimate models that exclude this interaction term and explore specifications with higher-order polynomials.

4.2 Identifying assumptions

Continuity assumption. The first assumption to make the RD design valid is the smoothness of all covariates at the cut-off point. We expect that the changes in our potential outcomes are solely due to the treatment initiated at the cut-off point. No other changes or discontinuities occur at the policy cut-off. Since children are considered treated if their parents were born after the cut-off policy date, this assumption is only satisfied when all other relevant covariates related to parents' birth date, in this case, grandparents' characteristics, are continuous in 1980Q1.

We conduct a balance check on a list of predetermined characteristics of both maternal and paternal grandparents using the main specifications and controlling for the time trend in grandparents' birth year, since younger people are naturally more likely to be treated by the policy. These characteristics include birth year, literacy, employment status, and whether either grandparent is a member of the Communist Party. Table 3 shows evidence that our design satisfies the continuity assumption. Across all specifications, we do not observe discontinuities of grandparents' pre-determined characteristics at the cut-off quarter.[10]

No manipulation. The second assumption ensures that participants are unable to sort themselves on either side of the cut-off point. In our context, parents' birth quarters must not be manipulated around the policy date. Even though some families were pre-aware of the policy in early 1979, because grandmothers need a gestation period of ten months, there is minimal to no opportunity for manipulation at the cut-off quarter (1980Q1).

We also conduct manipulation tests using Stata command `rddensity` proposed by Cattaneo et al. (2018). Figure 2 demonstrates the results. We see no discontinuity in the

[10]A balance check for the fathers' dataset also supports the continuity assumption of the design. See Table E.2 in Appendix E.

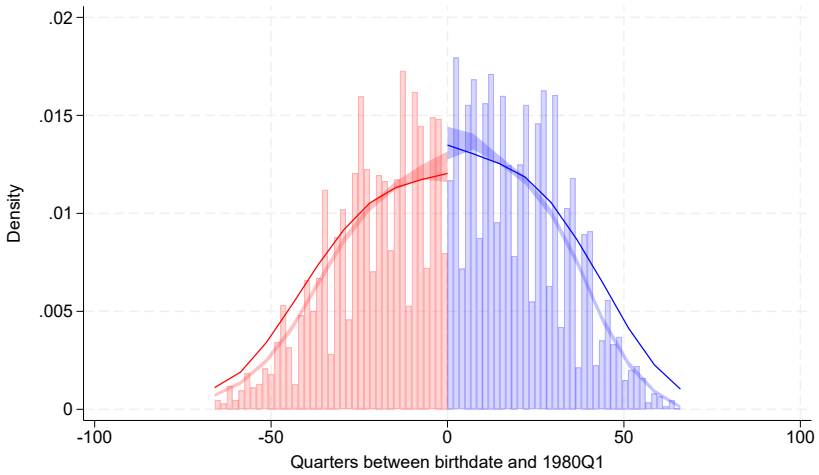
Table 3. Pre-determined characteristics of mothers' parents

	Dependent variable is:						
	Maternal grandfather			Maternal grandmother			Either
	(1) Age	(2) Literacy	(3) No Work	(4) Age	(5) Literacy	(6) No Work	(7) Communist
Policy	-0.000	-0.068	0.012	-0.000	-0.006	0.008	0.021
	(0.000)	(0.061)	(0.030)	(0.000)	(0.022)	(0.024)	(0.018)
Mean	59.208	0.816	0.062	60.951	0.621	0.169	0.197
Observations	3,555	3,555	3,555	3,555	3,555	3,555	3,555
R^2	1.000	0.182	0.140	1.000	0.229	0.156	0.142

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. Columns (1)-(3) show the characteristics of mothers' fathers, and columns (4)-(6) show the characteristics of mothers' mothers.

density of the running variable at the cutoff point, indicating no evidence of manipulation at the policy cutoff. This supports the validity of the running variable and strengthens the credibility of our regression discontinuity design.[11]

Figure 2. Manipulation tests: mothers' birth quarters



Notes: The figures depict manipulation tests based on density discontinuity using Stata command `rddensity` (Cattaneo et al., 2018). Solid lines display point estimates, separately estimated on each side of the cut-off, with shaded areas indicating 95% confidence intervals. Confidence intervals are not centered at the density point estimates because they have been bias-corrected (Cattaneo et al., 2022).

[11]The manipulation test using the fathers' dataset also indicates no statistically significant discontinuity in the density of the running variable at the cutoff. See Figure E.1 in Appendix E.

5 Results

In this section, we present the main findings of our study. We begin by examining the policy's effects on the first generation (mothers), and then turn to its intergenerational effects on children's health. We also investigate other potential channels, including household expenditure on children's health and parent–child interactions, to clarify the mechanisms underlying the policy's intergenerational effects.

5.1 The policy's effects on mothers

Mothers' sibling composition. Table 4 shows how the policy affects mothers' sibling composition. The results indicate a significant shift toward smaller family structures after the policy was implemented. Specifically, column (1) shows that exposure to the policy significantly increases the probability that a mother has no siblings by 7.9 percentage points (pp). This implies a substantial increase in the likelihood of being an only child.

Columns (2) through (5) decompose sibling composition by gender and birth order. The policy significantly reduces the likelihood of having an older brother or an older sister. In contrast, the coefficients for having a younger brother or younger sister are small and statistically insignificant. Taken together, these results suggest that the policy effectively increases the prevalence of single-child households and reduces the likelihood that the index mother was born into a pre-existing sibling group, thereby substantially reshaping family structure.[12]

Table 4. Policy effects on mothers' sibling composition

	(1) No siblings	(2) Older Brother	(3) Younger Brother	(4) Older Sister	(5) Younger Sister
Policy	0.079***	-0.101*	0.043	-0.084*	-0.019
	(0.011)	(0.042)	(0.035)	(0.040)	(0.038)
Mean	0.101	0.321	0.424	0.323	0.304
Observations	3,537	3,537	3,537	3,537	3,537
R^2	0.295	0.293	0.263	0.277	0.188

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mother' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables.

[12]We conduct the same analysis using the dataset of fathers in Appendix E Table E.3, and find that their sibling sizes do not change. This finding is consistent with the hypothesis that the policy was more likely to affect families with a firstborn girl.

Mothers' health. Table 5 examines the long-term effects of the policy on mothers' health outcomes. Overall, the policy improves general physical health outcomes for mothers while having no statistically significant effect on psychological distress. For mothers born after the policy date, the policy significantly increases their *health index* by 0.07, which is about 18% of a standard deviation (SD)[13], indicating an improvement in their overall health (columns 1 and 2). In terms of mental distress (columns 3 and 4), the estimated effects are positive but small (approximately 0.025) and not statistically significant. Table D.1 in Appendix D reports the effects on individual health components of *Health Index*. In general, mothers affected by policy exhibit better health as they are less likely to report discomfort, chronic disease, or hospitalization.[14]

Table 5. Policy effects on mothers' health status

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.071**	0.068*	0.028	0.025
	(0.026)	(0.033)	(0.025)	(0.024)
Policy × Quarter	Yes	No	Yes	No
Mean	0.041	0.041	0.277	0.277
Observations	3,550	3,550	3,472	3,472
R^2	0.071	0.071	0.064	0.064

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. Regressions (1) and (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes (See Table D.1). *Distress* is a binary variable where 1 indicates psychological distress. *Distress* equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

Mothers' demographic socioeconomic characteristics. Table 6 shows the effects of the policy on mothers' demographic and socioeconomic characteristics. The policy significantly reduces the number of children by 0.133, relative to an average of 1.764 children per mother, indicating lower fertility among mothers born after the policy. At the same time, the policy increases the educational attainment and income of those affected mothers. The probability of having a college degree or higher rises by 8.4 percentage points (pp). Log annual income increases by 0.556, implying a substantial proportional increase

[13]This standard deviation is 0.39 in Table 1.

[14]Table E.4 in Appendix E reports the policy's effects on fathers' health outcomes. We find a significant positive effect on fathers' physical health index, but a negative effect on their mental health.

in earnings. In addition, the policy raises the social position of women,[15] as their likelihood of holding a leading position increases by 0.8pp.[16]

Overall, these results suggest that policy exposure is associated with lower fertility and improved socioeconomic status in adulthood, consistent with long-term gains in human capital and economic outcomes among affected mothers (Hu and Yang, 2025).

Table 6. Policy effects on mothers' demographic and social characteristics

	(1) Number of children	(2) College+	(3) Ln(Yearly Income)	(4) Social Position
Policy	-0.133**	0.084**	0.556**	0.008**
	(0.042)	(0.027)	(0.198)	(0.003)
Mean	1.764	0.212	5.461	0.011
Observations	3,555	3,555	3,089	3,555
R^2	0.223	0.219	0.238	0.025

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *College+* takes 1 if a mother has a college degree or higher. *Social Position* is a binary variable equal to 1 if a mother holds a leading position within the Chinese Communist Party, state administration, public institutions, or enterprises.

We expect these benefits for mothers may extend to the next generation, contributing to improved health outcomes for children. Better maternal health can enhance prenatal and early-life conditions, while higher income and educational attainment enable greater investments in nutrition, healthcare, and a supportive home environment. To confirm this, we now turn to the policy's effects on children's health.

5.2 The policy's intergenerational health effects on children

In this section, we provide evidence for the intergenerational health effects of the policy on the second generation (children). The results suggest significant improvements in the physical and mental health of children born to mothers after the policy came into effect (Table 7).

In particular, children of mothers born after the policy came into effect experience an improvement in *health index* by 0.061 or about 15% of a standard deviation (SD).[17]

[15]Women are considered to have a high social status when they are leading cadres, who are high-ranking managers and officials holding leadership positions within the Chinese Communist Party (CCP), the state administration, public institutions, or enterprises.

[16]Table E.6 in Appendix E presents the policy effects on fathers' demographic and socioeconomic outcomes. In contrast to mothers, policy-exposed fathers tend to have more children, with no improvements in college attendance, income, or social status.

[17]This standard deviation is 0.39 in Table 1.

Moreover, these children are also reported to have better mental health, with a 9.3 percentage point (pp) decrease in the likelihood of being distressed.[18]

Table D.2 in Appendix D reports the effects on individual health components of *Health Index*. Children of mothers born after the policy came into effect are 3.5pp less likely to be sick and 2.7pp less likely to be hospitalized. Additionally, these children are 9pp more likely to rate themselves as healthy, representing an increase of around 23% relative to the mean. The policy is also associated with a 1.3pp increase in the probability that the interviewer rates the child as in good health.[19]

Table 7. Children's results using mothers' birth information

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.061***	0.055**	-0.093***	-0.094**
	(0.009)	(0.019)	(0.024)	(0.027)
Policy × Quarter	Yes	No	Yes	No
Mean	0.007	0.007	0.170	0.170
Observations	3,546	3,546	1,310	1,310
R^2	0.062	0.061	0.146	0.146

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. Regressions (1) and (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table D.2. *Distress* in column (4) equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

In Figure 3, we also present a visual representation of the policy's impact on children's physical and mental health using maternal birth information.[20] These figures complement the regression results and show a clear improvement in children's health and mental well-being associated with the policy-taking effects.

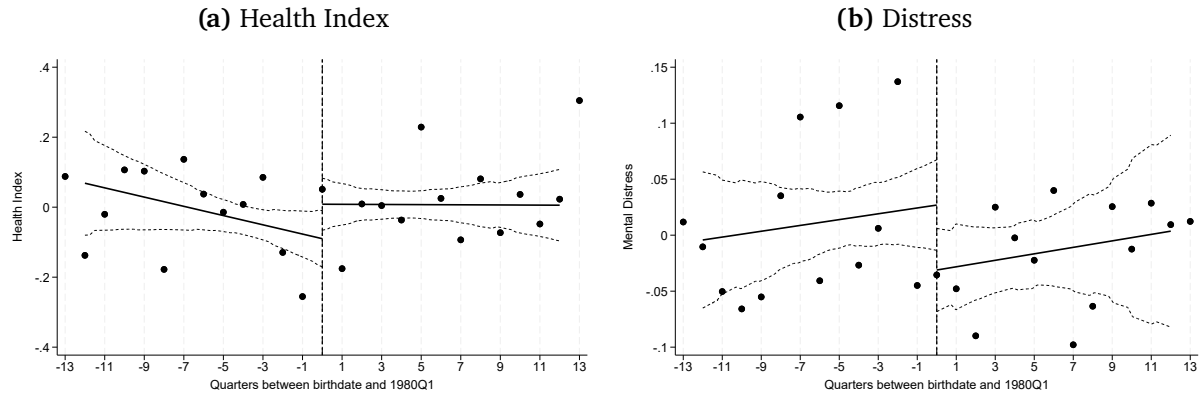
In Appendix B, we conduct a wide range of sensitivity checks to demonstrate the robustness of our results. Our results remain robust across alternative bandwidths, polynomial orders, and model specifications, including the use of months as the running variable, an alternative clustering scheme, and a donut exercise that excludes observations near the policy cutoff. We also perform placebo tests by applying the empirical

[18] In Table E.7 (Appendix E), we show the results for children using fathers' birth information. The results are consistent with those based on mothers' birth information, but are less statistically efficient. Children born to fathers affected by the policy experience an improvement in *health index* of approximately 0.05 SD. They also tend to be less distressed, though the difference is not statistically significant.

[19] See Table E.5 (Appendix E) for individual health outcomes using fathers' birth information.

[20] See Figure D.1 for quadratic polynomial regressions.

Figure 3. RD plots for children's health outcomes



Notes: The points depict binned residuals from a main regression of the outcome variable on a linear polynomial in birth quarter, along with other control variables. Solid lines display local linear regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals. Figure D.1 in Appendix D displays RD plots for quadratic polynomial regressions.

strategy to different cutoff points where no policy change occurred. We find no significant effects, which supports the validity of our identification strategy. In Appendix A, we also examine comparable items from the K6, CESD-20, and CESD-8 scales. We find consistent beneficial policy effects, with children being less likely to experience mental health problems.

Heterogeneity. In Appendix C, we also examine the heterogeneous effects by children's gender, children's age groups, and by mothers' birth order status (first-born versus later-born).

Children's cognitive outcomes. In addition to health outcomes, we also investigate the policy effects on children's cognitive outcomes. Cognitive outcomes are collected for school-age children, i.e. those aged 6 to 15, in the dataset. We observe that children born to mothers affected by the policy are slightly more likely to be selected for the key class at school, while the effects on Chinese and maths grades, measured on Likert scales (good and excellent indicators here), are not statistically significant. In this paper, we therefore focus on children's health and omit these results from the main text. The results can be found in the Appendix Table D.3.

Overall, the results on children provide evidence of the intergenerational transmission of health and household characteristics across generations. The improved health outcomes observed among children can be partly attributed to better maternal health and lower fertility shown in Section 5.1. The One Child policy has positive causal effects on mothers' health (Table 5), which, in turn, can be passed on to their children (Emanuel et al., 1992; Eriksson et al., 2005). Reduced fertility would also lead to increased human

capital investment per child, which is consistent with the quantity-quality trade-off.[21] In addition, higher parental education, income, and social status (Table 6) are likely to translate into improved child health outcomes. These factors enhance parental investments in nutrition, healthcare, and early-life environments, which are vital for child development. Our findings are consistent with the existing literature documenting how parental education (Chou et al., 2010; Keats, 2018) and family socioeconomic status (Currie et al., 2007; Poulain et al., 2019; Sepehri and Guliani, 2015) improve children's health. In the following section, we present additional evidence for increased investments in child health and explore other potential mechanisms underlying improved child health.

5.3 Within-household Mechanisms

In this section, we discuss other potential channels we can investigate empirically using the CFPS data to shed light on the policy's intergenerational effects. First, better health in children can be explained by higher investment in child health as family sizes shrink, which aligns with the quantity-quality trade-off. Second, parenting practices and parent-child interactions reveal that these parents are highly responsive while still maintaining appropriate levels of supervision, which helps explain the lower levels of psychological distress observed among their children.

Higher investment in child health. We provide evidence that children born to parents affected by the policy receive higher investments from their parents, given the fact that their family size is reduced. Table 8 presents the estimated effects of the policy on family income, overall expenditure, and expenditure specifically allocated towards children's health. We observe a statistically significant increase in total family income, although the magnitude is negligible compared to the mean (column 1).[22] Higher family income can be attributed to either greater inherited wealth or higher personal income, thanks to better education. This finding aligns with the positive effects on mothers' socioeconomic characteristics reported in Section 5.1. Policy-affected mothers are more likely to have no siblings, potentially receiving more wealth from their parents. They are also more educated and earn more than those not affected by the policy.

We find no significant effects on total expenditure or on the likelihood of children having public insurance (columns 2 and 3). However, children born to mothers affected by the policy are 6.3 percentage points more likely to have commercial or private health insurance (column 4). Additionally, column (5) shows a considerable increase in parents' spending on children's commercial insurance (0.438, a 33% increase compared to the mean). This evidence suggests that policy-affected mothers are more concerned about

[21] We will discuss this further in the next section.

[22] Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income.

Table 8. Family income and expenditure

	(1) Total Income	(2) Total Exp.	(3) Public Ins.	(4) Commercial Ins.	(5) Commercial Ins. Spending
Policy	0.154*	-0.018	-0.001	0.063***	0.438**
	(0.069)	(0.029)	(0.025)	(0.015)	(0.146)
Mean	10.703	10.903	0.709	0.204	1.296
Observations	3,403	3,399	3,456	3,443	3,438
R^2	0.260	0.302	0.162	0.071	0.079

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two are taken from family level expenditure and the rest are directly on children. Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income. We take natural logs of total income and expenditure (columns 1 and 2), medical expenditure (column 3), and commercial insurance spending (column 6). Public and commercial insurance in columns (4) and (5) are binary variables.

their children's health. They allocate more resources towards child healthcare and invest in preventive measures such as health insurance.[23]

Parenting and Family interactions. We now examine the parenting practices and interactions between parents and children to explain the lower level of distress among children. Previous research has extensively examined the relationship between child-rearing practices and children's anxiety. Children may lose their chances to advocate for their interests under parental psychological control, which triggers higher levels of mental distress (Chyung et al., 2022; Luebbe et al., 2014). McCoby (1983), building on the work of Baumrind (1971), identifies four parenting styles characterised by levels of demandingness and responsiveness. Parental demandingness or control significantly influences children's anxiety levels (Pinquart, 2017). High demands from parents cause worry and anxiety, especially for those with executive functioning deficits, to manage these concerns. Conversely, low parental demands reduce anxiety among children because they may not be worried about meeting parents' expectations (Lo et al., 2020; Wong et al., 2019). Meanwhile, parental responsiveness is another important element that decides the level of anxiety among children. High responsiveness from parents strengthens the family bond and fosters children's social and emotional development, whereas children

[23]However, our analysis of fathers' data reveals no increase in investment in children's health (Table E.9), which emphasizes that the improvements in child health primarily come from their mothers' concerns and investments.

with less responsive parents are more prone to mental disorders and struggle with social functioning (Davidov and Grusec, 2006; Miller-Slough et al., 2018).

We constructed several variables from the CFPS surveys to explore this mechanism. Table 9 reports the policy effects on the interactions between parents and children. We can see positive and significant impacts on the overall home environment, as parents are more actively involved in communicating with their children (column 1). However, there is no significant difference in their concern for children's education (column 2). From children's responses,[24] we see children more frequently communicate with their parents, either talking or arguing (columns 3-4). Since increased arguments may reflect greater parent-child engagement, especially for those early teenagers who are beginning to seek independence, the positive effect on arguing (column 3) does not necessarily indicate a poorer home environment.[25] Besides, we see null effects of children observing parents quarrelling with each other (column 5). Overall, our results suggest that household conversations increase, and as a result, children have more opportunities to speak up for themselves.

Table 9. Interaction between parents and children

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	0.018**	0.006	0.501**	0.441	0.007
	(0.006)	(0.010)	(0.148)	(0.284)	(0.186)
Mean	0.871	0.861	1.273	2.383	0.844
Observations	2,871	2,927	1,226	1,139	1,199
R^2	0.566	0.383	0.078	0.108	0.057

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables indicating interviewers' observations of whether parents communicate actively with their child and whether the home environment indicates that parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

Meanwhile, the responses of parents in Table 10 suggest that parents born after the policy cut-off tend to be more responsive and more engaged in children's education. They are more likely to forgo watching television to avoid disturbing their children (column 1) and to discuss school activities with them (column 2), consistent with the improved

[24] Only children aged 9-15 provide answers to these questions.

[25] Table D.4 displays the results when we restrict our sample to those aged 9 or above. The results stay consistent, suggesting more communication between parents and children.

within-household communication presented above. They are also more likely to check children's homework or require their children to complete homework (column 3) and to impose limits on their children's television viewing (column 4), indicating greater parental involvement in children's education. In addition, they are more likely to have saved for their children's education from an early age (column 5).[26]

Table 10. Parental care

	(1) Give up watching TV	(2) Discuss School	(3) Homework Check	(4) TV Restriction	(5) Save for Education
Policy	0.083***	0.026	0.030*	0.035**	0.030*
	(0.011)	(0.018)	(0.014)	(0.014)	(0.012)
Mean	0.245	0.105	0.327	0.200	0.251
Observations	2,170	2,247	2,196	2,231	3,457
R^2	0.095	0.083	0.130	0.075	0.099

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Outcomes in columns (1)-(4) are dummy variables constructed based on parents' responses: whether parents very often (6-7 times a week) give up watching TV to avoid disturbing their child (column 1), whether parents very often discuss happenings at school with their child this semester (column 2), whether parents very often ask their child to finish homework or check their child's homework (column 3), and whether parents restrict their child from watching TV or restrict the type of TV programs their child could watch (column 4). Column (5) is a dummy variable equal to 1 if the parent answered "yes" to the question of having started saving money for the child's education.

Literature on parenting styles suggests that the ideal parenting style is "authoritative", characterized by high responsiveness combined with appropriate parental control that supports child autonomy (Baumrind et al., 2010; Doepke and Zilibotti, 2017). Consistent with this framework, our findings show that policy-affected parents are highly responsive while still maintaining an appropriate level of control, such as monitoring homework and regulating television viewing. This balance of warmth, support, and supervision creates a home environment that is conducive to children's development. Our findings align with existing research on Chinese parents, showing that greater parental responsiveness and authoritative parenting improve child outcomes and reduce psychological distress, especially among single children (Liu et al., 2010; Lu and Chang, 2013; Nie et al., 2022).

6 Conclusion

The One-Child Policy (OCP), while formally relaxed in 2016, has left a profound and lasting legacy on China's demographic and social landscape. By restricting most urban

[26]We see a general null or somewhat negative effects on parental practices and parent-child interaction from the fathers' side in Table E.10 and Table E.11. In general, these results support our main finding using fathers' dataset that there is no discontinuity in children's level of distress at the policy cut-off.

families to one child for over three decades, the policy fundamentally altered family structures across generations. This paper provides new causal evidence on the intergenerational spillover effects of the OCP, examining specifically how the policy's enforcement on the first generation (parents) has shaped the health outcomes of the subsequent generation (children).

Utilizing a reduced-form regression discontinuity design (RDD) and data from the China Family Panel Studies (CFPS), we find significant improvements in both physical and mental health of children whose mothers were affected by the policy's enforcement. We emphasize the beneficial impacts on children from the mothers' side, given the unique benefits observed for urban Han daughters under the One-Child Policy. Specifically, these children are found to have a *health index* improvement of approximately 15% of a standard deviation and a 9.3 percentage point (pp) reduction in the probability of experiencing mental distress.

Our analysis highlights three main transmission channels through which these intergenerational gains occur. First, the mothers' socioeconomic status and health. Policy-affected mothers are more likely to be the only kids, leading to higher educational attainment, increased income, and improved physical health. These factors provide a strong foundation for their children's development. Second, reduced family size leads to greater household investment in each child's health, supporting the theoretical framework that lower fertility encourages higher-quality investment per child. Third, we find that policy-affected parents are highly responsive while maintaining appropriate control. This creates a better environment that significantly reduces psychological distress in the second generation.

These findings extend the existing literature on family planning by demonstrating that population control policies affect not only the targeted generation but also the next. While the OCP remains a subject of intense social and demographic debate, our paper suggests that it has yielded significant health benefits for subsequent generations. For policymakers, these results highlight the importance of considering the long-term, multigenerational spillover effects when evaluating the success and impact of family planning interventions.

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Appendix

A Description of Psychological Scales

CFPS uses different scales of mental health distress in different survey waves. One indicator is the Kessler Psychological Distress Scale (K6), developed by Kessler et al. (2002), which was asked in the 2010 and 2014 surveys. Respondents reported their experiences in the past month on items in Table A.1. We reverse code each item to score as 0 (never), 1 (once a month), 2 (2-3 times a month), 3 (2-3 times a week) and 4 (Almost every day) and aggregate them to a final score ranging from 0 to 24, with higher scores indicating greater depressive symptoms. While a score of 13 usually defines serious mental illness (Kessler et al., 2003), we use a lower threshold of K6 $\geq$ 5 to indicate moderate mental distress (Prochaska et al., 2012).

Another mental health indicator used in CFPS is the Center for Epidemiologic Studies Depression Scale (CES-D) (Radloff, 1977). The full 20-item CES-D was included in the 2012 and 2016 surveys, while an 8-item version was asked in the 2018 and 2020 surveys. Respondents rated their past-week status on items in Table A.1. Each item was scored as 1 (never(less than one day)), 2 (sometimes (1-2 days)), 3 (often(3-4 days)), and 4 (most of the time (5-7 days)). We reverse code items 4, 8, 12, 16 and aggregate those items, with the 20-item version ranging from 0 to 60 and the 8-item version from 0 to 24. Higher scores indicate more severe depression. The CES-D20 categorizes scores as follows: $\leq$ 16 indicates no to mild depression, 17-23 indicates moderate depression, and $\geq$ 24 indicates severe depression (Bi et al., 2023). In this paper, we use a CES-D20 cut-off of 16, corresponding to a CES-D8 cut-off score of 7, as these scores effectively identify individuals at risk of clinical depression in the Chinese context (Bi et al., 2023).

Both K6 and CES-D are frequently used to evaluate psychological distress and serious mental illness (Kessler et al., 2003; Kim et al., 2016; Weissman et al., 1977). Because these scales do not appear in all survey waves, we construct a consistent variable called *"distress"*. A child is coded as 1 (distressed) if their K6 score is $\geq$ 5 (Prochaska et al., 2012), CES-D8 score is $\geq$ 7, or CES-D20 score is $\geq$ 16 (Bi et al., 2023).

In Section 5.2, we show that children born to mothers affected by the policy are less likely to experience mental distress. Since constructing a single mental health indicator (*distress*) may raise concerns about comparability across different scales, we now focus on examining comparable items from the K6, CESD-20, and CESD-8 scales. Table A.2 shows how we map K6 items with CSED items and construct corresponding variables. Table A.3 reports the results on our constructed variables. Across all specifications, we can see consistent positive impacts of the policy on mental health. Although the estimated effect on *Depressed* is positive, it is small in magnitude and statistically insignificant. For all other mental health measures, the estimates indicate consistent beneficial effects of the policy, as children are less likely to experience mental health issues.

Table A.1. Items of psychological scales

	2010	2012	2014	2016	2018	2020
K6: Please select according to your statuses in the past month.						
(1) Feel depressed and cannot cheer up.	X		X			
(2) Feel nervous.	X		X			
(3) Feel agitated or upset and cannot remain calm.	X		X			
(4) Feel hopeless about the future.	X		X			
(5) Feel that everything is difficult.	X		X			
(6) Think life is meaningless.	X		X			
CES-D: Please select according to your statuses in the past week.						
(1) I am worried about some trivial things.		X		X		
(2) I have a poor appetite and do not want to eat.		X		X		
(3) I feel depressed despite the help from relatives and friends.		X		X		
(4) I find myself not worse than others.		X		X		
(5) I cannot concentrate on things.		X		X		
(6) I am in a low spirit.		X		X	X	X
(7) I find it difficult to do anything.		X		X	X	X
(8) I find the future promising.		X		X		
(9) I feel that I have been a loser for a long time.		X		X		
(10) I feel scared.		X		X		
(11) I cannot sleep well.		X		X	X	X
(12) I feel happy.		X		X	X	X
(13) I talk less than usual.		X		X		
(14) I feel lonely.		X		X	X	X
(15) I find that people are not friendly to me.		X		X		
(16) I have a happy life.		X		X	X	X
(17) I cried or I want to cry.		X		X		
(18) I feel sad.		X		X	X	X
(19) I find that others do not like me.		X		X		
(20) I feel that I cannot continue with my life.		X		X	X	X
Number of items	6	20	6	20	8	8

Notes: This table presents detailed items of the K6 scale and the CES-D scale and in which wave they were elicited.

Table A.2. Mapping of K6 and CES-D Items

Name of Variable	**K6 Items**	**CES-D Items**
Depressed	(1) Feel depressed and cannot cheer up.	(3) I feel depressed despite the help from relatives and friends; (6) I am in a low spirit;
Anxiety	(2) Feel nervous; (3) Feel agitated or upset and cannot remain calm.	(1) I am worried about some trivial things; (10) I feel scared.
Hopelessness	(4) Feel hopeless about the future.	(8) I find the future promising (reverse);
Difficulty	(5) Feel that everything is difficult.	(5) I cannot concentrate on things; (7) I find it difficult to do anything.
Worthlessness	(6) Think life is meaningless.	(20) I feel that I cannot continue with my life.

Notes: This table presents the corresponding items of psychological scales in K6 and CES-D. Item groupings are based on conceptual similarity rather than identical wording. CES-D items marked as reverse are positive statements, so their scores are reversed to match the rest of the scale.

Table A.3. Mental Distress: Similar items of K6 and CES-D

	(1) Depressed	(2) Anxiety	(3) Hopeless	(4) Difficulty	(5) Worthless
Policy	0.003	-0.098	-0.012	-0.045*	-0.006
	(0.017)	(0.051)	(0.049)	(0.019)	(0.005)
Mean	0.070	0.101	0.102	0.067	0.015
Observations	1,310	513	512	1,309	1,067
R^2	0.066	0.218	0.216	0.068	0.078

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

B Robustness Checks

Bandwidth sensitivity. We show bandwidth sensitivity in Appendix Figure B.1. Each sub-graph reports coefficient estimates and 95% confidence intervals of our main results for bandwidths ranging from 5 to 20 quarters. Our baseline results show robustness to changes in bandwidth. For *Health Index*, as the bandwidth increases, the positive effects of the policy become increasingly statistically significant. Similarly, for children's mental health *Distress*, larger bandwidths yield more significant policy effects, reflected in consistent reductions in distress, likely due to increased statistical power from a larger sample size.

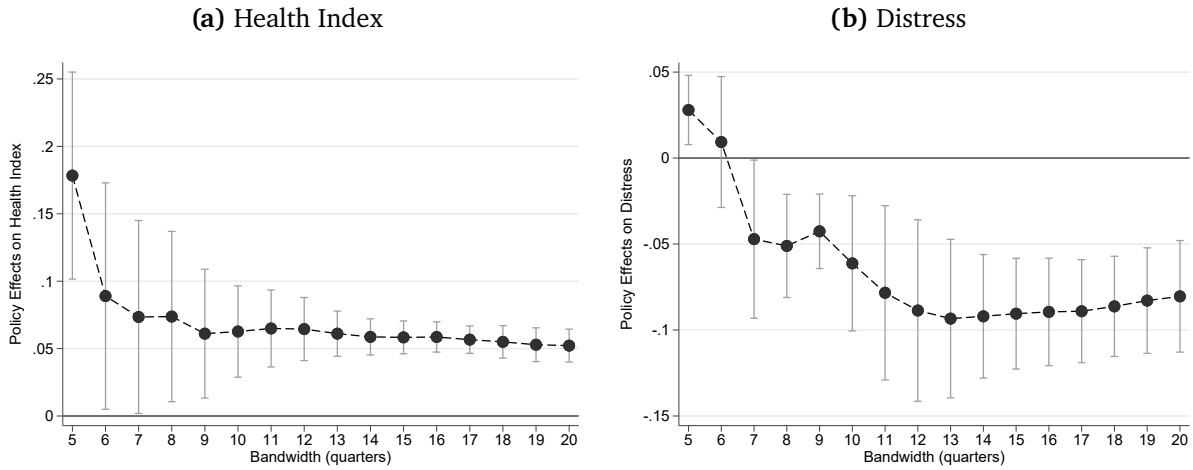
Choice of polynomial orders. In our main analysis, we use a linear polynomial of our running variable – mothers' birth quarters, which is the most common choice in RD designs. We show the sensitivity of our results to RD polynomials up to the fourth order in Appendix Figure B.2. The results suggest that our estimated effects on *Health Index* are not sensitive to the choice of polynomial order. However, higher-order polynomials produce implausibly large coefficients, suggesting potential overfitting. For *Distress*, policy effects disappear when we use third- or fourth-order polynomials, yielding null results. We do not view this as a concern, as high-order polynomials in regression discontinuity designs are known to be unstable and may absorb local variation, reducing precision rather than reflecting genuine effects.

Different specifications. We test the robustness of our main results using various specifications, as shown in Appendix Table B.1. In column (1), we keep our main specification but use months rather than quarters as the running variable. Column (2) further applies a quadratic polynomial in birth months. In column (3), we remove the triangle kernel weights to see if our results are sensitive to the weighting schemes. Column (4) alters the clustering approach by clustering at the mother's birth year and province instead of birth year alone. In column (5), we conduct a donut exercise by removing all observations within 1 quarter of the policy cut-off and keeping the rest of the sample to fit our main specification.

Across all specifications, we can see consistent positive results of the policy on children's health: children born to mothers affected by the policy exhibit better physical health index, and they are less likely to experience mental distress. The estimates for both *Health Index* and *Distress* remain stable in magnitude and statistically significant. Especially, the donut exercise (column 5) yields similar and statistically significant coefficients, indicating that the estimated policy effects are not driven by observations near the cutoff.

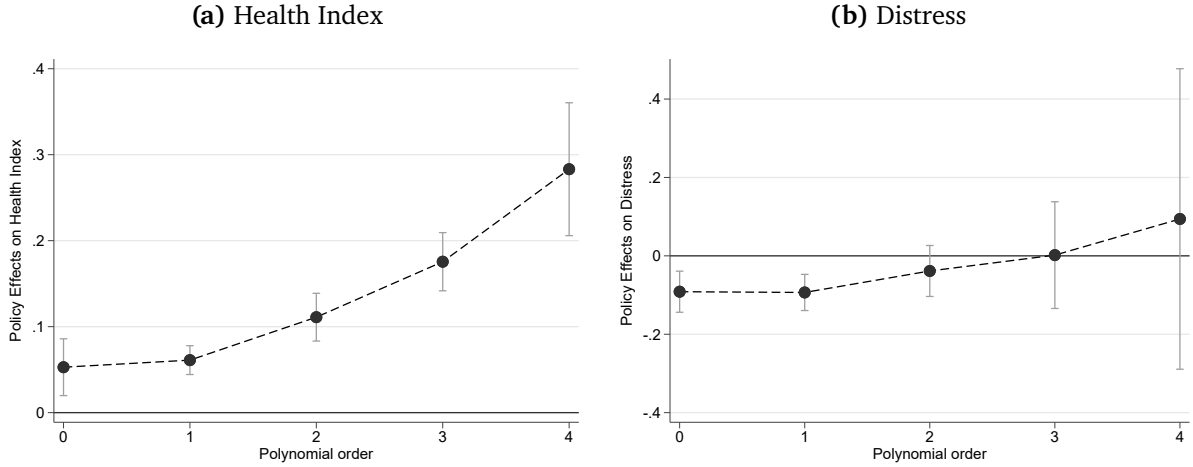
Placebo tests. Our identification strategy assumes that there is a discontinuity in the treatment at the policy implementation date. One way to test against failures of this assumption is to use placebo tests with different policy cut-offs. In Appendix Figure B.3, we reproduce our main specification results with alternative policy cut-offs up to 4 quarters prior and post the policy cut-off employed – 1980 Q1. We find no significant effects from the policy at the placebo cut-offs for *Health Index*, indicating the validity of our identification strategy. For children's mental distress, we observe a significant effect at the 1979 Q4 placebo cut-off. While this effect is noteworthy, it does not persist across other outcomes, suggesting it may be due to random variation rather than a systematic issue with our identification strategy.

Figure B.1. Sensitivity of results to bandwidth choices



Notes: Each sub-graph reports coefficient estimates and confidence intervals for different bandwidths from 5 to 20 quarters. Each dot indicates the RD estimate using the specified bandwidth. Capped spikes represent 95% confidence intervals of the estimates.

Figure B.2. Sensitivity of results to different orders of polynomial



Notes: Each dot represents the RD estimate using the specified order of RD polynomial. Capped spikes represent 95% confidence intervals of the estimates.

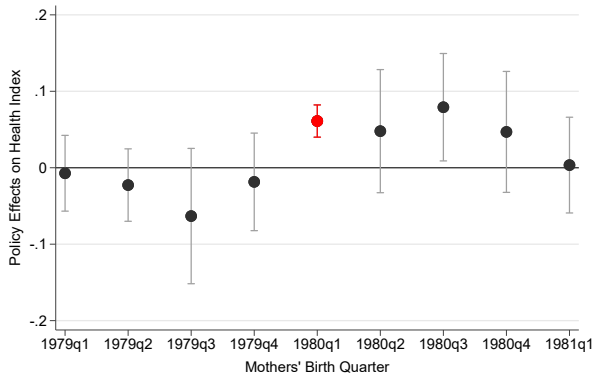
Table B.1. Robustness to different specifications

	Monthly Running (Linear)	Monthly Running (Quadratic)	No weights	Double Clusters	Donut 1Q
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent variable is: Health Index					
Policy	0.055***	0.089***	0.047**	0.061**	0.055***
	(0.006)	(0.017)	(0.016)	(0.026)	(0.014)
Mean	0.007	0.007	0.007	0.007	0.007
Observations	3,947	3,947	3,857	3,546	3,426
R^2	0.060	0.061	0.053	0.062	0.070
Panel B. Dependent variable is: Distress					
Policy	-0.094***	-0.074*	-0.089***	-0.093***	-0.090***
	(0.016)	(0.035)	(0.015)	(0.031)	(0.019)
Mean	0.170	0.170	0.170	0.170	0.173
Observations	1,501	1,501	1,460	1,310	1,269
R^2	0.136	0.137	0.108	0.146	0.147

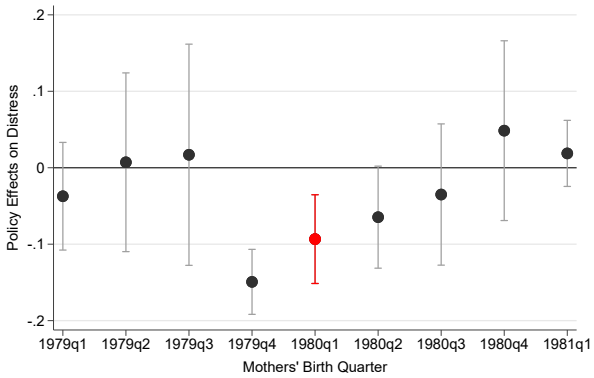
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Standard errors are in parentheses and clustered by mothers' birth years in (1) (2) (3) and (5). In column (4), standard errors are clustered by mothers' birth year and province.

Figure B.3. Placebo 1980 Q1 cut-offs

(a) Health Index



(b) Distress



Notes: This figure tests different policy cut-offs up to 4 quarters prior and post the policy cut-off employed in this paper. The policy cut-off we choose for this paper is 1980Q1, marked in red.

C Heterogeneity on children's outcomes

In this section, we examine the heterogeneous effects of the policy by children's gender and age groups, as well as by mothers' birth order status (first-born versus later-born).

Table C.1 presents the effects of the policy passed onto boys and girls separately. We run our main specification on separate subsamples of boys and girls. Boys born to a mother born after the policy have a significantly higher health index with an increase of 0.072 index points or 18% of a standard deviation (SD).[1] They are also less likely to experience distress, with a drop of 10.9 percentage points (pp). For girls, the estimates indicate similar patterns, though the effects are not statistically significant. This difference may be due to adolescent girls' tendency to rate themselves more conservatively in self-assessments, whether in self-rated health or distress levels (Boerma et al., 2016; Van Droogenbroeck et al., 2018). Research has shown that girls often report lower self-rated health and higher levels of psychological distress than boys, which may explain the observed discrepancies between health outcomes (Breidablik et al., 2009; Jerdén et al., 2011).

Table C.1. Heterogeneous effects by gender

	Health Index		Distress	
Gender groups	Boy	Girl	Boy	Girl
	(1)	(2)	(3)	(4)
Policy	0.072**	0.056	-0.109**	-0.052
	(0.029)	(0.029)	(0.037)	(0.046)
Mean	0.04	0.04	0.19	0.17
Observations	1,816	1,730	656	654
R^2	0.074	0.113	0.175	0.228

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

Table C.2 presents the impact of the policy on mothers passed on to children across different age groups (0-12 and 13-15). We group children under 12 as those who are in primary school or less than primary school age, and those aged 13-15 as those in middle school, according to the Chinese education system. Overall, the results indicate a generally better health status for both age groups. The improvement in *health index* is larger in magnitude and more statistically significant for children aged 13-15, with

[1]This standard deviation is 0.39 in Table 1.

an increase of 0.105 or 26.9% of a SD compared to 0.048, about 12% of a SD for the younger age group. However, the reduction in *distress* is larger among younger children, with a drop of 10.7 percentage points, while the decline is only 6.9 percentage points (around 30%) for the older group.

Table C.2. Heterogeneous effects by age groups

	Health Index		Distress	
Age groups	≤ 12	> 12	≤ 12	> 12
	(1)	(2)	(3)	(4)
Policy	0.048*	0.105**	-0.107**	-0.069**
	(0.021)	(0.036)	(0.034)	(0.023)
Mean	0.03	0.07	0.15	0.21
Observations	2,932	614	727	583
R^2	0.080	0.140	0.174	0.235

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

Table C.3 demonstrates the impact of the policy with two different control groups: first-born mothers vs later-born mothers. First, we restrict the sample to mothers who are either the only child or the first-born (columns 1 and 3). This subsample analysis allows us to assess the policy's effects as driven specifically by the absence of maternal siblings, helping to isolate the sibling effect from other potential influences. We can see that the effects on physical and mental health are not statistically significant. Second, we restrict the control group to mothers who are not first-borns (columns 2 and 4). For this subgroup, we see that children born to policy-affected mothers have a significantly higher physical health index. They are also less likely to be distressed. Collectively, the findings suggest that these positive health effects of policy are not driven solely by the absence of a sibling but also by other factors the policy might affect.

Table C.3. Heterogeneous effects by birth order

	Health Index		Distress	
Birth order	First	Later	First	Later
	(1)	(2)	(3)	(4)
Policy	-0.013	0.117***	-0.030	-0.137***
	(0.015)	(0.019)	(0.032)	(0.027)
Mean	0.009	0.001	0.157	0.167
Observations	2,592	2,793	869	959
R^2	0.052	0.069	0.179	0.145

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Columns (1) (3) restrict the control group to first-born mothers only. Columns (2) (4) restrict the control group to mothers who are not first borns.

D Additional Tables and Figures

Table D.1. Mothers' health: component outcomes of Health Index

	(1) No Discomfort	(2) No Chronic Disease	(3) No Hospital	(4) Self-rated Health	(5) Observed Health
Policy	0.058**	0.061***	0.081***	-0.103**	-0.001
	(0.019)	(0.007)	(0.017)	(0.028)	(0.003)
Mean	0.752	0.925	0.922	0.216	0.980
Observations	3,473	3,473	2,107	3,550	2,983
R^2	0.042	0.041	0.071	0.218	0.037

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in columns 2 to 6. *No Discomfort* takes 1 if a mother reported no physical discomfort in the last two weeks. *No Chronic Disease* is a dummy variable indicating whether a mother was not diagnosed with a chronic disease in the past six months. *No Hospital* is also a dummy variable which is equal to 1 if a mother was not hospitalized last year due to illness or injury. *Self-rated Health* takes 1 if they rated themselves as healthy. *Interviewer-observed Health* is a binary variable, with 1 indicating good health.

Table D.2. Children's health: component outcomes of Health Index (using mothers' birth information)

	(1) No Sick	(2) No Hospital	(3) Self-rated Health	(4) Observed Health
Policy	0.035**	0.027**	0.090***	0.013**
	(0.010)	(0.010)	(0.023)	(0.005)
Mean	0.732	0.794	0.389	0.978
Observations	3,463	3,461	1,312	1,759
R^2	0.098	0.101	0.126	0.062

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The policy cut-off is 1980Q1. Standard errors are in parentheses and are clustered by mothers' birth years. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. Only those aged 10 and over responded to the self-rated health question, and *self-rated health* is equal to 1 if children rate themselves as healthy. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best). The *observed health* variable equals 1 if the rating is greater than or equal to 4. The interviewers only assessed the health of those children who were present at the interview.

Table D.3. Children's cognitive outcomes

	(1) Key Class	(2) Above Average Chinese	(3) Above Average Math
Policy	0.046*	-0.014	-0.066
	(0.020)	(0.036)	(0.035)
Mean	0.093	0.665	0.683
Observations	2,051	2,094	2,096
R^2	0.078	0.139	0.144

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's age and gender. The interaction between treatment and running variables is included in all regressions. These outcomes are for children of school age, i.e., 6 to 15 years old in the dataset.

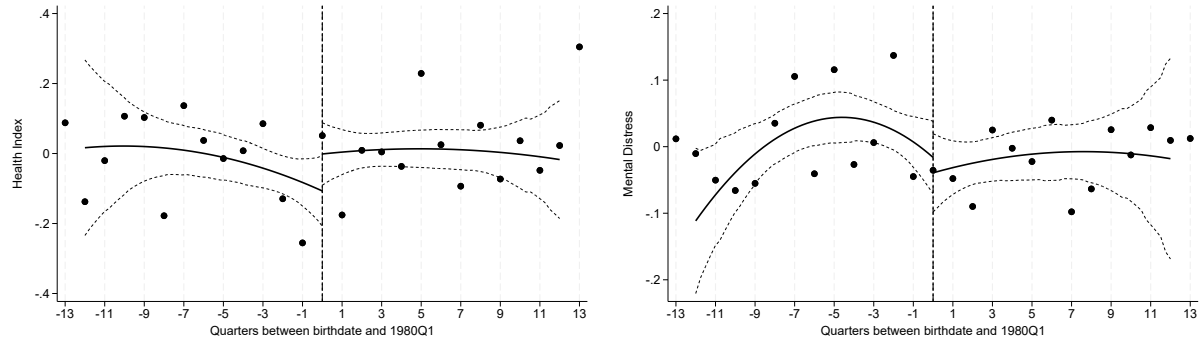
Table D.4. Effects from mothers' side: interaction between parents and children, restricted to children above 9 years old

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	0.053***	0.041	0.501**	0.441	0.007
	(0.011)	(0.022)	(0.148)	(0.284)	(0.186)
Mean	0.820	0.820	1.273	2.383	0.844
Observations	1,216	1,246	1,226	1,139	1,199
R^2	0.590	0.413	0.078	0.108	0.057

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables, showing interviewers' observations on whether parents communicate with their child actively and on whether home environment indicates parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

Figure D.1. Quadratic polynomial: RD plots for children's health outcomes

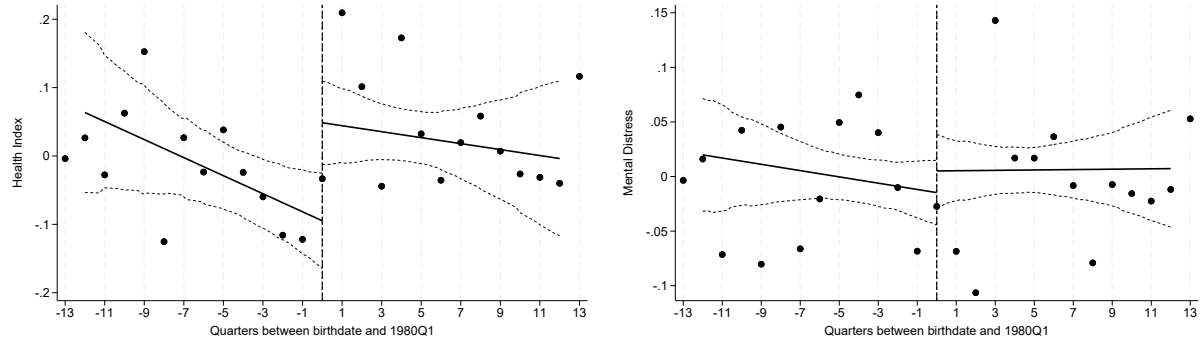
(a) Health Index **(b)** Distress



Notes: The points depict binned residuals from a main regression of the outcome variable on a quadratic polynomial in birth quarter, along with other control variables. Solid lines display quadratic polynomial regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals.

Figure D.2. RD plots for mothers' health outcomes

(a) Health Index **(b)** Distress



Notes: The points depict binned residuals from a main regression of the outcome variable on a linear polynomial in birth quarter, along with other control variables. Solid lines display local linear polynomial regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals.

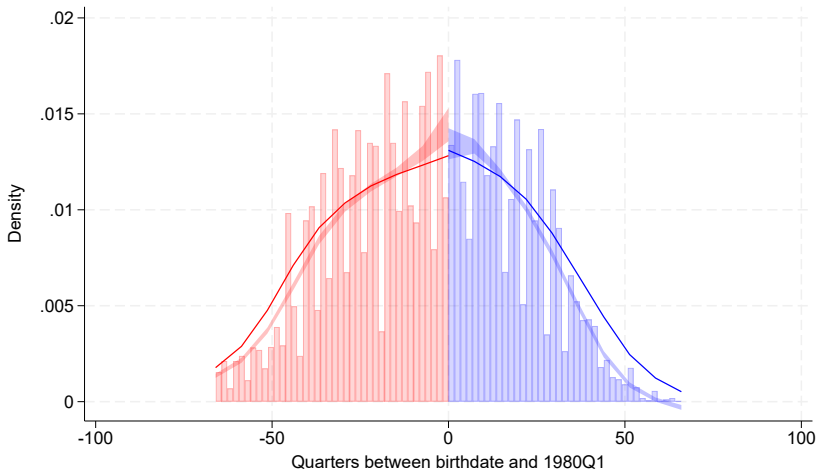
E The policy's effects on fathers and children born to these fathers

Summary Statistics. Table E.1 reports descriptive statistics for the fathers sample, including characteristics of the fathers and their children. Most statistically significant differences between the control and treatment groups are concentrated in age-related variables. This pattern is expected given the policy design, as individuals born later (and therefore younger at the time of the survey) are more likely to be exposed to the treatment.

Balance check. A balance check for the fathers' dataset supports the continuity assumption of the design. Across specifications, we find no discontinuities in grandparents' predetermined characteristics at the cutoff, with the exception of *Communist* which is significant only at the 10% level.

Manipulation test. The manipulation test (Figure E.1) using the fathers' dataset shows no evidence of a density discontinuity at the cutoff, indicating no manipulation of the running variable.

Figure E.1. Manipulation tests: fathers' birth quarters



Notes: The figures depict manipulation tests based on density discontinuity using Stata command `rddensity` (Cattaneo et al., 2018). Solid lines display point estimates, separately estimated on each side of the cut-off, with shaded areas indicating 95% confidence intervals. Confidence intervals are not centered at the density point estimates because they have been bias-corrected (Cattaneo et al., 2022).

The policy's effects on fathers. Table E.3 presents the policy effects on fathers' demographic characteristics. In contrast to the results for mothers, we observe no significant change in fathers' sibling size. This finding aligns with our hypothesis that the policy

Table E.1. Descriptive statistics

	All observations				Within 10-quarter bandwidth			
	All	Control	Treated	Diff	All	Control	Treated	Diff
A. Health Outcomes								
Health Index	0.01	0.04	-0.01	0.05***	0.00	0.01	-0.00	0.01
	(0.39)	(0.45)	(0.31)	(8.74)	(0.37)	(0.38)	(0.36)	(0.93)
No sickness last month	0.72	0.76	0.67	0.09***	0.71	0.72	0.70	0.02
	(0.45)	(0.43)	(0.47)	(12.48)	(0.45)	(0.45)	(0.46)	(1.50)
Not hospitalized last month	0.78	0.82	0.73	0.09***	0.78	0.78	0.77	0.01
	(0.42)	(0.39)	(0.44)	(12.87)	(0.42)	(0.41)	(0.42)	(1.17)
Self-rated health (healthy = 1)	0.43	0.43	0.42	0.02	0.40	0.39	0.41	-0.02
	(0.49)	(0.50)	(0.49)	(0.93)	(0.49)	(0.49)	(0.49)	(-0.75)
Interviewer-observed health (≥ 4 on 1-7 scale)	0.98	0.98	0.97	0.01*	0.97	0.98	0.97	0.01
	(0.15)	(0.14)	(0.16)	(2.03)	(0.16)	(0.14)	(0.18)	(1.70)
Distress indicator based on K6 and CESD	0.18	0.18	0.17	0.01	0.17	0.16	0.18	-0.02
	(0.38)	(0.39)	(0.37)	(1.15)	(0.37)	(0.37)	(0.38)	(-0.77)
B. Demographics								
Child's age	7.92	9.74	5.66	4.08***	7.60	8.11	7.10	1.02***
	(4.49)	(4.20)	(3.77)	(64.55)	(4.12)	(4.23)	(3.94)	(8.22)
Child's gender (female = 1)	0.46	0.45	0.46	-0.01	0.48	0.47	0.48	-0.01
	(0.50)	(0.50)	(0.50)	(-0.94)	(0.50)	(0.50)	(0.50)	(-0.66)
Child's birthyear	2007	2004	2010	-5.99***	2007	2007	2008	-1.30***
	(5.23)	(4.56)	(3.94)	(-88.80)	(3.74)	(3.95)	(3.41)	(-11.64)
Mother's age	34.85	38.28	30.54	7.74***	33.49	34.61	32.38	2.23***
	(6.22)	(5.26)	(4.38)	(97.60)	(4.23)	(4.11)	(4.07)	(17.37)
Mother's birthyear	1980	1976	1985	-9.64***	1981	1980	1983	-2.47***
	(6.65)	(5.01)	(4.07)	(-131.08)	(3.13)	(2.95)	(2.81)	(-27.71)
Father's age	36.79	40.60	31.86	8.75***	35.30	36.47	34.16	2.31***
	(6.39)	(5.10)	(4.09)	(117.27)	(3.51)	(3.31)	(3.32)	(22.55)
Father's birthyear	1978	1973	1984	-10.71***	1980	1978	1981	-2.63***
	(6.79)	(4.66)	(3.59)	(-163.58)	(1.53)	(0.73)	(0.81)	(-113.26)
Family size	5.08	4.72	5.52	-0.80***	5.30	5.06	5.53	-0.46***
	(1.89)	(1.64)	(2.07)	(-26.34)	(1.95)	(1.75)	(2.11)	(-7.95)
C. Father's characteristics								
Health Index	0.02	0.02	0.01	0.01*	0.02	0.02	0.02	0.00
	(0.42)	(0.45)	(0.37)	(2.15)	(0.40)	(0.40)	(0.40)	(0.15)
Distress indicator based on K6 and CESD	0.18	0.18	0.17	0.01	0.17	0.16	0.18	-0.02
	(0.38)	(0.39)	(0.37)	(1.15)	(0.37)	(0.37)	(0.38)	(-0.77)
No siblings	0.07	0.06	0.09	-0.02***	0.10	0.09	0.11	-0.02
	(0.26)	(0.25)	(0.28)	(-4.18)	(0.30)	(0.29)	(0.31)	(-1.36)
Number of children	1.76	1.78	1.74	0.04***	1.77	1.72	1.81	-0.09***
	(0.78)	(0.82)	(0.74)	(3.37)	(0.82)	(0.80)	(0.83)	(-3.77)
College+	0.17	0.14	0.19	-0.05***	0.20	0.21	0.19	0.03*
	(0.37)	(0.35)	(0.40)	(-8.17)	(0.40)	(0.41)	(0.39)	(2.11)
Ln(Yearly Income)	5.40	5.17	5.73	-0.56***	5.58	5.67	5.50	0.17
	(4.89)	(4.83)	(4.95)	(-6.25)	(4.94)	(4.92)	(4.97)	(1.00)
Social Position (Leading Cadres)	0.01	0.01	0.00	0.01***	0.01	0.01	0.01	0.00
	(0.08)	(0.10)	(0.06)	(4.30)	(0.09)	(0.09)	(0.09)	(0.04)
D. Grandparents' characteristics								
Grandfather's age	61.26	64.58	53.86	10.71***	57.24	58.92	55.36	3.56***
	(9.39)	(8.50)	(6.67)	(66.41)	(6.26)	(6.06)	(5.94)	(15.67)
Grandmother's age	62.74	65.55	56.44	9.11***	59.49	61.15	57.65	3.50***
	(8.74)	(8.15)	(6.44)	(58.57)	(6.57)	(6.58)	(6.05)	(14.58)
Literacy (grandfather)	0.74	0.70	0.82	-0.12***	0.78	0.77	0.80	-0.03
	(0.44)	(0.46)	(0.38)	(-13.02)	(0.41)	(0.42)	(0.40)	(-1.64)
Literacy (grandmother)	0.54	0.50	0.62	-0.12***	0.60	0.60	0.61	-0.01
	(0.50)	(0.50)	(0.49)	(-10.97)	(0.49)	(0.49)	(0.49)	(-0.52)
Unemployment (grandfather)	0.13	0.14	0.11	0.03***	0.11	0.11	0.10	0.01
	(0.34)	(0.35)	(0.32)	(4.06)	(0.31)	(0.31)	(0.31)	(0.47)
Unemployment (grandmother)	0.23	0.24	0.20	0.04***	0.20	0.19	0.20	-0.01
	(0.42)	(0.43)	(0.40)	(4.40)	(0.40)	(0.40)	(0.40)	(-0.49)
Either of grandparents is communist	0.10	0.14	0.05	0.09***	0.11	0.15	0.08	0.07***
	(0.30)	(0.35)	(0.22)	(19.75)	(0.32)	(0.35)	(0.27)	(6.87)
Observations	15922	8813	7109	15922	4367	2151	2216	4367

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses. "All" means the whole sample, "Treated" means fathers have no siblings, and "Control" means mothers have siblings. "Diff" shows the mean difference between treated and control groups. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best).

Table E.2. Pre-determined characteristics of fathers' parents

	Dependent variable is:						
	Paternal grandfather			Paternal grandmother			Either
	(1) Age	(2) Literacy	(3) No Work	(4) Age	(5) Literacy	(6) No Work	(7) Communist
Policy	0.000	0.007	0.027	0.000	-0.016	-0.015	-0.111*
	(0.000)	(0.053)	(0.023)	(0.000)	(0.083)	(0.018)	(0.049)
Mean	58.620	0.769	0.078	61.055	0.494	0.173	0.267
Observations	2,357	2,357	2,357	2,357	2,357	2,357	2,357
R^2	1.000	0.348	0.141	1.000	0.311	0.146	0.259

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. Columns (1) (2) (3) show the characteristics of fathers' fathers, and columns (4) (5) (6) show the characteristics of fathers' mothers.

primarily constrained families whose first child was female, as those families faced the strongest pressure to have a second child (typically to have a son) before the policy's strict enforcement.

Table E.3. Policy effects on fathers' sibling composition

	(1) No siblings	(2) Older Brother	(3) Younger Brother	(4) Older Sister	(5) Younger Sister
Policy	-0.023	-0.027	-0.046	-0.108*	0.305***
	(0.049)	(0.043)	(0.027)	(0.045)	(0.035)
Mean	0.218	0.220	0.117	0.334	0.206
Observations	2,346	2,346	2,346	2,346	2,346
R^2	0.357	0.323	0.187	0.361	0.221

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are reported in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables.

In terms of health (Table E.4), for fathers exposed to the policy, we observe a significant improvement in physical health, with their health index increasing by 0.134 or 33.5% of a standard deviation.[2] As shown in Table E.5, these improvements are consistent across various individual health components. In contrast, the policy has a significant adverse effect on mental health. Policy-affected fathers are approximately 17 percentage points more likely to report distress.

[2]This standard deviation is 0.40 in Table E.1.

Table E.4. Policy effects on fathers' health status

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.134***	0.128*	0.172***	0.167***
	(0.029)	(0.050)	(0.009)	(0.018)
Policy × Quarter	Yes	No	Yes	No
Mean	0.013	0.013	0.257	0.257
Observations	2,354	2,354	2,253	2,253
R^2	0.092	0.089	0.126	0.124

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. Regressions (1) (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table E.5. *Distress* is a binary variable where 1 indicates psychological distress.

Table E.5. Fathers' health status: component outcomes of Health Index

	(1) No Discomfort	(2) No Chronic Disease	(3) No Hospital	(4) Self-rated Health	(5) Observed Health
Policy	0.096**	0.068*	0.038**	0.108***	-0.002
	(0.026)	(0.027)	(0.013)	(0.023)	(0.009)
Mean	0.807	0.911	0.963	0.225	0.981
Observations	2,253	2,250	1,315	2,354	1,898
R^2	0.061	0.092	0.075	0.227	0.046

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. *No Discomfort* takes 1 if a father reported no physical discomfort in the last two weeks. *No Chronic Disease* is a dummy variable indicating whether a father was not diagnosed with a chronic disease in the past six months. *No Hospital* is also a dummy variable which is equal to 1 if a father was not hospitalized last year due to illness or injury. *Self-rated Health* takes 1 if they rated themselves as healthy. *Interviewer-observed Health* is a binary variable, with 1 indicating good health.

Table E.6 reports the effects on fathers' socioeconomic outcomes. In contrast to the effects observed for mothers, fathers exposed to the policy tend to have more children. We also find no significant change in college attendance rates, suggesting no improvement in their educational attainment. Moreover, the policy does not increase fathers' income or enhance their social status.

Table E.6. Policy effects on fathers' demographic and social characteristics

	(1) Number of children	(2) College+	(3) Ln(Yearly Income)	(4) Social Position
Policy	0.120**	0.002	-0.502*	-0.029***
	(0.045)	(0.046)	(0.215)	(0.006)
Mean	1.765	0.209	7.430	0.023
Observations	2,357	2,357	2,165	2,357
R^2	0.325	0.262	0.358	0.074

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. College+ takes 1 if a father has a college degree or higher.

The intergenerational health effects on children born to policy-affected fathers. Table E.7 presents the intergenerational health effects of the policy on the next generation – children. Children of fathers exposed to the policy exhibit better physical health, with their health index improving by about 0.05 or 13.5% of a standard deviation.[3] However, we find no statistically significant effects on mental health, although the estimates are negative, suggesting lower levels of distress.

Table E.7. Children's results using fathers' birth information

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.050***	0.051***	-0.042	-0.042
	(0.005)	(0.008)	(0.055)	(0.060)
Policy × Quarter	Yes	No	Yes	No
Mean	0.000	0.000	0.168	0.168
Observations	2,346	2,346	735	735
R^2	0.060	0.060	0.160	0.160

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. Regressions (1) (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table E.8. *Distress* in column (4) equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

[3]This standard deviation is 0.37 in Table E.1.

Table E.8. Children's Health: component outcomes (using fathers' birth information)

	(1)	(2)	(3)	(4)
	No Sick	No Hospital	Self-rated Health	Observed Health
Policy	0.081***	0.039*	-0.062	0.050***
	(0.016)	(0.016)	(0.042)	(0.010)
Mean	0.709	0.775	0.400	0.975
Observations	2,298	2,295	736	1,072
R^2	0.088	0.101	0.123	0.094

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. Only those aged 10 and over responded to the self-rated health question, and *self-rated health* is equal to 1 if children rate themselves as healthy. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best). The *observed health* variable equals 1 if the rating is greater than or equal to 4. The interviewers only assessed the health of those children who were present at the interview.

Table E.9. Effects from fathers' side: family income and expenditure

	(1)	(2)	(3)	(4)	(5)
	Total Income	Total Exp.	Public Ins.	Commercial Ins.	Commercial Ins. Spending
Policy	-0.215**	0.055	-0.013	-0.111**	-0.753**
	(0.079)	(0.081)	(0.022)	(0.029)	(0.197)
Mean	10.755	10.916	0.717	0.183	1.165
Observations	2,293	2,241	2,292	2,286	2,285
R^2	0.321	0.340	0.169	0.098	0.107

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two are taken from family level expenditure and the rest are directly on children. Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income. We take natural logs of total income and expenditure (columns 1 and 2), medical expenditure (column 3), and commercial insurance spending (column 6). Public and commercial insurance in columns (4) and (5) are binary variables.

Table E.10. Effects from fathers' side: interaction between parents and children

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	-0.001	0.008	-0.159	-0.372	0.112
	(0.013)	(0.009)	(0.194)	(0.452)	(0.247)
Mean	0.844	0.849	1.093	2.480	0.681
Observations	1,911	1,965	693	669	679
R^2	0.541	0.390	0.133	0.128	0.098

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables, showing interviewers' observations on whether parents communicate with their child actively and on whether home environment indicates parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

Table E.11. Effects from fathers' side: parental care

	(1) Give up watching TV	(2) Discuss School	(3) Homework Check	(4) TV Restriction	(5) Save for Education
Policy	-0.115**	-0.032*	-0.024	0.045	-0.130***
	(0.032)	(0.015)	(0.031)	(0.037)	(0.030)
Mean	0.240	0.107	0.308	0.199	0.231
Observations	1,361	1,407	1,373	1,398	2,290
R^2	0.133	0.103	0.167	0.074	0.114

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Outcomes in columns (1) to (4) are dummy variables constructed based on parents' responses: whether parents very often (6-7 times a week) give up watching TV to avoid disturbing their child (column 1), whether parents very often discuss happenings at school with their child this semester (column 2), whether parents very often ask their child to finish homework or check their child's homework (column 3), and whether parents restrict their child from watching TV or restrict the type of TV programs their child could watch (column 4). Column (5) is a dummy variable equal to 1 if the parent answered "yes" to the question of having started saving money for the child's education.