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**Working Paper**

## Long-term effects of environmental policies on educational performance: Evidence from China

IRENE Working Paper, No. 23-04

**Provided in Cooperation with:**

Institute of Economic Research (IRENE), University of Neuchâtel

*Suggested Citation:* Khelifa, Siwar; He, Jie (2023) : Long-term effects of environmental policies on educational performance: Evidence from China, IRENE Working Paper, No. 23-04, University of Neuchâtel, Institute of Economic Research (IRENE), Neuchâtel

This Version is available at:

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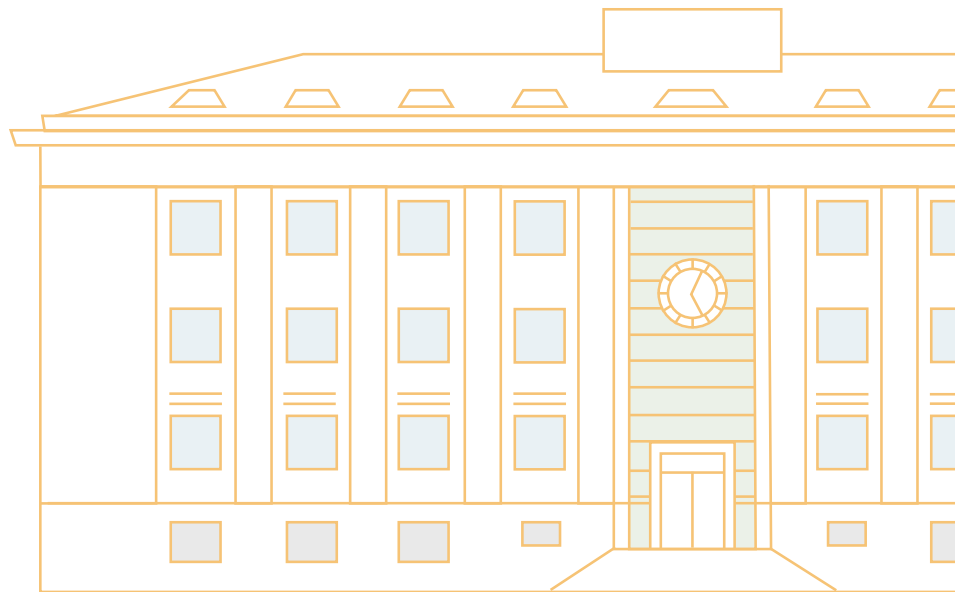
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# Long-Term Effects of Environmental Policies on Educational Performance: Evidence from China

Siwar Khelifa and Jie He

# Long-Term Effects of Environmental Policies on Educational Performance: Evidence from China

Siwar Khelifa\*      Jie He†

October 2023

## Abstract

This paper examines the overall long-term effects of the Two Control Zones policy, implemented by the Chinese government to reduce air pollution, on children’s human capital development. Estimates show that exposure to this policy, during the year of birth, is associated, 15 years later, with an increased probability to obtain better standardized test scores and thus to join a higher quality high school and an increased probability to join an academic high school. These results provide an additional evidence in favor of environmental policies as promising inputs for human capital formation. The beneficial effects are found to be accentuated among girls and children born to fathers with low education levels, suggesting that environmental regulations may help reducing some of the educational disparities, in a developing country context. Projecting forward, results also suggest better future higher education and labor market outcomes. The findings are robust to various alternative hypotheses and specifications.

**JEL Codes:** I18, I24, J24, Q51, Q56

**Keywords:** Education, environmental policy, early-life conditions, China.

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# 1 Introduction

The role of human capital in enhancing countries' productivity and economic growth has long been recognized (e.g., Schultz, 1961; Romer, 1986). Likewise, at the individual level, both cognitive and non-cognitive skills have been shown to predict success in different aspects of social and economic life, including schooling, earnings, criminality and others (e.g., Heckman et al., 2006, 2018). Its proven importance may, therefore, explain the significant amounts of investments, by both parents and governments, in children's human capital. Governments around the world, for example, were found to allocate an average of 4.5% of their GDP to education (The World Bank, 2017).<sup>1</sup>

In addition to its health and education investments, the government's role also includes remediation policies that aim to counter risk factors, for children's human capital deterioration, that are beyond the scope of individual control. One of these risk factors is air pollution. However, even though developing countries have been documenting the highest levels of air pollution worldwide, environmental regulations continue to be a controversial issue in these countries. Their little official commitment, at the recent 2022 United Nations Climate Change Conference (COP27), for implementing measures that would curb pollutant emissions corroborates this claim.<sup>2</sup> Hence, to motivate the adoption and ensure the enforcement of environmental regulations in developing countries, an internal political engagement is what may be priorly needed. As the resistance of these countries may, to a great extent, be explained by the concern that the economic costs associated with such regulations exceed their benefits, more evidence on the later may be needed. Indeed, little has been done, in developing countries, to understand the long-term effects of government environmental interventions, and this may substantially undermine their total welfare impact. Whether, and to what extent, the benefits of exposure to an environmental policy, early in life, persist in the long-term, in a developing country context, is therefore an important question yet to be answered. The claim of persistent effects into future periods is motivated by the abundant evidence on the fetal and early-life origins of adult outcomes (e.g., Almond and Currie, 2011;

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<sup>1</sup>Both monetary and time parental investments in children are shown to be determinants of children's educational and economic success (e.g., Ho, 2019; Bono et al., 2016). Government education and health investments were also found to improve exposed children's educational and labor market outcomes (e.g., Duflo, 2001; Havnes and Mogstad, 2011; Bharadwaj et al., 2013).

<sup>2</sup>The COP27 was held in Sharm el-Sheikh, Egypt. Details on the conference are available at <https://unfccc.int/cop27>.

Almond et al., 2018).<sup>3</sup>

This paper provides some of the first empirical evidence relating early-life exposure to an environmental policy to long-term cognitive ability outcomes, in a developing country context, particularly in China. The accelerated development of this country’s economy, in recent decades, has been associated with increasing ambient air pollution levels, raising concerns about the negative consequences on public health (Deng et al., 2015; Chen et al., 2017a), happiness and life satisfaction (Zhang et al., 2017; Zheng et al., 2019) and the economic activity (Chang et al., 2019; He et al., 2019; Li and Zhang, 2019).<sup>4</sup> Therefore, in an attempt to control the high levels of pollutants’ emissions, and particularly the Sulfur dioxide (SO<sub>2</sub>), the Chinese government implemented, in 1998, the Two Control Zones (TCZ) policy, which imposed stringent environmental restrictions in locations exceeding nationally mandated pollution standards. In this paper, we examine how the TCZ policy affected long-term educational performance, measured 15 years later, of those exposed to its implementation in their year of birth (in utero and first year of life). In doing so, we investigate whether cohorts that were born just before and just after the implementation of the policy exhibit persistent differences in their future educational performance, using cohorts born in locations not targeted by the policy as a counterfactual control group. However, one challenge for our empirical design is the non-randomness in the assignment of the TCZ designation. Hence, the TCZ status may be confounded with trends in unobservable characteristics that contribute, at the same time, to reductions in air pollution and/or changes in parental labor market opportunities, and to improvements in children’s future educational outcomes. To address this issue, we show that little associations exist between the TCZ status and the changes in the observable characteristics, around the time of the policy implementation, suggesting, therefore, the absence of significant correlations of the TCZ status with the unobservables (Altonji et al., 2005). Moreover, even though we focus on the overall long-term effects of the TCZ policy, inclusive of any possible channel, we investigate, in a further step, its particular effect that goes through changes in air quality.

Our main results show that cohorts exposed to the TCZ policy, during their first year of

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<sup>3</sup>The “fetal origins hypothesis” and its extensions into early childhood embody the idea that in-uterus and early-life conditions and shocks may lead to a wide range of lasting effects, which, in some cases, would arise even in the absence of observable effects at the time of exposure.

<sup>4</sup>One important reason of the high levels of air pollution in China is the heavy reliance on coal as its primary source of energy. Since 1990 and until 2015, coal accounted for more than 72% of the energy produced in China (NBS of China, 2019).

birth, exhibit improved educational outcomes measured around age 15. Particularly, we find that exposure to the TCZ policy, between conception and age 1, is associated with a 12.5 percentage point increase in the probability to get better results in a standardized high school entrance exam, and hence to attend a higher quality high school, and an 18.7 percentage point increase in the probability to join an academic high school, instead of a specialized/technical one which focuses on manual labor training. Projecting forward, we also suggest better future higher education and labor market outcomes, stemming from being able to attend academic and better quality high schools, suggesting additional persistent effects of the TCZ policy.<sup>5</sup> Furthermore, we find that important benefits associated with early-life exposure to the TCZ policy relate to girls and to children born to low educated fathers. These results suggest that environmental regulations may be used as a potential mechanism to reduce some of the disparities in educational performance. If the increased cognitive attainment is also translated into wider labor market opportunities in later years, then the TCZ policy may help reducing intergenerational transmission of inequality.

This paper contributes to the previous literature in three different ways. First, we add to the literature relating early-life pollution exposure to long-term human capital outcomes, particularly educational outcomes. Few studies assessed this effect in a developing country context (e.g. Bharadwaj et al., 2017; Rosales-Rueda and Triyana, 2019; Molina, 2021).<sup>6</sup> We further this research by considering, in the Chinese context, the persistent effects of exposure to another common pollutant, the Sulfur dioxide ( $\text{SO}_2$ ), on long-term educational outcomes.<sup>7</sup>

Second, this paper contributes to the empirical literature providing support for the theory of fetal origins and early-life determinants of long-run outcomes, and particularly, to the smaller part of the literature that examines the overall long-term effects of environmental regulations (Nilsson, 2009; Isen et al., 2017). Isen et al. (2017) reported a statistically significant

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<sup>5</sup>In addition to being an important determinant of labor market outcomes, education also shapes a range of nonmarket outcomes (e.g., Oreopoulos and Salvanes, 2011; Heckman et al., 2018). Considering the non-pecuniary returns of schooling serve only to enlarge the welfare effects of the TCZ policy.

<sup>6</sup>Molina (2021) showed that in utero exposure to thermal inversions leads to lower adult cognitive abilities in Mexico, while Bharadwaj et al. (2017) reported that fetal exposure to carbon monoxide (CO) and particulate matter ( $\text{PM}_{10}$ ) has a negative effect on fourth grade test scores in Chile. However, Rosales-Rueda and Triyana (2019) found no apparent significant effect of in utero exposure to air pollution on children's cognitive function in Indonesia.

<sup>7</sup>Previous studies on the relationship between air quality and education in China have almost exclusively focused on the short term effects of air pollution. Examples include Zivin et al. (2020) who investigated the contemporaneous impacts of fires on the National College Entrance Examination (NCEE) scores of exposed individuals. Zhang et al. (2018a) also studied the short-term effects of exposure to air pollution on cognitive performance using cognitive test scores.

relationship between an environmental policy in the United States, the Clean Air Act (CAA) of 1970, and later-life earnings. Nilsson (2009), however, exploited geographical variation in air lead levels, induced by the gasoline lead level regulations by the Swedish government, to investigate the effects of reduced early childhood lead exposure on long-run education and earnings. We add to this literature by providing evidence on the long-term benefits of an environmental policy, in a developing country context. Extrapolating results from the previous studies, focused on developed countries, may yield misleading conclusions. In case of non-linear relationships between air quality or parental economic conditions and long-term educational outcomes, the marginal effects may depend on their initial values. However, children of developed countries may be enjoying relatively smaller levels of air pollution, and better social and economic conditions.<sup>8</sup> Results will, therefore, be different between the two types of countries.<sup>9</sup>

Finally, the contribution of this paper is policy-relevant. Tanaka (2015) provided evidence on the contemporaneous improvements in infant health, by showing a reduction in infant mortality, following the TCZ policy implementation. However, focusing on short-term gains may underestimate the true total benefits of an environmental policy, particularly since some effects, such as the effects on mental development, may first become apparent only later on. We provide, in this paper, evidence on additional persistent human capital benefits of environmental policies that should be additionally considered when weighing the costs and the benefits of such policies.<sup>10</sup> Moreover, since one of the TCZ policy's prime targets was regulating coal consumption, the findings of this paper are particularly relevant for increasing the incentive, for developing countries, to effectively adopt actions in accordance with the

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<sup>8</sup>For example, in 1995, in China, the total suspended particulates level exceeded by four times that of the United States in 1970, the year when the CAA was amended (Tanaka, 2015). At the same time, in the 1990s, China's poverty population was determined to be around 260 million (Angang et al., 2005), while, in the same period, the number of poor Americans was determined to be between 31 and 39 million (Chaudry et al., 2016).

<sup>9</sup>Other reasons why developing countries should not rely on estimates from developed countries include the potential difference in the implementation and the enforcement of environmental policies in the two types of countries, hence, probably, making effects of these policies on air pollution levels and on parental earnings different. Magnitudes of the effects may also not be the same if children, from developed and developing countries, have different initial health status and/or their respective parents had different levels of knowledge about avoiding pollution.

<sup>10</sup>This is particularly important in a developing country context, where the costs of environmental regulations may be economically burdensome. Several studies documented the TCZ policy's different economic costs for the country. These costs include a decrease in sectoral exports in targeted locations (Hering and Poncet, 2014), hindrance of firm productivity (Tang et al., 2020) and a fall in foreign direct investment (Cai et al., 2016).

call, made at the 26<sup>th</sup> United Nations Climate Change Conference (COP26), to achieve a transition away from coal power generation.

The remainder of the paper is structured as follows. Section 2 presents the TCZ policy’s background and implementation. Section 3 suggests a conceptual framework describing the relationship between the environmental policy and educational outcomes. Empirical models are detailed in Section 4. Section 5 describes the data, and results are presented in Section 6. Section 7 provides some robustness checks, and Section 8 concludes.

## 2 The Two Control Zones policy

Alongside years of China’s fast economic growth, air pollution, including fine particulates such as PM<sub>10</sub> or PM<sub>2.5</sub>, or toxic gas, such as SO<sub>2</sub>, increased to levels harmful to human health. Total SO<sub>2</sub> emissions, for example, went from 18.4 million tons in 1990 to 23.7 million tons in 1995 (SEPA, 1996), making the country’s SO<sub>2</sub> concentrations among the highest in the world. In Beijing, for instance, SO<sub>2</sub> concentration levels reached 90  $\mu\text{g}/\text{m}^3$  in 1995, much higher than the World Health Organization (WHO) guideline of 50  $\mu\text{g}/\text{m}^3$  (WHO, 2000). SO<sub>2</sub> being one of its substantial triggers, the area affected by acid air expanded, from the 1980s to the mid-1990s, by more than 1 million km<sup>2</sup> (Yang and Schreifels, 2003).

In response, the Chinese government developed a number of environmental regulatory laws and policies. One of them is the 1987 Air Pollution Prevention and Control Law (APPCL). This law failed, however, to reduce air pollution, mainly because it had a vague framework for the control of air pollution, and couldn’t meet the needs of the rapid economic development of the 1990s. It was, therefore, amended in 1995 in order to motivate cleaner production technologies and regulate pollutant emissions and coal combustion. However, during this first revision of the APPCL, lawmakers had to compromise to industries and some government departments that prioritized economic development (Feng and Liao, 2016). As a result, it was also largely not enforced and had low efficacy in reducing air pollution. Despite that, one of the important points of the 1995 APPCL was the proposition of a regional scheme that identified priority areas in which air quality needs to be improved and the spread of acid rain needs to be limited.<sup>11</sup> This proposition was approved and put into effect, only, in

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<sup>11</sup>Article 27 of the 1995 APPCL specifies: “The environmental protection department under the State Council together with relevant departments under the State Council may, in light of the meteorological,



January of 1998 as the Two Control Zone policy (State Council, 1998).

**Criteria** Targeted locations of the TCZ policy were chosen based on the records of acid rain and sulfur dioxide from previous years.<sup>12</sup> Particularly, a location was designated as SO<sub>2</sub> pollution control zone if (1) the average annual ambient SO<sub>2</sub> concentrations had been higher than the national Class II standard (i.e. 60 $\mu$ g/m<sup>3</sup>), (2) the daily average concentrations of SO<sub>2</sub> had been larger than the Class III standard (i.e. 100 $\mu$ g/m<sup>3</sup>), or (3) the SO<sub>2</sub> emissions were important. A location was designated as acid rain control zone if (1) the average annual precipitation’s pH values were equal to or less than 4.5, (2) the Sulfate deposition exceeded the critical load, or (3) the SO<sub>2</sub> emissions were significant.

These areas, designated as TCZ, at the second or the third administrative level, are reported in a document, published by the State Council, known as the “Official Reply to the State Council Concerning Acid Rain Control Areas and Sulfur Dioxide Pollution Control Areas” (hereafter “Official Reply”).<sup>13</sup> Particularly, among 333 prefecture cities across 27 provinces in China, 175 were labeled as TCZ which, according to Hao et al. (2001), accounted for 11.4% of China’s territory, 40.6% of its population, 62.4% of its GDP, and 58.9% of its total SO<sub>2</sub> emissions in 1995. Geographically, the acid rain control zones involve 14 provinces, concentrated in the south, while SO<sub>2</sub> pollution control areas include 63 cities and their administrative counties with severe SO<sub>2</sub>, mainly located in the north (see Appendix Figure A1).

**Enforcement** The TCZ policy has set several measures that limit the use and production of high-sulfur coal and promote the establishment of clean coal technology. Examples of these measures that appear in the “Official Reply” include the following

- Existing mines with a coal bed having above 3 percent sulphur contents should close up, while no new mines of this type are allowed to be established.
- Existing coal mines with above 1.5 percent sulphur contents are required to install coal washing facilities, while new coal mines of this type are not allowed to operate without these facilities.

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topographical, soil and other natural conditions, delimit the areas where acid rain has occurred or will probably occur and areas that are seriously polluted by sulfur dioxide as acid rain control areas and sulfur dioxide pollution control areas, subject to approval by the State Council.”

<sup>12</sup>The exact years are not specified in the original document.

<sup>13</sup>China has four formal administrative levels, which, classified from the highest to the lowest, are: provinces/autonomous regions/municipalities, prefectures/prefecture-level cities, counties/county-level cities/districts and townships/towns/sub-districts. For simplicity, in what follows, we use “county” to refer to different third administrative level areas.

- The sulphur contents of fuel coal and fuel heavy oil in cities should be reduced according to the prescriptions of the local governments.
- Existing power plants using coal with sulfur content above 1 percent are required to take measures to reduce the discharge of sulphur dioxide before the year 2000, and to install flue gas desulfurization (FGD) equipment before the year 2010, while new and renovated power plants of this type are not allowed unless the desulfurization facilities have been installed.<sup>14</sup>
- Enterprises from industries causing serious pollution (such as in the chemical, the metallurgical, the building materials or the nonferrous metal industries) are required to install technical facilities for treatment of waste gas or take other measures to reduce the discharge of waste gas.
- A call to conscientiously collect emission fees from non-complying polluters, and to financially help major pollutant-discharging units to treat sulphur dioxide pollution, by using no less than 90% of the collected fees.

Concerned local governments and departments were asked, according to the same document, to design relevant plans and take effective measures in order to implement these and other objectives and requirements of the TCZ policy. Moreover, in order to ensure the well enforcement of the policy, the National Environmental Protection Bureau (NEPB), in addition to providing guidance, was in charge of supervising and inspecting the implementation of the different measures in the designated areas.

**Targets by the end of 2000** The main environmental targets of the TCZ policy to be reached, by the end of 2000, include reducing to the national class II standard, the urban ambient SO<sub>2</sub> concentration in 131 of the designated cities (74.86% of the TCZ cities), and reducing to the national standards, all SO<sub>2</sub> emissions from industrial sources (Hao et al., 2001).

**Variation in Regulatory Stringency** Aside from the regulation stringency across TCZ and non-TCZ areas, originating from the fact that the different measures were mainly implemented and enforced within the designated TCZ areas and much less in non-TCZ areas, there may be other sources of variation in regulation stringency between and within the TCZ locations.

First, since local governments were in charge of effectively implementing the policy's objectives, there may be a worry about a variation in the intensity of the policy's implementation across local governments. This type of variation could be relevant because it could

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<sup>14</sup>In coal power plants, FGD helps removing at least 95% or more of the SO<sub>2</sub> in the flue gases.

affect the extent of pollution reduction and the effects on the local economy in the different TCZ areas. However, the presence of the NEPB should minimize such differences.

A second type of variation in the regulation stringency can be observed between the different industries/sectors and between firm types. The TCZ policy had a higher impact on pollution-intensive industries, particularly those heavily reliant on coal, while non-polluting industries were not concerned by the different measures. Moreover, since the power sector is heavily affected by the TCZ regulations, firms relying more on energy or electricity may be more affected than other firms because of the potential energy supply shortages. There may also be differences between privately owned firms and state-owned firms, with the the former being more affected by the policy in consistence with the preferential political treatment of state-owned firms in China. These assumptions are supported by the findings in Hering and Poncet (2014), where they showed that the negative effects of the TCZ policy on exports were sharper in the more polluting industries and mainly driven by privately owned firms.

**Effectiveness** Evidence at the national level provided support to the effectiveness of the TCZ regulatory actions in reducing pollution emissions and improving air quality. National SO<sub>2</sub> emissions, for example, decreased from 23.7 million tons in 1995 to 19.95 million tons in 2000 (Gao et al., 2009). Decomposing this positive effect, air pollution and acid rain are found to have fallen substantially more in the designated TCZ locations, already in the short time between 1998 and 2000. In fact, different pieces of evidence already show the successful implementation of the TCZ requirements. For example, by the end of 1999, mines producing more than 50 million tons of coal with sulfur content over 3% had been shut down in TCZ locations (Hao et al., 2001). Also, because of their massive levels of pollutants emission, the State Power Corporation closed down, by the end of 1999, different small thermal power plants with output capacity less than 50 Megawatts (MW) (Hao et al., 2001). Moreover, small factories with inefficient technology that produced high levels of pollution have been closed. For example, by May 2001, 784 product lines in small cement and glass plants, 404 lines in iron and steel plants, and 1422 other pollution sources were shut down (He et al., 2002). Moreover, efforts were also put to adopt cleaner technologies. By the end of 2000, for example, different power plants, with total capacity of more than 10,000 MW, were found to have installed FGD equipment (Tanaka, 2015).<sup>15</sup>

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<sup>15</sup>Despite the successful implementation of the different measures of the TCZ policy, the SO<sub>2</sub> discharge fees system may have been only partially successful in controlling SO<sub>2</sub> pollution. According to He et al. (2002),

Due to this quick and successful implementation of the TCZ instructions, the different TCZ areas have witnessed a substantial fall of pollution emissions and improved overall air quality. For example, the SO<sub>2</sub> emissions dropped by about 3 million tons in the TCZ locations (Tanaka, 2015), with 84.3% of severely polluting firms decreased their SO<sub>2</sub> emissions to the standard between 1998 and 2000 in TCZ areas (Cai et al., 2016). Also, the number of cities, among TCZ cities, succeeding in decreasing their pollution levels to reach the SO<sub>2</sub> concentration standards (i.e., the Class II standard) rose from 81 in 1997 to 93 in 1998 (12 more cities), to 98 in 1999 (5 more cities) (He et al., 2002), and to 102 TCZ cities in 2000 (4 more cities) (Cai et al., 2016). Finally, comparing the evolution of SO<sub>2</sub> concentration levels between a number of TCZ and non-TCZ locations during the period from 1991 to 2000, Tanaka (2010) showed that, while SO<sub>2</sub> concentration was much higher in TCZ areas before the policy’s implementation, SO<sub>2</sub> was almost constant between 1995 and 1997, then decreased only in the TCZ localities afterwards (See Appendix Figure A2).<sup>16</sup>

Other studies investigating the effects of the TCZ policy on different outcomes also show the success of its implementation. Hering and Poncet (2014), for example, showed that the TCZ policy decreased sectoral exports in the targeted locations, in the years 1998-2003. Cai et al. (2016) showed that the policy was responsible for decreasing the foreign direct investment in TCZ areas, in the years between 1998 and 2001, while Tanaka (2015) showed that it significantly reduced infant mortality in the designated areas, during the years between 1998 and 2000. These findings further prove the effective and quick enforcement of the TCZ regulations during the 1998-2000 period.

### 3 Conceptual framework

In this section, we explore the possible channels through which the TCZ policy may have affected long-term human capital formation.

The first channel concerns changes in air quality, which itself can affect long-term outcomes in two different ways. First, damage to neurological development and physical health from early-life exposure to air pollution is biologically plausible. The reduced oxygen or

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that is because the levy standards were very low compared to the abatement cost of SO<sub>2</sub>, while the actual amount refunded to help controlling SO<sub>2</sub> pollution is very low compared to the required 90% level.

<sup>16</sup>TCZ and non TCZ locations (prefecture-level cities) used in the comparison are not reported. Pollution data come mainly from the China Environment Yearbooks and from City Statistic Yearbooks when available.

cellular and organ damages suffered by the exposed mother may result in lower nutrition and oxygen flows to the fetus, directly impairing the brain development (Sunyer and Davand, 2019).<sup>17,18</sup> Such factors may even provoke a number of physical problems, such as a shorter gestation period or a low weight at birth (Dejmek et al., 1999; Liu et al., 2019). The later was shown to have long-term effects on different outcomes, including IQ and education (McCormick et al., 1996; Black et al., 2007), poorer language and social skills (Hack et al., 1992), and behavioral problems such as increased attention deficit (Pharoah et al., 1994). Pollutants can also directly migrate to the fetus' system through the bloodstream, causing potential damage to fetal neural cells and to the respiratory and cardiovascular system. The later potential health complications may, for example, have future effects on school attendance. By showing that the TCZ implementation was associated with a lower incidence of low weights at birth, Tanaka (2015) provided one piece of evidence that the TCZ policy may have affected the long-term educational performance, through the above described health channel. Second, air pollution reductions, in China, were shown to increase productivity and labor supply of workers (Fan and Grainger, 2019; Chang et al., 2019; He et al., 2019), suggesting that parents may have been able to ensure higher incomes, following the TCZ policy. If early-childhood parental investments are increased as a consequence of these changes in income, then an additional positive effect on the long-term human capital outcomes of the exposed children may be expected. Such a claim is motivated by the evidence on the role of parental investments in determining the formation of both noncognitive and cognitive skills (Cunha and Heckman, 2008). It has been, particularly, shown that better nutrition and access to higher quality health care during pregnancy and early childhood is a simulating factor for long-term better cognitive abilities (Martorell, 2017; Currie, 2020).

The second channel concerns a direct income effect that is totally independent of changes in air quality. As shown in Section 2, different mines, thermal power plants and factories had been shut down in TCZ areas, as instructed by the TCZ regulations, while employment by newly established foreign firms in TCZ locations have decreased. The policy implementation may have, therefore, negatively affected labor market opportunities and earnings capacity of

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<sup>17</sup>As the brain development of the fetus may start since week four and continue even after birth, the longer the exposure of the mother during this period to air pollution, the bigger the potential damage to the brain may be.

<sup>18</sup>In animal models, prenatal exposure to air pollution modifies brain development, generally causing long-term problems in functions associated with memory systems (Brockmeyer and d'Angiulli, 2016).

some parents, particularly those working in the more TCZ-targeted industries and firms. If parental investments in children decreased as a consequence of this effect, long-term educational outcomes of the exposed children would have been negatively affected.<sup>19</sup>

Given the above potential channels, we present a simplified framework of how the TCZ policy may have affected the educational performance of early-life exposed children. An individual's educational performance,  $E$ , is a function of cognitive ability,  $A$ , health stock,  $h$ , individual characteristics,  $X$ , family characteristics,  $F$  and county characteristics,  $C$ :<sup>20</sup>  $E = e(A, h(I_1, I_2), X, F, C)$ , where  $I_1$  represents the inputs into the child's stock of health from early childhood, and  $I_2$  are the inputs during the following years until the outcomes are observed.<sup>21</sup> The cognitive ability,  $A$ , also depends on inputs in early and later life,  $z_1$  and  $z_2$ , respectively, on the health stock,  $h$ , and on other characteristics. Formally,  $A = a(i_1, i_2, h(I_1, I_2), X, F, C)$ . Hence, educational performance is given by

$$E = e(a(z_1, z_2, h(I_1, I_2), X, F, C), h(I_1, I_2), X, F, C) \quad (1)$$

Focusing on the TCZ policy, we assume that cohorts born in TCZ counties, right after the policy's implementation, should have been exposed to better air quality and probably higher or lower parental income, during their early life (conception and first year of birth), compared to those born just before the TCZ policy in the same counties. However, both cohorts would be exposed to the same environmental conditions at older ages. This means that, following the implementation of the TCZ policy, there will be variations in  $I_j$  for the two cohorts, as a result in the changes in air quality and/or in parental income, but where  $j=1, 2$  for cohorts born just after the TCZ policy and  $j=2$  for cohorts born just before the TCZ policy, in TCZ areas. Comparing these two groups allows, therefore, to detect the additional effect of all possible changes in early-life inputs (such as air quality and parental income),  $I_1$ , following

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<sup>19</sup>In the case where the reduced earnings of parents is accompanied with a higher degree of stress and conflict at home, it may further reinforce the negative effects of the decrease in income, as exposure to high levels of cortisol in utero negatively affects children's cognition, health, and educational attainment later in life (Aizer et al., 2016).

<sup>20</sup>An extensive body of literature has shown that cognitive ability is strongly associated with a variety of educational outcomes including standardized test scores, school completion, post-secondary education participation and other measures of student performance (Marks, 2013).

<sup>21</sup>Modeling the exact structure of these relationships is beyond the scope of this paper as they are much more complex and may involve other parts that are not presented here. Given the data availability, we focus on the reduced form relationship between early-childhood variation in inputs into health and cognitive ability and long-term educational performance.

the TCZ policy, on educational performance at older ages:

$$\frac{\partial E}{\partial I_1} = \frac{\partial e}{\partial a} \frac{\partial a}{\partial h} \frac{\partial h}{\partial I_1} + \frac{\partial e}{\partial h} \frac{\partial h}{\partial I_1} \quad (2)$$

Using this reduced-form model, we can say that a variation in  $I_1$  affects health stock,  $h$ , which itself would affect educational performance via two channels: either directly ( $\frac{\partial e}{\partial h} \frac{\partial h}{\partial I_1}$ ) or indirectly through its effect on the cognitive ability ( $\frac{\partial e}{\partial a} \frac{\partial a}{\partial h} \frac{\partial h}{\partial I_1}$ ).<sup>22</sup>

In what follows, we try, first, to identify the overall effect of the TCZ environmental policy on future educational performance of early-life exposed children ( $\frac{\partial E}{\partial I_1}$ ). Then, we focus on the effect of the policy through its impact on pollution levels.

## 4 Empirical framework

### 4.1 Long-term effects of exposure to the TCZ policy

The main goal of this study is to estimate the overall long-term effects of exposure to the TCZ policy, in a very early stage of life (in utero and before age 1), on educational performance. In other words, we are testing the sign and the magnitude of the effect in Eq. (2). We adopt a difference-in-differences (DID) approach comparing cohorts born into TCZ counties before and after the policy's implementation, relative to counterfactual:

$$Y_{ict+15} = \beta_0 + \beta_1(TCZ_c \times Post_t) + \theta_1 X_i + \theta_2 Z_{ct} + \gamma_c + \kappa_{pt} + u_{ict} \quad (3)$$

where outcome  $Y_{ict+15}$  is a measure of educational performance, 15 years later, for individual  $i$ , born in county  $c$  in year  $t$ .<sup>23</sup>  $TCZ_c$  is an indicator variable equal to 1 if county  $c$  was designated as TCZ in 1998, and  $Post_t$  is an indicator that equals 1 for the years after the

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<sup>22</sup>Aside from its direct effects, environmental policies may also affect long-term outcomes through two features of the skill formation model, which are not considered in our simplified framework, self-productivity and dynamic complementarities. The former refers to the idea that higher skills in one period create higher skills in subsequent periods, while the later embodies the idea that stocks of capacities acquired by a given period raise the productivity of investments in later periods (Cunha and Heckman, 2007).

<sup>23</sup>In the context of our paper, using education measures instead of earnings to investigate the long-term effects of an environmental regulation presents several advantages. First, reported education is less likely to be subject of serious measurement errors than reported earnings, especially in rural areas where a high number of workers are self-employed. Second, contrary to using earnings, life cycle biases are less probable to bias estimations when using education measures, as education is generally completed early or mid-twenties (Black and Devereux, 2011).

TCZ was implemented.<sup>24</sup> The interaction term equals 1 for TCZ counties after 1997.  $\beta_1$  is the key parameter that estimates the DID long-term effect of the TCZ policy, and corresponds to the effect in Eq. (2).  $X_i$  is a vector of time-varying individual characteristics that may affect the educational performance at older ages, including the number of siblings and indicators for gender, ethnicity and urban *hukou*. Individual  $i$ 's parental characteristics are also included in  $X_i$ , covering indicators for the mother's and father's levels of education (five separate indicators for each parent), the age of each parent at birth and continuous measures of both of the parents' education.  $Z_{ct}$  is a vector of weather variables and county characteristics at the year of birth. Weather controls include a second-degree polynomial in precipitation and seven variables of 10-Fahrenheit degree temperature bins. Each 10-degree bin includes the number of days with the maximum temperature falling in the given 10-degree bin: 33-39, 40-49, 50-59, 60-69, 70-79, 80-89 and 90-99. Another variable that includes the number of days with the maximum temperature  $\geq 100$  is also added. We include precipitation and temperature variables since weather conditions may interfere with air pollution levels, and at the same time, affect later life outcomes, as has been previously documented by different studies (e.g., Maccini and Yang, 2009; Hu and Li, 2016).<sup>25</sup> Finally, county characteristics at year of birth include a demographic variable (total population) and an economic variable (GDP per capita), used to control for their impact on air pollution levels and their potential effect on long-term educational performance.<sup>26</sup>  $\gamma_c$  are birth county fixed effects that absorb any time-invariant, unobserved determinants of educational performance for individuals born in a specific county. Finally,  $\kappa_{pt}$  are the province-by-year of birth fixed effects, which non-parametrically absorb any time-varying determinants of long-term educational performance for individuals born in a specific year and province. It helps purging, for example, any potential effects induced by policy changes at the province level. In all of our estimations, we cluster the standard errors at the county level to allow for county-specific correlated errors over time.

Early-childhood period is known as a critical and sensitive period of development. More-

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<sup>24</sup>Note that the non-TCZ designation does not necessarily mean that these locations are not affected by the environmental policy. It is more relevant to call TCZ areas as those where the policy is more stringent, compared to the non-TCZ areas. If TCZ policy was also responsible for reductions in air pollution in non-TCZ designated areas (either directly by enforcing the same regulations in these areas, or indirectly by reducing the traveling pollutants from TCZ areas), that would only introduce a downward bias for our results.

<sup>25</sup>Rain may help washing away air pollutants, while temperature affects their formation.

<sup>26</sup>County characteristics may also allow to control for selection into fertility.



over, empirical estimates of multistage production functions for children’s cognitive and noncognitive skills suggest the preeminence of early childhood interventions, for the disadvantaged, compared to remedial actions later in life (Cunha et al., 2010). The preference for such earlier government interventions, for children, may be supported by self-productivity and dynamic complementarities mechanisms. Therefore, even though beneficial effects may be observed in different stages of the childhood, the earlier an environmental intervention is provided, the higher its beneficial effects for subsequent outcomes may be, since the stock of skills for older children may have already been negatively affected by air pollution and hence the complementarity effect will be lower.<sup>27</sup> However, if sensitivity to air quality and to changes in parental income is higher at older ages, later childhood exposure to the environmental policy may be more or at least as effective as exposure at early childhood. This question is of clear policy relevance. Additional measures may be needed for the age-group with less gains, in terms of educational performance, in order to reinforce the initial benefits of the environmental policy. Eq. (3) focuses on the long-term effects of exposure to the TCZ implementation between conception and age 1, and does not allow to test the variation in these effects across cohorts in age, at the time of the policy’s implementation. To do so, we expand our sample to children born in years 1992–1999. All individuals born in 1999 in TCZ counties should have been exposed to the impacts of the TCZ policy implementation from conception onward. Those born in 1998 benefit from the TCZ policy in their year of birth. They may, thus, be partially exposed to its impacts in utero and fully exposed from birth onward. Those born in years before 1998 experience the TCZ policy effects from age 1 or older depending on their year of birth. The interaction term in Eq. (3) is, therefore, replaced by all the possible interactions between birth year indicators and TCZ indicator, inducing the following distributed lag model:

$$Y_{ict+15} = \phi_0 + \sum_{k=1992}^{1999} \phi_k(TCZ_c \times 1(\text{year} = k)) + \theta_1 X_i + \theta_2 Z_{ct} + \gamma_c + \kappa_{pt} + \eta_{ict} \quad (4)$$

where  $1(*)$  is an indicator variable that equals 1 if year is  $k$ . As not all of the  $\phi_k$ ’s are identified in the presence of county fixed effects, we normalize the coefficient for the 1997

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<sup>27</sup>Later government interventions, for disadvantaged children, would not be as effective if early and late investments are not perfect substitutes and if early life investments promote those made in later years (Cunha and Heckman, 2007).

cohort to be zero ( $\phi_{1997} = 0$ ). The other  $\phi_k$ 's coefficients measure, therefore, the effect of the TCZ policy on individuals who were age  $(1998 - k)$  in 1998, in TCZ counties, with respect to those exposed to the policy's implementation at age 1.<sup>28</sup> Control variables are those from model 3.

## 4.2 TCZ policy and changes in air quality

In the previous subsection, we looked at the overall net effects of early-life exposure to the TCZ environmental policy on long-term educational performance. Under the assumption that the TCZ designation is responsible for a variation in the county-level air pollution, Section 3 shows how the TCZ policy may have affected long-term outcomes through its effect on air quality.<sup>29</sup>

Panel A of Figure A6 shows the trends in county-level annual means of SO<sub>2</sub> concentration of TCZ and non-TCZ counties from 1980 to 2000.<sup>30,31</sup> As expected, SO<sub>2</sub> concentration levels were much higher in TCZ counties, compared to those designated as non-TCZ. However, the two groups of counties seem to have followed the same trend of increasing levels of the SO<sub>2</sub> concentrations until 1996, the year following the promulgation of the 1995 APPCL. Starting mainly from 1998, after the policy's implementation, SO<sub>2</sub> concentrations decreased for both groups of counties; however, the reduction in TCZ counties seems more maintained, especially between 1999 and 2000.<sup>32,33</sup>

In order to estimate the specific effect, that goes through the reductions in the SO<sub>2</sub> pollutant concentrations, of the TCZ policy, we build a two-step model. We first consider

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<sup>28</sup>A negative age of exposure refers to exposure since conception.

<sup>29</sup>As explained in Section 3, air quality affects educational outcomes of exposed children through its effect on children's health and parental income. However, health outcomes and parental income around the year of birth are unavailable in our dataset, making it impossible to directly investigate the magnitude of these two mechanisms.

<sup>30</sup>Considered counties are those included in the CHIP13 urban and rural surveys and for which pollution data is available at the county level.

<sup>31</sup>Average SO<sub>2</sub> concentration levels on Figure A6 do not seem to reflect the expected values, as those in the TCZ areas should exceed the national Class II standard of  $60\mu g/m^3$ . True SO<sub>2</sub> concentration levels are, therefore, being measured with error, but seem to capture the right trends in SO<sub>2</sub> concentration variations (the evolution of SO<sub>2</sub> concentration levels, during the period from 1992 to 2000, looks very similar to the one reported by Tanaka (2010)). Due to lack of other reliable pollution data in our study area, we present our analysis using this data. So results are to be taken with caution.

<sup>32</sup>Even though reductions in SO<sub>2</sub> concentrations may have started in 1997, those happening since the TCZ policy was put into effect, appear to be more important. We further show, in Section 7, that it is more plausible to consider 1998 as the cut-off year in our analysis.

<sup>33</sup>Panel B of Figure A6 shows a new trend of rising SO<sub>2</sub> concentrations since 2003 and that may be due to China's Accession to the World Trade Organization during that year.

a difference-in-differences regression that allows to measure the effect of the TCZ policy on SO<sub>2</sub> concentration levels:

$$SO_{2ct} = \lambda_0 + \lambda_1(TCZ_c \times Post_t) + \alpha_1 X_i + \alpha_2 Z_{ct} + \gamma_c + \kappa_{pt} + \epsilon_{ict} \quad (5)$$

where  $SO_{2ct}$  is the annual mean of SO<sub>2</sub> concentration in county  $c$  and year  $t$ , measured in  $\mu g/m^3$ .  $TCZ_c$  and  $Post_t$  indicators and other control variables are as defined in model 3. In the second step, we investigate the effect of the TCZ-induced changes in air quality, in children’s county and year of birth, on their long-term outcomes, using the predicted concentrations of SO<sub>2</sub> from Eq. (5):

$$Y_{ict+15} = \delta_0 + \delta_1 S\hat{O}_{2ct} + \tau_1 X_i + \tau_2 Z_{ct} + \gamma_c + \kappa_{pt} + v_{ict} \quad (6)$$

Section 3 suggests that the TCZ policy may have affected children’s outcomes in a way other than pollution reduction, by decreasing parental income. If this income effect was in place, then we should expect the results of Eq. (6) to be lower than the real effects of reductions in air pollution.

## 5 Data and Descriptive Statistics

### 5.1 School outcomes

Our data on educational performance are retrieved from the fifth wave survey of the China Household Income Project of the year 2013 (hereafter CHIP13). It was conducted, in July and August of 2014, by the Annual Household Survey Office of Integration of Urban and Rural in National Bureau of Statistics, to collect data on the year 2013. It surveys rural and urban households from 15 provinces, 126 cities and 234 counties from east, center and west areas of China. It covers a variety of individual and household level social and economic information including income, assets, expenditure, employment, education and other demographic information. In this paper, we make use of both the urban and the rural surveys of CHIP13. Below we describe the measures of educational performance used in our analysis. This data is matched to other datasets based on the child’s county of residence at the time of the survey.

*High School Entrance Exam Scores.* High schools in China select students based on their high school entrance exam (HSEE) score, a standardized test at the province level and known as *zhongkao* in China. Students usually get tested in Chinese, Mathematics, English, Physics, Chemistry, Political Science and PE. Better quality high schools have higher threshold scores than lower quality high schools. While students opt for the best school among the ones they were accepted in, it is reasonable to assume that HSEE test scores correlate with high school quality. We follow Sun (2019) in proxying the HSEE scores by the quality of high school. The CHIP13 asked respondents to report the type of their high school if they ever studied in high school. Six types are proposed: (1) National or provincial level key middle school, (2) City or district level key middle school, (3) County level or other key middle school, (4) Non key middle school, (5) Specialized secondary school/vocational senior secondary school/technical school, (6) Others. As more and better education resources are allocated to “key” and higher administrative level schools, compared to “non-key” and lower administrative level schools, we define the high quality high schools as those belonging to types (1), (2) and (3), assuming that children succeeding to attend these schools have better educational performance than their peers joining schools of types (4), (5) or (6).

*Academic High School.* Junior middle school graduates have the choice between joining an academic high school or a specialized/technical high school. Students wanting to attend an academic senior middle school must pass the high school entrance examination. Students also have the option to attend specialized, vocational and technical schools which provide a variety of training programs for future manual jobs. This educational path is opted for either by choice or because students failed the *zhongkao* exam.

*Qualify for High School.* Compulsory education in China includes a total of nine years of schooling in primary and junior middle schools, starting typically at the age of six. Attending high school or dropping out can be determined by the success or failure in the high school entrance exam. Moreover, those who succeeded the examination may further self-select into this additional education, so that only students that think they can do well in high school will choose to continue their education. This category includes individuals that reported having more than 9 years of formal education. In the absence of this information, we use the reported educational level, including those who attended, or are attending at the end of 2013, a senior middle school, a vocational senior secondary school, a technical school, a

specialized secondary school or in a higher level of education.

## 5.2 Air Pollution and TCZ Regulatory Status

We use SO<sub>2</sub> concentration data from the satellite-based Aerosol Optical Depth (AOD) retrievals provided by the National Aeronautics and Space Administration (NASA).<sup>34</sup> We particularly make use of the M2TMNXAER version 5.12.4 from the Modern-Era Retrospective Analysis for Research and Applications version 2 (MERRA-2).<sup>35</sup> This product, offering monthly AOD data since 1980 in grids of 0.5 degree (50 km) latitude\*0.625 degree (60 km) longitude, has been used in a number of previous studies in China (e.g., Chen et al., 2017b; Deschenes et al., 2020). To make it more adaptable to our analysis, we aggregate the SO<sub>2</sub> concentration data from grid to county level for each month, and then to annual level for each county.<sup>36</sup>

To identify whether each county in CHIP13 data was designated as TCZ in 1998, we rely on the “Official Reply” document.<sup>37,38</sup>

## 5.3 Weather

Weather data are obtained from the Global Summary of the Year (GSOY) dataset, collected from the National Centers for Environmental Information (NCEI) under the National Oceanic and Atmospheric Administration (NOAA) of the United States. It includes temperature and precipitation measures for different weather stations across China, computed from the summary of the day observations of the Global Historical Climatology Network Daily dataset (GHCN-D). In addition to climate records, the latitude and longitude of each station’s location is provided. We rely on this location information to match each county represented in CHIP13 to the nearest weather station<sup>39</sup>.

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<sup>34</sup>We cannot use ground-based pollution data as they are not available for years before 2000 at the county level. Chen et al. (2017b) found no statistical difference between the AOD data and the ground-based pollution data in China, conditional on geographic and year fixed effects.

<sup>35</sup>Available at [https://goldsmr4.gesdisc.eosdis.nasa.gov/opendap/MERRA2\\_MONTHLY/M2TMNXAER.5.12.4/contents.html](https://goldsmr4.gesdisc.eosdis.nasa.gov/opendap/MERRA2_MONTHLY/M2TMNXAER.5.12.4/contents.html)

<sup>36</sup>Details on how pollution data are manipulated are described in Appendix B.2.

<sup>37</sup>Following previous studies investigating the effects of the TCZ policy, we consider counties belonging to the acid rain and SO<sub>2</sub> control zones as consisting of one treatment group. That is because the conducted regulatory actions were the same in the two areas (e.g., Tanaka, 2015).

<sup>38</sup>More details on how TCZ status is affected to counties in CHIP13 are described in Appendix B.1.

<sup>39</sup>The number of weather stations changed over time, so in some cases, we linked more than one station to the same county.

## 5.4 County Characteristics

County level characteristics come from EPS China Data.<sup>40</sup> These characteristics include county-year information on GDP per capita and total population.<sup>41</sup>

## 5.5 Sample

For our main analysis, we restrict the sample to cohorts born in 1995-1999, the years immediately surrounding the policy implementation.<sup>42</sup> This results in a sample of 3363 rural (70%) and urban (30%) children, living in 216 different county. In order to match this data with other datasets at the county $\times$ year-of-birth level, the GB codes of counties were replaced by the county names (Ministry of civil affairs, 2014). An additional 5 observations were excluded from the sample, due to the lack of information on the county name corresponding to the GB code of these observations' counties, which reduces the sample to 3358 children, living in 214 different counties. About 32% of the sample did not get the corresponding county-level pollution data, while 9% of them have missing county characteristics data.<sup>43</sup> Considering all the other missing values of the control variables, the final sample, used for each estimation, is different depending on the data availability for the considered outcome variable.<sup>44</sup> There are 1539 children in the remaining sample, for the qualification to enter high school outcome, and 1005 children, for the outcomes about higher quality high school and academic high school attendance. About 50% of the children, considered in each sample, lived in a county that is designated as TCZ.

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<sup>40</sup> Available at: <http://www.epschinadata.com/>.

<sup>41</sup> See Appendix B.3 for details about how county characteristics are manipulated.

<sup>42</sup> The choice of our sample is constrained by education data availability. Educational performance variables should be observed at age 15 for each child. However, information for cohorts born later than 1999 are not yet observed when the survey was conducted in 2014. Those born in 1999 are aged 15 in 2014. The CHIP13 survey took place in July and August of 2014, while *zhongkao* exam is usually taken early June. Therefore, some of those born in 1999 may be already in possession of their *zhongkao* results, at the time of the survey, while others are not. This induces the issue of whether the 1999 sample is sufficiently representative of the 1999 cohort. As the missing outcomes' data are due to age-related inability to participate in the measurement, because of the survey timing, and unrelated to the educational outcomes, missingness should not be a problem. Consequently, despite the smaller number of observations for the 1999 cohort, it is included in the analyses because it is more informative to use all available data that contribute to the estimation of the effects. See Appendix Figure A3 for a summary of when each birth cohort should have joined high school.

<sup>43</sup> Since we only use observations with available county level SO<sub>2</sub> concentrations, this results in 1760 observations, collected from 91 different counties, when running the first step of the model from Section 4.2 (i.e., Eq. (5))

<sup>44</sup> About 36% of the total sample have missing information about whether they joined high school, while about 56% of them did not report information about the quality/type of their high school.

For the comparison of the long-term effects by age of exposure at the time of the TCZ implementation, we expand our sample to individuals born between 1992 and 1999. This leads to a sample of 5696 rural and urban children. 9 observations are additionally dropped for the lack of information on the county names corresponding to their GB codes, which results in a sample of 5687 children. Considering all the missing values of the control variables, there are 3189 children in the remaining sample, for the outcome variable about the qualification for high school, and 2073 children, for the outcomes about which high school they attended. About 50% of the children, considered in each sample, lived in a county that is designated as TCZ.

## 5.6 Descriptive statistics

Using the sample of individuals born between 1992 and 1997, Appendix Table A1 presents the coefficients on the TCZ indicator from separate regressions for each of the observable characteristics. As the TCZ status was not randomly assigned, it is expected to see differences between the two localities in the pre-TCZ period. Particularly, we found that TCZ counties tend to include children with a lower number of siblings, a higher mother’s and father’s education and a higher probability of having the urban *hukou*. There also seems to be differences in weather indicators between the two areas. TCZ counties have a higher average temperature and a higher total precipitation per year.

# 6 Empirical results

## 6.1 Exposure to TCZ policy in early childhood

Table 1 presents our difference-in-differences estimates of the TCZ implementation’s effects on educational outcomes of those born in TCZ counties, 15 years later. The dependent variable in panel A is the likelihood of attending a high quality high school. Column (1) which controls for weather and county characteristics in addition to county and province-by-year of birth fixed effects, shows that those exposed to the TCZ policy implementation during conception and before age 1 are more likely to attend a higher quality high school, compared to counterfactual. More controls are added across the columns, addressing the

Table 1 – Effect of TCZ Implementation in the year of birth on long-term educational outcomes

	(1)	(2)	(3)	(4)
A. High quality high school				
TCZxPost	0.855** (0.349)	0.998*** (0.344)	0.998*** (0.356)	1.012*** (0.361)
Observations	939	939	939	939
Pseudo R <sup>2</sup>	0.174	0.226	0.254	0.260
B. Academic high school				
TCZxPost	0.892** (0.435)	1.117** (0.488)	1.289** (0.523)	1.299** (0.531)
Observations	709	709	709	709
Pseudo R <sup>2</sup>	0.184	0.229	0.282	0.282
C. Qualify for high school				
TCZxPost	-0.021 (0.371)	0.143 (0.411)	0.231 (0.414)	0.206 (0.417)
Observations	1126	1126	1126	1126
Pseudo R <sup>2</sup>	0.126	0.204	0.229	0.257
Child characteristics	No	No	No	Yes
Parents characteristics1	No	Yes	Yes	Yes
Parents characteristics2	No	No	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes	Yes

*Notes:* This table reports regression coefficients from 12 separate pooled Probit regressions. Individual-level micro data is used for these regressions. The sample includes all observations for the years 1995-1999 and remains the same across all columns for each outcome. The outcome in panel A is a dummy variable that equals 1 if attending a high quality high school and 0 otherwise, the outcome in panel B is a dummy variable that equals 1 if attending an academic high school and 0 otherwise, and the outcome in panel C is a dummy variable that equals 1 if attending a high school and 0 otherwise. Estimates reported are that of the TCZxPost variable, which is an indicator that equals one for counties designated as TCZ interacted with an indicator that equals one for birth years after 1997. All regressions are unweighted and include controls for county characteristics (population, GDP per capita), weather variables (second-degree polynomials in annual precipitation, 10-degree bins based on daily maximum temperatures), province by year of birth fixed effects and county fixed effects. Additional controls are listed in the table and cover parents characteristics1 (continuous measures of both mother’s and father’s education, mother’s age at birth), parents characteristics2 (father’s age at birth, indicators for levels of education of both the mother and father) and individual characteristics (gender, ethnicity, number of siblings, urban hukou). Standard errors in parentheses are clustered at the county level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . See Appendix Table A2 for details about these estimations.



concern that time varying individual characteristics that may explain the long-term outcomes are correlated with the TCZ status. Children’s outcomes may differ by parents’ education, for example, while systematic differences exist in the mother’s and father’s education between TCZ and non-TCZ counties (Appendix Table A1). We find that the exposure to the TCZ policy implementation, early in life, is associated with obtaining better exam results, and therefore, a higher predicted probability of joining a higher quality high school, in models including parents characteristics (columns (2) and (3)) and child characteristics (column (4)).<sup>45</sup>

Panel B of Table 1 provides results using the likelihood of attending an academic high school with respect to attending a specialized/technical high school as the dependent variable. Estimates show that cohorts exposed to the TCZ implementation in utero and before age 1 are more likely to attend an academic high school. The effects are positive and significant across the different specifications adding more controls.

Finally, panel C estimates the effect on the probability of high school enrollment. The estimate is negative when we only control for weather and county characteristics, but becomes positive when we further control for parents and child characteristics. Differently from the previous outcomes, the coefficients fail to be statistically significant. In sum, our results suggest that the majority of the effect of the TCZ policy is occurring with respect to exam results and, therefore, the type of high school to attend instead of the qualification to enter high school.<sup>46</sup>

The key identifying assumption for the validity of these results is that trends in outcomes between TCZ and non-TCZ counties would have evolved similarly in the absence of the policy, conditional on covariates. Figure 1 presents, from Eq. (4), estimates of the interaction terms between TCZ indicator and children’s year of birth dummies, compared to that of the 1997 year dummy, using high quality high school, academic high school and qualify for high school variables as dependent variables. These pairwise comparisons come in support for the identification assumption for Eq. (3): as the trends before the TCZ implementation

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<sup>45</sup>Each model is considered as independent of and incomparable to the other models in the different columns (see e.g., Buis, 2017).

<sup>46</sup>This may be explained by the fact that most parents (or children) may consider their children (themselves) too young to start working upon graduation from middle school (even though the legal age for working in China is 16). Therefore, they may pressure their children to continue their studies in high school even if the children do not want or cannot succeed further education. Therefore, this variable may not constitute a good proxy for cognitive abilities, as expected above.

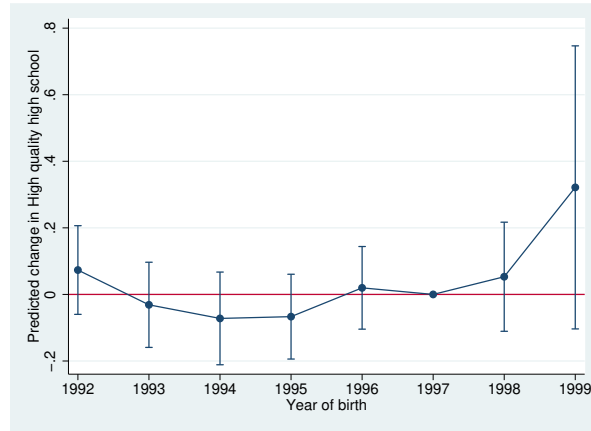
between the treatment and control cohorts are almost similar for the three outcomes, we can suggest that the trends after 1998 would have been similar without the effects of the TCZ policy. We also notice some improvements in the outcomes of children born in TCZ counties, during the two years after the TCZ policy implementation.<sup>47</sup> These variations suggest additional benefits of exposure to the implementation of an environmental regulation between conception and age 1, compared with cohorts exposed to the same regulation at age 1 and later.

Table 2 provides additional evidence for the validity of our identification assumption. TCZ status is not randomly assigned. It is affected, in part, based on emission levels and concentrations of SO<sub>2</sub> pollutant in each location. Therefore, it is possible that the estimated TCZ effects are reflecting impacts of the factors that are conditioning air quality in the respective counties. In Table 2, we test for potential differences in trends of observable characteristics, following the TCZ policy implementation, between TCZ and non-TCZ counties. Each characteristic is used as the dependent variable in a separate regression, to compute the coefficient on the interaction term from Eq. (3). As we cannot test for the treatment status not covarying with unobservable characteristics, we assume that the absence of significant relations with observable characteristics implies an absence of significant correlations with unobservables (Altonji et al., 2005). Results suggest that trends, between TCZ and non-TCZ counties, in most of the observable characteristics, that may be determinants of the educational outcomes, are similar, further supporting our research design. Particularly, the small correlation with local economic trends, measured by the coefficient on GDP per capita, suggests little probability that economic shocks would bias our results. There are, however, two covariates that exhibit significant differences: number of siblings and precipitation. For this reason, we later replicate all regressions dropping the number of siblings variable to test for any changes in estimates. Other policies affecting educational outcomes, implemented around the time of the TCZ policy, may also confound our results. The 9<sup>th</sup> Five-Year Plan, for example, decides on the different policies to implement for the years 1996-2000. We present, therefore, the balancing test results for the period 1996-1999 as a robustness check. The TCZ designation seems to balance the trends in important determinants of educational

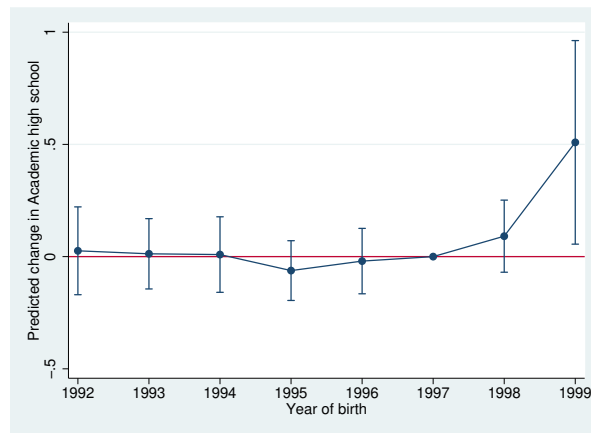
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<sup>47</sup>The 1999 cohort is represented by a smaller number of observations. Consequently, AMEs on that cohort may end up with large standard errors and wide confidence intervals. However, this loss of precision does not condemn the usefulness of the results.

(a) High quality high school



(b) Academic high school



(c) Qualify for high school

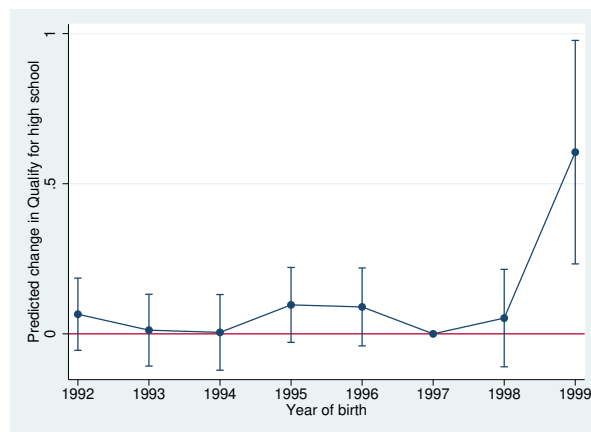


Figure 1 – Effect of TCZ implementation on long-run education outcomes by year of birth (FE probit). This figure presents regression coefficients, from Eq. (4). We plot, for each outcome, indicated in the subheadings, the coefficients on the interactions between birth year indicators and the indicator for TCZ status, and their associated 90% Confidence Interval. The coefficient on the interaction between TCZ and birth year 1997 is normalized to zero. Standard errors are clustered by county.

outcomes, considering this period.

*Incidental parameter problem.*—In our basic specification, we estimate a fixed effects model where we include county and year of birth dummies to capture unobserved heterogeneity. However, considering the large number of per-county observations for some counties but the few observations for others, the fixed effects estimations may result in an incidental parameter problem (Neyman and Scott, 1948). To address this issue, we consider two alternative estimation approaches. First, we estimate the Correlated Random Effects (CRE) model for the three educational outcomes, by adding county-specific year averages of all time-varying covariates, rather than the county dummies, and the number of yearly data entries of each county (Wooldridge, 2019).<sup>48</sup> The inclusion of these within-county means accounts for county time-invariant unobserved heterogeneity. This approach tries to approximate a “fixed effects” strategy without estimating many incidental parameters. Panel A of Table 3 reports the results. The TCZ policy implementation is positively and significantly associated with a higher probability to attend an academic high school, for those exposed between conception and age 1. The effect on the probability to attend a higher quality high school is positive but not precisely estimated. We reexamine the pre-existing trends using the CRE estimations of Eq. (4), for the three educational outcomes. Estimates of the interaction terms between each TCZ indicator and children’s year of birth dummy, compared to that of the 1997 year dummy, are presented in Appendix Figure A4. Comparisons of each marginal effect, for years before 1998, with that of 1997 are close to zero and non significant for all but the qualification for high school variable. These pairwise comparisons suggest that the trends for the two outcomes, high quality high school and academic high school, before the TCZ implementation, between the TCZ and non-TCZ cohorts, are almost similar, supporting the identification assumption of Eq. (3). The Figure also shows that the estimates of the differences in AMEs, for those born in 1998-99, are positive and larger than zero, suggesting improvements in the outcomes of those born in TCZ counties, during the two years after the TCZ policy implementation. To determine how large these effects are, we compute the DID estimates of the TCZ implementation effect, measured in probability of outcomes. Results are presented in Panel B of Table 3. The average difference-in-differences in probability of attending a high quality high school is 12.5 percentage points, while it reaches 18.7 percent-

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<sup>48</sup>Wooldridge (2019) developed the correlated random effects approach to be used for nonlinear models under unbalanced panels.

Table 2 – Balancing test by TCZ status

	Trend difference	
	1992-1999	1996-1999
Male	0.040 (0.116)	0.251* (0.140)
Han	0.304 (0.444)	-0.101 (0.739)
Number of siblings	-0.121* (0.066)	-0.063 (0.074)
Urban hukou	-0.092 (0.193)	0.126 (0.230)
Father's years of education	0.130 (0.228)	0.045 (0.273)
Mother's years of education	-0.261 (0.287)	-0.074 (0.302)
Mother's age at birth	0.002 (0.305)	0.048 (0.379)
Father's age at birth	0.290 (0.301)	0.048 (0.395)
Total precipitation	84.70* (44.23)	56.14 (34.51)
Average temperature	0.064 (0.072)	0.067 (0.075)
Population (10000 persons)	0.012 (0.024)	0.003 (0.015)
GDP per capita	0.082 (0.066)	0.054 (0.044)

*Notes:* The estimates are that of the coefficients of the interaction term of TCZ indicator and a Post indicator (=1 for 1998 and 1999). Separate regressions are estimated while using each of the respective variables as the dependent variable based on Eq. (3). Note that outliers created due to the linear interpolation of GDP per capita and Population (10000 persons) variables are dropped when running the regressions for these variables. Column (1) reports results using all those born between 1992 and 1999, while column (2) reports results for those born between 1996 and 1999. See notes to Table 1.

age points for the probability of attending an academic high school. The two estimates are statistically significant at the 1% conventional level. This implies that our results are consistent with an important effect of exposure to the TCZ intervention between conception and age 1.<sup>49</sup> Second, we present estimates from the Linear Probability Model (Panel C of Table 3). Limitations of the LPM include getting some of the predicted probabilities outside of the unit interval. However, that does not necessarily compromise the usefulness of the estimates (Wooldridge, 2010). Moreover, to account for the violation of the homoskedasticity assumption, we report cluster-robust standard errors, adjusted at the county level. We find similar results in terms of significance and signs of the effects.<sup>50</sup> The TCZ policy implementation, during first year of birth, is associated with higher probabilities of attending a higher quality high school and an academic high school.<sup>51</sup>

## 6.2 Mechanism: changes in air quality

The conditional mixed-process (CMP) technique, developed by Roodman (2011), is used to explore how the TCZ policy affected long-term educational outcomes, through its effect on air pollution.<sup>52</sup>

Table 4 presents the results using individual micro-data.<sup>53</sup> Although the data on SO<sub>2</sub> concentration are imperfect (albeit the best we can get, given the availability of this informa-

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<sup>49</sup>Tanaka (2015) showed that the TCZ policy was responsible for a decrease in the infant mortality rate by 20% in cities designated as TCZ, and that the highest reduction in infant mortality was among infants with low education mothers. Those additional individuals ‘saved’ thanks to the TCZ policy may have had higher or lower educational outcomes than the average, hence affecting the measured effects of the TCZ policy either upwards or downwards.

<sup>50</sup>We reexamine the pre-existing trends using the LPM estimations of Eq. (4). Appendix Figure A5 displaying estimates of the interaction terms between TCZ indicator and children’s year of birth dummies provides some evidence supporting the identification assumption for Eq. (3): the trends before the TCZ implementation between the TCZ and non-TCZ cohorts are almost similar for the two outcomes, high quality high school and academic high school, at the 90% confidence interval. Moreover, reexamining, the potential differences in trends, following the TCZ policy, between TCZ and non-TCZ counties, for the three covariates (Male, Han and Urban hukou) in Table 2, using the LPM, yields non-significant estimates, under the two samples (1992-1999 and 1996-1999).

<sup>51</sup>It is also possible to overcome the incidental parameter problem using the analytical and jackknife bias corrections proposed by Fernández-Val and Weidner (2016). Their approach can be used in the case of both time and cross-sectional fixed effects, but weak dependence is imposed. Conditional maximum likelihood estimations can also be used, however, they impose conditional independence across time (Wooldridge, 2010).

<sup>52</sup>The CMP technique allows to build a framework similar to that of the two-stage least squares, but while allowing for different sample sizes in each step of the estimations. We were, therefore, able to use a much larger dataset, covering more counties, in the first step (Eq. (5)), to estimate the relationship between the TCZ implementation and SO<sub>2</sub> concentration levels.

<sup>53</sup>Note that estimations include only observations with available county level SO<sub>2</sub> concentrations.

Table 3 – Effect of TCZ Implementation in the year of birth on long-term cognitive outcomes, CRE

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. CRE model</i>			
TCZxPost	0.473 (0.298)	1.002** (0.392)	0.192 (0.314)
Observations	997	959	1425
Pseudo R <sup>2</sup>	0.183	0.234	0.237
<i>Panel B. Differences in probability (CRE)</i>			
TCZ=0	0.425*** (0.055)	0.054 (0.152)	-0.370 (0.248)
Confidence Interval (95%)	[0.318, 0.532]	[-0.243, 0.352]	[-0.856, 0.116]
TCZ=1	0.551*** (0.055)	0.241** (0.117)	-0.302 (0.280)
Confidence Interval (95%)	[0.444, 0.658]	[0.012, 0.471]	[-0.851, 0.248]
TCZxPost	0.125*** (0.041)	0.187*** (0.064)	0.069 (0.085)
Confidence Interval (95%)	[0.045, 0.206]	[0.061, 0.313]	[-0.098, 0.236]
<i>Panel C. LPM</i>			
TCZxPost	0.242** (0.106)	0.154* (0.089)	0.039 (0.066)
Observations	1005	1005	1539
R <sup>2</sup>	0.338	0.335	0.298
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* This table reports, in Panel A, regression coefficients from 3 separate CRE probit regressions. All regressions include controls for county characteristics (population, GDP per capita) and their year means for each county, weather variables (second-degree polynomials in annual precipitation, 10-degree bins based on daily maximum temperatures) and their year means for each county. We also include the number of yearly data entries of each county. Additional controls are listed in the table and include parents characteristics1, parents characteristics2, individual characteristics, and province by year of birth fixed effects. Panel B reports the marginal effects (in probability metric) of Post, in the TCZ (TCZ=1) and non-TCZ (TCZ=0) counties, following CRE probit estimations of each outcome variable. Estimates for TCZxPost, in Panel B, correspond to the pairwise comparisons of the average marginal effects. Panel C reports regression coefficients from 3 separate LPM regressions. These regressions further control for county fixed effects. Standard errors in parentheses are clustered at the county level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

tion), coefficients  $\lambda_1$  from Eq. (5) (*First step*) are negative and significant, suggesting that the TCZ policy may have reduced the county-level annual mean  $\text{SO}_2$  concentrations, relative to non-TCZ counties, in the post-TCZ period. The validity of these results relies on the assumption that trends in  $\text{SO}_2$  concentrations between TCZ and non-TCZ counties are similar in the years prior to the TCZ policy implementation, conditional on covariates. Figure A7 suggests that the two groups of counties may have had similar trends in  $\text{SO}_2$  concentrations before 1998, in the years closer to 1998 (Panel A).<sup>54</sup> The first set of coefficients, in Panel A of Table 4 (*Second step*), suggest that a  $1 \mu\text{g}/\text{m}^3$  decrease in the annual  $\text{SO}_2$  concentration, in the year of birth, increases the predicted probabilities of attending an academic high school and a higher quality high school. The estimated effects are significant at the conventional levels.

*Incidental parameter problem.*—Panel B of Table 4 reports results using the LPM to estimate the relationship between  $\text{SO}_2$  concentrations and educational outcomes (*Second step*). Estimates are compatible with an increase in the probabilities of attending an academic high school and a higher quality high school, following a decrease in  $\text{SO}_2$  concentrations. However, these effects are not precisely estimated.<sup>55</sup>

### 6.3 Treatment effect heterogeneity

Our main results show positive effects of exposure to the TCZ policy in one’s year of birth on children’s long-term outcomes, positively affecting their educational performance. However, this average effect may be hiding important differences in the effects of the TCZ policy across subgroups.<sup>56</sup>

First, based on the son-preference rooted culture in China, males may already initially receive better protection from exposure to air pollution and probably higher parental monetary investments. Biological arguments, on the other hand, suggest that female fetuses are

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<sup>54</sup>Coefficients for years 1992 and 1993 are significantly negative, indicating that the difference in  $\text{SO}_2$  concentrations, between TCZ and non-TCZ counties in our estimation sample, are smaller in years 1992 and 1993, compared to that of 1997.

<sup>55</sup>It is also possible to extend the CRE approach to produce the predicted  $\text{SO}_2$  concentrations following the TCZ implementation, by relying on the two-stage control function approach, as suggested by Papke and Wooldridge (2008) and Lin and Wooldridge (2019). We don’t present this approach as it was not possible, in our case, to produce bootstrapped standard errors, to correct for adding the first-step residuals in the second stage.

<sup>56</sup>Because of the lower sample size for each of the subgroups, and the resulting potential incidental parameter problem, only estimates from the CRE model are reported.



Table 4 – Effect of TCZ Implementation in the year of birth on long-term cognitive outcomes: effects through changes in air quality

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. Fixed effects model</i>			
<i>Second step</i>			
SO <sub>2</sub> concentration	-2.765** (1.159)	-2.955*** (0.373)	-0.765 (1.764)
Observations	648	499	844
<i>First step</i>			
TCZxPost	-0.265* (0.135)	-0.256** (0.129)	-0.256** (0.128)
Observations	1760	1760	1760
<i>Panel B. LPM</i>			
<i>Second step</i>			
SO <sub>2</sub> concentration	-1.434 (0.874)	-0.385 (0.503)	-0.148 (0.317)
Observations	685	685	1109
<i>First step</i>			
TCZxPost	-0.256** (0.128)	-0.256** (0.128)	-0.256** (0.128)
Observations	1760	1760	1760
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* This table reports coefficients from 6 separate regressions. Each regression estimates, in *Second step*, the effects of exposure, in the year of birth, to SO<sub>2</sub> concentrations, predicted using *First step*. SO<sub>2</sub> concentration is measured by the annual means. Regressions are performed using the CMP strategy where two different sample sizes are used for the estimations of Eq. (5) (*First step*) and Eq. (6) (*Second step*). Panel A reports results of Eq. (6) based on the fixed effects probit while Panel B reports results of Eq. (6) from the LPM. See notes to Table 1.

less physiologically sensitive to environmental shocks than male fetuses (Tanaka, 2015). To examine which of the two groups better benefited from the TCZ policy, we estimate our model for the subsamples of boys and girls. The DID estimates, measured in probability of outcomes, are reported in Table 5. Panel A highlights that boys are statistically significantly more likely to get additional beneficial effects from being exposed to the TCZ policy between conception and age 1, in terms of having better exam results, and thus, attending higher quality high schools. Moreover, even though early-life exposure to the TCZ policy is associated with a higher predicted probability to attend an academic high school rather than a specialized/technical one, for both boys and girls, most of the increase in this probability relates to girls. This indicates that environmental policies may contribute to reducing some of the gender gap in education.

We further examine heterogeneity in the treatment effect by parents' education. A higher parental education may reflect a higher knowledge and a higher socioeconomic status, allowing better protection against children's exposure to air pollution. However, children of low-educated parents may lack that type of protection, be more exposed to air pollution during pregnancy if their mothers are more likely to work in the outside and already suffer lower initial health endowments, making them more vulnerable to air pollution. This may suggest that the beneficial effects of the TCZ policy are concentrated among children of low-educated parents. However, if these parents learned to avoid outdoor pollution, thanks to their long-term exposure to it, or they were more negatively affected, in terms of their income, by the TCZ regulations, then effects may be lower for children of low-educated parents.<sup>57</sup> In panels B and C of Table 5, we report results from sub-samples divided by the father's and the mother's education attainments, respectively. We find that benefits associated with the TCZ policy significantly relate to children of low educated fathers, in terms of better exam results and, therefore, attending a higher quality high school. While, exposed children of high educated mother or father are more likely to get into an academic high school, instead of a specialized/technical one. In terms of magnitude, the benefits are larger for the relatively disadvantaged children, suggesting that the TCZ policy may have helped reducing some of the educational disparities between the two groups of children.

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<sup>57</sup>TCZ regulations resulted in the shutting down of multiple mines, thermal power plants and factories. If lower educated parents were more likely to work in these affected units, then they may be more negatively affected in terms of employment and earnings.

Table 5 – Heterogeneity in the TCZ effects across Subpopulations: differences in probability (CRE)

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. Boys and girls</i>			
Boys	0.195*** (0.048)	0.118*** (0.045)	0.190 (0.153)
Observations	502	459	733
Girls	-0.057 (0.173)	0.430** (0.218)	-0.096* (0.058)
Observations	452	416	478
<i>Panel B. High and low education of father</i>			
High father's education	0.075 (0.064)	0.233*** (0.034)	-0.044 (0.069)
Observations	536	486	581
Low father's education	0.437** (0.195)	0.050 (0.196)	0.041 (0.109)
Observations	429	386	727
<i>Panel C. High and low education of mother</i>			
High mother's education	0.096 (0.063)	0.206** (0.088)	-0.190 (0.154)
Observations	488	420	405
Low mother's education	0.144 (0.203)	-0.086 (0.212)	0.079 (0.126)
Observations	480	459	819
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* Each entry reports the difference between the marginal effects (in probability metric) in the TCZ and non-TCZ counties (following the CRE estimation for the considered subsample). High (low) father's and mother's education is defined as having a number of years of schooling that is above or equal (below) 9. See notes to Table 3.

## 6.4 Projected higher education and wage effects

Given the available data in CHIP13 and the birth cohorts that we consider, it is not possible to directly assess the long-term effects of the TCZ policy on labor market outcomes. However, a broad literature in economics shows that educational achievements are strong determinants of employment and earnings later in life (Chetty et al., 2011; Currie et al., 2012). It is plausible therefore, to think that improvements in education outcomes translate into better overall economic status, later in life. To check whether this is also true for the measures of educational performance employed in our analysis, we investigate their effects on higher education outcomes and earnings, using older cohorts from the same survey. We particularly focus on the following outcome variables:

*National College Entrance Exam score.* Every year, and in order to join a higher education institution in China, high school 12th grade students must take the National College Entrance Exam (NCEE, or gaokao). It is considered as a high stakes exam as it is the one and only determinant of post-secondary education admission. Students are to choose one of two important tracks, arts or science, in their first year of high school. The NCEE exam papers differ depending on the chosen track; however, mathematics, English and Chinese are three compulsory exams for both tracks. The full score for the NCEE is set at 750 points. Respondents of CHIP13 were asked to report their NCEE scores.

*Higher education institutions participation.* Post-secondary education institutions select students depending on their NCEE score being higher than a threshold set by these institutions.

*Earnings.* Respondents of CHIP13 were asked to report their annual total income (total wage income which may include various monetary subsidies or net business income) in 2013 in Yuan. When the income is earned by more than one person, survey participants divide the total sum by person.

Table 6 shows that attending a high quality high school is associated with an increase by about 46 points in the NCEE score, an increase in the probability of having a NCEE score above the 75%-tile of the score distribution by 22.5 percentage points, a decrease in the probability of having that score below the 25%-tile of the distribution by 19.2 percentage points, and an increase by 16.9 percentage points in the probability of attending college/university (column (3)). Given our results on the positive association between the TCZ policy and the

probability to attend a higher quality high school, results in Table 6 suggest that exposure to the TCZ policy, early in life, may have also positively affected higher education outcomes. Particularly, since we have estimated a 12.5 percentage points increase in the predicted probability to join a higher quality high school, following exposure to the TCZ policy, we can predict, plugging in these numbers, a TCZ gain of about 2.81 percentage points in the probability to get a NCEE score above the 75%-tile of the score distribution, about 2.4 percentage points in the probability to get a NCEE score below the 25%-tile of the distribution, and about 2.11 percentage points in the probability to attend college/university, for the exposed cohorts.<sup>58</sup> Table 7 also reveals positive significant effects of being in an academic high school and being in a high quality high school on future earnings. Additional controls are added in each column. We particularly control for the years of education in columns (3)-(5), as the high quality high school and the academic high school variables may be affecting earnings through their effect on education level. However, the coefficients on these two variables remain positive and significant, suggesting a direct effect as well. Attending a high quality high school increases the future earnings by about 29.43% (column (5)). While attending an academic high school, rather than a specialized/technical one, increases future earnings by about 18.77% (column (5)). These results suggest that the TCZ policy exposure, in early childhood, may have also positively affected earnings.

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<sup>58</sup>These calculations are made under two assumptions. First, we assume that the effect of high school quality on the different outcomes, for the 1998-1999 TCZ birth cohorts, is similar to that for the 1990-1994 birth cohorts from the whole sample. Second, the two effects, that of the TCZ policy on the probability to attend a higher quality high school and that of the high school quality on each of the outcomes, are assumed independent.

Table 6 – Effect of school quality on higher education outcomes, 1990-1994 birth cohorts

	(1)	(2)	(3)
A. NCEE score			
High quality high school	51.424*** (4.798)	48.049*** (4.896)	46.314*** (4.927)
Observations	1457	1457	1457
R <sup>2</sup>	0.073	0.404	0.411
B. NCEE score > 75%-tile			
High quality high school	0.667*** (0.084)	0.828*** (0.104)	0.805*** (0.105)
AME	0.217*** (0.025)	0.233*** (0.027)	0.225*** (0.027)
Observations	1186	1186	1186
R <sup>2</sup>	0.046	0.169	0.174
C. NCEE score < 25%-tile			
High quality high school	-0.490*** (0.083)	-0.707*** (0.106)	-0.683*** (0.108)
AME	-0.167*** (0.027)	-0.200*** (0.028)	-0.192*** (0.029)
Observations	1039	1039	1039
R <sup>2</sup>	0.028	0.184	0.188
D. Went college			
High quality high school	0.675*** (0.115)	0.854*** (0.157)	0.820*** (0.162)
AME	0.180*** (0.029)	0.181*** (0.031)	0.169*** (0.031)
Observations	654	654	654
R <sup>2</sup>	0.054	0.244	0.265
Individual characteristics	No	Yes	Yes
Parents characteristics	No	No	Yes
County fixed effects	No	Yes	Yes
Province fixed effects	No	Yes	Yes
Test year fixed effects	No	Yes	Yes

*Notes:* This table reports regression coefficients from 12 separate regressions. The sample includes all individuals born in 1990-1994 (aged 19-23 when outcomes are observed) and remains the same across all columns for each outcome. The outcome in panel A is the log of NCEE scores. Outcomes in panels B and C are dummy variables that equal 1 if the child had a NCEE score above the 75%-tile and below the 25%-tile of the score distribution, respectively, and 0 otherwise. The outcome in panel D is a dummy variable that equals 1 if the child attended college/university and 0 otherwise. AME= Average Marginal Effect. Additional controls are added in each column and include individual characteristics (gender, ethnicity, urban/rural survey indicator), parents characteristics (continuous measure of both mother's and father's education, mother's and father's age at birth), in addition to county, province and year of NCEE test fixed effects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 7 – Returns to education, 1979-1985 birth cohorts

	(1)	(2)	(3)	(4)	(5)
	log(earnings)				
High quality high school	0.213*** (0.056)	0.348*** (0.064)	0.269*** (0.067)	0.272*** (0.068)	0.258*** (0.067)
years of education			0.049*** (0.014)	0.050*** (0.015)	0.049*** (0.015)
Observations	701	701	701	701	701
R <sup>2</sup>	0.020	0.397	0.410	0.413	0.430
	log(earnings)				
Academic high school	0.199*** (0.057)	0.257*** (0.065)	0.181*** (0.067)	0.182*** (0.067)	0.172*** (0.066)
years of education			0.058*** (0.014)	0.058*** (0.015)	0.055*** (0.015)
Observations	701	701	701	701	701
R <sup>2</sup>	0.017	0.381	0.401	0.403	0.421
Individual characteristics	No	Yes	Yes	Yes	Yes
Parents characteristics	No	No	No	Yes	Yes
Job characteristics	No	No	No	No	Yes
County fixed effects	No	Yes	Yes	Yes	Yes
Province fixed effects	No	Yes	Yes	Yes	Yes

*Notes:* This table reports regression coefficients from 10 separate regressions. The sample includes all individuals born in 1979-1985 (aged 28-34 when outcomes are observed) and remains the same across all columns. The outcome variable is the log annual earnings in 2013. Additional controls are added in each column and include individual characteristics (gender, ethnicity, urban/rural survey indicator, age and age square in 2013), parents characteristics (continuous measure of both mother's and father's education, mother's and father's age at birth), job characteristics (self-employment indicator, industry, occupation), in addition to county and province fixed effects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 7 Robustness checks

In this section, we further explore the robustness of our results.

*Selective migration.*—First, we examine whether the perceived improvements in air quality and changes in the labor market opportunities, due to the TCZ implementation, changed the characteristics of the population in TCZ counties, resulting therefore in changes in the characteristics of the children born in these counties. This raises the concern that part of the positive effects on the long-run educational outcomes, we are getting, may be driven by these changes in characteristics, instead of a causal impact from the early-life exposure to the TCZ policy. We show that this may not be the case. First, unlike most of other countries, permanent internal migration is particularly a complicated process in China. The *hukou* system relates each Chinese citizen to its place of birth and restricts the change of the registered residence unless meeting certain requirements.<sup>59</sup> As a result, there are mainly two types of internal migrants in China, those who migrate repeatedly over short periods of time and without changing their *hukou*, and those who migrate permanently, succeeding to change theirs. Temporary migrants constitute the majority of migrants in the country. They usually leave their family behind in their place of origin, and this is even more likely when health care is needed, such as in the case of a pregnancy, since access to health services is related to one’s *hukou* place of residence.<sup>60</sup> Permanent migrants, however, generally move with other family members.<sup>61</sup>

Previous literature suggests that permanent migrants may have higher socioeconomic status (Hu et al., 2011). Our estimates may, therefore, be biased if their permanent migration was motivated by the potential benefits of the TCZ policy for their future children.<sup>62</sup> However, in Table 2, we show that the TCZ implementation did not induce differential changes in parental education or in GDP per capita, in the years after TCZ designation, in the newly

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<sup>59</sup>Examples of these requirements include marriage, getting a stable job or owning a permanent house in the destination.

<sup>60</sup>In addition to the *hukou* system, another important reason for temporary migration and for leaving other family members behind is the land tenure system applied in the country. Chinese rural households are denied private ownership of agricultural land and are only granted land-use rights through contracts, according to criteria such as the household’s labor force. Under these circumstances, the migration of some household members generates a risk of land expropriation.

<sup>61</sup>The annual number of permanent migrants in China is generally much smaller compared to the number of temporary migrants (see e.g., Hu et al., 2011).

<sup>62</sup>Chen et al. (2017b) showed that migration at the county level may react to increases in air pollution, and that well educated people at the beginning of their professional careers are driving this migration. However, this is not evidence that permanent migration may occur because of an environmental policy.



regulated counties. These results suggest little evidence for a bias in our estimates due to differential sorting based on observable characteristics. It would have been ideal if we had information on the permanent migration of families in our dataset; however, CHIP13 provides data only on whether survey participants changed their *hukou* from rural to urban. Only 0.74% of all children born between 1995 and 1999 had a parent that have made such a change. We also do not have information on the origin and destination areas of those who changed their *hukou*, making it difficult to check whether these movements are made from non-TCZ to TCZ locations. However, even though these migrations have low probability to be motivated by the TCZ policy, we reexamine, in a further step, our main results while dropping children whose parents have changed their *hukou* from rural to urban.<sup>63</sup> Main findings from the fixed effects, CRE and the linear probability models remain the same in terms of significance and almost the same in terms of magnitudes of the effects.<sup>64</sup>

*County of residence versus county of birth.*—In the previous robustness check, we addressed the issue that parents with particular characteristics may have migrated to TCZ counties, expecting a better air quality for their future unborn children, and hence biasing our estimates. In that case of parental migration, children are, therefore, born and living in the same county until high school outcomes are observed. However, another concern in our analysis is that we are only observing the county in which the child is living at the time of the survey. The child may have migrated after birth. If the current county of residence is different from the one in which they were born, this may constitute a potential source of measurement error for the treatment variable. However, as mentioned above, permanent migration with changes of the *hukou* registration is difficult in China, while temporary migration usually occurs while children are left behind. An estimated 69 million children were “left-behind” in rural areas of China in 2015 (UNICEF, 2018). This is not only because of the relatively high living costs in cities, but also because of the additional educational charges parents may suffer, as the children’s access to education is related to their *hukou* of birth. Therefore, unless there is a change in the *hukou* and children effectively changed counties, there is little probability that the county of birth and the county where the high school outcomes are measured, are

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<sup>63</sup>Migrations from rural to urban areas may be motivated by economic rather than environmental reasons, since rural areas are generally endowed with better air quality. Moreover, we only consider children born in 1998-1999 in our post-TCZ analysis, two years after the TCZ policy implementation, suggesting short terms for the changing of the *hukou* which may be a long process and must be supported by a legitimate reason such as employment.

<sup>64</sup>Tables are omitted for simplicity but are available upon request.

different. Moreover, CHIP13 asks participants to report whether the place of their current *hukou* registration is within or out of the county. 2% of the total sample of children born between 1995-1999 reported in a positive way. Even though this information does not necessarily imply that birth and high school counties are different, it may introduce a measurement error with respect to the county characteristics variables we are using. To address this issue, we reconduct our main analysis while dropping those who (and/or whose parents) changed their *hukou* from rural to urban and those whose current administrative *hukou* is out of the county where they are being surveyed. Main results from the fixed effects, CRE and the linear probability models remain the same in terms of significance and magnitudes of the effects.<sup>65</sup>

*Differences in trends.*—TCZ counties already exhibited a lower number of siblings in the pre-TCZ period (Appendix Table A1), however, in Table 2, we also found that the TCZ policy implementation is associated with a decreased number of siblings. In fact, because of the one child policy (1979-2015), the majority of parents in China could only have one child. However, this rule was less enforced in some areas, and had some exceptions (for example, minority women were allowed to have two or more children) (Zhang, 2017). If, after the TCZ policy, parents, that can have more than one child, became aware of the effects of air quality on their children’s outcomes (while air-pollution is suspected to have increased in the 2000s), or if they were more negatively affected, in terms of their income, then they may have opted for a lower number of children during the following years. This change in the fertility behavior may bias our results if the number of siblings significantly affects the educational performance of each child. However, in the 1996-1999 specification, the TCZ status balances the trends in the number of siblings (suggesting that the effect was probably very small), but is associated with a higher number of males. If parents opted for a lower number of children, and it resulted in selective pregnancies with only males, then our results may be biased if gender significantly affects the educational outcomes. For this reason, we replicate our main results without introducing the number of siblings and/or the indicator for gender as covariates. Results (for fixed effects models, CRE models and LPM) remain the same in terms of sign and significance.<sup>66</sup>

*Alternative clustering.*—Our main results are clustered at the county level, to account for

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<sup>65</sup>Tables are omitted for simplicity but are available upon request.

<sup>66</sup>Tables are omitted for simplicity but are available upon request.

the fact that the variation we use to estimate the effects of the TCZ policy is at the county-level. In this robustness check, we cluster the standard errors at a higher administrative level, the province level, to account for both autocorrelation and spatial correlation across counties within each province.<sup>67</sup> We find that accounting for dependence within provincial level increases our standard errors, but the significance levels for the different estimates remain the same, using the fixed effects and the CRE models. The effect on the academic high school variable, from the LPM, is, however, less precisely estimated, due to the much higher standard error.<sup>68</sup>

*Urban versus rural.*— In our baseline results, we use data from both the urban and rural CHIP13 household surveys. If urban areas are more likely to be designated as TCZ than rural areas, then a concern is that our results are being driven by simple comparisons between rural and urban administrative divisions. To ensure that this is not the case, we restrict our analysis to using only the rural sample.<sup>69</sup> Estimates, in Appendix Table A3, show slightly lower significance levels, compared to the main results, which can be explained by the higher standard errors, which may itself be due to the lower sample sizes. However, DID effects, for the probability to attend a higher quality high school (Panel B) or an academic high school (Panel A), remain significant, at the 10% level, suggesting that the effect we found is not produced by comparisons between urban and rural children.

*Alternative research design.*—The results presented until now are based on a difference-in-differences design that uses the overall available sample while controlling for different characteristics. However, a concern about a bias in our estimates may still arise if omitted heterogeneity that is related to the TCZ designation is driving the apparent difference in our outcomes between those born in TCZ counties and those born in non-TCZ counties, rather than the real effect of the treatment. To further test the robustness of our results, we use the propensity score matching method to restrict our sample to TCZ and non-TCZ counties that have similar characteristics. We use both mother’s and father’s years of education, weather variables and county characteristics variables as covariates to compute propensity

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<sup>67</sup>Note that each province can include counties with different TCZ designation: TCZ or non-TCZ.

<sup>68</sup>Tables are omitted for simplicity but are available upon request.

<sup>69</sup>It is also possible to address this issue by restricting the analysis to only the urban sample. However, the later is much smaller compared to the rural one for the different outcome variables. This may be due to the fact that the CHIP13 rural survey covered a higher number of individuals, around 39000 individuals from 11013 households while the urban survey covered only around 20000 individuals from 7175 urban households.

scores of being designated as TCZ.<sup>70</sup> In a second step, we investigate the long-term effects of TCZ implementation on children’s educational performance using only the matched sample. Appendix Table A4 reports results from our DID estimation of the TCZ effects using the matched sample. Estimates remain the same across the specifications, in terms of sign and significance, increasing the confidence in our empirical strategy. The DID estimates of the TCZ implementation effect, measured in probability of outcomes, are presented for the CRE model. They vary little compared to the main results. The average difference-in-differences in probability of attending a high quality high school is 14.3 percentage points, while it reaches 16.5 percentage points for the probability of attending an academic high school.<sup>71</sup>

*1995 as a placebo cut-off year*—The 1995 amendment of the APPCL was shown to have been weakly implemented, as priority was strongly given to the economic development at that time (Feng and Liao, 2016). Despite the documented very weak enforcement of 1995 APPCL, Appendix Figures A2 and A6 suggest a slight reduction in SO<sub>2</sub> concentrations in 1997, one year before the TCZ implementation. To check for potential positive effects of the 1995 APPCL, we repeat the main analysis using the sample of children born between 1992 and 1997, with 1995 being the placebo cut-off year. 1995-1997 cohorts represent, therefore, the “post”-policy observations in this specification. The estimates in Appendix Table A5 for the different outcome variables are non significant, confirming that the trends between the TCZ and non-TCZ counties are similar in the period before 1998. This result coupled with the documented more drastic actions taken to implement the TCZ policy regulations starting from 1998, and the effects of the TCZ policy on different outcomes shown to take place only after 1998 (see Section 2), support using 1998 as the cut-off year.

*Fewer observations in the post-TCZ years*.—The Compulsory Education Law of China indicates that children become eligible for starting primary school only if they have reached their 6th birthday before (or in) August 31. As a result, those born in the same year but after this cutoff date should wait until the following school year. In this case, we can suspect that among the 1998 cohort used in our sample, only those born between January and August would have started high school in 2013, while those born in September-December in the same year and those born in January-August of 1999 would have started high school in 2014,

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<sup>70</sup>The logistic regression with the nearest-neighbor matching within the caliper (1:1 pairing) is used to construct the propensity scores.

<sup>71</sup>Note that Stata does not account for the fact that propensity scores, used for the matching procedure, are estimated not the real ones.

and finally those born between September and December of 1999 would have entered high school in 2015. Therefore, given the survey year of CHIP13 (2014), we would be observing educational outcomes for the 1998 birth cohort but only for the children enrolled earlier in school (born January-August) in the 1999 cohort.

However, it has been shown that not all schools strictly abide by this age rule. As many parents prefer their children to start school as early as possible, they may, therefore, resort to different ways, such as paying extra fees or using personal relationships, to enroll their children ahead of schedule (Zhang et al., 2018b). The enforcement of the cutoff rule is, therefore, highly imperfect. Using the 2010 wave of the China Family Panel Study, Chen and Park (2021) showed that 42.19% of surveyed children started school earlier than expected, while Zhang et al. (2018b), using data from the 1992 Survey of the Living Situation of Chinese Children and a logistic regression, showed that those born in a month after the cutoff date were significantly more likely to start school earlier. Similarly, CHIP13 sample shows that some children, born between September and December of 1999, started primary school, and therefore high school, earlier than expected.<sup>72</sup>

Despite having educational data for children born in the different months of 1998 and 1999, the problem of potentially lower number of observations in the post-TCZ years persists because of the timing of the CHIP13 survey. A number of children, born in 1998 and 1999, that would have started high school in 2014 or in 2015 may have not reported their high school information because they did not have it yet at the time of the survey, in July and August of 2014.

To address this issue, we reexamine our results while combining our CHIP13 sample with a sample from a different dataset, the sixth wave survey of the China Household Income Project of the year 2018 (hereafter CHIP18).<sup>73,74</sup> Focusing on the same sample of children born in 1995-1999, the years just around the TCZ implementation year, we present the different estimates in Appendix Table A6. The results confirm that exposure to the TCZ policy

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<sup>72</sup>We inferred this from the fact that they reported high school information from 2014, while they should have started high school in 2015. However, we cannot check the overall amount of noncompliers in the CHIP13 as there are no information about the starting age of schooling.

<sup>73</sup>The reason for using CHIP18, which was conducted in 2019 to collect data mainly from 2018, as a robustness check, instead of in the main analysis, is their very recent availability to the public.

<sup>74</sup>As in the case of CHIP13, we use both rural and urban surveys. In this dataset, children born in 1998 and 1999 are aged 19 and 20 in 2018, therefore, all of them would have finished high school by that year, whatever their month of birth. Data from CHIP18 was also matched with weather and county characteristic data, following the same rules as done for CHIP13.

implementation, in one’s year of birth, is associated with obtaining better scores in the high school entrance exam, and therefore, a higher predicted probability of joining a higher quality high school (FE and linear probability models), and with a higher probability to attend an academic rather than a specialized/technical high school (all models).<sup>75</sup> Particularly, we find that the average difference-in-differences in probability of attending an academic high school are 11.6 and 9.3 percentage points, using the FE and the CRE models (slightly smaller than the 18.7 percentage points effect reported using only CHIP13). The LPM reports an effect of 9.4 percentage points for the higher quality high school (close to the 12.5 percentage points effect reported using only CHIP13). As previously shown, no effects are found for the qualification for high school outcome.<sup>76,77</sup>

*Post-secondary education outcomes.*—Finally, Section 6.4 shows that some of the post-secondary education outcomes may have been positively affected as a result of exposure to the TCZ policy during the year of birth, since these outcomes are positively related to the type of high school attended. In a further step, we use data from CHIP18 to directly test the effect of exposure to the TCZ implementation, in the year of birth, on different post-secondary education outcomes. We use the sample of children born in 1995-2000 from both rural and urban households.<sup>78</sup> In addition to the outcomes that appear in Table 6, we also consider an indicator for whether the child participated in the NCEE. This variable is different from the indicator for whether the child attended college/university. Both may reflect the willingness to get post-secondary education. However, the former distinguishes those who think they have a higher ability from those who think the opposite, since only those who think they can succeed would participate in the NCEE, while the later distinguishes those who effectively succeeded from those who did not.<sup>79</sup> Another outcome that we investigate

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<sup>75</sup>Despite the fact that with a higher sample size, the incidental parameter problem is almost absent, we still present results from the CRE and the linear probability models.

<sup>76</sup>We examine the pre-existing trends using the 1992-1999 sample for the three outcomes. Results suggest that the trends for the qualification for high school and joining an academic high school outcomes, before the TCZ implementation, between the TCZ and non-TCZ cohorts, are almost similar, supporting the identification assumption of Eq. (3). However, the parallel trend assumption for the high quality high school outcome is verified only when using the CRE model (not for the FE and the linear probability models). We also examine, the potential differences in trends, following the TCZ policy, between TCZ and non-TCZ counties, for the different observable characteristics. The TCZ designation seems to balance the trends in the more important determinants of educational outcomes. Figures and Tables are omitted here for simplicity but are available upon request.

<sup>77</sup>Main results remain the same using also the sample of children born in 1995-2000.

<sup>78</sup>In 2018, the year when the survey was conducted, these children are at least 18 years old, the age at which they are expected to finish high school, take the NCEE and start college/university.

<sup>79</sup>‘Ability’ refers to the ability to learn and to perform exams.

is whether the NCEE score is missing in the data despite having that information. Sun (2019) suggested that the missing NCEE score indicator carries information on a person’s ability. He assumes that individuals with a higher ability are more likely to report their NCEE score, more able to remember it or more willing to share it than people with a lower ability. Results are presented in Appendix Table A7. Most of the effects have the expected signs but are not precisely estimated. Results from the fixed effects model suggest that the likelihood of a missing NCEE score is lower for those exposed to the TCZ policy during their year of birth, suggesting that early-life exposure to the policy may be associated with an increased ability. In terms of differences in probability, we find that exposure, early in life, to the TCZ implementation increased the probability to participate in the NCEE by 6.9 percentage points.<sup>80</sup> Using the alternative CRE model, to account for any potential incidental parameter problems, we find that only estimates for the college/university outcome are significant. Particularly, the average difference-in-differences in probability of attending college/university is 7.1 percentage points.<sup>81</sup> The smaller estimated effects and the lower significance levels we get here, compared to the high school outcomes’ effects investigated in our main analysis, are presumably due to the positive selection of high school students. In other words, since high school is not compulsory, only students that think they would succeed will enter high school. Moreover, according to the literature analyzing human development, the effect of an early investment in disadvantaged children is reduced if not followed up by later investments (Cunha and Heckman, 2007). Hence, the smaller estimated effects may be due to a lessened effect, of the early life exposure to the TCZ policy, if later air quality has worsened.

Overall, all estimates remain similar across the different robustness checks, suggesting that the main results are not driven by inappropriate identification assumptions.

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<sup>80</sup>We examined the pre-existing trends using the 1992-2000 sample for the different outcomes. the FE model results suggest that the trends before the TCZ implementation, between the TCZ and non-TCZ cohorts, for all outcomes, but the indicator about whether the NCEE score is above the 75%-tile of the score distribution, are almost the same. Moreover, we find that trends, between TCZ and non-TCZ counties, in most of the observable characteristics, that may be determinants of the educational outcomes, are similar (using the 1992-2000 and 1996-2000 samples). Figures and Tables are omitted here for simplicity but are available upon request.

<sup>81</sup>This result is to be taken with caution. The parallel trend assumption for the outcome of whether the child attended college/university does not seem to be verified when using the CRE model.

## 8 Conclusion

In this paper, we provide the first empirical evidence, in the context of a developing country, of the persistent benefits of early-life exposure to an environmental policy, in terms of educational performance. In doing so, we consider one of the largest-scale air pollution policy experiments ever implemented in a developing country, the TCZ policy. We compare cohorts born in TCZ counties, subjected to particularly stringent environmental directives, just before and after the TCZ implementation, to the same difference among cohorts born in non-TCZ counties.

Considering the cohorts in our sample counties, the results are consistent with an important relationship between environmental regulation exposure, in utero and before age 1, and educational performance, 15 years later. Particularly, we find that the exposed children had an increased predicted probability to attend a better quality high school, by 12.5 percentage points, which can be explained by a higher ability to obtain better scores in a standardized high school entrance exam. They are also found to be more likely to join an academic, rather than a specialized/technical high school which focuses on manual labor training, by 18.7 percentage points.

Projecting forward, we show that our high school educational performance measures have effects on later educational attainment and earnings, suggesting even more benefits of the TCZ policy. Given these results, the beneficial effects of the TCZ policy may be economically significant, not only at the individual level, but also at the country's level. The TCZ area was home to about 504 million people in 1998, and the average annual number of new born children in 1998-1999 in TCZ locations was around 7 million children.<sup>82</sup> This means that effects of the TCZ policy may include providing a larger proportion of higher skilled labor force for the country, further boosting its economic development, when these cohorts reach prime working age. For example, if we assume a 1% increase in productivity and an annual income at US\$14,500 (around 100,000 Yuan), this transforms, into around one billion U.S. dollars ( $14,500 \times 7M \times 1\%$ ) in increased productivity annually, for each of the 1998 and 1999 exposed cohorts. In order to keep and increase a country's competitiveness,

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<sup>82</sup>The TCZ area accounted for 40.6% of China's population, which reached 1.242 milliard in 1998 and 1.253 milliard in 1999. Numbers of new births are from The World Bank's Health Nutrition and Population Statistics website, available at <https://databank.worldbank.org/source/health-nutrition-and-population-statistics>.



environmental regulations are, therefore, at least as important as, other government human capital investments.

Moreover, we find that the beneficial effects of exposure to the environmental policy, since conception and before age 1, are reinforced among cohorts born to low-educated fathers and among girls. These results suggest that environmental policies enhance educational performance while also contributing to a more positive distributional effect on education. If the increased cognitive attainment is translated into wider labor market opportunities in later years, then the TCZ policy may help reducing intergenerational transmission of inequality.

Our results are of direct importance for policy makers. If, in assessing environmental regulations, considered benefits are limited to health-related outcomes (diseases, hospitalizations, mortality), then our results suggest that such assessments are underestimating the gains from these regulations. Taking into account the additional persistent benefits on cognitive outcomes is of high importance in motivating the implementation of environmental regulations in developing countries.

Finally, our analysis has been challenged by multiple data limitations, hence the need to further examine the long-term effects of the TCZ policy on educational and economic outcomes, using larger-scale and more recent datasets. Moreover, little could have been done, in this paper, to understand the mechanisms behind these associations (only a suggestive evidence that the TCZ policy effects act through improvements in  $\text{SO}_2$  concentrations was presented). Previous studies provided evidence for the positive effects on health outcomes, such as birth weight (Tanaka, 2015), however, health provides only a partial explanation. A deeper understanding of other potential channels, including the effects of the environmental regulations on parental income, which can be either positive or negative, and how that may affect parental investments in their children, is of great importance to maximize the beneficial effects of these environmental policies. Furthermore, even though we find results suggesting no differential benefits from exposure to the TCZ implementation at ages between 1 and 6, that does not imply the absence of positive effects. Future research may be needed to further examine the beneficial effects at these slightly older ages, which may enlarge the benefits of early-life exposure to the TCZ policy, beyond the ones examined in this paper.

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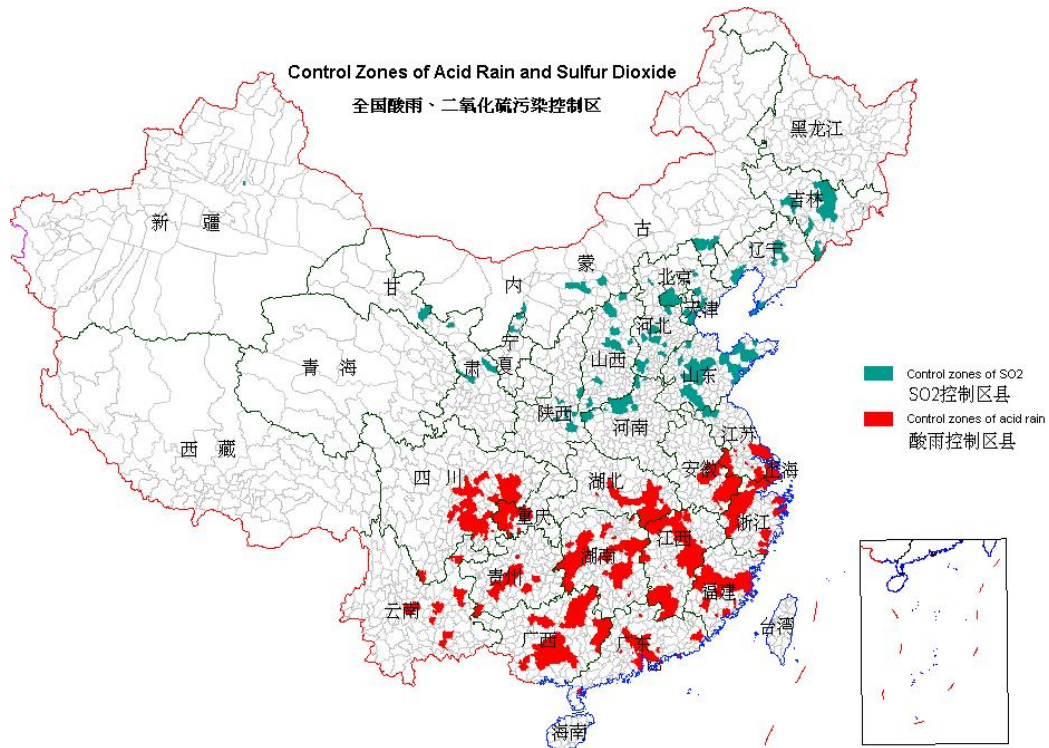
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# Appendices

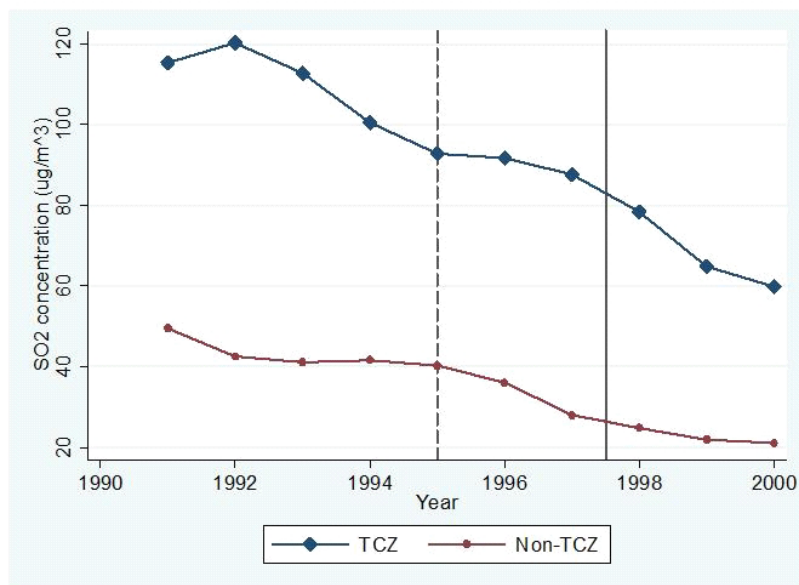
## A Appendix Figures and Tables

Figure A1 – The Two Control Zones Locations



Source: China Atlas of Population and Environment (1990-1999)  
(All China Data Center)

Figure A2 – Trends in SO<sub>2</sub> concentrations by TCZ status (Tanaka, 2010)

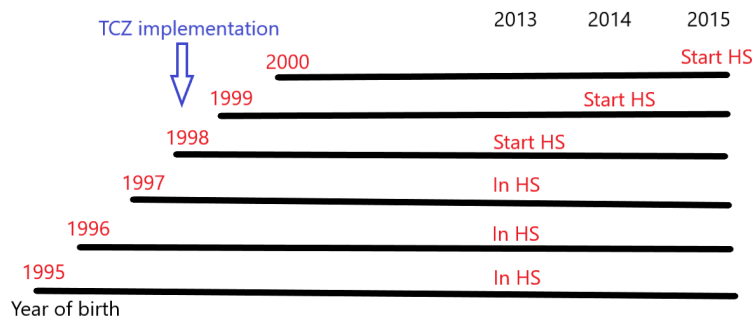


Source: Tanaka (2010).

Notes: This figure plots the general trend of SO<sub>2</sub> concentrations ( $\mu\text{g}/\text{m}^3$ ) between TCZ and non-TCZ localities. “The annual mean is calculated using the total population as the weight. The dotted vertical line indicates the timing of 1995 APPCL amendment, and the solid vertical line indicates the timing of TCZ policy implementation in January 1998” (Tanaka, 2010).



Figure A3 – Year of joining high school for each birth cohort



*Notes:* This figure plots for each birth cohort the year at which they should have joined high school. HS= High School.

Table A1 – Difference in characteristics by TCZ status before 1998

	(1)
	<b>1992-1997</b>
Male	-0.0387 (0.0358)
Han	0.236 (0.232)
Number of siblings	-0.218* (0.118)
Urban <i>hukou</i>	0.395*** (0.101)
Father's years of education	1.026*** (0.216)
Mother's years of education	1.474*** (0.287)
Mother's age at birth	0.0265 (0.209)
Father's age at birth	0.354 (0.258)
Total precipitation	311.6*** (82.80)
Average temperature	2.775*** (0.613)
Population (10000 persons)	0.0176 (0.123)
GDP per capita	0.636*** (0.156)

*Notes:* Each entry in this Table is the coefficient on the TCZ variable from a separate regression of each of the respective variables on the TCZ indicator (=1 if county is designated as TCZ) and year of birth fixed effects. Regressions are estimated using individual micro-data for the years before the TCZ implementation (1992-1997). Note that outliers created due to the linear interpolation of GDP per capita and Population (10000 persons) variables are dropped when running the regressions for these variables. Standard errors in parentheses are clustered by county. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

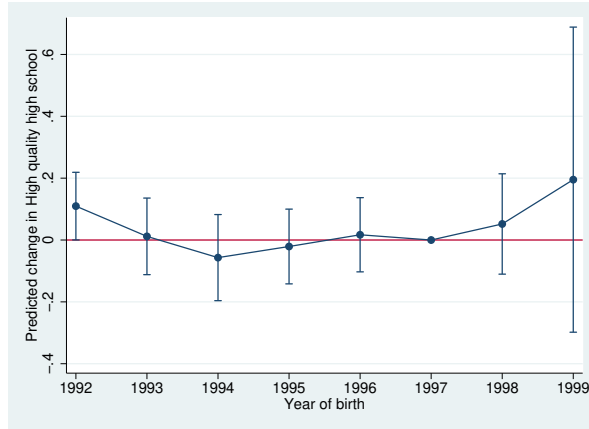
Table A2 – Effect of TCZ implementation in the year of birth on long-term educational outcomes

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
TCZxPost	1.012*** (0.361)	1.299** (0.531)	0.206 (0.417)
Male	-0.114 (0.126)	-0.063 (0.159)	-0.457*** (0.118)
Han	0.486 (0.333)	0.050 (0.367)	-0.263 (0.239)
Number of siblings	0.082 (0.099)	-0.011 (0.100)	-0.185*** (0.070)
Urban hukou	0.281 (0.203)	0.014 (0.218)	0.607** (0.266)
Father's years of education	0.111* (0.065)	-0.001 (0.090)	0.063 (0.062)
Mother's years of education	-0.105 (0.072)	-0.022 (0.106)	0.028 (0.067)
Mother's age at birth	0.038 (0.026)	0.058* (0.034)	0.037 (0.023)
Father's age at birth	-0.021 (0.028)	-0.059* (0.032)	-0.011 (0.020)
Never schooled (Father)	0.444 (0.994)	-1.649 (1.487)	-9.174*** (1.156)
Elementary school (Father)	0.296 (0.605)	-1.605 (0.978)	-8.741*** (0.791)
Middle school (Father)	0.065 (0.415)	-1.564** (0.741)	-8.742*** (0.631)
Secondary school (Father)	-0.059 (0.270)	-0.751 (0.592)	-8.580*** (0.555)
University (Father)	<i>ref</i>	<i>ref</i>	<i>ref</i>
Never schooled (Mother)	-2.535** (1.038)	0.002 (1.538)	-1.022 (0.980)
Elementary school (Mother)	-2.233*** (0.714)	-0.868 (1.020)	-1.216* (0.723)
Middle school (Mother)	-1.811*** (0.501)	-0.809 (0.786)	-0.720 (0.572)
Secondary school (Mother)	-0.765** (0.319)	-0.166 (0.582)	-0.422 (0.459)
University (Mother)	<i>ref</i>	<i>ref</i>	<i>ref</i>
Total precipitation	0.000 (0.001)	0.003 (0.002)	-0.001* (0.001)
Total precipitation squared	-0.000 (0.000)	-0.000* (0.000)	0.000* (0.000)
Nbr days with 33<max temp<39	0.018 (0.044)	0.023 (0.069)	0.030 (0.036)
Nbr days with 40<max temp<49	0.008 (0.036)	-0.170*** (0.052)	-0.006 (0.042)
Nbr days with 50<max temp<59	-0.002 (0.037)	0.062 (0.063)	0.069* (0.035)
Nbr days with 60<max temp<69	0.069** (0.031)	0.116** (0.049)	-0.018 (0.028)
Nbr days with 70<max temp<79	0.042 (0.034)	0.140** (0.059)	0.010 (0.045)
Nbr days with 80<max temp<89	-0.023 (0.032)	-0.015 (0.044)	-0.019 (0.038)
Nbr days with 90<max temp<99	0.023 (0.036)	0.087* (0.044)	0.032 (0.041)
Nbr days with max temp>100	-0.001 (0.015)	0.030 (0.033)	-0.009 (0.024)
Population (10000 persons)	1.460* (0.839)	-1.937 (1.340)	0.454 (0.881)
GDP per capita	0.053 (0.344)	-0.359 (0.809)	-0.115 (0.364)
County fixed effects	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes
Observations	939	709	1126
Pseudo R <sup>2</sup>	0.260	0.282	0.257

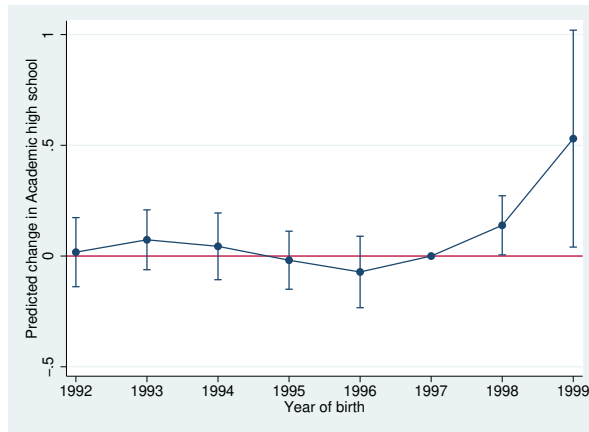
Notes: See notes to Table 1.

Figure A4 – Effect of TCZ implementation on long-run education outcomes by year of birth (CRE)

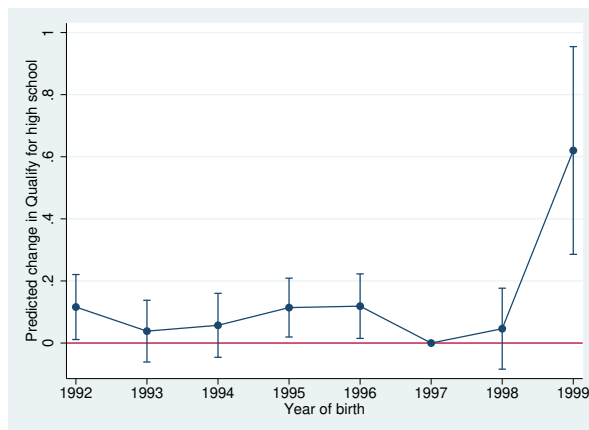
(a) High quality high school



(b) Academic high school



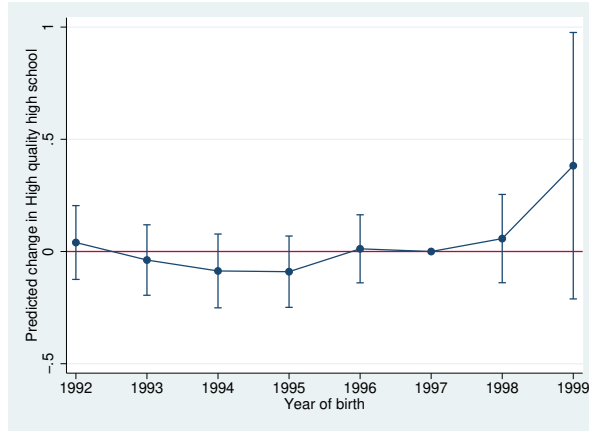
(c) Qualify for high school



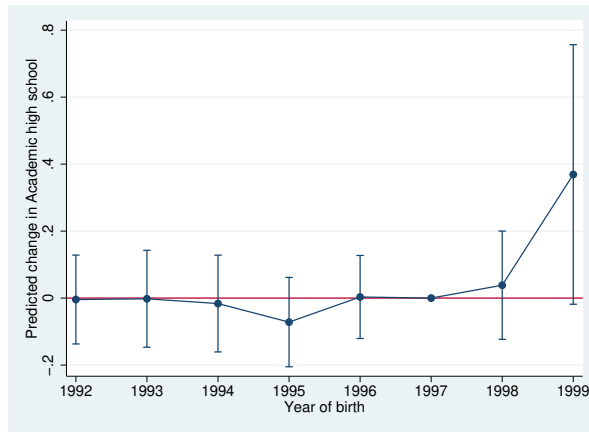
Notes: See notes to Figure 1.

Figure A5 – Effect of TCZ implementation on long-run education outcomes by year of birth (LPM)

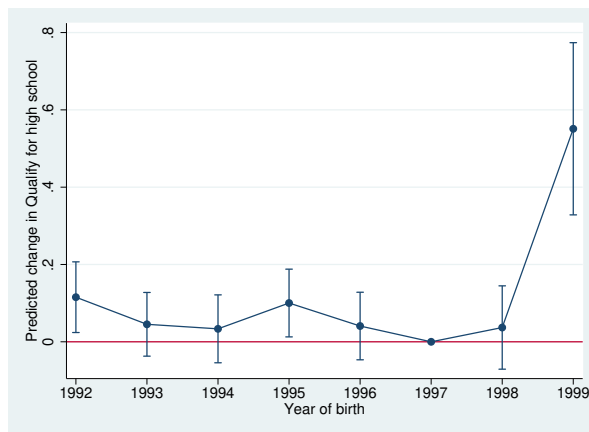
(a) High quality high school



(b) Academic high school



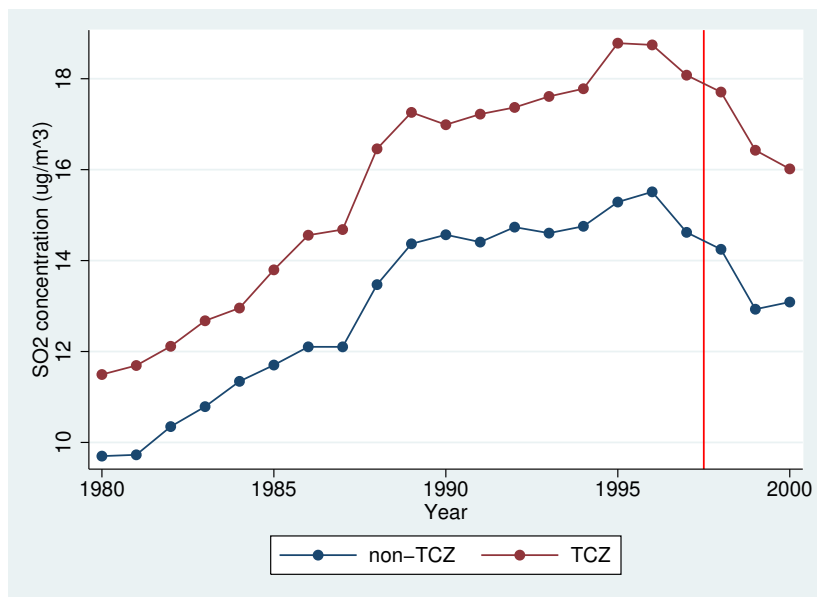
(c) Qualify for high school



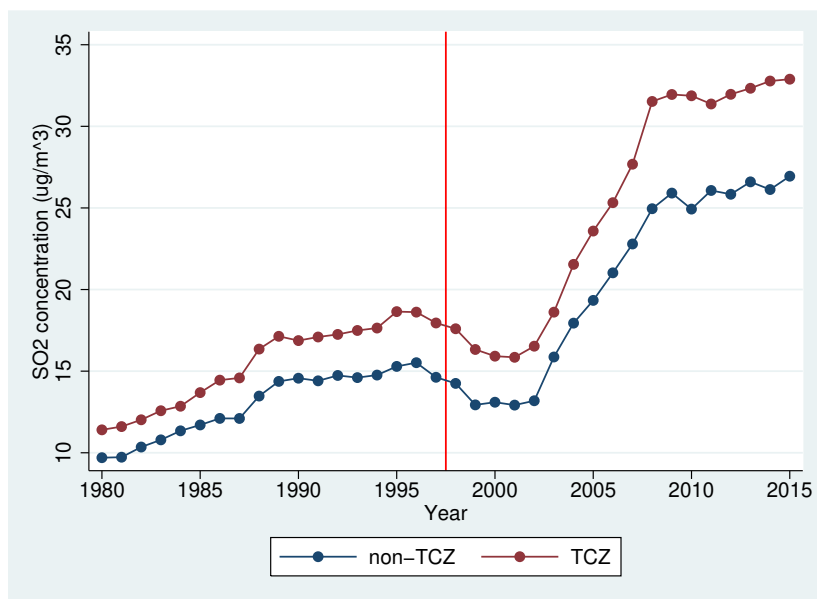
Notes: See notes to Figure 1.

Figure A6 – Trends in SO<sub>2</sub> concentrations by TCZ status (CHIP13)

(a) Panel A



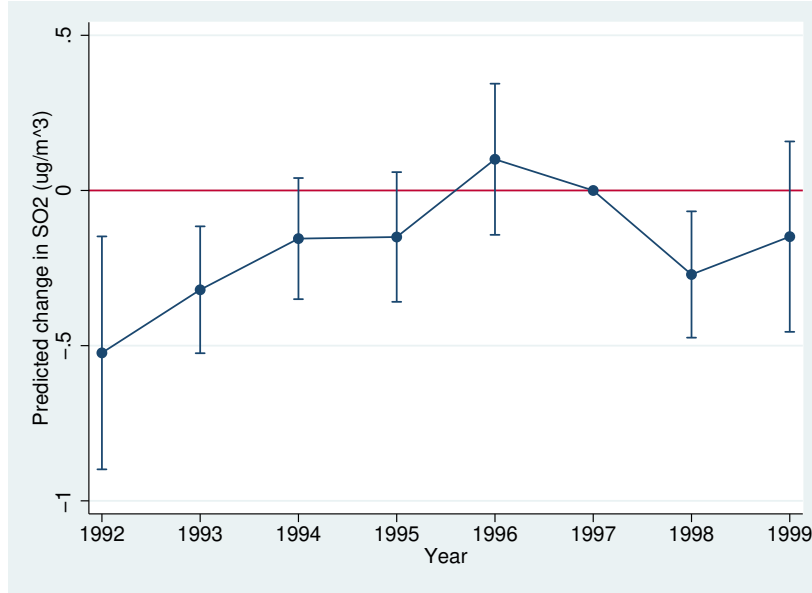
(b) Panel B



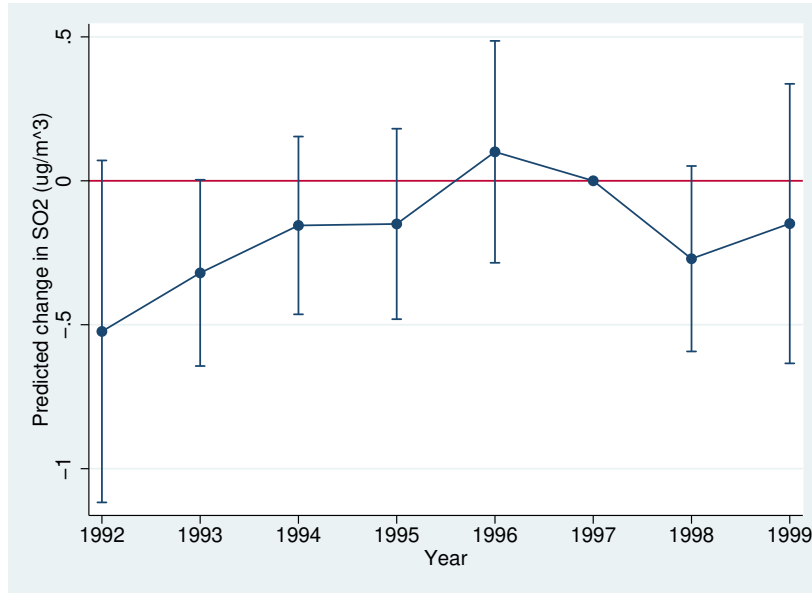
Notes: These figures plot the trends of SO<sub>2</sub> ( $\mu\text{g}/\text{m}^3$ ) between TCZ and non-TCZ counties. The annual means are calculated using all counties included in the CHIP13 urban and rural surveys and that have available pollution data at the county level. Panel A covers variations in trends between 1980 and 2000, while panel B covers that between 1980 and 2015. The red vertical line indicates the timing of TCZ policy implementation in January 1998, located between the annual average values of 1997 and 1998.

Figure A7 – Effect of TCZ implementation on county SO<sub>2</sub> concentration by year

(a) Panel A



(b) Panel B



Notes: Each panel of this figure presents regression coefficients from the distributed lag model in Eq. (4), where the dependent variable used is  $SO_{2ct}$ , the annual SO<sub>2</sub> concentration by county. The sample used covers all counties used to estimate Eq. (5), and that have available pollution data at the county level only. We plot the coefficients on the interactions between birth year indicators and the indicator for TCZ status, and their associated 90% (Panel A) or 99% (Panel B) Confidence Interval. The coefficient on the interaction between TCZ and birth year 1997 is normalized to zero. Standard errors are clustered by county.

Table A3 – TCZ effects in rural areas

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. CRE model</i>			
TCZxPost	0.408 (0.612)	0.896* (0.506)	0.285 (0.379)
Observations	615	563	1004
Pseudo R <sup>2</sup>	0.173	0.243	0.186
 <i>Panel B. LPM</i>			
TCZxPost	0.390* (0.206)	0.193 (0.172)	0.102 (0.120)
Observations	638	638	1084
R <sup>2</sup>	0.376	0.419	0.311
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* Each entry reports the DID estimate, using CRE (Panel A) and LPM (Panel B) regressions. County fixed effects are included in the LPM estimations. See notes to Table 3 and Table 1.



Table A4 – Effect of exposure to air pollution in the year of birth on long-term cognitive outcomes: PSM-DID method

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. Fixed effects model</i>			
TCZxPost	2.215*** (0.674)	3.167*** (0.845)	0.337 (0.533)
Observations	511	322	611
Pseudo R <sup>2</sup>	0.288	0.337	0.271
<i>Panel B. CRE model</i>			
TCZxPost	0.622 (0.398)	1.019** (0.476)	0.597 (0.392)
Observations	583	522	829
Pseudo R <sup>2</sup>	0.190	0.309	0.259
TCZxPost (differences in probability)	0.143*** (0.049)	0.165*** (0.051)	0.065 (0.113)
<i>Panel C. LPM</i>			
TCZxPost	0.362** (0.150)	0.277** (0.115)	0.055 (0.080)
Observations	620	620	1012
R <sup>2</sup>	0.425	0.469	0.347
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* This table reports regression coefficients from 9 separate regressions. County fixed effects are used in estimations of Panel A and Panel C. TCZxPost reports the DID coefficients from the estimations, and TCZxPost (differences in probability) reports the average DID in probability of outcomes (pairwise comparisons of the average marginal effects in TCZ and non-TCZ counties). See notes to Table 3 and Table 1.

Table A5 – TCZ effects with 1995 cut-off year (1992-1997 cohorts)

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. Fixed effects model</i>			
TCZxPost	-0.063 (0.172)	-0.139 (0.237)	0.098 (0.144)
Observations	1913	1670	2564
Pseudo R <sup>2</sup>	0.209	0.213	0.244
<i>Panel B. CRE model</i>			
TCZxPost	-0.027 (0.150)	-0.261 (0.194)	0.052 (0.117)
Observations	1924	1917	2783
Pseudo R <sup>2</sup>	0.136	0.169	0.223
<i>Panel C. LPM</i>			
TCZxPost	-0.013 (0.057)	-0.029 (0.050)	-0.017 (0.030)
Observations	1928	1928	2938
R <sup>2</sup>	0.256	0.236	0.278
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* Each entry reports an estimate of the TCZxPost variable, which is an indicator that equals one for counties designated as TCZ interacted with an indicator that equals one for birth years 1995-1997, using FE probit (Panel A), CRE probit (Panel B) and LPM (Panel C) regressions. County fixed effects are included in the FE probit and LPM estimations. The sample includes all observations for the years 1992-1997.

Table A6 – Effect of TCZ implementation in the year of birth on long-term outcomes - CHIP13 &amp; CHIP18

	(1)	(2)	(3)
	High quality high school	Academic high school	Qualify for high school
<i>Panel A. Fixed effects model</i>			
TCZxPost	0.290** (0.134)	0.321** (0.161)	0.053 (0.166)
Observations	2731	2492	3447
Pseudo R <sup>2</sup>	0.192	0.169	0.224
TCZxPost (differences in probability)	0.059 (0.041)	0.116*** (0.042)	0.031 (0.044)
Confidence Interval (95%)	[-0.021, 0.138]	[0.034, 0.199]	[-0.056, 0.118]
<i>Panel B. CRE model</i>			
TCZxPost	0.120 (0.128)	0.253* (0.143)	0.071 (0.133)
Observations	2793	2754	3971
Pseudo R <sup>2</sup>	0.118	0.116	0.216
TCZxPost (differences in probability)	0.043 (0.039)	0.093*** (0.031)	0.019 (0.032)
Confidence Interval (95%)	[-0.034, 0.120]	[0.033, 0.153]	[-0.044, 0.082]
<i>Panel C. LPM</i>			
TCZxPost	0.094** (0.044)	0.067* (0.036)	-0.002 (0.028)
Observations	2812	2812	4103
R <sup>2</sup>	0.254	0.200	0.232
Child characteristics	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes

*Notes:* This table reports regression coefficients from 9 separate regressions, and the corresponding marginal effects. Individual-level micro data is used for these regressions. The sample includes all observations for the years 1995-1999 from CHIP13 and CHIP18. A control for the year of survey was included in the different regressions. See notes to Table 1 for details about the variables. Standard errors in parentheses are clustered at the county level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A7 – Effect of TCZ implementation in the year of birth on post-secondary outcomes - CHIP18

	(1)	(2)	(3)	(4)	(5)	(6)
	Take NCEE	Missing NCEE score	NCEE score>75%-tile	NCEE score<25%-tile	Went college	NCEE score
<i>Panel A. Fixed effects model</i>						
TCZxPost	0.200 (0.155)	-1.160** (0.552)	0.112 (0.224)	0.137 (0.311)	0.266 (0.172)	1.141 (1.475)
Observations	2462	344	1300	1087	2458	1581
(Pseudo) R <sup>2</sup>	0.250	0.262	0.213	0.249	0.303	0.372
TCZxPost (differences in probability)	0.069* (0.039)	-0.137 (0.124)	0.000 (0.071)	0.030 (0.075)	0.073 (0.045)	
Confidence Interval (95%)	[-0.008, 0.146]	[-0.380, 0.106]	[-0.139, 0.140]	[-0.117, 0.178]	[-0.015, 0.161]	
<i>Panel B. CRE model</i>						
TCZxPost	0.198 (0.130)	-0.226 (0.303)	0.127 (0.181)	0.036 (0.229)	0.296** (0.136)	
Observations	2636	1000	1506	1377	2635	
Pseudo R <sup>2</sup>	0.206	0.255	0.146	0.231	0.261	
TCZxPost (differences in probability)	0.052 (0.037)	-0.028 (0.038)	0.036 (0.050)	0.016 (0.058)	0.071* (0.037)	
Confidence Interval (95%)	[-0.020, 0.125]	[-0.103, 0.046]	[-0.061, 0.133]	[-0.097, 0.130]	[-0.002, 0.144]	
<i>Panel C. LPM</i>						
TCZxPost	0.043 (0.042)	-0.042 (0.029)	0.004 (0.061)	0.004 (0.058)	0.050 (0.040)	
Observations	2636	1673	1581	1581	2662	
R <sup>2</sup>	0.307	0.327	0.291	0.363	0.355	
Child characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Parents characteristics1	Yes	Yes	Yes	Yes	Yes	Yes
Parents characteristics2	Yes	Yes	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
County characteristics	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
ProvinceXyears fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* This table reports regression coefficients from 15 separate regressions estimated with three different models as indicated in each panel (columns (1)-(5)). Column (6) is estimated using OLS. Individual-level micro data is used for these regressions. The sample includes all observations for the years 1995-2000. Take NCEE (column (1)) is a dummy variable that equals 1 if the child decided to participate in the NCEE and 0 otherwise, Missing NCEE score (column (2)) is a dummy variable that equals 1 if the child did not report his/her NCEE score despite taking the exam and 0 otherwise. The outcome in column (6) is the standardized NCEE score of each child. See notes to Table 6 for the definition of outcomes in columns (3)-(5). Standard errors in parentheses are clustered at the county level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## B Data Appendix

### B.1 Counties' TCZ Designation

We respect the following rules in identifying TCZ treatment and control counties:

- We consider the name, area and divisional status (either district, county-level city, or county) as of 1998 for each location in CHIP13.
- Third level administrative locations explicitly listed as TCZ in the “Official Reply”, or which belong to prefectures that are listed as TCZ, are assigned the TCZ status.
- All other third level administrative locations are assigned the non-TCZ status.

### B.2 Pollution Data

Pollution data is extracted using a polygon shapefile with the third-level administrative divisions of China as of 2015 (Hijmans, R. and University of California, Berkeley, Museum of Vertebrate Zoology, 2015). We consider the following rules in treating the pollution data:

- Changes in names and divisional status are considered, in order to get the right data for the right years.
- When a location is merged into another one, we use data on the newly merged area. One exception is the data used for Qijiang district which is the merged area of Qijiang county and Wansheng district in 2011. We use pollution data for Qijiang County as it is the only available data.
- when a location is divided into two different locations, I consider the new location. As we are interested in the concentration of air pollutants, smaller areas allows to have a better measure of these concentrations. However, due to data availability problem, the data for the area before division is used in some cases.

### B.3 Income and Demographic Data

For the county level socioeconomic data, we consider the following rules:

- Changes in names and divisional status are considered, in order to get the right data for the right years.
- When a location is merged into another one, the sum of the information from the two locations is used, even in years of birth when the 2 locations are still independent. This is because we cannot, from the CHIP13 data, distinguish who is from which of the two locations (before the merge). In cases where we are unable to get data for one of the two locations, we use data for only one location considering individuals born in the second location as originally born in the first one.
- When a location is divided into two different locations, we use data from the original one location before division if division happened after the year of birth of the individuals

(which is the case for the whole sample, all divisions in our sample happened after 1999).

- Some counties have incomplete data on GDP per capita and total population, requiring us to interpolate the missing data.
- Because of data availability, Gross Regional Product per capita was used for counties and districts of Jiangsu province and for some districts of Yunnan province.
- We deflate GDP data by province-year CPI, taking Beijing-1998 as the base province-year. For the few missing values of provincial CPI, we use China's national CPI (1998, being the base year).
- Socioeconomic data for Beijing municipality at the third level administrative division are not available. Therefore, we allocate to each district from Beijing the same municipality level socioeconomic data.

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