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Mother's education and child development: Evidence from the compulsory school reform in China☆

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ABSTRACT

This paper investigates the causal impact of mother's schooling on various outcomes of adolescent development by exploiting the temporal and geographical variations in the enforcement of compulsory schooling laws in China. Using data from China Family Panel Studies, we find that mother's education increases adolescents' school enrollment, math test scores, college aspiration, and internal locus of control related to education. Mother's education also improves adolescent mental health status and reduces the incidence of underweight. We also find considerable gender heterogeneity in the effects of mother's education. The results further indicate that mother's education leads to an increase in family resources for children and an improvement in maternal mental health and parenting, which we interpret as potential mechanisms behind our findings.

1. Introduction

It has been widely documented that human capital formation during childhood is closely associated with later-life outcomes.[1] To promote children's well-being and enhance equality of opportunity, many developed countries have implemented various policies targeting children, especially disadvantaged children (Currie and Aizer, 2016). To design effective policies, understanding the origins and development of capabilities is crucial (Conti and Heckman, 2014). In this paper, we provide additional evidence on the role of maternal schooling in the process of capability formation. Specifically, we investigate the causal impact of mother's schooling on a wide range of offspring outcomes in adolescence and explore the underlying mechanisms in the context of a developing country.[2]

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[1] Childhood characteristics have been found to be a strong predictor of adult health, educational attainment, employment, wages, criminal behavior, and life satisfaction (Currie and Thomas, 1999; Case et al., 2005; Heckman et al., 2006; Fletcher, 2013; Frijters et al., 2014; Anderson et al., 2015).

[2] According to the World Health Organization (WHO), an adolescent refers to an individual between the ages of 10 and 19 years. Adolescence is a transitional period between childhood and adulthood and is one of the most rapid phases of human development.

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The prominent role of parents' schooling, especially mother's schooling, in the process of child cognitive and noncognitive development has been emphasized in the literature (Grossman, 2000). Mothers' education can influence their children's outcomes through nature (genes), nurture (environment), and particularly, interactions between the two. For instance, the intergenerational associations of human capital may be largely driven by inherited genetic factors (Wolfe and Behrman, 1987; Rowe, 1994; Behrman and Rosenzweig, 2002). Regarding nurture, mothers with more education may create more nurturing and healthier home environments for child development, such as having more economic inputs, better parenting behavior, greater information processing capacity, and higher efficiency in human capital investment (Kornrich and Furstenberg, 2013; Guryan et al., 2008; Kalil et al., 2012).

Economists are particularly interested in the nurturing effect (i.e., the causal effect) of mother's education from a policy perspective. Promoting gender equality in education is a priority area of development policy for developing countries, including China. Public policies targeting investments in women's education improve the education of one generation in the short run, and have long-run societal benefits by improving child outcomes. Notably, a causal link from the mother's education to child outcomes is difficult to demonstrate. The association between mother's education and child outcomes might be driven by unobserved maternal characteristics, for example, inherited abilities.

A number of studies have investigated reforms in compulsory schooling laws to identify the causal impacts of mother's education on child outcomes in developed countries. The findings have been mixed. For example, Holmlund et al. (2011) find that the intergenerational effects of schooling are significant but small in Sweden. Black et al. (2005) find that the effect of mother's education on children's education is only significant for sons but insignificant for daughters in Norway. Consistent with Black et al. (2005), Lundborg et al. (2014) also find positive effects of mother's education on sons' cognitive and noncognitive skills and health status in Sweden. Focusing on children aged 7–15, Oreopoulos et al. (2006) find that parental education has reduced the incidence of grade repetition in the United States.[3] However, Lindeboom et al. (2009) find little evidence that mother's education has a causal effect on the health of infants and children aged 7, 11, and 16 in the United Kingdom.

In addition to exploiting compulsory schooling reforms, Currie and Moretti (2003) use college openings in the United States and find a positive effect of maternal education on infant health. Carneiro et al. (2013) exploit geographic variation in schooling costs during the mother's adolescence and show that maternal education has improved children's test scores, reduced the incidence of behavioral problems and grade repetition, but had no effect on obesity at ages 7–8 and 12–14.[4] However, McCrary and Royer (2011) find weak evidence of a causal relationship between mother's education and infant health in the United States, using school entry policies as an instrument for education. In summary, despite the growing attention to mother's education, the literature still falls short of an adequate understanding of the causal mechanisms, especially for developing countries.

Compared with developed countries, developing countries generally provide less social protection for children, in terms of nutrition, health, education, and other child-related services. Thus, mothers may play a more important role in the process of child development. Only a few of studies have been conducted to identify the causal effect of maternal education in low-income countries. They find that increased maternal education has increased early child health investments in Uganda (Keats, 2018), improved child test scores in Pakistan (Andrabi et al., 2012), reduced child mortality, and increased schooling attainment of children in Zimbabwe (Grépin and Bharadwaj, 2015; Agüero and Ramachandran, 2018).

Our paper contributes to the literature by investigating whether increases in education of mothers from rural origins improve adolescent development in China. The existing studies mainly focus on maternal education impacts on one aspect of child development or infant outcomes in China. For example, Chen and Li (2009) show that the effect of mother's education on adopted young children's height in China is significantly positive and similar to that on the own birth children, suggesting that the education effect is mainly postnatal nurturing. Based on the 1968 education reform in Taiwan, the study by Chou et al. (2010) shows significant positive effects of both parents' schooling on infant birth outcomes. However, both studies present little evidence on the mechanisms behind the observed effects.

Using China Family Panel Studies (CFPS), we provide new evidence on the beneficial effects of mother's education on a wide set of child outcomes during adolescence in China. Focusing on mothers from rural origins, our results demonstrate that increases in their education induced by the compulsory schooling reform have increased the child's school enrollment, math test scores, college aspiration, and internal locus of control related to education. Mother's education also improves adolescent mental health status and reduces the incidence of underweight. These findings have valuable implications for future comparison studies on the effectiveness of public policies on child economic and social well-being in developing countries.

We identify the causal effects of mother's education on child development by exploiting the exogenous temporal and geographical variations in the enforcement of compulsory schooling laws (CSLs) in China. In 1986, the Chinese government promulgated CSLs for the first time, making 9 years of education mandatory for all school-age children (i.e., ages 6–15). This reform was gradually implemented in different provinces at different times, between 1986 and 1994. We exploit variations within and between cohorts in reform exposure to construct an instrument for schooling. Some recent studies show that an increase in education induced by this reform has increased

[3] Because they have used the sum of mother's and father's education as the key regressor, they cannot identify the separate effect of mother's education.

[4] Some studies have used a twin design and found a strong positive effect of father's schooling, but a small positive or no significant effect of mother's education on child schooling (see Behrman and Rosenzweig, 2002; Pronzato, 2012). Another branch of the literature has attempted to separate the nurturing effect from the nature effect by focusing on adoptive parents and their adopted children (Sacerdote, 2002, 2007; Plug, 2004; Björklund et al., 2006; Chen and Li, 2009). Most of these studies find that both adoptive parents' schoolings are important for the intergenerational transmissions of schooling, but the effect of mother's education becomes small or negligible, when controlling for education of her husband (Plug, 2004).

individual earnings (Fang et al., 2012), affected noncognitive outcomes (Chen et al., 2018), but had mixed effects on health outcomes (Huang, 2015; Xie and Mo, 2014). We add to this literature by focusing on the impact of this reform on the next generations.

Another contribution of this paper is that the rich information on child well-being provided by CFPS allows us to examine the effects of mother's schooling on various outcomes of adolescent children (i.e., ages 10–19), including school enrollment, educational aspiration, cognition, internal locus of control, self-reported general health, mental health, and anthropometric outcomes. Most of the literature has focused on early life outcomes of children or educational attainment of young adult children, and only Carneiro et al. (2013), Oreopoulos et al. (2006), and Lindeboom et al. (2009) have analyzed some aspects of adolescence. Moreover, few of these papers have examined mental health of the children. The mental health of adolescents in developing countries has not been thoroughly studied even though mental health problems are a leading health problem for adolescents and have long-lasting effects throughout life (Kieling et al., 2011; Goodman et al., 2011). Our findings regarding various adolescent outcomes may provide additional evidence on the causal relationship between mother's education and children's success in the longer term, and contribute to our understanding of the process of human capital formation.

Furthermore, we find considerable gender heterogeneity in the effects of mother's education. The positive effects on school enrollment, math test scores, and internal locus of control are concentrated among girls, whereas the beneficial effects on adolescents' mental health and nutritional status are concentrated among boys. Further evidence suggests two important channels for the observed pattern. First, these effects are likely to arise from the positive effects of mother's education on family resources and economic inputs in their children. Our results provide suggestive evidence that maternal education is associated with greater bargaining power of mothers within the household in China, and thus leads to better outcomes of children, especially for girls (Leight and Liu, 2018; Lundberg et al., 1997). Second, increased education also leads to better maternal mental health and positive parenting, which may have important impacts on child outcomes (Schepman et al., 2011). In contrast with the literature on developed countries, we find little evidence that education affects a woman's marriage status, fertility, birth age, and birth weight of the child.

Our study also relates to the growing literature on socioeconomic persistence across generations in developing countries, especially China. Research has demonstrated that China has a lower level of intergenerational income mobility than most developed countries, and parental education has been widely regarded as an important mechanism for transmitting economic status from parents to children (e.g., Gong et al., 2012; Qin et al., 2016; Fan, 2016). The literature has also documented an intergenerational transmission of education and health from parents to children in China (Eriksson et al., 2014). In this paper, we provide direct evidence that part of the intergenerational transfer of socioeconomic status may be through the effect of mothers' education.

This paper proceeds as follows. Section 2 introduces the institutional background in China. The identification strategy is specified in Section 3. Section 4 describes the data and main variables of interest. Section 5 presents the results and various robustness checks. Section 6 discusses potential mechanisms. Section 7 concludes.

2. Institutional background

Since the 1980s, China has been developing its education system in accordance with the needs of economic and social development. In 1986, China promulgated the Compulsory Education Law and decided to achieve universal nine-year basic education gradually throughout the country. For the first time in China, the law stipulated that primary (6 years) and junior high school (3 years) educations were mandatory and free for school-age children in China: all children should enroll in school at age 6 (or sometimes age 7) and receive compulsory education, regardless of gender, nationality, or race. Additionally, it was illegal for any organization or individual to employ school-age children or adolescents.

A typical child who started school at age 6 would complete the nine-year formal schooling by age 15 and decide whether to pursue further education or work. Although the law stipulates that the nine-year compulsory education should be tuition-free (Article 10), tuition-free nine-year compulsory education was not strictly enforced immediately after the enactment and was taken, instead, as a long-term target of the government. Students still had to pay enrollment fees to attend school.[5] Children from households which could not afford the fees tended to drop out of school earlier than the nine-year stipulation (Brown and Park, 2002). Instead of a serious penalty, only verbal admonishment or criticisms were administered from the local government to parents who failed to send their school-age children to school (Xie and Mo, 2014).

One interesting feature of the 1986 CSLs in China is that it was accompanied by restrictions of employment for adolescents. In 1986, the government set the minimum age for admission to employment or work at 16 years for the public sector; in 1988, the age was also applied to the private sector,[6] which was reaffirmed by the *Provisions on the Prohibition of Using Child Labor* in 1991. Employers who hired child laborers younger than age 16 would be penalized, including warnings, fines, suspension of business operations, and withdrawal of business licenses. As the CSLs did not declare the school-leaving age, the minimum employment age was influential, especially for the first few cohorts. Thus, children younger than age 16, when the CSLs took effect, would receive more schooling than they otherwise would have had, and younger cohorts benefited more from the CSLs than the cohorts close to age 16.[7]

[5] Compulsory education was not provided free of charge in rural China until 2006 and in urban China until 2008 (Xiao et al., 2017).

[6] They are *Temporary provisions on State Businesses Employing Staff* in 1986 and *Provisional Regulations of the People's Republic of China on Private Enterprises* in 1988.

[7] Some countries have different school leaving age and minimum employment age. Based on the CSLs and minimum employment ages between 1915 and 1970 in the United State, Oreopoulos et al. (2006) construct the minimum length of time required in school before being allowed to obtain a work permit as the instrument for education.

The various levels of governments had different responsibilities for compulsory education. The central government was responsible for formulating nationwide policies and regulations, and helping financing compulsory education in low-income areas. Provincial governments made overall plans to promote compulsory education within the province, provided financial assistance to poor counties, and supervised the progress of the law's implementation at the city and county levels. Local governments at the county and city levels were mainly responsible for the provision and financing of compulsory education. In rural China, villages had great responsibility to mobilize school-age children to attend school, improve the quality of school facilities, and become involved in the supervision of school administration (Xie, 2002).

Due to regional differences in economic and development conditions, the reform has been implemented in stages across provinces and the cities and counties within the provinces (Liu, 2007).[8] Economically developed and coastal provinces (or municipalities), such as Zhejiang, Beijing, Chongqing, Sichuan, Hebei, and Jiangxi, were among the first to enforce the reform. Tibet was the last province to implement the reform, in 1994. Column 1 in Table B1 in the Appendix shows the time when the compulsory schooling law was introduced at the province level. Because the school year in China is from the beginning of September to the following July, for those provinces that initiated the reform in or after September, the actual reform year was regarded as the following year. Based on birth year and province of birth, the first cohort of mothers potentially affected is regarded as those aged 15 in the actual reform year of each province, shown in column 3 of Table B1.

As scheduled, nine-year compulsory education was first popularized in large cities by 1990, followed by medium-sized cities and some economically developed counties by 1995. By 2000, China basically realized the goal of universal compulsory education, covering 85% of its population. As illustrated in Fig. 1, the junior secondary school enrollment rate dramatically increased from 68.4% in 1985 to 97.9% in 2003. The percentage of junior graduates entering senior secondary schools increased from 41.7% in 1985 to 60.2% in 2003 (National Statistics Bureau, 2004).

In rural China, girls have a noticeable disadvantage in education compared with boys, because of traditional gender role attitudes, son preference, and different expected returns to girls' and boys' schooling (Hannum et al., 2009). Since the 1980s, both males and females from rural origins have seen improvements in education, and the gender gap in education has been narrowing over time and already diminished to relatively small extent under the compulsory education system (Zeng et al., 2014). According to a report by World Bank (2006), between 1990 and 2000, the enrollment rate of 7-year-old children rose from 72% to 95% for rural girls and from 77% to 96% for rural boys; the dropout rate of children aged 7–14 decreased from 16% to 3% for rural girls and from 11.7% to 2.6% for rural boys. In this paper, as the education impact of the CSLs for males is insignificant, we focus on the effect of the CSLs on education of females with rural *hukou* at birth.

3. Econometric strategy

To identify the causal impact of mother's education on child outcomes, we estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 S^M_{ipkt} + \beta_2 X_{it} + \theta^M_k + \tau^M_p + \omega_t + u_{it} \tag{1}$$

where Y_{it} indicates the outcome variables of child i in wave t. S^M_{ipkt} is the education of child i's mother (denoted by the subscript M) who was born with rural *hukou* in year k in province p. β_1 is the coefficient of interest, which captures the effect of mother's education on child development. X_{it} contains a vector of mother's and child's demographic characteristics, such as mother's ethnicity and child's age and gender.[9] θ^M_k are mother's birth year fixed effects. τ^M_p are mother's province-of-birth fixed effects. ω_t are the survey wave fixed effects. The OLS strategy would result in biased estimates of the coefficient β_1, because the error term u_{it} may contain unobserved environmental and genetic factors plausibly correlated with both mother's schooling and child outcomes.

To address the endogeneity problem, we employ the instrumental variable (IV) strategy by exploiting the China compulsory schooling law implemented nationwide from 1986 to 1994 as an exogenous source of changes in mothers' schooling levels. As different provinces implemented the reform at different times, we use within and between cohort variation in reform exposure as the instrument for mother's education. The key identifying assumption is that the CSLs affect child outcomes only through the years of schooling obtained by mothers, conditional on the other control variables.

One concern is that the timing of the reform implementation at the province level may be nonrandom, because the provincial governments decided their effective years for implementing the law according to local economic and development conditions. In Eq. (1), the province fixed effects account for all the time-invariant differences across provinces, and the cohort fixed effects control for all the unobserved factors that varied uniformly across cohorts. Notably, there may be province-cohort-specific unobservables correlated with both the timing of the reform and child outcomes.

To address this concern, we use two means to control for province-cohort-specific unobservables, denoted by $Trend^M_{pk}$. First, we include linear cohort trends at the province level, that is, the interaction terms of mother's province-of-birth dummies with a linear cohort trend, and test the robustness of our results by further including the province-specific quadratic cohort trends. Second, we allow the cohort effects to vary with the predetermined province characteristics that were important determinants of the timing of the reform across provinces. We add the interactions of mothers' year-of-birth dummies and provincial characteristics in 1985, including

[8] Within each province, full compliance with the law proceeded at a pace according to local conditions. Thus, our measure of reform exposure, defined based on the reform year at the province level, may contain measurement errors.

[9] We do not control for mother's age, because it is perfectly collinear with cohort and survey wave fixed effects.

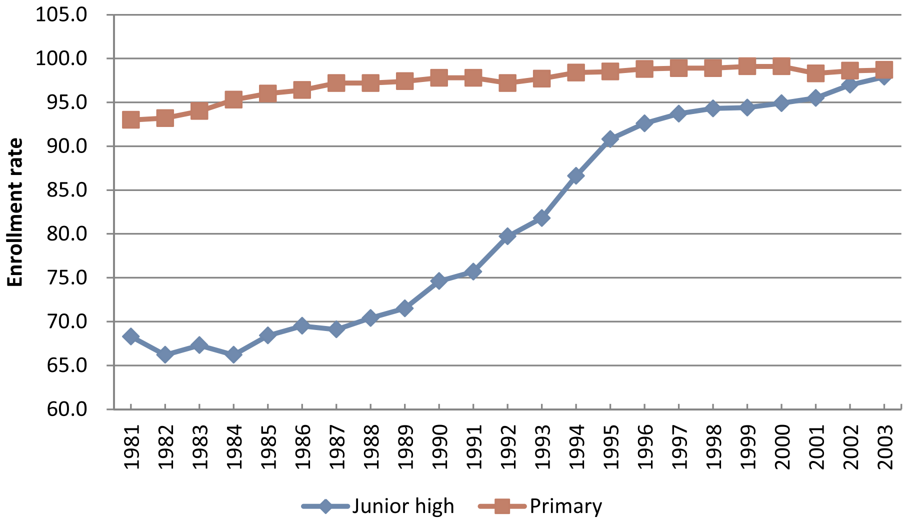

Fig. 1. School enrollment rate in China 1981–2003.
Data source: China Statistical Yearbook 2004.

GDP per capita, government expenditure per capita, and the fraction of junior high school graduates.[10] This flexibly controls for cohort variations in the time path of the outcome variables that were generated by the key determinants of the CSLs timing.

We use the following difference-in-differences (DID) specification to estimate the impact of the CSLs on maternal education:

$$S_{ipkt}^{M} = \gamma_0 + \gamma_1 Exposure_{pk}^{M} + \gamma_2 X_{it} + \theta_k^{M} + \tau_p^{M} + Trend_{pk}^{M} + \omega_t + \varepsilon_{ipkt} \tag{2}$$

where $Exposure_{pk}^{M}$ is a measure of reform exposure status for cohort k in province p and serves as an instrumental variable for mother's education. Different from developed countries that generally extended compulsory education by 1–3 years, it was the first time that China had used the 1986 law to specify 9-year compulsory schooling for all children. Younger cohorts, who had longer exposure to the CSLs during the compulsory education period, were affected more than the older cohorts. We classify mothers into three groups based on their age: those aged 5–10 are fully exposed, those aged 11–15 are partially exposed, and those aged 16 and older are nonexposed. There are two reasons for this classification. First, teenagers are more likely to participate in market work than children under age 10 in developing countries (Edmonds, 2008). Few studies on child labor in China have demonstrated that rural teenagers participate more in market and domestic work than urban teenagers, and child labor is negatively associated with a child's academic achievement and school enrollment (He, 2016; Tang et al., 2018). Thus, the opportunity cost of school attendance may be greatest for rural girls aged 16 and older because they were allowed to work, followed by those aged 11–15 (Hannum, 2003; Oreopoulos et al., 2006). The second reason is that in the early 1980s, the length of primary school was 5 years in most rural areas of China (Eble and Hu, 2017; Li, 2005).[11] Assuming that children enroll in primary school at age 6, they would leave school at age 11, after completing primary school.[12] Thus, our main measure of exposure to the reform is an ordinal variable ranging from 0 to 2, with 0 being no exposure, 1 being partial exposure, and 2 being full exposure.[13] As a robustness check, we also construct two dummy variables for partial and full exposure.

[10] The GDP and government expenditure data for each province are from the 1986 China Statistical Yearbook. The fraction of junior high school graduates for each province is constructed based on the 1% sample of the 1982 Chinese Population Census. Regressing the effective year of the CSLs in each province on these three key province-level variables yields a R^2 of about 0.4, suggesting that these variables explain approximately 40% of the variation in the CSLs timing. As a robustness check, we also add interactions between mothers' cohort dummies and additional provincial characteristics in 1985, including total population, population density, and the percentage of urban population. The point estimates remain mostly stable, whereas the precision of the estimates decreases for math test scores, internal locus of control, and depressive symptoms due to a poor fit of the first stage regressions for a limited sample size.

[11] In the early 1980s, the length of junior high school varied by region, ranging from two to four years (Eble and Hu, 2017; Li, 2005). Some females might not be in the correct grade for their age. Among those aged 15 and under in the reform year, some may have completed junior high school and be unaffected by the CSLs. Among those aged 16 and older, some may be affected by the reform because they have not graduated from junior high school. Thus, the mother sample might be misclassified with respect to reform exposure status. Some measurement errors in the reform exposure variable would lead to a potential underestimation of the effect of the CSLs on mother's schooling.

[12] In our sensitivity analysis, we also present results where those aged 12–15 are considered partially exposed.

[13] As a robustness check, we have also assumed that exposure to the reform follows a simple linear function of distance to the minimum working age of 16 in the reform year. The IV results without controlling for linear cohort trends at the province level are reported in column 2 of Table B2 in the Appendix, and are qualitatively similar to our main results, but the estimates for school enrollment become insignificant. However, adding province-specific trends can lead to a poor fit in the first stage regressions, due to high collinearity between the linear measure of reform exposure and the trends.

Another potential concern about our estimation strategy is that individuals might migrate across provinces for compulsory education in a systematic manner; however, because migration was strictly restricted by China's household registration (*hukou*) system until the late 1980s, interprovincial migration was rare during the 1980s and the 1990s (Fan, 2005). Moreover, migrant children were generally excluded from access to public education because they were denied the benefits of local *hukou* (Chen and Feng, 2013). Despite the central government mandating compulsory education for migrant children since the late 1990s, migrant children whose families cannot satisfy the requirement of excessive amounts of documentation or pay expensive fees are not allowed to attend local public schools (Wu, 2013; Montgomery, 2012).

The third concern relates to changes in the timing of marriage and fertility associated with women's increased education due to the CSLs. If female education leads to a postponement of marriage or motherhood, then we observe a selected sample of children. The literature on the United States has found no effects of education on fertility (McCrary and Royer, 2011; Oreopoulos et al., 2006). Fort et al. (2016), using the mandatory schooling reforms, show that females' education has a significantly negative effect on fertility in England, but no significant effect in Continental Europe. Using the female sample born between 1961 and 1985 from the CFPS 2010–2016 (i.e., the same cohorts as our main sample of mothers), we find no evidence that the CSLs affect the probability of marriage and having a teenage child.[14] This result implies that the sample selection may not be serious in this study.

Taking Eq. (2) as the first stage in a two-stage least-squares (2SLS) system of equations, we estimate the following equation in the second stage.

$$Y_{it} = \beta_0 + \beta_1 \hat{S}_{ipkt}^{M} + \beta_2 X_{it} + \theta_k^M + \tau_p^M + Trend_{pk}^M + \omega_t + u_{it} \tag{3}$$

where $\hat{S}_{ipkt}^{M}$ is the fitted value of maternal schooling from the first stage regression and $Trend_{pk}^M$ denotes province-specific trends (as previously defined). The IV estimator of the coefficient β_1 identifies the average effects of mother's schooling on child outcomes for women whose education was positively affected by the 1986 Compulsory Education Law, namely, local average treatment effects on compliers (Imbens and Angrist, 1994). These mothers would not have received additional years of education if the reform had not been implemented in their provinces of birth. They are likely from households with low socioeconomic status relative to always-takers; thus, the marginal effects of their schooling may be higher than the average effects for the population.[15]

4. Data and variables

In this paper, we use data from the four waves of China Family Panel Studies (CFPS), conducted by the Institute of Social Science Survey of Peking University. The CFPS is a large-scale, nationally representative, longitudinal study on Chinese family and society, which collects data at the individual, household, and community levels and provides comprehensive information on economic activities, family dynamics, education, health, migration, income, assets, and so forth. The national baseline survey launched in 2010 contains approximately 14,960 households in 25 provinces. Three waves of full-sample follow-up surveys were conducted in 2012, 2014, and 2016, respectively.

In our analysis, we focus on the pooled sample of adolescents aged 10–19 years with mothers' detailed information. We exclude approximately 10% of the mothers with urban *hukou* at birth,[16] and restrict our main analysis to the cohorts ranging 10 years on either side of the first affected cohort, that is, born between 1961 and 1985. The full sample contains 6419 children and 13,377 child-wave observations. The sample sizes for the regression analysis are different for different outcomes, depending on the data availability.

4.1. Reform exposure

For each mother, we define an ordinal variable measuring reform exposure status: it takes the value 0, 1, and 2 indicating no exposure, partial exposure, and full exposure to the reform, respectively, based on mother's year of birth and province of birth. By the time the CSLs were implemented in each province, those aged 16–25 are considered as the nonexposed cohorts, those aged 11–15 as the partially exposed cohorts, and those aged 5–10 as the fully exposed cohorts. In the sample, approximately 51% of mothers were not exposed, 32% were partially exposed, and 17% were fully exposed to the reform.

4.2. Children outcomes

We have multiple measures for adolescent development, namely, school enrollment, educational aspiration, cognitive ability, internal locus of control, self-reported general health, mental health, and anthropometric outcomes. For the detailed construction of adolescent outcome variables, see Appendix A.

[14] We have also estimated the effect of women's education on childlessness, the number of children, and maternal age at first birth, and find no significant effects of women's education.

[15] The literature using compulsory schooling laws or other instruments has shown that the IV estimates are generally as large as the OLS estimates, especially when the impacts of education are heterogeneous (e.g. Currie and Moretti, 2003; Chou et al., 2010; Oreopoulos et al., 2006; Carneiro et al., 2013).

[16] We find similar results when including the sample of mothers with urban *hukou* at birth, but the estimates for math test scores, internal locus of control, and K6 mental health scale have become marginally significant.

School enrollment is an important measure of child quality and schooling attainment in developing countries, as has been suggested in the literature (Li et al., 2008; Glewwe et al., 2001). In this paper, school enrollment is a binary variable indicating whether an adolescent is enrolled in school. We look at school enrollment for all adolescents aged 10–19 as well as those aged 16–19.[17]

Children's educational aspirations may translate into positive behavior and better educational achievement. For example, Bandura et al. (2001) suggest that teenagers' academic aspirations influence their actual educational achievement and career choice. For this analysis, we construct a binary variable that equals 1 if the desired educational attainment of the child is at least college.

The cognitive abilities are measured by two scores from the vocabulary and math tests administered among children aged 10 or older in the CFPS. There are 34 verbal questions and 24 standardized mathematics questions in CFPS 2010 and 2014. We obtain a verbal test score, from 0 to 34, and a math test score, from 0 to 24, that reflect an individual's vocabulary and mathematical ability, respectively. To allow for comparisons of children of different ages, we use age-gender standardized scores for both cognitive measures in our analysis.[18]

Internal locus of control is a psychological concept developed by Rotter (1966), assessing the extent to which individuals believe that their own efforts and actions affect their outcomes, compared with external forces beyond their control (e.g., fate, luck). As a widely used noncognitive outcome in the literature, internal locus of control has been found to be associated with educational attainment and labor market outcomes (e.g., Coleman and DeLeire, 2003; Heckman et al., 2006). For the purpose of this analysis, we measure a child's internal locus of control using a binary variable indicating whether the child believes that education is important for future achievement, based on related survey questions in CFPS 2010–2014 (see Appendix A for more details).

For self-assessed general health, adolescent respondents are asked in CFPS 2012–2016 how they rated their current health status on a 5-point scale (excellent, very good, good, fair, or poor). We define a binary health indicator that takes the value of 1 if the adolescent reports being in poor or fair health and 0 otherwise.

Following the wide practice in the literature (Kessler et al., 2002;Kessler et al., 2003), we use Kessler psychological distress scale (K6), collected in CFPS 2010 and 2014, to measure adolescents' mental health.[19] This scale ranges from 0 to 24, with higher scores indicating lower psychological well-being. In addition to the continuous measure, we also create an indicator of whether the respondent has a K6 score no less than 4, indicating moderate psychological distress or depressive symptoms, as has been suggested in the literature (Zhang et al., 2017; Sakurai et al., 2011).[20]

Following the literature that has investigated children's health in developing countries (Chen and Li, 2009; Strauss and Thomas, 1998), we construct a height-for-age z-score and two anthropometric outcomes of being thin or being overweight, using the reference standards from the WHO growth chart. Adolescents are classified as thin with a BMI-for-age z-score less than –2 and as overweight with a BMI-for-age z-score more than 1. Overweight and underweight can place children at risk for health issues.

4.3. Other variables and descriptive statistics

We consider several channels through which mother's education may affect child outcomes. First, the data contain information regarding children's family structure (parents' marital status, number of siblings) and initial conditions at birth (maternal birth age, birth weight). Second, we have several variables measuring family resources, namely, children's *hukou* status, fathers' years of schooling, mothers' employment status, parental income, household educational expense per child, and extracurricular classes. Third, we construct two measures of mental health for mothers as proxies for family stress and two measures of parenting style.

Table 1 summarizes key dependent and independent variables used in the analysis. On average, mothers have 5.2 years of schooling; those who are not exposed, partially exposed, and fully exposed to the reform have 4.5, 5.6, and 6.4 years of schooling, respectively. Children whose mothers had greater exposure to the reform have higher school enrollment rates, higher college aspirations, more internal locus of control, better mental health, higher height z-scores, but a higher likelihood of being overweight. Mothers with longer reform exposure are younger, less likely to be Han, and have younger children.

5. Results

5.1. Effects of CSLs on maternal education

We start our empirical analysis by providing OLS estimates for the impact of CSLs on maternal education, based on Eq. (2). Table 2 reports the baseline results of the DID specification. Robust standard errors are clustered at the province year-of-birth level.

[17] Despite the compulsory schooling laws and the minimum age for employment, dropout during the compulsory education period remains a challenge in China. The Ministry of Education reported that the dropout rate during the compulsory education period was 6.6% in 2016. The junior high dropout rates based on survey data were much higher than that reported by the government: between 17.6% and 31% of rural students dropped out during junior high school from 2007 to 2013 (Shi et al., 2015).

[18] As the descriptive statistics presented in Table 1 are aggregated across all age groups, the mean and standard deviations of these two cognitive variables are not exactly 0 and 1. Additionally, we also construct two alternative standardized measures that are normalized by age or by age-gender-education, and obtain similar results as those presented in Table 3.

[19] CFPS 2012 and 2016 have used the Center for Epidemiologic Studies Depression Scale (CES-D) to measure depressive symptoms; however, in CFPS 2016, only 20% of the respondents have been randomly selected to answer all the survey questions related to CES-D. As a robustness check, we have also estimated the effect of mother's education on the CES-D score of children, and the results are similar to those presented in the paper.

[20] Zhang et al. (2017) use k6 to estimate that the prevalence of depressive symptoms is about 31.5% among Chinese adults.

Table 1
Summary statistics.

	Sample size	Full sample		By mother's exposure status		
		Mean	Std. Dev.	Not exposed	Partially exposed	Fully exposed
Panel A: Outcome variables						
School enrollment	12,974	0.845	0.362	0.790	0.872***	0.959***
School enrollment (16–19 years)	5260	0.654	0.476	0.640	0.669**	0.769***
College aspiration	12,293	0.439	0.496	0.413	0.433**	0.528***
Cognition-math	6446	−0.035	0.959	−0.041	−0.044	0.006
Cognition-word	6406	0.012	0.921	0.012	0.010	0.016
Internal locus of control: education	5363	0.531	0.499	0.447	0.581***	0.734***
Self-reported health is poor	9241	0.074	0.261	0.076	0.069	0.075
K6 mental health scale	6524	3.011	3.492	3.162	2.840***	2.798***
Depressive symptoms	6524	0.339	0.474	0.362	0.316***	0.301***
Height *z*-score	12,598	−0.587	1.533	−0.633	−0.557***	−0.507***
Underweight	11,133	0.122	0.327	0.122	0.129	0.110
Overweight	11,133	0.132	0.339	0.099	0.134***	0.213***
Panel B: Instrumental variable						
Mother‘s reform exposure status	13,377	0.660	0.750	0	1	2
Panel C: Explanatory variables						
Mother's education	13,376	5.191	4.181	4.518	5.636	6.365
Mother's age	13,377	40.435	4.462	43.321	38.715	35.046
Mother's year of birth	13,377	1973	4.635	1969	1975	1979
Mother's ethnicity (Han = 1)	13,377	0.883	0.321	0.897	0.869	0.867
Child's age	13,377	14.500	2.835	15.399	14.142	12.483
Child is a boy	13,377	0.519	0.500	0.525	0.518	0.503
Panel D: Potential channels						
Family structure and initial conditions at birth						
Parents’ marital status	6419	0.979	0.144	0.978	0.976	0.987
Number of siblings	6419	1.067	1.019	1.229	0.892	0.883
Maternal birth age	6419	25.876	4.109	27.469	24.777	23.018
Birth Weight	2705	6.327	1.153	6.295	6.376	6.340
Family resources						
Child has urban *hukou*	6419	0.147	0.354	0.137	0.153	0.165
Father's years of schooling	6355	6.961	3.975	6.767	6.961	7.543
Mother has paid work	6285	0.206	0.405	0.165	0.233	0.281
Parental income (10k)	7353	2.140	2.133	1.878	2.367	2.682
Educational expense per child (10k)	13,232	0.277	0.446	0.275	0.289	0.256
Have extracurricular classes	10,869	0.121	0.326	0.097	0.137	0.149
Family stress and parenting style						
Mother‘s K6 mental health scale	6798	3.538	3.996	3.782	3.375	2.906
Mother's depressive symptoms	6798	0.393	0.488	0.418	0.390	0.300
Parenting score reported by parents	7539	19.646	4.452	19.307	19.651	20.171
Positive response to child's low school grades	7678	0.835	0.371	0.815	0.836	0.866

Note: Paired *t*-test is applied for mean differences in outcome variables of the (partially or fully) exposed and unexposed group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2
First-stage estimations for mother's years of schooling.

	IV: an ordinal measure of reform exposure			IV: two dummies for reform exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
Reform exposure status	0.892***	0.922***	0.860***			
	(0.224)	(0.237)	(0.234)			
Fully exposed to reform				1.684***	1.677***	1.767***
				(0.450)	(0.480)	(0.477)
Partially exposed to reform				1.007***	0.987***	0.838***
				(0.258)	(0.258)	(0.263)
Province*mother cohort trend	N	Y	Y	N	Y	Y
Cohort*1985 province char.	N	N	Y	N	N	Y
First-stage *F*-statistics	53.206	47.062	32.821	27.847	24.198	16.555
N	13,376	13,376	13,287	13,376	13,376	13,287

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

All columns of the regressions include the child's age, gender, survey wave indicators, mother's ethnicity, province-of-birth fixed effects, and birth cohort fixed effects.

The first three columns of Table 2 show the coefficients on mother's reform exposure status. In column 1, without controlling for province-specific trends, the estimate associated with the exposure variable is positive, large (0.90), and statistically significant, with the first-stage *F*-statistic at approximately 53. Column 2 adds interactions between province dummies and birth year, allowing for differential linear cohort trends in years of schooling across provinces. Column 3 further allows the cohort trends in maternal years of schooling to vary with certain provincial characteristics in the initial period. The more flexible specifications do not appreciably change the results: the coefficients obtained remain positive, large (0.92 and 0.86), and precisely estimated. In our preferred specification in column 3, the value of the *F*-statistic is approximately 33, suggesting that the reform has a strong impact on maternal years of schooling.[21]

To test whether the DID estimates are sensitive to alternative measures of reform exposure, we apply two indicators for full exposure and partial exposure in the last three columns of Table 2. The results show that the coefficients of both indicators are positive and highly significant, and their magnitudes are consistent with the baseline estimate in column 3. Specifically, column 6 shows that being fully (partially) exposed to the reform increases schooling by approximately 1.77 years (0.84 years) for mothers. The null hypothesis that the full-exposure coefficient is twice the magnitude of the partial-exposure coefficient cannot be rejected at the 1% level in all specifications, which justifies our main measure of reform exposure and also implies that the results are robust to alternative measures of reform exposure. Nevertheless, the first-stage *F*-statistics suggest that, compared with two exposure indicators, our main measure of reform exposure is a stronger predictor of maternal education.

We also test whether the DID estimates are sensitive to narrower ranges around the first affected cohort, as shown in Panel A of Table B4 in the Appendix. Additionally, a mother may appear in the dataset multiple times, either because we pool different waves of the CFPS in the baseline analysis or a mother may have multiple children. We retain one observation per child in Panel B and one observation per mother in Panel C. The results show that reform exposure and exposure indicators are significant in all regressions. Though the F-statistics decline as sample size decreases, the DID estimates are robust to narrower ranges of cohorts and different sample specifications.

To interpret the DID estimate as the effect of CSLs, we must assume the parallel trend assumption that the cohort differences in education, in the absence of the reform, would have been similar, on average, across provinces with different effective years of the reform. To test the parallel cohort trends assumption, we follow Lundborg et al. (2014) and graphically present how maternal education changed before and after the reform implementation. Specifically, we regress maternal years of schooling on a sequence of dummies for time periods up to 8 years before and 10 years after the reform, birth cohort dummies, province dummies, and wave dummies, using one observation per mother.

Fig. 2 illustrates the estimates of the timing dummies with 95%-confidence intervals. Number 0 on the x-axis is when the reform was implemented in the province and the mother was 15 years old, that is, the first affected cohort. The other numbers are measured as the difference between age 15 and the age of the individual when the reform took effect. The reference group is the cohorts born 9 or 10 years before the first affected cohort. Consistent with our first-stage results, Fig. 2 highlights a significant jump in schooling for the first affected cohort, and another significant jump for the cohorts born 5 years after the first affected cohort (i.e., those aged 10 and younger in the reform year). The coefficients of timing dummies before the reform implementation are statistically insignificant, providing strong evidence for no particular trends in maternal years of schooling prior to the CSLs in the province. On average, maternal education levels are higher in post-reform years than they were in pre-reform years. Moreover, the fully exposed cohorts have more schooling than the partially exposed cohorts.

5.2. Main results

Table 3 reports the effects of maternal years of schooling on aspects of child development. In column 1, the OLS estimates based on Eq. (1) show that mother's education is significantly, positively correlated with children's school enrollment, college aspiration, and two measures of cognitive ability, that is, the math and verbal test scores. We do not find the same correlation for internal locus of control. The OLS results also suggest significant associations between mother's education and a higher height-for-age z-score and lower probabilities of being underweight or overweight, but no significant association for self-reported general health and mental health.

The IV estimates applying reform exposure as an instrument for maternal years of schooling are presented in columns 2–4. In column 2 that controls for province and cohort fixed effects, the IV estimates are larger in magnitude than OLS estimates for all the outcomes, except verbal test scores, height-for-age z-score, and overweight. We find that mother's education improves school enrollment, college aspiration, math test scores, and internal locus of control. Mother's education also reduces the K6 mental health scale and risk of depressive symptoms and being underweight.

Although we realize that the OLS estimators probably have an upward bias due to the omission of ability or genes, we posit two

[21] We have also estimated the effect of CSLs on father's education in Table B3 in the Appendix. The estimates are positive but insignificant, which is consistent with Li and Liu (2014). They find that local primary school availability has a strong positive effect on girls' schooling but no significant effect on boys' education in rural China. One possible interpretation is that parents in rural China tend to prioritize sons' education, due to pro-son bias and the gender gap in returns to education (Song et al., 2006). Thus, we cannot investigate the causal effect of father's schooling on child outcomes in this paper.

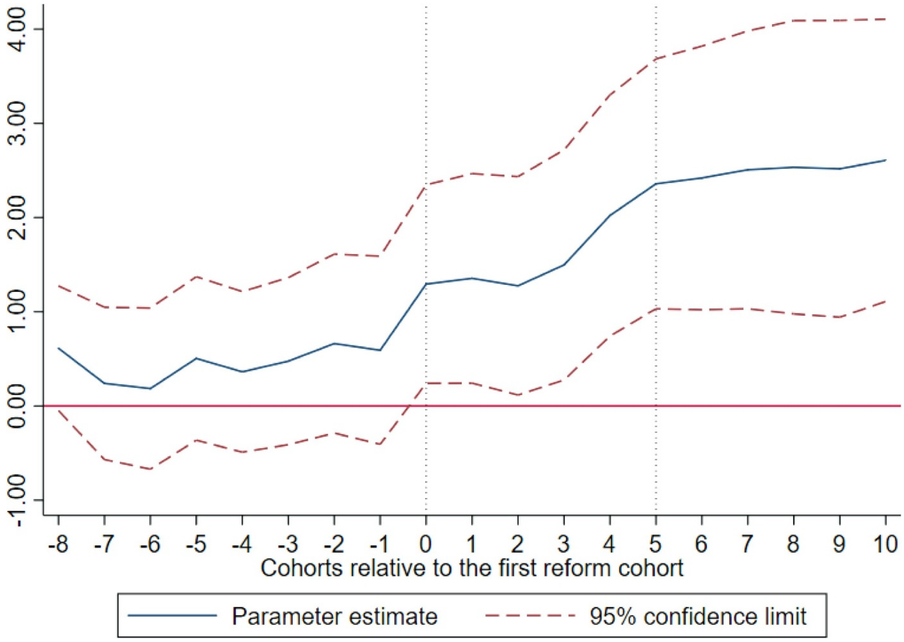

Fig. 2. Changes in maternal years of schooling before and after the reform implementation.
Note: The figure reports the estimates of timing dummies with 95%-confidence intervals in the regression of maternal years of schooling on a sequence of dummies for time periods up to 8 years before and 10 years after the reform, birth cohort dummies, province dummies, and wave dummies, using one observation per mother. On the *x*-axis, time 0 is when the reform was implemented in the province and the mother was 15 years old, i.e., the first reform cohort, and the other numbers indicate the difference between age 15 and the age of individuals when the reform took effect. The reference group is the cohorts born 9 and 10 years before the first reform cohort.

explanations for the larger IV estimates. First, the IV estimates capture the effect of education on compliers, that is, women whose education increased due to the CSLs. As suggested in studies that have used the CSLs as instruments (e.g., Chou et al., 2010; Oreopoulos et al., 2006), the marginal effect of education for the compliers is likely to be larger, because the primary effect of the CSLs is at the bottom of the schooling distribution. Second, the OLS estimates may suffer from attenuation bias because of the classical measurement error in maternal education. However, as the OLS estimates are not systematically biased toward zero, these results might not be driven by only measurement error.

We next present the IV results based on Eq. (3), including province-specific linear trends in column 3, and the interaction terms between the birth cohort dummies and three key provincial characteristics in 1985 in column 4. The estimates are slightly changed in magnitude but remain economically and statistically significant for many outcomes. In particular, our preferred specification in column 4 shows that an additional year of maternal education leads to increases in the likelihood of school enrollment by 5 percentage points (or 6%) for all adolescents and by 11 percentage points (or 16.8%) for adolescents aged 16–19, and an increase in the likelihood of having college aspiration by 13.4 percentage points (or 30.5%). One more year of maternal schooling increases the math test score by 0.127 standard deviations. Compared with the OLS estimate, the IV estimate for the verbal test score remains positive but has become insignificant. Furthermore, the estimates for internal locus of control suggest that one more year of mother's education increases children's belief in the importance of education by 7.5 percentage points (or 14%).[22]

Regarding health outcomes, the IV results in column 4 demonstrate that mother's education has no significant effect on children's self-reported health but improves their mental health, which is different from the OLS results. An extra year of maternal schooling significantly reduces children's K6 mental health scale by 0.44 (or 0.126 standard deviations) and the rate of having depressive symptoms by 5.9 percentage points (or 17.4%). There is no significant evidence that an increase in mother's schooling would affect the height-for-age *z*-score and probability of being overweight; however, it significantly reduces the probability of being underweight by 4.1 percentage points (or 33.6%).[23]

Compared to the evidence from developed countries, our estimated effects of mother's education are similar but slightly larger in magnitude. For example, Carneiro et al. (2013) find that the effects of mother's education on math test scores amount to 0.06 and 0.107 standard deviations for white and black adolescents in the U.S. Lundborg et al. (2014) find that the effects on health index and

[22] We have also estimated the results for child's views on the importance of talent and efforts for one's future success. The estimates are positive but statistically insignificant.

[23] As a further check, we have also investigated children's reading and exercising behaviors. We find some evidence that children with more educated mothers are significantly more likely to read books (for purposes other than work or exams) and do physical exercises. The results are presented in Table B5 in the Appendix.

Table 3
Estimated effects of maternal schooling for adolescents.

	Estimate on mother's schooling years OLS (1)	IV: reform exposure status (2)	(3)	(4)
School enrollment	0.011***	0.024*	0.027*	0.050***
	(0.001)	(0.014)	(0.014)	(0.017)
First-stage F-statistics		49.808	41.560	30.765
N	12,973	12,973	12,973	12,887
School enrollment (16–19 years)	0.022***	0.055*	0.072**	0.110***
	(0.002)	(0.030)	(0.037)	(0.037)
First-stage F-statistics		21.376	14.832	12.093
N	5259	5259	5259	5230
College aspiration	0.018***	0.079***	0.089***	0.134***
	(0.001)	(0.025)	(0.028)	(0.035)
First-stage F-statistics		47.110	40.546	29.455
N	12,293	12,293	12,293	12,209
Cognition-math	0.047***	0.128**	0.112*	0.127**
	(0.004)	(0.052)	(0.061)	(0.060)
First-stage F-statistics		30.101	21.771	19.340
N	6446	6446	6446	6410
Cognition-word	0.043***	0.000	0.029	0.062
	(0.003)	(0.050)	(0.062)	(0.060)
First-stage F-statistics		26.396	18.910	16.847
N	6406	6406	6406	6370
Internal LOC: education	0.001	0.058**	0.057*	0.075*
	(0.001)	(0.028)	(0.034)	(0.040)
First-stage F-statistics		18.972	10.794	9.060
N	5363	5363	5363	5328
Self-report health is poor	−0.001	−0.015	−0.022	−0.023
	(0.001)	(0.018)	(0.016)	(0.019)
First-stage F-statistics		23.503	24.394	16.138
N	9240	9240	9240	9178
K6 mental health scale	0.006	−0.547**	−0.379*	−0.440**
	(0.013)	(0.217)	(0.204)	(0.206)
First-stage F-statistics		30.636	23.893	20.380
N	6524	6524	6524	6488
Depressive symptoms	0.001	−0.066**	−0.049*	−0.059**
	(0.002)	(0.028)	(0.026)	(0.028)
First-stage F-statistics		30.636	23.893	20.380
N	6524	6524	6524	6488
Height z-score	0.058***	0.051	−0.045	−0.060
	(0.005)	(0.078)	(0.081)	(0.077)
First-stage F-statistics		53.390	47.982	37.633
N	12,598	12,598	12,598	12,517
Underweight	−0.003***	−0.050***	−0.042**	−0.041**
	(0.001)	(0.019)	(0.018)	(0.018)
First-stage F-statistics		48.519	46.466	34.164
N	11,133	11,133	11,133	11,062
Overweight	−0.003***	0.000	−0.001	−0.012
	(0.001)	(0.014)	(0.015)	(0.019)
First-stage F-statistics		48.519	46.466	34.164
N	11,133	11,133	11,133	11,062
Province*mother cohort trend	N	N	Y	Y
Cohort*1985 province char.	N	N	N	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

cognition amount to 0.1 standard deviations in Sweden. It suggests that improving women's education may have a stronger impact on child quality in developing countries.

Furthermore, column 1 of Table 4 reports the reduced-form estimates for the effects of CSLs implementation on children's outcomes. Because the first-stage effects are sufficiently strong, we observe significant reduced-form effects on various outcome variables of the children. These estimates indicate that adolescent children whose mother has greater exposure to the CSLs had a higher likelihood of school enrollment, more college aspiration, higher math test scores, higher internal locus of control, better mental health, and lower probabilities of being underweight than those whose mothers were less exposed.

One potential concern with the estimates is that we have so many outcomes that some significant results may be found simply by chance, even if the reform has zero effects. To address this concern regarding multiple inference, we control the false discovery rate

Table 4
Reduced-form estimates for impacts of the reform.

	Estimate on reform exposure	Unadjusted p values	FDR-adjusted p values
School enrollment	0.042***		
	(0.013)	0.001	0.004
N	12,887		
School enrollment (16–19 years)	0.103***		
	(0.029)	0.000	0.002
N	5230		
College aspiration	0.113***		
	(0.021)	0.000	0.000
N	12,209		
Cognition-math	0.129**		
	(0.062)	0.039	0.059
N	6410		
Cognition-word	0.058		
	(0.061)	0.345	0.414
N	6370		
Internal LOC: education	0.057**		
	(0.023)	0.012	0.024
N	5328		
Self-report health is poor	−0.016		
	(0.013)	0.208	0.278
N	9179		
K6 mental health scale	−0.453**		
	(0.184)	0.014	0.025
N	6488		
Depressive symptoms	−0.061***		
	(0.023)	0.009	0.022
N	6488		
Height z-score	−0.057		
	(0.069)	0.415	0.452
N	12,517		
Underweight	−0.039***		
	(0.014)	0.006	0.018
N	11,062		
Overweight	−0.012		
	(0.019)	0.540	0.540
N	11,062		
Province*mother cohort trend	Y		
Cohort*1985 province char.	Y		

Note: Standard errors clustered at the province year-of-birth level are reported in parentheses in column 1. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

(FDR, i.e., the expected proportion of rejections that are type I errors) using the procedure proposed by Benjamini and Yekutieli (2001) and report FDR-adjusted p values in the last column of Table 4. For the most significant outcomes, the FDR-adjusted p values are slightly larger than the unadjusted p values based on the t distribution. Overall, our results are not sensitive to FDR adjustment.

5.3. Sensitivity analysis

5.3.1. Placebo tests

The validity of our identification strategy relies on the assumption that mother's reform exposure only affects children's outcomes through its effect on mother's schooling. One potential concern is that our reform exposure variable may pick up some structural changes other than the CSLs that are also correlated with adolescent development. In this subsection, we conduct several tests to gauge the plausibility of this assumption.

First, we perform a placebo test and assume that the CSLs had been implemented 4, 6, 8 years earlier than the actual reform year of each province. We construct the “placebo” reform exposure in a similar manner as our main treatment measure, and add it in the reduced-form estimations. We also control for the actual reform exposure in these regressions due to systematical overlaps between placebo and real reforms for some cohorts. The “placebo” reform exposure should have no impact on mother's years of schooling and, more importantly, no effects on eight outcomes of adolescents that are significantly affected by the actual reform exposure in Table 4.

As reported in Table 5, the results show that adding the “placebo” reform exposure does not change the reduced-form estimates on the actual reform exposure; moreover, the coefficients on the “placebo” reform exposure are either statistically insignificant or have the opposite sign. It provides supportive evidence that our main results are due to the actual CSLs as opposed to other unobserved mechanisms.

Table 5
Placebo tests for impacts of the reform.

		Estimate on reform exposure Suppose CSLs 8 years before (1)	Suppose CSLs 6 years before (2)	Suppose CSLs 4 years before (3)
Mother's year of schooling	Placebo reform	0.173	−0.330	−0.023
		(0.383)	(0.284)	(0.239)
	Actual reform	0.884***	0.885***	0.860***
		(0.237)	(0.233)	(0.234)
N		13,287	13,287	13,287
School enrollment	Placebo reform	−0.008	0.027	−0.024
		(0.021)	(0.017)	(0.015)
	Actual reform	0.040***	0.040***	0.042***
		(0.013)	(0.013)	(0.013)
N		12,887	12,887	12,887
School enrollment (16–19 years)	Placebo reform	−0.034	0.036	−0.045
		(0.031)	(0.027)	(0.030)
	Actual reform	0.099***	0.099***	0.097***
		(0.029)	(0.029)	(0.029)
N		5230	5230	5230
College aspiration	Placebo reform	0.008	−0.024	0.017
		(0.028)	(0.023)	(0.024)
	Actual reform	0.114***	0.115***	0.113***
		(0.021)	(0.020)	(0.021)
N		12,209	12,209	12,209
Cognition-math	Placebo reform	0.092	−0.050	−0.072
		(0.065)	(0.058)	(0.052)
	Actual reform	0.142**	0.134**	0.128**
		(0.061)	(0.062)	(0.062)
N		6410	6410	6410
Internal LOC: education	Placebo reform	−0.010	0.006	−0.062***
		(0.029)	(0.025)	(0.021)
	Actual reform	0.056**	0.057**	0.057**
		(0.023)	(0.023)	(0.023)
N		5328	5328	5328
K6 mental health scale	Placebo reform	−0.135	0.324	−0.041
		(0.220)	(0.198)	(0.179)
	Actual reform	−0.471**	−0.481***	−0.453**
		(0.186)	(0.184)	(0.185)
N		6488	6488	6488
Depressive symptoms	Placebo reform	−0.005	0.053**	−0.006
		(0.030)	(0.026)	(0.025)
	Actual reform	−0.062***	−0.066***	−0.061***
		(0.024)	(0.023)	(0.023)
N		6488	6488	6488
Underweight	Placebo reform	0.011	0.037**	−0.010
		(0.019)	(0.015)	(0.017)
	Actual reform	−0.037**	−0.042***	−0.039***
		(0.015)	(0.014)	(0.014)
N		11,062	11,062	11,062
Province*mother cohort trend		Y	Y	Y
Cohort *1985 province char.		Y	Y	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

Our second test is a permutation test: we use the baseline sample of mothers but randomly assign the effective year of the reform to provinces (Rosenbaum, 2007). Specifically, our sample is from 28 birth provinces. We randomly divide the 28 birth provinces into seven groups. Each permuted group contains the same number of provinces as the corresponding group defined by the actual reform year (see Table B1 in the Appendix) and is imposed the placebo reform year between 1986 and 1994. Next, the "placebo" reform exposure is constructed based on the simulated reform year and mothers' birth year. We estimate the placebo treatment effect on maternal years of schooling using the same model specification as in Eq. (2), and the placebo treatment effects on adolescent outcomes in the reduced-form estimations.

To approximate the permutation distribution, we conduct the random assignment 1500 times. The distributions of the 1500 placebo estimates are plotted in Fig. 3 and are used for statistical inference. The p value of the permutation placebo test is the proportion of placebo estimates greater than or equal to in absolute value the estimated effect of the actual reform in our baseline analysis (i.e., the first-stage estimate of 0.86 in column 3 of Table 2, and the reduced-form estimates in column 1 of Table 4). The

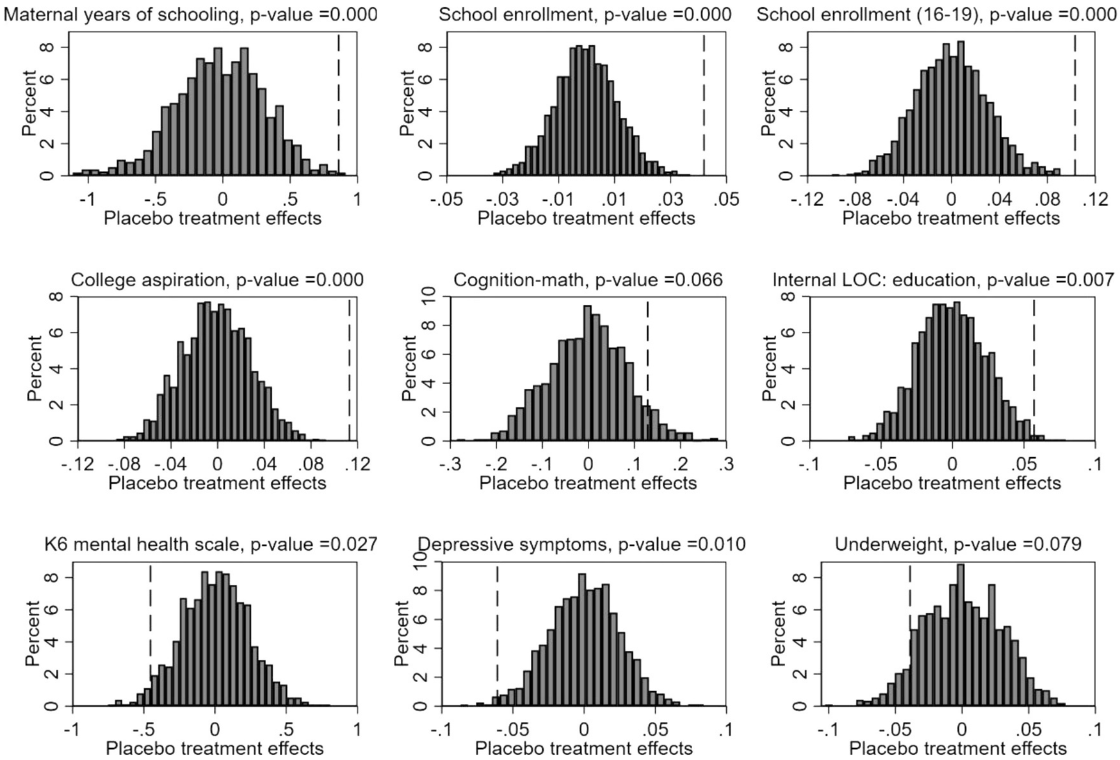

Fig. 3. Estimated coefficients from the permutation placebo tests.

Note: We randomly assign the effective year of the reform to provinces and construct the "placebo" reform exposure based on the simulated reform year and mothers' birth year, using the baseline sample. The histogram displays the distribution of the placebo estimates from 1500 random assignments. The *p* value of the permutation placebo test is the proportion of placebo estimates greater than or equal to in absolute value the estimated effect of the actual reform from the baseline analysis (indicated by the dashed line).

figure illustrates that the *p* values for all outcomes are smaller than 0.10. These checks lend further credence to our identification strategy.

5.3.2. *Confounding factors*

Next, we investigate whether our estimates for mother's increased education due to the reform exposure would be confounded by other policies or factors, such as migration, one-child policy and new cooperative medical scheme.

First, since the late 1980s, China has been marked by a massive migration of rural residents to urban areas in search of jobs and higher incomes. Parents typically leave their children behind with grandparents or other relatives in rural areas. These left-behind children are vulnerable to psychological and developmental problems (Zhou et al., 2014). Our main estimates may be biased if the cohort trends in internal migration are associated with the exposure to the CSLs. To address this concern, we add controls for the interaction terms between the prevalence of migrant workers at the province level in 1982, which captures the initial migration networks, and mother's birth year dummies.[24] As shown in column 1 of Table 6, the baseline results are robust to controlling for the effects of migration.

Second, China's one-child policy, introduced around 1980, has reduced fertility and may increase human capital investment for each child as predicted by the Becker–Lewis quantity–quality model (Rosenzweig and Zhang, 2009). Since 1984, the enforcement of the fertility policy has been localized, so that different provinces had different mandated limits. As a check that provincial variations in the fertility policy are not confounding the results, we control for the average monetary penalty rate for an unauthorized birth, formulated in years of household income, in the residence province when the mother was 20 years old, which are from Ebenstein (2010). The estimates in column 2 of Table 6 are similar to our main results.

Third, to provide public health insurance for rural residents, China launched a nationwide project known as the New Cooperative Medical Scheme (NCMS) in 2003, which had been expanded to all rural counties by 2008. The access to health insurance may improve the mother's prenatal care, reduce infant mortality, and increase investment in children's human capital (Chen and Jin, 2012). If the roll-outs of the NCMS and the CSLs are correlated, we may erroneously attribute difference in child outcomes to mother's increased education. To investigate this possibility, we control for interactions between the effective years of the NCMS at the county level and mother's birth year dummies. In column 3 of Table 6, the estimates are very similar, which increase our

[24] The provincial migration rate is constructed based on the 1% sample of the 1982 Chinese Population Census.

Table 6
Robustness check controlling for the potential confounding factors.

	IV estimate on mother's schooling years Control for migration trends (1)	Control for one-child policy (2)	Control for the NCMS (3)
School enrollment	0.050***	0.045***	0.054***
	(0.017)	(0.015)	(0.019)
N	12,887	12,887	12,887
School enrollment (16–19 years)	0.096**	0.112***	0.110***
	(0.037)	(0.038)	(0.037)
N	5230	5230	5230
College aspiration	0.137***	0.130***	0.131***
	(0.036)	(0.033)	(0.037)
N	12,209	12,209	12,209
Cognition-math	0.099	0.130**	0.128**
	(0.063)	(0.060)	(0.060)
N	6410	6410	6410
Cognition-word	0.044	0.065	0.067
	(0.069)	(0.058)	(0.061)
N	6370	6370	6370
Internal LOC: education	0.071*	0.070*	0.073*
	(0.037)	(0.038)	(0.043)
N	5328	5328	5328
Self-report health is poor	−0.027	−0.021	−0.019
	(0.020)	(0.017)	(0.020)
N	9178	9178	9178
K6 mental health scale	−0.418*	−0.420**	−0.459**
	(0.220)	(0.197)	(0.219)
N	6488	6488	6488
Depressive symptoms	−0.063**	−0.057**	−0.057*
	(0.030)	(0.026)	(0.030)
N	6488	6488	6488
Height *z*-score	−0.072	−0.046	−0.068
	(0.082)	(0.071)	(0.081)
N	12,517	12,517	12,517
Underweight	−0.036**	−0.039**	−0.040**
	(0.017)	(0.017)	(0.019)
N	11,062	11,062	11,062
Overweight	−0.005	−0.012	−0.016
	(0.020)	(0.018)	(0.020)
N	11,062	11,062	11,062
Province*mother cohort trend	Y	Y	Y
Cohort*1985 province char.	Y	Y	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

confidence that the results reflect the effect of the CSLs.

We have to admit that we cannot rule out all province-cohort-specific unobservables that may be correlated with both mother's education and child outcomes. However, these sensitivity exercises provide supportive evidence that these other unobservables are unlikely to drive our main results.

5.3.3. Other robustness checks

In this subsection, we explore the robustness of the results using alternative specifications in province-specific trends, alternative measures of reform exposure and alternative samples. First, we include province-specific quadratic cohort trends in column 1 of Table 7. This more flexible specification does not appreciably change our main estimates in column 4 of Table 3, except that the effect on depressive symptoms becomes marginally significant at the 15% level ($p = 0.115$).

Second, we estimate model (3) using two indicators of reform exposure (i.e., full exposure and partial exposure) as instruments for maternal years of schooling. Column 2 in Table 7 shows that the effects of mother's education on most outcomes are similar to our main results. Thus, our main findings are robust to the more flexible specification of instrumental variables.

Third, prior to the 1986 law, there was no formal regulation on school entry age in China, and children typically started their schooling at ages 6 or 7. If children enter primary school at age 7, they would be 12 years old or older after finishing primary school. Thus, we consider the specification where those aged 12–15 are defined as partially exposed. In column 3 of Table 7, the results for most outcomes are similar to our main results. Nevertheless, the effects of maternal education on math test scores and underweight have become insignificant.

Fourth, due to the restrictions of the *hukou* system, we impose a plausible assumption: the province where a mother received her

Table 7
Robustness check using alternative specifications in province-specific trends and reform exposure.

	IV estimate on mother's schooling years Add quadratic trends (1)	Use two dummies for exposure as instruments (2)	Define those aged 12+ as partially exposed (3)	Use residential province at age 12 (4)
School enrollment	0.085***	0.049***	0.033*	0.051***
	(0.029)	(0.017)	(0.020)	(0.017)
N	12,887	12,887	12,887	12,884
School enrollment (16–19 years)	0.107***	0.106***	0.078*	0.111***
	(0.037)	(0.036)	(0.041)	(0.038)
N	5230	5230	5230	5231
College aspiration	0.163***	0.134***	0.142***	0.138***
	(0.049)	(0.036)	(0.053)	(0.037)
N	12,209	12,209	12,209	12,206
Cognition-math	0.182***	0.145**	−0.044	0.124**
	(0.065)	(0.062)	(0.093)	(0.060)
N	6410	6410	6410	6409
Cognition-word	0.088	0.087	0.017	0.063
	(0.065)	(0.059)	(0.071)	(0.061)
N	6370	6370	6370	6369
Internal LOC: education	0.071*	0.075*	0.092*	0.082*
	(0.040)	(0.041)	(0.053)	(0.043)
N	5328	5328	5328	5325
Self-report health is poor	−0.033	−0.024	−0.015	−0.024
	(0.027)	(0.019)	(0.024)	(0.020)
N	9178	9178	9178	9176
K6 mental health scale	−0.396*	−0.448**	−0.416	−0.432**
	(0.225)	(0.206)	(0.286)	(0.207)
N	6488	6488	6488	6487
Depressive symptoms	−0.046	−0.060**	−0.067*	−0.059**
	(0.028)	(0.027)	(0.040)	(0.028)
N	6488	6488	6488	6487
Height *z*-score	−0.035	−0.066	0.093	−0.046
	(0.089)	(0.078)	(0.110)	(0.077)
N	12,517	12,517	12,517	12,514
Underweight	−0.047**	−0.045**	−0.022	−0.039**
	(0.021)	(0.019)	(0.019)	(0.018)
N	11,062	11,062	11,062	11,058
Overweight	−0.015	−0.007	−0.008	−0.011
	(0.022)	(0.020)	(0.023)	(0.020)
N	11,062	11,062	11,062	11,058
Province*mother cohort trend	Y	Y	Y	Y
Cohort*1985 province char.	Y	Y	Y	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

compulsory education is the same as the province of birth. As an additional check on this assumption, we assign reform exposure based on mother's residential province when she was 12 years old.[25] The results in column 4 of Table 7 are qualitatively similar.

In column 1 of Table 8, we drop mothers in provinces that initiated the reform in September, because the true first year of implementation may be uncertain. The results do not change much. The effect of maternal education on internal locus of control has become marginally significant at the 15% level, but the effect on verbal test scores is now positive and significant.

In the main analyses, we use the cohorts within 10 years earlier and later than the first affected cohort. As a robustness check, we restrict the sample to a smaller range of observations around the first affected cohort and test whether the 2SLS estimates are sensitive to narrower ranges of cohorts. In Table 8, we use an 8-year window (column 2) and a 6-year window (column 3) around the first affected cohort. The results are very similar to our main results.

One potential concern is that some mother–child pairs may appear multiple times in the study sample, as we pool multiple waves of the CFPS in the main analyses. If some outcomes are not malleable during adolescence, our results based on the pooled sample may be overestimated. To test the sensitivity of our results, we re-estimate the models using the first observation for each child in column 4 of Table 8. The results remain mostly unchanged.

[25] Approximately 1.63% of mothers in our sample lived in different provinces at age 12 from their provinces of birth.

Table 8
Robustness check using alternative sample specifications.

	IV estimate on mother's schooling years Drop provinces that initiated the reform in Sept. (1)	[−8,8] (2)	[−6,6] (3)	Keep one observation for each child (4)
School enrollment	0.054***	0.062***	0.068***	0.035**
	(0.019)	(0.023)	(0.024)	(0.016)
N	8177	11,950	10,300	6328
School enrollment (16–19 years)	**0.101*****	**0.076****	**0.106*****	**0.093****
	(0.033)	(0.032)	(0.031)	(0.043)
N	3239	4870	4210	3726
College aspiration	0.144***	0.165***	0.159***	0.154***
	(0.038)	(0.047)	(0.048)	(0.048)
N	7737	11,335	9784	6151
Cognition-math	0.239***	0.154**	0.155***	0.113*
	(0.075)	(0.064)	(0.059)	(0.067)
N	4086	5982	5174	5059
Cognition-word	0.132*	0.069	0.061	0.104
	(0.074)	(0.072)	(0.072)	(0.072)
N	4050	5940	5133	5015
Internal LOC: education	0.074	0.053	0.062*	0.094*
	(0.050)	(0.038)	(0.037)	(0.051)
N	3432	4945	4237	4821
Self-report health is poor	−0.016	−0.024	−0.014	−0.033
	(0.020)	(0.023)	(0.022)	(0.025)
N	5772	8488	7282	5237
K6 mental health scale	−0.657***	−0.476*	−0.476**	−0.439**
	(0.192)	(0.252)	(0.238)	(0.220)
N	4142	6056	5235	5100
Depressive symptoms	−0.037*	−0.045	−0.049*	−0.049*
	(0.022)	(0.029)	(0.028)	(0.027)
N	4142	6056	5235	5100
Height *z*-score	−0.016	−0.012	−0.034	−0.020
	(0.092)	(0.087)	(0.087)	(0.094)
N	7936	11,602	10,003	6219
Underweight	−0.062**	−0.032*	−0.032*	−0.057**
	(0.025)	(0.019)	(0.019)	(0.025)
N	7037	10,264	8859	5762
Overweight	−0.005	−0.014	−0.019	−0.014
	(0.024)	(0.021)	(0.021)	(0.023)
N	7037	10,264	8859	5762
Province*mother cohort trend	Y	Y	Y	Y
Cohort*1985 province char.	Y	Y	Y	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

5.4. *Heterogeneous effects by gender*

In Table 9, we examine whether the role of mother's education varies with the gender of the child. The results demonstrate that mother's education significantly increases children's college aspiration for both boys and girls; however, the effects of mother's education on other outcomes differ substantially between boys and girls. For girls, the IV estimates suggest that maternal schooling has significantly increased school enrollment rate, math test scores, and internal locus of control. By contrast, boys gain little or less from mother's increased education, in terms of those outcomes.

The possible explanation for the gender differences in the effects of mother's education is that girls generally had lower school enrollment than boys in rural China, because of the traditional pro-son bias and perceived lower returns to and greater opportunity costs of girls' education (Connelly and Zheng, 2003; Brown and Park, 2002; Hannum, 2003). Mothers with more education provide a role model for daughters and have more bargaining power regarding girls' educational spending, thereby improving girls' education-related outcomes (Leight and Liu, 2018; Song et al., 2006; Zhao and Glewwe, 2010).

With respect to health outcomes, the estimates in Table 9 indicate a significant improvement in mental health and a significant decrease in the likelihood of being underweight among boys with more educated mothers; however, for girls, we find no significant effects for health outcomes. Our findings are consistent with Carneiro et al. (2013), who also find that the impact of maternal schooling on behavior problems is only significant for boys but insignificant for girls aged 12–14.

Table 9
Estimations by child gender.

	IV estimate on mother's schooling years Boys	Girls
School enrollment	0.040	0.060***
	(0.027)	(0.021)
N	6667	6220
School enrollment (16–19 years)	0.054**	0.073***
	(0.026)	(0.028)
N	2662	2568
College aspiration	0.158***	0.113***
	(0.057)	(0.040)
N	6311	5898
Cognition-math	0.014	0.227**
	(0.083)	(0.100)
N	3290	3120
Cognition-word	−0.001	0.112
	(0.096)	(0.074)
N	3274	3096
Internal LOC: education	0.015	0.130**
	(0.055)	(0.063)
N	2758	2570
Self-report health is poor	−0.023	−0.019
	(0.033)	(0.023)
N	4799	4379
K6 mental health scale	−0.747**	−0.245
	(0.346)	(0.265)
N	3319	3169
Depressive symptoms	−0.096**	−0.042
	(0.047)	(0.037)
N	3319	3169
Height z-score	−0.173	−0.008
	(0.140)	(0.087)
N	6477	6040
Underweight	−0.060**	−0.013
	(0.030)	(0.020)
N	5736	5326
Overweight	0.009	−0.021
	(0.033)	(0.020)
N	5736	5326
Province*mother cohort trend	Y	Y
Cohort*1985 province char.	Y	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

6. Mechanisms

In this section, we explore several potential channels through which mothers' education can transmit into better child outcomes. Specifically, we use the same model specification in Eqs. (2) and (3) to estimate the effects of mother's schooling on the manifold dimensions of a child's family structure, resources, and environment.[26]

6.1. Family structure and initial conditions at birth

Mother's education may affect child outcomes through its positive effect on family structure (Currie and Moretti, 2003). The trade-off between quantity and quality of children suggests that more educated mothers are likely to have fewer children and thus invest more in each child (Becker and Lewis, 1973). In Panel A of Table 10, the IV estimates show that mother's education has no significant effects on marital status and number of children. Although these results are inconsistent with the relevant studies on developed countries (Currie and Moretti, 2003; Lundborg et al., 2014; Carneiro et al., 2013) and some developing countries (Keats, 2018; Dincer et al., 2014), they are plausible given the low proportion of single-parent households and the birth control policies in China.

One potential mechanism through which mother's education improves child outcomes is the biological endowment passed from

[26] Because children's initial condition of birth, family structure, *hukou* status, fathers' years of schooling, and mothers' employment status do not change or change little over time, we use one observation per child for the related regressions.

Table 10
Mechanism analysis – family structure and initial conditions at birth.

	Estimate on mother's schooling years			
	OLS	IV: reform exposure status		
	(1)	(2)	(3)	(4)
Panel A. Family structure				
Parents' marital status	−0.000	0.002	0.001	−0.008
	(0.001)	(0.008)	(0.010)	(0.012)
N	6419	6419	6419	6374
Number of siblings	−0.048***	0.075	0.003	−0.049
	(0.005)	(0.084)	(0.077)	(0.063)
N	6419	6419	6419	6374
Panel B. Initial conditions at birth				
Maternal birth age	0.000	0.009	0.006	0.006
	(0.001)	(0.009)	(0.012)	(0.014)
N	6419	6419	6419	6374
Birth weight	0.014**	−0.068	−0.114	−0.088
	(0.006)	(0.055)	(0.075)	(0.073)
N	2705	2705	2705	2684
Province*mother cohort trend	N	N	Y	Y
Cohort*1985 province char.	N	N	N	Y

Note: Standard errors clustered at the province-year of birth level are reported in parentheses. Each child is an observation. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

mother to child. In Panel B of Table 10, the OLS and IV estimates show no significant effect of mother's education on maternal birth age. Although the OLS estimates show that mother's education is positively associated with birth weight of the child, the IV estimates are insignificant after dealing with the endogeneity.[27]

6.2. Family resources

One obvious possibility is that more educated women may have greater economic resources and bargaining power within the household that lead to more investments in children's education, nutrition and health care. We explore this mechanism by examining the effects of mother's education on several measures of family socioeconomic characteristics.

Given the unique household registration system in China, *hukou* is an important determinant of individuals' socioeconomic status (Han et al., 2015). Children were granted *hukou* according to mothers' *hukou* status until 1998. Since 1998, children's *hukou* can be inherited from either their mother or father. Importantly, *hukou* status affects a child's eligibility to access the local public school and health care systems. The IV estimates in Panel A of Table 11 indicate that one extra year of mother's schooling significantly increases the likelihood that the child has an urban *hukou* by approximately 4 percentage points.[28]

Mother's schooling may affect child outcomes indirectly through father's schooling due to a mating effect. The IV estimates show that one additional year of mother's education leads to a significant increase in father's education by 0.75 years (for those with a spouse). This finding is consistent with Lundborg et al. (2014) and Carneiro et al. (2013) and implies that assortative mating may be one channel underlying the effects of mother's education. Although we cannot estimate the causal effect of father's education on child outcomes due to the absence of a valid instrument, having a more educated father may not fully drive our results, because the literature shows that the effects of father's education are either insignificant (Lundborg et al., 2014) or weaker than the maternal effects (Holmlund et al., 2011).[29]

Furthermore, the IV estimates in Panel A of Table 11 show that a one-year increase in mother's education significantly increases the likelihood of mothers being employed by 4.8 percentage points and leads to 2790 yuan higher income of the couple.[30] Despite the evidence of socioeconomic benefits associated with mothers' additional schooling, it is still valuable to analyze the direct inputs into developing the child's human capital. The results demonstrate that one extra year of mother's schooling significantly increases household educational expenditure per child in the previous year by 620 yuan (or approximately 22%), and the probability that

[27] The sample size is smaller due to missing values in the birth weight variable.

[28] To test whether the impact of maternal education on child development is entirely driven by its impact on hukou status of children, we conduct an additional robustness check by focusing on children with rural hukou. The results, which are available from the authors upon request, are virtually unchanged.

[29] When controlling for father's education, we still find significant effects of mother's education on most outcomes of children; thus, the effects of mother's education cannot be fully explained by assortative mating.

[30] Note that we did not look at mother's individual income because more than half of mothers in our sample are doing farm work, and it is hard to measure their individual incomes. Using the subsample of mothers being employed, we also find a significant positive effect of education on mother's individual income.

Table 11
Mechanism analysis – family resources and environment.

	Estimate on mother's schooling years			
	OLS	IV: reform exposure status		
	(1)	(2)	(3)	(4)
Panel A. Family resources				
Child has urban *hukou*	0.028***	0.031*	0.034*	0.042**
	(0.001)	(0.017)	(0.020)	(0.021)
N	6419	6419	6419	6374
Father's years of schooling	0.409***	0.761***	0.692**	0.749***
	(0.015)	(0.249)	(0.284)	(0.281)
N	6355	6355	6355	6312
Mother has paid work	0.025***	0.021	0.030	0.048*
	(0.002)	(0.022)	(0.024)	(0.027)
N	6285	6285	6285	6244
Parental income	0.063***	0.219	0.343*	0.279*
	(0.008)	(0.145)	(0.179)	(0.168)
N	7353	7353	7353	7303
Educational expense per child	0.014***	0.046**	0.045**	0.062**
	(0.002)	(0.021)	(0.021)	(0.026)
N	13,232	13,232	13,232	13,143
Have extracurricular classes	0.014***	0.048***	0.048***	0.054***
	(0.001)	(0.017)	(0.017)	(0.020)
N	10,869	10,869	10,869	10,794
Panel B. Family stress and parenting style				
Mother's K6 mental health scale	−0.096***	−0.542**	−0.680**	−0.824***
	(0.016)	(0.235)	(0.273)	(0.268)
N	6798	6798	6798	6759
Mother's depressive symptoms	−0.012***	−0.033	−0.054**	−0.064**
	(0.002)	(0.023)	(0.025)	(0.027)
N	6798	6798	6798	6759
Parenting score reported by parents	0.204***	−0.325	−0.253	−0.013
	(0.015)	(0.302)	(0.281)	(0.284)
N	7233	7233	7233	7179
Positive response to child's low school grades	0.010***	0.034	0.036	0.047*
	(0.001)	(0.022)	(0.023)	(0.029)
N	7362	7362	7362	7305
Province*mother cohort trend	N	N	Y	Y
Cohort*1985 province char.	N	N	N	Y

Note: Standard errors clustered at the province year-of-birth level are reported in parentheses. We use one observation per child for child's *hukou*, father's schooling, and mother's employment status. For other outcomes, each child-wave is an observation. All regressions include dummies variables for year of birth and province at birth, mother's ethnicity, child's age, gender, and survey wave indicators. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and * respectively.

current students have extracurricular classes by 5.4 percentage points.

These findings are in line with the literature (Carneiro et al., 2013; Brown, 2006; Leight and Liu, 2018; Lundberg et al., 1997) and offer some evidence that an important channel through which mother's education influences child outcomes is through a general improvement in family resources and probably greater bargaining power of educated mothers.

6.3. Family stress and parenting style

More educated mothers may provide an environment more conducive to healthy child development, regardless of family resources. First, we investigate whether mother's additional schooling contributes to her own psychological well-being, which may improve child outcomes indirectly, as suggested in the literature (Akee et al., 2018; Cesarini et al., 2016). Mother's psychological well-beings are measured by the Kessler 6-item psychological distress scale and a binary indicator of depressive symptoms, constructed in the same manner as child mental health outcomes. In Panel B of Table 11, the results indicate that an increase in mother's schooling significantly reduces mother's K6 mental health scale and the risk of depressive symptoms.

We can relate our results to the recent literature on the role of maternal mental health in the development of offspring outcomes. Studies on England show that maternal emotional problems contribute to the increasing trend in adolescent emotional problems (Schepman et al., 2011) and are associated with children's adulthood mental health and long-run economic costs (Johnson et al., 2013). Using Australian data, Le and Nguyen (2018) find that children aged 0–13 with depressed mothers are more likely to require extra medical care, especially for boys. Together, these findings and ours suggest that better maternal mental health associated with an increase in mother's schooling may be another mechanism underlying the improvements in child outcomes in our paper.

Additionally, mother's education may also influence child outcomes indirectly through its impact on parenting behavior (Davis-Kean, 2005). There is a growing body of literature on the connection between parenting style and child health and academic achievement (e.g., Burton et al., 2002; Dooley and Stewart, 2007). Education may provide mothers with important knowledge and parenting skills that enable them to better support and facilitate their children's learning and development.

In this study, we have two measures of parenting style: one score based on 6 related questions answered by the child's primary caregiver and a binary variable measuring parents' positive response to child's low school grades (see Appendix A for more details). Unfortunately, we do not know whether those measures exactly refer to mother's parenting style in the CFPS data.[31] As shown in Panel B of Table 11, the OLS estimates indicate significant positive associations between mother's education and both measures of parenting style. However, the IV estimates are not significant for the parenting score and only significant at the 10% level for parental response to child's low school grades in column 4. Nevertheless, it provides modest evidence that mothers with more education are likely to adopt a positive parenting style.[32]

7. Conclusion

Although the association between mother's education and child welfare has been demonstrated extensively, the empirical evidence on the causality and mechanisms are far from conclusive, especially for developing countries where women lag behind men in many aspects, including education. This paper contributes to the literature by identifying the causal effects of mother's education on a wide range of child outcomes during adolescence and exploring the potential mechanisms behind the causal effects in the context of rural China. To this end, we exploit that the 1986 Compulsory Education Law was implemented gradually during a 9-year period across provinces.

We find that this reform has significantly increased educational attainment by approximately 1.77 years for mothers with full exposure and 0.84 years for mothers with partial exposure in rural China. Our main results show that mother's education significantly increases adolescents' school enrollment, college aspiration, and math test scores, promotes their internal locus of control related to education, and improves their mental health and nutritional status.

We also present evidence on the mechanisms behind the estimated effects. First, an increase in women's schooling leads to substantial socioeconomic benefits for their children, including urban *hukou*, better-educated fathers, and higher parental incomes. Educated mothers are more likely to be employed and spend more on children's education. In addition, we demonstrate that more education results in an improvement in mother's mental health and parenting. Thus, the maternal education effect on child outcomes may operate through its impacts on family resources, maternal mental health, and parenting. Different from the literature on developed countries, we rule out changes in marital status, fertility, maternal birth age and birth weight as alternative explanations in this analysis.

We further observe strong gender differences in how mother's education affects child development. The results suggest that girls, in particular, benefit more from improved maternal education in terms of school enrollment, math test scores, and internal locus of control, and we assert that this result is probably due to greater family resources and increased bargaining power of mothers. Moreover, boys' mental health and underweight status are more sensitive to mother's education than those outcomes for girls, which might be explained by the finding that education leads to better mental health among mothers.

Notably, our IV estimates can only identify local average treatment effects for so-called "compliers." Those should mainly be females at the lower end of the education distribution who changed their education levels in response to the implementation of the Law on 9-year Compulsory Education. Although we find no significant effect of the compulsory education laws on father's education, it is an important topic for future research to examine the effects of father's education on child development based on other contexts and additional data.

Our findings about the nurturing effect of mother's education in human capital formation of children have important policy implications for public policies in China and many other developing countries. Investment in women's education may have significant economic and social returns. Public programs that aim to increase women's education may benefit multiple future generations and reduce intergenerational inequality.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jce.2019.04.001.

Appendix A. Construction of main outcome variables

Educational aspiration

To measure educational aspiration, the CFPS asked each adolescent the following survey question: What is the minimum level of

[31] CFPS asked the related survey questions for children aged 10–15. In our sample, about 96% of children live with their mothers, and we find no significant effect of mother's education on children's likelihood of living with mothers. Thus, we restrict the sample to children living with their mothers in the regressions for parenting style, so that these two measures are likely to reflect mother's parenting behavior.

[32] We have also conducted a mediation analysis by adding family resource, mother's mental health, and parenting style in the regressions for child outcomes. The results (not presented but available upon request) suggest that family resources are important transmission channels for all adolescent outcomes except mental health, while mother's mental health is the main channel through which mother's education improves adolescents' mental health. For math test scores, both family resources and parenting are important channels underlying the effect of mother's education.

education you think you should attain? The answers include “primary school,” “junior high school,” “senior high school,” “3-year college,” “4-year college/bachelor's degree,” “master's degree,” and “doctoral degree.” We construct a binary variable that equals 1 if the desired educational attainment is at least college.

Vocabulary and math test scores

The vocabulary and math test scores are based on 34 verbal questions and 24 standardized mathematics questions in CFPS 2010 and 2014. All the test items were obtained from the standard textbooks in primary and secondary schools and used to assess individual education achievements, that is, crystallized intelligence.[33] Specifically, the tests listed the questions in order of increasing difficulty and assigned respondents different starting points depending on their education levels.

The respondents were divided into three education groups. The low education group (primary school or below) started from the first questions in both tests; the middle education group (junior middle school) started from the ninth question in the verbal test and thirteenth question in the math test; and the high education group (high school and above) started from the twenty-first question in the verbal test and nineteenth question in the math test. The tests ended if the respondents failed to answer three successive questions.

The test score is the rank order of the most difficult question that the respondent has answered correctly or the rank order of the starting question minus one if no answer was correct. Thus, we obtain a verbal test score, from 0 to 34, and a math test score, from 0 to 24, that reflect an individual's vocabulary and mathematical ability, respectively. To increase the comparability of the tests for students of different ages, both test scores are normalized by age-gender group to obtain a mean of zero and standard deviation of one.

Internal locus of control

In the CFPS, children were asked about their views on the importance of education, talent, and efforts for future success. For these questions, wave 2010 surveyed children aged 12, 14, and 16 and older, wave 2012 surveyed children aged 10–15, and wave 2014 surveyed children aged 10–15 who were newly-interviewed or had missed the survey questions in previous wave.[34] CFPS 2010 asked children to report on 5-point scales (e.g., 1 = strongly disagree, 5 = strongly agree). CFPS 2012 and 2014 asked children to rate on 10-point scales (e.g., 0 = least important, 10 = most important). We measure child's internal locus of control by using a binary variable indicating whether the child believes that education is important for future achievement. Specifically, we let this binary outcome variable be equal to 1 if the responses were “agree” or “strongly agree” in CFPS 2010 or the ratings were 8–10 in CFPS 2012 and 2014, and 0 otherwise.

Mental health

In CFPS 2010 and 2014, the Kessler psychological distress scale contained six questions that asked about the following feelings and thoughts during the past month: depressed, nervous, upset, hopeless, everything was difficult, and life was meaningless. The response to each question is scored on a 5-point scale from 0 (never) to 4 (almost daily). We sum across these six items for a total K6 score ranging from 0 – 24, and higher scores indicate lower psychological well-being.

Parenting style

For children aged 10–15, the primary caregivers or adult family members living with them were asked to answer 6 questions about how often they gave up watching TV due to children's study, discussed with children about school life, asked children to finish homework, checked children's homework, restricted children from watching TV, and restricted the types of TV programs children could watch. We sum up all the responses (on a 5-point Likert scale) to construct an index of parenting style, where the total score ranges from 6 to 30. Higher scores indicate more effective interactions between parents and children.

The primary caregivers were also asked about the most common reaction if the children bring back a transcript with a score lower than their expectations. We construct a binary variable measuring parents’ response to child's low school grades, which equals one if the parents have a positive response, including contacting the teacher, encouraging the child to study harder, or helping the child more, and zero otherwise (i.e., physical punishment, scolding the child, restricting the child's activities, or no action).

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[33] Two sets of cognitive assessments have been alternatively used in different waves of the CFPS. CFPS 2012 and 2016 follow the Health and Retirement Study in the United States and include word recall and numerical series tests that evaluate individual memory and ability to think. However, the numerical series tests had a high non-response rate (Xie et al., 2017).
[34] CFPS 2016 did not have these survey questions related to internal locus of control.

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