

Does primary school duration matter? Evaluating the consequences of a large Chinese policy experiment

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ABSTRACT

Nearly all governments provide primary schooling, but surprisingly little is known about how changes to the duration of primary school affect educational attainment and performance in the labor market. We study a Chinese policy which extended the duration of primary school from five years to six but did not change the curriculum. We exploit its gradual rollout over space and time to generate causal estimates of its impact on educational attainment and subsequent labor market outcomes. We find that the policy has small, largely positive effects on post-primary educational attainment, and raises average monthly income by 2.6%. The policy is progressive, generating higher returns (5–8%) among both women and the least educated. We estimate the policy has already reallocated 450 million years of labor from work to schooling and we generate cost-benefit estimates to quantify this tradeoff, highlighting the large public finance implications of this policy decision.

1. Introduction

Nearly all countries regulate the duration of primary and secondary schooling. The literature on compulsory schooling has studied the impacts of changes to the age at which students are permitted to leave schooling and enter the labor force. This work has found substantial gains in health, wealth, and longevity to increasing the age at which students are first allowed to leave school (e.g., Angrist & Krueger, 1991; Devereux & Hart, 2010; Oreopoulos, 2007). Another key aspect of this regulation is the policy decision setting the number of years needed to finish each level of schooling.

In many countries, there are differentials in access and cost between levels of schooling, e.g., the introduction of school fees at middle or high school, or a limited number of admissions slots at higher levels (c.f., Lavy 1996; Orazem & King 2007). As a result, the duration of each level of schooling, and particularly primary school, has a large potential impact both on how many total years students spend in school and, as a result, on their later earning power. Examining Demographic and Health Surveys (DHS) datasets for the 74 developing countries where years of schooling data is collected, we find bunching in the number of

completed years of education at those years where a level of schooling is completed - usually primary and lower secondary - in 48 of these.¹ While recent work has begun to study the impacts of changes to school duration at the secondary and tertiary levels (Arteaga, 2018; Morin, 2013; Pischke, 2007),² ours is the first paper we are aware of studying how changes at the primary level, the most common stopping point we see in our DHS data, affect educational attainment and labor market outcomes.

In this paper, we study a policy in China which extended the duration of primary school from five years to six while holding the national curriculum and duration of all other levels of schooling unchanged. We exploit the fact the policy was rolled out gradually over space and time to generate three sets of empirical results. First, we measure how the extra year of primary school affects overall educational attainment. Second, we estimate how the policy affects performance in the labor market. Third, we estimate the public finance implications of this policy which, to date, has reallocated more than 450 million years of labor from employment to schooling.³

This policy differs importantly from policies studied in most prior work on compulsory education and the returns to schooling in the labor

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¹ These patterns are also common in developed countries. According to US Current Population Survey data, 73 percent of 30–64 year olds in the US spend exactly as many years in school as is needed to earn either a secondary or post-secondary credential.

² See also other work by Krashinsky (2014), Büttner and Thomsen (2015), Meyer and Thomsen (2016), Huebener, Kuger, and Marcus (2017), Liwiński (2018a,b), and Meyer, Thomsen, and Schneider (2019).

³ Calculation given in footnote 7.

market. Most previous studies (e.g., those summarized in Card, 1999) use changes in compulsory schooling which also add to the set of skills students are meant to acquire. For example, both Angrist and Krueger (1991) and Oreopoulos (2006) study policies which induce students to spend an additional year in high school and, in so doing, advance further in the high school curriculum. This constitutes an increase in skills on the extensive margin. Our policy induces all students to spend six years learning the same material that unaffected students were made to learn in five, which instead is intended to increase skills on the intensive margin.⁴ Furthermore, it changed the parameters of the schooling decision students faced. To earn any given credential (primary, middle, high, or tertiary), students affected by the policy had to spend an additional year in school (and thus out of the labor market).⁵

We identify the causal effect of the Chinese six year primary education policy on schooling and labor market outcomes using a stacked regression discontinuity (RD) design.⁶ Our running/forcing variable is month-by-year-of-birth (as in Oreopoulos, 2006, and Erten & Keskin, 2018, among several others). We recenter this variable around the policy implementation year in a given locality, and stack cutoffs (as in Pop-Eleches & Urquiola, 2013, and Abdulkadiroğlu, Angrist, & Pathak, 2014) across different policy implementation years.

We determine if, when, and how the policy is implemented in each of China's prefectures by collecting and coding thousands of official government documents, known as "educational gazetteers," which report implementation at the local level, and match this to new survey and census data from China. We restrict our sample to the 280 prefectures (of 345) where we are sure the policy was implemented. Our identification strategy compares outcomes of treated and untreated individuals within each of these prefectures, using only those leaving primary school within an optimal bandwidth around the year when the policy took effect. We also generate parallel results using a difference-in-differences specification. As we are unable to generate a satisfactory test of the parallel trends assumption and the language of the policy suggests that this assumption is likely to be violated (details discussed in Section 3.1), we present these results in Appendix 5. They largely mirror the results we present in the body of the paper.

We first estimate the impact of this policy on years of schooling and the attainment of educational credentials. We find that the average number of years of completed primary schooling increased from 5.2 to nearly six for affected individuals. We find small changes in the proportion of individuals getting middle school and high school degrees, but no evidence that the policy changed the characteristics (parents' education, number of siblings) of who earns which credential.

We then estimate the impact of the policy on labor market performance. We find that the policy does not affect individuals' likelihood of being employed, but makes them slightly more likely (1.8 percentage points, or 6.7 percent) to work in government. Our main labor market result is that the policy increases monthly income by 2.63 percent, with about 20 percent of this gain coming from the increase in credentials, and the other 80 percent coming from income gains conditional on highest credential earned. The magnitude and precision of our estimate of the policy's impact on income are robust to a battery of specification and robustness checks, including a permutation test. We then show that the magnitude of our estimate changes little after accounting for potential mediators such as experience differentials between affected and unaffected individuals.

⁴ Morin (2013) studies a similar change at the secondary level using a policy change in Canada.

⁵ Though middle school was made compulsory in 1986, in Appendix 4 we show evidence that the rollout of the compulsory middle school policy has little effect on whether or not individuals in our sample complete middle school or earn at least a middle school credential.

⁶ This could also be called an event study; here we use the RD label, as we use the machinery for establishing causal inference in that literature (Imbens & Lemieux, 2008; Lee & Card, 2008; Lee & Lemieux, 2010).

Despite these low overall returns, we find the policy is redistributive. While it induces nearly all to forgo a year of earnings, its benefits are concentrated among a key disadvantaged group in China: those with only a primary or middle school education, whose income increases by between five and eight percent as a result of the policy. We argue that the basic cognitive and non-cognitive/socio-emotional skills reinforced in this extra year are most likely to help those with lower skill levels. Furthermore, our finding is consistent with other work showing that extending the time children spend in school most helps struggling students (Clay, Lingwall, & Stephens Jr, 2016; Dobbie & Fryer, 2013; Meghir & Palme, 2005).

A primary impact of this policy is the reallocation of human resources from work to school. We estimate that the policy has reallocated more than 450 million years, or more than 900 billion person-hours, from the labor market to the pursuit of schooling to date.⁷ We perform a cost-benefit analysis to quantify the public finance implications of this massive reallocation of resources. We borrow the framework used in Duflo (2001) to compare the lifetime value of the increase in monthly income conferred by the extra year of schooling to the estimated cost of the lost year of productive activity in the labor market. Our preferred specification finds that the policy generates an overall net gain, though its benefits decrease over time with the decline in the per-cohort proportion of individuals with low levels of education, i.e., those who benefit most.

Our paper contributes to two active lines of inquiry. First, we add to the rich literature attempting to understand the education and labor market effects of changes to compulsory education policy (e.g., Card 1999; Stephens & Yang 2014). We advance this work in a few key ways. First, as mentioned at the beginning of this section, the policy we study constitutes an intended increase in skills on the intensive margin, as opposed to on the extensive margin studied in the vast majority of previous work. Second, we contribute to a nascent literature studying changes to the duration of schooling, as opposed to school leaving age. Furthermore, it is the first paper we are aware of to study a change in the duration of primary school, as opposed to changes in the duration of secondary or tertiary schooling as in Morin (2013) and Arteaga (2018). Third, the policy changes the duration of school for all students, not just a subgroup, and has already affected the lives of hundreds of millions of individuals.

The second line of inquiry we contribute to is the wider set of studies using large changes in education policy from developing countries to assess the merits of different policy options and their distributional effects (e.g., Banerjee, Cole, Duflo, & Linden 2007; Duflo 2001; Lucas & Mbiti 2012). To this work, we add the first analysis of a key policy lever - the duration of primary school - and show that it has potentially important redistributive properties.⁸ This is particularly pertinent for policymakers in the many developing countries with a sizable proportion of individuals who do not progress beyond primary or lower secondary school.

The rest of the paper proceeds as follows. In Section 2, we discuss the history of education in modern China and describe the policy we study. In Section 3, we describe the data we use and our identification

⁷ China's educational yearbooks estimate that 332,321,868 children graduated with six years of primary school between 1984 and 2009. The number of students leaving primary school between 2010–2017 under the six-year regime, assuming negligible drop-out from primary school, is 120,060,612. The number graduating under this regime between 1981 and 1984 is not listed in the yearbooks. Using the proportions given in Figure A.1, we estimate that it is likely to be no more than a few million students. We assume individuals spend 2,000 h working in a year. We multiply the number of affected individuals to date by the year of lost labor hours each forgoes to generate an estimate of approximately 905 billion person-hours of labor reallocated to schooling between 1981 and 2017.

⁸ García (2018) studies the distributional effects of the German change to the length of secondary school in the mid-2000's.

strategy. Section 4 contains empirical results related to educational attainment and Section 5 provides our empirical results relating to the labor market, a series of robustness checks, and our cost-benefit analysis. Section 6 concludes.

2. Setting

This paper studies the education system in China after the Cultural Revolution ended in 1976. China's system resembles those of the US and many other developed and developing countries, with primary school, middle school, high school, and then tertiary education.⁹ In this section, we provide more detail on our setting and on the purpose and details of the policy we study.

In the aftermath of the chaos caused by China's Cultural Revolution (1966–1976), the country moved to standardize its education system. To this end, the *Full-Time Ten-Year Primary and Middle Education Teaching Plan (Draft)* was passed in January 1978. This mandated national harmonization of the duration and structure of primary, middle, and high school in all provinces, setting the duration of primary school to be five years in schools across the country. At the end of 1980, the Central Committee of the Communist Party of China and State Council issued the *Decision on Several Problems Relating to Universal Primary Education*, the policy whose changes we use for our analysis. This policy mandated that the total years of primary and secondary education be extended to twelve years, including a shift from five to six years of primary school.¹⁰ This policy was announced early on in Deng Xiaoping's time as China's de facto leader and at the beginning of the country's transition from a planned to a more market-oriented economy. One of Deng's early directives was that education should “face the demands of the new era and meet head-on the challenges of the technological revolution,” part of his larger move in the late 1970s and early 1980s to prepare China's labor force to adapt to this new economic arrangement (Vogel, 2011).

The structure of the system prior to and after the policy we study is shown visually in Fig. 1. The letter of the law allowed gradual adoption of the primary school duration change across localities, with the central government putting more initial pressure on urban schools (Liu, 1993). In practice, roughly 60 percent of localities switched to a six year system between 1981 and 1993, relatively few made the change in the mid-1990s, and the rest shifted in the late 1990s and 2000s, reaching near-universal adoption in 2005. Figure A.1 plots national data on the proportion of students in six year (or equivalent) primary school systems, showing this gradual adoption of the policy over time.

This policy usually did not change the age at which children entered school, nor did it change the primary school, middle school, or high school curricula.¹¹ Rather, the intent of the change was that primary school students be taught the same material over a longer period of time to ensure mastery of the curriculum. Prior to the policy, the primary curriculum was very dense. A government document from the time clarifying the policy added that a goal of the six year primary reform was “to alleviate the heavy burden of primary school students

and to improve teaching quality” by teaching this mandated material at a gentler pace over a longer period of time.¹²

To collect a richer account of what happened during this extra year and how it was perceived, we conducted a focus group discussion in Shandong Province with a team of middle school teachers and former students who were affected by the policy. Participants reported that in the first year or two after the reform, the content of the extra year consisted of a review of what was covered in the fifth year of primary school and the addition of elective courses, such as physical education and music. The teachers reported that, after this adjustment process, the primary school curriculum previously covered in five years was extended more smoothly over six years.¹³ In practice, this meant more time allowed for review and ensuring the foundational concepts of the primary curriculum were mastered by all students. Generally, respondents felt the extra year was most likely to have helped those of lower ability. More than half the respondents mentioned the loss of a year of productive work as the main downside of the policy.

The extra year of schooling posed logistical and personnel challenges. The policy required primary schools to hold and teach an additional cohort of children, but these schools were given no additional resources to do so. Both gazetteer records and our interviews indicated that the burden of housing and schooling this extra cohort in a given primary school involved assigning more work to existing teachers and dividing up existing facilities, as opposed to building new structures and hiring new staff. When asked about the effect of this extra burden on the quality of education, respondents generally thought it unlikely to have a substantial impact. This claim is consistent with the rote nature of Chinese primary education during this time, which we argue is likely to dampen a possible negative relationship between class size and learning. As we document later in the paper, this additional burden was gradually offset by a secular decline in cohort sizes over time.

The gazetteers document that the transition from five to six years of primary school was carried out in a number of ways. In Table A.1, we show six examples of gazetteers' reports of how the policy was enacted in different implementing cities and counties. In some cases, the transition was accomplished by enforcing the policy fully, forcing all students in affected cohorts to remain in primary school an extra year. In other instances, a portion of the exiting cohort of students was sent on to middle school after their fifth year of primary school while the rest remained to finish a sixth year. This practice was explained in the gazetteers as a method to smooth the flow of students during the first year or two of transition, after which all subsequent cohorts would then take six years.¹⁴

The decision of when to implement the policy was made at the local level. Though upper-level pressure certainly played a factor, as we discuss in Section 3, most counties had the ultimate say on the year in which the switch was made.¹⁵ In the next section, we describe how our identification strategy avoids the need to have random timing of implementation, and we address the other issues surrounding discretion in timing of implementation and the attendant concerns of omitted variable bias.

⁹ In China, the levels of schooling known as middle school and high school in the US are referred to as junior middle school and senior middle school. We refer to them here as middle school and high school for ease of exposition.

¹⁰ In Shanghai and a few other localities, this policy was implemented instead by requiring that middle school last four years instead of the usual three. There are no cases we are aware of in which the number of hours of school per day changed as a result of the policy.

¹¹ The policy did not change students' eligibility for middle or high school, or these schools' capacity to take students. Enrollment rates were also low during most of this period - in 1995 only 90% of students progressed to middle school and 50% progressed to high school - meaning that a missing cohort of primary graduates would have only a dampened effect on the flow of students to higher levels.

¹² Jiaoyubu guanyu quanrizhi liunianzhi xiaoxue jihuade anpai yijian, published August 15, 1984, accessed from www.pkulaw.com/chl/87934.html on February 13, 2019.

¹³ While this caused a temporary disruption to middle schools, as for one year there was an absent or halved entering class, no teacher in our interviews mentioned this disruption. From this, we infer that whatever disruption the policy caused to the operation of middle schools (and, by association, high schools) was temporary and secondary in importance to the experience of the students.

¹⁴ In the next section we note that this variety in transition methods will attenuate our effect estimates slightly, as it will lead to some misclassification of treatment status around the implementation year.

¹⁵ Local educational gazetteers document that, in most cases, counties within a prefecture implemented the policy in the same year or within a few years of each other.

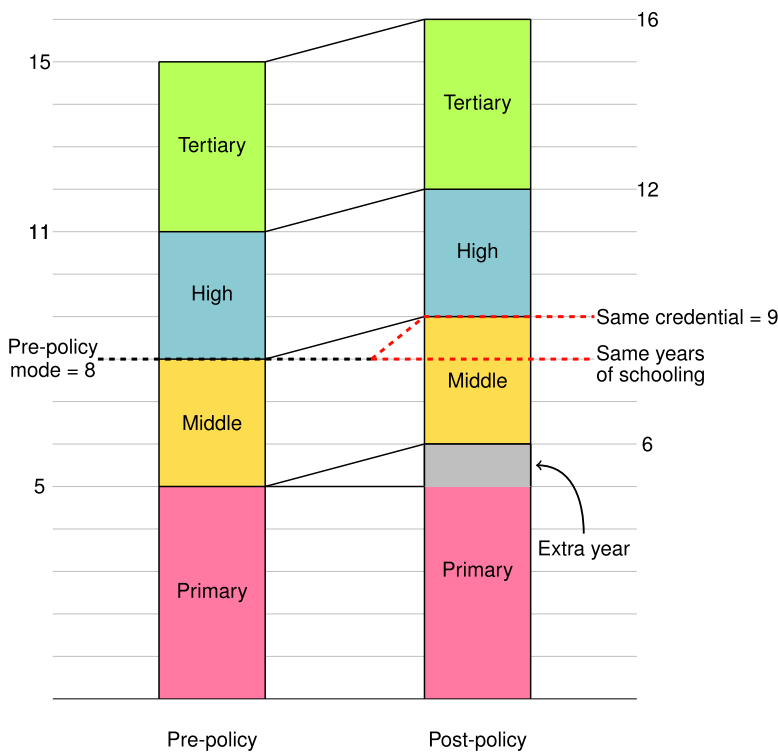


Fig. 1. Years of schooling to earn credentials, pre- and post-policy. This figure depicts the duration and structure of the Chinese education system before the policy is implemented in the left column and after the policy is implemented in the right column. The y-axis represents the number of years needed to complete a credential. Middle, high, and tertiary refer to junior high school, senior high school, and university, respectively. The thick lines connecting the left and right columns depict the change in the total number of years it takes to earn each credential as a result of the policy. The dashed lines show the trade-off that the modal student faces after the policy.

3. Research design

This section describes the research design of the paper. We describe our empirical strategy and discuss predictions for two main sets of coefficient estimates. We then provide an overview of the data sources we use alongside a description of how we identify where and when the policy occurred. We conclude the section by showing evidence that the main identifying assumptions for our research design are satisfied and addressing a set of issues which could confound causal interpretation of our results.

3.1. Empirical strategy

This section describes our empirical strategy to study the effect of the policy on schooling and labor market outcomes. Our identification strategy is a stacked regression discontinuity (RD) design with a discrete running/forcing variable (Lee & Card, 2008). We define our running variable as the locality-specific distance to policy implementation in years and months, that is, the number of years and months between an observation's birth year and month and the birth year and month of the first affected children in the locality.¹⁶ This choice of running variable is identical to that of Clark and Royer (2013) and Erten and Keskin (2018), among several others, and the method of stacking cutoffs similar to Pop-Eleches and Urquiola (2013) and Abdulkadiroğlu et al. (2014). This strategy compares outcomes of individuals finishing primary school just before the policy is implemented in a given locality (county or prefecture) to those in the same locality finishing primary school just after implementation. The gradual rollout of the policy across time and space allows us to make this comparison while controlling flexibly for locality and cohort-by-province fixed effects.

Following Imbens and Lemieux (2008) and Lee and Lemieux (2010), our main estimating equation is an ordinary least squares regression of Y_{iclp} , the outcome of interest for individual i in birth cohort c and

locality¹⁷ l in province p , on a short set of key regressors:

$$Y_{iclp} = \beta_0 + \beta_1 \text{Treated}_{cl} + \beta_2 (t_{cl} | t_{cl} \geq 0) + \beta_3 (t_{cl} | t_{cl} < 0) + \beta_4 V_i + \xi_m + \mu_l + \eta_{cp} + \epsilon_{iclp} \quad (1)$$

Here Treated_{cl} is an indicator variable equal to 1 if the individual finishes primary school in or after the first affected cohort in her locality. t_{cl} is the locality-specific distance-to-treatment (in month-by-year units) for cohort c . We estimate the coefficient on distance-to-treatment separately for treated and untreated groups to account for pre- or post-policy trends, e.g., the possibility that the effect may differ as time since policy implementation increases and counties get better at implementing the policy. This ensures that β_1 captures only the difference between pre- and post-policy means (Gelman & Imbens, 2014).¹⁸ V_i is a vector of predetermined characteristics which includes, at the individual level, gender, ethnicity, residence permit status, and urban/rural residence, which can vary within a county or prefecture. Birth month (ξ_m), locality (μ_l) and cohort-by-province (η_{cp}) fixed effects are also included in all specifications. Using this specification, β_1 gives the average treatment effect for a small window around the implementation year; in other words, the estimate applies to all individuals, not just compliers, but only those in a narrow age range such that they graduate from primary school within a limited time bandwidth around policy implementation.

All regression results we present use robust standard errors clustered at the level of the running variable bin (month-by-year distance to policy implementation), as recommended in Lee and Card (2008). We restrict our potential sample to cohorts leaving primary school between 1976 and several years before the sample is drawn (1995 in the census data and 2003 in the CFPS data) to give most individuals sufficient time to finish their schooling career

¹⁷ As mentioned earlier, locality refers to the county in the CFPS data and the prefecture in the census.

¹⁸ In Table A.1 we give six examples from gazetteers of how the policy is implemented. These demonstrate the need to control for the possibility of implementation varying over time.

¹⁶ Year of primary school entry + (month of primary entry - 1)/12, using the annual school entry cutoff month, September, to differentiate cohorts.

Table 1
Effects of the policy on schooling outcomes.

Outcome	CFPS		Census	
	(1) Urban	(2) Rural	(3) Urban	(4) Rural
Years of primary schooling	0.498*** (0.066)	0.625*** (0.067)		
Years of post-primary schooling	0.057 (0.498)	0.168 (0.191)		
Highest credential: at least primary school	0.0451 (0.0480)	−0.0087 (0.0344)	−0.0022* (0.0012)	−0.0010 (0.0027)
Highest credential: at least middle school	0.0333 (0.0306)	0.0347 (0.0397)	0.0008 (0.0031)	0.0008 (0.0048)
Highest credential: at least high school	0.0048 (0.0712)	0.0041 (0.0385)	0.0211*** (0.0075)	0.0027 (0.0028)
Dropped out of school, any level			0.0012 (0.0011)	−0.0020 (0.0021)
Number of observations	1,164	2,240	107,422	199,126

Each cell presents a treatment effect estimate from a separate regression (specification given in Eq. (1)) with the relevant robust standard error below, in parentheses. Standard errors are clustered at the county (CFPS) or prefecture (census) level. Columns 1 and 2 show results using CFPS data and 3 and 4 show results using census data. The dependent variable in rows 3–6 is coded as 0 = No and 1 = Yes.

before being observed.¹⁹ We then conduct an optimal bandwidth calculation using the cross-validation procedure recommended by Imbens and Lemieux (2008) and used in Clark and Royer (2013). This generates a suggested bandwidth of six years and six months on either side of the treatment threshold, which is the sample we use in all of our main results. For our main results (Tables 1 and 3) we also report results from the a smaller bandwidth - three years and five months - which was the second-best optimal bandwidth generated by the cross-validation procedure.

Another common strategy to use in this type of setting is a difference-in-differences (DD) design. In Appendix 5, we describe in depth why this strategy is inappropriate for our setting. The main reason is that, after reading many documents about the policy's implementation, it became clear that the parallel trends assumption necessary for a DD design is almost certainly violated in a way that will generate non-standard bias in estimates. On the other hand, the RD strategy protects against this in two ways: one, by limiting the window of time that any one locality can contribute to our estimation results, and two, by comparing treated and untreated only within localities and then aggregating this difference. We show in our appendix that a DD with a balanced panel generates results similar to ours, but a more standard DD with a larger time window does not for the reasons we describe.

3.1.1. Predictions

We have two main outcomes of interest: the effect of the policy on post-primary educational attainment, and its effect on labor market performance. Here we briefly discuss our predictions for these outcomes. For the vast majority of affected individuals, we anticipate that the extra year should not affect post-primary schooling decisions. As we will present in Fig. 4, prior to the policy's implementation there was an extensive amount of bunching at credential attainment years. This pattern suggests a high value of credential attainment, and we anticipate that the majority of students affected by the policy will choose to earn the credential they would have earned in the absence of the policy, even if it requires an additional year spent in school. For the labor market, we predict that students who leave school earlier in the cycle (e.g. after completing primary or middle school) will gain more from this additional year of learning and practicing the primary curriculum

than will students progressing further. This stems from the fact that students in this latter group have opportunities at higher levels of schooling to practice the skills they learn in primary school and build on them, and so are unlikely to reap as large of a benefit from an additional year of primary school as those who leave earlier. Furthermore, because the extra year of primary school is not visible to employers at time of hiring but should be visible through higher productivity on the job, we expect the main effects to be on wages and not employment.

3.2. Data sources

Our sources of data are listed in Table A.2. There are two main sources of observational data: the 2005 China 1% Inter-censal National Population Sample Survey (also known as the “mini-census”) and the 2010 wave of the China Family Panel Studies (CFPS).²⁰ The 2005 Chinese mini-census collects basic data on family structure, highest educational credential attained, health, and income, and contains 2.6 million observations.²¹ The CFPS is a nationally representative panel data set containing information from over 30,000 individuals in rural and urban China across 25 provinces, representative of 94.5% of China's population.²² Summary statistics on demographic, education, and employment characteristics of our sample population are given in Table A.3 for each data set, separately for rural and urban residents.

Most of our main results come from the census data. We include the CFPS as a complement, as it allows us to show a more detailed picture of how the policy was implemented within a given locality. We then use the large sample size of the census data to generate precise estimates of the coefficients we are after: the effect of the policy on educational attainment and labor market outcomes.

3.3. Determining treatment status

We also collect our own data from two sets of national archives to determine which observations in the census data were affected by the policy we study. The shift to six year primary school was implemented at different times in different places both across and within China's provinces, as shown in Figure A.2. We hired a team of research assistants to read through prefecture and county educational gazetteers stored in the Chinese National and Peking University Archives, to determine if, when, and how the policy was implemented in each locality.²³ Figure A.3 shows a page from one of these gazetteers.

In China, the largest geographical unit is the province, followed by the prefecture, followed by the county, then the township and, finally, the village. The census data is at the resolution of the prefecture, and we determine the year the policy was implemented, separately for rural and urban residents, in 280 of the 345 prefectures in the census. The gazetteers document that implementation sometimes occurred in a different year in the rural regions of a given county or prefecture than it

²⁰ We use only the first wave of the CFPS for two reasons: one, because we use the CFPS only for education results, and education levels do not change for the vast majority of those in our sample between waves. Two, younger individuals from subsequent waves are more likely to be affected by China's dramatic expansion of tertiary education in the early 2000's which, given its somewhat gradual rollout, is a large potential confound.

²¹ Though the full sample is approximately 13 million observations, researchers are granted access to 20% sub-samples of the parent dataset.

²² The data include all provinces but Tibet, Xinjiang, Inner Mongolia, Hainan, and Ningxia. The CFPS is conceived of as a panel, with six waves planned, taking place in 2010, 2012, 2014, 2016, 2018, and 2020. The project is organized by a team of economists and sociologists at Peking University and collects a rich set of data on family structure, income, expectations, and several other social and economic indicators. Detailed information about the sampling structure and overall plan for CFPS is available in Xie and Lu (2015).

²³ Recent work by) uses Chinese gazetteers to identify when land-reform policy was implemented in different counties across the country.

¹⁹ 1976 marked the end of the Cultural Revolution and the end of the chaos it brought to the educational system of China.

did in that locality's urban areas. We match these gazetteer accounts to the census data on household registration (hukou) status to generate different implementation years for rural and urban residents in a given prefecture.²⁴ In most cases, the policy is implemented contemporaneously or nearly contemporaneously across urban (and, separately, rural) parts of different counties within a prefecture.²⁵ In around 20 cases, the policy is rolled out so gradually across counties within a prefecture that it is implausible to assign a single policy year appropriate for the entire prefecture. We exclude these cases from our analysis. We also exclude prefectures that changed to a system of five years of primary school plus four years of middle school and prefectures that have no record of policy implementation in the currently available educational gazetteers. Of those 65 prefectures in the census we exclude, 45 either implemented the policy too gradually or instead changed to the 5 + 4 system. The remaining 20 had no record of implementing the policy in the currently available educational gazetteers.

In the CFPS dataset, the location of observations is anonymized to the province level, which prevents use of the gazetteers to determine treatment status. Instead, we use the detailed data the CFPS collects on how many years individuals spend in each level of schooling to identify the year in which the policy was implemented within each (anonymized) county. Among individuals living in a given county, we apply a mean-shift algorithm (Fukunaga & Hostetler, 1975) which generates the most likely cohort in which the number of years spent in primary school jumps from five to six.²⁶ Its implementation in our context is straightforward; for observations in a given county, we regress individual-level years of primary school on a constant and an indicator function for having graduated in or after a given year as given in Eq. (2):

$$s_i = \gamma_0 + \gamma_1 \mathbf{1}\{t_i \geq t^*\} + \epsilon_i \quad (2)$$

We chose a functional form with only a break and no pre- or post-trends based on both the descriptions of the policy implementation we read in the gazetteers and the patterns we saw in the county-specific histograms plotting years of primary school by cohort (such as that depicted in Figure A.4). We estimate 27 regressions for each county, corresponding to every possible treatment year in our data, $t^* \in [1981, 2007]$. In this equation, s_i is the number of years individual i spent in primary school, t_i is the year in which she graduated from primary school, and ϵ_i is an i.i.d. error term. The year (t^*) with smallest sum of squared residuals (ssr) is the predicted treatment year for that county.²⁷ This exercise generates a treatment year for each county in our estimation sample.²⁸ In Appendix 3, we use national statistics and the application of both archival and mean shift methods to a third observational data set, the China Labor-Force Dynamics Survey, to

²⁴ We classify rural/urban status using the individual's hukou, which is either agricultural (rural) or non-agricultural (urban).

²⁵ There are on average nine counties per prefecture. In some cases, one or two counties implement the policy one to three years before or after the majority of other counties. In these cases, we code the prefecture's policy implementation year as that of the majority of its counties. We can control for whether or not the policy was implemented gradually; doing so does not materially affect our estimates. This gradual implementation will lead to some misclassification of treatment status, which Lewbel (2007) shows may attenuate our effect estimates somewhat.

²⁶ This mean-shift approach is similar to that used Munshi and Rosenzweig (2013).

²⁷ An example of this process is shown in Figure A.4.

²⁸ Beginning with 162 counties in the CFPS, we exclude the 18 counties from Shanghai, as they implemented the policy by extending middle school instead of primary school. Of the remaining 144, we include only those 112 counties in which we can detect a clear policy change - in the gazetteers and histograms, we see that some counties in other provinces, especially Shandong, also use the 5 + 4 model. Appendix 3 lists the inclusion criteria used to determine this sample. Our empirical results are robust to using data from all 144 non-Shanghai counties.

corroborate the reliability of the mean shift method's identified treatment years.²⁹

Note that we use these estimated years to then estimate subsequent treatment effects for post-primary schooling in the CFPS data. We do not formally adjust for this in our calculation of standard errors of subsequent estimates. Our reasoning is as follows: doing so would generate larger confidence intervals than we currently have. Even without this adjustment, the CFPS confidence intervals are much larger than those for the census, and do not regularly reject zero effects. Therefore we rely primarily on the census results (which do not require this adjustment) to inform our interpretation, and assume that such an adjustment for our CFPS results would only intensify the pattern we see: that most are consistent with the inability to reject the null of zero impact of the policy on post-primary schooling.

3.4. Testing assumptions behind our research design

For causal interpretation of our results, we require that within our geographical unit of interest, there is continuity in the conditional expectation of the outcome variable across the assignment threshold (Lee & Lemieux, 2010).³⁰ This requirement contains two main conditions - one, that the policy did not cause a change in the predetermined characteristics of who appears in our sample (i.e., selection or attrition bias), and two, that no other policy occurred concurrently with the one we study.

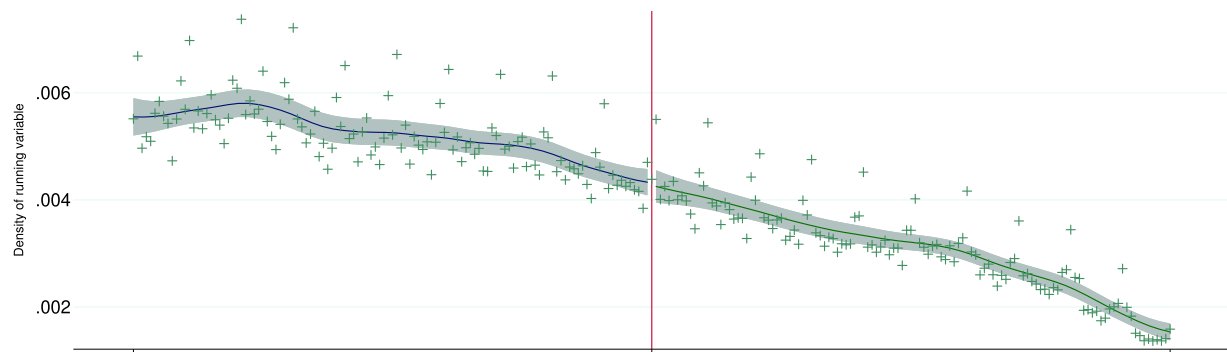
First, we address the potential for policy-related selection and attrition biases. This can be summarized by two questions: one, did the policy induce affected individuals to migrate or otherwise attrit from our sample? Two, might there be heterogeneity in such attrition across gender? To address these questions, we test for continuity in the conditional expectation across the threshold of treatment assignment of three fundamental predetermined characteristics: the density of the running variable, gender composition of cohort, and proportion of individuals with a household registration certificate (hukou) from an urban area. As recommended by McCrary (2008), in Fig. 2 we plot the density of the running variable, testing for bunching on one side of discontinuity and failing to reject the null of no bunching. Furthermore, as recommended by Lee and Card (2008), we use our main regression equation to estimate the "effect" of the treatment on the three predetermined variables for each dataset. These results are given in Table A.4; in all cases we fail to reject a zero effect.

There is little evidence of overall absence due to migration: relatively few individuals - 6.83% of our census sample - are observed away from the place where their hukou (government-issued household residence permit) is registered. We exclude these observations, as their treatment status is more difficult to pin down because we do not have data on precisely when they moved or how long they have been staying in their current locality. We include individuals who have ever migrated and who are observed in the county/prefecture that their hukou is assigned to (in other words, migrants within localities and migrants who have either temporarily or permanently returned). We use Eq. (1) to estimate the effect of the policy on the propensity to have ever migrated among those in CFPS sample, and we find no evidence of an effect ($\beta_1 = 0.00986$, $\sigma = 0.00726$).

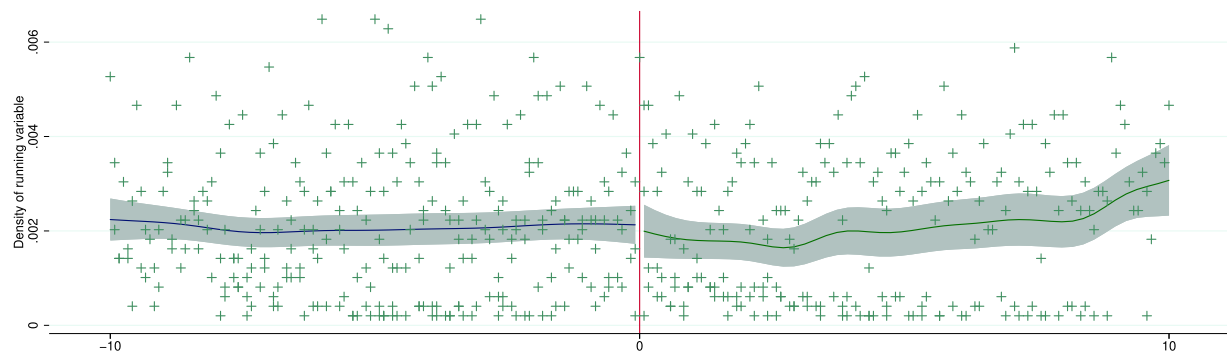
The second concern is omitted variables bias from the other policies implemented over the period of our study. Many economic and policy

²⁹ The China Labor-Force Dynamics Survey (CLDS) is a panel survey similar to CFPS. We use it to corroborate the reliability of the Mean Shift method we use on CFPS. We do not use the CLDS for our main regressions as it has neither the large sample size of the census nor the fine-grain locality information of the CFPS (CLDS indicates only which prefecture individuals are in, not which county).

³⁰ As we are comparing within prefectures and counties, we do not need the timing of the policy to be randomly assigned across localities (Black, Devereux, & Salvanes, 2005; Meghir & Palme, 2005).



Panel A: Census data



Panel B: CFPS data

Fig. 2. Density of running variable. This figure plots the density of the running variable (distance in time, measured by months and years, to treatment year/month) for the CFPS and census data. Corresponding regressions testing for continuity in the expectation of the treatment variable, proxied for by predetermined characteristics, are provided in Table A.4 and fail to reject the null of continuity.

changes occurred in the 30 year window we study. While some of the observations in our sample were undoubtedly affected by these policies, our identification strategy protects against the risk that they systematically bias our overall results. Specifically, we compare only observations within the same locality immediately before and after the policy implementation year, and aggregate this difference across many different localities with different implementation years over the period of our study. As we exploit the staggered rollout of the policy over 25 years, such external policies implemented at the national level should only attenuate our effect estimates, affecting some individuals treated by the policy we study and others who are not, and thus introducing more statistical noise (Lee & Card, 2008; Lewbel, 2007).

The remaining risk in this vein is that some other policy had the same staggered rollout pattern as the one we study and was coordinated across the same counties and prefectures to be contemporaneous with ours. If this were the case, our treatment effect estimates would conflate the impacts of the two policies. We conduct a series of analyses to examine the possibility and scope for such bias, and conclude from the evidence summarized below that it is highly unlikely that there was concurrent, coordinated implementation of another policy which would violate our exclusion restriction.

First, in our main estimating equation we include cohort-by-province fixed effects to flexibly control for potential province-specific time trends. Second, we see no evidence of such coordination either in the historical record or the mapped implementation years. Our coding of the gazetteers also uncovered no mention of a policy or external influence that was regularly coincident with implementation of the six year primary policy. Given the official nature of these documents, this strongly suggests

absence of a consistent, officially-sanctioned confounding policy. The geography of the timing of implementation in each of China's prefectures according to archival records displays no such pattern; Figure A.2 provides a heat map of prefecture implementation years, with lighter shades indicating earlier implementation. In addition, we find no evidence of a statistically significant correlation between the timing of implementation and the proportion of individuals working for the government in a given prefecture, a test for the possibility that timing of implementation was correlated with some unobserved government-driven labor market condition.

Two other major education policy changes occurred during this period and deserve separate attention. First, a separate policy issued in April 1981 by the Ministry of Education mandated that the duration of high school to be extended from two years to three by the end of 1985. This implementation occurred over a much shorter time frame than the extension of primary school from five to six years: by 1984, 90% of students in high school were in three year programs; in contrast, it was not until 2003 that more than 90% of primary school students were in six year programs (National Institute of Education Sciences, 1984). Second, in 1986, the Chinese government made middle school compulsory. We show in Appendix 4 that this law appears to have little impact on the middle school attainment of observations in our estimation sample. We argue this is likely due to two main factors. One, there is widely documented porous enforcement of the law in rural areas (Fang, Eggleston, Rizzo, Rozelle, & Zeckhauser, 2012). Two, in urban areas education levels are already high at the time of the policy announcement and so the law is likely to bind only for relatively few urban residents.

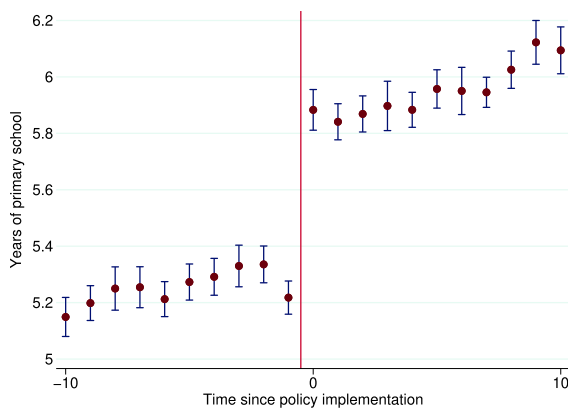


Fig. 3. Event study of years of primary school before and after policy change. This plot uses CFPS data to show distance-to-treatment-year bin means of the number of years affected and unaffected individuals spent in primary school. The vertical line separates the affected (to the right of the line) and unaffected (to the left) cohorts, stacking the different policy implementation years at 0. We plot this in year bins instead of month-by-year bins for ease of viewing. Figure A.5 is an analogous figure using month-by-year bins instead, consistent with what we use in the regressions.

4. Empirical results - educational attainment

In this section, we estimate the impact of the policy on educational attainment. First, we show that the policy was indeed effective at extending the number of years individuals spent in primary school from five years to six. We then estimate the impact of this change on subsequent schooling outcomes, including years spent in post-primary schooling, whether or not an individual attains primary, middle, and high school credentials, and drop-out. We finish this section looking at the effect of the policy on subgroups of interest and on the characteristics of individuals by the highest educational credential earned. In this section and the next, we estimate results separately for urban and rural areas, following convention from previous work on schooling and the labor market in China and reflecting the fact that the labor market and other relevant parameters for schooling decisions differ systematically between the two (Liu & Zhang, 2013). As described in Section 3, we classify rural/urban status using the individual's hukou (household registry permit), which is either agricultural (rural) or non-agricultural (urban).

4.1. Primary schooling

We first examine whether the policy achieved its desired effect of increasing primary school for affected individuals. Fig. 3 plots distance-to-treatment bin means and confidence intervals for the number of years spent in primary school among individuals in our CFPS sample. Prior to implementation of the policy, the mean is between 5.2 and 5.3, that is, between 20 and 30% of this sample spends more than the required five years in primary school. This comprises mainly individuals performing poorly in school who were made to repeat a grade.³¹ At the policy implementation year the mean jumps to nearly 5.9, increasing to just over six years by the end of the ten year period included here. Results from the regression analog to this exercise are presented in the first row of Table 1.

We estimate that the policy causes a 0.498 year increase in the number of years spent in primary school for urban residents who finish primary school within our optimal bandwidth sample restriction. For

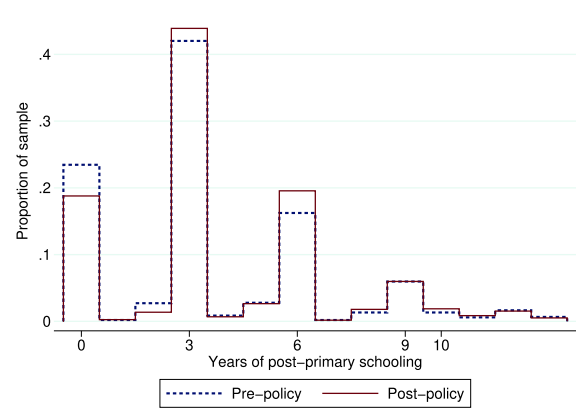


Fig. 4. Distribution of post-primary schooling before and after policy change. This figure shows the probability mass functions of post primary schooling for observations in the CFPS data restricted to our optimal bandwidth/estimation sample. Note that the bunches at 3, 6, 9, and 10 correspond to middle school, high school, technical college, and university credential attainment years, respectively.

rural residents, the increase is 0.625. The smaller bandwidth sample generates nearly identical results.

There are several reasons why we do not estimate an immediate jump to six years of primary school. First, according to data from gazetteers and interviews, the policy was often rolled out in a way that split one or two cohorts of students in half to ease the transition, e.g., in the first year, sending half of fifth graders on to middle school, while retaining the other half in primary school for an additional year. The second reason is that we retain in our sample some counties where the fidelity of implementation appears to be less than perfect. If we include only those counties where the mean shift test precisely identifies a break, this estimate comes close to 0.8. Finally, as we are using recall data asking about an event that occurred between 5 and 25 years prior to the time of data collection, we anticipate some measurement error in estimates of when the shift happened.

To be conservative, we present reduced form estimates of the effect of the policy on subsequent outcomes as our central results, and give the instrumental variable (IV) coefficient estimates in parenthetical notes for the main labor market comparisons of interest. Our motivation for this is as follows: a sixth year of primary schooling post-policy is a deliberate expansion of the primary curriculum, as opposed to a forced repetition of the fifth year of primary school. Even for those who would have spent six years of primary school under the old system, the nature of the sixth year of primary school changes dramatically with the implementation of the policy. In addition, taking the first stage from the CFPS data and applying it to the census, which includes data from a more representative sampling of counties as well as from five provinces not included in the CFPS, would involve a greater deal of uncertainty than we prefer.

To generate these parenthetical IV calculations, we divide the urban reduced form estimates by an adjusted version of the “first stage” given in the first row of Table 1. We show in Table 2 that the first stage estimates do not differ meaningfully between men and women, and so when estimating IV coefficients we use the same first stage estimate for both. We assume the vast majority of children reporting six years of primary school after our imputed policy implementation date are under the new system and the vast majority of those reporting six years of primary school before this date are under the old system. This suggests that in order to generate our first stage estimate, we should add the pre-policy rate of completing six years of schooling, which is 0.278 in urban areas (0.272 in rural areas), to our estimated coefficient of the impact of the policy on completed years of primary school. This yields a first stage of 0.776 in urban areas and 0.897 in rural areas or, equivalently, guidance to multiply the urban reduced form estimates by 1.29 and the

³¹ Data from the baseline wave of the China Education Panel Survey (CEPS), a new, nationally representative dataset collected by scholars at Renmin University of China, corroborate this claim. In the CEPS dataset, approximately 16% of surveyed individuals repeated at least one year in school.

Table 2
Heterogeneity, by gender, in the effect of policy on education outcomes.

Outcome	CFPS		Census	
	(1) Urban	(2) Rural	(3) Urban	(4) Rural
<i>Years of primary schooling</i>				
Treated - male	0.5143*** (0.0695)	0.6591*** (0.0786)		
Treated - female	0.4811*** (0.0780)	0.5836*** (0.0647)		
<i>Years of post-primary schooling</i>				
Treated - male	0.0135 (0.5458)	0.1352 (0.2146)		
Treated - female	0.1047 (0.5354)	0.2081 (0.2126)		
<i>Graduated from primary school</i>				
Treated - male	0.0383 (0.0540)	−0.0126 (0.0376)	−0.0024* (0.0013)	−0.0079*** (0.0028)
Treated - female	0.0525 (0.0520)	−0.0041 (0.0394)	−0.0020 (0.0013)	0.0054* (0.0050)
<i>Graduated from middle school</i>				
Treated - male	0.0533 (0.0334)	0.0238 (0.0450)	−0.0040 (0.0034)	−0.0230*** (0.0057)
Treated - female	0.0115 (0.0350)	0.0478 (0.0459)	0.0052 (0.0033)	0.0229*** (0.0050)
<i>Graduated from high school</i>				
Treated - male	0.0240 (0.0820)	0.0104 (0.0432)	−0.0023 (0.0085)	0.0037 (0.0032)
Treated - female	−0.0161 (0.0755)	−0.0033 (0.0417)	0.0422*** (0.0077)	0.0017 (0.0028)
<i>Dropped out of school, any level</i>				
Treated - male			0.0012 (0.0011)	−0.0001 (0.0023)
Treated - female			0.0013 (0.0012)	−0.0038 (0.0024)
Number of observations	1,164	2,240	107,422	199,126

This table shows our gender-specific estimates of how the policy affected educational attainment. We replace the single treatment variable with two variables - a dummy for being male interacted with the treatment and a dummy for being female interacted with the treatment. We report these two coefficients only for each dependent variable (i.e., there are either four or five separate regressions presented in each column). Columns 1 and 2 show results using CFPS data and 3 and 4 show results using census data. All dependent variables not referring to years of schooling are coded as 0 = No and 1 = Yes.

rural estimates by 1.11 to generate the relevant IV estimates.

4.2. Post-primary schooling

This policy was implemented in each locality at a time when over 75% of students went on to get at least some post-primary schooling. The majority of affected individuals could potentially hold total years of schooling constant by offsetting the additional year of primary school with one less year of post-primary school, as depicted for the modal student in Fig. 1. In this subsection, we study this choice and, more broadly, how the policy affected completion of post-primary schooling.

Fig. 4 shows the distribution of post-primary schooling among individuals in our optimal bandwidth sample, separately for those exposed (post-policy) and unexposed (pre-policy) to the policy we study. This figure captures the key intuition behind our estimates of the effect of the policy on post-primary schooling: there is extensive bunching at credential attainment years for both treated and untreated groups, with a small difference between the treated and untreated groups in the location of this bunching.

The regression results for our schooling outcomes are given in the rest of Table 1, providing a formal analysis of the patterns we observe in Fig. 4. The second row shows our estimate of the effect of the policy on

post-primary years of schooling to be 0.057 in urban areas (0.168 rural), neither of which are statistically significant at traditional confidence levels. We interpret these estimates as evidence of a zero effect of the policy on years of completed post-primary schooling. This implies that the vast majority of Chinese citizens induced by the policy to attend an extra year of primary school chose not to offset this with less post-primary schooling.

We next use the census data to examine the effect of the policy on credential attainment. The census has coarser data on educational achievement (only highest credential attained, not years spent in each level of schooling) but is two orders of magnitude larger than the CFPS data. In the third, fourth, and fifth rows of Table 1, we estimate the effect of the policy on whether or not an individual earns at least a primary, middle, or high school credential, using both census and CFPS data. The effect of the policy on primary school completion is very close to zero and largely insignificant, with marginally significant evidence that it may have caused a slight decrease in primary school completion in urban areas. Our estimates show no evidence of a change in completion of middle school; for high school, the census data suggests that there was a slight increase in completion in urban areas (but not rural) as a result of the policy.³² Using the census data, we find no effect of the policy on the pooled probability of dropping out of any level of schooling, shown in the final row of Table 1. Here again, the estimates we generate using the sample restricted to the alternative optimal bandwidth of three years and five months approximate those presented in Table 1.

4.3. Effects by gender and testing for changes in composition

Next we estimate the effects of the policy by gender of affected individuals and test for a change in the composition of the types of individuals who earn each credential. The motivation for these tests is that the small estimates in Table 1 could mask two countervailing phenomena: first, some individuals advancing further than they would by virtue of the skills gained in the extra year, and second, others reducing post-primary schooling by an entire credential. We perform two exercises to investigate this possibility. First, we present gender-specific estimates of the results in Table 1; second, we explicitly test for changes in composition of background characteristics at each level of schooling.

Table 2 shows treatment effect estimates for the same outcomes examined in Table 1, but with a slightly different format. We replace the single treatment variable with interactions between the treatment variable and a dummy for membership in the mutually exclusive and exhaustive subgroups of interest, i.e., male and female, and excluding the un-interacted treatment variable from the equation. We find that the policy has a small negative effect on the completion of primary and middle school for boys in rural areas.³³ Importantly, this is a decrease from high completion levels: the baseline completion rates of primary and middle school for rural boys are 0.94 and 0.66, respectively. For urban girls, we see a positive impact on completion of high school, again from a high baseline (0.55). We see no significant effect on the probability of dropping out.

While the CFPS and census estimates largely align, in the cases where they do not, e.g., high school graduation for urban girls, the (much more precise) point estimates generated using the census data fall well within the confidence interval around the CFPS point estimate. For interpretation, we place more weight on the census estimate because of the data set's higher level of representativeness and the greater precision of the estimate.

Next, we estimate a version of our main empirical specification to

³² We acknowledge that this lack of increase may have been a desired effect of the policy.

³³ The effects for urban boys are an order of magnitude smaller and generally not statistically significant.

test for compositional changes. We replace the single treatment variable with four dummy variables for the treatment interacted with an individual's highest educational credential (primary, middle, high, or tertiary). We present three tests - whether the policy affects the number of siblings, paternal education, and maternal education of individuals holding each credential.³⁴ We estimate this in the same way as we estimate the heterogeneous effects presented in Tables 2 and 4, interacting the treatment with each subgroup and omitting the un-interacted variable. Our results are given in Table A.5. The estimates show only one statistically significant coefficient out of 12 estimates, the magnitude of which is also small. As a whole, these results are consistent with chance and suggest that the treatment does not materially affect the characteristics of who holds which credential.

There are large and dynamic economic and social differences between regions in China (Kanbur & Zhang, 2005; Zhang & Zou, 2012). Due to these disparities, another obvious dimension of interest for heterogeneity analysis is intra- and inter-regional heterogeneity. In the robustness appendix, we describe the literature attempting to measure these differences. We then explain how our choice of identification strategy prevents precise estimation of heterogeneity on these dimensions.

5. Empirical results - the labor market

In this section, we estimate the effects of the six year primary education policy on affected individuals' labor market outcomes. We show our main results, discuss robustness and heterogeneity, and then conduct a cost-benefit analysis of the policy over the period 1981–2050.

In the main results of this section, we restrict our attention to urban residents. This restriction follows the vast majority of empirical work on the returns to schooling in China, for example, Li (2003), Li, Liu, and Zhang (2012), and most of the studies included in Liu and Zhang (2013)'s 2013 meta-analysis. This body of work motivates the focus on urban results with several claims, including 1) in rural areas treatment effect estimates would be muddled by the difficulty of measuring productivity in agriculture using existing household survey data, 2) in rural areas, schooling is likely to also affect labor allocation decisions on the intensive margin, i.e., how much time to spend working in agriculture vs. in the non-agricultural labor market, and 3) in rural areas, there is greater concern about selective loss to migration. For completeness, we present our estimates for rural residents in Table A.6.

Since the policy had economically small but sometimes statistically significant effects on the proportion of people who earn middle school and high school credentials, we present two versions of our main results. The first uses Eq. (1) as written; the second adds fixed effects for the highest credential earned so that β_1 captures the change in labor market outcome, conditional on highest credential, that accrues as a result of the policy.

5.1. Main labor market results

We use Eq. (1) to estimate the effect of the policy on three labor market outcomes: one, employment status; two, if working, whether the individual is employed by the government; and three, monthly income. As described in the predictions in Section 3, our main dependent variable of interest is the natural logarithm of monthly income,³⁵ though we present the employment results first for ease of exposition.

³⁴ We use the child's number of siblings to proxy for resources allocated to the child, and her/his parents' highest credential (mother's and father's separately) to proxy for scholastic ability.

³⁵ When estimating the effect of the policy on income, we drop those 340 observations from the optimal bandwidth estimation sample (142 in the treatment group, 198 in the control; out of 106,549 observations) who are working but report zero monthly income.

We use the 2005 mini-census data for all of the analyses in this section due to its large sample size.³⁶

We present regression results for the urban sample in Table 3. We find no evidence that the policy had any effect on whether or not an individual is working, with a treatment effect very close to zero (0.4 percent, from a treated group mean of 77.6 percent; estimate shown in column 1) and a confidence interval which rejects anything larger in magnitude than a 1.6 percentage point increase or a 0.8 percentage point decrease in this probability. In column 2, we present our estimate of the effect of the policy on whether the individual works for the government (as opposed to for the private sector or in a state-owned enterprise). We observe a 1.9 percentage point increase, from a baseline of 28 percent. This decreases to 1.4 percentage points when we control for credential fixed effects.

Next, we estimate the effect of the policy on log monthly income. These data come from the response to the census question: "in the last month (or calculating monthly income from last year's annual income), what was your overall income?" We estimate an increase of 2.63% in monthly income, statistically significant at 95% confidence.³⁷ We plot the event study graph with a locally estimated polynomial smoother (estimated separately for the treated and untreated groups) in Figure A.6. This shows a similar pattern - a discrete jump at the threshold of approximately 2.5 log points. When we control for credential fixed effects, the estimate drops to 2.0%, suggesting that about one fifth of the gain in wages came from the change in credentials, with the other four fifths coming from the returns to an additional year of schooling, controlling for highest credential. The IV estimate for these results is a 3.39 percent increase in income, or a 2.58 percent gain after controlling for credential fixed effects.

We next conduct a permutation test to examine whether our research design would mechanically generate a difference between the treated and control groups unrelated to the effect of the policy. In this test, we generated 1,000 draws of randomly selected years for each prefecture (sampled from the full support of the estimation sample's potential years, 1981–1997). Then, using the treatment status assigned by these placebo years, we estimate the placebo treatment effect on wages for each draw. Fig. 5 gives the probability density function for these estimates. The placebo treatment effect estimates are normally distributed, with a mean of 0.00027 and a standard error of 0.0099, putting the true estimate of 0.0263 well beyond two standard deviations from the mean. We conclude that the sign and significance of our estimates are not merely a mechanical result of our research design.

As in all exercises attempting to estimate the labor market returns to a year of schooling, we face the problem that an individual with one more year of schooling has one less year of experience in the labor market, and so our estimate may capture the returns to a year of schooling minus the returns to a year of experience. We calculate upper and lower bounds on this potential contribution, as in Manski (2013). Our lower bound is 0 if we assume that the labor market returns to a year of age are entirely due to maturity and not to work experience specifically. To calculate the upper bound, we estimate the return to a year of experience in the labor market at a given age, δ_a , and weight this by the age composition of our estimation sample ω_a , e.g., $\sum_{age=a} \omega_a \delta_a$.³⁸ This generates an upper bound of 0.0168, meaning that

³⁶ Though China was strictly a command economy as recently as 1978, reforms enacted in the 1980s and 1990s pushed the Chinese labor market to more closely resemble that of a market economy as early as the late 1990s (Cai, Park, & Zhao, 2008). By the time we observe individuals in 2005, we expect most workers to earn wages that are at least strongly correlated with their relative productivity (Zhang, Zhao, Park, & Song, 2005).

³⁷ For rural residents, our estimate is statistically insignificant and the point estimate is less than 0.1%.

³⁸ This weight ω_a is calculated by dividing the number of people of a given age a by the total number of people of working age (both as of 2005, the year of our census data), according to our census data.

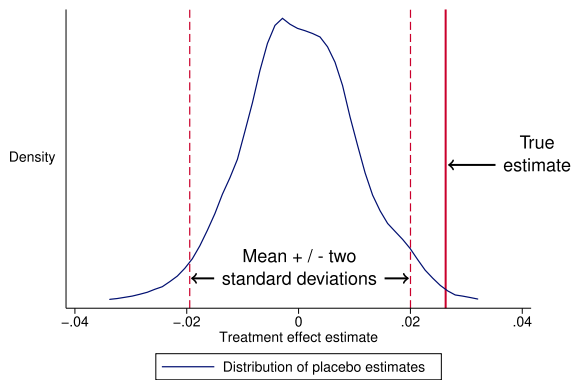


Fig. 5. Permutation test results. Data source: census. This figure plots the distribution of effect estimates of the placebo treatment on log monthly income from 1,000 draws of placebo years, using Eq. (1). We also show a confidence interval of two standard deviations from the mean. For reference, the overall effect from Table 3 is shown as a thick, solid vertical line, labeled as the “true estimate”.

Table 3
Effects of the policy on labor market outcomes.

	(1) Currently employed	(2) Works for government	(3) Log monthly income
Treated (β_1) (no credential fixed effects)	0.004 (0.006)	0.019*** (0.007)	0.026** (0.011)
Treated (β_1) (with credential fixed effects)	−0.000 (0.006)	0.014*** (0.006)	0.020* (0.010)
Number of observations	106,549	82,607	82,267

Data: census. All samples include only urban residents and non-migrants. Robust standard errors are given below the coefficient estimate in parentheses and are clustered at prefecture level. Estimates are generated using Eq. (1). Columns 2 and 3 use only those observations in our sample who are currently employed.

the Manski bounds on our overall estimate for the effect of the policy on income are (0.0263, 0.0431). In the robustness appendix, we present exploratory results to try to disentangle whether the policy affected cognitive and non-cognitive skills of affected individuals.

5.2. Heterogeneity in effects

We next explore heterogeneity in treatment by subgroups, shown in Panels A–C of Table 4 and Fig. 6. The coefficients in a given panel are estimated jointly, using the exhaustive set of interacted effects and excluding the un-interacted treatment variable from the equation as in Table 2. For the estimates in Panels B and C, we also include credential fixed effects for reasons described above. In each panel, we report all relevant group \times treatment coefficients.

First, we investigate effect heterogeneity by the highest credential an individual holds. Given previous work on the distributional effects of extra instructional time (Dobbie & Fryer, 2013; Meghir & Palme, 2005), we anticipate larger gains for those with lower credentials. As predicted, the coefficient estimates reported in Panel A of Table 4 show that the income gains from the policy are monotonically decreasing in highest educational credential. For exposition, we plot the relevant coefficient estimates and their confidence intervals in Fig. 6. This pattern is consistent with the goals of the policy, and imply that the policy is progressive: all who are affected give up a year of earnings, and those with the least education reap the largest gains in later life income. Note that the interpretation of these results is clouded somewhat by the fact that we observe a positive effect of the policy on high school credential

Table 4
Subgroup effects of the policy on labor market outcomes.

	(1) Currently employed	(2) Works for government	(3) Log monthly income
<i>Panel A: by highest credential</i>			
Primary school	0.018 (0.016)	0.021* (0.013)	0.085*** (0.030)
Middle school	−0.001 (0.007)	0.012** (0.006)	0.051*** (0.013)
High school	0.009 (0.007)	0.030*** (0.007)	0.028*** (0.011)
Post-secondary	−0.010 (0.007)	0.000 (0.008)	−0.014 (0.010)
R-squared	0.124	0.275	0.394
Number of observations	106,114	82,488	82,149
<i>Panel B: by gender</i>			
Female	0.012* (0.007)	0.022*** (0.007)	0.040*** (0.011)
Male	−0.013** (0.006)	0.006 (0.006)	0.002 (0.011)
R-squared	0.124	0.275	0.394
Number of observations	106,114	82,488	82,149
<i>Panel C: by employer</i>			
Private sector	–	–	0.049*** (0.012)
State-owned enterprise	–	–	0.010 (0.012)
Government	–	–	−0.021** (0.011)
R-squared	–	–	0.399
Number of observations	–	–	82,149

Data: census. In each panel, we present coefficients from a single regression generated using a modified version of Eq. 1 to generate estimates of heterogeneity across groups as defined in the panel title. Presented coefficients are for dummy variables for membership in the group given in left column (e.g., those whose highest credential is primary school) interacted with the treatment dummy. Robust standard errors are given below the coefficient estimate in parentheses, clustered at prefecture level. Panel A coefficients, when weighted by proportion of the sample corresponding to the proportion of each group among urban residents in the census, sum to 0.0209.

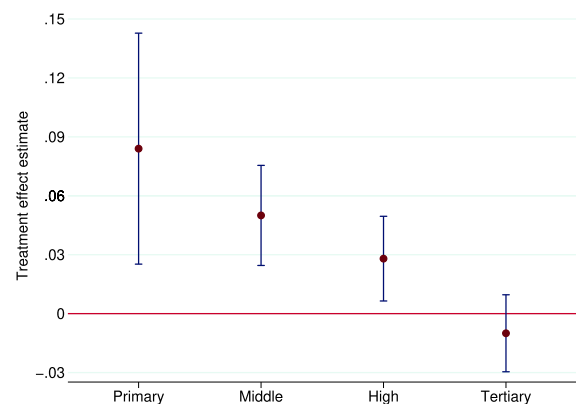


Fig. 6. Effect of the policy on log monthly income, by highest credential. This figure plots the treatment effect estimates and corresponding 95% confidence intervals for the effect of the policy on log monthly income, conditional on highest credential. Treatment effect estimates and standard errors are presented in Panel A of Table 4, along with relevant details on the regression specification. We cannot reject equality of the estimated effect of the policy on years of schooling, propensity to drop out of school, and receipt of credential for the four different credential groups (primary, middle, high, tertiary).

attainment for urban girls. Nonetheless, these patterns suggest the policy is associated with a large gain in welfare for those holding lower credentials.

Panel B of Table 4 shows that the policy slightly increases the proportion of women who are working and increases the proportion who work for the government. We estimate a four percent income gain for women in column 3. While government work is better paid than private sector work in our data, the income gain for women is not driven by a change in employment type. After controlling for type of employer, women still reap a 3.8 percent income gain from the policy.³⁹

Panel C shows that the income of private sector workers affected by the policy is five percent greater than those who are unaffected. Employees of state-owned enterprises gain little from the policy, however, and employees of the government appear to pay an income penalty for having spent an extra year in primary school as a result of the policy. This difference is unsurprising, as pay is almost certainly more closely linked to the relative productivity of labor in the private sector than in the government (Li, Li, Wu, & Xiong, 2012). Independently run Wald tests reject equality of the private sector coefficient with that of either other group.

Behind both the positive effects for women's income and the null overall effects for men's income is the same variation by subgroup we see in Panel A - returns to the policy estimated separately for each gender monotonically decrease by highest credential, with large returns for primary and middle school credential holders and smaller or negative returns for those with high school or tertiary credentials. We also see larger gains among those working in the private sector for both men and women.⁴⁰ These two sets of analyses, however, generate wide confidence intervals which include zero, speaking to the limitations of this research design. Comparing the treated and untreated, within subgroups of subgroups in each locality, limited to a narrow bandwidth around the treatment year, leaves us with too few observations per locality to generate precise estimates using the RD design as specified.

5.3. Cost-benefit analysis

We next use our results to generate an estimate of the costs and benefits of the program. We borrow our framework directly from Duflo (2001), focusing on the direct private gains and losses and ignoring the other potential benefits of increased income (e.g. decreases in fertility and child morbidity). Though we are aware that this type of exercise involves a precarious amount of uncertainty (Manski, 2013), we believe it is important to try to gain some insight into the net effect of such a large reallocation of resources.

As in Duflo's analysis, we choose our time frame to span from the first cohort in which some students leaving primary school are affected by the policy, 1981, to the end of 2050, and we use the same annual discount rate, 5%, as used in Duflo (2001).⁴¹ Our cost estimate has as its sole argument the lost year of wages⁴², w_{it} , that affected students i forgo during the year t they spend in school instead of in the labor market. We multiply this cost by θ , the proportion of individuals induced to spend an extra year in school because of the policy. We add this scale factor to account for the fact that, in our optimal bandwidth sample, about 25% of students spend a sixth year in primary school under the five year system and only 5% spend a seventh year in primary school under the six year system. Given these figures, we set $\theta = 0.8$.

$$Cost = \theta \sum_i \frac{w_{it}}{(1+r)^{t-1}} \quad (3)$$

For each cohort, we determine what proportion of individuals leave

school with a primary, middle, high school, or tertiary credential. We calculate the value of the year of wages the individual forgoes as the wages they would have earned with zero years of experience and the highest credential they ultimately obtain in the year they earn that credential. For example, an individual born in 1975 would start school in 1981. If her highest educational credential is a middle school degree and she was affected by the policy (and so she spent nine years in school), her "lost year" would be worth the average wages of those obtaining a middle school degree in 1990 in their first year of work. We then calculate the total value of the years lost for all students in each cohort from 1981 to the last cohort entering the labor force in 2050, using the same formula for the value of wages used in the benefit calculation below. Unlike Duflo, we do not incorporate a deadweight loss of taxation, as we see the policy as a transfer and assume there is no productive activity displaced by the policy other than the reallocation of affected individuals' time.⁴³

Our structure for estimating the benefit of this policy is also taken directly from Duflo's analysis. Specifically, we estimate the sum of income gains for all affected cohorts over the time frame we have chosen:

$$Benefit = \sum_t \sum_c \frac{\alpha GDP(t) S(c, t) P(c) \beta_1}{(1+r)^{t-1}} \quad (4)$$

Here α is the share of labor in GDP,⁴⁴ $S(c, t)$ is the size of cohort c divided by the total working population in year t . $P(c)$ is the proportion of cohort c affected by the policy,⁴⁵ and β_1 is our estimated effect of the policy on income. We sum the benefits earned by each cohort in the labor force in each year, assuming people work from when they leave school until age 65.⁴⁶

In Table 5, we present four cost-benefit estimates for the period 1981–2050, varying two key assumptions about the nature of β_1 . In the left column, we present estimates using the average treatment effect estimate of β_1 , while the figures in the right column use credential-specific estimates (i.e., different β_1 for primary, middle, high school, and university degree holders, respectively, as seen in Panel A of Table 4) weighted by the proportion of individuals in a given cohort holding each credential. The right column thus accounts for the large, positive changes in educational attainment over this time, using credential-specific treatment effects and allowing the distribution of highest educational credential to vary by cohort. In the top row, we present estimates using our urban sample, and in the bottom row, our results are the weighted sum of estimates from rural and urban China generated in separate regressions, again weighted by the cohort-specific ratio of rural to urban residents. This accounts for the fact that returns are lower in rural China and, until 2011, more than half of China's population was rural. Estimates for β_{rural} are given in Table A.6.

The sign of the estimate depends on how we treat rural areas. Using

³⁹ Results not in table but available from authors.

⁴⁰ Results available from authors.

⁴¹ We make a few additional assumptions related to the life cycle: one, that the extra year of primary school does not induce individuals to remain in the workforce for longer at the end of life; two, constant life expectancy; and three, fixed gender participation rates in the workforce.

⁴² In Appendix 6 we discuss other potential costs and benefits and our decision not to include them in this calculation.

⁴³ The new curriculum may require additional teacher training, but because we do not observe this and teachers made no mention of it in our qualitative work, we do not attempt to estimate the cost of such training.

⁴⁴ This labor share data come from Karabarbounis and Neiman (2014). The rest of the data used in this section was downloaded from stats.gov.cn and the World Bank's World Development Indicators, projected forward using multi-year moving averages.

⁴⁵ This is a slight over-estimate of the benefits accruing to cohorts in the first few years of implementation, as we derive this proportion affected measure from national statistics and not local gazetteers. Given the patterns of implementation we observe in the gazetteers, however, gradual implementation applies to a minority of localities. Among those cases, the vast majority reach full implementation in one to three years.

⁴⁶ This is a simplifying assumption. For those working in factories, the official retirement age is 60 for men and 50–55 women, but individuals often work well beyond these ages. In addition, the official age is slated to be changed in the next five years. Using the official retirement age would reduce the amount of years during which benefits accrue and thus reduce our estimate of the benefits of the policy.

Table 5
Cost-benefit calculation: 1981–2050.

β_1 estimate used for rural areas	Assumption about heterogeneity in β_1 by highest credential held	
	Using average treatment effect for all	Using credential-specific treatment effects
Using estimates from urban areas	33,498	62,825
Using rural effect estimates from our data	–15,181	–144,688
Costs	117,047	–
Cost-benefit calculation for cohort leaving primary school in 2015 (urban, per-credential β_1)	–155	

*Estimates in millions of 2015 US Dollars.

the coefficient estimate from urban areas to generate the rural figures, we find the policy generates a net gain. If we use the rural estimates to generate the rural figures, we estimate a net loss. Note that we use our reduced form estimates of β_1 for this analysis. We calculated an analog to this table using IV estimates; while this method increased the magnitude of the estimates in each cell, the signs were unchanged. This stems from the fact that the sign of the estimate is driven largely by whether we use the negative estimated returns to those in rural areas with either a high school or tertiary credential.

To provide an estimate of the current per-year cost of the policy, we compare the value of a lost year of productive work to the lifetime productivity benefits of the extra year of primary schooling for the cohort leaving primary school in 2015. We estimate this to be a net loss of approximately 155 million dollars, or less than \$15 per person in the cohort.

We offer two further observations. First, China is a victim of its own success in increasing educational attainment. The largest benefits accrue to those with the lowest credentials, but the proportion of individuals in a cohort with these lower credentials is steadily decreasing over time. We interpret this as evidence of the following claim: while the policy was a success based on its original aims (to increase skills, particularly among the least educated), it may have outlived its purpose given the dramatic increase in average educational attainment over time.

The second observation is that the per-person cost of the policy is very small and we are likely understating the benefits. Prior research suggests there are several possible positive social impacts of this policy change, most of which we either lack the data to analyze or are not powered to test for, that could well add to the benefits side of our calculation. These include a beneficial effect of the policy on crime, health, and mortality (Lleras-Muney, 2005; Lochner & Moretti, 2004). It is likely too early to measure the mortality effects, but we anticipate various other salutary effects of the policy. We leave these to future research.

6. Conclusion

In this paper, we study the impact of extending the duration of primary school on subsequent educational and labor market outcomes. To do so, we exploit the gradual rollout of a Chinese policy reform over 25 years which extended the length of primary school from five years to six. To date, this reform has changed the primary school experience of hundreds of millions of individuals. We find that it does not affect post-primary educational attainment, likely due to the importance of educational credentials in the Chinese labor market. In the labor market, those affected by the reform enjoy relatively small income benefits on average, but larger gains accrue to two (relatively) disadvantaged groups: women and those with low educational attainment. We estimate that the policy's benefits likely exceed its costs but, over time, these benefits dwindle due to the dramatic increase in educational attainment in China and, thus, the decrease in the size of the group likely to benefit most. We argue that this pattern suggests that China's six year primary education policy may have outlived its purpose.

More broadly, our study highlights the public finance and human welfare implications of the policy decision that every government makes in setting the duration of primary school. We find that this policy decision affects how many years the vast majority of individuals will ultimately spend in school, as well as their subsequent earning power in adulthood. For other countries with less rapid growth in educational attainment, such a change may well be an easily implementable and progressive policy option. This is a particularly important message for the many developing countries with a sizable proportion of individuals who never go beyond primary school.

Finally, these results indicate the need for further study of the development of non-cognitive skills in primary school. In the robustness appendix, we show that we are unable to find evidence of an effect of the policy on cognitive skills, consistent with other studies of educational interventions that show wage effects long after any measurable cognitive gains wane (Chetty et al., 2011; Heckman, 2006). We suspect that, in addition to the curricular knowledge students gain, students also practice other skills while in school. For example, being made to come to class on time and sit still for the duration of a school day may nurture discipline, punctuality, and attentiveness. These skills comprise some of those contained in measures of conscientiousness, one of the “big five” non-cognitive skills. Such skills carry substantial returns in most modern labor markets (Heckman & Kautz, 2012), and the extra year of primary school is likely to have developed these further, particularly for those who finish their educational careers with only a primary or middle school credential. We hope that future research can help illuminate how extra time spent in primary school, a formative stage of education, develops these skills as well.

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Supplementary material

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