# The Importance of Educational Credentials: Schooling Decisions and Returns in Modern China* 

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#### Abstract

Government policy sets the number of years of schooling needed to attain educational credentials such as a high school diploma. We exploit a policy change in China to estimate the impact of this policy choice on schooling decisions and labor supply. The policy extended the length of primary school by one year and was rolled out gradually across China over 25 years. We show this has reallocated 850 billion person-hours from labor to schooling to date, as the vast majority of affected individuals chose to earn a credential despite the additional year in school this required. A key contribution of our paper is to estimate the returns to an additional year of schooling while holding highest credential constant. We find the year generates a two percent gain in monthly income, with somewhat higher returns for China's disadvantaged. This is much smaller than most estimates which do not separate the returns to additional schooling from those to earning a credential. We show that the policy, while redistributive, has generated a likely net loss of tens of billions of dollars. We interpret these results through a model of signaling and human capital accumulation and conclude that a high signaling value of earning a credential, also known as "credentialism," plays a crucial role in household schooling decisions and in the returns to schooling in modern China.


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## I. Introduction

A key decision every government makes is to determine the length of primary and secondary schooling. This policy choice is intended to affect all citizens, potentially determining how they spend the early productive years of their lives. In this paper, we exploit a policy change in China to estimate this type of policy's impact on schooling decisions and labor supply. We then interpret our results through the lens of the literature on the relative importance of signaling and human capital accumulation in the returns to schooling.

In 1980 the Chinese government announced that it would increase by one the number of years needed to complete primary school while leaving unchanged the national curriculum and length of all other levels of schooling. This policy was rolled out gradually across localities over 25 years and has induced over 400 million people to spend an additional year in primary school so far. The policy was implemented in each locality at a time when over $75 \%$ of individuals proceeded to middle school or beyond and the modal student (also the median) left school after receiving her middle school diploma. When affected by the policy, this modal student could either keep total years of schooling constant by forgoing the credential she would have attained in the absence of the policy ${ }^{1}$, or keep credential attainment constant by spending an additional year in school. Figure I depicts the Chinese education system before and after the policy change and the modal student's post-policy schooling decision.

We identify the causal effect of the policy on schooling and labor market outcomes using a simple regression discontinuity design. We compare treated and untreated individuals within each locality ${ }^{2}$, restricting our attention to those leaving primary school within a few years of when the policy took effect. This approach is similar to recent work studying the impact of changes in compulsory education in the UK (Oreopoulos, 2006; Clark and Royer, 2013). Unlike those studies, which examine the impacts of a policy implemented at once across an entire nation, we take advantage of the fact that our policy was rolled out at different times in different places across China to net out cohort, place, and cohort-by-region fixed effects. This protects against the threat of upward bias from differential regional trends, shown to affect similar exercises in the US (Stephens and Yang, 2014). To determine the treatment status of individuals in our data,

[^1]we collect information from educational gazetteers stored in China's national archives which document if, when, and how the policy was implemented in each locality.

We begin our analysis by estimating the impact of this change on credential attainment and years of schooling. There is extensive bunching at credential attainment years prior to implementation of the policy. After the policy, we show that this bunching persists. Years of schooling increased by nearly one for affected individuals, and we find no evidence of a decrease in credential attainment or years of post-primary schooling to offset the extra primary year.

This yields two central results: one, the value of a credential was high enough that nearly all affected individuals chose to forgo an additional year of wages and pay an additional year of schooling expenditure in order to attain the credential (e.g., a high school diploma) they would have earned in the absence of the policy. Two, the policy has reallocated nearly one trillion person-hours from the labor market to the pursuit of schooling to date ${ }^{3}$.

We find no evidence that the policy changed the characteristics of who earns which credential. This allows us to estimate the return to an extra year of primary school, holding highest educational credential constant. We exploit the large sample size of the 2005 Chinese mini-census to generate a precise but small estimate: the extra year increases monthly income by $2.03 \%$. We find no evidence that the additional year affected other labor market indicators such as entrepreneurial activity, employment status, and type of employer, i.e. private vs. government, suggesting that the income gains we observe are likely to be flowing through the human capital accumulation channel and not through selection into different types of employment.

Our main estimate of the return to an additional year of schooling is one-third to one-seventh the size of most credibly identified estimates from the developed world (Angrist and Krueger, 1991; Ashenfelter, Harmon, and Oosterbeek, 1999; Oreopoulos, 2006) and the $8-14 \%$ per-year premium to earning a credential seen in our data. We suspect that a main reason for this discrepancy is our ability to isolate the returns to additional schooling from the returns to an additional credential. Our estimate is similar in magnitude to that of Li, Liu, and Zhang (2012), who estimate these returns in China using schooling differences within twin pairs in order to mitigate bias from the correlation between schooling levels and unobserved ability.

[^2]A primary impact of this policy is the reallocation of 850 billion person-hours from work to school. We perform a cost-benefit analysis leaning heavily on that of Duflo (2001), comparing the lifetime value of the increase in monthly income conferred by the extra year of schooling to the cost of the lost year of productive activity in the labor market. As in Duflo's study, our time frame ranges from the first graduating cohort affected by the policy in 1981 to 2050. This exercise generates four sets of estimates. In all but the most favorable scenario, the costs of the policy exceed its benefits by at least tens of billions of US dollars.

We estimate higher returns from this extra year accruing to those with less schooling, the intended beneficiaries of the policy. This result echoes that of research from India showing that providing extra remedial education is most likely to help those with relatively low test scores (Banerjee et al., 2007). It is also akin to more recent work showing that increased instructional time is an important mechanism behind the success of a group of high-performing New York charter schools in improving outcomes for the underprivileged (Dobbie and Fryer, 2013). We measure a small but significant difference between the distribution of cognitive skills test scores in the treated and untreated groups. This adds to recent evidence that there can be measurable adult returns to childhood interventions decades after the intervention runs its course (Heckman, 2006; Chetty et al., 2011).

We develop a simple model of the household's schooling decision. We use the model to capture the idea that the relative importance of the signaling and human capital accumulation channels in driving the returns to schooling determines how households respond to the policy we study. Our empirical results are consistent with the version of the model in which signaling plays the dominant role in driving schooling decisions and the returns to schooling. We also conduct a back-of-the-envelope calculation to estimate the relative importance of the two channels in the naive returns to schooling we observe in the cross-section. Using parameter estimates from our analysis and other recent research from China (Li, Liu, and Zhang, 2012), the calculation suggests that between 57 and 60 percent of these returns are flowing through the signaling channel.

Finally, we draw policy implications from our research. Our cost-benefit results show that the policy shifted a tremendous amount of resources at a likely fiscal loss. This highlights the importance of a seemingly arbitrary policy choice: how long should each level of schooling last? To study the generalizability of our findings, we examine Demographic and Health Surveys (DHS) data collected in 74 countries. We find patterns similar to those we observe in China, and
consistent with a high value of credential attainment, in 48 of these countries. This suggests that government decisions on the length of each level of schooling have massive resource implications beyond the Chinese setting.

Our paper contributes new evidence to three lines of empirical research on schooling. One line highlights a set of cases where schooling decisions are driven by the signaling value of attaining a credential (Lang and Kropp, 1986; Bedard, 2001). To it, we contribute evidence from a more general case where the entire population is shocked with a change in the length of time required to attain a credential. The second line of research shows significant positive returns to endowing an individual with a credential while holding years of schooling constant. This work uses naturally occurring, plausibly exogenous variation to causally estimate the value of a credential, exploiting either arbitrary test passing thresholds or comparing otherwise similar education reforms that vary in whether they also confer a credential on the treated population (Tyler, Murnane, and Willett, 2000; Grenet, 2013; Clark and Martorell, 2014). Our study contributes evidence from the converse of what these papers study - we hold highest educational credential constant and estimate the returns to a marginal change in schooling. The third line uses large changes in schooling policy from the developing world to assess the merits of different policy options (Duflo, 2001; Lucas and Mbiti, 2012). To it, we add evidence from a readily implementable policy that has, to the best of our knowledge, not yet been scrutinized.

The rest of the paper proceeds as follows. In Section II, we discuss the history of education in modern China and describe the policy we study. In Section III, we describe the data we use and our identification strategy. Section IV contains empirical results related to educational attainment and Section V provides our empirical results relating to the labor market. In Section VI, we interpret our results using a simple conceptual framework, use several ancillary analyses to discuss alternative explanations for the bunching at credential attainment years we observe, perform the cost-benefit analysis, and draw policy implications. Section VII concludes.

## II. A Brief History of Primary and Secondary Education in Modern China

China's education system has grown substantially in size and scope since the end of the Chinese Civil War in 1949. Education levels at that time were quite low: only $20 \%$ of the population was literate, and fewer than $40 \%$ of school-aged children were enrolled in school (Hannum, 1999). The new government increased spending on primary education after independence and vastly
expanded the number of schools across the nation at all levels (Liu, 1993). During the period 1950-1980, a series of policy experiments and natural disasters racked China, leaving in its wake an educational system which varied greatly in scope and structure across provinces. Even so, literacy jumped to more than $50 \%$ by 1976 and average educational attainment rose to over 7 years (UNICEF, 1978). After the Cultural Revolution ended in 1976, China's education system moved towards standardization (Hannum et al., 2008). In January 1978, the Full-Time Ten-Year Primary and Middle Education Teaching Plan (Draft) mandated national harmonization of the length and structure of primary, middle, and high school ${ }^{4}$ in all provinces. This set the length 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 ${ }^{5}$. It allowed gradual adoption of the primary school length change across localities, putting more pressure on urban schools (Liu, 1993). Appendix Figure A. 1 plots national data on the proportion of students in six year (or equivalent) primary school systems, showing gradual adoption of the six year primary system between 1980 and 2005. About 60 percent of localities switched to a six year system between 1981 and 1993, relatively few made the change in the mid-1990's, and the rest shifted in the late 1990's and 2000's, reaching near-universal adoption in 2005. As explained in Footnote 3, we estimate that the policy has induced approximately 425 million students to spend a sixth year in primary school so far.

This policy was announced early on in Deng'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." The policy we study was implemented as part of his larger move in the late 1970's and early 1980's to prepare China's labor force to adapt to this new economic arrangement (Vogel, 2011).

This policy did not change the age at which children entered school, nor did it change the primary school, middle school, or high school curricula. Rather, the intent of the change was

[^3]that primary school students be taught the same material over a longer period of time to ensure mastery of the curriculum.

We conducted a series of structured qualitative interviews with affected students, teachers, and parents to collect a richer account of what happened during this extra year and how it was perceived. Students and teachers 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. After the adjustment process was completed, students and teachers reported that the primary school curriculum previously covered in five years was extended more smoothly over six years. 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 extra cohort of children, but these schools were given no additional resources to do so. Gazetteer records and our interviews indicated that the additional 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 secularly declining 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. Appendix Table A. 1 gives six examples of how the policy was enacted, taken from gazetteers in different implementing cities and counties across the country. In some cases, the transition was accomplished by enforcing the policy immediately, forcing all students who had not graduated from primary school, including those in fifth grade at the time, to remain in primary school an extra year. In other cases, it was accomplished by selecting a cohort of students (e.g. third graders) after which all students must complete six years of primary
schooling. 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.

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 III, most counties had the ultimate say on the year in which the switch was made ${ }^{6}$. Our data bears this out, and in section III.C we address the issues surrounding discretion in timing of implementation and the attendant concerns of omitted variable bias.

Our gazetteer data show no evidence of any other policy change which was regularly coincident with the change we study. A separate policy issued in April 1981 by the Ministry of Education mandated that the length 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, 1984). In 1986, the Chinese government made middle school compulsory, but we show in Section VI that this law appears to have little impact on the middle school attainment of observations in our estimation sample. We argue this is due to two factors. One, there is widely documented porous enforcement of the law in rural areas (Fang et al., 2012). Two, in urban areas education levels are already high at the time of the policy announcement and so the law is binding for relatively few urban residents.

## III. Data and Identification Strategy

This section describes the data sources and empirical methods of the paper. We show evidence that the main identifying assumptions for the research design are satisfied, and address a set of issues which could confound causal interpretation of our results.

## III.A. Data Sources

Our sources of data are listed in Table I. There are two main sources of observational data: the 2005 Chinese mini-census and the 2010 wave of the China Family Panel Studies (CFPS). The

[^4]2005 Chinese mini-census collects basic data on family structure, highest educational credential attained, health, and income, and contains 2.6 million observations ${ }^{7}$. The CFPS is a new, 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 ${ }^{8}$. Summary statistics on demographic, education, and employment characteristics of our sample population are given in Appendix Table A. 2 for each data set, separately for rural and urban residents.

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 Appendix Figure A.2. We hired a team of research assistants to read through 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 ${ }^{9}$. Appendix Figure A. 3 shows a page from one of these gazetteers.

We determine the year the policy was implemented, separately for rural and urban residents ${ }^{10}$, in 280 of the 345 prefectures ${ }^{11}$ in the census data. Of those 65 prefectures in the census we are unable to code, 45 either implemented the policy gradually across counties within a prefecture, so that we could not identify a unique prefecture-level treatment year, or instead changed to a system of five years of primary school with four years of middle school instead of the six primary plus three middle format we study. The remaining 20 had no record of implementing the policy in the currently available educational gazetteers.

## Determining Treatment Status in the CFPS Data

We use the gazetteer data to identify which individuals in the census are treated by the policy and which are not. In the CFPS dataset, the location of observations is anonymized to the provincial level, which prevents us from using the gazetteers to determine treatment status. Nonetheless,

[^5]the CFPS has a few traits which make it particularly desirable. It collects detailed data on how many years individuals spend in each level of schooling and identifies which individuals reside in a given county. The gazetteers document that the policy is sometimes implemented at different times between counties within prefectures. As a result, analysis at the county level is important to understand precisely how the policy was rolled out in each location.

We apply a mean-shift algorithm (Fukunaga and Hostetler, 1975) to the CFPS data to determine treatment status for observations in the CFPS. Our algorithm generates, for each county, the most likely cohort in which the number of years spent in primary school jumps from five to $\operatorname{six}^{12}$. 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:

$$
\begin{equation*}
s_{i}=\gamma_{0}+\gamma_{1} \cdot \mathbf{1}\left\{t_{i} \geq t^{*}\right\}+\varepsilon_{i} \tag{1}
\end{equation*}
$$

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 $\varepsilon_{i}$ is an i.i.d. error term. The year $\left(t^{*}\right)$ with smallest sum of squared residuals (ssr) is the predicted treatment year for that county ${ }^{13}$. This exercise generates a treatment year for each county in our estimation sample ${ }^{14}$. 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 ${ }^{15}$, to corroborate the reliability of the mean shift method's identified treatment years.

[^6]
## III.B. Empirical Strategy

Our identification strategy is a simple regression discontinuity design with a discrete running variable: distance in years between an individual's birth cohort and the first cohort affected by the policy change we study (Lee and Card, 2008). We compare 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 cohort, place, and cohort-by-region fixed effects.

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 and Lemieux, 2010). We test this assumption on three fundamental predetermined characteristics which could affect our dependent variables, either through sorting or another selection mechanism: relative cohort size, gender composition of cohort, and proportion of individuals with a household registration certificate (hukou) from an urban area. Figure II plots these data, condensed to distance-to-treatment-year (the number of years between an observation's cohort and the first affected cohort) bin means. This figure shows no visible discontinuity at the treatment threshold. Due to the discrete nature of the running variable, we cannot run a McCrary (2008) test for bunching. Instead, as recommended in 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. In all cases we fail to reject a zero effect.

Following Imbens and Lemieux (2008) and Lee and Lemieux (2010), our main estimating equation is an ordinary least squares regression of $y_{l c i}$, the outcome of interest for individual $i$ in birth cohort $c$ and locality ${ }^{16} l$, on a short set of key regressors:

$$
\begin{equation*}
y_{l c i}=\beta_{0}+\beta_{1} * \text { Treated }_{l c}+\beta_{2}\left(t_{l c} \mid t_{l c} \geq 0\right)+\beta_{3}\left(t_{l c} \mid t_{l c}<0\right)+\beta_{4} V_{i}+\lambda_{c}+\mu_{l}+\eta_{c r}+\varepsilon_{l c i} \tag{2}
\end{equation*}
$$

Here Treated $_{l c}$ is an indicator variable equal to 1 if the individual belongs to a cohort finishing primary school in or after the first affected cohort in her locality. $t_{l c}$ is the locality-specific distance-to-treatment-year. We estimate the coefficient on distance-to-treatment-year separately for treated and untreated groups to account for pre- or post-policy trends (e.g., the possibility

[^7]that the effect may differ over time elapsed since the treatment year, as counties get better at implementing the policy) in order to ensure that $\beta_{1}$ captures only the difference between pre- and post-policy means ${ }^{17}$ (Gelman and Imbens, 2014). $V_{i}$ 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. Locality $\left(\mu_{l}\right)$, cohort $\left(\lambda_{c}\right)$, and cohort-by-region ${ }^{18}\left(\eta_{c r}\right)$ fixed effects are also included in all specifications unless otherwise stated.

Following Lee and Card (2008), we test that our estimated coefficients are stable across the choice of how many years around the treatment threshold we include in the estimation sample. We show this stability for two main empirical results. For the rest, our estimates use the sample limited to five years before or after the first treated cohort in each place. All regression results we present use robust standard errors clustered at the county or prefecture level (Bertrand, Duflo, and Mullainathan, 2004). We restrict our time frame to cohorts leaving primary school between $1976^{19}$ and several years before the sample is drawn (1995 in the census data and 2003 in the CFPS data) to give most individuals enough time to finish their schooling career before being observed.

## III.C. Potential confounders

The implementation of this policy across space and time was decided upon by local (province, prefecture, and county-level) bureaucrats. We have shown evidence that our main identification assumption is upheld and, 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, and Salvanes, 2005; Meghir and Palme, 2005). If, however, there were another policy or external phenomenon correlated with both when the policy was implemented in a given locality and the later schooling decisions or labor market outcomes of affected individuals, our results would suffer from omitted variable bias.

Though it is impossible to rule out the existence of any external driver of both the timing of the policy and our outcomes of interest, we fail to find evidence of such a factor among a set of likely candidates. First, as recommended by Stephens and Yang (2014), we control for cohort-by-region

[^8]fixed effects. If such an external phenomenon were geographically auto-correlated, for example due to intra-provincial policy coordination, these fixed effects would dampen its impact on our estimates. Second, we show the geography of the timing of implementation in each of China's prefectures according to archival records. Appendix Figure A. 2 provides a heat map of prefecture implementation years, with lighter shades indicating earlier implementation. This map shows a wide distribution of timing of implementation with no obvious geographical pattern aside from later implementation in some prefectures in the central region. Third, we searched the gazetteers for mention of a policy or external influence that was regularly coincident with the implementation of the policy we study and found no such pattern. As these documents serve as official records of policy implementation, this strongly suggests absence of a consistent confounder.

Finally, as in Black, Devereux, and Salvanes (2005), we use OLS to estimate the relationship between timing of policy implementation and prefecture-level characteristics (e.g. gender composition, mean income, and proportion of respondents working for the government) for those prefectures in our estimation sample. While correlations here do not necessarily threaten causal interpretation of our results, they give a descriptive account of what may have driven implementation choice. The results of this regression are given in Appendix Table A.3, and show that only the central and northeastern regional dummies are statistically significant correlates of timing of implementation, consistent with the map in Appendix Figure A.2.

A final concern is the potential for migration to bias our results. We cannot reliably estimate treatment effects for migrants for two reasons: there are far fewer of them in our data and their treatment status is more difficult to pin down because we do not have data on precisely when they moved. In light of these limitations, we exclude migrants from our analyses. If the treatment effect is different for migrants and non-migrants or the policy affects who is likely to migrate, our estimates will differ from the true population average treatment effect. As there are far more ruralurban than urban-urban migrants, this is more likely to be a concern in rural areas. We test for an effect of the treatment on cohort size and characteristics of individuals in our rural sample, and find no significant relationship between treatment status and cohort size or gender composition. This suggests the extra year of primary school did not affect the propensity to migrate for the population as a whole, or differentially for men and women. Beyond these tests, there is little we can do about this concern, but the sample size of the census gives us the statistical power to conduct these tests with precision and the relatively high response rate of the CFPS (97\% for
households, $72 \%$ for identified adults within households) suggests that, at the very least, any migration-induced selection bias will be minimal (Lv and Xie, 2012).

## IV. Empirical Results - Schooling

In this section, we estimate the impact of the policy on individuals' schooling outcomes. First, we show that the policy was indeed effective at extending the number of years individuals spent in primary school by one year. We then estimate the impact of this change on individuals' later schooling outcomes. These outcomes include years spent in post-primary schooling, whether or not an individual attains one of two post-primary credentials (a middle school or high school diploma), and drop-out. We finish this section looking at the effect of the policy on vulnerable subgroups and on the characteristics of individuals with each credential.

We first examine whether the policy had its desired effect of increasing primary school for affected individuals. Figure III plots distance-to-treatment-year bin means of the proportion of individuals spending six or more years in primary school in our CFPS sample, overlaid on estimates of their confidence intervals. Prior to implementation of the policy, the proportion of students spending at least six years in primary school is between 20 and $30 \%$ of the population. This 20-30\% comprises mainly individuals performing poorly in school who are made to repeat a grade ${ }^{20}$. At the policy implementation year it jumps to over $80 \%$, increasing to nearly $100 \%$ over the next ten years. Results from the regression analog to this exercise are presented in the first row of Table II. We estimate that the treatment causes a 0.547 increase (SE 0.029 ) in the probability of taking a sixth year of primary school for those who graduate from primary school within five years after the policy is implemented.

This is an underestimate of the "first stage" of the policy: 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. Furthermore, as shown in Figure III, this probability estimate increases as we increase the bandwidth around the treatment year. We argue these circumstances give us a sharp rather than a fuzzy discontinuity, allowing us to consider none of the pre-policy group and nearly all of the post-policy group to be "treated."

Recall that this policy was implemented in each locality at a time when over $75 \%$ of students

[^9]went on to get at least some post-primary schooling. 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 ${ }^{21}$. Figure IV shows, separately for untreated and treated observations finishing primary school within five years of the treatment year, the distribution of post-primary schooling. This figure summarizes our main empirical results related to the effect of the policy on post-primary schooling. We see extensive bunching at credential attainment years and little visible difference between the treated and untreated groups in either the location or the magnitude of this bunching ${ }^{22}$.

The regression results for our schooling outcomes are given in the rest of Table II. The second row shows our estimate of the effect of the policy on total years of schooling to be 0.660 , significant at the $1 \%$ level. 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. Our estimate of the effect of the policy on years of post-primary schooling is positive ( 0.093 ) but statistically indistinguishable from zero. The standard errors we generate can exclude anything larger than a 0.32 year decrease in post-primary schooling in response to the extension of primary school and also admit positive estimates of up to a 0.5 year increase. In Panel A of Figure V, we show that our point estimate of the impact of the policy on post-primary schooling is stable over nine different bandwidth choices, as recommended in Lee and Card (2008). In no case are we able to reject a zero effect ${ }^{23}$.

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 fourth and fifth rows of Table II, we estimate the effect of the policy on whether or not an individual earns at least a middle school credential and whether or not she earns at least a high school credential, using both census and CFPS data. The effect of the policy on middle school completion, estimated using the census data, is negative but small and insignificant ( 0.0049, SE 0.0030 , from an untreated group average of 0.725 ). We can reject anything larger in

[^10]magnitude than a one percentage point decrease on the probability of completing middle school. The estimated effect on finishing high school is small, positive (0.0063), and significant at the $10 \%$ level. We find no effect on the probability of dropping out. These small standard errors speak to the statistical power the census affords us relative to the CFPS in measuring even small effects. In short, we find no evidence of a decrease in post-primary schooling to offset the lengthening of primary school.

One possible explanation for this overall pattern of no net change in post-primary schooling is a change in composition of who earns which credential. The zero effect 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. To test for this possibility, we perform two exercises. First, we look for changes among those subgroups we would expect to be most likely to be induced by the policy to offset the extra primary year with fewer post-primary years; second, we explicitly test for changes in composition of background characteristics at each level of schooling.

Previous work has shown that Chinese households in the 1980's and 1990's often chose to allocate fewer resources to women (Li, 2003). Income is also much lower in rural areas of China than in urban areas, and women from rural areas are thus doubly disadvantaged. We anticipate the extra year of forgone wages needed to earn a credential poses the greatest burden for these three groups. If this is true, we are most likely to find a downward shift in their post-primary schooling to offset the extra year of primary school.

We test these predictions in Table III, which shows the subgroup-specific treatment effect estimates for the same outcomes examined in Table II. The estimated treatment effects are largely negative, as predicted, and consistently so for rural women, the most disadvantaged group. The magnitude of the estimates is uniformly small, however, and only for dropout rates do they reach statistical significance at the $10 \%$ level. We estimate but do not present effect estimates for other groups (such as men, those from western and non-western provinces, and urban areas), which we find to be more consistently positive but similarly small relative to their respective standard error estimates.

The second exercise estimates a version of our main empirical specification to 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). For outcome variables, we use a set of predetermined characteristics to proxy for household resources allocated to the child and scholastic ability, the most likely predictors of adjustment on the credential margin. We proxy for resources allocated to the child with number of siblings and gender. We use parents' highest credential (mother's and father's separately) to proxy for scholastic ability. Wald tests of the equality of the treatment-by-credential level coefficients tell us whether the proportion of individuals with the predetermined characteristic of interest holding a given credential changes, relative to that proportion among those holding other credentials, across the treatment threshold. We use the CFPS data for these tests, and fail to reject equality of the treatment-by-credential coefficients on number of siblings, mother's and father's highest educational credential, and gender (p-values $0.740,0.665,0.135$, and 0.660 , respectively). We conclude from these analyses that the characteristics of who earns which credential are unlikely to change substantially as a result of the policy.

## V. Empirical Results - The Labor Market

In the previous section we studied the effect of a policy which adds an extra year to primary school on schooling outcomes, assuming individuals could adjust their level of post-primary schooling up or down to compensate for or reinforce the policy's effects. We found that the policy induced the vast majority of Chinese citizens to spend an extra year in primary school while leaving their highest educational credential unaffected. In this section, we will treat the variation from this policy as an experiment which induced all affected individuals to spend an extra year in school while holding their highest credential constant. We defend our approach at the end of the section. Under this interpretation, we can use our identification strategy to isolate the labor market returns to the human capital accumulated during a year of schooling from the signaling effect of receiving an additional credential that often confounds such work (Weiss, 1995; Card, 2001).

We use Equation 2 to estimate the effect of this additional year of schooling on various labor market outcomes, including employment status, type of employment for the employed (i.e., self-employment, government sector employment, and private sector employment), and monthly income. Though China was strictly a command economy as recently as 1978, reforms enacted in the 1980's and 1990's pushed the Chinese labor market to more closely resemble that of a market economy as early as the late 1990's (Cai, Park, and Zhao, 2008).

We use the 2005 mini-census data for all of the analyses in this section due to its large sample
size ${ }^{24}$. We restrict our attention to urban residents, as in rural areas treatment effect estimates would be muddied by the impact of the treatment on the decision to work in agriculture or not and selective loss to migration is more of a concern. Our main dependent variable of interest is the natural logarithm of monthly income ${ }^{25}$. We also investigate the effects of the policy on whether the individual is employed and whether she is employed in a government job, in the private sector, or works as an entrepreneur. In the regression results presented in this section, we add highest credential fixed effects to the right hand side of Equation 2 and use the same sample restricted to five years on either side of the treatment year for estimation ${ }^{26}$.

Regression results are given in Table IV. 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 ( 0.26 percent, from a treated group mean of 77.7 percent) and standard errors precise enough to reject anything larger than a 1.2 percentage point increase or a 0.8 percentage point decrease in this probability. Our estimate of the effect of the policy on whether the individual works for the government (as opposed to for the private sector or as an entrepreneur) is similarly precisely estimated and indistinguishable from zero. This result suggests that the policy is unlikely to have had a large effect on whether or not an individual is working and, if so, whom she is working for.

Next, we estimate the effect of the policy on the natural $\log$ of monthly income. We add employer-type fixed effects to this specification to more precisely estimate our parameter of interest, the income returns to the extra year of schooling ${ }^{27}$. Our first specification uses cohort and locality (prefecture) fixed effects. Here we find a gain of $1.94 \%$ in monthly income, statistically significant at a $99 \%$ confidence level. Recent work (Stephens and Yang, 2014) shows that previous efforts to estimate the returns to schooling using strategies similar to ours may have been biased upward and suggests including cohort-by-region fixed effects to mitigate this bias. Implementing this recommended approach, we next estimate Equation 2 with the addition of cohort fixed effects specific to each of China's regions (East, Northeast, Central, and West). Our treatment effect estimate increases slightly, to a $2.03 \%$ gain, and remains significant (the standard

[^11]errors change by less than three hundredths of a percentage point).
Our treatment effect estimates are also stable across bandwidth choice. Panel B of Figure V presents our treatment effect estimates for log monthly income and their confidence intervals for the same nine different choices of sample bandwidth. This figure demonstrates the stability of both the magnitude of the coefficient and its ability to reject a zero effect across a wide range of bandwidth choice.

There is still concern that our research design, if specified incorrectly, could mechanically generate a difference between the treated and control groups unrelated to the effect of the policy. To test for this, we conduct a Monte Carlo exercise in which we draw 1,000 placebo treatment 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. Figure VI gives the probability density function for these estimates. The placebo treatment effect estimates are normally distributed, with a mean of -0.001 and a standard error of 0.0084 , putting the true estimate of 0.0203 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.

We next explore heterogeneity in treatment by subgroups, shown in Panels B-D of Table IV. The coefficients shown here are from an estimating equation similar to equation 2 , only replacing the single treatment variable with interactions between the treatment variable and a dummy for membership in the mutually exclusive and exhaustive subgroups of interest (e.g. men and women, government and non-government workers) and excluding the un-interacted treatment variable from the equation for ease of interpreting each subgroup-by-treatment coefficient. Panel B shows that the estimated gain from the additional year is monotonically decreasing in highest educational credential, consistent with the goals of the policy and with the proportional contribution of an additional year of schooling diminishing as total years of schooling increases. As mentioned in the introduction, this result is also consistent with a study that identifies increased instructional time as a key mechanism contributing to the success of a set of New York City charter schools in raising achievement among underprivileged students (Dobbie and Fryer, 2013).

Panel C shows a higher return to the extra year for women than for men, consistent with other work on returns to schooling by gender in urban China (Hannum, Zhang, and Wang, 2013). Panel D shows that private sector workers appear to enjoy all of the wage premia from the extra
year. 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 et al., 2012). Independently run Wald Tests reject equality of the coefficients for both differences.

We attempted to investigate the possibility of further conditional treatment effect heterogeneity, e.g. gender-by-occupation or gender-by-education level returns, but our research design is too data-intensive to precisely estimate these effects, even using the census data. 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.

We next test for a difference between the treated and untreated in cognitive skills, as measured by a test administered to adult respondents in the CFPS survey. Figure VII plots the kernel density functions for treated and untreated individuals using the five year bandwidth sample. The two distributions track each other quite closely, but there is a visible rightward shift in the treated distribution. A Kolmogorov-Smirnov test rejects the equality of the two distributions with a pvalue of 0.003 . The difference in distributions is most stark in the left tails, precisely what we anticipated given the nature of the extra year (Meghir and Palme, 2005; Dobbie and Fryer, 2013). The difference between the the left tails of the two distributions is 0.1 to 0.3 standard deviations, very similar to the estimated impact of a remedial primary education program in India (Banerjee et al., 2007). This result, alongside our estimate of the effect of the extra year on monthly income, adds to evidence that childhood interventions which may initially generate increases in test scores or cognitive skills often bring labor market returns decades after the initial intervention (Heckman, 2006; Chetty et al., 2011).

Our estimates of the effect of the year of primary schooling on monthly income are small compared to naive estimates of the Mincerian return to an additional year of schooling generated using our census data. Columns (1) and (2) of Table V provide these cross-sectional estimates in relative terms. Column (1) gives the naive estimate of the total income bonus from gaining a given credential relative to the preceding credential, for example, the additional earnings someone with a high school credential earns relative to someone with only a middle school credential. Column (2) lists the per-year premium to each credential, calculated by dividing the value in Column (1) by the number of years it takes to earn that credential. We place our estimates and those of $\mathrm{Li}, \mathrm{Liu}$, and Zhang (2012) at the bottom of this column for comparison. Our naive estimates of the per-
year premium to earning a credential are between four and seven times as large as our average treatment effect estimate of the return to the extra primary year. Our estimates are also small relative to benchmark estimates of the returns to an additional year of schooling in developed countries (Ashenfelter, Harmon, and Oosterbeek, 1999; Card, 2001).

We claim that much of this gap between our estimate and the naive mincerian return is driven by our ability to shut down the signaling contribution to the returns to schooling by varying years of schooling while holding highest credential constant. To evaluate the merits of this claim, we next explore potential sources of downward bias on our estimate of the effect of the extra year of schooling on income. The first is attenuation bias stemming from possible misclassification of observations around the treatment threshold. If attenuation bias were a problem, we would expect the treatment effect estimate to increase with the number of years around the bandwidth we include in our sample, as this would increase the proportion of correctly identified observations. Panel B of Figure V shows that our effect estimate is quite stable across the different bandwidth specifications, suggesting attenuation is not likely to be a concern for interpretation of this coefficient estimate.

The second issue is the possibility of a gap between our treatment effect estimate, which estimates the effect of the policy on self-reported monthly income, and the true returns to schooling. Though it is common to "inflate" the reduced form treatment effect coefficient through dividing it by the proportion of individuals affected by the policy (the "first stage"), we argue that is inappropriate in our scenario for the following reasons. First, the proportion affected is unstable over bandwidth choice: the five year bandwidth estimate is 0.55 , while the 10 year estimate is almost 0.8 . Second, as noted above, the effect estimate of the policy on monthly income is stable across bandwidth choice, suggesting that we may have superior classification in our urban census sample using the gazetteer data than in our rural and urban CFPS sample. Finally, as argued in Section IV, the nature of the policy we study allows us to treat the change in the proportion of each cohort in "six year primary school" across the treatment threshold as close to one.

Another potential explanation for this discrepancy is the difference between partial and general equilibrium effects. The policy we study affects a far larger proportion of the population than most studies which generate large effects, and it could be that the competition among many workers with the same ability gain drives down the labor market returns to the extra year. There are two reasons why we think this is unlikely to be the case. One, another study of an educational
policy change which increases years of schooling for nearly half of the UK population finds much larger labor market returns (Oreopoulos, 2006). Two, we test for these general equilibrium effects and find no evidence of their existence ${ }^{28}$.

The context in which we generate our estimate could also contribute to the gap between our estimates and mincerian returns. While the remedial nature of this additional year could possibly explain the small effect estimate, we note that the last year of both middle school and high school is also review, suggesting the extra year that we study is not so unusual in its content (Larmer, 2014). Our estimates of the returns to this sixth year may therefore be similar to the returns to these other years of schooling. In further support of this claim, we note that other well-identified estimates of the returns to schooling in China are similar in magnitude. Li, Liu, and Zhang (2012) estimate that an additional year of schooling brings a 2.7 to $3.8 \%$ increase in earnings, using within-twin-pair differences in schooling to mitigate the potential for unobserved ability correlated with credential attainment to bias the results.

Finally, we need to account for the fact that income increases with age and that some of this may be due to the returns to experience. As in all exercises attempting to estimate the returns to a year of schooling, we face the conundrum 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). The lower bound is 0 , reflecting a world in which the returns to a year of age are entirely due to maturity. To calculate the upper bound, we assume that the difference between the average income of a 36 year old and a 35 year old with the same credential is entirely due to experience, and we calculate a weighted average of the per-year-of-age income premium we see in our data: $\sum_{a g e=t} \omega_{t}\left(\overline{\text { Income }_{t}}-\overline{\text { Income }_{t-1}}\right)$, where $\omega_{t}$ is the proportion of individuals in our estimation sample of age $t$. This generates an upper bound of 0.0168 , suggesting that the upper bound of the range of estimates corrected for this bias would be 0.0371 , still less than half of the lowest value in our naive mincerian per-year return estimates.

We can use these results and three assumptions to make a back-of-the-envelope calculation about the relative contribution of the signaling and human capital accumulation channels to the

[^12]correlation between schooling and income. Our first assumption is that the returns to schooling flow only through two channels, the returns to skills gained and returns to the signal provided by the individual's highest educational credential. Our second assumption is that the final year of middle school and high school, each of which is also a review, yield the same return as our estimate of the return to the sixth year of primary school. Our final assumption is that the other years of schooling give twice the boost of the review year (i.e. $4.06 \%$, slightly larger than the per-year estimate Li, Liu, and Zhang (2012) generate using twins for identification). Under these assumptions, we estimate that the signaling channel accounts for $57.2 \%$ of the returns to a middle school degree and $60.0 \%$ of the returns to a high school degree we observe in the cross-section.

## VI. Interpretation and Discussion

This section of the paper has four parts. First, we model a household's decision about their child's schooling to formally link the household response to the policy we study and the relative importance of signaling and human capital accumulation in driving the returns to schooling. Next, we enumerate a set of other mechanisms which could generate similar results, describing their empirical plausibility in our setting. We then perform a cost-benefit calculation of the policy's impacts and finish the section with a discussion of the policy implications of our analysis.

## VI.A. Modeling the Schooling Decision

To formalize how we interpret our empirical results through the lens of the signaling/human capital accumulation literature, we use a simple model of a household's schooling decision based on Becker (1975). In our model, the household decides after which discrete year, $s \in[0, S]$, its child will leave post-primary schooling ${ }^{29}$ and enter the labor market ${ }^{30}$. This decision's arguments are the expected future benefits of schooling, $b(s)$, and the costs of schooling, $c(s)$. We assume that the benefits are increasing and concave, i.e. $b^{\prime}(s)>0$ and $b^{\prime \prime}(s)<0$, and that costs are increasing and convex, $c^{\prime}(s)>0$ and $c^{\prime \prime}(s)>0$. The first order condition of this calculus deter-

[^13]mines the optimal level of schooling, $s^{*}$, satisfying $b\left(s^{*}\right)-c\left(s^{*}\right) \leq b(s)-c(s)$ for all $s \neq s^{*}$ and $b\left(s^{*}\right)-c\left(s^{*}\right), b(s)-c(s) \geq 0$.

We assume that $b(s)$ is a function of the human capital accumulated (skills acquired) in a given year of schooling and the signal conferred, $b(s)=f\left(h_{t}, c_{t}\right)$, where $h_{t}$ is the human capital contribution of year $t$ and $c_{t}$ is the signal conferred in that year. We assume that the accumulation of human capital is a continuous, gradual process, such that $b(s)=f\left(h_{t}, \cdot\right)$ is smooth and nonzero within a range $(\underline{b}, \bar{b})$ such that the probability mass function (pmf) of schooling is not degenerate. Within the signaling component, we restrict our attention to the signaling value of earning an educational credential, also known as credentialism, as in Hungerford and Solon (1987). The idea we aim to capture is that earning a credential conveys information to employers about valuable, unobservable characteristics unrelated to the process of human capital accumulation through schooling. For intuition, we borrow the idea from Fang (2001) that completing a seemingly arbitrary task displays to potential employers your ability to accomplish similar tasks on the job. In this setting, for example, earning a high school diploma conveys your ability to sit still in a seat, hand in homework on a regular basis, and avoid expulsion for a period of 12 years. Such traits may well be desirable and otherwise unobservable for firms looking to hire low-level white collar employees.

The first link from this framework to our empirical results studies the pmf of schooling, $f(s)$, as in Lang and Kropp (1986) and Bedard (2001). We argue that, in the absence of confounding institutional or contextual factors ${ }^{31}, f(s)$ provides information about the relative contributions of human capital accumulation and signaling in the returns to schooling. In a world where human capital accumulation is the dominant driver of the returns to schooling, the pmf of schooling in the population would then closely resemble the probability density function of the underlying characteristic(s) which induce heterogeneity in households' schooling choices for their children. The relative contribution of signaling/credentialism is revealed by the extent of bunching at credential attainment years, as this is an indirect measure of the benefit individuals enjoy at certain levels of $s, s_{i} \in\left\{S_{c}\right\}$, where a credential is conferred and $b(s)$ discontinuously increases. This yields our first result.

Proposition 1: If credentialism is the dominant driver of the returns to schooling, then $f\left(s_{i}\right) \gg f\left(s_{j}\right) \ll f\left(s_{k}\right)$ for any two consecutive credential attainment years $s_{i}, s_{k} \in\left\{S_{c}\right\}$, and any

[^14]non-credential attainment year $s_{j} \notin\left\{S_{c}\right\}$ satisfying $s_{i}<s_{j}<s_{k}$. This result follows directly from our assumptions about the marginal benefit of credential and non-credential attainment years. Appendix Figure A. 6 shows the distribution of schooling, assuming a uniform distribution of the source(s) of heterogeneity, for the two regimes.

This framework also generates substantially different predictions of the response to the policy we study. If we assume that, for the majority of the population, the main effect of the policy is to raise the cost of all additional post-primary schooling ${ }^{32}$, we generate our next proposition.

PRoposition 2:

$$
\left.\left.\sum_{i} \mathbf{1}\left\{s_{i_{\text {post }}} \neq s_{i_{\text {pre }}}\right\}\right|_{\text {credentialism dominant }} \ll \sum_{i} \mathbf{1}\left\{s_{i_{\text {post }}} \neq s_{i_{\text {pre }}}\right\}\right|_{\text {human capital accum. dominant }}
$$

In prose, if human capital accumulation is the dominant driver of schooling returns, we predict a decrease in the number of years of post-primary schooling for a far larger proportion of the population than if the signaling contribution were to be dominant. If signaling is the dominant driver of returns, we would predict no change in the equilibrium number of years of post-primary schooling for most, save the few for whom the increase in marginal cost is enough to edge the marginal cost schedule above the marginal benefit peak of the pre-policy equilibrium attainment level. The proof for this is straightforward. The main credentialism assumption is that there exist credential attainment years $s_{i} \in\left\{S_{c}\right\}$ such that $b\left(s_{i}\right)>b\left(s_{j}\right)$ for all $s_{j} \notin\left\{S_{c}\right\}$ in a neighborhood around $s_{i}$. This implies that for a given cost change $c \rightarrow c^{\prime},\left.P\left(\frac{\partial s^{*}}{\partial c}=0\right)\right|_{\text {credentialism dominant }}>$ $\left.P\left(\frac{\partial s^{*}}{\partial c}=0\right)\right|_{\text {human capital accum. dominant }}$ if $s^{*} \in\left\{S_{c}\right\}$, which gives our result. Appendix Figure A. 7 displays a case demonstrating the intuition behind this proposition.

In the next part of our framework, we engage with a prominent institutional constraint in the Chinese context: the high school and college entrance exams. Here we look for predictions about heterogeneity in the policy's effect on schooling. We study two likely determinants of whether a student ascends from one level of schooling to the next, household wealth and the child's genetic endowment. This is motivated by the important roles that heterogeneity in genetic endowment and material circumstances play in determining children's schooling careers and overall development (Black, Devereux, and Salvanes, 2005).

The object of interest is whether a student ascends from one level of schooling to the next: $s_{a} \in\{0,1\}$. We assume all families face the same benefit function $b(s)$, and that family wealth $w$

[^15]and genetic endowment $e$ determine the cost of keeping the child in school as she advances to the next level: $c(s)=g(w, e)$. We assume this cost function is decreasing in wealth and endowment $\left(\frac{\partial c(s)}{\partial w}<0, \frac{\partial c(s)}{\partial e}<0\right)$, capturing two notions. The first is that the higher the child's endowment, the less test prep the family must purchase to ensure the child passes the entrance exam. The second is that the cost of keeping the child in school and out of the labor market is felt less acutely as the family becomes richer.

We study a continuum of households heterogeneous in wealth, $w$, and child endowment, $e$. We assume that the distribution of these two characteristics is continuous over a range that produces a non-degenerate distribution of schooling in both the signaling- and human capital accumulationdominant scenarios, and that $e \perp w$. To simplify, we assume it is always preferable to go to school. There are absolute minimum levels of endowment, $\underline{e}$, necessary to even potentially pass the entrance exam, and wealth, $\underline{w}$, necessary to pay for school should the student pass the exam, such that if $e<\underline{e}$ or $w<\underline{w}$, the student does not ascend: $s_{a}=0$. In addition, if both are exceeded, additional expenditure of wealth is necessary for all but those with the highest endowment. This assumption captures a key feature of the Chinese education system as well as of many others in East and Southeast Asia: entrance exams are high-stakes and highly manipulable via test preparation (Lee, 2011; Jayachandran, 2014). This expenditure includes both money spent on tutoring, particularly exam preparation courses, and time. Time resources consist of parental time spent helping the child with her studies and time the family allows the child to spend doing homework and not housework (Zhang, Hannum, and Wang, 2008).

We assume the amount of wealth necessary to ascend is inversely related to the child's endowment ${ }^{33}$. These assumptions generate a schooling threshold in the endowment-wealth space, shown in Panel A of Appendix Figure A.8, determined by the two absolute minima and the function determining the minimum combinations of endowment and wealth needed to pass the test.

In this framework, there are two direct effects of the compulsory primary education expansion policy, as shown in Panel B of Appendix Figure A.8.

Proposition 3: $\bar{w}_{\text {pre }}<\bar{w}_{\text {post }}$. This follows directly from the nature of the policy: adding a year to the length of primary school increases the wealth threshold for ascent, as it requires an

[^16]additional year of expenditures and forgone wages to be borne for all households wanting to send their children on to further schooling. Table III presents our results testing for this effect; we find some evidence that rural households and females (who were allocated fewer resources in the household during our study period) may have been affected as predicted, though these estimates are small in practical terms.

Proposition 4: ${\underset{e}{p r e}}>\underline{e}_{\text {post }}$. By making all children go through an extra year of primary education, the schooling reform disproportionately helps those with lower endowments to pass the entrance exam, reducing the absolute endowment threshold ${ }^{34} \underline{e}$. This aspect of our theory is informed by the results of Meghir, Palme, and Simeonova (2013), who find that a compulsory education reform in Sweden had a similar differential impact on the cognitive skills of those with initially lower ability endowments. As we do not have data on cognitive skills collected before the policy is implemented, we are unable to take this test to the data. Nonetheless, this prediction of the model suggests that a policy such as the one we study would be redistributive towards those with lower genetic endowments.

This simple model formalizes how we interpret our empirical results. We see extensive bunching at credential attainment years and little difference between the pre- and post-policy distribution of post-primary schooling, both consistent with credentialism being the dominant driver of the returns to schooling. Also predicted by the model, we find (weak) evidence that the most vulnerable groups may have reduced their highest credential in response to the additional costs imposed by the policy.

These results are, of course, not conclusive proof of the primacy of the signaling channel. The model abstracts from many important aspects of reality which could generate the patterns we observe in the absence of an important signaling channel. In the next subsection, we enumerate a set of relevant concerns, describe their theoretical implications, and discuss their empirical plausibility in explaining our results.

## VI.B. Alternative Explanations for Bunching at Credential Attainment Years

In our theoretical framework, we abstract from several phenomena which could generate bunching at credential attainment years and inelasticity of demand for credentials with respect to credential

[^17]length in the absence of credentialism. The most prominent of these potential confounders are compulsory schooling laws, price differentials between levels of schooling, supply constraints, high human capital accumulation in credential attainment years, the lottery value of sitting a high school or college entrance exam, the cultural value of credential attainment, and reporting bias. In this section we deal with each in turn, providing evidence which suggests that few of these issues are of concern and none can account for more than a fraction of the bunching we set out to explain.

China's 1986 compulsory education law stipulated that all Chinese students had to complete primary and middle school. While the policy could conceivably have generated much of the bunching at the middle school credential attainment year we observe, we estimate that it is unlikely to have done so. Recent empirical work shows the law was gradually implemented and remains porously enforced, with many places not implementing it until the mid-2000's (Fang et al., 2012). Using province-specific policy implementation years from that study, we estimate the policy's rollout has no effect on whether individuals attain a middle school credential or on the number of years they spend in primary school ${ }^{35}$. We believe this null result is driven by porous enforcement of the law in rural areas and the fact that, in urban areas, high educational attainment levels (as seen in our data) make the law non-binding for the vast majority of affected individuals. Further supporting the claim of porous enforcement in rural areas, recent work has shown that as recently as 2010 the proportion of students not completing middle school was over $20 \%$ in some areas of rural China (Mo et al., 2013). In Panel A of Appendix Figure A.9, we show that the cohort-specific proportion of students who get less than a full middle school education (three years of post-primary schooling) is stable at around $20 \%$ throughout most of the period we study.

The increase in school fees students face as they advance to each higher level of schooling is also unlikely to explain much of the bunching we observe. Though there is evidence that these school fee jumps pose binding constraints for China's rural poor (Liu et al., 2009), it appears that cost differentials do not determine schooling levels for the majority of the Chinese population. Reductions in school fees have little impact on overall enrollment, even in rural China (Shi, 2012; Chyi and Zhou, 2014), and there is an abundance of qualitative accounts documenting that

[^18]the seats in Chinese universities are oversubscribed (Kipnis, 2011; Butrymowicz, 2012; Larmer, 2014). We test for a relationship between the cost of schooling and enrollment, using our main estimating equation and the introduction of school fees at the tertiary level in 1995 as plausibly exogenous variation in the cost gap between levels of schooling (Mok, 2000). We find no evidence that the increase in cost at this time is associated with a change in the proportion of individuals advancing to tertiary schooling.

Supply constraints are another possible driver of bunching at credential attainment years. While this explanation surely accounts for some of the bunching we observe, the importance of demand exceeding supply in generating bunching at credential attainment years is decreasing over the duration of our study. As China was continuously expanding its supply of schooling over the study period, the fertility reduction policies of the late 1970's and early 1980's reduced the number of individuals in a cohort competing for a given number of spots in school. National-level data plotted in Panel B of Appendix Figure A. 9 shows that supply constraints are being continually relaxed both at the university and high school levels over the period we study (Ministry of Education of the People's Republic of China, 2013).

Supply constraints are most salient at the transition from high school to tertiary education. Among those bunching at the high school credential attainment year, demand exceeding supply of university spots can explain only a fraction of the bunching we observe. In our estimation sample, $56 \%$ of high school finishers did not ascend to tertiary education. It is highly unlikely that these individuals finish their schooling in this year because the ultimate year of schooling generates a valuable increase in human capital. In China, the final year of high school is spent almost exclusively on review for the entrance examination to the next level. This year of review, similar to "cram school," is unlikely to contribute to human capital accumulation to the same extent as earlier years in the credential (Larmer, 2014). Another possibility is that the expectation or "lottery" value of sitting the college entrance exam is great enough to make it worthwhile for many to endure the final, review year of high school (Banerjee and Duflo, 2011). National statistics show that, during our study period, 20-50\% of individuals who make it to the final year of high school but do not ascend to tertiary education do not attempt the college entrance exam ${ }^{36}$. This means that at least half of the bunching we observe at the end of high school cannot be

[^19]explained by the lottery value story.
In China, the cultural value of credential attainment is important. This value, however, is almost exclusively placed on receipt of a college degree (Kipnis, 2011; Loyalka et al., 2013). As shown in Figure IV, only a small portion of our sample goes on to this stage and most of the rest know early on if they are likely to ascend to that level. This candidate explanation is therefore of little help in explaining the large amount of bunching at non-tertiary credential attainment years.

The last concern we engage is that measurement error from recall bias leads us to overestimate the extent of bunching. We provide three pieces of evidence that this is unlikely to be the case. First, we note that our CFPS data collects the years in which individuals enter and leave each level of schooling, reducing the risk of reporting bias relative to surveys which ask only how many years are spent in a given level. Second, we see that respondents appear to accurately report when the number of years of primary school needed to attain a credential changes from five to six, even when it is 25 years prior to their being interviewed. This suggests that our data on selfreports of the amount of time spent in each level of schooling are unlikely to suffer from recall bias. Third, we examine drop-out rates in a sub-sample of the educational gazetteers which report statistics on how many children are in each level of school. Overall, we find very low drop-out rates for middle and high school (less than 5\% per year), particularly in urban areas, consistent with our data on self-reports of years spent in school.

## VI.C. Cost-benefit Analysis

We next use our results to generate an estimate of the net benefit of the program. We borrow our framework directly from Duflo (2001), focusing on the private gains and losses and ignoring the other potential benefits of increased wages (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), it is useful to gain some insight into the net effect of such a tremendous 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 assume an annual discount rate of $5 \%$. Our cost estimate has as its sole argument the lost year of wages ${ }^{37}$, $w_{i t}$, that affected students $i$ forgo during the year $t$ they spend in school instead of in the labor

[^20]market:
$$
\text { Cost }=\sum_{i} w_{i t}
$$

For each cohort, we determine what proportion of individuals leave school with a primary, middle, high school or tertiary credential, and count the year lost as the last year they spent in school ${ }^{38}$. 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 assume there is no productive activity displaced by the policy other than the reallocation of affected individuals' time.

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:

$$
\text { Benefit }=\sum_{t} \sum_{c} \alpha G D P(t) S(c, t) P(c) \beta
$$

Here $\alpha$ is the share of labor in $\operatorname{GDP}^{39}, S(c, t)$ is the size of cohort $c$ divided by the total working population in year $t . \mathrm{P}(\mathrm{c})$ is the proportion of cohort $c$ affected by the policy, and $\beta$ 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 $65^{40}$.

In Table VI, we present four estimates, varying two key assumptions about the nature of $\beta$. The first is whether to assign the average treatment effect to all individuals or to take into account the changing educational profile of the Chinese citizenry over this time and use the credentialspecific treatment effects and data on the distribution of highest educational credential in each cohort. The left column's figures use the average treatment effect estimates, while the figures in the right column use credential-specific estimates. The second is to decide how to estimate the effects for rural China. So far we have presented estimates for urban areas only, citing concerns about migration, the possibility for intensive or extensive reallocation of labor between agricul-

[^21]ture and the non-farm rural labor market, and the great changes affecting rural China over this period. The policy, however, was implemented in both urban and rural areas and, until 2011, more than half of China's population was rural. To assess the likely fiscal impact of the policy on all of China, rural and urban, we present two sets of estimates. In the first row, we use the urban estimates on returns for both urban and rural residents. In the second row, we use the treatment effect estimates specific to urban and rural areas (the average treatment effect in the left column, then the credential-specific effects estimated off of only rural residents) weighted by the population share in a given year for the two groups.

Under three of the four scenarios, the policy's costs exceed its benefits by tens of billions of dollars. 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 2014. We estimate this too to be a net loss of 456 million dollars. These figures underestimate the total costs, as we do not compute the value of parents' expenditures during that extra year on students' food, clothing, and other necessities. A conservative estimate of these would add hundreds of dollars to the per-person cost of the policy, which is tens of billions more in costs over the policy's lifetime.

## VI.D. Policy Implications

We conclude this section with the policy implications of our research. Together, our results highlight the massive amount of resources that can be reallocated by a seemingly arbitrary policy choice: how many years should children spend in each level of school? To gain an idea of the generalizability of this finding, we analyzed the most recent DHS data from those 74 countries in which the number of years a respondent spends in school is collected. In 48 of these countries, we found evidence of bunching at multiple credential attainment years similar to that shown in Figure IV. We are less confident about the robustness of the DHS data to concerns about reporting error than we are for the Chinese data, but these patterns are consistent with our claim that government decisions on how long to make each level of schooling have massive resource implications ${ }^{41}$.

Though we estimate the policy to be a net loss in China's case, it was redistributive: those with

[^22]lower levels of schooling gained the most from it. This section's analysis reveals that bunching at credential attainment years is commonly found in developing countries. In such countries, a policy similar to the one we study which adds a year to the lowest level of schooling could be an effective way to ensure the less well-off gain skills valued in the labor market. The bleak costbenefit estimates we generate in China's case are driven by the large increase in the proportion of individuals with secondary and tertiary credentials over time. In a country where the average number of years spent in school is lower and grows less quickly than it did in China between 1980 and 2015 , the benefits of such a policy could well exceed the costs.

## VII. Conclusion

In this paper, we exploited a massive policy change in China's educational system to study how household decisions on children's schooling respond to extending the length of primary school by one year. We observe extensive bunching at credential attainment years both before and after the policy. The vast majority of affected individuals spent an additional year in school, sacrificing an entire year of earnings, in order to earn the credential they would have attained in the absence of the policy. These results allowed us to generate a new parameter estimate of the returns to a year of schooling, holding highest educational credential constant. We found a small but precisely estimated two percent increase in monthly income which is higher for individuals with less schooling, those whom the policy set out to assist.

We then framed these results in relation to previous work on the roles of signaling and human capital accumulation in generating returns to schooling. We interpret our results as suggestive that the signaling value of attaining a credential is the dominant driver of two important phenomena in modern China: household schooling decisions and the high returns to schooling we see in the cross-section. Finally, we estimated the costs and benefits of the nearly one trillion person-hours this policy has reallocated from the labor market to the pursuit of schooling, finding that in most scenarios the policy generates a net loss of tens of billions of dollars.

In our policy analysis, we argue that our paper shines light on an important research lacuna: government policy which sets the length of each level of schooling is under-researched given the amount of resources at stake. In addition to the value of the time these policies reallocate, over three trillion US dollars are spent worldwide on education by national governments each year (World Bank, 2012). Essentially every government on the planet must decide how long
nationally-sanctioned school will last and how much to spend on it. Providing more informed guidance on how to make these decisions is an important next step for education research in economics.

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TABLES
Table I: Data sources

| Data <br> source | Year(s) <br> collected | Sample <br> size | Relevant <br> contents | Policy year <br> identification <br> method | Geographic <br> specificity |
| :--- | :--- | :--- | :--- | :--- | :--- |
| County educational <br> gazetteers | Various | $345^{*}$ | Date policy implemented, <br> implementation details | N/A | County** |
| Mini-census | 2005 | $2,585,481$ | Schooling: highest credential, dropout <br> Labor market: income, employment | Archival <br> records | Prefecture** |
| China Family Panel <br> Studies (CFPS) | 2010 | 33,600 | Schooling: years spent in each level <br> of schooling | Algorithm | County |
| Labor market: income, employment | China Labor-force <br> Dynamics Survey <br> (CLDS) | 2012 | 16,253 | Schooling: years spent in each level <br> of schooling | Algorithm <br> and archival <br> records |

[^23]Table II: Effects of the policy on schooling outcomes - average treatment effects

| Outcome | CFPS | 2005 Census |
| :--- | :---: | :---: |
| Probability: at least six <br> years of primary school | $.547 * * *$ <br> $(.0294)$ |  |
| Years spent in all levels | $.66^{* * *}$ |  |
| of school | $(.209)$ |  |
| Years of post-primary | .0928 |  |
| schooling | $(.204)$ |  |
| Highest credential: at least <br> middle school | .00368 | -.00495 |
|  | $(.0319)$ | $(.00303)$ |
| Highest credential: at least <br> high school | .000299 | $.00628^{*}$ |
| Dropped out of school, | $(.0263)$ | $(.00327)$ |
| any level |  | -.00017 |
| Observations in sample | 2,680 | 243,548 |

* $\mathrm{p}<0.10,{ }^{* *} \mathrm{p}<0.05,{ }^{* * *} \mathrm{p}<0.01$. Each cell presents a treatment effect estimate from a separate regression with the relevant robust standard error below, in parentheses. Standard errors are clustered at the county (CFPS) or prefecture (census) level.

Table III: Effects of the policy on schooling outcomes for vulnerable subgroups

| Outcome | Female | Rural | Rural female |
| :--- | :---: | :---: | :---: |
| Years of post-primary schooling |  |  |  |
| CFPS | -0.2020 | 0.2357 | -0.0472 |
|  | $(0.2951)$ | $(0.2673)$ | $(0.4008)$ |
| Graduated from middle school |  |  |  |
| CFPS | -0.0355 | 0.0550 | 0.0304 |
|  | $(0.0498)$ | $(0.0462)$ | $(0.0696)$ |
|  |  |  |  |
| Census | -0.0057 | -0.0049 | -0.0044 |
|  | $(0.0043)$ | $(0.0046)$ | $(0.0063)$ |
| Graduated from high school |  |  |  |
| CFPS | -0.0221 | 0.0238 | 0.0133 |
|  | $(0.0400)$ | $(0.0195)$ | $(0.0352)$ |
| Census |  |  |  |
|  | 0.0043 | -0.0016 | -0.0030 |
| Dropped out of school, any level | $(0.0039)$ | $(0.0025)$ | $(0.0028)$ |
| Census | $0.0037^{*}$ | -0.0003 | $0.0047^{*}$ |
|  | $(0.0019)$ | $(0.0021)$ | $(0.0028)$ |
|  |  |  |  |
| Observations in CFPS sample | 1,277 | 1,293 | 592 |
| Observations in Census sample | 126,081 | 157,308 | 81,490 |

* $\mathrm{p}<0.10,{ }^{* *} \mathrm{p}<0.05,{ }^{* * *} \mathrm{p}<0.01$. Each cell presents a treatment effect estimate from a separate regression with the relevant robust standard error below, in parentheses. Standard errors are clustered at the county (CFPS) or prefecture (census) level.

Table IV: Effects of the policy on labor market outcomes

| Outcome | Estimates |
| :--- | :---: |
| Panel A: average treatment effects | 0.0026 |
| Currently employed | $(0.0052)$ |
|  |  |
| Works for government | 0.0035 |
|  | $(0.0062)$ |
| Log of monthly income, using cohort and place | $0.0194^{* * *}$ |
| fixed effects | $(0.0073)$ |
|  |  |
| Log of monthly income using cohort, place, and | $0.0203^{* * *}$ |
| cohort-by-region fixed effects | $(0.0071)$ |
|  |  |
| Panel B: effect on log of monthly income, by highest credential |  |
| Primary school | $0.0694^{\dagger * *}$ |
|  | $(0.0279)$ |
| Middle school | $0.0503 * * *$ |
|  | $(0.0095)$ |
| High school | $0.0257^{* * *}$ |
|  | $(0.0092)$ |
| Tertiary schooling | $(0.0076)$ |
| Panem C: effect on log of monthly income, by gender |  |
| Men | $(0.0098$ |
| Panel D: effect on log of monthly income, by employer |  |

* $\mathrm{p}<0.01,{ }^{* *} \mathrm{p}<0.05,{ }^{* * *} \mathrm{p}<0.01$. Data: census. For first row, $\mathrm{N}=86,240$. For all other regressions, $\mathrm{N}=66,425$. Panel A presents results from four different regressions. In panels $B, C$, and $D$, we present coefficients from a single regression as specified in the panel title. Panels B, C, and D present coefficients of a dummy variable for membership in the group given in the left column (e.g. those whose highest credential is primary school) interacted with the treatment dummy. 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. ${ }^{\dagger}$ Panel B coefficients, when weighted by proportion of sample given in column 4 of Table V, sum to 0.0213 .

Table V: Putting our estimates in context

|  | (1) | (2) | (3) | (4) <br> Percent of |
| :---: | :---: | :---: | :---: | :---: |
| Credential | Credential <br> premium | Per-year <br> return | Percent <br> estimation <br> sample |  |
| Primary school | - | - | 62.3 | 3.0 |
| Middle school | 23.7 | 7.93 | 66.9 | 28.9 |
| High school | 25.3 | 8.42 | 77.7 | 31.8 |
| Technical college | 25.7 | 8.58 | 92.1 | 22.8 |
| University | 55.7 | 13.9 | 95.8 | 13.4 |
| Our estimate of the return to <br> a sixth year of primary school <br> Li et al. (2012) twins estimate | - | $1.9-2.0$ | - | - |
| of the returns to year of schooling |  | $2.7-3.8$ | - | - |

$\mathrm{N}=85,048$. Data: census. Sample: 5-year bandwidth, employed urban residents. Degree premia in Column (1) are measured relative to next highest credential: middle school relative to primary earners, high school relative to middle, and technical college and university both relative to high school. Column (2) presents the figures in Column (1) divided by the number of years needed to attain the relevant credential.

Table VI: Cost-benefit calculation: 1981-2050

|  | Assumption about heterogeneity <br> in $\beta_{1}$ by highest credential held |  |
| :---: | :---: | :---: |
| $\beta_{1}$ estimate <br> used for rural areas | Using average <br> treatment effect for all | Using credential-specific <br> treatment effects |
| Using estimates from urban areas | $-36,462$ | 34,902 |
| Using rural effect estimates from our data | $-82,390$ | $-212,220$ |
| Costs | 178,497 | - |
| Cost-benefit calculation for cohort leaving <br> primary school in 2014 (urban $\beta_{1}$, average $\left.\beta_{1}\right)$ | -456.4 |  |

[^24]
## Figures

Figure I: Length of each credential, pre- and post-policy

## Years of schooling



This figure depicts the length 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 under normal circumstances. Before the policy, it took five years to finish a primary school credential, as represented by the height of the box labeled "Primary" in the left column. Middle, High, and Tertiary refer to junior high school, senior high school, and university, respectively. To finish Middle School before the policy is implemented, an individual would need 8 years of school - five years of Primary and three of Middle, which can be read from the top line of the "Middle" box. The dashed bold line stemming from the y -axis at $\mathrm{y}=8$ indicates that prior to the policy, the median and mode of years of schooling is 8 . The right column depicts the change introduced by the policy, adding an extra year to the time it takes to attain a Primary credential. The lines connecting the left and right column depict the change in the total number of years it takes to earn each credential. The dashed red lines show the trade-off that the modal student faces after the policy.

Figure II: Predetermined characteristics across the treatment threshold


Figure III: Proportion of students spending at least six years in primary school before and after policy change


Data source: CFPS

This plot shows distance-to-treatment-year bin means of the proportion of individuals spending at least six years in primary schooling before and after the policy. The vertical line separates the affected (to the right of the line) and unaffected (to the left) cohorts.

Figure IV: Distribution of post-primary schooling before and after policy change


This plot shows the probability mass functions of post primary schooling for observations in the CFPS data graduating from primary school within five years before (pre-) and after (post-) the policy implementation year in each county.

Figure V: Stability of main regression estimates across bandwidth choice
Panel A-Treatment effect estimates and confidence intervals for post-primary schooling


Data source: CFPS
Dependent variable: years of post-primary schooling

Panel B-Treatment effect estimates and confidence intervals for log monthly income


Figure VI: Monte Carlo analysis - distribution of effect estimates of placebo treatment on log monthly income from 1,000 draws of placebo years


Figure VII: Kernel density of cognitive skills test score, by treatment status


Data source: CFPS

This plot shows the kernel density of cognitive skills test scores for observations from the CFPS data, plotted separately for those in affected and unaffected cohorts, in the five year bandwidth sample.

## APPENDIX

## Appendix 1-Appendix Figures

Figure A.1: National data on proportion of students in six year primary education


Figure A.2: Timing of implementation by prefecture


Note: In some cases, prefecture boundaries have changed since the archive was published. In these cases, we take the prefecture level-average of the treatment year in all previous prefecture capitals falling within the new prefecture. In prefectures that are colored white, we are unable to assign a unique prefecture-level treatment year for reasons discussed in Section III.

Figure A．3：Example of a page from a county gazetteer

第二章 建国后小学救育 25

| 续表 |
| :--- |
| 1979 |
| 1980 |
| 1981 |

2．学制，课程设置及教法
1976年，全县小学实行 ti 年制。1984年，按照省教育厅要求，将小学九午制改为六午制，并在两午内完成。按照教育部门的要求，境内小学采取农村以乡，城镇以校为单
 1985年应届毕业生三分之一升入初中，留校三分之二升入六年。

1976午秋，学校所开课程有语文，数学，自然，地理，历史，高乐，美术，体育，劳动，使用全国统缠教材。1982年增设思想品德课。1984年，仍延用原有的课程设㽝及教材。

1976年后，各小学校遂步㬵复正常教学秩序。1979年，县革委会发出了55劳文件，摘掉了小学附设的初中屍。恢侮了中心校管理体制。1981年，学校工作的立足点转为以教学为中心，注重提高教学质是，加强教学研究利改革，强调＂双基＂（基础知识，基本技能）教学。要求教师立足课堂，提高课堂教学效果，着眼点放在培养学生能力，羽发学生智力上。

1984午，各校进䘕教学改革试验，坚持以学生为主体，以教师为主导，昘面进行教学T作粘体改革。按照数育局要求，各校成立了课外活动小组，开展了＂第二课堂＂活动。学校在加绌基础知识教学问时，还开展了小学语文，数学知识竟赛。是年，全县小学毕业生双科合格率达 $90 \%$ 。

## 第二节 教育体制改革后的小学教育

## －，学校发展概况

1985午，全县有小学校 335 所，其中教育部门和集体办 320 所，其他部门办 15 所，议立下仲点65个（教育部门和集体外58个，其他部门外7个）。在校学生80464人（女学生38583人），其中教育部门利集体办76 135人，其他部门外4329人。教学㭚2 431个，其中教育部门外和集体非 2294 个（复式班 105 个），其他部门外 137 个（复式班 2个）。一部制学校7所，有教学班42个。教职T3 653人，其中教育部门朴1848人，集体小 605 人，其他部门办 200 人。

1987年，林海，三合乡划州H城巾（现洮北区）后，境内有学校 318 所（小学中心校28所），教学点58个。在校学生 74992 人，教学玶 2334 个（复式形 100 个，二部制班 56个），教职T． 3517 人。

1989年，城内小学建成2座教学楼，建筑面积6460平方米。其中，实验小学教学楼建筑向伿3260半方米；第四小学教学楼建筑面积3200平方米。

Figure A.4: Example of mean shift algorithm identifying year of policy implementation


Figure A.5: Mean schooling levels before and after policy implementation, by distance to treatment year cohort





Data source: CFPS

Figure A.6: Predicted distribution of schooling by dominant channel


Figure A.7: Predicted policy response, by dominant channel

## Human capital accumulation



Signaling
MC/MB


Figure A.8: Incorporating heterogeneity in wealth and genetic endowment into our model
Panel A: The schooling ascent threshold in the wealth/endowment space


Panel B: The predicted impact of the policy on schooling ascent


Figure A.9: Evidence of students' ability to adjust on lower and upper margins
Panel A: Ability to adjust on lower margin

*Vertical line at annoucement of compulsory middle school Data source: CFPS

Panel B: Ability to adjust on upper margin


Figure A.10: National statistics and province-level CDFs of policy implementation
First 12 provinces


These plots show the proportion of individuals enrolled in six-year primary schools in the 24 nonShanghai CFPS provinces. The sequence of hollow circles represent the proportion of treated counties in a given province by year according to county-specific implementation years identified in the CFPS data. The sequence of solid circles are from publicly available, Chinese Ministry of Education statistics on the number of schoolchildren benefiting from six year education in a given province by year. Measurement error and policy experimentation cause these to briefly trend downward in a few cases.

Figure A.11: National statistics and province-level CDFs of policy implementation
Second 12 provinces


These plots show the proportion of individuals enrolled in six-year primary schools in the 24 nonShanghai CFPS provinces. The sequence of hollow circles represent the proportion of treated counties in a given province by year according to county-specific implementation years identified in the CFPS data. The sequence of solid circles are from publicly available, Chinese Ministry of Education statistics on the number of schoolchildren benefiting from six year education in a given province by year. Measurement error and policy experimentation cause these to briefly trend downward in a few cases.

Figure A.12: Comparing archival and algorithmically identified treatment years


Table A.1: Anecdotes on implementation of six year primary education from across China

| Locality | Strategy of Changing 5-Year to 6-Year Primary Education | Source |
| :---: | :---: | :---: |
| Beijing | In 1969, the length of primary education was shortened to five years. Starting from September 1st, 1980, the length of primary education was prolonged to six years, with the fifth-grade students continuing to be in the old system and students of other grades entering into the new system. | Beijing General Education Chronicle (Part I) |
| Xinle <br> County, <br> Hebei <br> Province | In 1967, the length of primary education was shortened to five years. Starting from August 1985, first-grade students and one half of second-grade students entered into the new six year system, while the rest of students remained in the old system. | Xinle Educational Chronicle |
| Nanjing City, Jiangsu Province | In 1969, the length of primary education was shortened to five years. Starting from 1982, the length of primary education was prolonged to six years in urban districts and children started primary education at the age of six. By contrast, the length of primary education remained to be five years until 1999 in five other counties (Jiangning, Jiangpu, Liuhe, Gaochun, and Lishui). | Nanjing Educational Chronicle |
| Wuyi County, Zhejiang Province | In 1984, first-grade students entered into the new six year system in half of primary schools, with the other half following the next year. In 1987, however, all primary schools were required to resume the five year system. In September 1999, all primary schools were restored to the new system of six years. The final cohort under the old system graduated in June 2004. | Wuyi Educational Chronicle |
| Dongying District, Shandong Province | In 1997, the compulsory education system changed from the 5-3 (years of primary school-years of middle school) to the 5-4 system. In 2003, first-grade students entered into the new 6-3 system. | Dongying District Chronicle (1998-2005) |
| Xishui County, Hubei Province | In 1986, the first-grade students entered into the new 6-3 system in the primary schools located in county seats, while other primary schools remained in the old 5-3 system. In 1987, the first-grade students in the primary schools located in the township entered into the new 6-3 system. In 1991, first-grade students in the remaining primary schools entered into the new 6-3 system. | Xishui Educational Chronicle (1986-2006) |

Table A.2: Summary statistics

|  | Data set |  |  |  |
| :--- | :--- | :--- | :--- | :--- |
|  | Census |  | CFPS |  |
| Proportion | Rural | Urban | Rural | Urban |
| Demographics |  |  |  |  |
| Female | 0.518 | 0.517 | 0.458 | 0.494 |
| Ethnic minority | 0.172 | 0.070 | 0.081 | 0.050 |
|  |  |  |  |  |
| Highest credential |  |  |  |  |
| Middle school | 0.575 | 0.337 | 0.489 | 0.417 |
| High school | 0.060 | 0.319 | 0.157 | 0.231 |
| Tertiary | 0.007 | 0.302 | 0.029 | 0.141 |
|  |  |  |  |  |
| Labor market characteristics |  |  |  |  |
| Currently working | 0.909 | 0.780 | 0.597 | 0.693 |
| Private sector | 0.181 | 0.317 | 0.151 | 0.320 |
| Entrepreneur | 0.074 | 0.120 | 0.383 | 0.195 |
|  |  |  |  |  |
| Observations | 157,308 | 86,240 | 1,293 | 1,387 |

This table presents descriptive statistics of demographic, educational, and labor market characteristics for our two main datasets using the five-year bandwidth estimation sample. This is the same sample used to generate the regression estimates presented in the paper's main tables.

Table A.3: Regression of prefecture characteristics on timing of treatment

| Variable | Coefficient | Standard error |
| :--- | :---: | :---: |
| Proportion male | -1.281 | 6.558 |
| Proportion minority | 0.023 | 0.751 |
| Proportion urban | 0.020 | 1.352 |
| Proportion with government job | 1.700 | 3.515 |
| Mean log income | -0.322 | 0.657 |
| Mean education level (1-5 scale) | 0.122 | 0.829 |
| Central region | $-1.340^{* * *}$ | 0.472 |
| Northeast region | $-1.940^{* * *}$ | 0.503 |
| Western region | -0.323 | 0.503 |
|  |  |  |
| Treatment year summary statistics | 1986.1 | 2.229 |

Data: census. Robust standard errors in parentheses.
$\mathrm{N}=240$ prefectures. $* \mathrm{p}<0.10, * * \mathrm{p}<0.05,{ }^{* * *} \mathrm{p}<0.01$

## Appendix 3 - Details on Our Use of the Mean Shift Model

As we use an algorithm to determine treatment year status for observations in the CFPS, we introduce a few safeguards to ensure that we are not erroneously choosing a year when there is no change in treatment status or not enough evidence to determine the year. Specifically, we exclude counties in which:

1. The cohort-level median number of years of primary school in a given county is never less than 5.5 before the identified treatment year or is never more than 5.5 after;
2. The mean shift model predicts a treatment year after which individuals have less primary schooling, that is, $\gamma_{1}<0$ in equation 1 ;
3. There are more than four cohorts for which we have no observations within five years before or after the identified treatment year (that is, data for at least five of the ten cohorts in this county's restricted bandwidth sample are missing).

This exercise excludes 32 of the 144 non-Shanghai counties, or about $78 \%$ of the sample. All of the results we show are qualitatively similar with the entire 144 county sample and are available by request.

As a final check of accuracy, we visually compare the treatment year generated by the mean shift model to each county's histogram. In four cases, our inspection of the histograms suggested a year more than one year different than the mean shift model's choice and we use this visually identified year in our main analyses, controlling for those counties whose year was chosen visually as opposed to algorithmically.

To check that these algorithmically identified years are credible, we conduct two exercises. First, we compare the mean shift years for all counties within each province to national statistics on provincial-level implementation, shown in Appendix Figures A. 10 and A.11. The two series track closely and their correlation is 0.7759 . Second, we apply both the mean shift and archival match policy year identification methods to the China Labor-force Dynamics Survey data. Appendix Figure A. 12 shows the distribution of the difference between archival and algorithmically identified treatment years. These values are normally distributed around 0 , and in $67 \%$ of prefectures the years identified by the two methods are within 3 years of each other. This amount of
discrepancy is not surprising, as in the gazetteers we see that counties occasionally implemented at different times within a prefecture. Additionally, the number of observations in cohort-byprefecture bins in the Labor-force Dynamics dataset is small, which implies imprecision (relative to using the CFPS) in our ability to algorithmically determine the implementation year.

## Appendix 4 - Details of Cost-benefit Analysis

This section outlines our data sources and extrapolation assumptions for the cost benefit analysis we perform. The costs of the policy we include consist of one year of forgone wages for all affected individuals. We assume the value of one individual's forgone year of wages is equal to $\frac{\alpha * G D P_{t}}{N_{t}}$, where $N_{t}$ is the size of the active workforce in year $t$. We calculate if and when the forgone year falls for each individual born on or after 1969 (as the first affected cohort would be leaving primary school in 1981). To calculate the total number losing a given year in each cohort, we multiply the total number of individuals in each cohort (generated using data from the Chinese National Statistical Bureau and World Bank World Development Indicators data) by the proportion of individuals with a given credential in each cohort (calculated from the CLDS data). We assume the lost year is the year a person would have entered the labor force in the absence of the policy, e.g. for someone who was born in 1980 and finishes school with a middle school credential, we calculate the value of the work they would have done in 1995 when they would have left school in the absence of the policy (starting school at age 7, spending five years in primary school and three years in middle school). We assume that those leaving school after primary school do not lose a year of work, as they will leave school at the age of 12 and are not likely to immediately enter gainful employment. For simplicity, we assume that everyone enters school at age 7.

The benefits from the policy consist of the boost in earnings we estimate for affected individuals accruing throughout their lives. This parameter is taken directly from our estimates in Section V. We assume that the benefits affected individuals enjoy are constant throughout their lifetimes, and calculate the total value of the earnings gain for each affected individual up to the year 2050. Table A. 4 provides our data sources for each of these figures and assumptions used to extrapolate into the future and where data is not available.

We exclude a few potential sources of costs and benefits, believing them to be several orders of magnitude smaller than the lost year of wages and the lifetime of income gains. On the costs
side, we exclude the cost of hiring new teachers and the cost of building new facilities. We do so because archival records suggest that in the short term, the current staff was used to meet most of the increased personnel needs imposed by the policy and classrooms could be split, thus requiring no sizable infrastructure additions. In the long term, the one child policy reduced the number of children in schools, and so existing staff levels and infrastructure house a smaller and smaller number of children. We assume that this would largely offset the need for long term staff and infrastructure adjustments to accommodate the extra cohort of primary students. On the benefits side, we disregard the possible benefit of the creation of new teacher jobs, as our assumption about the staffing costs of the policy implies very few additional jobs would be created. As in Duflo's approach, we exclude the possible beneficial impact of the extra year of schooling on health, fertility and other non-financial outcomes.
Table A.4: Cost-benefit calculation details

| Data point | Source | Range of data | Range for extrapolation | Assumptions used | What used for | Notes |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| Birth rate per 1,000 women | stats.gov.cn | 1966-2013 | N/A | N/A | Size of cohort | Assume: cohort size constant 2013-2050 |
| Number of women | stats.gov.cn | 1970-2013 | N/A | N/A | Size of cohort | - |
| Total labor force | World Bank WDI (World Dev. Indicators) | 1981-2015, five-yearly estimates for 2020-2050 | Gaps in estimates for 2020-2050 | Linear trend between estimates | Total labor force | Close to stats.gov.cn figures, but with predictions to 2050 |
| Proportion of cohort with each education level | China Labor-force Dynamics Survey (CLDS) | Birth cohorts 1969-1987 | Birth cohorts 1987-2035 | Linear trends from previous 9 years and rules of probability $(0 \leq P \leq 1)$ | CB estimates with cohort-specific effects | Begin extrapolation at 1987 cohort to avoid measurement error |
| Proportion of cohort affected by policy | China's National <br> Bureau of Statistics | 1981-2010 | 2010-2050 | Full coverage after 2010 | Proportion affected by policy | Equate 5 year primary +4 year middle with 6 primary +3 middle |
| Alpha (labor's share of GDP) | Karabarbounis and Neiman (2014) | 1992-2009 | $\begin{aligned} & 1981-1991 \\ & 2010-2050 \end{aligned}$ | 1981-91: average of $\mathrm{t}+1$ to $\mathrm{t}+3$; 2010-50: average of t-1 to t-3 | Argument in final calculation | - |
| $\begin{aligned} & \text { GDP (constant } \\ & 2005 \text { US\$) } \end{aligned}$ | World Bank WDI | 1981-2013 | 2014-2050 | Growth rate declines annually from 2013 rate to $4 \%$, annual decrease of 0.15\% | Argument in final calculation | - |


[^0]:    *Eble (corresponding author): Department of Economics, Brown University. 64 Waterman Street, Providence, RI 02912. Email: alexander_eble@brown.edu; Hu: Dongling School of Economics and Management, University of Science and Technology Beijing, 30 Xueyuan Road, Haidian District, Beijing, China 100083. Email: feng3hu@gmail.com. We would like to thank Andrew Foster, Emily Oster, and John Tyler for extensive feedback and guidance, and Alexei Abrahams, Anna Aizer, Dionissi Aliprantis, Marianna Battaglia, Natalie Bau, Nate Baum-Snow, Ken Chay, Andrew Elzinga, John Friedman, David Glancy, Nate Hilger, Rob Jensen, Melanie Khamis, Eoin McGuirk, Bryce Millett-Steinberg, Sri Nagavarupu, Gareth Olds, Anja Sautmann, Rajiv Sethi, Jesse Shapiro, Zach Sullivan, Felipe Valencia, and seminar participants at APPAM, Brown, Cornell, NEUDC, SOLE, and Wesleyan for helpful suggestions. Eble gratefully acknowledges support from the United States National Science Foundation through a Graduate Research Fellowship and an IGERT Fellowship, and financial and computing resources from the Brown University PSTC. Hu acknowledges financial support from the National Natural Science Foundation of China (71373002, 71133003, 71420107023). All remaining errors are our own. JEL codes: I25, I26, J24, O15.

[^1]:    ${ }^{1}$ Though middle school was made compulsory in 1986, in Section VI.B we show evidence that the rollout of the compulsory middle school policy has little effect on whether or not individuals complete middle school or earn at least a middle school credential.
    ${ }^{2}$ County or prefecture, depending on the level of geographic resolution in each data source.

[^2]:    ${ }^{3}$ 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 2010-2015 under the six-year regime, assuming negligible drop-out from primary school, is $90,045,459$. 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 hours 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 0.85 trillion person-hours of labor reallocated to schooling between 1981 and 2015.

[^3]:    ${ }^{4}$ In China, middle school and high school are referred to as junior middle school and senior middle school. We refer to them here as middle school and high school to facilitate a layperson's understanding.
    ${ }^{5}$ In Shanghai and a few other localities, this policy was implemented instead by requiring that middle school last four years instead of three.

[^4]:    ${ }^{6}$ 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.

[^5]:    ${ }^{7}$ Though the full sample is approximately 13 million observations, researchers are granted access to $20 \%$ sub-samples of the parent dataset.
    ${ }^{8}$ 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. For this analysis, we use only the 2010 wave. 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 Lv and Xie (2012).
    ${ }^{9}$ Recent work by Almond, Li, and Zhang (2013) uses Chinese gazetteers to identify when land-reform policy was implemented in different counties across the country.
    ${ }^{10}$ In many prefectures, urban implementation occurred before rural implementation.
    ${ }^{11}$ Counties, prefectures, and provinces are the Chinese geographic divisions of interest to this study. There are several counties in a prefecture and several prefectures in a province.

[^6]:    ${ }^{12}$ This mean-shift approach is similar to that used Munshi and Rosenzweig (2013).
    ${ }^{13}$ An example of this process is shown in Appendix Figure A.4, which shows the histogram of cohort-mean years of primary school in a county and plots the ssr estimates generated by Equation 1 for each treatment year. The ssr sequence reaches its minimum at 1997, where we also observe a clear shift upwards in mean years of primary school from approximately five to six.
    ${ }^{14}$ 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 only those 112 counties in which we can detect a clear policy change. 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.
    ${ }^{15}$ 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).

[^7]:    ${ }^{16}$ As mentioned earlier, this is at the county-level in the CFPS data and prefecture in the census.

[^8]:    ${ }^{17}$ In Appendix Table A. 1 we give six examples from gazetteers of how the policy is implemented which speak to the need to control for the possibility of implementation varying over time.
    ${ }^{18}$ China can be separated into four well-recognized regions: East, Northeast, Central, and West.
    ${ }^{19}$ This coincides with the end of the Cultural Revolution and the end of the chaos it brought to the educational system of China.

[^9]:    ${ }^{20}$ 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.

[^10]:    ${ }^{21}$ This assumes the extra year of primary school does not confer a major gain in skills sufficient to induce individuals to proceed further in schooling. We provide evidence for this assumption in the next section.
    ${ }^{22}$ Zero years of schooling is the end of primary school, three is the end of middle school, six the end of high school, nine the end of technical college, and ten the end of university.
    ${ }^{23}$ In Appendix Figure A.5, we plot the trends over distance-to-treatment year bin means in raw data (left column) and cohort-demeaned residuals (right column) for total years of schooling (top row) and years of post-primary schooling (bottom row). We see the same patterns as in the regression coefficients: an upward jump of about one year of total schooling at the treatment threshold, and no downward jump in post-primary schooling at the treatment threshold, both for the raw and demeaned data.

[^11]:    ${ }^{24}$ By 2005, we expect most workers to earn wages that are at least strongly correlated with their relative productivity (Zhang et al., 2005).
    ${ }^{25}$ When estimating the effect of the policy on income, we drop those 266 observations from the five-year bandwidth estimation sample ( 163 in the treatment group, 103 in the control; out of 66,691 observations) who are working but report zero monthly income.
    ${ }^{26}$ We do not have labor histories for individuals, and so cannot apply the normal Mincerian equation using years worked (experience) as an independent variable. Instead, we assume individuals are gaining experience in each year they are not in school. Under this assumption, the birth year (cohort) and credential level fixed effects are a sufficient statistic for the experience of the individual.
    ${ }^{27}$ Results omitting these fixed effects are similar in magnitude, varying by less than $0.5 \%$.

[^12]:    ${ }^{28}$ To run this test, we divide our sample into three groups based on when the policy was implemented: 1981-85, 1986-1990, and 1991-1995. The earlier the implementation, the more individuals exposed to the extra year of primary schooling, and so the closer the area is to the general equilibrium state of all workers being treated with this extra year. Though the earliest implementing group has a smaller treatment effect estimate than the later two groups $(0.013,0.033$, and 0.031 for early, middle, and late implementers), consistent with the general equilibrium effect being smaller than the partial equilibrium effect, a Wald test fails to reject the equality of these three coefficients (p-value of the f-test: 0.32 ).

[^13]:    ${ }^{29}$ Recall that in our estimation sample, nearly $80 \%$ of observations go on to get at least some post-primary schooling and primary school is compulsory for all.
    ${ }^{30} \mathrm{We}$ abstract from discussions about investment in children depending on the bargaining outcome between individual parents and/or parents and the child. For a treatment of these issues, see Bobonis (2009). We also abstract from issues of multiple children and inequality aversion here. As educational levels are rising over this period, multiple children and inequality aversion on the part of the parents will only make it more likely for us to observe a downward shift in postprimary attainment among treated individuals, as the shock of "extra" schooling to one child raises expenditure and also pushes the parent to reallocate from the affected child to unaffected children. We see no such downward shift. Finally, we assume once a child leaves school, she is unable to return.

[^14]:    ${ }^{31}$ The most probable of these confounding factors are discussed in the next subsection.

[^15]:    ${ }^{32}$ This cost increase comprises a year of forgone wages, a year of school and transport fees and, assuming the child moves away from home after the completion of school, an additional year of food and living costs.

[^16]:    ${ }^{33}$ There is a large literature, starting with Becker and Tomes (1976), on whether expenditure on children's education is positively or negatively correlated with children's endowments. Our assumption here is only about the likelihood of a child passing an entrance exam, and is an extension of two simple assumptions: 1) a child's likelihood of passing an entrance exam is positively related to both her endowment and the amount of resources spent on exam preparation, and 2) endowment and exam preparation are substitutes in the production function for passing an entrance exam.

[^17]:    ${ }^{34}$ Note that we could generate the same result using a relative instead of an absolute threshold. A relative threshold that allows only a fixed proportion of students to advance (e.g. grading on a curve) would also yield this result in the presence of noise in the entrance exam which generates a nonzero probability of failing the test inversely related to endowment and expenditure.

[^18]:    ${ }^{35}$ Fang et al. (2012) estimate that the policy generated a 0.8 year increase in the average number of years spent in middle school. Though we can replicate their results using their chosen empirical specification, when we apply the treatment years identified in that study to our data using our RD design, we find no significant difference between treated individuals in terms of either years spent in middle school or the likelihood that individuals completed at least a middle school degree.

[^19]:    ${ }^{36}$ Source: China Education Statistical Yearbooks. This $20-50 \%$ estimate masks the fact that few technical high school students take the college entrance exam, while a much larger proportion of regular high school students do so. We are unable to differentiate between those in technical and regular high school in the census data.

[^20]:    ${ }^{37}$ We assume that the extra year of primary school does not induce individuals to remain in the workforce for longer. In Appendix 4 we discuss other costs and our decision not to include them in this calculation.

[^21]:    ${ }^{38}$ 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 1990.
    ${ }^{39}$ This labor share data comes 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.
    ${ }^{40}$ 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.

[^22]:    ${ }^{41}$ Countries in which we observe bunching at credential attainment years had more than twice the per-capita GDP than those without bunching. If productive characteristics for non-agricultural occupations are less observable than those for agricultural work (under the assumption it is more difficult to observe brains than brawn), the greater signaling among richer countries we observe is consistent with a larger contribution of signaling to schooling returns in economies with more developed non-agricultural labor markets.

[^23]:    *Number of prefectures for which we collect gazetteer data
    **Note: There are multiple counties in each prefecture

[^24]:    *Estimates in millions of 2015 US Dollars. See Section VI for details on calculations.

