The Impact of An Un(der)funded Inclusive Education Policy: Evidence from the 2013 China Education Panel Survey

Abstract

Using the 2013 China Education Panel Survey (CEPS), we study the impact of accessing better schools – a 2008 inclusive education policy through which the central government mandated urban public schools to exempt migrant children from tuition and temporary schooling fees. Whereas the non-disclosure rule regarding geographical location of CEPS sampling units precludes the control of locational characteristics, we identify the causal effect of the policy by exploiting constituent elements of CEPS's primary sampling units. Namely, we only use non-migrant rural *hukou* children living in counties in the nationally representative sample as the control group (*the never-takers*), while, in the treatment group, we only include migrant children who are currently living in China's top 120 migrant-receiving counties or city districts, and Shanghai. We also distinguish migrant children who started urban schooling before and after 2008 as separate treatment groups of *always-takers* and *compliers,* respectively. Using the Inverse Probability Weighted Regression Adjustment (IPWRA) approach, we find that the *average treatment effect* of the policy on migrant children is around 0.18 SD, as measured by a standardised cognitive test score – a large effect. We also present complementary evidence that the average treatment effect tends to be larger for municipalities and provincial capitals, consistently with the notion that the (potential) value-added of attending urban schools is higher the larger the initial gap with rural schools.

Keywords: school access reform; migrant children; discrimination, inclusive education.

1. Introduction

Universal access to primary, and more broadly compulsory, education is generally viewed as a suitable strategy to alleviate poverty and reduce inequality in society, as it raises human capital, especially for school-age children of low-income families, and ease the realisation of other positive outcomes associated with education (Carneiro and Heckman, 2003; Cunha and Heckman, 2009) - better access to the labour market and public services in general, such as health; choices affecting family formation and fertility decisions; intergenerational choices about children's education, to name a few.

However, universal access to education requires the removal of several barriers. Among these, school fees are perhaps the most obvious, as poorer households can not always view their children's education as a priority vis-à-vis other pressing survival needs. In addition, as found by the literature on the effects of the World Bank-Unicef School Fee Abolition Initiative of 2005, empirical evidence in favour of no fees is not conclusive, as one might expect. In several African countries the abolition of school fees led to higher school enrolment among children from low-income households (Al-Samarrai and Zaman, 2007; Lukas and Mbiti, 2012; Borkum, 2012; Blimpo et al, 2019), but did not correspondingly raise completion rates (Langsten, 2017; Kan and Klase, 2021). Segregation between socio-economic groups was also unaffected. In countries with higher average income, such as Colombia and Germany, the abolition of school fees resulted in both higher enrolment and completions (Barrera-Osorio et al, 2007; Riphahn, 2012; Hübner, 2012), but heightened segregation, as the children of high income groups have been pulled away from government schools in favour of private, and more elitist, education (Cordini et al, 2019).

Besides price barriers, universal access to education is affected by non-price factors - principally residence requirements. In general, these are set by the school and can be satisfied by simply changing residence to an address within the catchment area. This often involves a cost that may not be afforded by low-income households (Taylor and Gorard, 2001; Lubienski et al, 2013), though, as an extreme, deception is used (Noreisch, 2007). In some cases, however, residence cannot be changed, as can be the case for migrants, and access to education is severely constrained, particularly for the children of the most disadvantaged groups. For example, children arriving in the United States as undocumented migrants can attend local schools but they often do not for fears of being apprehended or expelled, or because they need to find a source of income to survive[1]. The cohort of undocumented school-age migrant children who do not interact with the US school system is large, and adds to issues related to social and economic marginalisation (Hirschman, 2001; Flores, 2010; Diaz-Strong, 2021). In the case of rural-urban migrants' children in China, the inability to shift residence from the town of origin (*hukou*[2]) is institutional, and until recently forced migrant parents to either pay extra fees to urban schools (generally of lesser quality) open to enrol non-resident children, or forego living together with them so they could get schooled in their place of residence under the guardianship of grandparents or other relatives and friends.

We contribute to the literature on the effects of universal access to (compulsory) schooling by analysing a national policy shift in China, which forbid urban schools to charge extra fees and discriminate against migrants' children on the basis of their residence. The shift is significant because it was implemented across the entire nation immediately[3] after its announcement in 2008, and enabled migrant families to redefine and improve their choices of work location, living arrangements, and preferred school for their children. As the *hukou* is the principal tool for population management in China, our analysis contributes new evidence of the effects of its costs and the benefits of its removal. To our knowledge, this paper is the first to analyse the effect of the 2008 policy on migrant children's educational outcomes. In addition, our paper contributes to the wider international literature studying the effects of non-price barriers to universal school access, and provides an empirical benchmark for the potential benefit of attending school for school-age children that, due to institutional settings, miss out on formal education.

[1] In the US as much as 15% of these children do not attend school, opting instead for low-paid jobs and possibly crime (Diaz-Strong, 2021). The phenomenon is significant to the extent that it led to a Supreme Court decision to remove asking for immigration status for cohorts subject to mandatory schooling. Yet, economic pressures induce many of these children and teenager to avoid enrolment and find earnings in whatever forms they can. Research has however shown that the problem of non-enrolment affects way more than unaccompanied minors in the US to extend to several parts of low-socio-economic status migrants (Greenberg et al, 2016): strict eligibility criteria, combined with schools' limited resources and capacity constrains limit the ability of schools to absorb the demand for education from immigrant families in low socio-economic status. Qualitative research suggests affordability is a priority area for policy support and intervention, as is investment in increasing the supply of schools in areas where poorer immigrants live.

[2] The *hukou* status, China's classification system of urban and rural dwellers according to own or ancestral residence as of 1955-1958, is one of China's most distinct policies for managing internal migration (Chan and Wang, 2008; Chan, 2009). It is also an institutional source of inequality (Meng, 2012), as it has been discriminating against rural-urban migrants and their families across various domains: from restricting access to government jobs, public goods and services, to imposing a premium on prices afforded to urban residents. Rural *hukou* migrants are over-represented in "dirty, dangerous, and demeaning" urban jobs and in self-employment (Giulietti, Ning and Zimmermann, 2012), and experience worse health status than their urban *hukou* counterparts (Zhang and Kanbur, 2005). Their children, too, attain less education relative to their urban peers (Li, 2010; Wu, 2011; Liu 2005; Biavaschi, Giulietti and Zimmermann, 2015).

[3] The gap between the announcement (12th August) and the policy commencement dates (1st September) was less than 20 days.

For the empirical analysis we use the China Education Panel Survey (CEPS) of 2013, and apply a novel identification strategy to bypass their undisclosed geographical location by relying on the types of primary sampling units of the survey. This approach enables us to identify the causal effect of the policy on migrant children's education.

We find that the *average treatment effect* (ATE) of accessing urban schools by migrant children, triggered by the 2008 policy reform, is around 0.18 standard deviations of a standardised cognitive test score – a large effect. In particular, the increase in cognitive scores for Grade 9 migrant children is 5 times as high as that for Grade 7 children. The disproportionate increase for post-reform migrants has effectively allowed them to catch up with their pre-reform counterparts. With reference to gender differences, migrant boys appear to benefit more from urban schooling than migrant girls. However, taking advantage of the one-year panel for Grade 7 children, it turns out that the value-added of one year of urban schooling is marginally higher for girls. We also present evidence that the Average Treatment Effect (ATE) tends to be larger for municipalities and provincial capitals, consistently with the notion that the (potential) value-added of attending urban schools is higher the larger the initial gap with rural schools.

The rest of the paper is organised as follows: we present a brief review of the literature in Section 2, while leave to Appendix A the background and some salient aspects of the 2008 policy. In Section 3 we sketch a simple theoretical framework, while in Section 4 we present the CEPS and its features together with summary statistics. Identification strategies and other methodological information are discussed in Section 5. The results are presented in Section 6. Section 7 provides some concluding remarks.

2. Literature review

Although the *hukou* system remains an effective mechanism to control population movements and a persistent source of inequality, the central government has been introducing partial reforms to modernise it in parallel with the China's economic transformation and development. Since the 1990s, a set of social policies opened up urban schools to rural-migrant children so they could acquire education in the city where their parents work rather than be schooled in the rural *hukou* place of residence under the guardianship of grandparents and relatives.

More inclusive education took centre place with the Free Compulsory Education Reform of 2006, a conditional in-kind transfer program aimed at reducing education fees in rural areas

(Xiao et al, 2017; Tang et al, 2020). The reform received a boost in 2008, when the central government mandated urban public schools to exempt migrant children from tuition fees and the so-called "temporary schooling fees". This sharp policy change removed a substantive financial obstacle affecting migrant children's access to schooling and subsequent educational investment decisions in cities throughout China, easing their path towards better inter-generational socio-economic integration with their urban peers (Wang et al, 2020).

The inequality between rural and urban *hukou* holders living in Chinese cities is documented by a large literature spanning economics, sociology, demography, and social sciences to name a few. In general, the main preoccupation of this stream of research is to report the conditions, choices, and labour market outcomes of rural-urban migrants, often in the context of their inability to receive local welfare support relative to equivalent urban residents. Rural-urban migrants live in a city only *de facto* but not *de jure*. They have limited access to public services and social benefits, including unemployment support, health insurance, pensions, and subsidies for housing and education (Chan, 2008; Cai, Park and Zhao, 2011; Frijters, Lee and Meng, 2010). It is therefore unsurprising that this literature consistently finds inferior outcomes for the migrant group when compared with urban residents in both China and internationally (OECD, 2011), though migrants generally fare better than rural residents. Only in the past decade, thirty years from the economic reforms started in 1978, has the rural *hukou* of migrants been positively sought to alleviate domestic labour shortages (Zhang et al, 2011), and its possible signal for hard-working inclinations (Kuhn and Shen, 2014).

Comparatively less research investigates the inter-generational effects of a rural *hukou* on the educational attainment of migrants' children. In general, research on this topic supports the hypothesis that differences in educational attainment in China predominantly reflect rural-urban differences rather than geographic location or the level of economic development (Qian and Smyth, 2008), and that the rural-urban gap in children education is a bottleneck in the accumulation of human capital, and an area where government intervention is called for (Liu, 2005; Zhang, Li and Xue, 2015).

Several analyses provide valuable insights, especially with reference to documenting the extent of disadvantage faced by migrants' children due to limited school resources (Hu, 2012; Ziao and Liu, 2014) or the relative poverty and limited social network of their family, which affects the children's mental and overall wellbeing (Yiu and Yun, 2017). Family conditions and relative unawareness about free compulsory education, and outright discrimination from schools themselves, also contribute to hinder migrants' children from being enrolled in urban schools (Li, 2020).

A line of research within this literature investigates how migrants' children affect the performance of their urban peers when attending urban schools. Evidence is mixed (Chen and Feng, 2019; Liang et al, 2019). Negative peer effects are found in rural schools (Liang et al, 2019), though varying the mix of students endowed with different abilities may reverse this effect (Min et al, 2019). In contrast, in urban settings, migrants' children appear to sort according to school quality (Chen and Feng, 2019), and have positive spillovers on the rest of the class (Wang et al, 2018). Research in this literature however puts a limited emphasis on causal relation: the empirical analyses do not exploit random assignments features, even when using CEPS data, or tend to be based on samples surveyed in one or a handful of cities, or on qualitative interviews. This raises the possibility that the results are influenced by omitted variables, constraining the scope to generalise the findings to design clear recommendations for education policy.

Only in recent times has wider and more structured data been made available, principally through the China Education Panel Survey (CEPS). This has enabled researchers to gain a better understanding of what underpins the observed educational outcomes and to evaluate the effect of recent educational reforms. Some of the research based on CEPS confirms the findings of the earlier literature, especially with reference to the negative externalities for children left behind (Wang and Zhu, 2021), and how gender differences (Luo et al, 2021) and parenting style (Zhang et al, 2020) affect the academic performance of migrants' children. This body of research typically focuses on cognitive outcomes, as they are regarded as one of the most essential predictors of children's development,[4] estimating the effect of holding a rural *hukou* rather than relaxing some of its educational consequences with the recent policy reforms.

The effects of China's recent educational policies in favour of better access to schools remain under-researched, and predominantly concerned with the 2006 reform, which catered for students living in rural areas but not migrants' children attending schools in urban areas.

[4] In the last several decades, studies have examined the possible impacts of parental characteristics, such as family income and parental education, on children's cognitive outcomes (Duncan, Yeung, Brooks, & Smith, 1998). Families and schools are both primary sites that impact children's cognitive outcomes, especially academic performance. It was found that family capital and school capital both have great impacts on academic performance as both of them can help improve academic performance, but school capital has a weaker impact than family capital, and the impact of the structure of social strata on academic performance changes due to the mediating role of family capital (Parcel & Dufur, 2001).

In particular, it is found that the 2006 reform increases the quantity and quality of the human capital of the children who were affected by it (Xiao et al, 2017). It also reduced considerably child labour among boys, but not girls, especially among families with low socio-economic status (Tang et al, 2020).

To our knowledge, however, there is no study of the effects of the subsequent 2008 reform, which extends the free compulsory schooling to migrants' children enrolled in urban schools – i.e. the main contribution of this paper. The closest study to ours is Guo and Zhao (2019) who use the 2013 CEPS to analyse the effect of *hukou* status and perceived fair access to local senior high schools on migrant students' achievement in Chinese, math and English. Their analysis however is not carried out with the aim of finding the casual effects of the inclusive education policy introduced in 2008: in fact Guo and Zhao (2019) only relate the findings to a general proposal by the central government in 2012 to unify the *hukou* system to ensure rural and urban residents[5]. In contrast, we are able to trace the effects of the specific policy reform of 2008, and undertake a rigorous policy evaluation distinguishing between compliers, always takers and never takers using the Inverse Probability Weighted Regression Adjustment approach.

Our contribution in this context is hence two-fold. First, we study the direct policy impact on the most relevant outcomes for the large population of migrant children in China, adding new evidence to the literature that studies well-defined social policy reforms addressing inclusive education. The existing literature, including that from China, is mostly concerned about peer effects and externalities caused by the presence of migrant children.

Second, in an attempt to uncover policy-relevant causal effects, we propose a novel way to exploit the quasi-experimental design of the CEPS survey. In addition, we show that our results are robust to different ways of allowing for selection on unobservables (e.g. different exclusion restrictions and panel approach focusing on the "value-added"), notwithstanding the clear heterogeneity across city tiers, school grades and student genders.

3. A simple theoretical framework

One of the principal objectives of schooling is to enhance children's cognitive skills, as these are important determinants of several outcomes, which span education, health, wellbeing, and

[5] As of 2021, this policy goal is still far from being achieved, even though limited progress has been made, especially in lower tier cities and less developed regions.

labour market. Typically, cognitive skills are positively associated with such outcomes, so that given:

$$Y = f(X)$$

where Y = outcome of interest and X = characteristics, including x cognitive skills, the first derivative of Y with respect to x is positive, i.e.: $f'(x) > 0$

In turn, the vector of cognitive skills x reflect other inputs that relate to individual characteristics as well as other influences z that favour the development of such skills, like schooling and parental support, as in:

$$x = g(Z) = g(z_1, z_2, z_3 \ldots z_n)$$

The 2008 policy change directly affected one such z: the ability of rural children to attend an urban school in the same location where their migrant parents work, which is typically better resourced than the designated rural school of *hukou* origin[6].

We model the policy parameter Z to capture the cost of being "distant from parents" (proxying also for barriers to access better schools in urban areas). $S(Z)$ represents the quality of attended schools affected by the policy parameter. $\frac{\partial S(Z)}{\partial Z} < 0$ suggests that lower administrative costs result in more migrant children enrolling in urban schools (better schools), which we interact with individual characteristics of the students to yield the specification:

$$y_i = X_0'\check{\gamma} + X_1'S(Z)\widetilde{\mu_1} + X_2'S(Z)\widetilde{\mu_2} + S(Z)\check{\delta} + \eta Z + \theta_i \tag{1}$$

where X_0 = characteristics of students in rural area; X_1 = characteristics of students migrating before the reform. X_2 = characteristics of students migrating after the reform. Migrant students may benefit from better educational resources in urban school directly, such as better teachers and equipment, and it is likely that better educational environment may also affect academic achievements by affecting migrant students' inputs, such as inputs and peer effect.

4. Data and sample

CEPS is a large-scale, nationally representative longitudinal survey by Renmin University of China. The 2013 baseline survey of Grade 7 and Grade 9 students include 5 different questionnaires for students, parents, core subject teachers, head teachers, and school principals. The survey follows a stratified, multistage sampling design with probability

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6 Luo et al. (2020) show that per-pupil spending in urban schools is 25% higher than in rural schools in the CEPS baseline.

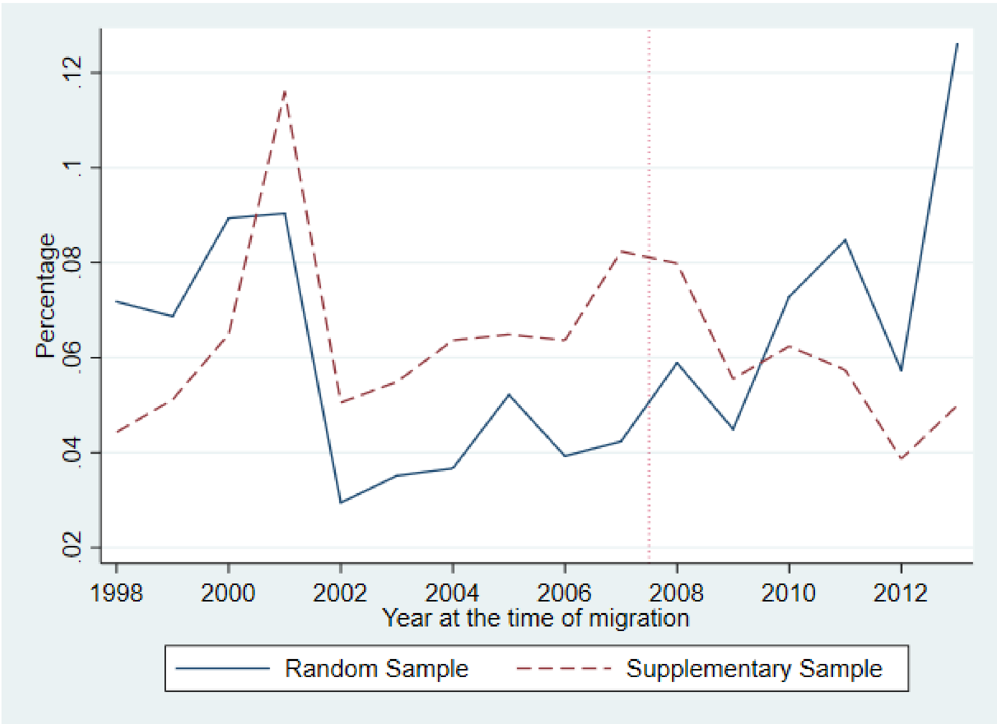
proportional to size (PPS), including approximately 20,000 students in 438 classrooms of 112 schools across 28 county-level units in mainland China. All Grade 7 students in the baseline survey were interviewed again when they were in Grade 8. Taking advantage of the random class assignment design in the survey, a rapidly growing literature has examined a range of topics including peer effect and teacher-gender effect, and transmission of beliefs (Gong et al, 2018; Eble and Hu, 2019; Huang and Zhu, 2020). However, using the random design limits the potential of the data as it restricts the sample to a quarter of the full sample (Yu, 2020).

Instead of using the random design in the survey, we make use of the inherent sampling design to examine the effect of the inclusive education policy on migrant children. In particular, to evaluate the effect of the 2008 urban school access policy, the population of interest is restricted to Grade 7/9 children with a rural *hukou*, who are currently studying either in their original place of *hukou* registration (the *rural natives*) or as migrant children in urban schools. We exclude urban-to-urban migrant students, local urban students studying in a rural school or local rural students studying in an urban school, as their motivation could be rather different from the rural-to-urban migrants for reasons that are unobservable to us.

The working sample is thus constructed exploiting the structure of CEPS. There are 3 types of Primary Sampling Units (PSUs) in the CEPS: Type 1 sample includes 15 counties and city-districts randomly drawn from 2,870 counties or city-districts across the country. Type 2 sample includes 3 counties/disctricts randomly drawn from the 18 counties/districts in Shanghai. Type 3 sample includes 12 counties/districts randomly drawn from the top 120 migrant-receiving counties/districts. We group Types 2 and 3 as migrant-destination counties, which we term the *supplementary sample* while calling Type 1 as the *random sample* or *nationally representative sample*.

Figure 1 shows the distribution of migrant children by year of migration by CEPS subsamples. While the random sample suggests a general upward trend from 2002 onwards, the pattern for migrant children in the supplementary sample, which forms the treatment group in the main analysis, indicates a clear peak around 2008. We interpret this as *prima facie* evidence that migrant families to the top migrant-receiving destinations responded positively to the new policy initative around 2008.

Figure 1: Distribution of migrant children by year of migration and subsamples



Note: Horizonal axis shows the calendar year at the time of migration. Vertical axis shows the density of distribution.

Educational outcomes, by Hukou and sample

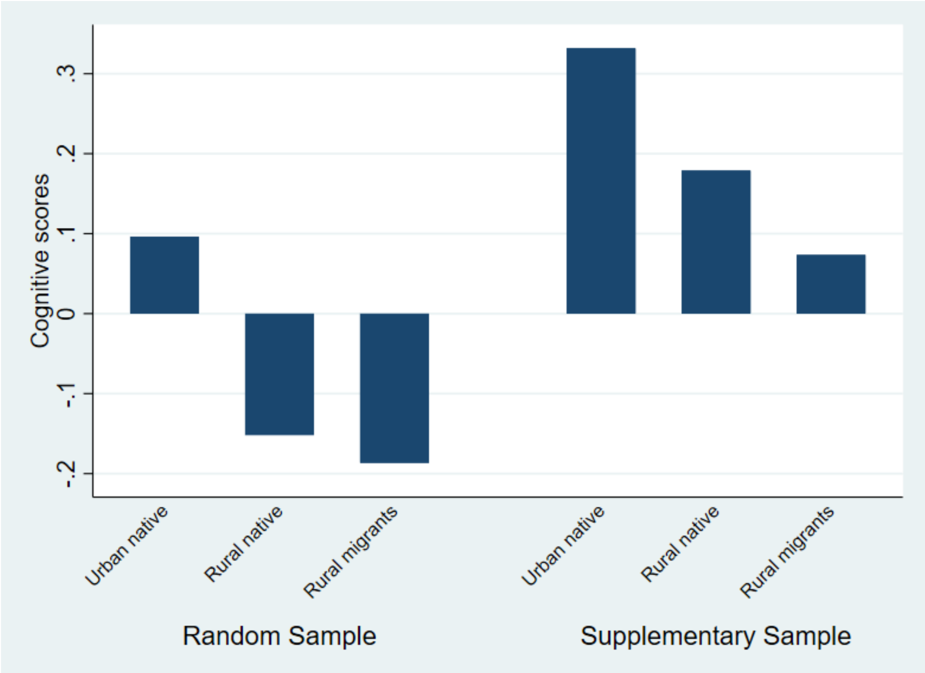
Figure 2 shows the mean standardised cognitive test scores by student type and subsamples. Student type is defined according to the *hukou* and migrant status, while subsamples refer to either the nationally representative random sample or the supplementary sample consisting of the top migration-receiving regions respectively. We exclude urban-to-urban migrants as this group is not particularly interesting and the sample size is relatively small.[7]

For the nationally representative subsample in the left panel, the mean scores are 0.1 and -0.15 for urban and rural native students respectively, implying a staggering urban-rural gap in the order of 0.25 SD. Rural-to-urban migrant children on average performed slightly worse than their rural counterparts who stayed behind in rural schools, by around 0.03 SD. In contrast, both urban and rural native students in the supplementary sample in the right panel have much higher cognitive scores. Indeed, even the rural natives in the supplementary sample significantly outperform the urban natives in the random sample by around 0.08 SD. Rural migrants in the supplementary sample have a mean score of 0.07, a substantial

[7] The urban-to-urban and urban-to-rural migrants account for around 5% and 1% of the full sample, respectively.

improvement compared to the mean of -0.15 for the rural natives in the nationally representative subsample in the left panel. The sharp contrast across the two subsamples reflects the fact that the top migration destinations in the supplementary sample are predominantly the most economically developed regions in China attracting the bulk of rural migrants. It also highlights the risk of deriving spurious patterns if the variation in sample origins is not properly accounted for.

Figure 2: Standardised cognitive scores by student type and subsamples



Sample

Due to concerns for disclosure, the geographical information of CEPS primary sampling units (PSMs) and schools has been kept anonymous.[8] This rule precludes the control of locational characteristics using aggregate statistics matched at the county or city-district level. In order to address potential migration sorting, which will bias the results, we adopt a novel identification strategy. This relies on using the types of primary sampling units to identify the causal effect of the policy on the schooling choice of migrant children. Specifically, we only use non-migrant rural *hukou* children living in counties in the nationally representative sample as the control group (*the never-takers*), while, as treatment group, we only include migrant children who are currently living in China's top 120 migrant-receiving counties or city districts, as well as Shanghai in the supplementary sample. As a result, we focus on the

[8] See http://ceps.ruc.edu.cn/info/1029/1044.htm. This is deemed necessary, given that there are only 28 primary sampling units (counties or city districts) in the CEPS, each of which containing 4 randomly selected schools.

identification of the causal effect of the inclusive schooling policy for randomly selected migrants from counties to the top migrant-receiving regions: this would capture the bulk of rural-to-urban migration in China. Note that intra-county migration, where commuting might be feasible and the child might not be treated as a migrant from the educational authority's perspective, has been ruled out by sample construction. Moreover, migrant children who started urban schooling before and after 2008 are regarded as separate treatment groups, as *always-takers* and *compliers* respectively (Angrist and Imbens 1995; Rubin 2005).

While our sample selection strategy implies that we lose migrants in the nationally representative sample, which account for just over half of all counties and districts (15 out 28 PSMs), the fact that all migrants in the treatment group are living in China's top migration-receiving regions ensure much greater homogeneity in migration motivation, family and social networks among others, which are unobservable but likely affect the migration decision.[9] On the other hand, all rural native children in the control group come from the nationally representative sample. We argue that this strategy greatly facilitates causal inference in the following empirical analysis, by decoupling the treatment and the control group using two mutually exclusive subsamples, and thus mitigating concerns for selection bias and measurement error. This strategy also allows us to focus on the schooling decision of the children concerned conditional on their parents' migration decisions.

In terms of the Neyman-Rubin Potential Outcomes Framework (for a description see Rubin, 1974 and 2005), the control group comprise all native rural *hukou* students studying in rural schools in counties (i.e. excluding rural residents in the more developed municipalities, provincial capitals or prefecture cities) of *hukou* registration in the nationally representative sample. These are the *never-takers*.

The treatment group comprises migrant children studying in the Top 120 migrant-receiving counties/districts in the country plus Shanghai (i.e. drawn from the *supplementary sample*). Note that we have excluded from the supplementary sample local students who have a rural *hukou* but may enrol in urban or rural schools, as their selection mechanisms and policy applicability might be very different from the normal migrant students. According to whether they migrated before or after the implementation of the 2008 policy, we can further

[9] It is conceivable that motivation for migration differ substantially between major migration-receiving regions and other regions. The former is likely to be driven by strong economic factors such as higher wages and better career opportunities while the latter (typically involving shorter migration distances and more circular/seasonal migration) could be strongly affected by family circumstances and social networks etc. Using the migration-receiving regions only could also control for the opportunities to accessing schools as a result of strong heterogeneity of policy enforcement across regions.

classify the migrant children as *always-takers* and *compliers*. While the former might still be partially affected by the policy, e.g. by no longer having to pay fees or the possibility of switching schools, only the latter group is directly affected by the policy. Note that any "treatment effect" identified from the pre-2008 group could be confounded by a selection effect and an effect of longer exposure to urban schooling relative to the latter treatment group.

Table 1 shows the breakdown of analytical sample by control and treatment status and city-tier. With the treatment group, we also distinguish between migrant students who enrolled in urban schools pre- or post-reform, respectively. In total, we have 2,265 rural native students in the control group and 634 migrant students in the treatment group, of which 229 migrated post-reform. Given that these children are in Grade 7 at least in 2013, they would have been in Grade 2 or above by the time migration occurred, and hence expected to attend urban schools under the compulsory education law.

Table 1: Composition of the Analytical Sample

Panel A, sample decomposition

	Treatment Group (supplementary sample)					Control Group (random sample)
	Larger Cities		Smaller Cities			
City type	**Municipal**	**Provincial**	**Prefecture**	**County**	**Total**	**County**
Control Group						2,229
Treatment type:						
Pre-reform Migrants	149	139	90	27	405	
Post-reform Migrants	65	67	66	31	229	
Total	214	206	156	58	634	2,229

Notes: The sample only includes individuals with rural *hukou*. The control group only includes rural *hukou* students living in counties of *hukou* registration in the nationally representative sample.

Panel B, average cognitive scores

	Treatment Group (supplementary sample)				Control Group (random sample)
City type	**Municipal**	**Provincial**	**Prefecture**	**County**	**County**
Control Group					-.247
Treatment type:					
Pre-reform Migrants	.099	.081	.126	-.345	
Post-reform Migrants	.002	.045	-.161	-.153	

Notes: The sample only includes individuals with rural *hukou*. The control group only includes rural *hukou* students living in counties of *hukou* registration in the nationally representative sample.

The relatively small cell sizes for the treatment group, especially for migrants to counties, in Panel A, might result in estimates being over-sensitive to alternative specifications. Therefore, we group municipal and provincial cities together as *larger cities* on the one hand, and combining prefectural and county-level cities as *smaller cities* on the other hand. This ensures that even the smallest cell, i.e. post-2008 migrants to smaller cities, has almost 100 observations.[10]

5. Empirical strategy: IPWRA

When Ordinary Least Squares (OLS) is applied to equation (1), it uses a detailed set of controls to address the methodological challenge of rural *hukou* students selecting into urban schooling on observable characteristics. In our setting, the variable of interest is the school choice of rural-origin children, in the context of the 2008 inclusive education policy, which mandated the exemption of tuition fees of migrant children with no local *hukou* in urban schools. The reference group is children who stayed in the rural origin (*the control group*). One complication is that we have two treatment groups, one who migrated after the policy change in 2008 (*the compliers*), the other who migrated before 2008 (*the always-takers*). This means that the empirical model is specified as (for simplicity we present the cross-sectional case as an example):

$$y_i = \alpha + \beta_1 M_1 + \beta_2 M_2 + X'_i \gamma + T_j + \epsilon_i \tag{2}$$

where M_1 and M_2 denote the pre- and post-2008 migrant children respectively, with rural students schooled at the place of *hukou* registration as the reference category, X represent a vector of control variables. The dependent variables are standardized cognitive scores or other outcomes. ϵ_i is the error term. Subscript i denotes individuals. T_j denotes the teachers' characteristics.

However, OLS does not attempt to allow endogenous treatment and relies on (typically) linear functional form assumptions. We therefore use the inverse probability weighted regression adjustment (IPWRA) – see Black and Smith (2004) and Walker and Zhu (2018). The method of Inverse Probability Weighted Regression Adjustment is effectively an extension of the more conventional Propensity Score Matching (PSM) to estimate unbiased treatment effects when we have confounders, but it allows for multiple treatments. Another

[10] Appendix Figure A1 shows the mean standardised cognitive scores by city tiers for urban natives in the supplementary sample. The lack of a monotonic pattern might reflect the relatively small number (13 in total) of districts/counties we have.

advantage of IPWRA is the property of *double robustness*, which ensures consistency of the estimator even if either the treatment model or the outcome model (but not both), is mis-specified (see Wooldridge 2007; Imbens and Wooldridge 2009).

IPWRA proceeds in two stages. In the first stage, a selection model is estimated using multinomial logit. The inverse of the predicted probability of individual-specific choice of each possible treatment and control state is then used as weights in the second-stage outcome equation regression estimation. The idea is to create counterfactuals by attaching greater weights to treated individuals with a low propensity to be in the treatment group and untreated individuals with a low propensity to be in the control group.

Although it is possible to estimate IPWRA without exclusion restrictions, i.e. variables affecting urban schooling decisions but not cognitive scores directly, identification based on functional forms alone are not robust. In practice, we use three alternative exclusion restrictions, namely the migrant child attended nurseries from age 3, any parent smokes, and any parent has a drinking problem, to ensure our findings are robust. The intuition here is these variables reflect parental preferences regarding children's education or parents' own time discounting which might affect the urban schooling decision, but not children's cognitive scores conditional on urban schooling choice and other controls. For instance, a systematic review and network analysis concludes that there is moderate but consistent evidence that high time-discounting is a risk factor for smoking and unsuccessful cessation, even in longitudinal studies (Barlow et al, 2017). By comparing the IPWRA treatment effect estimates using three alternative exclusion restrictions it is possible to get a good sense of the sensitivity of the empirical findings.

The 2008 inclusive education policy is best viewed as a policy change that widened the choice set of rural origin children, by reducing the cost of urban schooling. Given the data limitations and that our interest is the impact of the 2008 policy on the schooling choice for all rural original Grade 7 children in CEPS, we do not attempt to disentangle the schooling choice for the children and the migration decisions of their parents in the empirical analysis. What we estimate is an intention-to-treat effect of the policy change on the schooling choice of rural *hukou* children, conditional on their parents' decision to migrate to the top migration receiving areas including Shanghai.

Following the best practice, we use 3 alternative exclusion restrictions (attending nurseries by age 3, parental smoking and parental drinking problems) in the mlogit estimation in step 1 for identification and get very similar results rather than simply relying on functional forms.

This should go a long way in mitigating concerns for estimation biases arising from selection into treatment. A comparison between the post and pre-2008 migrant children, and between the Average Treatment Effect (ATE) and the Average Treatment Effect on the Treated (ATT) which differ in the relative weights assigned to the treatment vs the control group, should also provide useful insight on the likely direction and magnitude of potential selectivity biases. [11]

6. Empirical Results

We first present IPWRA estimates of the ATE using the full analytical sample which include both Grade 7 and Grade 9 children, and only distinguish between migrant children who started urban schooling before and after the 2008, who correspond to the *always takers* and *compliers*, respectively. This is then supplemented by the estimation of both ATE and the Average Treatment Effect on the Treated (ATT), in specifications allowing differential treatment effects between larger and smaller cities. This is then followed by an estimation of the panel version of the treatment effect of the value-added of one year of urban schooling using the one-year panel for Grade 7 children in the original 2013 survey only. We then present evidence on heterogeneous treatment effects by the gender of the migrant child, the school grade, or the policy regarding access to local senior high schools. Finally, we explore possible channels through which the 2008 reform affect cognitive scores, such as aspirations, study time and educational expenses.

Table 2 presents the headline IPWRA results. The treatment effect is only allowed to differ according to whether the onset of urban schooling postdates the 2008 urban school access reform. The conditional means of standardized cognitive scores for the control group of rural native students attending rural schools are about -0.24, regardless of the exclusion restriction used. Similarly, there appears to be little variation in the estimates ATEs across columns, suggesting the results are highly robust with respect to the exclusion restriction used in the selection equation. While part of the ATE for the pre-reform migrant students might be attributed to the 2008 policy change, it is impossible to disentangle this from selection on unobservable factors that may or may not be related to the policy change. The ATE for the post-reform migrant children identifies the average causal effect of the policy change on a randomly selected rural *hukou* child living in rural counties. The 0.18 SD increase, which is in line with existing research on the effects of reduced costs of schooling (Murnane and

[11] Appendix B provides more technical details of the IPWRA methodology, including the differences between ATE and ATT.

Ganimian, 2014), is not only statistically significant at 5%, but also significant in magnitude and accounts for almost three-quarters of the 0.25 SD urban-rural gap indicated in Figure 2. Note that, by definition, the post-reform migrant students were unable to have received a complete urban schooling (maximum 5 years between 2008 and 2013 when the survey was conducted). To the extent that the beneficial effect of urban schooling is cumulative, the treatment effect would have been even larger if the migrant were able to enrol in urban schools from Grade 1.

Table 2: IPWRA ATE

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
ATE			
Pre-reform Migrants	0.276***	0.275***	0.268***
	(0.08)	(0.08)	(0.08)
Post-reform Migrants	0.182**	0.184**	0.177**
	(0.07)	(0.07)	(0.07)
PO mean			
Rural Natives	-0.240***	-0.241***	-0.240***
	(0.03)	(0.03)	(0.03)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: N = 2,863. Standard errors clustered at the school level in parentheses. ***,** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 3 allows the treatment effect to vary by city sizes, in order to capture any heterogeneous effect across city-tiers. This would be useful if there is systematic variation by city population size or the administrative hierarchies in demand for urban school places by migrant children or in the implementation of the policy.[12] For pre-reform migrant students, the ATEs are 0.38 and 0.30 SDs for migrants to larger and smaller cities, respectively. These estimates are statistically significant at the 5% level, at least. For the group of compliers who migrated post-reform, the ATEs are significant at 1% for migrants to larger and smaller cities alike, with a 10-percentage point difference in favour of the former. Conditional on city tier, the ATEs for the post-reform children are smaller than those for their earlier arrival counterparts, possibility due to both a positive sorting and longer exposure to urban schooling for the pre-reform migrant children. Moreover, the estimates are remarkably robust to the alternative exclusion restriction used in the selection model.

[12] Figure 1A in Guo and Zhao (2019) shows that the share of student without local *hukou* is monotonically decreasing from municipalities, through provincial capitals and prefectural cities and reaches the minimum in county-level cities, possibly driven by demand patterns.

Table 3: IPWRA ATE, City-tier Specific Treatment Effects

Dep var: Cognitive scores	(1)	(2)	(3)
ATE			
Pre-2008 Larger Cities Migrants	0.379***	0.364***	0.364***
	(0.09)	(0.09)	(0.09)
Pre-reform Smaller Cities Migrants	0.299**	0.308**	0.287**
	(0.12)	(0.12)	(0.12)
Post-reform Larger Cities Migrants	0.337***	0.346***	0.338***
	(0.12)	(0.12)	(0.12)
Post-reform Smaller Cities Migrants	0.239***	0.232***	0.233***
	(0.07)	(0.07)	(0.07)
PO mean			
Rural Natives	-0.240***	-0.241***	-0.240***
	(0.03)	(0.03)	(0.03)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: N = 2,863. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

To the extent that there is remaining selection on unobservables into the treatment group, e.g. rural students with larger potential gains from urban education are more likely to migrate as a result of the policy, the estimation of the Average Treatment Effects for the Treated (ATT) could shed light on the robustness of the main results of ATEs in Table 3 and indicate the direction of potential biases. Therefore, we present in Table 4 the corresponding ATT estimates using the same specification as in Table 3. By and large, ATEs and ATEs are of the same order of magnitude. However, ATEs exceed ATTs for larger cities while the converse is true for smaller cities. Conditional on the timing of migration, while the ATEs in Table 3 suggest that the treatment effect for a randomly selected rural *hukou* child would be larger if she migrated to larger cities, the ATTs in Table 4 suggest that the treatment effects are larger for smaller city migrants among rural children who actually migrated. Note that the ATTs are very all precisely determined, with statical significance at the 1% level.

Table 4: IPWRA ATT, City-tier Specific Treatment Effects

Dep var: Cognitive scores	(1)	(2)	(3)
ATT			
Pre-reform Larger Cities migrants	0.343***	0.347***	0.348***
	(0.06)	(0.06)	(0.06)
Pre-reform Smaller Cities migrants	0.416***	0.421***	0.416***
	(0.10)	(0.10)	(0.10)
Post-reform Larger Cities migrants	0.259***	0.255***	0.257***
	(0.07)	(0.07)	(0.07)
Post-reform Smaller Cities migrants	0.347***	0.354***	0.352***
	(0.08)	(0.08)	(0.08)
PO mean			
Rural Natives	-0.251***	-0.255***	-0.256***
	(0.05)	(0.05)	(0.05)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: N = 2,863. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

To address further concerns for unobserved individual heterogeneity, we take advantage of the one-year panel for Grade 7 children in the pooled sample in the 2013 CEPS. These children were interviewed again one year later when they were in Grade 8. While we lose over half of the sample size compared to the pooled sample, we are now able to control for the lagged cognitive score which serves as a proxy for unobserved individual heterogeneity. Effectively, our model using the revised sample and model specification now estimates the "value-added" in terms of the gain in cognitive score as a result of receiving different treatments for one year, as compared to receiving one-year of rural schooling for the control group.

Table 5 suggests that for the post-reform migrants, one (extra) year of urban schooling increases cognitive scores by 0.25 SD, a very large average treatment effect which is also statistically significant at the 1% level. However, for pre-reform migrant children, one (extra) year of urban schooling only increases cognitive scores by about half as much as their post-reform counterparts, at 0.13 SD. The estimate is only statistically significant at the 10% level, and is indeed slightly lower than the value-added for rural schooling in the control group.

Table 6 is similar to Table 5, but allows the treatment effect to differ by city-tier. For larger and smaller city migrants, the ATEs are about 0.31 and 0.27 SDs respectively, both significantly higher than the 0.16 SD benchmark for the control group. It is interesting to note that the ATE is insignificantly different from zero for pre-reform migrants to larger cities, but highly significant and of similar magnitude for pre-reform smaller city migrants.

Table 5: IPWRA ATE PANEL

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
ATE			
Pre-reform migrants	0.131*	0.131*	0.131*
	(0.08)	(0.08)	(0.08)
Post-reform migrants	0.248***	0.238***	0.235***
	(0.08)	(0.09)	(0.09)
PO mean			
Rural native	0.159***	0.157***	0.159***
	(0.05)	(0.05)	(0.05)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: N = 1,266. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 6: IPWRA ATE PANEL, City-tier Specific Treatment Effects

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
ATE			
Pre-reform Larger Cities migrants	0.071	0.090	0.087
	(0.11)	(0.11)	(0.11)
Pre-reform Smaller Cities migrants	0.311***	0.312**	0.318***
	(0.11)	(0.12)	(0.12)
Post-reform Larger Cities migrants	0.313***	0.296***	0.312***
	(0.10)	(0.10)	(0.10)
Post-reform Smaller Cities migrants	0.266**	0.274**	0.248**
	(0.13)	(0.12)	(0.12)
PO mean			
Rural native	0.159***	0.157***	0.159***
	(0.05)	(0.05)	(0.05)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: N = 1,266. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

In Table 7 we present evidence on heterogeneous treatment effects by gender. While the cross-sectional data suggests migrant boys have higher ATEs than migrant girls conditional on migration timing, this pattern is reversed in the panel estimation.

Table 7: IPWRA, ATE by Gender

Panel A, Cross-section Data

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Male (N=1,379)			
Pre-reform Migrants	0.358***	0.345***	0.343***
	(0.10)	(0.09)	(0.09)
Post-reform Migrants	0.233**	0.224**	0.240**
	(0.10)	(0.10)	(0.10)
Female (N=1,484)			
Pre-reform Migrants	0.258***	0.272***	0.260***
	(0.09)	(0.09)	(0.09)
Post-reform Migrants	0.146	0.150	0.132
	(0.10)	(0.10)	(0.10)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Panel B, Panel Data

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Male (N=605)			
Pre-reform Migrants	0.090	0.066	0.055
	(0.10)	(0.11)	(0.10)
Post-reform Migrants	0.225**	0.232**	0.229**
	(0.10)	(0.10)	(0.10)
Female (N=661)			
Pre-reform Migrants	0.168**	0.176**	0.176**
	(0.08)	(0.09)	(0.09)
Post-reform Migrants	0.280***	0.233**	0.274***
	(0.09)	(0.10)	(0.09)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 8 presents evidence on heterogeneous treatment effects by grade. While the ATE for the pre-reform migrants in Grade 7 is up to twice as large as for the post-reform migrants, this gap between the two treatment groups almost vanishes in Grade 9. On the other hand, the ATEs for migrant students in Grade 9 are much higher than those for Grade 7. This is mostly likely driven by the longer exposure to urban schooling for Grade 9 students. Moreover, the disproportionate increase for post-reform migrants has effectively allowed them to catch up with their pre-reform counterparts. This suggests that the children who migrated as a result of the free compulsory education policy will catch up with their more advantaged pre-reform arrival peers, despite a significant gap when they first started urban schooling.

Table 8: IPWRA, ATE by Grade

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Grade7 (N=1,445)			
Pre-reform Migrants	0.184**	0.195**	0.185**
	(0.08)	(0.08)	(0.08)
Post-reform Migrants	0.102	0.111	0.093
	(0.08)	(0.08)	(0.08)
Grade 9 (N=1,418)			
Pre-reform Migrants	0.516***	0.508***	0.511***
	(0.14)	(0.13)	(0.13)
Post-reform Migrants	0.507***	0.537***	0.539***
	(0.15)	(0.15)	(0.15)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 9 explores heterogeneous treatment effects by local high school access policy. The own and class-level standardised cognitive scores in Table A1 in the Appendix suggest that schools that allow migrant students access to local high schools tended to be lower in quality, than schools not allowing access. On the other hand, migrants attending schools allowing access to local high schools also get lower ATEs than their counterparts who are enrolled in schools that do not allow access. This gap is about 0.35 SD for the pre-reform migrants and 0.10 for the post-reform migrants.

Table 9: IPWRA ATE, by local high school access policy

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Pre-reform Migrants Allowed to Enrol	0.107	0.095	0.107
in Local High-school	(0.09)	(0.10)	(0.09)
Post-reform Migrants Allowed to Enrol	0.247***	0.249***	0.249***
in Local High-school	(0.08)	(0.08)	(0.08)
Pre-reform Migrants not Allowed to	0.457***	0.445***	0.447***
Enrol in Local High-school	(0.10)	(0.09)	(0.09)
Post-reform Migrants not Allowed to	0.349**	0.361**	0.343**
Enroll in Local High-school	(0.15)	(0.15)	(0.15)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Notes: Migrant students are classified by whether they are allowed to enrol in local high schools (without special conditions) according to the principal's response, and whether they migrated after the reform. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

In Table 10 we explore possible channels through which the urban school access reform improves the education of migrant children. For post-reform migrant children (the *compliers*), there is suggestive evidence that the policy has had significant positive effect on school attendance, study time on homework by parents, time on private tuition classes and expenditure on private tuition, all of which likely to improve their educational attainment. On the other hand, there are also hints of negative effects of urban schooling on migrant children's self-confidence or gloominess, presumably reflecting the increased pressure.

Table 10: IPWRA ATE of 2008-reform on Attitudes and Study Time

Cognitive scores	(1)	(2)	(3)	(4)
ATE	Go to school although feeling unwell	Work on study although don't like	Work on study although takes more time	Clearly express opinions
Pre-reform migrants	0.097*	-0.077	-0.021	-0.001
	(0.06)	(0.05)	(0.05)	(0.05)
Post-reform migrants	0.170***	-0.114	-0.058	-0.105
	(0.06)	(0.07)	(0.06)	(0.06)
Continued:				
ATE	Rapid responsiveness	Rapid in learning new knowledge	Curiosity	Confidence
Pre-reform migrants	0.065	0.060	0.014	0.004
	(0.05)	(0.06)	(0.05)	(0.05)
Post-reform migrants	-0.012	-0.067	0.044	-0.123**
	(0.06)	(0.06)	(0.06)	(0.06)
Continued:				
ATE	Expectation of having a degree	Depression	Gloomy	Unhappy
Pre-reform migrants	0.014	0.059	0.139*	0.057
	(0.04)	(0.09)	(0.08)	(0.10)
Post-reform migrants	0.041	0.069	0.164*	-0.039
	(0.05)	(0.09)	(0.08)	(0.08)
Continued:				
ATE	Unmeaningful	Sadness		
Pre-reform migrants	0.228***	0.078		
	(0.09)	(0.09)		
Post-reform migrants	0.133	-0.022		
	(0.10)	(0.10)		
Continued:				
ATE	Time on homework by teacher	Time on homework by parents	Time on private tuition	Expenditure on private tuition
Pre-reform migrants	0.319	0.321	0.825***	1064.250***
	(0.29)	(0.21)	(0.17)	(205.10)
Post-reform migrants	0.292	0.413**	0.483**	544.553***
	(0.25)	(0.20)	(0.19)	(187.57)

Notes: N = 2,863. The behaviours are described using single variable rather than the constructed variable. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

IPWRA is essentially a generalisation of the more conventional Propensity Score Matching (PSM) method. As a robustness check, Table 11 presents the PSM estimates for each of the two treatments separately, using the same control group as in the main analysis. The results show that the PSM estimates are broadly comparable with the IPWRA estimates in Table 2, although the ATEs are now larger for post-2008 migrant children.

Finally, Table 12 repeats Table 8, but for boys and girls separately. The results are remarkably similar, suggesting no significant gender patterns.

Table 11: Propensity score matching, ATE

	(1)	(2)	(3)	(4)
ATE				
Pre-reform Migrants	0.210^{***}	0.158^{**}		
	(0.06)	(0.07)		
Post-reform Migrants			0.298^{***}	0.271^{***}
			(0.05)	(0.09)
Instruments	Yes	Yes	Yes	Yes
Control	No	Yes	No	Yes
N	2458	2458	2634	2634

Note: The instruments are pre-determined variables, including whether attending nursery school, whether parents drinking and(or) smoking. The control variables are consistent with the results above. Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 12: IPWRA, ATE by Grade and gender

Panel A, male

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Grade7 (N=710)			
Pre-reform Migrants	0.226^{**}	0.211^{**}	0.220^{**}
	(0.10)	(0.10)	(0.10)
Post-reform Migrants	0.190	0.195^{*}	0.206^{*}
	(0.12)	(0.11)	(0.11)
Grade 9 (N=669)			
Pre-reform Migrants	0.563^{***}	0.588^{***}	0.580^{***}
	(0.15)	(0.14)	(0.15)
Post-reform Migrants	0.589^{***}	0.600^{***}	0.584^{***}
	(0.20)	(0.21)	(0.20)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Panel B, female

Dep var: Cognitive scores	**(1)**	**(2)**	**(3)**
Grade7 (N=735)			
Pre-reform Migrants	0.163*	0.190**	0.168*
	(0.09)	(0.09)	(0.09)
Post-reform Migrants	-0.038	-0.029	-0.055
	(0.10)	(0.10)	(0.11)
Grade 9 (N=749)			
Pre-reform Migrants	0.470***	0.455***	0.477***
	(0.17)	(0.17)	(0.17)
Post-reform Migrants	0.518***	0.557***	0.582***
	(0.17)	(0.17)	(0.17)
Selection variables			
Nursery	+		
Drinking		+	
Smoking			+

Note: Standard errors clustered at the school level in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

7. Conclusions

Using the 2013 China Education Panel Survey, we investigate the effect of easing migrant children's enrolment into urban schools, triggered by the 2008 urban school access policy on migrant children's educational outcomes. A novel identification strategy exploiting specific features of the database is adopted to mimic a quasi-experimental design in which the control group comprises non-migrant students staying behind in counties of rural *hukou* origin while the treatment group comprises their migrant counterparts enrolling in urban public schools in China's top migrant-receiving city-districts and counties. To allow for multiple treatment effects, we estimate an IPWRA model using three alternative exclusion restrictions.

Across all model specifications, our results are remarkably robust with respect to the choice of the exclusion restriction. The estimated headline *average treatment effect* (ATE) of the policy on a standardised cognitive test score for migrant children is a statistically significant 0.18 SD, equivalent to three quarters of the urban-rural gap in the nationally representative subsample. The ATEs tend to be larger for migrants to municipalities and provincial capitals, using both cross-sectional data and the panel of Grade 7 migrant children in the original survey. The latter specification allows an estimation of the value-added of one-year of urban schooling, which is shown to be up to twice as much as that for the non-migrant children studying in rural schools.

Moreover, we find significantly larger ATEs for Grade 9 migrant children relative to their Grade 7 counterparts, reflecting the longer exposure to urban schoolings for the former group. Migrant boys appear to benefit more from urban schooling than migrant girls. However, taking advantage of the one-year panel for Grade 7 children, it turns out that the value-added of one year of urban schooling is marginally higher for girls.

Our findings indicate that migrant children do benefit from this urban school access policy, despite the remaining entry barriers imposed by local governments on their parents' formal employment contracts, duration of social security contributions and even levels of education. Moreover, our results suggest that more years of urban schooling have a cumulative effect which helps to close the massive urban-rural gap in educational attainment in China.

One limitation of our study is that we are unable to disentangle the impact of the inclusive education policy on the school schooling choice of the rural *hukou* children from the migration and location decisions of their parents, due to a lack of the relevant information in the CEPS. Indeed, with information on the full choice set of schools, our analysis is necessarily restricted to the extensive margin of urban schooling for rural origin students, conditional on their parents' migration decisions. We leave these challenges to the future when more tailored surveys of migrant parents and their school aged children become available.

Children of migrants are set to be the drivers of China's urbanization process in the coming decades. Facilitating full access of all eligible migrant children into urban public schools is not only a matter of promoting equity, but also likely to result in substantial efficiency gains for given public investment in compulsory education. To the extent that migrant children are disproportionately enrolled in lower quality urban public schools, as suggested by earlier research, the excess returns to urban schooling on their cognitive scores should be an underestimate.

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Appendix

Figure A1: Standardised cognitive scores of urban native students by city tiers, supplementary sample only

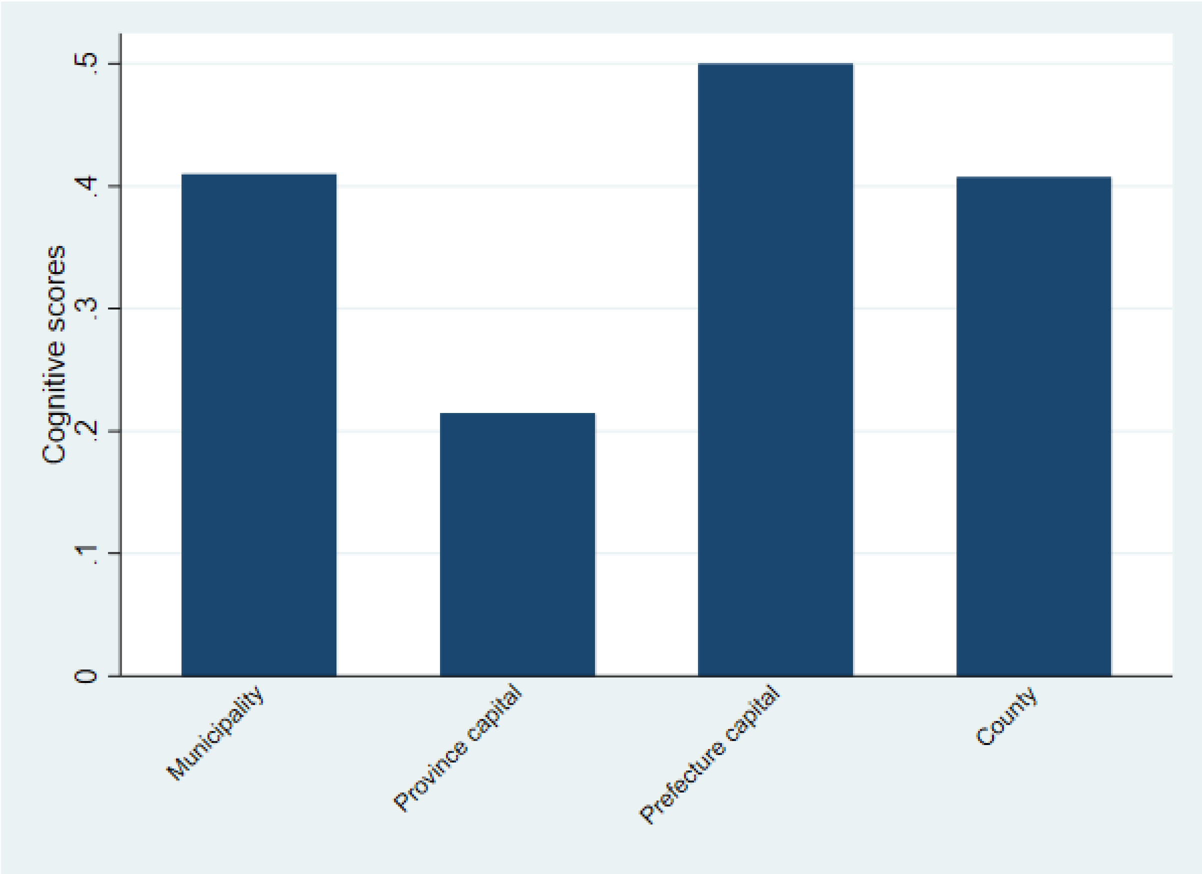

Table A1: Individual and Class-level cognitive scores, by local high school access policy

	Cognitive scores	Cognitive scores class-level
Rural natives	-0.247	-0.312
Pre-reform Migrants allowed to be enrolled in Local High-school	-0.055	-0.086
Post-reform Migrants allowed to be enrolled in Local High-school	-0.064	-0.193
Pre-reform Migrants not allowed to be enrolled in Local High-school	0.151	0.166
Post-reform Migrants not allowed to be enrolled in Local High-school	-0.042	0.054

Appendix A

Institutional settings and policy change

The children of migrant workers face a dilemma when it comes to educational choices. The *hukou* system entitles rural-born children to obtain school places only in the village and towns of origin. Going to school in the place registered in the rural *hukou* implies being looked after by grandparents or other relatives if parents are migrants and work in urban areas from where commuting is not possible. Alternatively, these children might stay with their migrant parents, but their access to urban public schools is plagued by uncertainty about what education may be available. The volume of children opting for the latter choice is large: according to the 2010 population census, there were around 35.81 million migrant children under 17 years old staying with their migrant parents, partly attracted by the hope of benefiting from the higher education quality available in the cities of destination (Yang 2017).

With the 1986 Compulsory Education Law, China introduced a system of 9-year compulsory education whereby "no tuition or miscellaneous fee may be charged in the implementation of compulsory education" (MoE 2009). However, It was not until 2006 and 2008 that free compulsory education was fully implemented in rural and urban areas nationwide, respectively, due to the complexity and diversity of the financing sources for education (Xiao et al. 2017).

During the two decades of transition, the rapidly increasing number of migrant children was managed inorganically, and with *ad hoc* policy measures. In 1992, the central government issued "Rules for the Implementation of the Compulsory Education Law", which in theory enabled parents of migrant children (without a local urban *hukou*) to apply to local governments of their place of residence for temporary schooling, subject to the payment of a "temporary schooling fee". The 1996 "Measures for School-age Children and Adolescents among the Urban Floating Population (Trial)" further regulates the standard of "temporary schooling fees". The 2001 "Decision on the Reform and Development of Basic Education" (*Guo Fa* [2001] No. 21) introduced a "two-oriented" principle, namely that migrant children's schooling should be "mainly administered by local governments of net migrant-receiving areas and mainly relying on full-time public primary and secondary schools", but the policy was not given the corresponding fiscal policy support. A 2006 survey of about 5,800 migrant students and their parents in 12 cities by the China National Institute for Education Research concluded that only 60.9% of them studied in urban public schools, with the rest studying in unregulated and often substandard “migrant schools” established since late 1990s (Tian and Wu 2010, Hu 2012). Moreover, 32.7% and 40.0% of migrant parents in the survey found it “very difficult” and “quite difficult” to find study places in urban public schools, respectively.

Qualitative research on education policy regarding migrant children in the 1986-2006/2008 transition period concludes that ambiguous policy goals and weak incentives “grant local governments and schools scope to act with discretion. Non-implementation of sufficient funding and school access policy result from self-interested and habitual decisions of local governments” (Hu 2012).

The situation changed in 2006, when a free compulsory education reform for rural areas was launched. This policy exempted all rural students in compulsory education from tuition and miscellaneous fees (Xiao et al, 2017; Tang et al, 2020). However, migrant students who were not enrolled in schools in “home counties” as per their *hukou* status were still ineligible for the new ‘free fee’ status.

In 2008, the State Council issued "The Circular of the State Council on Exempting Students' Tuition and Miscellaneous Fees for Compulsory Education in the City", which for the first time mandated urban public schools to exempt all children in compulsory education, including migrant children, from tuition and miscellaneous fees, and so-called “temporary schooling fees” (GoSC 2008) from Autumn Semester 2008. The stated objective of the policy initiative is to “implement the ‘Compulsory Education Law of the People's Republic of China’, implement the scientific development concept, and promote educational equity”. This implies that at least in principle, compulsory education had finally been made free for all children, migrant children included, in state primary and secondary education, more than two decades after the enactment of the 1986 Compulsory Education Law.

The central government has also offered appropriate financial rewards to provinces which have solved the problem of compulsory education for children with migrants relatively well. However, the new policy is still un(der)-funded as local governments are not adequately compensated by central government transfer payments. Implementation appears patchy, with local governments reluctant to remove the remaining barriers. Some cities in Guangdong province even adopted a new "point-based system" after pilot schemes to regulate a surging demand, under the framework of the New-type Urbanisation Plan (Wang 2020).[13]

Appendix B

Technical notes on IPWRA

The IPWRA approach compares individuals who have similar propensities to receive a given treatment, by weighting the sparse part of the distribution more heavily. It proceeds in the following steps:

1) Calculate an individual's probability of receiving potential treatment M_1 and M_2 (for pre- and post-2008 migration respectively), with rural native students attending rural schools as the control group, in Step 1 using multinomial logit with V denoting the pre-determined variables prior to the migration:

$$P(V) = \Pr\left(M = M_j\right) = \frac{V'\beta_j}{\sum_k V'\beta_k} \ for\ all\ j = 0, 1 \quad \text{(A1)}$$

2) Run a weighted OLS version of equation (2) in Step 2, using the weight as the inverse of probability of receiving the treatment j (i.e. 1/P(V) from Step 1 for treated groups, and 1/(1-P(V)) for the control group):

$$y_i = \alpha + \beta_1 M_1 + \beta_2 M_2 + X'_i\gamma + T_j + \epsilon_i \quad \text{(2)}$$

3) The ATE (Average Treatment Effect) is the difference between the probability-weighted outcomes y_i if all individuals received various treatments and the outcomes of not receiving treatments.

4) The ATT (Average Treatment Effect for the Treated) differs from ATT in using the treatment group as the reference, in the sense that treatment group members are assigned weights of 1 while the control group members are assigned weights of $p(V)/(1-p(V))$.

While both methods assume unobservables don't affect **both** the outcome and the probability of receiving treatments, IPWRA has the clear advantage of being "doubly robust" with respect to functional forms through reweighting the OLS in Step 2. Compared to PSM

[13] In 2015, the State Council finally enacted the policy of allowing the designated per capita education expenditure to follow the migrant child. However, this is still perceived as being heavily under-funded by migrant-receiving regions which tend to be much richer and spending more per student.

which only allows binary treatment, IPWRA is a generalisation in allowing multiple treatments.

In our setting, pre-2008 migrant children should also benefit from the 2008 policy change, as they no longer have to pay tuition fees or temporary schooling fees (plus they might also switch to better schools).