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Pursuing a brighter future: Impact of the *Hukou* reform on human capital investment in migrant children in China

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ABSTRACT

China's *Hukou* system, established in 1958, institutionalized social welfare disparities. This study examined the impact of China's *Hukou* reform—an institutional change initiated in 2014 that aimed to gradually expand the coverage of basic social benefits from the local *Hukou* population to the entire resident population—on the education expenditures of migrant households and the human capital development of migrant children using panel data from the China Family Panel Survey from 2012 to 2018. Drawing on regional variations in the pace of the reform, we applied the difference-in-differences method, and found that the reform significantly increased the investment in education for migrant households, with the increase mainly arising from in-school expenditures (with the exception of sponsorship fees) rather than from off-school expenditures. The analysis of the underlying mechanism shows that, first, although the reform has expanded the access to public education for migrant children, the local governments did not invest more in public education following the reform (substitution effects). Second, the *Hukou* reform appears to have eliminated resource discrimination and improved quality within the public education system, which increases migrant families' expectations for their children's education; this also prompts them to increase their in-school education expenditures to improve the quality of their children's education (flypaper effects). Finally, increases in migrant family income also play a role (income effects). Our study has strong practical implications for policymakers, who need to sustain the supply of human capital for economic development by providing education to migrant children, while moderating the effects of social welfare reforms to reduce possible governance risks.

1. Introduction

In the context of large-scale urbanization, China's household registration (*Hukou*) system has long been regarded as a dual welfare

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arrangement that has led to inequalities between migrants and local urban residents (Cai, 2011). Compared to local residents, the migrants without a local *Hukou* were discriminated on the job market and had limited access to social services such as hospitals and schools, which constrained industrialization, the migration scale, and the public financial capacity in the cities where migrants live (Fan, 2002; Liu, 2005; Whalley & Zhang, 2007). While similar welfare systems are common in countries undergoing economic transition, governments have increasingly focused on eliminating institutional obstacles to migrants' welfare through reforms (Boräng, 2015; Juárez et al., 2019; Sabates-Wheeler & Koettl, 2010). China, as the most populous country, has the largest number of rural–urban migrants (297.53 million in 2023). However, the *Hukou* system has been a serious obstacle in the lives of Chinese migrants and also poses a challenge to China's economic transition in the new era. Numerous studies have explored how *Hukou* reforms relate to income (Pi & Zhang, 2016), housing (Wu & Zhang, 2018), consumption (Wang, Ai, & Huang, 2021), and savings (Fields & Song, 2020). However, few studies have investigated the relationship between *Hukou* reforms and migrant families' human capital investment.

This study thus focuses on the nationwide *Hukou* reform initiated by the Chinese government in 2014, which aimed to restructure welfare by eliminating *Hukou* restrictions in cities with <3 million inhabitants and relax *Hukou* restrictions in cities with >3 million residents. This reform is likely to change the economic behavior and decisions of migrant families in two ways. First, migrant families may be more likely to migrate due to reduced migration costs. Second, they may change household investment (e.g., in their children's human capital) because of their increased labor productivity and higher income (Pi & Zhang, 2016). However, given the dramatic growth in the number of migrants and the increasing education expectations, public education resources have fallen short of fulfilling the educational needs of migrant children. Compared to other social benefits, providing equal access to public education has been relatively slow, being considered the "last mile" in the *Hukou* reform. Essentially, children's education forms the basis for migrant families to accumulate human capital and achieve social mobility, making the investment in education of immense significance to both migrants and policymakers. Therefore, this study evaluates the impact of the 2014 *Hukou* reform on the investment in education for migrant children (i.e., who migrate with their parents). Specifically, considering that the 2014 *Hukou* reform targeted migrant children only at primary and junior high schools (i.e., compulsory education), we investigate whether and how this reform has affected compulsory education expenditure for migrant families.

To thoroughly examine the effects of the reform, we combine panel data from the China Family Panel Survey (CFPS) from 2012 to 2018 with *Hukou* reform information proxied by city size. In China, local governments remain independent in implementing the policies of the national government and often set the pace and rhythm of policy adjustments based on their own realities (Sun, Peng, & Huang, 2011). Many studies on China's *Hukou* reform tend to ignore this aspect, while measuring the reform process using a one-size-fits-all approach and neglecting the regional differences in reform implementation. To address this issue, we consider the substantial variations in the pace of reform across cities of different sizes in China and employ the difference-in-differences (DID) method to investigate the impact of the *Hukou* reform on the investment in education for migrant children by including both the total expenditure and the structure of education expenditure.

The *Hukou* reform in 2014 focused on the equalization of public services, including education; that is, migrant children can receive compulsory education at the migration destinations without needing their families to pay additional costs (e.g., sponsorship fees). Therefore, the funds saved by these migrant families can be used for other purposes. In this process, there is a "substitution effect" between the government and families in terms of their expenditure on education, with the reform reducing the investment in education for migrant families (Chi & Qian, 2016). However, as an important pathway to long-term human capital accumulation, children's education is investment-oriented, with families seeking to earn human capital gains in the future by investing in their education in the present (Foster & Rosenzweig, 1996; Glewwe & Jacoby, 2004). On the one hand, the reform has relaxed the credit constraints faced by migrant families in their destination cities, expanded employment channels, and raised income levels, thus increasing children's investment in education and creating an "income effect" (Das, 2021; Naoi et al., 2021). This surplus income could be allocated to all family members, not being necessarily spent on children's education (Mimura, 2021). Furthermore, the reform has been conducive to the long-term settlement of migrant families in destination cities. Compared to rural areas, the higher quality of education in cities can raise the expectations related to higher education and education investments; thus, migrant families might use the education expenditures saved due to the reform as immediate investments in their children's education to improve the quality of their education and, thus, create a "flypaper effect" (Ambler, Aycinena, & Yang, 2015; Canavire-Bacarreza, Chong, Ríos-Avila, & Yáñez-Pagans, 2020; Shi, 2012). The Chinese public education system can only guarantee the completion of the legal years of education for children and provide a basic quality of education. As such, families need to rely on household-level funds to provide their children with high-quality education to attain human capital returns in the future. As few studies investigate how the *Hukou* reform works through these channels, it remains an important question worthy of further attention.

We obtained three main findings. First, *Hukou* reform significantly increased total education expenditure on children who migrated with their parents. Specifically, *Hukou* reform increases the household total education expenditures by 82.4%. Second, the increase in total education expenditure was mainly increased in-school rather than off-school expenditure, indicating that the reform has led to a gradual flow of previously off-school expenditure to in-school expenditure. Third, we tested three channels and identified that the flypaper and income effects outweighed the substitution effect. Specifically, although the *Hukou* reform made it easier for migrant children to enter public schools, government expenditures for public education did not increase following the reform. Rather, local governments intentionally or unintentionally used tricks in equalizing public education resources, including placing children of the non-local *Hukou* population in public schools outside their own school districts in areas where public education resources were scarce. This partially explains why private education expenditures by migrant families was not substituted by government expenditure. In contrast, the government attempted to send a signal of providing higher quality of compulsory education in the way that more migrant children were enrolled in the key class where best education resources are allocated to students, which led to higher educational expectations in migrant parents who thus invested more on in-school expenditures. Not surprisingly, this flypaper effect was

accompanied by higher incomes for migrant families following the reform.

This study fills this gap in the literature in three ways as follows. First, it adds to the growing literature on the factors linked to children's education expenditures. Most research in this area has focused on the effects of household characteristics on the expenditure for children's education, such as household size (Conley & Glauber, 2006), income (Karki Nepal, 2016), risk preferences (Tabetando, 2019), migration and remittances (Askarov & Doucouliagos, 2020), and social networks (Zuluaga, 2013); child gender (Azam & Kingdon, 2013; Vogel & Korinek, 2012; Wongmonta & Glewwe, 2016); as well as parental education, age, and occupation (Jenkins, Anyabolu, & Bahramian, 2019; Yan, Peng, Hao, Irfan, & Wu, 2021). Several recent studies have focused on the impact of environmental characteristics and changes on household education expenditures, such as income uncertainty (Kazianga, 2012; Sirisankanan, 2017), inequality of opportunity (Song & Zhou, 2019), and gender wage differentials (Wang & Cheng, 2021), the findings mostly demonstrating negative effects. Other studies have examined the effects of policies actively targeting specific populations in different countries, including pension schemes (Canavire-Bacarreza et al., 2020), education subsidies (Ambler et al., 2015; Naoi et al., 2021), compulsory education reforms (Kubota, 2016), and tuition fee waiver policies (Chi & Qian, 2016; Shi, 2012), among others. We add to this line of work by providing evidence from a quasi-natural experiment of top-level institutional shocks in a developing country context.

Second, this study adds to the extensive literature that examines the structure of education expenditures. For example, Mu and Du (2017) found that the increase in family education expenditure caused by the pension reform was spent on off-school educational activities. Shi (2012) suggested that the intra-household flypaper effect is caused by the expenditure on voluntary education. Das (2021) found a significant positive effect of income on private tutoring expenditure that did not influence on other educational expenditure. Kubota (2016) studied the educational reform that reduced school instructional time in Japan in 2002, and found that the reform increased off-school expenditures to varying degrees. Focusing on the impact of the exemptions in China's compulsory education law, Chi and Qian (2016) found that Chinese parents spend more on academic-related courses than on interest classes. For Chinese families, the purpose of off-school expenditures is not to develop children's interests and supplement in-school education, but to enhance their academic performance for further education. However, the findings of existing studies do not focus on rural migrants. As Chinese domestic migrants are a vulnerable group, they lack the necessary experience to invest in improving the quality of their children's education due to their low human capital level. Further, it is unclear how migrant families allocate their spending across educational activities, which constitutes an important empirical problem to consider.

Third, this study enriches the existing literature on the effects of the *Hukou* reform on the target population by focusing on the social welfare, income (Pi & Zhang, 2016; Shi, 2018; Wang, Akgüç, Liu, & Tani, 2020), and changes in household consumption of migrants (Chen, Lu, & Zhong, 2015; Wang et al., 2021). This approach thus complements extant studies by examining the effect of the 2014 *Hukou* reform on the expenditure on the education of migrant children, as a proxy of long-term household-level investment in human capital. We also explore different mechanisms by which the reform affects the structure of expenditure on education by testing the role of the substitution, flypaper, and income effects.

The remainder of this paper is structured as follows. Section 2 provides the historical background of the *Hukou* reform, focusing on institutional details relevant to the research topic. Section 3 describes the data sources and outlines the research methodology. Section 4 presents and discusses the main results and Section 5 draws conclusions.

2. Background to China's *Hukou* reform

During the Mao era, which began in 1949, China had planned an economic system and national strategy that prioritized the development of the heavy industry, drawing on the large-scale economic construction experience of the former Soviet Union. As developing heavy industry requires more capital than labor, this system required constructing an urban–rural dual structure that could support industrial development at the expense of agriculture. This required a large number of people to engage in agricultural production to provide the material basis for the development of heavy industry (Li & Yang, 2005). The agricultural population had to be highly spatially concentrated in rural areas to create a stable external environment for the development of the heavy industry (Zhang, Wang, & Lu, 2019). As such, the central government issued the Regulations of *Hukou* Registration in 1958, which established a *Hukou* system based on the type of *Hukou* status (agricultural and non-agricultural) and *Hukou* location (Song, 2014), making the *Hukou* status hereditary by law (Montgomery, 2012). The *Hukou* system limited the free migration of the agricultural population, while guaranteeing the urban population (industrial workers) access to infrastructure and social benefits (Pi & Zhang, 2016). Subsequently, social benefits were distributed differentially based on *Hukou* status (rural and urban), deepening the dualism of urban-rural relations and becoming a source of social inequality in China. Under this system, the majority of the labor force was confined to agricultural activities, leading to a decline in labor productivity. The income workers earned from agriculture only provided food and clothing, but could not compensate for a lack of human capital, such as the knowledge and skills gained in the production process. This situation lasted until the late 1970s when China launched its large-scale reform and opening up.

In 1978, the reform and opening up led by Deng Xiaoping was marked by the gradual lifting of restrictions on the free migration of the labor force, including the agricultural population, who were now allowed to enter cities; thus, the *Hukou* reform was initiated. This process was accompanied by the decentralization of the government's *Hukou* management function. Compared to the previous period, when the central government set the criteria for *Hukou* conversion and regulated the process (Tang, 2013), local governments were granted greater adjudicative power over setting the criteria and number of *Hukou* admissions within their administrative areas (Chan, 2009; Wang, 2005). In reality, driven by the national strategy of focusing on economic development, many agricultural laborers migrated from rural to urban areas to work in non-agricultural sectors, becoming an important labor resource for regional economic development. As the influx of the non-local *Hukou* population was increasingly accepted by local governments, their *Hukou*

management gradually changed from focusing on the administrative control of population migration to the development and utilization of migrant human capital. The breakdown of population migration control and the changes in the function of the *Hukou* system, coupled with the implementation of the household responsibility system in the agricultural sector, freed hundreds of millions of laborers from lower-productivity sectors, such as agriculture. They thus entered higher-productivity sectors, such as manufacturing and services through unprecedented large-scale migration across urban and rural areas and among regions.[1] Consequently, the optimal allocation of human resources among economic sectors directly led to a significant increase in labor productivity.

Notably, prior to the 2012 slowdown, China's economic development depended on optimizing the demographic dividend through the local decentralization of the *Hukou* system and the tournament model of economic development.[2] As China's economy developed, relying on the optimization of the labor force became unsustainable and labor productivity growth has gradually slowed down since 2010. This slowdown is partly due to the convergence of the labor productivity levels between urban and rural areas in China as early as 2004, reaching the Lewis turning point of urban–rural labor migration (Cai & Wang, 2010). Moreover, the massive cross-regional labor migration (mainly from inland to coastal areas) in the following six years also contributed to the convergence of labor productivity levels between regions. The narrowing of the wage gap prevented further labor movement to economically developed regions.

However, central and local governments did not realize the unsustainability of the demographic dividend during this period. The *Hukou* system, being dominated by local governments, still set relatively strict restrictions on the non-local *Hukou* population acquiring local *Hukou* and enjoying social benefits,[3] which further reduced the demographic dividend.

With China's slowdown since 2012, coupled with the rapid aging of the population and the shrinking external market demand due to the global recession, central and local governments began to realize that the persistent labor supply shortage might be a long-term critical problem limiting China's economic development. Therefore, in the 12th Five-Year Plan for National Economic and Social Development, the *Hukou* reform was considered a priority for China's economy (Zhang, 2012). The purpose of the reform was to expand access to public services and social benefits for the migrant population. As such, the reform could substantially reduce the cost of labor migration and promote the human capital development of the migrant labor force so that China's economy could gradually shift from being quantity-based to being quality-based. Subsequently, the central government promulgated the National New-Type Urbanization Plan (2014–2020) in 2014 to promote the *Hukou* reform. Specifically, it lowered the threshold for acquiring local *Hukou,* and provided equal access to basic public services, setting an urban population of 3 million as the threshold for abolishing restrictions on *Hukou* conversion. That is, cities with an urban population below 3 million were to gradually abolish restrictions on settlement, while cities with a population above 3 million were to reasonably determine the conditions for acquiring a *Hukou* while actively expanding the coverage of basic public services to the resident population and facilitating access to basic public services in urban areas for the rural–urban migrants (i.e., those working and living in urban areas without a local city *Hukou*). Fig. A1 shows that productivity growth mirrored (although with a larger magnitude) the growth rate of the number of migrant workers before 2014. After 2014, the gap was enlarged, with the former rising quickly and the latter remaining stable. This indicates that the productivity growth was potentially due to the improvements in human capital driven by the *Hukou* reform.

Notably, under a decentralized system, the *Hukou* reform in a particular region was managed by the local government. The central government set the direction of the reform and established general standards, but reform practice was applied by local governments according to their economic and social development situation. In other words, local governments dictated the pace and timing of the reform according to the requirements of the central government. However, in reality, lowering the threshold for obtaining local *Hukou* and expanding social welfare coverage to include the non-local *Hukou* population were not always consistent with the requirements of the central government, with differences in the local governments' implementation of the reform (Table A1). Zhang et al. (2019) corroborated this finding; specifically, they collected and organized household registration policy documents on settlement and the equalization of basic public services at prefecture level cities to construct a *Hukou* registration index for 120 cities from 2000 to 2013 and from 2014 to 2016 based on four aspects: investment and tax, housing purchase, talent introduction, and ordinary employment. We mainly used the ordinary employment index because it is more applicable to rural-to-urban migrants.[4]

Fig. A2 identifies the differences in the *Hukou* registration index before and after 2014 in different cities. Most large cities (with an urban population above 3 million) had negative differences in the *Hukou* registration index, indicating that they raised the standards for obtaining local *Hukou* and set higher barriers to accessing basic public services, with some exceptions, such as Nanjing, Changsha, and Chengdu. Conversely, most third-, fourth-, and fifth-tier cities (with an urban population below 3 million) had positive differences

[1] Between 2004 and 2023, the number of Chinese rural migrants reached a maximum of 293 million.

[2] China's economy began to decelerate in 2012, from double-digit to single-digit growth, entering a "new normal."

[3] The *Hukou* reform led by the local governments during this period had two objectives: the unification of *Hukou* types (i.e., abolishing the distinction between local agricultural and non-agricultural *Hukou)* and allowing the non-local *Hukou* population to obtain a local *Hukou* in small cities and towns with a population below 500,000. However, it is generally agreed that these reforms have not benefited the majority of migrants (Song, 2014).

[4] This index considers four requirements for migrants to obtain local *Hukou*. The first requirement is educational attainment, with values assigned as 6, 9, 12, 15, 16, 19, and 22 for a city that requires migrants to complete primary school, junior high school, high school, college (technical secondary school), undergraduate, master, and doctoral degrees, respectively. The other requirements include whether the migrants have stable employment, stable housing, and enrollment in public service scheme, with values of 0 for non-requirement cities, 1 for cities that have specific requirements, and equal to the number of years if the city requires so. The projection pursuit, entropy, and equal weight methods were used to construct the index.

in the *Hukou* registration index, indicating that these cities lowered the criteria for obtaining local *Hukou* and facilitated access to basic public services for the non-local *Hukou* population. However, some small- and medium-sized cities in developed regions, such as Wenzhou, Shantou, Zhanjiang, and Dandong, showed the opposite trend. In Fig. A2, the size of the dots reflects the magnitude of the differences in the *Hukou* registration index before and after 2014. Even among cities of the same population size, the degree of the *Hukou* reform varied greatly due to the autonomous decisions of the local governments.

3. Methods

3.1. Data

Our primary analysis combines data on the *Hukou* reform information (based on city size) with data from a nationally representative and biennial longitudinal household survey, the China Family Panel Study (CFPS). The CFPS is designed to collect individual-, family-, and community-level longitudinal data through stratified, multistage, and multilevel probability sampling. In total, 37,354 observations from 14,960 sampled households were obtained, covering 25 provinces, cities, and autonomous regions in mainland China, which represent 95% of the country's population. For analysis, we utilized four types of information from 2012 to 2018, including the demographic characteristics, household status, spending on education, and economic status of each child and their parents.[5] Only data on migrant children were retained for the main analysis because the *Hukou* reform is more likely directly affect the education of migrant children,[6] as they are more vulnerable compared to local children (Wei & Gong, 2019).

Our sample is restricted to migrant children in the "compulsory education stage (including primary school and junior high school)," who attended primary or junior high school in 2014 or earlier and were still in the compulsory education stage by 2018. This led to a final sample of 3393 observations. The above selection criteria has several advantages. First, the reform's impact on public education resources can be identified more clearly during the compulsory education stage (i.e., primary and junior high school) because this stage is regulated by policies related to *Hukou* in terms of both eligibility and education quality, with no other policy interference. Second, the guarantee of compulsory education for migrant children in cities spans the implementation point of the 2014 reform; thus, the differences in education expenditures for migrant children can be attributed to the different stages of reform promotion and implementation.

The outcomes of interest are total education expenditure; in-school expenditure as a whole and its components, including sponsorship fees and other fees; and off-school expenditures. The in-school expenditure includes school fees, meals, accommodation, bus fares, textbooks, reference books, learning aids, sponsorship fees, and school activity fees. Off-school expenditure includes expenses incurred for talent training, mental development classes, tutoring, coaching for competitions, and other classes. The total education expenditure is calculated as the sum of in- and off-school expenditure. All education expenditures measured in the survey year were deflated based on the consumer price index in each province. These rich survey data allowed us to analyze a diverse range of education expenditures at the individual level, which differs from previous studies that used national or provincial survey data or nationally representative cross-sectional data (Cheng, 2021; Meyerhoefer & Chen, 2011; Qian & Smyth, 2011; Shi, 2012; Yan et al., 2021) This study also uses a broader sample and a longer follow-up period, which is conducive to the more accurate estimation of the effect of the *Hukou* reform on the spending of migrant families on education.

As the reform mainly took place in small cities with urban population below 3 million, we constructed the *Hukou* reform indicator using city size information, with small cities considered as the treatment group and those with an urban population above 3 million as the control group. Following An, Qin, Wu, and You (2024), we also changed the threshold to 5 million and used the *Hukou* registration index to verify the robustness of the results. Zhang et al. (2019) constructed the *Hukou* registration index system for 120 cities from 2000 to 2016 and used a projection pursuit model (PPM) to classify and measure investment, house purchase, high-end employment, and ordinary employment. Taking the 2014 national level *Hukou* reform as the cut-off point, the values for each *Hukou* registration index type before and after 2014 were obtained. Owing to the generally low levels of wealth and human capital among rural migrants, most of them have been unable to settle in urban areas or access basic public services through investment, house purchase, or high-end employment, their only access thus being ordinary employment. Therefore, we chose the difference between the ordinary employment index before and after 2014 to represent the level of the *Hukou* reform (i.e., subtracting the index after 2014 from that before 2014).[7]

We conducted additional analyses using data on province-level urban per capita education expenditure and government inputs from 2012 to 2018 from the Chinese Statistical Yearbook. We used household- and regional-level control variables. The household-level control variables included parents' highest level of education, average parental age and age-squared, family size, parental occupation, average parental health status, and number of children in school, in line with the recent literature (Jenkins et al., 2019; Qian & Smyth, 2011; Song & Zhou, 2019; Yan et al., 2021; Shi et al., 2023). To mitigate the endogeneity of the *Hukou* reform, we used regional-level control variables, namely the marketization index (obtained from China's Marketization Index of China's Province, edited by Wang, Fan and Zhu (2018), per capita GDP, number of primary and secondary school students, number primary and

[5] We matched parental information from the adult questionnaire with family information from the family questionnaire for each child.

[6] Since CFPS does not directly identify the rural migrants in the sample, we relied on the migrant population identification method on the official website to determine the agricultural *Hukou* population living in cities and not the local *Hukou* population that are rural migrants; we then also considered the children living in the same space as migrant children.

[7] The 120 cities covered by the *Hukou* registration index did not exactly match the cities in the CFPS database; for cities that could not be matched, we used the *Hukou* registration index of adjacent cities with similar economic conditions.

secondary school, government revenue, government expenditure, per capita income, and per capita fiscal expenditure for primary and secondary schools (obtained from the China Urban Statistical Yearbook). We show the summary statistics for the outcomes of interest and main control variables for the full sample in Table 1 and for the treatment (city size <3 million) and control groups (city size ≥3 million) in Table A2. Specifically, Table 1 shows the regional differences in the ordinary employment index changes, which decreased from 0.493 to 0.421.

Since the *Hukou* reform was implemented effectively in cities with population below 3 million, we further obtained descriptive statistics of the investment in education for rural migrant households in large and small cities, as shown in Table 2. The maximum growth rate of all education expenditures in large cities occurred in 2014, except for sponsorship fees, which occurred in 2016. By contrast, the largest growth of the total education expenditure in small cities occurred after the reform, namely in 2016 and 2018. This result suggests the potentially positive impact of *Hukou* reform on the investment in the education of migrant children in small cities.

3.2. Estimation strategy

To evaluate the effects of the 2014 *Hukou* reform on migrant families' investment in education, we estimated the following equation:

$$lnC_{ijt} = \alpha + \beta \times \left(treat_j \times post\right) + \gamma \times X + \lambda_i + \delta_t + \mu_j + \varepsilon_{ijt} \tag{1}$$

lnC_{ijt} is a measure of the investment in education for individual i living in city j in year t. The key explanatory variable, $treat_j$, refers to cities with an urban population below 3 million, and measures the extent to which the *Hukou* reform in that city has advanced (i.e., the change in the ease of access to public education for the non-local *Hukou* population).[8] *post* is the implementation point, taking a value of 0 for the year 2014 and earlier and a value of 1 for years after 2014. We controlled for individual fixed effects, λ_i, to absorb the effect of factors that do not vary over time at the individual level. We also included year fixed effects, δ_t, and city fixed effects, μ_j. X is a set of household- and region-level control variables. The key parameter of interest, β, estimates how the investment in education changed in response to the *Hukou* reform.

The validity of the estimation depends on the uniqueness of the *Hukou* reform shock and the conditional exogeneity of city size. Our first concern is that the effects could also be compounded by the *Hukou* reforms before 2014. As discussed in Section 2, prior institutional adjustments to the *Hukou* system at the national level occurred in the 1990s and focused on easing the free migration of labor (Yang & Zhou, 1999). After the State Council issued "Opinions on Further Promoting the *Hukou* Reform" in 2014, municipalities nationwide issued supporting documents. All provinces completed the *Hukou* reform in 2016 as required.[9] An et al. (2024) provided some preliminary evidence that the previous reforms did not have different results across different city sizes.

We also verified whether the *Hukou* barriers in the small cities with a population below 3 million suddenly changed after 2014 compared with large cities with a population above 3 million. We used the *Hukou* index difference as the outcome variable and an indicator for whether the city has a population above 3 million as the treatment variable. The results in Panel A of Table A3 suggest that, after 2014, the ordinary employment index and the composite index suddenly decreased in small cities compared to large ones. As we did not identify any significant changes in the other three types of *Hukou* registration index, including investment, house purchase and high-end employment, this result suggests that the *Hukou* reform mainly targeted migrant workers. The results remain robust if we use the threshold of 5 million as treatment status (Panel B of Table A3), which is consistent with the summary statistics in Table A4 showing a decrease in the *Hukou* registration index for all cities with population below 3 million, an average increase of 0.005 for cities with population between 3 and 5 million, and an average increase of 0.167 for large cities with population between 5 and 10 million. This result suggests that the reforms mainly took place in cities with a population size below 3 million, which aligns with the overall design of the *Hukou* reform at the national level. However, many cities with a population above 3 million raised the *Hukou* threshold and, consequently, the difficulty of accessing basic public services. As such, in these cities, the reform has moved in the opposite direction of the original national-level design. Table A5 shows another piece of evidence regarding the changes after the 2014 reform, indicating that small sized (< 3 million) cities had the highest rate of floating population growth, while the growth rate in large sized cities decreased during 2016–2018. This is why we used the *Hukou* registration index and threshold of 5 million (An et al., 2024) as the treatment status for robustness checks, in addition the threshold of 3 million as a continuous treatment for the main analysis.[10]

However, the progress of the *Hukou* reform in each city under the decentralized system was not exogenous, but likely related to city characteristics and the result of self-selection by each city. Therefore, we included city fixed-effects in the model to exclude the possible estimation biases caused by unobservable city-level characteristics that do not change over time (e.g., geography and culture). In addition, to address the possibility of omitted-variable bias, we controlled for some urban characteristics that vary over time and by city. To mitigate any possible endogeneity, we controlled for the marketization index, per capita GDP, government revenue,

[8] This study identifies the effect of the *Hukou* reform on the education expenditure of migrant children using the DID method. However, to determine the effect brought about by the *Hukou* reform rather than the *Hukou* registration index, we measured the degree of advancement of the *Hukou* reform using the difference in the *Hukou* registration index before and after 2014 in each city.

[9] Table A1 shows the dates when all provinces issued policy documents on the *Hukou* reform.

[10] The results using both thresholds remained identical in the robustness checks. However, we used a threshold of 3 million because the national-level *Hukou* changes for cities with populations of 5 million mainly occurred in 2019. With the issuance of the "Key Tasks of New Urbanization Construction in 2019" by the National Development and Reform Commission, the government attempted to fully liberalize and relax the *Hukou* settlement criteria for cities with population sizes below 5 million.

Table 1
Summary statistics.

Variables	Mean	S.D.	Mean	S.D.	Mean	S.D.
All Cities	Full Sample		Before		After	
Treat	0.820	0.384	0.828	0.377	0.813	0.390
Hukou registration Index	0.459	0.243	0.493	0.192	0.421	0.270
Total education expenditures, log	5.955	2.944	5.289	3.210	6.642	2.461
In-school expenditures, log	5.553	2.888	5.042	3.119	6.081	2.523
In-school expenditures other than sponsorship fees, log	5.522	2.891	5.030	3.111	6.031	2.548
Sponsorship fees, log	0.100	0.814	0.055	0.624	0.147	0.970
Off-school expenditures, log	1.701	3.214	1.345	2.854	2.069	3.511
Household-level control variables						
Parental education level	3.163	1.043	3.145	1.043	3.181	1.043
Parental age	36.077	5.986	34.343	5.815	37.872	5.621
Age of children	11.232	3.408	9.467	3.014	13.054	2.774
Family size	5.287	1.980	5.319	1.997	5.254	1.963
Parental health status	2.779	0.860	2.753	0.866	2.806	0.852
Parental occupation[a]	–	–	–	–	–	–
Number of children in school	1.103	0.747	0.877	0.724	1.337	0.696
Regional-level control variables						
Marketization index	7.147	1.710	6.795	1.620	7.509	1.725
Per Capita GDP, log	10.943	0.550	10.871	0.577	11.017	0.510
Number of students in primary and secondary schools	32.237	45.138	29.334	34.518	35.234	53.794
Number of primary and secondary schools	351.492	399.339	345.178	376.384	358.920	421.715
Public budget revenue, log	13.564	1.532	13.399	1.527	13.734	1.518
Public budget expenditure, log	14.112	1.279	13.916	1.273	14.314	1.253
Per capita income,log	10.267	0.307	10.129	0.280	10.410	0.265
Per capita fiscal expenditure for primary and secondary schools,log	9.235	0.423	9.015	0.396	9.461	0.342
Number of observations		3393		1723		1670

Notes: This table reports summary statistics for the response and principal control variables. The parental educational level is an ordinal variable that takes the following values: 1 = illiterate, 2 = elementary school, 3 = junior high school, 4 = high school, 5 = college, 6 = bachelor's degree, 7 = master's degree. The parental health condition is an ordinal variable with the following values: 1 = very healthy, 2 = healthy, 3 = moderately healthy, 4 = fair, 5 = not healthy.

[a] Please refer to https:www.isss.pku.edu.cn/cfps/ for CFPS occupation codes, including paramedics, firefighters, elementary school teachers, restaurant service workers, plant production workers, etc.

Table 2
Descriptive statistics for education expenditures.

	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
Year	Mean	Rate	Mean	Rate	Mean	Rate	Mean	Rate	Mean	Rate
<3 million										
2012	1403.097	0.000	1142.028	0.000	1118.794	0.000	22.30923	0.000	254.6308	0.000
2014	1779.928	0.269	1312.129	0.149	1295.708	0.158	7.824968	−0.649	450.7979	0.770
2016	2522.624	**0.417**	1916.567	**0.461**	1889.643	**0.458**	20.96552	**1.679**	592.1122	0.313
2018	3000.573	0.189	1789.083	−0.067	1706.072	−0.097	10.22679	−0.512	1201.245	**1.029**
>3 million										
2012	1972.504	0.000	1503.307	0.000	1427.832	0.000	75.18248	0.000	464.8175	0.000
2014	3043.426	**0.543**	1936.257	**0.288**	1848.206	**0.294**	44.02516	−0.414	1080.881	**1.325**
2016	4056.829	0.333	2332.398	0.205	2140.829	0.158	96.66667	**1.196**	1712.52	0.584
2018	5376.404	0.325	2073.956	−0.111	2037.825	−0.048	30.30303	−0.687	3298.252	0.926

Notes: This table shows the means and growth rates by year and population size.

government expenditure, per capita income, number of students in primary and secondary schools, number of primary and secondary schools, and per capita fiscal expenditure for primary and secondary schools (Tabetando, 2019; Wang & Cheng, 2021; Yan et al., 2021).

In addition, to investigate the dynamic effects, we conducted an event study by examining the differences in investment in education over years. We introduced the interaction terms between the $treat_j$ and year dummies as follows:

$$lnC_{ijt} = \alpha + \sum_{t=2012}^{2018} \beta\left(treat_j \times year_t\right) + \gamma \times X + \lambda_i + \delta_t + \mu_j + \varepsilon_{ijt} \tag{2}$$

4. Results

Here, we show whether and how the *Hukou* reform affected migrant families' education investment in their children. We also conduct a series of sensitivity tests to ensure our main results are robust.

4.1. Main results

First, we explored the effect of the *Hukou* reform on migrant families' total investment in education. Column 1 of Table 3 shows the estimation of Eq. (1) with the total education expenditure as the dependent variable and household- and region-level control variables; individual, year, and city fixed effects are also controlled for. The results indicate a statistically significant and economically meaningful effect of the *Hukou* reform on total educational expenditure. Specifically, the *Hukou* reform increased the household total education expenditures by 82.4%.[11]

Next, we investigated the changes in the structure of the education expenditure and estimated Eq. (1) with specific expenditure items reflecting education expenditure as the dependent variable. Column 2 of Table 3 shows the results for in-school expenditure, indicating that the *Hukou* reform increased in-school expenditure by 131.2%. Column 3 shows the estimation results for off-school expenditure, suggesting that the *Hukou* reform reduced off-school expenditure by 94.8%. Combining the above estimates of in- and off-school expenditure, we can conclude that the increase in total education expenditure was mainly caused by the rise of in-school spending, indicating that the reform led to the gradual change of previously off-school expenditure into in-school expenditures.

To explore the impact of the reform on in-school expenditure, we further decomposed in-school expenditure into sponsorship fees and in-school expenditure other than sponsorship fees. Sponsorship fees refer to the extra fees paid by the families of migrant children before the reform to obtain local public education due to their non-local *Hukou* status. Theoretically, they represent an institutional cost of obtaining public education affected by the *Hukou* reform in each city. Column 4 shows the statistically insignificant effect of the *Hukou* reform on sponsorship fees. This result could be because, before the reform, some cities limited access of migrant children to public education in terms of *Hukou* management and social welfare arrangements. Thus, fewer migrant children were able to access public education by paying sponsorship fees,[12] forcing most migrant families to enroll their children in migrant schools. Such schools do not require sponsorship fees but provide a poorer-quality education because of the lack of qualified teachers, inadequate teaching materials, and poor sanitation. Data from the pre-reform period show that $<1\%$ of the migrant children's parents paid sponsorship fees for access to public education, with $>80\%$ of migrant children attending migrant schools. Since the private education institutions chosen by migrant families cannot usually meet the basic quality requirements of compulsory education (Bao, 2006), many families compensate the low-quality of in-school education through off-school tutoring and training. Column 5 shows that the *Hukou* reform led to a 147.7% increase in in-school expenditures other than sponsorship fees. Therefore, we can infer that the increase in in-school expenditures resulting from the reform is mainly due to the increase in in-school expenditures other than sponsorship fees.[13]

We further examine the dynamic effects of the *Hukou* reform on education expenditure based on Eq. (2). The regression results are presented in Table 4. The estimated coefficients on total education expenditure are 0.648 in 2016, being significant at the 10% level, indicating that the positive effect of the reform was stronger in the short term and decreased slightly in the long term. Using in-school expenditure as the dependent variable, the estimated coefficients were 0.705 and 0.924 in 2016 and 2018, respectively. This result indicates that the positive effect of the reform has increased significantly over time. The same trend was identified for in-school expenditures other than sponsorship fees, with estimated coefficients of 0.731 and 0.973 in 2016 and 2018, respectively. However, different from the baseline regression, we found weak evidence of a reduction in sponsorship fees induced by the *Hukou* reform in 2016, with an estimated coefficient of -0.310; however, the effect was small and significant only at the 10% level. Moreover, it disappeared in 2018, thus offsetting the overall effect of the reform on sponsorship fees. The *Hukou* reform also had a significant negative impact on off-school expenditure only in 2018, with an estimated coefficient of -1.168 at the 5% significance level. This indicates that the reform caused a reduction in the off-school expenditures of migrant households. Based on the trends over time, we can infer that the reform affected the total education expenditure of migrant households in the short term (2016) mainly by redirecting sponsorship fees to other in-school educational expenditures. Meanwhile, in the long term (2018), the reform prompted the flow of previously off-school expenditures to in-school educational expenditures other than sponsorship fees.

4.2. Robustness checks

We conducted robustness checks by varying the identification strategy, variable measurements, and sample size to test the sensitivity of the main results. To ensure that the main results did not vary by the different criteria used for dividing the treatment and control groups under the reform, we did three additional checks. First, considering the variation in policy implementation across

[11] $e^{0.601} - 1 = 0.824$

[12] The impact of these barriers on migrant children's access to education is reflected in two ways as follows. First, increasing sponsorship fees is equivalent to raising the transaction price of public education, thus making public education unaffordable for migrant families. Second, many cities use a point system to determine the scope of the access to basic public services for the non-local *Hukou* population. Public resources, such as public education, often require more points, being beyond the reach of disadvantaged migrant groups and severely restricting migrant children' access to public education.

[13] In Table A6, we use robust standard errors clustered at the individual level and the results remain identical.

Table 3
Effects of *Hukou* reform on education expenditure.

VARIABLES	Total Education Expenditures	In-School Expenditures	Off-School Expenditures	Sponsorship Fees	In-School Expenditures Other Than Sponsorship Fees
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.601**	0.838***	−0.667**	−0.191	0.907***
	(0.306)	(0.318)	(0.328)	(0.130)	(0.315)
Household control variable	Y	Y	Y	Y	Y
Regional control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.197	0.143	0.127	0.039	0.137
Observations	3393	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. The values between parentheses are robust standard errors clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 4
Effects of *Hukou* reform on education expenditure over time.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×year12	−0.079	−0.079	−0.101	−0.101	−0.181	−0.181	−0.004	−0.004	0.199	0.199
	(0.300)	(0.306)	(0.303)	(0.312)	(0.300)	(0.309)	(0.072)	(0.073)	(0.324)	(0.320)
Treat×year16	0.648*	0.648*	0.705**	0.705*	0.731**	0.731**	−0.310*	−0.310*	−0.174	−0.174
	(0.332)	(0.365)	(0.343)	(0.371)	(0.337)	(0.365)	(0.159)	(0.159)	(0.368)	(0.382)
Treat×year18	0.463	0.463	0.924**	0.924**	0.973**	0.973**	−0.024	−0.024	−1.168**	−1.168**
	(0.347)	(0.367)	(0.384)	(0.407)	(0.380)	(0.403)	(0.112)	(0.113)	(0.461)	(0.472)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.197	0.197	0.143	0.143	0.131	0.131	0.042	0.042	0.137	0.137
Observations	3393	3393	3393	3393	3393	3393	3393	3393	3393	3393

Notes: All regressions control for individual, time, city fixed effects, as well as the household- and region-level control variables. All dependent variables are log-transformed. Rows 1–3 show the cross-term coefficients of the *Hukou* reform and dummy variables for 2012, 2016, and 2018, respectively. The values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level, while the values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. *** p < 0.01, ** p < 0.05, * p < 0.1).

different cities, we replaced the treat dummy in Eq. (1) with the differences in the *Hukou* registration index, which, as mentioned before, allow us to consider not only the policy itself but also the degree to which it has been put into practice. Panel A in Table A7 shows consistent results, suggesting that the *Hukou* reform led to higher total education expenditures that stemmed from in-school education expenditures other than sponsorship fees. Specifically, a 1% increase in the *Hukou* registration index difference led to a 1.573% increase in the total education expenditure. Second, the cities with a difference in the *Hukou* registration index greater than zero (i.e., cities that relaxed *Hukou* restrictions) were used as the treatment group, while the cities with a difference in the *Hukou* registration index below zero were used as the control group. Eq. (1) was re-estimated for the total education expenditure and the variables reflecting the structure of this expenditure. Panel B in Table A7 shows that the results are similar to those of the base regression. Third, using a population of 5 million as the threshold did not significantly change the results (Panel C in Table A7). In addition, we adopted an instrumental variable strategy, using the interaction term between the post indicator and treatment status, which draws on city size as the instrument for the interaction term between the post indicator and the difference in the *Hukou* registration index. The first-stage estimation showed that small cities (with a population below 3 million) were associated with a large difference in the *Hukou* registration index and the second stage estimation showed similar results compared to the baseline regressions (Tables A8–A9).

Since only two years (2012 and 2014) are available before the reform, we could not examine parallel trends. We thus included data

from the CFPS for 2010 to roughly plot the parallel trends regarding the total education expenditure.[14] Using 2014 as reference, Fig. A3 indicates that none of the estimated coefficients for the pre-reform years 2010 and 2012 showed significant differences, while for post-reform years 2016 and 2018, almost all coefficients are significant (with the exception of sponsorship fees). In addition, to ensure that the treatment and control groups are more likely to be comparable, we adopted the PSM-DID model to re-estimate Eq. (1) and found that the main results changed very little (Table A10).

Another concern arises from the fact that local governments can determine the schedule for implementing the policy, which may have led to reverse causality. We ruled out this possibility by two additional checks. First, we regressed the indicator for whether a city is treated in a specific year on three city-level in-school resources proxies, finding that these variables are not related to the implementation of the *Hukou* reform policy (Table A11). Second, using the 3 million urban population threshold as a cutoff, we restricted the sample to the years 2016 and 2018 and employed a rigorous regression discontinuity (RD) analysis,[15] finding that the main results remain robust (Table A12).

We also excluded certain observations. First, since families with multiple children may have complex incentives for education investment, we retained the oldest child from these families in the sample and replicated the primary analysis with the trimmed sample. As shown in Table A13, the results are fairly consistent with those of the baseline regression. Second, we excluded municipalities and first-tier cities (i.e., Beijing, Shanghai, Tianjin, Chongqing, Guangzhou, and Shenzhen), which are likely associated with stricter *Hukou* restrictions, and found that the main results remained robust (Table A14). Third, we excluded the sample living in treatment cities adjacent to the control cities,[16] and found that the main results did not vary significantly (Table A15).

There was the risk that data attrition and selection bias could also affect our main results. To examine whether data attrition could have biased our main estimation, we used an attrition indicator—which is equal to 1 if the household is excluded from the sample in the next period—on the *Hukou* reform. We found that the coefficient was insignificant (Table A16). Another concern was that the *Hukou* reform may have affected whether children migrated with their parents. We ruled out this possibility by showing that the *Hukou* reform is not associated with whether children are left behind (Table A17). We also showed that the *Hukou* reform was not linked to changes in family characteristics (Table A18). To test whether sample selection could have biased our estimation, we re-estimated Eq. (1) with the full city sample including all children (i.e., rural–urban migrant children, rural left-behind children, and children with city *Hukou*). The baseline regressions and event studies remained consistent (Tables A19–A20). Tables A21–A22 showed similar results when we used the full rural–urban migrant sample, regardless of the migration status of children.[17] Table A23 shows that among the rural–urban migrant sample, there was a positive impact of the *Hukou* reform on the total educational expenditure for those whose children migrated with them rather than for those whose children were left behind. This result additional validated our focus on migrant children. In addition, we conducted a placebo test, drawing on the urban sample. Since the *Hukou* reform focused more on migrants without local *Hukou*, the urban population with local *Hukou* was likely to be unaffected. As expected, Tables A24–A25 show that all coefficients were insignificant.

To verify that the estimates were not influenced by other policies or unobserved factors, we performed a placebo test using the random assignment method (Chetty, Looney, & Kroft, 2009). Subjects were randomly assigned to the treatment and control groups 1000 times. Fig. 1 shows that the scatter of all estimated coefficients is concentrated around 0, far from their true values, with most coefficients insignificant at the 10% level. This suggests that the reform's effect on the educational expenditures of migrant households is unlikely to be influenced by other unobserved factors, thus supporting the identification strategy.

Overall, the robustness tests confirmed our main findings. Therefore, there is sufficient evidence that the *Hukou* reform of 2014 increased the educational expenditure of migrant families.

4.3. Mechanism analysis

As mentioned in the introduction, the *Hukou* reform might have affected the educational expenditures of migrant households through three mechanisms—the substitution, flypaper, and income effects. Echoing the current literature, the income effect mainly refers to more educational investment arising from the remittance of migrant parents (Shi, 2022) and the flypaper effect is associated with potential increased voluntary educational spending on children who received the fee reductions because of the *Hukou* reform (Shi, 2012). In addition, while some studies highlight the substitution between educational investments and current spending on other childrearing commodities, this study mainly focuses on whether the private educational investment is substituted by the government (Messacar & Frenette, 2019). This sub-section discusses this role.

4.3.1. The role of substitution effects: Does the government invest more?

The relaxation of *Hukou* restrictions may have promoted more equal access to public education for migrant children. Consequently,

[14] We do not include 2010 data in the main analysis because: (i) there is no detailed information on the structure of education expenditures and (ii) some important variables for the mechanisms are unavailable.

[15] Under this scenario, we estimate the impacts of the *Hukou* reform in the situations in which cities are selected for treatment based on whether their population size exceeds 3 million.

[16] We were concerned that some migrants in large cities with more controls on *Hukou* (control cities) would further migrate to small adjacent cities that relax *Hukou* restrictions (treatment cities).

[17] Our original migrant sample only included families whose children migrated with them, but we then included those whose children were left behind in rural areas.

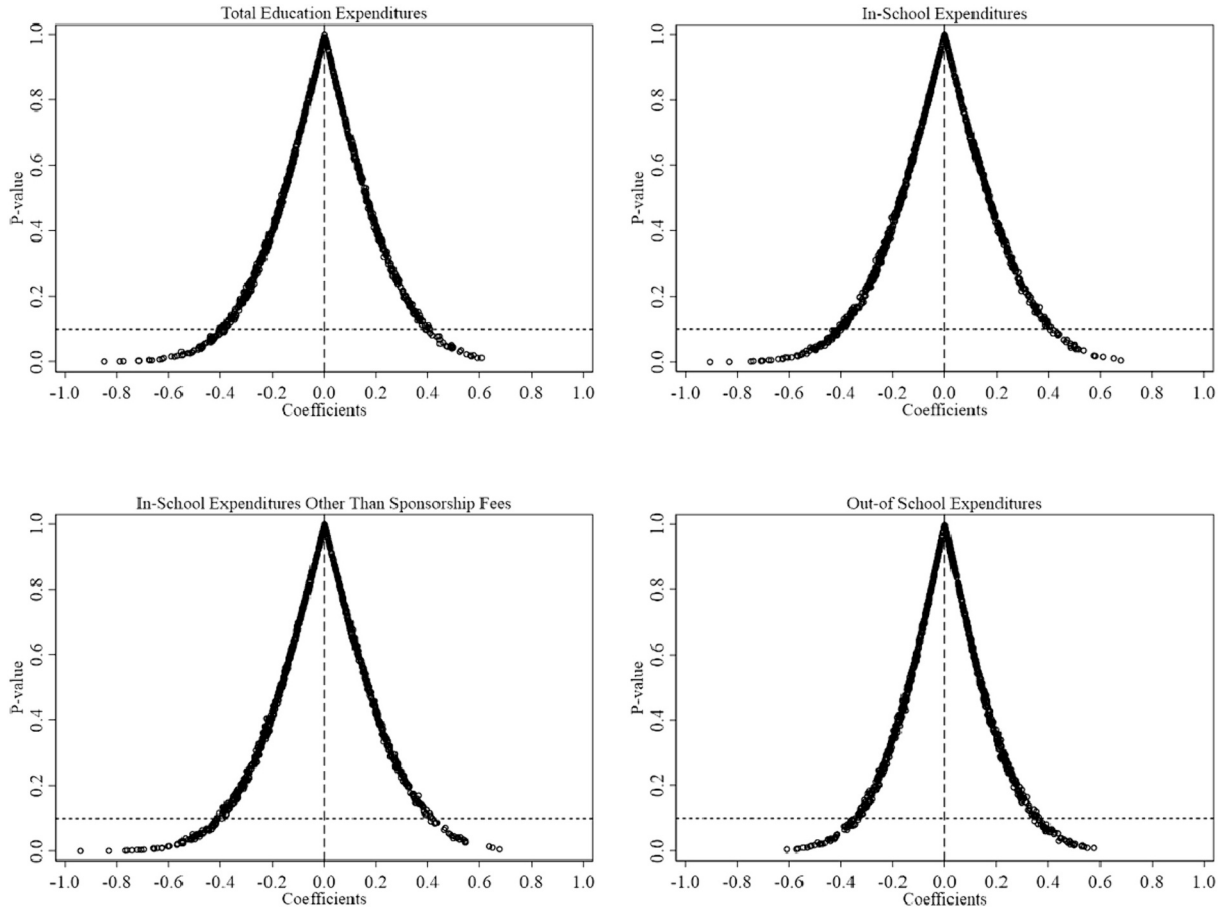

Fig. 1. Placebo test.
Notes: This figure is a scatter plot of the *p*-values of the difference-in-differences (DID) coefficients on the explanatory variables. The horizontal line indicates statistical significance at the 10% level.

the additional educational fees that migrant families paid for their children to enter public schools were saved and redirected to other purposes, resulting in a reduction in the total investment in education and in creating a substitution effect.

To test whether this was the case, we first identify how *Hukou* reform affected the probability of getting access to public schools. Column 1 of Table 5 provides the regression results for Eq. (1) using free access to public education as the dependent variable. The results show that the reform significantly increased the probability of receiving free public education by migrant children. Tables A26–A27 provide additional evidence that more migrant children were enrolled in public schools after the reform and that the reform significantly decreased the probability of enrolling then in migrant schools.

As migrant children were more likely to be enrolled in public schools after the *Hukou* reform, we further verified whether the reform is associated with higher government expenditure on child education. We collected macro-level information, namely per pupil financial spending on education for primary and secondary students and total government education expenditure as the dependent variables, given that city-level demographic, economic, and social development indicators were controlled for. Eq. (1) was re-estimated, and the results are shown in Table 6. Columns 1 and 2 show that the reform did not have a significant impact on the government's education expenditure per pupil or on the total education expenditure.

If we combine this with the previous micro-level analysis, although more migrant children have entered the public education system from migrant schools as a result of the reform, there was no corresponding increase in local government expenditure on education. This phenomenon is partly because the urban public education system has not reached the critical point for economies of scale; that is, it could still absorb more children from the non-local *Hukou* population without increasing overall investment. Another possible reason is that, when many non-local *Hukou* children, including rural migrants, enter the public education system, local governments experience a lag in education expenditure due to public financial resource constraints. Consequently, the de facto reduction in the government's per capita education expenditure may have incentivized the public education system to promote equal access to educational resources for the children of both the local and non-local *Hukou* populations. This sent a signal to the non- local *Hukou* population, including rural migrants, of the potential improved quality of their children's education, thus incentivizing them to invest more in education to compensate for the lack of government investment to maintain and improve the overall quality of their

Table 5
Results of mechanism analysis.

VARIABLES	Free Access to Public Education	Commuting Distance	Education Expectation	Scoring Expectation	Key Class	Parental Income
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×post	0.265***	0.236*	0.474***	1.550***	0.558***	0.674**
	(0.099)	(0.133)	(0.175)	(0.513)	(0.152)	(0.332)
Control variable	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y
SE	robust	robust	robust	robust	robust	robust
R-squared	0.398	0.104	0.030	0.027	0.112	0.315
Observations	3393	3393	3393	3393	3393	3393

Notes: Robust standard errors appear between parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 6
Effects of *Hukou* reform on macro variables.

VARIABLES	Per Pupil Financial Expenditure	Government Education Expenditure
	(1)	(2)
Treat×post	0.018	0.071
	(0.028)	(0.047)
Control variable	Y	Y
Time fixed effects	Y	Y
City fixed effects	Y	Y
SE	robust	robust
R-squared	0.821	0.687
Observations	657	657

Notes: All regressions control for city and year-fixed effects. The control variables used are number of students in primary and secondary schools, number of primary and secondary schools, number of employed staff and workers, industrial output value, per capita GDP, regional GDP growth rate, proportion of tertiary industry to GDP, government budget income and expenditure, total population, and size of the *Hukou* population. Robust standard errors appear between parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

children's education.

Nevertheless, local governments also created the phenomenon of rent-seeking in public education organizations, potentially leading to new invisible discrimination. When promoting top-level institutional changes, such as the *Hukou* reform, the central government usually introduces strategies to monitor local governments. In this context, although the decentralized system gave local governments some discretion in adjusting and implementing the reform, they still needed to follow the instructions of the central government. To provide equal access to public education for children of the non-local *Hukou* population, local governments were required to fulfill the quantitative targets specified by the central government.[18] However, most cities faced local imbalances in the supply and demand of public education resources. Owing to the strict constraints imposed by central government, local governments intentionally or unintentionally used various schemes in equalizing access to public education, including placing children of the non-local *Hukou* population in areas where public education resources were scarce. This spatial separation of education and residence also changed the structure of the education expenditure. Column 2 of Table 5 lists the regression results for Eq. (1) with commuting distance as the explanatory variable. The reform significantly increased the commuting distance to school for migrant children, suggesting that part of the increase in education expenditure was caused by the increased commuting costs. Therefore, migrant families had to overcome the invisible discrimination generated by the local government's entitlement scheme by increasing education expenditure. However, this did not improve migrant children's human capital, but wasted the limited resources of migrant families.

4.3.2. The flypaper effect: Do migrant parents have higher expectations for the education of their children?

While the substitution effect depends on whether the government invested more in public education following the reform for providing migrant children with free access to the public education system, the flypaper effect relies on the reform raising the educational expectations of migrant children. As the reform promoted the long-term stability of migrant families in cities, thus greatly reducing the likelihood of relocation, the higher educational returns increased their educational expectations (Mussa, 2013). As such, the savings from the reform were redirected to education to improve its quality, thus creating a flypaper effect.

Columns 3 and 4 of Table 5 show that the reform raised migrant children's educational expectations, in terms of both long-

[18] Since 2014, the central government has assigned annual urbanization targets to provincial and urban areas, including targets for basic public services, such as education, to be expanded to the non-local *Hukou* population.

(expectations of children's educational attainment) and short-term outcomes (expectations of children's grades). We also found heterogeneity along the dimension of parental education. Tables A28–A29 show that less educated parents were more likely to have higher expectations and, thus, invest more in the education of their children. This is consistent with the studies showing that those who are less educated tend to invest more in their children (Iddrisu, Danquah, Quartey, & Ohemeng, 2018; Kenayathulla & Banu, 2016). Furthermore, the reform may have changed the pattern of educational resource allocation based on gender among migrant households (Almond, Li, & Zhang, 2019). Since male productivity is generally higher than female productivity and men have a greater responsibility toward their families in East Asian countries, traditional rural families allocate more educational resources to boys (Aslam & Kingdon, 2008; Azam & Kingdon, 2013; Vogel & Korinek, 2012). Therefore, we expect that the reform's effects will be stronger among boys. However, as shown in Tables A30–A31, by adding a gender dummy variable, no significant differences were found for total education expenditure and expectations. Therefore, for traditional rural households that migrated into cities, the *Hukou* reform was conducive to raising the educational expectations for girls in migrant families and reducing gender discrimination in the intra-household allocation of educational resources.

There are two ways to improve education quality for migrant children. One is to improve performance through off-school training to compensate for poor quality in-school education. The other is to rely on schools to improve quality through allocating better-quality teachers, creating a good learning environment, and supplying effective learning materials. Either way, migrant families need to consider the signals of quality education when allocating education expenditure, as the prospect of a high-quality education enhances parents' perceived returns and stimulates the corresponding investment and behavior (Cattaneo, 2012; Mussa, 2013). Before the reform, migrant children were more likely to rely on off-school training to bridge the gap between private and public schools in terms of education quality because their access to the public education system was limited. When the reform facilitated their access to public education, the function of off-school training to bridge the quality gap vanished immediately. However, if migrant families wished to continue off-school training to individualize and improve the education quality, they needed to either pay higher fees or find alternative solutions. The low financial and human capital levels of migrant families make it harder for them to meet these requirements, thus disqualifying off-school training in expenditure allocation decisions. On the other hand, the reform required the elimination of discrimination in public education against the children of the non-local *Hukou* population, including the removing the segregation of teaching spaces and the allocation of teachers and teaching resources within schools (Cowley, 1999). Although some new types of discrimination remained, the reform sent signals of improving the quality of public education for the children of the non-local *Hukou* population, prompting migrant families to allocate more educational expenditure to in-school learning.

To verify this mechanism, we analyzed whether migrant children were enrolled in a key class to signal improved school education quality. This is because enrollment in a key class implies that students have access to the best educational resources of the school and the quality of education is enhanced. Column 5 of Table 5 shows the estimation results based on Eq. (1). The reform significantly boosted the probability of attending key classes for migrant children, signaling that the quality of school education improved and, thus, the increase in total education expenditure due to the reform concentrated on in-school expenditure. We also verified the above mechanism for the time dimension. Table A32 shows the change over time in the reform's impact on migrant children's attendance of key classes. The effect was stronger in 2018 than in 2016. Combined with the previous findings regarding changes in the structure of education expenditure, we found that migrant households tended to increase their in-school expenditures other than sponsorship fees at the beginning of the reform, with no change in off-school expenditures. As the signal of improving the quality of school education became stronger with the advancement of the reform, migrant families significantly reduced their off-school expenditure and allocated them to in-school expenditure instead. Additionally, Table A33 shows that parents tended to urge their children to spend more time studying rather than engaging in recreational activities such as watching TV.

4.3.3. The role of income effects: Do migrant parents earn more?

The above analysis suggests that, with the progress of the *Hukou* reform, the government did not invest more in public education; rather, it attempted to signal higher educational quality in public schools, which triggered increased private investment in the education of migrant children. The premise is that the reform has substantially increased the income of migrant families.

To test the effect on income, we re-estimated Eq. (1) using parental income as the dependent variable. Column 6 of Table 5 shows a strong positive association between the reform and higher parental income. This is consistent with Song (2014), who found that the *Hukou* reform led to higher incomes for migrants through reduced migration costs and higher human capital levels, thus allowing more resources to be allocated to the education of their children (i.e., income effect). One concern regarding this explanation is that migrant workers invest more in their children not because they earn more but because the expenditures on other items (e.g., health) potentially decreased. However, the results rule out this concern, showing that the *Hukou* reform did not have a significant effect on health expenditure (see Table A34).

Our results also show the reform had a significant positive impact of on the total education expenditure, indicating that the income and flypaper effects outweigh the possible substitution effect. This finding is in line with the literature on the institutional determinants of education (Chi & Qian, 2016; Das, 2021; Shi, 2012). The reform significantly increased the probability of receiving free public education by migrant children, thus satisfying their demand for quantity. While no increase in the government expenditure on education following the reform was identified, rational migrant households with increased educational expectations, tended to invest the education savings from the reform along with the additional increased income in what they perceived as high-quality education, thus increasing their total investment in education.

5. Conclusions

Over the past few decades, many countries undergoing economic transition and rapid urbanization have developed systems to promote the social welfare of migrants, achieving impressive results in terms of human capital development and poverty reduction. Further, in most countries undergoing urbanization-driven economic transition, the welfare distribution determines the livelihoods and incomes of low-skilled immigrant groups. A fair system of welfare distribution can improve their livelihoods and increase the investment in the human capital of their offspring, thus ensuring both the full integration of immigrant families into society and providing a continuous labor supply. As one of the most successful cases among developing countries, China's *Hukou* reform has greatly facilitated equitable access to social benefits for its domestic migrant groups, which make up a significant proportion of the population, while also increasing labor productivity and the incomes of migrant families. Nevertheless, few studies have investigated how this systemic change affected the long-term education-based human capital development for specific target populations.

To fill this gap in the literature, this study investigated the widely overlooked positive effects of the welfare reform on long-term human capital accumulation among migrant families and identified its effects on their investment in education. Specifically, we evaluated the effects of the 2014 *Hukou* reform on the investment in education of migrant families. Our results showed that the reform significantly increased migrant households' total investment in education, with the flypaper and income effects outweighing the substitution effect. First, the reform expanded migrant children's access to public education. However, local governments have not increased public education expenditure, although the public education system has absorbed a large number of children from the non-local *Hukou* population. Therefore, the increase in migrant families' investment in education prompted by the reform is likely to be a stopgap measure caused by the public education system failing to compensate for the lack of government investment, thus increasing the risk of rent-seeking. Policymakers should be aware that both aspects could trigger new invisible discrimination within the public education system. For instance, the reform raised in-school expenditure by increasing the commuting distance to school for migrant children. This is because local governments face local imbalances in the supply and demand of public education and, thus, assign migrant children to distant school districts, resulting in a higher expenditure on education for migrant families.

Second, there were changes in the expenditure structure because the reform made it easier for migrant children to attend key classes, further eliminating resource discrimination within the public education system and providing improved education quality to migrant families. Consequently, migrant families were encouraged to redirect the savings from sponsorship fees and off-school expenditure, which were used before the reform to compensate for the lack of education quality. Third, additional investment in education can result from increased income, encouraging the personal investment in improved education.

Our findings thus contribute to the literature by verifying the assumptions of family economic theory and the standard economic model for optimizing investment in children's education. Our study also has strong practical implications for the development of human capital in migrant children. Specifically, we show that policymakers must balance providing educational benefits to migrant children with maintaining economic growth.

Author statement

All remaining errors and omissions are our own.

Declaration of competing interest

None.

Data availability

The main data from China Family Panel Study (CFPS) are available upon request at http://www.isss.pku.edu.cn/cfps/en/index.htm.

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Appendix A. Appendix

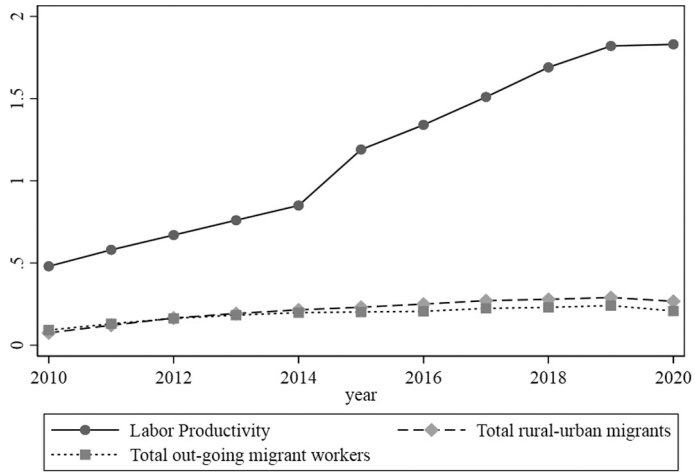

Fig. A1. Growth rate of labor productivity and the number of migrant workers in China, 2010–2020.
Notes: The graph plots the total number of migrant workers and out-going migrant workers in China's secondary and tertiary industries from 2010 to 2020. Labor productivity in 2000 and total number of migrant workers and out-going migrant workers in 2008 are taken as the reference and normalized to 1. Data are obtained from China Statistical Yearbook and Migrant Workers Monitoring Survey Report.

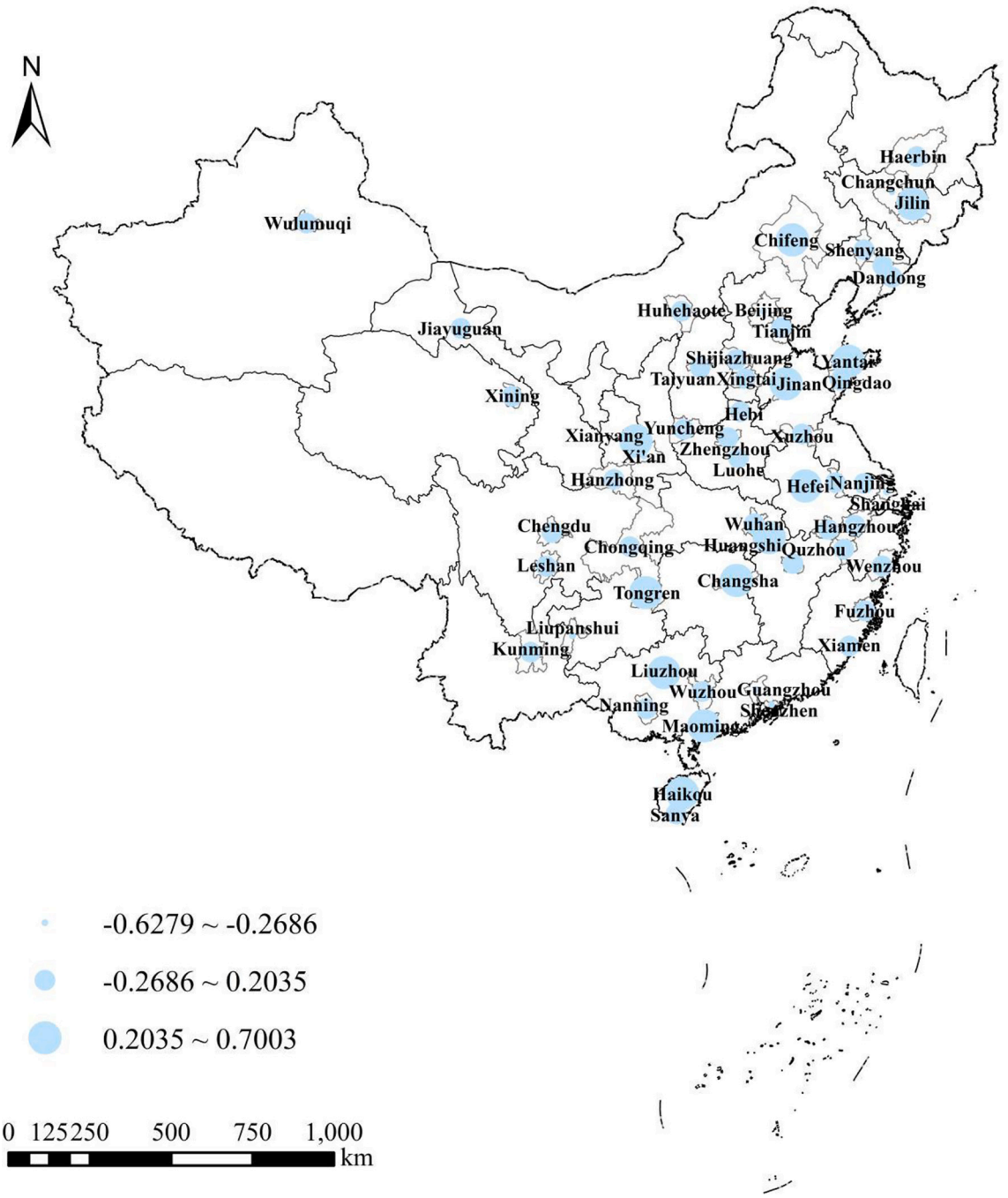

Fig. A2. Difference in the ordinary employment index for selected Chinese cities before and after 2014.
Notes: The index difference is defined as the pre-2014 index minus the post-2014 index. The size of the dots shows the size of the index difference, that is, the larger the dots, the larger the index difference, and vice versa. The data used is the ordinary employment index.

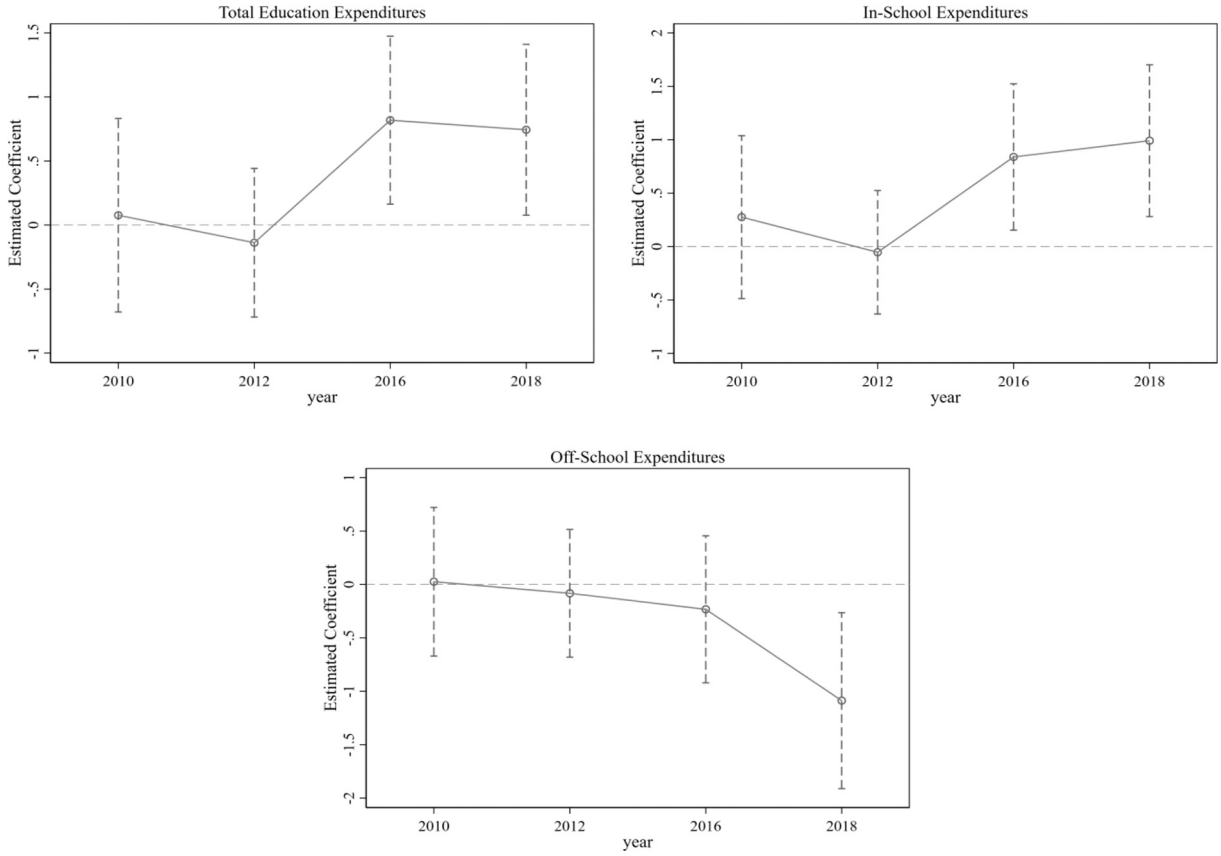

Fig. A3. Parallel trend test plot.
Notes: The graphs show the parallel trend scatter plots for each explanatory variable. Regression analysis includes individual, year, and city fixed effects, as well as control variables.

Table A1
Schedule of implementation of *Hukou* reform documents for each province.

Province	Time	Province	Time
Xinjiang	2014.09.30	Guizhou	2015.05.04
Heilongjiang	2014.11.01	Anhui	2015.05.08
Henan	2014.11.04	Hunan	2015.05.11
Gansu	2014.11.10	Yunnan	2015.05.29
Shandong	2014.11.19	Guangdong	2015.06.24
Hebei	2014.11.20	Liaoning	2015.07.10
Sichuan	2014.11.22	Chongqing	2015.08.25
Jiangxi	2014.12.21	Hubei	2015.09.06
Jiangsu	2014.12.29	Inner Mongolia	2015.09.08
Shanxi	2015.01.14	Zhejiang	2015.12.10
Jilin	2015.01.21	Hainan	2015.12.24
Qinghai	2015.01.27	Shanghai	2016.04.15
Fujian	2015.02.11	Tianjin	2016.04.20
Guangxi	2015.02.25	Tibet	2016.06.01
Shaanxi	2015.03.19	Beijing	2016.09.08
Ningxia	2015.03.20		

Notes: This table lists the implementation time of the *Hukou* reform for each province in China.

Table A2
Summary statistics.

Variables	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A: Treatment Cities	Full Sample		Before		After	
Total education expenditures,log	5.797	2.960	5.087	3.247	6.544	2.410
In-school expenditures,log	5.439	2.906	4.859	3.160	6.048	2.472

(*continued on next page*)

Table A2 (*continued*)

Variables	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A: Treatment Cities	Full Sample		Before		After	
In-school expenditures other than sponsorship fees,log	5.419	2.907	4.852	3.155	6.016	2.487
Sponsorship fees,log	0.082	0.719	0.045	0.552	0.122	0.859
Off-school expenditures,log	1.512	3.046	1.200	2.709	1.840	3.334
Household-level control variables						
Parental education level	3.093	1.034	3.088	1.041	3.097	1.027
Parental age	35.995	6.036	34.199	5.818	37.884	5.676
Age of children	8.120	3.397	6.362	2.988	9.970	2.761
Family size	5.423	2.050	5.452	2.071	5.394	2.029
Parental health status	2.770	0.878	2.745	0.886	2.796	0.870
Parental occupation	–	–	–	–	–	–
Number of children in school	1.122	0.771	0.886	0.736	1.370	0.728
Regional-level control variables						
Marketization index	6.964	1.676	6.647	1.603	7.298	1.687
Per Capita GDP,log	10.851	0.537	10.775	0.556	10.930	0.504
Number of students in primary and secondary schools	20.603	33.325	17.891	15.865	23.454	44.707
Number of primary and secondary schools	221.891	138.943	218.575	136.327	225.379	141.609
Public budget revenue,log	13.699	0.839	13.493	0.807	13.916	0.817
Public budget expenditure,log	13.087	1.088	12.912	1.060	13.270	1.088
Per capita income,log	10.217	0.264	10.081	0.234	10.359	0.214
Per capita fiscal expenditure for primary and secondary schools,log	9.163	0.360	8.942	0.297	9.396	0.261
Number of observations		2784		1427		1357
Panel B: Control Cities	Full Sample Before		Before		After	
Total education expenditures,log	6.673	2.760	6.259	2.836	7.064	2.631
In-school expenditures,log	6.078	2.743	5.924	2.751	6.223	2.732
In-school expenditures other than sponsorship fees,log	5.995	2.768	5.887	2.735	6.097	2.800
Sponsorship fees,log	0.181	1.150	0.103	0.895	0.255	1.345
Off-school expenditures,log	2.565	3.775	2.040	3.390	3.061	4.049
Household-level control variables						
Parental education level	3.484	1.024	3.419	1.009	3.546	1.037
Parental age	36.467	5.738	35.037	5.759	37.819	5.388
Age of children	8.745	3.411	6.976	3.091	10.419	2.803
Family size	4.663	1.467	4.676	1.427	4.652	1.505
Parental health status	2.821	0.770	2.793	0.767	2.847	0.774
Parental occupation	–	–	–	–	–	–
Number of children in school	1.020	0.617	0.834	0.661	1.195	0.516
Regional-level control variables						
Marketization index	7.980	1.616	7.512	1.512	8.423	1.588
Per Capita GDP,log	11.365	0.391	11.333	0.439	11.394	0.338
Number of students in primary and secondary schools	85.425	53.199	84.499	45.293	86.301	59.781
Number of primary and secondary schools	946.458	608.148	955.527	534.583	937.882	671.090
Public budget revenue,log	16.000	1.245	15.958	1.122	16.041	1.352
Public budget expenditure,log	15.745	1.362	15.746	1.223	15.744	1.483
Per capita income,log	10.498	0.376	10.361	0.360	10.628	0.344
Per capita fiscal expenditure for primary and secondary schools,log	9.562	0.563	9.368	0.581	9.746	0.478
Number of observations		609		296		313

Notes: This table reports summary statistics for the dependent variables and principal control variables. Treatment and control cities are cities with an urban population of <3 million or more, respectively. The parental educational level is an ordinal variable that takes the following values: (1 = illiterate, 2 = elementary school, 3 = junior high school, 4 = high school, 5 = college, 6 = bachelor's degree, 7 = master's degree). The parental health condition is an ordinal variable that takes the following values: (1 = very healthy, 2 = very healthy, 3 = moderately healthy, 4 = fair, 5 = not healthy).

Table A3
Association between city size and *Hukou* registration index.

Variables	Investment	Home Purchase	High-End Employment	Ordinary Employment	Composite
	(1)	(2)	(3)	(4)	(5)
Panel A: population < 3 million, treatment group; population ≥3 million, control group					
treat×post	0.019	0.030	0.112	−0.208***	−0.111**
	(0.035)	(0.025)	(0.081)	(0.067)	(0.052)
City fixed effect	Y	Y	Y	Y	Y
Time fixed effect	Y	Y	Y	Y	Y
SE	robust	robust	robust	robust	robust
R-squared	0.135	0.032	0.413	0.184	0.033
Observations	240	240	240	240	240
Panel B: population < 5 million, treatment group; population ≥5 million, control group					

(*continued on next page*)

Table A3 (*continued*)

Variables	Investment	Home Purchase	High-End Employment	Ordinary Employment	Composite
	(1)	(2)	(3)	(4)	(5)
treat×post	−0.009	0.014	−0.111	−0.431***	−0.236***
	(0.034)	(0.027)	(0.111)	(0.118)	(0.080)
City fixed effect	Y	Y	Y	Y	Y
Time fixed effect	Y	Y	Y	Y	Y
SE	robust	robust	robust	robust	robust
R-squared	0.133	0.026	0.405	0.268	0.058
Observations	240	240	240	240	240

Notes: Cities with an urban population of less than or above 3 million or 5 million are set as the treatment/control group in this table. 2000–2013 is defined as the pre-reform period, and 2014–2016 is defined as the post-reform period. All regression analyses included city - and year fixed effects. Values between parentheses are robust standard errors clustered at the individual level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A4
Regional differences in *Hukou* Registration Index.

	Number of cities	Mean	Median	S.D.	Min	Max
>10 million	3	−0.585	−0.587	0.010	−0.594	−0.575
5–10 million	4	−0.167	−0.122	0.344	−0.628	0.204
3–5 million	12	−0.005	−0.021	0.172	−0.295	0.243
1–3 million	32	0.098	0.085	0.182	−0.224	0.518
0.5–1 million	33	0.152	0.109	0.217	−0.413	0.700
<0.5 million	36	0.082	0.103	0.187	−0.403	0.492
Total	120	0.072	0.094	0.230	−0.628	0.700

Notes: This table lists statistics of the ordinary employment index differences by city size.

Table A5
Descriptive statistics for floating population.

Year	>5 million		3–5 million		<3 million	
	Floating Population	Growth Rate	Floating Population	Growth Rate	Floating Population	Growth Rate
2012	449.292		182.864		18.246	
2013	460.683	0.025	199.877	0.093	17.702	−0.030
2014	467.860	0.016	213.797	0.070	19.949	**0.127**
2015	479.960	0.026	233.853	0.094	22.549	**0.130**
2016	486.670	0.014	247.863	0.060	22.730	0.008
2017	489.085	0.005	251.422	0.014	23.835	**0.049**
2018	478.997	−0.021	253.078	0.007	25.703	**0.078**

Notes: This table lists statistics of the floating population by city size and year.

Table A6
Effects of *Hukou* reform on education expenditure.

Variables	Total Education Expenditures	In-School Expenditures	Off-School Expenditures	Sponsorship Fees	In-School Expenditures Other Than Sponsorship Fees
	(1)	(2)	(5)	(4)	(3)
Treat×post	0.601**	0.838***	−0.667**	−0.191	0.907***
	(0.285)	(0.300)	(0.321)	(0.130)	(0.298)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	robust	robust	robust	robust	robust
R-squared	0.197	0.143	0.127	0.039	0.137
Observations	3393	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Values between parentheses are robust standard errors clustered at the individual level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A7
Robustness tests: changing the division of the treatment and control groups.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

(*continued on next page*)

Table A7 (*continued*)

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: *Hukou* registration index										
dindex(log) × post	1.573***	1.573***	1.923***	1.923***	1.895***	1.895***	−0.001	−0.001	−0.960**	−0.960**
	(0.390)	(0.415)	(0.400)	(0.408)	(0.400)	(0.408)	(0.123)	(0.123)	(0.377)	(0.403)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.204	0.204	0.152	0.152	0.145	0.145	0.037	0.037	0.128	0.128
Observations	3393	3393	3393	3393	3393	3393	3393	3393	3393	3393
Panel B: *Hukou* registration index difference > 0, treatment group; *Hukou* registration index difference < 0, control group										
treat×post	0.601***	0.601**	0.750***	0.750***	0.731***	0.731***	−0.008	−0.008	−0.861***	−0.861***
	(0.230)	(0.242)	(0.236)	(0.247)	(0.237)	(0.248)	(0.068)	(0.070)	(0.200)	(0.207)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.199	0.199	0.145	0.145	0.138	0.138	0.037	0.037	0.133	0.133
Observations	3393	3393	3393	3393	3393	3393	3393	3393	3393	3393
Panel C: population < 5 million, treatment group; population ≥5 million, control group										
treat×post	1.461***	1.461***	1.859***	1.859***	1.795***	1.795***	0.106	0.106	−0.485	−0.485
	(0.372)	(0.407)	(0.395)	(0.424)	(0.396)	(0.425)	(0.107)	(0.107)	(0.377)	(0.392)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.203	0.203	0.150	0.150	0.143	0.143	0.037	0.037	0.125	0.125
Observations	3393	3393	3393	3393	3393	3393	3393	3393	3393	3393

Notes: All regressions control for individual, time, and city fixed effects, as well as household- and region-level control variables. All dependent variables are log-transformed. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level. Values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A8
IV: First-stage.

VARIABLES	dindex(log) × post
	(1)
Treat×post	0.471***
	(0.108)
Control variable	Y
Individual fixed effects	Y
Time fixed effects	Y
City fixed effects	Y
SE	cluster
R-squared	0.410
Observations	3393

Notes: This table shows the first-stage of 2sls, using the interaction term between the treatment status (whether the city size is smaller than 3 million) and post dummy as the instrument for the interaction term between the changes in *Hukou* index and post dummy. All regressions control for individual, year, and city fixed effects. Values between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A9
IV: Second stage.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
dindex(log) × post	1.509*	1.805**	1.964**	−0.426	−1.613*
	(0.860)	(0.809)	(0.821)	(0.482)	(0.911)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.200	0.148	0.144	0.020	0.125
Observations	3393	3393	3393	3393	3393

Notes: This table shows the second stage of 2sls, using the interaction term between the treatment status (whether the city size is smaller than 3 million) post dummy as the instrument for the interaction term between the changes in *Hukou* index and post dummy. All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Values between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A10
Robustness test: PSM-DID estimate of the effects of *Hukou* reform on education expenditure.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.558**	0.814***	0.870***	−0.119	−0.723**
	(0.276)	(0.280)	(0.278)	(0.102)	(0.295)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.172	0.117	0.114	0.028	0.096
Observations	3138	3138	3138	3138	3138

Notes: This tables is the estimated results of psm-did using nearest neighbor matching. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. All dependent variables are log-transformed. Values between parentheses are robust standard errors. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A11
Correlation between in-school resources and the schedule for *Hukou* reform.

VARIABLES	Treat×post
	(1)
Number of students in primary and secondary schools	−0.000
	(0.001)
Number of primary and secondary schools	0.227
	(0.201)
Per capita fiscal expenditure	0.000
	(0.000)
Control variable	Y
Individual fixed effects	Y
Time fixed effects	Y
City fixed effects	Y
SE	robust
R-squared	0.060
Observations	3393

Notes: This table shows the correlation between in-school resources and the schedule for *Hukou* reform, The dependent variable is Treat×post, which indicates whether the city is treated in a specific year, and independent variables include those represent the educational resources of each city. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. Values between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A12
Robustness test: RD estimate of the effects of *Hukou* reform on education expenditure.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Bandwidth: 1 million					
LATE	1.323*	2.149***	2.086***	0.789	−3.004**
	(0.732)	(0.629)	(0.644)	(3.359)	(1.511)
Control variable	Y	Y	Y	Y	Y
Observations	1670	1670	1670	1670	1670

Notes: This table using the 3 million urban population threshold as a cutoff to employ a rigorous Regression Discontinuity (RD) analysis with a bandwidth of 1 million and control for covariates at household- and region-levels. All dependent variables are log-transformed. Values between parentheses are robust standard errors. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A13
Robustness test: including one child for each family.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×post	0.635**	0.635**	0.872***	0.872***	0.949***	0.949***	−0.177	−0.177	−0.760**	−0.760**
	(0.296)	(0.296)	(0.316)	(0.316)	(0.313)	(0.313)	(0.143)	(0.143)	(0.349)	(0.349)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.232	0.232	0.164	0.164	0.159	0.159	0.034	0.034	0.155	0.155
Observations	2672	2672	2672	2672	2672	2672	2672	2672	2672	2672

Notes: We include only one child for all families. For those with multiple children, we retain the oldest child. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. All dependent variables are log-transformed. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A14
Robustness tests: excluding special cities.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Delete municipalities										
Treat×post	0.689*	0.689*	0.645*	0.645*	0.757**	0.757**	−0.218	−0.218	−0.331	−0.331
	(0.359)	(0.393)	(0.359)	(0.390)	(0.355)	(0.385)	(0.167)	(0.167)	(0.368)	(0.389)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.213	0.213	0.149	0.149	0.143	0.143	0.037	0.037	0.124	0.124
Observations	3205	3205	3205	3205	3205	3205	3205	3205	3205	3205
Panel B: Delete first-tier cities										
Treat×post	0.634*	0.634*	0.628*	0.628*	0.777**	0.777**	−0.274	−0.274	−0.511	−0.511
	(0.340)	(0.363)	(0.346)	(0.380)	(0.344)	(0.376)	(0.181)	(0.181)	(0.390)	(0.398)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.232	0.232	0.172	0.172	0.166	0.166	0.041	0.041	0.125	0.125
Observations	3175	3175	3175	3175	3175	3175	3175	3175	3175	3175

Notes: We excluded municipalities and first-tier cities. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. All dependent variables are log-transformed. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level. Values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A15
Robustness tests: excluding the treatment cities adjacent to the control group.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×post	0.517*	0.517*	0.799***	0.799**	0.868***	0.868***	−0.186	−0.186	−0.762**	−0.762**
	(0.289)	(0.310)	(0.303)	(0.321)	(0.300)	(0.318)	(0.132)	(0.133)	(0.323)	(0.330)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.187	0.187	0.140	0.140	0.133	0.133	0.043	0.043	0.126	0.126
Observations	3080	3080	3080	3080	3080	3080	3080	3080	3080	3080

Notes: We exclude the sample living in cities adjacent to the control cities. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. All dependent variables are log-transformed. Values between columns 1, 3, 5, 7, and 9 in parentheses are robust standard errors clustered at the individual level. Values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A16
Robustness test: data attrition.

VARIABLES	Samples
	(1)
Treat×post	−0.048
	(0.036)
Control variable	Y
Individual fixed effects	Y
Time fixed effects	Y
City fixed effects	Y
SE	robust
R-squared	0.177
Observations	3393

Notes: The dependent variable is an indicator equal to 1 if the household drops from the panel sample in the next period. All regressions control for individual, year, and city fixed effects. Robust standard errors appear in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A17
Robustness test: effects of *Hukou* reform on whether children migrate.

VARIABLES	Full sample	Rural Migrant Sample
	(1)	(2)
Treat×post	−0.011	0.169
	(0.090)	(0.151)
Control variable	Y	Y
Individual fixed effects	Y	Y
Time fixed effects	Y	Y
City fixed effects	Y	Y
SE	robust	robust
R-squared	0.087	0.128
Observations	6713	4559

Notes: The dependent variables are indicators for whether children migrate with their parents. All regressions control for individual, year, and city fixed effects. Columns 1is full sample, and columns 2 is rural migrant sample. Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A18
Effects of *Hukou* reform on family characteristics.

VARIABLES	Age	Parental education	Parental age	Family size	Parental health	Parental occupation	Number of school-aged children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×post	−0.056	0.003	0.147	0.092	−0.009	−1235.018	0.089

(*continued on next page*)

Table A18 (*continued*)

VARIABLES	Age	Parental education	Parental age	Family size	Parental health	Parental occupation	Number of school-aged children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(0.037)	(0.012)	(0.145)	(0.092)	(0.068)	(1921.174)	(0.062)
Control variable	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster	cluster	cluster
R-squared	0.987	0.211	0.898	0.105	0.076	0.218	0.397
Observations	3393	3393	3393	3393	3393	3393	3393

Notes: The dependent variables are family characteristics control variables. All regressions control for individual, year, and city fixed effects. Values between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A19

Robustness test: effects of *Hukou* reform on full sample.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.413**	0.387**	0.415**	−0.105	−0.507**
	(0.188)	(0.196)	(0.196)	(0.071)	(0.212)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.194	0.136	0.131	0.012	0.125
Observations	6713	6713	6713	6713	6713

Notes: We used the full children sample in this table. All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Values between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A20

Robustness test: time effects of *Hukou* reform on full sample.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×year12	0.056	0.056	0.025	0.025	0.007	0.007	0.009	0.009	0.283	0.283
	(0.208)	(0.215)	(0.212)	(0.221)	(0.210)	(0.219)	(0.061)	(0.062)	(0.251)	(0.253)
Treat×year16	0.511**	0.511**	0.372*	0.372*	0.399*	0.399*	−0.125	−0.125	−0.007	−0.007
	(0.201)	(0.212)	(0.209)	(0.220)	(0.209)	(0.220)	(0.091)	(0.094)	(0.231)	(0.235)
Treat×year18	0.328	0.328	0.434*	0.434*	0.445*	0.445*	−0.150	−0.150	−0.705**	−0.705**
	(0.229)	(0.242)	(0.247)	(0.262)	(0.244)	(0.258)	(0.110)	(0.118)	(0.302)	(0.312)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.194	0.194	0.136	0.136	0.131	0.131	0.057	0.057	0.127	0.127
Observations	6713	6713	6713	6713	6713	6713	6713	6713	6713	6713

Notes: We used the full children sample in this table. All regressions control for individual, time, city fixed effects, as well as the household- and region-levels' control variables. All dependent variables are log-transformed. Rows 1–3 show the cross-term coefficients of the *Hukou* reform and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A21

Robustness test: effects of *Hukou* reform on both rural-urban migrant children and rural left-behind children.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.539**	0.636**	0.657**	−0.154	−0.515*

(*continued on next page*)

Table A21 (*continued*)

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
	(0.269)	(0.276)	(0.276)	(0.102)	(0.264)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.212	0.158	0.153	0.036	0.117
Observations	4559	4559	4559	4559	4559

Notes: The sample includes both migrant children and left-behind children. All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Values between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A22
Robustness test: time effects of *Hukou* reform on both rural-urban migrant children and rural left-behind children.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×year12	−0.032	−0.032	−0.102	−0.102	−0.138	−0.138	0.001	0.001	0.446	0.446
	(0.272)	(0.280)	(0.277)	(0.288)	(0.274)	(0.285)	(0.066)	(0.066)	(0.292)	(0.292)
Treat×year16	0.610**	0.610*	0.522*	0.522*	0.523*	0.523*	−0.233**	−0.233**	−0.018	−0.018
	(0.287)	(0.312)	(0.292)	(0.314)	(0.289)	(0.311)	(0.117)	(0.117)	(0.302)	(0.307)
Treat×year18	0.404	0.404	0.701**	0.701**	0.717**	0.717**	−0.037	−0.037	−0.723*	−0.723*
	(0.303)	(0.322)	(0.329)	(0.351)	(0.326)	(0.349)	(0.099)	(0.100)	(0.385)	(0.400)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.212	0.212	0.158	0.158	0.153	0.153	0.037	0.037	0.128	0.128
Observations	4559	4559	4559	4559	4559	4559	4559	4559	4559	4559

Notes: The sample includes both migrant children and left-behind children. All regressions control for individual, time, city fixed effects, as well as the household- and region-levels' control variables. All dependent variables are log-transformed. Rows 1–3 show the cross-term coefficients of the *Hukou* reform and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A23
Heterogeneity of whether rural left-behind children.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.299	0.498*	0.544*	−0.159	−0.454*
	(0.273)	(0.281)	(0.281)	(0.103)	(0.267)
Treat×post×lb	−0.357*	−0.465*	−0.392*	0.035	−0.358
	(0.208)	(0.242)	(0.235)	(0.073)	(0.221)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.193	0.141	0.138	0.036	0.118
Observations	4559	4559	4559	4559	4559

Notes: The lb. variable represents whether children are left-behind children. All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A24
Robustness test: effects of *Hukou* reform on urban sample.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Treat×post	0.094	0.019	0.069	−0.095	−0.026
	(0.292)	(0.331)	(0.328)	(0.195)	(0.423)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.173	0.132	0.131	0.124	0.156
Observations	2154	2154	2154	2154	2154

Notes: The sample refers to children with urban *Hukou*. All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Values between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A25
Robustness test: time effects of *Hukou* reform on urban sample.

VARIABLES	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	Total Education Expenditures		In-School Expenditures		In-School Expenditures Other Than Sponsorship Fees		Sponsorship Fees		Off-School Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×year12	0.043	0.043	0.351	0.351	0.347	0.347	−0.026	−0.026	−0.628	−0.628
	(0.359)	(0.312)	(0.383)	(0.375)	(0.382)	(0.370)	(0.137)	(0.137)	(0.531)	(0.479)
Treat×year16	0.182	0.182	0.261	0.261	0.358	0.358	−0.074	−0.074	−0.005	−0.005
	(0.318)	(0.372)	(0.354)	(0.412)	(0.353)	(0.418)	(0.179)	(0.181)	(0.466)	(0.556)
Treat×year18	0.004	0.004	−0.040	−0.040	−0.075	−0.075	−0.157	−0.157	−0.698	−0.698
	(0.378)	(0.461)	(0.433)	(0.522)	(0.427)	(0.510)	(0.221)	(0.180)	(0.602)	(0.632)
Control variable	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster	robust	cluster	robust	cluster
R-squared	0.173	0.173	0.133	0.133	0.132	0.132	0.124	0.124	0.159	0.159
Observations	2154	2154	2154	2154	2154	2154	2154	2154	2154	2154

Notes: The sample refers to children with urban *Hukou*. All regressions control for individual, time, city fixed effects, as well as the household- and region-levels' control variables. All dependent variables are log-transformed. Rows 1–3 show the cross-term coefficients of the *Hukou* reform and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 between parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A26
Annual descriptive statistics of school types for migrant children.

School Types	Public school	Migrant school	International school	Other schools	Total
2012	294	492	0	1	787
2014	417	515	2	2	936
2016	820	37	3	35	895
2018	707	14	4	50	775
Total	2238	1058	9	88	3393

Notes: This table statistics the school types of migrant children by year.

Table A27
Effects of *Hukou* reform on school type.

VARIABLES	Migrant school	Migrant school	International school	International school	Other schools	Other schools
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×post	−0.089***	−0.089***	−0.000	−0.000	−0.001	−0.001
	(0.022)	(0.030)	(0.005)	(0.005)	(0.001)	(0.001)
Control variable	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y

(*continued on next page*)

Table A27 (*continued*)

VARIABLES	Migrant school	Migrant school	International school	International school	Other schools	Other schools
	(1)	(2)	(3)	(4)	(5)	(6)
SE	robust	cluster	robust	cluster	robust	cluster
R-squared	0.217	0.217	0.081	0.081	0.999	0.999
Observations	3393	3393	3393	3393	3393	3393

Notes: The dependent variables are types of schools for migrant children. All regressions control for individual, year, and city fixed effects. Values in columns 1, 3, 5 between parentheses are robust standard errors clustered at the individual level. Values in columns 2, 4, 6 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A28
Mechanism analysis: heterogeneous effects of *Hukou* reform on education expenditures by parental education.

VARIABLES	Total Education Expenditures	Total Education Expenditures	In-School Expenditures	In-School Expenditures	Off-School Expenditures	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×post	1.005***	1.005***	1.165***	1.165***	−0.809**	−0.809**
	(0.298)	(0.318)	(0.309)	(0.326)	(0.326)	(0.333)
Treat×post×edu	−0.612**	−0.612**	−0.648**	−0.648**	0.511**	0.511**
	(0.267)	(0.279)	(0.280)	(0.294)	(0.230)	(0.241)
Control variable	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y
SE	robust	cluster	robust	cluster	robust	cluster
R-squared	0.206	0.206	0.150	0.150	0.130	0.130
Observations	3393	3393	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. Parental education is an indicator of whether parents' educational attainment is senior high school and above. All dependent variables are log-transformed. Values in columns 1, 3 and 5 between parentheses are robust standard errors, while values in columns 2, 4 and 6 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A29
Mechanism analysis: heterogeneous effects of *Hukou* reform on education expectation by parental education.

VARIABLES	Education Expectation	Education Expectation	Scoring Expectation	Scoring Expectation
	(1)	(2)	(3)	(4)
Treat×post	0.518***	0.518**	1.624***	1.624***
	(0.177)	(0.205)	(0.531)	(0.525)
Treat×post×edu	−0.228*	−0.228*	−0.258	−0.258
	(0.120)	(0.134)	(0.577)	(0.639)
Control variable	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y
SE	robust	cluster	robust	cluster
R-squared	0.030	0.030	0.027	0.027
Observations	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. Values in columns 1 and 3 between parentheses are robust standard errors, while values in columns 2 and 4 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A30
Mechanism analysis: heterogeneous effects of *Hukou* reform on education expenditures by children's gender.

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
Gender (0 "girl", 1 "boy")					
Treat×post	0.534	0.694	0.745	−0.163	−0.478
	(0.461)	(0.460)	(0.453)	(0.214)	(0.395)
Treat×post×gender	0.121	0.263	0.295	−0.051	−0.368
	(0.237)	(0.232)	(0.232)	(0.058)	(0.233)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y

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Table A30 (*continued*)

VARIABLES	Total Education Expenditures	In-School Expenditures	In-School Expenditures Other Than Sponsorship Fees	Sponsorship Fees	Off-School Expenditures
	(1)	(2)	(3)	(4)	(5)
City fixed effects	Y	Y	Y	Y	Y
SE	robust	robust	robust	robust	robust
R-squared	0.197	0.143	0.138	0.039	0.129
Observations	3393	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. All dependent variables are log-transformed. Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A31
Mechanism analysis: heterogeneous effects of *Hukou* reform on education expectation by children's gender.

VARIABLES	Education Expectation	Education Expectation	Scoring Expectation	Scoring Expectation
	(1)	(2)	(3)	(4)
Gender (0 "girl", 1 "boy")				
Treat×post	−0.011	−0.011	1.413**	1.413**
	(0.147)	(0.191)	(0.589)	(0.567)
Treat×post×gender	−0.111	−0.111	0.245	0.245
	(0.109)	(0.125)	(0.521)	(0.608)
Control variable	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y
SE	robust	cluster	robust	cluster
R-squared	0.029	0.029	0.027	0.027
Observations	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. Values in columns 1 and 3 between parentheses are robust standard errors, while values in columns 2 and 4 between parentheses are robust standard errors clustered at the city level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A32
Effects of *Hukou* reform on key class: time dimension.

VARIABLES	Key Class
	(1)
Treat×year12	0.531
	(0.376)
Treat×year16	0.543**
	(0.222)
Treat×year18	0.607***
	(0.193)
Control variable	Y
Individual fixed effects	Y
Time fixed effects	Y
City fixed effects	Y
SE	robust
R-squared	0.126
Observations	3393

Notes: The dependent variable is whether the class is a key class. Regressions control for time fixed effects and other control variables: parental education, average parental age and squared age, family size, parental occupation, average parental health status, number of children in school, and per pupil financial expenditure on education. Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Table A33
Mechanism analysis: effects of *Hukou* reform on play time vs. study time.

VARIABLES	TV Viewing Time Per Week	Non-Weekend Study Hours	Weekend Study Hours	Study Time Per Week	Parental supervision Hours
	(1)	(2)	(3)	(4)	(5)
Treat×post	−2.017***	0.584***	0.365**	0.950***	1.926***
	(0.780)	(0.213)	(0.175)	(0.284)	(0.663)

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Table A33 (*continued*)

VARIABLES	TV Viewing Time Per Week	Non-Weekend Study Hours	Weekend Study Hours	Study Time Per Week	Parental supervision Hours
	(1)	(2)	(3)	(4)	(5)
Control variable	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y
SE	cluster	cluster	cluster	cluster	cluster
R-squared	0.107	0.072	0.086	0.098	0.148
Observations	3393	3393	3393	3393	3393

Notes: All regressions control for individual, year, and city fixed effects. Values between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Table A34
Effects of *Hukou* reform on medical expenditure.

VARIABLES	Medical Expenditures	
	(1)	(2)
Treat×post	−0.306	−0.306
	(0.220)	(0.253)
Control variable	Y	Y
Individual fixed effects	Y	Y
Time fixed effects	Y	Y
City fixed effects	Y	Y
SE	robust	cluster
R-squared	0.060	0.060
Observations	3393	3393

Notes: The dependent variable is medical expenditures and log-transformed. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. Values in columns 1 between parentheses are robust standard errors clustered at the individual level, while values in columns 2 between parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

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