# Pursuing a brighter future: Impact of *Hukou* reform on the 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 examines the impact of China's *Hukou* reform – an institutional change that was initiated in 2014 and aimed at gradually expanding 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 reform, we apply the difference-in-differences method and find that the reform has significantly increased investment in education in migrant households, with the increase mainly arising from in-school expenditures (with the exception of sponsorship fees) rather than off-school expenditures. Analysis of the mechanism shows that first, although the reform has expanded access to public education for migrant children, the local governments do not invest more in public education following the reform (substitution effects). Second, the *Hukou* reform appears to eliminate resource discrimination and signal quality improvement within the public education system, which raises migrant families' expectations for their children's education, prompting them to increase their in-school education expenditures to improve the quality of education for their children (the flypaper effects); Finally, increases in migrant family income may 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.

**JEL Codes:** D10, I38, J68

**Keywords:** China's *Hukou* reform; migrant children; education expenditure; human capital

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## 1. Introduction

In the context of large-scale urbanization, China's household registration (*Hukou*) system has long been regarded as a dual welfare arrangement, which has led to inequalities between migrants and local urban residents (Cai, 2011). Compared to local residents, migrants without a local *Hukou* were discriminated against in the job market and had limited access to social services like hospitals and schools, which acted as a constraint on industrialization, the scale of migration, and public financial capacity in the city where they live (Fan, 2002; Liu, 2005; Whalley and Zhang, 2007). While similar welfare systems are common in countries undergoing economic transition, governments have been increasingly committed to eliminating institutional obstacles to migrants' welfare through reforms (Sabates-Wheeler and Koettl, 2010; Boräng, 2015; Juárez et al., 2019). China, as the country with the largest population worldwide, has the largest number of rural-urban migrants (292.51 million in 2021); the *Hukou* system has been a serious obstacle in the lives of Chinese migrants, and poses a challenge to China's economic transition in the new era. Numerous studies have explored how *Hukou* reforms are related to income (Pi and Zhang, 2016), housing (Wu and Zhang, 2018), consumption (Wang et al., 2021), and savings (Fields and Song, 2020). However, few studies have investigated the relationship between *Hukou* reforms and migrant families' investment in human capital investment.

This study focuses on the nationwide *Hukou* reform initiated by the Chinese government in 2014, aimed at welfare restructuring by eliminating *Hukou* restrictions in cities of less than 3 million people and relaxing *Hukou* restrictions in cities of more than 3 million people. The reform is likely to change the economic behavior and decisions of migrant families in two ways. First, they may be more likely to migrate due to reduced migration costs. Second, they may change household investment (for example, in their children's human capital) because of increased labor productivity and higher income (Pi and Zhang, 2016). However, with the dramatic growth in the number of migrants and increased education expectations, public education resources have fallen short, leaving the educational needs of migrant children not fully met. Compared to other social benefits, providing equal access to public education has been relatively slow, and is considered the "last mile" in the *Hukou* reform. Essentially, children's education is the basis for migrant families to accumulate human capital and achieve social mobility, making investment in education of immense significance to both migrants and policy makers. Therefore, this study evaluates the impact of the 2014 *Hukou* reform on the investment in education for migrant children (who migrate with their parents). Specifically, considering that the 2014 *Hukou* reform involved migrant children only at primary and junior high schools (i.e., compulsory education), we investigate whether and how China's *Hukou* reform has affected compulsory education expenditure for migrant families.

To thoroughly examine the effects of this reform, we combine panel data from the China Family Panel Survey (CFPS) from 2012 to 2018 with the *Hukou* registration index data compiled by the China Household Finance Survey and Research Center. In China, local governments remain independent in implementing policies of the national government and often determine the pace and rhythm of policy adjustments based on their own realities (Sun et al., 2011). Many

studies on China's *Hukou* reform tend to ignore this, often measuring the reform process using a one-size-fits-all approach and neglecting the regional differences in implementing the reform. To address this issue, our study uses the *Hukou* registration index to expose the substantial variations in the pace of reform across cities of different sizes in China. Employing the difference-in-difference (DID) method, we investigate the impact of the *Hukou* reform on investment in education for migrant children. In addition to total expenditure, we explore the impact of the *Hukou* reform on the structure of education expenditure for migrant children.

The *Hukou* reform in 2014 focused on promoting the equalization of public services, including education, that is, migrant children can receive compulsory education in the migration destinations without needing their families to pay additional cost (e.g., sponsorship fees). Thus, the saved funds can be used for other purposes. In this process, there is a "substitution effect" between the government and families in terms of expenditure on education, with the reform reducing the investment in education of migrant families (Chi and Qian, 2016). However, as an important pathway to long-term human capital accumulation for families, children's education is investment-oriented, with families seeking to earn human capital gains in the future by investing in their children's education in the present (Foster and Rosenzweig, 1996; Glewwe and Jacoby, 2004). On the one hand, the reform has effectively relaxed the credit constraints faced by migrant families in destination cities, expanded employment channels, and raised income levels, thus increasing children's investment in education and creating an "income effect" (Naoi et al., 2021; Das, 2021). This surplus income could be allocated to all members and not 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, higher quality of education in cities can raise the expectations of higher education and education investments; thus, migrant families might allocate the education expenditures saved due to the reform to immediate investments in their children's education to improve the quality of their education and create a "flypaper effect"(Shi, 2012; Ambler et al., 2015; Canavire‐Bacarreza et al., 2020). The Chinese public education system can only guarantee the completion of the legal years of education for children and provide the basic quality of education. However, families need to rely on household-level funds to provide their children with high-quality education to attain human capital returns in the future. Since few studies investigate how *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, each 1% increase in the *Hukou* registration index difference led to a 1.54% increase in total education expenditure. 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 the gap in the literature in three ways. 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 like household size (Conley and Glauber, 2006), income (Karki Nepal, 2016), risk preferences (Tabetando, 2019), migration and remittances (Askarov and Doucouliagos, 2020), and social networks (Zuluaga, 2013); child gender (Vogel and Korinek, 2012; Azam and Kingdon, 2013; Wongmonta and Glewwe, 2016); as well as parental education, age, and occupation (Jenkins et al., 2019; Yan et al., 2021) on expenditure on children's education. 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 and Zhou, 2019), and gender wage differentials (Wang and Cheng, 2021), with 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 reform (Kubota, 2016), tuition fee waiver policies (Shi, 2012; Chi and Qian, 2016), 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 speaks to 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 expenditure on voluntary education. Das (2021) found a significant positive effect of income on private tutoring expenditure while having no impact on other educational expenditure. Kubota (2016) studied an 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 exemptions from China's compulsory education law, Chi and Qian (2016) found that Chinese parents spend more on academic-related courses than interest classes. For Chinese families, the purpose of off-school expenditure 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 are not specifically extrapolated to rural migrants. As Chinese domestic migrants a are a vulnerable group among, 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 not clear how migrant families allocate their spending across educational activities, which constitutes an important empirical problem to research.

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

The remainder of this paper is structured as follows. Section 2 provides a historical background to 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 summarizes the main conclusions of the study.

## 2. Background to China's Hukou Reform

During the Mao era, which began in 1949, China planned an economic system and national strategy that prioritized the development of 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 necessitates constructing an urban-rural dual structure that supports industrial development at the expense of agriculture. This requires a large number of people to engage in agricultural production to provide the material basis for the development of heavy industry (Li and Yang 2005). The agricultural population must be highly spatially concentrated in rural areas to create a stable external environment for the development of heavy industry (Zhang and Lu, 2019). Given this, the central government issued the Regulations of *Hukou* Registration in 1958, which established the *Hukou* system based on the type of *Hukou* status (agricultural and non-agricultural) and *Hukou* location (Song, 2014) and made 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 and 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 was only sufficient to provide food and clothing, and could not compensate for a lack of human capital, such as 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 free migration of the labor force, including the agricultural population, who were now allowed to enter cities, thus initiating the *Hukou* reform. 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 (Wang, 2005; Chan, 2009). 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 system gradually changed from focusing on the administrative control of population migration to the development and utilization of migrant human capital. With the breakdown of population migration control and changes in the function of the *Hukou* system, coupled with the implementation of the household responsibility system in the agricultural sector, hundreds of millions of laborers were freed from lower-productivity sectors, such as agriculture. They entered higher-productivity sectors, such as manufacturing and services, through unprecedented large-scale migration across urban and rural areas and regions[1]. Consequently, the optimal allocation of human resources among economic sectors directly brought about a significant increase in labor productivity.

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

However, central and local governments did not realize the unsustainability of the demographic dividend during this period. The *Hukou* system, 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 demographic dividend.

With China's slowdown from double-digit to single-digit economic growth since 2012, coupled with the rapid aging of the population and shrinking external market demand due to global recession, central and local governments began to realize that the persistent labor supply shortage might be a long term and critical problem limiting China's economic development.

[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; thus, China's economy has entered a "new normal."

[3] The *Hukou* reform led by local governments in this period had two objectives: the unification of *Hukou* types (i.e., the abolition of 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 of less than 500,000. However, it is generally agreed that these reforms have not really benefited the majority of migrants (Song, 2014).

Therefore, in the 12th Five-Year Plan for National Economic and Social Development, *Hukou* reform was considered a priority for China's economy (Zhang, 2012). The purpose of the *Hukou* reform was to expand access to public services and social benefits to the migrant population. Such 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 gradually shifts from being quantity-based to being quality-based. Subsequently, central government promulgated the National New-Type Urbanization Plan (2014-2020) in 2014 to promote the reform of *Hukou*. It lowered the threshold for acquiring local *Hukou,* and provided equal access to basic public services, explicitly setting an urban population of 3 million as the threshold for abolishing restrictions on *Hukou* conversion. That is, cities with an urban population of less than 3 million were to gradually abolish the restrictions on settlement, while cities with a population of more than 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 the access to basic public services in urban areas for the rural-urban migrants (those working and living in urban areas without a local city *Hukou*). Appendix Figure A1 shows that productivity growth (although with a larger magnitude) mirrored the growth rate of the number of migrant workers before 2014. After 2014, the gap was enlarged with the former climbing quickly and the latter remaining stable. This indicates that productivity growth was potentially due to improvements in human capital driven by the *Hukou* reform. Appendix Figure A2 and Table A1 show the effect of *Hukou* reform on labor productivity proxied by labor unit output, indicating that more extensive *Hukou* restrictions are associated with lower labor productivity. In other words, eliminating these restrictions is likely to increase labor productivity.

Notably, under a decentralized system, *Hukou* reform in a particular region was managed by local government. Central government set the direction of the reform and established the general standards, while the specific reform practice was applied by local government following its own economic and social development situation. In other words, local governments dictated the pace and timing of the *Hukou* reform in their regions according to the requirements of the central government. However, in reality, the lowering of the threshold for obtaining local *Hukou* and the expansion of 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 government's implementation of the *Hukou* reform (Appendix Table A2). Zhang and Lu (2019) corroborated this finding using a *Hukou* registration index constructed from a textual analysis of policies on settlement and equalization of basic public services in 120 cities around 2014. Figure 1 identifies the differences in the *Hukou* registration index before and after 2014 in different cities[4]. The majority of big cities (with an urban population of more than 3 million) had a negative difference in the *Hukou* registration index, indicating that they raised the standards for obtaining local *Hukou* and set higher barriers to access basic public services, with some exceptions, such as Nanjing, Changsha, Chengdu, and Shijiazhuang. Conversely, most third-, fourth-, and fifth-tier cities (with an urban population of less than 3 million) had a positive

[4] There are many *Hukou* registration indexes, from which we use the composite and ordinary employment indexes because they are more applicable to rural-to-urban migrants.

difference in the *Hukou* registration index, indicating that these cities lowered the criteria for obtaining local *Hukou* and reduced the difficulty of accessing basic public services for the non-local *Hukou* population. However, some small- and medium-sized cities in developed regions, such as Wenzhou, Shantou, Zhanjiang, Dandong, showed the opposite trend. In Figure 1, the size of the dots reflects the magnitude of the differences in the *Hukou* registration index before and after 2014. It is clear that even among cities of the same population size, the degree of *Hukou* reform varied greatly due to the autonomous decisions of local governments.

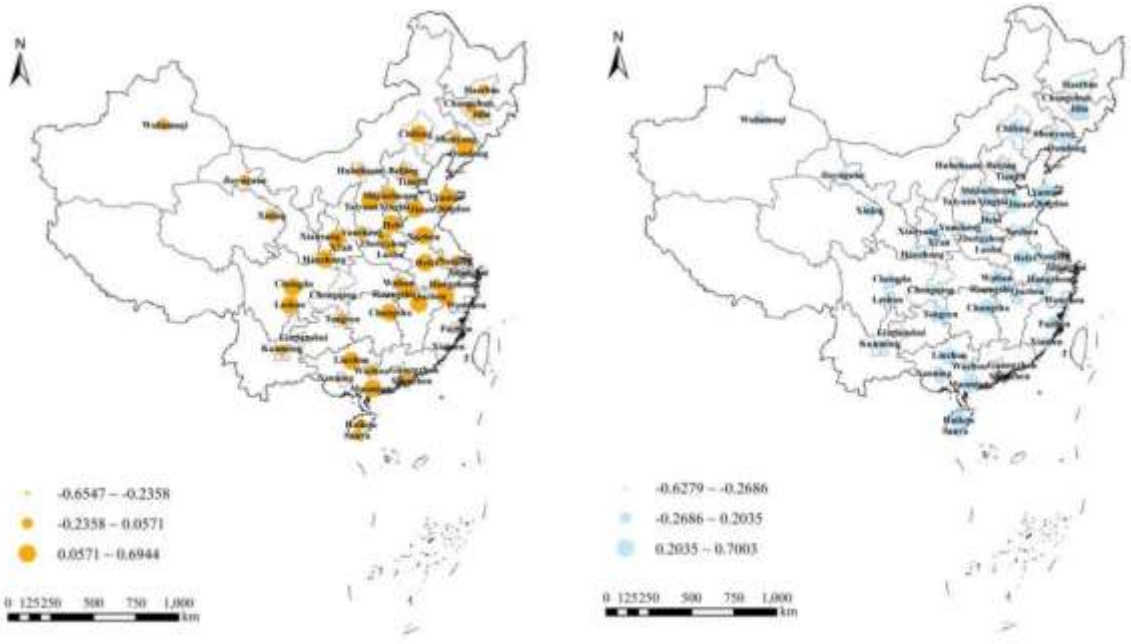



Figure 1. Difference in the *Hukou* registration index for selected Chinese cities before and after 2014
Note: The index difference is defined as the pre-2014 index minus the post-2014 index. The size of the dots shows the size of the index difference, that is, the larger the dots, the larger the index difference, and vice versa. The data used in the left panel is the composite index and the data used in the right panel is the ordinary employment index.

## 3. Methods

### 3.1 Data

Our primary analysis combines data on the *Hukou* registration index with 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, multilevel probability sampling. In total, 37,354 observations in 14,960 sampled households were obtained, covering 25 provinces, cities, and autonomous regions in mainland China, representing 95% of the country's population. For our 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 to have a direct impact on the education of migrant children[6] as they are more vulnerable compared to local children (Wei and 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 in the compulsory education stage (i.e., primary and junior high school), as this stage is regulated by policies related to *Hukou* both in terms of 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 the reform promotion and implementation.

The outcomes of interest in this study 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 include expenses incurred for talent training, mental development classes, tutoring, coaching for competitions, and other classes. Total education expenditure is calculated as the sum of in-school and off-school expenditure. All education expenditures measured in the survey year were deflated based on the consumer price index of each province. The rich survey data allowed analysis of a range of education expenditures at the individual level, which is different from previous work that used national or provincial survey data or nationally representative cross-sectional data (Meyerhoefer and Chen, 2011; Qian and Smyth, 2011; Shi, 2012; Cheng, 2021; Yan et al., 2021) This study has a broader sample and a longer follow-up period, which is conducive to more accurate estimation of the effect of the *Hukou* reform on migrant families spending on education.

The *Hukou* reform is measured using the China *Hukou* registration index, constructed by the China Household Finance Survey and Research Center. Zhang and Lu (2019) conducted a more comprehensive review and quantitative analysis of *Hukou*-related policies, such as settlement at the national and provincial levels as well as in 251 cities. They constructed a *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 *Hukou* reform at the national level as the cut-off point, the values for each *Hukou* registration index type before and after 2014 were obtained. Values before 2014 were

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

[6] Since CFPS does not directly identify the rural migrants sample, we relied on the migrant population identification method prompted by the official website to determine the agricultural *Hukou* population living in the city and not local *Hukou* as rural migrants and further designated the children living in the same space as the migrant children.

calculated based on the text of the *Hukou* reform policy t of each city from 2000 to 2013, while those after 2014 were based on the same information from 2014 to 2016. The index measures two properties: the ease of obtaining local *Hukou* for the non-local *Hukou* population using different channels (the higher the value of the index, the stricter the requirements for settling in this city) and the ease of access to basic public services by the non-local *Hukou* population in the migration destination. Owing to the generally low levels of wealth and human capital among rural migrants, most have been unable to settle in urban areas or access basic public services through investment, house purchase, or high-end employment, with their only access being ordinary employment. Therefore, we chose the difference between the ordinary employment index before and after 2014 to represent the level of *Hukou* reform (i.e., subtracting the index after 2014 from that of before 2014). A positive sign was that the reform in a particular city facilitated access to public education for migrant children. The 120 cities covered by the *Hukou* registration index did not exactly match the cities in the CFPS database, so for cities that could not be matched, we used the *Hukou* registration index of adjacent cities with similar economic conditions[7].

We also conducted additional analyses using data on provincial-level urban per capita education expenditure and government inputs from 2012 to 2018, obtained 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 (Qian and Smyth, 2011; Jenkins et al., 2019; Song and Zhou, 2019; Yan et al., 2021). To mitigate for the endogeneity of the *Hukou* registration index, we used regional-level control variables, including the marketization index (obtained from China's Marketization Index of China's Province, edited by Wang et al., (2021)), per capita GDP, the number of primary and secondary school students, government revenue, and expenditure (obtained from 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 (cities with positive differences in *Hukou* registration index) and control groups (cities with negative differences in *Hukou* registration index) (Appendix Table A3). Specifically, Table 1 shows the regional differences in changes in the ordinary employment index, which decreased from 0.493 to 0.421.

Table 1
Summary Statistics

| VARIABLES | Mean | S.D. | Mean | S.D. | Mean | S.D. |
|---|---|---|---|---|---|---|
| All Cities | Full Sample | | Before | | After | |
| *Hukou* registration Index | 0.459 | 0.243 | 0.493 | 0.192 | 0.421 | 0.270 |

[7] First, we identified cities with similar economic situation within the same province and region based on the GDP ranking of each city in the corresponding year. Then, we selected cities with close physical distances based on geographical location. However, we prioritized the economic situation. If the city code was missing, we used the average *Hukou* registration index of non-capital cities in the same province. Finally, the *Hukou* registration indexes of 151 cities were matched.

| | | | | | | |
|---|---|---|---|---|---|---|
| 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 |
| Out-of-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[8] | - | - | - | - | - | - |
| 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 Primary and Secondary Student | 32.237 | 45.138 | 29.334 | 34.518 | 35.234 | 53.794 |
| 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 |
| 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).

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

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 | | | | | | | | | | |

[8] 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.

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 2012 | 1403.097 | 0.000 | 1142.028 | 0.000 | 1118.794 | 0.000 | 22.3092 | 0.000 | 254.6308 | 0.000 |
| 2014 | 1779.928 | 0.269 | 1312.129 | 0.149 | 1295.708 | 0.158 | 7.82496 | -0.649 | 450.7979 | 0.770 |
| 2016 | 2522.624 | **0.417** | 1916.567 | **0.461** | 1889.643 | **0.458** | 20.9655 | **1.679** | 592.1122 | 0.313 |
| 2018 | 3000.573 | 0.189 | 1789.083 | -0.067 | 1706.072 | -0.097 | 10.2267 | -0.512 | 1201.245 | **1.029** |
| >3 million | | | | | | | | | | |
| 2012 | 1972.504 | | 1503.307 | | 1427.832 | | 75.1824 | | 464.8175 | |
| 2014 | 3043.426 | **0.543** | 1936.257 | **0.288** | 1848.206 | **0.294** | 44.0251 | -0.414 | 1080.881 | **1.325** |
| 2016 | 4056.829 | 0.333 | 2332.398 | 0.205 | 2140.829 | 0.158 | 96.6666 | **1.196** | 1712.52 | 0.584 |
| 2018 | 5376.404 | 0.325 | 2073.956 | -0.111 | 2037.825 | -0.048 | 30.3030 | -0.687 | 3298.252 | 0.926 |

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

### 3.2 Estimation Strategy

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

$$\ln C_{ijt} = \alpha + \beta \times (dindex_j \times post) + \gamma \times X + \lambda_i + \delta_t + \mu_j + \varepsilon_{ij} . \quad (1)$$

$lnC_{ijt}$ is a measure of investment in education for individual $i$, living in city $j$ in year $t$. The key explanatory variable, $dindex_j$, is the difference between the *Hukou* registration index before and after 2014 in city $j$, which 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)[9]. *post* denotes 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 $\lambda_i$ to absorb the effect of factors that do not vary over time at the individual level. We also included year fixed effects $\delta_t$ and city fixed effects $\mu_j$. $X$ is a set of household- and region-level control variables. The key parameter of interest, $\beta$, estimates how investment in education changes in response to each 1% increase in the *Hukou* registration index.

The validity of the estimation depends on the uniqueness of the *Hukou* reform shock and the conditional exogeneity of the change in the *Hukou* registration index. Our first concern is that the effects could also be compounded by the *Hukou* reforms before 2014. As discussed in Section 2, all prior institutional adjustments to the *Hukou* system at the national level occurred in the 1990s and were focused on easing the free migration of labor (Yang and 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 [10]. An et al. (2020) provided some preliminary evidence, showing that the previous reforms did not have different results across different city sizes.

We also verify whether the *Hukou* barriers in the small cities with a population < 3 million suddenly changed after 2014 compared with large cities with a population above 3 million. We use the *Hukou* index difference as the outcome variable and adopt an indicator for whether the city has a population $\geqslant$3 million as the treatment variable. The results shown in Panel A of Appendix Table A4 suggest that after 2014, the ordinary employment index and the composite

[9] 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 measure the degree of advancement of the *Hukou* reform using the difference in the *Hukou* registration index before and after 2014 in each city.

[10] Appendix Table A1 collates the time for all provinces to issue policy documents on the *Hukou* reform.

index suddenly decreased in small cities compared to big cities. This result suggests that the *Hukou* reform mainly targeted migrant workers since we did not identify any significant changes in the other three types of *Hukou* registration index, including investment, house purchase and high-end employment. The results remain robust if we use the threshold of 5 million as the treatment status (Panel B of Appendix Table A4), which is consistent with the summary statistics shown in Appendix Table A5, where we found a decrease in the *Hukou* registration index for all cities with populations <3 million, an average increase of 0.005 for cities with populations between 3 and 5 million, and an average increase of 0.167 for large cities with populations between 5 and 10 million. This result suggests that the reforms mainly took place in cities with a population size ≤ 3 million, which aligns with the overall design of the *Hukou* reform at the national level. However, many cities with a population of more than 3 million raised the *Hukou* threshold and, consequently, difficulty in accessing basic public services. Reforms in these cities have moved in the opposite direction of the original national-level design. Appendix Table A6 shows another piece of evidence about the changes after the 2014 reform, indicating that the small size (<3 million) cities had the highest rate of floating population growth while the growth rate in large size cities decreased in 2016-2018. This why, we used the threshold of 3 million and 5 million (An et al., 2022) as the treatment status for robustness, in addition to using the *Hukou* registration index as a continuous treatment for the main analysis[11].

However, the progress of the *Hukou* reform in each city under the decentralized system was not exogenous but was likely related to the city characteristics and the result of self-selection by each city. Therefore, we included city fixed-effects in the model to exclude 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 needed to control for some urban characteristics that vary over time and by city. To mitigate possible endogeneity, we controlled for marketization index, per capita GDP, government revenue, government expenditure, and number of primary and secondary school students (Tabetando, 2019; Wang and Cheng, 2021; Yan et al., 2021).

In addition, to verify the parallel-trends assumption of the DID, we conduct an event study by examining the differences in investment in education over years. We introduced the interaction terms between the *Hukou* registration index difference and year dummies to form Eq. 2 as:

$$\ln C_{ijt} = \alpha + \sum_{t=2012}^{2018} \beta(dindex_j \times year_t) + \gamma \times X + \lambda_i + \delta_t + \mu_j + \varepsilon_{ijt} \qquad (2)$$

## 4. Results

In this section, we show whether and how *Hukou* reform affects migrant families' education investment in their children. We conduct a series of sensitivity es to ensure our main results are robust.

### 4.1. Main Results

[11] In addition, we showed that the results using both thresholds remain identical in the robustness checks. However, we used a threshold of 3 million because the national-level *Hukou* changes for cities with a population 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 criteria of *Hukou* settlement for cities with a population size of less than 5 million.

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 total education expenditure as the dependent variable and household- and region-level control variables, individual, year and city fixed effects controlled. The results indicate a statistically significant and economically meaningful effect of *Hukou* reform on total educational expenditure. Further, a higher *Hukou* registration index difference is associated with higher total education expenditure. Specifically, each 1% increase in the *Hukou* registration index difference would lead to a 1.54% increase in total education expenditure.

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) |
| dindex(log)×post | 1.542*** | 1.877*** | -0.902* | -0.062 | 1.873*** |
| | (0.466) | (0.460) | (0.541) | (0.134) | (0.459) |
| 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 | 0.203 | 0.148 | 0.125 | 0.027 | 0.144 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 |

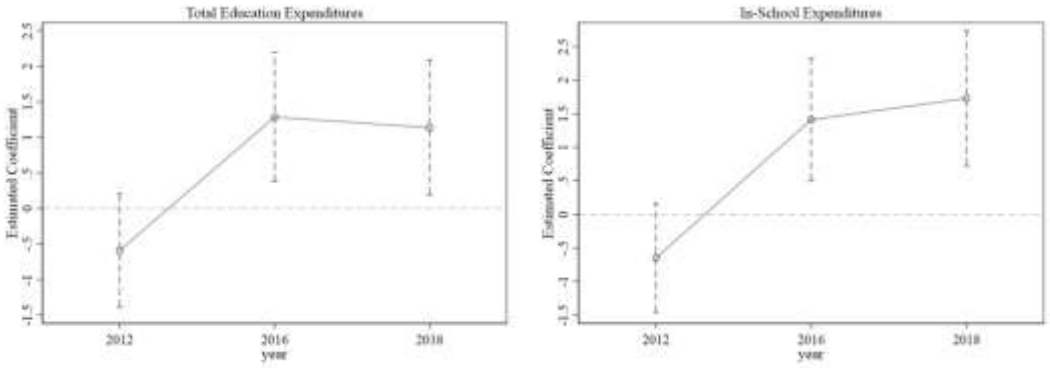
Note: All regressions control for individual, year, and city fixed effects. Values in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Next, we investigated changes in the structure of education expenditure and estimated Eq. 1 with specific expenditure items reflecting education expenditure as dependent variable. Column 2 of Table 3 shows the results for in-school expenditure, indicating that a 1% increase in the *Hukou* registration index would increase in-school expenditure by 1.88%. Column 3 shows the estimation results for off-school expenditure, suggesting that a 1% increase in the *Hukou* registration index would reduce off-school expenditure by 0.90%. Combining the above estimates of in-school and off-school expenditure, we conclude that the increase in total education expenditure is mainly caused by the rise of in-school spending, indicating that the reform led to a gradual flow of previously off-school expenditure into in-school expenditures.

To explore the reform's impact 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 families of migrant children before the reform to

obtain local public education due to their non-local *Hukou*. Theoretically, they represent an institutional cost of obtaining public education that could be affected by the *Hukou* reform in each city. Column 4 shows statistically insignificant effect of the *Hukou* reform on sponsorship fees. This result could be because, before the reform, 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 poor-quality education because of the lack of qualified teachers, inadequate teaching materials, and poor sanitation. Data from the pre-reform period show that less than 1% of migrant children paid sponsorship fees for access to public education, with more than 80% of migrant children attending migrant schools for children of rural-urban migrants. Since the private education institutions chosen by migrant families usually cannot meet the basic quality requirements of compulsory education (Bao, 2006), many families would compensate for the low-quality of in-school education through off-school tutoring and training. Column 5 shows changes in in-school expenditures other than sponsorship fees. The results show that a 1% increase in the *Hukou* registration index would lead to a 1.87% increase in in-school expenditures other than sponsorship fees. That is, the higher the degree of advancement of reforms in cities, the higher the in-school expenditure other than sponsorship fees of migrant families. Thus, we infer that the increase in in-school expenditures resulting from the reform is mainly due to increases in in-school expenditures other than sponsorship fees[13].

We further conducted a parallel-trends analysis. For total education expenditure and structural variables, we estimated Eq. 2 and plotted parallel trends (Figure 2). Using 2014 as a reference, we find none of the estimated coefficients for the pre-reform year 2012 showed significant differences, while for the post-reform years 2016 and 2018, almost all the coefficients are significant (with the exception of sponsorship fees). This indicates that the differences in education expenditure between treatment and control groups were likely brought about by the *Hukou* reform.




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

[13] In Appendix Table A7, we use robust standard errors clustered at the individual level and find that the results remains identical.

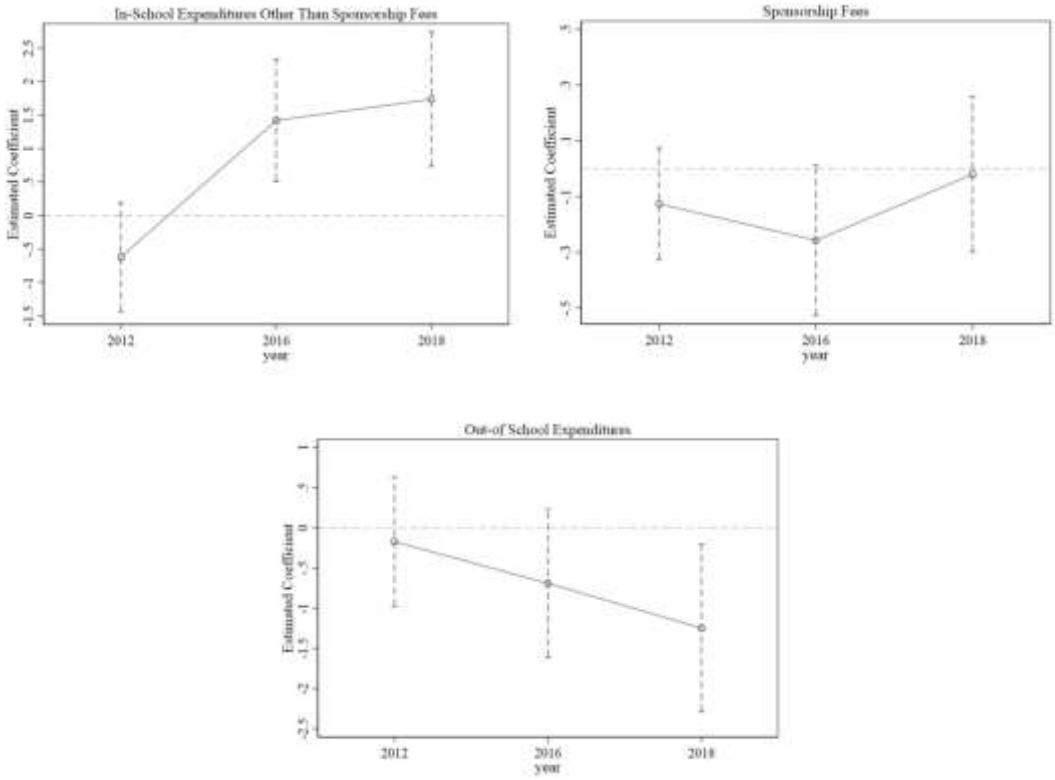



Figure 2. Parallel trend test plot.
Note: The graphs show the parallel trend scatter plots for each explanatory variable. Regression analysis includes individual, year, and city fixed effects, as well as control variables.

The corresponding regression results are presented in Table 4. The estimated coefficients for total education expenditure were 1.350 and 1.218 in 2016 and 2018, respectively, being significant at the 1% level, indicating that the positive effect of the reform was stronger in the short term and slightly decreased in the long term. Using in-school expenditure as the dependent variable, the estimated coefficients were 1.459 and 1.788 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 1.462 and 1.802 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 -0.260, but 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 had a significant negative impact on off-school expenditure only in 2018, with an estimated coefficient of -1.283 at the 0.5% significance level. This indicates that the reform caused a reduction in off-school expenditures by migrant households. Based on the trends over time, we can infer that the reform impacted the total education expenditure of migrant households in the short term (2016) mainly through the redirection of sponsorship savings 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.

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) |
| dindex(log)×year12 | -0.543 | -0.543 | -0.597 | -0.597 | -0.571 | -0.571 | -0.152 | -0.152 | -0.093 | -0.093 |
| | (0.408) | (0.440) | (0.411) | (0.437) | (0.411) | (0.437) | (0.098) | (0.095) | (0.409) | (0.308) |
| dindex(log)×year16 | 1.350*** | 1.350** | 1.459*** | 1.459*** | 1.462*** | 1.462*** | -0.260** | -0.260* | -0.666 | -0.666 |
| | (0.463) | (0.676) | (0.467) | (0.526) | (0.467) | (0.524) | (0.119) | (0.153) | (0.472) | (0.683) |
| dindex(log)×year18 | 1.218** | 1.218** | 1.788*** | 1.788*** | 1.802*** | 1.802*** | -0.001 | -0.001 | -1.283** | -1.283** |
| | (0.477) | (0.473) | (0.51) | (0.558) | (0.510) | (0.561) | (0.134) | (0.136) | (0.533) | (0.523) |
| 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 | 0.149 | 0.149 | 0.145 | 0.145 | 0.005 | 0.005 | 0.126 | 0.126 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

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

### 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 with different criteria for dividing the treatment and control groups under the reform used, we employed two other approaches. First, cities with a difference in the *Hukou* registration index greater than zero (i.e., cities that had relaxed *Hukou* restrictions) were used as the treatment group, while cities with a difference in the *Hukou* registration index less than zero were used as the control group. Second, based on the requirements of the reform at the central level, urban cities with populations of less than 3 million in 2014 were used as the treatment group, and those with more than 3 million were used as the control group. For total education expenditure and variables reflecting the structure of expenditure, Eq.1 was re-estimated. Table A8 shows the results are similar to those of the base regression. Using a population of 5 million as the threshold did not significantly change the results. In addition, we adopted an Instrumental Variable strategy, using the interaction term between the post indicator and the treatment status that 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 smaller than 3 million) were associated with a large difference in the *Hukou* registration index and the second stage estimation shows similar results compared to the baseline regressions (Appendix Tables 9-10).

To ensure that the results of the baseline regression do not change with level of advancement of the *Hukou* reform in each city, we replaced the PPM with Zhang et al.'s (2019)

equal weight method to calculate the ordinary employment index in the *Hukou* registration index system. As shown in Appendix Table A11, the results are generally consistent with the estimated coefficients of the baseline regression.

We also excluded certain observations. First, we excluded cities without *Hukou* registration index information rather than matching them with adjacent cities as we had done previously. The results barely changed (Appendix Table A12). Second, 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 using the trimmed sample. As shown in Appendix Table A13, the results are fairly consistent with the baseline regression. Third, we excluded municipalities and first-tier cities (including Beijing, Shanghai, Tianjin, Chongqing, Guangzhou, and Shenzhen) which are likely associated with stricter *Hukou* restrictions and found that the main results remained robust (Appendix Table A14). Fourth, we excluded the sample living in treatment cities adjacent to the control cities (with a difference in the *Hukou* registration index of less than zero)[14] and found that the main results did not significantly vary (Appendix Table A15).

There was a risk that data attrition and selection bias could also affect our main results. To examine whether data attrition would bias 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 *Hukou* reform. We found that the coefficient was insignificant (Appendix Table A16). Another concern was that *Hukou* reform may affect whether children migrate with their parents. We ruled out this possibility by showing that *Hukou* reform is not associated with whether children are left behind (Appendix Table A17). We also show that *Hukou* reform is not linked to changes in family characteristics (Appendix Table A18). To test whether sample selection would bias our estimation, we re-estimated Eq.1 with the full city sample including all the children (rural-urban migrant children, rural left-behind children and children with city *Hukou*). The baseline regressions and event studies remained consistent (Appendix Tables A19-20). Appendix Tables A21-22 showed similar results when we used the full rural-urban migrant sample, regardless of the migration status of children[15]. Appendix Table A23 shows that among the rural-urban migrant sample, there was a positive impact of *Hukou* reform on total educational expenditure for those whose children migrated together with them rather than for those whose children were left behind. This result provided additional validity for our focus on migrant children. In addition, we did a placebo test, drawing on the urban sample. Since *Hukou* reform focused more on migrants without local *Hukou*, the urban population with local *Hukou* was likely to be unaffected. As expected, Appendix Tables A24-25 shows that all the 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 et al., 2009). Subjects were randomly assigned to the treatment and control groups 1000 times. Figure 3 shows that the scatter of all estimated coefficients is concentrated around 0, far from their true values, with most of 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, supporting the identification strategy.

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

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

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

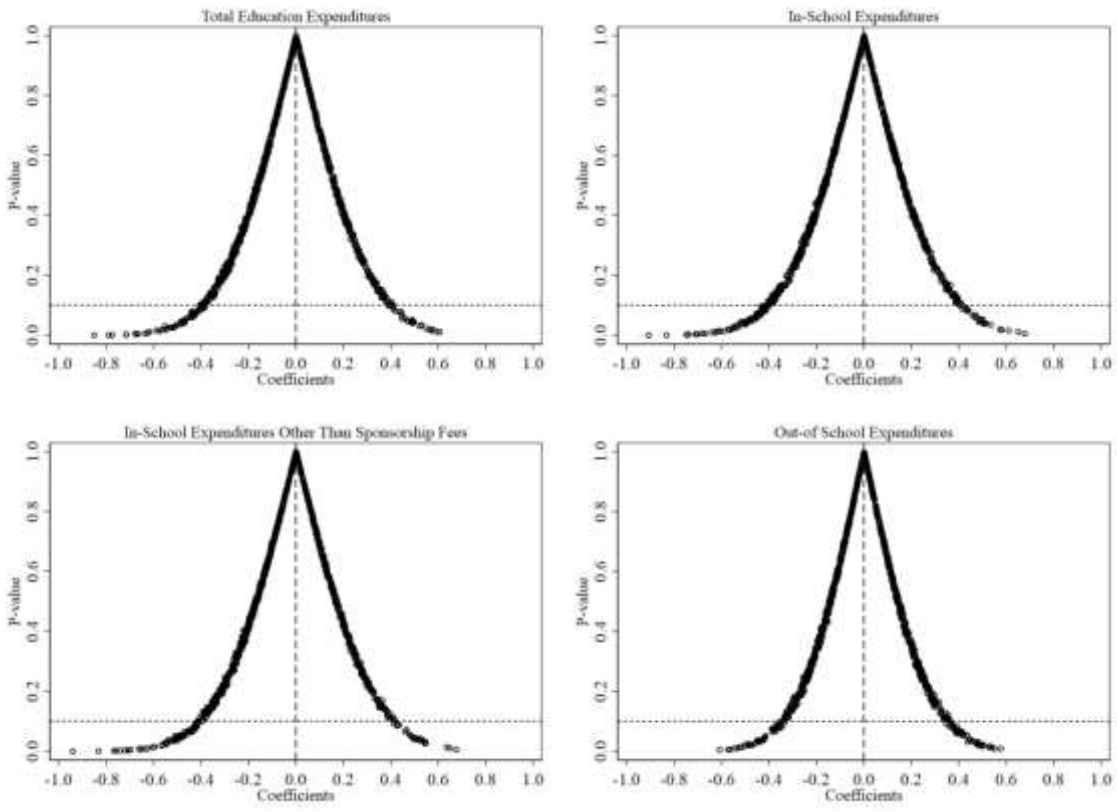



Figure 3. Placebo test

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

### 4.3. Mechanism Analysis

The reform might have affected the educational expenditures of migrant households through three mechanisms, including substitution, flypaper and income effects. This sub-section discusses whether and how they could play a role.

#### 4.3.1 The role of substitution effects: does government invest more?

The relaxation of *Hukou* restrictions, may have promoted more equal access to public education for migrant children. Consequently, the additional educational fees that migrant families may have paid for their children to enter public schools were saved and redirected to other purposes, resulting in a reduction in total investment in education and creating a substitution effect.

To test whether this was the case, we first show 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 a dependent variable. Results show that the reform significantly increased the probability of receiving free public education for migrant children. Appendix Tables A26-27 provide additional evidence that more migrant children were enrolled in public schools after the reform, and the reform significantly decreased the probability of enrolling in a migrant school for migrant children.

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) |
| dindex(log)×post | 0.422*** | 0.272* | 0.470*** | 1.480** | 0.540*** | 1.172*** |
| | (0.144) | (0.145) | (0.164) | (0.719) | (0.160) | (0.424) |
| 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 | 0.215 | 0.027 | 0.018 | 0.130 | 0.316 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

As migrant children were more likely to be enrolled in public schools after *Hukou* reform, we further verified whether *Hukou* reform is associated with higher government expenditure on child education. We collected macro-level information, including 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 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 total education expenditure.

If we combine this with the previous micro-level analysis, it can be seen that although more migrant children have entered the public education system from migrant schools as a result of the reform, there has been 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 economy of scale; that is, it can still absorb more children from the non-local *Hukou* population without increasing overall investment. Another possible reason is that when many non-local *Hukou* population, including rural migrants, enter the public education system, local governments tend to 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 children of both the local and non-local *Hukou* populations. This sent a signal to the non-local *Hukou* population, including rural migrants, highlighting the potential improved quality of their children's education, attracting them to invest more in education to compensate for the lack of government investment and to maintain and improve the overall quality of their children's education.

Nevertheless, local governments could also bring about the phenomenon of rent-seeking in public education organizations, potentially spawning new invisible discrimination. In 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[16]. 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 tricks 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 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 increased commuting costs. Thus, migrant families had to overcome 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 was a waste of the limited resources of migrant families.

Table 6
Effects of *Hukou* Reform on Macro Variables

| VARIABLES | Per Pupil Financial Expenditure | Government Education Expenditure |
|---|---|---|
| | (1) | (2) |
| dindex (log)×post | 0.050 | 0.087 |
| | (0.036) | (0.053) |
| Control variable | Y | Y |
| Time fixed effects | Y | Y |
| City fixed effects | Y | Y |
| SE | robust | robust |
| R-squared | 0.510 | 0.812 |
| Observations | 657 | 657 |

Note: All regressions control for city and year-fixed effects. The control variables used are: number of primary and secondary school students, 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 in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

### 4.3.2 The role of the flypaper effect: do parents have higher expectations for the education of migrant children?

While the substitution effect depends on whether the government invests more in public education following the reform, providing migrant children with free access to the public education system, the flypaper effect relies on the reform to raise the educational expectations of

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

migrant children. As the reform promoted the long-term stability of migrant families in cities, greatly reducing the likelihood of relocation, the higher level of educational returns increased their educational expectations (Mussa, 2013), and 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 the migrant children's educational expectations, both in terms of long-term (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. Appendix Tables A28-29 showed 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 studies showing that those who are less educated tend to invest more in their children (Kenayathulla, 2016; Iddrisu et al., 2018). Furthermore, the reform may have changed the pattern of educational resource allocation based on gender among migrant households (Almond et al., 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 and Kingdon, 2008; Azam and Kingdon, 2013; Vogel and Korinek, 2012). Therefore, it is expected that the reform's effects will be stronger among boys. However, as shown in Appendix Tables A30-31, by adding a gender dummy variable, no significant differences were found for total education expenditure and education expectations. Thus, for traditional rural households that migrated into cities, the *Hukou* reform was conducive to raising the educational expectations in migrant families for girls 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 the poor quality of in-school education. The other is to rely on schools to improve quality through allocating quality teachers, creating a good learning environment, and supplying effective learning materials. Either way, migrant families need to consider signals that characterize quality education when allocating education expenditure, as the prospect of a high-quality education enhances parents' perceived returns and stimulates 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 regarding 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. On the one hand, if migrant families wished to continue to use off-school training to individualize and improve the quality of education, they needed to either pay higher fees or find alternative solutions. However, 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 within public education against children of the non-local *Hukou* population, including the removing 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 children of the non-local *Hukou* population, prompting migrant families to allocate more educational expenditure to in-school learning.

To verify the mechanism, we chose 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. Clearly, the reform significantly boosted the probability of attending key classes for migrant children, signaling that the quality of school education has improved, and thus the increase in total education expenditure due to the reform is concentrated in in-school expenditure. We also verified the above mechanism in terms of the time dimension. Appendix 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 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 to improve the quality of school education became stronger with the advancement of the reform, migrant families significantly reduced their off-school expenditure and allocated them instead to in-school expenditure. In addition, Appendix Table A33 shows that parents tend to urge their children to spend more time on 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 *Hukou* reform, the government did not invest more in public education; rather, it attempted to send a signal of higher educational quality in public schools, which triggered more private investment in the education of migrant children from migrant families. 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. In Column 4 of Table 5, we see an extremely strong positive association between the reform and higher parental income. This is consistent with Song (2014) who found that *Hukou* reform led to higher incomes for migrants through a reduction of migration costs and an improvement in human capital levels, thus allowing more resources to be allocated to the education of their children (i.e., income effect). One concern about this explanation is that migrant workers invest more in their children not because they earn more but potentially because the expenditures on other items (such as on health) decreased. However, the results helped to rule out this concern, showing that the *Hukou* reform did not have a significant effect on health expenditures. (See Appendix Table A34)

Our results show the reform had a significant positive impact of on total education expenditure, indicating that the income and flypaper effects outweigh the possible substitution effect. This finding is in line with the literature on institutional determinants of education (Shi, 2012; Chi and Qian, 2016; Das, 2021). The reform significantly increased the probability of receiving free public education for migrant children, thus satisfying their demand for quantity. While no increase in 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 perceive 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 human capital development and poverty reduction. Further, in most countries undergoing urbanization-driven economic transition, welfare distribution determines the livelihoods and incomes of low-skilled immigrant groups. A fair system of welfare distribution can improve livelihoods and increase investment in the human capital of their offspring, ensuring both the full integration of immigrant families into society and providing a continuous labor supply. As one of the most successful cases in the developing world, China's *Hukou* reform has greatly facilitated equitable access to social benefits for domestic migrant groups, which make up a significant proportion of the population, while increasing labor productivity and the income of migrant families. Nevertheless, few studies have investigated how this systemic change affected the long-term education-based human capital development in families of specific target populations.

To fill this gap in the literature, this study investigated the widely overlooked positive effects of welfare reform on long-term human capital accumulation among migrant families and identified its effects on their investment in education. Specifically, we evaluated the effect 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 flypaper and income effects outweighing the substitution effect. The first point is that 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 non-local *Hukou* populations. Thus, 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, 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 schools 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 higher expenditure on education as migrant families have to pay more to access public education.

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

Our findings 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 has strong practical implications for the development of human capital in migrant children. We show that policymakers must balance providing educational benefits to migrant children and maintaining economic growth.

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# Appendix

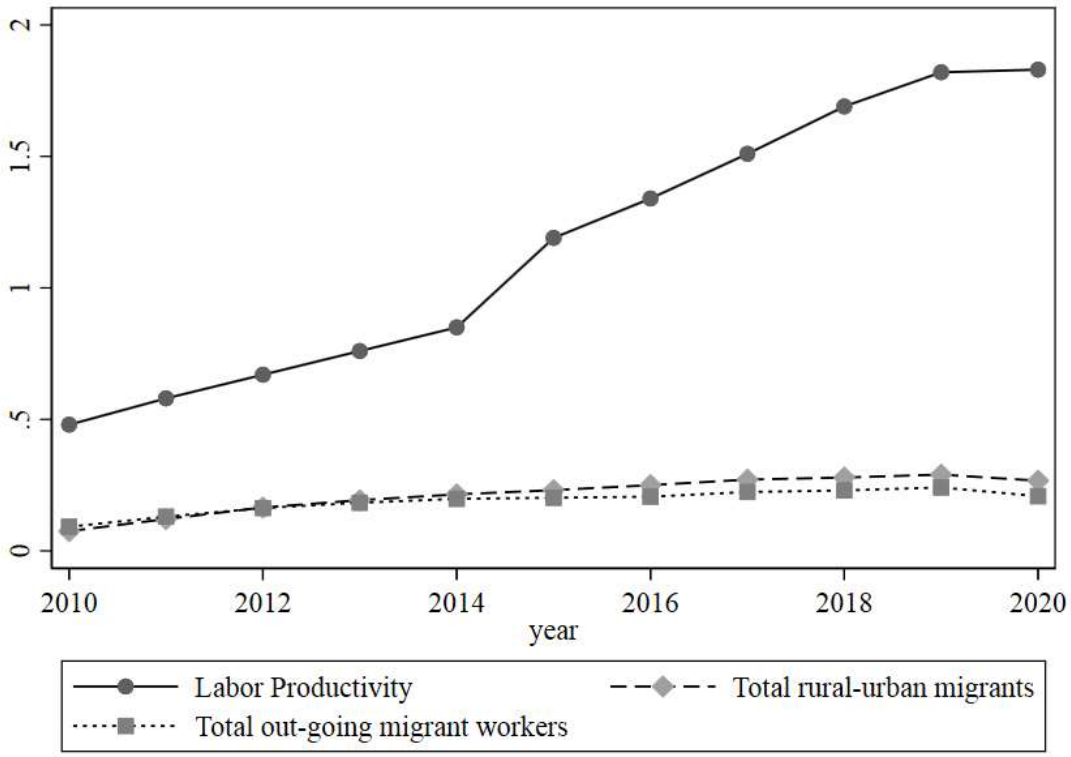


Figure A 1. Growth rate of labor productivity and the number of migrant workers in China, 2010-2020

Note: The graph plots the total number of migrant workers and out-going migrant workers in China's tertiary and secondary 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.

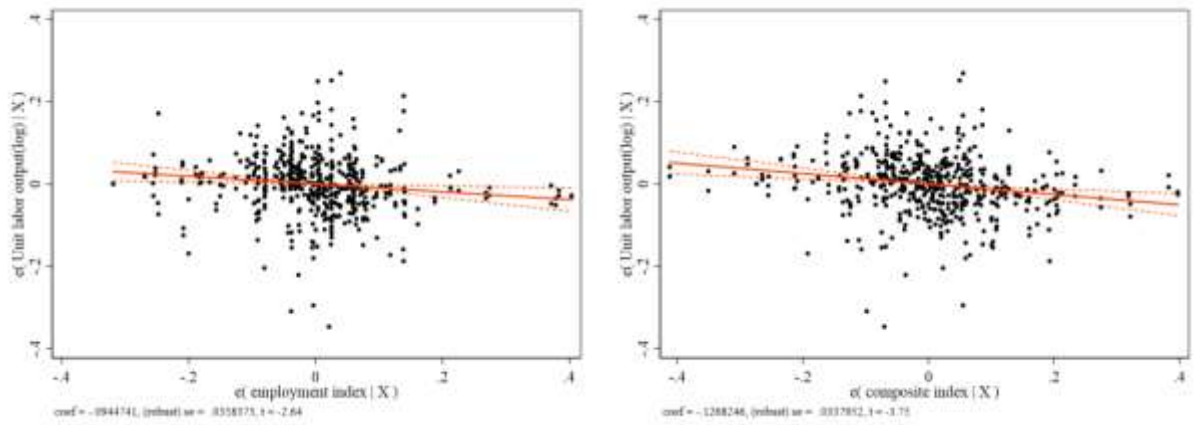


Figure A 2. *Hukou* registration index and unit labor output in the tertiary sector

Note: The left panel shows the relationship between tertiary industry unit labor output and the composite index. The right panel shows the relationship between tertiary industry unit labor output and the ordinary employment index. The regression analysis includes province and year fixed effects. Corresponding estimates are reported in Appendix Table A1. Data source: provincial and municipal statistical yearbooks.

Appendix Table A 1 : Correlation Between *Hukou* Registration Index and Unit Labor Output

| VARIABLES | Unit Labor Output | Unit Labor Output |
|---|---|---|
| | (1) | (2) |
| Ordinary Employment index | -0.094*** | - |
| | (0.035) | |
| Composite index | - | -0.127*** |
| | | (0.034) |
| City fixed effect | Y | Y |
| Time fixed effect | Y | Y |
| SE | robust | robust |
| R-squared | 0.665 | 0.673 |
| Observations | 520 | 520 |

Note: The dependent variable is the unit labor output in the tertiary sector, taken as logarithm. The independent variables in columns 1 and 2 are the ordinary employment and composite indexes, respectively. All regressions control for urban and time fixed effects. Robust standard errors appear in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 2 : 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 | | |

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

Appendix Table A 3 : Summary statistics

| VARIABLES | Mean | S.D. | Mean | S.D. | Mean | S.D. |
|---|---|---|---|---|---|---|
| Panel A：Treatment Cities | Full Sample | | Before | | After | |
| *Hukou* registration Index | 0.368 | 0.124 | 0.421 | 0.170 | 0.314 | 0.165 |
| Total education expenditures ,log | 6.047 | 2.868 | 5.243 | 3.242 | 6.857 | 2.152 |
| In-school expenditures ,log | 5.699 | 2.825 | 5.010 | 3.154 | 6.393 | 2.246 |
| In-school expenditures other than sponsorship fees ,log | 5.660 | 2.832 | 4.994 | 3.145 | 6.331 | 2.291 |
| Sponsorship fees ,log | 0.084 | 0.749 | 0.053 | 0.595 | 0.116 | 0.876 |
| Out-of school expenditures ,log | 1.631 | 3.134 | 1.445 | 2.879 | 1.817 | 3.363 |
| **Household-level control variables** | | | | | | |
| Parental education level | 3.129 | 1.018 | 3.111 | 1.020 | 3.148 | 1.016 |
| Parental age | 36.111 | 5.978 | 34.353 | 5.799 | 37.880 | 5.626 |
| Age of children | 11.180 | 3.432 | 9.414 | 3.082 | 12.960 | 2.789 |
| Family size | 5.380 | 2.155 | 5.388 | 2.161 | 5.373 | 2.151 |
| Parental health status | 2.771 | 0.868 | 2.735 | 0.879 | 2.808 | 0.855 |
| Parental occupation | - | - | - | - | - | - |
| Number of children in school | 1.106 | 0.783 | 0.871 | 0.746 | 1.342 | 0.748 |
| **Regional-level control variables** | | | | | | |
| Marketization index | 6.974 | 1.507 | 6.662 | 1.404 | 7.288 | 1.542 |
| Per Capita GDP ,log | 10.934 | 0.501 | 10.854 | 0.511 | 11.015 | 0.478 |
| Number of Primary and Secondary Student ,log | 22.425 | 39.017 | 18.762 | 17.728 | 26.114 | 52.089 |
| Public budget revenue ,log | 13.206 | 1.087 | 13.010 | 1.079 | 13.402 | 1.059 |
| Public budget expenditure ,log | 13.768 | 0.889 | 13.542 | 0.871 | 13.995 | 0.848 |
| Number of observations | | 1985 | | 996 | | 989 |
| Panel B：Control Cities | Full Sample | | Before | | After | |
| *Hukou* registration Index | 0.653 | 0.192 | 0.634 | 0.167 | 0.671 | 0.303 |
| Total education expenditures ,log | 5.824 | 3.044 | 5.351 | 3.167 | 6.329 | 2.823 |
| In-school expenditures ,log | 5.348 | 2.963 | 5.086 | 3.071 | 5.628 | 2.818 |
| In-school expenditures other than sponsorship fees ,log | 5.328 | 2.962 | 5.078 | 3.064 | 5.594 | 2.826 |
| Sponsorship fees ,log | 0.122 | 0.897 | 0.058 | 0.662 | 0.191 | 1.090 |
| Out-of school expenditures ,log | 1.800 | 3.322 | 1.207 | 2.816 | 2.434 | 3.687 |
| **Household-level control variables** | | | | | | |
| Parental education level | 3.210 | 1.076 | 3.191 | 1.072 | 3.231 | 1.080 |
| Parental age | 36.037 | 5.998 | 34.328 | 5.840 | 37.861 | 5.619 |
| Age of children | 11.306 | 3.373 | 9.541 | 2.919 | 13.191 | 2.748 |
| Family size | 5.156 | 1.694 | 5.224 | 1.743 | 5.082 | 1.638 |
| Parental health status | 2.790 | 0.849 | 2.778 | 0.849 | 2.803 | 0.849 |
| Parental occupation | - | - | - | - | - | - |
| Number of children in school | 1.100 | 0.693 | 0.884 | 0.693 | 1.330 | 0.614 |
| **Regional-level control variables** | | | | | | |
| Marketization index | 7.390 | 1.936 | 6.978 | 1.862 | 7.830 | 1.917 |
| Per Capita GDP ,log | 10.956 | 0.612 | 10.895 | 0.656 | 11.021 | 0.554 |
| Number of Primary and Secondary Student ,log | 46.071 | 49.376 | 43.817 | 45.078 | 48.477 | 53.515 |
| Public budget revenue ,log | 14.069 | 1.885 | 13.932 | 1.855 | 14.215 | 1.907 |
| Public budget expenditure ,log | 14.598 | 1.557 | 14.430 | 1.531 | 14.777 | 1.565 |

| | | | |
|---|---|---|---|
| Number of observations | 1408 | 727 | 681 |

Note: This table reports summary statistics for the dependent variables and principal control variables. Treatment and control cities refer to those with positive and negative differences in the *Hukou* index. 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).

Appendix Table A 4 : 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 | | | | | |
| 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 |

Note: 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 in parentheses are robust standard errors clustered at the individual level. (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Appendix Table A 5 : Regional differences in *Hukou* Registration Index

| | Citys | 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 population size.

Appendix Table A 6 : Descriptive Statistics for Floating Population

| | >5 million | | 3-5 million | | <3 million | |
|---|---|---|---|---|---|---|
| Year | 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** |

Note: This table lists statistics of the floating population by population size.

Appendix Table A 7 : 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) |
| dindex(log)×post | 1.542*** | 1.877*** | -0.902** | -0.062 | 1.873*** |
| | (0.382) | (0.390) | (0.377) | (0.112) | (0.390) |
| 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 | 0.203 | 0.148 | 0.125 | 0.027 | 0.144 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 |

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

Appendix Table A 8 : 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) |
| **Panel A:** *Hukou* registration index difference>0, treatment group; *Hukou* registration index difference<0, control group | | | | | | | | | | |
| treat×post | 0.593*** | 0.593** | 0.752*** | 0.752** | 0.752*** | 0.752** | -0.024 | -0.024 | -0.874*** | -0.874*** |
| | (0.224) | (0.281) | (0.232) | (0.302) | (0.233) | (0.304) | (0.061) | (0.076) | (0.201) | (0.272) |
| 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 | 0.141 | 0.141 | 0.137 | 0.137 | 0.027 | 0.027 | 0.131 | 0.131 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |
| **Panel B:** population <3 million, treatment group; population>=3 million, control group | | | | | | | | | | |
| treat×post | 0.578** | 0.578 | 0.843*** | 0.843* | 0.918*** | 0.918* | -0.193 | -0.193 | -0.570* | -0.570 |
| | (0.282) | (0.454) | (0.293) | (0.472) | (0.292) | (0.467) | (0.118) | (0.204) | (0.308) | (0.424) |
| 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 | 0.196 | 0.196 | 0.139 | 0.139 | 0.136 | 0.136 | 0.031 | 0.031 | 0.124 | 0.124 |
| 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 | 1.553*** | 1.553*** | 1.796*** | 1.796*** | 1.779*** | 1.779*** | 0.0312 | 0.0312 | -0.187 | -0.187 |
| | (0.350) | (0.428) | (0.362) | (0.425) | (0.364) | (0.426) | (0.097) | (0.121) | (0.331) | (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 | 0.204 | 0.204 | 0.151 | 0.151 | 0.145 | 0.145 | 0.033 | 0.033 | 0.122 | 0.122 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: The dependent variables are education expenditures. All regressions control for individual, time, and city fixed effects, as well as household- and region-level control variables. Values in 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 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 9 : 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 |

Note: 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 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1

Appendix Table A 10 : 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 |

Note: 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. Values in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 11 : Robustness Test: *Hukou* Registration Index Using the Equal Weight Method

| 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) |
| dindex(log)×post | 2.625** | 2.625* | 3.257*** | 3.257** | 3.049** | 3.049* | -0.195 | -0.195 | -4.831*** | -4.831*** |
| | (1.174) | (1.551) | (1.230) | (1.646) | (1.188) | (1.564) | (0.418) | (0.479) | (1.116) | (1.486) |
| 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 | 0.196 | 0.196 | 0.139 | 0.139 | 0.128 | 0.128 | 0.032 | 0.032 | 0.131 | 0.131 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: This tables shows the results using ordinary employment index calculated by the equal weight method. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. Values in 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 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 12 : Robustness Tests: Excluding Cities Without *Hukou* Index Information

| 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) |
| dindex(log)×post | 1.262*** | 1.262** | 1.660*** | 1.660*** | 1.610*** | 1.610*** | -0.026 | -0.026 | -1.059** | -1.059* |
| | (0.442) | (0.516) | (0.459) | (0.495) | (0.459) | (0.503) | (0.138) | (0.186) | (0.436) | (0.610) |
| 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 | 0.123 | 0.123 | 0.076 | 0.076 | 0.070 | 0.070 | 0.009 | 0.009 | 0.110 | 0.110 |
| Observations | 2893 | 2893 | 2893 | 2893 | 2893 | 2893 | 2893 | 2893 | 2893 | 2893 |

Note: This Table shows the results using the original sample in which some cities are not matched with the related *Hukou* index. All regressions control for individual, time, city fixed effects, as well as household- and region-levels' control variables. Values in 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 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 13 : 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) |
| dindex(log)×post | 1.408*** | 1.408*** | 1.875*** | 1.875*** | 1.852*** | 1.852*** | 0.005 | 0.005 | -1.157*** | -1.157** |
| | (0.411) | (0.454) | (0.438) | (0.469) | (0.437) | (0.466) | (0.132) | (0.160) | (0.382) | (0.530) |
| 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 | 0.167 | 0.167 | 0.161 | 0.161 | 0.028 | 0.028 | 0.152 | 0.152 |
| Observations | 2672 | 2672 | 2672 | 2672 | 2672 | 2672 | 2672 | 2672 | 2672 | 2672 |

Note: 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. Values in columns 1, 3, 5, 7, and 9 in parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 14 : 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 | | | | | | | | | | |
| dindex(log)×post | 2.183*** | 2.183*** | 2.326*** | 2.326*** | 2.327*** | 2.327*** | 0.031 | 0.031 | -0.560 | -0.560 |
| | (0.537) | (0.586) | (0.525) | (0.638) | (0.525) | (0.640) | (0.151) | (0.147) | (0.449) | (0.741) |
| 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 | 0.210 | 0.210 | 0.154 | 0.154 | 0.150 | 0.150 | 0.027 | 0.027 | 0.123 | 0.123 |
| Observations | 3210 | 3210 | 3210 | 3210 | 3210 | 3210 | 3210 | 3210 | 3210 | 3210 |
| **Panel B:** Delete first-tier cities | | | | | | | | | | |
| dindex(log)×post | 1.910*** | 1.910** | 2.050*** | 2.050** | 2.113*** | 2.113** | -0.216 | -0.216 | -1.713*** | -1.713** |
| | (0.652) | (0.845) | (0.690) | (0.942) | (0.692) | (0.956) | (0.219) | (0.184) | (0.575) | (0.840) |
| 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 | 0.156 | 0.156 | 0.152 | 0.152 | 0.030 | 0.030 | 0.128 | 0.128 |
| Observations | 3176 | 3176 | 3176 | 3176 | 3176 | 3176 | 3176 | 3176 | 3176 | 3176 |

Note: 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. Values in 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 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 15 : 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) |
| dindex(log)×post | 1.338*** | 1.338** | 1.747*** | 1.747*** | 1.725*** | 1.725*** | -0.028 | -0.028 | -0.980** | -0.980* |
| | (0.407) | (0.516) | (0.415) | (0.499) | (0.415) | (0.499) | (0.119) | (0.143) | (0.395) | (0.557) |
| 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 | 0.144 | 0.144 | 0.136 | 0.136 | 0.030 | 0.030 | 0.133 | 0.133 |
| Observations | 2953 | 2953 | 2953 | 2953 | 2953 | 2953 | 2953 | 2953 | 2953 | 2953 |

Note: 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. Values in 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 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 16 : Robustness Test: Data Attrition

| VARIABLES | Samples |
|---|---|
| | (1) |
| dindex(log)×post | -0.030 |
| | (0.047) |
| Control variable | Y |
| Individual fixed effects | Y |
| Time fixed effects | Y |
| City fixed effects | Y |
| SE | robust |
| R-squared | 0.176 |
| Observations | 3393 |

Note: 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 ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 17 : Robustness Test: Effects of *Hukou* Reform on Whether Children Migrate

| VARIABLES | Full sample | Rural Migrant Sample |
|---|---|---|
| | (1) | (2) |
| dindex(log)×post | 0.006 | 0.011 |
| | (0.024) | (0.030) |
| 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 |

Note: 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 ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 18 : 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) |
| dindex(log)×post | 0.006 | -0.011 | 0.033 | 0.224** | 0.042 | 4,032.750 | 0.187 |
| | (0.031) | (0.016) | (0.148) | (0.092) | (0.099) | (2,863.260) | (0.120) |
| 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 | 0.996 | 0.037 | 0.875 | 0.096 | 0.060 | 0.211 | 0.307 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

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

Appendix Table A 19 : 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) |
| dindex(log)×post | 0.732* | 0.825* | 0.824* | -0.007 | -0.716* |
| | (0.430) | (0.458) | (0.479) | (0.092) | (0.377) |
| 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 | 0.187 | 0.136 | 0.130 | 0.055 | 0.124 |
| Observations | 6713 | 6713 | 6713 | 6713 | 6713 |

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

Appendix Table A 20 : 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) |
| dindex(log)×year12 | -0.026 | -0.026 | -0.146 | -0.146 | -0.116 | -0.116 | 0.001 | 0.001 | 0.279 | 0.279 |
| | (0.267) | (0.256) | (0.274) | (0.277) | (0.273) | (0.278) | (0.087) | (0.092) | (0.301) | (0.283) |
| dindex(log)×year16 | 0.847*** | 0.847* | 0.811*** | 0.811** | 0.845*** | 0.845* | -0.015 | -0.015 | -0.372 | -0.372 |
| | (0.270) | (0.480) | (0.276) | (0.411) | (0.279) | (0.445) | (0.091) | (0.108) | (0.312) | (0.436) |
| dindex(log)×year18 | 0.550* | 0.550 | 0.685** | 0.685 | 0.670** | 0.670 | 0.004 | 0.004 | -0.867** | -0.867** |
| | (0.318) | (0.451) | (0.333) | (0.583) | (0.332) | (0.580) | (0.135) | (0.143) | (0.376) | (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 | 0.187 | 0.187 | 0.136 | 0.136 | 0.130 | 0.130 | 0.055 | 0.055 | 0.125 | 0.125 |
| Observations | 6713 | 6713 | 6713 | 6713 | 6713 | 6713 | 6713 | 6713 | 6713 | 6713 |

Note: 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. Rows 1-3 show the cross-term coefficients of the *Hukou* registration index and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 in parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 21 : 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) |
| dindex(log)×post | 1.164*** | 1.494*** | 1.485*** | -0.102 | -0.894* |
| | (0.436) | (0.446) | (0.449) | (0.122) | (0.489) |
| 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 | 0.203 | 0.156 | 0.150 | 0.035 | 0.122 |
| Observations | 4559 | 4559 | 4559 | 4559 | 4559 |

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

Appendix Table A 22 : 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) |
| dindex(log)×year12 | -0.363 | -0.363 | -0.434 | -0.434 | -0.398 | -0.398 | -0.146 | -0.146 | 0.172 | 0.172 |
| | (0.369) | (0.398) | (0.382) | (0.443) | (0.381) | (0.445) | (0.090) | (0.090) | (0.352) | (0.282) |
| dindex(log)×year16 | 1.162*** | 1.162* | 1.316*** | 1.316** | 1.327*** | 1.327*** | -0.276** | -0.276** | -0.779* | -0.779 |
| | (0.409) | (0.628) | (0.414) | (0.509) | (0.414) | (0.507) | (0.110) | (0.138) | (0.407) | (0.598) |
| dindex(log)×year18 | 0.771* | 0.771* | 1.262*** | 1.262** | 1.264*** | 1.264** | -0.025 | -0.025 | -0.862* | -0.862* |
| | (0.432) | (0.442) | (0.467) | (0.526) | (0.467) | (0.530) | (0.119) | (0.121) | (0.469) | (0.442) |
| 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 | 0.156 | 0.156 | 0.151 | 0.151 | 0.037 | 0.037 | 0.122 | 0.122 |
| Observations | 4559 | 4559 | 4559 | 4559 | 4559 | 4559 | 4559 | 4559 | 4559 | 4559 |

Note: 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. Rows 1-3 show the cross-term coefficients of the *Hukou* registration index and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 in parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 23 : 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) |
| dindex(log)×post | 1.471*** | 1.797*** | 1.783*** | -0.106 | -0.853 |
| | (0.449) | (0.448) | (0.450) | (0.134) | (0.528) |
| dindex(log)×post×lb | -2.373*** | -2.337*** | -2.305*** | 0.035 | -0.312 |
| | (0.797) | (0.812) | (0.809) | (0.153) | (0.960) |
| 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 | 0.206 | 0.159 | 0.153 | 0.035 | 0.122 |
| Observations | 4559 | 4559 | 4559 | 4559 | 4559 |

Note: The lb variable represents whether children are left-behind children. 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).

Appendix Table A 24 : 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) |
| dindex(log)×post | -0.116 | -0.314 | -0.302 | 0.050 | -0.355 |
| | (0.504) | (0.560) | (0.609) | (0.154) | (0.463) |
| 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 | 0.162 | 0.125 | 0.121 | 0.122 | 0.151 |
| Observations | 2172 | 2172 | 2172 | 2172 | 2172 |

Note: The sample refers to children with urban *Hukou*. All regressions control for individual, year, and city fixed effects. Values in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 25 : Robustness Test: Time Effects of *Hukou* Reform on Urban Sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| dindex(log)×year12 | 0.569 | 0.569 | 0.580 | 0.580 | 0.604 | 0.604 | 0.169 | 0.169 | -0.040 | -0.040 |
| | (0.416) | (0.417) | (0.434) | (0.481) | (0.431) | (0.475) | (0.169) | (0.165) | (0.583) | (0.525) |
| dindex(log)×year16 | 0.281 | 0.281 | 0.164 | 0.164 | 0.250 | 0.250 | 0.203 | 0.203 | 0.065 | 0.065 |
| | (0.405) | (0.405) | (0.412) | (0.456) | (0.424) | (0.541) | (0.171) | (0.184) | (0.609) | (0.605) |
| dindex(log)×year18 | 0.008 | 0.008 | -0.281 | -0.281 | -0.336 | -0.336 | 0.042 | 0.042 | -0.931 | -0.931 |
| | (0.519) | (0.826) | (0.530) | (0.963) | (0.531) | (0.969) | (0.276) | (0.281) | (0.697) | (0.624) |
| 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 | 0.163 | 0.163 | 0.126 | 0.126 | 0.123 | 0.123 | 0.123 | 0.123 | 0.152 | 0.152 |
| Observations | 2172 | 2172 | 2172 | 2172 | 2172 | 2172 | 2172 | 2172 | 2172 | 2172 |

Note: 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. Rows 1-3 show the cross-term coefficients of the *Hukou* registration index and dummy variables for 2012, 2016, and 2018, respectively. Values in columns 1, 3, 5, 7, and 9 in parentheses are robust standard errors clustered at the individual level, while values in columns 2, 4, 6, 8, and 10 in parentheses are robust standard errors clustered at the city level. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Appendix Table A 26 : 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 |

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

Appendix Table A 27 : 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) |
| dindex(log)×post | -0.253*** | -0.253*** | 0.005 | 0.005 | -0.001 | -0.001 |
| | (0.030) | (0.040) | (0.003) | (0.004) | (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 |
| SE | robust | cluster | robust | cluster | robust | cluster |
| R-squared | 0.232 | 0.232 | 0.081 | 0.081 | 0.999 | 0.999 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: 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 in parentheses are robust standard errors clustered at the individual level. Values in columns 2, 4, 6 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 28 : 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) |
| dindex(log)×post | 2.044*** | 2.044*** | 2.152*** | 2.152*** | -0.351 | -0.351 |
| | (0.417) | (0.515) | (0.414) | (0.476) | (0.422) | (0.706) |
| dindex(log)×post×edu | -2.731*** | -2.731*** | -2.118* | -2.118* | -1.227 | -1.227 |
| | (0.971) | (0.981) | (1.138) | (1.091) | (0.915) | (1.175) |
| 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 | 0.172 | 0.172 | 0.121 | 0.121 | 0.117 | 0.117 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: 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. Values in columns 1, 3 and 5 in parentheses are robust standard errors, while values in columns 2, 4 and 6 in parentheses are robust standard errors clustered at the city level. (***p < 0.01, **p < 0.05, *p < 0.1).

Appendix Table A 29 : 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) |
| dindex(log)×post | 2.973** | 2.973** | -0.010 | -0.010 |
| | (1.219) | (1.223) | (0.142) | (0.150) |
| dindex(log)×post×edu | -4.749*** | -4.749*** | 0.152 | 0.152 |
| | (1.673) | (1.639) | (0.175) | (0.234) |
| 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 | 0.045 | 0.045 | 0.043 | 0.043 |
| Observations | 3393 | 3393 | 3393 | 3393 |

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

Appendix Table A 30 : 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")** | | | | | |
| dindex(log)×post | 1.602*** | 2.232*** | 2.171*** | 0.050 | -0.351 |
| | (0.524) | (0.537) | (0.536) | (0.164) | (0.528) |
| dindex(log)×post×gender | -0.291 | -1.107 | -1.037 | -0.171 | -0.633 |
| | (0.741) | (0.743) | (0.744) | (0.232) | (0.732) |
| 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 | 0.200 | 0.147 | 0.140 | 0.033 | 0.127 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 |

Note: 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).

Appendix Table A 31 : 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")** | | | | |
| dindex(log)×post | 0.020 | 0.020 | 0.880 | 0.880 |
| | (0.126) | (0.147) | (1.259) | (1.244) |
| dindex(log)×post×gender | -0.040 | -0.040 | -1.789 | -1.789 |
| | (0.167) | (0.233) | (1.714) | (2.017) |
| 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 | 0.046 | 0.046 | 0.045 | 0.045 |
| Observations | 3393 | 3393 | 3393 | 3393 |

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

Appendix Table A 32 : Effects of *Hukou* Reform on Key Classes: Time dimension

| VARIABLES | Key Classes |
|---|---|
| | (1) |
| dindex(log)×year12 | 0.230 |
| | (0.167) |
| dindex(log)×year16 | 0.460** |
| | (0.198) |
| dindex(log)×year18 | 0.626*** |
| | (0.235) |
| Control variable | Y |
| Individual fixed effects | Y |
| Time fixed effects | Y |
| City fixed effects | Y |
| SE | robust |
| R-squared | 0.129 |
| Observations | 3393 |

Note: 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).

Appendix Table A 33 : 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) |
| dindex(log)×post | -2.207*** | 1.140*** | 0.754*** | 1.894*** | 1.184** |
| | (0.849) | (0.344) | (0.258) | (0.501) | (0.473) |
| 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 | 0.122 | 0.164 | 0.100 | 0.164 | 0.094 |
| Observations | 3393 | 3393 | 3393 | 3393 | 3393 |

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

Appendix Table A 34 : Effects of *Hukou* Reform on Medical Expenditure

| VARIABLES | Medical Expenditures | |
|---|---|---|
| | (1) | (2) |
| dindex(log)×post | -0.208 | -0.208 |
| | (0.249) | (0.309) |
| Control variable | Y | Y |
| Individual fixed effects | Y | Y |
| Time fixed effects | Y | Y |
| City fixed effects | Y | Y |
| SE | robust | cluster |
| R-squared | 0.056 | 0.056 |
| Observations | 3393 | 3393 |

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