

Helping Your Children Soar: Does Public Education Provision Affect Private Expenditure on Children?

Pei Gao, Yiqing Lü, Xin Zhou¹

Abstract

This paper studies how parental investment in children responds to changes in public educational opportunities. Using transactions made via China UnionPay bankcards, we identify two types of expenditure on children: extra-curriculum training and other child support. We exploit a quasi-experimental shock on public educational opportunities – a merger between two districts with substantially different educational resources in Shanghai. The merger allows students to apply for schools across the two districts. Using a difference-in-differences estimation, we find that, compared to a third control district, cardholders in the district with fewer high-performing public schools spent more on children after the merger. The effect is stronger for cardholders who have children of pre-high-school age and for those who live closer to the old border, but disappears for those who have adult children. Lastly, we analyze the implication of the merger on social inequality and examine several alternative explanations. Overall, our findings suggest that parents invest more in children once the opportunity to obtain high-quality public education is improved. Thus, equal allocation of public educational resources could encourage private parental investment in children.

Keywords: parental investment, child-related expenditure, public education, district merger

JEL Classification: H31, H41, J13, R53, D12

¹ Pei Gao, New York University Shanghai, p.gao@nyu.edu; Yiqing Lü, New York University Shanghai, yiqing.lu@nyu.edu; Xin Zhou, New York University Shanghai, xinzhou@nyu.edu. For helpful comments, we thank Kaiji Chen, Raquel Fernandez, Hans-Martin von Gaudecker, Nina Guyon (discussant), Steven Lehrer, Yu-Hsiang Lei, Andres Liberman, Stijn Van Nieuwerburgh, Ryo Okui, Daniel Xu, Xiaoyun Yu, and seminar participants at the 4th Fanhai Economics and Finance workshop, NYU Shanghai, NYU Stern China Initiative Research Luncheon, Shanghai University of Finance and Economics, Fudan University, and the Asian Bureau of Finance and Economic Research 2019, and Econometric Society (Asian Meeting). We are grateful to Feng Ding, Minwei Tang and Lei Zheng at China UnionPay Advisors and Yun Dai for research support. Any errors are attributable solely to the authors.

1. Introduction

Parental investment constitutes an indispensable part of human capital development in children. The existing literature provides extensive evidence that parental investment in varying forms is vital for shaping children's long-term outcomes in health, cognitive and non-cognitive skills, education attainment, and income (Carneiro and Heckman, 2003; Driessen, Smit, and Slegers, 2005; Cunha, Heckman, Lochner, and Masterov, 2006; Sacerdote, 2007). It is therefore of paramount importance to understand the incentives for parental investment in children. Extensive literature shows that access to high-performing schools and good neighborhoods has a positive impact on children's outcomes (Chetty et al., 2016; 2018; Chyn, 2018; Lavy, 2010; Deming et al., 2014). Thus, perceived opportunities to obtain such access may influence parental investment decisions. There, however, exists an alarming disparity in educational opportunities. One important source of such inequality arises from the uneven provision of public education. For examples, school zones vary substantially in education quality (Coate and Knight, 2007). In this paper, we study how parental investment in children responds to changes in children's opportunities to obtain public education of high-quality.

A large number of studies focus on the direct impact of public education on children, the indirect impact that is mediated through parental investment is largely underexplored and also theoretically unclear. The provision of public education can either promote or crowd out private parental investment in children. If the effect of public investment in child outcome is additive to that of parental investment (Becker, 1981; Becker and Tomes, 1976 and 1986), then the two are substitutes, whereas if the effect of public investment in child outcome is multiplicative to that of parental investment (Goldberger, 1989), then the two are complementary. In other words, changes in the rate of return of parental investment that result from the provision of public education could either amplify or reduce parental investment in children.

Understanding how parental investment in children responds to changes in public educational opportunities is also of normative significance, especially given the fact that governments worldwide face budget constraints that limit the size and distribution of educational resources (Fryer and Katz, 2013). Despite its theoretical importance and relevance for policy, the debate on the interplay of these two types of input of child development is still ongoing, since empirical work, if any, is scarce and inconclusive. The empirical research in this literature presents two major challenges. First, parental investment in children varies in its form and is

hard to observe. Current studies focus on time investment in children, which often involves parental behaviours at home and in schools to support children's academic performance and social competence. For example, Pop-Eleches and Urquiola (2011), using Romanian data, find that students who attend higher-achievement schools receive less homework-related help from their parents than other children. In contrast, Gelber and Isen (2013) show that the Head Start program in the US causes a substantial increase in parents' involvement with their children. Unfortunately, one of the most important forms of parental investment, monetary spending on children, is largely overlooked in the extant literature due to the difficulty of observing micro-level categories of household expenditure.²

To identify parental monetary investment in children, this paper uses the credit and debit card transactions of China UnionPay to trace child-related expenditure. Since UnionPay is the only interbank payment network in China that intermediates all card-based expenditures, all the spending on children in China via bankcards enters into our data. Transaction data features two important advantages over consumer expenditure surveys. First, card transactions reflect real-time purchases as opposed to survey respondent's self-reported spending in the past that might be biased and inaccurate. Second, transaction data is of high frequency, which enables researchers to study immediate changes in spending. Specifically, in this paper we divide child-related expenditure into two categories: extra-curriculum (EC) spending and other child support (OCS). Utilizing transaction information about merchant categories, we define EC as payment to private institutions that provide extra-curriculum training for children, and OCS as expenditure on child clothing, toys, zoo visits, etc. We believe that EC captures the parental investment in children's tangible skills that directly contribute to their school outcomes, whereas OCS reflects parental investment in children's intangible skills, which are more likely to be associated with children's social and emotional competence, such as self-confidence and communication skills.

The second empirical challenge lies in isolating the causal effects of access to high-performing schools on parental behaviour, because most people choose where they live carefully. For example, parents who value education highly are likely to both spend more on children and live in places with better public educational resources. To facilitate a causal

²Using data from Zambia and India, Das et al. (2011) find that household spending substantially offsets variations in predictable school grants, but unpredictable grants have no impact on household spending.

interpretation, we exploit an exogenous shock to students' access to public education: a merger between two districts in Shanghai, Jing'an (JA) and Zhabei (ZB). This shock has several appealing features that make it a suitable setting for answering our research question. First, school district policies in China forbid students to apply for high schools outside their own district. Before the merger, JA was endowed with significantly better educational resources than ZB, therefore the opportunity to get into a good high school was slimmer in ZB. The merger lifted the barriers so that students could apply for schools across these two districts. Given the previous gap in education resources, the merger markedly opened up schools choices for ZB students, therefore the chance of getting into a high-performing high school was improved. Second, different from school district mergers in many other countries that are usually influenced by voters in these affected districts (Coate and Knight, 2007; Gordon and Knight, 2008), the merger in Shanghai was a centralized political decision that was intended to increase the land supply of JA district rather than to improve the educational resources in ZB.

We construct a panel dataset that consists of the monthly expenditure of 1,300,690 bankcards for 12 months, 6 months before and 6 months after the merger. The average total expenditure per card per month is 1,897 RMB. The average EC expenditure on children is 10 RMB and that for OCS is 2 RMB. Expenditure on children in the full sample is low because childless cardholders are also included. Thus, we create a subsample by including only cardholders who are identified as having pre-high-school children. In this subsample, the average child-related expenditure is 468 RMB per month, which accounts for 24% of the total expenditure of a card. The data shows that parents tilt heavily toward investing in children's tangible skills that directly contribute to academic performance: EC spending accounts for 22% of the total expenditure, whereas OCS accounts for only 2%.

This paper employs a difference-in-differences strategy (DID) by including a third district, Huangpu (HP), as the control group. As another inner ring district in Shanghai, HP is bordered with both JA and ZB and is not affected by the merger. Our baseline estimation compares the logarithm of monthly expenditure on children of ZB cardholders to that of HP cardholders before and after the merger. To control for all unobservable characteristics of cardholders that may confound with the change in expenditure patterns, we include card fixed effects in all specifications. We find that cardholders in ZB spent 2.6% more than those in HP on children after the merger. More specifically, EC went up by 0.5%, and OCS by 2.1%. Unlike child-related

expenditure, total spending in ZB did not change relative to that in HP, implying that ZB cardholders had to cut down other spending to increase their expenditure on children. The fact that we do not detect any significant increase in total expenditure also suggests that the effect of the merger was confined to parental investment in children. We also use a more flexible specification to evaluate the dynamic effect of the merger. First, we do not detect any persistently different trends in total expenditure, EC, or OCS before the merger, which indicates a parallel trend leading up to the merger. Second, we find that the increase in child expenditure is immediate and long-lived.

Because only cardholders who have children of pre-high-school age are directly affected, our baseline specification captures intent-to-treat (ITT) estimates. To better gauge the treatment effect, we perform the same DID analysis within the subsample of cardholders that are identified as having children of pre-high-school age. The results are consistent with what we find in the full sample but with larger magnitudes: that ZB cardholders with young children spent 15% more in EC and 14% more in OCS than HP cardholders with young children but no difference was made to total expenditure. We then confirm that the effect of the merger was indeed significantly greater for cardholders with young children by running a cross-sectional analysis using the full sample.

Next, we explore a second dimension of heterogeneity, i.e., distance to the old border. We examine whether the effect of the merger on expenditure varies with residential location, since daily commuting time is an important concern for school choices. Naturally, cardholders in ZB who live closer to the old border should have a greater incentive to send their children to high-performing schools that are located in the old JA than those who live farther away.³ We identify cardholders who are likely to live within 2 km of two underground stations in ZB, one closest to and the other farthest from the old border. We find that after the merger the cardholders who lived farthest away from the border indeed spent less on children than did those who lived closest to the border. In this analysis, we also do not find any significant differences in total expenditure.

To understand the implication of the merger on social inequality, we examine another heterogeneous effect depending on income. We use average monthly total expenditure per bankcard before the merger as a proxy for cardholder's personal income. We show that after the

³ In JA, all the top-tier high schools are non-boarding.

merger the poorer cardholders increased their spending on EC, while the wealthier cardholders spent more on OCS. This finding implies that different social classes responded to the perceived improvement in public educational opportunities differently. The rich perhaps have already invested sufficiently in their children's tangible skills before the merger; thus, they responded to by increasing only OCS spending. The poorer cardholders, due to financial constraint, prioritized their investment in EC that can directly link to children's academic outcomes. This interesting finding suggests that the merger could lower the inequality in parental investment in children's tangible skills, in contrast may widen the gap in intangible skills investment across different income groups.

Taken together, we consistently show that improved opportunities to obtain high-quality public education encouraged parental investment in children in ZB, as it could increase parents' perceived return on private investment in children. It, however, could affect household spending through other alternative mechanisms.

First, the merger has intensified competition among students, which could also generate more investment in children. We conduct several tests to examine the competition channel. We first show that the merger introduced more competition for the JA students than it did for the ZB students. As noted above, ZB had fewer top-tier high schools per capita than JA before the merger; consequently, the average admission score was higher in ZB. This gap in the admission score narrowed after the merger, suggesting that the competition to get into top-tier schools in ZB became easier. Consistently, the direction of student flows across the two districts after the merger also verifies this finding. Furthermore, we compare the change in child-related expenditure of cardholders in JA to that of cardholders in HP. We find no significant change in EC, and a significant but a very moderate increase in OCS (only 30% of the magnitude for ZB cardholders) after the merger. If intensified competition is the primary channel that drives our results, we should expect to see a similar or even greater effect on the child-related expenditure of JA cardholders. Thus, our previous findings is unlikely to be entirely driven by more intensified competition among students after the merger.

A second potential mechanism is that the merger could have led to housing appreciation, thereby generating a positive wealth shock for ZB residents. By looking at real estate data, we find neither the price nor the transaction volume of residential apartments in ZB after the merger experienced any noticeable increases than the HP district. In addition, if potential gains in

housing wealth could explain the increase in spending on children after the merger, we should expect to see an overall positive impact on total expenditure rather than only increases in expenditure on children. As discussed, across specifications we do not detect any significant changes in total expenditure. Finally, we identify cardholders who owned apartments in their own districts and find that their expenditure on children did not change significantly compared to those without apartments after the merger. In short, housing appreciation cannot be the primary mechanism through which the merger affected parental investment.

Third, it is also possible that the merger incentivised households in ZB with lower income to keep up their consumption level relative to households in JA that are wealthier on average. If “keeping up with the Joneses” type of preference (Abel, 1990) drives our baseline result, according to this theory, the effect should decline with wealth. In other words, richer cardholders in ZB should be less driven to keep up with cardholders in JA than should poorer cardholders in ZB. In contrast, we find that the increase in OCS spending is more pronounced among the richer cardholders and that EC spending is not sensitive to personal wealth. These findings do not align with the preference-based explanation.

We also conduct several robustness checks to verify our results. First, to further isolate the effect of the merger on parental investment, we run a placebo test on cardholders whom we identify as parents with adult children (those who before the merger made payments to vocational schools after junior high school or universities), and the effect disappears. It suggests that the effect of the merger is likely to be specific to cardholders who have children of pre-high-school age. Furthermore, we discuss the possible issue of one person owning multiple cards. It is difficult to interpret the economic magnitudes of our baseline results (2.6% in the full sample and 28.5% in the young children subsample) at the individual level because one person could own several cards. To address this concern, we repeat the analysis using a subsample that includes debit cards only. Underlying this exercise is the common notion that people have greater incentive to own multiple credit cards in order to earn bonus points and repay the overdraft on other credit cards than debit cards. For the debit card subsample, we consistently find a significant increase in child-related expenditure (1.11% in the full sample and 40.08% the young children subsample).

To the best of our knowledge, this paper presents the first empirical evidence of parental pecuniary investment in children. By exploiting a universal bankcard transaction data in

Shanghai, we are able to construct two types of parental investment in children – tangible and intangible investment. The extant literature mainly focuses on parental time involvement with children in response to a change in the provision of public education, and reaches mixed conclusions (Pop-Eleches and Urquiola, 2011; Gelber and Isen, 2013). Based on the setting of a district merger in China that promotes equal access to high-performing public schools, we show that monetary investment made by parents responds positively to the provision of public education. Thus, this paper also sheds light on a long-standing theoretical debate (Becker, 1981; Becker and Tomes, 1976 and 1986; Goldberger, 1989) over the interplay between public and private investment in children by finding evidence that supports the complementarity effect.

Second, this paper contributes to the literature that studies the decision making of parental investment in children. Different from existing literature that explores the setting of lotteries determining a material improvement in educational quality and the setting of providing education voucher to disadvantaged households (Currie and Thomas, 1995; U.S. Department of Health and Human Services, 2010; Gelber and Isen, 2013; Chetty, Hendren, and Katz, 2016);⁴ the merger in this paper neither directly subsidizes parents nor guarantees students' attendance to better schools. Rather, it only improves the chance of children to attend high-performing schools, therefore increasing parents' perceived returns to the investment in their children. Our results indicate that simply improving the perceived educational opportunities could stimulate private investment in children.

Our paper also adds to the literature that studies the optimal size of a school district. School district consolidation is prevalently observed in the global context. For instance, the number of school districts in the United States has shrunk from about 117,000 to about 15,000 today. Extensive studies focus on the analysis of the same trade-off arising from a school district merger – reaching economies of scale versus retaining local control over school quality – and report mixed findings on its full impact on students' long-term outcomes (Andrews, Duncombe, and Yinger, 2002; Heinesen, 2002; Gordon and Knight, 2008). Our result that equalizing education resources stimulates private parental investment in children who were initially disadvantaged alludes to implications of education resource consolidation above and beyond the commonly analyzed trade-off.

⁴ There are different social programs, such as Moving to Opportunities and Head Start in the US.

More broadly, this paper speaks to the literature studying financing decisions of human capital investment, majority of which focus on college students. People use both private and public credit to finance their investment in education. Lochner and Monge-Naranjo (2011) study the interaction of government and private lending and find that expansions of government student loan programs only partially crowd-out private lending; while our paper shows an increase in private investment in children as a response to better provision of public education. The comparison of these findings indicate that private investment respond stronger in the early stage of human capital development, which could potentially due to a higher marginal return to education at this stage (Cunha, Heckman, Lochner, and Masterov, 2006).

The interesting features of our setting generate several policy implications that are of wide applications. First, the merger is characterized by effectively a *redistribution* policy that equalizes the educational resources across two districts; thus, it requires no additional fiscal subsidies. Second, rather than getting an actual treatment of winning a ticket to high-performing schools, the merger in our setting offers no guarantee for future enrolment but only an improved opportunity.⁵ We show that parental investment also responds to a mere increase in the chance of better education. As the fiscal constraint on public education has always been a challenge worldwide that inhibits education development, our study provides what might be a practical and cost-effective solution to alleviate such constraint. By building a more egalitarian and meritocratic education system, governments may reap the benefit of stimulating parental investment in children.

Our paper proceeds as follows. Section 2 details the context of the merging of districts in Shanghai in 2015. Section 3 describes our data. Section 4 outlines our econometric framework and results. We provide several robustness tests in Section 5 and discuss alternative mechanisms in Section 6. Section 7 concludes.

2. Institutional Background

This paper exploits a quasi-experimental shock to students' access to public education – the district merger in Shanghai – to examine how parental investment in children respond to changes in public education provision. On September 7, 2015, Shanghai Municipal Government suddenly

⁵ We observe increases in expenditure on children after the merger in both districts, albeit more moderate ones in JA. We interpret the increase in ZB as mainly driven by improved educational opportunities and the increase in JA by more competition.

announced that districts Jing'an (JA) and Zhabei (ZB) would be merged into a new district named after the former, effective from November 2015. The old JA district used to be the smallest district in Shanghai with a rich cultural heritage and concentrated high-end shopping centers. It was generally regarded as one of the most affluent districts in the city.⁶ In contrast, ZB district, also located inside the inner ring, had a humbler economy over a larger area. A map of the inner ring districts in Shanghai is presented in the Appendix (Figure 1). In 2015, the old JA had a resident population of 2,486,000, a land area of 7.62 km² and a GDP of 73.2 billion RMB, compared to ZB's resident population of 8,485,000, a land area of 29.26 km² and a GDP of 74.8 billion RMB.

Unlike school district mergers studied in the literature that are usually an endogenous decision made by multiple partners (Gordon and Knight, 2008; Coate and Knight, 2007),⁷ this merger in Shanghai was a purely centralized political decision seeking potential economic synergies. The government officially claimed that the merger was to "improve the city's layout, making administrative divisions better suited for the city's development, lift the urban function, and promote sustainable regional development." The general interpretation and conjecture of the official statement were that this merger intended to increase the land supply of JA district and to upgrade the use of land in ZB district in the hope of increasing revenues source for local governments.⁸

Before the merger, most of the local public services, such as hospitals, were not exclusive to local residents, with one exception, i.e., the access to public high schools.⁹ Public high school admission in Shanghai is based on students' performance in a city-level standard entrance examination, but the selection is limited within each district. In other words, students can only apply for high schools within their own district. We offer an illustration of the system of public school entrance in Shanghai in the Appendix (Figure 2). The merger between JA and ZB lifted the admission barrier and allowed students to apply for high schools across the two districts; thus, both students and schools had a larger pool to apply for and select from.

⁶ Shanghai is divided into 16 districts, 7 of which are considered downtown districts (located within the inner ring).

⁷ Studies working in the US context show that mergers must be approved by voters in both districts.

⁸ Under the Chinese fiscal system, one of the most important revenue resources for municipal government is the revenue from land sales. For Shanghai city, about 30 % of its revenues in 2015 came from land sales. Market prices for urban land are governed by a rating system with 10 ranks; land with a higher rank is associated with a higher price. Land in the old JA district ranks higher than that in ZB, and a district merger could upgrade old ZB land, thus generating higher revenue for the local government.

⁹ The school zone of public primary and junior high schools is divided on the basis of a smaller unit, e.g. at postcode level, therefore the district merger did not affect the access to public primary and junior high schools.

Before the merger, the old JA district was endowed with more top-tier public high schools exclusively for its own residents.¹⁰ According to Figure 1, before the merger the old JA had 7 top-tier high schools, while the old ZB had 8, but with a population three times the size of that of JA. Naturally, the top-tier school density (school per capita) in old ZB was less than half its level in JA, suggesting that students in old ZB had had to face much stronger competition to get into a top-tier high school. The slimmer opportunities for ZB students to get into a high-quality school before the merger is also reflected in Figure 2: the average admission score for top-tier high schools in ZB was much higher than that in JA.

[[Insert Figures 1 and 2 about here]]

Given the uneven distribution of high-quality educational resources across these two districts, the merger was generally perceived as a pronounced improvement in school choices for ZB students, because the merger increased opportunities for ZB students, who were initially disadvantaged, to get into a high-performing school. We confirm this idea in Figures 2 and 3. Figure 2 shows that admission scores for top-tier high schools in JA increased more than those in ZB, suggesting stronger positive selection in JA after the merger. Figure 3 reports the data on student flows between these two districts in the years 2016 and 2017. While 92.6% of the old ZB students who studied in the old JA schools after the merger managed to enrol into top-tier high schools, only 55.8% of the old JA students who studied in the old ZB schools enrolled into the top-tier high schools in ZB. The two figures combined suggest that students from ZB would have been likely to benefit more from the merger.

[[Insert Figure 3 about here]]

3. Data

The unique dataset that this paper uses is the credit and debit card transaction dataset provided by China UnionPay. Founded in 2002, China UnionPay is an association for China's banking card industry, operating under the approval of the People's Bank of China (the central bank of China). UnionPay is the only interbank payment network in China; it intermediates all card-based expenditures. It is the largest network in the world in terms of both the number and value of transactions, ahead of Visa and Mastercard. It transfers funds electronically in a Point-of-Sale

¹⁰ China has a two-tier public schools system in which top-tier schools (重点中学) explicitly receive much better resources and support than normal schools (普通中学), therefore show much better academic performances.

(POS) network and is the only interbank network in China, linking all the Automatic Teller Machines (ATMs) of all banks throughout the country.

We use all the bankcard transactions in the three districts of Shanghai, namely, Zhabei, Jing'an and Huangpu, to identify total expenditure and specific expenditure on children per card. All transactions on child-related items made via bankcards entered our data. Our data includes the transactions of 1,300,690 cards over 274 merchant categories between April 2015 and March 2016. Each observation records the location, time, value, and the merchant category of the transaction. We aggregate the data to the card-month level. Given that bankcard transactions have a high penetration in the retail spending in China, our data represent a sizeable proportion of household real spending activities. According to official statistics from the Central Bank of China (2015), bankcard transactions accounted for 48% of overall spending in the retail sales of consumer goods in the third quarter of 2015, and we believe that the ratio should be even higher in Shanghai, as it is the most commercialized metropolis in China.

We examine two types of expenditure on children, namely, extra-curriculum (EC) spending and other child support (OCS). Specifically, EC includes payment to private institutions that provide extra-curriculum training (e.g., Shao Nian Gong) and stationery. OCS includes expenditure on children's clothing, toys, zoo visits, etc. EC captures parental investment in children's tangible skills, which could directly enhance student's academic performance, while OCS spending may help develop such intangible children's skills as are largely believed to be associated with social competence, such as curiosity, self-confidence, etc. (Chen and French, 2008). Because the school zone of public primary and junior high schools is divided on the basis of a unit smaller than the district, no district merger should affect the access to public primary and junior high schools. In addition, because China mandates nine years of compulsory education, parental expenditure on formal education earlier than high school does not reflect the parent's own investment decisions. Thus, we do not include payment to formal education before high school.

One limitation of our data is nevertheless the lack of demographic information on cardholders. To overcome this limitation, we develop algorithms with the research team at UnionPay to explore the knowledge graph of cardholders. In the context of this paper, our knowledge graph aims to uncover the residence location of each cardholder at the district level

for the main analysis of the paper and within 2 kilometers of several underground stations for the cross-sectional analysis.

Specifically, we infer the residence location for each cardholder by employing a score-based algorithm that reads into the card's historical expenditure one year before the merger. Underlying our methodology is the assumption that the places where purchases are made are correlated with the residence location, but to a varying degree. For example, people are more likely to go to a laundry that is in their neighborhood, whereas the restaurant where they might dine is less indicative of their home address. Thus, we assign a score ranging from 1 to 10 to different spending categories. A higher score is assigned to property management, utilities, laundries, kindergartens, primary and middle schools, and a lower score is assigned to ATMs, restaurants, supermarkets, convenience stores, etc. Since each merchant has an address, we aggregate, for each card, the scores of all transactions by district. Finally, the district that receives the highest score is viewed as the primary residence district of the cardholder. In total, we identify 334,024 cards with primary residence location in JA, 860,890 cards in ZB, and 105,776 cards in HP. In Section 4.4, we discuss the algorithm that identifies the residence location of each cardholder within 2 kilometres of several underground stations.

We summarize the key variables in Table 1 for the full sample and by residence districts. Average total expenditure per card per month is 1,897 RMB. Debit cards have a higher expenditure (2,706 RMB) than credit cards (1,262 RMB). Given that the annual income per capita in Shanghai was 47,710 RMB in 2015 and that the private saving ratio was around 50%¹¹, our data are likely to capture a considerable proportion of actual spending. The average EC expenditure on children is 10 RMB, and for OCS it is 2 RMB. Because the full sample covers all cards, including childless cardholders, child expenditure in the full sample is likely to underestimate the parental investment. Comparing districts, ZB and JA residents spend similar amounts, close to 2000 RMB per month, and HP residents spend a third more. This is also largely consistent with the notion that HP is the center of downtown Shanghai.

[[Insert Table 1 about here]]

To assess the expenditure on children more precisely, we identify cardholders who have children below high-school age by examining their historical spending. We identify cards that spent on EC, OSC, and formal schooling before high school, and on children's hospitals one year

¹¹ More details at <http://www.shanghai.gov.cn/nw2/nw2314/nw24651/nw31071/nw31120/u21aw734365.html>.

before the merger. We summarize the key variables for the subsample of cardholders who have young children in Table 2. While total expenditure for this subsample is as high as that of the full sample, and is also as high in district-by-district comparisons, the child expenditure is much higher in Table 2. Child expenditure is on average 468 RMB per card per month, which accounts for 24% of total expenditure. We also find that parental investment in child skills tilts heavily toward tangible skills. Average EC spending is 458 RMB, accounting for 22% of total expenditure, whereas average OCS spending is 10 RMB, accounting for only 2% of total expenditure.

[[Insert Table 2 about here]]

4. Empirical Results

In this section, we examine the difference in the expenditure of cardholders between JB and HP district (our control group, as noted in the section on data) before and after the merger using a DID strategy for the full sample (Section 4.1), a pre-trend analysis (Section 4.2), a subsample of cardholders with pre-high-school children (Section 4.3), heterogeneous results on distance to the older border (Section 4.4), and heterogeneous results regarding income level (Section 4.5).

4.1. Baseline Analysis

We conduct an empirical analysis at the card-month level for the year that starts from March 2015 and ends in February 2016, 6 months before and 6 months after the announcement of the merger. It covers all the cardholders whose residence location was either in ZB or HP (the control group). By focusing on monthly expenditure in a one-year horizon, we can study the immediate effect of the merger and rule out other confounding changes that would take more time to materialize, for example, new schools or establishments built in these districts as a specific response to the merger. The DID estimation is in Equation (1).

$$C_{it} = \alpha + \beta (Zhabei_i \times After_t) + \gamma_i + w_t + \varepsilon_{it} \quad (1)$$

where C_{it} refers to the logarithm of monthly expenditure for card i in month t . It includes total child-related expenditure, extra-curriculum spending (EC), and other child support spending (OCS). The dummy variable $After_t$ equals 1 for months starting from September 2015. $Zhabei_i$ is a dummy variable that equals 1 if a cardholder's residence location is in the old ZB district and 0 if it is in HP. In this specification, we control for all the time-invariant differences

across cardholders (e.g., gender, education, etc.) by including card fixed effects γ_i . We also include month fixed effects w_t to control for changes over time that affect both districts in the same way. All standard errors are clustered at the card level.

[[Insert Table 3 about here]]

Since not every cardholder had pre-high-school children, our baseline specification captures only the intent-to-treat (ITT) estimates. Table 3 presents the results. Columns (1)–(3) show that after the merger cardholders in ZB spent 2.6% more on children-related items than those in HP. It corresponds to an economic magnitude of 0.3 RMB more per month after the merger. Both extra-curriculum (EC) spending and other child support (OCS) spending increased. EC went up by 0.5%, and OCS by 2.1%. Although these effects are all statistically significant, the economic magnitude is somewhat limited. This is probably due to the fact that not all cardholders have children. We will discuss this issue below in Section 4.3.

In contrast, columns (4)–(6) suggests that after the merger total monthly expenditure, either through credit cards or debit cards, did not vary much between ZB and HP. This finding implies that the ZB cardholders had to cut down other spending to increase their expenditure on children. In unreported tests, we explore some categories of adult-exclusive expenditure that are likely to be cut down in such circumstances, namely, coffee, cigarettes and alcohol, and beauty (including beauty salons and cosmetics). We find that only beauty expenditure declined for cardholders in ZB after the merger, suggesting that mothers probably cut down their beauty spending to subsidize their increased spending on children.

4.2.Pre-trend Analysis

In this section, we allow for a more flexible specification and evaluate the dynamic effects of the merger. While the underlying assumptions of DID estimation are not directly testable, it also helps us provide a pre-trend analysis to verify whether expenditure patterns differed between the two districts in the months leading up to the merger. We replace $Zhabei_j \times After_t$ in Equation (1) with $Zhabei_j \times w_t$. Table 4 reports the results.

[[Insert Table 4 about here]]

In Panel A of Table 4, we use the full sample and take August 2015 as the reference month. Overall, we do not find a systematic difference in either child expenditure or total expenditure in the months leading up to September 2015. For example, Column (3) in Panel A of Table 4 shows that there was no significant difference in OCS spending between these two districts before the

merger. The effect of the merger started to bite in September, precisely the month when the merger was announced. In addition, the impact on OCS persists in the 6 months after the merger. Figure 4 further illustrates the findings on total child expenditure with a 95% confidence interval. As shown, the cardholders in ZB started to spend significantly more than those in HP just after the merger, and the spending gaps ranged from 1.67% to 3.49%.

[[Insert Figure 4 about here]]

EC spending exhibits seasonality. Column (2) in Panel A of Table 4 shows that, after the merger, the spikes in EC occurred in September and January, which correspond to the beginning of an academic year and the winter holiday. Extra-curriculum training schools usually start new programs in these months and require tuition fees to be paid upon enrolment.

In Panel B of Table 4, we extend the sample period and include only the months in which EC spending is likely to have been concentrated, in order to better understand the dynamic effect of seasonality on EC. We include January 2015, June 2015, September 2015, January 2016, and June 2016; they correspond to the beginning of either a school break or an academic year. We conduct a similar pre-trend analysis as in Panel A and take January 2015 as the reference month. Overall, we do not find a persistent difference in EC spending in the months before the merger. In addition, EC spending in January 2016, and June 2016 for cardholders in ZB increased significantly in relation to those in HP after the merger.

In the Appendix, we provide a similar dynamic analysis by utilizing a longer period of sample that starts 12 months before and ends 12 months after the announcement of the merger. We presents the results in Figure A4. We find that both EC and OCS spending exhibit a persistently increasing trend after the merger.

4.3.Subsample with Young Children

While the baseline analysis indicates a significant increase in child expenditure for ZB cardholders after the merger, the economic magnitude is limited. This is likely to have been caused by the fact that not all cardholders have children. In other words, the baseline specification captures only the intent-to-treat (ITT) estimates by treating all the cards in ZB as affected by the merger. In fact, the district merger should directly impact only the cardholders whose children were below high-school age. In an ITT framework, the effect of the district merger in our baseline results should increase mechanically if the fraction of cardholders who have pre-high-school children was higher in ZB than the one in HP.

As discussed in Section 3, we cannot observe cardholders' demographic information. To better gauge the effect of the merger on parental spending on children, we identify a group of cardholders who have children of below high-school age (hereafter, young children) on the basis of a bankcard's transaction history. Specifically, we identify the cards used for spending on tuition fees for primary schools or junior high schools, children's hospitals, EC or OSC, one year before the merger. We are fully aware that the algorithm to create the subsample of cardholders with young children may not be precise; however the statistical error is more likely to be type I (failing to include all the cardholders with young children) than type II (including childless cardholders in the sample). We therefore first perform the same DID analysis for this subsample of cardholders with young children and then offer a cross-sectional analysis using the full sample in Panels A and B of Table 5 respectively.

[[Insert Table 5 about here]]

Panel A of Table 5 presents the subsample results. We find that the effect of the merger is significant for both child expenditure categories, EC and OCS spending. Noticeably, the ZB cardholders who are identified as having young children spent 15% more on EC and 14% more in OCS than the HP cardholders in the same category. In total, spending on children went up by 25.1%. This is in sharp contrast with the moderate increases in the full sample analysis (i.e., 0.5% in EC, 2% in OCS, and 2.5% in total spending). Measuring by RMB, total spending on children went up by 117 yuan per month in this subsample. Consistent with the results in the full sample, we do not find any significant changes in total expenditure.

$$C_{it} = \alpha + \beta (Zhabei_i \times After_t \times Kid_i) + Other\ interactions + \gamma_i + w_t + \varepsilon_{it} \quad (2)$$

To further test whether the magnitude of the effect is significantly higher for cardholders with young children, we conduct a cross-sectional analysis using the full sample. We include a triple interaction between Zhabei×After×Kid as in Equation (2); it allows for additional variation along the dimension of whether a cardholder is regarded as having young children. We report the results in Panel B of Table 5.

First, we find that the effect of the merger is more pronounced for cardholders with young children than those without, with magnitudes of 25.1% more in total spending on children, 13.6% more in EC, and 12.2% more in OCS. Second, the DID coefficient of Zhabei×After stays significant for total child expenditure and for OCS after adding the triple interaction term, suggesting that our algorithm indeed fails to identify all the cardholders with children. Taken

together, the results in this section lend further support to the baseline analysis by showing that the merger is likely to impact most on cardholders with young children.

[[Insert Table 6 about here]]

Our classification of young children in Table 5 relies on historical expenditure on children that includes types of expenditure likely to be correlated with parents' innate willingness and ability to invest in their children. Thus, our previous estimates may overestimate the effect of the merger on child expenditure. We therefore use a more conservative classification by using only historical spending on tuition fees for primary schools or junior high schools, as these schoolings are compulsory in China and less likely to be subject to selection issues. Table 6 report the results.

We find that a positive effect of the merger on child expenditure also holds based on the new classification of having young children. The ZB cardholders who are identified as having young children spent 5.5% more on EC and 8.3% more in OCS than the HP cardholders in the same category. In total, spending on children went up by 13.7%. The magnitudes, however, are indeed smaller than in Panel A of Table 5.

4.4.Heterogeneity: Distance to the Old Border

One major concern for our previous subsample analysis is that we rely on historical child-related expenditure to detect whether a cardholder has a young child or not. Thus, the algorithm may select parents who care about children and are more willing to spend on children into our subsample, therefore overestimating the effect of the merger. In this section, we explore another dimension of heterogeneity which uses all the categories of expenditure that are more likely to be independent of future child expenditure: distance to the old border between JA and ZB.

In JA, all the top-tier high schools are non-boarding. Thus, the cardholders in ZB who live closer to the old border between JA and ZB should have a greater incentive to send their children to the top-tier schools located in JA. The Appendix (Figure 3) shows that Underground Line One passes through ZB and JA. On this line, Hanzhong Road (汉中路) Station in ZB district is closest to JA, and Gongkang Road (共康路) Station in ZB is farthest from JA. There are six more underground stations between these two stops.

We infer the distance of the ZB cardholders to the eight underground stations in ZB by employing a similar score-based algorithm as the one we used to identify a cardholder's residential district (see the Data section). Underlying our methodology is the assumption that the

location of a purchase is correlated with the home address. We sift through each card’s historical spending one year before the merger. But we require more precise identification of the residence location this time – within 2 km of each underground station. To do this, we use the Global Positioning System (GPS) provided by Baidu Map for each merchant. We calculate the coordinate distance from each merchant to the nearest of the eight underground stations. We keep the transactions with merchants who are within a 2km radius of the nearest underground station. We then employ the same scoring system for the merchants as described in the Data section. We aggregate for each card the scores of all transactions by underground station. Finally, the underground station that receives the highest score is viewed as nearest to the primary residence location of the cardholder.

$$C_{it} = \alpha + \beta \text{ Far}_i \times \text{After}_t + \gamma_i + w_t + \varepsilon_{it} \quad (3)$$

We perform a DID analysis within the ZB cardholders as in Equation (3). Specifically, we compare the change in child expenditure after the merger between the cardholders who live close to the Gongkang Road Station with to those close to the Hanzhong Road Station. We construct a dummy variable *Far*, which equals 1 if the cardholder lives near Gongkang Road Station, and 0 if near Hanzhong Road Station. Table 7 presents the results.

[[Insert Table 7 about here]]

Because all top-tier high schools in JA are non-boarding, daily commuting is important for school choice. In other words, the effect of the merger tends to be stronger for cardholders living near the Gongkang Road Station than for cardholders near the Hanzhong Road Station, since it is more costly and time consuming for parents and children to attend schools that require a long commute. For total child expenditure, we find that cardholders near the Gongkong Road Station spent less on children after the merger than did those near the Hanzhong Road Station. The effect is most noticeable for OCS spending: cardholders near the Gongkong Road Station spent 1.5% less on OCS than those near the Hanzhong Road Station. Regarding EC spending, after including only the associated months (September, June and January, as suggested in Table 4), cardholders living far away from the old border spent 0.91% less on EC compare to those close to the border. In this analysis, we also find no significant increase in total expenditure.

4.5.Heterogeneity: Personal Income

We explore another dimension of heterogeneity – personal income – to examine whether the merger effect varies across different income groups. As noted earlier in the Introduction, the

extant literature provides mixed findings on the effect of public education provision on parental investment in children, specifically, their time investment. One possible explanation is that the effect may vary across families with different economic endowments. On the one hand, more prosperous families invest more resourcefully in their children and therefore are more likely to exploit the improved educational opportunities brought by the merger. On the other, as wealthier families have more outside alternatives to public high school, such as private high schools, these families are less motivated to respond to the merger.

$$C_{it} = \alpha + \beta (Zhabei_i \times After_t \times Income_i) + Other \text{ interactions} + \gamma_i + w_t + \varepsilon_{it} \quad (4)$$

Because we do not observe a cardholder’s actual income levels, we calculate the average monthly total spending for each card before the merger to indicate personal income. We believe that consumer spending is indicative of a cardholder’s actual income level. In Equation (4), we include a triple interaction term $Zhabei_i \times After \times Income_i$ to capture the different treatment effect of varying income levels. Table 8 reports the results.

[[Insert Table 8 about here]]

We find that after the merger wealthier cardholders spent more on OCS than more impoverished cardholders. A 1% increase in personal income is translated into a 0.12% increase in OCS spending. In contrast, we do not find any significant differences in EC spending between the rich and poor cardholders either we include the full 12 months (Column 2) or only the associated months (September, June and January in Column 3). Parents may perceive clothes and zoo visits as less necessary for their children than skill training, since the former does not directly improve a child’s performance. When the perceived opportunity of getting into a good public school improves, poorer parents prioritize their investment in tangible skills over that in intangible skills. This finding suggests that parental investment in children’s tangible skills is less elastic than their investment in intangible skills.

In addition, we create 10 deciles of cardholders in Zhabei and Huangpu districts based on their historical total expenditure one year before the merger. We run the baseline DiD analysis for each decile of cardholders by comparing the change in EC spending and OCS spending of cardholders in Zhabei and Huangpu for each decile of cardholders. We plot the DiD coefficients for each decile in Figure 5. We find that wealthier cardholders spent more on OCS than more impoverished cardholders; while the poorer cardholders responded stronger on EC.

[[Insert Figure 5 about here]]

Our findings in this section suggest that access to better educational resources could promote more investment in children’s academic performance across all social classes, and particularly for the more impoverished parents. The weaker effect for the rich perhaps on EC may be due to the fact that children from rich households have other options outside the public-school system, for instance studying abroad or attending private schools. The stronger effect for the poor cardholders is an encouraging result that implies that this merger potentially encourages equality in education investment in children across social classes. However, wealthy families indeed invested more in child intangible skills after the merger, implying that other aspects of human capital, such as social skills and confidence, are more likely to be subject to the persistence of income inequality.

5. Robustness Tests

5.1. Subsample of Debit Cards

One limitation of this dataset is that we cannot match card ID to cardholder. As a result, we cannot make inferences for one individual on the basis of card-level analysis because it is possible for one person to own several cards.

[[Insert Table 9 about here]]

To see the sensitivity of our results to the possibility of owning several cards, we repeat the DID analysis using a sample that includes debit cards only; the results are presented in Table 9. We restrict our sample to debit cards, because people are more likely to possess several credit cards than several debit cards, given that credit cards can earn bonus points and repay the overdraft of other credit cards. In Panel A of Table 9, we find that after the merger total spending on children increases by 1.11% for ZB debit-card holders, compared to 2.6% when all cards are included. The slightly smaller effect is perhaps to be explained by the fact that childless cardholders tend to be young people who are less eligible to apply for credit cards. Then, to better gauge the effect of multiple cards, we select a subsample of the previous group that covers only debit-card holders with young children in Panel B. In this sub-subsample, the effect jumps to 40.08%, compared to 28.5% when all cards are considered. In short, the impact of the merger on expenditure on children is more pronounced if we look only at debit-card holders with young children.

Overall, our findings suggest that the merger also has a significant effect on the sample of debit cards, but at the individual level the magnitude is inferred to be sensitive to the issue of multiple cards.

5.2. Placebo Test: Subsample with Adult Children

To see whether the effect of the merger is specific to cardholders who have young children (below high-school age children), we run a placebo test by examining whether the merger also affects cardholders who are identified as having adult children (too old to go to high school) or as too young to have school-age children.

To do so, we identify the cards used to pay tuition fees to tertiary education institutions, including universities, vocational schools and correspondence schools, one year before the merger. The assumption here is that the cardholders who paid for such tertiary education are either parents with children of post-secondary-school age or university/college students themselves. Neither group would be affected by the high-school-district merger. We then perform the same DID analysis within this subsample.

[[Insert Table 10 about here]]

Table 10 presents the results. Unlike the findings in Table 5, the merger presented an impact that was no different for ZB cardholders or HP cardholders. Neither EC nor OCS experienced increases after the merger. In short, the results in this section further confirm that the effect of the merger was specific to cardholders who had young children.

6. Alternative Channels

In section 5, we find that the merger between ZB and JA positively affected the ZB cardholders' investment decisions in their children. We argue that their improved entitlement to accessing high-quality public schools could increase perceived educational returns, thus encouraging greater parental investment in children. However, other factors may have altered at the same time too. For example, the merger may induce cardholders in ZB to catch up with those in JA if they possess "keeping up with Joneses" type of preference (Abel, 1990). According to this theory, the marginal utility of relative wealth (an agent's own wealth relative to the peer's) should decline with the agent's wealth level. Thus, the importance of status should decline with wealth. In other words, richer cardholders in ZB should be less driven to keep up with cardholders in JA than should poorer cardholders in ZB. Our findings in Section 4.5 that the increase in OCS spending

is more pronounced among the richer cardholders and that EC spending does not vary to personal wealth are inconsistent with the preference-based explanation. In this section, we discuss two other possible mechanisms through which the merger may have impacted on cardholders' expenditure decisions.

6.1.Housing Appreciation

The first altered channel would have taken effect if there had been housing appreciation in the old ZB district. As discussed in Section 2, the old JA was regarded as one of the most affluent districts in Shanghai, with a rich cultural heritage and concentrated shopping centers, leading to high real estate prices. ZB had a much humbler economy and extended over a larger area, with consequently lower housing prices. It is possible that, after the merger, housing prices in the former ZB would go up, now that properties could claim to be in JA, and converge to the prices in the old JA. In addition, as cardholders who have young children are also more likely to own residential properties, they may have been the ones who benefited most from such a housing appreciation. In this section, we test whether housing appreciation was the primary driver that affected the changes in the expenditure patterns of ZB cardholders.

First, we document both the prices and transaction volumes of the residential properties in ZB and HP districts (see Figure 6). It is clear that after the merger neither the housing prices nor the transaction volume in ZB experienced a significant surge within half a year of August 2015. This, contrary to what one might have expected, means that the housing market in ZB remained relatively stable after the merger. Thus, there was no actual positive wealth shock for the ZB house owners.

[[Insert Figure 6 about here]]

Second, although we do not find evidence of actual housing appreciation, the residents in ZB might have responded to the merger by upwardly adjusting their anticipation of real estate prices; this could also have influenced expenditure patterns. If this was the case, instead of observing an increase in child expenditure alone, we should expect to see a rise in total expenditure for ZB cardholders after the merger. However, across different specifications (Column 4 of Table 3 and Column 4 of Table 5), we consistently find that the merger did not bring significant changes in total expenditure and the effect of the merger is confined to only child-related expenditure. These findings lend further support to our interpretation that the

merger affected expenditure on children through improved educational opportunities but not through anticipated gains in wealth.

Third, we further identify cardholders who are property owners in their own districts by examining the payments made to local property service agencies and real estate agencies. If it had been a housing appreciation that primarily drove the previous results, then cardholders who were property owners should have responded in more pronounced ways to the merger than did non-property owners. We test this idea in Table 11.

[[Insert Table 11 about here]]

In Panel A of Table 11, we examine whether the effect of the merger was stronger for house owners by including a triple interaction $Zhabei \times After \times House$. One challenge arises when interpreting the results in Panel A: parental status tends to be positively associated with property ownership. In other words, cardholders with children are also more likely to own residential properties. To separate the effect of housing price appreciation from improved educational opportunities, we employ an interaction between $Zhabei \times After \times Child \times House$ under the same DID strategy in Panel B. *House* is a dummy variable that indicates whether a cardholder is a property owner in their own district. We find that in the group that is identified as parents of young children, the effects of the merger on EC and OCS did not differ significantly between cardholders with and those without properties. Taken together, our findings further confirm that the merger was unlikely to have affected a ZB cardholder's expenditure decisions because of higher anticipated gains in the real estate market.

6.2.Stronger Competition

Another possible explanation is that parents anticipated fiercer competition after the merger and thus invested more in their children. As discussed previously, each student is allotted to a public high school in Shanghai on the basis of her/his performance in a standard examination. Indeed, by enlarging the candidate pool, the district merger may have intensified the competition. We conduct several tests to examine the competition factor.

First, the detailed study in Section 2 shows that the merger if anything even weakened the competition among students in the old ZB, whereas it intensified the competition in the old JA. As shown in Figure 1, the old ZB had only half as many top-tier high schools per capita as JA had. Hence, students in the old ZB used to face much stronger competition to get into a good public high school. This is also reflected in Figure 2, which shows that the average admission

score of top-tier high schools was higher in ZB before the merger than after it. Consistently, the gap in average admission score also narrowed after the merger. Figure 3 further shows that the merger allowed a higher percentage of good students than before to enroll in top-tier high schools in the old JA.

Given that the merger tended to introduce more competition to JA than to ZB, if the increase in expenditure on children in ZB was driven by competition brought by the merger, we should expect an even more noticeable effect for the cardholders in JA. To test this, we compare the change in expenditure on children by cardholders in JA to that of cardholders in HP by employing the same DID strategy. We report the results in Table 12.

[[Insert Table 12 about here]]

We consider all the cardholders in columns (1)-(3) and a subsample of cardholders whom we identify having young children in columns (4)-(6). In contrast with the results in Tables 3 and 5, we find that JA cardholders after the merger did not increase their EC spending, as compared to HP cardholders. Second, we observe a significant but only a moderate increase in OCS spending by JA cardholders; and the magnitude of the effect is only 30% of the effect when ZB is compared to HP. In unreported tests, we directly compare the change in expenditure on children of JA cardholders to that of ZB cardholders. The findings consistently show that ZB cardholders have a higher increase in child expenditure, both EC and OCS, as compared to JA cardholders,

In conclusion, we believe that the increase in parental investment in ZB was more likely to be driven by improved opportunities to invest in education, and the mild response in JA was possibly driven by stronger competition.

7. Conclusion and Discussion

This paper examines whether parental investment in children responds to changes in the provision of public education and presents what is, to our knowledge, the first evidence of parental pecuniary investment in children. We exploit an exogenous shock in public education provision brought by a district merger in Shanghai. The merger improved the opportunities of ZB students, who were initially disadvantaged, to get into high-performing public high schools. We construct a unique transaction-level dataset of China UnionPay to capture all expenditure on children via bankcards across the three districts in Shanghai.

We exploit a simple DID framework. This paper shows that cardholders indeed increased their spending on children, in both extra-curriculum training and other child support, in response to the improved educational opportunities. The effect is both immediate and persistent. In addition, we further explore the heterogeneity in cardholders' response to the merger and find that the effect is stronger for cardholders who have children of pre-high-school age and for those who live closer to the old border, but disappears for those who have adult children. Last, we discuss potential alternative changes that might have influenced expenditure decisions, and rule out the possibility that the previous results were driven by the increased competition and housing appreciation that may have resulted from the merger.

This paper fills gaps in the literature on the interplay between public education and child education and presents important policy implications. Many public programs that have been widely studied in the literature usually require monetary subsidies for education provision. The district merger in Shanghai was a redistribution of educational resources based on a meritocratic exam across two districts; it thus did not require additional fiscal subsidies. Our findings suggest that without extra public spending, simply improving the perceived opportunity to get into high-quality schools could stimulate the private investment in children, thus supplementing the inadequacy in public investment.

This study also opens interesting questions for future research. For example, cultural values often influence education decisions; it is, therefore, unclear whether the pattern of investment in children that we reveal is a feature of Chinese parents that also exists in other societies. In addition, because this paper is silent on children's school outcomes, more empirical research that can link children's long-term outcomes to parents' pecuniary investment in them will be needed to complete our understanding of the role of the interplay between public and parental investment in the development of children's human capital.

References

- Abel, Andrew B., 1990, Asset prices under habit formation and catching up with the Joneses, *American Economic Review*, 80 (2), 38–42.
- Andrews, M., Duncombe, W. and Yinger, J., 2002. Revisiting economies of size in American education: are we any closer to a consensus? *Economics of Education Review*, 21(3), pp.245-262.
- Becker, G., 1981. A treatise on the family Harvard University Press. *Cambridge, MA*, 30.
- Becker, G.S. and Tomes, N., 1976. Child endowments and the quantity and quality of children. *Journal of Political Economy*, 84(4, Part 2), pp.S143-S162.
- Becker, G.S. and Tomes, N., 1986. Human capital and the rise and fall of families. *Journal of Labor Economics*, 4(3, Part 2), pp.S1-S39.
- Carneiro, P.M. and Heckman, J.J., 2003. Human capital policy.
- Central Bank of China, “General Report on the Operations of Chinese Payment System, 2015 Q3,” Technical Report, Central Bank of China 2015.
- Chen, X. and French, D.C., 2008. Children's social competence in cultural context. *Annual Review of Psychology*, 59, pp.591-616.
- Chetty, R., Hendren, N. and Katz, L.F., 2016. The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, 106(4), pp.855-902.
- Chyn, E., 2016. Moved to opportunity: The long-run effect of public housing demolition on labor market outcomes of children. *Unpublished paper. University of Michigan, Ann Arbor*.
- Coate, S. and Knight, B., 2007. Socially optimal districting: a theoretical and empirical exploration. *The Quarterly Journal of Economics*, 122(4), pp.1409-1471.
- Cunha, F., Heckman, J.J., Lochner, L. and Masterov, D.V., 2006. Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education*, 1, pp.697-812.
- Currie, J., and Thomas, D., 1995. “Does Head Start Make a Difference?” *American Economic Review* 85(3): 341.
- Cullen, J.B., Jacob, B.A. and Levitt, S., 2006. The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica*, 74(5), pp.1191-1230.
- Das, J., Dercon, S., Habyarimana, J., Krishnan, P., Muralidharan, K. and Sundararaman, V., 2011. School inputs, household substitution, and test scores. The World Bank.

- Deming, D.J., Hastings, J.S., Kane, T.J. and Staiger, D.O., 2014. School choice, school quality, and postsecondary attainment. *American Economic Review*, 104(3), pp.991-1013.
- Driessen, G., Smit, F. and Slegers, P., 2005. Parental involvement and educational achievement. *British Educational Research Journal*, 31(4), pp.509-532.
- Fos, V., Liberman, A. and Yannelis, C., 2017. Debt and human capital: Evidence from student loans.
- Fryer Jr, R.G. and Katz, L.F., 2013. Achieving escape velocity: neighborhood and school interventions to reduce persistent inequality. *American Economic Review: Papers & Proceedings*, 103(3), pp.232-37.
- Gelber, A. and Isen, A., 2013. Children's schooling and parents' behavior: Evidence from the Head Start Impact Study. *Journal of Public Economics*, 101, pp.25-38.
- Goldberger, A.S., 1989. Economic and mechanical models of intergenerational transmission. *The American Economic Review*, 79(3), pp.504-513.
- Gordon, N. and Knight, B., 2008. The effects of school district consolidation on educational cost and quality. *Public Finance Review*, 36(4), pp.408-430.
- Gordon, N. and Knight, B., 2009. A spatial merger estimator with an application to school district consolidation. *Journal of Public Economics*, 93(5-6), pp.752-765.
- Heinesen, E., 2005. School district size and student educational attainment: evidence from Denmark. *Economics of Education Review*, 24(6), pp.677-689.
- Hoxby, C.M., 2003. School choice and school productivity. Could school choice be a tide that lifts all boats?. In *The economics of school choice* (pp. 287-342). University of Chicago Press.
- Hoxby, C.M. and Murarka, S., 2009. *Charter schools in New York City: Who enrolls and how they affect their students' achievement* (No. w14852). National Bureau of Economic Research.
- Katz, L.F., 2015. Reducing inequality: Neighborhood and school interventions. *Focus*, 31(2), pp.12-17.
- Lavy, V., 2010. Effects of free choice among public schools. *The Review of Economic Studies*, 77(3), pp.1164-1191.
- Lochner, L.J., and Monge-Naranjo, A, 2011, The nature of credit constraints and human capital, *American Economic Review* 101(6), pp.2487–2529.
- Pop-Eleches, C. and Urquiola, M., 2013. Going to a better school: Effects and behavioral responses. *American Economic Review*, 103(4), pp.1289-1324.

Rickford, J.R., Duncan, G.J., Gennetian, L.A., Gou, R.Y., Greene, R., Katz, L.F., Kessler, R.C., Kling, J.R., Sanbonmatsu, L., Sanchez-Ordonez, A.E. and Sciandra, M., 2015. Neighborhood effects on use of African-American vernacular English. *Proceedings of the National Academy of Sciences*, 112(38), pp.11817-11822.

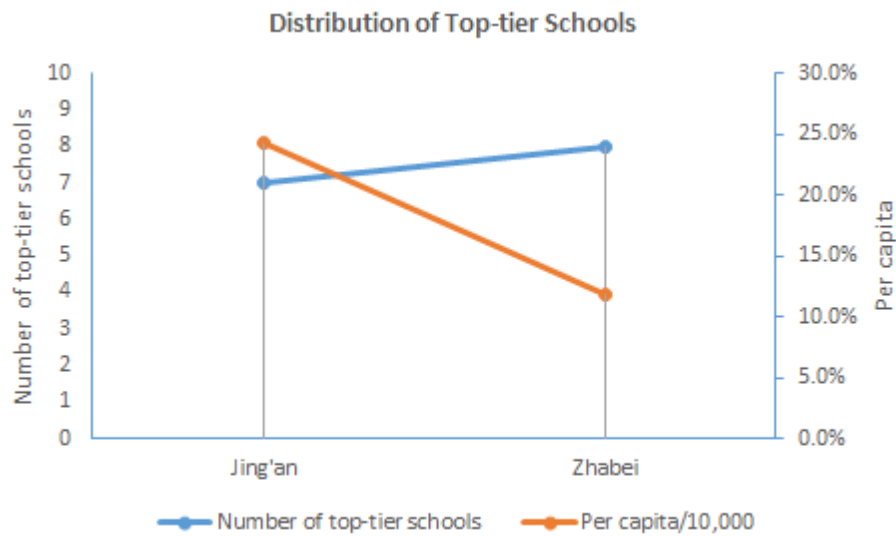
Rothstein, J. and Rouse, C.E., 2011. Constrained after college: Student loans and early-career occupational choices. *Journal of Public Economics*, 95(1-2), pp.149-163.

Sacerdote, B., 2007. How large are the effects from changes in family environment? A study of Korean American adoptees. *The Quarterly Journal of Economics*, 122(1), pp.119-157.

U.S. Department of Health and Human Services, Administration for Children and Families. 2010. "Head Start Impact Study, Final Report."

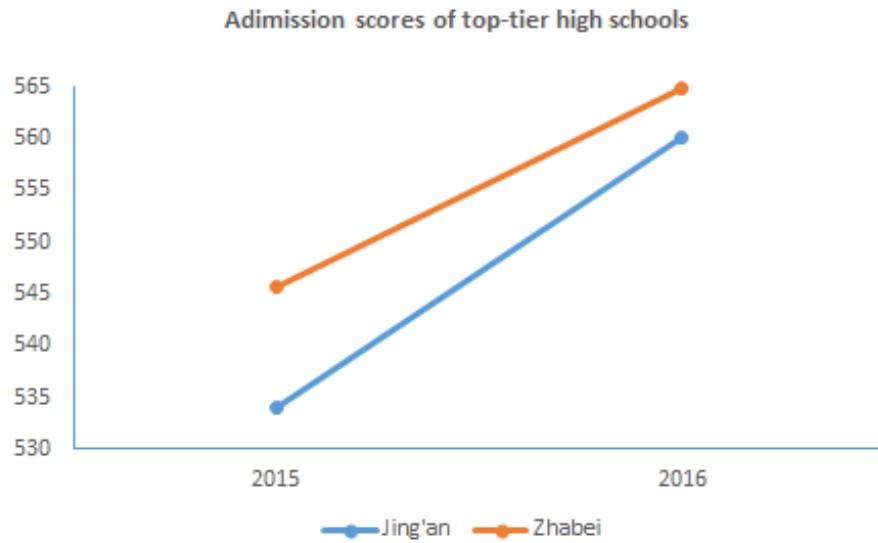
Figures

Fig. 1. Supply of Top-tier High Schools in Zhabei *vs* Jing'an



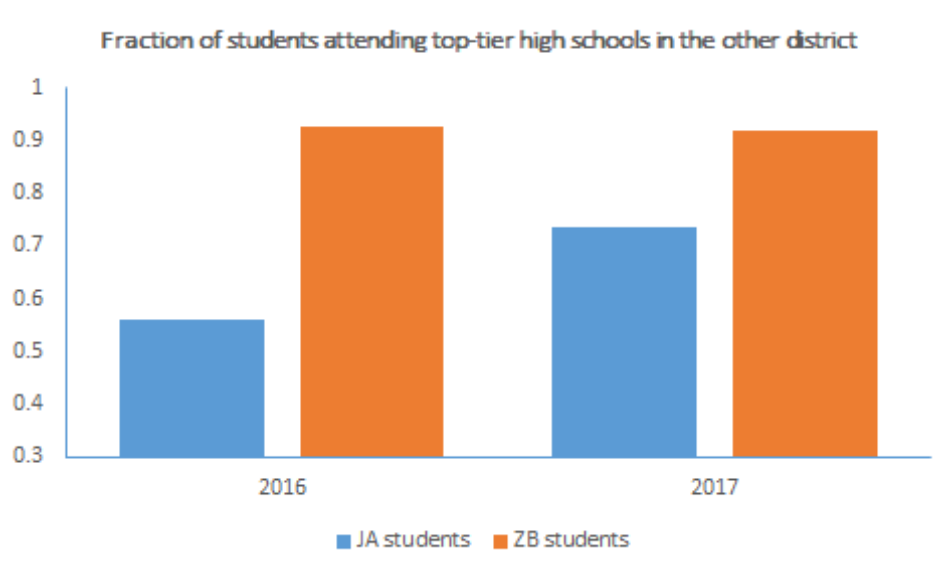
Notes: This figure presents the inequality in public school provision between Jing'an and Zhabei before the merger. The blue line displays the number of top-tier public high schools in each district (left y-axis), while the orange line reports the density of top-tier public high schools over district population in 10,000 (right y-axis).

Fig. 2. Admission Scores Before and After the Merger



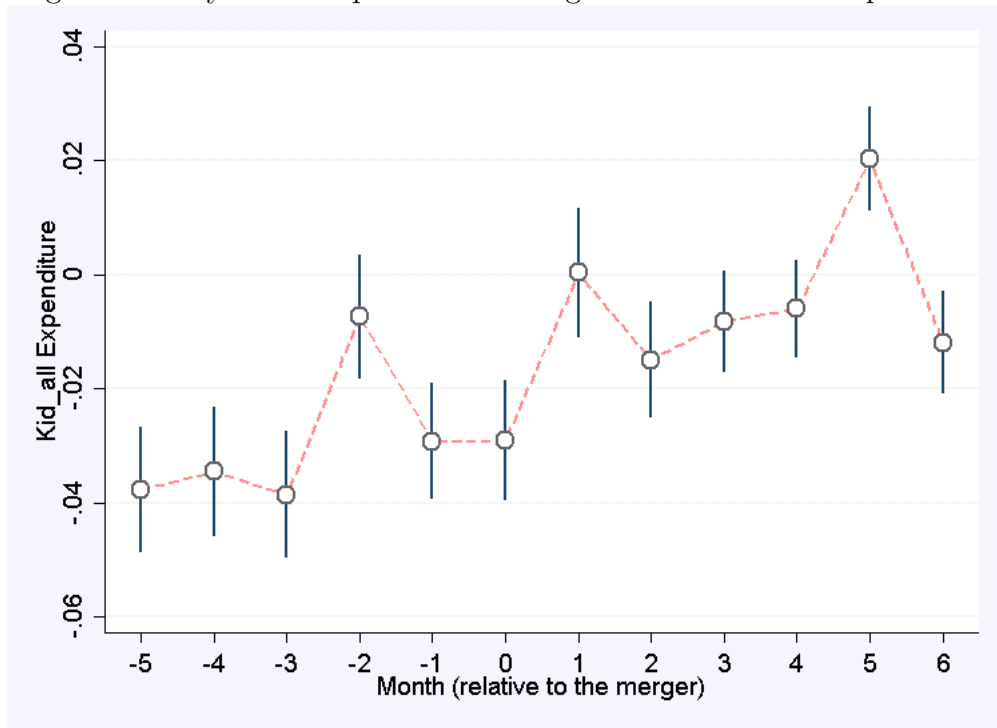
Notes: This figure displays the average admission scores of the standard entrance examination for top-tier high schools by districts in years 2015 and 2016. The orange line represents the average score in Zhabei district, and the blue line Jing'an district.

Fig. 3. Student Flows After the Merger



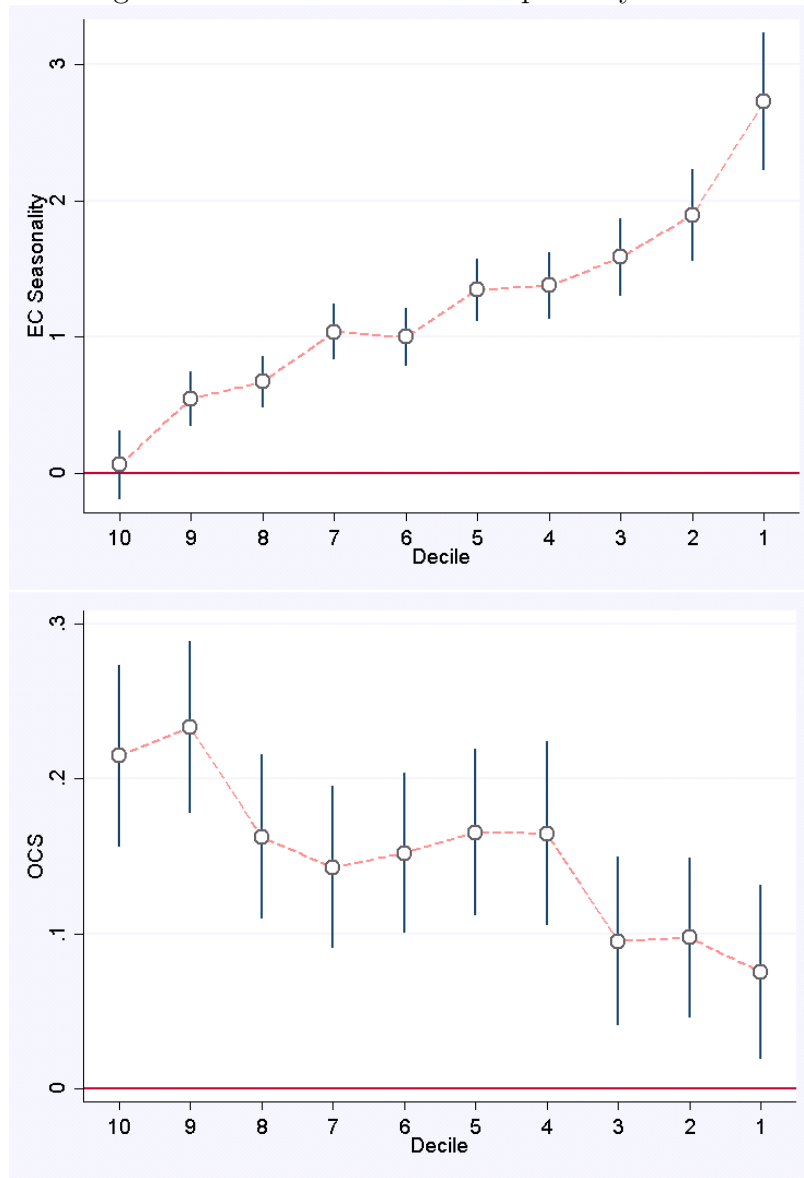
Notes: This figure reports the data on student flows between Zhabei (ZB) and Jing'an (JA) in the years 2016 and 2017. We calculate student flows from ZB to JA as the number of ZB students who enrolled in JA top-tier high schools over the number of ZB students who enrolled in JA high schools. We calculate student flows from JA to ZB as the number of JA students who enrolled in ZB top-tier high schools over the number of JA students who enrolled in ZB high schools. The orange bars represent student flow from ZB to JA, while the blue bars the student flow from JA to ZB.

Fig. 4. The Dynamic Impact of the Merger on Total Child Expenditure



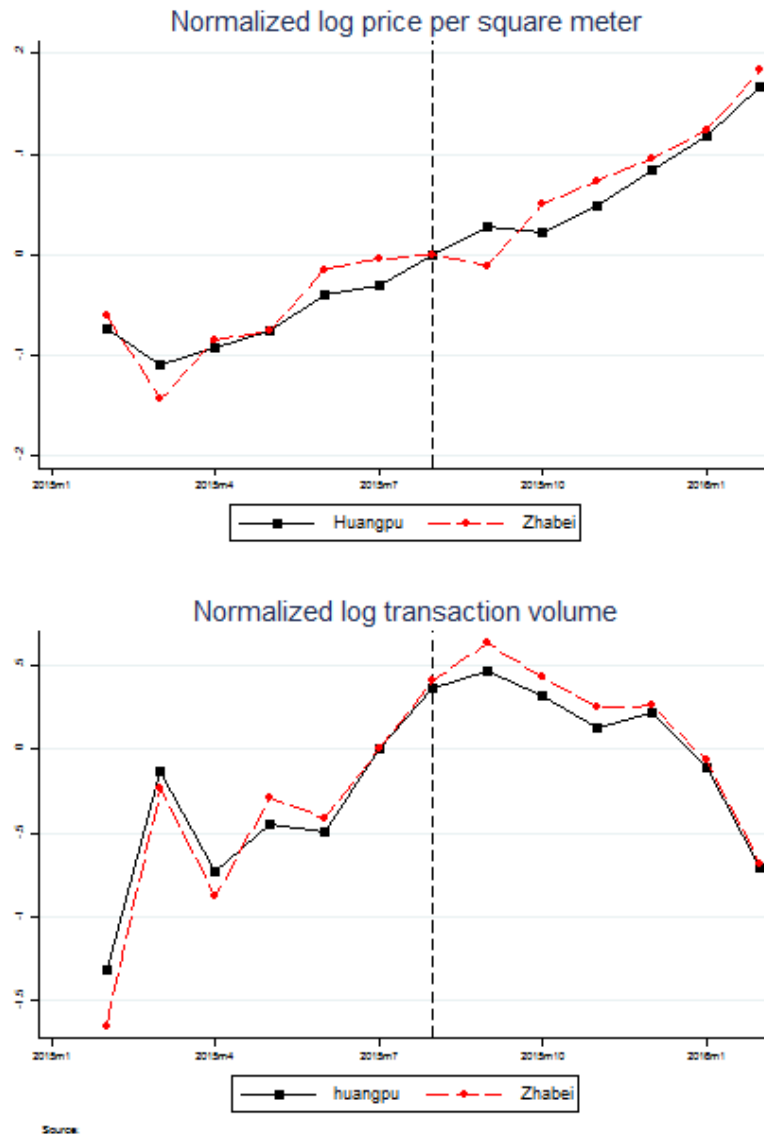
Notes: This figure presents the DiD coefficients with a 95% confidence interval in Table 4. The vertical line indicates months where month 0 corresponds to August 2015.

Fig. 5. Historical Total Consumption by Deciles



Notes: We create 10 deciles of cardholders in Zhabei and Huangpu districts based on their historical total expenditure one year before the merger. This figure displays the DiD coefficients with a 95% confidence interval of subsample analyses that compare the change in EC spending and OCS spending of cardholders in Zhabei and Huangpu for each decile of cardholders. For EC spending, we keep June 2015, Sep 2015, Jan 2016, and June 2016. On the horizontal axis, 10 indicates the highest level of total historical consumption, and 0 indicates the lowest.

Fig. 6. Housing Market In ZB *vs* HP



Notes: This figure displays the logarithms of average housing price per square meter and total transaction volume, normalized at their corresponding levels in August 2015, for all residential properties by districts over time. The dotted line indicates August 2015. The red line represents Zhabei district and the black line Huangpu district. Data is obtained from China Real Estate Price Platform run by China Real Estate Association.

Tables

Table 1: Summary Statistics for Main Variables

VARIABLES	N	mean	sd	p50	min	max
<i>Panel A: Full Sample of Zhabei and Huangpu</i>						
Total expenditure	3,788,260	1,897.03	14,228.96	400.00	1.01	9,721,200.00
Credit	2,122,671	1,262.39	6,660.32	300.34	1.04	3,534,023.00
Debit	1,665,589	2,705.82	20,069.56	600.00	1.01	9,721,200.00
Child expenditure	3,788,260	11.96	415.49	0.00	0.00	150,258.00
EC	3,788,260	9.71	409.25	0.00	0.00	150,258.00
OCS	3,788,260	2.24	70.25	0.00	0.00	42,723.00
<i>Panel B: Zhabei Sample</i>						
Total expenditure	3,542,071	1,792.95	14,407.52	386.50	1.04	9,721,200.00
Credit	1,975,140	1,123.05	6,272.79	288.56	1.04	3,534,023.00
Debit	1,566,931	2,637.36	20,453.67	569.80	1.05	9,721,200.00
Child expenditure	3,542,071	12.20	407.38	0.00	0.00	105,876.00
EC	3,542,071	9.87	400.71	0.00	0.00	104,800.00
OCS	3,542,071	2.33	71.83	0.00	0.00	42,723.00
<i>Panel C: Huangpu Sample</i>						
Total expenditure	246,189	3,394.49	11,247.16	1,180.50	1.01	2,008,622.00
Credit	147,531	3,127.88	10,378.80	1,005.50	1.40	2,008,622.00
Debit	98,658	3,793.17	12,422.41	1,591.62	1.01	823,804.70
Child expenditure	246,189	8.42	518.33	0.00	0.00	150,258.00
EC	246,189	7.43	516.70	0.00	0.00	150,258.00
OCS	246,189	0.99	41.16	0.00	0.00	9,736.00
<i>Panel D: Jing'an Sample</i>						
Total expenditure	1,607,768	1,949.14	12,209.49	447.10	1.01	4,010,000.00
Credit	1,008,700	1,442.68	7,711.95	343.10	1.05	1,900,984.00
Debit	599,068	2,801.91	17,285.13	669.00	1.01	4,010,000.00
Child expenditure	1,607,768	15.98	462.40	0.00	0.00	70,000.00
EC	1,607,768	12.39	442.83	0.00	0.00	70,000.00
OCS	1,607,768	3.58	133.08	0.00	0.00	48,806.00

Notes: This table reports summary statistics for the main variables. Panel A reports the full sample including all cards in Zhabei and Huangpu districts, Panel B all cards in district Zhabei, Panel C all cards in district Huangpu, and Panel D all cards in district Jing'an.

Table 2: Summary Statistics for Subsample of Cardholders with Young Children

Variable	N	mean	sd	p50	min	max
<i>Panel A: Full Sample</i>						
Total expenditure	79,032	2,092.72	9,021.31	511.10	1.60	1,200,000.00
Credit	70,264	1,830.09	5,661.05	483.00	1.60	272,000.00
Debit	8,768	4,197.35	21,721.38	1,000.00	2.00	1,200,000.00
Child expenditure	79,032	468.19	2,775.91	0.00	0.00	150,258.00
EC	79,032	458.61	2,773.93	0.00	0.00	150,258.00
OCS	79,032	9.57	87.90	0.00	0.00	5,702.00
<i>Panel B: Zhabei Sample</i>						
Total expenditure	76,630	2,069.88	8,938.48	510.00	1.60	1,200,000.00
Credit	68,456	1,811.65	5,595.35	483.00	1.60	272,000.00
Debit	8,174	4,232.47	21,946.20	1,000.00	2.00	1,200,000.00
Child expenditure	76,630	466.04	2,688.65	0.00	0.00	105,876.00
EC	76,630	456.22	2,686.53	0.00	0.00	104,800.00
OCS	76,630	9.83	89.05	0.00	0.00	5,702.00
<i>Panel C: Huangpu Sample</i>						
Total expenditure	2,402	2,821.38	11,329.04	548.59	3.50	387,009.40
Credit	1,808	2,528.10	7,718.77	482.91	3.50	150,258.00
Debit	594	3,714.09	18,359.01	900.00	6.00	387,009.40
Child expenditure	2,402	536.56	4,787.76	0.00	0.00	150,258.00
EC	2,402	535.05	4,787.81	0.00	0.00	150,258.00
OCS	2,402	1.51	33.80	0.00	0.00	1,000.00

Notes: This table reports summary statistics for the main variables of a subsample that only includes cardholders identified as having pre-high-school children. Panel A reports the full subsample including all such cards in Zhabei and Huangpu, Panel B all such cards in district Zhabei, and Panel C all such cards in district Huangpu.

Table 3: Baseline DiD Results

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
	Expenditure on Children			Total Expenditure		
	Total	EC	OCS	Total	Credit	Debit
After*Zhabei	0.0256*** (0.0026)	0.0045*** (0.0017)	0.0213*** (0.0019)	-0.0101 (0.0081)	-0.0045 (0.0104)	-0.0198 (0.0129)
Card Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,771,182	3,771,182	3,771,182	3,771,182	2,111,732	1,659,450
R-squared	0.001	0.001	0.001	0.005	0.005	0.004

Notes: This table reports the differences in expenditure between cardholders in Zhabei district and Huangpu district before and after the merger. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending(OCS), respectively. Dependent variable in Column (4) is total expenditure. Columns (5) and (6) examine total expenditure by credit and debit cards separately. The baseline specification includes card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Dynamic Impacts of the Merger on Expenditure

Panel A				
Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total
	Total	EC	OCS	Expenditure
April'15*Zhabei	-0.0038 (0.00587)	-0.0054 (0.00422)	0.0016 (0.00407)	-0.0085 (0.0167)
May*Zhabei	-0.0079 (0.00586)	-0.0059 (0.00405)	-0.0020 (0.00423)	-0.0092 (0.0162)
June*Zhabei	0.0226*** (0.00563)	0.0190*** (0.00419)	0.0037 (0.00378)	0.0025 (0.0158)
July*Zhabei	0.0007 (0.00541)	0.0009 (0.00373)	-0.0002 (0.00392)	-0.0084 (0.0153)
Sep*Zhabei	0.0357*** (0.00577)	0.0182*** (0.00405)	0.0180*** (0.00420)	-0.00912 (0.0153)
Oct*Zhabei	0.0148*** (0.00557)	-0.0018 (0.00405)	0.0167*** (0.00382)	0.0105 (0.0151)
Nov*Zhabei	0.0245*** (0.00494)	0.0044 (0.00328)	0.0202*** (0.00378)	-0.0568*** (0.0151)
Dec*Zhabei	0.0259*** (0.00483)	0.0031 (0.00318)	0.0228*** (0.00364)	-0.0731*** (0.0150)
Jan'16*Zhabei	0.0515*** (0.00509)	0.0227*** (0.00324)	0.0294*** (0.00394)	-0.0083 (0.0154)
Feb*Zhabei	0.0180*** (0.00536)	-0.0018 (0.00372)	0.0198*** (0.00386)	0.0103 (0.0154)
March*Zhabei	0.0326*** (0.00551)	0.0003 (0.00361)	0.0324*** (0.00419)	0.0237 (0.0161)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	3,788,260	3,788,260	3,788,260	3,788,260
R-squared	0.001	0.001	0.001	0.005

Panel B: EC Seasonality	
Dep. Var.	EC
June'15* Zhabei	0.0042 (0.0040)
Sep* Zhabei	0.0032 (0.0048)
Jan'16* Zhabei	0.0117*** (0.0039)
June* Zhabei	0.0198*** (0.0046)
Card Fixed Effect	Yes
Month Fixed Effect	Yes
R-squared	0.0002
N	1,734,740

Notes: This table reports the dynamic effect of the merger on cardholders in Zhabei district relative to those in Huangpu district. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. Columns (1)-(4) include the full sample and take month August 2015 as the reference month. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending(OCS), respectively. Dependent variable in Column (4) is total expenditure. Column (5) analyzes seasonality of EC by including Jan 2015, June 2015, Sep 2015, Jan 2016, and June 2016, and takes Jan 2015 as the reference month. All dependent variables are in logarithm. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Young Children Analysis

Panel A: Young Children Subsample: the DID Results				
Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total expenditure
	Total	EC	OCS	
After*Zhabei	0.285*** (0.0777)	0.153** (0.0747)	0.140*** (0.0212)	-0.001 (0.0673)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	78,532	78,532	78,532	78,532
R-squared	0.011	0.035	0.037	0.006
Panel B: Cardholders with Children <i>vs</i> the Others without				
After*Zhabei	0.0197*** (0.00221)	0.0015 (0.00104)	0.0182*** (0.00194)	-0.0103 (0.00817)
After*Child	-0.0788 (0.0753)	-0.0576 (0.0724)	-0.0211 (0.0199)	0.0072 (0.0670)
After*Zhabei* Child	0.251*** (0.0774)	0.136* (0.0743)	0.122*** (0.0213)	0.0053 (0.0679)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	3,771,182	3,771,182	3,771,182	3,771,182
R-squared	0.002	0.001	0.001	0.005

Notes: This table reports the DiD results for a subsample that only includes cardholders who are identified as having pre-high-school children. Panel A includes cardholders that are identified as having young children based on their historical expenditure on children. Panel B reports the results of a cross-sectional analysis of the effect of the merger with respect to having young children. Child is a dummy variable that equals 1 if a cardholder is identified as having young children, and 0 otherwise.. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Defining Young Children based on Compulsory Schooling

Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total expenditure
	Total	EC	OCS	
After*Zhabei	0.1373*** (0.0424)	0.0549* (0.0335)	0.0832*** (0.0197)	-0.4478 (0.4310)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	5731	5732	5733	5734
R-squared	0.0123	0.0107	0.006	0.0243

Notes: This table reports the DiD results for a subsample that only includes cardholders who are identified as having pre-high-school children based on a stricter standard to select cardholders who have young children: we only includes cardholders who paid tuition fees for primary or junior high schools before the merger. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Heterogeneous Effects of the Merger: Distance to the Old Border

Dep. Var.	(1)	(2)	(3)	(4)	(5)
	Expenditure on Children			Total Expenditure	
	Total	EC	EC (Seasonality)	OCS	
After*Far	-0.0162*** (0.0044)	-0.0010 (0.0022)	-0.0091* (0.0052)	-0.0152*** (0.0039)	-0.0064 (0.0107)
Card Fixed Effect	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	405,169	405,169	137,561	405,169	405,169
R-squared	0.0014	0.0017	0.0013	0.0009	0.0049

Notes: This table reports the DiD results for a subsample that only includes cardholders who are identified as residing within 2km of underground stations Hanzhong Road and Gongkang Road. Far is a dummy variable that equals 1 if the cardholder resides within 2km of underground stations Gongkang Road, and 0 if within 2km of Hanzhong Road. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(4) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (5) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Heterogeneous Effects of the Merger: Personal Income

Dep. Var.	(1)	(2)	(3)	(4)	(5)
	Expenditure on Children			Total Expenditure	
	Total	EC	EC (Seasonality)	OCS	
After*Zhabei	0.0200*** (0.0022)	0.0014 (0.0010)	0.0185*** (0.0048)	0.0186*** (0.0019)	-0.0096 (0.0081)
After*Income	-0.174 (0.179)	-0.151 (0.176)	0.0120* (0.0070)	-0.0234 (0.0290)	0.140 (0.109)
After*Zhabei*Income	0.352* (0.180)	0.236 (0.177)	-0.0059 (0.0067)	0.1240*** (0.0299)	-0.120 (0.109)
Card Fixed Effect	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	3,771,182	3,771,182	3,771,182	3,771,182	3,771,182
Number of ID	966,666	966,666	966,666	966,666	966,666
R-squared	0.002	0.001	0.004	0.001	0.005

Notes: The table presents the results of a cross-sectional analysis of the effect of the merger with respect to personal income. Income is proxied as the average historical monthly total expenditure for each card. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Debit Card Subsample: the DiD Results

Panel A: Debit Card Sample				
Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total Expenditure
	Total	EC	OCS	
After*Zhabei	0.0111*** (0.0027)	0.0042** (0.0106)	0.0070*** (0.0020)	-0.0198 (0.0129)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	1,659,450	1,659,450	1,659,450	1659450
R-squared	0.0002	0.0000	0.0002	0.0043
Panel B: Debit Card & Young Children Sample				
After*Zhabei	0.4008*** (0.1417)	0.2942** (0.1403)	0.1188*** (0.0227)	0.1320 (0.1275)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	8,705	8,705	8,705	8,705
R-squared	0.0021	0.0005	0.0050	0.0074

Notes: The panel A of this table reports the DiD results for a subsample that includes only debit cards. The panel B presents the DiD results for a subsample that includes debit cards that are identified as having pre-high-school children. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Placebo Test: Adult Children Subsample

Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total Expenditure
	Total	EC	OCS	
After*Zhabei	0.0319 (0.0662)	-0.0169 (0.0513)	0.0488 (0.0418)	-0.4491*** (0.0800)
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	238,274	238,274	238,274	238,274
R-squared	0.000	0.000	0.000	0.004

Notes: This table reports the DiD results in a subsample only includes cardholders who are identified as having old children or no children. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Alternative Mechanism: Housing

<i>Panel A: Triple Interaction</i>				
Dep. Var.	(1)	(2)	(3)	(4)
	Expenditure on Children			Total Expenditure
	Total	EC	OCS	
After*Zhabei*House	0.192 (0.1718)	0.176 (0.1717)	0.016** (0.0072)	0.556* (0.3114)
Other Interactions	Yes	Yes	Yes	Yes
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	3,771,182	3,771,182	3,771,182	3,771,182
R-squared	0.001	0.001	0.001	0.005
<i>Panel B: Fourth Interaction</i>				
After*Zhabei*Child*House	-0.264 (0.220)	-0.226 (0.215)	-0.031 (0.047)	-0.748** (0.337)
Other Interactions	Yes	Yes	Yes	Yes
Card Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
Observations	3,771,182	3,771,182	3,771,182	3,771,182
R-squared	0.002	0.001	0.001	0.005

Notes: The panel A of this table presents the results of a cross-sectional analysis of the effect of the merger with respect to property ownership. The panel B presents the results of a cross-sectional analysis within a group that identified as having pre-high-school children, whether the effects of the merger differ between cardholders with and without properties. House is a dummy variable that equals 1 if a cardholder is identified as having properties in his or her own district, and 0 otherwise. Child is a dummy variable that equals 1 if a cardholder is identified as having young children, and 0 otherwise. Zhabei is a dummy variable that equals 1 if a cardholder resides in district Zhabei, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Dependent variables in Columns (1)-(3) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. Dependent variable in Column (4) is total expenditure. We include other interactions, card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: ** $p < 0.01$, * $p < 0.05$, $p < 0.1$.

Table 12: The Impact of the Merger: Jing'an *vs* Huangpu

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	Full Sample			Young Children Subsample		
Expenditure on Children	Total	EC	OCS	Total	EC	OCS
After*Jing'an	0.010*** (0.0027)	0.002 (0.0017)	0.008*** (0.0020)	0.127* (0.0715)	0.077 (0.0695)	0.051*** (0.0196)
Card Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,837,605	1,837,605	1,837,605	90,358	90,358	90,358
R-squared	0.000	0.000	0.000	0.002	0.002	0.001

Notes: This table reports the differences in expenditure between cardholders in Jing'an and Huangpu before and after the merger. Jing'an is a dummy variable that equals 1 if a cardholder resides in district Jing'an, and 0 if in Huangpu. After is a dummy variable that equals 1 if the month is after August 2015, and 0 otherwise. All dependent variables are in logarithm. Columns (1)-(3) examine the full sample, and Columns (4)-(6) the subsample that includes only cardholders that are identified as having pre-high-school children. Dependent variables in Columns (1)-(3) or (4)-(6) are total child spending, extra-curriculum spending (EC) and other child support spending (OCS), respectively. The baseline specification includes card fixed-effects and month fixed-effects. Standard errors in parenthesis are clustered at the card level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix 1. Variable Definitions

Variables	Definitions
Total consumption	Monthly total expenditure of each card
Kid consumption	Monthly total expenditure on child-related items of each card
Extra-curriculum spending (EC)	EC includes expenditure on private institutions that provide extra-curriculum training and on stationary
Other child spending (OCS)	OCS includes expenditure on children clothing, toys, and zoos
After	Dummy variable that equals 1 if the month is after August, 2015, and 0 otherwise
Zhabei	Dummy variable that equals 1 if a cardholder resides in the district Zhabei, and 0 otherwise
Kid	Dummy variable that equals 1 if a cardholder spent on EC, OSC, formal schooling that is before high schools, and children hospitals one year before the merger, and 0 otherwise
Far	Dummy variable that equals 1 if a cardholder lives within 2 km of Gongkang Road Station, and 0 if within 2 km of Hanzhong Road Station
Income	Mean of monthly total consumption for each card before the merger
House	Dummy variable that equals 1 if a cardholder made payments to local property services agencies and real estate agencies one year before the merger, and 0 otherwise
Jing'an	Dummy variable that equals 1 if a cardholder resides in the district Jing'an, and 0 otherwise

Fig. A 1. Shanghai Map

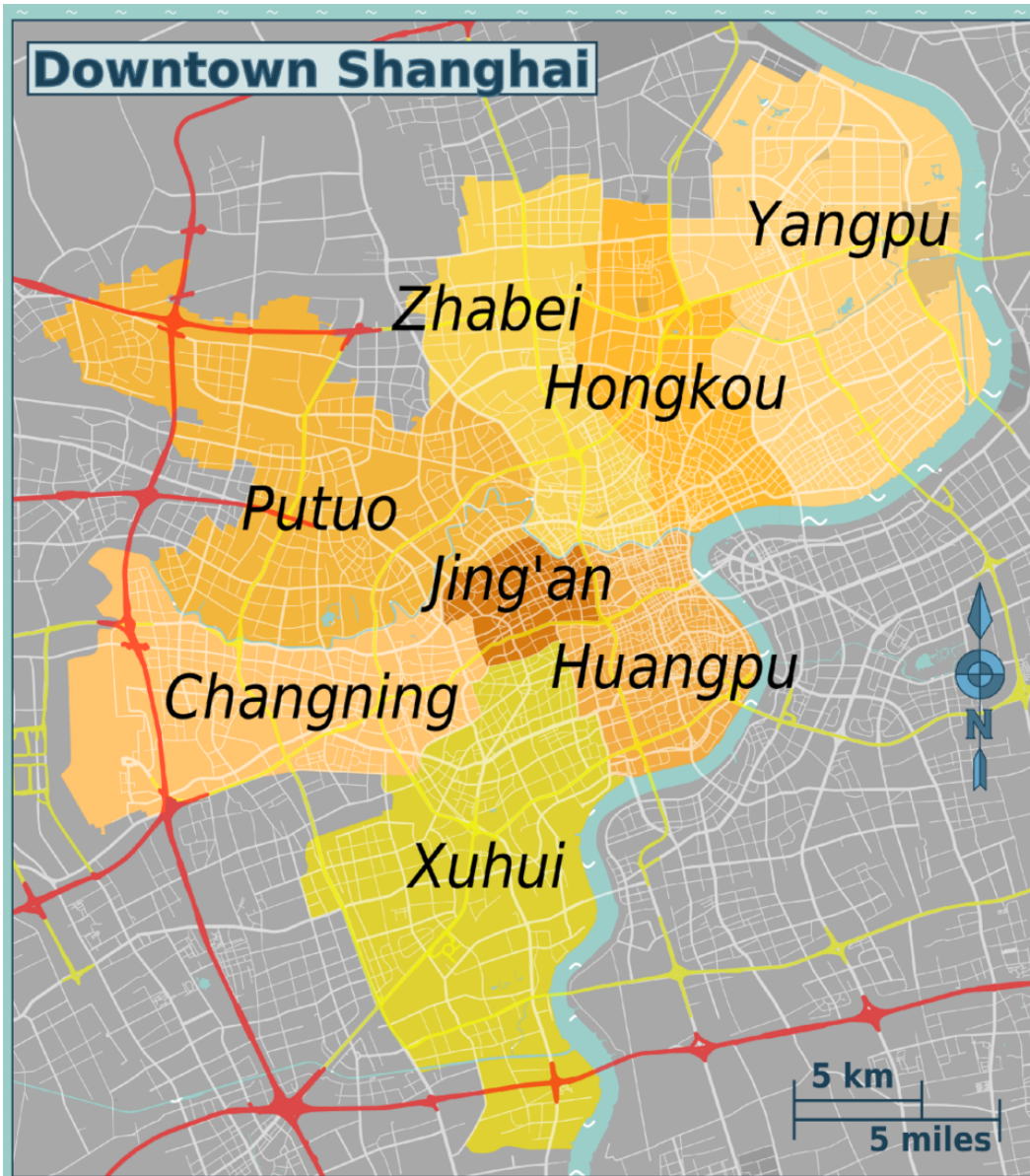


Fig. A 2. Public School Entrance in Shanghai

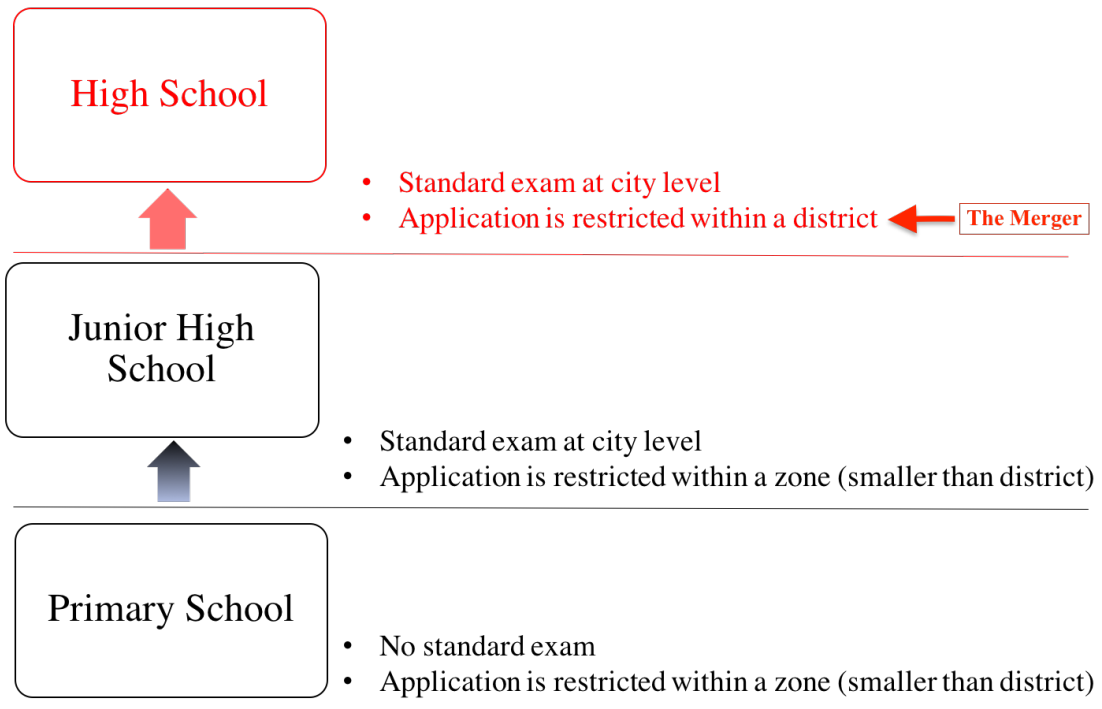
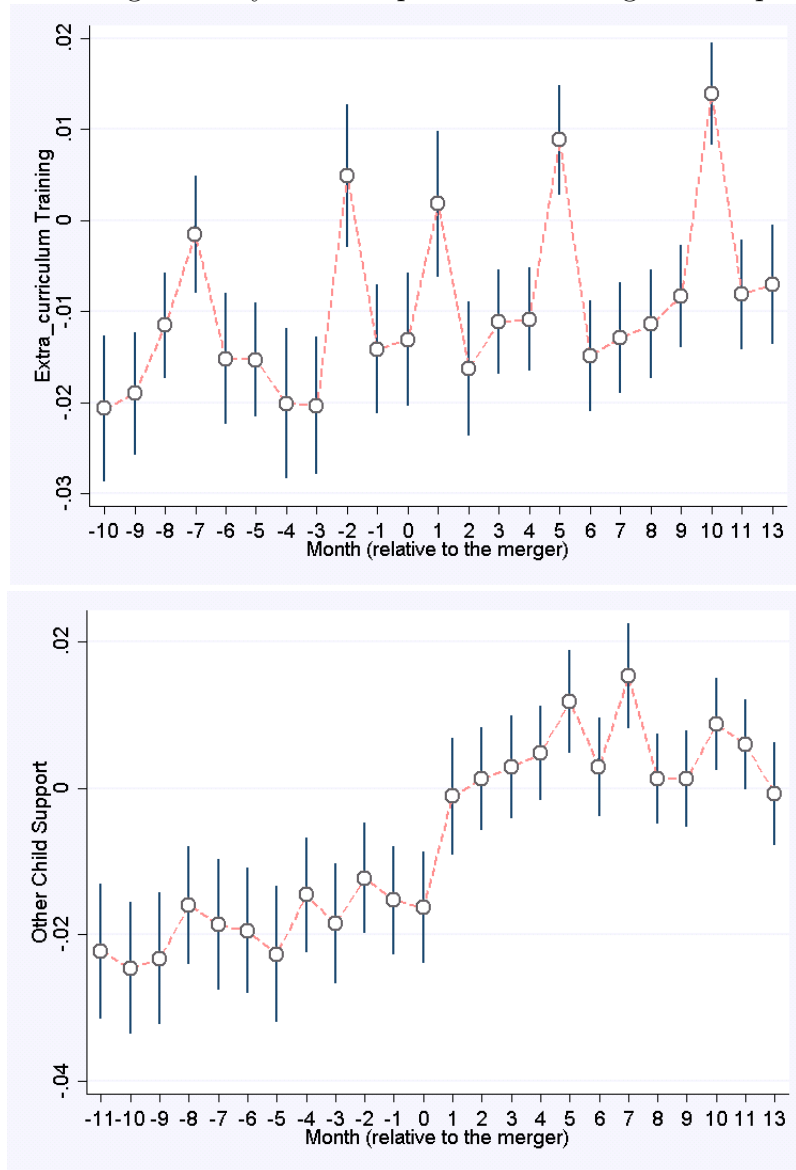


Fig. A 3. Underground Stations along Line 1 in Shanghai



Fig. A 4. Long-term Dynamic Impacts of the Merger on Expenditure



Notes: This figure presents the DiD coefficients with a 95% confidence interval of dynamic analysis similar in Table 4 but with a longer period data that starts 12 months before and ends 12 months after the announcement of the merger. The vertical line indicates months where month 0 corresponds to August 2015.