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Jinan University and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Effects of Childhood Peers on Personality Skills*

Despite extensive literature on peer effects, the role of peers on personality skill development remains poorly understood. We fill this gap by investigating the effects of having disadvantaged primary school peers, generated by random classroom assignment and parental migration for employment. We find that having disadvantaged peers significantly lowers conscientiousness, agreeableness, emotional stability, and social skill. The implied effects of a 10–15 percentage point change in the classroom proportion of disadvantaged peers are comparable to the effects of popular early childhood interventions. Furthermore, we find suggestive evidence that these effects are driven by the peers' personality skills.

JEL Classification: I21, D62, O15

Keywords: peer effect, noncognitive skill, left-behind children, human capital, Big-5

Corresponding author:
Shuaizhang Feng
School of Economics and Institute for Economic and Social Research
Jinan University
Guangzhou 510632
China
E-mail: shuaizhang.feng@jnu.edu.cn

* We would like to thank Alexandra and Lee Benham, Dalton Conley, Steve Durlauf, Joonhwi Joo, Elira Kuka, Xi Yang, Fabrizio Zilibotti, Junsen Zhang, Sisi Zhang, and the seminar participants at the Workshop on Peer Effects at Jinan University, the 4th Forum on Children of Migrants at Jinan University, the Brookings-Jinan China Microeconomic Policy Forum 2020, the 2020 International Symposium on Contemporary Labor Economics, the Graduate School of Policy Studies (GRIPS), the H-Trio Seminar at Seoul National University, the Korean Labour Economic Association monthly seminar, the Jinan University Brownbag seminar, the Econometric SocietyWinter Meeting 2020 at the University of Nottingham, and the 2021 HCEO-IESR faculty workshop for helpful comments and suggestions.

A growing body of literature documents the importance of childhood peers in shaping various life cycle outcomes. While peers may directly affect children's behaviors that generate short-run outcomes such as academic achievement, it is unclear how they affect long-run outcomes realized in adulthood. Some scholars suggest that children's personality skill may be the link between childhood peers and adult outcomes, given that academic achievement has been shown to play only a limited role as an underlying channel (Carrell, Hoekstra and Kuka 2018; Bietenbeck 2020). Evidence on the effects of childhood peers on personality skills remains inconclusive, however.

In this paper, we fill this gap by asking the following two questions: (i) What are the effects of childhood peers on personality skills? and (ii) Can the childhood peer effects be explained by peers' academic achievement or personality skills? Answers to these questions have important implications for the understanding of human capital development in childhood. First, finding peer effects would show that childhood peers are inputs in the development of personality skills, which generate subsequent human capital and a wide range of life cycle outcomes (Almlund et al. 2011). This would also support the conjecture made in the literature that personality skills are channels that connect childhood peers and long-run outcomes. Second, the importance of "nurture" would be reinforced in the nature-nurture debate of human capital development (e.g., Herrnstein and Murray 1994; Heckman 1995), given that childhood peers are a part of "nurture" that can be shaped by parenting or policy. Third, considering only academic achievement as a measure of peer quality and as an outcome of peer effects may lead to misleading evaluations of policies that affect peer composition.

We identify peer effects by comparing primary school children in China across classrooms based on their exposure to disadvantaged classmates. We represent the "disadvantaged" status of children by whether they are "left-behind children," meaning one or both of their parents temporarily migrated away from home. Many parents in rural areas find local wages insufficient to fund necessary household consumption and are forced to look for employment elsewhere. Their children are often left behind at home, however, because their residential designation, called *hukou*, makes it difficult for them to attend public schools and enjoy other public services in the host region. Despite remittances from migrating parents, left-behind children display lower academic achievement, more behavioral problems, and more depressive symptoms (Zhang et al. 2014; Li, Liu and Zang 2015; Meng and Yamauchi 2017).

Our identification strategy would be flawed if the distribution of disadvantaged peers across classrooms is correlated with other unobservable factors that also determine children's development. We make two assumptions to bolster our identification against this concern. First, we assume that children do not self-select into classrooms based on their own characteristics or those of their classmates. This assumption is supported by the fact that primary schools in our sample randomly assign students to classrooms, as required by the government mandate banning ability tracking in primary schools.[1] In addition, we surveyed school principals in our sample and confirmed that they had strong incentives to follow the random assignment protocols.

Second, we assume that parents' migration statuses are not endogenous to the characteristics of their children's peers. This assumption is necessary because the left-behind status of the children's peers could affect the parents' migration decisions. Furthermore, parents may be subject to temporal shocks or time-invariant unobservable characteristics that would affect both parents' migration decisions and the developmental outcomes of children attending the same schools.[2] To guard against these threats, we define left-behind children as those who were left-behind in the first semester of the first grade. Because outcome measures were collected when the children were in grades 4–6, we reduce identification threats from temporal shocks affecting migration decisions and reverse causality from the left-behind status of the children's peers. We also include school-cohort-wave fixed effects to account for time-invariant unobservable characteristics common to the children in the same schools.

A remaining identification threat is that parents may decide whether to migrate in response to their children's realized peer composition. We provide suggestive evidence that parents' migrations are motivated by household consumption needs rather than children's education. Furthermore, we test and do not reject the null effects of peer composition on parents' migration decisions.

In our analysis, we distinguish the peer effects "givers" and "recipients" by restricting the analysis sample to the children who were not left behind during the primary school period. This restriction helps us avoid the mechanical correlation between the characteristics of the peers and the target child that arises when leave-one-out averages are used (Angrist 2014; Carrell, Hoekstra and Kuka 2018) but limits our analysis to those from potentially better-endowed households.

Our data set consists of extensive surveys in China on children's home environment, school environment, and Big-5 personality traits, linked to school administrative data on test scores and classroom assignments. Personality skills are measured based on the Big-5 model of personality, which has been extensively validated in economics and psychology literature. For example, Almlund et al. (2011) show that Big-5 measures predict schooling and earnings as much as cognitive skill measures such as IQ scores. Big-5 measures have also been used to investigate the effects of personality skills on labor market performance (Dohmen and Falk 2011; Deming 2017) and marital sorting (Dupuy and Galichon 2014).

[1]Chung and Zou (2020), Huang and Zhu (2020), Wang (2021), and Xu, Zhang and Zhou (2020) also exploited random classroom assignment to study peer effects in Chinese schools.

[2]School assignment is based on geographical proximity.

We find that being exposed to disadvantaged peers has significant negative effects on the personality skill development of children in primary school, but not on their academic achievement. A 10 percentage point increase in the classroom proportion of left-behind children in the fourth grade reduces conscientiousness by 0.182, agreeableness by 0.190, emotional stability by 0.237, and social skill by 0.147 in standard deviation unit, measured in grades 4–6. The implied effects of a 10 to 15 percentage point change in the proportion of left-behind peers are comparable to the effects of highly effective early childhood interventions such as the Perry Preschool Program and the Project STAR (Elango et al. 2016; Chetty et al. 2011). We also show that left-behind children are primarily disadvantaged in personality skills but not in academic achievement, suggesting that peer effects on personality skills are mainly driven by the average personality skills of the left-behind peers.

Our study contributes to the literature on human capital development by showing that childhood peers are important inputs in the production of personality skills. Prior literature showed that personality skills respond to one's previous human capital accumulation, parental investment, education, childhood interventions, and even life-threatening events, but it remained an open question whether peers affect personality (i.e., noncognitive) skills (Almlund et al. 2011; Cunha, Heckman and Schennach 2010; Ertac 2020). We fill this gap by showing substantial childhood peer effects on personality skills as measured by the Big-5 inventory. The results are also consistent with the conjecture that personality skills are the channels behind childhood peer effects on long-run outcomes (Carrell, Hoekstra and Kuka 2018; Bietenbeck 2020).

Our results underscore the need to account for personality skills both as outcome measures and as measures of peer quality. Failing to account for personality skills as outcome measures would have led us to underestimate the negative effects of disadvantaged peers. Furthermore, we would not have been able to capture the left-behind children's disadvantage in personality skills if we focused only on their academic achievement. Finally, given that personality skills play an important role in explaining the effects of disadvantaged peers, a viable policy response to counteract negative peer effects may be to improve the personality skills of children. Several programs have been shown to improve children's personality skills, which are malleable throughout childhood and adolescence (Elango et al. 2016).

The remainder of the article is organized as follows. In Section 1, we place our study in the human capital development literature and the literature on peer effects in education. In Section 2, we discuss the institutional background and empirical strategy. Section 3 presents the empirical results, and Section 4 discusses results related to the channels of peer effects. Section 5 presents discussion and interpretation of results, followed by conclusion in Section 6.

1 Literature Review

1.1 Personality Skill Development

Personality skills[3] are inputs in the production of cognitive skills and a wide range of life cycle outcomes such as schooling, earnings, health, and crime (Almlund et al. 2011). Personality skills remain malleable throughout childhood and adolescence, whereas the productivity of cognitive skills declines with age (Cunha, Heckman and Schennach 2010). Known determinants of personality skills include genetic transmission, prior accumulation of human capital, education, parenting, family structure, income, natural disasters, and health shocks (Cunha, Heckman and Schennach 2010; Akee et al. 2018; Marsaudon 2019; Ertac 2020).

Peer influence has not been formally incorporated and rigorously investigated as an input in the production of personality skills in childhood, although scholars recognized that peer groups could potentially play an important role (Harris 1995; Gifford-Smith and Brownell 2003; Little 2020). Models of peer interaction in developmental psychology suggest that children sort into peer groups based on similarities in observable characteristics and behaviors at least from the first grade in primary school. Peers in these groups serve as sources of information or role models for behavioral or cultural norms. Children adopt these norms to assimilate into peer groups or maintain social relationships, shaping their academic and personality development in the process. This process implies that children in the same peer group would show similarities in behaviors and preferences, as reported in the empirical literature in both psychology and economics (Ertac 2020). The literature does not extend to showing the causal effect of peers on personality skill development, however.

1.2 Peer Effects on Academic Achievement and Noncognitive Skills

The bulk of the literature on childhood peer effects is focused on short-run effects on academic achievement as measured by test scores. Most studies found that being exposed to high-skilled or high-achieving peers has a positive impact on academic achievement,[4] though exceptions exist.[5] Other studies, similar in design to our own, focused on the effects of being exposed to low-skilled or disadvantaged peers, showing a negative impact

[3] These are also known as noncognitive skills, character skills, behavioral skills, or socio-emotional skills.

[4] Given that the literature is large, we mainly focus on studies examining the effects of peers in middle school or below. Studies showing evidence from Western countries include Hoxby (2000), Betts and Zau (2004), Hoxby and Weingarth (2006), Vigdor and Nechyba (2007), Lavy, Paserman and Schlosser (2011), Lavy and Schlosser (2011), Imberman, Kugler and Sacerdote (2012), Lavy, Silva and Weinhardt (2012), Black, Devereux and Salvanes (2013), Burke and Sass (2013), Sojourner (2013), Fruehwirth (2014), Gibbons and Telhaj (2016), Balestra, Eugster and Liebert (2020), Fletcher, Ross and Zhang (2020), and Balestra, Sallin and Wolter 2021, among others. Kang (2007) studies a sample from South Korea. An increasing number of studies focused on evidence from China: Ding and Lehrer (2007), Carman and Zhang (2012), Li et al. (2014), Lu and Anderson (2015), Min et al. (2019), Wang and Zhu (2019), Chung and Zou (2020), and Wang (2021).

[5] Angrist and Lang (2004), Ohinata and Van Ours (2013), and Abdulkadiroğlu, Angrist and Pathak (2014) did not find evidence of childhood peer effects on academic achievement. Dobbie and Fryer Jr (2014) also reported null effects, using a sample of high school students.

on academic achievement.[6] These effects are observed for preschool and kindergarten peers as well (Graham 2008; Elder and Lubotsky 2009; Neidell and Waldfogel 2010; Bietenbeck 2020).

Focusing on the effects of peers' behavioral aspects on academic achievement, Neidell and Waldfogel (2010) found that kindergarten children's cognitive achievement suffered when their peers displayed more aggressive behaviors related to classroom disturbance. Golsteyn, Non and Zölitz (2021) found that having more persistent peers in the same study section improved the academic achievement of college students.

Compared with the number of studies on academic achievement outcomes, relatively fewer studies examined peer effects on noncognitive skill outcomes. For example, Xu, Zhang and Zhou (2020) found that low-ability classmates in Chinese middle schools had negative peer effects on school engagement, emotions, and educational expectations. Imberman, Kugler and Sacerdote (2012) found that an increase in the number of evacuee students in schools resulting from Hurricanes Katrina and Rita led to increases in absenteeism and disciplinary infractions of incumbent students in Houston's secondary schools. Bifulco, Fletcher and Ross (2011) showed that high school students exposed to disadvantaged peers displayed more unruly behavior at school.

Zárate (2019) conducted a randomized experiment at Peruvian high schools in which the students were divided into four groups based on academic achievement and social skill. By randomly assigning students to dormitories, he showed that high-social-skill students improved the social skill of their low-social-skill peers, but high-achieving peers did not improve their peers' social skill or academic achievement. This study provides evidence that social skill can be more "contagious" than academic achievement among high school students. Our study adds to Zárate's by showing peer effects among primary school students on different measures of personality skills as measured by the Big-5 inventory.

1.3 Potential Mechanisms of Peer Effects

Some studies found suggestive evidence that students' behaviors are important channels underlying peer effects in schools. Lei (2021) found that adolescent boys exposed to peers from disrupted home environments did not display lower academic achievement but displayed more problematic behaviors in the short run and worse long-run outcomes in adulthood, suggesting that behaviors were channels underlying peer effects. Hong and Lee (2017) found that peer effects on academic outcomes among college students were stronger when the students shared similar personality characteristics, implying similar intermediary roles of personalities.

Others conjectured further that behaviors or noncognitive skills may mediate long-run effects of childhood

[6] These include Gould, Lavy and Paserman (2009), Carrell and Hoekstra (2010), Lavy, Paserman and Schlosser (2011), Kristoffersen et al. (2015), Ahn and Trogdon (2017), Hu (2018), Balestra, Eugster and Liebert (2020), Huang and Zhu (2020), Xu, Zhang and Zhou (2020), and Zhao and Zhao (2021).

peers. Carrell, Hoekstra and Kuka (2018), while showing that exposure to disadvantaged peers in primary school led to lower earnings in adulthood, noted that the effects on test scores in adolescence could not fully explain the effects on earnings. They proposed changes in noncognitive skills as a potential explanation for the remaining variations, but their evidence was not statistically significant. Bietenbeck (2020) examined the effects of peers who repeated kindergarten (plausibly representing disadvantaged status) on intermediate childhood outcomes and long-run educational attainment. The negative peer effects on academic achievement faded out after a year. Surprisingly, students exposed to the repeaters had better intermediate behavioral outcomes and long-run educational attainment. The author conjectured that teachers, parents, and students might have adjusted behaviors to improve the noncognitive skills of affected students, leading to better educational attainments.

2 Background and Empirical Strategy

2.1 Parental Migration and the Left-Behind Children (LBC)

Many parents in rural China temporarily migrate to urban regions to find employment, leaving their children behind at home for months or even years under the care of the remaining parent, grandparents, or relatives. As of 2015, the number of children left behind by one or both migrating parents reached 69 million, representing a quarter of all children in China (UNICEF 2020). LBC suffer various disadvantages compared to children who are not left-behind in academic achievement, health, and well-being (Zhang et al. 2014; Li, Liu and Zang 2015; Meng and Yamauchi 2017).

Parents choose migration primarily because limited income and employment opportunities in their home region are insufficient for household consumption needs. Studies of temporary migrant workers, which include migrating parents of LBC, show that the most important determinants of migration are higher income in the destination areas than in the home areas (Li and Zahniser 2002; Meng and Zhao 2018). Guang and Zheng (2005) argued that workers consider migration a "second best option" and only migrate when comparable local employment is unavailable, given the material and psychic cost of migration and the poor working and living conditions many temporary migrants experience in the host regions.

These migrant workers are discouraged from taking their children with them in part because of the household registration system called *hukou*. The *hukou* system designates each person a resident of a specific location and as either a "rural" or "urban" type. (Song 2014). Because of this system, migrant workers face significant challenges accessing various public resources in the host region, such as social insurance coverage, social welfare benefits, and public education for their children. For example, attending public schools in the host region is difficult for the children of migrant workers because public schools prioritize children with local *hukou* for the limited spaces. Many migrant students enroll in private migrant schools, which are typically of lower quality and higher cost than public schools (Chen and Feng 2013, 2017).

Remittances from the migrating workers are spent primarily on consumption rather than investment or housing (Zhu et al. 2014; Démurger and Wang 2016). Education expenditure does not increase, and may in fact decrease, for these households (Démurger and Wang 2016; Askarov and Doucouliagos 2020). Migrant workers are more likely to send home remittances when they received assistance from home at the start of the migration and when the family faces large medical expenses, but migrant workers' own education level and family size do not predict remittance decisions (Cai 2003; Akay et al. 2014).

In summary, we believe that most migrating parents with LBC aim to meet household consumption needs at the expense of their own immediate well-being and to the disadvantage of their children. This explanation is consistent with the literature on temporary migrants, remittances, and the LBC discussed in this section. It is also in accordance with the evidence from studies using in-depth interviews with the primary caregivers of LBC (Mu and Hu 2016; Zhang et al. 2016). These studies show that primary motivations for parental migration include "making ends meet," making contributions to social events such as weddings and funerals, and raising funds for their sons' future marriages. The caregivers in these studies were concerned about the difficulty of compensating for the potential negative effects of parental migration on children's educational attainments. Zhang et al. (2016) noted that none of the caregivers mentioned funding the cost of education as one of the motivations for parental migration.

2.2 Identification

We identify childhood peer effects by comparing children in the same primary schools across classrooms according to their exposure to disadvantaged classmates. An important threat to this identification strategy is selection bias (Manski 1993). Because we define peers at the classroom level, our identification strategy is undermined if children self-select into classrooms based on characteristics that are correlated with both their own personality development and the classroom proportion of LBC. For instance, if children with higher personality skills are systematically assigned to classrooms with fewer LBC, then the effects of left-behind peers would be biased downward. This selection bias problem would not arise if children were assigned to classrooms regardless of the characteristics related to personality skill development and left-behind status. Random assignment is one such mechanism.

Our first identification assumption is that children do not self-select into classrooms. This assumption is based on the institutional feature of the primary schools in our sample such that children were assigned to classrooms at random, conditional on some observable characteristics.[7] In our sample, children were randomly assigned to classrooms twice, once in the first semester of the first grade and once in the first semester of the fourth grade. Assignments in the first grade were explicitly balanced in gender within classrooms but were otherwise random. Assignments in the fourth grade were conditional on gender and (in some cases) academic achievement in the third grade to balance the distribution of initial academic achievements across classrooms.

To further bolster our confidence on this assumption, we conducted interviews with the principals of the schools in our sample. According to the interviews, schools have a strong incentive to ensure randomization so as to avoid complaints from anxious parents and to avoid burdening teachers with overly disruptive classes. Schools were also subject to a government mandate that forbids tracking based on academic performance at the primary school level (Strauss 2013). Finally, we conduct a series of tests in Section 2.5 and show that the observed patterns are consistent with random assignment of students to classrooms.

Another threat to identification is that parents' migration decisions may be endogenous to the personality skill development of their children's peers. First, there may be a reverse-causality problem in which a parent's migration decision depends on the migration status of other parents. The parent may have received assistance or information related to labor migration from other parents or may have felt more open to migrating after learning that left-behind status was common among the child's classmates. Second, parents' migration decisions may be related to temporal shocks, such as economic hardship, or time-invariant characteristics that affect both the migration decision of the parents and the personality skill development of the children. These characteristics may be correlated within schools because school assignment is based on geographic proximity (Dong and Li 2019).

Our second identification assumption is that the children's left-behind status is determined independently of their classmates' characteristics, including their left-behind statuses and personality skill development. We support this assumption in several ways. First, we define left-behind status as having been left behind in the first semester of the first grade, immediately after the initial random classroom assignments were made. Because outcome measures were collected between grades 4 and 6, this strategy minimizes the possibility that parents' migration decisions are affected by the parents of other children attending the same primary school.[8] It also minimizes the reverse-causality channel from the target children's outcomes (in grades 4 to 6)

[7] Random assignment is a common feature of primary and secondary education in China, which has often been used to study education peer effects in the literature. Examples include Hu (2018), Wang and Zhu (2019), Chung and Zou (2020), Huang and Zhu (2020), Wang (2021), Wang and Zhu (2021), and Zhao and Zhao (2021).

[8] The catchment area for each school covers areas with tens of thousands in population, making it unlikely that parents knew each other

to the migration decision of their peers' parents (in grade 1), and the effects of temporal shocks. Second, we include school-cohort-wave fixed effects in our empirical specification, so that our estimates are comparisons across classrooms and within school, cohort, and wave. Because school assignment is based on geographical proximity, the fixed effects strategy can account for location-based time-invariant unobservable characteristics shared by children attending the same school.

The second identification assumption may yet be violated if the parents of the target children decide whether to migrate after observing the realized classroom composition of the children's peers. In that case, peer effects would be confounded with the child's own left-behind status even with random classroom assignment. We believe such cases are likely to be rare, however. As discussed in Section 2.1, the literature on temporary migrant workers shows that children's education was not among the primary motivations for parental work migration, and educational expenditures did not increase with remittances. Instead, most migrant workers aimed to meet the immediate consumption needs of the household, enduring harsh conditions in unskilled, temporary jobs. We also test whether the realized classroom compositions are associated with the parents' migration status in Table A4 of the Web Appendix. The test results do not reject the identification assumption.[9]

We restrict the analysis sample to those who were never left behind during the primary school period. Using the entire sample with leave-one-out averages as the main peer effect variables leads to mechanical correlation between the child's characteristics and those of the child's peers.[10] We break this mechanical correlation by distinguishing the "receivers" (never-LBC) from the "givers" (LBC) and restricting the analysis sample to the "receivers" (Angrist 2014; Carrell, Hoekstra and Kuka 2018). By doing so, however, the analysis sample now consists of children with potentially better home environments than the population of primary school children.

2.3 Empirical Model

Our baseline model is

$$Y_{it} = \alpha + \gamma_1 \overline{LB^{class1a}_{-i,1a}} + \gamma_2 \overline{LB^{class4a}_{-i,1a}} + \theta X_{it} + \rho_{sct} + \varepsilon_{it}, \tag{1}$$

where Y_{it} is the outcome of child i in grade t. $\overline{LB^{class1a}_{-i,1a}}$ is the proportion of classmates in the first semester of the first grade who were left behind during that semester. $\overline{LB^{class4a}_{-i,1a}}$ is the proportion of classmates in the first semester of the fourth grade who were left behind in the first semester of the first grade. As discussed in Section

before their children started attending school together.

[9]If parents indeed abstained from migration out of greater concern for their children when they were more exposed to disadvantaged peers at school, our estimates can be interpreted as upper-bound estimates because these parents would be more protective of their children from the peers' influence.

[10]For example, the academic achievement of students highly ranked within a classroom would be negatively correlated with the leave-one-out averages of their classmates, most of whom are ranked lower.

2.2, left-behind status is defined as having been left behind in the first semester of the first grade. Peer groups are measured in the first semesters of the first grade and the fourth grade to avoid concerns about endogenous switching of classrooms after random classroom assignments. $\overline{LB}^{class1a}_{-i,1a}$ and $\overline{LB}^{class4a}_{-i,1a}$ represent the first grade and the fourth grade peers, respectively. Their variations are caused by fluctuations in the proportions of the LBC at the classroom level within each school-cohort due to random assignment processes. γ_1 and γ_2 are the coefficients of interest.

Other control variables include the child's gender; sibling composition (a set of indicators for being the only child, having one older sibling, one younger sibling, two or more siblings, and unknown); mother's education level (high school attendance or more, junior high school attendance, primary school attendance or less, and unknown); math and Chinese test scores in the second semester of the third grade; teacher's characteristics including gender; marital status (single, married, or divorced); age; work experience; education level (university, vocational college, less than college); income; main subject (Chinese or other); and class sizes in the first semester of the first grade and of the fourth grade. We include ρ_{sct}, the school-cohort-wave fixed effects, to account for potential time-invariant unobservable characteristics affecting both personality skill development and parents' migration status. The error term ε_{it} consists of unobserved individual-level and class-level factors that contribute to the outcome, clustered at the school-cohort level.

2.4 Data

We use a data set called the Longitudinal Study of Children's Development in Mianzhu. It is designed and collected by the Survey Data Center at Jinan University in Guangzhou (Survey Data Center 2017–2018), to capture the developmental environment and outcomes of children in rural China. Over 6,000 children were interviewed in 18 primary schools in Mianzhu county, Sichuan province. We use the first two waves of the survey. The first wave was collected in October 2017, the second wave in November 2018. The children were in grades 4 through 6 at the time of the survey. Their parents, primary guardians at home (if parents were migrating), and teachers were also surveyed in person. Migrant parents were separately surveyed by telephone. Children's IQ scores were measured using Raven's matrices test (Raven and Raven 2003). Children's math and Chinese test scores and the classroom assignment for each semester since grade 1 were collected from school administrative data.

Children's left-behind status is measured by the following questions: "Did your father/mother leave home for work, for at least three months, returning home no more than once a week?" Children responded for each parent and for each semester from the first semester of the first grade up until the time of the survey.

Table 1: Summary Statistics

	Entire Sample (1)	Never LB (2)	LB in 1a (3)	Difference (3) - (2) [p-Value]
Urban	0.236	0.433	0.129	-0.303 [0.000]
Female	0.499	0.527	0.485	-0.042 [0.001]
Mother's Education ≥ 10	0.250	0.384	0.182	-0.202 [0.000]
Mother's Education 7-9	0.375	0.360	0.390	0.029 [0.012]
Mother's Education 0-6	0.239	0.187	0.271	0.085 [0.000]
Mother's Education Unknown	0.136	0.069	0.158	0.089 [0.000]
Family Size	1.506	1.475	1.486	0.011 [0.478]
Class Size in Semester 1a	43.446	45.334	42.427	-2.907 [0.000]
Class Size in Semester 4a	42.274	44.806	40.782	-4.024 [0.000]
Teacher Female	0.629	0.668	0.603	-0.065 [0.000]
Teacher Married	0.817	0.863	0.817	-0.047 [0.000]
Teacher's Age	40.901	41.083	41.173	0.090 [0.675]
Teacher's Years of Experience	21.737	21.963	22.108	0.146 [0.594]
Teacher's University Degree	0.273	0.289	0.261	-0.028 [0.009]
Teacher's Income (in 1000 CNY)	3.780	3.769	3.790	0.021 [0.251]
Respondent-Teacher Teaching Chinese	0.648	0.671	0.650	-0.021 [0.061]
Math	0.000	0.105	0.006	-0.099 [0.000]
Chinese	0.000	0.109	-0.009	-0.118 [0.000]
IQ	0.000	0.054	0.020	-0.033 [0.147]
Openness to Experience	0.000	0.084	-0.035	-0.119 [0.000]
Conscientiousness	0.000	0.128	-0.057	-0.185 [0.000]
Extroversion	-0.000	0.069	-0.023	-0.092 [0.000]
Agreeableness	-0.000	0.093	-0.047	-0.139 [0.000]
Emotional Stability	-0.000	0.099	-0.062	-0.162 [0.000]
Social Skill	-0.000	0.106	-0.052	-0.159 [0.000]
N	11843	3087	3935	
Number of Individuals	8113	2272	2847	
Number of Class-Cohorts	199	181	182	
Number of School-Cohorts (Clusters)	70	64	64	
Number of Schools	18	18	18	

Notes: p-values in brackets are calculated using standard errors clustered at the school-cohort level. Semester 1a indicates the first semester of the first grade. LB: left-behind.

Measures of the children's personality skills are based on the Big-5 model of personality, one of the most popular models of personality in economics and psychology (Almlund et al. 2011; Humphries and Kosse 2017) and verified in different countries (Schmitt et al. 2007). The Big-5 model describes a person's personality using five sub-dimensions: openness to experience, conscientiousness, extraversion, agreeableness, and emotional stability (also known as neuroticism). In addition to the Big-5 measure, we also study a social skill measure based on a short version of the Qingnian Zhongguo Personality Scale (Wang and Cui 2004), which measures one's tactfulness in social interactions. This measure is relatively unexplored in the literature. Questionnaires for the Big-5 measure and the social skill measure appear in Table A1 of the Web Appendix.

Individual personality skills are estimated using factor analysis, where we use each questionnaire item to estimate factor scores. These are then normalized to be mean 0 and standard deviation 1 within school-cohort-wave. Each Big-5 subscale has 4 item measures, social skill 9 item measures. We focus on the measures reported by teachers. Although the data set contains measures reported by the primary guardians and the children themselves as well, teacher-reported measures have the lowest attrition rate and the highest reliability in our sample (Feng et al. 2021). We report robustness of our results using all available measures in Table A11 of the Web Appendix.

The survey data set is linked to an administrative data set from schools and Mianzhu county's bureau of education. The administrative data includes students' math and Chinese test scores and classroom assignments from the first semester of the first grade until the semester current at the time of the survey.

We place two additional sample restrictions to create the analysis sample. First, we restrict the sample so that teacher-reported personality measures are based only on homeroom teachers, who would have interacted most with the sample children. Whereas the 2018 wave teacher responses were reported entirely by homeroom teachers, 27 out of 138 teachers who responded in the 2017 wave were not the homeroom teachers of the sample children. For consistency, we only use observations where the reports were made by the homeroom teacher of each child, dropping 232 observations from the 2017 wave sample.

Second, 4.66% of the children in the sample had different class numbers between the first semester of the first grade and the second semester of the third grade, meaning that they switched classes during this time period. They were included in the sample with their original classroom assignment. As a robustness check, we restrict the sample to those whose class number remains the same between the first grade and the third grade. These results appear in Section 3.4,

Descriptive statistics of the entire sample and the analysis sample are shown in Table 1. The first column presents the descriptive statistics of the entire sample available in the data, regardless of the left-behind status of the children. The second column represents the analysis sample, consisting of those who were not left behind between the first grade and the time of the survey. They are the "receivers" of peer effects. The third column represents those who were left behind in the first semester of the first year, the "givers" of peer effects.

Table 1 shows that compared to the never-LBC, LBC are mostly in rural areas and from low-SES households as measured by the mother's level of education, consistent with the characteristics of LBC shown in other studies (Mu and Hu 2016). Average class size is smaller for the LBC, possibly to their advantage. Other school environment and teacher characteristics are mostly similar between the two groups. However, the LBC are significantly disadvantaged in personality skills, and, to a lesser extent, in academic achievements. The difference in average IQ score is small and insignificant. Compared with average children in the entire sample, children in the analysis sample are relatively advantaged in family background and skill measures, while the LBC are relatively disadvantaged in these measures.

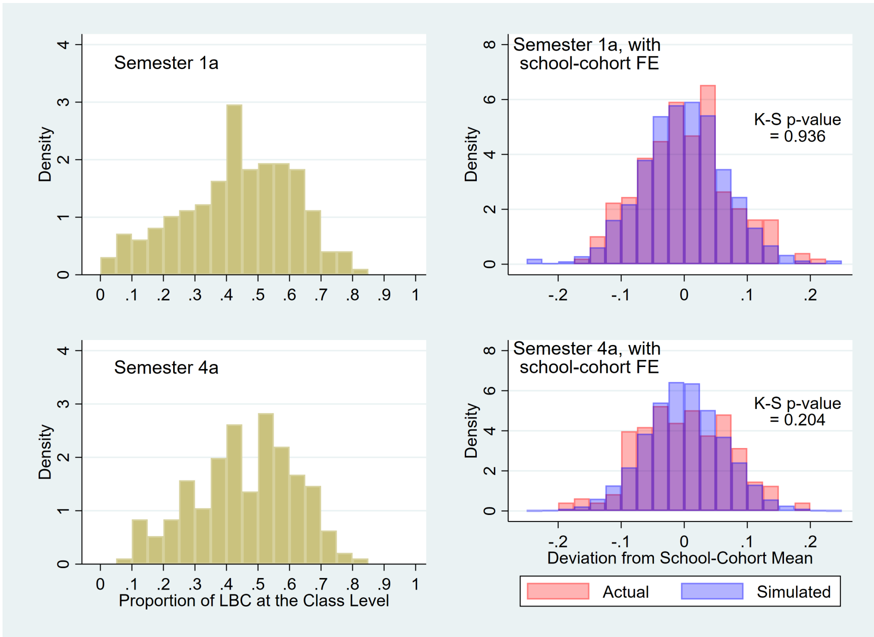
2.5 Balance Tests

Our identification strategy critically relies on the assumption that students were randomly assigned to classrooms. Although there are institutional features in place that make it highly likely that random assignment was carried out, we provide empirical tests to further bolster our assumption. Left-behind status is defined as having been left behind in the first semester of the first grade, consistent with our identification strategy. For the tests in Figure 1, we include each child only once in the sample by dropping the repeated appearance in the second wave, to avoid double counting when testing for random classroom assignment.

The first column of Figure 1 shows the classroom distribution of LBC in the first semesters of the first grade and the fourth grade. The classroom proportion of LBC is spread out between 0.1 and 0.9, with slightly higher concentration around 0.4. If the students were sorted by the left-behind status or related characteristics, there would be more classrooms at the either end of the distribution.

In the second column, we follow Bietenbeck (2020) by performing 1,000 Monte Carlo simulations in which students were randomly assigned to classes within school-cohorts in each wave. The number and the size of classes and the number of LBC in each school-cohort was fixed at their values in the original data. In each of the simulated samples, we regress the left-behind status indicator variable on school-cohort fixed effects and collect the residuals. We then calculate the mean of this residual for each class by wave. We draw a histogram of these means from the pooled 1000 sets of simulated residuals and compare it with the histogram based on the actual data, on the right column of Figure 1. We test and do not reject the equality of the two distributions using the two-sample Kolmogorov-Smirnov test.

Figure 1: Distribution of Left-Behind Children across Classrooms



Note: Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. LB: left-behind. To save space and focus on the range of deviations that really matters in the FE residual histograms, we combine distributions outside of (-0.25, 0.25) into the bins on the two edges of this interval. The height of the bars are scaled so that the sum of their area equals 1.

In the Web Appendix, we provide additional balance tests. First, we test the equality of distributions between the distribution of peers' characteristics across classrooms and the simulated distribution for each of the peers' characteristics (Figures A1–A7). Second, we test whether the classroom proportion of LBC is associated with the child's characteristics and teacher's characteristics (Table A2). Third, we test whether the child's characteristics are associated with those of her classmates (Table A3). All of our test results are consistent with our identification assumptions.

3 Effects of Childhood Peers on Personality Skills

3.1 Baseline Peer Effects

Table 2 presents the estimates of peer effects from Equation (1), where outcome measures are normalized to be mean 0 and standard deviation 1 within each school-cohort-wave. The estimates show that a 10 percentage point increase in the proportion of LBC in the fourth grade classroom lowers never-LBC's conscientiousness by 0.182, agreeableness by 0.190, emotional stability by 0.237, and social skill by 0.147 in standard deviation unit. These personality skill measures, in particular conscientiousness and emotional stability, are highly predictive of years of education and earnings (Almlund et al. 2011). Once accounting for the effects of fourth-grade peers, the first-grade peers are not predictive of personality skills in grades 4–6. While first graders may have had meaningful peer interactions (Harris 1995), it is impossible to distinguish whether the effects from the first-grade peers faded out or were nonexistent because the first-grade LBC were not yet clearly disadvantaged.

The lower panel of Table 2 shows that math grades, Chinese grades, and IQ scores are not affected by left-behind peers. This result is consistent with those of Wang and Zhu (2021) who found negative effects of left-behind peers on mental health but not on academic achievement in middle school.

Table 2: Impacts of Past and Current Left-Behind Peers on Children's Outcomes

Dependent =	**Consc** (1)	**Agree** (2)	**Extro** (3)	**Openn** (4)	**E.Stability** (5)	**Social** (6)
Proportion of 1a-Peers LB in 1a	0.252 (0.285)	0.114 (0.327)	0.065 (0.394)	0.226 (0.324)	0.579 (0.427)	0.065 (0.313)
Proportion of 4a-Peers LB in 1a	-1.824 (0.520)	-1.904 (0.629)	-0.688 (0.482)	-0.518 (0.632)	-2.370 (0.606)	-1.469 (0.599)
Other Controls	✓	✓	✓	✓	✓	✓
School-Cohort-Wave FE	✓	✓	✓	✓	✓	✓
F-Test for Two LB Proportions	6.2	4.6	1.1	0.4	7.7	3.2
p-Value for F-Test	[0.004]	[0.013]	[0.337]	[0.673]	[0.001]	[0.050]
N	3087	3087	3087	3087	3087	3087
Individuals	2272	2272	2272	2272	2272	2272
Class-Cohorts	181	181	181	181	181	181
School-Cohorts (Clusters)	64	64	64	64	64	64
Schools	18	18	18	18	18	18
Dependent =	**Math** (1)	**Chinese** (2)	**IQ** (3)			
Proportion of 1a-Peers LB in 1a	-0.302 (0.248)	-0.262 (0.270)	0.207 (0.279)			
Proportion of 4a-Peers LB in 1a	0.022 (0.264)	-0.091 (0.241)	0.384 (0.314)			
Other Controls	✓	✓	✓			
School-Cohort-Wave FE	✓	✓	✓			
F-Test for Two LB Proportions	1.2	0.5	1.1			
p-value for F-Test	[0.321]	[0.605]	[0.333]			
N	3087	3087	3087			
Individuals	2272	2272	2272			
Class-Cohorts	181	181	181			
School-Cohorts (Clusters)	64	64	64			
Schools	18	18	18			

Notes: Standard errors are clustered at the school-cohort level in parentheses. The sample is restricted to children who had never been left behind since grade one. Other control variables include: dummies for mother's education levels and number and birth order of siblings; child's gender; child's math and Chinese normalized test scores in the second semester of grade 3; teacher's gender; age; experience; education level; marital status; income and the subject of teaching; the size of the current class and the reference child's grade-one class. Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. *Consc*: conscientiousness; *Agree*: agreeableness; *Extro*: extroversion; *Openn*: openness to experience; *E.Stability*: emotional stability; *Social*: social skill.

3.2 Nonlinear Effects

Estimates from Equation (1) do not capture potential nonlinearities in effects across the range of the classroom proportion of LBC, which is distributed widely between 0 and 1 (Figure 1). In this section, we test whether peer effects remain the same at different levels of classroom LBC proportions. The model is:

$$\begin{aligned} Y_{it} = \alpha &+ \gamma_1 \times \mathbb{1}(\overline{LB^{class1a}_{-i,1a}} \leq \text{median}) \times (\overline{LB^{class1a}_{-i,1a}} - \text{median}) \\ &+ \delta_1 \times \mathbb{1}(\overline{LB^{class1a}_{-i,1a}} > \text{median}) \times (\overline{LB^{class1a}_{-i,1a}} - \text{median}) \\ &+ \gamma_2 \times \mathbb{1}(\overline{LB^{class4a}_{-i,1a}} \leq \text{median}) \times (\overline{LB^{class4a}_{-i,1a}} - \text{median}) \\ &+ \delta_2 \times \mathbb{1}(\overline{LB^{class4a}_{-i,1a}} > \text{median}) \times (\overline{LB^{class4a}_{-i,1a}} - \text{median}) \\ &+ \theta X_{it} + \rho_{sct} + \varepsilon_{it}. \end{aligned} \tag{2}$$

This is a linear spline model with a single "knot" connecting two line segments at the sample median values of the peer effects variables. For example, γ_2 identifies the effect of the fourth-grade peers when the classroom proportion of LBC is below the sample median, and δ_2 identifies this effect when the classroom proportion of LBC is above the sample median. γ_1 and δ_1 similarly identify the effects of the first-grade peers.

Table 3 shows that peer effects are smaller in magnitude and marginally significant when children are exposed to peers with above-median proportions of LBC in the fourth grade. This result is consistent with the effect of an additional disruptive peer diminishing as the classroom proportion of disruptive peer increases. The median value of $\overline{LB^{class4a}_{-i,1a}}$ is 0.371 (0.367 for $\overline{LB^{class1a}_{-i,1a}}$), meaning that when more than approximately a third of the classmates are LBC in the fourth grade, peer effects are only about half of the baseline effects reported in Table 2. We do not find evidence of nonlinear peer effects from the left-behind peers in the first grade.

3.3 Heterogeneous Effects

Several studies show that peer effects may be heterogeneous in the characteristics of the children affected by their peers, such as baseline academic achievement (Ding and Lehrer 2007; Carman and Zhang 2012; Burke and Sass 2013), household socio-economic status (SES; Bertoni, Brunello and Cappellari 2020; Wang 2021),

Table 3: Linear Spline Model

Dependent =	**Consc** (1)	**Agree** (2)	**Extro** (3)	**Openn** (4)	**E.Stability** (5)	**Social** (6)
$\mathbb{1}(\overline{LB^{class1a}_{-i,1a}} \leq \text{median}) \times (\overline{LB^{class1a}_{-i,1a}} - \text{median})$	0.149 (0.517)	-0.581 (0.584)	-0.746 (0.552)	-0.207 (0.565)	-0.390 (0.755)	-0.610 (0.431)
$\mathbb{1}(\overline{LB^{class1a}_{-i,1a}} > \text{median}) \times (\overline{LB^{class1a}_{-i,1a}} - \text{median})$	0.300 (0.488)	0.794 (0.495)	0.984 (0.631)	0.744 (0.405)	1.546 (0.569)	0.734 (0.522)
$\mathbb{1}(\overline{LB^{class4a}_{-i,1a}} \leq \text{median}) \times (\overline{LB^{class4a}_{-i,1a}} - \text{median})$	-3.375 (1.622)	-3.681 (1.992)	0.244 (1.240)	0.602 (1.550)	-4.395 (1.727)	-3.019 (1.422)
$\mathbb{1}(\overline{LB^{class4a}_{-i,1a}} > \text{median}) \times (\overline{LB^{class4a}_{-i,1a}} - \text{median})$	-1.080 (0.590)	-1.195 (0.722)	-1.352 (0.589)	-1.181 (0.600)	-1.606 (0.633)	-0.868 (0.715)
Other Controls	✓	✓	✓	✓	✓	✓
School-Cohort-Wave FE	✓	✓	✓	✓	✓	✓
Joint F-Test For Both Changes in Slope at the Median	1.2 [0.305]	3.0 [0.057]	1.7 [0.195]	0.9 [0.408]	7.1 [0.002]	3.8 [0.029]
N	3087	3087	3087	3087	3087	3087

Notes: Standard errors are clustered at the school-cohort level in parentheses. The sample is restricted to children who had never been left behind since grade one. Other control variables include: dummies for mother's education levels and number and birth order of siblings; child's gender; child's math and Chinese normalized test scores in the second semester of grade 3; teacher's gender; age; experience; education level; marital status; income and the subject of teaching; the size of the current class and the reference child's grade-one class. Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. *Consc*: conscientiousness; *Agree*: agreeableness; *Extro*: extroversion; *Openn*: openness to experience; *E.Stability*: emotional stability; *Social*: social skill.

and gender.[11] We test for heterogeneity along the dimensions mentioned above using the following model:

$$\begin{aligned} Y_{it} = \alpha &+ \gamma_1 \overline{LB^{class1a}_{-i,1a}} + \gamma_2 \overline{LB^{class4a}_{-i,1a}} \\ &+ \delta_{21} \times \mathbb{1}(\text{TEST}_{i,3b} < \text{average}) \times \overline{LB^{class4a}_{-i,1a}} \\ &+ \delta_{22} \times \mathbb{1}(\text{EDUC}^{mother}_{i} \leq 6) \times \overline{LB^{class4a}_{-i,1a}} \\ &+ \delta_{23} \times \mathbb{1}(\text{GIRL}_i) \times \overline{LB^{class4a}_{-i,1a}} + \theta X_{it} + \rho_{sct} + \varepsilon_{it} \end{aligned} \tag{3}$$

where $\mathbb{1}(\text{TEST}_{i,3b} < \text{average})$ is an indicator that equals 1 if the child's test score (average of math and Chinese) is below school-cohort-wave average. $\mathbb{1}(\text{EDUC}^{mother}_{i} \leq 6)$ is an indicator for the mother's years of education, and $\mathbb{1}(\text{GIRL}_i)$ is an indicator for the child's gender. We focus on the fourth-grade peers only.

Table 4 shows that the negative effects of left-behind peers on personality skills are greater for the children with low academic achievement. The estimates in the second row suggest smaller negative peer effects for those from low-SES households as measured by the mother's education level, but the differences are not significant. In the last row, although peer effects seem to affect boys more than girls, none of the results are significant.

[11] See Angrist and Lang (2004), Lavy, Silva and Weinhardt (2012), Lu and Anderson (2015), Balsa, Gandelman and Roldán (2018), Wang and Zhu (2019), Fletcher, Ross and Zhang (2020), Balestra, Sallin and Wolter (2021), and Lei (2021).

Table 4: Heterogeneous Impacts of Left-Behind Peers by Past Test Scores, Mother's Education, and Gender of the never-LBC

Dependent =	**Consc** (1)	**Agree** (2)	**Extro** (3)	**Openn** (4)	**E.Stability** (5)	**Social** (6)
Proportion of 1a-Peers LB in 1a	0.280	0.130	0.083	0.245	0.586	0.103
	(0.287)	(0.319)	(0.390)	(0.323)	(0.422)	(0.315)
Proportion of 4a-Peers LB in 1a	-1.684	-1.995	-0.661	-0.620	-2.360	-1.524
	(0.527)	(0.615)	(0.508)	(0.654)	(0.635)	(0.614)
$\times$ (Test in 3b $<$ Average)	-0.621	-0.167	-0.313	-0.448	-0.348	-0.333
	(0.110)	(0.147)	(0.177)	(0.157)	(0.148)	(0.115)
$\times$ (Mother's Educ $\leq$ 6 or Unknown)	0.134	0.114	0.077	0.432	0.282	-0.026
	(0.269)	(0.260)	(0.265)	(0.260)	(0.237)	(0.258)
$\times$ Girl	0.036	0.196	0.091	0.202	0.026	0.308
	(0.274)	(0.286)	(0.198)	(0.258)	(0.223)	(0.225)
Other Controls	✓	✓	✓	✓	✓	✓
School-Cohort-Wave FE	✓	✓	✓	✓	✓	✓
N	3087	3087	3087	3087	3087	3087

Notes: Standard errors are clustered at the school-cohort level in parentheses. The sample is restricted to children who had never been left behind since grade one. Other control variables include: dummies for mother's education levels and number and birth order of siblings; child's gender; child's math and Chinese normalized test scores in the second semester of grade 3; teacher's gender; age; experience; education level; marital status; income and the subject of teaching; the size of the current class and the reference child's grade-one class. Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. *Consc*: conscientiousness; *Agree*: agreeableness; *Extro*: extroversion; *Openn*: openness to experience; *E.Stability*: emotional stability; *Social*: social skill.

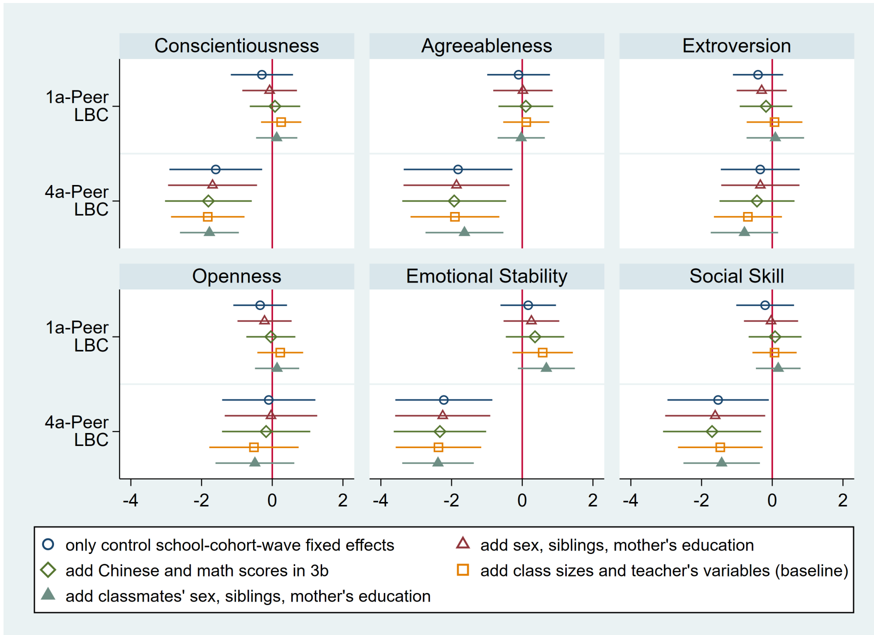
In the Web Appendix, we allow the effects to differ by the gender of the affecting peers and the affected children (Table A5). The results are suggestive of more negative effects from left-behind girls, although the differences are often insignificant.

3.4 Robustness Tests

In this section, we examine the robustness of our results to some alternative specifications. Figure 2 presents robustness results for different sets of control variables. Each subfigure presents five models, distinguished by shapes. Control variables are successively added to reconstruct the baseline specification represented by empty squares.

Estimates represented by filled triangles go beyond baseline specification by including peers' characteristics such as gender, number of siblings, and the mother's education level. These estimates address the concern that left-behind status may proxy other elements of disadvantaged status such as the low education levels of parents. It could be a potential concern given that migrant parents tend to be poorly educated, and a series of studies reported significant effects of being exposed to peers with parents of different education levels (Bifulco, Fletcher and Ross 2011; Bertoni, Brunello and Cappellari 2020; Fletcher, Ross and Zhang 2020). Our estimates show that the results are robust to these specifications. Tables A7 and A8 in the Web Appendix present more details of these estimates.

Figure 2: Peer Effects with Different Control Variables

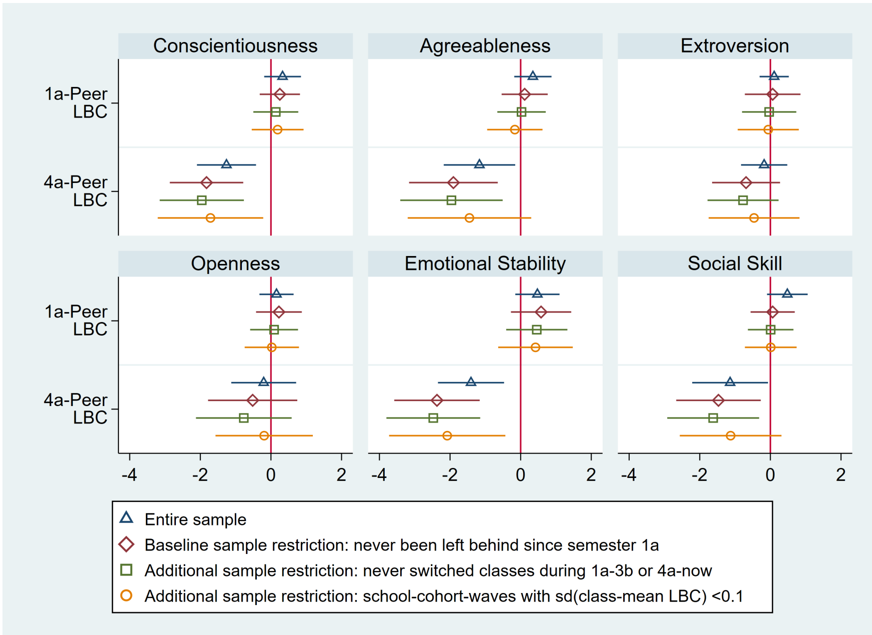


Note: Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. The bars represent 95% confidence intervals.

Figure 3 presents robustness results for different sample restrictions. Estimates represented by triangles are based on the entire sample, not restricted to the never-LBC. Squares are based on a more restrictive sample compared to the baseline, including only those who never switched classrooms between randomized classroom assignments in the first grade and the fourth grade. Finally, circles are based on a sample further restricted to those in school-cohorts *not* showing large variations in the proportion of LBC across classrooms. This sample is restricted to school-cohort-waves where the standard deviation of the classroom proportion of LBC is less than 0.1. We reasoned that if some school-cohorts assigned students so that students were sorted by their left-behind status, there would be high variability in the proportion of LBC across classrooms for those school-cohorts. Figure 3 shows that our results are robust to different sample restrictions. See Table A9 in the Web Appendix for more details.

We conduct additional robustness tests in the Web Appendix. We test baseline results (i) by either removing the fourth-grade peer effect variable or the first-grade peer effect variable (Tables A7, A8); (ii) with different levels of clustering of the error term, including 18 schools (Table A10); (iii) with alternative definitions of peers' left-behind status, defined by either-parent absence, father's absence, mother's absence, and both-parent absence (Table A11); (iv) with alternative measurements of personality skills (Table A12); and (v) by restricting

Figure 3: Peer Effects with Different Sample Restrictions



Note: Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. The bars represent 95% confidence intervals. Baseline sample restrictions also include having personality skills evaluated by a headroom teacher.

the sample to each of the two waves of survey (Table A13). Our results are robust to these specifications.

In Table A14 of the Web Appendix, we use alternative definitions of peers' left-behind status based on the timing of being left behind. The negative peer effects are stronger when the left-behind status is defined as being left behind in the first grade than when it is defined as the third grade. The effects are strongest for the peers who were left behind in both the first grade and the third grade.

4 Channels to Peer Effects on Personality Skills

To more fully understand the nature of childhood peer effects, we now turn to the second question, how childhood peers affect personality skills. Specifically, we focus on peers' academic achievement and personality skills as potential channels.

Unfortunately, we cannot answer this question directly because personality skills are not measured before the randomization in the fourth grade. Below, we describe an indirect approach that provides suggestive evidence of the relative importance of these two channels.

4.1 Peers' Average Characteristics and the Proportion of LBC

We first aim to show that in the entire sample, the average personality skills of peers go down as the proportion of LBC among peers increases. This is probable, because as Table 1 and Table A6 show, LBC are primarily disadvantaged in personality skills but not in academic achievement.[12] Focusing on the peers in the first semester of the fourth grade, we relate their average left-behind status in the first semester of the first grade and the average test scores in the second semester of the third grade, all prior to the fourth grade random assignment, to their average test scores and personality skills in the fourth grade. We estimate the following model with classroom-level leave-one-out average peer characteristics:

$$\overline{Y_{-i,t}^{\text{Academic},class4a}} = \alpha_1 + \gamma_{11}\overline{Test_{-i,3b}^{class4a}} + \gamma_{12}\overline{LB_{-i,1a}^{class4a}} + \theta_1 X_{it} + \rho_{sct} + \varepsilon_{1,it}, \tag{4}$$

$$\overline{Y_{-i,t}^{\text{Personality},class4a}} = \alpha_2 + \gamma_{21}\overline{Test_{-i,3b}^{class4a}} + \gamma_{22}\overline{LB_{-i,1a}^{class4a}} + \theta_2 X_{it} + \rho_{sct} + \varepsilon_{2,it}, \tag{5}$$

where $\overline{Y_{-i,t}^{\text{Academic},class4a}}$ and $\overline{Y_{-i,t}^{\text{Personality},class4a}}$ are the $n-1$ averages of classmates' academic achievement and personality skill in the first semester of the fourth grade, respectively. To simplify the models, we estimate a single factor for the personality skill and a single factor for the academic achievement. The personality skill factor is calculated as the major factor of the Big-5 personality and social skill measures. The academic achievement factor is the major factor of math test scores, Chinese test scores, and IQ scores. Factors are based on measures current at the time of the survey and are all normalized within the school-cohort-wave. $\overline{Test_{-i,3b}^{class4a}}$ is the average of classmates' academic achievement defined as the sum of math and Chinese test scores in the second semester of the third grade and normalized within the school-cohort-wave.[13] Other control variables include all of the control variables in the baseline model and their leave-one-out averages among classmates in the fourth grade and the first grade. The error terms are clustered at the school-cohort level.

In terms of peers' group averages, we expect $\gamma_{11} > 0$ if past academic achievement predicts current academic achievement. Similarly, we expect $\gamma_{21} > 0$ if past academic achievement predicts current personality skill. If past academic achievement does not predict current personality skill, we expect $\gamma_{21} = 0$. Similarly, $\gamma_{12} < 0$ if past left-behind status predicts current (low) academic achievement and $\gamma_{22} < 0$ if past left-behind status predicts current (low) personality skill.

The first row of the left panel of Table 5 shows that the fourth-grade peers' $n-1$ average third-grade test score predicts their fourth-grade average test score but not their average personality skill. Similarly, the second

[12]A companion paper more rigorously shows that for the children in our data set, being left behind by migrating parents negatively affects personality skills but not academic achievement. This result is available upon request.

[13]IQ tests were administered only at the time of the survey, so third-grade IQ scores are not available.

row shows that the fourth-grade peers' first-grade $n-1$ average left-behind status predicts their fourth-grade average personality skill but not their fourth-grade average test score. These results suggest that the classroom proportion of LBC is a proxy for the proportion of peers with low personality skill but not low academic achievement.

To rigorously establish the underlying channels, we need measures of average personality skills of fourth-grade peers before the fourth-grade classroom reassignment. But we only have access to peers' left-behind status and test scores before the fourth grade. Given that, as a group, children who were left behind in the first grade show low personality skills in the fourth grade, we assume that a similar disadvantage would have also existed in the third grade, one year closer to the first grade, which is used to designate the left-behind status. This assumption is therefore plausible if the effects of own left-behind status on personality skills do not change radically in the course of one year.

4.2 Peer Effects through Academic Achievement and Personality Skill

Next, we estimate the impact of peers' $n-1$ average past academic achievement and left-behind status on one's own academic achievement and personality skills. We proxy peers' low personality skills by their left-behind status, as discussed in the previous paragraphs.

The following models are estimated:

$$Y_{i,t}^{\text{Academic}} = \alpha_1 + \gamma_{11}\overline{Test_{-i,3b}^{class4a}} + \gamma_{12}\overline{LB_{-i,1a}^{class4a}} + \theta_1 X_{it} + \rho_{sct} + \varepsilon_{1,it}, \tag{6}$$

$$Y_{i,t}^{\text{Personality}} = \alpha_2 + \gamma_{21}\overline{Test_{-i,3b}^{class4a}} + \gamma_{22}\overline{LB_{-i,1a}^{class4a}} + \theta_2 X_{it} + \rho_{sct} + \varepsilon_{2,it}. \tag{7}$$

The dependent variables are child i's academic achievement and personality skill outcomes measured at the time of the survey. γ_{22} represents the personality-to-personality channel. γ_{21} represents the achievement-to-personality channel. γ_{11} and γ_{12} capture the two peer effect channels for the academic achievement outcome. X_{it} is a row vector of individual characteristics included in the baseline model (Equation (1)) and ρ_{sct} represents school-cohort-wave fixed effects. The error terms are clustered at the school-cohort level. The sample consists of never-LBC. All other baseline sample restrictions are applied.

The right panel of Table 5 shows that, on the one hand, classroom peers' average left-behind status lowers personality skills but not academic achievements of never-LBC. On the other hand, classroom peers' average academic achievements do not significantly affect the academic achievements or personality skills of never-LBC. To the extent that the proportion of LBC is a good proxy for the proportion of peers with low personality skills, as we assumed in this section, these results show that the effects of the left-behind peers in childhood on personality skills are driven by the peers' low personality skills.

In the Web Appendix, we test the robustness of these results by (i) including different sets of control variables (Table A15), (ii) clustering the error term at the school level instead of at the school-cohort level (Table A15), and (iii) using alternative sample restrictions (Table A16). Out results are robust to these alterations.

We reiterate the caveats mentioned earlier in this section. We are unable to relate peers' personality skills before grade 4 to children's outcomes because personality skills are measured in grades 4–6 and the classroom assignment occurred at the beginning of the fourth grade. Instead, we have shown that the negative peer effects on personality skills are driven by the peers' average left-behind status, while also showing that peers' average personality skills (but not academic achievement) decreases in the proportion of LBC. Our interpretation relies on the assumption that LBC in our sample are primarily characterized by their low personality skills as they affect other children in the same classroom.

Table 5: Peer Effects: Academic Achievement Channel vs. Personality Skill Channel

Dependent:	**4a-Classmates'**		**Self's**	
	Academic (1)	Personality (2)	Academic (3)	Personality (4)
4a-Classmates' Test Scores in 3b	0.518	0.475	0.056	0.071
	(0.067)	(0.274)	(0.115)	(0.292)
4a-Classmates' Being LB in 1a	0.107	-1.308	0.201	-1.686
	(0.178)	(0.653)	(0.225)	(0.615)
Other Controls	✓	✓	✓	✓
School-Cohort-Wave FE	✓	✓	✓	✓
N	3087	3087	3087	3087

Notes: Standard errors are clustered at the school-cohort level in parentheses. The sample is restricted to never-LBC. "Academic" indicates the major component of the three academic achievement variables: math test score, Chinese test score and the IQ score. "Personality" indicates the major component of the six personality skill outcome variables: openness to experience, conscientiousness, extroversion, agreeableness, emotional stability, and social skills. "Other control variables" include: semester-1a-classmates' average test score in semester 3b, their proportion of left-behind children, and the same set of other control variables in the baseline regressions (dummies for mother's education levels and number and birth order of siblings, child's gender, child's math and Chinese normalized test scores in the second semester of grade 3, teacher's gender, age, experience, education level, marital status, income and the subject of teaching, the size of the current class and the reference child's grade-one class). Semester 1a indicates the first semester of the first grade. Semester 4a indicates the first semester of the fourth grade. Semester 3b indicates second semester of the third grade. LB: left-behind.

5 Discussion and Interpretation

To gain perspective, we place the magnitude of our estimates in a larger context by comparing them to the effects of other inputs to children's personality skill production. The Perry Preschool Program, a two-year-long center-based program implemented to three-year-old children, improved children's conscientiousness measured at ages 4–7 by 0.273 in standard deviation unit. By our estimates, based on older children in grades 4–6, this is equivalent to a 15 percentage point decrease in the proportion of left-behind peers. The Abecedarian program and Infant Health and Development Program had even smaller impacts on conscientiousness, by 0.047 and 0.075 in standard deviation unit, respectively (Elango et al. 2016).

Chetty et al. (2011) analyzed the impacts of Project STAR, where children were randomly allocated to classrooms of different sizes at kindergarten and in grades 1–3. Children's behavioral measures were collected in grades 4 and 8, based on a teacher survey of student behaviors including effort, initiative, engagement in class, and whether the student values school, which are similar to the conscientiousness scale. They showed that a 1 percentile improvement in kindergarten class quality led to an increase in the children's behavior index by a 0.153 percentile in grade 4 and by a 0.128 percentile in grade 8. Although difficult to compare directly, these estimates are also qualitatively consistent with our findings.[14]

Akee et al. (2018) used a sample of Native American youths in low-SES households to study the effects of unconditional household cash transfers on the measures of conscientiousness, agreeableness, and neuroticism (i.e., emotional stability). The authors showed that an increase in annual household income of approximately 3,500 USD increased conscientiousness by 0.21 and agreeableness by 0.27 in standard deviation unit when the children were 15–17 years old. The effects of annual cash transfers, representing a 15.4% increase in income for an average household, are comparable to the effects of a 10–15 percentage point change in the proportion of left-behind peers.

It would be useful to interpret our estimates by anchoring them to long-run outcomes. Cunha, Heckman and Schennach (2010) estimated a linear anchoring equation using NLSY79 child supplement data, relating age 14 skill measures to years of schooling at age 19. The estimated coefficient on noncognitive skill is 0.993,[15] implying that one standard deviation unit increase in behavioral measure in middle school is associated with almost an additional year of schooling in young adulthood. Using their estimates, a back-of-the-envelope calculation shows that a 10 percentage point reduction in the proportion of disadvantaged peers in childhood would be associated with a $0.182 \times 0.993 = 0.181$ increase in years of schooling.[16] Although currently infeasible, it would be useful as a future research to anchor our estimates using a Chinese sample.

[14]In a standard normal distribution, a movement from 10th to 14th percentile corresponds to an increase in the Z-score by about 0.2, which is close to the improvement in personality skill measures implied by a 10 percentage point decrease in the proportion of LBC. Assuming a linear relationship between kindergarten class quality and the grade 4 behavioral index in their study, a 4 percentile improvement in outcome corresponds to a 27 percentile increase in kindergarten class quality. Keep in mind that the relationship between a percentile change and a Z-score change depends on the position in the standard normal distribution.

[15]Table A10-1 in their Supplementary Material

[16]Chetty et al. (2011) estimated that a "1 percentile improvement in noncognitive measures in grade 4 is associated with a $106 gain in earnings" (p. 1652). Assuming that a 0.2 standard deviation increase corresponds to a 4 percentile increase in behavioral skill, the implied effect on outcome is about $400 increase in earnings.

Policies such as detracking, desegregation, or ability mixing are potentially helpful in improving the achievement outcomes of under-achieving students by exposing them to better academic environments. These policies, however, can lower the outcomes of others by exposing them to disadvantaged or under-achieving peers. Whereas the childhood peer effect on academic achievement is well documented, our results show that the negative effects of disadvantaged peers can be found on personality skills even when academic achievement is unaffected. Furthermore, the peers may be disadvantaged in personality skills even when they are not disadvantaged in academic achievement. To fully evaluate policies that change peer composition, it is important to go beyond academic achievement and consider personality skills both as measures of peer quality and as outcomes of peer interactions.

Considering that personality skills may be the channel between childhood peer effects and long-run outcomes, an effective policy response may be to improve children's personality skills. Several early childhood interventions improved life cycle outcomes mediated by better childhood behaviors, whereas their effects on academic achievements faded out relatively quickly (Chetty et al. 2011; Heckman, Pinto and Savelyev 2013; Elango et al. 2016). In addition, personality skills remain malleable while cognitive skills quickly stabilize beyond early childhood (Almlund et al. 2011).

6 Conclusion

We investigate the effects of disadvantaged classroom peers in primary school on the development of personality skills. For identification, we rely on the random assignment of children to classrooms and the fact that LBC's disadvantage is a result of their parents' migration away from home for employment. Our empirical design compares children (not left behind themselves) based on their exposure to left-behind classroom peers.

We show that disadvantaged classmates in primary schools have substantially negative impacts on children's personality skill development, but with no effect on their academic achievement. The magnitude of these effects is comparable to the effects of well-known early childhood intervention programs (Elango et al. 2016) or the effects of a cash transfer program that increased household income by 15% (Akee et al. 2018). These effects are stronger for those with low baseline academic achievement. Importantly, our evidence suggests that these effects are likely to be driven by the low personality skills of LBC, not by their low academic achievement or low SES.

We contribute to the literature on human capital development by showing that peers are important inputs in the production of personality skills. This result also supports the conjecture that personality skills mediate the effects of childhood peers on long-run outcomes including schooling and earnings. We further emphasize the need to account for children's personality skill development in the evaluation and implementation of education policies. Our conclusions would have been misleading had we only focused on academic achievement, which played only a small role in explaining the effects of left-behind peers. Finally, our findings on the channels to peer effects suggest that improving children's personality skills, rather than focusing only on their academic achievements, may be an effective way to promote children's human capital development while reducing the negative externalities of disadvantaged peers.

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