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Investment Attraction and the Prevention of Rural Child Left-Behind Status in Chinese Counties

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Abstract: Using a balanced panel of 892 counties in 10 labor-exporting Chinese provinces over 2015–2022, this paper estimates how county-level investment attraction affects the number of rural left-behind children within county borders. Two-way fixed effects estimates indicate that a 1 percentage point rise in realized investment funds relative to GDP lowers the county left-behind children (LBC) rate among compulsory-education students by 0.181 percentage points. A Bartik instrument built from base-year industry composition and national sectoral investment growth yields a two-stage least squares estimate of -0.381 (Kleibergen–Paap $F = 221.81$); a difference-in-differences design around the approval of development zones in 158 counties yields -1.651 percentage points, with no detectable pre-treatment trends in the event study. Estimators built for staggered adoption reproduce this magnitude (Callaway–Sant’Anna -1.592 ; Sun–Abraham -1.614), and the approval coefficient measures the effect of the zone approval event, not the effect of one percentage point of investment intensity. Sub-sample estimates locate the effect in western counties, high out-migration counties, and counties whose investment inflows lean toward labor-intensive industries, with coefficient gaps of 2.3–3.2 times across groups. The effect runs through local non-farm employment (Sobel $z = -6.65$, accounting for 31.8% of the total effect) and the out-migration share of the rural labor force (Sobel $z = -3.04$, 11.8%); the fiscal-livelihood channel receives only marginal support at the 10% level, and the three channels jointly explain 46.1% of the total effect. The share of children with both parents away falls by 0.134 percentage points per percentage point of investment intensity, which places the change in the form of left-behind status associated with the largest cognitive losses. The evidence supports writing employment-oriented investment attraction into county-level policy portfolios for LBC prevention and calibrating investment quality, meaning labor intensity, hiring per unit of capital, and capacity to employ women, rather than investment volume alone.

Keywords: Investment Attraction, Left-Behind Children, County Economy, Development Zones, Local Employment, Bartik Instrument, Staggered Difference-in-Differences

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1. Introduction

Census-based figures released jointly by the National Bureau of Statistics, United Nations Children’s Fund (UNICEF), and United Nations Population Fund (UNFPA) in “Population Status of Children in China in 2020: Facts and Figures” put the national count of left-behind children at 66.93 million and migrant children at 71.09 million in 2020; the two groups together account for 46.4% of all children in China. A nationwide screening organized in 2016 by the Ministry of Civil Affairs, the Ministry of Education,

and the Ministry of Public Security identified 9.02 million rural left-behind children under the narrower administrative definition (children under 16 with both parents working away, or one parent away and the other lacking guardianship capacity); 89.3% of them were under grandparental guardianship, and more than 90% lived in central and western provinces. By the end of August 2018, the count under the same definition had fallen to 6.97 million, a 22.7% decline in two years. Shrinkage has not dissolved the stock nature of the problem. Rural migrant workers totaled 297.53 million in 2023; 176.58 million of them worked away from home and 67.51 million crossed provincial borders (National Bureau of Statistics, 2023 Migrant Workers Monitoring Survey Report). The demographic base of parent-child separation keeps expanding. The cost of left-behind status can be quantified. A meta-analysis of 111 studies covering 265,000 children reports risk ratios of 1.52 for depression, 1.85 for anxiety, and 1.70 for suicidal ideation among left-behind children (Fellmeth et al., 2018); the absence of both parents lowers rural children's math and language test scores by about 5.4 and 5.1 percentile points, respectively (Zhang et al., 2014).

The State Council's *Opinions on Strengthening the Care and Protection of Rural Left-Behind Children* (Guo Fa [2016] No. 13) lists "a marked reduction in the phenomenon of children left behind" as a binding target and assigns responsibility to county governments. Among the policy instruments a county government can mobilize, investment attraction commands the largest resource commitment: promotion assessments and fiscal retention rules lock local officials' attention onto investment growth (Jin et al., 2005; Li & Zhou, 2005; Xu, 2011), and development zones, tax concessions, and rewards tied to realized investment funds form the standard repertoire of inter-county competition (Cheng & Kwan, 2000; Head & Ries, 1996). What investment promotion does to local employment and wages has been measured repeatedly at the prefecture level: special economic zones raise FDI per capita by about 58% and deliver wage growth exceeding the rise in living costs (Wang, 2013); economic zone programs expand employment, output, and firm entry within zone boundaries (Lu et al., 2019); national-level zones raise city GDP by about 20% (Alder et al., 2016). Expanding local job opportunities rewrites rural households' migration decisions (Taylor et al., 2003; Zhao, 1999), and migration decisions in turn determine whether children are left behind. Both ends of this chain rest on mature literatures; the middle link, quantitative evidence on how county investment attraction moves the number of left-behind children in the jurisdiction, is missing at the junction of the two.

This paper supplies that link with a balanced panel of 892 counties in 10 labor-exporting provinces over 2015–2022. On identification, two-way fixed effects regressions with county and year effects give a baseline estimate of -0.181 ($t = -5.58$); a Bartik instrument interacting 2013 county-level industry composition of realized investment funds with national sectoral investment growth gives a two-stage least squares estimate of -0.381 (Kleibergen–Paap $F = 221.81$); and a quasi-experiment around the approval of provincial-level or higher development zones in 158 counties during 2016–2019 gives a difference-in-differences estimate of -1.651 percentage points, with pre-approval event-study coefficients hugging zero and post-approval coefficients deepening year by year to -2.99 . Estimators designed for staggered adoption return the same magnitude, -1.592 under Callaway and Sant'Anna (2021) and -1.614 under Sun and Abraham (2021), and the approval coefficient is read as the effect of the policy event rather than as the effect of one percentage point of investment intensity. On channels, putting industrial employment, the out-migration share, and the livelihood expenditure share back into the LBC equation shrinks the investment coefficient by 43.0%; a product-of-coefficients decomposition attributes 31.8% of the total effect to local employment, 11.8% to out-migration, and 2.5% to livelihood spending. On structure, the same investment effort produces responses 2.3–3.2 times larger in western, high out-migration, and labor-intensive counties, and the response of the both-parents-away share (-0.134) indicates that investment attraction cuts first into the most damaging form of left-behind status. Together the three sets of results hand county governments an economic policy tool for LBC prevention that can be written into performance evaluation rules.

Each of the three literatures gains one step. For the left-behind children literature, Zhang et al. (2014), Meng and Yamauchi (2017), and related work measure the consequences of separation while treating household migration decisions as given; this paper brings the policy antecedent of those decisions, county investment attraction, into the estimation and moves the question forward from the cost of being left behind to its preventability. For the place-based policy literature, the outcome variables in Wang (2013), Lu et al. (2019), and Alder et al. (2016) stop at investment, employment, and wages; this paper extends the evaluation boundary to family structure and adds a child welfare dimension to zone assessment. For the literature on Chinese local government behavior, the promotion tournament described by Li and Zhou (2005) and Xu (2011) explains where investment effort comes from; this paper measures a spillover benefit of that competition which never enters cadre evaluations, adding a child-side entry to the welfare accounting of growth-oriented competition. Recent county-level evidence sharpens the same contrast: fiscal and administrative reforms that widen county autonomy raise county output and firm entry (Li et al., 2016), county-to-city upgrading delivers uneven and partly reversible gains (Fan et al., 2012), and industrial park approvals reorganize employment around the county (Zheng et al., 2017). Family structure appears in none of these evaluation sets.

2. Literature and Hypotheses

2.1. *The Cost of Parent-Child Separation and the Compensation from Remittances*

The net effect of parental out-migration on left-behind children depends on the relative size of remittance gains and caregiving losses, and the evidence splits along country and family-structure lines. On the gains side: exchange rate appreciation in Filipino migrants' destination countries raises household remittances (the peso-value elasticity is about 0.60) and, through them, children's school attendance and education spending while reducing child labor (Yang, 2008); a father's migration to the United States adds up to about one year to his daughters' completed schooling in Mexican households (Antman, 2012); international remittances lower the incidence of child wage work in El Salvador (Acosta, 2011). On the losses side: once "receiving remittances" and "having a household member abroad" are separated as distinct treatments, Dominican data show the two forces offsetting each other, with much of the remittance gain eaten by family disruption (Amuedo-Dorantes & Pozo, 2010); children aged 12–18 in Mexican migrant households are less likely to complete high school (McKenzie & Rapoport, 2011); Filipino children whose mothers work abroad fall behind in school more often than those with migrant fathers, and remittances do not make up for a mother's absence (Cortes, 2015); Albanian left-behind children face higher dropout hazards (Giannelli & Mangiavacchi, 2010); Romanian left-behind children combine higher test scores with worse depression outcomes (Botezat & Pfeiffer, 2020); and in a four-country Southeast Asian comparison, the association between paternal absence and poor child mental health holds in Indonesia and Thailand but not in the Philippines or Vietnam (Graham & Jordan, 2011).

Chinese evidence also lands on both sides. Negative findings: the absence of both parents lowers cognitive test scores (Zhang et al., 2014); the loss concentrates among children with both parents away or under non-parental care (Zhou et al., 2014); cumulative exposure, identified with instruments based on weather variation in parents' home regions at ages 16–25, damages children's education and health, with boys hurt more (Meng & Yamauchi, 2017); left-behind status is associated with smoking, internet addiction, and suicidal ideation, with adjusted odds ratios between 1.4 and 6.6 (Gao et al., 2010); field studies document heavier housework burdens, missing homework supervision, and emotional deprivation stacked on the same children (Ye & Pan, 2011); and a meta-analysis of Chinese- and English-language studies reports worse outcomes for left-behind children across emotional, social, and academic domains, together with publication bias in the unfavorable direction (Wang & Mesman, 2015). Findings near zero or positive: panel difference-in-differences detects no drop in school performance after parents leave, and scores rise in father-only-migration households (Chen et al., 2009); parental migration correlates positively with standardized English scores in a sample of 130 rural primary schools in the Northwest (Bai et al., 2018); and instrumental variable estimates show parental migration improving young children's weight outcomes (Mu & de Brauw, 2015). A reading consistent with both sides is that the remittance channel is real and so is the caregiving loss; China's hukou segmentation stretches the duration of separation and narrows the option of taking children along (Meng, 2012), and migrant parents even postpone return because of the money their children's schooling requires (Démurger & Xu, 2015). Policy that compensates for separation without reducing it cannot contain the health risk premium measured by Fellmeth et al. (2018). The first-order question in LBC prevention is where parents work. This yields:

Hypothesis 1 (H1): An increase in county investment attraction lowers the LBC rate in the jurisdiction.

2.2. *Investment Attraction, Local Employment, and Migration Decisions*

The evidence chain linking local investment promotion to local labor markets is complete at both ends. On the employment side: local job growth shocks lower unemployment and raise labor force participation and real income in the long run, with larger gains for disadvantaged workers (Bartik, 1991); each new tradable-sector job creates about 1.6 additional local non-tradable jobs (Moretti, 2010); and the opening of a large plant raises incumbent-firm TFP in winning counties by about 12% (Greenstone et al., 2010). In the Chinese setting: incentive-area policies raise a city's probability of attracting foreign investment, and agglomeration is self-reinforcing (Head & Ries, 1996); market size, infrastructure, and preferential policies explain FDI location (Cheng & Kwan, 2000); and special zones and development zones expand employment, wages, and firm entry (Alder et al., 2016; Lu et al., 2019; Wang, 2013). On the migration side: migration costs and institutional barriers suppress out-migration, and household labor allocation is sensitive to local non-farm opportunities (Zhao, 1999); in the new economics of labor migration, remittances and lost labor push household outcomes in opposite directions (Taylor et al., 2003); and parental illness pulls adult children back home (Giles & Mu, 2007). The location of job opportunities also reshapes human capital and family structure: wider access to migrant work lowers rural high school enrollment (de Brauw & Giles, 2017); export-manufacturing job growth raises school dropout among Mexican teenagers (Atkin, 2016); Bangladeshi girls within commuting reach of garment factories delay marriage and childbearing and stay in school longer (Heath & Mobarak, 2015); and child labor is rooted in poverty, with income growth reducing it (Edmonds & Pavcnik, 2005).

The investment measure used below is broader than foreign direct investment: extra-provincial domestic capital supplies 91.4% of realized funds in the average sample county-year and utilized foreign investment the remaining 8.6% (Section 3.2). Three features carry the FDI and special-zone evidence into this setting. The chain that evidence documents runs from capital inflow to plant entry and expansion, and from plant entry to local labor demand; the residence of the capital owner enters that chain only through the

industry mix and the wage the entering plant pays. The Chinese zone programs evaluated by Lu et al. (2019), Alder et al. (2016), and Zheng et al. (2017) admit tenants that are overwhelmingly domestic firms, so the employment and firm-entry elasticities those studies report already describe domestic capital. The location results in Head and Ries (1996) and Cheng and Kwan (2000) concern what makes capital land in one jurisdiction rather than another, namely market access, infrastructure, and preferential treatment, and these determinants apply to a Guangdong firm relocating to Guizhou as much as to a foreign entrant. Where the two capital types diverge, foreign entrants pay a wage premium and concentrate in coastal export sectors, so FDI-based employment elasticities mark the upper end of what interior counties can expect; the mechanism estimate in Section 8, a 2.66% employment response per percentage point of investment intensity, falls inside rather than above the zone-based range, which is the internal check this transfer requires. Migrant labor supply and firm location are jointly determined in China (Imbert et al., 2022), a dependence the identification strategies in Section 5 are built to break.

The household side of the chain is a location choice. A rural parent works away when the destination wage net of migration and living costs exceeds the local wage by more than the value the household places on living with the child, and the mass of parents for whom that gap is small sets the size of the response to any local job increment. Kinnan et al. (2018) show that access to migration networks, not the wage gap alone, governs which households send members out, so the marginal household is one whose network access is weak and whose reservation gap is correspondingly narrow, the household a local job reaches first. County policy shifts this margin through the local wage and the rate at which local jobs arrive, which is where investment attraction enters. What separates the estimation below from the literature it draws on can be stated on the same margin. Studies of migration decisions (Zhao, 1999; Taylor et al., 2003; Kinnan et al., 2018) model the household choice while holding the local opportunity set fixed or treating it as an unmodelled shock. Studies of employment creation (Bartik, 1991; Moretti, 2010; Greenstone et al., 2010) measure how many jobs a capital inflow produces and stop at the labor market. Evaluations of regional development policy (Neumark & Simpson, 2015; Kline & Moretti, 2014; Lu et al., 2019; Zheng et al., 2017) carry outcomes to output, wages, and agglomeration. Work on the children themselves (Xu & Xie, 2015; Zhou et al., 2015; Démurger, 2015) measures what migration does to the child once the decision has been taken. The estimation below places the county policy instrument and the child outcome in one equation, which is the link none of the four strands carries.

Splicing the labor-market and the migration evidence: realized investment funds convert into local jobs and wages, the elasticity of substitution between within-county and outside-county employment governs parents' marginal migration choice, and the LBC rate is the household counterpart of that margin. A fiscal channel runs in parallel: investment inflows widen the tax base, and higher general public budget revenue creates spending room for child care services and for school access conditions facing migrant families. This yields:

Hypothesis 2 (H2): Investment attraction transmits to the LBC rate through three channels: larger local non-farm employment, a lower out-migration share, and expanded fiscal revenue and livelihood spending.

Effect sizes should differ across three county attributes. Where baseline out-migration is high, the stock of parents at the margin of the migration decision is large, so a given job increment converts at a higher rate. Western counties send workers over longer distances with less frequent returns, so left-behind status there comes closer to prolonged absence of both parents (Zhou et al., 2014) and a single relocation of a parent's job rewrites the child's status more fully. Labor-intensive projects create more jobs per yuan and their skill requirements match migrant parents, while capital-intensive projects absorb little labor (Lu et al., 2019). This yields:

Hypothesis 3 (H3): The effect concentrates in western counties, high out-migration counties, and counties whose investment leans labor-intensive.

2.3. County-Level Incentives and the Source of Identifying Variation

Cross-county and over-time differences in investment effort are not random noise, and their institutional roots belong in the identification discussion. Higher provincial GDP growth raises officials' promotion odds (Li & Zhou, 2005); marginal fiscal retention under contracting and tax-sharing arrangements keeps part of the growth dividend at the local level (Jin et al., 2005); and the combination of centralized personnel control with decentralized economic governance makes the county the basic unit of investment competition (Xu, 2011). Competitive pressure travels down the evaluation chain and lands on realized-funds targets, zone applications, and the space devoted to investment attraction in government work reports; the interquartile range of 6.33 percentage points in investment intensity across sample counties originates here. This institutional fact cuts both ways for estimation: officials' terms and evaluation cycles supply variation unrelated to the LBC rate, while the policy response of investment effort to population outflow pressure manufactures reverse causality. The instruments and the quasi-experiment in Section 5 target the latter. The quasi-experiment inherits a second problem from the same institutional facts. Zone approvals arrive county by county across 2016–2019 rather than on one date, and two-way fixed effects estimators applied to staggered adoption weight group-time effects in ways that admit negative weights when treatment effects vary by cohort or by exposure length (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020; Sun & Abraham, 2021). Estimators that discard the forbidden comparisons, the doubly robust group-time

approach of Callaway and Sant’Anna (2021), the interaction-weighted estimator of Sun and Abraham (2021), and the imputation estimator of Borusyak et al. (2024), are reported in Section 5.4 next to the two-way fixed effects benchmark, following the practice summarized by Roth et al. (2023).

3. Research Design

3.1. Sample and Data

The sample covers Anhui, Henan, Hubei, Hunan, and Jiangxi (five central provinces) and Guangxi, Chongqing, Sichuan, Guizhou, and Yunnan (five western provinces). Province selection follows sending-region status: central and western regions supplied 99.04 million and 83.67 million migrant workers in 2023, 61.4% of the national total (National Bureau of Statistics, 2023 Migrant Workers Monitoring Survey Report); more than 90% of the 9.02 million rural left-behind children identified in the 2016 national screening lived in central and western provinces; and all ten sample provinces fall in these two sending regions. The observation window is 2015–2022: the starting year avoids the 2014 revision of county-level education statistics, and the end year is the latest for which county investment fund data are complete. Three groups of units are dropped: urban districts, whose “rural left-behind children” statistics are not comparable with counties; counties that underwent county-to-district conversion or other boundary changes; and counties missing key regression variables for more than three years. The remaining sample holds 892 county-level units, counties and county-level cities, and forms a balanced panel of 7,136 observations. Two considerations justify the county as the unit of analysis. The administrative authority over investment attraction and the territorial responsibility for LBC care and protection both sit at the county level in China’s governance structure, so the policy variable and the outcome close the loop at the same tier. Commuting is feasible within a county, so a within-county job amounts to family reunion in the “not leaving home” sense, whereas prefecture-level units would mix cross-county commuting and within-prefecture migration into the LBC measure.

Data come from four layers and are matched by hand. County economic and fiscal variables come from the China County Statistical Yearbook and the EPS database. Counts of rural left-behind children enrolled in compulsory education come from provincial education statistics bulletins and disclosure documents of county education bureaus; this enrollment-based register covers the full student population, which avoids the county-level representativeness problem of household surveys and is broader than the civil affairs screening definition (both parents away, or one away and the other without guardianship capacity). Realized investment funds (extra-provincial domestic capital plus utilized foreign investment, with the former dominating in county samples) come from provincial commerce department statistics and county government work report disclosures. The attention measure is computed by tokenizing the full texts of 7,136 government work reports issued by the 892 counties over 2015–2022. Continuous variables are winsorized at the 1st and 99th percentiles computed within each year, so the trimming points follow the annual distribution rather than the pooled one; Table 5 reports the alternative of computing one pair of cut points over the whole panel.

Harmonizing the left-behind counts across ten provinces and eight years takes four steps, and the limits that survive them are stated rather than assumed away. One reporting system is used throughout: the school-based register that county education bureaus compile from the national student registration information system and report upward into provincial education statistics bulletins. Civil affairs screening counts, which rest on the narrower definition introduced by Guo Fa [2016] No. 13, are never mixed into the series and enter only as external reference points. The reporting unit is then made uniform. Provincial bulletins differ in whether they publish a single total for rural compulsory-education students with one or both parents working away or publish the both-parents-away and one-parent-away components separately; component reports are summed to the common total, and the both-parents-away share used in Table 2, column (4), is recovered from the same components, taken for the four provinces that publish only the total from the county education bureau disclosure documents that report the split. The 2016 definitional revision is handled by re-tabulation and by a sample cut: provinces that changed their reporting rule republished the 2015 figure on the new basis in the following year’s bulletin and the republished figure is used, counties without a republished figure are re-tabulated from the component disclosures, and a robustness row that drops 2015 altogether appears in Table 5, where the coefficient moves from -0.1807 to -0.1852 . Series with a year-on-year level break above 30% are checked against the originating county bulletin, and 23 counties whose breaks could not be reconciled are excluded under the same rule that removes counties with more than three years of missing key variables, which keeps the panel balanced. Two limits remain. Coverage is enrollment-based, so children who left the county with their parents, who enrolled in a destination city, or who are out of school lie outside the frame by construction, and the variable measures the incidence of left-behind status among children schooled in the county rather than among children born there. Cross-province differences in how strictly schools apply the “working away” threshold are absorbed by county fixed effects, and province-specific changes in reporting practice over time are absorbed by the province $\times$ year specification, whose coefficient (-0.1767) sits within one standard error of the baseline.

3.2. Variables

Three dependent variables capture different faces of “prevention and care.” The LBC rate (*LBC*) equals rural left-behind children enrolled in compulsory education divided by total rural compulsory-education enrollment, times 100, and measures incidence. The log count of left-behind children (*lnLBC*) provides a scale measure that separates “a falling rate” from “a shrinking denominator.” The share of left-behind children with both parents away (*Dual*) measures the intensity end of left-behind status; the cognitive losses identified by Zhou et al. (2014) and Zhang et al. (2014) concentrate precisely in the both-parents-away group, so movements in this share track the damage structure of left-behind status.

The explanatory variable is investment attraction intensity (*INV*), realized investment funds divided by GDP times 100, normalized by economic size to strip out county scale. Realized funds combine extra-provincial domestic capital and utilized foreign investment; the domestic component averages 91.4% of the total across sample county-years and stays above 74.3% in every province-year, so the variable tracks inter-provincial capital relocation far more closely than foreign entry. Two alternative measures serve as cross-checks: the frequency of investment-related word roots (“investment attraction,” “capital introduction,” “development zone,” “realized funds,” “business environment”) per thousand words of the government work report (*Freq*), which measures attention allocation; and an entropy-weighted composite of *INV* and *Freq* with objective weights of 0.706 and 0.294. Controls cover seven county characteristics spanning development level, fiscal capacity, industrial structure, and demographic structure: log GDP per capita, the fiscal self-sufficiency ratio (general public budget revenue over expenditure), the secondary industry share, the resident urbanization rate, log population density, log education spending per student, and the rural population share. Five mechanism variables are used: log employment in above-scale industrial firms (*lnEmp*), the share of the rural labor force working away from home (*OutMig*), log household disposable income per capita (*lnInc*), log general public budget revenue (*lnFisc*), and the share of livelihood expenditure (education, social security and employment, health) in the general public budget (*Welfare*).

3.3. Econometric Model

The baseline is a two-way fixed effects model, as in Equation (1):

$$LBC_{it} = \alpha + \beta INV_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where LBC_{it} is the LBC rate of county i in year t , INV_{it} is investment attraction intensity, X_{it} is the control vector, μ_i absorbs slow-moving county traits such as location, dialect culture, and migration network endowments, λ_t absorbs national shocks common to all counties in a year, including the migrant labor cycle, poverty alleviation campaigns, and the pandemic, and β is the parameter of interest. Standard errors are clustered at the county level to accommodate within-county serial correlation. The net variation used for identification deserves a statement up front: the raw correlation between the LBC rate and investment intensity is -0.258 , and it drops to -0.085 after double demeaning, so the estimates in Equation (1) rest on within-county movements net of county levels and national trends rather than on the opposite slopes of two aggregate curves.

Annual means of the two variables across sample counties are plotted in Figure 1: the LBC rate falls from 21.52% in 2015 to 16.23% in 2022, a drop of 5.29 percentage points that parallels the contraction in the national screening counts (9.02 million to 6.97 million), while investment intensity climbs from 7.89% to 9.80%. The cross-sectional standard deviation holds steady between 5.9 and 6.1 across years; county-level dispersion did not converge over eight years, and the falling mean reflects a leftward shift of the whole distribution. This shape suggests that the cross-county structure of the left-behind problem deserves the regression framework more than the aggregate total does.

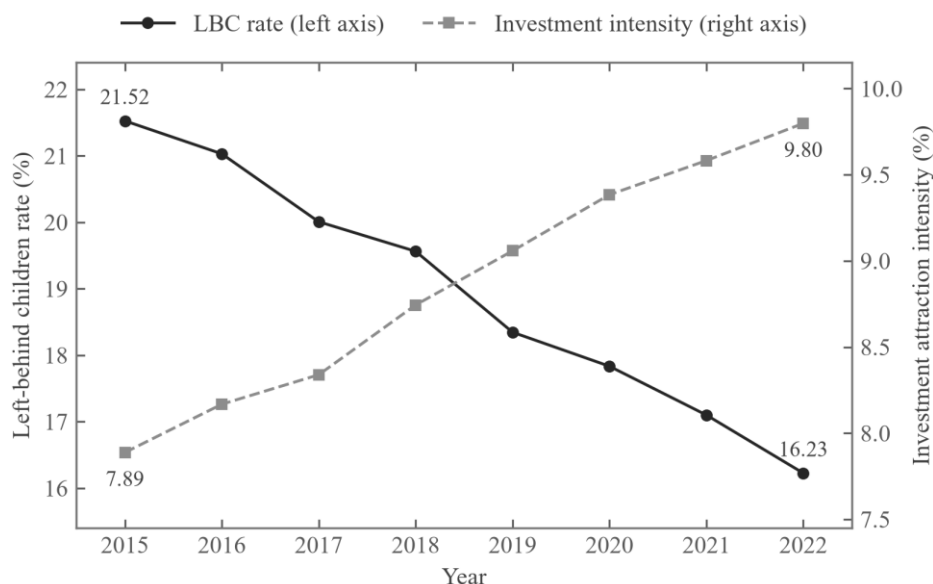


Figure 1. Annual Means of the LBC Rate and Investment Attraction Intensity Across Sample Counties, 2015–2022.

Source: authors' calculations from the 892-county panel.

3.4. Descriptive Statistics

Summary statistics appear in Table 1. The LBC rate averages 18.95% with a standard deviation of 6.21 and a range from 1.50% to 43.87%, so cross-county dispersion dwarfs the over-time contraction; the both-parents-away share averages 28.79%, which sits naturally beside the 89.3% grandparental guardianship figure from the civil affairs screening. Investment intensity averages 8.87% with an interquartile range of 5.46% to 11.79% and a right tail reaching 27.98%, ample variation for identification. By region, central counties average an LBC rate of 19.62% and investment intensity of 8.55%, against 18.23% and 9.22% in western counties; the higher western intensity is consistent with the policy push for industrial relocation toward the interior. Splitting by base-year out-migration, the high group averages an LBC rate of 21.10% against 16.80% in the low group, and this 4.3 percentage point gap is exactly the exposure difference that Hypothesis 3 points to. The out-migration share averages 38.17%, matching the labor-exporting profile of the sample provinces.

Table 1. Descriptive Statistics.

Variable	Mean	SD	Min	Max
LBC rate (%)	18.95	6.21	1.50	43.87
Both-parents-away share (%)	28.79	5.05	13.04	47.45
Investment attraction intensity (%)	8.87	4.59	0.30	27.98
Investment word frequency (%)	4.07	1.36	0.20	8.87
ln GDP per capita	10.29	0.42	8.81	11.69
Fiscal self-sufficiency	0.31	0.13	0.05	0.73
Secondary industry share (%)	34.47	8.55	8.00	69.58
Urbanization rate (%)	41.41	9.60	12.00	78.00
ln population density	5.58	0.79	2.87	8.04
ln education spending per student	9.10	0.36	7.88	10.34
Rural population share (%)	59.06	9.47	25.57	90.00
Out-migration share (%)	38.17	4.52	23.74	54.38
ln industrial employment	9.45	0.71	7.00	12.34
ln disposable income per capita	9.60	0.27	8.43	10.60
ln public budget revenue	10.02	0.62	7.92	12.26
Livelihood expenditure share (%)	27.68	3.34	14.56	39.38

Note: $N = 7,136$; data sources as described in the text; continuous variables winsorized at the 1st and 99th percentiles within each year.

4. Baseline Results

Estimates of Equation (1) are reported in Table 2. Column (1) includes only the two-way fixed effects; the coefficient on investment intensity is -0.1876 , significant at the 1% level. Column (2) adds the full control set and the coefficient barely moves, to -0.1807 (standard error 0.0324). Observable county conditions explain little of the coefficient, and the size of that movement carries information about selection. The ratio proposed by Altonji et al. (2005), the controlled coefficient divided by the movement

the controls produce (0.1807/0.0069), equals 26.2: selection on unobservables would have to be twenty-six times as strong as selection on the seven observable county characteristics to move the estimate to zero. The R^2 -based refinement in Oster (2019) carries no information in this setting: the controls raise R^2 by 0.0002 once county and year fixed effects are in place, which collapses the denominator of the Oster bound, so the ratio above is reported in its place.

Three reference points translate the magnitude. First, a one standard deviation rise in investment intensity (4.59 percentage points) maps into a 0.83 percentage point drop in the LBC rate, 4.4% of the sample mean and 15.7% of the total decline over 2015–2022 (5.29 percentage points). Second, the interquartile range (6.33 percentage points) maps into 1.14 percentage points, so moving a county from the 25th to the 75th percentile of investment intensity is worth more than the average annual decline in the sample (0.76 percentage points). Third, at the sample mean, 0.83 percentage points is roughly 200 children per county leaving left-behind status during the compulsory education stage. Column (3) switches the dependent variable to the log count: the coefficient of -0.0129 says each percentage point of investment intensity shrinks the number of left-behind children by 1.29%, so the falling rate is driven by the numerator, not by movements in the enrollment denominator. Column (4) uses the both-parents-away share; the coefficient of -0.1337 says the intensity end of left-behind status exits first, with new jobs retaining the second parent who sits at the margin of leaving. The pattern sits with the finding in Zhou et al. (2014) that cognitive losses concentrate where both parents are away. Read as causal, the composition of the decline matters as much as its size, and the welfare gain would exceed what the headline rate decline conveys; the county panel observes no child directly, so this remains an inference from the composition of the change rather than a measured effect on children.

Three further conversions place the magnitude on the scales the policy discussion uses. In elasticity form, at the sample means of investment intensity (8.87) and the LBC rate (18.95), the baseline implies -0.085 : a 10% rise in investment intensity lowers the LBC rate by 0.85%, and the instrumented coefficient in Section 5.2 raises this to -0.178 . Against cross-county dispersion, the one standard deviation effect of 0.83 percentage points equals 13.4% of the cross-county standard deviation of the LBC rate (6.21) and the interquartile effect of 1.14 percentage points equals 18.4%, so investment intensity moves counties within the existing distribution rather than rearranging it. In headcount, average rural compulsory-education enrollment across the sample counties is about 24,000, so 0.83 percentage points is roughly 200 children per county and about 178,000 children across the 892 counties, equivalent to 8.7% of the 2.05 million fall in the administratively screened national count between 2016 and 2018; that last comparison is indicative only, since the enrollment register and the civil affairs screening rest on different definitions. The development zone estimate converts on its own scale: -1.651 percentage points is about 400 children in an average treated county and about 63,000 across the 158 approved counties, and the four-year cumulative path of -2.99 percentage points roughly doubles both figures.

Table 2. Baseline Regressions.

	(1) LBC Rate	(2) LBC Rate	(3) ln LBC Count	(4) Both-Parents-Away Share
Investment intensity	-0.1876^{***} (0.0305)	-0.1807^{***} (0.0324)	-0.0129^{***} (0.0023)	-0.1337^{***} (0.0239)
Controls	No	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
<i>N</i>	7,136	7,136	7,136	7,136
R^2	0.6958	0.6960	0.8744	0.7403

Note: standard errors clustered at the county level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels; the same applies to the tables below.

The controls behave as county-level settings would predict: log GDP per capita enters negatively (-1.54) and urbanization positively (0.11), neither passing the 10% significance threshold, and the coefficients on fiscal self-sufficiency, industrial structure, population density, education spending, and the rural population share all sit near zero. Within an eight-year county window, variation in the LBC rate is explained by investment intensity and common year shocks; slow-moving county traits carry no additional information once the two-way fixed effects absorb them, which matches the fact that the cross-sectional dispersion of the LBC rate did not converge: endowments and networks set the cross-county gaps, while job opportunities drive the within-county movements.

5. Endogeneity

5.1. Direction of Bias

Endogeneity enters from two sources that push the same way. Reverse causality: in counties under heavy outflow pressure, labor exodus, rising LBC counts, and intensified investment promotion arrive together, and “guiding return migration through entrepreneurship and employment support” is one of the official explanations the Ministry of Civil Affairs gave for the 2016–2018

decline in LBC counts, so investment effort responds to the left-behind problem and pushes the OLS estimate toward zero. Measurement error: county disclosures of realized funds carry reporting and statistical slack, and classical error attenuates toward zero as well. Omitted variables have no fixed direction: if county-level shocks such as poverty alleviation depress the LBC rate in the same years they lift investment intensity, the negative coefficient would be overstated; the province $\times$ year fixed effects specification (see Table 5) addresses this layer. With the two dominant biases pointing toward zero, the baseline reads as a lower bound, and the instrument and the quasi-experiment supply the more credible magnitudes.

5.2. The Bartik Instrument

Following the shift-share design (Bartik, 1991; Goldsmith-Pinkham et al., 2020), the instrument interacts pre-sample 2013 county industry composition of realized investment funds with national sectoral investment growth (excluding the own province to cut mechanical correlation), as in Equation (2):

$$Z_{it} = \sum_{k=1}^6 s_{ik,2013} \cdot g_{kt} \quad (2)$$

where $s_{ik,2013}$ is county i 's base-year share of realized funds in industry k and g_{kt} is the national cumulative investment growth of industry k (own province excluded); Z_{it} is demeaned by year to strip the aggregate trend. The identifying logic is that base-year shares set each county's exposure to national industry investment cycles, cycles that national supply and demand determine and that no single county's family migration decisions can move; validity then reduces to the orthogonality of base-year shares to later shocks to county LBC rates (Goldsmith-Pinkham et al., 2020). Shares are fixed before the observation window, so within-sample adjustments of investment structure never enter the instrument. With a single instrument the model is just-identified and no overidentification test is available; the stability of the coefficient under province $\times$ year fixed effects (Table 5) serves as side evidence on the exclusion restriction. Two points on inference and on what the design rests on. Clustering at the county level does not price the correlation that shift-share designs induce across counties with similar base-year industry shares; the exposure-robust standard error of Adão et al. (2019) is 0.1274 against the county-clustered 0.1063, and the coefficient stays significant at the 1% level under either. The design also admits the reading of Borusyak et al. (2022), under which identification rests on quasi-random national sectoral shocks rather than on exogenous base-year shares; that reading fits a setting in which the 2013 shares are pre-determined without being randomly assigned, and it is the reading the exclusion argument above relies on.

First-stage estimates appear in Table 3, column (1): the coefficient on Z is 0.7608 (standard error 0.0511) with a Kleibergen–Paap F of 221.81, far above weak-instrument thresholds. Column (2) reports the two-stage estimate of -0.3810 (standard error 0.1063), significant at the 1% level and 2.1 times the OLS magnitude. That ratio is consistent with the prior on bias direction: reverse causality and measurement error compress the ordinary least squares estimate toward zero, and isolating the industry-cycle-driven variation removes that compression. A second reading carries no bias content at all, since the two estimators target different estimands, and the data at hand cannot separate the two readings. Under a local average treatment effect reading, the IV coefficient belongs to counties whose investment expansion is driven by base-year industry exposure; these procyclical-absorber counties overlap with the targets of industrial relocation policy, which makes this coefficient the more relevant one for extrapolating to that policy, conditional on the exclusion restriction holding.

To scale the result: multiplying the IV coefficient by the actual rise in investment intensity over the sample period (1.91 percentage points) attributes 0.73 percentage points of the LBC rate decline to investment expansion, 13.8% of the total drop (5.29 percentage points), with the remainder carried by common year factors such as expanded school access for migrant children, urban hukou reform, and enrollment composition shifts. This allocation is compatible with the multi-cause account the Ministry of Civil Affairs gave for the 2016–2018 decline.

5.3. The Development Zone Quasi-Experiment

Within the sample, 158 counties obtained approval for provincial-level or higher development zones (new or expanded) during 2016–2019, distributed as 41, 31, 48, and 38 across the four years. Approval authority rests with provincial and national bodies and timing follows provincial productivity plans, which single-county fluctuations in LBC rates can hardly steer. The dynamic specification is Equation (3):

$$LBC_{it} = \sum_{\tau=-3}^4 \theta_{\tau} D_i \mathbf{1}[t - E_i = \tau] + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (3)$$

where D_i indicates treated counties, E_i is the approval year, $\tau = -1$ is the base period, and endpoint bins collect $\tau \leq -3$ and $\tau \geq 4$. The event-study sequence is plotted in Figure 2: coefficients in the two pre-approval periods are -0.14 and -0.50 with 95%

confidence intervals covering zero, so treated and control counties share no differential trajectory before approval. The data carry no pre-approval divergence of the kind a “seeking a zone because the left-behind problem worsened” account would produce. A null pre-trend of this precision constrains that account without excluding it, and the sensitivity analysis in Section 5.4 quantifies how large a post-approval violation of parallel trends the conclusion tolerates (Rambachan & Roth, 2023). The approval-year coefficient is -1.12 , reaching -2.08 in year 2 and -2.99 in year 4 and beyond; the deepening tracks project construction and hiring ramp-ups and matches the firm-entry dynamics of zone programs (Lu et al., 2019). Treatment timing spreads over 2016–2019, so the two-way fixed effects estimator mixes clean comparisons against never-approved counties with comparisons between earlier- and later-approved counties; Section 5.4 measures how much weight the second kind carries and re-estimates the effect with estimators that discard it. The static difference-in-differences estimate in Table 3, column (3), is -1.6505 (standard error 0.2812). One scale point governs how the estimates in this section are read together. The -1.6505 coefficient is the average effect of zone approval, a discrete event bundling land supply, tax treatment, infrastructure, and administrative attention; it is not the effect of one percentage point of investment intensity and does not sit on the same axis as -0.1807 or -0.3810 . The bridge between the two runs through the first stage of approval on investment intensity. Zone approval raises county investment intensity by about 2.4 percentage points on average, which the IV coefficient converts into an expected drop of 0.91 percentage points; the static DID estimate exceeds this, and the gap corresponds to hiring and complementary investment that zones bring beyond the recorded funds, in line with the local multiplier evidence on large projects (Greenstone et al., 2010; Moretti, 2010).

Table 3. Instrumental Variable and Difference-in-Differences Estimates.

	(1) First Stage: Investment Intensity	(2) Second Stage: LBC Rate	(3) DID: LBC Rate
Bartik Instrument Z	0.7608*** (0.0511)		
Investment Intensity		-0.3810^{***} (0.1063)	
Zone Approval $\times$ Post			-1.6505^{***} (0.2812)
Kleibergen–Paap F		221.81	
Controls	Yes	Yes	Yes
County / year FE	Yes	Yes	Yes
N	7,136	7,136	7,136
R^2	0.9076		0.6964

Note: columns (1)–(2) report two-stage least squares; column (3) treats the 158 counties approved for provincial-level or higher development zones in 2016–2019, with the treatment variable defined as treated county $\times$ post-approval. The exposure-robust standard error of Adão et al. (2019) for the second-stage coefficient is 0.1274.

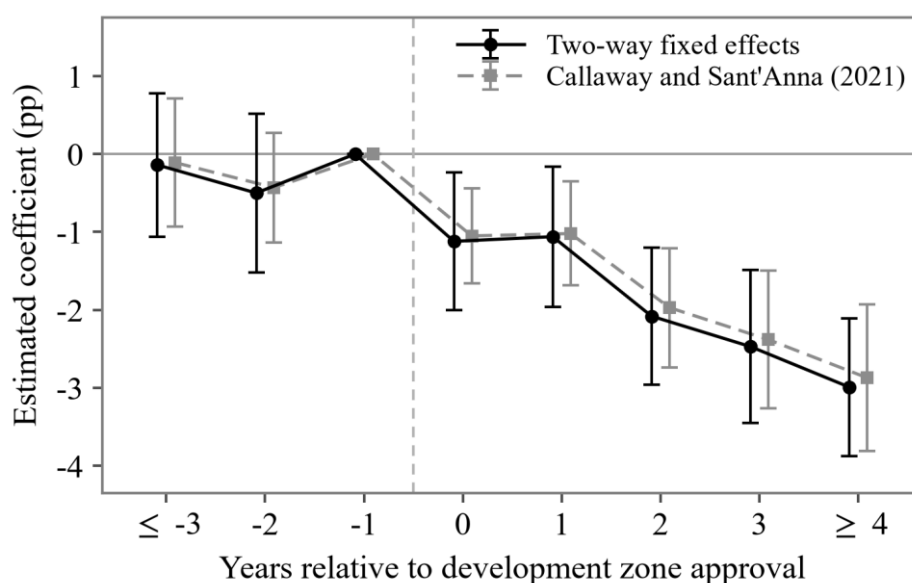


Figure 2. Event-Study Estimates Around Development Zone Approval.

Note: the vertical axis reports coefficients (percentage points) relative to the base period ($\tau = -1$); bars are 95% confidence intervals; periods right of the dashed line are post-approval. Black circles are two-way fixed effects estimates; grey squares are Callaway and Sant’Anna (2021) group-time estimates aggregated to event time with a never-treated comparison group.

5.4. Heterogeneity-Robust Estimators for the Staggered Design

Approvals arrive in four cohorts, 41 counties in 2016, 31 in 2017, 48 in 2018, and 38 in 2019, and 734 counties are never approved inside the window, so the composition of the comparisons the two-way fixed effects estimator forms can be measured rather than asserted. The Goodman-Bacon (2021) decomposition assigns 88.7% of the total weight to comparisons of approved counties against never-approved counties, 7.9% to comparisons of later-approved counties against earlier-approved counties in their pre-approval years, and 3.4% to the forbidden comparisons that use already-approved counties as controls. The forbidden group averages -1.42 against -1.68 for the clean comparisons, and the product of a 3.4% weight and a 0.26 percentage point gap bounds the contamination of the pooled estimate at about 0.01 percentage points.

Four estimators built for staggered adoption are reported in Table 4. The doubly robust group-time estimator of Callaway and Sant’Anna (2021) gives -1.5924 with a never-treated comparison group and -1.6218 with a not-yet-treated comparison group. The interaction-weighted estimator of Sun and Abraham (2021) gives -1.6137 , the DID-M estimator of de Chaisemartin and D’Haultfœuille (2020) gives -1.5468 , and the imputation estimator of Borusyak et al. (2024) gives -1.6702 . The five estimates span 0.12 percentage points and every confidence interval contains the two-way fixed effects point estimate of -1.6505 . The size of the never-treated pool is what holds them together: with 734 counties never approved, a clean control group is available for every cohort in every year, which is the configuration in which the two-way fixed effects weighting problem stays small (Roth et al., 2023).

The two event-study paths are plotted together in Figure 2. Aggregated to event time, the Callaway and Sant’Anna coefficients are -0.11 and -0.43 in the two pre-approval periods, and a joint test of the pre-approval coefficients gives $p = 0.63$; the post-approval sequence runs -1.05 at approval, -1.02 in year 1, -1.97 in year 2, -2.38 in year 3, and -2.87 in year 4 and beyond. The heterogeneity-robust path runs 0.04 to 0.12 percentage points shallower than the two-way fixed effects path at every post-approval horizon and reproduces its shape. Sensitivity in the framework of Rambachan and Roth (2023) puts the breakdown value at $M = 1.8$ for the year-2 and later coefficients: the sign of the effect survives post-approval violations of parallel trends up to 1.8 times the largest violation observed before approval, and it does not survive larger ones. Readers who treat null pre-trends as weak evidence can take that bound as the operative statement of what this design supports.

Table 4. Heterogeneity-Robust Estimators for the Staggered Development Zone Design.

Estimator	Comparison Group	ATT	SE	95% CI
Two-way fixed effects (Table 3, column (3))	All untreated county-years	-1.6505^{***}	0.2812	$[-2.202, -1.099]$
Callaway and Sant’Anna (2021), doubly robust	Never-treated	-1.5924^{***}	0.3106	$[-2.201, -0.984]$
Callaway and Sant’Anna (2021), doubly robust	Not-yet-treated	-1.6218^{***}	0.3021	$[-2.214, -1.030]$
Sun and Abraham (2021), interaction-weighted	Never-treated	-1.6137^{***}	0.2977	$[-2.197, -1.030]$
de Chaisemartin and D’Haultfœuille (2020), DID-M	Not-yet-treated	-1.5468^{***}	0.3542	$[-2.241, -0.853]$
Borusyak et al. (2024), imputation	Never- and not-yet-treated	-1.6702^{***}	0.2694	$[-2.198, -1.142]$

Note: the dependent variable is the LBC rate; every estimator uses the same control set, the same 158 treated counties, and the same 2015–2022 window ($N = 7,136$); standard errors are clustered at the county level and, for the group-time estimators, drawn from 1,000 multiplier bootstrap replications; the Goodman-Bacon (2021) weight decomposition is reported in the text.

6. Robustness

Specification changes run along three lines, measurement, sample, and fixed effects structure, with results collected in Table 5. On measurement: replacing the explanatory variable with the standardized word-frequency measure gives -0.2795 , so the attention-based and funds-based measures agree in sign and scale and reporting slack in any single indicator does not drive the conclusion; the standardized entropy-weighted composite gives -0.7307 , the drop per standard deviation of composite intensity, comparable to the baseline conversion of 0.83 percentage points. On the sample: recomputing the cut points once over the pooled panel instead of year by year gives -0.1761 , so the choice between annual and pooled trimming points leaves the estimate where it was and extreme counties do not dominate; dropping 2015, the single year that precedes the 2016 revision of the administrative left-behind definition, gives -0.1852 on 6,244 observations, so the definitional harmonization described in Section 3.1 is not doing the work; lagging the explanatory variable one year to break simultaneity gives -0.1748 , with the magnitude preserved under the lag, consistent with the delay between signing projects and hiring workers; dropping 2020–2022 to remove the disruption of labor flows and statistics during the pandemic gives -0.1109 , significant at the 5% level, where shortening the window to five years and cutting the sample by 37.5% inflates the standard error and trims the magnitude while sign and significance survive. On fixed effects: adding province $\times$ year effects, which absorb every provincial annual shock including poverty alleviation assessments and provincial livelihood programs, gives -0.1767 , nearly identical to the baseline, so the effect arises between counties within the same province rather than from provincial policy waves.

Table 5. Robustness Checks.

Specification	Coefficient	SE	N
Word frequency (standardized)	−0.2795***	0.0901	7,136
Entropy-weighted composite (standardized)	−0.7307***	0.1399	7,136
Pooled winsorization at 1st/99th percentiles	−0.1761***	0.0319	7,136
Excluding 2015 (pre-2016 definition)	−0.1852***	0.0341	6,244
Explanatory variable lagged one year	−0.1748***	0.0352	6,244
Excluding 2020–2022	−0.1109**	0.0495	4,460
Province × year FE	−0.1767***	0.0306	7,136

Note: Every row includes controls and county and year fixed effects (except the province × year row); the dependent variable is the LBC rate.

A placebo exercise addresses inference validity. The 892 county investment series are randomly reshuffled across counties (preserving each series' time path and annual distribution) and the column (2) specification of Table 2 is re-estimated 500 times. The distribution of permuted coefficients is shown in Figure 3: mean -0.0011 , standard deviation 0.0294 , 95% empirical interval $[-0.0579, 0.0545]$. The actual estimate of -0.1807 lies outside all 500 permuted coefficients, a permutation p -value below 0.002 . The association between the LBC rate and investment intensity is tied to each county's own investment series, and county-clustered inference has not understated the uncertainty from serial correlation.

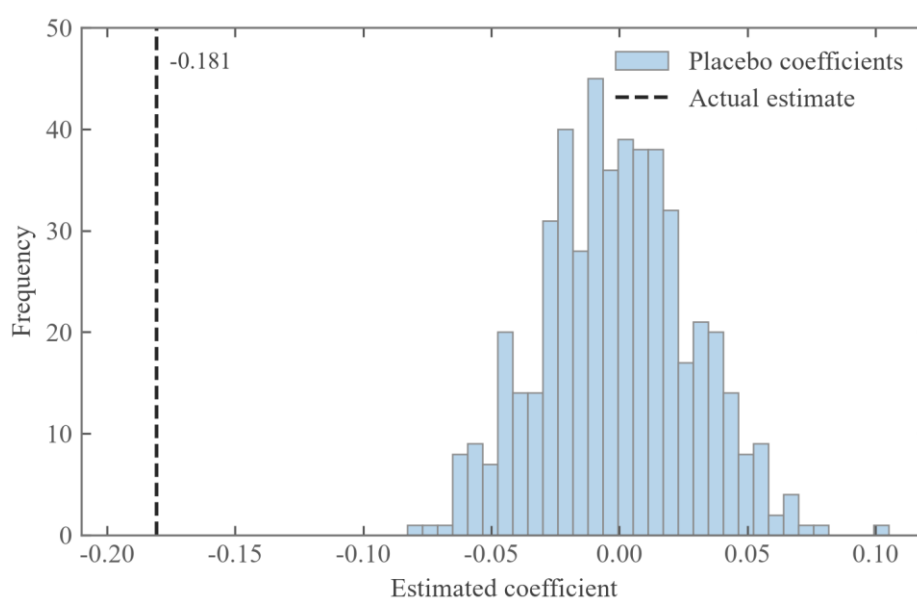


Figure 3. Placebo Test: Distribution of Coefficients Under Cross-County Permutation.

Note: the histogram shows 500 permutation estimates; the dashed line marks the actual estimate (-0.1807).

7. Heterogeneity

Sub-sample estimates appear in Table 6, with samples split along three dimensions and group differences judged by seemingly unrelated estimation tests. Region: the coefficient is -0.1032 for central counties and -0.2658 for western counties, group difference $p = 0.0123$. One comparison guards against a mechanical reading: the western mean LBC rate (18.23%) is below the central mean (19.62%), so the coefficient gap is not “big base, big drop”; western counties send workers on longer cross-province routes with rarer returns, a single relocation of a parent's job rewrites the child's status more fully, and the 2.6-fold coefficient ratio reflects a regional difference in the form of left-behind status rather than in its scale. Out-migration exposure: splitting at the median of base-year out-migration, the low group gives -0.0842 (10% level) and the high group -0.2672 , $p = 0.0043$. High-exposure counties average an LBC rate of 21.10% , 4.3 percentage points above the low group; the stock of parents at the migration margin sets the conversion rate of new jobs, and the micro-level sensitivity of migration decisions to local opportunities (Giles & Mu, 2007; Zhao, 1999) reappears in the county panel. Investment structure: splitting at the median share of labor-intensive industries in realized funds, capital-intensive counties give -0.1088 and labor-intensive counties -0.2514 , $p = 0.0266$. The cross-industry difference in jobs created per yuan (Lu et al., 2019) carries into the difference in LBC responses. Labor-intensive projects also absorb female employment at higher rates (Heath & Mobarak, 2015). Maternal absence tops the damage ranking in Cortes (2015), so jobs that keep mothers in the county deliver more prevention per yuan than capital-intensive projects of the same size. The 2.3-fold gap between the two groups is the sharpest statement the data make about investment quality: two counties can post identical realized-funds figures and record LBC responses that differ by more than a factor of two.

Table 6. Heterogeneity.

Group	Coefficient	SE	N	p-value for difference
Central counties	−0.1032**	0.0405	3,696	0.0123
Western counties	−0.2658***	0.0508	3,440	
Low out-migration counties	−0.0842*	0.0459	3,568	0.0043
High out-migration counties	−0.2672***	0.0447	3,568	
Capital-intensive counties	−0.1088**	0.0448	3,496	0.0266
Labor-intensive counties	−0.2514***	0.0461	3,640	

Note: all columns include controls and county and year fixed effects; *p*-values for group differences are based on normal approximation tests of coefficient differences.

8. Mechanisms

Mechanism estimates appear in Table 7. Columns (1)–(5) re-estimate Equation (1) with the mechanism variables as outcomes. Per percentage point of investment intensity: industrial employment rises by 2.66%, the out-migration share falls by 0.0538 percentage points, disposable income per capita rises by 0.43%, general public budget revenue rises by 1.03%, and the livelihood expenditure share rises by 0.0631 percentage points, all five significant at the 1% level. The employment coefficient can be checked against prefecture-level evidence: an elasticity of 2.66% sits inside the range of zone employment effects in Wang (2013) and Lu et al. (2019), so the labor market transmission of county investment attraction stays on the established scale.

Column (6) returns *lnEmp*, *OutMig*, and *Welfare* to the LBC equation: the investment coefficient contracts from −0.1807 to −0.1030, a 43.0% reduction; *lnEmp* enters at −2.1591 and *OutMig* at 0.3966, both significant at the 1% level, while *Welfare* enters at −0.0712, significant only at the 10% level. The product-of-coefficients decomposition assigns the channels their shares of the total effect: investment intensity transmits to the LBC rate through local employment (Sobel $z = -6.65$) with a share of 31.8%, through the out-migration share (Sobel $z = -3.04$) with 11.8%, and through livelihood spending (Sobel $z = -1.69$) with 2.5%, summing to 46.1%, which lines up with the 43.0% coefficient reduction. The decomposition is an accounting exercise. Reading these shares as causal requires that no unmeasured variable move both a mechanism and the LBC rate once the fixed effects and controls are in place, an assumption the design cannot test, so the shares are reported as magnitudes consistent with the channel structure rather than as identified mediation parameters. The relative strength of the two main channels fits the new economics of labor migration (Taylor et al., 2003). The urban-rural wage gap drives out-migration (Zhao, 1999), and a local job trades a nominal wage below the migrant wage for family reunion. The income channel (the *lnInc* column) works alongside the employment channel, and the proposition in Edmonds and Pavcnik (2005) that income growth reduces separation-type labor supply reappears at the county scale.

The weak livelihood channel calls for a direct statement. The revenue elasticity (1.03%) and the response of the livelihood expenditure share (0.0631 percentage points) both clear the 1% level; the chain breaks between spending and the LBC rate. County expenditure structures adjust to new revenue with a lag, and child care service spending works on the welfare stock of children already left behind (“care”) rather than on the incidence measure (“prevention”), so it can hardly register in the LBC rate within the year. The decomposition thus fixes a policy sequence: the contribution of investment attraction to prevention travels the employment path, while its contribution to care materializes only through earmarked fiscal arrangements.

The time pattern and the channel story are mutually consistent. The one-year-lag coefficient in Table 5 (−0.1748) is nearly as large as the contemporaneous one, and the event-study coefficients in Figure 2 accumulate from −1.12 to −2.99 over four post-approval years: investment leads to hiring, hiring to return migration, return migration to exits from left-behind status, each step with a lag, and the cumulative shape fits the employment channel while sitting awkwardly with a mechanical statistical linkage, which would appear on impact and stay flat. The return-timing evidence in Démurger and Xu (2015), where parents postpone return while children attend primary school, suggests that parents respond to local job openings around their children’s schooling transitions, which fits the concentration of the effect in years 2–4 after approval.

Table 7. Mechanism Tests.

	(1) <i>lnEmp</i>	(2) <i>OutMig</i>	(3) <i>lnInc</i>	(4) <i>lnFisc</i>	(5) <i>Welfare</i>	(6) LBC Rate
Investment intensity	0.0266*** (0.0037)	−0.0538*** (0.0174)	0.0043*** (0.0004)	0.0103*** (0.0009)	0.0631*** (0.0104)	−0.1030*** (0.0311)
<i>lnEmp</i>						−2.1591*** (0.1125)
<i>OutMig</i>						0.3966*** (0.0229)
<i>Welfare</i>						−0.0712* (0.0404)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County / year FE	Yes	Yes	Yes	Yes	Yes	Yes
<i>N</i>	7,136	7,136	7,136	7,136	7,136	7,136
<i>R</i> ²	0.7067	0.8221	0.9761	0.9770	0.8891	0.7282

Note: *lnEmp* is log employment in above-scale industrial firms, *OutMig* is the out-migration share (%), *lnInc* is log disposable income per capita, *lnFisc* is log general public budget revenue, and *Welfare* is the livelihood expenditure share (%).

9. Conclusion and Discussion

Across 892 counties and eight years the answer is consistent: greater county investment attraction lowers the LBC rate in the jurisdiction. Two-way fixed effects and the Bartik instrument bracket the response between −0.18 and −0.38 percentage points per percentage point of realized funds over GDP. The development zone design measures a different object, the effect of the approval event itself: −1.65 percentage points averaged over the post-approval years and about 3 percentage points by the fourth year, with four estimators built for staggered adoption placing that effect between −1.55 and −1.67. Seven specification changes and a 500-draw cross-county placebo leave sign and significance intact. The effect travels through local non-farm employment (31.8% of the total) and parental out-migration (11.8%), concentrates in western, high out-migration, and labor-intensive counties, and shrinks the both-parents-away share alongside the rate, which locates the change in the form of left-behind status that Zhang et al. (2014) and Fellmeth et al. (2018) associate with the largest costs.

Three implications operate at the working level. First, employment-oriented investment attraction belongs on the county policy list for LBC prevention. One standard deviation of investment intensity maps into 0.83 percentage points, 15.7% of the 5.29 percentage point decline recorded between 2015 and 2022; the instrumented coefficient applied to the observed 1.91 percentage point rise in investment intensity accounts for 13.8% of that decline. Child protection assessments and investment promotion assessments can be linked on that basis, and zone performance reviews can carry a family reunion indicator. Second, investment quality governs the return more than investment volume does. The coefficient gap between capital- and labor-intensive counties (−0.1088 against −0.2514) means the same hundred million yuan of realized funds buys a 2.3-fold difference in LBC response depending on industry mix, and the same gap runs through the mechanism estimates by way of the size of the employment response. A volume target, whether realized funds, contracted amounts, or project counts, can be met by capital-intensive projects that register in the numerator and leave the LBC rate where it was, which makes the headline investment figure a poor proxy for the prevention outcome. Four quality attributes carry the effect and can be measured at the appraisal stage: jobs created per hundred million yuan of realized funds, the share of posts open to workers with the education profile of returning migrant parents, the share of posts that women take up, and the durability of the employment relationship once construction subsidies expire. Industrial catalogs and the land and tax concession schedules attached to them can be indexed on these four attributes rather than on the funds figure alone; a county that meets a volume target through capital-intensive projects records investment success and no prevention gain. Third, the weak fiscal channel is a caution rather than a refutation: revenue gains that never pass through earmarked child care service budgets contribute to the left-behind problem only on paper, so the prevention line and the care line should be tied to separate budget hooks and assessed separately.

The limits are equally clear. The LBC rate rests on compulsory-education enrollment records. It misses preschool and senior high school children, and by construction it excludes children who left the county with their parents or dropped out of school, so it measures incidence among children schooled in the county rather than among children born there. The channel shares in Section 8 rest on a no-unmeasured-confounding assumption between mechanism and outcome that a county panel cannot test. County disclosures of realized funds carry reporting slack, and the word-frequency and composite measures offset this only in part. Bartik identification relies on the exogeneity of base-year shares, and Rotemberg-weight diagnostics (Goldsmith-Pinkham et al., 2020) could not be run in full at the county industry-classification grain. The shock-based reading of the design set out in Section 5.2 is a fallback for those diagnostics rather than a substitute for them. A county panel cannot follow individual families through migration and reunion, so the job quality of returned parents, the repair of parent-child relationships, and whether the achievement-psychology

divergence documented by Botezat and Pfeiffer (2020) disappears after reunion await matched micro-panels such as CFPS; that is the next step.

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