Special Economic Zones and Human Capital Investment: 30 Years of Evidence from China

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By exploiting the large quantity and rich variety of special economic zones (SEZs) in China, this study investigates how such zones affect human capital investment. Results show that SEZs significantly increase the local high school enrollment rate, but the impact varies across zone types: technology-oriented zones encourage education, while export-led zones discourage it. The increased job opportunities and wage premiums inside SEZs for employees with high school education increase high school enrollment, while such opportunities and wages for employees with middle school education decrease enrollment. A very small portion of the impact, if any, can be attributed to increased income. (*JEL* O14, O15, J24)

Special economic zones (SEZs) refer to geographically delimited areas where governments facilitate industrial activity through fiscal and regulatory incentives and infrastructure support. SEZs have been widely used in most developing and many developed economies. By 2018, there were nearly 5,400 SEZs across 147 economies and more than 500 new ones were in the pipeline (UNCTAD, 2019). The host countries range from OECD countries to developing economies in Asia, Africa, and South America. SEZs have attracted attention for their role as a potential growth engine. For example, economists have examined the local economic impacts of SEZs in the United States and European countries, and found that SEZs generally foster agglomeration economies, thus generating economic gains in the targeted areas (for a review, see Austin, Glaeser, and Summers, 2018; Neumark and Simpson, 2015). Besides their impact on local economic growth, SEZs have direct impacts on local job markets by increasing more job opportunities and/or offering higher wages. It is estimated that 68 million people worked in SEZs around the globe in 2007 (The Economist, 2015). Such job market opportunities are expected to affect human capital investment.

A few studies have examined the effects of industrial development on human capital investment, but there is no consensus. For example, Atkin (2016) examines the growth of export manufacturing in Mexico and finds a reduction in educational attainment because of increased opportunity costs (i.e., young people choose employment over education). Cascio and Narayan (2022) show that fracking, a technological breakthrough in the oil and gas

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industry, demands less-educated workers and therefore has led to an increase in the teen male dropout rate. In contrast, Oster and Steinberg (2013) find that job growth in the IT service sectors in India has led to increased school enrollment, because the outsourcing of technical support functions to India has increased the wage premium for skills obtained in school. Moreover, Le Brun, Helper, and Levine (2011) find some evidence of increased education and some heterogeneous effects depending on industry and gender. These studies suggest that the effect of SEZs may vary with the nature of the industry being promoted. Specifically, technology-oriented SEZs tend to demand more high-skilled workers, potentially encouraging young individuals to acquire more education, while export-led zones offer a wage premium for low-skilled workers, which may increase the opportunity cost of schooling and induce students to drop out. To clarify this point and help reconcile the mixed results across studies, we examined the effect of SEZs on educational attainment in China and explore differences by type of SEZ.

China has played an important role in the development of SEZs. Although the first modern SEZ was established in Ireland in 1959, the popularity of SEZs began to grow in the 1980s when China started to embrace the idea (The Economist, 2015). By 2010, China had launched more than 1,600 SEZs. Besides the large number of SEZs, China is also characterized by large variations in the types of SEZs. For instance, technology-oriented SEZs (technology SEZs), including economic and technological development zones (ETDZs) and high-tech industrial development zones (HIDZs), have been set up to promote high-tech industrialization and foster technology-based innovation. At the same time, many export-led SEZs (export SEZs), such as export-processing zones (EPZs), serve as sites for export processing and include labor-intensive sectors. With the various kinds of SEZs in China, we not only investigated the overall impact of SEZs on educational investment over a 30-year history, but also identified the channels for the heterogeneous effects.

In this study, we examined over 1,600 state- and province-level SEZs across China between 1980 and 2009, covering only about 0.1% of the total land area but contributing more than 10% of the country's GDP and approximately one-third of foreign direct investment (FDI) during the last year of our dataset. Combining the geocoded data of SEZs and population census information, we empirically examined the effect of SEZs on high school enrollment, an important human capital investment decision beyond the requirement for compulsory education at the county/district level in China,[1] for over 30 years. Based on individual records in the population census, we calculated the share of those who ever enrolled in high school education or above for each county cohort. The main explanatory variable is whether a county had an SEZ when the county cohort reached 15 years old, the age at which Chinese students usually finish middle school.

The main analysis exploits a difference-in-difference (DID) set-up that compares county cohorts exposed to an SEZ at the age of 15 to those that are not. In addition to the basic DID identification, a rich set of factors is controlled for, including county-specific time trends, province-specific year dummies, and county-type year dummies. However, as SEZs were phased in at different times over the 30-year period, and the effect of SEZs may vary across

[1] In China, five levels of local administrative units exist (from the highest to the lowest level): province, prefecture, county/district, town/jiedao, and village/community. To simplify the notation, we use county to represent county/district, henceforth.

counties and years, the traditional DID estimation could be biased in our setting, according to several recent studies (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). Therefore, we used the DID_M estimation proposed by de Chaisemartin and D'Haultfoeuille (2020) to report the main results; we provided the traditional DID estimation for robustness. Overall, we found a positive and statistically significant impact of SEZs on education: the establishment of an SEZ increases the high school enrollment rate by 3.1 percentage points, which is 11.3% of the high school enrollment rate during the 1980s. An instrumental variable (IV) identification based on predetermined cost-side considerations in the placement of SEZs also confirms the positive impact of SEZs on educational attainment. An event study exploiting a 19-year window centered around the launch of SEZs further validates the choice of the exposure age and regression specification.

The impact of SEZs on education varies by type of SEZ. Technology SEZs increase human capital investment, while export SEZs decrease it. The opposite impacts for technology-oriented zones and export-led zones replicate the mixed findings in the previous studies and motivate the exploration of three possible channels. The first is the income channel, in which parents invest more in their children's human capital if the establishment of an SEZ increases household income. The second is the job opportunity channel, in which the availability of low-skill jobs reduces high school enrollment, while the expected availability of high-skill jobs increases it. The third is the wage premium channel, which assumes that the introduction of an SEZ generates a higher wage premium for high-skill jobs.

An exploration of these mechanisms suggests that the overall impact of SEZs is closely tied to the job opportunity channel and the wage premium channel, while the income channel may account for a very small share of the effect, if any. The number of newly employed high (middle) school graduates inside SEZs per capita significantly increases (decreases) high school enrollment rate. Besides the new employees, the existing stock of employees with high school education is also associated with a rise in high school enrollment rate, but the effect is smaller in magnitude. A larger wage premium for high (middle) school graduates also encourages (discourages) high school enrollment.

This study contributes primarily to two strands of literature. The first is the literature on place-based policies. Scholars have investigated the local economic impact of place-based policies in the United States and European countries. These studies focus on outcomes like investment and productivity (e.g., Criscuolo et al., 2019; Devereux, Griffith, and Simpson, 2007), the number of new firms (e.g., Kline and Moretti, 2014; Mayer, Mayneris, and Py, 2017), employment (e.g., Neumark and Kolko, 2010; Gobillon, Magnac, and Selod, 2012; Freedman, Khanna, and Neumark, 2021), or quality of life (e.g., Reynolds and Rohlin, 2014). Several scholars have recently evaluated the economic outcomes of SEZ programs in China. For instance, Alder, Shao, and Zilibotti (2016) and Wang (2013) identify the causal effect of SEZs on local economic growth in China during its economic reform period using macro-level data. Zheng et al. (2017) investigate firm-level data to assess the effects of SEZs on total factor productivity and employment growth. This study complements the existing literature and provides solid evidence on the educational impact of SEZs in China over 30 years.

The second strand of literature to which this study contributes is the debate on individual decision-making regarding human capital investment. In a seminal book, Becker (1964) outlines a standard framework in which an individual faces a trade-off between the long-run benefits of human capital accumulation and the short-run return to labor in the presence of labor demand shocks. Several recent studies explore how local economic conditions affect human capital investment from different perspectives (e.g., Adukia, Asher, and Novosad, 2020; Atkin, 2016; Cascio and Narayan, 2022; Oster and Steinberg, 2013). A few recent studies explore the impact of globalization on local educational attainment in China (see, e.g., Jiang, Kennedy, and Zhong, 2018; Li, 2018; Li, Lu, Song, and Xie, 2019; Liu, 2017). These studies highlight the role of trade policy in the demand for labor with different skill levels, but they are limited to the export sector. By exploiting the diverse types of SEZs across more than 2,400 counties in China over 30 years, the present study illustrates how different types of SEZs influence local educational outcomes. Our analysis reconciles the mixed evidence of previous studies and finds that high-tech zones increase human capital investment, while export-led zones reduce it. The results support the job opportunity cost and return to schooling hypotheses.

The remainder of the paper is organized as follows. Section 2 outlines a conceptual framework for SEZs and highlights several channels through which they can influence local educational attainment. Section 3 sets the context for SEZs in China and describes the data used for analysis. Section 4 presents the empirical strategy and the main results. Section 5 investigates the mechanisms. Section 6 is our conclusion.

I. Conceptual Framework

Regarding how SEZs may affect educational choices, there are typically two theoretical perspectives, which are not mutually exclusive—one treats schooling as a consumption good and the other views education as an investment (Becker, 1964). From the consumption perspective, a household enjoys utility from consumption and education. If education is a normal good, an increase in household income will allow the household to consume more of all goods, including schooling. An SEZ can increase household income in several ways. For example, SEZs may provide parents with better employment or business opportunities, thus increasing household income. In addition, local families might receive compensation for expropriated land or might receive more rent for land that they own. Existing literature shows the positive connection between wealth and education. Goldin and Katz (1997) relate secondary education expansion to income and wealth in the United States. Edmonds, Pavcnik and Topalova (2009, 2010) demonstrate that Indian parents were less motivated to invest in their children's education when they experienced a relative rise in poverty due to a loss in tariff protection. Therefore, SEZs are likely to contribute to higher educational attainment through the income channel. However, if education is a "bad" because of the disutility associated with school attendance, as in Lazear (1977), SEZs can be negatively associated with schooling via the income effect.

From the investment perspective, a person's choice of education depends on the trade-off between long-term educational benefits and short-term costs. For simplicity, we assume there are two education levels—low and high. The short-term costs of a high level of education include both the direct cost of schooling (e.g., tuition, travel cost) and forgone labor income for less-educated individuals.[2] The long-term benefits consist of improved job opportunities

[2] As a simplifying assumption, we did not consider the impact of job experience on future income.

and increased wages. The launch of an SEZ can affect the job market conditions for both education levels, and the relative size of the impacts on the two groups influences the overall impact of SEZs on education. If an SEZ focuses on industries that mainly require less-educated workers, there is no short-term benefit to acquiring additional education, as individuals are immediately induced to work with low education. Atkin (2016) finds that many Mexican youth drop out as a result of the arrival of low-tech export-driven jobs. In our study, export-led SEZs may have a similarly discouraging effect on educational investment. In contrast, technology-oriented SEZs may offer many more job opportunities or/and much higher wages for highly educated individuals than those with low education. When improvement in the long-term benefits dominates the short-term wage gain, a person will be encouraged to obtain more education. Therefore, the type of SEZ matters. The effect manifests in the job opportunity channel or the wage premium channel or both.

In sum, the two theoretical perspectives suggest that an SEZ influences a person's educational choice through three possible channels: (1) income, (2) job opportunity, and (3) wage premium. By exploring variations in SEZs in China, we can provide empirical evidence on which of these channels are important.

II. Background and Data

A. *SEZs in China*

Since the end of the 1970s, the Chinese government has embraced the idea of establishing SEZs—specific areas for piloting reforms, introducing modern technology, and stimulating export-led growth. China's SEZs have generated substantial economic benefits to the local economy despite the high cost in infrastructure investment, as well as forgone tax revenues and land rent (Alder, Shao, and Zilibotti, 2016; Wang, 2013; Zeng, 2015). Between 1980 and 1984, the first set of cities hosting state-level SEZs achieved significant economic growth, with Shenzhen's GDP growing at 58% annually, Zhuhai's at 32%, Xiamen's at 13%, and Shantou's at 9% (Zeng, 2015). The success of citywide SEZs in boosting trade and investment has encouraged the governments at different administrative levels in China to create various types of zones.[3] As well as the ETDZs, HIDZs, and EPZs defined above, there are bonded zones (BZs), border economic cooperation zones (BECZs), and others. All SEZs benefit from favorable policies in terms of tax deductions, custom duty deductions, reduced land-use prices, flexibility in signing labor contracts, and favorable conditions for financing (Alder, Shao, and Zilibotti, 2016). However, each type of zone has a different economic focus. ETDZs and HIDZs provide incentives for stimulating the development of domestic high-tech firms, while EPZs promote FDI and foster growth in export-oriented and labor-intensive sectors. BZs and BECZs are intended to facilitate trade and FDI inflow with neighboring countries, all under the

[3] SEZs in China are authorized by four different administrative levels: (1) state, (2) province, (3) prefecture, (4) county (or district). The favorable policies in SEZs are offered by the governments, which are motivated to establish such place-based programs in order to attract investment. SEZs set up by higher levels of government, such as the state-level zones, tend to receive more beneficial policies. The provincial governments have focused on ETDZs, HIDZs, industrial parks, tourism zones, and specialized products within their jurisdictions. See https://www.china-briefing.com/news/chinas-economic-development-zones-types-incentives. Also, see Lu, Wang, and Zhu (2019) for detailed descriptions of SEZs and the associated preferential policies.

authority of the central government. This study primarily focuses on state- and province-level SEZs, for which detailed geographic information is available. We consider 238 state-level SEZs and 1,435 province-level SEZs established by the end of 2009.

An SEZ is not established randomly. Instead, the timing and location of its establishment depend on the trade-off between the relative benefits and costs. SEZs in China were first introduced in coastal areas and then spread to inland areas. Some types of SEZs need to be located in specific areas. For example, EPZs are usually close to ports, while BECZs are in border areas. Because the activities carried out in SEZs are typically land-intensive, suburban fringe areas are often ideal locations for SEZs due to the relatively low land cost and large size of land available not far from a city. Being located in suburban fringe areas also implies that SEZs can simultaneously affect both urban and rural areas.

Two important features of SEZs in China are relevant to investment in education. First, industrial firms located within SEZs create numerous job opportunities in the local economy. Zeng (2015) shows that, in 2007, SEZs accounted for more than 10% of total urban employment. Second, SEZs differ with regard to the skill level required for employment in the firms that are attracted by the SEZ opportunities. ETDZs and HIDZs are likely to absorb highly skilled workers because they are major platforms for attracting foreign investment and incubating high-tech firms. Meanwhile, EPZs tend to provide more low-skill job opportunities. Thus, both perceived and actual returns to education can be affected by the job demand in SEZs, in terms of the number of job opportunities and the wage premium for skills.

B. *Data*

For the main analysis, three sets of data were used: those related to education, SEZs, and county-level characteristics. This study focuses on county-level information to examine the effect of SEZs on local human capital investment, for several reasons. First, higher-level administrative units (i.e., prefecture-level city or province) often comprise more than one SEZ, making it challenging to identify the local impact of each SEZ. Second, there is a lack of data for units below counties. Third, the jobs inside a zone account for an average of 23% of the jobs in the host county, generating a significant shock to the county-level labor market.

Data on Education.—The data on education are obtained from four million personal records from the 2010 National Population Census, which provides information on gender, education level, home address, marital status, occupation, year and month of birth, birthplace, Hukou type,[4] and ethnicity (Han or minorities). Middle school dropouts find few employment opportunities and there is little variation in dropouts for young children at the compulsory middle school education level. Hence, this study uses high school enrollment rate as the primary indicator of human capital investment. A high school enrollment dummy variable is created, which takes the value of 1 if a person has received at least some high school education, and 0 otherwise. A county's high school enrollment rate is defined as the ratio between those who received at least some high school education and those who completed middle school in the same cohort. A cohort comprises all individuals born over a one-year period (from

[4] In China, Hukou is a citizen registration system, under which one is a citizen of the locality in which one's mother is a citizen. Citizenship confers specific local benefits—access to health care, free public education, legal housing, better access to jobs—for which non-citizens are not eligible. In general, there are two types of Hukou: urban and rural Hukou.

September to August). Cohorts are defined by the year in which the majority of the cohort turns 15 and faces the decision of whether to attend high school. For example, the 2008 cohort consists of individuals aged 15 in 2008. For Chinese children, the official enrollment age for primary school is six, primary school education lasts for six years, and middle school education takes three years. Therefore, most individuals make the decision on whether to enroll in high school when they are 15 years old, although some might make the decision earlier or later. [5] In this study, we constructed a dataset for county-cohorts between 1980 and 2009 using data from the 2010 National Population Census.[6]

Following Atkin (2016), we restricted the sample to non-migrants. Non-migrants are defined as those who report being born in the same county where their residence is officially registered (with local Hukou).[7] The exclusion of migrants can reduce the potential estimation bias as SEZs may attract better-educated persons to move into their host counties. However, the use of a sample of non-migrants cannot solve all the potential biases due to migration. For example, a person who leaves his/her home county after obtaining some high school education can cause a downward bias in the calculation of high school enrollment rate in his/her birth county. In addition, some individuals are classified as non-migrants but actually have lived in other counties, and their decisions surrounding human capital investment were affected by situations in other counties. We cannot correct these biases due to data limitations. Ideally, we would need to know the county in which the individual lived at age 15, but such information is not available in the 2010 National Population Census. The census provides information on the Hukou (official registration) address and current living address in detail at the county level, but if it is different from the Hukou address, information on birthplace and residence five years earlier is only available at the prefecture or province level but not detailed to the county level. However, the migration issue can only cause a minor bias, if any; the robustness check shows that including migrants or reducing the possibility of migration has little impact on the estimation. In total, non-migrants represent 79% of the full census sample. We dropped counties with fewer than five observations per cohort when constructing data on the county-level enrollment rates, leaving 2,413 counties in our sample.

Data on SEZs.—We built a dataset covering SEZs at the state- and province-level between 1980 and 2009. We geocoded each zone and identified which county a zone belongs to, based on the information on the exact boundaries of a zone provided by the "Bulletin List for the Official Boundaries of Chinese Industrial Parks."[8] On average, the size of an SEZ is smaller

[5] The majority of the country uses a six-year system, although some rural areas still used a five-year system until the 1990s. A person can also enroll in school earlier or later than the regulated age or repeat grades idiosyncratically. In the case of a person in the 2008 cohort who entered primary school late or repeated a grade, he/she may be faced with the high school enrollment decision in 2009 or 2010, but his/her data contribute to the 2008 cohort statistics. Similarly, a person who started school earlier and then decided whether to enroll in high school before age 15 still belongs to the 2008 cohort. Because of these inaccuracies, our estimation should be understood as the lower bound of the true effect.

[6] We did not use data from the 2010 cohort because some of its members were likely still in middle school when the survey was conducted.

[7] According to China's population census reporting system since 2000, a migrant (floating people) is a person whose place of residence differs from the registered residence (Hukou) and who has been away from the latter for more than six months. The 2010 Population Census data show that China has a floating population of 221 million.

[8] This report published by the Ministry of Natural Resources of China (Ministry of Land and Resources of China before 2018) provides information on state- and province-level SEZs in China since the 1980s, including the zone's name, code, location, year of establishment, planning area, and geographic boundaries (citywide SEZs are not included). Based on the precise geographical boundaries, we geocoded the zones and merged them with geocoded county data to determine to which county the zone belongs.

than that of a county. The average park size is 6.34 square kilometers, while the size of a county is about 3,370 square kilometers. Figure 1 shows the trends of SEZs at both the state and province levels. Over time we can observe an increase in the number of SEZs, especially those established by provincial governments.

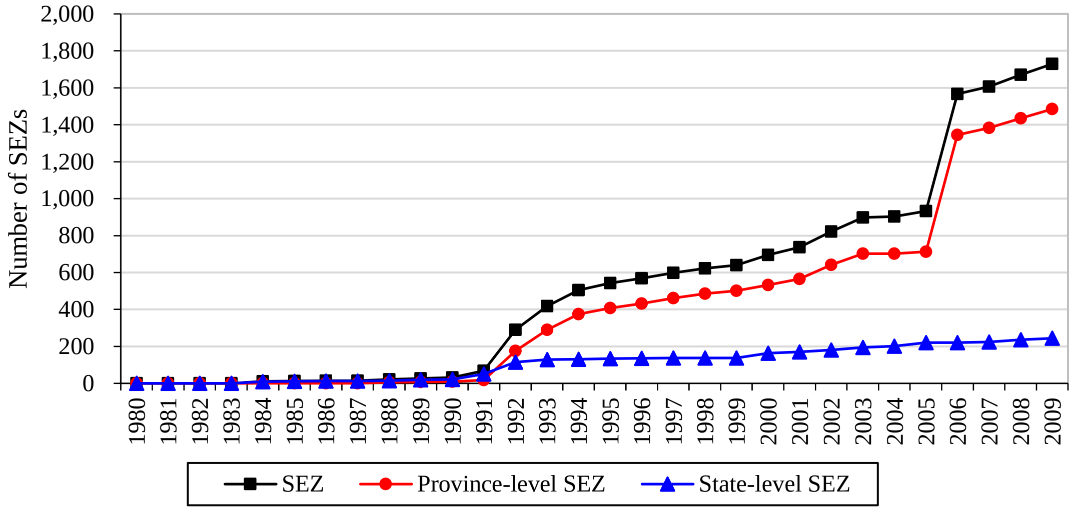

FIGURE 1. THE TRENDS IN THE NUMBER OF SEZS 1980–2009

Data on Other Characteristics.—A set of variables related to local economic and social characteristics is constructed using various sources, including China County Statistical Yearbooks, Fiscal Statistics of Prefectures, Cities, and Counties 1998–2007, China's Annual Survey of Industrial Firms (ASIF) 1998–2007, and the 2004 National Economic Census. The first two sources provide information on total GDP, population, fiscal expenditure, and revenues at the county level. The ASIF includes all the state-owned and non-state-owned enterprises with annual sales of more than RMB 5 million in the industrial sector, providing detailed information regarding a firm's name, location, number of employees, wage, output performance, and financial indicators. Using location information for firms, we matched them to the corresponding counties and SEZs, and then calculated the labor market conditions inside and outside SEZs for each county. The summary statistics of these variables are presented in Table 1.

TABLE 1—SUMMARY STATISTICS

Variable	Mean	Std. Dev.	Min	Max
Panel A: County (1980–2008)				
High school enrollment rate (Age 15)	0.397	0.274	0	1
Whether to own a SEZ	0.181	0.385	0	1
The number of SEZs	0.222	0.544	0	11
The total area of SEZs (km^2)	1.703	7.006	0	232.523
Panel B: Employment information (1998–2007)				
New employees per capita in SEZs	0.000	0.005	-0.132	0.142
Lagged total stock employees per capita in SEZs	0.004	0.016	0.000	0.465
New employees per capita outside SEZs	0.000	0.027	-2.406	0.899

Lagged total stock employees per capita outside SEZs	0.043	0.086	0.000	2.634
Share of employees with college degree or above in SEZs	0.078	0.078	0.000	0.899
Share of employees who completed high school in SEZs	0.735	0.261	0.001	1.000
Share of employees with middle school or below in SEZs	0.187	0.222	0.000	0.959
Share of employees with college degree or above outside SEZs	0.113	0.035	0.014	0.362
Share of employees who completed high school outside SEZs	0.343	0.032	0.164	0.502
Share of employees with middle school or below outside SEZs	0.543	0.063	0.232	0.822
GDP per capita (log)	8.295	2.058	5.964	13.161
Educational expenditure per capita (log)	4.718	1.215	3.318	9.023

III. Empirical Strategies And Main Results

To test the impact of SEZs on educational investment, we exploited variations in the existence and timing of SEZs across counties using the following equation:

(1) $$\boldsymbol{Y}_{it} = \beta \boldsymbol{SEZ}_{it} + \delta_i + \omega_{jt} + \gamma_i t + \boldsymbol{Poor}_i \tau_t + \boldsymbol{Port}_i \eta_t + \varepsilon_{it} \ ,$$

where Y_{it} is high school enrollment rate for the cohort of age 15 in year t in county (or district) i. The key predictor, SEZ_{it}, is a dummy variable that takes the value of 1 if county i has one or more SEZ(s) in year t when the cohort is aged 15, and 0 otherwise; that is, once a county is assigned 1 for a certain cohort, SEZ_{it} remains 1 for younger cohorts. Since several counties have more than one SEZ, and substantial heterogeneity exists in the size of SEZs, we considered other measures of SEZ_{it} in the robustness checks. The parameter δ_i estimates the county fixed effects. The term ω_{jt} estimates the province-specific yearly variation for province j in year t for $i \in j$, allowing the year fixed effects to vary across provinces. If a province has any specific policy or situation in a certain year that may similarly affect counties within its jurisdiction, this impact is captured by ω_{jt} and does not affect the estimation of β. By controlling for the province-specific year effects, we essentially compared counties within the same province. As the analysis is based on a 30-year panel dataset and different counties may have different growth potential, a county-specific time trend, $\gamma_i t$, is also included. In addition, we also controlled for poor-county-specific year dummies, $Poor_i \tau_t$, and port-county-specific year dummies, $Port_i \eta_t$. A poor county ($Poor_i$ = 1) is an officially identified state-level "Poor County" that is entitled to favorable policies and resources from the central government. The poor-county-specific year dummies control for the effect of the "8-7 plan,"[9] the second wave of China's poverty alleviation program implemented during the sample period (Meng, 2013). A county is categorized as a port county ($Port_i$ = 1) if it has a seaport. The port-county-specific year dummies control for possible differences in terms of openness and economic liberalization, which may have stronger influence on port cities (Fujita and Hu, 2001; Wang, 2013). The error term, ε_{it} is clustered at the county level to account for possible serial correlation within a county.

Equation (1) is essentially a DID identification with additional controls. Several recent studies illustrate that the existence of heterogeneous treatment effects— i.e., the same treatment has different effects among different samples or at different times—may cause bias in the traditional DID estimation if the treatments are phased in at different times over a long panel

[9] The program was called the "8-7 Plan" because its primary objective was to raise the majority of the remaining 80 million poor above the government's poverty line within seven years.

(Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021). The traditional DID method essentially takes the earlier treated observations as the comparison group for the later treated observations by assuming the treatment effect is stable. If the treatment effect is increasing over the years, the traditional DID method takes some part of the increasing effect as the year fixed effect, and then generates under-estimation of the treatment effect. Similarly, a decreasing treatment effect causes overestimation. To address such bias, we adopted a very recently developed two-way fixed effects estimator: the DID_M estimator in de Chaisemartin and D'Haultfoeuille (2020) (the DID_M estimator, hereafter). The DID_M method essentially estimates an equation as in (2):

$$\boldsymbol{Y}_{it} = \sum_{k\geq -L, k\neq -1}^{U} \gamma_k \boldsymbol{D}_{it}^{k} + \delta_i + \omega_{jt} + \gamma_i t + \boldsymbol{Poor}_i \tau_t + \boldsymbol{Port}_i \eta_t + \varepsilon_{it} \,. \tag{2}$$

Let s_i denote the year when county i launched an SEZ, and L and U be the largest numbers of years tested before and after s_i. We defined $D_{it}^{k} = 1$ if $t - s_i = k$, and 0 otherwise for $-L \leq k \leq U$ and $k \neq -1$. In other words, we took the year immediately before the launch of SEZs ($t - s_i = -1$) as the base and calculated the coefficients for all other years relative to the base. Therefore, the DID_M method directly offers a test of the pre-treatment trend (γ_k for $k < -1$) and an examination of the treatment effect over time (γ_k for $k \geq 0$). By averaging the yearly treatment effects, the average effect of SEZs can be determined. We set $U = 5$ for the average effect reported in the tables, but tested for robustness with $U = 13$, the maximum number that can be computed with the current STATA package.

A. *Main Analysis: Effect of SEZs on High School Enrollment*

In Table 2, we presented the results of the DID_M estimation of the effect of SEZs on high school enrollment rates in Panel B and compared them with the traditional DID estimation in Panel A. For each panel, Column (1) presents the most parsimonious two-way fixed effects specification controlling for the county fixed effects and the year dummies. Columns (2) and (3) incrementally add province-year dummies (and therefore drop the year dummies) and county-specific year trends, and Column (4) illustrates the full specification including all the controls in Equation (1). In Panel A, the inclusion of the province-year dummies reduces the two-way fixed effects estimation in Column (1) by almost half, and then the additional county-year trends and specific county-year dummies slightly modify the coefficient. With the full specification in Column (4), the traditional DID result suggests that the establishment of SEZs increases the high school enrollment rate by 0.0263.

Panel B of Table 2 presents the results of the DID_M estimation. In the first row, DID_M is the estimated average impact of SEZs based on the effects over the first five years starting from the launch of the SEZ t up until t+5. The striking pattern with the DID_M estimation is that the inclusion of additional control variables only slightly changes the estimation. With the DID_M estimation, the inclusion of county-year dummies only modifies the estimation from 0.0313 in Column (1) to 0.0322 in Column (2) of Panel B, while it changes the coefficient from 0.0469 to 0.0238 in the corresponding columns of Panel A when the traditional DID is used. Hence, the province-specific yearly variation may be one source of heterogeneous treatment effects. According to the result in Column (4) of Panel B, hosting SEZs increases high school enrollment rates for exposed cohorts by about 3.1 percentage points. In other words, for every

1,000 persons, 31 additional individuals are encouraged to enroll in high school if their county has an SEZ at the age of 15. As the high school enrollment rate was 27.5 % in China during the 1980s, the SEZ effect is about 11.3% of the baseline rate (3.1/27.5 = 11.3%).

TABLE 2—MAIN RESULTS: THE EFFECTS OF SEZS ON HIGH SCHOOL ENROLLMENT RATES AT AGE 15

	(1)	(2)	(3)	(4)	(5)
Panel A		Traditional DID			2SLS
SEZ	0.0469	0.0238	0.0292	0.0263	0.3513
	(0.0041)	(0.0039)	(0.0044)	(0.0043)	(0.1350)
N	69,967	69,966	69,971	69,971	69,967
R^2	0.656	0.679	0.713	0.714	0.455
Panel B	de Chaisemartin and D'Haultfoeuille (2020)'s DID_M estimators				
DID_M	0.0313	0.0322	0.0325	0.0308	
	(0.0047)	(0.0062)	(0.0073)	(0.073)	
$DID_M^{pl,1}$	-0.0111	-0.0048	-0.0052	-0.0046	
	(0.0056)	(0.0052)	(0.0051)	(0.0050)	
$DID_M^{pl,2}$	0.0070	0.0029	0.0025	0.0022	
	(0.0050)	(0.0048)	(0.0070)	(0.0049)	
$DID_M^{pl,3}$	0.0018	-0.0003	-0.0006	-0.0006	
	(0.0060)	(0.0052)	(0.0051)	(0.0050)	
County fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	no	no	no	no
Province×year fixed effects	no	yes	yes	yes	yes
County-year trends	no	no	yes	yes	yes
Poor-county×year fixed effects	no	no	no	yes	yes
Port-county×year fixed effects	no	no	no	yes	yes

Notes: Standard errors in parentheses are clustered at the county level. The coefficient reported for DID_M in this table is the simple average of the yearly effects starting from the opening year t to t+5 using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020). The DID_M Stata package also provides the standard errors directly. $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$ are the placebo estimators for the three pre-treatment time periods, t–2, t–3, and t–4, respectively.

In Panel B, we also reported three pre-treatment estimators – $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$.[10] Since de Chaisemartin and D'Haultfoeuille (2020) use t = –1 as the reference period, the three superscripts, 1, 2 and 3, correspond to the second, third, and fourth years, respectively, before the establishment of SEZs. As shown in Columns (1)–(4) of Panel B, most pre-treatment estimators are small and not significantly different from 0.[11] This indicates that, before hosting an SEZ, counties did not experience significant changes in educational outcomes compared to those that never hosted one.

Figure 2 plots both pre-treatment and post-treatment trends. Two points are worth highlighting. First, prior to the establishment of an SEZ, the high school enrollment rates in the

[10] According to de Chaisemartin and D'Haultfoeuille's (2020) new command, because the dynamic effect is estimated, t = –1 is the reference period. Thus, the first placebo estimator, $DID_M^{pl,1}$, compares the difference in high school enrollment rate between counties with and without an SEZ in t–1 with the difference between these two types of counties in t–2. $DID_M^{pl,2}$ performs the same comparison of treated and untreated counties between t–1 and t–3. $DID_M^{pl,3}$ makes such a comparison between t–1 and t–4.

[11] There is only one exception in column 1, which indicates the importance of including province-specific year fixed effects.

SEZ counties are similar to those in non-SEZ counties, and the difference is small and statistically insignificant. The pre-treatment trend also validates our use of the DID_M estimator. Second, 14 years of post-treatment estimates show a slightly increasing trend in the effect of SEZs, with some ups and downs. The ups and downs may reflect actual fluctuations in the effect and/or sample selection, because fewer counties are involved in the estimation of the longer period. Despite this, the estimated effect of SEZs remains statistically significant for all years starting from the launch of SEZs.

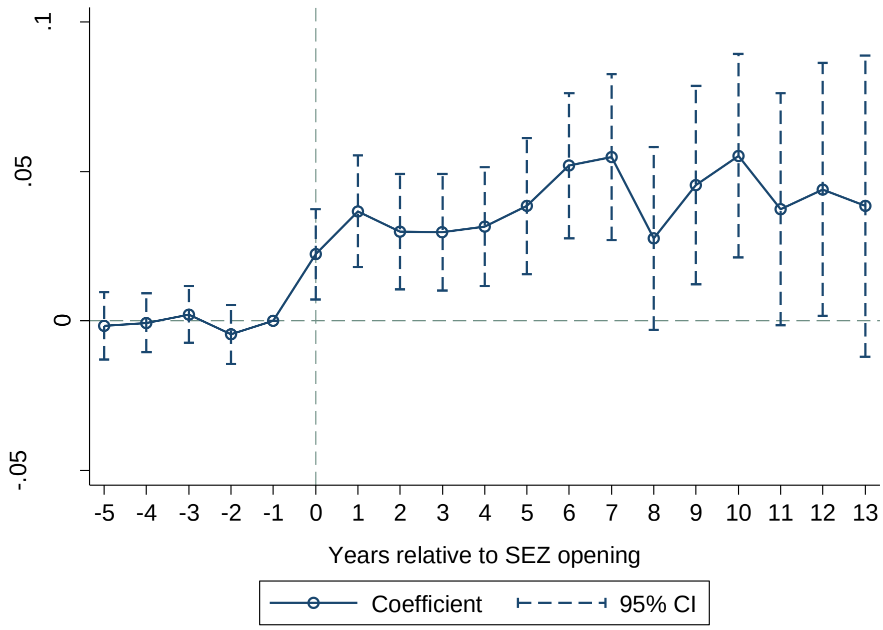

FIGURE 2. EVENT STUDIES: THE EFFECTS OF SEZS ON HIGH SCHOOL ENROLLMENT RATES AT AGE 15

Notes: The figure plots the coefficients along with their corresponding 95% confidence intervals for γ_k according to equation (2) using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020).

B. *Heterogeneity Analysis*

We conducted several heterogeneity analyses to better understand the effect of SEZs on local human capital investment. We first explored whether the effects on local educational outcomes differ across different types of SEZs. China's state- and province- level SEZs can be grouped into four broad types based on their stated missions and economic focus. The first type of SEZ is technology SEZs, including ETDZs and HIDZs. Technology SEZs are broadly defined as zones that attract technology-intensive firms and foster technological innovation. They account for 83.9% of all sample zones. A second type of SEZ is export SEZs, with a focus on labor-intensive sectors. This type is mainly composed of EPZs that are designed to promote export-oriented sectors by processing imported raw materials and exporting final goods that do not enter China's mainland. The second type has 57 zones, accounting for 3.41% of our sample zones. The third type comprises border SEZs, including both BZs and BECZs. All BZs and BECZs are located in border cities; their main functions are to facilitate trade with bordering foreign countries, with the latter additionally encouraging the entry of e-commerce platforms, payment, logistics, warehousing, and financial service enterprises. There are 45 BZs and BECZs in total. The "other" category comprises all other types of SEZs, such as state-level tourism zones and provincial-level specialized products zones.

We modified Equation (1) to test the heterogenous impact of SEZs on high school enrollment rates by substituting the main predictor with dummy variables for each of these four categories. Table 3 reports the results for each type of SEZ based on the DID_M estimations. The pre-treatment estimators for different types of SEZs are found to be insignificant, supporting the common trend assumption, with only one exception, in Column (2) for border SEZs. To deal with the pre-trend problem, we re-did the analysis using a matched sample for counties with border SEZs.[12] As shown in Column (2'), the matched sample maintains the common trend assumption, and the estimated effect of border SEZs is very close to the magnitude in Column (2) (–0.1103 vs. –0.0957).

The differences in impacts across types of SEZs are of great interest because they shed light on the mixed nature of the findings in the literature. Column (1) shows that the effect of technology SEZs on cohort schooling is 0.0310, positive and statistically significant at the 1% level. However, the effect of export SEZs is found to be –0.1103, negative and statistically significant, in Column (2'). Columns (3) and (4) display positive effects of border-related zones and zones in the "other" category: 0.0172 and 0.0305, respectively. Further tests suggest that the differences between the effect of export SEZs and those of all other three types are statistically significant.[13] The effect of technology SEZs is 0.0138 higher than that of border SEZs in magnitude, but close to that of the "other" category; the differences across them are not statistically significant. Figure 3 presents trends before and after treatment for each type of SEZ. All the pre-treatment trends validate the use of the DID_M estimators. The post-treatment estimates in Panel (a) show a pronounced upward trend in the effect of technology SEZs. These drive the overall path in Figure 2 because they comprise around 84% of our SEZ sample. At the same time, in Panel (b), we observe a dramatic decline in the impact of export SEZs.

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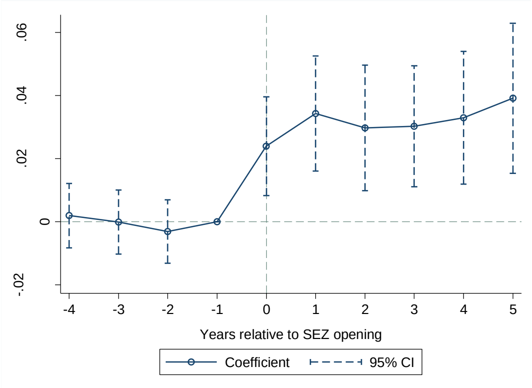
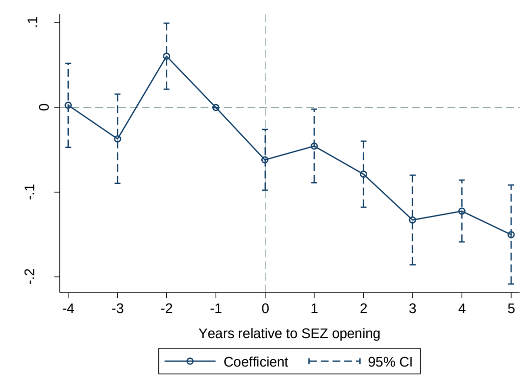
[12] The first border SEZs were established in 1990s, so the matching is based on the high school enrollment rate in the 1980s.

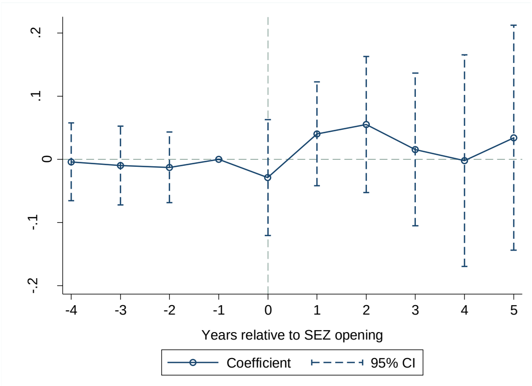
[13] We conducted two types of statistical tests. One was the two-sample t-test using point estimates and standard errors in Table 3. The test assumes no correlation between point estimates. The other test pooled all types of SEZs and added variables on each type of SEZ in the traditional DID estimation. The variables are largely 0 or 1, indicating whether a certain type of SEZ is present in a county-year. When a county has more than one type of SEZ in a certain year, the variable on each type of SEZ is 1/K, where K is the number of types of SEZs. The conclusions are consistent in the two tests: The effect of EPZs is significantly different from the effect of each of other three types, and the other three are not significantly different.



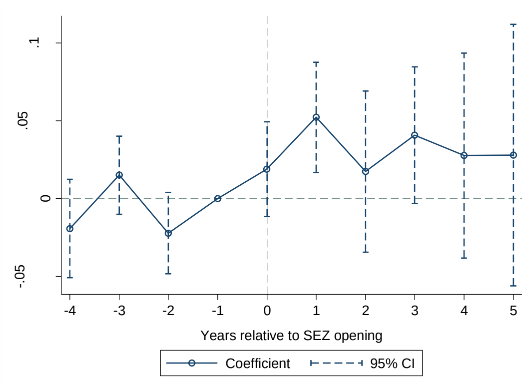
a. Technology SEZs



b. Export SEZs



c. Border SEZs



d. Other SEZs

FIGURE 3. EVENT STUDIES: THE EFFECTS OF DIFFERENT TYPES OF SEZS ON HIGH SCHOOL ENROLLMENT RATES AT AGE 15

Notes: Technology SEZs, Export SEZs, Border SEZs, and Other SEZs are technology-oriented zones, export-led zones, border related zones, and zones in the other category. Each graph plots the coefficients along with their corresponding 95% confidence intervals for γ_k according to Equation (2) using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020).

TABLE 3—HETEROGENEITY ANALYSIS: THE EFFECTS OF SEZS BY SEZ TYPE

	SEZ Types				
	Technology SEZs	*Export SEZs*	*Export SEZs*	*Border SEZs*	*Other SEZs*
	(1)	(2)	(2')	(3)	(4)
DID_M	0.0310	-0.0957	-0.1103	0.0172	0.0305
	(0.0075)	(0.0165)	(0.0541)	(0.0497)	(0.0167)
$DID_M^{pl,1}$	-0.0032	0.0604	0.0283	-0.0128	-0.0222
	(0.0051)	(0.0199)	(0.0172)	(0.0285)	(0.0134)
$DID_M^{pl,2}$	-0.0001	-0.0370	0.0004	-0.0098	0.0151
	(0.0052)	(0.0270)	(0.0205)	(0.0319)	(0.0128)
$DID_M^{pl,3}$	0.0019	0.0024	0.0233	-0.0041	-0.0192
	(0.0052)	(0.0252)	(0.0195)	(0.0314)	(0.0161)

Notes: Technology SEZs, Export SEZs, Border SEZs, and Other SEZs are technology-oriented zones, export-led zones, border related zones, and zones in the other category. Control variables include county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level. The coefficient reported for DID_M in this table is the simple average of the yearly effects starting from the opening year t to t+5 using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020). The DID_M Stata package also provides the standard errors directly. $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$ are the placebo estimators for the three pre-treatment time periods, t–2, t–3, and t–4, respectively. As $DID_M^{pl,1}$ is significant in Column (2), we ran an analysis with matched sample for counties with EPZ, and Column (2) presents the results.

We then investigated whether SEZs affect cohort schooling differently across gender, region, and area. As shown in Columns (1) and (2) of Table 4, schooling increases significantly for girls when an SEZ is introduced locally, but there is no significant effect on boys' educational investment. Columns (3)–(5) show that the effects of SEZs on local educational attainment are most pronounced in central counties and least in western counties. Columns (6) and (7) show that the effects in urban and rural areas are both insignificant, but the point estimate for urban areas is a bit larger. In all columns, the placebo estimators are reported and none of them are statistically significant, meeting the common trend assumption.

TABLE 4—HETEROGENEITY ANALYSIS: THE EFFECTS OF SEZS BY GENDER, REGIONS, AND AREAS

	Gender		Different regions			Areas	
	Girl	Boy	Eastern	Central	Western	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DID_M	0.0263	0.0129	0.0285	0.0416	0.0184	0.0279	0.0120
	(0.0111)	(0.0097)	(0.0110)	(0.0132)	(0.0140)	(0.0193)	(0.0136)
$DID_M^{pl,1}$	-0.0027	0.0000	-0.0048	-0.0017	-0.0099	-0.0074	-0.0035
	(0.0067)	(0.0060)	(0.0066)	(0.0088)	(0.0100)	(0.0079)	(0.0055)
$DID_M^{pl,2}$	-0.0019	-0.0002	0.0030	-0.0009	0.0025	0.0026	0.0015
	(0.0068)	(0.0068)	(0.0071)	(0.0085)	(0.0108)	(0.0086)	(0.0061)
$DID_M^{pl,3}$	-0.0038	0.0014	-0.0037	0.0080	-0.0016	0.0117	-0.0100
	(0.081)	(0.0073)	(0.0073)	(0.0090)	(0.0110)	(0.0081)	(0.0061)

Notes: Control variables include county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level. The coefficient reported for DID_M in this table is the simple average of the yearly effects starting from the opening year t to t+5 using the $\mathrm{DID_M}$ estimation suggested by de Chaisemartin and D'Haultfoeuille (2020). The DID_M Stata package also provides the standard errors directly. $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$ are the placebo estimators for the three pre-treatment time periods, t–2, t–3, and t–4, respectively.

C. *Robustness Check*

We conducted several additional robustness checks to verify the findings. First, some counties in the sample have more than one SEZ, and the land area varies across zones. These variations are not captured in the dummy variable for whether a county hosts an SEZ. In Table 5, Columns (1) and (2) repeat Equation (1) using alternative measures for SEZs—the total number of SEZs and the total area of SEZs in each county, respectively. The estimates confirm that SEZs have positive and statistically significant effects on local educational outcomes.

Second, we excluded the four star cities—Beijing, Shanghai, Tianjin, and Chongqing, which are municipalities directly under the central government. Each of these cities hosts more than 10 state- and province-level SEZs. Since they are home to large interest groups with strong political power, these cities might be favored for various kinds of resources other than SEZs (Chen, Henderson, and Cai, 2017). This phenomenon may generate omitted variable problems. Column (3) of Table 5 reports the $\mathrm{DID_M}$ estimator in terms of the treatment effect obtained by dropping counties in these star cities. The magnitude of the SEZ effect size (0.0308) is identical to that in the primary estimation result (Column (4) of Panel B in Table 2).

Third, we examined whether the treatment effect of SEZs is robust to sample selection related to migration issues. In Column (4) of Table 5, we re-estimated the specification (1) and expanded the sample by including migrants who are living a county away from their registered residence. The individual records used to calculate the county cohorts increase by 27% (1.27=1/0.79, the latter of which is the share of non-migrants in the 2010 population census sample). The estimated coefficient is 0.0263, which is close to our main estimate. The slightly smaller magnitude (0.0263-0.0308=-0.0045) goes against the common belief on migration selection—SEZs did not seem to attract highly educated individuals more. Columns (5) and (6) compare estimates from different datasets. In Column (5), we used non-migrants in the *2000 Population Census data* (the 5th round of population census in China) to examine the effect of SEZs for the 1980–1999 cohorts. In Column (6), we restricted the sample to be exactly the same cohorts but used the *2010 Population Census data* (the sixth population census data in China). There is an additional 10 years during which people in the 2010 sample could move, so migration would be more of an issue in the 2010 data than in the 2000 data. Despite this, the estimated effects of the SEZs are largely similar across the two samples—0.0223 and 0.0262, respectively. Columns (4)–(6) together suggest that, if selection by migrants causes an estimation bias, the bias should be small.

Fourth, we explored whether our primary estimation is robust to the varying population sizes at the county level. The main analysis uses data at the county-cohort level. If different counties have very different population size, the average of county cohorts may be different from the national average. As such, we estimated the impact of SEZs on schooling based on individual data. Column (7) Table 5 reports the DID_M estimators without controlling for personal attributes, while column (8) additionally controls for gender and ethnicity. We obtained estimates very similar to our primary estimation (0.0304 and 0.0301 versus 0.0308).

TABLE 5—ROBUSTNESS CHECK

	Total SEZ number (1)	Total SEZ area (2)	Drop four star cities (3)	Migrants included (4)	Sample from the 2000 National Population Census (5)	Subsample with those who made high schooling decisions before 2000 (6)	Individual level data without personal attributes (7)	Individual level data with personal attributes included (8)
SEZ	0.0094	0.0007						
	(0.0037)	(0.0003)						
DID_M			0.0308	0.0263	0.0223	0.0262	0.0304	0.0301
			(0.0073)	(0.0069)	(0.0089)	(0.0110)	(0.0066)	(0.0066)
$DID_M^{pl,1}$			-0.0057	-0.0021	0.0025	-0.0066	-0.0028	-0.0028
			(0.0052)	(0.0043)	(0.0057)	(0.0081)	(0.0030)	(0.0030)
$DID_M^{pl,2}$			0.0024	0.0064	0.0057	0.0062	-0.0027	-0.0029
			(0.0050)	(0.0043)	(0.0066)	(0.0076)	(0.0036)	(0.0036)
$DID_M^{pl,3}$			0.0006	-0.0036	-0.0039	-0.0003	0.0017	0.0017
			(0.0053)	(0.0046)	(0.0059)	(0.0071)	(0.0033)	(0.0034)
N	69,971	69,971						
R^2	0.713	0.713						

Notes: Control variables include county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level. The coefficient reported for DID_M in this table is the simple average of the yearly effects starting from the opening year t to t+5 using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020). The *DID_M* Stata package also provides the standard errors directly. $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$ are the placebo estimators for the three pre-treatment time periods, *t*–2, *t*–3, and *t*–4, respectively.

Fifth, we tested the validity of using age 15 as the exposure age for high school enrollment decisions. Our identification strategy is built on the assumption that children are disproportionately affected by the opening of SEZs locally at age 15, because this is the age at which most students decide whether or not to attend high school. Using SEZ exposure at other ages as the main predictor, we repeated the baseline estimation to examine whether 15 is a valid exposure age. We ran 7 regressions for hypothetical exposure ages ranging from 13 to 19. If 15 is a valid exposure age, considering other ages as the exposure age will cause misclassification and generate attenuation bias. When a higher age is used, some county cohorts who were not treated by an SEZ are misclassified as 1 in the SEZ_{it}, thereby reducing the estimated effect. Similarly, if a younger age is used, the control group will contain county cohorts who were actually affected by SEZs, thereby reducing the treatment effect. Figure 4 plots coefficients of these 7 DID_M estimators and the associated 95% confidence intervals. Indeed, the DID_M estimators show that the largest positive impact occurs at age 15, supporting our belief that 15 is the appropriate age of exposure.

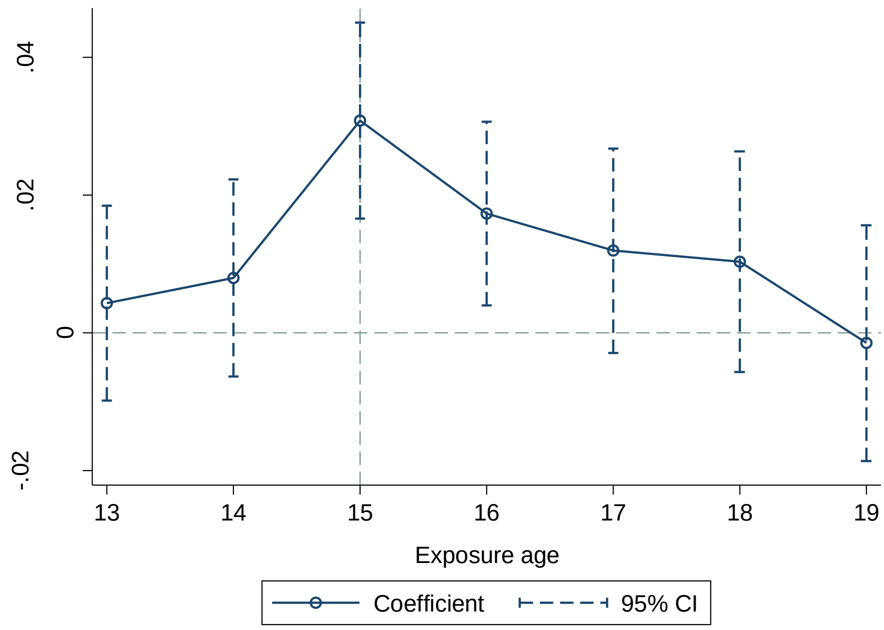


FIGURE 4. THE EFFECTS OF SEZS ON HIGH SCHOOL ENROLLMENT RATES AT DIFFERENT EXPOSURE AGES

Notes: The figure plots the coefficients along with their corresponding 95% confidence intervals for the effect of SEZs using different exposure ages. The coefficient is the simple average of the yearly effects starting from the opening year *t* to *t*+5 using the DID_M estimation suggested by de Chaisemartin and D'Haultfoeuille (2020).

Last, we explored the effect of SEZs on other educational decisions, including middle school enrollment, college enrollment, middle school completion, and high school completion. Middle school is compulsory in China, and this policy has been implemented very well. In addition, there are very few job opportunities for teenagers aged 15 or below. Therefore, SEZs should not affect middle school enrollment or completion. During the sample period, college enrollment was very competitive and largely constrained by enrollment quotas; students who had potential for college enrolled in high school. Consequently, SEZs should hardly influence college enrollment. Table 6 presents the estimations on other educational choices, for which we used the exposure to SEZs at relevant ages as the main predictor. Columns (1)–(3) suggest that, when there is supposed to be no effect, a null effect is found. These are essentially placebo analyses, validating the estimation regression and excluding other potential factors that could explain the impact on high school enrollment rates. For completeness, in Column (4), we also reported the effect of SEZs on high school completion rates conditional on the enrollment. SEZs could influence high school completion, but the null effect might be due to the low potential for improvement in the high school completion rate conditional on the enrollment, given that the average high school completion rate is as high as 94.4%.

TABLE 6—THE EFFECTS OF SEZS ON VARIOUS ENROLLMENTS AND COMPLETIONS

	Middle school enrollment rate at age 12 (1)	College enrollment rate at age 18 (2)	Middle school completion rate at ages 13–15 (3)	High school completion rate at ages 16–18 (4)
DID_M	-0.0006	0.0000	-0.0018	-0.0009
	(0.0032)	(0.0132)	(0.0022)	(0.0031)
$DID_M^{pl,1}$	-0.0020	0.0022	-0.0007	-0.0001
	(0.0021)	(0.0083)	(0.0005)	(0.0009)
$DID_M^{pl,2}$	0.0032	-0.0000	0.0003	-0.0009
	(0.0021)	(0.0084)	(0.0005)	(0.0004)
$DID_M^{pl,3}$	0.0016	-0.0022	-0.0003	0.0004
	(0.0025)	(0.0092)	(0.0004)	(0.0008)

Notes: Control variables include county fixed effects, county-time trends, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level. The key predictors represented by DID_M are the exposure to SEZs at relevant ages. The coefficient reported for DID_M in this table is the simple average of the yearly effects starting from the opening year *t* to *t*+5 using the $\mathrm{DID_M}$ estimation suggested by de Chaisemartin and D'Haultfoeuille (2020). The DID_M Stata package also provides the standard errors directly. $DID_M^{pl,1}$, $DID_M^{pl,2}$ and $DID_M^{pl,3}$ are the placebo estimators for the three pre-treatment time periods, *t*–2, *t*–3, and *t*–4, respectively.

D. *IV Estimation Based on Cost-Side Considerations*

Another potential pitfall in this setting is the non-randomness of the distribution of SEZs across counties (Wang, 2013; Khan et al., 2021). We tested for the common trends in the $\mathrm{DID_M}$ estimation, and this section provides additional support using an instrumental variable approach. Following the forecasting method proposed by Lipscomb, Mobarak, and Barham (2013), we predicted the likelihood of the placement of SEZs based on predetermined local geographic attributes over each of the five-year periods (see Appendix Table 1). To further alleviate the endogeneity concern, we adopted a jackknife method developed by Jackson, Johnson, and Persico (2016), and estimated the probability of having an SEZ by excluding all data from its host province. This "leave-out" estimate partly alleviates the concern about a weak instrument problem as it is less likely to violate the exclusion restriction criterion. We ranked the counties within each province based on the estimated probability, as was done by

Duflo and Pande (2007) and Lipscomb, Mobarak, and Barham (2013), and generated a 0/1 variable on the predicted $\widehat{SEZ}_{it}$, for the top N_j counties if the province launched N_j SEZs during that period. The predicted $\widehat{SEZ}_{it}$ serves as an instrumental variable for the actual SEZ_{it}. Appendix Table 2 presents the first-stage result. Column (5) in Table 2 shows the second-stage result from 2SLS. Although the point estimate is much larger than those in the traditional DID or DID_M estimation, the 2SLS estimate confirms the positive impact of SEZs on high school enrollment rates.

IV. Mechanisms

The previous sections have shown that SEZs generally increase human capital investment, but different types of SEZs can demonstrate opposite effects. This section explores the mechanisms behind these effects. Based on the conceptual framework in Section 2, we empirically tested three possible mechanisms: income channel, job opportunity channel, and wage premium channel.

A. *The Income Channel*

To test the income channel, we constructed a subsample covering the period 1998–2007, for which we procured reliable data across counties. We first repeated the baseline regression to check the similarity between the subsample and the full sample. Because subsequent explorations of other channels cannot be estimated using the DID_M method, we relied on the traditional two-way fixed effects regression in this section. The coefficient of the DID estimator in Column (1) of Table 7 is 0.023 with statistical significance; this is close to 0.026, which is the coefficient in Column (4) of Panel A of Table 2 for the full sample. Therefore, the subsequent findings from this subsample are likely to explain the results in the full sample.

We then examined the effects of an SEZ on local income and expenditure; the former is represented by the logarithm of GDP per capita, while the latter is the logarithm of government educational expenditure per capita. The data on county-level GDP per capita was obtained from the China County Statistical Yearbooks for the period 1998–2007. The fiscal educational expenditure at the county level was obtained from the Fiscal Statistics of Prefectures, Cities, and Counties for the same period. The necessary, but not sufficient, condition for establishing an income channel is that SEZs affect income and income affects educational choice. The result in Column (2) of Table 7 shows that the introduction of an SEZ has a positive effect on local GDP per capita (statistically significant at the 1% level). The result in Column (3) suggests that government educational expenditure does not increase after the establishment of an SEZ; the point estimation is very small and far from being significant (t = 0.33).[14]

Since the logarithm of GDP per capita increases following the launch of an SEZ, we further tested whether it affects educational choice. The regression in Column (4) replaces the SEZ dummy with the logarithm of GDP per capita as the main predictor, and the result shows a significant impact of GDP per capita on high school enrollment rates. Therefore, the income channel might be involved. However, the product of the coefficients in Columns (2) and (4) is

[14] The result is similar if we do not take the log of government educational expenditure.

only 0.00075 (0.188*0.004), much smaller than the effect (0.023) shown in Column (1) of Table 7. In other words, the impact of an SEZ that operates via the income channel accounts for 3.27% (0.00075/0.023) of the total effect. Column (5) considers both the SEZ dummy and GDP per capita as predictors, and shows that the inclusion of GDP per capita has only a small impact on the estimated effect of SEZs (0.022 in Column (5) versus 0.023 in Column (1)). The regression in Column (5) may suffer from the problem of endogeneity, as SEZs can affect GDP. In Column (6), we used the residuals from Column (4) as the dependent variable.[15] In other words, we excluded all the determinants of high school enrollment rates that can be explained by GDP per capita; therefore, the coefficient in Column (6) reflects the influence of SEZs via channels other than income. The point estimation, 0.022, in Column (6) is the same as that in Column (5) at the fourth digit.

Overall, the income channel may explain a small proportion of the SEZ effect, if any, leaving a large amount of room for other mechanisms. This result is consistent with some earlier studies that show an insignificant effect of the income channel, such as Adukia, Asher, and Novosad (2020). In our case, the small effect from the income channel may be explained by the fact that SEZs are not likely to be located in very poor areas, and income is not likely to be a binding constraint in obtaining high school education in the areas where SEZs are located.

TABLE 7—MECHANISM I: THE INCOME CHANNEL

	High school enrollment rate	log(GDP per capita)	log(Educational expenditure per capita)	High school enrollment rate	High school enrollment rate	Residual from (4)
	(1)	(2)	(3)	(4)	(5)	(6)
SEZ	0.0227	0.1877	0.0095		0.0220	0.0220
	(0.0078)	(0.0700)	(0.0284)		(0.0078)	(0.0078)
log(GDP per capita)				0.0041	0.0041	
				(0.0015)	(0.0015)	
N	23,468	23,468	23,468	23,468	23,468	23,468
R^2	0.769	0.806	0.890	0.770	0.770	0.000

Notes: This table contains the county-level data from 1998 to 2007. Control variables include county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level.

B. *The Job Opportunity Channel*

As discussed in the theoretical framework, introducing an SEZ may affect local cohort schooling by expanding employment opportunities. If some job opportunities offered by SEZs require only middle school education or below, the increased job opportunities will raise the opportunity cost of schooling for youths, thus leading to a reduction in local educational attainment. Meanwhile, if job opportunities require more skilled labor, an SEZ would increase the chance of finding a job for a high school graduate or above, thereby encouraging educational investment.

[15] Our approach differs from the two-step approach, which takes the residual of one predictor and uses it as a predictor in the second step.

To investigate the job opportunity channel, we examined whether the effect of hosting an SEZ on local schooling differs across the levels of education required by the new jobs or the existing jobs. We constructed two geocoded datasets for this analysis. One is a geocoded dataset for SEZs that contains the exact boundaries for each zone, based on the information from the "Bulletin List for the Official Boundaries of Chinese Industrial Parks." The other is the geocoded industrial firm dataset. The firm-level data are obtained from ASIFs conducted by the National Bureau of Statistics of China between 1998 and 2007. We geocoded firms using their address information from the ASIFs and identified those located within SEZs. From the ASIFs, we obtained information regarding the size of the labor force employed by firms at the end of the year and calculated the number of new employees by subtracting the employment size in year t from year t–1. After aggregating the firm-level data, we obtained the number of new employees and the total number of employees both inside and outside SEZs at the county level. By dividing the population size of each county, we calculated the per capita figures both inside and outside SEZs.

To obtain information on employment at different levels of education, we exploited the information from the 2004 National Economic Census, which provides firm-level data on labor across educational backgrounds. According to this census, employees can be classified into three groups in terms of their educational background: college degree or above, high school education, and middle school or below. We calculated the shares of these three groups for 523 four-digit manufacturing sectors. Then, we multiplied these shares by the number of new or total employees in firms by matching the four-digit manufacturing sectors. By aggregating those numbers across firms, we obtained the number of new or total employees at different education levels within and outside SEZs for each county. Per capita data are the numbers divided by the population size.

To clarify the proposed procedure, the shares of education levels were calculated at the national level. As long as a firm belongs to a specific four-digit sector, the educational distribution of its employees is considered as fixed at the average sectoral level in the year 2004 regardless of whether the firm is more high-tech than the average or recruits more educated employees in any year. The disadvantage of this procedure is inaccuracy—the calculated employment opportunities of the different education levels may not reflect the reality. However, inaccuracy is associated with a substantial advantage in terms of reducing endogeneity. If the local market has more job seekers with high school education, firms may recruit a larger number of high-skilled employees. The calculated data blocks this channel of reverse causality. Figure 5 shows the changes in the shares of employees with different education levels inside and outside SEZs, respectively. Overall, SEZs generate a reduction in the share of employees with middle school education or below and an increase in the share of employees with high school education. Furthermore, the changes inside SEZs are more sizable compared to those outside SEZs.

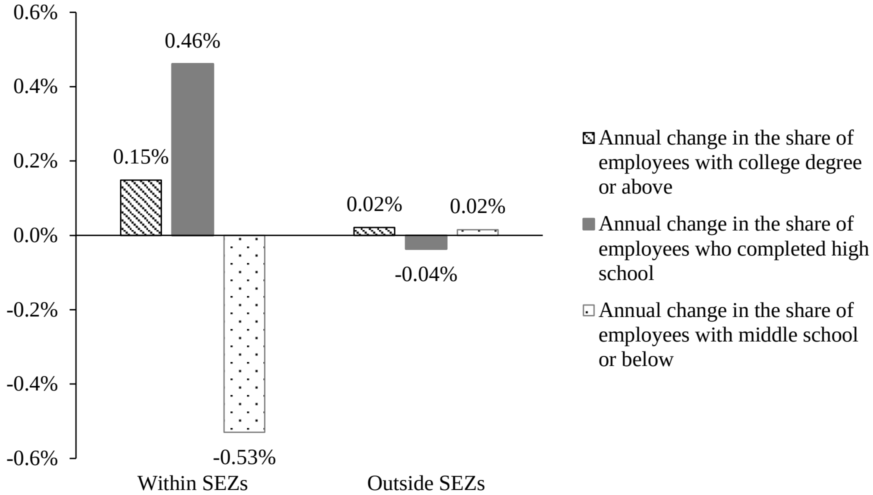


FIGURE 5. THE AVERAGE OF ANNUAL CHANGE IN THE SHARE OF LABORS WITH DIFFERENT EDUCATION LEVELS

Table 8 reports the results of the job opportunity channel in explaining the effect of SEZs on local educational attainment. Because the labor market recruitment may respond to labor supply, to further mitigate the possible reverse causality issue, the outcome variable is at year *t* while the key predicting variables are all at year *t*–1; that is, all the numbers of new or existing employees occur in the year before a cohort makes a decision about high school enrollment. Column (1) estimates Equation (1) but replaces the SEZ dummy with four county-level job opportunities variables: new employees per capita in SEZs, total employees per capita in SEZs, new employees per capita outside SEZs, and total employees per capita outside SEZs. The number of new employees per capita in SEZs has a positive and significant effect (at the 5% level). The coefficient suggests that one job opportunity in SEZs for every 100 persons will increase high school enrollment rate by 0.0243 at the county level. The effect size of the other three variables is small, and none of the coefficients are statistically significant at the conventional 5% level.

To decompose the job opportunity channel, Column (2) of Table 8 replaces the number of employees per capita with the corresponding numbers for the three levels of education. The results are consistent with theoretical predictions: more job opportunities requiring high school education in SEZs increase high school enrollment rates; conversely, more job opportunities requiring middle school education or below are associated with a lower high school enrollment rate. The coefficient for the number of new employees with college degree or above inside SEZs is found to be negative but not statistically significant.

As a whole, the number of total employees per capita in SEZs (Column (1) of Table 8) does not have a significant correlation with the high school enrollment rate. However, the stocks of employees at different education levels (Column (2) of Table 8) show strong differential impacts. The variables for the total number of employees are expected to capture the "demonstration effect" among middle school students—students may form expectations based on employees who are already working in those firms. The results confirm the existence of such an effect: observing that high school graduates have better job opportunities in SEZs encourages students to enroll in high school.[16]

New job opportunities or the stocks of employees outside SEZs do not show any significant effect, regardless of whether these measures are combined or separated for different education levels. One explanation for the insignificance of these effects is that the establishment of new firms or the closure of existing firms outside SEZs is not as frequent as inside SEZs. Thus, the within-county variation outside SEZs is small. Another possibility is that wages outside SEZs are not attractive; the next subsection explores the channel of wage premium.

TABLE 8—MECHANISM II: THE JOB OPPORTUNITY CHANNEL

	High school enrollment rate	
	(1)	(2)
New employees per capita in SEZs	2.4292	
	(1.1021)	
Total employees per capita in SEZs	1.2450	
	(0.8100)	
New employees per capita outside SEZs	-0.0428	
	(0.0789)	
Total employees per capita outside SEZs	0.0470	
	(0.0750)	
New employees with college degree or above per capita in SEZs		-2.4242
		(6.0784)
New employees who completed high school per capita in SEZs		17.8284
		(1.0324)
New employees with middle school or below per capita in SEZs		-11.4233
		(1.2952)
Total employees with college degree or above per capita in SEZs		-21.7187
		(7.0415)
Total employees who competed high school per capita in SEZs		9.3766
		(1.4603)
Total employees with middle school or below per capita in SEZs		-0.6889
		(1.3699)
New employees with college degree or above per capita outside SEZs		-0.1276
		(2.0083)
New employees who completed high school per capita outside SEZs		-0.6744
		(1.2267)
New employees with middle school or below per capita outside SEZs		0.4225
		(0.6877)
Total employees with college degree or above per capita outside SEZs		0.8515
		(3.2196)
Total employees who competed high school per capita outside SEZs		0.3882
		(1.7180)
Total employees with middle school or below per capita outside SEZs		-0.4312
		(0.8267)
N	15,290	15,290
R^2	0.817	0.833

Notes: This table contains the county-level data from 1998 to 2007. Other control variables include log of GDP per capita and educational expenditure per capita, county fixed effects, county-time trends, province-year fixed effects, poor-county×year

[16] The negative coefficients for employees with college degree or above inside SEZs are difficult to interpret. One possibility is that if a firm recruits more individuals with college degree or above, job characteristics and wage premiums might be less favorable for high school graduates.

fixed effects, and port-county×year fixed effects. Predictors reported in the table are at time t–1. Standard errors in parentheses are clustered at the county level.

C. *The Wage Premium Channel*

The positive effect of SEZs on local schooling can also be explained by the higher wages associated with longer schooling. Table 9 tests whether firms within SEZs offer higher wages, using data from the ASIFs. In the regression model, the dependent variable is the logarithm of average wage at the firm-year level. The key predictor, SEZ, is equal to 1 if the firm is located inside an SEZ, and 0 otherwise. Other controls include variables for whether the firm is a state-owned enterprise, whether the firm is an FDI, the logarithm of employment size, and the firm's age. In addition, the county fixed effects, province-year dummies, and four-digit-industry fixed effects are also controlled for. The coefficient of SEZ (Column (1) of Table 9) indicates that the wage in firms within SEZs is, on average, 7.8% ($e^{0.075}$ – 1) higher than in firms outside SEZs.

Column (2) of Table 9 reports the estimates of the wage premium across education levels. We replaced the SEZ dummy with five variables for the share of each education level both inside and outside SEZs (employees with college degree or above in SEZs, employees who completed high school in SEZs, employees with middle school education or below in SEZs, employees with college degree or above outside SEZs, and employees who completed high school outside SEZs). The reference group is the share of those who completed middle school or below outside SEZs. The coefficients increase monotonically as the levels of education increase both inside and outside SEZs, suggesting the existence of a wage premium for longer education. In addition, the comparison between the coefficients for the same education level shows the existence of a wage premium for working inside SEZs. Figure 6 plots the average wage premium for each education level. The wage premium is calculated as the ratio between the exponential values of the estimated coefficients within SEZs and those outside SEZs. For example, the wage premium for employees with college degree or above within SEZs is calculated as 7.6% ($e^{1.0439}/e^{0.9795}$ – 1), in which 1.0439 and 0.9705 are the within-SEZs and outside-SEZs estimated coefficients, respectively. Similarly, the wage premium associated with working inside SEZs is 12.7% ($e^{0.4309}/e^{0.3117}$ – 1) for employees with high school education and 4.3% ($e^{0.0418}$ – 1) for those with middle school education or below. The wage premium for high school education is much higher than that for middle school education or below.

TABLE 9—WAGE PREMIUM OF WORKING IN SEZS

	log(wage)	
	(1)	(2)
SEZ	0.0753 (0.0132)	
Share of employees with college degree or above in SEZs		1.0439 (0.1070)
Share of employees who completed high school in SEZs		0.4309 (0.0810)
Share of employees with middle school or below in SEZs		0.0418

		(0.0326)
Share of employees with college degree or above outside SEZs		0.9705
		(0.0427)
Share of employees who completed high school outside SEZs		0.3117
		(0.0354)
log(*Employment*)	-0.0160	-0.0162
	(0.0023)	(0.0023)
SOE	-0.0480	-0.0531
	(0.0067)	(0.0065)
FDI	0.2615	0.2635
	(0.0149)	(0.0145)
Age	-0.0000	-0.0000
	(0.0000)	(0.0000)
Constant	9.2916	9.0799
	(0.0129)	(0.0161)
County fixed effects	yes	yes
Industry fixed effects	yes	yes
Province×year fixed effects	yes	yes
N	1,321,440	1,321,440
R^2	0.462	0.466

Notes: This table contains the firm-level data from 1998 to 2007. Standard errors in parentheses are clustered at the county level.

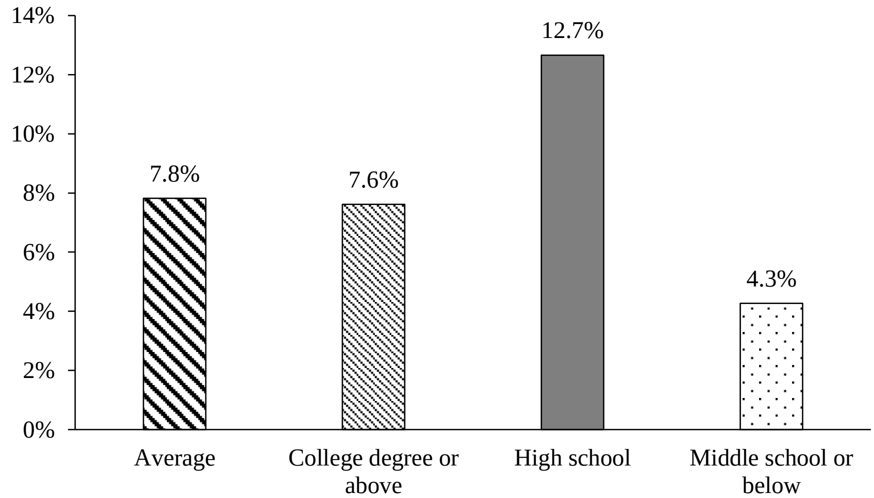


FIGURE 6. WAGE PREMIUM BY EDUCATION LEVEL

Next, we formally examined whether the return to schooling plays a substantial role in explaining the impact of SEZs on educational investment. To calculate county-specific wage premiums, we estimated specifications similar to those in Table 9 and calculated them separately for each county that launched SEZs between 1998 and 2007.[17] We avoided calculating wage premiums at the county-year level due to the volatility of data, so the calculated wage premium is fixed for each county and does not vary across years. As the estimated county-level wage premiums are somewhat messy, we further generated a set of variables on "high wage premium," which are assigned the value of 1 if the SEZ's wage premium is higher than the median value, and 0 otherwise. Counties without SEZs are assigned

[17] The estimated wage premium is a lower bound of the impact of SEZs on local wages because the labor market is not fully isolated and firms inside SEZs may raise the overall wage level in the area.

0 in these variables. Table 10 presents the regression results by adding the interactions of SEZs and high wage premium dummies to Equation (1). [18] In Column (1), we considered the wage premium dummy without differentiating across education levels. The coefficient of the interaction suggests that the launch of an SEZ can increase high school enrollment rate by 4.41 percentage points if SEZs raise average wage premiums from below median to above median, demonstrating a strong wage premium channel. The coefficient of SEZs is positive, but very small and without statistical significance, suggesting that SEZs have minimal effect in counties with a wage premium below the median. The regression in Column (2) decomposes the interaction in Column (1) into three interactions for each education level. Because the interactions related to college and middle school have negative but small coefficients, the wage premium that SEZs offer employees who completed high school is responsible for the encouraging effect.

TABLE 10—MECHANISM III: THE WAGE PREMIUM CHANNE (1)

	High school enrollment rate	
	(1)	(2)
SEZ×High wage premium of SEZs	0.0441	
	(0.0215)	
SEZ	0.0070	0.0082
	(0.0128)	(0.0298)
SEZ×High wage premium for college degree or above		-0.0009
		(0.0116)
SEZ×High wage premium for high school education		0.0592
		(0.0294)
SEZ×High wage premium for middle school or below		-0.0218
		(0.0272)
N	14,613	14,613
R^2	0.796	0.797

Notes: This table contains the county-level data from 1998 to 2007. Other control variables include log of GDP per capita and educational expenditure per capita, county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level.

Table 11 deepens the analysis by replacing the indicator on SEZs in Table 10 with two other indicators—the numbers of new employees and the number of total employees hired in SEZs. In Column (1), the coefficient of the interaction term between new SEZ job opportunities and the high wage premium dummy is positive and significant at the 5% level. The interaction term between the total employees and the wage premium dummy is also positive, but has a much smaller magnitude. Column (2) presents the results of the interactions between job opportunities and wage premium for each education level. The wage premium for high school education inside SEZs makes the corresponding new job opportunities more attractive and increases high school enrollment rates. The interaction with total employment size is also found to have a positive but insignificant effect. However, if an SEZ offers a high wage premium for middle school education or below, the new job opportunities within the SEZ exert a negative impact on high school enrollment rates. Overall, the results in Table 11 support the wage

[18] Because the wage premium is fixed at the county level, we did not include the variable on wage premium, which is collinear with the county dummies.

premium channel: a high wage premium for high school education improves the SEZ effect on high school enrollment rates, while a high wage premium for middle school education or below weakens the SEZ effect.

TABLE 11—MECHANIMS III: THE WAGE PREMIUM CHANNEL (2)

	High school enrollment rate	
	(1)	(2)
New employees per capita in SEZs	7.7400	
×High wage premium	(1.1287)	
Total employees per capita in SEZs	2.4902	
×High wage premium	(1.7743)	
New employees per capita in SEZs		0.1022
×High wage premium for college degree or above		(1.5652)
New employees per capita in SEZs		6.8776
×High wage premium for high school education		(1.4940)
New employees per capita in SEZs		-3.3925
×High wage premium for middle school or below		(1.3522)
Total employees per capita in SEZs		2.0342
×High wage premium for college degree or above		(1.6814)
Total employees per capita in SEZs		2.5008
×High wage premium for high school education		(1.7308)
Total employees per capita in SEZs		-2.0613
×High wage premium for middle school or below		(1.6882)
New employees per capita in SEZs	0.1721	3.5138
	(0.6339)	(1.3375)
Total employees per capita in SEZs	0.0707	1.5830
	(0.7171)	(1.7496)
New employees per capita outside SEZs	-0.0278	-0.0169
	(0.0480)	(0.0506)
Total employees per capita outside SEZs	0.0888	0.0649
	(0.0667)	(0.0683)
N	10,988	10,988
R^2	0.825	0.827

Notes: This table contains the county-level data from 1998 to 2007. Other control variables include log of GDP per capita and educational expenditure per capita, county fixed effects, county-time dummies, province-year fixed effects, poor-county×year fixed effects, and port-county×year fixed effects. Standard errors in parentheses are clustered at the county level.

V. Conclusion

As a widespread and economically important set of policies, SEZs have been spreading rapidly around the world. Previous studies have investigated whether SEZs can generate substantial economic gains in spite of certain resource distortions. However, the evidence in terms of the impact of SEZs on educational attainment is mixed because SEZs differ in skill requirements. This study complements the existing literature on the educational impacts of SEZs by exploiting a variety of SEZs in China over a span of 30 years.

Using county-level data between 1980 and 2009 in a two-way fixed effects estimation with heterogenous treatment effects, our DID_{M} estimators show that SEZs had a positive impact on educational investment over the sample period. For every 1,000 students, an additional 31 were encouraged to enroll in high school if an SEZ existed in their local area when they finished middle school, at approximately 15 years old. The pre-treatment trend provides support for the DID_M identification, and the post-treatment trend reveals a consistent pattern of the effect. We also used other ages as the exposure age, but the estimated effect is the largest at age 15. The pre-treatment trend, combined with the fact that the largest effect is estimated for age 15, demonstrates the reliability of the proposed regression specification. An IV approach provides further support for the positive educational effect of SEZs.

Besides the overall positive effect of SEZs on educational investment, different SEZs are found to have opposite effects: technology-oriented zones encourage high school enrollment, while export-led zones discourage it. Such findings help us reconcile mixed evidence of how SEZs affect local educational attainment in the existing literature. Further analysis shows that both job opportunities and wage premiums help explain these varying effects. The introduction of SEZs increases job opportunities and wage premiums at all education levels. More job opportunities and higher wage premiums for high school education encourage high school enrollment, while those for middle school education or below discourages it. The overall effect of SEZs on education depends on the relative magnitude of the job opportunity channel and the wage premium channel.

This analysis is the first study to systematically explore the impact of SEZs on human capital development in China. A positive average effect of SEZs suggests that SEZs may generate long-run economic gains from the perspective of human capital investment. The negative impact due to job opportunity and wage premium for middle school education or below, especially in export-led SEZs, calls for caution. As countermeasures, the government may consider offering financial support to students enrolling in high schools, or increasing the costs for firms recruiting fresh middle school graduates.

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