UNIVERSITY OF OTTAWA

DOCTORAL THESIS

Essays on Development Economics in China

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Declaration of Authorship

I, Pu SUN, declare that this thesis titled, “Essays on Development Economics in China” and the work presented in it are my own. Nonetheless, I appreciate the insights and suggestions from professors at the University of Ottawa and attendees at multiple conferences. I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this University.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Signed: Pu Sun

Date: April 25, 2025

“民惟邦本，本固邦宁。”

— 《尚书》

“*The people are the foundation of the nation; when the foundation is secure, the nation will be at peace.*”

— *Book of Documents*, complied by Confucius

Abstract

The rapid rise of China in recent decades presents a unique and attractive context for contributing and testing theories in development economics. Beneath the remarkable growth, there seems to be a “helping hand” from the government to direct and facilitate the development. In my doctoral thesis, I employ rigorous applied econometrics methods to investigate topics in development economics from two major events: the Wenchuan earthquake in 2008 and the implementation of the Targeted Poverty Alleviation program in 2015.

Chapters 1 and 2 both study the Wenchuan earthquake in 2008, one of the most destructive earthquakes in modern China. In Chapter 1, I use county-level panel data from 2003 to 2019 to reveal the variation in the earthquake effect over time, as the largest economic setback takes place in 2008. However, the economic recovery in the following years drives severely affected counties to catch up with non-severely affected counties at the end of the sample period. I also find that the average effect of the Wenchuan earthquake causes a decline of 12% in GDP per capita for those severely affected counties, despite that the method tends to ignore the post-earthquake recovery. Consequently, the economic suffering from the earthquake is considered as short-term.

In Chapter 2, I use the exogenous exposure to the 2008 Wenchuan earthquake and the age of students at the time of the disaster to estimate its impact on educational attainment. The findings reveal that students younger than 15 years old in severely affected counties experience a reduction of 0.36 years in schooling, equivalent to a 3.7% decline. I propose one mechanism to explain this outcome: the protective role of compulsory education, which helps prevent early dropouts but ends after junior high school. In summary, this analysis provides insights into how natural disasters can disrupt human capital formation of young students.

Chapter 3 shifts the focus to the government-led Targeted Poverty Alleviation program by evaluating its impact on urban-rural income inequality in China. Income inequality is defined as the ratio of the urban net income to the rural net income, both in per capita form. The main finding suggests that the program leads to a 3.8% reduction in the urban-rural income ratio in poverty counties, compared with non-poverty counties, which implies a discernible convergence in income levels. I discuss that the significant increase in rural income per capita, improved rural employment, and more government spending largely contribute to the narrowing urban-rural inequality in poverty counties.

Keywords: natural disaster, economic development, educational attainment, poverty alleviation, urban-rural income inequality

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Dedicated to my parents, SUN Changchun and XIN Qiuping, who have been giving me freedom and support to pursue what I desire.

Chapter 1

Still in Pain? The Economic Suffering from the Wenchuan Earthquake in China

1.1 Introduction

Natural disasters have become recurring events in human history, such as earthquakes, hurricanes, and wildfires. In recent years, understanding their consequences from different perspectives are flourishing due to their close attainment to daily life as well as the development of causal inference methods (Deschênes and Greenstone, 2007; Cavallo et al., 2011). Even though, the opinions still remain divided regrading whether a subsequent recovery will be expected after one natural disaster. Some catastrophes have led to prolonged and widespread negative economic impacts, possibly due to the large destruction of infrastructures and buildings (Krichene et al., 2021; Cavallo et al., 2013; Joseph, 2022). On the other hand, some studies argue that the destruction has been temporary and has even stimulated economic recovery after severe disasters (Strobl, 2011; Dell et al., 2012; Deryugina et al., 2018). Following this path, this study investigates the long-term pattern of a natural disaster and the mechanisms at play in this paper.

Specifically, I focus on the 2008 Wenchuan earthquake in Sichuan province, China, which presents as the most destructive disaster the country has suffered in the 21st century.[1] The Wenchuan earthquake registered a magnitude of 8.0 on the Richter scale.

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[1] Wenchuan is the name of county where the earthquake epicentre is. See more in Background.

In the aftermath, governmental bureaus collaborated to assess the severity of damage across this area. After evaluation, counties in Sichuan are classified as severely affected counties ("severe counties" hereafter) and non-severely affected counties ("non-severe counties" hereafter) based on their exogenous exposures to the earthquake. This classification yields two balanced types of county as they are similar in various aspects during the pre-earthquake period. Thus, the main research question in this paper attempts to understand the long-term effect of the earthquake to severe counties, compared with non-severe counties.

There are at least two advantages to choosing the Wenchuan earthquake as the setting. First, although historical records show that China is earthquake-prone (He et al., 2021), significant earthquakes occur only in limited regions,[2] while the rest rarely experience earthquakes. This geographic variation provides a natural "never treated" control group, which could help establish causal relations regarding earthquake effects. Second, destructive earthquakes are not frequent in Sichuan either. In the three decades before 2008, only two earthquakes occurred in the province, one in 1981 with 150 deaths and another in 2001 with 3 deaths. Both are far less deadly than the Wenchuan earthquake, which claimed nearly 90,000 lives.[3] The accidental natural disaster minimizes any anticipation effects in Sichuan and also makes the Wenchuan earthquake an exogenous shock for analysis.

I use a dynamic Difference-in-Differences (DID) method to estimate the earthquake effect on economic performance. The empirical analysis first suggests that severe and non-severe counties display pre-trends in GDP per capita, and then the largest contraction in severe counties occurs in the earthquake year of 2008. The next several years see a gradual attenuation to the effect, which implies a sign of economic recovery in severe counties. Therefore, the dynamic analysis proves that the Wenchuan earthquake only has a short-term impact on economic performance. Besides, I also use the traditional DID method to examine the average earthquake effect. After the earthquake, severe counties suffer from a 12% decline in GDP per capita relative to non-severe counties. However, it should be reminded that the average estimation does not fully describe the

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[2] Southwestern region (such as parts of Sichuan province) and the eastern coastal region (some parts of Guangdong province) belong to this category. See from `https://www.gov.cn/ztzl/prdz/content_635516.htm`.

[3] See from `https://en.wikipedia.org/wiki/List_of_earthquakes_in_China`.

trajectory of the post-earthquake period.

The robustness and credibility of the main findings come from the inherent exogeneity of earthquakes, as their occurrence cannot be accurately predicted or influenced by human activity. However, a potential treat to identification still arises. For example, the official evaluation of earthquake exposure may consider endogenous factors to the economic outcome when deciding the severity status; or the designation of treatment has the risk of manipulation. If so, the estimation in the baseline specification would deviate from the real effect. To address this concern, I conduct several robustness checks, such as using alternative outcome and the treatment variable, spillover effect test, and placebo test. In general, the results from those checks could confirm the consistency of the main findings.

I explore the potential mechanisms that explain the diminishing earthquake effect and economic recovery in severe counties. First, the analysis indicates that the Wenchuan earthquake has minimal impact on the employment structure in severe counties, particularly within the rural and industrial sector. This stability in employment composition preserves local labourers and facilitates future recovery once the short-term pain is over. Next, fixed-asset investment in these counties display a surge shortly after the earthquake, supporting reconstruction efforts in Sichuan at the same period. Though it eventually reverts to levels comparable to non-severe counties, this temporary investment spike still suggests that asset renovation likely plays an important role in the recovery process. Last, the earthquake leads to an immediate increase in government spending. Like fixed-asset investment, this fiscal expansion in severe counties allows for extensive reconstruction efforts to repair affected areas. Besides, this fiscal pressure proves temporary as spending levels have normalized within a few years.

My study contributes to the existing literature in several key aspects. First, it directly adds to the body of research on the Wenchuan earthquake by providing a county-level evaluation of its economic consequences and portraying the post-disaster development trajectory. Previous studies on the Wenchuan earthquake primarily focus on the individual or micro-level analyses, such as behavioural changes (Deng et al., 2015; Filipski et al., 2019; Yao et al., 2019; Yin et al., 2022), disruptions in education (Liu and Xu, 2021; Lu et al., 2023b; Park et al., 2025), firm aids and exports (Bulte et al., 2018; Li et al., 2024), and government subsidy (Park and Wang, 2017). Only few studies examine this earthquake from a more "aggregate" perspective, such as the simulation of recovery through high-way transportation (Wu and Ishiro, 2024), the role of public expenditures in economic growth (Yao et al., 2021), and post-disaster technological progress (Deng et al., 2022). By comparison, my study differs from theirs in exploring the earthquake-induced economic dynamics at the county level. Exploiting exogenous variation in earthquake exposure, I demonstrate how differences in disaster severity shape long-term economic outcomes. The findings indicate that while severe counties suffered substantial economic losses in the short term, they ultimately recovered to pre-earthquake levels by the end of the study period. This evidence supports the notion that affected regions can achieve long-term economic recovery, enriching the understanding of the earthquake's overall impacts.

Second, the results contribute to the broader literature of post-disaster evaluations by offering novel empirical insights into the ambiguous relation between natural disasters and economic recovery. The evolution of causal analysis enables scholars to study the economic impacts of natural disasters with greater credibility, such as earthquakes (Barone and Mocetti, 2014; Pathak and Schündeln, 2022; Hanaoka et al., 2018), hurricanes (Boustan et al., 2012; Strobl, 2011; Gröger and Zylberberg, 2016; Mahajan and Yang, 2020), wildfires (Ho et al., 2023; Boomhower et al., 2023; Wang and Lewis, 2024), floodings (Boustan et al., 2012; Gallagher and Hartley, 2017; Kucuk and Ulubasoglu, 2024), and droughts (Jia, 2014; Fernández et al., 2023). However, the literature has not reached a census whether a post-disaster recovery is achievable. Some suggest that capital accumulation and increased labour force participation can drive recovery (Burton and Hicks, 2005; Hallegatte and Dumas, 2009; Noy, 2009; Noy and Vu, 2010), whereas other studies argue that earthquakes can deepen economic vulnerabilities, particularly in developing countries where recovery is often uncertain (Skidmore and Toya, 2002; Kahn, 2005; Barone and Mocetti, 2014; Joseph, 2022). Through the vigorous empirical investigation in this study, the findings support the recovery hypothesis at the county level after the earthquake.

Besides, the topic of this study is closely aligned with the role of external shocks in shaping long-term economic development. This strand of literature typically examines how unexpected and exogenous historical events disrupt established economic trajectories. For instance, the introduction of New World crops centuries ago led to population growth in China (Chen and Kung, 2016), Europe (Nunn and Qian, 2011), and Africa (Cherniwchan and Moreno-Cruz, 2019). Research on American military presence during wartime explores its effects on convergence of economic activities (Davis and Weinstein, 2002; Miguel and Roland, 2011; Brodeur et al., 2018). In this study, I also analyze the disruption of the Wenchuan earthquake on the pre-existing growth pattern, using non-severe counties as the convincing control group.

The remaining part of this paper is organized as follows: Section 1.2 introduces the historical background of the Wenchuan earthquake and government reactions afterwards and the conceptual framework describing potential paths after a natural disaster. Section 1.3 describes the data set for the empirical analysis that will be explained in detail in Section 1.4. Section 1.5 presents empirical evidence of the Wenchuan earthquake on economic performance along with robustness checks. Section 1.6 discusses some potential channels for the earthquake effect to pass on affected counties. And Section 1.7 concludes the study.

1.2 Background

1.2.1 The Wenchuan Earthquake and Government Response

Around 2:28 pm on May 12, 2008, a catastrophic earthquake struck in Sichuan, a southwestern province in China. This seismic event, known as the Wenchuan earthquake, proved to be one of the deadliest earthquakes in recent Chinese history.[4] China Earthquake Administrative promptly located the epicentre in Yinxiu Town, Wenchuan County, Sichuan Province (31° N, 103.4° E), registering a magnitude of 8.0 on the Richter scale.[5] Figure A1 displays the Wenchuan earthquake epicentre in China. Although the epicentre was situated in Sichuan Province, the seismic waves reverberated across a significant portion of China, leading to substantial impacts on neighbouring provinces as well.

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[4] Before the Wenchuan earthquake, the most destructive was the Tangshang earthquake in 1976.

[5] See from `https://www.cea.gov.cn/cea/dzpd/zqsd/2815725/index.html`

Despite ongoing aftershocks, the then-Prime Minister promptly flew from Beijing to Sichuan and led the rescue efforts just two hours after the earthquake struck. On the same day, the Sichuan Provincial Committee of the Communist Party of China and the Sichuan Provincial Government approved the initial rescue plan and established the earthquake relief headquarters, with support pledged from other provinces sending troops and medical teams to assist. International condolences poured in, with some countries dispatching medical personnel to aid in the rescue efforts.[6]

Although the government's response to the sudden disaster was prompt, the earthquake resulted in significant losses.[7] Approximately 70,000 people lost their lives during the earthquake, with an additional 18,000 reported missing. The destruction was widespread, with around 20 million square meters of urban residential areas and approximately 7,000 school buildings reduced to rubble (Compilation Committee of the Sichuan Earthquake Relief Chronicle of the Wenchuan Earthquake, 2018). By September 2008, the central government estimated that the financial toll, such as the destruction of houses, buildings, roads, and bridges, reached 845 billion RMB ($\approx$ 122 billion USD), a similar level as Hurricane Katrina caused (Deryugina et al., 2018).[8]

Following the three-week rescue operation, the focus shifted to rebuilding the destroyed areas. Over 40,000 projects were initiated, covering various sectors including housing, infrastructure, healthcare, education, and industry. The funds allocated for rehabilitation and reconstruction in Sichuan amounted to 1.7 trillion RMB ($\approx$ 245 billion USD), exceeding the provincial GDP in 2008 by 35%. From 2008 to 2010, the Ministry of Finance provided substantial subsidies to Sichuan, totalling 6.45 billion RMB ($\approx$ 1 billion USD), equivalent to 4.6% of the provincial fiscal revenues in 2008. Severe counties also received assistance from partner provinces outside of Sichuan for three years, focusing on rebuilding residential houses, public services, and infrastructure. Each partner province committed to providing support equivalent to at least 1% of their provincial fiscal revenues annually.[9] By the end of 2011, the reconstruction efforts were nearing completion, with nearly all national projects and budget investments

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[6] See from `https://www.gov.cn/jrzg/2008-05/23/content_989714.htm`.

[7] See from `http://news.bbc.co.uk/2/hi/asia-pacific/7424262.stm`.

[8] See from `http://www.npc.gov.cn/zgrdw/npc/zt/2008-09/05/content_1448390.htm`.

[9] See from `https://www.gov.cn/zwgk/2008-06/18/content_1019966.htm`.

finalized.[10]

1.2.2 Conceptual Framework

Understanding the long-term economic impact of earthquakes remains an area of ongoing debate among economists due to a variety of development patterns observed in real life. Here I intend to list and summarize the main theoretical claims found in the literature. For simplicity, GDP per capita serves as the primary metric for measuring economic performance or the quality of life in a given region throughout the discussion (Becker et al., 2005; Dell et al., 2012; Barone and Mocetti, 2014). Figure 1.1 illustrates several potential growth patterns for severe counties after the Wenchuan earthquake occurred, assuming GDP per capita (y_{base}) in a representative non-severe county grows at a constant rate of g_{base}. All scenarios depict a discrete drop in GDP per capita in the earthquake year, likely due to capital destruction and/or the loss of effective labour ($y_{after} < y_{base}$ and $g_{after} < 0 < g_{base}$). These trajectories diverge over time with distinct outcomes eventually (Hsiang and Jina, 2014). In general, three scenarios for severe counties could happen after the earthquake: "come back strong", "pain is over", and "still in pain".

The most optimistic scenario is labelled as "come back strong", which underscores that experiencing the earthquake would trigger faster development in severe counties after a short-term drop. In this scenario, the disaster acts as a catalyst, leading to a higher post-earthquake growth rate than the baseline level ($g_{after} > g_{base}$). In the end, severe counties are expected to surpass non-severe counties which are assumed to be the baseline ($y_{after} > y_{base}$). Unlike the concept of "creative destruction," where market forces or social ideologies drive the transformation, the recovery from the earthquake can be facilitated by aid from international and domestic organizations and government subsidies. These sources can provide reliable financial support for reconstruction efforts, particularly in transparent and efficient governments (Gignoux and Menéndez, 2016; Huang and Hosoe, 2017; Heger and Neumayer, 2019). Destroyed equipment is replaced by newer and more advanced ones, thereby enhancing productivity levels (Skidmore and Toya, 2002; Hallegatte and Dumas, 2009; Hornbeck and Keniston,

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[10] See from `https://www.gov.cn/test/2012-02/02/content_2056707.htm`.

2017).

In the "pain is over" scenario, severe counties run into a finite period of accelerated growth after the earthquake. However, unlike the previous scenario, the development eventually converges to the baseline trend over the long term without surpassing it ($y_{after} \approx y_{base}$). This indicates that while severe counties recover from the initial shock, they do not sustain economic growth rates that outpace counterfactual development in the long run. The underlying reasons for this outcome are similar to those discussed in the "come back strong" scenario, particularly during the initial stage of recovery. Reconstruction efforts and an influx of external aid and government subsidies facilitate economic recovery. However, achieving a higher post-disaster growth rate than the baseline trend is not guaranteed, as it depends on various factors such as the effectiveness of reconstruction efforts and the inherent resilience of the severe counties. This scenario aligns with the neoclassical growth theory (Solow, 1956), which posits that economies tend to converge towards a new steady state over time due to diminishing returns on capital. In this context, although the reconstruction process helps severe counties regain their economic footing, the long-term growth rate stabilizes at the pre-disaster level, reflecting the natural tendency of economies to return to a steady state.

In the most pessimistic scenario, severe counties fail to achieve a growth rate higher than the baseline level, as evidenced by $g_{after} < g_{base}$. Moreover, the initial decline in GDP per capita persists in the long run, indicating a lack of economic recovery, as expressed by $y_{after} < y_{base}$. In the end, the earthquake leaves a permanent scar on the development of severe counties. Barone and Mocetti (2014) have found that cities with a weaker institutional quality struggle to recover to their pre-disaster levels following earthquakes. This lack of recovery may be attributed to factors such as resource misallocation, corruption, and elite capture, which hinder effective post-disaster reconstruction efforts. Cross-country comparisons also suggest that developing countries are more susceptible to this scenario compared to developed nations (Kahn, 2005; Strobl, 2011; Loayza et al., 2012; Joseph, 2022). Effective governance typically helps avoid this scenario. At the individual level, residents of affected regions may alter their consumption behaviour by prioritizing immediate needs over long-term savings (Sawada and Shimizutani, 2008; Yao et al., 2019; Yin et al., 2022). This shift in preferences reduces the rate of capital accumulation and further contributes to the prolonged economic downturn in the aftermath of the disaster.

1.3 Data

I exploit different data sources in this study to conduct an empirical analysis. The primary data source focuses on the aftermath of the Wenchuan Earthquake in 2008, drawing detailed information from *The Sichuan's Relief Chronicle of the Great Wenchuan Earthquake* published 10 years after the disaster.[11] This series documents a wide range of critical aspects about the Wenchuan earthquake, including damage evaluation, disaster relief, epidemic prevention, and reconstruction.

Between June and July 2008, the Sichuan Provincial Government collaborated with the National Wenchuan Earthquake Expert Committee, the China Earthquake Administration, the National Bureau of Statistics, and other ministries from the State Council to assess the severity of damage across various counties in the affected region.[12] Then they constructed a disaster index, considering factors such as average earthquake intensity (30%), deaths and missing persons (30%), number of collapsed houses (20%), geological disaster risk (10%), and resettlement per ten thousand people (10%).[13] Counties then were categorized into four groups based on the index: the most severe county (the index is above 0.4), severe county (between 0.15 to 0.4), less severe county (between 0.01 to 0.15), and least affected county (less than 0.01). In summary, Sichuan has 10 most severe counties and 29 severe counties out of 180.[14] Figure 1.2 presents their geographic locations along with the epicentre. Those severely affected counties are located around the epicentre in relatively short distances. With the data, I match the distribution of severe counties, earthquake intensity scale,[15] and earthquake-related deaths at the county level. Besides, the distribution of deaths is uneven among counties and

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[11] The copyright belongs to the Local Chronicles Office in Sichuan province.

[12] See from `https://www.gov.cn/zwgk/2008-09/23/content_1103686.htm`.

[13] Resettlement aims to evacuate residents from potentially dangerous areas and provide a temporary residence for families whose houses were destroyed. Usually, resettlement occurs within the same county.

[14] Outside Sichuan Province, there are 12 more severe counties in neighbouring provinces.

[15] China adopts its own standard to measure earthquake intensity, known as China Seismic Intensity Scale. This standard includes 12 scales with a higher number representing a stronger intensity. The data on the county-level earthquake intensity is obtained from the China Earthquake Administration. See from `https://www.gov.cn/wszb/zhibo262/content_1085953.htm`.

positively related to the severity of the earthquake. According to the statistics, 97.6% of death tolls and 98.4% of missing persons were concentrated in the 10 most severe counties (Compilation Committee of the Sichuan Earthquake Relief Chronicle of the Wenchuan Earthquake, 2018). In this study, I treat most severe and severe counties together as severe counties (treatment counties) and the rest as non-severe counties (control counties).

The second source is county-level data obtained from the *Sichuan Statistical Yearbook* spanning the years 2003 to 2019. Each year, the Sichuan Bureau of Statistics takes the responsibility of carrying out household surveys in Sichuan and compiling the yearbook under the supervision of the National Bureau of Statistics. The data include indicators about GDP, population, employment, fiscal revenue and expenditure, investment, land area, and so on. Besides, the local CPI during the study period is used for adjusting monetary indicators to the 2007 price level. In this study, I choose the year 2019 as the end date for two reasons. Firstly, the data collection period ended just before the global outbreak of the coronavirus in early 2020. The growth patterns during the COVID-19 pandemic might differ significantly from those in normal circumstances due to quarantine or lockdown policies implemented in China. Secondly, given that this study aims to explore whether the earthquake had a long-term impact, the post-earthquake period of 11 years is already sufficiently long to provide meaningful evidence.

Last, I obtain each county's longitude and latitude from the National Geomatics Center of China to calculate the linear distance to the epicentre. I also match each county with the relief degree of land surface to account for altitude difference in the region from You et al. (2018).[16]

1.4 Identification Strategy

I use a Difference-in-Differences (DID) approach to investigate the impact of the Wenchuan earthquake on the county-level economy in Sichuan province. The earthquake effect defined in this study surfaces during the post-earthquake period through

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[16] Data on China's relief degree of land surface can be accessed at `https://www.geodoi.ac.cn/doi.aspx?Id=887`.

comparisons among counties with different but exogenous degrees of damage. Thus, the key assumptions here are twofold: First, severe counties do not diverge from non-severe counties in the outcome of interest before the earthquake. This is often mentioned as the parallel trend assumption. Second, the locally reported degrees of severity are orthogonal to economic outcomes of interest, which is also the conventional treatment of earthquakes as exogenous in the literature (Barone and Mocetti, 2014; Wang et al., 2017).

Table 1.1 presents a balance check of the statistical characteristics between severe and non-severe counties before the earthquake (2003-2007). The time frame allows for exploring any systematic differences. Columns (1) and (2) provide the mean of the variable accompanied by the standard deviation in the parenthesis. Column (3) record their mean differences conditional on the year fixed fixed, with significance levels included. In panel A, severe counties have a slight advantage in GDP per capita over non-severe counties, but their difference is insignificant. Both types of counties display similar economic sector compositions, population sizes, fiscal conditions, arable lands share, and fixed-asset investment. This implies that the classification of severe and non-severe counties yields to a treatment group and a convincing control group.

I compare their county-fixed characteristics in panel B. On average, severe counties are located approximately 114 km closer to the epicentre than non-severe counties. This proximity suggests a stronger seismic impact, consistent with the expectation that counties closer to the epicentre experience more severe destruction. The significant disparity in earthquake intensity further supports this claim. While 30% of non-severe counties experience an intensity scale of 5 or below, none of severe counties fall into this category. Among counties with an intensity scale above 5 (strong earthquake intensity), the difference in intensity is substantial, nearly 2 scales. Related to this, severe counties have a much higher earthquake-related death rate than non-severe counties.[17] For every ten thousand persons, 131 deaths are reported in severe counties, while about 0.2 deaths in non-severe counties. In the appendix, figure A2 visualizes the correlation between earthquake-related deaths and distance to the epicentre. In summary, the balance check shows that the classification in this study sufficiently captures variation in earthquake exposure while maintaining negligible per-existing economic disparities.

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[17] This indicator combines both reported deaths and missing persons.

I present the event-study model to estimate the yearly effects following the Wenchuan earthquake. It can be expressed as follows by setting one year prior to the earthquake as the reference:

$$
\begin{aligned}
y_{it} = \alpha + \sum_{j=-5}^{-2} \beta_j Severe_i \times Lag_{jt} + \sum_{k=0}^{11} \beta_k Severe_i \times Lead_{kt} \\
+ x'_{it}\gamma + \delta_i + \eta_t + \epsilon_{it} \qquad (1.1)
\end{aligned}
$$

where y_{it} denotes the outcome of interest for county i at the year to the earthquake t; $Severe_i$ is a treatment dummy about earthquake severity which takes 1 only if county i was reported as severe by the local government; Lag_{jt} or $Lead_{kt}$ are lag or lead year dummies which is equal to 1 only if $j = t$ or $k = t$; x'_{it} represents a set of county-level controls; δ_i and η_t absorb county- and year-level fixed effects; ϵ_{it} captures the unobservable random shocks to the outcome variable. Standard errors are clustered at the county level. In table A1, I examine the exogeneity of treatment assignment using the pre-earthquake sample. If the pre-existing economic development could predict the likelihood of being reported as a severe county, the estimation equation presented above might lead to a biased result. However, columns (1)-(3) show that GDP per capita has no significant predictive power in determining treatment status. In columns (4) and (5), I further test whether other pre-existing factors, such as population or investment, may have influenced the classification, but the results again indicate no evidence of endogeneity. In the next section, I also explore an alternative treatment measure as part of robustness checks.

The random nature of earthquake exposure suggests that systematic differences between severe and non-severe counties are unlikely to exist before the disaster. The estimation model can validate this assumption if β_j (for lag dummies) are found to be insignificant. Besides, another advantage of this approach is its ability to estimate the by-year earthquake effects, providing detailed insights into the economic performance of severe counties over time. Statistically, β_k represents the relative difference in outcomes between the two types of county against their difference in 2007.

In addition, I estimate a more standard DID model, using the following equation:

$$
y_{it} = \alpha + \beta Severe_i \times Post_t + x'_{it}\gamma + \delta_i + \eta_t + \epsilon_{it} \qquad (1.2)
$$

where $Post_t$ is a post-earthquake time dummy which is equal to 1 if the year is after the earthquake, and the rest of the settings remain the same. If the results from the event-study model suggest parallel trends in severe and non-severe counties before the earthquake, β measures the average causal effect of the earthquake to severe counties in the outcome of interest. Besides, it is already assumed here that severe counties should receive a homogeneous impact from the earthquake.

1.5 Empirical Evidence on the Earthquake Effect

The main purpose of this study is to evaluate how the Wenchuan earthquake has impacted the county-level economic performance in Sichuan province. To achieve that, I focus on the variation of GDP per capita through the years to estimate the earthquake effect. Before an in-depth exploration of regression results, I conduct a preliminary examination through the depiction of GDP per capita trends in severe and non-severe counties from 2003 to 2019, as illustrated in figure A3. Severe counties maintain their economic advantage until a discrete slump in 2008, with non-severe counties overtaking in GDP per capita. This observation aligns with the statistical description provided earlier. Despite this initial shift, the disparity in economic performance persists over the years, albeit with a gradual narrowing of the gap between them. In the final two years of the study period, severe counties begin to close the economic divide and even surpass non-severe counties in 2019. This trend could be indicative of the “pain is over” or “come back strong” scenario outlined in the conceptual framework, suggesting a period of economic recovery.

In the subsequent paragraphs, I report and interpret the empirical evidence of the earthquake effect first. Next, I adapt several robustness checks to test the consistency of my main finding.

1.5.1 Dynamic Earthquake Effect

In this section, I provide the empirical evidence of dynamic earthquake effects. The results are obtained by estimating the event-study design outlined in Eq. (1.1). The specification integrates the two-way fixed effects (county and year) to absorb variations at the county and year levels, while the standard errors are clustered at the county level. An advantage of this approach is to show the evolution of the earthquake effect from a longer frame. The outcome of interest has been modified as the natural log of GDP per capita (adjusted at the 2007 price level). Since the 1-year lag estimate is omitted due to multicollinearity, the rest can be interpreted as the difference in the outcome between severe and non-severe counties relative to the level in the year just before the earthquake.

Figure 1.3 illustrates the estimated earthquake effects on GDP per capita, along with their 95% confidence intervals. The baseline specification does not include any controls. Before the earthquake, the estimates are all insignificant, implying no systematic pre-existing differences. This supports the parallel trends assumption in the DID strategy in Eq. (1.2) where the trend of non-severe counties can severe as a counterfactual case for the trend of severe counties in the absence of the earthquake. More importantly, the figure reveals a significant negative economic impact on severe counties. The effects vary over time. In 2008 (period 0 on the horizontal axis), GDP per capita in severe counties contracts by approximately 20% relative to non-severe counties, marking the largest decline in the post-earthquake period. Over the next five years (periods 1 to 5), the economic setback persists with declines of around 10%, but the power of effect gradually diminishes. By 2016 (period 8), severe counties catch up with non-severe counties in the outcome. The estimates become statistically insignificant and remain until the end of the study period.

The economic performance over 11 years after the earthquake suggests that the earthquake effect on severe counties has been predominantly short-term in nature, despite the initial suffering in GDP per capita. Seven years after the earthquake, the economic recovery in severe counties brings them back to the counterfactual level where no earthquake had happened. Over a longer time frame, the recovery enables severe counties to stay at a similar level as non-severe counties. In the Appendix, table A2 provides the detailed estimates in each year. A threat to the identification arises from two additional earthquakes of 7.0-Richter scale that struck Sichuan in 2013 and in 2017.[18] Despite their lower intensity and more localized damage, I control for their

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[18] The earthquake in 2013 is referred as the Lushan earthquake, while the one in 2017 is the Jiuzhaigou earthquake.

potential confounding effects by adding two interaction terms between the distance to their epicentres and the post-earthquake dummies, denoted as $Distance_{2013} \times Post_{2013,t}$ and $Distance_{2017} \times Post_{2017,t}$. The specification in column (2) reports similar estimates to column (1), with one notable difference: the estimate in period 9 becomes significantly negative. This suggests that the 2013 Lushan earthquake has minimal impact, while the 2017 Jiuzhaigou earthquake contributes to an additional economic decline. To further assess the robustness of the baseline findings, I introduce different types of controls. Column (3) accounts for the influence of pre-earthquake economic structure by including the shares of agricultural and industrial sectors in 2007, interacted with the post-earthquake dummy. Column (4) controls for geographic characteristics by adding the interaction between county altitude differences and the post-earthquake dummy. Finally, column (5) considers population density in 2007 interacted with the post-earthquake dummy to capture potential demographic effects. Across all specifications, the estimates in the pre- and post-earthquake periods are not sensitive to the inclusion of controls.

1.5.2 Average Earthquake Effect

The analysis of dynamic earthquake effects above supports the key assumption of a standard DID model. Table 1.2 provides the regression results from estimating Eq. (1.2). Columns (1)-(5) use the full sample (2003-2019) to capture any longer-term effect that might be revealed over an extended period. The significantly negative estimates are stable and not sensitive to the types of controls. I then restrict the sample before 2017 in columns (6) and before 2013 in column (7) to reveal the short-term earthquake effect which will be compared with the estimate in column (1). Besides, it also excludes potential influences from the 2013 and 2017 earthquakes. The estimated magnitude diminishes when using a sample with a longer post-earthquake period. This suggests that the economic impact of the earthquake becomes less pronounced over time and also indicates a recovery phase, which aligns well with the event-study results. The decline is primarily concentrated in the initial years following the earthquake. To further test this claim, I select the sample from 2003 to 2007 and 2015 to 2019 and re-estimate the same regression equation in column (8). The estimate now loses significance, indicating that the difference in the outcome during the last 5 years are similar as the pre-earthquake level. In other words, severe counties have recovered economically after a short-term disruption.

If taken the specification in column (2) as the baseline to interpret, severe counties loss approximately 12% of GDP per capita after the earthquake. By a back-of-envelope calculation, it translates to a substantial loss of 2,486 RMB (approximately $324) per person in severe counties compared to non-severe counties, considering its mean value in non-severe counties to be 20,717 RMB. One should be cautious when interpreting the earthquake effect from the DID model. The empirical results do not imply that non-severe counties are immune to any possible drop in GDP per capita. Instead, the estimate represents the post-earthquake difference between severe and non-severe counties relative to the pre-earthquake difference. From the statistical perspective, the earthquake effect is the average treatment effect to the treated (ATT). The decline in GDP per capita should be regarded as an average effect only in severe counties, rather than all counties in Sichuan province.

Last, it should be reminded that only checking the average effect of the Wenchuan earthquake does not provide the whole picture of development pattern. The empirical evidence in this section shows the earthquake effect varies across years. The estimated 12% reduction in GDP per capita merely represents an average across the entire post-earthquake period. Although this recovery is obscured by focusing solely on the average effect, it becomes evident when the magnitude of the impact diminishes over a longer time frame.

1.5.3 Robustness Checks

The previous analysis suggests the Wenchuan earthquake has brought the short-term economic pain to severe counties. I introduce a series of robustness checks to assess the consistency of the main findings in this part, which is crucial to confirming the causal claims of this study.

Heterogeneity Test

This section discusses whether the earthquake has heterogeneous effects on GDP per capita across different economic sectors. Using the same settings in Eq. (1.1), figure A4 plots the event-study analysis for the agricultural, industrial, and tertiary sectors. From panel (a), severe counties have higher agricultural GDP per capita before the earthquake, relative to non-severe counties. Although this pre-existing difference violates the strict parallel trends assumption in a DID model, the sharp drop into negative territory in the earthquake year may still reflect the disruptive effect of the disaster.[19] In contrast, severe and non-severe counties have similar trends in the industrial and tertiary sectors, as are shown in panels (b) and (c). Both of them confirm the causal interpretation of the significant declines observed in these sectors. Beyond the immediate impact, the figure also reveals sectoral differences in the recovery trajectories. In both the agricultural and industrial sectors, the economic gap between severe and non-severe counties gradually diminishes and becomes statistically insignificant several years later. Different from them, the tertiary sector shows a persistent gap that remains negligible until the end of the study period, suggesting that service-based industries in severe counties have yet to fully rebound. In summary, the heterogeneity analysis suggests that while severe counties experience economic recovery in the agricultural and industrial sectors, they continue to lag in the tertiary sector.

Alternative Outcome

The outcome of interest in the baseline specification is GDP per capita, which captures aggregate production. However, this indicator may not fully reflect changes in social welfare (Jorgenson, 2018). While production and consumption are typically cyclical in an equilibrium economy, natural disasters can disrupt this relationship. Following a disaster, private consumption may stagnate due to household asset losses, even as factories resume production after a certain period (Nakamura et al., 2013; Gignoux and Menéndez, 2016; Yin et al., 2022). This divergence suggests that the recovery of GDP per capita alone may not provide strong evidence on the recovery of demand. Therefore, I use total retail sales of consumer goods as a measure of private consumption

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[19] The magnitude of the decline is not precisely estimated.

in this check. This indicator captures the monetary value of physical goods and services sold to individuals and social groups through financial transactions but also measures consumer demand and willingness to spend within the economy.

Similar to the previous settings in Eq. (1.1), figure A5 depicts dynamic earthquake effects on retail sales per capita (in the natural log form). Before the earthquake, the standardized difference in consumption between severe and non-severe counties are insignificant. After the earthquake, retail sales in severe counties sharply decline and remain significantly lower for the next three years. However, from 2010 to 2013 (periods 2 to 5) and again in 2019 (period 11), the differences become insignificant again. In the Appendix table A3, I present the estimates of the average effect on retail sales of consumer goods and confirm the same finding. Overall, this analysis reinforces the economic recovery of severe counties from the demand side.

Alternative Treatment

In the next robustness check, I first use the variation in county-level earthquake intensity scales to estimate the earthquake effect on the economic outcome. This approach offers two key advantages over the binary treatment in the baseline analysis. First, it allows for a finer assessment of heterogeneous effects among severe counties, based on intensity scales. Second, it largely mitigates the concern about human intervention, thus better reflecting the true severity at the county level.

Table 1.3 presents the estimates from this robustness check, following the same specifications as Eq. (1.2). The interaction term between the intensity scale and the post-earthquake dummy serves as the primary explanatory variable in columns (1)-(3).[20] The significantly negative estimates align with the main findings, as counties receive stronger earthquake intensity would suffer greater economic losses. Besides, the specification with a shorter post-earthquake period reveals a stronger decline in GDP per capita, which indicates the case of economic recovery over time.

I also replace the treatment dummy with the linear distance to the epicentre (in the natural log form) and document the estimates in columns (4)-(6) of Table 1.3. In columns (7)-(9), I modify the treatment dummy into another binary variable ("Far

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[20] In this measure, counties with an earthquake intensity scale of 5 or below serve as the reference group.

From Earthquake") which equals 1 if a county is located beyond the 50th percentile of the distance distribution. Those significantly positive estimates support the expectation that counties farther from the epicentre receive less impacts from the earthquake. Again, comparing the full-sample specification with those using a shorter post-earthquake period reveals evidence of economic recovery.

Spillover Effect

The classification of severe and non-severe counties in this study has provided robust estimations. Ideally, only severe counties are subject to the earthquake effect, which satisfies the Stable Unit Treatment Value Assumption (SUTVA). However, the assumption may be violated if spillover effects extend to non-severe counties. To assess the presence of spillover effects, I test whether non-severe counties bordering severe counties also experience the post-earthquake change in the outcome. I define neighbour counties as non-severe counties that share a direct border with severe counties and introduce an interaction term between the neighbour dummy and post-earthquake dummy ($Neighbour_i \times Post_t$) in Eq. (1.1) and Eq. (1.2). This term captures potential spillover effect from severe counties to adjacent non-severe counties. Figure A6 displays the revised event-study analysis of GDP per capita, which barely differs from the baseline result in figure 1.3. Spillover effects, even if existed, do not pose a threat to the identification strategy.

Table A4 presents the two types of evidence on negligible spillover effects. Columns (1)-(3) document the estimates of the revised average earthquake effect, suggesting that the results are consistent with the main findings. Besides, spillover effects are marginally significantly only in a longer time frame. In columns (4)-(6), I exclude those non-severe counties bordered with severe counties, and the estimates remain largely unchanged too.

Migration

I investigate whether the Wenchuan earthquake induces migration in severe counties. A cross-country study by Mahajan and Yang (2020) suggests that households in disaster-stricken areas may relocate in search of better opportunities. It is plausible that the migration of wealthier families might worsen the economic decline in their home counties, as they typically represent higher productivity. Such migration could bias the baseline estimation of the earthquake effect. Figure A8 illustrates county population levels and growth trends from 2003 to 2019, and the data show little evidence of a large-scale demographic shift following the earthquake. Figure A7 further presents the dynamic effects on county population. The gap in population does not widened between severe and non-severe counties. In table A5, columns (1)–(3) report the average earthquake effect on population, while columns (4)–(6) incorporate the control for potential spillover effects. The insignificant estimates across these specifications indicate that mass migration does not occur in response to the natural disaster in Sichuan province.[21]

The unique design of the Hukou (registered residence) system in China increases the cost of migrating to another area. It is probably the reason for the lack of migration following disasters (Whalley and Zhang, 2007; Bao et al., 2011; Huang and Dong, 2025). Residents without a local Hukou often face barriers to accessing social resources such as healthcare and education available to the locals (Zhang et al., 2020; Vendryes, 2011). While changing one's Hukou status is not excessively challenging across Sichuan province, the processing time can still impede migration efforts. I acknowledge that some families still choose to migrate between counties after the earthquake, but their impact on the transfer of wealth and the overall economy may have been limited. In the Appendix A.3, I present a theoretical model which implies that limited mobility can improve post-earthquake economic recovery by preventing skilled workers from moving around. In the section, I further analyze this problem from the perspective of county employment.

Placebo Test

In this section, I conduct the placebo test to reinforce the main results. First, I randomly assign the treatment of being a severe county and repeat the regression in Eq. (1.2) 1,000 times to plot the distribution of the estimates. With 39 severe counties

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[21] It should be noted that the resettlement right after the earthquake only represents temporary relocation of residence, usually within the same county. Thus, local resettlement should not counted as inter-county migration.

out of 180 in total, each county has a 22% probability of receiving the treatment. In the appendix, figure A9 illustrates the distribution of the simulated estimates from the 1,000 regression results and its kernel density. The vertical dashed line represents the average earthquake effect as a comparison. From the figure, the majority of simulations deviate from the actual effect, ruling out the spurious earthquake effect. This placebo test indicates that the decline in GDP per capita in the post-earthquake period should be attributed to the exposure of the earthquake severity.

Then, I explore hypothetical scenarios in which the earthquake is assumed to have occurred at different times, specifically testing whether severe counties display signs of economic decline in the pre-earthquake period. To do this, I interact hypothetical post-earthquake dummies with the severe county status, assuming the earthquake occurred in 2004, 2005, and 2006. Maintaining the same settings as in Eq. (1.2), Table A6 presents the estimates from this falsification test with the pre-earthquake sample. In summary, the estimates are much smaller in magnitude and consistently insignificant across all columns.

The direct explanation is that the timing of the earthquake is crucial for understanding its economic impact, as evidenced by the result. The specifications rule out the possibility that severe counties already diverge from non-severe counties prior to the disaster. This result also strengthens the identification by implying a non-existent pre-trend difference.

1.6 Discussion: Mechanisms of Economic Recovery

I investigate the mechanisms that severe counties manage to recover after short-term pain in this section. The supply side of an economy typically comprises three fundamental elements: technology, labour, and capital. While technology is less likely to undergo discrete changes from a natural disaster, the other two components are more likely to be influenced temporary shocks. Meanwhile, the presence of local governments are hard to ignore in the process of recovery. Thus, I focus on the following aspects: employment structure, fixed-asset investment, and fiscal expenditures.

1.6.1 Employment Structure

It is possible that the earthquake exerts minimal influence on employment level in severe counties, which promotes the economic recovery in later periods. Given this, I examine whether the Wenchuan earthquake alters the labour market in severe counties. Likewise, the analysis takes the specification outlined in Eq. (1.1) with the outcome of interest now being the number of employees in rural, industrial, and tertiary sectors.

Figure 1.4 presents these yearly estimates along with their corresponding 95% confidence intervals. To ensure comparability, the coefficients in the event-study plots are standardized to the year just before the earthquake. Severe and non-severe counties have similar employment structures measured by the number of employees before the earthquake. Right after the earthquake, severe counties experience a temporary decline in industrial and tertiary employment, while no significant impact exists in the rural labour market. In the following years, the gap of employment levels disappears in both the rural and industrial sectors, indicating that these sectors recover relatively quickly from the initial shock. However, the tertiary sector in severe counties displays more volatility. Tertiary employment experiences significant relative declines in 2014 (period 6) and again in 2019 (period 11). This pattern aligns with previous heterogeneous findings in figure A4 that the tertiary sector remains the slowest to recover from the earthquake’ s economic impact.

This section prompts a thoughtful exploration into the economic setback and subsequent recovery through an analysis of the number of employees. Resilience in employment may help explain the economic recovery in the baseline findings. Another implication here is that the stability of local labour market over the years could also rule out a large-scale migration from severe counties, further validating the causal claim made in this study. Aside from this, it is equally important to distinguish between the extensive and intensive margins of labour market outcomes. In the Appendix A.4, I attempt to provide an analysis on the earthquake effect at the individual level to shed light on the intensive margin of labour market outcomes. These findings highlight the importance of examining both the extensive and intensive margins of labour market outcomes to gain a more comprehensive understanding of the economic impact. Nonetheless, I remain open to further exploration to capture the effect on labour market thoroughly.

1.6.2 Fixed-Asset Investment

Investment in fixed assets represents the spending on the reconstruction of infrastructure and the rebuilding of homes in affected regions, as well as the renovation of existing assets, which becomes the top priority after the disaster. As those efforts aim to restore life normalcy and improve production efficiency (Deng et al., 2022), the recovery years observed years later might emerge through this channel.

I apply Eq. (1.1) with fixed-asset investment per capita as the outcome of interest and derive the yearly estimates. Figure 1.5 illustrates that severe and non-severe counties display similar levels of fixed-asset investment prior to the earthquake. However, severe counties experience a substantial increase in fixed-asset investment after that, particularly from 2009 to 2011, coinciding with the period of large-scale government-led reconstruction. By 2012, as reconstruction efforts reach completion, fixed-asset investment in severe counties begins returning to levels comparable to those of non-severe counties. Given that investment often functions as a lagging economic indicator, this temporary surge likely contributes to economic recovery in subsequent years. This pattern reflects a short-term response to the earthquake, aimed at supporting immediate economic stabilization and recovery.

A question arises as to whether the surge in investment observed in severe counties between 2009 and 2011 results from a "crowding-out" effect on non-severe counties. This could be this case if reconstruction efforts divert resources away from non-severe counties. Figure A10 illustrates trends in fixed-asset investment per capita, which does not support this hypothesis. Investment levels in non-severe counties show no sudden disruptions during the post-earthquake period. This observation reinforces the interpretation that the positive estimates identified in the event-study design are driven purely by reconstruction in response to the earthquake, rather than resource reallocation at the expense of non-severe counties.

1.6.3 Fiscal Conditions

Another aspect of the investigation focuses on the role of county governments which are responsible for leading post-earthquake reconstruction efforts. Following the Wenchuan earthquake, these governments received substantial fiscal transfers from the provincial or central government to finance recovery initiatives (Barone and Mocetti, 2014; Park and Wang, 2017). Consequently, an expansion in fiscal capacity is expected to be reflected in increased of fiscal expenditures, which serve as proxy for the scale of government operations (Zhu et al., 2021; Masi et al., 2024).

Using the same approach in Eq. (1.1), figure 1.6 illustrates the differences in fiscal expenditures per capita between severe and non-severe counties. Prior to the earthquake, both display parallel trends in the fiscal outcome. However, the sharp surge of expenditures in severe counties is observed in the earthquake year of 2008. From 2008 to 2010, they spend drastically more than non-severe counties, likely due to affluent subsidies from various sources, as discussed in the Background section. However, the surge in spending proves to be temporary. Beginning in 2011, severe counties return to expenditure levels similar to those of their non-severe counterparts. This suggests that the temporary fiscal stimulus play a role in facilitating economic recovery.

The substantial increase in fiscal expenditures does not “crowd-out” intergovernmental transfers to non-severe counties in the same jurisdiction. By presenting trends in fiscal expenditures per capita from 2003 to 2019, figure A11 shows that fiscal expenditures per capita in non-severe counties stably grow during the key reconstruction period (2009-2012), with no notable disruptions or declines. This suggests that the fiscal support received by severe counties comes from upper-level governments and does not influence resource allocations to non-severe counties.

The temporary effect on fiscal conditions is also depicted by the evolution of fiscal dependency in figure A12, measured as the ratio of fiscal revenue to expenditure. The pattern closely mirrors that of GDP per capita, as fiscal dependency in severe counties deteriorates significantly in the three years following the earthquake. This decline reflects increased budget deficits and heightened fiscal pressures because of the large expansion in fiscal expenditures. In subsequent years, fiscal dependency in severe counties financially returns to the pre-earthquake level.

1.7 Conclusion

The Wenchuan earthquake in 2008 has brought tremendous damage to the affected residents and local economic activities. After the earthquake, experts assessed earthquake severity and then classified counties in Sichuan province as severe and non-severe. There are no significant pre-existing differences between them under this classification, which provides a good opportunity to explore the impact of natural disasters on development trajectory.

The analytical framework employs a Difference-in-Differences approach to evaluate the earthquake effect. The results from the event-study design suggest that the largest economic contraction occurs in the earthquake year of 2008: GDP per capita in severe counties decreases by about 20% relative to that in non-severe counties. The negative effect persists but diminishes over years. In 2016, severe counties catch up with non-severe counties in GDP per capita for the first time, suggesting the recovery from the Wenchuan earthquake in the former. Therefore, this empirical analysis claims that the impact of the Wenchuan earthquake turns out to be short-lived.

Furthermore, I also investigate the potential mechanisms to explain the main findings, which are stable employment structure and temporary surge in fixed-asset investment and fiscal expenditures. First, the earthquake does not alter the employment structure nor lead to an increase in the unemployment population across sectors, which lays a foundation for the later economic recovery. Then, the surge in fixed-asset investment in severe counties plays a significant role in stimulating economic recovery during the post-earthquake period. With a lagging effect, more investment contributes to the restoration of economic activity and growth in affected regions. Next, an analysis of fiscal expenditures indicates that severe counties spend considerably more than non-severe counties during the first three years after the earthquake. The increase in government operations may help explain local recovery years later. Back to the research question: are severe counties "still in pain"? And the answer should be "no".

Tables

Table 1.1: Balance Check Before the Wenchuan Earthquake

	(1) Non-Severe Counties	(2) Severe Counties	(3) Conditional Difference
Panel A: County-Year Variables			
GDP per capita (RMB)	9741.463	10474.182	-738.274
	(8002.782)	(6577.866)	(1202.732)
Agricultural Share	0.282	0.277	0.005
	(0.143)	(0.137)	(0.025)
Industrial Share	0.380	0.388	-0.009
	(0.156)	(0.155)	(0.027)
Population (10k persons)	46.177	42.756	3.424
	(35.323)	(31.491)	(5.833)
Rural Population Share	0.776	0.804	-0.028
	(0.352)	(0.591)	(0.028)
Fiscal Expenditures per capita (RMB)	1444.965	1446.344	-2.759
	(1183.621)	(1011.651)	(174.993)
Fiscal Revenues per capita (RMB)	308.262	379.255	-71.284
	(377.571)	(444.976)	(73.273)
Arable Lands Share	0.165	0.149	0.017
	(0.129)	(0.130)	(0.024)
Fixed-Asset Investment per capita (RMB)	5127.692	6328.576	-1205.664
	(5533.922)	(6079.327)	(962.240)
Panel B: County-Fixed Characteristics			
Minority County	0.305	0.205	0.100
	(0.462)	(0.409)	(0.082)
Altitude Difference	1.980	2.395	-0.416
	(1.915)	(1.817)	(0.343)
Distance to Epicentre (km)	274.358	159.871	114.486***
	(117.427)	(90.042)	(20.289)
Share of Earthquake Intensity Scale <= 5	0.305	0.00	0.305***
	(0.462)	(0.00)	(0.074)
Strong Earthquake Intensity	6.327	8.103	-1.777***
	(0.715)	(1.619)	(0.199)
Death Rate (/10k persons)	0.225	131.125	-130.870***
	(1.162)	(411.176)	(34.496)

Note: This table documents the balance check between severe and non-severe counties in Sichuan province before the Wenchuan earthquake in 2008. All monetary indicators have been adjusted at the 2007 price. Each cell reports the variable's mean and standard deviation in the parenthesis. The third column calculates the mean difference conditional on the year (except those county-fixed variables in panel B), while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table 1.2: Average Earthquake Effect on GDP per capita

Dependent Variable: ln(GDP per capita)	Full Sample: 2003-2019					2003-2016	2003-2012	2003-2007 and 2015-2019
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Severe × Post	-0.1144*** (0.0415)	-0.1247*** (0.0409)	-0.1270*** (0.0349)	-0.1155*** (0.0339)	-0.1244*** (0.0342)	-0.1339*** (0.0408)	-0.1590*** (0.0396)	-0.0709 (0.0448)
Distance Control		✓	✓	✓	✓			
Structure Control			✓	✓	✓			
Geographic Control				✓	✓			
Density Control					✓			
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	3042	3042	3042	3042	3042	2507	1798	1788

Note: An observation is an individual county at one year in the sample spanning from 2003 to 2019. The outcome of interest is GDP per capita (adjusted at the 2007 price) in its natural log form. Coefficients are estimated in Eq. (1.2) at the county level. The full sample is used in columns (1) and (5), while the 2003-2016 sample in columns (6) and the 2003-2012 sample in (7). In column (8), only the pre-earthquake sample and the 2015-2019 sample are used. Distance control has the interaction term between the distance to their epicentres in the 2013 and 2017 earthquakes and their respective post-earthquake dummies; structure control includes the share of agricultural and industrial sectors in county GDP in 2007 both interacted with the post dummy ; geographic control considers county altitude difference interacted with the post dummy; density control represents county density in 2007 interacted with the post dummy. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

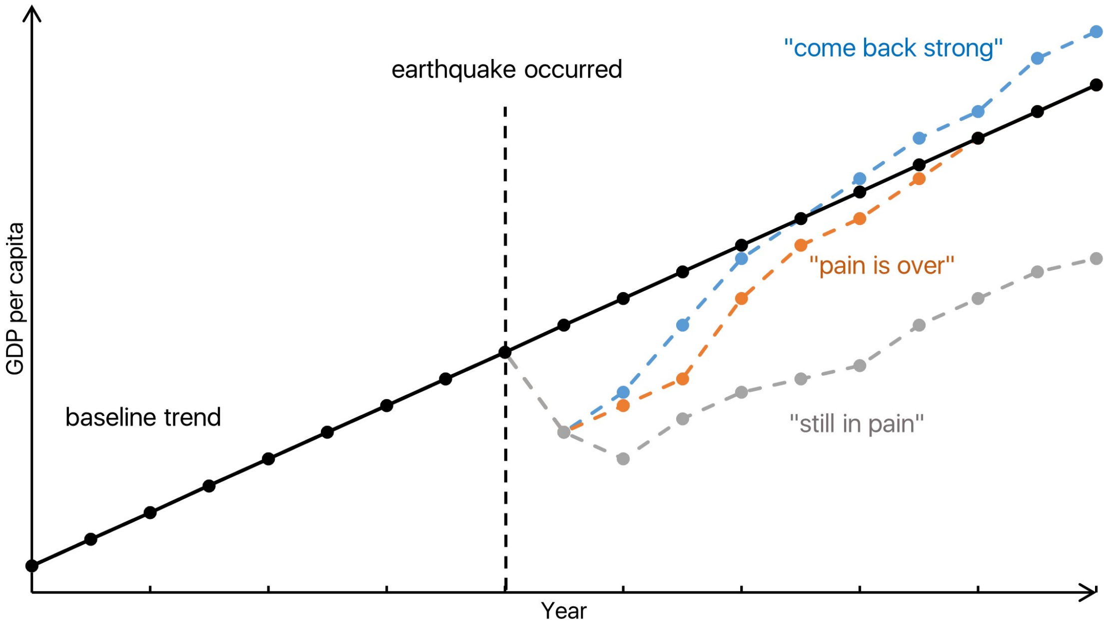
TABLE 1.3: Alternative Treatment

Dependent Variable: ln(GDP per capita)	Intensity			Distance					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earthquake Intensity Scale × Post	-0.0527***	-0.0594***	-0.0680***						
	(0.011)	(0.011)	(0.010)						
Distance to Epicentre × Post				0.1209***	0.1347***	0.1501***			
				(0.022)	(0.022)	(0.020)			
Far From Earthquake × Post							0.0976***	0.1087***	0.1224***
							(0.032)	(0.031)	(0.029)
Sample Period	Full	<2017	<2013	Full	<2017	<2013	Full	<2017	<2013
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	3049	2511	1798	3049	2511	1798	3049	2511	1798

Note: An observation is an individual county at one year in the sample spanning from 2003 to 2019. The outcome of interest is GDP per capita (adjusted at the 2007 price) in its natural log form. Coefficients are estimated in Eq. (1.2) at the county level. Columns (1)-(3) record the estimates if using the earthquake intensity scale, instead of the treatment dummy. Similarly, the treatment dummy is replaced by the natural log of the distance to the epicentre in columns (4)-(6). Another distance-related binary dummy in columns (7)-(9) is equal to 1 if a county is located beyond the 50th percentile of the distance distribution. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

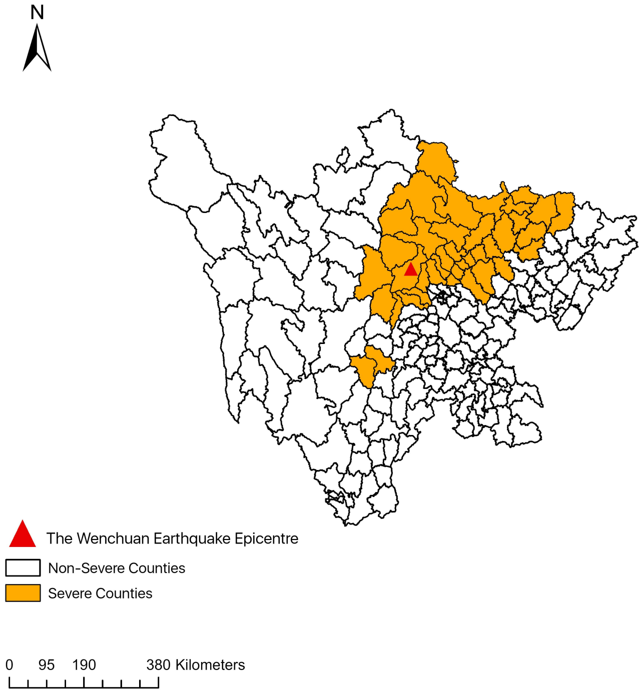
Figures

FIGURE 1.1: Possible Growth Patterns After Earthquake



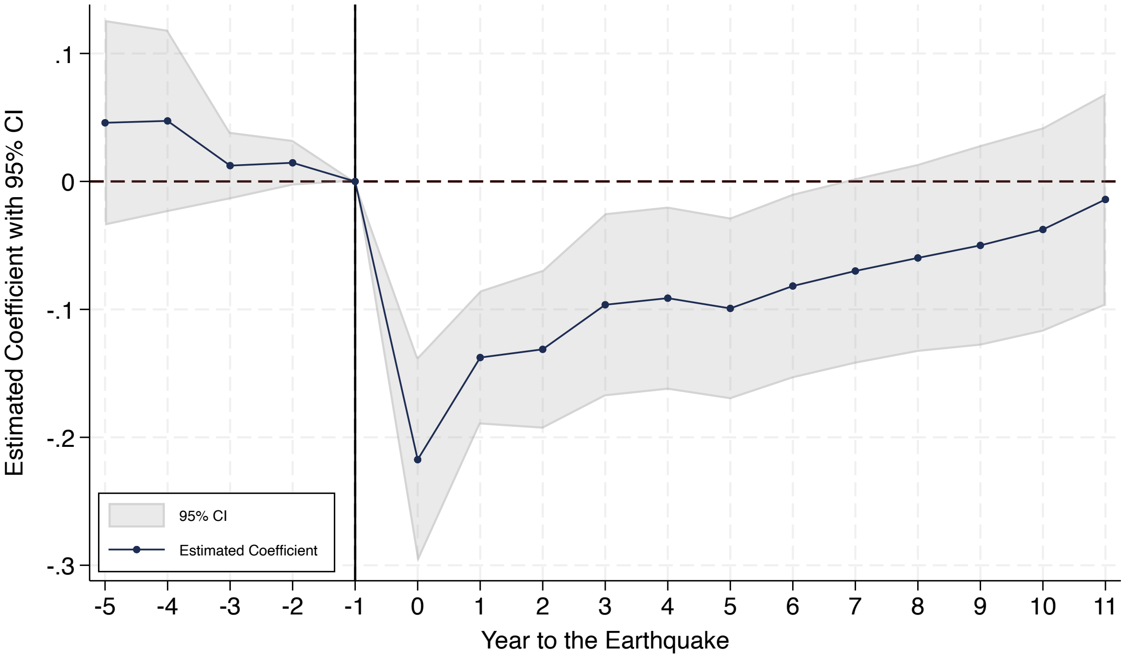
Note: Hsiang and Jina (2014) originally proposed the four hypotheses about growth patterns after a disaster. I redrew and modified the figure to fit into the background of this study.

FIGURE 1.2: Counties in Sichuan Classified by Severity



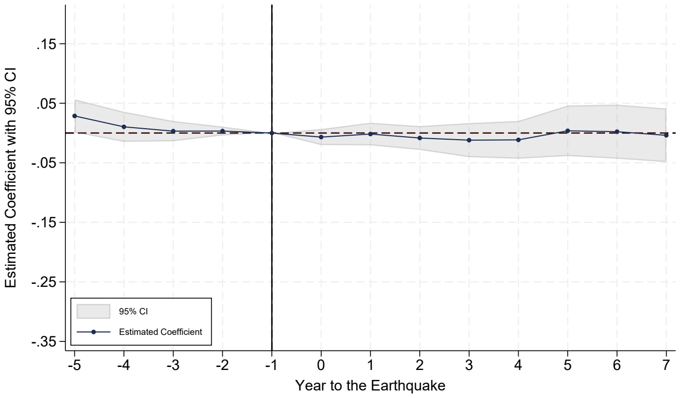
Note: In the map of Sichuan province, those in orange are severe counties.

Figure 1.3: Event-Study Analysis of GDP per capita

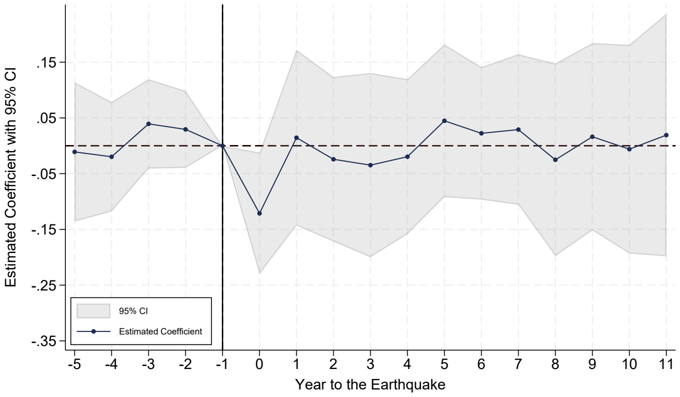


Note: The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure from the event-study regression in Eq. (1.1). The outcome of interest is GDP per capita in the natural log form. The vertical dashed line indicates the reference year in 2007, one year before the earthquake. Standard errors are clustered at the county level.

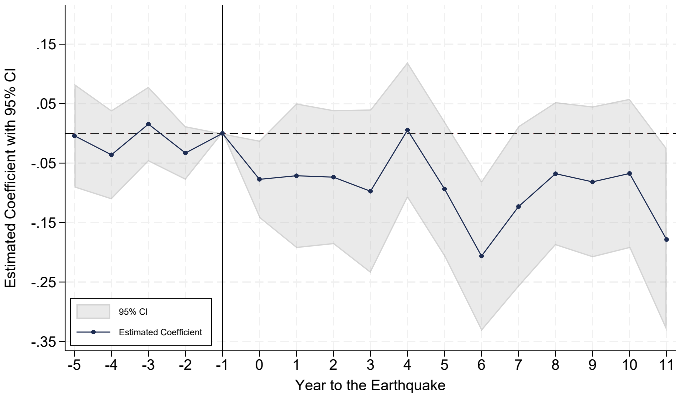
Figure 1.4: Effect on Employees by Sector



(a) The Outcome is ln(Rural Employees)



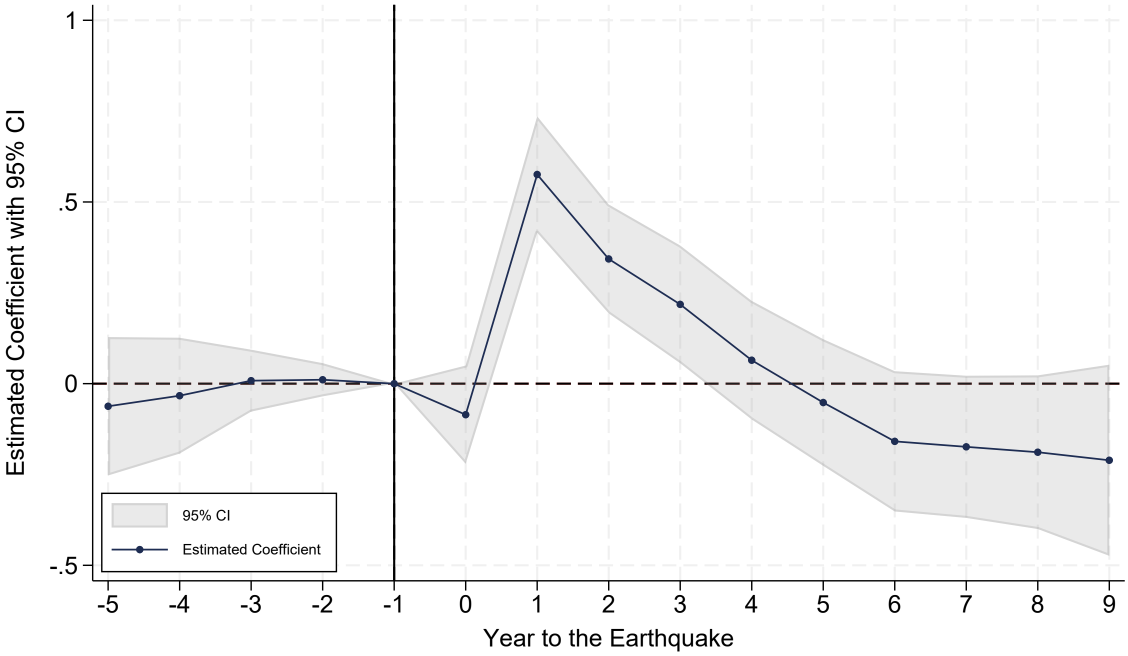
(b) The Outcome is ln(Industrial Employees)



(c) The Outcome is ln(Tertiary Employees)

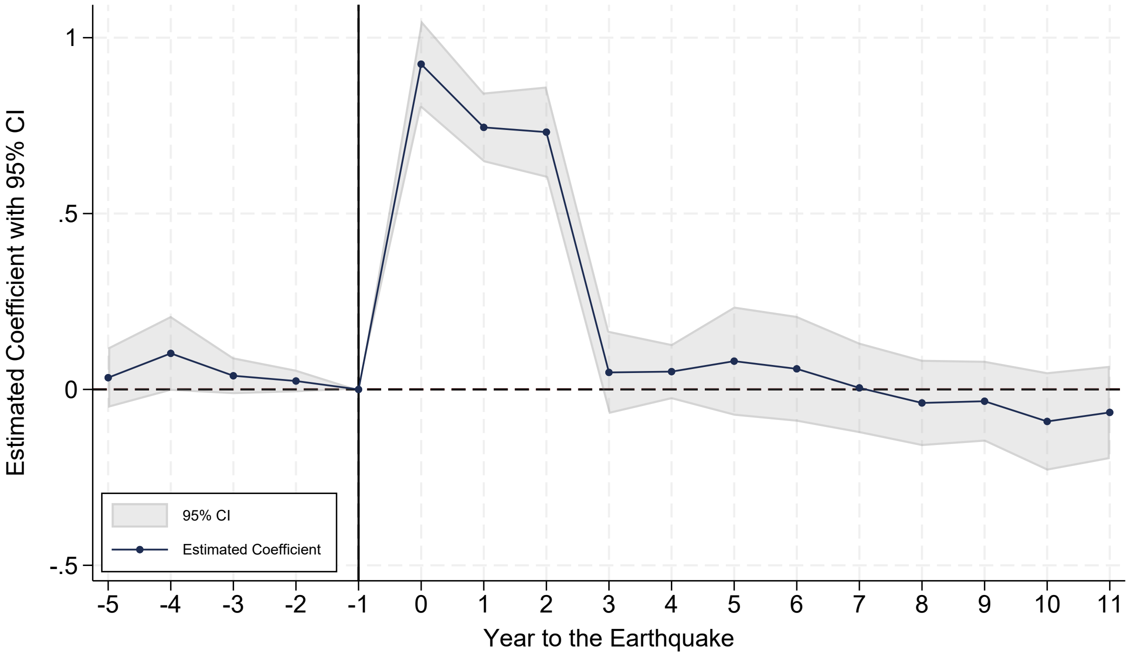
Note: The event-study regression is the same as proposed in Eq. (1.1), but the outcomes of interest are the natural log of rural, industrial, and tertiary employees. The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure. The vertical dashed line indicates the reference year in 2007, one year before the earthquake, and the standard errors are clustered at the county level.

Figure 1.5: Effect on Fixed-Asset Investment



Note: The event-study regression is the same as proposed in Eq. (1.1), but the outcome of interest is the natural log of fixed-asset investment per capita. The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure. The vertical dashed line indicates the reference year in 2007, one year before the earthquake, and the standard errors are clustered at the county level.

Figure 1.6: Effect on Fiscal Expenditure per capita



Note: The event-study regression is the same as proposed in Eq. (1.1), but the outcome of interest is the natural log of fiscal expenditures per capita. The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure. The vertical dashed line indicates the reference year in 2007, one year before the earthquake, and the standard errors are clustered at the county level.

Chapter 2

Too Young To Get Over: Earthquake Exposure and Educational Attainment

2.1 Introduction

Disruptive events experienced at a young age can have profound and lasting consequences. For instance, children who have faced adversities may grow a few centimetres shorter (Dercon and Porter, 2014; Van den Berg et al., 2016), earn lower wages (Chen and Zhou, 2007; De Giorgi et al., 2023), or experience reduced likelihood of completing higher education alongside increased likelihood of mental health issues (Chen et al., 2019; Laird et al., 2020). These negative outcomes are predominantly concentrated in human capital accumulation. However, it usually remains a challenge to disentangle the impact of a specific adversity from other confounding factors, such as family socioeconomic status or regional economic development. To address this concern, I focus on an exogenous natural disaster in China to investigate how a destructive event can influence children's human capital accumulation.

The 2008 Wenchuan earthquake is the most destructive earthquake in contemporary China. The unpredictability and randomness of the earthquake provide a quasi-natural experiment, allowing for the isolation of its causal impact on children's educational outcomes from other endogenous factors. After the assessment organized by local governments, counties in Sichuan can be classified as "severe counties" and "non-severe counties", based on their exposure to the earthquake. The exogenous county-level exposure to the earthquake consists of the first source of variation to estimate the earthquake effect on educational attainment. The second source stems from the age of the students. In China, the Compulsory Education Law mandates that students complete nine years of education in primary and junior high school and prohibits dropouts during this period.[1] Students typically reach the final year of compulsory education at around age 15, after which they are legally permitted to join the labour force. In this study, I use the age-15 threshold to evaluate the earthquake effect on young students' educational attainment.

By combining these two sources of variation, I employ a cohort Difference-in-Differences (DID) approach to provide empirical evidence on the long-term effects of the Wenchuan earthquake on human capital accumulation. The empirical analysis suggests that the younger cohorts in severe counties achieve, on average, 0.36 fewer years of education following the earthquake, representing a 3.7% reduction in educational attainment. I apply an event-study design to assess the by-cohort effect, using the educational outcome of the age-15 cohort as the reference group. The findings reveal negligible differences in years of schooling between older cohorts in severe and non-severe counties, supporting the validity of the cohort DID model. In contrast, the younger cohorts in severe counties display significant reductions in years of schooling. Overall, the results consistently demonstrate the adverse impact of the earthquake on youth education.

I propose that the end of compulsory education is the main mechanism underlying the declining effect on education. After the earthquake, the younger cohorts in severe counties display a higher likelihood of discontinuing their studies. However, the Compulsory Education Law delays such dropouts. Once this legal requirement ends, these younger students often choose to enter the labour market rather than remain in high school. By contrast, older cohorts are not constrained by the law; students in high school represent a self-selected group determined to pursue further education and are therefore less sensitive to the earthquake's impact. The empirical analysis supports the argument. Compared to the younger cohorts in non-severe counties, those in severe counties have a reduced likelihood of high school graduation, while the difference in

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[1] Details can be found in Article 2 of the *Compulsory Education Law of the People's Republic of China* (1986 edition). The 9-year compulsory education comprises 6 years of primary school and 3 years of junior high school.

junior high school education is absent. Additional evidence suggests a higher dropout rate after compulsory education among the younger cohorts in severe counties.

My study makes several key contributions to the existing literature. First, it expands our understanding of the educational consequences of the Wenchuan earthquake by providing evidence on age-related heterogeneous effects to students. Some literature also examine the impact of the Wenchuan earthquake on education but with different emphases. For example, Lu et al. (2023b) and Leng and Liu (2022) study the impact on college entrance exam scores (the exam is usually set after high school graduation); another close study is Liu and Xu (2021) which focuses on the "unexpected" benefits for female students in affected poverty counties. Beyond the literature on the Wenchuan earthquake, both Wang et al. (2017) and Tian et al. (2022) explore the impact from the 1976 Tangshan earthquake in China on educational outcomes. Paudel and Ryu (2018), Shidiqi et al. (2023), Andrabi et al. (2023) investigate the impacts from earthquakes on education graduations in Nepal, years of schooling in Indonesia, and academic test performance in Pakistan, respectively. To the best of my knowledge, this study is the first to use the age-15 threshold to discover the heterogeneous impact of the Wenchuan earthquake on education. In China, education before high school is mandatory under the Compulsory Education Law, which prevents early dropouts. After the earthquake, young students in severe counties appear more likely to enter the labour market and discontinue their education, a pattern not observed among the older cohorts. The main findings in this study also corroborate the positive effect of the law on improving educational attainment (Fang et al., 2012; Xiao et al., 2017; Liu et al., 2024).

Next, this study is closely related to the discussion of disruptive events during early life, a period when children are particularly sensitive to external shocks. Studies has documented military conflict on the tolerance for domestic violence (La Mattina and Shemyakina, 2024), parental divorce and death on educational outcomes and cognitive development (Boggess, 1998; Chen et al., 2019; De Giorgi et al., 2023; Frimmel et al., 2024), and famine on physical health and risk preferences (Porter, 2010; Dercon and Porter, 2014; Chen and Zhou, 2007; Guo et al., 2024; Chen et al., 2024). Those studies focus on the long-term consequence, while this study offers medium-term evidence of the negative impact on education.

The remaining part of this paper is organized as follows: Section 2.2 introduces the

Chinese education system.[2] Section 2.3 and 2.4 describes the data set and identification strategy for the empirical analysis. Section 2.5 resents empirical evidence of the earthquake effect on educational attainment along with robustness checks. Section 2.6 discusses some mechanisms to explain the main finding. And Section 2.7 concludes the study.

2.2 Background

In section 1.2, I have introduced the 2008 Wenchuan earthquake from its direct damage, government response, and reconstruction efforts afterwards. I omit this part in this chapter to avoid repetition. Instead, I provide a brief introduction to the contemporary education system in China and government response in education after the earthquake.

Historically, China has developed and managed its education systems. Education has long served as a key component of state services across ancient dynasties. The current education system consists of five educational stages: pre-school education, primary education, junior secondary education, senior secondary education, and higher education. During the pre-school stage, children aged 3 to 6 attend kindergarten to prepare for school life. Primary and junior secondary education form the 9-year compulsory education period, during which most students spend 6 years in primary school and 3 years in junior high school.[3] Since the enactment of the Compulsory Education Law in 1986, children aged 6 to 15 must attend school, and neither individuals nor organizations are allowed to recruit students under the age of 16.[4] In 2007, 99% of the school-age students registered in primary and junior high schools across China, signifying a 8% increase from 2002.[5]

Beyond the compulsory education, students who choose to continue their studies advance to the next stage based on their scores on the selective high school entrance exam ("zhongkao"). Senior secondary education comprises two types of schools: regular high schools and secondary vocational schools. Regular high schools prepare students

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[2] To avoid repetition, I introduce another aspect of research background, while the introduction of the Wenchuan earthquake can be found in Chapter 1.

[3] In some regions, students spend 5 years in primary school and 4 years in junior high school, though this is less common.

[4] The exceptions to the restriction are regulated by the state.

[5] See from the Ministry of Education: `http://www.moe.gov.cn/jyb_xwfb/xw_fbh/moe_2069/moe_2070/moe_2126/moe_1968/tnull_33105.html`.

for the college entrance exam ("gaokao") by offering a broad academic curriculum, while secondary vocational schools provide training tailored to meet the needs of the production and service sectors.[6] The higher education or the tertiary education in China is more flexible in types and more selective in its admissions. Generally speaking, tertiary vocational education trains students in professional expertise over a three-year program, while undergraduate education requires four years to study a chosen major systematically. After that, qualified students may then pursue graduate studies to earn master's or doctoral degrees.

Within the scope of this study, I focus on students in compulsory education and high school education. The Wenchuan earthquake destroyed nearly 6,000 school residences an led over 3 million students to suspend their studies (Compilation Committee of the Sichuan Earthquake Relief Chronicle of the Wenchuan Earthquake, 2018). On May 18th, just 6 days after the earthquake, the provincial government dispatched teams to assess school residences, designate temporary safe locations for students, and devise plans to restart education. Their swift responses yielded positive outcomes. For instance, students in Wenchuan where the epicentre locates gradually resumed schooling in safe places staring from May 27th. In addition, the college entrance exam of that year was postponed by a month later in some counties due to the earthquake. Once the immediate threat of earthquake disappeared, school reconstruction became a priority. Local governments initiated plans to build new schools and renovate existing ones. In Aba prefecture,[7] 132 out of 182 projects in total on education were scheduled for completion between 2008 and 2009.

2.3 Data

I exploit different data sources in this study to conduct an empirical analysis. The primary dataset in this study is the 2015 National 1% Population Sample Survey. In China, a national population census is conducted every 10 years, while a population sample survey is carried out every five years. The 2015 survey provides detailed information on respondents, including name, gender, age, ethnicity, education level,

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[6] For simplicity, both types are referred to as "high schools" throughout this study.

[7] Wenchuan county is under the administration of Aba prefecture, along with other 11 counties.

industry, occupation, migration status, social security, marriage, childbirth, death, housing situation, and more.[8]

I use the 2015 survey rather than the 2010 census because the 2015 survey is more distant from the earthquake, allowing me to capture medium- to long-term effects. focus on respondents born between 1985 and 2000, or those aged 8 to 23 in 2008. As noted earlier, age 15 marks the endpoint of compulsory education, and the youngest cohort in the 2015 survey that has reached this point consists of individuals born in 2000. In my analysis, I retain respondents in Sichuan province at the time of the survey and focus on those with local Hukou (registered residence). This study mainly examines students of different age cohorts at the time of the earthquake. One limitation of the dataset is that it does not directly record total years of schooling. Instead, it asks for the highest degree obtained and education completion status. To overcome this limitation, I follow the methodology used in previous studies such as Wang et al. (2017); Chen et al. (2020); Liu and Xu (2021) to code the years of schooling based on respondents' answers to these two questions. The details of the coding process are outlined in Appendix B.2.

Next, as in Section 1.3, I use the same classification approach to categorize counties in Sichuan into severe counties and non-severe counties, based on the official assessment from *The Sichuan's Relief Chronicle of the Great Wenchuan Earthquake* (Compilation Committee of the Sichuan Earthquake Relief Chronicle of the Wenchuan Earthquake, 2018). I match the residence of respondents with the severity status after the earthquake. I supplement the county-level dataset with each county's longitude and latitude from the National Geomatics Center of China and the relief degree of land surface to measure altitude difference from You et al. (2018).

In the appendix, table B1 provides the summary statistics of the final sample. On average, respondents spend 10 years of schooling. About 46% of them were younger 15 years old in 2008, and 50% are male. The majority are Han Chinese (89%), slightly below the national average of 92%. Besides, over 62% hold rural Hukou status, and about 21% report living in severe counties.

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[8] See from `https://www.stats.gov.cn/zt_18555/zdtjgz/cydc/xw/202302/t20230221_1917242.htm`.

2.4 Identification Strategy

Individual educational attainment is shaped by a variety of factors, including personal endowments, family socioeconomic status, macroeconomic conditions, and even unexpected random events (Boggess, 1998; Heckman and Rubinstein, 2001; Lundberg, 2013; Wang et al., 2017; Witteveen, 2021). Isolating the influence of one specific factor is challenging due to their interdependence. Building on these studies, I exploit two sources of exogenous variation: earthquake exposure and age in the year of earthquake.

In Chapter 1, I have conducted the balance check in table 1.1, suggesting that severe counties are not significantly different from non-severe counties before the earthquake. Thus, the unpredictability of occurrence and severity in the earthquake provides an exogenous shock across counties in Sichuan province.

On the other hand, students within in the same county may respond differently to the impact of earthquake depending on their age. In this study, I assume that the older cohort (students above 15 years old in 2008) would achieve similar educational outcomes in severe and non-severe counties, conditional on personal and family socioeconomic factors.

The age-15 cutoff is widely accepted in the Chinese context (Wang et al., 2017; Yin et al., 2022; Huang, 2015), and the assumption above is reasonable for two reasons. First, the Compulsory Education Law serves as a legal barrier that retains students who otherwise leave school prematurely. The implementation of the Compulsory Education Law is successful in China; by 2001, over 90% of primary school graduates advanced to junior high school.[9] Once the restriction of the law no longer applies, the external shock could motivate more graduates in junior high schools to enter the labour market instead of continuing their education, either due to loss of family assets or a pessimistic outlook on the returns to education. Second, the older cohorts, such as those already in high school or university, are more likely to continue their education. Since they are no longer bound by the compulsory education, their willingness to study can be driven either by family expectations or self-motivation.

Under the assumption, the younger cohorts (students below and including 15 years old in 2008) are the group most affected by the earthquake. The older cohorts are

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[9] See from `https://www.gov.cn/gongbao/content/2001/content_60920.htm`.

positioned as a counterfactual scenario where the younger cohorts would have achieved in the educational outcome if without the earthquake. Lin and Long (2020) express a similar argument and suggest that China's admission to the WTO reduces youth schooling as more students in high export-exposed regions opt to enter the labour market after turning 15, rather than staying in school.

I employ a cohort Difference-in-Differences (DID) model to estimate the impact of the Wenchuan earthquake on educational attainment among young students. This approach draws on the foundational work of Duflo (2001), which evaluates the effect of school construction on years of schooling and future earnings using a cohort DID model. In the context of China, Chen et al. (2020, 2024) adopt similar methodologies to analyze policy implications on educational outcomes during the Cultural Revolution (1966-1976). In summary, the following equation describes my cohort DID model:

$$y_{igc} = \alpha + \beta Severe_c \times \mathbb{1}(Age_g \leq 15) + x_{igc}^{'}\gamma + \eta_g + \sigma_c + \varepsilon_{igc} \tag{2.1}$$

where y_{igc} denotes the educational outcome for individual i belonging to cohort g in county c. Individuals are grouped by age, and g represents their ages in 2008. $Severe_c$ is a treatment dummy of earthquake severity which takes 1 only if a county was reported as severe in the official documents. $\mathbb{1}(Age_g \leq 15)$ is an indicator function conditional on individual age in 2008. It is equal to 1 if an individual of cohort g was younger than 15 years old in 2008 ("the younger cohorts"). x_{igc} represents a set of individual and household control variables, such as gender, ethnicity, or household conditions. Last, ε_{igc} captures unobservable and random shocks to the outcome of interest. Standard errors are clustered at the county level.

A caveat in this specification is the age effect. Since students in the youngest cohort are only 15 years old at the time of the 2015 survey, they may continue their education beyond junior high school, and the survey does not capture their final educational attainment. I address this issue by including cohort-fixed effects, η_g, which control for differences across age cohorts and ensure that comparisons are made within the same age groups. This effectively mitigates concerns regarding the age effect. Besides, σ_c represents the county-fixed effects that absorb other time-invariant county characteristics. The coefficient β captures the earthquake effect, measuring the difference in educational outcomes between the younger cohorts in severe counties and those in non-severe counties.

One condition for a valid causal estimation is that the older cohorts in severe and non-severe counties should be comparable to serve as a counterfactual for the younger cohorts. Table 2.1 presents a summary of their characteristics. Panel A details individual and household characteristics, while Panel B compares county-fixed characteristics. The last column reports the differences between the older cohorts in severe and non-severe counties conditional on prefecture and cohort, except in Panel B. From the table, the older cohorts in severe and non-severe counties are similar in terms of education level, age, gender, ethnicity, and Hukou status. The only discrepancy is in number of rooms, where the older cohorts in severe counties have more rooms in their residence. There are significant differences among earthquake-related variables in Panel B. Severe counties received stronger earthquake intensity and registered much higher earthquake-related death rates. Severe counties are 121 kilometres closer to the earthquake epicentre than non-severe counties. Overall, the criterion for the cohort-DID model to provide causal estimation is already satisfied.

Next, I propose the by-cohort DID model to understand how the earthquake would influence students of different ages. Similarly, the model is specified as follows:

$$y_{igc} = \alpha + \sum_{j=8, j\neq 15}^{20} \beta_j Severe_c \times \mathbb{1}(Age_g = j) + x_{igc}^{'}\gamma + \eta_g + \sigma_c + \varepsilon_{igc} \tag{2.2}$$

where j denotes the individual's age in 2008 ranging from 8 to 20. The rest settings remain the same as in Eq. (2.1), and standard errors are clustered at the county level. The coefficients, β_j, capture the differential impact of the earthquake on educational outcomes for cohorts residing in severe versus non-severe counties, relative to the age 15 cohort in the earthquake year. Under my assumption, I expect β_j (where $j \in [16, 20]$) will be insignificant. This is because the older cohorts usually already completed compulsory education by the time the earthquake struck, making them less susceptible to the variation in earthquake severity. The insignificance also indicates the parallel trends of the older cohorts in severe and non-severe counties, making them the counterfactual scenario for the younger cohorts in the absence of earthquake.

2.5 Empirical Evidence on the Earthquake Effect

The empirical analysis evaluates the impact of the Wenchuan earthquake on educational attainment in Sichuan province, with a particular focus on changes in years of schooling to estimate the earthquake effect. Before presenting the regression results, figure 2.1 depicts trends in average years of schooling. The horizontal axis indicates birth cohorts, while the vertical axis represents the cohort-average years of schooling. A vertical dashed line marks the birth year, 1993. Students born in this year would reach 15 years old when the earthquake occurred. Students from severe counties generally demonstrate higher average years of schooling compared to those in non-severe counties. However, a declining trend in years of schooling is evident among the younger cohorts, likely reflecting the age effect: these students may still be completing compulsory education or pursuing studies in high school or university. The gap between the younger cohorts in severe and non-severe counties appears narrower but requires further investigation. The subsequent analysis employs regression-based methods to provide empirical evidence on the causal relationship between the Wenchuan earthquake and educational attainment.

2.5.1 Baseline Results

The primary assumption of the empirical framework is that the older cohorts are statistically similar across severe and non-severe counties. Therefore, any changes in the educational attainment of the younger cohorts in severe counties can be attributed solely to the earthquake. Table 2.2 reports the estimates from a standard cohort Difference-in-Differences (DID) model. Column (1) shows a significantly negative coefficient of -0.353, indicating that the younger cohorts in severe counties attained 0.35 fewer years of schooling due to the earthquake. Including individual and household-level control variables in columns (2) and (3) does not alter the results, as the estimates remain consistently negative.

Using the specification in column (3), the reduction represents approximately 3.7% of the average 9.83 years of schooling observed for the younger cohorts in non-severe counties. Compared to the literature, the size of this decline can offset about half of the educational gains attributed to school construction in Duflo (2001) and exceed the adverse effect of China's access to the WTO on educational attainment documented in Lin and Long (2020). This comparison highlights the substantial impact of the Wenchuan earthquake on education.

$\hat{\beta}$ here should be considered as an conservative estimation of the negative impact on educational attainment. First, although I have excluded migrated individuals from the sample, migration may still occur between the earthquake year and the survey year, particularly in severe counties. Better-educated students are likely relocate for education or employment, which would result in an underestimation of the negative impact on the younger cohorts remaining in severe counties. In the robustness check, I will fix this issue and test the consistency of the baseline results. Second, the older cohorts in severe counties could also be negatively affected by the earthquake. For instance, some might be still in junior high school when the earthquake struck because they have repeated grades before. Others may choose not to pursue higher education, such as university, due to the earthquake. Taken together, these factors suggest that the earthquake effect can decrease educational outcomes by more than the baseline estimation.

Next, I present the by-cohort results from the Wenchuan earthquake on youth education, using the specification of Eq. (2.2). Figure 2.2 plots the estimates with their 95% confidence intervals when considering all controls.[10] Each dot represents the estimated coefficient of the interaction term between the severe dummy and the age dummy in 2008. Since β_{15} is omitted due to multicollinearity, any cohort-level difference in the outcome between severe and non-severe counties has already been adjusted to the difference of the age 15 cohort. One advantage of analyzing the by-cohort effect is to understand the impact of the Wenchuan earthquake on students across different ages. From the figure, those insignificant estimates for students above the age of 15 indicate the Wenchuan earthquake barely influences the older cohorts in educational attainment, which again reinforces the parallel trends assumption. By comparison, the younger cohorts in severe counties attain significantly fewer years of education, which is aligned with the cohort-DID result described above.

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[10] In the Appendix, table B2 documents the detailed estimations.

2.5.2 Heterogeneity Tests

Even among the younger cohorts in severe counties, the earthquake's impact on educational attainment may vary according to personal characteristics. To examine this heterogeneity, I adopt a triple-difference strategy by augmenting the baseline specification with an interaction term: $Severe \times \mathbb{1}(Age \leq 15) \times \mathbb{1}(Character = 1)$.

Table B3 reports the estimates from these heterogeneity tests by personal characteristics. Column (1) indicates that young male students in severe counties suffer a larger reduction in years of schooling than their female counterparts. This result is also supported by Liu and Xu (2021); Column (2) reveals a surprising outcome: the reduction in schooling is more pronounced among young Han students in severe counties. Finally, column (3) shows no significant difference between young rural and urban students in terms of the negative earthquake shock.

2.5.3 Robustness Checks

In this section, I conduct several robustness checks for the baseline results. According to the identification assumption in the cohort DID model, there should be no divergence in education between the older cohorts in severe and non-severe counties.

Alternative Timings

In table 2.2 column (4), I modify the older cohorts by excluding those older than 18 in 2008. The new older cohorts are only marginally older than the younger cohorts, and they often operate within a similar educational environment. Despite the consistency in the estimate, the magnitude implies a larger decreasing effect on the younger cohorts in severe counties.

Some respondents complete compulsory education at ages earlier or later than 15, which constitutes an exception to the assumption that students finish at 15. I redefine the cohorts in column (5): respondents aged 8 to 13 represent the younger cohorts, while those aged 18 to 23 constitute the older cohorts. The estimate is still significantly negative.

Then I assume the earthquake occurred in 2003/2004/2005, and the younger cohorts now are defined as students under 15 in 2003/2004/2005. From columns (6) to (8), the insignificant estimates suggest that the timing of the earthquake plays a critical role in understanding the effect on education. And the baseline results are not spurious.

Alternative Treatment

Another concern is that the treatment dummy ("severe county") may capture endogenous factors that correlate with educational outcomes. For instance, the evaluation of geological disaster risk, contributing 10% to the disaster index, may not be entirely objective and could be influenced by subjective judgment. To address this concern and complement the baseline results, I present the estimates in table 2.3 by altering the treatment dummy to three continuous variables: earthquake-related death rate, earthquake intensity scale, and the linear distance to the epicentre. To account for counties with no recorded deaths, I modify the death rate by taking the natural log of 0.0001 plus the death rate per ten thousand persons.

Columns (1) and (2) report significantly negative estimates when using the earthquake-related death rate, with or without control variables. This indicates that the younger cohorts in counties with higher death rates from the earthquake tend to have fewer years of schooling, highlighting the adverse effect of the earthquake on education. This claim is also supported in the negative estimates in columns (3) and (4) when using earthquake intensity scale. The stronger the younger cohorts' exposure to earthquake is, the larger the reduction in educational attainment will be.

The third specification replaces the treatment with the linear distance to the epicentre, shown in columns (5) and (6). The estimates are significantly positive, suggesting that the younger cohorts in counties further from the epicentre attain more years of schooling. The different signs are consistent with the expectation that counties closer to the epicentre experience more severe impacts.

Alternative Specifications

I conduct several more robustness checks to validate the declining effect of the earthquake on educational attainment, with results presented in Table B4. In column (1), I redefine the younger and older cohorts by using age 18 as the threshold instead of 15. The insignificant estimate suggests that high school students are less impacted by the earthquake compared to those in junior high or primary school. This supports the conclusion that the declining effect on educational attainment primarily affects young students (Dercon and Porter, 2014; Wang et al., 2017; Shidiqi et al., 2023; Huang and Dong, 2025). In column (2), I apply county-level distance to the epicentre as weights to re-estimate the coefficient. Column (3) modifies the specification by clustering standard errors at the prefecture level, as a prefecture is an administrative layer just above a county. In column (4), I exclude respondents residing in urban districts. Across all these alternative specifications, the estimates remain consistent with the baseline results and are significantly negative. In summary, these robustness checks further reinforce the reliability of the baseline findings.

Migration

The baseline identification assumes that respondents' counties of residence in the 2015 survey match their counties of residence at the time of the 2008 earthquake, thereby overlooking potential migration over the 7 years. In Section 1.5.3, I demonstrate that large-scale migration is unlikely following the earthquake at the county level. Nonetheless, individual migration decisions may still bias the cohort-DID estimation. In the 2015 survey, respondents report their permanent residence from 5 years earlier. I define migrants as those who indicate a change in county of residence during that period, about 8% of the sample. Although this imperfect method does not capture migration between 2008 and 2010, it represents a significant improvement over the original assumption.

First, I remove migrants from the sample and re-estimate the specification in Eq. (2.1). The restricted sample includes only respondents who remained in the same county during the past 5 years. Table 2.4 columns (1) and (2) report significantly negative estimates. The robustness of these results implies that migration has a negligible effect on the identification strategy.

Alternatively, I match respondents' reported residence from 5 years ago with the county severity status. This procedure reassigns migrants originating from severe counties to non-severe counties, or the opposite flow. Columns (3) and (4) present the estimates under the revised specification.[11] The estimates remain largely unchanged compared with the baseline results.

Last, I test whether having lived in severe counties after the earthquake increases the likelihood of migration between 2010 and 2015. In columns (5) and (6), the outcome variable is replaced by a binary indicator of migration. The results indicate that respondents in severe counties are no more likely to migrate than those in non-severe counties.[12] In summary, migration does not threat the identification.

2.6 Discussion

In this section, I examine potential mechanisms behind the decline in educational attainment following the Wenchuan earthquake. One argument claims that the greater financial loss in severe counties reduces affordability of education. Although this claim holds some validity, it fails to explain why the older cohorts in severe counties achieve similar years of schooling as those in non-severe counties.

In China, age 15 marks as a critical threshold because students typically finish the 9-year education by that age. This threshold distinguishes the older cohorts from the younger ones in this study. The Compulsory Education Law prevents young students from dropping out early, regardless of personal preferences. The students only leave school after fulfilling the mandatory requirement. In contrast, education beyond junior high school is optional. The older cohorts face no such legal restriction and may enter the labour market if they choose not to pursue further education. Consequently, those who continue their education among the older cohorts form a self-selected group that is less vulnerable to external shocks compared with the younger cohorts.

To examine this mechanism, I use the framework of Eq. (2.1) while replacing binary educational outcomes that denote the attainment of various educational stages. This approach allows an investigation into differences in education completion. Table 2.5 reports the corresponding estimates.

Column (1) employs a binary indicator equal to 1 if a respondent has completed or is currently receiving tertiary education. However, since some students may still be in

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[11] Note that "county" in the county-fixed effects refers to the county of residence five years earlier. It is the same in columns (5) and (6) as well.

[12] The controls only include personal characteristics and exclude household conditions.

junior high school at the time of survey, and it is unclear whether they will eventually obtain tertiary education. To partly mitigate this issue, I restrict the sample to individuals with at least 9 years of schooling.[13] The estimate is statistically indistinguishable from zero, suggesting that the earthquake does not reduce the likelihood of attaining tertiary education for the younger cohorts.

Column (2) assesses the earthquake effect on high school education, while column (3) broadens the definition by including students currently attending high school. The results indicate that younger cohorts in severe counties are less likely to complete or pursue high school education. In column (4), the full sample is used with the outcome variable indicating junior high school completion. The estimate in this specification shows no significant negative effect, implying that the earthquake does not hinder the completion of junior high education.

Additionally, table B5 present the evidence of the earthquake effect on school dropout after the completion of the 9-year compulsory education. To ensure comparability, I restrict the sample to respondents with at least 9 years of schooling, as these have faced the decision between continuing education and entering the labour market. The estimates in columns (1) and (2) are significantly positive, indicating that the earthquake increases the likelihood of dropout among the younger cohorts in severe counties. I also examine the potential effect of the earthquake on early marriage in columns (3) and (4) by limiting the sample to respondents aged over 20.[14] The estimates suggest that younger cohorts in severe counties do not exhibit a higher propensity for early marriage.

Overall, these findings support that the earthquake' s negative impact on education is most pronounced at the high school level among younger cohorts. The Compulsory Education Law has effectively prevented dropouts prior to high school. However, once the legal requirement ends, students in severe counties show a higher likelihood of discontinuing their education.

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[13] Alternatively, one may restrict the analysis to cohorts born between 1993 and 1997, as these individuals are more likely to have completed high school if they did not drop out after junior high school. The result remains similar.

[14] In China, the legal age for marriage is 20 for women and 22 for men. At the time of the survey, only men in the 1993 cohort reached 22. For simplicity, I restrict the sample to respondents above 20. The results remain similar when running regressions by gender separately.

2.7 Conclusion

Experiencing disruptive events during childhood can have negative consequences, yet isolating their specific effects from other socioeconomic factors poses a significant challenge. In this study, I exploit the exogenous exposure to the 2008 Wenchuan earthquake to examine how such a large-scale disruption influences educational attainment.

I employ a cohort Difference-in-Differences (DID) model that exploits variation in both earthquake exposure and student age at the time of the disaster. The leading hypothesis is that students younger than 15 years old at the time of the earthquake , typically in the last year of compulsory education, respond differently to the earthquake compared to the older cohorts.

The result reveals that the younger cohorts (15 years old and below) in severe counties attain 0.36 fewer years of schooling relative to their counterparts in non-severe counties,a reduction of 3.7% in educational attainment. In contrast, older cohorts display no significant differences in schooling between severe and non-severe counties. These main findings remain robust under various specifications.

The completion of compulsory education offers a plausible explanation. The younger cohorts may opt to enter the labour market after the 9-year compulsory education. In contrast, the older cohorts who continue their education are a self-selected group that is less affected by the earthquake. This study thus provides novel evidence on the impact of an early-life disruptive event on human capital accumulation.

Tables

Table 2.1: Summary Statistics of the Older Cohorts

	(1) Non-Severe Counties	(2) Severe Counties	(3) Conditional Difference
Panel A: Individual and Household Characteristics			
Years of Schooling	9.886	10.864	-0.303
	(3.276)	(2.984)	(0.307)
Age in 2008	19.484	19.651	-0.046
	(2.203)	(2.231)	(0.116)
Male	0.517	0.502	0.020
	(0.500)	(0.500)	(0.026)
Han People	0.896	0.925	-0.035
	(0.305)	(0.263)	(0.023)
Rural Hukou	0.646	0.710	-0.111
	(0.478)	(0.454)	(0.067)
Number of Rooms	4.520	4.890	-0.939**
	(2.465)	(2.778)	(0.473)
House Area (m^2)	138.608	138.102	-10.645
	(69.720)	(72.112)	(10.288)
Household Size	4.568	4.080	0.305
	(1.669)	(1.425)	(0.187)
Car Ownership	0.142	0.212	-0.006
	(0.349)	(0.409)	(0.038)
Observations	5292	1483	
Panel B: County-Fixed Characteristics			
Earthquake Intensity Scale	5.888	8.132	-2.244**
	(0.838)	(1.630)	(0.195)
Death Rate (/10k persons)	0.260	132.091	-131.8***
	(1.199)	(407.683)	(34.956)
Distance to Epicentre (km)	273.096	151.812	121.3***
	(120.053)	(87.641)	(20.912)
Altitude Difference	2.020	2.446	-0.426
	(1.911)	(1.813)	(0.347)
Observations	134	38	

Note: This table documents the statistical description of the older cohorts (above 15 years old in 2008) between severe and non-severe counties in Sichuan province. In the last column, the mean difference of each variable is calculated conditional on the cohort- and prefecture-fixed effects (except those in Panel B), while *, **, and *** denote the significance levels at 10%, 5%, and 1% respectively.

Table 2.2: The Earthquake Effect on Years of Schooling to Young Students

Dependent Variable: Years of Schooling	Age in 2008: 8-23			Age in 2008: 8-18	Age in 2008: 8-13 and 18-23	If Earthquake was in 2003/2004/2005		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Severe × $\mathbb{1}(\text{Age} \leq 15)$	-0.353**	-0.355**	-0.363***	-0.476***	-0.481***			
	(0.139)	(0.137)	(0.137)	(0.167)	(0.153)			
Severe × $\mathbb{1}(\text{Age} \leq 15 \text{ in } 2003)$						-0.175		
						(0.163)		
Severe × $\mathbb{1}(\text{Age} \leq 15 \text{ in } 2004)$							-0.178	
							(0.142)	
Severe × $\mathbb{1}(\text{Age} \leq 15 \text{ in } 2005)$								-0.169
								(0.141)
Personal Controls		✓	✓	✓	✓	✓	✓	✓
Household Controls			✓	✓	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	12536	12536	12536	8207	9415	12536	12536	12536

Note: The dependent variable is the coded years of schooling at the individual level. The whole sample is used in regressions in columns (1)-(3), but they differ in personal or household controls. In column (4), respondents aged above 18 are excluded. In column (5), respondents aged between 14 and 17 are excluded. Columns (6)-(9) are placebo tests if assuming that the earthquake occurred in 2003/2004/2005. Each column controls for cohort- and county-fixed effects. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote the significance levels at 10%, 5%, and 1% respectively.

Table 2.3: Alternative Measures of Earthquake Exposure

Dependent Variable: Years of Schooling	Death Rate		Earthquake Intensity		Distance to Epicentre	
	(1)	(2)	(3)	(4)	(5)	(6)
ln(0.0001+Death Rate) × $\mathbb{1}$(Age ≤ 15)	-0.042***	-0.044***				
	(0.015)	(0.014)				
Earthquake Intensity × $\mathbb{1}$(Age ≤ 15)			-0.209***	-0.218***		
			(0.052)	(0.049)		
ln(Distance to Epicentre) × $\mathbb{1}$(Age ≤ 15)					0.445***	0.446***
					(0.104)	(0.099)
Controls		✓		✓		✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	12536	12536	12536	12536	12536	12536

Note: This table presents the estimates from the robustness checks, where the dependent variable is individual years of schooling. Columns (1) and (2) record estimates if using the earthquake-related death rate, instead of the severe dummy; Columns (3) and (4) record estimates if using earthquake intensity scale; Columns (5) and (6) use the distance to the epicentre. Each column controls for cohort- and county-fixed effects. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote the significance levels at 10%, 5%, and 1% respectively.

Table 2.4: Robustness Check and Migration Decision

Dependent Variable:	Years of Schooling				Migration	
	Exclude Migrants		Severity Before Migration			
	(1)	(2)	(3)	(4)	(5)	(6)
Severe × $\mathbb{1}(\text{Age} \leq 15)$	-0.355**	-0.361***				
	(0.140)	(0.137)				
Severe County 5 Years Ago × $\mathbb{1}(\text{Age} \leq 15)$			-0.325***	-0.337***		
			(0.137)	(0.135)		
Severe County 5 Years Ago					-0.006	-0.005
					(0.010)	(0.009)
Controls		✓		✓		✓
Cohort Fixed Effects	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓		
Observations	11557	11557	12536	12536	12536	12536

Note: The dependent variable is individual years of schooling from columns (1)-(4). Migrants are excluded in columns (1) and (2); the severity status is changed as the status of county where respondents resided five years ago in columns (3) and (4). In columns (5) and (6), the dependent variable is the binary indicator of migration. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote the significance levels at 10%, 5%, and 1% respectively.

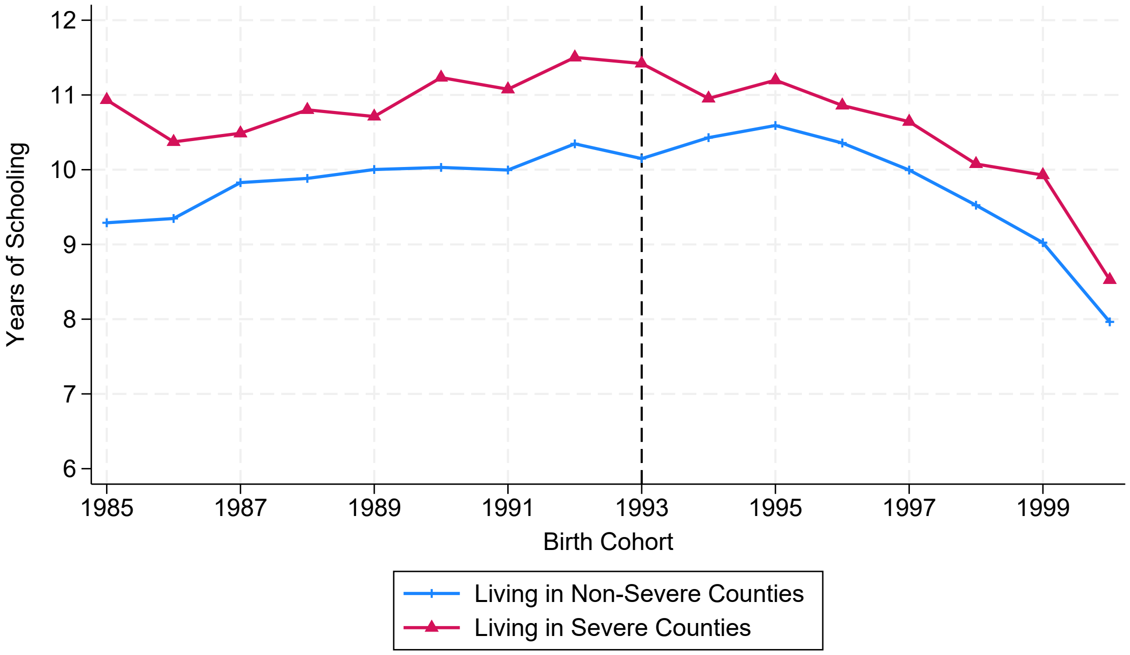
Table 2.5: The Earthquake Effect on Educational Stages

Dependent Variable:	Tertiary Education	High School Education		Junior High School Education
	(1)	(2)	(3)	(4)
Severe × $\mathbb{1}(\text{Age} \leq 15)$	-0.030	-0.063**	-0.070***	0.011
	(0.023)	(0.024)	(0.023)	(0.015)
Controls	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓
Observations	10438	10438	10438	12536

Note: This table presents the impact of the earthquake on the likelihood of holding an educational degree among young cohorts in severe counties. The dependent variable in column (1) is a binary indicator equal to 1 if a respondent has completed or is receiving tertiary education. Similarly, column (2) assesses the earthquake effect on high school education, while column (3) broadens the definition by including students currently attending high school. Only respondents with at least 9 years of schooling are kept from columns (1) to (3). In column (4), the binary indicator equal to 1 if a respondent has completed junior high school education. Standard errors are clustered at the county level, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

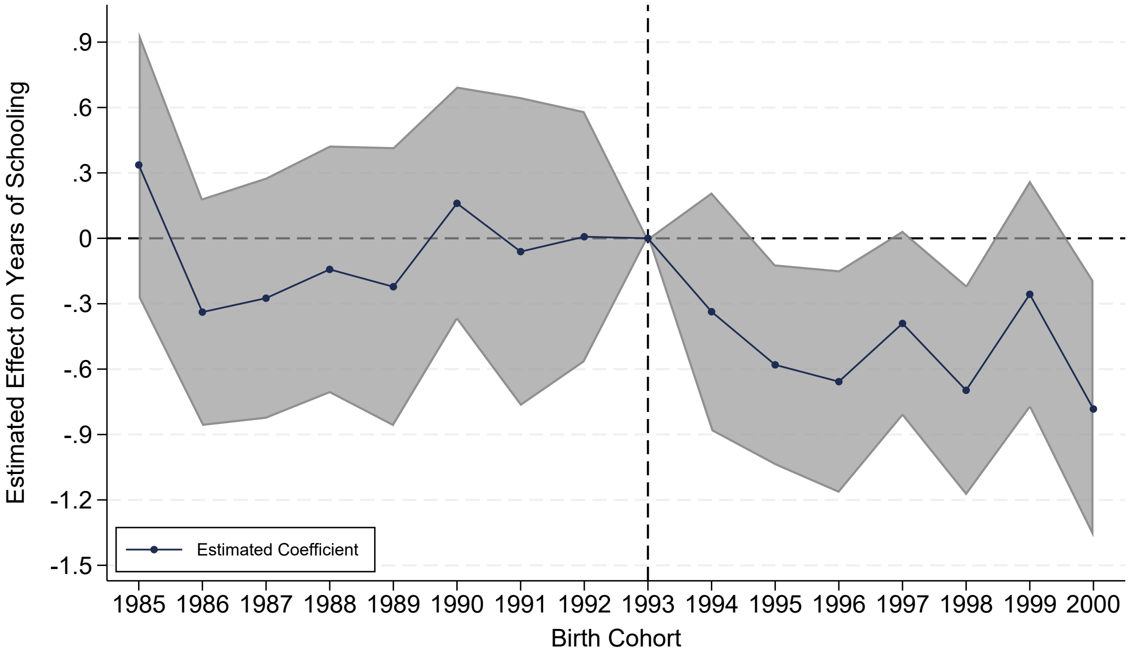
Figures

Figure 2.1: The Trend in Years of Schooling by Different Cohorts



Note: In this figure, each point represents the average years of schooling for a given birth cohort. The vertical dashed line marks the birth cohort who reaches 15 years old when the earthquake occurred. The red and blue connected lines correspond to cohorts residing in severe and non-severe counties as of the survey year, respectively.

Figure 2.2: The Earthquake Effect on Years of Schooling by Cohorts



Note: The figure presents the estimates and their confidence intervals at 95% level from the event-study design. Each point represents the point estimates, with the grey-shaded area indicating the confidence intervals. The horizontal axis indicates birth cohorts of respondents, while vertical dashed line marks the 1993 birth cohort who reach 15 years old in 2008. The regression model controls for personal and household characteristics, as well as cohort and county fixed effects. Standard errors are clustered at the county level.

Chapter 3

Less Poor is More Equal: Evidence from the Targeted Poverty Alleviation Program in China

3.1 Introduction

To make the pie bigger, or to improve the way pie is divided?[1] That is a question about efficiency and equity. Over the past three decades of hardworking efforts, China has transformed from one of the world's most underdeveloped countries to a high middle-income economy. The country has achieved a historic milestone in poverty alleviation by lifting over 770 million people out of absolute poverty, accounting for nearly 70% of global poverty reduction during the same period.[2] This remarkable achievement is illustrated in figure 3.1(a), which shows a dramatic decline in the poverty rate from 88.1% in 1981 to just 0.2% in 2019.[3] However, these efforts have also contributed to an unintended consequence: increasing regional inequality (Jones et al., 2003; Chan et al., 2014), particularly the widening urban-rural divide (Zhao, 1999; Sicular et al., 2007; Zhou and Song, 2016). As shown in figure 3.1(b), while both urban and rural income levels steadily increases alongside the countrywide growth, the gap between them also enlarges. If left unaddressed, the growing urban-rural income disparity

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[1] See from http://www.qstheory.cn/international/2022-12/19/c_1129218316.htm.

[2] See from http://www.china.org.cn/chinese/2021-06/02/content_77513397.htm.

[3] Data obtained from the World Bank and Global Extreme Poverty, see https://pip.worldbank.org/home.

undermines poverty alleviation efforts (Yao et al., 2004; Iniguez-Montiel, 2014) and poses challenges to social stability.

In response to these challenges, the Chinese government started to emphasize on equitable distribution of income and wealth. Launched in 2015, the Targeted Poverty Alleviation (TPA) program explicitly expressed its goals to eradicate absolute poverty but also reduce economic disparities in underdeveloped regions. Earlier poverty alleviation efforts often prioritized overall growth and overlooked the by-product effect of deepening urban-rural divide. The TPA program adopts a more targeted approach in rural area. For example, it identifies impoverished households and then addresses the root causes of poverty through tailored support measures. The program is expected to largely improve rural income levels since the majority of the poverty-stricken population resides in rural areas (Park et al., 2002). In urban areas, its impact on income levels may be negligible due to its rural-oriented nature. Then it can be inferred that the program also contributes to narrowing the urban-rural income gap. This outcome is also closely aligned with the guideline of "common prosperity".

This study aims to empirically test whether the TPA program has succeeded in achieving this anticipated outcome. In the empirical analysis, I assess the impact of the Targeted Poverty Alleviation (TPA) program on urban-rural income inequality using a Difference-in-Differences (DID) approach. Income inequality is measured as the income ratio of the urban net income per capita to rural net income per capita. Given that the TPA program primarily targets poverty counties, I leverage the variation in the poverty status to estimate the program effect on the income ratio. However, an outstanding concern of endogeneity arises in the estimation because the poverty status is largely related to economic conditions years before the program. Or the process of the designation is not considered random. If ignored, the endogeneity would bias the estimation from a standard DID model. Thus, I employ the Propensity Score Matching (PSM) method to reduce pre-program differences and then match comparable counties just with different poverty statuses.

After balancing pre-program differences with the PSM weights, the main findings reveal that the TPA program reduces the income ratio in poverty counties by 3.8% relative to non-poverty counties. Besides, the PSM weights can correct a 77% overestimation observed in the unweighted model, demonstrating the need to address initial imbalances. The results in the event-study design confirm the parallel trends assumption required by the DID approach and display a downward trend in the income ratio. In sum, the TPA program has effectively reduced urban-rural income inequality in poverty counties.

Place-based policies like the TPA program might extend effects on neighbouring non-poverty counties (Lu et al., 2019), which leads to an underestimation of the real program impact. To address this, I consider a proxy for non-poverty counties in prefectures with at least one poverty county and test whether they are affected by the program. And the results are against this possibility. This finding reinforces that the reduction in urban-rural income inequality should be a direct outcome in poverty counties, rather than being driven by external or indirect factors.

I explore three mechanisms that help explain the main findings of this study. First, I build on the rural-oriented nature of the TPA program and hypothesize that its implementation primarily benefits the targeted group–the rural poor (Rozelle et al., 1998; Montalvo and Ravallion, 2010; Meng, 2013). I examine changes in both urban and rural net income per capita, and the results suggest that only rural net income per capita significantly increases in poverty counties. The scale is approximately equivalent to 8.3% of the poverty line at the time. By contrast, the program has little effect on the urban income level.

Second, I present evidence that the increase in rural incomes is linked to improved job opportunities in rural areas. Similar to the income channel, the TPA program significantly boosts rural employment in poverty counties, while urban employment remains unaffected. The income and employment channels reinforce the rural-oriented design of the program and its targeted impact on poverty alleviation.

Last, I examine the role of county governments, which act as the core administrative units responsible for implementing the program (Fan et al., 2000; Zhu et al., 2021). My finding reveals a significant increase in fiscal expenditure per capita in poverty counties relative to non-poverty counties following the program. This result highlights the crucial role of local governments in allocating resources and ensuring the successful execution. Their active involvement in the program reflects their commitment to achieving its poverty reduction objectives.

This study contributes to the literature in the following ways. First, it expands the growing body of research on China's poverty alleviation efforts by introducing income inequality as a novel perspective. Since the Chinese government officially initiated poverty alleviation programs in 1986, previous studies have typically examined efficiency-related outcomes such as economic development and income gains (Rozelle et al., 1998; Montalvo and Ravallion, 2010; Meng, 2013; Zhang et al., 2021, 2023), political influence (Li and Wu, 2022; Han et al., 2022), infrastructure (Qin and Chong, 2018; Xiao et al., 2022), or environmental influence (Yuan and Wang, 2021). Relatively, few have examined program effects through the lens of inequality and social justice. This study addresses this critical gap in the literature by providing empirical evidence that the TPA program has directly contributed to narrowing urban-rural income disparities within poverty counties. The analysis also enriches the current understanding of broader social impacts in poverty alleviation programs.

Second, the topic of this study is closely related to rising inequality during economic development, while the analysis may provide a solution to it. Previous poverty alleviation programs in China often widened urban-rural income inequality (Ravallion and Chen, 2007; Zhang, 2021), despite their success in lifting individuals out of poverty. By contrast, the TPA program primarily delivers resources to rural areas. The findings here show that only rural income and employment increase, with no impact on urban areas. At the county level, adjacent non-poverty counties also remain unaffected. Both highlight the program's place-based and well-targeted design. These results align with boarder evidence that targeted policies can reduce inequality without causing unintended positive spillovers (Dahl and Lochner, 2012; Chetty and Saez, 2013; Li, 2014; Bastian, 2020).

It should be noted that this study is different itself from (Tang et al., 2022; Zhou et al., 2023) in several key aspects. First, the county-level sample provides finer details than the prefecture-level data.[4] Using county-level data not only provides a larger number of observations and greater degrees of freedom but also ensures a more accurate capture of variations in poverty status designation at the county level, given that the TPA program's designation of poverty status took place at the county level. Second, I apply stringent criteria in sample selection by excluding coastal provinces in the east region and county-level districts (*"qu"*). Coastal provinces in East China

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[4] In China, a prefecture is usually the direct leader of several counties.

are historically at the forefront of economic development in the country, thus their exclusion prevents potential overestimation of the program effect by focusing on regions more representative of the national landscape. Similarly, the exclusion of county-level districts ensures that the analysis focuses on the "real" county-level comparisons, minimizing potential confounding factors from different layers of administration. Last, I incorporate time trends in the analysis to address concerns regarding differences in initial conditions between poverty counties and non-poverty counties. While propensity score matching (PSM) helps minimize differences between them, weighted samples may still present challenges in achieving optimal comparability. I ensure that the estimation remains robust and unbiased conditional on the inclusion of different time trends, thereby enhancing the reliability of the study's findings.

The subsequent sections of this paper are organized as follows: Section 3.2 provides an overview of the Targeted Poverty Alleviation program. Section 3.3 builds the conceptual framework for this study and explain why the program works on reducing the urban-rural income inequality. Sections 3.4 and 3.5 describe the data sources and methodology for the empirical analysis, with the results outlined in Section 3.6. Section 3.7 discusses some potential mechanisms to explain the primary findings, while Section 3.8 concludes.

3.2 Background: Targeted Poverty Alleviation

Poverty alleviation in China has historically been closely tied to rural development, as over 80% of the population resided in rural areas in 1978. I briefly summarize the historic account of poverty alleviation in the Appendix C.1, which lays a groundwork for future works.

In earlier decades, the "broad and general" approach adopted in poverty alleviation may have been effective when a substantial share of the population lived below the poverty line. However, it became increasingly inefficient as the poverty rate declined. Local officials often lacked clarity about the exact number of poverty-stricken individuals within their jurisdictions due to ambiguous criteria. And eligible households failed to receive necessary assistance. Therefore, the challenge of uplifting the remaining impoverished population was particularly difficult, considering that these individuals had remained in poverty despite previous programs. Given this situation, the Targeted Poverty Alleviation program was launched in 2015, marking China's final phase in the fight against poverty.[5] The TPA program introduces a clear and systematic identification process for poverty-stricken individuals. The local governments are required to establish a comprehensive database detailing impoverished households through in-depth inquiries at the grassroots level. This database is frequently updated to ensure accuracy by adding or removing beneficiaries as circumstances changed.[6] This shift represented a significant improvement in poverty alleviation practices by targeting individual households and identifying the specific causes of their poverty. In short, the TPA program is tailored to support the rural poor directly with more effective allocation of resources. At the same time, the urban-rural inequality is expected to narrow under this framework of "common prosperity".

Counties serve as the core administrative unit in the TPA program.[7] Officially designated as "national poverty alleviation and development priority counties", or simply "poverty counties", these areas are the primary recipients of fiscal transfers from higher levels of government and bear the responsibility for alleviating poverty at the grassroots level. In 2014, 592 counties are designated, approximately 20% of the total county-level administrative units in China.[8] The list of those poverty counties is accessible on the official website of the National Rural Revitalization Administration.[9] Though the exact criteria of the designation does not become public, counties with disadvantaged economic conditions, such as higher percentages of the poverty-stricken population, are more likely to be designated. Table C1 summarizes the distribution of poverty counties, with an average ratio of 28% across 21 middle and western provinces. However, they are discernibly different in the distribution of poverty counties: the two south-western provinces (Guizhou and Yunnan) exhibit poverty county ratios of around 57%,

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[5] The concept of "targeted poverty alleviation" was introduced during president Xi Jinping's visit in 2013. See from `http://cpc.people.com.cn/n1/2022/0625/c444826-32456402.html`.

[6] The central government also revised the poverty line to align with current development standards and price levels. In 2015, the annual net income threshold was set at 2800 RMB, approximately $2.2 per day. See `http://politics.people.com.cn/n1/2015/1215/c70731-27932806.html`.

[7] Counties here encompass all count-level administrative units, including county-level cities. For brevity, "counties" will be used to refer to all such units.

[8] Counties in the Xizang Autonomous Area, also known as Tibet, are not included in the list due to their highest average altitudes and unique culture. Instead, they belong to another development program.

[9] Formerly known as the State Council Leading Group Office of Poverty Alleviation and Development before February 2021.See from `https://nrra.gov.cn/art/2012/3/19/art_50_23706.html`.

while Heilongjiang in a northeastern region records the lowest ratio at 11%. Figure 3.2 presents the overall geographic distribution of poverty counties (indicated by black dots) in China, primarily concentrated in the inland middle and western regions. In contrast, coastal provinces in the east do not host any poverty counties, owing to their robust economic performance. It is important to note that the absence of a "poverty county" designation does not imply the complete absence of poverty-stricken individuals in those regions. Still, both table and figure underscore the regional disparities prevalent in China.

In practice, poverty counties extend targeted support to villages and poverty-stricken individuals through multiple aspects. These include direct assistance for improving livelihoods, enhancing agricultural production, and providing access to essential services such as healthcare and education. Beyond household-level interventions, local governments in poverty counties also undertake significant infrastructure development projects aimed at fostering long-term economic growth. These efforts include constructing highways to improve connectivity, providing electricity in some remote rural areas, and renovating dilapidated houses. Those initiatives not only address immediate needs but also lay the foundation for sustainable rural development by creating better economic opportunities and living conditions.

3.3 Conceptual Framework

In this section, I develop a conceptual framework grounded in the existing literature and economic theories to explain how the Targeted Poverty Alleviation program could reduce urban-rural income inequality. I begin with a simple model where ID_i denotes the urban-rural income difference in county i. The income difference can be expressed as a function of urban and rural net income per capita:

$$ID_i = F(U_i, R_i)$$

where U_i and R_i represent the urban and rural net income per capita, respectively, and $F(\cdot)$ is a continuous and differentiable function. To ensure that income differences respond as expected to changes in income, the partial derivatives of $F(\cdot)$ with respect to U_i and R_i satisfy the following conditions:

$$\frac{\delta F(\cdot)}{\delta U_i} > 0; \quad \frac{\delta F(\cdot)}{\delta R_i} < 0$$

These conditions imply that an increase in urban income deepens income inequality, while an increase in rural income narrows the gap.

The implementation of the TPA program introduces an exogenous intervention in a representative poverty county, potentially influencing both urban and rural income levels. Specifically, let $U_i = U(p_i)$ and $R_i = R(p_i)$, where p_i is a binary indictor that equals 1 if county i is designated as poverty county. The impact of the TPA program on urban-rural income inequality can then be expressed as:

$$\frac{\delta ID_i}{\delta p_i} = \overbrace{\frac{\delta F(\cdot)}{\delta U_i}\frac{\delta U(p_i)}{\delta p_i}}^{\text{spillover effect}} + \overbrace{\frac{\delta F(\cdot)}{\delta R_i}\frac{\delta R(p_i)}{\delta p_i}}^{\text{direct effect}}$$

This framework provides a theoretical foundation for understanding how the TPA program could reduce urban-rural income inequality. Based on this decomposition, the TPA program primarily targets rural poverty, focusing on improving income levels in rural areas. In the model, this implies no spillover effect, or $\frac{\delta U(p_i)}{\delta p_i} = 0$. In practice, the majority of poverty-stricken individuals reside in mountainous central and western regions, where public infrastructure remains underdeveloped and resources are relatively scarce (Liu et al., 2017; Xiao et al., 2022). The program prioritizes these underprivileged households as its main beneficiaries (Chen et al., 2009; Qin et al., 2021). Local governments often facilitate the targeted interventions, including relocation from isolated and remote ares to places with better access to employment opportunities, education, and essential services (Zhang et al., 2023). These measures are narrowly focused on the rural poor and do not extend to groups outside the targeted population, reflecting the program's rural-oriented nature.

Second, the program also improves the productivity by providing training sessions. During the TPA program, poverty alleviation coordinators, many of whom are local civil servants, are matched with poverty-stricken households and provided training or employment information, thereby assisting villagers in securing better-paying jobs (Zhang et al., 2021). The improved productivity usually translates into a wage rise, even with reduced working hours (Bandiera et al., 2017). The income gains generated under the TPA program are predominantly concentrated within the agricultural sector, which remains the primary livelihood for many rural households (Rozelle et al., 1998; Montalvo and Ravallion, 2010). By increasing the earnings of farming-dependent households, the program enhances rural income levels. In the model, this is represented as $\frac{\delta R(p_i)}{\delta p_i} > 0$. Under the assumption of no spillover effect on urban income, the increase in rural income leads to a narrowing of urban-rural income inequality.

Lastly, the implementation tends to improve bureaucratic management in rural areas. Within the current political structure in China, local leaders' career prospects are closely tied to their performance of governance (Li and Zhou, 2005; Bardhan, 2020), incentivizing efforts in poverty alleviation once their districts are classified as poverty counties. Furthermore, under the national anti-corruption campaign, increased transparency in governance has reduced the likelihood of staying in poverty (Han et al., 2022). New village officials, typically more educated and open-minded than their predecessors, have positively influenced village development, leading to increased registrations of low-income villagers, individuals with disabilities, and recipients of poverty subsidies (He and Wang, 2017). Coordinators in the TPA program are required to pay regular visits and identify the causes of poverty for each targeted household. Some might leverage their positions within local government organizations to more effectively aid households in overcoming poverty (Zhang et al., 2021).

3.4 Data

I mainly rely on county-level data to conduct an empirical analysis. The primary source comes from *China Statistical Yearbook (County-level)*, compiled annually by the National Bureau of Statistics of China. These yearbook data encompass various indicators, including local GDP per capita, the agricultural share of GDP, the industrial share of GDP, fiscal conditions, and more. I also refer to county annual *Statistical Communique on National Economic and Social Development* and the *Report on the Work of the Government* for complementing missing values in some areas.[10] I use provincial

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[10] The two reports usually summarize the works and achievements in the past year and the goals in the current year. Despite the extra effort, certain indicators are still absent from local government reports.

consumer price index to adjust monetary indicators to the 2010 price level. With the yearbook data, I could match the treatment status with county characteristics.

I select 2019 as the endpoint of the study period for two primary reasons. First, the data collection concludes just before the global outbreak of COVID-19 in early 2020. The pandemic introduced significant disruptions, such as lockdowns and quarantine policies, which could distort economic growth patterns and complicate the interpretation of program effects. Second, the Targeted Poverty Alleviation (TPA) program officially completed in 2020. Using 2019 as the endpoint avoids potential distortions from an intensified push in the final year to meet its targets, promising a more consistent evaluation of the program effect.

The last source is geographic data, including each county's longitude and latitude, obtained from National Geomatics Center of China. Besides, each county is matched with the relief degree of land surface (RDLS) data to account for average altitude difference in the county (You et al., 2018).[11]

To ensure regional comparability, I first excluded eight eastern provinces where no poverty counties are located.[12] I then excluded five additional provinces due to extensive missing data.[13] Finally, I omitted city districts within prefectures, referred to as "*qu*," because they lack significant rural areas. Including these districts could introduce noise and overestimate the treatment effect. As a result, my sample consists of 1,183 counties, including 415 designated poverty counties and 768 non-poverty counties.

The focus of this study is the income ratio within a county, calculated as the ratio of urban net income per capita to rural net income per capita (Zhang, 2021).[14] Each year officers from the local Bureau of Statistics conduct a household sample survey to record economic and livelihood-related indicators. For instance, a selected urban household reports wage, operational income, property income, and transfer income, collectively referred to as household total income. Urban net income per capita is estimated by deducting personal tax and social security contributions from the sum, divided by the

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[11] Data on China's RDLS can be accessed at `https://www.geodoi.ac.cn/doi.aspx?Id=887`.

[12] These wealthier provinces include Beijing, Tianjin, Shandong, Jiangsu, Shanghai, Zhejiang, Fujian, and Guangdong.

[13] Yunnan, Xinjiang, Gansu, and Qinghai were removed for this reason, while Xizang (Tibet) was excluded as it was not part of the TPA program, as explained in the background section.

[14] The term "county" itself is a broader concept than in casual conversation, referring not only to the core/urban area but also the surrounding rural regions. The official name for the urban area is "town/county district," signifying the dual structure within Chinese counties.

number of family members. Rural households also report those income sources, but their expenses slightly differ.[15] This income ratio, presented in the aforementioned form, is advantageous as it remains unaffected by provincial and yearly price level variations, serving as a reasonable proxy for urban-rural income inequality on average.

Figure 3.3 illustrates the trends in the income ratio over the period from 2010 to 2019, and the vertical line marks the year of program implementation in 2015. Counties have become more equal in the urban-rural income distribution, which fits the tune of inequality reduction proposed by the central government. Nonetheless, poverty counties maintain a higher income ratio throughout the study period, which implies the reality in China: the within-county income inequality is more pronounced in poorer regions. Besides, the visual inspection provides limited information about whether the trends appear parallel prior to the program implementation, nor whether the income ratio in severe counties declines quicker after.

3.5 Identification Strategy

In this section, I introduce a Difference-in-Difference (DID) approach to estimate the impact of the TPA program on urban-rural income inequality. The identification strategy leverages the variations in the poverty status and program implementation timing. The DID framework relies on two key assumptions.

First, the poverty designation for counties should remain consistent throughout the study period. Some counties might lose their poverty status before the program ends if their poverty rates decrease to an extremely low level or rural income levels substantially rise. However, their economic improvement does not lead to immediate exclusion from the program because local governments still prepare for a final verification survey which will evaluate achievements and correct potential misclassifications.[16]

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[15] The net income equals to the total income minus operational expenses, various taxes, depreciation of productive fixed assets, and net expenditures on gifts to relatives and friends. See from `http://tjj.gxzf.gov.cn/zt/ywzt/ywzttjzs/tjllzs/t18715591.html`.

[16] According to some news reports, removing "the hat of poverty counties" before 2020 feels similar as passing the midterm exam of poverty eradication, but the utmost final exam will be placed in the later half of 2020. It barely implies no poverty-stricken population within the county, while poverty alleviation efforts will still be carried. The exclusion from the list of poverty counties has more nominal meanings to local governments. See from `http://paper.people.com.cn/zgjjzk/html/2020-04/15/content_1983276.htm` and `https://www.xinshao.gov.cn/xsxfpb/xfpcx/202401/58408dec99524431928f75a1a8c5c6e0.shtml`.

As these counties remain de facto poverty counties, I assume they continue to receive the program effect until 2020.

Second, making causal claims requires counties should be comparable before the program despite the poverty status, which is unlikely to hold in this study. In the designation of poverty counties, the central government usually selected from candidates with weaker economic development. Table 3.1 compares means and standard deviations of some county-level variables from 2010 to 2014, just before the implementation. The last column records the differences between poverty and non-poverty counties with the year fixed effects. Overall, disparities are evident across several dimensions. Urban income in poverty counties reaches to 3.35 times rural income, compared to a lower ratio of 2.39 in non-poverty counties, consistent with figure 3.3. Differences in GDP per capita and sectoral composition further highlight the economic disadvantage of poverty counties, which tend to be less industrialized and economically weaker. This economic gap extends to the fiscal domain, as poverty counties generate lower fiscal revenue per capita, but their governments spend at similar scales. Geographically, poverty counties tend to have greater altitude variations and are located farther from the nearest ports, reinforcing the notion that these counties are typically situated in mountainous or remote areas. In conclusion, the non-randomness in the poverty status can bias the estimation if not addressed.

3.5.1 Propensity Score Matching

To mitigate the concern of pre-existing differences between poverty and non-poverty counties, I augment the simple DID model with the Propensity Score Matching (PSM) strategy. It involves matching observations with similar characteristics but in different groups by estimating the probability of treatment assignment for each observation (Rosenbaum and Rubin, 1983). In this paper, I employ a logit regression model to predict the score, $\pi_i = Pr_i(NP = 1|X_i)$, representing the likelihood of being treated, with a set of covariates. Next, I use the default kernel matching strategy and maintain the weights for poverty counties as one, while assigning inverse probability weights for non-poverty counties as $\pi_i/(1-\pi_1)$ (Ertefaie and Stephens, 2010). The covariates used to predict propensity scores are designed to capture pre-existing differences between poverty and non-poverty counties, ensuring that the comparisons reflect initial conditions prior to the implementation. These covariates include GDP per capita, agricultural share, fiscal revenue per capita, altitude difference (RDLS), and the distance to the nearest port, all measured during the period from 2010 to 2014.

Figure C4 illustrates the kernel density distributions of the propensity scores for the treatment and control groups. Before matching, the kernel density of propensity scores reveals a clear separation: the distribution of non-poverty counties is concentrated closer to the origin, while that of poverty counties is clustered slightly to the left of 1. This pattern reflects the strong predictive power of the selected covariates in determining the poverty county designation, as the distributions align well with the actual classification of poverty and non-poverty counties. Besides, the improved alignment of the density curves following matching confirms the effectiveness of the PSM strategy in balancing observable differences. Table C2 further presents this improvement by presenting the revised pre-program comparisons. The table reveals that the disparities between poverty and non-poverty counties have been largely reduced across most covariates, except for expenditure per capita and saving per capita.

3.5.2 PSM-DID Model

The previous sections applies the PSM weights to address the comparability between poverty and non-poverty counties, which significantly improve balance across observable characteristics. Building on this foundation, I employ a baseline PSM-DID model as the identification strategy. This model leverages two key sources of variation: (1) whether a county is designated as a poverty county, and (2) whether the Targeted Poverty Alleviation (TPA) program has been implemented. The model is specified as follows:

$$y_{ipt} = \beta NP_i \times Post_t + \delta_1 x'_{i,10} \times Post_t + \delta_2 g(x'_{i,10}, t) + \kappa_i + \theta_p \times \eta_t + \epsilon_{ipt} \tag{3.1}$$

where y_{ipt} represents the outcome of interest in county i, province p in year t; NP_i is the treatment dummy that equals 1 if county i is designated as a poverty county; $Post_t$ is the post-program dummy that equals 1 if the program is in effect ($t \geq 2015$).

I incorporate county-level fixed effects (κ_i) to account for unobserved and time-invariant heterogeneity, such as geographic features. However, trends in outcomes may differ across provinces or initial conditions, potentially biasing the results. I introduce year-province fixed effects ($\theta_p \times \eta_t$) to absorb variations specific to provinces and time periods. Furthermore, I include the interaction between county-level controls (GDP per capita in 2010, agricultural share in 2010, and fiscal revenue per capita in 2010) and different time dummies to account for differential trends driven by initial county-level differences ($x'_{i,10} \times Post_t$ and $g(x'_{i,10}, t)$), following approaches used in Gentzkow (2006); Li et al. (2016); Lu et al. (2023a). The function $g(\cdot)$ captures the form of the time trend (linear, quadratic, or cubic). ϵ_{ipt} represents unobserved shocks to the outcome of interest. In summary, β captures the causal effect of being designated as a poverty county under the TPA program on urban-rural income inequality. Standard errors are clustered at the county level to account for within-county correlation over time.

In the baseline model, it is important to ensure that the estimation of β remains conditionally uncorrelated with the error term. However, there exists another potential source of bias from any extending effect that poverty counties may have exerted on non-poverty counties. If this case indeed exists but remains unaccounted for, it could lead to an underestimation of the program effect. Following the literature (Miguel and Kremer, 2004; Lu et al., 2019), I assume that any potential extending effect is confined within the same prefecture, excluding cross-prefecture or cross-province influences. Consequently, the localized effect under consideration operates at the prefecture level, with its model represented as follows:

$$\begin{aligned} y_{ipt} = {} & \alpha NC_c \times Post + \beta NP_i \times Post_t + \\ & \delta_1 x'_{i,10} \times Post_t + \delta_2 g(x'_{i,10}, t) + \kappa_i + \theta_p \times \eta_t + \epsilon_{ipt} \end{aligned} \tag{3.2}$$

where NC_c will be 1 only if a prefecture has at least one poverty county under its administration. The rest settings follow Eq. (3.1). In this model, α represents the potential spillover effect from poverty counties to non-poverty counties in the same prefecture during the TPA program. Thus, $\alpha + \beta$ is the revised program effect.

3.5.3 The Even-Study Design

In contrast to the previous models that rely on a single key interaction term to estimate the causal effect, an event study design could assess the effect each year and serve as a robustness check. This approach differs from the baseline model because (1) the year dummies in an event study design are typically divided into lag years (before the program) and lead years (after the program) to indicate whether and for how long a program is implemented, and (2) the interaction terms between year dummies and the treatment dummy are derived to validate the practice of the baseline model by showing no pre-program differences and then estimate the program effect by year. By setting 2015 as the reference year, Eq. (3.2) describes the event study model as follows:

$$\begin{gathered} y_{ipt} = \sum_{j=-1}^{-5} \beta_j NP_i \times Lag_{jt} + \sum_{k=1}^{4} \beta_k NP_i \times Lead_{kt} + \\ \delta_1 x'_{i,10} \times Post_t + \delta_2 g(x'_{i,10}, t) + \kappa_i + \theta_p \times \eta_t + \epsilon_{ipt} \end{gathered} \tag{3.3}$$

where y_{it}, NP_i, and $Post_t$ are already defined above; Lag_{jt} is a lag dummy that is only equal to 1 if year t is exactly $|j|$ year(s) before 2015;[17] $Lead_{jt}$ is a lead dummy that is only equal to 1 if year t is exactly k year(s) after 2015.[18] The setting of the controls and types of fixed effects are the same as in Eq. (3.1). Then, β_j and β_k display the change of the outcome in poverty counties as to non-poverty counties each year before and after the TPA program. I expect β_j to be not significant but β_k to be significantly negative. The expectation has twofold implications: First, the insignificance of β_j validates the parallel trend in poverty and non-poverty counties, a critical assumption in using a DID model; Second, the program takes effect in poverty counties by significantly declining the urban-rural income difference.

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[17] Mathematically, $Lag_{jt} = \mathbb{1}(t = 2015 - |j|)$, $j \in \{-1, -2, -3, -4, -5\}$.

[18] Also, $Lead_{kt} = \mathbb{1}(t = 2015 + k)$, $k \in \{1, 2, 3, 4\}$.

3.6 Empirical Results

In this section, I present empirical evidence on the effectiveness of the TPA program in reducing urban-rural income inequality. Each specification in this study will include the PSM weights unless otherwise noted. I begin by showing the results from the standard DID model, which serves as the baseline outcome. Then, the event-study design illustrates the annual effects observed in the post-implementation period. Finally, I conduct several robustness checks to ensure the consistency and reliability of the baseline results.

3.6.1 The PSM-DID Results

Using Eq. (3.1) outlined in the previous section, table 3.2 presents the estimates derived from the PSM-DID model, with the natural log of the income ratio within a county as the outcome of interest. Standard errors are clustered at the county level and reported in parentheses. All specifications include county fixed effects and province-year fixed effects. Across all the columns, the estimates are significantly negative. In column (1), the unweighted estimate is presented for comparison, while column (2) incorporates the PSM weights to balance pre-existing disparities between poverty and non-poverty counties. The difference in magnitude reveals that omitting PSM weights would overestimate the program effect by at least 77%. Columns (3) and (4) introduce additional controls: an interaction term between 2010 control variables and the post-program time dummy, as well as a cubic time trend. Despite these inclusions, the estimated magnitudes remain largely unchanged, suggesting the robustness of the results. For further verification, table C3 in the Appendix presents estimations using linear and quadratic time trends. These results remain highly consistent with the main findings.

As both NP_i and $Post_t$ are binary variables, the coefficient of their interaction term ($NP_i \times Post_t$) captures the causal effect of the TPA program on urban-rural income inequality in poverty counties. The preferred specification in column (3) reveals a 3.8% reduction in the income ratio relative to non-poverty counties following the program.[19] This result is consistent with figure 3.3, which illustrates a steeper decline in the income

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[19] $e^{-0.039} - 1 \approx -0.038$

ratio for poverty counties after 2015. A back-of-the-envelope calculation shows that the program decreases the income ratio by approximately 0.09 in poverty counties, based on the mean value observed in non-poverty counties. In the Discussion section, I will further assess the influence on rural income to provide a more comprehensive understanding of the mechanisms driving this outcome. However, it is important to interpret this estimate with caution, as it reflects the average treatment effect on the treated counties (poverty counties) rather than the entire sample.

3.6.2 The Event-Study Results

Next, I display the estimates with their 95% confidence intervals for each year in figure 3.4, relying on Eq. (3.3).[20] The vertical axis depicts the estimates of β_j and β_k, representing the yearly difference in the income ratio between poverty counties and non-poverty counties relative to the reference year, 2015. The pre-program estimates are statistically insignificant, suggesting that poverty and non-poverty counties demonstrate similar trends in the income ratio prior to the implementation when applying the PSM weights. This validates the parallel trends assumption essential for the DID framework, further affirming the robustness of the baseline results.

In the post-program period, the coefficients β_k turn significantly negative, indicating a strong and consistent program effect in reducing the income ratio. The event study findings not only corroborate the causal impact of the TPA program but also reveal an interesting pattern: the magnitude of the effect grows progressively larger as the program approaches its conclusion. This suggests that the program effect intensifies over time, likely reflecting cumulative benefits from targeted interventions. Overall, these results confirm the effectiveness of the TPA program in narrowing the urban-rural income divide.

3.6.3 Robustness Checks

In this section, I perform several robustness checks to strengthen the validity of the main findings. A key concern arises from the place-based nature of the TPA program,

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[20] In the Appendix, table C4 documents the yearly estimates from the equation.

as the designation of poverty counties might indirectly influence neighbouring non-poverty counties. For example, farmers in non-poverty counties may relocate to nearby poverty counties to take advantage of program benefits. If such extending effects exist, the baseline estimates could underestimate the true program effect. To address this concern, I conduct the analysis by estimating the potential extending effect using the specification in Eq. (3.2). Table 3.3 presents the results. Across the specifications, the extending effect is found to be statistically insignificant, regardless of the inclusion of different control variables. This suggests that neighbouring non-poverty counties are not significantly affected by the program, reinforcing the precision and targeting effectiveness of the place-based policy. Moreover, the coefficient $\hat{\beta}$ remains consistent in both sign and magnitude compared to the baseline outcome, indicating that the standard DID model does not suffer from underestimation of the program effect.

To further reinforce the robustness of the baseline results, I conduct additional checks summarized in table 3.4. First, I address concerns regarding the clustering of standard errors. In columns (1) and (2), I re-estimate β from Eq. (3.1) by clustering the standard errors at the prefecture and province levels, respectively. The estimates remain consistent with the baseline results, confirming that the program effect is not sensitive to the clustering strategy. Next, in column (3), I extend the analysis by exclusively including western provinces in the sample.[21] The results yield a slightly larger estimate than the baseline, suggesting that the TPA program is even more effective in relatively underdeveloped regions. In column (4), I replace the natural log of the income ratio with its level form as the outcome of interest. The estimate remains significantly negative, affirming the robustness of the baseline findings to alternative outcome specifications. Lastly, in column (5), I perform a falsification test by assuming the TPA program was implemented in 2013. Using the sample from 2010 and 2014, I find no significant effect from this "placebo" program. This result again validates the assumption of parallel trends between poverty and non-poverty counties in the pre-treatment period.

To rule out any potential spurious program effect, I conduct an additional test by randomly assigning poverty status to counties and estimating the program effect under this random assignment. In my sample, 415 out of 1172 counties are designated

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[21] These provinces include Neimeng, Sichuan, Ningxia, Guangxi, Guizhou, Chongqing, and Shaanxi.

as poverty counties, representing approximately 35% of the total. To replicate this, I generate a binary treatment variable where each county has a 35% probability of being assigned as a poverty county. This random assignment does not consider any underlying factors, ensuring it is purely random. In this test, the PSM weights are no longer applicable, as the assignment lacks the actual targeting criteria. Therefore, the results should be directly comparable to column (1) of Table 3.2, which reports the unweighted estimate. I then repeat the regression in Eq. (3.1) 1,000 times and document the distribution of the key estimates in Figure 3.5. The horizontal axis displays the estimated coefficients, and the vertical axis represents the fraction of observations within each bin. The solid line depicts the kernel density estimate. The results show that the simulated program effects are minimal and statistically insignificant, with the vast majority of estimates clustering around zero. This pattern stands in stark contrast to the baseline estimate of -0.069, which lies considerably outside the simulated distribution.

Taken together, these robustness checks confirm the reliability of the baseline results and underscore the robustness of the estimated program effect on reducing urban-rural income inequality.

3.7 Discussions

Following the empirical evidence that the TPA program has reduced urban-rural income inequality in poverty counties, I will discuss the mechanisms in this section through which the program effect could be attributed to the implementation.

3.7.1 Income Increase

Based on the conceptual framework, I investigate a potential channel through which the TPA program tends to deliver greater benefits to impoverished individuals compared to their wealthier counterparts (Rozelle et al., 1998; Montalvo and Ravallion, 2010; Chen et al., 2009; Qin et al., 2021). Given that the program adopts a targeted and place-based approach, I hypothesize that the decline in urban-rural income inequality arises primarily from a larger increase in rural income levels within poverty counties.

However, this narrowing of the income gap does not necessarily imply a change in urban income. This deduction aligns with the inherently rural orientation of the program. Therefore, if urban income also increases or decreases, it could indicate a spillover effect from rural to urban areas within a county.

To test this hypothesis, I use the same specification as in Eq. (3.1), replacing the outcome variable with rural net income per capita and urban net income per capita, respectively. Table 3.5 presents the estimation results. In columns (1) and (2), the program leads to a significant increase of 2.4% in rural net income per capita in poverty counties relative to non-poverty counties. This effect remains robust after controlling for time trends. By contrast, the non-significant estimates in columns (3) and (4) suggest that urban income remains largely unaffected, regardless of poverty designation. These findings indicate no evidence of a spillover effect and provide strong support for the hypothesis that the program solely benefits rural areas.

The results further underscore the rural-oriented nature of the TPA program. Given that the average rural income per capita in non-poverty counties is 9,665 RMB, the estimated program effect translates into an increase of approximately 232 RMB in rural income per capita for residents in poverty counties. This is equivalent to 8.3% of the poverty line at that time. Besides, the magnitude of this effect is more than double that of the program evaluated by Meng (2013), which examines another poverty alleviation program implemented between 1994 and 2000.

3.7.2 Employment Expansion

While the income mechanism provides a direct explanation for the baseline results, I explore deeper into understanding the origin of the increased rural income levels here. An apparent explanation for the rise in rural income levels could be attributed to the expansion of job opportunities for the rural labour force. To evaluate this claim, I analyze the change in the number of employees per thousand persons in rural areas, the industrial sector, and the tertiary sector, using the same framework in Eq. (3.1).

Table 3.6 presents the results in columns (1) to (3), the change in the number of employees across different categories. The findings indicate that the program led to an increase in the number of rural employees in poverty counties compared to non-poverty counties. This expansion of job opportunities for the rural labour force may translate into higher income levels over time. Since the industrial and tertiary sectors are predominantly located in urban areas, changes in their employee numbers serve as proxies for urban employment. In line with the rural-oriented nature of the program, there is no significant change observed in urban employment, consistent with the lack of impact on urban income. While some data may be missing, potentially leading to biased estimations, the overall narrative still suggests that improved outcomes in the rural labour market serve as a plausible mechanism for the observed results.

3.7.3 Government Spending Increase

Since the primary implementation of the TPA program is concentrated in poverty counties, county governments receive intergovernmental transfers to facilitate the execution under the existing fiscal system.[22] They should ensure the delivery of targeted interventions, such as infrastructure improvement (Fan et al., 2000). As a result, the program may lead to an increase in government spending in these counties.

To test this, I use Eq. (3.1) to estimate the program effect on fiscal expenditure per capita. The result is presented in column (6) of Table 3.6. The significantly positive coefficient indicates that, following the implementation of the TPA program, poverty counties increases their government spending by approximately 3% relative to non-poverty counties. This magnitude aligns with findings from Zhu et al. (2021), which document a comparable reduction in government spending when counties exit the program.

3.8 Conclusion

In this study, I evaluate the effect of the Targeted Poverty Alleviation program on urban-rural income difference in China, leveraging the county-level data from 2010 to 2019. Before the implementation of the program, poverty counties and non-poverty counties were different across various dimensions, including economic development and

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[22] See more in Appendix C.1.

sectoral composition. I rely on the Propensity Score Matching method to minimize these pre-program imbalances and then employ a Difference-in-Differences model to estimate the effect.

The baseline finding reveals that the TPA program led to a 3.8% greater decline in urban-rural income inequality, measured by the ratio of urban net income per capita to rural net income per capita, within poverty counties relative to non-poverty counties. The event-study analysis corroborates the finding showing consistent results over time and further validating the causal interpretation.

Importantly, the analysis demonstrates that the observed reduction in income inequality is concentrated in poverty counties, as the program does not exhibit spillover effects on neighbouring non-poverty counties within the same prefecture. This finding reinforces the place-based nature of the TPA program and highlights its precision in targeting impoverished regions. Additional robustness checks confirm the stability and reliability of the baseline results.

To explore the mechanisms behind the observed decline in income inequality, I examine changes in rural income, employment, and government spending. First, I find that the rural net income per capita in poverty counties increases by 2.4% relative to non-poverty counties, while urban incomes remain largely unaffected, underscoring the program’ s rural focus. Second, employment opportunities improve in poverty counties, but this improvement is concentrated in rural sectors rather than urban-based secondary or tertiary employment. Together, these results affirm the rural-oriented nature of the TPA program. Finally, I identify a significant increase in government spending per capita within poverty counties during the program period, highlighting the pivotal role of county governments in implementing and sustaining the program.

Tables

Table 3.1: Statistical Description Before the Program

	Non-Poverty counties	Poverty counties	Conditional Differences
Urban-Rural Income Ratio	2.39	3.35	-0.97***
	(0.67)	(0.89)	(0.05)
ln(GDP per capita)	10.08	9.48	0.60***
	(0.64)	(0.49)	(0.03)
Agricultural Share	0.20	0.26	-0.06***
	(0.11)	(0.11)	(0.01)
Industrial Share	0.50	0.40	0.10
	(0.15)	(0.15)	(0.01)
ln(Revenue per capita)	7.07	6.48	0.59***
	(0.90)	(0.71)	(0.04)
ln(Expenditure per capita)	8.30	8.30	0
	(0.60)	(0.48)	(0.03)
ln(Saving per capita)	9.50	9.10	0.41***
	(0.52)	(0.52)	(0.03)
ln(Primary School Students)	4.16	4.24	-0.08***
	(0.33)	(0.34)	(0.02)
ln(High School Students)	3.83	3.84	-0.01
	(0.30)	(0.35)	(0.02)
Altitude Difference	0.76	1.17	-0.40***
	(1.06)	(1.00)	(0.06)
Distance to the Closet Port (km)	570.74	588.65	-17.91
	(316.75)	(293.93)	(18.42)

Note: This table documents the statistical description of variables before the TPA program (2010-2014). The third column calculates the mean difference conditional on the year. Standard deviations are reported in parentheses, while *, **, and *** in the last column denote significance levels at 10%, 5%, and 1% respectively.

Table 3.2: Baseline Results: the PSM-DID Estimates

	Unweighted	PSM Weighted		
	(1)	(2)	(3)	(4)
NP × Post	-0.069*** (0.007)	-0.039*** (0.010)	-0.039*** (0.011)	-0.039*** (0.011)
Controls in 2010 × Post			✓	✓
Cubic Time Trend				✓
County FE	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓
Observations	10475	8150	8150	8150

Note: Coefficients are estimated through Eq. (3.1), weighted by the PSM score except in column (1). The dependent variable across all columns is the natural log of the income ratio defined in the main text. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. Only column (4) adds the cubic time trend of the controls. County and year-province fixed effects are all controlled. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table 3.3: Potential Effect on Non-Poverty Counties

	(1)	(2)
Extending Effect	0.005	0.005
	(0.014)	(0.014)
NP × Post	-0.040***	-0.040***
	(0.011)	(0.012)
Controls in 2010 × Post	✓	✓
Cubic Time Trend		✓
County FE	✓	✓
Province × Year FE	✓	✓
Observations	8150	8150

Note: Coefficients are estimated through Eq. (3.2) with the PSM score weights. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. Then they are interacted with the post dummy. Column (2) adds the cubic time trend of the controls. County and year-province fixed effects are all controlled. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table 3.4: Other Robustness Checks

	Cluster at Prefecture	Cluster at Province	Western Provinces	Income Ratio	If TPA Started in 2013
	(1)	(2)	(3)	(4)	(5)
NP × Post	-0.039***	-0.039**	-0.043**	-0.179***	
	(0.013)	(0.014)	(0.018)	(0.040)	
NP × Post_{2013}					-0.023
					(0.015)
Controls in 2010 × Post	✓	✓	✓	✓	
County FE	✓	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓	✓
Observations	8150	8150	2941	8150	3736

Note: Coefficients are estimated through Eq. (3.1), weighted by the PSM score. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. In columns (1) and (2), the standard errors are clustered at the prefecture and province level. In column (3), only western provinces are considered in the specification. The dependent variable is the natural log of the income ratio defined in the main text, except in column (4). In column (5), I assume the TPA program started in 2013 and only use the sample from 2010 to 2014 for regression. County and year-province fixed effects are all controlled, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table 3.5: Change in Urban and Rural Income

	Rural Income		Urban Income	
	(1)	(2)	(3)	(4)
NP × Post	0.0240**	0.0240**	-0.0090	-0.0087
	(0.0101)	(0.0101)	(0.0085)	(0.0085)
Controls in 2010 × Post	✓	✓	✓	✓
Cubic Time Trend		✓		✓
County FE	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓
Observations	8875	8875	8153	8153

Note: Coefficients are estimated through Eq. (3.1), weighted by the PSM score. The dependent variable is rural net income per capita in columns (1) and (2) and urban net income per capita in columns (3) and (4), both at the 2010 price level and then being transformed in the natural log form. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. Then they are interacted with the post dummy. Only columns (2) and (4) add the cubic time trend of the controls. County and year-province fixed effects are all controlled. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

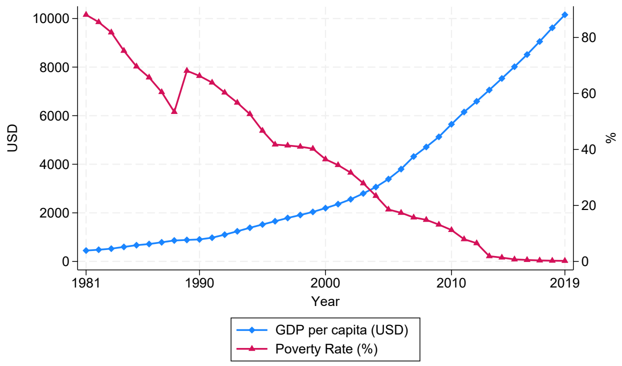
Table 3.6: Change in Employment, Primary Sector, and Fiscal Expenditure

	Rural Employees	Industrial Employees	Tertiary Employees	Fiscal Expenditure
	(1)	(2)	(3)	(4)
NP × Post	0.034**	-0.018	0.001	0.030**
	(0.014)	(0.062)	(0.046)	(0.015)
Controls in 2010 × Post	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓
Observations	5759	6406	6406	8907

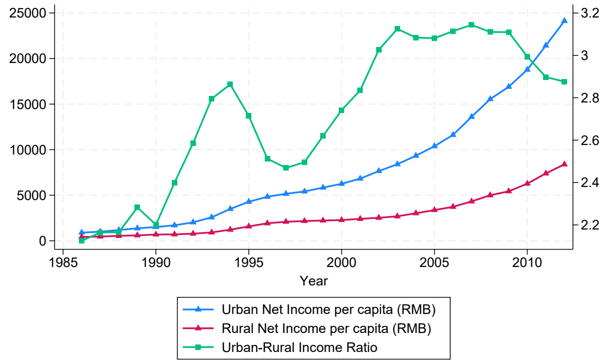
Note: Coefficients are estimated through Eq. (3.1), weighted by the PSM score.The outcome is rural employees per ten thousand persons in column (1), industrial employees per ten thousand persons in column (2), tertiary employees per ten thousand persons in column (3), and fiscal expenditure per capita in column (4), all being transformed in the natural log form. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. County and year-province fixed effects are all controlled, as well as the cubic time trend of controls in 2010. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Figures

FIGURE 3.1: GDP per capita, Poverty Rate, and Income in China



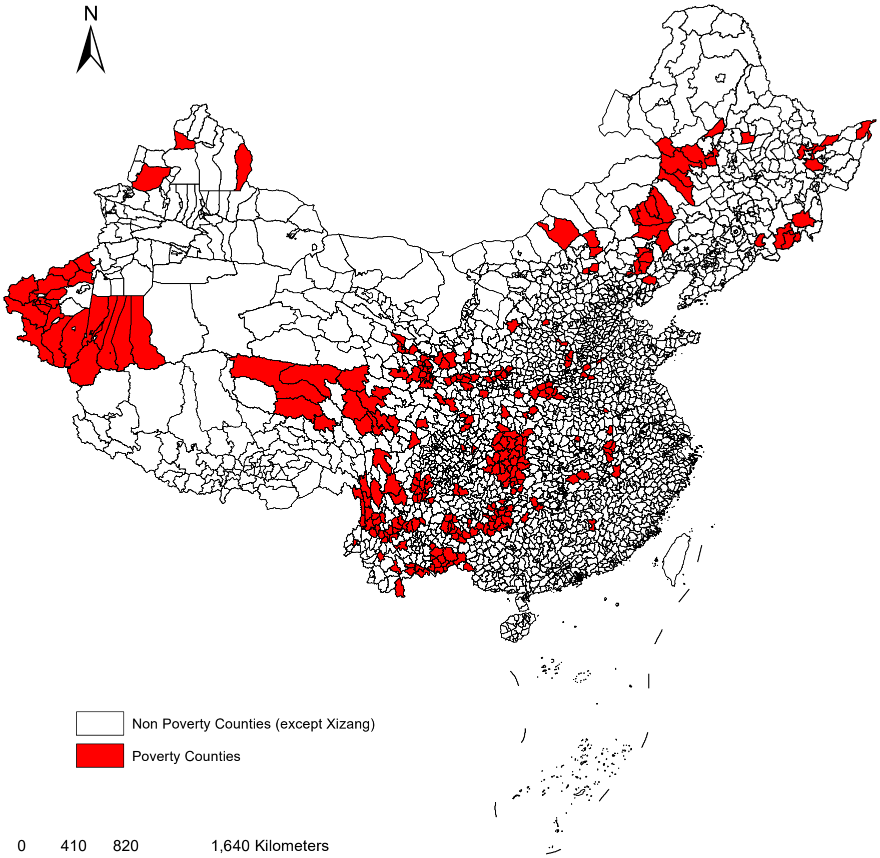
(A) Economic Growth and Poverty Alleviation



(B) Income and Income Inequality

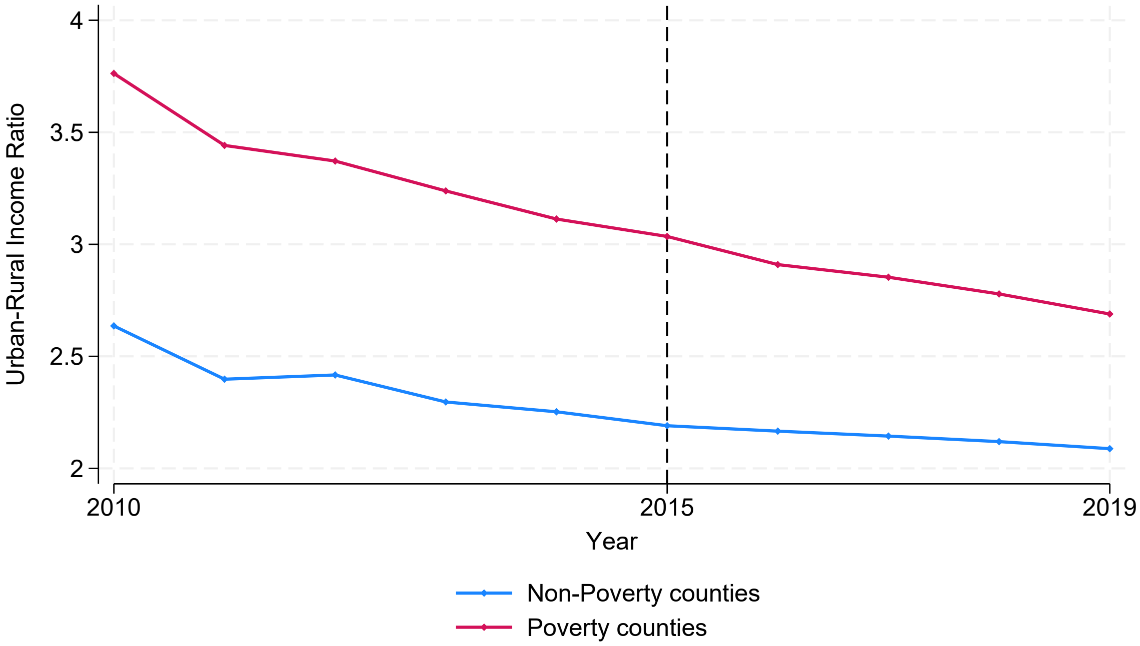
Note: Data are obtained from the World Bank and Global Extreme Poverty and National Bureau of Statistics. In (a), GDP per capita (plotted in the left vertical axis) has been adjusted in the 2015 US dollar, while the poverty rate (plotted in the left vertical axis is the percentage of the population living on less than 1.90 US dollars per day in the 2011 PPP. In (b), urban and rural net income per capita (plotted in the left vertical axis) are under the current price, while the urban-rural income ratio (plotted in the right vertical axis) is measured by the ratio of urban net income per capita to rural net income per capita.

FIGURE 3.2: Distribution of Poverty Counties in China



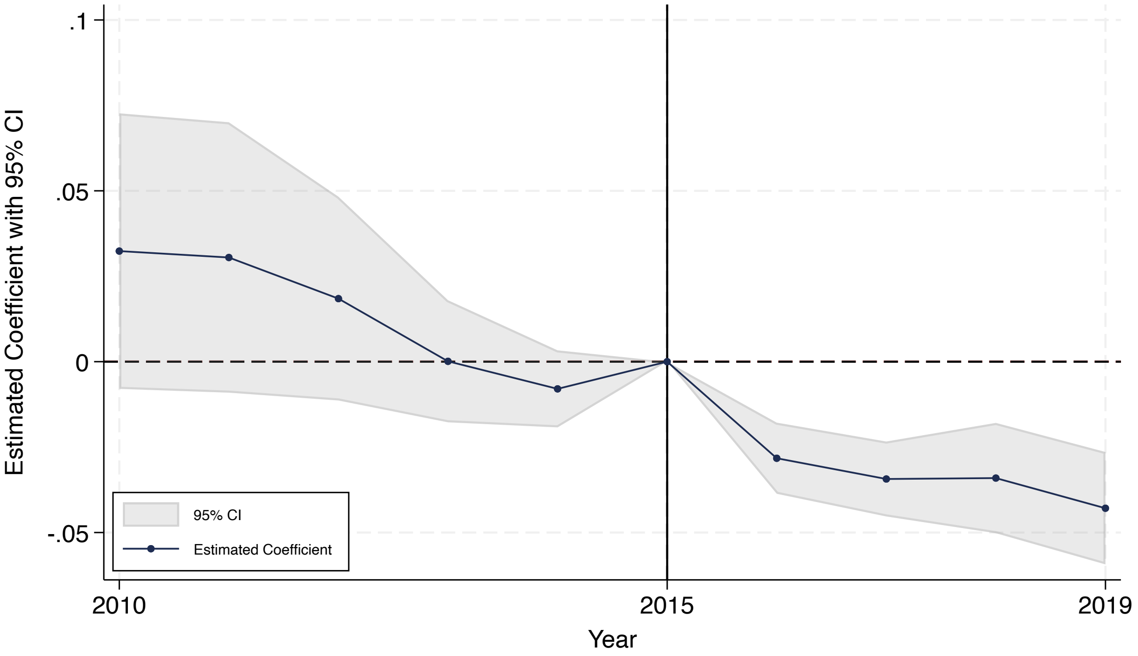
Note: The figure plots the distribution of poverty counties in China. The red colour suggests being designated as a poverty county in 2014, otherwise not. It should be noted that Xizang is not included in the Targeted Poverty Alleviation program and serves as an exception.

Figure 3.3: Trends In Urban-Rural Income Ratio from 2010 to 2019



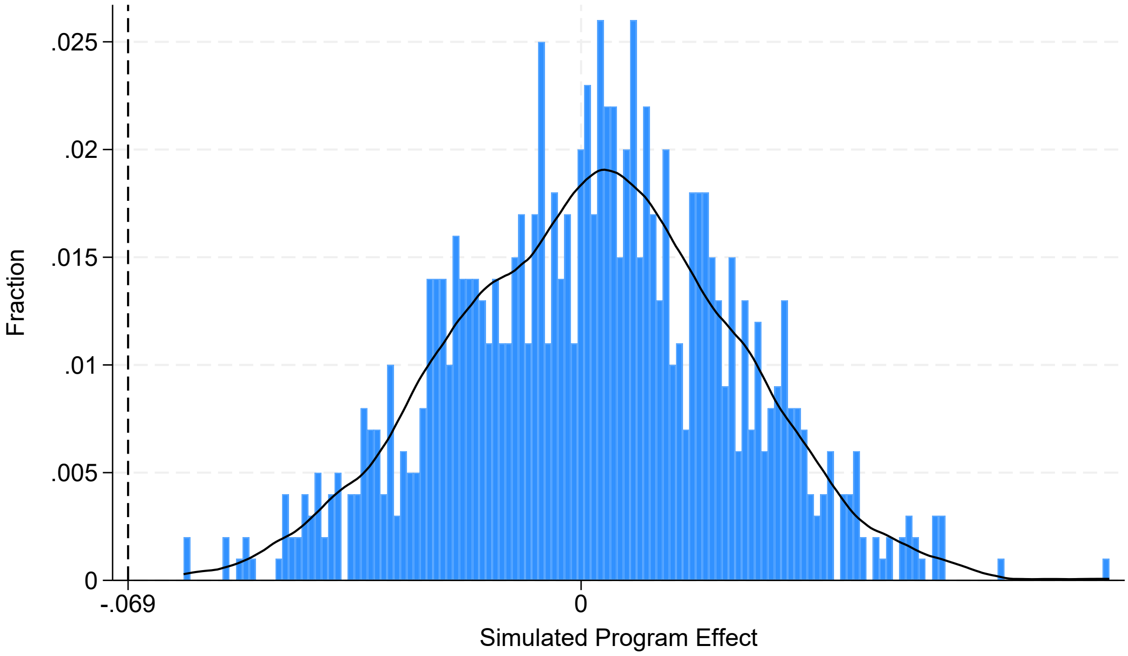
Note: All data are from *China Statistical Yearbook (County-level)*. The urban-rural income ratio is calculated by the urban net income per capita over the rural net income per capita. The red line represents poverty counties defined in the main text, while the blue line represents those non-poverty counties in only 21 provinces with at least one poverty county.

Figure 3.4: The Event-Study Analysis of the Income Ratio (log)



Note: Coefficients and their 95% confidence intervals in each year are the estimated coefficients through Eq. (3.3), weighted by the PSM score. The dependent variable is the natural log of the income ratio defined in the main text. The vertical line stands for the reference year, 2015. The standard errors are clustered at the county level

FIGURE 3.5: Estimates from Randomly Assigning Treatment



Note: By randomly assigning the status of poverty county and repeating the regression described in Eq. (3.1) 1000 times, the estimated coefficients are plotted on the horizontal axis, while the vertical axis is the fraction of each bin. Besides, a kernel density estimate is provided as the solid line. By comparison, the placebo estimates follow the distribution around 0, which is significantly away from the baseline estimate.

Appendix A

Appendix to Chapter 1

A.1 Tables

Table A1: Exogeneity Test of Treatment

Dependent Variable: Being a Severe County					
	Time Frame: 2003-2007				
	(1)	(2)	(3)	(4)	(5)
ln(GDP per capita)	0.0551	0.0606	-0.0107		
	(0.0446)	(0.0490)	(0.0425)		
ln(Population)				0.0100	
				(0.0320)	
ln(Fixed-Asset Investment per capita)					0.0407
					(0.0312)
Prefecture FE			✓	✓	✓
Year FE		✓	✓	✓	✓
Observations	898	898	898	898	898

Note: This table tests whether the pre-earthquake (2003-2007) county-level characters could influence being reported as a severe county after the earthquake. The key regressor of interest is GDP per capita from columns (1) to (3), population in column (4), and fixed-asset per capita in column (5).The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table A2: Event-Study Estimates of GDP per capita

Dependent Variable: ln(GDP per capita)	(1)	(2)	(3)	(4)	(5)
5-Year Lag	0.046	0.046	0.047	0.047	0.047
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
4-Year Lag	0.047	0.047	0.047	0.047	0.047
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
3-Year Lag	0.012	0.012	0.012	0.012	0.012
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
2-Year Lag	0.015	0.015	0.015	0.015	0.015
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
0-Year Lead	-0.217***	-0.217***	-0.216***	-0.205***	-0.214***
	(0.041)	(0.041)	(0.039)	(0.036)	(0.035)
1-Year Lead	-0.138***	-0.138***	-0.136***	-0.126***	-0.134***
	(0.027)	(0.027)	(0.027)	(0.026)	(0.025)
2-Year Lead	-0.131***	-0.131***	-0.130***	-0.119***	-0.128***
	(0.031)	(0.031)	(0.028)	(0.027)	(0.027)
3-Year Lead	-0.096***	-0.096***	-0.095***	-0.084***	-0.093***
	(0.036)	(0.036)	(0.031)	(0.030)	(0.030)
4-Year Lead	-0.091**	-0.091**	-0.090***	-0.079***	-0.088***
	(0.036)	(0.036)	(0.032)	(0.030)	(0.031)
5-Year Lead	-0.099***	-0.096***	-0.095***	-0.084**	-0.093***
	(0.036)	(0.036)	(0.034)	(0.033)	(0.033)
6-Year Lead	-0.082**	-0.078**	-0.077**	-0.067**	-0.076**
	(0.037)	(0.037)	(0.034)	(0.034)	(0.034)
7-Year Lead	-0.070*	-0.066*	-0.065*	-0.055	-0.064*
	(0.037)	(0.037)	(0.035)	(0.034)	(0.035)
8-Year Lead	-0.060	-0.056	-0.055	-0.045	-0.054
	(0.037)	(0.038)	(0.036)	(0.035)	(0.035)
9-Year Lead	-0.050	-0.084**	-0.100***	-0.082**	-0.090**
	(0.040)	(0.040)	(0.038)	(0.036)	(0.036)
10-Year Lead	-0.038	-0.071*	-0.087**	-0.069*	-0.078**
	(0.040)	(0.040)	(0.038)	(0.036)	(0.036)
11-Year Lead	-0.014	-0.048	-0.065*	-0.048	-0.056
	(0.042)	(0.040)	(0.039)	(0.037)	(0.037)
Distance Control		✓	✓	✓	✓
Structure Control			✓	✓	✓
Geographic Control				✓	✓
Density Control					✓
County FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	3049	3049	3049	3049	3049

Note: Coefficients are estimated in Eq. (1.1) at the county level, while the outcome of interest is the GDP per capita (adjusted at the 2007 price and natural log form). Distance control has the interaction term between the two interaction terms between the distance to their epicentres in the 2013 and 2017 earthquakes and their respective post-earthquake dummies; structure control includes the share of agricultural and industrial sectors in county GDP in 2007 both interacted with the post dummy ; geographic control considers county altitude difference interacted with the post dummy; density control represents county density in 2007 interacted with the post dummy. The county and year-fixed effects are included in all columns. The reference year is 2007, one year before the Wenchuan earthquake. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table A3: Alternative Outcome of Interest

Dependent Variable: ln(Retail Sales of Consumer Goods)	Full Sample: 2003-2019	2003-2016	2003-2012
	(1)	(2)	(3)
Severe × Post	-0.1010**	-0.0984**	-0.1104**
	(0.0436)	(0.0424)	(0.0434)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	3048	2510	1797

Note: The outcome of interest is retails sales of consumer goods (adjusted at the 2007 price) in its natural log form. The full sample is used in columns (1) and (2), while the sample after 2017 is excluded in column (3) and after 2013 is excluded in column (4). The controls are distance control, structure control, geographic control, and density control. The county and year-fixed effects are included in all columns. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table A4: Test of Spillover Effects

Dependent Variable: ln(GDP per capita)	Full Sample			Exclude Neighbour Counties		
	(1)	(2)	(3)	(4)	(5)	(6)
Severe × Post	-0.1296***	-0.1493***	-0.1709***	-0.1295***	-0.1492***	-0.1709***
	(0.043)	(0.042)	(0.041)	(0.043)	(0.042)	(0.041)
Neighbour × Post	-0.0664*	-0.0686*	-0.0541			
	(0.038)	(0.037)	(0.034)			
Sample Period	Full	<2017	<2013	Full	<2017	<2013
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	3049	2511	1798	2522	2077	1488

Note: The outcome of interest is GDP per capita (adjusted at the 2007 price) in its natural log form. Columns (1)-(3) add an interaction term between the neighbour dummy and post-earthquake dummy. Columns (4)-(6) exclude those neighbour counties (non-severe counties bordered with severe counties). The county and year-fixed effects are included in all columns. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table A5: Test of Migration Induced by the Earthquake

Dependent Variable: Population	(1)	(2)	(3)	(4)	(5)	(6)
Severe × Post	-0.8894	-0.8403	-0.8283	-0.6011	-0.5760	-0.6271
	(1.393)	(1.280)	(1.070)	(1.452)	(1.334)	(1.131)
Neighbour × Post				1.3073	1.1986	0.9136
				(2.146)	(1.899)	(1.394)
Sample Period	Full	<2017	<2013	Full	<2017	<2013
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	3049	2511	1798	3049	2511	1798

Note: The outcome of interest is county-level population. Columns (4)-(5) add an interaction term between the neighbour dummy and post-earthquake dummy. The county and year-fixed effects are included in all columns. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

TABLE A6: Falsification Test of Baseline Results

Dependent Variable: ln(GDP per capita)	If the Earthquake in 2004	If the Earthquake in 2005	If the Earthquake in 2006
	(1)	(2)	(3)
Severe × Post_{2004}	-0.028 (0.029)		
Severe × Post_{2005}		-0.038 (0.035)	
Severe × Post_{2006}			-0.028 (0.026)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	898	898	898

Note: The outcome of interest is GDP per capita (adjusted at the 2007 price) in its natural log form. Coefficients are estimated in Eq. (1.2) at the county level. I only keep the pre-earthquake sample and then assume the earthquake was in 2004, 2005, and 2006, represented in columns (1), (2), and (3) respectively. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

Table A7: Effect on Labour Market Outcomes

	Labour Force Participation = 1	Employment = 1	Weekly Working Hours	Weekly Wage
	(1)	(2)	(3)	(4)
Panel A: Rural Labour Market				
Severe × Post	0.005	-0.008	0.093**	0.031
	(0.013)	(0.011)	(0.038)	(0.056)
Observations	2510	1175	2433	2419
Panel B: Urban Labour Market				
Severe × Post	0.021	0.009	-0.009	-0.002
	(0.016)	(0.012)	(0.040)	(0.063)
Observations	1694	1653	1597	1603
Personal Controls	✓	✓	✓	✓
Occupation-Year Fixed Effects			✓	✓
Year Fixed Effects	✓	✓		
County Fixed Effects	✓	✓	✓	✓

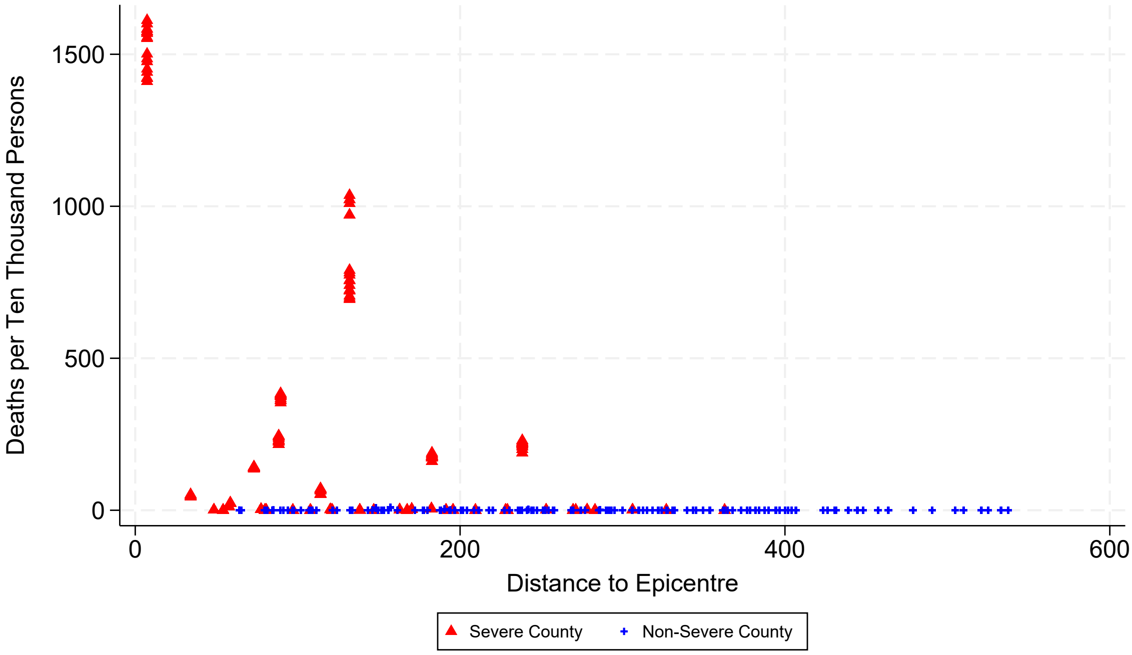
Note: I construct individual-level panel data using two waves of the China Household Income Project (CHIP) surveys, conducted in 2008 and 2009. The labour market outcome in columns (1), (2), and (3) are labour force participation, employment status, and weekly working hours. The personal controls include gender, age, marriage status, number of children, education level, health status, Hukou type, and working experience. I also account for the occupation-year fixed effects and county fixed effects. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

A.2 Figures

Figure A1: The Location of The Wenchuan Earthquake Epicentre

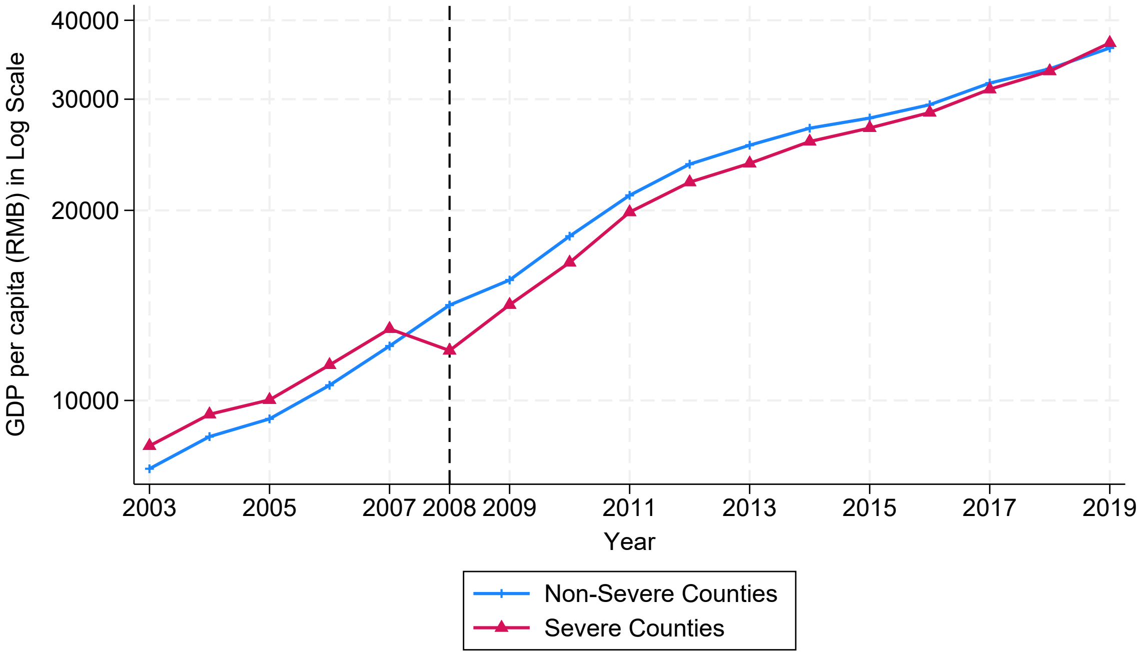
Note: This map shows the location of the Wenchuan earthquake epicentre in China, and the black lines plot the provincial boundaries.

FIGURE A2: Earthquake-Related Death Rate and Distance to Epicentre



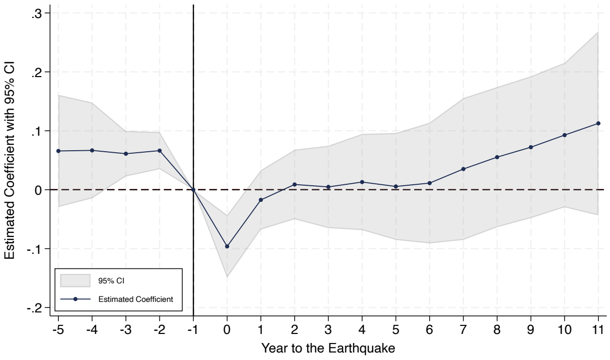
Note: The scatter plot depicts the correlation between the earthquake-related deaths and the distance to the earthquake epicentre at the county level. The triangle marker represents severe counties, while the plus marker is used for non-severe counties.

Figure A3: Trends of GDP per capita From 2003 to 2019

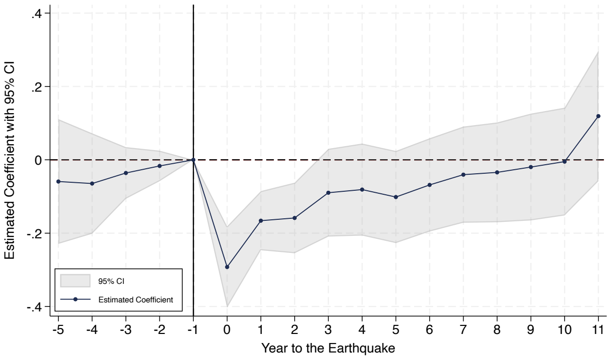


Note: This figure plots the trends of GDP per capita in severe and non-severe counties. The vertical axis represents GDP per capita in the log scale, adjusted at the 2007 price, while the horizontal axis represents the years, from 2003 to 2019.

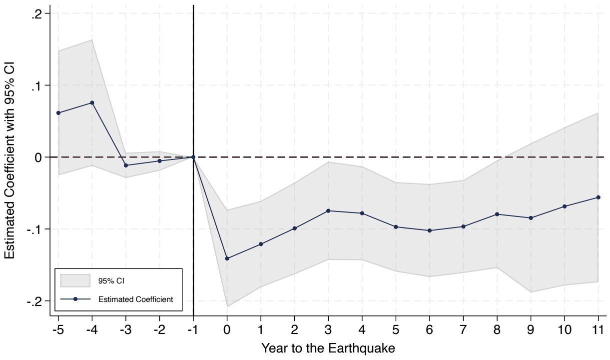
Figure A4: Sectoral Event-Study Analysis



(a) The Outcome is ln(GDP per capita in Agricultural Sector)



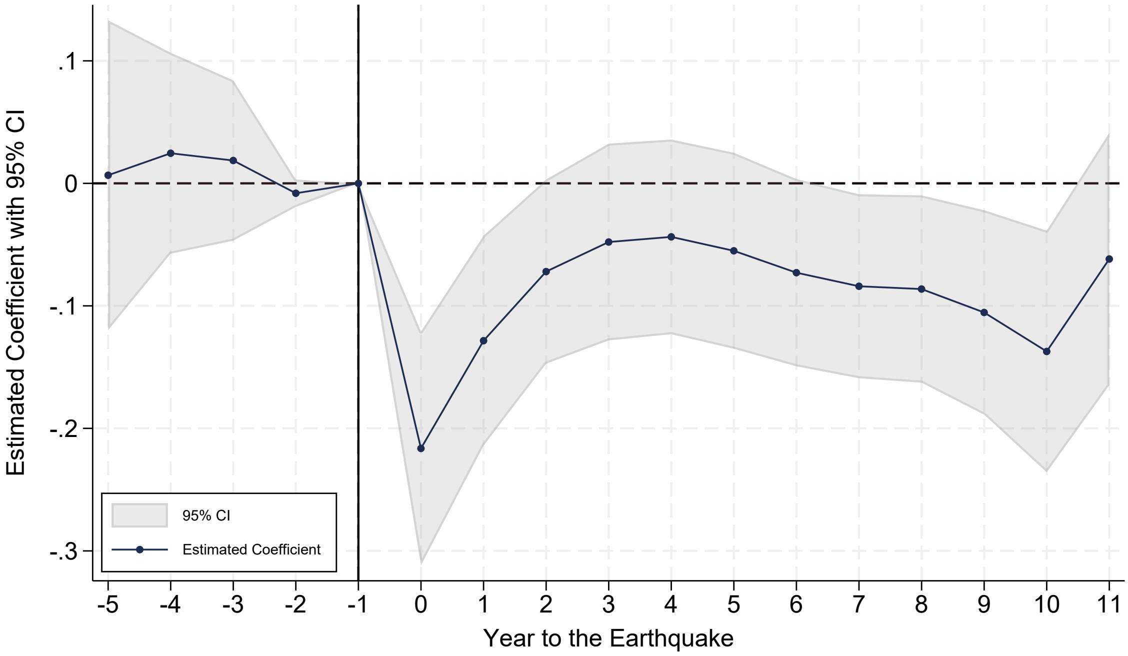
(b) The Outcome is ln(GDP per capita in Industrial Sector)



(c) The Outcome is ln(GDP per capita in Tertiary Sector)

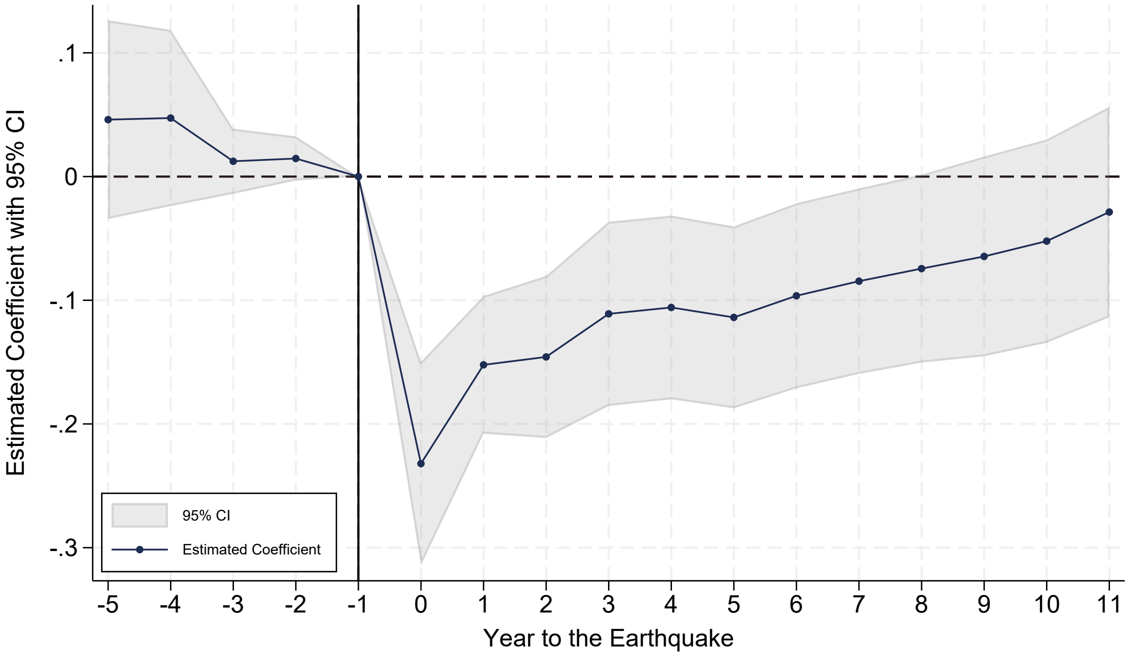
Note: The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure from the event-study regression in Eq. (1.1). Each subfigure represents a sectoral GDP per capita in the natural log form. The vertical dashed line indicates the reference year in 2007, one year before the earthquake. Standard errors are clustered at the county level.

FIGURE A5: Effect on Total Retail Sales of Consumer Goods



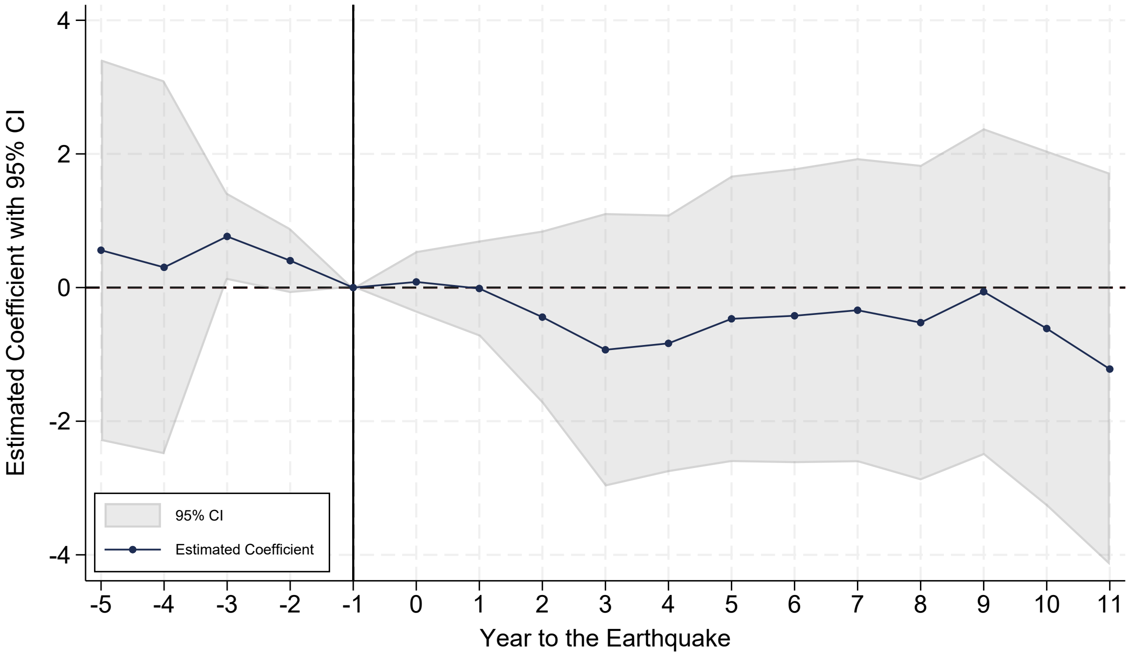
Note: The event-study regression is the same as proposed in Eq. (1.1), but the outcome of interest is the natural log of total retail sales of consumer goods per capita. The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure. The vertical dashed line indicates the reference year in 2007, one year before the earthquake, and the standard errors are clustered at the county level.

Figure A6: Event-Study Analysis with Potential Spillover Effects



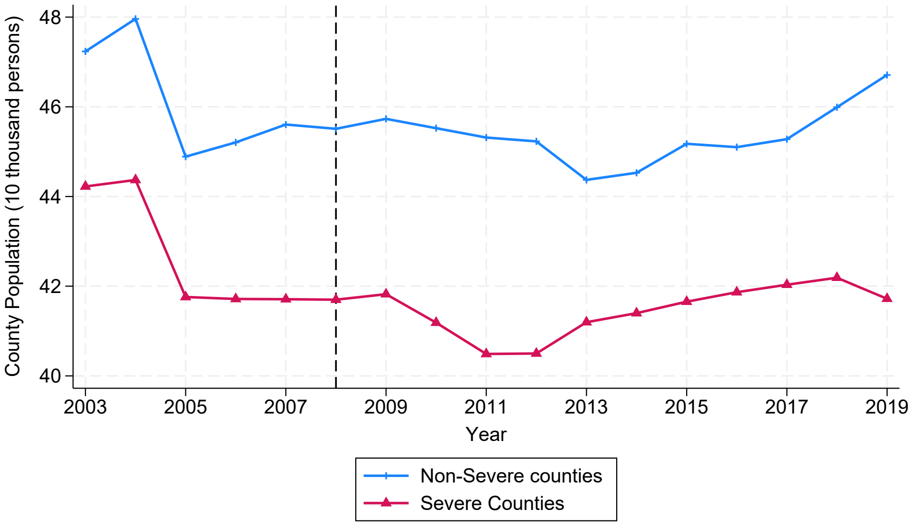
Note: The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure from the event-study regression in Eq. (1.1), considering potential spillover effects. The outcome of interest is GDP per capita in the natural log form. The vertical dashed line indicates the reference year in 2007, one year before the earthquake. Standard errors are clustered at the county level.

Figure A7: Effect on County Population

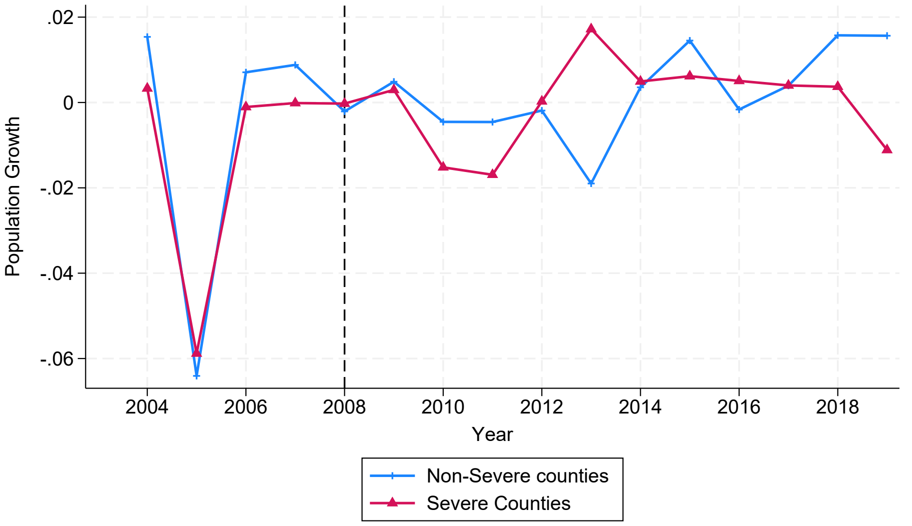


Note: The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure from the event-study regression in Eq. (1.1). The outcome of interest county population. The vertical dashed line indicates the reference year in 2007, one year before the earthquake. Standard errors are clustered at the county level.

Figure A8: Population Change from 2003 to 2019



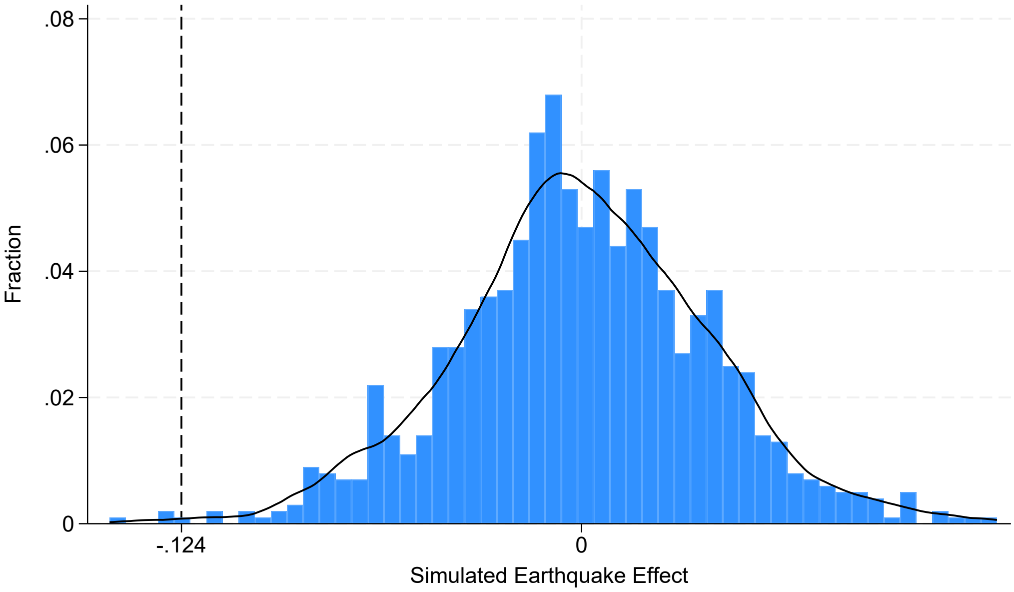
(A) Trends in Population



(B) Trends in Population Growth

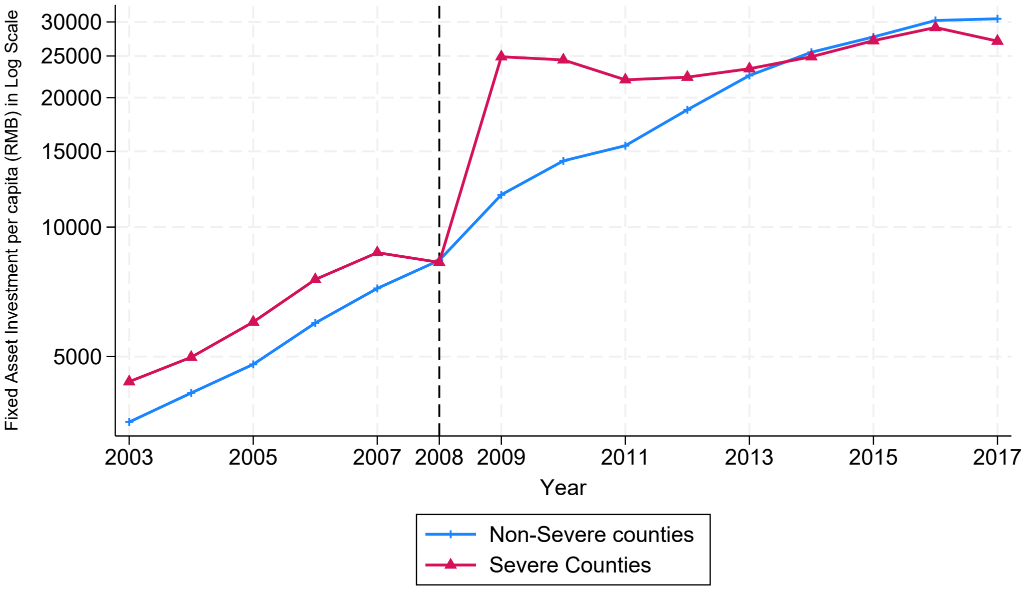
Note: This figure plots the trends of county population and population growth in severe and non-severe counties. The vertical axis is a unit of ten thousand persons, while the horizontal axis represents time frame from 2003 to 2019.

Figure A9: Distribution of the Simulated Effect



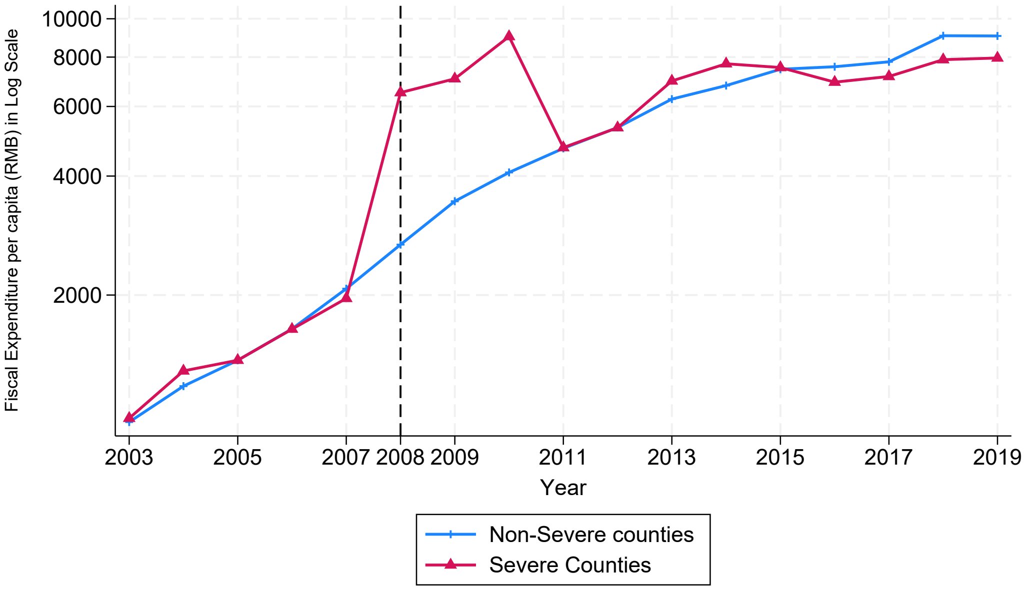
Note: I re-ran the regression in Eq. (1.2) 1,000 times, randomly assigning the treatment dummy variable based on the percentage of severe counties. The estimates were plotted as a distribution, each represented as a fraction. The vertical line on the plot denotes the earthquake effect observed in the baseline outcomes.

Figure A10: Trends of Fixed-Asset Investment per capita



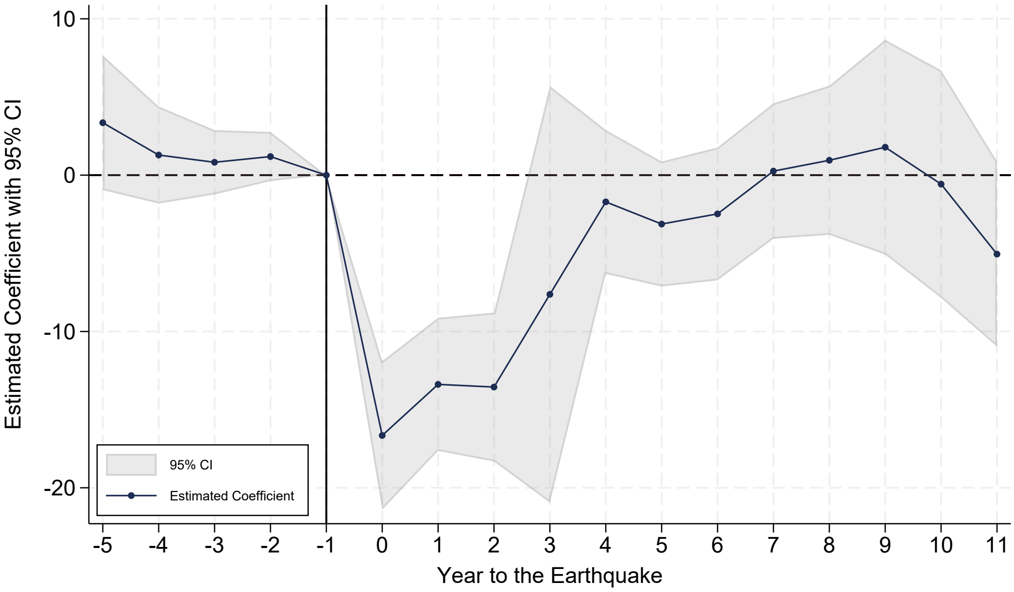
Note: This figure plots the trends of fixed-asset investment per capita in severe counties and non-severe counties. The vertical axis represents fixed-asset investment per capita in the log scale, adjusted at the 2007 price, while the horizontal axis represents the time frame from 2003 to 2017.

Figure A11: Trends of Fiscal Expenditure per capita



Note: This figure plots the trends of fiscal expenditure per capita in severe counties and non-severe counties. The vertical axis represents fiscal expenditure per capita in the log scale, adjusted at the 2007 price, while the horizontal axis represents the time frame from 2003 to 2019.

Figure A12: Effect on Fiscal Dependency



Note: The event-study regression is the same as proposed in Eq. (1.1), but the outcome of interest is the fiscal dependency, fiscal revenue over fiscal expenditure. The estimated coefficients in each year with their 95% confidence intervals are plotted in this figure. The vertical dashed line indicates the reference year in 2007, one year before the earthquake, and the standard errors are clustered at the county level.

A.3 The Wenchuan Earthquake Migration Model

The Hukou system in China imposes restrictions on internal migration, preventing residents from relocating to other regions and accessing public benefits without certain administrative procedures. In the context of this study, a key implication of this system is that it increases the likelihood that skilled workers remain in affected areas, thereby mitigating the potential loss of human capital and productivity that could arise under conditions of unrestricted labour mobility. This paper presents a theoretical framework to illustrate how limited mobility, enforced by the Hukou system, may have facilitated economic recovery and development in severe counties.

I consider a representative severe county in a two-period model ($t = 0, 1$) where the earthquake strikes in $t = 0$. The production function uses the standard Cobb-Douglas form:

$$Y_t = AK_t^{\alpha} L_t^{1-\alpha},\ \alpha \in (0,1) \tag{A.1}$$

where Y_t represents output, A is the constant level of technology, K_t is the capital stock, and L_t is the labour force at time t. And the per worker production is:

$$y_t = \frac{Y_t}{L_t} = A\left(\frac{K_t}{L_t}\right)^{\alpha} = Az_t^{\alpha} \tag{A.2}$$

where $z_t = \frac{K_t}{L_t}$ is the capital-labour ratio. From this expression, per worker production is positively correlated with the capital-labour ratio. The depreciation rate of capita is δ, and the saving rate in the county is s, both parameters being exogenous and between 0 and 1. In a typical scenario without shocks, capital accumulation follows:

$$K_{t+1} = sY_t + (1-\delta)K_t = sAK_t^{\alpha} L_t^{1-\alpha} + (1-\delta)K_t \tag{A.3}$$

Suppose each period a constant share ($M \in$ (0,1)) of the labour force chooses to migrate, reflecting the strictness of the Hukou system. Migration is lower with Hukou enforcement than without, i.e., $M^{no\ hukou} > M^{hukou}$. Besides, I assume the natural birth rate is zero. When the earthquake strikes in period 0, a fraction γ of the initial capital stock is destroyed, but the death toll among the labour force is negligible. However, migration occurs as residents respond to the current Hukou system constraints, leading to a reduction in skilled workers, which in turn reduces productivity. For simplicity, the post-earthquake productivity level is modelled as $(1-M)A$.

The next step is to analyze how the strictness of the Hukou system influences the per worker production through the capital-labour ratio. Firstly, when the earthquake strikes at period 0, the capital and labour become $(1-\gamma)K_0$ and $(1-M)L_0$, respectively, with productivity reduced to $(1-M)A$. The capital-labour ratio in period 1 is:

$$\begin{aligned} z_1 = \frac{K_1}{L_1} = \frac{sY_0 + (1-\delta)(1-\gamma)K_0}{L_1} &= \frac{s(1-M)AK_0^{\alpha}[L_0(1-M)]^{1-\alpha} + (1-\delta)(1-\gamma)K_0}{L_0(1-M)} \\ &= sAz_0^{\alpha}(1-M)^{1-\alpha} + (1-\delta)(1-\gamma)z_0(1-M)^{-1} \end{aligned} \tag{A.4}$$

In this equation, the key parameter of interest is the migration rate M. Taking the derivative of z_1 with respect to M, I obtain:

$$\frac{dz_1}{dM} = -sAz_0^{\alpha}(1-M)^{-\alpha} + (1-\delta)(1-\gamma)z_0(1-M)^{-2} \tag{A.5}$$

The sign of this derivative depends on the relative sizes of the terms. Specifically, if:

$$M < 1 - \left[\frac{(1-\delta)(1-\gamma)}{sA(1-\alpha)}\right]^{\frac{1-\alpha}{2-\alpha}} = M_{critical} \tag{A.6}$$

where $\left[\frac{(1-\delta)(1-\gamma)}{sA(1-\alpha)}\right]^{\frac{1-\alpha}{2-\alpha}}$ is assumed to be between 0 and 1, then the first-order derivative of z_1 with respect to M is negative.

The enforcement of the Hukou system restricts labour migration, keeping M lower than $M_{critical}$. Then:

$$\frac{dy_1}{dM} = \frac{dy_1}{dz_1} \cdot \frac{dz_1}{dM} < 0 \tag{A.7}$$

This implies that limited mobility can improve post-earthquake economic conditions by preserving skilled workers and preventing a substantial reduction in productivity. Intuitively, fewer skilled workers migrating helps maintain the local technology level, supporting faster recovery and growth.

A.4 Individual Labour Market Outcomes

I construct an individual-level panel dataset using the longitudinal survey on Rural Urban Migration in China (RUMiC). The (RUMiC) survey was initiated as a collaborative effort between the Australian National University, the University of Queensland, the Beijing Normal University, and the Institute of Labor Economics (IZA), to better understand the socioeconomic and demographic impacts of rural-to-urban migration in China. To be more specific, its questionnaires have covered many critical aspects of quality of life, such as household demographic characteristics, health conditions, social networks, and household finance (Akgüç et al., 2014).

I use two waves of the RUMiC survey conducted in 2008 and 2009, which provides a unique opportunity to capture the short-term impacts of the Wenchuan earthquake, as it occurred between the two survey periods. For this analysis, I only focus on answers from adults (aged 18 and older) residing in Sichuan province and match their residence with the severity status at the county level. The empirical specification is expressed as:

$$y_{ict} = \alpha + \beta Severe_c \times Post_t + x'_{ict}\gamma + \delta_c + \eta_t + \epsilon_{ict}$$

where y_{ict} represents labour market outcomes for individual i in county c in year t. Specifically, those outcomes include labour force participation, employment status, weekly working hours, and weekly wage.[1] $Severe_c$ is a severity indicator that is equal to 1 if the county is classified as severely affected by the earthquake; $Post_t$ a time indictor that is equal to 1 if the year is after the earthquake; x'_{ict} includes a set of individual characteristics, such gender, age, marriage status, number of children, education level, minority, health status, Hukou type, and work experience; δ_c and η_t control the county and year fixed effects; ϵ_{ict} measures unobservable random shocks. Standard errors are clustered at the household level in this analysis.

Table A7 present the estimates in the previous specification. The dependent variables in columns (1) and (2) reflect the extensive margin of labour market outcomes, while those in columns (3) and (4) analyze the earthquake effect on the intensive margin. In the final two columns, the specification incorporates occupation-year fixed

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[1] Weekly wages have been adjusted to 2007 price levels and transformed into their natural log form, as have weekly working hours.

effects instead of year fixed effects alone. This adjustment captures for potential influences of occupation type on the intensive margin. Overall, the Wenchuan earthquake does not significantly affect either the extensive or intensive margins of labour in the short term. Respondents in severe counties, whether in rural or urban areas, do not experience changes in labour force participation, employment status, or wage levels due to the earthquake. The only exception is among rural residents in severe counties, whose weekly working hours increase by approximately 9% following the earthquake.

Appendix B

Appendix to Chapter 2

B.1 Tables

TABLE B1: Summary Statistics of Sample

	Observations	Mean	Standard Deviation	Minimum	Maximum
Years of Schooling	12536	10.05	3.01	0.00	19.00
Age in 2008	12536	15.92	4.50	8.00	23.00
Younger Than 15 in 2008	12536	0.46	0.50	0.00	1.00
Male	12536	0.51	0.50	0.00	1.00
Han People	12536	0.89	0.32	0.00	1.00
Rural Hukou	12536	0.62	0.49	0.00	1.00
Number of Rooms	12536	4.56	2.51	1.00	30.00
House Area (m^2)	12536	138.36	70.42	5.00	850.00
Household Size	12536	4.46	1.57	1.00	15.00
Car Ownership	12536	0.15	0.35	0.00	1.00
Severe County	12536	0.21	0.41	0.00	1.00

Note: This table documents the statistical description of the sample in this study. Each column reports the number of observations, mean, standard deviation, minimum, and maximum values.

TABLE B2: Detailed Estimates in Event-Study Design

Dependent Variable: Years of Schooling	(1)	(2)	(3)
Severe × Age 20 in 2008	-0.248	-0.209	-0.143
	(0.281)	(0.286)	(0.288)
Severe × Age 19 in 2008	-0.323	-0.237	-0.222
	(0.318)	(0.323)	(0.324)
Severe × Age 18 in 2008	0.052	0.047	0.160
	(0.276)	(0.274)	(0.271)
Severe × Age 17 in 2008	-0.192	-0.122	-0.061
	(0.386)	(0.374)	(0.359)
Severe × Age 16 in 2008	-0.045	-0.078	0.007
	(0.298)	(0.296)	(0.292)
Severe × Age 14 in 2008	-0.478	-0.402	-0.336
	(0.290)	(0.282)	(0.278)
Severe × Age 13 in 2008	-0.653***	-0.661***	-0.580**
	(0.250)	(0.242)	(0.233)
Severe × Age 12 in 2008	-0.715***	-0.715***	-0.657**
	(0.261)	(0.258)	(0.259)
Severe × Age 11 in 2008	-0.397	-0.425*	-0.391*
	(0.247)	(0.231)	(0.216)
Severe × Age 10 in 2008	-0.809***	-0.756***	-0.697***
	(0.252)	(0.249)	(0.245)
Severe × Age 9 in 2008	-0.449*	-0.356	-0.257
	(0.265)	(0.273)	(0.265)
Severe × Age 8 in 2008	-0.812***	-0.838***	-0.783***
	(0.300)	(0.295)	(0.298)
Personal Controls		✓	✓
Household Controls			✓
Cohort Fixed Effects	✓	✓	✓
County Fixed Effects	✓	✓	✓
Observations	12536	12536	12536

Note: This table presents the detailed estimates in the event-study design. Columns differ by controls included. All regressions include cohort and county fixed effects, with standard errors clustered at the county level. Standard errors are shown in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

TABLE B3: Heterogeneous Tests by Gender, Ethnicity, and Residence

Dependent Variable: Years of Schooling	Male	Han	Rural
	(1)	(2)	(3)
Severe × $\mathbb{1}$(Age $\leq$ 15) × $\mathbb{1}$(Male=1)	-0.298*** (0.103)		
Severe × $\mathbb{1}$(Age $\leq$ 15) × $\mathbb{1}$(Han=1)		-1.135** (0.469)	
Severe × $\mathbb{1}$(Age $\leq$ 15) × $\mathbb{1}$(Rural=1)			0.238 (0.151)
Controls	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓
County Fixed Effects	✓	✓	✓
Observations	12536	12536	12536

Note: This table presents the estimates from heterogeneity tests, where the dependent variable is individual years of schooling. All regressions include cohort and county fixed effects, with standard errors clustered at the county level. Standard errors are shown in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table B4: Alternative Specifications

Dependent Variable: Years of Schooling	Age 18 as Cutoff	Distance Weight	Clustering at Prefecture	Excluding Districts
	(1)	(2)	(3)	(4)
Severe × $\mathbb{1}(\text{Age} \leq 18)$	-0.169 (0.141)			
Severe × $\mathbb{1}(\text{Age} \leq 15)$		-0.435*** (0.157)	-0.377** (0.149)	-0.535*** (0.151)
Controls	✓	✓	✓	✓
Cohort Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓
Observations	12536	12536	12536	9233

Note: This table presents the estimates from the robustness checks, where the dependent variable is individual years of schooling. Column (1) uses 18 as the cutoff to differentiate the younger and older cohorts; Column (2) uses the linear distance to the epicentre as weight in the specification; Column (3) clusters the standard errors at the prefecture level; Column (4) excludes observations residing in city districts in the sample. *, **, and *** denote the significance levels at 10%, 5%, and 1% respectively.

TABLE B5: The Earthquake Effect on Dropout and Marriage

Dependent Variable:	Dropout after Compulsory Education		Married	
	(1)	(2)	(3)	(4)
Severe × $\mathbb{1}(\text{Age} \leq 15)$	0.070***	0.070***	0.045	0.039
	(0.023)	(0.023)	(0.029)	(0.026)
Controls		✓		✓
Cohort Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓
Observations	10438	10438	9126	9126

Note: This table presents the impact of the earthquake on the likelihood of dropping out after compulsory education and marriage. Standard errors are clustered at the county level, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

B.2 Coding for Education

The translation of the questions about the highest degree achieved and education completion status is here:[1]

- Question 15: What is your level of education?

  Never attended school; Primary school; Junior high school; High school; Vocational school; College (Associate degree); University (Bachelor's degree); Graduate school

- Question 16: What is your academic completion status?

  Currently enrolled; Graduated; Incomplete; Dropped out; Other

In the Chinese education system, students typically spend 6 years in primary school, 3 years in junior high school, and 3 years in either high school or vocational school. After completing the 9-year compulsory education, students may choose to continue their studies by enrolling in either high school or vocational school. Following this, they are expected to spend 3 years in college or 4 years in university.[2] Lastly, I assume a standard duration of 3 years for graduate studies, as the questionnaire does not distinguish between master ' s and doctoral programs in graduate school. In summary, years of schooling can be coded in the following way based on the answer:

Never attended school → 0
Primary school → 6
Junior high school → 9
High school → 12
Vocational school → 12
College (Associate degree) → 15
University (Bachelor's degree) → 16
Graduate school → 19

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[1] The original questionnaire can be found: https://www.stats.gov.cn/zt_18555/zdtjgz/cydc/xw/202302/t20230221_1917243.htm.

[2] Certain disciplines, such as medicine, require 5 years of university study. However, this analysis focuses on the most common scenario.

In Question 16, it is unclear which grade respondents are currently in if they have not indicated ”Graduated” as their completion status. Following the approach of Wang et al. (2017); Chen et al. (2020); Liu and Xu (2021), I assign half of the duration of the highest level of education if they have not yet graduated. For instance, if a respondent selects ”High school” in Question 15 and ”Currently enrolled” in Question 16, this indicates that the individual is still attending high school at the time of the survey. In this case, I assign 9 years (representing the completion of junior high school) + 1.5 years (representing half of the high school duration) = 11.5 years of schooling.

Appendix C

Appendix to Chapter 3

C.1 Poverty Alleviation since 1978

In stark contrast to its current status, China was among the most underdeveloped countries in the world during the 1970s, with the majority of its population leading a frugal and modest life.[1] The economic transformation commenced following the 1978 opening-up reforms when the central government made the pivotal decision to prioritize economic development and integrate into the global market. Among many challenges, the most urgent was to improve the quality of life for the Chinese people.

The initial phase of market-oriented reforms (1978–1986) brought significant benefits to rural farmers, marking a fundamental departure from the previous centrally planned framework. A key element of this reform was the introduction of the household responsibility system, which decentralized agricultural production by granting households contractual rights over land use. This system proved highly effective in boosting agricultural productivity and improving farmers' living standards (Li et al., 1998; McMillan et al., 1989). Until 1986, the Chinese government formally initiated poverty alleviation programs. (I) The first round from 1986 to 1993 was spearheaded by the Leading Group for Economic Development (the Leading Group), which identified poverty counties across the nation based on a variety of economic indicators. While the program demonstrated an overall positive impact, it also uncovered instances of malpractice, particularly concerning the politicization and economic gains associated with being designated as a poverty county (Park et al., 2002). Local officials sought to include districts that did not meet the criteria for poverty-stricken status, driven by the

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[1] According to the World Bank, China's GDP per capita in 1970 was $113.2 in current USD, ranking 154th out of 170 countries with available data.

allure of substantial financial transfers from higher levels of government. Consequently, subsequent rounds adopted a more cautious approach to targeting and classification. (II) The second round, also known as the 8-7 Plan, spanned from 1994 to 2000, with the ambitious objective of uplifting 80 million rural poor out of poverty within 7 years.[2] The State Council augmented the work-for-dole funds and the poverty-alleviation-discount funds by 1 billion RMB each. Furthermore, adjustments were made to promote regional equilibrium, whereby poverty alleviation funds for the six more affluent coastal provinces remained static, enabling the surplus funds to be channelled to the less developed central and western regions. (III) The third round started in the early 2000s and witnessed the implementation of a more comprehensive and quantifiable criterion of poverty status. Known as the "631 index", it encompassed the poverty population (60%), rural income per capita (30%), and per capita GDP and fiscal revenues (10%).[3] This phase focused on contiguous poor areas, including ethnic minority regions, historical revolutionary bases, border areas, and underprivileged zones, aligning with the Western Development strategy of 2000.[4]

In terms of financial support, the central government has consistently increased fiscal funds allocated for poverty alleviation, subject to annual audits conducted by the Ministry of Finance. As depicted in figure C1, cumulative funds during the initial phase before 1985 barely surpassed 3 billion RMB, before witnessing a surge to over 20 billion RMB between 1985 and 1993. Since the turn of the 21st century, funds surged significantly even over 100 billion RMB. The initial five years of the most recent phase (2011-2015) witnessed an expenditure that exceeded the total accumulated amount from 2001 to 2010 by 45 billion RMB. In summary, the pronounced growth of funds over the years attests to the Chinese government's resolute commitment to eradicating absolute poverty nationwide.

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[2] In Chinese, 10 million is *"qian wan"*, so 80 million is *"8 qian wan"*. The official announcement of the plan: `https://www.gov.cn/gongbao/shuju/1994/gwyb199412.pdf`.

[3] See `http://cn.chinagate.cn/povertyrelief/2012-08/09/content_26182170.htm`.

[4] The Western Development strategy is a large-scale program to reduce regional imbalance by boosting economic development in the western areas of mainland China.

C.2 Table

Table C1: Provinces with Poverty Counties in 2014

Province	Number of Poverty Counties	Total Number of Counties	Ratio
Hebei	39	171	0.228
Shanxi	35	119	0.294
Neimeng	31	102	0.304
Jilin	8	60	0.133
Heilongjiang	14	128	0.109
Anhui	19	105	0.181
Jiangxi	21	100	0.210
Henan	31	158	0.196
Hubei	25	103	0.243
Hunan	20	122	0.164
Guangxi	28	110	0.255
Hainan	5	24	0.208
Chongqing	14	38	0.368
Sichuan	36	183	0.197
Guizhou	50	88	0.568
Yunnan	73	129	0.566
Shaanxi	50	107	0.467
Gansu	43	86	0.500
Qinghai	15	43	0.349
Ningxia	8	22	0.364
Xinjiang	27	103	0.262
Total	592	2101	0.282

Note: The total number of counties in each province is from *China Statistical Yearbook 2014* and the list of poverty counties from `http://nrra.gov.cn/art/2012/3/19/art_50_23706.html`.

Table C2: Statistical Description After Matching

	Non-Poverty counties	Poverty counties	Weighted Differences
ln(GDP per capita)	9.49	9.49	0.00
	(0.40)	(0.49)	(0.06)
Agricultural Share	0.24	0.26	-0.01
	(0.11)	(0.11)	(0.02)
Industrial Share	0.41	0.39	0.02
	(0.14)	(0.15)	(0.02)
ln(Revenue per capita)	6.32	6.45	-0.12
	(0.66)	(0.69)	(0.09)
ln(Expenditure per capita)	8.16	8.27	-0.11*
	(0.43)	(0.45)	(0.06)
ln(Saving per capita)	9.25	9.10	0.15***
	(0.40)	(0.52)	(0.06)
ln(Primary School Students)	4.22	4.21	0.01
	(0.32)	(0.33)	(0.81)
ln(High School Students)	3.86	3.84	0.02
	(0.30)	(0.33)	(0.04)
RDLS	0.88	1.01	-0.13
	(0.54)	(0.68)	(0.08)
Distance to the Closet Port (km)	530.47	544.36	-13.90
	(195.31)	(252.70)	(0.64)

Note: This table documents the statistical description of variables before the TPA program in poverty and non-poverty counties (2010-2014), weighted by the PSM score. Standard deviations are reported in parentheses, while *, **, and *** in the last column denote significance levels at 10%, 5%, and 1% respectively.

Table C3: Baseline Results with Different Time Orders

	Linear Trend		Quadratic Trend	
	(1)	(2)	(3)	(4)
NP × Post	-0.039***	-0.039***	-0.039***	-0.039***
	(0.011)	(0.011)	(0.011)	(0.011)
Controls in 2010 × Post		✓		✓
County FE	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓
Observations	8150	8150	8150	8150

Note: Coefficients are estimated through Eq. (3.1), weighted by the PSM score. The dependent variable across all columns is the natural log of the income ratio defined in the main text. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. Then they are interacted with the post dummy. Columns (1) and (2) use the linear trend of the controls, while columns (3) and (4) use the quadratic trend. County and year-province fixed effects are all controlled. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

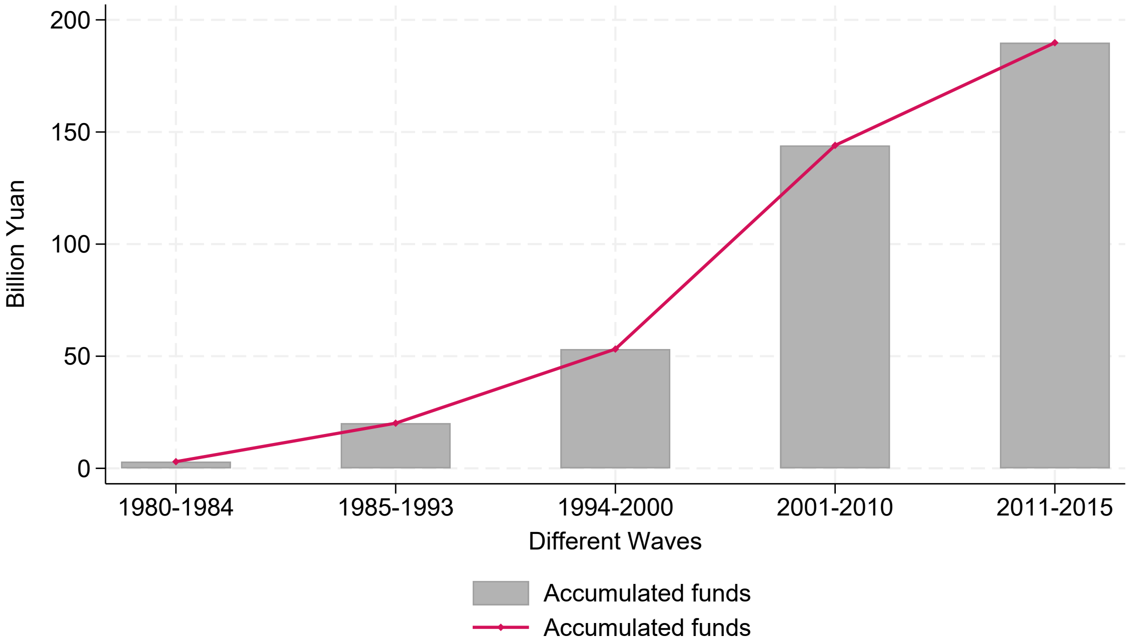
TABLE C4: Detailed Estimates from the Event-Study Design

	(1)	(2)	(3)
5-Year Prior	0.033*	0.032	0.037
	(0.019)	(0.021)	(0.023)
4-Year Prior	0.031*	0.030	0.032
	(0.019)	(0.020)	(0.021)
3-Year Prior	0.019	0.018	0.018
	(0.014)	(0.015)	(0.015)
2-Year Prior	0.000	0.000	-0.001
	(0.008)	(0.009)	(0.009)
1-Year Prior	-0.008	-0.008	-0.008
	(0.005)	(0.006)	(0.005)
1-Year After	-0.028***	-0.028***	-0.027***
	(0.005)	(0.005)	(0.005)
2-Year After	-0.034***	-0.034***	-0.033***
	(0.006)	(0.006)	(0.006)
3-Year After	-0.034***	-0.034***	-0.033***
	(0.008)	(0.008)	(0.009)
4-Year After	-0.043***	-0.043***	-0.043***
	(0.008)	(0.008)	(0.010)
Controls in 2010 × Post		✓	✓
Cubic Time Trend			✓
County FE	✓	✓	✓
Province × Year FE	✓	✓	✓
Observations	8150	8150	8150

Note: Coefficients and their 95% confidence intervals in each year are the estimated coefficients through Eq. (3.3), weighted by the PSM score. The dependent variable is the natural log of the income ratio defined in the main text. Controls include GDP per capita in 2010, agricultural share in 2010, fiscal revenue per capita in 2010. Then they are interacted with the post dummy. Then they are interacted with the post dummy. Only column (3) adds the cubic time trend of the controls. County and year-province fixed effects are all controlled. The standard errors are clustered at the county level and reported in parentheses, while *, **, and *** denote significance levels at 10%, 5%, and 1% respectively.

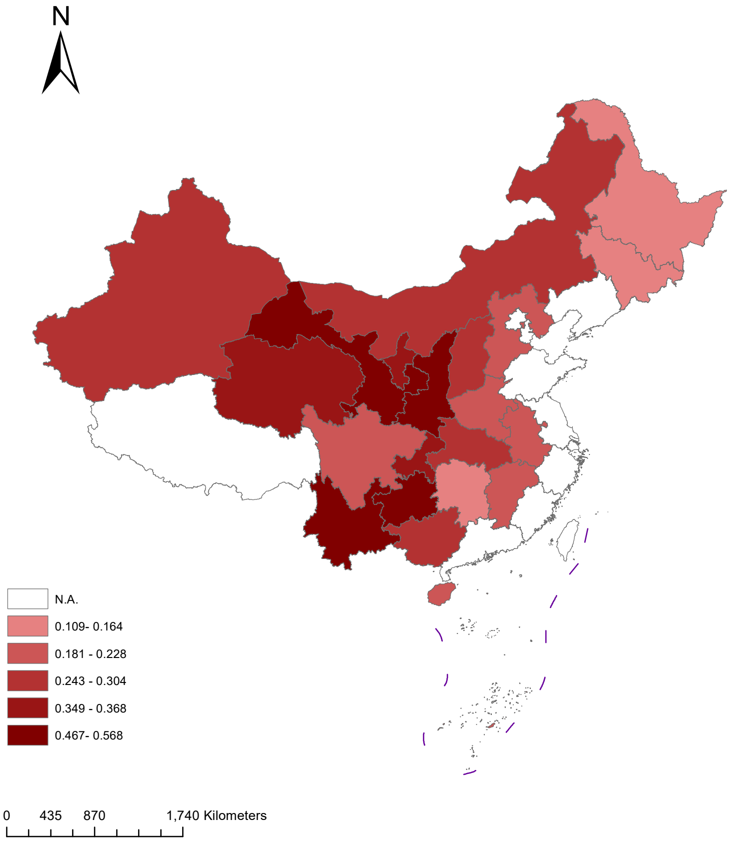
C.3 Figures

Figure C1: Fiscal Funds for Poverty Alleviation in Each Wave



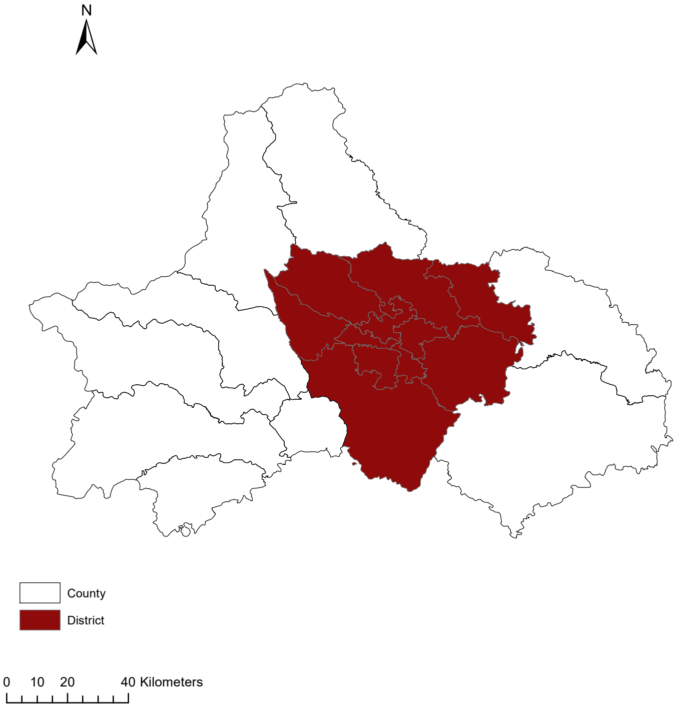
Note: Data are collected from an official report which lists the fiscal funds for poverty alleviation in each wave. The latest wave only displays the funds accumulated during 2011 and 2015, not 2020. See from `http://f.china.com.cn/2016-09/12/content_39281638.htm`.

FIGURE C2: The Ratio of Poverty Counties in Each Province



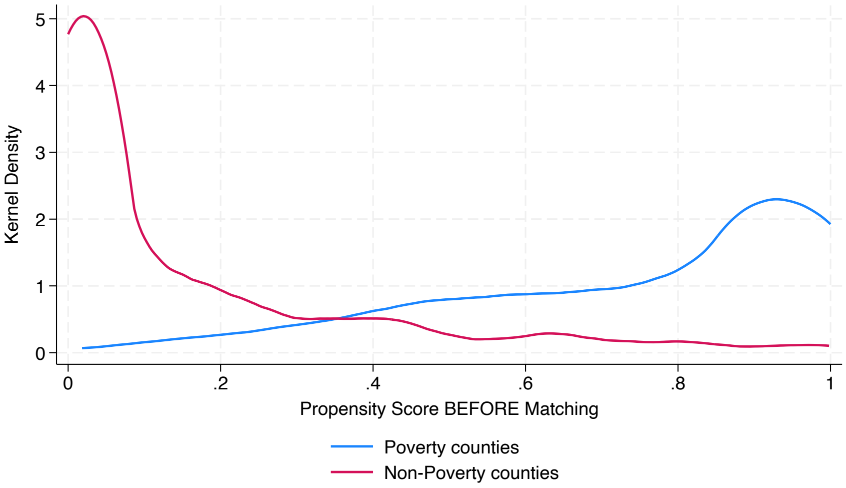
Note: Based on the administrative districts in 2014, the ratio of poverty counties is calculated and presented. The darker red colour represents a higher ratio of poverty counties. Besides, Xizang is an exception as is explained in the previous figure.

Figure C3: Administrative Units of an Example Prefecture

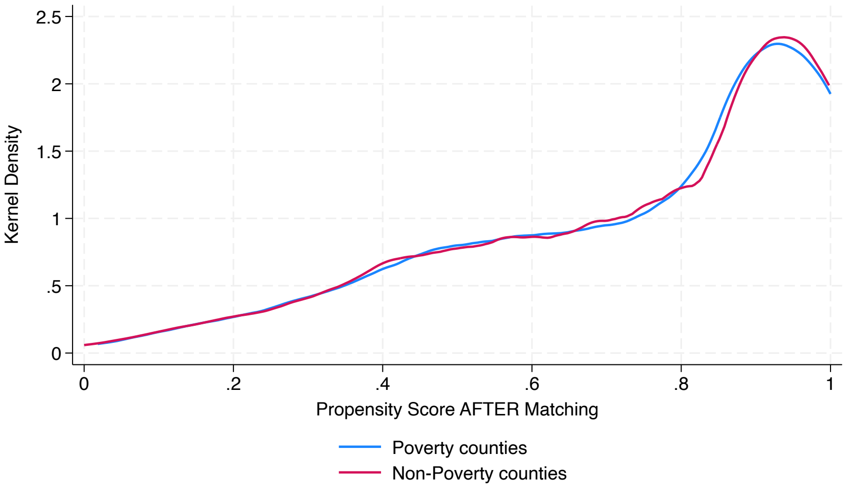


Note: Taking a Chinese prefecture as an example, all of its divisions are county-level in the administrative ranking. However, the dark red area is usually called districts, rather than counties as the rest of county-level divisions. Besides, districts often are the political and economic centre of the prefecture.

Figure C4: The Kernel Density of Propensity Score Before and After Matching



(A) The density of propensity score before matching



(B) The density of propensity score after matching

Note: Based on the kernel matching strategy, individual counties in the treatment and control groups are assigned propensity scores. The upper figure plots the kernel density of the score of the two groups before matching, while the lower plots after matching.

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