Boosting or Busting: The Impact of Industrial Zones on Local Income Dynamics and Labor Outcomes in Vietnam

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Abstract

This paper investigates the empirical relationship between the establishment of industrial zones and labor outcomes in Vietnam, focusing on variables such as local income inequality, levels of household annual income and expenditure per capita, hourly wages, and share of labor income. Using commune-level panel data constructed from Vietnam's national household survey dataset and a staggered event estimation strategy, the establishment of IZs leads to an increase in individual wage inequality, especially for women; a slight increase in hourly wages and share of labor income, but a decrease in expenditure per capita and rising house value. Policies ensuring fair wages and access to essential amenities, social safety net programs to assist vulnerable workers and reduce income disparities, and regulations to support female workers are recommended.

Keywords: *Industrial Zones, Labor Outcomes, Income Inequality, Labor Laws, Living Standards*

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1 Introduction

Ho Chi Minh's thoughts on Vietnam's development trajectory stated that increasing production and productivity must be strongly associated with the advancement of the people's welfare and living standards, which in turn encourages labor participation and sustained development (Ho, 1947). Therefore, the Vietnamese central government places great emphasis on achieving economic growth while simultaneously addressing social equity and promoting welfare advancement (Central Committee of the Communist Party of Vietnam, 2008). As one of the main policy arms of a manufacturing-led growth strategy, the development of multiple industrial zones (IZs) throughout the country is expected to promote industrial development, attract foreign investment, and stimulate job creation (Central Committee of the Communist Party of Vietnam, 1997). As of the end of 2021, 397 IZs have been established in 61 out of 63 provinces, among which 291 IZs have been put into operation, and 106 IZs are under construction (MPI[1], 2021).

The effects of IZs on local economies are generally ambiguous. On the one hand, they have attracted foreign investment and technology, as well as domestic enterprises, and have contributed to the structural transformation of the Vietnamese economy, increasing export-import turnover, and thereby creating employment opportunities. On the other hand, though, the development of IZs led to the loss of productive agricultural land, low pay, and poor working conditions for industrial workers, who generate a significant portion of GDP[2]. Labor organizations and public agencies have raised concerns regarding the pressing issues of unmet livable wages (IWTU[3], 2018, 2022; Oxfam, 2019), lack of social infrastructure and recreational support for workers in IZs (MCST[4] & VGCL[5], 2021). Then, public policies ensuring workers' livelihood, are of foremost importance and directly impact Vietnam's equitable and sustainable development. Understanding the relationship between the establishment of IZs on local income dynamics and labor outcomes

[1]Ministry of Planning and Investment

[2]According to Vietnam National Union of Workers in Industry and Trade (2017), 15 million workers nationwide account for only about 33% of the total labor force and 17% of the country's population, but they contribute more than 60% of the GDP and more than 70% of the state budget.

[3]Institute for Workers and Trade Union

[4]Ministry of Culture, Sports and Tourism

[5]Vietnam General Confederation of Labour

is central to the policy question of how to channel better regulations and supervision of IZs' operations and effectively support disadvantaged groups while promoting industrial growth.

In my thesis, I examine the impact of establishing an industrial zone (IZ) on local income dynamics and labor outcomes at the commune level in Vietnam. I use panel data on local characteristics for 6 years from 2010 to 2020 with a 2-year interval, constructing from the Vietnam Household Living Standard Survey (VHLSS). First, I construct a commune-level dataset from household and individual survey data, containing data on local income dynamics and labor outcomes at the commune level in Vietnam, focusing on outcomes such as income inequality, average household income and expenditure per capita, average hourly wages, house value, and share of labor income[6] (using household weights for the aggregation). Second, I employ web scraping techniques to gather data on IZs' addresses, years of establishment, and total areas, and define "treatment" (i.e., IZ establishment) at the commune-level.

To identify the impact of IZ establishment on local labor outcomes, my empirical strategy is a staggered event study design, exploiting the heterogeneity in treatment timing of IZ establishment in a commune, in which I first employ propensity score matching for selecting a comparable control group. For estimating the propensity score, I use variables such as the area of the communes (in squared kilometers), number of villages, residing households and individuals, number of emigrants and immigrants, main ethnicity, region, whether they are in a remote location, and infrastructural development level[7]. For this identification strategy to work, it has to be the case that the treatment and control groups are nearly identical or have a parallel trend in outcomes prior to the treatment. In other words, there would be no differences in outcomes for these two groups in the absence of treatment.

My main results are the following: First, in the short- and medium-term, i.e. up to 15-17 years after, IZ establishment has a slightly negative impact on household expenditure per capita. Second, in the short- and medium-term, IZ establishment has a positive impact

[6]See detail variable description in Section 4.1.1
[7]See detail variable description in Section 4.1.3

on average individual hourly wages but accompanied by an increase in wage inequality. Third, in the long run, from 17-20 years onward, IZ establishment leads to higher housing prices, lower household income inequality, and higher household expenditure. Some of my results confirm existing findings, such as increases in housing prices, and rising individual income inequality.

In what follows, in Section 2 I overview the background and institutional settings related to the development of industrial zones (IZs) in Vietnam, the process of establishing IZs, the current institutional framework surrounding them, and the socioeconomic challenges and discussions regarding their operations. The Decree 36/CP issued in 1997, on Regulations on industrial zones, export processing zones, and high-tech zones has placed IZs development as a key policy instrument in Vietnam's industrialization strategy. The implementation of Decree 36/CP resulted in a significant increase in the number of IZs, as it provided local governments with incentives and legal mechanisms to attract investment toward these zones. These incentives included fiscal benefits, favorable loans, and subsidies for infrastructure construction. IZs have attracted both foreign and domestic investment, contributing to advanced technology and economic transformation. However, they also brought negative consequences such as loss of agricultural land, low wages, and poor working conditions. Labor organizations have raised concerns and called for improved policies for workers.

In Section 3, I review the related literature exploring the impact of IZs on socioeconomic outcomes in other countries, and relevant studies on the impact of IZs in Vietnam. In general, the scholarship on IZs' relationship with labor outcomes has mixed findings. Some empirical results suggest that IZs/industrial clusters increase employment and entrepreneur probability (as in Italy - De Blasio & Di Addario, 2005), promote regional economic growth and rural household income, reduce the poverty rate (as in China - Guo, Jing, Xu, & Yang, 2020, 2022; Park, Shen, & Chen, 2022), and lead to a somewhat decrease in inequality (as in Cambodia - Brussevich, 2020). However, IZs have also been found to decrease job mobility for white-collar workers (as in Italy - De Blasio & Di Addario, 2005), to increase inequality across occupations and regions (as in China - Guo,

Jing, Xu, & Yang, 2020, 2022; Park, Shen, & Chen, 2022), and to unchanged wages but to increase land values (as in Cambodia - Brussevich, 2020). Regarding literature specific to IZs/industrial clusters in Vietnam, most research focuses on their spatial patterns, management, roles in FDI attraction, and contribution to regional and national poverty reduction (e.g., Nguyen & Tien, 2021; Do et al., 2020; Viet & Thao, 2013). There are some field and qualitative studies investigating health and working conditions of factory workers in some particular IZs (Pham et al., 2019; Nguyen et al., 2020), but to date, there has not been a study that empirically estimates the impacts of IZs on local income dynamics and labor outcomes.

In Section 4, I describe the data source and discuss in detail how I construct variables and measurements used in this thesis. First, I construct commune-level panel data using six waves of household and individual survey data components from the VHLSS (2010-2020, with a 2 year-interval) and extract variables from the commune components of the surveys. Apart from aggregated data, all averaged variables are weighted means using household statistical weights, and I also use imputation strategies to impute the sample of commune general characteristics which are missing for the years 2016, 2018, and 2020. In addition, I web-scrape the data on IZs, such as name, address, year of operation, and area in hectares, with the main aim to construct my treatment variable, IZ establishment.

In Section 5, I present my identification strategy to estimate the causal effect of IZ establishment on local labor outcomes using a matched sample. In this strategy, I first employ matching using propensity score estimation, to select a comparable control group based on unchanged observable characteristics. Second, I use an event study strategy that exploits the staggered adoption of treatment across communes. I also discuss key assumptions for this extension of a difference-in-differences empirical strategy, under what conditions should they hold, and some potential biases. Essentially, constant treatment effects and parallel trend assumptions are required. By using a staggered treatment design, I relax the assumption of constant treatment effects over time. The parallel trend assumption, on the other hand, cannot be formally tested due to the lack of pre-treatment data. However, propensity score matching helps address this concern, because the matched

treated and control groups are comparable conditional on the specified observed characteristics, and therefore hold comparable expectation paths. Another assumption is that there should be no anticipatory effects before treatment. I cannot account for this due to the same reason of data limitations, and this assumption is likely to be violated due to known treatment path[8]. This violation of the no-anticipation assumption may lead to a downward bias in our estimates. In addition, because many control communes are in the neighborhood of the treated ones, so the estimates are likely to be downwardly biased due to spillover effects.

In Section 6, I present the main findings of this thesis[9]. The establishment of an Industrial Zone has a positive impact on average hourly wages, particularly from year 3 to year 14 after the IZ starts operating, by 1 to 34 thousand Dong, with the longer operation of the IZ in a commune leading to higher increases. Household per capita expenditure shows a slightly decreasing trend in the short- and long-run, with significance only found for negative estimates. The biggest difference in household per capita expenditure between a commune with an IZ and those without is a reduction of 7.6 million Dong 12 years after its establishment. However, in the long term, there is an increase of 3-21.5 million Dong. House value shows no immediate short-run reaction, but increases after 5 years, particularly from year 11 onward, with estimates ranging from nearly 5 million to 24 million VND per square meter, and most of them are statistically significant. The labor income share increases until 17 years after the first IZ establishment, then decreases, although not significantly. Labor income share can increase by 6 to 22 percentage points for communes with an IZ. While IZs do not have a clear impact on hourly wages for male workers, female workers experience a slight increase. They can earn from 6.6 to 60.6 thousand VND more compared to counterparts in communes without an IZ. For blue-collar workers, the presence of IZ(s) tends to increase hourly wages by up to 17 thousand Dong, with statistical significance observed in years 7 and year 9. Household inequality shows a decrease after the medium term but without significance. On the other

[8]The treatment timing is determined as the year when an industrial zone begins operating, which means there are preceding years during which the future treatment trajectory is anticipated, including the negotiation phase, license acquisition, and construction period.

[9]See detail variable description in Section 4.1.1

hand, income inequality between individuals expands until year 15 after IZ establishment, with statistically significant estimates. Taking only significant results into account, the interquartile range in individual hourly wage increases by 13.46 thousand Dong in year 1, and up to 26.46 thousand Dong in year 13.

I contribute to two strands of literature. First, I add to the literature on empirical studies of IZs and industrial clusters[10] and their impacts on local income dynamics and labor outcomes by extending it to the Vietnam context, which has not been done before. Second, I contribute to the literature that studies IZs in Vietnam by providing a quantitative analysis of the link between IZs and the locals' socioeconomic outcomes. This is the first study that uses a nationwide sample at the commune level to estimate empirically the impacts of IZs on local income dynamics and labor outcomes in Vietnam.

My findings could have some policy implications for targeting and identifying the channels through which public policies could address social equity while pursuing economic growth led by manufacturing and industrialization. Three main policy areas include guaranteeing adequate wages and access to social amenities, social safety net programs targeted at assisting vulnerable workers and diminishing income inequalities, and regulations designed to support female workers.

2 Background and Institutional Settings

Vietnam's economic growth has been driven by the manufacturing industry, in parallel with the development of multiple industrial zones (IZs) throughout the country, as a part of public policy tools aiming to promote industrial development, attract foreign investment, and stimulate job creation. As an industrial policy tool adopted by many developing countries, IZs are supposed to complement market forces by helping to address market failures, according to Zeng (2021). These failures may include problems with land markets, such as land unavailability, ownership issues, and resettlement challenges, as

[10]Unlike IZs, industrial clusters usually do not have a clearly defined geographical boundary. In the literature, it is up to the researchers to measure or identify an industrial cluster of industry agglomeration. Chain et al. (2018) systematize the literature on quantitative methodologies applied to the measurement of industrial clusters

well as inadequate industrial infrastructure that is necessary for industrial agglomeration (Zeng, 2021). In addition, poor regulatory and business environments, which are caused by coordination failures between the government and the private sector or within the government, are also among the challenges that IZs are meant to overcome (Zeng, 2021).

In Vietnam, after the Reform (Doi Moi) was carried out in 1986 and the Foreign Investment Law was enacted in 1987, Decree 36/CP was issued on April 24, 1997, on Regulations on industrial zones, export processing zones, and high-tech zones, which has placed IZs development as a key policy instrument in Vietnam's industrialization strategy. The number of IZs established shot up as this Decree gave incentives and legal pathways for local governments to attract investment towards IZs using many fiscal incentives, favorable loans, and/or subsidies for infrastructure construction. According to a report of the MPI in 10/2021, 291 IZs have been put into operation, and 106 IZs are under construction.

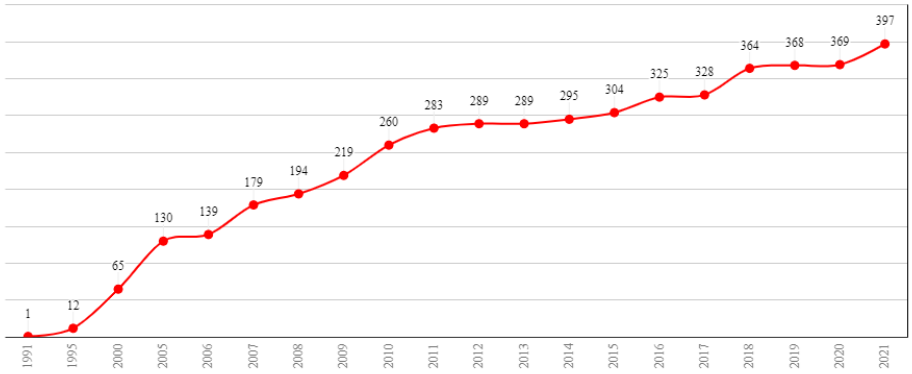


Figure 1: Number of established IZs in Vietnam (including ones that have not started operating). Source: Vietnam Industrial Zones (VIZ)

An Industrial Zone (IZ) can be established by the central government or authorized by it and supervised by the provincial management board of industrial zones. Before constructing and operating an IZ, the investor must obtain a Certificate of approval of investment or a Certificate of investment registration and project implementation. The requirements for obtaining this authorization are outlined in Decree 35/2022/ND-CP[11]. Essentially, an IZ is defined as an area with definite geographical boundaries, specialized

[11]Revised from past decrees: 82/2018/ND-CP, 164/2013/ND-CP, 29/2008/ND-CP

in manufacturing industrial goods and providing services for industrial production. Its establishment must comply with approved regulations on IZ development, have favorable conditions or the ability to build technical and social infrastructure, and integrate industrial park development planning with urban development planning, population distribution, housing, and social amenities for workers in industrial zones.

IZs have attracted foreign investment and technology, as well as domestic enterprises, and have contributed to the structural transformation of the Vietnamese economy, increased export-import turnover, and created employment for the labor force. More specifically, until 2012, they attracted US$86 billion in domestic and foreign investment, generating 23% of industrial production value and 25% of export turnover, contributing US$5.9 billion to the state budget and creating about 1.76 million jobs (Hung, 2012).

However, there are also negative consequences associated with the development of industrial zones, including the loss of productive agricultural land, low pay, and poor working conditions for industrial workers, who generate a significant portion of GDP. According to surveys done by the IWTU (2018 & 2022), the average income of workers in IZs' enterprises has improved but remains at a low level. In addition, workers are often required to work overtime, putting in strenuous efforts with long hours. While workers receive additional income from bonuses, allowances, subsidies, and other forms of support from their employers, their overall income still falls short of meeting the standard of living and daily expenses (IWTU, 2018, 2022). The VGCL and other labor organizations have been calling for better public policies that aim to better regulate and supervise the operation of IZs and support disadvantaged groups when promoting industrial growth.

3 Literature Review

This part of the paper presents a review of the relevant literature on the relationship between IZs as well as industrial clusters and socioeconomic outcomes. This strand of literature is part of an extensive scholarship on industrial clustering. Theoretical analysis of industrial agglomeration has focused on spatial patterns (Krugman, 1993; Gokan et al., 2016), the benefits of agglomeration effects through the role of intermediate good produc-

tion (Fujita & Hamaguchi, 2001), technological and knowledge spillover (Krugman, 1991; Feldman, 1999; Berliant et al., 2006; Alkon, 2018), concentrated labor pools (Figueiredo et al., 2014, Berliant et al., 2006), economies of transport density (Mori & Nishikimi, 2002; Behrens et al., 2006), poverty alleviation (Nadvi & Barrientos, 2004, Wu el al., 2021), and industrial growth and competitiveness (Schmitz & Nadvi, 1999; Giannecchini et al., 2018). Empirical research has examined cluster trends (Montana & Nenide, 2008), clustering policies (Weijland, 1999), collective actions in clusters (Kennedy, 1999), and the role of industrial clusters in facilitating growth (Sonobe et al., 2003). Notably, Grover et al. (2021) conducted a meta-analysis on developing countries, while other studies have provided case-specific analyses of industrial agglomeration. In what follows I present the empirical works that look into the positive and negative impacts of industrial clustering/IZs on some socioeconomic outcomes in both developed and developing countries.

Gibbs and Bernat (1997) conducted an early study on the impact of industrial clusters on labor market outcomes in the US. They examined the relationship between industry clusters and workers' earnings in rural and urban labor markets. The authors defined industry clusters as groups of establishments located near each other, sharing input needs or engaging in supplier-customer relationships. Their findings using traditional OLS revealed that workers employed within clusters earned more compared to similar workers outside the clusters. Moreover, the wage premium for cluster-employed workers was twice as high in rural markets compared to urban markets, and the wage premiums varied significantly across industries.

In a more recent study, Fowler and Kleit (2014) examined the link between industrial clusters and poverty rates in the US. They analyzed county-level data, considering economic, demographic, and geographic factors. The study used empirical models incorporating a spatial weight matrix to account for spatial effects. Functional clusters were identified based on employment concentration within a commuting zone. Three cluster attributes were developed: cluster counts, depth of clusters in terms of industry mix, and the share of employment within a commuting zone belonging to a cluster. The findings showed that the presence of industrial clusters was associated with lower poverty

rates. Regions with dispersed employment across multiple industries within the same cluster performed better than those with concentrated employment in a single industry. However, not all cluster associations had positive effects on poverty rates.

De Blasio and Di Addario (2005) conducted a study on industrial agglomeration in Italy, specifically focusing on its impact on wage structure, employment probabilities, and worker mobility. They analyzed data from the Survey of Household Income and Wealth, which includes information on wages, education, and work experience at the microeconomic level. The study examined specialized "Industrial Districts," which are spatially concentrated clusters of small and medium-sized enterprises (SMEs) involved in specific stages of manufacturing production. The findings indicate that industrial agglomeration increases employment opportunities, the likelihood of entrepreneurship, and vertical mobility for blue-collar workers, while decreasing job mobility for white-collar workers. However, no overall wage premiums or differential returns to seniority or education were observed.

Research on the impact of industrial clustering in developing countries, particularly China, is relatively recent and focuses on the policy question of China's economic growth and industrial performance. Guo, Jing, Xu, and Yang (2020) examined the effects of clustered entrepreneurial firms with different strengths and ownership structures on local economic growth, regional disparity, and urban-rural income inequality within a region. They used a county-level density-based index (DBI) to measure clustering in China, based on firm density within each industry in a county. The study created a panel of county-level cluster indices to measure the existence, strength, and ownership structure of industrial clusters. Data on per capita GDP, per capita household income, and other economic and demographic variables were collected from the China Socioeconomic Development Statistical Database. The authors employed two-stage estimations and Granger tests to identify causal relationships. Their results indicated that industrial clustering promotes regional economic growth and reduces urban-rural income inequality by increasing rural residents' income. However, these effects were not significant in highly urbanized regions, particularly megacities.

A recent study by the same authors (2022) investigates the impact of industrial clustering on rural income, poverty, and inequality in China. The study used a pooled cross-sectional dataset from the China Household Income Project in 1995, 2002, and 2007. Industrial clusters were measured using the DBI method, similar to a previous study by Guo et al. (2020), and data from the ASIFP provided detailed firm-level information. The authors employed traditional OLS regression; the findings indicated that clustering increased the total income of rural households, particularly nonfarm income, and reduced intra-county income inequality. Industrial clusters also helped lower extreme poverty rates and narrow income gaps. Specialization, urbanization, and industrialization did not have similar effects on rural household income or inequality.

Park, Shen, and Chen (2022) investigated the impact of high-tech clusters on inequality, with a specific focus on the Chinese industrial policy "Made in China 2025" and its effects on labor demand, wages, and living costs across regions and skill groups. The study utilized various datasets, such as online job postings, city-level statistical yearbooks, housing rent databases, and firm registration records. Through data analysis, the authors examined labor market dynamics, economic outcomes, housing market trends, and firm locations. Propensity score matching (PSM) and event-study analysis were employed to compare pre-policy local characteristics and create a comparable control group. The findings revealed an increase in labor demand and wages for non-routine occupations in the pilot cities, while routine occupations did not experience significant wage changes. However, living costs surged in the pilot cities after policy implementation, suggesting a potential rise in inequality. Spillover effects were analyzed by comparing neighboring cities of the pilot group with those in the control group, indicating a short-term decrease in labor demand and wages in neighboring areas, exacerbating regional inequality.

Brussevich (2020) conducted research on the impact of Cambodia's Special Economic Zones (SEZs) on local labor markets, focusing on a bounded zone for economic purposes. The author constructed a geo-tagged SEZ database containing information on location, entry, firms, investment, age, and employment. This dataset was matched with Cambodia's household survey data at the district level from 2007 to 2017. The event-study

methodology was employed to analyze changes in employment, wages, income levels, income inequality, price levels, and high school dropout rates following SEZ entry. Two strategies were used to identify causal effects: an inverse propensity score based on initial district characteristics and an alternative control group with similar characteristics. The findings indicate an increase in female employment within SEZs, limited effects on aggregate formal employment share, predominantly local labor hired by foreign-owned firms in SEZs, a decrease in inequality in hosting districts, unchanged wage levels, and rising land values. Agglomeration effects were found to be limited, and small spillovers were observed in neighboring districts, including increased high school dropout rates and slight positive effects on female employment.

Regarding the literature that studies industrial clustering in Vietnam, a work analyzing this matter using a theoretical approach is by Kikuchi (2007) who builds theoretical models and investigates the economic agents that are crucial to establishing IZs and spreading economic growth throughout a country. Empirically, in general, the academic literature on industrial clustering in general and/or industrial zone policy in Vietnam, to the best of my knowledge, has limited its focus on the spatial pattern (Nguyen-Chi et al), attracting FDI (Nguyen & Tien, 2021; Hoang et al., 2022, Tien & Nuong, 2020), management practice and performance (Do et al., 2020; Massard et al., 2018) and contribution to poverty reduction (Viet & Thao, 2013). There are some case studies that investigate health and working conditions of factory workers in some particular IZs. For example, Pham et al (2019) investigated health disparities between local and migrant industrial workers in Vietnam. Their cross-sectional study included 289 participants from three industrial areas. Their findings revealed that migrant workers experienced more health problems compared to local workers. Nguyen et al (2020) examined the determinants of health outcomes among industrial zone workers in Hanoi. By analyzing 501 surveys from households living in informal sector housing within industrial zones, this study sheds light on the impact of living and working environments in industrial zones on worker health, revealing that gender significantly influenced health, with women experiencing poorer outcomes across all measured indicators.

In sum, there is some empirical evidence of the effects of IZs and industrial clusters on individuals' socioeconomic outcomes for both developed and developing countries, namely Italy, China, and Cambodia. Some of them found positive relations between IZ establishments and employment, regional economic growth, and rural household income. Others suggest a negative association with job mobility for white-collar workers, occupational and regional inequality, and a decrease in living standards. Regarding literature specific to IZs/industrial clusters in Vietnam, there has not been a study that empirically estimates these impacts on local income dynamics and labor outcomes at the national level. Given that, this paper contributes to the aforementioned literature by extending the study of the relationship between IZs and local socioeconomic outcomes to Vietnam, and presenting new empirical evidence on that link.

4 Data

4.1 Commune-level panel data, constructed using VHLSS

First, the data set to be used for analyzing labor market outcomes is a commune-level panel data, constructed using the Vietnam Household Living Standards Survey (VHLSS) from 2010 to 2020, with a 2-year interval. The survey is conducted by the General Statistics Office of Vietnam (GSO VN) and has household and individual components, which provide information on basic demographic characteristics related to living standards; education level; health and healthcare; labor and employment; income; consumption expenditure; durable goods; housing, electricity, water, and sanitation facilities; participation in poverty alleviation programs; household business; and a commune component which surveys the communes' general characteristics (GSO VN, n.d.). The VHLSS has a nationwide sample coverage that was representative at national, regional, urban, rural, and provincial levels; Table 7 (appendix) reports details on sample size for 6 waves from 2010 to 2020. From 2002 to 2009, VHLSS has been conducted regularly every two years. From 2010 to 2020, these surveys are conducted annually, the odd-numbered-year surveys only collect data on demographics, employment, and income (GSO VN, n.d.). Prior to 2010,

administrative units were coded based on the old coding system which could not be used to match with the new one since 2010. I also do not have access to odd-year surveyed data, therefore, the largest sample that could be used in this paper is the 6 most recent years 2010, 2012, 2014, 2016, 2018, and 2020.

Using the information on demographic and socioeconomic characteristics of the household and individual, I construct panel data with communes as the principal units of analysis, using weighting adjustments[12]. Household weight data is missing for the year 2018, so for this year the averaged values are non-weighted. I also extract commune general characteristics from the commune components of the surveys. Monetary variables are recorded in units of 1 thousand Dong. In what follows I describe in detail the extracted and constructed variables[13].

4.1.1 Main outcome variables

The main outcomes are average household income and expenditure per capita, average wages per hour, average wages per hour for blue-collar workers, men and women, the share of labor income, housing value, and inequality measurements.

Average household income per capita is the commune's weighted mean of a household's total annual non-capital income divided by household size. Specifically, for each household observation, I aggregate their surveyed income from employment; from farms, forestry, and aquaculture activities; from domains of production and business, non-agricultural, forestry, and aquaculture services; the processing of agricultural, forestry, and aquatic products; and other non-capital income such as gifts or social benefits. I then divided this total income by number of household members. Then for each commune in each year, I take the weighted mean of that variable as the average annual household income per capita.

Similarly, average household expenditure per capita is the commune's weighted mean

[12]Except for the number of households and individuals surveyed, all other aggregated variables are calculated using household weights.

[13]It is worth noting that, one should analyze the resulting estimates with care. Even though the average data is constructed using weighting adjustments, which makes the sample representative, the number of surveyed households and individuals in each commune is very limited

of a household's total annual consumption expenditure divided by household size. Things that go into this calculation are expenditures on food and drinks, non-food items, fuels, and recreation and cultural activities. This aggregated annual expenditure is then divided by household size for each household observation, and the commune's average value is calculated using weighted mean.

Average wages per hour is the weighted mean of an individual's last month's compensation from employment divided by hours worked. Taking data on employment from the individual component of the surveys, I calculate each person's hourly wages by dividing their last month's income by last month's total working hours, only for people who reported being employed and getting paid. The commune's average value is then calculated by taking the mean of this variable within that commune, adjusted for individuals' household weight. This variable is recorded for different groups (blue-collar workers[14], men, and women).

Share of labor income is the proportion of a commune's surveyed individuals' total income that comes from labor compensation, or wages. To calculate this, first I aggregate two values, the total income, and income from employment from all surveyed individuals in the commune, using individuals' household weights. Second, I divide income from employment by the total income to get the share of labor income in the commune's total income.

House value is the commune's weighted mean of households' accommodation's worth[15] divided by their total residential area. For each household, its accommodation's worth is decided by asking if the whole accommodation were put on sale, how much it would be worth. I then divide this value by the household's total residential area and get the house value in thousand Dong per square meter. The next step is, similarly, taking the commune's weighted average using household weights.

Inequality measurements are the interquartile range between the 25th percentile and the 75th percentile, for household income per capita and for individual hourly wages. The

[14]See Table 8: Classification of Employment and Economic Activities

[15]From questionnaire: "If the whole accommodation were now put on sale, how much do you think it would be worth?"

larger this range, the more unequal a commune is.

4.1.2 Variables used for matching

Commune characteristic variables that are extracted from the commune component of the survey include: i) the area of that commune, in squared kilometers; ii) the number of villages in the commune; iii & iv) the number of households and individuals registered as permanent residents in the commune; v & vi) the number of people emigrating from and immigrating to within the surveyed year; and vii) the region that the commune is located in. The survey also provides information on viii) if the commune is classified as a poor commune[16], and ix) if it is located in a remote area or not[17]. The main ethnicity of the commune population is also an important factor; as Vietnamese society is very homogeneous, other ethnic minorities tend to have fewer advantages in languages, communication, and education. I construct variable x) "Kinh" equals one if one of the three most populated ethnic groups in the commune is Kinh, the majority in Vietnam[18], and zero otherwise. To proxy for the levels of infrastructure development in the commune, I use binary variables indicating xi) if there is at least one motor road connecting to the commune's People's Committee office, xii) connecting to the houses that surveyed households residing in, and xiii) if the commune is connected to the national electricity grid.

These data on commune general characteristics, which I plan to use for matching between the treated and the control groups using propensity score estimation, are missing for 2016, 2018, and 2020. One way to overcome this is doing imputation based on previous years' trends. For variables that are not likely to change such as total area, Kinh, region, and remote location, I impute the missing data by the last observation carried forward (LOCF) method. For binary variables indicating infrastructure development, it is possible that they changed over the years. However, since both the space- and time-based

[16]According to the government's Decree No. 135/1998/QD-TTg.

[17]"Remote areas" are defined as sparsely populated areas, located deep in forests, mountains, or wetlands, far from economic and cultural centers, inconvenient traffic, and difficult to travel to; the economy is often backward and underdeveloped (according to the Government's Decree No. 114/2017/ND-CP.)

[18]More than 85% of the population

correlation is seriously missing, it is difficult to accurately impute this continuous missing sample using temporal or spatial methods (Liu et al., 2020). Therefore, I also use LOCF to impute these variables and make an assumption that the infrastructure conditions in the missing communes did not change from 2016 to 2020. For the number of households and individuals registered, as well as the number of immigrants/emigrants, I apply the interpolation and random forest method as suggested by Liu et al. (2020) for continuous missing samples, using data points at the communal level to impute the missing data values.

These variables are pre-determined, unchanged, and related to both selection into treatment and outcomes of interest, and therefore would be effective covariates when estimating the propensity score (Stuart, 2010, as in Harris, 2016). What is more, findings from simulation studies in medical research suggest that including covariates related to both the intervention and the outcome can effectively reduce most bias. (Austin, Grootendorst, & Anderson, 2007, as in Harris, 2016).

4.1.3 Control variables

For control variables to be used in the event study specifications, I construct the number of households and individuals surveyed, average household size, and average living area per capita. The first two are counts of the total number of households and individuals who were sampled into the survey in each commune. Average household size is the commune's weighted mean of each household's number of household members[19]. The living area per capita for each household is its total residential area divided by household size. I then take the weighted mean of this variable for each commune.

Average household size and average living area per capita could be used to proxy for the level of urbanization and modernization in each commune. The tight relationship between family size and the level of modernization and urbanization has been theoretically analyzed (Margavio & Mann, 1989; Hareven, 1976) and empirically proved (Paydarfar, 1975; Stinner, 1982; Mayowa, 2019), in which higher levels of modernization are linked

[19]From questionnaire: "Household members are those who share accommodation and meals from 6 months or more over the last 12 months, and share a pool of incomes and expenditures."

to a decrease in fertility rates and household size in urban areas. Similarly, existing literature shows a correlation between rural-urban categorization and the size of dwellings, indicating that rural areas generally have larger per capita living spaces compared to urban areas (Bian & Lu, 2014; Knight & Song, 1999; Bobkov et al., 2021). Unlike variables mentioned in section 4.1.2, while these two variables could very likely be associated with the treatment status (i.e. having an operating IZ), they are not pre-determined, and therefore are not used in the propensity score estimation and the sequential matching algorithm.

4.2 Industrial Zone data

Data on industrial zones are scraped from Vietnam Industrial Park and Investment Information Consulting Portal (VIPIIP), Vietnam Industrial Zone Portal (VIZ), and Tokyo Development Consultant Vietnam (TDC) websites. Owners of these websites are industrial service consulting agencies: Indochina International Consulting Co. LTD, Vietnam Industrial Zone Portal Corporation, and Tokyo Development Consultant Vietnam (TDC), respectively. They are both consulting agencies for IZ developer corporations, and brokers between those and manufacturing companies looking for industrial properties. The websites host information on IZs that have been licensed to operate, including those which have not been built yet. I wrote a program to scrape data from these websites for all industrial zones whose construction is finished and are operating in Vietnam, listed until 2020. The main data collected is from TDC websites, the other two are used to complement where TDC data is missing.

The extracted information includes the name, location, year of establishment, and total area. Locations of zones are collected at the commune level. The year of operation, the entry year, is considered the (construction) completion year recorded in the above websites. Information on covered area, investors, initial total investment, number of firms and employment, industrial rent, and warehouse rent, is desired, but since the above websites do not report this full information for all of the industrial zones, these variables can not be used due to severe missing data. Using the information on the Industrial

Zones' address, I merged this Industrial Zone data with the panel data at the commune level described above.

I create my main treatment variables: treat, equal to 1 if the commune is in the treatment group (has at least one IZ operating in its area); treated, equal to 1 if the commune has an IZ operating in its area at year t); time-to-treat, describes how long the first established zone has been operating; time-to-treat-pre, similar to time-to-treat but include how many years until the first zone was established, or the pre-treatment periods (these values are 0 in time-to-treat).

There are some communes in which more than one industrial zones are operating (22 out of 183 communes), but this double treatment effect is beyond the scope of this research. For this paper, one should not worry about the contamination of the double treatment on the longer-term estimates Because the kernel density plot in Figure 4 (appendix) indicates that the distance between IZs establishments in one commune (in terms of establishment time) is randomly distributed. Another note is that 81 out of 183 communes with IZs were never surveyed in any of the VHLSS waves. Since the commune data sample is representative of the population (GSO VN, 2011, 2013, 2015, 2017, 2022), I would argue that these non-surveyed communes are completely random and therefore, the surveyed communed with IZs are still representative of all.

4.3 Descriptive statistics

My sample is an unbalanced panel of 11,367 observations before and 1,008 observations after matching. Table 1 shows the summary statistics for my whole sample, before matching. Table 2 shows the summary statistics of the matched sample that goes into the event study specifications. The samples are unbalanced since there are some communes that are not surveyed in all 6 years 2010, 2012, 2014, 2016, 2018, and 2020. The maximum lag after treatment is 25 years and the maximum pre-treatment period is 9 years. Before matching, the percentage of poor communes represented in the sample is 19%, this number of remote communes is 25%. After matching, the numbers are 2.4 and 1.9%, respectively. Almost all of the communes surveyed are connected to the national grid.

While the mean hourly wages[20] of workers do not seem to change much after matching, that in general and gender-wise increased: 4.7 to 6.7 in general, 3.7 to 5.9 for female, and 5.9 to 7.7 for male wage earners.

Table 1: Descriptive Statistics for Full Sample

Statistic	N	Mean	St. Dev.	Min	Max
Province code	11,367	48.836	28.059	1	96
District code	11,367	495.169	274.648	16	973
Commune code	11,367	18,051.620	9,531.351	382	32,248
Year	11,367	2,014.340	3.085	2,010	2,020
Year IZ entered	252	2,006.667	6.584	1,995	2,019
IZ size (ha)	252	354.294	272.983	35	1,556
Treated	11,367	0.019	0.137	0	1
Treat	11,367	0.022	0.147	0	1
Time to treat	11,367	0.188	1.588	0	25
Time to treat (with pre)	11,367	0.177	1.607	−9	25
Area	11,367	35.026	50.147	0.170	830.270
Villages	11,367	9.482	5.515	2	44
Households	11,367	2,118.454	1,118.018	155	10,050
Individuals	11,367	8,862.463	4,842.027	726	47,800
Emigrants	11,367	96.928	193.199	0	3,112
Immigrants	11,367	87.496	212.822	0	7,232
Kinh	11,367	0.950	0.217	0	1
Region	11,367	2.921	1.298	1	5
Poor	11,367	0.193	0.395	0	1
Remote	11,367	0.251	0.434	0	1
Road to PC	11,367	0.986	0.117	0	1
Road to Surveyed HH	11,367	0.914	0.281	0	1
Electricity	11,367	0.992	0.090	0	1
Surveyed HH	11,367	4.642	3.475	2	24
Avg. HH size	11,367	3.841	0.966	1.000	9.333
Avg. living area	11,365	23.072	10.733	3.500	125.278
House value	11,365	5,374.082	6,508.417	0.000	243,333.300
Labor income share	11,367	0.273	0.238	0	4.323
HH income per cap.	11,367	102,329.400	243,916.300	1,364.222	6,656,857.000
HH expenditure per cap.	11,367	16,152.910	9,515.095	707.063	129,403.200
Surveyed individuals	11,367	25.121	21.817	3	280
Hourly wages	11,366	4.711	7.080	0.000	298.358
Surveyed females	11,366	12.718	11.501	1	162
Hourly wages (Female)	11,343	3.709	8.515	0	588.319
Surveyed males	11,361	12.410	11.078	1	143
Hourly wages (Male)	11,341	5.863	9.218	0.000	495.603
Hourly wages (Workers)	7,087	17.305	24.934	0.000	1,607.567
HH inequality	9,302	53,185.070	54,118.690	0	1,013,700
Ind. wage inequality	11,366	10.508	12.454	0.000	330.000

[20]in thousand Dong

Table 2: Descriptive Statistics for Matched Sample

Statistic	N	Mean	St. Dev.	Min	Max
Province code	1,008	55.038	26.870	1	94
District code	1,008	556.557	254.870	16	945
Commune code	1,008	20,051.560	8,746.138	397	31,618
Year	1,008	2,014.389	3.168	2,010	2,020
Year IZ entered	252	2,006.667	6.584	1,995	2,019
IZ size (ha)	252	354.294	272.983	35	1,556
Treated	1,008	0.217	0.413	0	1
Treat	1,008	0.250	0.433	0	1
Time to treat	1,008	2.118	4.936	0	25
Time to treat (with pre)	1,008	1.994	5.051	−9	25
Area	1,008	18.388	18.728	2.310	186.800
Villages	1,008	6.533	3.507	2	25
Households	1,008	2,704.087	1,155.421	365	7,270
Individuals	1,008	11,044.660	5,122.028	2,380	28,214
Emigrants	1,008	159.590	292.569	0	2,635
Immigrants	1,008	204.328	337.829	0	2,805
Kinh	1,008	0.977	0.149	0	1
Region	1,008	2.345	0.792	1	5
Poor	1,008	0.024	0.153	0	1
Remote	1,008	0.019	0.136	0	1
Road to PC	1,008	1.000	0.000	1	1
Road to Surveyed HH	1,008	1.000	0.000	1	1
Electricity	1,008	0.995	0.070	0	1
Surveyed HH	1,008	4.643	3.453	3	12
Avg. HH size	1,008	3.666	0.915	1.000	7.333
Avg. living area	1,008	24.916	12.308	4.000	110.833
House value	1,008	7,768.076	7,531.527	335.227	62,057.140
Labor income share	1,008	0.382	0.265	−0.363	0.997
HH income per cap.	1,008	65,645.810	119,905.400	3,898.806	2,132,864.000
HH expenditure per cap.	1,008	19,058.790	11,629.170	2,072.681	129,403.200
Surveyed individuals	1,008	23.471	19.290	3	162
Hourly wages	1,008	6.671	10.843	0.000	298.358
Surveyed females	1,008	11.982	10.318	1	94
Hourly wages (Female)	1,002	5.866	19.360	0.000	588.319
Surveyed males	1,007	11.500	9.644	1	77
Hourly wages (Male)	1,006	7.706	7.059	0.000	53.125
Hourly wages (Workers)	794	17.482	15.940	0.000	281.250
HH inequality	829	71,144.850	67,280.650	0	1,013,700
Ind. wage inequality	1,008	13.502	13.933	0.000	265.625

5 Methodology

5.1 Propensity Score Matching

5.1.1 Pre-analysis using non-matched data

One major identification issue is the non-random establishment of IZs. The literature points out that industrial clustering tends to be concentrated in regions with more developed and connected infrastructure, with more population, and theoretically, cheaper land prices and lower wages (Nguyen-Chi et al, 2022; Hoang et al, 2022, Yang et al, 2017). The t-test results for the difference between the main outcome variables of interest of the treatment and control groups are reported in Table 3. It shows that prior to the establishment of an industrial zone, the treated communes[21] have, on average, lower hourly wages, lower living standards (measured by annual per capita expenditure), and lower housing value. However, these differences are not significant and there are differences in interested outcome variables. Interestingly, Table 4 shows that IZ establishments seem to have a positive correlation with these outcomes, but a negative one with income inequality.

Table 3: T-test for mean differences in outcome variables, pre-treatment

Variable	Mean diff.	Mean control	Mean treat	p-value	LB	UB
HH expenditure per cap.	-684.52	15992.37	16676.90	0.600	-3318.39	1949.35
HH income per cap.	32884.31	103257.99	70373.68	0.126	-9686.70	75455.33
House value	-2276.23	5265.34	7541.56	0.014	-4067.78	-484.67
Labor income share	-0.02	0.27	0.29	0.659	-0.10	0.07
Hourly wages	0.03	4.59	4.56	0.975	-1.90	1.96
Hourly wages (Female)	-0.59	3.56	4.15	0.557	-2.63	1.44
Hourly wages (Male)	0.78	5.77	4.99	0.422	-1.18	2.74
Hourly wages (Workers)	6.16	17.25	11.09	0.001	2.68	9.64
Ind. wage inequality	-0.13	9.12	9.25	0.939	-3.63	3.37
HH inequality	-4366.66	52346.43	56713.09	0.591	-20760.53	12027.20

Because of the non-random nature of an IZ entry, the OLS estimator in a linear regression would be biased. For example, a commune in a remote, wetland area would not be selected to build an industrial zone, and is likely to have worse labor outcomes due to disadvantageous natural and economic conditions. Comparing those communes' outcomes

[21]Where data is available: only 33 observations of 12 out of 183 communes have pre-treatment observations

Table 4: T-test for mean differences in outcome variables, after treatment

x

Variable	Mean diff.	Mean control	Mean treat	p-value	LB	UB
HH expenditure per cap.	-8229.29	15992.37	24221.67	0.000	-10196.00	-6262.59
HH income per cap.	44086.40	103257.99	59171.59	0.000	36056.13	52116.67
House value	-5300.36	5265.34	10565.70	0.000	-6429.20	-4171.52
Labor income share	-0.26	0.27	0.53	0.000	-0.30	-0.22
Hourly wages	-6.24	4.59	10.83	0.000	-9.00	-3.48
Hourly wages (Female)	-7.95	3.56	11.51	0.004	-13.31	-2.59
Hourly wages (Male)	-4.72	5.77	10.49	0.000	-5.80	-3.63
Hourly wages (Workers)	-2.85	17.25	20.11	0.001	-4.53	-1.18
Ind. wage inequality	-5.05	9.12	14.17	0.000	-6.88	-3.22
HH inequality	-43778.71	52346.43	96125.14	0.000	-53452.90	-34104.51

with another commune having industrial zones located in plain land areas with motor road access would generate a strongly biased treatment effect. Therefore, I use propensity score matching to match the treated and control group based on observables such as geographic region, and infrastructure development level. These are unchanged characteristics (that for infrastructure according to the assumption made above), potentially have an impact on whether a commune is selected into treatment or not, and because they are related to local's economic conditions, they are likely to have a relationship with the interested outcome variables as well. Matching on additional pre-treatment outcome variables is desired too, for example, initial average wages, income, expenditure, and education levels prior to the establishment of IZs would help to select a more comparable control group. However, due to data availability (I only have data from 2010 - 2020 while a majority of the industrial zones were built before that), I could not match by pre-treatment characteristics but only by unchanged observables which are not subject to endogeneity bias.

Table 5: T-test for mean differences in covariates, before matching

x

Variable	Mean diff.	Mean control	Mean treat	p-value	LB	UB
Area	17.25	35.41	18.16	0.000	15.38	19.11
Villages	2.40	9.54	7.13	0.000	1.94	2.86
Households	-662.79	2103.76	2766.55	0.000	-801.20	-524.39
Individuals	-2550.76	8805.91	11356.68	0.000	-3153.87	-1947.66
Emigrants	-65.24	95.48	160.72	0.010	-114.96	-15.52
Immigrants	-189.84	83.29	273.12	0.000	-240.40	-139.27
Kinh	-0.03	0.95	0.98	0.000	-0.05	-0.02
Poor	0.17	0.20	0.02	0.000	0.15	0.19
Remote	0.23	0.26	0.02	0.000	0.21	0.25
Road to PC	-0.01	0.99	1.00	0.000	-0.02	-0.01
Road to Surveyed HH	-0.09	0.91	1.00	0.000	-0.09	-0.08
Electricity	0.00	0.99	0.99	0.965	-0.01	0.01

5.1.2 Propensity score estimation

I estimate the propensity score for each commune by running a logit model in which the outcome variable is the treatment status of having an industrial zone operating:

$$p(x) = Pr(D_i = 1|X_i = x) \tag{1}$$

The specification employs vector X of commune characteristics from Table 5 to calculate the propensity score for each commune, which is the probability of treatment (having an IZ operating) conditional on those characteristics. In other words, it is the predicted probability of being in the treated group, given the estimates from the logit model on a vector of observed covariates (Rosenbaum and Rubin, 1983). Based on this predicted probability, I perform nearest-neighbor matching without replacement and with Variable Ratio Matching equals 3 to match treated observations to their comparison units. When the ratio is greater than 1, each treated unit is given a value that represents the number of control units to which they will be matched (Ming and Rosenbaum, 2000). According to Ming and Rosenbaum (2000), although matching one treated unit with more than one control would lose some precision in the estimator, this approach typically produces a better balance than fixed ratio matching (match 1 treated vs 1 control). Additionally, I impose exact matching on region and remote variables, to ensure that the comparison is done for communes from the same geographical region and have the most similar natural

Table 6: T-test for mean differences in covariates, after matching

x

Variable	Mean diff.	Mean control	Mean treat	p-value	LB	UB
Area	0.30	18.46	18.16	0.785	-1.86	2.46
Villages	-0.80	6.33	7.13	0.002	-1.31	-0.29
Households	-83.29	2683.27	2766.55	0.308	-243.51	76.94
Individuals	-416.03	10940.65	11356.68	0.245	-1118.49	286.43
Emigrants	-1.51	159.21	160.72	0.955	-54.12	51.10
Immigrants	-91.73	181.40	273.12	0.001	-146.71	-36.75
Kinh	-0.01	0.97	0.98	0.342	-0.03	0.01
Poor	0.00	0.02	0.02	1.000	-0.02	0.02
Remote	-0.01	0.02	0.02	0.538	-0.03	0.01
Road to PC	0.00	1.00	0.99	0.512	-0.01	0.02

Notes: Variables Road to Surveyed HH and Electricity are removed by t-test command because of zero variance.

characteristics. The total number of control units entering further analysis is 771 year-communes observations of 249 control communes, to be compared with 257 year-commune observations of 47 treated ones. In Table 6 I present the results of statistical tests for differences in means of the matched samples. Except for the number of immigrants and the number of villages, all other differences are non-significant. In the next step of doing the staggered event strategy, I control for these variables as a robustness check.

Figure 5 (appendix) gives the empirical Q-Q plots for visualization of the distributional balance between treated and control groups, before and after matching. In line with the marginal differences shown in Table 6, matching outputs a better control group and reduces the confounding bias when estimating the average treatment effect to the extent that it manages to largely remove the differences in observable characteristics that plausibly determine the treatment status and my interested outcomes. According to Rosenbaum and Rubin (1983), even for a small sample, scalar propensity score adjustment is sufficient to eliminate bias resulting from all observable factors. In addition, Heckman et al. (1997) concluded that conditions for low bias in matching estimators are an ample set of variables related to treatment status and outcomes, that the nonexperimental comparison group is drawn from the same category (in their case: local labor markets) as the treated units, and that the measurement of the dependent variable is same for treated and control groups. All three conditions hold in this case. While I can not match based on

pre-treatment observations of outcome variables, covariates indicating commune natural characteristics and infrastructure level are argued to be sufficient to reduce the bias due to differences between the treatment and control groups, especially when these differences are related to treatment status and local income dynamics and labor outcomes, as discussed above. I also include remote and region in the conditional probability model and apply exact matching on these variables, so the matched treated and control communes would be drawn from the same category.

However, while providing bias reduction, PSM does not entirely eliminate the omitted variable bias (OVB), because it only accounts for observed covariates and not latent characteristics. In other words, the estimates still suffer from any hidden bias when the zero conditional mean (ZCM) assumption does not hold because of unobserved heterogeneity that could be correlated with the treatment status and outcome variables. This endogeneity that is caused by omitted variable bias, especially due to unobserved temporally invariant heterogeneity, could be effectively eliminated by using panel analysis. Considering this and the heterogeneous treatment timing, I employ a staggered event study design to estimate the causal relationship between industrial zones and local labor outcomes.

5.2 Staggered Event Study

I use an event study strategy that exploits the staggered adoption of treatment (here is the establishment of an industrial zone) across communes. An event study design is a staggered adoption design in which units are treated at various intervals, first treated at period t_0, and may or may not include units that are never treated (Sun and Abraham, 2021). The main specification with standard two-way fixed effect (TWFE) is as follows:

$$Y_{it} = \lambda_i + \delta_t + \sum_{l=0}^{L} \tau_{il} D_{itl} + \epsilon_{it} \tag{2}$$

where Y_{it} are the dependent variables of interest, namely: weighted mean of annual income and expenditure; hourly wages, for men, women, and blue-collar workers; labor income share, and inequality measure. Inequality is proxied by the differences between the 75th and 25th income quantiles, and the ratio of blue-collar workers' hourly compensation

compared to that of executive/leader positions.

The first two terms on the right-hand side are commune and year FE, respectively. The following term is a target treatment effect, where $D_{itl} = 1$ if district i has an IZ entry l period ago at year t, L is the number of lags covered in the samples. $D = 0$ for all observations of the control group. $l_{min} = 0$ because this specification does not account for pre-treatment periods, due to inadequate data. It captures the temporal heterogeneity in treatment effects, τ_i is then the coefficient of interest showing the average IZ effect on commune i, l years after the start of operation of an industrial zone. ϵ_{it} represents any unobservable factors that is not correlated with the treatment.

A causal interpretation of two-way fixed effects DiD estimates requires both treatment effects to be constant over time and a parallel trends assumption (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2022). By applying a generalization version of DiD strategy, staked-by-event design, I relax the first assumption by allowing for dynamics in treatment effects between groups (communes) over time. The second assumption means that in the absence of the event, the outcomes of the treatment and control groups would run parallel. However, this parallel trend assumption can not be tested formally, due to data unavailability of pre-treatment observations. This assumption could hold, arguably, for the matched sample using propensity score estimation, because intuitively the treated and control groups are comparable conditional on the specified observed characteristics, and therefore hold comparable expectation paths. This alleviates the concerns of endogeneity issues when estimating treatment effect to an extent to which this assumption is plausible.

Another identification assumption is that there must be no anticipatory effects in the years leading to treatment (Sun and Abraham, 2021). This assumption requires potential outcomes in any l periods before treatment to be equal to the baseline outcome on average. Due to the same reason as for the parallel trend assumption, I can not account for this anticipation effect. This no-anticipation assumption is likely to be violated. The treatment timing is defined as the year when an industrial zone starts its operation, so there are years before which the future treatment path is known, during the negotiation

phase, obtaining a license, and through the construction period. This potential anticipatory effect would lead to a downward bias in the magnitude of our estimation, if the outcome variables start to react even before the start date of an IZ operation.

Another potential bias is that some untreated communes neighbor the ones with an IZ(s), especially when matching is done with an exact match on region variable. The coefficient estimates from this design then should be regarded as the lower bound of the real treatment effect, because of the spillover between neighboring communes.

To check if the obtained coefficients are robust when controlling for other factors, namely the level of modernization and urbanization, proxied by household living area per capita and average household size, as well as the number of immigrants and the number of villages, I estimate the following equation:

$$Y_{it} = \lambda_i + \delta_t + \sum_{l=0}^{L} \tau_{il} D_{itl} + \Omega X_{it}^{`} + \epsilon_{it} \tag{3}$$

where the added term $X_{it}^{`}$ is a vector of controls with corresponding coefficients Ω.

6 Empirical Results and Analysis

In this section, I present the empirical results of the staggered event study approach to try to measure the impacts of Industrial Zones on some local income dynamics and labor outcomes. Figure 2 plots the estimates and the 95% confidence interval of the effect of industrial zone establishments on some communes' economic outcomes of interest. In panels A and H, there is an outlier at year 19, so I complement Figure 2 by a shorter time span limited until year 19 after the first IZ establishment in a commune, to show more clearly the estimates and CIs for those years before. Table 10 (Appendix) presents results from Equation (3). Each specification corresponds to one dependent variable of interest; all have standard errors clustered within a commune. The measurement units when interpreting are percentage points for labor income share and 1,000 VND (one thousand Vietnamese Dong) for the rest of the variables.

The effects on household expenditure per capita are not straightforward. Results in

Figure 2: Effects of IZ Establishment

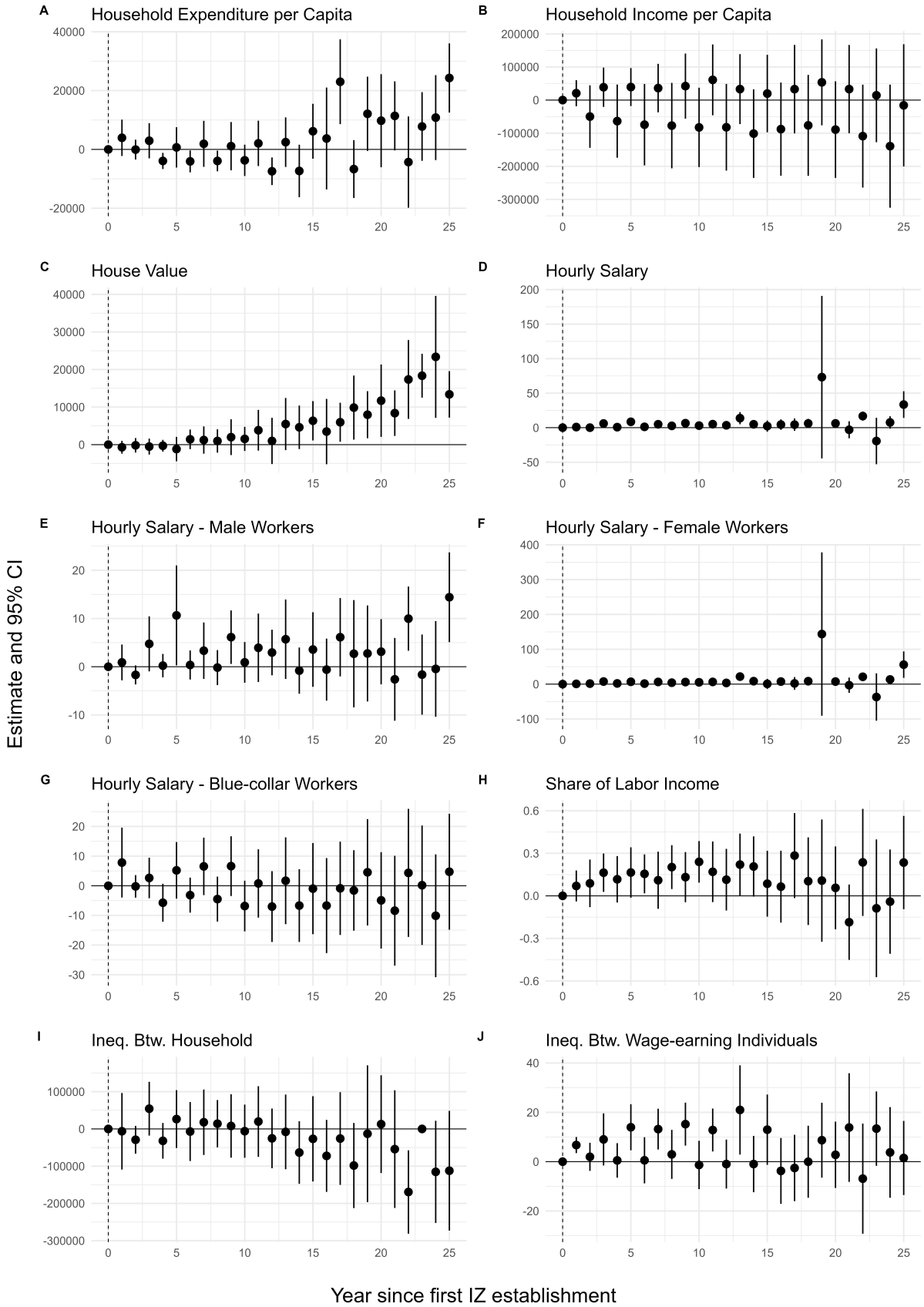
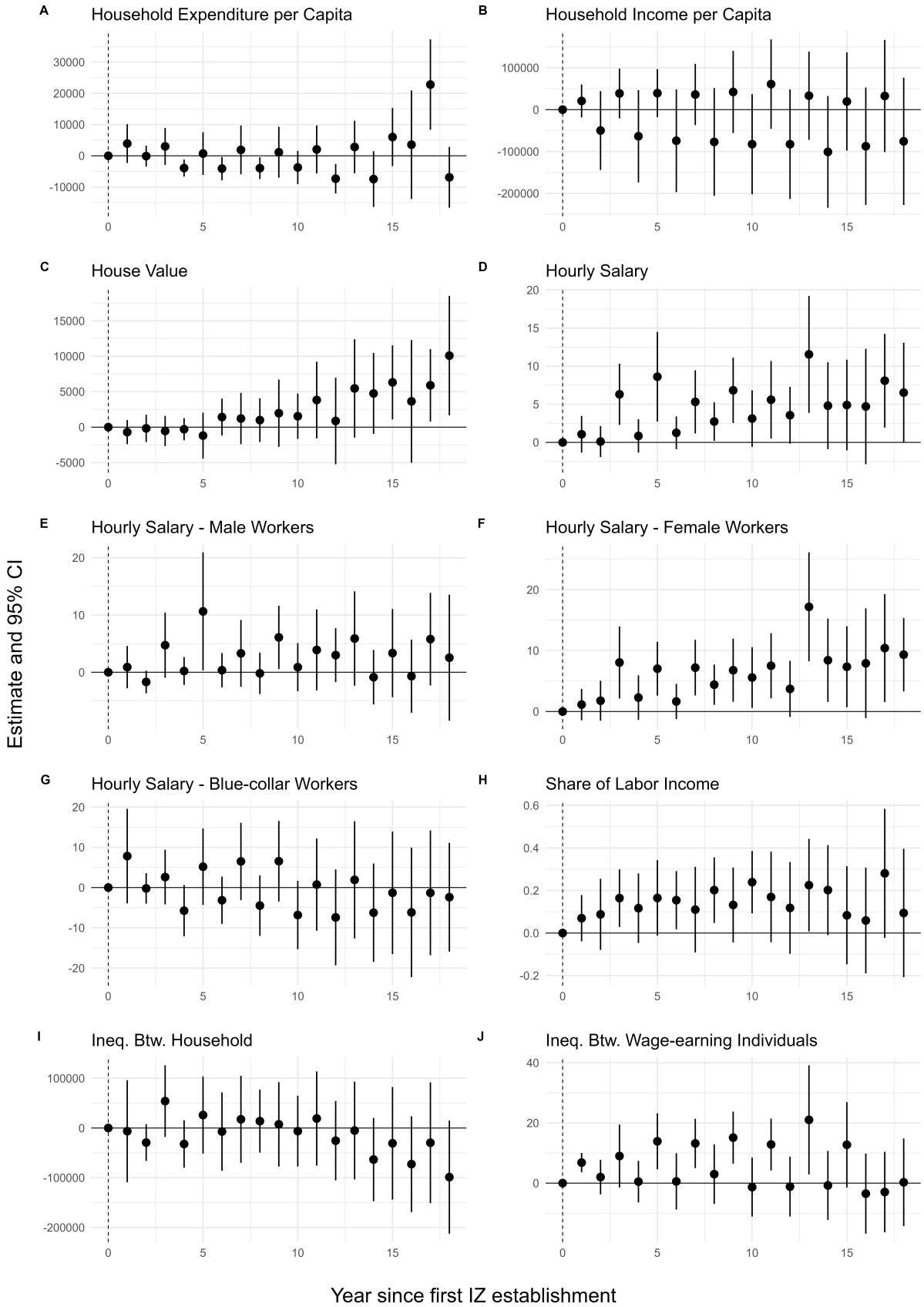


Figure 3: Effects of IZ Establishment, 19-year-span



column (1) shows that, in the short- and medium-run (less than 15-17 year), this variable is on a slightly decreasing trend, and significance is only recorded for negative estimates. The biggest difference between a commune with an IZ and its counterparts without one is 7.4 million Dong less in household expenditure per capita, recorded in year 12 after its establishment. However, things seem to improve in the long term, after 17 years, with an increase of 11-24.3 million Dong which is statistically significant.

The effect on household per capita income is reported in column (2). These results are more ambiguous. The coefficients fluctuate around zero, and none is statistically significant. Results from this specification indicate that IZ establishment has no significant impact on household per capita income.

House value shows no clear reaction to an IZ establishment in the immediate short-run but starts to hike after 5 years since the operation date. This increase is more pronounced from year 15 onward, with the magnitude from 6.4 million to 23.4 million VND per squared meter, and almost all of the estimates after 15 years are statistically significant, as shown in column (3).

The share of the commune's income coming from labor compensation (wages), or labor income, appears to increase up until years 15-17 after the first IZ establishment and go on a downward trend afterward, although this trend is not really pronounced and statistically significant. In general, labor income share could increase from 15.5 to 28.3 percentage points for communes with an IZ operating in its area.

Column (5) shows that the establishment of an Industrial Zone has a positive impact on the average hourly income for wage-earning individuals, and this relationship is significant mostly 3 years after the IZ starts operating. In general, the average hourly wage is increased by a magnitude from 2.6 to 33.45 thousand Dong, and the number tends to be higher the longer an IZ operates in a commune. There are some periods at which the coefficients are negative (after 2 years, 21 years, and 23 years), however, they are not statistically significant.

Columns (6) and (7) report the coefficient estimates of hourly wages for female and male workers, respectively. While an IZ does not have a clear impact on the male cohort,

female workers seem to benefit from a slight increase in hourly wages. In general, they could earn from 4.1 to around 21 thousand VND more than their counterparts living in a commune without an IZ. Looking back to specification (5) where impacts on general hourly wages are reported, we can see that this slight improvement mostly comes from the positive impacts on female workers' outcomes.

Regarding occupation, impacts on blue-collar workers' average hourly wages are shown in column (8). In general, the presence of IZ(s) has no significant impact on the wages of blue-collar workers, as the majority of the estimates do not significantly differ from zero. There is only one period in which it is statistically significant: at year 4 after an IZ starts its operation, the average hourly wages of blue workers decreased by 5.7 thousand Dong.

Columns (9) and (10) turn to the inequality measurements. As defined above in Section 4.1.1, inequality between households is the interquartile range between the 25th and 75th percentile of household income per capita, and inequality between individuals is the interquartile range between the 25th and 75th percentile of the hourly wages of wage-earning individuals. The larger this range in one commune, the greater the income inequality in such a commune. The estimates for household inequality seem to be on a generally decreasing trend, however, none of the estimates are significant. In contrast, income inequality between individuals seems to expand, at least up until year 15 after an IZ establishment, and these estimates are statistically significant. If we consider only significant results, the 25th-75th interquartile range in individual hourly wages increases by 6.7 thousand Dong in year 1, and up to 20.97 thousand Dong in year 13.

These results are robust when controlling for the level of modernization and urbanization of the communes, the number of surveyed households and individuals, the number of villages and the number of immigrants. By visually inspecting, Figures 6 and 7 reveal no immediate difference from the results in Figure 2. Tables 11 and 12 present in detail results from equation 3 with two different sets of control variables. The magnitude, directions, as well as a significant level of these results, can be evidence for the structural validity of our main estimates, i.e. the temporal dynamic impacts of IZs on socioeconomic outcomes.

One note of caution is that, because there are only 6 years of observations in our samples, these estimates may be biased because we do not observe prior years of those communes that have already had industrial zones established many years before and less than desired number of observations after treatment. However, unlike the asymptotic biases caused by the anticipation and spillover effect, this problem could be improved in future research using a larger sample size.

In general, with the exception of one outlier at year 19 due to insufficient data points for estimation, there was a slight improvement in hourly wages, particularly for women, and an increase in labor income share in the short and medium term, while household expenditure per capita decreased. This suggests that in communes with industrial zones, there has been a shift in the labor structure from non-wage sectors to wage-earning employment. However, the increase in wages did not keep pace with the change in non-wage income, resulting in a decline in average annual expenditure per capita. This raises concerns about the well-being of individuals who rely on non-wage income sources.

Meanwhile, there was no significant improvement in hourly wages for male and blue-collar workers, as well as household income per capita, but an increase in individual wage inequality. Considering the rise in house value, and with that: the potential increase in living costs, low-wage workers would have difficulties making ends meet with their stagnant wages. This calls for policies to help address the discrepancy between rising living costs and stagnant wages, ensuring that workers can meet their basic needs and maintain a decent standard of living.

7 Conclusion

My thesis contributes to the understanding of the link between IZs establishment and local income dynamics and labor outcomes by providing new empirical evidence on its impacts on the local average income and expenditure, hourly wages, housing prices, and income inequality. Using the micro household data and general commune component from VHLSS for 6 years from 2010 to 2020, with a 2-year interval, I construct panel data of socioeconomic and other demographic, and geographic characteristics at the commune

level. The treatment data (information on IZs) is web-scraped and merged with the aforementioned panel data. To deal with the endogeneity problem of non-random selection of an IZ establishment, I applied a staggered event study design that exploits heterogeneity in treatment timing, incorporated with a propensity score matching strategy to select a comparable control group.

I find that, first, household expenditure per capita slightly decreases in the short and medium term, with significance only for negative estimates. However, there is an increase in the long run. On the other hand, household income per capita is not significantly impacted. House values exhibit a delayed increase, which becomes more pronounced after 5 years of operation. While labor income share initially increases but shows no significant trend afterward, average hourly wages for wage-earning individuals improve with IZ presence, particularly after 3 years of operation. Breaking down on gender and occupation wise, female workers benefit from higher wages, while impacts on male and blue-collar workers' wages are not significant. In addition, income inequality between individuals tends to expand, while household inequality decreases slightly but insignificantly. These results remain robust when accounting for various control variables.

There are certain limitations of this paper. Because there are limited data waves accessible, the key assumptions that are parallel trend and no-anticipation assumptions can not be formally tested. While I attempt to overcome the endogeneity problem by incorporating a staggered event study with a propensity score matching strategy, the endogenous effects of an IZ establishment can not be entirely ruled out. Given that and the potential spillover and anticipation effects discussed above in Section 5.2, this research's findings can serve as lower-bound estimations for the effects of an IZ establishment on local socioeconomic outcomes.

A potential avenue for future research would involve investigating the precise mechanism underlying this empirical relationship, including its connection to foreign direct investment (FDI), firm ownership, and structural changes within local and national economies. Such a study would require a more extensive dataset on labor market outcomes, spanning a sufficiently long period with adequate data points prior to the

treatment, as well as a comprehensive range of information on industrial zones (IZs). This information would encompass factors such as investment amount and sources, ownership forms, number of firms, employment figures, and industries. Conducting such a study would shed more light on the bigger picture of the interplay between local labor outcomes and the global value chain, highlighting the crucial role played by IZs in the manufacturing and supply chain.

8 Policy Recommendations

The findings of this study have several policy implications. First, to address the stagnant hourly wages for blue-collar workers, the decrease in household expenditure per capita, and rising housing costs, it is crucial for central and local governments to implement policies that ensure livable wages and access to social amenities. This can be achieved through: i) Strict enforcement of labor laws: Increase the frequency of examination and investigation into IZs' operations to ensure compliance with labor standards; ii) Revision of minimum wages: Regularly review and adjust minimum wages to align with the living standards in the communes where IZs are located; iii) Provision of affordable housing, healthcare facilities, and recreational support: Develop affordable housing projects, improve healthcare infrastructure, and establish recreational facilities in and around IZs to enhance the well-being of workers and their families.

Second, given the increase in inequality among wage-earning individuals, it lies in the judiciary of the central government to prioritize the implementation of social safety net programs to support vulnerable workers and reduce income disparities. Concrete steps include: i) Income redistribution policies: Implement progressive taxation systems to redistribute wealth and provide targeted financial assistance to low-income households; ii) Targeted welfare programs: Design welfare programs that specifically address the needs of vulnerable groups, such as subsidies for education, healthcare, and housing for low-income workers; iii) Social assistance schemes: Establish programs that offer temporary financial support and job training for individuals facing economic hardships, ensuring their smooth transition to stable employment.

Third, the increase in wages for female workers suggests a growing participation of women in higher-paying jobs. To further support women in the workplace, the following measures should be considered: i) Maternity leaves and pregnancy care: Ensure the provision of adequate maternity leaves and necessary healthcare support during pregnancy to promote the well-being of female workers; ii) Equal opportunity policies: Enforce and strengthen policies that promote gender equality in the labor market, including measures to eliminate gender-based discrimination and bias in hiring and promotions.

9 Appendix

Table 7: VHLSS Sample Size

x

Year	Communes	Households	Individuals
2010	2199	9399	36999
2012	2219	9399	36655
2014	1716	9399	36081
2016	3133	46995	160263
2018	3133	46995	170529
2020	3132	46980	173164

Table 8: Classification of Occupations

Classification	Code	Occupation
Public workers	01	Officers
	02	Non-officers
	03	Other members of the armed forces
	11	Agencies of the Communist Party of Vietnam
	12	The National Assembly and Office of the State President
	13	The Government
	14	People's courts and people's prosecutorates
	15	Local people's councils and people's committees
	16	Mass organizations; Vietnam Fatherland Front; Labour Confederation; Women's Union; Farmers' Union; Youth Union; Veterans' Association;
	17	Private organisations; humanitarian organizations; organizations for other particular benefits;
Executives/Leaders	18	Major organisations (groups, general corporations and the like)
	19	Small organisations (companies, businesses, and enterprises, small schools)
Specialists	21	Natural sciences and technology
	22	Healthcare
	23	Education and training
	24	Business and management
	25	IT and communication
	26	Legal, cultural and social affairs
	31	Technicians in science and technology
	32	Technicians in healthcare
	33	Business and management
	34	Legal, cultural and social affairs
	35	Technicians in IT and communication
	36	Average-level teachers
White-collar Workers	41	General officers and desk-based officers
	42	Customer service staff
	43	Data and input enumerators
	44	Other office assistants
Farming Workers	61	Labourers with market-demanded skills in agriculture
	62	Labourers with market-demanded skills in forestry, fisheries and hunting
	63	Labourers in agriculture, fisheries, hunting and collection of farm produce for self-subsidy
Blue-collar Workers	71	Construction-related workers (except electricians)
	72	Metal smiths, mechanics and other workers related
	73	Handcrafters, and printing-related workers
	74	Electricians and electronics workers
	75	Workers in food-processing, woodwork, garment making, and other handicrafts, and other workers related
	81	Operators of fixed machines and equipment
	82	Machine assembling workers
	83	Vehicle drivers and operators of moving equipment
Manual Workers	51	Personal service staff
	52	Sales staff
	53	Personal care staff
	54	Security service staff
	91	Cleaners and domestic helps
	92	Low-skilled labourers in agriculture, forestry and fisheries
	93	Workers in mining, construction, industry, and transport
	94	Assistants in food preparation
	95	Street-based and sales-related labourers
	96	Waste collectors and other low-skilled labourers

Figure 4: Kernel Density Plot of Year Difference

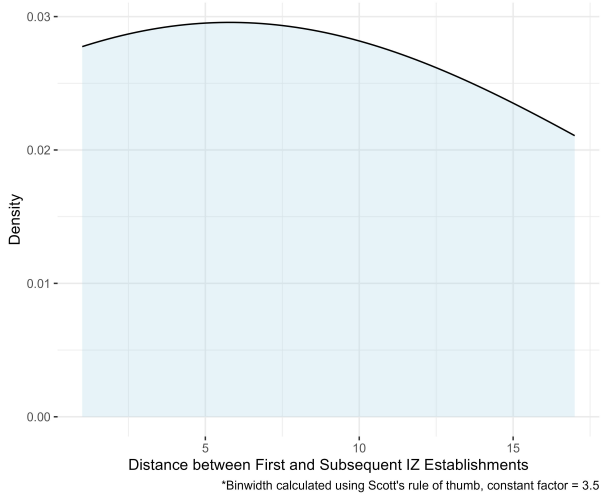


Figure 5: Distributional Balance

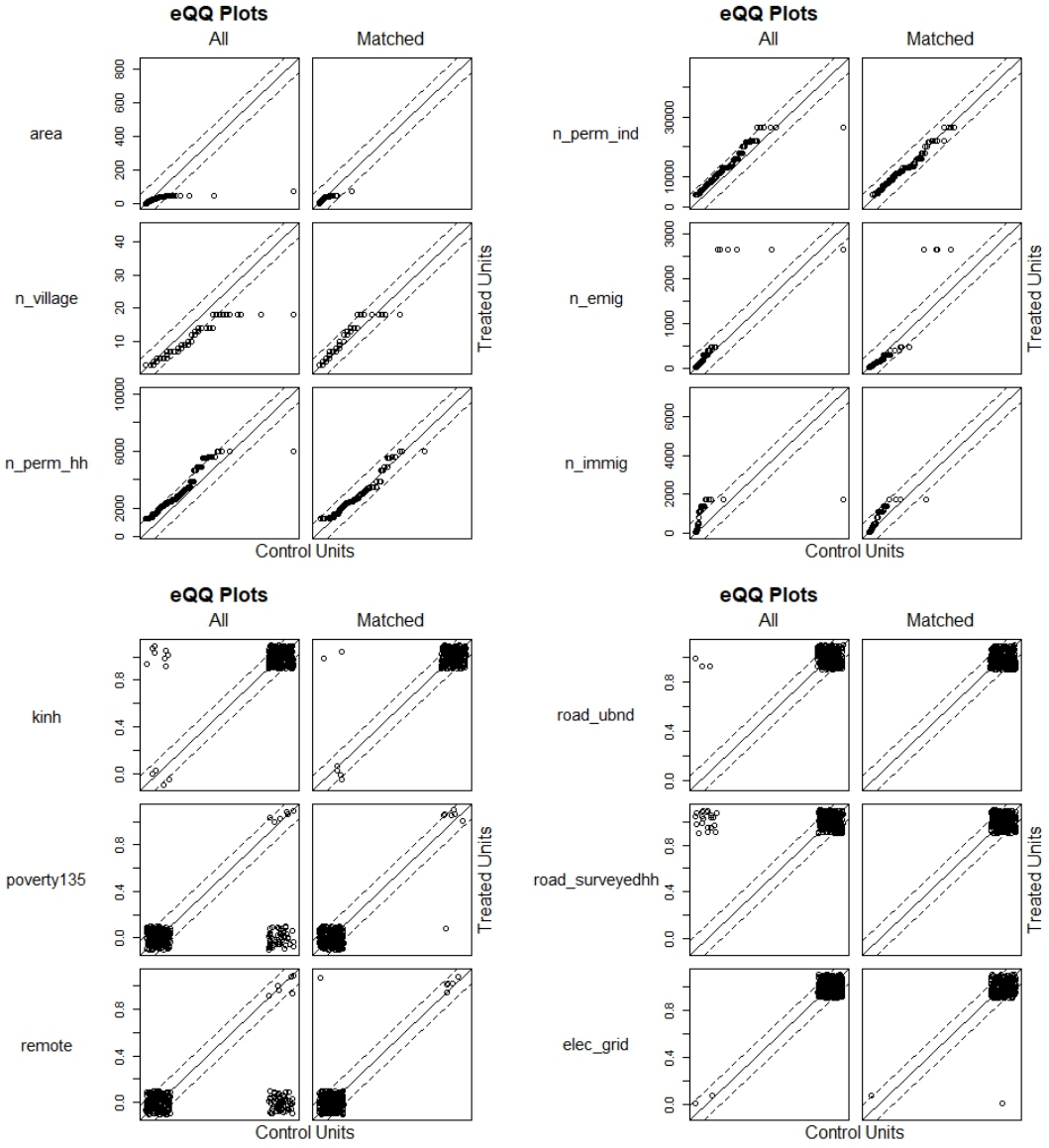


Table 9: Propensity Score Estimation

	Dependent variable:
	treat
Area	-0.013^{***}
	(0.004)
Villages	-0.096^{***}
	(0.018)
Households	0.0003
	(0.0002)
Individuals	-0.00000
	(0.00004)
Emigrants	-0.0002
	(0.0003)
Immigrants	0.001^{***}
	(0.0002)
Kinh	-1.040^{*}
	(0.545)
Poor	-0.695
	(0.444)
Remote	-1.735^{***}
	(0.442)
Road to PC	13.839
	(659.740)
Road to Surveyed HH	15.058
	(310.229)
Electricity	-0.193
	(0.727)
Observations	11,367
Log Likelihood	$-1,083.473$
Akaike Inf. Crit.	2,190.947
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 10: Effects of IZ Establishment, Main Specifications

Dep. Var.:	HH expend. pc.	HH income pc.	House value	Labor income share	Hourly wages	Hourly wages (Female)	Hourly wages (Male)	Hourly wages (Workers)	Ind. wage inequ.	HH ineq.
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Year 1	3,946.4	20,790.5	-693.8	0.0697	0.8502	0.7118	0.8929	7.819	-6,255.5	6.731***
	(3,152.5)	(20,097.5)	(873.6)	(0.0557)	(1.246)	(1.363)	(1.892)	(6.015)	(52,427.4)	(1.664)
Year 2	-68.88	-49,827.0	-178.1	0.0881	-0.0135	1.494	-1.694*	-0.2135	-29,147.6	1.960
	(1,720.9)	(48,178.8)	(981.8)	(0.0857)	(0.9527)	(1.489)	(1.006)	(1.938)	(19,039.1)	(2.903)
Year 3	2,949.5	38,819.6	-515.3	0.1633**	6.006***	7.442**	4.744	2.630	54,289.2	9.022*
	(3,052.6)	(30,295.3)	(1,084.6)	(0.0691)	(2.104)	(3.084)	(2.911)	(3.489)	(36,820.6)	(5.392)
Year 4	-3,922.5***	-63,592.4	-294.3	0.1167	0.7243	2.033	0.2242	-5.729*	-31,996.0	0.5067
	(1,399.0)	(56,423.6)	(798.8)	(0.0835)	(1.076)	(1.825)	(1.241)	(3.262)	(24,459.4)	(3.567)
Year 5	684.7	39,377.2	-1,177.6	0.1645*	8.451***	6.754***	10.65**	5.193	26,398.3	13.95***
	(3,498.7)	(29,288.6)	(1,655.1)	(0.0907)	(2.857)	(2.229)	(5.288)	(4.871)	(39,635.3)	(4.764)
Year 6	-4,070.0**	-74,322.3	1,415.6	0.1550**	1.180	1.493	0.3624	-3.166	-7,012.4	0.5510
	(1,901.7)	(62,770.7)	(1,330.6)	(0.0701)	(1.102)	(1.438)	(1.536)	(3.010)	(40,395.3)	(4.771)
Year 7	1,894.9	36,229.0	1,227.3	0.1099	4.873**	6.267***	3.325	6.530	17,914.3	13.20***
	(3,993.1)	(37,365.9)	(1,853.6)	(0.1028)	(2.135)	(2.364)	(2.986)	(4.941)	(44,832.6)	(4.216)
Year 8	-3,924.7**	-77,123.8	975.6	0.2025**	2.606**	4.147**	-0.1778	-4.499	13,998.1	2.950
	(1,794.3)	(65,859.3)	(1,584.0)	(0.0789)	(1.280)	(1.617)	(1.851)	(3.864)	(32,424.9)	(5.070)
Year 9	1,104.7	42,258.4	1,989.3	0.1318	6.443***	5.958**	6.127**	6.614	7,913.2	15.21***
	(4,182.6)	(50,182.7)	(2,429.9)	(0.0902)	(2.210)	(2.732)	(2.830)	(5.140)	(43,435.9)	(4.440)
Year 10	-3,715.7	-82,550.7	1,528.9	0.2397***	2.950	5.221**	0.9027	-6.840	-6,036.4	-1.338
	(2,716.7)	(61,174.3)	(1,638.0)	(0.0748)	(1.906)	(2.535)	(2.153)	(4.358)	(36,428.6)	(4.999)
Year 11	2,048.4	61,096.5	3,838.4	0.1695	5.042*	6.364**	3.926	0.7776	19,793.5	12.83***
	(3,935.9)	(54,617.8)	(2,766.5)	(0.1091)	(2.679)	(2.938)	(3.619)	(5.877)	(48,408.8)	(4.434)
Year 12	-7,437.3***	-82,148.4	989.4	0.1138	3.409*	3.440	2.944	-7.026	-25,333.1	-0.9795
	(2,391.8)	(66,810.7)	(3,140.5)	(0.1109)	(1.831)	(2.236)	(2.410)	(6.090)	(40,849.3)	(5.066)
Year 13	2,459.5	33,175.3	5,482.4	0.2203**	13.73***	21.63***	5.699	1.682	-7,946.2	20.97**
	(4,297.2)	(53,932.3)	(3,540.6)	(0.1117)	(4.493)	(6.234)	(4.203)	(7.470)	(51,148.3)	(9.228)
Year 14	-7,326.4	-101,285.8	4,618.0	0.2069*	4.856*	8.486**	-0.8071	-6.682	-63,424.4	-0.9697
	(4,552.5)	(68,334.8)	(2,952.2)	(0.1084)	(2.882)	(3.416)	(2.443)	(6.263)	(42,881.1)	(5.822)
Year 15	6,170.4	19,702.3	6,356.0**	0.0855	2.241	0.7795	3.566	-0.9688	-26,604.8	12.96*
	(4,763.2)	(59,748.3)	(2,669.7)	(0.1186)	(4.149)	(7.371)	(3.945)	(7.842)	(58,402.5)	(7.272)
Year 16	3,708.1	-87,789.3	3,485.9	0.0647	4.444	7.341	-0.6018	-6.690	-72,384.9	-3.750
	(8,841.4)	(71,829.8)	(4,437.9)	(0.1293)	(3.873)	(4.624)	(3.281)	(8.178)	(49,318.8)	(6.812)
Year 17	23,002.4***	33,046.4	5,963.9**	0.2838*	4.326	2.056	6.118	-0.8655	-25,829.2	-2.563
	(7,352.9)	(68,217.1)	(2,650.6)	(0.1530)	(4.592)	(9.281)	(4.143)	(8.027)	(63,494.0)	(6.864)
Year 18	-6,688.4	-76,388.3	9,862.9**	0.1032	6.114*	8.414***	2.695	-1.586	-98,258.2*	-0.0343
	(5,020.6)	(77,813.7)	(4,353.9)	(0.1576)	(3.401)	(3.214)	(5.673)	(6.924)	(58,295.2)	(7.448)
Year 19	12,104.4*	53,659.3	7,967.2**	0.1076	73.18	143.8	2.747	4.536	-12,937.7	8.695
	(6,443.9)	(66,213.8)	(3,196.3)	(0.2199)	(60.07)	(119.7)	(5.081)	(9.152)	(93,718.4)	(7.750)
Year 20	9,764.2	-89,188.6	11,707.0**	0.0563	6.141**	7.328*	3.114	-4.927	12,815.3	2.779
	(8,074.2)	(74,465.4)	(4,918.6)	(0.1491)	(2.866)	(3.930)	(3.450)	(8.294)	(66,964.3)	(6.858)
Year 21	11,401.3*	33,189.0	8,385.4***	-0.1860	-3.052	-3.088	-2.613	-8.412	-54,252.5	13.84
	(5,988.2)	(68,052.5)	(3,093.9)	(0.1355)	(6.293)	(11.18)	(4.378)	(9.446)	(80,608.2)	(11.24)
Year 22	-4,310.0	-108,906.3	17,350.1***	0.2358	16.77***	21.06***	9.971***	4.339	-169,358.1***	-6.887
	(7,922.9)	(79,335.6)	(5,352.8)	(0.1927)	(3.393)	(4.638)	(3.397)	(11.02)	(57,110.5)	(11.39)
Year 23	7,789.8	14,394.4	18,345.8***	-0.0880	-19.37	-36.98	-1.637	0.1773		13.39*
	(5,955.3)	(72,299.4)	(2,976.2)	(0.2478)	(17.16)	(34.72)	(4.241)	(10.28)		(7.696)
Year 24	10,813.5	-139,275.9	23,367.6***	-0.0410	7.553	13.40***	-0.4543	-10.12	-115,290.1	3.749
	(7,365.4)	(94,913.9)	(8,283.8)	(0.1876)	(4.584)	(4.744)	(5.060)	(10.57)	(69,987.1)	(9.379)
Year 25	24,288.6***	-15,837.2	13,385.8***	0.2347	33.45***	55.92***	14.41***	4.720	-112,208.3	1.487
	(6,005.8)	(94,210.6)	(3,158.6)	(0.1679)	(9.846)	(19.36)	(4.748)	(9.978)	(82,023.2)	(7.663)
Fixed-effects										
commune	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	1,008	1,008	1,008	1,008	1,008	1,002	1,006	794	829	1,008
R^2	0.72903	0.44140	0.76521	0.67149	0.47029	0.42949	0.55737	0.45731	0.52942	0.44878
Within R^2	0.09229	0.00792	0.09903	0.05423	0.23751	0.25644	0.05547	0.01604	0.05659	0.03014

Clustered (commune) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Effects of IZ Establishment, Adding Control Variable Set 1

Dep. Var.:	HH expend. pc.	HH income pc.	House value	Labor income share	Hourly wages	Hourly wages (Female)	Hourly wages (Male)	Hourly wages (Workers)	Ind. wage inequ.	HH ineq.
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Year 1	3,561.4	23,631.8	-495.1	0.0908*	1.164	1.138	1.072	8.095	-5,348.5	6.847***
	(3,218.0)	(20,072.0)	(906.3)	(0.0511)	(1.252)	(1.392)	(1.887)	(6.076)	(52,546.0)	(1.557)
Year 2	-331.1	-48,451.6	-63.11	0.1020	0.1640	1.709	-1.572	0.0825	-29,796.2*	2.025
	(1,965.8)	(49,134.0)	(963.8)	(0.0696)	(0.8177)	(1.316)	(0.9584)	(1.778)	(15,377.4)	(2.637)
Year 3	2,814.1	40,318.9	-374.9	0.1787**	6.313***	7.841**	4.936*	2.589	54,107.2	9.053
	(3,080.7)	(32,277.0)	(1,080.9)	(0.0704)	(2.110)	(3.227)	(2.867)	(3.617)	(37,783.7)	(5.533)
Year 4	-3,647.8**	-64,243.4	-350.7	0.1078	0.7576	2.086	0.2244	-5.582*	-34,092.8	0.2803
	(1,420.3)	(56,098.8)	(804.6)	(0.0821)	(1.040)	(1.797)	(1.209)	(3.205)	(21,291.6)	(3.377)
Year 5	568.5	40,395.8	-1,044.8	0.1807**	8.744***	7.117***	10.85**	5.442	26,961.7	14.02***
	(3,321.7)	(30,247.8)	(1,598.5)	(0.0912)	(2.835)	(2.320)	(5.246)	(4.804)	(39,129.5)	(4.636)
Year 6	-3,119.1	-76,244.2	1,114.1	0.1073	0.9661	1.287	0.0873	-3.379	-16,326.3	-0.2382
	(2,071.1)	(61,204.2)	(1,481.5)	(0.0785)	(1.159)	(1.686)	(1.471)	(2.876)	(31,154.3)	(4.718)
Year 7	2,335.0	34,249.3	1,134.6	0.0995	4.965**	6.370***	3.403	6.662	16,497.0	12.80***
	(3,836.2)	(38,187.5)	(1,762.1)	(0.0907)	(2.079)	(2.218)	(2.987)	(4.920)	(44,119.8)	(4.052)
Year 8	-3,231.3*	-82,180.5	687.5	0.1742**	2.263*	3.662**	-0.3328	-4.666	12,027.0	2.702
	(1,920.4)	(64,876.1)	(1,709.3)	(0.0733)	(1.221)	(1.574)	(1.829)	(3.883)	(26,305.0)	(4.967)
Year 9	1,228.8	40,363.6	1,977.5	0.1370	6.384***	5.831**	6.160**	6.835	16,286.8	15.60***
	(3,892.4)	(50,975.0)	(2,224.3)	(0.0839)	(2.196)	(2.678)	(2.816)	(5.307)	(42,936.2)	(4.496)
Year 10	-3,138.8	-86,675.2	1,305.4	0.2182***	2.710	4.879**	0.8053	-7.086	-8,237.8	-1.545
	(2,656.7)	(60,270.4)	(1,619.5)	(0.0766)	(1.794)	(2.391)	(2.079)	(4.372)	(30,786.3)	(4.803)
Year 11	2,678.8	58,341.2	3,711.7	0.1549	5.262**	6.626**	4.099	1.009	16,349.4	12.06***
	(3,868.9)	(55,464.3)	(2,734.1)	(0.1066)	(2.662)	(2.990)	(3.592)	(5.877)	(47,922.2)	(4.461)
Year 12	-7,022.0***	-85,834.9	816.8	0.0997	3.342*	3.319	2.986	-7.169	-25,579.9	-1.451
	(2,183.0)	(66,761.4)	(3,019.5)	(0.1093)	(1.805)	(2.213)	(2.395)	(6.034)	(32,973.1)	(4.840)
Year 13	2,176.4	36,839.8	5,713.6	0.2411**	14.20***	22.30***	5.932	2.294	-4,528.7	20.95**
	(4,168.1)	(55,008.4)	(3,512.1)	(0.1065)	(4.427)	(6.294)	(4.167)	(7.590)	(48,132.2)	(8.889)
Year 14	-7,023.8	-104,833.8	4,213.9	0.1600	4.123	7.616**	-1.284	-7.860	-71,835.6*	-1.652
	(4,779.6)	(66,507.0)	(3,043.7)	(0.1070)	(2.556)	(3.052)	(2.198)	(6.169)	(37,168.6)	(5.689)
Year 15	5,430.5	23,474.9	6,545.4**	0.1056	2.518	1.286	3.725	-1.183	-36,102.5	12.65*
	(4,719.1)	(60,352.6)	(2,690.9)	(0.1074)	(3.804)	(6.928)	(3.778)	(7.588)	(56,357.9)	(7.493)
Year 16	3,565.8	-88,876.1	3,202.6	0.0302	3.849	6.693	-1.012	-7.796	-87,351.9*	-4.505
	(9,342.5)	(69,863.7)	(4,501.2)	(0.1189)	(3.625)	(4.293)	(3.157)	(7.775)	(47,345.8)	(6.631)
Year 17	22,697.7***	36,538.7	6,165.9**	0.3014**	4.695	2.627	6.287	-0.4265	-24,237.7	-2.561
	(6,859.5)	(69,476.6)	(2,575.2)	(0.1386)	(4.579)	(9.292)	(4.090)	(7.738)	(62,044.9)	(6.647)
Year 18	-6,106.1	-82,427.0	9,491.8**	0.0682	6.083*	8.367***	2.820	-2.470	-126,733.1**	-1.877
	(5,253.9)	(79,531.7)	(4,467.4)	(0.1536)	(3.283)	(3.075)	(5.604)	(6.844)	(54,273.0)	(7.276)
Year 19	12,676.0**	51,496.0	7,777.6**	0.0819	73.27	143.9	2.789	4.236	-24,420.2	7.642
	(6,318.0)	(66,630.2)	(3,174.2)	(0.2148)	(60.16)	(119.9)	(4.893)	(8.848)	(86,940.6)	(7.804)
Year 20	10,278.1	-96,636.2	11,284.3**	0.0210	5.976**	7.080*	3.221	-5.556	-24,370.6	0.9538
	(8,072.9)	(75,578.5)	(4,956.1)	(0.1594)	(2.726)	(3.815)	(3.304)	(8.219)	(67,251.2)	(6.961)
Year 21	9,699.7*	39,546.8	9,013.3***	-0.1010	-2.298	-2.080	-1.995	-7.501	-38,693.4	14.65
	(5,752.7)	(72,386.6)	(3,091.1)	(0.1221)	(6.103)	(10.78)	(4.472)	(9.330)	(81,448.6)	(10.86)
Year 22	-3,642.0	-118,823.0	16,781.1***	0.1888	15.89***	19.85***	9.633***	3.330	-145,878.4***	-7.528
	(7,782.7)	(78,753.5)	(5,577.5)	(0.1838)	(3.136)	(4.285)	(3.255)	(11.16)	(46,922.0)	(11.08)
Year 23	8,532.1	9,605.4	18,023.8***	-0.1233	-19.56	-37.18	-1.718	-0.4036		12.46
	(5,719.7)	(71,933.3)	(2,985.4)	(0.2358)	(17.23)	(34.88)	(4.175)	(10.15)		(7.675)
Year 24	4,924.8	-119,360.0	22,138.8*	-0.2328	2.797	8.646	-4.237	-22.51**	-172,298.3**	-1.273
	(10,692.3)	(82,384.7)	(11,392.0)	(0.2571)	(5.982)	(6.534)	(6.405)	(11.18)	(83,727.6)	(10.65)
Year 25	17,917.9**	13,849.0	12,663.2**	0.0790	29.57***	52.46***	10.92*	-6.804	-154,486.7*	-3.203
	(7,929.5)	(71,276.3)	(5,325.5)	(0.2339)	(10.45)	(19.82)	(5.775)	(10.41)	(83,623.5)	(9.877)
Surveyed HH	1,249.8	-4,227.6	88.79	0.0156	0.6790	0.6697	0.5273	1.903**	5,393.2	0.5408
	(909.9)	(6,819.9)	(682.8)	(0.0272)	(0.7231)	(0.8674)	(0.7319)	(0.8976)	(7,138.5)	(1.106)
Surveyed individuals	-22.01	-401.0	2.337	0.0024***	0.0033	-0.0105	0.0281	0.0217	81.87	0.0297
	(58.07)	(401.4)	(21.27)	(0.0007)	(0.0478)	(0.0955)	(0.0208)	(0.0425)	(404.9)	(0.0474)
Avg. HH size	-524.4	7,677.7	190.6	0.0006	-0.3059	-0.2188	-0.5083	-0.1561	19,107.5***	1.154
	(396.8)	(6,978.9)	(337.3)	(0.0133)	(0.5076)	(0.8791)	(0.4070)	(1.040)	(6,687.0)	(0.8504)
Avg. living area	87.79***	-695.2	-41.54*	-0.0041***	-0.0688**	-0.0962**	-0.0359*	-0.0649	427.8**	0.0030
	(25.78)	(538.8)	(22.96)	(0.0007)	(0.0271)	(0.0450)	(0.0200)	(0.0493)	(207.1)	(0.0488)
Fixed-effects										
commune	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	1,008	1,008	1,008	1,008	1,007	1,002	1,006	822	825	824
R^2	0.73234	0.29248	0.68882	0.67744	0.41478	0.37773	0.46389	0.55597	0.61419	0.53768
Within R^2	0.10861	0.00611	0.05900	0.12113	0.18407	0.20030	0.03387	0.04903	0.12726	0.11284

Clustered (commune) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Figure 6: Effects of IZ Establishment, Adding Control Variable Set 1

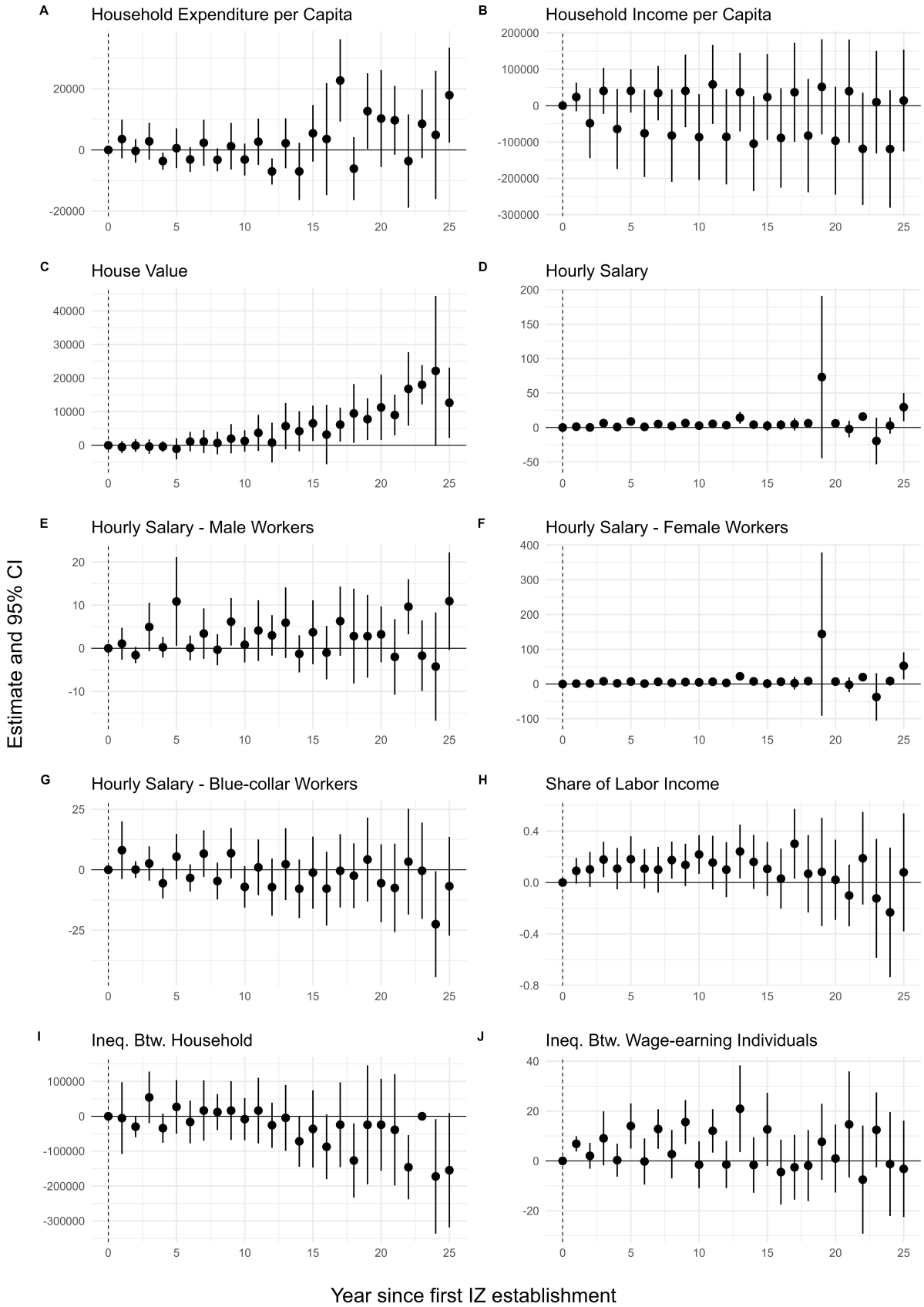
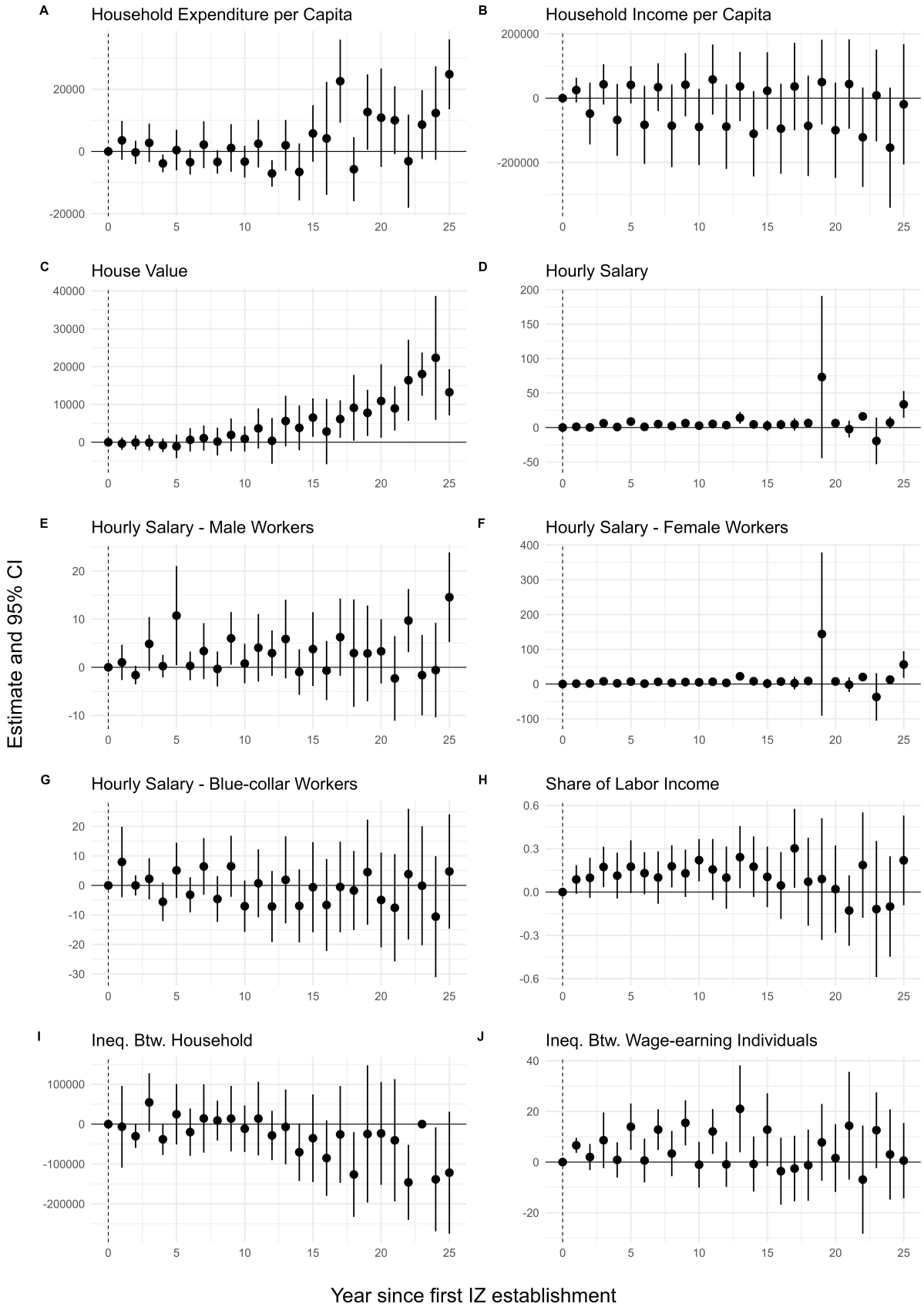


Table 12: Effects of IZ Establishment, Adding Control Variable Set 2

Dep. Var.:	HH expend. pc.	HH income pc.	House value	Labor income share	Hourly wages	Hourly wages (Female)	Hourly wages (Male)	Hourly wages (Workers)	Ind. wage inequ.	HH ineq.
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Year 1	3,554.5	25,125.7	-362.2	0.0871*	1.110	1.095	1.022	7.928	-6,532.4	6.617***
	(3,193.5)	(19,596.5)	(845.7)	(0.0506)	(1.221)	(1.385)	(1.869)	(6.095)	(52,349.0)	(1.505)
Year 2	-311.9	-47,944.5	-78.38	0.0986	0.1564	1.712	-1.617	-0.0052	-30,314.0**	1.995
	(1,914.6)	(48,954.4)	(977.7)	(0.0707)	(0.8147)	(1.296)	(0.9839)	(1.743)	(15,083.7)	(2.639)
Year 3	2,743.7	42,872.9	-115.1	0.1737**	6.194***	7.727**	4.858*	2.223	54,510.3	8.624
	(3,154.2)	(31,930.5)	(1,055.8)	(0.0715)	(2.097)	(3.249)	(2.852)	(3.569)	(37,367.1)	(5.618)
Year 4	-3,857.3***	-67,668.1	-797.4	0.1138	0.7862	2.095	0.2368	-5.551*	-38,061.8*	0.8699
	(1,447.2)	(56,928.7)	(913.5)	(0.0809)	(1.030)	(1.755)	(1.195)	(3.320)	(20,069.9)	(3.547)
Year 5	430.9	41,230.4	-1,091.2	0.1758*	8.658***	7.043***	10.74**	5.091	24,798.0	13.96***
	(3,324.0)	(29,562.6)	(1,594.2)	(0.0931)	(2.832)	(2.324)	(5.253)	(4.774)	(38,655.4)	(4.675)
Year 6	-3,467.1*	-82,873.6	633.5	0.1306*	1.038	1.246	0.3036	-3.179	-20,041.7	0.6339
	(2,022.0)	(61,869.0)	(1,569.1)	(0.0743)	(1.117)	(1.475)	(1.519)	(3.041)	(30,234.7)	(4.412)
Year 7	2,179.4	34,074.3	1,093.0	0.1003	4.904**	6.295***	3.372	6.426	14,272.3	12.82***
	(3,842.1)	(37,808.2)	(1,692.2)	(0.0924)	(2.067)	(2.217)	(2.966)	(4.897)	(43,714.1)	(4.103)
Year 8	-3,374.4*	-85,697.3	177.4	0.1781**	2.325*	3.722**	-0.3369	-4.616	8,974.5	3.358
	(1,920.8)	(65,533.1)	(1,870.0)	(0.0742)	(1.236)	(1.552)	(1.850)	(3.964)	(25,502.4)	(4.552)
Year 9	1,112.8	41,668.3	1,932.4	0.1295	6.292***	5.769**	6.020**	6.489	13,821.5	15.49***
	(3,905.8)	(50,259.6)	(2,211.9)	(0.0836)	(2.185)	(2.657)	(2.796)	(5.278)	(41,970.8)	(4.549)
Year 10	-3,280.6	-89,341.7	894.6	0.2206***	2.743	4.912**	0.7841	-7.048	-11,585.5	-1.037
	(2,614.8)	(60,416.8)	(1,704.7)	(0.0747)	(1.784)	(2.370)	(2.094)	(4.448)	(29,821.5)	(4.592)
Year 11	2,477.3	58,058.0	3,653.5	0.1562	5.185*	6.531**	4.061	0.7185	13,813.7	12.09***
	(3,902.2)	(55,560.8)	(2,701.9)	(0.1075)	(2.649)	(2.985)	(3.585)	(5.865)	(47,125.8)	(4.518)
Year 12	-7,069.0***	-88,375.9	371.5	0.0996	3.413*	3.411	2.952	-7.158	-28,355.6	-0.9103
	(2,166.9)	(67,000.8)	(3,081.6)	(0.1098)	(1.811)	(2.218)	(2.405)	(6.145)	(31,535.4)	(4.552)
Year 13	1,977.6	36,396.2	5,607.6	0.2417**	14.13***	22.22***	5.881	1.880	-6,909.5	21.03**
	(4,149.1)	(54,855.5)	(3,407.8)	(0.1098)	(4.433)	(6.280)	(4.165)	(7.519)	(47,974.3)	(8.759)
Year 14	-6,594.4	-110,743.3	3,807.0	0.1755	4.509	7.968**	-0.9849	-6.923	-70,416.9*	-0.7536
	(4,672.5)	(67,613.8)	(3,013.6)	(0.1069)	(2.790)	(3.280)	(2.409)	(6.312)	(36,808.3)	(5.555)
Year 15	5,765.9	22,894.4	6,513.1**	0.1050	2.684	1.543	3.792	-0.6064	-35,301.2	12.78*
	(4,633.5)	(61,117.7)	(2,592.3)	(0.1067)	(3.919)	(6.922)	(3.905)	(7.787)	(56,025.9)	(7.350)
Year 16	4,173.2	-94,847.4	2,824.6	0.0455	4.315	7.132*	-0.6709	-6.643	-85,184.1*	-3.589
	(9,257.2)	(71,293.6)	(4,406.6)	(0.1181)	(3.592)	(4.281)	(3.139)	(7.956)	(48,347.3)	(6.741)
Year 17	22,577.1***	36,279.2	6,127.5**	0.3026**	4.651	2.597	6.264	-0.5360	-25,735.0	-2.533
	(6,810.9)	(69,299.5)	(2,519.7)	(0.1395)	(4.560)	(9.250)	(4.086)	(7.826)	(62,092.4)	(6.597)
Year 18	-5,721.8	-85,918.0	9,103.4**	0.0715	6.371*	8.679***	2.941	-1.728	-126,422.8**	-1.214
	(5,252.6)	(79,596.3)	(4,435.9)	(0.1553)	(3.367)	(3.107)	(5.699)	(6.832)	(54,164.1)	(7.176)
Year 19	12,657.8**	49,962.0	7,763.3**	0.0899	73.31	144.0	2.885	4.497	-24,649.3	7.785
	(6,183.9)	(67,146.4)	(3,118.2)	(0.2151)	(60.10)	(119.9)	(5.072)	(9.075)	(87,877.5)	(7.741)
Year 20	10,834.1	-99,598.1	10,895.2**	0.0200	6.322**	7.484*	3.322	-4.943	-23,329.1	1.602
	(8,070.5)	(75,467.4)	(4,960.8)	(0.1543)	(2.813)	(3.934)	(3.407)	(8.196)	(65,876.6)	(6.832)
Year 21	10,011.0*	43,722.3	8,947.4***	-0.1282	-2.295	-1.879	-2.307	-7.569	-40,609.4	14.38
	(5,573.7)	(70,870.7)	(2,982.5)	(0.1244)	(6.173)	(10.73)	(4.485)	(9.277)	(78,258.6)	(10.85)
Year 22	-3,137.2	-121,521.2	16,382.3***	0.1865	16.21***	20.23***	9.704***	3.820	-146,128.3***	-6.906
	(7,634.4)	(78,795.5)	(5,463.0)	(0.1861)	(3.149)	(4.206)	(3.349)	(11.28)	(48,070.0)	(10.90)
Year 23	8,603.8	8,449.9	18,012.9***	-0.1180	-19.49	-37.09	-1.635	-0.1236		12.59
	(5,650.8)	(72,728.7)	(2,913.9)	(0.2406)	(17.24)	(34.92)	(4.258)	(10.28)		(7.635)
Year 24	12,331.1	-153,861.0	22,309.7***	-0.1004	7.132	12.73***	-0.5841	-10.60	-138,503.3**	3.026
	(7,672.1)	(95,228.3)	(8,357.1)	(0.1775)	(4.437)	(4.524)	(5.009)	(10.46)	(66,526.7)	(9.086)
Year 25	24,791.4***	-19,074.5	13,212.1***	0.2192	33.61***	56.21***	14.56***	4.716	-121,871.8	0.5826
	(5,724.3)	(95,324.3)	(3,118.8)	(0.1583)	(9.905)	(19.56)	(4.755)	(9.878)	(78,109.0)	(7.582)
Villages	312.9	9,651.7	1,443.5	-0.0086	-0.2055	-0.2148	0.0085	-0.2189	10,511.1	-1.847**
	(403.7)	(7,777.1)	(1,389.0)	(0.0222)	(0.3083)	(0.6437)	(0.7634)	(0.6440)	(9,080.3)	(0.9039)
Avg. HH size	-715.3	4,204.3	200.0	0.0213**	-0.2774	-0.3099	-0.2716	0.0198	19,637.1***	1.419*
	(486.3)	(4,484.2)	(296.3)	(0.0108)	(0.3628)	(0.5496)	(0.3385)	(0.8732)	(4,584.8)	(0.7825)
Avg. living area	87.09***	-721.9	-41.14*	-0.0039***	-0.0682**	-0.0966**	-0.0339*	-0.0629	439.1**	0.0049
	(25.88)	(541.6)	(22.63)	(0.0007)	(0.0267)	(0.0442)	(0.0200)	(0.0494)	(200.8)	(0.0484)
Immigrants						1.455				
						(786,339.8)				
Fixed-effects										
commune	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	1,008	1,008	1,008	1,008	1,008	1,002	1,006	794	829	1,008
R^2	0.73670	0.44568	0.76926	0.69514	0.47285	0.43112	0.55893	0.45837	0.56172	0.45335
Within R^2	0.11798	0.01552	0.11457	0.12234	0.24119	0.25857	0.05881	0.01795	0.12135	0.03818

Clustered (commune) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Figure 7: Effects of IZ Establishment, Adding Control Variable Set 2



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