**CHARLES UNIVERSITY**

FACULTY OF SOCIAL SCIENCES

Center for Economic Research and Graduate Education

# Essays in Development Economics

**2025** **Trang Thanh Tran**

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# Essays in Development Economics

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**Dissertation Committee:**

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# Contents

# Abstract

This dissertation investigates how individuals in developing countries adjust to policy reforms and social shocks, with a focus on labor market outcomes and subjective well-being. Using case studies from Vietnam and Kyrgyzstan, I examine both regulation changes and sudden disruptions, highlighting the dynamics between institutional design, labor market structure, and individual resilience.

In the first chapter, I study Vietnam's 2008 introduction of differentiated minimum wages, documenting short-term declines in formal employment but long-term increases in total employment driven by informal sector growth. In the second chapter, I analyze the 2012 unification of minimum wages for domestic and foreign firms, showing that the reform boosts foreign firm employment growth and has positive spillover effects for domestic firms in districts with higher intensity of foreign direct investment. The third chapter, co-authored with Azizbek Tokhirov and Riga Qi, explores both the short and long-term effects of displacement from the 2010 inter-ethnic violence in Kyrgyzstan, revealing persistent reductions in well-being that take years to recover, especially for those without social support from friends or family.

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# Introduction

This dissertation explores how individuals in developing countries respond to structural and policy-induced shocks, with a focus on labor market outcomes and well-being. The first two chapters examine the effects of public policies, particularly minimum wage regulations, on employment dynamics and the interplay between formal and informal sectors. These chapters contribute to understanding how regulatory interventions shape labor allocation and whether such policies promote inclusive growth in contexts characterized by high informality and weak law enforcement. The third chapter turns to an acute social shock-forced displacement caused by ethnic conflict-and investigates its long-term effects on individual well-being. Together, these chapters provide empirical evidence on how vulnerable populations adapt to economic and social disruptions, highlighting the importance of institutional design, labor market structure, and social support systems in shaping individual resilience.

The first chapter examines how minimum wage changes influence employment in both formal and informal sectors. I use the 2008 reform in Vietnam, which introduced regionally differentiated minimum wage levels, and analyze individual-level survey data from 2004 to 2014. Using an event study design, I find that total employment increases significantly four years after the policy change, primarily driven by a rise in informal employment. In contrast, the short-run effects show a decline in formal sector employment, along with a reduction in working hours in primary jobs. There is also an increase in the likelihood that formal workers take on informal secondary jobs, suggesting that workers adjust to wage policy changes by diversifying their income sources. While the dataset does not offer full panel coverage at the individual level, the survey design allows me to construct two-year panels for a subset of

the population. These panels show no significant evidence of workers transitioning between the formal and informal sectors in response to minimum wage changes. This suggests that observed employment shifts are more likely explained by movements into or out of the labor force rather than the reallocation between sectors.

The second chapter investigates the effects of Vietnam's 2012 minimum wage unification reform, which mandated equal minimum wage levels for domestic and foreign-invested firms. Prior to the reform, foreign firms had been subject to higher minimum wages, creating a dual wage structure within the formal sector. The policy aimed to standardize labor costs across ownership types and reduce distortions in the labor market. Using firm-level panel data and a difference-in-differences strategy, I find that, following the reform, foreign firms expanded their employment by 28% relative to domestic firms. This employment growth is particularly pronounced in the manufacturing sector, where foreign direct investment is more prevalent and labor demand is more sensitive to cost structures. At the aggregate district level, I find no significant changes in total employment. However, in districts with a higher concentration of foreign firms, domestic employment increases in the post-reform period. This suggests that raising labor standards in domestic firms to match those of foreign firms does not necessarily harm local employers. Instead, the reform may have created spillover effects, through increased labor demand, technology transfer, or competitive pressures, that contributed to local job creation. These findings highlight the potential for labor standard harmonization to foster inclusive employment growth in regions that are more integrated into global production networks.

The third chapter, co-authored with Azizbek Tokhirov and Riga Qi, explores the long-term effects of forced displacement on the well-being of individuals who return to their conflict-affected communities. We focus on the 2010 inter-ethnic violence in southern Kyrgyzstan, the deadliest communal conflict since the country's independence. Using nationally representative individual-level panel data covering the period from 2006 to 2019, we investigate how short-term, conflict-induced displacement affects subjective well-being in the years following return. Our analysis relies on a multidimensional subjective well-being index, capturing

various life domains including health, housing, security, and outlook for the future. The results show that even a week-long displacement episode leads to significant and persistent reductions in reported well-being among those affected. While we find evidence that the well-being of displaced individuals gradually converges to the levels of non-displaced peers, full recovery takes several years, indicating a lasting imprint of displacement on psychological and social dimensions of life. These long-term effects are particularly pronounced for individuals without strong social support networks during the displacement period.

# Chapter 1

# Effects of minimum wage on the formal and informal sectors: Evidence from Vietnam

## 1.1 Introduction

Minimum wage policy is often used as an anti-poverty tool to improve the well-being of low-wage workers in both developed and developing countries. However, much of the existing research has focused on developed countries with relatively small informal sector (Cengiz et al., 2019; Dustmann et al., 2022; Giupponi et al., 2024). This leaves a gap in understanding how minimum wage policies impact economies with large informal labor markets, which is common in many developing countries.

However, the informal sector deserves greater attention in the minimum wage literature for two reasons. First, in many developing and emerging economies, the informal sector employs a large share of low-wage workers who are often not covered by labor regulations. According to the International labor Organization (ILO), informal employment made up about 85% of the workforce in Africa and 70% in Asia and the Pacific in 2016 (Bonnet et al., 2019). Second, there is potential for competition between the formal and informal sectors for low-wage workers, especially in jobs requiring minimal qualifications. As a result, many workers may opt to stay in insecure informal jobs due to their flexibility and potential for

tax avoidance, despite the lack of social benefits and legal protections (Meghir et al., 2015; Weber, 2015). Given these issues, the informal sector can amplify or limit the effectiveness of minimum wage policies. This paper, therefore, sheds light on the effects of the minimum wage policy in a country with high levels of informality.

Specifically, in this paper, I investigate the effects of variations in the minimum wage levels on both formal and informal sectors in Vietnam, where the informal employment rate is approximately 70% (GSO[1]). I exploit the introduction of regional variations in minimum wage levels in 2008 as a quasi-natural experiment. Before 2008, the minimum wage level was uniform across the country. The regionalization of the minimum wage was specifically designed to account for differences in prices and local labor market conditions across regions. By 2009, the government reclassified the entire country into four categories, with the highest category mainly applying to the two major cities of the country, Hanoi and Ho Chi Minh City. The sudden introduction of variation in minimum wage levels across regions in 2008 allows me to investigate its effects on the formal and informal sectors.

The labor market models developed by Harris and Todaro (1970), Gramlich et al. (1976), and Mincer (1976) provide the theoretical foundation for this paper. These models predict that increasing the minimum wage would raise wages for workers in the low-tail wage distribution, thereby decreasing labor demand in the formal sector. Consequently, displaced workers would move to the informal sector. The Gramlich-Mincer model allows informal workers to quit their jobs and look for one in the formal sector while unemployed. This implies that a higher minimum wage in the formal sector may, on the other hand, result in a lower labor supply and, thus, higher wages in the informal sector. To empirically test these predictions, I use the Vietnam Household Living Standard Survey (henceforth VHLSS) from 2004 to 2014, conducted every two years by the General Statistics Office, to examine the impact of regional minimum wage variations on employment. The analysis exploits the staggered introduction of Vietnam's regional minimum wage system, which replaced a uniform national minimum wage with district-specific wage levels starting in 2008 and generated heterogeneous wage shocks across districts. I classify a district as treated from the first year it moved out of

[1]General Statistic Office in Vietnam.

the lowest minimum wage category and implement an event-study difference-in-differences design to compare employment outcomes before and after treatment between treated and never-treated districts. Identification relies on parallel trends between treated and untreated districts prior to the reform, supported by pre-treatment dynamics.

I find three key results. First, I find that increases in the minimum wage do not harm employment but rather increase overall total employment. Workers in a district with a higher minimum wage increase are 3.4 percentage points more likely to be in the labor force to work in either the formal or informal sector four years after the policy is implemented.[2] However, there are divergences in effects when I examine the impact of minimum wage policy on each sector separately. The increase in informal employment mostly drives the rise in total employment.

Second, I explore both effects of minimum wage variation on the extensive margin (i.e., probability of being employed) as well as on the intensive margin (i.e., average working hours). I find a decline in formal employment on the extensive margin immediately following an increase in the minimum wage, i.e., a classification into a higher minimum wage bracket. However, this negative effect tends to dissipate after around two years. Meanwhile, employment in the informal sector gradually increases following minimum wage hikes.

On the intensive margin, I observe a reduction in working hours within the formal sector, while no significant changes occur in the informal sector. Formal workers in districts classified into higher minimum wage brackets experience an 8.2% decline in working hours four years after the policy implementation (approximately one hour less per day). This suggests that firms in the formal sector offer fewer hours,part-time positions or fewer overtime hours,to offset the increased labor costs from higher minimum wages. This reduction in hours has a ripple effect on workers' decisions regarding their labor supply; given potentially more free time due to fewer working hours, I find that workers in the formal sector are more likely to have secondary jobs based in the informal sector. Workers in districts with higher increases are associated with an 8.7 percentage point increase in the probability of having a secondary

[2]In 2010, the minimum wage in districts with higher increases was, on average, 18% higher (∼130k VND) compared to districts with lower minimum wage increases.

job compared to districts with lower minimum wage increases. Finally, consistent with fewer hours worked in the formal sector, I find a negative effect of higher minimum wages on overall monthly income in the formal sector, though not an effect on hourly wages in the formal sector, suggesting that secondary employment offsets reductions in primary job income in the formal sector.

Overall, the data I exploit in this paper is not panel data on the individual level, precluding me from studying to what extent the effects are overall due to compositional changes in the workforce in each sector after changes in the minimum wages. However, usually, 50 % of respondents in any survey round of the VHLSS survey are resampled in the next round two years later. These two 2-year panels allow me to investigate whether minimum wage changes cause movements of workers between sectors. However, my findings show no significant movement of people across sectors. This suggests that much of the employment adjustments in the formal and informal sectors may be due to movement in and out of the overall labor force rather than the cross-sector movement.

My findings on no significant rellocation effects are in line with the results of Derenoncourt and Montialoux (2021). Their paper suggests competition between firms operating in the formal and informal sectors within an oligopsonistic labor market, as proposed by Berger et al. (2019). Furthermore, Meghir et al. (2015) state that, in a country with a high level of informal employment, low-skilled workers can be found in both sectors, are mobile across sectors and can potentially secure employment in either sector. This is due to the coexistence of formal and informal firms within a certain range of productivity distribution (La Porta & Shleifer, 2008).

This paper contributes to three strands of the literature. First, it adds to the growing research on minimum wage policies in developing countries, including those with large informal sectors. The evidence on wage and employment effects in the formal and informal sectors is mixed, and much of the focus has been on Latin America. Some studies, like Maloney and Mendez (2004), show positive income effects in both sectors, while others, such as Ham (2018), report negative income effects but positive employment effects in the informal sector.

Turning to spillover effects in the informal sector, Gindling and Terrell (2007) finds no such effects. My findings reveal a short-term reduction in formal employment, though no long-term impact, and a persistent rise in informal employment. These findings are consistent with Samutpradit (2024), which supports the hypothesis of the existence of a spillover effect. An explanation for this is that informal employment may serve as a stepping stone for some workers before transitioning to the formal sector. Informal jobs generally require fewer initial certifications or skills than formal ones. Additionally, the limited negative effects on income observed in my study offer insights into the role of minimum wage policies in countries with high rates of minimum wage payment non-compliance and weak enforcement (Khamis, 2013). For instance, the non-compliance rate in Vietnam increased from 5% in 2011 to 9% in 2013 (Hansen et al., 2016).

These findings contrast with much of the evidence from developed countries, where the employment effects of minimum wage increases remain debated. A large body of influential work finds small or zero short-run employment effects following minimum wage increases (Baker et al., 1999; Card, 1992; Card & Krueger, 1994; Katz & Krueger, 1992; Wellington, 1991), while other studies document negative employment responses, particularly for low-skilled or young workers (Giuliano, 2013; Neumark, Wascher, et al., 2007; Neumark et al., 2014). More recent contributions emphasize that employment effects in developed countries often emerge gradually through reduced job creation rather than immediate job losses (Meer & West, 2016), or operate primarily through changes in the composition of low-wage jobs with limited long-run effects on aggregate employment (Cengiz et al., 2019).

My findings indicate that, in developing countries with large informal sectors and weak enforcement, adjustment to minimum wage increases occurs more quickly and through different margins, with short-run reductions in formal employment and a persistent increase in informal employment. This interpretation is consistent with Derenoncourt et al. (2021), who emphasize the role of institutional context and labor market segmentation in shaping employment responses to minimum wage policies.

Second, I contribute to the literature on the employment effects of minimum wages, focusing

on both the extensive and intensive margins. Despite the extensive research on employment effects, most studies focus on total employment or the extent of job loss or creation following minimum wage increases on the extensive margin. There is less empirical evidence on the intensive margin, and the results remain mixed. Some studies report no effects neither on full-time or part-time employed women (Connolly & Gregory, 2002) or teenage labor (Couch & Wittenburg, 2001; Zavodny, 2000). Conversely, some research documents a reduction in working hours (Burauel et al., 2020; Jardim et al., 2022; McGuiness & Redmond, 2018; Michl, 2000), without affecting overall employment rates. For example, Michl (2000) argues that the absence of adverse employment effects in Card and Krueger (1995) is due to firms' adjustments by reducing working hours. My findings on the reduction in worked hours support the hypothesis that firms use hours as a tool to offset higher labor costs. This could explain why the negative employment effects on formal employment are short-term and disappear two years after the policy implementation.

Finally, this study contributes to the literature on the minimum wage in Vietnam. Previous studies have briefly mentioned spillover effects in the Vietnamese labor market. C. V. Nguyen (2013) finds adverse employment effects among low-wage workers and the reallocation of workers from the formal sector to self-employment by exploiting variations in the uniform base minimum wage (which was applied for all domestic enterprises before 2008). Del Carpio et al. (2013) also shows adverse employment effects on formal workers. Hansen et al. (2016) finds that wage inequality declined in the formal sector but rose in the informal sector. My study departs from previous studies by exploiting the regional minimum wage differentiation for domestic firms as a source of identifying variation instead of using changes in the country-wide minimum wage before its regionalization, which allows me to control for the effects of potentially spurious country wide trends in my outcome variables on my results.

The rest of the paper proceeds as follows. Section 1.2 describes the implementation of the minimum wage policy in Vietnam. Section 1.3 presents the data and some descriptive evidence. Section 1.4 presents the empirical model and identification strategies. Section 1.5 documents the employment effects and the reallocation effects within sectors. In section

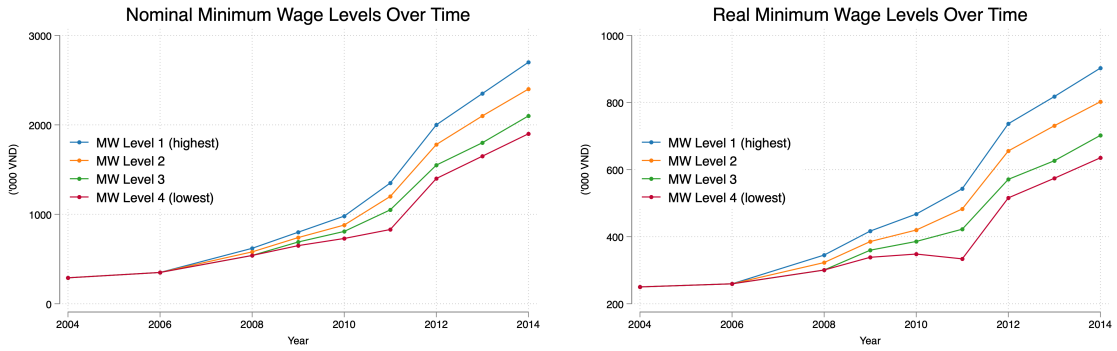
1.6, I discuss income effects on formal and informal sectors. Section 1.7 presents the further evidence on employment effects. Section 1.9 shows the sensitivity checks, and Section 1.10 concludes the paper.

## 1.2 Minimum wage policy in Vietnam

**Policy** The first minimum wage was officially defined in 1995[3]. The stated purpose of the minimum wage was to correspond with the cost of living and ensure that simple labor was compensated adequately under normal working conditions. It also served as a standard for calculating salaries for other types of labor, as stated in the labor Code of 1994. Since then, a single base minimum wage was applied for all domestic registered enterprises. However, despite annual adjustments for inflation, the base wage did not fully account for regional differences in economic development, and according differences in living costs. To address this, the central government decided in 2008 to regionalize the minimum wage policy to account for variations in local prices and labor market conditions. The central government leads the process of setting regional minimum wages in cooordination with the Ministry of Labor - Invalids and Social Affairs, the Vietnam General Confederation of Labor (i.e., employee representatives), and the Vietnam Chamber of Commerce and Industry (i.e., employer representatives) (Decision 1055/QD-TTg).

Prior to 2011, Vietnam maintained distinct minimum wage levels for the foreign and formal private sectors, even within the framework of regionalization policies. However, in 2011, these wage levels were standardized across both sectors. Because my analysis focuses specifically on minimum wage effects within the private sector, I have excluded the foreign sector from consideration. Given that the proportion of workers in the foreign sector is relatively small,approximately 1-2% (Table 1.2) of the workforce,excluding them is unlikely to affect the study's results.

[3]Decree No.25-CP and Decree No.26-CP.




**(a)** Nominal Minimum wage **(b)** Real Minimum wage

**Figure 1.1:** Evolution of nominal and real minimum wage across levels over the years

Source: GSO, The World Bank. Notes: The real minimum wage is adjusted according to CPI, base year 2000

Figure 1.1 displays the evolution of nominal and real minimum wage levels [4]. Initially 2008, following the minimum wage regionalization policy, all districts were assigned to one of three minimum wage levels, with 10% of districts falling into the top two levels and the rest into regional MW level 3 (the lowest level). The highest wage level applied to districts in Ho Chi Minh City and Hanoi, the two major metropolitan areas. Other two levels applied to the rest of the country. In 2009, the government reclassified the entire country into four categories. As a result, approximately 60% (Table 1.1) of districts remained at the lowest level of MW throughout the entire analysis period (i.e., never upgraded).

Each year, districts face one of three scenarios: either they move to a higher regional minimum wage level (e.g., from region 3 in 2008 to region 2 in 2009), or they remain at their current level, or they downgrade to a lower level. Table 1.1 shows that approximately 38% of districts in the sample experienced an "upgrade" in their minimum wage level. In rare cases, some districts were downgraded to a lower wage level, which only occurred in 1.5% of districts. It should be noted that all four regional minimum wage levels are adjusted annually to account for inflation. Figure 1.2 shows maps of all districts in Vietnam and how they were assigned and reassigned to the four MW levels from 2008 to 2014.

[4]The absolute amounts of minimum wage can be found in the Appendix Table A1.

**Table 1.1:** Districts by Year of First Minimum Wage Level Change (Upward) 2008-2014

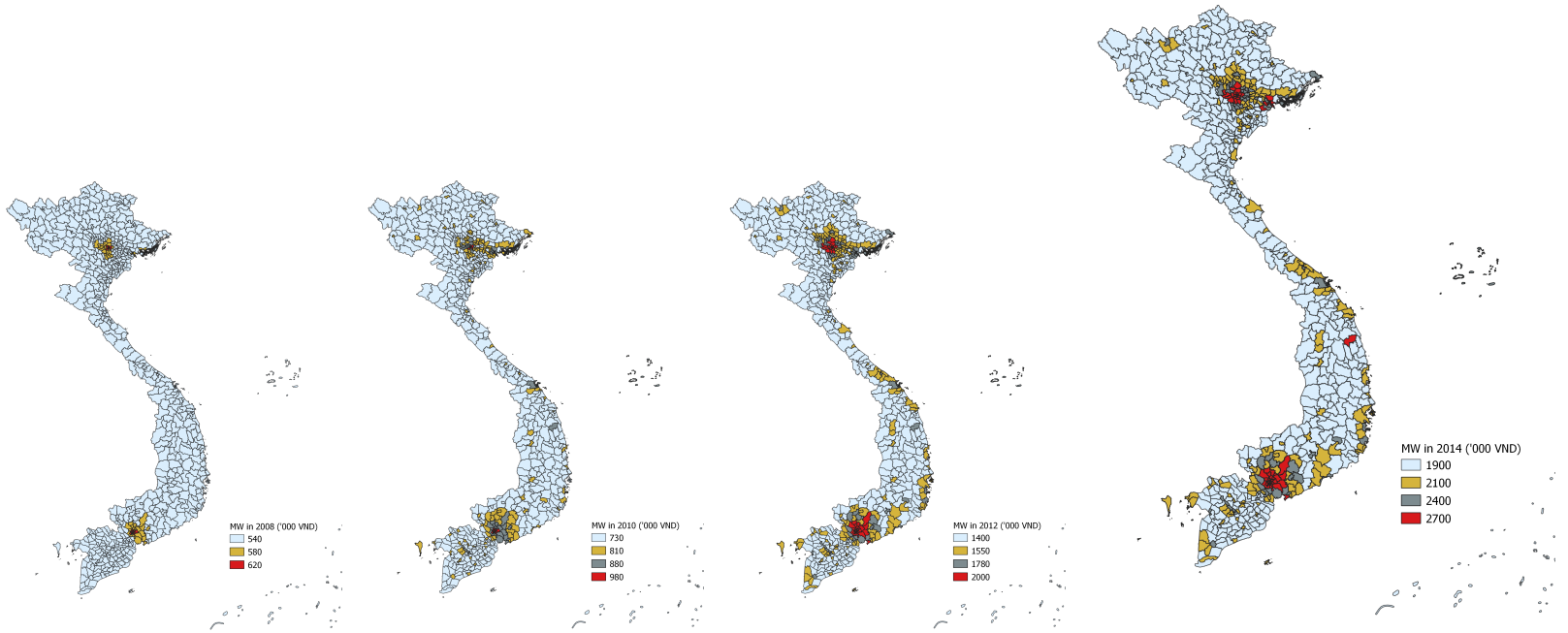
| Year | Districts | % Share |
|---|---|---|
| 2008 | 21 | 3.2 |
| 2010 | 135 | 20.4 |
| 2012 | 92 | 14 |
| 2014 | 1 | 0 |
| Never Upgraded (Lowest Minimum Wage Level) | 413 | 62.4 |
| Total districts | 662 | 100 |

Source: Author's calculation from VHLSS 2004-2014.
Notes: All districts of Ho Chi Minh city and Ha Noi are excluded from the sample as explained in section 3.1.

The nominal minimum wage increased from 2008 to 2010, ranging from 35-58% across different regions[5], depending on the region. However, the real minimum wage increase was less substantial, with an increase of 16-35%[6] depending on the area. Furthermore, the ratio of minimum wage to median wage (the Kaitz index) rose from 30 to 35% from the early 2000s to the late 2000s (Rani et al., 2013). The minimum-to-median wage ratio also experienced a higher rate, from 38 to 40%. These ratios were lower than those in other developing countries such as India (75%), the Philippines (98%), and Indonesia (105%) (Rani et al., 2013). Therefore, the impact of Vietnam's minimum wage policy on the labor market may differ from that of other developing countries.

**The assignment of MW level** The regional minimum wage is determined annually or biannually and assigned to each district by the central government through negotiations involving three parties: the Ministry of Labor, Invalids and Social Affairs, the Vietnam General Confederation of Labor (representing employees), and the Vietnam Chamber of Commerce and Industry (representing employers).

[5]Author's calculation from Table A1; for instance: (730-540)/540 = 35%.
[6]Adjusted by CPI base year 2000.




**(a)** MW in 2008 **(b)** MW in 2010 **(c)** MW in 2012 **(d)** MW in 2014

**Figure 1.2:** Minimum wage levels over years

Source: Authors calculation. Notes: The map represents the changes in nominal minimum wage level across years in VN currency, from 2008 to 2014. The red color illustrates the highest level of minimum wage, while the gray-blue illustrates the lowest level of it.

## 1.3 Data and descriptive evidence

### 1.3.1 Data

**Data description** This analysis uses the Vietnam Household Living Standards Survey (hereafter VHLSS), collected every two years by the General Statistics Office of Vietnam (GSO). In particular, I use VHLSS data for 2004, 2006, 2008, 2010, 2012, and 2014. The number of households in the surveys is stable across these survey rounds, varying between 9180 and 9402. In each VHLSS survey round, 50 % of communes, the rural equivalent of an urban ward, are randomly selected to be re-sampled in the next VHLSS survey round. The exception is the 2008 survey round, in which no communes were selected for re-surveying two years later. The VHLSS is representative at the rural, urban, and district levels.

I limit the sample to individuals of working age (i.e., 15 to 65). The VHLSS collects the income details of all household members above the age of five. To analyze the employment effects of regional minimum wages in the formal and informal sectors, ideally, the data would contain a variable distinguishing formal and informal employment as working with or without social insurance. VHLSS has only collected such information about social security since 2010. To approximate each individual's formal/informal employment status before 2010, I used other information collected in the survey about the type of economic enterprises in which the individual works. Possible answers include working either 1) in a family business as a registered enterprise, 2) in a family business which is an unregistered enterprise, 3) for another household business, 4) in a state sector, 5) in a collective economy, 6) in a private registered enterprise; 7) in a foreign enterprise. Following the ILO definition (ILO., 2013), those working in unregistered enterprises, including own-account workers and contributing family workers, are informal workers. So, any participant who chooses either 2 or 3 is in informal employment. While those responding 1), 6), and 7) are formally employed. Furthermore, in this paper, I consider agriculture sector as a separate sector from informal sector. As a result, the agriculture sector is considered separate from the informal sector in this analysis. In this way, I am able to primarily distangle the effects of the minimum wage

policy on the informal sector and the structural transformation documented in McCaig and Pavcnik (2013) (i.e., people move out of agriculture to the formal sector - higher productivity sector).

Table 1.2 presents descriptive statistics for the worker-level sample from the VHLSS, covering the year 2004 to 2014. Panel A reports demographic variables for individuals in the sample. Their average age is 36, and approximately 65% of the sample are married. Educational attainment is varied: 17% of workers have a college degree or higher, 19% hold a high school diploma, and 32% have completed secondary school[7]. Panel B shows the average monthly wage of workers in both the formal private and informal sectors, conditional on the availability of wage data. Because individuals may have primary and secondary jobs, the total monthly wage reported includes earnings from both sources.

Panel C summarises the sectoral distribution of individuals in the sample into the informal sector, private formal sector, agriculture, state, and foreign sectors. Vietnam's labor market is dominated by the informal and agriculture sectors, which employ over 31% and 53% of wage workers, respectively. In contrast, only 5% of the labor force is employed in the private formal sector, while the remainder works in the state and foreign investment sectors. Because this analysis focuses on the regional minimum wage in the private sector, the state and foreign sectors, which have their own minimum wage policies, are excluded. In the pre-treatment period, before 2008, 44% of workers hold secondary jobs. Table B3 shows that informal workers are more likely to have secondary job, with 42% holding an additional job, compared to 26% of formal workers.

**Descriptive statistics on control and treated districts** Table B2 presents the economic and demographic characteristics of districts in 2006. Columns (1) and (2) document districts that were assigned to minimum wage levels 3 and 2, respectively, in 2008. The highest level (level 1) applied exclusively to districts in Hanoi and Ho Chi Minh City. These two metropolitan areas are excluded from the analysis due to concerns about potential outlier effects. Hanoi and Ho Chi Minh City differ substantially in infrastructure and access to

[7]Vietnam follows a standard 12-year education system, which consists of five years of primary school, four years of secondary school, and three years of high school.

**Table 1.2:** Descriptive statistics

| Workers aged 15 to 65 years old | 2004-2006 | 2008-2014 |
|---|---|---|
| **Panel A: Demographic variables** | | |
| Female | 0.50 | 0.50 |
| Age | 36.48 | 37.99 |
| Married | 0.72 | 0.76 |
| Education attainment: | | |
| *No Training* | 0.01 | 0.11 |
| *Primary school* | 0.28 | 0.27 |
| *Secondary school* | 0.32 | 0.31 |
| *High school* | 0.14 | 0.17 |
| *College or above* | 0.25 | 0.15 |
| **Panel B: Wage variables** | | |
| Monthly wage of formal worker ('000 VND) | 974.70 | 3191.23 |
| Monthly wage of informal worker ('000 VND) | 811.02 | 2406.16 |
| Below current MW | 0.08 | 0.13 |
| *Formal sector* | 0.03 | 0.02 |
| *Informal sector* | 0.07 | 0.13 |
| Below prev. MW | 0.05 | 0.09 |
| **Panel C: Working variables** | | |
| Having secondary job | 0.44 | 0.39 |
| Working sector: | | |
| *Informal sector* | 0.31 | 0.30 |
| *Collective* | 0.01 | 0.00 |
| *Private (formal) sector* | 0.04 | 0.06 |
| *State sector* | 0.09 | 0.09 |
| *Foreign Inv. sector* | 0.01 | 0.02 |
| *Agriculture sector* | 0.54 | 0.51 |
| Observations | 38,069 | 75,013 |

Source: Author's calculation from VHLSS 2004-2014. Notes: The table shows descriptive statistics for working individuals 15-65 years of age reporting at least one employment type in the survey. The statistic of having secondary job only considers formal and informal workers, excluding those employed in other sectors (e.g., state sector, foreign sector). The wage is nominal one.

capital and labor, which could influence the baseline results. As a result, only districts assigned to levels 2 and 3 are considered here. Column (3)-(5) reports the districts at level 3 (the lowest level) in 2008 but assigned to level 4, 3, or 2 in 2010.

Considering first 2 columns in the Table B2, it shows that the districts assigned to level 2

have a population with higher average education and income than districts assigned to level 3. However, in terms of total employment, there are no significant differences with respect to gender or marital status. Interestingly, people in district level 3 are less likely to have secondary jobs, with 29% holding secondary jobs, compared to 43% in level 2 districts. This disparity could be attributed to the more job opportunities available in district level 2, as it is more developed than district level 3. Furthermore, a higher percentage of workers in level 3 districts fell below their respective 2008 minimum wage level (22%) compared to just 11% in level 2 districts, indicating that the 2008 minimum wage increase was a larger shock to district level 3 than to district level 2. Regarding working variables, district level 2 has a higher share of people working in informal and formal sectors and a lower share of people in the agriculture sector compared to district level 3, where 55% of the workforce is employed in agriculture. In district level 2, however, the informal sector plays a more significant role, accounting for 40% of employment. District level 2 enjoys a more diversified economic base, with agriculture, manufacturing, and logistics sharing the employment contribution.
For the last three columns (3)-(5), the data indicate that there is not much difference between district level 2 and district level 3 in terms of demographics, wages, or employment variables. The comparison of level 4 to levels 3 and 2 follows the same pattern observed between level 3 and level 2 districts in 2008, suggesting that districts assigned to level 4 share similar disparities in education, income, job opportunities, and sectoral employment when compared to districts at levels 3 and 2.

#### 1.3.1.1 Descriptive evidence

I next briefly review how changes in the minimum wage affected the overall wage distribution in my sample. One of the reasons is that potentially weak law enforcement in many developing countries may raise concerns about the effectiveness of minimum wage legislation enforcement in Vietnam. In Figure 1.3, I plot the difference between the natural logarithm of nominal income and the logarithm of the minimum wage at the district at the time over

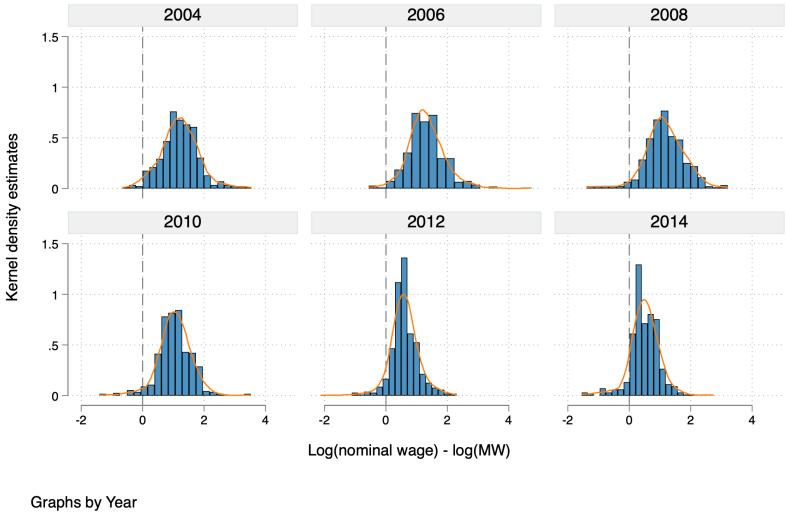



**Figure 1.3:** Densities of the difference between income and minimum wage over years of the districts having minimum wage increases in 2008

Source: author's calculation from VHLSS. Notes: The figures plot the kernel density estimates (with a bandwidth of 0.2) of the difference between the log(nominal income) (pooling from formal and informal income) and the log(regional minimum wage) of the treated districts over the years.

the years [8]. The graphs illustrate compression in the low tail of wage distribution after the central government implemented the regional minimum wage policy. However, enforcement is imperfect, as the fraction of individuals earning below the regional minimum wage level has gradually increased. Additionally, there is a clear jump in the density of individuals earning around the regional minimum wage level, indicating that the policy affects jobs near this threshold. The fraction of wages falling below the minimum wage has slightly increased, potentially due to rising non-compliance rates, 5% in 2011, 8% in 2012, and 9% in 2013 (Hansen et al., 2016).

## 1.4 Empirical model and identification

The shift from a previously uniform country-wide to a regionalized minimum wage system resulted in wage shocks of varying intensities across districts. In 2008, all districts were reas-

[8]The same graph for districts having minimum wage increases in 2010 can be found in Appendix Figure A3.

signed into three minimum wage levels, with 10% of districts,primarily located in Hanoi and Ho Chi Minh City (the two biggest municipalities),falling into the two highest levels. The remaining districts were classified under the lowest level of regional minimum wage level 3. From 2009 onward, the government further regionalized the minimum wage into four levels, with Hanoi and Ho Chi Minh City consistently placed at the highest level (level 1). Districts with level 3 in 2008 would either assigned to levels 4, 3, or even 2 (only a few districts) in 2009. Districts with level 2 in 2008 would either fall into level 3 or level 2 in 2009. As a result, districts that shared the same minimum wage level in 2008 may have found themselves with different levels in 2010.

I define a district as "treated" from the first time onwards that it was not in the lowest category. Thus, a district is classified as "never-treated" if it consistently remained at the lowest minimum wage level throughout the study period. Thus, for the purpose of my analysis, I maintain a district as treated even if it gets "downgraded" to a lower level. However, this happens rarely; only 1% of the districts ever experienced a downgrade. Moreover, a district can have a jump in the regional MW level several times during the analysis period. In that case, however, I consider the first time a district is moved to a higher level as the point of treatment; I do not adjust the treatment indicator further if the district is assigned to an even higher level later. The treatment status of a district remains after the first treatment. Table 1.1 summarizes the number of treated districts from 2008 to 2014.

Note that I define treatment as a binary variable, rather than using a continuous measure based on regional minimum wage levels, for the following reasons. First, in 2008, all districts were affected by the new regional minimum wage policy, leaving no control group in a continuous treatment framework. Instead, I exploit variations in exposure to the minimum wage to classify districts as either strongly treated (the treated group) or weakly treated (the control group). Second, Callaway et al. (2024) demonstrate that continuous treatment in a linear Two-Way Fixed-Effects (TWFE) model is difficult to interpret and prone to selection bias. Therefore, to simplify the analysis and facilitate the testing of the parallel trends assumption for identification, I opt for a binary treatment variable.

To analyze the effects of the minimum wage on my outcome variables, I implement the following event study design.

$$Y_{idt} = \alpha + \sum_{k=-4}^{-2} \beta_k \times treated_{dk} + \sum_{k=0}^{3} \beta_k \times treated_{dk} + X_{idt} + \delta_d + \gamma_t + \epsilon_{idt} \tag{1.1}$$

Where $treated_{dk}$ is a binary indicator that captures whether an observation from district d has been treated k periods before (k > 0), or will be treated in k period (k < 0), or is being treated for the first time in the current period (k=0). The baseline outcome variable $Y_{idt}$ capture employment outcomes of individual $i$ (e.g., formal/informal employment), at district $d$ at time $t$. Because of the biannual availability of household survey, I use relative survey-year to event year, instead relative time-year to event. The coefficients of interest are $\beta_k$ which estimates income/employment effect of regional minimum wage policy k survey period before and after "treatment". Finally, $X'$ are controls (i.e., age, education attainment, marriage status, rural/urban, having secondary job or not) and $\delta$ and $\gamma$ indicate district and time fixed effects.

To examine heterogeneity in treatment effects across genders, I run separate specifications that include an interaction term between the treatment indicator $treated_{dk}$ and a female/-male respondent indicator:

$$\begin{aligned} Y_{idt} = \alpha + \sum_{-4,k\neq-1}^{3} \beta_k \text{ x } treated_{dk} \text{ x } female_i + \\ \sum_{4,k\neq-1}^{3} \beta_k \text{ x } treated_{dk} + \tau X_{idt} + \delta_d + \gamma_t + \epsilon_{idt} \end{aligned} \tag{1.2}$$

**Identification:** My identification strategy assumes that in the absence of the minimum wage policy, the income and employment outcomes of the treated and untreated districts would follow parallel trends over time. Under that assumption, using district and year-fixed effects, I isolate the impact of the minimum wage reform within districts over time.

A potential concern that may invalidate the parallel trend assumption would be omitted time varying confounders that may correlate with the treatment indicator over time. For example,

local market conditions could influence both the setting of minimum wage levels and individual income or employment outcomes. First, I control for rural or urban dummy indicator in the specification. Second, I incorporate "province x year" fixed effects to capture unobserved shocks at the province level, ensuring that the analysis controls for province-specific variations over time.[9] If significant local labor market shocks were significant, the estimates would likely change across various specifications, as these shocks would be partially captured by the "province × year" fixed effects.

**Inference:** Primarily, I use standard errors clustered at the district level, i.e., the level at which the minimum wage variation occurs in my data. However, because both the outcomes and the assignment of minimum wage levels may exhibit spatial correlation across districts, relying solely on clustered standard errors may not fully account for this dependence. I thus use Conley standard errors (Conley, 1999) with a distance cutoff of 40km as in the main specification.[10] Given an average centroid-to-centroid distance between districts in my dataset is approximately 20 km, 40km-bandwidth covers not only neighboring districts, but also their adjacent neighbors, capturing spatial dependencies up to two districts away (Ho et al., 2022). The Conley standard error is reported in squared parentheses in all table results.

To address the potential bias in difference-in-difference models arising from different treatment timings and dynamic treatment effects, (Bertrand et al., 2004; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Roth et al., 2023), I also report robustness checks using the new difference-in-difference estimators suggested by L. Sun and Abraham (2021) and Cengiz et al. (2019), Callaway and Sant'Anna (2021) and Borusyak et al. (2024).

## 1.5 Employment effect

This section presents the main results of estimating equation 2.2. I document the results graphically in Figure 1.4, plotting the $\beta_k$ coefficients and 95% confidence intervals. Due to the small number of observations of outcomes ten years before the policy (k=-5), I only

[9]There are about 662 districts and 62 province in my sample.

[10]I also estimate with cutoffs of 30km and 50km as robustness checks. Results are discussed in section F.4.

report eight years before the policy rather than ten years as documented in the model. A standard event study analysis requires the common trend assumption to identify the effect of regional minimum wage incidence. This assumption implies that the treatment and control groups exhibit similar trends in outcomes (e.g., income and employment levels) prior to the policy change. Fixed effects effectively differentiate the country-level pre-treatment means and year-level shocks when this assumption holds. The change in regional minimum wage policy in period 0 is represented by the dashed line in the graphs. Therefore, the left side of the graphs up to the dashed line represents the pre-treatment trend.

### 1.5.1 Overall employment effect

Traditional economic theory often anticipates that a rise in the minimum wage will adversely impact overall economic employment. The intuition is that the minimum wage increase induces labor costs for the firms, potentially leading to worker displacement or reduced hiring. Hence, in this section, I test for the dis-employment effect, where an increase in the minimum wage may crowd employees out of the labor force. The potential outcome $employed_{idt}$ equals one if individual $i$ in district $d$ is working in either the formal private or informal sector (e.g., with or without social insurance), zero otherwise (e.g., agriculture sector). Contrary to the expected dis-employment effect, I observe an increasing trend in total employment, including full-time and part-time jobs, beginning two years after the policy implementation and reaching statistical significance four years post-implementation, as shown in Figure 1.4. Workers in districts with higher minimum wage increases have a 3.4 percentage point higher total employment rate than those with lower minimum wage increases. Given a baseline participation in the non-agricultural workforce of 32% in 2006, this result indicates an 11% increase in the probability of total employment among workers in treated as opposed to control districts, as defined in the previous section.

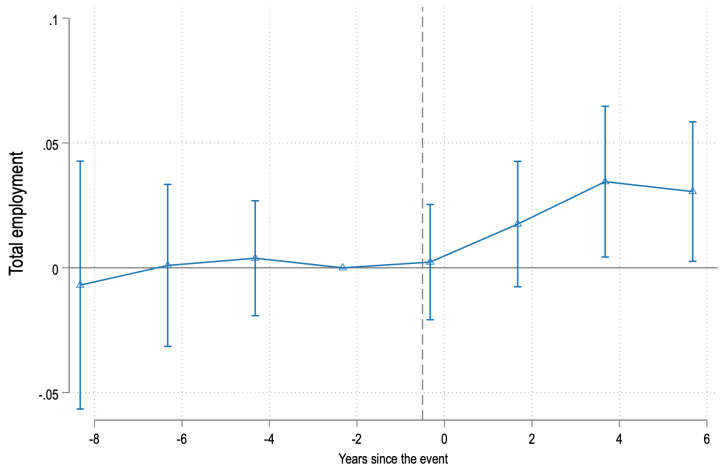



**Figure 1.4:** Total employment, either formal or informal sector

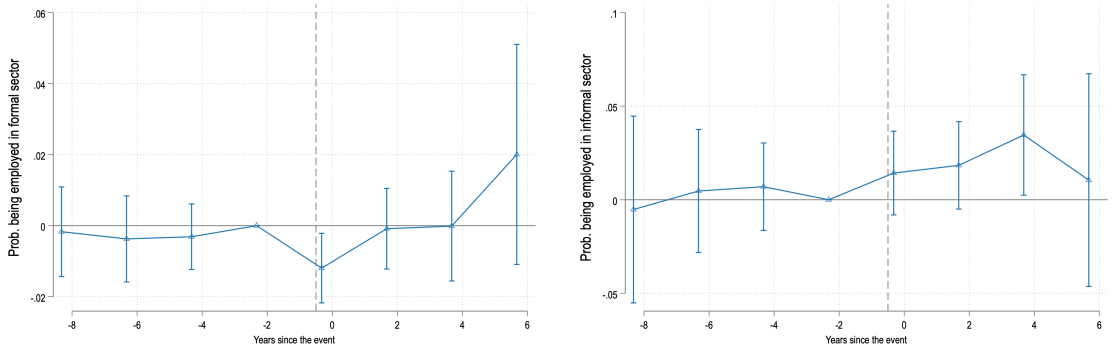
Source: VHLSS 2004-2014. Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, secondary job status, and urban/rural area. Full regression results are shown in Appendix Table C5.

What drives the increase in total employment?. One possible explanation is the migration of individuals from unemployment or other sectors, such as agriculture sector, into the formal or informal sectors. While more individuals are entering the labor force, the effects of regional minimum wage policy on work intensity (e.g., hours worked) remain unclear. It is also uncertain whether these employment effects vary across sectors or whether both sectors exhibit similar employment growth patterns. These issues are explored in the following section, I will decompose employment effects across sectors.

### 1.5.2 Extensive margin by sector

I decompose the overall effect into formal/informal sectors to investigate the drivers of the rise in total employment. I examine both the probability of being employed (i.e., the extensive margin) and working hours (i.e., the intensive margin) on each sector separately. The former measure is defined as an indicator, taking the value of one if an individual is employed (e.g., formal or informal sector respectively) and zero otherwise. The latter calculates each individual's monthly working hours. Both measures are based on the worker's primary job.

Results are plotted in the Figure 1.5.




**(a)** Formal sector **(b)** Informal sector

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**Figure 1.5:** Extensive margin

Source: VHLSS 2004-2014. Notes: The informal sector is defined by social security ownership. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects on the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, and having a secondary job. Full regression results are shown in Appendix Table C5

The figures indicate that the pre-policy trends are flat and not significantly different from zero, supporting the common trend assumption. Within the first two years, there is a significant negative effect on formal employment, with workers in districts experiencing higher minimum wage increases being 1.2 percentage points less likely to be formally employed compared to a baseline formal employment rate of 3.7% in 2006. Meanwhile, the probability of employment in the informal sector increases over time. Workers in higher minimum wage increase districts are associated with a 3.5 percentage points higher probability of having an informal job four years after the policy than those in lower minimum wage increases.

The initial drop in formal employment suggests that firms may require time to adjust their hiring behaviors in response to the minimum wage change, leading to an immediate reduction in employment in the short run. This adjustment period may involve immediate reductions in employment through layoffs or decreased hiring, leading displaced formal workers to reallocate to the informal sector despite its insecurities.

The rise in informal employment in a district with a higher minimum wage increase may be driven by an influx of individuals from the formal sector or the agriculture sector or the pool

of unemployed, resulting in the expansion of the informal sector. This pattern is intuitive, as it is generally easier to secure a job in the informal sector, which typically requires fewer certifications, skills, or qualifications. Further, informal employment may serve as a stepping stone for some workers before transitioning to the formal sector (Tümen, 2016). However, this pattern is limited to a subset of workers rather than applying to all informal workers (O'Higgins, Viegelahn, et al., 2021).

The finding in the informal sector is in line with Meer and West (2016), who showed that the employment effect would increase in both magnitude and significance over time. The effect in period 6 is noisy (e.g., large standard errors) because there were few observations in the six years following the policy's implementation.

### 1.5.3 Intensive margin by sector

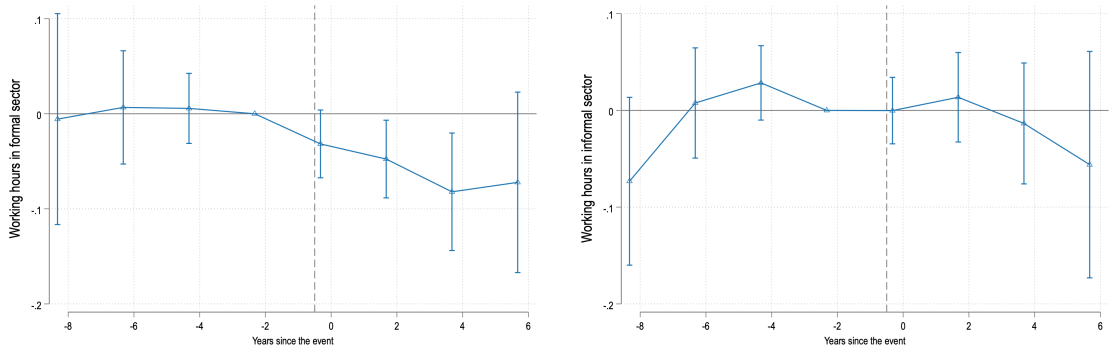
In this section, I now investigate the effects of the minimum wage policy on the intensity of work, proxied by the hours worked, on each sector separately. The results document a significant reduction in hours worked, as shown in Figure 1.6.

Figure 1.6a plots the results in the formal sector. Unlike the short-term effects of formal employment, this reduction in hours of formal workers persists and intensifies over time. Workers in districts with higher minimum wage increases experience an 8.2% decline in working hours four years after the policy implementation. Given a baseline average of 8.2 hours per day in 2006, this corresponds to nearly one hour less worked per day. Although the effect is modest, it strongly suggests that firms adjust working hours in response to the minimum wage increase. Even though the labor demand of the formal sector returns to the pre-policy level two years after the policy, firms continue to use working hours to manage labor costs under the new minimum wage regulations.

The gradual intensification of the adverse effects on working hours in the formal sector, coupled with stable labor demand in the medium term, suggests a labor market where individuals retain formal jobs but work fewer hours. While previous studies, such as those by McGuiness and Redmond (2018) and Jardim et al. (2022), report short-term adverse

effects on hours worked without affecting employment opportunities my analysis reveals that the reduction in hours persists and intensifies over time in the formal sector.

Figure 1.6b plots the results in the informal sector, showing no significant effect on working hours. However, it is difficult to accurately measure the hours in the informal sector due to many reasons, such as irregular work schedules, multiple jobs, and different roles held at the same time.




**(a)** Formal sector **(b)** Informal sector

**Figure 1.6:** Working hours

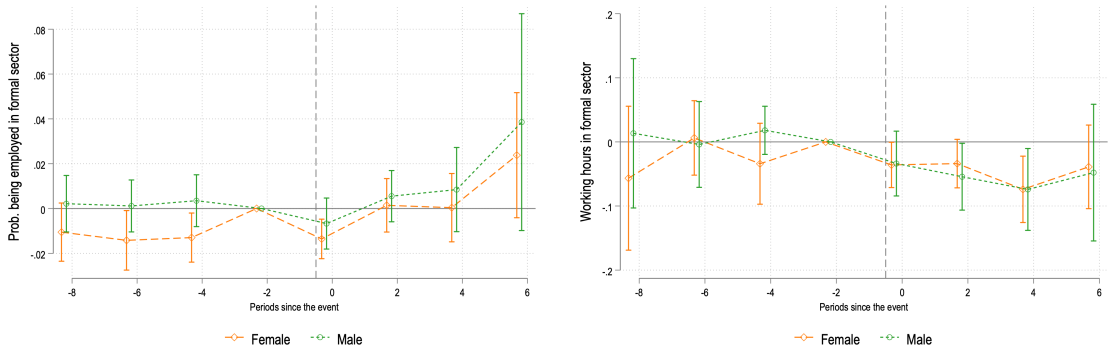
Source: VHLSS 2004-2014. Notes: The informal sector is defined by social security ownership. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, secondary job status, and urban/rural area. Full regression results are shown in Appendix Table C5

One potential concern is anticipation effects. Policies rarely take effect overnight and are often announced in advance; for example, the central government may advertise the regional minimum-wage changes months before formal implementation. Such advance information can induce behavioral adjustments of firms or workers before implementation, resulting in the violation of the no anticipation effects assumption in event-study design. To test for this, I re-assign the event one wave earlier for each treated cohort (e.g., 2006 instead of 2008 event, 2008 instead of 2010 event, etc), and re-estimate the event study using the new reference year (i.e., lag0). If anticipation exists, the coefficient of pseudo-event year (i.e., lag0) should be statistically different from zero, and post-policy coefficients should be smaller relative to the baseline specification because part of the effect is now captured earlier. By contrast, in the

absence of anticipation, the new lag0 coefficient should be indistinguishable from zero and the treatment effects are attenuating. Appendix Figures F7 and F8 document this pattern. First, the graphs show that the coeficients of the pseudo-event year are all indistinguishable from zero. The p-value of the t-test are all too high so that we fail to reject the null hypothesis. Second, the post-coefficients are smaller than in the baseline specification, which is consistent with assumption of no anticipation.

### 1.5.4 Heterogeneity by gender

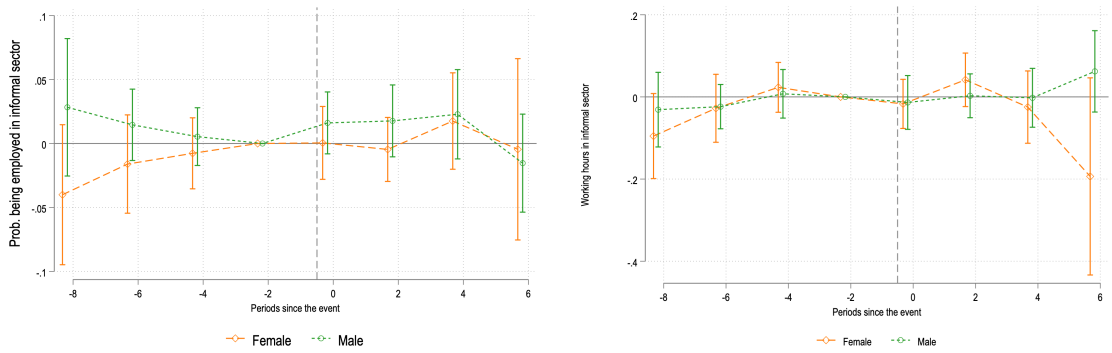
I also investigate whether employment effects are heterogenous across genders. While total employment rates are similar for men and women (McCaig & Pavcnik, 2015), the share of low-wage female workers is higher than that of males (Cling et al., 2011; International Labour Organization, 2022). Several studies suggest that the impact of minimum wage policies varies by gender, as documented in the literature (DiNardo et al., 1995; S. Li & Ma, 2015; Majchrowska & Strawiński, 2018).




**(a)** Extensive margin **(b)** Intensive margin

**Figure 1.7:** Formal sector

Source: VHLSS 2004-2014. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects, and individual control variables: age, educational attainment, marital status, and having a secondary job. Full regression results are shown in Appendix Table E7




**(a)** Extensive margin **(b)** Intensive margin

.

**Figure 1.8:** Informal sector

Source: VHLSS 2004-2014. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects, and individual control variables: age, educational attainment, marital status, and having a secondary job. Full regression results are shown in Appendix Table E7

The results of the extensive and intensive margins are in Figures 1.7 and 1.8. In the formal sector, the short-run disemployment effect is more profound for female workers, who may be more vulnerable and less represented in the overall labor market. However, the impact on working hours is nearly identical for both genders. In the informal sector, while male workers are more likely to have employment than females, there are no significantly different effects on working hours. The industry structure may influence this higher employment rate among men, with manufacturing and logistics dominating job opportunities, as shown in Table B3 in the Appendix.

## 1.6 Income effect

Generally, the findings on the reduction in working hours have essential implications for the effects of minimum wages on workers' total earnings. On the one hand, if the decrease in hours is fully offset by an increase in hourly wages following the minimum wage policy, monthly income would remain stable. On the other hand, if there is no significant increase in hourly wages following the minimum wage policy, the reduction in working hours would lead to a decrease in total monthly income. Hence, in this sector, I investigate the effects of

regional minimum wage policy on income in both sectors separately. The focus here is on the logarithm of the monthly or hourly income from a worker's primary job.

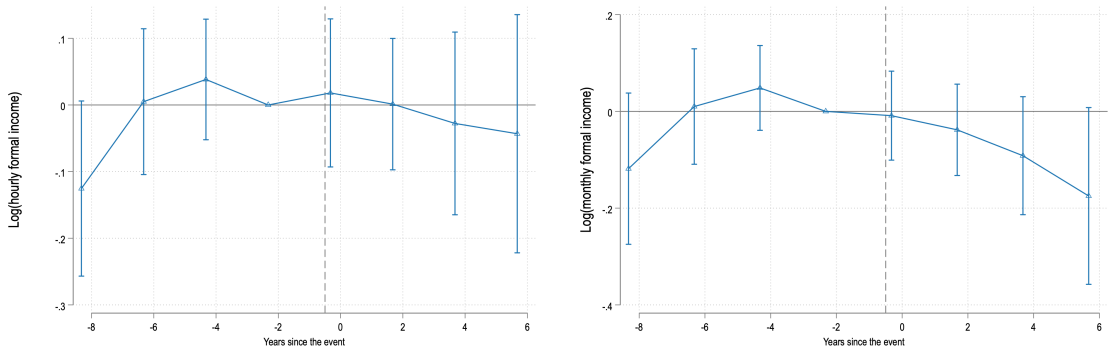
### 1.6.1 The formal sector

The results on formal income are plotted in Figure 1.9. Figure 1.9a shows no significant impact on hourly income, while Figure 1.9b reveals a downward trend in monthly income. Workers in a district with higher minimum wage increases are likely experiencing a decreasing trend in formal income compared to those with lower minimum wage increases. While these findings may seem surprising,especially as the literature often anticipates a rise in hourly income following minimum wage increases,they align with the employment effects discussed earlier. In regions with larger minimum wage hikes, firms responded by cutting working hours, which reduces employees' monthly earnings compared to regions with smaller minimum wage increases.

From the perspective of labor demand, it might seem counterintuitive that firms reduce working hours without increasing formal employment, as this could affect production performance. However, firms may maintain production levels despite fewer working hours, if the same amount of work may be completed in shorter time.

### 1.6.2 The informal sector

Figure 1.10 presents the income effects on the informal sector. Overall, the findings indicate that minimum wage policies do not significantly affect income in the informal sector. Although the results are statistically insignificant, they show a declining trend after the policy implementation. This decrease may be associated with the expansion in the informal sector discussed in the previous section. It is important to mention that the VHLSS data used in this analysis is self-reported, potentially introducing measurement error, particularly in the reporting of earnings. Hence, it would be too noise data to potentially detect any expected positive effects on hourly wage. Given these limitations, the primary objective of this paper remains to examine the employment effects of the minimum wage policy.




**(a)** Hourly formal income **(b)** Monthly formal income

**Figure 1.9:** Income effects on the formal sector

Source: VHLSS 2004-2014. Notes: The outcome is a logarithm of hourly/monthly formal wage. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, having a secondary job, and urban/rural area. Full regression results are shown in Appendix Table C4.

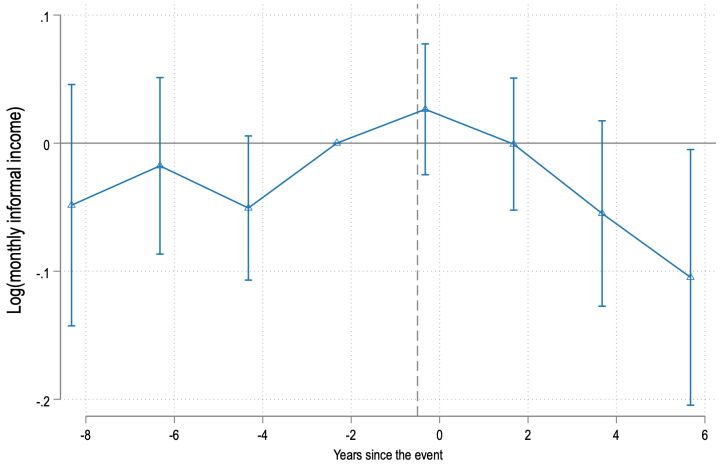



**Figure 1.10:** Income effects on the informal sector

Source: VHLSS 2004-2014. Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, having a secondary job, and urban/rural area. Full regression results are shown in Appendix Table C4.

## 1.7 Further evidence on employment effect

From the above results, the minimum wage fails to raise hourly wages, and fewer working hours potentially result in a decline in monthly income. For low-wage workers, this could disrupt their financial stability and possibly impact their ability to cover basic living expenses. Additionally, reduced working conditions at their primary job may create uncertainty about their career. With the additional free time due to fewer hours worked, workers might pursue secondary employment to secure income and alleviate these risks. Given these possibilities, it is worth investigating the likelihood of workers taking on a second job using the same model specification in equation (1), in which the dependent variable is one if the worker holds a secondary job and zero otherwise. The findings in Figure 1.11 suggest that reducing working

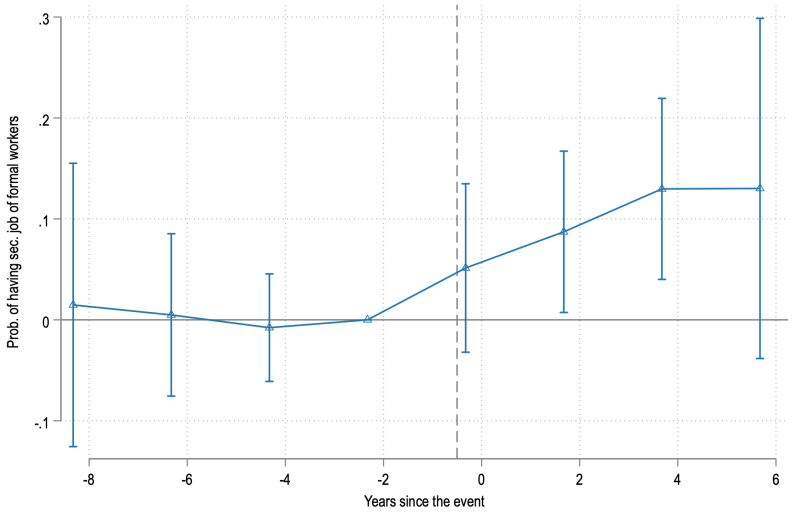



**Figure 1.11:** Probability of a formal worker holding a secondary job

Source: VHLSS 2004-2014. Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, and urban/rural area. Full regression results are shown in Appendix Table C5.

hours in primary jobs leads formal workers to seek additional employment elsewhere. Workers in treated districts are 8.7 percentage points more likely to have a secondary job than those in control districts. The baseline outcome is 34% in the year 2006. The data shows that

secondary jobs are often low-skilled labor tasks (e.g., agricultural work, retail, services) in the informal sector. While these jobs may not be well-paid, they still can contribute to monthly earnings. As a result, the effects on income may be less pronounced. Unfortunately, the individual cross-sectional data does not allow me to investigate the income effects on individuals who did not have a secondary job prior to the minimum wage increase but took one up as a result. Instead, I can only observe income changes for individuals who held secondary jobs before and after the policy. I run the same specification on total income from both jobs, and the results in Figure 1.12 show an upward trend in total income despite a drop at time 0. This contrasts with the income effect on primary jobs, suggesting that those with secondary employment are not adversely affected by reduced income from their primary job. However, despite this compensation, reduced hours in formal employment may still limit access to social benefits, potentially pushing employees toward insecure, unstable jobs in the informal sector,ultimately posing risks to their overall social welfare.

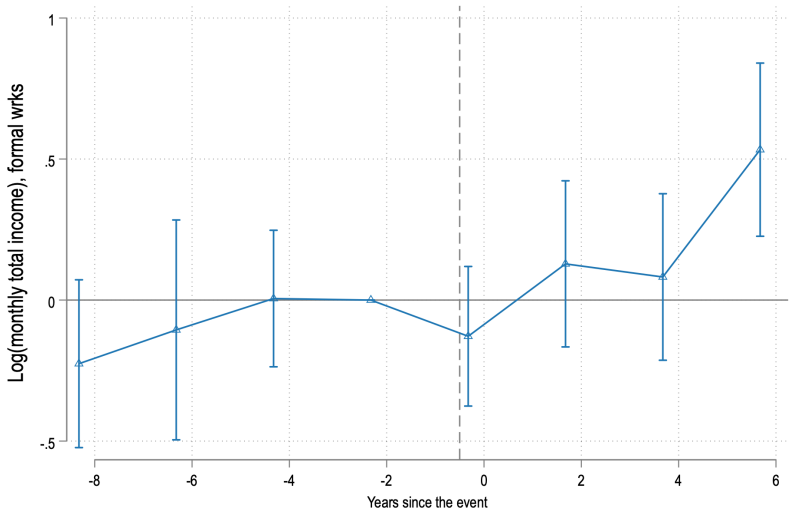



**Figure 1.12:** Total income of a formal worker holding a secondary job

Source: VHLSS 2004-2014. Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, and urban/rural area. Full regression results are shown in Appendix Table C4.

## 1.8 Movement mechanism

Thus far, I have investigated how minimum wage policies affect the formal and informal sectors separately. However, the literature also suggests that minimum wage policy can drive workers to move between sectors (as in the Gramlich-Mincer model). In the previous section, I documented a sharp drop in formal employment and a rise in informal employment immediately after the implementation of the minimum wage policy. To explore whether the increase in informal employment stems from compositional changes (e.g., displacement from the formal sector or shifts in workforce demographics, such as an influx of younger, less-skilled workers) or from the effects of the minimum wage policy, I analyze two subset of data from my overall VHLSS dataset. These datasets cover the 2006–2008 and 2010–2012 periods. I selected these periods because there is no linkage between the 2008 and 2010 surveys, as explained in the data section. In the two subsets of the data, I consider the treatment that occurred in 2008 and 2012, respectively, while treating those districts not treated in these years as control.

Because the data collection did not trace workers across the two data rounds who left the districts, this analysis only looks at workers who remain in the same district. It therefore does not account for the possibility of migration between districts due to the minimum wage increases.

I estimate the standard difference-in-difference with two periods for individual $i$ in year $t$ to capture the general movement of any individual:

$$Y_{idt} = \alpha + \beta_1 treated_{dt} \times post_{dt} + \beta_2 treated_{dt} + \beta_3 post_{dt} + \tau X_{idt} + \delta_d + \sigma_t + \epsilon_{idt} \qquad (1.3)$$

In this analysis, I consider several movements of workers separately. $Y_{idt}$ is the outcome variable for individual $i$ in district $d$ at time $t$, representing reallocation from the formal to the informal sector in column (1) in Table 1.3, from agriculture to the formal or informal sector in column (2) and (3) in Table 1.3, or from unemployment to employment in column (4) in Table 1.3. For example, in the case of $Formal \rightarrow Informal$, the outcome variable

$Y_{idt}$ equals one if the individual is employed in the informal sector and zero if employed in the formal sector.

**Table 1.3:** Reallocation effects on workers

| | Formal→Informal | Agri→Formal | Agri→Informal | Unemployment→Employment |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: 2006-2008** | | | | |
| treat×post | -0.004 | 0.002 | -0.006 | 0.014 |
| | (0.042) | (0.004) | (0.032) | (0.029) |
| Mean of dep. var. | 0.90 | 0.05 | 0.39 | 0.69 |
| $N$ | 2882 | 4582 | 7950 | 4544 |
| **Panel B: 2010-2012, excluding the treated districts before** | | | | |
| treat×post | -0.0000862 | -0.00577* | 0.0285 | 0.00549 |
| | (0.0118) | (0.00349) | (0.0228) | (0.0148) |
| Mean of dep. var. | 0.97 | 0.01 | 0.35 | 0.78 |
| $N$ | 2414 | 4972 | 8558 | 3230 |
| **Panel C: 2010-2012, including the treated districts before** | | | | |
| treat×post | 0.00646 | -0.00378 | 0.0237 | 0.00493 |
| | (0.0103) | (0.00367) | (0.0150) | (0.0101) |
| Mean of dep. var. | 0.94 | 0.03 | 0.42 | 0.74 |
| $N$ | 4258 | 5844 | 11184 | 6068 |

Notes: The sample is the individual panel data from 2006 to 2008 in Panel A and from 2010 to 2012 in Panel B. "Formal→Informal" indicates the movement of people from the formal into the informal sector. "Agri→Formal" indicates the movement of people from agriculture into the formal sector. "Agri→Informal" indicates the movement of people from agriculture into the informal sector. "Unemployment→Employment" indicates the movement of people from being unemployed to employed. Robust clustered standard errors are in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

Due to the staggered implementation of the minimum wage policy across districts, I consider two cases for the 2010–2012 panel. First, I analyze only the districts treated during 2011–2012, excluding those treated earlier in 2008 and 2010. This isolates the effect on newly treated districts. Second, I add back all districts treated earlier (whose treatment status though does not change between 2010-2012) to provide a more comprehensive analysis. By comparing these two cases, I aim to capture the nuanced impacts of the minimum wage policy, accounting for variations in treatment timing and district-specific effects.

The results are displayed in Table 1.3. The primary concern is the movement of displaced formal workers into the informal sector. However, column (1) does not find evidence for that. The coefficient is statistically insignificant and even indicates a slight decrease of 0.4 percentage point in the movement from formal to informal employment in the 2006–2008

panel, which is negligible given the 90% baseline rate in 2006[11]. Given the insignificant and mixed effects observed across various panels, the data does not support the hypothesis that displaced formal workers are transitioning to the informal sector.

When examining movements from agriculture to other sectors, columns (2) and (3) show mixed and statistically insignificant effects. The statistical insignificance of the results in column (2) is unsurprising, as it is challenging for agricultural workers to find formal employment due to skill and qualification requirements. However, I also do not observe any significant effects on the transition from agriculture to the informal sector. Given Vietnam's agriculture-based economy and ongoing structural transformation, the regional minimum wage reform does not seem to have contributed to this transformation.

These findings are in line with Derenoncourt and Montialoux (2021), who also do not observe any significant reallocation of low-productivity workers to the informal sector. Their study suggests that it is more relevant to consider the theory of competition among firms in both the formal and informal sectors within an oligopsonistic labor market, as proposed by Berger et al. (2021). As Meghir et al. (2015) state, in a country with a high informality rate, low-skilled workers can be found in both sectors, are mobile across sectors, and can potentially secure employment in either sector. This is attributed to the coexistence of formal and informal firms within a certain range of productivity distribution (La Porta & Shleifer, 2008).

## 1.9 Threats to validity and sensitivity checks

In this section, I discuss the validity of the parallel trend assumption and evaluate the robustness of the results. This includes testing against various TWFE estimators, accounting for potential confounding factors, and adjusting for different bandwidths in the Conley standard error approach. I also present robustness checks for the three key outcomes: the probability of having a job, total hours worked, and monthly income separately in the formal or informal

[11]Here, the 90% does not refer to the descriptive statistics in Table 1.2, but rather to the share of informal workers when considering only two employment categories: formal and informal jobs. Specifically, dividing 0.31 by 0.31 + 0.04 from Table 1.2 yields approximately 0.9, or 90%.

sector.

### 1.9.1 Alternative TWFE estimators

The traditional assumptions for difference-in-difference are parallel trends, no anticipation, and homogenous treatment timing. However, recent studies have highlighted concerns regarding the impact of staggered treatment timings and dynamic treatment effects on traditional two-way fixed-effects (TWFE) estimates. L. Sun and Abraham (2021) shows that the coefficients of leads and lags will be contaminated by other leads and lags' information due to the heterogeneity in treatment. Hence, I implement the Sun and Abraham estimator (hereby SA) as a robustness check. In the SA estimation, the control group (i.e., the never-treated group) is the district with no regional minimum wage jump (i.e., always remains at the last MW level) or that was treated in the last year of the sample (i.e., 2014).

The second estimation is the stacked regression proposed by Cengiz et al. (2019). Here, the clean control group is the districts that were not yet treated or never treated during the event window. Because the never-treated group accounts for a large share of sample observations (i.e., 413 districts of 662 districts as Table 1.1). There is an insignificant variation in treatment timing, so I expect to find a lower likelihood of bias in TWFE staggered DiD regressions. I also implement other robust estimators introduced by Callaway and Sant'Anna (2021) and Borusyak et al. (2024), which relax the strong assumptions on homogeneity in treatment effects. These allow for heterogeneous treatment effects across groups and across time. Notably, for Borusyak et al. (2024) estimator, I only estimate six pre-periods (i.e., three survey waves) given that adding too many pre-periods increases the standard errors (Borusyak et al., 2024). Utimately, I use only L. Sun and Abraham (2021) and Cengiz et al. (2019) as the appropriate estimations on period t=-8. In these alternative estimations, I include the individual controls (e.g., age, educational attainment, marital status, secondary job, and urban/rural area).

The results from the alternative estimations are consistent with those from the traditional TWFE staggered DiD regressions. As shown in Figure F8 in the Appendix, there are no

significant differences among the estimators regarding monthly income. For employment effects, the SA estimator shows a slight difference in the last lag (i.e., lag 6) but with the same direction of effects as in the traditional TWFE staggered DiD regression.

### 1.9.2 Potential confounders

Regarding omitted confounders, I conducted a sensitivity analysis to check the robustness of the baseline estimates to local market conditions. Instead of the simple"Year" fixed effect, I include the"Province x Year" fixed effects to control for any local shocks at the province level that might affect both treatment and outcomes. Any provincial-level shock (i.e., higher aggregate level compared to the district level) would have the same effects across all districts in that province. Figure F10 and Figure F11 in the Appendix illustrate the results. Both figures show that the baseline estimates are robust across specifications that share similar trends in treatment effects.

### 1.9.3 Different bandwidths

As mentioned in the inference session, I adopt a Conley standard error 40km cutoff in the baseline. However, to check the sensitivity and robustness of the baseline results to different cutoffs (i.e., bandwidth), I conduct the same specification with different bandwidths (e.g., 30km and 50km). Although the standard errors vary slightly, the coefficients maintain statistical significance, as in baseline specification across the bandwidth. The results are documented in Figure F12, Table F9, Table F10, Table F11, Table F12. This demonstrates that my findings are not sensitive to the specific choice of bandwidth.

## 1.10 Discussion and conclusion

This paper investigates the labor market effects of variations in minimum wage levels in Vietnam, a developing country with a high rate of informal employment, induced by the introduction of regional variations in minimum wage levels after a previously uniform minimum wage level in the country. The analysis reveals that Vietnam's introduction of regional

variation in minimum wage levels has no overall disemployment effect on the labor market in districts with relatively higher minimum wage following the reform. If anything, it positively impacts total employment four years after its implementation. However, the employment effects diverge when examined by sector. While informal employment consistently rises, there is a short-term drop in formal employment (the extensive margin) following the enactment of the policy. Although this decline in formal employment is consistent with the literature on adverse employment effects, it is only temporary. Additionally, I do not detect significant reallocation effects of people between sectors (e.g., formal to informal sector) when analyzing two separate 2-year panels of individual workers. Thus, it supports the hypothesis that the effects on formal and informal sector are not due to the compositional changes, rather its the true effects of regional minimum wage policy in each sector.

One of the findings on firms' adjustments in response to the minimum wage policy is a reduction in hours worked (the intensive margin). Furthermore, the increased likelihood of employees holding secondary jobs suggests that reduced hours in their primary jobs may have driven them to seek additional employment to increase their overall income (i.e., utilizing the extra free time).

Additionally, the analysis reveals a decreasing trend in monthly formal earnings, though it is statistically insignificant. While these findings contradict traditional minimum wage literature, they are consistent with the findings on reducing working hours for formal workers. I do not find a similar negative trend on incomes for individuals who obtain secondary informal jobs, confirming that the secondary employment helps offset income losses from primary jobs. However, as discussed earlier, this reliance on secondary jobs, which are typically informal and require no special skills, may introduce additional insecurities for employees. In the long run, informal secondary employment may not provide workers with a stable or sustainable solution.

Because this analysis primarily focuses on the labor supply side,representing only one aspect of the minimum wage policy debate,future research could benefit from examining the demand side. A potential extension of this study would involve a deeper investigation into firms' re-

sponses to minimum wage policy, including their hiring practices, wage-setting mechanisms, and productivity across industries, with particular attention to the differential impacts between tradable and non-tradable sectors (Magruder, 2013).

# Appendices

## A Background

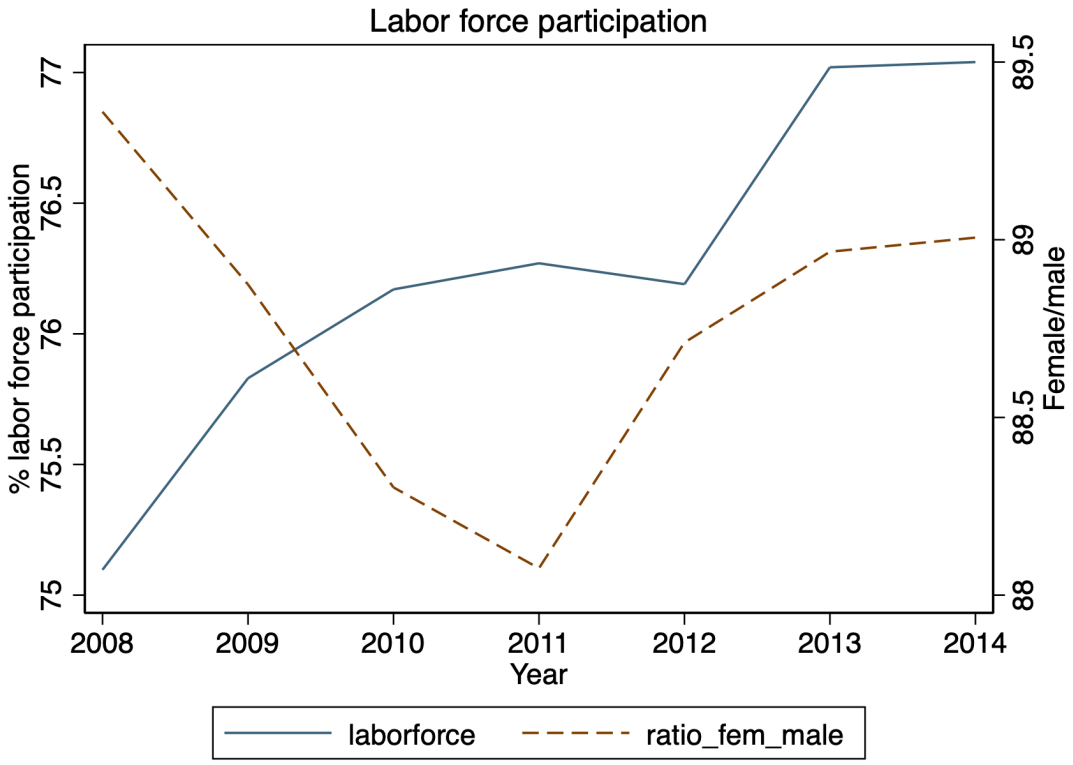


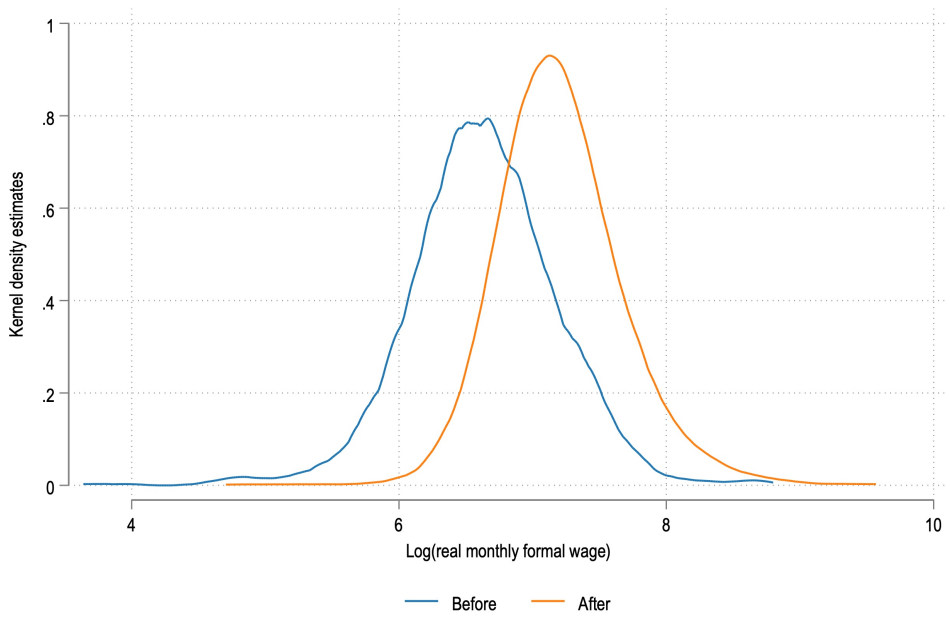

**Figure A1:** Employment in Vietnam during 2008-2014.

Source: World Bank. Notes: The left column displays the % total employment of popupation over 15 years old. The right column displays the ratio of female to male total employment rate.

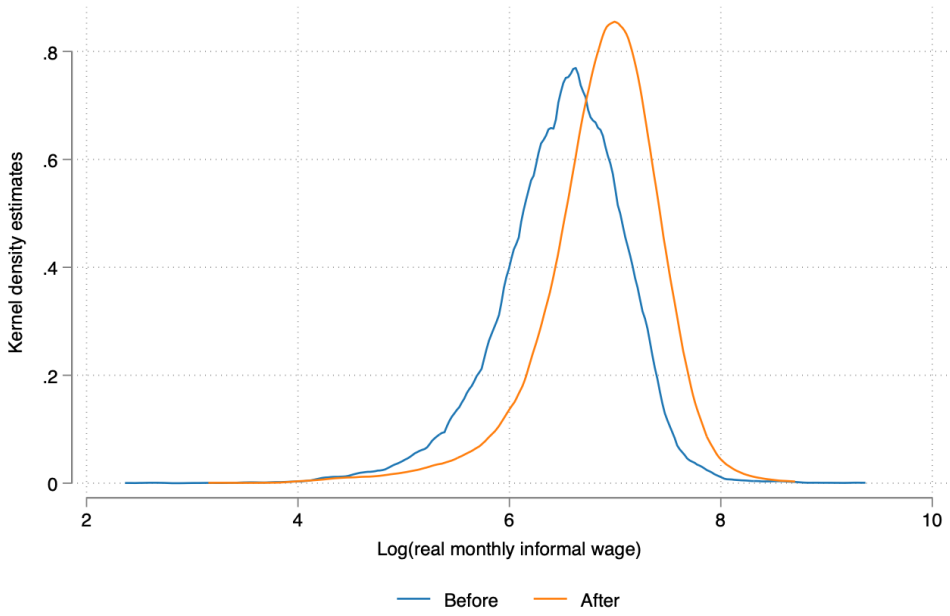
**Table A1:** Monthly minimum wage for the formal private sector from 2004 to 2014

| | Nominal MW | | | | Real MW | | | |
|---|---|---|---|---|---|---|---|---|
| **Year** | **Region I** | **Region II** | **Region III** | **Region IV** | **Region I** | **Region II** | **Region III** | **Region IV** |
| **2004** | 350 | 350 | 350 | 350 | 294.1 | 294.1 | 294.1 | 294.1 |
| **2006** | 450 | 450 | 450 | 450 | 333.58 | 333.58 | 333.58 | 333.58 |
| **2008** | **620** | **580** | **540** | **540** | **345.1** | **323** | **300** | **300** |
| **2010** | 980 | 880 | 810 | 730 | 467.5 | 420 | 386.4 | 348.22 |
| **2012** | 2000 | 1780 | 1550 | 1400 | 736.7 | 655 | 571 | 515.7 |
| **2014** | 2700 | 2400 | 2100 | 1900 | 902.6 | 802.4 | 702 | 635.2 |

Wages are in '000 VND per month. CPI with base year 2000. Author's calculation




**(a)** Formal sector




**(b)** Informal sector

**Figure A2:** Income distribution across sectors, before and after the minimum wage increases

Source: Authors calculation. Notes: These curves plot the Kernel density estimates of wage compression, that is calculated by the difference between log(real MW) - log(real wage), before and after the minimum wage policy across sectors


**Figure A3:** Densities of the difference between income and minimum wage, group treated in 2010

Source: author's calculation from VHLSS. Notes: The figures plot the kernel density estimates (with a bandwidth of 0.2) of the difference between log(nominal income) (pooling formal and informal sector) and log(regional minimum wage) of the treated districts over years.

# B Descriptive statistics

**Table B2:** Baseline statistics of districts in 2006, by MW level assigned in 2008 and 2010

| | MW level in 2008 | | MW level in 2010 of district-level-3 in 2008 | | |
|---|---|---|---|---|---|
| | MW level 3 | MW level 2 | MW level 4 | MW level 3 | MW level 2 |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Demographic variables** | | | | | |
| Female | 0.498 (0.500) | 0.500 (0.500) | 0.499 (0.500) | 0.497 (0.500) | 0.483 (0.500) |
| Age | 36.877 (12.689) | 37.537 (12.438) | 36.527 (12.881) | 37.907 (12.044) | 38.205 (11.792) |
| Marital status | | | | | |
| *Single* | 0.230 (0.421) | 0.248 (0.432) | 0.232 (0.422) | 0.227 (0.419) | 0.218 (0.414) |
| *Married* | 0.724 (0.447) | 0.708 (0.455) | 0.723 (0.448) | 0.727 (0.446) | 0.733 (0.443) |
| Educational level | | | | | |
| *No Training* | 0.007 (0.083) | 0.001 (0.028) | 0.008 (0.088) | 0.004 (0.061) | 0.004 (0.067) |
| *Primary school* | 0.290 (0.454) | 0.218 (0.413) | 0.298 (0.457) | 0.264 (0.441) | 0.252 (0.434) |
| *Secondary school* | 0.315 (0.465) | 0.343 (0.475) | 0.315 (0.465) | 0.319 (0.466) | 0.310 (0.463) |
| *High school* | 0.141 (0.348) | 0.271 (0.445) | 0.119 (0.324) | 0.203 (0.402) | 0.256 (0.437) |
| *College or above* | 0.248 (0.432) | 0.167 (0.373) | 0.260 (0.439) | 0.210 (0.407) | 0.178 (0.383) |
| **Panel B: Wage variables** | | | | | |
| Monthly formal inc., '000 VND | 1,078.011 (597.192) | 1,190.774 (773.312) | 982.288 (519.429) | 1,193.954 (675.412) | 1,283.926 (617.916) |
| Monthly informal inc., '000 VND | 905.212 (632.119) | 1,081.332 (774.016) | 882.141 (593.440) | 948.399 (728.421) | 955.776 (416.591) |
| Having secondary job | 0.435 (0.496) | 0.285 (0.452) | 0.474 (0.499) | 0.316 (0.465) | 0.274 (0.446) |
| Below 2008 MW | 0.219 (0.414) | 0.109 (0.312) | 0.272 (0.445) | 0.132 (0.339) | 0.102 (0.304) |
| **Panel C: Working variables** | | | | | |
| Working sector | | | | | |
| *Informal sector* | 0.311 (0.463) | 0.407 (0.492) | 0.275 (0.446) | 0.420 (0.494) | 0.454 (0.498) |
| *Collective* | 0.004 (0.066) | 0.004 (0.063) | 0.003 (0.058) | 0.007 (0.082) | 0.011 (0.105) |
| *Private (formal) sector* | 0.040 (0.195) | 0.063 (0.242) | 0.030 (0.169) | 0.069 (0.253) | 0.102 (0.304) |
| *State sector* | 0.088 (0.284) | 0.153 (0.360) | 0.063 (0.244) | 0.165 (0.371) | 0.185 (0.389) |
| *Foreign Inv. sector* | 0.011 (0.106) | 0.075 (0.263) | 0.005 (0.073) | 0.031 (0.172) | 0.029 (0.168) |
| *Agriculture sector* | 0.545 (0.498) | 0.299 (0.458) | 0.624 (0.485) | 0.309 (0.462) | 0.218 (0.414) |
| Industries | | | | | |
| *Agriculture, Forest, Fishery* | 0.545 (0.498) | 0.299 (0.458) | 0.624 (0.485) | 0.309 (0.462) | 0.218 (0.414) |
| *Mining* | 0.038 (0.190) | 0.008 (0.089) | 0.041 (0.199) | 0.026 (0.159) | 0.029 (0.168) |
| *Manufacturing* | 0.105 (0.307) | 0.226 (0.418) | 0.088 (0.283) | 0.159 (0.366) | 0.174 (0.379) |
| *Energy* | 0.001 (0.029) | 0.007 (0.085) | 0.001 (0.027) | 0.001 (0.023) | 0.007 (0.082) |
| *Water supply* | 0.015 (0.123) | 0.025 (0.156) | 0.011 (0.103) | 0.029 (0.168) | 0.038 (0.191) |
| *Construction* | 0.000 (0.021) | 0.002 (0.049) | 0.000 (0.017) | 0.001 (0.032) | 0.000 (0.000) |
| *Retail sales about vehicles* | 0.052 (0.222) | 0.076 (0.266) | 0.044 (0.205) | 0.075 (0.264) | 0.089 (0.285) |
| *Logistics* | 0.133 (0.339) | 0.174 (0.380) | 0.107 (0.309) | 0.212 (0.409) | 0.227 (0.419) |
| *Simple labor* | 0.004 (0.064) | 0.006 (0.080) | 0.004 (0.060) | 0.006 (0.076) | 0.002 (0.047) |
| N | 17,613 | 1,245 | 13,279 | 3,803 | 449 |

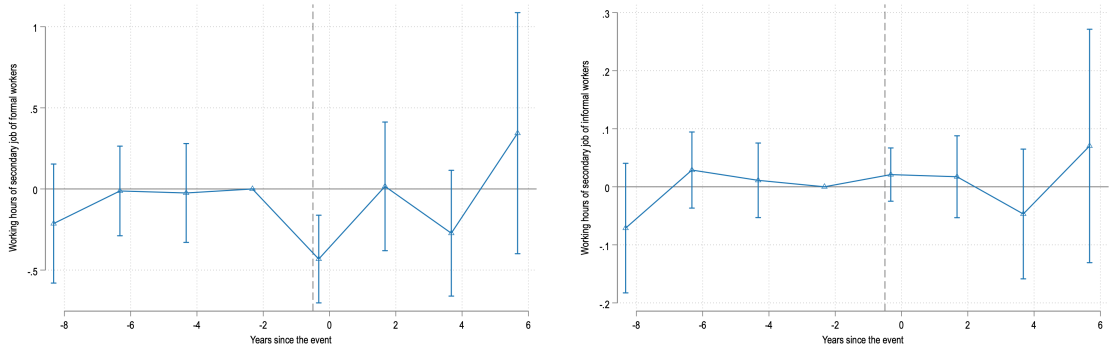
Source: Author's calculations based on VHLSS 2006. Notes: The sample includes individuals aged 15 to 65. The table presents baseline statistics of districts in 2006. Column (1) and (2) report the districts assigned to MW level 3 or 2 in 2008. Conditional on assigned to MW level 3 in 2008, column (3)-(5) report the districts assigned to level 4, 3 or 2 in 2010. In 2008, there are three minimum wage (MW) levels, but districts assigned to level 1,the highest MW level, located in Ho Chi Minh City and Hanoi, are excluded from this analysis. Income is the nominal one.

**Table B3:** Descriptive statistics by sector, VHLSS 2004-2014

| | 2004-2006 | | 2008-2014 | |
|---|---|---|---|---|
| | Formal sector | Informal sector | Formal sector | Informal sector |
| **Panel A: Demographic variables** | | | | |
| Female | 0.385 (0.487) | 0.449 (0.497) | 0.450 (0.498) | 0.459 (0.498) |
| Age | 29.719 (10.658) | 36.086 (11.808) | 31.532 (9.823) | 37.885 (11.985) |
| Marital status | | | | |
| *Single* | 0.500 (0.500) | 0.248 (0.432) | 0.340 (0.474) | 0.197 (0.398) |
| *Married* | 0.482 (0.500) | 0.706 (0.456) | 0.635 (0.482) | 0.750 (0.433) |
| *Wisdowed* | 0.012 (0.109) | 0.028 (0.166) | 0.010 (0.100) | 0.029 (0.166) |
| *Divorced* | 0.003 (0.053) | 0.013 (0.113) | 0.011 (0.106) | 0.018 (0.132) |
| *Separated* | 0.004 (0.059) | 0.005 (0.070) | 0.004 (0.065) | 0.007 (0.081) |
| Education attainment | | | | |
| *No Training* | 0.010 (0.099) | 0.010 (0.097) | 0.025 (0.156) | 0.094 (0.292) |
| *Primary school* | 0.251 (0.434) | 0.331 (0.471) | 0.153 (0.360) | 0.289 (0.453) |
| *Secondary school* | 0.327 (0.469) | 0.342 (0.474) | 0.274 (0.446) | 0.359 (0.480) |
| *High school* | 0.263 (0.441) | 0.143 (0.350) | 0.346 (0.476) | 0.185 (0.388) |
| *College or above* | 0.148 (0.355) | 0.175 (0.380) | 0.202 (0.401) | 0.073 (0.260) |
| **Panel B: Wage variables** | | | | |
| Monthly income, '000 VND | 974.685 (580.425) | 811.021 (552.620) | 3,191.227 (2,349.780) | 2,406.155 (1,518.935) |
| Below current MW | 0.027 (0.162) | 0.068 (0.252) | 0.025 (0.155) | 0.129 (0.335) |
| Below prev. MW | 0.017 (0.129) | 0.035 (0.183) | 0.033 (0.179) | 0.085 (0.279) |
| **Panel C: Working variables** | | | | |
| Having secondary job | 0.265 (0.441) | 0.428 (0.495) | 0.178 (0.383) | 0.408 (0.491) |
| Industries: | | | | |
| *Mining* | 0.011 (0.106) | 0.114 (0.318) | 0.016 (0.127) | 0.008 (0.088) |
| *Manufacturing* | 0.504 (0.500) | 0.204 (0.403) | 0.576 (0.494) | 0.274 (0.446) |
| *Energy* | 0.004 (0.059) | 0.001 (0.032) | 0.005 (0.068) | 0.001 (0.029) |
| *Water supply* | 0.052 (0.221) | 0.033 (0.178) | 0.006 (0.079) | 0.003 (0.051) |
| *Construction* | 0.001 (0.027) | 0.000 (0.021) | 0.091 (0.288) | 0.179 (0.383) |
| *Retail sales about vehicles* | 0.144 (0.351) | 0.139 (0.346) | 0.122 (0.328) | 0.292 (0.455) |
| *Logistics* | 0.182 (0.386) | 0.385 (0.487) | 0.091 (0.287) | 0.159 (0.366) |
| *Simple labor* | 0.004 (0.065) | 0.015 (0.120) | 0.009 (0.094) | 0.051 (0.219) |
| N | 1,412 | 11,846 | 2,377 | 25,482 |

Sources: Author's calculation from VHLSS 2004-2014. Notes: The sample is restricted to people from 15 to 65 years old, working in either formal or informal sector.

# C Baseline results




**(a)** Formal workers **(b)** Informal workers

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**Figure C4:** Working hours on secondary job

Source: VHLSS 2004-2014. Notes: The informal sector is defined by social security ownership. The outcome is the working hours on secondary job of either formal or informal worker. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately for the whole sample, and for females and for males. All regression models include "district" and "year" fixed effects, and individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Full regression results are shown in Appendix Table C5

**Table C4:** Income effect on individual - Baseline estimates

| | Log(monthly income) | | Log(hourly income) | | Log(total monthly income) |
|---|---|---|---|---|---|
| | Formal sector | Informal sector | Formal sector | Informal sector | of formal workers |
| | (1) | (2) | (3) | (4) | (5) |
| -8 | -0.118 | -0.048 | -0.125* | -0.045 | -0.226 |
| | (0.080) | (0.048) | (0.067) | (0.051) | (0.152) |
| -6 | 0.010 | -0.018 | 0.005 | -0.054 | -0.106 |
| | (0.061) | (0.035) | (0.056) | (0.039) | (0.199) |
| -4 | 0.048 | -0.051* | 0.038 | -0.080*** | 0.005 |
| | (0.045) | (0.029) | (0.046) | (0.029) | (0.123) |
| 0 | -0.009 | 0.026 | 0.018 | -0.001 | -0.128 |
| | (0.047) | (0.026) | (0.057) | (0.024) | (0.126) |
| 2 | -0.038 | -0.001 | 0.001 | -0.038 | 0.128 |
| | (0.048) | (0.026) | (0.050) | (0.030) | (0.150) |
| 4 | -0.092 | -0.055 | -0.028 | -0.123*** | 0.082 |
| | (0.062) | (0.037) | (0.070) | (0.041) | (0.151) |
| 6 | -0.175* | -0.105** | -0.043 | -0.142*** | 0.533*** |
| | (0.093) | (0.051) | (0.091) | (0.037) | (0.157) |
| Obs | 3450 | 14185 | 3450 | 14185 | 726 |

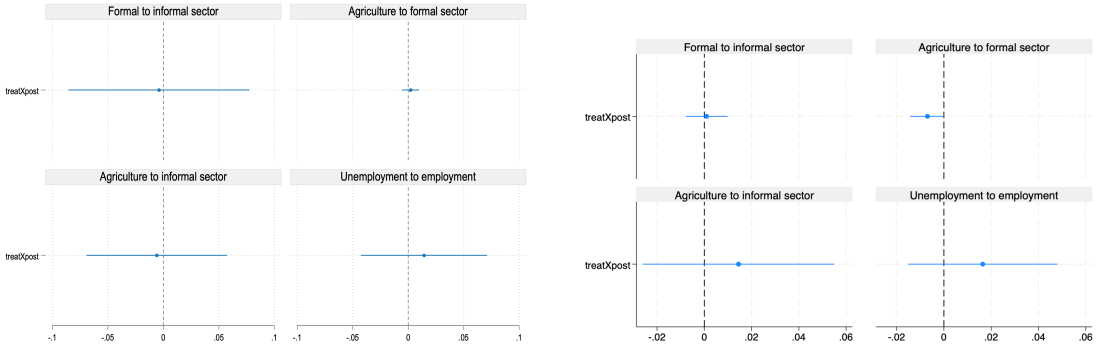
Sources: VHLSS 2004-2014. Notes: "Total monthly income" is defined as the total income from both primary and secondary jobs of formal workers. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

**Table C5:** Employment effect on individual - Baseline estimates

| | Labor force | Probability of being employed | | Working hours | | Probability of having secondary job | |
|---|---|---|---|---|---|---|---|
| | participation | Formal sector | Informal sector | Formal sector | Informal sector | Formal wrks | Informal wrks |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| -8 | -0.007 | -0.002 | -0.005 | -0.006 | -0.073* | 0.015 | 0.024 |
| | (0.025) | (0.006) | (0.025) | (0.057) | (0.044) | (0.072) | (0.033) |
| -6 | 0.001 | -0.004 | 0.005 | 0.007 | 0.008 | 0.005 | 0.008 |
| | (0.017) | (0.006) | (0.017) | (0.030) | (0.029) | (0.041) | (0.019) |
| -4 | 0.004 | -0.003 | 0.007 | 0.006 | 0.028 | -0.008 | 0.006 |
| | (0.012) | (0.005) | (0.012) | (0.019) | (0.020) | (0.027) | (0.015) |
| 0 | 0.002 | -0.012** | 0.014 | -0.032* | -0.000 | 0.051 | 0.005 |
| | (0.012) | (0.005) | (0.011) | (0.018) | (0.018) | (0.043) | (0.014) |
| 2 | 0.018 | -0.001 | 0.018 | -0.048** | 0.014 | 0.087** | -0.024 |
| | (0.013) | (0.006) | (0.012) | (0.021) | (0.024) | (0.041) | (0.018) |
| 4 | 0.034** | -0.000 | 0.035** | -0.082*** | -0.013 | 0.130*** | -0.025 |
| | (0.015) | (0.008) | (0.016) | (0.032) | (0.032) | (0.046) | (0.021) |
| 6 | 0.031** | 0.020 | 0.010 | -0.072 | -0.056 | 0.130 | -0.023 |
| | (0.014) | (0.016) | (0.029) | (0.048) | (0.060) | (0.086) | (0.034) |
| Obs | 108174 | 108174 | 108174 | 3611 | 34036 | 3612 | 34038 |

Sources: VHLSS 2004-2014. Notes: Column (4) and (5) indicate the working hours in formal/informal sector as the primary job of worker. Column (6) and (7) represents the probability of having a secondary job of formal/informal workers respectively. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

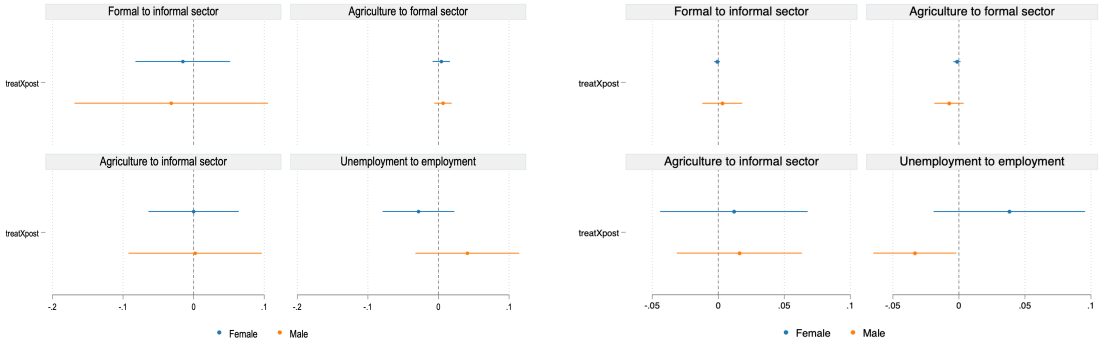
# D Further evidence on the movement




**(a)** Panel 2006-2008 **(b)** Panel 2010-2012

**Figure D5:** Movement across sectors

Source: VHLSS panel data 2006-2008 and 2010-2012. Notes: All curves depict event study estimates and the corresponding 95% confidence bands. Robust clustered standard errors are depicted. With each dependent variable, the figures present treatment effects separately varying genders. All regressions models include "district" and "year" fixed effects, individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. The results are reported in Table 1.3 in the appendix




**(a)** Panel 2006-2008 **(b)** Panel 2010-2012

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**Figure D6:** Movement across sectors, by gender

Notes: All curves depict event study estimates and the corresponding 95% confidence bands. Robust clustered standard errors are depicted. With each dependent variable, the figures present treatment effects separately varying genders. All regressions models include "district" and "year" fixed effects, individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Source: VHLSS panel data 2006-2008 and 2010-2012.

# E Heterogeneity by gender

**Table E6:** Income effects on individual, across gender

| | Log(monthly income) | | Log(hourly income) | | Log(total monthly income) |
|---|---|---|---|---|---|
| | Formal sector | Informal sector | Formal sector | Informal sector | of formal workers |
| | (1) | (2) | (3) | (4) | (5) |
| -8Xfemale | -0.073 | 0.001 | -0.017 | -0.048 | -0.117 |
| | (0.117) | (0.061) | (0.107) | (0.071) | (0.539) |
| -6Xfemale | -0.028 | 0.012 | 0.044 | -0.015 | 0.439 |
| | (0.063) | (0.053) | (0.070) | (0.046) | (0.295) |
| -4Xfemale | -0.012 | 0.009 | -0.026 | 0.013 | -0.189 |
| | (0.051) | (0.039) | (0.058) | (0.043) | (0.451) |
| 0Xfemale | -0.005 | 0.047 | -0.002 | -0.019 | 0.056 |
| | (0.054) | (0.040) | (0.052) | (0.033) | (0.615) |
| 2Xfemale | 0.023 | 0.117*** | -0.010 | -0.066* | 0.102 |
| | (0.053) | (0.034) | (0.053) | (0.034) | (0.392) |
| 4Xfemale | 0.062 | 0.114*** | -0.073 | -0.061 | -0.683 |
| | (0.047) | (0.041) | (0.049) | (0.040) | (0.460) |
| 6Xfemale | 0.023 | 0.191** | -0.167* | -0.193* | -3.206*** |
| | (0.043) | (0.093) | (0.086) | (0.106) | (0.601) |
| Obs | 3450 | 14185 | 3450 | 14185 | 726 |

Sources: VHLSS 2004-2014. Notes: All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

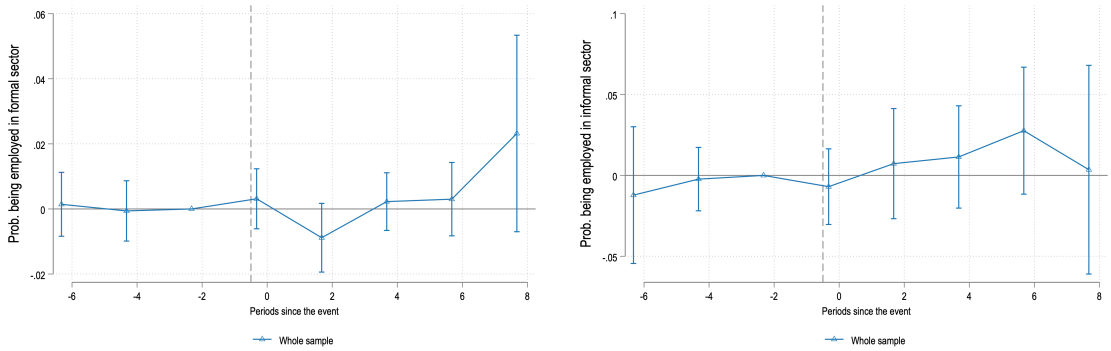
**Table E7:** Employment effect on individual, across gender

| | Labor force | Probability of being employed | | Working hours | | Probability of having secondary job | |
|---|---|---|---|---|---|---|---|
| | participation | Formal sector | Informal sector | Formal sector | Informal sector | Formal wrks | Informal wrks |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| -8Xfemale | -0.071*** | -0.012 | -0.060** | -0.064 | 0.004 | 0.116 | 0.058** |
| | (0.022) | (0.008) | (0.025) | (0.045) | (0.036) | (0.138) | (0.028) |
| -6Xfemale | -0.046** | -0.016** | -0.030* | 0.021 | 0.031 | -0.037 | 0.038 |
| | (0.018) | (0.006) | (0.017) | (0.035) | (0.021) | (0.046) | (0.024) |
| -4Xfemale | -0.031** | -0.015** | -0.015 | -0.037 | 0.050** | 0.009 | 0.032 |
| | (0.013) | (0.007) | (0.013) | (0.033) | (0.024) | (0.052) | (0.027) |
| 0Xfemale | -0.026 | -0.005 | -0.020 | 0.001 | 0.032 | 0.029 | 0.004 |
| | (0.016) | (0.005) | (0.016) | (0.022) | (0.021) | (0.054) | (0.018) |
| 2Xfemale | -0.031** | -0.004 | -0.027** | 0.019 | 0.022 | 0.015 | -0.003 |
| | (0.013) | (0.005) | (0.012) | (0.021) | (0.024) | (0.041) | (0.017) |
| 4Xfemale | -0.015 | -0.008 | -0.007 | 0.002 | 0.058** | 0.027 | 0.019 |
| | (0.017) | (0.006) | (0.019) | (0.018) | (0.024) | (0.048) | (0.027) |
| 6Xfemale | -0.015 | -0.014 | -0.000 | -0.001 | -0.025 | 0.039 | 0.042 |
| | (0.018) | (0.017) | (.) | (0.020) | (0.017) | (0.079) | (0.034) |
| Obs | 108174 | 108174 | 108174 | 3611 | 34036 | 3612 | 34038 |

Sources: VHLSS 2004-2014. Notes: All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

# F Sensitivity checks

## F.1 No anticipation check

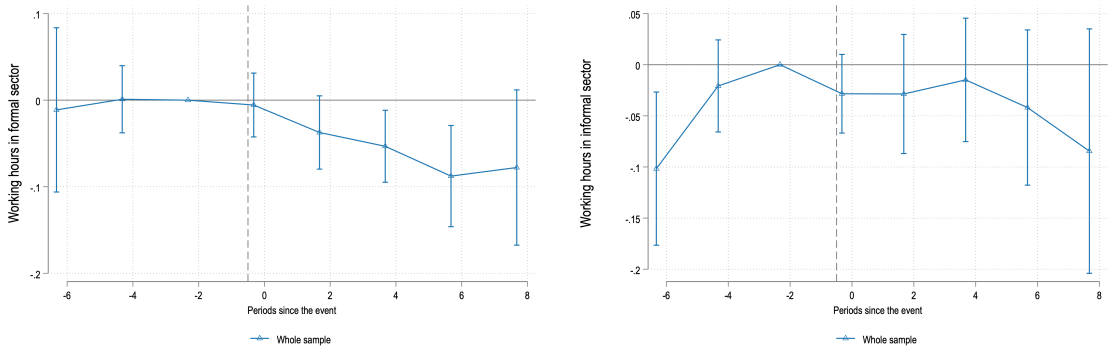



**(a)** Formal sector **(b)** Informal sector

.

**Figure F7:** Working hours

Source: VHLSS 2004-2014. Notes: The treatment year now is one wave earlier than the true treatment cohort (e.g., 2006 instead of 2008 treatment year, 2008 instead of 2010 treatment year, etc). The informal sector is defined by social security ownership. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, secondary job status, and urban/rural area.




**(a)** Formal sector **(b)** Informal sector

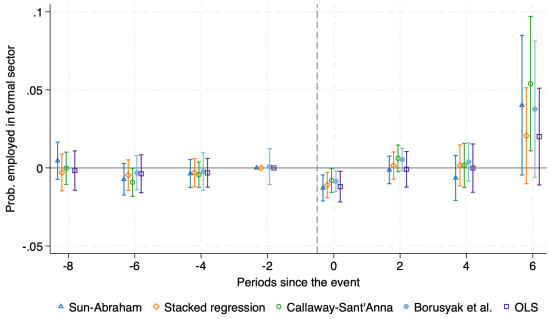
.

**Figure F8:** Working hours

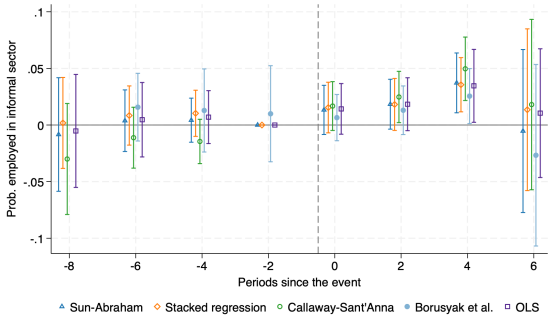
Source: VHLSS 2004-2014. Notes: The treatment year now is one wave earlier than the true treatment cohort (e.g., 2006 instead of 2008 treatment year, 2008 instead of 2010 treatment year, etc). The informal sector is defined by social security ownership. All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately for the whole sample. All regression models include "district" and "year" fixed effects and individual control variables: age, educational attainment, marital status, secondary job status, and urban/rural area.

## F.2 Alternative DID estimators

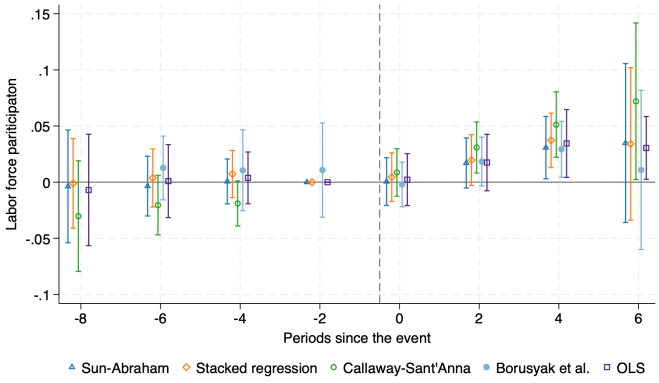
### Replication of baseline results



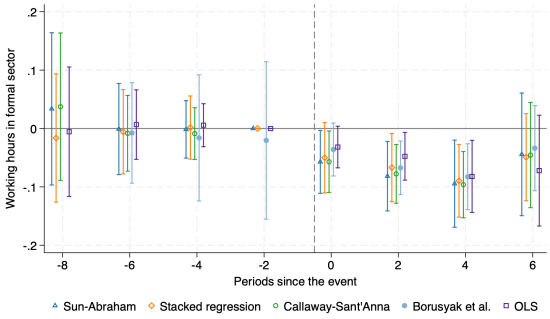

**(c)** Extensive margin in formal sector



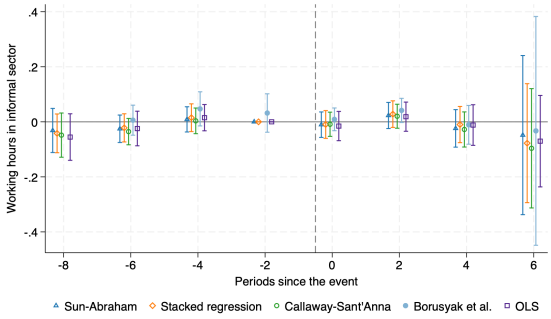

**(d)** Extensive margin in informal sector



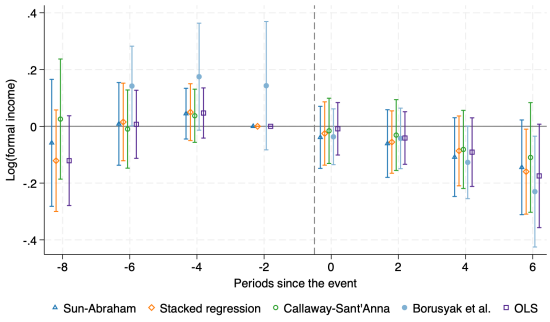

**(e)** total employment



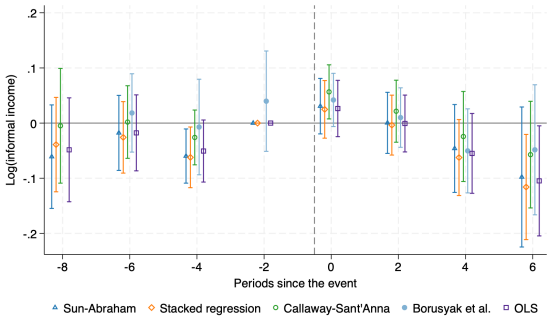

**(f)** Intensive margin in formal sector




**(g)** Intesive margin in informal sector




**(h)** Income effect in formal sector

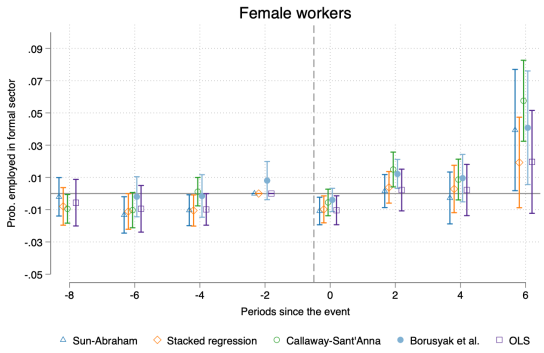



**(i)** Income effect in informal sector

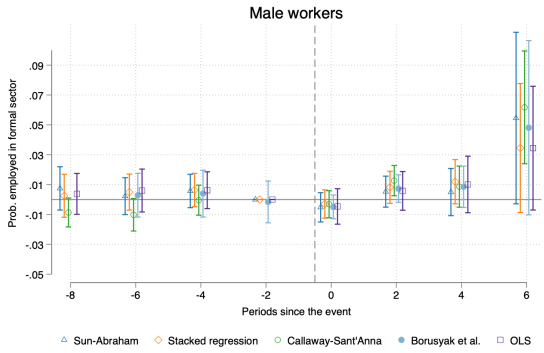
**Figure F8:** Alternative TWFE estimators

Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Robust clustered standard error are depicted in SA estimation, while Conley standard errors are in OLS. All regressions models include "district" and "year" fixed effects, and individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Source: VHLSS 2004-2014.

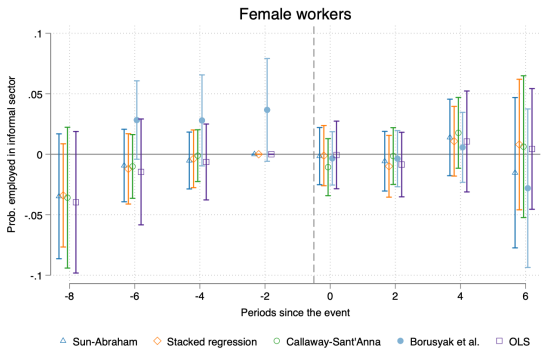
## Replication of results across genders



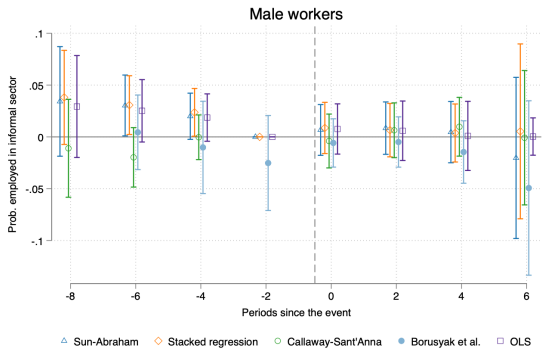

**(a)** Employment effect in formal sector



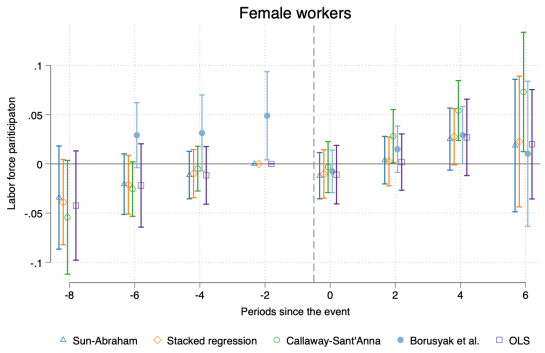

**(b)** Employment effect in formal sector



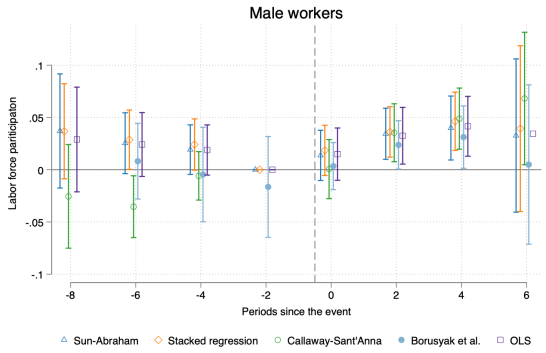

**(c)** Employment effect in informal sector




**(d)** Employment effect in informal sector

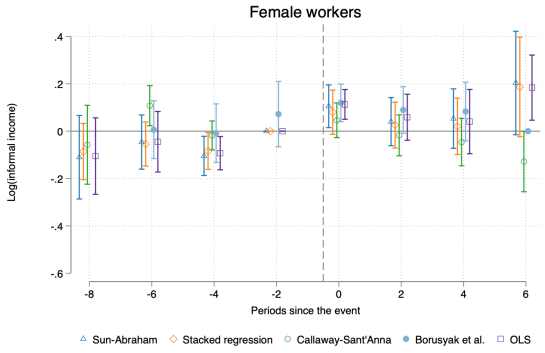



**(e)** total employment

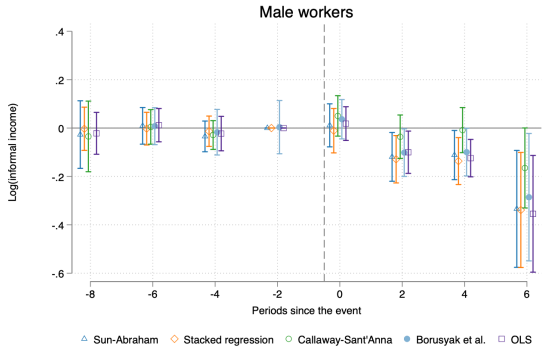



**(f)** total employment

**Figure F9:** Alternative estimators, by gender

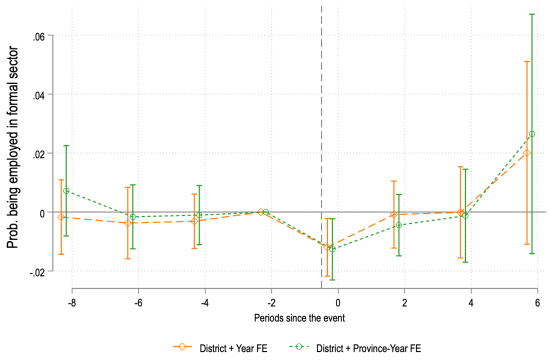



**(g)** Income effect in informal sector

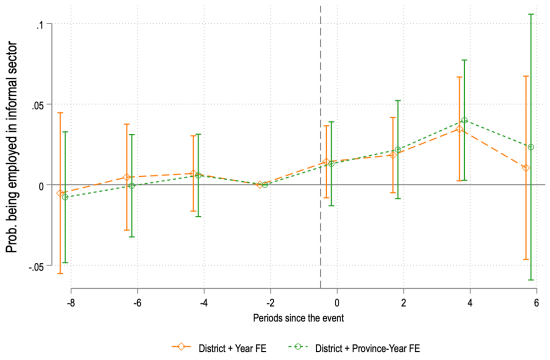



**(h)** Income effect in informal sector

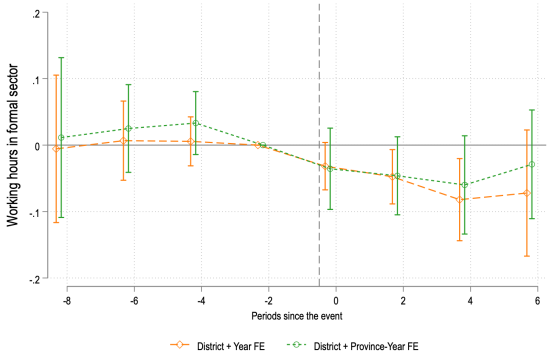
## F.3 Respective to FEs



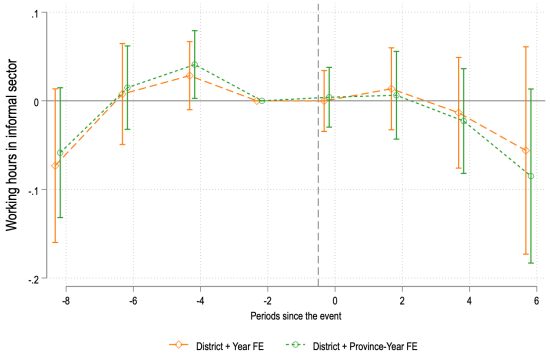

**(a)** Extensive margin in formal sector



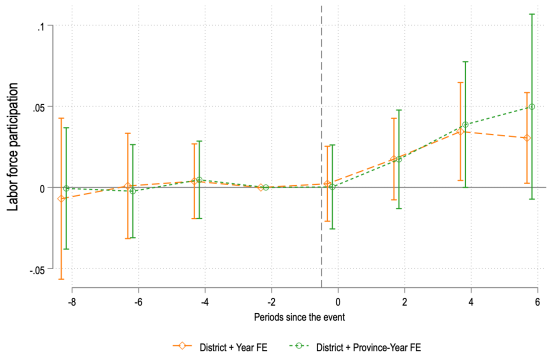

**(b)** Extensive margin in informal sector



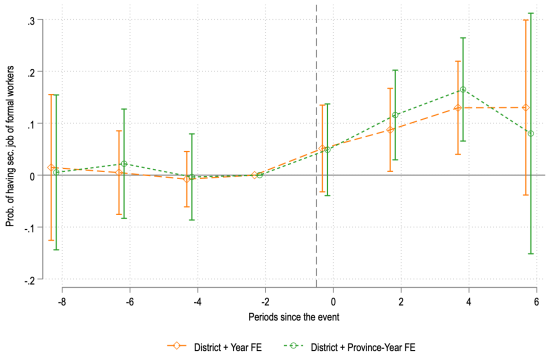

**(c)** Intensive margin in formal sector




**(d)** Intensive margin in informal sector



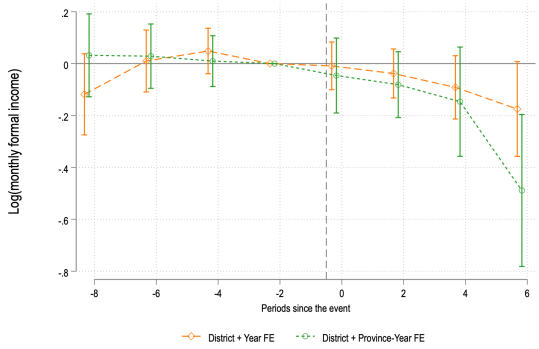

**(e)** total employment



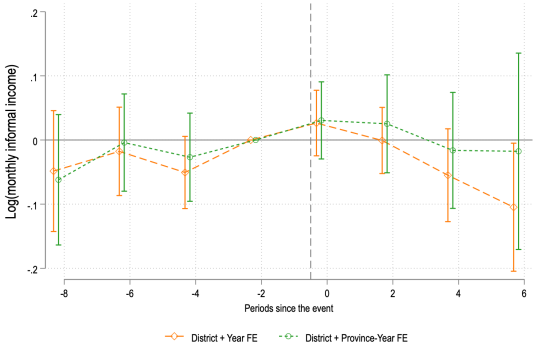

**(f)** Prob. of having secondary job of formal workers

**Figure F10:** Employment effects, respect to FEs

Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately varying respect to controls. All regressions models include"district" and"province x year" fixed effects, and individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Full regression results are shown in Appendix Table F18. Source: VHLSS 2004-2014 and GSO.




**(a)** Log(monthly formal income)




**(b)** Log(monthly informal income)

**Figure F11:** Income effect - robust respect to controls

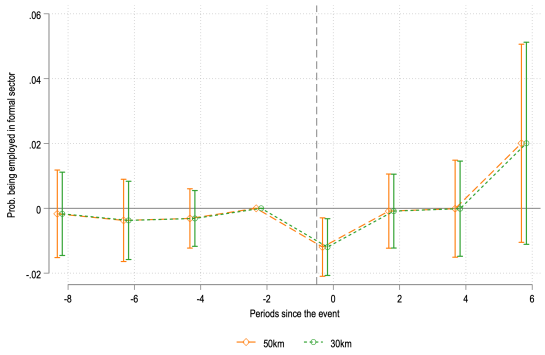
Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately varying respect to controls. All regressions models include"district" and"province x year" fixed effects, and individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Full regression results are shown in appendix Table F17. Source: VHLSS 2004-2014 and GSO.

**Table F8:** Income effect on individual w.r.t controls

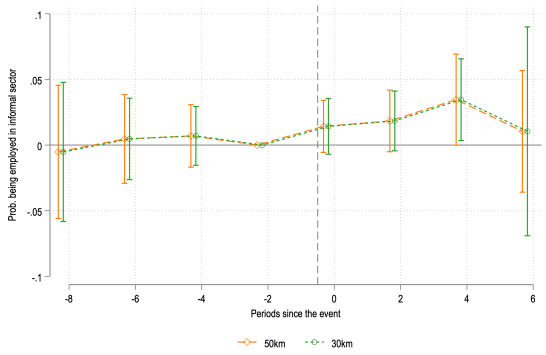
| | Log(monthly income) | | | | Log(hourly income) | | | | Log(total monthly income) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Formal sector | | Informal sector | | Formal sector | | Informal sector | | of formal workers | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| -8 | -0.118 | 0.032 | -0.048 | -0.062 | -0.125* | 0.001 | -0.045 | -0.060 | -0.226 | -0.535 |
| | (0.080) | (0.082) | (0.048) | (0.052) | (0.067) | (0.090) | (0.051) | (0.051) | (0.152) | (0.359) |
| -6 | 0.010 | 0.029 | -0.018 | -0.004 | 0.005 | 0.007 | -0.054 | -0.039 | -0.106 | -0.724** |
| | (0.061) | (0.063) | (0.035) | (0.039) | (0.056) | (0.065) | (0.039) | (0.041) | (0.199) | (0.297) |
| -4 | 0.048 | 0.010 | -0.051* | -0.027 | 0.038 | -0.020 | -0.080*** | -0.058* | 0.005 | -0.280 |
| | (0.045) | (0.050) | (0.029) | (0.035) | (0.046) | (0.058) | (0.029) | (0.032) | (0.123) | (0.208) |
| 0 | -0.009 | -0.046 | 0.026 | 0.031 | 0.018 | -0.015 | -0.001 | 0.000 | -0.128 | 0.178 |
| | (0.047) | (0.074) | (0.026) | (0.031) | (0.057) | (0.087) | (0.024) | (0.025) | (0.126) | (0.160) |
| 2 | -0.038 | -0.081 | -0.001 | 0.025 | 0.001 | -0.041 | -0.038 | -0.014 | 0.128 | -0.053 |
| | (0.048) | (0.065) | (0.026) | (0.039) | (0.050) | (0.068) | (0.030) | (0.029) | (0.150) | (0.281) |
| 4 | -0.092 | -0.147 | -0.055 | -0.016 | -0.028 | -0.092 | -0.123*** | -0.086*** | 0.082 | -0.068 |
| | (0.062) | (0.107) | (0.037) | (0.046) | (0.070) | (0.117) | (0.041) | (0.031) | (0.151) | (0.284) |
| 6 | -0.175* | -0.489*** | -0.105** | -0.018 | -0.043 | -0.381** | -0.142*** | -0.066 | 0.533*** | -0.618* |
| | (0.093) | (0.149) | (0.051) | (0.078) | (0.091) | (0.149) | (0.037) | (0.067) | (0.157) | (0.370) |
| District & Year FE | ✓ | | ✓ | | ✓ | | ✓ | | ✓ | |
| District & Province-Year FE | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ |
| Obs | 3450 | 3450 | 14185 | 14185 | 3450 | 3450 | 14185 | 14185 | 726 | 726 |

Sources: VHLSS 2004-2014. Notes: All baseline regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

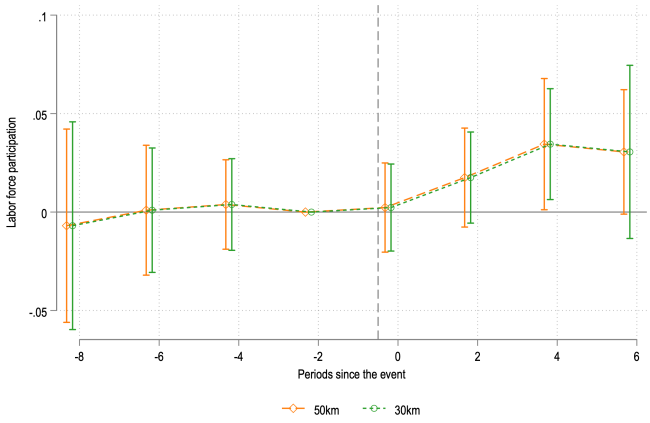
## F.4 Different bandwidths



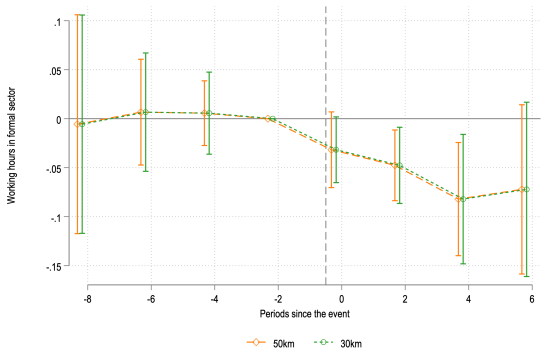

**(a)** Extensive margin in formal sector



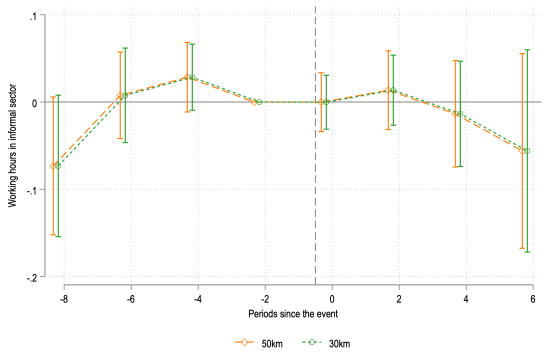

**(b)** Extensive margin in informal sector



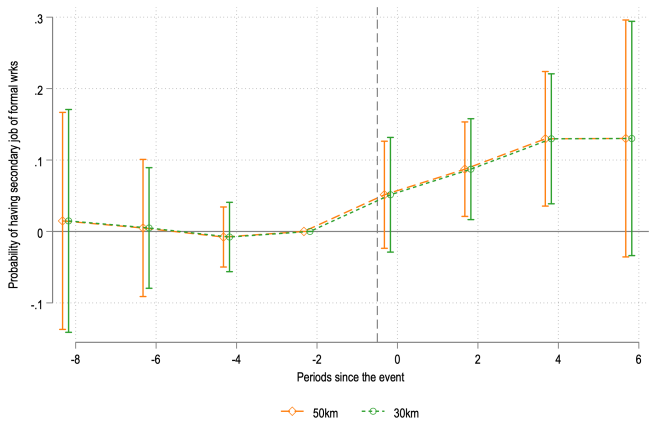

**(c)** total employment



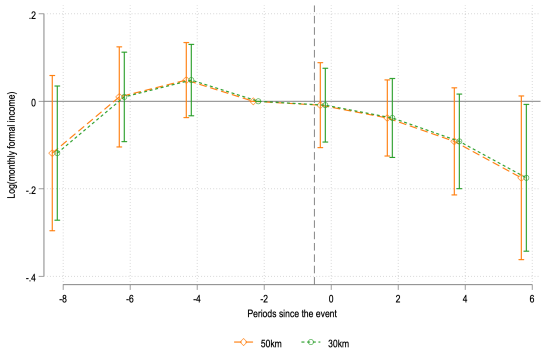

**(d)** Intensive margin in formal sector



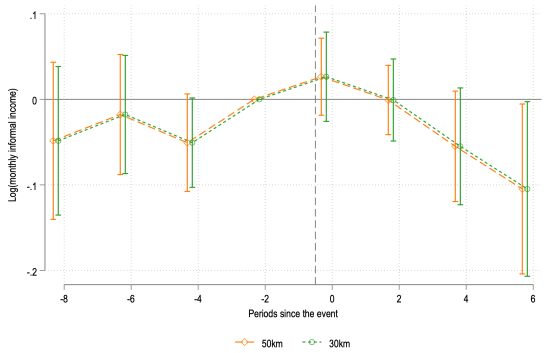

**(e)** Intesive margin in informal sector




**(f)** Probability of having secondary job of formal workers




**(g)** Income effect in formal sector




**(h)** Income effect in informal sector

**Figure F12:** Effects respective to different bandwidths

Notes: All curves depict event study estimates and the corresponding 95% confidence bands obtained by estimating equation (2). Conley standard errors are depicted. The figures present treatment effects separately varying respect to controls. All regressions models include "district" and "year" fixed effects, and individual control variables: age, educational attainment, marital status, holding a secondary job, and urban/rural area. Full regression results are shown in Appendix Table F9, F10. Source: VHLSS 2004-2014

**Table F9:** Income effect on individual - bandwidth = 30km

| | Log(monthly income) | | Log(hourly income) | | Log(total monthly income) |
|---|---|---|---|---|---|
| | Formal sector | Informal sector | Formal sector | Informal sector | of formal workers |
| | (1) | (2) | (3) | (4) | (5) |
| -8 | -0.118 | -0.048 | -0.125 | -0.045 | -0.226 |
| | (0.078) | (0.044) | (0.080) | (0.051) | (0.182) |
| -6 | 0.010 | -0.018 | 0.005 | -0.054 | -0.106 |
| | (0.052) | (0.035) | (0.045) | (0.037) | (0.183) |
| -4 | 0.048 | -0.051* | 0.038 | -0.080*** | 0.005 |
| | (0.042) | (0.027) | (0.046) | (0.026) | (0.128) |
| 0 | -0.009 | 0.026 | 0.018 | -0.001 | -0.128 |
| | (0.043) | (0.027) | (0.052) | (0.025) | (0.121) |
| 2 | -0.038 | -0.001 | 0.001 | -0.038 | 0.128 |
| | (0.046) | (0.024) | (0.049) | (0.030) | (0.166) |
| 4 | -0.092* | -0.055 | -0.028 | -0.123*** | 0.082 |
| | (0.055) | (0.035) | (0.067) | (0.040) | (0.141) |
| 6 | -0.175** | -0.105** | -0.043 | -0.142*** | 0.533* |
| | (0.086) | (0.052) | (0.087) | (0.039) | (0.272) |
| Obs | 3450 | 14185 | 3450 | 14185 | 726 |

Sources: VHLSS 2004-2014. Notes: I adopt a bandwidth 30km for Conley SE. "Total monthly income" is defined as the total income from both primary and secondary jobs of formal workers. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

**Table F10:** Employment effect on individual - bandwidth 30km

| | Labor force | Probability of being employed | | Working hours | | Probability of having secondary job | |
|---|---|---|---|---|---|---|---|
| | participation | Formal sector | Informal sector | Formal sector | Informal sector | Formal wrks | Informal wrks |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| -8 | -0.007 | -0.002 | -0.005 | -0.006 | -0.073* | 0.015 | 0.024 |
| | (0.027) | (0.007) | (0.027) | (0.057) | (0.041) | (0.080) | (0.032) |
| -6 | 0.001 | -0.004 | 0.005 | 0.007 | 0.008 | 0.005 | 0.008 |
| | (0.016) | (0.006) | (0.016) | (0.031) | (0.028) | (0.043) | (0.019) |
| -4 | 0.004 | -0.003 | 0.007 | 0.006 | 0.028 | -0.008 | 0.006 |
| | (0.012) | (0.004) | (0.011) | (0.021) | (0.019) | (0.025) | (0.016) |
| 0 | 0.002 | -0.012*** | 0.014 | -0.032* | -0.000 | 0.051 | 0.005 |
| | (0.011) | (0.004) | (0.011) | (0.017) | (0.016) | (0.041) | (0.016) |
| 2 | 0.018 | -0.001 | 0.018 | -0.048** | 0.014 | 0.087** | -0.024 |
| | (0.012) | (0.006) | (0.012) | (0.020) | (0.020) | (0.036) | (0.019) |
| 4 | 0.034** | -0.000 | 0.035** | -0.082** | -0.013 | 0.130*** | -0.025 |
| | (0.014) | (0.007) | (0.016) | (0.034) | (0.031) | (0.046) | (0.022) |
| 6 | 0.031 | 0.020 | 0.010 | -0.072 | -0.056 | 0.130 | -0.023 |
| | (0.022) | (0.016) | (0.041) | (0.045) | (0.059) | (0.084) | (0.031) |
| Obs | 108174 | 108174 | 108174 | 3611 | 34036 | 3612 | 34038 |

Sources: VHLSS 2004-2014. Notes: I adopt a bandwidth 30km for Conley SE. Column (4) and (5) indicate the working hours in formal/informal sector as the primary job of worker. Column (6) and (7) represents the probability of having a secondary job of formal/informal workers respectively. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

**Table F11:** Income effect on individual - bandwidth = 50km

| | Log(monthly income) | | Log(hourly income) | | Log(total monthly income) |
|---|---|---|---|---|---|
| | Formal sector | Informal sector | Formal sector | Informal sector | of formal workers |
| | (1) | (2) | (3) | (4) | (5) |
| -8 | -0.118 | -0.048 | -0.125 | -0.045 | -0.226 |
| | (0.091) | (0.047) | (0.078) | (0.052) | (0.149) |
| -6 | 0.010 | -0.018 | 0.005 | -0.054 | -0.106 |
| | (0.058) | (0.036) | (0.051) | (0.036) | (0.169) |
| -4 | 0.048 | -0.051* | 0.038 | -0.080*** | 0.005 |
| | (0.044) | (0.029) | (0.044) | (0.029) | (0.130) |
| 0 | -0.009 | 0.026 | 0.018 | -0.001 | -0.128 |
| | (0.050) | (0.023) | (0.062) | (0.019) | (0.121) |
| 2 | -0.038 | -0.001 | 0.001 | -0.038 | 0.128 |
| | (0.044) | (0.021) | (0.051) | (0.029) | (0.159) |
| 4 | -0.092 | -0.055* | -0.028 | -0.123*** | 0.082 |
| | (0.062) | (0.033) | (0.073) | (0.042) | (0.123) |
| 6 | -0.175* | -0.105** | -0.043 | -0.142*** | 0.533** |
| | (0.095) | (0.051) | (0.095) | (0.034) | (0.228) |
| Obs | 3450 | 14185 | 3450 | 14185 | 726 |

Sources: VHLSS 2004-2014. Notes: I adopt a bandwidth 50km for Conley SE. "Total monthly income" is defined as the total income from both primary and secondary jobs of formal workers. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

**Table F12:** Employment effect on individual - bandwidth 50km

| | Labor force | Probability of being employed | | Working hours | | Probability of having secondary job | |
|---|---|---|---|---|---|---|---|
| | participation | Formal sector | Informal sector | Formal sector | Informal sector | Formal wrks | Informal wrks |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| -8 | -0.007 | -0.002 | -0.005 | -0.006 | -0.073* | 0.015 | 0.024 |
| | (0.025) | (0.007) | (0.026) | (0.057) | (0.040) | (0.078) | (0.030) |
| -6 | 0.001 | -0.004 | 0.005 | 0.007 | 0.008 | 0.005 | 0.008 |
| | (0.017) | (0.006) | (0.017) | (0.028) | (0.025) | (0.049) | (0.020) |
| -4 | 0.004 | -0.003 | 0.007 | 0.006 | 0.028 | -0.008 | 0.006 |
| | (0.012) | (0.005) | (0.012) | (0.017) | (0.020) | (0.021) | (0.016) |
| 0 | 0.002 | -0.012*** | 0.014 | -0.032 | -0.000 | 0.051 | 0.005 |
| | (0.012) | (0.005) | (0.010) | (0.020) | (0.017) | (0.038) | (0.014) |
| 2 | 0.018 | -0.001 | 0.018 | -0.048*** | 0.014 | 0.087*** | -0.024 |
| | (0.013) | (0.006) | (0.012) | (0.018) | (0.023) | (0.034) | (0.019) |
| 4 | 0.034** | -0.000 | 0.035* | -0.082*** | -0.013 | 0.130*** | -0.025 |
| | (0.017) | (0.008) | (0.018) | (0.029) | (0.031) | (0.048) | (0.024) |
| 6 | 0.031* | 0.020 | 0.010 | -0.072 | -0.056 | 0.130 | -0.023 |
| | (0.016) | (0.016) | (0.024) | (0.044) | (0.057) | (0.085) | (0.026) |
| Obs | 108174 | 108174 | 108174 | 3611 | 34036 | 3612 | 34038 |

Sources: VHLSS 2004-2014. Notes: I adopt a bandwidth 50km for Conley SE. Column (4) and (5) indicate the working hours in formal/informal sector as the primary job of worker. Column (6) and (7) represents the probability of having a secondary job of formal/informal workers respectively. All regressions include "District" and "Year" fixed effects, and individual characteristics such as: age, educational attainment, marital status, having a secondary job or not, urban/rural area. Conley standard errors are reported in parentheses. Significant levels are * $p < .10$, ** $p < .05$, *** $p < .01$

# Chapter 2

# Racing to the top? Foreign firms and local markets: Evidence from Vietnam

## 2.1 Introduction

Foreign direct investment (FDI) is widely recognized as a key driver of economic growth and diversification, especially for developing economies integrating into global value chains. A large body of literature examines the role of multinational enterprises (MNEs), a proxy for globalization, in shaping host country outcomes. Much of this research focuses on their effects on domestic firm performance (Aitken & Harrison, 1999; Carluccio & Fally, 2013; Haskel et al., 2007; Javorcik et al., 2018; Kee, 2015), or labor market dynamics (Abebe et al., 2022; Alfaro-Urena et al., 2022; Carluccio & Fally, 2013; Poole, 2013; Setzler & Tintelnot, 2021). Given these wide-ranging effects, many governments actively seek to attract FDI through various policy tools.

The strategy for attracting foreign investment depends on a country's specific characteristics, including its level of development, labor market structure, and institutional capacity. But a common approach involves adjusting labor costs. In practice, however, governments traditionally impose higher standards, such as minimum wages (MW), specifically on foreign firms. This policy is based on the assumption that foreign firms have a greater financial

capacity to absorb these costs, thereby allowing host countries to raise labor standards while shifting part of the burden of higher labor costs onto foreign capital (Garetto et al., 2025). While this approach may help improve working conditions, it can also place foreign firms at a competitive disadvantage relative to domestic firms. At the same time, maintaining lower standards for domestic firms is intended to enhance their competitiveness by enabling them to undercut foreign firms in local markets. However, this strategy may have unintended consequences. By shielding domestic firms from competition or providing cost advantages, it reduces their incentives to upgrade. Domestic firms may be less likely to engage in technology adoption, skill development, or management improvements, which are key spillover channels through which FDI can benefit the local economy (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021).

This paper examines Vietnam's 2011 minimum wage unification policy, which eliminated the wage differential between foreign and domestic firms, thereby removing the unequal treatment of the FDI sector. I investigate how this reform affects employment in the foreign-invested sector, a key measure of FDI performance, as well as its potential spillover effects on domestic firms operating in the same labor markets.

Historically, Vietnam implemented a dual MWs system, where foreign invested enterprises were subject to a higher MW than their domestic counterparts. In some years, the MW in the foreign sector was nearly double that of the domestic sector. That discrepancy effectively conferred a labor cost advantage to domestic enterprises, potentially influencing competition between the two sectors. In late 2011, however, the central government announced a significant policy reform aimed at unifying the MW framework across sectors. The reform entails raising the minimum wage of domestic firms to align with that of the foreign sector, closing the wage gap between the two sectors. This change removed the labor cost advantage previously held by domestic firms, making foreign firms relatively more competitive. Given the significant role of FDI in Vietnam, averaging around 5% of GDP but peaking at approximately 10% in 2008-a notably high level among East Asian economies as shown in Figure B2-it is important to understand the impacts, if any, on the performance of foreign firms.

In this paper, I first examine how this shift in relative labor costs affected foreign firms' employment at the firm level. Using panel data from the Vietnamese Enterprise Census (VES), an annual survey conducted by Vietnam's General Statistics Office (GSO), I employ a difference-in-differences approach. Foreign firms are treated as the treatment group, given that the policy reform effectively granted them a new competitive advantage in terms of labor costs by eliminating the wage gap they previously faced. I compare outcomes such as employment before and after the reform between foreign firms and domestic firms. The analysis exploits within-firm variation over time and includes firm and year fixed effects, to control for time-invariant firm characteristics and common time trends, while focusing on within-firm employment adjustments. The results indicate a sizable relative increase in employment among foreign firms compared to domestic firms, with an estimated effect of approximately 28 percent. Event-study estimates show no evidence of differential pre-trends and suggest that these relative employment gains persist and strengthen over time. Importantly, these relative effects do not necessarily reflect absolute employment growth in foreign firms. To better understand the source of the estimated relative effects, I implement a simple specification that includes only indicators for foreign ownership, the post-reform period, and their interaction, without firm or year fixed effects. This specification shows that employment declines among domestic firms following the reform, while employment in foreign firms remains largely unchanged, indicating that the estimated relative effects are primarily driven by contraction in domestic employment rather than expansion in the foreign sector. The relative employment effects are similar in regions with high foreign direct investment (FDI) intensity and are more pronounced in the manufacturing sector, where the estimated differential amounts to approximately a 33 percent increase in employment.

I then investigate the heterogeneity based on whether foreign firms trade with domestic firms in the same region. There is a possibility that increased labor costs may be passing through to foreign firms via supply chain linkages. To examine this channel, I merge the VES with the Vietnam Technology and Competitiveness Survey (TCS), which provides detailed sourcing information of *manufacturing firms* from 2010 to 2018. I do not find evidence of significant

pass-through effects. A possible explanation is that the increase in labor costs is not large enough to have a detectable impact on foreign firms that are linked to domestic partners. I also analyze heterogeneity by pre-policy FDI firm characteristics. The results suggest that young, large, and more productive firms were more likely to expand employment in response to the policy.

The firm-level analysis documents the expansion of foreign firms' employment following the reform; however, this approach may not fully capture broader aggregate-level dynamics. Specially, the firm-level analysis overlooks potential spillover effects from foreign to domestic firms, particularly given district-level variations in foreign firm density and initial MW gaps. These initial gaps stem from Vietnam's multi-tier MW system, in place from 2008 to 2011, which assigned each district to a MW tier and set higher MWs for foreign than for domestic firms, resulting in district- and time-specific differences in MW levels between foreign and domestic sectors prior to the 2011 reform. Thus, I conduct a district-level analysis that leverages cross-district variations in the size of the MW gap and the pre-reform share of foreign firms.

The findings indicate that the MW gap seems not to have any signficant effect on overall district employment, whereas the share of foreign firms is positively associated with total employment. Event study estimates further indicate that this positive effect strengthens over time. A sectoral breakdown reveals that while the MW gap does not significantly affect domestic employment, a higher share of foreign firms is linked to increased domestic employment. In contrast, the share of foreign firms is negatively associated with foreign-sector employment four years after the reform. This does not necessarily imply that districts with a high share of foreign firms are experiencing a decline in foreign employment. Rather, it suggests that foreign employment growth in these districts is slower compared to districts with a lower share of foreign firms. This possibly reflects the catch-up channel where the low-foreign-firms districts still have more room to expand both in terms of labor supply and infrastructure development.

The absence of a significant MW gap effect on foreign employment may stem from the

relatively small employment share of foreign firms, which averaged just 9% of total district employment before 2011 (Figure E19). Overall, while MW standardization appears to have limited effects, the presence of foreign firms contributes positively to local labor markets, likely through mechanisms such as technology spillovers, knowledge diffusion, and market reallocation (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021).

This paper contributes to three strands of literature. First, it adds to the literature on FDI policy flexibility, particularly the view advocated by UNCTAD (2003) and Faundez (2004), which emphasizes that host countries should tailor their FDI-related labor market policies to align with their development objectives. The shift in Vietnam's minimum wage system reflects how labor market regulation changes can influence FDI dynamics. As Parcon (2008) argues, labor regulations may reduce FDI inflows through cost channels but can simultaneously enhance productivity, thereby attracting higher-quality investment. This is consistent with institutional theory, which posits that institutional strength plays a crucial role in drawing FDI to emerging markets (Lucas, 1993). By harmonizing wage standards across foreign and domestic sectors, the Vietnamese government signaled institutional maturity and policy coherence, characteristics that appeal to technology-intensive and long-term investors who value regulatory stability alongside infrastructure and labor quality, as shown in the case of China (Hou et al., 2021). In the Vietnamese case, the reform may reflect a strategic shift from relying solely on low labor costs to a more balanced and sustainable environment for attracting high-quality FDI.

Additionally, this paper also adds to the debate on whether countries should set differentiated minimum wage floors by industry, occupation, or skill level. In the case of Vietnam, the difference in minimum wage systems for foreign and domestic sectors was eliminated by raising the minimum wage for domestic firms. This shift reflects an effort to align domestic MWs with those applicable to MNEs, thereby removing the competitive disadvantage previously faced by MNEs. As highlighted by the ILO (2017), such complex wage-setting systems require periodic revision to keep pace with changes in industry structures, occupational

categories, and local labor market conditions, like in the Vietnam setting. Differentiated minimum wage settings initially served as a policy tool to protect the domestic sector. However, since the MW also serves as a proxy for labor standards, maintaining a dual-wage system can become increasingly misaligned with international norms. As a result, the policy was revised to reflect rising labor standards and broader global expectations. Thus while differentiated wage structures may be effective in the early stages of attracting FDI, retaining such systems without adjustment risks undermining an alignment with international labor standards.

Second, this study adds to the literature on the impact of foreign firms and MNEs on labor standards in host countries. Existing literature documents how foreign firms may generate spillover effects that raise labor standards among domestic firms. Furthermore, the previous studies find that trading with MNEs can elevate labor standard among domestic partners, such as improved working conditions in export-oriented manufacturing (Tanaka, 2020) and reductions in the incidence of child labor (Edmonds & Pavcnik, 2005). On the other hand, Im and McLaren (2023) find the *ambiguous* effects of international trade on collective bargaining rights. Nevertheless, both theoretical and empirical studies primarily focus on the determinants of labor standard adoption by domestic firms. Im and McLaren (2023) ultimately concludes that larger countries are more likely to raise labor standard due to their greater capacity to absorb the associated costs compared to smaller countries. This paper offers a new perspective by examining a context in which the government actively intervenes, mandating labor standards for domestic firms to match those already imposed on foreign firms. In doing so, the study contributes to the literature on state intervention in shaping institutional environments and enforcing local practices, particularly when these practices reflect global norms (Guillén, 2000). Furthermore, this paper extends the analysis to investigate the impact on foreign firms themselves, an area that has received relatively little attention in the existing literature.

Additionally, this paper engages with the literature on the spillover effects of FDI in host countries. The expansion of domestic employment in regions with a high concentration of foreign firms aligns with previous studies finding that FDI can benefit host economies through

knowledge transfer, technological diffusion, and market reallocation (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021).

Lastly, this paper contributes to the minimum wage literature in Vietnam. Previous studies have mainly examined the effects of either the national or regional MW on various outcomes. C. V. Nguyen (2013) and Del Carpio et al. (2013) document adverse employment effects among low-wage and formal workers, exploiting variations in the national and regional MWs, respectively. Other studies, such as Hansen et al. (2016) and Sakellariou and Fang (2014), highlight a decline in wage inequality in the private sector, but not in the informal or state sectors. To the best of my knowledge, this is the first study to investigate the unification of the MW policy and its effects on the Vietnamese labor market. The findings suggest that this policy positively influences employment growth in both the foreign and domestic private sectors, particularly in regions characterized by high levels of FDI intensity.

The rest of the paper proceeds as follows. Section 2.2 and 2.3 describe the unified MW policy and data and descriptive evidence. Section 2.4 presents the analysis at the firm level. Section 2.5 documents the analysis at the district level. Section 2.6 concludes the paper.

## 2.2 The unified minimum wage policy

The minimum wage for the foreign investment sector was first introduced in the late 1990s, following the adoption of the 1987 Law on Foreign Investment. Initially, the wage was set at $50 per month. In 1992, the system was revised to introduce four regional wage levels for the foreign sector, ranging from $30 to $35 per month, depending on the region. At that time, however, there was no minimum wage in place for the domestic sector. It was not until 1993 that a *base* minimum wage was established for both the state sector and the private domestic sector.

This structure remained largely unchanged until 2006, when the government first introduced regionalized minimum wages for the private domestic sector. The purpose of this policy was, and is, to account for differences in living costs and labor market conditions across districts.

The central government leads the process of establishing regional minimum wages, coordinating closely with the Ministry of Labor, Invalids, and Social Affairs; the Vietnam General Confederation of Labor (representing employees); and the Vietnam Chamber of Commerce and Industry (representing employers). Initially, in 2008, there were three different minimum wage levels to which districts were assigned. After 2008, the central government reclassified these into four levels, and this system has remained unchanged to the present day. It is important to note that the regional minimum wage for the private domestic sector is distinct from and consistently higher than the base minimum wage applied to the state sector.

Prior to 2012, both the foreign and domestic sectors operated under separate regionalized minimum wage schedules. While both systems used regional differentiation, the foreign sector minimum wages were typically higher than those for the domestic sector. Consequently, the wage gap between the two sectors varied by region and over time, depending on how adjustments were made within each sector and region. For example, Table A1 shows that in 2006, the lowest minimum wage in the foreign sector was twice that of the domestic sector. Figure 2.1 further illustrates that while this inter-sectoral gap remained relatively stable from 2006 to 2011, it was not until late 2011 that the government implemented a unification policy, standardizing minimum wages across both sectors.

This unification meant that, beginning in 2012, the same minimum wage rates applied to both domestic and foreign firms within each region, thereby eliminating the long-standing wage gap. This policy shift is widely seen as a response to Vietnam's accession to the World Trade Organization (WTO) in 2007 (Thành et al., 2017).

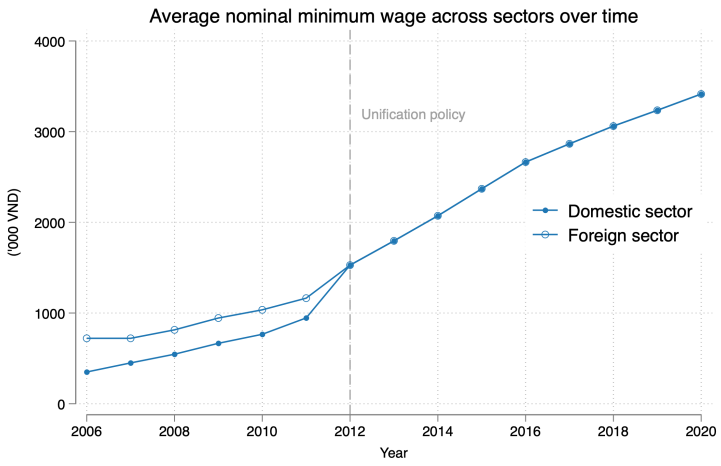



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**Figure 2.1:** Average nominal minimum wage across sectors over time

Source: GSO. Notes: The average minimum wage is averaged out over levels for domestic and foreign sectors. More details of minimum wage levels are found in Table A1

## 2.3 Data and descriptive statistics

### 2.3.1 Data

**Firm data** This paper utilizes data from the Vietnamese Enterprise Census (hereafter VES) for the period 2006–2019. The VES has been conducted annually by Vietnam's General Statistics Office (hereafter GSO) since 2000. The census covers all registered formal enterprises in Vietnam, providing comprehensive information on firm demographics, ownership, employment, capital stock, turnover, export-import activities and production. The survey includes only registered enterprises; therefore, informal firms without registration would not be included here. To construct the panel firm-level data, I combine the geolocation and tax code of each firm to have a unique identifier across years. Overall, the unbalanced panel consists of approximately 98,500 firms during the 2006-2019 period.

Based on survey data, I define a foreign firm as any enterprise with a positive share of foreign ownership. State-owned enterprises (SOEs) are defined as firms in which the state holds more than 50% of the capital. Private domestic firms comprise firms with less than 50% or no state

capital, including private enterprises and partnerships. In the baseline analysis, I classify both SOEs and private domestic firms under the broader category of domestic firms.[1]

By that definition, foreign firms account for approximately 24% of total employment in the market, with fully foreign-owned firms comprising up to 84% of that share, as shown in Table 2.1. The share of foreign firm employment is more pronounced in the manufacturing sector, representing approximately 43% of total manufacturing employment. Meanwhile, the majority of employment, which is around 70%, is concentrated in private domestic firms.

**Table 2.1:** Share of firm types before 2012

| | Employment | | Firm counts | |
|---|---|---|---|---|
| | Full Sample | Manufacturing | Full Sample | Manufacturing |
| N | 40,486,456 | 20,974,249 | 1,212,741 | 228,917 |
| Foreign firms | 0.237 | 0.427 | 0.029 | 0.106 |
| 100% ownership | 0.840 | 0.860 | 0.827 | 0.889 |
| State-owned firms | 0.063 | 0.027 | 0.007 | 0.006 |
| Private domestic firms | 0.694 | 0.543 | 0.960 | 0.885 |

Source: VES 2006–2011. Notes: Foreign direct investment firms (FDIs) include those with positive foreign capital. The state-owned firms (SOEs) include a wide range of state capital shares, ranging from 50% to 100%.

I also match the VES data with the Vietnam Technology and Competitiveness Survey (TCS) from 2010 to 2018. The TCS covers a subset of manufacturing firms included in the VES and provides detailed supply chain information, particularly regarding firms' sourcing activities. Specifically, the survey asks firms to report the share of raw materials and intermediate inputs sourced from suppliers within the same province versus those from different provinces. This firm-level sourcing data is unique, as most existing studies typically rely on input-output (IO) tables to infer industry linkages. However, IO tables capture relationships at the industry level and may not accurately reflect actual firm-level transactions or geographic sourcing patterns. By using the TCS, I can observe direct firm-to-supplier sourcing behavior, offering a more precise and granular view of supply chain linkage.

**Other Data** To assess whether foreign firms perform differently in regions with higher levels of foreign direct investment (FDI), I merge firm-level data with provincial FDI statistics

[1]As a robustness check, I exclude SOEs from the domestic firm category.

from the GSO, including both the total number of registered foreign projects and the total amount of registered foreign capital, reported as both annual and cumulative figures. Figure B7 illustrates that FDI is distributed across the country, with certain regions exhibiting a higher concentration of foreign investment.

### 2.3.2 Descriptive statistics

Table 2.2 presents the descriptive statistics of foreign and domestic firms before 2012, based on the VES dataset. The statistics include total year-end employment, firm age, size, gender composition, and value-added per worker. The data show that foreign firms are generally much larger and slightly older, despite accounting for a relatively small share of the labor market. In contrast, domestic firms tend to be smaller, with a significant proportion employing fewer than 20 workers. Specifically, the majority of domestic firms are small enterprises, with approximately 78% of firms having fewer than 20 employees, and only about 5% having more than 100 employees. In contrast, foreign firms are more likely to be medium or large-sized enterprises, with 34% employing between 20 and 100 workers, and 42% employing more than 100 workers.

Figure 2.2 further illustrates this pattern, showing that the share of foreign firms increases along the employment distribution, both in the full sample and within the manufacturing sector. The gap between foreign and domestic firms appears to be narrowing slightly in the manufacturing sector.

**Table 2.2:** Descriptive statistics of firms before 2012

| | Full sample | | |
|---|---|---|---|
| | Domestic firms | Foreign firms | p-value |
| N | 1,177,310 (97.1%) | 35,432 (2.9%) | |
| Total employment - end of year | 26.252 (111.097) | 270.742 (747.332) | <0.001 |
| Age | 4.311 (5.441) | 6.049 (5.018) | <0.001 |
| **Size** | | | |
| < 20 employees | 0.783 (0.412) | 0.235 (0.424) | <0.001 |
| 20-100 employees | 0.171 (0.376) | 0.344 (0.475) | <0.001 |
| > 100 employees | 0.047 (0.211) | 0.422 (0.494) | <0.001 |
| Share of female workers | 0.359 (0.199) | 0.485 (0.252) | <0.001 |
| Log labor productivity (VA per worker) | 8.811 (0.773) | 9.526 (1.202) | <0.001 |
| Capital intensity | 0.216 (0.412) | 0.484 (0.500) | <0.001 |
| **Industry** | | | |
| Manufacturing | 0.174 (0.379) | 0.687 (0.464) | <0.001 |
| Services | 0.656 (0.475) | 0.255 (0.436) | <0.001 |
| Others | 0.171 (0.376) | 0.058 (0.234) | <0.001 |
| | Manufacturing | | |
| N | 204,587 (89.4%) | 24,330 (10.6%) | |
| Total employment - end of year | 58.787 (203.557) | 368.432 (881.896) | <0.001 |
| Age | 5.211 (6.707) | 6.282 (4.730) | <0.001 |
| **Size** | | | |
| < 20 employees | 0.611 (0.487) | 0.123 (0.328) | <0.001 |
| 20-100 employees | 0.271 (0.444) | 0.334 (0.472) | <0.001 |
| > 100 employees | 0.118 (0.323) | 0.543 (0.498) | <0.001 |
| Share of female workers | 0.366 (0.223) | 0.509 (0.262) | <0.001 |
| Log labor productivity (VA per worker) | 8.748 (0.749) | 9.287 (1.104) | <0.001 |
| Capital intensity | 0.470 (0.499) | 0.544 (0.498) | <0.001 |

Source: VES 2006-2011. Notes: Foreign firms (FDIs) include those with any level of foreign ownership. Domestic firms include both state-owned firms (SOEs) and private formal firms. Total employment is the value recorded at the end of the year. VA stands for value added, calculated as the sum of a firm's profit EBITDA and total salaries paid to employees. Capital intensity is calculated as ratio of capital to the number of employees at the end of the year. Mean(SE): p-value from a pooled t-test. Sign. levels are: *p < 0.1, **p < 0.05, ***p < 0.01

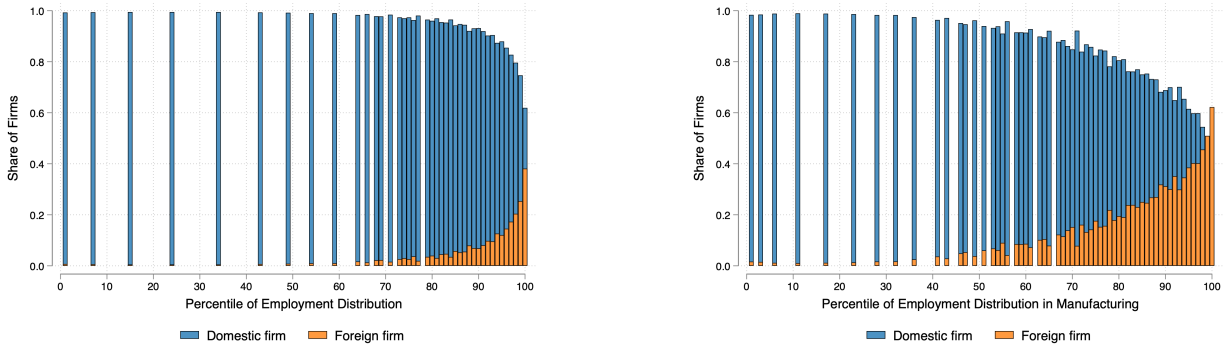



**Figure 2.2:** Share of firms in each percentile of employment distribution

Source: VES 2006-2019.

## 2.4 Firm-level effects

### 2.4.1 Empirical strategy and identification

Before 2011, the higher minimum wage in the foreign sector put foreign firms at a comparative disadvantage due to higher labor costs. In contrast, the lower minimum wage in the domestic sector gave domestic firms a cost advantage, making them more competitive. The standardization policy in late 2011 eliminated the disadvantages on foreign firms and made foreign firms more competitive with respect to their labor costs. To analyze how this policy change affected labor demand, I use a difference-in-differences (DiD) approach. This method allows me to compare firm-level outcomes before and after the policy change, between foreign firms (treatment group) and domestic firms (control group). While the domestic sector was directly affected by the policy, I treat the foreign sector as the treatment group to examine how its relative outcomes changed once domestic sector's wage advantage disappeared. This approach allows me to assess the impact of the policy on the relative performance of the foreign sector compared to the domestic sector. The control group in my analysis consists of domestic entreprises, both (fully or partially) state-owned enterprises and private enterprises, while FDI enterprises can be considered "treated" in terms of the removal of a competitive disadvantage.

This firm-level approach using firm panel data allows me to exclude the potential effects on firm entry or exit (e.g., extensive margin). Formally, I estimate the following specification:

$$Y_{idt} = \alpha + \beta \times (foreignfirm_{idt} \times Post_t) + \sum_s \gamma_s \times (Year_s \times foreignMW_{dt}) + \delta_i + \gamma_t + \epsilon_{idt} \quad (2.1)$$

where $Y_{idt}$ denotes firm-level outcomes of firm $i$ (e.g., the natural logarithm of the number of employees at the end of the year, number of female/male workers hired at the end of the year) located in district $d$ in year $t$. Meanwhile, $foreignfirm_{idt}$ indicates whether firm $i$ is in the foreign sector, and $Post_t$ is a dummy variable equal to 1 if year $t$ is after 2011, i.e. after the reform that equated the minimum wage in the foreign and domestic sector.

Consequently, coefficient $\beta$ captures the effects of the unification policy on foreign firms before and after. Firm and year fixed effects, denoted by $\delta_i$ and $\gamma_t$, control for time-invariant firm characteristics and country-wide time trends. Additionally, in robustness checks, I include $industry \times year$ and $region \times year$ fixed effects to account for sector-specific and regional time trends, reducing potential biases from industry- or location-specific shocks[2]. The error term $\epsilon_{idt}$ accounts for unobserved factors. In the model, I cluster standard errors at the district level to capture within-district correlation. Since there are four MW levels assigned to different districts and these levels change over time within districts, the interaction term $Year_s \times foreignMW_{dt}$ allows the effect of the foreign minimum wage to vary across years, capturing dynamic responses.

I also estimate dynamic treatment effecs in the following event-study specification:

$$\begin{aligned} Y_{idt} = \alpha + \sum_{k \neq 2011} \beta_k \times (foreignfirm_{idt} \times Year_k) \\ + \sum_s \gamma_s \times (Year_s \times foreignMW_{dt}) + \delta_i + \gamma_t + \epsilon_{idt} \end{aligned} \tag{2.2}$$

where 2011 is the reference year, and $Year_k$ is the year indicator. The specification has the same set of controls and fixed effects. The identification strategy assumes that in the absence of the unification policy, the outcomes of foreign and domestic firms would follow parallel trends over time. Hence, the estimates of the $\beta_k$ parameters over the pre-reform period allow me to test this parallel trend assumption in that period before the implementation of the policy.

### 2.4.2 Results

To examine the employment effects of the minimum wage unification reform, I use the natural logarithm of total employment at the firm level as the outcome variable.[3] The estimation

[2]There are six regions in Vietnam: Central Highlands, Mekong River Delta, North Central, Northern Midland, Red River Delta and Southest, as shown in Figure B8(c) in the Appendix.

[3]I winsorize key variables, including employment, capital stock, value added, and labor compensation, at the 99% level to reduce the influence of outliers and obtain a cleaner sample.

results from equation 2.1 are presented in Table 2.3. Column (1) reports estimates from a specification without fixed effects, capturing the raw difference in employment changes between foreign and domestic firms following the reform. Columns (2) onward progressively introduce firm, year, industry–year, and region–year fixed effects to control for time-invariant firm characteristics, common macroeconomic shocks, and sectoral and regional dynamics.

Across specifications, the estimates indicate a sizable relative increase in employment among foreign firms compared to domestic firms following the reform, with an effect of approximately 28 percent. This effect reflects differential employment adjustments across ownership types rather than absolute employment growth in foreign firms. The estimated coefficient becomes slightly larger when industry–year and region–year fixed effects are included, suggesting that controlling for sector-specific and region-specific shocks strengthens the relative employment difference between foreign and domestic firms.

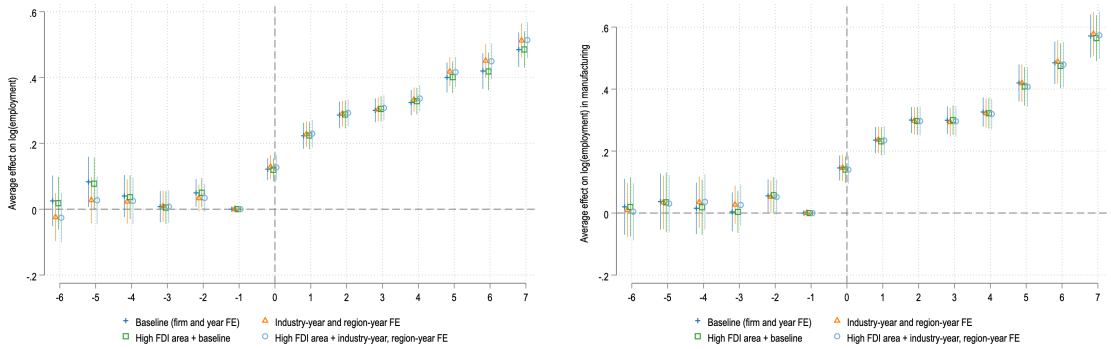
**Table 2.3:** Effects on total employment of firm

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | High FDI region | | | |
| Log(employment) | | | | Manufacturing | | | | | Manufacturing |
| FDI firm x post | -0.040 | 0.247*** | 0.274*** | 0.284*** | 0.275*** | 0.250*** | 0.275*** | 0.279*** | 0.273*** |
| | (0.058) | (0.020) | (0.019) | (0.026) | (0.025) | (0.021) | (0.020) | (0.027) | (0.026) |
| Firm FE | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | | ✓ | | ✓ | | ✓ | | ✓ |
| Region-year FE | | | ✓ | | ✓ | | ✓ | | ✓ |
| Obs | 4366836 | 4151110 | 4151110 | 759726 | 759726 | 3546750 | 3546750 | 657693 | 657693 |
| Dep var pre-treat mean | 2.34 | 2.34 | 2.34 | 3.09 | 3.09 | 2.32 | 2.32 | 3.06 | 3.06 |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The event study in Figure 2.3 illustrates the dynamic treatment effects as captured by equation 2.2 on the logarithm of the number of employees. The graph on the left presents the baseline estimates, which show a statistically significant effect on employment in foreign firms following the policy implementation. The effects are large in magnitude and persist over time. Moreover, the pre-policy treatment effects are mostly close to zero, supporting the parallel trends assumption underlying the identification strategy. The results are still

robust across different sets of fixed effects, for example when additionally controlling for industry x year and region x year fixed effects, as column (2) in Table 2.3 shows.




**(a)** Baseline **(b)** Manufacturing

**Figure 2.3:** Effects on firm's employment

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

To explore whether the effects of foreign ownership vary by local economic condition, I restrict the analysis to regions with a high concentration of FDI. These regions are possibly characterized by more developed industrial infrastructure, greater exposure to global markets, and a greater presence of multinational firms. These regions are identified as those in which the accumulated FDI in 2011 exceeds the national median level. The treatment effects presented in columns (5) and (6) of Table 2.3 are the same magnitude in effects with the baseline specification. Furthermore, the event study depicted in Figure 2.3 is also consistent with these findings.

**Manufacturing sector** The manufacturing sector has always played a pivotal role in Vietnam's economic growth, contributing a significant one-fifth to GDP[4]. Moreover, it is particularly notable for attracting a major share of foreign direct investment, especially in industries such as electronics, textiles, and machinery. As shown in Table B3, the manufacturing industry accounted for 48% of total registered capital as of 2011. Furthermore, Table 2.2 highlights the manufacturing sector's prominence among foreign firms, with approximately

[4]Figure B3 shows the share of manufacturing as % of GDP.

70% of these firms operating within manufacturing. Thus, it is particularly valuable and insightful to investigate how foreign manufacturing firms respond to the implementation of the equalization policy compared to domestic firms.

Column (3) in Table 2.3 presents the estimated effect of the policy on foreign manufacturing firms. The coefficient is slightly larger than the baseline estimates, corresponding to an approximate 33% increase in employment. Figure 2.3(b) further illustrates this pattern, showing a steady and gradual increase in employment among foreign manufacturing firms after the unification policy. Column (7) and (8) show that there is not much difference in the estimates in the regression focusing on high FDI regions.

Overall, the findings indicate that the minimum wage unification policy had a significant positive impact on employment growth among foreign firms. A plausible explanation is that the policy, by increasing the minimum wage for domestic firms to match the higher wage levels already mandated for foreign firms, effectively reduced the relative labor cost disadvantage previously faced by foreign enterprises. Simply put, the unification policy made their labor costs relatively less expensive compared to domestic firms than before. Consequently, this reduction in relative labor costs allowed foreign firms to expand employment following the policy reform.

Broadly, this result provides new insight into how rising labor standards can influence the behavior of foreign firms themselves. While much of the existing literature focuses on how MNEs may induce higher labor standards in domestic firms, through trade linkages or competitive pressures, less attention has been paid to how foreign firms respond when institutional changes raise the overall regulatory floor. In this case, by mandating uniform labor standards across firm ownership types, the government removes a long-standing asymmetry that had placed foreign firms at a relative cost disadvantage. The findings suggest that the standardization of labor regulations enhances the relative competitiveness of foreign firms, facilitating their expansion following the reform.

### 2.4.3 Heterogeneity analysis

#### 2.4.3.1 Gender heterogeneity

In this section, I examine firm-level employment outcomes by gender. The outcome variables are the number of female and male employees reported at the end of each year. Table 2.4 presents estimation results based on equation 2.1. Panel A shows the treatment effects on female workers, while Panel B reports the effects on male workers. Column (1) indicates that foreign firms increased their employment of female workers by approximately 15% relative to domestic firms. Male employment in foreign firms also experienced a significant increase, about 29% higher compared to domestic firms. The absolute magnitude of the treatment effect is correspondingly larger for males, which could be due to the already higher employment levels of males at the baseline. The p-value from an F-test comparing the interaction terms across the female and male regressions indicates that the difference in coefficients is statistically significant.

The effects remain consistent when focusing on regions with a high concentration of FDI, as shown in column (5), and are slightly stronger within the manufacturing sector, as shown in column (3). After controlling for industry-year and region-year fixed effects, the estimated impact on female employment increases further to around 18–19%, while no significant change is observed for male employment. This potentially reflects substantial variation in female employment shares across sectors, which may lead to compositional bias if only firm and year fixed effects are included. The effect on female employment increases significantly within manufacturing. This may be driven by female-intensive industries, such as textiles, fur, and footwear, which together account for around 20% of manufacturing employment, as shown in Figure B5. In these industries, women constitute over 70% of the workforce, as illustrated in Figure B6.

The generally stronger effects on male employment may be attributed to persistent gender discrimination in the labor market, particularly regarding maternity leave, family responsibilities, and other gendered expectations. Although female employment gains in the manu-

facturing sector are notable, they do not fully close the gender gap in treatment effects. This is partly because other male-dominated manufacturing industries, such as metal, electronic, chemical or plastic products (i.e., account for up to 35% of manufacturing), tend to prefer male workers, possibly due to physical labor requirements, longer shifts, or technical job demands. Figure 2.4 presents the dynamic treatment effects of the policy on employment,

**Table 2.4:** Effects on total employment of firm by gender

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A: Female** | | | Manufacturing | | High FDI region | |
| FDI firm x post | 0.147*** | 0.187*** | 0.175*** | 0.181*** | 0.150*** | 0.186*** |
| | (0.020) | (0.018) | (0.026) | (0.024) | (0.021) | (0.019) |
| Obs | 4151042 | 4151042 | 759591 | 759591 | 3546713 | 3546713 |
| Dep var pre-treat mean | 1.19 | 1.19 | 1.94 | 1.94 | 1.19 | 1.19 |
| **Panel B : Male** | | | | | | |
| FDI firm x post | 0.290*** | 0.295*** | 0.342*** | 0.315*** | 0.290*** | 0.294*** |
| | (0.020) | (0.020) | (0.026) | (0.025) | (0.021) | (0.020) |
| Obs | 4150848 | 4150848 | 759501 | 759501 | 3546536 | 3546536 |
| Dep var pre-treat mean | 1.84 | 1.84 | 2.50 | 2.50 | 1.81 | 1.81 |
| p-value of F-test | 0.028 | 0.000 | 0.028 | 0.000 | 0 .165 | 0.000 |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | ✓ | | ✓ | | ✓ |
| Region-year FE | | ✓ | | ✓ | | ✓ |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes, separately for female and male employment. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. The p-value of the F-statistic corresponds to a test of equality between the interaction terms from the female and male regressions. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

disaggregated by gender. The estimates show statistically significant and positive impacts on both male and female employment following the minimum wage unification policy. To investigate whether differences in the treatment effects between male and female workers are driven by composition effects (i.e., differences in firm types or industry composition), I further analyze the impacts separately for each gender across different industries and firm types. Table C5 in the Appendix presents detailed results from this analysis. Across all specifications, the findings still show the increase in both gender employment after the policy regarldess of the firm type or characteristics.

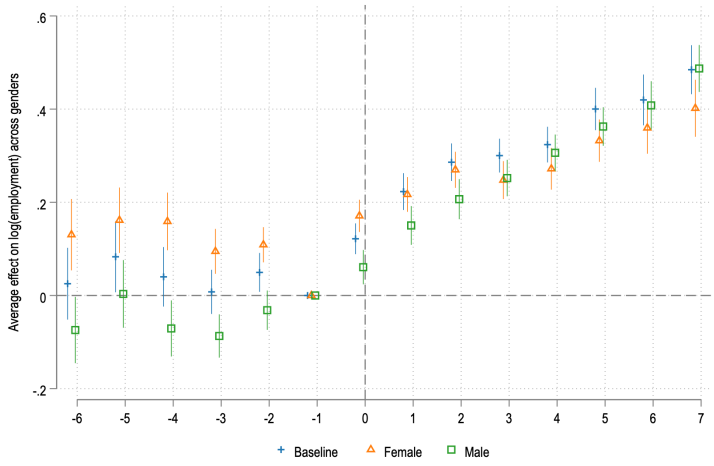



**Figure 2.4:** Effects on firm's employment by gender

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects. Sources: VES 2006-2019

#### 2.4.3.2 Firm characteristics heterogeneity

To test for heterogeneous effects across firm characteristics, I interact the treatment variable with firm characteristics. However, some characteristics may be endogenous and could respond to treatment. To address this concern, I fix these characteristics to their 2011 values, the year prior to the unification policy, or use the average across all available pre-treatment years, depending on the characteristic. I estimate the following model:

$$\begin{aligned} Y_{idt} = \alpha + \beta \times (foreignfirm_{idt} \times Post_t \times characteristics\_bf2012_{id}) \\ + \sum_s \beta_s \times (Year_s \times foreignMW_{dt}) + \delta_i + \gamma_t + \epsilon_{idt} \end{aligned} \tag{2.3}$$

Where $characteristics\_bf2012$ is a dummy indicating the firm type before 2012.

Table 2.5 presents results differentiated by firm characteristics. Column (1) reports the treatment effects for young firms, defined as those aged two years or younger as of 2011. The findings indicate that the unification policy has positive effects on employment for young

firms in the baseline specification, and this result remains similar for manufacturing firms or in high FDI regions, as shown in column (5) and (9). Being a young foreign firm is associated with about a 10% higher level of employment compared to domestic firms.

Column (2) presents the effects of the unification policy on large firms, defined as those with more than 200 employees prior to 2012. The treatment effects are comparable between the baseline and firms located in high FDI regions, as column (10) shows, with large foreign firms showing about a 26-28% increase in employment compared to domestic counterparts. However, these effects appear significantly smaller when focusing specifically on firms within the manufacturing sector. This suggests that large manufacturing firms might face different constraints or characteristics that limit the impact of the policy.

**Table 2.5:** Employment effects by firm's chacracteristics

| Log(employment) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | Manufacturing | | | | High FDI region | | | |
| FDI firm x post | 0.270*** | 0.274*** | 0.199*** | 0.169*** | 0.320*** | 0.307*** | 0.237*** | 0.170*** | 0.274*** | 0.282*** | 0.204*** | 0.181*** |
| | (0.023) | (0.022) | (0.023) | (0.027) | (0.027) | (0.030) | (0.026) | (0.041) | (0.024) | (0.022) | (0.024) | (0.028) |
| FDI firm x post x Young firm | 0.099*** | | | | 0.102** | | | | 0.100*** | | | |
| | (0.024) | | | | (0.037) | | | | (0.026) | | | |
| FDI firm x post x Large firm | | 0.276*** | | | | 0.164*** | | | | 0.259*** | | |
| | | (0.034) | | | | (0.041) | | | | (0.035) | | |
| FDI firm x post x Top 15th TFP firm | | | 0.042* | | | | 0.028 | | | | 0.037 | |
| | | | (0.019) | | | | (0.021) | | | | (0.019) | |
| FDI firm x post x Continuer/Survivor | | | | 0.077** | | | | 0.125*** | | | | 0.069** |
| | | | | (0.025) | | | | (0.038) | | | | (0.026) |
| N | 2791232 | 2791232 | 2791232 | 4150067 | 536473 | 536473 | 536473 | 754259 | 2358115 | 2358115 | 2358115 | 3538094 |

Notes: The table reports estimates in equation 2.3 for impacts on firm-level outcomes. Clustered SE at district level in parentheses. Firm characteristics such as age (young), size (large), and TFP are measured in the pre-policy period. Young firms are firms aged 2 years at most in 2011. Large firms are those with more than 200 employees in 2011. Top 15th TFP firms are defined as those belonging to the top 15th percentile of the TFP distribution. Continuer/Survivor is the firm operating in time t and t+1. All models control for firm and year fixed effects. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Another key dimension examined is total factor productivity (TFP). I estimate TFP using the control function approach developed by Wooldridge (2009), which addresses simultaneity and selection bias by using intermediate inputs, such as materials, as proxies for unobserved productivity shocks. TFP is calculated as the residual from a value-added production function, net of the estimated contributions of labor and capital. The underlying hypothesis is that more productive firms are better positioned to leverage the unification policy for employment expansion. Column (3) supports this hypothesis, showing positive and statistically significant treatment effects for high-TFP firms, defined as those in the top 15th percentile of the TFP distribution. These results remain robust when the threshold is broadened to include firms above the median TFP level, as reported in Table C6 in the Appendix. However, the treatment effects are no longer statistically significant for high-TFP firms within the manufacturing sector and in high-FDI regions.

The elimination of cost disparities following the policy may have incentivized the entry of new FDI firms, contributing to overall employment growth. If this is the case, the observed increase in employment would be driven more by new firm entry than by the expansion of existing firms. To investigate this possibility, I examine whether the employment effects differ among firms that continue operating over time. Specifically, I define continuers as firms that are active in period $t$ and remain in operation in period $t + 1$. Column (4) shows that foreign continuers exhibit approximately 8% higher employment compared to their domestic counterparts. This treatment effect remains consistent even when restricting the analysis to firms located in regions with high levels of FDI. However, unlike other firm characteristics, the employment effect of being a foreign firm is more pronounced among manufacturing firms, as shown in column (8). This may reflect the fact that surviving foreign manufacturers are better positioned to scale up production, adopt advanced technologies, or integrate into global supply chains—factors that facilitate sustained employment growth over time. Another more restrictive approach is to look at only firms operating throughout the sample period (i.e., a balanced panel). The results presented in Section 2.4.3 remain consistent under this more restrictive specification.

#### 2.4.3.3 Transport infrastructure access

One may argue that foreign firms are better positioned to take advantage of the standardized minimum wage policy due to their superior access to transport infrastructure. The underlying mechanism is that improved transport connectivity enhances firm performance by expanding market access and reducing both logistical and input costs (Bonadio, 2024; Donaldson, 2018; Ducruet et al., 2024; Faber, 2014; Martincus et al., 2017). To examine this channel empirically, I use the distance to the nearest harbor as a proxy for transport infrastructure access. Since distance can be zero for firms located directly at a harbor, I apply the inverse hyperbolic sine (IHS) transformation (i.e., $x_{id} = log(X_{id} + \sqrt{X_{id}^2 + 1})$ where $X_{id}$ is the distance of firm $i$ in district $d$ to the closest harbor (in kilometers). This transformation allows me to handle zero values while maintaining comparability with logarithm transformations. To test the channel, I estimate the following specification:

$$\begin{aligned} Y_{idt} = \alpha + \beta \times (foreignfirm_{idt} \times Post_t \times Distance_{id}) \\ + \sum_s \beta_s \times (Year_s \times foreignMW_{dt}) + \delta_i + \gamma_t + \epsilon_{idt} \end{aligned} \tag{2.4}$$

Table 2.6 presents the estimation results of equation 2.4 for the full sample, the manufacturing sector, and high-FDI regions, respectively. Across all specifications, the results suggest that proximity to harbors does not significantly increase employment among foreign firms.

However, when looking at the spatial distribution of harbors across Vietnam visualized in Figure B8(b) in the Appendix, harbors are unevenly distributed along the coastline. For instance, the Red River Delta and North Central have relatively dense harbor coverage, while regions such as the Central Highlands and Northern Midland lack any significant port infrastructure. Thus, given the regional disparity in harbor availability, I further explore the role of harbor proximity within each region. This allows me to assess whether the impact of transport infrastructure varies geographically and potentially drives heterogeneous effects.

The results, presented in Table 2.7, indicate that foreign firms expand employment overall, but the magnitude of this expansion varies across regions. In particular, the results reveal

**Table 2.6:** Employment effects by geolocation: Distance to the closest harbor

| | (1) | (2) | (3) |
|---|---|---|---|
| Log(employment) | | Manufacturing | High FDI region |
| FDI firm x post | 0.293*** | 0.187 | 0.271** |
| | (0.075) | (0.115) | (0.088) |
| FDI firm x post x Dist. to harbor | -0.012 | 0.028 | -0.006 |
| | (0.023) | (0.032) | (0.027) |
| Firm FE | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ |
| N | 4230726 | 770600 | 3597910 |

Notes: The table reports estimates in equation 2.4 for impacts on firm-level outcomes where I interact the treatment with distance to the closest habor. Clustered SE at district level in parentheses. All models control for firm and year fixed effects. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Table 2.7:** Employment effects by regions

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | RRD | NM | NC | CH | SE | MRD |
| FDI firm x post | -0.113 | 1.157** | -0.087 | 2.853 | 0.390*** | 0.577 |
| | (0.129) | (0.415) | (0.181) | (1.441) | (0.100) | (0.474) |
| FDI firm x post x Dist. to harbor | 0.126** | -0.214* | 0.052 | -0.597* | -0.033 | -0.096 |
| | (0.039) | (0.089) | (0.054) | (0.290) | (0.033) | (0.112) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 1308921 | 187940 | 596507 | 117791 | 1642356 | 377210 |

Notes: The table reports estimates in equation 2.4 for impacts on firm-level outcomes across different regions. RRD is Red River Delta, NM is Northern Midland, NC is North Central, CH is Central Highlands, SE is Southeast, and MRD is Mekong River Delta. Clustered SE at district level in parentheses. All models control for firm and year fixed effects. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

heterogeneous effects linked to the distance from major harbors. Most coefficients are negative, regardless of statistical significance, suggesting that in many areas, greater distance from ports may dampen the employment gains of foreign firms, possibly due to higher logistical costs and reduced access to trade infrastructure. However, the Red River Delta stands out with a positive effect. This may reflect the region's economic dynamism, dense population, and relatively well-developed infrastructure, which mitigate the disadvantages of being located farther from ports. In contrast, in less developed regions, foreign firms may rely more heavily on port proximity for operations, making distance a more binding constraint on their employment expansion.

#### 2.4.3.4 Supply chain linkage

In this section, I examine the heterogenous effects varying the direct connection with domestic firms. Specially, I investigate whether supply chain linkages to domestic firms affect the employment response of foreign firms following the equalization minimum wage policy. While foreign firms were not directly affected by the policy, they may have faced indirect cost increases if they sourced raw or intermediate materials from domestic suppliers who experienced rising labor costs, called pass through effects. This mechanism could potentially mitigate the employment gains observed among foreign firms, particularly if input cost pass-through raised production costs.

To test this supply chain channel, I interact the treatment variable with measures of firms' reliance on domestic input suppliers, both within the same province and from other provinces. If input cost spillovers are present, I expect foreign firms that are more exposed to domestic suppliers to exhibit smaller employment gains after the policy. The variable of interest is a continuous measure indicating the percentage of raw or intermediate materials sourced from domestic suppliers, ranging from 0 to 100 percent. It is noted that this information is available only for manufacturing firms.

The results in Table 2.8 show no statistically significant effect of supply chain linkages, measured by local or inter-province material sourcing, on foreign firm employment. This

**Table 2.8:** Employment effects by input suppliers

| Panel A: Same province | (1) | (2) |
|---|---|---|
| FDI firm x post | 0.505*** | 0.454*** |
| | (0.064) | (0.057) |
| FDI firm x post x Raw material | -0.000 | |
| | (0.001) | |
| FDI firm x post x Interm. material | | -0.001 |
| | | (0.001) |
| Obs | 32359 | 38474 |
| Panel B: Different province | | |
| FDI firm x post | 0.496*** | 0.408*** |
| | (0.066) | (0.059) |
| FDI firm x post x Raw material | -0.002 | |
| | (0.002) | |
| FDI firm x post x Interm. material | | 0.001 |
| | | (0.001) |
| Firm FE | ✓ | ✓ |
| Year FE | ✓ | ✓ |
| Obs | 24260 | 32593 |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes where I interact the treatment with share of raw/intermediate material suppliers from same/different province. Clustered SE at district level in parentheses. All models control for firm and year fixed effects. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

suggests that foreign expansion is not negatively affected by minimum wage changes passed through domestic supply chains. Despite potential cost increases among upstream suppliers, foreign firms continue to expand. This indicates that the direct labor cost advantages from wage equalization outweigh any indirect supply chain cost pressures. These findings suggest that foreign firms, possibly due to their larger scale and flexibility, can absorb upstream shocks without altering their sourcing strategies.

### 2.4.4 Sensitivity checks

#### 2.4.4.1 Firm ownership

In the baseline specification, I combine all types of foreign firm ownership. However, firms with 100% foreign ownership may respond differently from other ownership categories (i.e., more than 50% and less than 100% foreign investment) due to their reduced reliance on

local stakeholders. To investigate this, I restrict the treatment group exclusively to 100% foreign-owned firms. Table D7 presents these results, showing that the treatment effects remain statistically significant and become even stronger for fully foreign-owned firms.

Another concern involves domestic firms, in which I include both state-owned enterprises (SOEs) and private domestic firms. However, SOEs differ from private firms in several ways, such as size and labor market share. Although SOEs account for a small portion of the labor market, approximately 0.7% as shown in Table 2.1, their sizes are comparable to those of foreign firms. This suggests that their response to the unified minimum wage may differ from that of private domestic firms. Therefore, I exclude SOEs from the analysis in this section. As shown in Table D8, the results remain consistent with the baseline, indicating that the findings are not driven by SOEs.

#### 2.4.4.2 Balanced sample

The main results are estimated using an unbalanced panel, which may be influenced by firm entry and exit. In the heterogeneity analysis, I previously addressed this by focusing on firms that continued operating between period $t$ and $t+1$. In this section, I implement a more restrictive robustness check using a balanced panel, which includes only firms that are observed throughout the entire analysis period. This approach further mitigates concerns about compositional changes in the sample. Figures D15 and D16 show that the results remain robust when using this stricter sample of continuing firms.

#### 2.4.4.3 Potential outliers

One potential concern is the presence of outliers. Hanoi and Ho Chi Minh City are major metropolitan areas in Vietnam, where more foreign firms are located and infrastructure and access to capital and labor are significantly better. As a result, the baseline results could be disproportionately influenced by these outliers. Additionally, these provinces are consistently classified under the highest bracket of the four-bracket minimum wage system in Vietnam, which may dampen the observed effects of the policy.

To address this concern, I exclude Hanoi and Ho Chi Minh City from the analysis. The results remain robust across different specifications, suggesting that the effects of the unification policy are not driven solely by these major metropolitan areas. Figures D17 and D18 in Appendix represent these findings.

## 2.5 District-level effects

### 2.5.1 Empirical strategy

The previous section's firm-level analysis shows that foreign firms expanded employment following the minimum wage unification policy. While this provides valuable insights into firm-level behavior, it does not fully capture the broader, potential aggregate effects of foreign sector expansion at the district level. Although the panel data allow me to compare foreign and domestic firms' responses to the policy, the analysis does not directly indicate the effects on domestic firms. The district-level approach, by aggregating over all firms, allows for the identification of broader spillover effects, including potential impacts of foreign firms on domestic employment.

On the one hand, increased foreign presence can heighten competition, potentially reducing demand for domestic firms' products and raising their average costs. On the other hand, foreign firm expansion may generate positive externalities, such as knowledge transfer, technology diffusion, and labor mobility, that benefit domestic firms, regardless of whether they are directly linked to foreign firms or not (Brambilla et al., 2009). Moreover, following the unification policy, domestic firms also face relatively higher labor costs, as their previous wage advantage over foreign firms was eliminated. As a result, two potentially opposing forces may shape domestic firm growth: exposure to the foreign sector, and the upward pressure on labor costs from the policy reform.

This section investigates how the policy affected aggregate labor market outcomes at the district level. First, I test whether total district-level employment responds to the policy, conditional on exposure to foreign-sector activity and the extent of minimum wage gap

closure. If foreign-sector expansion generates spillovers, districts with greater foreign-sector presence should benefit more. Second, I decompose these effects by examining foreign and domestic sector employment separately, to identify distributional impacts of the reform. To document these dynamics, this section investigates the aggregate effects of the policy on district labor market. I exploit two sources of variation across districts. First, the pre-policy minimum wage gap between foreign and domestic firms, which captures variation in the degree of policy-induced wage compression across districts. Specially, I construct the gap for a given district $d$ in year $t$ as:

$$GAP_{dt} = \frac{foreignMW_{dt} - domesticMW_{dt}}{domesticMW_{dt}} \tag{2.5}$$

where $foreignMW_{dt}$ is the MW in foreign sector in district $d$ at year $t$, while $domesticMW$ is the MW in domestic sector. $GAP_{dt}$ is the average the minimum wage gap measure over the years prior to the unification policy in 2011. It is important to note that in 2006 and 2007, a single domestic minimum wage applied uniformly across all districts, while foreign-sector wages varied by region, as shown in Appendix Table A1. As a result, the wage gap during this period was mechanically driven by regional foreign wage assignments and did not reflect meaningful inter-sectoral policy differences. Including these early years introduces volatile and non-parallel pre-trend estimates of gap measure treatment in the event study, likely due to structural noise rather than anticipatory behavior. Therefore, I exclude 2006 and 2007 from the analysis. Starting in 2008, both domestic and foreign minimum wages vary across districts, making the wage gap measure economically meaningful and policy-relevant. Additionally, to avoid potential mean reversion bias, I calculate the average wage gap using only the years 2008 to 2010.

$$\overline{GAP}_d = \sum_{2008}^{2010} GAP_{dt} \tag{2.6}$$

Second, the pre-policy share of foreign firms, which captures differences in foreign sector presence across districts, allows me to evaluate whether districts more exposed to foreign

firms experienced differential outcomes. I calculate the share of foreign firms as the ratio of foreign firms to total firms in each district for each year prior to 2011, and then take the average across these pre-2011 years (i.e., from 2008 to 2010).

$$\overline{sharеforeignfirm}_d = \sum_{2008}^{2010} shareforeignfirm_{dt} \tag{2.7}$$

Figure E20 in the Appendix presents the distributions of both treatment variables, along with their respective means and standard deviations. The mean share of foreign firms before 2012 is 2%, with a standard deviation of 4%. For the minimum wage gap, the mean is 58%, and the standard deviation is 6%.

I estimate the effect of both continuous treatment variables on district-level outcomes as follows:

$$\begin{aligned} Y_{dt} = \alpha + \beta_1 \times (\overline{shareforeignfirm}_d \times Post_t) + \beta_2 \times (\overline{GAP}_d \times Post_t) \\ + \sum_s \gamma_s \times (foreignMW_{dt} \times Year_s) + \delta_d + \gamma_t + \theta_{rt} + \epsilon_{dt} \end{aligned} \tag{2.8}$$

where $Y_{dt}$ denotes the district-level outcome of firm $i$ (e.g., the natural logarithm of number of employees, number of firms) at district $d$ in year $t$. For easy intepretation, I standardize the outcome of interest. $Post_t$ is a dummy variable equal to 1 if year $t$ is after 2011. District and year fixed effects, denoted by $\delta_d$, $\gamma_t$, and $\theta_{rt}$ control for time-invariant district characteristics, common time trends and region-by-year specific shocks or trends, respectively.

Similar to firm-level analysis, I also investigate the dynamic treatment effect following the specification:

$$\begin{aligned} Y_{dt} = \alpha + \sum_{k=2008, k\neq 2011}^{k=2019} \beta_{1k} \times (\overline{shareforeignfirm}_d \times Year_k) \\ + \sum_{k=2008, k\neq 2011}^{k=2019} \beta_{2k} \times (\overline{GAP}_d \times Year_k) \\ + \sum_s \gamma_s \times (foreignMW_{dt} \times Year_s) + \delta_d + \gamma_t + \theta_{rt} + \epsilon_{dt} \end{aligned} \tag{2.9}$$

where 2011 is the reference year. $Year_k$ is the year dummy indicator.

### 2.5.2 Results

Table 2.9 presents the estimates from Equation 2.8. Column (2) shows that both treatments, the initial MW gap between the domestic and foreign sectors, and the share of foreign employment, are positively associated with overall district-level employment. However, only the effect of the foreign employment share is statistically significant.

Figure 2.5 illustrates the dynamic effects of both treatments over time. While the estimates for the MW gap treatment are generally positive, the large standard errors in the pre-treatment period raise concerns about the validity of the parallel trends assumption. To address this, I conduct a joint F-test for the significance of all pre-treatment coefficients. The test yields F(3, 689) = 0.76 with a p-value of 0.5174, indicating that I cannot reject the null hypothesis that the pre-trend coefficients are jointly equal to zero. This suggests that the pre-treatment dynamics are statistically indistinguishable from zero and provides support for the parallel trends assumption.

**Table 2.9:** Employment effects on district level outcome

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Log(employment) | | | | Firm counts | |
| | Total emp. | Total emp. | Foreign emp. | Domestic emp. | Foreign firms | Domestic firms |
| MW gap x post | 1.014 | 1.011 | 0.184 | 0.720 | 0.026 | 0.292 |
| | (0.820) | (0.819) | (1.258) | (0.743) | (1.014) | (0.460) |
| Share of foreign firms x post | | 0.370 | -0.479 | 0.621** | 0.014 | 0.574*** |
| | | (0.198) | (0.260) | (0.199) | (0.240) | (0.162) |
| District FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Region x year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Obs | 8215 | 8215 | 4324 | 8215 | 8215 | 8215 |
| Dep var pre-treat std. mean | -0.14 | -0.14 | -0.13 | -0.11 | -0.14 | -0.22 |
| Dep var pre-treat mean | 7.78 | 7.78 | 6.69 | 7.64 | 0.88 | 4.59 |

Notes: The table reports estimates in equation 2.8 for impacts on district-level outcomes. Clustered SE at district level in parentheses. Source: VES 2008-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To examine compositional effects by firm ownership type, I estimate regressions using the logarithm of either foreign or domestic employment at the district level as the dependent variable. Column (3) shows that the minimum wage gap does not have a statistically significant

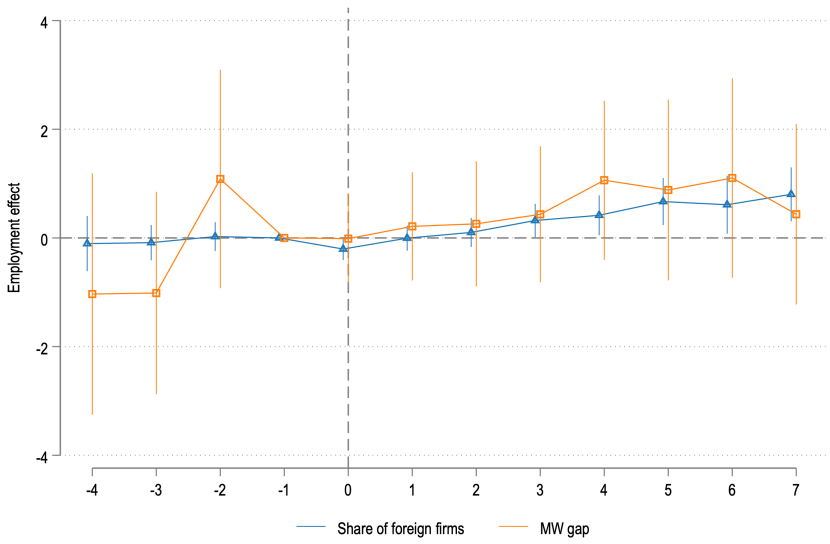
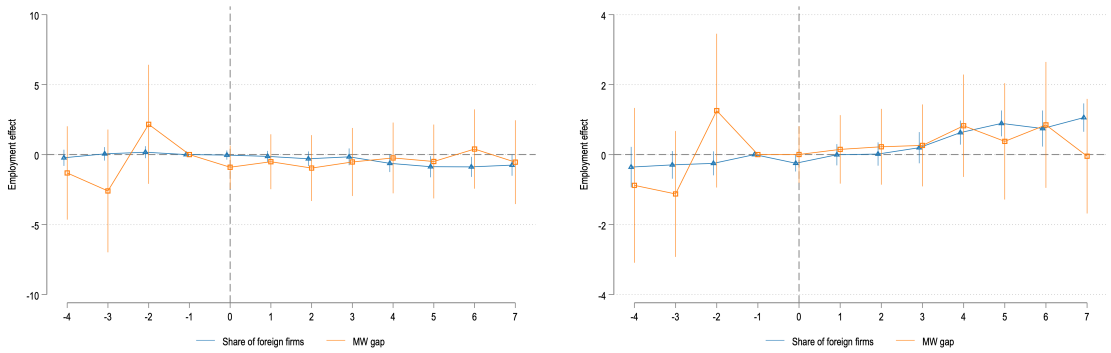



**Figure 2.5:** Effects on district's employment

The figure plots event-study estimates of the policy's effect on district-level outcomes in equation 2.9. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

effect on foreign employment. At first glance, this appears to contrast with the firm-level analysis, where the unification policy is associated with an increase in the employment of foreign firms. However, since foreign firms account for only about 18% of total district-level employment on average, their employment gains at the firm level may be diluted in aggregate district-level measures. Figure 2.6 futhers support the findings where the MW gap shows no significant effects on foreign employment. In addition, a higher pre-policy share of foreign firms is also not significantly associated with changes in foreign employment.

In contrast, Column (4) reveals a different pattern for domestic-sector employment. While the minimum wage gap does not have a statistically significant effect on domestic employment, the share of foreign firms is positively associated with domestic-sector employment. Figure 2.6(b) visually supports these findings. The effects of the MW gap remain relatively flat after the policy, whereas the effect of the foreign firm share becomes statistically significant starting four years post-reform. The positive effect may reflect spillover benefits from the presence of foreign firms, such as knowledge diffusion, supply chain linkages, or increased




**(a)** Foreign firms **(b)** Domestic firms

**Figure 2.6:** Effects on employment by ownership

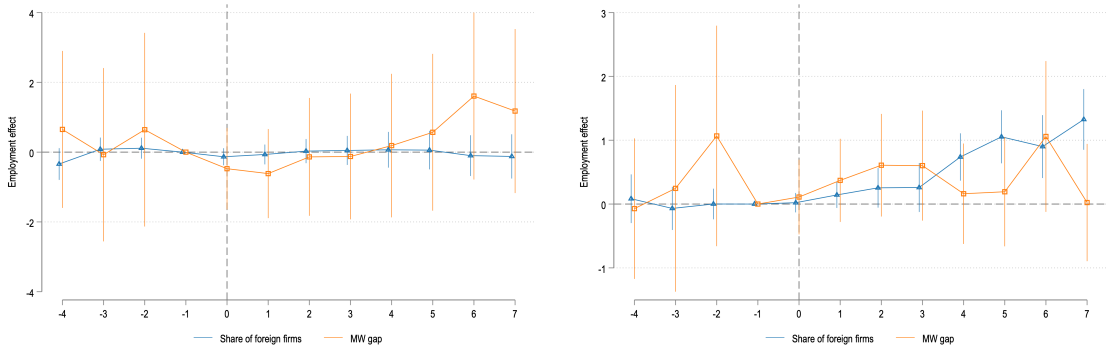
The figure plots event-study estimates of the policy's effect on district-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

local demand (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021). These effects are more likely to manifest at the district level, where inter-firm interactions take place, rather than at the individual firm level.

To further investigate whether these patterns are driven by changes in firm entry and exit, I analyze firm counts across sectors (i.e., extensive margin). Column (5) indicates that neither the MW gap nor the pre-policy share of foreign firms significantly affects the number of foreign firms, as also shown in Figure 2.7, whereas column (6) shows that a higher share of foreign firms prior to the policy is positively associated with the number of domestic firms. These results are in line with the results in column (4). In particular, the increase in domestic firm counts observed at the district level is driven by the positive coefficient on the share of foreign firms, suggesting that districts with a greater foreign firm presence experience higher net domestic firm entry following the reform. One plausible interpretation is that foreign firms foster a local business environment that facilitates domestic firm entry through positive spillovers, such as knowledge diffusion, technology transfer, or increased local demand for intermediate inputs and services.

These district-level results reflect net changes in firm counts and therefore do not rule out

the possibility that some incumbent domestic firms lose competitiveness and exit the market following the reform. Rather, the findings are consistent with a reallocation process in which less productive domestic firms exit while new domestic firms enter in response to spillovers generated by foreign firms. Figure 2.7(b) further highlights that these effects become more pronounced four years after the policy implementation.[5]




**(a)** Foreign firms **(b)** Domestic firms

**Figure 2.7:** Effects on firm counts by ownership

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. Dependent variables are in $log(1+y)$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects. Sources: VES 2008-2019

Taken together, these results suggest that domestic firms may benefit from proximity to foreign firms, in line with existing literature on the positive externalities of FDI for host economies. Previous studies have documented that foreign firm presence can enhance local firm performance through mechanisms such as knowledge spillovers, labor mobility, and technological diffusion (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021).

Overall, the district-level analysis complements the firm-level results by highlighting broader labor market adjustments that may not be captured at the firm level. While firm-level results show that foreign firms expand employment on average relative to the domestically owned enterprises following the reform, the district-level analysis suggests a more complex

[5]Additional analyses using district-level entry and exit rates further support this interpretation. See Figures E24, E25, E26, and E27 in the Appendix.

adjustment. The increase in labor standards does not appear to adversely affect overall district employment. Moreover, in regions with a high pre-policy share of foreign firms, domestic employment rises, possibly reflecting positive spillovers such as knowledge transfer, increased local demand, or labor mobility from foreign to domestic firms.

### 2.5.3 Heterogeneity by industry

The literature on MNE spillover effects primarily focuses on the manufacturing sector, where input–output linkages between foreign and domestic firms—both forward and backward—are more pronounced. In contrast, there is still limited evidence on such spillovers in the services or retail sectors (Verhoogen, 2023). Nevertheless, some studies document positive spillover effects of FDI on the upgrading of local retail suppliers and improvements in household welfare in regions more exposed to foreign firms (Atkin et al., 2018; Iacovone et al., 2015; Javorcik et al., 2018). Motivated by this, I examine heterogeneous effects across manufacturing, services, and retail sectors.

The results, presented in Table 2.10, show that the share of foreign firms is significantly and positively associated with domestic employment in all three sectors, with relatively similar magnitudes. This supports the argument that foreign firm presence generates positive spillovers for local firms, possibly through knowledge diffusion or supply chain integration. However, the effects on foreign-sector employment remain statistically insignificant.

The MW gap shows no significant effects in manufacturing or services. In contrast, it is positively associated with both total and domestic employment in retail, suggesting that wage standardization may contribute to job growth in this low-wage sector.

Overall, the findings underscore the importance of sectoral context; while foreign firm presence consistently benefits domestic employment, the effects of a standardization policy are more nuanced and vary by industry.

The dynamic treatment effects are presented in Appendix Figures E28, E29, E30, E32, and E33.

**Table 2.10:** Employment effects on district level outcomes across industries

| | Manufacturing | | | Services | | | Retail | | |
|---|---|---|---|---|---|---|---|---|---|
| Dep. var.: Log(emp.) | Total emp. | Frg. emp. | Dom. emp. | Total emp. | Frg. emp. | Dom. emp. | Total emp. | Frg. emp. | Dom. emp. |
| MW gap x post | 0.503 | -1.312 | 0.320 | 0.336 | 0.307 | 0.346 | 0.949* | 1.671 | 1.034* |
| | (1.064) | (1.131) | (0.976) | (0.710) | (2.154) | (0.741) | (0.468) | (1.607) | (0.491) |
| Share of frg. firms. x post | 0.019 | -0.087 | 0.486* | 0.295 | -0.766 | 0.418* | 0.400** | 0.427 | 0.467** |
| | (0.259) | (0.242) | (0.246) | (0.219) | (0.594) | (0.212) | (0.150) | (0.723) | (0.157) |
| District FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Obs | 7768 | 3867 | 7750 | 8062 | 1922 | 8062 | 7995 | 420 | 7989 |
| Dep var pre-treat std. mean | 0.29 | 0.43 | 0.23 | -0.20 | -0.40 | -0.17 | -0.67 | -0.96 | -0.65 |
| Dep var pre-treat mean | 6.71 | 6.77 | 6.48 | 5.71 | 4.79 | 5.69 | 4.78 | 3.48 | 4.78 |

Notes: The table reports estimates in equation 2.8 for impacts on district-level outcomes across industries. Clustered SE at district level in parentheses. Source: VES 2008-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

## 2.6 Conclusion

This paper examines the employment effects of Vietnam's 2011 minimum wage (MW) unification policy, which eliminated the MW differentials between domestic and foreign firms. Using firm-level panel data and a difference-in-differences framework, I find that foreign firms significantly expand employment following the reform, particularly in the manufacturing sector. This is consistent with a reduction in relative labor costs enhancing their competitive position. These effects are persistent and larger over time.

To capture broader labor market dynamics, I complement the firm-level analysis with district-level evidence. I construct two key measures: the pre-reform MW gap, defined as the difference between foreign and domestic sector minimum wages, and the district-level share of employment in foreign-invested firms. Exploiting cross-district variation in these variables, the results indicate that the MW gap does not negatively affect overall district employment. Moreover, districts with a higher foreign firm presence experience gains in domestic employment. These gains are likely driven by spillover mechanisms such as technology transfer, skill upgrading, and increased local demand associated with foreign firm activity (Aitken & Harrison, 1999; Javorcik et al., 2018; Keller, 2021; Keller & Yeaple, 2009; Setzler & Tintelnot, 2021). In contrast, foreign firms, while expanding individually, contribute less to aggregate employment growth at the district level due to their relatively modest employment share.

Taken together, the findings suggest that harmonizing minimum wage standards across sectors does not hinder labor market performance. On the contrary, such policy alignment may generate positive employment spillovers, particularly in high-FDI, manufacturing-intensive regions, thereby supporting more inclusive and sustained economic development in emerging economies.

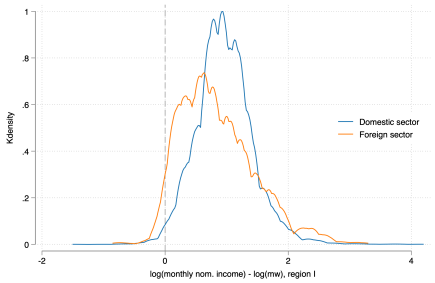
# Appendices

## A Minimum wage policy

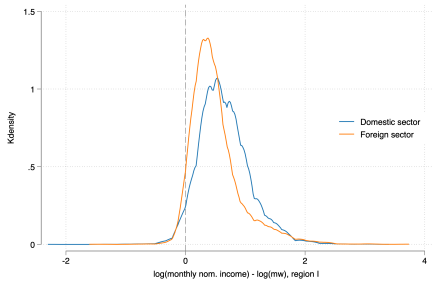
**Table A1:** Nominal regional minimum wage levels by sector (2006-2019)

| Year | Domestic Sector | | | | Foreign Sector | | | |
|---|---|---|---|---|---|---|---|---|
| | Region I | Region II | Region III | Region IV | Region I | Region II | Region III | Region IV |
| 2006 | 350 | 350 | 350 | | 870 | 790 | 710 | |
| 2007 | 450 | 450 | 450 | | 870 | 790 | 710 | |
| 2008 | 620 | 580 | 540 | | 1000 | 900 | 800 | |
| 2009 | 800 | 740 | 690 | 650 | 1200 | 1080 | 950 | 920 |
| 2010 | 980 | 880 | 810 | 730 | 1340 | 1190 | 1040 | 1000 |
| 2011 | 1350 | 1200 | 1050 | 830 | 1550 | 1350 | 1170 | 1100 |
| 2012 | 2000 | 1780 | 1550 | 1400 | 2000 | 1780 | 1550 | 1400 |
| 2013 | 2350 | 2100 | 1800 | 1650 | 2350 | 2100 | 1800 | 1650 |
| 2014 | 2700 | 2400 | 2100 | 1900 | 2700 | 2400 | 2100 | 1900 |
| 2015 | 3100 | 2750 | 2400 | 2150 | 3100 | 2750 | 2400 | 2150 |
| 2016 | 3500 | 3100 | 2700 | 2400 | 3500 | 3100 | 2700 | 2400 |
| 2017 | 3750 | 3320 | 2900 | 2580 | 3750 | 3320 | 2900 | 2580 |
| 2018 | 3750 | 3320 | 2900 | 2580 | 3750 | 3320 | 2900 | 2580 |
| 2019 | 3750 | 3320 | 2900 | 2580 | 3750 | 3320 | 2900 | 2580 |

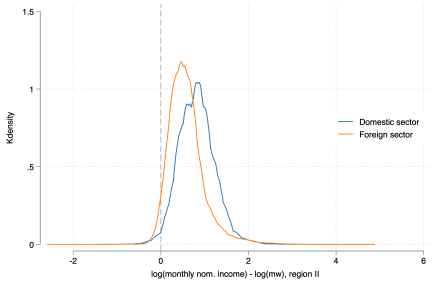
Sources: GSO



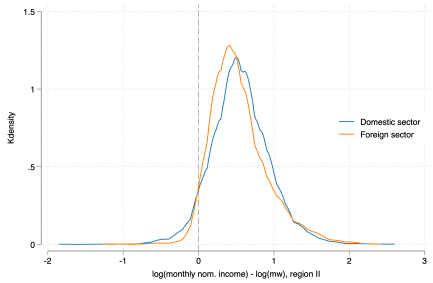

**(a)** Region I, 2011



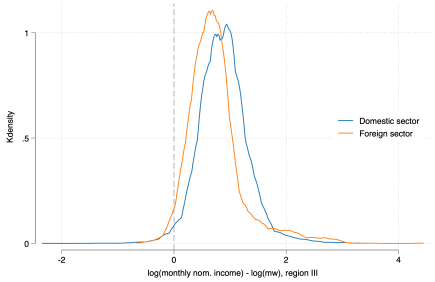

**(b)** Region I, 2012



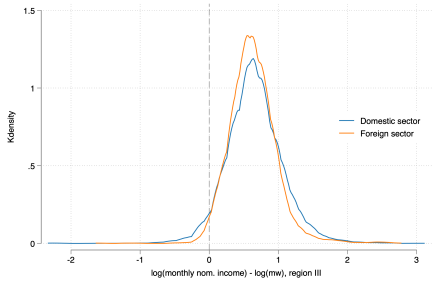

**(c)** Region II, 2011



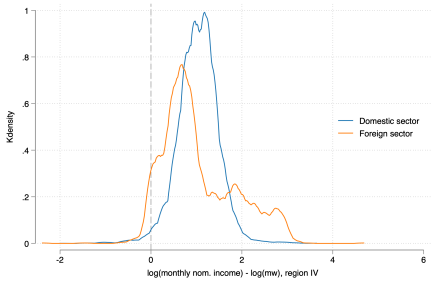

**(d)** Region II, 2012



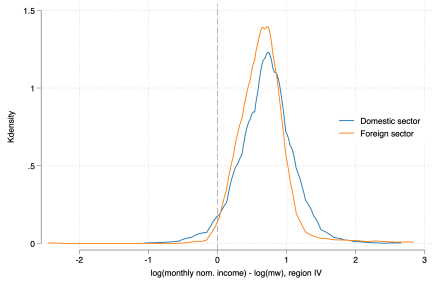

**(e)** Region III, 2011




**(f)** Region III, 2012




**(g)** Region IV, 2011




**(h)** Region IV, 2012

**Figure A1:** Log-wage distributions relative to the minimum wage, by region and year

Sources: Labor force survey and GSO

**Table A2:** Foreign vs Domestic Employment GAP (%) by Region

| Year | Region 1 | Region 2 | Region 3 | Region 4 |
|---|---|---|---|---|
| 2006 | 148.57 | 125.71 | 102.86 | |
| 2007 | 93.33 | 75.56 | 57.78 | |
| 2008 | 61.29 | 55.17 | 48.15 | |
| 2009 | 50.00 | 45.95 | 37.68 | 41.54 |
| 2010 | 36.73 | 35.23 | 28.40 | 36.99 |
| 2011 | 14.81 | 12.50 | 11.43 | 32.53 |

Sources: GSO. Notes: The percentage is calculated as: $\frac{ForeignMW - DomesticMW}{DomesticMW}$

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# B Descriptives and insitutional context

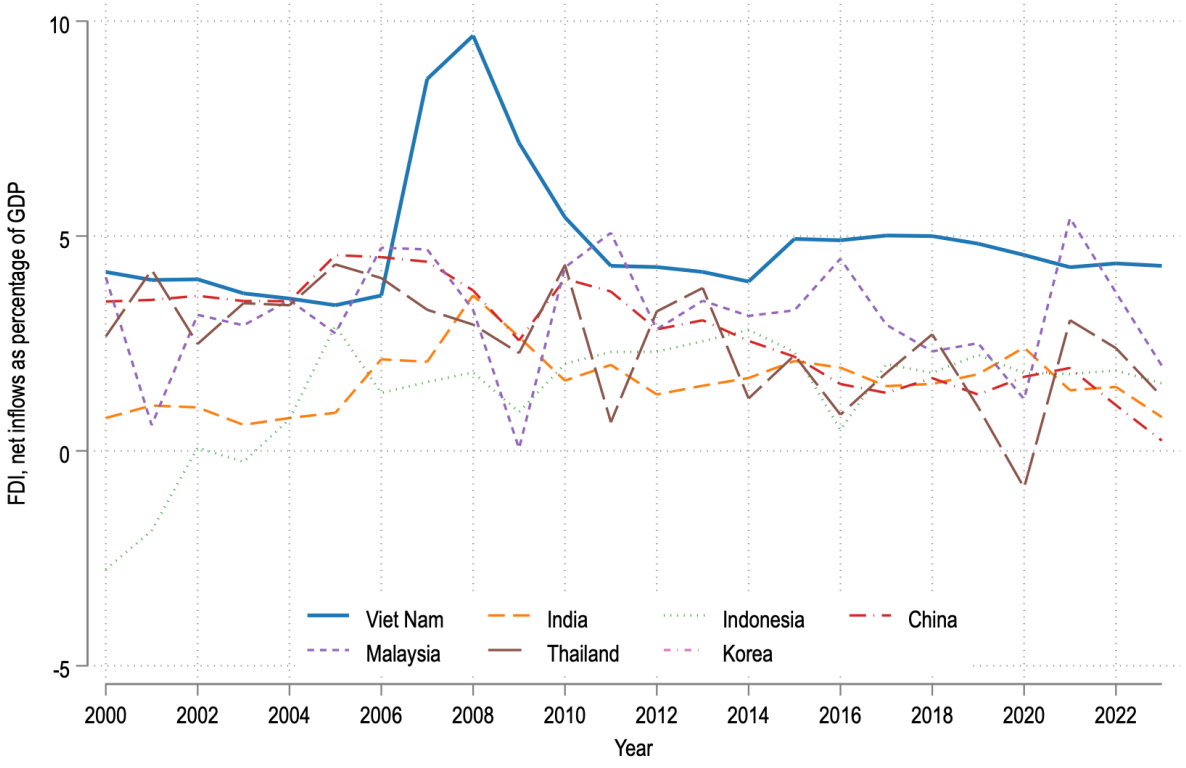



**Figure B2:** Foreign direct investment (FDI), net flows as percentage of GDP

Source: The World Bank.

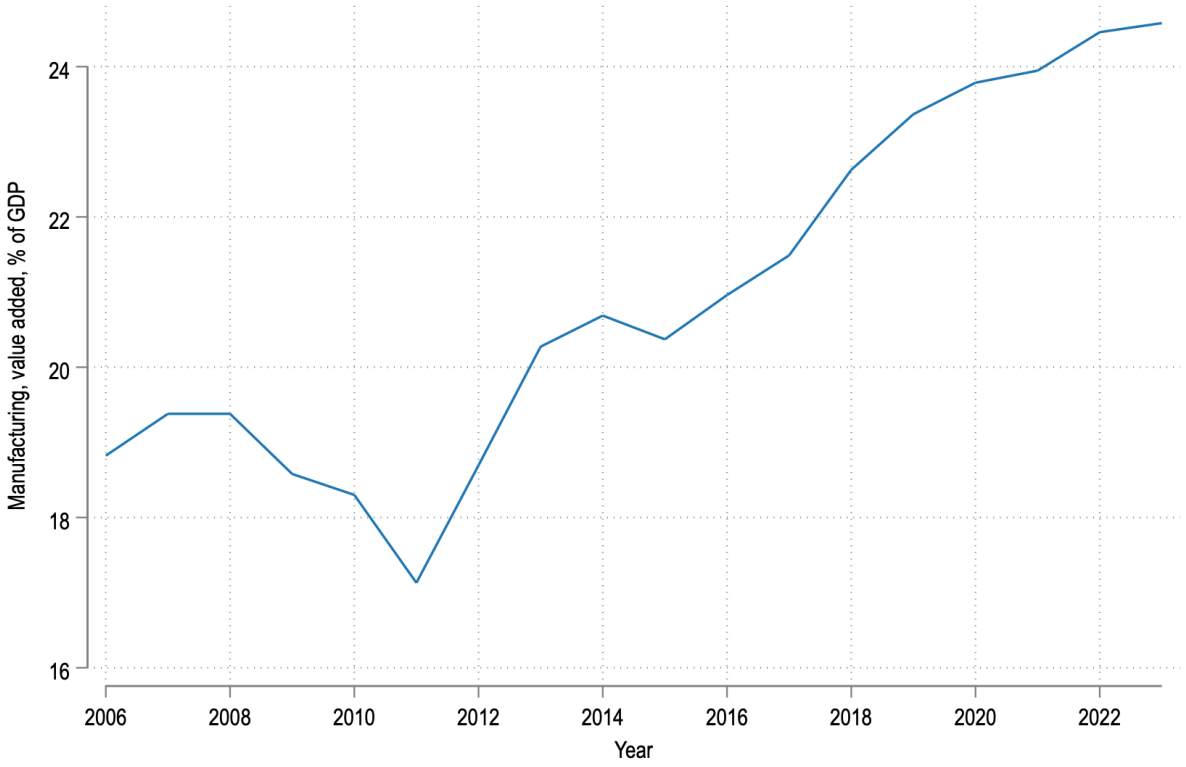



**Figure B3:** Manufacturing sector, value added as percentage of GDP

Source: The World Bank.

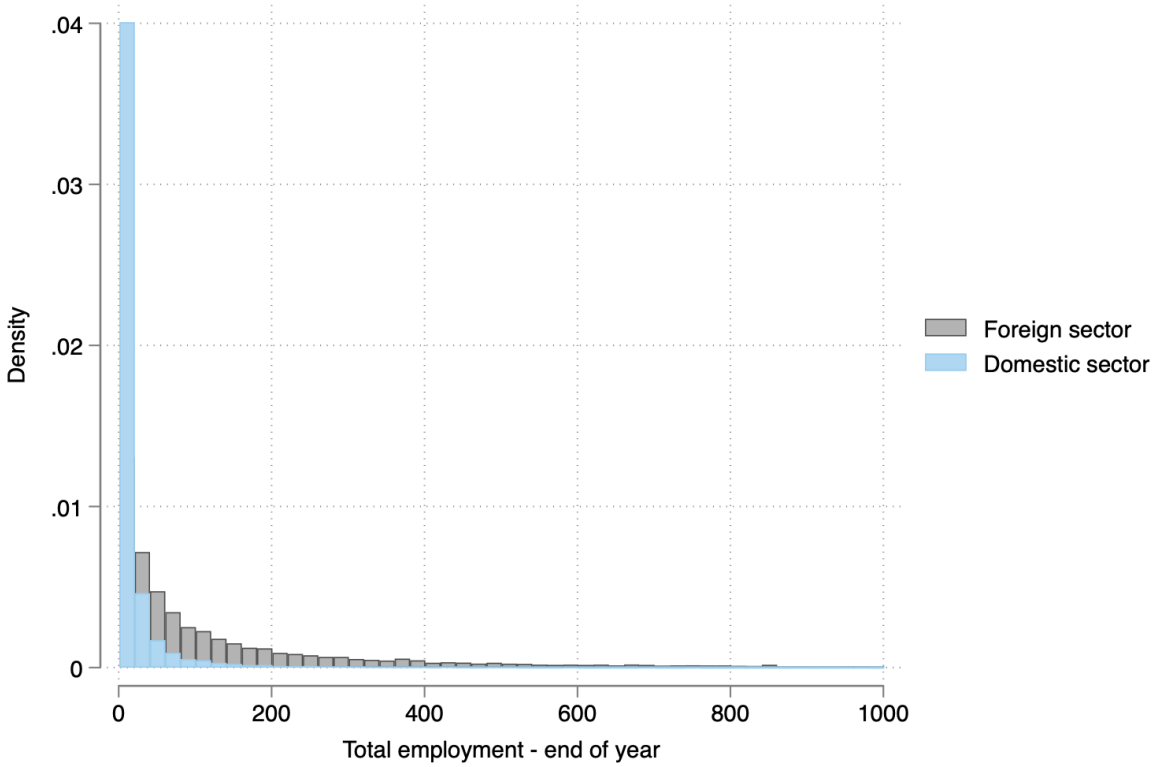



**Figure B4:** Density of employees in foreign and domestic firms

Source: VES 2006-2019. Notes: The sample is restricted to firms having less than 1000 employees.

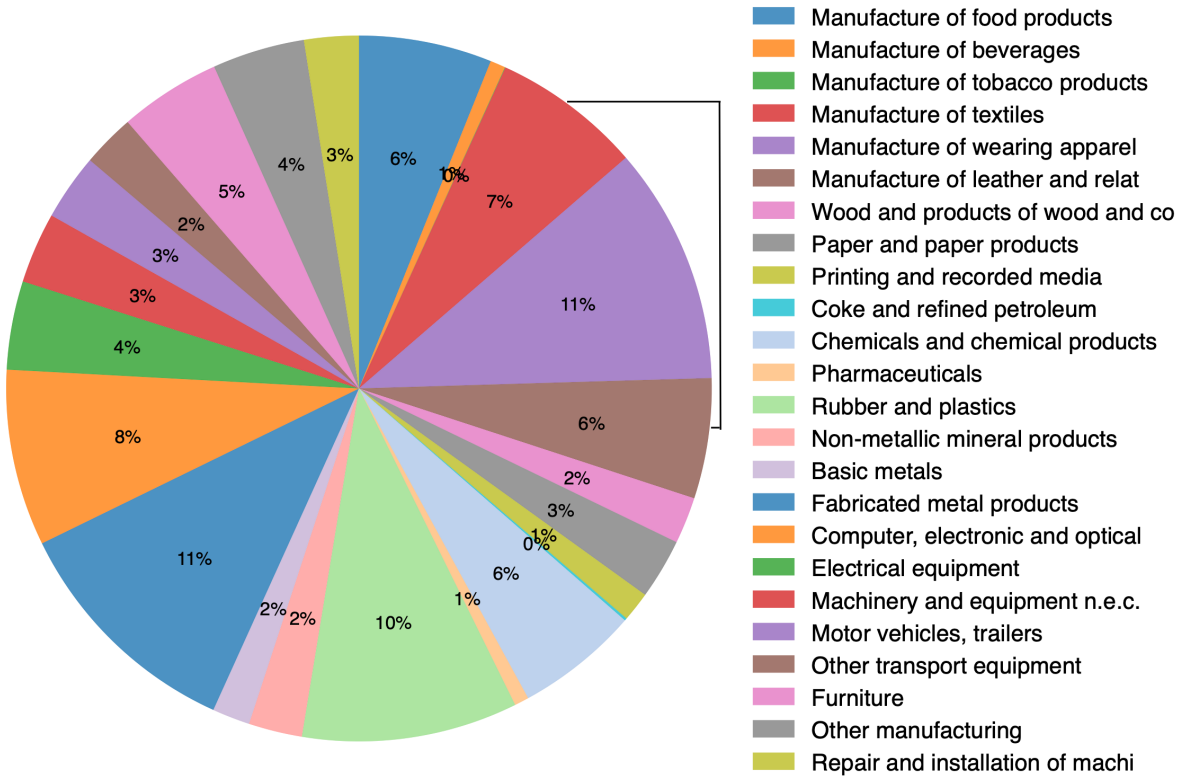
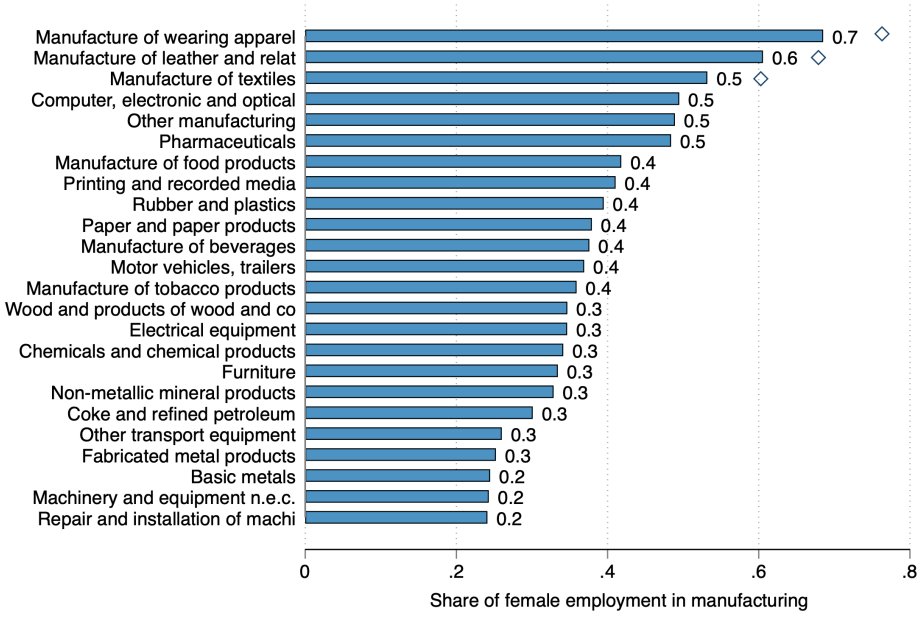
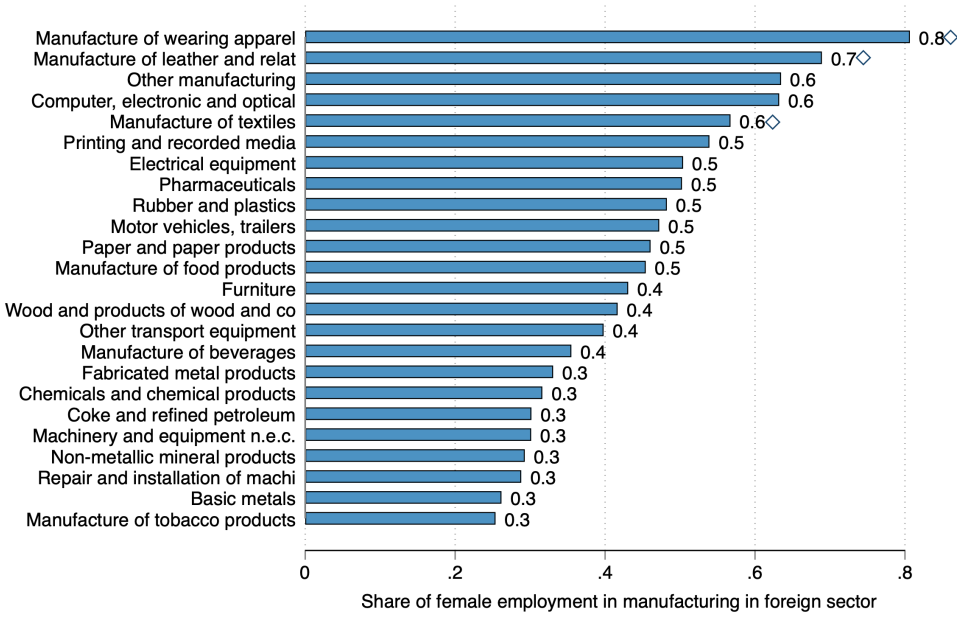



**Figure B5:** Share of industries in manufacturing

Notes: The line highlights three industries in which females have a competitive advantage: textiles, fur, and footwear. Source: VES 2006-2019.



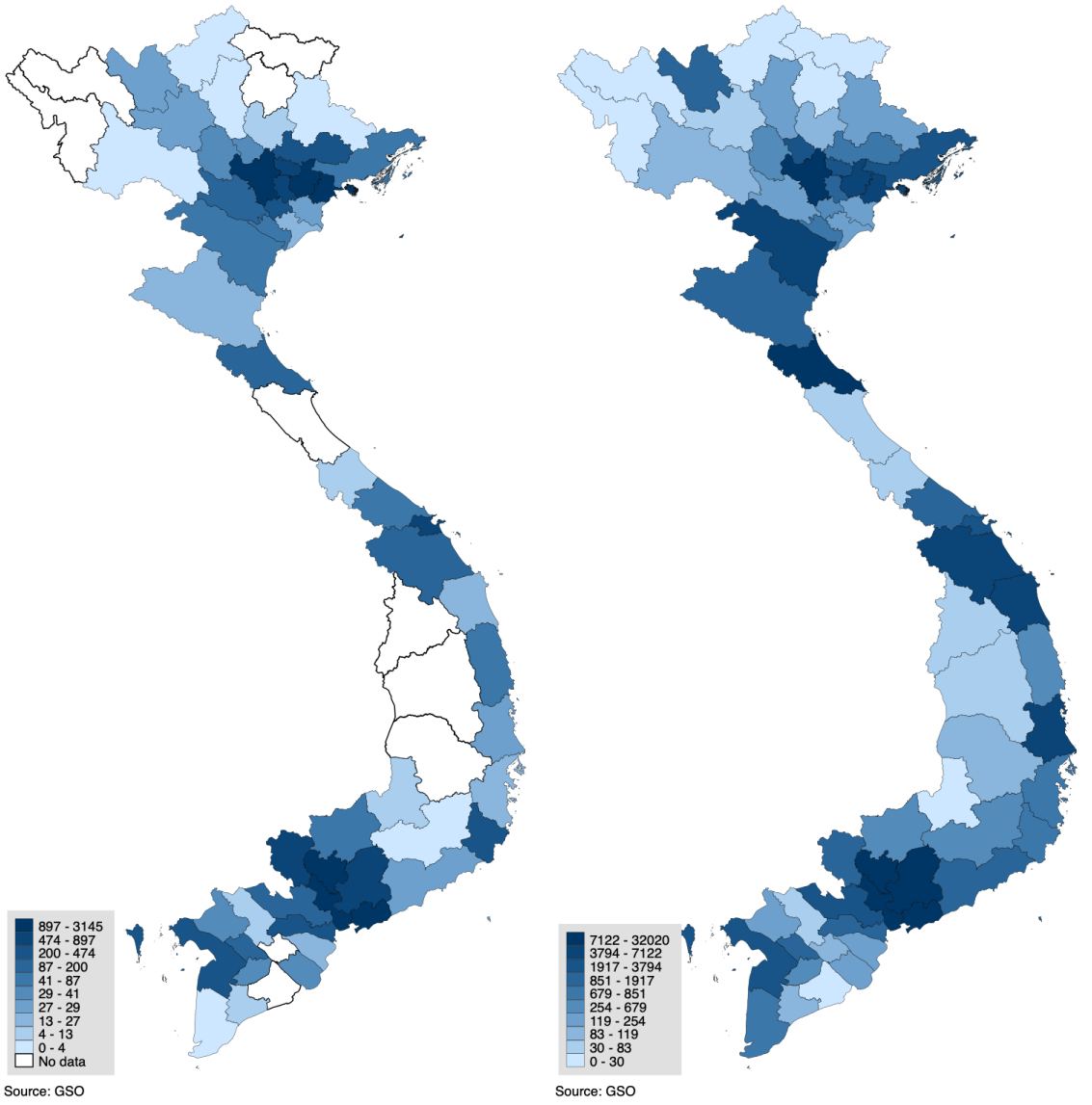

**(a)** Manufacturing




**(b)** Manufacturing in foreign sector

**Figure B6:** Share of female employment

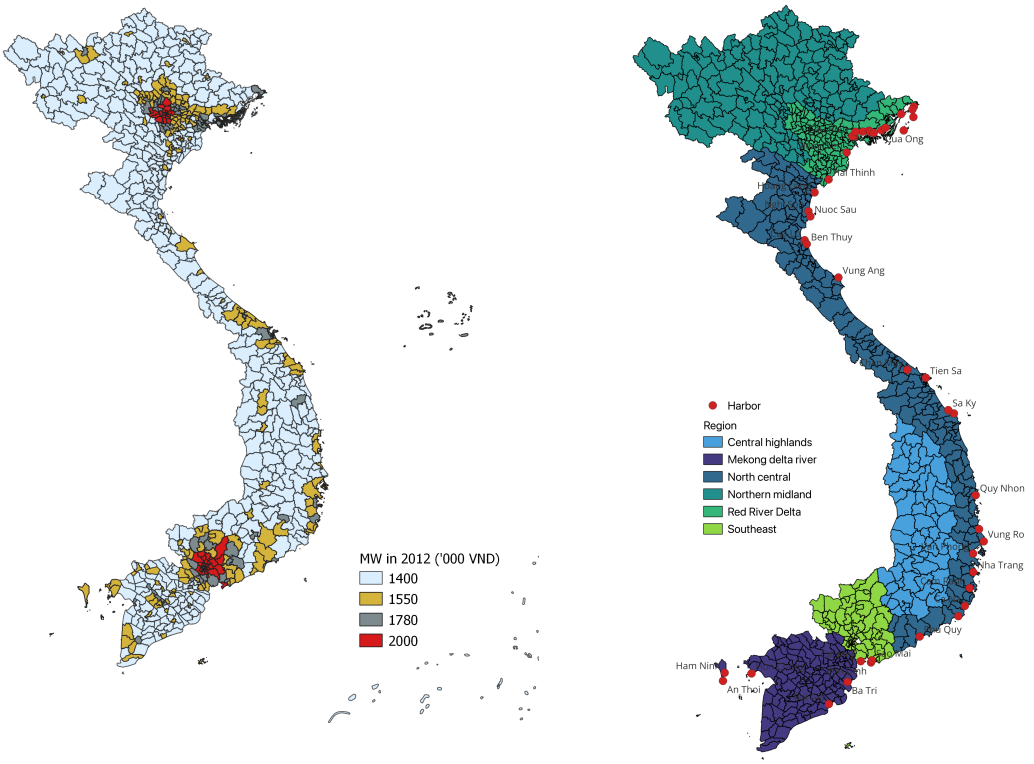
Source: VES 2006-2019. Notes: Markers on the graph are to highlight the industries in which females have a competitive advantage: textile, footwear, and fur.




**(a)** Total registered capital FDI **(b)** Total accumulated registered capital FDI

**Figure B7:** Total (accumulated) registered capital FDI in 2011

Source: GSO. Notes: The map represents the total (accumulated) registered capital FDI in 2011 in VND' 000. "No data" means that there is no FDI into that province in 2011.




**(a)** MW over years **(b)** Region

**Figure B8:** Vietnam Map

Source: Authors calculation. Notes: The left map illustrates the nominal minimum wage levels across districts in Vietnam (VND) in 2012. The right map highlights the six regions, namely the Central Highlands, Mekong River Delta, North Central, Northern Midland, Red River Delta, and Southeast.

**Table B3:** Accumulated number of FDI projects and accumulated total registered capital by Industry as of 2011

| Industry | No. of FDI Projects | Tot. Regt. Capital | % of capital |
|---|---|---|---|
| Agriculture, forestry and fishing | 495 | 3264.5 | 1.66 |
| Mining and quarrying | 71 | 3015.5 | 1.53 |
| Manufacturing | 7661 | 94675.8 | 48.04 |
| Electricity, gas, stream and air conditioning supply | 72 | 7391.6 | 3.75 |
| Water supply, sewerage, waste management and remediation activities | 27 | 2401.9 | 1.22 |
| Construction | 852 | 10324.1 | 5.24 |
| Wholesale and retail trade; repair of motor vehicles and motorcycles | 690 | 2119.1 | 1.08 |
| Transportation and storage | 321 | 3256.8 | 1.65 |
| Accommodation and food service activities | 319 | 10523.3 | 5.34 |
| Information and communication | 736 | 5709.5 | 2.90 |
| Financial, banking and insurance activities | 75 | 1321.6 | 0.67 |
| Real estate activities | 377 | 48155.9 | 24.43 |
| Professional, scientific and technical activities | 1162 | 976.1 | 0.50 |
| Administrative and support service activities | 107 | 188.0 | 0.10 |
| Education and training | 154 | 359.2 | 0.18 |
| Human health and social work activities | 76 | 1081.9 | 0.55 |
| Arts, entertainment and recreation | 131 | 3602.6 | 1.83 |
| Other service activities | 114 | 711.5 | 0.36 |

Sources: GSO. Registered capital is measured in millions of VND.

**Table B4:** Descriptive statistics of firms in high FDI regions before 2012

| | Full sample | | |
|---|---|---|---|
| | Domestic firms | Foreign firms | p-value |
| N | 982,743 (96.7%) | 33,547 (3.3%) | |
| Total employment - end of year | 25.233 (111.415) | 267.468 (741.144) | <0.001 |
| Age | 4.127 (5.234) | 6.078 (5.050) | <0.001 |
| **Size** | | | |
| < 20 employees | 0.794 (0.404) | 0.238 (0.426) | <0.001 |
| 20-100 employees | 0.161 (0.368) | 0.343 (0.475) | <0.001 |
| > 100 employees | 0.044 (0.205) | 0.419 (0.493) | <0.001 |
| Share of female workers | 0.364 (0.197) | 0.480 (0.251) | <0.001 |
| Log labor productivity (VA per worker) | 8.849 (0.767) | 9.566 (1.195) | <0.001 |
| Capital intensity | 0.214 (0.410) | 0.475 (0.499) | <0.001 |
| **Industry** | | | |
| Manufacturing | 0.175 (0.380) | 0.682 (0.466) | <0.001 |
| Services | 0.673 (0.469) | 0.264 (0.441) | <0.001 |
| Others | 0.153 (0.360) | 0.054 (0.227) | <0.001 |
| | Manufacturing | | |
| N | 171,572 (88.2%) | 22,872 (11.8%) | |
| Total employment - end of year | 55.882 (206.048) | 366.162 (877.648) | <0.001 |
| Age | 5.035 (6.477) | 6.347 (4.758) | <0.001 |
| **Size** | | | |
| < 20 employees | 0.626 (0.484) | 0.126 (0.331) | <0.001 |
| 20-100 employees | 0.264 (0.441) | 0.333 (0.471) | <0.001 |
| > 100 employees | 0.110 (0.313) | 0.542 (0.498) | <0.001 |
| Share of female workers | 0.365 (0.220) | 0.504 (0.260) | <0.001 |
| Log labor productivity (VA per worker) | 8.797 (0.731) | 9.321 (1.097) | <0.001 |
| Capital intensity | 0.454 (0.498) | 0.537 (0.499) | <0.001 |

Notes: Foreign firms (FDIs) include those with foreign ownership up to and exceeding 50%. Domestic firms include both state-owned firms (SOEs) and private formal firms. The sample is restricted to regions with high FDI, defined as those with accumulated FDI above the median level in 2011. Mean(SE): p-value from a pooled t-test. Sign. levels are: *p < 0.1, **p < 0.05, ***p < 0.01. Source: VES 2006-2011.

# C Additional heterogeneity analysis

**Table C5:** Employment effects on firm by gender and industry

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | High FDI region | | | | | | | |
| **Panel A: Female** | Services | | Manufacturing | | Services | | Above med. female share | | Below med. female share | |
| FDI firm x post | 0.193*** | 0.193*** | 0.170*** | 0.178*** | 0.203*** | 0.201*** | 0.216*** | 0.277*** | 0.154*** | 0.163*** |
| | (0.023) | (0.022) | (0.027) | (0.025) | (0.022) | (0.021) | (0.022) | (0.020) | (0.021) | (0.020) |
| Obs | 2765081 | 2765081 | 657588 | 657588 | 2398921 | 2398921 | 1370814 | 1370814 | 1352787 | 1352781 |
| Dep var pre-treat mean | 0.97 | 0.97 | 1.92 | 1.92 | 0.98 | 0.98 | 1.42 | 1.42 | 0.95 | 0.95 |
| **Panel B: Male** | | | | | | | | | | |
| FDI firm x post | 0.230*** | 0.225*** | 0.334*** | 0.310*** | 0.238*** | 0.230*** | 0.234*** | 0.240*** | 0.301*** | 0.302*** |
| | (0.023) | (0.024) | (0.028) | (0.026) | (0.022) | (0.023) | (0.020) | (0.019) | (0.028) | (0.025) |
| Obs | 2765032 | 2765032 | 657509 | 657509 | 2398873 | 2398873 | 1370734 | 1370734 | 1352733 | 1352727 |
| Dep var pre-treat mean | 1.49 | 1.49 | 2.48 | 2.48 | 1.48 | 1.48 | 1.48 | 1.48 | 2.22 | 2.22 |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ |
| Region-year FE | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes, separately for female and male employment. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. Source: VES 2006-2019.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Table C6:** Employment effects by firm characteristics

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Manufacturing | High FDI regions |
| FDI firm x post | 0.163*** | 0.282*** | 0.172*** |
| | (0.030) | (0.035) | (0.032) |
| FDI firm x post x Above median TFP firm | 0.087** | -0.010 | 0.080** |
| | (0.028) | (0.035) | (0.029) |
| Firm FE | ✓ | ✓ | ✓ |
| District FE | ✓ | ✓ | ✓ |
| N | 2791232 | 536473 | 2358115 |

Notes: The table reports estimates in equation 2.3 for impacts on firm-level outcomes. Clustered SE at district level in parentheses. All models control for firm and year fixed effects. Source: VES 2006-2019 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

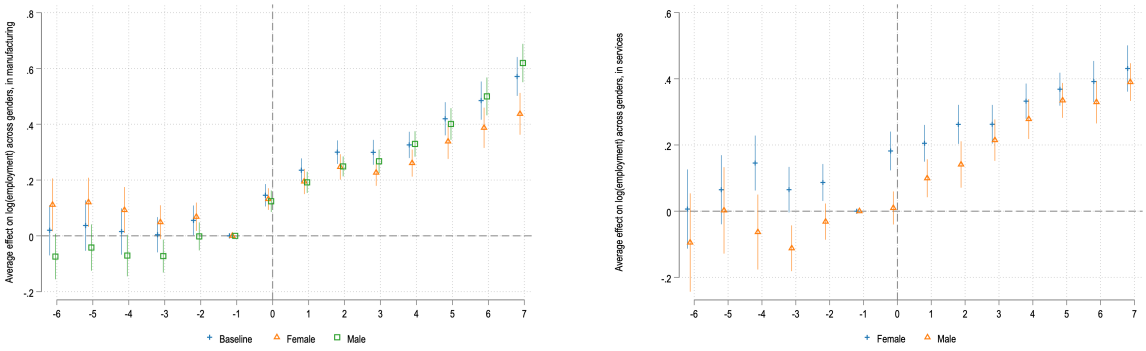



**Figure C9:** Effect on firm's employment by gender and industry

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

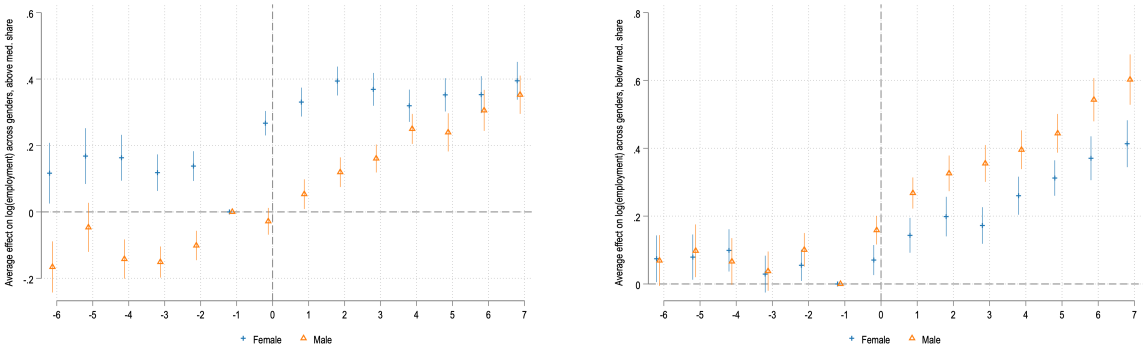



**Figure C10:** Effect on firm's employment by gender and firm type

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

# D Sensitivity checks

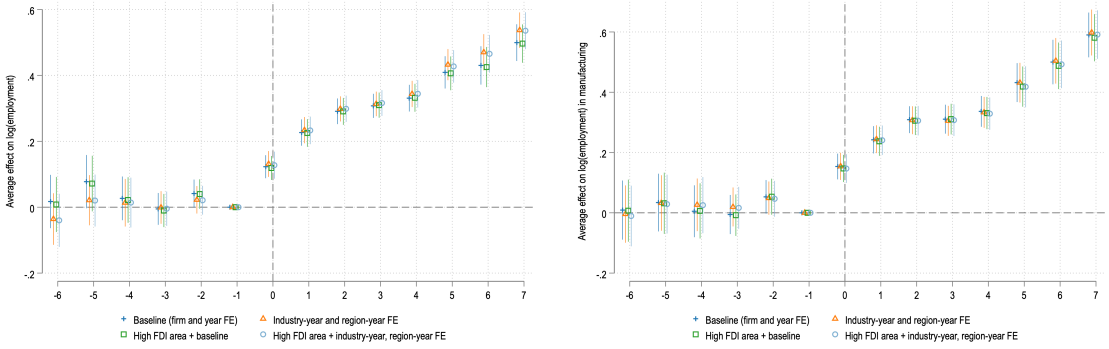
## D.1 Firm ownership

### Including only 100% foreign-owned firms

**Table D7:** Effects on total employment of firm, only including 100% foreign-owned firms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | | High FDI region | | | |
| Log(employment) | | | Manufacturing | | | Manufacturing | |
| FDI firm x post | 0.266*** | 0.298*** | 0.304*** | 0.268*** | 0.298*** | 0.299*** | 0.293*** |
| | (0.022) | (0.021) | (0.027) | (0.023) | (0.022) | (0.029) | (0.027) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | ✓ | | | ✓ | | ✓ |
| Region-year FE | | ✓ | | | ✓ | | ✓ |
| Obs | 4249397 | 4249397 | 771266 | 3617653 | 3617653 | 665929 | 665929 |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes. The sample only includes 100% foreign-owned firms and excludes firms with any positive share of foreign investment. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$




**(a)** Baseline **(b)** Manufacturing

**Figure D11:** Effects on firm's employment

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The sample only includes 100% foreign-owned firms and excludes firms with any positive share of foreign investment. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

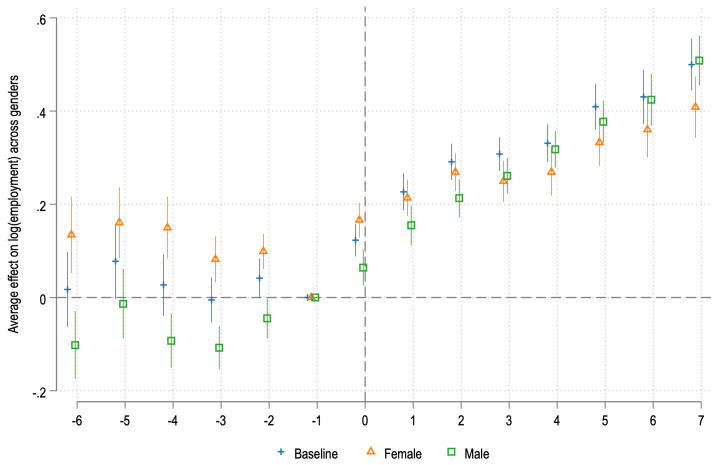



**Figure D12:** Effect on firm's employment by gender

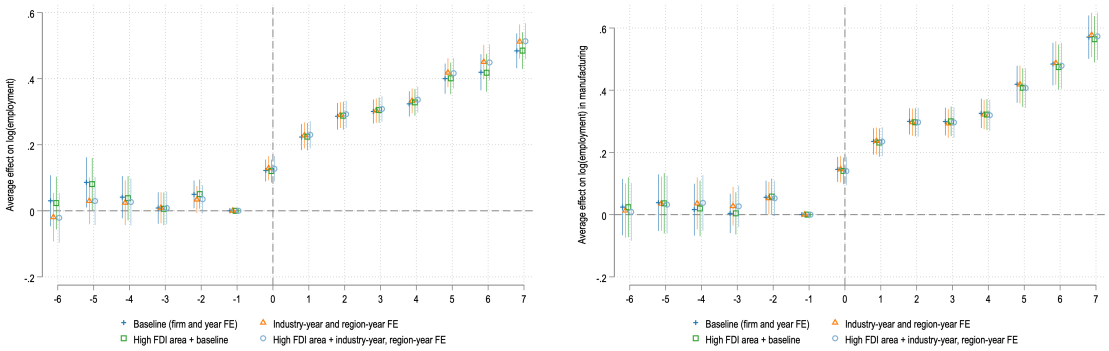
Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The sample only includes 100% foreign-owned firms and excludes firms with any positive share of foreign investment. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

## Excluding state-owned firms

**Table D8:** Effects on total employment of firm, excluding state-owned firms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | | High FDI region | | | |
| Log(employment) | | | Manufacturing | | | Manufacturing | |
| FDI firm x post | 0.250*** | 0.278*** | 0.288*** | 0.254*** | 0.279*** | 0.282*** | 0.277*** |
| | (0.021) | (0.019) | (0.026) | (0.021) | (0.020) | (0.027) | (0.026) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | ✓ | | | ✓ | | ✓ |
| Region-year FE | | ✓ | | | ✓ | | ✓ |
| Obs | 4252104 | 4252104 | 775720 | 3624134 | 3624134 | 670209 | 670209 |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes. The sample excludes state-owned firms. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$




**(a)** Baseline **(b)** Manufacturing

**Figure D13:** Effects on firm's employment

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The sample excludes state-owned firms. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

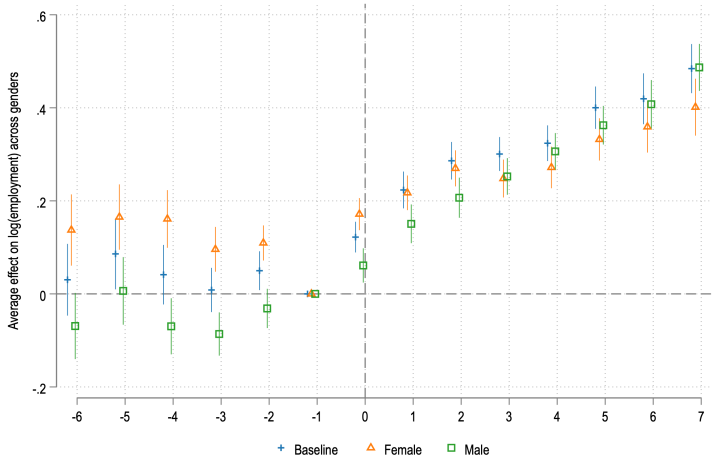
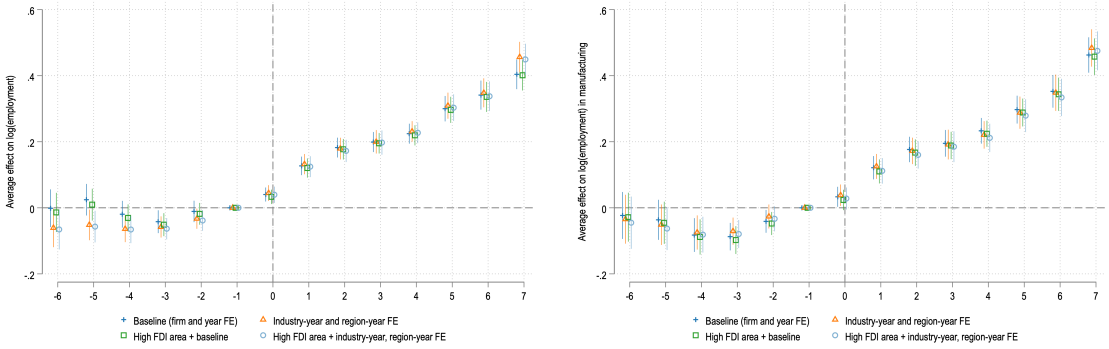



**Figure D14:** Effect on firm's employment by gender

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The sample excludes state-owned firms. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

## D.2 Balanced sample




**(a)** Baseline **(b)** Manufacturing

**Figure D15:** Effects on firm's employment

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The sample is balanced panel data. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

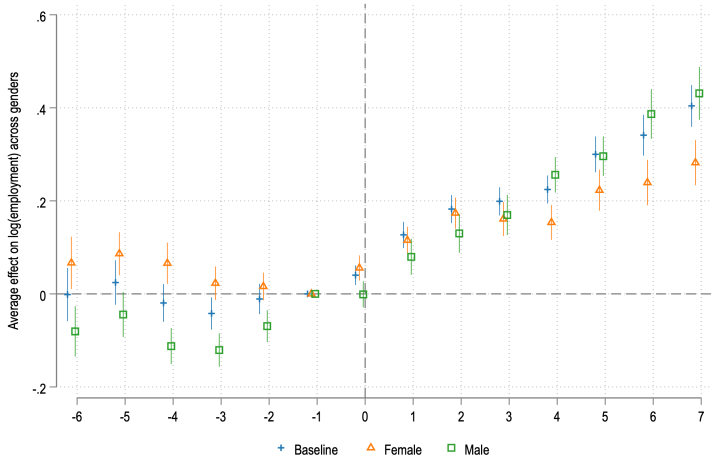



**Figure D16:** Effect on firm's employment

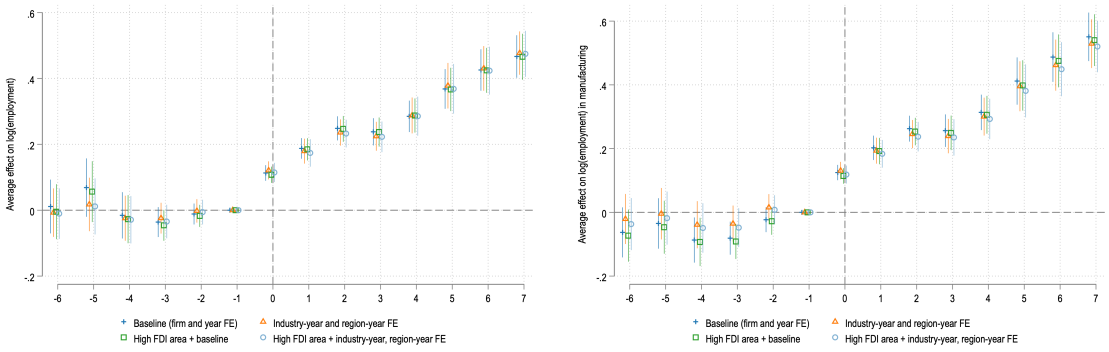
Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The sample is balanced panel data. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

## D.3 Potential outliers

**Table D9:** Effects on total employment of firm

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | | High FDI region | | | |
| | | | Manufacturing | | | Manufacturing | |
| FDI firm x post | 0.272*** | 0.272*** | 0.341*** | 0.280*** | 0.273*** | 0.340*** | 0.295*** |
| | (0.024) | (0.024) | (0.025) | (0.025) | (0.026) | (0.028) | (0.028) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry-year FE | | ✓ | | | ✓ | | ✓ |
| Region-year FE | | ✓ | | | ✓ | | ✓ |
| Obs | 2095049 | 2095049 | 421098 | 1499154 | 1499154 | 322247 | 322247 |

Notes: The table reports estimates in equation 2.1 for impacts on firm-level outcomes. The sample excludes Ha Noi and Ho Chi Minh City. Clustered SE at district level in parentheses. High FDI region is defined as one with accumulated FDI above the median level in 2011. Source: VES 2006-2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$




**(a)** Baseline **(b)** Manufacturing

**Figure D17:** Effects on firm's employment

Notes: The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. Ha Noi and Ho Chi Minh City are excluded from the analysis. The regression model includes firm and year fixed effects, and, depending on the specification, controls for industry x year and region x year fixed effects. Sources: VES 2006-2019

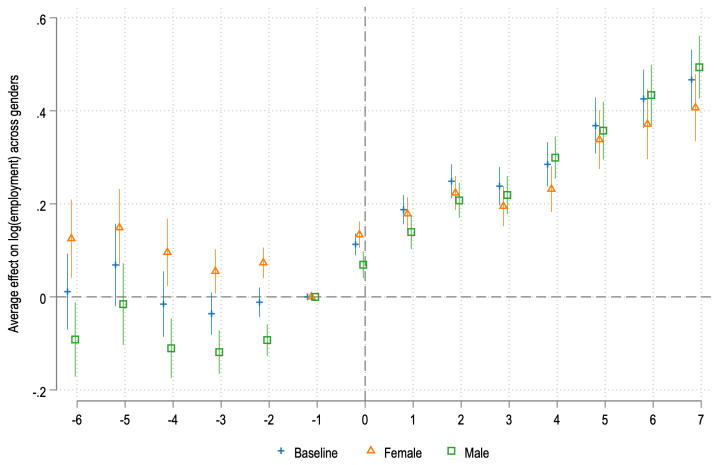



**Figure D18:** Effect on firm's employment by gender

The figure plots event-study estimates of the policy's effect on firm-level outcomes. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2006-2019

# E District level analysis

## E.1 Descriptive statistics

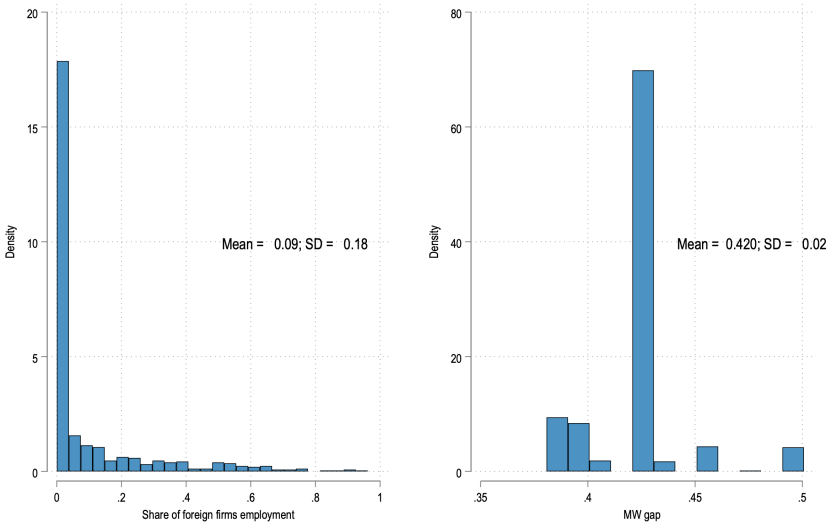



**Figure E19:** Distribution of two treatments

Notes: Both treatments: share of foreign firms employment and MW gap are calculated over years 2008-2010. Sources: VES 2008-2010

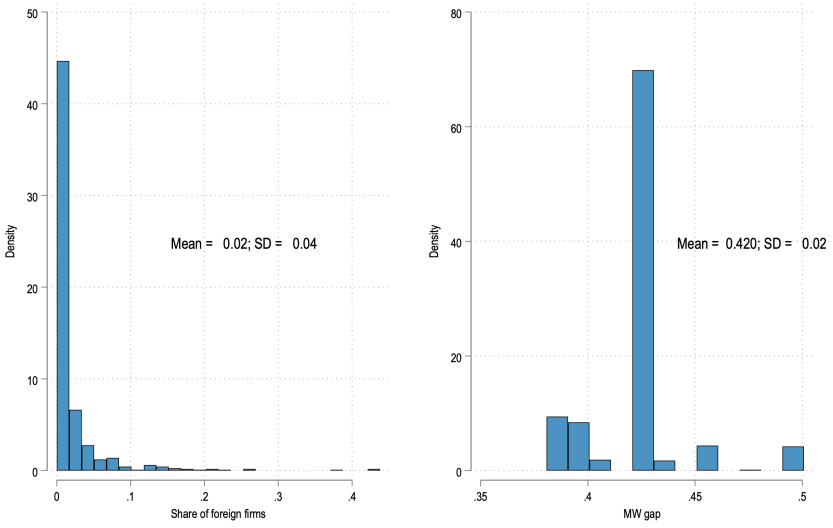



**Figure E20:** Distribution of two treatments

Notes: Both treatments: share of foreign firms and MW gap are calculated over years 2008-2010. Sources: VES 2008-2010

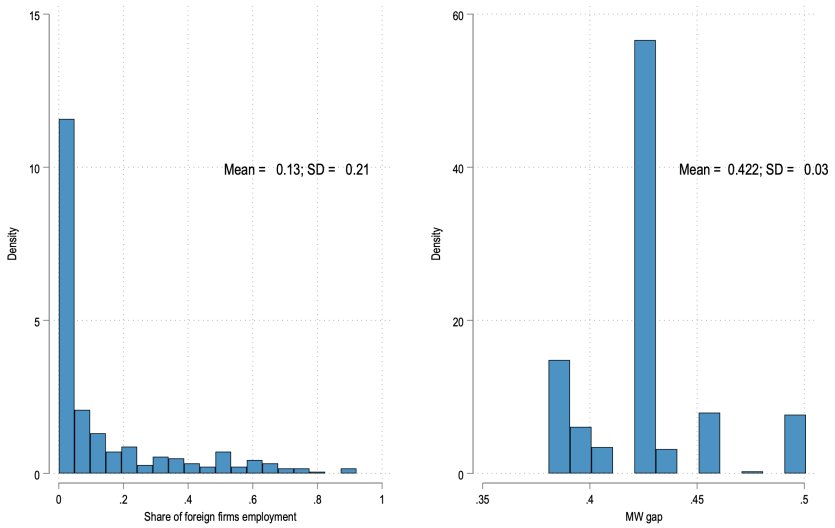



**Figure E21:** Distribution of two treatments in high FDI region

Notes: Both treatments: share of foreign firms employment and MW gap are calculated over years 2008-2010. Sources: VES 2008-2010

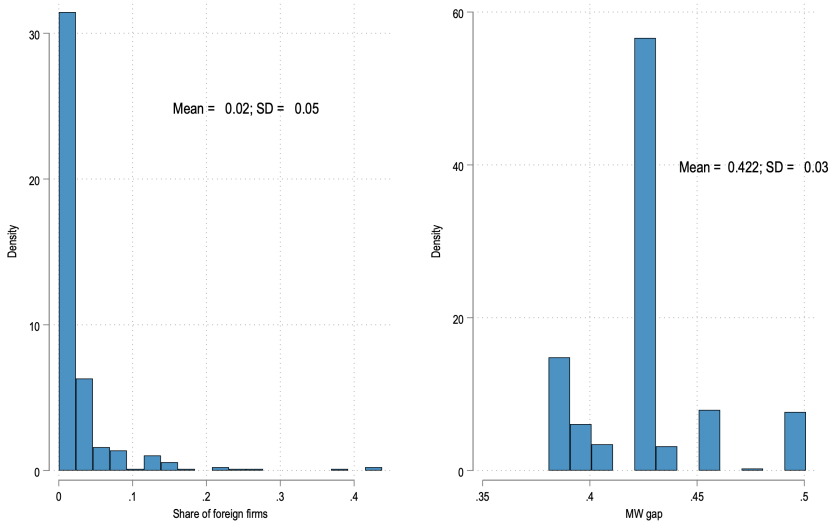
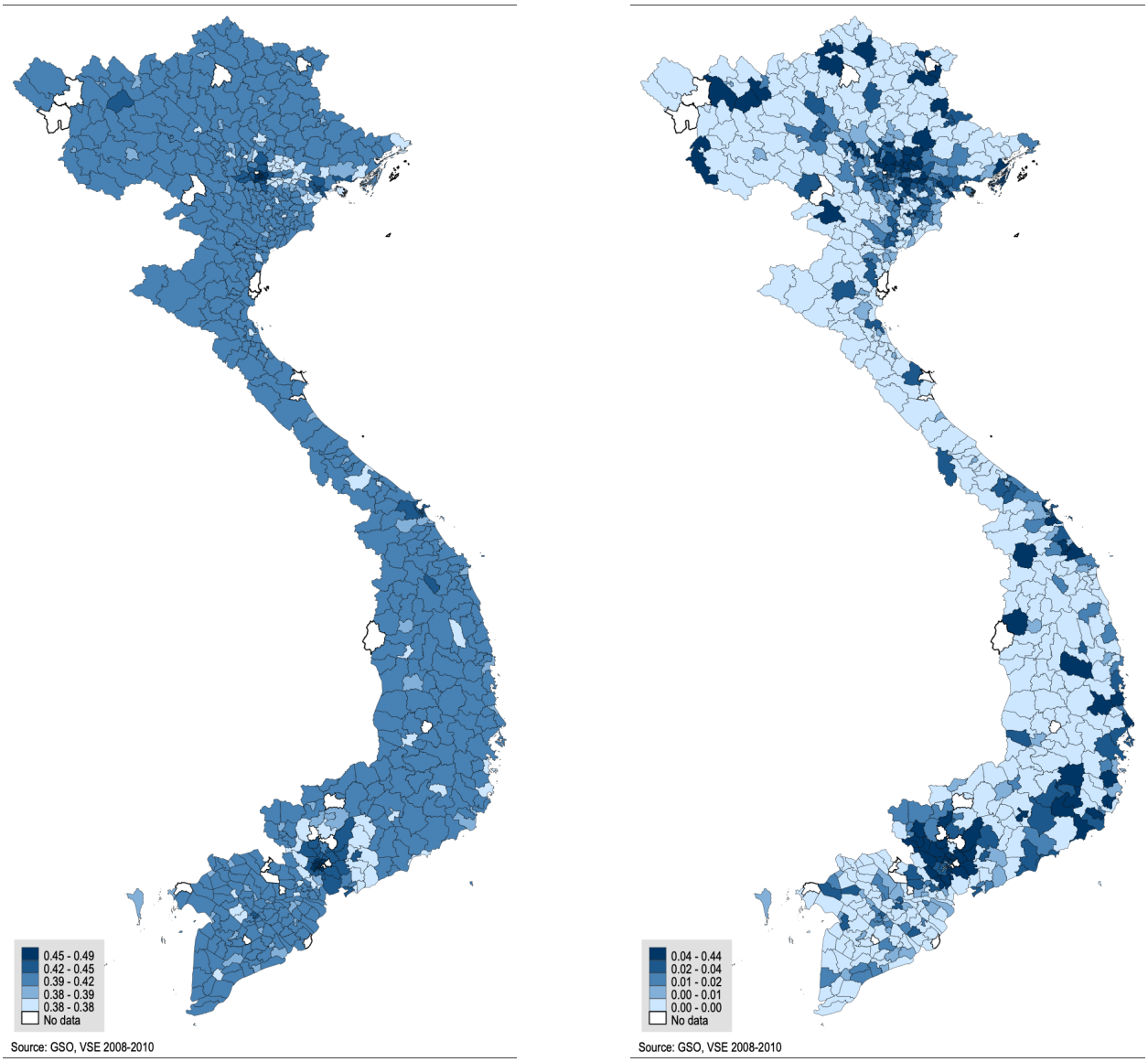



**Figure E22:** Distribution of two treatments in high FDI region

Notes: Both treatments: share of foreign firms and MW gap are calculated over years 2008-2010. Sources: VES 2008-2010




**(a)** MW gap **(b)** Share of foreign firms

**Figure E23:** Spatial distribution of two treatments

Notes: Both treatments: share of foreign firms employment and MW gap are calculated over years 2008-2010. Sources: VES 2008-2010

## E.2 Additional district level results

### Entry/exit rate

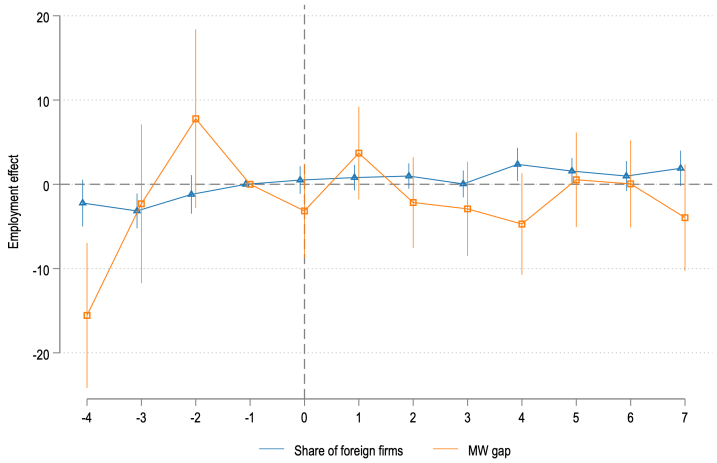
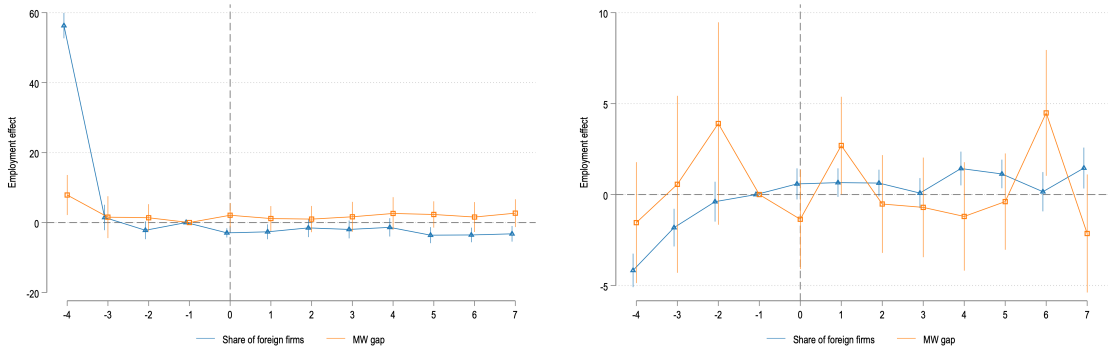



**Figure E24:** Effects on entrant rate at district level

Notes: The figure plots event-study estimates of the policy's effect on district-level outcomes in equation 2.9. Entrant rate at time $t$ is measured as the ratio between firms operating in time $t$ but not in time $t-1$ over total number of firms in district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019




**(a)** Foreign firms **(b)** Domestic firms

**Figure E25:** Effects on entrant rate at district level

Notes: The figure plots event-study estimates of the policy's effect on district-level outcomes in equation 2.9. Entrant rate at time $t$ is measured as the ratio between firms operating in time $t$ but not in time $t-1$ over total number of firms in district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

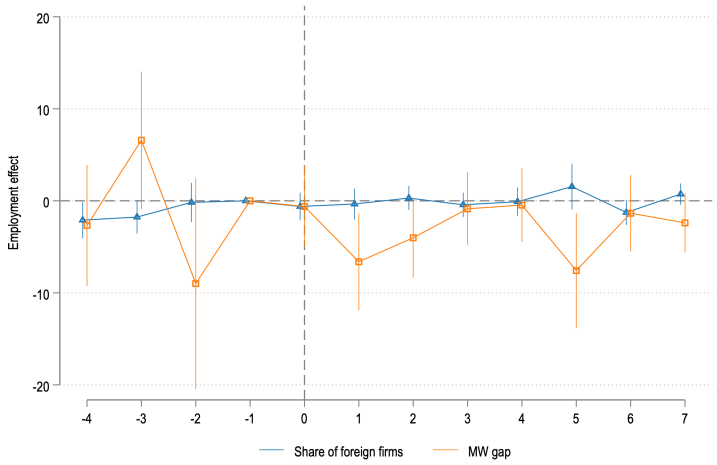
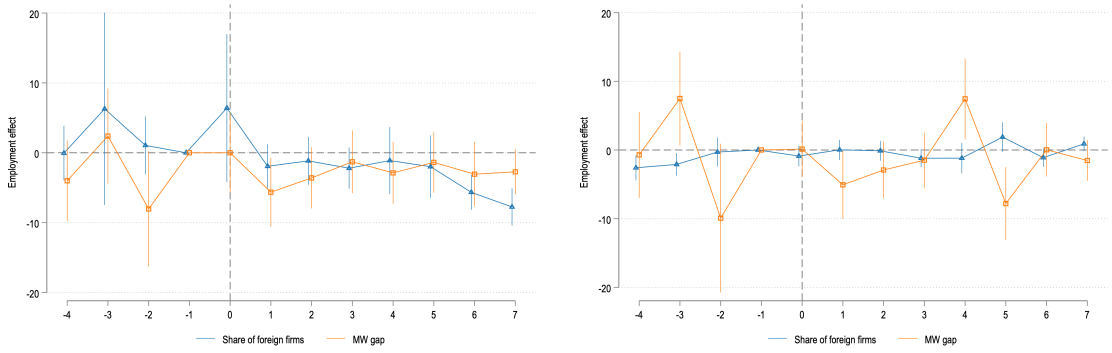



**Figure E26:** Effects on exit rate at district level

Notes: The figure plots event-study estimates of the policy's effect on district-level outcomes in equation 2.9. Exit rate at time $t$ is measured as the ratio between firms operating in time $t$ but not in time $t+1$ over total number of firms in district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019




**(a)** Foreign firms **(b)** Domestic firms

**Figure E27:** Effects on exit rate at district level

Notes: The figure plots event-study estimates of the policy's effect on district-level outcomes in equation 2.9. Exit rate at time $t$ is measured as the ratio between firms operating in time $t$ but not in time $t+1$ over total number of firms in district $d$ at time $t$.The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

## Manufacturing

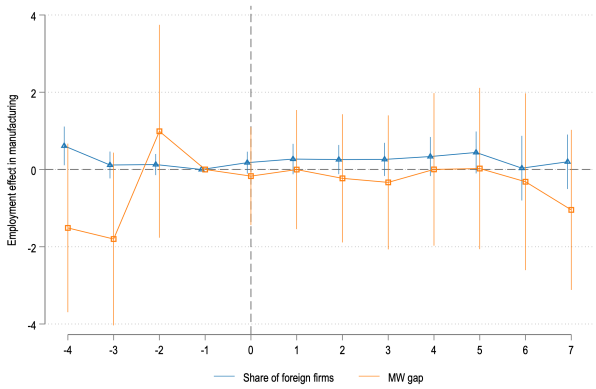
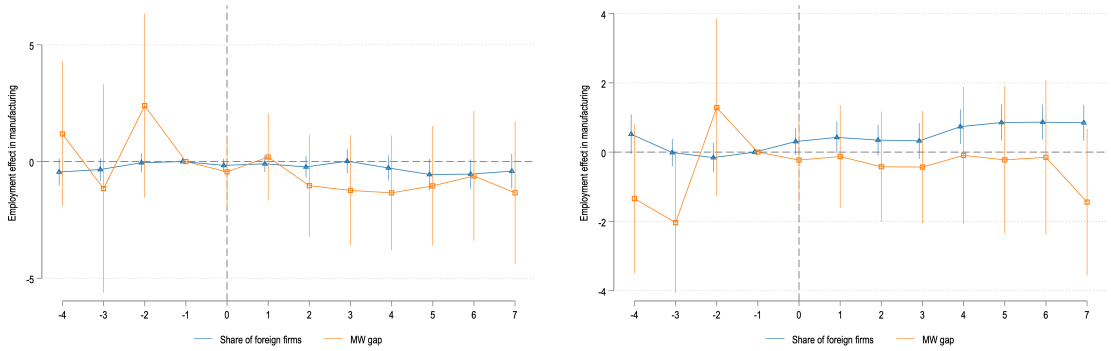



**Figure E28:** Effects on district's employment within manufacturing

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019




**(a)** Foreign firms **(b)** Domestic firms

**Figure E29:** Effects on employment by ownership within manufacturing

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

## Services

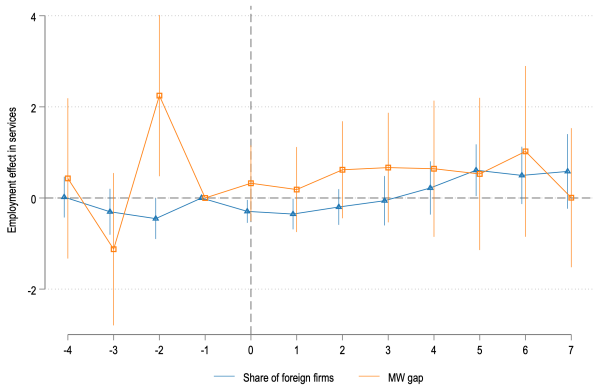
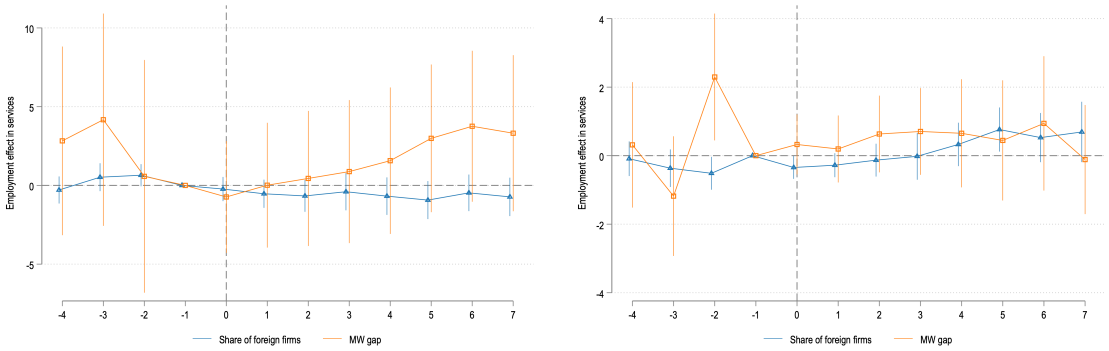



**Figure E30:** Effects on district's employment within services

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019




**(a)** Foreign firms **(b)** Domestic firms

**Figure E31:** Effects on employment by ownership within services

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

## Retail

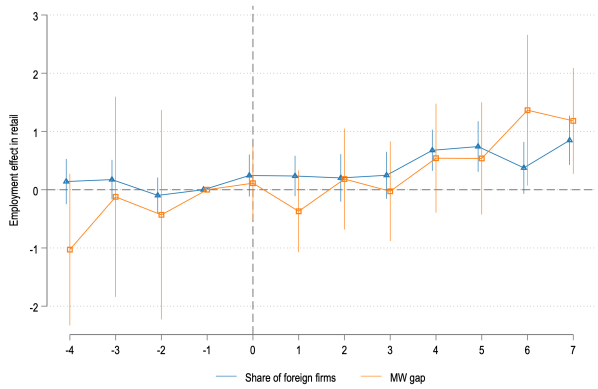
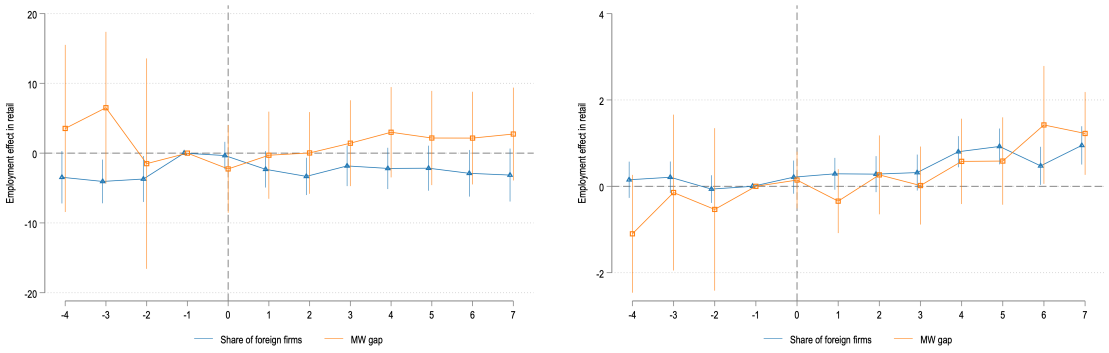



**Figure E32:** Effects on district's employment within retail

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019




**(a)** Foreign firms **(b)** Domestic firms

**Figure E33:** Effects on employment by ownership within retail

The figure plots event-study estimates of the policy's effect on district-level outcomes. The outcome is the logarithm of employment of district $d$ at time $t$. The 95% confidence intervals are displayed and standard errors are clustered at the district level. The regression model includes district and year fixed effects. Sources: VES 2008-2019

# Chapter 3

# The subjective well-being effects of short-term displacement: Evidence from inter-ethnic conflict in Kyrgyzstan

*Co-authored with Azizbek Tokhirov and Riga Qi (CERGE-EI)*

## 3.1 Introduction

Inter-ethnic conflicts typically lead to forced displacement, as affected populations flee their homes to escape targeted violence (Lischer, 2007). The conflict between Kyrgyz and Uzbeks that struck Kyrgyzstan in June 2010 resulted in 470 deaths, mostly among the Uzbek minority, and temporarily displaced approximately 200,000 people (Hager et al., 2019; Hennicke & Brück, 2022). The conflict was chaotic but short-lived and lasted only weeks, allowing the majority of the displaced to return home quickly (Akiner, 2016). Using both country-representative and panel data from surveys conducted four years before the conflict, three months after it, and up to nine years later, this study is among the first to document the negative associations and transitional dynamics of short-term forced displacement on returnees.

The main concern is that, after experiencing a traumatic event, victims are normally reluc-

tant to reveal economic information due to stress, distrust, or fear of repercussions (Fujii, 2010), which makes the true impact harder to estimate. We instead focus on satisfaction-related questions about various aspects of life that better capture post-trauma well-being changes while avoiding the issue of information disclosure. Another concern is the potential selection bias among those who were displaced but later decided to return (Kondylis, 2010; Shemyakina & Plagnol, 2013). We address this by employing instrumental variable (IV) and difference-in-differences (DD) approaches. The former relies on distance instruments, which we use to capture proximity to destruction and hiding places, while the latter focuses on the average well-being at the community level before and after displacement.

We find that displacement is associated with persistently lower well-being. The affected domains include satisfaction with health, security, dwelling, and future prospects. In line with the nature of the conflict, in which ethnic Kyrgyz from neighboring villages motivated by patriotic fervor attacked Uzbeks and their property following the political vacuum created by the president's ousting two months earlier (Hager et al., 2019), we conduct across- and within-ethnicity heterogeneity analyses and find that the negative association with well-being is observed only among displaced Uzbeks, both men and women. Support from close friends and family members plays a key role in mitigating the loss in well-being, indicating that displaced Uzbeks who lacked support during displacement experience substantially larger well-being losses. We find that the well-being damage persists over several years, with full recovery observed only after around six years. Another characteristic of the conflict, namely that it subsided as suddenly as it had begun, without further repercussions (Akiner, 2016), is reflected in our findings, which show that perceptions of health and security recovered faster than satisfaction with dwelling and future outlook. We interpret these transitional dynamics as being consistent with rebounding trust and a temporary increase in spending.

Our paper contributes to three strands of literature: the consequences of inter-ethnic conflict (Cordell & Wolff, 2009), the impact of displacement (Lischer, 2007), and the trajectory of well-being recovery after shocks (Lucas, 2007). For the first two strands, we identify the affected areas and explore the potential mechanisms driving these effects. For the latter, we

provide new evidence to address the debate between cumulative causation (Myrdal, 1957) and hedonic adaptation (Brickman & Campbell, 2010), with results that are consistent with hedonic adaptation.

## 3.2 Data and empirical specification

In this study, we integrate data from three sources: the 2006 Life in Transition (LiT) survey, the 2009 Kyrgyz Census, and the 2010–2019 Life in Kyrgyzstan (LiK) study. The LiK dataset serves as the primary source, collected annually every fall since its inception in September 2010 until 2013, and again in the fall of 2016 and 2019, while we use the LiT survey and the Kyrgyz Census to construct control variables and perform robustness checks. For 2010, we have 2 main samples: the unrestricted sample, based on country-representative data, and the restricted sample, based on the balanced panel. The latter spans through 2019. We also have 2006 and 2010 repeated cross-sectional samples, created for DD estimations based on communities tracked over the years.

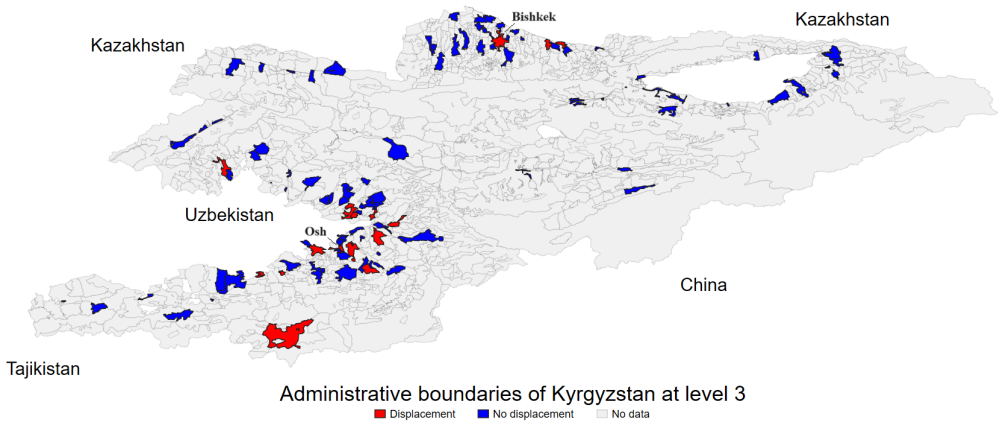



**Figure 3.1:** Displacement patterns

Notes: This figure depicts the distribution of the displacement patterns in Kyrgyzstan based on the 2010 LiK data.

The treatment variable is binary and based on the individual's experience of displacement during the 2010 ethnic conflict, asked retrospectively. We use definitions of displacement at both the individual and community levels. To construct the control group, we exclude

individuals who are family members of, or personally acquainted with, those displaced.[1] We present the distribution of displacement-related variables in Table 3.1. In line with the nature of the conflict, the table indicates that only 9 individuals who were neither Kyrgyz nor Uzbek were displaced. It also shows the age of the displaced, the short-term nature of the displacement, and the absence of significant gender-based differences in displacement. We further disaggregate the data by displacement experience and demonstrate that Uzbeks were more severely affected because they were more likely to stay without friends or family either in a displacement camp or abroad than Kyrgyz.

**Table 3.1:** Displacement statistics

| | *Ky. men* | *Ky. women* | *Uz. men* | *Uz. women* | *Other* |
|---|---|---|---|---|---|
| Age | 34.45 | 31.38 | 42.86 | 38.57 | 39.00 |
| # weeks of Displacement | 1.03 | 0.75 | 1.06 | 1.00 | 0.78 |
| Stayed w. friends & family | 30 | 55 | 19 | 38 | 4 |
| Stayed w/o friends & family | 1 | 0 | 17 | 38 | 5 |

Notes: This table provides descriptive statistics, with the first two rows showing average values and the last two indicating the number of people.

In Table 3.2, we show that, prior to displacement, the communities (at administrative level 2) that experienced displacement were comparable to those without displacement in 2006 and 2009, with the exception of ethnic composition. This aligns with Hennicke and Brück (2022), who finds that ethnic inequalities intensified displacement during the 2010 ethnic conflict.

For the outcome variable, we use both aggregated and individual measures of satisfaction, assessed on an ordinal scale. The satisfaction domains include health, job, household and personal income, dwelling, family life, children's education quality, security, and children's future. To obtain the aggregated measure, we averaged the responses across 9 domains and rounded the result to the nearest integer. We present the average well-being differences between displaced and non-displaced in Figure 3.2. The baseline comparisons indicate that

[1]The survey's treatment-related questions are in Appendix Figure A1, while the location of the treated individuals is shown in Figure 3.1.

**Table 3.2:** Descriptive statistics and balancing tests at community level

| | *Displaced* | | | *Non − displaced* | | | Absolute |
|---|---|---|---|---|---|---|---|
| | N | Mean | St. Dev. | N | Mean | St. Dev. | difference |
| **2006 LiTs:** | | | | | | | |
| Kyrgyz population | 6 | 0.564 | 0.278 | 23 | 0.818 | 0.215 | 0.255** |
| Uzbek population | 6 | 0.260 | 0.263 | 23 | 0.026 | 0.083 | 0.234*** |
| Education level: | | | | | | | |
| Secondary | 6 | 0.525 | 0.093 | 23 | 0.473 | 0.043 | 0.052 |
| Higher | 6 | 0.155 | 0.135 | 23 | 0.175 | 0.133 | 0.020 |
| Labor force participation | 6 | 0.602 | 0.102 | 23 | 0.488 | 0.224 | 0.114 |
| Consumption per capita (in logs) | 6 | 6.607 | 0.231 | 23 | 6.610 | 0.210 | 0.003 |
| General trust | 6 | 2.336 | 0.579 | 23 | 2.140 | 0.563 | 0.195 |
| **2009 Census:** | | | | | | | |
| Kyrgyz population | 6 | 0.578 | 0.085 | 23 | 0.842 | 0.169 | 0.264*** |
| Uzbek population | 6 | 0.298 | 0.135 | 23 | 0.048 | 0.094 | 0.251*** |
| Education level: | | | | | | | |
| Secondary | 6 | 0.439 | 0.027 | 23 | 0.444 | 0.036 | 0.005 |
| Higher | 6 | 0.065 | 0.029 | 23 | 0.076 | 0.041 | 0.011 |
| Labor force participation | 6 | 0.425 | 0.053 | 23 | 0.436 | 0.034 | 0.011 |
| Population (in logs) | 6 | 11.762 | 0.888 | 23 | 11.283 | 0.546 | 0.480 |
| % of who moved in $\leq$ 1 year ago | 6 | 0.028 | 0.021 | 23 | 0.026 | 0.025 | 0.001 |

Note: This table provides descriptive statistics and the results of balancing tests at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$ with p-values based on two-way t-tests.

displacement caused a significant decrease in subjective well-being, but its negative effects diminished over time. In the non-DD samples, we measure the outcome on the original 0–10 LiK scale, whereas in the DD samples, we measure it on a 1–5 scale (due to the nature of the LiT well-being measure).[2]

**Table 3.3:** Descriptive statistics at individual level

| | $Displacedcommunity$ | | | $Non-displacedcommunity$ | | | Absolute |
|---|---|---|---|---|---|---|---|
| | N | Mean | St. Dev. | N | Mean | St. Dev. | difference |
| Age | 207 | 36.807 | 14.257 | 6,968 | 40.519 | 16.535 | 3.712*** |
| Man | 207 | 0.338 | 0.474 | 6,968 | 0.469 | 0.499 | 0.131*** |
| Kyrgyz | 207 | 0.416 | 0.494 | 6,968 | 0.676 | 0.468 | 0.260*** |
| Uzbek | 207 | 0.541 | 0.500 | 6,968 | 0.094 | 0.292 | 0.447*** |
| Education: | | | | | | | |
| with Prim. tech./Sec. | 207 | 0.628 | 0.485 | 6,968 | 0.696 | 0.460 | 0.068** |
| with Higher | 207 | 0.189 | 0.392 | 6,968 | 0.159 | 0.366 | 0.030 |
| Marital status: | | | | | | | |
| Single | 207 | 0.029 | 0.168 | 6,968 | 0.050 | 0.218 | 0.021 |
| Married | 207 | 0.744 | 0.438 | 6,968 | 0.672 | 0.470 | 0.072** |
| Household size | 207 | 5.425 | 1.916 | 6,968 | 5.205 | 2.310 | 0.221 |
| Has a child | 207 | 0.952 | 0.215 | 6,968 | 0.947 | 0.224 | 0.005 |
| Household head | 207 | 0.304 | 0.461 | 6,968 | 0.360 | 0.480 | 0.056 |
| Months worked last 12 months | 207 | 5.459 | 5.672 | 6,968 | 6.399 | 5.706 | 0.940** |
| Household income (in logs) | 207 | 7.342 | 1.014 | 6,968 | 7.611 | 0.846 | 0.269*** |
| Household expenses (in logs) | 207 | 7.094 | 0.792 | 6,968 | 6.867 | 0.851 | 0.228*** |
| General trust | 207 | 2.918 | 1.009 | 6,968 | 3.051 | 0.890 | 0.133** |
| Risk level | 207 | 3.990 | 3.246 | 6,968 | 3.803 | 3.386 | 0.187 |

Note: This table provides descriptive statistics and the results of balancing tests at the individual level based on the 2010 LiK. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$ with p-values based on two-way t-tests.

Given the binary nature of the treatment and ordered outcome, we employ ordered probit as the primary estimation method:

$$Pr(Well-being_j = i) = Pr(k_{i-1} < \beta Displ_j + X'\theta + u_{ij} \leq k_i), \tag{3.1}$$

where $Well-being_j$ is the ordinary outcome for individual $j$, $k_i$ ... $k_I$ are 5 (DD) or 11

[2]The survey questions are in Appednix Figure A2.

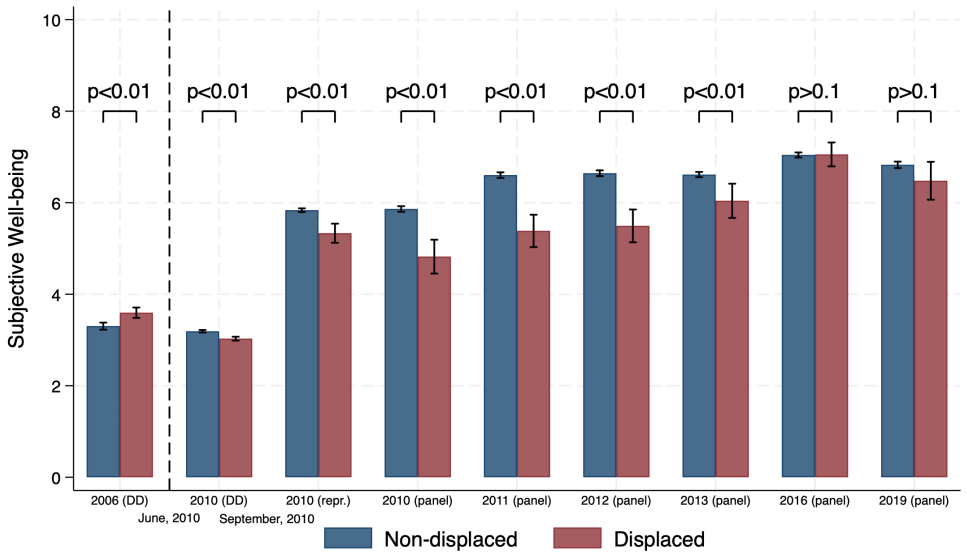



**Figure 3.2:** Subjective well-being of the displaced and non-displaced over time

Note: This figure depicts well-being differences among displaced and non-displaced across years with p-values based on two-way t-tests.

(non-DD) cut-points, *Displ* is the binary treatment, $X$ is the vector of controls, and $u_{ij}$ is assumed to be normally distributed.

Since displacement may have been determined non-randomly, we also employ IV and DD estimations. Our DD approach relies on the assumption that, in the absence of the conflict, aggregate well-being patterns in communities with and without displaced individuals would have followed parallel trends. This assumption is supported by the absence of significant pre-displacement differences, as demonstrated in Table 3.2.

Following Hager et al. (2019), who employed the proximity of neighborhoods to armored military vehicles as an instrument for the community-level measure of the 2010 ethnic conflict, we use dwelling location as an instrument for individual-level displacement. We consider two distance-based measures at the household level to instrument individual-level displacement: (1) the average distance to the main road, community town hall, and agricultural market, and (2) the average distance to the nearest school and hospital.

The rioters who caused the conflict in the neighborhoods typically came from outside and

were heading toward the city center, destroying properties along the way (Hager et al., 2019). Therefore, the first instrument is expected to capture the degree of connectivity of the household to the destruction path. Conversely, school and hospital buildings offered opportunities for individuals to seek refuge. To address potential concerns regarding the exclusion restriction, we first restrict the sample to individuals who were born in the dwelling where they currently reside, ensuring that they are accustomed to the amenities and that their direct effects on well-being are not pronounced. More formally, in Table 3.4, we show that when the instruments are averaged and traced back to 2006, they are not directly associated with well-being measures but do predict displacement occurrence at the community level.

**Table 3.4:** Displacement, well-being, and distances

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Well-being in 2006 | | | Displaced in 2010 | | |
| Distance 1 | 0.017 | | 0.003 | -0.064** | | -0.068** |
| (hall/road/mkt.) | (0.037) | | (0.049) | (0.026) | | (0.030) |
| Distance 2 | | 0.027 | 0.024 | | -0.042* | 0.007 |
| (school, hospital) | | (0.042) | (0.06) | | (0.023) | (0.026) |
| Estimation | OLS | OLS | OLS | OLS | OLS | OLS |
| R-squared | | | 0.02 | | | 0.15 |
| Controls | No | No | No | No | No | No |
| Observations | 29 | 29 | 29 | 29 | 29 | 29 |

Note: This table presents the results of community-level OLS estimations with robust SE. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

## 3.3 Results

### 3.3.1 Baseline analysis

Table 3.5 presents the short-term well-being associations of displacement. We first report the OLS specification with a continuous outcome variable. After accounting for baseline individual controls and community fixed effects, the findings indicate that displacement is

associated with a decline in well-being exceeding 6%, relative to the mean well-being score of 5.83 observed among the non-displaced subsample. In the ordered probit specifications in Columns (2) and (3), we replace community fixed effects with community-level controls, including exposure to conflict and pre-riot socio-economic characteristics, to address data saturation. We also account for individual-level variables such as risk and trust preferences, as well as household income (Column 2) and expenses (Column 3). The negative effects of displacement remain statistically significant and are more pronounced in areas exposed to the ethnic conflict.

In Columns (4) and (5), we report the instrumented effects of displacement, using the limited-information maximum likelihood estimator with a probit link and the ordered probit in the final stage (Roodman, 2011). We select this estimator to account for the binary nature of the treatment and the ordinal nature of the outcome. Consistent with expectations, individuals residing farther from areas of destruction and potential hiding places are less likely to experience displacement. Moreover, since we control for a variety of economic variables and restrict the sample to those who never changed their place of living, the distance measures are plausibly expected to influence well-being primarily through their effects on displacement. In the second stage, the effects of displacement remain negative; however, the magnitude is larger. Finally, in Column (6), the DD estimations also indicate that the effects of displacement on well-being are negative.

To show that the results are not driven by the aggregation, we analyze the original well-being data in Figure 3.3. In these specifications, we use baseline controls, excluding those that might be correlated with the respective well-being dimensions. Thus, instead of the varying controls as in Table 3.5, we compare restricted and non-restricted sample results. DD estimations are absent due to a lack of disaggregated data. The graph shows that displacement is associated with increased dissatisfaction in several domains, including health, housing, security, and prospects for children's future.[3] Conversely, satisfaction with economic

[3]We attribute the large standard errors in Figure 3.3 for satisfaction with family income and life to the non-linear estimation and limited variation within these subcategories.

**Table 3.5:** The short-term effects of displacement

| | Dependent variable: Well-being | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | | | | DD |
| Displaced | -0.347*** | -0.339*** | -0.432*** | -1.416*** | -1.494*** | |
| | (0.116) | (0.095) | (0.111) | (0.196) | (0.221) | |
| Displaced × Post | | | | | | -0.628*** |
| | | | | | | (0.238) |
| Riots/destruction/ | | -0.273*** | -0.257** | -0.259*** | -0.299*** | |
| violence | | (0.103) | (0.102) | (0.099) | (0.109) | |
| | | | | Displaced | Displaced | |
| Distance 1 | | | | -0.114*** | -0.230*** | |
| (hall/road/market) | | | | (0.029) | (0.038) | |
| Distance 2 | | | | -0.222*** | -0.153*** | |
| (school, hospital) | | | | (0.035) | (0.039) | |
| Estimation | OLS | Oprobit | Oprobit | CMP | CMP | Oprobit |
| First stage F-stat. | | | | 105.16 | 74.74 | |
| Baseline ind. ctrls | Yes | Yes | Yes | Yes | Yes | Yes |
| Community ctrls | No | Yes | Yes | Yes | Yes | No |
| Extra ind. ctrls | No | No | Yes | Yes | Yes | Yes |
| Community FE | Yes | No | No | No | No | Yes |
| Region ×urb. FE | No | Yes | Yes | Yes | Yes | No |
| Individuals | 7,175 | 7,175 | 7,175 | 7,175 | 5,186 | 6,388 |
| Communities | 49 | 49 | 49 | 49 | 49 | 29 |

Note: This table presents the results of OLS, ordered probit and conditional (recursive) mixed process estimations with SE clustered at the community level. The description of control variables and full regression results are in Table B1 in the Appendix. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

domains, children's education, and family life appears largely unaffected by displacement.

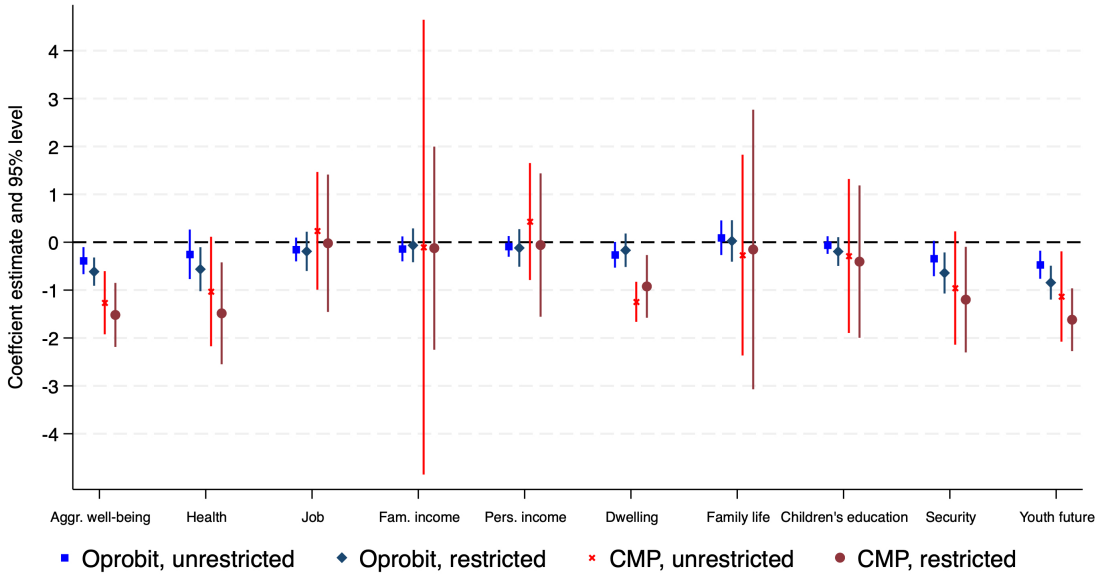



**Figure 3.3:** The disaggregated short-term effects of displacement

Note: This figure depicts the results of restricted (living where born) and unrestricted estimations with baseline controls and SE clustered at the community level. The description of the full regression results for the preferred specification (CMP, restricted) are in Tables B2 and B2 in the Appendix.

### 3.3.2 Heterogeneity analysis

We present the results of the heterogeneity analysis in Table 3.6. Given the inter-ethnic nature of the conflict, we first separately compare displaced Kyrgyz and Uzbek individuals with their non-displaced counterparts. The well-being of displaced Kyrgyz individuals converges to that of non-displaced individuals within less than three months following the displacement. The well-being of displaced Uzbek individuals remains lower. We further restrict the sample to Uzbeks only and separately compare displaced individuals who stayed with family or friends and those who did not stay with family or friends to non-displaced individuals. This suggests that the negative effects are observed only in the latter group. The final disaggregation of individuals who did not stay with family or friends indicates that the displacement associations for men and women are similar.

In Figure 3.4, we present evidence on the dynamics of the estimated associations over time.

**Table 3.6:** The short-term effects of displacement: heterogeneity analyses

| | (1) Kyrgyz vs. All | (2) Uzbek vs. All | (3) Uz. w. sup. vs. Uzbeks | (4) Uz. alone vs. Uzbeks | (5) Uz. alone vs. Uzbeks | (6) Uz. alone vs. Uzbeks |
|---|---|---|---|---|---|---|
| Displaced | -0.016 | -0.645*** | -0.049 | -0.985*** | -0.920** | -0.965*** |
| | (0.152) | (0.224) | (0.473) | (0.273) | (0.395) | (0.240) |
| $H_0$: $\beta_1 = \beta_2$ | $p$=0.037 | | $p$=0.045 | | $p$=0.895 | |
| Estimation | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,062 | 7,088 | 712 | 710 | 672 | 693 |
| Communities | 49 | 49 | 21 | 21 | 21 | 21 |

Note: This table presents the results of ordered probit estimations with SE clustered at the community level. The regression coefficients and SE are based on separate regressions, while the p-values are based on the regressions where both coefficients under consideration are included in the regression together. Column (1) compares displaced Kyrgyz to all non-displaced; Column (2), displaced Uzbeks. Columns (3)–(4) distinguish Uzbeks by staying with/without friends or family; Columns (5)–(6) further split by gender. The full regression results are in Table B2 in the Appendix. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

We first verify that the aggregated and disaggregated effects in the panel subsample are comparable to those observed in the main estimations. In the first subfigure, we show that the aggregate negative effects of the 2010 displacement and conflict at the community level diminish over time. The association of the conflict becomes insignificant within a year, whereas that of displacement is no longer statistically distinguishable from zero by 2016. Regarding the effects of displacement on individual well-being categories, as shown in the second subfigure, health and security recover first, followed by satisfaction with dwelling and future prospects.

The last subfigure analyzes the effects of displacement on trust and household expenditures. The results indicate that health and trust recover simultaneously. Subsequently, households increase their consumption, which may reflect adjustments related to security and dwelling concerns. Once these issues are resolved, individuals report lower concern about their future. Well-being domains not directly impacted by displacement, as shown in the third subfigure, appear to respond to changes in other domains. For example, when household expenditures increase, individuals are less likely to report dissatisfaction with their current job and family

income.

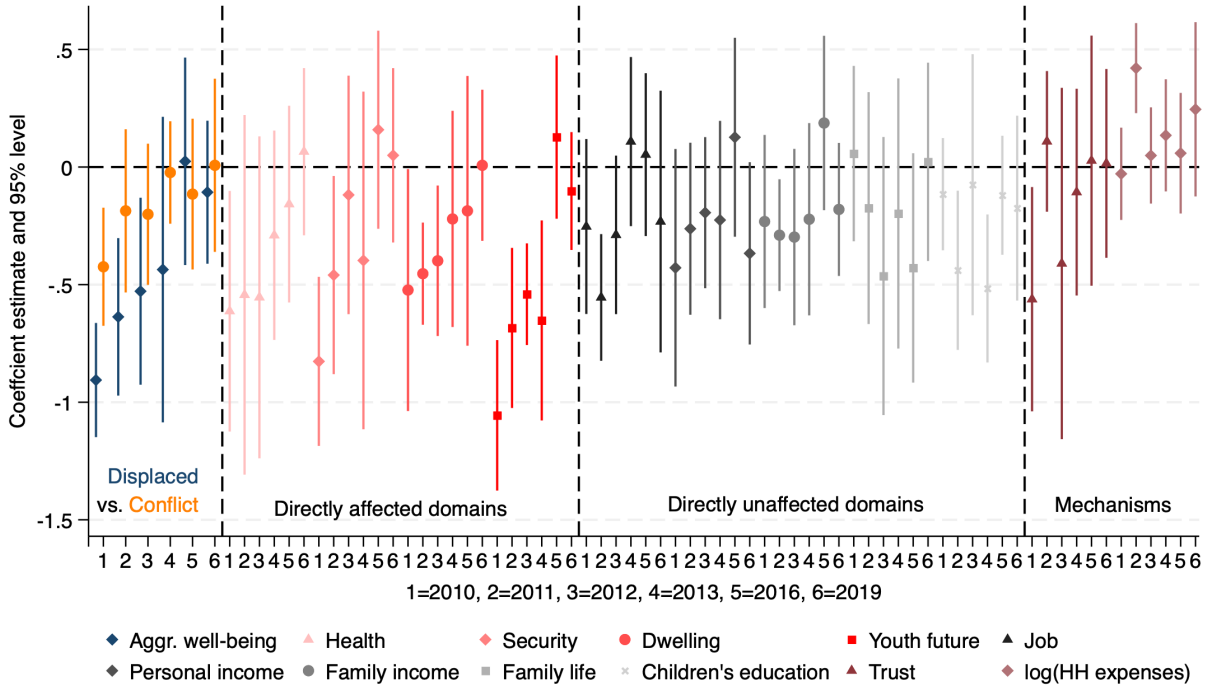



**Figure 3.4:** The medium-term effects of displacement

Notes: This figure depicts the results of ordered probit and OLS (only for household expenses) estimations based on the balanced sample of 2,866 individuals observed over 9 years in 49 communities. The first subplot shows the effects of displacement and conflict; the rest, only displacement. The full regression results for the aggregated well-being measure are in Table B4 in the Appendix.

## 3.4 Conclusion

This study provides new evidence on how short-term forced displacement during the June 2010 inter-ethnic conflict in Kyrgyzstan is associated with changes in the subjective well-being of returnees. Leveraging both repeated cross-sectional and panel data from 2006–2019, and employing ordered-probit, IV, and difference-in-differences approaches, we aim to distinguish the effects of displacement from broader conflict exposure. We find that displacement is associated with sizable well-being losses that persist over several years, with the largest declines observed immediately upon return and gradually converging toward pre-displacement levels only after around six years. The negative effects are concentrated among ethnic

Uzbeks—the group most directly targeted—especially those who lacked support from family or friends during displacement. Both men and women in this subgroup experienced comparably large well-being disruptions. Additionally, psychological domains (health, security, dwelling satisfaction, and outlook for the future) are more strongly affected, whereas economic domains (job, income, family life, and children's education) remain relatively less affected.

These findings underscore the role of social support in shaping the well-being consequences of displacement and point to the importance of targeting psychological and community-reintegration services for former conflict-affected populations. By documenting both the depth and duration of well-being losses—and presenting evidence consistent with hedonic adaptation over time—this paper contributes to the literatures on ethnic conflict, displacement impacts, and post-shock well-being dynamics.

# Appendices

## A Survey questions

| | | |
|---|---|---|
| In June 2010, did you have to leave your place of living? | Yes | 1 |
| | No | 2 |
| After how many weeks or months did you return? *(Indicate **either** weeks **or** months)* | Month _____ | 1 |
| | Week _____ | 2 |
| Where did you stay most of the time during the absence? | In the same town/village with friends/family | 1 |
| | Elsewhere in Kyrgyzstan with friends/family | 2 |
| | Displacement/ refugee camp within Kyrgyzstan | 3 |
| | Uzbekistan | 4 |
| | Abroad (except Uzbekistan) | 5 |
| | Others | 6 |
| Do you personally know any people who are **not members of your household** and who were forced to flee their place of living in June 2010? | Yes | 1 |
| | No | 2 |

**Figure A1:** Treatment-related survey questions

**Life in Kyrgyzstan**

| How satisfied are you today with the following areas of your life? Please rate them from 0 (completely dissatisfied) to 10 (completely satisfied) | 0 (completely dissatisfied) | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 (completely satisfied) | Not applicable |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Your health | | | | | | | | | | | | |
| Your job (if employed) | | | | | | | | | | | | |
| Your household income | | | | | | | | | | | | |
| Your personal income | | | | | | | | | | | | |
| Your dwelling | | | | | | | | | | | | |
| Your family life | | | | | | | | | | | | |
| The quality of education at your children's school | | | | | | | | | | | | |
| Your security | | | | | | | | | | | | |
| Childrens'/young generation's future | | | | | | | | | | | | |

| Life in Transition | 1. Strongly disagree | 2. Disagree | 3. Neither agree nor disagree | 4. Agree | 5. Strongly agree | -98. Not applicable | -97. Don' t know |
|---|---|---|---|---|---|---|---|
| *All things considered, I am satisfied with my life now* | | | | | | | |

**Figure A2:** Outcome-related survey questions

# B Additional table results

**Table B1:** The short-term effects of displacement: complete regression results

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being |
| Displaced | -0.492** | -0.347*** | -0.385*** | -0.339*** | -0.432*** | -1.416*** | -1.494*** | 0.432* |
| | (0.237) | (0.116) | (0.144) | (0.095) | (0.111) | (0.196) | (0.221) | (0.238) |
| Post | | | | | | | | -0.393*** |
| | | | | | | | | (0.103) |
| Displaced × Post | | | | | | | | -0.628*** |
| | | | | | | | | (0.238) |
| **Baseline controls:** | | | | | | | | |
| Age | | -0.007*** | -0.005*** | -0.007*** | -0.005*** | -0.005*** | -0.005*** | -0.006*** |
| | | (0.002) | (0.001) | (0.002) | (0.001) | (0.001) | (0.002) | (0.001) |
| Man | | 0.172*** | 0.115*** | 0.065** | 0.078** | 0.062** | 0.066** | 0.135*** |
| | | (0.046) | (0.027) | (0.030) | (0.030) | (0.030) | (0.032) | (0.023) |
| Kyrgyz | | 0.332* | 0.080 | 0.121 | 0.069 | 0.077 | 0.118 | 0.226 |
| | | (0.185) | (0.129) | (0.105) | (0.102) | (0.100) | (0.131) | (0.139) |
| Uzbek | | 0.069 | 0.024 | -0.099 | -0.095 | -0.026 | -0.040 | -0.074 |
| | | (0.301) | (0.199) | (0.186) | (0.181) | (0.173) | (0.211) | (0.144) |
| Household head | | -0.156*** | -0.099*** | -0.032 | -0.075** | -0.063* | -0.073 | -0.100*** |
| | | (0.058) | (0.037) | (0.039) | (0.036) | (0.035) | (0.049) | (0.035) |
| Single | | -0.573*** | -0.316*** | -0.239*** | -0.252*** | -0.257*** | -0.261*** | -0.197*** |
| | | (0.077) | (0.045) | (0.047) | (0.045) | (0.044) | (0.057) | (0.056) |
| Married | | 0.275*** | 0.176*** | 0.219*** | 0.192*** | 0.202*** | 0.197*** | 0.178*** |
| | | (0.058) | (0.040) | (0.040) | (0.038) | (0.039) | (0.044) | (0.027) |
| Has a child | | 0.129 | 0.083 | 0.102 | 0.076 | 0.063 | -0.001 | -0.045 |
| | | (0.171) | (0.108) | (0.105) | (0.108) | (0.108) | (0.115) | (0.089) |
| Household size | | 0.023 | 0.018 | 0.045*** | 0.033** | 0.031** | 0.033** | 0.050*** |
| | | (0.013) | (0.012) | (0.013) | (0.014) | (0.013) | (0.014) | (0.012) |
| Prim. tech./Sec. education | | 0.154 | 0.107 | 0.116* | 0.113* | 0.099 | 0.126 | 0.064 |
| | | (0.109) | (0.074) | (0.068) | (0.067) | (0.064) | (0.082) | (0.067) |
| Higher education | | 0.623*** | 0.397*** | 0.327*** | 0.336*** | 0.326*** | 0.424*** | 0.281*** |
| | | (0.136) | (0.088) | (0.084) | (0.082) | (0.081) | (0.090) | (0.080) |
| Months worked last 12 months | | 0.007 | 0.006* | -0.000 | 0.004 | 0.003 | 0.001 | 0.002 |
| | | (0.005) | (0.004) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| Urban resident | | 0.057 | -0.482** | | | | | -0.097 |
| | | (0.502) | (0.227) | | | | | (0.330) |
| **Extra controls:** | | | | | | | | |
| log(HH income per member) | | | | 0.275*** | | | | |
| | | | | (0.039) | | | | |
| log(HH con-n per member) | | | | | 0.184*** | 0.187*** | 0.155*** | 0.262*** |
| | | | | | (0.052) | (0.052) | (0.047) | (0.055) |
| General trust 2 | | | | 0.092 | 0.097 | 0.069 | -0.035 | 0.096 |
| | | | | (0.103) | (0.103) | (0.099) | (0.113) | (0.079) |
| General trust 3 | | | | 0.349*** | 0.367*** | 0.341*** | 0.278*** | 0.249*** |
| | | | | (0.101) | (0.103) | (0.100) | (0.107) | (0.081) |
| General trust 4 | | | | 0.542*** | 0.559*** | 0.527*** | 0.412*** | 0.462*** |
| | | | | (0.120) | (0.129) | (0.125) | (0.138) | (0.103) |
| Risk level 2 | | | | -0.063 | -0.027 | -0.026 | -0.025 | |
| | | | | (0.113) | (0.116) | (0.118) | (0.152) | |
| Risk level 3 | | | | -0.141 | -0.152 | -0.119 | -0.166 | |
| | | | | (0.123) | (0.135) | (0.134) | (0.149) | |

**Table B1:** The short-term effects of displacement: complete regression results (continued)

| | (1) | (2) | (3) | (4) | (6) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being |
| Risk level 4 | | | | -0.132 | -0.112 | -0.083 | -0.099 | |
| | | | | (0.128) | (0.131) | (0.127) | (0.164) | |
| Risk level 5 | | | | -0.132 | -0.112 | -0.083 | -0.099 | |
| | | | | (0.128) | (0.131) | (0.127) | (0.164) | |
| Risk level 6 | | | | 0.089 | 0.089 | 0.099 | 0.048 | |
| | | | | (0.091) | (0.091) | (0.088) | (0.104) | |
| Risk level 7 | | | | 0.073 | 0.086 | 0.108 | 0.115 | |
| | | | | (0.102) | (0.106) | (0.101) | (0.127) | |
| Risk level 8 | | | | 0.098 | 0.135 | 0.141 | 0.149 | |
| | | | | (0.106) | (0.113) | (0.111) | (0.129) | |
| Risk level 9 | | | | 0.322*** | 0.366*** | 0.391*** | 0.395*** | |
| | | | | (0.092) | (0.099) | (0.093) | (0.103) | |
| Risk level 10 | | | | 0.369*** | 0.376*** | 0.397*** | 0.264** | |
| | | | | (0.107) | (0.106) | (0.104) | (0.128) | |
| Risk level 11 | | | | 0.330*** | 0.336*** | 0.350*** | 0.313** | |
| | | | | (0.111) | (0.109) | (0.106) | (0.128) | |
| **Shocks at the community level during the last 12 months:** | | | | | | | | |
| Climate shocks | | | 0.013 | 0.004 | 0.029 | 0.039 | 0.092 | |
| | | | (0.128) | (0.136) | (0.153) | (0.152) | (0.162) | |
| Riots, destruction, violence | | | -0.220** | -0.273*** | -0.257** | -0.259*** | -0.299*** | |
| | | | (0.103) | (0.103) | (0.102) | (0.099) | (0.109) | |
| Economic shocks | | | -0.104 | -0.116 | -0.111 | -0.086 | -0.061 | |
| | | | (0.134) | (0.129) | (0.137) | (0.137) | (0.135) | |
| **Community controls a year before the shock:** | | | | | | | | |
| % Kyrgyz pop. (2009) | | | -0.028*** | -0.032*** | -0.032*** | -0.031*** | -0.030*** | |
| | | | (0.010) | (0.009) | (0.009) | (0.009) | (0.009) | |
| % Uzbek pop. (2009) | | | -0.020** | -0.020** | -0.023** | -0.019* | -0.017 | |
| | | | (0.010) | (0.010) | (0.010) | (0.010) | (0.011) | |
| % with Secondary edu. (2009) | | | 0.010 | 0.025 | 0.026 | 0.021 | 0.019 | |
| | | | (0.024) | (0.022) | (0.024) | (0.024) | (0.024) | |
| % with Higher edu. (2009) | | | -0.043* | -0.022 | -0.018 | -0.017 | 0.001 | |
| | | | (0.024) | (0.023) | (0.023) | (0.023) | (0.036) | |
| % Moved in $\leq$ 1 year ago (2009) | | | 0.084** | 0.077** | 0.078** | 0.079** | 0.086** | |
| | | | (0.036) | (0.034) | (0.035) | (0.034) | (0.042) | |
| % in Labor force (2009) | | | -0.013 | -0.021 | -0.018 | -0.020 | -0.018 | |
| | | | (0.020) | (0.016) | (0.018) | (0.017) | (0.018) | |
| Relative pop. size (2009) | | | -16.480*** | -19.875*** | -17.280*** | -18.353*** | -18.926*** | |
| | | | (5.009) | (4.469) | (4.373) | (4.176) | (3.892) | |
| Estimation | OLS | OLS | Oprobit | Oprobit | Oprobit | CMP | CMP | Oprobit |
| Community FE | No | Yes | No | No | No | No | No | Yes |
| Region and urban FE | No | No | Yes | No | No | No | No | No |
| Region×urban FE | No | No | No | Yes | Yes | Yes | Yes | No |
| Individuals | 7,175 | 7,175 | 7,175 | 7,175 | 7,175 | 7,175 | 5,186 | 6,388 |
| Communities | 49 | 49 | 49 | 49 | 49 | 49 | 49 | 29 |

Note: This table presents the results of estimations with SE clustered at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

**Table B2:** The disaggregated short-term effects of displacement I

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Well-being | Health | Dwelling | Security | Youth future |
| Displaced | -1.519*** | -1.485*** | -0.923*** | -1.199** | -1.619*** |
| | (0.341) | (0.543) | (0.334) | (0.563) | (0.335) |
| Age | -0.005*** | -0.025*** | 0.002 | 0.002 | -0.000 |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.002) |
| Man | 0.110*** | 0.118*** | 0.048 | 0.123*** | 0.046 |
| | (0.034) | (0.038) | (0.039) | (0.038) | (0.030) |
| Kyrgyz | 0.129 | -0.064 | -0.036 | 0.279 | 0.452** |
| | (0.159) | (0.121) | (0.109) | (0.193) | (0.211) |
| Uzbek | 0.123 | 0.070 | 0.058 | 0.172 | 0.372 |
| | (0.236) | (0.213) | (0.218) | (0.225) | (0.258) |
| Household head | -0.101** | -0.056 | -0.074 | -0.123** | -0.018 |
| | (0.051) | (0.043) | (0.052) | (0.048) | (0.048) |
| Single | -0.316*** | -0.093 | -0.258*** | -0.175*** | -0.056 |
| | (0.057) | (0.058) | (0.076) | (0.063) | (0.059) |
| Married | 0.185*** | 0.063 | 0.071* | 0.017 | 0.039 |
| | (0.043) | (0.045) | (0.036) | (0.032) | (0.039) |
| Has a child | 0.003 | 0.090 | -0.007 | 0.024 | 0.004 |
| | (0.111) | (0.107) | (0.083) | (0.087) | (0.098) |
| Household size | 0.020 | 0.022** | 0.026** | 0.011 | -0.016 |
| | (0.013) | (0.011) | (0.013) | (0.013) | (0.011) |
| Prim. tech./Sec. education | 0.105 | 0.132** | 0.100 | 0.091 | -0.015 |
| | (0.086) | (0.067) | (0.085) | (0.082) | (0.078) |
| Higher education | 0.451*** | 0.311*** | 0.352*** | 0.108 | 0.086 |
| | (0.095) | (0.075) | (0.105) | (0.122) | (0.101) |
| Months worked last 12 months | 0.004 | 0.019*** | -0.001 | -0.009** | 0.002 |
| | (0.004) | (0.005) | (0.004) | (0.004) | (0.005) |
| Urban resident | -0.371* | -0.334** | -0.297* | -0.216 | -0.390 |
| | (0.217) | (0.167) | (0.166) | (0.182) | (0.250) |
| Climate shocks | 0.061 | 0.005 | -0.010 | 0.203 | 0.031 |
| | (0.144) | (0.141) | (0.133) | (0.182) | (0.180) |
| Political riots/destruction | -0.235** | -0.267** | -0.181 | -0.044 | -0.189 |
| | (0.117) | (0.111) | (0.127) | (0.144) | (0.128) |
| Economic shocks | -0.057 | 0.111 | -0.003 | -0.089 | -0.193 |
| | (0.138) | (0.140) | (0.146) | (0.139) | (0.155) |
| % Kyrgyz pop. (2009) | -0.028*** | -0.022*** | -0.032*** | 0.001 | -0.011 |
| | (0.010) | (0.007) | (0.011) | (0.012) | (0.009) |
| % Uzbek pop. (2009) | -0.016 | -0.019** | -0.023** | 0.002 | 0.002 |
| | (0.011) | (0.009) | (0.012) | (0.012) | (0.010) |
| % with Secondary edu. (2009) | 0.008 | 0.024 | 0.023 | -0.022 | -0.024 |
| | (0.025) | (0.027) | (0.028) | (0.033) | (0.023) |
| % with Higher edu. (2009) | -0.037 | 0.032 | -0.014 | -0.055 | -0.080*** |
| | (0.034) | (0.027) | (0.033) | (0.040) | (0.031) |
| % Moved in $\leq$ 1 year ago (2009) | 0.064* | 0.022 | 0.042 | 0.124** | 0.075* |
| | (0.038) | (0.034) | (0.039) | (0.050) | (0.039) |
| % in Labor force (2009) | -0.013 | -0.003 | -0.013 | 0.023 | -0.016 |
| | (0.020) | (0.016) | (0.019) | (0.021) | (0.022) |
| Relative pop. size (2009) | -17.930*** | -7.964 | -15.394*** | -11.850*** | -17.400*** |
| | (4.357) | (5.311) | (5.510) | (4.368) | (4.158) |
| Estimation | CMP | CMP | CMP | CMP | CMP |
| Region and urban FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 5,186 | 5,186 | 5,186 | 5,186 | 5,186 |
| Communities | 49 | 49 | 49 | 49 | 49 |

Note: This table presents the results of estimations with SE clustered at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

**Table B2:** The disaggregated short-term effects of displacement II

| | (6) Job | (7) Fam. income | (8) Pers. income | (9) Fam. life | (10) Children's edu. |
|---|---|---|---|---|---|
| Displaced | -0.022 | -0.125 | -0.060 | -0.152 | -0.406 |
| | (0.732) | (1.083) | (0.764) | (1.490) | (0.812) |
| Age | 0.000 | 0.000 | -0.003 | 0.000 | 0.001 |
| | (0.003) | (0.002) | (0.003) | (0.002) | (0.001) |
| Man | 0.090 | 0.063* | 0.180*** | -0.016 | -0.067 |
| | (0.060) | (0.033) | (0.047) | (0.057) | (0.043) |
| Kyrgyz | -0.087 | 0.023 | -0.083 | -0.009 | 0.146 |
| | (0.147) | (0.129) | (0.127) | (0.144) | (0.109) |
| Uzbek | -0.182 | 0.002 | -0.250 | -0.324 | 0.056 |
| | (0.247) | (0.245) | (0.198) | (0.376) | (0.210) |
| Household head | -0.024 | -0.155*** | -0.013 | 0.008 | 0.068 |
| | (0.063) | (0.045) | (0.064) | (0.072) | (0.047) |
| Single | -0.021 | -0.175** | -0.143 | -0.746*** | 0.049 |
| | (0.105) | (0.083) | (0.091) | (0.147) | (0.092) |
| Married | -0.033 | 0.031 | 0.014 | 0.849*** | 0.134** |
| | (0.044) | (0.045) | (0.054) | (0.130) | (0.056) |
| Has a child | 0.080 | -0.062 | -0.014 | -0.095 | -0.053 |
| | (0.115) | (0.097) | (0.103) | (0.129) | (0.096) |
| Household size | 0.000 | 0.017 | 0.000 | 0.040*** | 0.010 |
| | (0.014) | (0.014) | (0.014) | (0.012) | (0.011) |
| Prim. tech./Sec. education | 0.155 | -0.014 | 0.046 | -0.089 | -0.087 |
| | (0.117) | (0.078) | (0.113) | (0.080) | (0.084) |
| Higher education | 0.712*** | 0.309*** | 0.473*** | 0.148 | 0.120 |
| | (0.105) | (0.083) | (0.111) | (0.114) | (0.107) |
| Months worked last 12 months | 0.034*** | 0.011*** | 0.026*** | 0.003 | 0.007** |
| | (0.009) | (0.004) | (0.007) | (0.005) | (0.003) |
| Urban resident | -0.127 | -0.255 | -0.232 | -0.051 | -0 .170 |
| | (0.165) | (0.207) | (0.206) | (0.191) | (0.130) |
| Climate shocks | -0.200** | 0.053 | -0.212* | 0.102 | 0.019 |
| | (0.094) | (0.147) | (0.111) | (0.144) | (0.076) |
| Political riots/destruction | -0.241** | -0.158 | -0.259** | -0.219* | -0.165** |
| | (0.115) | (0.124) | (0.128) | (0.120) | (0.074) |
| Economic shocks | -0.013 | -0.069 | -0.139 | -0.003 | - 0.088 |
| | (0.115) | (0.139) | (0.115) | (0.177) | (0.115) |
| % Kyrgyz pop. (2009) | -0.031*** | -0.023*** | -0.029*** | -0.036*** | -0.018*** |
| | (0.007) | (0.008) | (0.009) | (0.009) | (0.005) |
| % Uzbek pop. (2009) | -0.026*** | -0.018* | -0.025** | -0.028*** | -0.006 |
| | (0.009) | (0.011) | (0.010) | (0.011) | (0.007) |
| % with Secondary edu. (2009) | 0.010 | 0.016 | -0.022 | 0.029 | 0.022 |
| | (0.025) | (0.028) | (0.029) | (0.025) | (0.017) |
| % with Higher edu. (2009) | -0.006 | -0.021 | -0.035 | 0.043 | -0.006 |
| | (0.025) | (0.028) | (0.027) | (0.027) | (0.027) |
| % Moved in $\leq$ 1 year ago (2009) | -0.041 | 0.021 | -0.019 | -0.003 | 0.004 |
| | (0.035) | (0.038) | (0.043) | (0.038) | (0.028) |
| % in Labor force (2009) | -0.043** | -0.013 | -0.028 | 0.017 | -0.007 |
| | (0.017) | (0.022) | (0.021) | (0.019) | (0.012) |
| Relative pop. size (2009) | -15.514*** | -7.208 | -18.916*** | -11.892** | -8.804*** |
| | (4.232) | (6.426) | (5.880) | (4.896) | (2.905) |
| Estimation | CMP | CMP | CMP | CMP | CMP |
| Region and urban FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,568 | 5,186 | 4,568 | 4,810 | 4,763 |
| Communities | 49 | 49 | 49 | 49 | 49 |

Note: This table presents the results of estimations with SE clustered at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

**Table B3:** The short-term effects of displacement: complete heterogeneity analyses

| | (1) Well-being | (2) Well-being | (3) Well-being | (4) Well-being | (5) Well-being | (6) Well-being | (7) Well-being | (8) Well-being |
|---|---|---|---|---|---|---|---|---|
| Displaced Kyrgyz | -0.016 | | | | | | | |
| vs. non-displaced | (0.152) | | | | | | | |
| Displaced Uzbek | | -0.645*** | | | | | | |
| vs. non-displaced | | (0.224) | | | | | | |
| Uzb. displaced w. frds.\fam. | | | -0.345 | | | | | |
| vs. non-displaced | | | (0.215) | | | | | |
| Uzb. displaced w/o frds.\fam. | | | | -0.949*** | | | | |
| vs. non-displaced | | | | (0.226) | | | | |
| Uzb. displaced w. frds.\fam. | | | | | -0.049 | | | |
| vs. non-displaced Uzbek | | | | | (0.473) | | | |
| Uzb. displaced w/o frds.\fam. | | | | | | -0.985*** | | |
| vs. non-displaced Uzbek | | | | | | (0.273) | | |
| Uzb. displaced man w/o frds.\fam. | | | | | | | -0.920** | |
| vs. non-displaced Uzbek | | | | | | | (0.395) | |
| Uzb. displaced woman w/o frds.\fam. | | | | | | | | -0.965*** |
| vs. non-displaced Uzbek | | | | | | | | (0.240) |
| Age | -0.005*** | -0.005*** | -0.005*** | -0.005*** | -0.006 | -0.007 | -0.006 | -0.007 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.005) | (0.006) | (0.006) | (0.006) |
| Man | 0.115*** | 0.114*** | 0.114*** | 0.114*** | 0.155* | 0.125 | 0.128 | 0.124 |
| | (0.027) | (0.027) | (0.027) | (0.027) | (0.087) | (0.092) | (0.090) | (0.095) |
| Kyrgyz | 0.080 | 0.075 | 0.081 | 0.074 | | | | |
| | (0.128) | (0.128) | (0.128) | (0.128) | | | | |
| Uzbek | 0.098 | 0.082 | 0.109 | 0.087 | | | | |
| | (0.214) | (0.212) | (0.211) | (0.213) | | | | |
| Household head | -0.100*** | -0.101*** | -0.099*** | -0.100*** | -0.356*** | -0.351*** | -0.380*** | -0.351*** |
| | (0.038) | (0.037) | (0.037) | (0.038) | (0.116) | (0.127) | (0.121) | (0.128) |
| Single | -0.316*** | -0.307*** | -0.314*** | -0.307*** | -0.489 | -0.475 | -0.498 | -0.487 |
| | (0.045) | (0.047) | (0.047) | (0.046) | (0.362) | (0.385) | (0.385) | (0.388) |
| Married | 0.175*** | 0.176*** | 0.173*** | 0.176*** | 0.209* | 0.265** | 0.246** | 0.252** |
| | (0.041) | (0.041) | (0.041) | (0.041) | (0.123) | (0.105) | (0.112) | (0.110) |
| Has a child | 0.081 | 0.083 | 0.083 | 0.081 | -0.104 | -0.203 | -0.241 | -0.173 |
| | (0.110) | (0.109) | (0.111) | (0.111) | (0.201) | (0.207) | (0.218) | (0.221) |
| Household size | 0.020 | 0.019 | 0.020 | 0.020 | 0.044** | 0.039** | 0.042** | 0.043** |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.018) | (0.018) | (0.018) | (0.019) |
| Prim. tech./Sec. education | 0.095 | 0.110 | 0.107 | 0.102 | 0.273** | 0.227** | 0.237** | 0.221** |
| | (0.074) | (0.074) | (0.074) | (0.074) | (0.111) | (0.101) | (0.106) | (0.100) |
| Higher education | 0.392*** | 0.399*** | 0.398*** | 0.396*** | 0.008 | -0.022 | 0.008 | -0.029 |
| | (0.088) | (0.089) | (0.088) | (0.088) | (0.253) | (0.237) | (0.279) | (0.238) |
| Months worked last 12 months | 0.006* | 0.006* | 0.006* | 0.006* | 0.003 | 0.009 | 0.011 | 0.009 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.010) | (0.014) | (0.015) | (0.014) |
| Urban resident | -0.479** | -0.484** | -0.479** | -0.474** | 0.162 | 0.093 | 0.123 | 0.070 |
| | (0.226) | (0.225) | (0.225) | (0.224) | (0.437) | (0.446) | (0.470) | (0.460) |
| Climate shocks | 0.022 | 0.015 | 0.018 | 0.017 | 0.734** | 1.085*** | 0.987*** | 1.078*** |
| | (0.128) | (0.127) | (0.127) | (0.126) | (0.285) | (0.292) | (0.288) | (0.290) |
| Political riots/destruction | -0.237** | -0.221** | -0.236** | -0.228** | -0.108 | -0.032 | -0.117 | -0.028 |
| | (0.104) | (0.102) | (0.103) | (0.102) | (0.430) | (0.349) | (0.345) | (0.365) |
| Economic shocks | -0.093 | -0.102 | -0.095 | -0.095 | -0.461* | -0.616*** | -0.648*** | -0.615*** |
| | (0.137) | (0.135) | (0.136) | (0.135) | (0.245) | (0.207) | (0.194) | (0.205) |
| % Kyrgyz pop. (2009) | -0.028*** | -0.028*** | -0.028*** | -0.028*** | -0.127*** | -0.115*** | -0.122*** | -0.116*** |
| | (0.010) | (0.010) | (0.009) | (0.010) | (0.034) | (0.029) | (0.031) | (0.029) |
| % Uzbek pop. (2009) | -0.021** | -0.020** | -0.021** | -0.021** | -0.117*** | -0.122*** | -0.124*** | -0.123*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.033) | (0.030) | (0.032) | (0.030) |
| % with Secondary edu. (2009) | 0.011 | 0.010 | 0.011 | 0.010 | 0.253*** | 0.213*** | 0.241*** | 0.217*** |
| | (0.024) | (0.024) | (0.024) | (0.024) | (0.057) | (0.064) | (0.069) | (0.062) |
| % with Higher edu. (2009) | -0.043* | -0.043* | -0.043* | -0.044* | 0.060 | -0.004 | 0.019 | -0.003 |
| | (0.024) | (0.024) | (0.024) | (0.024) | (0.067) | (0.086) | (0.080) | (0.088) |
| % Moved in $\leq$ 1 year ago (2009) | 0.082** | 0.082** | 0.081** | 0.084** | -0.195** | -0.139* | -0.162** | -0.140 |
| | (0.036) | (0.036) | (0.036) | (0.036) | (0.082) | (0.084) | (0.082) | (0.087) |
| % in Labor force (2009) | -0.016 | -0.014 | -0.016 | -0.016 | -0.190*** | -0.217*** | -0.208*** | -0.219*** |
| | (0.020) | (0.020) | (0.020) | (0.020) | (0.059) | (0.070) | (0.070) | (0.069) |
| Relative pop. size (2009) | -15.200*** | -16.813*** | -15.698*** | -15.934*** | -31.547*** | -39.709*** | -37.095*** | -40.141*** |
| | (5.202) | (4.964) | (5.018) | (5.254) | (12.101) | (14.014) | (13.503) | (13.911) |
| Estimation | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit |
| Region and urban FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,062 | 7,088 | 7,028 | 7,027 | 712 | 710 | 672 | 693 |
| Communities | 49 | 49 | 49 | 49 | 21 | 21 | 21 | 21 |

Note: This table presents the results of estimations with SE clustered at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

**Table B4:** The aggregated short- and medium-term effects of displacement in the balanced sample: complete regression results

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being |
| Displaced | -0.906*** | -0.637*** | -0.528*** | -0.436 | 0.024 | -0.107 |
| | (0.124) | (0.171) | (0.203) | (0.331) | (0.225) | (0.155) |
| Age | -0.004** | -0.005*** | -0.001 | -0.002 | -0.000 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Man | 0.145*** | -0.034 | 0.020 | -0.039 | 0.048 | -0.058 |
| | (0.047) | (0.055) | (0.045) | (0.050) | (0.045) | (0.038) |
| Kyrgyz | -0.093 | -0.194 | -0.027 | -0.002 | -0.245 | -0.135 |
| | (0.148) | (0.196) | (0.195) | (0.150) | (0.172) | (0.098) |
| Uzbek | -0.092 | -0.422 | -0.129 | 0.047 | -0.328 | -0.114 |
| | (0.253) | (0.262) | (0.259) | (0.194) | (0.228) | (0.185) |
| Household head | -0.107* | 0.007 | -0.076* | 0.068 | -0.062 | -0.017 |
| | (0.060) | (0.059) | (0.041) | (0.058) | (0.045) | (0.034) |
| Single | -0.432*** | -0.150 | -0.378*** | -0.287*** | -0.252* | -0.039 |
| | (0.095) | (0.152) | (0.117) | (0.102) | (0.148) | (0.161) |
| Married | 0.212*** | 0.283*** | 0.297*** | 0.266*** | 0.196*** | 0.351*** |
| | (0.064) | (0.074) | (0.077) | (0.055) | (0.073) | (0.054) |
| Has a child | 0.059 | 0.111 | -0.033 | -0.046 | -0.013 | 0.075 |
| | (0.153) | (0.144) | (0.122) | (0.107) | (0.108) | (0.129) |
| Household size | 0.024* | 0.012 | 0.010 | 0.001 | 0.021 | 0.035*** |
| | (0.014) | (0.016) | (0.013) | (0.009) | (0.015) | (0.009) |
| Prim. tech./Sec. education | 0.042 | 0.064 | 0.161* | -0.082 | -0.039 | 0.125 |
| | (0.100) | (0.078) | (0.084) | (0.089) | (0.076) | (0.080) |
| Higher education | 0.301** | 0.347*** | 0.495*** | 0.183* | 0.228* | 0.414*** |
| | (0.134) | (0.112) | (0.112) | (0.110) | (0.117) | (0.100) |
| Months worked last 12 months | 0.003 | 0.007 | 0.021*** | 0.022*** | 0.022*** | 0.011* |
| | (0.004) | (0.007) | (0.007) | (0.005) | (0.007) | (0.006) |
| Urban resident | -0.262 | -0.431* | -0.551** | -0.506*** | -0 .446** | 0.331 |
| | (0.204) | (0.224) | (0.244) | (0.174) | (0.209) | (0.226) |
| Climate shocks | -0.066 | 0.023 | 0.142 | 0.086 | 0.055 | -0.050 |
| | (0.180) | (0.221) | (0.171) | (0.177) | (0.221) | (0.196) |
| Political riots/destruction | -0.424*** | -0.186 | -0.201 | -0.023 | -0.115 | 0.007 |
| | (0.128) | (0.177) | (0.153) | (0.111) | (0.164) | (0.188) |
| Economic shocks | -0.218 | -0.299 | -0.147 | -0.179 | 0.325 | -0.004 |
| | (0.165) | (0.226) | (0.204) | (0.151) | (0.207) | (0.176) |
| % Kyrgyz pop. (2009) | -0.035*** | -0.026** | -0.021 | -0.038*** | -0.021 | 0.015 |
| | (0.010) | (0.011) | (0.017) | (0.012) | (0.014) | (0.012) |
| % Uzbek pop. (2009) | -0.023** | -0.021 | -0.029* | -0.062*** | -0 .028* | 0.018 |
| | (0.011) | (0.013) | (0.018) | (0.012) | (0.016) | (0.012) |

**Table B4:** The aggregated short- and medium-term effects of displacement in the balanced sample: complete regression results (continued)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Well-being | Well-being | Well-being | Well-being | Well-being | Well-being |
| % with Secondary edu. (2009) | 0.001 | 0.025 | -0.021 | -0.021 | -0 .049 | 0.037 |
| | (0.024) | (0.032) | (0.036) | (0.028) | (0.036) | (0.033) |
| % with Higher edu. (2009) | -0.094*** | 0.008 | -0.004 | -0.004 | 0 .016 | -0.018 |
| | (0.028) | (0.035) | (0.029) | (0.019) | (0.035) | (0.030) |
| % Moved in $\leq$ 1 year ago (2009) | 0.059 | -0.003 | 0.149*** | 0.000 | 0.037 | 0.050 |
| | (0.045) | (0.055) | (0.056) | (0.042) | (0.049) | (0.060) |
| % in Labor force (2009) | -0.026 | -0.027 | 0.009 | -0.065*** | -0.029 | 0.020 |
| | (0.022) | (0.032) | (0.033) | (0.019) | (0.023) | (0.027) |
| Relative pop. size (2009) | -25.798*** | -7.024 | 1.845 | 5.456 | -5.679 | 5.752 |
| | (5.196) | (7.093) | (9.001) | (5.064) | (7.252) | (6.473) |
| Estimation | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit | Oprobit |
| Region and urban FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Communities | 49 | 49 | 49 | 49 | 49 | 49 |

Note: This table presents the results of estimations with SE clustered at the community level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

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