Essays on Income Inequality: the Role of Institutions, Firms and Neighborhoods

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Essays on Income Inequality: the Role of Institutions, Firms and Neighborhoods

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(met een samenvatting in het Nederlands)

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Emiel van Bezooijen
Amsterdam, May 2024

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CHAPTER 1

Introduction

1.1 Motivation

The gains from economic growth experienced by developed countries over the past decades have not been equally shared within societies. There has been a broad trend of soaring income inequality, in which those at the top of the income distribution have seen their wages rapidly increase, while wage growth at the bottom and the middle of the income distribution has been relatively limited (Cingano, 2014; OECD, 2015; Chancel et al., 2022). Income inequality also shows a strong persistence across generations, with children whose parents rank at the bottom of the income distribution being less likely to reach a high point in the income distribution themselves. While considerable heterogeneity across regions exists, such patterns of limited intergenerational economic mobility are observed in many developed countries (Bratberg et al., 2017; OECD, 2018a; Narayan et al., 2018). The strong negative association between income inequality and intergenerational mobility observed within and across regions further raises concerns that inequality today not only carries over but is further exacerbated from one generation to the next (Corak, 2013; OECD, 2018a).

While some degree of income inequality may act as an incentive for people to invest in their human capital, work hard, and innovate, excessive and entrenched income inequality can stifle economic growth, erode social cohesion, and undermine trust in institutions (Dabla-Norris et al., 2015; Stiglitz, 2015; Krueger, 2018). Moreover, income inequality can signal inequalities of opportunities, raising concerns that some groups in society are increasingly left behind. Due to its potential impact on many facets of society, the rise in income inequality and limited upward mobility occupies a central position in the public debate, with former US President Barack Obama referring to these trends as the "defining challenge of our time" (Obama, 2013).

To address the potentially adverse consequences of income inequality and limited upward mobility, there is a need for careful and evidence-based policy design. This requires rigorous research investigating the underlying causes of income inequality and the role of existing policies and institutions to address it. For this reason, this dissertation studies two drivers of income inequality: firm wage-setting power and exposure to neighborhood characteristics during childhood. In addition, I investigate the effectiveness of a key labor market institution – the minimum wage – in addressing income inequality.

A large literature emphasizes the role of structural shifts in the economy as driving forces behind the rise in income inequality. Technological change (e.g., Katz and Murphy, 1992; Autor et al., 2003; Acemoglu and Autor, 2011; Goos et al., 2014) and globalization (e.g., Autor et al., 2014, 2016b) have raised the relative demand for high-skilled or non-routine labor, contributing to the wedge in wages between those in high-skilled or low-skilled jobs and those in routine or non-routine occupations. While this literature focuses on the supply and demand factors that drive the widening of wage differential between different types of workers, another strand of literature emphasizes the role of firms.

A mounting body of empirical work documents disparities in the wages paid by different firms to observably similar workers. Variation in this 'firm-component' of wages or 'firm-premium' has contributed to the rise in earnings inequality in a variety of countries (e.g., Abowd et al., 1999; Card et al., 2013a; Song et al., 2019; Bonhomme et al., 2023). Moreover, both the differential sorting of various groups to high-wage firms and differences in the firm-premia paid to workers belonging to different groups have contributed to wage disparities by gender (e.g., Card et al., 2016; Bruns, 2019), race (Gerard et al., 2021), and migration background (Dostie et al., 2023). This literature points to imperfect labor market competition, in which firms have wage-setting – or monopsony – power over their workers. Monopsony power, which can arise due to various reasons such as search frictions (e.g., Manning, 2003) or heterogeneous worker preferences (e.g., Card et al., 2018), enables firms to mark down workers' wages relative to their marginal productivity.

Firm wage-setting power can have important implications for income inequality. If firms have greater monopsony power over certain groups of workers, for example, women or migrants, they can mark down wages more for these groups. Different types of workers may also be disproportionally employed in occupations or industries characterized by imperfect competition, driving a wedge in the wages between those workers and workers in more competitive segments of the labor market. Moreover, in monopolistic labor markets, the transmission of firm heterogeneity in productivity to wages is amplified, contributing to wage differentials between those employed in more or less productive firms.

Monopsony power also affects the way institutions may impact inequality. A prime example of this is the minimum wage: models of perfect labor market competition predict that minimum wages reduce employment, while under monopsonistic competition, such disemployment effects might be negligible, or higher minimum wages may even raise employment. To understand the roots of income inequality and the potential of policies to address it, it is essential to quantify the degree of firm wage-setting power and investigate how wage-setting power varies across different types of workers and segments of the labor market.

While the aforementioned literature focus on cross-sectional income inequality, another body of work seeks to understand the causes of persistent inequality across generations. It is well-established that family background characteristics such as parental earnings and education strongly predict children's outcomes later in life (Black and Devereux, 2011; Björklund and Salvanes, 2011; Jäntti and Jenkins, 2015). Children born to families with an advantageous economic background tend to have better economic outcomes themselves compared to those children born into poorer conditions.

There is a long tradition in economics seeking to explain this high persistence of economic prosperity – or limited intergenerational mobility – through the lens of a human capital production function (e.g. Becker and Tomes, 1986, 1979; Cunha and Heckman, 2007; Becker et al., 2018). In this framework, high-earning parents are assumed to possess innate abilities positively associated with human capital, which they partially transmit to their children. In addition, high-earnings parents are believed to invest more in their children's education due to a complementary between innate ability and the returns to human capital investment and due to relatively lower credit constraints (for an overview of this literature, see Mogstad and Torsvik, 2023). Disparities in parental innate abilities and credit constraints are carried over to the next generation and exacerbated by an increasing return to human capital in the labor market.

However, a growing literature highlights large differences in intergenerational mobility across areas within countries, even after accounting for differences in parental background (e.g., Chetty et al., 2014; Heidrich, 2017; Corak, 2020; Deutscher and Mazumder, 2020; Eriksen and Munk, 2020; Kenedi and Sirugue, 2023). This observation has led to a resurgence of empirical studies establishing a causal link between the neighborhoods in which children grow up and their outcomes later in life, such as long-run education attainment and earnings (e.g., Ludwig et al., 2013; Chetty et al., 2016b; Chetty and Hendren, 2018a,c; Chyn, 2018; Deutscher, 2020; Nakamura et al., 2022). Such 'neighborhood effects' can impact intergenerational mobility and can widen the gap in upward mobility between different groups. A prime example is individuals with a migration background, who often exhibit lower rates of upward mobility compared to natives (e.g., OECD, 2018b; Elk et al., 2019) and are often disproportionally represented in neighborhoods with characteristics commonly associated with poor education outcomes, including lower school quality and higher crime rates. Understanding the nature of these neighborhood effects and how they contribute to disparities in intergenerational mobility between groups is vital for designing policies promoting equality of opportunity and enabling children to climb the economic ladder.

This dissertation seeks to contribute to our understanding of the sources of and potential remedies to (excessive) income inequality. Each of the three independent chapters that comprise this dissertation approaches the topic of income inequality from a different perspective. In Section 1.2, I discuss the main objective of each respective chapter and their contributions to the literature. I discuss the data and empirical strategies used in this dissertation in Section 1.3. A brief overview of each chapter is provided in Section 1.4.

1.2 Objectives and Main Contributions

Chapter 2 investigates the role of a key labor market institution – the minimum wage – in reducing labor market inequality. By setting a lower bound to wages at the bottom of the wage distribution, the minimum wage is an important policy instrument to counteract lower-tail inequality. However, because minimum wages raise employers' labor costs, they may have adverse consequences for employment among affected workers.

An extensive literature has assessed this potential downside of the minimum wage (for recent overviews, see Belman and Wolfson, 2014; Dube, 2019; Neumark and Shirley, 2022). The empirical evidence suggests that for the economy as a whole, such disemployment effects are absent or negligibly small, at least at prevailing minimum wage levels (Manning, 2021a). However, this does not rule out more sizable employment effects for the most strongly affected groups. A prime example is young workers, who are strongly over-represented among minimum wage workers.

In Chapter 2, I study the impact of a large increase in the age-specific minimum wage for 20–22-year-olds in the Netherlands on the employment outcomes of these age groups and their overall earnings. This chapter makes several contributions to the literature. First, the majority of the empirical literature has focused on the impact of a change in the general minimum wage. In contrast, studies on the impact of age-specific minimum wages are scarce, despite age-based minimum wage differentiation being very common. Second, I study the impact of the minimum wage in a context where disemployment effects among affected groups are most likely to occur: the increase in the age-specific minimum wage is sizable and has a non-negligible bite, and firms have the ability to quickly adjust their workforce, due to the high incidence of flexible and part-time work arrangements among young workers. Third, my empirical design allows me to trace the impact of the minimum wage throughout the (age-specific) wage distribution. This allows me to study whether the minimum wage also affects the wages of workers already paid above the minimum wage.

In Chapter 3, I investigate the degree of wage-setting power of firms in the Netherlands. In a perfectly competitive labor market, market forces are assumed to determine wages, and the labor supply curve to individual firms is perfectly elastic. However, a growing body of empirical work shows that the labor supply curve facing firms is not infinitely elastic, which grants some wage-setting – or monopsony – power to firms and allows them to pay workers below their marginal productivity (for recent overviews, see Langella and Manning, 2021; Manning, 2021c; Ashenfelter et al., 2022a). The labor supply elasticity facing the firm can be used to quantify the degree of firm wage-setting power. Rooted in a model of dynamic monopsony in which firm wage-setting power stems from search frictions, Manning (2003) shows that the firm labor supply elasticity can be recovered from estimates of the separation elasticity with respect to wages. Using this result, a large body of empirical studies has found evidence of substantial firm monopsony power across many countries, industries, and occupations (for a survey of this literature, see Sokolova and Sorensen, 2021).

However, these studies typically estimate the separation response to individual wage changes. As wages can vary for reasons other than differences in firm wage-setting policies (e.g., transitory shocks to individuals' job prospects), using all variation in individuals' wages can lead to biased estimates of separation elasticities, which might not accurately reflect the degree of monopsony power in the labor market. Ideally, one would use exogenous variation in wages that is attributable to firm wage-setting discretion to estimate separation elasticities. This requires isolating wage variation due to firm wage-setting policies from wage variation reflecting (unobserved) worker differences or transitory shock to individuals' job prospects.

I contribute to the literature on firm-wage setting power by quantifying the degree of monopsony power in the Netherlands using the matched instrumental variable event-study approach developed by Bassier et al. (2022). This approach allows for the identification of causal separation responses to wage variation induced by firm wage-setting policies. Moreover, I contribute to the literature by exploring how monopsony power varies across different subgroups of workers and industries.

Chapter 4 studies the impact of neighborhood co-ethnic concentration and co-ethnic earnings on the education outcomes of refugee children. Refugees often face steep barriers to economic and civic integration and generally lack behind natives in education outcomes and earnings. As refugees, and migrants more broadly, tend to settle in neighborhoods with a relatively high concentration of other migrants, particularly those with a similar ethnic background, there is a widespread concern that the segregation of refugees in such 'ethnic enclaves' hampers their long-term (labor market) integration.

While there is a growing literature showing that refugees arriving in the host country as adults may benefit from residing in (economically established) ethnic enclaves (Edin et al., 2003; Damm, 2009b; Martén et al., 2019), the literature investigating the impact of ethnic enclaves on the education outcomes of refugee children is scarce and often fails to address essential endogeneity issues.

I contribute to the literature on ethnic enclaves, and neighborhood effects more broadly, on education outcomes of children, by exploiting a Dutch refugee dispersal policy that generates plausible exogenous variation in initial neighborhood (enclave) characteristics. The dispersal policy allows me to overcome several identification challenges, giving my estimates a causal interpretation.

1.3 Data and Methodology

In this section, I discuss the data and empirical strategies employed in this dissertation. I begin by discussing the former. Throughout this dissertation, I use detailed administrative data from Statistics Netherlands (CBS). The CBS data are non-public but available for statistical and scientific research under certain conditions. For Chapters 2 and 3, I use rich employer-employee matched data, sourced from the registers of the Dutch tax administration, covering the entire population of employed workers in the Netherlands. These data contain information on earnings, type of employment contracts, hours worked, sector, and various other job attributes. For Chapter 4, which focuses on education – rather than labor market outcomes, I use data on education enrollment and degrees obtained, sourced from registry data from the Dutch Ministry of Education. Information on various demographic characteristics, as well as longitudinal information on individuals' residency and household composition history, is based on data from the Dutch population registers. For the analysis presented in Chapter 4, I combine information on collective asylum reception centers, migration motives, residency spells, and demographics, to identify refugees and construct a novel dataset with monthly information on individuals' neighborhood composition over the period 1999-2021.

In Chapter 2 I study the causal impact of an increase in the Dutch minimum wage applicable to workers aged 20–22, effective July 2017, on the employment outcomes and earnings of these age groups. I first compute monthly distributions of hourly wages for each age group. I assign individual jobs to a wage-bin relative to the age-specific new real hourly minimum wage and collapse the data to the number of jobs and total hours worked by age, wage-bin, and month. I subsequently use a difference-in-differences design to estimate the average treatment on the treated (ATT) effect of the minimum wage increase on bin-specific total hours worked (total number of jobs) for treated age groups, using 23-25-year-olds as a control group. The estimates have a causal interpretation under the following assumptions: 1) the change in each constituent wage-bin relative to the new minimum wage should be the same for the treated and control groups (*parallel trends assumption*) and 2) the changes in the number of jobs and total hours worked for each age group depends on the treatment status of that age group alone, so that there are no cross-age spillover effects (*stable unit treatment value assumption*). A more detailed description of the methodology and various exercises to assess the validity of the identifying assumptions are discussed in Chapter 2.

In Chapter 3, I use a matched IV event-study approach to estimate separation elasticities with respect to the firm-component in wages. Intuitively, the event-study approach isolates the separation response with respect to the firm-component of wages by mimicking the following experiment: consider two workers employed at the same *'origin'* firm with comparable employment histories in terms of tenure at the firm, hiring wage, and wage growth trajectories. These workers move to different *'intermediate'* firms at the same time, where one worker moves to a 'high-wage' firm and the other moves to a 'low-wage' firm, experiencing a differential change in their own wage. The key identifying assumption is that, conditional on prior employment histories, the move to the type of intermediate firm is exogenous. I follow these workers for 16 quarters following the transition from the *'origin'* to the *'intermediate'* firms and estimate the probability of separating from the *'intermediate'* firm in response to the change in the own wage experienced during the initial transition.

To isolate the exogenous firm-component of the wage change, I use an instrumental variable (IV) approach: I instrument the change in the own wage with the difference in firm average coworker wages between the intermediate and origin firm. In Chapter 3, I show that the instrument is highly correlated with the endogenous regressor (change in the own wage) and therefore satisfies the *instrument relevancy* requirement. Under the assumption that the change in the firm average coworker wage only affects the probability of separating from the *intermediate* firm through its effect on individuals' own wage (*instrument exogeneity*), the causal effect that I identify is a (weighted) local average treatment effect (LATE). It describes the separation response to the wage change of compliers.

In Chapter 4, I exploit a refugee dispersal policy in the Netherlands in which refugees are quasi-randomly assigned to their first address outside collective reception centers. This dispersal policy acts as a natural experiment that allows me to address two major challenges in identifying the causal impact of the neighborhood concentration and earnings of individuals with a shared ethnic background (co-ethnics) on the education outcomes of refugee children. First, individuals could not choose their initial neighborhood, circumventing the issue of *endogenous sorting* into neighborhoods with a higher (lower) concentration of co-ethnics. I assess the validity of the assumption of random assignment to initial neighborhoods in Chapter 4. Second, by comparing individuals assigned to the same neighborhood but belonging to different ethnic groups, I isolate the impact of co-ethnic concentration from neighborhood characteristics shared across individuals, irrespective of ethnic affiliation (i.e. *correlated effects*). Since refugees are not restricted in their residential mobility after initial assignment, the causal effect that I identify an intent-to-treat effect (ITT): the causal impact of being *assigned* to a neighborhood with a higher initial (lower) concentration of co-ethnics.

1.4 Outline and Summary of Chapters

In this section, I briefly summarize each of the chapters of this dissertation. The chapters are organized as independent articles, each with separate introductions and conclusions. Chapter 5 concludes this dissertation with a discussion of the main findings, potential limitations, and directions for further research.

1.4.1 The Young Bunch: Youth Minimum Wages and Labor Market Outcomes

In this chapter, I study the effect of an increase in the age-specific minimum wage for 20-22-year-olds in the Netherlands on the employment outcomes of these age groups at the extensive and intensive margins (i.e. the number of jobs and hours worked), as well as on their overall earnings. In July 2017, the age-specific minimum wage for 20-22-year-olds was raised by around 15 to 19% for affected workers, while the minimum wage for workers aged 23 and over remained virtually unchanged.

Using a difference-in-differences design, I show that the increase in the age-specific minimum wage did not reduce the number of jobs held or the total hours worked by affected workers but led to a reallocation of employment around the new minimum wage. While the number of jobs (hours worked) paying below the new minimum declined, this was fully compensated by an increase in the number of jobs (hours worked) paying at or slightly above the new minimum wage. I show that spillovers – increases in employment in jobs slightly above the new minimum – account for almost 70% of the total wage increase. These spillovers predominantly occur within €2.50 of the new minimum wage and are found for firms' incumbent workers and new hires. Exploring various dimensions of effect heterogeneity, I find that the minimum wage increase led to increased hours worked in full-time jobs and by non-student workers, suggesting that the policy more positively impacted workers who rely on low-wage jobs for a living. I do not find evidence that firms substitute incumbent workers for differently or higher-skilled new hires, nor do I find evidence of 'offsets' regarding contract quality.

Chapter 2 is coauthored with Anna Salomons and Wiljan van den Berge, both affiliated with Utrecht University School of Economics at the time of conducting the study. This Chapter is accepted for publication at the *ILR Review* and an earlier version has been published as a CPB Discussion Paper

1.4.2 Monopsony in the Netherlands: A mover-based approach

In this chapter, I study the degree of monopsony power of firms in the Netherlands. I use an IV event-study approach, recently developed by Bassier et al. (2022), to estimate separation elasticities with respect to the firm-component of individuals' wages. Using data on wages and employment histories of the universe of Dutch workers between 2010-2021, I estimate a separation elasticity with respect to the firm-component of wages of -3.6 or a labor supply elasticity of 7.2. This implies substantial wage-setting power of firms, with a potential markdown of wages of 12% in the Dutch labor market. Exploring heterogeneity in labor supply elasticities across sectors and subgroups of workers, I find larger potential markdowns for workers at the top and bottom of the wage distribution, workers in specialized business and business support services, and women.

Chapter 3 is joint work with Ronja Röttger, who was a graduate student at Utrecht University School of Economics at the time of writing and currently affiliated with the Technology & Policy Research Initiative at Boston University

1.4.3 New home, old neighbors? Ethnic enclaves and refugees' education outcomes

In this chapter, I study whether the concentration and earnings of individuals with a shared ethnic background in the neighborhood – henceforth ethnic enclaves – affect the education outcomes of refugee children in the Netherlands. I exploit a refugee dispersal policy between 1999 and 2009 that quasi-randomly assigned refugees to their first address outside of collective reception centers. This random assignment allows me to address two major challenges in identifying the causal impact of ethnic enclaves on school performance. I find that the impact of being assigned to a neighborhood with a higher share of co-ethnics is moderated by neighborhood co-ethnic earnings. Assignment to a neighborhood with a higher concentration of co-ethnics adversely affects (long-term) education outcomes when neighborhood co-ethnic earnings are low, while the effect turns positive when neighborhood co-ethnic earnings are high. In addition, while neighborhood co-ethnic earnings positively affect school performance, this effect is amplified when co-ethnic comprise a larger share of the neighborhood population.

While this chapter is single-authored, I have greatly benefited from valuable inputs from Cécile Magnée who was affiliated with CPB Netherlands Bureau for Economic Policy Analysis at the time of writing and is currently affiliated with the Dutch Immigration and Naturalisation Service.

CHAPTER 2

The Young Bunch: Youth Minimum Wages and Labor Market Outcomes

2.1 Introduction

The past decades have witnessed increasing inequality in many developed countries (OECD, 2015). Minimum wage increases are an important policy instrument to counteract inequality at the bottom of the labor market, as real minimum wages have important consequences for the evolution of lower-tail inequality (Lee, 1999; Autor et al., 2016a; Engbom and Moser, 2018). However, because minimum wages raise employers' labor costs, they may have adverse consequences for employment among affected groups of workers.

In this paper, we study one of the most strongly affected groups: young workers, who are greatly over-represented among minimum wage workers. We examine the impact of a recent increase in the age-specific minimum wage for 20–22-year-olds in the Netherlands on the employment outcomes of these age-groups at the extensive and intensive margins (i.e. the number of jobs and hours worked), as well as on their overall earnings. We also study heterogeneity in these effects by considering the most affected industries, as well as impacts across different types of labor contracts and worker demographics. This 2017 reform increased age-specific minimum wages by around 15 to 19% for affected workers, with virtually no change for older minimum-wage workers.

The context we study is of interest for several reasons. First, the increase in the minimum wage is directly aimed at specific age-groups. While the majority of the empirical literature focuses on the impact of a change in the general minimum wage, relatively few studies have considered the impact of a minimum wage increase that is only legally binding for workers of specific ages, even though such age-specific minimum wages are very common. A clear consensus has not yet emerged, with some studies finding zero or positive impacts on youth labor market outcomes while others find negative effects or have mixed results.

Second, the increase in the age-specific minimum wage is sizable and has a non-negligible bite, because, as in many other countries, Dutch youth employment is concentrated in low-wage jobs. However, Dutch youth employment is high: in 2019, the Dutch employment rate of 15–24-year-olds was 65.3%, compared to 51.2% in the US and 54.1% in the UK (OECD, 2021). This allows us to study the impact of the minimum wage in a context where firms have a strong incentive to adjust their workforce and production process, particularly in sectors where youth workers account for a large share of the labor force such as retail, and bars and restaurants. In addition, firms have the ability to adjust their workforce quickly because flexible and part-time work arrangements are common among young workers.

To credibly measure the causal effects of minimum wages on youth labor market outcomes, we use an empirical design exploiting the fact that somewhat older workers are not directly affected by the youth minimum wage increase. To inform on impacts for low-wage workers, we study how the number of jobs and hours worked change around the new minimum wage, following Cengiz et al. (2019). This also allows us to ask whether minimum wage increases have raised wages for workers already being paid above the minimum wage. We exploit detailed administrative records on all youth workers in the Netherlands, in contrast to the more commonly used survey data. This gives us precise estimates and confidence that results are not driven by misreported hours or wages, which is a concern in this literature (Autor et al., 2016a).

2.2 Related literature and contribution

A large literature has assessed the potential downside of minimum wages predicted by perfectly competitive labor market models: decreased employment for low-wage workers. The empirical evidence suggests that for the economy as a whole, such disemployment effects are absent or negligibly small, at least at prevailing minimum wage levels (for overviews, see Belman and Wolfson, 2014; Dube, 2019). On the other hand, Gregory and Zierahn (2022) study a setting where minimum wages are set above the median sectoral wage level, showing that such very large increases in the minimum wage can lead to sizable disemployment effects.

The absence of disemployment effects for moderate minimum wage increases can be understood as resulting from efficiency wages (e.g. Rebitzer and Taylor, 1995), inelastic labor demand, or employers' monopsony power (e.g. Burdett and Mortensen, 1998; Manning, 2003). Increased minimum wages could for example reduce worker turnover and monitoring costs, as well as raise worker productivity, offsetting any direct increase in firms' wage costs. Labor demand could be relatively inelastic because minimum wage workers are not a large share of firms' labor costs; minimum-wage workers are not easily substitutable for other factors of production; and/or product demand is inelastic in the (often non-tradable) sectors where minimum-wage workers are employed. Further, in imperfectly competitive labor markets, raising the minimum wage increases labor supply, making the employment effects theoretically ambiguous. See Manning (2021b) for a recent overview of the literature's findings and theories consistent with absent disemployment effects.

However, this does not rule out bigger effects for the most strongly affected groups: a prime example is young workers, who are strongly over-represented among minimum wage workers. Studying this group is also important given the recent declines in labor force participation in both the US and the Netherlands for young, low-skilled men (Dillingh et al., 2018; Aguiar et al., 2021). While some recent studies for the US also find small or near-zero employment effects of the minimum wage for these groups (e.g. Dube et al., 2016; Allegretto et al., 2017), others report adverse effects on youth labor market outcomes (e.g. Neumark et al., 2014b,a). Our study contributes new empirical evidence to this literature, which has not yet reached a consensus.

Age-specific minimum wages are very common[1] but the majority of the empirical literature focuses on the impact of a change in the general minimum wage. Our study contributes to a small literature which examines the impact of a minimum wage increase that is only legally binding for workers of specific ages.[2] Also here, a clear consensus has not yet emerged, with some finding zero or positive impacts on youth labor market outcomes (Portugal and Cardoso, 2006; Hyslop and Stillman, 2007) while others find negative effects or have mixed results (Pereira, 2003; Shannon, 2011).[3]

[1]Many countries have age-specific minimum wage systems or have had youth minimum wage provisions in the past. Implementation differs across countries, with variation in the number of age-gradients, the youth-adult minimum wage ratio, and the age threshold which separates youth and adult minimum wages (see Grimshaw et al., 2014; Marimpi and Koning, 2018).

[2]Some studies use age-discontinuities in the minimum wage to study the impact of youth minimum wages on youth labor market outcomes. For example, Dickens et al. (2014); Kreiner et al. (2020); Kabátek (2021) consider the impact on individual labor market outcomes when individual workers cross an age threshold, resulting in a higher applicable minimum wage. While these studies focus on minimum wage variation embedded in the prevailing youth minimum wage system, we focus on minimum wage changes resulting from a *change* in the age-dependent minimum wage system. Kabátek (2021) studies the Dutch minimum wage before the reform: we replicate his descriptive analyses in Appendix 2.B using data two years before and after the reform, and find similar results.

[3]A policy report has also considered the 2017 policy change we study (ter Weel et al., 2018), finding similar results. The key difference between their study and ours is that we focus on the impact of the minimum wage change around the minimum wage while they focus on all workers in the affected age groups, which could lead to biased estimates if there are other changes higher up the wage distribution (Cengiz et al., 2019). Our setup furthermore allows us to study wage spillovers. Finally, ter Weel et al. (2018) include data from 2018, while we restrict our main analyses to 2017. Estimates including 2018 could be confounded by other policies—as also acknowledged in ter Weel et al. (2018)– that we discuss below.

2.3 Data

2.3.1 Youth minimum wages in the Netherlands

The adult minimum wage in the Netherlands covers all workers aged 23 and older. For workers aged 15 to 22, the youth minimum wage is defined as a stepwise increasing fraction of the adult minimum wage. In January 2017, right before the reform we study, the youth minimum wage ranged from 35% of the adult minimum wage for 15-year-olds to 85% of the adult minimum wage for 22-year-olds (column 1 in Table 2.1). Workers become eligible for the next step in the minimum wage in the month of their birthday. The minimum wage is biannually adjusted to keep pace with average collectively bargained wage growth.

The Netherlands has no hourly minimum wage. Instead, minimum wages are defined per day, week, and month. The hourly minimum wage depends on what constitutes a full-time workweek. This is agreed upon in collective bargaining agreements, which often cover a whole sector, but large firms frequently also have their own collective bargaining agreement. Full-time workweeks always range between 36 and 40 hours per week.

The minimum wage reform. In July 2017 and July 2019 the youth minimum wage for workers aged 18 and older was increased in two steps. The increase was proposed in October 2016 and confirmed into law in January 2017.[4] The minimum wage for workers aged 15–17 remained unaltered. Table 2.1 shows the change in both the rate of the age-specific minimum wage relative to the adult minimum wage, and the resulting increase in the real weekly minimum wage by age-group for both steps of the reform. The first column reports the youth minimum wage as a share of the adult minimum wage before each step of the reform. The third column reports the youth minimum wage in real euros per week before the reform. For example, prior to the 2017 reform, 22-year-olds had a minimum wage that was 85% of the adult minimum wage, or 299 euros per week, compared to 352 euros per week for those aged 23 or older.

On July 1, 2017, the minimum wage increased overnight by 18.7% for 22-year-olds, 18.3% for 21-year-olds, and 14.9% for 20-year-olds. At the same time, the minimum wage for adults was only raised by 0.9%, reflecting biannual real wage indexation. On July 1, 2019, the minimum wage was raised by another 19.1% for 21-year-olds, and 15.7% for 20-year-olds. The minimum wage for adults, now also including 22-year-olds, was raised by 1.2%. Minimum wages for 18–19-year-olds increased by relatively small amounts. We will not study these here, but instead focus on the sizable minimum wage increase for 22-year-olds.[5] We focus on this age group because a wage subsidy policy was introduced to compensate employers for

[4]See Tweede Kamer der Staten-Generaal (2016b) for the proposal to increase the youth minimum wage, and Tweede Kamer der Staten-Generaal (2016a) for the accompanying explanatory memorandum. Appendix 2.A discusses the details of the policy change and the additional policies mentioned below in detail, citing and translating the relevant government documents.

[5]We find that the employment impacts of these small increases for 18 and 19-year-olds are around zero. Estimates are available upon request.

increased minimum wages of 20- and 21-year-olds, as we discuss next.

Table 2.1 The two-step youth minimum wage reform

	MW as a % of adult MW		Real weekly minimum wage		
Age	Pre-reform	Post-reform	Pre-reform	Post-reform	% Change
			Step 1: July 2017 reform		
23 and older	100	100	352	355	0.9
22	85	100	299	355	18.7
21	72.5	85	255	302	18.3
20	61.5	70	217	249	14.9
19	52.5	55	185	195	5.7
18	45.5	47.5	160	169	5.3
			Step 2: July 2019 reform		
22 and older	100	100	351	355	1.2
21	85	100	298	355	19.1
20	70	80	246	284	15.7
19	55	60	193	213	10.4
18	47.5	50	167	178	6.6

Notes: MW = minimum wage. Real euros relative to 2015.

2.3.2 Additional policies

As part of a compensation scheme for the minimum wage increase, the Dutch government announced temporary wage subsidies to firms employing youth minimum wage workers. Further, there is a policy targeting low-wage workers earning above the adult minimum wage. These policies, which were announced before the minimum wage increase came into effect, could have important confounding effects: here, we outline how this impacts our estimates for 22-year-olds, who are our baseline treatment group; and 18–21-year-olds, who we do not use at baseline.

Policy impacting 22-year-olds. Workers aged 22 (and aged 21, in the case of the second reform in 2019) were ineligible for the main wage subsidy (the so-called JLIV subsidy), discussed below. This ineligibility was known to firms, as we outline in detail in Appendix 2.A.1. However, some 22-year-olds were eligible for another subsidy for low-wage workers that was introduced in January 2017. This subsidy (the *lageinkomensvoordeel* or LIV subsidy) is aimed at firms employing workers who earn between 100% and 125% of the *adult* minimum wage.[6] Eligibility for the subsidy is based on two criteria: (1) the worker must work at least 1,248 hours in the firm over one calendar year, and (2) the worker must earn between 100% and 125% of the adult minimum wage on average over the calendar year. Because

[6]Specifically, it subsidizes 10% of wages for workers earning between 100% and 110% of the minimum wage and 5% of wages for workers earning between 110% and 125% of the minimum wage up to a maximum of 2,000 and 1,000 euros per year, respectively. The LIV subsidy for hours worked in year t is automatically paid out in September of year $t + 1$.

of these stringent eligibility criteria, only 5.7% of workers aged 22 at the time of the minimum wage reform (July 2017) are eligible over the year 2017. For those initially earning below the new minimum wage for 22-year-olds, 7.5% are eligible. In Section 2.5.5, we analyze how this policy impacts our findings.

Policy impacting 18–21-year-olds. Workers aged 18–21 (and aged 18–20 in the case of the second reform in 2019) were generally not eligible for the LIV subsidy discussed above[7]: however, they were covered by a much more comprehensive compensation scheme, the *jeugd lageinkomensvoordeel*, or JLIV subsidy. This subsidy was announced in November 2016, and implemented on January 1, 2018 (Tweede Kamer der Staten-Generaal, 2016c; Staatsblad van het Koninkrijk der Nederlanden, 2017b). The announcement mentions that there would be no compensation for hours worked by youth minimum wage workers in 2017 when the youth minimum wage increased, as we document in Appendix 2.A.2. Instead, the subsidy compensated firms for hours worked by 18–21-year-old workers from 2018 onward, covering those earning up to 117% of the minimum wage for 21-year-olds and up to 135% for 20-year-olds. The first compensation was paid out automatically to firms in September 2019, covering hours worked in 2018.[8]

At the time of announcement of the subsidy, it was also announced that the baseline subsidy for 2018 would be increased by 50%, with the stated purpose to compensate for the higher labor costs that firms incurred in 2017 due to the minimum wage increase (Tweede Kamer der Staten-Generaal, 2016c). As a result, the 2018 subsidy was around 120% of the increase in the minimum wage for 18–21-year-olds. For hours worked in 2019 the subsidy amount was lower, around 50% of the increase in the minimum wage. Subsidy amounts were lowered again in 2020 and the subsidy will be abolished in 2024.

Choice of treatment group. The JLIV subsidy clearly weakens the link between labor costs and the minimum wage increase for two of the potential treated age-groups, 21- and 20-year olds, and also could lead to wage spillovers for these age-groups because the subsidy covers workers earning up to 117% of the minimum wage for 21-year-olds and up to 135% for 20-year-olds. If firms took into account the subsidy they would receive in 2019 over their youth employment in 2018 when making employment decisions in 2017, we would underestimate any potential negative effects of the minimum wage increase. We therefore do not consider 21- and 20-year-olds in our baseline analysis of the 2017 minimum wage reform, focusing exclusively on 22-year-olds as our treatment group.[9] Similarly, when considering the 2019 minimum wage reform as a robustness check, we focus our analyses on 21-year-olds as firms knew they would not receive the subsidy for these workers–

[7]Due to the stringent criteria, only 3.7% (1.9%) of workers aged 21 (20) were eligible. For those initially earning below the new minimum wage only 1.1% (0.25%) were eligible.

[8]The subsidy only compensates firms for the increase in gross wages and not for other wage components (e.g. holiday allowance and contributions to the employee insurance), decreasing the effective compensation in labor costs.

[9]Estimates for 21- and 20-year-olds are shown in an appendix, for completeness. In these analyses, we consider only the first six months following the reform in July 2017 because there was no direct compensation of hours worked over 2017.

see Appendix 2.A.[10]

2.3.3 Data construction

Data sources. We construct a panel of monthly employment records of the universe of 20–27-year-olds with residency in the Netherlands, over 2007–2017. We merge several high-quality administrative sources collected by Statistics Netherlands, covering the entire population of the Netherlands. Employment data are based on income statements from employers to the Employee Insurance Agency (UWV). Individuals in the employment data are matched to municipal register data containing information on individual demographic characteristics such as gender, date of birth, residency spells, and country of birth for both the individual and their parents. Because many young workers are still in education, we match education enrollment data, which include information on the date of enrollment and graduation, as well as the level and type of education.

The employment data contain information on hours worked and gross monthly earnings for each individual worker's employment spell, separately by firm. In addition, we observe several job characteristics, such as the type of contract (e.g. intern, on-call-employee, temporary work agency; and fixed term or open-ended contract) and sector (but not occupation) of employment.

We define a job as a single employer-employee relationship during a given month. For individuals who have multiple employment relationships with the same firm in a given month, we sum the gross monthly earnings and hours worked at the firm-level.[11] We compute workers' average hourly wage for each job-month by dividing gross earnings by hours worked. This hourly wage measure is an approximation of the contracted hourly wage. We deflate hourly wages to 2015 euros using the annual consumer price index.

Determining the applicable minimum wage. As outlined above, we need the full-time workweek for each worker to determine the applicable hourly minimum wage. However, we do not observe workers' actual full-time workweek, so we approximate it as follows. For each job we observe the number of full-time equivalent days worked. We divide this by the number of working days in a month to obtain the part-time share of each job. We then divide the actual hours worked observed in the data by the calculated part-time share to obtain implied full-time hours for each job. We take average implied full-time hours over the months worked

[10] Also here, we show results for 20-year-olds, where the subsidy confounds effects, in an appendix for completeness.

[11] Monthly earnings are defined as gross basic income, which excludes premiums and special payments. Income for overtime hours worked are included in gross basic income as long as compensation for these hours worked is the same as the contractual wage. Hours worked are defined as basic hours worked, i.e. the total hours over which an employee receives a wage during an employment spell, including any overtime hours for which employees receive the same hourly compensation. Employers are obliged to pay the minimum wage over all hours worked, including any overtime: that is, they cannot increase overtime hours as a way to avoid minimum wage compliance.

in a calendar year, and assign each job to the closest category out of either 36, 38, or 40 hours per week.[12]

Sample restrictions. We restrict the sample in the following ways. First, we exclude jobs of individuals without a registered address in the Netherlands in a month. This removes around 2.5% of the jobs in the raw employment data. Second, we exclude jobs that are internships, sheltered employment arrangements, or directors / major shareholderships, since the minimum wage does not apply to these job types. This removes an additional 4.3% of jobs.

2.3.4 Descriptives

Table 2.2 shows descriptives for our sample, averaged over 2007–2017. This highlights a number of characteristics of the Dutch youth labor market. First, a non-negligible share of young workers earn the minimum wage: around 11% of workers aged 20–22 and 10% of workers aged 23–25. On average in the Netherlands, around 6% of workers earn the minimum wage (CBS, 2021). Second, many young workers are working while enrolled in education: 67 to 53% of 20–22-year-olds, 26% of 23–25-year-olds, and 12% of 26–27-year-olds. This contributes to the high incidence of part-time work, as seen from low average weekly hours and the low share of full-time jobs. Minimum wage workers are more likely to be enrolled in education and hold part-time jobs than those earning more than the minimum wage. Third, the majority of young workers are employed in temporary contracts, and temporary contract incidence is higher among minimum wage workers compared to non-minimum wage workers.[13] On-call and temp agency work are also relatively common, with the former more frequent for the youngest age-groups.

Minimum wage employment of 20–22-year-olds is concentrated in wholesale and retail trade, food and beverage services, and the employment placement industry (including temporary help agencies[14]). Together these account for around 60% of all minimum wage jobs held by these workers. Retail trade accounts for the largest share of minimum wage employment, accounting for 30% of minimum wage jobs held by 20-year-olds and 27% to 22% of minimum wage jobs held by 21–22-year-olds. While men and women are about equally represented, first- or second-generation migrants are over-represented among minimum wage workers, particularly among 23–27-year-olds. Lastly, while between 26% and 29% of minimum wage jobs among 20–22-year-olds are held by individuals not enrolled in education who live with their parents, a non-negligible share of minimum wage jobs are held by individuals in single- or two person households. In particular, among 20–22-year-olds, between 7% and 18% of minimum wage jobs are held by individuals in one- or two-person households who are not enrolled in education. In our analyses, we study impacts on minimum-wage workers not enrolled in ed-

[12]See Appendix 2.E for details. Our results are robust to instead assigning every worker a 36 or a 40 hour work week (the lower and upper bound, respectively).

[13]A temporary contract is defined as a contract with a fixed end date or a fixed duration.

[14]In the Netherlands, as is common in other countries, temporary help agencies are for-profit organizations which do not receive additional subsidies from the government.

ucation (47% for 22-year-olds) separately because students are more likely to be transient occupants of minimum-wage jobs.

Table 2.3 summarizes the real wage distributions of the number of jobs and total hours worked, expressed as the distance from the real minimum wage in €1 increments, for 20–27-year-olds over January to June 2017, the six-month period leading up to the first step of the minimum wage reform. This highlights that youth employment is strongly concentrated in low-wage jobs: around 10% of jobs held by 20–22-year-olds as well as 23–25-year-olds pay no more than the minimum wage, and more than 50% pay no more than €2.00 above the minimum wage. By contrast, employment of 26–27-year-olds is less concentrated in low-wage jobs.[15] This concentration of young workers in jobs paying close to the minimum wage underscores the importance of minimum wage policy for this labor market segment.

[15]Further, the share of total hours worked at low wages is slightly below the share of the total number of jobs, reflecting that lower-wage jobs have lower weekly hours on average.

Table 2.2 Summary statistics on the Dutch youth labor market by age-group

	Age 20		Age 21		Age 22		Ages 23–25		Ages 26–27	
	MW	> *MW*	*MW*	> *MW*	*MW*	> *MW*	*MW*	> *MW*	*MW*	> *MW*
Job characteristics										
Full-time	0.20	0.20	0.23	0.27	0.26	0.34	0.29	0.48	0.34	0.59
Hours worked per week	17.49	17.45	18.68	19.89	19.81	22.44	21.27	27.45	23.74	31.19
Real hourly wage	4.72	9.48	5.52	10.82	6.52	12.06	7.76	13.70	7.66	15.18
Temporary contract	0.69	0.64	0.68	0.63	0.67	0.61	0.66	0.54	0.64	0.45
On-call contract	0.16	0.23	0.15	0.19	0.15	0.16	0.12	0.10	0.09	0.05
Temporary help agency contract	0.10	0.13	0.11	0.13	0.12	0.13	0.18	0.11	0.19	0.08
Worker characteristics										
Female	0.53	0.50	0.52	0.50	0.51	0.51	0.51	0.51	0.50	0.50
Migration background	0.21	0.19	0.22	0.19	0.23	0.20	0.28	0.21	0.35	0.23
Sector of employment										
Wholesale trade	0.04	0.04	0.05	0.04	0.05	0.04	0.05	0.05	0.04	0.06
Retail trade	0.30	0.22	0.27	0.18	0.22	0.15	0.16	0.11	0.14	0.08
Food & beverage service	0.16	0.12	0.14	0.10	0.13	0.08	0.10	0.05	0.08	0.04
Employment placement	0.14	0.17	0.16	0.17	0.18	0.17	0.24	0.14	0.25	0.11
Other	0.36	0.46	0.39	0.52	0.43	0.56	0.45	0.65	0.47	0.71
Educational enrollment										
Student	0.67	0.63	0.60	0.52	0.53	0.42	0.40	0.24	0.21	0.11
Non-student household types										
Living with parents	0.26	0.29	0.28	0.34	0.29	0.35	0.25	0.27	0.18	0.15
Not living with parents	0.07	0.08	0.12	0.14	0.18	0.23	0.35	0.49	0.61	0.75
Unique individuals	14,533	116,390	14,968	120,802	15,835	124,264	46,254	396,335	17,031	291,277
Total population	207,450		208,537		209,183		630,091		419,469	
Share minimum wage workers	0.11		0.11		0.11		0.10		0.06	
Monthly job observations	18,977,221		19,643,549		20,160,057		62,589,324		42,866,818	

Notes: MW = minimum wage. Table reports means over monthly jobs observations in the sample period 2007–2017 (N=164,175,204). For each age-group, the first column reports means for jobs with hourly wages at or below the minimum wage and the second column reports means for jobs with hourly wages above the minimum wage. Hourly wages are in real euros relative to 2015.

Table 2.3 Wage distributions in the Dutch youth labor market

	Age 20		Age 21		Age 22		Ages 23–25		Ages 26–27	
	Jobs	Hours	Jobs	Hours	Jobs	Hours	Jobs	Hours	Jobs	Hours
Distance to MW										
≤€ 0	0.11	0.11	0.11	0.11	0.11	0.10	0.10	0.08	0.06	0.04
€ 0 – € 1	0.21	0.19	0.17	0.15	0.15	0.13	0.17	0.14	0.10	0.08
€ 1 – € 2	0.19	0.18	0.17	0.15	0.17	0.15	0.15	0.14	0.10	0.09
€ 2 – € 3	0.14	0.15	0.14	0.14	0.14	0.15	0.13	0.13	0.10	0.10
€ 3 – € 4	0.11	0.11	0.12	0.13	0.12	0.13	0.11	0.12	0.10	0.11
€ 4 – € 5	0.08	0.08	0.09	0.10	0.10	0.11	0.09	0.10	0.10	0.11
€ 5 – € 6	0.05	0.06	0.07	0.08	0.07	0.08	0.07	0.09	0.09	0.10
≥€ 6	0.12	0.12	0.13	0.14	0.14	0.15	0.17	0.20	0.34	0.37

Notes: MW = minimum wage. Each row reports the fraction of total employment in each € 1 increment distance from the relevant hourly minimum wage, averaged over monthly jobs observations between January 2017 – June 2017 (N=15,099,912). For each age-group, the first column reports means for jobs and the second column reports means for hours worked.

2.4 Empirical approach

To identify the impact of the youth minimum wage on young workers' employment and earnings, we exploit the increase in the youth minimum wage on July 1, 2017. We use a difference-in-differences design to estimate the impact of the minimum wage on the number of jobs and total hours worked by 22-year olds.[16] We adopt the bunching approach developed in Cengiz et al. (2019), disaggregating the aggregate employment effect of the minimum wage by constituent wage bins throughout the hourly wage distribution. The impact of the minimum wage is then inferred by examining changes in employment in wage bins locally around the minimum wage.

The intuition behind this approach is that an increase in the minimum wage induces a change in the wage distribution for affected workers. The distribution is altered in three ways. First, if firms comply with the higher legislated minimum wage, there is a reduction in the number of jobs that were paying below the minimum wage prior to the reform. Second, not all jobs previously paying below the new minimum wage need to disappear. Jobs that are preserved and experience a mandated wage increase could appear at the new minimum wage, creating a spike at the new minimum wage. Third, the minimum wage change may induce spillover effects – changes in employment further up the wage distribution. Such spillovers could be driven by various factors, such as labor-labor substitution (Fairris and Bujanda, 2008; Clemens et al., 2021), hedonic-based labor supply substitution (Phelan, 2019), fairness concerns about maintaining within-firm wage-hierarchies (Giuliano, 2013; Dube et al., 2019), reallocation of employment toward higher paying firms (Dustmann et al., 2021), and increased job-search because the new minimum wage exceeds the reservation wage for more workers (Flinn, 2006).

[16] Analyses for 21- and 20-year-olds are reported in Appendix 2.C.1. We focus on 22-year-olds for reasons outlined above.

However, these spillover effects are likely to fade out further up the wage distribution, and any changes in the distribution observed beyond this fade-out are likely not caused by minimum wage increases. This implies that the employment effect of the minimum wage should be inferred by examining employment changes locally around the new minimum wage.

To estimate the impact of the increase in the minimum wage on the number of jobs and hours worked, we first compute monthly distributions of hourly wages for each age-group. Each job in our sample is assigned to a bin relative to the age-specific new real hourly minimum wage effective July 2017 (defined as MW_a), henceforth referred to as a (wage) bin. The width of the bins in our baseline specification is €0.50.[17] For example, jobs with an hourly wage between the post-reform age-specific minimum wage MW_a and $MW_a + 0.49$ are assigned to $bin_a = 0$ and jobs with a real hourly wage between $MW_a - 0.01$ and $MW_a - 0.50$ are assigned to $bin_a = -1$. The distance to the minimum wage is winsorized at −€3 and +€12 of MW_a, yielding a total of 30 bins. Next, we collapse the data to the number of jobs and total hours worked by age, wage-bin, and month.

Workers aged 23–25, who are not affected by the minimum wage reform, serve as our control group. For these workers, we calculate the same age-specific bins relative to their (the adult) minimum wage.[18]

Descriptive evidence. Figure 2.1 describes the data and illustrates our approach. Panel A shows the pre- and post-reform distributions of hours worked relative to the new minimum wage for the treated age group: 22-year-olds. It is clear that the reform induced a rightward shift in the wage distribution, with a substantial spike at the new minimum wage. There also appears to be an increase in jobs higher up the wage distribution, up to about 3 euros (i.e. 6 wage-bins) above the new minimum wage. The second figure shows the distribution of hours worked for the same time period for our control group (23–25-year-olds), relative to their minimum wage. There is no evident change in the control group's distribution following the reform in the minimum wage for treated age groups. Our difference-in-difference approach compares the change in employment in each wage bin for treated age groups to the change in employment in the same wage bin for the control group. We then sum over employment changes across wage bins to arrive at the total employment effect.

Note that for the control group, bins below the new minimum wage should be empty since there has been no shift in their minimum wage. However, due to measurement error we have some observations below the new minimum wage. This is common in the literature (e.g. Kabátek (2021) finds the same using earlier Dutch data). We have also estimated our baseline models artificially setting these

[17]Both the minimum wage and the wages in each job are rounded down to the nearest 50 cents, following Cengiz et al. (2019) to address potential measurement error in wages. However, our results are unaffected when we do not round hourly wages before assigning them to wage bins relative to the unrounded new minimum wage.

[18]In Appendix Tables 2.C.1 and 2.C.2 we also show results using workers aged 26–27 as a control group: results have the same sign but are larger in magnitude. We present the more conservative estimates as a baseline.

bins to zero – essentially making the analysis of the bins below the minimum wage a 'before-after' comparison instead of a difference-in-differences, and find very similar results.[19]

Estimating equation. We use the following regression specification to estimate the impacts of the minimum wage on employment and earnings of treated workers:

$$Y_{bat} = \sum_{b=-6}^{23} \sum_{\substack{\tau \notin \{-6,...,-1\}; \\ \tau=-30}}^{5} \beta_{b\tau} \times I_{treat} + \gamma_{ba} + \delta_{bt} + \eta_{bam} + \varepsilon_{bat}, \tag{2.4.1}$$

where b indexes wage bins relative to the new minimum wage (such that $b = 0$ includes the new minimum wage); a indexes age groups (22, 23, 24 and 25), t time in months; and τ month relative to treatment (such that $\tau = 0$ in the month of the reform). As such, Y_{bat} is the outcome variable for bin b at time t for age-group a. Further, m denotes calendar months. The coefficients of interest are $\beta_{b\tau}$ for $\tau \geq 0$. These bin-specific coefficients are obtained from the treatment dummy I_{treat}, which equals 1 if the minimum wage was raised for age-group a τ months from time t. We include up to 30 months before treatment to estimate pre-trends. Conditional on our controls, these coefficients capture the difference-in-differences estimates of the minimum wage increase on our outcome variables by wage bin in period τ, relative to the average of the six-month pre-treatment period ($\tau \in \{-6, ..., -1\}$). In our main analysis, we restrict the estimation window to the six months immediately following the reform, as discussed above.

Outcome variables. We estimate the impact of the minimum wage increase on two outcome variables. The first is the monthly number of jobs in each wage bin, relative to the population of 22-year-olds. In other words, it is the age-specific number of jobs per capita by wage bin. This captures employment adjustments at the extensive margin. The second outcome variable is the total hours worked per capita by wage bin, capturing employment adjustments both at the extensive and intensive margins. This is defined as monthly total hours worked in each wage bin, relative to the population.[20] The normalization by age-specific populations accounts for changes in population by age that could (mechanically) impact the number of jobs held and total hours worked by workers of different ages.

Control variables. We include a full set of wage bin by age-group fixed effects, γ_{ba}, to capture time-invariant differences in the shape of the wage distribution by age-group. Hence, we only retain changes in employment by age-group and/or wage bin over time. The specification additionally controls for wage bin by month fixed effects, δ_{bt}, absorbing any evolution in the wage distribution shared across age-groups due to for example the business cycle. To flexibly account for age and

[19] See Appendix Figure 2.C.8.

[20] The population of 22-year-olds is the monthly number of individuals aged 22 with a registered address in the Netherlands.

bin-specific seasonality, we also include wage bin by age-group by calendar month fixed effects η_{bam}.[21]

Assumptions for causal identification. Causal interpretation of our estimates relies on two assumptions: parallel trends, and the Stable Unit Treatment Value Assumption (SUTVA). Parallel trends means that absent the reform, the change in each constituent wage bin relative to the new minimum wage should be the same for the treated and control groups. In support of this assumption, we estimate leading terms up to 30 months before the treatment and find no evidence of pre-trends (shown below).

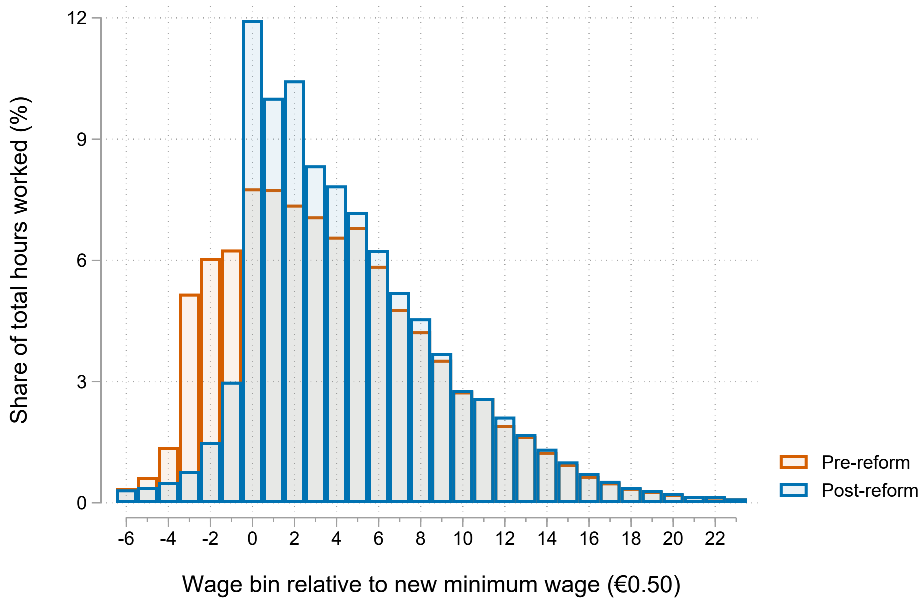
SUTVA requires an absence of spillover effects to our control group such that changes in the number of jobs and total hours worked for each age-group depend on the treatment status of that age-group alone. Violation of SUTVA would bias the estimated employment effects of the minimum wage. If, for example, firms substitute employees aged 22 with employees aged 23, because 22-year-olds are becoming more expensive due to the minimum wage increase, using 23-year-olds as a control group overestimates the job loss experienced by 22-year-olds. While choosing older age-groups as a control group may limit the threat of cross-age spillover effects, it is also less likely that the parallel trends assumption is satisfied. For this reason, we choose 23–25-year-olds as our baseline control group. As a robustness check we use 26–27-year-olds and find similar results.[22]

[21]Because we estimate treatment effects up to 30 months prior to treatment (i.e. starting in January 2015), these seasonal effects are identified using variation from January 2007 up to December 2014.

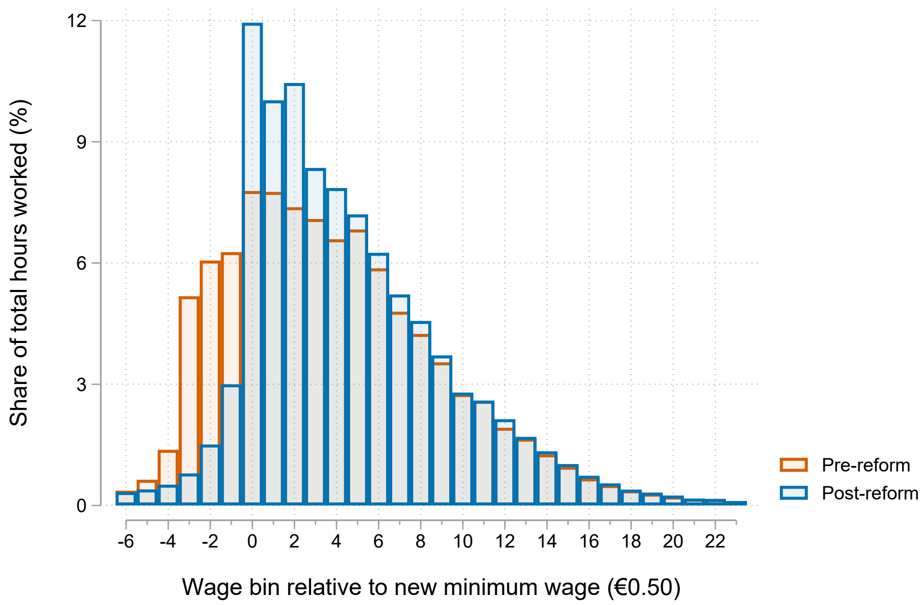
[22]See Appendix Tables 2.C.1 and 2.C.2.

Figure 2.1 Wage distributions for treated and control age groups before and after the minimum wage change

(a) Treated age groups: 22-year-olds



(b) Control group: 23–25-year-olds



Notes: We calculate the monthly share of total hours worked for each €0.50 bin relative to the relevant new minimum wage in the six months prior to the reform and the six months after the reform, and then plot averages. Distributions are censored at 3 euros below and 12 euros above the new minimum wage.

2.5 The impact of youth minimum wages on labor market outcomes

2.5.1 Impact of the 2017 minimum wage increase on hours worked and number of jobs

We begin our analysis by estimating the effect of the 2017 increase in the minimum wage on the number of jobs and hours worked in each €0.50 wage bin for 22-year-olds. Figure 2.2 shows results for hours worked, presenting averages of bin-specific parameters from equation (2.4.1) over the first six months following the reform. Changes are normalized by the average age-specific number of hours worked per capita over the six months prior to the reform such that these estimates reflect the change in each wage bin as a percentage of the average six-month pre-reform total number of hours worked. For example, an increase of 5% in a given bin means that, in that bin, hours worked increased by 5% relative to total hours worked by 22-year-olds (hereinafter: treated workers) in the six months prior to the reform.[23] The orange line presents running cumulative estimated effects.[24] Standard errors are calculated using the delta method.[25]

We highlight three main findings. First, the increase in the minimum wage has not changed total hours worked by treated workers. The cumulative impact of the minimum wage becomes statistically indistinguishable from zero beyond the wage bin €1.50 above the new minimum wage. This corresponds approximately to the 50$^{\text{th}}$ percentile of the six-month post-treatment hourly wage distribution. At the same time, we observe a clear and precisely estimated reduction in the number of hours worked in wage bins below the new minimum wage for treated workers, which shows that the minimum wage increase had a substantial bite. The decline in hours worked below the new minimum wage amounts to around 15% of total pre-reform employment among treated workers. When moving further up the wage distribution, the cumulative impact of the minimum wage turns slightly positive. However, this positive effect is small and fades out as we move further along the wage distribution.[26]

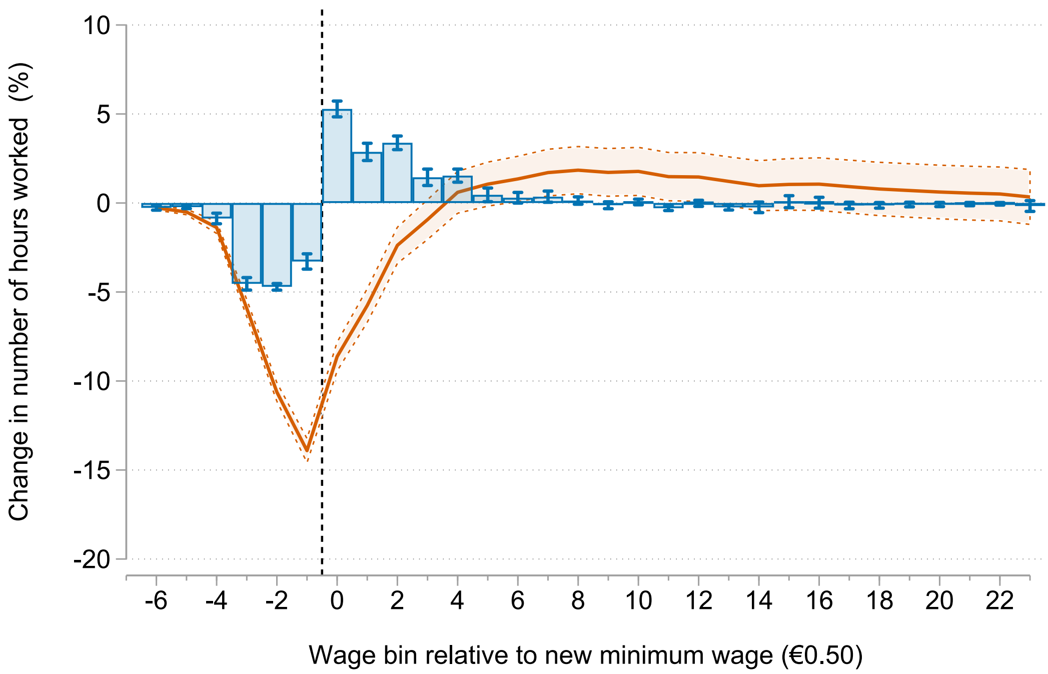
[23]In Appendix Figure 2.C.7 we report results showing the employment change in a bin relative to employment in the pre-reform period in that specific bin instead of relative to total employment by age. We find similar patterns to our main results, but much noisier estimates in particular higher up the distribution where the bins are sparsely populated. The advantage of the scaling we employ in the main text is that the effects per bin can simply be added to get the total employment effect, given by the orange line in the main figures.

[24]The line is constructed by calculating the running sum of the estimated effects up to each bin. For example, at 2 euros above the new minimum wage, the orange line shows the total change in hours worked (relative to total hours worked in the six months prior to the reform) following the reform for bins up to 2 euros above the new minimum wage.

[25]We use robust standard errors throughout: as an alternative, we have tried clustering by age $\times$ wage bin, which substantially shrinks standard errors.

[26]Since the hourly minimum wage depends on the standard workweek in each sector, firms could in principle adjust the standard workweek to (partially) adjust to the minimum wage. In practice this is not easy, since the standard sectoral workweek is laid out in a collective bargaining agreement and also covers workers not affected by the minimum wage reform. In Appendix Figure

Figure 2.2 Impact of 2017 minimum wage increase on number of hours worked by 22-year-olds by wage bin (blue bars) and the running sum (orange line)



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show, by age group, the average estimated change in total hours worked in each €0.50 wage-bin in the six month post-reform period relative to the total hours worked in the six months prior to the reform. E.g., 5% for bin 1 reflects a 5% increase in hours worked by 22-year-olds earning the new minimum wage and up to €0.50 above it, relative to total hours worked by 22-year-olds in the pre-reform period. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

Second, total hours worked at the new minimum wage increase markedly, creating a spike at the bin containing the minimum wage. This is consistent with some jobs being preserved and experiencing a wage increase in accordance with the higher legislated minimum wage.

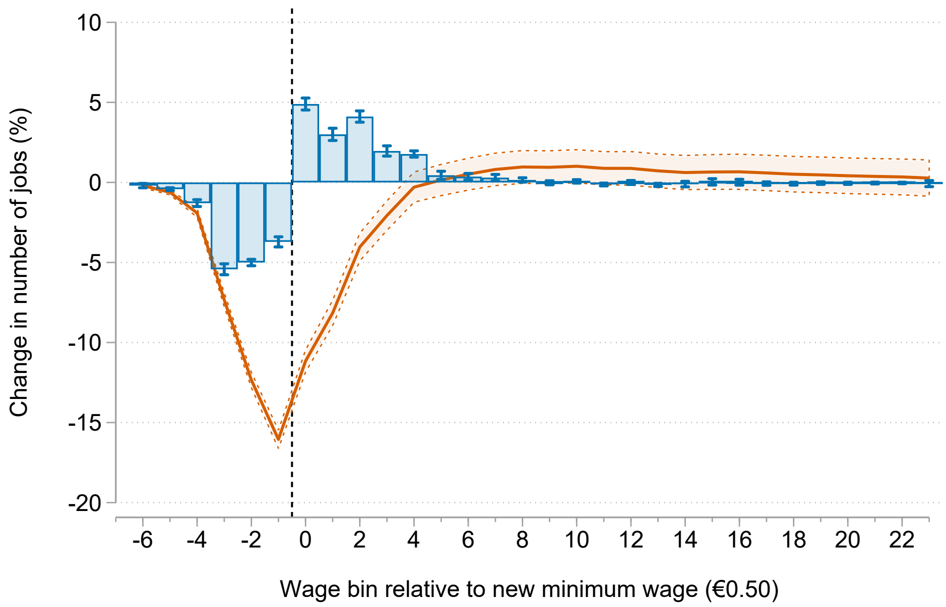
Third, there are substantial spillover effects to jobs above the minimum wage: the number of hours worked does not only increase at the new minimum wage, but also in bins above the minimum. These spillovers fade out further up the wage distribution. For each treated age-group, the impact of the minimum wage largely fades out beyond €4 above the minimum wage. Over 90% of spillover effects occur in wage bins up to €2.50 above the new minimum. This point corresponds to approximately the 64$^{\text{th}}$ percentile of the six-month post-treatment hourly wage distribution. These findings are in line with previous studies finding spillover effects

2.D.1 we show that there has been no change in contracted hours surrounding the reform.

up to €2 – €3 above the minimum wage (e.g. Brochu et al., 2018; Cengiz et al., 2019; Harasztosi and Lindner, 2019; Gopalan et al., 2020).

We perform an analogous analysis for the number of jobs in Figure 2.3. The reallocation of jobs around the new minimum wage broadly mirrors the results for total hours worked. However, the cumulative impact of the minimum wage on the number of jobs becomes statistically indistinguishable from zero at any point beyond €2 above the new minimum wage. As with total hours worked, the minimum wage increase had a substantial bite. The reduction in jobs below the new minimum wage is slightly larger compared to the reduction in total hours, reflecting that low-wage youth workers work relatively fewer hours. As with hours worked we find substantial spillover effects, with the vast majority occurring within wage bins up to €2.50 above the new minimum wage.

Figure 2.3 Impact of the 2017 minimum wage increase on the number of jobs held by 22-year-olds by wage bin (blue bars) and the running sum (orange line)



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show, by age group, the average estimated change in the number of jobs in each €0.50 wage-bin in the six month post-reform period relative to the number of jobs in the six months prior to the reform. E.g., 5% for bin 1 reflects a 5% increase in the number of jobs held by 22-year-olds earning the new minimum wage and up to €0.50 above it, relative to the number of jobs held by 22-year-olds in the pre-reform period. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

We do not find any reallocation of hours worked around the minimum wage for our control group, 23–25-year-olds, compared to older age groups (either 26–

27-year-olds or 30–35-year-olds).[27] This finding is reassuring since positive changes in low-paid hours worked for our control group relative to older workers would be indicative of positive spillovers of the youth minimum wage increase, violating SUTVA. We do see some small positive changes in wage bins higher up the wage distribution suggesting other labor demand shifts may be occurring for these higher-paid workers. In particular, we find some small positive changes in hours worked for 23–25-year-olds compared to 30–35-year-olds high up the distribution.[28] This underscores the importance of studying effects for wage bins close to the minimum wage, to avoid capturing effects much further up the wage distribution which are unlikely to be caused by minimum wage changes (Cengiz et al., 2019).

2.5.2 Dynamic impacts

We continue our analysis by tracing the impact of the minimum wage increase resulting from the July 2017 reform month-by-month from January 2015 to right before the start of the second step of the reform, June 2019. This exercise serves a dual purpose. First, we can assess the presence of preexisting trends by examining leading terms. Second, by tracing the impact of the minimum wage over the months following the reform, we can document potential dynamic treatment effects. While we find no short-term impact of the reform, the mid- and long-term impacts may differ if firms face adjustment costs (Sorkin, 2015; Aaronson et al., 2018). Therefore, we may underestimate potential employment losses when focusing on the six-month post-reform period.

We extend our primary sample to June 2019 and estimate a model similar to equation (2.4.1), but now including terms up to $\tau = 23$. We subsequently sum the estimated $\beta_{b\tau}$ over the wage bins below the new minimum wage ($-6 \leq b \leq -1$), and over the wage bins including the new minimum wage up to €4.00 above the new minimum wage ($0 \leq b \leq 8$). We choose €4.00 because we find no spillovers beyond this point. As before, changes are normalized by age-specific employment per capita in the month prior to the reform.

Figure 2.4 shows the month-by-month changes in 'missing' ($-6 \leq b \leq -1$) and 'excess' ($0 \leq b \leq 8$) total hours worked by treated workers.[29] Importantly, we find no evidence of pre-trends up to 30 months before the reform, indicating that treated low-wage workers are on parallel trends with low-wage workers aged 23–25, conditional on our controls. The absence of pre-trends also indicates that there are no anticipatory effects. Second, there is a sharp decline in missing hours worked and a concomitant increase in excess hours worked at the time of treatment. The employment response remains relatively stable over the entire period of the reform.

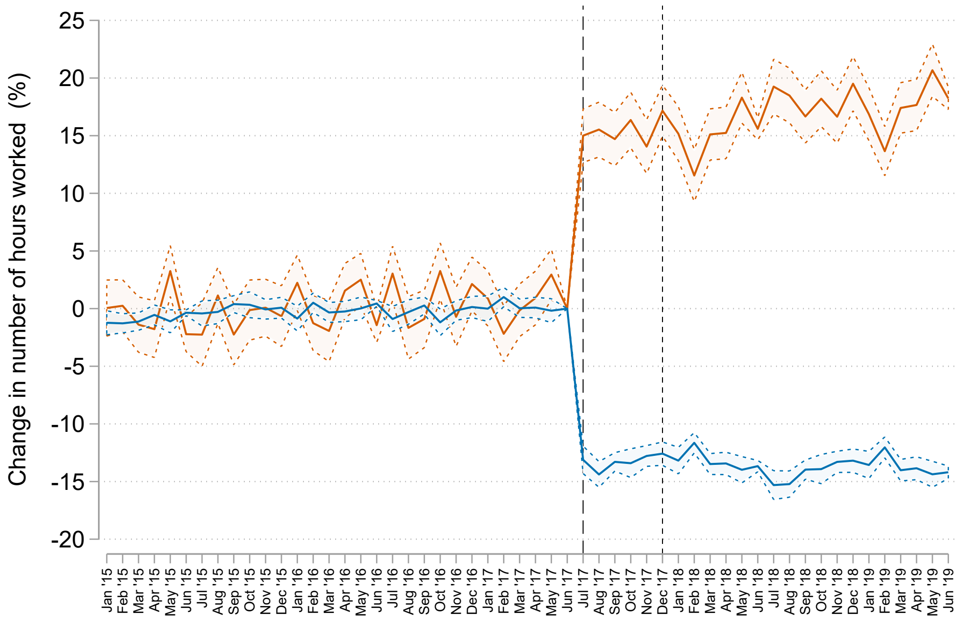
[27] Results from this placebo test are reported in Appendix Figure 2.C.9 and Appendix Table 2.C.13. One possible explanation for this finding is the frequent use of age-specific pay scales in collective bargaining agreements that cover most firms. Such frictions might prevent firms from also having to adjust wages of older workers if younger workers see a wage increase. Another possible reason is that workers of different ages are segregated in different firms.

[28] Note that these workers' wages are too high to qualify for the LIV subsidy.

[29] Appendix Figures 2.C.3 and 2.C.4 show corresponding estimates for hours worked and the total number of jobs held by 21- and 20-year olds.

This suggests that the adjustment occurs quickly, such that our baseline results discussed in the previous section are informative in the somewhat longer run, as well.

Figure 2.4 Impact of the 2017 minimum wage increase on number of hours worked above and below the new MW by 22-year olds from January 2015 to June 2019



Notes: MW = minimum wage. Figure plots estimates of the monthly change in hours worked up to 4 euros above the new MW in orange and hours worked below the new MW in blue. The omitted month is the one before the first minimum wage increase (June 2017). Shaded areas are 95% confidence intervals. Vertical dashed lines show the six month period used to infer the short-term impact of the minimum wage increase.

2.5.3 Quantifying employment elasticities and spillover shares

As is common in the literature, we use our results to calculate the overall short-term impact of the minimum wage increase on age-specific employment, and the elasticities with respect to the minimum wage and the own wage. We use the estimated six-month average changes in employment up to a point in the post-reform wage distributions where the impact of the minimum wage fades out, which we call $\overline{W}$. Since we find no evidence of spillovers beyond €4.00 above the minimum wage, we choose $\overline{W} = 4$ at baseline. This choice restricts our calculations to jobs around the minimum wage, so that any incidental changes higher up the wage distribution are not included. We find similar results using $\overline{W} = 3$ or $\overline{W} = 5$.[30]

[30] Appendix Tables 2.C.1 and 2.C.2 report results. These tables also show results for $\overline{W} = \infty$, which includes changes for the entire wage distribution rather than only low-wage jobs. This is not our preferred specification as changes much higher up the wage distribution are unlikely to be caused by minimum wage changes, but our main findings are similar: we do not find evidence of

Results are reported in Table 2.4: the first column for hours worked and the second for jobs: as before, we focus on results for 22-year-olds ('treated workers').[31]

Table 2.4 Effects of the 2017 minimum wage increase on employment, average wages, wage elasticities and spillover share for 22-year-olds

	Hours	Jobs
Δ Employment	0.017**	0.008
	(0.007)	(0.005)
Employment Elasticity wrt MW	0.096**	0.046
	(0.038)	(0.029)
Own-Wage Elasticity (OWE)	0.567***	0.226
	(0.216)	(0.143)
Wage Elasticity wrt MW (MWE)	0.217***	0.254***
	(0.008)	(0.006)
Δ Average Wage	0.039***	0.045***
	(0.001)	(0.001)
Spillover Share	0.681***	0.675***
	(0.012)	(0.008)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Elasticities with respect to the minimum wage are based on an increase in the minimum wage of 18.7% for 22-year-olds, minus the increase in the minimum wage for adults of 0.9%. All estimates are based on around 82 million monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We find that the minimum wage hike increased total hours worked by 1.7% when considering changes up to €4.00 above the new minimum wage (first row of Table 2.4). In line with this, the elasticity of total hours worked to the minimum wage (Minimum Wage Elasticity or MWE) is positive, estimated at 0.096 for treated workers, and the 95% confidence interval rules out MWEs below 0.022. For jobs we can rule out MWEs below below −0.011 for treated workers (second row in Table 2.4). Note that this is a short-term elasticity and hence may understate the long-term responsiveness of overall low-wage employment if firms face adjustment costs. However, we find no substantial decline in low-wage employment in the 24 months following the reform (Figure 2.4), suggesting that any such adjustments have already been made.

A disadvantage of the MWE is that it is difficult to compare across studies be-

disemployment effects, and spillover shares are still sizable at 47%, compared to 68% when using $\overline{W} = 4$.

[31] Estimates for 21- and 20-year olds are reported in Appendix Table 2.C.3.

cause the minimum wage may have a different bite in different settings. To facilitate direct comparisons of our findings to those reported in the literature, we calculate the implied own-wage elasticity (OWE), which measures the responsiveness of employment to a change in the average wage. The OWE effectively normalizes the MWE by the change in the average wage due to the minimum wage increase, recognizing that a more binding minimum wage increase tends to have a larger impact on the average wage.

The fourth rows of Table 2.4 shows that the increase in the minimum wage had a significant impact on average wages, with an elasticity of 0.217 based on hours and 0.254 based on jobs. We obtain the own-wage elasticity of low-wage hours worked by dividing the estimated change in low-wage employment up to €4.00 above the new minimum wage by the change in average wages up to this point in the distribution. Results are reported in the third row of Table 2.4, showing an estimated OWE of 0.567 for hours worked and 0.226 for jobs. Although estimates are imprecise, the 95% confidence interval clearly rules out OWEs below −1 for both hours and jobs. This implies that total earnings increase for treated workers.

Our findings are most directly comparable to studies that also consider changes in youth minimum wages. Our findings are in line with Hyslop and Stillman (2007), who study the impact of an age-specific minimum wage reform in New Zealand on workers aged 16–19 over 2001–2002. Their implied MWEs of weekly hours worked are 0.49 for 16–17-year-olds and 0.09 for 18–19-year-olds.[32] On the other hand, Pereira (2003) finds that the 1987 repeal of a lower minimum wage for 18–19-year-olds in Portugal had a negative impact on the relative employment and hours worked of this age-group. The reported MWEs between −0.4 and −0.2 are well outside the 95% confidence interval of MWEs found in our setting. However, the Portuguese reform resulted in a 35.5% increase in the real minimum wage, almost twice the size of the one we study.

Looking beyond studies considering age-specific minimum wages, our results are consistent with recent studies on the impact of changes in the general minimum wage on teenage employment which generally find no or modest effects (e.g. Allegretto et al., 2011; Dube et al., 2016; Gittings and Schmutte, 2016; Allegretto et al., 2017; Cengiz et al., 2019). On the other hand, our estimated MWEs are well outside the range of those reported in some recent studies that find a negative impact of general minimum wage increases on teenage employment (e.g. Thompson, 2009; Neumark et al., 2014a,b; Sabia et al., 2016, 2012).[33]

Lastly, we quantify the relative importance of spillover effects in the total wage impact, following the approach of Cengiz et al. (2019). First, we compute a counterfactual change that would occur if there were no spillovers. This is the wage increase that results from moving the jobs that were initially below the new mini-

[32] Implied elasticities are not reported by Hyslop and Stillman (2007) but computed by Belman and Wolfson (2014).

[33] For an extensive overview of this literature see Neumark and Wascher (2008); Belman and Wolfson (2014); Dube (2019). For recent meta-studies see Doucouliagos and Stanley (2009); Belman and Wolfson (2014); Wolfson and Belman (2019).

mum wage to exactly the new minimum wage.[34] Next, we infer the spillover share of the total wage impact by taking the difference between the actual and 'no spillover' wage increase. We find that in the absence of spillovers, wages would increase by 1.2% for treated workers. Results for employment are very similar, as seen in the second column of Table 2.4. This implies spillovers are quantitatively important, constituting around 68 percent of the total found wage gains resulting from the 2017 minimum wage increase.

2.5.4 Comparison to the 2019 minimum wage increase

We repeat the analysis using variation in the minimum wage for 21-year-olds induced by the July 2019 reform (recall that 22-year-olds were already at the adult minimum wage, so that nothing changed for them in 2019). We focus our analysis on 21-year-olds (henceforth: treated workers) as these are unaffected by the wage subsidy for young workers.[35] For this analysis we extend our primary sample to December 2019. We assign jobs to €0.50 wage bins based on the difference between the hourly wage and the age-specific minimum wage effective July 2019, and estimate equation (2.4.1).[36]

Figure 2.5 shows the impact of the minimum wage increase as the six-month averaged change in the number of hours worked for treated workers in each wage bin. We again normalize by the average total number of hours worked per capita by age-group over the six months prior to the reform. Similar to the 2017 reform, the 2019 increase in the minimum wage did not result in reduced employment for treated workers.[37] The cumulative impact of the minimum wage is positive beyond €2.50 above the new minimum, but converges to zero again after around €7 above the new minimum wage. There is a clear reduction in hours worked in wage bins below the new minimum wage, showing that the additional increase in the minimum wage had a substantial bite. In line with our findings for the 2017 reform, we observe a spike in hours worked at the minimum wage and additional increases in hours worked in wage bins above the new minimum wage. As for the prior reform, the cumulative impact of the minimum wage on the number of low-wage jobs is statistically indistinguishable from zero.[38]

We perform a placebo test using 22-year-olds and find small positive estimates on their hours worked in 2019 compared to 23–25-year-olds. This suggests that there might be some spillovers towards 22-year-olds in 2019, even though we find no negative impacts on hours worked for 21-year-olds. Results are shown in Appendix Figure 2.C.10 and Appendix Table 2.C.13. Overall, we conclude that employment effects of the 2017 and 2019 minimum wage reforms are very similar, but we put more stock in our baseline estimates for 2017 where we find no employment effects

[34] For more details, see Appendix 2.F.

[35] Results for 20-year-olds are shown in Appendix Figure 2.C.5.

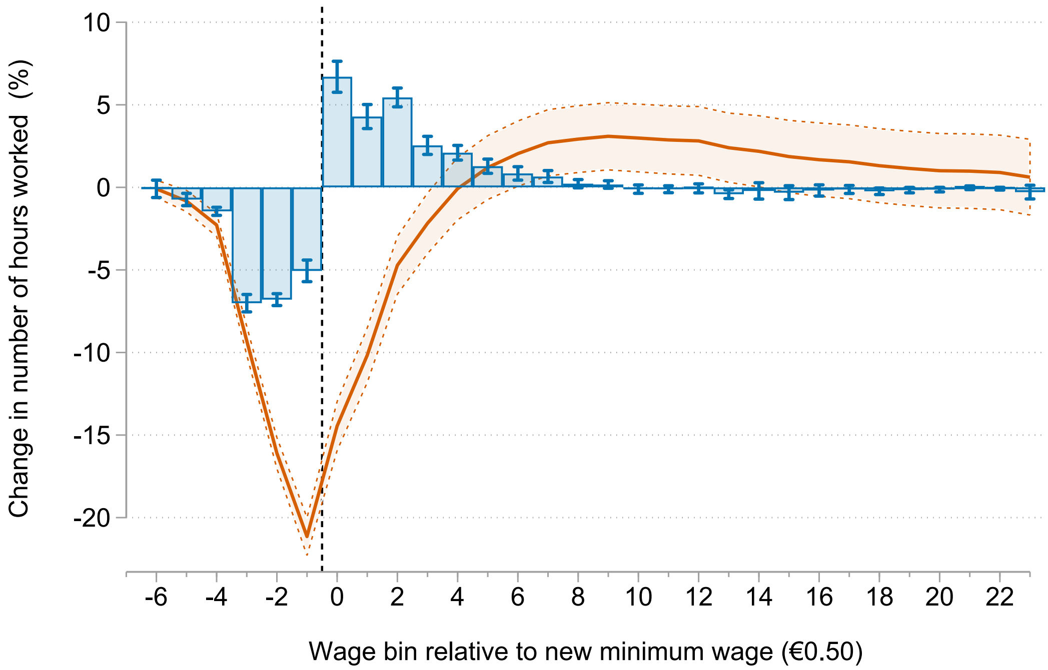
[36] We still estimate leading terms starting in January 2015, such that leading terms now start at $\tau = -54$ instead of $\tau = -30$.

[37] Appendix Table 2.C.4 contains the estimated employment effects and elasticities.

[38] See Appendix Figure 2.C.6.

for non-treated age groups.

Figure 2.5 Impact of the 2019 minimum wage increase on number of hours worked by 21-year olds by wage bin (blue bars) and the running sum (orange line)



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show the average estimated change in total hours worked in each €0.50 wage-bin in the six month post-reform period relative to the total hours worked in the six months prior to the reform. E.g., 7% for bin 1 reflects a 7% increase in hours worked by 21-year-olds earning the new minimum wage and up to €0.50 above it, relative to total hours worked by 21-year-olds in the pre-reform period. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

2.5.5 Robustness to LIV wage subsidy

A potential concern with our results is the impact of the LIV wage subsidy, discussed in Section 2.3.2. Figure 2.6 therefore shows estimates of our baseline models where we exclude workers who are eligible for the LIV subsidy. Specifically, we restrict the sample to employment spells with fewer than 1,248 hours worked in a calendar year. Note that this sample selection is more restrictive than the LIV-eligibility requirements, which additionally impose bounds on average wages. While 16.9% of workers aged 22 at the time of the reform would be eligible for the LIV subsidy based on their hours worked over 2017, only 5.7% meet both LIV-eligibility requirements.[39] Panel A shows results for 22-year-olds in the 2017 reform, and panel B for 21-year-olds in the 2019 reform. Reassuringly, these estimates are very similar to the ones for the full sample, and we do not find evidence of negative impacts on hours worked when using the same wage-bin cut-off. That is, among the large majority of treated workers who were not eligible for any wage subsidy, there is no evidence of aggregate employment declines.

A concern might be that firms strategically retain workers until they work at least 1,248 hours in the year: this would allow firms to obtain the subsidy and could bias our estimates of employment effects.[40] To test this, Figure 2.7 shows the distribution of hours worked in 2017 and 2016 for all 22-year-olds. We find no evidence of a shift in hours worked towards the 1,248 hour cut-off: both distributions almost completely overlap. In Appendix Figure 2.C.13 we show that there is also no shift in hours worked in the second half of 2017.

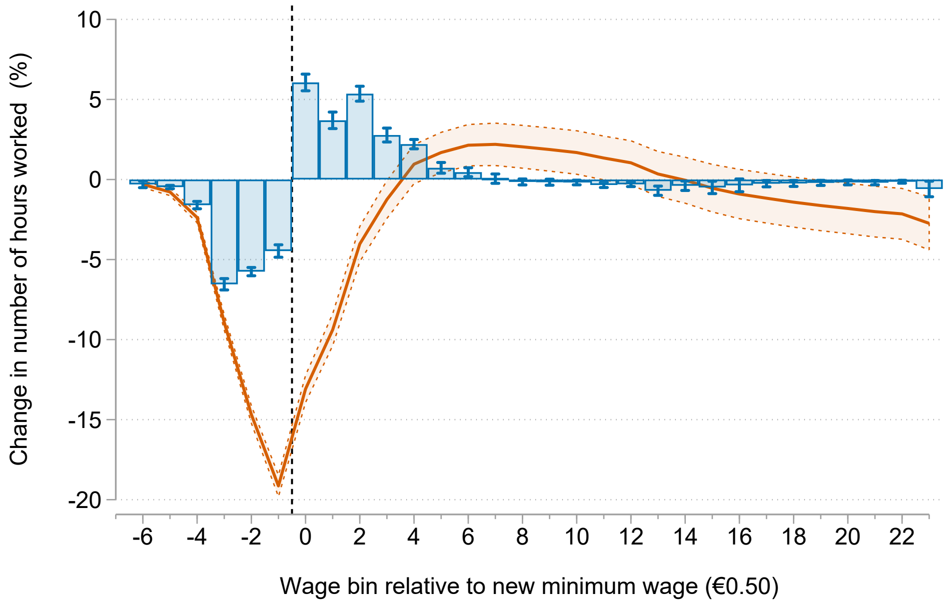
In conclusion, we do not uncover an impact of the LIV subsidy on the aggregate employment effects that we find. The share of workers affected by the subsidy is small and there is no apparent strategic behavior by firms. However, we cannot rule out that some firms retain workers affected by the minimum wage increase in order to benefit from the subsidy.

[39] Among 22-year-olds with wages initially below the new minimum wage, 23% meet the hours worked requirement of the LIV and 7.5% meet both LIV requirements.

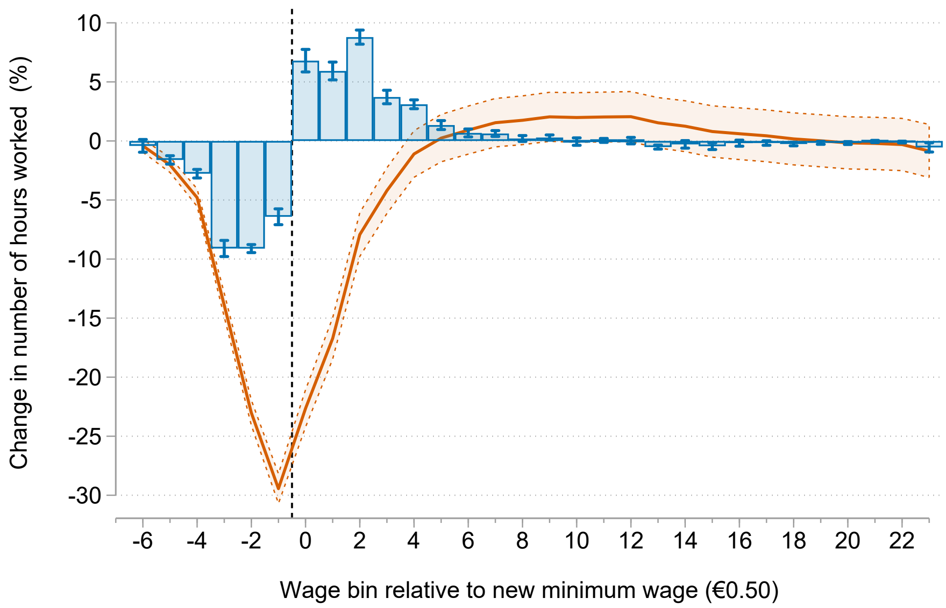
[40] Note that for 22-year-old workers initially earning below the new minimum wage, this strategy only works if they also start earning substantially more than the new minimum wage so that their average wage over the year is at least the adult minimum wage.

Figure 2.6 Robustness check: Removing subsidy-eligible workers

(a) Estimates for 22-year-olds in the 2017 minimum wage reform

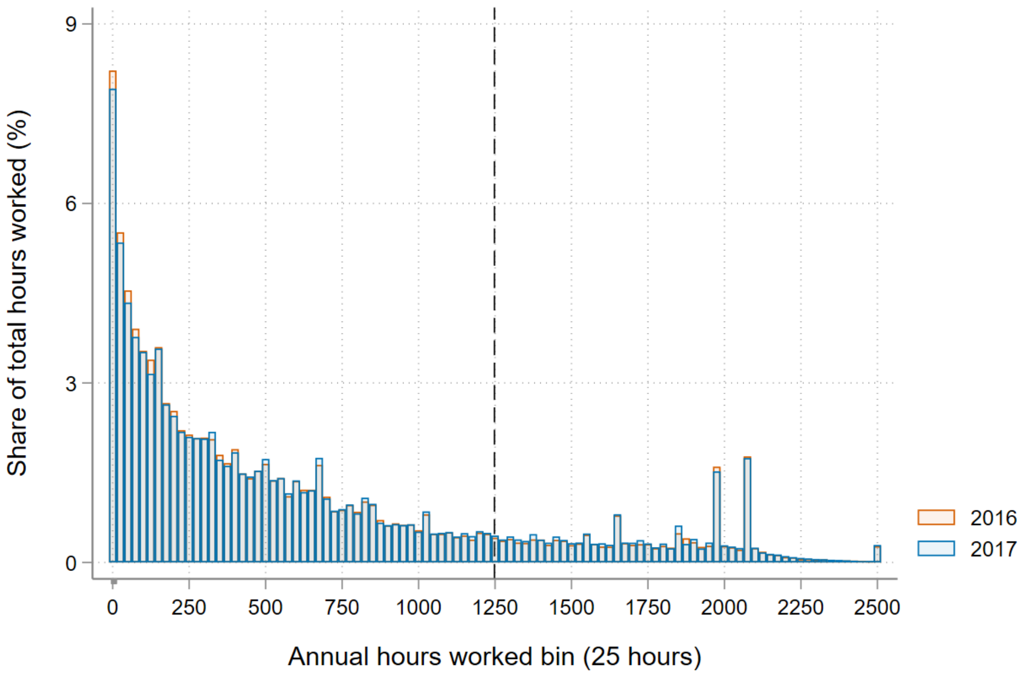


(b) Estimates for 21-year-olds in the 2019 minimum wage reform



Notes: Sample restricted to individuals working fewer than 1,248 hours per year and therefore ineligible for the LIV subsidy. The dashed vertical line is positioned at the new age-specific minimum wage. Blue bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked by age group over the six-months before the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is the 95% confidence interval calculated using the delta method.

Figure 2.7 Robustness check on bunching in distribution of hours worked due to subsidy



Notes: Hours worked by all 22-year-old workers in the main sample in 2016 and 2017. The dashed vertical line is positioned at 1,248 hours, where workers become potentially eligible for the LIV subsidy.

2.6 Effect heterogeneity

Although we find no employment effects of the minimum wage increase, there may be underlying heterogeneity which obscures adverse outcomes for some groups of workers. Of particular concern would be a rising incidence of flexible work arrangements; more adverse effects in specific industries with a high incidence of low-wage employment; and any adverse impacts concentrated among workers who are not transitory occupants of minimum-wage jobs. We study these in turn, followed by a decomposition into effects for incumbent workers, recruits from other firms, and labor market entrants. Table 2.5 presents results for hours worked by 22-year-olds, our baseline group of treated workers.[41]

2.6.1 Heterogeneity by contract type

Even absent negative employment impacts, employment could shift towards more flexible arrangements in response to minimum wage increases (Datta et al., 2020). This is of particular concern in the Netherlands, where flexible work arrangement have become very common over the past twenty years (CBS, 2020).

We therefore study the impact of the minimum wage on employment changes across different contract types. Specifically, we distinguish fixed-term versus permanent contracts; part-time versus full-time contracts; and on-call versus temp agency versus regular contracts. These contract types can overlap in different ways across the three groups: e.g. one worker could be on a fixed-term, part-time, regular contract; and another could be on a permanent, part-time, on-call contract. Using our primary sample, we collapse the number of jobs and total hours worked in each contract type by age, wage bin, and month. We estimate equation (2.4.1) using age-specific employment in a contract type in bin b at time t, relative to the age-specific population, as our outcome variable.

First, we find some evidence of a rise in flexible employment arrangements for workers affected by the minimum wage increase relative to the control group, as shown in the top row of Table 2.5. In particular, hours worked in fixed-term and temp agency work contracts increase. However, hours worked in full-time contracts rise too, and more strongly than in part-time contracts; as do hours worked in regular contracts. This suggests that the small number of additional hours worked up to €4.00 above the new minimum wage are predominantly in temp agency, fixed-term, and full-time contracts. Most importantly, we do not find a corresponding decline in other types of contracts, implying the expansion in a subset of contract types is not coming at the expense of others. Second, the increase in total hours worked in full-time and temp agency work is partially driven by an increase along the extensive margin as well.[42]

[41] Appendix 2.C.2 shows similar results for 21- and 20-year-olds, as well as estimates for the other outcomes including wage elasticities and spillover shares. Appendix 2.C.2 shows that our findings also hold for jobs.

[42] Results for the number of jobs are reported in Appendix 2.C.2.

Table 2.5 Heterogeneity in effects of the 2017 minimum wage increase on hours worked by 22-year olds

	1. Contract duration		2. Contract work hours		3. Contract type		
	Fixed-term	*Permanent*	*Full-time*	*Part-time*	*On-call*	*Temp agency*	*Regular*
Δ Total Hours	0.017***	0.018	0.022**	0.012**	0.009	0.038***	0.015*
	(0.006)	(0.011)	(0.011)	(0.005)	(0.009)	(0.009)	(0.008)
	4. Industry						
	Wholesale	*Retail*	*Food*	*Temp*	*Other*		
Δ Total Hours	0.016	-0.013	0.021*	0.031***	0.020***		
	(0.013)	(0.014)	(0.011)	(0.008)	(0.008)		
	5. Educational enrolment		6. Gender		7. Migration background		
	Student	*Non-student*	*Female*	*Male*	*Migration*	*No migration*	
Δ Total Hours	0.008	0.021***	0.018***	0.016**	0.007	0.019***	
	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	
	8. Worker relationship to firm						
	Incumbent	*Recruit*	*Entrant*				
Δ Total Hours	0.017**	0.015	0.013				
	(0.007)	(0.012)	(0.013)				

Notes: Workers are categorized in exhaustive and mutually exclusive groups for each of the 8 separate analyses. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Estimates are based on around 82 million job spells. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2.6.2 Heterogeneity by industry

We continue exploring effect heterogeneity by zooming in on several industries that together account for the largest share of low-wage employment for treated workers, estimating equation (2.4.1) separately for these industries. These are shown in the second row of Table 2.5: wholesale; retail; food and beverage services; and employment placement, temporary employment provision, and payrolling (labeled 'temp'). We include all other industries as a separate category (labeled 'other').

The minimum wage increase has a heterogeneous impact across industries. Most prominently, and in line with findings for contract types, we find the largest increase in hours worked in temp agency work, followed by food and beverage services. However, importantly, we do not find negative effects on hours worked for any of these most-exposed industries: the negative point estimate for retail is very small and not statistically significant. The minimum wage increase had the largest bite in the food and beverages industry, where average wages increased by 6 to 7%. It had the smallest bite in temp agency work, where average wages increased by around 4%. For each industry we can confidently rule out own-wage elasticities below -1, with the most negative point estimate for retail at -0.264, but with a standard error of 0.284.[43]

2.6.3 Heterogeneity by demographics

We also study the extent of labor-labor substitution within age-groups by looking at different demographic groups.[44] Specifically, we estimate our baseline model separately for students and non-students; females and males; and workers with and without a migration background.[45] Results are shown in the third row of Table 2.5.[46]

We find no evidence of labor-labor substitution in hours worked: none of the groups we study experience a decline in working hours following the minimum wage reform. However, most of the additional hours worked following the reform are worked by non-students, and people without a migration background. The minimum wage had the largest bite for students, whose hourly wages increase by around 4 to 5% on average. We find small increases in hours worked (and in the number of jobs) for non-students, but no corresponding declines for students. Overall, these results suggest little labor-labor substitution, and more favorable employment outcomes for workers who are not enrolled in education and therefore

[43]Details are shown in Appendix 2.C.2.

[44]Recent studies uncover mixed evidence of labor-labor substitution in the US, with Cengiz et al. (2019) finding no such effects, but Giuliano (2013) showing an increase in teenage employment relative to older workers following an overall minimum wage increase and Horton (2018) showing an increase in hours worked of more productive workers.

[45]Students are defined as being enrolled in education. If there is up to a 3 month gap in enrollment, we continue to classify people as students during that gap to account for e.g. summer holidays. An individual with a migration background was either born in a foreign country, or one of their parents was born in a foreign country.

[46]Appendix Table 2.C.11 reports estimates for jobs.

likely less transient occupants of minimum-wage jobs.

2.6.4 Effects for incumbents, recruits, and entrants

Lastly, we explore minimum wage impacts separately for incumbent workers, recruits, and entrants by exploiting the panel dimension of our data. We classify workers as 'incumbent' if they worked in the same firm in the month prior, as 'recruit' if they were employed in a different firm in the month prior, and as 'entrant' if they were not employed in the month prior.[47] By documenting worker flows at the firm level, this analysis is informative about labor-labor substitution as well as mechanisms underlying spillover effects.

The fourth row of Table 2.5 shows that hours worked increase most strongly for incumbent workers, though no declines are detected for recruits and entrants.[48] The lack of negative employment effects is consistent with Cengiz et al. (2019), and also suggests employers are not replacing incumbent workers with differently or higher-skilled new hires.

The existence of spillovers for these different worker groups can be informative about theoretical mechanisms driving such spillovers. Previous papers have not separately distinguished incumbents, recruits, and entrants, but either include recruits with incumbent workers (Cengiz et al., 2019)—finding spillovers are less prevalent among labor market entrants– or include recruits with entrants (Gopalan et al., 2020), finding this combined group has experienced spillovers also. In our case incumbents, recruits, and entrants all have substantial and similarly sized spillover shares, although spillover shares are somewhat larger for incumbents among 21- and 20-year-olds[49]. Larger spillovers for incumbents are consistent with within-firm relative pay concerns and/or increased bargaining power (Dube et al., 2019; Gopalan et al., 2020); but the existence of sizable spillovers for recruits and entrants suggests search frictions, workers' outside options, and reservation wages of nonemployed workers may also play a role (Flinn, 2006).

2.7 Conclusion

We study the employment impacts of a sizable minimum wage increase for young workers, using a difference-in-differences approach combined with detailed administrative data from the Netherlands. To avoid confounding effects of a subsidy for low-wage workers which took effect in 2018 for some of the age-groups affected by the minimum wage increase, we focus our results on 22-year-old workers, whom

[47] The majority of workers in our sample are incumbents: recruits and entrants together make up 7% of hours worked in any month.

[48] Appendix Table 2.C.8 shows negative effects for recruits and entrants among workers aged 20 and 21. These effects are quite substantial, leading to own-wage elasticities smaller than -1 for 20-year-old recruits and entrants, and an own-wage elasticity around -1 for 21-year-old recruits. However, these hours effects are primarily driven by changes at the intensive margin, as shown in Appendix Table 2.C.12.

[49] See Appendix Table 2.C.8)

firms knew were not eligible for the subsidy throughout. We find no evidence that the minimum wage has reduced the number of jobs held or total hours worked by affected workers. This is because the increase in the age-specific minimum wage has resulted in a reallocation of employment around the new minimum wage: the number of jobs and hours worked in wage bins below the new minimum wage is reduced, but this is fully compensated by an increase in the number of jobs and hours worked in wage bins at or slightly above the new minimum wage. While we focus our analysis on 22-year-old workers and the 2017 minimum wage increase, we find similar impacts for younger affected workers and from a second minimum wage increase which was implemented in 2019.

The robust absence of negative effects is in large part due to substantial spillover effects from the minimum wage further up the wage distribution, accounting for almost 70 percent of the total wage increase. These spillover effects are concentrated among low-wage workers, with around 90% of spillovers occurring within €2.50 above the new minimum wage, and found for firms' incumbent workers as well as its new hires.

Our results clearly rule out own-wage elasticities below -1, indicating that the minimum wage increase led to a rise in overall earnings for affected workers. Although we uncover some effect heterogeneity, the average effects do not obscure adverse employment outcomes in some sectors or for some groups of workers. We also do not find evidence of 'offsets' in terms of contract quality: most of the increase in hours worked occurs in full-time jobs and for non-student workers. This suggests that workers who rely on low-wage jobs for a living are more positively impacted by the policy. We do, however, see some evidence of a reallocation of hours worked (though not employment) towards firms' incumbent workers, relative to hours worked by workers who are hired from other firms and by workers hired from non-employment. This implies firms are not replacing their incumbent workers with differently or higher-skilled new hires.

The absence of negative employment effects of the minimum wage is consistent with a low demand elasticity for young low-paid workers, for example because demand for goods and services produced by these workers is inelastic (such that consumers effectively pay for the minimum wage increase); because firms' profit margins decrease; or because minimum wage workers are a small share of firms' total costs even in sectors where they account for a large share of labor costs. It could also be explained by models of imperfect labor market competition, which predict such effects if the minimum wage is not set too high. These models include efficiency wages (e.g. Rebitzer and Taylor, 1995) and models incorporating various sources of monopsony power through frictions (e.g. see Burdett and Mortensen, 1998; Manning, 2003). While we do not distinguish specific mechanisms, our finding that in particular non-student, full-time, incumbent workers are seeing increases in hours worked is consistent with firms focusing on more stable and productive employment relationships following the minimum wage increase, as would be predicted by some variants of these models.

2.A Characteristics of the Dutch minimum wage reform

The legal foundation of the Dutch minimum wage system is grounded in the 'Wet minimumloon en minimumvakantiebijslag' – henceforth WML. On October 14, 2016, a proposal to amend this law was published (see Tweede Kamer der Staten-Generaal 2016b). The proposed amendments to the WML were signed into law on the 25th of January 2017 (Staatsblad van het Koninkrijk der Nederlanden, 2017b).

The major proposed amendment to the WML was to lower the eligibility age for the adult minimum wage, as laid out in article 7 of the WML, from 23 to 22 (see article 1B of the proposal) and to 21, two years later (see Article X of the proposal).

The accompanying explanatory memorandum to the proposal (see Tweede Kamer der Staten-Generaal 2016a) further outlined the intended adjustment of the age-specific minimum wage gradients for workers aged 18–20 (see Section 2.2.2 and Table 2 in Section 2.3.1 of the memorandum). The proposed amendments to the WML would effectively raise the minimum wage for workers aged 18–22 to varying degrees.

The memorandum to the proposal further discussed additional measures to dampen the potential adverse effects of the minimum wage increase on the employment prospects of affected workers (see Section 2.3 of the memorandum). These policies could have confounding effects: we now outline in detail how this impacts our estimates for 22-year-olds (our baseline treatment group), and younger groups, which we do not use at baseline. Throughout, we cite relevant legal documents which were part of the public record at the time.

2.A.1 Policy impacting 22-year-olds

Workers aged 22 (and those aged 21 during the second step of the reform) would be eligible for a low-wage subsidy, the lage-inkomensvoordeel– henceforth LIV. The LIV, which is part of the law called 'Wet tegemoetkomingen loondomein' (or Wtl for short), commenced on 1 January 2017, prior to the minimum wage reform. The LIV subsidy was announced in 2015 and confirmed into law in 2016, about 2 years before the youth minimum wage reform. The LIV subsidy partially compensates employers in the wage costs for employees earning between 100 and 125% of the adult minimum wage. Specifically, it subsidizes 10% of wages for workers earning between 100% and 110% of the minimum wage and 5% of wages for workers earning between 110% and 125% of the minimum wage up to a maximum of 2,000 and 1,000 euros per year respectively. As workers aged 22 would be eligible for the adult minimum wage after July 2017, they could become eligible for the LIV subsidy in 2017.

For a firm to receive the LIV subsidy for a worker, two criteria have to be met: (i) the worker must work at least 1,248 hours in the firm over one calendar year (about 27 hours per week at the typical 47 weeks worked per year for Dutch workers who work all year), and (2) the worker must earn between 100% and 125% of the adult minimum wage on average over the entire calendar year. This means that 22-

year-old workers affected by the reform who earned below the new minimum wage before July 2017 and started earning the new minimum wage after July 2017 would not qualify for the LIV because their average wage over 2017 would be below the adult minimum wage. Due to these stringent requirements, the LIV covered 5.7% of workers aged 22 at the time of the reform, based on their average wage and their total hours worked over 2017.[50] The hours requirement further restricts eligibility: while 36% of 22-year-old workers met the wage requirement of the LIV in 2017, only 15.9% of those additionally met the hours requirement.

The LIV subsidy could confound our baseline estimates of the impact of the minimum wage increase for 22–year-olds over the second half of 2017 in two ways. First, employment estimates could be confounded if employers choose to retain workers or increase working hours in order to meet the hours worked requirement of 1,248 hours over 2017 for those workers earning at least the adult minimum wage over the entire year. Second, there is an incentive to raise wages somewhat more for workers who are close to qualifying for the subsidy, which could mean we overestimate the spillover effects of the minimum wage. Removing the 16.9% of workers who were eligible for the LIV solely based on their hours worked shows that the results remain similar. We also find no evidence of bunching around the annual hours threshold over 2017 and we find no bunching in the second half of the year either. All results are reported in Section 2.5.5.

2.A.2 Policy impacting 18–21-year-olds

The amendment of the WML also raises the minimum wage for workers aged 18–21. Since their respective minimum wages remain below the adult minimum, they are not targeted by the LIV subsidy outlined above. The memorandum to the proposed amendment mentions the intention to introduce an additional subsidy that compensates employers for the increased labor costs associated with the minimum wage increase for workers aged 18–21:

> *'In order to accommodate employers ... the government intends, in consultation with the social partners, to compensate employers for these wage cost increases simultaneously with the increase in the (youth) minimum wage'.* – Tweede Kamer der Staten-Generaal (2016a) p.14 (October 14, 2016)

On November 9, 2016 an amendment to the initial proposal was published, which formalized the additional subsidy (see Tweede Kamer der Staten-Generaal 2016c). This subsidy is called the youth low-wage subsidy (*jeugd lageinkomensvoordeel*, henceforth JLIV). The amendment states that this subsidy will be introduced on January 1, 2018 and would exist as a complement to the LIV subsidy aimed at adult workers.

[50] The LIV covered 7.5% of workers aged 22 at the time of the reform with wages initially below the new minimum wage.

Under the JLIV subsidy, employers receive a fixed amount per hour worked by JLIV-eligible workers aged 18–21 (see Table 1 of Tweede Kamer der Staten-Generaal 2016c). JLIV-eligibility is based on an employee's age on December 31 of the preceding year, and their average wage in the calendar year over which the subsidy is calculated. Employees are JLIV-eligible if their average wage falls within predetermined and age-specific bounds (see Figure 1 of Tweede Kamer der Staten-Generaal 2016c).

The JLIV-subsidy, in line with the other instruments of the Wtl, is applied on a calendar-year basis. This means that employers receive the subsidy over the hours worked by JLIV-eligible workers from 2018 onwards. Since the minimum wage was raised on July 2017, the amendment to the proposal contains a clause stating that employers are compensated for the increase in labor costs over the second half of 2017 through an adjustment of the subsidy amounts for 2018:

> *'If the age of entitlement to the adult minimum wage is decreased from 23 to 22, the minimum youth wages will also be increased at the same time. When the decrease of this age ... takes effect on 1 July 2017, this fact will be taken into account in the amount of the minimum youth wage subsidy per paid employee hours for the 2018 calendar year. Because in this situation the youth wages will already be increased as of 1 July 2017, but the paid hours in 2017 cannot be taken into account for the 2018 calendar year, this will be compensated by a one-off increase in the amounts for the paid hours in calendar year 2018 when determining the minimum youth wage benefit for this calendar year.'* – Tweede Kamer der Staten-Generaal (2016c) p.14

The one-off increase in the JLIV subsidy over 2018 ended up being a 50% increase in the predetermined subsidy amount over that year.

Note that the JLIV only covers younger workers (aged 18–21) and not the 22–year-olds we use for identification at baseline. For younger workers, the subsidy weakens the link between labor costs and the minimum wage increase, and also could potentially lead to wage spillovers for these age-groups because the subsidy covers workers earning up to 117% of the minimum wage for 21-year-olds and up to 135% for 20-year-olds. This is the reason we focus on 22-year-old workers at baseline.

The amendment of the WML, including the details of the JLIV-subsidy, was published on January 8, 2017, and formally accepted on January 25 of that same year.[51] In this amendment, the minimum wage was set to increase on 1 July 2017 and the JLIV subsidy was set to come into effect on 1 January 2018. These dates were confirmed into law on 12 April 2017 (see Staatsblad van het Koninkrijk der Nederlanden 2017a).

[51] Staatsblad van het Koninkrijk der Nederlanden (2017b)

2.B Exploiting the age discontinuity

Some previous papers have leveraged age discontinuities to estimate the impact of minimum wages on employment levels and flows. Most closely related to our setting is Kabátek (2021), who exploits the Dutch age discontinuities in the youth minimum wage prior to the reform we study.[52] He finds that job separations rise right before the minimum wage increase in the month of a worker's birthday. He also finds increases in hiring in the month of the birthday and the few months after. This shows that there is reshuffling of employment around age cutoffs and the accompanying shifts in minimum wages, and suggests firms might specialize in hiring workers of certain ages with accompanying minimum wages. However, it does not imply aggregate employment of affected workers is necessarily declining.

In this section we provide descriptive evidence showing that these patterns are found both before and after the minimum wage reform we study. Figure 2.B.1 shows the number of job separations for 17- and 19–23-year-olds (replicating Figure 5 in Kabátek 2021) around each birthday before the reform (averaged over 2014–2016) and after the reform (averaged over 2017–2019). The increase in separations right before workers' birthdays is striking, and even more so after the reform. For hires the impact is less clear. While Kabátek (2021) finds increases in hiring after workers' birthdays, we do not observe this. Of course hiring conflates both labor demand and labor supply effects, so it is unclear what exactly is driving these patterns.

Figure 2.B.2 distinguishes separations by age group. We find an increase in separations in particular for workers turning 19 and 20, even more so after the reform than before. This is consistent with Kabátek (2021). In particular, the findings in Kabátek (2021) are driven by workers turning 19 and 20, and to a lesser extent by workers turning 21. In the 2017 and 2019 reforms, 19- and 20-year-olds have also seen increases in the minimum wage, but the gap between them and older workers has actually increased in the reform (see Table 2.1). The gap between 19- and 20-year-olds and 20- and 21-year-olds was 9%-points (relative to the adult minimum wage), which increased to 15%-points for both age groups in the 2017 step of the reform. In the 2019 step of the reform the gap increased again, to 20%-points.[53] The gap between 18- and 19-year-olds has remained relatively stable at about 15%. Since the gaps and the 'birthday effects' have not decreased with the policy change we study, they cannot be the explanation for why we do not find disemployment effects at the aggregate level.

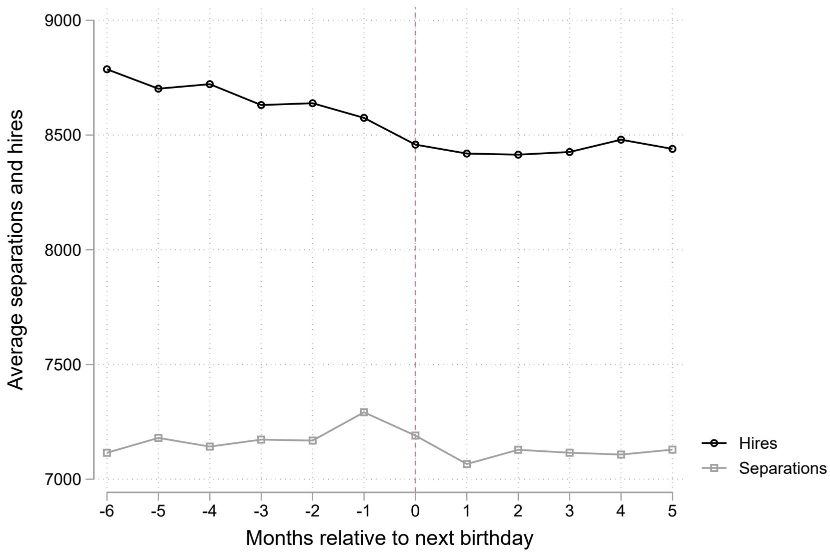
A key difference between these birthday effects and our main results is that we use plausibly exogenous variation in the minimum wage policy. By contrast, birthday effects are estimated in an equilibrium context where firms can exactly anticipate when labor costs will increase and for whom, making identification of causal impacts of minimum wage changes more difficult. Lastly, we study workers

[52]Kreiner et al. (2020) exploit a 40% discontinuity at age 18, finding substantial negative employment effects; whereas Dickens et al. (2014) exploit a 16-20% discontinuity at age 22 in the UK, finding a positive impact on the employment rate of low-skilled workers at age 22.

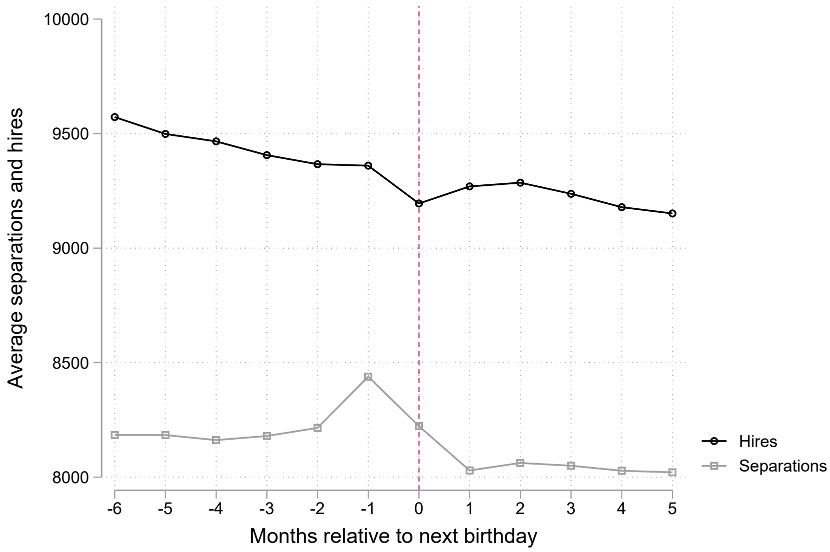
[53]In percentages, the gap between 19- and 20-year-olds (20- and 21-year-olds) was 27% (21%) in 2017 and 33% (25%) in 2019.

Figure 2.B.1 Hires and separations around birthdays for 17- and 19–23-year olds, replicating Figure 5 in Kabátek (2021) for 2014–2016 and 2017–2019.

(a) 2014–2016



(b) 2017–2019

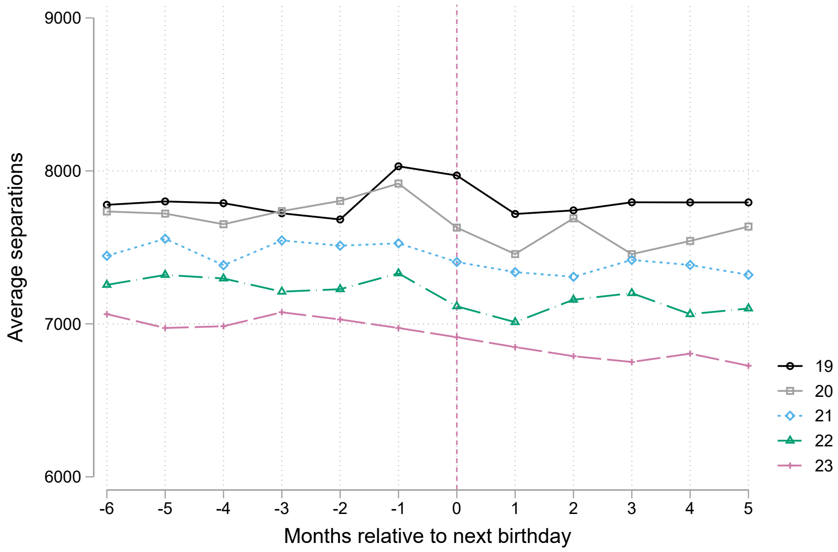


Notes: Figure plots average counts of hires and separations by month to birthday month for workers aged 17 and 19–23 for 2014–2016 and 2017–2019. Hires are defined as workers hired for a main job by a firm where they did not previously work during this or the previous calendar year. Separations are defined analogously as workers separating from their main job and not returning to the firm this or next calendar year.

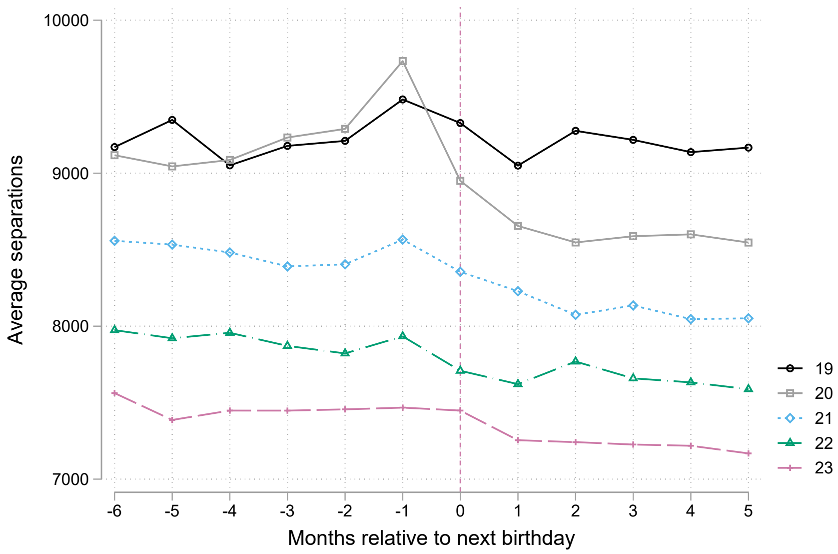
aged 20–22 where birthday effects are negligible.

Figure 2.B.2 Separations by age around birthdays for 2014–2016 and 2017–2019.

(a) 2014–2016



(b) 2017–2019



Notes: Figure plots average counts of separations by month to birthday month for workers by age for 2014–2016 and 2017–2019. Separations are defined as workers separating from their main job and not returning to the firm this or next calendar year.

2.C Additional results and robustness checks

2.C.1 Additional results

Table 2.C.1 Employment impacts and elasticities of the minimum wage on hours worked with varying cutoff points of the wage distribution ($\overline{W}$) and using different control groups

	Up to MW + Wage Bin						
	+3 euros		+4 euros		+5 euros		∞
	Control group ages						
	23–25	26–27	23–25	26–27	23–25	26–27	23–25
A. 22-year-olds							
Δ Total Hours	0.011*	0.015**	0.017**	0.028***	0.017**	0.033***	0.003
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
Elasticity wrt real MW	0.059*	0.086**	0.096**	0.155***	0.097**	0.185***	0.019
	(0.035)	(0.039)	(0.038)	(0.044)	(0.038)	(0.048)	(0.044)
Own-Wage Elasticity (OWE)	0.355*	0.479**	0.567***	0.820***	0.602**	0.983***	0.222
	(0.207)	(0.208)	(0.216)	(0.216)	(0.235)	(0.230)	(0.510)
Wage Elasticity wrt MW	0.250***	0.268***	0.217***	0.243***	0.187***	0.220***	0.086***
	(0.008)	(0.010)	(0.008)	(0.011)	(0.008)	(0.011)	(0.014)
Δ Average Wage	0.044***	0.048***	0.039***	0.043***	0.033***	0.039***	0.015***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Spillover Share	0.662***	0.671***	0.681***	0.701***	0.674***	0.710***	0.472***
	(0.012)	(0.014)	(0.012)	(0.014)	(0.013)	(0.015)	(0.081)
B. 21-year-olds							
Δ Total Hours	0.009	0.015*	0.017**	0.030***	0.017**	0.035***	0.006
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.011)
Elasticity wrt real MW	0.053	0.085*	0.099**	0.170***	0.096**	0.202***	0.032
	(0.044)	(0.046)	(0.046)	(0.051)	(0.047)	(0.054)	(0.056)
Own-Wage Elasticity (OWE)	0.275	0.406**	0.503**	0.758***	0.520**	0.911***	0.308
	(0.224)	(0.211)	(0.228)	(0.211)	(0.253)	(0.225)	(0.531)
Wage Elasticity wrt MW	0.284***	0.309***	0.253***	0.288***	0.215***	0.259***	0.103***
	(0.011)	(0.013)	(0.010)	(0.013)	(0.010)	(0.014)	(0.020)
Δ Average Wage	0.049***	0.054***	0.044***	0.050***	0.037***	0.045***	0.018***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
Spillover Share	0.735***	0.738***	0.753***	0.768***	0.746***	0.774***	0.613***
	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)	(0.013)	(0.074)
C. 20-year-olds							
Δ Total Hours	0.009***	0.016*	0.020**	0.035***	0.018*	0.039***	0.002
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)	(0.011)
Elasticity wrt real MW	0.068	0.114*	0.146**	0.249***	0.128*	0.282***	0.014
	(0.063)	(0.065)	(0.065)	(0.072)	(0.067)	(0.076)	(0.082)
Own-Wage Elasticity (OWE)	0.319	0.476*	0.639**	0.914***	0.621*	1.051***	0.170
	(0.290)	(0.258)	(0.278)	(0.239)	(0.320)	(0.253)	(0.980)
Wage Elasticity wrt MW	0.305***	0.344***	0.289***	0.345***	0.241***	0.314***	0.081**
	(0.017)	(0.020)	(0.015)	(0.020)	(0.016)	(0.020)	(0.036)
Δ Average Wage	0.043***	0.048***	0.040***	0.048***	0.034***	0.044***	0.011**
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.005)
Spillover Share	0.742***	0.745***	0.772***	0.787***	0.757***	0.792***	0.476**
	(0.017)	(0.017)	(0.014)	(0.015)	(0.018)	(0.015)	(0.230)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on different $\overline{W}$ threshold levels based on the distance to the new minimum wage, as well as different control groups. Our baseline models are reported in column 3, using $\overline{W} = 4$ and a control group of 23–25-year-olds. All estimates are obtained using January–June 2017 as reference period. Elasticities with respect to the minimum wage are based on increases in the real minimum wage of 18.7%, 18.3%, and 14.9% for 22, 21, and 20-year-olds, respectively, minus the increase in the real minimum wage for adults of 0.9%. For each $\overline{W}$, estimates using 23–25-year-olds as the control group are based on around 82 million monthly job spells collapsed to 15,840 age × wage-bin × month cells. Estimates using 26–27-year-olds as the control group are based on around 62 million monthly job spell collapsed to 11,880 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6 in each panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.2 Employment impacts and elasticities of the minimum wage on total jobs with varying cutoff points of the wage distribution ($\overline{W}$) and using different control groups

	Up to MW + Wage Bin						
	+3 euros		+4 euros		+5 euros		∞
	Control group ages						
	23–25	26–27	23–25	26–27	23–25	26–27	23–25
A. 22-year-olds							
Δ Total Jobs	0.002	0.001	0.008	0.011*	0.009*	0.015**	0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
Elasticity wrt real MW	0.009	0.006	0.046	0.060*	0.053*	0.083**	0.015
	(0.028)	(0.029)	(0.029)	(0.031)	(0.030)	(0.033)	(0.032)
Own-Wage Elasticity (OWE)	0.043	0.028	0.226	0.280**	0.269*	0.389***	0.110
	(0.140)	(0.140)	(0.143)	(0.144)	(0.149)	(0.150)	(0.234)
Wage Elasticity wrt MW	0.282***	0.289***	0.254***	0.267***	0.228***	0.246***	0.139***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.009)
Δ Average Wage	0.050***	0.052***	0.045***	0.047***	0.041***	0.044***	0.025***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Spillover Share	0.655***	0.660***	0.675***	0.687***	0.677***	0.697***	0.612***
	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)	(0.010)	(0.023)
B. 21-year-olds							
Δ Total Jobs	-0.002	-0.002	0.006	0.008	0.007	0.013**	0.004
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Elasticity wrt real MW	-0.011	-0.014	0.032	0.047	0.041	0.072**	0.022
	(0.031)	(0.031)	(0.032)	(0.033)	(0.033)	(0.035)	(0.037)
Own-Wage Elasticity (OWE)	-0.050	-0.062	0.142	0.196	0.186	0.304**	0.137
	(0.143)	(0.137)	(0.143)	(0.138)	(0.150)	(0.144)	(0.230)
Wage Elasticity wrt MW	0.307***	0.317***	0.282***	0.298***	0.253***	0.275***	0.163***
	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.012)
Δ Average Wage	0.053***	0.055***	0.049***	0.052***	0.044***	0.048***	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Spillover Share	0.737***	0.740***	0.755***	0.764***	0.758***	0.773***	0.728***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)	(0.019)
C. 20-year-olds							
Δ Total Jobs	-0.001	-0.001	0.007	0.010*	0.008	0.014**	0.006
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Elasticity wrt real MW	-0.005	-0.009	0.050	0.069*	0.060	0.101**	0.042
	(0.038)	(0.039)	(0.040)	(0.042)	(0.041)	(0.044)	(0.048)
Own-Wage Elasticity (OWE)	-0.022	-0.036	0.203	0.261*	0.248	0.372**	0.232
	(0.162)	(0.157)	(0.159)	(0.154)	(0.167)	(0.158)	(0.262)
Wage Elasticity wrt MW	0.330***	0.344***	0.308***	0.331***	0.282***	0.314***	0.179***
	(0.010)	(0.011)	(0.010)	(0.012)	(0.010)	(0.013)	(0.019)
Δ Average Wage	0.046***	0.048***	0.043***	0.046***	0.039***	0.044***	0.025***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)
Spillover Share	0.756***	0.759***	0.777***	0.787***	0.780***	0.797***	0.751***
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.025)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on different $\overline{W}$ threshold levels based on the distance to the new minimum wage, as well as different control groups. Our baseline models are reported in column 3, using $\overline{W} = 4$ and a control group of 23–25-year-olds. All estimates are obtained using January–June 2017 as reference period. Elasticities with respect to the minimum wage are based on increases in the real minimum wage of 18.7%, 18.3%, and 14.9% for 22, 21, and 20-year-olds, respectively, minus the increase in the real minimum wage for adults of 0.9%. For each $\overline{W}$, estimates using 23–25-year-olds as the control group are based on around 82 million monthly job spells collapsed to 15,840 age × wage-bin × month cells. Estimates using 26–27-year-olds as the control group are based on around 62 million monthly job spell collapsed to 11,880 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6 in each panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Results for 21- and 20-year olds

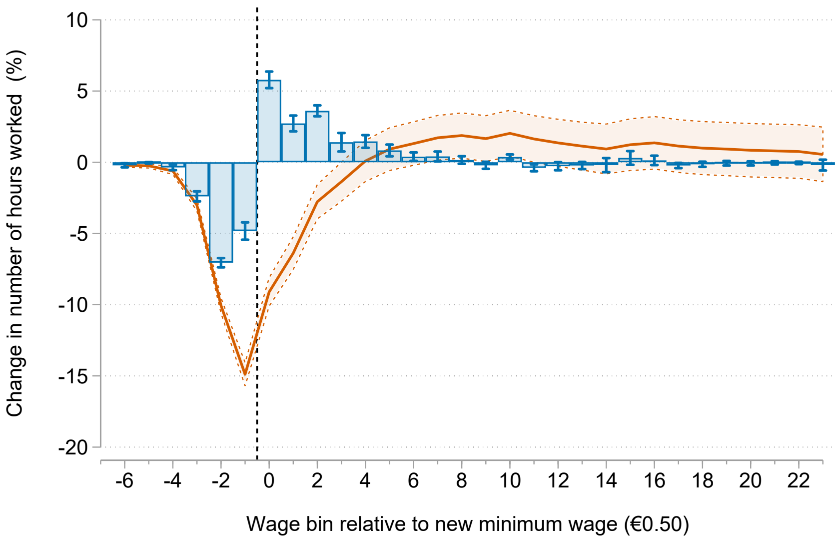
Table 2.C.3 Effects of the 2017 minimum wage increase on employment, average wages, wage elasticities and spillover shares for 21- and 20-year-olds

Employment outcome:	Hours		Jobs	
Worker age:	21	20	21	20
Δ Total Employment	0.017**	0.020**	0.006	0.007
	(0.008)	(0.009)	(0.006)	(0.006)
Employment Elasticity wrt MW	0.099**	0.146**	0.032	0.050
	(0.046)	(0.065)	(0.032)	(0.040)
Own-Wage Elasticity (OWE)	0.503**	0.639**	0.142	0.203
	(0.228)	(0.278)	(0.143)	(0.159)
Wage Elasticity wrt MW (MWE)	0.253***	0.289***	0.282***	0.308***
	(0.010)	(0.015)	(0.007)	(0.010)
Δ Average Wage	0.044***	0.040***	0.049***	0.043***
	(0.002)	(0.002)	(0.001)	(0.001)
Spillover Share	0.753***	0.772***	0.755***	0.777***
	(0.011)	(0.014)	(0.007)	(0.008)

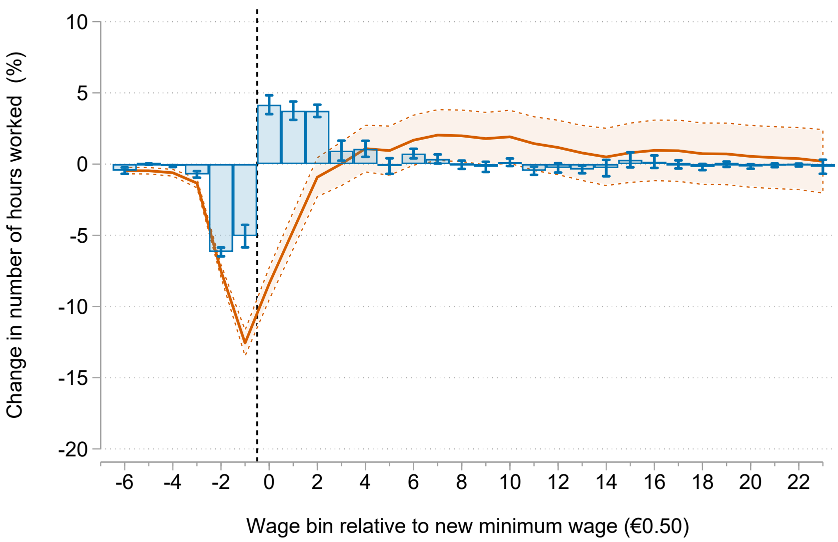
Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Elasticities with respect to the minimum wage are based on increases in the minimum wage of 18.3%, and 14.9% for 21, and 20-year-olds, minus the increase in the minimum wage for adults of 0.9%. All estimates are based on around 82 million monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 2.C.1 Impact of the 2017 minimum wage increase on the number of hours by wage bin (blue bars) and the running sum (orange line)

(a) 21-year-olds



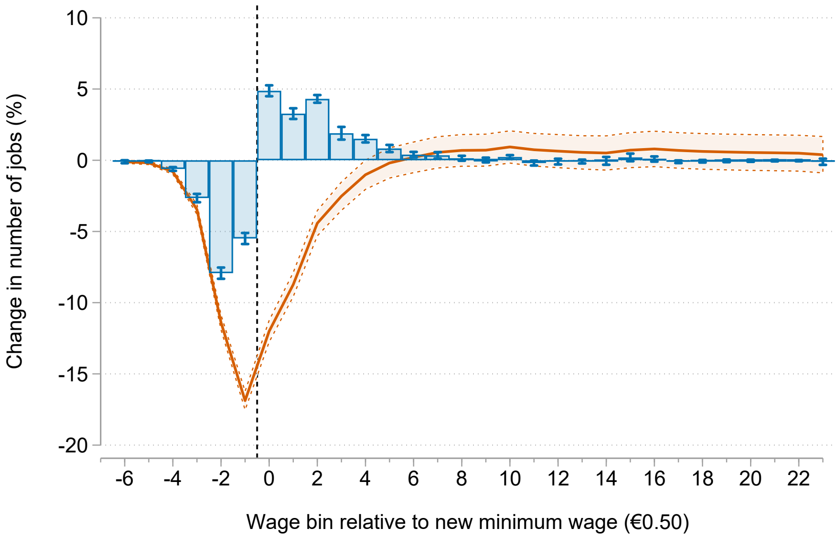
(b) 20-year-olds



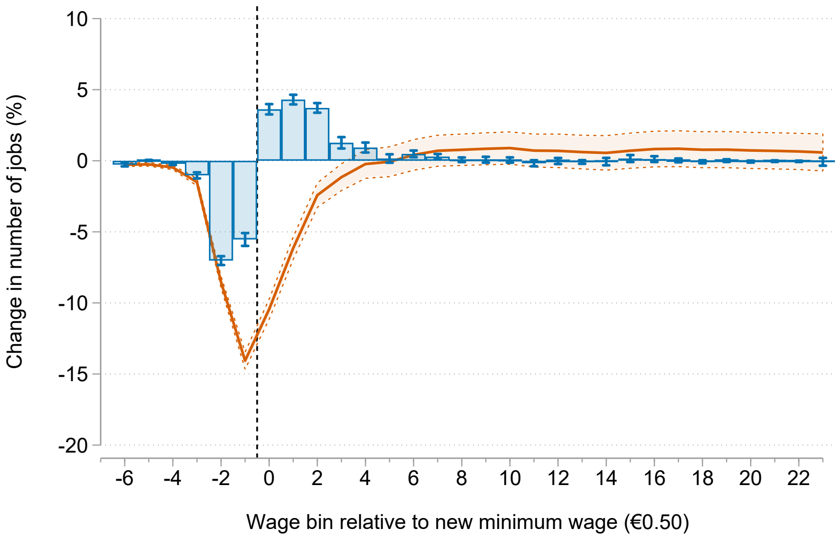
Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show, by age group, the average estimated change in the number of jobs in each €0.50 wage-bin in the six month post-reform period relative to the number of jobs in the six months prior to the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

Figure 2.C.2 Impact of the 2017 minimum wage increase on the number of jobs by wage bin (blue bars) and the running sum (orange line)

(a) 21-year-olds



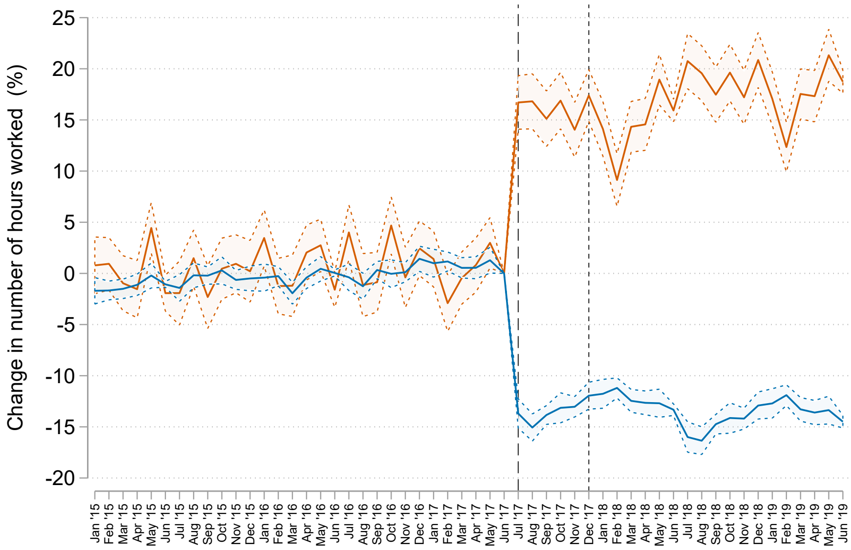
(b) 20-year-olds



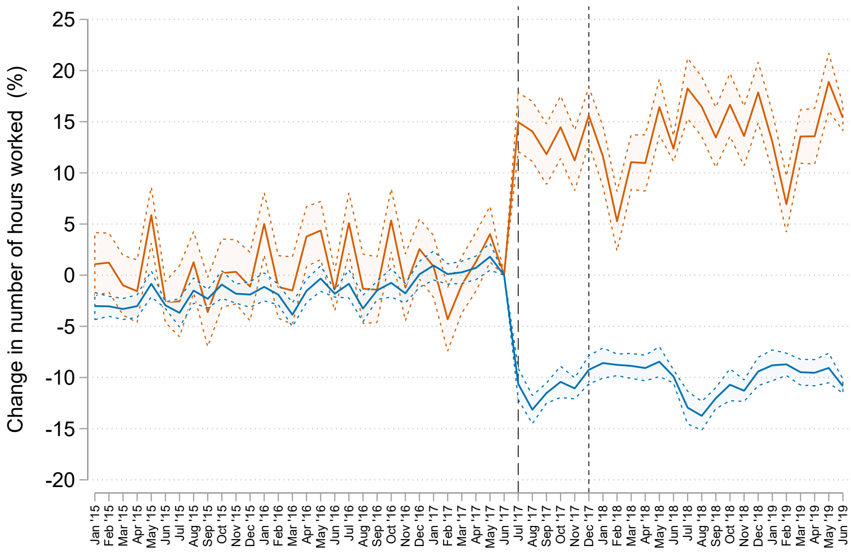
Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show, by age group, the average estimated change in the number of jobs in each €0.50 wage-bin in the six month post-reform period relative to the number of jobs in the six months prior to the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

Figure 2.C.3 Impact of the 2017 minimum wage increase on number of hours worked above and below the new MW by 21- and 20-year olds from January 2015 to June 2019

(a) 21-year-olds



(b) 20-year-olds

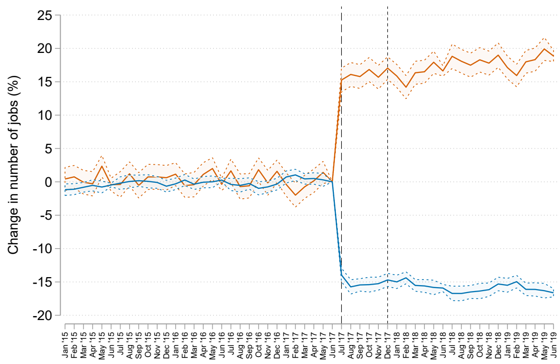


Notes: MW = minimum wage. Figure plots estimates of the monthly change in hours worked up to 4 euros above the new MW in orange and hours worked below the new MW in blue. The omitted month is the one before the first minimum wage increase (June 2017). Shaded areas are 95% confidence intervals. Vertical dashed lines show the six month period used to infer the short-term impact of the minimum wage increase.

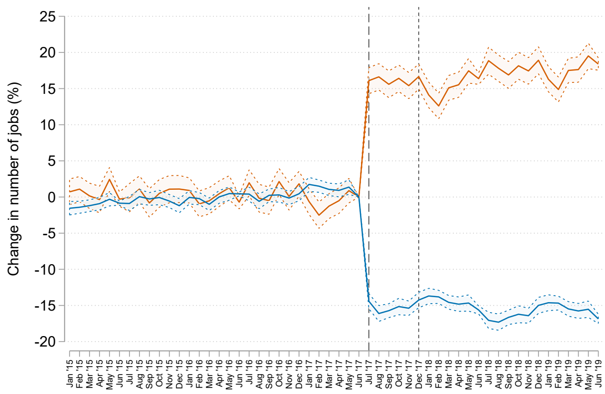
Dynamic impacts on the number of jobs

Figure 2.C.4 Impact of minimum wage increase on number of jobs over time

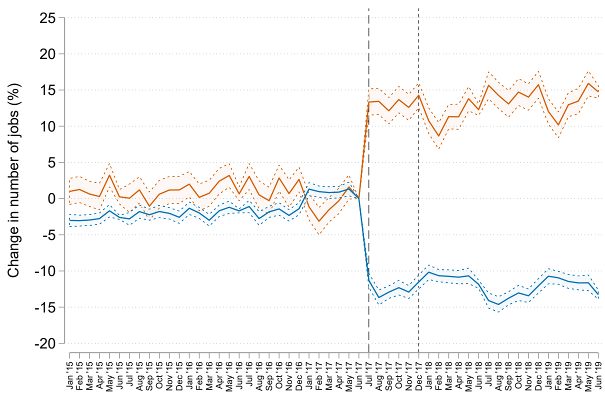
(a) 22-year-olds



(b) 21-year-olds



(c) 20-year-olds



Notes: The graph plots estimates of the change in jobs above the new MW and jobs below the new MW in a month, relative to June 2017, the month before the first minimum wage increase. The shaded areas show the 95% confidence interval. Vertical dashed lines show the six month period used to infer the short-term impact of the minimum wage increase.

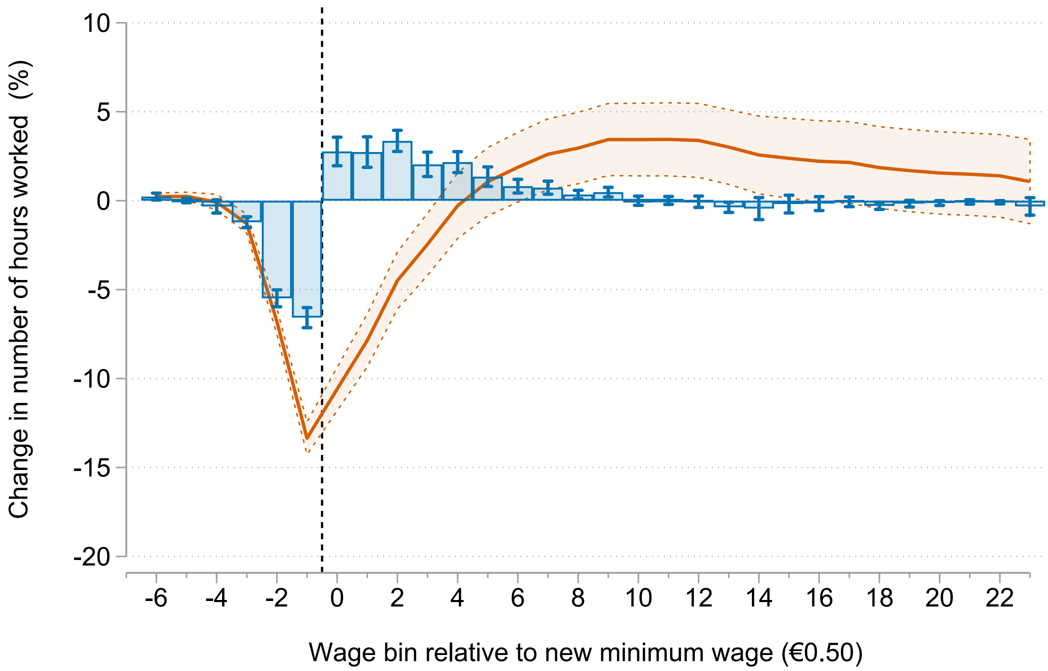
Comparison to the 2019 minimum wage increase

Table 2.C.4 Effects of the 2019 minimum wage increase on employment, average wages, wage elasticities and spillover share for 21-year-olds

	Up to MW + Wage Bin			
	+4 euros		∞	
	Hours	Jobs	Hours	Jobs
Δ Employment	0.027***	0.006	0.010	0.002
	(0.010)	(0.012)	(0.008)	(0.009)
Employment Elasticity wrt MW	0.151***	0.035	0.055	0.012
	(0.057)	(0.065)	(0.047)	(0.05)
Own-Wafe Elasticity (OWE)	0.535***	0.228	0.176	0.052
	(0.204)	(0.427)	(0.151)	(0.221)
Wage Elasticity wrt MW (MWE)	0.333***	0.151***	0.362***	0.228***
	(0.011)	(0.022)	(0.009)	(0.015)
Δ Average Wage	0.060***	0.027***	0.065***	0.041***
	(0.002)	(0.004)	(0.002)	(0.003)
Spillover share	0.702***	0.510*	0.693***	0.635**
	(0.013)	(0.066)	(0.010)	(0.021)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on different $\overline{W}$ threshold levels based on the distance to the new minimum wage in 2019. Estimates are obtained using January–June 2019 as reference period and 23–25-year-olds as control group. Elasticities with respect to the minimum wage are based on an increase in the minimum wage of 19.1% for 21-year-olds, minus the increase in the minimum wage for adults of 1.2%. All estimates are based on around 97 million monthly job spells collapsed to 18,720 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

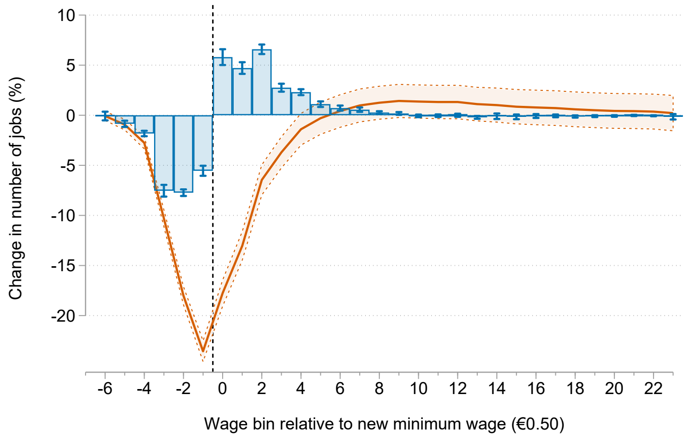
Figure 2.C.5 Impact of the 2019 minimum wage increase on number of hours worked by 20-year-olds by wage bin



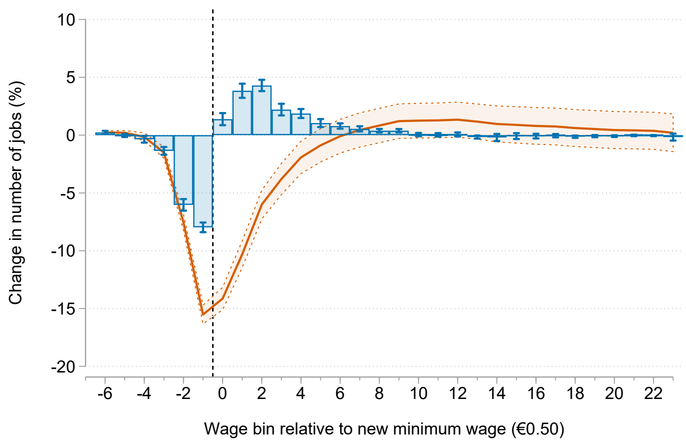
Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show the average estimated change in total hours worked in each €0.50 wage-bin in the six month post-reform period relative to the total hours worked in the six months prior to the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

Figure 2.C.6 Impact of 2019 minimum wage increase on number of jobs by wage bin

(a) 21-year-olds



(b) 20-year-olds



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. The bars show the average estimated change in total hours worked in each €0.50 wage-bin in the six month post-reform period relative to the total hours worked in the six months prior to the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is a 95% confidence interval calculated using the delta method.

2.C.2 Heterogeneity analyses

Heterogeneity analysis for hours worked

Table 2.C.5 Effects on hours worked by contract duration, work hours, and type

	Workers aged 22	Workers aged 21	Workers aged 20
A. Contract duration			
Fixed-term contract	0.017***	0.017***	0.024***
	(0.006)	(0.007)	(0.008)
Permanent contract	0.018	0.017	0.011
	(0.011)	(0.014)	(0.018)
B. Contract work hours			
Full-time	0.022**	0.033**	0.039**
	(0.011)	(0.014)	(0.0181)
Part-time	0.012**	0.005	0.010
	(0.005)	(0.006)	(0.007)
C. Contract type			
On-call	0.009	0.012	0.017*
	(0.009)	(0.009)	(0.010)
Temp agency work	0.038***	0.036***	0.050***
	(0.009)	(0.010)	(0.012)
Regular	0.015*	0.015	0.016
	(0.008)	(0.010)	(0.012)

Notes: Each panel contains exhaustive and mutually exclusive contract types (i.e. fixed-term vs. permanent; full-time vs. part-time; on-call vs. temp agency vs. regular). Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Estimates are based on around 82 million monthly job spells, collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.6 Effect heterogeneity across industries

	Wholesale	Retail	Food	Temp	Other
A. 22-year-olds					
Δ Total Hours	0.016	-0.013	0.021*	0.031***	0.020***
	(0.013)	(0.014)	(0.011)	(0.008)	(0.008)
Minimum-Wage Elasticity (MWE)	0.093	-0.074	0.120*	0.173***	0.114***
	(0.072)	(0.080)	(0.062)	(0.046)	(0.043)
Own-Wage Elasticity (OWE)	0.459	-0.264	0.304*	0.938***	1.096***
	(0.347)	(0.284)	(0.156)	(0.239)	(0.375)
Wage elasticity wrt MW	0.234***	0.324***	0.402***	0.227***	0.146***
	(0.014)	(0.012)	(0.008)	(0.009)	(0.011)
Δ Average Wage	0.042***	0.058***	0.072***	0.040***	0.026***
	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
Spillover Share	0.644***	0.592***	0.724***	0.647***	0.734***
	(0.022)	(0.014)	(0.006)	(0.014)	(0.022)
B. 21-year-olds					
Δ Total Hours	0.003	0.003	0.003	0.020**	0.025**
	(0.016)	(0.015)	(0.012)	(0.009)	(0.01)
Minimum-Wage Elasticity (MWE)	0.018	0.017	0.015	0.118**	0.144**
	(0.09)	(0.088)	(0.071)	(0.054)	(0.056)
Own-Wage Elasticity (OWE)	0.074	0.055	0.039	0.656**	1.140**
	(0.36)	(0.289)	(0.184)	(0.296)	(0.396)
Wage elasticity wrt MW	0.287***	0.334***	0.396***	0.230***	0.184***
	(0.019)	(0.014)	(0.009)	(0.012)	(0.016)
Δ Average Wage	0.050***	0.058***	0.069***	0.040***	0.032***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Spillover Share	0.672***	0.747***	0.818***	0.682***	0.763***
	(0.022)	(0.013)	(0.005)	(0.016)	(0.023)
C. 20-year-olds					
Δ Total Hours	0.040**	-0.002	-0.012	0.026***	0.035***
	(0.019)	(0.016)	(0.012)	(0.01)	(0.012)
Minimum-Wage Elasticity (MWE)	0.287**	-0.018	-0.089	0.187***	0.252***
	(0.135)	(0.111)	(0.087)	(0.072)	(0.087)
Own-Wage Elasticity (OWE)	1.141**	-0.070	-0.207	0.837***	1.523***
	(0.507)	(0.449)	(0.202)	(0.309)	(0.455)
Wage elasticity wrt MW	0.291***	0.269***	0.443***	0.298***	0.240***
	(0.03)	(0.021)	(0.014)	(0.02)	(0.028)
Δ Average Wage	0.041***	0.037***	0.062***	0.042***	0.033***
	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)
Spillover Share	0.751***	0.713***	0.804***	0.723***	0.813***
	(0.028)	(0.023)	(0.007)	(0.019)	(0.033)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Elasticities with respect to the minimum wage are based on increases in the real minimum wage of 18.7%, 18.3%, and 14.9% for 22, 21, and 20-year-olds, respectively, minus the increase in the real minimum wage for adults of 0.9%. Wholesale is wholesale trade, Retail is retail trade, Food is food and beverage service activities (not hotels), Temp is employment placement, temporary employment provision and payrolling. The approximate number of job spells estimates are based on is 3.8 million for wholesale trade; 10.5 million for retail trade; 5.5 million for food and beverages; 12.8 million for employment placement, temporary employment provision and payrolling; and 48 million for other industries. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6 in each panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.7 Effect heterogeneity across demographic groups

	Student	Non-student	Female	Male	Migration background	No migration background
A. 22-year-olds						
Δ Total Hours	0.008	0.021***	0.018***	0.016**	0.007	0.019***
	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Minimum-Wage Elasticity (MWE)	0.044	0.116***	0.101***	0.091**	0.042	0.109***
	(0.034)	(0.044)	(0.038)	(0.041)	(0.039)	(0.039)
Own-Wage Elasticity (OWE)	0.180	0.819***	0.574***	0.560**	0.224	0.663***
	(0.139)	(0.300)	(0.211)	(0.243)	(0.209)	(0.229)
Wage elasticity wrt MW	0.295***	0.186***	0.228***	0.209***	0.228***	0.214***
	(0.007)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)
Δ Average Wage	0.053***	0.033***	0.041***	0.037***	0.041***	0.038***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Spillover Share	0.684***	0.676***	0.670***	0.690***	0.655***	0.687***
	(0.011)	(0.015)	(0.012)	(0.014)	(0.012)	(0.013)
B. 21-year-olds						
Δ Total Hours	0.009	0.022**	0.012	0.021**	0.007	0.020**
	(0.006)	(0.01)	(0.009)	(0.008)	(0.008)	(0.008)
Minimum-Wage Elasticity (MWE)	0.052	0.126**	0.069	0.122**	0.039	0.114**
	(0.037)	(0.06)	(0.051)	(0.047)	(0.048)	(0.048)
Own-Wage Elasticity (OWE)	0.204	0.766**	0.341	0.637***	0.201	0.574**
	(0.146)	(0.354)	(0.247)	(0.236)	(0.25)	(0.235)
Wage elasticity wrt MW	0.302***	0.221***	0.265***	0.243***	0.239***	0.256***
	(0.009)	(0.013)	(0.012)	(0.011)	(0.01)	(0.011)
Δ Average Wage	0.053***	0.038***	0.046***	0.042***	0.042***	0.045***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Spillover Share	0.762***	0.744***	0.753***	0.753***	0.725***	0.760***
	(0.011)	(0.015)	(0.013)	(0.012)	(0.012)	(0.012)
C. 20-year-olds						
Δ Total Hours	-0.008	0.046***	0.017	0.023**	-0.004	0.026***
	(0.006)	(0.015)	(0.011)	(0.009)	(0.009)	(0.01)
Minimum-Wage Elasticity (MWE)	-0.055	0.330***	0.125	0.162**	-0.027	0.188***
	(0.047)	(0.107)	(0.078)	(0.063)	(0.066)	(0.069)
Own-Wage Elasticity (OWE)	-0.229	1.586***	0.564	0.692***	-0.129	0.810***
	(0.198)	(0.507)	(0.342)	(0.261)	(0.319)	(0.285)
Wage elasticity wrt MW	0.287***	0.276***	0.280***	0.295***	0.261***	0.295***
	(0.013)	(0.023)	(0.02)	(0.015)	(0.016)	(0.017)
Δ Average Wage	0.040***	0.039***	0.039***	0.041***	0.036***	0.041***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Spillover Share	0.766***	0.769***	0.770***	0.774***	0.737***	0.780***
	(0.016)	(0.021)	(0.021)	(0.013)	(0.018)	(0.015)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. The approximate number of job spells estimates are based on is 25 million for students; 56 million for non-students; 41 million for females; 40 million for males; 17 million for individuals with a migration background; and 65 million for individuals without a migration background. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.8 Effect heterogeneity: incumbents, recruits, and entrants

	Incumbents	Recruits	Entrants
A. 22-year-olds			
Δ Total Hours	0.017**	0.015	0.013
	(0.007)	(0.012)	(0.013)
Minimum-Wage Elasticity (MWE)	0.097**	0.084	0.071
	(0.038)	(0.07)	(0.071)
Own-Wage Elasticity (OWE)	0.589**	0.392	0.253
	(0.227)	(0.326)	(0.249)
Wage elasticity wrt MW	0.213***	0.261***	0.328***
	(0.008)	(0.016)	(0.019)
Δ Average Wage	0.038***	0.046***	0.058***
	(0.001)	(0.003)	(0.003)
Spillover Share	0.681***	0.673***	0.681***
	(0.012)	(0.024)	(0.025)
B. 21-year-olds			
Δ Total Hours	0.020**	-0.038***	-0.014
	(0.008)	(0.013)	(0.013)
Minimum-Wage Elasticity (MWE)	0.117**	-0.217***	-0.080
	(0.047)	(0.077)	(0.076)
Own-Wage Elasticity (OWE)	0.603**	-1.044***	-0.284
	(0.238)	(0.392)	(0.273)
Wage elasticity wrt MW	0.249***	0.258***	0.336***
	(0.01)	(0.022)	(0.024)
Δ Average Wage	0.043***	0.045***	0.058***
	(0.002)	(0.004)	(0.004)
Spillover Share	0.760***	0.653***	0.675***
	(0.011)	(0.031)	(0.028)
C. 20-year-olds			
Δ Total Hours	0.025***	-0.038**	-0.049***
	(0.009)	(0.016)	(0.014)
Minimum-Wage Elasticity (MWE)	0.180***	-0.270**	-0.348***
	(0.067)	(0.117)	(0.098)
Own-Wage Elasticity (OWE)	0.802***	-1.233**	-1.24***
	(0.291)	(0.573)	(0.401)
Wage elasticity wrt MW	0.285***	0.277***	0.338***
	(0.016)	(0.046)	(0.039)
Δ Average Wage	0.040***	0.039***	0.047***
	(0.002)	(0.006)	(0.005)
Spillover Share	0.780***	0.637***	0.672***
	(0.014)	(0.061)	(0.044)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Heterogeneity analysis for jobs

Table 2.C.9 Effects on employment in jobs by contract duration, work hours, and type

	Workers aged 22	Workers aged 21	Workers aged 20
A. Contract duration			
Fixed-term contract	0.008	0.006	0.009*
	(0.006)	(0.006)	(0.005)
Permanent contract	0.004	0.003	0.006
	(0.011)	(0.009)	(0.008)
B. Contract work hours			
Full-time	0.039**	0.034**	0.023**
	(0.017)	(0.013)	(0.010)
Part-time	0.001	-0.002	0.002
	(0.005)	(0.005)	(0.005)
C. Contract type			
On-call	-0.006	-0.007	-0.005
	(0.009)	(0.009)	(0.009)
Temp agency work	0.040***	0.023**	0.019**
	(0.01)	(0.010)	(0.009)
Regular	0.007	0.007	0.010
	(0.008)	(0.007)	(0.006)

Notes: Each panel contains exhaustive and mutually exclusive contract types (i.e. fixed-term vs. permanent; full-time vs. part-time; on-call vs. temp agency vs. regular). Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Estimates are based on around 82 million monthly job spells, collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.10 Effect heterogeneity across industries

	Wholesale	Retail	Food	Temp	Other
A. 22-year-olds					
Δ Total Jobs	0.001	-0.015	0.005	0.013	0.015***
	(0.010)	(0.010)	(0.011)	(0.008)	(0.006)
Minimum-Wage Elasticity (MWE)	0.005	-0.084	0.029	0.071	0.085***
	(0.055)	(0.058)	(0.062)	(0.045)	(0.033)
Own-Wage Elasticity (OWE)	0.022	-0.310	0.064	0.319	0.688***
	(0.247)	(0.216)	(0.139)	(0.200)	(0.252)
Wage elasticity wrt MW	0.257***	0.321***	0.457***	0.265***	0.170***
	(0.011)	(0.011)	(0.008)	(0.008)	(0.008)
Δ Average Wage	0.046***	0.057***	0.081***	0.047***	0.030***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Spillover Share	0.653***	0.597***	0.725***	0.612***	0.734***
	(0.016)	(0.013)	(0.006)	(0.012)	(0.014)
B. 21-year-olds					
Δ Total Jobs	-0.014	-0.005	-0.018	0.012	0.016**
	(0.011)	(0.011)	(0.011)	(0.009)	(0.007)
Minimum-Wage Elasticity (MWE)	-0.083	-0.026	-0.102	0.068	0.089**
	(0.061)	(0.063)	(0.063)	(0.052)	(0.041)
Own-Wage Elasticity (OWE)	-0.323	-0.092	-0.238	0.313	0.633**
	(0.240)	(0.220)	(0.147)	(0.240)	(0.273)
Wage elasticity wrt MW	0.294***	0.318***	0.443***	0.269***	0.204***
	(0.013)	(0.013)	(0.009)	(0.011)	(0.011)
Δ Average Wage	0.051***	0.055***	0.077***	0.047***	0.036***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Spillover Share	0.702***	0.753***	0.814***	0.666***	0.768***
	(0.014)	(0.012)	(0.005)	(0.012)	(0.015)
C. 20-year-olds					
Δ Total Jobs	0.016	-0.011	-0.022**	0.018**	0.022***
	(0.011)	(0.012)	(0.011)	(0.008)	(0.008)
Minimum-Wage Elasticity (MWE)	0.118	-0.076	-0.156**	0.130**	0.161***
	(0.079)	(0.085)	(0.077)	(0.058)	(0.059)
Own-Wage Elasticity (OWE)	0.472	-0.325	-0.330**	0.518**	0.948***
	(0.309)	(0.374)	(0.161)	(0.225)	(0.318)
Wage elasticity wrt MW	0.288***	0.253***	0.486***	0.321***	0.249***
	(0.018)	(0.020)	(0.012)	(0.018)	(0.019)
Δ Average Wage	0.040***	0.035***	0.068***	0.045***	0.035***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Spillover Share	0.752***	0.712***	0.805***	0.761***	0.809***
	(0.017)	(0.023)	(0.006)	(0.014)	(0.021)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Elasticities with respect to the minimum wage are based on increases in the real minimum wage 18.7%, 18.3%, and 14.9% for 22, 21, and 20-year-olds, respectively, minus the increase in the real minimum wage for adults of 0.9%. Wholesale is wholesale trade, Retail is retail trade, Food is food and beverage service activities (not hotels), Temp is employment placement, temporary employment provision and payrolling. The approximate number of job spells estimates are based on is 3.8 million for wholesale trade; 10.5 million for retail trade; 5.5 million for food and beverages; 12.8 million for employment placement, temporary employment provision and payrolling; and 48 million for other industries. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses; obtained using the delta method for rows 3–6 in each panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.11 Effect heterogeneity across demographic groups

	Student	Non-student	Female	Male	Migration background	No migration background
A. 22-year-olds						
Δ Total Jobs	-0.001	0.015**	0.007	0.009	0.008	0.008
	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.005)
Minimum-Wage Elasticity (MWE)	-0.006	0.086**	0.039	0.053	0.047	0.045
	(0.031)	(0.037)	(0.029)	(0.032)	(0.032)	(0.030)
Own-Wage Elasticity (OWE)	-0.026	0.513**	0.188	0.266	0.211	0.230
	(0.127)	(0.213)	(0.141)	(0.161)	(0.142)	(0.150)
Wage elasticity wrt MW	0.298***	0.216***	0.260***	0.247***	0.270***	0.249***
	(0.005)	(0.008)	(0.006)	(0.007)	(0.006)	(0.006)
Δ Average Wage	0.053***	0.039***	0.046***	0.044***	0.048***	0.044***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Spillover Share	0.685***	0.662***	0.667***	0.685***	0.659***	0.680***
	(0.007)	(0.011)	(0.008)	(0.009)	(0.008)	(0.008)
B. 21-year-olds						
Δ Total Jobs	-0.004	0.016*	0.002	0.010	0.006	0.005
	(0.005)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
Minimum-Wage Elasticity (MWE)	-0.020	0.093*	0.009	0.055	0.035	0.031
	(0.031)	(0.048)	(0.034)	(0.034)	(0.034)	(0.033)
Own-Wage Elasticity (OWE)	-0.081	0.482**	0.040	0.246	0.151	0.139
	(0.125)	(0.244)	(0.150)	(0.152)	(0.147)	(0.149)
Wage elasticity wrt MW	0.305***	0.252***	0.288***	0.276***	0.286***	0.280***
	(0.006)	(0.01)	(0.007)	(0.008)	(0.007)	(0.007)
Δ Average Wage	0.053***	0.044***	0.050***	0.048***	0.050***	0.049***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Spillover Share	0.764***	0.740***	0.759***	0.751***	0.731***	0.762***
	(0.006)	(0.011)	(0.007)	(0.008)	(0.007)	(0.007)
C. 20-year-olds						
Δ Total Jobs	-0.012**	0.041***	0.006	0.008	0.003	0.008
	(0.005)	(0.011)	(0.006)	(0.006)	(0.006)	(0.006)
Minimum-Wage Elasticity (MWE)	-0.088**	0.296***	0.045	0.056	0.025	0.057
	(0.038)	(0.078)	(0.044)	(0.040)	(0.040)	(0.042)
Own-Wage Elasticity (OWE)	-0.358**	1.206***	0.183	0.223	0.099	0.232
	(0.157)	(0.314)	(0.178)	(0.159)	(0.158)	(0.169)
Wage elasticity wrt MW	0.298***	0.317***	0.309***	0.307***	0.312***	0.307***
	(0.009)	(0.016)	(0.011)	(0.010)	(0.009)	(0.011)
Δ Average Wage	0.042***	0.044***	0.043***	0.043***	0.043***	0.043***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Spillover Share	0.781***	0.764***	0.776***	0.779***	0.764***	0.781***
	(0.009)	(0.014)	(0.010)	(0.008)	(0.008)	(0.009)

Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. Estimates for students are based on around 25 million job spells. The approximate number of job spells estimates are based on is 25 million for students; 56 million for non-students; 41 million for females; 40 million for males; 17 million for individuals with a migration background; and 65 million for individuals without a migration background. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.C.12 Employment effects for incumbents, recruits, and entrants

	Incumbents	Recruits	Entrants
A. 22-year-olds			
Δ Total Jobs	0.008	0.011	0.007
	(0.005)	(0.010)	(0.010)
Minimum-Wage Elasticity (MWE)	0.045	0.061	0.042
	(0.03)	(0.056)	(0.055)
Own-Wage Elasticity (OWE)	0.229	0.252	0.144
	(0.151)	(0.234)	(0.188)
Wage elasticity wrt MW	0.247***	0.288***	0.335***
	(0.006)	(0.011)	(0.011)
Δ Average Wage	0.044***	0.051***	0.060***
	(0.001)	(0.002)	(0.002)
Spillover Share	0.677***	0.653***	0.665***
	(0.008)	(0.014)	(0.013)
B. 21-year-olds			
Δ Total Jobs	0.007	-0.012	-0.005
	(0.006)	(0.010)	(0.009)
Minimum-Wage Elasticity (MWE)	0.042	-0.07	-0.028
	(0.033)	(0.06)	(0.055)
Own-Wage Elasticity (OWE)	0.189	-0.281	-0.103
	(0.149)	(0.241)	(0.200)
Wage elasticity wrt MW	0.278***	0.306***	0.325***
	(0.007)	(0.013)	(0.013)
Δ Average Wage	0.048***	0.053***	0.056***
	(0.001)	(0.002)	(0.002)
Spillover Share	0.762***	0.707***	0.702***
	(0.007)	(0.013)	(0.013)
C. 20-year-olds			
Δ Total Jobs	0.010	-0.004	-0.022***
	(0.006)	(0.011)	(0.008)
Minimum-Wage Elasticity (MWE)	0.069*	-0.031	-0.158***
	(0.041)	(0.077)	(0.060)
Own-Wage Elasticity (OWE)	0.283*	-0.118	-0.601**
	(0.166)	(0.290)	(0.235)
Wage elasticity wrt MW	0.305***	0.337***	0.321***
	(0.01)	(0.023)	(0.017)
Δ Average Wage	0.043***	0.047***	0.045***
	(0.001)	(0.003)	(0.002)
Spillover Share	0.782***	0.746***	0.744***
	(0.008)	(0.021)	(0.017)

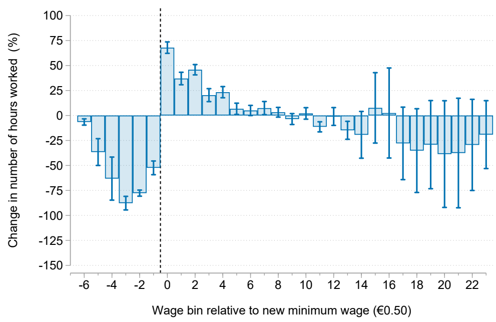
Notes: MW = minimum wage. Table reports estimated effects of the increase in the age-specific minimum wage based on a $\overline{W}$ threshold level of €4.00 above the new minimum wage. Estimates are obtained using January–June 2017 as reference period and 23–25-year-olds as control group. All monthly job spells collapsed to 15,840 age × wage-bin × month cells. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2.C.3 Robustness checks

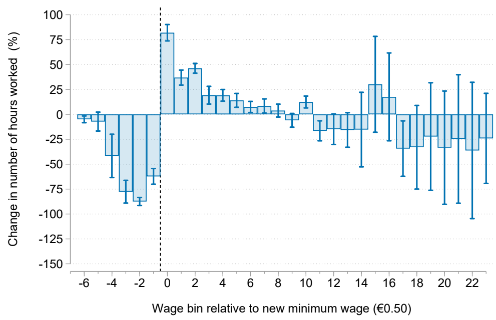
Scaling effects by employment per bin

Figure 2.C.7 Alternative version of Figure 2.2 where we scale by employment per bin instead of total employment.

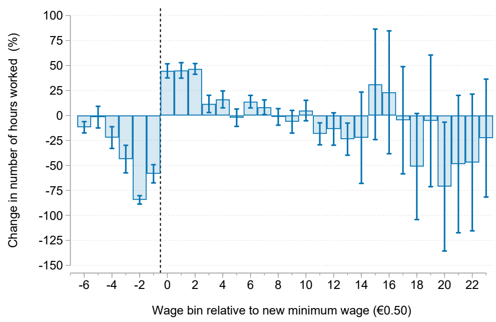
(a) 22-year-olds



(b) 21-year-olds



(c) 20-year-olds

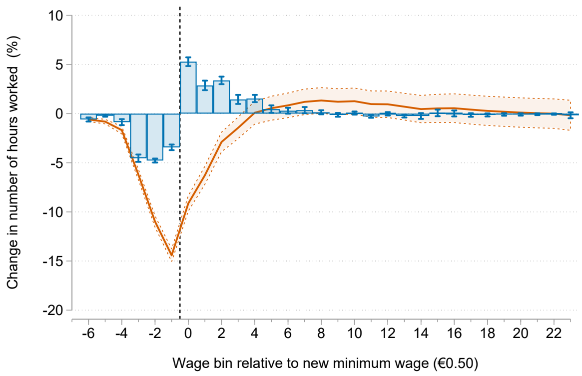


Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked per bin in the six month pre-reform period. Error bars show the 95% confidence interval using robust standard errors. Note that these estimates cannot be added to calculate the total employment effect. The first bins right below the new minimum wage do not decline by 100% because of measurement error and non-compliance.

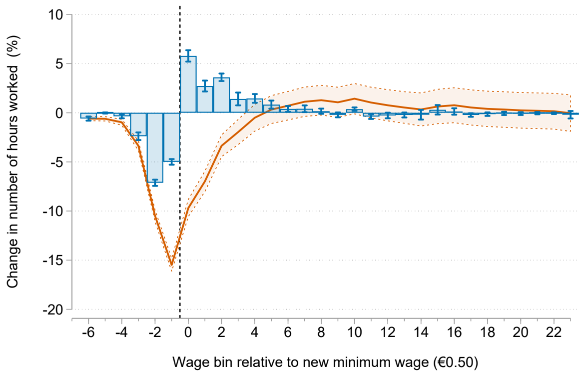
Empty below new minimum wage bins for control group

Figure 2.C.8 Impact of 2017 minimum wage increase on number of hours worked by wage bin

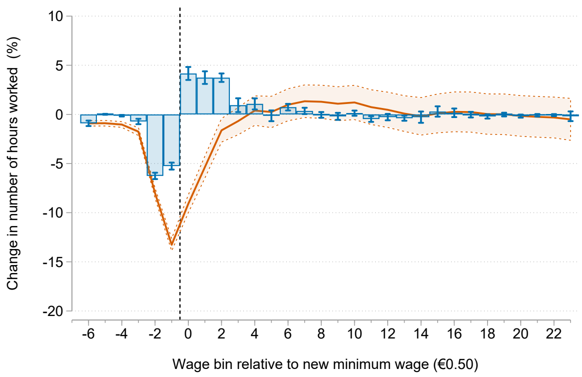
(a) 22-year-olds



(b) 21-year-olds



(c) 20-year-olds

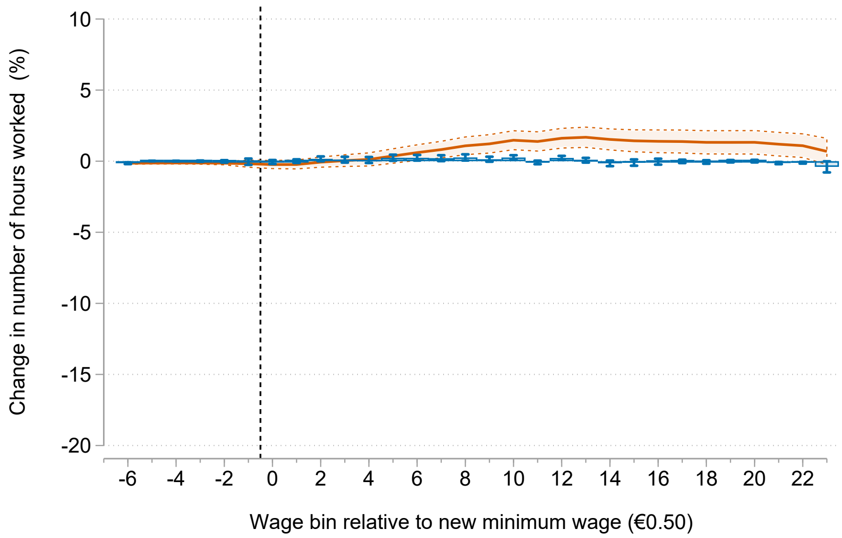


Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Blue bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked by age group over the six-months before the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line is the running sum of estimated changes across wage bins; the shaded area is the 95% confidence interval calculated using the delta method.

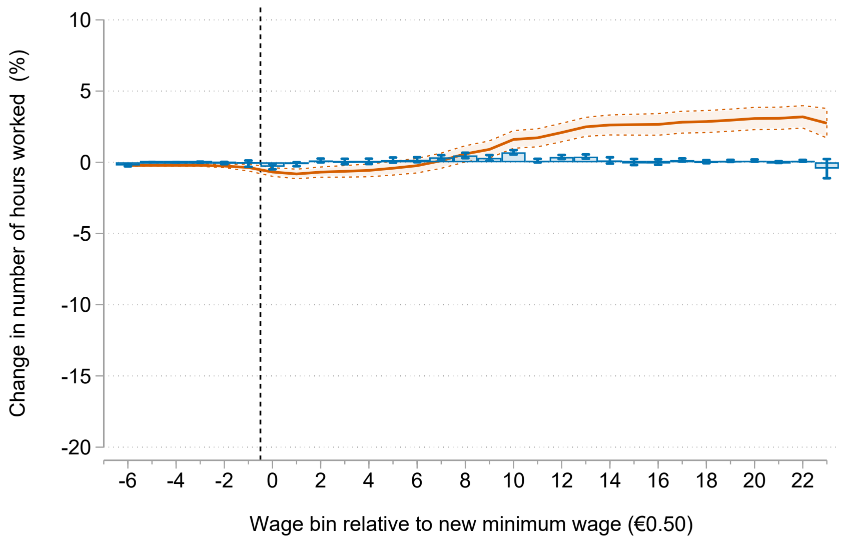
Placebo test using 23–25-year-olds as treated age-groups

Figure 2.C.9 Comparing the change in total hours for the unaffected 23–25-year-olds with two older control groups.

(a) Comparing 23–25-year-olds with 26–27-year-olds



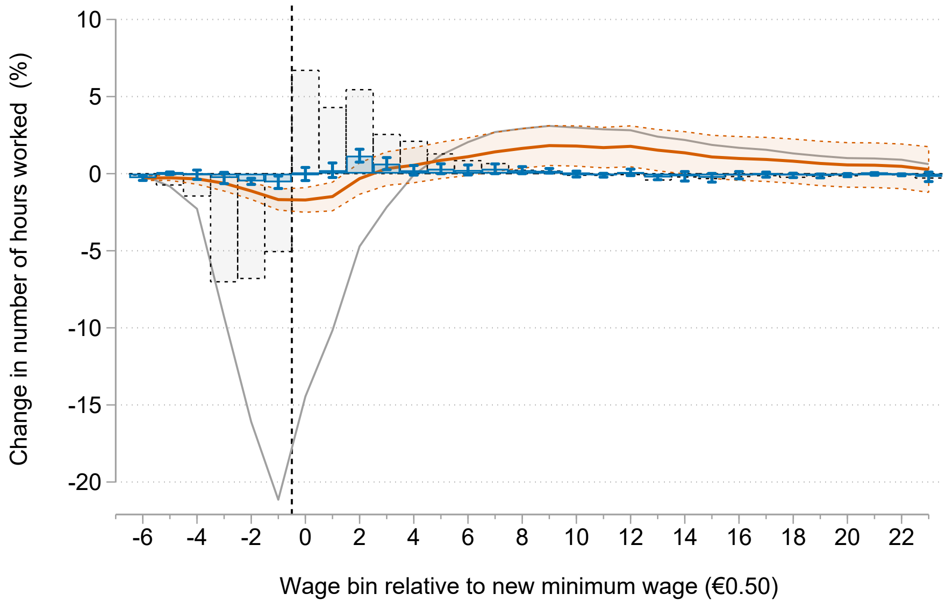
(b) Comparing 23–25-year-olds with 30–35-year-olds



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Blue bars show the estimated change in total hours worked in each €0.50 wage-bin over the same six month windows as our main analyses. Error bars show the 95% confidence interval using robust standard errors. The orange line shows the running sum of estimated changes across wage bins; the shaded area shows the 95% confidence interval calculated using the delta method.

Placebo test using 22-year-olds as treated age-groups in 2019

Figure 2.C.10 Impact of the 2019 minimum wage increase on number of hours worked by 22-year-olds by wage bin



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked by age group over the six-months before the reform. Blue bars show results for 22-year-olds (placebo estimates); gray bars show results for 21-year-olds (treatment estimates) for comparison. The lines show running sums of estimated changes across wage bins, in orange for 22-year-olds and in gray for 21-year-olds. The orange shaded area is the 95% confidence interval of the running sum for 22-year-olds calculated using the delta method.

Table 2.C.13 Placebo tests : Effects on number of hours worked

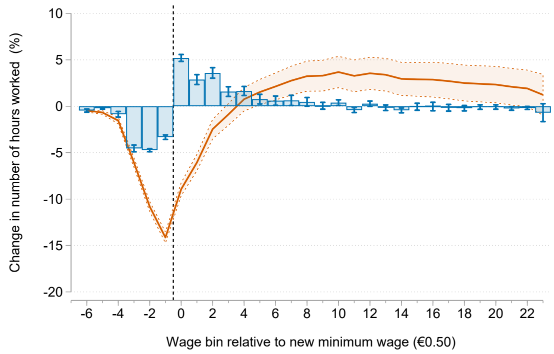
Placebo treated age-group:	22-year-old in 2019		23–25-year-old in 2017			
Control age-group:	23-25		26-27		30-35	
Up to MW + Wage Bin:	+4 euros	∞	+4 euros	∞	+4 euros	∞
Δ Employment	0.014**	0.003	0.005	0.005	0.001	0.024***
	(0.006)	(0.008)	(0.004)	(0.006)	(0.004)	(0.006)

Notes: MW = minimum wage. Table reports estimated placebo effects of the increase in the age-specific minimum wage for age groups that were not affected. Estimates are reported for different $\overline{W}$ threshold levels based on the distance to the new minimum wage. Columns 2-3 report estimates for 22-year-olds for the 2019 step of the reform using January–June 2019 as reference period and 23–25-year-olds as control group. Columns 4-7 report estimates for 23-25-year olds for the 2017 step of the reform using January–June 2017 as reference period and different control groups. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

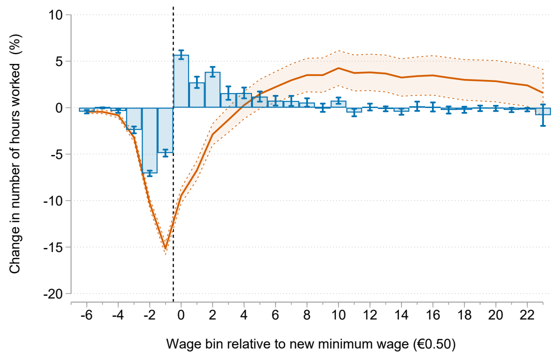
Different control age-groups

Figure 2.C.11 Impact of 2017 minimum wage increase on number of hours worked by wage bin using 26–27-year olds as control group

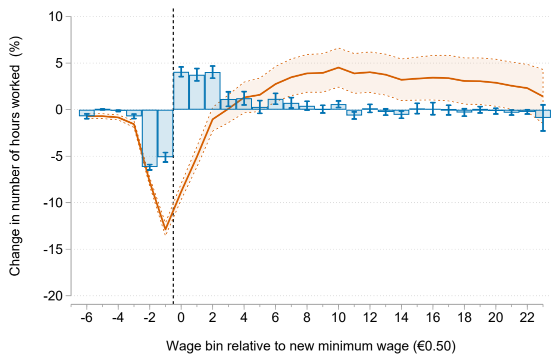
(a) 22-year-olds



(b) 21-year-olds



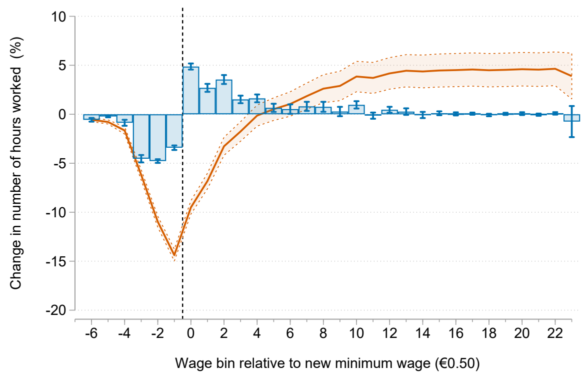
(c) 20-year-olds



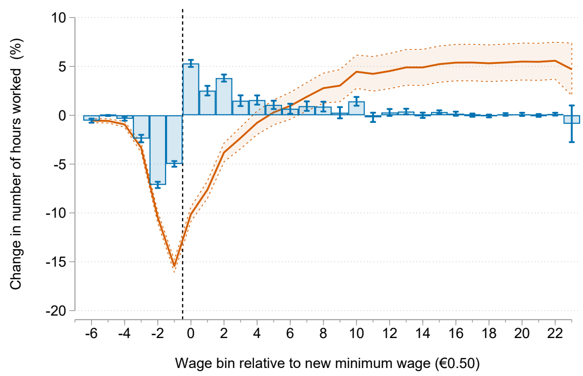
Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Blue bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked by age group over the six-months before the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line shows the running sum of estimated changes across wage bins; the shaded area shows the 95% confidence interval calculated using the delta method.

Figure 2.C.12 Impact of 2017 minimum wage increase on number of hours worked by wage bin using 30–35-year olds as control group

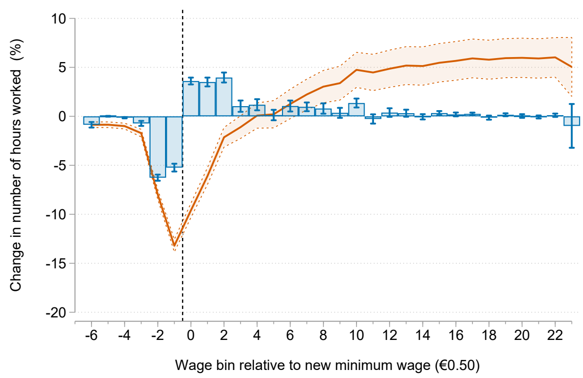
(a) 22-year-olds



(b) 21-year-olds



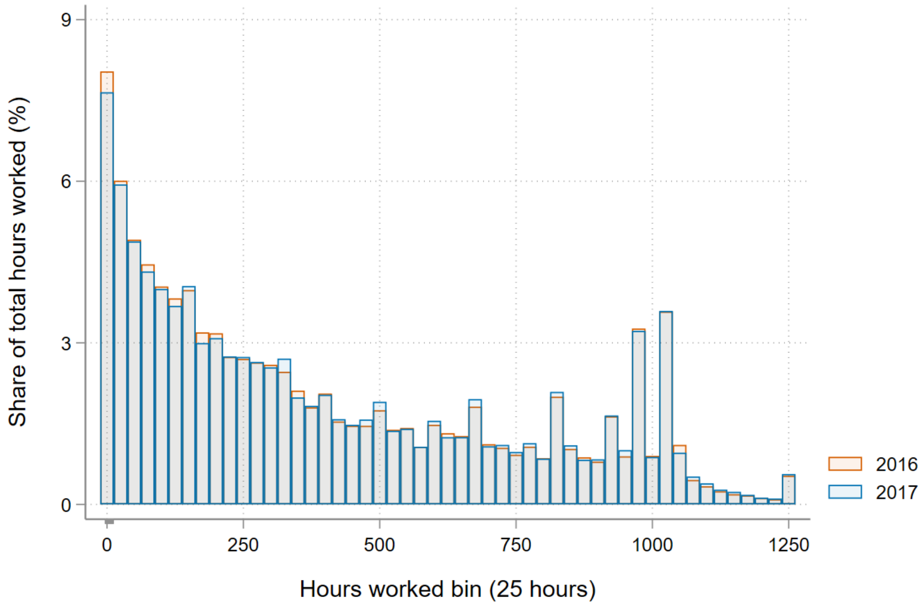
(c) 20-year-olds



Notes: The dashed vertical line is positioned at the new age-specific minimum wage. Blue bars show the estimated change in total hours worked in each €0.50 wage-bin over the six month post-reform period relative to the average total hours worked by age group over the six-months before the reform. Error bars show the 95% confidence interval using robust standard errors. The orange line shows the running sum of estimated changes across wage bins; the shaded area shows the 95% confidence interval calculated using the delta method.

Hours distribution in second half of 2017

Figure 2.C.13 Robustness check on bunching in distribution of hours worked due to subsidy

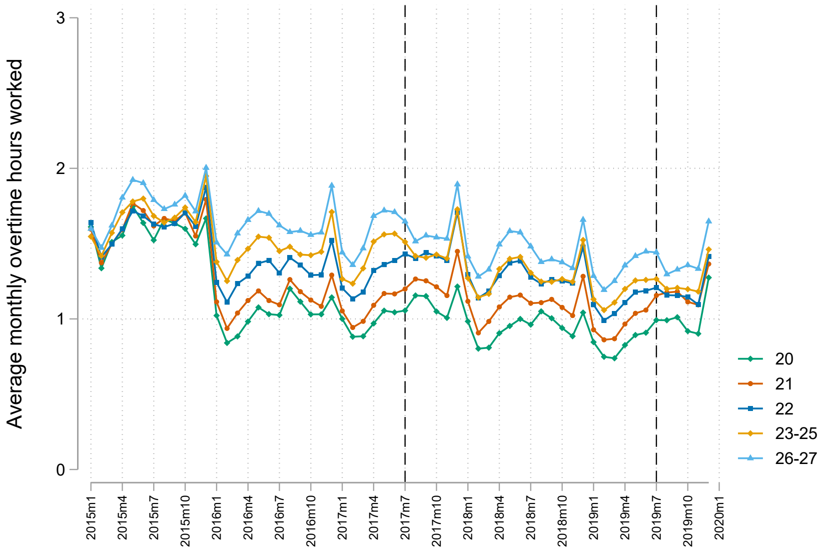


Notes: Hours worked by all 22-year-old workers in the main sample in the second halves of 2016 and 2017.

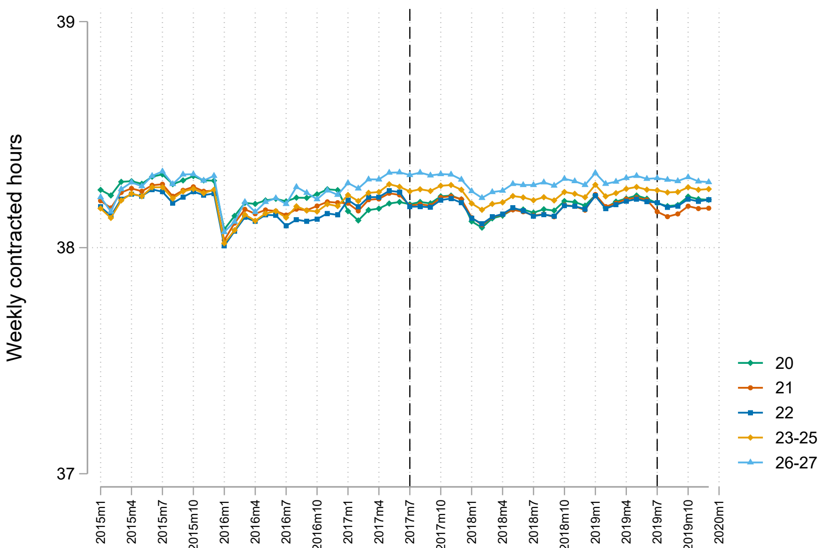
2.D Labor market trends in low-wage jobs

Figure 2.D.1 Youth labor market trends

(a) Average monthly overtime hours worked in low-wage jobs



(b) Average weekly contracted hours in low-wage jobs



The dashed vertical lines indicate the months of the Dutch youth minimum wage reforms.

2.E Hourly minimum wage computation

As discussed in the main text, the hourly minimum wage depends on the number of hours that constitute a full-time workweek, which can differ across sectors, firms, and individual labor contracts. We therefore calculate the applicable minimum wage for each job (defined as a firm $\times$ worker observation) separately, using the following procedure:

1. For each job, we observe full-time-equivalent days worked. We divide this by the working days in a month to calculate the part-time share of each job.
2. We then divide observed hours worked per month for each job by the part-time share to obtain the implied full-time hours worked.
3. We drop outliers in implied full-time hours per month: these are observations above the 99th percentile or below the 1st percentile of the distribution.
4. We then compute average hours worked for each job over the calendar year and divide this by 4.33 to obtain average hours worked per week (or 4.35 if it is a leap year).
5. Finally, we assign each job to one of three hours categories that minimizes the distance between the category and the hours calculated in step 4: 36, 38 or 40.
   - If we cannot assign a category, we assign the mode within the firm.
   - If an individual works in primary or secondary education, we assign 36.86 hours, which are the full-time hours worked according to the collective labor agreement.

2.F Employment effect computations

2.F.1 Overall employment effects and minimum wage elasticity

We compute the employment effects and employment elasticities with respect to the minimum wage (MWE) as follows.

First, for each treated age-group we estimate the change in the number of jobs and total hours worked per capita in each €0.50 wage bin b in period τ, relative to the average of the six-month pre-treatment period ($\tau \in \{-6, ..., -1\}$) using regression equation (2.4.1). These effects are captured by coefficients $\beta_{b\tau}$.

Second, we sum the estimated changes in each wage bin up to the wage bin where the impact of the minimum wage is found to be negligible, represented by $\overline{W}$. Third, we normalize the estimated change in each wage bin by the average age-specific number of jobs (hours worked) per capita over the six months before treatment, $EP_{\tau\in\{-6,...,-1\},a}$.

This yields the estimated change in the number of jobs (hours worked) in period τ and the average of the six-month pre-reform period, $\tau \in \{-6, ..., -1\}$, expressed as a percentage of total pre-treatment employment:

$$\Delta E_{a,\tau} = \frac{\sum_{b=-6}^{\overline{W}} \beta_{b\tau}}{EP_{\tau\in\{-6,...,-1\},a}}$$

We subsequently average $\Delta E_{a,\tau}$ for $\tau \geq 0$ to obtain the short-run impact of the increase of the minimum wage on overall low-wage employment, ΔE_a. Next, we compute the MWEs of the number of jobs (total hours worked) by dividing ΔE_a by the change in the real applicable minimum wage, minus the increase in the real minimum wage for adults.

2.F.2 Own-wage elasticity

In order to obtain the own-wage elasticity (OWE) of the number of jobs and total hours worked for each treated age-group, we first use the estimated $\beta_{b\tau}$ from (2.4.1) to obtain the change in the average hourly wage of low-wage workers. For this purpose, we first compute the change in wage bill up to threshold wage bin $\overline{W}$:

$$\Delta WB_{a,\overline{W}} = \frac{\frac{1}{6}\sum_{\tau=0}^{5}\sum_{b=-6}^{\overline{W}} \beta_{b\tau} \times (MW_a + 0.50b)}{\sum_{b=-6}^{\overline{W}} \overline{w}_{b,a} \times EP_{\tau\in\{-6,...,-1\},a,b}},$$

where $\overline{w}_{b,a}$ is the average wage in wage bin b for treated age-group a between $\tau = -6$ and $\tau = -1$ (i.e. the six months prior to treatment). $EP_{\tau\in\{-6,...,-1\},a,b}$ is the average total employment (i.e. jobs or hours) per capita in wage bin b for treated age-group a between $\tau = -6$ and $\tau = -1$. The denominator is the average cumulative wage bill to the age-specific population over the bins up to wage bin $\overline{W}$ in the six months before treatment. The numerator is the change in the total wage bill to population, averaged over the 6 post-reform months.

Next we compute the change in employment up to threshold $\overline{W}$:

$$\Delta E_{a,\overline{W}} = \frac{\frac{1}{6}\sum_{\tau=0}^{5}\sum_{b=-6}^{\overline{W}}\beta_{b\tau}}{\sum_{b=-6}^{\overline{W}} EP_{\tau\in\{-6,\ldots,-1\},a,b}}$$

Finally, we compute the change in the average wage up to threshold $\overline{W}$ as follows:

$$\Delta W_{a,\overline{W}} = \frac{\Delta WB_{a,\overline{W}} - \Delta E_{a,\overline{W}}}{1 + \Delta E_{a,\overline{W}}}$$

The own-wage elasticities are computed by dividing $\Delta E_{a,\overline{W}}$ by $\Delta W_{a,\overline{W}}$, using the delta method to compute the standard errors.

2.F.3 Spillover share

To obtain the spillover share of the increase in the average wage, we first calculate the change in the average wage up to threshold $\overline{W}$ in the absence of spillovers. This is done by moving each missing job or hour worked under the new minimum wage exactly to the new minimum wage:

$$\Delta W_{A,\overline{W},nsp} = \frac{\frac{1}{6}\sum_{\tau=0}^{5}\sum_{b=-6}^{-1}\beta_{b\tau} \times 0.50b}{\sum_{b=-6}^{\overline{W}} \overline{w}_{b,a} \times EP_{\tau\in\{-6,\ldots,-1\},a,b}},$$

Finally, we compute the spillover share as follows:

$$1 - \frac{\Delta W_{A,\overline{W},nsp}}{\Delta W_{A,\overline{W}}}$$

CHAPTER 3

Monopsony in the Netherlands: A mover-based approach

3.1 Introduction

Recently, there has been a proliferation of empirical studies documenting disparities in the wages paid by different firms to observably similar workers (e.g., Abowd et al., 1999; Card et al., 2013a; Song et al., 2019; Bonhomme et al., 2023). Variation in this 'firm-premium' and the sorting of different types of workers to 'high-premium' firms has been shown to contribute to wage dispersion by skill (e.g., Card et al., 2013b), gender (e.g., Card et al., 2016; Bruns, 2019), race (e.g., Gerard et al., 2021), and migration background (e.g., Dostie et al., 2023). These studies point to imperfect labor market competition, in which firms have wage-setting power over their workers.

The observation that firms have some autonomy in wage determination has led to a resurgence of studies that seek to quantify the degree of firm wage-setting – or monopsony – power in the labor market (for recent overviews, see Manning, 2021d; Ashenfelter et al., 2022b; Card, 2022). A key measure of monopsony power is the labor supply elasticity to individual firms – henceforth labor supply elasiticity.[1] In a perfectly competitive labor market, the labor supply elasticity is perfectly elastic and firms pay workers the exogenously determined market wage. In monopsonistic labor markets, the labor supply elasticity is not perfectly elastic and firms face an upward-sloping labor supply curve, which enables them to mark down workers' wages relative to workers' marginal productivity. Smaller values of the labor supply elasticity indicate that workers' labor supply to individual firms is less sensitive to wages paid, resulting in greater monopsony power. Upward-sloping labor supply

[1] Throughout this paper, we use the term labor supply elasticity to refer to the labor supply elasticity to *individuals firms*, which should be distinguished from the market-level labor supply elasticity.

curves can be the result of various factors, such as search frictions (e.g., Manning, 2003) or heterogeneous worker-preferences over (non-pecuniary) workplace amenities (e.g., Card et al., 2018).

We quantify the degree of monopsony power in the Netherlands by estimating the firm-level labor supply elasticity and exploring how the labor supply elasticity varies across different workers and industries. As shown by Manning (2003), estimates of the labor supply elasticity can be recovered by estimating the separation (or recruitment) elasticity with respect to wages.[2] We estimate the separation elasticity using an instrument variable (IV) event-study approach, recently developed by Bassier et al. (2022). This approach allows us to estimate workers' separation responses with respect to the firm-component of wages, which captures differences in firm wage-setting policies.

Intuitively, the event-study approach mimics an experiment in which we have two workers who work at the same origin firm and have similar work histories (e.g., in terms of firm tenure and pre-separation wages). These two workers move to different intermediate firms around the same time. The intermediate firms differ in the average wage paid to their workers. We assume that the move from the origin to the intermediate firm is exogenous conditional on worker histories. Thus, from the two workers, one worker happens to move to a 'high-wage' firm and the other happens to move to a 'low-wage' firm. The two workers experience different changes in their own wage when moving from the origin to the intermediate firm. To isolate the variation in the workers' own wage that is due to differences in the wage-setting policies of the intermediate firms, we instrument the change in workers' own wage with the difference in the average coworker wage between the origin and intermediate firms. Subsequently, we follow the workers for 16 quarters after the initial transition and estimate the separation elasticity with respect to the wage, using only the variation in own wages stemming from differences in intermediate firms' wage-setting policies.

Using employer-employee matched data covering the universe of jobs in the Netherlands over the period 2010-2021, we find evidence of substantial firm wage-setting power in the Dutch labor market. Overall, we estimate a separation elasticity of -3.62 in our preferred specification, with an implied labor supply elasticity facing a firm of 7.24. This suggests that in the Dutch labor market as a whole, firms have a potential to mark down wages with respect to workers' marginal productivity by approximately 12%.[3] We find that separations to non-employment have a lower wage elasticity than separations to employment. Exploring heterogeneity by gender, we find a lower labor supply elasticity for women compared to men. This suggest that firms can potentially mark down wages more for the former. Exploring heterogeneity across the wage distribution, we find that workers at the bottom and top of the wage distribution have the lowest labor supply elasticity, although po-

[2]Rooted in a model of dynamic monopsony, in which monopsony power arises due to search frictions, Manning (2003) shows that the steady-state elasticity of the labor supply facing a firm can be expressed as twice the separations elasticity or twice the recruitment elasticity.

[3]The implied markdown is compute as $1-\frac{\epsilon}{1+\epsilon}$, where ϵ is the firm-level labor supply elasticity, computed as $-2\times$ the separation elasticity.

tential markdowns are more than twice as large for the highest-earning individuals. Consistent with a higher degree of monopsony power at the top of the wage distribution, we find the lowest separation elasticity in the specialized business service industry.

We make several contributions to the literature on monopsony power. Using the framework outlined in Manning (2003), there have been many empirical studies estimating separation elasticities to quantify the degree of monopsony power that firms possess (for a survey of this literature, see Sokolova and Sorensen, 2021). These studies typically estimate workers' separation elasticities with respect to their own wage. However, using all variation in individuals' wages may lead to biased estimates of the labor supply elasticity, as individuals' wages reflect many factors besides the wage-setting policies of their employers.[4] We contribute to the literature by providing new estimates of the firm-level labor supply elasticity using the event-study approach of Bassier et al. (2022). This approach yields an estimate of the labor supply elasticity identified of plausible exogenous variation in the firm-component of wages, which is directly informative about the degree of monopsony power in the labor market. We show that estimates of the separation elasticity are about seven times smaller when using all variation in individuals' wages. This highlights the importance of estimating separation elasticities based on the firm component of wages.

We additionally contribute to the literature by investigating monopsony power using administrative data covering the whole Dutch economy. Most previous studies that use plausibly exogenous variation in wages to identify separation elasticities or recruitment elasticities have focused on specific occupations, firms, or industries (e.g., Ransom and Oaxaca, 2010; Ransom and Sims, 2010; Caldwell and Oehlsen, 2018; Dube et al., 2020). Economy-wide estimates of the labor supply elasticity, obtained using explicit identification strategies are scarce. Using the same methodology as the one adopted in this paper, Bassier et al. (2022) estimate labor supply elasticities for Oregon and Bassier (2023) for South Africa. In addition, whereas we use data on the universe of jobs in the Netherlands, previous studies that quantify firm wage-setting power in the economy as a whole have generally relied on survey data or random samples of the universe of employment records.[5] Moreover, whereas we use information on hourly wages, most of the previous literature use variation in annual, quarterly or daily wage to estimate separation elasticities. These wage measures may lead to attenuation bias due to measurement error associated with hours (Bassier et al., 2022).

[4]Variation in individual wages is likely to capture factors that influence the job opportunities of individual workers. For example, changes in a person's health or family circumstances could both effect individual's wages and separation probabilities, leading to omitted variable bias. More generally, variation in individual wages due to factors other than differences in firm wage-setting policies (e.g., permanent skill differences) can be considered measurement error and could lead to attenuation bias.

[5]Notable exceptions are Webber (2015, 2016, 2022), who uses the near universe of employment records in the US, Bassier et al. (2022) who use population-wide employment records from Oregon, and Bassier (2023) who uses data on the universe of South African formal sector workers

By focusing on a European country – i.e. the Netherlands – we additionally contribute to the literature, which has predominantly focused on the US. Moreover, studies outside of the US often lack explicit identification strategies to isolate the firm-component of wages (e.g., Hirsch et al., 2010, 2018; Van den Berge et al., 2020; Bachmann et al., 2022; Hirsch et al., 2022a,b).[6] Due to the stark differences in labor market institutions between the United States and many European countries, investigating the degree of monopsony power in a European setting can be informative about the role of labor market institutions as potential barriers to firm wage-setting power.

Finally, we contribute to the literature by exploring how labor supply elasticities vary by gender, workers' position in the wage distribution, and industry. Differences in the degree of monopsony power can contribute to wage dispersion in the economy as a whole and can contribute to wage gaps between different subgroups of workers (e.g., the gender wage gap). Exploring heterogeneity in monopsony power therefore contributes to our understanding of the sources of wage inequality.

The paper is structured as follows: Section 3.2 describes the matched employer-employee data we are using. Section 3.3 discusses the matched event study approach. Section 3.4 presents the estimates from the matched event study approach and our heterogeneity analysis and Section 3.5 concludes.

3.2 Data

We construct a quarterly panel of individual employment spells over the period 2010-2021, using detailed employer-employee matched administrative data collected by Statistics Netherlands. The employment data is based on income statements from employers to the Dutch Tax Authority and Employee Insurance Agency (UWV) and covers the full universe of jobs in the Netherlands. The unprocessed employment data contains monthly employment records at the individual firm level with information on gross monthly earnings, hours worked, and various job characteristics such as the type of contract and the sector of employment.

We first collapse the monthly data to the quarterly level, yielding a quarterly panel of individual employment spells (consecutive quarter observations with the same employer). Note that individuals may have multiple parallel employment spells in the same quarter at different employers. The unprocessed quarterly employment spell panel contains around 402 million observations corresponding to 13 million individuals and around 1 million firms.

We subsequently impose several sample restrictions broadly in line with Bassier

[6]The studies by Hirsch, Bachmann, and their co-authors focus on Germany and investigate firm-level labor supply elasticities between men and women (Hirsch et al., 2010), across immigrant and native workers (Hirsch and Jahn, 2015), across business cycles (Hirsch et al., 2018) as well as across different job task groups (Bachmann et al., 2022). Van den Berge et al. (2020) estimate labor supply elasticities for individual firms in the Netherlands between 2006 and 2018. Using the approach introduced by Webber (2015), they find an average labor supply elasticity of 1.3, which is close to the 'naive' estimate presented in this paper. Focusing on another non-US country, Booth and Katic (2011) estimate labor supply elasticities for Australia.

et al. (2022) and the literature using matched employer-employee data (e.g., Card et al., 2013a; Sorkin, 2018; Song et al., 2019; Lachowska et al., 2020; Lamadon et al., 2022). Here, we provide a brief summary of the sample selection procedure. More comprehensive details can be found in table 3.A.1. We omit entire employment spells with fewer than 100 hours worked per quarter on average over the spell, with hourly wages less than two euros in any quarter, and spells that are shorter than 3 quarters. We convert the data to a worker-level quarterly panel by retaining the spell at the dominant employer in each quarter.[7] We omit small firms with fewer than five employees in any year.[8] Finally, we exclude firms in the public administration sector. Our final worker-level panel consists of over 265 million observations and contains information on 9.5 million workers in 125 thousand firms. Table 3.A.1 in Appendix 3.A provides summary statistics for each step of the sample selection procedure.

For each employment spell in the worker panel, we identify the hire and separation quarter and the type of employment transition. Specifically, we define employment-to-employment (E-E) transitions as transitions between employers with a maximum unemployment duration of 90 days.[9]

For our analysis, we extract a matched event study panel from the final worker-level panel. Around each employment-to-employment (E-E) separation identified in the worker-quarter panel, we isolate an event window of 9 pre-separation quarters and 17 post-separation quarters. All the E-E separations are stacked by the calendar quarter of the initial transition (t) when an individual moves from an initial firm (origin firm, $O(i)$) to another firm (intermediate firm, $I(i)$). An event is defined as a group of workers (at least two workers) transitioning from the same origin firm to (different) intermediate firms in the same calendar quarter.

We impose some further sample restrictions. We exclude events where more than 10 workers move between the same origin and intermediate firm. Such events likely capture changes in firm identifiers due to mergers and acquisitions or other changes in the production or ownership structure rather than actual E-E transitions.[10] Only workers aged 23-65 at the time of the event are retained.[11] We additionally restrict the sample to individuals with an employment spell of at least four quarters at

[7]The dominant employment spell is defined as the spell with the highest average earnings for each individual per quarter.

[8]As a robustness check we further limit the sample to firms with less than ten and 20 employees.

[9]Since we have information on the start and end date of each spell, we can flexibly define E-E transitions with different unemployment duration thresholds. We have experimented with different durations, including considering only direct transitions. These different thresholds yield comparable results. Here, we deviate from Bassier et al. (2022) who define E-E transitions based on quarter-to-quarter employment at different firms, allowing for a maximum unemployment gap of 178 days.

[10]This excludes around a third of initial E-E transitions. Setting slightly different cutoffs does not alter the results.

[11]The lower age bound is set to circumvent issues related to lower age-specific minimum wage applying to workers aged 15–22 in the Netherlands.

the origin firm to ensure that at least two full quarters of wages are observed.[12] Finally, we only retain individuals who do not move back to the origin firm within the 17 post-event quarters. Both the change in own wages and co-worker wages at the transition between the origin- and intermediate firms are winsorized at the 1% top and bottom tails.

Sample summary statistics for the matched event study panel are provided in Table 3.A.2. The full sample consists of close to 1.4 million initial E-E separations corresponding to around 1.2 million workers and 47 thousand origin firms. There are over 200 thousand events – i.e. E-E transitions from origin firm by calendar quarter. These workers transition to approximately 74 thousand intermediate firms. Over two-thirds of workers re-separate from the intermediate firm to 37 thousand final firms within 16 quarters after the initial E-E transition. Panel B of Table 3.A.2 shows the summary statistics for our baseline specification with interacted controls for event, hire wage and tenure. Panel C shows summary statistics for our preferred specification including interacted controls for event, hire wage, tenure and three quarters of pre-separation wages. The sample consists of approximately 290 thousand initial E-E separations corresponding to around 277 thousand individuals and 12 thousand origin firms.

3.3 Methodology

I briefly discuss the intuition behind the dynamic monoposony model as outlined in Bassier et al. (2022) in Section 3.3.1 .[13]. In Section 4.4, I discuss our empirical approach. The discussions in Sections 3.3.1 and 4.4 draws extensively on Bassier et al. (2022).

3.3.1 Dynamic monopsony model of Bassier et al. (2022)

Suppose a worker i is employed at firm j at time t, denoted by f_{ijt}, and transitions to another firm j' at time $t+1$ with probability $Pr(f_{ij't+1}|f_{ijt})$. A key feature of the Bassier et al. (2022) model is that i's marginal productivity consists of an individual-specific component A_i – e.g., reflecting skill – and a firm-specific component p_j. Since A_i is individual-specific, it is fixed across firms, and should therefore no affect the transition probabilities $Pr(f_{ij'\neq jt+1}|f_{ijt})$. In a steady state, the total employment probability of workers of productivity type i by firm j paying wage W_{ij}, denoted as q_{ij}, is equal to $\frac{Rij(W_{ij})}{Sij(W_{ij})}$.[14] Here $R_{ij}(W_{ij})$ is the total recruitment probability and $S_{ij}(W_{ij})$ is the total separation probability. In the steady state, monopsonists maximize profits subject to $q_{ij} = \frac{R_{ij}(W_{ij})}{S_{ij}(W_{ij})}$. As shown by Bassier et al. (2022), at the optimum, firm j sets the log wage of workers of productivity type i to $w_{ij} = \alpha_i + \phi_j$.[15] Here $\alpha_i \equiv log(A_i)$ and is an individual-specific wage-component

[12]This facilitates conditioning on hire wages and end wages at the origin firm.

[13]For a more detailed exposition, see Section 1.3 of Bassier et al. (2022)

[14]See Manning (2003) for a derivation of this result.

[15]This follows from the assumption that total match marginal productivity is given by $y_{ij} = A_i p_j$. Taking logs implies that the individual- and firm-specific productivity components are

that is common to all workers of type i. $\phi_j \equiv log(\frac{\epsilon_j}{1+\epsilon_j}p_j)$ is a firm-specific wage component chosen by a firm, and $\frac{\epsilon_j}{1+\epsilon_j}$ is a markdown relative to firm-specific marginal productivity p_j.

The key assumption made by Bassier et al. (2022) is that only the firm-component of the wages changes along with firm j's choice of q. This stems from the assumption that the individual-specific wage-component α_i is portable across firms and therefore should not affect individual i's decision to supply labor to firm j or j'. As a result, the labor supply elasticity of worker type i to firm j, $\frac{dlog(qij)}{dwij}$, reduces to $\frac{dlog(qij)}{d\phi_j} = \epsilon(\phi_j)$. By the steady state assumption, $\epsilon(\phi_j) = \gamma(\phi_j) - \eta(\phi_j)$, where $\gamma(\phi_j) = \frac{1}{E[R_{ij}]}\frac{dE[R_{ij}]}{d\phi_j}$ and $\eta(\phi_j) = \frac{1}{E[S_{ij}]}\frac{dE[S_{ij}]}{d\phi_j}$ are the recruitment and separation elasticities, respectively. Imposing that $\gamma(\phi_j)$ and $\eta(\phi_j)$ are constant, as in Manning (2003), than $-\eta = \gamma$ and $\epsilon = -2\eta$.[16]

The traditional approach to estimating η is to regress a worker's separation (or hazard) rate on their own log wages. However, from the firm's perspective, the relevant separation elasticity is based on the separation response of workers due to variation in ϕ_i. An estimate of the separation elasticity facing the firm will be given by:

$$E[s_{ijt}|w_{ijt}] = E[s_{ijt}|\phi_{j(i,t)}] = \eta(\phi_{j(i,t)}) \tag{3.3.1}$$

where s_{ijt} equals 1 if individual i separates from firm j at time t. We can obtain an estimate of the separation elasticity from $\hat{\eta} = \frac{\eta'(\phi_{j(i,t)})}{E[s_j]}$. If firms pay relatively little to all of their employees, i.e. the firm component of wages is low, then we would expect more separations to occur. If a firm offers low wages, then workers are more likely to receive better offers from other firms and separate from their current firm.

3.3.2 Matched IV event-study approach

3.3.3 General approach

The previous Section discussed the importance of focusing on the firm-specific component of wage variation when estimating separation elasticities to quantify the degree of monopsony power in the labor market. In what follows, we describe

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additively separable, which does not allow for a match-specific productivity component.

[16]Manning (2003) shows that in steady-state, employment at a firm paying a given wage w is given by $N(w) = \frac{R(w)}{S(w)}$, where $R(w)$ shows the flow of recruits and $S(w)$ captures the separation rate. Further, from this, it can be shown that the employment elasticity of labor supply to the firm ϵ is given by $\epsilon = \epsilon_R - \epsilon_S$, where ϵ_R shows the recruitment elasticity and ϵ_S shows the elasticity of separations. Thus, the labor supply elasticity to firms is given by the recruitment elasticity minus the separations elasticity. Moreover, this can further be decomposed into $\epsilon = \theta^R{\epsilon_R}^E + (1-\theta^R){\epsilon_R}^N - \theta^S{\epsilon_S}^E - (1-\theta^S){\epsilon_S}^N$, where the recruitment elasticity is decomposed into the elasticity of recruitment from employment ${\epsilon_R}^E$ and from non-employment ${\epsilon_R}^N$. θ^R captures the share of recruits from employment. The separations elasticity is decomposed into the elasticity of separation to employment ${\epsilon_S}^E$ and to non-employment ${\epsilon_S}^N$. θ^S captures the share of separations to employment.

the IV event-study approach, developed by Bassier et al. (2022), which allows us to identify the firm-component of wages and to estimate the causal separation response to variation in the wage arising from differences in firms' wage-setting policies.

Suppose the assignment function of workers to firms at time t is given by:

$$f_{ijt} = G_{jt}(\{\bar{w}_k\}, \{w_{ir}, f_{ik'r}\}_{r<t}) \tag{3.3.2}$$

Equation 3.3.2 states that the assignment of worker i to firm j at time t is governed by a worker i's past wages w_{ir} and firm assignments $f_{ik'r}$ – henceforth denoted as $L(History_{i,t})$ – and by firms' average log wages $\bar{w}_k$. This assumption says that conditional on $L(History_{i,t})$, the assignment to firm j is predicted by the average wage $\bar{w}_j$ paid to workers at firm j. Note that in the dynamic monopsony model outlined in the previous Section, worker-specific A_i productivity does not affect f_{ijt}. Equation 3.3.2 relaxes this assumption, allowing for path-dependency in which an individual's past wage and firm assignment can affect the probability of the worker being assigned to a given firm j.

Following the intuition of the dynamic monopsony model outlined above, the log wage of worker i employed at time t can be expressed as:

$$w_{ijt} = \sum_j \phi_j \bar{w}_j f_{ijt} + L(History_{i,t}) + \epsilon_{ijt} \tag{3.3.3}$$

Since $L(History_{i,t})$ includes lagged wages and indicators for past firm assignments, focusing on the time of transition t, Equation 3.3.5 can be rewritten as:

$$w_{ijt} - w_{ijt-1} = \tilde{\phi}(\bar{w}_j - \bar{w}_{j'})(f_{ijt} - f_{ij't-1}) + L(History_{i,t}) + \nu_{ijt} \tag{3.3.4}$$

Equation 3.3.4 states that the change in the wage of worker i between t and $t-1$ depends on their employment history $L(History_{i,t})$ and the change in the average wage of firm j and j'. The latter reflects the difference between the firm wage-setting policies of j and j'. The dynamic monopsony model outlined in Section 3.3.1 imposes that the firm-component of wages is the same for all the workers in firm j. This assumption is relaxed in Equation 3.3.4, allowing the firm-component of wages to be heterogeneous – e.g., due to match effects – so that $w_{ijt} - w_{ijt-1}$ can be different for workers switching to the same firm. We can obtain an estimate of the separation elasticity with respect to firm-wage setting policies by regression the separation rate at time $t+k$ on the wage change associated with the transition at time t, controlling for $L(History_{i,t})$:

$$S_{ijt+k} = \eta \Delta w_{ijt} + L(History_{i,t}) + \epsilon_{ijt} \tag{3.3.5}$$

To isolate the variation in Δw_{ijt} that is due to the difference in firm wage-setting policies, we instrument Δw_{ijt} by $\Delta \bar{w}_j$, using Equation 3.3.4 as a first-stage.

Intuitively, the event-study approach mimic the following experiment: consider two workers $i \in \{1, 2\}$ who are both employed at origin firm $O(i)$ at time $t-1$.

These workers are very similar in their past employment history and wages. Both workers transition to different intermediate firms $I(i)$ at time t, experiencing a different change in their own wage. We subsequently estimate the probability of 're-separating' from firm $O(i)$ (either to final firm $F(i)$ or non-employment) over the next k periods, using only the variation in own wage that is due to the difference in the average wage paid by firms O and I, which is assumed to be exogenous conditional on workers' employment histories.

3.3.4 Empirical implementation

Following Bassier et al. (2022), we stack all observations by the date of initial transition quarter t, when a worker i moves from origin firm $O(i)$ to intermediate firm $I(i)$. We subsequently estimate a worker's 're-separation' probability over the next k quarters, where $k \in \{1, .., 16\}$, conditional on fully saturated interactions of indicators for $O(i)$, octiles for the initial wage and tenure at $O(i)$, calendar quarter of the $O(i) - I(i)$ transition d, and octiles of three quarters of pre-transition wages, all interacted with event quarter t. Consequently, we are comparing workers with nearly identical wage and employment trajectories at the same origin firm, who transitioned to an intermediate firm during the same quarter. We regress

$$\begin{aligned} s^I_{i,t+k} =& \eta_k(w_{i,I(i),t} - w_{i,O(i),t-1}) \\ &+ L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k} \end{aligned} \tag{3.3.6}$$

Since L contains indicators for $O(i)$ and wages at $O(i)$, all the variation that identifies η_k comes from $w_{i,I(i),t}$. We instrument $w_{i,I(i),t} - w_{i,O(i),t-1}$ with $\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}$ from the first-stage regression

$$\begin{aligned} w_{i,I(i),t} - w_{i,O(i),t-1} =& \phi(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}) \\ &(f_{I(i),t} - f_{O(i),t-1}) + L(History_{i,t,d}) + \epsilon_{i,t} \end{aligned} \tag{3.3.7}$$

where $\bar{w}$ are average coworker wages. Note that all wages are measured as log hourly wages. The corresponding reduced form is given by

$$\begin{aligned} s^I_{i,t+k} =& \delta(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}) \\ &+ L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k} \end{aligned} \tag{3.3.8}$$

The separation elasticity is estimated as $\hat{\eta} = \frac{\delta_k}{\phi \bar{s_k}}$.

3.4 Main findings

We begin the exposition of our main findings with a discussion of the first-stage. Figure 3.1 shows the estimates from the first-stage Equation 3.3.7, for the event quarter from $t - 9$ to $t + 16$. We control for worker's history by including fully saturated interactions between indicators for starting wages of workers at $O(i)$ in octiles, tenure at $O(i)$ in octiles, the calendar quarter of transition from $O(i)$ to $I(i)$, and the three quarters of pre-separation wages in octiles.

Figure 3.1 shows two key findings. First, conditional on workers' history, the wages of individuals who moved to high- or low-wage $I(i)$ firms follow parallel trends prior to the $O - I$ transition. This serves as a falsification test on the exogeneity assumption of the $O(i)$ firm wage. Note that since we condition on three quarters of pre-separation wages octiles, we effectively impose parallel trends for the three quarter prior to the $O - I$ transition.[17] However, no such restrictions are imposed for earlier event-quarters. Second, there is a clear jump in the own wage for workers who move to a higher average wage firm at time t, relative to those who transition to lower average wage firms. Moreover, this jump in the own wage is highly persist and is relatively stable.[18] At time t the estimated coefficient on ϕ is 0.23, which means that, on average, workers who transition to a $I(i)$ firm with a 1% higher average coworker experience a 0.23% higher increase in their own wage. Moreover, we can interpret this as 23% of the variation in overall wages being driven by the firm-component in wages (Bassier et al., 2022; Bassier, 2023). This falls well within the range of the firm-component in total wage variation identified in the literature (e.g., Card et al., 2013b; Bonhomme et al., 2023) and is a similar magnitude found in Bassier (2023).

Figure 3.2 shows the estimated impact of higher firm average coworker wages on the retention probability for up to 16 quarters after the $O - I$ transition. For each $k \in \{1, .., 16\}$, we estimate

$$\begin{aligned} R_{i,t+k} =& \delta_t \Delta ln(\bar{w}_{i,I(i),t}) + \\ & L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k} \end{aligned} \tag{3.4.1}$$

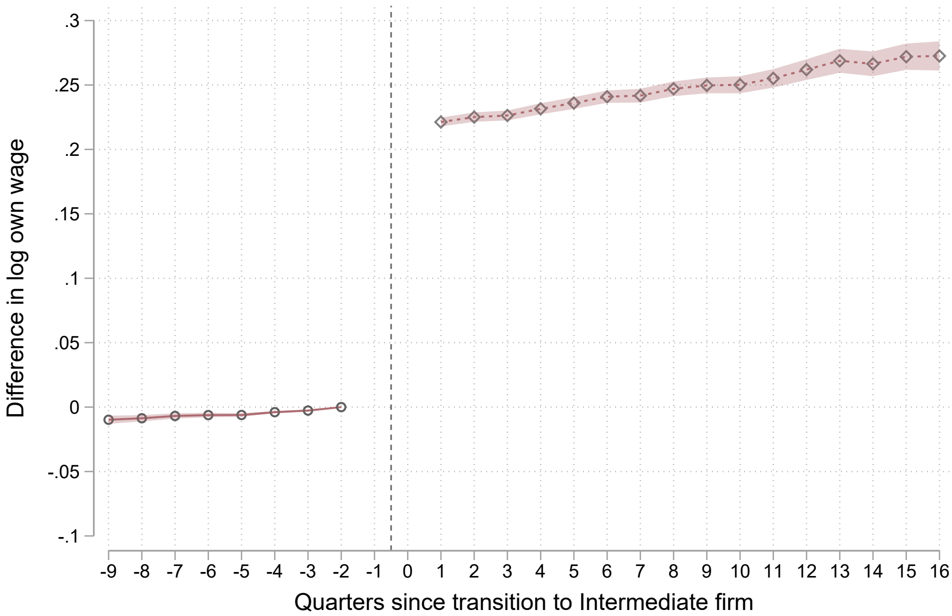
where $R_{i,t+k}$ takes on a value of one if a worker is retained at firm $I(i)$ in event quarter k. The black line in Figure 3.2 shows the average retention probability for workers in the sample. The red line shows the predicted retention probability for workers who transition to intermediate firms with a log point higher firm average coworker wage. The gap between the average and the predicted retention probability identifies the causal effect of workers being assigned to firms with a log point higher average coworker wage. After four quarters the gap is approximately -0.3, which widens to more than -0.4 after 16 quarters. This shows that workers who transition high-wage $I(i)$ firm are substantially less likely to separate in the quarters after the $O - I$ transition.

After having shown that the change in firm average wages is a strong predictor

[17] Appendix figure 3.B.1 shows the first stage estimates from a regression without controlling for three quarters of pre-separation wages before the transition at time t. This is the specification adopted by Bassier et al. (2022). In this figure, we can see a positive pre-trend. This indicates that workers who move to a firm with higher average coworker wages are on different wage trajectories before and after the move to the intermediate firm. Therefore, we also match workers on wages earned three quarters before the O-I transition to ensure that we are comparing similar workers, i.e. workers who are on the same wage trajectories.

[18] 3.B.1 shows a slightly upward-sloping trend over the post-transition quarters. This could suggest that workers transitioning to high-wage firms are on slightly different wage trajectories.

Figure 3.1 First-stage results of individual wage change on firm average wage difference



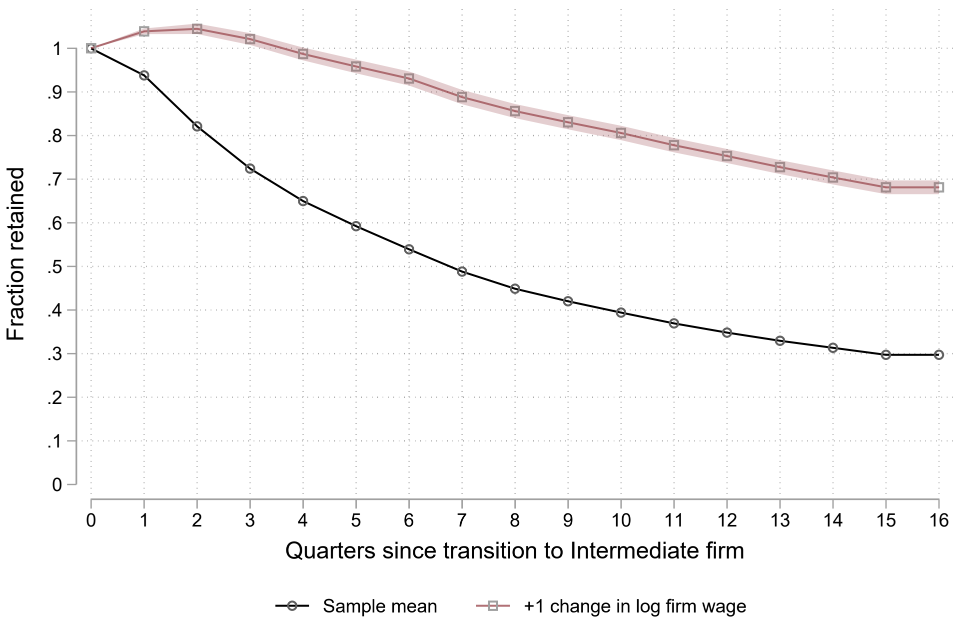
Notes: Figure shows the coefficients from the first-stage regression (see equation 3.3.7) of individual wage changes on the difference in firm average wages. The coefficients are plotted separately for each event-time quarter $k \in \{-9, .., 16\}$. The difference in firm average wages ($\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}$) refers to the change in firm average wages for individual i who moves from the origin firm in $t-1$ to the intermediate firm in t. We control for worker history $L(History_{i,t,d})$, including the origin firm identifier, initial wages at the origin firm in octiles, origin firm tenure in octiles, calendar quarter of $O-I$ transition, and three quarters of lagged wages before event time t. The shaded area shows the 95% confidence intervals. Robust standard errors are cluster at the origin firm level by time.

of the change in individuals' own wage, and that firm average wages have a clear impact on the separation probability, we combine these two results and estimate the separation elasticity with respect to the firm-component of wages over a 16-quarter window following the $O - I$ transition.

Table 3.1 shows the first-stage estimates as well as the estimates of the separation elasticities with different sets of control variables in line with equations 3.3.7 and 3.3.8, respectively. Column 1 reports estimates of a specification without additional control for worker histories. Similar to Bassier et al. (2022), we find that once we control for the identify of $O(i)$ (Column 2) the estimated separation elasticity is much larger. This suggests that past firm assignment contains important information about exogenous separation rates. Column 3 reports estimates from the preferred specification of Bassier et al. (2022), which additionally controls for the initial wage and tenure at firm $O(i)$. Our preferred specification is shown in Column 4, where we additionally control for three quarters of pre-separation wages before the transition. Controlling for age at the $O - I$ transition and job characteristics (Columns 5 and 6) yield nearly identical estimates as those in Column 4. This suggests that most of the relevant information on exogenous separation rates is captured by our preferred specification.

Using our preferred specification (Column 4), the first-stage coefficient on the

Figure 3.2 Retention probability for workers moving to firms with higher average wages



Notes: Figure shows coefficients from a regression of the retention probability of workers at the intermediate firm $R_{i,t+k}$ on $\delta(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1})$. The retention probability at the intermediate firm is shown separately for the first 16 quarters after the $O - I$ transition, hence for each event-time period $k \in \{1, 16\}$. We control for worker history $L(History_{i,t,d})$, including the origin firm identifier, initial wages at the origin firm in octiles, origin firm tenure in octiles, calendar quarter of O-I transition, and three quarters of lagged wages before event time t. The shaded area shows the 95% confidence intervals. Robust standard errors are cluster at the origin firm level by time.

difference in firm average coworker wages is 0.23. The separation elasticity is −3.62. This suggests that if the firm wage-component would decrease by 1%, then 3.62% of employees would be unwilling to work at the intermediate firm. Using the 2× separations elasticity rule, this suggests a labor supply elasticity of 7.2. This labor supply elasticity indicates a potential wage markdown of approximately 12% for the Dutch labor market as a whole.[19] The separation elasticity is lower for $E - N$ transitions compared to $E - E$ transitions. This suggests that employment-to-non-employment transitions are less response to changes in firm wage-setting policies.

The estimated labor supply elasticity is clearly far off the perfectly elastic labor supply elasticity implied by perfect competition. However, our estimate is substantially larger compared to other studies using quasi-experimental research designs (e.g., Caldwell and Oehlsen, 2018; Cho, 2018; Dube et al., 2019; Kroft et al., 2020), who report labor supply elasticities between 2 and 5. Most comparable to our paper are Bassier et al. (2022) and Bassier (2023), who use the same empirical design and find elasticities of 4.2 and 1.6 for Origin and South Africa, respectively. This suggest that, at least compared to the US and South Africa, firms in the Netherlands have substantially less monopsony power. Recent studies for Germany, using the traditional approach to estimate separation elasticities, report labor supply elas-

[19]Computed as $1 - \frac{\epsilon}{1+\epsilon}$ where ϵ is the labor supply elasticity facing the firm.

ticities between 1 and 4 (e.g., Hirsch et al., 2010; Hirsch and Jahn, 2015; Hirsch et al., 2018; Bachmann et al., 2018; Hirsch et al., 2022a). To compare our estimates to most previous work, we estimate a specification using OLS (Columns 7-8). The implied labor supply elasticity is 0.966, which is more comparable to estimates from previous studies, albeit at the lower end. This highlights the importance of isolating the firm-component of wages to make inferences about the degree of monopsony power: estimates based on the entire variation in individual wages likely underestimate the separation elasticity and therefore, overestimate the wage-setting power of firms..

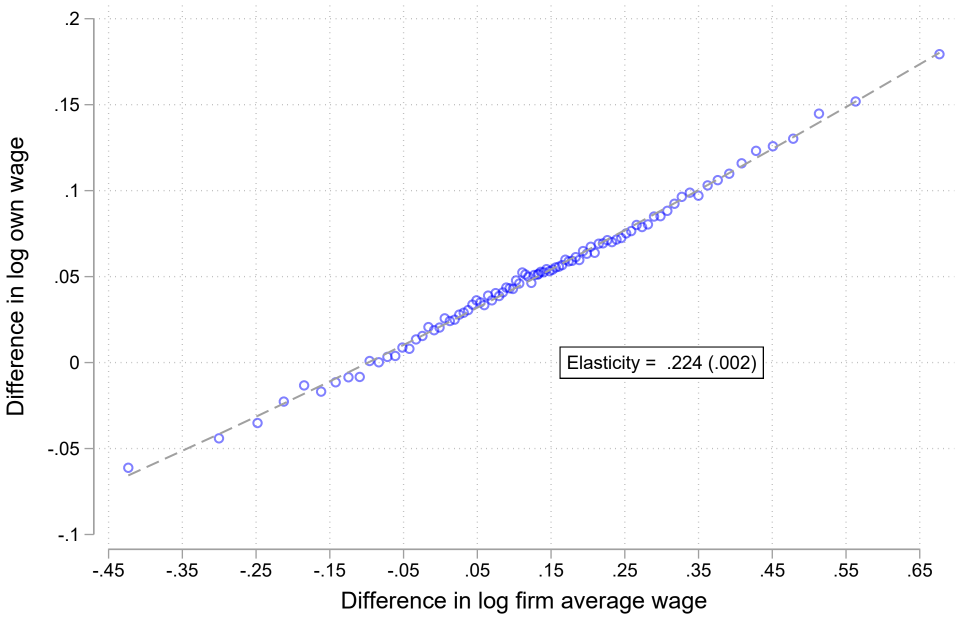
Table 3.1 Separation elasticities based on the firm wage component

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First stage	0.161	0.166	0.215	0.228	0.229	0.230		
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)		
IV estimates								
Separations	-2.094	-5.048	-4.144	-3.620	-3.621	-3.705	-0.482	-0.483
	(0.036)	(0.049)	(0.046)	(0.057)	(0.076)	(0.079)	(0.017)	(0.023)
E-E separations	-3.388	-6.657	-5.534	-4.938	-4.852	-5.000	-0.549	-0.555
	(0.062)	(0.074)	(0.080)	(0.101)	(0.132)	(0.139)	(0.030)	(0.040)
E-N separations	-1.252	-4.186	-3.416	-2.912	-2.975	-3.034	-0.464	-0.459
	(0.027)	(0.047)	(0.048)	(0.060)	(0.084)	(0.088)	(0.018)	(0.025)
Obs (million)	14.271	13.336	5.407	2.468	1.309	1.181	2.468	1.309
O-I moves	1,416,876	1,390,162	627,492	289,821	159,878	144,320	289,821	159,878
Nr of events	236,179	209,465	73,159	39,558	22,096	20,101	39,558	22,096
Fstat (IV)	13,551	25,228	22,060	12,374	6,888	5,891		
Interacted controls								
Time	Y	Y	Y	Y	Y	Y	Y	Y
× firm		Y	Y	Y	Y	Y	Y	Y
× Hire wage × tenure			Y	Y	Y	Y	Y	Y
× 3 qrt wage lags				Y	Y	Y	Y	Y
× Age					Y	Y		Y
× Job Char.						Y		Y
OLS							Y	Y

Notes: The full instrumental variables specification is provided in Equation 3.3.7 and 3.3.8 in the main text. The outcomes $s^j_{i,t+k}$ indicate separation, E-E separation, and E-N separation such that s is missing for all periods after a single re-separation. Each regression includes fixed effects (interacted controls) for worker history ($L(History_{i,t,d})$) where × indicates that fixed effects are interacted. All regressions include "*time*" fixed effects, referring to the calendar quarter of the O-I transition, "*firm*" refers to the origin firm, "*hire wage*" and "*3 qrt wage lags*" refer to the octiles of the hire wage, and the three quarters of pre-separation wages at the origin firm, "*tenure*" refers to the octile of tenure at the origin firm, "*age*" refers to age at the event time (6 categories) and "*Job characteristics*" refer to the contract type (i.e. fixed or permanent contracts) and job type (i.e. flexible work arrangement, on-call, or regular) at the origin firm at the quarter prior to the $O-I$ transition. The sample is restricted to the post-t period. Both the change in own wage and coworker wage are trimmed at the 1 percent tails. Columns 7 and 8 report OLS estimates without using the instrument. Standard errors are clustered at the level of the origin firm × initial separation quarter (event). Reported elasticities are computed by dividing the regression coefficients by the average relevant sample re-separation rate.

As a robustness check, we assess whether our estimates are driven by outliers. Figure 3.3 and 3.4 show the binned scatterplots for the first stage and the IV regressions, corresponding to Column 4 in Table 3.1. Figure 3.B.3 shows an approximately (positive) linear relationship between $\Delta ln(w_{i,t+1})$ and $\Delta ln(\bar{w}_{i,I(i),t})$. Similarly, Figure 3.B.4 shows that the (negative) relationship between separations and $\Delta ln(\bar{w}_{i,I(i),t})$ is nearly constant, except for in the upper-tail. Overall, the estimated labor supply elasticities reported in Table 3.1 appear to not be driven by outliers.[20]

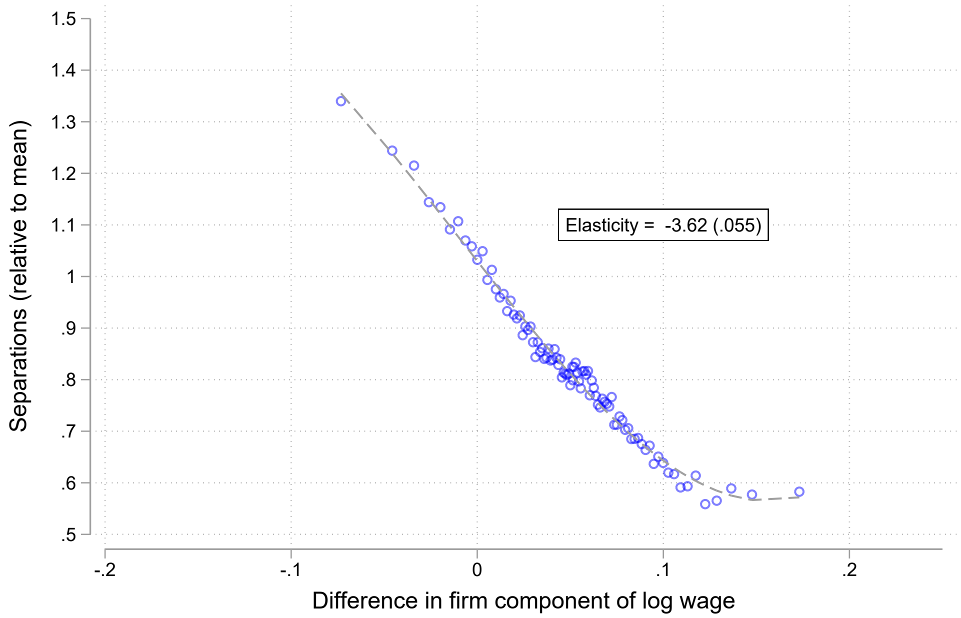
Figure 3.3 Binned scatterplot of the first-stage



Notes: The figure shows the first-stage relationship between $\Delta ln(w_{i,t+1})$ and $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 4 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

[20] Figure 3.C.1 shows similar results focusing on E-E transitions only.

Figure 3.4 Binned scatterplot of the separation responses



Notes: The figure shows the relationship between separations and $\Delta ln(w_{i,t+1})$, instrumented by $\Delta ln(\bar{w}_{i,I(i),t})$ using a control function: controlling for the residuals from a regression of $\Delta ln(w_{i,t+1})$ on $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 4 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

3.4.1 Separation elasticities for different groups of workers

In this Section, we estimate the separation elasticity for different subsamples of workers. Panel A of Table 3.2 shows the separation elasticity estimates by octiles of pre-separation wages at the origin firms. We find that labor supply to the firm is the least elastic at the bottom and the top of the wage distribution. However, compared to those in the lowest octile of pre-separation wages, the labor supply elasticity is more than twice as small among those in the highest octile. This suggest that Dutch firms have more monopsony power over high-wage workers. In contrast to our findings, Bassier et al. (2022) find that workers in the top quartile of pre-separation wages have the highest labor supply elasticities. However, our findings are in line with Seegmiller (2021), who shows that high-productivity firms have more monopsony power and that the most skilled workers have the lowest labor supply elasticities. As high-skilled workers often earn higher wages and tend to sort into more productive firms, this could contribute to the pattern shown in Panel A Table 3.2. Firm-specific human capital and other factors that limit worker mobility between firms (e.g., stringent contract requirements for high-skilled immigrants) could further contribute to monopsony power over high-wage workers.

Panel B of Table 3.2 shows estimated labor supply elasticities for selected industries. Consistent with a higher degree of monopsony power over high-wage workers, we find the lowest labor supply elasticity in the specialized business and business support services.[21]

Female workers have lower separation elasticities than male workers (Panel C) indicating that firms have more wage-setting power over female employees. This is in line with previous research, showing that the labor supply elasticity of women is smaller compared to men. Possible reasons include lower mobility of women and different preferences over non-wage amenities of jobs (Ransom and Oaxaca, 2010; Hirsch et al., 2010; Webber, 2016). The estimated labor supply elasticities are 7.43 for men and 5.90 for women. Assuming that marginal productivity is the same (on average) between men and women, and firms fully exploit there wage-setting power, this would imply women are paid 3.1% less purely as a result of the disparity in labor supply elasticities.[22]

[21]The specialized business service industries, e.g., include consultancy, legal services, tax consultancy, administration, architects, research and development.

[22]Following Webber (2016): $ln(w_F) - ln(w_m) \approx \frac{w_F - w_M}{w_M} = \frac{\epsilon_M - \epsilon_F}{\epsilon_F(\epsilon_M + 1)}$.

Table 3.2 Separation elasticities based on firm wage component: heterogeneity between groups of workers

	First stage	se	Sep.	se	E-E Sep.	se	Movers	Mark-down
Panel A: Octile of Pre-separation Wage								
Octile 1	0.254	(0.004)	-3.175	(0.086)	-3.987	(0.134)	67,219	0.136
Octile 2	0.236	(0.004)	-3.740	(0.118)	-5.176	(0.201)	43,825	0.118
Octile 3	0.242	(0.004)	-4.143	(0.133)	-5.868	(0.257)	34,578	0.108
Octile 4	0.246	(0.005)	-4.421	(0.171)	-6.653	(0.312)	28,063	0.102
Octile 5	0.258	(0.007)	-4.162	(0.193)	-5.921	(0.350)	23,684	0.107
Octile 6	0.215	(0.007)	-3.754	(0.251)	-5.427	(0.476)	23,377	0.118
Octile 7	0.167	(0.007)	-2.844	(0.263)	-3.943	(0.454)	28,685	0.150
Octile 8	0.157	(0.005)	-1.216	(0.171)	-1.094	(0.318)	40,390	0.291
Panel B: Selected Industries								
Manufacturing	0.227	(0.010)	-1.720	(0.237)	-2.382	(0.555)	16,760	0.225
Wholesale & Retail	0.164	(0.007)	-2.642	(0.260)	-4.275	(0.509)	20,177	0.159
Specialized Business Services	0.184	(0.009)	-1.090	(0.298)	-0.921	(0.594)	9,754	0.314
Business Support Services	0.296	(0.006)	-1.426	(0.114)	-1.562	(0.175)	53,313	0.260
Healthcare	0.143	(0.008)	-1.687	(0.316)	-1.492	(0.580)	37,820	0.229
Panel C: Gender								
Male	0.217	(0.003)	-3.716	(0.081)	-5.068	(0.138)	128,939	0.119
Female	0.221	(0.003)	-2.950	(0.086)	-3.713	(0.149)	117,059	0.145

Notes:Reported results are based on regression specification as in Column 4 of Table 3.1. Standard errors, reported in parentheses, are clustered at the origin firm × initial separation quarter. Industry is defined at the one-digit level based on the SBI-2008 Industry classification. Results are reported only for industries with sufficient observations. (Implied) mark-down are calculated as $\frac{\epsilon}{(1+\epsilon)}$ where ϵ is $-2\times$ the separation elasticity.

3.5 Conclusion

There is increasingly more evidence of substantial wage-setting or monopsony power of firms in the labor market. Following the seminal work of Manning (2003), many studies quantify monopsony power by estimating the labor supply elasticities facing individual firms, derived from estimates of the separation elasticity with respect to individuals' own wages. However, these estimates may not reflect the true degree of monopsony power, as individuals' wages vary for reasons unrelated to differences in firm wage-setting policies. In this paper, we use an IV event-study approach developed by Bassier et al. (2022) to quantify the degree of monopsony power in the Dutch labor market. This approach allows us to estimate separation elasticities using only variation in wages driven by firm wage-setting policies.

Using administrative employer-employee match data for the period 2010-2021, we find a separation elasticity of -3.62 in the Dutch labor market, with an implied labor supply elasticity facing the firm of 7.24. This suggest that in the Dutch labor market as a whole, firms could potentially mark down wages relative to workers' marginal productivity by about 12%. Exploring heterogeneity, we find that firms have more wage-setting power at the tails of the wage distribution, although potential markdowns are the greatest among high-wage workers. Moreover, we find that men supply labor more elastically compared to women, so that monopsony power of firms could contribute to the gender wage-gap in the Netherlands.

There are several limitations to our approach. First, our event-study design identifies labor supply elasticities by focusing on 'movers' – i.e. those who switch between firms. These workers may have higher separation elasticities compared to workers who remain at one firm over our sample period. Consequently, our estimates may underestimate the true degree of monopsony power in the Netherlands. Second, we do not explicitly disentangle the potential sources of monopsony power or delve into the mechanisms that could drive the differences labor supply elasticities between subgroups (e.g., men and women). This will be an important avenue for future research. Third, while the labor supply elasticities we identify measure the degree of monopsony power and the potential of firms to mark down wages, we do not examine the degree to which firms actually make use of their monopsony power. Future research could seek to quantify the extent to which firms exercise their monopsony power and investigate the role of labor market institutions as potential barriers.

3.A Summary statistics

Table 3.A.1 Sample selection summary statistics

Selection step	Obs.	Workers	Firms	Firm per worker (mean)	Separation rate (mean)	Hire rate (mean)	E-E Sep. rate (mean)	Quarterly earnings (mean)	Weekly hours worked (mean)	Hourly wage (mean)	Firm size (mean)
Full sample	402,244,651	13,089,569	1,086,136	3.36	0.104	0.106	0.048	6,481	25.87	17.48	4,687
								(6,066)	(13.38)	(42.15)	(12,434)
Hours ¿100	352,207,722	11,946,046	937,083	2.58	0.073	0.076	0.040	7,322	28.95	18.38	4,453
								(6,013)	(11.25)	(15.65)	(12,069)
Hourly wage ¿ 2	350,928,537	11,931,893	934,097	2.58	0.073	0.076	0.040	7,339	28.97	18.41	4,457
								(6,013)	(11.23)	(15.64)	(12,074)
Spell length ¿ 3 qrt	337,716,571	10,807,698	734,177	2.16	0.054	0.056	0.031	7,493	29.38	18.61	4,437
								(6,010)	(11.05)	(15.63)	(11,998)
Worker panel	326,007,405	10,807,698	713,256	2.13	0.042	0.052	0.021	7,661	29.94	18.73	4,430
								(6,027)	(10.71)	(15.69)	(11,969)
Excl. Publ. Adm.	301,558,550	10,321,133	656,941	2.13	0.043	0.054	0.022	7,480	29.68	18.39	3,164
								(6,113)	(10.89)	(15.98)	(9,379)
Firm size > 5	265,965,913	9,543,298	125,336	1.96	0.041	0.051	0.021	7,674	29.89	18.76	3,577
								(6,155)	(10.75)	(15.61)	(9,912)

Notes:Summary statistics for each step of the sample selection procedure are shown. The first row shows summary statistics for the full (unprocessed) sample. We select the sample as follows: We omit entire employment spells with fewer than 100 hours worked per quarter on average over the spell, hourly wages less than two euros in any quarter, employment spells shorter than three quarters. Then the data are converted to a worker-level quarterly panel retaining the employment spell at the dominant employer each quarter. In this sample, we exclude public administration sector focusing on private sector employment and we omit small firms with less than five employees in any year.

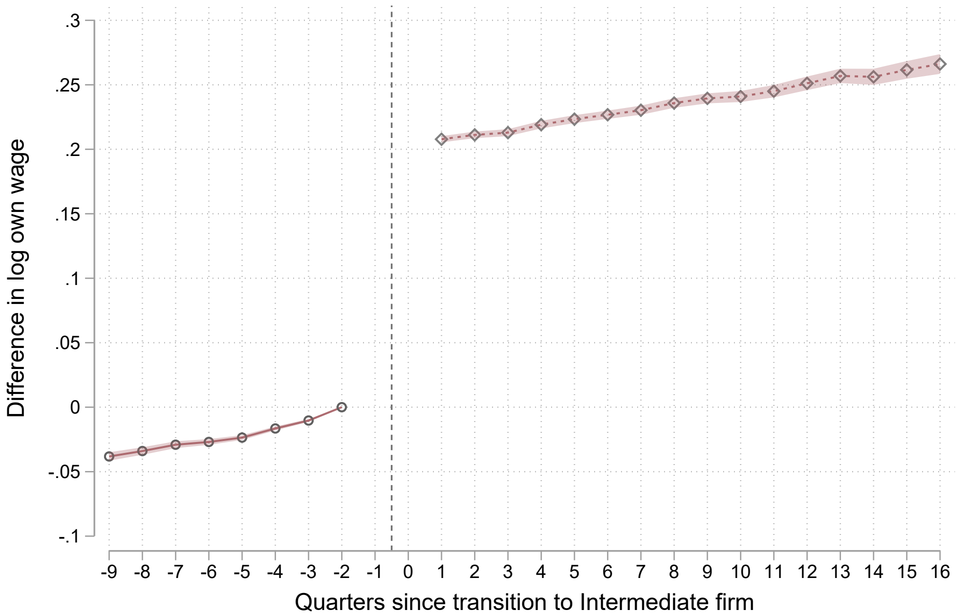
Table 3.A.2 Summary statistics for the matched event study panel

	O-I Moves (Total)	Individuals (Total)	Firms (Total)	Events (Total)	Nr workers per event (Mean)	Hourly wage (Mean)	Firm size Mean	Nr workers per cell (Mean)	Age (Mean)	Tenure (Mean)	Δ (log) own wage (Mean)	Δ (log) co-worker wage (Mean)
Panel A												
Origin	1,390,162	1,182,318	47,425	209,465	109	17.49	3,880	108.957	37.04	9.94	0.063	0.042
					(356)	(8.31)	(9,883)	(356.075)	(10.57)	(6.43)	(0.284)	(0.173)
Intermediate	1,390,162	1,182,318	74,291			18.23	2,469					
						(8.61)	(7,533)					
Final	585,980	449,257	37,161			18.37	3,385					
						(8.59)	(9,929)					
Panel B												
Origin	627,492	569,559	20,095	73,159	206	16.09	6,846	26.51	35.80	8.75	0.106	0.047
					(501)	(8.01)	(13,177)	(70.06)	(10.56)	(5.86)	(0.290)	(0.175)
Intermediate	627,492	569,559	54,762			16.84	2,724					
						(8.25)	(7,885)					
Final	276029	236,027	28,644			17.14	4,014					
						(8.28)	(10,950)					
Panel C												
Origin	289,821	277,567	12,461	39,558	188	16.97	8,923	8.84	36.62	9.49	0.110	0.049
					(432)	(9.75)	(15,107)	(17.67)	(10.71)	(5.72)	(0.291)	(0.171)
Intermediate	289,821	277,567	39,098			17.71	2,590					
						(9.87)	(7,482)					
Final	123,587	115,189	19,570			17.74	4,139					
						(9.66)	(11,144)					
Panel D												
Origin	159,878	156,071	7,558	22,096	216	16.72	11,985	4.31	35.91	9.26	0.127	0.054
					(450)	(10.06)	(17,627)	(4.91)	(11.14)	(5.64)	(0.295)	(0.173)
Intermediate	159,878	156,071	28,762			17.49	2,491					
						(10.07)	(7,104)					
Final	68,751	66,123	14,493			17.55	4,286					
						(9.76)	(11,288)					

Notes:All employment-employment transitions in the main worker-quarter panel are identified, an event-window is isolated (8 pre-separation and 16-post separation), and are stacked. "Events" refers to origin firm by calendar quarter cells within which workers are compared. Each panel refers to the estimation sample based on different interacted controls. Panel A includes interacted controls for origin firm and calendar quarter (i.e. event). Panel B by additionally includes controls for octiles of hiring wage and tenure (at the origin firm). Panel C additionally includes controls for octiles of three quarter pre-separation wages (baseline specification). Panel D further includes controls for age at the initial O-I transition and job characteristics in the pre-event quarter.

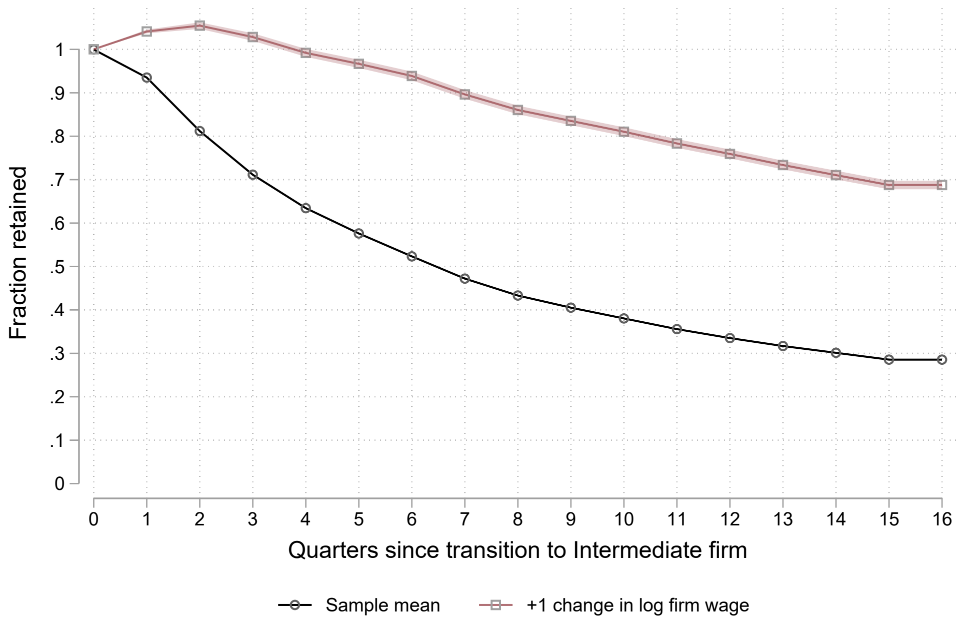
3.B Event study with different worker history controls

Figure 3.B.1 First stage regression of individual wage change on firm average wage difference without controlling for pre-separation wages



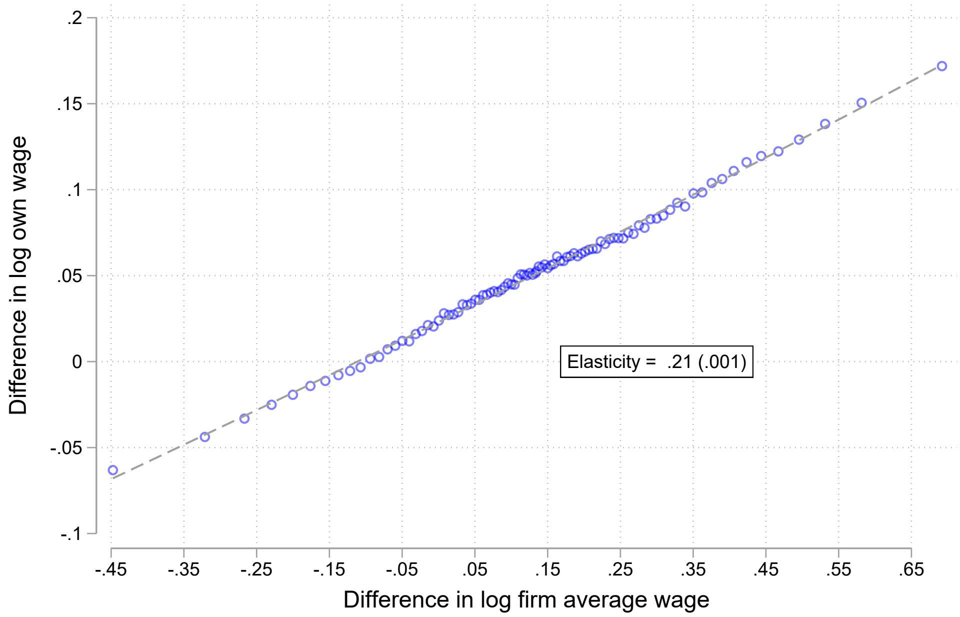
Notes: Figure shows the coefficients from the first-stage regression (see equation 3.3.7) of individual wage changes on the difference in firm average wages. The coefficients are plotted separately for each event-time quarter $k \in \{-9, .., 16\}$. The difference in firm average wages $(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1})$ refers to the change in firm average wages for individual i who moves from the origin firm in $t-1$ to the intermediate firm in t. Controls for worker history $L(History_{i,t,d})$ correspond to Column 3 of Table 3.1. The shaded area shows the 95% confidence intervals. Robust standard errors are cluster at the origin firm level by time.

Figure 3.B.2 Retention probability for workers at higher-wage firms without controlling for pre-separation wages



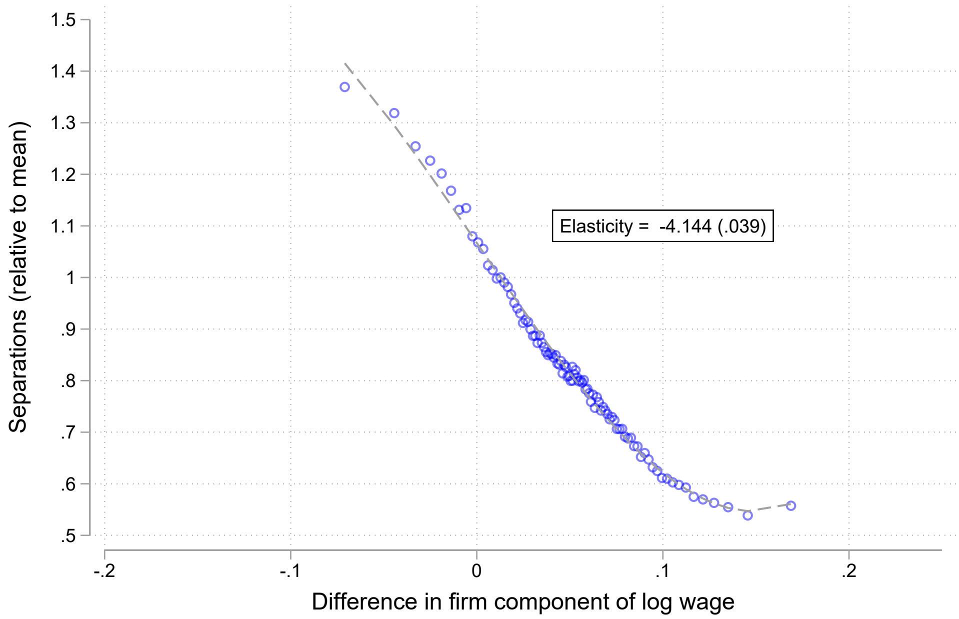
Notes: Figure shows coefficients from a regression of the retention probability of workers at the intermediate firm $R_{i,t+k}$ on $\delta(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1})$. The retention probability at the intermediate firm is shown separately for the first 16 quarters after the $O - I$ transition, hence for each event-time period $k \in \{1, 16\}$. Controls for worker history $L(History_{i,t,d})$ correspond to Column 3 of Table 3.1. The shaded area shows the 95% confidence intervals. Robust standard errors are cluster at the origin firm level by time.

Figure 3.B.3 Binned scatterplot first stage without controlling for pre-separation wages



Notes: The figure shows the first-stage relationship between $\Delta ln(w_{i,t+1})$ and $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 3 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

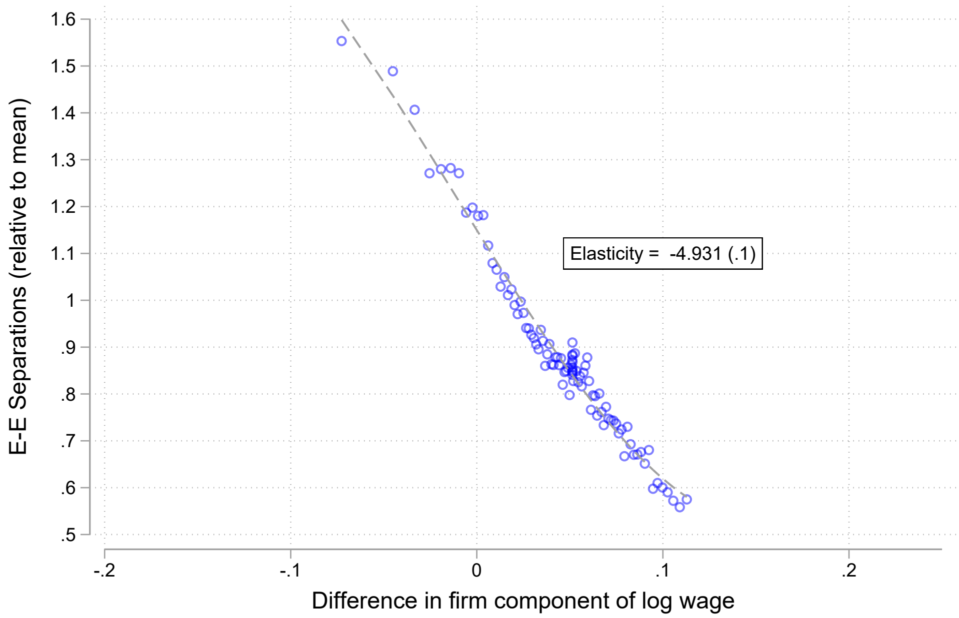
Figure 3.B.4 Binned scatterplot separation response without controlling for pre-separation wages



Notes: The figure shows the relationship between separations and $\Delta ln(w_{i,t+1})$, instrumented by $\Delta ln(\bar{w}_{i,I(i),t})$ using a control function: controlling for the residuals from a regression of $\Delta ln(w_{i,t+1})$ on $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 3 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

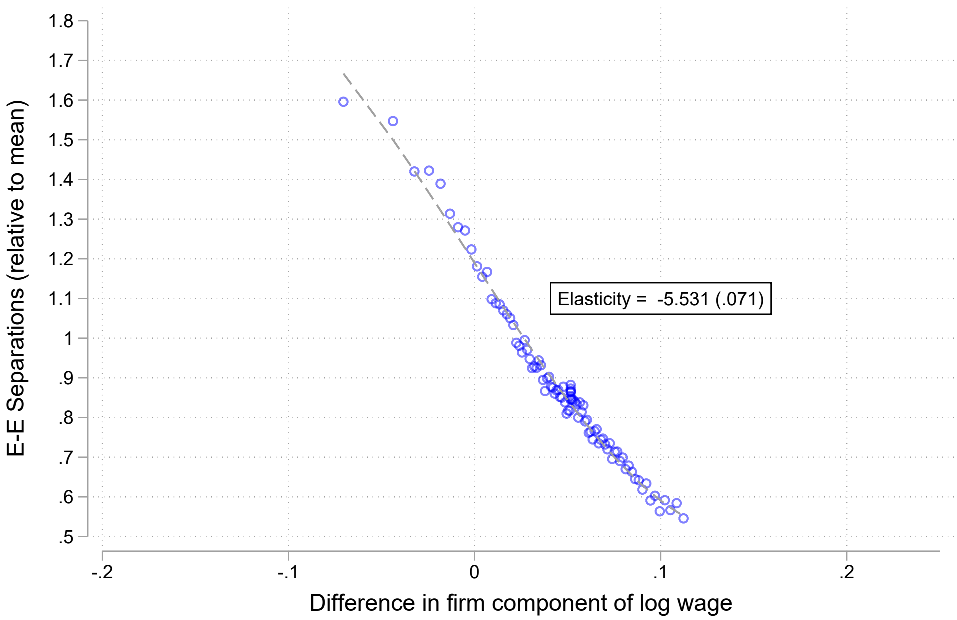
3.C Binned scatterplots for employment-to-employment separations

Figure 3.C.1 Binned scatterplot for employment-to-employment separations controlling for pre-separation wage



Notes: The figure shows the relationship between employment-to-employment separations and $\Delta ln(w_{i,t+1})$, instrumented by $\Delta ln(\bar{w}_{i,I(i),t})$ using a control function: controlling for the residuals from a regression of $\Delta ln(w_{i,t+1})$ on $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 4 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

Figure 3.C.2 Binned scatterplot for employment-to-employment separations without controlling for pre-separation wage



Notes: The figure shows the relationship between employment-to-employment separations and $\Delta ln(w_{i,t+1})$, instrumented by $\Delta ln(\bar{w}_{i,I(i),t})$ using a control function: controlling for the residuals from a regression of $\Delta ln(w_{i,t+1})$ on $\Delta ln(\bar{w}_{i,I(i),t})$. Controls for $L(History_{i,t,d})$ are from the specification in Column 3 of table 3.1. The first 16 quarters after the move from the origin to the intermediate firm are considered.

CHAPTER 4

New home, old neighbors? Ethnic enclaves and refugees' education outcomes

4.1 Introduction

Many European countries have experienced a large inflow of asylum seekers over the past decades. Between 2012 and 2019 alone, over 2.1 million asylum seekers were granted a protective status in EU member states (Eurostat, 2022). Since these refugees often face steep barriers to economic and civic integration (Brell et al., 2020; Frattini et al., 2020), understanding the factors that may facilitate or impede integration into the host society is of great importance: both from a policy and scientific perspective.

Newly arrived refugees, and immigrants more broadly, tend to settle in urban areas with a relatively high concentration of other immigrants, particularly migrants with a similar ethnic background (Åslund, 2005; Damm, 2009a; Liebig and Spielvogel, 2021), resulting in the formation of 'ethnic enclaves'. Such enclaves are commonly believed to hamper the assimilation process of refugees and migrants more broadly.

The concern that residential segregation into ethnic enclaves is harmful to refugees' long-term integration has led several countries to adopt dispersal policies that restrict the (initial) residential choice of newly arrived refugees by centrally allocating refugees to specific areas.[1] A growing body of empirical evidence, however, shows that living in an (established) ethnic enclave could facilitate the (short-term) labor market integration among migrants arriving as adults (e.g Edin et al., 2003; Damm, 2009b; Martén et al., 2019; Battisti et al., 2022). Yet, empirical evidence on the effect of ethnic enclaves on the education outcomes of refugees who

[1] For example, variants of dispersal policies are used in Denmark, Sweden, Germany, the Netherlands, and Switzerland.

arrive as children is relatively scarce. This question is of particular importance, as the educational achievements of immigrant children are key determinants for their future labor market prospects and social integration and are therefore central to the long-term integration into the host country.

In this paper, I study whether the earnings and the concentration of individuals with a shared ethnic background in the neighborhood of residence – henceforth ethnic enclaves – affect the school performance of refugee children. Identifying the causal effect of ethnic enclaves is challenging for two key reasons. First, individuals might self-select into ethnic enclaves based on (unobservable) characteristics. If these characteristics are systematically related to education outcomes such endogenous residential sorting would confound estimates of the effect of ethnic enclaves. Second, ethnic concentration could be correlated with (unobserved) local characteristics common to all residents, irrespective of ethnic affiliation (e.g., local labor market conditions or school quality). Such 'correlated effects' raise the possibility of falsely attributing the influence of the characteristics of the common environment to the presence of individuals with a shared ethnic background (Manski, 1993).

I address these issues by exploiting a refugee dispersal policy in the Netherlands from 1999 until 2009, which quasi-randomly allocates refugees to their initial address. As refugees could not choose their first place of residence, the policy generates plausibly exogenous variation in the initial residential composition of refugees' assigned neighborhoods. Moreover, the sustained implementation period of the policy, the large number of origin countries, and neighborhoods of assignment induce variation in ethnic composition between ethnic groups within neighborhoods. This allows us to isolate the impact of ethnic enclaves on education outcomes from correlated local characteristics by relying solely on within neighborhood and between ethnic group variation.

I show that being assigned to a neighborhood with a higher initial share of co-ethnic neighbors affects the school performance of refugee children who arrive in the Netherlands before the age of 14, and that the effect of neighborhood co-ethnic concentration is moderated by the earnings of co-ethnic network members, especially for later-age education outcomes. At relatively low levels of neighborhood co-ethnic earnings, assignment to a neighborhood with a higher initial share of co-ethnics lowers the probability of obtaining an academic secondary education track diploma by age 19 and higher education enrollment by age 21. At relatively high levels of neighborhood co-ethnic earnings, I find a modest positive effect of neighborhood co-ethnic concentration. Higher co-ethnic earnings have a positive impact on both primary school outcomes and later education outcomes, and the positive effect is amplified when co-ethnic neighbors comprise a larger share of the neighborhood population.

This paper relates to several strands of literature. First, I contribute to the literature on the causal link between the neighborhood that children are exposed to during childhood and outcomes later in life, such as educational attainment and earnings (e.g., Chetty et al., 2016a; Chetty and Hendren, 2018a,c; Deutscher, 2020; Laliberté, 2021), by studying whether particular types of neighborhood characteristics – the concentration and earnings of individuals from one's own ethnic background – affect the education outcomes of refugee children. Neighborhoods act as central places for the formation of social networks, which are an important source of norms and aspirations, role models, and facilitate information transmission, which in turn may affect human capital formation (Coleman, 1988; Portes, 1998). As individuals tend to form social networks with those similar in demographic, socioeconomic and cultural dimensions such as gender, age, ethnicity and religion (see McPherson et al., 2001), the spatial concentration of individuals with a common ethnic background may facilitate the formation of ethnic networks.[2] In the presence of ethnically stratified social networks, individuals who grow up in the same locality are exposed to a potentially different set of values, social contacts, and resources (i.e. ethnic capital) depending on their ethnic affiliation Borjas (1992, 1995). If social interactions within ethnic groups are more frequent or more influential than social interactions between ethnic groups, the characteristics of the co-ethnic network can have an effect on children's human capital accumulation, above and beyond neighborhood effects (i.e. factors common to all individuals in the neighborhood).

There is a growing literature that exploits dispersal policies to identify the causal impact of local ethnic composition on a variety of outcomes among immigrants, such as welfare usage (Åslund and Fredriksson, 2009; Kristiansen et al., 2022), employment and earnings (e.g., Edin et al., 2003; Damm, 2009b, 2014; Beaman, 2012; Martén et al., 2019; Battisti et al., 2022), self-employment (Dagnelie et al., 2019), host-country language acquisition (Høholt Jensen et al., 2022; Danzer and Yaman, 2016), and residential mobility (Damm, 2009a). However, this literature has primarily focused on migrants who arrive in the host-country as adults, while the literature focusing on immigrant children is relatively scarce. I contribute to this literature by exploiting a refugee dispersal policy to identify the causal effect of neighborhood co-ethnic concentration and co-ethnic earnings on the education outcomes of refugee children.

The literature focusing on adult migrants generally finds that larger and economically established co-ethnic networks exert a positive impact on adult migrants' labor market integration. These studies emphasize the importance of co-ethnic networks in the dissemination of job-relevant information and network-based job referrals (e.g., Damm, 2009b; Beaman, 2012; Battisti et al., 2022; Egger et al., 2022), and hiring by co-ethnic business owners (Dagnelie et al., 2019). However, these mechanisms may not directly translate to the impact on the education outcomes of their children. While larger co-ethnic networks have a positive impact on early labor market entry, they could also reduce (parental) investment in formal human capital accumulation (e.g Battisti et al., 2022) and host-country language proficiency (Lazear, 1999; Danzer and Yaman, 2016; Laliberté, 2019; Høholt Jensen et al., 2022). Parental human capital and host-country language proficiency can have important intergenerational spillovers on the education outcomes of their children

[2]The tendency of intra-ethnic social network formation has been extensively shown in the context of friendship formation in schools (e.g Baerveldt et al., 2007; Currarini et al., 2009, 2010; Kruse et al., 2016; Smith et al., 2016).

(Foged et al., 2023). While parental labor market integration might have a positive impact on the education outcomes of their children (e.g., due to increased resource availability), lower parental host-country language proficiency and reduced formal human capital accumulation could have an adverse effect. Moreover, while individuals in the co-ethnic network may be an important source of information relevant to integration in the labor market, information relevant to human capital accumulation of immigrant children (e.g., the workings of the education system, knowledge of school quality) might not disseminate in a similar fashion or might not be present within the network. In addition, if the labor market integration of parents occurs within segregated labor markets (e.g., in ethnically homogeneous firms) this might limit social integration. Which of these effects dominate is ultimately an empirical question.

I contribute to the sparse literature studying the link between ethnic enclaves and the human capital acquisition of immigrant children. Most previous studies do not explicitly account for potential endogenous sorting into ethnic enclaves, and have focused on children with a migration background, irrespective of their (parents) reason for migrating (e.g., Grönqvist, 2006; Bygren and Szulkin, 2010; Fleischmann et al., 2013; Hermansen, 2023). Moreover, studies that address the issue of endogenous sorting measure ethnic enclaves at larger levels of geographical aggregation, which may be too large to adequately capture the social network in which individuals are embedded (e.g., Chakraborty et al., 2019; Danzer et al., 2022). I contribute to this literature by using an explicit identification strategy, giving the estimates a causal interpretation, and by focusing on a specific type of migrants – refugees. In addition, I measure local characteristics at the neighborhood level, which better captures the social environment in which children are embedded.

Most closely related to this study is Åslund et al. (2011), who exploit a Swedish refugee dispersal policy between 1987-1991 to study the causal effect of co-ethnic enclave size and the share of high-educated co-ethnics in the initial neighborhood of residence on refugee children's education outcomes. They find that the ethnic human capital, as measured by the share of high-educated co-ethnics, in the neighborhood of assignment positively impacts compulsory school GPA at age 16 and the probability of graduating from upper-secondary school by age 19. I contribute to this literature by providing novel evidence for a more recent refugee cohort in the Netherlands. Moreover, I study both early- and later-age education outcomes to investigate whether the impact of ethnic enclaves already materializes at an early age. This is particularly relevant in a setting of early school tracking.

The paper proceeds as follows. In Section 4.2, I discuss the Dutch dispersal policy and other relevant features of the institutional setting. In Section 4.3, I describe the data used for the empirical analysis. I outline the empirical approach in Section 4.4. Section 4.5 reports the main results and Section 4.6 concludes.

4.2 Institutional Setting

To identify the causal effect of neighborhood ethnic composition on refugee children's education outcomes, I use a special feature of the Dutch asylum context: the random allocation of refugees to housing throughout the country. This Section provides a description of the Dutch context with regards to asylum and education. The focus is on the period 1999-2009, since this is the time frame the research population received a temporary permit to stay in the Netherlands and was housed accordingly.

4.2.1 Dutch asylum policies

Refugees are migrants who have received a temporary permit that permits them to stay in the Netherlands and recognizes their claim for asylum. Before receiving this permit, these migrants are called asylum seekers. In order for them to receive a temporary permit, they have to go through several steps in the Dutch asylum procedure.

Dutch Asylum Procedure

Upon arrival in the Netherlands, asylum seekers have to report to an application center ('*aanmeldcentrum*', henceforth, AC) and state the type of protection they need. Next, the identity of the asylum seekers is checked through their official documents and fingerprints. The identification and registration process is done by the Dutch Immigration and Naturalisation Service and the Aliens Police.

Once this identification and registration process is concluded and if there are no issues regarding national security or active applications in other EU-countries, asylum seekers are admitted to the regular asylum procedure. During this procedure, asylum seekers receive housing and weekly stipends to pay for basic needs. Housing is provided by the Central Agency for the Reception of Asylum Seekers (*Centraal Orgaan opvang asielzoekers*, henceforth–COA). The COA allocates asylum seekers to housing facilities. There are different types of housing facilities. Besides collective asylum seeker reception centers (AZCs), there are alternative housing options when the AZCs are filled to capacity. Asylum seekers can be (temporarily) housed in hotels, pensions or campings; COA-administered social housing units in the municipalities; or make use of a self-care arrangement, where asylum seekers can voluntarily take up residency with friends or relatives and report back at AZCs at regular intervals. In principle, individual preferences are not taken into account and allocation is determined by the phase of the asylum procedure and the availability of housing (Arnoldus et al., 2003; Achard, 2022).

Once the asylum seeker is granted residency, they receive the refugee status and is moved from an AZC to an independent housing facility.[3] This move to

[3]During the sample period, various types of residency permits were granted. These permits vary in duration, access to the labor market and social security, possibility of family-reunification, and integration requirements. See Appendix 4.J for more details.

independent housing is governed by a dispersal policy that aims to evenly spread refugees across the Netherlands. This dispersal policy is crucial for the identification approach and is outlined next.

Dutch Dispersal Policy

Once asylum seekers have been granted a (temporary) residency permit, they obtain a refugee-status and are housed in regular housing in municipalities. In most cases, refugees reside in social housing units owned by housing associations. While the accommodation of asylum seekers in collective reception facilities is the responsibility of the COA, municipalities are responsible for accommodating refugees.

By law, Dutch municipalities have to provide housing to refugees in accordance with a biannually determined municipal target. Every six months, municipalities receive a quota for the number of refugees they have to accommodate. The quota is based on the projected number of refugees due for placement and is directly proportional to the share of the Dutch population residing in a given municipality.

In principle, refugees are allowed to look for housing themselves. However, the vast majority of refugees are assigned their initial place of residence outside COA-administered housing, while around 30% find housing by themselves (Arnoldus et al., 2003; Klaver and van der Welle, 2009). Municipalities reserve part of their social housing stock and register the reserved social housing units with the COA. Assignment of refugees to the registered social housing units is done centrally by the COA, according to a first-in-first-out principle based on the time individuals have spent in the collective accommodations and suitable housing availability.[4] The COA seeks to take the type of housing unit (e.g., no single person in family housing unit), the rental price, and the municipal household composition into account when matching refugees to housing units.[5]

When assigning refugees to regular housing, individuals' preferences are not taken into account (Arnoldus et al., 2003; Kristiansen et al., 2022). However, the COA considers four 'hard' placement criteria; (i) the presence of close family members (i.e. spouses, parents, children) already residing in the Netherlands, (ii) the requirement of medical treatment, (iii) enrollment in an educational institution, and (iv) existence of pre-existing employment relations.[6] In cases where these criteria apply, the COA seeks to assign refugees to municipalities within a 50-kilometer radius of the respective family members, hospital, educational institute or workplace.

Refugees only receive one housing offer, which they cannot reject unless the rejection is deemed valid with respect to the aforementioned placement criteria. This means that there is no bargaining over housing offers. Refugees who reject

[4]The order of placement is determined by the time a refugee registered at the AC, not the moment a permit has been granted.

[5]When assigning singles to housing with pre-existing households, the COA takes into account gender, nationality, and religion.

[6]Criteria (ii) only applies when medical treatment can only be offered in a specific hospital. Criteria (iii) only applies when a particular education is only offered in a specific municipality. Criteria (iv) only applies when individuals can support 50% of livelihood.

the housing offer lose their right to accommodation by the COA, making declining housing offers costly.

Throughout the years, the Netherlands has struggled with providing enough social housing in general. Since refugees typically receive social housing accommodation after receiving their permit, this social housing shortage affected the dispersal policy process, making it harder to move refugees from COA housing to regular housing in a timely fashion. The size of the social housing units also plays a role, since most consist of units suitable for families. This makes the placement of single-person households particularly challenging, resulting in longer waiting periods.

Dutch Education System

The Netherlands has an early tracking schooling system with a compulsory school age from 5 to 16, with tracking taking place at age 12.[7] Dutch primary school consists of 8 eight grades.[8] In grade 8, usually around the age of 12, most students take a primary school exit test. This test evaluates the students' language and math skills.[9] Test results, in combination with a secondary school track recommendation by their teacher, determines the track that children attend in secondary school.[10] The teacher recommendation is a subjective evaluation of student ability, reflecting the teacher's expectations of the student's future academic achievement.

Three main secondary education tracks prepare children for subsequent education trajectories. The pre-vocational secondary education track takes four years and gives access to vocational programs. This track is subdivided into different levels, with a different emphasis on vocational- and theoretical education. The higher general secondary education takes five years and gives access to professional colleges. The pre-university track takes six years and gives access to research universities.[11]

The secondary school track recommended by the teacher and the test score does not have to be the track that the student graduates in. Some schools offer classes combining students from adjacent tracks for the first one or two years of secondary school (Oosterbeek et al., 2021; Dillingh et al., 2022). Students in these classes can enter another track in the second or third year of secondary school than the one initially assigned to them. Students who do not perform well may move to a lower

[7]Children generally start primary school around the age of 4. When students do not have a secondary vocational, a secondary general or a pre-university degree before the age of 16, they have to stay in school until the age of 18.

[8]The first two grades can be regarded as kindergarten.

[9]There are several exit tests available, of which the Central End Test (CET) is most commonly used.

[10]Prior to the academic year 2014/2015, the scores for the exit test and the school-advice jointly determined the secondary education track. Since the academic year 2014/2015, the school-advice has become the decisive factor.

[11]Besides the three main secondary education levels, the Netherlands also has a special education and a practical secondary education level. These two types of education levels are designed for students with learning- and behavioral difficulties. Due to the distinct learning abilities of the students that enroll in special and practical education, often they do not partake in the school exit test and/or have been enrolled at a special primary school (Dillingh et al., 2022).

track. After receiving a secondary school diploma, students can 'stack' degrees by enrolling at the final years of the adjacent higher track.[12]

4.3 Data and Sample Selection

Data. I use rich administrative data, collected by Statistics Netherlands. The data contain information on demographic characteristics, including date of birth, sex, and country of birth, covering all individuals in Dutch municipal registers between 1995 and 2021. The data further contain longitudinal records on the place of residence and household arrangements. This allows us to determine an individual's exact address of residence, as well as the composition of the household they belong to, at any point in time. For individuals born abroad, I have information on their migration motive (e.g., family reunification or formation, asylum, study).

I use information on all COA-administered housing over 1995–2012 to identify individuals residing in collective reception centers and to determine the date at which individuals move to regular housing. COA-administered housing data contain information on the address, exact opening and closing dates and type of housing facility.[13]

I consider education outcomes at three stages of children's education career. For primary school outcomes, I observe the secondary school track recommendation by the teacher. I construct a binary indicator whether a child received a higher general secondary education or pre-university track recommendation — henceforth academic track. For later education outcomes, I observe the highest track diploma obtained by age 19 and higher education (professional college or university) enrollment by the age 21. I construct a binary indicator whether a child obtained an academic track diploma by age 19 and a binary indicator whether a child enrolled in higher education by age 21.[14]

Sample Selection. I restrict the sample to migrants originating from the 35 largest refugee-sending countries to the Netherlands (see Appendix 4.G) who migrated for asylum or family–reunification reasons, and first registered in the Netherlands between 1999 and 2009.[15] It is critical for the identification strategy that refugees

[12]Appendix 4.I provides additional information on education for asylum seekers.

[13]These include collective receptions centers, as well as the addresses of individuals making use of alternative housing arrangements.

[14]Not all children obtain a secondary school diploma by the time they are 19 years old: not all individuals graduate from secondary school by age 19, some children discontinue their education after they have reached the compulsory school age of 18, or children may enter vocational tertiary education without obtaining a secondary education diploma.

[15]Asylum seekers are not directly registered in the municipal registers upon arrival in the Netherlands. Only those residing in the collective reception centers for a prolonged period of time, those whose asylum claim has been granted, or those who have taken up residency outside of the collective reception centers, appear in the municipal registers. The latter could occur when individuals have obtained refugee status and have moved to regular social housing or when individuals make use of COA-administrated housing outside of the collective reception centers. Prior to 2000, the COA registered individuals to the municipality after being hosted for a year. This period was reduced to six months from 2000 onward. Children who are born in the Netherlands

were subject to the Dutch dispersal policy and did not use pre-existing networks to influence their allocation to regular housing. For this reason, I limit our sample to individuals whose first registered address is a collective reception centre and who did not make use of alternative housing arrangement before moving to regular housing.[16] I further exclude individuals with previously arrived family member already residing in regular housing.[17] I define the assigned address as the first address outside COA-administered housing. I extend the initial sample by including family members who registered at the placement address within a year of the earliest placed family member. From this sample, I extract a sample of children for the main analysis. I restrict the sample to children aged 14 or younger at the moment of arrival. This ensures that the children in the sample spent at least two years in the Dutch education system. Not all education outcomes of interest are observed for every child, resulting in two partially overlapping samples: one sample with children for whom I observe primary school outcomes and one sample of children for whom I observe education outcomes at ages 19 and 21.[18]

Variable definitions. I define co-ethnics as all individuals with at least one common language and belonging to the same ethnolinguistic cluster of countries – henceforth language-region fellows. Following Høholt Jensen et al. (2022), I base common languages on the official language(s) of individuals' origin country (UN, 2017). Ethnolinguistic clusters are groups of countries clustered based on their region and cultural distance to the Netherlands (see Jennissen et al., 2021, for a more detailed description). An individual's country of origin is based on their own country of birth, as well as the country of birth of their parents. Those not born in the Netherlands are assigned their country of birth as their country of origin. Individuals born in the Netherlands to two foreign-born parents are assigned their maternal country of birth.[19] In case only one parent is born outside of the Netherlands, I assign the country of birth of the foreign-born parent as the country of origin. Note that this definition of shared ethnicity includes both foreign-born individuals (i.e. first-generation migrants) and their descendants (i.e. second-generation migrants). Results are robust to using a more narrow definition of of ethnicity, most commonly adopted in the literature, based on country of origin – henceforth co-nationals.

I measure local (ethnic) characteristics at the neighborhood level. Municipalities in the Netherlands are subdivided into one or more neighborhoods. These neighborhoods can be subdivided into one or more smaller micro-neighborhoods.

are registered in the municipal registers, even if their parents have not been registered yet.

[16]Since I restrict the sample to those whose first registered address is a COA-location, I exclude those refugees who resided in the collective reception centers for a relatively short time prior to moving to regular housing, as well as those who never resided at a COA-location. This groups accounts for sizable 20% of all individuals who first registered in the municipal registers with a recorded asylum migration motive between 1999 and 2009.

[17]These include parents, children, grandparents and siblings. Note that I can only identify siblings when at least one parent is registered in the municipal registers.

[18]See Appendix 4.A for a more details on the sample selection procedure.

[19]This is the approach taken by Statistics Netherlands. The reason for assigning the maternal, rather than the paternal, country of birth stems from the fact that information on maternal country of birth is more often available.

The micro-neighborhoods comprise a cluster of buildings, akin to a few blocks in US cities, with a relatively homogeneous function (e.g., residential, business, recreation) or rural areas. The subdivision of (micro) neighborhoods takes into consideration physical boundaries such a major roads and rivers.[20] As a robustness check, I measure local characteristics at the municipality level.

I measure co-ethnic concentration as the number of individuals with a shared ethnic background in the neighborhood, relative to the total neighborhood population. As discussed in Section 4.1, while co-ethnic concentration itself may facilitate the formation of co-ethnic networks, the characteristics of the ethnic network — i.e. ethnic capital - could drive the potential effect of ethnic enclaves on education outcomes. I proxy for co-ethnic capital using the (log) mean daily earnings of co-ethnics aged 25-65 in the neighborhood.[21] Similar measures have been used in the context of adult migrant labor market integration (e.g Damm, 2014; Edin et al., 2003; Damm, 2009b).

4.3.1 Descriptive statistics

Table 4.3.1 reports descriptive statistics for the refugee children (Panel A) and neighborhood characteristics (Panel B) across the two estimation samples.

Education outcomes. There is a sizable school performance gap between refugees and native children. Around 30% of refugees receive an academic recommendation from their teacher (Column 1), while 47% of natives who receive such a recommendation. About 24% of children obtain an academic track diploma by age 19 (Column 2), compared to 42% among natives. Note that not all individuals obtain a secondary education diploma by age 19, among those that do, 34% of children obtain an academic track diploma, while 49% of native children do so. About 42% of refugee children enroll in higher education by the age of 21, while the corresponding share among natives is 52%. The large discrepancy between the share of individuals who obtain an academic track diploma by age 19 and share of individuals who enroll in higher education by age 21 is partially attributable to alternative education trajectories where individuals first obtain a vocational degree before continuing in higher education.

Individual and household characteristics. Children in the sample typically arrive in the Netherlands well before the age of 12, at which children generally

[20]The subdivision of geographical units is subject to change over time (e.g., due to shifts of physical boundaries, the construction of demolishing of buildings, change in primary function, or re-classifications of municipalities). To ensure that I can track various local characteristics over time, I re-code the 'old' area division into the 2021 division, corresponding to 352 municipalities, 3,248 neighborhoods, and 14,080 micro-neighborhoods.

[21]Previous studies focusing on the effects of ethnic capital on education outcomes have used the educational attainment of adult co-ethnics as a measure of ethnic capital (e.g., Åslund et al., 2011; Borjas, 1995; Chakraborty et al., 2019). The (distribution of) education attained by co-ethnic in the locality arguably captures several dimensions of ethnic capital that could affect education outcomes (e.g., educational resources, norms and aspirations emphasizing education, and role models). Unfortunately, data on educational attainment among adults is only sparsely available.

enter secondary school. This means that most children spent a few years in Dutch primary schools. The average age at arrival is around 3 for those in the primary school sample (Column 1) and around 37% are born in the Netherlands. For those in the later education outcomes sample (Column 2) the average age at arrival is around 7 and 4% of children are born in the Netherlands.

Children spent a considerable amount of time in the collective reception centers prior to placement in regular housing, 565–628 days on average.[22]

The vast majority on children are placed simultaneously with their parent(s). Only 4-11% join previously placed household members within a year of initial placement. At the time of placement, 25% of children belong to single parent households. The vast majority of children born abroad migrated to the Netherlands with an asylum motive (81–89%). Individuals who migrated to be reunited with previously arrive family members account for 8–16%.

Most children come from families with a poor education background. Only 10-13% of mothers and 18-21% of fathers have at least a college degree.[23]

Origin countries. There is clear variation in the language-region and countries of origin (see Table 4.B.1 in Appendix 4.B). Children originating from Central-Asia with Farsi as their official language (e.g., Iran and Afghanistan) comprise the largest ethnic group, 24-38%, followed by Arabic speaking individuals from Arab countries (e.g., Iraq, Syria, Sudan) and Sub-Sahara Africa (e.g., Somalia), and individuals from Mid- and Eastern-Europa with a Serbo-Croatian language (e.g., from Bosnia and Serbia).

Assigned neighborhood characteristics. The concentration of co-ethnics in the assigned neighborhood is low. Children are assigned to neighborhood with around 13,500 residents on average. The average number of co-ethnics in the assigned neighborhood is 111-125, accounting for only 0.62-0.72% of the total neighborhood population. About 10% of children have no co-ethnic neighbors in the assigned neighborhood at the time of placement. The co-ethnic share is lower at the municipality level (see Table 4.B.2 in Appendix 4.B), consistent with residential sorting along ethnicity within larger geographical areas. The average daily earnings of adult co-ethnics is considerably lower compared to those of the broader neighborhood migrant- and overall population.

[22] Note that I do not observe the actual time of arrival in the reception centers but only the first moment of registration in the municipal registers. Consequently, the time spent in the reception centers reported in Table 4.3.1 serves as a lower bound.

[23] Information on parental educational attainment is available for around 30% of children in the samples.

Table 4.3.1 Summary statistics

	Sample	
	Primary school outcome	Later education outcomes
	A. Individual and household characteristics	
Education outcomes		
Academic track recommendation	0.30	
Academic track diploma by age 19		0.24
Higher education enrollment by age 21		0.43
Individual Characteristics		
Age at arrival	2.82	7.17
	(2.92)	(3.71)
Age at placement	4.50	8.80
	(3.30)	(3.61)
Female	0.49	0.46
Born in NED	0.38	0.04
Days registered in coll. recep. centres	628.01	565.55
	(752.33)	(692.57)
Parental education		
Mother's Education		
Missing	0.32	0.28
<College/University	0.90	0.87
College/University	0.10	0.13
Father's Education		
Missing	0.27	0.26
<College/University	0.82	0.79
College/University	0.18	0.21
Household characteristics		
Couple with children	0.75	0.75
Single parent	0.25	0.25
Household size	4.42	4.78
	(1.39)	(1.43)
Parent(s) empl. prior to placement	0.20	0.18
Migration motive		
Asylum	0.81	0.89
Family reunification	0.16	0.08
Unkown	0.04	0.03
	B. Assigned neighborhood characteristics	
Number of co-ethnic neighbors	124.49	111.58
	(496.88)	(473.39)
Co-ethnics share (%)	0.72	0.62
	(1.72)	(1.41)
Ln(avg. co-ethnic daily earnings)	4.16	4.14
	(0.40)	(0.40)
Number of residents	13,552	13,508
	(12,594)	(12,962)
No co-ethnics	0.10	0.10
Observations	3,987	4,805

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. Individual and household characteristics are measured at the time of placement.

4.4 Empirical Approach

4.4.1 Empirical model

I estimate the impact of neighborhood ethnic concentration and ethnic earnings on the education outcomes of refugee children. To avoid bias arising from endogenous post-assignment residential sorting, I model refugees' education outcomes as a function of the *initial* residential (ethnic) composition of the assigned neighborhood at the time of placement. I estimate the following model:

$$y_{icka} = \alpha_1 ES_{ka} + \alpha_2 EQ_{ka} + \beta X_i + \gamma_a + \gamma_{cr} + \gamma_\tau + \epsilon_{icka}, \tag{4.4.1}$$

where y_{ika} is the education outcome for individual i from origin country c belonging to ethnic group k who was placed in neighborhood a.

the main explanatory variables of interest are the share of co-ethnics (ES_{ka}) and the log mean daily earnings of adult (25-65-year-olds) co-ethnics (EQ_{ka}) in the assigned neighborhood a.[24] I measure assigned neighborhood characteristics at the time of placement τ — i.e the registration date at the first address that is not a collective reception centre. Since ethnic concentration in the sample is low, one might be concerned that the variation used for identification is exaggerated due to measurement error and transitory fluctuations. To address this concern, I average assigned locality characteristics over the twelve months prior to placement.

The parameters of interest are α_1 and α_2, which provide estimates of the intent-to-treat (ITT) effects of the *initial* ethnic concentration and ethnic daily earnings in the assigned neighborhood, a, under the following assumptions: i) assignment to initial addresses is random, conditional on observable personal- and household characteristics known to the COA prior to placement and may therefore have affected assignment, and ii) there are no omitted correlated effects.[25] I limit the potential of bias due to omitted correlated effect by relying solely on *within* neighborhood variation. Specifically, the baseline specification includes neighborhood of assignment fixed effects, which ensures that I account for time-invariant neighborhood characteristics to the extent that they are common across individuals, irrespective of one's ethnic affiliation. The effect of the neighborhood co-ethnic share and co-ethnic earnings is identified from variation between individuals from different ethnic groups assigned to the same neighborhood and from variation between individuals of the same ethnic groups assigned to different neighborhoods.

[24] I apply a $log(1+X)$ transformation to the mean ethnic earnings measure as to avoid having to omit individuals without co-ethnic adults with positive earnings in the neighborhood. I include a dummy which takes on a value of one if no co-ethnics aged 25-65 with positive earnings reside in the neighborhood. Thus, the effect of ethnic earnings is solely identified of variation in ethnic earnings conditional on having at least one 25-65-year-old co-ethnic with positive earnings in the neighborhood.

[25] The research design has an intention-to-treat nature, because individuals are unconstrained in their residential choice after initial assignment and can move away from the assigned locality before their education outcomes are observed. The treatment received — i.e. the cumulative exposure to co-ethnic neighbors until education outcomes are observed, might differ from the treatment assigned — i.e initial exposure to co-ethnic neighbors at the time of assignment.

To account for unobserved heterogeneity between different origin countries and arrival cohorts, as well as between different arrival cohorts from the same origin country constant across assigned neighborhoods, the baseline specification includes origin country c by arrival year r fixed effects, γ_{cr}.[26] These absorb systematic differences in, for example, educational traditions, linguistic distance to the Dutch language, and general integration dispositions, both between origin countries and between arrival cohorts. Additionally, I include year of placement fixed effects, γ_{τ}.

I also control for various personal- and household characteristics measured at the date of placement, included in vector X_i. This controls for age at arrival, mother's/father's age at arrival, household type (single parent, partners with children), household size, number of siblings, migration motive, whether parents were employed prior to placement, and the education level of the highest educated parent.[27] Since time spent in the reception centers could be related to the (initial) type of permit received and also have a direct effect on integration trajectories, I control a categorical variable for the maximum number of years spent in the collective reception centers by any household members and the days registered in reception centers.

4.4.2 Balance tests

The identification strategy critically depends on the random assignment of refugees to their first residence, conditional on the individual- and household characteristics observed by the COA. While this assumption cannot be formally tested, I assess its validity by examining whether initial characteristics of the assigned neighborhood are systematically related to pre-migration parental educational attainment.[28] Parental educational attainment was not explicitly used as part of the assignment mechanism and is strongly related to children's education outcomes. Absence of systematic differences in the characteristics of the assigned neighborhood between individuals with differential (parental) educational attainment would therefore bolster confidence in the assumption that assignment was indeed unaffected by unobservable characteristics.

Using a sample of household heads, I separately regress the following neighborhood characteristics on pre-migration educational attainment: the share of co-ethnics; the share of migrants; the log mean daily earnings of co-ethnics, migrants, and all residents; and the degree of urbanization. I additionally control for individual- and household characteristics at the of placement: age at arrival; household type; household size; days registered in the collective reception centers;

[26]Arrival year r refers to the year of arrival of the earliest arrived household member. As discussed in Section 4.3, I do not have information on the exact arrival date in the Netherlands. I take the date of first registration in the municipal registers as the moment of arrival.

[27]In case the mother/father is not present in the placement household, I assign arbitrary low values to parental characteristics and include an indicator for whether the mother/father is present.

[28]See Appendix 4.H for details on the construction of the pre-migration educational attainment variable.

sex; migration motive; country of origin; and year of placement.[29]

Table 4.D.1 in Appendix 4.D reports the results of the balancing tests. I do not find evidence of a systematic relationship between household heads' educational attainment and initial neighborhood characteristics. Unsurprisingly, larger households are less likely to be assigned to more urbanized neighborhoods, which are often characterized by a relatively limited stock of family housing units, a higher concentration of migrants, and lower average earnings. The balance test supports the identification assumption of conditionally random assignment of refugees to their initial neighborhood of residence.

4.4.3 Post-assignment residential mobility

I identify the causal impact of the *initial* co-ethnic concentration and co-ethnic earnings in the *assigned* neighborhood. While exposure to these initial neighborhood characteristics can have a direct effect on school performance, initial exposure to neighborhood characteristics likely also affects education outcomes to the extent that they are predictive of the types of neighborhoods children are exposed to during childhood. High post-assignment residential mobility and low-persistence of local characteristics would weaken the link between initial exposure and total exposure during childhood. Figure 4.4.1 shows that post-assignment mobility is limited. After five years, 70% of children still reside at the initial placement address and 78% remain in the assigned neighborhood. After ten years, the shares of stayers in the assigned neighborhood is still 57%. This shows that for a substantial share of the sample, the assigned locality largely determines the area in which individuals grow up.

4.5 Results

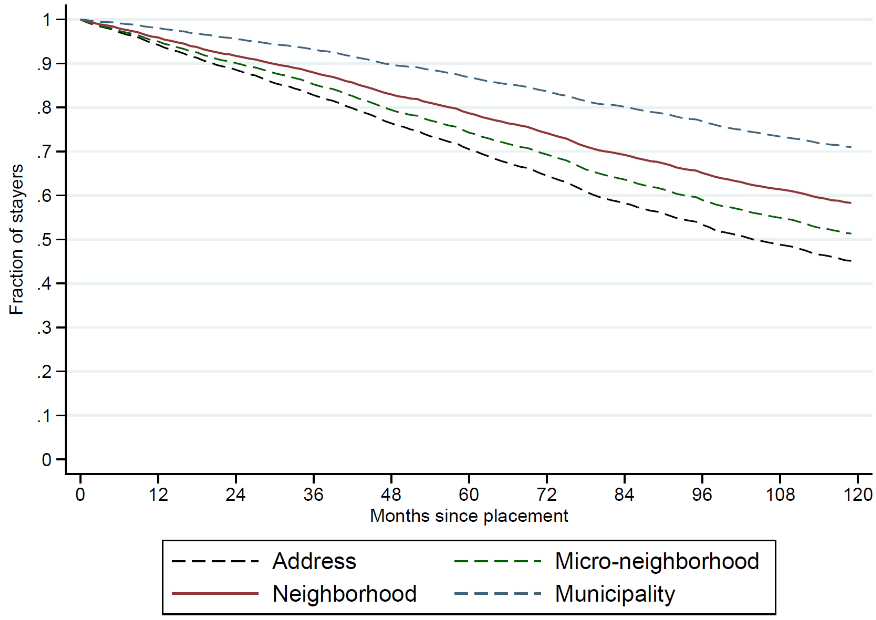
4.5.1 The impact of initial co-ethnic concentration and ethnic earnings on school performance

Table 4.5.1 presents the main estimates of interest: the effect of the initial co-ethnic share and the (log) mean daily co-ethnic earnings – henceforth co-ethnic earnings – on the education outcomes of refugee children. For each education outcome, the odd-numbered Columns in Table 4.5.1 report estimates from the baseline model 4.4.1.

The baseline estimates highlight two key findings. First, the effect of the initial co-ethnic share differs across education outcomes. Assignment to a neighborhood with a larger share of co-ethnic neighbors has a positive effect on the probability of receiving an academic track recommendation (Column 1) but adversely affects the probability of obtaining an academic track diploma by age 19 (Column 5). There is no effect of the initial co-ethnic share on higher education enrollment by age 21 (Column 5). A within-neighborhood standard deviation higher initial co-ethnic

[29] I define household heads as the first adult placed at the assigned address. If multiple adults are simultaneously placed, I select the male to be the household head.

Figure 4.4.1 Post-placement residential mobility



share increases the probability of receiving an academic track recommendation by 1.8 percentage points and lowers the probability of obtaining an academic track diploma by age 19 by 1.2 percentage points.[30] These are modest effects, roughly equal to 10% of the refugee-native performance gap in primary school and 6.6% of the performance gap in obtaining an academic track diploma.

Second, neighborhood co-ethnic earnings has a positive impact on the probabilities of receiving an academic track recommendation (Column 1) and obtaining an academic track diploma by age 19 (Column 3). The baseline estimates show no effect of co-ethnic earnings on higher education enrollment (Column 5). A within-neighborhood standard deviation higher co-ethnic earnings results in a 1.46 and 1.08 percentage point higher probability of receiving an academic track recommendation and obtaining an academic track diploma, respectively.

In the baseline specification, I model the initial co-ethnic share and co-ethnic earnings in the assigned neighborhood as separable entities in the human capital production function. However, the effect of co-ethnic concentration may be conditioned by the characteristics (i.e. co-ethnic capital) of the neighborhood co-ethnic network. Analogously, the influence of co-ethnic capital within the co-ethnic network may be amplified when children's social environment is disproportionally comprised of individuals from one's own ethnic group. To capture this, I include an interaction effect between the initial share of co-ethnics and co-ethnic earnings in the assigned neighborhood. Estimates from this specification are reported in the even-numbered Column of Table 4.5.1.

[30] Table 4.F.1 in Appendix 4.F reports the variation in the key explanatory variables. Within-neighborhood variation refers to the residual variation within neighborhoods after accounting for origin by year of arrival fixed effects (Column 6 in Table 4.F.1).

Table 4.5.1 Effects of assigned co-ethnic share and co-ethnic earnings on education outcomes

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Co-ethnic share (%)	0.019***	-0.077	−0.014*	−0.390***	-0.009	−0.344**
	(0.007)	(0.190)	(0.008)	(0.146)	(0.009)	(0.172)
Ln(co-ethnic earnings)	0.066**	0.061*	0.049*	0.027	0.002	-0.018
	(0.032)	(0.033)	(0.027)	(0.028)	(0.033)	(0.034)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.023		0.089***		0.079**
		(0.045)		(0.034)		(0.040)
Adjusted R^2	0.129	0.129	0.151	0.152	0.198	0.199
R^2	0.377	0.377	0.356	0.357	0.392	0.392
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level and averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned neighborhood. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I find a positive interaction effect between the neighborhood co-ethnic share and co-ethnic earnings for all education outcomes, although this effect is statistically insignificant for the primary school outcome (Column 2). Figure 4.5.1 facilitates interpretation by showing the estimated marginal effect of the initial co-ethnic share in the assigned neighborhood at different values of neighborhood co-ethnic earnings.

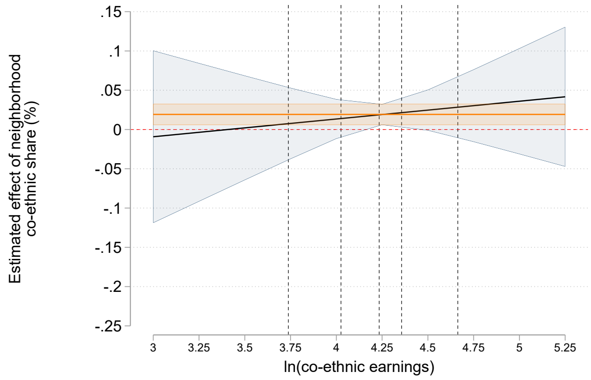
When neighborhood co-ethnic earnings are low, assignment to a neighborhood with a higher co-ethnic share adversely affects later education outcomes, while the effect on primary school performance is negligible. Evaluated at the 10^{th} percentile of the neighborhood co-ethnic earnings distribution (see Table 4.B.3, Column 6), a within-neighborhood standard deviation in the initial co-ethnic share in the assigned neighborhood lowers the probability of obtaining an academic track diploma by age 19 with 4.71 percentage points and lowers the probability of higher education enrollment by age 21 with 4.01 percentage points. Relative to the sample mean, this corresponds to a 19.6% lower probability of obtaining an academic track diploma by age 19 and a 9.3% lower probability of higher education enrollment by age 21. These are substantial effects: a 4.71 percentage points lower probability of obtaining an academic track diploma corresponds to 26.1% of the average refugee–native school performance gap. The magnitude of this effect is roughly as large as the impact of arriving in the Netherlands 2.14 years later or 34% of the performance gap between individuals with and without at least one high-educated parent (see Table 4.C.4).

By contrast, when neighborhood co-ethnic earnings are high, assignment to a neighborhood with a higher initial share of co-ethnic has a positive effect on school performance, although the estimates are imprecise. Even for low levels of neighborhood co-ethnic earnings, a higher co-ethnic share has a modest positive impact on primary school performance. For later education outcomes, the estimated impact turns positive around the 75$^{\text{th}}$ percentile of the neighborhood co-ethnic earnings distribution. Evaluated at the 95$^{\text{th}}$ percentile, a within-neighborhood standard deviation in the co-ethnic share raises the probability of receiving an academic track recommendation by 2.8 percentage points, the probability of obtaining an academic track diploma by 1.85 percentage points, and the probability of enrolling in higher education by 1.81 percentage points.

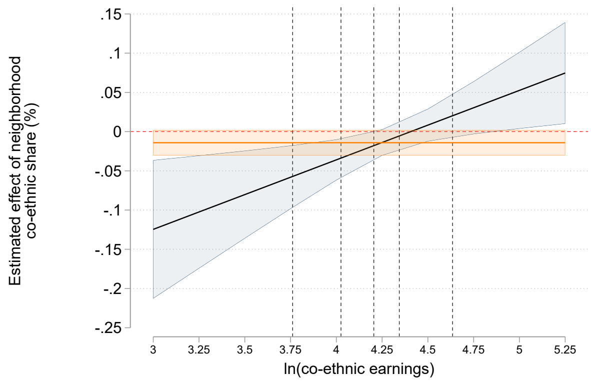
The positive interaction effects reported in Columns 3, 5, and 6 of Table 4.5.1 indicate that the positive impact of neighborhood co-ethnics earnings on school performance is amplified when neighborhood co-ethnic concentration is high. Evaluated at the sample mean co-ethnic share, a within-neighborhood standard deviation higher co-ethnic earnings raises the probability of receiving an academic track recommendation by 1.53 percentage points, the probability of obtaining an academic track diploma by age 19 by 2.13 percentage points, and the probability of higher education enrollment by 0.96 percentage points. At the 95$^{\text{th}}$ percentile of the neighborhood co-ethnic share distribution, the corresponding percentage point increases are 3.34 for the probability of receiving an academic track recommendation, 6.62 for the probability of obtaining an academic track diploma, and 4.96 for the probability of enrolling in higher education.

Figure 4.5.1 Estimated effect of initial co-ethnic share (%) in assigned neighborhood on outcomes

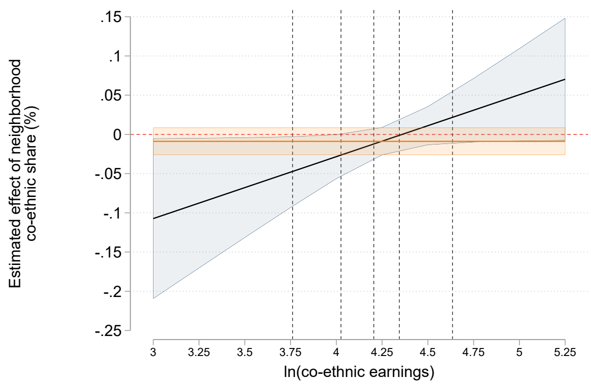
(a) Academic track recommendation



(b) Academic track diploma at age 19



(c) Higher education enrollment by age 21



Notes: The blue line shows the estimated marginal effect of the initial co-ethnics share on education outcomes at different values of log mean daily ethnic earnings. Estimates are based on those reported in Columns 2, 4, and 6 of Table 4.5.1. The vertical orange line shows the estimated effect of the initial co-ethnics share on education outcomes from the baseline model. The shaded areas show the 95% confidence interval. Vertical dashed lines are at the 10^{th}-, 25^{th}-,50^{th}-, 75^{th}-, and 95^{th} percentile of the sample distribution of neighborhood mean daily ethnic earnings.

Overall, the findings are consistent with ethnic network members exerting an impact on refugee children's school performance, both at the early and later stages of their education career. Consistent with Åslund et al. (2011) and the ethnic capital hypothesis of Borjas (1992, 1995), I show that the ethnic capital embedded within the ethnic network has a positive impact on school performance, above and beyond neighborhood characteristics common to all residents. However, in contrast to Åslund et al. (2011), I find that ethnic capital moderates the impact of being assigned to a neighborhood with a higher co-ethnics share: when average co-ethnic earnings are low, a higher concentration of co-ethnics adversely affects education outcomes, especially at a later stage of the education career. Similar moderating effects are observed by Hermansen (2023) and Bygren and Szulkin (2010). Moreover, I show that neighborhood co-ethnic earnings have a larger positive impact on school performance when neighborhood co-ethnic concentration is relatively high and co-ethnics are therefore more likely to comprise a larger share of a child's social environment.

4.5.2 Robustness

Alternative definitions of ethnicity. I have so far defined co-ethnicity as having at least one common language and originating from the same ethnolinguistic cluster of countries. Table 4.C.1 in Appendix 4.C reports results when I define co-ethnicity based on country of origin (i.e. co-nationals). This definition of co-ethnicity has been frequently used in the literature (e.g., Edin et al., 2003; Damm, 2009b; Danzer and Yaman, 2016). The results are qualitatively similar when using this alternative ethnic definition. The effect of being assigned to a neighborhood with a higher initial share of co-nationals is moderated by the mean co-national earnings in the neighborhood and the moderation effect is more pronounced for later education outcomes.

Alternative neighborhood definitions The preferred geographical unit for measuring local characteristics is neighborhoods. However, if individuals form social networks beyond the boundaries of the neighborhood, measuring ethnic concentration at a broader geographical level would more accurately co-ethnic networks. Table 4.C.3 reports estimates where co-ethnic concentration and co-ethnic earnings are measured at the municipality level. In line with the main findings, average co-ethnic earnings in the municipality positively affect primary school performance. However, I find no effect of the municipality's share of co-ethnics or average co-ethnic earnings on later education outcomes. This highlights the importance of measuring local characteristics at the geographical level at which social interactions likely take place.

Alternative functional form of ethnic concentration. While the measure of ethnic concentration – the share of co-ethnics in the local population – has been previously used in the literature (e.g., Danzer and Yaman, 2016; Høholt Jensen et al., 2022), others have used the logarithm of the number of co-ethnics as an alternative measure (e.g., Edin et al., 2003; Damm, 2009b; Åslund et al., 2011; Martén et al., 2019). Table 4.C.2 shows qualitatively similar results using this alternative measure of ethnic concentration: for the primary school outcome, I find a positive effect of neighborhood co-ethnic earnings. For later education outcomes, neighborhood co-ethnic earnings moderate the impact of the initial co-ethnic share, although the effect is no longer significant when considering the probability of higher education enrollment.[31]

Additional robustness analysis. I assess the sensitivity of the main findings by performing various additional robustness checks. Results are reported in Table 4.C.5. I find no evidence that the findings are driven by noise resulting from very few observations per assigned neighborhood (Columns 1-2) nor due to a small number of observations per origin country (Columns 3-4). In addition, results are robust to the exclusion of individuals from former Yugoslavia (Columns 5-6), or Afghanistan and Iraq (Columns 7-8). This suggests that the findings are not driven by refugees from European countries culturally closer to the Netherlands nor solely by individuals from the two largest origin countries. Finally, excluding individuals assigned to a neighborhood with a collective reception center (Columns 9-10), or those who arrive at the placement address at a later date (Columns 11-12) does not alter the main findings.

4.5.3 Average exposure to neighborhood co-ethnic concentration and co-ethnic earnings

The reduced-form estimates discussed so far are well-defined causal effects of being assigned to a neighborhood with higher (lower) initial co-ethnic concentration and co-ethnic earnings on refugees school performance. While the initial characteristics of the assigned neighborhood may have a direct effect on school performance, the actual exposure to local (co-ethnics) characteristics may be what ultimately affects education outcomes. The initial neighborhood characteristics could therefore affect school performance to the extent that they predict the local characteristics that children are exposed to until their education outcomes are observed.

I investigate this by estimating a model in which I assume that the actual exposure to neighborhood (co-ethnics) characteristics affects education outcomes. As a proxy for actual exposure, I use the average neighborhood co-ethnic share and co-ethnics earnings from the time of placement until the moment education outcomes are observed.[32] Since refugees are unconstrained in their post-placement residential mobility, average neighborhood characteristics are endogenous. To account for post-placement endogenous residential sorting, I use an instrumental variable (IV) approach in which I instrument the average neighborhood characteristic with the

[31] To avoid having to omit individuals without any co-ethnics in the assigned neighborhood, I use ln(1 + number of co-ethnics) and include an indicator for cases without any co-ethnics. The estimates therefore identify the effect of being assigned to a neighborhood with more co-ethnics, conditional on the presence of at least one co-ethnic neighbor.

[32] It is arguably the entire sequence of neighborhood characteristics that children are exposed to that affect school performance. Since only initial neighborhood characteristics are exogenous, I do not have sufficient instruments to identify such a model.

initial characteristics in the assigned neighborhood.[33]

The IV approach yields local average treatment effects of the average neighborhood co-ethnic share and co-ethnic earnings on school performance under the following assumptions. First, the initial neighborhood characteristics need to be highly correlated with average neighborhood characteristics (instrument relevance). Given the limited post-placement mobility and generally high persistence of neighborhood demographic compositions, initial neighborhood characteristics are strongly positively correlated with the average neighborhood characteristics that children are exposed to during childhood (see Columns 1 and 2, Table 4.5.2. Panel B of Table 4.5.2 reports first-stage estimates of the excluded variables in the second stage. Across the three outcome variables, the F-test statistic on the instruments well exceeds 10, suggesting that the instruments are strong. Second, conditional on average neighborhood characteristics, initial neighborhood characteristics need to be excludable from second-stage regressions (instrument validity). That is, the effect of initial neighborhood characteristics on school performance operates solely through their effect on average neighborhood characteristics. This assumption would be violated if early exposure to neighborhood characteristics has a larger effect on school performance than later exposure, as implied by a 'skills-beget-skill' model (e.g., Cunha and Heckman, 2007). Third, all individuals assigned to a neighborhood with a higher co-ethnic concentration (co-ethnic earnings) are exposed to a higher average co-ethnic share (co-ethnic earnings) than had they been assigned to a neighborhood with lower co-ethnic concentration (co-ethnic earnings) (monotonicity).

Table 4.5.2 reports the estimates from the IV approach. The estimates show a similar pattern as the reduced form estimates in Table 4.5.1, although the estimates are larger in magnitude. This suggest that the initial characteristics of the assigned neighborhood largely determine the type of co-ethnic environment that children are exposed to during childhood, and that this actual exposure strongly affects school performance. Several factors could contribute to the divergence between the ITT and LATE estimates. The effect of neighborhood co-ethnic characteristics could be cumulative, so that longer exposure to a certain type of neighborhood environment has a larger impact on school performance (Chetty et al., 2016b; Chetty and Hendren, 2018b). This prolonged exposure is captured in the LATE estimates. Moreover, after initial placement, refugees are no longer granted priority status when applying for social housing. Due to substantial waiting times for social housing (often exceeding several years), this puts considerable constraints on post-placement mobility. Therefore, it could be that post-placement mobility is primarily driven by, for example, higher-educated families moving out of social housing.[34] The IV approach implicitly corrects for post-placement residential mobility, and therefore likely captures the impact of neighborhood co-ethnic characteristics among those who are most constrained in their post-placement mobility. This group of children

[33]Similar instruments have been used by, for example, Edin et al. (2003); Åslund and Fredriksson (2009); Åslund et al. (2011)

[34]Unfortunately, I do not have sufficient information on parental background characteristics to formally test this hypothesis.

Table 4.5.2 Effects of average co-ethnic share and co-ethnic earnings on education outcomes

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 2SLS estimates						
Co-ethnic share (%)	0.026***	-0.041	-0.017	−1.137**	-0.011	−1.072*
	(0.008)	(0.451)	(0.011)	(0.477)	(0.012)	(0.550)
Ln(co-ethnic earnings)	0.219**	0.214*	0.211*	0.124	0.011	-0.063
	(0.104)	(0.112)	(0.113)	(0.118)	(0.151)	(0.155)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.016		0.262**		0.249*
		(0.106)		(0.111)		(0.128)
Panel B. First-stage F-statistic of the excluded instruments						
Average co-ethnic share (%)	57.324	61.481	54.443	71.739	45.859	49.547
Average co-ethnic earnings	46.843	31.745	44.959	30.279	45.959	28.543
Average co-ethnic share (%) × Average co-ethnic earnings		44.959		42.570		52.699
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are averaged over the period from placement until education outcomes are observed and instrumented by local characteristics at the assigned neighborhood averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned neighborhood. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

might also be the ones that are particularly susceptible to peer effects. Finally, it could be that initial neighborhood characteristics have an effect on school performance, beyond their effect on average neighborhood characteristics – i.e. the exclusion restriction no longer holds. If the effect of initial- and average neighborhood characteristics have the same sign, this could lead to an upward bias in the LATE estimates.

The estimates reported in the even-numbered Columns suggest that at the 10^{th} percentile of the average neighborhood co-ethnic earnings distribution (see Table 4.B.4), a percentage point increase in the average neighborhood co-ethnic share lowers the probability of obtaining an academic track diploma by age 19 and higher education enrollment by age 21 by 18.6 and 15.6 percentage points, respectively. These are substantial effects, comparable in magnitude to the performance gap between children with- and without at least one highly educated parent. At the 95^{th} percentile of the average neighborhood co-ethnic earnings distribution, a percentage point increase in the average neighborhood co-ethnic share results in a 7.9 percentage point higher probability of obtaining an academic track diploma and an 8.3 percentage point higher probability of higher education enrollment.

4.6 Conclusion

I study the impact of neighborhood co-ethnic concentration and co-ethnic earnings on the school performance of refugee children, by exploiting a Dutch refugee dispersal policy between 1999 and 2009, which quasi-randomly allocated refugees to their first place of residence. The random assignment of refugees to their initial place of residence allows us to address the identification challenge of endogenous residential sorting. Moreover, by relying solely on within-neighborhood variation, I isolate the effect of co-ethnics neighbors from the effect of broader contextual neighborhood factors shared by all residents.

Using rich administrative data, I find that the initial share of individuals with a shared ethnic background in the assigned neighborhood affects the school performance of refugee children, and that this effect is moderated by neighborhood co-ethnic earnings. Assignment to a neighborhood with a higher concentration of co-ethnics adversely affects (long-term) education outcomes when neighborhood co-ethnic earnings are low, while the effect turns positive when neighborhood co-ethnic earnings are high. This is consistent with the formation of social networks along ethnicity and the ethnic capital hypothesis of Borjas (1992, 1995). In addition, using an IV approach, I show that the impact of average exposure to neighborhood co-ethnic characteristics is larger in magnitude compared to initial exposure. These findings, in combination with low post-assignment residential mobility, suggest that prolonged exposure to neighborhood co-ethnic characteristics affect long-term education outcomes.

This paper has some limitations that future research should address to further our understanding of the links between ethnic neighborhood environments and education outcomes. First, the treatment effects I identify may hide heterogeneity across different groups of refugees. Future research should aim to identify which children are most affected by the local ethnic environment. Second, I rely on a proxy measure of (co-ethnic) social networks – i.e. the neighborhood-level concentration of co-ethnics – to identify the influence of co-ethnic peers on school performance. To further understand the role of co-ethnic peers in shaping immigrant education outcomes, future research should use more fine-grained and direct measures of social networks to identify the (most) influential peers. While the neighborhood co-ethnic networks in the sample are too small to render such an analysis meaningful, a logical next step could be to further disaggregate neighborhood co-ethnic peers into smaller subgroups. This could inform us about whether the impact of co-ethnic network members operates through children's own friendship networks or through co-ethnics in their parent's generation. Third, while I show that the neighborhood average co-ethnic earnings, as a proxy for ethnic (social) capital, is a key moderating factor of the impact of neighborhood co-ethnic concentration, identifying the dimensions of (co-ethnic) social capital embedded in children's social network is an important step to identifying the mechanisms that drive this result. Fourth, I focus on a specific group of immigrants – refugee children – who are assigned to a neighborhood with a relatively low co-ethnic concentration. Future research should investigate the generalizability of the findings to the broader migrant population and to cases where co-ethnics comprise a more substantial share of children's social environment.

4.A Baseline sample selection procedure

1. Select all first- and second generation migrants who who first registered in the municipal population registers between 1999 and 2009. *Obs. 1,452,610*
2. Retain individuals who first address in the Netherlands is a collective asylum reception centers. *Obs. 103,272*
3. Retain individuals who do not reside in COA-administered housing besides collective reception centers. *Obs. 60,071*
4. Omit individuals with discontinuous residency spells in the Netherlands. *Obs. 56,330*
5. Retain those who at regular address (not COA-administered) prior to 31 December 2009. *Obs. 42,461*
6. Retain those with a recorded asylum-, or family reunification motive. *Obs. 42,258*
7. Retain those originating from the 35 largest refugee sending countries. *Obs. 39,117*
8. Omit individuals belonging to an institutional household while residing at the placement address. *Obs. 37,068*
9. Omit individuals who join household members already residing at the placement address. *Obs. 33,118*
10. Add all individuals who join within the first year of placement while still residing at the placement address. *Obs. 38,309*
11. Retain households in which the placement address is the first non-COA-administered address for all household members. *Obs. 35,492*
12. Retain households if all household members registered in the municipal registers from 1999 onward. *Obs. 34,637*
13. Omit household when a household member was registered at COA-administered housing which was not a collective reception center. *Obs. 33,693*
14. Retain households if all household members originate from 35 largest refugee sending countries. *Obs. 33,003*
15. Omit household if any household member had family members registered in the Netherlands who are not part of the household. *Obs. 30,972*
16. Omit household if household head is younger than 18 at the time of placement. *Obs. 30,661*

4.B Additional descriptive statistics

Table 4.B.1 Individual characteristics: ethnic groups

	Sample	
	Primary school outcome	Later education outcomes
	Origin country	
Afghanistan	21.6	33.9
Iraq	14.8	12.1
Somalia	8.4	4.9
Azerbaijan	6.2	6.1
Bosnia-Herzegovina	4.1	4.9
Russia	4,0	2.8
Syria	3.6	4.1
Burundi	3.6	2.1
Iran	3.1	3.7
Armenia	3.0	2.8
Sudan	2.4	2.0
Serbia	2.4	2.4
Kosovo	2.2	2.5
Myanmar	2.1	1.1
Angola	2.0	1.7
Congo (DR)	1.9	1.4
Ethiopia	1.8	1.3
Sierra Leone	1.6	0.6
China	1.4	0.2
Ukraine	0.9	1.0
Other	8.9	8.4
	Language-region cluster	
Central-Asia: Farsi	24.8	37.8
Arabic countries: Arabic	20.6	17.8
Sub-Sahara Africa: Arabic	8.8	5.3
Mid- and Eastern Europe: Serbo-Croatian	7.7	9.2
Sub-Sahara Africa: French	6.5	4.0
Central-Asia: Azerbaijani	6.2	6.1
Mid- and Eastern Europe: Russian	4.3	3.0
Sub-Sahara Africa: English	3.8	2.1
Mid- and Eastern Europe:Armenian	3.0	2.8
Mid- and Eastern Europe: Albanian	2.2	2.5
East-Asia: Burmese	2.1	1.1
Sub-Sahara Africa: Portuguese	2.0	1.7
Sub-Sahara Africa: Amharic	1.8	1.3
East-Asia: Chinese	1.4	0.2
Arabic countries: English	1.0	1.1
Mid- and Eastern Europe: Ukrainian	0.9	1.0
Central-Asia: Uzbek	0.7	0.6
Mid- and Eastern Europe: Macedonian	0.5	0.4
South-Asia: Tamil	0.5	0.4
Other	1.1	1.7
Observations	3,987	4,805

Notes: Table reports the sample shares (%) for each ethnic group.

Table 4.B.2 Assigned local characteristics

	Sample			
	Primary school outcome	Later education outcomes	Primary school outcome	Later education outcomes
	Neighborhood		Municipality	
Number of co-nationals	25.36	24.79	134.47	135.00
	(55.24)	(52.97)	(339.94)	(318.09)
Co-national share (%)	0.18	0.18	0.13	0.13
	(0.29)	(0.28)	(0.16)	(0.15)
Number of language-region fellows	124.49	111.58	847.27	720.06
	(496.88)	(473.39)	(4554.22)	(3756.10)
Language-region fellows share (%)	0.72	0.62	0.54	0.49
	(1.72)	(1.41)	(1.08)	(0.94)
Ln(avg. co-national daily earnings)	4.06	4.02	4.08	4.03
	(0.50)	(0.50)	(0.44)	(0.43)
Ln(avg. language-region fellows daily earnings)	4.16	4.14	4.19	4.15
	(0.40)	(0.40)	(0.35)	(0.36)
Migrant share (%)	17.47	16.73	15.06	14.75
	(12.15)	(11.53)	(8.81)	(8.55)
Ln(avg. migrant daily earnings)	4.50	4.48	4.53	4.52
	(0.16)	(0.16)	(0.13)	(0.13)
Number of residents	13,552	13,508	82,187	83,120
	(12,594)	(12,962)	(114,923)	(115,946)
Ln(avg. daily earnings)	4.58	4.56	4.61	4.59
	(0.13)	(0.12)	(0.11)	(0.10)
No co-nationals	0.18	0.19	0.06	0.04
No language-region fellows	0.10	0.10	0.03	0.02
Observations	3,987	4,805	3,987	4,805

Notes: Table reports the sample shares (%) for each ethnic group.

Table 4.B.3 Assigned neighborhood initial co-ethnic characteristics: descriptive statistics

	Co-nationals			Language-region fellows		
	Share of neighborhood population (%)		Ln(avg. daily earnings)	Share of neighborhood population (%)		Ln(avg. daily earnings)
	(1)	(2)	(3)	(4)	(5)	(6)
	Primary school outcome					
Mean	0.178	0.256	4.059	0.716	0.905	4.156
Standard deviation	0.290	0.330	0.504	1.725	1.913	0.419
Percentile						
10^{th}	0.000	0.031	3.497	0.002	0.057	3.738
25^{th}	0.014	0.075	3.892	0.057	0.152	4.025
50^{th}	0.087	0.173	4.143	0.230	0.334	4.234
75^{th}	0.241	0.319	4.331	0.587	0.781	4.357
90^{th}	0.431	0.529	4.522	1.578	2.158	4.533
95^{th}	0.599	0.792	4.691	3.259	3.896	4.663
99^{th}	1.233	1.471	5.159	8.844	9.536	5.043
	3,987	2,584	2,584	3,987	3,104	3,104
	Later education outcomes					
Mean	0.178	0.253	4.023	0.623	0.784	4.140
Standard deviation	0.280	0.316	0.496	1.411	1.561	0.408
Percentile						
10^{th}	0.000	0.034	3.497	0.000	0.059	3.761
25^{th}	0.014	0.078	3.871	0.062	0.159	4.025
50^{th}	0.094	0.169	4.094	0.238	0.325	4.205
75^{th}	0.243	0.314	4.304	0.558	0.697	4.344
90^{th}	0.430	0.532	4.466	1.298	1.640	4.500
95^{th}	0.613	0.770	4.615	2.490	3.082	4.635
99^{th}	1.212	1.355	5.056	8.137	8.831	5.050
	4,805	3,106	3,106	4,805	3,737	3,737

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. Columns 2-3 and 4-5 report statistics conditional on the presence of at least one co-ethnic adult in the assigned neighborhood with positive earnings.

Table 4.B.4 Assigned neighborhood initial co-ethnic characteristics: descriptive statistics

	Co-nationals			Language-region fellows		
	Share of neighborhood population (%)		Ln(avg. daily earnings)	Share of neighborhood population (%)		Ln(avg. daily earnings)
	(1)	(2)	(3)	(4)	(5)	(6)
	Academic track recommendation					
Mean	0.261	0.298	4.055	0.903	0.963	4.151
Standard deviation	0.402	0.419	0.435	1.833	1.880	0.372
Percentile						
10^{th}	0.006	0.027	3.583	0.030	0.048	3.772
25^{th}	0.040	0.068	3.906	0.107	0.134	4.021
50^{th}	0.134	0.170	4.095	0.339	0.379	4.199
75^{th}	0.320	0.361	4.291	0.807	0.857	4.343
90^{th}	0.607	0.681	4.477	2.210	2.402	4.493
95^{th}	0.946	1.050	4.642	4.036	4.244	4.627
99^{th}	1.836	1.961	5.053	9.702	10.082	4.959
Observations	3,987	3,471	3,471	3,987	3,732	3,732
	Academic track diploma by age 19					
Mean	0.295	0.325	4.069	0.865	0.912	4.174
Standard deviation	0.448	0.461	0.398	1.621	1.654	0.335
Percentile						
10^{th}	0.009	0.029	3.629	0.035	0.055	3.813
25^{th}	0.052	0.076	3.909	0.126	0.157	4.055
50^{th}	0.163	0.193	4.100	0.378	0.407	4.208
75^{th}	0.346	0.377	4.285	0.812	0.866	4.349
90^{th}	0.681	0.735	4.474	2.045	2.138	4.494
95^{th}	1.059	1.116	4.633	3.710	3.800	4.607
99^{th}	2.254	2.537	4.993	8.691	8.749	4.958
Observations	4,805	4,329	4,329	4,805	4,545	4,545
	Higher education enrollment by age 21					
Mean	0.312	0.333	4.088	0.917	0.947	4.186
Standard deviation	0.468	0.477	0.376	1.665	1.685	0.317
Percentile						
10^{th}	0.012	0.029	3.679	0.040	0.055	3.854
25^{th}	0.058	0.075	3.930	0.141	0.159	4.063
50^{th}	0.173	0.197	4.109	0.405	0.424	4.215
75^{th}	0.364	0.388	4.288	0.866	0.897	4.355
90^{th}	0.708	0.739	4.482	2.226	2.285	4.494
95^{th}	1.097	1.133	4.639	3.953	4.019	4.602
99^{th}	2.510	2.579	5.017	8.442	8.739	4.937
Observations	4,805	4,484	4,484	4,805	4,642	4,642

Notes: Local (co-ethnic) characteristics are measured at the neighborhood level, and averaged from the month of placement until the month of assessment. Columns 2-3 and 4-5 report statistics conditional on the presence of at least one co-ethnic adult in the neighborhood with positive earnings.

4.C Tables for robustness analysis

Table 4.C.1 Effects of assigned co-ethnic share and co-ethnic earnings on education outcomes: co-nationals

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Co-ethnic share (%)	-0.024	-0.307	-0.023	-0.561^{***}	0.013	-0.549^{***}
	(0.052)	(0.341)	(0.037)	(0.195)	(0.037)	(0.207)
Ln(co-ethnic earnings)	0.025	0.016	0.008	-0.012	-0.029	-0.051^{*}
	(0.028)	(0.030)	(0.024)	(0.025)	(0.027)	(0.029)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.069		0.134^{***}		0.140^{***}
		(0.083)		(0.048)		(0.050)
Adjusted R^2	0.125	0.125	0.149	0.150	0.198	0.199
R^2	0.374	0.374	0.354	0.355	0.391	0.392
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned neighborhood. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.C.2 Effects of assigned number of co-ethnics and co-ethnic earnings on education outcomes

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Ln (number of co-ethnics)	0.002	0.010	-0.016	-0.092^{***}	-0.015^{*}	-0.053
	(0.015)	(0.040)	(0.012)	(0.031)	(0.014)	(0.037)
Ln(co-ethnic earnings)	0.067^{**}	0.073^{*}	0.051^{*}	-0.001	0.003	-0.022
	(0.032)	(0.040)	(0.027)	(0.033)	(0.033)	(0.041)
Ln (number of co-ethnics) × Ln(co-ethnic earnings)		-0.002		0.019^{***}		0.010
		(0.009)		(0.007)		(0.008)
Adjusted R^2	0.128	0.127	0.151	0.152	0.198	0.198
R^2	0.376	0.376	0.355	0.356	0.392	0.392
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned neighborhood. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.C.3 Effects of assigned co-ethnic share and co-ethnic earnings on education outcomes: municipality level

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Co-ethnic share (%)	0.017	-0.191	-0.006	0.061	0.004	-0.185
	(0.015)	(0.424)	(0.013)	(0.283)	(0.015)	(0.304)
Ln(co-ethnic earnings)	0.057^{*}	0.051	0.037	0.039	-0.005	-0.011
	(0.033)	(0.036)	(0.029)	(0.031)	(0.032)	(0.033)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.048		-0.016		0.044
		(0.099)		(0.066)		(0.071)
Adjusted R^2	0.127	0.126	0.149	0.149	0.198	0.197
R^2	0.375	0.375	0.354	0.354	0.391	0.391
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) municipality level, and averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.C.4 Table 4.5.1 continued

	Academic track recommendation		Academic track diploma by age 19		Higher education enrollment by age 21	
	(1)	(2)	(3)	(4)	(5)	(6)
Individual characteristics						
Female	0.003	0.003	0.027*	0.027*	0.064***	0.064***
	(0.017)	(0.017)	(0.014)	(0.014)	(0.016)	(0.016)
Age at arrival	−0.066***	−0.066***	−0.022***	−0.022***	−0.026***	−0.026***
	(0.012)	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)
Days registered at collective reception centres.	−0.000***	−0.000***	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Later placement	0.007	0.006	0.007	0.008	-0.040	-0.040
	(0.034)	(0.034)	(0.044)	(0.045)	(0.050)	(0.050)
Born in NED	0.045	0.045	-0.066	-0.066	-0.074	−0.075*
	(0.032)	(0.032)	(0.042)	(0.042)	(0.045)	(0.045)
Household characteristics						
ref cat: partners with children						
Single parent	-0.027	-0.025	0.026	0.032	0.007	0.012
	(0.079)	(0.079)	(0.072)	(0.072)	(0.078)	(0.078)
Household size	-0.036	-0.036	0.014	0.016	0.007	0.009
	(0.030)	(0.030)	(0.029)	(0.029)	(0.031)	(0.031)
Number of siblings	0.010	0.009	-0.018	-0.020	-0.017	-0.019
	(0.029)	(0.030)	(0.028)	(0.028)	(0.030)	(0.030)
Mother present at placement	-0.068	-0.070	-0.063	-0.072	-0.143	-0.151
	(0.131)	(0.131)	(0.117)	(0.116)	(0.131)	(0.131)
Father present at placement	0.049	0.050	0.088	0.102	0.160	0.172
	(0.107)	(0.107)	(0.097)	(0.097)	(0.106)	(0.106)
Mother's age at arrival	0.002	0.002	0.002	0.002	0.004*	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Father's age at arrival	-0.002	-0.002	-0.001	-0.002	-0.003	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Parent empl. prior to placement	-0.006	-0.006	0.021	0.020	-0.023	-0.024
	(0.029)	(0.029)	(0.027)	(0.027)	(0.028)	(0.028)
Parental education						
ref cat: < College/University						
Missing	0.122***	0.121***	0.136***	0.138***	0.155***	0.156***
	(0.039)	(0.039)	(0.029)	(0.029)	(0.031)	(0.031)
College/university	0.081**	0.080**	0.137***	0.140***	0.176***	0.179***
	(0.040)	(0.040)	(0.025)	(0.025)	(0.029)	(0.029)
Maximum time spent in coll. recep. centres (family)						
ref cat: < 1 year						
1 − 2 years	-0.002	-0.002	0.017	0.018	-0.015	-0.014
	(0.035)	(0.035)	(0.033)	(0.033)	(0.036)	(0.036)
2 − 3 years	0.028	0.029	0.104**	0.108**	-0.047	-0.044
	(0.052)	(0.052)	(0.048)	(0.048)	(0.054)	(0.054)
> 3 years	0.118*	0.120*	0.106	0.113*	0.043	0.049
	(0.065)	(0.065)	(0.067)	(0.067)	(0.076)	(0.076)
Migration motive						
ref cat: asylum						
Family reunification	0.021	0.020	-0.014	-0.016	0.000	-0.001
	(0.039)	(0.039)	(0.043)	(0.043)	(0.044)	(0.044)
Unknown	−0.107*	−0.107*	0.038	0.040	0.014	0.015
	(0.059)	(0.059)	(0.052)	(0.052)	(0.069)	(0.069)
Adjusted R^2	0.129	0.129	0.151	0.152	0.198	0.199
R^2	0.377	0.377	0.356	0.357	0.392	0.392
Observations	3,987		4,805		4,805	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) municipality level, and averaged over the twelve months prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Robust standard errors (in parentheses) are clustered by origin country × assigned neighborhood. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4.C.5 Effects of assigned co-ethnic share and co-ethnic earnings on education outcomes: additional robustness analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	> 5 observations per neighborhood		> 20 observations per origin country		Excluding former Yugoslavia		Excluding Afghanistan and Iraq		No coll. recep. centre in assigned neighborhood		No later placements	
Outcome: Academic track recommendation												
Co-ethnic share (%)	0.012	-0.009	0.019***	-0.026	0.017**	-0.078	0.001	-0.078	0.025***	0.007	0.026***	-0.174
	(0.008)	(0.198)	(0.007)	(0.191)	(0.007)	(0.193)	(0.017)	(0.252)	(0.007)	(0.201)	(0.008)	(0.221)
Ln(co-ethnic earnings)	0.036	0.034	0.068**	0.066**	0.094***	0.089**	0.073*	0.070*	0.077**	0.077**	0.058	0.048
	(0.035)	(0.036)	(0.032)	(0.033)	(0.034)	(0.035)	(0.040)	(0.041)	(0.035)	(0.035)	(0.035)	(0.038)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.005		0.011		0.022		0.018		0.004		0.047
		(0.046)		(0.045)		(0.045)		(0.059)		(0.047)		(0.052)
Adjusted R^2	0.135	0.134	0.129	0.129	0.133	0.133	0.148	0.147	0.144	0.144	0.127	0.127
R^2	0.328	0.328	0.376	0.376	0.390	0.390	0.456	0.456	0.401	0.401	0.391	0.391
Observations	2,564		3,956		3,567		2,426		3,568		3,462	
Outcome: Academic track diploma by age 19												
Co-ethnic share (%)	-0.015	−0.396**	−0.014*	−0.355**	-0.014	−0.360**	-0.010	−0.546**	-0.003	−0.268*	-0.016	−0.472***
	(0.009)	(0.160)	(0.008)	(0.147)	(0.009)	(0.157)	(0.019)	(0.216)	(0.009)	(0.158)	(0.010)	(0.151)
Ln(co-ethnic earnings)	0064 ∗ ∗	0.041	0.050*	0.029	0.059**	0.038	0.013	−0.007	0.050	0.034	0.049*	0.023
	(0.029)	(0.030)	(0.027)	(0.028)	(0.029)	(0.031)	(0.042)	(0.042)	(0.031)	(0.032)	(0.027)	(0.029)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.090**		0.080**		0.082**		0.126**		0.062*		0.107***
		(0.037)		(0.034)		(0.036)		(0.050)		(0.037)		(0.035)
Adjusted R^2	0.142	0.143	0.148	0.149	0.168	0.169	0.168	0.170	0.169	0.170	0.154	0.156
R^2	0.300	0.301	0.352	0.353	0.377	0.378	0.454	0.456	0.383	0.383	0.363	0.365
Observations	3,359		4,768		4,224		2,512		4,194		4,530	
Outcome: Higher education enrollment by age 21												
Co-ethnic share (%)	-0.012	-0.295	-0.009	−0.308*	-0.009	-0.288	0.003	-0.200	0.007	-0.279	-0.007	−0.398**
	(0.010)	(0.192)	(0.009)	(0.174)	(0.009)	(0.180)	(0.019)	(0.250)	(0.010)	(0.187)	(0.011)	(0.185)
Ln(co-ethnic earnings)	0.007	-0.011	0.001	-0.017	0.003	-0.014	−0.085*	−0.092*	0.015	-0.003	-0.004	-0.026
	(0.035)	(0.037)	(0.033)	(0.034)	(0.036)	(0.037)	(0.048)	(0.049)	(0.036)	(0.037)	(0.034)	(0.035)
Co-ethnic share (%) × Ln(co-ethnic earnings)		0.067		0.071*		0.066		0.048		0.067		0.092**
		(0.045)		(0.041)		(0.042)		(0.058)		(0.044)		(0.043)
Adjusted R^2	0.184	0.184	0.197	0.198	0.201	0.201	0.245	0.245	0.214	0.214	0.190	0.191
R^2	0.334	0.334	0.389	0.390	0.401	0.402	0.505	0.505	0.416	0.416	0.390	0.391
Observations	3,359		4,768		4,224		2,512		4,194		4,530	

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level at the month prior to placement. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome (Columns 1-2) the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.D Balance test

Table 4.D.1 Balancing test: initial (assigned) neighborhood characteristics and individual characteristics of household heads

	Share of neighborhood population (%)			Ln(avg. daily earnings)				Degree of
	Co-nationals	Language-region fellows	Migrants	Co-nationals	Language-region fellows	Overall	Migrants	urbanization
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education attainment								
ref cat: < College/University								
Missing education	0.015	0.061	-0.557	-0.004	-0.035	0.002	0.005	-0.000
	(0.011)	(0.059)	(0.499)	(0.042)	(0.035)	(0.005)	(0.007)	(0.021)
College/University	-0.003	-0.000	0.746	0.021	0.063	0.011	0.010	0.052
	(0.017)	(0.094)	(0.795)	(0.067)	(0.056)	(0.009)	(0.013)	(0.033)
Individual characteristics								
Age at arrival	0.001	−0.004	0.052	0.003	0.003	-0.000	−0.000	0.001
	(0.004)	(0.004)	(0.039)	(0.003)	(0.002)	(0.000)	(0.000)	(0.001)
Female	0.039	0.018	2.156*	0.026	0.083	−0.000	-0.001	0.014
	(0.027)	(0.150)	(1.270)	(0.107)	(0.089)	(0.013)	(0.017)	(0.053)
Employed prior to placement	0.021	0.028	0.557	-0.079	−0.116	-0.014	−0.019	-0.020
	(0.020)	(0.113)	(0.952)	(0.080)	(0.067)	(0.015)	(0.019)	(0.040)
Household type								
ref cat: Single person								
Single parent	-0.023	−0.457***	−5.435***	0.030	0.108	0.034**	0.043**	-0.084
	(0.031)	(0.173)	(1.460)	(0.123)	(0.103)	(0.015)	(0.019)	(0.061)
Partners without kids	-0.069	−0.832***	−4.561*	0.029	0.044***	0.024	0.045	-0.063
	(0.057)	(0.318)	(2.690)	(0.023)	(0.018)	(0.020)	(0.036)	(0.112)
Partners with kids	-0.005	−0.528***	−5.414***	0.019	0.021	0.031*	0.047**	-0.100
	(0.034)	(0.188)	(1.590)	(0.134)	(0.112)	(0.016)	(0.021)	(0.066)
Household size	−0.010***	−0.061***	−1.271***	-0.001	0.024*	0.002	0.003	−0.041***
	(0.004)	(0.024)	(0.201)	(0.017)	(0.014)	(0.002)	(0.003)	(0.008)
Time registered in coll. recep. centres								
ref cat: < 1 year								
$1-2$ years	0.021	0.118	0.871	0.023	-0.030	-0.007	-0.008	0.008
	(0.016)	(0.087)	(0.736)	(0.062)	(0.052)	(0.007)	(0.010)	(0.031)
$2-3$ years	0.015	0.137	0.235	0.001	-0.037	0.006	0.007	-0.030
	(0.021)	(0.117)	(0.987)	(0.083)	(0.069)	(0.010)	(0.013)	(0.041)
> 3 years	-0.008	0.082	-0.762	-0.130	−0.108	0.015	0.018	-0.062
	(0.027)	(0.150)	(1.272)	(0.107)	(0.089)	(0.013)	(0.017)	(0.053)
	Test of joint significance of education categories (including missing education)							
Prob $> F$	0.223	0.566	0.330	0.907	0.323	0.160	0.177	0.362
Adjusted R^2	0.151	0.241	0.053	0.494	0.435	0.114	0.047	0.034
R^2	0.215	0.298	0.125	0.532	0.478	0.181	0.119	0.107
Observations	3535							

Notes: Sample of household heads. Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. Individual and household characteristics are measured at the time of placement. All specifications include origin country × arrival year and placement year fixed effects. Degree of urbanization is a dummy equal to one if addresses per $km^2 > 1500$. Robust standard errors (in parentheses) are clustered at the origin country × (assigned) neighborhood level.

4.E IV approach: first-stage regressions

Table 4.E.1 IV approach: first-stage regressions

	Endogenous regressors: average from placement until assessment				
	Co-ethnic share (%)	Ln(co-ethnic earnings)	Co-ethnic share (%)	Ln(co-ethnic earnings)	Co-ethnic share (%) × Ln(co-ethnic earnings)
	(1)	(2)	(3)	(4)	(5)
	Outcome: Academic track recommendation				
Instrument: initial ...					
Co-ethnic share (%)	0.825***	-0.017	-1.092	-0.105	−1.899***
	(0.078)	(0.011)	(0.990)	(0.141)	(0.293)
Ln(co-ethnic earnings)	-0.001	0.293***	-0.101	0.301***	0.002
	(0.056)	(0.063)	(0.063)	(0.033)	(0.024)
Co-ethnic share (%) × Ln(co-ethnic earnings)			0.450**	0.0224	0.457***
			(0.225)	(0.033)	(0.069)
F-test statistic	57.324	46.843	61.481	31.745	44.959
Observations			3,987		
	Outcome: Academic track diploma by age 19				
Instrument: initial ...					
Co-ethnic share (%)	0.808***	−0.018*	0.322	0.0104	−1.455***
	(0.100)	(0.011)	(0.899)	(0.113)	(0.236)
Ln(co-ethnic earnings)	0.072	0.265***	0.0428	0.241***	-0.006
	(0.059)	(0.060)	(0.053)	(0.026)	(0.021)
Co-ethnic share (%) × Ln(co-ethnic earnings)			0.115	-0.00258	0.350***
			(0.193)	(0.026)	(0.055)
F-test statistic	54.443	44.959	71.739	30.279	42.570
Observations			4,805		
	Outcome: Higher education enrollment by age 21				
Instrument: initial ...					
Co-ethnic share (%)	0.780***	-0.0145	0.557	0.036	−1.330***
	(0.101)	(0.009)	(0.970)	(0.096)	(0.203)
Ln(co-ethnic earnings)	0.070	0.271***	0.056	0.220***	-0.014
	(0.059)	(0.053)	(0.057)	(0.024)	(0.018)
Co-ethnic share (%) × Ln(co-ethnic earnings)			0.0525	-0.00875	0.319***
			(0.210)	(0.023)	(0.047)
F-test statistic	45.859	45.959	49.547	28.543	52.699
Observations			4,805		

Notes: Instruments: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level and averaged over the twelve months prior to placement. Endogenous regressors: local (co-ethnic) characteristics averaged over the months from placement until the month of assessment. All model specifications include assigned neighborhood fixed effects, origin country by arrival year fixed effects, and year of placement fixed effects. For the primary school outcome the model specification additionally include year of assessment fixed effects. Additional individual and household controls are measured at placement and include: age at arrival, mother/father age at arrival, gender, household type, household size, number of siblings, days registered at collective reception centers, highest parental education attainment, indicator for parental employment prior to placement, migration motive, and maximum years spent in collective reception centers by household members. Robust standard errors (in parentheses) are clustered by origin country × assigned municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.F Variation in key explanatory variables

Table 4.F.1 Identifying variation

	Standard deviations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Language-region fellows							
Sample	Neighborhood co-ethnic share (%)							
Primary education	1.725	1.479	1.418	1.256	1.024	0.949	0.938	0.931
Later education	1.411	1.221	1.182	1.072	0.862	0.823	0.821	0.811
Sample	Ln(co-ethnic earnings)							
Primary education	0.369	0.355	0.332	0.259	0.244	0.222	0.220	0.217
Later education	0.360	0.349	0.336	0.253	0.239	0.220	0.219	0.218
	Co-nationals							
Sample	Neighborhood co-ethnic share (%)							
Primary education	0.290	0.252	0.245	0.189	0.161	0.149	0.148	0.146
Later education	0.280	0.263	0.255	0.189	0.169	0.155	0.154	0.151
Sample	Ln(co-ethnic earnings)							
Primary education	0.406	0.388	0.367	0.298	0.278	0.252	0.251	0.247
Later education	0.398	0.382	0.367	0.298	0.282	0.263	0.262	0.259
Fixed effects								
Origin country		Y	Y		Y	Y	Y	Y
× arrival year			Y			Y	Y	Y
Neighborhhod				Y	Y	Y	Y	Y
Placement year							Y	Y
Additional controls								Y

Notes: Local (co-ethnic) characteristics are measured at the (assigned) neighborhood level, and averaged over the twelve months prior to placement. Table reports the standard deviations of the residual variation in the explanatory variables after inclusion of controls.

4.G List of origin countries

Table 4.G.1 Language-region clusters and origin countries

Language-region cluster	Countries
Arabic countries: Arabic	Yemen Jordan Oman United Arab Emirates **Egypt** Morocco Tunisia **Saudi Arabia Iraq** Bahrain Palestinian territories Qatar **Libya** Mauritania **Algeria Syria Sudan Lebanon Kuwait**
Central-Asia: Azerbaijani	**Azerbaijan**
Central-Asia: Farsi	**Iran Afghanistan Tajikistan**
Central-Asia: Russian	**Kazakhstan Kyrgyzstan**
Central-Asia: Uzbek	**Uzbekistan**
Mid- and Eastern Europe: Armenian	**Armenia**
Mid- and Eastern Europe: Georgian	**Georgia**
Mid- and Eastern Europe: Macedonian	**North-Macedonia**
Mid- and Eastern Europe: Romanian	**Moldova** Romania
Mid- and Eastern Europe: Russian	**Russia Belarus**
Mid- and Eastern Europe: Albanian	Albania **Kosovo**
Mid- and Eastern Europe: Serbo-Croatian	**Croatia Serbia Montenegro Bosnia-Herzegovina**
Mid- and Eastern Europe: Ukrainian	**Ukrain**
East-Asia: Burmese	**Myanmar**
East-Asia: Chinese	**China** Taiwan Macao Hongkong
Sub-Sahara Africa: Amharic	**Ethiopia**
Sub-Sahara Africa: Arabic	Djibouti Morocco **Somalia** Chad Comoros **Eritrea**
Sub-Sahara Africa: English	Botswana Ghana Cameroon Kenya Lesotho Malawi Namibia Seychelles Tanzania Zambia Zimbabwe **Liberia** Mauritius South-Sudan **Nigeria Rwanda Sierra Leone Uganda** Gambia **Eritrea** Eswatini
Sub-Sahara Africa: French	Burkina Faso **Burundi Congo (DR)** Djibouti Equatorial Guinea Cameroon **Congo** Senegal Seychelles Chad Benin Togo Mali Comoros Côte d'Ivoire Rwanda Niger Gabon **Guinea** Madagascar Central-African Republic
Sub-Sahara Africa: Portuguese	**Angola** Guinea-Bissau Cape Verde Mozambique
South-Asia: English	India **Pakistan**
South-Asia: Tamil	**Sri Lanka**

Notes: Origin countries of children in the estimation samples are in bold. Countries with multiple official languages can belong to multiple language-region clusters.

4.H Parental educational attainment

Information on educational attainment is based on several registers from publicly funded educational institution and the Employee Insurance Agency, supplemented with information from labor force surveys. This data has some limitations. First, coverage among migrants and earlier birth cohorts is relatively low. Second, educational attained abroad might be underestimated. Since education attained in the Netherlands is subject to endogeneity issues, I only consider educational attainment among those who never enrolled in the Dutch education system. Consequently, information on (imputed) pre-migration educational attainment is only available for 61.65% of the household heads in the sample.

4.I Education for asylum seekers

Asylum seekers with the compulsory schooling age attend school even if their asylum claim has not (yet) been granted. Children aged 4 to 12 enroll in primary school: either in regular schools and specialized schools for migrants ('newcomer facilities'). The latter is a school solely for refugees (often directly linked to a AZC facility) or for migrant children in general. Children attending regular primary schools receive additional help with the Dutch language and generally follow the standard curriculum. In newcomer facilities, which children can attend for a maximum of two years, the curriculum places more emphasis on social skills, socioemotional development, and the Dutch language (Magnée, 2020). The organization of education for refugee children is the responsibility of the local (municipal) government. This local autonomy induces spatial variation in the specific organization of education of refugees. In principle, individuals are free to choose their own school. However, in practice, children often attend the nearest school with available spots (Magnée, 2020). Asylum seekers aged 12 to 18, who have been in the Netherlands a maximum of two years, enroll in International Transmission Classes for approximately two years, after which they enroll in regular secondary education.

4.J Residency permits

The IND is responsible for granting or denying permits Formally, the IND should reach a first decision on an asylum claim within six months.[35] In practice, processing times may exceed the formal processing time, depending on the number of asylum claims being processed by the IND. Asylum seekers who have been awaiting the final decision by the IND for longer than six months are allowed access to the Dutch labor market under strict conditions.[36]

As of April 2001, only one temporary type of permit is granted. The permit is valid for five year.[37] After five years, refugees can apply for a permanent residency permit, conditional on having passed an integration exam. The temporary permit grants unrestricted access to the Dutch labor market and welfare system.

Prior to April 2001, three different residency permits could be granted. An A-status was granted to refugees admitted under the terms of the Geneva Convention and was valid for an indefinite period. Refugees who could not be sent back to their origin country for humanitarian reasons could be granted a temporary residency permit on humanitarian grounds (VTV-permit). This permit was valid for three years. Finally, a conditional temporary permit (VVTV-permit) could be granted to groups of asylum seekers, usually from the same nationality or region, who could not be admitted on individual grounds but could not be sent back due to the general situation in their origin country. The VVTV-permit was valid for one year, and after three years, VVTV-permit holders could apply for an A- or VTV-status. When the situation in the origin country improved sufficiently, the VVTV-permit could be repealed. Besides their duration, these permits varied in various aspects, such as access to the labor market and social security, possibility of family-reunification, and integration requirements.

[35]This can be extended to a year if further information collection is required.

[36]First, the employer must apply for a special permit ('tewerkstellingsver- gunning') prior to hiring. Second, the work has to meet normal working conditions (e.g., wages have to meet the industry standards). Under these conditions, asylum seekers are allowed to work for a maximum of 14 weeks (24 weeks as of 2008) in a 52-week period.

[37]prior to 2004, the permit was valid for three years.

The type of permit granted may impact a refugee's integration trajectory in several ways. First, due to the differences in rights and obligations attached to the three types of residency permits, many asylum seekers initially granted a VTV- or VVTV-permit appealed in an attempt to obtain a 'better' permit. The resulting lengthy asylum procedures and prolonged stay in the collective reception centers could slow down integration in the host-society (Bakker et al., 2014; Åslund et al., 2022). Second, in contrast to A- and VTV-permit holders, VVTV-status holders had limited access to the labor market and social security, were not required to partake in integration courses, and were constrained in their residential mobility.

CHAPTER 5

Conclusion

This dissertation consists of three empirical investigations intimately related to the broader theme of income inequality. The first Chapter studies the role of a key labor market institution – the minimum wage – in reducing income inequality. The second and third Chapters investigate two potential causes of income inequality: the wage-setting power of firms and exposure to neighborhood characteristics during childhood. Throughout this dissertation, I use detailed administrative data from the Netherlands and employ a variety of quasi-experimental research designs.

In what follows, I briefly summarize each empirical investigation and discuss the main findings and potential policy implications (focusing on the Dutch context) in Section 5.1. In Section 5.2, I discuss the main contributions of this dissertation. Section 5.3 concludes by discussing the main limitations and by providing directions for future research.

5.1 Summary and Discussion of Main Findings

5.1.1 The Young Bunch: Youth Minimum Wages and Labor Market Outcomes

By setting a lower bound to wages at the bottom of the wage distribution, the minimum wage is an important policy instrument to counteract lower-tail inequality. However, due to increased employers' labor costs, minimum wages may also reduce the demand for low-wage labor, potentially hurting the employment prospects of affected workers. Chapter 2 studies the impact of a 15-19% increase in the age-specific minimum wage in July 2017, applicable to workers aged 20-22 in the Netherlands, on the employment outcomes and earnings of these affected age groups. My co-authors and I use a difference-in-differences design that allows us to trace the impact of the minimum wage increase throughout the age-specific wage distribution. Specifically, we assign individuals to wage-bins defined relative to the new minimum wage and estimate the change in the number of jobs and total hours worked in each wage bin among affected workers, using slightly older workers as a control group.

We find no evidence that the sizable increase in the age-specific minimum wage reduced the number of jobs held or the total hours worked by affected workers, even in the most affected industries. While employment in wage-bins below the new minimum wage declined, this is fully compensated by an increase in the number of jobs and hours worked in wage-bins at or slightly above the new minimum wage. We find substantial spillover effects of the minimum wage: employment in wage-bins up to €4 above the new minimum wage increased. These spillovers are concentrated in low-wage jobs, with around 90% of these spillover effect occurring within €2.50 above the new minimum wage. Exploring effect heterogeneity, we find no evidence of 'offsets' in contract quality or adverse employment effects among different groups of workers. However, hours worked by non-students and workers with full-time jobs increased, suggesting that the policy more positively impacted workers who rely on low-wage jobs for a living.

Overall, the findings presented in Chapter 2 suggest that the increase in the minimum wage for 20-22-year-olds effectively raised wage for low-wage workers in the affected age groups, compressing the age-specific wage distributions and reducing lower-tail income inequality, without causing a reduction in the number of jobs held or the total hours worked by these workers.

Policy implications. Prior to 2024, the Dutch minimum wage system differentiated between workers with a contracted 36-, 38-, or 40-hour full-time workweek. As of January 2024, a uniform minimum wage was introduced, effectively increasing the minimum wage for those with a contracted 38- or 40-hour workweek by around 9% and 15%. Moreover, during the 2023 national elections, most parties incorporated plans for future (and sometimes sizable) minimum wage increases in their election programs. The findings presented in Chapter 2 suggest that these recent and potential future increases in the (general) minimum wage may not lead to significant disemployment effects. Moreover, the spillover effects identified in Chapter 2 could suggest that wages of workers slightly further up the wage distribution may also rise, further reducing lower-tail wage inequality.

However, it is important to note that Chapter 2 focuses on age-specific minimum wage increases, which may have a relatively limited impact on (some) firms' total labor cost compared to large increases in the general minimum wage. Minimum wage increases which have a substantially larger bite in firms' labor costs could potentially incentivize firms to restructure production or their workforce, which could lead to disemployment effects among some workers. Moreover, Chapter 2 does not explicitly explore the mechanisms driving spillover effects. These mechanisms can have different implications for the effects of increasing the minimum wage. For example, spillovers can arise due to reallocation of employment to more efficient firms, potentially driving the least efficient firms out of the market. Spillovers can also occur when a higher minimum wage incentivizes those out of the labor force to supply labor, with some of these individuals finding employment in jobs paying slightly above the minimum wage, potentially increasing aggregate employment.

5.1.2 Monopsony in the Netherlands: A mover-based approach

Firm wage setting – or monopsony – power refers to a situation in which firms face an upward-sloping labor supply curve, which allows them to mark down wages relative to workers' marginal productivity. Monopsony power can contribute to income inequality if firms are able to mark down wages more for different types of workers or when different types of workers are disproportionally employed in industries or occupations characterized by higher degrees of firm wage-setting power. Moreover, the impact of firm heterogeneity on wages is amplified in monopsonistic labor markets, contributing to wage dispersion between those in high- or low-productivity firms.

In Chapter 3, my co-author and I estimate the labor supply elasticity to Dutch firms, used to quantify the degree of monopsony power, and explore how this firm labor supply elasticity varies across different types of workers and industries. We use an IV event-study approach recently developed by Bassier et al. (2022) to estimate the causal separation response of workers to differences in the firm-component of wages. This event-study approach compares similar workers who separate from the same origin firm around the same time and move to different intermediate firms, experiencing different wage changes. We estimate the separation elasticity with respect to the firm-component of wages by estimating the probability of separating from the intermediate firm in response to the change in the own wage experienced during the initial transition. To isolate the firm-component of this wage change, aimed at capturing differences in firm wage-setting policies, we instrument the change in the own wage by the difference in average coworkers' wage at the origin and intermediate firm. Finally, we use our estimate of the separation elasticity to infer the labor supply elasticity to the firm.

Using data over the period 2010-2021, we find evidence of substantial monopsony power in the Dutch labor market, with a firm-level labor supply elasticity of 7.24. This estimate implies that in the Dutch labor market as a whole, firms can potentially mark down wages relative to workers' marginal productivity by around 12%. We find that the labor supply elasticity varies by gender, with a lower labor supply elasticity for women than men. This suggests that firms can potentially mark down wages more for the former, potentially contributing to the gender wage-gap. Exploring heterogeneity across the wage distribution, we find that workers at the bottom and top of the wage distribution have the lowest labor supply elasticity. However, potential markdowns are more than twice as large for the highest earning individuals. This suggests that firm wage-setting power has the potential to depress wages for low-wage workers, contributing to lower-tail inequality but could also compress the wage distribution at the upper-tail.

Policy implications. The findings presented in Chapter 3 point to frictions in the Dutch labor market, which give rise to monopsony power of firms over their workers. Such monopsony power can stem from a number of sources, including but not limited to costly job search, constraints in geographical or occupational mobility, and information asymmetries between firms and workers. In Chapter 3, we do not explicitly identify the distinct sources of monopsony power, which may vary across different labor market segments or subgroups of workers. While future research is needed for the design of targeted policies, several potential policy-levers are available to policy-makers. Policies that promote workers' labor market mobility by reducing search costs (e.g., extending unemployment insurance), removing geographical mobility constraints (e.g., promoting work-from-home arrangements), increase workers outside options (e.g., subsidizing life-long-learning programs), or reduce information asymmetries (e.g., require wage-posting and standardization of some workplace amenities), may limit firm wage-setting power. Moreover, monopsony power can be reduced by policies that limit firms' outside options for (cheaper) labor, such as imposing wage floors or other forms of employee protection (e.g., increasing severance pay or limitations on flexible contracts).

5.1.3 New home, old neighbors? Ethnic enclaves and refugees' education outcomes

Refugees often face steep barriers to economic and civic integration and generally lag behind natives in education and labor market outcomes. Refugees and migrants more broadly, tend to settle in neighborhoods with a relatively high concentration of other migrants, particularly those with a similar ethnic background. There is a widespread concern that the segregation of refugees in such 'ethnic enclaves' hampers their long-term (labor market) integration, contributing to the refugee-native wage-gap. In Chapter 4, I investigate whether neighborhood co-ethnic concentration and co-ethnic earnings affect the education outcomes of refugee children. I exploit a Dutch refugee dispersal policy between 1999 and 2009, which quasi-randomly assigned refugees to their initial address. The dispersal policy induces plausible exogenous variation in initial neighborhood composition, which allows me to overcome several identification challenges, giving my estimates a causal interpretation.

I find that neighborhood co-ethnic earnings moderate the effect of neighborhood co-ethnic concentration. Assignment to a neighborhood with a higher concentration of co-ethnics adversely affects refugees' long-term education outcomes when neighborhood co-ethnic earnings are low, while a higher concentration of co-ethnics positively affects education outcomes when neighborhood co-ethnic earnings are high. Moreover, while higher neighborhood co-ethnic earnings positively affect refugee children's school performance, this effect is amplified when co-ethnic concentration is high. Overall, these findings suggest that growing up in 'high-quality' ethnic enclaves improves education outcomes for refugee children, reducing the school performance gap between refugees and natives, while growing up in 'low-quality' enclaves has the opposite effect. As education is a key determinant of later labor market outcomes, the (ethnic) composition of the neighborhood in which refugees grow up can have important consequences for their labor market integration later in life.

Policy implications. Chapter 4 shows that the (ethnic) composition of the neighborhood that refugee children are assigned to affects their long-term school performance. This is informative for the design of refugee dispersal policies. As of 2016, the COA has experimented with matching refugees to municipalities to improve the labor market prospects of *adult* refugees. My findings suggest that incorporating neighborhood co-ethnic composition in this matching process could potentially improve education outcomes for refugee children as well. Recently, the Dutch government introduced new legislation – the *spreidingswet* – which requires all Dutch municipalities to provide housing accommodations to asylum seekers. Asylum seekers often spent considerable time in these accommodations prior to moving to regular social housing. Chapter 4 suggests that taking into account the demographic composition of the area in which these accommodations will be located, as well as the ethnic background of asylum seekers when assigning individuals to specific accommodations, has the potential to improve asylum seekers' long-term education outcomes.

Moreover, the finding that assigning refugee children to neighborhoods with a high concentration of low-earning co-ethnics can harm their long-term education outcomes highlights the importance of place-based policies. Such policies should complement the resources embedded within children's co-ethnic networks, potentially muting the negative impact of larger resource-scarce co-ethnic networks without imposing further restrictions on individuals' residential mobility. However, this requires further research on the mechanisms that drive the impact of co-ethnic networks on education outcomes and the identification of the most important (co-ethnic) resources, which is beyond the scope of the analysis presented in Chapter 4.

5.2 Main contributions

This dissertation contributes to our understanding of the causes income inequality and the potential of policies to address it. Each Chapter of this dissertation approaches the topic of income inequality from a different perspective, contributing to various strands of literature.

The first Chapter of this dissertation contributes to the vast body of literature studying the effects of the minimum wage – a key labor market institution aimed at addressing lower-tail income inequality. While previous research has predominantly focused on the general minimum wage, I study the effect of age-specific minimum wages, which are common in many countries. Moreover, I trace the impact of the minimum wage throughout the wage distribution, which allows me to identify spillover effects of the minimum wage. The second Chapter contributes to the literature on firm wage-setting power. I contribute to this literature by quantifying the degree of monopsony power in the Netherlands and investigating how this varies across different workers and industries. The third Chapter contributes to the literature on neighborhood exposure effects and intergenerational mobility. I contribute to this literature by focusing on the effect of a specific neighborhood characteristic – the concentration and earnings of individuals with a shared ethnic background – on the education outcomes of refugee children. By doing so, this Chapter additionally contributes to the literature on the effect of local ethnic concentration and the migrant assimilation more broadly.

More generally, each Chapter of this dissertation contributes to its respective literature by using high-quality data and state-of-the-art empirical designs. Throughout this dissertation, I make use of detailed administrative data collected by Statistics Netherlands, covering the entire Dutch population. This level of coverage and data granularity is often not available to researchers but is required for all of the questions that I seek to answer in this dissertation. Moreover, I use a variety of quasi-experimental empirical research designs to identify causal effects, which is essential to further our understanding of the causes of and remedies to income inequality.

5.3 Main Limitations and Directions for Future Research

Throughout this dissertation, I use rigorous empirical methodologies to establish causal effects. However, the main limitation common to each empirical investigation is the limited degree to which I can identify the underlying mechanisms that drive these effects. Identifying these causal mechanisms is important, both scientifically and for the design of public policies.

In Chapter 2, I find evidence of large spillover effects of the minimum wage. Such spillover effects can arise for several reasons, such a fairness concerns, reallocation of employment, restructuring of production, and job search. While I focus on aggregate labor market outcomes, future research should further utilize employer-employee matched data to investigate the impact of the minimum wage on individual workers and firms. Such research is necessary to answer questions that can inform us about the mechanisms that drive these spillovers. Examples of such questions are "Do spillover effects occur within or between firms?", "Do firms adjust their workforce composition and wage-structure?" and "How does the minimum wage affect workers' employment transitions?".

In Chapter 3, I document the substantial monopsony power of Dutch firms. While exploring how monopsony power varies across individuals and labor market segments helps to inform researchers on which potential mechanisms to explore, future research should seek to further disentangle the sources of monopsony power. Monopsony power can arise through a number of channels such as labor market concentration, search frictions, and heterogeneous worker preferences. Future research should further investigate how these channels operate and relate to each other, as well as how these mechanisms may differentially affect subgroups of workers. In addition, while I quantify the degree of potential monopsony power, future research should further investigate how much of this monopsony power is exercised by firms and how institutions (e.g., unions or minimum wage policies) can act as barriers to potentially harmful effects.

In Chapter 4, I show that neighborhood co-ethnic concentration affects the education outcomes of refugee children and emphasize the moderating role of co-ethnic earnings. To further our understanding of how co-ethnic networks influence childrens' human capital accumulation, future research should use more fine-grained and direct measures of co-ethnic networks to identify the most influential ethnic peers. In addition, I use co-ethnic earnings as a proxy for the (social) resources – or ethic capital – embedded in the co-ethnic network. Future research should investigate which resources are the most important moderating factors, exploring different dimensions of ethnic capital.

Another limitation, which mostly pertains to Chapters 2 and 4, relates to the generalizability of my findings. Chapter 2 studies the impact of an age-specific minimum wage increase. While relevant in its own right, such a minimum wage increase may have a more limited impact on firms' behaviour compared to an increase in the general minimum wage, as wages paid to young workers often account for a relatively small share of firms' total labor costs. Chapter 4 studies the impact of neighborhood ethnic composition and education outcomes of refugees in in context where neighborhood co-ethnic concentration is generally low. Future research should investigate the generalizability of my findings to different groups of migrants and cases in which co-ethnics comprise a more sizable share of the neighborhood population.

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Nederlandstalige Samenvatting

Over de afgelopen decennia zijn de economische baten van sterke economische groei in veel economisch ontwikkelde landen niet evenredig verdeeld in de samenleving. Er is een brede trend van stijgende inkomensongelijkheid, waarbij degenen aan de top van de inkomensverdeling hun lonen snel hebben zien stijgen, terwijl de loongroei aan de onderkant en in het midden van de inkomensverdeling relatief beperkt is geweest. Inkomensongelijkheid toont ook een sterke persistentie over generaties, waarbij kinderen van ouders die onderaan de inkomensverdeling staan minder kans hebben om zelf een hoog punt in de inkomensverdeling te bereiken. De sterke negatieve associatie tussen inkomensongelijkheid en intergenerationele inkomensmobiliteit, wekt verdere zorgen dat ongelijkheid vandaag de dag niet alleen blijft bestaan, maar ook verder wordt versterkt van de ene generatie op de andere.

Hoewel enige mate van inkomensongelijkheid als stimulans kan dienen voor mensen om te investeren in hun menselijk kapitaal, hard te werken en te innoveren, kan overmatige en diepgewortelde inkomensongelijkheid de economische groei beperken, sociale samenhang aantasten en het vertrouwen in instituties ondermijnen. Bovendien kan inkomensongelijkheid een signaal zijn van kansenongelijkheid, waarbij sommige groepen in de samenleving steeds meer achterblijven. Vanwege de potentiële impact op vele facetten van de samenleving is het van belang dat we de onderliggende oorzaken van inkomensongelijkheid in kaart brengen en onderzoeken hoe beleidsmaatregelen en instituties overmatige inkomensongelijkheid tegen kunnen gaan. In dit proefschrift onderzoek ik twee mogelijke drijfveren van inkomensongelijkheid: de marktmacht van werkgevers op de arbeidsmarkt en blootstelling aan buurtkenmerken tijdens de jeugd. Daarnaast onderzoek ik de effectiviteit van een belangrijke arbeidsmarkt institutie – het minimumloon – bij het aanpakken van inkomensongelijkheid.

In dit proefschrift maak ik gebruik van gedetailleerde administratieve data verzameld en vertrekt door het Centraal Bureau voor de Statistiek (CBS). Voor hoofdstukken twee en drie maak ik gebruik van gegevens gebaseerd op de polis administratie van het UWV. Deze databron bevat informatie over lonen, gewerkte uren en verschillende baankarakteristieken voor alle banen in Nederland. Voor hoofdstuk vier, dat focust op onderwijsuitkomsten, gebruik ik informatie over onderwijsinschrijvingen en behaalde diploma's gebaseerd op de registers van de Dienst Uitvoering Onderwijs. Informatie over verschillende demografische kenmerken, adres- en huishoudensgeschiedenis zijn gebaseerd op bevolkingsregisters.

Voor elk onderzoek in dit proefschrift maak ik gebruik van quasi-experimentele onderzoekstechnieken om causale effecten te onderzoeken. In hoofdstuk twee, waarin ik het effect onderzoek van een verhoging in het Nederlands jeugdminimumloon voor 20-22-jarigen op de arbeidsmarktuitkomsten voor deze leeftijdsgroepen, gebruik ik een *difference-in-differences* methode. In hoofdstuk drie, waarin ik de marktmacht van bedrijven op de Nederlandse arbeidsmarkt onderzoek, gebruik ik een *instrumental variable* methode in combinatie met een *matched event-study* methode. In hoofdstuk vier, waarin ik de relatie tussen buurtkarakteristieken en onderwijsuitkomsten onder vluchtelingen onderzoek, maak ik gebruik van een *natural experiment* waarin de (quasi)willekeurige toewijzing van vluchtelingen aan buurten tot exogene variatie in buurtkarakteristieken leidt. Voor een uitgebreide beschrijving van elke methode verwijs ik naar de betreffende hoofdstukken in dit proefschrift.

Dit proefschrift bestaat uit drie opzichzelfstaande onderzoeken, die het overkoepelende thema van inkomensongelijkheid vanuit een ander perspectief benaderen en die afzonderlijk van elkaar gelezen kunnen worden. Elk onderzoek maakt verschillende contributies aan de gerelateerde literatuur. Voor een gedetailleerde bespreking van de relevante literatuur en de bijdragen van elk onderzoek verwijs ik naar de betreffende hoofdstukken van dit proefschrift. In zijn geheel draagt dit proefschrift bij aan de kennis over de onderliggende drijfveren van inkomensongelijkheid en de effectiviteit van beleidsmaatregelen om overmatige inkomensongelijkheid tegen te gaan door gebruik te maken van administratieve data van hoogstaande kwaliteit en geavanceerde empirische methodes. De dekking en nauwkeurigheid van de gebruikte gegevens is vaak niet beschikbaar voor onderzoekers, maar is vereist voor alle vragen die ik in dit proefschrift tracht te beantwoorden. Bovendien maak ik gebruik van verschillende quasi-experimentele empirische methodes om causale effecten te identificeren, wat essentieel is om ons begrip van de oorzaken van en potentiële beleidsmaatregelen tegen inkomensongelijkheid verder te vergroten.

Hieronder geef ik een korte samenvatting van elk hoofdstuk en bespreek ik de belangrijkste bevindingen.

5.1 Minimumjeugdlonen en arbeidsmarktuitkomsten

Door een ondergrens voor lonen aan de onderkant van de loonverdeling vast te stellen, is het minimumloon een belangrijk beleidsinstrument om inkomensongelijkheid aan de onderkant van arbeidsmarkt tegen te gaan. Echter, vanwege de verhoogde arbeidskosten voor werkgevers, kunnen minimumlonen ook de vraag naar laagbetaalde arbeid verminderen, wat een nadelig effect op de werkgelegenheid van getroffen werknemers kan hebben. In hoofdstuk twee onderzoek ik, samen met mijn coauteurs, het causale effect van een 15-19% verhoging van het leeftijdsgebonden minimumloon voor 20-22-jarigen in Nederland – die plaatsvond in juni 2017 – op de arbeidsmarktuitkomsten voor deze leeftijdsgroepen.

Wij maken gebruik van een *difference-in-differences* methode waarin we de arbeidsmarktuitkomsten van 20-22-jarigen vergelijken met die van iets oudere leeftijdsgroepen. Specifiek kijken we naar veranderingen in het aantal banen en gewerkte uren op verschillende punten in de loonverdeling. Hierdoor kunnen we het effect van de verhoging van het jeugd-minimumloon over de gehele leeftijdsspecifieke loonverdeling traceren.

Ons onderzoek laat zien dat de verhoging van het jeugd-minimumloon niet heeft geleidt tot een daling in het aantal banen en gewerkte uren onder 20-22-jarigen. We documenteren een sterke daling in het aantal banen en werkte uren tegen lonen onder het nieuwe minimumloon, hetgeen verwacht wordt als werkgevers zich houden aan het nieuwe minimumloon. Deze daling werd echter volledig gecompenseerd door een stijging in het aantal banen en gewerkte uren tegen het nieuwe minimumloon en tegen lonen die net boven het minimumloon liggen. We vinden aanzienlijke 'overloopeffecten' van de minimumloonsverhoging: het aantal banen en werkte uren tegen lonen tot €4.00 boven het nieuwe minimumloon namen toe. Deze overloopeffecten zijn met name geconcentreerd in laagbetaalde banen, met lonen tot €2.50 boven het nieuwe minimumloon.

We vinden geen bewijs dat de verhoging van het jeugd-minimumloon heeft geleid tot een afname in contractkwaliteit of nadelige werkgelegenheidseffecten onder verschillende groepen werknemers of in bepaalde sectoren. Echter, het aantal gewerkte uren door niet-studenten en werknemers met een voltijdbaan nam toe, wat suggereert dat het beleid een positiever effect had op de werkgelegenheid voor werknemers die afhankelijk zijn van laagbetaalde banen voor hun levensonderhoud.

Samengevat laat ons onderzoek zien dat de verhoging van het minimumloon voor 20-22-jarigen niet heeft geleid tot een afname in werkgelegenheid terwijl de lonen aan de onderkant van de loonverdeling zijn toegenomen voor deze leeftijdsgroepen.

5.2 Monopsonie in Nederland: een methode gebaseerd op werknemer transities tussen bedrijven

Monopsonie macht verwijst naar een situatie waarbij werkgevers marktmacht bezitten op de arbeidsmarkt, die hen in staat stelt om lagere lonen te betalen dan het gangbare marktloon, zonder een aanzienlijk deel van hun werknemers te verliezen. Anders dan in een arbeidsmarkt gekarakteriseerd door perfecte concurrentie hebben (sommige) werkgevers in een arbeidsmarkt gekarakteriseerd door monopsonistische concurrentie loonzettingsmacht. Deze loonzettingsmacht kan bijdragen aan inkomensongelijkheid als werkgevers meer loonzettingsmacht hebben over bepaalde groepen werknemers of wanneer arbeid van bepaalde groepen werknemers onevenredig geconcentreerd is in sectoren of beroepen die worden gekenmerkt door sterkere mate van loonzettingsmacht.

In hoofdstuk 3 schatten mijn coauteur en ik de arbeidsaanbodelasticiteit aan Nederlandse bedrijven – een veelgebruikte maatstaf om loonzettingsmacht te kwantificeren. Daarnaast onderzoeken wij in hoeverre deze elasticiteit varieert tussen verschillende soorten werknemers en sectoren. Om arbeidsaanbodelasticiteiten te schatten, gebruiken we een *event-study* methode, ontwikkeld door Bassier et al. (2022).

Met deze methode vergelijken we werknemers met een sterk overeenkomend arbeidsmarktverleden die in hetzelfde kwartaal bij dezelfde 'oorspronkelijke' werkgever vertrekken naar verschillende 'tussentijdse' werkgevers. Vervolgens onderzoeken we hoe gevoelig de kans dat deze werknemers ook bij de 'tussentijdse' werkgevers vertrekken is voor de loonverandering ervaren tijdens de initiële transitie. Hierbij vergelijken we de vertrekkans van de vergelijkbare werknemers die tijdens de initiële transitie verschillende loonveranderingen hebben ervaren. Om het deel van het verschil in loonveranderingen te isoleren dat toe te wijzen is aan verschillen in loonzettingsbeleid van verschillende 'tussentijdse' bedrijven, instrumenteren we de loonverandering tijdens de initiële transitie met de verandering in het gemiddelde loon van collega's bij de 'oorspronkelijke' werkgever en de verschillende 'tussentijdse' werkgevers. We gebruiken de geschatte vertrekelasticiteiten met betrekking tot het werkgeversaandeel in lonen om de arbeidsaanbodelasticiteit aan Nederlandse bedrijven af te leiden.

Met behulp van gegevens over de periode 2010-2021 vinden we bewijs van aanzienlijke loonzettingsmacht van werkgevers op de Nederlandse arbeidsmarkt, met een arbeidsaanbodelasticiteit aan Nederlandse bedrijven van 7.24. Deze schatting suggereert dat werkgevers lonen potentieel kunnen verlagen ten opzichte van de marginale productiviteit van werknemers met ongeveer 12%. We vinden dat de arbeidsaanbodelasticiteit varieert per geslacht, met een lagere arbeidsaanbodelasticiteit voor vrouwen dan voor mannen. Dit suggereert dat bedrijven potentieel lonen meer kunnen verlagen voor vrouwen, wat mogelijk bijdraagt aan de loonkloof tussen mannen en vrouwen. Bij het verkennen van heterogeniteit over de loonverdeling vinden we dat werknemers aan de onder- en bovenkant van de loonverdeling de laagste arbeidsaanbodelasticiteit hebben. Echter, mogelijke verlagingen zijn meer dan twee keer zo groot voor de hoogst verdienende werknemers. Dit suggereert dat loonzettingsmacht van bedrijven het potentieel heeft om lonen voor laagbetaalde werknemers te drukken, wat bijdraagt aan hogere inkomensongelijkheid, maar ook de kan leiden tot een compressie van de loonverdeling vanaf de bovenkant.

5.3 Nieuw thuis, oude buren? Etnische enclaves en de onderwijsuitkomsten van vluchtelingen

Vluchtelingen staan vaak voor grote barrières met betrekking tot economische en maatschappelijke integratie en lopen vaak achter op autochtonen wat betreft onderwijs- en arbeidsmarktresultaten. Vluchtelingen, en migranten in bredere zin, hebben de neiging zich te vestigen in buurten met een relatief hoge concentratie van andere migranten, vooral die met een vergelijkbare etnische achtergrond –zogenoemde etnische enclaves. Er bestaat een wijdverbreide zorg dat de segregatie van vluchtelingen in dergelijke etnische enclaves hun (arbeidsmarkt) integratie belemmert, wat bij kan dragen aan het loonverschil tussen vluchtelingen en autochtonen.

In Hoofdstuk 4 onderzoek ik of de concentratie en looninkomsten van personen met eenzelfde etnische achtergrond in de buurt waar mensen wonen invloed heeft op de onderwijsuitkomsten van vluchtelingenkinderen. Onderwijsuitkomsten zijn van belang, omdat ze van grote invloed zijn op arbeidsmarktuitkomsten op de lange termijn. Om het causale effect van co-etnische concentratie en co-etnische looninkomsten in de buurt te onderzoeken maakt ik gebruik van een vluchtelingen-spreidingsbeleid in Nederland tussen 1999 en 2009. Onder dit vluchtelingenspreidingsbeleid werden vluchtelingen (quasi)willekeurig toegewezen aan hun eerste woning buiten de asielopvang. Dit zorgt voor zo goed als willekeurige variatie in de oorspronkelijke demografische samenstelling van de buurt van toewijzing.

Dit onderzoekt toont aan dat de invloed van co-etnische concentratie in de buurt van toewijzing op onderwijsuitkomsten onder vluchtelingenkinderen afhankelijk is van looninkomsten van personen met eenzelfde etnische achtergrond. Toewijzing aan een buurt met een hogere concentratie van personen met eenzelfde etnische achtergrond heeft een negatieve invloed op de onderwijsresultaten van vluchtelingen op de lange termijn wanneer de co-etnische looninkomsten laag zijn, terwijl een hogere co-etnische concentratie een positief effect heeft op onderwijsresultaten wanneer de co-etnische inkomsten hoog zijn. Bovendien toont dit onderzoek aan dat hogere looninkomsten onder co-etnische buren een positief effect heeft op onderwijsuitkomsten en dat dit positieve effect versterkt wordt wanneer de co-etnische concentratie hoog is.

Curriculum Vitae

Emiel van Bezooijen was born in Voorburg, the Netherlands, in 1996. He obtained a Bachelor's degree in Economics and Business Economics at Utrecht University School of Economics (U.S.E) in 2017. After his Bachelor's degree, Emiel continued his education at U.S.E, following a two-year Research Master program in Multidisciplinary Economics. He wrote his master thesis while being an research intern at Netherlands Bureau for Economic Policy Analysis (CPB). After graduating from the Research Master cum laude in 2019, Emiel started as a PhD Candidate at U.S.E. In the spring of 2022, Emiel was a visiting researcher at Aarhus University's Department of Economics and Business Economics in Aarhus (Denmark). As of August 2023, Emiel works as a researcher at the Knowledge and Innovation program at the CPB.

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