Topics on Interactions of Public Policies and Labor Markets

by

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Vienna, 31 August 2025

Lili Katalin Márk

Disclosure of co-author contribution

Chapter 2: The Incentive Effects of Sickness Benefit for the Unemployed – Analysis of a Reduction in Potential Benefit Duration

Joint work with Márton Csillag

The original idea of the paper came from Márton Csillag and I was involved in the project from the beginning. We both contributed equally to developing empirical strategies and directions of the project. I wrote most of the codes for data cleaning and data analysis. Márton wrote the first draft of the paper, which I later edited and added new sections to.

Chapter 3: Firm Heterogeneity and the Impact of Payroll Taxes

Joint work with Anikó Bíró, Réka Branyiczki, Attila Lindner and Dániel Prinz

Anikó and Réka came up with the idea for this paper. Anikó, Dániel, Réka, and I developed the original concept. Anikó, Réka, and I contributed equally to writing the codes for the empirical analysis. Réka and I reviewed the literature. Attila contributed to the theoretical part and provided insights about the direction of our research. All five of us contributed to designing the empirical analysis and writing the paper.

Abstract

In my thesis, I examine how public policies targeting specific groups affect labor market outcomes in Hungary using administrative data.

My first chapter focuses on the labor market participation among mothers in the years following childbirth. I estimate the substitution effect of paid parental leave – that is, the distortionary effect of losing all benefits if a mother chooses to work – by evaluating a Hungarian reform. In 2014, restrictions on working for mothers receiving parental leave benefits were abolished for children aged one to two. As a result, the monthly employment rate of affected mothers increased by 3.2 percentage points. However, most mothers still stay home for reasons other than the estimated substitution effect, such as the income effect of paid parental leave, social norms, and the unavailability of childcare. Descriptive evidence suggests that, for those who complied with the policy, an earlier return to work may have increased the likelihood of remaining with their previous employer beyond the period of job protection, which could improve their labor market trajectories.

In chapter 2 (joint with Márton Csillag), we analyze the impact of a unique "sickness benefit for the unemployed" on benefit claiming and employment. In Hungary, employees could claim sickness benefits within three days of losing their job, which enabled them to extend their potential benefit duration by 90 days during their nonemployment spell. This provided a huge incentive to report sick at the onset of unemployment. In 2007, the maximum number of days of "sickness benefit for the unemployed" was halved. First, we demonstrate that higher-income individuals and workers with longer employment histories were more likely to claim sickness benefits, even when controlling for various health variables. These groups benefit most from using sickness benefits instead of unemployment benefits. Second, we find that a large portion of lost sickness benefit days were substituted by taking unemployment insurance benefits. Third, we demonstrate that the reform decreased the job-finding rate right after the pre-reform maximum duration and increased the job-finding rate right after the new maximum.

In Chapter 3 (joint with Anikó Bíró, Réka Branyiczki, Attila Lindner and Dániel Prinz) we study the impact of a large payroll tax cut for older workers on employment and wages in Hungary. By exploiting administrative data and applying a difference-in-differences empirical strategy, we document a modest employment increase equivalent to a labor demand elasticity of -0.3 and pass-through rate

of 22%. These average effects mask large heterogeneity across firms. Employment mainly increases at low-productivity, low-paying firms, while no jobs are created at high-productivity, high-paying firms. At the same time, the tax cut is passed through to wages at high-productivity, high-paying firms, while low-productivity, low-paying firms do not share the benefits of the tax cut with their workers. These results point to important heterogeneity in the incidence of payroll tax cuts across firms, highlighting that workers at different firms benefit differently from payroll taxes. They also demonstrate that payroll taxes can have a significant impact on the composition of jobs in the labor market.

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1.1 Introduction

As the extensive literature on the relationship between parental leave policies and maternal employment shows, these policies significantly impact mothers' short-term labor market participation (e.g. Lalive et al., 2014). Extended periods of leave can have negative, unintended consequences for mothers in the labor market, such as decreased human capital accumulation or discrimination against women of childbearing age, resulting in lower employment chances and wages (Turon, 2022). Consequently, from a policy perspective, it is important to help mothers remain connected to the labor market while on parental leave. Parental leave schemes typically require mothers to stay at home and care for their child. In this paper, I examine a unique policy that financially incentivizes mothers to work by allowing them to work full-time while receiving parental leave benefits.

I exploit a reform in Hungary's parental leave rules in 2014, called the "Paid Parental Leave Extra" (or PPL Extra for short), which relaxed restrictions on working while someone is on paid parental leave and allowed mothers to work full time after their child turned one, while still receiving the parental leave benefit. Previously, a mother could only work after her child turned two without losing a substantial amount of parental leave benefits. Compared to a counterfactual scenario based on the previous rules, this reform effectively provided mothers with an average wage increase of approximately 60% during the second year of a child's life. The original motivation for the reform was to increase maternal employment while children are young, without hurting families by cutting benefits.[1] The reform was also accompanied by an increase in state nursery capacities.

Using the Hungarian social security records (the "Admin 3" database) and an event-study design, I find a temporary 3.2 percentage point (pp) increase in the average monthly employment rate during the second year of a child's life, compared to the baseline rate of 9.8%. Although this is not a negligible impact in relative terms (32% increase), the main employment patterns for mothers do not change substantially. When I examine the employment trajectories of treated (post-reform) and control (pre-

[1] The government that made this policy change – still in power since 2010 – has made "family-friendliness" and "work-based economy" as two of their flagship objectives.

reform) mothers relative to their pre-birth employment and their counterfactual employment absent children, I observe that the overall employment patterns surrounding childbirth remain similar, with the majority of mothers not working until their child's second birthday. Examining the evolution of this employment increase by the child's age reveals an increasing employment effect ranging from 1.7 percentage points (pp) at the 13th month to 5.4 pp at the 23rd month.

My results on employment increases during the second year of the first child of a mother remain robust when different pre- and post-reform cohorts of mothers are included in the analysis and for different sample definitions, e.g. where I only keep mothers with one child, or mothers who do not have a second child within 3 years. Employment impacts beyond the third birthday of the first child of a mother seem to be related to having a second child or not. In my baseline specification, there is an insignificant, decreasing employment effect for the 3rd to 5th years that may be related to rising fertility rates during this period. Some robustness checks suggest that mothers with only one child may experience a small, medium-term employment increase (between the third and fifth birthdays). However, the causal interpretation of overall impacts beyond the second year is questionable due to increased fertility in the third year and the endogeneity of fertility and employment decisions of mothers.

As a result of the reform, many mothers returned to the labor market earlier than they would have under the old rules. I examine whether this earlier return might have longer-term consequences for their labor market outcomes. However, I also find an impact on fertility in the third year that causes compositional differences among working mothers, so causal interpretation is not possible. By decomposing the main employment effect into employment at their previous employer vs. a new one, I demonstrate that the increase in employment is due to mothers returning to their previous employers, which seems to have a permanent impact of staying with that employer for up to five years. I present further descriptive evidence on the earlier return of mothers to the labor market and its potential association with staying with the pre-birth employer with a higher chance. My results suggest that mothers who return to work early are more likely to stay with their pre-birth employer, even after job protection ends. This may result in higher wages and further advantages. In a follow-up paper, Bíró et al. (2025) provide causal evidence of the earlier return on the labor market trajectories of mothers.

My paper contributes to the literature in several ways. First, analyzing this type of reform allows us to measure the substitution effect of paid parental leave. The substitution effect is the distortionary impact of paid parental leave due to the kink in the budget constraint at zero hours of work, which means a mother must give up all the benefits if she decides to work any hours. In the Hungarian context, 3.2% of mothers are out of the labor market before their child's second birthday due to the substitution effect, as also shown by our follow-up paper (Bíró et al., 2025). Our results also show that around 80% of mothers do not participate in the labor market in the first two years after childbirth for reasons other than the substitution effect. These reasons include the income effect of paid parental leave, social norms, and the limited local availability of childcare.

Second, beginning in the 2000s, several Central and Eastern European (CEE) countries, including the Czech Republic in 2004, Romania in 2007, and Slovakia in 2011, relaxed return-to-work restrictions associated with paid parental leave. However, my study and our follow-up paper (Bíró et al., 2025) are the first to study the impact of such reforms. My paper is most closely related to a small set of papers examining reforms that provide similar, albeit not identical, financial incentives for mothers to return to employment earlier (Baertsch and Malte (2024) – Germany, Bičáková and Kalíšková (2019), Mullerova (2016), and Pertold-Gebicka (2020) – the Czech Republic, Ziegler and Bamieh (2023) – Austria,). These countries (Austria, the Czech Republic, and Germany) all have strong social norms regarding gender roles and maternal employment. The key motivation behind the policies analyzed in these papers is to provide mothers with a faster pathway back to the labor market without giving up parental leave benefits if they choose to return earlier. Thus, policymakers in these countries likely seek alternatives to reducing the duration of parental leave. In Austria and the Czech Republic, this is achieved by introducing flexible parental benefit schemes that allow mothers to receive the same total amount of benefits over a shorter period, resulting in higher monthly benefits. The impact on maternal employment is minimal in Austria (Ziegler & Bamieh, 2023), but substantial in the Czech Republic (Mullerova, 2016). Baertsch and Malte (2024) analyze a reform closer to the Hungarian case. This reform relaxes restrictions on working while on parental leave and allows mothers to work during paid parental leave while keeping the total benefit amount constant and increasing the duration of paid parental leave. The authors find that high-income mothers have a 3 percentage point (approximately 15%) increase in the probability of returning to work before their child's first birthday.

Third, I present evidence regarding maternal employment and parental leave from a CEE country, where causal evidence is scarce (Bičáková & Kalíšková, 2022). Some of these countries, like Hungary, provide the longest paid parental leave in the world, along with countries such as the Czech Republic, Estonia, and Slovakia. Most existing papers on parental leave policies investigate those in Western European countries, where the parental leave benefits are more restrictive (see for example Bergemann & Riphahn, 2022; Dahl et al., 2016; Lalive et al., 2014; Lalive & Zweimüller, 2009; Schönberg & Ludsteck, 2014). Some of the few existing causal estimates related to parental leave policies in CEE countries estimate the impact of the extension of paid parental leave from three to four years in the Czech Republic (Bičáková & Kalíšková, 2019; Mullerova, 2016) and these papers find much larger employment impacts than studies from Western Europe that analyze reforms with shorter leave durations (Lalive & Zweimüller, 2009; Schönberg & Ludsteck, 2014) suggesting that analyzing parental leave policies at different leave durations might provide useful and new insight.

Furthermore, the economic literature on Hungarian parental leave policies is limited due to a lack of data from the 1990s, when significant policy changes occurred. Bálint and Köllő (2008) analyze the impact of parental leave policies on female labor market outcomes using the Labor Force Statistics. Due to these limitations, however, they cannot uncover causal relationships and mostly provide a descriptive analysis of the topic. They highlight that, although the original goal of parental leave was to support job-protected leave for mothers and thus help reconcile work and family life, by the end of the 2000s, most recipients were loosely attached to the labor market, and parental leave policies provided strong incentives to stay away from the labor market for extended periods. A few papers address the historical development of Hungarian parental leave policies, highlighting their role in maintaining traditional gender norms in the country (Göndör, 2012; Ignits & Kapitány, 2006).

The remainder of this chapter proceeds as follows. The next section, "Institutional background" (Section 1.2), provides a summary of maternal employment, child penalties, social norms, and forms of childcare in Hungary. It also summarizes family leave policies, the broader policy context in Hungary, and the details of the "Paid Parental Leave Extra" reform. In Section 1.3 I introduce the data and our sample restrictions. Section 1.4 describes the empirical strategy and discusses the identification assumptions. Section 1.5 presents my main results, robustness checks, and heterogeneity analyses. Section 1.6 offers an overview of the mechanisms of the reform's impact, including its effect on fertility, illustration of the earlier return of mothers to work, and its implications for staying with the earlier employer, a simple complier analysis, an explanation of the substitution and income effect of paid parental leave, and an analysis of a potentially unintended consequence of the policy change: the increased parental leave benefit take-up of full-time working fathers. Finally, I conclude in Section 1.7.

1.2 Institutional background

1.2.1 Female and maternal employment and child penalties in Hungary

There has been a steady increase in the female employment rate in Hungary since 2010 from below the EU average (54.3% vs. 56.8% in 2010) to well above it (68.2% vs. 63.4% in 2021). However, as Köllő (2018) points out the substantial growth in both female and male employment from 2010 is largely due to the widespread public work program introduced during this period, the rise in foreign employment and the shrinking working-age population. The employment rate of mothers whose youngest child is less than 3 years old is much lower than in most other European countries (OECD Family Database 2021, Chart LMF1.2.F[2] In Hungary, the employment rate for this group was around 10% (excluding those on maternity leave) and 20% (including them) until 2021. These figures are similar to those in the Czech Republic and the Slovak Republic, where parental leave policies are comparably generous. For comparison, the employment rate of mothers of young children who are employed and not absent on parental leave was 32.3% in Austria, 38.8% in Germany, 45.3% on average across the EU, and 56% in the United States in 2021. Overall, the employment rate of mothers with young children appears to be closely linked to a country's parental leave policies, as most mothers tend to remain on leave for the full duration available.

The overall gender gap in employment has been close to the OECD average and above that of most European countries since 2010 fluctuating between 10 and 14 percentage points during these years while both male and female employment rates increased (OECD Family Database). As in most European countries, nearly the entire gender gap – 88% – in employment can be attributed to the child

[2] Interpretation of employment rates for mothers of young children (under age 3) in the OECD Family Database requires caution, as countries vary in how they classify workers on maternity or parental leave. Until 2021, Hungary and the Czech Republic did not count mothers on maternity or parental leave as employed, but this changed in 2021, resulting in a sharp increase in their employment rates. In contrast, Austria and Germany had already counted such mothers as employed before 2021, meaning that earlier cross-country comparisons overstated the differences in the share of mothers actively working and not on leave. A decomposition of total employment rates into (1) employed and not absent on leave, (2) employed and absent on maternity leave, and (3) employed and absent on parental leave is only available from 2021. This decomposition reveals, for example, that the similar overall employment rates in Germany (61.7%) and the United States (58.8%) in 2021 mask a key difference: in Germany, 22.9 percentage points of the 61.7% are in fact on maternity or parental leave, while this is only 2.8 percentage points in the U.S.

penalty (compared to 100% in Austria and the Czech Republic, Child Penalty Atlas). The gender pay gap is also close to the OECD average and is exceptionally high among workers with tertiary education (the second highest among all OECD countries).

The child penalty[3] in employment for mothers is much larger during the first two years after childbirth than in Europe and the US (89% vs. 32% in Europe and 23% in the US), but it is becomes very similar by the seventh year since the birth of the first child (see Panel (a) of Appendix Figure A.1.1). The annual pattern of the child penalty reflects the parental leave policies in different countries and it shows a similar trajectory in Hungary, the Czech Republic and the Slovak Republic, where mothers are also entitled to three years of parental leave (see Panel (b) of Appendix Figure A.1.1). Although parental leave is shorter in Germany and in Austria, child penalties are pretty similar to those observed in Hungary (see Panel (c) of Appendix Figure A.1.1). Despite comparable lengths of paid parental leave in Scandinavian countries and in Austria and Germany, child penalties are much lower in the Scandinavian countries, suggesting the importance of additional factors – such as the availability of child care and prevailing social norms – in explaining variation in child penalties.

1.2.2 Family leave policies in Hungary

History of paid maternity and parental leave The origins of parental leave policies date back to the socialist regime in Hungary. *Paid maternity leave* (for six months) was available for mothers already from 1955[4] and it replaced 100% or 50% of the previous wage depending on the length of the employment history. Maternity leave was extended until the child was 2.5 years old in 1967 as a reaction to a few challenges the government faced (Göndör, 2012). First, increased labor market participation of women brought challenges in reconciling work and family life, similar to trends in other European countries. Second, the end of the 60s saw a substantial inflow to the labor force due to the restrictions on abortion in the beginning of the 50s (the so-called "Ratkó-era"). Paid maternity leave offered an official status for mothers outside employment helping to manage the excess labor supply. Third, paid parental leave also offered a cheaper and quicker solution for the lack of nursery capacities. Fourth, the government also hoped that paid parental leave would lead to higher fertility (Makay, 2017). Hungary

[3] The child penalty is defined as the difference in employment due to the birth of a first child relative to counterfactual employment, accounting for calendar time and age (Kleven et al., 2019).

[4] A much shorter unpaid maternity leave was available since 1884 and a shorter paid maternity leave was introduced in 1927 (Tárkányi, 2001)

was the first European country to introduce paid maternity leave for such a long period and other countries only followed decades later (Makay, 2017). The strengthening of traditional gender roles with the early introduction of generous paid maternity leave contrasted with the more egalitarian culture that was typical in other socialist Eastern-European countries (Akbulut-Yuksel et al., 2025; Boelmann et al., 2025; Fuchs-Schündeln & Schündeln, 2006).

As of the 1967 reform, a flat *maternity leave allowance (MLA)* was offered to mothers who had at least 12 months of employment history during the previous 1.5 years before giving birth starting after the first six months of maternity leave and lasting until the child was 2.5 years old. The allowance amounted to 40% of the average wage of mothers and included job protection. There were several modifications to the original scheme over the next two decades, mostly reflecting short-term government goals; for example, when the government realized there were not enough kindergarten places available, they extended the maximum duration to three years. Eligibility was gradually extended to mothers with less employment history and part-time employment was allowed during maternity leave after certain age threshold of the child (1.5 years old). Fathers became eligible for the *parental leave allowance (PLA)* as well in 1982 after their child turned one year old (or earlier if they were single parents). From this point onward I will refer to it as *paid parental leave / parental leave allowance* instead of *paid maternity leave / maternity leave allowance*. However, the vast majority of people on *paid parental leave* were still mothers. Paid parental leave soon gained popularity after its introduction and most mothers used it.

Paid parental leave (that is available to either the father or the mother after the child turns 1 year old) with a *parental leave benefit (PLB)* offering a replacement rate of 65-75% was introduced in 1985 for parents with long enough employment histories for up to 1.5 years ans was then extended to 2 years in 1988. Working was not allowed during someone received PLB. Parental leave benefit was temporarily eliminated for the years 1996-1999 (by the so-called "Bokros-package") and then reintroduced in 2000 (Bálint & Köllő, 2008). The earlier flat *parental leave allowance* (for up to 3 years) was tied to having sufficient employment history until 1995, then it became means-tested for a short period and it has been a universal transfer since 1999 until today. Since 1999, one could work full-time from home while receiving *parental leave allowance*, since 2006 full-time employment was allowed without restrictions. Since then the main elements and rules of eligibility of family leave policies were mostly unchanged until recent years. It is important to note, that the nominal amount of the *parental leave allowance* has been 28,500 HUF since 2008 so the importance of this transfer among all family-related policies has been decreasing and the insurance-based *parental leave benefit* that is tied to the earlier wage became more desirable.

To sum up, paid maternity leave has a long history in Hungary and the initial introduction was partly motivated by short-term political goals in an *ad hoc* manner. The original flat maternity (later parental) leave allowance became universal in 1999 so it effectively became a social transfer rather than what the original idea was, which was to provide job-protected parental leave. There is still job protection for 3 years after birth, which normalizes full-time motherhood as a desirable and acceptable role for working women, but it is generally allowed to work after the exhaustion of the insurance-based *parental leave benefit* since the 2000s even if someone receives *parental leave allowance*.

Paid parental leave in the 2010s In this subsection I provide a summary of the family leave policies that were available for mothers giving birth in/after 2010, which will be the control period for our analysis.

Paid maternity leave is available for mothers for 24 weeks and it can be started 4 weeks prior to the due date at the earliest. A mother is eligible if she has at least 365 days[5] of employment during the 2 years before giving birth (or the due date). The benefit amounts to 70% of the previous wage.

After the exhaustion of paid maternity leave either parent can go on *paid parental leave (PPL)* that also provides a 70% replacement of the previous wage, but it is capped at the 70% · 2 · minimum wage for up to the second birthday of the child *(parental leave benefit, PLB)*. The eligibility criteria for PPL is to have 365 days of employment during the 2-year-period before the birth date of the child in case of mothers or before the start of eligibility in case of fathers.

Parental leave allowance (PLA) in the amount of 28,500 HUF is paid until the third birthday of the child. If a family is eligible for both PLB and PLA, the family can choose whichever option is best for them, so eligible families typically use PLB until age 2 or until the parent on leave returns to work and then switch to PLA until the child's third birthday.

Until 2014, working was not allowed while receiving PLB, and the number of weekly hours worked was capped at 30 hours for PLA. Before 2014 the main goal of PLB was to provide income replacement

[5]Before May 1, 2010, 180 days were enough for eligibility.

while parents cared for their children, and it was also not allowed to use institutional childcare services during the period of parental leave. If a family was eligible for parental leave transfers for multiple children at the same time, they had to choose which transfer they would like to receive.

1.2.3 The "Paid Parental Leave Extra" reform

Since 2014, under the PPL Extra reform package, parents receiving parental leave benefit or allowance were allowed to work full-time after their child turned 1[6]. The eligibility to work while receiving the benefit did not depend on the date of childbirth; however, the reform was grandfathered: anyone with a child aged 12 to 23 months at the time the reform came into effect was allowed to work while receiving parental leave benefit or allowance contrary to the preceding rules. For example, if a mother gave birth in June 2012, her child was 18 months old when the new rules started, so she still had 6 months during which she could work while keeping the benefit. I study the impact of this reform on maternal employment.

The original intention of the government was to provide incentives for mothers to return to work earlier, however, the reform had some unintended consequences that point in the opposite direction. Since the restrictions on employment while on parental leave were eliminated, fathers could go on paid parental leave while working full-time and mothers could still stay at home to take care of the child. If the father's income was higher this effectively increased the income a family could receive without the mother returning to work, thus providing further incentives for her to stay home. Furthermore, some families effectively became for the parental leave benefit in caes when the mother would not have been eligible for PLB because she was not employed or did not have enough employment history but the father was. Makay (2021) reports that by 2019 significantly more men received PLB than in earlier years and the share of men on PLB was highest in some disadvantaged municipalities of Hungary. Based on the Labor Force Survey in Hungary fathers' take-up only started increasing a few years after the reform (see Section 1.6.6 for details) and it likely did not play a significant role in the short-term impacts (for mothers giving birth in 2014 at the latest) of the policy, that I analyze.

The reform package had other elements as well. First, it allowed families to receive parental leave transfers for more children at the same time. This provided incentives to decrease the gap between the birth of siblings, as families did not need to strategically wait 2-3 years in order to maximize the

[6]This age threshold was decreased to 6 months in 2016.

benefits they receive and through this this may have had a fertility-increasing effect (see Section 1.6.1 for a discussion of the potential impact on fertility and Appendix Figure A.1.4 for the increasing trend in fertility at the time). Second, university students became eligible for the parental leave benefit.

1.2.4 Social norms in Hungary

The long history of paid parental leave in Hungary has shaped the current social norms regarding the role of mothers in the family and in the labor market. While the female employment rate increased substantially during the second part of the 20th century the introduction of paid maternity leave reinforced the role of mothers as the main caregivers in families effectively assigning double workload for mothers (Göndör, 2012). Today, the majority view in Hungarian society is that mothers should stay at home with their young children for 2 or 3 years. Based on a 2016 survey, 74% of the Hungarian population agreed that it is not acceptable for mothers to return to work before their child turns 3 years old (Makay, 2018). Bálint and Köllő (2008) report that the majority of mothers on parental leave (87.5%) wait until their youngest child turns 3 to return to work and mothers spend on average 4.7 years at home on parental leave based on the Labor Force Survey in 1993-2005.

Kleven (2022) uses the epidemiological approach to illustrate the role of social norms in explaining variations in child penalties across time and space, and shows that child penalties for immigrants in the U.S. from different countries closely reflect those of their origin countries'. Hungary's example (Kleven, 2022, Figure A.17) is striking as it shows that Hungarian immigrant mothers in the US tend to stay outside the labor force for 2-5 years after the birth of their first child, leading to one of the highest overall child penalties in employment among immigrant groups – even without the institutional setting that provides direct incentives to do so. It corroborates the mechanism by which old institutions shape social norms.

1.2.5 Forms of childcare in Hungary

In Hungary, state nurseries are primarily operated by municipalities. Access to these nurseries depends on parental employment, and fees are means-tested but cannot exceed 25% of the family's net per capita income (KSH, 2013).

In the 1990s, after the transition from the socialist regime, the expansion of institutional childcare was not a policy priority, largely due to persistently high unemployment over the next one to two decades. An increase in government funding in the late 2000s led to an increase in nursery school spots. Availability of nursery schools (measured by the number of spots over the number of children aged 0–2) grew from 10% in 2005 to 15% by 2011, marking the beginning of our period of interest. Since then, there has been a greater emphasis on opening new nurseries, especially due to the growing problem of skilled labor shortages in subsequent years. After 2011, during our sample period, nursery coverage increased moderately and reached a plateau of 17% by 2013.

This expansion was largely driven by family daycare centers, which are smaller facilities with fewer operating restrictions. These centers were aimed at increasing coverage in rural areas, and about 50% of new nursery places were in them. Between 2009 and 2017, kindergartens were also permitted to create mixed-age groups including up to five nursery-aged children per group. Furthermore, the maximum group size allowed by law increased from 10 to 12 in 2010, which alleviated pressure on nurseries to meet the rising demand

Overall, the expansion of institutional childcare before our main analysis period (2011–2017) significantly improved access to affordable, formal childcare for working mothers of young children. However, regional differences in coverage remain, with higher availability in larger cities than in smaller municipalities (Szabó et al., 2022).

1.3 Data and sample

We use linked employer-employee administrative data from Hungary, the "Admin3" database[7], compiled by the Databank of the Centre for Economic and Regional Studies. The dataset covers years 2003–2017 for a random sample of 50% of the population.[8] Data on births are collected monthly from 2009 onwards and include births that take place as part of in-patient care in public hospitals covered by the National Health Insurance [9], which account for 98% of all births in 2018 (Veroszta et al., 2022). I use monthly data for employment. An individual is defined as an employee if the pension authority records employment on the 15th of the month and they have positive earnings and/or if they are on

[7] Source: https://adatbank.krtk.mta.hu/en/adatbazisok/elerheto-adatbazisok/ (last opened: November 23, 2023)

[8] Monthly labor force status and wage indicators come from the Central Administration of National Pension Insurance. Demographic indicators and data on births are from the National Health Insurance Administration. The firm-specific indicators come from the National Tax and Customs Administration of Hungary.

[9] Births in private hospitals and home births are therefore not included in our data.

sick-leave.

Although both parents are eligible for insurance based paid parental leave, I focus on mothers in this analysis as I cannot identify fathers in our dataset. Furthermore, the take-up rate for mothers is much higher than for fathers. In 2016, 3,800 men were actually on paid parental leave, receiving either the parental leave benefit or the parental leave allowance, while this figure for women was around 232,000 (Köllő & Fazekas, 2018, p205). Third, the stated goal of the reform was to encourage earlier maternal return to work, and I focus on that outcome.

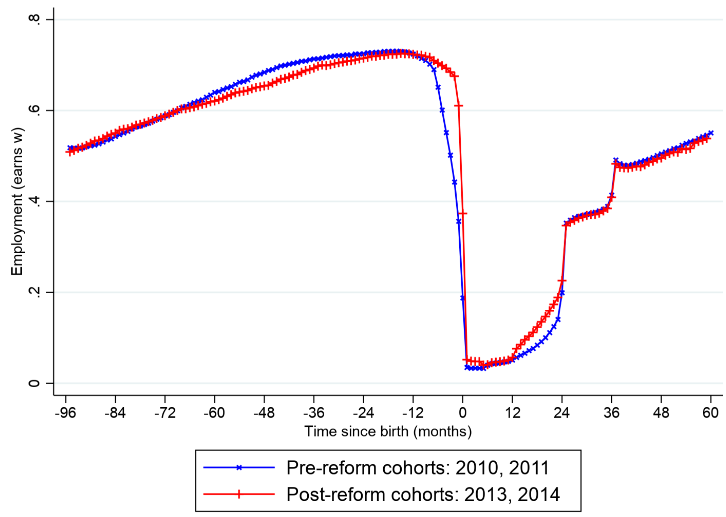
I restrict our sample to first births by searching for the first observed birth in the data (only available from 2009 onwards) and restricting the sample to those women who did not receive any childcare benefits during 2003-2009 or in the period when we do not actually observe birth events. I use cohorts of first-time mothers from 2010-11 (pre-reform) and 2013-14 (post-reform) in our estimations.

As I focus on women after the birth of their first child, some do not return to work because they extend their parental leave due to a second child; in fact, 12.5% of first-time mothers in our sample give birth to their second child within two years. Nevertheless, I focus on employment after a woman's first child, rather than after her last child. First, this is a standard approach in the child penalty literature which suggests that the timing of the first child has the most profound impact. Second, I also do this because it is easier to identify first births in our data with reasonable precision than last births, as I can use the preceding information on childcare transfers. Doing it the other way around, I would have a downward bias in my estimates because the proportion of "false" last births would increase as we go forward in time. For example, for someone who gives birth in 2010, I can follow her for the next six years and see if she has another child, but for someone whose last observed birth is in 2016, I can only follow for one year.

We exclude the 9-month period before giving birth (approximately the pregnancy period) and the first 6 months after birth because changes in the reporting method of sick leave for young mothers cause a large fraction of mothers to disappear from employment during pregnancy in the pre-reform years and/or appear as employed shortly after giving birth. This is illustrated in Figure 1.1, which shows the raw employment rates for pre- and post-reform mothers in our sample by months relative to the birth.

Although the reform mostly affected those parents who were eligible for the higher parental leave

Figure 1.1: Employment rate over monthly event time



Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009.

benefit, I do not restrict the sample for eligible mothers (appr. 75% of all mothers) in our baseline specification. First, the changing practice of recording sick-leave during pregnancy over our sample period makes it impossible to precisely identify eligible mothers. Second, there has been an increasing trend in female employment over our sample period, which lead to an increased share of eligible mothers and compositional changes among them. Third, I am interested in the net employment effect of the reform. An increased employment rate among eligible mothers could be accompanied with a decreased employment rate among the non-eligible mothers due to spillovers.

Figure 1.1 shows the raw employment rates by months relative to the month of giving birth to the first child, separately for the pre-reform and post-reform cohorts of mothers. Women increase their employment until they become pregnant, in line with the incentives created by the parental benefit scheme – that is to make sure they are eligible for the parental leave *benefit*, especially since the amount of parental leave *allowance* has remained the same since 2008. Most of these women remain employed until the time of giving birth to secure their eligibility, although this is masked by a limitation in our data: some mothers who go on sick leave while pregnant are not registered as employed, particularly for the 2010-2011 cohorts. The difference between post- and pre-reform cohorts around 36-60 months before giving birth is due to the 2008 crisis.

Table 1.1 provides summary statistics for the pre- and post-reform cohorts of first-time mothers. There are 40,432 pre-reform observations and 37,195 post-reform observations. The average age of mothers at the time of giving birth is 29 years old. A higher share of mothers in the post-reform cohorts have a second child within three years (29% vs. 26%). The two groups have similar employment histories, and their pre-birth wages relative to the mean wage are identical at 0.87. However, there are some differences in occupational composition, with slightly fewer white-collar workers in the post-reform cohort, mostly due to a higher share of mothers for whom last occupation is unknown. I also observed a slight increase in part-time work.

1.4 Empirical strategy and identification

1.4.1 Empirical strategy

To evaluate the impact of the new policy that allowed mothers to work full-time while they receive parental leave benefit, I employ a standard event time analysis combined with difference in differences. I estimate the following regression.

$$\begin{aligned} Emp_{itj} = \alpha + \beta PostReform_i + \sum_{t=-96}^{60} \gamma_t D_{it}^{event} + \sum_{t=-96}^{60} \delta_t PostReform_i \cdot D_{it}^{event} + \\ + \phi \mathbf{D_{ij}^{year}} + \zeta \mathbf{D_{it}^{Age}} + \xi \mathbf{Quarter_j} + \varepsilon_{it} \end{aligned} \tag{1.1}$$

where Emp_{itj} is a binary indicator of employment of mother i in calendar month j and t month relative to the birth of her first child. D_{it}^{event}, $t = -96, ..., 60$ are a set of binary variables for each event month -96th, ... , 60th and with the 24th month before childbirth serving as the reference period. The variable $PostReform_i$ is a binary variable that is equal to one for mothers who gave birth to their first child during 2013–2014 (i.e., after the reform), and zero for those who gave birth during 2010–2011 (before the reform). The coefficients δ_t capture the difference between treated and control cohorts in each month t relative to giving birth. The specification also includes a full set of year dummies $\mathbf{D_{ij}^{year}}$, age dummies $\mathbf{D_{it}^{Age}}$ and quarterly dummies $\mathbf{Quarter_j}$ to control for time, age and quarterly seasonality, respectively.

The difference-in-differences approach by which I compare pre- and post-reform cohorts follows

Table 1.1: Descriptive statistics of pre-reform and post-reform mothers

	(1) Before mean	(2) After mean	(3) Comparison b	t
Age at childbirth	28.95	28.51	0.45***	(10.71)
Has second child within 3 years	0.26	0.29	-0.03***	(-8.89)
Employment history				
Months employed in last 5 years	39.31	37.36	1.95***	(12.68)
Wage relative to mean wage	0.87	0.87	0.01	(1.22)
Working time 1 year ago				
Unknown	0.08	0.12	-0.04***	(-17.41)
Full-time	0.84	0.78	0.06***	(19.10)
Part-time	0.08	0.10	-0.02***	(-7.90)
Occupation 1 year ago				
No info	0.07	0.11	-0.05***	(-20.59)
Manager, political/religional/ngo leader	0.05	0.05	0.00*	(2.09)
Professional	0.19	0.19	0.01	(1.66)
Other white collar	0.36	0.31	0.05***	(12.18)
Skilled blue collar	0.20	0.20	0.00	(1.25)
Assembler, machine op.	0.06	0.07	-0.01***	(-3.96)
Unskilled laborer	0.07	0.08	-0.00	(-1.73)
Observations	40432	37195	77627	

Note: The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. Wage, occupation and working hours show the latest observed data during the 18th-24th months before giving birth. The wage is reported relative to the mean wage in Admin3.

the methodology of Kleven et al. (2024), who analyze the 1961 introduction of paid parental leave in Austria – among other reforms in Austria. That reform is similar in nature, as it was grandfathered (see Section 1.2.3 for clarification about the reform that I analyze), so the authors exclude the transition year and retain only those parents who were either clearly ineligible or fully eligible for parental leave.

1.4.2 Identification

Identification relies on the assumption that after controlling for calendar year effects, age effects and quarterly seasonality, the only difference remaining between the pre- and post-reform cohorts is the reform itself.

In fact, the situation of female workers on the labor market was changing substantially during the period of our analysis. Female employment and real wages were growing and fertility was also rising during these years. In Appendix Figure A.1.2 I plot the evolution of female employment in the data I use for all women aged 18-50 (Panel (a)) and by age groups (18-25, 26-40, 41-50 on Panel (b)). First, we observe seasonal patterns in the monthly employment rate. Second, the female employment rate increased steadily from 2010 (from 55% to almost 60% by the beginning of 2018). It is unclear how this general increase affected mothers with young children, but they certainly faced somewhat different labor markets depending on when they were trying to return to work after giving birth. In Panel (b), we see that this increase is mainly driven by the increase in employment rates of the youngest age group (18-25), whereas the majority of mothers are older than that (see Appendix Figure A.1.3). Nevertheless, the most convincing evidence that our identification assumption holds is the lack of pre-trends on our event-time plots (see Figure 1.2 and Figure A.2.9).

Increasing fertility trends[10] may interfere with our estimation strategy. Since I run estimates by the relative time of a mother's first birth, their short-term employment path after giving birth is closely dependent on whether they have another child soon after the first is born. Fertility has been increasing since 2010 just as in many other European countries (OECD Family Database), but the rise has been somewhat steeper in Hungary. Several government policies were introduced during these years to increase fertility (e.g. the 2011 family tax break extension that provides large incentives to have a third child, studied by Szabó, 2023). This increasing trend might bias our employment effects downwards, especially after the second year. Furthermore, the PPL Extra reform package itself may have increased

[10] Based on the OECD Family Database fertility has been on the rise since 2010 and reached the EU average in 2020.

fertility by eliminating the incentive to wait two years in order to maximize total benefits. I show the impact of the reform on fertility in section 1.6.1. If a fertility-increasing effect exists it can be considered as a channel through which this reform affects the labor supply of mothers.

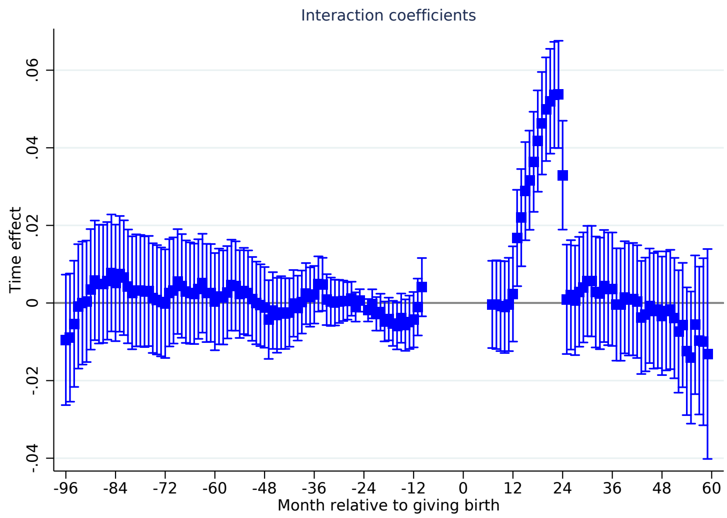
We illustrate the increase in fertility by two measures in the Appendix Figure A.1.4. In Panel (a) I show the share of mothers in our sample who have a second child within three years. This share increases from 26.5% to 29.3% between the pre- and post-reform cohorts. Note that this increase could simply be a side effect of our sampling strategy, in that I identify real first births with more noise towards the beginning of the sample (see Section 1.3). More precisely, the increase that we see might just be due to having more births in the beginning of the period, that I classify as first births but are actually second, third, or higher-order births. Panel (b) shows the total fertility rate from the OECD Family Database. We observe an increasing trend for this period from 1.24 in 2011 to 1.44 in 2015 (and even higher in later years). The two measures point to very similar conclusions. Second, I run robustness estimates on the sample of mothers without a second child in Figure A.1.6 and our main results are unchanged. Based on these estimates, there could be differences in the employment paths of mothers after age 2 of the first child, but these differences are much lower than our main estimates for the 13th-24th months.

1.5 Results

1.5.1 Main estimates on maternal employment

Figure 1.2 shows the estimated interaction coefficients denoted by $\hat{\delta}_j$ from equation 1.1. There are no differences between the pre- and post-reform cohorts during the 96 months (eight years) before giving birth. The only period during which we observe higher employment levels for the post-reform cohorts is between the 12th and 24th months after giving birth, when the incentive to work emerges. The coefficients increase over time, from 0.017 in the 13th month to 0.054 in the 23th month, suggesting that the reform had a growing impact as the first child aged. For comparison, the baseline employment rates for mothers in the 2010-2011 cohorts were between 5.7% in the 13th month and 20% in the 24th month after giving birth (see Figure 1.1 for raw employment rates by relative time for the pre- and post-reform cohorts). This implies a relative increase in employment of 30–50% during the affected months. Overall, the figure provides compelling evidence that the reform increased the employment of affected

Figure 1.2: Monthly event time treatment coefficients on monthly employment rates, for births in 2013-14 vs. in 2010-11



Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009. The plotted coefficients are $\hat{\delta}_t$ coefficients from equation 1.1. Standard errors are clustered at the individual level.

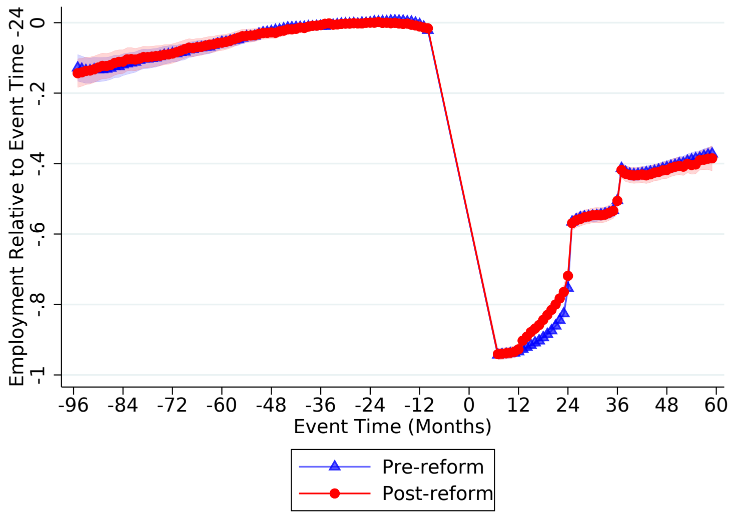
mothers during the second year of their first child's life but did not further impact the employment of mothers from the 25th to 59th months. Although not significantly different from zero, the coefficients tend to decline with the child's age after the 24th month.

Figure 1.3 illustrates the impact of the reform by showing the monthly event-time coefficients relative to the counterfactual employment absent children predicted by our model in equation 1.1 for both the pre- and post-reform cohorts[11]. I plot the event-time coefficients $\hat{\delta}_j$ from equation 1.1 as a fraction of the counterfactual value without children $P_t \equiv \hat{\delta}_j / E[\tilde{Y}_{itj}|t]$, where $\tilde{Y}_{itj}$ is the predicted value from equation 1.1 excluding the event-time coefficients. Thus, these figures show the postpartum drop in employment as a fraction of the counterfactual employment rate 24 months before giving birth, while accounting for the mothers' age, the calendar year, and quarterly seasonality.

This figure resembles the raw employment rates in Figure 1.1 in several key aspects: an increase in employment during the years before giving birth, a significant decrease in employment immediately after giving birth, and gradual increases in employment at ages 2 and 3 that correspond with the in-

[11]We use the same methodology as on the child-penalty plots of Kleven et al. (2019), but now instead of comparing fathers and mothers, I compare pre- and post-reform cohorts.

Figure 1.3: Monthly event time coefficients relative to counterfactual employment absent children for pre- and post-reform cohorts



Note: See sample restrictions and employment definition below Figure 1.2. I plot event time coefficients $\hat{\delta}_t$ from equation 1.1 as fraction of counterfactual value absent children $P_t \equiv \hat{\delta}_t / E[\tilde{Y}_{itj}|t]$, where $\tilde{Y}_{itj}$ is the predicted value from equation 1.1 without including the event time coefficients.

stitutional incentives. Both figures show that employment rates between months 12 and 24 are higher for the post-reform cohort. In Figure 1.3 pre-trends are fully aligned when controlling for age and year effects, notably including the employment impacts of the 2008 economic crisis. Controlling for year and age effect, we observe a slow increase in employment only until the 48th month before giving birth. The employment rate of mothers decrease by almost 100% during the first year, remains flat at around 55% lower employment between the 24th and 36th months and stabilizes at 40% lower employment after age 3.

Figure 1.3 shows that the reform increased maternal employment in months when a direct financial incentive to work more was offered. However, it did not drastically change the overall evolution of maternal employment in the first five years following childbirth. The same employment patterns remain visible, including the characteristic jumps at ages two and three, and no long-term employment impacts are observable.

Although the reform eliminated the incentive to wait two years before returning to the labor market in order to avoid losing parental leave benefits, a substantial increase in employment is still observed from the 24th to the 25th month. There are a few possible explanations for this pattern. First, family income significantly decreases at the age of two, so this increase in employment may be a reaction to that, or, in other words, it may be explained by the income effect. Second, parents might not immediately realize that they can work and receive parental leave benefits simultaneously. Third, persistent social norms might play a role, as there is a strong tradition of mothers staying home with their children for at least two years.

1.5.2 Robustness checks

In this section I present robustness checks for our main estimates.

First, I present the results when different sets of first-time mother cohorts are included in the sample: 2011 vs. 2013, 2010–11 vs. 2013–15, 2010–11 vs. 2013–16, 2010–11 vs. 2013–17. Our baseline estimate used the cohorts of 2010–11 as pre-reform or control cohorts and the 2013–14 cohorts as post-reform or treated cohorts. Our results are shown in Appendix Figure A.1.5. The overall pattern of the reform's impact is similar in all the robustness estimations: we observe significantly higher monthly employment rates 13-24 months after the birth of the first child, and the increase is greater for later months. For the sample using only the 2011 and 2013 cohorts (Panel (a)) there are some fluctuations in the pre-trends, probably because the event-time regression cannot filter out calendar-year effects well with only two cohorts. The pre-trends in the other three panels look good; however, we see a weakly significant permanent decrease in employment rates after the 24th month, especially when using the widest sample (Panel (d): 2010-–2011 vs. 2013—2017). Our preferred specification uses two pre-reform and two post-reform cohorts, so the sample remains balanced during our observation period (we observe at least seven pre-birth and three post-birth years for everyone in the sample).

Second, I modify the birth events taken into account in Appendix Figure A.1.6. As a reminder, for our baseline estimates, I define the relative time around the first birth observed for a mother in the sample. I further restrict the sample of mothers by excluding those who have received child care transfers earlier ensuring that I focus solely on first births (we observe birth events only from 2009, though we observe child care transfers as early as 2003). Panel (a) does not use information on previous child care transfers to correct for the sample, and the results are very similar. Panel (b) focuses on mothers with only one observed birth, and Panel (c) focuses on first-time mothers who do not have a second child within three years (with the correction as in our baseline sample). Our main estimates remain nearly identical across these latter model versions; the only difference is the appearance of positive, albeit mostly insignificant, employment effects after the 24th month.

Third, I included the 2012 cohort in our estimation. Since the reform affected everyone on paid parental leave after January 1, 2014, the 2012 cohort was a transitional group affected only partially by the policy. For example, if a child was born in June 2012 and turned one in June 2013, the reform would have only affected the last six months of parental leave. We expect to see an employment effect for this group of mothers, but it should be smaller than what I find for later cohorts who were "fully" affected by the policy. This is exactly what I find (see Appendix Figure A.1.7).

Lastly, in Appendix Figure A.1.8, I show placebo estimates comparing 2010 vs. 2011 and 2013 vs. 2014. Most of the coefficient estimates are not significantly different from zero, and the point estimates are much smaller than those in our main estimates.

1.5.3 Heterogeneity by working time

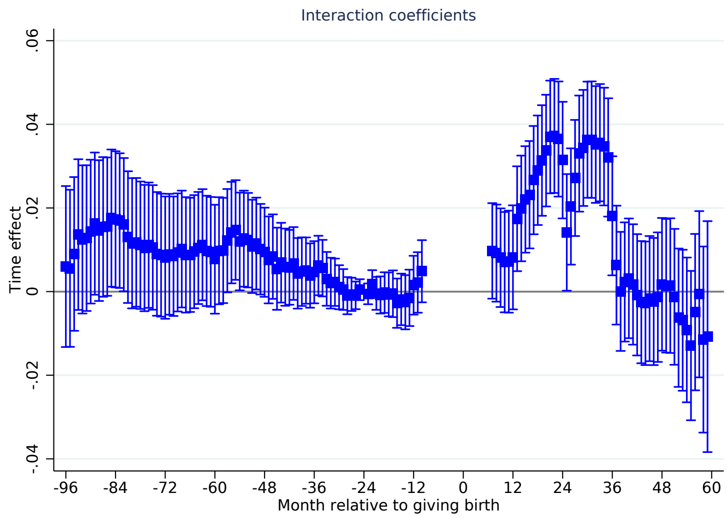
The reform provided incentives for a more flexible return to the labor market. Previously, a mother had to forfeit all parental leave benefits if she chose to work before her child turned two. This only made sense if she earned enough to compensate for the lost benefits and childcare costs. Now that mothers can earn parental leave benefits while working, they have fewer constraints, and it makes more sense to return to work part-time.

Figure 1.4 shows how the reform affected full-time (> 30 hours) and part-time (at most 30 hours) employment separately. First, there is a decreasing pre-trend in full-time employment and an increasing pre-trend in part-time employment. Second, most of the overall employment impact is driven by full-time employment. However, considering the low share of part-time employment in Hungary, the remaining share of the employment impact, that is due to part-time employment is not negligible.

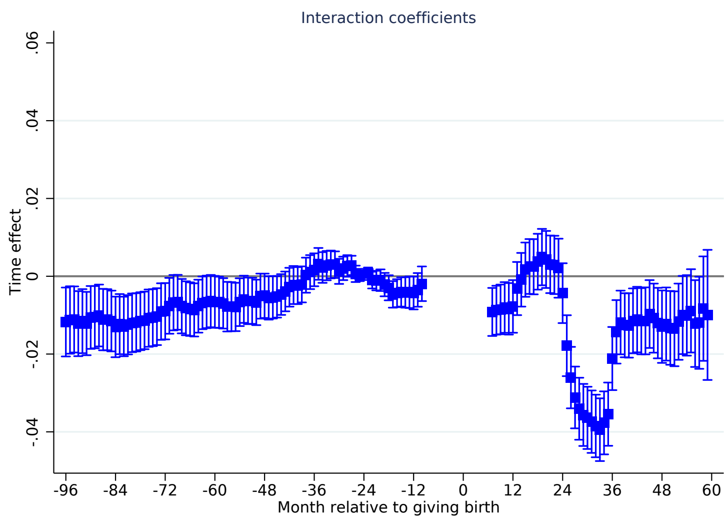
Before the policy, someone could work only up to 30 hours per week while receiving parental leave *allowance*. In response to the policy, we see a shift from part-time employment to full-time employment during months 24 through 36, i.e., the period during which the only available parental leave transfer is the flat PL allowance.

Figure 1.4: Heterogeneous effects by full-time vs. part-time employment

(a) Full-time (> 30 hours)



(b) Part-time (<= 30 hours)



Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009. The plotted coefficients are $\hat{\delta}_t$ coefficients from equation 1.1, where the outcome variable is 1 if someone works more than 30 hours per week (full-time) on Panel (a) and is 1 if someone works at most 30 hours per week (part-time) on Panel (b). Thus, I decompose the main employment effect from Figure 1.2 into full-time and part-time employment. Standard errors are clustered at the individual level.

1.6 Mechanisms

Our main analysis focuses on the impact of the policy on maternal employment. Here, I provide an overview of the channels through which the policy affects maternal employment, which helps us understand the mechanisms behind our main findings. I discuss the reform's impact on fertility and returning to the previous employer. Additionally, I describe the characteristics of compliers, explain how our main estimates identify the substitution effect of paid parental leave, and demonstrate the reform's unintended long-term impact on fathers.

1.6.1 Impact on fertility

We analyze the impact on fertility by estimating a model similar to the one used for the impact on employment (see equation 1.1), except our left-hand side variable is a binary variable for giving birth in a given month. Since giving birth is not defined until the first birth, there are only positive event times in the estimation sample. Consequently, I also do not include the $PostReform_i$ variable on its own in the regression to net out pre-childbirth differences between the pre- and post-reform cohorts. Figure 1.5 shows the δ_t coefficients which show the difference in the probability of having another child between the pre- and post-reform cohorts of mothers while controlling for age effects, year, and quarterly seasonality effects.

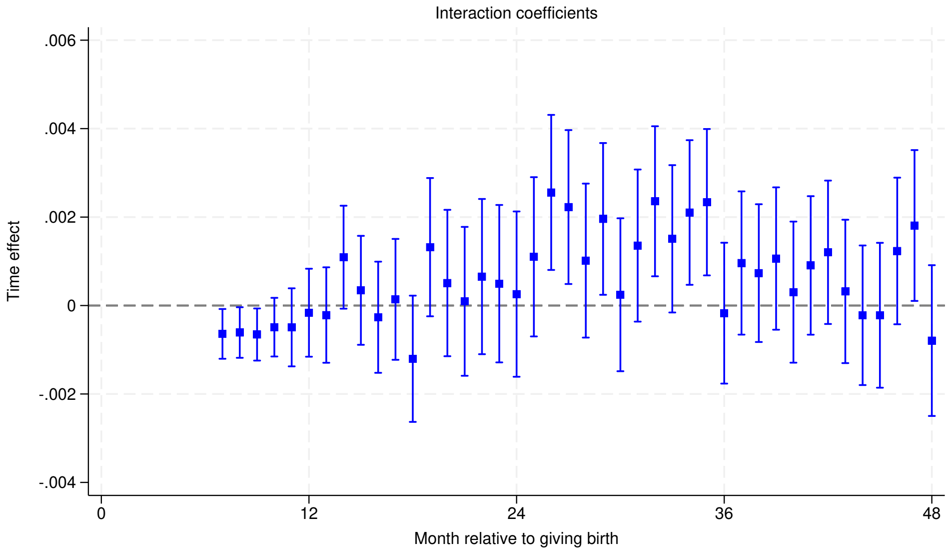
Figure 1.5 shows that the reform had a positive impact on the probability of giving birth, particularly during the third year after the birth of the first child. In some months (e.g. the 26th, 27th, 34th and 35th), the impact exceeds 0.2 pp (compared to a baseline 1.1-1.5%). This suggests that the reform may have affected mothers' employment decisions by impacting fertility[12].

1.6.2 An earlier return to work

Based on our main results (Figure 1.2) it appears that some mothers have returned to work earlier due to the reform, while the employment rate of mothers after the 24th month remained unaffected. First, I discuss which mothers tend to return to work before their child turns three (i.e., when they exhaust all parental leave benefits). Then, we examine who the mothers are who likely responded to the policy

[12] Bíró et al. (2025) use a different methodology to assess the reform's impact on employment and to further analyze the role of an earlier return to the labor market on later labor market outcomes. They use a much narrower timeframe and smaller cohorts, include women who do not give birth as controls, and find no fertility impact with that strategy.

Figure 1.5: Monthly treatment coefficients on the binary variable of giving birth in a specific month, for cohorts giving birth in 2013-14 vs. 2010-11



Note: The plotted coefficients are the estimated $\hat{\delta}_t$ coefficients from a regression equation similar to equation 1.1, where the outcome variable is a binary variable indicating whether or not a woman gave birth in a given month. I leave out the $PostReform_i$ variable and only include positive event times. Standard errors are clustered at the individual level.

change and whether this earlier return has any consequences for their future labor market outcomes.

First, I present some descriptive statistics on the characteristics of mothers who return to work sooner. In Table 1.2, we can compare mothers who return to work within three years ("early returners") with those who return later or never ("late returners"). On average, late-returners are slightly younger at the time of giving birth (28 years old) than early-returners (30 years old). The reason for the later return may be related to the arrival of a second child within two to three years of the first child, which is much more frequent among late-returners (36% vs. 19%). Late-returners have an employment history that is about 12 months shorter and earn 73% of the national average wage one year before giving birth, compared to 97% for early-returners. There are more white-collar workers and full-time workers among early returners.

The reform induced a substantial share of mothers to return to work earlier than they would have without it; before their child turns two. Panel (a) of Figure 1.6 illustrates this by plotting the share of mothers who return to work each month relative to the month they gave birth. Returning to employment occurs in the first month of work after maternity leave. There are significant spikes in reemployment timing at the 25th and 37th months, when the parental leave benefit and allowance payments

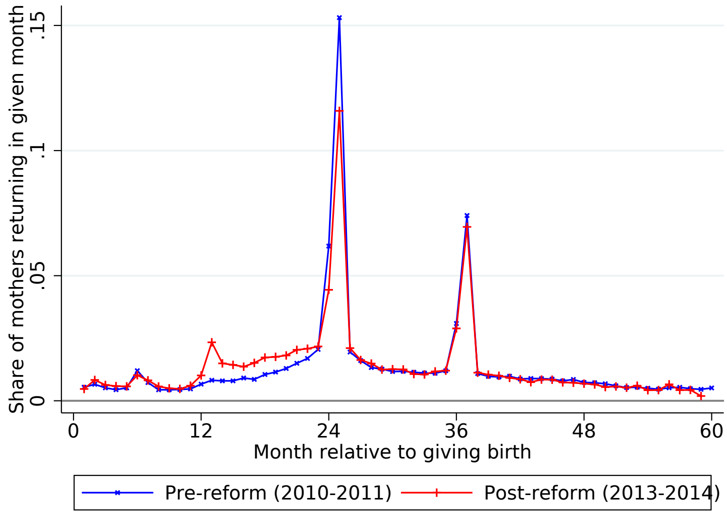
Table 1.2: Descriptive statistics of mothers, returning to work within 3 years vs. later

	(1) Within 3 years mean	(2) Later mean
Age at childbirth	29.84	27.74
Has second child within 3 years	0.20	0.36
Employment history		
Months employed in last 5 years	44.55	32.30
Wage relative to mean wage	0.97	0.73
Working time 1 year ago		
Unknown	0.05	0.12
Full-time	0.88	0.76
Part-time	0.06	0.12
Occupation 1 year ago		
No info	0.05	0.10
Manager, political/religional/ngo leader	0.06	0.04
Professional	0.24	0.12
Other white collar	0.39	0.30
Skilled blue collar	0.17	0.24
Assembler, machine op.	0.05	0.08
Unskilled laborer	0.05	0.12
Observations	22040	17144

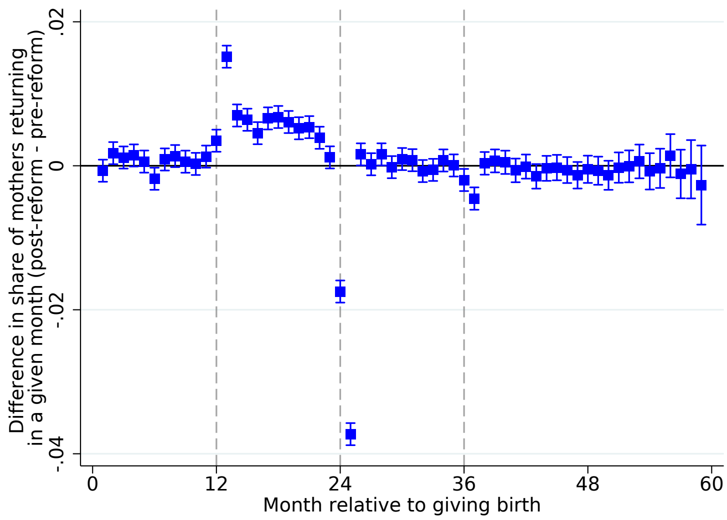
Note: The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. Wage, occupation and working hours show the latest observed data during the 18th-24th months before giving birth. The wage is reported relative to the mean wage in Admin3.

Figure 1.6: Share of mothers returning to employment in a given month

(a) Share returning

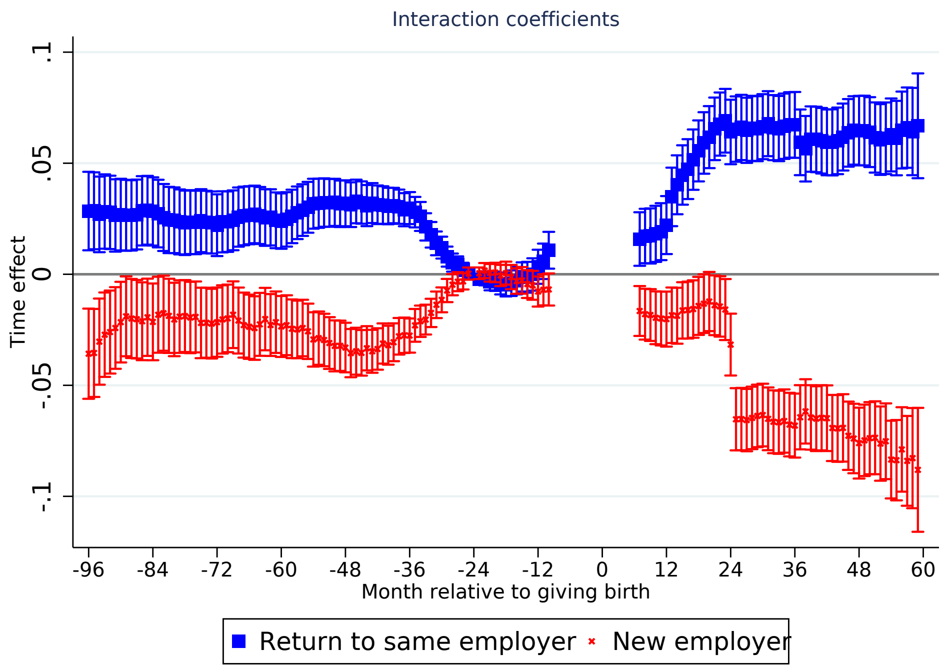


(b) Difference 2013–14 vs. 2010–2011



Note: The time of return to work is the month following the birth of a child when a mother first returns to work after her maternity leave.

Figure 1.7: Decomposition of employment impact by returning to previous employer vs. working at a new employer



Note: The plotted coefficients are $\hat{\delta}_t$ coefficients from equation 1.1, where the outcome variable is 1 if someone works at their previous employer (blue) and is 1 if someone works at a new employer (red). Standard errors are clustered at the individual level.

end, respectively. However, another, much smaller spike appears at the 13th month after the reform. Substantially more mothers in post-reform cohorts return to work between the 12th and 22nd months (7 pp more), while there are corresponding decreases at months 24–25 (-5.5 pp) and 36–37 (-0.65 pp), as shown in Panel (b) of Figure 1.6.

1.6.3 Increased probability of returning to the previous employer

Next, we will explore the increased probability of staying with the pre-birth employer that happens due to the earlier return of mothers induced by the reform. Figure 1.7 shows the decomposition of our main employment estimates in Figure 1.2 into employment at the previous employer vs. a new employer. Blue shows employment at the same firm where the mother worked before giving birth. There is a positive pre-trend, meaning treated mothers are more likely to work at the same firm for a longer period of time. Conversely, we observe a negative pre-trend for working at a different employer than the last employer before giving birth. The positive employment impact for the treated cohorts is

Figure 1.8: Returning to the previous employer by relative quarters, pre- and post-reform

(a) Share returning to earlier employer

(b) Share working at their earlier employer in the 40th month

(c) Difference, share returning to earlier employer

(d) Difference, share working at earlier employer in the 40th month

Note: Employment is defined monthly as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave.

entirely due to returns to the previous employer, and the impact on employment at the previous employer remains permanent during the third to fifth years after childbirth. This suggests that, although the overall employment rate of mothers during the third to fifth years after childbirth is the same for pre- and post-reform cohorts, a higher percentage of mothers return to and remain with their previous employers permanently.

Figure 1.8 highlights that the persistent impact on employment at the previous employer is due to mothers who returned to work earlier because of the reform. It also provides descriptive statistics on returning to the pre-birth employer. Panel (a) of Figure 1.8 shows the share of mothers who returned to their previous employer out of all the mothers who returned in a given quarter, categorized by the quarter in which they returned to work. Panel (c) shows the difference between the pre- and post-reform cohorts. Among pre-reform mothers, the later they return to work, the lower the chances are that they will return to their previous employer until the sixth quarter. After the sixth quarter, the highest share of mothers who return to their previous employer occurs in the ninth and thirteenth quarters, when many job-protected mothers likely return to the labor market. Those who return after the job-protection period have a much lower chance of returning to their original employer (20% vs. 40-60% for those returning during quarters 4-13). The share of those who return to their previous employer is similar for pre- and post-reform cohorts. However, there is a substantial difference in quarters 5-9: the share returning to their previous employer is about 11 pp higher for post-reform mothers (15 pp in the 5th, 8 pp in the 6th and 7th, 12 pp in the 8th and 14 pp in the 9th quarter). This is consistent with Figure 1.7 which shows that the employment impact of the reform is entirely due to mothers returning to their previous employers. Panel (b) of Figure 1.8 shows, in a similar format, the probability of working at the previous employer 40 months after giving birth and illustrates that post-reform mothers who returned to work earlier tend to work at their previous employers more than a year later.

1.6.4 Complier analysis

Early returners to the labor market surely have different characteristics than later returners. They earn more, they have longer employment histories and they are somewhat older. They are also more likely to be white-collar workers and they are more likely to have had full-time jobs before giving birth (Table 1.2 and Table A.1.1). These differences are not surprising, as the opportunity cost of staying home to care for children is higher for mothers with more established labor market careers.

In light of the policy that we analyze, it is interesting to see who are the compliers to this policy change, i.e., those mothers who decide to work because of the reform's incentive. We distinguish between mothers who would have worked even without the reform (*always-workers*) and mothers who are working because of the reform (*compliers*), to perform a simple complier analysis (similarly to and based on Imbens & Wooldridge, 2009; Zurla, 2021).

Since we observe each first-time mother only once, either before or after the policy change, we cannot know their counterfactual employment status. For example, for post-reform mothers, we cannot

Table 1.3: Complier analysis

	Pre	Post			
	Always-workers		Compliers	All mothers	Never-workers
Last wage (in 2014 HUF)	244,484	232,026	190,413	202,390	189,855
Age at birth	29.56	28.81	26.32	28.76	28.43
White-collar	0.67	0.64	0.55	0.58	0.54
Budapest	0.26	0.22	0.10	0.21	0.19
	Pre				
	Always-workers		Compliers		p-value
Last wage (in 2014 HUF)	244,484		190,413		0.0055
Age at birth	29.56		26.32		0.0000
White-collar	0.67		0.55		0.0078
Budapest	0.26		0.10		0.0000
			Compliers	All mothers	p-value
Last wage (in 2014 HUF)			190,413	202,390	0.4889
Age at birth			26.32	28.76	0.0000
White-collar			0.55	0.58	0.3729
Budapest			0.10	0.21	0.0018

Note: Pre-reform is for the 2011 cohort, post-reform is for the 2013 cohort of mothers.

know if they would have been employed if they had given birth before the reform. Thus, we cannot directly observe the type of mother. As is common in the literature, we make the monotonicity assumption: we assume that everyone who decided to work before the reform would also work after the reform, so there are no *defiers*. We calculate the percentage of mothers who return to work before the 24th month after childbirth, both before and after the reform: 21.5% and 27.9%, respectively. Due to the *no-defiers assumption*, early-returning mothers before the reform are all *always-workers*, so the pre-reform characteristics of working mothers show the characteristics of *always-workers*. Due to the *no-defiers assumption*, the share of *never-workers* can be estimated by the percentage of mothers who did not return by the 24th month from the post-reform cohorts: 100% - 27.9% = 72.1%. Thus, the share of *compliers* is estimated to be 6.4%.

Table 1.3 reports the average wage one year before giving birth (in 2014, HUF), age at the time of childbirth, the share of white-collar workers, and the share of workers in Budapest, the capital,for pre-reform employed mothers. We assume that these average values are equal to the average values of *always-workers*. Then, we report the average characteristics of post-reform employed mothers, i.e., the combination of *always-workers* (that we know) and of *compliers*: $x_{PostReform} = 0.215/(0.215 + 0.065)x_{AW} + 0.065/(0.215 + 0.065)x_C$. Since $x_{AW} \equiv x_{PreReform}$ based on our assumptions, we can infer x_C, the average characteristics of compliers. We also report these average characteristics for all mothers and never-takers separately.

Based on this analysis, *compliers* have a lower pre-birth wage, are younger, and are less likely to come from white-collar jobs or the capital city of Budapest than *always-workers*. *Compliers* are similar to *never-workers* (and all mothers) in terms of wages and occupations, but they are younger and less likely to live in Budapest.

1.6.5 Substitution and income effect of paid parental leave

By estimating the employment impact of this policy change we can approximate the role of the substitution effect of paid parental leave in the Hungarian context (Bíró et al., 2025). The substitution effect arises because a mother has to give up the parental leave benefit if she chooses to work. In the standard model of labor supply, this would be represented as a kink at zero hours worked, where a marginal increase in work hours would result in a lower income. This kink is represented by point B on Panel (a) of Figure 1.9. The reform – by enabling mothers to work while retaining the benefit – eliminates this kink from their budget constraints by shifting the budget constraint upward, as illustrated in Panel (b) of Figure 1.9. Thus, the reform incentivizes those mothers to work who would not have worked in the absence of the reform due to the substitution effect.

The income effect of paid parental leave acts as a disincentive to work even after the reform. At the income level provided by the parental leave benefit a mother might simply prefer not to work. However, mothers who prefer to work at this income level, are no longer affected by the substitution effect. Furthermore, theoretically, the reform might cause always-worker mothers to decrease their working hours due to the income effect the reform, although we do not find evidence of this in Figure 1.4.

Our main estimates showed a 3.2 pp increase in monthly maternal employment relative to the 9.8% baseline employment rate for mothers of children aged 1-2. This suggests that overall 3.2% of mothers had not worked during the second year of age of their first child due to the substitution effect. This suggests that, overall, 3.2% of mothers did not work during their first child's second year due to the substitution effect. This also suggests that the majority of mothers do not participate in the labor market for reasons other than the substitution effect of paid parental leave. These reasons include the income effect, strong preferences for staying home with their child, the limited availability of childcare facilities, and intra-household decision-making.

Understanding how parental leave discourages employment for mothers is highly relevant to increasing maternal employment. These results suggest that even if the substitution effect is eliminated from the paid parental leave scheme, the majority of mothers will still wait until their child turns two (or even three) to return to the labor market, even if a strong financial incentive like 60% of one's wage is offered[13].

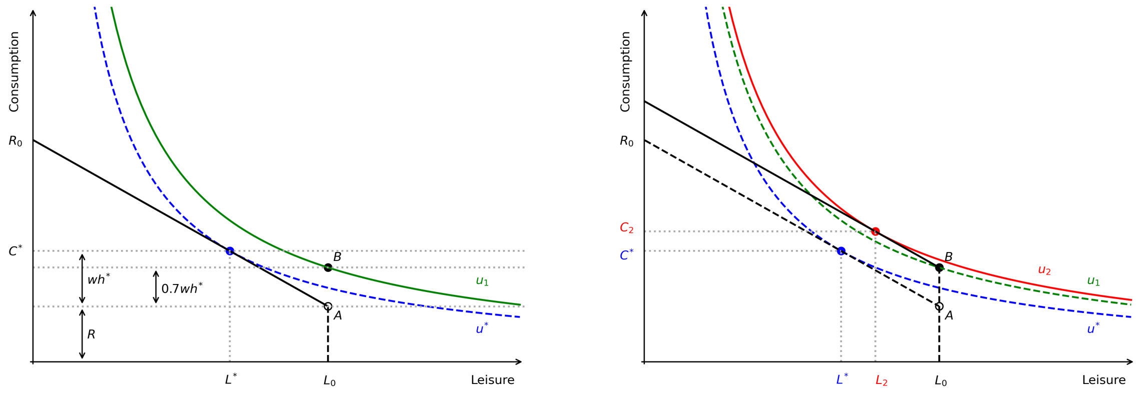
1.6.6 Fathers on parental leave

The reform made decisions about (1) who stays home with the child and (2) who takes parental leave independent. This had the unintended consequence of allowing fathers to take parental leave while working full-time, even if the mother stays home full-time. In most families, men are the main bread-

[13]That was the average amount of extra income one could get while working on parental leave compared to the pre-reform rules.

Figure 1.9: Illustration of the labor supply decision of mothers before and after the reform

(a) Pre-reform (b) Post-reform



Source: Figure 2, Bíró et al. (2025)

winners and have higher incomes. Furthermore, there could be cases in which the mother is not eligible for parental leave benefits, yet the father is. In these cases, the family's income would be maximized if the father took the benefit and continued working full-time while the mother stayed home to care for their child. This has two unintended negative consequences: First, the total family income may increase, which provides a disincentive for maternal employment. Second, the mother may lose insurance continuity, which results in a loss of months for her future pension.[14]

To understand the potential impact of these unintended consequences on families' reactions to the reform, we analyzed parental leave and employment rates in the Labor Force Survey from 2003 to 2020 for mothers and fathers separately. The main advantage of the LFS is that its sampling units are households (and, within those, families), which allows us to observe both parents.

Our focus is on families with at least one child between their first and second birthdays, i.e., families affected by the reform. Figure 1.10 shows the take-up of parental leave. The blue solid line at the top shows the percentage of families who took any kind of parental leave transfer. This percentage is about 95%, meaning that approximately 5% of families did not take any transfers, even though everyone was eligible for the flat parental leave allowance. The other two lines on the graph show parental leave benefit and allowance take-up, with the dashed red line and the long-dashed orange line, respectively.

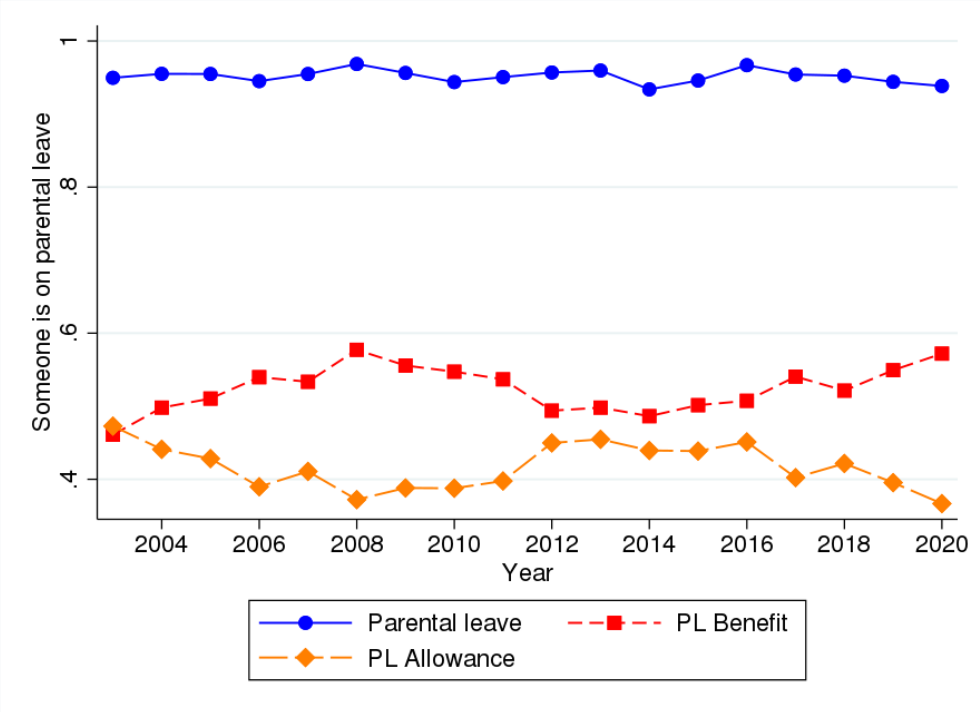
[14] Maternity leave benefits are only available to mothers for the first six months after giving birth, and they are automatically eligible if they are on paid maternity leave when they give birth. Thus, for first-time parents planning to have a second child within three years, it might not be income-maximizing in the long term for the father to take the parental leave (PL) benefit, even if he would receive a higher amount.

There is slight fluctuation in the share of families who take up these transfers, likely due to economic fluctuations causing eligibility to fluctuate, as eligibility is tied to employment history. Since 2008, the percentage of families who took the more generous parental leave (PL) benefit decreased from 57% to 49%, accompanied by an increase in PL allowance take-up from 37% to 44%. A few years after the reform, around 2016, the percentage of families receiving PL benefits increased again, while the percentage receiving PL allowances decreased. This could reflect better economic conditions, as well as the reform.

Figure 1.11 shows the share of families where the mother (father) takes up PL benefits and PL allowance. Mothers generally have a higher take-up rate for both types of parental leave transfers. Their take-up rate for PL benefits ranges from 46% to 58% throughout the entire period, while for fathers, it ranges from 0.1% to 3%. A similar difference is seen in the overall take-up of the PL allowance: 36%-45% for mothers versus 0.2%-1.2% for fathers. Looking at the parental transfer take-up of mothers in Panel (a), we see that the take-up of the two types of transfers moves in opposite directions, reflecting the changing share of mothers eligible for PL benefits. On the other hand, the transfer take-up for fathers in Panel (b) remains fairly stable at 0.4% for the PL benefit and 0.7% for the PL allowance until 2017. Starting in 2018, there is a clear increase in PL benefit take-up for fathers, reaching 1.6%, 2.1%, and 2.9%. This represents a nearly fivefold increase in PL benefit take-up for fathers between 2017 and 2020. This increase is likely due to the reform, although there is a four-year lag before take-up by fathers increases. This lag may be due to the complicated consequences of the policy change on family income-maximizing behavior, and the time it took families to understand these implications and adjust their behavior.

We also examine the employment rates of mothers and fathers who take up parental leave *benefit* or *allowance* on Figure 1.12. Before 2014, it was not legally possible to work while receiving parental leave *benefits*. In 2005, it became legal to work while receiving parental leave *allowances*. Despite this, we observe positive employment rates during these periods because the LFS definition of employment is based on whether a person worked at least one hour last week; it does not have to be formal employment. Nevertheless, we observe higher employment rates for mothers in Panel (a), starting in 2014 at 3% (versus up to 2% in previous years) and reaching 7% by 2020. The employment rates of fathers on parental leave in Panel (b) are much higher before the 2014 reform, with a clear jump in 2015 to

Figure 1.10: The use of parental leave transfers



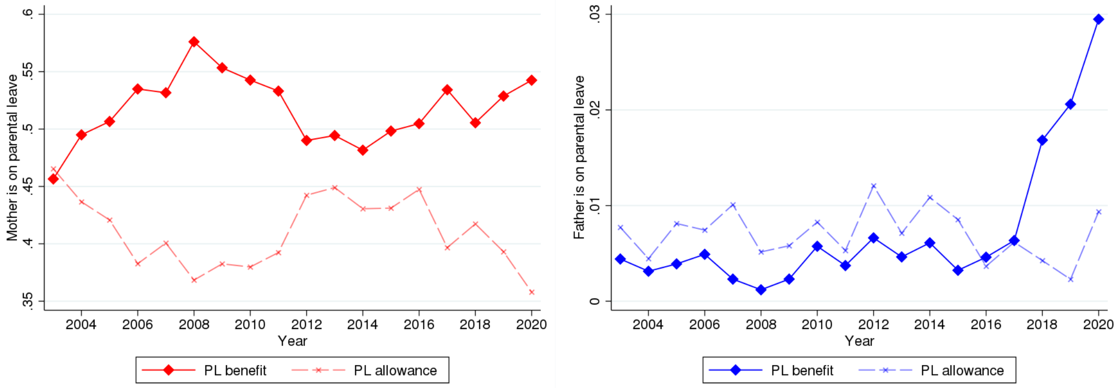
Note: Source data is the Hungarian Labor Force Survey, 2003-2020. The sample consists of families where there is at least one 1-year-old child (who is between her/his 1st and 2nd birthday). The figures show the share of such families where someone in the family takes up any kind of parental leave transfer: parental leave benefit and parental leave allowance.

above 80%. Overall, fathers seem to have worked while receiving parental leave transfers at higher rates even before the reform. Since 2014, the majority of fathers have worked while receiving parental leave transfers; however, this increase has been much more modest for mothers.

Overall, these figures suggest that the policy change allowing formal full-time employment while receiving parental leave *benefits* led to an increase in take-up among fathers. However, this likely affected only a small minority of families during our sample period, which ended in 2017. The increased take-up by fathers became visible only a few years after the reform, starting in 2018. The employment rate of fathers receiving parental leave (PL) *benefits* increased earlier, starting in 2015. For affected families, the reform increased total family income and, through this channel, led to an unintended disincentive for maternal employment. This phenomenon does not appear to have affected many families in our sample period (until 2017): it affected approximately 0.2% of families (as take-up by fathers was around 0.4% during the years 2014-2017, and their employment rate increased by 50 pp).

Figure 1.11: Parental leave transfer take-up, mothers and fathers

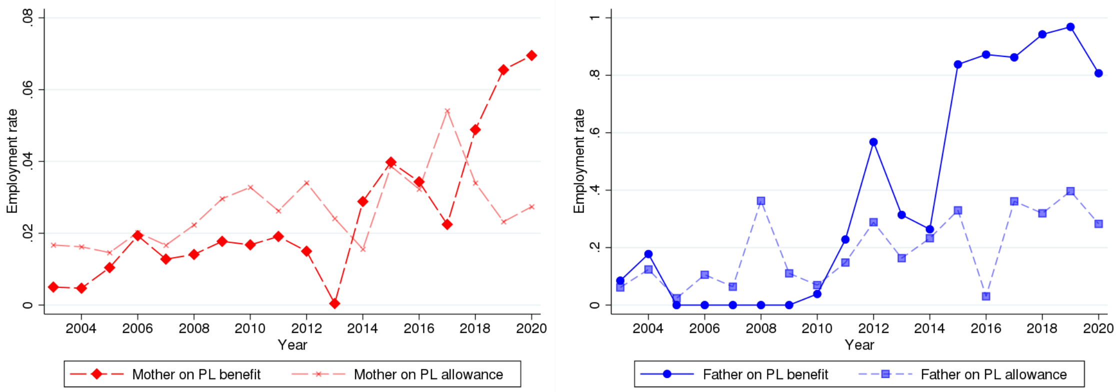
(a) Mother (b) Father



Note: Source data is the Hungarian Labor Force Survey, 2003-2020. The sample consists of families where there is at least one 1-year-old child (who is between her/his 1st and 2nd birthday). The figures show the share of such families where the mother or father takes parental leave benefits or allowances. Panel (a) shows the percentage of families in which the mother takes parental leave benefits or allowances, and Panel (b) shows the percentage of families in which the father takes parental leave benefits or allowances.

Figure 1.12: LFS employment rate, mothers and fathers on parental leave transfers

(a) Mother (b) Father



Note: Source data is the Hungarian Labor Force Survey, 2003-2020. The sample consists of families where there is at least one 1-year-old child (who is between her/his 1st and 2nd birthday). The figures show the employment rates of mothers (Panel (a)) and fathers (Panel (b)) who receive parental leave benefits or allowances. Employment is based on the standard LFS employment definition: someone is considered employed if they worked at least one hour in the past week or are temporarily out of work (e.g., due to illness or vacation). Someone on parental leave is not considered employed unless they earn income from another job.

1.7 Conclusion

This paper has analyzed the 2014 "Paid Parental Leave Extra" (PPL Extra) reform in Hungary, which relaxed the conditions for mothers to return to work while on paid parental leave. After the reform, mothers could work full-time and receive parental leave benefits once their child turned one. My primary focus has been the impact on maternal employment because the official target of the reform was to increase mothers' labor market participation.

My findings showed that the reform increased the monthly employment rate of mothers by an average of 3.2 percentage points (pp) during the 13th to 24th months after giving birth to their first child. This represents a 32% increase relative to the 9.8% baseline employment rate. After running several robustness checks, I have found that my main short-run employment result is robust, with a similar effect size in all specifications. However, the increase is not huge in absolute terms, and it did not change the overall employment patterns of mothers during the five-year period following the birth of their first child. The longer-term effects for the third to fifth year are less robust, though. For instance, we observe positive impacts for mothers with only one child and negative impacts for other analysis periods.

This empirical analysis allows us to approximate the substitution effect of paid parental leave in the Hungarian context, which has not been done in other countries. The substitution effect is a distortionary disincentive to work that occurs due to the high marginal tax rate at zero hours worked. In this case, mothers lose all the benefits if they choose to work any hours or in other words there is a kink in their budget constraints. Affected mothers would choose to work in the absence of the benefit and instead stay away from the labor market only due to this "kink" in their budget contraints. Understanding the possible size of the substitution effect is important for designing paid parental leave programs.

Hungary has a long history of relatively "well-paid" parental leave and a focus on home care for young children instead of institutional care. Consequently, social norms regarding the role of mothers in caring for their young children are very strong. Offering financial incentives seems to only marginally change mothers' labor market decisions, especially since a "well-paid" parental leave until a child's second birthday was still available for most families after the reform. Around 80% of mothers do not return to the labor market until their child is two years old. This suggests that the income effect of paid parental leave on mothers' labor supply is significant, though social norms, preferences, and limited childcare availability may also play a role.

I also find positive fertility effects in the third year after the birth of the first child. This is likely due to the element of the PPL Extra reform that removed the incentive to wait for three years for the second child and can possibly capture time trends in fertility. Increased fertility could be one way the reform package impacted maternal employment. On the other hand, due to the fertility effect, it is difficult to identify the reform package's longer-term employment effects and further impacts on labor market outcomes because increased fertility modifies the composition of mothers in the labor market.

Lastly, I have presented preliminary findings that suggest mothers induced by the reform to return to the labor market earlier seem to benefit from an increased likelihood of continuing to work with their previous employers for up to five years after childbirth. In our follow-up paper (Bíró et al., 2025) we use a different identification strategy with a narrower time frame and include women without children to net out time trends. Our results corroborate all my findings and also move forward by providing causal evidence about the role of an earlier return in later labor market trajectories of mothers.

2 Chapter 2: The Incentive Effects of Sickness Benefit for the Unemployed – Analysis of a Reduction in Potential Benefit Duration

Joint work with Márton Csillag

2.1 Introduction

Understanding how workers react to the incentives inherent in social insurance programs is paramount to creating benefit systems that protect employees from unexpected negative shocks and encourage them to return to work. There is growing evidence of important interactions between unemployment insurance benefits and benefits that provide compensation in case of prolonged sickness or temporary disability (see for example Hall & Hartman, 2010; Hall & Krueger, 2012; Henningsen, 2008, or OECD, 2018). Furthermore, workers use sickness and disability benefits if they are more generous (e.g. Larsson, 2006), or they come with less severe behavioral rules (e.g. van den Berg et al., 2019) and eligibility can be leniently granted; hence, they will take up sickness (or temporary disability) benefits more in times of recession (e.g. Andersen et al., 2019; Bratsberg et al., 2013).

We examine a case from Hungary in which the potential risk of misuse was significant: employees were eligible for long-term sickness benefits for up to three months after the end of an employment spell, provided that a doctor certified their illness within three calendar days of losing their job. In fact, this was the only form of sick leave during a spell of unemployment, as opposed to many other European countries, where sick leave is available for registered job seekers. Thus, workers could substantially prolong the duration of social insurance benefits in case of certified sickness by using this form of sickness benefit. We will call this form of sickness benefit "sickness benefit for the unemployed" (SBU for short). Furthermore, due to differences in the eligibility conditions and benefits schedules between unemployment insurance benefits (UIB or UI benefits) and long-term sickness benefits, some groups of workers, e.g., higher-earning workers with a long employment history (more than two years), had a particularly high incentive to claim sickness benefits, even for mild health issues, instead of UIB, in order to maximize their benefit amounts.

Our descriptive analysis shows that a large proportion of SBU users take the maximum benefit duration before and after the reform. Thus, the prevalence of 90-day sick leave periods is much higher among the unemployed than that of 90-day or longer sick leave periods among the employed. Furthermore, we estimate the responsiveness of displaced workers to financial incentives and the relationship between claiming long-term sickness benefits (instead of or prior to UI benefits) and relative gains. Indeed, we find that claim behavior is likely motivated by financial gains, conditional on a set of proxies for health status.

Second, we take advantage of a 2007 policy change that halved the potential benefit duration of "sickness benefit for the unemployed" (SBU) from 90 to 45 days but left the rules otherwise unchanged. We use this radical change to estimate the effect of potential benefit duration on claiming benefits for both unemployment and sickness, as well as the duration of non-employment. Using a difference-in-differences method, we compare job endings from the five-month period before the reform to job endings from the five months following the reform. Furthermore, we use the same periods from one year prior to control for seasonal differences.

We find that reducing the potential duration of SBU benefits substantially increases the probability of taking up unemployment benefits. On average, the reform results in 16 fewer days of SBU and 12 more days of UIB among SBU-takers. These findings suggest that many SBU recipients replaced lost SBU days with unemployment benefits, though not all of them.

In principle, the reduction in the potential duration of social insurance benefits could encourage job search and hence increase the probability of finding a job. Contrary to much of the international literature on unemployment insurance benefits (e.g. Nekoei & Weber, 2017; van Ours & Vodopivec, 2008), we do not find an increase in job finding one year after job loss. This finding is consistent with the idea that at least some long-term sickness benefit recipients have health conditions (as opposed to purely fraudulent claims) and is consistent with previous studies from Hungary (Galasi & Nagy, 2002). However, we show that a reallocation of job finding occurred from the week after the pre-reform maximum duration to the week after the new maximum duration, resulting in more individuals finding jobs within one week of the new expiration date of the "sickness benefit for the unemployed". These findings are due entirely to job finding at new employers and not due to returns to the previous employer. Furthermore, this reallocation occurs only among subgroups of affected individuals, especially those with higher wages and longer employment histories. Based on point estimates in our heterogeneity analyses, we also show that this reallocation of job finding occurs among individuals who were not on sick leave during the final days of their employment, however we do not find any heterogeneous impacts based on last year's health spending.

Our paper contributes to a narrow literature that highlights the interplay between unemployment insurance benefits and sickness insurance benefits by presenting evidence that workers use sickness benefits and unemployment benefits as substitutes. These papers analyze interesting policy settings in which the authors find evidence that recipients misuse sickness benefits as an alternative to or extension of unemployment insurance benefits. Notable examples come from Sweden (Hall, 2011; Hall & Hartman, 2010; Larsson, 2006) and Norway (Henningsen, 2008). Spikes in sick leave reports as UI benefits are exhausted have been documented by Larsson (2006) and Henningsen (2008). It has also been shown that incentive effects arise for high-wage earners due to the more generous sickness insurance benefit compared to the unemployment insurance benefit, which can be attributed to different replacement rates or maximum amounts of the two types of benefits (Hall, 2011; Henningsen, 2008). Our paper contributes to this literature by examining a unique policy setting, where job losers have only three days to apply for sickness benefits. Our paper is also related to the broader literature on social insurance schemes. More specifically, this literature shows that reforming segments of social security in isolation often leads to limited success, as unintended consequences and inflow to other segments of the welfare system may play a significant role.[15]

Second, a large body of research has been dedicated to estimating the impact of potential unemployment insurance benefit duration on non-employment duration (and re-employment wages). Our contribution to this literature is to show that a decrease in the potential benefit duration of "sickness benefit for the unemployed" affects the job-finding rate of unemployed individuals after exhausting benefits similarly to a decrease in the potential benefit duration of unemployment benefits. Much of the European literature finds a moderate elasticity of unemployment to potential benefit duration; however, there is significant variation in the findings (see Filges et al., 2018, for a review). Early work in Eastern Europe studying reforms to unemployment insurance benefit systems in the 1990s found that the transition rate to regular employment was moderately responsive to potential benefit dura-

[15] Some further related studies have showed that different requirements for receiving unemployment insurance benefit – vacancy referrals and job search monitoring – lead to the unintended consequence of unemployed individuals transitioning from unemployment to sickness or disability insurance (Brouwer et al. (2019) – Belgium, Lammers et al. (2013) – the Netherlands), van den Berg et al. (2023) and van den Berg et al. (2019) – Germany.

tion. This could be explained by the relatively small changes in potential benefit duration studied and the high structural unemployment during the studied period, which occurred after the transition from socialism. However, during periods of economic growth, a marked difference in the effects of benefit shortening was observed in Slovenia, as reported by van Ours and Vodopivec (2006)[16] which greatly sped up job-finding and in Hungary, as studied in Galasi and Nagy (2002) which did not find any positive effect of shortening unemployment insurance benefit entitlements on the transition to employment.

Additionally, attention is growing on the impact of potential benefit durations and replacement rates of sickness-related social insurance benefits. This is due to a pronounced rise in temporary disability benefit claimants and long-term sickness absences.[17] In Hungary, for example, subsequent governments claimed that reducing the length of sickness benefits for the unemployed was an effort to curb misuse of the system.[18] Many studies have shown that the duration of long-term sickness is responsive to the sick pay. However, the estimated elasticity of the number of days spent on long-term sick leave to sick pay ranges from 0.9 in Finland (Böckerman et al., 2018), through 0.45 in Hungary (Csillag, 2019) to nearly zero in Germany (Ziebarth, 2013). Several papers have investigated temporary disability benefits. In particular, Fevang et al. (2017) show that the return-to-work rate of temporary disability benefit recipients is responsive to economic incentives, though, to a lesser extent than UI benefit claimants. Andersen et al. (2019) also show that temporary disability claims are responsive to local labor market conditions in a similar way to UI benefit claims.[19]

Our paper is structured as follows. We describe the sickness and unemployment insurance benefit system in Hungary, as well as the policy change that we exploit in Section 2.2. This is followed by by an exposition of the dataset and an explanation of the construction of our sample and variables of interest in Section 2.3. We detail our empirical strategy and identifying assumptions in Section 2.4 and we present our results in Section 2.5. Section 2.6 concludes with a brief discussion.

[16]It should be noted that the Slovenian reform entailed not only larger reductions in potential benefit duration, but more activation and stricter job search monitoring was also introduced at the same time.

[17]This was particularly evident in some countries, such as in the Netherlands and Norway, as well as during the Great Recession in the US (see Maestas et al., 2021).

[18]Entry to disability benefits was fairly lenient in Hungary in the 1990s, and benefit levels were moderately high. This meant that 12 percent of the working-age population was on disability benefits in 2003.

[19]Bratsberg et al. (2013) show that mass layoffs lead to an increase in disability benefit claims, and that up to one-fourth of all disability claims are possible to attribute to job loss. Similarly, in Hungary, Bíró and Elek (2020) document a 50% increase in transitions to disability pensions following job loss due to mass layoffs. From the time path of health expenditures, they conjecture that this increase might be due to the diagnosis of previously undetected chronic diseases.

2.2 Sickness and unemployment insurance benefits in Hungary

2.2.1 Sickness benefit eligibility and rules

All employees in Hungary are covered by the Statutory Health Insurance, which covers absences due to both work-related or non-work-unrelated illnesses. Sick leave comprises two components: short-term and long-term sick leave. Short-term sick leave covers up to 15 working days in a calendar year, and employers are required to pay the sick pay[20].It is important to emphasize that this paper is about long-term sick leave. After exhausting their short-term sick leave, individuals can enter long-term sick leave under the condition that they have contributed to mandatory health insurance (to the Hungarian National Health Insurance Fund). Employees receive the sickness benefit while on long-term sick leave. The sickness benefit is co-financed by the employer (one-third) and social security (two-thirds). As with short-term sick pay, a general practitioner (GP) or specialist must certify the health condition, and there is no waiting period for sickness benefits. In general, a person applying for sickness benefits needs to be employed in a position that entails sickness insurance.[21].

A worker with a health impairment is entitled to long-term sick pay for a maximum of one year, unless they were insured for less than a year, in which case the length of the entitlement is equivalent to the duration of the insurance relationship. In this case, the number of sick leave days used by the worker during the 365 days prior to applying for new long-term sick leave is subtracted from the maximum entitlement period.

The amount of the sickness benefit depends on insurance (work) history, previous earnings and its replacement rate is lower than that of short-term sick pay.[22]. If an employee had at least 180 paid working days in the previous calendar year, for which they received earnings, then their sickness benefit is based on their daily average earnings during that period. Otherwise, the "reference period" for calculating previous earnings is the last employment period in which the employee was paid for 180

[20]Short-term sick leave is only paid if an employee's health condition prevents them from working, as certified by a general practitioner. There is no waiting period, and employees receive 80 percent of their earnings as sick pay, fully paid by the employer. All employees are entitled to employer-financed short-term sick leave; however, self-employed individuals, company owners, and those working under a civil law contract are not.

[21]This includes a much wider array of employment relationships than short-term sick leave does, for example, self-employment.

[22]In this respect, the sickness insurance system is very similar to those of several European countries, such as Austria or Germany. It is worth pointing out that there is no distinction between full- and part-time jobs in terms of health insurance; every day a person is insured counts, regardless of the number of hours worked. Likewise, part-time sickness leave is not an option.

consecutive days. For those without such a period of employment, sickness benefits are based on the statutory minimum wage.

The second building block for calculating sick pay is the replacement rate. Those with at least two years of continuous work experience have a replacement rate of 70 percent, while those with shorter work histories have a replacement rate of 60 percent, with no cap on the maximum benefit. Work histories with breaks of no more than 30 days count as continuous. Breaks are periods when an individual's health insurance is suspended or they are uninsured (e.g., unpaid leave, employer-initiated or unlicensed absences from work, incarceration, or unemployment). Note that periods of licensed sick leave, parental leave, and UI benefits do not count as breaks.

2.2.2 "Sickness benefit for the unemployed" and its reform in 2007

During the period studied, persons whose employment relationship recently ended were eligible for a specific version of long-term sickness benefit, known as "sickness benefit for the unemployed" or SBU for short given that the person still had sick leave days remaining. Individuals had to apply within three days after a job ending. (This was called "passive long-term sickness benefit" in Hungarian, to distinguish it from"active" long-term sickness benefits. "Passive" meant that there was no"active" employment spell to which the sickness period would relate.) There were two cases in which a person could apply for sickness benefit after job loss. First, if an ongoing sickness benefit spell occurred at the end of the employment relationship. Second, if a sickness began immediately after, within three calendar days of the end of the employment period.

Sickness benefits for the unemployed could be granted for up to 90 days after the end of an employment period, provided that the individual had at least that many days of sickness benefits remaining.[23] The maximum number of days for receiving "sickness benefit for the unemployed" was reduced to 45 for periods beginning after April 1, 2007. The amount of the sickness benefit is determined in the same way as the standard sickness benefit.

The possibility of receiving sickness benefits after an employment period ended was established in Act II of 1975 on Social Security, which allowed workers 15 days to apply for benefits after losing their job. This period was reduced to three days in 1995 (see Act XII of 1995). Originally, the potential

[23] Thus, this means that the individual had an employment relationship that lasted at least 90 days. In other words, the length of the employment relationship minus the number of days of sickness absence exceeded 90.

duration of paid "sick leave for the unemployed" was 365 days, the same as for employed applicants. However, due to increased costs, the maximum duration was gradually reduced through a series of reforms. One such reform, which took effect on April 1, 2007, is the focus of this article (see Act CXXII of 2006, Section 15). (4)).

Figure 2.1 illustrates how the reform affected different employees. Six scenarios are depicted along two dimensions: high or low wages (three or one-and-a-half times the minimum wage) and short, medium, or long employment histories (half a year, one year, or four years). In each figure we plot five benefit schedules: take-up of UIB, take-up of SBU before and after the reform and take-up of UIB and SBU before and after the reform.

The following insights can be gained from the figure. First, those with shorter employment histories gained more from the sickness benefit, as it was their only source of income during unemployment. This also means that the reform affected them the most since the potential period of total benefit receipt was effectively halved for them. Second, gains from claiming sickness benefits increase with previous earnings, particularly above the UI benefit earnings cap, which affected a significant portion of workers.

2.2.3 The use of long-term sickness benefit in Hungary

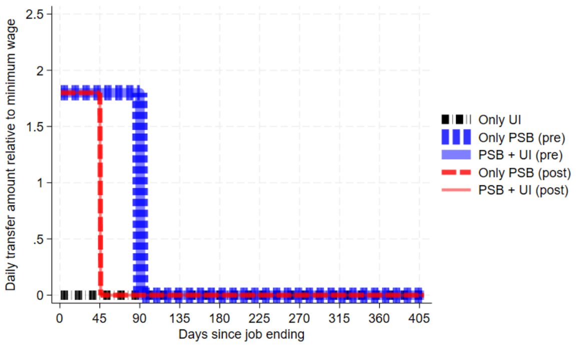
Overall, approximately 3 percent of employees were on sick leave, on average, in 2006, including "passive" sick leave. Of the total sick leave days, approximately 11 percent were "sickness benefits for the unemployed" (KSH, 2014). Spending on long-term sickness benefits amounted to around 0.4 percent of GDP. The number of total long-term sick days per (insured) person was around 13 days per year in 2006, which is comparable to statistics reported in Austria and Germany.

The main argument used by governments proposing cuts to long-term sickness benefits for the unemployed was that it would provide incentives to return to work. In their supporting arguments for later reforms to sickness benefits (not only for the unemployed), the government explicitly mentioned "free-riders" who took sick leave without good reason.[24] The decrease in the maximum duration of "sickness benefit for the unemployed" from 90 to 45 days is reflected in the decreased share of days spent in sickness leave for the unemployed relative to all sickness benefit days in 2007 – about 8 percent

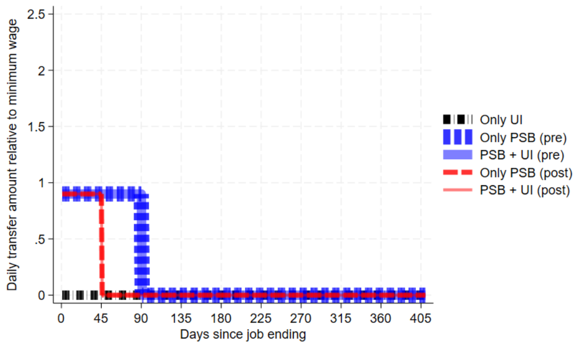
[24] Clearly, the threat of sanctions was the main deterrent to fraudulent sickness benefit claims. However, legislation regarding fraudulent claims did not significantly strengthen until 2012, when fraudulent claims became a felony and financial penalties were introduced for doctors who certify unwarranted sick leave.

Figure 2.1: Illustration of the reform for people in different situations based on their employment history and wage

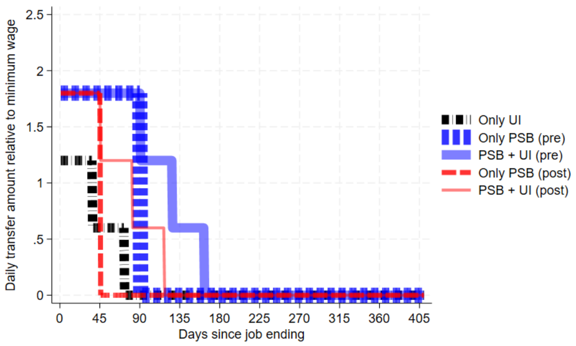
(a) High wage, half year of employment



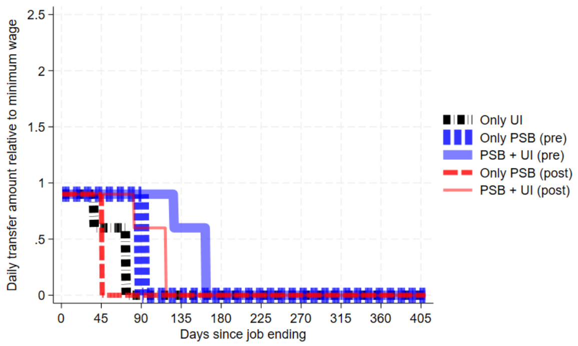
(b) Low wage, half year of employment



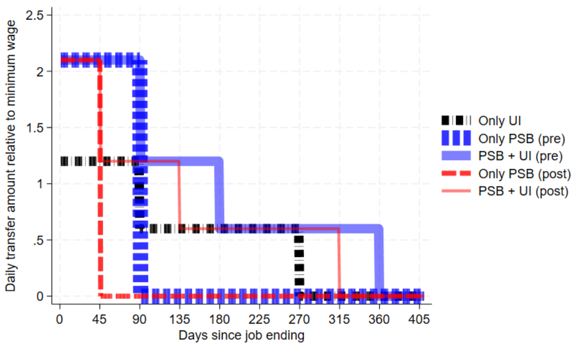
(c) High wage, 1 year of employment



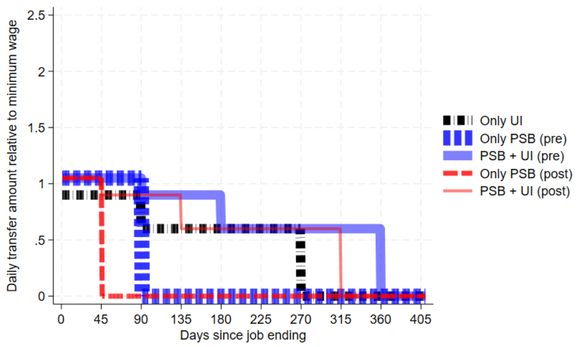
(d) Low wage, 1 year of employment



(e) High wage, 4 years of employment



(f) Low wage, 4 years of employment



Note: The figures illustrate how benefit schedules changed due to the reform for different scenarios based on wages and employment histories. A high wage is three times the minimum wage and a low wage is 1.5 times the minimum wage. 1.5 (3) times the minimum wage is the 82nd (94th) percentile of the wage distribution for males in our sample. Note that the employment history measures used to determine maximum unemployment insurance and sickness benefits are not the same. In the illustrations, we consider the measure that determines the maximum UI benefit duration: the number of days employed over the last four years. However, we use the continuous employment history variable (used to calculate the potential duration of sick leave) to show how many people could be represented by the different figures. In 2007, 180 days (365 days, or four years) was the 20th, 40th, and 74th percentile of the distribution for males in our sample.

in 2008.

2.2.4 Unemployment insurance benefit eligibility and rules

Similarly, all employees are eligible for unemployment insurance benefits (UIB or UI benefits, for short), which are significantly less generous for high earners. Eligibility for UI benefits was based on the number of insured days worked during the last four calendar years, with a minimum of one year of work during this period. Essentially, for every five calendar days of employment, a worker earned the right to one day of UI benefits. Thus, from 2006 to 2010, UI benefit eligibility could range from 72 to 270 days. It is important to note that people who voluntarily quit had a 90-day waiting period imposed. However, we cannot observe how jobs ended in the data we use and therefore cannot see whether voluntary quitters used sickness benefits. Finally, UI benefit recipients were generally not eligible for sickness benefits.

The UI benefit entitlement period was divided into two equal subperiods. The first subperiod was at most 90 days. UIB was conditional on an "active" job search. Refusal of behavioral conditions could lead to complete withdrawal of UIB. However, monitoring was not intense; job seekers generally met with their caseworkers once every three months. During the first period, job seekers received an earnings-related benefit; during the second, they received a flat-rate payment. Generally, the UI benefit replaced 60 percent of the person's prior earnings during the first period. This was calculated as the average earnings over the last full calendar quarter. However, UI benefits had relatively tight upper and lower caps: they could not be less than 60 percent of the minimum wage or more than 60 percent of twice the minimum wage. Roughly 43 percent of male full-time employees were affected by the maximum rule, and around 15 percent had earnings below the minimum rule. We must emphasize that, unlike in many other European countries, UI benefit recipients' health insurance is covered by the state, but they are not eligible for paid sick leave. Thus, for a long-term sick person, the only option was to apply for sickness benefits instead of UI benefits.[25]

[25]It is worth noting that, at the time, there was no "temporary disability benefit" (or similar) in Hungary. A person whose work capacity was seriously limited due to illness or accident (who lost at least 67% of their work capacity) could apply for a disability pension, which lasted until the retirement age for pensions. Starting January 1, 2008, this system was overhauled with the introduction of the "rehabilitation benefit," which applied to individuals who lost 50-79% of their work capacity but could, in principle, return to work following vocational rehabilitation. This benefit had significantly higher replacement rates than the disability pension but could be claimed for up to three years.

Table 2.1: Summary of benefit rules

	Passive sickness benefit	Unemployment benefit	Tempted to take-up PSB
Eligibility	Apply within 3 days of job ending	Involuntary job ending[26] (or 90 days waiting period) Employed at least 365 days in past 4 years	Voluntary job end Extraordinary termination Too short employment
Maximum duration	max{ Number of days being continuously employed ; 90/45 days}	max{ (Number of days employed in past 4 years) / 5 ; 270 days}	
Possible duration range	1-90/45	73-270	
Replacement rate	70% if at least 2 years of continuous employment, 60% otherwise	60% (for max. 36-90 days)	>2 years employment
Min. amount	minimum wage * replacement rate	60% of minimum wage based on start day	
Max. amount	No limit	2*60% of minimum wage based on start day	>2*minimum wage income

2.2.5 Comparison of sickness benefit and unemployment benefit

When comparing the incentives to take up "sickness benefits for the unemployed" and/or UI benefits, some general conclusions emerge. For those who were sick at the time of job loss, it was financially beneficial to first take up sickness benefits and then UI benefits. This way, they could extend the total period of receiving social insurance benefits. In fact, this would mean receiving the higher benefit amount for twice as long, which is offered for up to 90 days for those with long enough employment histories. However, certain groups had a particularly high incentive to take up sickness benefits. First, those who had a short employment history (less than 365 days of employment within four years) and were thus not eligible for UI benefits. Second, those whose relative gain from taking up long-term sickness benefits was high due to their high earnings, which were above the UI benefit cap. Third are workers with long enough continuous employment (at least two years) to qualify for the 70% replacement rate of sickness benefits, as opposed to the 60% replacement rate for shorter continuous employment or UI benefits. Last are voluntary job quitters who would not immediately have access to UI benefits but would have to wait 90 days. However, voluntary job quitters are likely not the majority of job endings in our sample; that is, they are not those who immediately start a new job.[27]. Table 2.1

[27] It is important to note that in our data, we cannot distinguish between voluntary quits and involuntary job-loss. Card et al. (2007b) exclude voluntary job quitters by restricting their sample to individuals who receive unemployment benefits before the end of the waiting period. We cannot follow the same strategy, as anyone could receive sickness benefits for 90 days after quitting a job—which is also the waiting period for UI benefits.

shows a comparison of the benefit rules.

2.3 Data and sample

Our analysis is based on the "Admin2" data, a large linked employer-employee administrative dataset compiled by the Databank of the Centre for Economic and Regional Studies. The complete dataset contains a 50 percent sample of Hungary's adult population from 2003 to 2011[28]. It includes administrative information from various data hosts in Hungary and covers simple demographics, employment and unemployment status, occupation, wages, firm identifiers, governmental transfers, and balance sheet data.

Our primary source was the National Pension Insurance data, which contains detailed employment histories. All periods when the individual was insured – e.g., accumulated days that contribute to pensions – were recorded, including the exact dates of the beginning and end of each period, as well as the "title" of each period. For our analysis, it is crucial that long-term sickness absence spells, both "active" (taken during an employment spell) and "passive" (synonym for "sickness benefit for the unemployed") are also indicated as insured periods, as well as spells of unemployment insurance benefits.[29] We cannot observe short-term sick leave spells. This dataset allows us to calculate the number of "continuously insured days" for each individual (which determines the replacement rate) and define the "reference period" for calculating sickness benefits. The data also contains labor income data aggregated by month, enabling us to reconstruct the earnings that serve as "reference income" for sick benefits and current earnings. Similarly, we can calculate the potential length of UI benefit eligibility and the potential UI benefit for each individual. Finally, the dataset also records the person's gender, day of birth, and detailed occupation codes for employment spells.

We also use variables from the National Health Insurance Fund that provide important information on yearly healthcare spending by category: inpatient care, outpatient care, medications, and the number of visits to a general practitioner[30]. The Hungarian Public Employment Service's un-

[28]Source: https://adatbank.krtk.mta.hu/en/adatbazisok/elerheto-adatbazisok/ (last opened: October 31, 2023) Note that a new version of the linked administrative data ("Admin 3") is already available. It contains a more detailed collection of health-related variables, but it only starts in 2009, which is later than our analysis period.

[29]More precisely, they are periods that contribute towards "number of insured days", but no contributions (health or pension) are deducted.

[30]Unfortunately, besides the sum spent on an individual's healthcare, we know nothing about their illnesses or health status.

employment registers also record periods of registered unemployment and receipt of unemployment insurance benefits.

2.3.1 Sample construction

The unit of our analysis is a job ending. We consider job endings within a 10 months window – 5 months prior to, and 5 months after – around the policy change on April 1, 2007. Thus, our data includes job endings between November 1, 2006, and August 31, 2007, as well as the same periods one year earlier, the "control year".

In our estimations, we only focus on people who are eligible to receive sickness benefit for its pre-reform potential maximum period, 90 days. This ensures that they are all fully affected by the policy change, that halved the maximum amount of sick leave available to them. We exclude all job endings, where the individual moved to another firm within one week, since we assume they had already searched for and found a job during their previous employment spell; therefore, the decision to claim benefits was irrelevant to them. Finally, we only consider employment spells that lasted at least 30 days[31]

Our sample consists of prime-age males (birth cohorts 1950-1984) since sickness benefit take-up after job endings is likely related to pregnancy and childbirth for females, who are not eligible for maternity and parental leave benefits[32].

As "sickness benefit for the unemployed" is used relatively infrequently (approximately 3% of job endings), we retain all job endings followed by the uptake of sickness benefits and randomly sample 20% of all other individuals.

2.4 Empirical strategy and identification

2.4.1 Empirical strategy

We are interested in how the reform affected the use of "sickness benefit for the unemployed" and unemployment insurance benefits, as well as its impact on job finding for sickness benefit recipients. Our empirical strategy uses a difference-in-differences (DiD) approach. We compare the five-month

[31] We do not know why or how the employment spell ended. Thus, we cannot observe if the job ending was voluntary.

[32] To be eligible for maternity leave benefits (for the first six months of a child's life) and parental leave benefits (from six months until the child's second birthday), one must have at least 365 days of employment within two years of giving birth and be employed at the time of birth.

period before and after the reform, as well as the same period one year earlier, which we use as a control. We use the "control year" to eliminate any seasonal patterns that could bias a simple before-and-after comparison around the date of the reform. We estimate models of the following form using different outcome variables:

$$\begin{gathered} y_i = \alpha_i + \beta_1 AprilAugust_i + \beta_2 ReformYear_i + \\ \beta_3 AprilAugust_i * ReformYear_i + X_i' \eta + \varepsilon_i \end{gathered} \tag{2.1}$$

In the above equation, each observation corresponds to a job ending and the outcome, y_i, is either a binary variable indicating whether or not a benefit was taken up or a job was found within a specific time period after the job ended, or a variable showing the number of days on sickness and unemployment benefits. We estimate the models using simple OLS for both binary and continuous outcome variables.

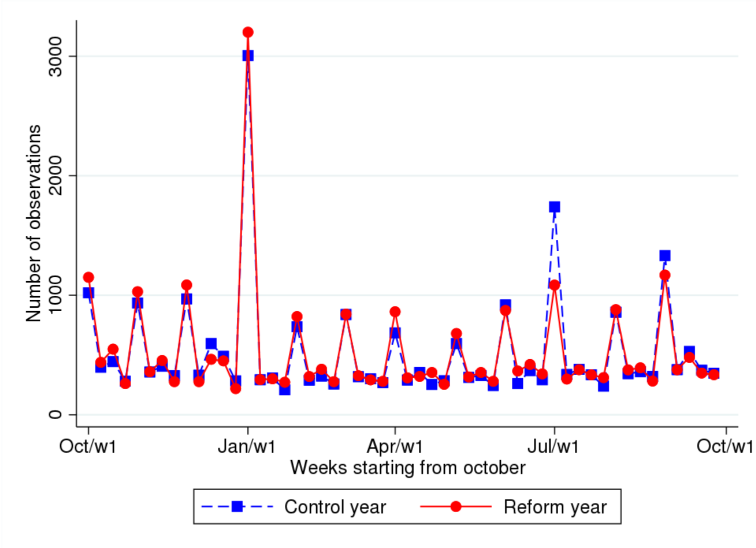
The $AprilAugust_i$ variable is 1 for the months of April through August and 0 for the months of November through March. The $Reformyear_i$ variable is 1 for observations between November 2006 and August 2007 (the reform year), and is 0 for job endings between November 2005 and March 2006 (the control year). We examine whether the policy change led to substantial changes in claiming sickness or unemployment benefits and subsequent job finding relative to the same time period one year earlier. Thus, the variable of interest is the interaction between $Reformyear_i$ and $AprilAugust_i$ with the coefficient, β_3.

2.4.2 Identifying assumptions

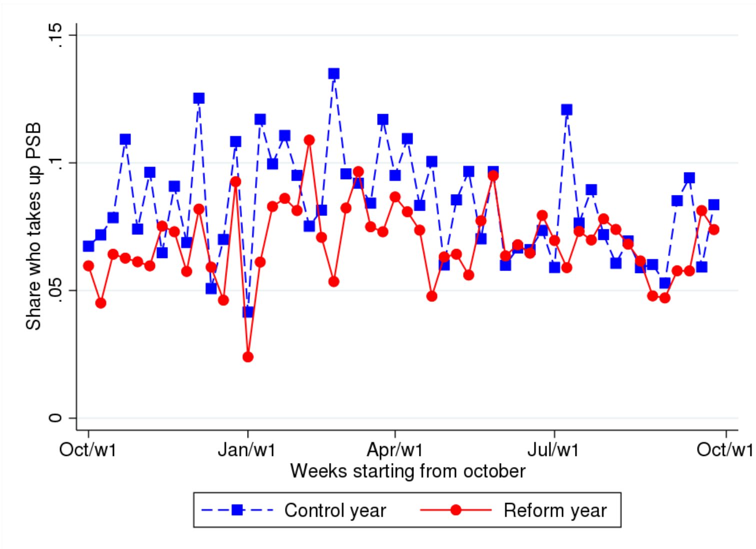
In all our estimations, our main identifying assumption is that job endings are quasi-random around the reform date and that there are no compositional changes in job losers, aside from any seasonal patterns similar in the reform and control years.

Figure 2.2: Number of job endings and share who takes up sickness benefit

(a) Number of job endings



(b) Share who takes up SBU



First, we demonstrate that there are no unusual patterns in the number of job terminations and SBU uptake around April 1, 2007, compared to the frequency of job terminations and SBU uptake in the previous year. Panel (a) of Figure 2.2 shows the weekly number of job terminations during the control and reform years. As can be seen, the weekly observation numbers are almost exactly the same in the two periods, and the seasonal patterns are very similar in the two years. Most importantly, there is no evidence of bunching before April 2007, suggesting that job endings were not strategically timed to receive the sickness benefit for a longer period. As expected, there are spikes in the first weeks of each month, with the largest spike occurring in the first week of January, reflecting the fact that most contracts end at the end of a month. (Note that the date assigned to a job ending is the first day of the subsequent period of unemployment.) The only week with a substantial difference in observations is the first week of July, when the number of job endings is much higher in the control year than in the reform year. This is due to a large number of people leaving a few larger firms. Ranking all the firm-week cells by number of observations shows that seven of the ten largest cells are from the first week of July 2006. These are job losses that occurred on June 30, 2006. To ensure that these job endings do not influence our main results, we repeated all our estimations, excluding job endings on this day, and found the same results.

Panel (b) of Figure 2.2 shows the share of job endings in which the person takes up "sickness benefit for the unemployed". Overall, the seasonal patterns are similar in the two years, but there are also some differences. It appears that the take-up rate is higher in the control year from October to May, but this difference diminishes after mid-May. This difference disappears due to a decrease in take-up in the control year rather than an increase in the reform year. Therefore, it is unlikely that these patterns are caused by the reform itself. Nevertheless, we will next report on the detailed characteristics of the groups before and after the policy change date in both the control and reform years, to see if there are any substantial compositional differences.

In Table 2.2, we present descriptive statistics for all job endings, and in Table 2.3 we present statistics for job endings that are followed by sickness benefit take-up. In the first four columns, we report the means of each variable for the four groups in our difference-in-differences analysis, followed by the estimated diff-in-diff interaction term with standard errors and t-values. The first column shows the months from November to March in the control year (November 2005 to March 2006). The second column shows the same months for the treatment year (November 2006 to March 2007). The next two columns show April to August 2006 and April to August 2007, respectively.

Based on Table 2.2, our overall sample appears to be fairly balanced, although we observe systematic differences over time that are filtered out by the difference-in-differences estimation strategy. Our coefficient estimate for the interaction term is significantly different from zero for the following: the proxy for UI eligibility; the share of people with less than 365 days of continuous employment history; and employer characteristics, such as public sector, size, and average wage. However, we do not see any such differences in our sick leave and health spending indicators.

When we examine the SBU recipient sample in Table 2.3 we reach similar conclusions with an even more balanced sample that may be partly due to the smaller sample size.

Although there are no major differences among the diff-in-diff groups in our analysis, some compositional changes occurred in the sample during the analysis period. Thus, we ran all estimations both with and without control variables and found very similar results. We report the models that include control variables.

When estimating the DiD model for job finding, an additional identifying assumption is required: the reform did not affect selection into "sickness benefit for the unemployed". This is shown in the first column of the first panel of Table 2.5.

2.5 Results

2.5.1 Descriptive analysis about the use of "sickness benefit for the unemployed"

Before discussing the effect of the reform on the uptake of the "sickness benefit for the unemployed," we present descriptive evidence on the characteristics of SBU recipients.

Based on a comparison of the summary statistics in Table 2.2 and Table 2.3 SBU recipients are slightly older (36 vs. 39), and SBU uptake does not appear to be driven by regional differences in unemployment levels. SBU recipients have longer employment histories, much higher earnings (approximately twice that of non-recipients), and higher shares of them work in white-collar jobs. A much larger proportion of them have worked in the public sector[33] (8-14% vs. 2-6%) and tend to work at larger, higher-paying firms and at firms where sick leave is more common.

SBU takers have worse health conditions, as evidenced by any health indicators related to past sick leave or healthcare spending. A much larger percentage of them have taken sick leave in the last days of their employment (15-18% vs. 6% on their last day). They are twice as likely to have been on sick leave at least once in the last 365 days (35-39% vs. 19-20%) and spent more days on sick leave (15-17 days vs. 9 days) in the last 365 days. One limitation of using past sick leave as a health proxy is that the number of sick leave days reflects health conditions, sick leave eligibility, and previous shirking simultaneously.

[33] Public employees have been shown to use sickness benefits more frequently in other countries as well. For example, in Sweden, public employees have been shown to use sickness benefits more frequently.(Lindbeck et al., 2008)

Table 2.2: Summary statistics for all job endings in our sample period

	Pre, control	Pre, treated	Post, control	Post, treated	DiD est.	S.E.	t-value
Age	36.929	37.430	36.255	36.975	0.218	0.183	1.195
Regional unemployment rate	0.080	0.078	0.079	0.077	0.000	0.000	-0.601
Proxy for UI eligibility	0.814	0.837	0.790	0.832	0.020	0.007	2.923
Less than 365 days of cont. emp. history	0.503	0.469	0.445	0.390	-0.020	0.009	-2.266
Sick pay is above UI cap	0.089	0.090	0.102	0.109	0.006	0.005	1.159
Wage at last firm	75507.258	85066.406	73128.086	88979.773	6292.530	5673.699	1.109
occupation==Manager, political/religional/ngo leader	0.037	0.034	0.031	0.034	0.006	0.003	1.862
occupation==Professional	0.032	0.036	0.049	0.056	0.003	0.004	0.769
occupation==Other white collar	0.062	0.070	0.078	0.089	0.003	0.005	0.662
occupation==Skilled blue collar	0.409	0.412	0.382	0.396	0.012	0.009	1.300
occupation==Assembler, machine op.	0.162	0.176	0.156	0.180	0.010	0.007	1.543
occupation==Unskilled laborer	0.294	0.269	0.301	0.241	-0.035	0.008	-4.306
occupation==No info	0.003	0.002	0.004	0.003	0.001	0.001	0.829
Public sector employer	0.024	0.033	0.041	0.058	0.009	0.003	2.587
No. of employees at last firm	3432.459	3039.039	4949.634	4001.943	-554.270	170.806	-3.245
Firms's average wage	0.700	0.710	0.743	0.783	0.030	0.010	3.117
By employer: % of sick leave last year	2.066	1.992	2.045	2.065	0.094	0.062	1.519
Continuous insurance period (censored 365 days)	286.232	292.972	284.303	294.418	3.375	1.754	1.925
Sickness benefit base amount (HUF 1000)	3.166	3.447	3.456	3.884	0.147	0.081	1.821
Was on sick leave during last 7 days of employment	0.058	0.055	0.059	0.057	0.000	0.004	0.021
At least 1 day sick leave during last 365 days	0.193	0.191	0.213	0.203	-0.008	0.007	-1.122
Days on sick leave during last 365 days	8.935	8.756	9.248	8.960	-0.109	0.553	-0.197
Days when eligible for sick leave during (last 365 days)	292.688	299.246	296.589	303.311	0.164	1.544	0.106
Share sick leave/eligible days	0.028	0.027	0.029	0.028	-0.001	0.002	-0.606
No. of sick days in last calendar year	6.802	6.580	5.727	6.017	0.513	0.472	1.087
Non-zero sickness days last cal. year	0.150	0.154	0.150	0.154	0.000	0.007	-0.018
Total health spending last year	48.704	49.286	48.932	53.227	3.714	2.863	1.297
Total spending on drugs (individual+SocSec) last year	19.521	22.547	20.610	24.109	0.473	1.567	0.302
Total spending on in+outpatient care last year	29.183	26.739	28.322	29.118	3.240	2.020	1.605
Positive outpatient care spending	0.880	0.868	0.874	0.872	0.010	0.006	1.654
Positive inpatient care spending	0.110	0.104	0.108	0.105	0.003	0.006	0.489
Health data is missing	0.186	0.182	0.175	0.168	-0.003	0.007	-0.393
Returns to s.e. within a year if finds job	0.240	0.223	0.131	0.102	-0.012	0.007	-1.776
Returns to last employer (cond. on finding job)	0.346	0.329	0.201	0.157	-0.027	0.010	-2.771
Observations	14554	14839	10628	10197			

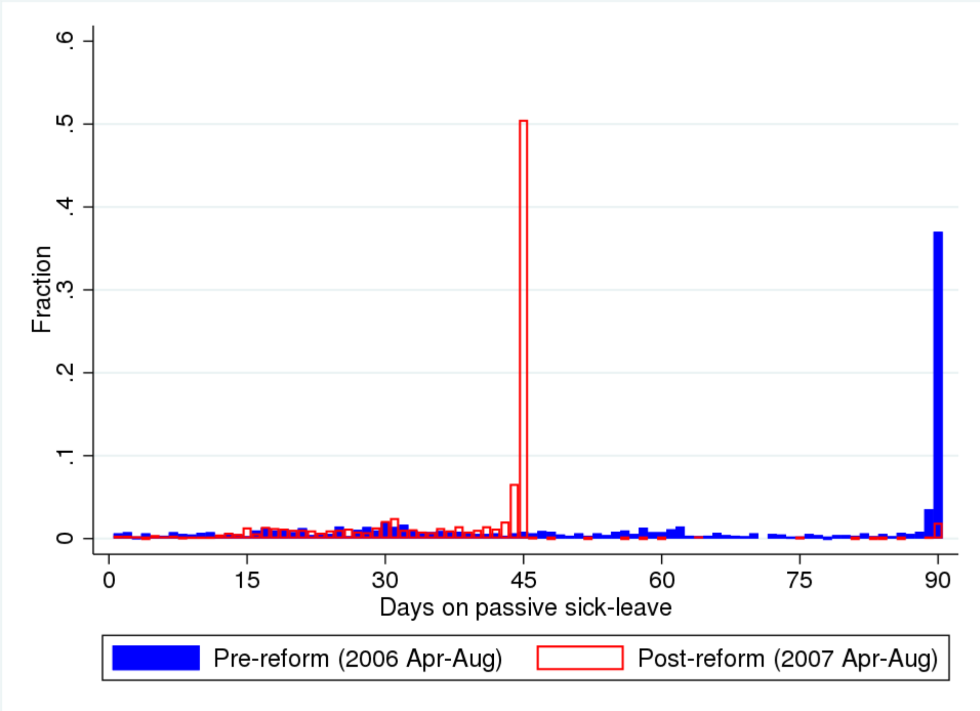
Note: Pre, control: Nov 2005 – Mar 2006; Pre, treated: Nov 2006 – Marc 2007; Post, control: Apr 2006 – Aug 2006; Post, treatment Apr 2007 – Aug 2007

Table 2.3: Summary statistics for job endings followed by the take up of "sickness benefit for the unemployed"

	Pre, control	Pre, treated	Post, control	Post, treated	DiD est.	S.E.	t-value
Age	39.280	39.920	39.660	39.085	-1.215	0.503	-2.417
Regional unemployment rate	0.080	0.079	0.079	0.078	0.000	0.001	0.117
Proxy for UI eligibility	0.881	0.888	0.912	0.932	0.014	0.015	0.931
Less than 365 days of cont. emp. history	0.334	0.326	0.267	0.228	-0.032	0.022	-1.442
Sick pay is above UI cap	0.251	0.256	0.264	0.267	-0.002	0.022	-0.103
Wage at last firm	173526.625	185805.781	144415.625	195411.078	38716.309	38294.945	1.011
occupation==Manager, political/religional/ngo leader	0.077	0.072	0.086	0.087	0.006	0.013	0.474
occupation==Professional	0.079	0.083	0.087	0.109	0.018	0.014	1.310
occupation==Other white collar	0.107	0.127	0.135	0.118	-0.038	0.016	-2.352
occupation==Skilled blue collar	0.304	0.313	0.285	0.286	-0.009	0.023	-0.388
occupation==Assembler, machine op.	0.207	0.203	0.185	0.217	0.035	0.020	1.776
occupation==Unskilled laborer	0.226	0.201	0.221	0.183	-0.013	0.020	-0.640
occupation==No info	0.000	0.001	0.001	0.001	-0.001	0.001	-0.537
Public sector employer	0.084	0.110	0.124	0.144	-0.007	0.016	-0.421
No. of employees at last firm	5858.547	5431.874	6655.097	6007.171	-221.252	576.965	-0.383
Last firm's average wage	1.112	1.079	1.082	1.129	0.080	0.035	2.277
By employer: % of sick leave last year	2.780	2.597	2.817	2.774	0.140	0.141	0.994
Continuous insurance period (censored 365 days)	312.500	313.876	322.292	329.993	6.325	4.040	1.566
Sickness benefit base amount (HUF 1000)	5.349	5.655	5.509	6.075	0.260	0.357	0.727
Was on sick leave during last 7 days of employment	0.186	0.159	0.170	0.148	0.004	0.018	0.235
At least 1 day sick leave during last 365 days	0.396	0.351	0.395	0.361	0.011	0.024	0.457
Days on sick leave during last 365 days	17.066	15.098	17.795	15.803	-0.025	1.835	-0.013
Days when eligible for sick leave during (last 365 days)	317.126	318.474	328.947	333.920	3.626	3.502	1.035
Share sick leave/eligible days	0.053	0.047	0.054	0.048	0.000	0.006	0.037
No. of sick days in last calendar year	12.199	11.525	11.106	10.989	0.556	1.672	0.333
Non-zero sickness days last cal. year	0.281	0.259	0.254	0.260	0.028	0.022	1.293
Total health spending last year	57.151	61.777	59.946	63.384	-1.187	6.571	-0.181
Total spending on drugs (individual+SocSec) last year	22.511	31.451	26.986	32.216	-3.710	3.323	-1.116
Total spending on in+outpatient care last year	34.640	30.326	32.960	31.168	2.522	5.085	0.496
Positive outpatient care spending	0.897	0.903	0.906	0.893	-0.019	0.015	-1.267
Positive inpatient care spending	0.132	0.115	0.126	0.118	0.009	0.016	0.584
Health data is missing	0.094	0.083	0.107	0.092	-0.004	0.014	-0.283
Returns to s.e. within a year if finds job	0.131	0.144	0.100	0.067	-0.046	0.016	-2.973
Returns to last employer (cond. on finding job)	0.194	0.214	0.154	0.101	-0.073	0.022	-3.234
Observations	1751	1729	1583	1537			

Note: Pre, control: Nov 2005 – Mar 2006; Pre, treated: Nov 2006 – Marc 2007; Post, control: Apr 2006 – Aug 2006; Post, treatment Apr 2007 – Aug 2007

Figure 2.3: Distribution of "sick leave for the unemployed" spell lengths before and after the reform



Additionally, we demonstrate that, over the past 365 days, the proportion of sick leave days relative to all insured days is 5% for SBU takers versus 2.8% for the entire sample.

Although health spending may capture the actual health status of individuals slightly better, we only observe these variables at the yearly level. Therefore, we always report values from the last calendar year. SBU takers are more likely to have positive inpatient and outpatient care spending, or any spending, during the last calendar year. Their total healthcare spending — including inpatient and outpatient care, as well as medication spending — during the last calendar year is also higher (52,000 vs. 35,000 HUF). It is important to note the limitations of interpreting health care spending as health proxies. Health care spending also reflects access to health care, and we cannot disentangle how much of the difference between the two groups is due to actual differences in health status versus differences in access (e.g., better network, higher income).

Figure 2.3 shows the distribution of sickness benefit spell lengths after job endings that started during the five months following the reform (April–August 2007, or post-reform) and during the same period of the previous year. In both periods, there is significant bunching at the maximum duration of "sick leave for the unemployed." In 2006, 37% of the spells were 90 days long. In the five post-reform months of 2007, half of the spells lasted 45 days, the new maximum. Besides the clustering of observations above 45 days at the new maximum, there are no other differences between the two distributions under 45 days.

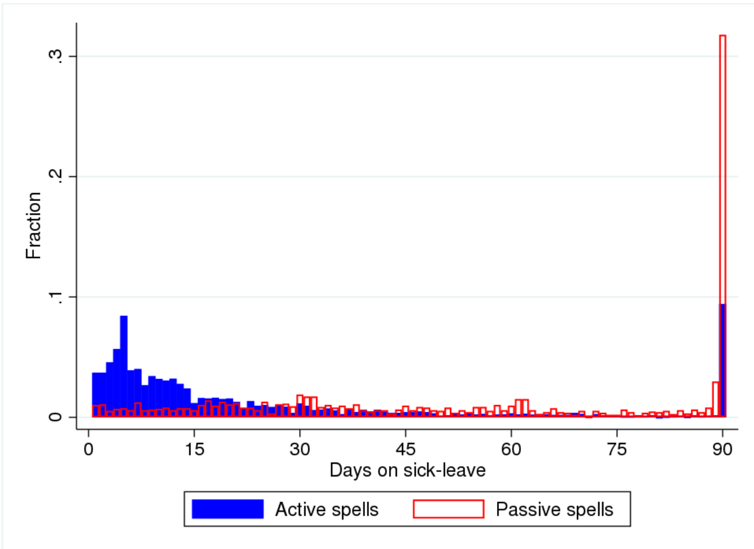
To get an idea of how typical 90+ or 45+ day sick leave spells are for the employed ("active" spells), we plot the "passive" (SBU) sick leave length distributions for these two periods next to the "active" sick leave length distributions starting in the same periods on Figure 2.4. Note that "active" sick leave spells can last up to 365 days. However, we censor the data at the maximum duration of "sick leave for the unemployed." For example, in panel (a), the 90-day spell length includes all "active" spells that are 90 days or longer. The distributions of "passive" and "active" sickness spell lengths look strikingly different. There is a much larger proportion of short spells (below 15 days) among employed people on sick leave. Only 9% of "active" spells are 90 days or longer in panel (a) (versus 32% of passive spells in 2006), and only 20% of "active" spells are 45 days or longer in panel (b) (versus 50% of passive spells).

These striking differences may reflect different underlying causes. First, people on "passive" sick leave and people on "active" sick leave can have very different underlying illnesses and health statuses for two reasons. First, longer, more serious illnesses can lead to job loss, so these periods may be longer due to more serious reasons for sick leave. Reverse causality is also possible: losing one's job can lead to a worsened health situation, such as depression or other mental health problems, which might be associated with longer periods of sick leave. Second, this large difference may indicate the misuse of "sick leave for the unemployed." There is a clear incentive for sickness benefit recipients to stay on sick leave as long as possible, thereby extending the duration of potential benefit receipt and receiving a higher amount than the unemployment benefit would provide to people with a longer employment history and higher wages.

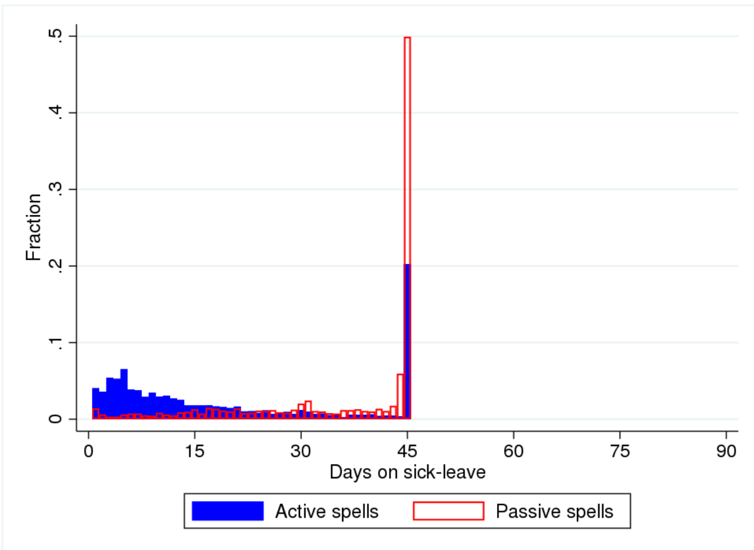
Another way to show that the "sickness benefit for the unemployed" was likely used as insurance against unemployment is to plot the inflow of people receiving the benefit over time relative to job loss. Figure 2.5 shows the percentage of people who reported being sick in a given week relative to their job ending, for the year prior to and the first week after their job ending. The rate at which people report being sick remains fairly constant at around 0.6-0.7% for most of the time, until about five weeks before job loss, when it increases to 0.9-1%. However, there is a clear spike in the first week after job loss, when 4% of people go on sick leave. Note that some fraction of these people would have reported sick even if they had not lost their job, and we would not see this if they were still using their

Figure 2.4: Distribution of "sick leave for the unemployed" spells and sick leave spells of employed people

(a) 2006 April - August

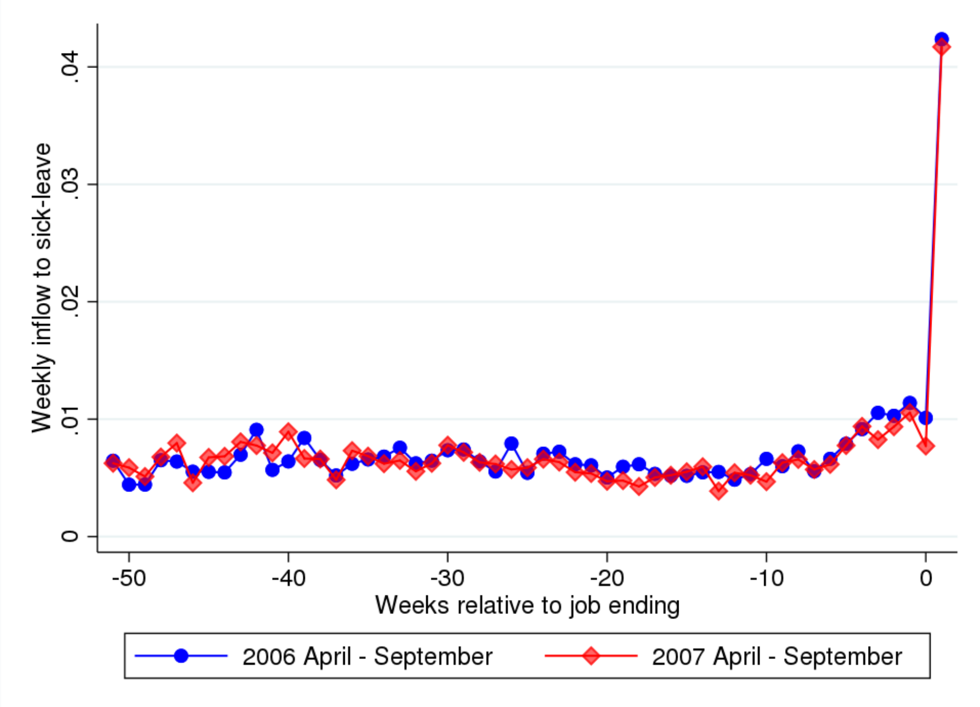


(b) 2007 April - August



Note: Sick leave spells of employed people ("active" sick leave spells) are censored at 90 days on panel (a) and at 45 days on panel (b), thus for "active" spells the bar at 90 and 45 in panel (a) and (b) respectively represent spells that are at least 90 or 45 days long.

Figure 2.5: Inflow to sickness benefit by weeks relative to job ending



Note: The figure plots the share of people entering sick leave during the year before job-ending and on the first week after job ending. Observations are weighted in accordance with the sampling procedure, which takes a 20% sample of people who do not take up sickness benefit after job loss.

short-term sick leave.

2.5.2 Drivers of "sickness benefit for the unemployed" take-up

Table 2.4 reports estimates from a model in which the outcome variable is the take-up of sickness benefits for employed individuals. The focus is on the association between belonging to certain subgroups of the population for whom the amount of sickness benefits is higher than that of unemployment insurance benefits[34]; these groups are those who are not eligible for unemployment insurance benefits[34]; those who have had a continuous insurance period of more than two years and are thus eligible for a replacement rate of 70% of their previous earnings (versus 60% for shorter periods); and those who likely hit the UIB cap (two times the minimum wage) due to their higher income.

Even after controlling for regional unemployment, health indicators, and employment history, we found that people with more than two years of continuous insurance were 5.5 percentage points more likely to take up sickness benefits after job loss. Additionally, people with income above the UIB

[34]The binary variable that we include in the estimation is the opposite: 1 if someone is eligible for UIB

Table 2.4: The determinants of "sickness benefit for the unemployed" (sample period: November 2005 – August 2006)

VARIABLES	(1) PSB take-up	(2) PSB take-up
Proxy for UI eligibility	0.036***	0.027***
	(0.004)	(0.004)
Dummy: continuous insurance period > 2 yrs	0.091***	0.055***
	(0.005)	(0.005)
Sick pay is above UI cap	0.220***	0.098***
	(0.009)	(0.010)
Age		0.000
		(0.002)
kor2		0.000
		(0.000)
Regional unemployment rate		0.391***
		(0.086)
No care, only outpatient, in- and outpatient care last year = 1, Outpatient care		0.018***
		(0.005)
No care, only outpatient, in- and outpatient care last year = 2, In- and outpatient care		0.017**
		(0.008)
Health data is missing		-0.033***
		(0.004)
Share sick-leave/eligible days		0.091***
		(0.033)
Public sector employer		0.166***
		(0.017)
occupation==Manager, political/religional/ngo leader		0.159***
		(0.014)
occupation==Professional		0.065***
		(0.014)
occupation==Other white collar		0.093***
		(0.010)
occupation==Skilled blue collar		0.038***
		(0.005)
occupation==Assembler, machine op.		0.061***
		(0.007)
occupation==Unskilled laborer = 0,		-
occupation==No info = 0,		-
l_last_size		0.011***
		(0.001)
l_last_firm_avwage		0.106***
		(0.006)
By employer:		
		(0.001)
Constant	0.050***	-0.058*
	(0.003)	(0.031)
Observations	30,222	28,212
R-squared	0.068	0.160
Mean outcome	0.132	0.132

Note: Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05.

cap were 9.8 percentage points more likely to do so. We also found that the probability of taking up sickness benefits increased by 2.7 pp for those likely eligible for UIB (approximately 90% of job losers). While people ineligible for UIB have more incentive to take up SBU and thus we would expect to find a negative coefficient here, we cannot identify these individuals precisely, and this regression analysis cannot be interpreted causally. Additionally, we observe that higher regional unemployment rates, previous healthcare spending, and sick leave usage are positively correlated with SBU uptake. People who are white-collar workers, have worked in the public sector, or have worked at larger, higher-paying firms where sick leave is more prevalent are more likely to receive sickness benefits after losing their job.

These results show that, when health, local labor market conditions, occupation, employment, and earnings history are similar, people with higher financial incentives are more likely to take up sickness benefits. If financial incentives drive the decision to take up sickness benefits, we might worry that these people would not actually need to be on sick leave. On the other hand, this relationship may mask an underlying mechanism. For example, people with higher incomes may also be more educated and have more knowledge about claiming sickness benefits.

2.5.3 Impact on benefit take-up

In this section, we evaluate the effect of shortening the duration of "sickness benefits for the unemployed" on the uptake of SBU and UIB. First, we examine transfer take-up using linear probability models for all job endings and job endings followed by some benefit, as well as for SBU recipients in the three panels of Table 2.5. The first column shows the binary outcome of taking up "sickness benefits for the unemployed." There is no impact on SBU take-up. The second column shows whether the individual took up unemployment insurance benefits and whether they did so right after job loss in the first week of unemployment. We find an overall increase of 1.6 percentage points in UIB take-up in response to the reform. This increase is likely entirely due to SBU takers, for whom we observe an increase of 11.7 percentage points in the take-up of unemployment insurance benefits in Panel C. This suggests that some SBU takers substituted lost sick leave days with unemployment insurance benefits.

The last two columns of Table 2.5 report findings that detail the timing of inflow to unemployment insurance benefits after job loss. In columns (4) and (5), we show UIB take-up for the week following the exhaustion of the maximum sick leave duration before and after the policy change. Our outcome

Table 2.5: Impact on sickness benefit and unemployment benefit take-up after job loss

A. All job endings					
	(1)	(2)	(3)	(4)	(5)
	SBU	UIB - within 97	UIB - 1st week	UIB - on 46-52	UIB - on 91-97
April-August	0.004	-0.065***	-0.073***	0.001	0.002
	(0.003)	(0.007)	(0.006)	(0.001)	(0.002)
Reform year	-0.014***	0.015**	-0.005	0.001	-0.003*
	(0.003)	(0.006)	(0.005)	(0.001)	(0.002)
April-August * Reform year	0.002	0.016*	-0.007	0.012***	-0.006**
	(0.004)	(0.009)	(0.008)	(0.002)	(0.003)
Observations	48,350	48,350	48,350	48,350	48,350
Mean outcome	0.133	0.415	0.292	0.008	0.034
B. SBU- or UIB-takers					
	(1)	(2)	(3)	(4)	(5)
	SBU	UIB - within 97	UIB - 1st week	UIB - on 46-52	UIB - on 91-97
April-August	0.008	-0.010*	-0.048***	0.003	0.005
	(0.006)	(0.005)	(0.010)	(0.003)	(0.004)
Reform year	-0.031***	0.018***	0.005	0.002	-0.007**
	(0.005)	(0.004)	(0.008)	(0.002)	(0.003)
April-August * Reform year	0.003	0.017**	-0.014	0.028***	-0.014**
	(0.009)	(0.007)	(0.014)	(0.004)	(0.006)
Observations	23,814	23,814	23,814	23,814	23,814
Mean outcome	0.271	0.848	0.486	0.017	0.070
C. SBU-takers					
	(1)	(2)	(3)	(4)	(5)
		UIB - within 97	UIB - 1st week	UIB - on 46-52	UIB - on 91-97
April-August		-0.033	-0.014	-0.006	-0.016
		(0.023)	(0.010)	(0.006)	(0.017)
Reform year		0.007	-0.011	0.011	-0.024
		(0.022)	(0.011)	(0.008)	(0.016)
April-August * Reform year		0.117***	0.000	0.198***	-0.105***
		(0.033)	(0.014)	(0.017)	(0.021)
Observations		6,386	6,386	6,386	6,386
Mean outcome		0.439	0.057	0.021	0.188

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is a binary variable indicating the take-up of SBU or UIB in a specific period. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table 2.6: Impact on the number of days receiving sickness benefit and unemployment benefit after job loss

A. All job endings			
	(1)	(2)	(3)
VARIABLES	Days PSB	Days UIB	Days Benefit
April-August	-0.086	-2.055*	-2.141*
	(0.196)	(1.143)	(1.164)
Reform year	-0.950***	0.663	-0.288
	(0.173)	(0.985)	(1.006)
April-August * Reform year	-1.024***	-0.483	-1.507
	(0.246)	(1.625)	(1.651)
Observations	48,350	48,350	48,350
Mean outcome	8.215	61.883	70.098

B. SBU- or UIB-takers			
	(1)	(2)	(3)
	Days PSB	Days UIB	Days Benefit
April-August	0.161	8.289***	8.450***
	(0.420)	(1.741)	(1.714)
Reform year	-1.990***	3.098**	1.108
	(0.333)	(1.355)	(1.343)
April-August * Reform year	-2.538***	-3.020	-5.558**
	(0.525)	(2.470)	(2.439)
Observations	23,814	23,814	23,814
Mean outcome	16.343	108.030	124.373

C. SBU-takers			
	(1)	(2)	(3)
	Days PSB	Days UIB	Days Benefit
April-August	-1.004	1.261	0.256
	(1.606)	(4.502)	(4.813)
Reform year	-1.991	-1.640	-3.631
	(1.610)	(4.315)	(4.626)
April-August * Reform year	-16.134***	11.889*	-4.244
	(2.098)	(6.541)	(6.926)
Observations	6,386	6,386	6,386
Mean outcome	59.782	80.745	140.528

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is the number of days when a person received SBU, UIB or any benefits in total. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

variable is now 1 if someone started their unemployment benefit spell during days 46–52 or 91–97 after job loss. Our results indicate an increased inflow to paid unemployment on days 46–52 and a decreased inflow on days 91–97 after job loss. This pattern is detectable in the entire job loser sample, amounting to a 1.2 percentage point (vs. a 0.8 percentage point pre-reform mean outcome) increase in the week after 45 days and a 0.6 percentage point (vs. a 3.4% pre-reform mean outcome) decrease in the probability of taking up UIB on these specific weeks. However, this pattern is clearly driven by SBU-takers, among whom we observe a 19.8% increase after 45 days (versus a 2.1% pre-reform mean outcome) and a 10.5% decrease after 90 days (versus an 18.8% pre-reform mean outcome).

The increase in unemployment claims during days 46-52 appears to stem from two sources: new UIB recipients (individuals who would not have used UIB absent the reform) and existing UIB recipients who begin claiming UIB benefits earlier in response to the reform. The share of people who start their UIB take-up only after 90 days after job loss is still high after the reform; e.g. it was 18.8% for SBU-takers pre-reform that decreases by 10.5 pp to 8.3%, suggesting that about 15-20% of paid unemployment spells starts on this week. This is likely due to the 90-day waiting period for voluntary quitters. Furthermore, this suggests that there may be more voluntary quitters among SBU recipients than in the general population, where the post-reform percentage of individuals beginning paid unemployment is less than 10% – 2.8% (pre-reform mean outcome of 3.4% minus the estimated coefficient of 0.6 percentage points) out of 43.1% (pre-reform mean outcome of 41.5% plus the estimated coefficient of 1.6 percentage points).

Table 2.6 shows our estimates of the impact of the reform on the number of days of sickness and unemployment insurance benefits. The reform's direct impact is evident in the reduction of sick leave days after job loss. On average, the number of days spent on sick leave after job loss decreased by one day for all job losers, while the amount of days spent on unemployment benefits remained unchanged. Focusing on SBU recipients, we see that the average number of sick leave days decreased by 16 days and was accompanied by an average increase of 12 days in unemployment insurance benefits. This suggests that a significant portion of the lost sick leave days were substituted by unemployment benefits, though likely not all.

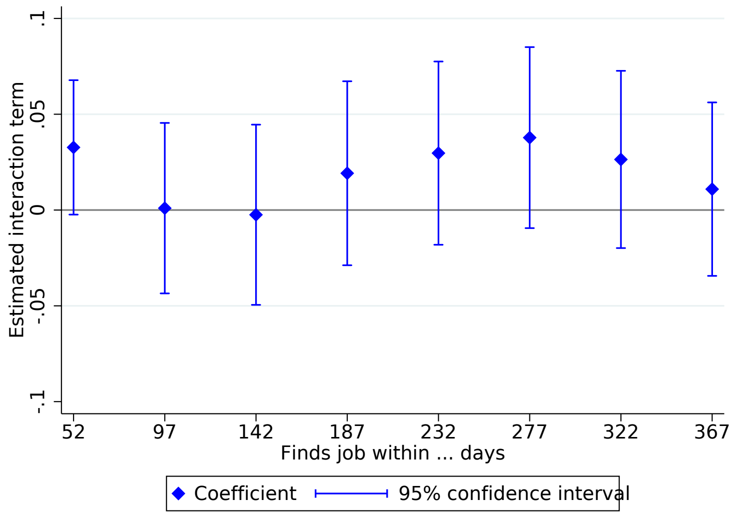
2.5.4 Impact on job-finding rate

Now, we analyze the impact of the reform on job-finding rates among SBU takers. Our findings are summarized in Figure 2.6 and reported in the in Table B.1.1 in the Appendix. Panel (a) of Figure 2.6 shows coefficient estimates of the interaction term from a series of linear probability estimations using equation 2.1 on the outcome of finding a job within 52, 97, ..., 367 days. This allows us to understand the dynamics of the impact on job finding. These outcome variables reflect 45-day intervals because we are interested in determining whether there is an impact on job finding around the exhaustion of the maximum benefit duration after 45 days. Among SBU takers, we find that job finding within 52 days increased by 3.1 percentage points (pp), or 21%, and we find no further impacts. (The size of the coefficients for job finding within 232 and 277 days is similar to or even larger than our estimated 3.1 percentage point increase, but they are statistically not significant and are also much smaller relative to the average job finding probabilities for these periods, that are 53.4% and 58.8%.) These results suggest that the reform did not increase the overall job-finding rate within a year of job loss but rather changed the timing of job-finding, thus decreasing the average time until starting a new job.

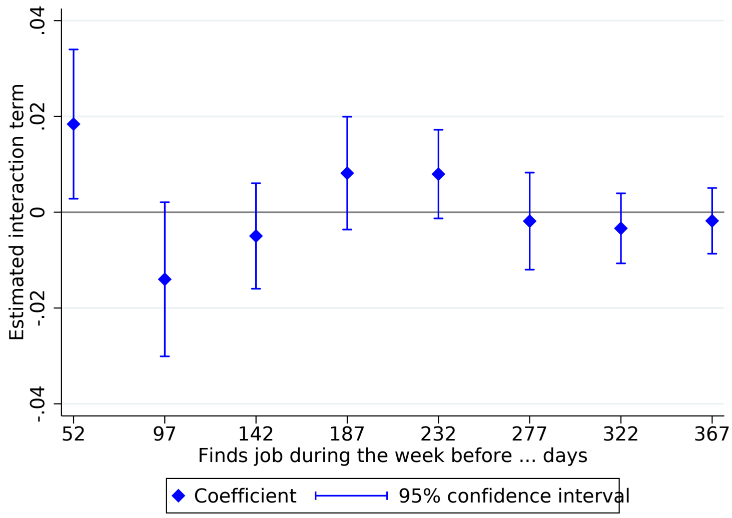
In panel (b) of Figure 2.6 we zoom in on job finding on specific weeks, similar to what we did in columns (4) and (5) of Table 2.5 for UIB take-up. These outcome variables reflect 45-day intervals because we are interested in finding out if there is an impact on job finding right after benefit exhaustion, specifically the week after benefits are exhausted. An increased probability of finding a job right after benefits are exhausted might be explained by different mechanisms. First, it could indicate the misuse of benefits, where individuals strategically time the start of a new job to maximize total benefit receipt. Second, it could be a sign of liquidity constraints for sick individuals. Nevertheless, we find that the probability of finding a job during the week after 45 days of unemployment increased by 1.8 percentage points (pp), almost doubling the proportion of the unemployed who found jobs during that week. This change is accompanied by a 1.5 percentage point (pp) decrease in the probability of finding a job during the week after 90 days of unemployment, i.e., right after the pre-reform maximum duration. These patterns suggest that some job-finding events that previously occurred after 90 days are now occurring after 45 days, indicating that a subset of SBU recipients were maximizing benefit take-up before the policy changes by starting a new job right after the maximum duration, and that a similar group of SBU recipients are following the same practice at the new maximum duration.

Figure 2.6: Main estimates on job finding of "sickness benefit for the unemployed"-takers

(a) Within x days



(b) During week before x days



Note: Each point shows the estimated β_3 coefficient of a regression of the form of Equation 2.1, where the dependent variable is finding a job within x days in panel (a) and finding a job during the week ending with day x in panel (b). Standard errors are clustered on the individual level. The detailed estimation results can be found in Appendix Table B.1.1. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

One potential explanation for these results is that employers and employees cooperate to exploit the system by temporarily laying off employees so they can receive the "sickness benefit for the unemployed," after which they return to their previous employer. This method of exploiting the social insurance system has been documented by Köllő (2001) in the context of unemployment insurance in the late 1990s. Köllő (2001) documents, using survey data, that around 20%-60% (depending on the year) of UIB recipients return to their previous employer. They typically spend three months receiving benefits in sectors with marked seasonality in the labor market, due to the nature of the jobs. Specifically, these sectors are agriculture and construction. In our sample of all job endings, 35% and 33% of job seekers who find a job within a year return to their original employer during the Nov–March period in the control and reform years, respectively. This share is 20% and 16% during April–August in the control and reform years, respectively. Returns are less common among SBU recipients (19%, 21%, 15%, and 10%, respectively).

When we analyze the impact of finding a job at a new employer versus returning to a previous employer, we introduce a third dimension and estimate a triple difference-in-differences (DDD) model. In this model, we include all job endings in our analysis to check if the difference-in-difference estimate on job finding rates differs significantly for SBU takers compared to other job losers. Applying the DDD model allows us to eliminate confounding time trends in return behavior that our simple DiD estimates reveal (see Tables B.1.2 and B.1.3 in the Appendix). We estimate the following model:

$$\begin{aligned} y_i = \alpha_i &+ \beta_1 AprilAugust_i + \beta_2 ReformYear_i + \\ &\beta_3 AprilAugust_i * ReformYear_i + \beta_4 SBUtaker_i + \\ &\beta_5 AprilAugust_i * SBUtaker_i + \beta_6 ReformYear_i * SBUtaker_i \\ &\beta_7 AprilAugust_i * ReformYear_i * SBUtaker_i \\ &+ X_i' \eta + \varepsilon_i \end{aligned} \tag{2.2}$$

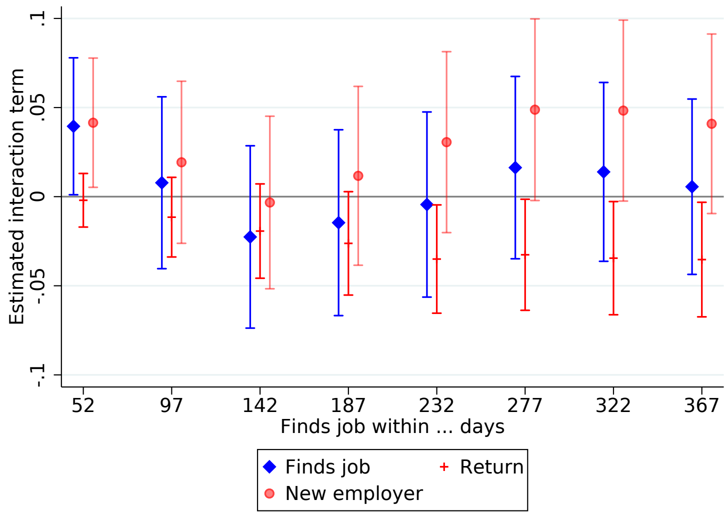
Again, we examine two sets of outcome variables: first, binary indicators of finding a job within d days, where $d = 52, 97, ..., 367$; second, binary indicators of finding a job on a specific week ($46 - 52$, $91 - 97$, ..., $361 - 367$ days after job ending). In this model β_3 shows the reform impact for those who do not take up SBU. The sum of β_3 and β_7 shows the impact for SBU-takers. Thus the statistical significance of the β_7 coefficient also shows whether the impact on the job-finding rate of SBU-takers significantly differs from that for non-takers. In other words, it shows whether there is an impact for SBU-takers on top of any general trends.

First, we replicate our main findings on job finding probabilities with the triple DiD strategy and obtain very similar results, which are presented in Appendix Table B.1.4. We find that the probability of finding a job within 52 days increased by 3.7 percentage points (pp), the probability of finding a job during days 46–52 increased by 1.6 pp, and the probability of finding a job during days 91–97 decreased by 1.6 pp. We decompose the DDD results into job finding at new employers (shown in Appendix Table B.1.5) and returning to previous employers (shown in Appendix Table B.1.6). As with our main DiD estimates, we plot the interaction coefficients showing the impact for SBU recipients in Figure 2.7: job finding in general (blue diamond shapes), job finding at a new employer (orange dots), and returning to the previous employer (red sticks). This decomposition shows that our results are fully due to job finding at new employers.

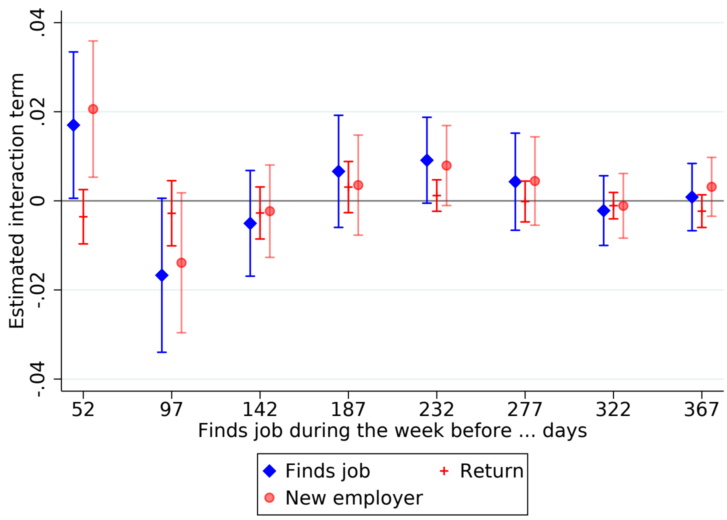
We provide robustness checks for our job-finding results in Table B.1.7 in the Appendix. We report different model versions for estimating the impact on job finding within 52 days in panel A, on job finding during the 46th-52nd days in panel B and on job finding during the 91st-97th days in panel C. Column (1) repeats our main findings with all the controls, in column (2) we leave out job endings from June 30, 2006, i.e. non-employment spells that start on July 1, 2006, due to the irregularly large number of job endings on that day that are due to a few big firms (see panel (a) of Figure 2.2). Columns (3), (4), (5) and (6) include different sets of control variables. Column (7) reports the results of estimating a logit model instead of a linear probability model. Finally, column (8) reports our findings when using a four-month time window around the reform date, instead of a five-month window. The size of our results regarding job finding within 52 days is similar, or even larger, in the different model versions, except for the 4-month window estimate, where the estimated impact is smaller and not statistically significant. Our estimates of job finding in the weeks following the new and old maximum durations are robust to different model specifications, in terms of both size and statistical significance.

Figure 2.7: Decomposition of main triple difference-in-differences estimates on job finding of SBU-takers to returns and job finding at a new employer

(a) Within x days



(b) During week before x days



Note: Each point shows the estimated β_7 coefficient of a regression of the form of Equation 2.2, where the dependent variable is finding a job (at a new / at the previous employer) within x days in panel (a) and finding a job (at a new / at the previous employer) during the week ending with day x in panel (b). Standard errors are clustered on the individual level. The detailed estimation results can be found in Appendix Tables B.1.4, B.1.5 and B.1.6. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

2.5.5 Heterogeneity analysis

In this section, we explore the characteristics of people who respond to the policy by finding a job earlier. We introduce a third dimension to our original diff-in-diff estimation in Table 2.7 to run heterogeneity estimates on job finding within 52 days and during days 46–52.

We demonstrate heterogeneity through the following characteristics: being on sick leave during the last seven days of employment, having an above-median health spending indicator, having more than two years of employment history, earning an above-median wage, and having an above-median job-finding probability, as estimated by a propensity score on a sample of job endings before the reform.

There is some indication that our main results regarding job finding within 52 days are driven by individuals who are not on sick leave at the end of their employment period (see column (1) in Panel A. of Table 2.7). Our estimate for those not on sick leave at the end of their employment period is 3.7 percentage points (pp), while the triple interaction coefficient – showing the differential for those on sick leave before the job ended – is similar in size but negative and insignificant. Focusing on job finding right after the maximum potential duration after the reform, we observe a 2.4 percentage point (pp) increase for individuals who were likely not or less sick, as measured by being on sick leave during the final days of employment. Meanwhile, the coefficient for the triple interaction coefficient is -3.8 pp. These results may be due to the underlying health conditions of individuals if being on sick leave at the end of an employment period indicates being sick. However, this measure may capture other mechanisms, such as shirking. When we proxy health status in column (2) of Table 2.7 with above-median health spending in the previous calendar year, we cannot find any heterogeneous impacts.

The positive impact of finding a job within 52 days is driven by individuals with longer employment histories and higher wages based on column (3) and column (4) in Table 2.7. Interestingly, individuals with a below-median predicted job-finding probability based on the pre-reform sample of job endings have a 2.3 percentage point (pp) higher job-finding rate the week after the post-reform maximum duration, as shown in column (5) of Panel B in Table 2.7.

Table 2.7: Impact on job finding probabilities within 52 days and on days 46-52, triple difference-in-differences regression estimates including interaction terms for subgroups

	A. Job finding within 52 days for subgroups				
	(1)	(2)	(3)	(4)	(5)
Subgroups:	Sick 7	> *med* health spending	> 2*yr* emp. history	> *med* wage	> *med* job finding prob.
April-August	0.006	0.008	0.030	0.019	0.012
	(0.014)	(0.018)	(0.019)	(0.018)	(0.015)
Reform year	-0.007	-0.010	-0.014	-0.007	-0.004
	(0.013)	(0.016)	(0.017)	(0.017)	(0.014)
April-August * Reform year	0.037*	0.029	-0.007	-0.015	0.024
	(0.020)	(0.025)	(0.027)	(0.025)	(0.022)
Apr-Aug * I	-0.007	-0.006	-0.041*	-0.029	-0.014
	(0.030)	(0.025)	(0.025)	(0.025)	(0.025)
Reform yr * I	-0.021	0.001	0.007	-0.007	-0.012
	(0.028)	(0.023)	(0.023)	(0.024)	(0.024)
Apr-Aug * Reform yr * I	-0.041	0.003	0.060*	0.089**	0.014
	(0.042)	(0.036)	(0.036)	(0.035)	(0.035)
Observations	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.150	0.150	0.150	0.150	0.150
No. of obs. in group	1344	3962	4547	3941	3929
	B. Job finding during days 46-52 for subgroups				
	(1)	(2)	(3)	(4)	(5)
Subgroups:	Sick 7	> *med* health spending	> 2*yr* emp. history	> *med* wage	> *med* job finding prob.
April-August	-0.001	0.003	0.013	-0.001	-0.009
	(0.006)	(0.008)	(0.008)	(0.007)	(0.006)
Reform year	-0.002	-0.004	0.001	-0.006	0.002
	(0.005)	(0.007)	(0.007)	(0.006)	(0.007)
April-August * Reform year	0.024***	0.017	-0.003	0.010	0.023**
	(0.009)	(0.011)	(0.012)	(0.010)	(0.010)
Apr-Aug * I	0.002	-0.007	-0.024**	0.001	0.017*
	(0.013)	(0.010)	(0.011)	(0.010)	(0.010)
Reform yr * I	0.011	0.006	-0.003	0.010	-0.005
	(0.014)	(0.010)	(0.010)	(0.010)	(0.010)
Apr-Aug * Reform yr * I	-0.038**	0.001	0.034**	0.017	-0.011
	(0.019)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.0205	0.0205	0.0205	0.0205	0.0205
No. of obs. in group	1344	3962	4547	3941	3929

Note: Triple difference-in-differences coefficients of regressions of the form of Equation 2.2 are reported. The dependent variable is finding a job within 52 days in panel A. and finding a job during days 46-52 in panel B. The third dimension is a different subgroup of individuals in each column. In column (1) they are people who were already on sick leave during the last week of their employment spell, in column (2) people with above-median health spending, in column (3) people with longer than 2 years of employment history, in column (4) people with above-median wage, and in column (5) people with above-median job-finding probability. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

2.6 Conclusion

In this paper, we have investigated the decision of taking up "sickness benefits for the unemployed" in a setting which provides strong incentives for take-up. Workers could claim sickness benefits ("sickness benefit for the unemployed") within three days of job loss and could extend their total (sickness and unemployment) insurance benefit duration substantially while some of them with higher income and longer tenure could also earn higher benefit amounts.

In a series of descriptive statistics we illustrate some signs of suspicious use, e.g. "passive" sickness spells (used while someone is unemployed) are substantially longer than "active" spells (used while someone is employed), the inflow to sickness benefit jumps up right after job loss and people with higher financial incentives take this benefit up more likely.

We have also shown that the substantial decrease in the maximum duration in "sickness benefit for the unemployed" (from 90 to 45 days) lead to a quicker transition to new jobs and a 3.1 pp, or 21%, increase in job finding within 52 days. This overall impact happens partly due to a specific reallocation of job finding on specific weeks, that is the combination of decreased job finding (by 1.5 pp) exactly on the week following the pre-reform maximum duration and an increased job finding (by 1.8 pp) on the week after the new maximum duration. We have showed that this impact is fully due to jobs at new employers and not returns to the previous employer. Results are driven by individuals who are less likely sick measured by being on sick leave already before the job ending, and by individuals with longer employment histories and higher earnings.

In some other European countries, registered job seekers are typically eligible for sickness insurance. In Scandinavian countries, it has been shown that there is a spike in sick leave right when registered unemployment status expires. This is seen as a sign of moral hazard (Henningsen, 2008; Larsson, 2006). By comparison, we have shown that the Hungarian policy seems even more prone to misuse, as the newly unemployed have only three days to apply for sickness benefits before applying for unemployment insurance. This policy may incentivize individuals who could find jobs during the unemployment benefit period to report sick and receive higher benefit amounts for a longer period. Another policy implication of this example is that policymakers should consider providing similar amounts of sickness and unemployment insurance benefits to avoid strategic substitution between different welfare programs.

3 Chapter 3: Firm Heterogeneity and the Impact of Payroll Taxes

Joint work with Anikó Bíró, Réka Branyiczki, Attila Lindner and Dániel Prinz

3.1 Introduction

Payroll taxes and employer social security contributions account for just under 40% of the tax wedge in developed countries (OECD, 2022c) and there is a longstanding interest in understanding the impact of these policies on employment and wages. The standard approach in public finance suggests that the market-level elasticities of labor supply and demand determine the employment and wage impacts and the incidence of payroll taxes (see e.g. Gruber, 1997; Rothstein, 2010). This approach typically assumes that firms passively accept market-level wages and so the incidence of the payroll tax will be homogeneous across firms and workers. However, a growing number of empirical studies highlight that firms play an active role in wage determination and significant wage premium differences are present across employers see for review (see for review Card et al., 2018).

In the presence of job heterogeneity and variation in rents across firms, the evaluation of tax policies should take into account their effect on the composition of jobs (Katz & Summers, 1989; Rodrik & Sabel, 2022; Rodrik & Stantcheva, 2021). The standard theory does not consider whether the incidence of a policy varies across different firm types or whether tax policies affect the composition of jobs, and contribute to the creation of "low wage, bad jobs" or "high wage, good jobs" (Katz & Summers, 1989).[35]

The lack of good jobs and the potential role of tax policies have been featured prominently in recent policy discussions (see e.g. Blanchflower, 2021). While unemployment rates are at historically low levels, wage inequality is growing and firms are a key driver of those trends (Card et al., 2013). Workers are often concerned more about the types of jobs they can find than unemployment. Recent evidence has suggested that having a poor-quality job can be worse than simply being unemployed (Chandola & Zhang, 2018). Creating more good jobs has also become a central goal for many governments (see e.g.,

[35]The definition of "good" jobs is necessarily slippery (Rodrik & Sabel, 2022). Here we use various proxies for good jobs including firm-level productivity, firm-level wage premium (Abowd et al., 1999), and measures based on revealed preferences (Bagger & Lentz, 2019).

"The Good Jobs Initiative" of the Biden-Harris administration[36]). Thus, understanding whether tax policies trickle down to workers and whether the effects on workers vary by firm types has first-order importance for policy making.

Still, the large body of evidence on the impact of tax policies on employment and wages ignores what types of jobs are created and whether the incidence of policies varies by firm type. In this paper, we fill this important gap in the literature by assessing the impact of a large reduction in payroll taxes on the composition of jobs in the economy and on workers' wages. To illustrate the important role firms could play in shaping the impact of tax policies, we discuss how the effect of tax cuts depends on the structure of the labor market. We highlight that deviating from standard perfectly competitive labor markets leads to rich predictions about the impact of tax cuts and it is a priori unclear whether a policy contributes to the creation of good jobs, or it creates bad jobs at the expense of good ones. In addition, it is also an empirical question whether workers share the benefits of the wage increase resulting from a tax cut equally or the pass-through of the tax cut to workers is substantial only at certain firms. In the presence of imperfect competition in the labor market, the incidence of the policy could be heterogeneous across firm types, while in many other models (e.g., under perfect competition) the incidence of the policy will be the same across firm types.

Motivated by these predictions and questions, we study the heterogeneous impact of an age-specific payroll tax cut in Hungary. In 2013 the monthly social security contribution decreased by HUF 14,500 ($66) for all over-55 private sector employees.[37] This led to a 5.3% decrease in the labor cost for an average over-55 private sector employee. Using rich administrative data, we estimate the impact of the policy in a difference-in-differences framework, comparing men above the age cutoff to men below it.[38] We find a large increase in employment in response to the policy. In response to the 5.3% decrease in labor costs, employment of the treated workers increased by 1.6%, implying a labor demand elasticity of -0.30 (s.e. 0.03).[39] We also calculate that the net present value of labor cost de-

[36]https://www.dol.gov/general/good-jobs

[37]The average monthly net wage (wage net of employer payroll tax) was HUF 230,700 ($1,045) in Hungary in 2013 (Hungarian Central Statistical Office, 2022), so the tax cut is about 6.3% of the average wage in 2013. A tax cut of equivalent size in the U.S. context would be $3500 per year based on the average salary in 2022 (US Bureau of Labor Statistics, 2022).

[38]We focus on men in the main analysis as for women there was a slight change in retirement rules that could have affected treated and untreated workers differently. We present the estimates for women in the Appendix C.3 and find similar firm heterogeneity as for men.

[39]We estimate that around one-third of the employment increase comes from elevated hiring from non-employment and two-thirds come from lower exit rates.

creased by 7.5% for workers at the treated ages, which implies an employment elasticity of -0.21 (s.e. 0.02). At the same time, the change in self-employment and public sector employment was limited, consistent with the ineligibility of these workers for the payroll tax cut.

In line with the prediction of models with labor market imperfections, we also find substantial heterogeneity across firm types. For a variety of measures of firm quality, the employment-increasing effect of the policy comes from low-quality firms and low-quality jobs, while the employment of older workers in high-quality firms is unchanged. The differential response to the policy by firm type cannot be explained by the lower relative value of the tax cut at high-quality firms. Even if the relative decline in labor cost is somewhat larger at low-quality firms, it is still non-negligible at high-quality firms (6% at low-quality firms vs. 4.5% at high-quality firms). The implied employment elasticity with respect to labor cost is statistically different between low-quality firms (-0.53, s.e. 0.05) and high-quality firms (0.01, s.e. 0.06).

We present several additional pieces of evidence to highlight that our results reflect firm heterogeneity and not other factors. First, we examine the effect of the policy throughout the entire wage distribution similarly to Cengiz et al. (2019). We find that employment increased mainly at the bottom of the wage distribution at low-quality firms, while we find no indication for substantial change in employment in the upper part of the wage distribution where the relative change in labor cost was limited.[40] This suggests that our estimates pick up the effect of the payroll tax cut. Furthermore, we show that heterogeneity in responses is present even if we restrict the sample to similar workers. Even among low-paid workers in low-paying occupations and among less-educated workers, we find different responses to the policy by firm type. This suggests that the differential responses to the payroll tax cut reflect firm heterogeneity and not simply the fact that better workers tend to work at better firms.

We also study the impact of the policy on wages. We estimate that the overall pass-through of the policy is small: out of $1 only 22 cents (s.e. 9 cents) benefit workers, while 78 cents (s.e. 9 cents) go to firms. We also find heterogeneous incidence by firm productivity: there is a significant increase in wages at high-productivity firms, but we find no change in wages at low-productivity firms. At high-quality firms the pass-through rate is 60 cents (s.e. 13 cents) on the dollar, while at low-quality firms the pass-through rate is close to zero and statistically insignificant. We also show that the pass-through

[40]Note that the tax cut was lump sum, which implies that at higher wages the change was smaller relative to total labor costs.

rate difference across firms is present for workers with low and high levels of education, though it is more prominent for the latter group.

We present several robustness checks to underscore these results. First, we vary the control group definition to make sure that our main estimates are not muted or exaggerated by the variation of the age-window used in the estimation and by potential spillovers to the control group (i.e., SUTVA violation). The main conclusions are unaffected by the choice of the control group.

Second, the comparison of the firm-level relationship between hiring treated workers and untreated workers before and after the reform suggests that firms that hired more treated workers after the reform did not cut their hiring of untreated workers. Accordingly, the policy is likely to have improved overall employment and not just led to substitution of treated workers for untreated ones.

Third, we also study how firms' responses depend on the windfall effects found to be important in the context of tax cuts affecting young Swedish workers (Saez et al., 2019). In particular, we show that the change in wages and the incidence differences across firm types do not depend on the size of windfall shocks firms experience and so our findings are robust to controlling for windfall shocks.

Fourth, our results are unlikely to reflect wage rigidities that could potentially bind low-quality and high-quality firms differently. Union membership is very low in Hungary and industry-level agreements are rare and set only weak requirements. Furthermore, we find that the heterogeneity between high- and low-quality firms is present even if we look at employment changes among similarly sized firms. Our estimates do not simply reflect the presence of a binding minimum wage either. The estimated change in employment is not concentrated at the minimum wage. Even among workers earning more than 150% of the minimum wage we find a significant increase in employment at low-productivity firms. This suggests that the employment change does not only come from some low-quality jobs becoming viable following the payroll tax cut.

Fifth, even if we exploit only within-industry variation in productivity we find similar responses to the policy. This highlights that our approach does not simply pick up cross-industry heterogeneity in the impact of the policy, and the incidence is heterogeneous across firms within the same industry.

These empirical findings together with our theoretical considerations point to interesting (and as far as we know so far undocumented) heterogeneity in the incidence of tax cuts. Workers employed by productive firms are able to extract more of the surplus from the tax cut and so the incidence of the tax cut (partly) falls on them. At the same time, older workers who are employed by less productive firms are benefiting from the tax cut through increased hiring, while firms capture a larger share of the surplus for these workers.

Finally, we discuss how the documented heterogeneous incidence of the policy alter the welfare assessment of payroll taxes by applying the Marginal Value of Public Funds (MVPF) framework (Hendren & Sprung-Keyser, 2020). We consider two scenarios: 1) when policy makers care only about the workers; 2) when policy makers care about the total welfare including firms' profit. Since a large share of the tax cut ends up at employers, particularly at low-quality firms, the policy has a relatively low MVPF if the policy maker only cares about worker welfare. The MVPF is significantly higher at high-quality firms with high pass-through rates than at low-quality firms with pass-through rates close to zero. If we also include the part of the tax cut that goes to employers, then the MVPF is higher. Importantly, in this case targeting low-quality firms with the tax cut has a higher MVPF than targeting high-quality firms because the employment creation effect dominates the wage effect. Our welfare analysis, therefore, highlights that it is important to take into account how payroll taxes affect the prevalence of good and bad jobs in the economy.

Since parallel to the tax cut for older workers, a tax cut affecting workers under 25 was also introduced, we can compare our estimated responses for older workers to impacts among younger workers. We find that the payroll tax cut increased employment of younger workers with little impact on wages. We also find heterogeneity patterns similar to those documented for older workers though contrary to older workers, we also find some job creation at higher quality firms. Furthermore, we find no indication for differential wage responses of treated and untreated cohorts for the young. The lack of wage responses could be explained by wage rigidities, which are more prevalent in the labor market of young workers.[41] This result is also consistent with a limited scope for wage negotiations at labor market entry (see Caldwell & Oehlsen, 2018).

Our paper relates to several strands of the literature. First, the paper is closely related to studies of age-based employment subsidies (Boockmann et al., 2012; Egebark & Kaunitz, 2018; Huttunen et al., 2013; Kramarz & Philippon, 2001; Saez et al., 2019; Svraka, 2019). Studying the labor market

[41] Minimum wages are more binding for younger workers. In addition, wage setting constraints might be more important when workers age out from the subsidy. If the tax cut were fully passed through at younger ages, once workers age out of the subsidy they could experience a wage cut. Notice that for older workers aging into the subsidy, increasing wages above the age threshold is easier.

consequences of such policies is particularly interesting given that they target vulnerable groups with relatively low employment rates. Improving the employment and wage prospects of these workers is a policy priority for many governments. Nevertheless, to date there is no conclusive evidence on whether such policies are successful. Some studies find non-negligible positive effects on employment (Egebark & Kaunitz, 2018; Kramarz & Philippon, 2001; Saez et al., 2019), while others find little evidence for employment effects (Boockmann et al., 2012; Huttunen et al., 2013). Our main contribution to this literature is that we focus on heterogeneity across firm types offering a potential explanation for the inconsistencies found in the literature.

Second, our study contributes to the literature on payroll tax incidence in general. Studies using payroll tax reforms to analyze incidence provide mixed evidence. Some studies find that the burden of the payroll tax is shifted on the workers (Anderson & Meyer, 2000; Gruber, 1997). However, some later studies find that the burden of the payroll tax is mostly borne by the employer (Benzarti & Harju, 2021; Ku et al., 2020; Kugler & Kugler, 2009; Saez et al., 2012, 2019).[42] Our results highlight that the incidence of payroll taxes depends on the types of firms and workers studied. Carbonnier et al. (2022) evaluate the incidence of business tax credits and Fuest et al. (2018) and Kennedy et al. (2024a, 2024b) the incidence of corporate income taxes and document some heterogeneity in incidence by worker type, but firm heterogeneity and the effects of the policy on the composition of jobs is mainly ignored in the literature so far. A notable exception is Stokke (2021), who document firm-level heterogeneities in the payroll tax cut, in line with our results, although they cannot study employment in their setting as the parallel trend assumption does not hold before the reform. Relatedly, Cottet (2024) shows that after a payroll tax cut for minimum-wage workers, liquidity-constrained and credit-constrained firms grow more. Giroud and Rauh (2019) and Cloyne et al. (2024) show industry-specific heterogeneities in the employment responses to corporate tax changes. In Appendix Table C.1.1 we provide further details of the studies that perform heterogeneity analyses of the effects of payroll tax or business tax policies. Previous literature has documented some heterogeneity in the response to the tax cuts by firm size and/or sector. We show that our heterogeneity results by firms' quality are distinct from those as they are present even within sectors (see Table C.1.12) and within size categories (see Table C.1.13).

[42] Bozio et al. (2019) reconcile these seemingly conflicting results by the tax-benefit linkage explanation. In our case, tax-benefit linkages are not directly affected by the reform as the payroll tax did not affect workers' future benefits, which were calculated based on wages and not based on social security contributions, a common feature of payroll tax cut policies.

Furthermore, our heterogeneity analysis by firm type is motivated by core wage setting models of the labor market. A long strand of studies documents the impact of various firm- and market-level shocks on wages and employment (see Card et al., 2018; Garin & Silvério, 2024; Jäger et al., 2020; Kline et al., 2019; Lamadon et al., 2022; Nallareddy et al., 2022). Nevertheless, studying worker and firm heterogeneity in pass-through rates has not been fully appreciated in this recent literature.

The remainder of this paper proceeds as follows. Section 3.2 studies the effect of payroll taxes in different models of the labor market with heterogeneous firms. In Section 3.3, we provide background on the payroll tax reform we study and describe the Hungarian administrative data used for our empirical analysis. We present our employment results in Section 3.4 and wage results in Section 3.5. We discuss welfare effects in Section 3.6. In Section 3.7 we provide results for younger workers. Section 3.8 concludes.

3.2 Tax cuts in different models of the labor market

We study the impact of payroll taxes under various assumptions about the structure of labor markets. We highlight that tax cuts do not only affect unemployment but could also change the composition of jobs in the economy. In some cases, tax cuts could create good jobs partly at the expense of bad jobs, which could be an unintended consequence of the policy. Furthermore, the incidence of tax policies can be heterogeneous across firm types if we deviate from the standard assumption of perfectly competitive labor markets. Our results are summarized in Table 3.1, where we discuss the predictions of various models for employment and wages, and whether those differ by firm productivity. In each case, we rely on the standard model commonly applied in the literature. We study the impact of a lump-sum tax cut as this is what was introduced in Hungary. Nevertheless, we abstract away from the age-specific nature of the tax cut and worker heterogeneity. These assumptions allow us to illustrate the impact of the policy in a more tractable environment, but our results do not hinge on those assumptions. We discuss the main intuitions underlying the various models here and provide further details including formal derivations in Appendix C.5. In Appendix C.5, we also implement a quantitative exercise and show that the size of the observed employment and wage responses are in line with the predictions of the standard sequential bargaining search models.

Table 3.1: Overview of the effect of tax cuts in different models of the labor market

Model	**Effect of tax cut on allocation** Low TFP		High TFP	**Incidence of the tax cut** Low TFP		High TFP
Sequential bargaining search (Cahuc et al., 2006)	Positive	$>$	Positive	Positive	$<$	Positive
Wage posting search (Burdett & Mortensen, 1998)	Zero	$=$	Zero	Positive	$<$	Positive
Monopsonistic competition, constant elasticity of firm-level labor supply	Positive	$>$	Negative	Positive	$=$	Positive
Monopsonistic competition (Card et al., 2018)	Positive	$>$	Negative	Positive	$<$	Positive
Perfectly competitive labor market (Melitz, 2003)	Positive	$>$	Negative	Positive	$=$	Positive

Note: Table summarizes the impact of payroll tax cuts under various assumptions about the structure of labor markets. In each case, we study the impact of a lump-sum tax cut in the presence of firms with heterogeneous productivity. We provide the intuition behind the results in Section 3.2, and we provide detailed derivation in Appendix C.5.

3.2.1 Search and matching with sequential bargaining.

We start our discussion by applying a search and matching model with on-the-job search and sequential bargaining à la Postel-Vinay and Robin (2002). In that model, firms need to put costly effort into meeting workers by posting vacancies. Once a firm and worker are matched there is a negotiation between them over wages that takes into account the worker's outside option. Individuals coming from unemployment use their unemployment benefits and the value of not working as an outside option. Individuals with jobs can use their current job in the negotiation. As a result, workers participate in a sequential bargaining process over their job ladder, which allows them to extract more and more rent in the negotiations.

When the bargaining power of workers is low, the model predicts that low-quality, low-productivity firms can hire mainly from unemployment, but earn large rents on those workers as their outside options are weak. At the same time, high-quality, high-productivity firms can employ more workers as they do not only hire from unemployment, but can also poach workers from low-productivity firms. Poached workers can get a larger share of the surplus or rent as they can use their previous job as an outside option in wage negotiations.

That structure of the labor market implies that the impact of the payroll tax cut is heterogeneous across firms. This heterogeneity comes from the fact that the payroll tax cut mainly benefits firms hiring from unemployment. On the other hand, whenever firms poach workers from other firms, competition drives up wages and so the tax cut will be passed through to workers, leaving no benefit at the firm. As a result, the tax policy will disproportionately encourage low-productivity firms to put more effort in hiring as those firms tend to hire from unemployment. At the same time, workers at high-productivity firms benefit from poaching and outside offers and so their wages will increase. Therefore, there is a natural heterogeneity in the incidence of tax policy across firm types in this framework. In addition to that, there is no guarantee that the equilibrium (without tax intervention) is optimal, so tax policy interventions can increase efficiency.

3.2.2 Search and matching with wage posting.

In a different type of search and matching environment, firms post take-it or leave-it wage offers. Once workers meet firms, they can decide whether to accept the wage offered by the firm or search further instead. In this framework, there are no individual-level negotiations over wages, and firms need to commit to higher wages to be able to poach workers from other firms. We derive the effect of the tax cut in the standard Burdett and Mortensen (1998) model. Firms meet searching workers randomly, and they cannot influence the probability of being met (e.g., by posting more vacancies). They only have one instrument to attract more workers: posting higher wages, which increases the probability that the randomly chosen worker accepts the offer once the firm and the worker have met. In equilibrium, more productive firms post higher wages and they can poach more workers from other firms. Nevertheless, the allocation of employment will be solely based on the ranking of firms. The tax cut does not affect that ranking and so employment will be unaffected in equilibrium.

At the same time, wage responses will depend on firm productivity. Lower productivity firms will post the same wage as before as they set wages close to the unemployment benefit. More productive firms, on the other hand, compete with each other for workers and will pass through part of the tax cut to workers. Therefore, similarly to the search and matching framework with sequential bargaining, we expect some heterogeneity in the incidence of the policy.

3.2.3 Monopsonistic competition.

We also derive the impact of the tax cut in the presence of monopsonistic competition in the labor market. We follow Card et al. (2018) and study the impact of the policy in the presence of monopsonistic competition. In that framework, firms face an upward-sloping labor supply function, which implies that they have to pay more to attract more workers. In response to the tax cut, the marginal benefit of hiring workers increases, which leads to firms' expansion. In the model, firms can only expand if they set higher wages. The relative size of the wage and employment responses depends on the elasticity of labor supply and how it varies across different firm types.

When the elasticity of labor supply is the same for all firms, there is full pass-through of the tax cut for all firms. As a result, wages will increase and we expect no heterogeneity in the incidence of the policy. Furthermore, the lump-sum nature of the tax cut also implies that wages will increase more in relative terms at low-productivity firms, which will induce a stronger employment response at those firms. In the model, aggregate labor supply is assumed to be inelastic, and so the stronger employment response at low-productivity firms will come at the expense of high-productivity firms.[43]

Card et al. (2018), on the other hand, apply a different parametrization of the firm-level labor supply as full-pass through of income shocks would not be consistent with the existing evidence on rent sharing. Under their parametrization, the elasticity of firm-level labor supply decreases with wages.[44] Low-productivity firms face more elastic labor supply, and so a small wage change allows them to expand more. At the same time, high-productivity firms face less elastic labor supply and need to increase wages more, so their expansion is more costly. Low-productivity firms as a result will only implement a small wage increase and expand. Assuming constant aggregate labor supply, this will come at the expense of high-productivity firms. High-productivity firms, on the other hand, increase wages more, but the less elastic labor supply implies that employment will still reallocate from them towards low-productivity firms. Such reallocation reflects that the lump-sum shock is larger (in relative terms) for low-paying than for high-paying firms. The differences in the elasticity of firm-level labor supply, therefore, lead to the heterogeneous incidence of the policy. At high-productivity firms, employment will decrease, but those firms will pass through a larger share of the payroll tax cut.

3.2.4 Perfectly competitive labor market.

Finally, we discuss the effect of the policy in the presence of perfect competition in the labor market. We apply a model with heterogeneous firms with some entry costs à la Melitz (2003). In this model,

[43]With an elastic aggregate labor supply curve, we can get a positive employment impact throughout the firm productivity distribution

[44]Berger et al. (2022) derives such an elasticity structure from labor market power and strategic interactions: larger firms face more elastic labor supply as they need to attract workers from other labor markets as well.

firms pay the same wage to workers but only the most productive firms enter the market. When the tax cut is introduced, some firms that were not viable before enter the labor market. This increases labor demand. With an inelastic labor supply, the main margin of adjustment is wages, while aggregate employment is unaffected. In that framework, there is reallocation from high-productivity firms to low-productivity firms entering the market. Furthermore, in the presence of perfect competition, there is no heterogeneity in the wage response across firms with different productivity. When labor supply is allowed to be elastic, the margin of adjustment can shift to employment and the pressure on wages will be more limited. Still the incidence of the policy will not be heterogeneous across firms.

3.2.5 Summary.

Imperfect competition in the labor market leads to heterogeneous pass-through of tax cuts to wages across firm types in most cases. The models usually predict that higher productivity firms will pass through a larger share of the tax cut. This is in stark contrast to models with perfect competition. At the same time, we find no clear pattern on whether the payroll tax cut changes the composition of jobs or not, which highlights the need for empirical assessment. The models also emphasize different margins of adjustment (e.g. firm entry under perfect competition), which we will also explore. Motivated by these examples, we study the heterogeneous impact of a payroll tax cut on employment and wages in the next sections.

3.3 Institutional setting and data

3.3.1 Institutional setting

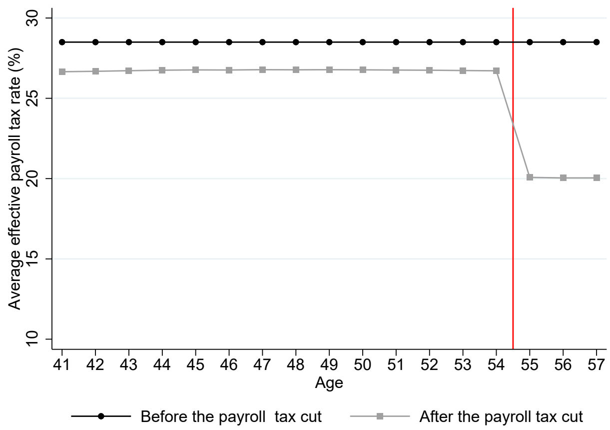
We study the impact of a large age-specific payroll tax cut instituted in Hungary in 2013. Before 2013, employers paid 28.5% of wages in social security contributions. In 2013, the government decreased the social security contributions of employers by around 14,500 Hungarian Forints (HUF, $66) per month for every employee older than 55. The average monthly salary net of employer payroll tax but before income tax and employee social security contributions was HUF 230,700 ($1,045) (Hungarian Central Statistical Office, 2022) so the payroll tax cut was 6.3% of the average salary.[45] The cut applied

[45]The exact rules were the following. The social security contribution paid by employers was decreased from 28.5% to 14%, but the total amount of the tax cut was capped at HUF 14,500. As the minimum wage in 2013 was HUF 98,000 ($444), almost everybody hit the cap. For the few workers who earned exactly the minimum wage at HUF 98,000 in 2013, the tax cut was HUF 14,250. In 2014, the minimum wage was raised to HUF 101,500 ($460).

to both new and ongoing private sector jobs. Workers in the public sector and the self-employed were not eligible for the cut.

Besides workers aged over 55, workers under the age of 25 were also eligible for the tax cut. We discuss the impact of the policy on them in Section 3.7. Furthermore, workers in elementary occupations[46] received the tax cut independently of their age.[47] In our primary analysis we include workers in elementary occupations, but our results are robust to the exclusion of those workers from the definition of private sector employment (see Appendix Table C.1.6).

Figure 3.1: Employers' social security contribution rate by workers' age



Note: Figure shows the average employer social security contribution rate by worker age for male workers in the private sector. After the reform all individuals over age 55 experienced a lump-sum tax cut. Certain individuals were also eligible for the tax cut independently of their age.

Figure 3.1 depicts the average effective payroll tax rate paid by employers by employee age before and after the payroll tax cut was implemented. It shows the discontinuity at age 55 after the policy took effect (in gray) compared to the constant rate of 28.5% before (in black). After the policy took effect the average tax rate is lower than 28.5% (rate without cut) at all ages due to the fact that workers in elementary occupations could get the tax cut independently of age. Furthermore, there is a drop from 26.3% to 20% or by about 6.3 percentage points from age 54 to 55. It is worth highlighting that such a drop in the tax rate does not create a discontinuity in hiring incentives at age 55. From the firm's perspective, hiring someone one day short of age 55 is almost the same as hiring someone at exactly

[46] Elementary occupations correspond to level 9 of International Classification of Occupations ISCO-08. According to the definition of the International Labour Organization (https://www.ilo.org/public/english/bureau/stat/isco/isco88/9.htm), elementary occupations consist of simple and routine tasks which mainly require the use of hand-held tools and often some physical effort. Some examples are washing, cleaning, delivering goods, simple farming and manufacturing tasks, hand packing.

[47] Long-term unemployed re-entering the labor market, people returning to work after child-care leave, or younger workers entering the labor market received the tax benefit for 2 years independently of their age. The prevalence of these other beneficiary groups is close to zero for those aged 52-57.

age 55 as the difference is simply the one day for which higher taxes need to be paid, while once age 55 is reached, the same amount of tax cut is received. That is why we apply a difference-in-differences empirical strategy described in detail in Section 3.4, instead of a regression discontinuity strategy. In addition, we also calculate the change in incentives that take into account dynamic considerations, i.e., the fact that the control group will age into the treatment group at some point (see the elasticity results based on net present value).

The reform only affected the social security contributions paid by employers, while the part paid by the employees was unaffected. Employees before and after the reform paid a 16% flat-rate tax and employee social security contributions of 18.5%. Furthermore, the reform did not affect the link between social security contributions and future benefits (such as pensions) as those are calculated based on net wages and not based on contributions to the social security funds.

The tax cut was first publicly discussed in the Parliament on July 2, 2012, shortly after it was announced. The legislation was passed on October 15, 2012, and the tax change was effective from January 1, 2013. Due to the relatively short period of time between the announcement and enactment of the reform, anticipatory effects appearing before the implementation of the tax cut are likely to be negligible and we find no evidence of such effects in our empirical analysis.

In the main analysis, we study the impact of the reform among older men between 2010 and 2015. Throughout this period there were no other major labor market policy changes that affected older men. For women only there were some minor changes in early retirement rules and early retirement rate was non-negligible at age 55-57. Therefore, we focus on men to make sure that our results are not driven by changes in the pension system but when we apply our difference-in-differences estimation we find very similar results for women (shown in Appendix Section C.3).

Around this period the overall employment rate in Hungary was 64%, slightly below the OECD average (66%). The employment rate of older people (age 55-64) was only 46%, substantially below the OECD average (58%). The unemployment rate decreased steadily between 2012 and 2015, which reflected a substitution of welfare programs with a public work scheme (Cseres-Gergely & Molnár, 2015). At the same time, employment in the private sector was relatively stable: the prime-age population share employed in the private sector increased slightly from 38% to 39% between 2012 and 2015. To make sure our results are not driven by the improvement of labor market conditions, we show robustness to restricting the sample to local labor markets with stable prime-age employment.

Since our primary focus is on the heterogeneous impact of the policy, it is worth discussing whether different types of firms face different labor market institutions. In Hungary, it is relatively easy to hire or dismiss workers (Tonin, 2009). Wage bargaining takes place mostly at the individual level. The rare collective wage bargaining is based on firm-level agreements and the coverage of these policies is low. The unionization rate was around 10% in this period, one of the lowest in the OECD (OECD, 2022a). The weak labor market institutions and the lack of any size-specific regulations imply that firms with different size or productivity face similar institutional constraints in setting wages and employment.

3.3.2 Data

We use linked employer-employee administrative data from Hungary covering years 2010–2015 on a random 50% sample of the 2003 population. Since our sample is drawn from the whole population (and not just those who have a job) our data can be used to study changes in employment in response to the policy. An individual is defined to be a private sector employee if the individual is employed on the 15th of a month at a private sector firm with double-entry bookkeeping.[48] We include part-time workers and calculate full-time equivalent employment (e.g., working 20 hours per week is considered as 0.5 employment).[49] Our data include both fixed-term and permanent contracts, but we do not directly observe the contract type in the administrative data. According to the Hungarian LFS, fixed-term contracts in this age group are rare (less than 10% of all employment contracts are fixed-term). Our main outcome in the wage regression is the (full-time equivalent) net wage as of May of each year. We define net wage (sometimes abbreviated to wage) as wage earnings net of employer payroll tax. This net wage measure is calculated before income tax and employee social security contributions are deducted and includes base payment, bonuses and overtime pay.

Appendix Table C.1.3 provides a comparison of employment statistics based on the administrative data we use with official statistics which are based on the Hungarian Labor Force Survey. These

[48] We focus on firms with double-entry bookkeeping as most quality measures (e.g. TFP) are only available for them. In 2012, 5.7% of men aged 52-57 worked at single-entry bookkeeping firms, while 36.2% worked at double-entry bookkeeping firms. In addition to that we exclude from the benchmark analysis seven firms which have more than 10,000 workers—very large and unique firms in the Hungarian context—to avoid outliers driving the results. In 2012, 3.2% of men aged 52-57 worked at firms with more than 10,000 workers. Appendix Table C.1.4 shows that our results are robust to the inclusion of the largest firms and single-entry bookkeeping firms—the estimated employment effects and heterogeneity results by firm quality are stronger under the extended definition.

[49] The share of part-time jobs was very low in this period. Among men, around 90% of all private sector jobs were full-time.

statistics are very similar, indicating the reliability of the employment indicators we define based on the administrative data.

We generate firm-specific indicators that we use in the heterogeneity analyses. Our baseline indicator of firm quality is the value added-based total factor productivity (TFP).[50] As another indicator of firm quality, we perform an Abowd, Kramarz, Margolis (AKM) style decomposition of wages (Abowd et al., 1999) and calculate firm wage premia.[51] We calculate the poaching index, the share of new hires coming from employment instead of unemployment following Bagger and Lentz (2019).[52] We classify firms as foreign-owned if foreign ownership is above 50%. In the Hungarian context foreign ownership is a strong predictor of firm productivity, export orientation, and quality (Kaminski, 1999).

In our main empirical analysis, we restrict the sample to men and use workers aged between 52-57 (with workers aged 52-54 serving as the control group and workers aged 55-57 comprising the treatment group). We do not study the employment change of workers older than age 58 as early retirement starts to play a role then.[53] We restrict our sample to the non-retired population to ensure that the estimated employment effects are not driven by the aging-out of already retired individuals from our sample. Appendix Table C.1.7 shows that the estimated employment change and the heterogeneity patterns remain similar if we include retired individuals in the sample. For the workers in our sample, the retirement age was 65 (and 64 for some older cohorts). We find no evidence that the cohorts with slightly older normal retirement age behave differently at age 52-57 so our main estimates are not driven by anticipation effects stemming from extending the retirement age.

Table 3.2 provides summary statistics on our data. The top panel suggests that the treatment and the control age groups are remarkably similar in terms of employment, wages and share of white collar

[50]We use the *prodest* Stata module of Rovigatti and Mollisi (2020) and apply the estimation procedure of Wooldridge (2009). We regress the logarithm of value added (gross revenue minus the cost of goods sold) on year effects, the logarithm of firm size (variable input) and the logarithm of subscribed capital (state variable), while using material and service costs as proxies for unobserved productivity. The TFP is the residual estimated from this regression.

[51]To estimate the firm wage premia, we use all sample years of the linked employer-employee administrative data. We regress wages on individual and firm effects, controlling for year effects, age squared, age cubed, and firm size.

[52]We collect all hires made during 2003-2015 for each firm and define the poaching index (PI) as the share of these hires that come directly from other firms. To make sure that a firm ID change does not lead to a false high poaching rate we apply the worker-flow method of detecting ID changes as in Saygin et al. (2021). This method can only be reliably applied for firms with at least 10 workers (corresponding, on average, to 5 observed workers in our 50% sample). We calculate the PI for firms with at least 15 hires in our sample between 2003-2015. For the rest of the firms, we impute the PI-based quality using linear and quadratic TFP and AKM firm fixed effects as predictors.

[53]The earliest age to retire was age 58 until 2011, but that possibility was abolished then. To retire at age 58, someone needed to have a long-term employment relationship and at least 37 years of employment history. Note that all workers aged between 52 and 57 between 2012 and 2015 (our main estimation sample) could only retire at the normal retirement age.

jobs. The middle panel summarizes the distribution of treatment and control workers across high- and low-quality firms. For each measure (except for foreign ownership), we divide firms into above-median and below-median groups, taking the median based on all private sector workers, irrespective of their age. The share of workers at high-quality firms is very similar in the treatment and control groups. Finally, in the bottom panel we examine the industry composition of treatment and control workers. Again, we find very small differences suggesting that the treatment and the control groups are similar.

Table 3.2: Summary statistics

	(1) Age 52-54 (Control)	(2) Age 55-57 (Treated)
Panel A: Labor market characteristics		
Private sector employment	0.34	0.32
Monthly private sector wage (HUF)	218,529	217,000
White collar job (private sector workers)	0.31	0.31
Panel B: Firm quality composition		
Above-median TFP	0.49	0.48
Above-median PI	0.34	0.34
Above-median AKM firm effect	0.49	0.48
Above-median firm-level average wage	0.51	0.51
Foreign-owned	0.23	0.22
Panel C: Industry composition		
Agriculture	0.08	0.08
Manufacturing	0.35	0.36
Construction	0.10	0.10
Wholesale and retail trade	0.11	0.10
Accommodation and food service	0.02	0.02
Transportation and storage	0.12	0.10
Administrative and support	0.05	0.06
Number of individuals	123,154	141,875

Note: The treatment group comprises ages 55-57 and the control group comprises ages 52-54. Panel A reports the share of workers employed in the private sector, the average monthly (full-time equivalent) wage of workers employed in the private sector, and the share of workers employed in the private sector in white collar jobs. Panel B reports share of workers at firms with above-median firm quality and at foreign-owned firms. Details on quality measures are provided in Section 3.3.

3.4 Effect on employment

3.4.1 Descriptive evidence

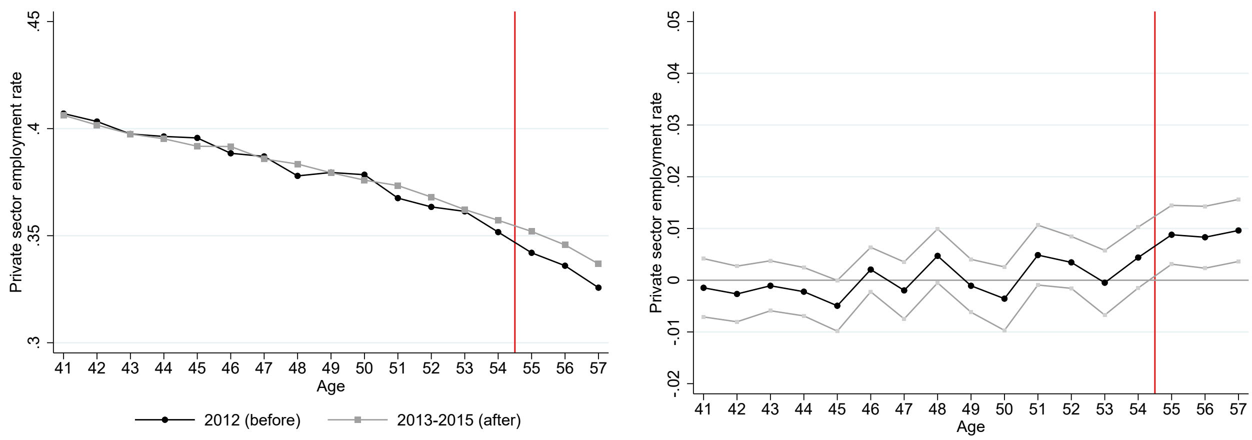
Figure 3.2 shows the share of men working at private sector companies by age before and after the payroll tax cut was introduced in 2013. Panel (a) shows raw employment rates by age before (year

2012, in black) and after (years 2013-2015, in gray) the policy took effect. The figure highlights that employment rates in the private sector gradually decline with age from 41% to 33%. Furthermore, employment rates were similar in 2012 and 2013-2015 for workers younger than 55, which highlights that private sector employment was relatively stable in this period.[54] Finally, there is a clear divergence for workers 55 and older who are affected by the tax cut.

Panel (b) shows the change in employment at private sector companies for men at each age—the difference between the 2012 (black line) and the 2013-2015 employment rate (gray line) shown in Panel (a). In the spirit of our difference-in-differences strategy, we subtracted the average employment change between 2012 and 2013-2015 for the workers between ages 41 and 54. The figure highlights that the employment change was significantly higher above the age 55 cutoff: a 55-year-old worker was 1 percentage point more likely to be employed shortly after the policy was introduced.

Figure 3.2: Employment in private sector companies by age

(a) Private sector employment rate　　(b) Change in private sector employment rate



Note: Panel (a) shows the private sector employment rate by age before and after the payroll tax cut. The black line shows the employment rate in year 2012 (before the implementation of the payroll tax cut) and the gray line for years 2013-2015 (after the implementation of the payroll tax cut). Panel (b) shows the difference in employment rates between years 2012 and 2013-2015 relative to the average change between ages 41 and 54, with the 95% confidence interval (standard errors clustered at the age × period level). The vertical red line shows the age threshold where the tax cut became effective from 2013.

3.4.2 Main results

To study the impact of the payroll tax cut in a difference-in-differences framework, we focus on workers aged 55-57 as our treatment group and workers aged 52-54 as our control group. As we discussed above, the labor market characteristics and the employment composition across firm types and indus-

[54] The average private sector employment rate between ages 41 and 54 in 2013-2015 is 38.4, while it is 38.3 in 2012.

tries are quite comparable across the two groups. We also explore below the sensitivity of the estimates to changing this treatment/control definition.

To study the impact of the tax cut on employment, we estimate the following equation

$$Emp_{it} = \theta_t + \sum_{k=52}^{k=57} \alpha_k \mathbb{I}[age_{it} = k] + \beta \mathbb{I}[t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \tag{3.1}$$

where Emp_{it} measures private sector employment of individual i in month t, θ_t are monthly time effects, $\mathbb{I}[age_{it} = k]$ are age effects, $\mathbb{I}[age_{it} \geq 55]$ is a dummy for the eligibility cut-off, which is age 55 in our context, and $\mathbb{I}[t \geq t_{reform}]$ is the post reform dummy, where t_{reform} is January 2013. In the baseline specification the t index runs from January 2012 to December 2015 and we restrict the sample to individuals who are between 52 and 57 years old. We cluster the standard errors at the age$\times$period level.

Our coefficient of interest is the β term which captures the differential change in private sector employment between the periods before and after the tax cut for treated workers relative to control workers. Panel A of Table 3.3 reports the baseline estimates of β—the difference-in-differences estimate of the impact of the tax cut on employment. We measure full-time equivalent private sector employment (Emp_{it}). Column (1) shows that private sector employment increased by 0.53 percentage points from a baseline of 33% or by 1.59 percent as a result of the payroll tax cut. In Table 3.3, we also calculate the implied labor demand elasticity. The effective tax cut was 6.6 percentage points (a 5.27% decrease relative to the baseline labor cost including the pre-employment payroll tax), which implies that the increase in employment corresponds to an employment elasticity of -0.30 (s.e. 0.03). Appendix Table C.1.5 shows that these results are virtually identical if instead of adjusting employment for working hours, we use a binary employment indicator.

Our elasticity estimate for overall employment is close to what others have found in the literature. For instance, Laun (2017) finds an employment elasticity of -0.22 for older workers in Sweden, while Huttunen et al. (2013) find an elasticity of -0.1 in Finland. For younger workers, Saez et al. (2019) find an employment elasticity of -0.23 in Sweden, while Egebark and Kaunitz (2018) estimate an elasticity of -0.3 in response to the young worker tax cut instituted during the Great Recession in Sweden.

We also investigate whether responses to the policy differ by firm type. Columns (2) and (3) of Table 3.3 summarize the key results. We use regression equation (3.1) with an outcome variable of being employed by a firm with below (column 2) or above (column 3) median total factor productivity. The results show that virtually all the employment increase comes from low-productivity firms, while the employment change is close to zero at high-productivity ones.

Table 3.3: Employment effects of the tax cut

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in private sector employment probability			
— Post × Treated	0.0053***	0.0053***	-0.0001
	[0.0005]	[0.0005]	[0.0004]
Panel B: Percent change in employment			
—Employment without tax cut	0.330	0.167	0.163
—Employment with tax cut	0.335	0.172	0.163
—Percent change in employment	1.59%	3.18%	-0.03%
Panel C: Percent change in labor cost $(1 + \tau_{ss})$			
—Labor cost without tax cut	1.27	1.26	1.28
—Labor cost with tax cut	1.20	1.18	1.22
—Percent change in labor cost	-5.27%	-6.02%	-4.45%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity based on percent change in labor cost	-0.30	-0.53	0.01
	[0.03]	[0.05]	[0.06]
Panel E: Elasticity based on net present value			
—Percent change in net present value of labor cost	-7.49%	-8.82%	-5.98%
—Implied elasticity	-0.21	-0.36	0.01
	[0.02]	[0.03]	[0.04]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Panel A of the table shows difference-in-differences estimates of the impact of the payroll tax cut on private sector employment for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms in Panel A. We report the β coefficient from regression equation (3.1) with the outcome variable being employed at a private sector firm (column 1), at a private sector firm with below-median productivity (column 2) and at a private sector firm with above-median productivity (column 3). Panel B calculates the percent change in employment using the difference-in-differences estimates from Panel A. The first row shows the employment rate in the treatment and control age groups in 2012 (before the reform). The second row adds to that baseline the estimated change from Panel A. The third row shows the percent change in employment relative to the baseline. Panel C calculates the percent change in labor cost analogously. Firms' labor cost is net wage times $(1 + \tau_{ss})$, where τ_{ss} is the employer social security contribution. Panel D calculates the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (Panel B) and labor cost (Panel C). Panel E calculates the percent change in the labor cost caused by the tax cut, taking into account tax cuts realized in the future (see Appendix C.2 for further details). The implied elasticity based on net present value of labor cost is the ratio of the percent change in employment (Panel B) and labor cost (Panel E). Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Table 3.3 also highlights that differences in employment responses cannot be fully explained by the differential impact of the policy on the change in labor cost. Since the amount of tax cut was the same for every worker, the proportional change in labor cost is slightly lower at high-productivity firms, which tend to pay more to their workers. Indeed, we calculate that the labor cost decreases more at low-TFP firms than at high-TFP firms (6.02% vs. 4.45%). Still, the change in labor cost was considerable even at high-TFP firms, with an 4.45 percent decline in labor cost. As a result, the employment elasticity with respect to cost of labor is precisely estimated for the high-TFP firms as well. The estimated elasticity is -0.53 (s.e. 0.05) at low-productivity firms and 0.01 (s.e. 0.06) at high-productivity ones, and the difference in responses to the tax cut between the two firm types are both statistically and economically significant.

3.4.2.1 Elasticity calculations based on the net present value of labor cost. Forward-looking firms might make hiring and firing decisions based on the net present value of labor cost. In our case, this implies that firms might consider that workers in the control group could reach age 55 and become eligible and benefit from the tax cut. To see whether this would alter our results, we calculate the net present value of the tax cut in the treated and control ages separately by taking into account worker age, the typical separation rate, and the discount rate. Panel E of Table 3.3 shows the net present value reduction in the treatment group (relative to the control group) in labor cost using a discount rate of 7% and retirement age 62. We calculate that the tax cut leads to a 7.49% reduction in labor cost in the treated age group. The implied elasticity is -0.21 (s.e. 0.02). This elasticity is somewhat lower (-0.21 vs. -0.30) than the elasticity based on the current change in labor cost.

In columns (2) and (3) of Table 3.3 we also calculate the net present value reduction in labor cost separately at low- and high-TFP firms. Since separation rates are lower at high-TFP firms, we apply different separation rates for the two groups. We calculate an 8.82% reduction in labor cost at low-TFP firms and 5.98% reduction at high-TFP firms. The implied elasticities are -0.36 (s.e. 0.03) and 0.01 (s.e. 0.04), respectively, a statistically and economically significant difference. In Appendix C.2 we provide further details about the calculation of the net present value of labor cost and we also show that the implied elasticity is not sensitive to the discount rate, separation rate, and retirement age applied.

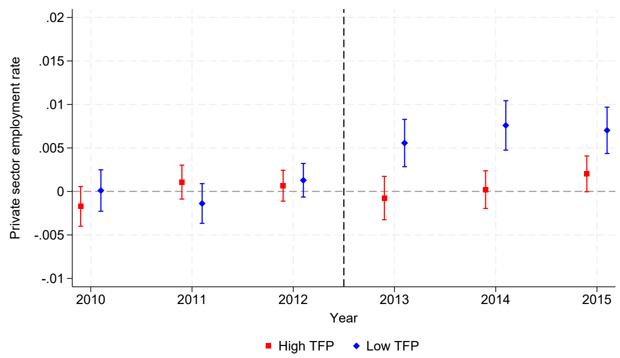
3.4.3 Robustness and credibility checks

3.4.3.1 Parallel trends. The standard identifying assumption in difference-in-differences regressions is that employment in the treatment and control groups would have evolved similarly in absence of the policy change. While this assumption cannot be tested directly, we can study whether the assumption holds pre-policy. To do that we estimate the evolution of the difference between the treatment and control groups over time using the following regression:

$$Emp_{it} = \theta_t + \sum_{k=52}^{k=57} \alpha_k \mathbb{I}[age_{it} = k] + \sum_{h=2010}^{h=2015} \beta_h \mathbb{I}[Year_t = h] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \tag{3.2}$$

where the variable definitions are the same as for equation (3.1), and we make the normalization $\sum_{h=2010}^{h=2012} \beta_h = 0$. In this regression the β_h coefficients show the difference between treatment and control workers in year h relative to the average difference in 2010–2012 and we report those in Figure 3.3. The red squares show the change in employment at high-TFP firms, where we use employment at above-median TFP firms as the dependent variable. The blue diamonds show the estimates at low-TFP firms, where we use employment at below-median TFP firms as the dependent variable. The figure highlights that prior to the introduction of the policy, the employment rates of treated and control workers evolved similarly both at high- and low-TFP firms, suggesting that the control workers are likely a good counterfactual for the treatment workers. At low-TFP firms, employment among treatment workers increased relative to the control group exactly when the reform was introduced in 2013. The impact on employment was between 0.5-0.8 percentage point over years 2013-2015 at low-productivity firms. At the same time, employment at high-productivity firms stayed similar among control and treatment workers.

Figure 3.3: Evolution of employment at low- and high-productivity firms



Note: We report the difference in employment between the 55-57 age group that was affected by the payroll tax cut and the 52-54 age group that was not affected by the tax cut relative to the average difference in 2010–2012. We report β_h coefficient of the regression equation (3.2) where the outcome variable is being employed at an above-median (in red) or at a below-median (in blue) TFP firm. 95% confidence intervals are reported with standard errors clustered at the age × period level.

3.4.3.2 SUTVA and changing the treatment and control definitions. Another key assumption in difference-in-differences style regressions is that the treatment does not affect the control group—the so-called stable unit treatment value assumption (SUTVA). The SUTVA can be violated if firms move away from hiring workers not eligible for the tax cut and replace them by hiring workers who are eligible for the tax cut. This substitution would have only a small effect on untreated workers as long as the share of treated workers is small in the economy.[55] Below we directly assess whether such substitution takes place by studying firms' hiring behavior before and after the policy and show that firms that hired more treated workers do not decrease their hiring of untreated workers.

SUTVA could also be violated as we move closer to the age threshold. This is because those close to the age threshold age into the treatment, which could affect their labor market opportunities.[56] This spillover effect of the treatment on the control group should be less important as we move further away from the age 55 cut-off. Indeed, Panel (b) of Figure 3.2 shows that relative to the average employment rate between ages 41 and 54, we estimate a slightly larger treatment effect, than relative to the average employment rate of those closer to the age cut-off. In Figure 3.4 we further explore the robustness of our employment results to alternative definitions of the treatment and control age groups. Panel (a) shows the estimates for overall employment, while Panel (b) shows the estimates for employment at low- and high-TFP firms separately. The first three estimates from the left keep the benchmark treatment definition (age 55-57), but use control groups farther away from the age 55 cut-off, defining as control group first those who are between 52 and 53 years old and then only 52-years-old individuals. Both the overall employment effect and the estimated difference between the low- and high-TFP firms are similar in these specifications. Next, we show estimates when the treatment group is narrowed, while keeping constant the benchmark control definition. We show estimates first when the treatment group covers only those between 56 and 57 and then when it covers only 57-year-old individuals. The estimated effects are virtually identical in all these specifications suggesting that our estimates are not sensitive to changing the age window in the estimation.

Finally, it is worth highlighting that the differences in separation rates between high-TFP and low-TFP firms could contribute to the heterogeneity in the estimated employment effects—if separation rates are lower at high-TFP firms then they are more willing to hire workers just under the cut-off age, therefore spillover effects may be more substantial at high-TFP firms. Still, as we discussed above when presenting elasticity calculations based on the net present value of the tax cut, we find a clear reduction in labor cost at high-TFP firms even if we take into account these differences in separation rates. Panel

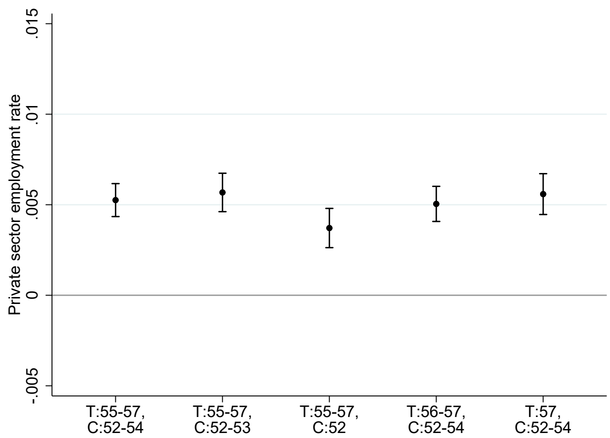
[55]In the standard neoclassical framework, the effect of price change of one input on the demand for another input depends on the share of the first input in the production process and the elasticity of substitution between the two inputs (see Hicks, 1932). Therefore, the change in demand for untreated workers will be small if the share of treated workers is small in the economy.

[56]The difference in incentives disappears as we go closer to the age 55 cut-off. This is why we do not apply a regression discontinuity approach here.

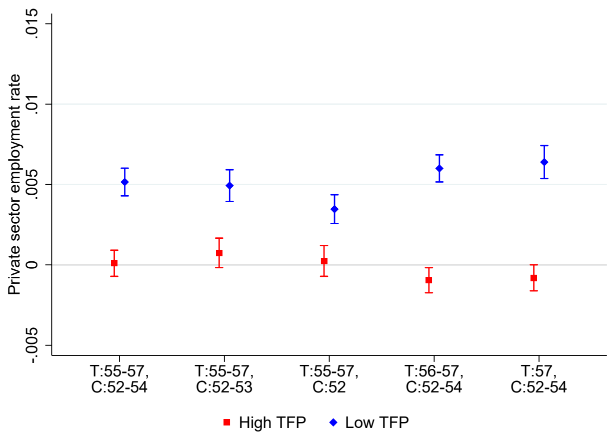
E of Table 3.3 shows that the reduction in labor cost is 8.82% at low-TFP firms and 5.98% at high-TFP firms implying an elasticity of -0.36 (s.e. 0.03) for low-TFP and 0.01 (s.e. 0.04) for high-TFP firms. Therefore, the lower separation rate at high-quality firms cannot explain the differential employment responses.

Figure 3.4: Employment estimates using alternative control and treatment definitions

(a) Employment at all private sector firms



(b) Employment by productivity



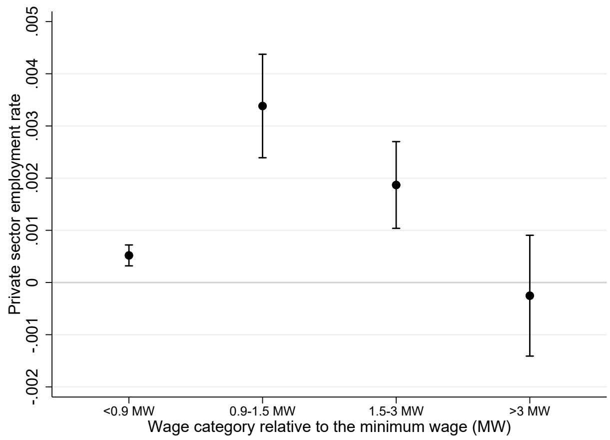
Note: We report estimates of the impact of the payroll tax cut on private sector employment based on equation (3.1) for alternative control and treatment definitions. The estimates show the the change in employment in the treatment age group relative to the change in employment in the control age group. In both panels, the first estimate replicates our baseline results and the subsequent estimates change the age composition of the control ("C") or treatment ("T") groups. 95% confidence intervals are reported with standard errors clustered at the age × period level.

3.4.3.3 **Effects across the wage distribution.**

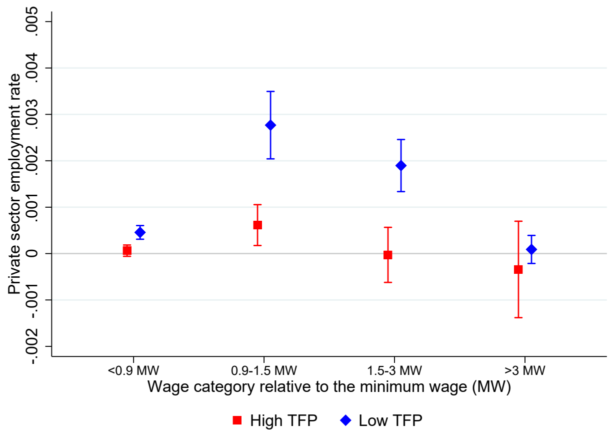
We estimate the change in employment throughout the entire distribution of wages, similarly to the approach of Cengiz et al. (2019). Since the payroll tax cut was lump-sum, we expect that employment would be mainly affected at the bottom of the wage distribution, while the employment effect would be close to zero in the upper part of the wage distribution, where the lump-sum tax cut only introduces a small (relative) change in labor cost. Panel (a) of Figure 3.5 shows the change in employment at all firms. The estimates show that the largest employment effects arise for workers earning between 90% and 150% of the minimum wage, but that there are also substantial effects for workers between 150% and 300% of the minimum wage. In line with the lump-sum nature of the tax cut, we do not find any change in employment above 300% of the minimum wage. Panel (b) of Figure 3.5 shows the employment changes separately for low- and high-productivity firms. The figure demonstrates that most employment changes occurred at low-TFP firms (blue diamonds). At the same time, the changes in employment at high-TFP firms (red squares) are very small and close to zero throughout the entire wage distribution. This partly reflects that there are fewer low-wage jobs at high-TFP firms (see Appendix Figure C.1.1 on the density of jobs in each wage category). Nevertheless, even if we consider the wage category between 150% and 300% of the minimum wage, where there is a high density of jobs at both low- and high-TFP firms we find clear differences in the employment changes: while the change in employment is substantial and statistically significant at low-TFP firms, the change in employment is close to zero at high-TFP firms.

Figure 3.5: Impact of the payroll tax cut across the wage distribution

(a) Employment at all private sector firms



(b) Employment by productivity



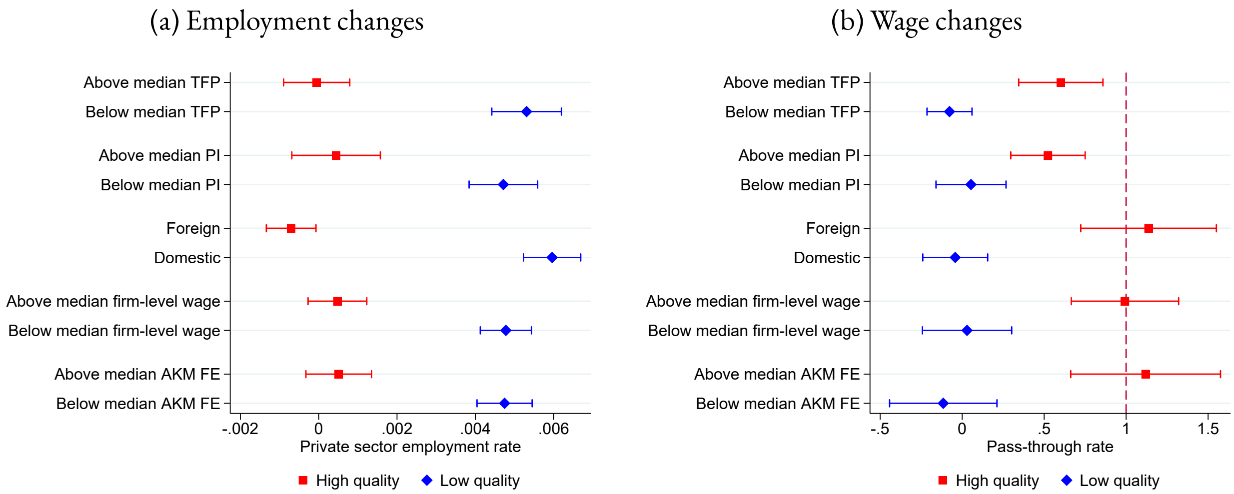
Note: We report the change in employment across the wage distribution. The estimates are based on equation (3.1), where the outcome variable is being employed in a private sector company in a given wage bin (less than the 90% of the minimum wage; between 90% and 150% of the minimum wage; between 150% and 300% of the minimum wage; or above 300% of the minimum wage). 95% confidence intervals are reported with standard errors clustered at the age × period level.

3.4.3.4 Placebo groups unaffected by the tax cut.

As we mentioned in Section 3.3, the reform only affected private sector employees, while the self-employed and workers in the public sector were unaffected by the tax cut. Employment in these groups therefore should not be affected by the policy change. Furthermore, it is also possible that changes in private sector employment simply reflect switching from the public sector or from self-employment. Appendix Table C.1.8 explores the source of the private sector employment increase by estimating our main regression equation (3.1) with mutually exclusive outcome variables: being employed in the private sector (including employment at single-entry bookkeeping firms and at firms with more than 10,000 workers, thus using a broader private sector employment definition than the definition used throughout the rest of the paper), being self-employed, working in the public sector, or being inactive/unemployed. Since these outcome variables are collectively exhaustive, the increase in one outcome must reflect a decline in other ones. Appendix Table C.1.8 shows that the tax cut had a positive effect on employment at private sector firms – due to the inclusion of the smallest (single-entry bookkeeping) firms, the estimated effect is stronger than the baseline results (see Appendix Table C.1.4 for a comparison of the definitions). Appendix Table C.1.8 also shows that there is a slight reduction in the likelihood of being self-employed but the estimated change is an order of magnitude smaller than the employment changes we found for private sector employees. As a result, the switch from self-employment to private sector employment can explain at most 15% of the total increase in private sector employment. Furthermore, the slight negative impact on self-employment was fully offset by the slight increase in public sector jobs. As a result, the increase in the share of private sector employees mainly comes from a decline in unemployment and inactivity. Appendix Figure C.1.2 corroborates these findings by replicating the descriptive evidence on changes in private sector jobs (Panel (b) of Figure 3.2) for public sector job (Panel (a)) and for the self-employed (Panel (b)). The change in employment in these two placebo groups is very small, suggesting that the increase in private sector employment in the treated age groups reflects the impact of the tax cut and not something else.

3.4.3.5 Effect by various firm quality measures. So far, we have focused on the heterogeneous effect of the policy along one dimension of firm quality: firms' total factor productivity. Nevertheless, there are other potential ways to measure firm quality. For instance, the search and matching model with sequential bargaining suggests that the heterogeneous incidence should also emerge if we consider high paying firms and firms characterized by high poaching index (share of new hires coming from other firms instead of unemployment). In Panel (a) of Figure 3.6, we replicate the heterogeneity analysis in the employment effects with other firm quality measures (for short-run effects see Appendix Table C.1.9).

Figure 3.6: Employment and wage changes in private sector companies: alternative firm quality measures



Note: Panel (a) reports estimates of the impact of the payroll tax cut on private sector employment based on estimating equation (3.1). Panel (b) reports estimated pass-through rates based on equation (3.5). The red vertical line corresponds to the full pass-through of the tax cut into higher wages. 95% confidence intervals are reported with standard errors clustered at the age × period level.

Foreign-owned firms are the most productive firms that are usually well integrated into the world economy. Those firms are offering the highest paying, highest quality jobs in the Hungarian context. The estimated employment change at those firms is close to zero and statistically insignificant. At the same time, domestic firms, which are usually less efficient, responded to the policy by creating many new jobs. A similar pattern can be observed when we measure firm-quality using the poaching index, average wages or AKM firm effects. Low-paying firms create many new jobs, changing the composition of jobs in the economy.

Overall these estimates highlight that the composition of jobs changes in response to the tax cut, as low-quality firms will create more jobs than high-quality ones. To make sure that the results are not driven by the endogenous response of total factor productivity and other quality measures to the reform, we replicate the heterogeneous effects using only pre-reform years to define the firm quality indicators. Our results are almost the same using the pre-reform definitions of firm quality measures (Appendix Table C.1.10).

3.4.3.6 Industry vs. firm type heterogeneity. We check whether the estimated heterogeneous effect of the tax cut on employment by firm productivity is driven by differences in the industry composition of high-productivity and low-productivity firms. To do so, we classify firms based on their within-industry relative productivity. We estimate a linear regression of the TFP indicator on level-1 Nomenclature of Economic Activities (NACE) industry codes, generate the residual and calculate its year-specific median. We then estimate the impact of the tax cut on employment at firms with above-median and below-median residualized TFP. The results reported in Panel A of Appendix Table C.1.12 indicate that the employment effect of the tax cut is driven by low-quality firms, even conditional on industry composition.

Panel B shows the main estimates by worker heterogeneity when we proxy workers' skill with occupation. We calculate the change in employment separately for low-paid and high-paid occupations. Low-paid occupations are those that pay below the median on average and high-paid occupations are those that pay above the median on average. The table shows that employment increased by a similar amount in both low-paid (0.28 percentage points) and high-paid (0.24 percentage points) occupations. Furthermore, there is clear heterogeneity within both low-paid and high-paid occupations: virtually all the employment change comes from low-TFP firms. Columns (5) and (6) also highlight that the employment elasticity is similar in low-paid and high-paid occupations. At low-TFP firms it is close to -0.50, while at high-TFP firms it is close to zero within both occupation groups.

3.4.3.7 Worker type vs. firm type heterogeneity. So far, we have focused on the heterogeneous responses to the policy by firm type. Nevertheless, the differential responses by firm type might simply reflect that different types of workers sort to different types of firms. For instance, high-skilled workers might have more bargaining power and they also tend to work at high-TFP firms. To explore the empirical relevance of this interpretation of our main findings, we estimate the employment effects and firm heterogeneity for workers with similar skills.

Table 3.4: Employment effects of the tax cut by subgroups

	(1)	(2)	(3)	(4)	(5)	(6)
		Employment			Elasticity	
	All firms	Low TFP	High TFP	All firms	Low TFP	High TFP
Panel A: By wage						
Jobs paying at most 1.5×minimum wage	0.0039***	0.0032***	0.0007***	-0.43	-0.48	-0.31
	[0.0005]	[0.0004]	[0.0002]	[0.06]	[0.06]	[0.09]
	{35%}	{27%}	{8%}			
	⟨0.1239⟩	⟨0.0922⟩	⟨0.0316⟩			
Jobs paying above 1.5×minimum wage	0.0016***	0.0020***	-0.0004	-0.17	-0.55	0.07
	[0.0005]	[0.0003]	[0.0005]	[0.05]	[0.08]	[0.09]
	{65%}	{24%}	{40%}			
	⟨0.2221⟩	⟨0.0748⟩	⟨0.1473⟩			
Panel B: By occupation						
Low-paid occupations	0.0028***	0.0030***	-0.0001	-0.29	-0.55	0.03
	[0.0004]	[0.0003]	[0.0002]	[0.04]	[0.05]	[0.05]
	{51%}	{28%}	{24%}			
	⟨0.1716⟩	⟨0.0956⟩	⟨0.0761⟩			
High-paid occupations	0.0024***	0.0023***	0.0001	-0.25	-0.47	-0.02
	[0.0006]	[0.0003]	[0.0005]	[0.06]	[0.06]	[0.11]
	{49%}	{19%}	{30%}			
	⟨0.1743⟩	⟨0.0716⟩	⟨0.1028⟩			
Panel C: By education						
Primary and lower-secondary education jobs	0.0038***	0.0037***	-0.0001	-0.29	-0.54	0.02
	[0.0005]	[0.0004]	[0.0003]	[0.04]	[0.06]	[0.05]
	{70%}	{37%}	{33%}			
	⟨0.2354⟩	⟨0.1140⟩	⟨0.1214⟩			
Upper-secondary education jobs	-0.0000	0.0004**	-0.0004	0.00	-0.22	0.34
	[0.0003]	[0.0002]	[0.0003]	[0.10]	[0.11]	[0.26]
	{16%}	{8%}	{8%}			
	⟨0.0547⟩	⟨0.0256⟩	⟨0.0291⟩			
Tertiary education jobs	0.0013***	0.0011***	0.0001	-0.54	-0.69	-0.15
	[0.0003]	[0.0002]	[0.0003]	[0.12]	[0.13]	[0.44]
	{14%}	{7%}	{7%}			
	⟨0.0528⟩	⟨0.0258⟩	⟨0.0270⟩			

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: We report employment effect estimates separately for various subgroups. We estimate the regression equation (3.1) using employment in a given subgroup (job or occupation) and firm type (all firms in column 1, below-median TFP firms in column 2, and above-median TFP firms in column 3) as the outcome variable. In curly brackets we report the subgroup share within each panel. In angle brackets we report the mean of the outcome variable in May 2012 – the probability of being employed in a given subgroup and firm type. In Columns (4)-(6) we calculate the employment elasticity with respect to the wage. Standard errors are reported in brackets, clustered at the age × period level. (N = 9, 003, 984 individual-months)

In Table 3.4 we replicate the main analysis for various skill groups. Panel A shows the estimates when we examine the change in employment at jobs earning at most 1.5 times the minimum wage and for jobs earning above that. This is a similar exercise as in Figure 3.5 where we studied the employment effects throughout the wage distribution. The workers earning at most 1.5 times the minimum wage are predominantly low skilled ones and we see that their employment also increases slightly at high-TFP firms. When we focus on higher skilled workers with wages above 1.5 times above the minimum wage, we still see a clear heterogeneity in the data. Almost all the employment changes come from low-TFP firms, while high-TFP firms do not hire more even if they employ many workers in that wage category. These results suggest that the heterogeneous employment effect by firm quality is not driven by the different earnings composition of jobs by firm quality.

Finally, in Panel C we study worker heterogeneity by education. Since we do not observe education directly, we again rely on occupation information in our data. First, we use the Hungarian Labor Force Survey[57] that has detailed information on education and occupation for the same individuals for a large sample of workers. We calculate the mode of the education level for each four-digit occupation. Then we assess the employment change by the modal education-level in each occupation.

The table shows that the employment increase mainly comes from the lowest-skilled workers with primary or lower-secondary education. There is also a slight increase in employment for workers with tertiary education and no change for workers with upper-secondary education. When we look at employment changes within an education group, we find clear indication for firm heterogeneity in all cases. Employment at low-TFP firms increased within every group and the elasticities vary between -0.22 and -0.69 (see column 5). These elasticities are statistically significant in all cases at the 5% level. At the same time, there is no evidence for significant employment change at high-TFP firms in any education group. The employment change is close to zero in all cases and the elasticities are statistically insignificant at the conventional levels. Overall, these findings highlight that the firm heterogeneity is present even if we focus on a group of workers with the same skill level and so our main results reflect firm heterogeneity and not only worker heterogeneity.

3.4.3.8 Effect on worker transitions and firm dynamics. Next, we decompose the effect of the tax cut on employment into the effect on new hires vs. separations. Then, we analyze whether the employment effects are driven by the entry of new firms as a consequence of the tax cut.

The estimated employment change can come from two sources: (1) workers who have been employed previously and stay employed at higher rates (incumbents) or (2) workers who were unemployed/inactive before and are hired (new entrants). Panel A of Appendix Table C.1.14 decomposes our main employment effect into these two groups. We define incumbent workers as those who had a job in the previous 12 months (between $t-1$ and $t-13$) and new entrants as those who had at least one month without a job in that period. Then we estimate regression equation (3.1) using private sector employment as the outcome separately for incumbents and new entrants.

[57] The Hungarian Labor Force Survey (Hungarian LFS) is very similar to the Current Population Survey in the United States.

Panel A of Appendix Table C.1.14 summarizes the key findings. Employment for new entrants increases by around 0.15 percentage point, which is around 28% of the overall 0.53 percentage point increase reported in Panel A of Table 3.3. This is nevertheless a quite substantial, 3.5% increase relative to baseline population share (4.3%) of new entrants. Employment for incumbents increases by 0.38 percentage point, which is 72% of the overall 0.53 percentage point increase in employment. This is a 1.3% increase relative to the baseline share (29%) of incumbents. These results highlight that the tax cut affected labor market transitions by inducing both higher labor market (re)entry rates and lower separation rates among workers in the treatment age group.

Besides labor market dynamics, we can also study the potential change in firm dynamics. A key prediction of models with perfectly competitive labor markets and firm heterogeneity à la Melitz (2003) is that employment creation should take place through firm entry. Panel B of Appendix Table C.1.14 shows the decomposition of the total change in employment into newly entering firms (firms that did not exist in the previous calendar year) and firms that existed before. Contrary to the prediction of models with perfectly competitive labor markets, we find that almost all the employment creation comes from firms that existed before, suggesting that no new firms were set up in response to the tax cut. Panel C corroborates these findings by showing that employment mainly increased at firms that existed before 2012, while the change in employment at newly created firms is negligible.

3.4.3.9 Labor market institutions and the minimum wage. As we noted before in Section 3.3.1, unions are weak in Hungary and central bargaining of wages is almost non-existent. As a result, larger firms do not usually face organized workforce with more institutional protections. Still to make sure that our results are not simply driven by large firms, we examine heterogeneity by firm size in Appendix Table C.1.13. We divide firms into two size categories, using the definitions of OECD (2022b): micro and small firms (1 to 49 employees) and medium-sized and large firms (50 or more employees). More refined categorization is hindered by the fact that the vast majority of the smallest (micro) firms have below-median TFP and the vast majority of large firms have above-median TFP. We find that employment at low-productivity firms increases in both firm size categories, while among high-productivity firms there is no consistent employment effect in either firm size category.

We also discuss the potential impact of minimum wages on our results. In the presence of binding minimum wages, the tax cut could make some jobs viable, which could explain why job creation takes place disproportionately at low-productivity firms. That might play some role: as we saw on Figure 3.5, some jobs were created around the minimum wage in response to the tax cut. Nevertheless, there is also significant job creation substantially above the minimum wage at low-TFP firms, which means that our findings do not simply reflect the interaction of the minimum wage with the tax cut.

We also showed in Section 3.4.3.8 that firm dynamics and new firms entering after 2012 are not the major source of job creation (see Appendix Table C.1.14) and around 78% of the jobs come from incumbent workers. This again suggests that the extra jobs are unlikely to simply reflect jobs that were not viable before.

3.4.3.10 The role of the economic environment. As we discussed in Section 3.3.1, the Hungarian labor market was booming in this period. To understand the importance of local economic conditions, we study the impact of the policy across local labor markets in Appendix Table C.1.16. The country consists of 197 districts. We first divide districts by unemployment rate in 2012 and study the impact separately in districts with above- and below-median unemployment rates in Panel A. The effect of the tax cut on employment is somewhat larger in regions with above-median unemployment rate, where the average unemployment rate was around 18.3%, than in regions with below-median unemployment rate, where the average unemployment rate was around 8.6% (0.65 percentage points vs. 0.55 percentage points). Nevertheless, the heterogeneity is very similar across firms, as almost all the employment change comes from low-TFP firms.

In addition, we also divide districts by the change in private sector employment rate in Panel B. In stable labor markets the change in private sector employment is less than 2 percentage points (in absolute value), while in improving labor markets the change is more than 2 percentage points. The change in employment and the heterogeneity pattern is very similar in booming and stable environments.[58] Overall, these findings suggest that local economic conditions are unlikely to play a major role in explaining our main findings.

3.4.3.11 Substitution. A common concern about targeted tax cuts is that firms may substitute treated workers for untreated ones. This substitution could bias our main estimates, if it leads to substantial change in employment in the control group. Nevertheless, as we discussed in Section 3.4.1, there is no indication of any significant change in employment in the data among individuals in the

[58] We do not have enough districts with substantial decline in labor market conditions and so we cannot study the impact of the tax cut in a recessionary environment.

control group. The lack of large employment responses in the control group is not surprising given that only a low share of the workers are treated and so the substitution effect on untreated workers should be limited.[59]

A different concern from the policy maker's perspective could be that firms that hire more treated workers might decide to hire fewer prime age or other untreated workers. We directly test the empirical relevance of this concern by studying the firm-level relationship between hiring treated and untreated workers before and after the policy change in Appendix Figure C.1.3. The figure shows the non-parametric relationship between the two-year change in firm-level employment of treated workers (considering workers both below the age 25 and above the age 55 thresholds) and that of untreated ones (relative to the employment at baseline). We calculate the pre-policy relationship by studying the change between 2010 and 2012 (black dots and line) and the post-policy relationship between 2012 and 2014 (blue stars and line). We also calculate the no substitution counterfactual (red squares and line): how much the pre-policy relationship would change if firms increased their hiring of treated workers by the observed average firm-level employment change from 2012 to 2014, but kept the hiring rate of untreated workers at the 2010 to 2012 level. This no substitution counterfactual is closely aligned with the post reform relationship, indicating that substitution from untreated workers is limited in our context.

3.5 Effect on wages

3.5.1 Main results

We study the impact of the tax cut on wages in this section. First, we study the impact on the wages of new entrants by estimating the following regression equation:

$$\ln w_{it} = \sum_{k=52}^{k=57} \alpha^k \mathbb{I}[age_{it} = k] + \theta \mathbb{I}[year_t \geq t_{reform}] + \beta \mathbb{I}[year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \quad (3.3)$$

where w_{it} is the net wage of individual i in May at year t. Note that for wages, we use annual data

[59] This argument is similar to the one made in Appendix Section B in Cengiz et al. (2019). Given that the share of treated workers in the aggregate production function is small, realistic values of labor-labor substitution put an upper bound on the size of employment changes of the untreated population.

throughout this section as this is the level of observation available.[60] In our case, t_{reform} is 2013.

A key limitation of the regression equation above is that it considers the same proportional wage changes across the entire wage distribution. Nevertheless, given the lump-sum nature of the tax cut, we expect that the proportional increase in wages will be quite small for high wage earners and could be much larger for low wage earners. To take this into account, we assess the impact of the policy by the tax cut rate – the size of the payroll tax cut relative to the wage in the previous year, formally $TCR_{it-1} = 14,500/w_{it-1}$, where HUF 14,500 is the tax cut amount. This variable goes from 14.5% for low wage earners to zero for very high wage earners, and reflects the percent change in wages that would occur if all of the tax cut were passed through to the worker. Notice that the tax cut rate is calculated for both treated and control workers. For the latter, the tax cut rate reflects the size of the tax cut (relative to their income) that would have been received if the workers were treated.

Then we estimate the following regression:

$$\ln w_{it} = \sum_{k=52}^{k=57} (\alpha_0^k + \alpha_1^k TCR_{it-1})\mathbb{I}[age_{it} = k] + (\theta_0 + \theta_1 TCR_{it-1})\mathbb{I}[year_t \geq t_{reform}] + \\ + (\beta_0 + \beta_1 TCR_{it-1})\mathbb{I}[year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \tag{3.4}$$

where we interact each term in regression equation (3.3) with the tax cut rate, TCR_{it-1}. To calculate TCR_{it-1}, we need to rely on the previous year's wage and so we can only estimate this regression for workers who worked in the previous year (incumbent workers). Furthermore, to make sure that our exposure measure TCR_{it-1} is not contaminated by the policy itself, we only use one post-policy year 2013 and one pre-policy year 2012 in the benchmark regression. Later we perform a robustness check where we define the tax cut rate based on wages two years before, formally $TCR_{it-2} = 14,500/w_{it-2}$, and then we use data from 2014 and 2012. In the benchmark specification we also focus on full-time, full-month workers, to minimize measurement error in wages, and present robustness checks which include part-time workers.

The results of the wage regressions are reported in Table 3.5. Column (1) estimates wage effects for new entrants using equation (3.3). The change in the wages of new entrants is small and statistically insignificant. The average tax cut rate for new entrants was around 0.11, suggesting that the pass-

[60] We only see annual income for employment relationships spanning the entire year. This is a common feature of administrative social security data (see e.g. German IAB data).

through rate for new entrants is around 21% (s.e. 0.17).[61]

Table 3.5 also shows the estimates for the incumbent workers for whom we can calculate the tax cut rate. Column (2) suggests that the average impact of the tax cut on wages among incumbent workers is positive. The coefficient showing the treatment effect post policy in relation to the tax cut rate (β_1) is 0.22 (s.e. 0.09). This implies that a \$1 increase in the tax cut would result in a 22 cent increase in wages on average, or that average pass-through is 22% with firms capturing 78% of the tax cut on average. This estimate is similar to the one found for new entrants, though it is more precisely estimated here. We also examine heterogeneity in this treatment effect. We estimate the following equation, using the notation of equation (3.4):

$$\begin{aligned} \ln w_{it} &= \sum_{k=52}^{k=57}(\alpha_0^k + \alpha_1^k TCR_{it-1} + \alpha_2^k Q_{j(i,t)} + \alpha_3^k TCR_{it-1}Q_{j(i,t)})\mathbb{I}[age_{it} = k]+ \\ &+ (\theta_0 + \theta_1 TCR_{it-1} + \theta_2 Q_{j(i,t)} + \theta_3 TCR_{it-1}Q_{j(i,t)})\mathbb{I}[year_t \geq t_{reform}]+ \\ &+ (\beta_0 + \beta_1 TCR_{it-1} + \beta_2 Q_{j(i,t)} + \beta_3 TCR_{it-1}Q_{j(i,t)})\mathbb{I}[year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \end{aligned} \quad (3.5)$$

where we interact all coefficients in equation (3.4) with $Q_{j(i,t)}$, the quality of firm j where individual i works at time t. To check that our estimates are not simply driven by transitioning to high-quality firms, in Appendix Table C.1.17 we show that the estimated treatment effects are robust to using the firm quality in the previous year.

[61] Since past wages are not observed for new entrants, we cannot calculate TCR_{it-1}. Therefore, we approximate the tax cut rate using the current wages, formally $TCR_{it} = 14,500/w_{it}$. This is the exact tax cut rate TCR if there is no pass-through. If part of the tax cut is passed through then we should have (w_{it} − Pass-through) in the denominator. Assuming 100% pass-through the average tax cut rate would be 0.12.

Table 3.5: Wage effects of the tax cut

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) log(wage)	(6) log(wage)	(7) log(wage)	(8) log(wage)
Post × Treat	0.022	-0.019*	0.008	0.007	-0.026	0.021**	-0.021**	0.011
	[0.018]	[0.010]	[0.007]	[0.006]	[0.113]	[0.009]	[0.009]	[0.016]
Post × Treat × TCR		0.221**	-0.077	-0.071	0.249	-0.191**	0.149*	-0.129
		[0.090]	[0.070]	[0.053]	[0.925]	[0.085]	[0.081]	[0.215]
HighTFP × Post × Treat			-0.046***	-0.041***	-0.068	-0.040***	-0.045***	-0.053**
			[0.013]	[0.011]	[0.118]	[0.006]	[0.014]	[0.021]
HighTFP × Post × Treat × TCR			0.678***	0.602***	0.905	0.600***	0.632***	0.780***
			[0.137]	[0.104]	[1.032]	[0.038]	[0.163]	[0.242]
Windfall × Post × Treat								0.546*
								[0.277]
Windfall × Post × Treat × TCR								-5.979**
								[2.588]
Pass-through rate								
All firms	0.208	0.221**						
	[0.168]	[0.090]						
Low TFP			-0.077	-0.071	0.249	-0.191**	0.149*	-0.129
			[0.070]	[0.053]	[0.925]	[0.085]	[0.081]	[0.215]
High TFP			0.602***	0.531***	1.154**	0.409***	0.781***	0.651***
			[0.131]	[0.110]	[0.425]	[0.107]	[0.121]	[0.097]
Observations	13,429	97,789	97,789	93,666	4,123	112,713	82,910	97,789
New entrant/incumbent	new	incumb	incumb	incumb	incumb	incumb	incumb	incumb
Workers	all	all	all	same firm	poached	all	all	all
Part-time included	no	no	no	no	no	yes	no	no
One vs. two yr change	one	one	one	one	one	one	two	one
Windfall rate included	no	no	no	no	no	no	no	yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Column (1) estimates the effect on wages for new entrants who entered the labor market in the current year and so have less than 12 months employment using equation (3.3). Columns (2)-(8) estimate the wage change for incumbent workers (who have been continuously employed in the previous 12 months). Column (2) estimates wage change for all firms using equation (3.4), while columns (3)-(8) estimate heterogeneity by firm productivity using equation (3.5). Column (3) shows wage changes for all incumbent workers, while columns (4) and (5) show estimates for workers who stayed at the same firm and workers who were poached to another firm, respectively. In all columns except column (6) we focus on full-time workers. In column (6) we also include part-time workers in the analysis. In all columns except in column (7), we compare the wage changes between 2012 and 2013. In column (7) we study two-year wage changes and compare the wage change between 2012 and 2014. In column (8), we also interact the treatment, age, year, and tax cut rate indicators with the firm specific windfall rate, which reflects the size of the windfall received by the firm as a result of the tax cut. The pass-through rate at low-productivity firms is the β_1 coefficient in equation (3.5), while at high-productivity firms it is the sum of the β_1 and the β_3 coefficient in equation (3.5). Standard errors are reported in brackets, clustered at the age × period level.

Column (3) of Table 3.5 shows the main estimates of treatment effect heterogeneity. The estimates show that the wage effects are driven by high-productivity firms. In high-quality firms, the pass-through rate is 60% (the sum of β_1 and β_3, which is 68% plus -8%) and statistically significant. At the same time, the pass through rate is close to zero and statistically insignificant at low-quality firms. This is consistent with the predictions of labor market imperfections but not with the perfect competition (see Table 3.1). The pass-through heterogeneity holds both for workers who remain at the same firm and workers who transition to another firm (columns (4) and (5)), although the pass-through rate of the tax cut is higher for those who change employer. This latter is more in line with the search model with sequential bargaining predicting that switchers should experience a larger gain.

3.5.2 Robustness and credibility checks

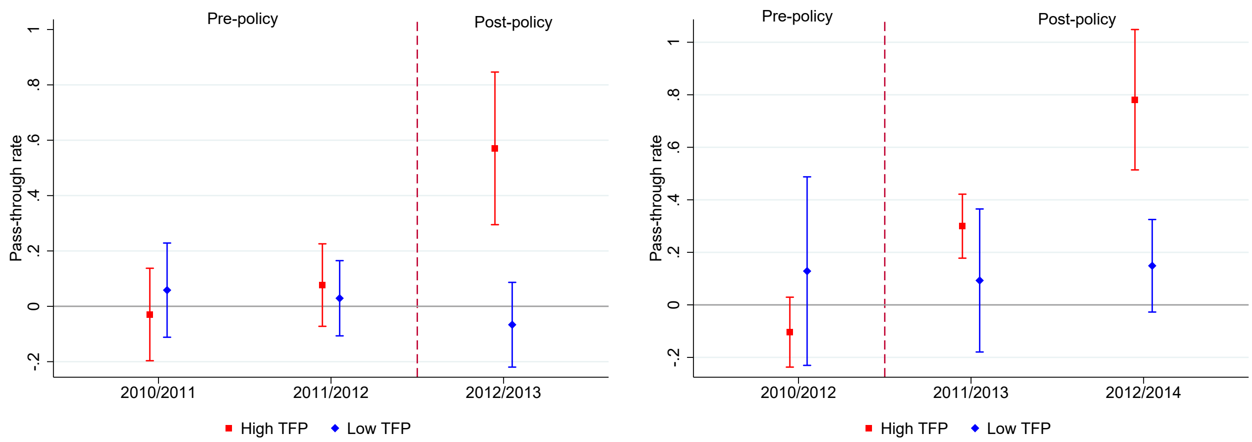
3.5.2.1 Parallel trends. Similarly to the employment estimates, the key identifying assumption in our difference-in-difference style regression is that wages in the treated age group would have evolved similarly to those in the control age group in the absence of the payroll tax cut. While this assumption cannot be tested directly, we can test whether the assumption holds in the pre-policy years. We estimate the same regression equation as for the main analysis, but we shift the time window to the pre-reform years and assume pre-reform (hypothetical) treatment years. Panel (a) of Figure 3.7 shows the estimated pass-through when we estimate regression equation (3.5) using years 2011-2012 (assuming $t_{reform} = 2012$) and 2010-2011 ($t_{reform} = 2011$). We report the estimated pass-through at low-productivity firms (β_1 from equation (3.5)) and high-productivity firms ($\beta_1 + \beta_3$ from equation (3.5)). In both pre-reform placebo analysis, we find no indication for any wage change at high- or low-productivity firms. The effects are therefore specific to the actual treatment year.

3.5.2.2 SUTVA and changing the treatment and control definitions. Similarly to the employment estimates we also study the sensitivity of our estimates to changing the treatment and control groups to alleviate the concerns related to spillovers to the control group and the potential violation of the SUTVA. Figure 3.8 shows the pass-through estimates for all firms (Panel (a)) and by firm quality (Panel (b)). The estimated patterns remain very similar if we define the control group farther away from the age 55 cut-off by using workers who are 52 and 53 years old or 52-year-olds only as the control group. We also explore how the estimates change if we define narrower treatment age groups. We show estimates when the treatment includes only those between 56 and 57 and when it includes only 57-year-olds. The estimated effects are similar in all these specifications suggesting that our estimates are not sensitive to changing the age window in the estimation.

3.5.2.3 Wage changes by tax cut rate. So far, we have assumed a linear relationship between the tax cut rate, TCR_{it-1} and wage changes. We also study the non-parametric relationship by estimating the change in wages for tax cut rate categories separately. In particular, we estimate regression equation (3.5) but replace the continuous tax cut rate variable with a set of dummy variables showing different levels of the tax cut rate. Figure 3.9 shows the main estimates separately for low- (blue diamonds) and high- (red squares) productivity firms. In the figure, past wages, w_{it-1}, increase from the left to the right and so the tax cut rate—the size of the (lump-sum) payroll tax cut relative to the wage—falls.

Figure 3.7: Evolution of wage changes in private sector companies

(a) One-year wage changes (b) Two-year wage changes

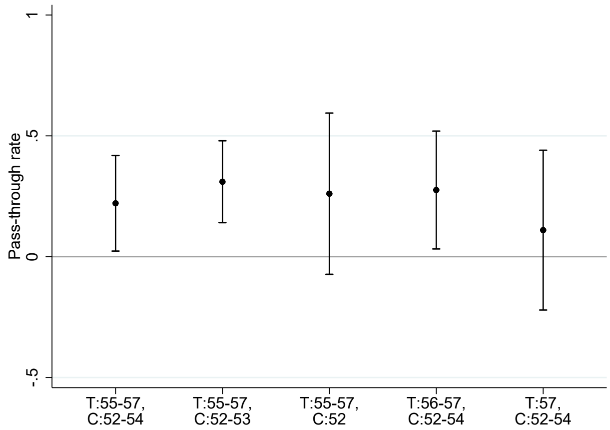


Note: Estimates of pass-through rates based on equation (3.5) are shown. Each result is based on the change in wages between the years indicated on the x-axis. Panel (a) shows changes over one-year intervals and Panel (b) over two-year intervals. 95% confidence intervals are reported with standard errors clustered at the age × period level.

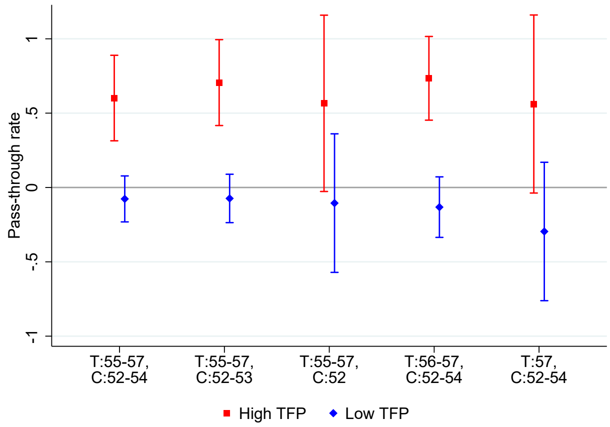
The figure demonstrates that at high tax cut rates there is a clear change in wages at high-productivity firms, but not at the low-productivity ones. Furthermore, as the tax cut rate decreases (from left to right) we see a decrease in wage changes at high-productivity firms as we would expect if the wage changes were driven by the tax cut. At low tax cut rate levels the wage changes are small for both high- and low-productivity firms. The non-parametric relationship between tax cut rate and wage changes, therefore, corroborates that the estimated wage changes at high-TFP firms are driven by the tax cut and not something else.

Figure 3.8: Wage changes using alternative control and treatment definitions

(a) Wages at all private sector firms

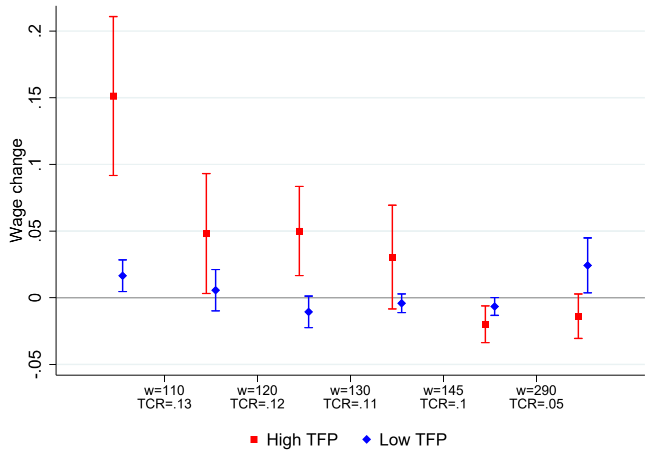


(b) Wages by productivity



Note: Estimates of pass-through rates based on equation (3.5) are shown for alternative treatment and control definitions. In both panels, the first estimate replicates our baseline results and the subsequent estimates change the age cutoffs for the control ("C") or treatment ("T") groups. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level, except for the third and fifth estimate points (T:55-57, C:52 and T:57, C:52-54), where we do not cluster the standard errors as one cluster would capture the entire treatment or control age group.

Figure 3.9: Wage changes at different levels of lagged wages



Note: Estimates based on a modified version of equation (3.5) are shown, where the linear tax cut rate TCR_{it-1} is replaced with categories of the tax cut rate TCR_{it-1}. We report the cut-off values of lagged wages (in thousands of Hungarian forints) and the corresponding tax cut rates TCR_{it-1} on the x-axis of the figure. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

3.5.2.4 Robustness to including part-time workers. Since in our data we do not perfectly observe hours worked, so far, we have focused on full-time workers whose wage information is more precisely estimated. Column (6) of Table 3.5 shows the estimated change in wages when we include part time workers in the sample. The estimated pass-through at high-productivity firms declines when including part-time workers (from 60% to 41%) but it remains both economically and statistically significant.

3.5.2.5 Robustness to two-year change. So far, we have focused on one-year changes post policy. We made this restriction because we wanted to make sure that the policy change itself does not affect the measure of the tax cut rate, TCR_{it-1}, through changes in the previous year's wage. As a robustness check, we redefine the tax cut rate as $TCR_{it-2} = 14500/w_{it-2}$ and study two-year changes. Column (7) of Table 3.5 shows the estimates when we examine two-year changes. The estimated pass-through is somewhat higher (78% vs. 60% at high-productivity firms). In Panel (b) of Figure 3.7 we also report two-year wage changes. It suggests that between 2010-2012, the wages of control and treated workers evolved fairly similarly with the divergence happening only when the tax cut was introduced in 2013.

3.5.2.6 Effect by various firm quality measures. Similarly to the employment estimates, we replicate the heterogeneity in the wage effects analysis using other indicators of firm quality. We report the results in Panel (b) of Figure 3.6 and in Appendix Table C.1.11. We see that a similar pattern of incidence emerges for a wide class of measures of "good" firms. Workers at foreign, high-poaching-index, high-wage, and high-wage-premium firms experienced substantial wage increases, ranging between around 50% to almost full pass through. At the same time, workers at domestic, low-poaching-index, low-wage and low-wage-premium firms did not experience any wage increases.

This suggests that the heterogeneity in incidence that we uncover is not tied to one specific quality measure and is a basic feature of the labor market. Comparing panels (a) and (b) of Figure 3.6 also demonstrates the heterogeneous incidence of the policy by firm type. While low-quality firms (blue diamonds) respond on the employment margin and not the wage margin, the opposite is true for high-quality firms (red squares).

3.5.2.7 Industry vs. firm heterogeneity. We also explore whether the differential pass-through rate is simply related to the industry composition of workers. We classify firms based on within-industry variation in TFP as discussed in Section 3.4.3.6. The results reported in Panel B of Appendix Table C.1.12 indicate that the estimated heterogeneity in the incidence of the tax cut remains very similar to the benchmark classification. The pass-through rate is 46% for high-productivity firms based on the within-industry classification vs. 60% based on the overall classification.

3.5.2.8 Effect by education categories. We estimate wage effects by education categories and report the results in Appendix Table C.1.19. Education is defined by the mode of the education level for each four-digit occupation (see Section 3.4.3.7 for details—here, to reduce the noise in the estimates, we consider two education categories: primary and lower-secondary on the one hand and upper-secondary and tertiary on the other hand). The table shows that for both education categories the pass-through rate of the tax cut is bigger at high-TFP and high wage premium firms. Also, the pass-through rate is higher and its heterogeneity is stronger at higher education category jobs, where the bargaining channel is likely to play a more important role (see e.g. Cahuc et al., 2006; Hall & Krueger, 2012).

3.5.2.9 Effect by firm size. We also examine the heterogeneity of wage effects by two firm size categories, using the same categorization as for the employment effects. The results reported in Appendix Table C.1.15 indicate that qualitatively the pattern of the wage effects is similar both at micro and small firms (size 1-49) and at medium-sized and large firms (size 50+), although the pass-through rate at high-quality firms is higher among medium-sized and large firms (65%) than among micro and small firms (45%).

3.5.3 Rent sharing and windfall effects

Recent empirical work shows that firms that received larger rents or windfalls as a result of a tax cut for younger workers, grew more rapidly in the context of Sweden (Saez et al., 2019). We study the presence of such windfall effects in the context of the tax cut for older workers in Hungary. The main results are summarized in Appendix Figure C.1.4. We compare firms that have a high share of treated workers aged 55 and above with firms that have a medium share of such workers. Similarly to Saez et al. (2019) we find mean reversion in the ratio of the windfall revenues to the total payroll (which we call exposure). Firm size, wages and sales revenue after the reform trend similarly for firms with high and medium shares of treated workers, and so we find no clear indication that windfall effects are important for this population. Interestingly, when we examine the impact of a tax cut on younger workers in Hungary in Section 3.7, we find remarkably similar findings as in Saez et al. (2019).[62] This suggests that the lack of windfall effects for older workers is unlikely to reflect the different economic environment, and that the tax cut impacts younger and older workers differently.

[62] Appendix Figure C.4.10 implements the same windfall analysis for younger workers. Similarly to Saez et al. (2019), we find no pre-trends between high exposure and medium exposure firms among younger workers, but document an increase in revenues and employment at high exposure firms (relative to medium exposure firm) after the tax cut.

Another important finding in Saez et al. (2019) is that firms shared the rents coming from the tax cut equally between young treated and untreated workers. Such rent sharing would work against finding any wage effects in our empirical design that compares the wage change between treated and untreated workers. Still, as we demonstrated above, we find clear indication of wage changes between treated and untreated workers for high-productivity firms.

Nevertheless, we directly assess the implication of rent sharing in column (8) of Table 3.5. We calculate the firm-level rent as in Saez et al. (2019) by taking the ratio of all the tax cuts instituted in 2013 (including those affecting younger workers and workers in elementary occupations) and the pre-reform total wage bill. We include this windfall measure in equation (3.5) and interact it with the age categories, the post reform dummy, and the post reform by treatment age dummy, and the interaction with the tax cut rate variable, TCR_{it-1} (including all other variables that are interacted with tax cut rate in equation (3.4)). The results show that including the windfall effects in the regression does not change the estimated pass-through at high- and low-productivity firms. If anything the estimated pass-through effects are slightly larger at high-productivity firms (65% instead of 60% in the benchmark estimate) and still close to zero at low-productivity firms once we take into account the windfall effects. Appendix Table C.1.18 also shows that the windfall effects do not change the pass-through estimates when other firm quality measures are applied.

The treated post-reform windfall coefficient in column (8) of Table 3.5 suggests that firms hit by larger windfall increase the wages of treated workers slightly more than the wages of untreated workers. Nevertheless, these effects are less important at lower wages, where the tax cut played a more important role. Furthermore, the effect of the windfall shock on wages was limited given that the average windfall rate was 2.7% in our sample. Overall, these findings underscore the important role of firm heterogeneity, which is present even if we take into account the firm-level windfall shocks brought by the policy.

3.6 Welfare analysis

In this section we evaluate the policy's welfare impact, taking into account its costs and fiscal externalities. We follow the method proposed by Hendren and Sprung-Keyser (2020) to calculate the Marginal

Value of Public Funds (MVPF) for the age-dependent payroll tax cut. We apply the following formula:

$$\text{MVPF} = \frac{\text{WTP}}{\text{Net Government Cost}}, \tag{3.6}$$

where the Willingness to Pay (WTP) is the sum of individuals' willingness to pay for the policy out of their own income and the net cost is the net impact of the policy on the government budget.

The WTP consists of three parts. First, the part of the tax cut that is received by workers enters workers' WTP with a positive sign. To calculate this, we first calculate the per capita average amount of the tax cut (using the employment rate and average effective tax cut). Then, based on the estimated pass-through in Table 3.5, we determine the fraction of the tax cut that goes to workers. Second, workers who gain employment as a result of the tax cut lose their unemployment benefits which enters their WTP with a negative sign. Here, we rely on the estimated treatment effects on employment (Table 3.5) and the average unemployment benefit as observed in our data. Third, workers who gain employment are paid wages by their employers which enters their WTP with a positive sign—to calculate this part of the WTP, we estimate the employment effect by wage categories. The net cost is the sum of the tax cut minus the benefits a non-employed person receives minus the taxes paid after the additional wage due to increasing employment.

We calculate the MVPF two different ways. Under the first approach, we assume the policy maker only cares about workers' welfare and the social marginal utility of employers is zero. In this version, we do not incorporate the part of the tax cut that goes to employers into the WTP. In an alternative calculation, we assume that social marginal utility is the same on workers and employers and so we incorporate the part of the tax cut that goes to employers into the WTP.

We present the calculations in Table 3.6. When the policy maker only cares about workers' welfare, the overall MVPF is 0.27. The low MVPF reflects the fact that our estimates imply that most of the tax cut benefited employers. The MVPF is much larger at high-productivity firms (0.51) than at low-productivity ones, where it is close to zero. The difference is mainly due to the higher pass-through rate of the tax cut to workers at high-quality firms. Our calculation, therefore, highlights that if policy makers mainly care about workers' welfare they should target high-quality firms with the tax cut.

Once we include the part of the tax cut going to employers into the WTP, the relationship between the MVPF and firm quality flips: payroll tax cuts targeting high-productivity firms have lower MVPF

Table 3.6: Marginal value of public funds

	(1) All firms	(2) Low TFP	(3) High TFP
(1) Direct cost	5116	2402	2774
(2) Tax cut going to workers	974	-159	1437
(3) Benefit receipt of non-employed who become employed	328	328	-6
(4) Additional net wages of non-employed who become employed	510	473	-10
(5) Additional tax revenue	438	401	-9
(1)-(3)-(5) Net cost	4349	1673	2789
(2)+(4)-(3) Willingness to pay (WTP), workers only	1155	-14	1433
(1)+(4)-(3) Willingness to pay (WTP), workers and firms	5297	2547	2770
Marginal value of public funds (MVPF), workers only	0.27	-0.01	0.51
Marginal value of public funds (MVPF), workers and firms	1.22	1.52	0.99

Note: We report per-worker average monthly amounts in HUF for workers aged 55 and above in each row. Row (1) reports the direct cost defined as the tax cut multiplied by the employment rate of the treatment group. Row (2) reports the tax cut received by workers based on the wage effect results reported in Table 3.5. Row (3) reports the benefits that non-employed individuals who become employed would have received based on the estimated employment effect of the reform and the average unemployment benefit amount. Row (4) reports the additional net wages received by non-employed individuals who become employed based on the estimated employment effect by wage categories. Row (5) reports the additional tax revenue defined as the total estimated income tax and social security contributions paid for workers who become employed. The marginal value of public fund (MVPF) is the ratio of willingness to pay and the net cost.

(0.99) than payroll tax cuts targeting low-productivity firms (1.52). This is because when the incidence of the tax cut between employers and employees does not matter, the employment creation effect will dominate the welfare calculations. Since employment creation mainly takes place at low-productivity firms, the MVPF will be larger for targeting these firms with the tax cut.

3.7 Effect on younger workers

Besides the payroll tax cut for older workers, a similar tax cut was also introduced for workers under age 25 in 2013. The tax cut led to a 6.6% reduction in the labor cost. We apply the same difference-in-differences model as for the older population to examine the impact of the policy on these workers. We summarize the basic results here and provide further details in Appendix Section C.4.

The overall impact of the tax cut on employment was larger for younger workers than for older workers (see Appendix Table C.4.22). The estimated employment elasticity with respect to the cost of labor is -0.77 (or -0.52 based on the net present value of the tax cut). We find similar heterogeneity in the employment responses of younger workers, though the strength of heterogeneity depends on the firm quality measure applied (see Appendix Figure C.4.8). We find that most jobs are created at firms with low AKM wage premia, low poaching rates, and at domestic firms, but contrary to the old, we find positive job creation even at better quality firms. Turning to wages, we find no indication of significant wage differences between treated and untreated younger workers (see Appendix Figure C.4.9).

Two points should be noted. First, similarly to us, Saez et al. (2019) find no differential change in wages in response to payroll tax cuts targeting young workers in Sweden.[63] Our findings highlight that wage pass-through differs among young and older workers. These differences could be explained by wage rigidity that constrains firms' pass-through differently for younger and older workers. For instance, passing through the tax cut to younger workers could mean a wage increase for a 22-24 years old and then a wage cut once workers reach age 25. At the same time, passing through the tax cut would simply mean that once age 55 is reached a pay raise is implemented. The latter might be more feasible than the former because workers dislike pay cuts (Bewley, 1998).

Second, the lack of wage responses for younger workers could be explained by that most young workers have little scope for wage negotiation in entry-level jobs (see Caldwell et al., 2025). The large share of new entrants also implies that workers who are entering the labor market, or workers in probationary period, have no credible outside option and so firms can hire them and extract all the rents. If the share of these types of workers is large in a labor market, there will be smaller differences in the hiring incentives of low- and high-productivity firms. Thus, these differences between young and old workers are consistent with models of imperfect competition in the labor markets.

3.8 Conclusion

This paper studies the implications of payroll tax cuts in the presence of imperfect competition in labor markets. We highlight that tax policies can have heterogeneous impact across firm types. As a result, tax policies may change the composition of jobs in the economy. To empirically assess these heterogeneous effects, we exploit the introduction of age-dependent payroll tax reductions in Hungary. Using rich administrative data, we show that in response to a large tax cut, both employment and wages increased among older workers affected by the policy. However, there is substantial heterogeneity across firm types. The positive effect of the payroll tax cut on employment is driven by low-quality firms, while the

[63] In Appendix Section C.4 we also replicate their firm-level analysis and show that our findings for the young are broadly consistent with theirs.

wage effect is mainly driven by high-quality firms. These estimated effects on employment and wages are in line with the predictions of the search model with sequential bargaining. While other imperfect competition models could potentially be enriched to explain the observed patterns, our findings are hard to reconcile with the neoclassical model of labor markets predominantly applied to evaluate the impact of payroll tax cuts.

Overall, our results highlight that at low-quality firms, the incidence of payroll tax cuts mainly falls on firms, while at high-quality firms, the incidence mainly falls on workers. Furthermore, universal tax cuts supporting all types of jobs and firms the same way could have some unintended consequences by creating bad jobs with little value for many workers. This aspect of payroll tax cuts should be considered in future evaluations of such policies.

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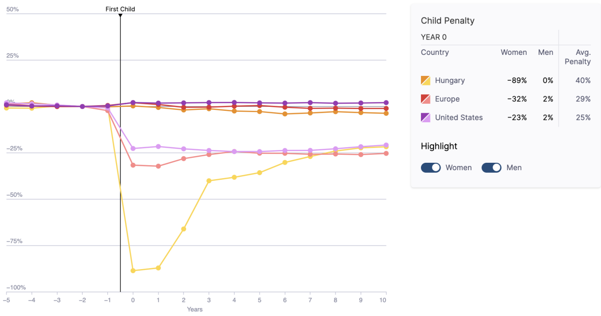
A Appendix for Chapter 1

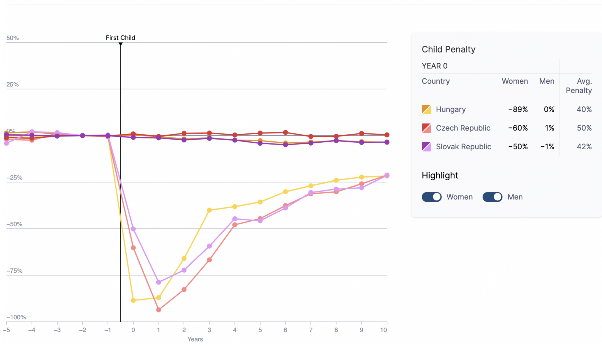
A.1 Additional figures and tables

Figure A.1.1: Child penalty in employment in Hungary and other countries

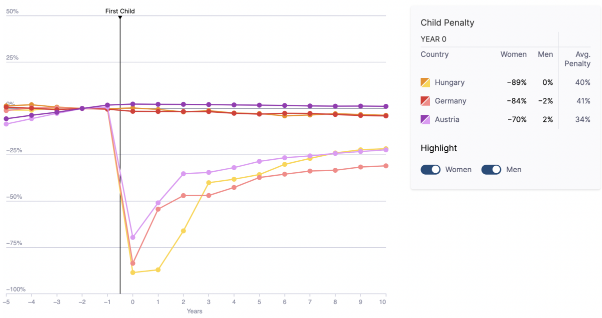
(a) Europe and United States



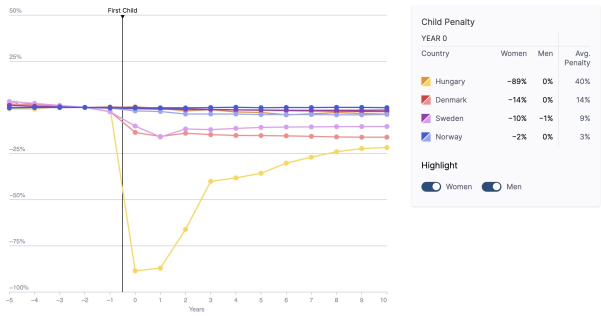
(b) Czechia and Slovak Republic



(c) Austria, Germany



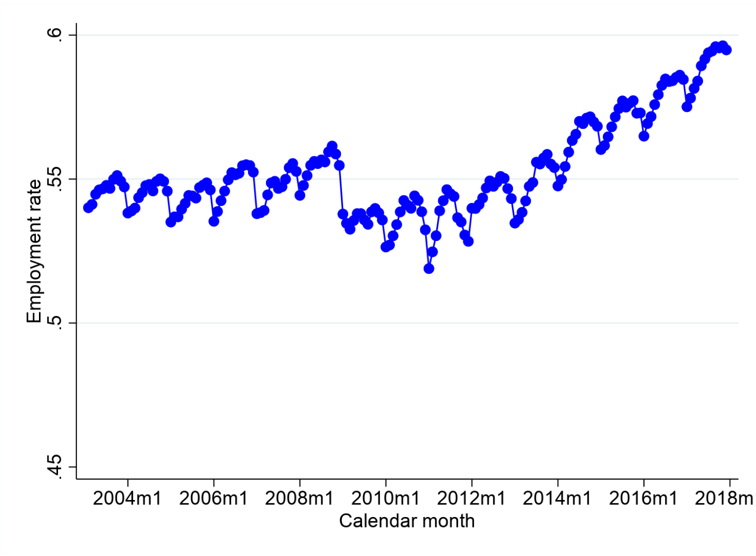
(d) Scandinavian countries



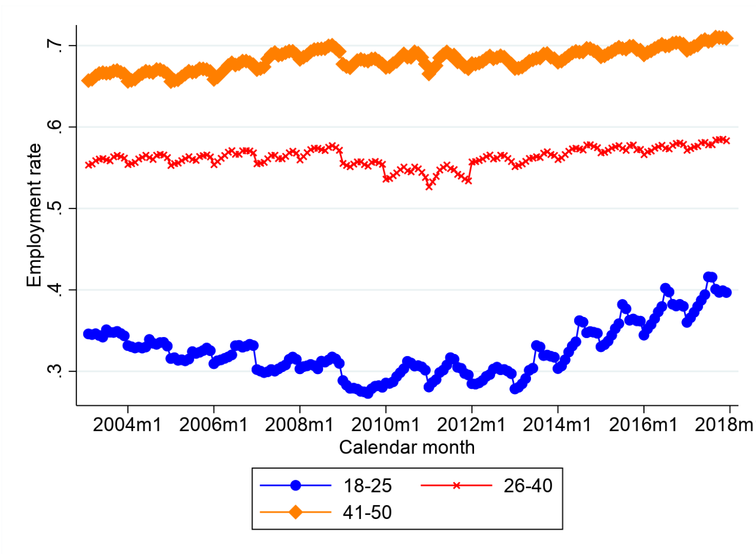
Note: Source: https://childpenaltyatlas.org/ (Last opened: December 14, 2023)

Figure A.1.2: Female employment trends

(a) Female employment rate (18-50)

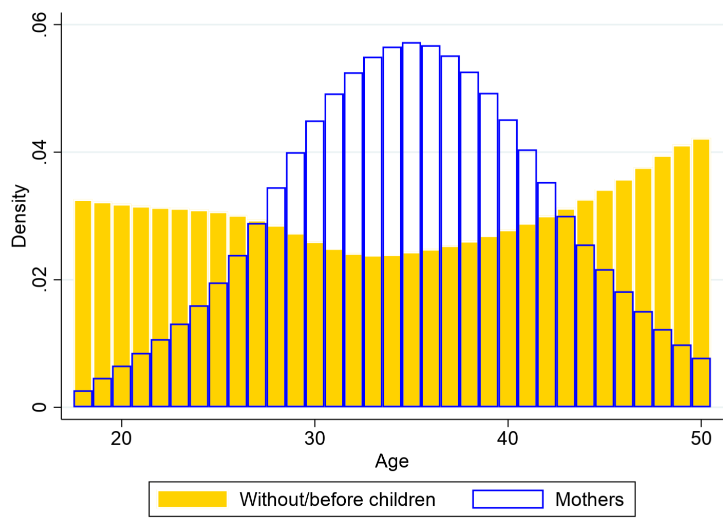


(b) Female employment rate by age groups



Note: Employment is defined monthly as earning wage or being on sick-leave

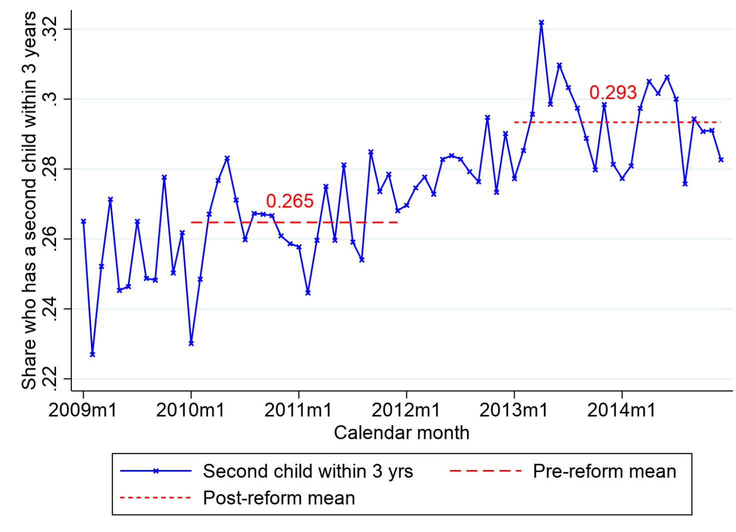
Figure A.1.3: Age distribution



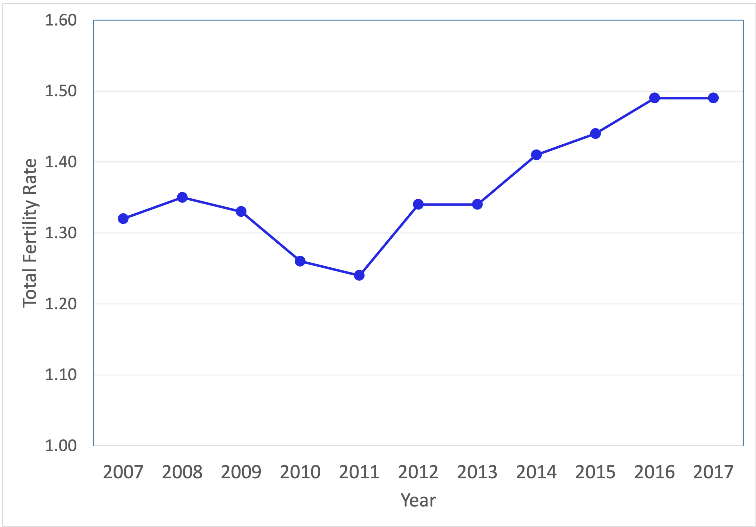
Note: Age distribution is shown for women without children and women with children calculated from the monthly Admin 3 data. Having a child is proxied by ever receiving child-related transfers.

Figure A.1.4: Fertility trends

(a) Share who has a second child within 3 years of giving birth to first child



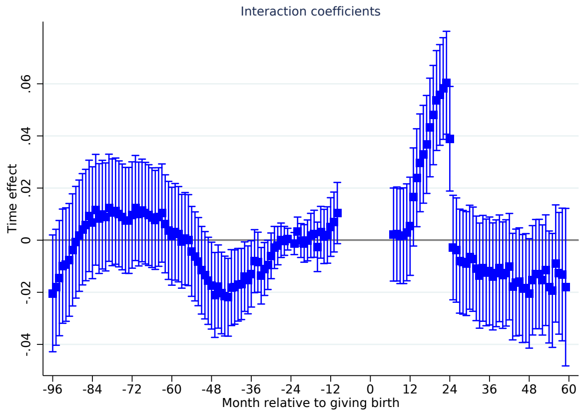
(b) Total Fertility Rate



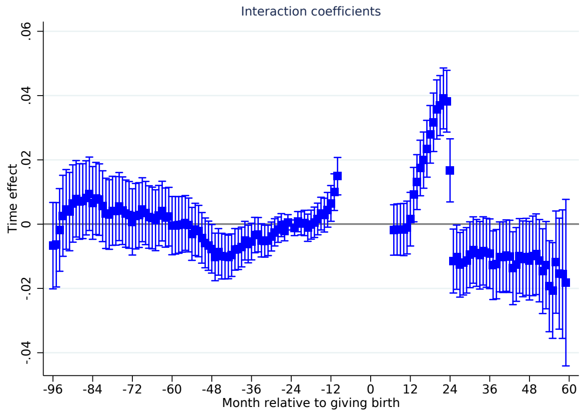
Note: the source for Panel (a) is the Admin 3 data for mothers in our estimation sample. The source for Panel (b) is the OECD Family Database

Figure A.1.5: Robustness estimations: changing the cohorts in the sample

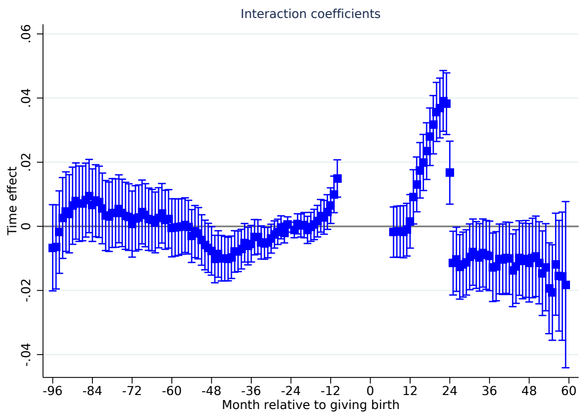
(a) 2011 vs. 2013



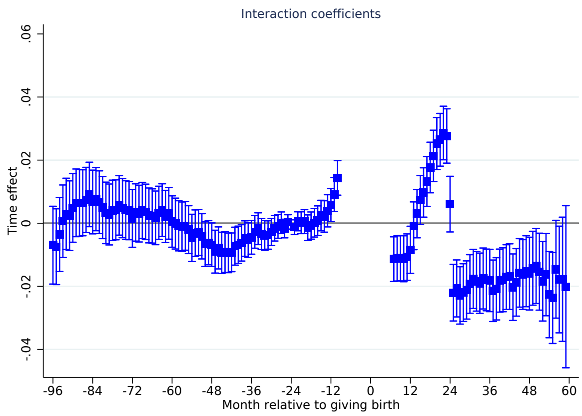
(b) 2010-11 vs. 2013-2015



(c) 2010-11 vs. 2013-2016



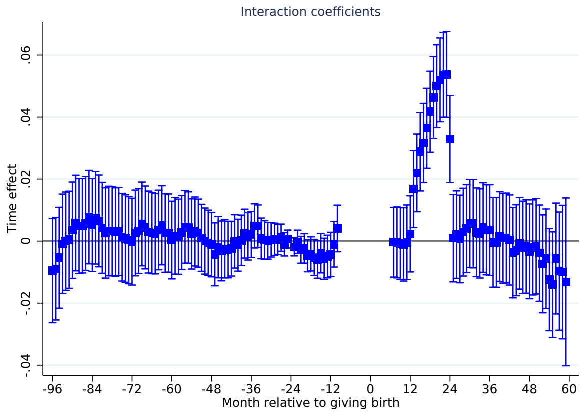
(d) 2010-11 vs. 2013-2017



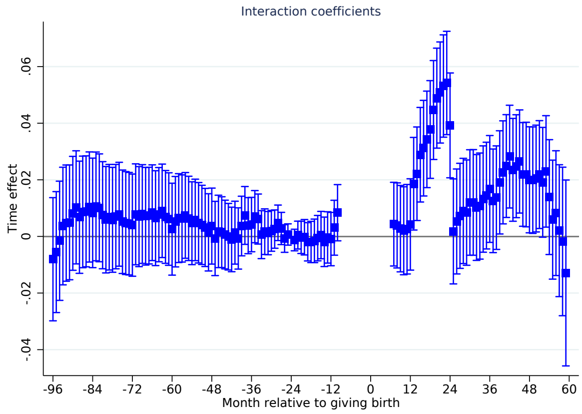
Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009. The plotted coefficients are $\hat{\delta}_t$ coefficients from equation 1.1. I modify the cohorts included in estimation across the different panels. Standard errors are clustered at the individual level.

Figure A.1.6: Robustness estimations: changing the sample

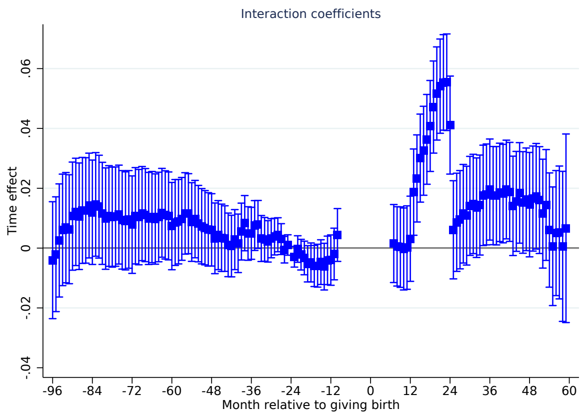
(a) First observed birth (without correction)



(b) Mothers with only 1 observed birth



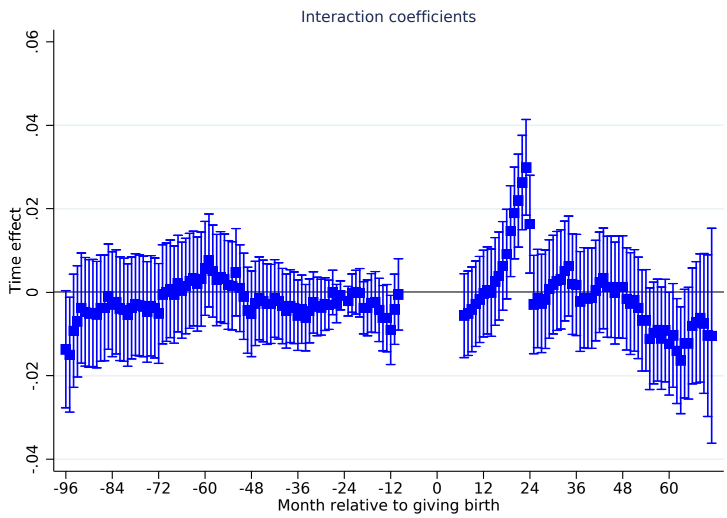
(c) Mothers with no second child within 3 years



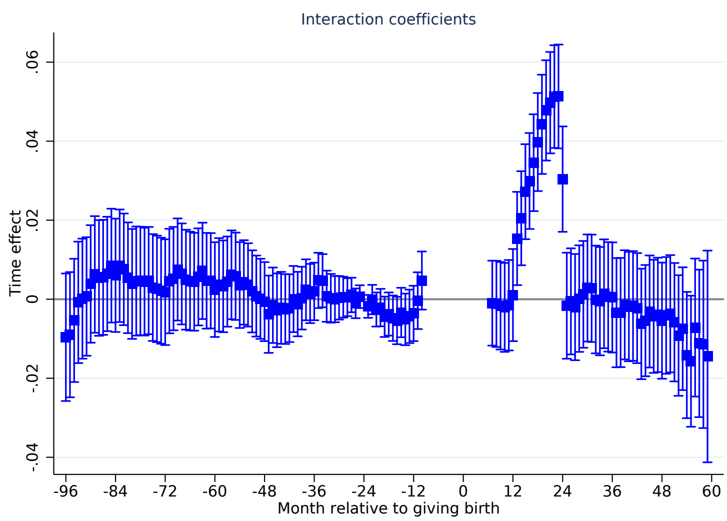
Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009 on panel (b) and panel (c). However, panel (a) plots the results without this restriction. Panel (b) shows the estimated effects for mothers for with one observed birth in the entire sample. Panel (c) shows the estimated effects for mothers who did not give birth to a second child within 3 years of their first birth. Standard errors are clustered at the individual level.

Figure A.1.7: Robustness estimations: including the cohort of 2012

(a) 2010-11 vs. 2012



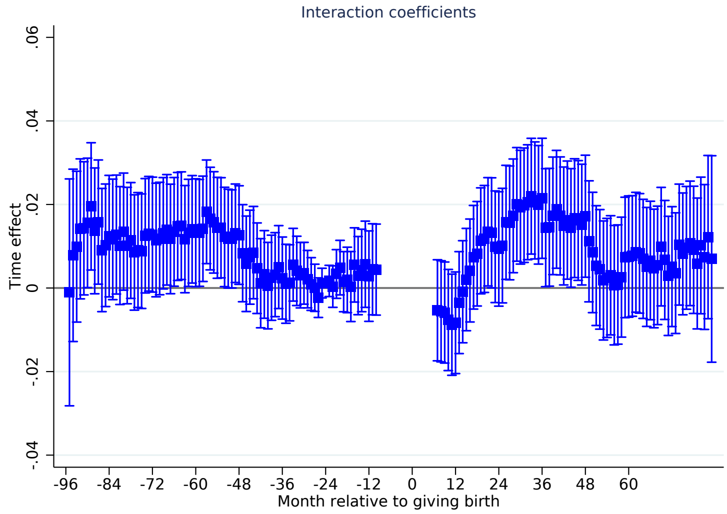
(b) 2010-11 vs. 2013-14



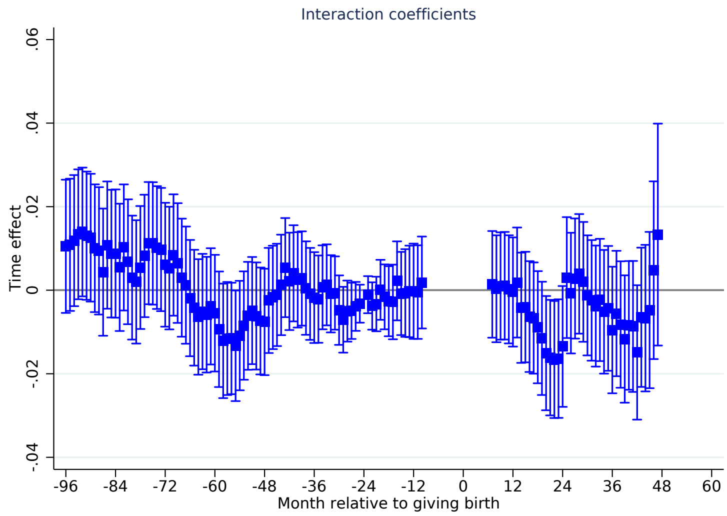
Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009. I estimate a modified version of equation 1.1, including the 2012 cohort of mothers and estimating treatment effects for them as well. Panel (a) plots the interaction coefficients for the 2012 cohort, and panel (b) plots the interaction coefficients for the 2013–14 cohorts. Standard errors are clustered at the individual level.

Figure A.1.8: Placebo estimates

(a) 2010 vs. 2011



(b) 2013 vs. 2014



Note: Employment is defined as earning a wage or being on sick-leave, but it excludes mothers who are on parental leave. Our sample consists of mothers who gave birth to their first child in either 2010–2011 (pre-reform cohorts) or 2013–2014 (post-reform cohorts). Since our data only includes births starting in 2009, I further restrict the sample to mothers who did not receive any child-related benefits prior to 2009. On Panel (a) cohort 2011 and on Panel (b) cohort 2014 is assigned to be the treated cohorts, while the control cohorts are 2010 and 2013, respectively. Standard errors are clustered at the individual level.

Table A.1.1: Descriptive statistics of mothers returning to work at different points

	(1) 1st year mean	(2) 2nd year mean	(3) 3rd year mean	(4) Later mean
Age at childbirth	29.77	29.33	30.05	27.74
Has second child within 3 years	0.27	0.20	0.19	0.36
Employment history				
Months employed in last 5 years	41.14	41.67	46.31	32.30
Wage relative to mean wage	1.00	1.11	0.91	0.73
Working time 1 year ago				
Unknown	0.09	0.08	0.04	0.12
Full-time	0.80	0.85	0.91	0.76
Part-time	0.10	0.08	0.05	0.12
Occupation 1 year ago				
No info	0.08	0.06	0.03	0.10
Manager, political/religional/ngo leader	0.08	0.06	0.05	0.04
Professional	0.21	0.26	0.24	0.12
Other white collar	0.34	0.37	0.41	0.30
Skilled blue collar	0.21	0.17	0.17	0.24
Assembler, machine op.	0.03	0.03	0.05	0.08
Unskilled laborer	0.06	0.05	0.04	0.12
Observations	2592	5490	13958	17144

Note: The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. Wage, occupation and working hours show the latest observed data during the 18th-24th months before giving birth. The wage is reported relative to the mean wage in Admin3.

A.2 Estimates on annual employment

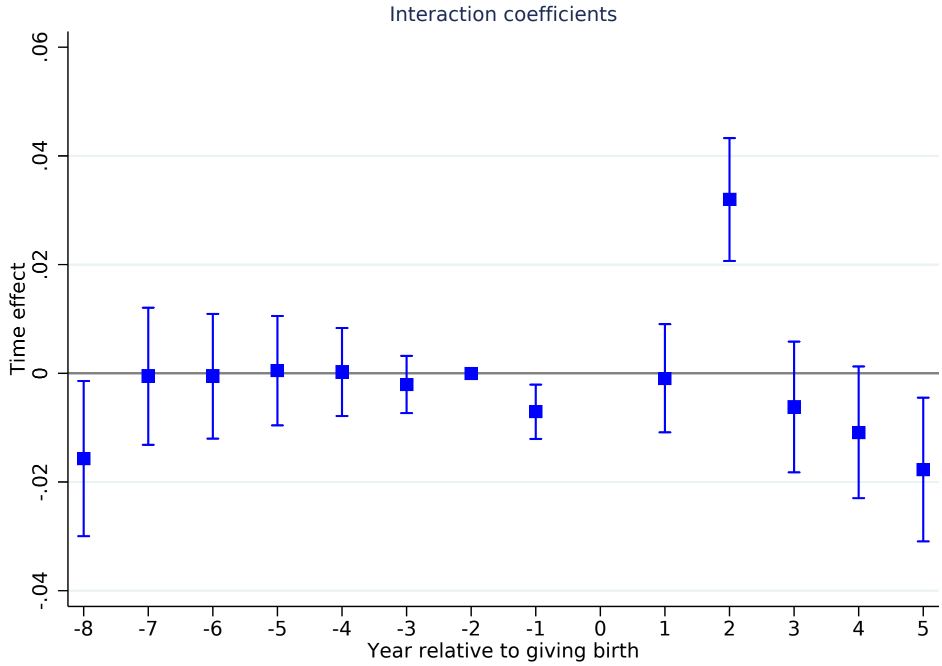
We also estimate a model on monthly data, but aggregating the event time to yearly level.

$$
\begin{aligned}
Emp_{itsj} = \alpha + \beta PostReform_i + \sum_{s=-8}^{5} \gamma_s D_{itsj}^{event} + \sum_{s=-8}^{5} \delta_s TreatedCohort_i \cdot D_{itsj}^{event} + \\
+ \phi \mathbf{D}_{\mathbf{itsj}}^{\mathbf{year}} + \zeta \mathbf{D}_{\mathbf{it}}^{\mathbf{Age}} + \xi \mathbf{Quarter}_{\mathbf{j}} + \varepsilon_{it}
\end{aligned}
\tag{A.2.1}
$$

where Emp_{itsj} is a binary indicator of the mother's employment i in calendar month j in month t relative to the month she gave birth and in year s relative to the year when she gave birth. s is 0 for the month of giving birth and the preceding 11 months, 1 for the first year following birth, 2 for the second year following giving birth, and so on. D_{itsj}^{event}, $s = -8, ..., 5$ are binary variables indicating whether a mother is in the -8th, ... , 5th year relative to the birth of their first children, and the baseline period is the 2nd year before childbirth, that is the 24th to 35th months before giving birth. Consequently, I now estimate yearly event time coefficients on the monthly data. The δ_s coefficients will show the average impact of the reform on monthly employment probabilities by year.

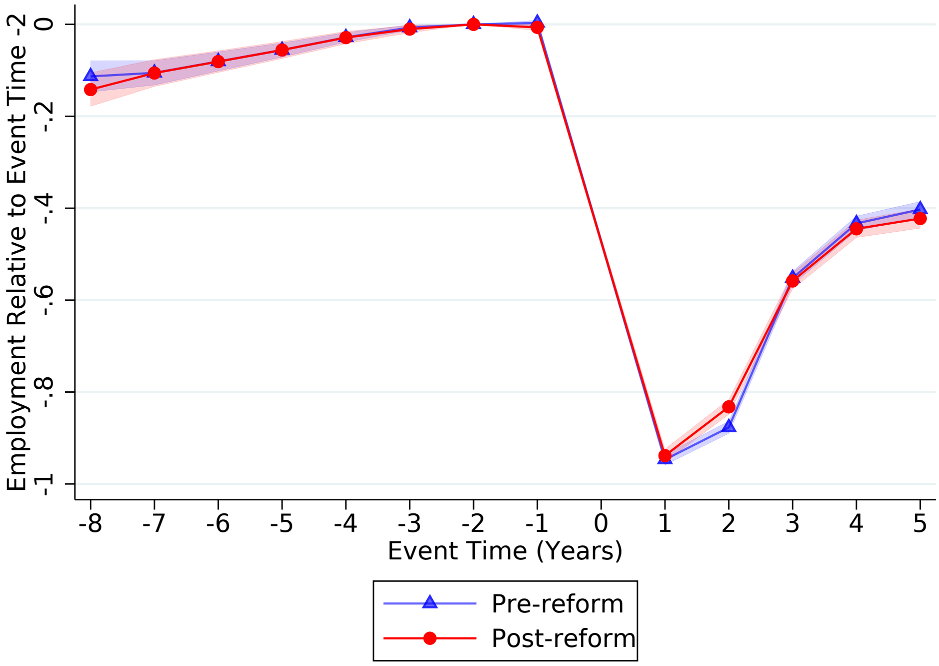
Figure A.2.9 shows the estimated $\hat{\delta_s}$ interaction coefficients form equation A.2.1. This figure shows a 3.2pp impact of the reform on the employment rates of mothers in the second year of their first child (baseline employment rate for cohorts 2010-2011: 9.8% leading to a 32% relative increase in employment). Now we see significant differences for the 8th year preceding childbirth and for the 5th year after childbirth. The decreasing employment of post-reform cohorts after the second year could be due to the increased fertility during this period. Figure A.2.10 shows the yearly version of Figure 1.3.

Figure A.2.9: Yearly event time treatment coefficients on monthly employment rates, for births in 2013-14 vs. in 2010-11



Note: Employment is defined monthly as earning wage or being on sick-leave, but it excludes mothers who are on parental leave. The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. The plotted coefficients are $\hat{\delta}_s$ coefficients from equation A.2.1. Standard errors are clustered at the individual level.

Figure A.2.10: Yearly event time coefficients relative to counterfactual employment in the absence children for pre- and post-reform cohorts



Note: Employment is defined monthly as earning wage or being on sick-leave, but it excludes mothers who are on parental leave. The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. I plot event time coefficients $\hat{\delta}_s$ from equation A.2.1 as fraction of counterfactual value absent children $P_t \equiv \hat{\delta}_s / E[\tilde{Y}_{itsj}|t]$, where $\tilde{Y}_{itsj}$ is the predicted value from equation A.2.1 without including the event time coefficients.

We present the estimated results for annual employment (defined as working at least for one month in a given year) and the number of months employed in a given year in panels (a) and (b) of Figure A.2.11, respectively. These models are an exact replication of the child penalty model of Kleven et al. (2019).

$$Emp_{isj} = \alpha + \beta PostReform_i + \sum_{s=-8}^{5} \gamma D^{event}_{isj} + \sum_{s=-8}^{5} \delta_t TreatedCohort_i \cdot D^{event}_{isj} + \\ + \mathbf{ŒD^{year}_{isj}} + \mathbf{1D^{Age}_{is}} + \varepsilon_{it} \tag{A.2.2}$$

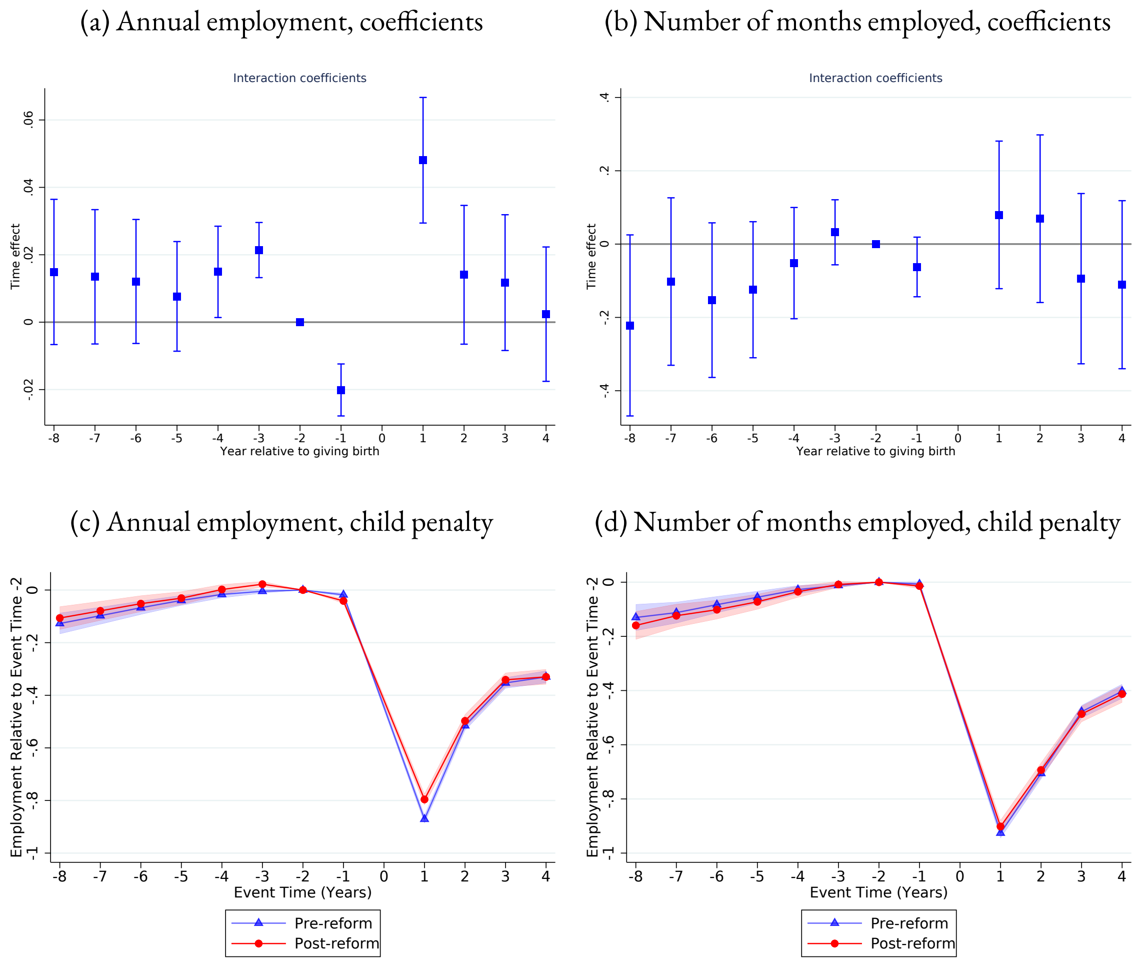
where Emp_{isj} is either a binary indicator of employment or the number of months employed for mother i in calendar year j in year s relative to the month she gave birth. s is 0 for the calendar year of giving birth. D^{event}_{isj}, $s = -8, ..., 5$ are binary variables indicating whether a mother is in the -8th, ... , 5th calendar year relative to the year of birth of their first children, and the baseline period is the 2nd year before childbirth.

This specification has the advantage of being directly comparable to the models estimated by Kleven et al. (2019). We can also gain insight into the impact of the intensive margin on employment by analyzing the impact on the number of months employed. However, a disadvantage of this specification in our case is that we cannot precisely estimate the impact during the period when the reform offered a monetary incentive to work, as the affected months (13th to 24th) are distributed between the first and second years after childbirth, depending on the exact childbirth date.

We observe a positive impact on annual employment in the first year after childbirth, which is 4.8 percentage points (Panel (a) of Appendix Figure A.2.11). This indicates that the likelihood of working at least one month during the calendar year after childbirth increased by 4.8 percentage points (pp) compared to a baseline of 11.5%, resulting in a 42% relative increase. However, when I analyze annual employment, I find significant pre-trends up to three years before the year of childbirth.

Looking at the number of months employed by calendar years in Panel (b) of Appendix Figure A.2.11, I find no significant impact, only a small, insignificant increase in the first and second years after childbirth.

Figure A.2.11: Estimates on annual employment



Note: These models are estimated on yearly data collapsed by calendar years relative to the year of giving birth. Annual employment on panel (a) is defined as working for at least one month in a calendar year. The sample consists of mothers who gave birth to their first child during 2010-2011 (pre-reform cohorts) or 2013-2014 (post-reform cohorts). Since we observe births only starting from 2009 I further restrict the sample for mothers who have not received any child-related benefits before 2009 since 2003, the first year in our data. The plotted coefficients are δ_s coefficients from equation A.2.2. Panel (c) and (d) show the child penalty plots corresponding to the outcome variables shown in panel (a) and (b). For details, see notes under Figure A.2.10. Standard errors are clustered at the individual level.

B Appendix for Chapter 2

B.1 Additional figures and tables

Table B.1.1: Impact on job finding probabilities, difference-in-differences regression estimates

	A. Job finding within ... days							
	(1) finds job in 52 days	(2) finds job in 97 days	(3) finds job in 142 days	(4) finds job in 187 days	(5) finds job in 232 days	(6) finds job in 277 days	(7) finds job in 322 days	(8) finds job in 367 days
April-August	0.005	0.007	-0.015	-0.039**	-0.055***	-0.050***	-0.043**	-0.023
	(0.012)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)
Reform year	-0.010	0.008	-0.001	-0.002	-0.001	-0.009	-0.004	-0.007
	(0.012)	(0.015)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
April-August * Reform year	0.031*	-0.000	-0.003	0.019	0.027	0.035	0.026	0.013
	(0.018)	(0.022)	(0.024)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.147	0.294	0.400	0.477	0.534	0.588	0.626	0.666
	B. Job finding during days ...							
	(1) finds job during days 45-52	(2) finds job during days 90-97	(3) finds job during days 135-142	(4) finds job during days 180-187	(5) finds job during days 225-232	(6) finds job during days 270-277	(7) finds job during days 315-322	(8) finds job during days 360-367
April-August	-0.001	0.004	-0.005	-0.008*	-0.006**	0.003	0.004	0.002
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
Reform year	-0.001	-0.001	0.001	-0.005	-0.002	-0.001	0.003	0.000
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
April-August * Reform year	0.018**	-0.015*	-0.004	0.009	0.008*	-0.002	-0.003	-0.001
	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.021	0.031	0.014	0.016	0.008	0.012	0.005	0.005

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is finding a job within x days in panel A. and finding a job during the week ending with day x in panel B., where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.2: Impact on job finding probabilities at a new employer, difference-in-differences regression estimates

	A. Job finding at a new employer within ... days							
	(1) finds job in 52 days	(2) finds job in 97 days	(3) finds job in 142 days	(4) finds job in 187 days	(5) finds job in 232 days	(6) finds job in 277 days	(7) finds job in 322 days	(8) finds job in 367 days
April-August	0.002	0.006	0.002	-0.011	-0.026	-0.025	-0.019	-0.001
	(0.012)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)
Reform year	-0.010	0.001	-0.000	-0.005	-0.015	-0.023	-0.021	-0.023
	(0.011)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
April-August * Reform year	0.037**	0.012	0.003	0.029	0.051**	0.067***	0.064***	0.051**
	(0.017)	(0.021)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.125	0.247	0.328	0.386	0.434	0.481	0.513	0.549
	B. Job finding at a new employer during days ...							
	(1) finds job during days 45-52	(2) finds job during days 90-97	(3) finds job during days 135-142	(4) finds job during days 180-187	(5) finds job during days 225-232	(6) finds job during days 270-277	(7) finds job during days 315-322	(8) finds job during days 360-367
April-August	-0.004	0.004	-0.005	-0.004	-0.005*	-0.000	0.003	-0.000
	(0.005)	(0.006)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Reform year	-0.002	-0.000	0.001	-0.003	-0.002	-0.003	0.002	-0.000
	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
April-August * Reform year	0.021***	-0.014*	-0.001	0.004	0.007	0.002	-0.002	-0.000
	(0.007)	(0.008)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.017	0.026	0.010	0.012	0.006	0.010	0.004	0.004

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is finding a job at a new employer within x days in panel A. and finding a job at a new employer during the week ending with day x in panel B., where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.3: Impact on returning probabilities to the previous employer, difference-in-differences regression estimates

	B. Returning to previous employer during days ...							
	(1) returns in 52 days	(2) returns in 97 days	(3) returns in 142 days	(4) returns in 187 days	(5) returns in 232 days	(6) returns in 277 days	(7) returns in 322 days	(8) returns in 367 days
April-August	0.003	0.001	-0.017**	-0.029***	-0.029***	-0.026**	-0.024**	-0.021**
	(0.005)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Reform year	0.000	0.007	-0.000	0.003	0.014	0.014	0.017	0.016
	(0.005)	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
April-August * Reform year	-0.006	-0.012	-0.007	-0.010	-0.024*	-0.032**	-0.038***	-0.039***
	(0.007)	(0.010)	(0.012)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.022	0.048	0.073	0.091	0.099	0.107	0.113	0.117

	B. Returning to previous employer during days ...							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	returns during days 45-52	returns during days 90-97	returns during days 135-142	returns during days 180-187	returns during days 225-232	returns during days 270-277	returns during days 315-322	returns during days 360-367
April-August	0.003	0.000	0.000	-0.004**	-0.001	0.003*	0.001	0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Reform year	0.001	-0.001	-0.001	-0.002	-0.001	0.002	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
April-August * Reform year	-0.003	-0.001	-0.003	0.005*	0.001	-0.004*	-0.002	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	6,433	6,433	6,433	6,433	6,433	6,433	6,433	6,433
Mean outcome	0.004	0.005	0.004	0.003	0.001	0.002	0.001	0.001

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is returning to the previous employer within x days in panel A. and returning to the previous employer during the week ending with day x in panel B., where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.4: Impact on job finding probabilities, triple difference-in-differences regression estimates

	A. Job finding within ... days							
	(1) finds job in 52 days	(2) finds job in 97 days	(3) finds job in 142 days	(4) finds job in 187 days	(5) finds job in 232 days	(6) finds job in 277 days	(7) finds job in 322 days	(8) finds job in 367 days
April-August	0.079***	0.034***	-0.048***	-0.084***	-0.085***	-0.070***	-0.054***	-0.048***
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Reform year	-0.003	-0.006	-0.026***	-0.026***	-0.025***	-0.021***	-0.017***	-0.017***
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
April-August * Reform year	-0.006	-0.003	0.025***	0.040***	0.040***	0.027***	0.020**	0.012
	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
SBU-taker	-0.008	-0.023**	-0.047***	-0.038***	-0.019	-0.005	0.002	0.001
	(0.009)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Apr-Aug * SBU-taker	-0.073***	-0.023	0.039**	0.051***	0.037**	0.026	0.017	0.031*
	(0.013)	(0.017)	(0.018)	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)
Reform y. * SBU-taker	-0.008	0.015	0.028	0.026	0.027	0.017	0.017	0.013
	(0.013)	(0.016)	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)
Triple interaction	0.037*	0.005	-0.027	-0.019	-0.013	0.008	0.007	0.001
	(0.019)	(0.024)	(0.026)	(0.026)	(0.026)	(0.026)	(0.025)	(0.025)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.177	0.320	0.437	0.510	0.561	0.609	0.643	0.675

	B. Job finding during days ...							
	(1) finds job during days 45-52	(2) finds job during days 90-97	(3) finds job during days 135-142	(4) finds job during days 180-187	(5) finds job during days 225-232	(6) finds job during days 270-277	(7) finds job during days 315-322	(8) finds job during days 360-367
April-August	0.002	-0.014***	-0.008***	-0.005***	0.000	0.007***	0.002**	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Reform year	-0.001	-0.005**	0.002	-0.000	0.000	0.002	0.001	-0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
April-August * Reform year	0.002	0.002	-0.000	0.002	-0.001	-0.007***	-0.002	-0.003*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
SBU-taker	0.004	-0.007	0.002	0.005	0.005*	0.003	-0.001	-0.011***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.002)
Apr-Aug * SBU-taker	-0.003	0.019***	0.003	-0.003	-0.006**	-0.004	0.001	0.007**
	(0.005)	(0.007)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Reform y. * SBU-taker	0.000	0.005	-0.001	-0.005	-0.003	-0.003	0.002	0.003
	(0.005)	(0.006)	(0.005)	(0.005)	(0.003)	(0.004)	(0.002)	(0.003)
Triple interaction	0.016**	-0.016*	-0.004	0.008	0.009*	0.005	-0.002	0.002
	(0.008)	(0.009)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.018	0.030	0.014	0.014	0.007	0.011	0.005	0.010

Note: β_i, $i = 1, 2, ..., 7$, coefficients of regressions of the form of Equation 2.2 are reported, where the dependent variable is job finding within x days in panel A. and job finding during the week ending with day x in panel B, where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** $p<0.001$, ** $p<0.01$, * $p<0.05$. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.5: Impact on job finding probabilities at a new employer, triple difference-in-differences regression estimates

	A. Job finding at a new employer within ... days							
	(1) finds job in 52 days	(2) finds job in 97 days	(3) finds job in 142 days	(4) finds job in 187 days	(5) finds job in 232 days	(6) finds job in 277 days	(7) finds job in 322 days	(8) finds job in 367 days
April-August	0.074***	0.077***	0.058***	0.044***	0.042***	0.049***	0.059***	0.066***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Reform year	0.000	0.002	-0.009*	-0.012**	-0.013**	-0.011*	-0.008	-0.004
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
April-August * Reform year	0.000	-0.003	0.013	0.025***	0.029***	0.025***	0.022**	0.014
	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
SBU-taker	0.001	0.030***	0.041***	0.051***	0.070***	0.085***	0.091***	0.097***
	(0.008)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
Apr-Aug * SBU-taker	-0.073***	-0.069***	-0.052***	-0.050***	-0.063***	-0.068***	-0.073***	-0.063***
	(0.013)	(0.016)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Reform y. * SBU-taker	-0.012	0.000	0.011	0.009	0.001	-0.008	-0.010	-0.017
	(0.012)	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Triple interaction	0.037**	0.015	-0.009	0.005	0.021	0.040	0.042	0.037
	(0.018)	(0.023)	(0.024)	(0.025)	(0.026)	(0.026)	(0.026)	(0.025)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.145	0.235	0.302	0.353	0.393	0.430	0.458	0.481

	B. Job finding at a new employer during days ...							
	(1) finds job during days 45-52	(2) finds job during days 90-97	(3) finds job during days 135-142	(4) finds job during days 180-187	(5) finds job during days 225-232	(6) finds job during days 270-277	(7) finds job during days 315-322	(8) finds job during days 360-367
April-August	0.004**	-0.002	-0.002*	-0.001	-0.000	0.003**	0.001	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reform year	-0.000	-0.002	-0.000	-0.000	0.000	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
April-August * Reform year	0.002	-0.000	0.001	0.001	-0.000	-0.004**	-0.001	-0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
SBU-taker	0.006*	0.006	0.004	0.004	0.004	0.004	-0.001	-0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
Apr-Aug * SBU-taker	-0.008*	0.007	-0.003	-0.003	-0.005	-0.003	0.002	-0.000
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)	(0.002)
Reform y. * SBU-taker	-0.001	0.002	0.002	-0.003	-0.002	-0.003	0.002	-0.001
	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Triple interaction	0.020***	-0.013*	-0.002	0.004	0.007	0.005	-0.001	0.004
	(0.008)	(0.008)	(0.005)	(0.006)	(0.004)	(0.005)	(0.004)	(0.003)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.014	0.017	0.008	0.010	0.006	0.008	0.004	0.005

Note: β_i, $i = 1, 2, ..., 7$, coefficients of regressions of the form of Equation 2.2 are reported, where the dependent variable is job finding at a new employer within x days in panel A. and job finding at a new employer during the week ending with day x in panel B, where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.6: Impact on returning probabilities to the previous employer, triple difference-in-differences regression estimates

	B. Returning to previous employer during days ...							
	(1) returns in 52 days	(2) returns in 97 days	(3) returns in 142 days	(4) returns in 187 days	(5) returns in 232 days	(6) returns in 277 days	(7) returns in 322 days	(8) returns in 367 days
April-August	0.005*	-0.043***	-0.106***	-0.128***	-0.128***	-0.118***	-0.113***	-0.115***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Reform year	-0.003	-0.008**	-0.017***	-0.014***	-0.012**	-0.010**	-0.010**	-0.013***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
April-August * Reform year	-0.006*	-0.001	0.012**	0.015**	0.011*	0.002	-0.002	-0.002
	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
SBU-taker	-0.009**	-0.053***	-0.088***	-0.090***	-0.090***	-0.090***	-0.089***	-0.097***
	(0.004)	(0.006)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Apr-Aug * SBU-taker	-0.001	0.046***	0.090***	0.100***	0.100***	0.094***	0.090***	0.094***
	(0.006)	(0.008)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
Reform y. * SBU-taker	0.003	0.015*	0.017*	0.017	0.026**	0.025**	0.027**	0.029**
	(0.005)	(0.008)	(0.010)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
Triple interaction	-0.000	-0.011	-0.017	-0.024*	-0.035**	-0.033**	-0.035**	-0.036**
	(0.008)	(0.011)	(0.013)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.032	0.085	0.136	0.157	0.168	0.179	0.186	0.194

	B. Returning to previous employer during days ...							
	(1) returns during days 45-52	(2) returns during days 90-97	(3) returns during days 135-142	(4) returns during days 180-187	(5) returns during days 225-232	(6) returns during days 270-277	(7) returns during days 315-322	(8) returns during days 360-367
April-August	-0.002**	-0.012***	-0.005***	-0.004***	0.000	0.004***	0.001**	-0.005***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Reform year	-0.001	-0.004**	0.002	-0.000	0.000	0.001	0.000	-0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
April-August * Reform year	0.000	0.002	-0.001	0.001	-0.001	-0.003***	-0.001*	0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
SBU-taker	-0.002	-0.013***	-0.002	0.001	0.001	-0.001	0.000	-0.009***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Apr-Aug * SBU-taker	0.005**	0.012***	0.005**	-0.000	-0.002	-0.001	-0.000	0.007***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Reform y. * SBU-taker	0.001	0.003	-0.002	-0.002	-0.001	0.001	0.000	0.004***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Triple interaction	-0.003	-0.003	-0.002	0.004	0.001	-0.000	-0.001	-0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	48,939	48,939	48,939	48,939	48,939	48,939	48,939	48,939
Mean outcome	0.004	0.014	0.005	0.004	0.001	0.003	0.001	0.005

Note: β_i, $i = 1, 2, ..., 7$, coefficients of regressions of the form of Equation 2.2 are reported, where the dependent variable is returning to the previous employer within x days in panel A. and returning to the previous employer during the week ending with day x in panel B, where x = 52, 97, ..., 367. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

Table B.1.7: Robustness of estimates on job finding within 52 days, during the 46-52nd days and the 91-97th days

A. Job finding within 52 days

VARIABLES	(1) Baseline	(2) W/o July 1 2006	(3) No controls	(4) Age control	(5) Health control	(6) Emp. history control	(7) Logit	(8) 4 m
April-August	0.005	0.000	0.004	0.006	0.006	0.005	0.006	-0.00622
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.0139)
Reform year	-0.010	-0.010	-0.009	-0.008	-0.010	-0.008	-0.011	-0.00119
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.0142)
April-August * Reform year	0.031*	0.036**	0.037**	0.033*	0.034*	0.030*	0.030*	0.0251
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.0203)
Observations	6,433	6,229	6,600	6,467	6,467	6,433	6,433	5,001
Age and reg. unemp.	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Health controls	Yes	Yes			Yes		Yes	Yes
Emp. history controls	Yes	Yes				Yes	Yes	Yes

B. Job finding during 46-52nd days

VARIABLES	(1) Baseline	(2) W/o July 1 2006	(3) No controls	(4) Age control	(5) Health control	(6) Emp. history control	(7) Logit	(8) 4 m
April-August	-0.001	-0.004	-0.002	-0.001	-0.001	-0.001	-0.001	-0.00147
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.00583)
Reform year	-0.001	-0.001	0.001	-0.001	-0.001	-0.001	-0.001	0.00140
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.00609)
April-August * Reform year	0.018**	0.021***	0.017**	0.019**	0.019**	0.018**	0.016**	0.0128
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.00903)
Observations	6,433	6,229	6,600	6,467	6,467	6,433	6,430	5,001
Age and reg. unemp.	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Health controls	Yes	Yes			Yes		Yes	Yes
Emp. history controls	Yes	Yes				Yes	Yes	Yes

C. Job finding during 91-97th days

VARIABLES	(1) Baseline	(2) W/o July 1 2006	(3) No controls	(4) Age control	(5) Health control	(6) Emp. history control	(7) Logit	(8) 4 m
April-August	0.004	0.003	0.004	0.004	0.004	0.004	0.004	0.00750
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.00686)
Reform year	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	0.00610
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.00675)
April-August * Reform year	-0.015*	-0.013	-0.014*	-0.014*	-0.014*	-0.014*	-0.016*	-0.0192**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.00951)
Observations	6,433	6,229	6,600	6,467	6,467	6,433	6,433	5,001
Age and reg. unemp.	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Health controls	Yes	Yes			Yes		Yes	Yes
Emp. history controls	Yes	Yes				Yes	Yes	Yes

Note: β_1, β_2 and β_3 coefficients of regressions of the form of Equation 2.1 are reported, where the dependent variable is job finding within 52 days in panel A., job finding during the 46th-52nd days and job finding during the 91st-97th days. Standard errors are clustered on the individual level and are shown in parentheses.*** p<0.001, ** p<0.01, * p<0.05. Column (1) shows our baseline estimates. Each other column presents a robustness estimate: in column (2) we leave out July 1 2006, when several mass layoffs happen; in columns (3)-(6) we include different sets of control variables. In column (7) we use logit estimation method. In column (8) we estimate on a narrower time period, in 4-month window around the reform date. Control variables: a binary indicator of public sector employer, occupation categories (6 groups), continuous employment history, sickness benefit base amount that is a function of previous daily earnings, the number of sick days last year, binary indicators for in- and outpatient care last year, age and regional unemployment.

B.2 Types of job endings and laws about lay off and sick-leave

Unfortunately, we cannot see the reason for a job ending in our data. Here we provide a list of the possible types of job endings based on the Hungarian Labor Code and summarize whether a job can be terminated in that way while someone is on sick-leave, and whether the employee is eligible for SBU and or UIB after such a job ending in Appendix Table B.2.8.

Types of job endings:

- **Mutual agreement** between the employer and the employee,
- **Ordinary termination**, period of notice is min. 30 days and increasing with tenure, half of the time exempted from work,
- **Voluntary quit**
- **Extraordinary termination**, if employee breaches obligations
- (With immediate effect during the **probationary period** – excluded from data)

Table B.2.8: Rules for job endings and SBU and UIB eligibility

Type of job ending	Allowed during sick-leave	SBU eligibility	UIB eligibility
Mutual agreement	Yes	Yes	Yes
Ordinary termination	NO	Yes	Yes
Voluntary quit	Yes	Yes	NO
Extraordinary termination	Yes	Yes	NO

The only data we have about the frequency of different types of job endings is available from the unemployment register of 2009. In Appendix Table B.2.9, we show the distribution of different kinds of job endings for the unemployment insurance benefit recipients of 2009. Since, the waiting period for UIB take-up is 90 days for voluntary job quitters, we show the frequencies separately for unemployed who claimed benefits within 90 days and later than 90 days to see if claiming later than 90 days is likely due to the waiting period for voluntary quitters.

The majority of employment spells end with mutual agreement in both groups (55% and 63%). Ordinary notice of termination by the employer happens in 13% of the cases. A big portion of employment spells end because it is the end of a fixed-term contract (22% an 8%). We can see that ordinary notice of termination by the employee (or in other words voluntary quits) does happen more frequently among benefit claimants over 90 days from job loss, but it is still just 5% of the cases for them, so it is unlikely that SBU claiming behaviour would be driven by voluntary quitters.

Since this table only includes unemployment benefit recipients it is not representative of our sample that includes all job endings and it is unclear how sick-leave and SBU take-up interacts with the types of job endings.

Table B.2.9: Types of job endings for the sample of UIB recipients in 2009

	Time since last job ending at UI spell start			
	<= 90		> 90	
Type of job ending	Freq.	Percent	Freq.	Percent
Mutual agreement	135,542	55.94	29,972	62.99
End of fixed-term contract	52,239	21.56	3,832	8.05
Ordinary notice of termination by employer	30,487	12.58	6,171	12.97
Employer's termination during the probationary period	10,203	4.21	1,884	3.96
Ordinary notice of termination by employee	3,614	1.49	2,392	5.03
Employee's termination during the probationary period	2,971	1.23	786	1.65
Extraordinary notice of termination by employer	2,383	0.98	1,323	2.78
Entrepreneurship termination	1,832	0.76	420	0.88
Other, no information	1,557	0.64	251	0.53
Extraordinary notice of termination by employee	1,081	0.45	288	0.61
Closure of employer	346	0.14	238	0.5
Pension	27	0.01	23	0.05
Never had employment contract	3	0	0	0
Total	242,285	100	47,580	100

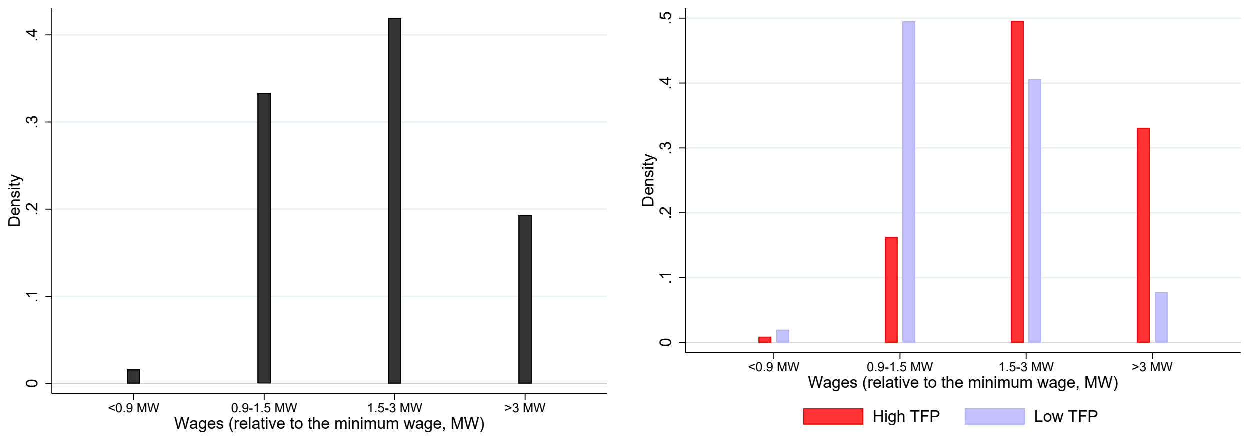
Note: Data source is the unemployment registry for 2009, Admin 2 data

C Appendix for Chapter 3

C.1 Additional figures and tables

Figure C.1.1: The wage distribution of private sector workers

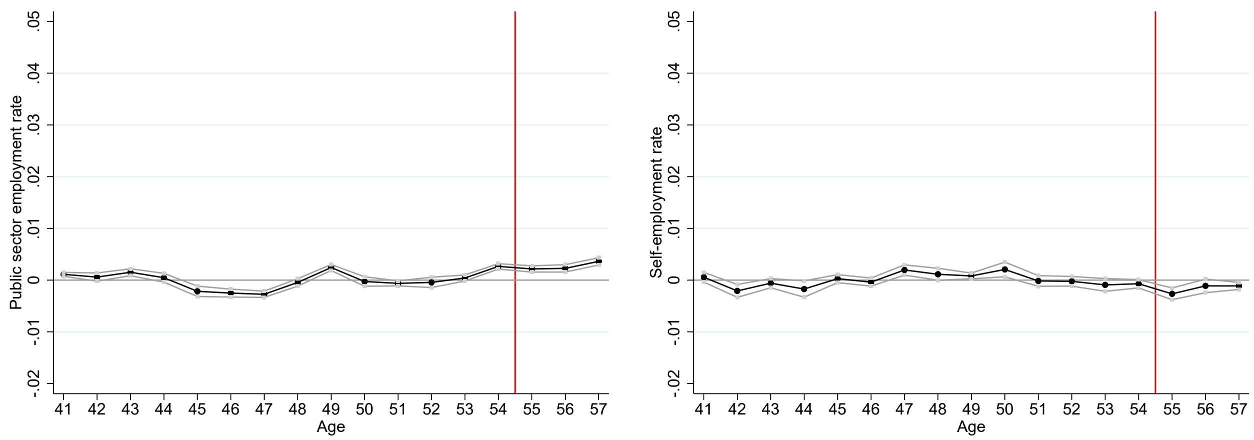
(a) All (b) By TFP



Note: Figure shows the density of workers aged 52-57 working at private sector companies (our main sample). We plot wage categories relative to the minimum wage. Panel (a) shows the distribution at all private sector firms. Panel (b) shows the distribution separately for workers at high-productivity (above-median TFP) firms (in red) and at low-productivity (below-median TFP) firms (in blue).

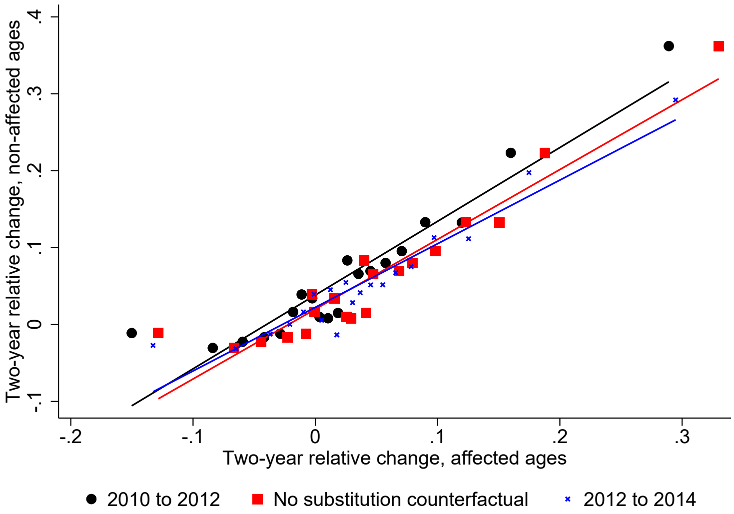
Figure C.1.2: Change in employment in sectors unaffected by the tax cut

(a) Public sector employment rate (b) Self-employment rate



Note: Figure shows the public sector employment rate in Panel (a) and the self-employment rate in Panel (b) by age before and after the introduction of the age-specific payroll tax cut affecting only private sector firms. The figure shows the difference in employment rates between years 2012 and 2013-2015 relative to the average change between ages 41 and 54, with the 95% confidence interval (standard errors clustered at the age × period level). The vertical red line shows the age threshold where the tax cut was effective for private sector workers. At the same time, nothing was changed at that age threshold for public sector workers or the self-employed.

Figure C.1.3: Relationship between firm-Level employment change in affected age groups and non-affected age groups

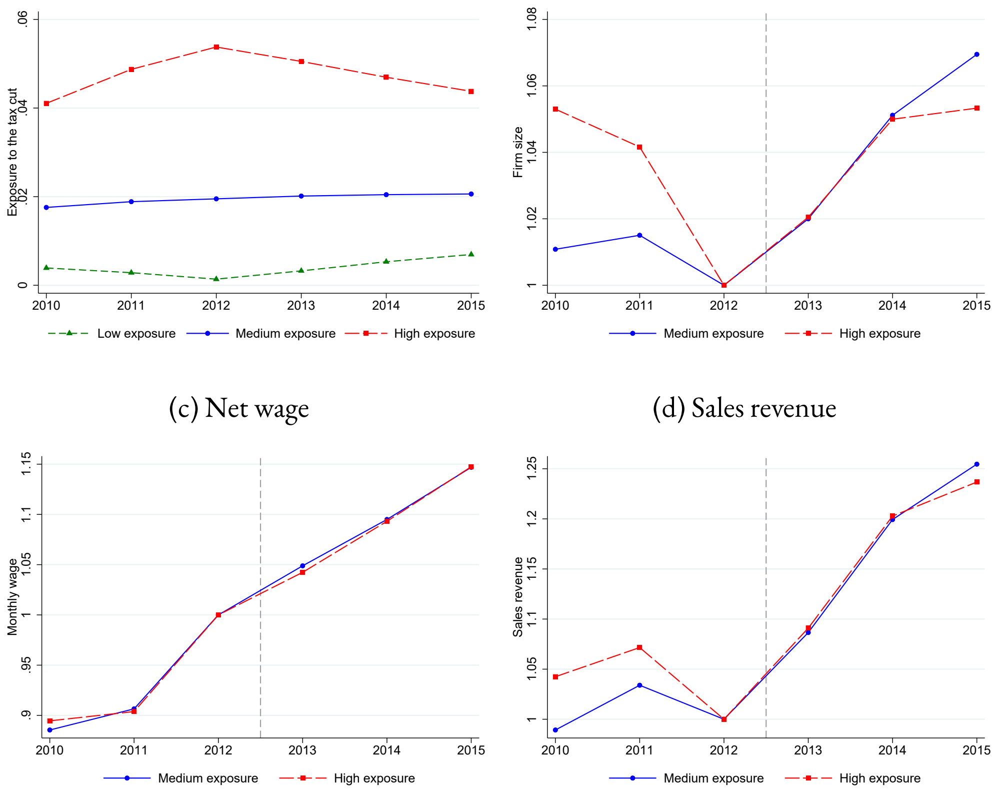


Note: Figure shows the relationship between firm-level two-year employment change in affected age groups and non-affected age groups before the introduction of the payroll tax cut (2010 to 2012, in black) and after the introduction of the payroll tax cut (2012 to 2014, in blue). On the x-axis, we indicate the two-year change from year t to year $t+2$ in the number of workers aged up to 24 or at least 55 (affected ages) relative to the observed firm size in year t. On the y-axis, we indicate the same two-year relative change in the number of workers aged 25-54 (unaffacted ages). We exclude firms with less than 10 workers and firms that are not in the sample throughout years 2010-2014. We show a binned scatterplot of the observations with a linear fitted regression line. The black dots and line refer to relative change from 2010 to 2012 (i.e., before the introduction of the tax cut). The blue dots and line refer to relative change from 2012 to 2014 (with the tax cut being introduced in 2013). The red squares and line correspond to a counterfactual scenario where we increase the 2012 employment in the affected age groups by 14.7%, which is the average firm-level employment change from 2012 to 2014, while employment changes in the unaffected ages are left at their 2010 to 2012 values. This later estimate, therefore, shows the relationship that would emerge if the 2010-2012 employment in the affected age groups increased as estimated, and firms did not substitute unaffected workers with affected workers by cutting their employment.

Figure C.1.4: Firm-level effects of payroll tax cuts

(a) Exposure to the tax cut (b) Firm size

(c) Net wage (d) Sales revenue



Note: Figure replicates the basic results of Saez et al. (2019). Using 2012 data, we calculate the firm-level exposure to the tax cut defined as the total tax cut based on workers aged 55 and above at the firm relative to the total payroll of the firm. We calculate the quartiles of the exposure, excluding firms with zero exposure, and group firms into three categories. "Low exposure" firms have either zero tax cut or belong to the bottom quartile. "Medium exposure" firms belong to the middle two quartiles. "High exposure" firms belong to the the top quartile. We compare the evolution of various outcomes of the firms in these groups, focusing on the medium and high exposure groups. Panel (a) shows the average exposure to the tax cut. Panel (b) shows firm size. Panel (c) shows average net wage. Panel (d) shows sales revenue.

Table C.1.1: Summary of empirical studies of payroll tax and business tax incidence with heterogeneity analysis

Paper	Type of shock	Heterogeneity in...			
		... employment ...		... wage (pass-through to workers) ...	
		... by firm	... by worker	... by firm	... by worker
Our paper	payroll tax cut, age discontinuity	low-quality firms	firm heterogeneity present in homogeneous worker groups	high-quality firms	firm heterogeneity present in homogeneous worker groups
Cloyne, Kurt, Surico (2024)	corporate income tax cut	goods producing firms	-	service sector firms	-
Cottet (2024)	payroll tax cut for MW workers	liquidity-constrained, credit-constrained firms	-	-	-
Kennedy, Dobridge, Landefeld and Mortenson (2024a)	corporate income tax cut, between firm variation by type of corporation	-	-	smaller firms (below 200), high liquidity firms	highly paid (top 5%), executive
Kennedy, Dobridge, Landefeld and Mortenson (2024b)	corporate income tax cut, between firm variation by type of corporation	-	-	-	heterogeneity at top 10% earners: men, older, longer-tenured workers
Carbonnier, Malgouyres, Py and Urvoy (2022)	introduction of business tax credits for wages below 2.5 MW	-	high-skilled workers: 1% decrease in labor cost, 0.5 pp higher high-skilled share at firm	-	high-skilled workers (60%) incumbent workers (65%) men have higher wage gains
Nallareddy, Rouen, and Serrato (2022)	corporate income tax cut, between state variation	-	-	-	higher income for all workers, capital income gains for top earners
Benzarti and Harju (2021)	payroll tax, firm-level variation at some capital depreciation threshold, then this is replaced by a single payroll tax rate	collective bargaining: no heterogeneity	low-skilled workers (no high-school or college degree) manual workers (vs. lower- and upper-level non-manual employees)	-	-
Stokke (2021)	payroll tax cut for some municipalities	none (no impact)	none (no impact)	high-productivity firms (45% both in the short and the medium term) low-productivity firms (no short-term impact and 22% medium-term impact)	high-ability workers
Ku, Schönberg and Seim (2020)	system of geographically differentiated payroll taxes was suddenly abolished	middle 50% size labor intensive	-	-	-

Note: If there are heterogeneous effects in employment or wage by firm or worker characteristics the subgroup for whom a significant impact is found are put in the table. Cells are filled with a "-" if there is no relevant heterogeneity analysis in the paper.

Table C.1.2: Summary of empirical studies of payroll tax and business tax incidence with heterogeneity analysis, *Continued*

Paper	**Type of shock**	**Heterogeneity in...**			
		... employment ...		**... wage (pass-through to workers) ...**	
		... by firm	**... by worker**	**... by firm**	**... by worker**
Bozio, Breda and Grenet (2019)	three reforms that increased social security contributions	-	-	limited heterogeity by sector and size: services sector (vs. industry) above-median size, tax-benefit linkage is a key driver in all subsamples	limited heterogeneity by gender and age: varying results for 3 reforms, tax-benefit linkage is a key driver in all subsamples
Giroud and Rauh (2019)	corporate income tax cut, between firm variation by type of corporation	tradable industries, footloose industries	-	-	-
Saez, Schoefer and Seim (2019)	payroll tax cut, age discontinuity	youth-intensive, credit-constrained	-	youth-intensive	none (employees benefit collectively)
Fuest, Peichl and Siegloch (2018)	local business tax, municipality-level and time variation	-	-	collective bargaining, domestic, firms operating in a single jurisdiction, small/medium size	low-skilled, blue–collar, young, and female employees bear a larger share of the tax burden

Note: If there are heterogeneous effects in employment or wage by firm or worker characteristics the subgroup for whom a significant impact is found are put in the table. Cells are filled with a “-” if there is no relevant heterogeneity analysis in the paper

Table C.1.3: Employment rate in the administrative data and in the Labor Force Survey

	(1) Administrative data	(2) Labor Force Survey
Panel A: Private and public sector		
Including self-employment	60.1%	61.6%
Excluding self-employment	49.4%	51.8%
Panel B: Private sector (excluding self-employment)		
All private sector firms	41.9%	
Double-entry bookkeeping firms	36.2%	
Double-entry bookkeeping firms with at most 10,000 employees	33.0%	

Note: Table reports employment rates in the non-retired population of men aged 52-57 in 2012. Column (1) reports employment rates based on the linked employer-employee administrative data used in this paper. Column (2) reports employment rates based on the Labor Force Survey (LFS) of the Hungarian Central Statistical Office, which is the European equivalent of the Current Population Survey (CPS). Panel A shows employment rates in the private and public sectors with and without the self-employed. Panel B shows private sector employment in all firms, double-entry bookkeeping firms, and double-entry bookkeeping firms with at most 10,000 employees. It displays statistics only based on the administrative data because civil servants and the type of the firm cannot be identified in the LFS. The employment category in the last row corresponds to the employment definition we use in this paper.

Table C.1.4: Employment effects of the tax cut for all private sector firms and for firms with double-entry bookkeeping

	(1)	(2)	(3)
		Employment	
	All firms	Low TFP	High TFP
Panel A: Double-entry bookkeeping firms,	0.0053***	0.0053***	-0.0001
excluding firms with more than 10,000 workers	[0.0005]	[0.0005]	[0.0004]
	⟨0.330⟩	⟨0.167⟩	⟨0.163⟩
Panel B: All firms, including single-entry bookkeping	0.0096***	0.0094***	0.0003
firms and firms with more than 10,000 workers	[0.0006]	[0.0006]	[0.0004]
	⟨0.409⟩	⟨0.227⟩	⟨0.181⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on private sector employment for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms. We report the β coefficient from regression equation (3.1). In angle brackets we report the mean of these outcome variables in May 2012—the probability of being employed in a given subgroup and type of firm. The β coefficient compares the change in employment among the 55 to 57 age group that was affected by the payroll tax cut relative to the change in employment among the 52 to 54 age group that was not affected by the tax cut. In Panel A, we report the results for the baseline category of private sector employment (excluding firms with more than 10,000 workers). In Panel B, we report the results for all firms, assuming that all single-entry bookkeping firms (for which firms the TFP is not observed) are below-median TFP firms. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Table C.1.5: Employment effects of the tax cut: extensive margin employment decisions

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— *Post* × *Treated*	0.0054***	0.0053***	0.0001
	[0.0005]	[0.0005]	[0.0004]
Panel B: Percent change in employment			
—Employment without tax cut	0.342	0.176	0.176
—Employment with tax cut	0.347	0.182	0.182
—Percent change in employment	1.58%	3.00%	0.05%
Panel C: Percent change in labor cost $(1 + \tau_{ss})$			
—Labor cost without tax cut	1.27	1.26	1.28
—Labor cost with tax cut	1.20	1.18	1.22
—Percent change in labor cost	-5.27%	-6.02%	-4.45%
Panel D: Implied elasticity (Panel B/Panel C)			
—Elasticity based on percent change in labor cost	-0.30	-0.50	-0.01
	[0.03]	[0.05]	[0.06]
Panel E: Elasticity based on net present value			
—Percent change in net present value of labor cost	-7.49%	-8.82%	-5.98%
—Implied elasticity	-0.21	-0.34	-0.01
	[0.02]	[0.03]	[0.04]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows the employment effect of the tax cut as in Table 3.3 with the difference that we focus on extensive margin employment decisions (whether to work or not) without taking into account working hours. ($N = 9,003,984$ individual-months)

Table C.1.6: Employment effects of the tax cut: excluding elementary occupations from employment definition

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Baseline employment definition			
Employment effect	0.0053***	0.0053***	-0.0001
	[0.0005]	[0.0005]	[0.0004]
Implied elasticity	-0.30	-0.53	0.01
	[0.03]	[0.05]	[0.06]
Panel B: Employment excluding elementary occupations			
Employment effect	0.0063***	0.0063***	-0.0000
	[0.0006]	[0.0005]	[0.0005]
Implied elasticity	-0.41	-0.73	0.00
	[0.04]	[0.06]	[0.07]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on employment. Panel A shows the baseline results. In Panel B only employment in non-elementary occupations is considered. This is motivated by the fact that workers in elementary occupations were eligible for the tax cut independently of their age. We estimate the impact of the reform using regression (3.1). In particular, we report the β coefficient and estimate the regression with the outcome variable being employed at a private sector firm (column 1), at a private sector firm with below-median TFP (column 2), and at a private sector firm with above-median TFP (column 3). The β coefficient estimates the change in employment among the 55 to 57 age group that was affected by the payroll tax cut relative to the change in employment among the 52 to 54 age group that was not affected by the tax cut. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months in both panels)

Table C.1.7: Employment effects of the tax cut: alternative sample definitions

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Baseline sample			
Employment effect	0.0053^{***}	0.0053^{***}	-0.0001
	[0.0005]	[0.0005]	[0.0004]
Implied elasticity	-0.30	-0.53	0.01
	[0.03]	[0.05]	[0.06]
Panel B: Sample with retirees			
Employment effect	0.0065^{***}	0.0062^{***}	-0.0001
	[0.0005]	[0.0004]	[0.0004]
Implied elasticity	-0.37	-0.64	0.01
	[0.03]	[0.05]	[0.06]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on employment under different sample definitions. Panel A replicates the baseline results reported in Panel A of Table 3.3. Panel B shows the same estimates with retirees included in the sample. In both panels we estimate the impact of the reform using regression (3.1). In particular, we report the β coefficient and estimate the regression with the outcome variable being employed at a private sector firm (column 1), at a private sector firm with below-median TFP (column 2), and at a private sector firm with above-median TFP (column 3). The β coefficient estimates the change in employment among the 55 to 57 age group that was affected by the payroll tax cut relative to the change in employment among the 52 to 54 age group that was not affected by the tax cut. Standard errors are reported in brackets, clustered at the age $\times$ period level. ($N = 9,003,984$ individual-months in Panel A, $N = 9,482,667$ individual-months in Panel B)

Table C.1.8: The effect of the tax cut on labor market status

	(1)
Private sector employment (41%)	0.0096***
	[0.0006]
Public sector employment (6.2%)	0.0016***
	[0.0003]
Self-employment (9.7%)	-0.0014***
	[0.0003]
Inactive/unemployed (42%)	-0.0101***
	[0.0007]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows the impact of the payroll tax cut on labor market status. Labor market status is determined based on four mutually exclusive categories: all type of private sector employment (41% of the 52-57 years old), public sector employment (6.2% of the 52-57 years old), self-employment (9.7% of the 52-57 years old) and inactivity/unemployment (42% of the 52-57 years old). To make sure that these categories are mutually exclusive, private sector employment (contrary to the benchmark analysis) also includes single-entry bookkeeping firms and firms with more than 10,000 workers (see Section 3.3.2 and for separate estimates for these firm categories see Table C.1.4). The population share of each labor market status category is reported in parentheses. We report the difference-in-difference estimates from equation (3.1) using being employed in the private sector (row 1), being employed in the public sector (row 2), being self-employed (row 3) and being inactive or unemployed (row 4) as the outcome variable. The difference-in-differences estimate compares the change in the outcome variable among the 55 to 57 age group that was affected by the payroll tax cut relative to the change in the outcome variable among the 52 to 54 age group that was not affected by the tax cut. Standard errors are reported in brackets, clustered at the age $\times$ period level. ($N = 9,003,984$ individual-months)

Table C.1.9: Employment effects of the tax cut: short-run estimates

	(1) Employment, baseline	(2) Employment, TFP	(3) Employment, PI	(4) Employment, foreign ownership	(5) Employment, firm-level wage	(6) Employment, AKM FE
All firms	0.0029***					
	[0.0005]					
Low-quality firms		0.0045***	0.0024***	0.0035***	0.0032***	0.0036***
		[0.0004]	[0.0005]	[0.0004]	[0.0003]	[0.0004]
High-quality firms		-0.0016***	0.0005	-0.0006	-0.0003	-0.0007
		[0.0005]	[0.0007]	[0.0003]	[0.0005]	[0.0006]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the short-run impact of the payroll tax cut on private sector employment. Column (1) and (2) replicate the analysis in Panel A of Table 3.3, but restrict the sample to the period between 2012 (the year before the policy change) and 2013 (the year after the policy change) instead of focusing on the period between 2012 and 2015 as in Table 3.3). We estimate the impact of the reform using regression (3.1). In particular, we report the β coefficient and estimate the regression with the outcome variable being employed at a private sector firm (row 1), at a low-quality private sector firm (row 2) and at a high-quality private sector firm (row 3). Columns (3)-(6) report robustness to using different quality measures. In column (3) we measure firm quality based on the poaching index (PI), reflecting the fraction of new hires poached from other firms instead of coming from unemployment. Column (4) reports estimates by ownership. In Hungary foreign-owned firms offer the highest-paying, highest-quality jobs. In column (5) we measure firm quality by the average wage the firms pays. Finally, in column (6) we measure firm quality based on the firm-level wage premium estimated using an Abowd, Kramarz, Margolis (AKM) style decomposition. Standard errors are reported in brackets, clustered at the age $\times$ period level. ($N = 4,711,215$ individual-months)

Table C.1.10: Employment and wage effects of the tax cut: robustness to using measures of firm-quality based on pre-reform years

Panel A: Employment	(1)	(2)	(3)	(4)	(5)
Firm quality uses pre-reform years only	TFP	PI	Foreign ownership	Firm-level wage	AKM FE
Low-quality firms	0.0059***	0.0060***	0.0062***	0.0040***	0.0032***
	[0.0005]	[0.0005]	[0.0004]	[0.0003]	[0.0004]
High-quality firms	-0.0006	-0.0009	-0.0008**	0.0013***	0.0010**
	[0.0005]	[0.0006]	[0.0003]	[0.0004]	[0.0004]
Panel B: Log(wage), pass-through rate					
Firm quality uses pre-reform years only	TFP	PI	Foreign ownership	Firm-level wage	AKM FE
Low-quality firms	-0.094	0.053	-0.105	0.219*	-0.113
	[0.119]	[0.085]	[0.139]	[0.113]	[0.078]
High-quality firms	0.547***	0.610***	1.236***	1.103***	1.019***
	[0.108]	[0.123]	[0.180]	[0.200]	[0.224]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on employment and wages, assessing robustness to defining firm quality using only pre-reform years (instead of all years). Panel A shows the effect of tax cut on employment and Panel B on wages. TFP, firm-level wage and foreign ownership are defined based on year 2012. The poaching index (PI) and AKM firm fixed effects are estimated using all pre-policy years (2003 and 2012). In Panel A we report the β coefficient from regression equation (3.1). In Panel B we report the pass-through rate. The pass-through rate at low-productivity firms is the β_1 coefficient on the Post×Treated×TCR term in equation (3.5), while at the high-productivity firms it is the sum of that coefficient and the β_3 coefficient on High-quality×Post×Treated×TCR in equation (3.5). Standard errors are reported in brackets, clustered at the age × period level.

Table C.1.11: Wage effects of the tax cut by various firm quality indicators

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) log(wage)
Post×Treated	0.008	-0.005	0.003	-0.004	0.010
	[0.007]	[0.012]	[0.011]	[0.016]	[0.020]
Post×Treated×TCR	-0.077	0.058	-0.042	0.030	-0.115
	[0.070]	[0.108]	[0.101]	[0.139]	[0.167]
High-quality×Post×Treated	-0.046***	-0.032***	-0.068***	-0.054***	-0.072***
	[0.013]	[0.004]	[0.014]	[0.008]	[0.014]
High-quality×Post×Treated×TCR	0.678***	0.464***	1.179***	0.963***	1.235***
	[0.137]	[0.056]	[0.211]	[0.051]	[0.160]
Pass-through rate					
Low-quality	-0.077	0.058	-0.042	0.030	-0.115
	[0.070]	[0.108]	[0.101]	[0.139]	[0.167]
High-quality	0.602***	0.521***	1.137***	0.993***	1.119***
	[0.131]	[0.113]	[0.211]	[0.167]	[0.233]
Observations	97,789	97,789	97,789	97,789	97,789
Quality measure	TFP	PI	foreign-owned	firm-level wage	AKM FE

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on private sector wages based on estimating equation (3.5). In each column, we interact all coefficients with an indicator for whether the firm is high quality (above-median with respect to the given quality measure or foreign-owned). In all columns we show the wage changes for all incumbent workers and we focus on full-time workers. In all columns we compare the wage changes between 2012 and 2013 to the wage changes between 2011 and 2012. In column (1) we repeat the results using TFP as a measure of quality reported in column (3) of Table 3.5. In column (2) we measure quality based on the poaching index (PI), reflecting the fraction of new hires poached from other firms instead of coming from unemployment. In column (3) we measure quality based on ownership. In Hungary foreign-owned firms are the most productive firms offering the highest-paying, highest-quality jobs. In column (4) we measure firm quality by the average wage the firms pays. Finally, in column (5) we measure firm quality based on the firm-level wage premium estimated using an Abowd, Kramarz, Margolis (AKM) style decomposition. The pass-through rate is calculated as in Table 3.5. Standard errors are reported in brackets, clustered at the age × period level.

Table C.1.12: Employment and wage effects of the tax cut: robustness to classification of firms based on within-industry TFP variation

	(1)	(2)	(3)	(4)
	Baseline		Net of industry composition	
	Low TFP	High TFP	Low TFP	High TFP
Panel A: Employment				
Employment effect	0.0053***	-0.0001	0.0041***	-0.0002
	[0.0005]	[0.0004]	[0.0005]	[0.0006]
Implied elasticity	-0.53	0.01	-0.40	0.03
	[0.05]	[0.06]	[0.05]	[0.10]
Observations	9,003,984	9,003,984	9,003,984	9,003,984
Panel B: Log(wage)				
Pass-through rate	-0.077	0.602***	-0.011	0.457***
	[0.070]	[0.131]	[0.074]	[0.140]
Observations	97,789	97,789	97,789	97,789

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on employment and wages, assessing the effect of using only within industry total factor productivity variations when we classify firms to low and high TFP. Panel A shows the effect of tax cut on employment and Panel B on wages. In Panel A we report the β coefficient from regression equation (3.1). In Panel B we report the pass-through rate. The pass-through rate at low-productivity firms is the β_1 coefficient on the Post×Treated×TCR term in equation (3.5), while at the high-productivity firms it is the sum of that coefficient and the β_3 coefficient on High-quality×Post×Treated×TCR in equation (3.5). Columns (1) and (2) repeat the baseline results from Tables 3.3 and 3.5. In columns (3) and (4), the median TFP is based on the residualized TFP from a linear regression of TFP on level 1 industry codes. As a result, the industry composition among low and high TFP firms will be similar. Standard errors are reported in brackets, clustered at the age × period level.

Table C.1.13: Employment effects of the tax cut: heterogeneity by firm size

	(1)	(2)	(3)
		Employment	
	All firms	Low TFP	High TFP
Firms with 1-49 workers	0.0015***	0.0015***	0.0001
	[0.0004]	[0.0003]	[0.0002]
	{39%}	{33%}	{6%}
	⟨0.1272⟩	⟨0.1074⟩	⟨0.0198⟩
Firms with 50+ workers	0.0036***	0.0035***	0.0001
	[0.0005]	[0.0003]	[0.0005]
	{61%}	{18%}	{43%}
	⟨0.2021⟩	⟨0.0608⟩	⟨0.1413⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on employment separately for micro and small-sized firms (1-49 workers) and medium and large firms (50+ workers). In the top row we report the β coefficient from regression equation (3.1) with the outcome variable being whether someone is employed at a micro/small sized firm, at a micro/small sized firm with below-median (column 2) or above-median (column 3) TFP. In the bottom row we report the β coefficient from regression equation (3.1) with the outcome variable being whether someone is employed at a medium/large sized firm, at a medium/large sized firm with below-median (column 2) or above-median (column 3) TFP. In curly brackets, we show the share of individuals working at different sized (and different productivity) firms, while in angle brackets we show the mean of the outcome variable in May 2012. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Table C.1.14: Employment effects of the tax cut for new entrant and incumbent workers and firms

	(1)	(2)	(3)
		Employment	
	All firms	Low TFP	High TFP
Panel A: New entrant or incumbent workers			
New entrant workers	0.0015***	0.0014***	0.0001
	[0.0002]	[0.0002]	[0.0001]
	⟨0.0425⟩	⟨0.0267⟩	⟨0.0159⟩
Incumbent workers	0.0038***	0.0039***	-0.0001
	[0.0005]	[0.0004]	[0.0004]
	⟨0.2873⟩	⟨0.1409⟩	⟨0.1464⟩
Panel B: New entrant or incumbent firms			
New entrant firms	0.0001	0.0002*	-0.0001***
	[0.0001]	[0.0001]	[0.00004]
	⟨0.0054⟩	⟨0.0045⟩	⟨0.0008⟩
Incumbent firms	0.0052***	0.0051***	0.0001
	[0.0005]	[0.0005]	[0.0004]
	⟨0.3247⟩	⟨0.1625⟩	⟨0.1622⟩
Panel C: Firms established before or after 2012			
Firms established after 2012	-0.0001	0.0002*	-0.0003***
	[0.0001]	[0.0001]	[0.0001]
	⟨0⟩	⟨0⟩	⟨0⟩
Firms existed in 2012	0.0053***	0.0051***	0.0002
	[0.0004]	[0.0004]	[0.0004]
	⟨0.3301⟩	⟨0.1670⟩	⟨0.1631⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences based on equation (3.1). In Panel A we study the change in employment for new entrants, who entered the labor market in the current year and so have less than 12 months employment, and for incumbent workers who have been continuously employed in the previous 12 months. In panel B we study the impact separately for new entrant firms, which were established in the current year and incumbent firms, which already existed in the previous year. In panel C we study separately the employment change at firms that existed before the payroll tax cut and at firms that were established after the payroll tax cut. In each panel the sum of new entrants and incumbents adds up to total employment and the employment rate in each of these categories (relative to the total population) in May 2012 is shown in angle brackets. In panel C the employment rate is zero because there is no employment in May 2012 at firms established after 2012. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Table C.1.15: Wage effects of the tax cut: heterogeneity by firm size

	(1)	(2)
	log(wage)	log(wage)
Post×Treated	-0.002	0.020
	[0.016]	[0.016]
Post×Treated×TCR	0.028	-0.234
	[0.136]	[0.174]
High TFP×Post×Treated	-0.027	-0.061***
	[0.034]	[0.017]
High TFP×Post×Treated×TCR	0.422	0.889***
	[0.285]	[0.167]
Pass-through rate		
Low TFP	0.028	-0.234
	[0.136]	[0.174]
High TFP	0.450	0.653***
	[0.290]	[0.128]
Observations	35,862	61,861
Firm size	1-49 workers,	50+ workers

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates based on equation (3.5). In column (1), the sample is restricted to workers employed at micro and small-sized firms (1-49 workers). In column (2), the sample is restricted to workers employed at medium and large firms (50+ workers). In both columns we show the wage changes for all incumbent workers and we focus on full-time workers. In both columns we compare the wage changes between 2012 and 2013 to the wage changes between 2011 and 2012. The pass-through rate is calculated as in Table 3.5

Table C.1.16: Employment effects of the tax cut: heterogeneity by local labor market conditions

	(1)	(2)
	Employment	
	Low TFP	High TFP
Panel A: By unemployment rate		
Districts with below-median unemployment rate in 2012	0.0055***	-0.0014**
	[0.0008]	[0.0007]
	⟨0.1807⟩	⟨ 0.2040⟩
Observations	3,603,336	3,603,336
Districts with above-median unemployment rate in 2012	0.0065***	-0.0003
	[0.0008]	[0.0008]
	⟨0.1706⟩	⟨0.1315⟩
Observations	3,938,028	3,938,028
Panel B: By change in labor market conditions		
Districts with stable labor market conditions	0.0050***	-0.0005
	[0.0005]	[0.0005]
	⟨0.1650⟩	⟨0.1585⟩
Observations	5,278,340	5,278,340
Districts with improving labor market conditions	0.0051***	0.0011
	[0.0007]	[0.0008]
	⟨0.1718⟩	⟨ 0.1601⟩
Observations	4,400,856	3,421,239
Panel C: By share of older workers		
Districts with below-median ratio of aged 55-57	0.0054***	-0.0007
	[0.0006]	[0.0006]
	⟨0.1538⟩	⟨0.1662⟩
Observations	4,287,445	4,287,445
Districts with above-median ratio of aged 55-57	0.0050***	0.0009*
	[0.0007]	[0.0004]
	⟨0.1808⟩	⟨0.1583⟩
Observations	4,716,539	4,716,539

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table explores the heterogeneity in the employment effects of the tax cut by local labor market characteristics. In Panel A, the employment changes are studied separately for districts with below- and above-median unemployment rates in 2012. The mean unemployment rate was 8.6% in districts with below-median unemployment rate and 18.3% in districts with above-median unemployment rate. In Panel B, we study the employment effects of the tax cut separately in stable and in improving labor markets. In districts with stable labor market conditions, the change in private sector employment rate between 2012 and 2015 was between -2 and +2 percentage points, with a mean of 0.1 percentage point. In districts with improving labor market conditions, the change in private sector employment rate between 2012 and 2015 was above +2 percentage points, with a mean of 3 percentage points. We exclude here the few deteriorating labor markets with more than -2 percentage points decline in private sector employment rate. In Panel C, we show employment effects separately for districts with below- and above-median shares of men aged 55 and 57 within the male population in 2012. The mean share was 0.074 in districts with a below-median share and 0.085 in districts with an above-median share. In each panel, and for each region, we apply the same difference-in-differences estimate as in Panel A of Table 3.3. In particular, we report the β coefficient from regression equation (3.1) with the outcome variable being employed at a private sector firm with below-median productivity (column 1) and at a private sector firm with above-median productivity (column 2). In angle brackets, we show the mean of the outcome variable in May 2012. Standard errors are reported in brackets, clustered at the age $\times$ period level.

Table C.1.17: Wage effects of the tax cut, using lagged firm quality measures

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)
Post×Treated	0.008	0.022***	-0.026***	0.011
	[0.005]	[0.007]	[0.006]	[0.013]
Post×Treated×TCR	-0.062	-0.174**	0.210***	-0.097
	[0.053]	[0.068]	[0.046]	[0.181]
High TFP×Post×Treated	-0.044***	-0.036***	-0.038***	-0.051**
	[0.023]	[0.008]	[0.017]	[0.018]
High TFP×Post×Treated×TCR	0.587***	0.484***	0.540***	0.687***
	[0.122]	[0.064]	[0.076]	[0.201]
Windfall rate×Post×Treated				0.561*
				[0.309]
Windfall rate×Post×Treated×TCR				-6.324**
				[2.601]
Pass-through rate				
Low TFP	-0.062	-0.174**	0.210***	-0.097
	[0.053]	[0.068]	[0.046]	[0.181]
High TFP	0.525***	0.310***	0.750***	0.590***
	[0.124]	[0.095]	[0.067]	[0.092]
Observations	97,789	112,713	82,910	97,789
New entrants vs. incumbents	incumbents	incumbents	incumbents	incumbents
Part-time included	no	yes	no	no
One vs. two year change	one	one	two	one
Windfall rate included	no	no	no	yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on wages when we use lagged firm quality ($Q_{j(it-1)}$) in the regression equation (3.5) instead of current firm quality $Q_{j(it)}$. Columns (1)-(4) estimate heterogeneity by firm productivity using equation (3.5) (but with lagged firm quality measure). In all columns except column (2) we focus on full-time workers. In column (2) we also include part-time workers in the analysis. In all columns except in column (3), we compare the wage changes between 2012 and 2013. In column (3) we study two-year wage changes and compare the wage change between 2012 and 2014. In column (4), we also interact the treatment, age, year, and tax cut rate indicators with the firm specific windfall rate, which reflects the size of the windfall received by the firm as a result of the tax cut. Following (Saez et al., 2019) we calculate this as the (lagged) ratio of age- and occupation specific payroll tax cuts payable after the reform and the total payroll. The difference-in-differences estimate compares the change in wages among the 55 to 57 age group that was affected by the payroll tax cut with the change in employment among the 52 to 54 age group that was not affected by the tax cut. The pass-through rate at low-productivity firms is the β_1 coefficient on the Post×Treated×TCR term in equation (3.5), while at high-productivity firms it is the sum of the β_1 coefficient and the β_3 coefficient on the High TFP × Post×Treated×TCR term in equation (3.5). Standard errors are reported in brackets, clustered at the age × period level.

Table C.1.18: Wage effects of the tax cut by various firm quality indicators, wage model extended with windfall indicator

	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) log(wage)	(5) log(wage)	(6) log(wage)	(7) log(wage)	(8) log(wage)	(9) log(wage)	(10) log(wage)
Post × Treated	0.008	0.011	-0.005	-0.001	0.003	0.006	-0.0004	0.011	0.010	0.025
	[0.007]	[0.016]	[0.012]	[0.011]	[0.011]	[0.008]	[0.016]	[0.019]	[0.020]	[0.024]
Post × Treated × TCR	-0.077	-0.129	0.058	-0.035	-0.042	-0.101	0.030	-0.094	-0.115	-0.272
	[0.070]	[0.215]	[0.108]	[0.104]	[0.101]	[0.129]	[0.139]	[0.189]	[0.167]	[0.237]
High-quality × Post × Treated	-0.046***	-0.053**	-0.032***	-0.036***	-0.068***	-0.070***	-0.054***	-0.065***	-0.072***	-0.082***
	[0.032]	[0.021]	[0.004]	[0.003]	[0.014]	[0.013]	[0.008]	[0.012]	[0.014]	[0.021]
High-quality × Post × Treat × TCR	0.678***	0.780***	0.464***	0.536***	1.179***	1.222***	0.963***	1.073***	1.235***	1.345***
	[0.137]	[0.242]	[0.056]	[0.064]	[0.211]	[0.235]	[0.051]	[0.109]	[0.160]	[0.255]
Windfall rate × Post × Treated		0.546*		0.772*		0.391		-0.111		-0.150
		[0.277]		[0.382]		[0.286]		[0.257]		[0.268]
Windfall rate × Post × Treat × TCR		-5.979**		-6.943***		-4.141**		-0.412		0.588
		[2.588]		[1.810]		[1.716]		[1.208]		[2.247]
Pass-through rate										
Low-quality	-0.077	-0.129	0.058	-0.035	-0.042	-0.101	0.030	-0.094	-0.115	-0.272
	[0.070]	[0.215]	[0.108]	[0.109]	[0.101]	[0.129]	[0.139]	[0.189]	[0.167]	[0.237]
High-quality	0.602***	0.651***	0.521***	0.501***	1.137***	1.121***	0.993***	0.979***	1.119***	1.074***
	[0.131]	[0.097]	[0.113]	[0.103]	[0.211]	[0.176]	[0.167]	[0.164]	[0.233]	[0.199]
Observations	97,789	97,789	97,789	97,789	97,789	97,789	97,789	97,789	97,789	97,789
Quality measure	TFP	TFP	PI	PI	foreign-owned	foreign-owned	firm-level wage	firm-level wage	AKM FE	AKM FE
New entrants vs. incumbents	incumbents	incumbents	incumbents	incumbents	incumbents	incumbents	incumbents	incumbents	incumbents	incumbents
Part-time included	no	no	no	no	no	no	no	no	no	no
One vs. two year change	one	one	one	one	one	one	one	one	one	one
Windall rate included	no	yes	no	yes	no	yes	no	yes	no	yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows the wage effects of the tax cut by various firm quality indicators. The odd columns in the table repeat the estimates of Table C.1.11 showing the difference-in-differences estimates of the impact of the payroll tax cut on private sector wages based on estimating equation (3.5). In all even columns, we also interact the treatment, age, year, and tax cut rate indicators with the firm-specific windfall rate, which reflects the size of the windfall received by the firm as a result of the tax cut. Following (Saez et al., 2019) we calculate this as the (lagged) ratio of age- and occupation specific payroll tax cuts payable after the reform and the total payroll. In columns (1) and (2) we use TFP as the firm quality indicator. In columns (3) and (4) we measure quality based on the poaching index (PI), reflecting the fraction of new hires poached from other firms instead of coming from unemployment. In columns (5) and (6) we measure quality based on ownership. In Hungary foreign-owned firms are the most productive firms offering the highest-paying, highest-quality jobs. In columns (7) and (8) we measure firm quality by the average wage the firms pays. Finally, in columns (9) and (10) we measure firm quality based on the firm-level wage premium estimated using an Abowd, Kramarz, Margolis (AKM) style decomposition. The difference-in-differences estimate compares the change in wages among the 55 to 57 age group that was affected by the payroll tax cut with the change in employment among the 52 to 54 age group that was not affected by the tax cut. Standard errors are reported in brackets, clustered at the age × period level.

Table C.1.19: Wage effects of the tax cut: heterogeneity by education

	(1) Primary and lower-secondary jobs, log(wage)	(2) Upper-secondary and tertiary jobs, log(wage)	(3) Primary and lower-secondary jobs, log(wage)	(4) Upper-secondary and tertiary jobs, log(wage)
Post × Treated	0.032***	-0.014	0.005	-0.001
	[0.010]	[0.011]	[0.078]	[0.208]
Post × Treated × TCR	-0.285***	0.209	-0.060	0.038
	[0.084]	[0.135]	[0.009]	[0.026]
High-quality × Post × Treated	-0.053***	-0.037	-0.042*	-0.081***
	[0.013]	[0.021]	[0.021]	[0.022]
High-quality × Post × Treated × TCR	0.643***	0.792**	0.731**	1.990***
	[0.130]	[0.308]	[0.276]	[0.458]
Pass-through rate				
Low-quality	-0.285***	0.209	-0.060	0.038
	[0.084]	[0.135]	[0.078]	[0.208]
High-quality	0.358*	1.001***	0.671**	2.028***
	[0.164]	[0.193]	[0.292]	[0.525]
Observations	66,180	30,794	66,180	30,794
Quality measure	TFP	TFP	AKM FE	AKM FE

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax cut on private sector wages based on estimating equation (3.5). The sample is split by education categories of jobs (measured at the previous year), which are defined by imputing the modal education level of employees of the same four-digit occupation code in the 2013 Labor Force Survey of the Central Statistical Office of Hungary. In all columns we show the wage changes for all incumbent workers and we focus on full-time workers. In all columns we compare the wage changes between 2012 and 2013 to the wage changes between 2011 and 2012. In columns (1) and (2), we use TFP as firm quality indicator, in columns (3) and (4) we use the firm-level wage premium estimated using an Abowd, Kramarz, Margolis (AKM) style decomposition. The pass-through rate is calculated as in Table 3.5. Standard errors are reported in brackets, clustered at the age × period level.

C.2 Elasticity calculations based on change in net present value of labor cost

Forward-looking firms consider not only tax cuts they realize today, but also the net present value of all the future streams of tax cuts. In this section, we calculate the employment elasticity based on the net present value of the tax cut. Even workers in our control group are affected by the tax cut as they might reach age 55 and so firms employing them can benefit from the tax cut in the future. The present value of tax cuts realized in the future depends on several factors—the discount rate, the expected retirement age, and the typical separation rate of workers at the firm (before reaching the retirement age).

We calculate the percent change in net present value of labor cost along the following steps. We use the percent change in labor cost as reported in Table 3.3, which is the percent difference in the labor cost of workers in the treatment and control group. This value varies with firm quality: -5.27% for all firms, -6.02% for low-TFP firms, and -4.45% for high-TFP firms. We discount the future savings with a rate of 7% as the Central Bank Base Rate was 7% as of January 1, 2012. We take into account workers' separation rate, and the fact that this separation rate varies by firm-quality. We use the 12-months separation rate of men aged 52-57 in 2011 as observed in our data. This rate is 17.9% for all firms, 22.3% for low-TFP firms, and 13.6% for high-TFP firms. We assume that all worker-firm relationships end at age 62 when workers retire.

We calculate the elasticity of employment as the ratio of the percent change in employment as reported in Panel B of Table 3.3 (1.59% for all firms, 3.18% for low-TFP firms, and -0.03% for high-TFP firms) and the percent change in net present value of labor cost. The results under the baseline parameters are reported in Panel A of Table C.2.20. These elasticity estimates are also reported in Panel E of Table 3.3.

Table C.2.20: Elasticity of employment based on net present value of labor cost

	(1)	(2)	(3)	(4)	(5)	(6)
	Percent change in net present value of labor cost			Elasticity		
	All	Low TFP	High TFP	All	Low TFP	High TFP
Panel A: Benchmark						
	-7.49	-8.82	-5.98	-0.21	-0.36	0.01
				[0.02]	[0.03]	[0.04]
Panel B: Discount rate (benchmark: 0.07)						
0.1	-7.65	-8.90	-6.22	-0.21	-0.36	0.00
				[0.02]	[0.03]	[0.04]
0.13	-7.75	-8.94	-6.39	-0.21	-0.36	0.00
				[0.02]	[0.03]	[0.04]
Panel C: Retirement age (benchmark: 62)						
60	-5.69	-7.21	-4.08	-0.28	-0.44	0.01
				[0.03]	[0.04]	[0.06]
64	-8.11	-9.37	-6.62	-0.20	-0.34	0.00
				[0.02]	[0.03]	[0.04]
Panel D: Separation rate						
Common job finding rate (0.348)	-7.76	-8.86	-6.55	-0.20	-0.36	0.00
				[0.02]	[0.03]	[0.04]
Common separation probability (0.179)	-7.49	-8.55	-6.32	-0.21	-0.37	0.00
				[0.02]	[0.04]	[0.04]

Note: Columns (1)-(3) report percent change in net present value of labor cost under various scenarios. Firms' labor cost is net wage times $(1 + \tau_{ss})$, where τ_{ss} is the employer social security contribution. The reform cut τ_{ss} for workers in the treatment group. Panel A calculates the percent change in net present value of labor cost under the benchmark parameters with discount rate 0.07, retirement age 62, and TFP-specific separation rate as observed in our data (0.18 for all firms, 0.22 for low-TFP firms, 0.14 for high-TFP firms). Panels B, C and D modify the discount rate, retirement age, and separation rate, respectively. Columns (4)-(6) calculate the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (as reported in Table 3.3) and labor cost (columns (1)-(3)). Standard errors are reported in brackets, clustered at the age × period level. (N = 9,003,984 individual-months)

In Panel B of Table C.2.20, we repeat the calculation of the percent change in net present value of labor cost and the elasticity of employment with two alternative discount rate values: 0.1 and 0.13. In Panel C, we use the benchmark discount rate (0.07) but consider a lower and a higher retirement age: 60 and 64. Finally, in Panel D, we use the benchmark discount rate and retirement age, but instead of using the separation rate we apply the job finding rates of the simulation exercise of Section C.5.1.6 (0.348). The rationale for applying the job finding rate is that firms in our model can only enjoy the benefit of the tax cut as long as workers do not find any other job offers that could be used in bargaining. Finally, the last row applies the same separation rate for high- and low-TFP firms.

Panels B, C, and D of Table C.2.20 demonstrate that the elasticities vary little across the different specifications. This highlights that the estimates are not sensitive to the modeling assumptions made in the benchmark case. The employment elasticity is always between -0.36 and -0.44 at low-TFP firms, while it is close to zero for high-TFP firms. In all specifications, the difference in responses to the tax cut between the two firm types is both statistically and economically significant.

C.3 Effect on women

Women were eligible for the payroll tax cut but they were also targeted by a pension policy introduced in 2011. The so-called "Women 40" policy grants an early retirement option for women with 40 years of work credits, regardless of age. Years spent on maternity benefits also count towards the work credits, with the restriction that a woman must have been employed for 32 years (or at least 25 years if she has 5 or more children). Unfortunately, our data do not allow us to determine eligibility as we do not observe the full employment history of older people in our sample.

Even though this reform is unlikely to have a major effect on the employment of the treated population (age 55-57), we exclude women from the main analysis to ensure that our results are not driven by the pension policy. In this section, we estimate the employment and wage effects of the payroll tax cut among older women.

Table C.3.21: Elasticity of employment: women

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— *Post* × *Treated*	0.0051***	0.0037***	0.0014***
	[0.0007]	[0.0005]	[0.0005]
Panel B: Percent change in employment			
—Employment without tax cut	0.236	0.130	0.106
—Employment with tax cut	0.241	0.134	0.107
—Percent change in employment	2.16%	2.85%	1.32%
Panel C: Percent change in labor cost $(1+\tau_{ss})$			
—Labor cost without tax cut	1.26	1.25	1.27
—Labor cost with tax cut	1.19	1.17	1.21
—Percent change in labor cost	-5.35%	-5.88%	-4.60%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity based on percent change in labor cost	-0.40	-0.48	-0.29
	[0.06]	[0.07]	[0.10]
Panel E: Elasticity based on net present value			
—Percent change in net present value of labor cost	-7.46%	-8.51%	-6.03%
—Implied elasticity	-0.29	-0.33	-0.22
	[0.04]	[0.05]	[0.08]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table applies the same analysis for women as Table 3.3 for men. ($N = 9,529,124$ individual-months)

C.3.1 Employment effects.

We estimate the same difference-in-differences model for women as for men, specified in equation (3.1). Among older women private sector employment increased by 0.51 percentage points (2.16%) as a result of the tax cut (see Table C.3.21). The overall employment effect was almost identical among men (0.53 percentage points, 1.59%). Table C.3.21 also shows the implied labor demand elasticity. The 5.35% decrease in labor costs and the resulting 2.16% increase in employment of women aged 55-57 over 2013-2015 imply a labor demand elasticity of -0.40. Overall, the employment effect and the implied labor demand elasticity are similar among older women and men, though somewhat larger among women.

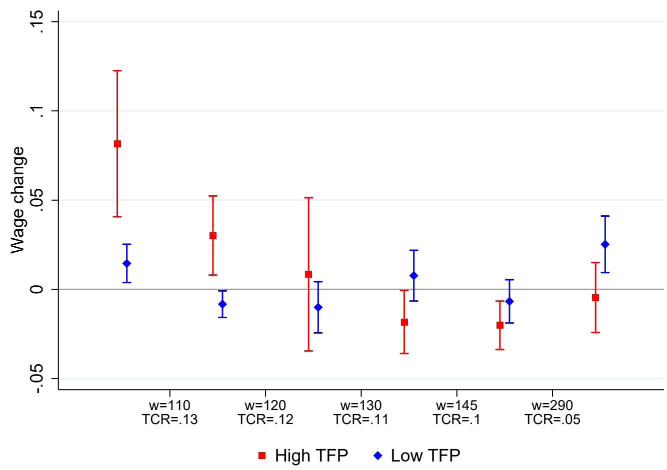
C.3.2 Heterogeneity by firm quality.

To investigate whether the employment effect for women differs by firm quality, we estimate the difference-in-differences model, specified in equation (3.1) with the outcome variable being employment either at a low-TFP or at a high-TFP firm. We apply exactly the same definition for low- and high-TFP firms as for men. Table C.3.21 shows that private sector employment of older women increases more at low-quality firms, the increase is 0.37 vs. 0.14 percentage points at low- vs. high-TFP firms. This translates into a -0.48 (s.e. 0.07) employment elasticity at low-TFP firms and a -0.29 (s.e. 0.10) employment elasticity at high-TFP ones. Therefore there is a clear and statistically significant difference in the employment responses at high- and low-quality firms albeit those differences are less stark for women than for men.

C.3.3 Wage effects by firm quality.

We also estimate the wage effects of the tax cut among older women. Figure C.3.5 shows the wage effects from 2012 to 2013 for women by firm quality at different levels of the effective tax cut. The patterns of wage effects are similar for women and men (see Figure 3.9 for men). Wages increase only at high-TFP firms and only at lower wage levels with a higher corresponding effective tax cut rate. However, the wage increase we see at high-productivity firms is somewhat smaller for women than for men.

Figure C.3.5: Wage changes at different levels of lagged wages: women



Note: Figure applies the same analysis for women as Figure 3.9 for men.

C.4 Effect on younger workers

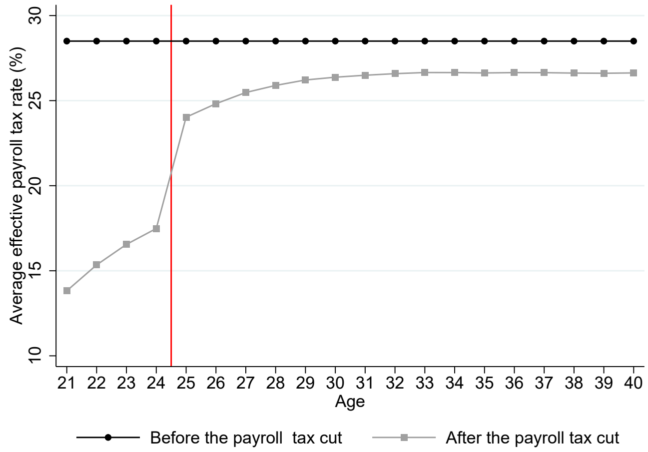
Parallel to the introduction of the payroll tax cut for older workers, a similar tax cut was applied for under-25 workers. We briefly summarize the main results we find for younger workers in Section 3.7 and we provide further details below.

We estimate the impact of the payroll tax cut in a difference-in-differences framework, comparing younger workers below the age 25 cutoff to workers just above (ages 22-24 vs. 25-27) during 2012-2015 (before and after the introduction of the tax cut in 2013). In 2015, the government introduced the Youth Guarantee Program recommended by the European Council, which targeted workers younger than age 25, however the take-up rate of the program was very small. In 2015 there were only a few thousand participants. The exclusion of the participants in the Youth Guarantee Program does not affect our results.

C.4.1 Employment effects.

Figure C.4.6 shows the effective average payroll tax rate for ages 20-40 before and after the implementation of the tax cut. We see a discontinuity at age 25 after the policy was implemented (in gray) compared to the constant rate of 28.5% before (in black). There is a jump from 17% to 24% from age 24 to 25, which is a slightly larger average effective tax cut than for workers above 55 (a cut of 7 vs. 6 percentage points for the younger and older age groups, respectively). At younger ages the effective tax cut decreases with age, which reflects the gradual increase in wages and thus the lower proportional tax cut. Furthermore, career starters received some extra tax cuts and the share of those workers steadily declines with age.

Figure C.4.6: Employers' social security contribution rate by workers' age: younger workers

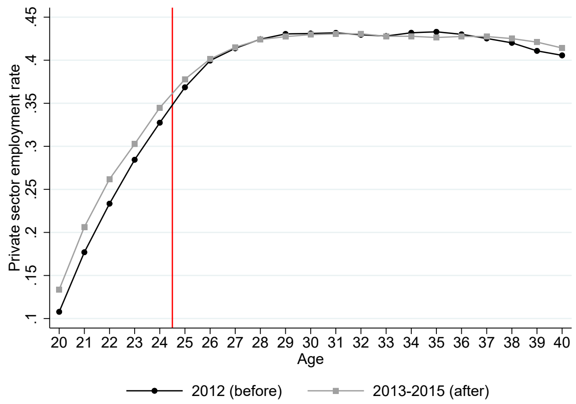


Note: Figure applies the same analysis for younger workers as Figure 3.1 for older workers. In particular, figure shows the average employer social security contribution rate by worker age for male workers in the private sector. Before the implementation of the payroll tax cut, the payroll tax rate was a flat 28.5%. Between 2013-2015 (after the implementation of the cut) all individuals over up to age 24 experienced a lump-sum tax cut of HUF 14,500 per month (around 6% of the average salary). Certain individuals were also eligible for the tax cut independently of their age (see Section 3.3.1 for the details).

Figure C.4.7 depicts employment in private sector companies for men by age before and after the payroll tax cut was introduced in 2013. Panel (a) shows raw employment rates by age before (year 2012, in black) and after the policy (years 2013-2015, in gray). It shows that employment rates increase rapidly with age between ages 20 and 26, are roughly constant between ages 26 and 35 and then start declining slowly. Comparing the period before and after the policy, this figure suggests that employment rates were similar in 2012 and 2013-2015 for most age groups, but show a clear divergence below 26.

Figure C.4.7: Employment in private sector companies by age: younger workers

(a) Private sector employment rate

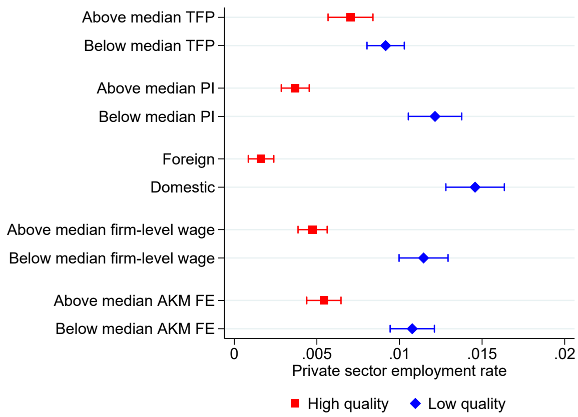


(b) Change in private sector employment rate

Note: Figure applies the same analysis for younger workers as Figure 3.2 for older workers.

Panel (b) shows estimates of the age-specific differences in employment at private sector companies for men before vs. after the payroll tax cut was introduced. It suggests that for ages above 25 changes in employment rates were close to zero (somewhat below zero at age 35 and at ages 39-40) but age-specific employment levels strongly diverge between the pre- and the post-reform periods among younger workers below 25. A 24-year-old worker was close to 2 percentage points more likely to be employed shortly after the policy was introduced (years 2013-2015). The gap widens as age decreases, which likely reflects the fact that in employment relationships formed at younger ages there is more time left until the tax cut phases out at age 25. Overall, this figure suggests that the payroll tax cut had a positive employment effect among younger workers. This effect is larger than for older employees above 55 (2 vs. 1 percentage point).

Figure C.4.8: Employment in private sector companies: alternative firm quality measures, younger workers



Note: Figure applies the same analysis for younger workers as Panel (a) of Figure 3.6 for older workers.

We estimate the same difference-in-differences regression for younger workers as for older workers (specified in equation (3.1)), where employees aged 22-24 are in the treatment group and the 25-27 age group acts as control group. Table C.4.22 shows the baseline results for younger workers. Among younger workers private sector employment increased by 1.6 percentage points (5.1%) as a result of the payroll tax cut, compared to the 0.53 percentage points (1.6%) increase among older workers. We also show the elasticity of employment in Table C.4.22. The 1.6 percentage points (5.1%) increase in employment and the 6.6% decrease in labor costs for the 22-24 age group over years 2013-2015 imply a labor demand elasticity of -0.77. Overall, the employment effect is larger and labor demand is more elastic for younger workers.

C.4.2 Heterogeneity by firm quality.

Figure C.4.8 shows the heterogeneity in the employment responses by various firm quality measures. We discuss these results in the main text.

C.4.3 Wage effects.

We assess the impact on wages among younger workers in a similar fashion as for older workers, using a modified version of equation (3.5) (replacing the linear tax cut rate in the last interaction term with categories of the tax cut rate). Figure C.4.9 shows the wage effects for younger workers from 2012 to 2013 at different levels of the effective tax cut rate. We find no significant change in wages at any level of the tax cut rate.

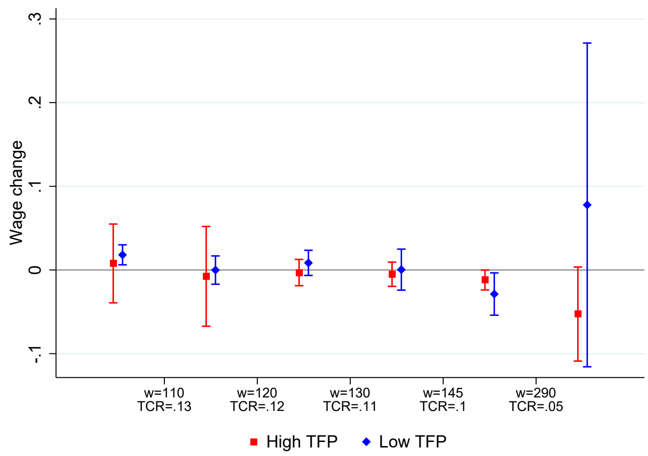
Table C.4.22: Elasticity of employment: younger workers

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— *Post* × *Treated*	0.0162***	0.0092***	0.0070***
	[0.0011]	[0.0006]	[0.0007]
Panel B: Percent change in employment			
—Employment without tax cut	0.317	0.142	0.175
—Employment with tax cut	0.333	0.151	0.182
—Percent change in employment	5.11%	6.45%	4.02%
Panel C: Percent change in labor cost $(1 + \tau_{ss})$			
—Labor cost without tax cut	1.25	1.23	1.26
—Labor cost with tax cut	1.17	1.15	1.18
—Percent change in labor cost	-6.61%	-7.03%	-5.96%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity based on percent change in labor cost	-0.77	-0.92	-0.67
	[0.05]	[0.06]	[0.07]
Panel E: Elasticity based on net present value			
—Percent change in net present value of labor cost	-9.81%	-10.02%	-9.21%
—Implied elasticity	-0.52	-0.64	-0.44
	[0.03]	[0.04]	[0.04]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table applies the same analysis for younger workers as Table 3.3 for older workers. ($N = 8,611,542$ individual-months)

Figure C.4.9: Wage changes at different levels of lagged wages: younger workers



Note: Figure applies the same analysis for younger workers as Figure 3.9 for older workers.

C.4.4 Why do we see some discrepancy between young and old workers' employment and wage responses?

The differences could reflect that young and old workers are operating in different types of labor markets. Young inexperienced workers are more likely to get uniform wages à la perfect competition. Bargaining options are often limited as most workers are new entrants, with temporary contracts, or on probation. This implies that the young inexperienced workers often lack outside options that could be used in negotiations. The search model with sequential bargaining predicts that employment should be less heterogeneous in that environment, and wages are also less affected. Constraints on wage setting could be also different for young and old. For instance, passing through the effect of the policy on young workers would mean paying more at age 24 and then less at age 25. This wage cut is probably less feasible than the wage increase once someone reached age 55.

Interestingly, when we focus on young but experienced workers we find more similarities to the observed pattern for old workers. Table C.4.23 shows that there is large heterogeneity in employment responses among experienced younger workers (those who enter the labor market around age 18), while we find limited heterogeneity among non-experienced younger workers who are entering the labor market at later ages.

Table C.4.23: Impact on employment by experience: younger workers

	(1)	(2)	(3)
		Employment	
	All Firms	Low TFP	High TFP
All workers	0.0162***	0.0092***	0.0070***
	[0.0011]	[0.0006]	[0.0007]
	⟨0.3171⟩	⟨0.1421⟩	⟨0.1750⟩
Experienced workers	0.0110***	0.0164***	-0.0054***
	[0.0020]	[0.0011]	[0.0018]
	⟨0.4821⟩	⟨0.2311⟩	⟨0.2510⟩
Non-experienced workers	0.0221***	0.0111***	0.0111***
	[0.0012]	[0.0007]	[0.0007]
	⟨0.3002⟩	⟨0.1330⟩	⟨0.1672⟩

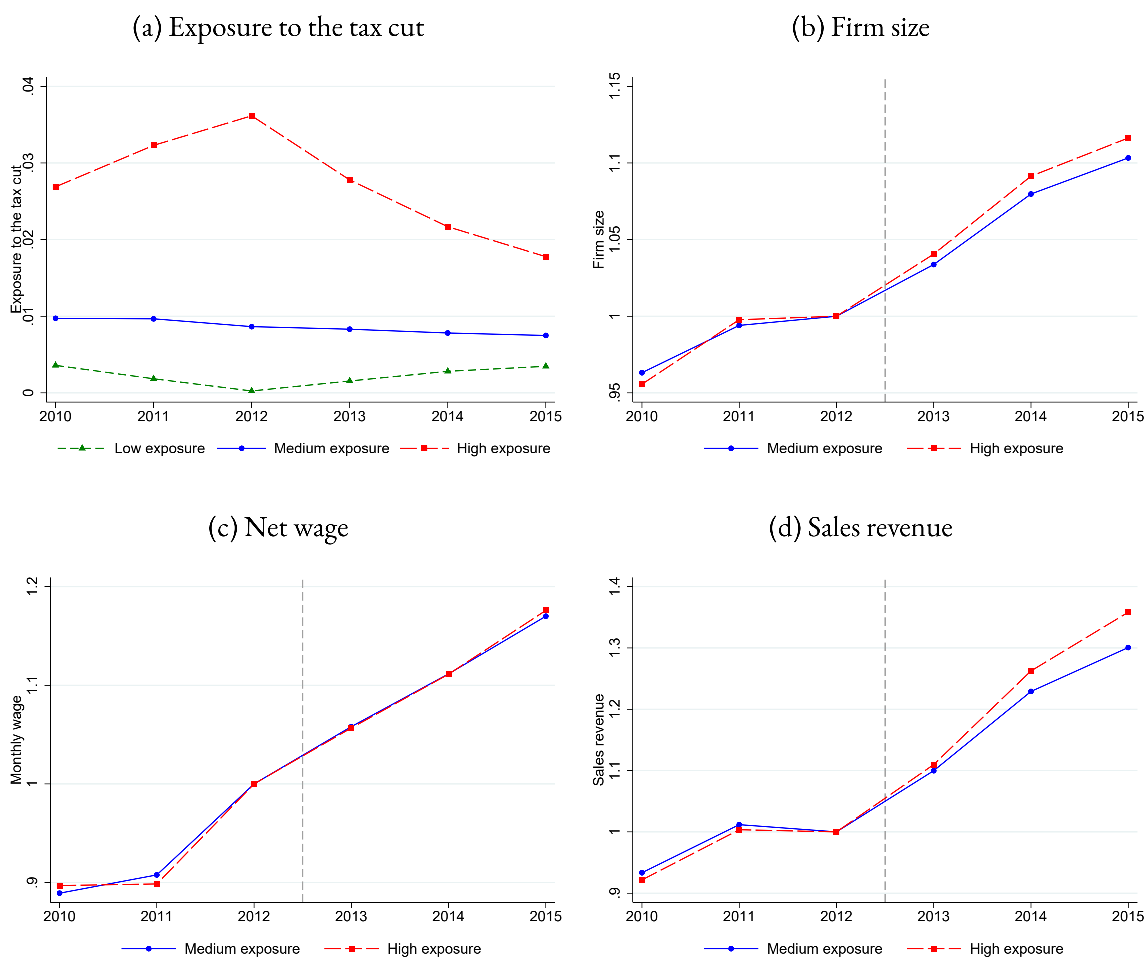
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Note: Table shows difference-in-differences estimates based on equation (3.1). We compare the change in employment among the 22 to 24 age group that was affected by the payroll tax cut with the change in employment among the 25 to 27 age group that was not affected by the tax cut. The sample is further split by working at least 6 months at ages 18-19 ("experienced" vs. "non-experienced"). The employment rate in each of these categories (relative to the total population) in May 2012 is shown in angle brackets. Standard errors are reported in brackets, clustered at the age × period level. ($N = 8,611,542$ individual-months; experienced young: $N = 707,259$ individual-months; non-experienced young: $N = 8,004,351$ individual-months.)

C.4.5 Windfall effects.

We also assess potential windfall effects at firms that already employed many younger workers from the treatment age group (below age 25) before the tax cut was implemented, following the strategy of Saez et al. (2019). We compare firms that have a high share of treated workers below age 25 with firms that have a medium share in 2012 (last pre-reform year), the same exercise as for older workers in Appendix Figure C.1.4. Again, Panel (a) of Figure C.4.10 indicates mean reversion in the exposure to the tax cut (ratio of the windfall revenues to the total payroll) and net wages trend similarly for firms with high and medium shares of younger treated workers (Panel (c)). However, we see some divergence in the evolution of firm size and sales revenue (Panel (b) and (d) of Figure C.4.10); both of them grew faster at firms with high exposure, suggesting a small positive impact of a larger tax windfall on growth. These figures are similar to the findings of Saez et al. (2019) on the young workers' tax cut in Sweden, suggesting that responses to a tax cut have many similar features in the two countries and economic environments. At the same time, the figures differ from what we found for older workers. This suggests that the windfall effects documented by Saez et al. (2019) might be less relevant for firms employing older workers.

Figure C.4.10: Firm-level effects of payroll tax cuts of younger workers



Note: Figure applies the same analysis for younger workers as Appendix Figure C.1.4 for older workers.

C.5 The effect of tax cuts in different labor market models

We present details of the models summarized in Section 3.2.

C.5.1 Search and matching with zero bargaining power of workers

First, we illustrate the impact of payroll taxes in the presence of search frictions. We introduce a tax cut in a framework with random search, heterogeneous firms, and sequential bargaining on wages (Postel-Vinay & Robin, 2002).

C.5.1.1 Setup Firms are heterogeneous and characterized by productivity $y \in [y_{min}, y_{max}]$, with continuous cumulative distribution function $\Psi(\cdot)$. Workers are homogeneous. Workers are either unemployed or employed. If unemployed, they receive leisure of value b (with $b < y_{min}$) and search for jobs with probability one. If employed, they receive wage w, search for a new job with probability $s \in [0, 1]$ and can separate from their job exogenously with probability $\delta \in [0, 1]$.

Firms advertise vacancies at an increasing and convex cost $\kappa(\cdot)$. Job market tightness is the ratio between total vacancies (v) and total search effort by the unemployed (u) and employed ($(1-\delta)(1-u)$):

$$\theta = \frac{v}{u + s(1-\delta)(1-u)}. \tag{C.5.1}$$

A searching worker locates an open vacancy with probability $\phi(\theta)$, increasing in θ. The probability for an open vacancy to meet a worker who is searching for jobs is $\phi(\theta)/\theta$, decreasing in θ.

Wage setting is based on sequential auction as in Postel-Vinay and Robin (2002). When an employed worker contacts an open vacancy, the prospective poacher and the incumbent employer observe each other's match qualities with the worker, and engage in Bertrand competition over contracts. The worker chooses the contract that delivers the larger value. First, we discuss the case when all the bargaining power is at the firms and so they are able to extract all rents from the workers (see e.g. Postel-Vinay and Robin, 2002 and Moscarini and Postel-Vinay, 2018).

C.5.1.2 Bellman equations The value of unemployment is the following:

$$V_u = b + \beta V_u, \tag{C.5.2}$$

where β is the discount factor. Thus,

$$V_u = \frac{b}{1-\beta}. \tag{C.5.3}$$

Notice that the probability of finding a job does not show up in the value of unemployment, which comes from the assumption that firms have all the bargaining power. We will relax that assumption later. Note also that employed workers will benefit from job offers as the competition between firms will drive up their wages.

Now we turn to specify the joint value to the firm and the worker from a match:

$$V(y,\tau) = y + \tau + \delta\beta V_u + (1-\delta)\beta V(y,\tau), \tag{C.5.4}$$

where τ is the lump-sum tax cut (we assume that $b + \tau < y_{min}$). Note, that since we assume that all the bargaining power is at the firms, the joint value of the match goes to the firm.

Firms need to post vacancies to find workers. The value of posting vacancies will be the following:

$$\begin{aligned} V_v(y,\tau) = \max_{v} \Bigg\{ & -\kappa(v) + \beta v \frac{\phi(\theta)}{\theta} \Bigg(P(u)\Big[V(y,\tau) - V_u\Big] + \\ & + (1-P(u)) \int_{y_{min}}^{y} \Big[V(y,\tau) - V(y',\tau)\Big] d\Gamma(y') \Bigg) \Bigg\} \end{aligned} \tag{C.5.5}$$

where $-\kappa(v)$ is the cost of posting v vacancies, which leads to $v\phi(\theta)/\theta$ chance to be matched to an applicant. In the value function above,

$$P(u) = \frac{u}{(u + (1-\delta)s(1-u))} \tag{C.5.6}$$

reflects the probability that a randomly drawn applicant is unemployed, which leads to the $V(y,\tau) - V_u$ profits, given that firms can extract all the surplus from the match. The chance that a randomly drawn applicant is employed is $1 - P(u)$ and the benefit of this from the firm's perspective depends on the previous employer of the applicant. If the applicant works at a more productive firm, then the firm cannot attract that worker and so there is no benefit from being matched to that applicant. That is why the integral goes only to y in the above formula. Nevertheless, if the firm meets with an applicant employed at a firm with lower productivity y', then the firm can poach that worker and acquire the difference between the new surplus ($V(y,\tau)$) and the surplus at the previous firm ($V(y',\tau)$). The chance that the firm meets with an employed worker at firm y' depends on the vacancy distribution function

$$\Gamma(y) = \frac{\int_{y_{min}}^{y} v(y', \tau) d\Upsilon(y')}{\int_{y_{min}}^{y_{max}} v(y', \tau) d\Upsilon(y')}, \tag{C.5.7}$$

where $v(y, \tau)$ is the optimal choice of vacancy of a firm y at tax cut level τ.

Plugging in $V(y, \tau)$ (equation (C.5.4)) and V_u (equation (C.5.2)) into equation (C.5.5), leads to:

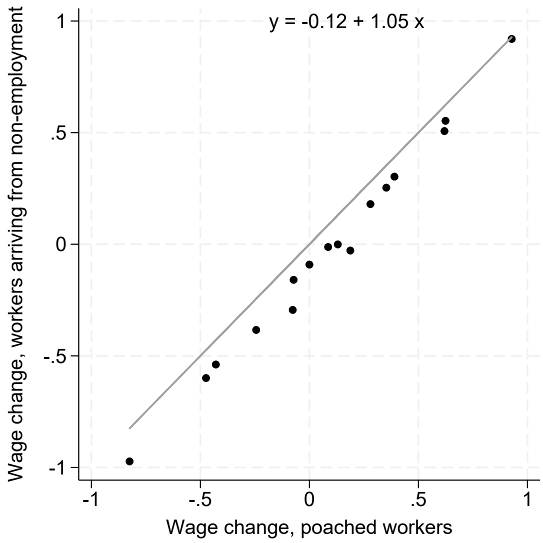
$$\begin{aligned} V_v(y, \tau) = \max_{v} \Bigg\{ & \underbrace{-\kappa(v)}_{\text{Cost of vacancy}} + \underbrace{v\frac{\phi(\theta)}{\theta}P(u)}_{\text{Probability of meeting unemployed}} \times \underbrace{\beta\left[\frac{y+\tau}{1-\beta+\delta\beta} - \frac{1-\delta\beta}{1-\beta+\delta\beta}\frac{b}{1-\beta}\right]}_{\text{Benefit of meeting with unemployed}} + \\ & + \underbrace{v\frac{\phi(\theta)}{\theta}(1-P(u))}_{\text{Probability of meeting employed}} \times \underbrace{\beta\int_{y_{min}}^{y} \frac{y-y'}{1-\beta+\delta\beta}\Gamma(y')}_{\text{Benefit of meeting employed}} \Bigg\}. \end{aligned} \tag{C.5.8}$$

This equation highlights the key trade-offs firms face when they decide about posting a vacancy. The first part reflects the cost of posting. The second part reflects the (expected) benefit of meeting an applicant who is unemployed, while the third part reflects the (expected) benefit of meeting with an applicant who is employed. The equation also highlights the key channels through which payroll taxes affect vacancy posting and employment. In particular, the tax cut only appears in the second part of this equation, which reflects the benefits of hiring from unemployment. At the same time, the tax cut has no impact on the third part of the value of vacancy posting, hiring from employment, as all firms receive the tax cut and the competition for workers will shift the surplus from the firms to the worker. Note that this shift in incidence of the policy will take place even if firms have all the bargaining power.

The equation, therefore, highlights that the tax cut increases the benefit of hiring from unemployment, while it has no effect on hiring from employment. It is worth noting that the model predicts a difference between hiring from employment and unemployment. In Appendix Figure C.5.11 we provide an indicative test of this prediction, which is a replication of Figure 1 of Di Addario et al. (2023). We use the same data as in our main analysis and restrict the sample to men aged 52-57 when entering their second job (corresponding to the age group which is the focus of our analysis). Following Di Addario et al. (2023), we plot the mean residualized change in log hiring wages between the first and second job of workers who arrived from non-employment to the second job against the mean residualized change of those who arrive from employment to the second job (i.e., poached workers).

The figure indicates that non-employment implies an average penalty of 12% on subsequent hiring wages. This is twice the penalty estimated by Di Addario et al. (2023). The slope of the fitted line is 1.05, which indicates that the non-employment penalty is similar across the wage distribution of firms.

Figure C.5.11: Hiring wage penalty for non-employment



Note: This figure replicates Figure 1 of Di Addario et al. (2023). The figure shows the mean change of residualized log hiring wage changes of workers arriving from non-employment (y-axis), as function of the mean change of residualized log hiring wage changes of workers poached from other firms (x-axis). Each point corresponds to a different pair of quartile of coworker wages at the first and second job. The continuous gray line is a 45-degree line. To create the figure, we use observations from 2009-2015 and restrict the sample to men. We consider wages earned in the private sector, deflated by aggregate real wage growth. We calculate the change in log hiring wages between the first and second job of workers. Here the first job is a job to which a worker entered from non-employment, and the second job is the next employment at a different firm. We consider job entries which were at most 5 years apart. We further restrict the sample to men aged 52-57 when entering the second job (corresponding to the age group which is in the focus of our analysis). Hiring wage is the average wage over the first 12 months of employment at the new job (or of fewer months if the employment lasted for less than 12 months). We perform a 90% winsorization on log hiring wages, and with an OLS regression, we net out the effect of age at entry at the first and second job, and monthly calendar date effects from the change in log hiring wages, and calculate the residuals.

C.5.1.3 Equilibrium Equilibrium is where firms optimally post vacancies up to the point where the marginal value of posting a vacancy equals its cost – they maximize equation (C.5.8). Furthermore, market tightness, θ, and the distribution of vacancies, $\Gamma(y)$, are consistent with firms' vacancy posting decisions.

The cumulative distribution of employment is $L(\cdot)$, with:

$$L(y) = (1-\delta)\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]L(y) + \phi(\theta)\Gamma(y)u, \tag{C.5.9}$$

where the first term on the right-hand side captures that part of employment that survives the exogeneous separation $(1-\delta)$ and is not poached by higher productivity firms $(1 - s\phi(\theta)(1-\Gamma(y)))$, whereas the second term $(\phi(\theta)\Gamma(y)u)$ captures the employment arriving from unemployment. Employment at firms with productivity y is the derivative of $L(y)$ with respect to y:

$$l(y) = (1-\delta)\left[\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]l(y) + s\phi(\theta)\gamma(y)\int_{y_{min}}^{y} l(y')dy'\right] + \phi(\theta)\gamma(y)u. \tag{C.5.10}$$

The steady-state rate of unemployment is:

$$u = (1-\phi(\theta))u + \delta(1-u). \tag{C.5.11}$$

Thus,

$$u = \frac{\delta}{\delta + \phi(\theta)}. \tag{C.5.12}$$

Firms maximize their profit and so they post vacancies up to the point where the marginal value of a vacancy is zero.

$$\kappa'(v(y,\tau)) = \beta\frac{\phi(\theta)}{\theta}\left(P(u)\left[\frac{y+\tau}{1-\beta+\delta\beta} - \frac{1-\delta\beta}{1-\beta+\delta\beta}\frac{b}{1-\beta}\right] + \right.$$
$$\left. + (1-P(u))\int_{y_{min}}^{y}\left[\frac{y-y'}{1-\beta+\delta\beta}\right]d\Gamma(y')\right). \tag{C.5.13}$$

The equilibrium solution of θ and $\Gamma(y)$ satisfies equations (C.5.1), (C.5.6), (C.5.7), (C.5.9), (C.5.12) and (C.5.13).

C.5.1.4 Wage The derivation of equilibrium wage levels is based on Postel-Vinay and Robin (2002).

Contracts can be renegotiated by mutual consent. If a worker of a firm with productivity y receives an outside offer from a firm with productivity y' then three events can occur:

1. *Worker is poached:* The poaching firm wins the competition over the incumbent firm if $y' > y$ and the wage increases.
2. *Wage renegotiation:* If the worker meets a firm that can deliver greater value than the current contract, but is less productive than the current firm, the contract is renegotiated and the worker stays.
3. *No change:* If neither of the above two conditions is met, the worker stays at the current firm and the wage remains unchanged.

The value of employment at firm of type y and at wage w is $V_e(w,y)$. A worker moves to a potentially better match with a firm type-y' if it offers at least the wage $\omega(y,y',\tau)$ defined by:

$$V_e(\omega(y,y',\tau),y) = V_e(y+\tau,y). \tag{C.5.14}$$

Lower offers are outbid by the type-y incumbent firm.

The Bellman equation for the value of employment is the following (corresponding to equation (16) of Postel-Vinay and Robin, 2002):

$$\underbrace{\left(\delta + \frac{1-\beta}{\beta} + s\phi(\theta)(1-\Gamma(q(w,y,\tau)))\right)}_{\text{Separation rate + discount rate + prob. of renegotiation or poaching}} \cdot \underbrace{V_e(w,y)}_{\text{Value of employment}} =$$
$$= \underbrace{U(w)}_{\text{Flow utility from wage}} + \underbrace{s\phi(\theta)\int_{q(w,y,\tau)}^{y} V_e(x+\tau,x)d\Gamma(x)}_{\text{Expected value from renegotiation}} +$$
$$+ \underbrace{s\phi(\theta)(1-\Gamma(y))V_e(y+\tau,y)}_{\text{Expected value from poaching}} + \underbrace{\delta V_u}_{\text{Expected value from job loss}}, \tag{C.5.15}$$

where $q(w,y,\tau)$ is the threshold productivity, defined by $\omega(q(w,y,\tau),y,\tau) = w$. In other words, $q(w,y,\tau)$ is the lowest productivity level y' such that competition between a type-y and a type-y' firm raises the wage above w (which equals y_{min} if $w = b$). $U(w)$ is the instantaneous utility flow from wage w. The second term on the right hand side of equation (C.5.15) captures the employment value after a wage increase at the current firm (reflecting that the incumbent firm needs to match the offer of the competitor), whereas the third term captures the value of employment at a higher productivity firm (after being poached, using equation (C.5.14)).

Assuming CRRA utility function with rate of relative risk aversion ζ ($U(x) = x^{1-\zeta}$), where $0 \leq \zeta < 1$, we can derive an expression for wages, following Appendix A.1. of Postel-Vinay and Robin (2002) and incorporating the tax cut (τ) into their model:

$$\ln \omega(y, y', \tau) = \frac{1}{1-\zeta} \ln \left[(y+\tau)^{1-\zeta} - \frac{(1-\zeta)s\phi(\theta)}{\frac{1-\beta}{\beta} + \delta} \int_{y}^{y'} (1-\Gamma(x))(x+\tau)^{-\zeta} dx \right]. \tag{C.5.16}$$

The wage of workers who have not been subject to wage bargaining yet is:

$$\ln \omega_u(y, \tau) = \frac{1}{1-\zeta} \ln \left[b^{1-\zeta} - \frac{(1-\zeta)s\phi(\theta)}{\frac{1-\beta}{\beta} + \delta} \int_{y_{min}}^{y} (1-\Gamma(x))(x+\tau)^{-\zeta} dx \right]. \tag{C.5.17}$$

The negative terms in the above two equations capture the option value of employment: workers accept lower wages to work at more productive firms because workers trade a lower wage now for increased chances of higher wages tomorrow (Postel-Vinay & Robin, 2002).

The equilibrium within-firm distribution of wages has two components, the employer effect (y) and a random effect (q) that characterizes the most recent wage mobility. We denote by $\tilde{G}(q|y)$ the cumulative distribution function of the conditional distribution of bargaining position within the pool of workers within type-y firms.

$$G(w|y) = \tilde{G}(q|y) = \frac{(1+\Upsilon(1-\Gamma(y)))^2}{(1+\Upsilon(1-\Gamma(q)))^2} \tag{C.5.18}$$

for all $q \in \{b\} \cup [y_{min}, y]$, where $\Upsilon = \phi(\theta)s/\delta$. Equation (C.5.18) is derived following the derivation on page 2341 of Postel-Vinay and Robin (2002).

C.5.1.5 Effects of the tax cut We now study the effect of changing the tax cut. We describe what happens to the steady-state equilibrium when we raise the tax cut amount.

First, let us point out that hiring intensity increases in firm productivity y because both the output and the acceptance rate increase with y in the right hand side of equation (C.5.13). Using that $\kappa(\cdot)$ is increasing in v leads us to Result C.5.1.5.

Hiring intensity is increasing in firm productivity: $\dfrac{\partial v(y,\tau)}{\partial y} > 0$.

Our next result follows directly from equation (C.5.13), using that $\kappa(\cdot)$ is increasing and convex in the amount of vacancies.

The partial effect of the tax cut (an increase in τ holding u constant) leads to more vacancy posting at all firms, formally $\dfrac{\partial v(y,\tau)}{\partial \tau} > 0$.

An immediate consequence of Result C.5.1.5 is that increased vacancy posting leads to tighter labor market. This itself lowers the equilibrium unemployment rate as it is shown in equation (C.5.12) (remember, $\phi(\theta)$ increases in θ).

Furthermore, equation (C.5.6) can be rewritten as:

$$P(u) = \frac{\delta}{\delta + (1-\delta)s\phi(\theta)}. \tag{C.5.19}$$

and so $P(u)$ will decrease as a consequence of the tax cut.

Note that the decrease in $P(u)$ has a feedback equilibrium effect on vacancy posting as it affects the right hand side of (C.5.13). Since the maximum value firms are willing to offer, $V(y', \tau)$, must be at least as high as the value of unemployment V_u, we have $V(y', \tau) \geq V_u$ for all y'. Notice that this implies that the left hand side of (C.5.13) will decrease, and so will vacancy posting, since $\kappa(\cdot)$ is increasing in v. Therefore, the equilibrium effect will dampen to some extent the immediate effect of the tax cut on vacancy posting. Nevertheless, we can rule out that the feedback effect is large enough to fully offset the initial increase in vacancy posting. To see that, assume the opposite is true and the feedback effect fully offsets the initial increase in vacancy posting. In such a situation there would be no feedback effect to begin with, leading to a contradiction.

As a consequence, the following result will be true:

The equilibrium unemployment rate (u) and the probability that a randomly drawn applicant is unemployed ($P(u)$) decrease in τ.

Now we turn to discussing the heterogeneity in response to the tax cut. Firms' optimality condition – equation (C.5.13) – implies that the change in the right hand side is the same for all types of firms in the absence of any equilibrium effects (i.e., unemployment rate is constant). Based on the convexity of the vacancy cost function $\kappa(\cdot)$ and using that $v(y, \tau)$ increases in y, it follows that the increase in vacancies ($v(y, \tau)$) is smaller at higher values of y.

To derive this result formally, we introduce the notation for the inverse of the first derivative of the cost function $\chi(\cdot) := (\kappa')^{-1}(\cdot)$. Using this notation, we can rewrite equation (C.5.13) as:

$$v(y,\tau) = \chi\left(\beta\frac{\phi(\theta)}{\theta}\left(P(u)\left[\frac{y+\tau}{1-\beta+\delta\beta} - \frac{1-\delta\beta}{1-\beta+\delta\beta}\frac{b}{1-\beta}\right] + \right.\right.$$
$$\left.\left. + (1-P(u))\int_{y_{min}}^{y}\left[\frac{y-y'}{1-\beta+\delta\beta}\right]d\Gamma(y')\right)\right). \tag{C.5.20}$$

It follows that

$$\frac{\partial^2 v(y,\tau)}{\partial\tau\partial y} = \chi''\left(\beta\frac{\phi(\theta)}{\theta}\left(P(u)\left[\frac{y+\tau}{1-\beta+\delta\beta} - \frac{1-\delta\beta}{1-\beta+\delta\beta}\frac{b}{1-\beta}\right] + \right.\right.$$
$$\left.\left. + (1-P(u))\int_{y_{min}}^{y}\left[\frac{y-y'}{1-\beta+\delta\beta}\right]d\Gamma(y')\right)\right)\cdot\beta\frac{\phi(\theta)}{\theta}P(u)\frac{1}{1-\beta+\delta\beta}. \tag{C.5.21}$$

In this formula the terms after the $\chi''(\cdot)$ expression are positive. Thus the sign of $\chi''(\cdot)$ needs to be determined:

$$\chi''(x) = ((\kappa')^{-1})''(x) = \left(\frac{1}{\kappa'(z)}\right)' = -\kappa''(z) < 0, \tag{C.5.22}$$

where $z = (\kappa')^{-1}(x)$ and in the last step we used the convexity of the $\kappa(\cdot)$ function. This leads us to Result C.5.1.5. The partial effect of the tax cut on vacancy posting decreases with firm productivity, formally $\frac{\partial^2 v(y,\tau)}{\partial\tau\partial y} < 0$.

Result C.5.1.5 implies that the partial effect of the policy is that employment increases more at low-quality firms than at high-quality firms. However, some of these effects will be offset by the decrease in the unemployment rate. The lower unemployment rate affects more negatively the low-quality firms than the high-quality ones (this can be seen from equation (C.5.5)). Unfortunately, it is not possible to derive analytically the equilibrium effect of the tax cut on the employment rate. In Section C.5.1.6, we provide simulation-based evidence that the equilibrium effects are small in practice and the derived partial effects dominate.

Turning to the impact of the tax cut on wages, we use equation (C.5.16) to derive the partial effect of the tax cut on the wage of workers who have been poached or had a wage bargaining. To simplify notation, let's use the shorthand notation $\Omega = \left[(y+\tau)^{1-\zeta} - \frac{(1-\zeta)s\phi(\theta)}{\frac{1-\beta}{\beta}+\delta}\int_y^{y'}(1-\Gamma(x))(x+\tau)^{-\zeta}dx\right]$.

$$\frac{\partial \ln \omega(y,y',\tau)}{\partial y'} = \frac{1}{1-\zeta}\frac{1}{\Omega}\left[-\frac{(1-\zeta)s\phi(\theta)}{\frac{1-\beta}{\beta}+\delta}(1-\Gamma(y'))(y'+\tau)^{-\zeta}\right]. \tag{C.5.23}$$

From this, we derive how the partial effect of the tax cut varies with firm productivity:

$$\frac{\partial^2 \ln \omega(y,y',\tau)}{\partial\tau\partial y'} = \frac{1}{1-\zeta}\frac{1}{\Omega^2}\frac{(1-\zeta)s\phi(\theta)}{\frac{1-\beta}{\beta}+\delta}\left[\Omega\zeta(1-\Gamma(y'))(y'+\tau)^{-\zeta-1} + (1-\Gamma(y'))(y'+\tau)^{-\zeta}\frac{\partial\Omega}{\partial\tau}\right], \tag{C.5.24}$$

which is clearly non-negative (positive except for at $y' = y_{max}$, where it reaches zero), using that $0 \leq \zeta < 1$. Note also that based on (C.5.16), the partial effect of the tax cut on the logarithmic wage and wage level of incumbents is positive at all levels of y and y'. We focus on the effect of the tax cut on the level of the wage, because in the empirical application, we estimate the effect of the tax cut *rate* on the log wage, which corresponds to the effect of the tax cut on the wage level. Let's denote by $\bar{\omega}(y',\tau)$ the average wage at a firm with productivity y'. Equation (C.5.24) shows that the impact of the tax cut on log wages, given y, increases with firm productivity. Ignoring the impact of the tax cut on the composition of incumbents, it follows that the impact of the tax cut on $\bar{\omega}(y',\tau)$ also increases with firm productivity. Therefore,

$$0 \leq \frac{\partial^2 \ln \bar{\omega}(y',\tau)}{\partial\tau\partial y'} = \frac{1}{\bar{\omega}}\frac{\partial^2\bar{\omega}(y',\tau)}{\partial\tau\partial y'} - \frac{1}{\bar{\omega}^2}\frac{\partial\bar{\omega}(y',\tau)}{\partial\tau}\frac{\partial\bar{\omega}(y',\tau)}{\partial y'}. \tag{C.5.25}$$

Based on (C.5.16), $\frac{\partial\omega(y,y'\tau)}{\partial\tau} > 0$, therefore, ignoring composition effects, $\frac{\partial\bar{\omega}(y'\tau)}{\partial\tau} > 0$. It follows that the non-negativity of $\frac{\partial\bar{\omega}(y',\tau)}{\partial y'}$ is sufficient for $\frac{\partial^2\bar{\omega}(y',\tau)}{\partial\tau\partial y'}$ being also non-negative. Under standard assumptions (see pages 2317-2318 of Postel-Vinay and Robin, 2002), the non-negativity of $\frac{\partial\bar{\omega}(y',\tau)}{\partial y'}$ holds. This leads us to Result C.5.1.5.

Ignoring the impact of the tax cut on the composition of incumbents at a firm, the effect of the tax cut on wages is on average positive for workers who already had a wage bargaining or have been poached. This effect increases with firm productivity $\left(\frac{\partial^2\bar{\omega}(y',\tau)}{\partial\tau\partial y'} \geq 0\right)$.

The wages at the lowest productivity firm are determined by equation (C.5.17), because once an employer receives an alternative offer she is poached by the competing (more productive) firm. As the option value is zero at the lowest productivity firms, the partial effect of the tax cut on wages is also zero for workers at the lowest productivity firms.

At firms above the lowest productivity, the partial effect of the tax cut on the wage of workers who had not had a wage bargaining is positive (the same reasoning applies as for the wage of the incumbents). Whether this positive effect increases with firm productivity depends on the relative role of the option value, since due to the option value, $\frac{\partial \omega_u(y,\tau)}{\partial y} < 0$. Therefore, even though $\frac{\partial \ln \omega_u(y,\tau)}{\partial \tau \partial y} \geq 0$ holds, it does not necessarily follow that $\frac{\partial \omega_u(y,\tau)}{\partial \tau \partial y} \geq 0$ is also satisfied.

The partial effect of the tax cut on wages of workers arriving from unemployment (who have not had a wage bargaining) is zero at the lowest productivity firms and positive at higher productivity levels: $\frac{\partial \omega_u(y, \tau)}{\partial \tau} = 0$ if $y = y_{min}$ and $\frac{\partial \omega_u(y, \tau)}{\partial \tau} > 0$ if $y > y$
min.

The equilibrium effect of the tax cut on wages cannot be derived analytically. First, its positive effect on $\phi(\theta)$ increases the negative wage implications of the option value in equations (C.5.16) and (C.5.17). On the other hand, we know from from Result C.5.1.5 that the tax cut shifts the distribution of vacancies towards less productive firms, thus $(1 - \Gamma)$ decreases as a consequence of the tax cut but this decreasing effect varies with firm productivity.

Note also that the wages of new entrants are driven by equation (C.5.17). Intuitively, younger workers enter the labor market as non-employed, thus, essentially, poaching and wage renegotiation are not relevant for them. This means that new entrants cannot use current wages as an outside option to achieve full surplus extraction – instead, they accept any offer (as the reservation threshold of firm productivity is zero), and can start bargaining over wages once employed. Also, the firm heterogeneity in the employment effects of the tax cut is smaller if all workers are new entrants since then low- and high-productivity firms hire from unemployment to the same extent, thus low-productivity firms no longer benefit disproportionately more from the tax cut.

C.5.1.6 Simulation In the search and matching framework with sequential bargaining, we quantify the impact of a tax cut that is 6% of the average wage in the economy. We assume that all bargaining power is at firms. The functional forms used in the simulations are the following. The cost function, based on Bagger and Lentz (2019) is:

$$\kappa(v(y, \tau)) = \frac{v(y, \tau)^{(1+1/c_v)}}{1 + 1/c_v},$$

where $c_v > 0$ determines curvature. The job-finding rate is similar to Moscarini and Postel-Vinay (2018): $\phi(\theta) = A\theta^{\alpha}$.

The parameters used in the simulations are the following:

- The tax cut is 6% of the average wage without tax: $\tau = \bar{w}_0 \times 0.06$.
- y has $Pareto(\lambda, y_{min})$ distribution, where λ is the scaling parameter and y_{min} is a drift that shifts the original Pareto distribution, such that the lower bound is equal to y_{min}. During the simulations $\lambda = 1.25$ and $y_{min} = 1000$.
- $\zeta = 0.95$, which is the exponent in the CRRA utility function, implying close to log-utility. The simulation results are robust to different ζ values. It primarily has an effect on the wage change.
- $A = 1/4$, to calibrate an unemployment rate of around 20%.
- $\alpha = 1/2$, similar to Moscarini and Postel-Vinay (2018).
- The employment-to-employment transition rate (EE) is 0.041, which is in line with the empirical data for Hungary (12-month transition rate between employers among the continuously

working older workers). The searching intensity (s) is a direct mapping of this parameter, see the derivations in Moscarini and Postel-Vinay (2018). To obtain s, we solve for:

$$\phi(\theta)(1-\delta)\delta s \int_0^1 \frac{1-x}{\delta + (1-\delta)s\phi(\theta)x} dx = EE. \tag{C.5.26}$$

- $\beta = 0.95$, which matches the monthly value of $0.95^{1/12}$ by Moscarini and Postel-Vinay (2018).
- $b = y_{min} = 1000$, thus the workers' outside option is the same as the output of the lowest productivity firm.
- $c_v = 0.006$, similarly to Bagger and Lentz (2019).
- Job destruction rate $\delta = 0.1$, corresponding to the 12-month separation rate observed in the data for Hungary.

Table C.5.24 displays the simulated impact of the tax cut on unemployment, job market tightness and job finding rate. The rate of unemployment decreases by 1.7 percentage points from its baseline rate of 22.3%. At the same time, both job market tightness and job finding rate increase as a consequence of the tax cut.

Table C.5.24: Steady-state parameters

Tax cut	0%	6%	Δ (15%)
Unemployment	0.223	0.206	-0.017
Job market tightness (θ)	1.935	2.380	0.445
Job finding rate	0.348	0.386	0.038

The tax cut increases the vacancy posting activities of firms. In line with our theoretical considerations, the impact is bigger at low-productivity firms. At low-productivity firms, the vacancies posted increase by 12%, whereas at more productive firms only by 8.3%. These simulated impacts are slightly higher if we ignore the equilibrium effects in the model. As a consequence of the increased vacancy posting activities, employment at less productive firms increases by 3.7%, while employment at more productive firms increase by 0.8%.

Turning to wages, the wage impact of the tax cut for workers who were not employed the previous period is essentially zero, while it is 2.3% for the rest of the workers ("incumbents"). Finally, among incumbent workers, the wage effect is small (0.8%) at low-productivity firms, whereas it is larger (2.9%) at high-productivity firms.

C.5.2 Search and matching with non-zero bargaining power of workers

In our baseline model presented in section C.5.1, we assumed that all the bargaining power is at firms, therefore they are able to extract all rents from the workers. Now, following Cahuc et al. (2006), we allow workers to have bargaining power. Also, as in Cahuc et al. (2006), we assume linear utility function ($U(x) = x$).

We follow the notation of our baseline model and denote by λ the bargaining power of workers.

The value of unemployment is the following:

$$V_u(\tau) = b + \beta\phi(\theta)\lambda \int_{y_{min}}^{y_{max}} V(x,\tau)d\Gamma(x) + \beta\phi(\theta)(1-\lambda)V_u(\tau) + \beta(1-\phi(\theta))V_u(\tau) =$$

$$= b + \beta\phi(\theta)\lambda \int_{y_{min}}^{y_{max}} V(x,\tau)d\Gamma(x) + \beta(1-\lambda)V_u(\tau), \quad \text{(C.5.27)}$$

where b is the value of leisure received when unemployed, β is the discount factor, $\phi(\theta)$ is the probability of locating an open vacancy, $y \in [y_{min}, y_{max}]$ is firm productivity, $\Gamma(\cdot)$ is vacancy distribution, and τ is the lump-sum tax cut. This expression differs from the value of unemployment in our baseline model (equation (C.5.2)) in that now, due to the presence of bargaining power, the value from a match is included in the value of unemployment. Since the value of the match increases in the tax cut, it also implies that the value of unemployment is positively related to the tax cut.

The joint value to the firm and the worker from a match is:

$$V(y,\tau) = y + \tau + \delta\beta V_u + (1-\delta)\beta V(y,\tau) + \lambda(1-\delta)\beta s\phi(\theta) \int_{y}^{y_{max}} (V(x,\tau) - V(y,\tau))d\Gamma(x), \quad \text{(C.5.28)}$$

where δ is the separation probability, s is the probability of job search if employed. The last term on the right hand side is new compared to the no-bargaining-power value function (equation (C.5.4)), reflecting the value workers derive from job offers.

The value of posting vacancies is the same as before (equation (C.5.5)), except for the benefit from posting a vacancy is now multiplied by $(1-\lambda)$:

$$V_v(y,\tau) = \max_{v} \Bigg\{ -\kappa(v) + \beta v\frac{\phi(\theta)}{\theta}(1-\lambda)\Bigg[P(u)\Big(V(y,\tau) - V_u(\tau)\Big) +$$

$$+ (1-P(u)) \int_{y_{min}}^{y} \Big(V(y,\tau) - V(x,\tau)\Big)d\Gamma(x)\Bigg]\Bigg\}. \quad \text{(C.5.29)}$$

As before, the tax cut has no impact on the last part of the value of vacancy posting, hiring from employment, as all firms receive the tax cut and the competition for workers will shift the surplus from the firms to the worker. The tax cut affects the benefits of hiring from unemployment. However, since τ increases $V_u(\tau)$, this benefit is smaller than when workers have no bargaining power.

Based on equation (A.15) in Cahuc et al. (2006), the equilibrium wage of worker at type-y' firm previously employed at type-y firm is:

$$\omega(y,y',\tau) = \lambda(y'+\tau) + (1-\lambda)(y+\tau) - (1-\lambda)^2 s\phi(\theta) \int_{y}^{y'} \frac{(1-\Gamma(x))}{\frac{1-\beta}{\beta} + \delta + s\phi(\theta)\lambda(1-\Gamma(x))}dx. \quad \text{(C.5.30)}$$

Therefore, without considering the equilibrium effects, there is a full pass-through of the tax cut to the wage of poached workers. The equilibrium wage of a worker arriving from unemployment is (based on equation (A.17) of Cahuc et al., 2006):

$$\omega_u(y,\tau) = \lambda(y+\tau) + (1-\lambda)y_{min} - (1-\lambda)^2 s\phi(\theta) \int_{y_{min}}^{y} \frac{(1-\Gamma(x))}{\frac{1-\beta}{\beta} + \delta + s\phi(\theta)\lambda(1-\Gamma(x))}dx. \quad \text{(C.5.31)}$$

Since workers have some bargaining power, the tax cut also increases the wage of workers arriving from unemployment, even without considering the general equilibrium effects.

To summarize, in a model à la Cahuc et al. (2006), firms still get surplus from the tax cut if they hire from unemployment, but less than if all bargaining power were at firms. As in our baseline model, since low-productivity firms tend to hire from unemployment, they will benefit disproportionately more from the tax cut. Competition between firms implies that the tax cut will benefit the workers more if they are poached or if they received an offer from another firm. However, the relative benefit compared to being hired from unemployment is smaller if workers have some bargaining power.

C.5.3 Search and matching with wage posting

We build on the wage posting model of Burdett and Mortensen (1989) and Burdett and Mortensen (1998), and follow specifically the framework of Bontemps et al. (1999) and Bontemps et al. (2000). This is an equilibrium search model, in which each firm selects a specific wage and offers that wage to any worker it meets. Importantly, in this model, firms do not re-negotiate with workers who find a better-paying job – this is a key difference from our baseline model.

There are L identical workers and N heterogeneous firms. The exogenous match destruction rate is δ. The arrival rate of job offers is ϕ_0 for the unemployed and ϕ_1 for the employed. The distribution of wage offers is $\Gamma(\cdot)$, and the reservation wage is ω_r. The discount rate is ρ. Firms are heterogeneous and characterized by productivity $y \in [y_{min}, y_{max}]$, with continuous cumulative distribution function $\Psi(\cdot)$.

In this setting, firms offer $\omega(y)$ to maximize profits, where τ is the tax cut and $l(\omega)$ is the number of workers:

$$(y + \tau - \omega)l(\omega). \tag{C.5.32}$$

The least productive firm (y_{min}) offers ω_r: $w(y_{min}) = \omega_r$.

Following the derivations of Bontemps et al. (1999) and Bontemps et al. (2000), equilibrium outcomes of this model are the following. Employment is

$$l(\omega) = \frac{L}{N}\frac{1 + \frac{\phi_1}{\delta}}{(1 + \frac{\phi_1}{\delta}(1 - \Gamma(\omega))^2}. \tag{C.5.33}$$

The reservation wage is

$$\omega_r = b + (\phi_1 - \phi_0)\int_{\omega_r}^{\infty} \frac{1 - \Gamma(\omega)}{\rho + \delta + \phi_1(1 - \Gamma(\omega))}d\omega, \tag{C.5.34}$$

where b is the unemployment benefit. The equilibrium wage is

$$\omega(y) = y + \tau - (1 + \frac{\phi_1}{\delta}(1 - \Psi(y))^2\left(\int_{y_{min}}^{y} \frac{1}{(1 + \frac{\phi_1}{\delta}(1 - \Psi(x))^2}dx + \frac{y_{min} + \tau - \omega_r}{(1 + \frac{\phi_1}{\delta})^2}\right), \tag{C.5.35}$$

with $\Gamma(\omega(y)) = \Psi(y)$.

It follows from the wage equation that the effect of τ on $\omega(y)$ at the least productive firm is 0 (using that $\Psi(y_{min}) = 0$), and at the most productive firm is $1 - \dfrac{1}{(1 + \frac{\phi_1}{\delta})^2}$.

In this model, the wage offer distribution remains unchanged even if τ changes, because of the monotonicity of its effect on wage. It follows not only that the reservation wage remains unchanged, but employment is also unaffected by the tax cut.

C.5.4 Monopsonistic competition

We follow Card et al. (2018) in presenting a model with monopsonistic competition, with the difference that we assume homogeneous workers. This is a model with differentiated products, which endows firms with power to set wages. Importantly, unlike in our baseline model, firms do not observe workers' outside options. As in Card et al. (2018), workers are fully informed about job opportunities and firms hire any worker who is willing to accept a job at the posted wage.

The utility of worker i from working at firm j is

$$U_{ij} = \lambda \ln(\omega_j - b) + {}_{ij}, \tag{C.5.36}$$

where b is a reference wage level, and the ${}_{ij}$ are independent draws from a type-I extreme value distribution. Workers then have logit choice probabilities of working at firm j:

$$p_j = \frac{(\omega_j - b)^\lambda}{\sum_{k=1}^{J}(w_k - b)^\lambda}, \tag{C.5.37}$$

with J denoting the number of firms in the market. Assuming that the number of firms is large, the firm-specific labor supply function is

$$\ln l(\omega_j) = \ln(p_j \cdot L) = \ln(C) + \lambda \ln(\omega_j - b), \tag{C.5.38}$$

where C is common to all firms in the market:

$$C = L\Big(\sum_{k=1}^{J}(w_k - b)^\lambda\Big)^{-1}, \tag{C.5.39}$$

Note, that aggregate labor supply is inelastic – aggregate labor supply equals to L:

$$\sum_{k=1}^{J} l(\omega_k) = C\sum_{k=1}^{J}(\omega_k - b)^\lambda = L. \tag{C.5.40}$$

The elasticity of firm-level labor supply is

$$e_j = \frac{\lambda \omega_j}{\omega_j - b}, \tag{C.5.41}$$

which is decreasing in w_j (higher paying firms face a more inelastic labor supply).

Firms' production function is $Y_j = y_j f(l(\omega_j))$, where y_j is productivity. Firms solve the cost minimization problem, where τ is the tax cut:

$$\min_{\omega_j}(\omega_j - \tau)l(\omega_j) \text{ such that } y_j f(l(\omega_j)) \geq Y. \tag{C.5.42}$$

The first-order condition equates the marginal factor cost to the marginal revenue product:

$$(\omega_j - \tau)\frac{1 + e_j}{e_j} = y_j f_l \square_j, \tag{C.5.43}$$

where e_j is the elasticity of labor supply, and $\square_j$ is the marginal cost of production which is equal to marginal revenue at the optimal Y. To simplify the following derivations, we make two assumptions.

First, we assume that the production function is linear in $l(\omega_j)$, therefore $f_l = 1$. Second, we assume that the marginal revenue is a fixed constant (i.e., there is a fixed output price), normalized to one. Using these simplifying assumptions and plugging in the elasticity of labor supply formula,

$$(\omega_j - \tau)\frac{\omega_j - b + \lambda\omega_j}{\lambda\omega_j} = y_j. \tag{C.5.44}$$

After rearrangement,

$$\omega_j \frac{1+\lambda}{\lambda} - \frac{b}{\lambda} - \tau\left(\frac{1+\lambda}{\lambda} - \frac{b}{\lambda\omega_j}\right) = y_j, \tag{C.5.45}$$

$$\omega_j = y_j \frac{\lambda}{1+\lambda} + \frac{b}{1+\lambda} + \tau\left(1 - \frac{b}{(1+\lambda)\omega_j}\right). \tag{C.5.46}$$

This leads to the quadratic equation:

$$\omega_j^2 - \left(y_j \frac{\lambda}{1+\lambda} + \frac{b}{1+\lambda} + \tau\right)\omega_j + \frac{\tau b}{1+\lambda} = 0. \tag{C.5.47}$$

Using that $\omega_j \geq b$, the unique viable solution of the wage equation is:

$$\begin{aligned} \omega_j = \frac{1}{2}\Bigg[& y_j \frac{\lambda}{1+\lambda} + \frac{b}{1+\lambda} + \tau + \\ & + \left(\left(y_j \frac{\lambda}{1+\lambda}\right)^2 + \left(\frac{b}{1+\lambda}\right)^2 + \tau^2 + y_j \frac{2\lambda b}{(1+\lambda)^2} + \frac{2\tau}{1+\lambda}(y_j\lambda - b) \right)^{1/2} \Bigg]. \end{aligned} \tag{C.5.48}$$

Differentiating the wage equation with respect to τ shows that the impact of the tax cut on the wage is positive and the pass-through rate is between 0 and 1:

$$\frac{\partial \omega_j}{\partial \tau} = \frac{1}{2} + \frac{1}{2}\left[\frac{\left(\tau + \frac{y_j\lambda - b}{1+\lambda}\right)^2}{\left(\tau + \frac{y_j\lambda - b}{1+\lambda}\right)^2 + \frac{4y\lambda b}{(1+\lambda)^2}} \right]^{1/2}, \tag{C.5.49}$$

Also, the pass-through rate of τ increases in firm productivity y_j if $y_j\lambda + b > \tau(1+\lambda)$, which holds if τ is relatively small.

Turning to the impact of the tax cut on employment, we use the labor supply result that $l(\omega_j) = C(\omega_j - b)^\lambda$, and plug in the above solution for the wage. We assume that the number of firms (J) is large and the impact of y_j on C is (approximately) zero. With this assumption,

$$\begin{aligned} \frac{\partial^2 l(\omega_j)}{\partial \tau \partial y_j} &= \frac{\partial\left(C\lambda(\omega_j - b)^{\lambda-1}\frac{\partial\omega_j}{\partial y_j}\right)}{\partial \tau} = \\ &= C\lambda(\omega_j - b)^{\lambda-1}\frac{\partial^2\omega_j}{\partial\tau\partial y_j} + C\lambda(\lambda-1)(\omega_j - b)^{\lambda-2}\frac{\partial\omega_j}{\partial y_j}\frac{\partial\omega_j}{\partial\tau} + \frac{\partial C}{\partial\tau}\lambda(\omega_j - b)^{\lambda-1}\frac{\partial\omega_j}{\partial y_j}. \end{aligned} \tag{C.5.50}$$

Card et al. (2018) argue that a labor supply elasticity of 4 is in line with a supply-side parameter of $\lambda \approx 0.08$, thus the $\lambda < 1$ assumption is reasonable. Under this assumption, if the wage effect of the tax cut increases in firm productivity, the second and the last terms in equation (C.5.50) are negative (also using that C decreases in the tax cut), the first term is positive.

We do not have an analytic solution for the sign of $\frac{\partial^2 l(\omega_j)}{\partial\tau\partial y_j}$ if $b > 0$. In this case, we look at the effect of the tax cut on employment at the extreme cases. The effect of the tax cut on employment is:

$$\frac{\partial l(w_j)}{\partial \tau} = \frac{\partial C}{\partial \tau}(w_j - b)^{\lambda} + C\lambda(w_j - b)^{\lambda-1}\frac{\partial w_j}{\partial \tau}. \tag{C.5.51}$$

In this expression, the first term is negative, the second term is positive. At the lowest productivity firm, $w_j \to b$, thus the first term in equation (C.5.51) approaches zero and the second term goes to infinity (using that $\frac{\partial w_j}{\partial \tau}$ is finite, between 0 and 1, and $\lambda < 1$). On the other hand, if firm productivity approaches infinity then $w_j \to \infty$, thus the first first term in equation (C.5.51) approaches $-\infty$ (using that $\frac{\partial C}{\partial \tau} < 0$) and the second term goes to zero.

It therefore follows that, under reasonable assumptions, the positive impact of the tax cut on wages increases with firm productivity. Intuitively, as more productive firms face a less elastic part of the labor supply curve, they need to increase wages more to attract more workers. At the same time, the share of workers employed at the most productive firms decreases.

Note, that if aggregate labor supply is allowed to be elastic then employment may increase at all firms as a consequence of the tax cut, similar to what we find under the search and matching model with sequential bargaining.

When $b = 0$, we have a special case where the elasticity of labor supply is constant (see equation (C.5.41)). In that case, there is full pass-through of the tax cut to wages without heterogeneity in the pass-through across firms. This is because if $b = 0$, the wage equation (equation (C.5.48)) simplifies to:

$$\omega_j = \frac{1}{2}\left[y_j\frac{\lambda}{1+\lambda} + \tau + \left(\left(y_j\frac{\lambda}{1+\lambda}\right)^2 + \tau^2 + \frac{2\tau}{1+\lambda}y_j\lambda\right)^{1/2}\right] = y_j\frac{\lambda}{1+\lambda} + \tau. \tag{C.5.52}$$

Under this specific case, the employment effect of the tax cut decreases with firm productivity. This follows from equation (C.5.51), setting $b = 0$ and plugging in the formula of C:

$$\frac{\partial l(w_j)}{\partial \tau} = L\left(\sum_{k=1}^{J}\omega_k^{\lambda}\right)^{-2}\frac{\lambda}{\lambda+1}\left[\omega_j^{\lambda-1}\sum_{k=1}^{J}\omega_k^{\lambda} - \omega_j^{\lambda}\sum_{k=1}^{J}\omega_k^{\lambda-1}\right], \tag{C.5.53}$$

where the expression in the square brackets is clearly positive at the lowest productivity (lowest wage) firm and negative at the highest productivity (highest wage) firm.

C.5.5 Firm heterogeneity with perfectly competitive labor market

We build on the seminal model of Melitz (2003) to analyze the impact of the tax cut in a model with monopolistically competitive firms. Production requires labor only. As standard in this literature, we assume that labor is inelastically supplied at its aggregate level L. Later we will relax this assumption. Each worker earns a common wage ω.[64] Consumers have constant elasticity of substitution (CES) preferences with elasticity of substitution $\sigma > 1$. There are M firms on the market, with M endogenously determined. Firms draw their random productivity y from cumulative distribution function $\Upsilon(\cdot)$. Entry and exit from the market is free and firms know their productivity before entry. The distribution of the productivity of firms operating in the market is given by □(.).

[64] Following Melitz (2003), we normalize the nominal wage to 1 and, therefore, focus on the real wage.

Consumers' utility is

$$U = \left[\int_{j \in \Omega} x(j)^{\frac{\sigma-1}{\sigma}} dj \right]^{\frac{\sigma}{\sigma-1}}, \tag{C.5.54}$$

where Ω is the set of available goods, $x(j)$ is the consumption of good j, and $\sigma > 1$. Consumers' budget constraint is

$$\int_{j \in \Omega} p(j) x(j) dj = I, \tag{C.5.55}$$

where $p(j)$ is the price of good j, and I is total income. In this setting, the demand function is

$$x(j) = \frac{1}{p(j)} \left(\frac{P}{p(j)} \right)^{\sigma-1} I, \tag{C.5.56}$$

where P is the aggregate price:

$$P = \left[\int_{j \in \Omega} p(j)^{1-\sigma} dj \right]^{\frac{1}{1-\sigma}}. \tag{C.5.57}$$

To produce $x(y)$ unit of goods, firms must hire $\frac{1}{y}x(y)$ workers, plus need to pay f fixed labor cost (to which the tax cut does not apply).[65] The profit function with τ as the tax cut is:

$$\pi(y) = p(y)x(y) - \frac{\omega - \tau}{y} x(y) - f. \tag{C.5.58}$$

Assuming monopolistic competition, firms do not consider their influence on aggregate price. The first order condition for the price level is:

$$p(y) = \frac{\sigma}{\sigma - 1} \frac{\omega - \tau}{y}. \tag{C.5.59}$$

There is a productivity cut-off y^* with $\pi(y^*) = 0$, below which productivity firms do not operate. It also follows that

$$P = \frac{\sigma}{\sigma - 1} (\omega - \tau) \left[\int_{y^*}^{y_{max}} y^{\sigma-1} M \square(y) dy \right]^{\frac{1}{1-\sigma}}, \tag{C.5.60}$$

$$x(y) = y^{\sigma} \frac{I}{\omega - \tau} \frac{\sigma - 1}{\sigma} \left[\int_{y^*}^{y_{max}} y^{\sigma-1} M \square(y) dy \right]^{-1}, \tag{C.5.61}$$

and the real wage is

$$\frac{\omega}{P} = \frac{\sigma - 1}{\sigma} \left[\int_{y^*}^{y_{max}} y^{\sigma-1} M \square(y) dy \right]^{\frac{1}{\sigma-1}} + \frac{\tau}{P}. \tag{C.5.62}$$

Since the tax cut decreases the cost of production, a larger tax cut implies that the productivity cut-off y^* decreases, i.e., less productive firms enter the market. The impact of the tax cut on the common real wage is positive. Neglecting the effect on the productivity cut-off, there is full pass-through of the tax cut to the real wage. The pass-through is further amplified by the effect on the productivity

[65] We follow the standard approach in the literature and use $x(j)$ and $x(y)$ interchangeably as each variety j is produced by one firm characterized by productivity y, thus the output can be written as a function of y.

cut-off.[66]

The firm-specific employment is:

$$l(y,\tau) = y^{\sigma-1}\frac{I}{\omega-\tau}\frac{\sigma-1}{\sigma}\left[\int_{y^*}^{y_{max}} y^{\sigma-1}M\square(y)dy\right]^{-1} + f, \tag{C.5.63}$$

and aggregate employment is:

$$L = \int_{y^*}^{y_{max}} l(y,\tau)M\square(y)dy = \frac{I}{\omega-\tau}\frac{\sigma-1}{\sigma} + Mf. \tag{C.5.64}$$

Turning back to the profit function, and denoting the firm-specific revenue with $r(y)$, we can rewrite the profit function as:

$$\pi(y) = r(y)\left(1 - \frac{\omega-\tau}{yp(y)}\right) - f = \frac{r(y)}{\sigma} - f. \tag{C.5.65}$$

Denoting the average revenue with $\bar{r}$ and the average profit with $\bar{\pi}$, it follows that:

$$\sigma M\bar{\pi} = M\bar{r} - \sigma Mf = L - \sigma Mf, \tag{C.5.66}$$

where in the last step we used that in equilibrium, aggregate revenue needs to equal total payment to labor, and that $\omega = 1$. Now, it follows that after rearranging equation (C.5.64) and using that $\omega = 1$,

$$I = (1-\tau)\left(\frac{\sigma}{\sigma-1}L - \frac{\sigma}{\sigma-1}Mf\right) = (1-\tau)\left(\frac{\sigma}{\sigma-1}L - \frac{L-\sigma M\bar{\pi}}{\sigma-1}\right) = (1-\tau)\left(L + \frac{\sigma}{\sigma-1}M\bar{\pi}\right). \tag{C.5.67}$$

Therefore, income equals unit labor cost multiplied by aggregate labor plus aggregate profits times the markup.

Looking at the employment effects of the tax cut, low-productivity firms enter the market, consequently, employment increases at low-productivity firms. Due to inelastic labor supply, aggregate employment remains unchanged, implying that employment has to decrease at least at some firms that were producing even before the tax cut (incumbent firms). The effect of the tax cut on employment is:

$$\frac{\partial l(y,\tau)}{\partial\tau} = \frac{\partial\left(\frac{1}{y}x(y) + f\right)}{\partial\tau} = y^{\sigma-1}\frac{\sigma-1}{\sigma}\frac{\partial\left((L + \frac{\sigma}{\sigma-1}M\bar{\pi})\left[\int_{y^*}^{y_{max}} y^{\sigma-1}M\square(y)dy\right]^{-1}\right)}{\partial\tau}. \tag{C.5.68}$$

Since the partial derivative in the last expression is the same for each firm, it follows that if the effect of the tax cut is negative on the employment at an incumbent firm then it has to be negative for all incumbent firms. Using that $\sigma > 1$, it also follows that the effect of the tax cut on employment decreases with firm productivity:

$$\frac{\partial^2 l(y,\tau)}{\partial\tau\partial y} = (\sigma-1)y^{\sigma-2}\frac{\sigma-1}{\sigma}\frac{\partial\left((L + \frac{\sigma}{\sigma-1}M\bar{\pi})\left[\int_{y^*}^{y_{max}} y^{\sigma-1}M\square(y)dy\right]^{-1}\right)}{\partial\tau} = \frac{\sigma-1}{y}\frac{\partial l(y,\tau)}{\partial\tau} < 0. \tag{C.5.69}$$

[66] This finding is similar to the results of Bilbiie et al. (2012), who show that deregulation and higher productivity cause steady-state marginal cost to increase. Bilbiie et al. (2012) argue that this result is due to the endogenous number of firms—higher productivity (or in our case, the tax cut) results in a more attractive business environment, which leads to more entry and a larger number of firms. This puts pressure on labor demand which leads to higher long-run marginal cost.

If we relax the assumption of inelastic labor supply, the positive effect of the tax cut on real wage still holds. Aggregate employment may then increase as a consequence of the tax cut, but the heterogeneity of the effect is ambiguous. Moreover, Kushnir et al. (2021) show that the existence of the equilibrium is not guaranteed for higher values of the labor supply elasticity.