American Economic Review 2019, 109(5): 1717–1763
https://doi.org/10.1257/aer.20171937

Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden†

By Emmanuel Saez, Benjamin Schoefer, and David Seim*

This paper uses administrative data to analyze a large employer-borne payroll tax rate cut for young workers in Sweden. We find no effect on net-of-tax wages of young treated workers relative to slightly older untreated workers, and a 2–3 percentage point increase in youth employment. Firms employing many young workers receive a larger tax windfall and expand right after the reform: employment, capital, sales, and profits increase. These effects appear stronger in credit-constrained firms. Youth-intensive firms also increase the wages of all their workers collectively, young as well as old, *consistent with rent sharing of the tax windfall.* (*JEL* H25, H32, J13, J23, J31, M51)

In recent decades, cuts to the employer portion of payroll taxes are often discussed as a policy lever to reduce labor costs, particularly for workers facing high unemployment rates such as low earners, the young, or the elderly.[1] The policy debate is framed as follows: the rationale for targeted payroll tax cuts is to boost employment for specific groups and business activity more generally; a potential drawback is that firm owners might instead just pocket the tax cut as a profit windfall. In the public economics literature, the received wisdom, based on the canonical competitive labor market model, is that the incidence of payroll taxes, even if nominally paid by employers, ultimately falls on workers' net market wages, leaving firms' gross labor costs unchanged.[2]

* Saez: University of California, 530 Evans Hall #3880, Berkeley, CA 94720 (email: saez@econ.berkeley.edu); Schoefer: University of California, 530 Evans Hall #3880, Berkeley, CA 94720 (email: schoefer@berkeley.edu); Seim: Stockholm University, Universitetsvagen 10 A, 106 91 Stockholm, Sweden (email: david.seim@ne.su.se). Thomas Lemieux was the coeditor for this article. We thank David Card, Raj Chetty, Johan Egebark, Anders Forslund, Peter Fredriksson, Matthew Gentzkow, Lena Hensvik, Simon Jäger, Lawrence Katz, Niklas Kaunitz, Patrick Kline, Tuomas Kosonen, Adriana Kugler, Magne Mogstad, Arash Nekoei, John Pencavel, Per Skedinger, David Sraer, David Strömberg, Jonas Vlachos, Danny Yagan, Eric Zwick, two anonymous referees, as well as numerous seminar and conference participants for helpful discussions and comments. Sam Karlin, Carl McPherson, Nina Roussille, and Julia Tanndal provided outstanding research assistance. We acknowledge financial support from NSF grant SES-1559014, the Center for Equitable Growth at UC Berkeley, Sloan Foundation grant 2013-10-22, and FORTE grant 2015-00490.

† Go to https://doi.org/10.1257/aer.20171937 to visit the article page for additional materials and author disclosure statements.

[1] For example, France has sharply cut employer payroll taxes on low paid workers as a way to reduce the labor cost of minimum wage workers (see, e.g., Piketty 1997 and Kramarz and Philippon 2001). The United States has a history of targeted employer credits for disadvantaged groups (Katz 1998). Several European countries have experimented with payroll tax cuts for the young or the elderly (see, e.g., OECD 2017).

[2] The underlying assumption is that aggregate labor demand is much more elastic than aggregate labor supply (see, e.g., Fullerton and Metcalf 2002). These incidence assumptions are adopted in the official statistics on the distribution of US Federal taxes (US Congressional Budget Office 2016).

In this paper, we analyze a large, long-lasting employer-borne payroll tax cut for young workers in Sweden. At the market level, we fully reject the sharp differentials in wages predicted by the canonical model: the directly treated young workers' market wages show no increase at all relative to the slightly older ineligible control group. In consequence, labor costs for young workers drop, and youth employment increases. Rather than through canonical market adjustment, we find that payroll tax incidence is transmitted at the firm level. Specific firms heavily exposed to the tax cut scale up labor and capital, and raise wages across the board, even for older employees never eligible for the tax cut themselves, consistent with labor market monopsony and rent sharing due to internal pay equity or union bargaining effects.[3]

Sweden has a large flat payroll tax rate of 31.4 percent, with no floor nor ceiling. The entire payroll tax is nominally paid by the employer. In 2007, a newly elected center-right government adopted a payroll tax cut targeted to young workers in two steps. On July 1, 2007, the payroll tax rate was cut to 21.3 percent for workers turning 19–25 during the calendar year. On January 1, 2009, the payroll tax rate was further cut down to 15.5 percent (a total cut of 16 points) and eligibility was raised to age 26. Hence, by 2009, the payroll tax rate on young workers was halved by the reform. The cut applied to both new and ongoing jobs. The motivation for the reform was to stimulate demand for young workers in light of high youth unemployment, as well as to boost business activity by reducing employer taxes. Administratively, the payroll tax cut was programmed into the government-provided payroll tax software, which employers use for monthly payroll payments. Hence, take-up of the age-specific reform was immediate, salient, and close to perfect. We analyze the payroll tax cut using population-wide Swedish administrative data linking employees to employers, and firm-level accounting data. Together, these features provide us with an ideal laboratory for our comprehensive study of payroll tax incidence and its transmission through market-level and firm-level mechanisms.

The first part of our empirical analysis focuses on the *market-level* tax incidence on wages and labor costs, and the associated employment effects building upon earlier studies using the same reform (Bennmarker, Calmfors, and Seim 2014; Skedinger 2014; and Egebark and Kaunitz 2013, 2018) and replicating some of their findings (see below). We reject the canonical prediction that market wages absorb the tax cut. Instead, we document a *perfect pass-through to labor costs*: average wages (measured as monthly full-time-equivalent salaries for all workers) are smoothly increasing in age across birth cohorts, with no discontinuity whatsoever at the age cutoff where the payroll tax cut applies and in years after the reform is in place. Correspondingly, we show that, after the reform, a sharp, policy-induced

[3] Underlying reasons include standard union-based insider/outsider considerations (as in Lindbeck and Snower 1986 and Blanchard and Summers 1986), and behavioral pay-equity constraints (as in Akerlof and Yellen 1990 and Bewley 2002). See Manning (2003) for a comprehensive survey of monopsony effects and Dube et al. (forthcoming) for a recent empirical analysis using online employers. Our empirical findings complement growing evidence for the role of within-firm pay equity constraints. Galuscak et al. (2012, Table 1) survey 15 European countries (excluding Sweden) to document rigid internal pay structures. Agell and Lundborg (1995, Tables 1 and 3) document the role of internal pay equity in Swedish manufacturing firms, and Blinder and Choi (1990) and Campbell and Kamlani (1997) do so for the United States. Card et al. (2012) document employee dissatisfaction to information treatments about pay inequality. Breza, Kaur, and Shamdasani (2018) document labor supply effects of pay inequality in an experimental setting. Dube, Giuliano, and Leonard (2019) show that workers' quits respond to within-firm wage premia rather than market wages.

age discontinuity in labor costs per worker (defined as wage earnings plus payroll taxes) emerges at the eligibility threshold age after the reform. These wage patterns, we show, cannot be explained by on-the-job wage rigidity or minimum wage floors, as they extend to new hires and take place throughout the wage distribution. While previous studies have found limited pass-through of payroll taxes to wages, our simple contrast between wages pre- and post-tax provides compelling and transparent graphical evidence for full incidence on firms.[4] Considering cohort employment rates as outcomes instead of wages, we confirm that employment rates of eligible younger workers do increase right after the reform, by 2–3 percentage points, compared to slightly older ineligible workers.[5]

Alternative models of wage determination emphasize labor market frictions. In such models, marginal products and marginal rates of substitution are not the sole determinants of wages. These models rationalize the growing body of evidence in labor economics that points to the role of individual firms in setting wages and in generating wage dispersion between similar workers (see, e.g., Card, Heining, and Kline 2013; Card et al. 2018). For our context of a tax windfall, we are particularly motivated by evidence of wages reflecting rents that firms share with workers. The second part of our empirical analysis therefore switches gears to how business activity and in particular wage growth respond to the payroll tax cut in the cross section of firms. Did the absence of sharp differentials in market wages mask pass-through to average wages through rent sharing?

Our identification strategy exploits the fact that the payroll tax cut generated firm-specific profit windfalls and cost reductions that were proportionate to a firm's payroll share of young, treated workers. We therefore take advantage of persistent between-firm variation in the share of young workers just before the reform as done in previous studies.[6] Grouping firms by that measure, it turns out that firms with a moderate share of young workers are an excellent control group for firms with a large share of young workers, as both types of firms move in parallel for a very wide range of outcome variables in pre-reform years and share similar pre-reform attributes (unlike firms with no or a very small share of young workers). There is a 19.8 percentage point differential in the payroll share of treated workers between the two groups of firms we compare. This would, at constant net wages, induce a 2.4 percent reduction in (average) labor costs (since gross wages of the young would

[4] Our finding of zero net market wage effects is consistent with the earlier results of Bennmarker, Calmfors, and Seim (2014); Skedinger (2014); and Egebark and Kaunitz (2013, 2018), who find either zero or modest effects of the Swedish youth payroll tax cut on net wages. In other contexts, some quasi-experimental studies also find limited or no incidence (e.g., Kugler and Kugler 2009 and Becerra 2017 for Colombia; Saez, Matsaganis, and Tsakloglou 2012 for Greece; Bozio, Breda, and Grenet 2017 and Cahuc, Carcillo, and Le Barbanchon 2014 for France). We provide a more detailed comparison with previous work in the working paper version of our study, Saez, Schoefer, and Seim (2017).

[5] Our employment results are also consistent with the earlier work by Skedinger (2014) who focuses on the retail sector and Egebark and Kaunitz (2013, 2018) who use individual-based difference-in-differences regressions with controls.

[6] Skedinger (2014) compares youth-intensive and non-youth-intensive retail sector firms and finds positive effects on profits. Malm et al. (2016) study all sectors and find a positive effect on profits in the retail and wholesale sector but not overall. Kaunitz and Egebark (2017) find a significant positive effect on gross investment (but not profits) in 2007–2008. Methodologically, our study provides transparent identification using pre-reform trends and tracking yearly outcomes into 2013. We focus on a narrower set of firms to obtain better identification. Substantively, we find effects on a much broader range of firms' outcomes. We also study different mechanisms, in particular the effect on wages through rent sharing, where we also link *employee-level outcomes* to firm-level shock exposure.

fall by 12.1 percent), recurring every month. We trace out outcomes longitudinally pre- and post-reform to analyze how firms use this sizable tax windfall.

We focus first on business activity to understand the overall effects of the payroll tax cut on firm behavior. We then proceed to analyzing wages within the firm, to understand potential rent-sharing responses that the aggregate focus on youth market wages might have concealed.

Firms with a large share of young workers grow faster after the reform (relative to firms with a moderate share of young workers), in terms of sales, profits, capital assets, and employment. Firm effects are larger for firms more likely to be credit constrained according to standard proxies such as age, size measured by sales, or liquid assets as a share of total assets. The growth results are therefore consistent with liquidity effects, whereby the payroll tax cut helps alleviate firms' credit constraints and stimulates expansion by injecting cash. We also find positive growth effects in less constrained firms, either because the credit constraint proxies are imperfect or because unconstrained firms might respond to lower costs of employing young workers.

Next, we study whether firms did pass on some of the tax windfall to workers' net wages through rent sharing. While average payroll taxes per worker do fall in the more exposed firms, these firms also raise *average net wages*, by 1.9 percent, which is close to the differential tax windfall that the highly exposed firms received (2.4 percent). Accordingly, we find that while firms have expanded, their level of *gross* wage per worker has ultimately not changed much.

Did tax windfalls trigger actual wage increases, or did composition shifts push up average wages in these growing firms? To eliminate composition bias, we merge our firm-level data with our population data on individual workers. We now track the labor market biographies of individual workers based on the firm they were working for just before the reform.

Our matched employer-employee analysis isolates the indirect rent-sharing spillovers because we restrict our sample to various *always-ineligible* age groups that never themselves directly benefited from the reform. Only through rent-sharing spillovers are their wages exposed to the firm-level tax windfall. Our identification of rent sharing off *within-firm* worker-level variation improves upon existing designs that rely on between-firm variation but lack micro markers for directly- versus indirectly-affected workers within the firm.[7]

We find that these always-ineligible individuals working in a large share young firm experience faster wage growth after the reform compared to workers initially employed by the control-group firms. The differential wage growth effect is 2.6 percent, close to the predicted tax windfall differential (a 2.4 percent reduction in average labor costs, gross of the rent-sharing response). Moreover, the wage effect is present for those workers staying with the initial employer, consistent with rent sharing rather than an improved job ladder and mobility to higher-paying firms. Therefore, in contrast to our initial zero effect on relative market wages of eligible

[7] For example, Van Reenen (1996) and Kline et al. (forthcoming) find positive wage effects of patent approvals within firms. Budd, Konings, and Slaughter (2005) show that rents are shared across plants of multinational firms. Fuest, Peichl, and Siegloch (2018) also find that municipal corporate tax changes in Germany are partly shifted to workers' wages.

young workers, our firm-level evidence for rent sharing reveals that workers do benefit, *collectively*, from the tax cut. The macro incidence might then still fall largely on (average) net wages. Additionally, we find that low earning employees benefit relatively more (in percentage terms) from the tax cut than high earners. These across-the-board wage increases at the firm level are also in line with our conjecture that within-firm pay equity concerns may have prevented direct incidence on the *market* wage of young workers. These wage frictions perhaps contributed to youth unemployment to begin with. In this context, an age-dependent employer payroll tax rate may help offset such wage frictions. We present in the online Appendix a model with pay equity constraints within firms and monopsony power for firms that rationalizes our findings.

This paper is organized as follows. In Section I, we describe the institutional setting, the payroll tax reform, and the data. In Section II, we present the *market*-level effects of the payroll tax cut on wages and employment. In Section III, we present the *firm*-level effects of the payroll tax windfall on hiring and business activity. In Section IV, we present the incidence effects on wages and rent sharing within the firm. Section V concludes.

I. Institutional Setting and Data

In this section, we first discuss the institutional setting of the payroll tax in Sweden and the payroll tax cut reform. Next, we present the data we use for the analysis.

A. *Payroll Tax Cut for Young Workers in Sweden*

Swedish Payroll Tax.—In Sweden, the entirety of the payroll tax on earnings is nominally paid by employers and the tax is proportional to wage earnings with no exemption and no cap. The payroll tax rate is uniform across industrial sectors and covers all employers public and private.[8] The top series in the solid line in Figure 1 depicts the normal payroll tax rate from 2004 to 2017. The normal tax rate has been quite stable around 31–32 percent over this period. Payroll taxes fund various benefits (such as pension, sickness, work injury, etc.) with some imperfect link between the generosity of benefits and the level of taxes paid (see Skedinger 2014).

Young Workers' Payroll Tax Cuts.—The second series in the dashed line in Figure 1 depicts the preferential payroll tax rate for young workers. In 2007–2009, a new center-right coalition government implemented a payroll tax cut targeted toward young workers in two steps. The payroll tax cut was part of the center-right coalition's election promise in 2006.[9] The explicit aim of this reform was to fight youth unemployment, which had risen in previous years, and was perceived in the public debate to be excessively high. It was enacted as a permanent tax change.

In 2007, the first step lowered the payroll tax rate by 11.1 points from 32.42 percent (main rate in 2007) down to 21.32 percent for workers aged 19 to 25. The

[8] Negotiated agreements between employers and unions generate sometimes extra payroll fees on top of the standard payroll tax discussed here. Skedinger (2014) provides more details.

[9] See, for example, *Dagens Nyheter,* "Reinfeldt vill skrota arbetsgivaravgifter för unga," August 12, 2006.

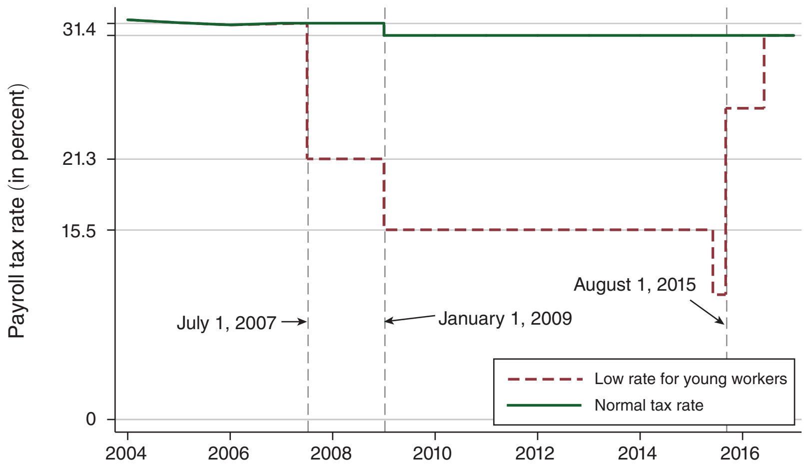


Figure 1. Preferential Payroll Tax Rate for Young Workers

Notes: The figure depicts the normal payroll tax rate (solid line) and the lower rate for young workers (dashed line) in Sweden over time. The payroll tax rate in Sweden applies to the totality of earnings and is nominally fully paid by employers. The first reform lowered the payroll tax rate for earnings received on or after July 1, 2007 for all workers turning 19 to 25 during the calendar year. The second reform further lowered the tax rate for earnings received on or after January 1, 2009 for all workers turning 26 or less during the calendar year. The reform was repealed in 3 steps on May 1, 2015, August 1, 2015, and on June 1, 2016. In the first step, the tax rate was actually lowered for workers aged 23 and below (remained 15.49 percent for ages 24–25, and increased to 31.42 percent for workers aged 26). In the second step, all workers aged 25 and less had their taxes increased to 25.46 percent and finally, in June 2016, they were increased to the normal rate.

reform was first mentioned in October 2006. The bill for this reform was voted by parliament on March 15, 2007, and took effect on July 1, 2007. It started to apply for earnings paid out on or after July 1, 2007 to all workers turning 19 to 25 during the calendar year.

In 2009, the second step further lowered the payroll tax rate down to 15.49 percent and increased eligibility to all workers turning 26 or less during the calendar year (instead of 19–25 in the first step). Eligible young workers tax rate was therefore 15.9 points lower than the main rate of 31.42 percent (as of January 1, 2009 and after). The bill for this second reform was voted by parliament on September 25, 2008 and started to apply on January 1, 2009.[10] To be precise, in 2009, the payroll tax cut applied to all workers born in 1983 or later on the totality of their 2009 earnings; in 2010, the payroll tax cut applies to all workers born in 1984 or later, etc.

Hence, a worker's *only* determinant of eligibility for a full calendar year is year of birth (and not actual age when the earnings are received), assessed against a rolling window of eligibility birth years. For a given year, our analysis is always based on birth-year cohorts: *age* is always defined as year of observation minus birth year, regardless of whether the person has actually reached her birthday or not during the year. Finally the payroll tax cut did not generate any reduction in the corresponding benefits of young workers. Hence, the payroll tax cut for the

[10] Government Bill 2008/09:7, *Kraftfullare nedsättning av socialavgifter för unga*.

young can be considered as a pure tax cut from the perspective of the young and their employers.

Implementation and Take-Up.—The payroll tax is administered by employers using government-provided software. Online Appendix Figure A1 illustrates the reporting of monthly earnings and payroll taxes by employers by showing the software for a 2013 snapshot. Every month, employers specifically type in the earnings paid to employees born in the different cohort categories, and the program displays the applicable tax rate and automatically calculates the payroll taxes due, ensuring almost perfect, immediate take-up. Employers always know the birth year of their employees as employees systematically provide their social security number, which includes the birth year, when starting an employment spell. In terms of enforcement, firms additionally have to send annual individual earnings reports (similar to US W2 forms) to the tax administration. Therefore, the tax administration can do an ex post reconciliation to check whether the payroll tax paid by employers over the year matches the theoretical payroll tax based on individual earnings reports. In case of discrepancies, the tax administration can send letters to help correct mistakes. From our conversations with the tax administration, mistakes were fairly rare. This suggests that take-up was close to 100 percent.[11] The direct cost of the payroll tax cut (ignoring any behavioral response) was around 0.8 percent of GDP per year, 2 percent of total annual tax revenue in Sweden, or 8 percent of total payroll taxes, a quantitatively large tax cut.

Other Contemporary Reforms.—The newly elected 2006 government implemented three additional reforms in 2007 that could also affect employment: an earned income tax credit, an extension of the maximum duration of temporary labor contracts from one to two years, and a new hiring subsidy (in the form of temporary payroll tax cuts) for people unemployed or disabled for at least one year. These three reforms are discussed in detail in Skedinger (2014). These reforms do not create a sharp discontinuity by age (although young workers are relatively more likely to benefit from the first two reforms) and hence are unlikely to confound our identification design. The temporary labor contract reform and the hiring subsidy could affect firms' labor demand decisions. As firms' behavior is central to our empirical findings, we explore in detail in online Appendix Section A.3 whether these two reforms could interact or affect our findings. Our conclusion is that these two alternative reforms cannot explain our results because they did not affect young workers differentially more than slightly older workers.[12]

Repeal in 2015–2016.—The left-wing opposition parties were against this payroll tax cut from the start. They lost the 2010 election but won the 2014 election on

[11] Many work subsidy programs have low take-up (and hence possibly low impact) because of administrative application costs for employers or stigma costs for beneficiaries. See Katz (1998) and Neumark (2013) for a survey and detailed discussions.

[12] The earned income tax credit reform affects the supply side by increasing the net-of-tax earnings of low income workers. However, we do not find any supply side response to our large payroll tax cut, even among the self-employed who are typically the most elastic. Therefore, it seems very unlikely that supply side responses to the earned income tax credit could explain our results.

September 14. Therefore, in 2015, the new center-left government abolished the payroll tax cut for young workers. The argument was that the reform was costly and the benefits in terms of reducing youth unemployment were debatable. The lower payroll tax rate for the young expired in three steps on May 1, 2015, August 1, 2015, and June 1, 2016, as depicted in Figure 1. The bill was passed on March 25, 2015 following a proposal put forward on October 7, 2014, just after the election.

After June 1, 2016, young workers again face the normal tax rate. Hence, the payroll tax cut lasted 9 years (and 6.5 years in its strongest form). Since our dataset ends in 2013, we cannot yet analyze the effects of the repeal. Studying whether the effects of the repeal are symmetric to the effects of the tax cut will be interesting (in light of compelling new evidence of asymmetric responses to tax increases versus decreases by Benzarti et al. 2017). We plan to study the repeal in future research.

Wage Setting in Sweden.—The Swedish labor market is to a great extent regulated and monitored in collective bargaining agreements (CBAs). An estimated 90 percent of all wage earners are covered by CBAs, with slightly lower figures for the private sector (Medlingsinstitutet 2015). These agreements are typically renegotiated every three years and they define the rules for wage bargaining. Many CBAs also prescribe a fall-back wage increase, but these are only operationalized in case the local bargaining between the employer and its employees fails (Fredriksson and Topel 2010). The wage concept used for CBA negotiations is the wage net of employer payroll taxes (but before income taxes). Wages are negotiated either at the hourly level or the full-time equivalent monthly level.

Fredriksson and Topel (2010) categorize CBAs by the influence that local bargaining parties have on wage determination. They conclude that 36 percent of all employees are covered by agreements where wages are bilaterally bargained between employer and employee. Another 57 percent are covered by agreements in which increases in total labor costs at the firm level are predetermined centrally, but the allocation of those increases are determined in local negotiations. Therefore, quite some scope for individual-level bargaining and differentiation in wage setting remains. Still, union bargaining quite possibly plays an important direct or indirect role in the results we obtain. Therefore, our results might not apply in other contexts where union bargaining is much less prevalent as in the United States.

Only 7 percent of Swedish workers have wage increases entirely set by the central agreement; this figure includes workers bound by the minimum wages. Sweden has no legislated minimum wage, but CBAs prescribe minimum wages that differ both across CBAs and within CBAs by age, experience (time spent working in the industry), tenure (time spent working in the firm), and education. In our robustness checks, we will investigate (but ultimately rule out) such minimum wage floors as the explanation for the absence of effects on wages.

B. *Administrative Data*

We use several administrative data registers at both the individual and the firm level, collected by Statistics Sweden for both individuals and firms.

Worker Data.—The basis of our individual-level analysis is the population of all Swedish residents (as of December 31 each year) aged 16 and above for years 1990–2013. We obtain annual earnings and employment spells for this population using the complete matched employer-employee records available for all years 1985–2013, with unique individual and firm identifiers. For each spell, these data record annual wage payments and months worked.[13] We collect annual earnings for each worker from the (highest-paying) employer, the wage concept we use to investigate the worker-level rent-sharing patterns in Section IVB.

We also add a number of outcome and demographic variables to the individual-level population at the annual level. From the Income Tax Register, we retrieve self-employment earnings and total wage earnings. From the Integrated Database for Labour Market Research (various administrative records compiled by Statistics Sweden), we obtain the level of education, unemployment history (days registered with the unemployment insurance agency as well as unemployment insurance received), gender, year, and month of birth.

We also link to this baseline population a matched employer-employee annual dataset (the Structure of Earnings Survey) that covers worker-level wages, occupational codes, and hours of work, for a very large sample of firms. The dataset covers all public sector employees and around 50 percent of private sector workers.[14] The information is collected during a measurement week (in September–November) for all workers employed for at least one hour during that week. The wage concept is the full-time equivalent monthly wage prevailing in the given month, including all fixed wage components, piece-rate and performance pay and fringe benefits.[15] We use this wage concept to study the incidence of the payroll tax cut on market wages in Section IIB.[16]

Firm Data.—The starting point for the firm-level analysis is the population of firms that are active at some point during 2003–2013. For these firms, we retrieve income statements and balance sheet information at the annual level, collected by the Tax Agency and administered by Statistics Sweden.[17] These records must be reported by all firms, even though not all components are relevant for tax purposes.[18] The unit of observation is the firm. However, in some instances, Statistics Sweden aggregates the firm-level information from the Tax Agency to the level of the corporate group and assigns a (weighted) average to each firm. Our baseline analysis sample therefore focuses on firms that are not part of a corporate group.

[13] These data are used to administer the social security and income tax systems in Sweden.

[14] The sample is a stratified random sample of firms, with larger weights on larger firms. All firms with more than 500 employees are included. Our wage results are robust to reweighting the wage sample to match the industry- and the firm-size distribution of the total population of employees.

[15] Fringe benefits are taxable and therefore recorded by the employer.

[16] These results are robust to instead considering the tax-based earnings measure instead.

[17] For some firms, the financial year is not the same as the calendar year. Statistics Sweden adjusts the income and balance sheet information for these firms to match the calendar year. To be precise, for a firm with financial year June–May, calendar year t's values are $5/12$ of financial year $t-1$'s values and $7/12$ of year t.

[18] Using the raw files from the Tax Agency, Statistics Sweden verifies basic accounting identities and if they do not hold, Statistics Sweden either imputes values (for small businesses), collects the annual reports, or approaches the firms with surveys. In our baseline analysis sample (described in detail in Section III), 1.33 percent of observations are corrected using one of those methods and our results are robust to excluding these corrected records.

II. Market-Level Effects

In this section, we first analyze the effects of the payroll tax reform on cohort-specific wages to determine the incidence of the payroll tax. Then we turn to the analysis of employment effects, again by cohort. We naturally use two definitions of wage earnings. First, we define *gross wage earnings* (sometimes abbreviated to gross wages) as wage earnings plus the employer payroll tax. Gross wage earnings are the total labor cost that employers pay for a given worker, including taxable fringe benefits.[19] Second, we define *net wage earnings* (sometimes abbreviated to net wages, or even just wages) as wage earnings net of employer payroll tax. It is the concept used for computing payroll taxes and is also the standard reference for compensation negotiations and contracts. There are no employee-level payroll taxes in Sweden, but there is an income tax assessed on net wage earnings (as well as on additional sources of income) with withholding at source, so that the worker's take-home paycheck is typically less than net wage earnings.

A. *Standard Competitive Model*

In this standard competitive spot market model, where the wage is determined such as labor supply equals labor demand, treated workers slightly below the age cutoff are naturally almost perfect substitutes for control workers slightly above the age cutoff. Suppose we start from a pre-reform equilibrium where these two groups are paid the same wage (and the same labor costs as payroll taxes are equal across age groups). When the payroll tax cut is introduced, the treated workers become cheaper to employers. Hence, employers hire more treated workers (and lay off control workers). With upward-sloping labor supply, these employment effects bid up the wage of treated workers until the labor costs of the two groups are again equalized. Hence, in the new equilibrium, there cannot be a discontinuity in labor costs at the age threshold, but there is a discontinuity in wages equal to the payroll tax differential between the two groups. The tax differential falls entirely on treated workers' wages (relative to control workers' wages).

Obviously, this benchmark is a vast simplification of how the labor market works in practice. There are frictions and costs in recruiting, training, and laying off workers that make the labor demand less than infinitely elastic (although similar results would still hold). There might be wage rigidities, either institutional or norm based, preventing employers from differentiating wages based on age, or adjusting wages as workers age out of the payroll tax cut. We will discuss all these elements in more detail after we examine the empirical evidence.

B. *Effects on Wages and Labor Costs*

To test the implications of the standard model, we evaluate whether net wages versus gross wages are discontinuous by age around the eligibility threshold after the reform. By definition, both wage concepts cannot be continuous after the reform,

[19] Nontaxable fringe benefits are very small in Sweden.

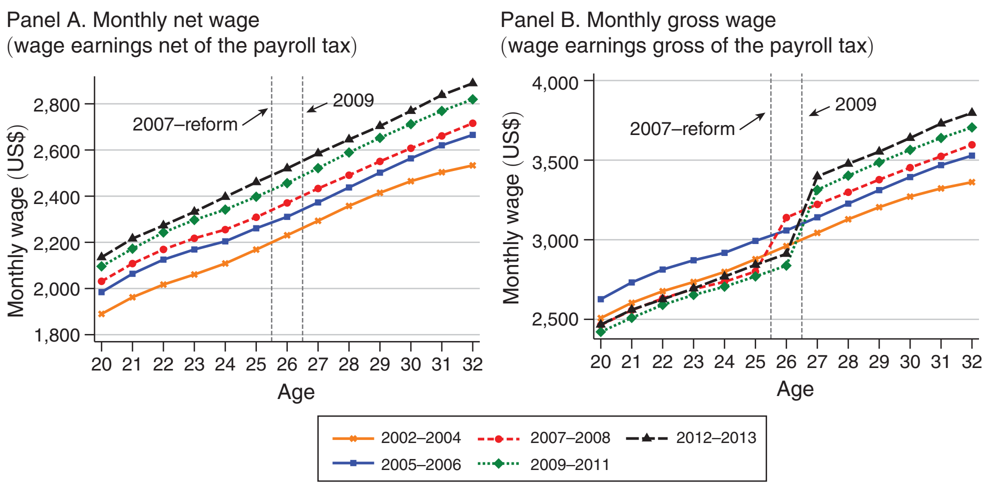


Figure 2. The Effect of the Payroll Tax Cut on Average Wages by Annual Cohorts

Notes: This figure depicts the average monthly wage in Sweden by age for different time periods using the Structure of Earnings Survey data for wage earners. The survey measures wages mostly for the month of September (with some measurements in October and November). Panel A depicts *net wages* defined as monthly wage earnings net of payroll taxes. Panel B depicts *gross wages* defined as monthly wage earnings gross of payroll taxes. The wage is defined as the full-time equivalent contracted monthly wage. It is adjusted for inflation (base-year 2003) and converted to US dollars using an exchange rate of 8.9 SEK/US$ (as of April 18, 2017). Age is defined as the age turned during the calendar year, which is the relevant concept for the payroll tax cut. The two dashed vertical lines depict the age thresholds under which the payroll tax cuts apply in 2007–2008 and 2009–2014 respectively. The sample includes all employees in the Structure of Earnings Survey which covers all industrial sectors (see Section IB). Panel A shows that net wages are continuous at the age thresholds and panel B shows that gross wages are discontinuous. This implies that employers do not adjust wages by age in response to the payroll tax cut even in the medium-term 5–6 years after the reform. Corresponding estimates are provided in Table 1.

so looking at both earnings concepts is a powerful and transparent way to tease out where the incidence falls. If gross wages paid by firms remain continuous, the incidence is entirely on workers' net wages. If net wages remain continuous, then firms experience full pass-through into the relative labor costs of young workers.

Data.—Our data source is the Structure of Earnings Survey; the sample is therefore all employees across all sectors for the month of September (or October–November) for each year (see Section IB for more details).[20] The wage is defined as the full-time equivalent contracted monthly wage, measured in September–November, CPI-deflated and converted to US dollars (8.9 SEK/US$ as of April 18, 2017). We use this measure to abstract from effects on hours worked.[21]

Figure 2 depicts net wages (in panel A) and gross wages (in panel B), averaged by age for different time periods. We consider the following periods: 2002–2004 and 2005–2006 are the pre-reform periods; 2007–2008 is the period affected by the first step of the reform (up to age 25);[22] 2009–2011 and 2012–2013 are the periods

[20] All of our wage results are robust to sample restrictions to only private sector workers or including both private and public sector workers. We therefore show results including all sectors.

[21] All of our wage results are robust to instead considering monthly earnings from the tax records.

[22] The reform started applying in July 1, 2007 so that it fully applies in September to November 2007.

affected by the second step of the reform (up to age 26). The two dashed vertical lines depict the age thresholds under which the payroll tax cuts apply in 2007–2008 and 2009–2014 respectively. Recall that age denotes end-of-calendar-year age, which determines eligibility for the full calendar year.

Net Wages.—Panel A in Figure 2 shows that net wages are smoothly increasing with age and across years before the reform. Importantly, net wages do not exhibit any discontinuity whatsoever at the age cutoff where the payroll tax cut applies, neither before nor after the reform.[23] In other words, wages of treated young workers do not adjust at all in response to the reform (relative to slightly older, ineligible workers). Note that even in 2012–2013, there does not appear to be any incidence on net wages, even in the medium term, 5–6 years after the reform.

Gross Wages.—Panel B in Figure 2 visualizes the corresponding effects on gross wages (labor costs), which consist of net wages plus the age-specific payroll tax rate in the given year. Before the reform, labor costs evolve smoothly across the eligibility thresholds. After the reform, there emerges a sharp, immediate discontinuity in average gross wages at the age threshold of the tax cut. This directly implies that the reform lowered relative labor cost of younger workers one to one, in the short as well as the medium run, even 5–6 years into the reform.

Hence, the two panels combined show very clearly that the payroll tax cut has no effect on net-of-payroll tax wages of young treated workers relative to slightly older untreated workers. The incidence is entirely on firms' labor costs. This finding of full incidence on firms goes starkly against the prediction of the standard model discussed above, which predicts no discontinuity in gross wages but a discontinuity in net wages. To our knowledge, the net and gross wage graphs combined with the sharp tax rate discontinuity among comparable workers are the simplest and most transparent evidence to date that employer payroll taxes do not get shifted to employees as predicted by the standard theory.[24] For this clean inference, the crucial features of the reform we study are that it was long-lasting, large, salient, applied to all young workers (not just new hires), had full, automatic take-up, and generated a sharp prediction since workers are nearly perfect substitutes around the age cutoff.

Regression Results.—Table 1 displays regression results on the incidence of the payroll tax, based solely on the aggregate cohort-year time series as depicted in the figures. We use the following basic difference-in-differences (DD) specification to estimate the treatment effect (γ):

$$w_{at} = \alpha_a + \beta_t + \gamma \cdot \mathbf{1}(a \leq a_{eligible}) \cdot \mathbf{1}(t \geq t_{reform}) + \varepsilon_{at}, \tag{1}$$

[23] The wage is increasing in age reflecting standard age, experience, and tenure effects on wages. Our graphical finding of the smoothness of the wage profile during the reform years replicates the findings of Egebark and Kaunitz (2018), who present a similar graph (their Figure 4) of wages by age using only years 2006, 2008, 2011, and focusing exclusively on net wages.

[24] Saez, Matsaganis, and Tsaklogou (2012) also find that employers bear the employer portion of payroll taxes using a cohort based payroll tax reform in Greece. Bozio, Breda, and Grenet (2017) also find employer payroll tax changes in France are borne by employers. But in both cases, the evidence is not as simple and compelling, as the tax differential in Greece or France applies only above an earnings threshold while it applies to the totality of earnings in the Swedish case we study here. Other payroll tax studies typically focus solely on net wages.

Table 1—Incidence on Monthly Wages

	Short run	Medium run benchmark
	(1)	(2)
Panel A. All workers		
Net wages	−1.551	−31.625
	(5.976)	(6.749)
Gross wages	−209.154	−404.698
	(27.677)	(7.528)
Pass-through to firms	1.007	1.085
	(0.269)	(0.046)
Panel B. Monthly birth data		
Net wages	0.995	−4.831
	(3.745)	(3.537)
Gross wages	−167.300	−390.675
	(15.491)	(4.543)
Pass-through to firms	0.994	1.013
	(0.185)	(0.025)
Panel C. Top 20 percent		
Net wages	23.489	13.555
	(16.083)	(11.601)
Gross wages	−238.699	−451.869
	(39.232)	(15.970)
Pass-through to firms	0.910	0.971
	(0.291)	(0.072)
Panel D. New hires		
Net wages	−20.226	−29.208
	(5.450)	(5.763)
Gross wages	−228.673	−391.232
	(28.106)	(5.529)
Pass-through to firms	1.097	1.081
	(0.284)	(0.036)
Observations (except panel B)	39	52

Notes: This table displays results on the incidence of the payroll tax cut for different subsamples of the data. This regression analysis is based on aggregate time series as depicted in the figures. We take the mean of the contracted monthly wage, including fringe benefits, fixed components, piece-rate, and performance pay, by age and time period (two pre-reform periods: 2002–2004 and 2005–2006; and three post-reform periods: 2007–2008, 2009–2011, and 2012–2013) for ages 20–32 (in panel B we focus on workers turning 24–28 during the year). We regress either the net wage (= monthly wage exclusive of payroll tax) or the gross wage (= monthly wage inclusive of payroll tax) on period-dummies, a dummy for being below the age-eligibility cutoff, age-dummies, and the interaction of being below the age-cutoff and the year being post-reform (see text and equation (1) for the exact specification). The table shows coefficients on the last regressor. We divide our analysis into *short-run* effects (2007–2008) in column 1 and medium-run effects (2009–2013) in column 2. Pass-through to firms is defined as the fraction of the payroll tax cut that benefits the employer. It is computed as the gross wage-coefficient divided by the gross wage-coefficient net of the net-wage-coefficient. Standard errors are computed using the delta-method. Panel A focuses on all workers; panel B on all workers using monthly birth data; panel C on the top 20 percent wage earners (defined within each age $\times$ year cell); and panel D on new hires. Number of observations in panel B are 180 (column 1) and 240 (column 2). Outcomes are expressed in CPI-adjusted US dollars.

where $a = 20, \ldots, 32$ denotes 13 age categories, t denotes the 5 time periods (2002–2004, 2005–2006, 2007–2008, 2009–2011, 2012–2013), w_{at} is the gross or net average wage outcome for age a and period t, $\mathbf{1}(a \leq a_{eligible})$ is a dummy for age below the eligibility cutoff, and $\mathbf{1}(t \geq t_{reform})$ is a post-reform dummy. The variable ε_{at} is the error term. The term γ is the coefficient of interest on the interaction age eligibility and post-reform; it denotes the treatment effect of the reform. Wages are again expressed in real US dollars and form the unit of the coefficients.

Panel A provides the estimates corresponding to Figure 2. In column 1, we focus on *short-run* effects (2007–2008 versus pre-reform) so that we use the 3 periods (2002–2004, 2005–2006, 2007–2008), $a_{eligible} = 25$ and $t_{reform} = 2007$. Hence, the regression is based on 39 observations (13 ages 20–32 times 3 periods) and we report conventional ordinary least squares (OLS) standard errors.[25] These OLS standard errors based on aggregate data are likely larger, and hence more conservative, than standard errors coming out of a micro-data based regression with clustering at the age $\times$ period level (or any finer clustering). In column 2, we focus on *medium-run* effects using instead four periods (2002–2004, 2005–2006, 2009–2011, 2012–2013) and hence excluding the period 2007–2008 when the reform is not fully phased in. In this case, $a_{eligible} = 26$ and $t_{reform} = 2009$.

Consistent with the graphs, we find large effects on gross wages and very small effects on net wages.[26] Tax incidence can be measured as the fraction of the payroll tax cut that benefits the employer, which we call the pass-through to firms. It is computed as the gross wage-coefficient divided by the gross-wage coefficient net of the net-wage coefficient. Standard errors are computed using the delta-method. We find a pass-through of 100 percent in both the short and long run.[27] We show in online Appendix Figure A3 that the discontinuity in net wages is fully present when zooming in on wages by monthly cohorts instead of quarterly cohorts as in Figure 2. Corresponding estimates based on such monthly cohorts are provided in panel B of Table 1, and are even closer to 100 percent pass-through to employers than our annual based estimates.

Implicit Contracts.—Wages could be rigid due to implicit contracts, whereby the firm promises a set of wage increases over time contingent on various outcomes. Such contracts may be incomplete and hence not contingent on possible payroll tax reforms, explaining why firms do not adjust wages in response to the payroll tax cut. To test this, the bottom panel of online Appendix Figure A4 shows the average wage for new hires. New hires are defined as having a new firm identifier (again for the month of September) as the main employer relative to the previous year. It includes both job-to-job transitions as well as new hires among previously non-employed individuals. These new hires are not affected by implicit wage contracts by definition. Yet, even for this subsample, we do not see any discontinuity arising after the reform. The corresponding regression estimates are reported in panel D of Table 1.

[25] Standard errors robust for heteroskedasticity are very close to our reported OLS standard errors, and significance levels are not affected (results not reported).

[26] The effect on net wages is actually significantly negative but quantitatively small in the medium run (column 2 in panel A). Even though panel A of Figure 2 does not show visible effects, small differences in age trends across years could be the source of the significant effect in the regression coefficient.

[27] Egebark and Kaunitz (2018), using individual-level DD regressions with controls, find small positive effects on wages in the order of 1–2 percent and often statistically significant. Their results would be consistent with a modest pass-through to workers of around 10 percent of the tax cut. Our simpler graphical analysis shows no wage effects at all. We have also checked that our results are robust to introducing individual controls for gender, education, and immigration status at the individual level.

They show complete pass-through to firms in both the short and the medium run, as high as in the overall sample. This implies that standard implicit contracts cannot explain our findings either.

More generally, in online Appendix Figure A5, we show that the absence of wage incidence on workers applies equally in high versus low turnover industries. Therefore, the absence of tax incidence on wages cannot be explained by the concern that all young hires will age out of the payroll tax eligibility on the job and that long-term jobs would mask tax incidence.

Minimum Wage Constraints.—Another explanation for full pass-through to firms is that wages are rigid due to minimum wages. In Sweden, the minimum wage varies by industry, occupation, and sometimes age (see Section I for more details).[28] In the standard competitive model, if young workers' prevailing net wages are constrained by the minimum wage (i.e., equilibrium wage for young workers is lower absent the minimum wage, and labor supply is rationed), then the payroll tax cut simply reduces labor costs. But as long as the post-reform net wages remain above the equilibrium wage, the incidence of the payroll tax cut would still fall fully on firms.

We test this possible explanation by repeating the wage analysis for the top 20 percent of the wage distribution conditional on age and year. This group is unlikely to be affected by the minimum wage floors. The corresponding regression-based estimates are reported in panel C of Table 1. They show a pass-through to firms of 91 percent in the short-run and 97 percent in the medium-run (panel A of online Appendix Figure A4 provides the supporting graphical evidence). Therefore, we do not see any significant incidence on net wages even in this subsample. This implies that binding minimum wages cannot explain our findings.

Wage Distributions.—To provide a nonparametric view of incidence across the wage distribution as well as to further explore whether pass-through to firms could be explained by rigidities or wage floors, we next look at the net-of-payroll tax wage densities in Figure 3. The figure depicts the monthly wage earnings densities for young workers (aged 22–24) affected by the payroll tax cut and slightly older workers (aged 27–29) not affected by the payroll tax cut pre-reform (pooling years 2002–2006) and post-reform (pooling years 2009–2013). In each period, both treatment and control group wages are deflated by a common index factor for each year based on the mean annual wage for the *control* group (ages 27–29).

Figure 3 shows that the post-reform wage densities of the young treated workers lie on top of the pre-reform densities of that age group. Hence, the absence of pass-through to workers is pervasive throughout the wage distribution, rather than just for the mean wages and the top quintile. Importantly, the net wage densities do not change from pre-reform to post-reform for the slightly older control group either, which validates our empirical strategy.[29]

[28] However, a close examination of these minimum wages shows that no age-specific provisions targeting workers eligible for the payroll tax cut were made after the reform takes place.

[29] Correspondingly, the gross wage density is shifted *uniformly* from pre-reform to post-reform for young treated workers. We depict this in online Appendix Figure A6.

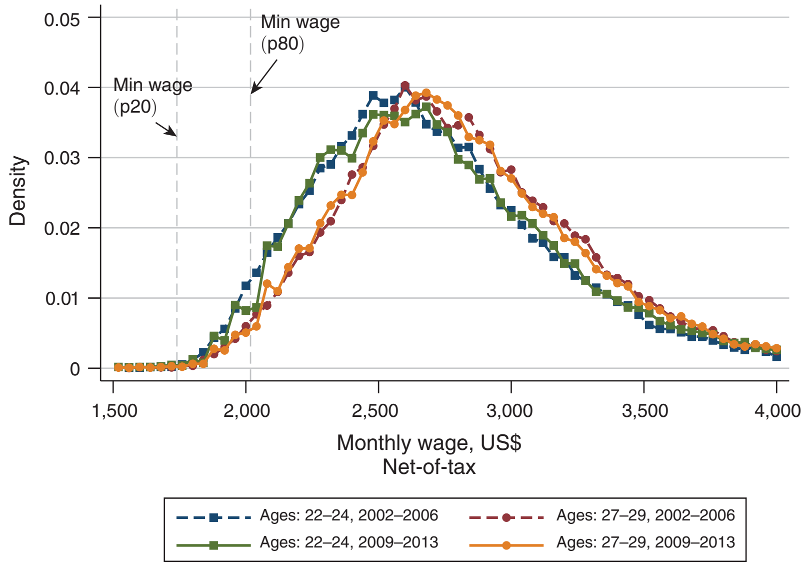


Figure 3. Net Monthly Wage Earnings Densities

Notes: This figure depicts the net monthly wage earnings (exclusive of payroll taxes) densities for young workers (aged 22–24) affected by the payroll tax cut in (in squares) and for slightly older workers (aged 27–29) not affected by the payroll tax cut (in circles) both pre-reform (pooling years 2002–2006 in dashed lines) and post-reform (pooling years 2009–2013 in solid lines). Wage earnings densities are measured typically in September (and sometimes October–November). Wages are adjusted for annual wage growth by first constructing a wage index based on the older individuals. Using this index, we deflate all workers' wages to 2013 values. The figure shows that the net wage earnings densities do not change from pre-reform to post-reform both for young treated workers and for the control slightly older workers. In particular, even the earnings density substantially above the minimum wages for young workers is unaffected. The figure depicts in vertical lines the twentieth and eightieth percentiles of minimum wages (as there are many minimum wages in Sweden based on industry, occupation, and tenure). This shows that the vast majority of young workers are paid above the minimum wage.

Finally, the density graph also implies that the incidence results cannot be due to minimum wage floors. To formally assess the impact of minimum wages on the estimated incidence, we retrieve digitized information on minimum wage floors for blue-collar workers during 2009–2013 at the level of the collective bargaining agreement and year.[30] Using industry and occupation codes, we match the wage floors to individual workers. Based on the workers in ages 22–24 with recorded minimum wages, we compute the twentieth and eightieth percentiles of minimum wages in 2009–2013 (as there are many minimum wages in Sweden based on industry, occupation, and tenure, see Section I for details). The figure depicts the location of those reference wages as vertical lines and even the wage density substantially above the minimum wages for young workers is unaffected by the reform. This graph also shows that the vast majority of young workers are paid above the minimum wage.

[30] We thank Anders Forslund, Lena Hensvik, Oskar Nordström Skans, and Alexander Westerberg for providing these data.

Summary.—Our findings are not the mechanical consequence of minimum wages, implicit labor contracts, or downwardly rigid wages on the job. They may reflect pay equity considerations within firms, perhaps mediated through union wage bargaining. These considerations manifest themselves as a form of wage rigidity preventing employers from *cross-sectionally* discriminating pay by age among similar workers, perhaps within firms. Indeed several studies have shown that workers respond to within-firm pay equity considerations (e.g., Card et al. 2012 or Dube, Giuliano, and Leonard 2019) or that unions care about equity in pay raises (e.g., Pencavel 1991). Therefore, firms may not be able to pass a large fraction of the tax cut to eligible young employees while not increasing pay as well for their slightly older employees. Our evidence on firm-level rent sharing in Section IV is consistent with such a phenomenon. In any case, our findings starkly contradict the standard model, which would predict 100 percent payroll tax incidence on workers at the age discontinuity. As employment is the channel through which incidence is passed on to workers in the standard model, we next turn to employments effects.

C. *Effects on Employment*

Overall Employment Effects.—Our wage results imply that young eligible workers are cheaper to employers than slightly older, ineligible workers. In the period 2009–2014, the payroll tax rate cut for young workers lowered their labor cost by 12.1 percent.[31] Effectively, an employer would save 12.1 percent of labor costs if she could switch from an ineligible older worker (say aged slightly above 26) to an eligible young worker (aged 26 or less), given the lack of net wage incidence. As these two groups of workers should be close substitutes, profit maximizing firms should want to hire more eligible workers or put more effort in retaining eligible workers (relative to ineligible workers). Indeed, this is the economic mechanism that eventually equalizes gross wages across treated and control groups in the standard competitive model. Even with rigid wages, we should see employment effects if firms care about labor costs when making their hiring decisions.

To analyze this, we examine first the employment rate in the labor force by age group and over time using the individual annual earnings data (see Section I for details). The employment rate is defined as the ratio of all employees to the labor force. The employees numerator is defined as all residents who are employed with annual wage earnings above a small annual threshold.[32] The labor force denominator is defined as all residents who are either (i) employed with annual wage earnings above the small annual threshold or (ii) unemployed (defined as having registered with the Unemployment Office at any point during the year).

Figure 4 depicts the employment rate by age and time periods. Age and time periods are defined as in Figure 2. We exclude the period 2007–2008 when the tax reform

[31] The tax rate for young workers is 15.49 percent while the normal rate is 31.42 percent, hence a reduction of labor costs of $(31.42 - 15.49)/(100 + 31.42) = 12.1$ percent.

[32] The small annual threshold is equal to \$4,940 in 2012 (and adjusted for inflation in other years). This small annual threshold corresponds approximately to working at 20 percent of full-time a full year at the minimum wage in the restaurant sector. Online Appendix Figure A12 compares our employment and unemployment measures for Swedes aged 20–34 with official statistics based on the labor force survey and shows that they line up fairly well.

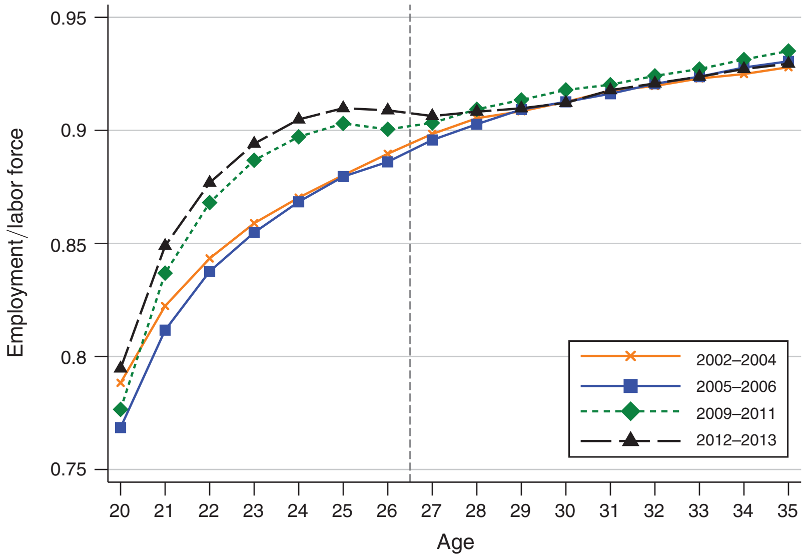


Figure 4. The Effect of the Payroll Tax Cut on Employment

Notes: The figure depicts the employees to labor force ratio by age and time periods. The employees numerator is all residents employed with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for inflation in other years). The labor force denominator is defined as all residents who are either (i) employees as just defined for the numerator; (ii) unemployed defined as having registered with the Unemployment Office at any point during the year. The figure shows a strong effect of the reform in increasing the employment rate of young targeted workers (corresponding estimates in Table 2).

was not yet fully phased in.[33] Two important findings emerge from the figure. First, the two pre-reform periods 2002–2004 and 2005–2006 show virtually superposed series with the fraction of employees increasing smoothly with age from around 77 percent at age 20 to 93 percent at age 35. This suggests that time trends are parallel pre-reform. Second, the employment rate is substantially higher in the post-reform periods of 2009–2011 and 2012–2013 but only for the treated groups aged 20 to 26. At ages 21–25, the employment rate is about 3 points higher in 2009–11 (relative to 2002–2006) and about 4 points higher in 2012–2013. In contrast, the employment rate is virtually the same in 2009–2011 and 2012–2013 (relative to 2002–2006) at ages 28 and above. Particularly striking is the fact that in 2012–2013, the employment rate is actually higher at age 25 than at ages 27–28, in sharp contrast with the steadily increasing employment rate pattern by age before the reform. This simple graphical approach provides perhaps the most compelling causal evidence to date of employment effects of targeted employer payroll tax cuts.[34]

Table 2 provides the corresponding estimates using a basic difference-in-differences regression based on the graphical output following the specification of equation (1). We consider the 4 periods 2002–2004, 2005–2006, 2009–2011, 2012–2013 (always

[33] The relative by-age employment rates show a treatment effect similar to the later years, but we see a large *parallel* upward shift for all cohorts in these months due to an aggregate expansion.

[34] Katz (1998) first provided similar graphical evidence of employment effects of hiring subsidies (but the series were noisier due to smaller sample size).

Table 2—Effect of Payroll Tax Cut on Employment Measures

	Effect (percentage points) (1)	Elasticity (2)
Employment/labor force (labor force)	0.021	0.21
	(0.0026)	(0.026)
Employment/(labor force + students)	0.023	0.27
	(0.0040)	(0.047)
Employment/population	0.014	0.23
	(0.0039)	(0.066)
Labor force/population	−0.0096	−0.11
	(0.0034)	(0.038)
Unemployment-employment transitions	0.011	0.23
	(0.0039)	(0.082)
Employment-unemployment transitions	−0.012	−2.26
	(0.0014)	(0.26)
Observations	64	64

Notes: This table presents effects of the payroll tax cut on various employment measures (by row) using the aggregated times series by age and time periods displayed in the figures. We regress each outcome variable on 16 age dummies (ages 20 to 35), a post-reform dummy, and the interaction of the post-reform dummy and an age eligibility (ages 20–26) dummy. The table shows coefficients on the last regressor. The time periods used are 2002–2004 and 2005–2006 (pre-reform) and 2009–2011 and 2012–2013 (post-reform), which is our benchmark frame. We exclude years 2007 and 2008 when the reform not yet fully phased in. Each regression is based on $16 \times 4 = 64$ observations and we report conventional OLS standard errors. The first column shows percentage point effects and the second transforms these effects into elasticities by dividing the percentage point-effect by the 2005–2006 average of the outcome variable within the treatment group and by the percent reduction in labor costs induced by the reform (which is 12.1 percent). The treatment effect provides an average effect across ages, weighted by age using the age distribution of the labor force in 2006. *Labor force* (LF) is defined as all residents who are either (i) *employed* with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for inflation in other years); or (ii) *unemployed* (defined as having registered with the Unemployment Office at any point during the year). *Employment* is defined as having annual wage earnings above the small annual threshold. In the second row, we add *students* (registered in any higher education institution during the year) to the *labor force* denominator. *Unemployment-employment transitions* are defined as the share of unemployed in year $t-1$ who become employed in year t and *Employment-unemployment transitions* are defined as the share employed in year $t-1$ who enter unemployment in year t.

excluding period 2007–2008 when the reform was only partially phased in) and 16 age groups 20 to 35. All regressions are run on just $16 \times 4 = 64$ aggregate cohort-period observations and we use again conventional OLS standard errors. Column 1 reports the effect expressed in percentage points while column 2 translates this effect into an elasticity estimate (using the fact that the payroll tax cut reduces labor costs by 12.1 percent). We estimate an employment effect of 2.1 percentage points with an implied elasticity of 0.21. Although employment effects are very significant, they still translate into a relatively modest elasticity, consistent with the previous findings of Egebark and Kaunitz (2018), who use individual-level DD regression with controls.[35]

[35] We have also checked that our results are robust to introducing individual controls for gender, education, and immigration status at the individual level.

The employment response we have uncovered is likely due to labor demand effects rather than labor supply effects because the net wage of eligible young workers does not increase (our first result).

Robustness.—Online Appendix Figure A8 provides robustness tests to our findings from Figure 4. In panel A of Figure A8, we add students to the labor force denominator. In panel B, we show employment effects when varying the earnings threshold for defining employees (keeping the labor force constant). Both graphs show that the employment effects we have obtained are robust to these alternative definitions. This finding also implies that the composition of newly created jobs, in terms of total earnings, did not decline. Row 2 in Table 2 reports the estimates when adding students to the labor force, showing only a minor effect on the size of the estimate.

Importantly, we use the employment to labor force ratio (instead of the employment to population ratio) because as there is no wage effects on workers from our previous analysis, there is no reason to expect strong labor supply participation responses from people outside the labor force joining the labor force by looking for a job. Two pieces of evidence presented graphically in online Appendix confirm this assumption.

First, online Appendix Figure A9 shows that there are no visible effects of the reform on the labor force to population ratio. However, the series for the labor force to population ratio are noisier and the pre-trends are not as parallel as for the employment to labor force ratio. As a result, there is less confidence that the labor force to population is unaffected by the reform. The regression-based analysis presented in Table 2 actually shows a fairly small negative effect of −0.96 points. Table 2 displays an employment to population effect of 1.4 percentage points (an elasticity of 0.23), similar in magnitude to the effects on the employment to labor force ratio but much less precisely estimated.

Second, we also show in online Appendix Figure A10 that self-employment earnings respond only modestly to the tax cut. These results replicate the earlier findings of Egebark (2016). As self-employment earnings are typically much more responsive to taxes than wage earnings, this further suggests that supply side responses are very modest.

Two caveats should be noted. First, the employment rates vary in level with the business cycle. In particular, the employment rate was much higher in 2007–2008 relative to all other years for all age groups so that the 2007–2008 employment data cannot easily be used to evaluate the effects of the reform on employment. In contrast, the employment rates at ages 28–35 are very close across all 4 other periods, providing us much stronger confidence that the higher employment rates at younger ages are indeed reform driven. Second, the labor force denominator only includes individual with earnings (employees) above a modest threshold or individuals who are formally registered as unemployed at any time during the year. It is possible that individuals not in our denominator could still be in practice looking for work even if not formally registered with the unemployment office.

In the online Appendix, we further extend the analysis of employment effects along two dimensions. First, we find the employment effects (even in percentage points) to be largest in areas with initially high youth unemployment, although treatment effects are present in all regions (online Appendix Figures A11 and A12). Second, we do not find evidence that those new jobs came at the expense of job quality for the treated young workers; in fact, we find that 80 percent of the employment rate increase was accounted for by longer employment relationships, rather than shorter unemployment spells (online Appendix Figure A13).

Summary.—Exploiting the sharp discontinuity around the eligibility threshold, our graphical evidence has documented that the payroll tax cut increased the employment rate by 2 percentage points exactly among the targeted worker groups. These positive employment effects combined with zero net-wage effects are puzzling to the standard tax incidence framework that relies on a competitive labor market: high labor demand elasticities and low labor supply elasticities predict small (close to zero) employment effects and full pass-through into net wages (with gross wages unaffected).[36] While these market-level findings may appear consistent with wage rigidities, our investigations clarify that minimum wage floors in Sweden or even conventional downward wage rigidity cannot explain the incidence into gross wages. In light of these nonstandard results at the market level, we next explore the role of firm-level mechanisms as potential transmission channels of tax incidence.

III. Effects of the Tax Windfall on Business Growth

Our market-level results presented in Section II contradict the standard public economics view. The payroll tax cut does not lead to higher relative wages for young workers, and hence translates fully into reduced labor costs for employers. Employers respond to the lower labor costs by employing more young workers. As firms are the beneficiaries of the payroll tax cut, an interesting question that arises and is indeed discussed in the public debate is whether firms just pocket the windfall from the tax reduction, or whether they use it to expand business activity.

We address this question using firm-level variation in exposure to the payroll tax cut generated by preexisting, persistent age composition of their workforce. Firms with a large share of young workers benefit from a larger payroll tax cut windfall than firms with few young workers. Therefore, it is possible to do a longitudinal analysis based on firms' pre-reform share of young workers.[37] We provide compelling graphical evidence with pre-trends, including the medium run, and also analyze a broader range of outcomes and mechanisms, and thereby differ and build on Skedinger (2014), Malm et al. (2016), and Kaunitz and Egebark (2017), who use a regression-based analysis of this firm level instrument.

Mechanism: Cash Injection versus Marginal Cost Reduction.—The response of firms to the payroll tax cut should depend on their share of young workers pre-reform through two potential channels. First, the payroll tax cut generates a larger cash flow windfall to firms with many young workers. If firms are credit constrained, such a

[36] In fact, from our results, the standard framework would infer a 0.22 labor demand elasticity with respect to labor costs (see Table 2) and an infinite labor supply elasticity (given the zero net wage increase).

[37] The minimum wage literature has used similar identification designs exploiting variation in the fraction of treated workers across firms (see, e.g., Card and Krueger 1994 and Harasztosi and Lindner 2017).

cash windfall could lead firms to expand and in particular hire and invest more. We call this channel the *cash* channel.[38] Second, the payroll tax cut lowers the overall marginal cost of production by reducing the cost of one production input, namely young eligible workers. This marginal-cost effect is stronger for firms whose production function leads them to always want to employ many young workers. We call this channel the (marginal) *cost* channel, akin to the scale effect in labor demand.

A given firm's pre-reform labor cost share of young treated workers is a good proxy for *both* the youth intensity of their production process as well as the cash injection from the tax windfall. Hence, our research design does not allow us to estimate separately these two effects, although we investigate whether the effects are stronger for firms more likely to be credit-constrained. Our main specifications estimate the combined effect of the cash channel and the cost channel using our longitudinal identification strategy.

Finally, unlike in the market-level investigations of employment, our firm-level design does not permit us to investigate substitution from old to young workers separately from scale effects. This is because our firm-level identification is constructed directly from a given firm's share of young workers (in 2006), and we find that this share young measure exhibits mean reversion that would mask all potential substitution patterns.

A. *Empirical Strategy and First Stage*

Our empirical strategy exploits the considerable between-firm variation in share of young workers pre-reform which generates firm-level variation in treatment intensity. We consider a balanced panel of firms active in every single year from 2003 to 2013.[39] We start in 2003 because this is the first year for which balance sheet data for firms are available.[40] We include firms with more than three employees in each year.[41] We consider only for-profit corporations domestically owned, hereby excluding sole proprietorships, partnerships, as well as all firms in the public and the nonprofit sector. Financial companies are not part of the data produced by Statistics Sweden and hence cannot be included in our analysis either. We also exclude firms that are part of a corporate group as Statistics Sweden sometime imputes values for such firms (see Section IB).

We split the balanced panel of firms into five groups based on their *share young*, which we define as the share of total wage earnings paid to young workers aged 19–25 in 2006. As young workers tend to be paid less than older workers, our share

[38] In the context of wage rigidity, Schoefer (2016) proposes this cash channel of labor costs as an alternative to the standard marginal cost channel, and discusses procyclical employer payroll taxes as a policy lever to stabilize firms' liquidity over the business cycle.

[39] We show below that our results are robust to considering an unbalanced panel of firms and that the reform did not have an effect on firms' survival. We also follow individual workers in some of our analyses regardless of whether they still work for their initial employer, or whether their initial employer still operates.

[40] Some outcomes such as employment and workers' wages are available for earlier years. For these outcomes, we have also checked that pre-reform parallel trends between treatment and control groups are robust to including more pre-reform years.

[41] A large number of firms are tiny (0, 1, 2, or 3 employees). For those firms, discrete adjustment of workers would generate extreme employment growth values (e.g., 100 percent for firms growing from 1 to 2). Hence, we drop these tiny firms. We have checked that our firm-level results are robust to weighting the firm observations by size (firm's 2006 employment). We prefer to use unweighted estimates for simplicity of presentation.

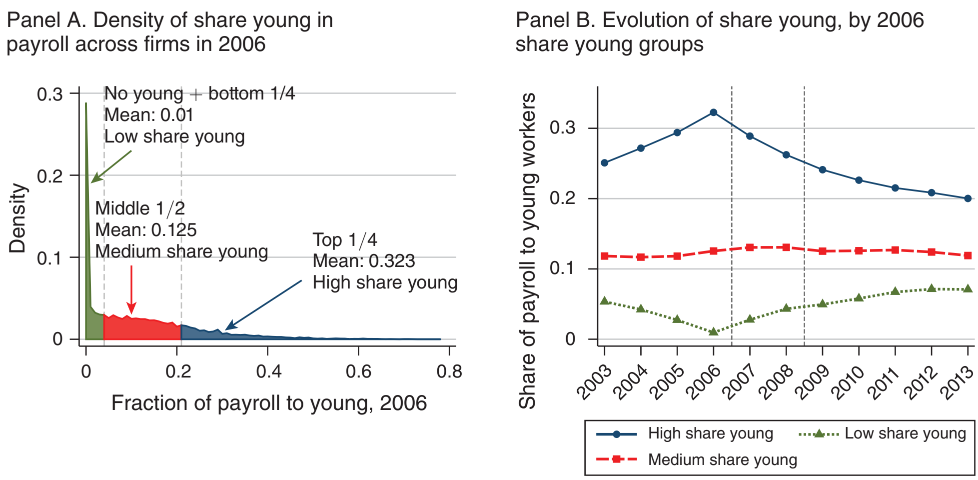


FIGURE 5. DIVIDING FIRMS BY PRE-REFORM SHARE YOUNG IN PAYROLL

Notes: This figure considers a balanced panel of firms active in every single year from 2003 to 2013 and with more than three employees in each year. We only include corporations (excluding the public sector, and foreign-owned corporations). We split firms into three groups based on the share of payroll paid to young workers (aged 19–25) in 2006 that we call *share young*. Panel A depicts the density distribution of share young. The *High share young* group (in blue) are firms in the top quartile share young (among firms with at least one young worker in 2006). The *Medium share young* group (in red) are firms in the middle two quartiles share young (among firms with at least one young worker in 2006). The *Low share young* is the rest (firms in the bottom quartile share young and firms with zero young workers depicted as a spike at zero in the figure). Panel B depicts the longitudinal average share young in each year for each of the three groups of firms. The spike/trough pattern around 2006 is due to mean reversion: firms with high share young in 2006 tend to have a lower share young before and after. There is substantial persistence in the share young across years implying that the tax windfall received by firms with a large share young in 2006 persists in subsequent years.

young is typically below the fraction of employees who are young. The five groups are defined as the four (unweighted) quartiles of firms with positive share young, and a mass of firms with precisely zero young share.[42]

Figure 5 illustrates our source of between-firm variation: pre-reform firm-level share young in 2006. Panel A depicts the density distribution of share young in 2006. The spike at zero represents the 22.6 percent of firms which have a share young of exactly zero. The bottom quartile is depicted in green, the middle two quartiles in red, and the top quartile in blue. The share young distribution has a long tail with substantial variation within the top quartile. We will sometimes exploit this additional variation by further breaking down the top quartile into a top 1/8 and the next 1/8. Correspondingly, as there is less first-stage variation in the middle of the distribution, we group together the middle two quartiles. To summarize, we call the bottom group that includes firms with 0 young workers and firms in the bottom quartile (in green), the *low share young*; we call firms in the two middle quartiles group (in red), the *medium share young*; and we call the top quartile group (in blue), the *high share young*. In addition, we sometimes split the top quartile group into two

[42] Slightly more than one-fifth of firms (22.6 percent) in our sample have a zero share young. As a result our split is very similar to splitting all firms, including those with zero share young, into five quintiles. Our results are also robust to defining quartiles after weighing firms by number of employees.

Table 3—Firm Descriptive Statistics by Share Young, 2006

	Share young		
	Low No young + bottom 1/4 (1)	Medium Middle 1/2 (2)	High Top 1/4 (3)
Fraction young	0.01	0.13	0.32
Number of workers	9.46	14.06	13.46
Gross annual wage earnings per employee	35.23	31.46	27.99
Total assets	713.09	868.23	670.86
Value added	701.27	887.57	743.94
Sales	1,249.69	1,827.04	1,821.04
Profits (EBIT)	68.73	83.79	66.13
Financial constraints			
FC: below median liquid assets/total assets	0.47	0.53	0.50
FC: below median sales	0.58	0.42	0.49
FC: below median firm age	0.45	0.46	0.55
Industries			
Agriculture and mining	0.04	0.05	0.07
Manufacturing	0.19	0.18	0.11
Construction	0.15	0.21	0.16
Wholesale and retail	0.23	0.27	0.34
Hotel and restaurants	0.02	0.05	0.13
Transport and communication	0.11	0.11	0.07
Property management, B2B	0.16	0.08	0.06
Education	0.02	0.02	0.01
Healthcare (not pharmaceutic firms)	0.05	0.02	0.00
Public services	0.02	0.03	0.05
Observations	5,698	5,265	2,632

Notes: This table provides statistics for a balanced panel of firms active in every single year from 2003 to 2013 and with more than three employees in each year. It partitions this sample into three groups based on the share of payroll paid to young workers (aged 19–25) in 2006 as depicted in Figure 5. The first group (*Low share young*) includes firms with zero young workers in 2006 and the bottom quartile (unweighted) of firms with at least one young worker in 2006. The second group (*Medium share young*) includes firms in the middle two quartiles (unweighted) of firms with at least one young worker in 2006. The third group (*High share young*) includes firms in the top quartile (unweighted) of firms with at least one young worker in 2006. Noncorporate businesses (such that sole proprietorships or partnerships) are excluded. Public sector firms and firms part of a corporate group are also excluded. Statistics for each group are displayed in each of the three columns. All statistics are for year 2006. All monetary variables expressed in US$1,000 (converted from SEK at the exchange rate 8.9 SEK/US$).

equally sized groups: the very top group (top 1/8) called the *very high share young* and the next group (next 1/8) called the *fairly high share young*.

Critical to our empirical design is the persistence of share young across years. We explore this in panel B of Figure 5, which depicts the average share young in each year for each of the three groups of firms. The spike/trough pattern around 2006 is due to mean reversion: firms with high share young in 2006 tend to have lower share young before and after. Conversely, firms with low share young in 2006 tend to mean revert up before and after. Most important, there is substantial *persistence* in the share young across years. While we do not explicitly estimate IV effects, this persistence graph presents the "first stage" of our strategy.

Table 3 provides further statistics as of 2006 on the three groups of firms depicted in Figure 5. Statistics for each of the three groups (low, medium, high) are reported in columns 1, 2, and 3. Two points are worth noting. First, firm statistics are not widely different across the three groups. Second, the top two groups in columns 2 and 3 are relatively more similar to each other than to the bottom group in column 1.

In particular, the top two groups have very similar average firm size measured either in terms of full-time employees or sales. As our analysis will show, the top two groups have very close pre-reform parallel trends for a very wide range of outcomes. In contrast, the pre-trends for the bottom group are not quite as well aligned. Furthermore, as panel B of Figure 5 showed, there is a much larger first-stage difference between the top two groups than between the bottom two groups. Hence, our empirical analysis will compare the top two groups and not use the bottom group.

The difference in fraction of payroll young in 2006 between the high share young group and the medium share young group is 19.8 points. As the payroll tax cut reduces labor costs of the young by 12.1 percent, this means that the tax windfall differential is 2.4 percent of total payroll initially. As a fraction of payroll, the tax windfall tapers off and is reduced by about half in the post-reform years for 2009–2013 relative to 2006 due to mean reversion. In 2006, the difference between the very high share young (and the medium share young) is 27.1 points while the difference between the fairly high share young (and the medium share young) is 12.5 points (see panel A of online Appendix Figure A14). Hence, we should expect differences in outcomes between the very high share young and the medium share young to be about twice as large as the differences between the fairly high share young and the medium share young.

B. *Firm-Level Results*

Methodology.—We generally consider two groups as in Figure 5: (i) firms in the middle two quartiles of share young in 2006 (medium share young) and (ii) firms in the top quartile of share young in 2006 (high share young). We plot the time series of average outcomes for these two groups of firms from 2003 (the first year we have comprehensive firm data) to 2013 (the latest year available). For each firm, we normalize outcomes relative to year 2006, i.e., $y_{f,t}/y_{f,2006}$, where $y_{f,t}$ is outcome y of firm f in year t. We then compute the straight average of these normalized firm-level outcomes for each of the two firm groups in each year.[43] Denoting by $\bar{y}_{h,t}$ and $\bar{y}_{m,t}$ these average normalized outcomes in year t for the high share young group (h) and medium share young group (m), we then simply plot the two time series $\bar{y}_{h,t}$, $\bar{y}_{m,t}$ for $t = 2003, \ldots, 2013$. For additional variation, we will further sometimes split the high share young top quartile group into two equally sized subgroups, the top $1/8$ (very high share young) and the next $1/8$ (fairly high share young) and we construct the corresponding time series $\bar{y}_{vh,t}, \bar{y}_{fh,t}$ for the very high (vh) and fairly high (fh) share young groups.

The corresponding quantitative effects are estimated based solely on these time series of group aggregates, in the following basic DD-specification and conventional OLS standard errors:

$$\bar{y}_{g,t} = \alpha_t + \beta_g + \gamma \cdot \mathbf{1}(t \geq 2007) \times \mathbf{1}(g = h) + \varepsilon_{g,t}, \tag{2}$$

[43] As mentioned, we take and report unweighted averages for simplicity. Our results are robust to considering weighted averages based on 2006 employment.

Table 4—Effect of Payroll Tax Cut on Firm Outcomes

	Benchmark: high versus medium share young (1)	Fairly high versus medium share young (2)	Very high versus medium share young (3)	Unbalanced panel: high versus medium share young (4)
Number of workers	0.046	0.028	0.065	0.033
	(0.0034)	(0.0034)	(0.0043)	(0.0042)
Total assets	0.058	0.039	0.077	0.016
	(0.013)	(0.015)	(0.012)	(0.024)
Sales	0.031	0.021	0.041	0.026
	(0.0041)	(0.0029)	(0.0064)	(0.0072)
Value added	0.061	0.040	0.082	0.040
	(0.0073)	(0.0072)	(0.0081)	(0.0073)
Profits (EBIT)	0.081	0.046	0.12	0.21
	(0.012)	(0.019)	(0.019)	(0.021)
Payroll tax per worker	−0.044	−0.025	−0.063	
	(0.0051)	(0.0036)	(0.0068)	
Gross wage per worker	0.0033	0.0035	0.0031	
	(0.0014)	(0.0013)	(0.0019)	
Net wage per worker	0.019	0.013	0.024	
	(0.00082)	(0.0013)	(0.00077)	

Notes: The table presents the effects of the payroll tax cut on various firm outcomes (by row) using the aggregated longitudinal times series by firm groups and year displayed in Figures 6 and 7. Outcomes at the firm level are always measured relative to 2006 outcomes so that they are naturally normalized to 1. The exception is *profits* which are normalized by value added in 2006 (as profits are often negative). For profits, we renormalize the aggregated time-series of profits/value added in 2006 by its value in 2006 within each group to be able to interpret coefficients as percent effects. In columns 1–3, we consider a balanced panel of firms from 2003 to 2013 and with more than three employees in each year and at least one young employee in 2006. Column 4 focuses on firms with more than three employees in 2006 independent of whether they are alive or not before or after. In this column, we reweight the age distribution of firms in the medium-share young group to match that of the high share young group. For inactive firms, we replace missing outcome variables with zeros. Entries for outcomes per worker (*Payroll tax, Gross wage, Net wage*) are accordingly left empty for the unbalanced panel in column 4. *Medium share young* is defined as firms in the middle two quartiles of share young in 2006 (among firms with positive share young in 2006). *High share young* is defined as firms in the top quartile of share young in 2006. *Very high share young* is defined as firms in the top 1/8 of share young in 2006. *Fairly high share young* is defined as firms in the next 1/8 of share young in 2006 (i.e., the top quartile excluding the top 1/8). In columns 1 and 4, we compare the high share young firms to medium share young firms. In column 2 we compare the fairly high share young firms to medium share young firms. In column 3 we compare the very high share young firms to medium share young firms. In all cases, the medium share young group is the "control" group and the other group is the "treatment" group. We use years 2003–2006 and 2009–2013 (excluding years 2007–2008 when the reform was only partially phased in). We regress each outcome variable on year dummies, a treatment dummy, and an interaction of the post-reform period dummy (2009+) and the treatment dummy. The coefficient on the interaction is reported. Each regression is based on 18 observations. Conventional OLS standard errors in parentheses.

where $g = h, m$ denotes the group (high share young versus medium), t denotes the year, α_t denotes year dummies, and β_g group dummies. The coefficient γ on the interaction term post-reform times high share young group is the coefficient of interest. Here, $\varepsilon_{g,t}$ is the error term. Because the reform is only partially phased in in 2007 and 2008, we exclude these years from the regressions and hence t runs from $2003, \ldots, 2006$ and $2009, \ldots, 2013$, i.e., nine years of data. With two groups, this regression is based just on 18 observations. However, as pre-trends are very parallel and effects generally very stable across post-reform years, even these basic regressions with conventional OLS standard errors deliver precise results.

The firm-level regression results are presented in Table 4. Column 1 reports the DD effects comparing the high versus medium share young firms. We also run the

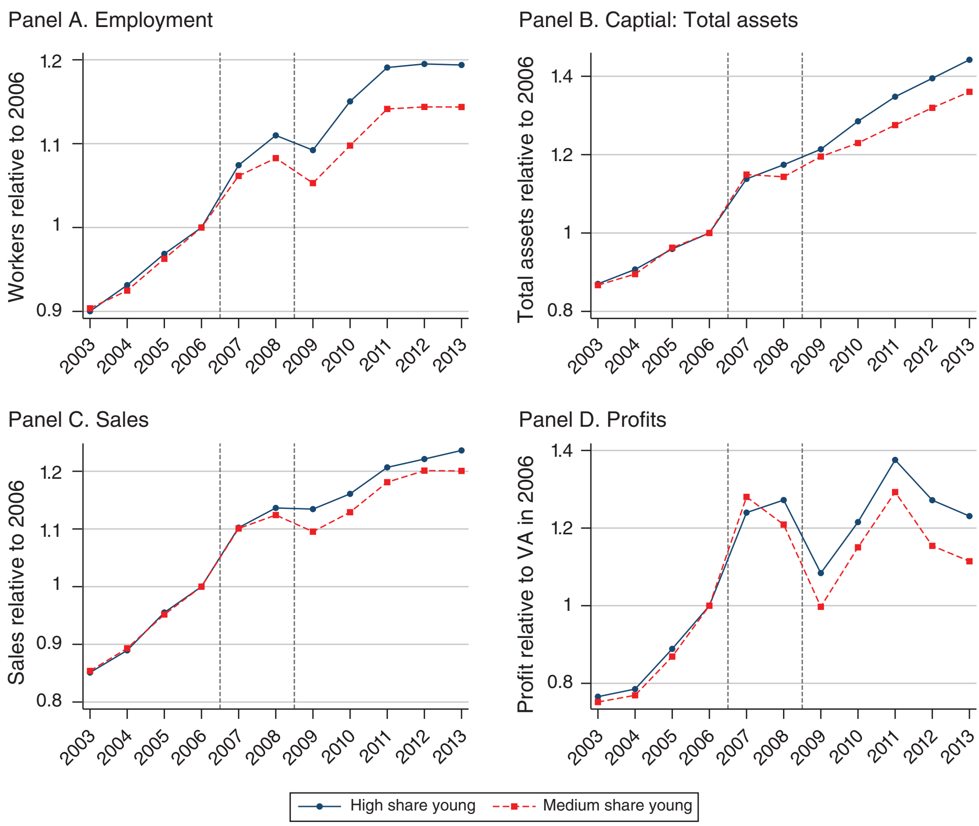


FIGURE 6. FIRM-LEVEL EFFECTS OF PAYROLL TAX CUT

Notes: This figure traces out various longitudinal outcomes of firms (relative to 2006) across a balanced sample of firms (operating in all years 2003–2013 with more than three employees in all years) by groups of firms. In each panel, we consider two groups of firms as in Figure 5: (i) firms in the middle two quartiles of share young in 2006 (*Medium share young*) and (ii) firms in the top quartile of share young in 2006 (*High share young*). Each panel depicts the (unweighted) average of the following outcomes (normalized for each firm to the 2006 value) over time by groups of firms. Panel A: *Employment* (defined as the average number of full-time-employees); panel B: *Capital: Total assets*; panel C: *Sales*; and panel D: *Profits* defined as EBIT (earnings before interest and taxes). For panel D, as profits can be negative, we normalize profits by value added in the firm in 2006 and then adjust the two series multiplicatively (so that they are normalized to 1 in 2006). All panels show that the two groups of firms have parallel pre-reform trends and the group with the largest young share (and hence largest tax windfall) experiences faster growth in employment, assets, sales, and profits after the reform. Corresponding estimates are provided in Table 4.

regressions contrasting the fairly high share young group versus the medium share young, and regressions contrasting the very high share young group versus the medium share young. These estimates are calculated exactly as above by just replacing the group h by group vh (or group fh). These additional results are reported in columns 2 and 3 of Table 4.

The graphical analysis discussed below is a critical element to provide compelling and transparent causal evidence of the payroll tax cut on firms. We view the regression results as a way to quantify the treatment effects already identified in the graphical analysis, and their precision.

Firm-Level Employment Effects.—We measure employment at the firm level by the number of workers receiving annual wage earnings from the firm above the same low threshold of \$4,940, as in Section IIC.[44] The effects on employment are depicted in panel A of Figure 6. We compare firms with medium share young (in red dashed line with squares) to firms with high share young (in solid dark blue line with circles).

Two important results stand out from panel A. First, there is a parallel trend in the growth of employees before 2006 across groups (recall that they line up at one in 2006 by normalization). Therefore, assuming that absent the reform the parallel trend would have continued, seems like a reasonable assumption. It is the critical identification assumption needed for our simple DD empirical strategy. Second, after the reform, firms with a higher share young in 2006 experience faster employment growth. The figure shows that the differential effect builds in 2007–2009 when the reform is phased in and seems stable from 2010–2013 at about 4–5 percent. Note that the two series remain very parallel after 2009 but with a clear (and hence stable) level effect. The fact that the change happens exactly when the reform starts and that it stabilizes after 2010 strongly suggests that this effect is indeed reform-driven.

To obtain additional variation, we additionally split up the high share young group into two subgroups and report their employment evolution in panel B of online Appendix Figure A14. The differential effect for the very high share young group (relative to the medium share young group) is about 2.5 times larger than the differential effect for the fairly high share young group (again relative to the medium share young group). This additional "dosage" effect is roughly proportional to the difference in first stage treatment intensity mentioned above. This quantitative relationship between employment growth and initial share young gives us further confidence that the effects we uncover are indeed causally driven by the payroll tax cut.

Corresponding regression estimates of the implied treatment effect are provided in the top row of Table 4. Comparing high share young versus medium share young firms, the payroll tax cut boosts employment by 4.6 percent, a precisely estimated effect with a standard error of 0.3 percent (based on an OLS regression with just 18 group-aggregated observations). This employment growth differential is with respect to a 2.4 percent initial differential in average labor costs that the reform induces between these two firm groups. Splitting the top group into two, we estimate an employment effect for fairly high versus medium share young at a somewhat smaller 2.8 percent, and the employment effect for very high versus medium share young is higher at 6.5 percent.

One concern could be that the Great Recession that peaked in 2009 and 2010 in Sweden affected youth intensive firm less creating a bias just after the reform is fully implemented. However, we find an effect of the reform starting in 2007 and 2008, before the Great Recession hit Sweden. The effects we find do not fade out after 2010, when the unemployment rate starts falling. Finally, the Great Recession in Sweden was fairly mild. More generally, we have also checked that our employment results are robust to controlling for industry interacted with the unemployment rate or even industry interacted with year-fixed effects.

[44] We also investigated effects on the number of workers that appear on the payroll of the firm and effects are similar.

Firms' Business Activity.—Panels B–D of Figure 6 consider 3 additional outcomes that capture firms' business activity: panel B total assets (this includes the book value of tangible and intangible capital assets as well as the market value of financial assets), panel C total sales (annual gross proceeds from all sales), panel D profits (defined as earnings before interest and taxes (EBIT)). For Panel D, as profits can be negative, we normalize profits by value added in the firm in 2006 and then adjust the two series multiplicatively so that they are normalized to 1 in 2006.[45] All three outcomes are obtained from the firms' balance sheet and income statement data used for the administration of the business tax. All four panels show that the two groups of firms have parallel pre-reform trends and the group with high share young (and hence largest cost reduction and tax windfall) experiences faster growth in assets, sales, and profits after the reform.

The graphs show that parallel trends are particularly good for total assets and sales, fairly good for profits, which should determine the confidence about the respective effects. But even for profits, the opening up after the reform is considerably larger than the gap in parallel trends before the reform. Therefore, the evidence shows that the payroll tax cut for employers was successful in boosting business activity along a number of dimensions. Note that sales effects could be due in part to producing and selling larger quantities, and in part to selling at higher prices. We unfortunately cannot distinguish between volume effects and price effects with our data.[46] But since employment as well as assets (our measure includes productive capital) expand, it is very likely that real sales increase as well.

Corresponding estimates are provided in Table 4, rows 2–5. The table also includes the effects on value-added which we use to assess the effects on the labor versus capital share just below. The regression results in column 1 show effects in the range of 3 to 6 percent, and precisely estimated. The table also shows that effects are systematically larger (and about twice as high) when comparing very high share young to medium share young (in column 3) relative to comparing fairly high share young to medium share young (in column 2). This provides further confirmation that the results we uncover are indeed driven by the payroll tax cut. The corresponding graphs showing the very high and fairly high groups time series are presented in online Appendix Figure A14.

Firm Survival.—The tax cut could have affected firm survival. This is an outcome of interest in its own right. However, such effects would also render our sample of a balanced panel of firms (2003–2013) endogenous to the reform. We address this question in online Appendix Figure A15 where we show that firms with a high share young in 2006 are no more likely to survive (or die) than firms with a medium share young. However for this exercise, it is necessary to reweight firms in the medium share young group to align their 2006 firm-age distribution to the high share young group, using the nonparametric reweighting method developed in DiNardo, Fortin,

[45] That is, we normalize firm f's profits in year t, $\pi_{f,t}$, by its 2006 value added, $VA_{f,2006}$, to obtain $\pi_{f,t}/VA_{f,2006}$. We then average this ratio across all firms in a given group g in year t, as our normalized average $\bar{\pi}_{g,t}$. We then plot the time series of $\bar{\pi}_{g,t}/\bar{\pi}_{g,2006}$ on panel D of Figure 6. It is equal to 1 for $t = 2006$.

[46] There is volume and average-price data for a smaller sample of manufacturing firms. Unfortunately, the sample size we have is too small to obtain reliable results on price effects. Furthermore, the price data are revenue divided by quantity, rather than eliciting actual micro unit prices.

and Lemieux (1996) (DFL reweighting). Absent such DFL reweighting, high share young versus medium share young firms do not exhibit parallel trends pre-reform in their likelihood to exist and hence comparisons post-reform would not be reliable.

This absence of survival effects justifies our use of the balanced panel for our main results. It also implies that the payroll tax cut affected firm outcomes only at the intensive scale margin, but not at the extensive margin.[47] This finding also implies that the Great Recession is unlikely to introduce a bias in our analysis as a differential effect of the Great Recession on young intensive firms would very likely translate into a differential survival rate during 2009 and 2010, the years when unemployment peaked.

Unbalanced Panel.—It is also possible to estimate firm effects using the full sample of firms (regardless of whether they operate in all years) and compare the two groups after DFL reweighting by age as done in online Appendix Figure A15, where we analyze survival. This exercise is presented in online Appendix Figure A16, where we trace out firm outcomes for employment, assets, sales, and profits relative to 2006 in 4 separate panels. In this case, non-operating firms are assigned zero values. Therefore, this analysis is fully robust to endogenous survival effects. Online Appendix Figure A15 shows that, thanks to DFL reweighting by firm-age, pre-trends are very well aligned for all outcomes (less so for the noisier variable of assets and profits).

After the reform, these unbalanced, DFL-reweighted graphs also show that firms with high share young expand employment, sales, and profits. Series on assets are noisy and do not generate a significant effect. Corresponding estimates are presented in Table 4, column 4. We prefer to use the balanced panel of firms active in all years 2003–2013 for our baseline results rather than this full sample because the balanced panel approach does not require any DFL reweighting, making the analysis simpler and more transparent.

Changing the Base Year.—All our results are based on dividing firm by share young in 2006. Selecting the treatment group based on 2006 generates a kink in the first stage around year 2006 due to mean reversion as we saw from Figure 5. One potential concern is that this first stage kink could translate into a kink in other outcomes, hereby generating a spurious effect around 2006. In online Appendix Figure A17, we show side-by-side that both pre-reform parallel trends and effects after 2006 survive if we instead select firms into the treatment versus control groups based on 2003 (instead of 2006).

C. *The Role of Credit Constraints*

Since the payroll tax cut affected all (young) workers, it entailed a large tax windfall and cash flow shock compared to for example hiring subsidies. Firms that are particularly constrained in their access to external finance (debt or equity) should be particularly responsive to the cash effect of the payroll tax cut (but less constrained

[47] We cannot credibly investigate firm entry in response to the policy as employment structure at entry is endogenous to the reform.

TABLE 5—FIRM EFFECTS OF PAYROLL TAX CUT BY FINANCIAL CONSTRAINT PROXIES

	FC proxy: Age of firm (1)	FC proxy: Liquid assets/total assets (2)	FC proxy: Sales (3)
Panel A. Outcome: Workers relative to 2006			
Less financially constrained	0.017	0.032	0.044
	(0.004)	(0.005)	(0.004)
More financially constrained	0.054	0.060	0.049
	(0.004)	(0.005)	(0.004)
F-test for equal effects	43.265	16.638	0.850
p-value for F-test	0.000	0.001	0.372
Panel B. Outcome: Assets relative to 2006			
Less financially constrained	0.040	0.043	0.034
	(0.015)	(0.014)	(0.016)
More financially constrained	0.053	0.068	0.075
	(0.015)	(0.014)	(0.016)
F-test for equal effects	0.395	1.561	3.199
p-value for F-test	0.540	0.232	0.095
Panel C. Difference in share young in 2006			
Less financially constrained	0.190	0.202	0.192
More financially constrained	0.203	0.191	0.201

Notes: This table displays the effects of the payroll tax cut on employment (panel A) and total assets (panel B) for financially constrained and unconstrained firms separately following the methodology of Table 4. We first divide firms based on share young in 2006 and compare firms with high share young to firms with medium share young. Second, we divide firms by financial constraint proxies: column 1 divides firms above (*unconstrained*) and below (*constrained*) median age in 2006; column 2 into above (*unconstrained*) median of liquid assets/total assets in 2006 and below; and column 3 divides firms into above (*unconstrained*) median sales in 2006 and below. We compute the mean outcome by year, treatment-control and financial constraint. We then regress the outcome on treatment, year-dummies and the interaction of treatment and post 2006 where all regressors are interacted with financial constraint-dummy, on the aggregate time series data (the complete graphical evidence is presented in Appendix Figures A18 and A19). We compare years 2009–2013 to years 1999–2006 (omitting the transitory years 2007–2008). Conventional OLS standard errors are reported in parentheses. In each panel and column, the table displays the treatment-effect for *less constrained* firms (first row) and for *more constrained* firms (second row) as well as the F-test and the associated p-value for the null of equal effects across constrained and unconstrained (third row). Panel C displays the difference in the 2006-share of payroll to young between high share and medium share firms to verify that the strength of the first stage is nearly identical in constrained versus unconstrained firms.

firms may still be affected through the standard marginal cost effect we discussed above). The literature in corporate finance has provided substantial evidence that cash windfalls matter for firms' growth perhaps due to credit constraints (see, e.g., Fazzari, Hubbard, and Petersen 1988 for a classic study). The policy discourse often loosely refers to the resources the payroll tax cut may free up for reinvestment in capital and labor, rather than the standard marginal-cost channel.

To understand whether credit constraints play a role in the firm-level effects we have uncovered, we follow a split-sample strategy by dividing the firms in 2006 by various proxies for financial constraints that have been used in the corporate finance literature (see, e.g., Farre-Mensa and Ljunqvist 2016). We consider two inputs: (i) employment and (ii) total assets (which include productive capital inputs). The results are presented in Table 5 and the corresponding time series graphs are presented in online Appendix Figures A18 and A19.

Table 5 displays the effects of the payroll tax cut on employment (panel A) and total assets (panel B) for financially constrained and less constrained (unconstrained) firms. We always compare firms in the high share young group to firms in the medium share young group but we also divide each of these two groups by financial constraint proxies. Column 1 divides firms above and below median age in 2006 as young firms are much more likely to be credit constrained. Column 2 divides firms into above versus below median of liquid assets/total assets in 2006 as firms with fewer liquid assets are much more likely to be credit-constrained. Finally, column 3 divides firms into above versus below median sales in 2006 as small firms are much more likely to be credit-constrained. The table shows the DD estimates for unconstrained firms and for constrained firms as well as the F-test and the associated p-value for the null of equal effects across constrained firms and unconstrained firms. The graphical evidence in online Appendix Figures A18 and A19 shows that the identification is compelling in the sense that pre-trends are systematically parallel (even for these additional subsamples), and a clear gap opens up right at the time of the reform and remains stable from 2010 to 2013.

Two key lessons emerge. First, in all six cases, the employment and asset growth effects on firms more likely to be credit-constrained are larger than for firms less likely to be credit constrained. However, only two of these six differences are statistically significant. Second, all 12 estimates of treatment exposure by share young are positive and significant. Therefore, these results are consistent with the credit constraint channel, although they certainly do not prove credit constraints as the sole driver of the effects. However, because our firm sample contains small firms (rather than the frequently studied US publicly traded firms), the "unconstrained" control group may be somewhat financially constrained too. Another issue is that the available credit constraints proxies might not be very accurate and misclassify firms (see Farre-Mensa and Ljunqvist 2016 for a discussion of these issues in the corporate finance literature).

A concern is that the heterogeneous effects by financial constraints are simply driven by a larger first stage difference in the share young between treatment and control for credit constrained firms (relative to less constrained firms). However, panel C of Table 5 shows that financial constraints are not systematically correlated with the treatment/control difference in 2006-share of the payroll to young worker.

Benchmarking the Implied Cash Effects.—A full model and assessment of the financial channel is beyond the scope of this paper and limited by the strong effects we find even for firms classified as less constrained. However, in online Appendix B, we evaluate our firm-level findings quantitatively by investigating whether the *size* of our treatment effect for the *average* firm could be *entirely and exclusively* rationalized by a credit constraints channel. Our benchmarks are existing estimates of the dollar-for-dollar sensitivity of capital (investment) to cash flow from the corporate finance literature. While our sample and particular design differ from existing US analyses with publicly traded, very large firms, our back of the envelope calculation suggests that our effects are of the same order of magnitude, and that the cash channel could play an important role in the firm-level effects. Specifically, our effects would correspond to a \$0.1–\$0.5 effect on capital stock per \$1 of tax windfall, which spans the range of existing estimates for the investment cash flow sensitivity for the average US Compustat firm (e.g., Fazzari, Hubbard, and Petersen 1988). The financial channel is therefore quantitatively plausible.

IV. Firm-Level Rent Sharing of the Tax Windfall

In Section IIB, we empirically rejected the standard prediction of full incidence on workers' net wages for directly treated young workers. The underlying conceptual framework is a frictionless labor market equilibrium that pins down one single market-clearing wage for each worker group. There is a growing body of evidence in labor economics that points to the role of individual firms in setting wages and in generating wage dispersion between similar workers, which may emerge in frictional labor markets (see, e.g., Card, Heining, and Kline 2013; Card et al. 2018). For our context of a tax windfall, we are particularly motivated by evidence of wages reflecting rents shared between the firm and the workers: after all, our nonstandard results imply that firms do experience labor cost reductions and thus receive tax windfalls. It is thus natural to investigate whether some of those rents might have been shared with the workers that happened to have been employed at those firms. We first investigate average wages at the firm level but then move to eliminate potential composition biases by following individual workers based on their pre-reform employer.

A. *Firm-Level Average Net Wages and Gross Wages*

We first apply our firm-level strategy from Section III to investigate how firms' average earnings per worker diverge in response to differentials in the tax windfall. Here we distinguish three firm-level concepts of average wages per worker: pre-tax average earnings inclusive of the payroll tax (gross wage earnings), and its two components: post-tax average earnings net of the payroll tax (net wage earnings) and the average payroll taxes paid per worker.

Figure 7 presents the graphical evidence. In each panel, we again compare firms with high share young versus firms with medium share young in the balanced panel of firms operating in each year 2003 to 2013 with more than three employees in each year. Panel A depicts the evolution (relative to 2006) of the average net wage earnings per worker in the firm. Panel B depicts the evolution (relative to 2006) of the average payroll taxes per worker in the firm. Panel C depicts the evolution (relative to 2006) of the average gross wage earnings per worker in the firm (i.e., the labor cost per worker paid by the employer). Importantly for our causal inference, pre-trends for all those variables are parallel between the different firm types.

Panel A shows that firms with a high share young (and hence the largest average-payroll reduction as well as the largest tax windfall) experience a faster increase in *net* wage earnings per worker. Panel B confirms that they do benefit in form of a lower payroll *tax payment* per worker. But panel C clarifies that the increase in net wages is so large that their *gross* wage payments, inclusive of payroll taxes, per worker actually do not differentially fall. Taken together, these three variants of labor costs suggests that the windfall payroll tax cut allows, or leads, firms to pay higher wages on average but that, thanks to the payroll tax cut, the labor cost per worker does not increase (but also does not decrease) on average.

Corresponding estimates are provided in column 1 of Table 4. Quantitatively, average net wage earnings increase by 1.9 percent in high share young firms

Figure 7. Firm-Level Effects on Net Wages, Payroll Taxes, and Gross Wages per Worker

Notes: This figure traces out outcomes of net wage earnings, payroll taxes, and gross wage earnings per worker (relative to 2006) across a balanced sample of firms over time by groups of firms. In each panel, we consider two groups of firms as in Figure 5: (i) firms in the middle two quartiles of share young in 2006 (*Medium share young*) and (ii) firms in the top quartile of share young in 2006 (*High share young*). Panel A depicts the evolution (relative to 2006) of the average net wage earnings per worker in the firm (i.e., earnings net of payroll taxes). Panel B depicts the evolution (relative to 2006) of the average payroll taxes per worker in the firm. Panel C depicts the evolution (relative to 2006) of the average gross wage earnings per worker in the firm (i.e., earnings gross of payroll taxes). Averages are taken across each group of firms in the balanced panel (but the composition of workers in each firm vary from year to year). The figure shows that firms with the largest young share (and hence the largest tax windfall) experience a faster increase in net wage earnings per worker, a lower payroll tax per worker, and in net experience no faster increase in gross wage earnings. This suggests that the windfall payroll tax cut allows firms to pay higher wages on average but that, thanks to the payroll tax cut, the labor cost per worker does not increase on average. Corresponding estimates are provided in Table 4.

(relative to medium share young firms). In contrast, the effect on average gross wage earnings per worker is only 0.33 percent. The reduction in payroll taxes paid per worker is estimated at 4.4 percent. In columns 2 and 3, we again split the high share young into 2 groups and find again much larger effects for the very high share young group in column 3 than for the fairly high share young group in column 2 (see online Appendix Figure A14 for the graphical time series evidence). Interestingly, the effects on labor costs are very close to 0 for both groups so that firms in the very high share young can increase net wages per worker by 2.4 percent (instead of 1.3 percent for the fairly high share young group).

Therefore, these results suggest that *at the firm level*, the tax cut not only stimulates business activity and employment but also benefits workers in the form of higher wage earnings. The total effect on value added is larger than the initial tax windfall but these large firm-specific effects might come at the expense of other firms.

Labor and Capital Shares.—How did the total effect of the tax windfall, including its effect on business activity and rent sharing, shift the share of labor and capital in value added? To ballpark this distributional outcome, our firm-level design suggests a simple calculation based on treatment effect estimates in Table 4. The highly exposed firms' count of workers increased by 4.6 percent and the net average wage per-worker increased by 1.9 percent compared to the control firms. Therefore, the net wage bill increased by the sum of these two effects, i.e., around 6.5 percent. Value added increased by 6.1 percent. The labor share therefore appears to have been fairly stable in response to the tax windfall.[48]

B. *Individual-Level Wage Responses to Firms' Tax Windfalls*

One concern with the firm-level analysis is that the composition of workers may change for the treatment group post-reform. After all, our evidence in Section III shows that treated firms expand across the board, in particular in employment (panel A of Figure 6); and, we know that the share of young workers mean-reverts moderately (panel B of Figure 5). If these new workers differ in their characteristics, they could drive up or down the *average* wages in the firms through composition effects.

Data and Sample.—To address this issue, we create a matched employer-employee version of our administrative firm- and worker-level data to conduct an *individual-level* analysis that follows workers over time. Instead of following firms, we now follow individual workers, grouping them by their 2006 employer (i.e., just before the reform), again using the split of firms by their share young as of 2006 as in Section III. We then trace out these individual worker-level outcomes from 1999 to 2013, regardless of whether they switch firms or drop out of employment (in which case their wage earnings are zero).[49] Our sample includes all workers aged 25–60 in 2006 matched to our firm sample from Section III as of 2006, but we initially do not impose any restrictions on job mobility or employment status outside of 2006. We pick this prime-aged worker group because they are old enough to have interpretable pre-trends. And, none of these workers will ever be *directly* affected by the payroll tax cut, as they are too old to benefit from it when the tax cut is implemented in 2007 (for workers aged 25 or less) and in 2009 (for workers aged 26 or less), rendering any wage effect consistent with rent-sharing patterns of the tax windfall. Our wage concept is now the annual earnings with a given employer in a given year, as we now use the tax-based income data in order to track individual workers across time and any future or past employer. We describe these data in Section IB.

Worker-Level Rent-Sharing Effects.—The results on average net wage earnings (i.e., exclusive of the payroll tax) are presented in Figure 8. The figure depicts the

[48] While profits increased by 8.1 percent, profits are a leveraged variable and only capture one component of the capital share (see Table 3 for firms' descriptive statistics).

[49] The panel is almost perfectly balanced, as international migration and deaths are the only attrition sources.

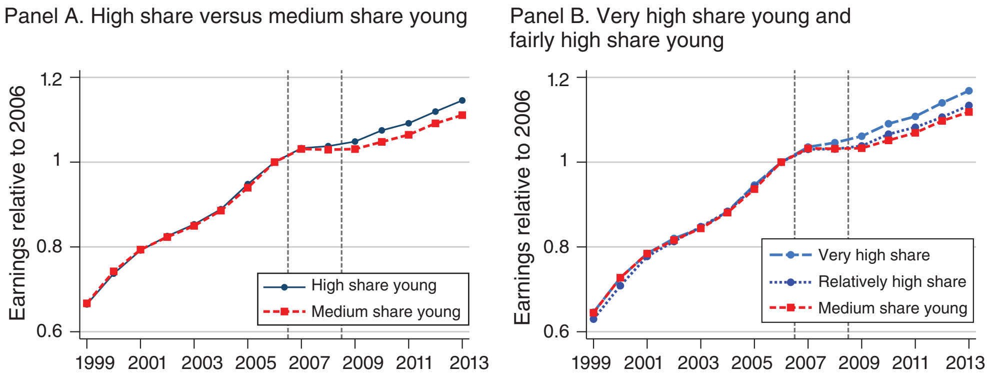


Figure 8. Effects of Payroll Tax Cut on Longitudinal Individual Net Wage Earnings

Notes: This figure traces out longitudinal individual net wage earnings (relative to 2006) based on share young at the firm the individual was working at in 2006. We consider the same balanced panel of firms as in Figure 5. We follow individual workers employed in these firms as of 2006 regardless of whether individuals change jobs or work (non-workers are assigned zero earnings). In panel A, we consider two groups of individuals: (i) individuals working in a medium share young firm in 2006 and (ii) individuals working in a high share young firm in 2006. In panel B, we further split the high share young group into two subgroups: (iia) individuals working in a fairly high share young, (iib) individual working in a very high share young. Both panels depicts the evolution (relative to 2006) of average individual earnings for all individuals aged 25–60 as of 2006. In both graphs, we DFL-reweight by five year age cells to control for the age structure across groups (as groups are selected based on fraction young and hence are not balanced in terms of the age distribution). Both panels show that individuals working in 2006 (just before the reform) in a firm with a large share young (and hence largest tax windfall) experience a faster increase in net wage earnings after the reform. Note that all these workers are too old to be directly affected by the payroll tax cut. Corresponding estimates are provided in Table 6.

evolution (relative to 2006) of the average wage earnings per worker group (i.e., 2006 firm group) for all individuals aged 25–60 as of 2006. In panel A, we consider two groups of individuals: (i) individuals working in a medium share young firm in 2006 and (ii) individuals working in a high share young firm in 2006. In panel B, we further split the high share young group into two subgroups: (iia) individuals working in a fairly high share young in 2006, (iib) individual working in a very high share young in 2006. In both graphs, we DFL-reweight by five-year age cells to control for the age structure across groups. DFL reweighting is necessary because the age distribution of workers is different across the two groups of firm by construction of the groups. Because of strong age effects in the profile of individual earnings growth (see Figure 2), the pre-trends could not be parallel without DFL reweighting (and would differ post-reform even absent a treatment effect). However, with DFL-reweighting, the pre-trends are close to perfectly parallel in both groups, i.e., raw pre-trend differences were entirely due to age composition differences.[50]

Three findings are worth noting. First, in both panels, pre-trends are very parallel giving us confidence that our difference-in-differences design is credible. Second, panel A shows that individuals working in 2006 (just before the reform) in a firm

[50] We have more pre-reform years than in our previous firm-level analysis. This is because individual earnings data start earlier than firm-level balance sheet data, and we only use firm-level data in 2006 for this exercise. We have also checked that our results are robust to further controlling (beyond age) for gender, industry, and education using DFL reweighting or a regression-based approach.

Table 6—Collective Incidence of Payroll Tax Cut on Individual Earnings

	Benchmark: high versus medium share young (1)	Fairly high versus medium share young (2)	Very high versus medium share young (3)
All ages (25–60)	0.026	0.015	0.037
	(0.0028)	(0.0043)	(0.0031)
Ages 25–30	0.034	0.017	0.046
	(0.0089)	(0.010)	(0.011)
Ages 31–40	0.028	0.0063	0.051
	(0.0036)	(0.0030)	(0.0055)
Ages 41–50	0.030	0.026	0.034
	(0.0020)	(0.0028)	(0.0039)
Ages 51–60	0.0048	−0.0019	0.012
	(0.0031)	(0.0048)	(0.0039)
Earnings below within-firm median	0.041	0.015	0.068
	(0.0049)	(0.0047)	(0.0084)
Earnings above within-firm median	0.018	0.015	0.025
	(0.0029)	(0.0046)	(0.0035)
Women	0.012	0.011	0.015
	(0.0034)	(0.0036)	(0.0049)
Men	0.022	0.020	0.023
	(0.0017)	(0.0024)	(0.0039)

Notes: This table shows effects on average individual net wage earnings (relative to 2006 earnings) at the worker-level of having been working in a firm with a high share young in 2006 (just before the reform). In column 1 we compare workers in high share young firms to workers in medium share young while columns 2 and 3 compares workers in very high and fairly high share young (respectively) to workers in medium share young. The first row shows effects on net wage earnings for those workers aged 25–60 in year 2006. The next four rows split the sample into different age groups. In rows 6 and 7 we instead split the sample of individuals aged 31–40 into below-median earnings in 2006 versus above. In the last two rows we split the sample of individuals aged 41–50 into women and men. In all estimations except the last two rows, we DFL-reweight the age-distribution of the workers in firms with medium-share young to match the age-distribution of those working in firms with a high share young, using five-year age-categories. The last rows DFL-reweight based on five-year age-categories and one-digit industry-categories, based on 2006-employment. All the estimates are based on a basic regression using solely the aggregated time series depicted in Figures A20 and 10. We compare years 2009–2013 to years 1999–2006. Conventional OLS standard errors in parentheses.

with a large share young (and hence largest tax windfall) experience a faster increase in earnings after the reform. Third, panel B shows that this positive individual earnings effect is larger yet for individuals working in firms with a very high share young than for individuals working in firms with a fairly high share young. This suggests that the wage effects we uncover are indeed driven by the payroll tax cut.

The earnings estimates are provided in the first row of Table 6 where we consider all workers aged 25–60. Following our standard approach, column 1 compares the high share young group to medium share young group and finds a positive individual earnings effect of 2.6 percent, a highly significant effect. This effect is somewhat larger in size to the average net-wage effect of 1.9 percent that we documented at the firm-level in Figure 7 and Table 4. This could be due to composition effects as the firm level analysis includes new hires who are often paid less. In columns 2 and 3, we again split the high share young into two groups and find again much larger effects, about twice as high, for the very high share young group in column 3.

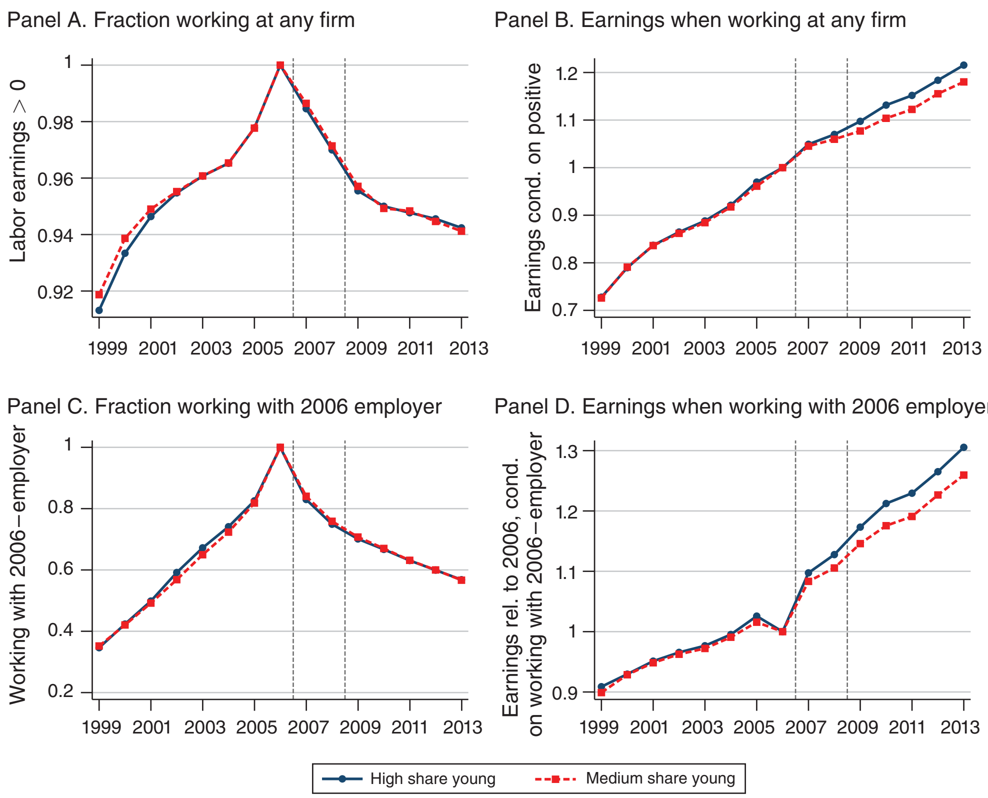


FIGURE 9. INDIVIDUAL-LEVEL EFFECTS ON EMPLOYMENT AND EARNINGS

Notes: This figure repeats the setting of Figure 8 where we follow individuals based on share young at the firm they are working at in 2006. We consider two groups of individuals: (i) individuals working in a medium share young firm in 2006 and (ii) individuals working in a high share young firm in 2006. In panel A, we plot the fraction of individuals with positive earnings during the year (regardless of employer). This fraction is equal to 1 in 2006 by definition of the sample. In panel C, we plot the fraction of individuals with positive earnings during the year with the same employer they were working at in 2006. This fraction is also by definition equal to 1 in 2006. In panel B, we plot the average earnings of individuals with positive earnings during the year (regardless of employer), i.e., conditional on employment. In panel D, we plot the earnings of the subset of those individuals that work with the 2006 employer in a given other year, that is only earners with the 2006 employer are plotted. All panels show parallel pre-reform trends. Panels A and B show that working at the high share young firm in 2006 (relative to medium share young) has no effect post-reform on the extensive margin of overall employment (panel A) or employment at the same firm (panel C). Panels B and D show that working at the high share young firm in 2006 (relative to medium share young) has a positive effect post-reform on the intensive margin of earnings (panel B) or on earnings with the 2006 employer (panel D). Hence, the effects we uncover are intensive margin effects on earnings and moreover exhibited by workers staying with the original 2006 employer.

These results imply that workers did benefit from the tax windfall their immediate employer received. Quantitatively, our individual-level wage effect lines up with the predicted reduction in average (or total) labor costs the high share young firm would experience over the medium share firms absent a wage response: 2.4 percent. As a result, our cross-sectional results reveal pass-through of firm's idiosyncratic exposure to the tax windfall onto (all) workers' net wages.

The Sources of the Wage Effect.—Our individual-level wage strategy differs from the firm-level perspective on average wages not only by removing composition bias.

By allowing for job mobility and nonemployment in our intent-to-treat design based on workers' 2006 employer, our design also allows workers to potentially benefit from the windfall by moving up the job ladder, or through lower unemployment risk. We explore this in Figure 9. This figure repeats the setting of Figure 8 where we follow individuals based on share young at the firm they are working at in 2006. In panel A, we plot the fraction of individuals with positive earnings during the year (regardless of employer). This fraction is equal to 1 in 2006 by definition of the sample. The figure does not show any effect along this extensive margin of employment. In panel B, we plot the average earnings of individuals with positive earnings during the year (regardless of employer), i.e., conditional on employment. Consistent with the absence of extensive effects from panel A, we find that the intensive-margin treatment effect on average earnings conditional on employment is similar to the unconditional earnings outcome that included zeros from Figure 8, panel A. In panel C, we plot the fraction of individuals with positive earnings during the year with the *same* employer they were working at in 2006. This fraction is also by definition equal to 1 in 2006. We do not find any effect on the fraction of individuals staying (or, in pre periods, already employed) with their 2006 employers. Finally, in panel D, we plot average earnings for the subset of individuals that work with the 2006 employer in a given other year (and discarding individuals who do not have any earnings with the 2006 employer). The series exhibit the original treatment effect on worker earnings. In summary, we find that *stayers* with the original employer drive the wage effect through *intensive margin* responses of earnings.

Rent Sharing?—Which mechanism may explain the wage effect at the firm level? Recall that none of the sampled workers aged 25–60 in 2006 ever benefit directly from the payroll tax cut as it applies only to workers aged 25 or less in 2007–2008 and 26 or less in 2009 and after. That is, we find that at the firm level, tax incidence spills over to workers ineligible for the tax cut.[51] Larger tax windfalls (from larger share of young workers pre-reform) appear to lead firms to offer (or workers to demand) wage increases to their employees *collectively*, not just to the young workers who trigger the windfall. This evidence is consistent with *rent sharing of windfalls* within the firm and consistent with earlier empirical evidence obtained in other (non-payroll tax) contexts. Our study differs from existing research in that we have a within-firm group of workers that we can cleanly identify as an unaffected group except for spillovers from rent sharing. In contrast to our market-level analysis from Section II, which showed that eligible workers' market wage did not benefit specifically and differentially, our firm-level evidence for rent sharing reveals that workers do benefit, collectively, from the payroll tax cut.

Next, we probe deeper into the collective tax incidence effects by considering heterogeneity by age and heterogeneity by initial earnings level within the firm, and

[51] Kline et al. (forthcoming) find positive effects of successful patenting on workers' wages using US matched employer-employee data for the cohort present at the patent approval only. This pattern could reflect performance-pay contracts. Our context differs in that the tax windfall benefits workers who are not directly treated (too old to ever be directly eligible themselves), and yet in the individual level design, we still document wage effects for those older workers. Rather than performance pay contracts, our findings are therefore consistent with rent-sharing mechanisms.

then explore effects on various percentiles of the earnings distribution (instead of considering only the average earnings as we have done here).

Heterogeneity by Age.—In Figure 7, we considered all workers aged 25–60. We now split this sample into four age groups: 25–30, 31–40, 41–50, 51–60 and estimate effects for each sub-age group. The graphical evidence for these 4 groups is presented in panels A–D of online Appendix Figure A20. The figure shows that pre-trends are parallel for each of the four groups and that positive effects arise just after the reform. The corresponding DD estimates are presented in rows 2–5 of column 1 of Table 6. All age groups except the oldest group 51–60 display significant earnings effect on the order of 3 percent and highly significant. The oldest group 51–60 estimate is smaller (0.5 percent) and not statistically significant. Columns 2 and 3 report the effects of comparing the medium share young groups to the fairly high share young group and the very high share young group. Again, the effects are systematically larger for the very high share young group. In this case, even the older group 51–60 displays a significant effect although fairly small in magnitude (1.2 percent). These results suggest that the collective incidence happens broadly and is not limited to workers most closely resembling the young treated group along the age dimension.

What is the effect of the reform on the wages of young workers directly affected by the reform? These workers are very young before the reform so that the pre-reform trends are shorter, noisier, and not quite parallel across the two groups of firms. Therefore, we unfortunately cannot credibly estimate the effect on the earnings of young workers directly benefiting from the tax cut when the reform starts.

Heterogeneity by Earnings Rank within the Firm in 2006.—Next, we split the sample by relative earnings groups within their 2006 employer. More specifically, we consider two groups: workers with 2006 earnings above the firm median in 2006 (high earners) and workers with 2006 earnings below the firm median in 2006 (low earners). We compare workers in a single broad cohort (aged 31–40) to get parallel trends and hence credible identification. Figure 10 depicts the average earnings in workers at high versus medium share young firms in 2006 for high earners in panel A and low earners in panel B. Pre-trends are parallel in both panels, and particularly so for lower earners in panel B. Interestingly, the percent effects on earnings are much larger, about twice as large, for *lower* earners than for higher earners.[52] This is confirmed in the last two rows of Table 6, which provide the corresponding estimates on wage earnings: 4.1 percent for lower earners, and 1.8 percent for higher earners, both highly significant and precisely estimated so that the two estimates are clearly statistically different. Again, columns 2 and 3 show that these differential effects between low and high earners are much stronger when using the very high share young group rather than the fairly high share young group. These within-firm results suggest that the collective incidence of the payroll tax cut on wages is progressive

[52] Note that selecting workers based on their position within the firm in 2006 creates visible mean reversion patterns. High earners experience a sharp increase in earnings in 2006 and a stagnation in earnings in 2007. Conversely, low earners experience a slow increase in earnings in 2006 and a sharp increase in earnings in 2007. Fortunately, these mean reversion effects are identical for the high share young group and the medium share young so that the credibility of the empirical design is not affected.

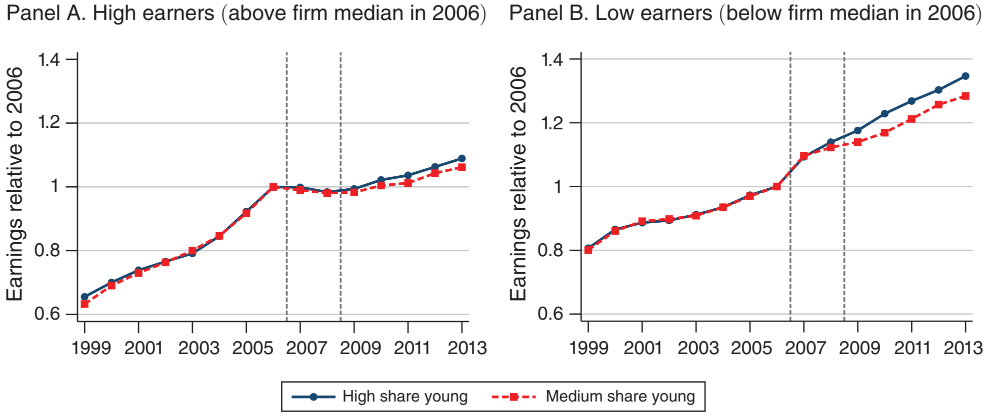


Figure 10. Effects on Longitudinal Individual Net Wage Earnings: High versus Low Earners

Notes: This figure is built like Figure 8 but considers a narrower group of workers aged 31–40 (in 2006) and splits this sample into high earners (panel A) versus low earners (panel B). Low (resp., high) earners are defined as workers with earnings below (resp., above) the median at the firm they were working in as of 2006. The median is defined relative to all workers (of any age) at the firm in 2006. The graphs show that individuals working in 2006 (just before the reform) in a firm with a large share of young workers (and hence largest tax windfall) experience a faster increase in net wage earnings after the reform. These positive earnings effects are more pronounced for low earnings workers (panel B) than for high earnings workers (panel A). This suggests that the collective tax incidence rent sharing following the tax windfall benefits low earning workers relatively more. Corresponding estimates are provided in Table 6.

within firms. Firms use the tax windfall to increase wages of non-eligible workers and increase wages of lower earners by more (in percentage terms). This evidence of progressive distribution of windfalls within firms could well be facilitated by the presence of unions, consistent with the analysis of Pencavel (1991, pp. 73–77).

Heterogeneity by Gender.—The last 2 rows of Table 6 examine the effects by gender. We focus on a single broad cohort (aged 41–50) and also DFL-reweight by broad industry group in 2006 to obtain parallel pre-trends and hence compelling estimates. The corresponding time series are depicted in online Appendix Figure A20, panels E–F. We find larger earnings effects for men (2.2 percent) than for women (1.2 percent) suggesting that men are able to capture a larger share of the rents generated by the tax windfall. This is consistent with the findings of Kline et al. (2017) on rent sharing of patents within firms.

Effects on the Earnings Distribution.—To cast further light on the distributional aspects of the collective tax incidence results, we come back to our initial design from panel A of Figure 8, but look at effects for other moments of the earnings distribution besides the mean (as we did in Figure 8). Instead, we now look at various percentiles: P20, . . . , P90 but we otherwise follow exactly the same methodology. We follow individual workers employed in high share young versus medium share young firms as of 2006 (and aged 25–60 in 2006) and regardless of whether individuals change jobs as in Figure 8. We again DFL-reweight by five-year age cells. Estimates for deciles P20, P30, . . . , P90 are presented in column 1 of Table 7. The estimates are monotonically decreasing across deciles (except for the P90 estimate

Table 7—Distributive Incidence of Payroll Tax Cut on Individual Earnings

Earnings percentile	Benchmark (1)	Placebo reform in 2003 (2)
P-20	0.064	−0.010
	(0.008)	(0.007)
P-30	0.057	0.005
	(0.010)	(0.006)
P-40	0.040	−0.002
	(0.007)	(0.003)
P-50	0.032	0.003
	(0.004)	(0.004)
P-60	0.023	0.002
	(0.003)	(0.002)
P-70	0.018	0.002
	(0.002)	(0.002)
P-80	0.013	0.004
	(0.001)	(0.004)
P-90	0.014	0.009
	(0.002)	(0.005)

Notes: This table shows effects on various percentiles of the individual net wage earnings distribution (relative to 2006) at the worker level for having been working in a firm with a high share young in 2006 (just before the reform). Estimates are constructed as in Table 6 but, instead of considering averages, we consider various percentiles P-20, P-30, . . . , P-90. Column 1 compares workers in high share young firms to workers in medium share young while columns 2 and 3 compare workers in very high and fairly high share young (respectively) to workers in medium share young. All the estimates are based on a basic regression using solely the aggregated time series depicted in online Appendix Figure A21. We exclude estimates for P-10 as low percentiles are noisy (see the P-10 graphical representation on Figure A21). We compare years 2009–2013 to years 1999–2006. Conventional OLS standard errors in parentheses.

being very slightly above P80). Hence, the effects of the payroll tax cut on individual earnings (as a percentage of 2006 earnings) are highest for the lowest earners and decreasing across the percentile distribution, implying that the rent sharing of the payroll tax cut is progressive across the distribution of individual earnings.[53] Using estimates of earnings levels at each percentile (as of 2006), the percentage effects we find suggests that the absolute dollar gains are also progressive: they are about constant for percentiles P20, P30, P40, and P50 and somewhat decreasing above P50. This evidence of progressive distribution of windfalls across the wage distribution is consistent with prior work on unions (see, e.g., Card 1996 and Card, Lemieux, and Riddell 2004).

As an additional placebo test, we repeat the analysis assuming that the reform took place in 2003 and comparing earnings in 1999–2002 versus 2004–2006 (always before the actual reform happened). The corresponding estimates are reported in column 2 of Table 7. This placebo test displays no significant wage effect for *any* percentile. There is no indication that the placebo effects are decreasing across percentiles; if anything, they are slightly increasing, although this increase is not

[53] The corresponding graphical evidence for each percentile is presented in online Appendix Figure A21. All graphs show parallel pre-trends and clear effects opening up at the time of the reform. It is clear from the figures that effects for lower percentiles are significantly larger than for higher percentiles.

statistically significant. This placebo result suggests that the progressive gradient is a causal effect of the reform.

The Implied Elasticity of Firm-Specific Labor Supply.—Together, the firm-level treatment effects of 4.6 percent on employment (Table 4, column 1), and of 1.9–2.6 percent on wages (Table 4, column 1 for net average wages at the firm; Table 6, column 1 for workers of all ages adjusted for composition) implies an elasticity of firm-specific labor supply of 1.8 to 2.4. This implied elasticity is consistent with existing estimates (see Manning 2003 and Ashenfelter, Farber, and Ransom 2010 for reviews). In the online Appendix, we propose a monopsony model that accounts for our key market and firm level facts with a pay equity constraint within firms.

Summary.—Our finding of firm-level collective tax incidence contrasts with the standard model of payroll tax incidence, whereby the market mechanism would limit wage increases to young workers eligible for the payroll tax cut, and would be identical for all young workers irrespective of their firm. A market wage analysis only would have missed these firm-level wage effects. They would also be masked in aggregate macro studies of homogeneous payroll taxes, where market-level incidence through standard mechanisms would be observationally equivalent to our firm-level transmission mechanism. While our study is naturally limited to the particular payroll tax cut in Sweden for young workers, our cuts of the data (inframarginal workers at heavily versus moderately exposed firms) do suggest an alternative transmission mechanism of payroll tax incidence into the rent component of wages, besides the canonical market-wage adjustment.

The firm-level wage effects are consistent with rent sharing, whereby the payroll tax cut increases firms' profits and part of these extra profits are distributed back to all workers (not just the tax cut eligible workers), with an intra-firm wage-equity constraint. A strength of our design is that we have used the eligibility features of the reform to identify workers never directly affected by the policy who yet benefit from its firm-level windfall through spillovers. Previous studies typically use firm-level shocks (industry rents, patents, or other shifters of productivity) without clean markers for directly versus indirectly affected workers within the firm.

V. Conclusion

Our paper provides a comprehensive analysis of a large employer payroll tax cut targeted to young workers in Sweden. The payroll tax cut was effective in reducing youth unemployment. Rather than the canonical market mechanisms, we have found that within-firm mechanisms were crucial in transmitting the incidence of the policy intervention onto labor market outcomes.

What Model Can Account for the Facts?—We have explained in detail why our empirical findings cannot be reconciled with the standard model of competitive spot labor markets. One parsimonious refinement can explain most of our empirical findings: *a wage equity constraint within firms*. We develop this model formally in the online Appendix but it can be described informally as follows. It is a competitive model where firms use both young and old workers for production. But they are constrained to pay the same net-of-payroll tax wages to young and old workers. This constraint creates involuntary unemployment among the young (their equilibrium wage should be lower than the old absent the constraint because their productivity is lower). In that context, a payroll tax cut for the young offsets the inefficiently high net wage, and hence reduces unemployment among the young, consistent with our market level results. It also benefits both young and old workers' wages in equilibrium, consistent with our findings that wages in general increase following the tax cut. Furthermore, we can also obtain the key results from our firm-level analysis by introducing two types of firms: (i) firms whose production technology leads them to hire mostly young workers and (ii) firms which use mostly old workers; the pay equity concerns constrain wages within each firm but not across firms. We also assume that there is heterogeneity in workers' preferences about what type of firm they want to work in, so that workers distribute themselves across firms even if all firms do not pay identical wages. This effectively creates a monopsony effect for firms, which face a finite supply elasticity of workers. In this extension, we obtain the following two additional results. First, following a payroll tax cut for the young, the youth-intense firms expand more (across the board) than the old-intense firms. Second, the wages of all workers (young and old) in youth-intense firms increase by more than wages in firms which use mostly old workers. We think that the pay equity constraint, perhaps driven by union wage bargaining, along with monopsony power of firms is the most plausible model that can explain our empirical findings. But it is conceivable that other mechanisms such as classical production complementarities between young and old workers could be at play as well.

Policy Consequences.—What are the policy consequences of our findings? First, targeted employer payroll tax cuts could be a useful tool to fight inefficiently high unemployment, which is particularly costly for young workers.[54] Payroll tax cuts can be targeted to groups with particularly high unemployment rates (such as the young, depressed geographical areas, or lower paid workers), as has been done in actual policy practice. Some particular features of the tax cut we study may have enhanced its effectiveness. It was employer borne, salient, administered in a way to ensure near-perfect, immediate and automatic take-up, it targeted young workers but was encompassing (i.e., applied not just to new hires out of unemployment or a subset), it was intended to be permanent, and it was large.[55]

Unlike cuts to minimum wages, an employer payroll tax cut lowers labor costs without lowering workers' take-home wages. Although such payroll tax cuts reduce government revenue, the cost per job created is not a meaningful statistic, as revenue could be recouped by increasing other taxes in a distributionally neutral way while preserving the beneficial employment effects. Second, there might be positive employment and business activity effects beyond the effects on the targeted

[54] Kahn (2010) and Oreopoulos, von Wachter, and Heisz (2012) document the long-term costs of entry into slack labor markets for young workers. The standard competitive model does not have inefficiently high unemployment. Our wage equity constraint within firms, as in our theoretical model in the online Appendix, is one particular price friction that triggers inefficient involuntary youth unemployment. More generally, search and matching models can have inefficient unemployment.

[55] Katz (1998) discusses many of these features as important factors in the effectiveness of wage subsidies.

population as firms most exposed to the tax cut increase hiring of all workers (and their wages), and thus in turn payroll tax payments. However, it is possible that such business gains could come at the expense of other firms. Hence, the general equilibrium effects are challenging to estimate. Third, the actual distributional incidence is actually much more complex than originally thought both according to the public finance workhorse model and the policy discourse. In the Swedish case we have analyzed, lower paid workers seem to have benefited disproportionately from the tax cut. Fourth, we found dramatic heterogeneity in the effectiveness of the tax cut by local unemployment and correspondingly heterogeneous costs, and scope to more narrowly target the policy.

The policy was pitched by proponents, who passed the bill when elected, as a way to stimulate employment among the young and business activity in general. It was criticized by opponents, who ultimately repealed the tax cut in 2015 when elected, as being too costly relative to the number of jobs created and a give-away to employers. Our qualitative findings align more with the former view than the latter, as our results show that there were positive employment effects among the youth, particularly so in regions with high youth unemployment rates. Furthermore, our results also show that the tax cut stimulated business activity and was in part redistributed back to workers, and particularly so to lower paid workers. Hence, employers did not just pocket the tax cut. A complete quantitative cost-benefit analysis is challenging as our difference-in-differences analysis cannot uncover all general equilibrium effects on older workers. However, the evidence we present paints a positive picture of the potential of targeted employer payroll tax cuts as powerful policy levers to reduce youth unemployment.

REFERENCES

Agell, Jonas, and Per Lundborg. 1995. "Theories of Pay and Unemployment: Survey Evidence from Swedish Manufacturing Firms." *Scandinavian Journal of Economics* 97 (2): 295–307.

Akerlof, George A., and Janet L. Yellen. 1990. "The Fair Wage-Effort Hypothesis and Unemployment." *Quarterly Journal of Economics* 105 (2): 255–83.

Ashenfelter, Orley C., Henry Farber, and Michael R. Ransom. 2010. "Labor Market Monopsony." *Journal of Labor Economics* 28 (2): 203–10.

Becerra, Oscar. 2017. "Labor Demand Responses to Payroll Taxes in an Economy with Wage Rigidity: Evidence from Colombia." Unpublished.

Bennmarker, Helge, Lars Calmfors, and Anna Seim. 2014. "Earned Income Tax Credits, Unemployment Benefits and Wages: Empirical Evidence from Sweden." *IZA Journal of Labor Policy* 3 (54).

Benzarti, Youssef, Dorian Carloni, Jarkko Harju, and Tuomas Kosonen. 2017. "What Goes up May Not Come Down: Asymmetric Incidence of Value-Added Taxes." NBER Working Paper 23849.

Bewley, Truman F. 2002. *Why Wages Don't Fall during a Recession.* Cambridge, MA: Harvard University Press.

Blanchard, Olivier J., and Lawrence H. Summers. 1986. "Hysteresis and the European Unemployment Problem." In *NBER Macroeconomics Annual,* Vol. 1, edited by Stanley Fischer, 15–90. Cambridge, MA: MIT Press.

Blinder, Alan S., and Don H. Choi. 1990. "A Shred of Evidence on Theories of Wage Stickiness." *Quarterly Journal of Economics* 105 (4): 1003–15.

Bozio, Antoine, Thomas Breda, and Julien Grenet. 2017. "Incidence of Social Security Contributions: Evidence from France." Unpublished.

Breza, Emily, Supreet Kaur, and Yogita Shamdasani. 2018. "The Morale Effects of Pay Inequality." *Quarterly Journal of Economics* 133 (2): 611–63.

Budd, John W., Jozef Konings, and Matthew J. Slaughter. 2005. "Wages and International Rent Sharing in Multinational Firms." *Review of Economics and Statistics* 87 (1): 73–84.

Cahuc, Pierre, Stéphane Carcillo, and Thomas Le Barbanchon. 2014. "Do Hiring Credits Work in Recessions? Evidence from France." IZA Discussion Paper 8330.

Campbell, Carl M. III, and Kunal S. Kamlani. 1997. "The Reasons for Wage Rigidity: Evidence from a Survey of Firms." *Quarterly Journal of Economics* 112 (3): 759–89.

Card, David. 1996. "The Effect of Unions on the Structure of Wages: A Longitudinal Analysis." *Econometrica* 64 (4): 957–79.

Card, David, Ana Rute Cardoso, Joerg Heining, and Patrick Kline. 2018. "Firms and Labor Market Inequality: Evidence and Some Theory." *Journal of Labor Economics* 36 (S1): S13–70.

Card, David, Jorg Heining, and Patrick Kline. 2013. "Workplace Heterogeneity and the Rise of West German Wage Inequality." *Quarterly Journal of Economics* 128 (3): 967–1015.

Card, David, and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4): 772–93.

Card, David, Thomas Lemieux, and W. Craig Riddell. 2004. "Unions and Wage Inequality." *Journal of Labor Research* 25 (4): 519–62.

Card, David, Alexandre Mas, Enrico Moretti, and Emmanuel Saez. 2012. "Inequality at Work: The Effect of Peer Salaries on Job Satisfaction." *American Economic Review* 102 (6): 2981–3003.

DiNardo, John, Nicole M. Fortin, and Thomas Lemieux. 1996. "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semiparametric Approach." *Econometrica* 64 (5): 1001–44.

Dube, Arindrajit, Laura Giuliano, and Jonathan Leonard. 2019. "Fairness and Frictions: The Impact of Unequal Raises on Quit Behavior." *American Economic Review* 109 (2): 620–63.

Dube, Arindrajit, Jeff Jacobs, Suresh Naidu, and Siddharth Suri. Forthcoming. "Monopsony in Online Labor Markets." *American Economic Review: Insights*.

Egebark, Johan. 2016. "Effects of Taxes on Youth Self-Employment and Income." IFN Working Paper 1117.

Egebark, Johan, and Niklas Kaunitz. 2013. "Do Payroll Tax Cuts Raise Youth Employment?" Institute for Evaluation of Labour Market and Education Policy Working Paper 2013:27.

Egebark, Johan, and Niklas Kaunitz. 2018. "Payroll Taxes and Youth Labor Demand." *Labour Economics* 55: 163–77.

Farre-Mensa, Joan, and Alexander Ljungqvist. 2016. "Do Measures of Financial Constraints Measure Financial Constraints?" *Review of Financial Studies* 29 (2): 271–308.

Fazzari, Steven M., Robert Glenn Hubbard, and Bruce C. Petersen. 1988. "Financing Constraints and Corporate Investment." *Brookings Papers on Economic Activity* 1: 141–206.

Fredriksson, Peter, and Robert H. Topel. 2010. "Wage Determination and Employment in Sweden since the Early 1990s: Wage Formation in a New Setting." In *Reforming the Welfare State: Recovery and Beyond in Sweden,* edited by Richard B. Freeman, Birgitta Swedenborg, and Robert H. Topel, 83–126. Chicago: University of Chicago Press.

Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch. 2018. "Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany." *American Economic Review* 108 (2): 393–418.

Fullerton, Don, and Gilbert E. Metcalf. 2002. "Tax Incidence." In *Handbook of Public Economics*, Vol. 4, edited by Alan J. Auerbach and Martin Feldstein, 1787–872. Amsterdam: Elsevier.

Galuscak, Kamil, Mary Keeney, Daphne Nicolitsas, Frank Smets, Pawel Strzelecki, and Matija Vodopivec. 2012. "The Determination of Wages of Newly Hired Employees: Survey Evidence on Internal Versus External Factors." *Labour Economics* 19 (5): 802–12.

Harasztosi, Péter, and Attila Lindner. 2017. "Who Pays for the Minimum Wage?" Unpublished.

Kahn, Lisa B. 2010. "The Long-Term Labor Market Consequences of Graduating from College in a Bad Economy." *Labour Economics* 17 (2): 303–16.

Katz, Lawrence F. 1998. *Wage Subsidies for the Disadvantaged.* New York: Russell Sage Foundation.

Kaunitz, Niklas, and Johan Egebark. 2017. "Payroll Taxes and Firm Performance." IFN Working Paper 1175.

Kline, Patrick, Neviana Petkova, Heidi Williams, and Owen Zidar. Forthcoming. "Who Profits from Patents? Rent-Sharing at Innovative Firms." *Quarterly Journal of Economics*.

Kramarz, Francis, and Thomas Philippon. 2001. "The Impact of Differential Payroll Tax Subsidies on Minimum Wage Employment." *Journal of Public Economics* 82 (1): 115–46.

Kugler, Adriana, and Maurice Kugler. 2009. "Labor Market Effects of Payroll Taxes in Developing Countries: Evidence from Colombia." *Economic Development and Cultural Change* 57 (2): 335–58.

Lindbeck, Assar, and Dennis J. Snower. 1986. "Wage Setting, Unemployment, and Insider-Outsider Relations." *American Economic Review* 76 (2): 235–39.

Malm, Arvid, Johan Eklund, David C. Francis, and Nan Jiang. 2016. "The Effect of the Swedish Payroll Tax Cut for Youths on Firm Profitability." World Bank Policy Research Working Paper WPS7854.

Manning, Alan. 2003. *Monopsony in Motion: Imperfect Competition in Labor Markets.* Princeton, NJ: Princeton University Press.

Medlingsinstitutet. 2015. *Avtalsrörelsen och lönebildningen År 2015*. Stockholm: Medlingsinstitutet.

Neumark, David. 2013. "Spurring Job Creation in Response to Severe Recessions: Reconsidering Hiring Credits." *Journal of Policy Analysis and Management* 32 (1): 142–71.

Oreopoulos, Philip, Till von Wachter, and Andrew Heisz. 2012. "The Short- and Long-Term Career Effects of Graduating in a Recession." *American Economic Journal: Applied Economics* 4 (1): 1–29.

Organisation for Economic Co-operation and Development. 2017. *Employment Database—Labour Market Policies and Institutions.* Paris: Organisation for Economic Cooperation and Development.

Pencavel, John. 1991. *Labor Markets under Trade Unionism: Employment, Wages, and Hours.* Cambridge, MA: Basil Blackwell.

Piketty, Thomas. 1997. "La redistribution fiscale face au chômage." *Revue Française d'Économie* 12 (1): 157–201.

Saez, Emmanuel, Manos Matsaganis, and Panos Tsakloglou. 2012. "Earnings Determination and Taxes: Evidence from a Cohort-Based Payroll Tax Reform in Greece." *Quarterly Journal of Economics* 127 (1): 493–533.

Saez, Emmanuel, Benjamin Schoefer, and David Seim. 2017. "Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden." NBER Working Paper 23976.

Saez, Emmanuel, Benjamin Schoefer, and David Seim. 2019. "Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden: Dataset." *American Economic Review.* https://doi.org/10.1257/aer.20171937.

Schoefer, Benjamin. 2016. "The Financial Channel of Wage Rigidity." Unpublished.

Skedinger, Per. 2014. "Effects of Payroll Tax Cuts for Young Workers." In *Nordic Economic Policy Review: Consequences of Youth Unemployment and Effectiveness of Policy Interventions, Number* 1, edited by Michael Rosholm and Michael Svarer, 124–69. Copenhagen: Norden.

US Congressional Budget Office. 2016. *The Distribution of Household Income and Federal Taxes, 2013.* Washington, DC: US Congressional Budget Office.

Van Reenen, John. 1996. "The Creation and Capture of Rents: Wages and Innovation in a Panel of UK Companies." *Quarterly Journal of Economics* 111 (1): 195–226.

Zwick, Eric and James Mahon. 2017. "Tax Policy and Heterogeneous Investment Behavior." *American Economic Review* 107 (1): 217–48.

Online Appendix of:
Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden

By Emmanuel Saez, Benjamin Schoefer, and David Seim

A Additional Empirical Results

A.1 Market-Level Wage Effects

Monthly cohorts. Figure A3 replicates Figure 2 but zooms into cohorts defined by month and year of birth instead of year of birth. For comparison with Figure 2, for any given year t when the wage is measured, monthly birth cohorts are translated into monthly age bins as of end of year t. For example, 27 in 2009 means being born in January 1982 (and ineligible for the tax cut). 26+11/12 in 2009 means being born in December 1983 and thus eligible for the tax cut. The top panel depicts *net wages* (monthly wage earnings net of payroll taxes). The bottom panel depicts *gross wages* (i.e. gross of payroll taxes). The top panel shows that the wages are continuous at the age thresholds, except for small school-year effects already present pre-reform and also away from the reform age threshold. The school system is based on calendar year of birth (and hence people born in December of year t are in general 1 year more advanced in their career path than people born in January of year $t + 1$). In contrast, the bottom panel of Figure A3 shows that the gross wage is discontinuous at the eligibility thresholds. Therefore, these results confirm the earlier findings from Figure 2. Corresponding estimates are provided in Table 1, Panel B, and are even closer to 100 percent pass-through to employers than our annual based estimates.

Long-term jobs vs. spot markets. Another potential explanation for the zero net-wage incidence points to the long-term nature of real-world employment relationships, whereas the conceptual framework applies to a spot market for labor. Some young employees will age out of the payroll tax cut over the course of the job. Indeed, it has been documented extensively by Bewley (2002) that employers believe that they cannot easily cut nominal wages as this has deleterious effects on morale and hence productivity of workers.[54]

Ex post, it would attenuate the scope for wage cuts as workers age across the threshold, and, anticipating this, employers would attenuate wage increases for the young ex ante.

However, a substantial fraction of young Swedish workers have short employment spells and hence would not be expected to ever age out of the payroll tax cut on the job, which we document in Appendix Figure A2 by plotting various percentiles of job length by age of hiring

[54]For the United States, Campbell and Kamlani (1997) document that 84 percent of employers deem a series of a higher wages followed by a cut *more* demoralizing than having paid the final low wage for the entire period. The specific question (8, on p. 779) refers to a 10 percent cut, almost exactly the wage cut required by our scenario (12 percent). The specific question is: "A. Assume that for the past five years, you paid wages that were 10 percent lower than the wages you actually paid. [...] B. Assume that for the previous four years, you had paid the same wages that you actually paid, and then cut wages by 10 percent in the current year. [...] In which situation would you expect workers' effort and morale to be worse?"

for individuals newly hired in 2000. It shows that the median spell length of young hires (aged 20-24) is less than two years. Hence, many such young workers could in principle be hired at higher wages.

Even for workers that are expected to age out of eligibility, such downward wage rigidity would merely attenuate initial pass-through to workers, not eliminate it entirely, as incidence can be spread across a smooth wage.[55] In our context, even a constant wage would exhibit a noticeable bump: even the barely-eligible median worker will spend at least *one full calendar year* – i.e. on average *half* of her two-year tenure – in the low-tax regime (since the eligibility criteria apply to cohorts by birth-year rather than daily age; consider the monthly cohorts in Figure A3); thus the cut lowers around half of her present-value labor costs.

In Appendix Figure A5, we empirically investigate whether net wages exhibit incidence in high turnover industries, in which shorter job spells should attenuate dynamic concerns associated with long-term jobs. Our turnover measure is the average job duration of new job spells. We compute the mean duration of new jobs in 2000 for workers aged 20-25, within each of our coarsest industry measure (10 industries). We then split industries by the median average job duration (weighted by 2000 employment). The top panel replicates our original net-wage analysis of Figure 2 separately for high-turnover industries, and the bottom panel does so for low-turnover industries. Even in the high-turnover industries, net wages exhibit no discontinuity around the age eligibility threshold during the reform years. This result is perhaps not surprising in light of the stability of the wage distribution we previously documented in Figure 3. But it does suggest that while turnover is already high among young workers, our incidence results hold up in subsamples even closer to a spot labor market.

Therefore, the absence of tax incidence on wages cannot be explained solely by the concern that all young hires will age out of the payroll tax eligibility on the job and that long-term jobs would mask tax incidence.

A.2 Market-Level Employment Effects

Heterogeneity by local unemployment rate. The stated goal of the policy was to reduce youth unemployment because of a perception among policy makers that youth unemployment was excessively high. In 2006, just before the reform, there was wide variation across Sweden's 21 regions in youth unemployment. Appendix Figure A11 provides a map of Sweden showing youth unemployment rates by quintiles (weighted by labor force size). Regions in the lowest quintile of youth unemployment rates had rates in the range 10.5-12.4 percent while regions in the highest quintile had youth unemployment rates in the range 20-23.3 percent, i.e. about twice as high. Hence, a natural question is whether the payroll tax cut is more effective at stimulating employment in regions where the unemployment rate is higher, and hence presumably furthest away from its efficient level.

The bottom panel of Figure 4 depicts the pre vs. post-reform employment rates by age (as we did in the top panel) but separately for bottom quintile regions (in dark red) and top quintile regions (in lighter red) in terms of youth unemployment rate in 2006. To reduce clutter in the graph, we consider only a single pre period of 2005-06 and a single post period of 2012-13. The graph shows that the employment effects of the payroll tax cut appear much larger in the high unemployment regions – for many cohorts in excess of 5 percentage points off a smaller initial base – than in the low unemployment regions.

[55] Elsby (2009) and Shimer (2004) present variants of these arguments in non tax contexts.

Formal employment effect estimates by quintiles of local youth unemployment in 2006 are presented in Table A2. Column (1) reports the average local youth unemployment rate in each quintile. Within each quintile, we follow the methodology from the first row of Table 2 to estimate employment effects. We regress employment to labor force ratio on period dummies, age dummies and the interaction of the post-reform dummy and a payroll tax cut eligibility dummy (ages 20-26). We show the estimated employment effects in column (2) of Table A2. The table shows that the employment effects are monotonically increasing with the local youth unemployment rate, from 1.0 percentage points in the bottom quintile up to 3.4 percentage points in the top quintile. Comparing columns (1) and (2), we can see that employment effects are increasing even relative to the local initial unemployment rate as the employment effect in the bottom quintile is 9.3 percent of the unemployment in 2006 in the bottom quintile but 16.0 percent of the unemployment in 2006 in the top quintile. Hence, besides replicating our nation-wide analysis across subregions, these results show that the payroll tax cut subsidy appears noticeably more effective in high unemployment regions, consistent with the stated goal of the policy.

Are wage effects different across these areas? In principle, with low unemployment rates, it might be difficult for employers to find young workers, perhaps leading to the biding up of their wage more in line with the canonical equilibrium predictions of tax incidence. Column (3) shows the estimates of the pass-through of the payroll tax cut to firms by the local unemployment rate following the method from Table 1. Estimates are slightly above 100 percent for all quintiles. Hence, there is no evidence that pass-through estimates are lower in low unemployment rate regions.

One concern about the differential employment effects we have uncovered is that regions with high initial youth unemployment rate might naturally mean-revert over time, leading the employment rate of youth to increase relative to regions with low youth unemployment rate even absent the reform. To address this concern, we generate a placebo analysis where we again split Swedish regions into quintiles, but do so based on 2002 unemployment rates. We then estimate employment effects comparing years 1998-2002 to years 2003-2006 (i.e., before the start of the reform). Column (4) in Table A2 displays the unemployment rates in each quintile, and they are roughly comparable in level and variation to the unemployment rates from the real experiment in column (1). However, the placebo employment effects presented in column (5) are all small (less than 0.5 percentage point in absolute value) and insignificant. In particular, the difference in placebo employment effects between the top quintile and the bottom quintile is less than 1 percentage point and insignificant (relative to 2.4 points and highly significant in the real experiment).

Hiring vs. separations. Are the employment effects we have uncovered due to more hiring of young individuals (inflow into employment) or fewer separations of young employees (outflow)? In other words, did unemployment spells shorten, or did employment spells become longer? To analyze this question, we break down the employment effects into worker-level unemployment-to-employment transition rates ("hiring" or the "job finding rate") and employment-to-unemployment transition rates ("separations").

We construct the transition rates by measuring the share of unemployed individuals in year $t-1$ who are employed in year t (unemployment-to-employment transition rate $\rho^{U\to E}$) as well as the share of employed individuals in year $t-1$ who become unemployed in year t (employment-to-unemployment transition rate $\rho^{E\to U}$).

With long-term employment relationships and unemployment, (un-)employment rates are pinned down by these transition rates: $\frac{\text{Emp}}{\text{LF}} = \frac{\rho^{U \to E}}{\rho^{U \to E} + \rho^{E \to U}}$, which is one minus the unemployment rate. *Shifts* in the employment rate are accounted for by the transition rates as $d \log \left(\frac{\text{Emp}}{\text{LF}} \right) = \left(1 - \frac{\text{Emp}}{\text{LF}} \right) \cdot \left(d \log \left(\rho^{U \to E} \right) - d \log \left(\rho^{E \to U} \right) \right)$.[56]

The age-specific employment effects are depicted in the top panel of Figure A13. This graph simply shows the difference in employment rates by age from pre-reform (2002-2006) to post-reform (2009-2013) using the series depicted in the top panel of Figure 4.

The bottom panel of Figure A13 decomposes these age-specific employment effects by plotting the effect on the log of the job finding rate (unemployment-to-employment transition rate) and on the log of the separation rate (employment-to-unemployment transition rate) separately by age. Appendix Table A3 presents the corresponding estimates.

Together (multiplied by the unemployment rate) the two rates indeed account for the employment effects. The bottom panel in Figure A13 show that about 80 percent of the employment effects from the top Panel are due to a reduction in the separation rate of young workers, which falls by 22 percent, and that about 20 percent of the employment effects from the top panel are due to an increase in the hiring rate of young unemployed workers, which increases by around 5 percent.[57] This decomposition suggests that employers respond to the payroll tax cut for young workers primarily by retaining such workers or by offering longer jobs, and only marginally by hiring specifically young workers. Perhaps this is due to the fact that hiring cannot be differentiated by age as easily as retention of existing workers. Moreover, these findings implies the average job quality for young treated workers increased in terms of duration, and that firms did not increase layoffs as workers age across the eligibility threshold. These turnover dynamics would be masked in net employment effects but occupy the policy discourse, particularly with marginal and temporary hiring subsidies that may incentivize churn (e.g. Katz 1998).

A.3 Robustness Check: Other Concurrent Reforms

There are two labor market reforms that coincide with the reform we study: (1) a reform to the structure of temporary contracts, and (2) a separate hiring subsidy for unemployed job seekers. Below we describe these reforms in detail and perform additional robustness checks. We conclude that neither reform appears to confound the treatment effects of the youth payroll tax cut we study, at the market and firm levels. The key reason is that neither policy was age-specific and did not benefit young workers more than somewhat older workers.

A.3.1 New Start Jobs: Hiring Subsidy for Unemployed Job Seekers

In 2007, a hiring subsidy for unemployed workers was introduced, the "New Start Jobs" program. First, we provide a detailed description of the reform. Second, we conduct robustness checks that confirm that our results cannot be due to this new program.

Description and eligibility. In January 2007, the government introduced a new program called the *New Start Jobs*. The policy meant that employers hiring individuals who have been

[56] We have collapsed the frequency of our workhorse data set at the annual level (employment status in November) and do not differentiate job switchers between panel observations, such that our decomposition may be subject to some degree of time aggregation bias, i.e. an overestimation of the decline in the separation rate.

[57] Recall that the employment rate increases by around 2.5 percent for the young. Table 2 reports the corresponding percentage point estimates for the pooled treatment effect.

absent from the labor market for at least 12 months (during the last 15 months), receive a subsidy equivalent to the payroll tax. In other words, employers do not have to pay any payroll tax for these workers. The subsidy duration is limited to the period equal to the worker's previous unemployment duration, but capped at five years. Furthermore, in 2009, the subsidy rate was increased to twice the payroll tax.

Special rules apply to the young, the old and immigrants. Young unemployed, aged 20-25, need a non employment duration of 6 months (as opposed to 12) to be eligible. For those young workers, the maximum subsidy period is *one* year (as opposed to at most five, for older workers). Young workers have to have been unemployed during those 6 months, or enrolled in some other social program. Just being a student does not qualify for the subsidy. Immigrants are automatically eligible for the subsidy (irrespective of non employment duration). Individuals aged 55-65 have a subsidy duration equal to twice the time in unemployment (capped at 10 years). From July 2010 on, older workers only have to be non employed for 6 months before eligibility.

Robustness to excluding jobs with hiring subsidy. The hiring subsidy could confound our results if it was used more intensively among the young (for our market-level analysis) or among firms with many young workers (for our firm-level analysis). Therefore, a simple way to assess this potential confound is to repeat our main results but excluding all workers benefitting from the hiring subsidy from our employment measures. If the hiring subsidy is the cause of the effects we find, then excluding workers benefitting from the subsidy should erase our results. In contrast, if our results persist unchanged after discarding workers benefitting from the subsidy, then the subsidy cannot explain our findings.

We have obtained administrative data on the universe of New Start Jobs beneficiaries from Statistics Sweden, and have linked this data set with our core earnings and firm level data set. This allows us to flag any worker who benefits from this subsidy. Results are presented in Figure A22. Panel (a) shows the share of employed workers aged 20-35 benefitting from this program (left y-axis) and the absolute number of participants (right y-axis) over time from 2007 to 2013. The panel shows that this was a relatively small program affecting between 0.5 percent to 2 percent of the employed aged 20-35 over the period 2007 to 2011. Panel (b) shows the share of employed workers benefitting from this program by age pooling years 2010 and 2011. It shows that participation in the program is only very slightly decreasing by age from around 2.2 percent at ages 21-26 and around 2 percent at ages 27-35. Therefore, this very small differential is unlikely to confound our results.

Panels (c) and (d) replicate our main results from Figure 4 (market level employment effects) and Figure 6(a) (firm level employment effects) but excluding workers benefitting from the hiring subsidy. In panel (c), workers benefitting from the hiring subsidy are excluded from both the employed numerator and the labor force denominator. Note that before 2007, the hiring subsidy does not exist so that the series for periods 2002-2004 and 2005-2006 are unchanged relative to Figure 4. The series for 2009-2011 and 2012-2013 are hardly affected by removing workers on the hiring subsidy.[58] This is the consequence of what we saw in panel (b): the fraction of the employed benefitting from the hiring subsidy is fairly constant by age. Therefore, we conclude

[58]We remove individuals from both the numerator and the denominator to preserve levels. If we removed those benefitting from the hiring subsidy only in the numerator (and not the denominator), the level of employment would fall by about 2 points across all age groups after 2007 making the comparison with pre-reform periods less transparent.

that the New Start Jobs hiring subsidy does not confound our market-level employment results.[59] In panel (d), workers benefitting from the hiring subsidy are excluded from employment counts at the firm level. Again, the new figure is virtually undistinguishable from the original Figure 6(a). Therefore, we conclude that the New Start Jobs hiring subsidy does not confound our firm-level employment results either.

A.3.2 Reform of Temporary Contracts

The second reform that coincided with the youth payroll tax cut was a reform to the structure of temporary contracts.

2007 reform of temporary contracts. In July 2007, the government reformed the structure of temporary contracts. Before the reform, temporary contracts were permitted under a set of limited circumstances (e.g. untenured faculty; seasonal jobs;...). The 2007 reform removed this condition, permitting temporary contracts under any circumstances but restricting the contracts to last at most two years. After two years, the contract had then to turn permanent (whereas before the reform, no such limit existed on contract renewals). There was no age-specific clause either before or after the reform.

It is difficult to determine the "net effect" of the 2007 contracts reform on the relative attractiveness of temporary and permanent contracts: on the one hand, temporary contracts were now broadly and unconditionally allowed, perhaps leading firms to substitute from permanent to temporary. On the other hand, for the existing stock of temporary contracts, those contracts were now required to turn permanent after two years, while previously indefinite renewals were possible.

Merging the labor force survey with our administrative data. Our administrative data does not have information on contract type; moreover, we do not see whether a continuing employment relationship churns through multiple temporary contracts or whether it has been on a single permanent relationship; similarly, we can not differentiate whether short employment relationships end because a temporary contract was exhausted or because a permanent contract was dissolved.

To make progress, we merge the micro data underlying the Swedish Labor Force Survey (LFS) to our administrative data at the worker level. The LFS samples about 30,000 households annually and forms the basis of the construction of official unemployment statistics (ILO standard). Importantly, the LFS contains information on age, employment status and also *contract type*, which comprises permanent, temporary, self-employed. The LFS is only a small sample of the full population, and therefore, the results we obtain using this match do not have as much precision as our full population results.

Robustness of the market-level employment effects. We confirm that our market level employment effects from Section II.C are not driven by a youth-biased expansion of temporary contracts. First, we replicate our employment effects in the LFS irrespectively of contract type. Second, we show that the employment results are driven primarily by permanent contracts.

[59]We also find that the regional heterogeneity cuts presented in Figure A12(b), where we sort regions by initial unemployment levels, are unaffected with this revised sample that eliminates program participants from the employment counts.

Third, we rationalize the unimportance of temporary contracts in our results by plotting the share of employment in temporary contracts by age and year and show that the reform did not expand the use of temporary contracts among the young and slightly expanded its use among slightly older workers.

Figure A23, panel (a) replicates our market-level employment results in the LFS. For each age group, we construct employment rates for each period, and plot these outcomes for our pre-reform and post-reform periods. Figure A23, Panel (b) combines these years in two separate pre and post periods. Consistent with the population-level administrative data, employment increases among younger workers. The series are naturally noisier due to much smaller sample size. In Figure A23, Panel (c) we present point estimates from a DD analysis of changes in employment from before to after the reform by age (solid blue circle series in panel (c)). The LFS traces out a similar (although noisier) hump-shaped treatment effect as our administrative data that we depicted on Figure A13, panel (a).

Second, we decompose the employment effect into permanent and temporary jobs in the same Figure A23, Panel (c). Permanent jobs are defined as all jobs that are not temporary contracts so that total employment is the sum of temporary and permanent jobs. The dashed green line shows the DD estimate on employment in temporary jobs by age; the red dotted line shows the same series for permanent jobs. The green line is centered around zero for both the younger workers and the older workers, suggesting that the temporary contracts reform did not lead to an expansion in temporary jobs in Sweden for our LFS sample. Moreover, there is no age gradient, which reveals that the reform did not spur temporary job creation for young workers. The red dotted line, permanent employment, therefore explains the increase in employment, which is more pronounced for the workers aged 26 and below.[60] We conclude that the treatment effect on youth employment is not explained by the reform to temporary contracts.

Third, we provide an explanation for this result. In panel (d) of Figure A23, we plot the share of temporary jobs in total jobs at each age, for the pre-reform period 2002-2006 and the post-reform period 2009-2013. Indeed, there is a smooth gradient by age in the likelihood of being in a temporary jobs from 75 percent for workers aged 20, down to about 40 percent for workers aged 26-27 (at the discontinuity threshold for our payroll tax cut reform) and down to 15 percent at age 35. However, this plot confirms that the share of temporary contracts has *stayed stable* for the young workers in the post 2007 period, compared to the pre 2007 period under the old regime. If anything, *older* workers have seen a slight expansion in temporary contracts as a share of employment. Therefore, the importance of temporary contracts has not increased for young workers after the payroll tax cut. This is perhaps consistent with the a priori ambiguous net effects (broader eligibility, but limitation to two years).

Robustness of firm-level results to temporary contracts reform. The temporary contract reform could affect our firm-level results if two conditions are met (1) the temporary contract reform reduced hiring/firing costs for those firms heavily relying on those contracts, and (2) firms heavily relying on those contracts tend to have a high share young.[61] Perhaps the

[60] A "swap" of temporary and permanent jobs emerges for older workers (aged 34-5) leaving total employment effect constant but these swap effects are not statistically significant due to relatively small sample size.

[61] Daruich, Di Addario and Saggio (2017) study an expansion of temporary contracts in Italy and find employment and wage effects. The features of the Swedish contracts reform contrast with Daruich, Di Addario and Saggio (2017) as the Swedish reform simultaneously capped temporary contracts use by requiring a job to turn permanent after two years. Moreover, we have already demonstrated that the reform did not increase the share

market-level robustness check (which indicated that the 2007 reform did not actually appear to have spurred hiring into temporary contracts) masked heterogeneity between firms.

We cannot conduct our firm-level analysis with the LFS since we cannot observe all workers at a given firm in the LFS repeated cross-sections; we therefore cannot "net out" temporary jobs from our firm-level employment count as in our firm-level robustness check regarding the hiring subsidy. Instead, we investigate directly the degree to which firm-level payroll share young and share on temporary contracts are correlated – and whether high share young firms expanded temporary contracts use following the 2007 reform.

Specifically, we use the micro (household) data from the LFS, merged to our administrative population data. We pool all cross sections of this matched sample before and after 2007 (i.e. firms that have at least one worker in the LFS in a given cross section). We then rank all firms in the matched sample by their share young, and group these firms into 10 equally sized groups. Within each bin, we use the LFS micro data to construct the fraction of workers on a temporary contract. We do so separately for years 2002-2006 (pre-reform) and 2007-2013) (post-reform).

Figure A23, Panel (e) plots this relationship for both periods. In both periods, we find a gradient: "younger" firms rely on temporary contracts more than "older" firms – a pattern mechanically expected from the worker-level age gradient of temporary contracts we previously documented in Figure A23, panel (d). Importantly however, there is no systematic increase in the use of temporary contracts from pre-reform to post-reform for young intensive firms (relative to other firms). To see the time series evolution, Figure A23, Panel (f) plots the evolution of the share in temporary contracts for the matched firms in the high and low share young groups, between 2003 and 2013. When the temporary jobs reform is introduced in 2007, if anything, the share in temporary jobs falls in the high share young firm group (heavily treated by the payroll tax cut we study). There is a brief uptick in 2011, and a return to (and below) pre-reform normal in 2013. By contrast, the control group (medium share young, less intensely treated by the reform) has a more stable evolution of share of workers on temporary contracts, but if anything ends 2013 with a slightly higher level than pre-reform 2006. In conclusion, while we see that indeed high share young firms are mechanically more relying on temporary contracts, the firm level results confirm the market-level results in panels (a) to (d) that if anything, temporary contracts have become less common among the young compared to slightly older peers.

We also note that we have already investigated turnover patterns in our worker-level longitudinal micro data in Figure 9, where we plot the retention and employment probability of workers already employed with a given firm in 2006, for all subsequent years and pre period years. The figure confirmed that there was no differential retention of those workers either before or after the reform, suggesting that the given cross section of the workforce did not in 2007 experience differential contractual treatment with regards to the permanent/temporary dimension. This is consistent with the firm-level correlation in Figure A23, Panel (f).

We therefore conclude that the 2007 reform to temporary contracts neither drive our market-level results nor our firm-level results, likely because that reform actually did not spur significant adoption of temporary contracts.

A.4 Firm-Level Survival and Balanced Panel

Firm survival. The tax cut could have affected firm survival. This is an outcome of interest in its own right. However, such effects would also render our sample of a balanced panel of

of workers on temporary contracts, in contrast to the Italian reform.

firms (2003-2013) endogenous to the reform. We address this question in Appendix Figure A15. For this exercise, we now consider all firms present in 2006 and operating with more than 3 workers in 2006, *regardless of whether they operate in other years* (or whether they have more than 3 workers when they operate). Firms are naturally assigned zero values for employment, sales, profits, etc. in years in which they do not operate. We then compare firms with a high share young in 2006 to firms with a medium share young in 2006, as we did for our benchmark analysis.

Panel (a) of Figure A15 plots the fraction of firms operating in each group for years 2003 to 2013. By definition of the sample, this fraction is equal to one in 2006. It can be lower than one before 2006 as some firms have not started yet; it can be lower than one after 2006 as some firms might cease to operate after 2006. The figure shows that firms with a high share young are substantially less likely to have operated before 2006 (relative to firms with a medium share young). In other words, high share young firms in 2006 are younger. Panel (a) also shows that high share young firms are slightly less likely to survive after 2006 than firms with medium share young. However, rather than a causal effect of the reform, this differential exit rate post-reform of high share young firms may be due to the fact that recently created firms tend to have both lower survival rates and higher shares of young workers.[62]

Therefore, to analyze compellingly whether the reform affects survival, we reweight firms in the medium share young group to align their 2006 firm-age distribution to the high share young group, using the nonparametric methods in DiNardo, Fortin, and Lemieux (1996) (DFL reweighting). We do so by partitioning each group into 8 firm-age based subsets and reweighting each subset so that, after reweighting, the fraction of firms in each age subset is equal across the two groups. We then plot again fraction of firms operating in each group for years 2003 to 2013 in Panel (b) of Figure A15. Panel (b) shows that, after this age based DFL reweighting, the survival curves align perfectly both pre and post-reform. The pre-reform alignment is expected by definition of DFL reweighting by age in 2006. The post-reform alignment then suggests that the reform has actually no effect on survival of high share young firms. That is, all of the exit effect was purely compositional with regards to firm-age differences. This absence of survival effects justifies our use of the balanced panel for our main results. It also implies that the payroll tax cut affected firm outcomes only at the intensive scale margin, but not at the extensive margin.[63] This finding also implies that the Great Recession is unlikely to introduce a bias in our analysis as a differential effect of the Great Recession on young intensive firms would very likely translate into a differential survival rate during 2009 and 2010, the years when unemployment peaked.

Unbalanced panel. It is also possible to estimate firm effects using the full sample of firms from Figure A15 (regardless of whether they operate in all years) and compare the two groups after DFL reweighting by age as done in Panel (b). This exercise is presented in Appendix Figure A16, where we trace out firm outcomes for employment, assets, sales, and profits relative to 2006 in four separate panels. In this case, non operating firms are assigned zero values. Therefore, this analysis is fully robust to endogenous survival effects. Figure A15 shows that,

[62]Intuitively, the reform would be expected to help high share young firms survive. Hence it should have pushed survival of high share young firms up in relative terms. That is why the differential survival we observe in Panel (a) is certainly due to differences in firms characteristics and not due to the reform.

[63]We cannot credibly investigate firm entry in response to the policy as employment structure at entry is endogenous to the reform.

thanks to DFL reweighting by firm-age, pre-trends are very well aligned for all outcomes (less so for the noisier variable of assets and profits).

After the reform, these unbalanced, DFL-reweighted graphs also show that firms with high share young expand employment, sales, and profits.[64] Series on assets are noisy and do not generate a significant effect. We prefer to use the balanced panel of firms active in all years 2003-2013 for our baseline results rather than this full sample because the balanced panel approach does not require any DFL reweighting, making the analysis simpler and more transparent.

B Benchmarking Implied Cash Effects

A full model and assessment of the financial channel is beyond the scope of this paper and limited by the strong effects we find even for firms that our imperfect proxies classify as less constrained. However, we can evaluate our firm-level findings quantitatively by investigating whether the *size* of our treatment effect for the *average* firm could be *entirely* rationalized by a credit constraints channel only. While our sample and particular design differ from existing U.S. analyses with publicly traded, very large firms, our back of the envelope calculation suggests that our effects are of the same order of magnitude, and that the cash channel could play an important role in the firm-level effects.

The standard estimation in the corporate finance literature obtains a dollar-for-dollar effect of a cash flow shock on capital investment (and thereby the capital stock) by regressing capital investment (or change) over lagged capital stock ($\frac{\mathrm{K}}{\overline{\mathrm{K}}}$ or $\frac{\mathrm{CapX}}{\overline{\mathrm{K}}}$) on (endogenous or exogenous) cash flow shifts divided by the lagged capital stock ($\frac{\mathrm{CF}}{\overline{\mathrm{K}}}$).

To benchmark our effects, we cast our treatment effect into an implied "dollar for dollar" version by rescaling appropriately. We then compare that implied effect to the range of existing estimates in the corporate finance literature for capital.[65]

The total-asset differential between the top group and the middle group opens up to 6 percent following the reform, i.e. $\Delta\frac{\mathrm{K}}{\overline{\mathrm{K}}} = 6percent$. The initial liquidity injection from the payroll tax cut corresponds to a 2.4 percent differential in total labor cost reduction for the top vs. the middle groups, i.e. $\Delta\frac{\mathrm{LC}}{\overline{\mathrm{LC}}} = 2.4percent$.[66]

Our tax windfall is a differential percentage shift in labor costs of 2.4 percent. We rescale it by firms' payroll-asset ratio in 2006 to obtain a dollar-for-dollar measure of the capital effect from the tax windfall that can be benchmarked against the standard estimates: $\frac{\overline{\mathrm{LC}}}{\overline{\mathrm{K}}} \cdot \Delta\frac{\mathrm{LC}}{\overline{\mathrm{LC}}} = \Delta\frac{\mathrm{LC}}{\overline{\mathrm{K}}}$.

For our sample of firms, the median labor cost-asset ratio is $\frac{\overline{\mathrm{LC}}}{\overline{\mathrm{K}}} = 0.7$; the mean ratio is around .9 with or without winsorization; going forward we use the mid-point of .8.[67]

This simple rescaling links the 6 percent shift in assets with a $0.8 \cdot 2.4percent = 1.92percent$ labor cost over asset shift, such that a \$1.92 in – annual – labor cost reduction – and thus a cumulative liquidity injection from the tax windfall of \$11.52 by the end of the six-year reform

[64]As expected from the entry/exit findings, without DFL reweighing, pre-trends for most outcomes are not parallel, hereby invalidating our key parallel trend assumption as we saw in Panel (a) of Figure A15.

[65]The literature has not estimated a coherent set of effects for employment, so we restrict our benchmarking to capital.

[66]The calculation is as follows: the payroll tax cut corresponds to 12.1 percent of youth labor costs in year one (assuming no wage changes or scale changes, which would amplify the implied effect). The initial difference in share young between these two groups is 19.8 percentage points. The product implies a 2.4 percent differential in total labor cost reduction for the top vs. the middle groups.

[67]We obtain similar ratios when we compute descriptive statistics for Swedish firms with similar sample restrictions (firm size) for 2006 using Bureau van Dijk data.

–, would be associated with the \$6 increase in the final stock of total assets, six years into the reform. Read through the lens of credit constraints only, our estimate therefore implies an \$0.52 capital stock-cash flow sensitivity. This compares to around \$.2 to \$.6 that the literature finds for publicly traded Compustat firms in the US (see e.g., Fazzari, Hubbard, and Petersen, 1988 for a classic study).

There are five reasons that may explain why our implied effect – if indeed due to credit constraints – falls in the upper range of existing estimates. First, our sample contains many small firms, whereas the benchmark estimates refer to publicly traded Compustat firms in the U.S., which presumably are much less constrained.[68] Second, as discussed above, the tax reform not only generated an inframarginal cash injection but also lowered marginal costs and may lead to expansion up through a conventional scale effect on top of the financial mechanism. Third, the benchmark estimates arise from variation in unexpected transient – i.e. one-time – shocks to cash flow, whereas we consider a persistent, expected series of tax windfalls. Such liquidity injections may imply considerably larger effects because they may increase the constrained firm's credit worthiness ex ante. Fourth and relatedly, our medium-run analysis revealed that firms scale up, which would generate additional resources starting year 2 through an indirect multiplier effect. Relatedly, the literature considers capital investment, our medium-run treatment requires a cumulative measure of capital stock growth. A short-run impact of incremental investment adds one to one to the capital stock (i.e. the cash flow sensitivities are similar whether capital stock or investment is the dependent variable, both normalized by lagged capital stock), whereas steady-state shifts are mediated by the depreciation rate. Fifth, note that our measure (total assets) also includes financial assets besides productive assets. While we find similar (yet noisier) percentage growth of fixed assets (and fixed tangible assets) in unreported specifications, the ratio of gross labor costs to those asset subtypes is considerably larger, which would imply a proportionately smaller dollar-for-dollar effect of the tax windfall into such subcategories of total assets. Concretely, the median labor cost/fixed asset ratio is 2.75, and the labor cost/fixed tangible asset ratio is 3.75. Accordingly, the implied dollar-for-dollar effect would then fall to the order of \$0.10–0.15.

In conclusion, our estimates may indeed reflect an interesting medium-run change in resources that constrained firms use to expand their business, and this implied effect is quantitatively consistent with the range of existing investment-cash flow sensitivities. Specifically, our effects would correspond to a \$0.1-\$0.5 effect on capital per dollar of tax windfall, which spans the range of existing estimates for the investment cash flow sensitivity of U.S. firms. While our firm activity findings could therefore be primarily driven by financial effects, we note that a conventional scale effect from marginal costs may also help explain the business growth patterns (albeit not the heterogeneity by financial constraints).

C A Simple Model with Pay Equity Constraints

We present a parsimonious labor market model that can account for most of our key findings. It adds one departure from the standard competitive model: a pay equity constraint that compresses *net* wages between worker types (here: young vs. old), and largely plays out within firms.

[68] For example, Zwick and Mahon (2017) investigate a broader cross section of U.S. firms including smaller private firms (resembling our sample), and find smaller firms exhibit dramatically larger responses to investment incentives than Compustat firms, which the authors attribute to credit constraints.

This pay equity constraint pushes the youth wage above the market-clearing level, which is below the old wage as the young are less productive than the old. (The old are on their labor supply curve and pin down everyone's net wage.) Hence, youth labor supply is rationed, youth unemployment emerges, and prevailing youth employment is labor-demand-determined.

The model accounts for the following nonstandard payroll tax facts we document: (i) The incidence of an employer payroll tax cut for the young falls fully into their labor costs, while (ii) their net wages do not change. (iii) Youth employment increases even if labor supply elasticities are small and despite a zero shift in net wages for the young. Augmenting the model with two firm types (youth intensive vs. old age intensive firms) can also replicate our cross-sectional firm-level effect, where (iv) high share young firms expand scale and (v) these firms raise wages by more in response to a payroll tax cut for the young.[69]

In this environment, moving from homogeneous to age-dependent employer payroll taxes can offset the labor cost distortion from the equity constraint on net wages, and implement the frictionless age gradient of employment.

We first present the model with a representative firm and household that will account for the market-level findings. As a benchmark, we first discuss the model without the wage friction as a frictionless benchmark, where labor demand and supply will be equilibrated and standard incidence predictions are borne out. We then discuss how pay equity constraints affect the labor market, as well as the effects of age-dependent payroll taxes. Labor demand comes from a wage-taking representative firm. Next, we augment this model with two types of firms and a firm-specific labor supply curve (monopsony) to account for the firm-level results on top of the market-level results. Finally, we calibrate this full model and investigate whether the calibrated model can account for the treatment effects presented in Table 4.

C.1 Households: Labor Supply

For young and old households $i \in \{y, o\}$, of equal mass, utility is quasi-linear in consumption c_i and employment n_i:

$$u(c_i, n_i) = c_i - \phi_i^{-1/\xi} \cdot \frac{n_i^{1+\frac{1}{\xi}}}{1+\frac{1}{\xi}} \tag{A1}$$

ξ guides Marshallian, Hicksian and Frischian labor supply elasticities.[70] ϕ_i is the taste for work.

Labor supply is a function of the wage w_i, and tastes ϕ_i and ξ:

$$n_i^s = \phi_i w_i{}^{\xi} \tag{A2}$$

[69] Four additional empirical findings are beyond the scope of our model. First, rent sharing in our model works through a monopsony mechanism (firm-specific labor supply curve), which stands in for richer mechanisms of rent sharing of tax windfalls. Second, credit constraints are not active, such that the marginal cost channel drives labor demand responses, and we do not model capital. Third, since our pay-equity constraint is specialized to be fully binding, we cannot generate the progressive wage effects within firms, although a slight extension to partial wage flexibility may do so. Fourth, we do not explicitly model worker flows through separations and hiring but consider net quantities, which stand in for long-term jobs.

[70] In line with our evidence, we model the extensive margin on employment n_i but preclude an intensive hours choice. ξ then captures the distribution of labor disutility in the respective age groups.

C.2 Representative Firms and Labor Demand

CES production with young and old workers. The production function is:

$$F(n_y, n_o) = \left(x_y \gamma_y n_y^\alpha + x_o \gamma_o n_o^\alpha\right)^{\frac{\beta}{\alpha}} \tag{A3}$$

where β denotes overall return to scale. γ_i is the productivity parameter of a given worker-age type i, where we assume $\gamma_y < \gamma_o$, i.e. younger workers are less productive than the old.[71] x_i is the production weight of type i, i.e. $\sum_{i=y,o} x_i = 1$. We introduce both γ_i and x_i because, when we turn to a version with multiple firm types with different weights x_i^f for each firm type f, reflecting technological bias.

Labor demand sets input i's marginal product equal to its gross wage (incl. payroll tax):

$$\beta x_i \gamma_i n_i^{\beta-1} \left[x_j \gamma_j \left(\frac{n_j}{n_i}\right)^\alpha + x_i \gamma_i\right]^{\frac{\beta}{\alpha}-1} = (1+\tau_i) w_i \tag{A4}$$

With CES, the ratio of these labor demand conditions implies: pins down desired input ratio $\frac{n_j}{n_i}$ as a function of labor costs:

$$\frac{n_j}{n_i} = \left[\frac{x_j}{x_i} \frac{\gamma_j}{\gamma_i} \frac{(1+\tau_i) w_i}{(1+\tau_j) w_j}\right]^{\frac{1}{1-\alpha}} \tag{A5}$$

Plugging in for $\frac{n_j}{n_i}$ in (A4) with the desired skill ratio (A5), we obtain n_i only as a function of the parameters of the production function and gross wages:

$$n_i^d = \left(\frac{\beta x_i \gamma_i}{(1+\tau_i) w_i}\right)^{\frac{1}{1-\beta}} \left[x_j \gamma_j \left[\frac{x_j}{x_i} \frac{\gamma_j}{\gamma_i} \frac{(1+\tau_i) w_i}{(1+\tau_j) w_j}\right]^{\frac{\alpha}{1-\alpha}} + x_i \gamma_i\right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1-\beta})} \tag{A6}$$

C.3 Benchmark: Frictionless Equilibrium – No Equity Constraints

Age gradients of labor market outcomes. Now consider the frictionless equilibrium without pay equity constraints. Our CES set-up could be extended to more than two age groups (rather than young and old) to trace out the worker ages corresponding to the empirical market-level age cuts, e.g. Figure 2 for employment and Figure 4 for net wages. Indeed, our analysis follows a difference-in-difference analysis, so we do not speak to aggregate absolute levels. So it is useful to not only focus on levels (end of this Section) but rather on the *age gradient* of labor market outcomes. This perspective is particularly convenient since our empirical analysis considers a shift in the payroll tax rate age profile, and because we will later on consider whether in general an age-dependent payroll tax regime may fully offset the wage friction (and thus restore the frictionless equilibrium we describe below as our benchmark).

[71] Note that the productivity parameters do not map into observables. In fact, in our model with equal labor costs, the *marginal product* of old and young workers are equal with pay-equity constraints by labor demand due to homogeneous gross wages.

To obtain the equilibrium, consider again the **age gradient of labor demand** from (A5):

$$\frac{n_i^d}{n_j^d} = \left[\frac{x_j}{x_i}\frac{\gamma_j}{\gamma_i}\frac{(1+\tau_i)w_i}{(1+\tau_j)w_j}\right]^{-\frac{1}{1-\alpha}} \tag{A7}$$

The **age gradient of labor supply** arises from $n_i^s = \phi_i {w_i}^{\xi}$:

$$\frac{n_i^s}{n_j^s} = \frac{\phi_i}{\phi_j}\left(\frac{w_i}{w_j}\right)^{\xi} \tag{A8}$$

We first derive the **age gradient of equilibrium net wages**, which is the model analogue of our empirical market-level Figure 2. Panel (a) shows an upward-sloping employment profile, which we will rationalize with productivity differences (or taste differences ϕ):

$$\frac{w_i^{eq}}{w_j^{eq}} = \left[\frac{x_i}{x_j}\frac{\gamma_i}{\gamma_j}\right]^{\frac{1}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_i}{1+\tau_j}\right]^{-\frac{1}{\xi(1-\alpha)+1}} \cdot \left(\frac{\phi_i}{\phi_j}\right)^{-\frac{1-\alpha}{\xi(1-\alpha)+1}} \tag{A9}$$

The wage path is affected by three factors: productivity differences, taste differences, and the payroll tax gradient. Taste differences can only affect wages if worker types aren't perfect substitutes ($\alpha = 1$), in which case labor demand is perfectly elastic between worker types. Productivity differences determine the wage gradient even if workers are perfect substitutes, in which case wages perfectly trace the differences in the productivity terms.

In terms of **payroll tax incidence into net wages**, the payroll tax gradient acts exactly as the productivity gradient. As in the standard incidence framework, with elastic labor demand between worker groups ($\alpha \approx 1$), workers' relative net wages bear the full incidence of payroll tax differences in the cross-section. This case is our benchmark and our prior for our empirical analysis, since around the age discontinuity, workers should be close to perfect substitutes. For $\alpha < 1$, labor demand is not perfectly elastic for a given age group, and then labor *supply* elasticities ξ will mediate the incidence: if $\xi \to 0$, then relative net wages absorb age-dependent payroll taxes, without any employment effect, irrespectively of the labor demand elasticity. But the closer α to one, the less relevant labor supply factors become for incidence into net wages.

The model's **age gradient of equilibrium gross wages** captures the flip side of the net wage incidence results. With elastic labor supply, gross wages take the incidence of payroll taxes. When labor demand is cross-sectionally perfectly elastic (α close to one), then gross wages are invariant in payroll tax rates:

$$\frac{(1+\tau_i)w_i^{eq}}{(1+\tau_j)w_j^{eq}} = \left[\frac{x_i}{x_j}\frac{\gamma_i}{\gamma_j}\right]^{\frac{1}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_i}{1+\tau_j}\right]^{\frac{\xi(1-\alpha)}{\xi(1-\alpha)+1}} \cdot \left(\frac{\phi_i}{\phi_j}\right)^{-\frac{1-\alpha}{\xi(1-\alpha)+1}} \tag{A10}$$

Figure 2, Panel (b) shows incidence for the age gradient of gross wages. We rejects the zero/small incidence into gross wages predicted by inelastic labor supply and elastic labor demand.

Finally, we consider the **age gradient of equilibrium employment**, the empirical analogue of which we trace our in market-level Figure 4. That Figure shows an upward-sloping employment profile. Our model-equivalent replicates this empirical fact if productivity factors increase in age (and if workers' tastes for work do not decline in an offsetting way):

$$\frac{n_i^{eq}}{n_j^{eq}} = \frac{\phi_i}{\phi_j}\left(\frac{w_i}{w_j}\right)^{\xi} \tag{A11}$$

$$= \left[\frac{x_i}{x_j}\frac{\gamma_i}{\gamma_j}\right]^{\frac{\xi}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_i}{1+\tau_j}\right]^{-\frac{\xi}{\xi(1-\alpha)+1}} \cdot \left(\frac{\phi_i}{\phi_j}\right)^{\frac{1}{\xi(1-\alpha)+1}} \tag{A12}$$

The employment incidence of payroll tax differences are limited by low assumed labor supply elasticities even when labor demand is very elastic. We do find differential employment impacts around the discontinuity that imply an equilibrium employment elasticity of around 0.21 (Table 2). With $\alpha = 1$, this would imply a labor supply elasticity (assuming a counterfactual equilibrium economy in which net wages increased) of 0.22, a realistic value. The tension is of course that the empirical results find a zero rather than 12 percent incidence on net wage differentials for treated young workers (see Figure 2, Panel (a)). A model with incidence along a standard, even moderately elastic labor supply is therefore not a good candidate for our facts.

Levels of age-specific labor market outcomes. Our empirical analysis of market-level effects exploits difference-in-difference analyses, and therefore examines relative shifts in the employment and wage profiles rather than absolute effects. For completeness we also present the closed forms of the **level of equilibrium employment and wages**, on which comparative statics could be performed:

$$n_i^{eq} = \left(\frac{\beta x_i \gamma_i \phi_i^{\frac{1}{\xi}}}{1+\tau_i}\left[x_j\gamma_j\left(\left[\frac{x_j}{x_i}\frac{\gamma_j}{\gamma_i}\right]^{\frac{\xi\alpha}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_j}{1+\tau_i}\right]^{-\frac{\xi\alpha}{\xi(1-\alpha)+1}} \cdot \left(\frac{\phi_j}{\phi_i}\right)^{\frac{\alpha}{\xi(1-\alpha)+1}}\right) + x_i\gamma_i\right]^{\frac{\beta}{\alpha}-1}\right)^{\frac{1}{\frac{1}{\xi}+(1-\beta)}} \tag{A13}$$

$$w_i^{eq} = \left(\phi_i^{-1} n_i^{eq}\right)^{\frac{1}{\xi}} \tag{A14}$$

$$(1+\tau_i)w_i^{eq} = (1+\tau_i)\left(\phi_i^{-1} n_i^{eq}\right)^{\frac{1}{\xi}} \tag{A15}$$

Standard incidence predictions are borne out because we are in the competitive labor market. Level analysis of incidence in this environment is only slightly more complicated than cross-sectional incidence in the age gradients.[72]

C.4 A Labor Market with Equity Constraints on Net Pay

We now show how a labor market with constraints provides a parsimonious refinement that helps the model account for the empirical facts. Wages for the young are constrained to equal those of the old workers due to pay equity constraints ($w_i = w_j$). *Old* labor supply and demand

[72]Most simply, with infinitely elastic labor demand ($\alpha = 1$ and $\beta = 1$), the expressions collapse to:

$$n_i = (x_i\gamma_i)^{\xi} \cdot (1+\tau_i)^{-\xi} \cdot \phi_i \tag{A16}$$

$$w_i = (x_i\gamma_i) \cdot (1+\tau_i)^{-1} \tag{A17}$$

$$(1+\tau_i)w_i = (x_i\gamma_i) \tag{A18}$$

Gross wages are constant; net wages take the full incidence; employment responses depend on ξ.

are in equilibrium and pin down the market-clearing old wage, which, due to our friction, also pin down youth wages. Such pay equity constraints distort the age gradient of net and gross wages, generating youth unemployment and nonstandard tax incidence patterns.

Equity-constrained net wages. The friction lies in the differentiation of net wages. While we could consider a variety of plausible reduced-form representations that capture this phenomenon (e.g. wage compression, a constraint on adjacent age group's maximal wage gap,...), we consider an exposition with identical wages:

$$\frac{w_i}{w_j} = 1 \tag{A19}$$

Old workers are on their labor supply curve, such that their labor market clears:

$$\phi_o w_o^{\xi} = n_o^s \tag{A20}$$

By contrast, youth labor supply exceeds the prevailing employment, given by labor demand:

$$\phi_y w_o^{\xi} = n_y^s > n_y^d \tag{A21}$$

Labor demand for factor i is:

$$n_i^d = \left(\frac{\beta x_i \gamma_i}{(1+\tau_i)w_i}\right)^{\frac{1}{1-\beta}} \left[x_j \gamma_j \left[\frac{x_j}{x_i} \frac{\gamma_j}{\gamma_i} \frac{(1+\tau_i)w_i}{(1+\tau_j)w_j} \right]^{\frac{\alpha}{1-\alpha}} + x_i \gamma_i \right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1-\beta})} \tag{A22}$$

Since wages are constrained to be identical, this expression becomes:

$$n_i^d = \left(\frac{\beta x_i \gamma_i}{(1+\tau_i)w_i}\right)^{\frac{1}{1-\beta}} \left[x_j \gamma_j \left[\frac{x_j}{x_i} \frac{\gamma_j}{\gamma_i} \frac{1+\tau_i}{1+\tau_j} \right]^{\frac{\alpha}{1-\alpha}} + x_i \gamma_i \right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1-\beta})} \tag{A23}$$

Equilibrium employment, net wages and gross wages of the old. We can now pin down the equilibrium employment level of the old, and therefore the old net and gross wages, which in turn pins down prevailing (disequilibrium) employment for the young and unemployment. By assumption, the old are on their labor supply curve, such that $w_o = (\phi_o^{-1} n_o^s)^{\frac{1}{\xi}}$. Plugging this in (A23) for $i = o, j = y$, we obtain equilibrium employment for the old and their net and gross wages:

$$n_o^{eq} = \left(\frac{\beta x_o \gamma_o \phi_o^{\frac{1}{\xi}}}{(1+\tau_o)} \left[x_y \gamma_y \left[\frac{x_y}{x_o} \frac{\gamma_y}{\gamma_o} \frac{(1+\tau_o)}{(1+\tau_y)} \right]^{\frac{\alpha}{1-\alpha}} + x_o \gamma_o \right]^{\frac{\beta}{\alpha}-1} \right)^{\frac{1}{1+\frac{1}{\xi}-\beta}} \tag{A24}$$

$$w_o^{eq} = \left(\phi_o^{-1} n_o^{eq}\right)^{\frac{1}{\xi}} \tag{A25}$$

$$(1+\tau_o)w_o^{eq} = (1+\tau_o)\left(\phi_o^{-1} n_o^{eq}\right)^{\frac{1}{\xi}} \tag{A26}$$

In contrast to employment level in the frictionless benchmark, the current expression does not contain any youth labor supply features (e.g. taste parameters) since they are off their labor supply curve.

Prevailing youth employment is labor-demand-determined, because net wages constrained to be equal (but are too high to clear the market because the young are less productive ($\gamma_y < \gamma_o$), and therefore moves in lock-step with old equilibrium employment given firm's optimal skill mix from (A5):

$$n_y^{diseq} = n_y^d = \left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\frac{(1+\tau_o)}{(1+\tau_y)}\right]^{\frac{1}{1-\alpha}} \cdot \underbrace{\left(\frac{\beta x_o \gamma_o \phi_o^{\frac{1}{\xi}}}{(1+\tau_o)}\right)^{\frac{1}{1+\frac{1}{\xi}-\beta}} \left[x_y\gamma_y\left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\frac{(1+\tau_o)}{(1+\tau_y)}\right]^{\frac{\alpha}{1-\alpha}} + x_o\gamma_o\right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1+\frac{1}{\xi}-\beta})}}_{=n_o^{eq}} \tag{A27}$$

Even the youth employment terms do not depend on youth labor supply terms (i.e. ϕ_y), unlike in the frictionless benchmark.

Age gradients. It is interesting to examine how the age gradients for employment, net wages and gross wages contrast with the frictionless equilibrium benchmark. We then turn to the incidence of age-dependent payroll taxes, and their potential to offset the underlying wage friction.

By construction, the friction manifests itself as a flattened **age gradient of net wages**:

$$\frac{w_y}{w_o} = 1 \tag{A28}$$

Since net wages are compressed due to the friction, the age *gradient* of net wages is always equal to one and are invariant in payroll tax differentials. (The wage *level* will endogenously change as pinned down by incidence in the old labor market.)

As a result, any payroll tax rate gradient therefore solely drives the **age gradient of gross wages**:

$$\frac{(1+\tau_y)w_y}{(1+\tau_o)w_o} = \frac{1+\tau_y}{1+\tau_o} \tag{A29}$$

The **age gradient of employment** is, for any given equilibrium old employment level n_o^{eq}, directly given by the firm's labor demand preferences facing equal net wages yet potentially different payroll tax rates:

$$\frac{n_y^{diseq}}{n_o^{eq}} = \left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\right]^{\frac{1}{1-\alpha}} \cdot \left[\frac{(1+\tau_y)}{(1+\tau_o)}\right]^{-\frac{1}{1-\alpha}} \tag{A30}$$

Comparison: frictionless equilibrium age gradient. Notably, the employment age gradient with equity constraints does not take into account any labor supply taste parameters of the young workers. To see this, compare the equity-constrained employment gradient with the frictionless age gradient for employment (A13).

Age-dependent employer payroll taxes to mimic the frictionless age gradient for gross wages and employment. Interestingly, payroll taxes can be set to have gross wages implement the frictionless age gradient for employment and gross wages (incl. a frictionless equilibrium with an arbitrary combination of payroll tax rates that may have been featured in the frictionless equilibrium to momic):[73]

$$\frac{1+\tau_y^*}{1+\tau_o^*} = \left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\right]^{\frac{1}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_y}{1+\tau_o}\right]^{\frac{\xi(1-\alpha)}{\xi(1-\alpha)+1}} \cdot \left(\frac{\phi_y}{\phi_o}\right)^{-\frac{(1-\alpha)}{\xi(1-\alpha)+1}} \tag{A31}$$

Our conceptual framework and the collection of all our findings suggest that some of this age gradient in unemployment is due to insufficient alignment of gross wages with productivity fundamentals along the life cycle, i.e. that the effective labor cost per efficiency unit of labor are decreasing with age. We find that a net-pay equity friction, largely operating within firms, emerges as a plausible underlying friction. The generalization of our results, empirical and theoretical, is that an age-specific employer payroll tax schedule will be an effective and simple way to equalize the employer-facing productivity-adjusted gross wages with wage constraints.[74] An age-specific employer payroll tax schedule is feasible because age is a fixed and easily observable attribute and therefore a suitable tag for differentiated tax rates.

Payroll tax cuts for the old only. Since the market for the old clears, standard competitive intuitions apply. Tax incidence is guided by relative demand (β, α) elasticities and supply elasticities (ξ). With prime-aged workers being inelastic in their labor supply, their net wages will take the incidence – i.e. old net wages will increase –, and labor costs of the old (gross wages) will only slightly decrease. Since the old wages determines the youth wage, this process *pushes up* the gross wage of the young, making them less attractive to hire.

Encompassing payroll tax cuts. An interesting scenario is an encompassing payroll tax cut, i.e. one that affects τ_y and τ_o equally. Employment for the young is determined by the old's employment and wage levels, which clear the labor market for the old (but not the young if productivity parameters or taste parameters differ). As a result, when *both* payroll tax rates change, intuitions are guided by standard incidence mechanisms for the old wages (and thus the young wage too, although that market does not clear). This prediction is consistent with aggregate net-wage incidence in response to encompassing payroll tax cuts. *As a result, encompassing payroll tax cuts need not be effective even if targeted payroll tax cuts are effective, if equity constraints exist.* As a result, targeted payroll tax cuts may be more effective than encompassing ones. This prediction also differentiates a pay-equity friction from a simple wage floor or wage rigidity, where payroll tax cuts would be effective, at least in the short run.

Short-run vs. long-run effects. (Encompassing) employer payroll tax cuts may be effective in the short run under wage rigidities, but may be offset once wages adjust and realize the standard incidence predictions.[75] By contrast, in the presence of cross-sectional pay equity constraints, age-graduated payroll taxes might be able to flatten and lift the age gradient of employment for young or otherwise disadvantaged workers even in the long run. In fact, we found no net-wage incidence even six years into the reform, and persistent employment effects.

[73] This tax reform need not be revenue-neutral. However, the base tax rate the old can be chosen arbitrarily.

[74] In fact, the prescription *depends* on some form of wage frictions not only as the source of the distortion but also for net wage incidence to not offset the labor cost reduction due to the payroll schedule.

[75] For this argument, see e.g. Bils and Klenow (2009).

Youth unemployment. In the Swedish case, youth nonemployment manifested itself as unemployment, which gave rise to the policy concerns that ultimately led to the intervention. A standard competitive model without frictions would not feature unemployment.[76] Our model generates a basic form of youth unemployment in form of rationed youth labor supply, i.e. the difference between labor supply – at the old wage $w_y = w_o$ –, and labor-demand-determined prevailing employment (A30). The presence of unemployment in form of rationed labor supply is a crucial ingredient in our model in that it rationalizes why employment increases can go along without net-wage changes even if labor supply elasticity ξ is very small, which the frictionless economy would struggle to explain.

Concretely, the count of young workers in unemployment is:

$$u_y = n_y^s - n_y^{diseq} = \phi_y w_o^{\xi} - n_y^{diseq} = \frac{\phi_y}{\phi_o} n_o^{eq} - n_y^{diseq} \tag{A32}$$

The unemployment *rate* $\tilde{u}$ is the ratio of the unemployed over the labor force, which here is desired labor supply:

$$\tilde{u} = \frac{u_y}{n_y^s} = \frac{n_y^s - n_y^{diseq}}{n_y^s} = 1 - \frac{n_y^{diseq}}{n_y^s} \tag{A33}$$

$$= 1 - \left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\right]^{\frac{1}{1-\alpha}} \cdot \left[\frac{(1+\tau_y)}{(1+\tau_o)}\right]^{-\frac{1}{1-\alpha}} \frac{n_o^s}{n_y^s} \tag{A34}$$

$$= 1 - \left[\frac{x_y}{x_o}\frac{\gamma_y}{\gamma_o}\right]^{\frac{1}{1-\alpha}} \cdot \left[\frac{(1+\tau_y)}{(1+\tau_o)}\right]^{-\frac{1}{1-\alpha}} \cdot \left(\frac{\phi_o}{\phi_y}\right) \tag{A35}$$

The youth unemployment *rate* is pinned down by two factors: labor supply (how many workers would like to work at the upward-distorted wage) and labor demand (the, downward-distorted) amount of jobs for the young).

First, *tastes* for labor supply may differ between the two groups such that when considering any given going – homogeneous – wage (that of the old), the young workers may be less or more included to supply labor than the old (at that wage). This is captured by $\frac{\phi_o}{\phi_y}$. For the useful benchmark case in which baseline tastes for labor are equal, this ratio is 1. We find this factor (taste differences explaining participation differences) less interesting because it would not carry over to an employment/population analysis.

The second source of unemployment is due to *labor demand.* It arises from the firm's upward-distorted cost of employing a young worker in efficiency units given the pay-equity constraint and the lower productivity fundamentals of the young. For $\phi_o = \phi_y$ and initially homogeneous tax rates $\tau_y = \tau_o$, we have youth unemployment as long as the young have *lower productivity* parameters $x_i \gamma_i$ than the old.

The following payroll tax regime can eliminate youth unemployment from equity constraints:

$$\left.\frac{1+\tau_y}{1+\tau_o}\right|_{\tilde{u}_y=0} = \frac{\gamma_y}{\gamma_o}\left(\frac{\phi_o}{\phi_y}\right)^{\frac{1}{1-\alpha}} \tag{A36}$$

Interestingly, this is generally not the schedule that would have the economy mimic the fric-

[76]While the payroll tax wedge would distort labor demand and supply as a labor wedge, each side of the market is on their respective demand and supply curves given gross and net wages.

tionless equilibrium.[77]

C.5 Two Types of Firms

Lastly, we sketch one refinement to the model that helps it account not only for our market-level findings but also the firm-level heterogeneity and employment and wage effects. Paralleling our empirical design, we introduce two types of firms $f \in \{Y, O\}$: the youth-intense firms Y and old-intense firms O. In addition, we assume that the workers have CES preferences for their labor allocation. We sketch the model and point to the relevant mechanisms, but economize on space by not again solving for the full equilibrium. **Crucially, the pay equity constraint works within firms, but not across firms.**

C.5.1 Households Labor Supply

Rather than supplying labor to one firm, young households y and old households o supply labor to youth-intense firms Y and old-intense firms O, such that:

$$n_y = n_y^Y + n_y^O \tag{A37}$$
$$n_o = n_o^Y + n_o^O \tag{A38}$$

We preserve quasilinear utility but allow for CES-like aggregation of labor disutilities that generate firm-specific labor supply curves (we suppress taste parameters ϕ_i^f):

$$u(c_i, n_i^O, n_i^Y) = c_i - \frac{\left[\left[(n_i^O)^{1+\frac{1}{\xi}}\right]^{\psi} + \left[(n_i^Y)^{1+\frac{1}{\xi}}\right]^{\psi}\right]^{\frac{1}{\psi}}}{1+\frac{1}{\xi}} \tag{A39}$$

$$= c_i - n_i^{1+\frac{1}{\xi}} \cdot \frac{\left[\left[\left(\frac{n_i^O}{n_i}\right)^{1+\frac{1}{\xi}}\right]^{\psi} + \left[\left(\frac{n_i^Y}{n_i}\right)^{1+\frac{1}{\xi}}\right]^{\psi}\right]^{\frac{1}{\psi}}}{1+\frac{1}{\xi}} \tag{A40}$$

The household incurs the standard ξ-guided disutility of total labor supply n_i, but also has preferences over smoothing out or concentrating labor supply between firm types, as guided by ψ. The individual utility maximization FOC gives for i's labor supply to firms f and $g \not\ni f$:

$$(n^{sf}_i)^{\frac{1}{\xi}} \left[\left[(n^{sf}_i)^{1+\frac{1}{\xi}}\right]^{\psi} + \left[(n_i^g)^{1+\frac{1}{\xi}}\right]^{\psi}\right]^{\frac{1}{\psi}-1} = w_i^f \tag{A41}$$

For $\psi = 1$, the firm-specific labor supply preferences are separable, which precludes between-firm spillovers through wages, which we will conveniently use for a tractable exposition.

[77]The reason is that part of youth unemployment arises from the wage that is "too high", which makes the marginal worker still strictly prefer to work at the net wage that is constrained to equal that of the (more productive) older workers; this component will persist even when payroll taxes achieve the frictionless employment gradient. We find this portion of unemployment less interesting, and in fact a policy-maker could eliminate this residual unemployment by *increasing the employee payroll tax* in practice.

C.5.2 Labor Demand

The production function for a given firm f is:

$$F^f(n_y, n_o) = \left(x_y^f \gamma_y (n_y^f)^\alpha + x_o^f \gamma_o (n_o^f)^\alpha\right)^{\frac{\beta}{\alpha}} \tag{A42}$$

where x_i^f now denotes the **firm-specific** weight in the production function of a given worker-age type i. so that $\sum_{i=y,o} x_i^f = 1$. **Age-bias x_i^f will generate the between-firm dispersion in youth intensity of firms in the model.**

As before, we can express the labor demand for the old again as follows:

$$n^{df}_o = \left(\frac{\beta x_o^f \gamma_o}{(1+\tau_o) w_o^f}\right)^{\frac{1}{1-\beta}} \left[x_y^f \gamma_y \left[\frac{x_y^f}{x_o^f} \frac{\gamma_y}{\gamma_o} \frac{(1+\tau_o) w_o^f}{(1+\tau_y) w_y^f}\right]^{\frac{\alpha}{1-\alpha}} + x_o^f \gamma_o\right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1-\beta})} \tag{A43}$$

Equilibrium. With the convenient case $\psi = 1$, the economy mirrors the representative-agent case discussed above.[78] Moreover, with the equity constraint in net pay *within firms*, we obtain the following equilibrium labor market outcomes:

$$n^{eqf}_i = \left(\frac{\beta x_i \gamma_i \psi_i^{\frac{1}{\xi}}}{1+\tau_i} \left[x_j \gamma_j \left(\left[\frac{x_i}{x_j} \frac{\gamma_i}{\gamma_j}\right]^{\frac{\xi\alpha}{\xi(1-\alpha)+1}} \cdot \left[\frac{1+\tau_i}{1+\tau_j}\right]^{-\frac{\xi\alpha}{\xi(1-\alpha)+1}}\right) + x_i \gamma_i\right]^{\frac{\beta}{\alpha}-1}\right)^{\frac{1}{\frac{1}{\xi}+(1-\beta)}} \tag{A44}$$

$$w^{eqf}_i = \left(\psi_i^{-1} n^{eqf}_i\right)^{\frac{1}{\xi}} \tag{A45}$$

$$(1+\tau_i) w^{eqf}_i = (1+\tau_i)\left(\psi_i^{-1} n^{eqf}_i\right)^{\frac{1}{\xi}} \tag{A46}$$

As in the market-level analysis, the firm-level factor that guides employment and wage effects is (now firm-specific) labor supply elasticity ξ. Here, we broadly interpret ξ as a tractable way to model rent-sharing-like patterns in a labor monopsony narrative.

Deriving the labor cost share of young workers as the mediator of the firm-level effects. Crucially for our identification design, we must show that the empirical sorting of firms by their labor-cost share young predicts larger elasticity of (old, but also overall) employment to a shift in the youth payroll tax rate *in the model*, and in turn into net wages paid by the firm. We do so with a simple comparative static argument of employment to the youth payroll tax.

Share young in the model. First, we define the empirically tractable statistic "payroll share of young in total payroll" in the model. Here it is endogenously chosen by profit-maximizing firms, with ultimate drivers of heterogeneity being differences in firm types' CES weight on

[78] The difference is now that, with $\psi \neq 1$, we cannot directly replace w_o^f as a function of only n_o^f from the worker utility function, but there are between-firm-type wage spillovers through the worker's non separable labor disutility. Since we our cross-sectional difference-in-difference (or dosage treatment) design cannot pick up such spillovers, we here consider $\psi = 1$ for clarify of exposition.

youth labor x_y^f:

$$\boxed{\sigma_y^f} \equiv \underbrace{\frac{n_y^f w_y^f (1+\tau_y)}{n_y^f w_y^f (1+\tau_y) + n_o^f w_o^f (1+\tau_o)}}_{\text{Data analogue: share young in total payroll}} = \underbrace{\frac{x_y^f \gamma_y \left[\frac{x_y^f}{x_o^f} \frac{\gamma_y}{\gamma_o} \frac{(1+\tau_o)}{(1+\tau_y)}\right]^{\frac{\alpha}{1-\alpha}}}{\left[x_y^f \gamma_y \left[\frac{x_y^f}{x_o} \frac{\gamma_y}{\gamma_o} \frac{(1+\tau_o)}{(1+\tau_y)}\right]^{\frac{\alpha}{1-\alpha}} + x_o^f \gamma_o\right]}}_{\text{Structural formulation in model}} \tag{A47}$$

The empirical variation of the cost share is plotted in Figure 5, the histogram in Panel (a) and group-specific time series in Panel (b).

Employment effects. For youth employment we find the following elasticity:

$$\frac{d\ln(n_y^f)}{d\ln(1+\tau_y)} = -\frac{1}{1-\alpha} - \frac{\beta-\alpha}{1-\alpha} \frac{1}{\frac{1}{\xi} + (1-\beta)} \boxed{\sigma_y^f} \tag{A48}$$

For old employment we find the following elasticity:

$$\frac{d\ln(n_o^f)}{d\ln(1+\tau_y)} = -\frac{1}{\frac{1}{\xi} + (1-\beta)} \frac{\beta-\alpha}{1-\alpha} \boxed{\sigma_y^f} \tag{A49}$$

Net wage effects are guided by the elasticity of old employment and the labor supply elasticity, and inherit the dependence on the share of youth labor costs in total labor costs:

$$\frac{d\ln(w_o^f)}{d\ln(1+\tau_y)} = \frac{1}{\xi} \frac{d\ln(n_o^f)}{d\ln(1+\tau_y)} = -\frac{\frac{1}{\xi}}{\frac{1}{\xi} + (1-\beta)} \frac{\beta-\alpha}{1-\alpha} \boxed{\sigma_y^f} \tag{A50}$$

That is, our share young variable in the model corresponds to *exactly* the firm-level variable share young variable we construct in the empirical analysis with firm-level heterogeneity arises, driven from differences in the CES weight on youth labor x_y^f. It guides both employment elasticities and the wage incidence in the model, directly providing structural justification of our empirical approach.

C.5.3 Calibrating the Firm Model to Match the Firm-Level Treatment Effects

Here we present a calibration of the model that generates theoretical effects in line with the empirical treatment effects on the firm-level employment and wage documented in Table 4.

Total firm employment. In the model, the elasticity of *firm-specific* employment – is an equilibrium outcome, i.e. consistent with firm-specific labor supply. The labor market for the old workers clears; we impose perfect pay equity for the young, leading their labor supply to be rationed. The predicted total employment response (young plus old labor) for firm $f \in \{Y, O\}$ (youth- vs. old-heavy, corresponding to the high share vs medium share young firms) is the average of elasticity of youth labor y (A48) and of old labor o (A49), weighted by firm's share young in employment (thus in payroll) σ_y^f:[79]

$$\frac{d\ln(n^f)}{d\ln(1+\tau_y)} = \sigma_y^f \frac{d\ln(n_y^f)}{d\ln(1+\tau_y)} + (1-\sigma_y^f)\frac{d\ln(n_o^f)}{d\ln(1+\tau_y)} \tag{A51}$$

$$= -\sigma_y^f \left[\frac{1}{\frac{1}{\xi} + (1-\beta)} \frac{\beta-\alpha}{1-\alpha} + \frac{1}{1-\alpha} \right] \tag{A52}$$

Parameters. ξ is the firm-specific labor supply elasticity, β is the degree of overall returns to scale in CES production; α is the parameter guiding the substitutability of old and young labor. σ_y^f, defined in Equation A47 and itself an endogenous outcome, is payroll share young for firms of type $f \in \{Y, O\}$, (youth-intense i.e. high share young firm Y; medium-share young firm O).

Treatment effects: model vs. data. The treatment effect on employment is 4.6 percent in Table 4. The treatment is youth-tax shift $d\ln(1+\tau_y)$ of around -12 percent. We therefore target a $4.6\%/(-12\%) \approx -0.38$ elasticity difference. The model's structural analogue of the treatment effect is the *difference* in the employment response between firm type Y and O in response to the youth tax shift $d\ln(1+\tau_y)$, and therefore can be expressed in terms of elasticity differences given the homogenous treatment:

$$\underbrace{\frac{\overbrace{d\ln(n^Y) - d\ln(n^O)}^{4.6\%}}{\underbrace{d\ln(1+\tau_y)}_{-12\%}}}_{\approx -0.38} = -\underbrace{[\sigma_y^Y - \sigma_y^O]}_{\approx 15\%} \cdot \underbrace{\left[\frac{1}{\frac{1}{\xi} + (1-\beta)} \frac{\beta-\alpha}{1-\alpha} + \frac{1}{1-\alpha} \right]}_{\substack{\text{Must be around } 0.38/15\% = 2.53 \\ \text{for model to match treatment effect}}} \tag{A53}$$

Calibrating the model. First we calibrate $\Delta\sigma_y = \sigma_y^Y - \sigma_y^O$. In Table 3 we show the pre-reform (2007) summary statistics incl. share young by firm group f, and in Figure 5b we show the group-specific evolution. The initial difference is around 20 percent, which falls towards 10 percent in the end. We empirically discipline the cross-sectional firm heterogeneity in share young by the mid-point, around $\Delta\sigma_y = \sigma_y^Y - \sigma_y^O = 0.15$. We have now reduced the calibration to targeting the bracketed right factor to around $0.38/15\% = 2.53$. There naturally are various parameterizations for our stylized model to match that target. We first note that the firm-specific labor supply elasticity is uniquely pinned down as $\xi = 2.3$ by the ratio of the firm-level employment effects (4.6 percent) to wage effects (2 percent) as in structural equation (A50). Then, for $\alpha = 0.2$ (youth and old workers are complements) and $\beta = 0.825$, we obtain 2.53 for the right bracketed term.[80] Finally, note that these effects are *reallocation effects*, not necessarily allowing us to extrapolate the between-firm heterogeneity results to aggregate employment gains (and we ignore between-firm-type product and labor market interactions).

[79]In the model, due to perfect pay equity, the payroll share is equal to the employment share.

[80]Overall production has decreasing returns somewhat weaker than the labor share parameter we find in Table 3, as we also find capital effects, but this parameter also captures downward-sloping product demand. An alternative calibration is $\alpha = 0.46$ (lower complementarity) and $\beta = 0.72$ (stronger decreasing returns), for example.

Wage effects and rent sharing. An important additional question is whether the model can also generate the *rent sharing effects.* The aforementioned expression denotes total effects including equilibrium adjustment, specifically the firm going down its labor supply curve. The equilibrium wage differential between the firm types is then given by $d\ln(w_y^Y) - d\ln(w_y^O) = \frac{1}{\xi}\cdot\left[d\ln(n_o^Y f) - d\ln(n_o^O)\right]$, a fact we used to calibrate $\xi = 2.3$. The implied labor supply elasticity to the firm is therefore a crucial ingredient both for the employment elasticity as well as driving the rent sharing effect, and it is encouraging that our findings imply a reasonably elastic value.

Assessing the implied absolute employment elasticities vs. relative treatment effects. An interesting cross validation check is to examine the *absolute* employment elasticities implied by this calibration fitted to match the cross-sectional treatment effect heterogeneity. While our difference-in-differences identification strategy identifies parameter sets of relative responses, the calibrated model permits to then back out and evaluate implied absolute elasticities. We present elasticities reflecting equilibrium employment movements to youth tax changes, i.e. net of the labor supply and thus rent sharing responses. We parameterize the employment elasticities for old labor (A49) and young labor (A48) as described in our calibration matching the relative treatment effect for employment. For share young levels, we use initial $\sigma_y^Y = 0.32$ and $\sigma_y^O = 0.12$ for high and medium share young firms (Table 3). For the absolute (rather than differential) elasticities, we obtain -1.64 for youth labor and -0.40 for old labor in the high share young firms. For the medium share young firms, the youth employment elasticity is -1.40, and -0.15 for old labor.

In short, the model calibrated to reasonable values appears to fit the key treatment effects estimated in our empirical analysis. Still, we note the stylized version of the model (two firm types, two labor types, perfect pay equity within the firm, absence of liquidity constraints, and a pure monopsony model of rent sharing), implying that alternative models and thus different parameterizations may well also account for the facts.[81]

Additional References

Bils, Mark, and Pete Klenow. 2008. "Further Discussion of Temporary Payroll Tax Cut during Recession(s)." unpublished working paper.

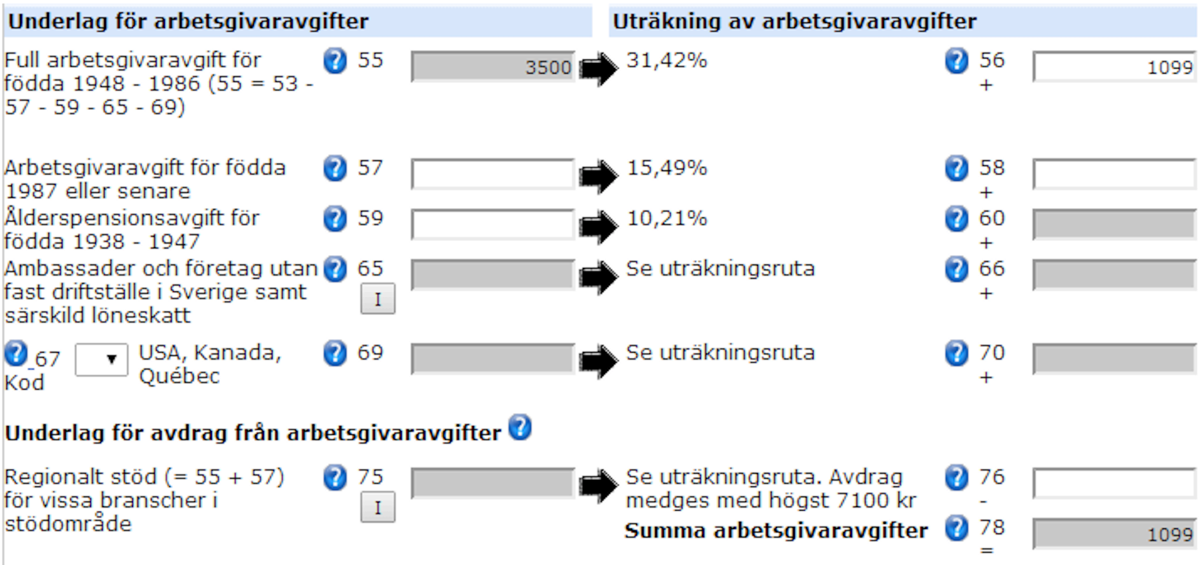
Daruich, Diego, Sabrina Di Addario, and Raffaele Saggio. 2018. "The Effects of Partial Employment Protection Reforms: Evidence from Italy", UC Berkeley Working Paper.

Elsby, Michael. 2009. "Evaluating the economic significance of downward nominal wage rigidity. *Journal of Monetary Economics*, 56(2), 154169.

Shimer, Robert. 2004. "The consequences of rigid wages in search models, *Journal of the European Economic Association*, 2(2-3), 469–479.

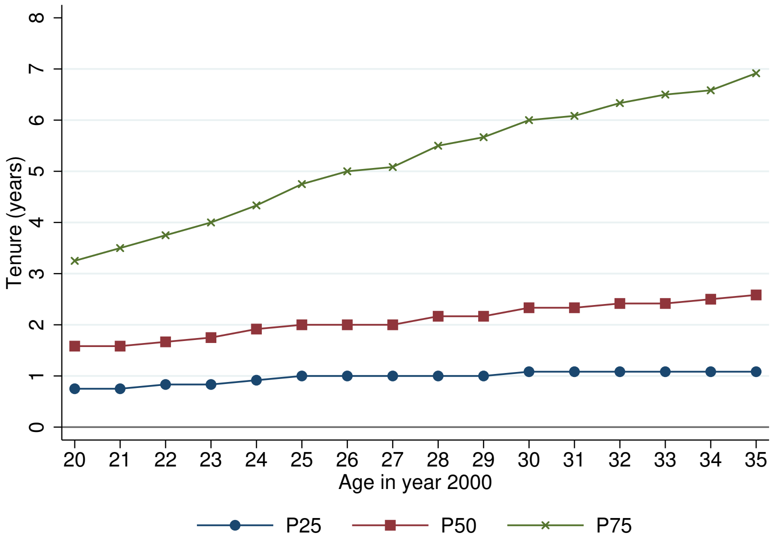
[81]In fact, in Appendix B we provide a quantitative assessment of whether the input and scale effects may be partially or even fully explained by liquidity effects, finding a positive answer. Liquidity effects are not present in the model, which presents a more standard monopsony mechanism (and we do find large effects even for plausibly unconstrained firms).

Figure A1: Payroll Tax Software for Employers



Notes: The figure illustrates the reporting of monthly earnings and payroll taxes by employers for year 2013, by depicting a snapshot of the government-provided software. Employers specifically type in the wage payments made to employees born in different cohort categories and the program automatically calculates the payroll taxes due, ensuring almost perfect take-up. Earnings for employees born in 1948-1986 are reported in box 55 and face the normal tax rate of 31.42 percent. Earnings for employees born in 1987 or after are reported in box 57 and face the lower 15.49 percent tax rate (a lower rate of 10.21 percent applies to older workers born in 1947 or earlier in box 59, which is not part of this study).

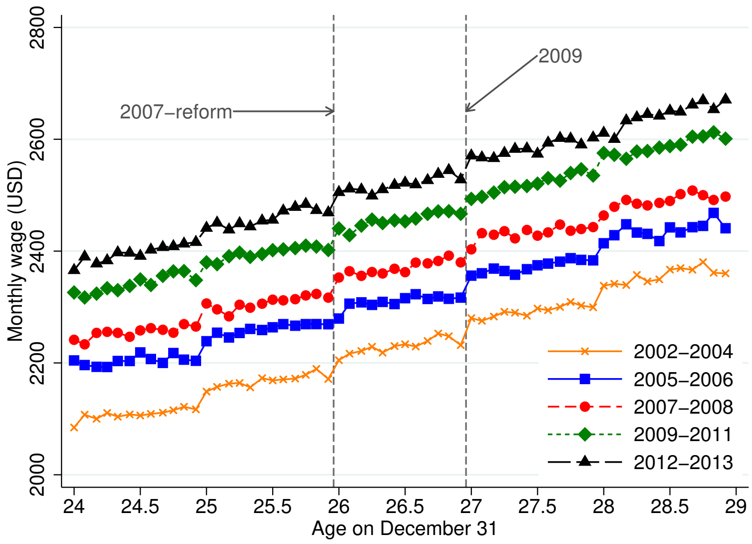
Figure A2: New Hires in 2000: Job Length by Age



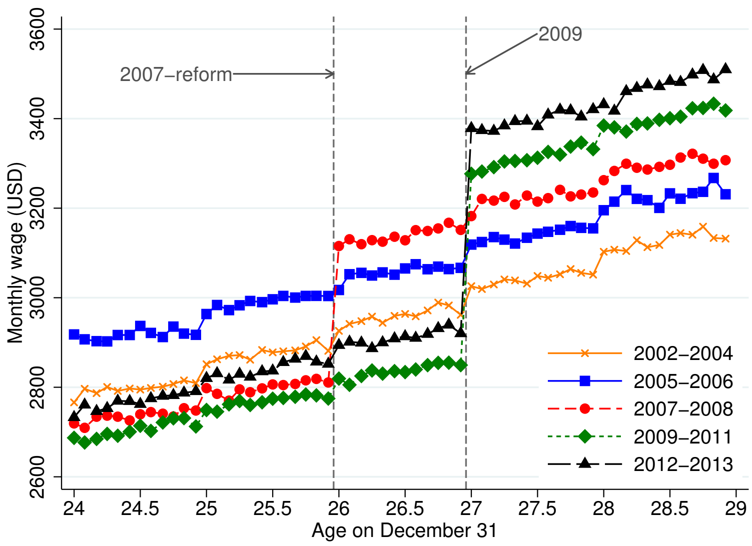
Notes: This figure depicts job length by age of hiring for individuals newly hired in 2000. A newly hired individual is defined as someone who never worked with the firm in the past but starts in 2000. We additionally require that earnings in 2000 from that employer exceed the minimum threshold of $4,940 (in 2012 dollars). For individuals with multiple new spells in 2000 exceeding that threshold, we select the spell with highest earnings. We then follow that spell over time and define a separation in year t as having earnings from that same employer (who hired them in 2000) in year t but not in year $t+1$ (the series are only slightly different if we allow for one-year-gaps in the spells, accounting for the generous parental leave system and sickness insurance). Age is defined as the age the person reaches during year 2000. The series depict the 25th, 50th and 75th percentiles of tenure, measured in months, for the work spells among such newly hired by age in 2000. We do not show the mean as the series are censored in 2013 (the last year of data). For young workers aged 20-26, the median spell is less than 2 years. The series implies that the absence of tax incidence on wages cannot be explained solely by the concern that all young hires will age out of the payroll tax eligibility on the job.

Figure A3: Effect of the Payroll Tax Cut on Average Wages by Monthly Cohorts

(a) Monthly net wage by month of birth



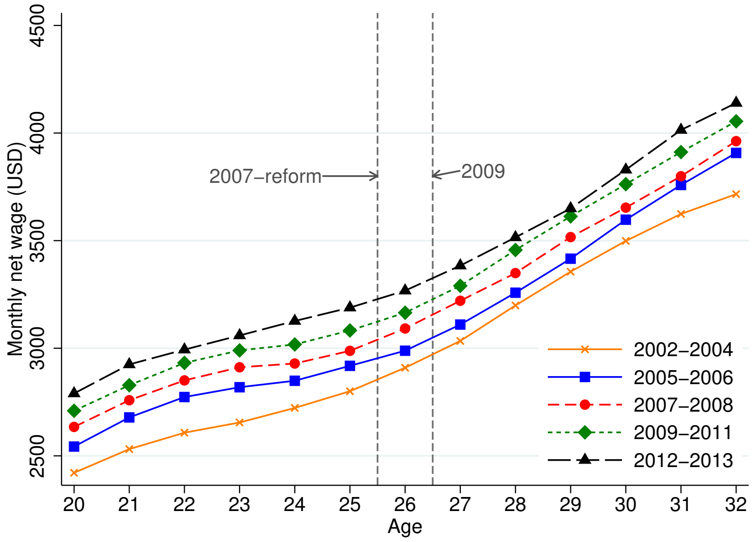
(b) Monthly gross wage by month of birth



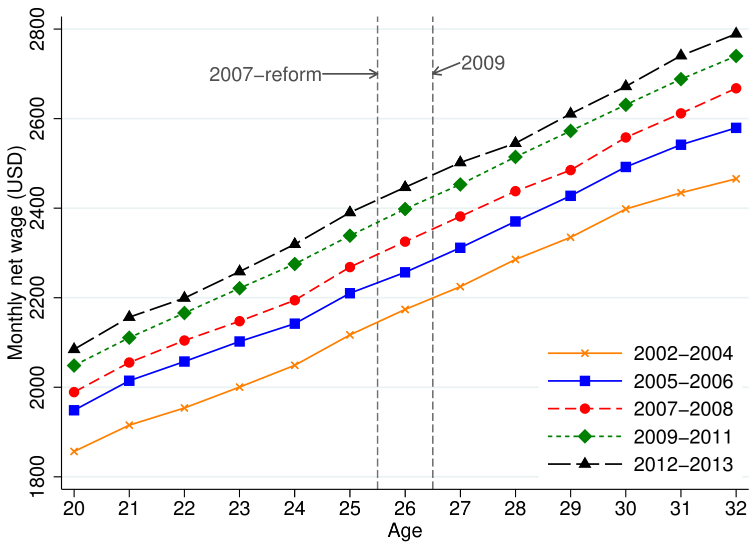
Notes: This figure replicates Figure 2 but zooming in by month of birth instead of year of birth. For comparison with Figure 2, for any given year t when the wage is measured, monthly birth cohorts are translated into monthly age bins as of end of year t. For example, 27 in 2009 means being born in January 1982 (and not eligible for the tax cut). 26+11/12 in 2009 means being born in December 1983 and eligible for the tax cut. The top panel depicts *net wages* defined as monthly wage earnings net of payroll taxes. The bottom panel depicts *gross wages* defined as monthly wage earnings gross of payroll taxes. The two dashed vertical lines depict the age thresholds under which the payroll tax cuts apply in 2007-08 and 2009-14 respectively. The top panel shows that the wages are continuous at the age thresholds. There are small positive discontinuities at each year threshold as the school system is based on calendar year of birth (and hence people born in December of year t are in general 1 year in advance in their career path relative to people born in January of year $t + 1$). The bottom panel shows that the gross wage is discontinuous at the tax reform age thresholds. Corresponding estimates are provided in Table 1.

Figure A4: The Effect of the Payroll Tax Cut on Net Wages for Subsamples

(a) Top 20 percent of the wage distribution



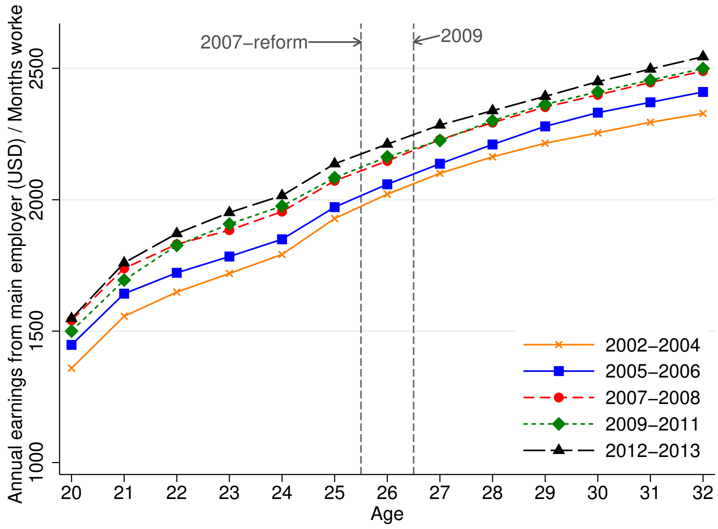
(b) New hires and job switchers



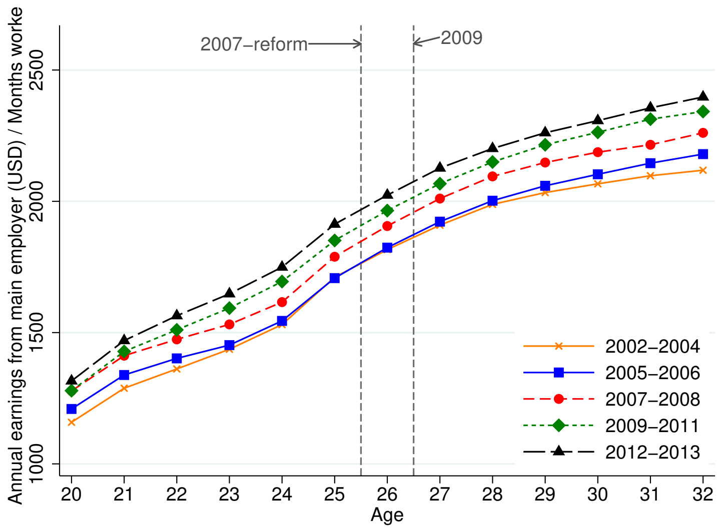
Notes: This figure depicts the average monthly net wage (i.e., exclusive of the payroll tax) in Sweden by age for different time periods using the Structure of Earnings Survey data generally for the month of September (with some measurements in October and November) of each year. We consider two specific subsamples. The top panel displays the average wage within the top 20 percent of the wage distribution conditional on age and year. This top group is not affected by the minimum wage floors. The bottom panel shows the average wage (measured in September) for new hires or job switchers, defined as having a new firm identifier as the main (i.e., highest paying) employer relative to September of the previous year. It includes both job-to-job transitions as well as new hires among the non employed. Both wage series are inflation adjusted (base-year 2003) and converted to USD using an exchange rate of 8.9 SEK/USD (as of 4/18/2017). Both graphs show no discontinuity in wages at the age thresholds implying that the absence of incidence on workers is not due to minimum wage floors (top panel) or rigid wages within a job spell (bottom panel). Corresponding estimates are provided in Table 1.

Figure A5: The Effect of the Payroll Tax Cut on Wages: High vs. Low Turnover Industries

(a) High Turnover Industries: Monthly Net Wages



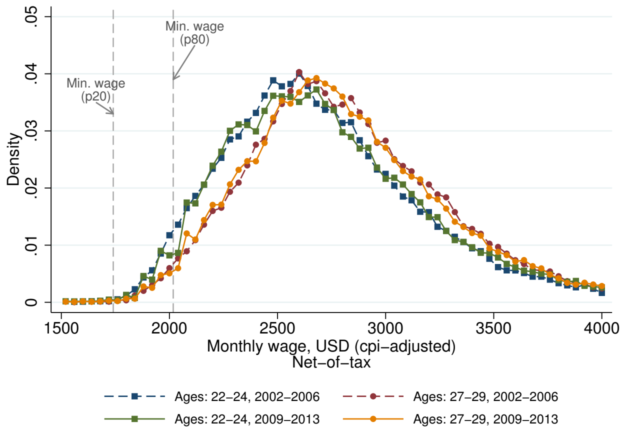
(b) Low Turnover Industries: Monthly Net Wages



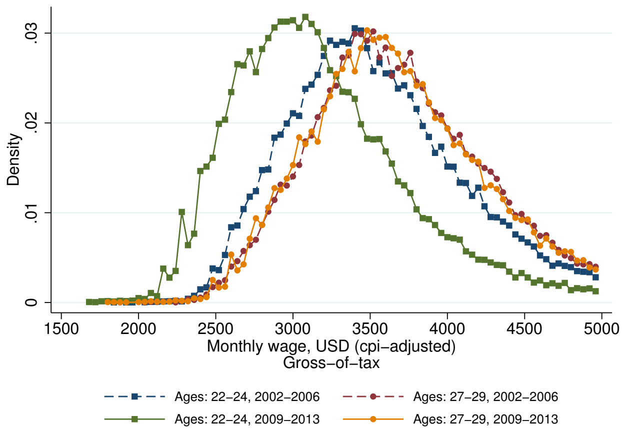
Notes: This figure examines robustness of our market-level wage results by replicating it separately for high and low turnover industries. Our turnover measure is the mean job duration of new job spells in 2000 for workers aged 20-25, within industry for the coarse 10-industry classification. We split industries by median (weighted by 2000 employment). Net wages are continuous at the age thresholds for both high and low turnover industries. The absence of wage incidence even for the high-turnover industries indicates that wage-smoothing in long-term wage contracts is unlikely to be the main explanation for the wage patterns. The wage measure is average monthly net wage (the full-time equivalent contracted monthly wage, net of payroll taxes) in Sweden by age and time periods using the Structure of Earnings Survey data. It is adjusted for inflation (base-year 2003) and converted to US dollars using an exchange rate of 8.9 SEK/USD (as of 4/18/2017). Age is defined as the age turned during the calendar year, which is the relevant concept for the payroll tax cut. The two dashed vertical lines depict the age thresholds under which the payroll tax cuts apply in 2007-08 and 2009-14 respectively.

Figure A6: Net and Gross Monthly Wage Earnings Densities

(a) Monthly net wage earnings (exclusive of payroll tax)



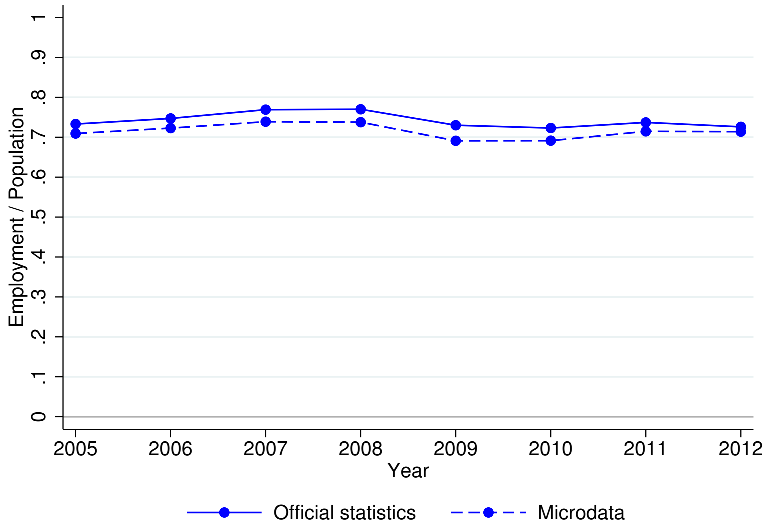
(b) Monthly gross wage earnings (inclusive of payroll tax)



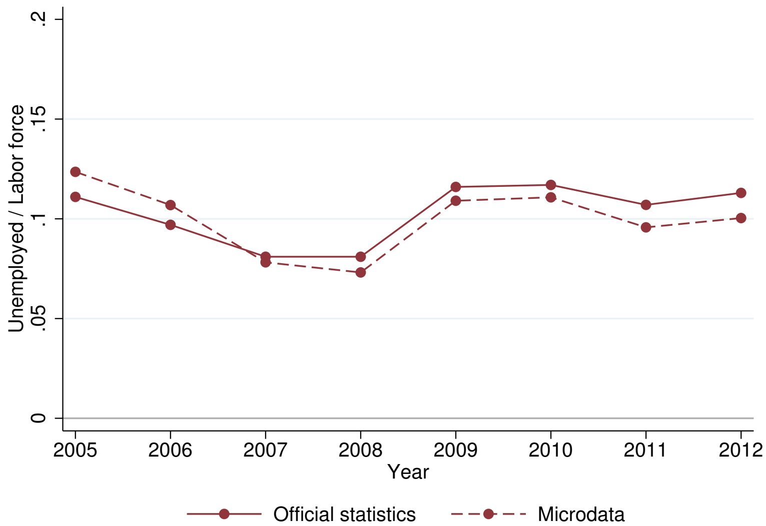
Notes: This figure depicts the monthly wage earnings densities for young workers (aged 22-24) affected by the payroll tax cut in (in squares) and for slightly older workers (aged 27-29) not affected by the payroll tax cut (in circles) both pre-reform (pooling years 2002-2006 in dashed lines) and post-reform (pooling years 2009-2013 in solid lines). The top panel depicts the densities for net wages. The bottom panel depicts the densities for gross wages. Wage earnings densities are measured typically in September (and sometimes October-November). Wages are adjusted for annual wage growth by first constructing a wage index based on the older individuals. Using this index, we deflate all workers' wages to 2013 values. The top panel shows that the net wage earnings densities do not change from pre-reform to post-reform both for young treated workers and for the control slightly older workers. In particular, even the earnings density substantially above the minimum wages for young workers is unaffected. The top panel depicts in vertical lines the 20th and 80th percentiles of minimum wages (as there are many minimum wages in Sweden based on industry, occupation, and tenure). This shows that the vast majority of young workers are paid above the minimum wage. The bottom panel correspondingly shows that the labor cost density is shifted uniformly from pre-reform to post-reform for young treated workers.

Figure A7: Comparing our Employment and Unemployment Measures with Official Statistics

(a) Comparison of the employment rates



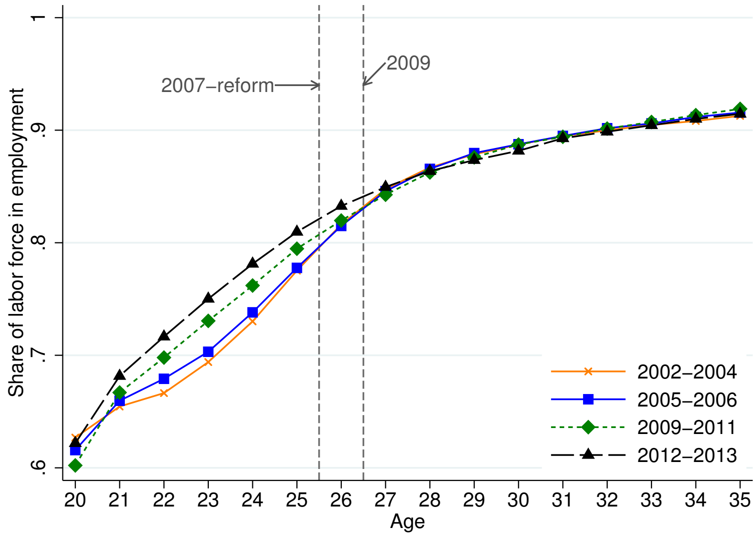
(b) Comparison of the unemployment rates



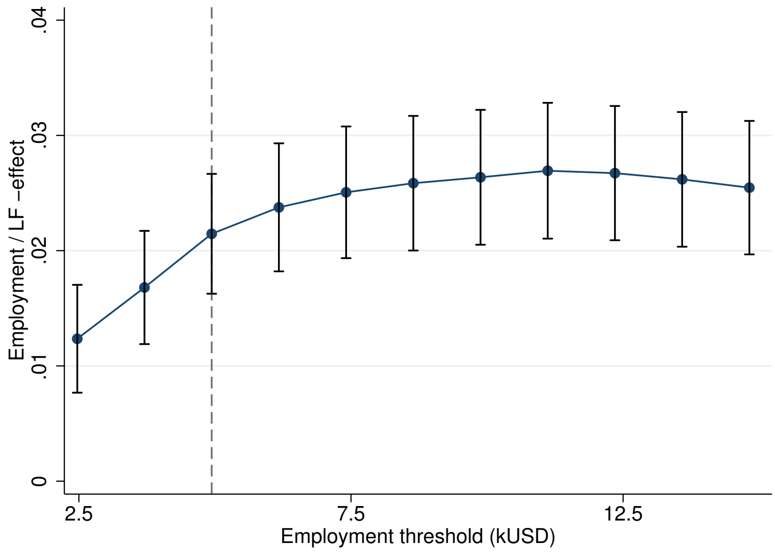
Notes: This figure compares our employment and unemployment measures with official statistics. Our measures are created using administrative full population data while official statistics are created using survey data. The top panel depicts the share of the population aged 20-34 in employment. The bottom panel considers the share of the labor force aged 20-34 (which includes the employed and the unemployed) in unemployment. In our data, a person is defined as employed when annual wage earnings (either from wages or self-employment) are above a minimum threshold of $4,940 in 2012 (and adjusted for inflation in other years). In official statistics, a person is defined as employed if he/she works at least one hour during the week of the survey (or has an employment contract, but was absent during the survey week). In our data, a person is defined as unemployed when he/she has zero earnings or earnings below the minimum threshold and has been registered with the unemployment agency for at least one day during the year. In official statistics, an individual is labelled unemployed if he/she is not employed but has applied for at least one job during the past four weeks.

Figure A8: The Effect of the Payroll Tax Cut on Employment to Labor Force: Robustness

(a) Adding students to labor force

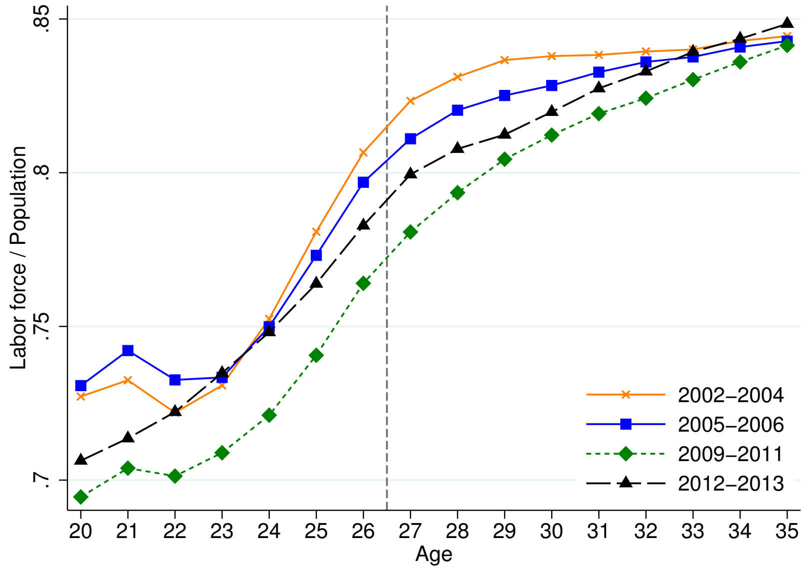


(b) Varying the earnings threshold to define employment



Notes: This figure investigates the robustness of the employment to labor force effects depicted in Figure 4, top panel. In the top panel, we add students to the labor force denominator. In the bottom panel, we keep the labor force constant (using the baseline definition) but vary the earnings threshold required for being labelled as employed. We then estimate the DD-specification in equation (1) and plot the coefficients of the reform-effect along with 95 percent-confidence intervals. Both graphs show that the employment effects we have obtained are robust to these alternative definitions. The estimates from adding students to the labor force are presented in row 2 of Table 2. The bottom panel shows that employment to labor force effects are strongest when employment is defined as annual wage earnings above $10,000.

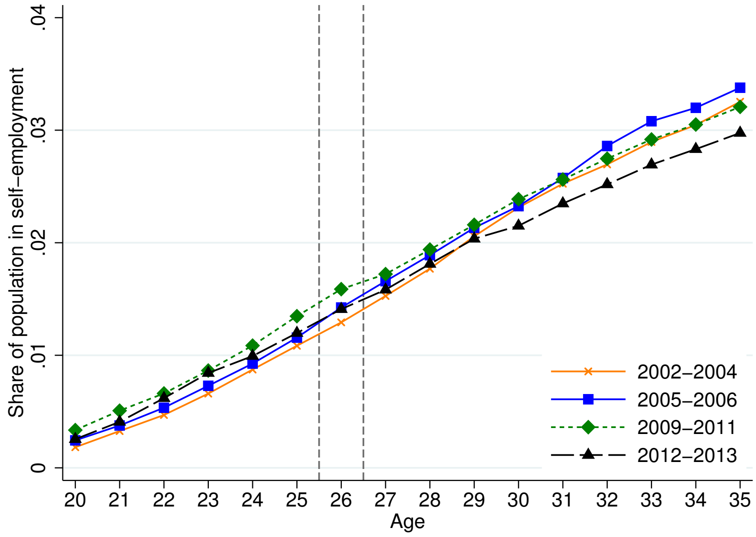
Figure A9: The Effect of the Payroll Tax Cut on Labor Force to Population Ratio



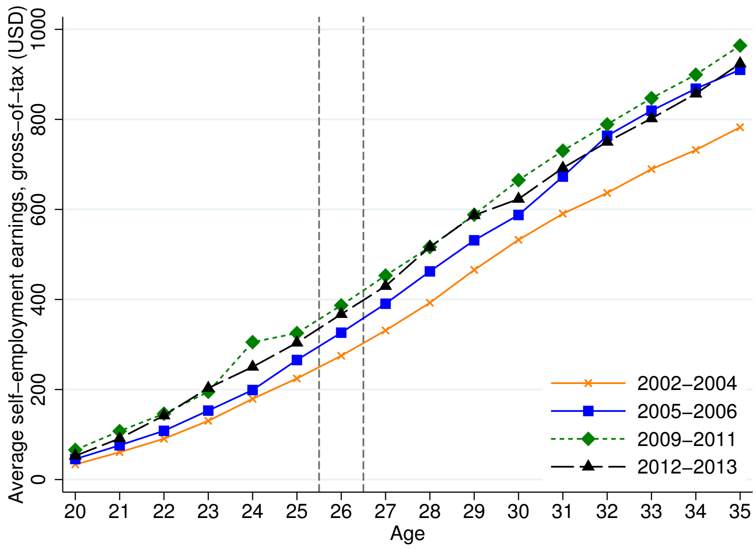
Notes: The figure shows the labor force to population ratio by age and time periods. The labor force numerator is defined as in Figure 4 as all residents who are either (i) employed with annual wage earnings above a small annual threshold; (ii) unemployed defined as having registered with the Unemployment Office at any point during the year. The small annual threshold is equal to $4,940 in 2012 (and adjusted for inflation in other years). The population denominator is defined as all residents. The figure does not show a clear impact of the tax cut on labor force to population suggesting no supply side response from individuals (although pre-trends are not very parallel and hence could mask small effects).

Figure A10: The Effect of the Payroll Tax Cut on Self-Employment

(a) Fraction of the population with self-employment earnings

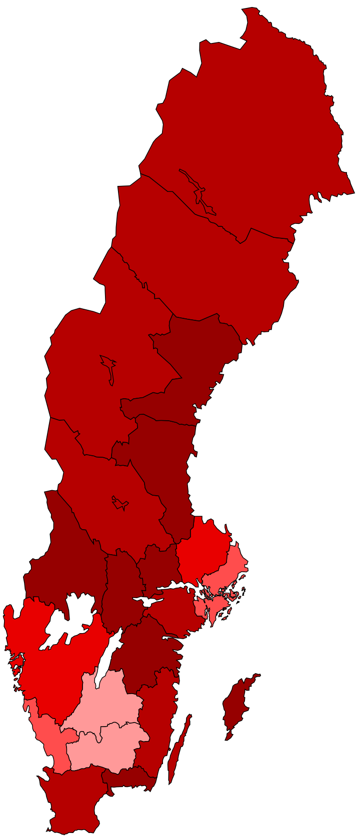


(b) Average self-employment earnings gross of tax (and including zeros)



Notes: This figure depicts the effects of the payroll tax cut on self-employment. The top panel plots the share of the population by age and groups of years with annual self-employment earnings above a minimum threshold of $4,940 (in 2012 and adjusted for inflation in other years) as used for our wage earnings analysis. The bottom panel plots average self-employment earnings in the full population (hence including zeros) by age and groups of years. Self-employment earnings are before payroll tax so that there is no mechanical effect of the reform. Both graphs suggest relatively modest effects on self-employment of the payroll tax cuts, hereby replicating the findings of Egebark (2016). However, the pre-trends are not as compellingly parallel as in our analysis of wage earnings, so that we have less confidence on the reliability of these self-employment effects. Regression results based on these graphs are somewhat sensitive to the age range chosen (results not reported).

Figure A11: Youth Unemployment Rate by Region 2006



Quintiles of Youth Unemployment Rate in 2006

Q5: Darkest $20 - 23.3 percent$

Q4: Dark $17.8 - 20 percent$

Q3: Medium $14.9 - 17.8 percent$

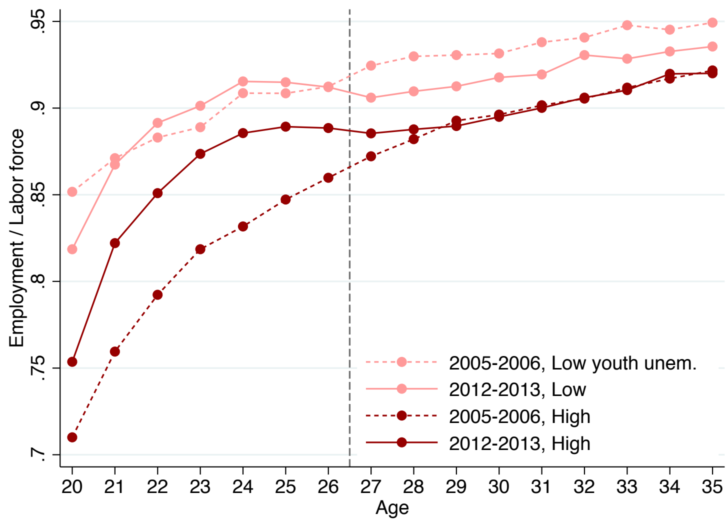
Q2: Light $12.4 - 14.9 percent$

Q1: Lighest $10.5 - 12.4 percent$

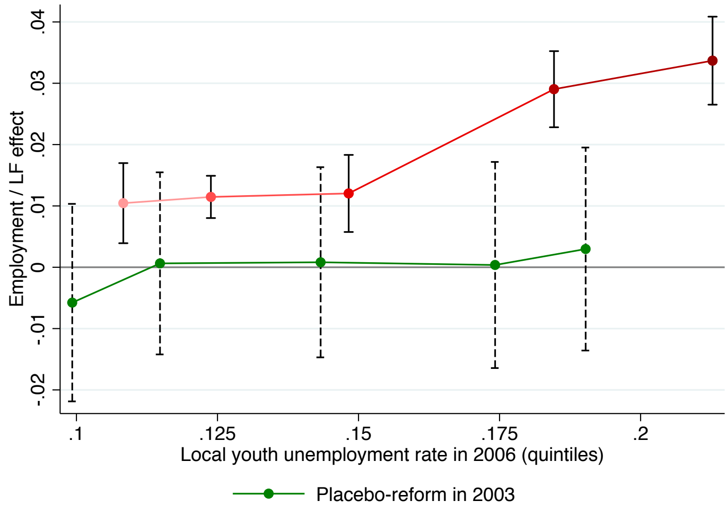
Notes: This figure depicts the heterogeneity in youth unemployment rate in 2006 across Swedish regions. Youth unemployment rate is defined as the unemployment rate (unemployed to labor force) among individuals aged 16-25. We follow the same definition as in our analysis on Figure 4, Panel (a). We divide all 21 regions of Sweden into five quintiles (population weighted) and use a color scale for each quintile from lightest (lowest unemployment rate) to darkest (highest unemployment rate). The legend next to the map displays the ranges of youth unemployment rates across each quintile. This division of regions underlies the analysis of heterogeneous employment effects by size of unemployment rate depicted in Figure 4, Panel (b) and Table A2.

Figure A12: Effects on Employment: High vs. Low Unemployment Regions

(a) Employees to labor force ratio: high vs. low unemployment regions



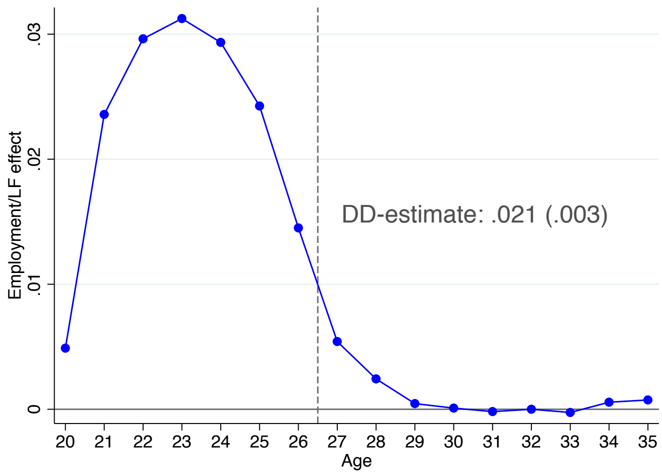
(b) Estimated effects by level of youth unemployment rate



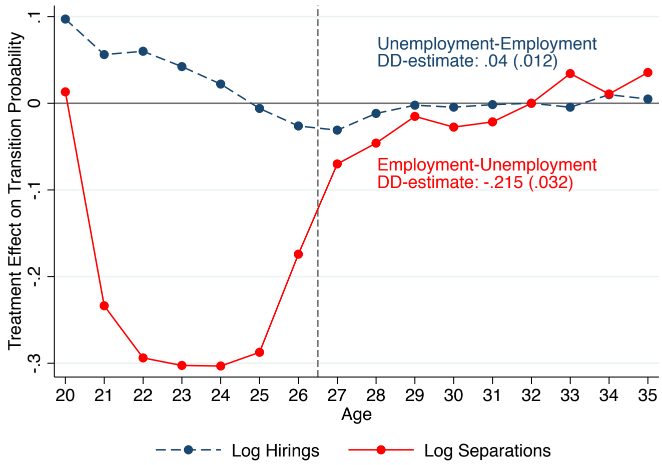
Notes: The top panel shows the share of employees in the labor force by age, pre-reform (2005-2006, in dashed line) and post-reform (2012-2013, in solid line) separately for bottom quintile regions with the lowest 2006 youth unemployment rate (10.5-12.4 percent, in light color) and top quintile regions with highest 2006 youth unemployment (20-23.3 percent, in dark color). See Appendix Figure A11 for a map of the regions. The top panel shows a strong effect of the reform in increasing the employment rate of young targeted workers (corresponding estimates in Table 2). The top panel shows that the employment effects of the payroll tax cut appear much larger in the high unemployment regions than in the low unemployment regions. The bottom panel depicts the corresponding estimates from Appendix Table A2 (see notes of the table for complete explanations).

Figure A13: Employment Effects and Hiring vs. Separation Decomposition

(a) Employment effects by age



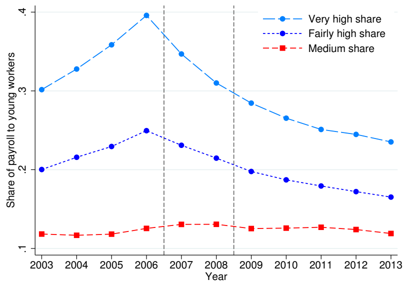
(b) Employment effects by age: hiring vs. separation



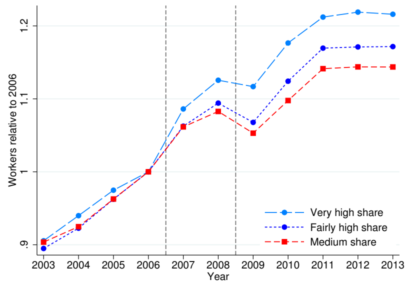
Notes: The top panel depicts the change in employment rates pre-reform (2002-6) vs. post-reform (2009-2013) by age. Using the time series depicted in Figure 4, we regress employment / LF on age dummies, period dummies and age dummies interacted with a dummy for the post-reform period for ages 20-35. The last set of dummies are shown in this graph (age 32 is the omitted category). The reported DD-estimate is simply the difference between the treatment group (age group 20-26) and the control group (age group 27-35) weighted by the labor force at each age in 2005-6 (corresponding to first row of Table 2). The bottom panel decomposes these employment effects into a hiring effect and a separation effect. The hiring effect is estimated as follows. We compute the share of unemployed individuals in year $t-1$ who find a job in year t and estimate the treatment effect on the log of that share using the same specification as for the top panel. The separation effect is estimated as follows. We compute the share of employed individuals in year $t-1$ who transition into unemployment in year t and again estimate the reform-effect on the log of that share. The reported DD-estimates are the differences between the treatment and control group before (2002-2006) and after the reform (2009-2013). The bottom panel shows that almost 4/5 of the employment effects from the top panel are due to a reduction in the separation rate of young workers and that about 1/5 of the employment effects from the top panel are due to an increase in the hiring rate of young workers.

Figure A14: Firm-level Effects: Very High and Fairly High vs. Medium Share Young

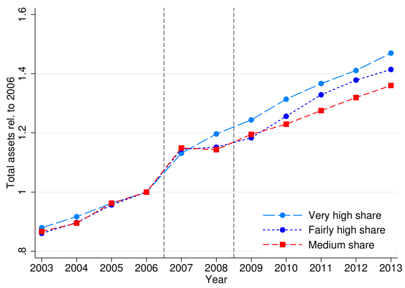
(a) First stage: share young



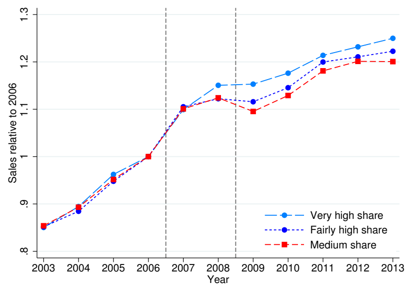
(b) Employment



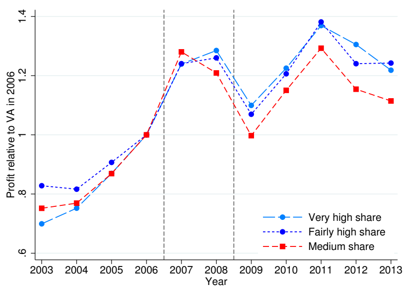
(c) Capital: total assets



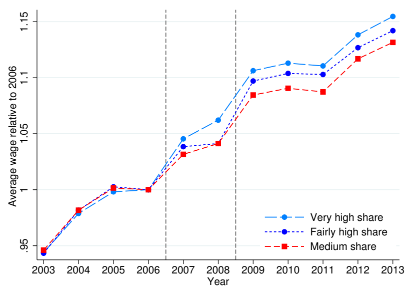
(d) Sales



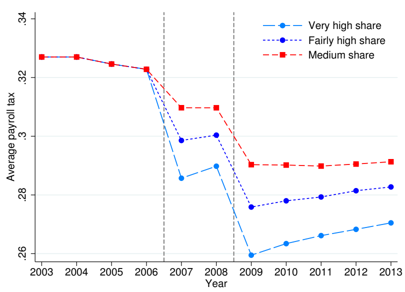
(e) Profits



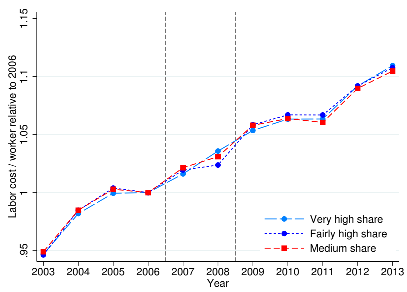
(f) Net wage earnings per worker



(g) Payroll tax per worker



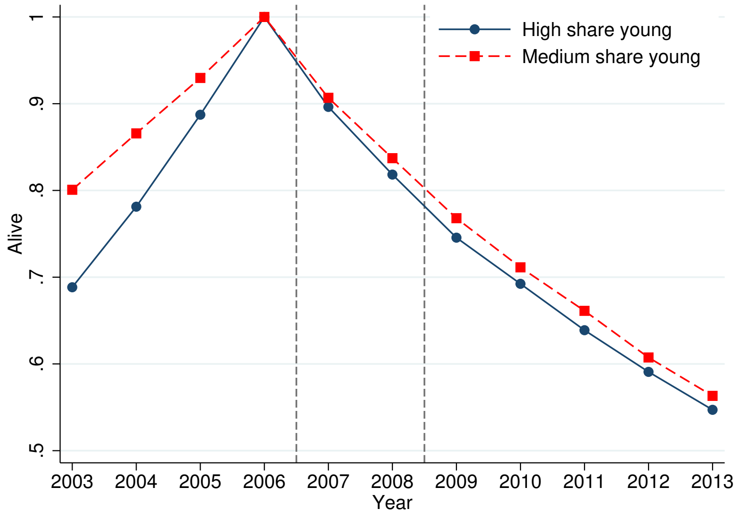
(h) Gross wage earnings per worker



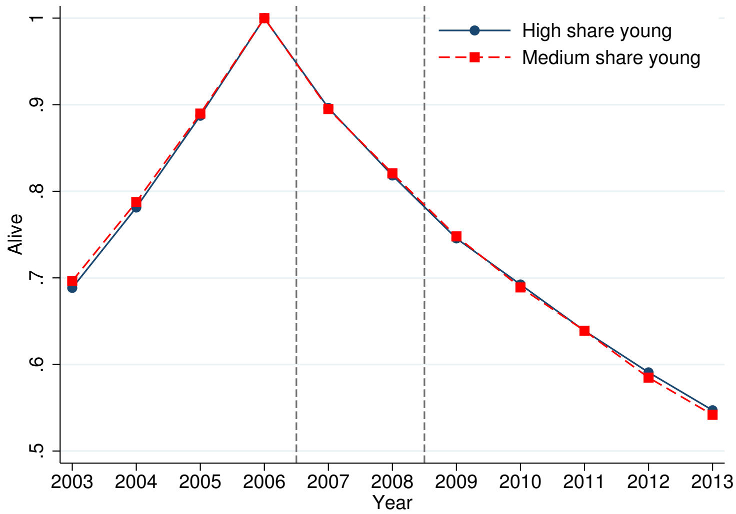
Notes: This figure reproduces the first stage Figure 5, the firm-level effects on business growth of Figure 6, and net and gross wages per worker of Figure 7 but further splitting the high share young group into 2 equally sized groups: (a) very high share young (firms in the top 1/8 of share young in 2006 with a share young of 39.6 percent on average in 2006), (b) fairly high share young (firms in the next 1/8 of share young in 2006 with a share young of 25 percent on average in 2006). All panels show parallel pre-trends (except profits) and larger effects for the very high share young group than for the fairly high share young group. Estimates are provided in Table 4.

Figure A15: Effect of Payroll Tax Cut on Firm Survival

(a) Fraction of firms operating (unweighted)



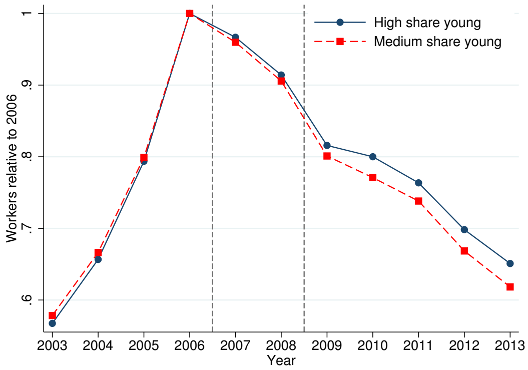
(b) Fraction operating after DFL reweighting by age in 2006



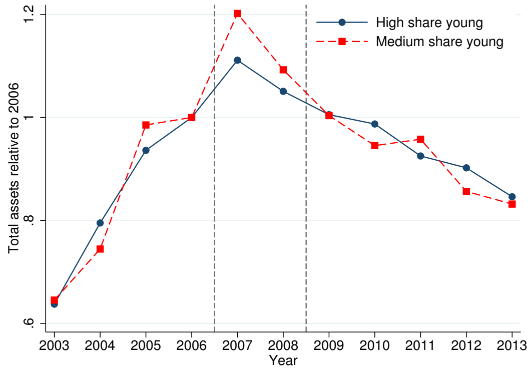
Notes: The figure analyses firm survival using the sample of all firms present in 2006 and operating with more than 3 workers in 2006, regardless of whether they operate in other years. We consider again firms with a high share young in 2006 vs. firms with a medium share young in 2006. Panel (a) plots the fraction of firms operating in each group for years 2003 to 2013. By definition of the sample, this fraction is equal to one in 2006. The panel shows that firms with high share young are younger (less likely to operate before 2006) and slightly less likely to survive (less likely to operate after 2006) than firms with medium share young. Therefore, to be able to analyze compellingly whether the reform affects survival, in Panel (b) we DFL-reweight firms in the medium share young group to align their 2006 age distribution to the high share young group. We do so by partitioning each group into 8 age based subsets and reweighting each subset so that, after reweighting, the fraction of firms in each age subset is equal across the two groups. We then plot again fraction of firms operating in each group for years 2003 to 2013 in Panel (b). The panel shows that post-reform survival rates across the two groups are identical suggesting that the reform does not differentially affect survival.

Figure A16: Firm-level Effects: Robustness to Including non operating Firms

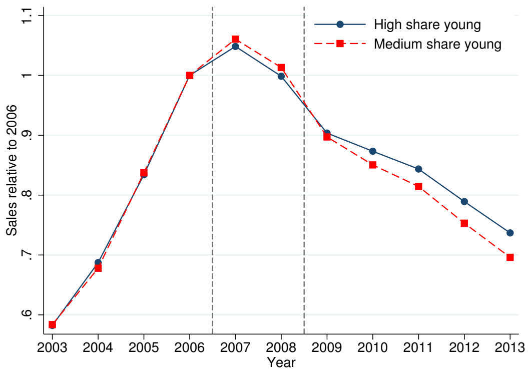
(a) Number of workers



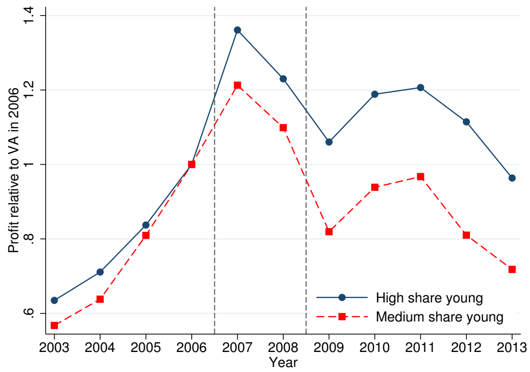
(b) Capital: Total Assets



(c) Sales



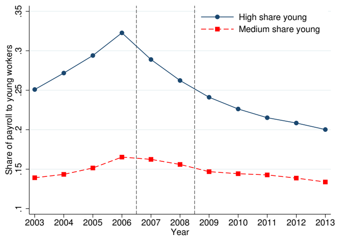
(d) Profits



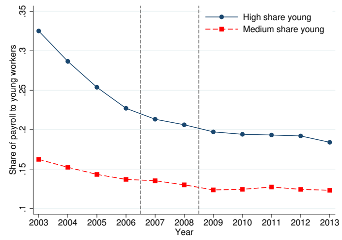
Notes: The figure proposes a robustness check of Figures 6 by including non operating firms instead of considering a balanced panel of firms operating in all years 2003-2013 as in the main text. In this graph, we consider all firms present in 2006 and operating with more than 3 workers in 2006, regardless of whether they operate in other years as in Figure A15. Firms are naturally assigned zero values for employment, assets, sales, and profits in years in which they do not operate. We then compare firms with a high share young in 2006 to firms with a medium share young in 2006 as in the main text. As in Panel (b) of Figure A15, we DFL reweight firms based on their age in 2006 in order to make the two groups comparable in terms of pre-trends. Panels (a-d) show that pre-trends are well aligned (except for profits) and that firms with high share young experience faster employment, sales, and profits growth after the reform consistent with the results using the balanced panel of firms in the main text. The series for total assets are noisier and do not show any significant effect. Corresponding estimates are presented in Table 4, column (4).

Figure A17: Firm-level Effects: Robustness to Changing the Base Year from 2006 to 2003

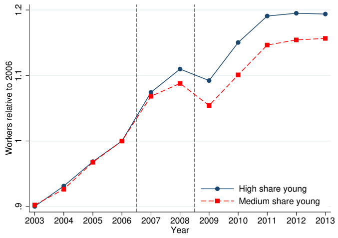
(a) First stage (2006)



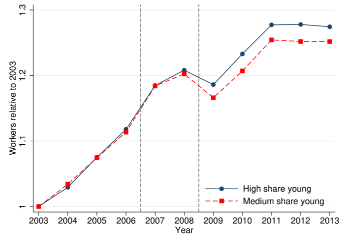
(b) First stage (2003)



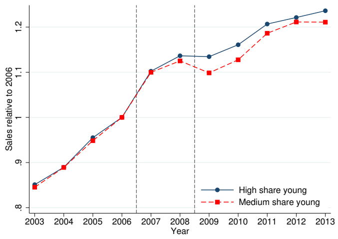
(c) Number of workers (2006)



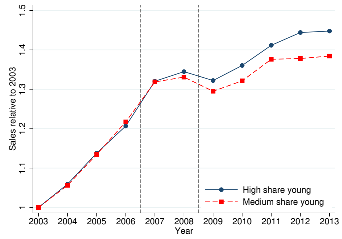
(d) Number of workers (2003)



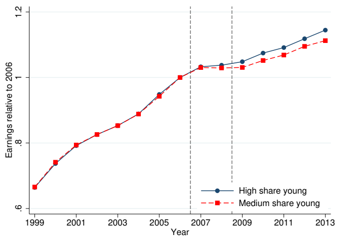
(e) Sales (2006)



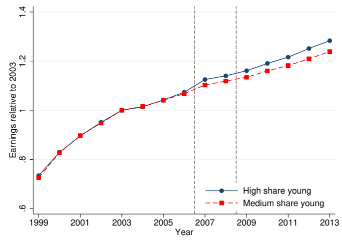
(f) Sales (2003)



(g) Individual earnings (2006)

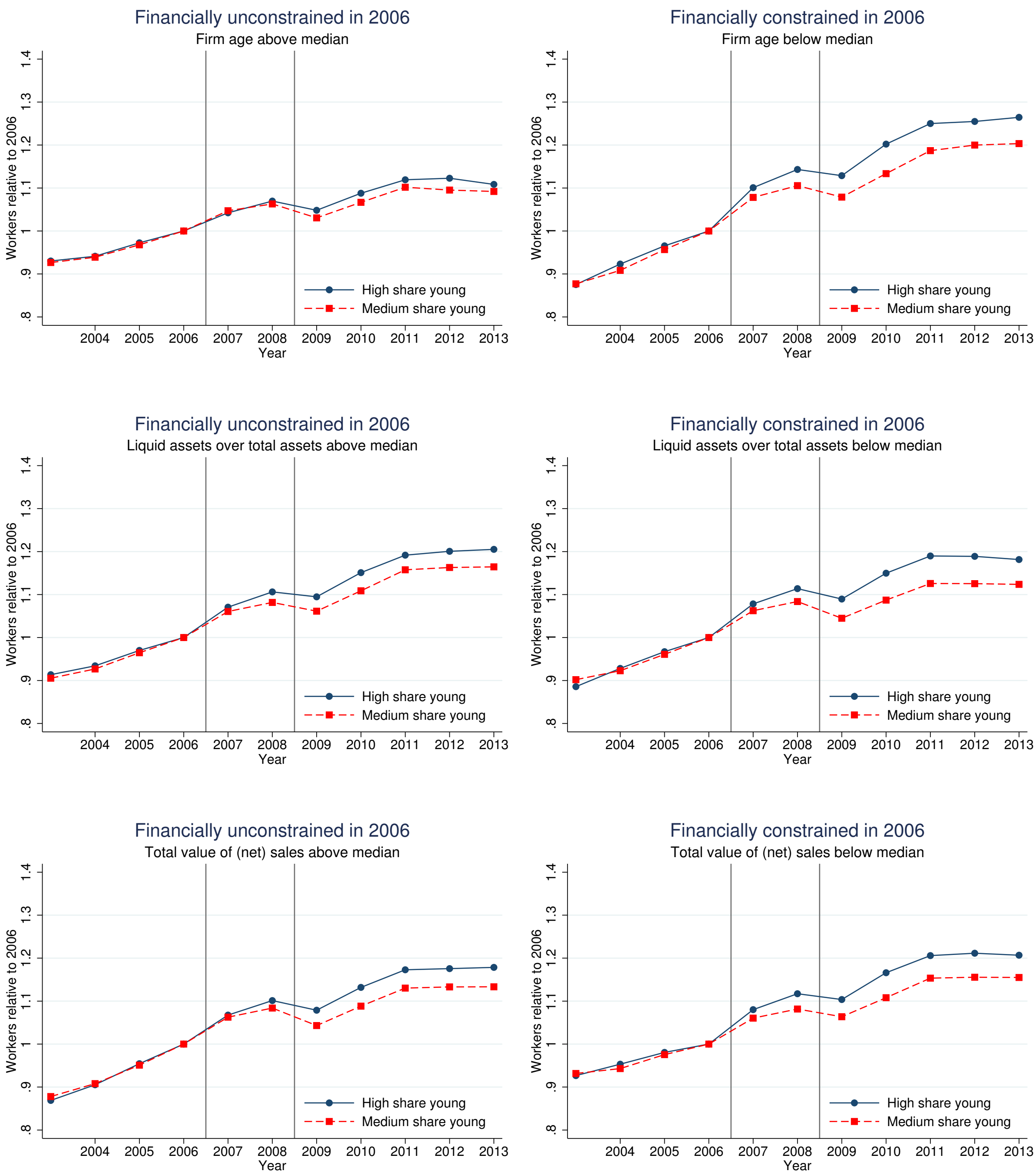


(h) Individual earnings (2003)



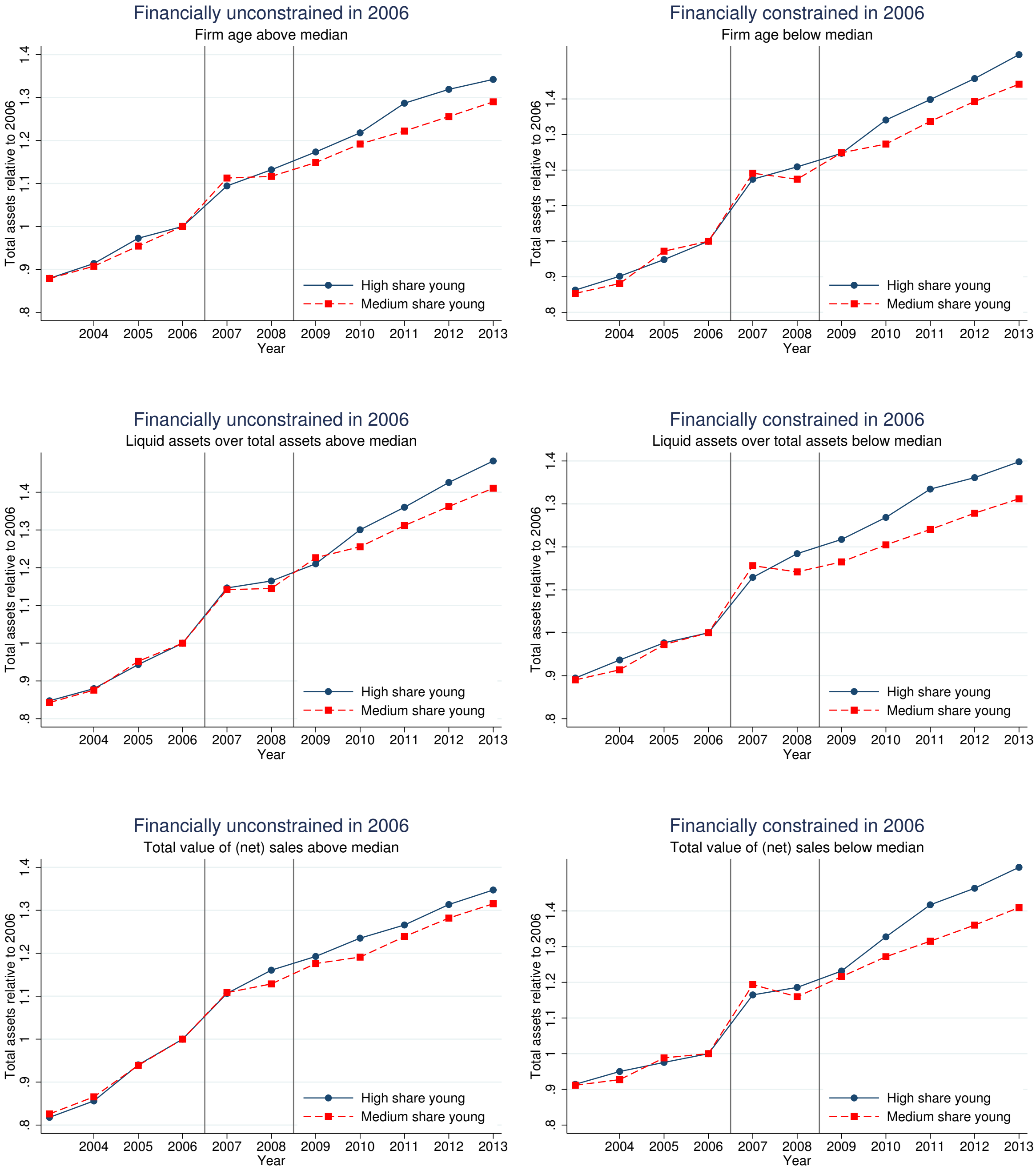
Notes: The figure proposes a robustness check by comparing firms' outcomes when defining the treatment and control groups based on year 2003 instead of year 2006. The left-hand-side panels consider the 2006 base year as in the main text while the right-hand-side panels consider the 2003 base year. In both cases, we still consider the balanced panel of firms operating in all years 2003-2013 as in the main text. For the left (right) side panels, the treatment group is defined as firms in the top quartile of share young in payroll in 2003 (2006) among firms with positive share young; the control group is defined as firms in the second quartile (below the top quartile) of share young in payroll in 2003 (2006). Note that we use here as control group the second quartile instead of the middle two quartiles as in the main text. This is because the bottom of the middle two quartiles does not exhibit parallel trends pre-reform when using the 2003 base year. The side-by-side graphs show that our first stage, and effect on number of workers, sales, and longitudinal individual earnings are robust to changing the base year to 2003. For all outcomes, pre-reform trends are parallel and an effect arises after the reform.

Figure A18: Firm Employment Effects by Credit Constraint Proxies



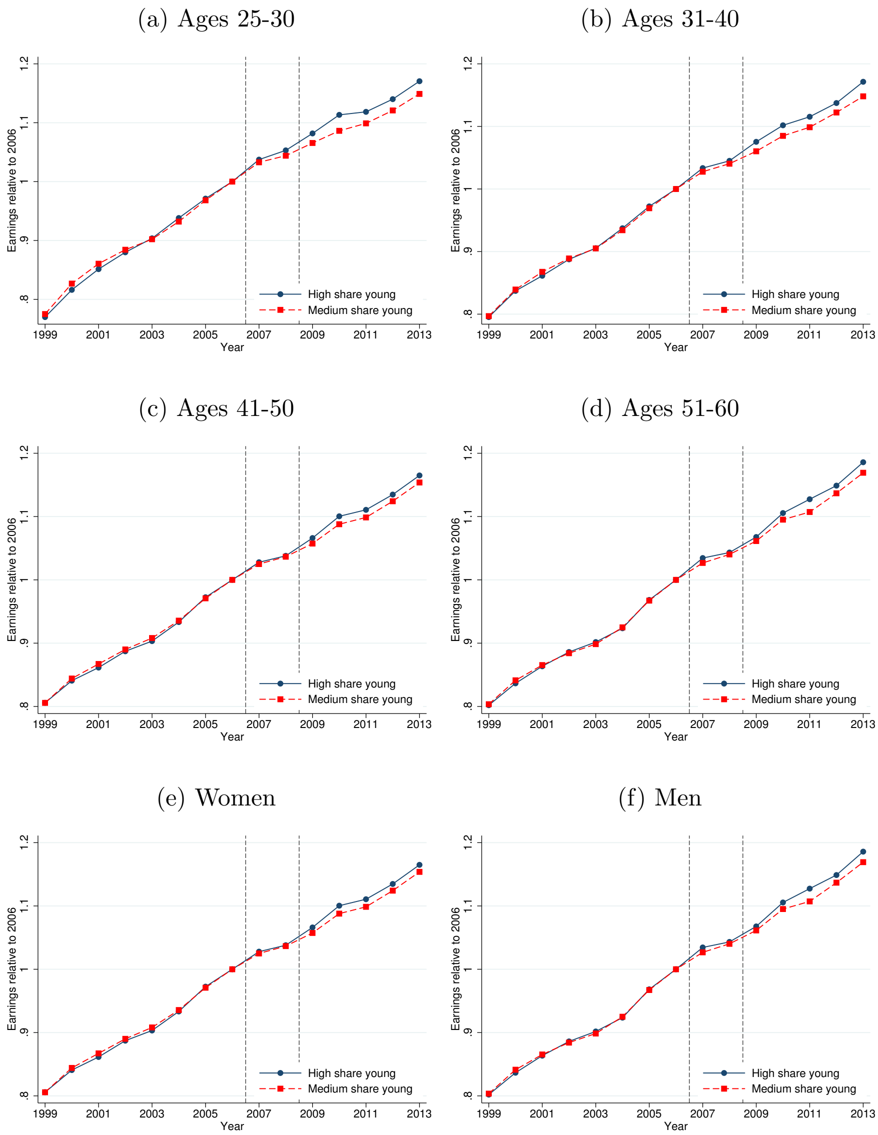
Notes: This figure repeats Figure 6, Panel (a) on the effects on firms of the payroll tax cut on the growth of employment (relative to 2006) but splitting the sample by proxies for credit constraints as of 2006. Each of the three rows considers a specific proxy for credit constraints: (1) age of the firm, (2) liquid assets over total assets, (3) size of firm measured by net sales. In each row, the left panel is for firms with low credit constraints and the right panel for firms with high credit constraint based on the proxy being above or below median (in 2006). In all cases, pre-trends are parallel supporting our identification assumption. Overall, we find employment responses in all types of firms, constrained or not, but responses are larger for firms more likely to be credit constrained based on the proxies (see Table 5 for corresponding estimates).

Figure A19: Firm Asset Growth Effects by Credit Constraint Proxies



Notes: This figure repeats Figure 6, Panel (a) on the effects on firms of the payroll tax cut on the growth of total assets (relative to 2006) but splitting the sample by proxies for credit constraints as of 2006. Each of the three rows considers a specific proxy for credit constraints: (1) age of the firm, (2) liquid assets over total assets, (3) size of firm measured by net sales. In each row, the left panel is for firms with low credit constraints and the right panel for firms with high credit constraint based on the proxy being above or below median. In all cases, pre-trends are parallel supporting our identification assumption. Overall, we find asset growth in all types of firms, constrained or not, but responses are larger for firms more likely to be credit constrained based on the proxies (see Table 5 for corresponding estimates).

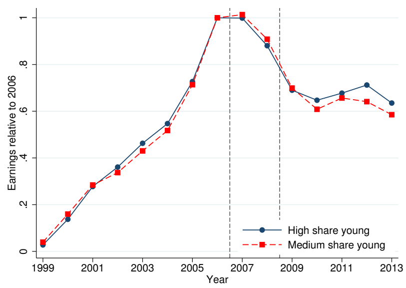
Figure A20: Individual Net Wage Earnings Effects by Age and Gender Groups



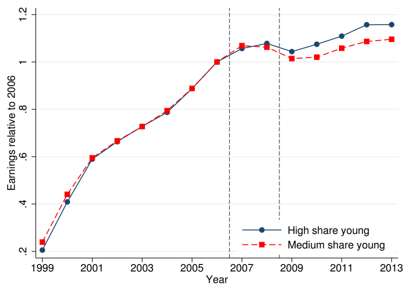
Notes: This figure repeats the average individual earnings longitudinal effects of Figure 8, Panel (a) but further splits the sample into age and gender groups. The top four panels consider the following 4 age groups (measured as of 2006): (a) ages 25-30, (b) ages 31-40, (c) ages 41-50, (d) ages 51-60. The bottom two panels consider the women and men separately (for the age group 41-50 where parallel trends for these gender specific groups are most closely parallel). In each panel, we estimate average individual earnings (relative to 2006) for (1) individuals who worked at a firm with high share young in 2006 and (2) individuals who worked at a firm with medium share young in 2006. The top 4 panels DFL-reweight the age-distribution of the workers in firms with medium-share young to match the age-distribution of those working in firms with a high share young, using 5-year age-categories. The bottom 2 panels DFL-reweight based on 5-year age-groups and 1-digit industry-categories, based on 2006-employment. All panels show that individuals working in a high share young firm in 2006 (which benefitted from a larger tax windfall) experience faster earnings growth on average. The Pre-trends are all parallel. Corresponding estimates are provided in Table 6.

Figure A21: Individual-level Effects on Net Wage Earnings Percentiles

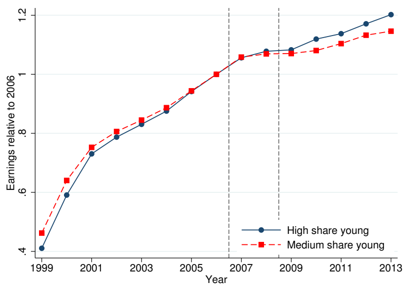
(a) 10th percentile



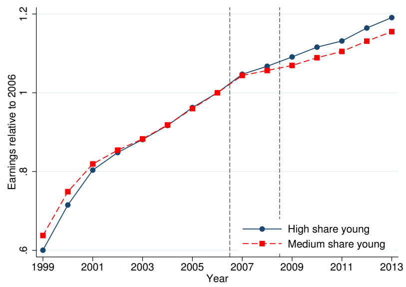
(b) 20th percentile



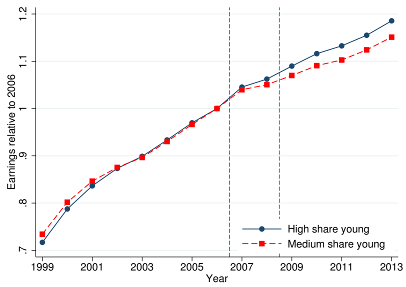
(c) 30th percentile



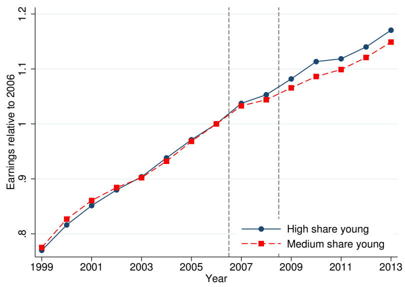
(d) 40th percentile



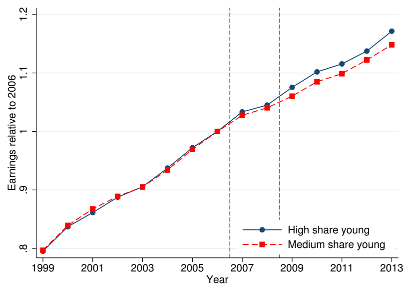
(e) 50th percentile



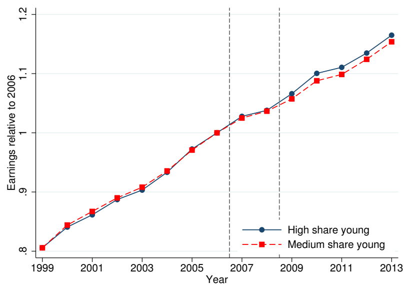
(f) 60th percentile



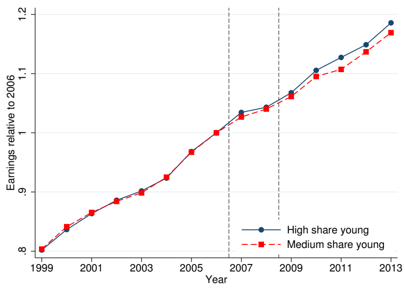
(g) 70th percentile



(h) 80th percentile



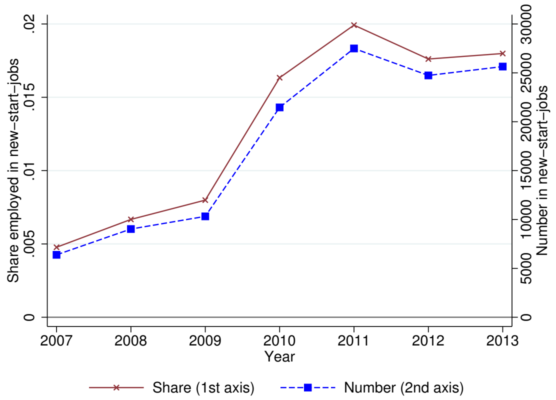
(i) 90th percentile



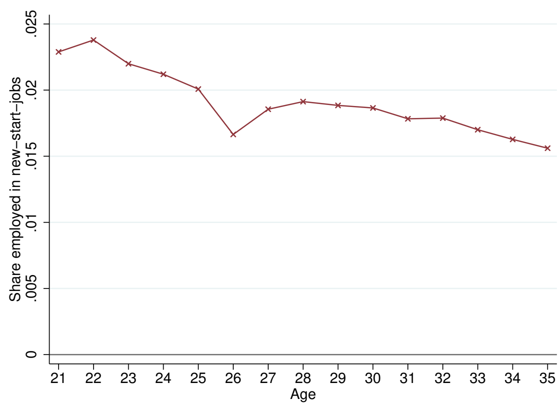
Notes: We repeat the analysis of Figure 8, Panel (a) but instead of considering *average* net wage earnings, we consider various *percentiles* of the net wage earnings distribution among workers based on share young at the firm the individual was working at in 2006. The graphs show that the positive effects on individual earnings of the payroll tax cut are more pronounced at the lower percentiles than at the higher percentiles. This implies that the collective tax incidence rent sharing following the payroll tax cut benefits low earning workers relatively more. Corresponding estimates are provided in Table 7.

Figure A22: Robustness Check: 2007 New Job Starts Hiring Subsidy Program Counfound

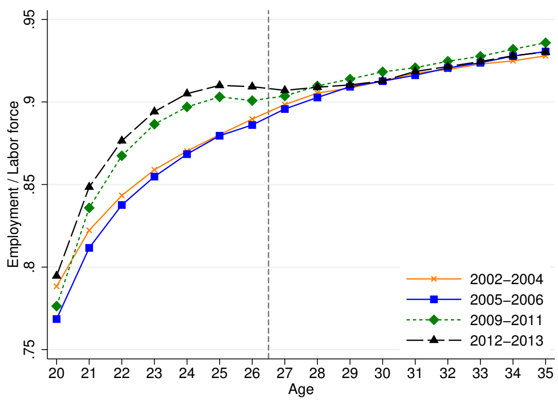
(a) Participation in the program by year



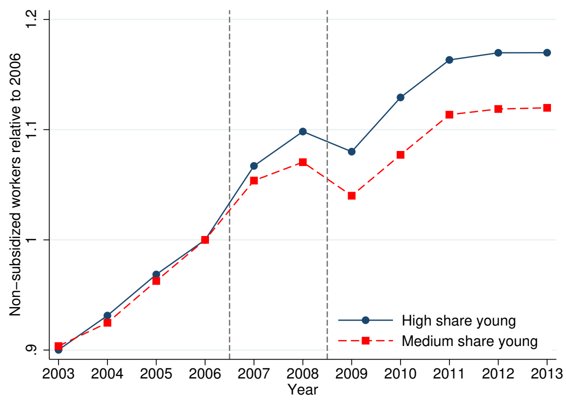
(b) Participation in the program by age (2010-11)



(c) Market-level employment results robustness

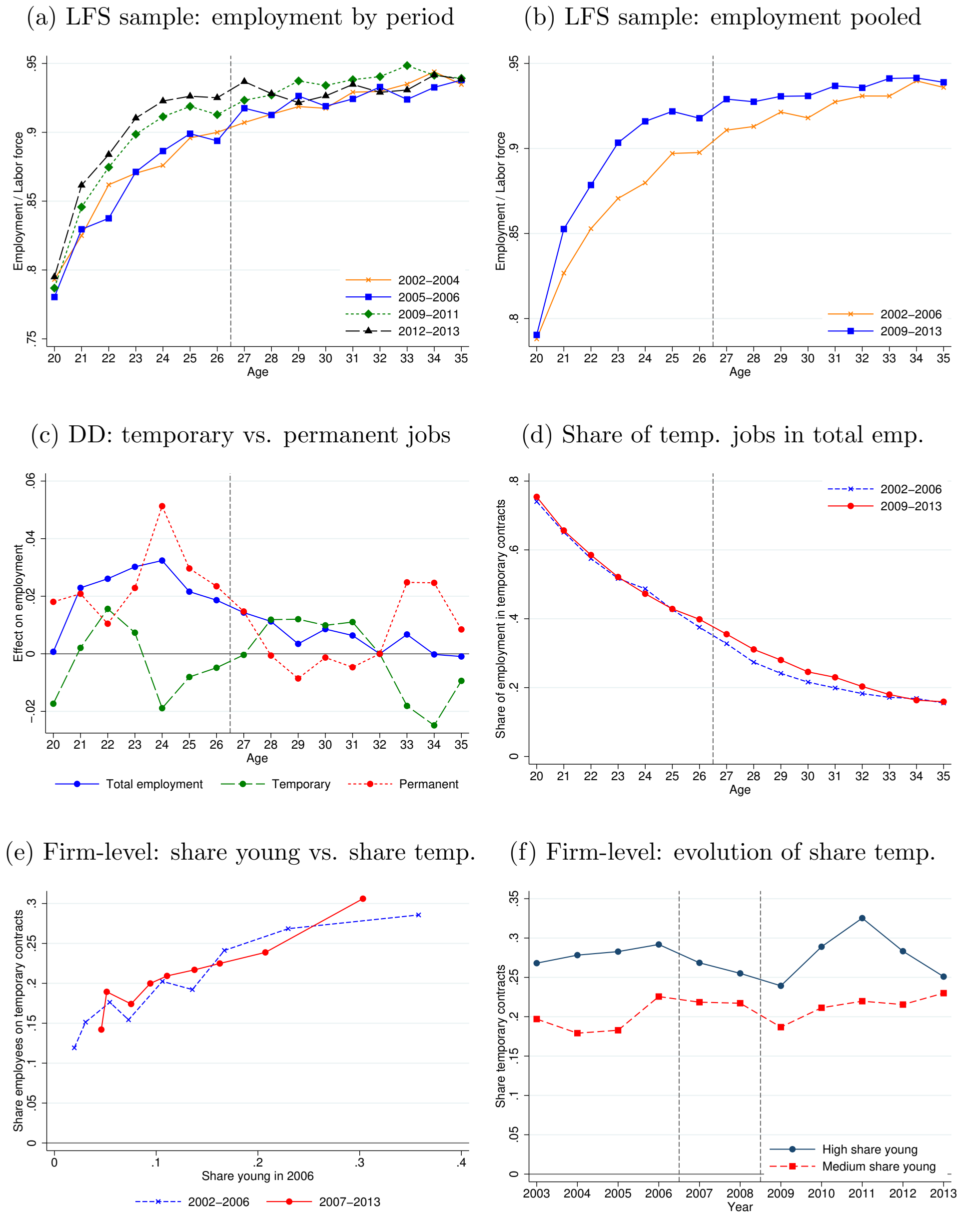


(d) Firm-level employment results robustness



Notes: In this figure, we explore the robustness of our results to excluding workers participating in the New Job Starts program. This program started in 2007 and provided a hiring subsidy eliminating payroll taxes temporarily for certain unemployed job seekers. We discuss the eligibility rules and the results in the online Appendix Section A.3.1. Panel (a) shows the share of employed workers aged 20-35 benefitting from this program (left y-axis) and the absolute number of participants (right y-axis) over time from 2007 to 2013. Panel (b) shows the share of employed workers benefitting from this program in years 2010-11 by age. Panels (c) and (d) replicate Figures 4 and 6(a) but excluding workers benefitting from the hiring subsidy. In panel (c), workers benefitting from the hiring subsidy are excluded from both the employed numerator and the labor force denominator. In panel (d), workers benefitting from the hiring subsidy are excluded from employment counts at the firm level. In both cases, the new figures are virtually undistinguishable from the original figures. Therefore, the New Job Starts hiring subsidy is unlikely to confound our results.

Figure A23: Robustness Check: 2007 Reform to Temporary Contracts Confound



Notes: This figure investigates whether the temporary-contracts reform in 2007 can confound our results. Panels (a)-(b) replicate our baseline market-level employment results using the Labor Force Survey data, which contain information on contract type. In Panel (c), we decompose the employment effects into temporary and permanent contracts. The positive employment effects are driven by permanent jobs not temporary contracts. Panel (d) depicts the share of employed workers in temporary jobs by age before and after the reform. The reform did not expand temporary contracts among the young and only slightly expanded temporary contracts among older workers. This explains why temporary contracts cannot confound our market-level employment results. Panels (e) and (f) present firm-level relationships between share of payroll eligible for the payroll tax cut we study, against share of workers on temporary contracts, and how these shares evolve over time. The share of temporary contracts remains stable from pre-reform to post-reform in both high share young firms and medium share young firms. Hence the temporary contract reform cannot confound our firm-level employment results. Complete details are in the online Appendix Section A.3.1.

Table A1: Short-run Effect of Payroll Tax Cut on Employment Measures

	(1) Effect (ppt)	(2) Elasticity
Employment / Labor Force (LF)	0.025	0.25
	(0.0028)	(0.028)
Employment / (LF+students)	0.035	0.41
	(0.0034)	(0.040)
Employment / Population	0.026	0.45
	(0.0039)	(0.066)
Labor force / Population	0.0077	0.085
	(0.0038)	(0.042)
Unemployment-Employment transitions	0.0040	0.084
	(0.0034)	(0.071)
Employment-Unemployment transitions	-0.011	-1.98
	(0.0015)	(0.27)
N	48	48

Notes: This table repeats the analysis of Table 2 but focusing on short-run effects when the reform was only partially phased in. We compare the 2007-08 period relative to pre-reform periods 2002-2004 and 2005-2006 (the main text table compares periods 2009-2011 and 2012-13 relative to pre-reform periods 2002-2004 and 2005-2006).

Table A2: Effects on Employment across Areas by Level of Initial Youth Unemployment Rate

	(1) Youth unempl. rate	(2) Empl. / LF Benchmark	(3) Pass-through to firms	(4) Youth unempl. rate, placebo	(5) Empl. / LF Placebo
Lowest quintile	0.108	0.010	1.22	0.099	-0.0058
		(0.0033)	(0.095)		(0.0082)
Second quintile	0.124	0.011	1.10	0.115	0.00063
		(0.0018)	(0.10)		(0.0076)
Third quintile	0.148	0.012	1.13	0.143	0.00081
		(0.0032)	(0.091)		(0.0079)
Fourth quintile	0.184	0.029	1.14	0.174	0.00037
		(0.0032)	(0.066)		(0.0086)
Top quintile	0.213	0.034	1.13	0.190	0.0030
		(0.0037)	(0.058)		(0.0084)

Notes: This table presents the effects of the payroll tax cut on employment / labor force by quintiles of local youth unemployment in 2006. We divide the 21 Swedish counties into five quintile groups by size of the youth (age 16-25) unemployment rate in 2006 (pre-reform) and weighting each county by the size of labor force aged 16-25 in 2006. The map of the counties is presented in Figure A11. Youth unemployment rates for each quintile are reported in column (1). Within each quintile, we follow the methodology from Table 2 and run a simple OLS regression of the aggregated time series (2002-2004; 2005-2006; 2009-2011 and 2012-2013) on 16 age dummies (ages 20-35), a post-reform dummy and the interaction of the post-reform dummy and an age eligibility dummy (ages 20-26). The number of observations per row are thus 64. Employment effects expressed in percentage points are reported in column (2). Employment effects are all significantly positive and larger in places with higher youth unemployment. Column (3) presents the estimates of the pass-through of the payroll tax cut to firms following the tax incidence methodology from Table 1. Columns (4) and (5) replicate columns (1) and (2) for a placebo reform in 2003 comparing years 1998-2002 vs 2003-2006. The placebo employment effects are all very small and insignificant.

Table A3: Effect of Payroll Tax Cut on Hiring vs. Separations

	(1) Effect (percentage points)	(2) Elasticity
Unemployment-Employment transitions	0.011	0.23
	(0.0039)	(0.082)
Employment-Unemployment transitions	-0.012	-2.26
	(0.0014)	(0.26)
N	64	64

Notes: This table presents effects of the payroll tax cut on hiring and separations following the model of Table 2. We regress each outcome variable on 16 age dummies (ages 20 to 35), a post-reform dummy, and the interaction of the post-reform dummy and an age eligibility (ages 20-26) dummy. The table shows coefficients on the last regressor. Unemployment-Employment transitions are defined as the share of unemployed in year $t-1$ who become employed in year t and Employment-Unemployment transitions are defined as the share employed in year $t-1$ who enter unemployment in year t.