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

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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]

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[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.

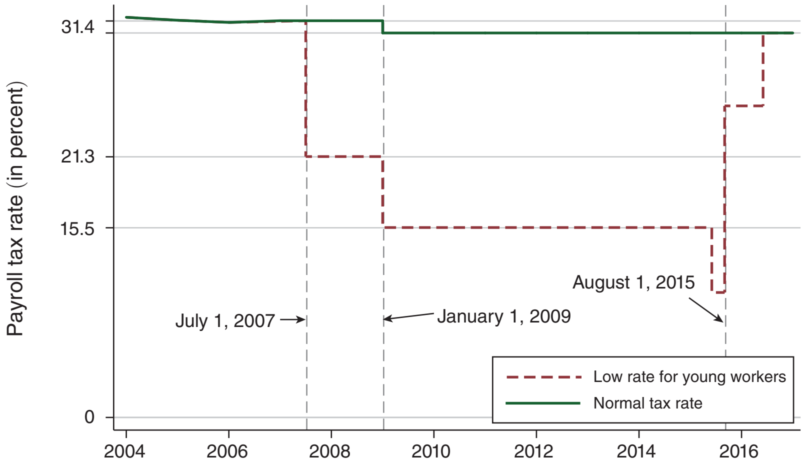

Figure 1. Preferential Payroll Tax Rate for Young Workers

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.

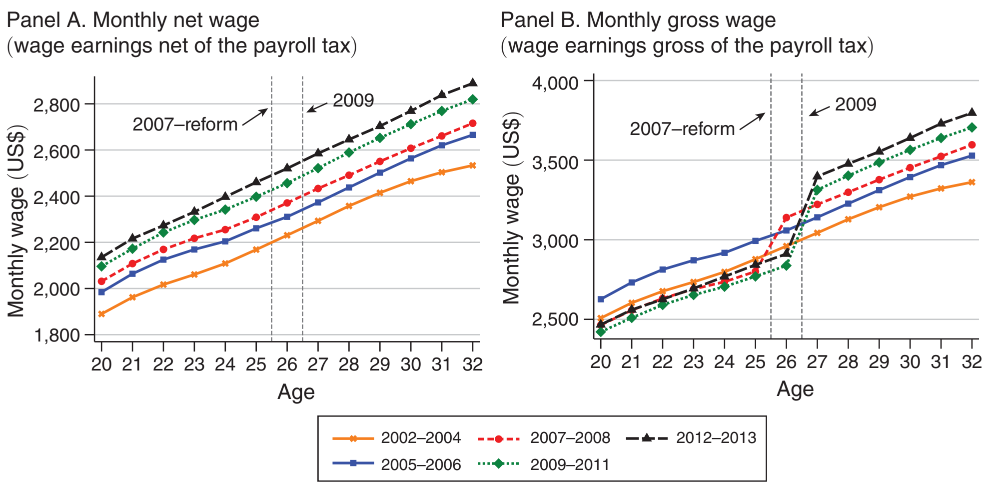

Figure 2. The Effect of the Payroll Tax Cut on Average Wages by Annual Cohorts

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 (1)	Medium run benchmark (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 × 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.

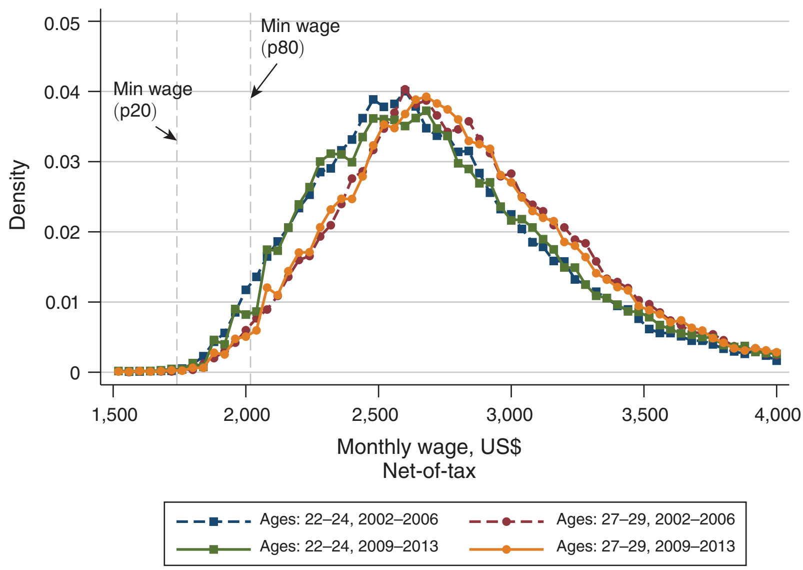

Figure 3. Net Monthly Wage Earnings Densities

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.

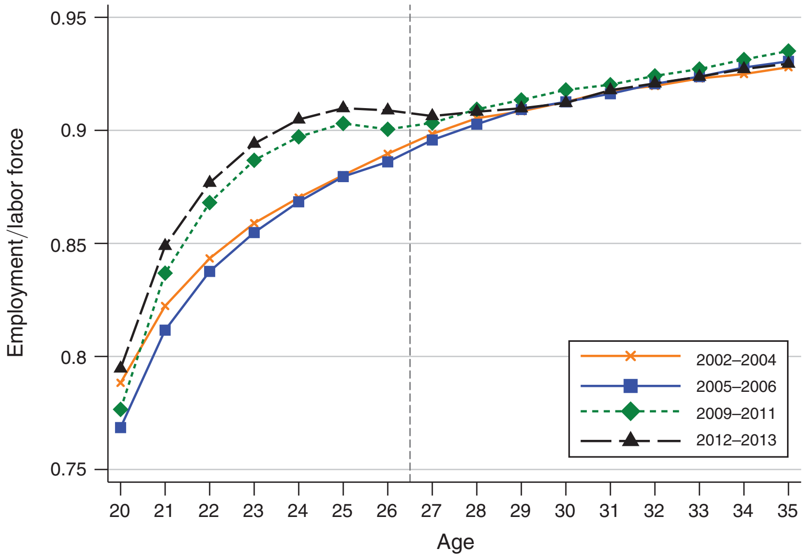

Figure 4. The Effect of the Payroll Tax Cut on Employment

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.

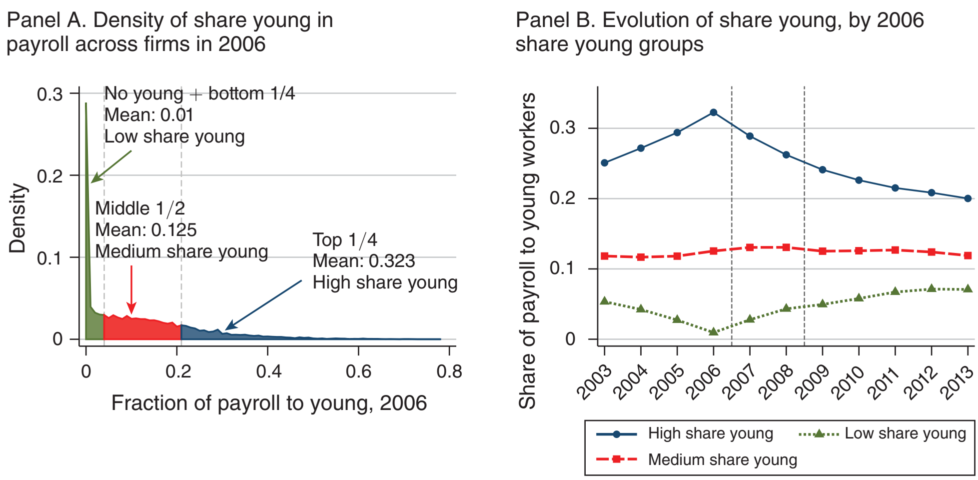

Figure 5. Dividing Firms by Pre-Reform Share Young in Payroll

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

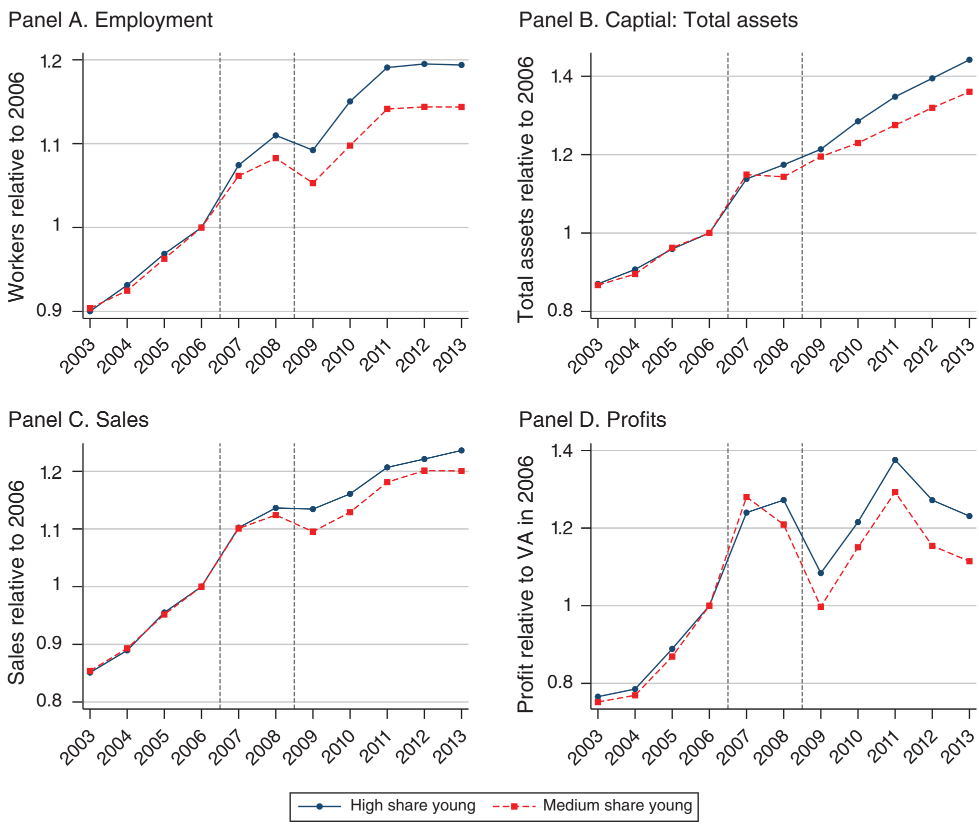

Figure 6. Firm-Level Effects of Payroll Tax Cut

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	FC proxy: Liquid assets/total assets	FC proxy: Sales
	(1)	(2)	(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.

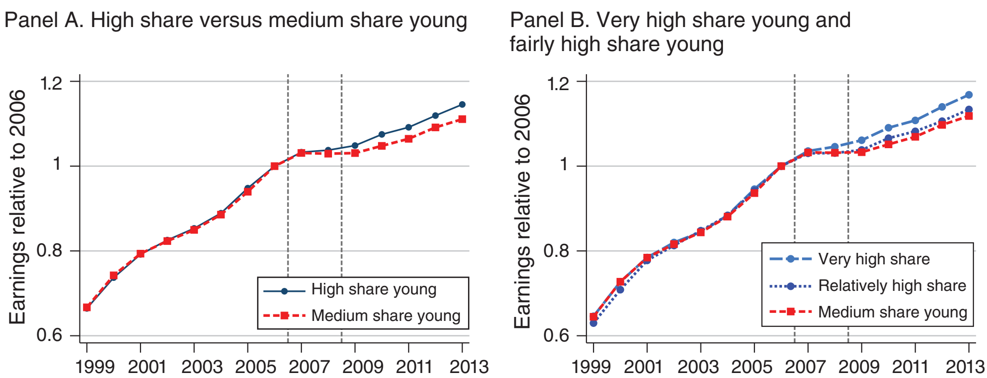

Figure 8. Effects of Payroll Tax Cut on Longitudinal Individual Net Wage Earnings

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.

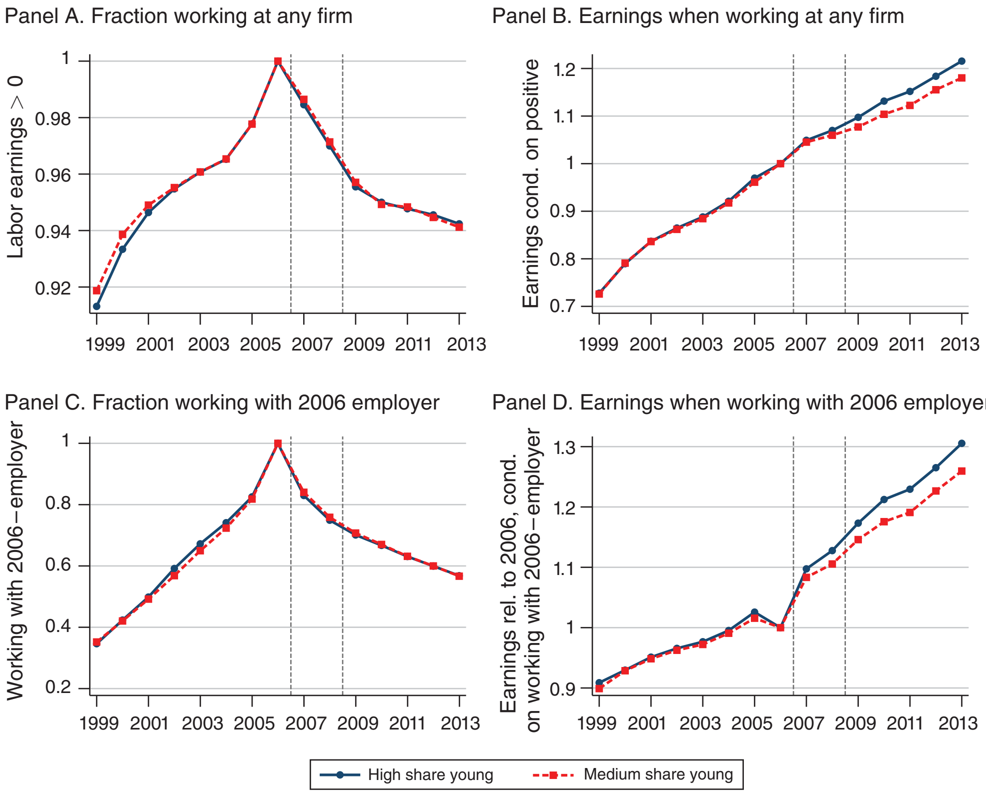

Figure 9. Individual-Level Effects on Employment and Earnings

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.

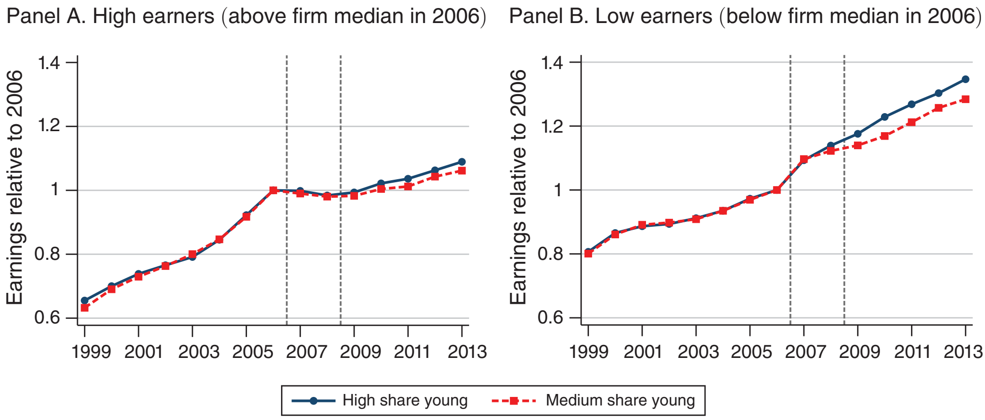

Figure 10. Effects on Longitudinal Individual Net Wage Earnings: High versus Low Earners

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.

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