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

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A Additional Empirical Results

A.1 Market-Level Wage Effects

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

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

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

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

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

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

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

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

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

A.2 Market-Level Employment Effects

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

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

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

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

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

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

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

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

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

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

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

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

A.3 Robustness Check: Other Concurrent Reforms

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

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

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

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

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

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

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

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

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

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

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

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

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

A.3.2 Reform of Temporary Contracts

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

A.4 Firm-Level Survival and Balanced Panel

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

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

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

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

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

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

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

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

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

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

B Benchmarking Implied Cash Effects

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

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

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

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

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

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

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

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

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

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

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

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

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

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

C A Simple Model with Pay Equity Constraints

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

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

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

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

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

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

C.1 Households: Labor Supply

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

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

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

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

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

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

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

C.2 Representative Firms and Labor Demand

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

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

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

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

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

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

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

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

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

C.3 Benchmark: Frictionless Equilibrium – No Equity Constraints

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Labor demand for factor i is:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Concretely, the count of young workers in unemployment is:

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

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

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

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

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

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

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

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

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

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

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

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

tionless equilibrium.[77]

C.5 Two Types of Firms

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

C.5.1 Households Labor Supply

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

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

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

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

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

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

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

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

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

C.5.2 Labor Demand

The production function for a given firm f is:

$$F^f(n_y, n_o) = \left(x^f_y \gamma_y (n^f_y)^\alpha + x^f_o \gamma_o (n^f_o)^\alpha\right)^{\frac{\beta}{\alpha}} \tag{A42}$$

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

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

$$n^{df}_{\ o} = \left(\frac{\beta x^f_o \gamma_o}{(1+\tau_o) w^f_o}\right)^{\frac{1}{1-\beta}} \left[x^f_y \gamma_y \left[\frac{x^f_y}{x^f_o} \frac{\gamma_y}{\gamma_o} \frac{(1+\tau_o) w^f_o}{(1+\tau_y) w^f_y}\right]^{\frac{\alpha}{1-\alpha}} + x^f_o \gamma_o\right]^{(\frac{\beta}{\alpha}-1)(\frac{1}{1-\beta})} \tag{A43}$$

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

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

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

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

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

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

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

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

youth labor x_y^f:

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

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

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

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

For old employment we find the following elasticity:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Additional References

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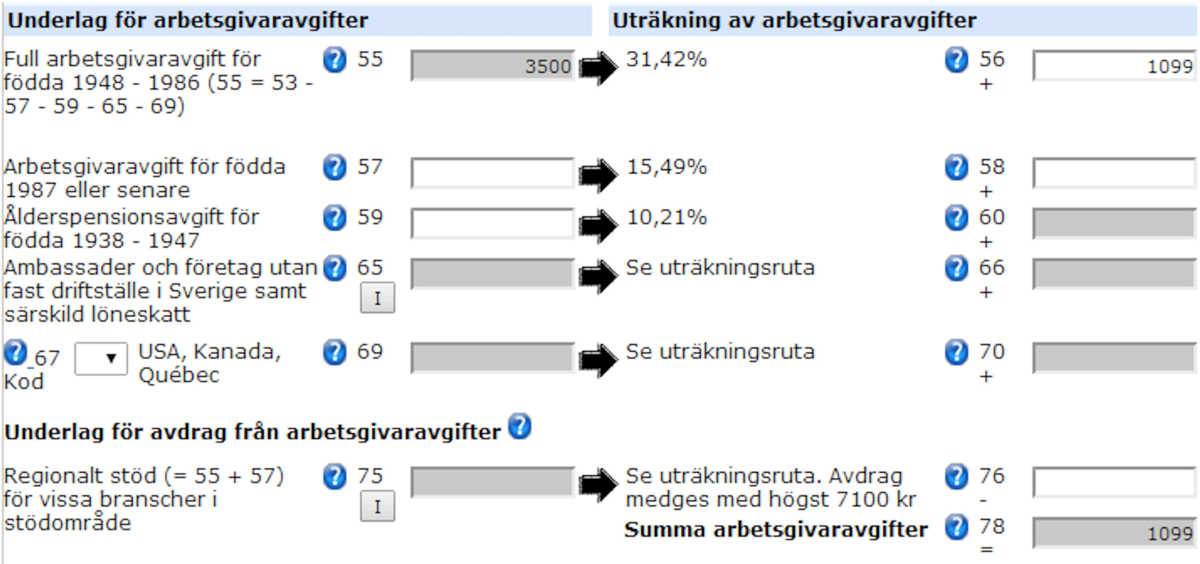
Daruich, Diego, Sabrina Di Addario, and Raffaele Saggio. 2018. "The Effects of Partial Employment Protection Reforms: Evidence from Italy", UC Berkeley Working Paper.

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

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

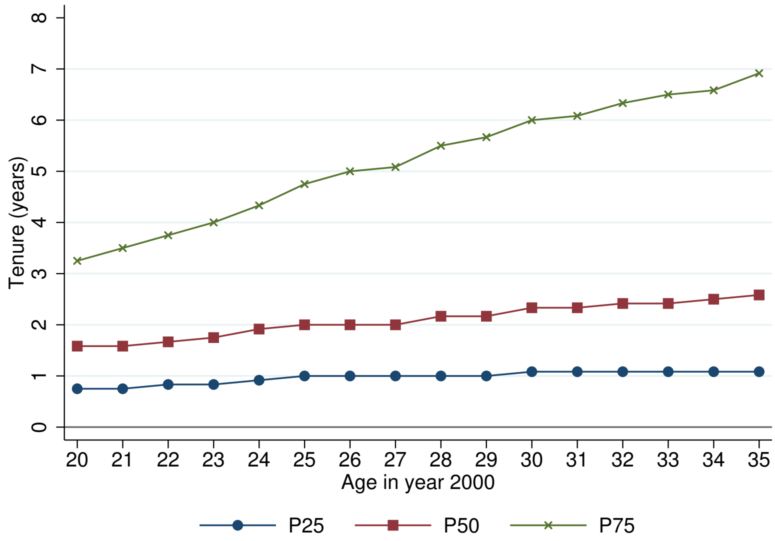
[81]In fact, in Appendix B we provide a quantitative assessment of whether the input and scale effects may be partially or even fully explained by liquidity effects, finding a positive answer. Liquidity effects are not present in the model, which presents a more standard monopsony mechanism (and we do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

Figure A1: Payroll Tax Software for Employers



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

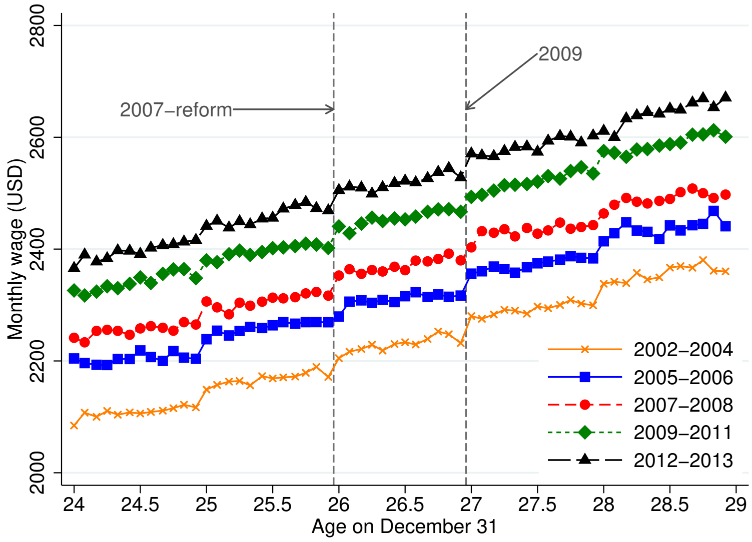
Figure A2: New Hires in 2000: Job Length by Age



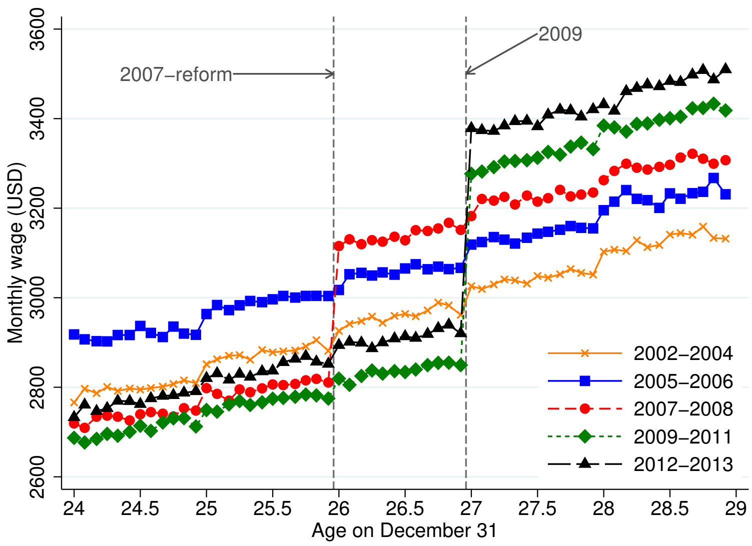
Notes: This figure depicts job length by age of hiring for individuals newly hired in 2000. A newly hired individual is defined as someone who never worked with the firm in the past but starts in 2000. We additionally require that earnings in 2000 from that employer exceed the minimum threshold of \$4,940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

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

(a) Monthly net wage by month of birth



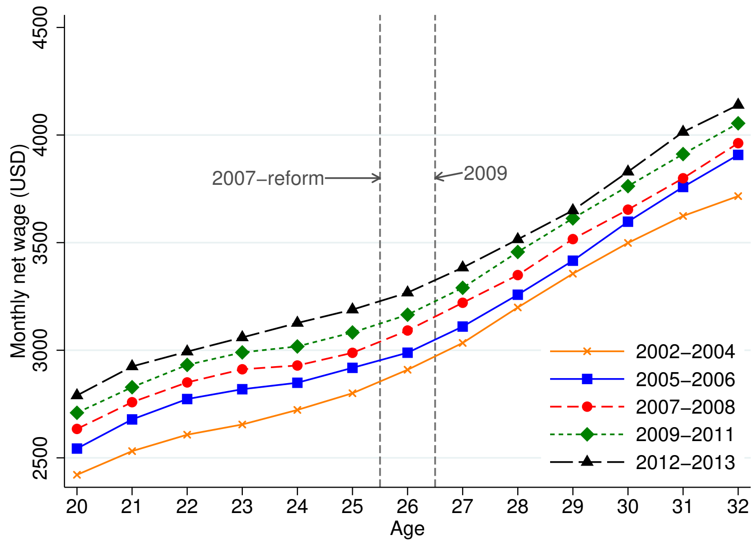
(b) Monthly gross wage by month of birth



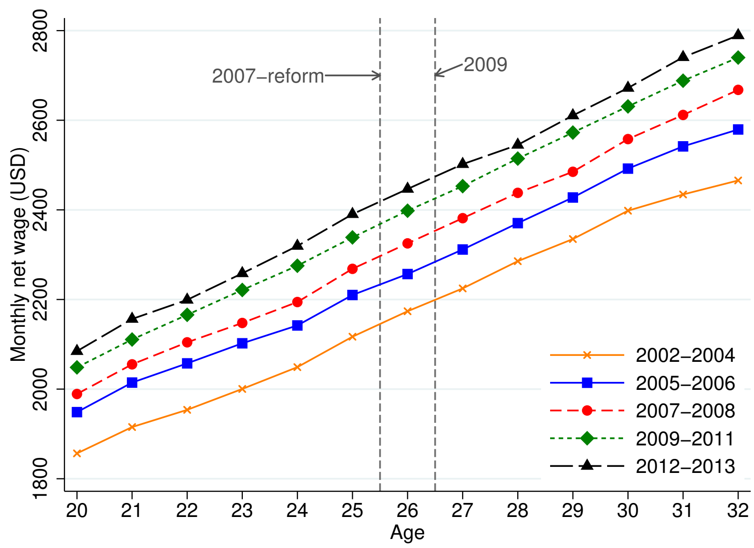
Notes: This figure replicates Figure 2 but zooming in by month of birth instead of year of birth. For comparison with Figure 2, for any given year t when the wage is measured, monthly birth cohorts are translated into monthly age bins as of end of year t. For example, 27 in 2009 means being born in January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

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

(a) Top 20 percent of the wage distribution



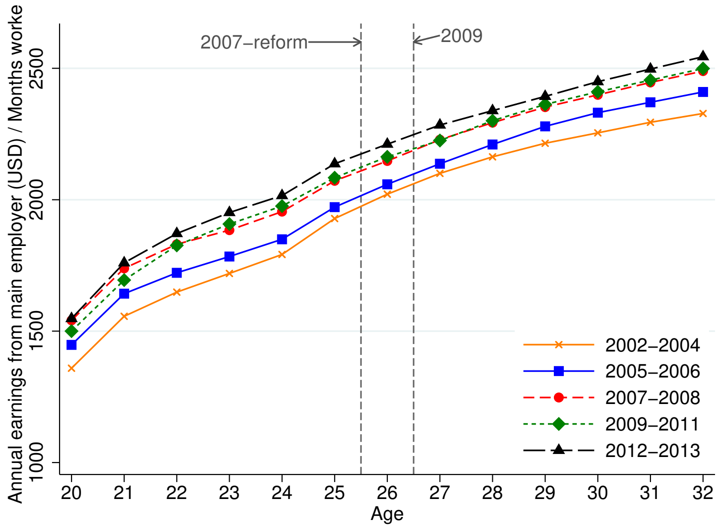
(b) New hires and job switchers



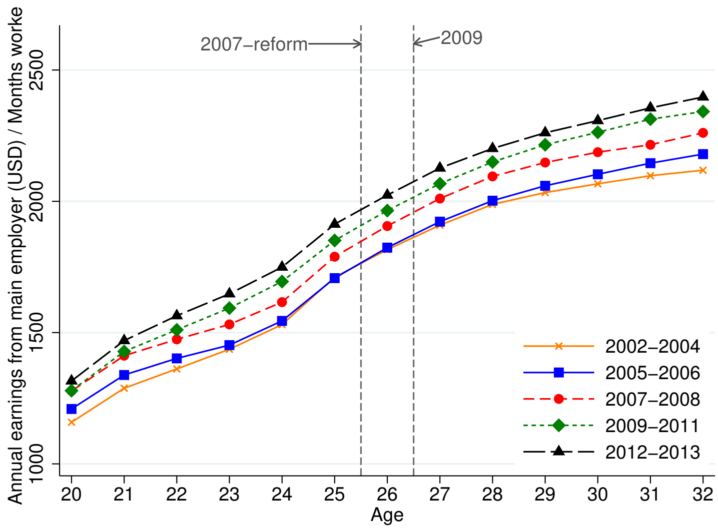
Notes: This figure depicts the average monthly net wage (i.e., exclusive of the payroll tax) in Sweden by age for different time periods using the Structure of Earnings Survey data generally for the month of September (with some measurements in October and November) of each year. We consider two specific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

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

(a) High Turnover Industries: Monthly Net Wages



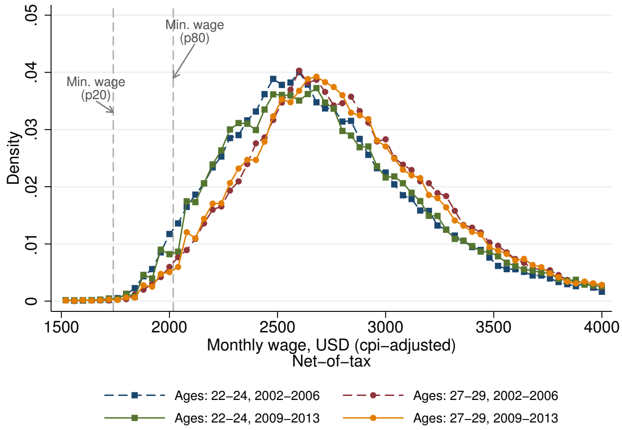
(b) Low Turnover Industries: Monthly Net Wages



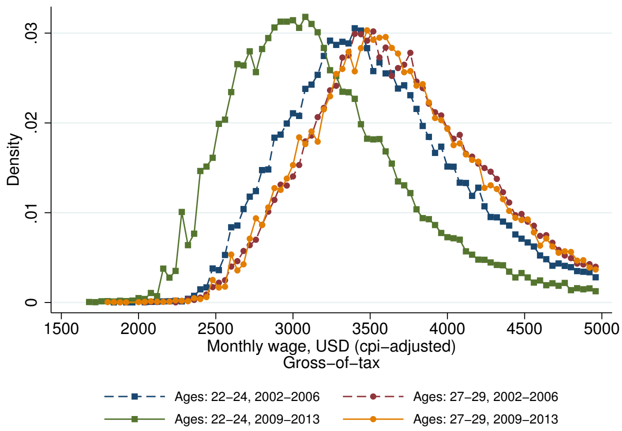
Notes: This figure examines robustness of our market-level wage results by replicating it separately for high and low turnover industries. Our turnover measure is the mean job duration of new job spells in 2000 for workers aged 20-25, within industry for the coarse 10-industry classification. We split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

Figure A6: Net and Gross Monthly Wage Earnings Densities

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



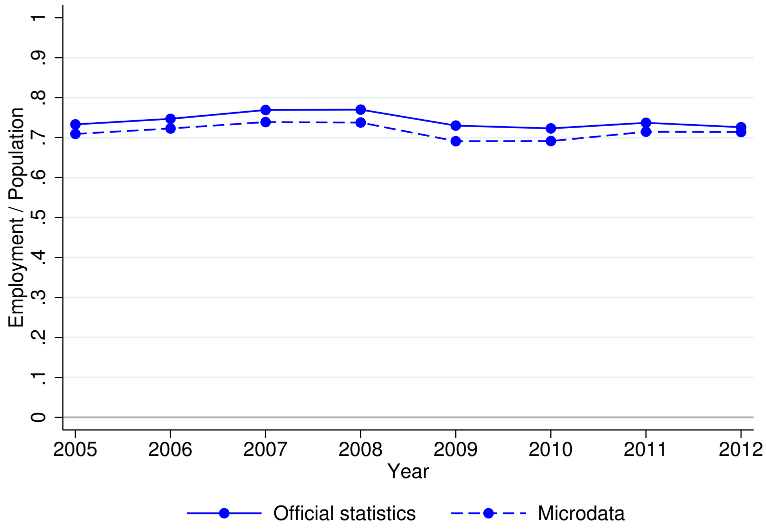
(b) Monthly gross wage earnings (inclusive of payroll tax)



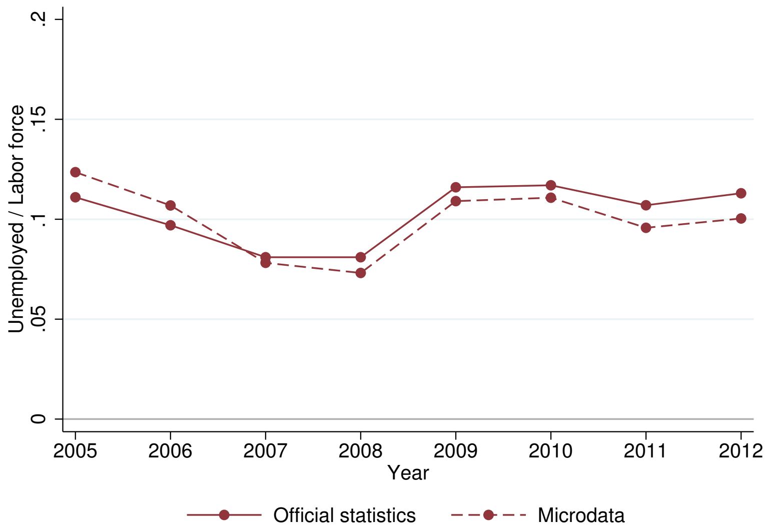
Notes: This figure depicts the monthly wage earnings densities for young workers (aged 22-24) affected by the payroll tax cut in (in squares) and for slightly older workers (aged 27-29) not affected by the payroll tax cut (in circles) both pre-reform (pooling years 2002-2006 in dashed lines) and post-reform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

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

(a) Comparison of the employment rates



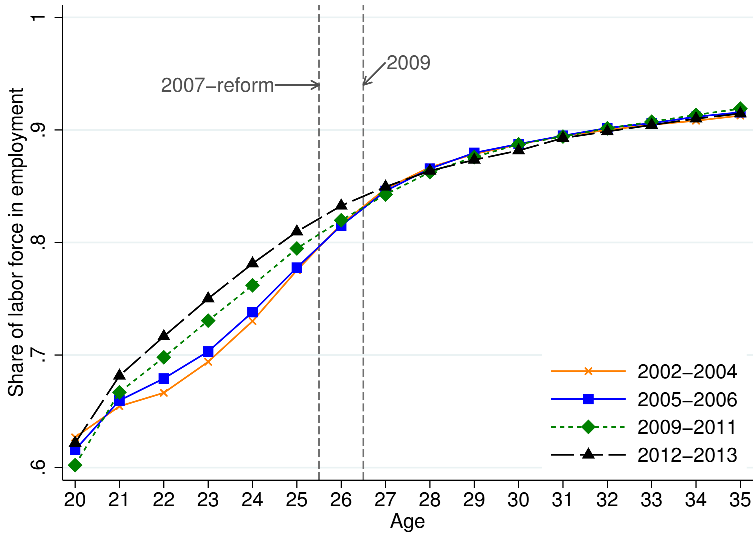
(b) Comparison of the unemployment rates



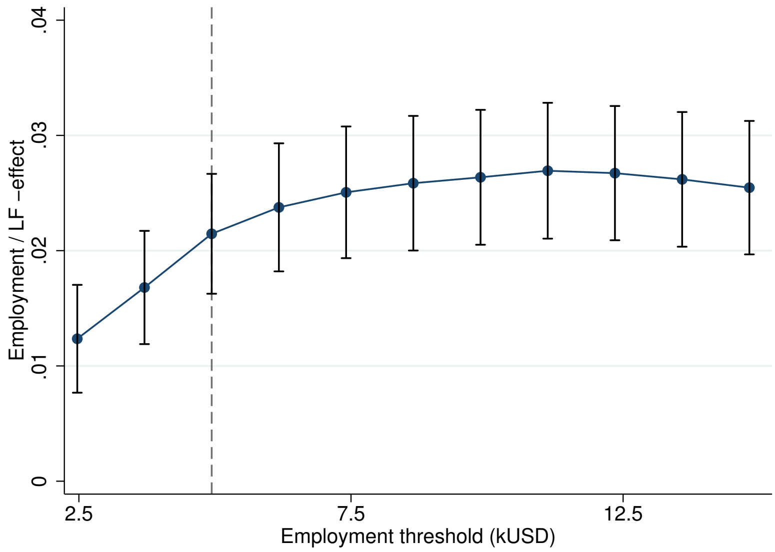
Notes: This figure compares our employment and unemployment measures with official statistics. Our measures are created using administrative full population data while official statistics are created using survey data. The top panel depicts the share of the population aged 20-34 in employment. The bottom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

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

(a) Adding students to labor force

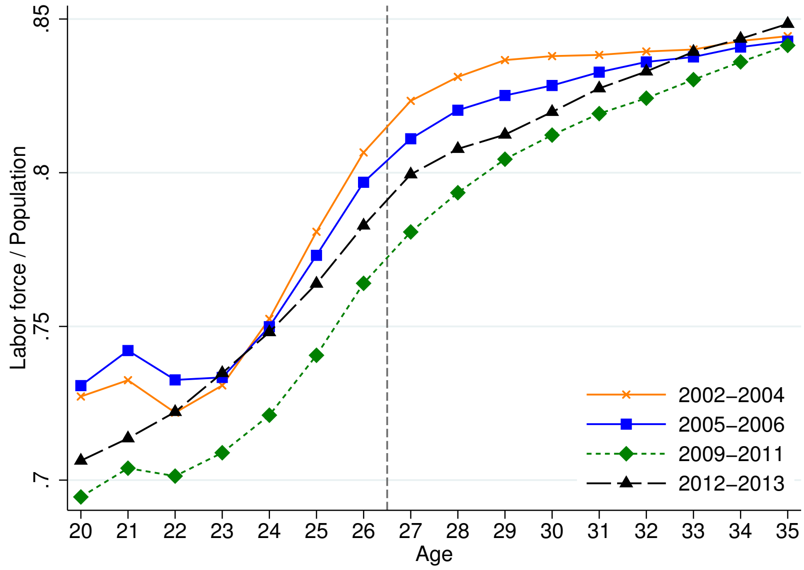


(b) Varying the earnings threshold to define employment



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

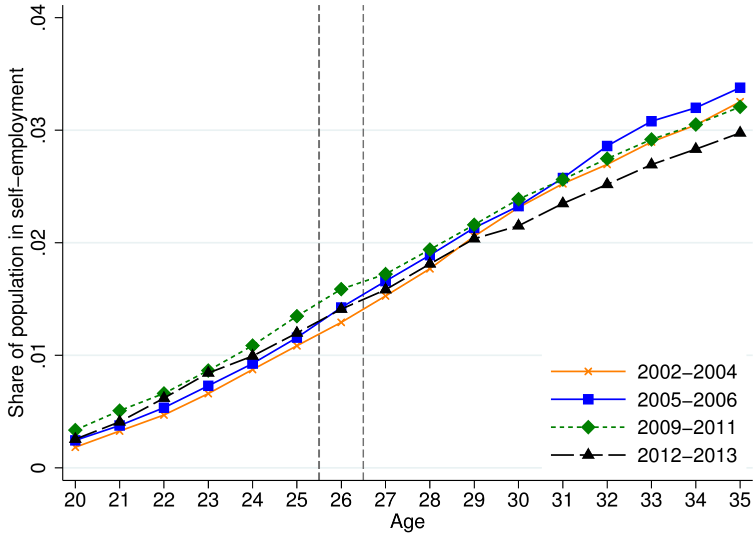
Figure A9: The Effect of the Payroll Tax Cut on Labor Force to Population Ratio



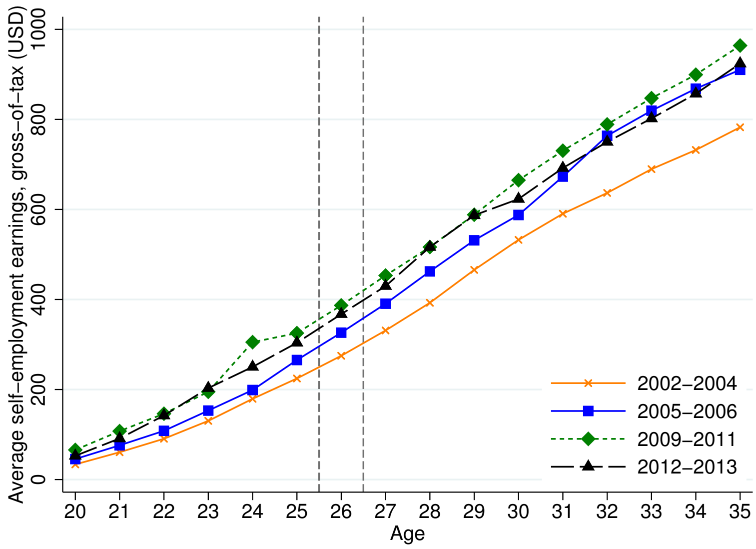
Notes: The figure shows the labor force to population ratio by age and time periods. The labor force numerator is defined as in Figure 4 as all residents who are either (i) employed with annual wage earnings above a small annual threshold; (ii) unemployed defined as having registered with the Unemployment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

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

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

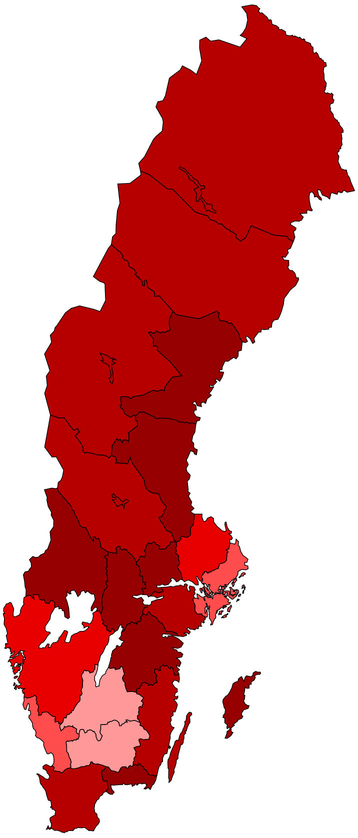


(b) Average self-employment earnings gross of tax (and including zeros)



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

Figure A11: Youth Unemployment Rate by Region 2006



Quintiles of Youth Unemployment Rate in 2006

Q5: Darkest $20 - 23.3 percent$

Q4: Dark $17.8 - 20 percent$

Q3: Medium $14.9 - 17.8 percent$

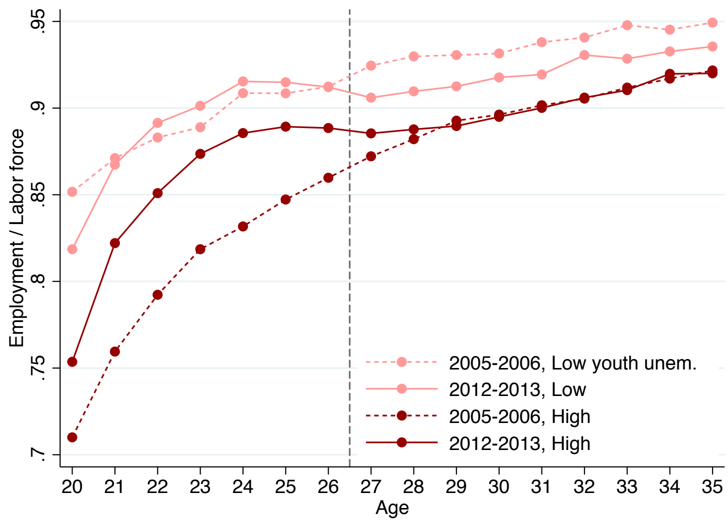
Q2: Light $12.4 - 14.9 percent$

Q1: Lighest $10.5 - 12.4 percent$

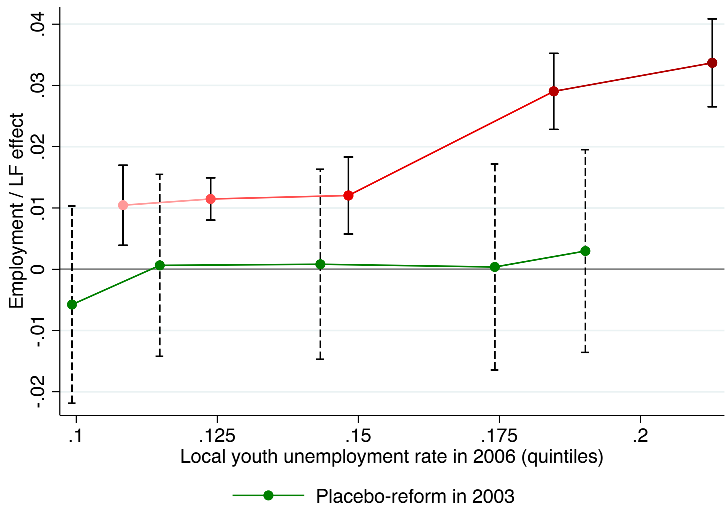
Notes: This figure depicts the heterogeneity in youth unemployment rate in 2006 across Swedish regions. Youth unemployment rate is defined as the unemployment rate (unemployed to labor force) among individuals aged 16-25. We follow the same definition as in our analysis on Figure 4, Panel (a). We divide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

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

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



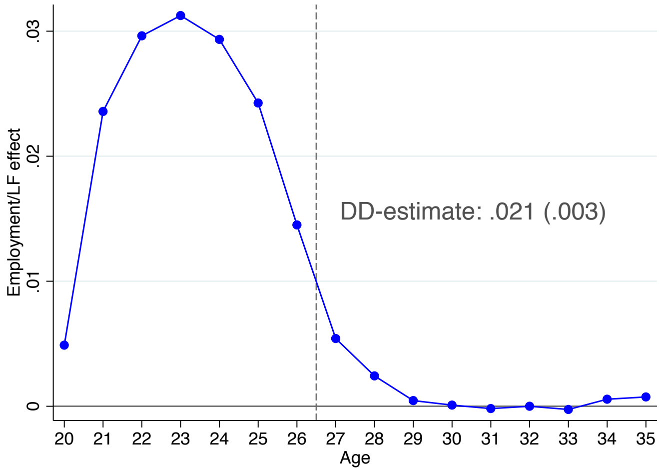
(b) Estimated effects by level of youth unemployment rate



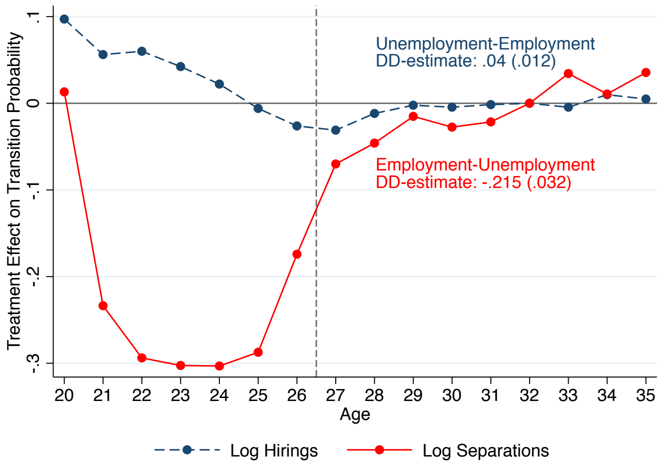
Notes: The top panel shows the share of employees in the labor force by age, pre-reform (2005-2006, in dashed line) and post-reform (2012-2013, in solid line) separately for bottom quintile regions with the lowest 2006 youth unemployment rate (10.5-12.4 percent, in light color) and top quintile regions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

Figure A13: Employment Effects and Hiring vs. Separation Decomposition

(a) Employment effects by age

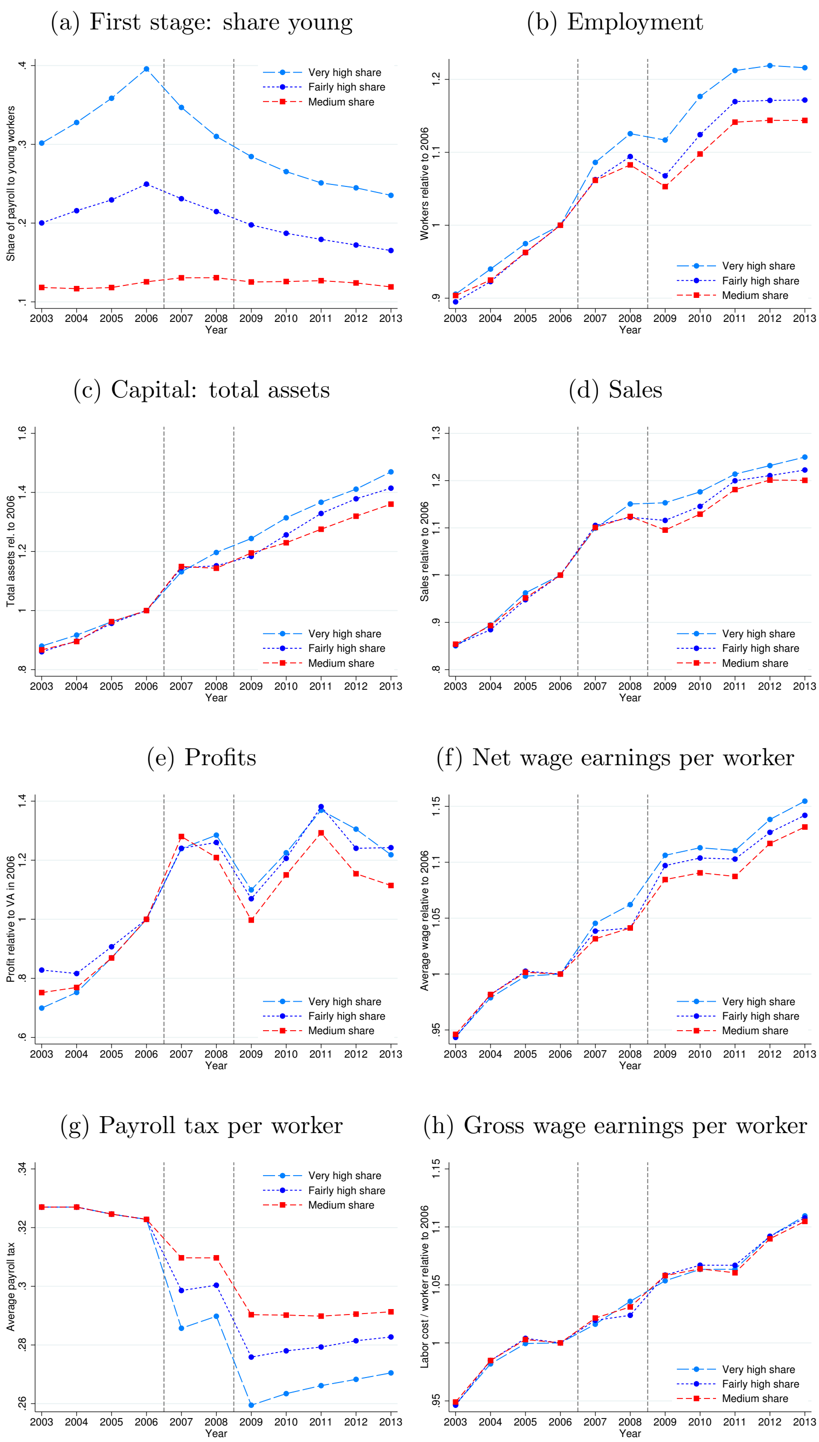


(b) Employment effects by age: hiring vs. separation



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

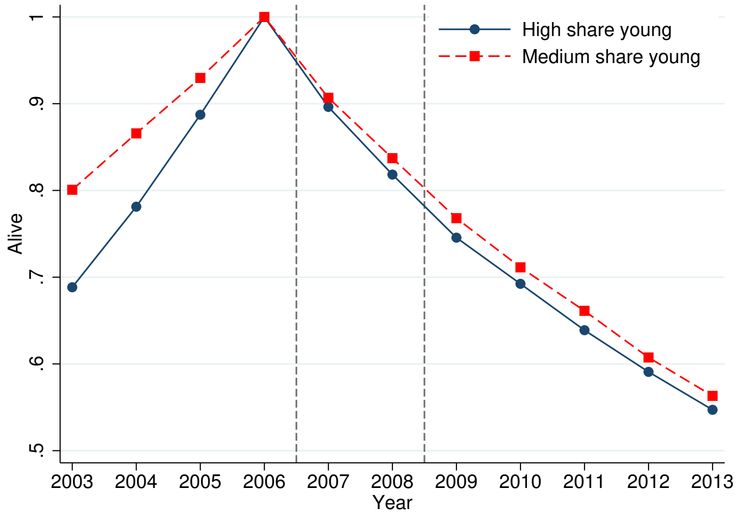
Figure A14: Firm-level Effects: Very High and Fairly High vs. Medium Share Young



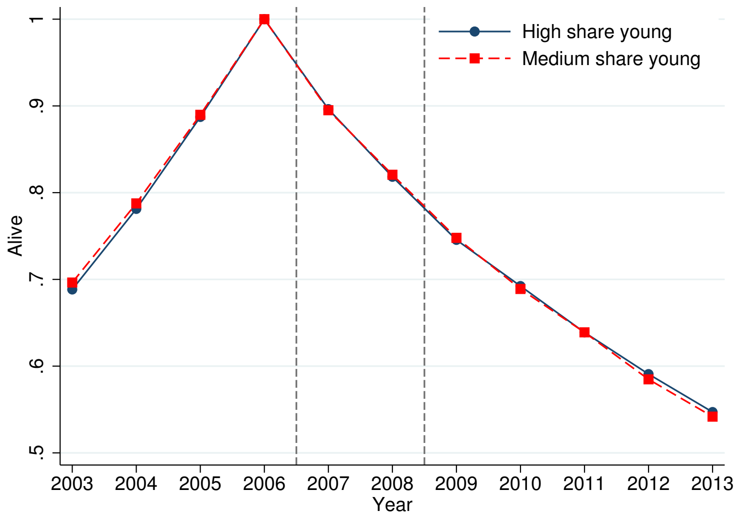
Notes: This figure reproduces the first stage Figure 5, the firm-level effects on business growth of Figure 6, and net and gross wages per worker of Figure 7 but further splitting the high share young group into 2 equally sized groups: (a) very high share young (firms in the top 1/8 of share young in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

Figure A15: Effect of Payroll Tax Cut on Firm Survival

(a) Fraction of firms operating (unweighted)

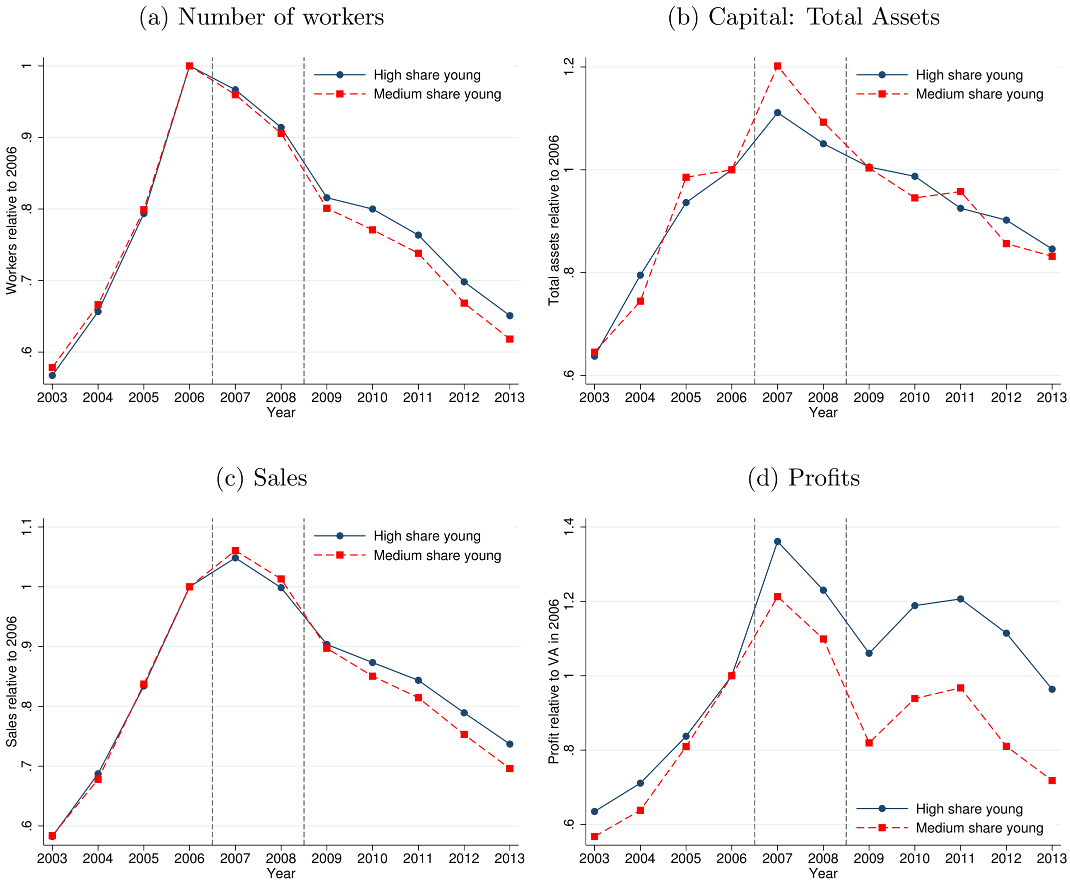


(b) Fraction operating after DFL reweighting by age in 2006



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

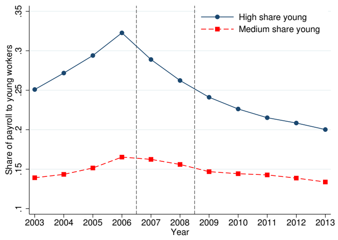
Figure A16: Firm-level Effects: Robustness to Including non operating Firms



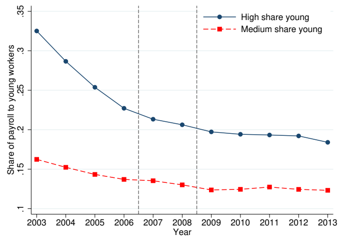
Notes: The figure proposes a robustness check of Figures 6 by including non operating firms instead of considering a balanced panel of firms operating in all years 2003-2013 as in the main text. In this graph, we consider all firms present in 2006 and operating with more than 3 workers in 2006, regardless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

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

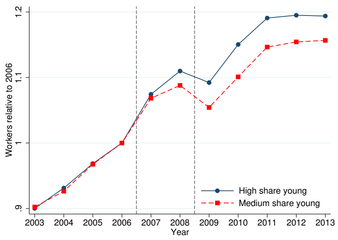
(a) First stage (2006)



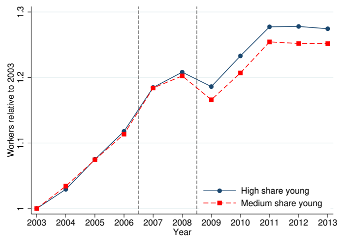
(b) First stage (2003)



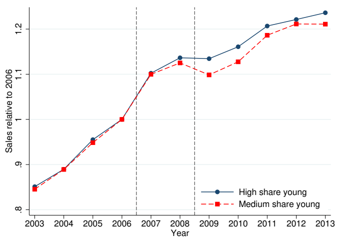
(c) Number of workers (2006)



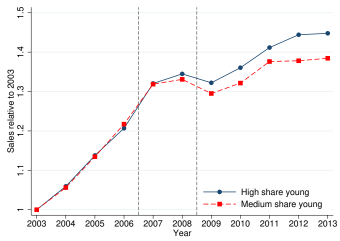
(d) Number of workers (2003)



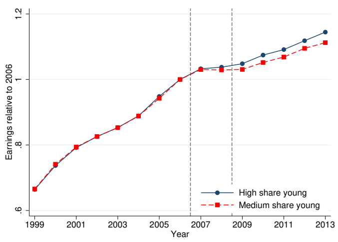
(e) Sales (2006)



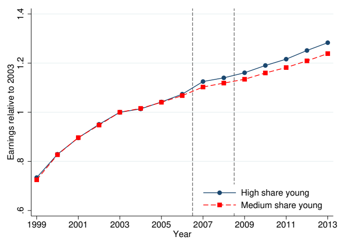
(f) Sales (2003)



(g) Individual earnings (2006)

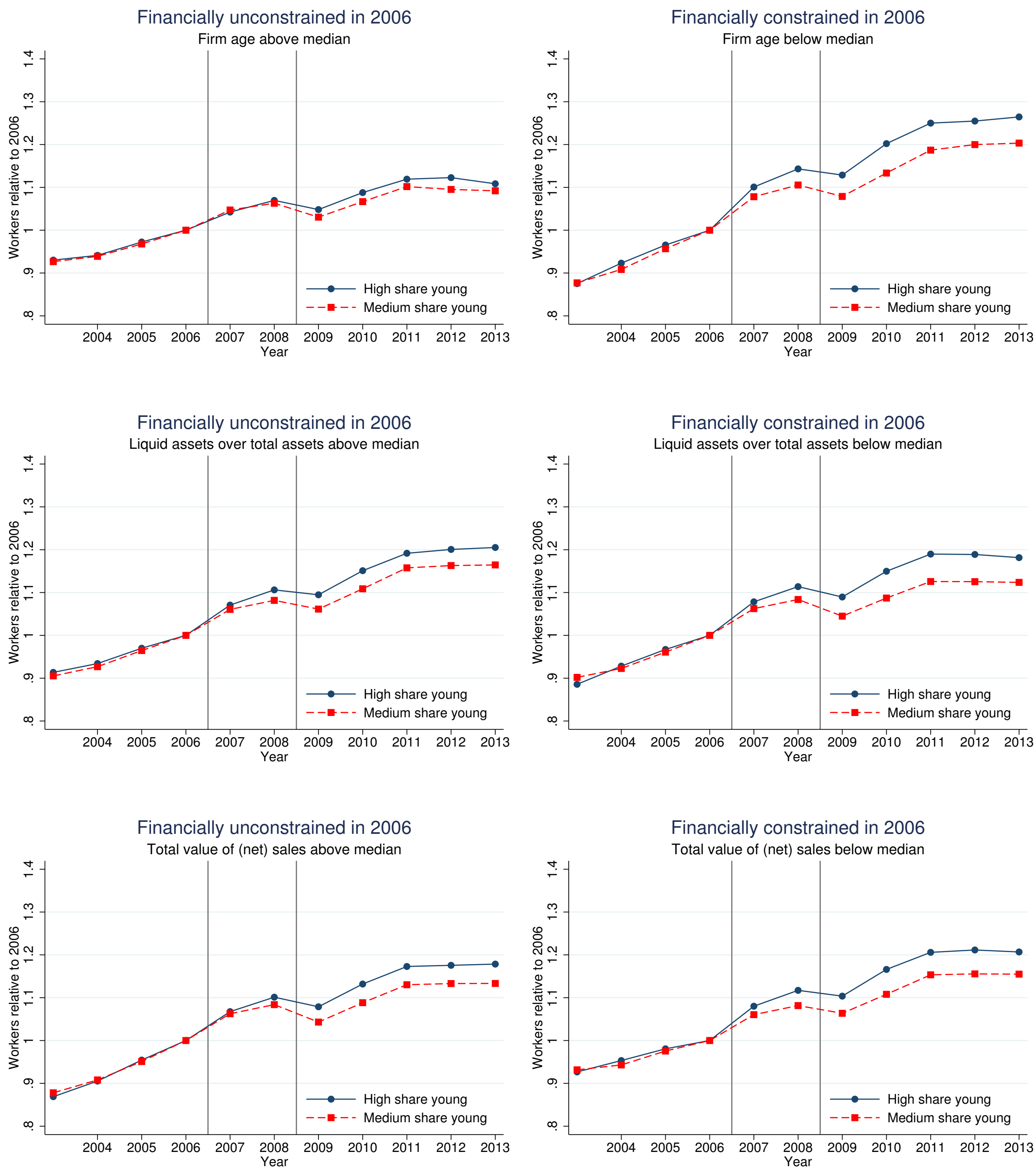


(h) Individual earnings (2003)



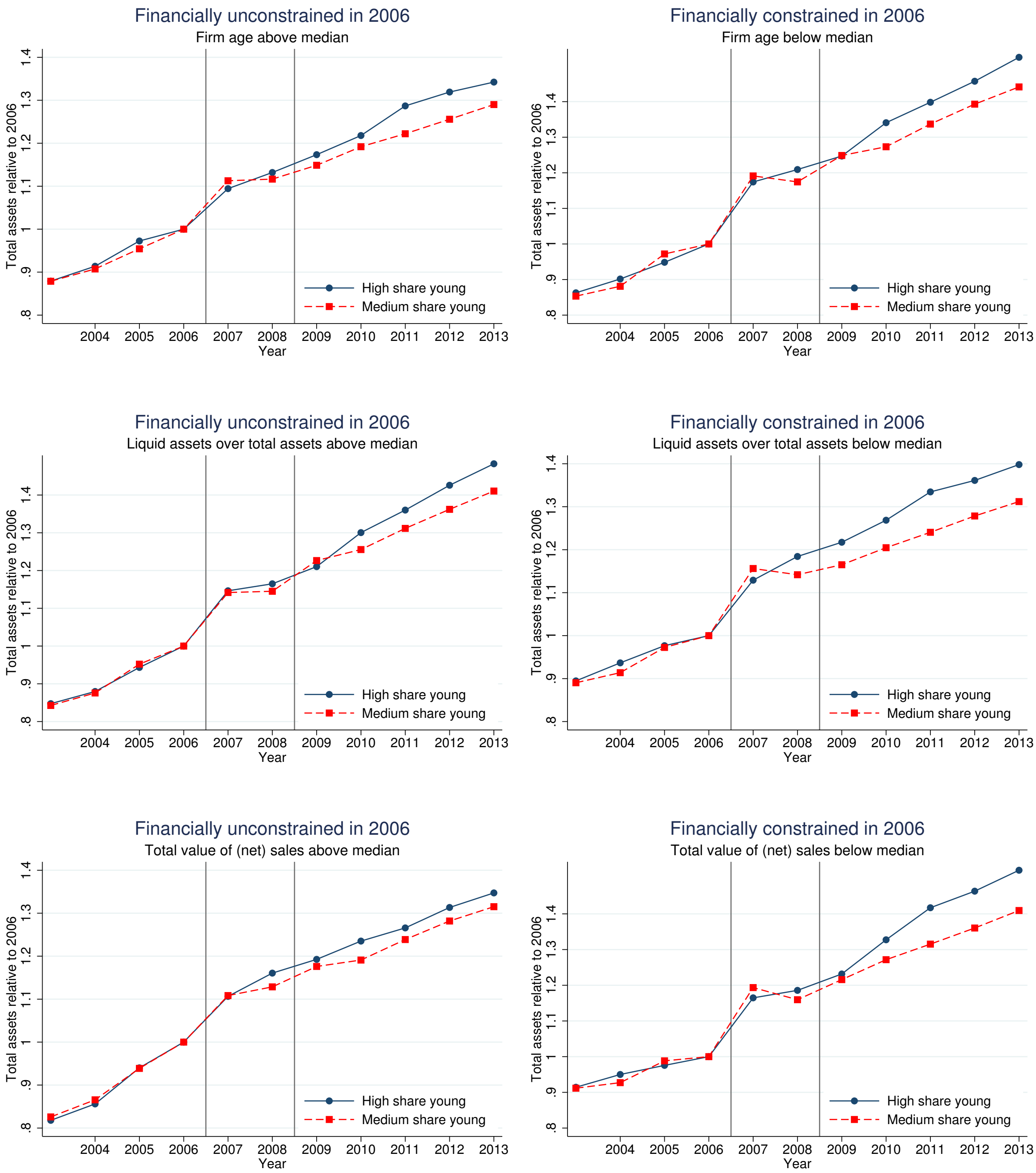
Notes: The figure proposes a robustness check by comparing firms' outcomes when defining the treatment and control groups based on year 2003 instead of year 2006. The left-hand-side panels consider the 2006 base year as in the main text while the right-hand-side panels consider the 2003 base year. In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

Figure A18: Firm Employment Effects by Credit Constraint Proxies



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

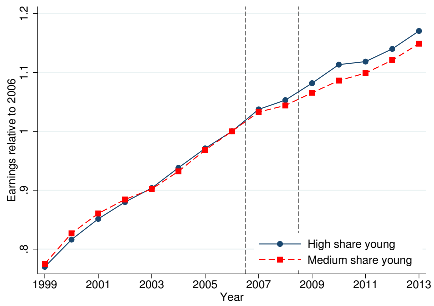
Figure A19: Firm Asset Growth Effects by Credit Constraint Proxies



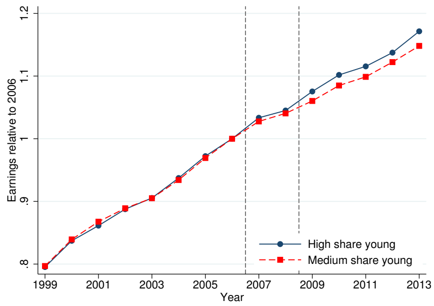
Notes: This figure repeats Figure 6, Panel (a) on the effects on firms of the payroll tax cut on the growth of total assets (relative to 2006) but splitting the sample by proxies for credit constraints as of 2006. Each of the three rows considers a specific proxy for credit constraints: (1) age of the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

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

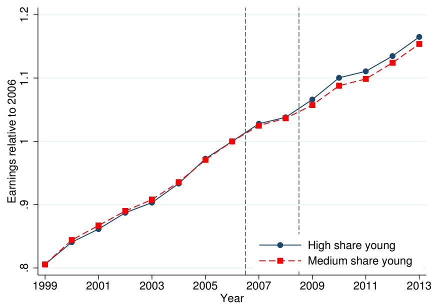
(a) Ages 25-30



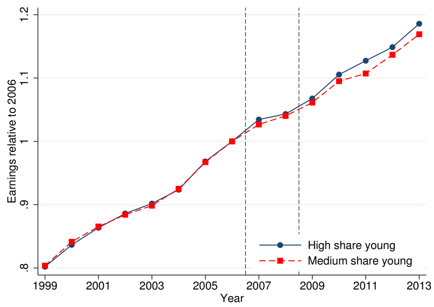
(b) Ages 31-40



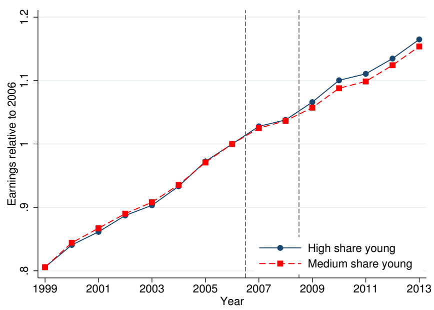
(c) Ages 41-50



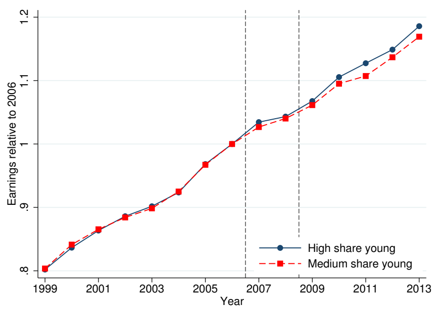
(d) Ages 51-60



(e) Women

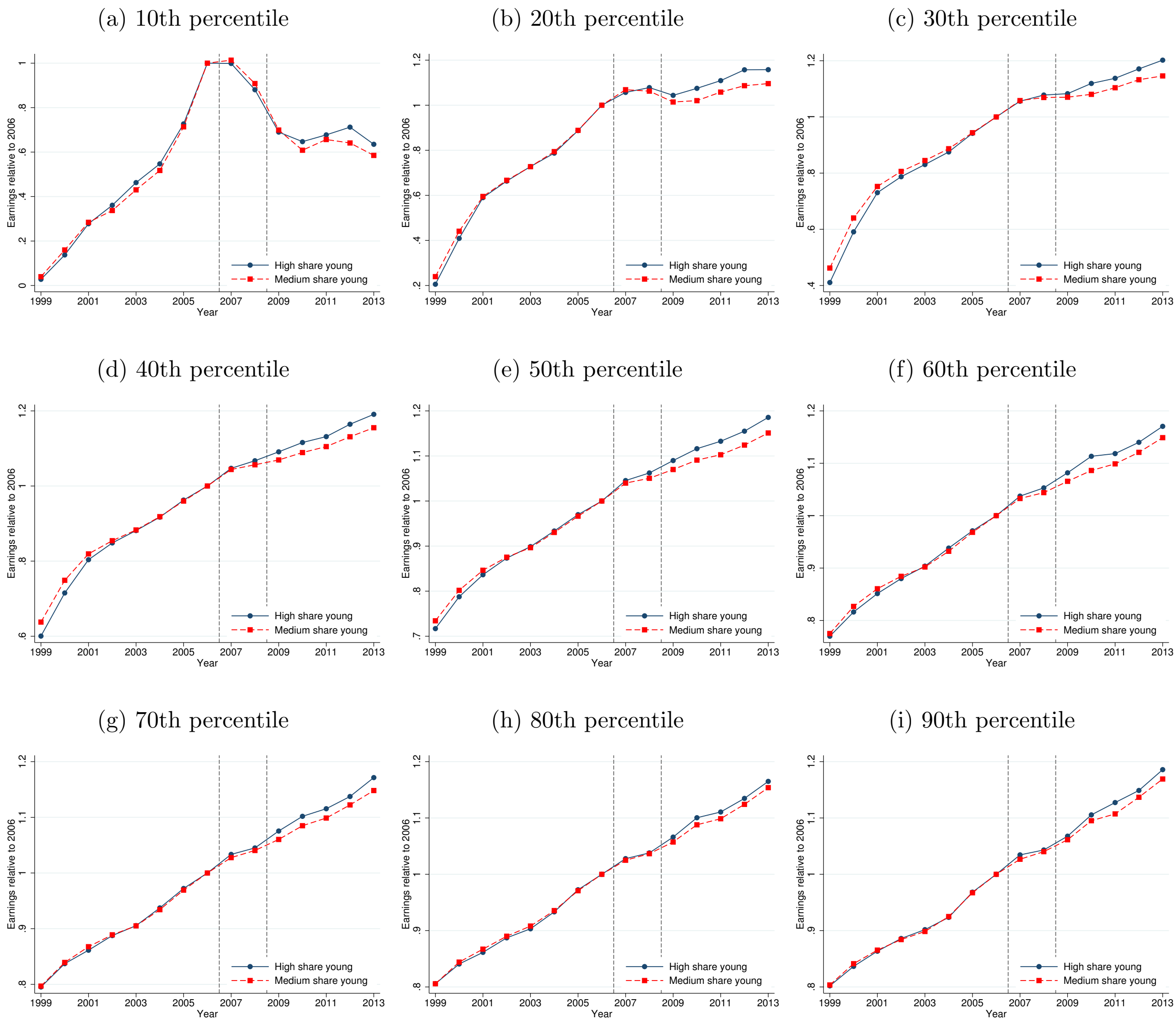


(f) Men



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

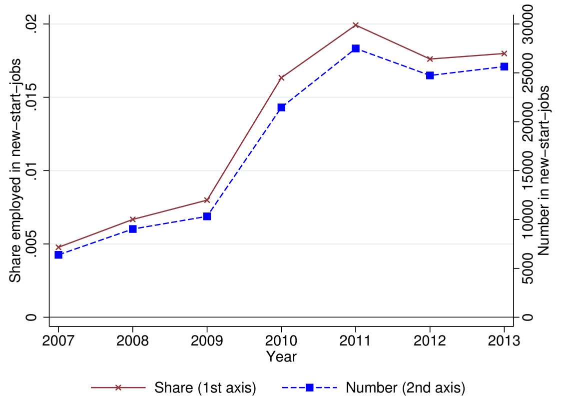
Figure A21: Individual-level Effects on Net Wage Earnings Percentiles



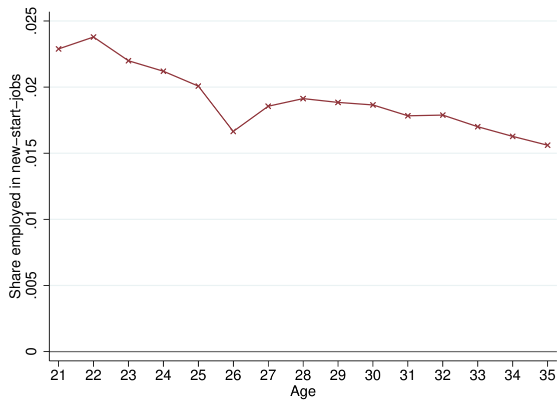
Notes: We repeat the analysis of Figure 8, Panel (a) but instead of considering *average* net wage earnings, we consider various *percentiles* of the net wage earnings distribution among workers based on share young at the firm the individual was working at in 2006. The graphs show that the positive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

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

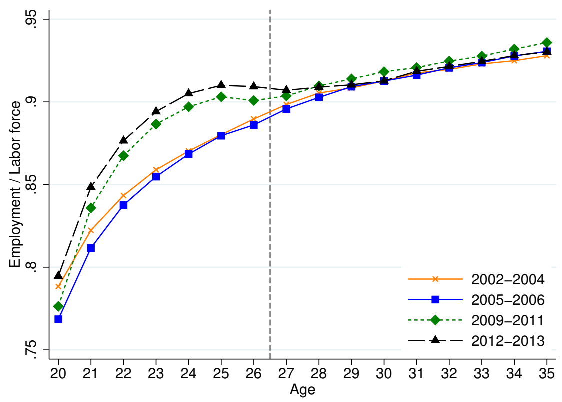
(a) Participation in the program by year



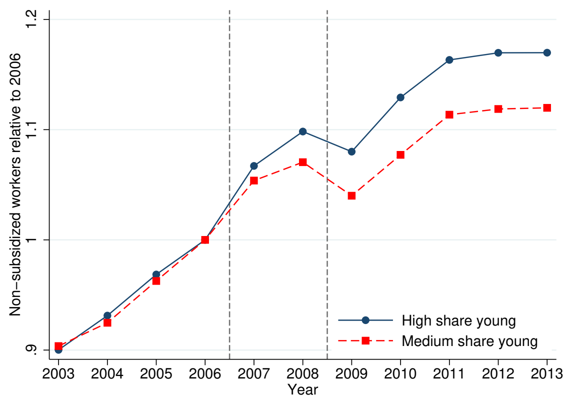
(b) Participation in the program by age (2010-11)



(c) Market-level employment results robustness

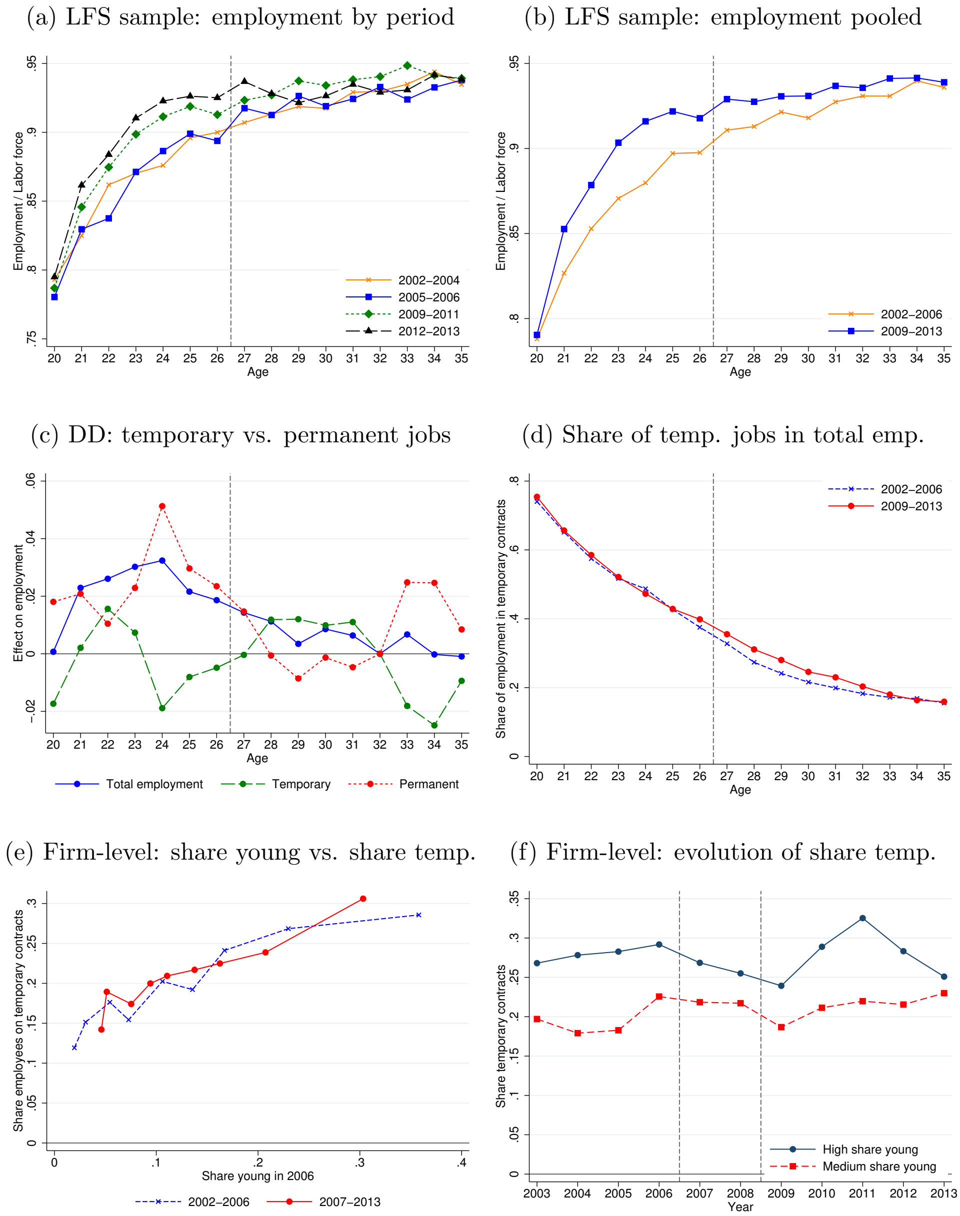


(d) Firm-level employment results robustness



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

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



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

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

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

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

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

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

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

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

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

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