Hysteresis from Employer Subsidies*

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Abstract

This paper uses administrative data to analyze a large and 8-year long employer payroll tax rate cut in Sweden for young workers aged 26 or less. First, we document that while active, the reform raised youth employment among the treated workers. The long-run effects are twice as large as the medium-run effects and likely driven by labor demand (as workers' take-home wages did not respond). Second, we document novel labor-demand-driven "hysteresis" from this policy – i.e. persistent employment effects even after the subsidy no longer applies – along two dimensions. *Over the lifecycle*, employment effects persist even after workers age out of eligibility, reflecting spillovers or cohort effects. *Two years after the repeal*, the treated groups' employment remains elevated at the maximal reform level. A fiscal implication is that the youth employment effects of the reform per dollar of the payroll tax cut are three times larger in the long-term than in the short-term, and when taking into account hysteresis.

Governments in advanced economies use an array of active labor market policies, in particular subsidies, to improve employment of disadvantaged groups such as low skilled workers, the young, the elderly, under-represented groups, or residents of disadvantaged areas.[1] Oftentimes, the policies are presented as a one-time push that could help individuals start working and improve their subsequent careers even after the policy no longer affects them directly. Yet, evidence for active labor market policies to entail such persistent effects on employment is scarce. The elusive policy persistence in the data is puzzling in light of the large body of evidence for employment hysteresis — i.e. persistent employment shifts even after the original cause has disappeared — from non-policy labor market shocks (such as job loss, recession or trade shocks, graduating in a bad economy).[2]

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[1] See OECD (2017) for a description of all such policies in OECD countries.

[2] Blanchard and Summers (1986) popularized the concept of employment hysteresis in the context of unemployment. For job loss shocks, see Topel 1990, Jacobson, Ruhm 1991, LaLonde and Sullivan 1993, Neal 1995,

Our point of departure is the observation that on the policy side, most of the evidence against hysteresis effects comes from the labor *supply* side, where the employment effects from welfare reform or in-work subsidies quickly vanish after the policies end with no lasting gains.[3] By contrast, whether labor *demand* side subsidies can have hysteresis effects remains an open question, despite growing evidence that labor-cost subsidies for disadvantaged groups can boost their employment, at least while the subsidies are active.[4]

Our paper breaks new ground by investigating whether labor demand side policies can deliver employment hysteresis. To do so, we study a large and long-lasting employer-borne payroll tax cut for young workers aged 26 or less in Sweden that started in 2007 but was then suddenly repealed 2015.[5] This setting is uniquely suited for a comprehensive and credibly identified test for hysteresis from labor demand policies in two ways. First, it features sharp age cutoffs of eligibility, permitting us to study *lifecycle hysteresis*, i.e. whether previously treated workers' employment biographies are affected after they have aged out of the subsidy. Second, the sudden and complete abolition of the subsidy after having been in place for multiple years, long enough to have treated cohorts over their entire initial careers, allows us to additionally measure persistent *aggregate hysteresis* effects on Swedish youth overall.

We start by looking at the incidence on wages to check the degree to which the employer subsidies actually shifted labor costs and can hence effectively served as a labor demand stimulus. We find no differential incidence on net-of-tax wages of young treated workers relative to slightly older untreated workers either when the tax cut was put in place or when it was repealed.[6] As a

Von Wachter and Bender 2006, Davis and Von Wachter 2011. For recession shocks, see Blanchard and Katz 1992, Yagan 2018. For trade shocks, see Autor et al. 2013, 2014. For graduating in a bad economy, see Kahn 2010 and Oreopoulos, Von Wachter, and Heisz 2012.

[3]For example, Card and Hyslop (2005) show that the self sufficiency project in Canada, which provided large work subsidies for three years, had a large effect on the labor force participation of recipients, but the effects vanish entirely within months after the subsidy ends. More recently, Miller et al. (2018) shows that an EITC experiment for childless singles in New York City increases their labor supply, but the effect disappears quickly after the subsidy ends (Miller et al. 2018). Symmetrically, earlier work on the Negative Income Tax experiments of the 1960s and 1970s in the United States showed negative effects (of modest size) on the labor supply of recipients but the negative effect disappears once the NIT ends (see e.g., Ashenfelter and Plan 1990 for basic nonparametric estimates). Also juxtaposing the effects of the introduction and the aftermath of the abolition of a sharply age-specific policy, Jäger et al. (2019) document absence of hysteresis in response to retirement incentives in the context of a UI reform in Austria.

[4]Evidence suggests that these policies can be effective if designed to be broad enough (so as not to stigmatize targeted populations) and if the administration is simple (see Katz 1998, Marx 2001, Kluve et al. 2007, and Neumark 2013 for surveys).

[5]Earlier work had found significant but modest positive effects on youth employment rates in the early years of the reform, Skedinger (2014) and Egebark and Kaunitz (2013, 2018), Saez, Schoefer, and Seim (2019). These studies have investigated neither the later years of the reform nor its reversal.

[6]Interestingly, the lack of asymmetry in wage incidence we document stands in contrast striking new work by Benzarti et al. (2017) for value-added-taxes, either due to differences in the natures of wage rigidity and consumer goods price rigidity, or because of different market structures. The absence of wage incidence during

result, the tax changes fully pass through into changes in the labor costs of young workers relative to slightly older untreated workers (in contrast to the standard view in public economics that employer payroll taxes are borne by employees). Specifically, the labor cost of young workers fell by 12% during the tax cut and then went up correspondingly by 14% when the tax cut was repealed, allowing to study employment effects.

Our first employment perspective is on the period while the policy was active and narrowly focusing on the directly treated age groups, to then study hysteresis. Here, we find large and growing employment effects on the directly treated youth aged 26 and below. The estimated employment effects are around 2 points 3–5 years into the reform. Strikingly, these effects *double*, to 4–5 points, by the 6th and 7th year into the reform.[7] This effect can be driven either by a slow adjustment in labor demand (if firms build up youth-specific capital, employer learning, or if discrimination against the young in the labor market takes time to erode). Or, the effect within the treatment group may be driven by hysteresis-like effects as exposure to the policy takes time to build up – two explanations we would not be able to cleanly separate as long as the policy remains active.

We next turn to a sharp test to cleanly isolate hysteresis effects, by studying the evolution of employment as workers age out of the policy (and hence are no longer cheaper to employers), as well as after the repeal of the reform. In a standard spot labor market, labor costs would immediately fall (as we did not find wage responses on time nor age dimensions), and therefore employers would no longer face differential incentives to hire young workers. Any employment effects after the policy switches off therefore constitute hysteresis effects by definition.[8]

We document substantial evidence for employment hysteresis on both margins in response to the employer subsidies. We start with lifecycle hysteresis. Here, several years into the reform, we find clear positive and growing hysteresis employment effects emerging among of young workers exposed to the tax cut after they age out and hence their wages are no longer subsidized. The multi-year lead-up these effects required explains why they were missed by the existing short-run analyses (and perhaps may never have emerged in shorter-run policies). The lifecycle hysteresis suggests that the positive youth employment gains caused by the reform have consequences for

the introduction of the policy is consistent with earlier analysis of the payroll tax introduction (Skedinger 2014, Egebark and Kaunitz 2013, 2018), Saez, Schoefer, and Seim 2019), which however has not studied the later years and most importantly its repeal and hence not potential asymmetries.

[7]The early, smaller effects are consistent with existing evaluation by (Skedinger 2014, Egebark and Kaunitz 2013, 2018), Saez, Schoefer, and Seim 2019).

[8]By adjusting slowly through hiring and separation dynamics, long-term jobs can exhibit less interesting hysteresis-like mechanism. However, employment rates quickly converge to steady state due to turnover, implying that such effects are negligible at our longer horizon post-repeal, and are unlikely to drive our results. In a future revision, we will provide a formal quantitative evaluation of this result.

the subsequent careers of directly treated workers. Such hysteresis effects are consistent with a significant body of work in labor economics discussed above. Or, spillover effects for young workers as a group may explain these effects, such that firms are more likely to hire young workers across the board, due to random search, learning about high youth productivity, or investment in youth-specific capital that complements slightly older workers as well.

Third, we turn to aggregate hysteresis in the time series, studying the aftermath of the repeal in 2015. Strikingly despite the large increase in youth labor costs, in the first two years the repeal for which we have data, we do not see *any* reduction in youth employment (if anything, even a differential increase). We view this as simple and compelling evidence for market-level hysteresis effects due to the uniquely clean and sharp difference-in-difference nature of our quasi-experimental variation (the abolition of the policy) compared to, e.g., trade shocks or recessions (where driving forces are not directly observed and hence nor is their own persistence). The persistent youth employment effects could be explained by the time it takes for employers to factor in the payroll tax change into their personnel decisions. Alternatively, it could reflect a persistent change in hiring decisions. For example, firms might have developed youth labor intensive technologies that cannot be reversed quickly. It is also conceivable that the payroll tax cut removed discrimination against the young in hiring decisions (such as posting job ads requiring prior years of experience or some minimum age) and that such discrimination does not come back after the tax cut repeal (as employers learn how to hire young workers and about their quality as workers).[9] Future years after the repeal will show whether the youth employment gains start to disappear.

One implication of our three employment findings combined is that the long-term employment effects of the reform are much larger than the effects initially estimated by previous work on the reform, which only studied the early years as well as ignored spillovers across the age distribution (Skedinger, 2014, Egebark and Kaunitz 2013, 2018, and Saez, Schoefer, Seim, 2019). In particular and cast in fiscal terms, conservatively assuming that all employment effects disappear after 2017 (the last year we can analyze which is the second year after the repeal), we find that employment effects per dollar of payroll tax cut are already three times as large relative to using our medium-run estimates here, or earlier estimates in Saez, Schoefer, Seim (2019).

[9]In the anti-discrimination literature, some studies have shown that affirmative action policies can have long-term effects on minority employment even after the policies end. For example, Miller and Segal (2012) studies affirmative action quotas imposed by federal courts in the 1970s for municipal police in the US and finds that black share gains due to the quotas do not erode after the policy ends. Even more striking, Miller (2017) studies federal affirmative action regulation of private employers, and finds that the black share of employees continues to grow even after private employers are no longer regulated. He argues that this persistence is driven by permanent changes in screening methods for potential hires.

These additional effects are largely due to the much larger effects in 2014 and 2015 and the "free years" with even larger magnitudes in 2016 and 2017.[10]

Our paper is connected to other literatures on hysteresis effects. First, there is a large literature that studies policies targeting disadvantaged areas (see Kline and Moretti 2014 for a recent survey). For example, the Tennessee Valley Authority program to develop the US South during the Great Depression recently analyzed by Kline and Moretti (2013) had permanent effects on economic development. More recent policies, such as the US empowerment zones seem to have more modest effects (see e.g., Busso, Gregory, and Kline 2013). Second, World War II was a huge labor demand shock for American women. While female labor force participation fell back shortly after the war (Goldin 1991), geographical variation shows that there was some long-run persistence (Acemoglu, Autor, Lyle 2004 and Goldin and Olivetti 2013). Third, a number of studies in behavioral economics have shown persistent effects of temporary policies on various individual behaviors (such as energy use, exercise, smoking, and voting).[11] In particular, Costa and Gerard (2018) provide a framework to analyze the welfare consequence of externality correcting policies when there is such hysteresis.

This paper is organized as follows. In Section I, we describe the institutional setting, the payroll tax reform, and the data. In Section II, we present the effects of the payroll tax cut on wages and employment. Section III presents calculations on the policy effectiveness, including payroll tax costs per job created. Section IV concludes.

I Institutional Setting and Data

In this section, we first discuss the institutional setting of the payroll tax in Sweden and the payroll tax cut reform and its subsequent repeal. Next, we present the data we use for the analysis.[12]

I.A Payroll Tax Cut for Young Workers in Sweden

Swedish payroll tax. In Sweden, the entirety of the payroll tax on earnings is nominally paid by employers and the tax is proportional to wage earnings with no exemption and no cap.

[10]The lifecycle hysteresis aspect, while still growing and hence potentially larger if the payroll tax cut had lasted even longer, has comparatively minor effects.

[11]See e.g. Charness and Gneezy (2009) for exercise; Allcott and Rogers (2014), Costa and Gerard (2018), and Ito, Ida, and Tanaka (2017) for energy use; Fujiwara, Meng, and Vogl (2016) for voting, and Giné, Karlan, and Zinman (2010) for smoking.

[12]More complete details can be found in Skedinger (2014), Egebark and Kaunitz (2013, 2018), or Saez, Schoefer, Seim (2019).

The payroll tax rate is uniform across industrial sectors and covers all employers public and private. The top series in the solid line in Figure 1 depicts the normal payroll tax rate from 2004 to 2019. The normal tax rate has been quite stable around 31-32 percent over this period.

Young workers' payroll tax cuts. The second series in the dashed line in Figure 1 depicts the preferential payroll tax rate for young workers. In 2007-9, a new center-right coalition government implemented a payroll tax cut targeted toward young workers. The payroll tax cut was part of the center-right coalition's election promise in 2006 (e.g. Dagens Nyheter, August 12, 2006). The explicit aim of this reform was to fight youth unemployment, which had risen in previous years, and was perceived in the public debate to be excessively high. It was enacted as a permanent tax change.[13]

On July 1st, 2007, the first step lowered the payroll tax rate by 11.1 points from 32.42 percent (main rate in 2007) down to 21.32 percent for all workers turning 19 to 25 during the calendar year. On January 1st, 2009, the second step further lowered the payroll tax rate down to 15.49 percent and increased eligibility to all workers turning 26 or less during the calendar year (instead of 19-25 in the first step). To be precise, in 2009, the payroll tax cut applied to all workers born in 1983 or later on the totality of their 2009 earnings; in 2010, the payroll tax cut applied to all workers born in 1984 or later, etc. The tax cut is directly administered through the payroll tax software used by employers where individual earnings and year of birth is reported on a monthly basis by employers. Therefore, the take-up is close to 100 percent.

Hence, a worker's *only* determinant of eligibility for a full calendar year is year of birth (and not actual age when the earnings are received), assessed against a rolling window of eligibility by birth (calendar) years. For a given year, our analysis is always based on birth-year cohorts: *age* is always defined as year of observation minus birth year – regardless of whether the person has actually reached her birthday or not during the year.

Repeal in 2015-16. The left-wing opposition parties were against this payroll tax cut from the start. They lost the 2010 election but narrowly won the 2014 election on September 14. Therefore, in 2015, the new center-left government abolished the payroll tax cut for young workers. The lower payroll tax rate for the young expired in three steps on May 1, 2015, August 1, 2015 and June 1, 2016, as depicted in Figure 1. The bill was passed on March 25, 2015 following a proposal put forward on October 7, 2014, just after the election. In the first

[13]Importantly, the reduced payroll taxes did not entail a change in generosity of any benefit programs for the treated young or their employers.

step on May 1, 2015 (not depicted on the figure), which lasted 3 months only, the tax cut was repealed for workers turning 26 in 2015.[14] In the second step on August 1, 2015, which lasted 10 months, all workers turning 25 and less the calendar year had their taxes increased to 25.46 percent (63% of the tax rate gap was closed). In the third step on June 1, 2016, the normal payroll tax rate applies to all workers. Therefore, the payroll tax cut lasted 9 years, and 6.5 years in its strongest form.

I.B Administrative Data

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

Our analysis is based on the full population of all Swedish residents (as of December 31 each year) aged 16 and above for years 1990-2013. We obtain annual earnings and employment spells for this population using the complete matched employer-employee records available for all years 1985-2013, with unique individual and firm identifiers. For each spell, these data record annual wage payments and months worked.[15] We also add a number of outcome and demographic variables to the individual-level population at the annual level. From the Income Tax Register, we retrieve self-employment earnings and total wage earnings. From the Integrated Database for Labour Market Research (various administrative records compiled by Statistics Sweden), we obtain the level of education, unemployment history (days registered with the unemployment insurance agency as well as unemployment insurance received), gender, year and month of birth.

We also link to this baseline population a matched employer-employee annual data set–the Structure of Earnings Survey–that covers worker-level wages, occupational codes and hours of work, for a very large sample of firms. The data set covers all public sector employees and around 50 percent of private sector workers.[16] The information is collected during a measurement week (in September-November, hence the 2016 data covers post-abolition wages) for all workers employed for at least one hour during that week. The wage concept is the full-time equivalent monthly wage prevailing in the given month, including all fixed wage components, piece-rate and performance pay and fringe benefits.[17] We use this wage concept to study the incidence of

[14]The payroll tax rate was actually further lowered down to 10.21% for workers turning 23 or less to be budget neutral as the new government was forced to rule under the budget of the opposition. As a result, the new government could not re-budget programs until August 1, 2015.

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

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

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

the payroll tax cut on market wages in Section II.A.

II Empirical Analysis

II.A Effects on Net and Gross Wages

We first analyze the effects of the payroll tax reform on cohort-specific wages to determine the incidence of the payroll tax and its effects on employment. *Gross wage earnings* are wage earnings plus the employer payroll tax. *Net wage earnings* are wage earnings net of employer payroll tax. It is the concept used for computing payroll taxes.

Following Saez, Schoefer, and Seim (2019), we evaluate whether net wages vs. gross wages are discontinuous by age around the eligibility threshold. By definition, both wage concepts cannot be continuous after the reform, so looking at both earnings concepts is a powerful and transparent way to tease out where the incidence falls. If gross wages paid by firms remain continuous, the incidence is entirely on workers' net wages. If net wages remain continuous, then firms experience full pass-through into the relative labor costs of young workers.

Figure 2 depicts the average monthly wage in Sweden by age for different time periods using the Structure of Earnings Survey data for wage earners. The survey measures wages mostly for the month of September (with some measurements in October and November). 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 wage is defined as the full-time equivalent contracted monthly wage. It is adjusted for inflation (base-year 2003) and converted to US dollars using an exchange rate of 8.9 SEK/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. 2003-2006 are pre-tax cut years (red square series). 2007-2015 are tax cut years (blue circle series). 2016-2017 are post-repeal years (green triangle series).[18] The reform applies to ages up to 26 in 2009-2014 and up to age 25 in 2007-8 and 2015 (as depicted by the two dashed vertical lines, see Figure 1 for details). The size of the tax cut is less in 2007-8 and 2015 as the tax cut is phased-in and phased-out. The sample includes all employees in the Structure of Earnings Survey, which covers all industrial sectors (see Section I.B).

The top panel shows that net wages are continuous at the age thresholds, and the bottom panel shows that gross wages are discontinuous. In the bottom panel, the discontinuity happens at age 25 to 26 in years 2007-2008 and 2015 when the tax cut did not cover workers aged 26.

[18] As described in Section I.B, the Structure of Earnings Survey covers September-November wages, so 2016 captures the full post-abolition period after June 1st, 2016.

In years, 2009-2014, when the reform applied to workers aged 26 as well, the discontinuity in gross wages is between ages 26 and 27.

This implies that employers do not adjust wages by age in response to the payroll tax cut and its subsequent repeal, and therefore absorb fully the tax discontinuity created by the age and time specific payroll tax reform.

Perhaps wages do not appear to respond in ongoing jobs due to wage rigidity or implicit contracts. To test this, we repeat Figure 2 but limiting the sample to new hires in Apppendix Figure A1. New hires are defined as having a new firm identifier (again for the month of September) as the main employer relative to the previous year. It includes both job-to-job transitions as well as new hires among previously nonemployed individuals. These new hires are not affected by implicit wage contracts by definition. Yet, even for this subsample, we do not see any discontinuity in net-wages either when the reform is put in place or when it is repealed. All the discontinuities are loaded on gross wages, and hence borne by the employer. This implies that standard implicit contracts or wage rigidity in ongoing jobs more generally, cannot explain our findings either.

Therefore, we find no effect on net-of-tax wages of young treated workers relative to slightly older untreated workers either when the tax cut was put in place or when it was repealed. This is consistent with earlier analysis of the payroll tax introduction which found no or very little effect on net-wages (Skedinger 2014, Egebark and Kaunitz 2013, 2018), Saez, Schoefer, and Seim 2019). These studies however did not study the longer-run nor the post-repeal period. This complete absence of incidence on net-wages also goes against the standard view in public economics that employer payroll taxes are borne by employees. It implies that the tax changes translate fully into changes in the labor costs of young workers relative to slightly older untreated workers. The labor cost of young workers went down by about 12% during the tax cut and then went up correspondingly by 14% when the tax cut was repealed, permitting us to study employment effects of the reform-induced cost reduction of youth labor next.

II.B Effects on Employment

Our wage results imply that young eligible workers are cheaper to employers than slightly older, ineligible workers when the reform is in place. In the period 2009-2014, the payroll tax rate cut for young workers lowered their labor cost by 12.1 percent.[19] Effectively, an employer would save 12.1 percent of labor costs if she could switch from an ineligible older worker (say aged slightly

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

above 26) to an eligible young worker (aged 26 or less), given the lack of net wage incidence. In Sweden, CVs typically include age so that employers can generally observe age before hiring. As these two groups of workers should be close substitutes, profit maximizing firms should want to hire more eligible workers or put more effort in retaining eligible workers (relative to ineligible workers). Indeed, this is the economic mechanism that eventually equalizes gross wages across treated and control groups in the standard competitive model. Even or especially with rigid wages, we should see employment effects if firms care about labor costs in their labor demand.

Methodology To analyze such resulting employment effects, we examine the employment rate in the labor force by age group and over time using the individual annual earnings data (see Section I for details). The employment rate is defined as the ratio of all employment to the labor force. The employment numerator is defined as all residents who are employed with annual wage earnings above a small annual threshold. The small annual threshold is equal to $4,940 in 2012 (and adjusted for median wage growth in other years). This small annual threshold corresponds approximately to working at 20 percent of full-time a full year at the minimum wage in the restaurant sector. The labor force denominator is defined as all residents who are either (i) employed with annual wage earnings above the small annual threshold or (ii) unemployed (defined as having registered with the Unemployment Office at any point during the year).

The top panel of Figure 3 depicts the employment rate by age and time periods without any adjustment. We refer to it as the *unadjusted* employment rate. Series in red squares are before the payroll tax cut is in place. Series in blue circles are when the reform is in place in three periods 2010-11, 2012-13, 2014-15.[20] Series in green triangles are after the repeal 2016-17 – and this post-repeal period serves as our additional time series for a DiD strategy to test persistence and aggregate hysteresis effects in all age groups.

The bottom panel of Figure 3 presents an *adjusted* difference-in-difference version of these results, transparently and conservatively controlling for the business cycle overall employment effects. Specifically, we measure employment rates by age and single years relative to 2006. The normalization is made by aligning the unemployment rate (one minus the employment rate) for ages 35-40 to the 2006 level for each year (multiplicatively) and then taking the difference in employment rates with 2006. Formally, if e_{at} is the employment rate in year t at age a, $u_{at} = 1 - e_{at}$ is the unemployment rate in year t at age a. The normalized unemployment

[20]We exclude 2007-8 when the reform was not yet fully phased-in and 2009 the first year when the reform was phased-in (as 2009 was a deep recession year making it less comparable to other years).

rates are $\hat{u}_{at} = u_{at} \cdot (u_{35-40,2006}/u_{35-40,t})$ and hence the normalized employment rates are $\hat{e}_{at} = 1 - \hat{u}_{at} = 1 - (1 - e_{at}) \cdot (u_{35-40,2006}/u_{35-40,t})$. We then plot on the bottom panel the series: $\hat{e}_{at} - \hat{e}_{a,2006} = (e_{at} - e_{a,2006}) \cdot (u_{35-40,2006}/u_{35-40,t})$. Graphically, this means adjusting series in the top panel multiplicatively to align unemployment rates at ages 35-40 across years and then taking the difference with 2006. That series can therefore be read as the deviation of employment rates by age and year (relative to 2006) expressed in points of employment and controlling for the overall level of unemployment across years. Compared to additive or multiplicative adjustment based on the employment rate, our strategy is most conservative.[21] The simple normalization complements and clarifies the more impressionistic findings from the raw employment series in the top panel.

The 35-40 workers hence serve as our "far-away" control group – none of these cohort members have ever been treated in their careers. This far-away control group also permits us to investigate spillovers onto the "nearby" control group (workers aged 27 and above), as would arise from lifecycle hysteresis effects.

We have three sets of results on the dynamics of the employment effects of the payroll tax cut and its repeal.

Long- vs. medium-run effects while the reform is active. First, the data reveal a clear increase in youth employment rates from pre-reform periods (2003-4 and 2005-6) to early years of the reform (2010-11 and 2012-13) as documented in Saez, Schoefer, and Seim (2019). In these early years, the employment effects are concentrated at ages 22 to 24 with smaller effects for workers close to the age threshold (25 and especially 26 year old). It is possible that effects are smaller for the oldest treated workers because employers understand that such workers will age out of the reform quickly (recall that employers can observe age on CVs of job applicants in most cases). Effects are also smaller for very young workers (aged 20 or 21), perhaps because such workers are very young relative to other employees making it more challenging for employers to find them, evaluate them, and incorporate them into their workforce.

Interestingly however, in 2014-2015, there is a clear further increase in youth employment

[21] These methods all imply that $\hat{e}_{at} - \hat{e}_{a,2006} = [e_{at} - e_{a,2006}] - x_{art} \cdot [e_{rt} - e_{r,2006}]$ for a reference age r (in our case 35-40). For our strategy, $x_{art} = \frac{1-e_{at}}{1-e_{rt}}$; had we chosen to multiplicatively adjust based on employment rates, $x_{art} = \frac{e_{at}}{e_{rt}}$; and $x_{art} = 1$ for additive adjustment. Since 35-40 employment is always larger than youth employment, $x_{art} < 1$, the multiplicative adjustment attenuates effects compared to an additive adjustment. Since employment rates are close to one, our unemployment-based method further attenuates effects compared to the employment-based one. Therefore, our method implies smallest youth employment deviation in the post-2006 years and hence the most conservative treatment effect among these options.

while the employment rates of older workers aged 30-40 remain stable. Therefore, the effect of the reform appears much stronger in 2014-2015 than in earlier years: as the reform matures, the employment effects both deepen and widen: the effects are larger at all ages but especially the very young. It is striking to see that series line up monotonically across years. By year 2015, the employment effect is about three times as large as in 2010.

Lifecycle hysteresis and spillovers on control ages. Second, workers slightly older than 26 and hence no longer eligible for the tax cut appear to have a higher employment rate in 2014-2015 than in earlier years. These workers were exposed to the reform, suggesting that exposure to the reform has hysteresis effects on employment (or that spillover effects on the group level may take longer to materialize). Both panels show clearly that the effect of the reform spills over gradually across slightly older groups. The spillover is almost perfectly monotonic, providing compelling evidence that it is reform-driven.

Post-repeal hysteresis for all young workers. Third and perhaps most striking, the employment effects of the young if anything keep increasing in 2016 and 2017 after the tax cut is repealed. This suggests that the positive employment effects of the reform do not vanish after the payroll tax cut ends – a clear indication of hysteresis effect at this group level.[22] Future years will show whether the youth employment gains start to disappear as more years elapse after the repeal.

Moreover, the lifecycle hysteresis, or spillover, effects also continue after the reform mirroring the reform-period shape (even appearing to be further increasing). This is particular clear in the normalized bottom panel, lending support to our normalization choice.

Placebo test: 2004-5. Finally, the bottom panel includes two pre-reform years 2004 and 2005, which can serve as a placebo test (recall that the bottom panel is normalized relative to 2006). For these years, we do not detect any employment effects at any age, further lending credence to our identification assumption.

Regression evidence. Table 1 provides the corresponding estimates using a basic difference-in-differences regression based on the graphical output, based solely on the aggregate cohort-year

[22] As a caveat, note that these quantitative effects post-repeal depend on our choice of normalization because employment rates are higher across the board in 2016-2017 as shown in the top panel of Figure 3. As discussed above, we have chosen the normalization that seems the most natural to us and which also minimizes the post repeal employment effects on the young.

time series as depicted in the figures. We use data at the year×age level with 21 age categories a (20 to 40), and 15 years t (2003 to 2017). We then group ages into 6 groups denoted by A, comprising 20-6, 27-8, 29-30, 31-2, 33-4, 35-40. Age group 35-40 is always the control group. We divide years into 5 periods denoted by T, comprising 2003-5, 2006 (base year), 2010-2, 2013-5, 2016-7. Separately for each age group A, we run the following basic difference-in-differences regression at the annual level, including ages in group A as well as the control group aged 35-40:

$$\hat{e}_{at} = \alpha_0 + \alpha_A \cdot \mathbb{1}(a \in A) + \sum_T \alpha_T \cdot \mathbb{1}(t \in T) + \sum_T \gamma_{AT} \cdot \mathbb{1}(a \in A \wedge t \in T) + \varepsilon_{at}. \qquad (1)$$

The regression includes the treatment age group A dummy, and a full set of period dummies. The coefficients of interest are the interactions γ_{AT} are reported in the table for each T and A (interactions exclude the 2006 control period). These coefficients are simply the average of the adjusted employment rates by age group and periods depicted in the bottom panel of Figure 3. Conventional OLS standard errors are reported.

The table confirms and quantifies the visual impression from the bottom panel of Figure 3. In 2010-12, the effect on employment for the treated young is 1.5 points. In 2013-15, this effect jumps to 3.2%. This shows that the effect double from the early years to the late years of the reform. In 2016-17, after the repeal, the effect increases further to 4.8 points. The effect for ages 27-28 is zero in the early years 2010-12 but becomes significant for year 2013-15 (1.1 point effect) and even larger after the repeal (2.3 points). At ages 29-30, we find a significant effect of 1.2 points after the repeal (recall that these workers were exposed to the tax cut when they were younger). The placebo years 2003-05 do not show any effect either for the treatment group or for the other ages confirming the validity of our estimation methodology.

Heterogeneity by gender. Figure 4 shows the employment rate effects by age and year relative to 2006 for males (top panel) and for females (bottom panel). The methodology is the same as in Figure 3, bottom panel, but splitting the sample by gender.[23] The y-axis scale on the male and female panels is the same for comparison. The figure shows a similar effect of the reform by gender in early years (2010-2012), but the effect on young female workers grows much more in subsequent years and remains much higher for females after tax cut is repealed (although hysteresis effects are present for both genders).

[23]That is, the normalization is made by aligning the unemployment rate (one minus the employment rate) for ages 35-40 by gender to the 2006 level for each year and then taking the difference in employment rates with 2006. For reference, raw employment rate series by gender, age, and years is presented in Appendix Figure A2.

Heterogeneity by local youth unemployment rate. Figure 5 shows the employment rates by age and year relative to 2006 for high youth unemployment rate areas (top panel) and for low youth unemployment rate areas (bottom panel). The methodology is the same as in Figure 3, bottom panel, but splitting the sample by areas based on their youth unemployment rates in 2006.[24] Areas are divided into quintiles using weights so that the number of young in the labor force in 2006 are roughly the same across all five groups. In 2006, just before the reform, there was wide variation across Sweden's 21 regions in youth unemployment. Regions in the lowest quintile of youth unemployment rates had rates in the range of 10.5-12.4% while regions in the highest quintile had youth unemployment rates in the range of 20-23.3%, i.e. about twice as high.

The figure reveals a much larger effect of the payroll tax cut in high youth unemployment areas, already in the medium run. Strikingly, in the longer run, the acceleration of the treatment effect the national analysis indicated is particularly pronounced in the high youth unemployment regions (an effect not driven by mean reversion as the flat pre-period gradients indicate).

Next, we discuss hysteresis results. In neither group do the gains immediately disappear as workers age out of the policy, since employment effects appear among the older ineligible workers, particularly so in the later years of the policy. Most strikingly however, in the high youth unemployment area the additional gains in employment are not more quickly undone, but the larger treatment effect persists even after the subsidies are abolished. As a result, even two years after the policy was repealed, the areas maintain the substantial gains towards convergence in youth unemployment, as illustrated in Appendix Figure A3, which plots the raw employment rate levels.

III Hysteresis Effects and Policy Effectiveness

We close with a quantitative assessment of how the employment dynamics we uncover affect the policy evaluation of the reform. We proceed in two parts.

Jobs Created. First, in Table 2, we calculate the count of job creation from the policy, and the contribution from hysteresis and direct effects. For each time period and age group, we compute the count of policy-induced job creation. Our unit is job-years as defined as beforehand

[24] Again, the normalization of employment rates is made by aligning multiplicatively the unemployment rate (one minus the employment rate) for ages 35-40 to the 2006 level for each year and then taking the difference in employment rates with 2006. Raw employment rate series by region, age, and years are presented in Appendix Figure A3. The scale on the two panels is the same for comparison.

(i.e. the workers with annual earnings above the threshold).[25] The table is organized as our regression table, with rows denoting periods and columns denoting age groups. The end of each rows and each column additionally includes a total count, and the bottom right corner reports the total jobs created. Each entry denotes the count, but also the share of that entry contributing to total job creation where a job is defined as 1 more employed person in a given year.

The table reveals that the policy created around 531,200 job-years. 90% of these jobs were created among the 20-26. However, within the jobs created among the 20-26 year olds, only around a fifth was created in the medium run. Two fifths of jobs created in this group were generated in the long run, due to the larger treatment effect that showed up in the later three years of the policy.

Most strikingly, the remaining two fifth of jobs created within the 20-26 year old group (213,965 jobs) were created *after* the repeal of the policy, hence from hysteresis effects. Across all groups, the two post-repeal years (2016-7) account for 273,301 (46.4%) of all jobs created (particularly striking as this post-period is only two years long, compared to our two three-year reform periods).

The lifecycle hysteresis effects are strongest among the 27-8 year olds, which alone already account for 55,479 jobs created. On a per-age-year basis (i.e. dividing by the number of life years in an age group), that group created around 28,000 jobs compared to around 76,000 in the treatment group, implying tremendous hysteresis effects around a third as large as the direct effects among the treated ages. Spillover effects around the even older groups are smaller while the policy is active, but strikingly start showing up even among the 29-30 year olds *after* the repeal.

Payroll Costs per Job Created. Second, we additionally provide simple evaluation of a narrow concept of fiscal costs per job created: in terms of payroll tax revenue foregone, but excluding all payroll tax revenue gain from the larger tax base, and from any other sources of

[25] To construct this reform-induced count of job creation, we therefore take the age-group and period specific effects γ_{AT} from DiD regression model in Equation (1), already reported in Table 1. Since the outcome variable is the employment/labor force ratio, we multiply it with the labor force body count of age group A from base period 2006 (consistent with the regression weighting), i.e. $\Delta E_{AT} = \gamma_{AT} \text{LF}_{A,\text{2005-6}}$, where Δ denotes the treatment effect from the policy, consistent with our identification assumption underlying the DiD design. We also construct variants of this measure, namely total age-specific job creation over all years $\Delta E_A = \sum_T \Delta E_{AT}$, total time-specific job creation over all cohorts $\Delta E_T = \sum_A \Delta E_{AT}$, and total overall employment effects $\Delta E = \sum_A \sum_T \Delta E_{AT}$ (In practice, our time periods combine multiple years, so we multiply a given time period's treatment effect by its length in years (3, and 2 for the post-repeal). We also leave out the pre-period in the set of A when cumulating employment effects, which are essentially zero (our placebo test).) The share of a given entry contributing to total job creation is $\Delta E_{AT}/\Delta E_T$.

associated tax revenue. This exercise is not comprehensive nor does it have normative content, but illustrates the fiscal role of hysteresis and long-run effects we document in a widely used policy number.

The intuitions are simple: first, the long-run effects combined with spillovers lead to larger overall job creation, as reported in Table 2. Second, spillovers of both types further increase this measure because additional jobs enter the numerator, but at zero cost in the denominator.

To gauge these effects, we conservatively hold fixed the employment structure again at 2006 so as to not count as gains the additional jobs created. We use our administrative data to calculate the payroll tax base for the workers aged 20-26, which is their labor income.

The gap between the regular tax rate, 31.4%, and the lowest preferential payroll tax rate, 15.4%, was 15.9ppt. Considering only the directly created jobs (i.e. among the treated 20-6 olds) during the medium run of the policy, the implied lost payroll tax revenue per year was \$4,659,848,000 (in 2017 USD). In the medium run, around 99,019 job-years were created over a three year period, such that the per-job year cost was \$141,180 in that period. By contrast, in the 2nd period we deem the long run, 212,732 job-years were created, such that the contemporaneous costs was *halved*, to \$64,063. Clearly, by definition, the post-repeal costs were \$0, hence any spillover into that period comes for free. On average, taking total job creation among the young, we therefore obtain an *average* per-job cost of \$52,634 excluding the older workers, and \$47,471 when including the spillovers.

Overall, the full analysis including hysteresis effects reduces the costs from \$141,180 to \$47,471, a dramatically reduction by *two thirds*, also compared to earlier estimates in Saez, Schoefer, Seim (2019) restricted to the early years of the reform. These additional effects are largely due to the much larger effects in 2014 and 2015 and the "free years" with even larger magnitudes in 2016 and 2017. The lifecycle hysteresis aspect, while still growing and hence potentially larger if the payroll tax cut had lasted even longer, has comparatively minor effects.

IV Conclusion

Which mechanisms may drive the hysteresis in youth employment? The employment response we have uncovered is likely due to labor demand effects rather than labor supply effects. This interpretation is plausible because the net wage of eligible young workers does not increase (our first result, in Section II.A), and hence labor costs were lowered one to one. Moreover, the presence of high youth unemployment to begin with suggests rationed labor supply and hence permits demand-determined employment.

For our hysteresis effects therefore, potential specific labor demand mechanisms generating hysteresis may be a delay in the factoring in of the payroll tax reversal into personnel decisions. Alternatively, it could reflect a persistent or even permanent change in hiring decisions. For example, firms might have developed youth intensive technologies that cannot be reversed quickly. It is also conceivable that the payroll tax cut removed discrimination against the young in hiring decisions (such as posting job ads requiring prior years of experience or some minimum age) and that such discrimination does not come back after the tax cut repeal (as employers learn about hiring young workers).

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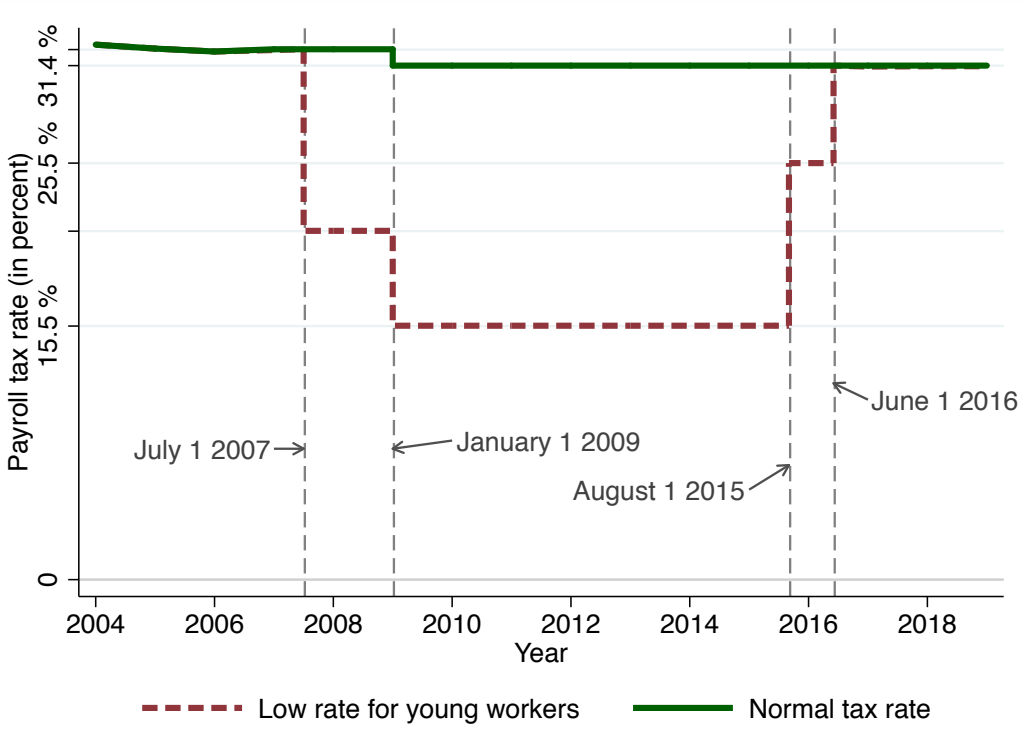
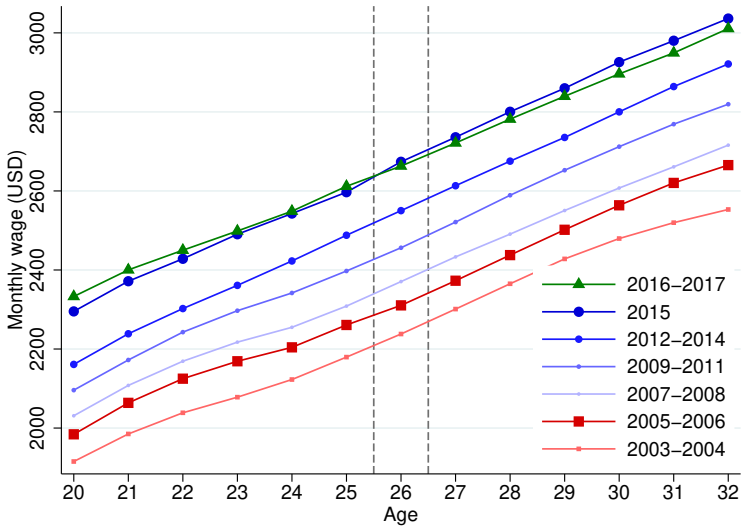
Figure 1: Preferential Payroll Tax Rate for Young Workers



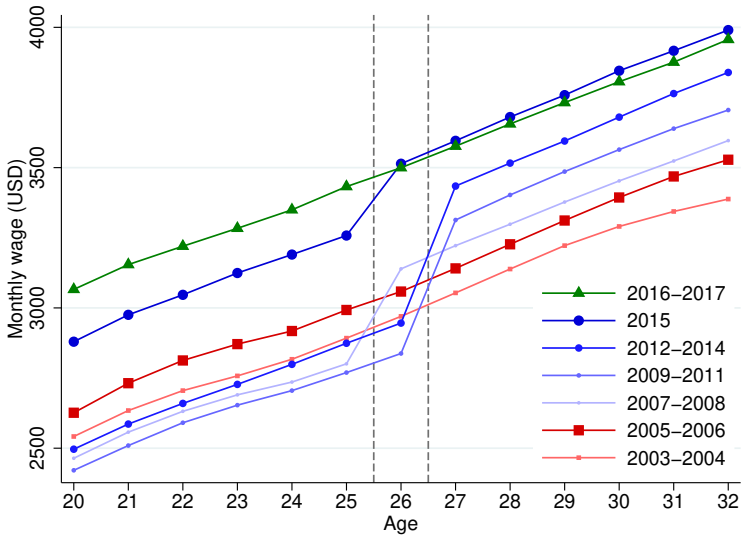
Notes: The figure depicts the normal payroll tax rate (solid line) and the lower rate for young workers (dashed line) in Sweden over time. The payroll tax rate in Sweden applies to the totality of earnings and is nominally fully paid by employers. The first reform lowered the payroll tax rate for earnings received on or after July 1, 2007 for all workers turning 19 to 25 during the calendar year down to 21.3 percent. The second reform further lowered the tax rate down to 15.5 percent for earnings received on or after January 1, 2009 for all workers turning 26 or less during the calendar year. The reform was repealed in three steps on May 1, 2015, August 1, 2015, and on June 1, 2016. In the first step on May 1, 2015 (not depicted on the figure), which lasted 3 months only, the tax cut was repealed for workers turning 26 in 2015 (and was actually further lowered for workers turning 23 or less to be budget neutral). In the second step on August 1, 2015, which lasted 10 months, all workers turning 25 and less had their taxes increased to 25.46 percent (63% of the tax rate gap was closed). In the third step on June 1, 2016, the normal payroll tax rate applies to all workers.

Figure 2: The Effect of the Payroll Tax Cut on Average Wages

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



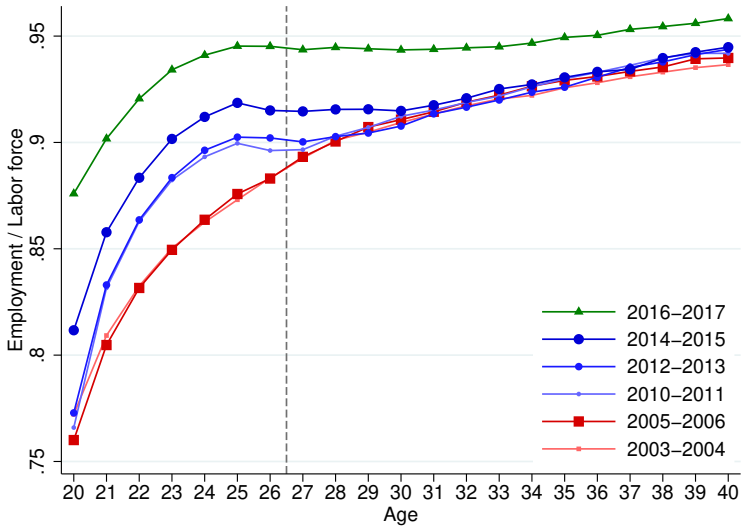
(b) Monthly gross wage (wage earnings gross of the payroll tax)



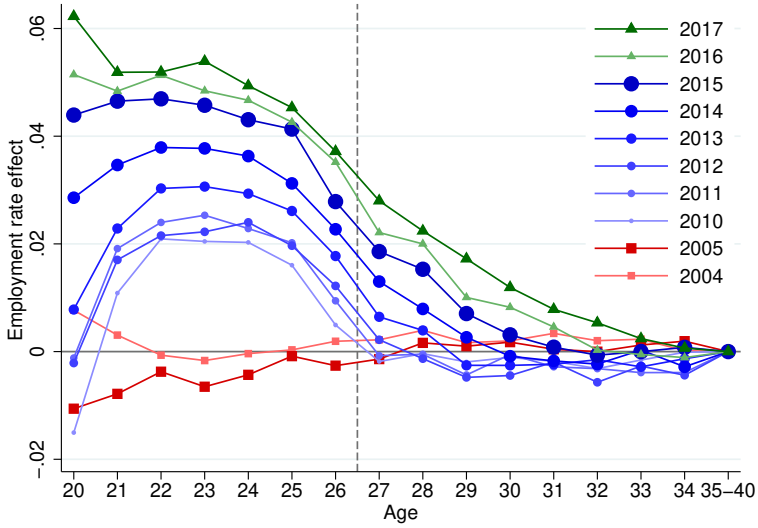
Notes: This figure depicts the average monthly wage in Sweden by age for different time periods using the Structure of Earnings Survey data for wage earners. The survey measures wages mostly for the month of September (with some measurements in October and November). The top panel depicts *net wages* 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 wage is defined as the full-time equivalent contracted monthly wage. It is adjusted for inflation (base-year 2003) and converted to US dollars using an exchange rate of 8.9 SEK/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. 2003-2006 are pre-tax cut years (red square series). 2007-2015 are tax cut years (blue circle series). 2016-2017 are post-repeal years (green triangle series). The reform applies up to age 26 in 2009-2014 and up to age 25 in 2007-8 and 2015 (as depicted by the two dashed vertical lines, see Figure 1 for details). The sample includes all employees in the Structure of Earnings Survey which covers all industrial sectors (see Section I.B). The top panel shows that net wages are continuous at the age thresholds and the bottom panel shows that gross wages are discontinuous. This implies that employers do not adjust wages by age in response to the payroll tax cut and its subsequent repeal.

Figure 3: The Effect of the Payroll Tax Cut on Employment

(a) Employment rates by age and period



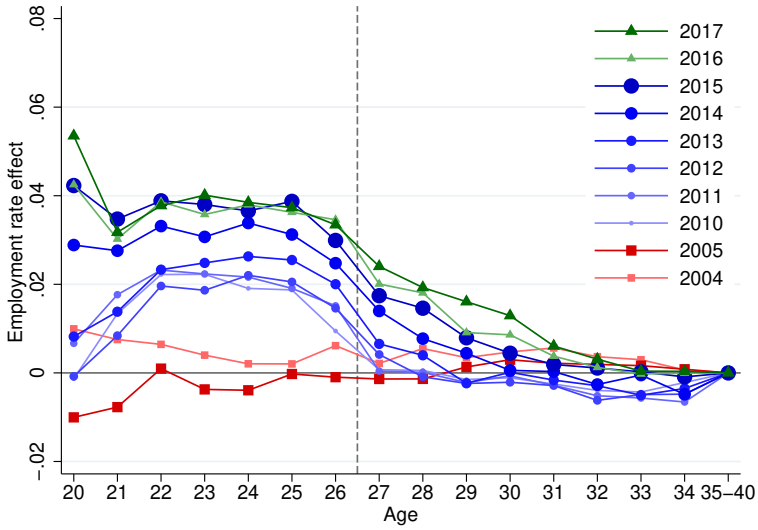
(b) Employment rates by age and year relative to 2006



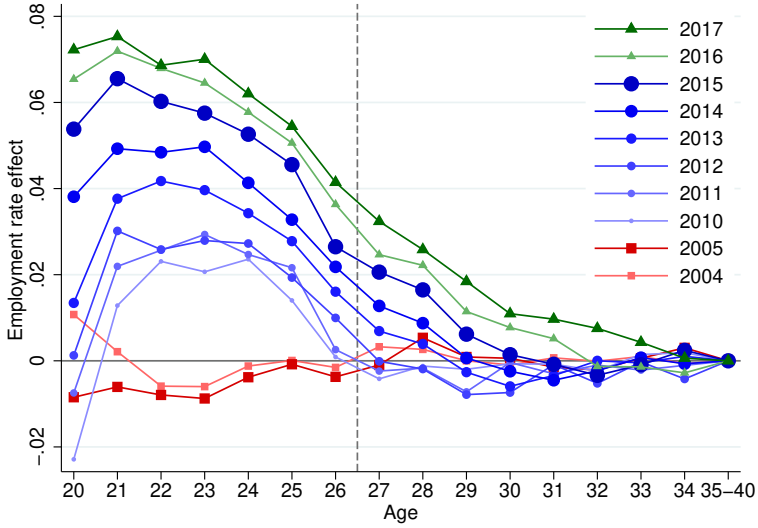
Notes: The top panel depicts the employment rate by age and time periods. The employment rate is the employment to labor force ratio. The employment numerator is all residents employed with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for median wage growth in other years). The labor force denominator is defined as all residents who are either (i) employees as just defined for the numerator; (ii) unemployed defined as having registered with the Unemployment Office at any point during the year. The bottom panel shows the employment rate by age and single years relative to 2006. The normalization is made by aligning multiplicatively the unemployment rate (one minus the employment rate) for ages 35-40 to the 2006 level for each year and then taking the difference in employment rates with 2006. The figure shows a strong and increasing effect of the reform in increasing the employment rate of young targeted workers. The effect does not diminish after the reform in 2016 and 2017. The figure also shows an increase in the employment rate of workers exposed to the reform after they aged out consistent with a hysteresis effect.

Figure 4: The Effect of the Payroll Tax Cut on Employment by Gender

(a) Male employment rates by age and year relative to 2006



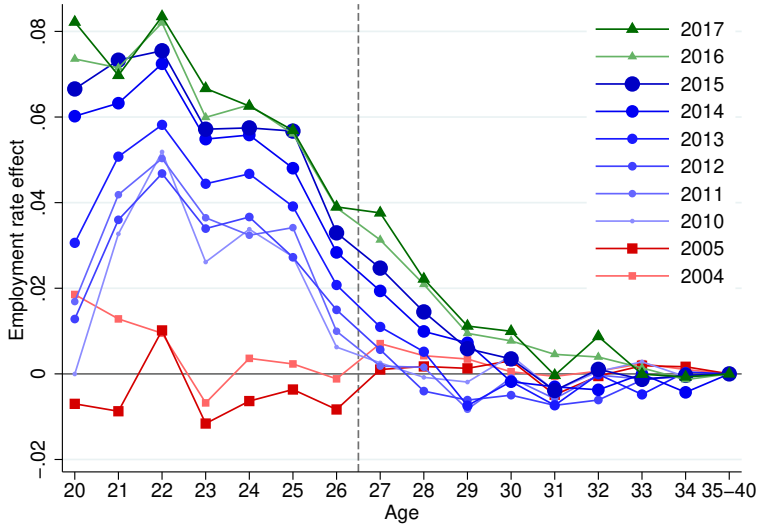
(b) Female employment rates by age and year relative to 2006



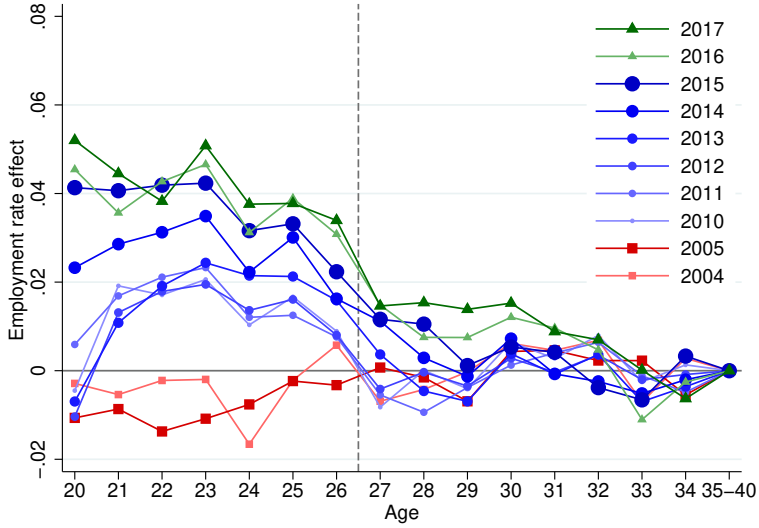
Notes: This figure shows the employment rates by age and year relative to 2006 for males (top panel) and for females (bottom panel). The methodology is the same as in Figure 3, bottom panel, but splitting the sample by gender. The normalization is made by aligning multiplicatively the unemployment rate (one minus the employment rate) for ages 35-40 to the 2006 level for each year and then taking the difference in employment rates with 2006 (raw employment rate series by gender, age, and years are presented in Appendix Figure A2). The scale on the male and female panels is the same for comparison. The figure shows a similar effect of the reform by gender in early years (2010-2012), but the effect on young female workers grows much more in subsequent years and remains much higher for females after tax cut is repealed.

Figure 5: The Effect of the Payroll Tax Cut on Employment by Local Unemployment Rate

(a) High unemployment rate areas



(b) Low unemployment rate areas



Notes: This figure shows the employment rates by age and year relative to 2006 for high youth unemployment rate areas (top panel) and for low youth unemployment rate areas (bottom panel). The methodology is the same as in Figure 3, bottom panel, but splitting the sample by areas based on their youth unemployment rates in 2006. Areas are divided into quintiles weighted by the number of young in the labor force in 2006 so that each quintile includes roughly the same size. The normalization of employment rates is made by aligning multiplicatively the unemployment rate (one minus the employment rate) for ages 35-40 to the 2006 level for each year and then taking the difference in employment rates with 2006 (raw employment rate series by gender, age, and years are presented in appendix Figure A2). The scale on the two panels is the same for comparison. The figure shows a much larger effect of the reform in high youth unemployment areas both during the reform and after the repeal, and also for people who age out of the reform. Hence, hysteresis effects appear much stronger in high unemployment areas.

Table 1: Effect of Payroll Tax Cut on Employment

	(1) Age 20-26 (percentage points)	(2) Age 27-28	(3) Age 29-30	(4) Age 31-32	(5) Age 33-34
Years 2003-2005 (placebo)	0.003	0.003	0.002	0.002	0.001
	(0.019)	(0.004)	(0.004)	(0.004)	(0.004)
Years 2010-2012 (tax cut)	0.015	-0.0004	-0.003	-0.003	-0.003
	(0.018)	(0.004)	(0.004)	(0.004)	(0.004)
Years 2013-2015 (tax cut)	0.032	0.011	0.001	-0.001	-0.001
	(0.018)	(0.004)	(0.004)	(0.004)	(0.004)
Years 2016-2017 (repeal)	0.048	0.023	0.012	0.005	0.0004
	(0.020)	(0.004)	(0.004)	(0.004)	(0.004)
N	156	96	96	96	96

Notes: This table presents the effects of the payroll tax cut on employment rates by periods (by row) and by age groups (by column) using the aggregated times series by age and year displayed in Figure 3 bottom panel. We first divide our "treated" ages into groups as follows: 20-26; 27-28; 29-30; 31-32 and 33-34. We then select each treatment age group together with the control group (ages 35-40) and regress the outcome variable (adjusted employment) on a dummy for being in the treated age group, 6 period dummies and interactions between the treatment-age-dummy and period dummies (excluding 2006 as the omitted category). The table reports coefficients on the interactions. We report conventional OLS standard errors. The treatment effect provides an average effect by age groups and periods using the age distribution of the labor force in 2006. Labor force (LF) is defined as all residents who are either (i) employed with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for median wage growth in other years); or (ii) unemployed (defined as having registered with the Unemployment Office at any point during the year).

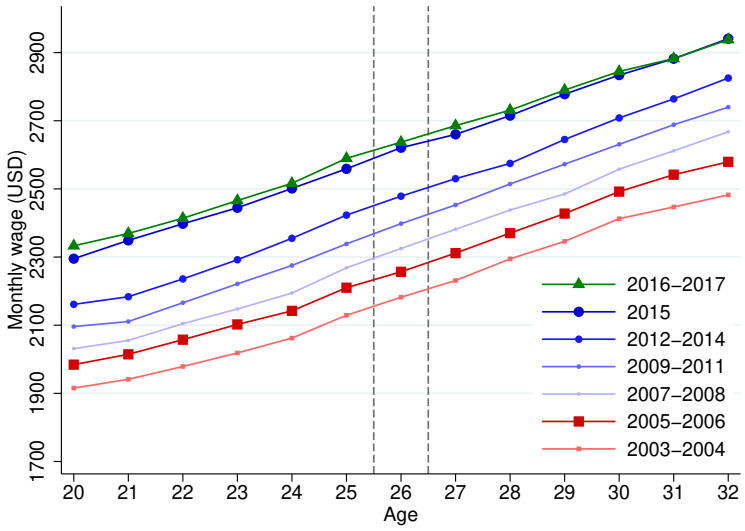
Table 2: Job Creation and Decomposition: Job Year Counts (and Share of Total), by Time Period and Age Group

	Age Groups					
	Directly Treated	*Spillovers*				
Periods	20-26	27-28	29-30	31-32	33-34	All Ages
2010-2012 *Medium Run*	99,019	-916	-6,598	-7,588	-6,826	77,091 (13.1%)
2013-2015 *Long Run*	218,216	23,312	2,651	-3,163	-2,440	238,577 (40.5%)
2016-2017 *Post-Repeal*	213,965	33,083	18,303	7,369	580	273,301 (46.4%)
All Years	531,200 (90.2%)	55,479 (9.4%)	13,886 (2.4%)	-3,382 (-0.6%)	-8,685 (-1.5%)	588,968 (100%)

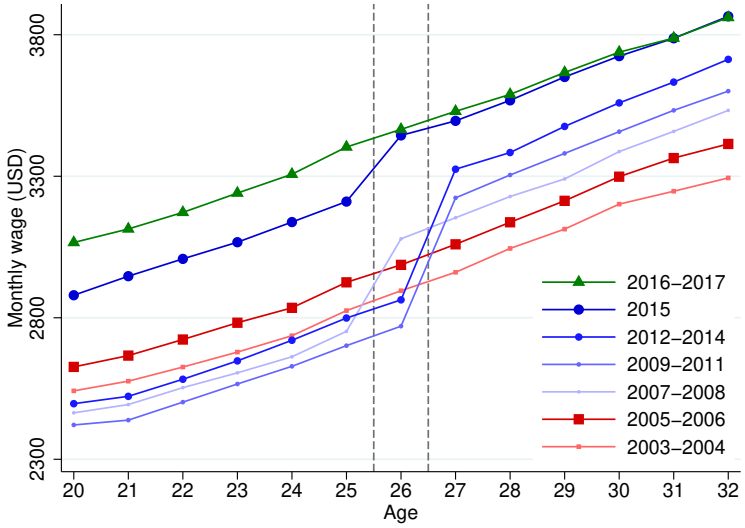
Notes: This table presents an assessment of reform-induced count of job-year creation. We therefore take the age-group and period specific effects γ_{AT} from DiD regression model (1), already reported in Table 1. We multiply it with the labor force body count of age group A from base period T being 2005-6, i.e. $\Delta E_{AT} = \gamma_{AT} \text{LF}_{A,\text{2005-6}}$. In the bottom line of each entry, we report the share of a given entry contributing to total job creation is $\Delta E_{AT}/\Delta E_T$. The last row contains age-specific job creation over all years $\Delta E_A = \sum_T \Delta E_{AT}$. The rightmost column presents total time-specific job creation over all cohorts $\Delta E_T = \sum_A \Delta E_{AT}$. The bottom right entry reports total overall employment effects $\Delta E = \sum_A \sum_T \Delta E_{AT}$ Since our time periods combine multiple years, we also multiply a given time period's treatment effect by its length in years (3, and 2 for the post-repeal).

Figure A1: The Effects on Average Wage for New Hires/Job Switchers

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



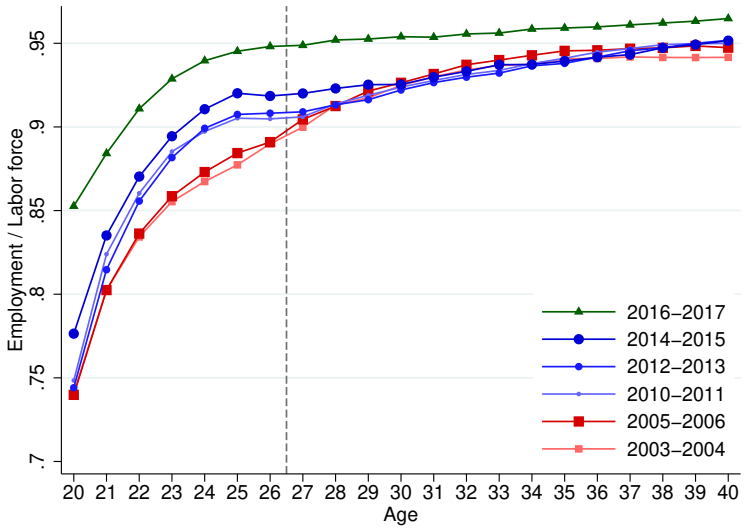
(b) Monthly gross wage (wage earnings gross of the payroll tax)



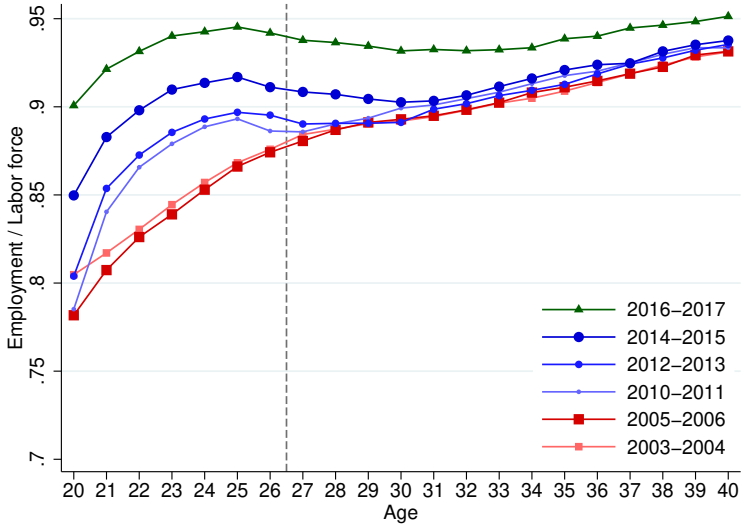
Notes: This figure repeats the average wage statistics displayed in Figure 2 but limiting the sample to new hires or job switchers, defined as having a new firm identifier as the main (i.e., highest paying) employer relative to September of the previous year. It includes both job-to-job transitions as well as new hires among the non employed. As we found in Figure 2, there is no discontinuity in net wages (top panel) and a corresponding discontinuity in gross wages due to the tax differentials. This implies that employers are unable to pass on the payroll tax cut or increase to young workers (relative to older workers).

Figure A2: Employment rates by Age, Years, and by Gender

(a) Male employment rates by age and years



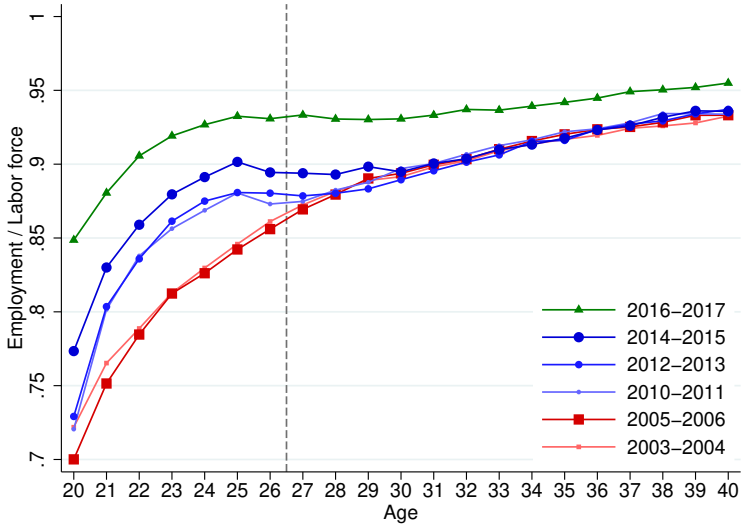
(b) Female employment rates by age and years



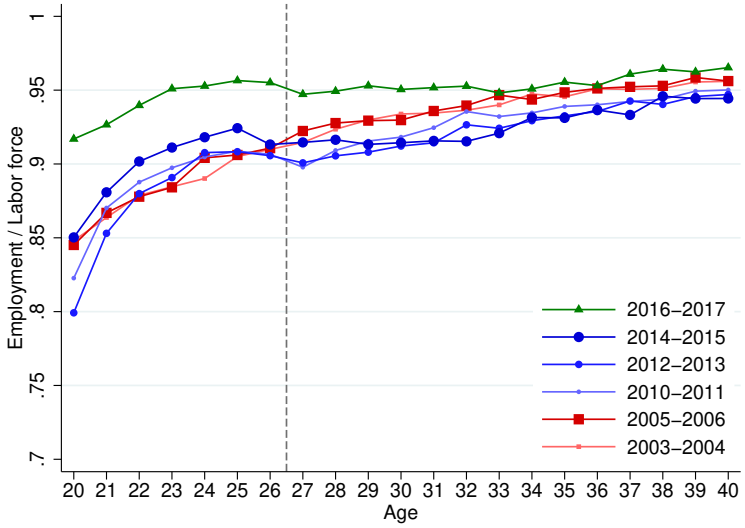
Notes: The figure depicts the employment rate by age and time periods for males (top panel) and for females (bottom panel). The employment rate is the employment to labor force ratio. The employment numerator is all residents employed with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for median wage growth in other years). The labor force denominator is defined as all residents who are either (i) employees as just defined for the numerator; (ii) unemployed defined as having registered with the Unemployment Office at any point during the year. The figure shows a strong and increasing effect of the reform in increasing the employment rate of young targeted workers. The effect appears much stronger for females than for males especially in later years. The figure also show an increase in the employment rate of workers exposed to the reform after they aged out consistent with a hysteresis effect for both genders.

Figure A3: Employment rates by Age, Years, and by Local Unemployment Rate

(a) High unemployment rate areas



(b) Low unemployment rate areas



Notes: The figure depicts the employment rate by age and time periods for high youth unemployment rate areas (top panel) and for low youth unemployment rate areas (bottom panel). The methodology is the same as in Figure 3, top panel, but splitting the sample by areas based on their youth unemployment rates in 2006. Areas are divided into quintiles weighted by the number of young in the labor force in 2006 so that each quintile includes roughly the same size. The employment numerator is all residents employed with annual wage earnings above a small annual threshold ($4,940 in 2012 and adjusted for median wage growth in other years). The labor force denominator is defined as all residents who are either (i) employees as just defined for the numerator; (ii) unemployed defined as having registered with the Unemployment Office at any point during the year. The figure shows a strong and increasing effect of the reform in increasing the employment rate of young targeted workers. The figure shows a much larger effect of the reform in high youth unemployment areas both during the reform and after the repeal, and also for people who age out of the reform. Hence, hysteresis effects appear much stronger in high unemployment areas.