Regulating Paid Time Off: Adverse Selection and Labor-Market Incidence*

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September 2026

Abstract

Almost all OECD countries mandate paid vacation days, yet whether such work-time regulation corrects a market failure has been difficult to answer without exogenous variation in leave across employers. We exploit a 2012 German court ruling that outlawed age-based vacation entitlements, mandating firms that tied leave to age to grant additional paid vacation days to workers under 30. Combining matched employer-employee data with cross-industry variation in the pre-reform discontinuity at age 30, we examine how expanding leave affects worker selection, retention, and pay. Several pre-determined measures of productivity indicate that more generous leave attracts less productive applicants, with an adverse-selection cost that far exceeds the direct cost of the leave, highlighting why markets may under-provide vacation. At the same time we find that the cost incidence of the regulation is mostly on firms, whereas workers benefit, as turnover falls and young mothers return to full-time work sooner. We find no offsetting wage or employment response, ruling out the pass-through predicted by compensating-differentials models.

JEL Classifications: J32, J42, J81

Keywords: Paid-time-off, vacation, amenities, adverse selection

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*We thank Ben Friedrich, Andy Garin, Alex Mas, Lowell Taylor, Nico Ziebarth and seminar/conference participants at UIUC, the Nebraska Labor Economics Summit, ZEW Mannheim, LMU Munich, SOLE Annual Meetings for valuable comments. Zhihan Liu, Pranava Kadiyala, Kala Tiede, Chloe Huang, Haoran Zhang and Jiaan Wang provided outstanding research assistance. We are grateful to Malte Kaukal for facilitating access to the VSE data.

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1 Introduction

Governments around the world regulate how much their citizens work. Ceilings on weekly hours, mandatory rest periods, and near-universal entitlements to paid vacation are among the oldest and most pervasive interventions in the labor market. These policies are often motivated by the common concern that a market left to its own devices grants workers too little time away from work. Support for such policies is also backed up by strong demand for more relaxed work conditions. Surveys find that workers place a high value on amenities like shorter workweeks, more time flexibility, and longer holidays. For instance, Maestas *et al.* (2023) report that workers' willingness to pay for additional vacation days is among the highest out of all the amenities they study. However, this raises a puzzle. If time off is so widely and intensely desired, then would the market not already provide it, making government mandates unnecessary?

The case for such mandates is more nuanced than it first appears. In a frictionless labor market, a minimum-leave requirement mostly relabels compensation: firms recover the cost of the leave through lower wages, and a worker who values the time off at its price would have already negotiated for it (Summers, 1989; Gruber, 1994). Mandated leave can therefore leave its intended beneficiaries worse off by lowering the wages of workers who are not willing to pay for more time off. The argument for intervention rests instead on frictions. Since Akerlof (1976) analysis of the "rat race," economists have recognized that when employers cannot observe how productive a worker is, competition can drive the labor market toward excessive hours and a systematic underprovision of time off.

A long-standing and influential literature has since examined how information frictions distort employment contracts and whether policy can improve market outcomes (Greenwald and Stiglitz, 1986; Summers, 1989; Landers *et al.*, 1996; Stantcheva, 2014; Nekoei, 2022). A recurring mechanism in this work is adverse selection, whereby relaxed working conditions appeal most to the workers who are least productive. A firm that unilaterally offers more time off then draws a worse pool of candidates and experiences a decline in the average productivity of its workforce. This selection penalty pushes the private cost of offering leave above its social cost, and the market outcome has too few holidays and too many hours. Regulation offers a solution. A mandate that raises paid time off at every firm at once leaves workers with nowhere to sort, neutralizing the selection channel and moving the market toward the socially efficient level of time off. The adverse selection problem becomes a central mechanism to rationalize why policy interventions to regulate time off work are needed.

For all its influence, this mechanism has proven remarkably hard to test. Generous leave tends to be found at firms that are attractive on other margins as well, and it is often expanded precisely when employers face tight labor markets and struggle to hire. As a result, the correlation between leave and productivity reflects employer quality and hiring conditions as much as any sorting on productivity. Testing the mechanism instead requires variation in how much time off workers receive that is divorced from the characteristics of the workers and firms involved.

This paper tests for adverse selection in response to additional paid days off by leveraging a 2012 ruling by Germany's Federal Labor Court that prohibited firms from conditioning paid vacation on workers' age. Prior to the court decision, many firms granted additional vacation days to workers who were age 30 and above. The ruling required that leave for younger workers be increased to match the number that had previously only been granted to older workers. The result was an increase in annual vacation for young workers in affected firms by up to four days, equivalent to a 14% increase in PTO days, with no corresponding change for older workers or for workers in firms whose agreements did not contain age-based provisions.

We combine a mandatory firm survey measuring vacation entitlements with matched employer-employee administrative records covering wages, employment, and work histories. Using a regression discontinuity design at age 30, we measure the bite of age-based provisions and construct industry-level exposure to the ruling. The main analysis estimates a continuous difference-in-differences design that compares young workers in industries with more and less exposure to the ruling, before and after 2012. Since the change only affected workers under 30, workers above the age cutoff at the same firms provide a natural placebo group to test for confounders that affect all workers in the same firm.

We first study the effect of the exogenous expansion in paid time off (PTO) on worker sorting and find evidence consistent with adverse selection. Industries more exposed to the ruling experienced declines in multiple measures of worker productivity. After the expansion in PTO, firms in the affected industries hire workers with lower pre-determined AKM worker fixed effects, leading to a net decrease in AKM effects among all young workers at the affected firms. Since the adverse selection mechanism is primarily about unobservable productivity, we show that the same decline holds if we residualize the AKM person effects for observable characteristics with a Mincer regression. These effects are sizable, with an additional day of paid leave reducing the quality of the new hire pool by two log points, creating an additional cost at least twice as large as the direct cost of providing the leave. However, AKM effects could be conflating productivity with taste for amenities. To address this, we build on the literature on employer learning–which shows that post-hire career progress reflects firms learning of unobserved productivity (Farber and Gibbons, 1996; Altonji and Pierret, 2001)– and use post-hire wage growth as an alternative proxy for unobserved productivity. These results confirm that the post-reform cohorts of new hires experience slower career progression after getting hired. The magnitude is similar to our AKM estimates, with wage growth three years out trailing by two log points. Finally, we show that the new cohorts also have lower prior labor-force experience. These results are specific to the young age group, while we see unchanged worker quality among the older placebo group. The results point towards the challenges with the unregulated provision of vacation days: firms that offer more generous leave systematically attract less career oriented and lower productivity workers, creating an additional cost that pushes firms to reduce PTO below the socially desirable level.

At the same time, data on worker flows show that adding more PTO makes these positions more attractive. We show that separation rates among the affected age group drop, while holding steady for unaffected ages. We then zoom in on labor supply of young mothers, a group that has received much policy attention and for whom additional leave is particularly valuable. The results show that the expansion of PTO raises full-time work among mothers of young toddlers (around 2-3 years of age). The increase in full-time work arises right after parental leave ends when the decision of whether to return to full-time work is salient. We see smaller changes for mothers of older children, where employment patterns in the treatment and control group catch-up and level off. We see next to no effects on mothers of younger children (who are often covered by parental leave). These effects suggest that the treatment effect is strongest for mothers who would eventually return to full-time work and get nudged to do so earlier, rather than changing the longer run decision of being a full-time working parent. A placebo test with mothers over age 30, who were unaffected by the reform, show unchanged return to work behavior. These results suggest that vacation can make full-time work more feasible during periods of high non-market time demand.

A final analysis sheds light on the incidence of the costs of the mandate. Despite the increase in mandated vacation, we find no detectable wage or employment effects for affected workers. A back-of-the-envelope calculation implies that full pass-through of the mandate's cost would require a wage decline of 0.7 percent for affected workers. Our estimates of compensating differentials are tightly centered on zero: the 95 percent confidence interval excludes wage decreases larger than 0.05 percent, ruling out not only full but virtually any meaningful pass-through to wages. These null effects are consistent with collective bargaining constraints and possible fears that wage cuts targeted at younger age groups would violate the same anti-discrimination laws that led to the overturning of age based PTO rules. Employment effects are similarly null across all age groups. These null effects are robust to controls for industry-specific time trends, firm size and age trends, and worker fixed effects. The cost of the expanded amenity does not appear to have been borne by workers through the channels predicted by the standard framework. The empirical pattern implies that firms absorbed the cost through some combination of monopsony rents, productivity gains from vacation time, or other departures from competitive labor markets.

This paper contributes to the literature on non-wage amenities by shifting the focus from workers' demand for amenities to the equilibrium supply of amenities. A large body of work measures the value that workers place on job attributes and shows that amenities are a quantitatively first-order component of compensation (Rosen, 1986; Mas and Pallais, 2017; Maestas *et al.*, 2023; Sorkin, 2018; Anelli and Koenig, 2026).[1] In contrast, less is known about the supply side: which amenities firms offer in equilibrium, whether the resulting menu is efficient, and the costs and benefits of policies that expand provision. Theoretical work highlights a reason why the market may underprovide valued amenities. If the workers who most value an amenity are the type that firms least wish to attract, no individual firm wants to be the generous outlier, and valued amenities remain scarce even when many workers would pay for them (Summers, 1989; Nekoei, 2022).

Distinguishing the selection view from the competitive benchmark requires what has been largely unavailable: exogenous variation in a mandated amenity, observed across many employers, with evidence on both who sorts in response and who bears the cost. Two related papers point towards evidence of this type of worker–firm sorting. Ouimet and Tate (2023) show that benefits are often standardized within firms and that a firm's benefit generosity predicts retention of low wage workers. Emanuel and Harrington (2024) provide causal within-firm evidence that remote work attracted less productive workers at a major US call-center. We complement this work in three ways. First, our variation is an exogenous, court-mandated expansion of an amenity, so we can measure the compositional response causally rather than infer it from equilibrium matching. Second, our evidence spans many employers and sectors in administrative matched data, for one of the most widespread and policy-relevant amenities. Third, the same variation lets us estimate the incidence of the mandate, connecting the selection evidence directly to the question of whether intervention helps workers and who pays.

The findings also add a new mechanism to the debate over worker hours. In the standard model, workers choose hours and time away from work by sorting across jobs. A long-standing

[1] The correlation between amenity valuation and unobserved productivity is widely discussed as a challenge for hedonic regressions (Brown, 1980; Hwang *et al.*, 1992; Lavetti, 2023); we treat this correlation not as an econometric nuisance but as the object of interest.

literature questions this benchmark, emphasizing hours constraints, rat-race incentives, tournaments, and nonlinear returns to long hours as reasons why workers may work more than they would want (Altonji and Paxson, 1988; Landers *et al.*, 1996; Goldin, 2014) and recent evidence documents substantial mismatch between desired and actual work hours (Lachowska *et al.*, 2023). Our results provide a selection based explanation for why some desirable work-leisure options may be unavailable in the market. If more generous time-off policies attract workers with lower labor-market attachment or productivity, jobs with such relaxed work-time arrangements will be scarce in equilibrium even when many workers value them. The exogenous vacation expansion we study provides direct evidence that generous work-time policies can generate adverse selection effects.

The paper also contributes to the literature on mandated benefits. The canonical framework asks whether the cost of employer mandates is pushed onto workers through lower wages or reduced employment (e.g., Summers, 1989; Gruber, 1994; Kolstad and Kowalski, 2016). Vacation mandates differ from typically studied benefits in two ways. First, valuations vary widely between workers, which can create the adverse-selection margin discussed above and therefore the scenario in which Summers (1989) highlights that mandates can improve upon market provision. Second, vacation directly reduces annual working time in a given employment relationship, making it a regulation of work quantity, different from regulations that only affect labor cost. This links such regulation to policy on work-hour limits, such as a mandated workweek reduction (Hunt, 1999; Crépon and Kramarz, 2002; Carry *et al.*, 2025). The incidence question is therefore not only who pays for a benefit, but how firms adjust when regulation changes the quantity of labor time embedded in a job. This distinction makes vacation a useful setting for testing whether the standard pass-through logic applies to amenities that affect both worker composition and labor input.

Finally, the paper relates to work on paid leave, including parental leave (see Olivetti and Petrongolo, 2017, for a review) and sick pay (Pichler and Ziebarth, 2020; Maclean *et al.*, 2025). Unlike these event-contingent entitlements, paid vacation is a general-purpose and gender-neutral benefit. Its effects may nevertheless be most relevant for workers with high demand on non-market time. In line with this, we see significant increases in returns to full-time work among mothers of young children after vacation generosity expands, consistent with the broader argument that temporal flexibility is central to mothers' careers (Goldin, 2014). Existing work on vacation has largely been descriptive, focusing on the relationship between vacation, hours, and wages (Altonji and Usui, 2007; Fakih, 2014). We provide quasi-experimental evidence on its provision, the workers it attracts, and the incidence of its cost.

The remainder of this paper is organized as follows. In section 2, we explain the court ruling that mandated additional PTO days for workers under the age of 30 in Germany. Section 3 describes the administrative data used in our analysis. In section 4, we discuss the empirical design and how we identify treatment variation across industries. We report our results in section 5. We conclude in section 6 by summarizing our findings.

2 Institutional Details

We study a 2012 court ruling in Germany that made it illegal for employers to increase the number of paid vacation days based on workers' age. Before 2012, it was common in many firms to grant additional holidays to individuals age 30 and above. For example, in the education and public administration sectors, employees younger than 30 were typically entitled to 26 days of holiday per year, whereas those age 30 and above were entitled to 29 days. However, the Federal Labor Court ruled in 2012 that this discontinuity in the holidays-age profile constituted as age-discrimination against younger workers. While the court case centered on government employees, the ruling applied more broadly to all firms that had similar discontinuities. The court ruling also included a leveling-up provision. Employers that had previously given extra vacation days to older workers had to increase the number of paid vacation days for younger workers to the same level.

The 2012 court decision provides us with multiple sources of variation to identify the labor market effects of mandating employers to provide additional paid time off. First, even before 2012, many firms did not grant extra vacation to older workers and these serve as a control group. The variation enables us to estimate a difference-in-difference comparing treated and untreated firms before and after the court ruling. Second, even among treated industries, there is variation in the magnitude of the discontinuity in PTO days at age 30 with some firms only offering one additional day at the age threshold and other firms offering three days. Third, since the court ruling only increased the vacation days of younger workers, older workers are a natural placebo group to probe the validity of the empirical design.

Instead of digitizing contract records to measure firm specific vacation rules, we use a data driven approach. This approach is based on pre-reform data on vacation days and measures whether vacation entitlements jump at age 30 and if so, by how many days. We implement this with an RDD regression at the 3-digit industry level around age 30. Defining treatment at the industry level is motivated by the fact that vacation rules in Germany are regularly set by industry standards, either enshrined in contract templates provided by employer associations or defined by union agreements. As a result, the vacation rules are relatively uniform within a given industry and the information loss from measuring exposure at the industry level is limited. Taking the industry approach has multiple advantages. First, it allows us to define treatment exposure even for new and small firms with limited observations around the age threshold, where it would be infeasible to estimate firm specific vacation jumps otherwise. Second, it makes it possible to use separate data sources for estimating vacation jumps and labor market outcomes. The outcomes can thus be measured in a much larger administrative dataset where vacation data is not available. The main drawback of using industry level treatment is the loss of statistical power: clustering the standard errors at the industry level reduces the degrees of freedom and the estimation does not exploit within industry variation in firm specific treatment exposure. In the rich German administrative data such power issues are less problematic and we can maintain a reasonable level of power throughout.

2.1 Predictions

To predict the effects of the court mandate, we distinguish between two sets of outcomes: its effect on worker selection and the incidence of its costs.

First, adverse selection models predict that increasing PTO days would reduce worker quality. Nekoei (2022) shows that when productivity is negatively correlated with preferences for amenities, firms may refrain from offering paid time off even when workers' willingness to pay exceeds its direct cost because offering it would attract less productive workers. As a result, the market under-provides the amenity relative to the efficient level. Applied to the German court ruling, this model yields a clear prediction: firms required to provide additional paid time off should attract and employ less productive workers after the mandate. We therefore test whether the productivity of new hires and the overall workforce declines at firms more affected by the reform.

Second, the incidence of the mandate on wages and employment is ambiguous. In the adverse-selection model, firms that provide less generous amenities initially pay a wage premium to recruit more productive workers. By weakening this sorting mechanism, the mandate eliminates part of that premium and therefore predicts lower wages. Similarly, in the canonical model of employer mandates, firms respond to higher labor costs by reducing wages or employment, with the allocation of these costs depending on how much workers value the mandated benefit (Summers, 1989; Gruber, 1994). If workers value paid time off at its cost to employers, the mandate should be fully offset by slower wage growth, with no effect on employment. If workers place no value on paid time off, its cost should instead reduce employment. Existing evidence indicates that workers are willing to pay up to 1% of their salary for each additional paid vacation day, making the assumption that workers place no value on paid time off unlikely (Maestas *et al.*, 2023).

However, several factors make the benchmark incidence predictions less clear in our setting. Legally, firms would violate anti-age-discrimination rules if they hired or paid workers differently based on age, so they may not be able to pass the incidence onto young workers. Moreover, wages could even rise if paid-time off improves productivity by reducing fatigue and burnout, but employers were not exploiting this benefit due to fear of adverse selection. Although no paper directly studies the effect of PTO on productivity, evidence from related literature suggests that this is a plausible mechanism. Collewet and Sauermann (2017) finds that the output of call center workers decreases with working time, and Chunyu *et al.* (2024) finds that paid sick leave policies have positive health externalities that improve labor productivity. Another channel by which PTO can increase wages is if firms have monopsony power. Dube *et al.* (2022) show that when workers view wages and amenities as complements and employers have market power, a mandated increase in an amenity can induce firms to raise wages as well.

In sum, the mandate yields a sharp prediction for worker selection but not for cost incidence. If adverse selection contributes to the under-provision of paid time off, worker quality should decline at firms affected by the mandate. By contrast, the responses of wages and employment are theoretically ambiguous because they depend on workers' valuation of paid time off, institutional constraints on wage adjustment, the effect of PTO on productivity, and firms' labor-market power.

3 Data

Our analysis uses two datasets: the Structure of Earnings Survey (VSE - Verdienststrukturerhebung) collected by Statistics Germany and the Sample of Integrated Employer-Employee Data (SIEED) provided by the Institute for Employment Research (IAB). The VSE data allows us to observe workers' number of paid vacation days, which we use to estimate the first-stage impact of the 2012 court ruling. Leveraging the variation in the magnitude of the first-stage effect across industries, we then use the SIEED to estimate the subsequent impact on the labor market.

3.1 VSE Data on Number of Paid Vacation Days

The VSE is a mandatory survey conducted every 4 years from 2006 to 2018. The data contains detailed labor market information on both employers and workers. At the employer level, the data contains information on firms' size, public sector involvement, and industry. At the worker level, we observe individuals' age, gender, and annual income. A unique variable in the VSE data that is not contained in other administrative records or surveys is that it measures the number of paid vacation days for which each individual is eligible. We use this novel feature of the data to estimate the direct effect of the 2012 court ruling.

The survey is designed to be representative of employment in sampled industries at the national and state levels. To collect the data, the VSE first collects a random sample of establishments, stratified by state, industry, and establishment-size class so that each state is adequately represented. Next, the survey is administered to a representative sample of employees within these establishments. The records are drawn from companies' payroll systems, which reduces the scope for measurement error compared to typical survey responses. While the data is called a survey, it more closely resembles administrative data as responses are mandatory and the response rate varies between 87% and 99%.[2]

The scope of the VSE widens across the four waves. In 2006, the survey excluded three types of firms: employers with fewer than 10 employees, agricultural industries, and the public administration sector. The survey was expanded to include all industries in 2010 and then firms of all sizes starting in 2014. However, the 2006 wave actually surveyed the most people out of all years because it included a one-time census of the entire public education sector. As of 2014, the only workers excluded from the sample are self-employed individuals, which make up about 10-13% of the German labor force during this time period. Aside from changes to the sample population, the industry codes were also updated in 2008.

We take several steps to harmonize the data over time. First, we use a crosswalk to build consistent industry definitions over time. The first year of the data uses a different industry code and we map each of the earlier industry codes to the corresponding 2008 codes that are used in all other years. For industries that were split across more than one target industry, we assign the code to all possible targets and weight each by the likelihood it is the correct one.[3]

A second harmonization step addresses the evolution of the sampling framework over time. First, we omit the agricultural industry from our analysis since data for these firms was only collected from 2010 onward. Second, we drop small firms with fewer than 10 employees as these were only added to the survey after 2014. Third, we drop civil servants from the analysis as their wages and employment are more rigid due to government restrictions. Lastly, we drop part-time workers from the sample to provide a clearer interpretation of the change in paid vacation days.

[2]Response rates were 87% in 2006, 99% in 2010, 98% in 2014 and 96% in 2018.

[3]The weights are the share of workers from an old industry that get classified in the new target industry. This comes from a matrix of worker re-classifications created by Statistics Germany. We drop any matches with $< 2\%$ probability to reduce the computational burden.

Appendix Table 1 compares the full dataset with the private sector in our analysis sample. Note that our restrictions eliminate the sharp increase in the number of sampled firms when small businesses are added to the sampling frame. A final step addresses the sampling frequency within firms. Some employers report data for all workers, while others only report for a (random) subset of their workers.[4] We use sample rate weights to make the sample representative and comparable over time.[5] As a result, the unweighted number of workers in the sample changes, while the weighted number of workers remains stable. The resulting sample covers between 25,000 and 30,000 firms per year, representing approximately 10 million full-time workers.

Although the VSE data contains information on workers' earnings and employment, its relatively small sample size is underpowered compared to the larger German administrative records. We instead use the SIEED data provided by the German Federal Employment Agency to measure the impact of the court ruling on the labor market.

3.2 SIEED Data on Labor Market Outcomes

The SIEED is a matched employer-employee panel constructed from German social security records. By construction, the SIEED data excludes civil servants as they do not pay into social security. To create the panel, the Institute for Employment Research (IAB) first creates a 1% sample of all establishments with at least one employee between 1975 and 2023. Next, the IAB extracts the entire work history of all workers who have ever been employed in one of these core sample establishments. In other words, the SIEED contains not only the full employment history for 1% of establishments, but also partial employment histories for firms in the same connected set. We will use the full sample when we analyzing workers (maximizing power) but when studying firms focus only on core sample firms as we lack full employment counts for the other firms.

Table 1 shows summary statistics of our sample, it covers approximately one million workers between the ages of 21 and 38 (4 million worker-year observations) between 2007 and 2018. The administrative records report individuals' demographics and wages. We use workers' age and industry to measure their exposure to the court ruling. Wages are reported as gross "daily wage", defined as the total wages paid over the year divided by the number of days employed. We deflate wages by the CPI to compare real wages over time. A key

[4]The unweighted number of workers varies over time due to changes in the sampling rate. The share of workers sampled per firm changed from 9% of workers in 2010 to 3.4% in 2014 and 3% in 2018.

[5]If a firm reports data for one in ten workers, each of the worker observations receives a weight of ten. While a worker has a weight of one if data on all workers is reported.

outcome is sorting across 3-digit industries in response to PTO changes and we verify that such patterns are frequent in the data. Around a quarter of workers are new to their respective 3-digit industry in a given year. Such entries to an industry happen either because of labor force entry, or due to job switching. The relatively high churn rate is partially due to the young age groups that we analyze sample and gives us sufficient variation to study sorting effects. For the analysis of the new hire composition of firms we focus on the core sample firms that are consistently observed and have data for approximately 400,000 new hires. One outcome of interest is the AKM worker fixed effect. The IAB provides a separate dataset with estimates of worker and firm AKM fixed effects, computed from the full universe of social security data. There are multiple vintages of these estimates and we focus on the AKM fixed effects estimated with data from the pre-period between 2003-2010 to focus on a pre-determined quality metric that is unaffected by the reform . Given such effects are measured over the 2003-2010 interval, we only observe AKM effects for the subset of workers who have worked in this period. The person AKM effects are measured in log wage units. On the firm side we track outcomes for 26,000 core sample firms. The average firm has eleven workers in the 21 to 38 age bracket, split roughly equally between the treated age bracket (21-29) and the control age (30-38).

4 Treatment Exposure

We estimate the impact of the reform on every 3-digit industry. Figure 1 illustrates this approach with the example of the education and public admin sector. The figure shows the average number of PTO before and after 2012. The data shows a clear discontinuity in paid vacation days at age 30 prior to 2012, with younger workers receiving 4 fewer days than the oldest workers. This discontinuity closed after the court ruling, leading to a uniform 30 vacation days regardless of age. The figure also shows that workers in this industry received an extra day of vacation at age 40. Such discontinuities are less common and typically smaller so our main analysis will focus only on the larger jumps that typically occur at age 30.

To measure treatment exposure, we implement a regression discontinuity design around age 30 with the following specification:

$$holiday_i = \beta_0 + \beta_1 v_i + \beta_2 d_i v_i + \beta_3 d_i + \epsilon_i \tag{1}$$

where $holiday_i$ is the number of vacation days to which worker i is entitled, v_i is worker i's age minus 30, and d_i is a dummy for age greater than or equal to 30. The sample is restricted to data from 2010, the last year before the reform for which we have PTO data and to workers within 8 years of age 30 which is intended to exclude individuals who are still in school. We define an industry as treated if the estimate of the discontinuity, β_3, is significant and positive at the 95% confidence level using robust standard errors.[6]

We estimate equation (1) separately for private sector firms and state-owned enterprises, as they may face different bargaining agreements. An advantage of separating the regression by firm ownership is that we identify some industries that are treated in only one of the two ownership types, but not on average. Since the SIEED data lacks information on firm's ownership-type, we define industries as treated if the employment-weighted average of the discontinuities is significant and use this average effect as the treatment intensity. For the control group, we take industries where neither the public or private sector show a discontinuity at age 30. Industries that are partially treated in one of the two ownership types are excluded from the analysis.

Figure 1 plots the distribution of the discontinuities in the treatment group and shows that the magnitude of the discontinuity varies from 0.5 to 3 additional days of PTO. Our empirical design will exploit this continuous measure of exposure in a continuous difference-in-difference design. Note that since our estimation strategy identifies the jump in holidays at age 30, it may understate the PTO gain for young workers. For example, young workers in Figure 1 gained 4 days of PTO due to the court ruling, but only 3 days relative to workers right above age 30. In such cases, we understate the policy's induced variation. While this effect is salient in this example industry, similar holiday jumps at age 40 are extremely uncommon, making these type of measurement issues a rare exception and we do not make adjustments for it.

We implement three tests to validate that our empirical design reflects a plausibly exogenous shock to PTO days. First, Appendix Figure 1 plots the average number of PTO days by age, separately for treated and control industries, before and after the court ruling. We find that the discontinuity at age 30 is only detectable in our treatment group. Moreover, the figure shows that PTO changes among older workers at age 40 is rare on average, even among the treated industries. To give a sense of the magnitude of the treatment, the figure implies that a 4 day increase in PTO at age 29 corresponds to an approximately 14% increase in PTO days. Second, Appendix Figure 2 plots the age distribution to show that the discontinuity in PTO days is not driven by selection (i.e. bunching) above or below the age 30 threshold.

[6]We have also tried estimating treatment using both finer and coarser industry levels. At the 4-digit level, we do not have enough observations within each industry to precisely estimate a discontinuity. On the other hand, using a 2-digit industry code pools together industries with different discontinuities in holidays and reduces the amount of treatment variation we can exploit.

Lastly, we next use a "hold-out" sample to investigate whether the treatment definition captures genuine discontinuities or merely statistical noise. In principle, given a large enough number of regressions, some of them will naturally estimate a statistically significant discontinuity at age 30 even if there is no discontinuity. Such misclassifications would introduce measurement error in our treatment variable and potentially bias our estimates downward. To quantify the importance of such effects, we test whether the treated industries, identified using only the 2010 data, exhibit similar PTO jumps in the 2006 data. If the treatment classification is only picking up noise, we would expect the jumps observed in 2010 to fade in the out-of-sample pre-reform years. We implement this analysis as a dynamic difference-in-difference, comparing PTO among young workers in the treated industries to the control industries over time.

Reassuringly, Figure 2 shows that PTO days is virtually the same in our "hold-out" data from 2006 as in the original sample from 2010. Moreover, since the VSE draws new samples of firms each round, the fact that we find a similar level of PTO in 2006 suggests that the treatment is not isolated among the specific firms in 2010, but is widespread among firms within the same 3-digit industry. This is consistent with the prevalence of contract templates provided by employer organizations and significant collective bargaining coverage in Germany. The data thus confirms that the results we see in 2010 are indeed externally valid and are representative of the patterns of the pre court-ruling period. Looking forward past the 2012 reform, we see that young workers in the treated industries gain additional vacation days, as required by the court ruling. In Panel B, we test for spurious trends in holidays among older workers who are unaffected by the reform. As expected, PTO remains unchanged in this group throughout.

Appendix Table 2 reports the treated and control industries with the largest number of workers surveyed in the data. The selection procedure determined that the largest industries affected by the 2012 court ruling were hospital, banking, and R&D. On the other hand, industries that did not have a discontinuity in PTO were in programming, trade, and automotive manufacturing. Although the treated and control industries likely differ along multiple characteristics, we show that our results are robust to comparing firms within the same 2-digit industries and find that labor market trajectories were trending similarly in more and less exposed industries prior to the court ruling.

5 Results

5.1 Evidence of Adverse Selection

This section tests for evidence of adverse selection after the court ruling increased the number of PTO days for young workers. Our analysis uses a continuous difference-in-difference design, leveraging variation in treatment intensity across industries. Our identification strategy relies on the assumption that, absent the 2012 reform, the trajectory of firms would have evolved similarly regardless of their baseline age-discontinuity in PTO days. We test this assumption in two ways. First, we show that treated and control industries exhibited similar pre-trends in the years leading up to the court ruling. Second, we examine whether the outcomes for workers above age 30 exhibit similar trends *post-reform*. Since older workers are not directly targeted by the policy, there should be minimal effect on these workers outside of general equilibrium responses and we use the 30-38 year old age group to run a placebo test.

Formally, we estimate the following regression

$$y_{it} = \beta D_{h(i)t} + \alpha_{f(i)} + \gamma_{c(i)t} + \varepsilon_{it} \tag{2}$$

where y_{it} is the productivity of worker i in year t. $D_{h(i)t}$ is the number of extra PTO days available in industry $h(i)$ due to the reform, estimated using the above regression discontinuity design. We control for firm ($\alpha_{f(i)}$) and year ($\gamma_{c(i)t}$) fixed effects interacted with characteristics of individual i. Our preferred specification controls for 2-digit industry by year fixed effects so that our estimates are identified from firms within closely related industries. The coefficient of interest, β, represents the effect of an additional day of paid time off on our outcome. For our analysis on worker quality, we restrict the sample to young workers in the panel of firms for which we have their full employment history. We cluster standard errors at the 3-digit industry level.

A first test studies changes in the types of workers at treated firms using the AKM worker FE. These effects are calculated by the IAB from population wide data covering 2003-2010, restricted to full-time workers (implemented by Bellmann *et al.* (2020) following Card *et al.* (2013)) . The AKM model controls for a cubic trend in worker age interacted with education levels. The worker effects thus capture worker's pay effects relative to other workers in the same age and education bin. We think of these AKM effects as a first proxy for differences in productivity between workers, net of education and age effects. The worker effects capture differences in log(wage) and thus provide a cardinal proxy for log point productivity differences.

Figure 3 plots the estimates of a dynamic version of equation (2), analyzing AKM fixed effects before and after the 2012 reform. These fixed effects are only available for people that worked full-time in the 2003-2010 time window. Towards the end of our analysis period, the sample of workers under 29 year old that meet this restriction becomes thin, and we therefore group the years 2016-2018 into a single final period bin. Panel (a) finds that young workers' AKM fixed effects were stable in the run-up to the ruling and started decreasing after the court mandated additional PTO days. Since the adverse selection mechanism is an extensive margin response, Panel (b) restricts our sample to new hires. We again find a similar decrease in unobserved worker quality after the court ruling. These estimates become less precise towards the later period of our sample.

Next, Figure 4 tests the robustness of our results to a series of stricter specifications. Focusing on new hires, these checks add controls for potential confounding trends to the two-period difference-in-differences of equation 2. The baseline specification controls for firm effects and for 2-digit industry by year fixed effects, which absorb time invariant firm characteristics and the impact of time varying industry wide shocks. These results showed parallel-pre trends, which we probe further by adding linear time trends that vary by firm characteristics. Specifically, we add trends for firm size and age quartiles, for an indicator for an educated workforce, and for average wage quintiles. We ensure that these controls do not condition on an outcome by classifying firms based on their pre-period characteristics. New entrants (with missing pre-period data) are assigned to a separate entrant group. A second check adds linear controls for worker characteristics. In particular, we add linear trends by gender and state of residence. The left panel of Figure 4 plots the results and shows that all three specifications show similar declines in worker AKM effects of around 2 log points for each additional day of PTO.

However, there are two conceptual challenges with our baseline approach. To start, the canonical theory of adverse selection is based on selection on *unobservables*. The lemon's market problem arises precisely because firms cannot observe worker productivity. Empirically, it is of course difficult to measure the unobservable. Although the baseline AKM worker effect measures differences within age and education bins and plausibly captures unobserved productivity components, it may also capture partially observable aspects. We address this in three ways. First, Ouimet and Tate (2023) show that selection on *observables* creates the same adverse selection problem if firms cannot discriminate on these observables. Second, we build a residualized version of the AKM effect, stripping out the role of observables and repeat the same exercise with the "residual" part that is not explained by observables. Third, we use alternative proxies for unobserved ability. Here we build on prior work that shows that post-hire wage growth is informative about learning about worker productivity (Altonji and Pierret, 2001; Gibbons and Katz, 1992). The idea is that wages grow faster when a worker exceeds productivity expectation, while wages stagnate when productivity disappoints.

A second challenge is that differences in AKM effects may capture preferences for amenities, rather than differences in productivity. Workers who value amenities a lot may sort into jobs where amenities are high and wages low. In the AKM approach, this may lead to low worker FE for amenity lovers, and part of the observed decline in AKM effects among new hires may be capturing sorting of amenity lovers into the jobs with more PTO. To address this, we measure worker quality with alternative proxies that do not capture taste for amenities. A first proxy is the post-hire wage growth described above. A second alternative proxy measures the labor force attachment throughout the career of the worker. We measure this as the share of time a worker was employed during their adult life, capturing both unemployment duration and frequency of job loss.

Figure 4 finds similar negative selection effects across all these measures. The first measure uses the AKM effects residualized for key observables. The idea is to capture worker quality that cannot be inferred from easily observable characteristics at the time of hiring. We regress the AKM fixed effects for standard Mincer factors and control for workers education, a cubic polynomial in potential experience, and for state of employment. The residualized AKM effects show a similar decline after the reform is implemented. The decline in worker type thus appears to be unrelated to easily observable worker productivity. The next specifications use workers' labor force attachment as measures of worker quality. We find that after the court ruling, each additional day of PTO decreases the time that workers have been employed since they were 18 by 0.05 percentage points. In our last specification, we use wage growth in the three years after being hired as an ex-post measure of selection. Consistent with an increase in negative selection as a result of the reform, we find that the wage growth of new hires decreased after the policy.

As a placebo check, we repeat this exercise for new hires age 30 and above, the age group not targeted by the reform. The results in Panel B show substantially smaller, mostly insignificant point estimates, confirming that the adverse selection is specific to the group that received extra PTO. Across the board, the placebo point estimates are smaller than what we saw in the treatment group. For AKM effects, estimates are negative before controlling for composition changes, but such effects disappear once we account for changes in the types of firms and people covered by the data. The results for labor force attachment and wage growth are insignificant throughout, sometimes even wrongly signed. In summary, the placebo tests show little evidence of spurious shocks to treated industries that may explain the productivity decrease among newly hired young workers, increasing confidence in the main results.

Taken together, the results show that increasing the number of PTO days changes the composition of new hires and attracts lower quality job seekers to the treated industries. Confirming the adverse selection challenge that makes decentralized market provision of PTO sub-optimal: Firms that offer more generous PTO attract less productive workers, driving up the cost for any firm to unilaterally offer more generous PTO. The magnitude of these costs is sizable. While the confidence intervals include a broad range of possible effect sizes, the point estimates for both AKM effects and post-hire wage growth show that an extra day of PTO shifts the productivity types of new hires down by between two to three log points.

It is important to note that our estimates capture the selection effect when an entire industry adopts more PTO days. Such selection effects would likely be even stronger when a single firm unilaterally increases its paid vacation days without a mandate on its competitors. The results show that selection effects create a sizable wedge between the private cost of offering PTO and the social cost, creating the incentives that result in under-provision of PTO. Workers end up having to choose from a set of jobs that require more work than is socially desirable. The results also highlight the potential power of mandated PTO. By forcing all firms to lift their PTO offering simultaneously, the selection effects are reduced, allowing the market to achieve higher PTO levels at lower cost and potentially creating a more efficient provision of PTO.

5.2 Separation, Retention and Working Mothers

Next we study the impact of PTO on worker separation and retention. If the PTO mandate indeed succeeds in fixing a market failure and creates more desirable levels of PTO, we would expect that, all else equal, workers become more attached to these jobs. We therefore next turn to worker exit rates.

For this analysis, we aggregate the SIEED data to the firm level and estimate the following difference-in-difference:

$$y_{ft} = \beta D_{h(f)t} + \gamma_f + \phi_{c(f)t} + \epsilon_{ft} \tag{3}$$

where y_{ft} is the share of workers aged 21 to 29 who leave firm f in year t. We weight observations by log age group employment plus one to reduce the noise from small cells and fix the weight over time by basing it on the average pre-period employment level. The analysis excludes years when firms shut down. Similar to the worker-level analysis, $D_{h(f)t}$

is the number of extra PTO days in industry h following the court ruling. Our baseline specification controls for firm (γ_f), and firm-characteristic-by-year fixed effects $\phi_{c(f)t}$. The baseline specification again uses 2-digit industry by year fixed effects and hence compares employers within the same sub-industry. Standard errors are clustered at the 3-digit industry level.

Panel A of Table 2 shows a decrease in exit rates among young workers after the court ruling, consistent with the notion that these workers are becoming more attached to their jobs when more generous PTO is available. Column 1 shows the results comparing treated and control firms across all industries. Column 2 is our baseline specification and adds two-digit industry by year fixed effects, thus only comparing firms within the same sub-industry category. Columns 3 and 4 add controls for linear time trends in firm size and age, respectively. The baseline specification shows a decline in the separation rate of around 1.6 percent for each additional day of paid time off.

We next repeat the same exercise for the placebo group of workers aged 30-38. This investigates whether treated firms may have experienced differential shocks that could explain the decline in turnover. The results in Panel B show that exit rates in the placebo age group are unchanged, with small and insignificant results across all specifications. The decline in separation rates are thus unique to the young age group that saw their PTO expand and supports the finding that the PTO mandate made these positions more attractive and improved worker retention. These results also provide a first indication that firms did not reduce the attractiveness of these positions by cutting wages, a point we will return to in the next section.

A sizable literature on parental-leave has analyzed the impact of such policies on the labor force participation of mothers. The response of mothers is also particularly interesting for our setting for at least two reasons. For one, we expect mothers of young children to have particularly strong demand for PTO. Both to be able to flexibly address coverage gaps in childcare, especially among smaller children, and because non-work time is particularly valuable when kids are young. Second, reducing child penalties and improving labor force attachment of mothers has been a policy objective across much of the developed world. While the PTO changes in this study provide a much smaller number of leave days than parental leave policies, PTO days are available past the first few months after birth. Since the challenges of combining parental commitments and full-time work extend beyond the immediate parental leave period, studying the role of PTO is an interesting intervention in its own right.

To estimate the effect of the 2012 court ruling on new mothers, we restrict the SIEED data to women who claim maternity leave between 2007 and 2016 and worked full-time before the leave. To illustrate our empirical design, Figure 5 plots the rate of full time work among mothers aged 21-29 in each of the first five years after childbirth. We follow the previous literature on maternity leave in Germany and define an indicator that turns on if a mother returns to full-time work after child birth (Schönberg and Ludsteck, 2014). Since the IAB variable for full-time status changes over time, we augment that variable and only code a spell as full time if the earnings reach at least 80% of the pre-leave earnings. The figure splits the data into four groups: treated and control industries and births that happen before and after the reform.

By construction, everyone in our sample is on leave in the year the child is born. Few mothers return in the year immediately following childbirth across all groups. The low return rate is consistent with the year-long paid maternity leave benefits that Germany offers. As the kids age, return to full-time work grows. In the pre-reform period, treatment and control industries have roughly similar return rates. After the PTO extension, the rate of full-time work increases among mothers of toddlers (2 to 3 years old) in industries that offer extended PTO. This gap then closes after the children turn four or five, as more women return to work. The uptick in returns is specific to the treated industries, while return rates stay broadly unchanged in the control industries.

We then implement a difference-in-differences analysis analogous to the figure above using the following regression:

$$y_{itk} \ = \ \sum_{\tau=0}^{2} \beta_\tau \left(D_{h(i)} \times \text{Post}_t \times \text{Child}^{\tau}_{itk} \right) + \gamma Post_t \ + \ \phi_{j(i)} \ + \ \alpha_\tau \ + \ \mathbf{X}'_{it\tau}\delta \ + \ \varepsilon_{itk} \tag{4}$$

where y_{itk} is an indicator variable for whether the new mother i has returned to full-time employment, within k years after giving birth in year t. $D_h(i)$ represents the increase in PTO days for individuals in industry h. Post_t is an indicator for whether the child was born after 2012, interacted with the age of the child $Child^{\tau}_{itk}$. $\phi_{j(i)}$ is a firm fixed effect, α_τ is a child-age-band fixed effect, and $\mathbf{X}_{itk}$ collects additional control variables. Standard errors are clustered by 3-digit industry.

The regression is equivalent to a difference-in-difference. The first difference compares, within an industry, women who gave birth before versus after 2012, and the second difference compares industries with more and less exposure to the 2012 court ruling. Given the small sample size, we do not estimate β_τ separately for each year since birth (k) but instead allow β_τ to cover two-year age windows. We group the child's age into three intervals spanning two years each: infants (0 to 1 year), toddlers (2 to 3 years) and young children (4 to 5 years), captured by the three categorical variables $Child^{\tau}_{itk}$.

Consistent with the figure, Table 3 finds that more PTO leads to significantly higher returns to full-time work among mothers of young toddlers. Although there is no detectable impact on mothers of infants nor long-run effects at older ages, all our point estimates are positive. Since the accommodation for working mothers likely differs substantially across workplaces, all regressions include controls for firm fixed effects that capture differences in culture and time invariant amenities at the workplace. Panel (b) repeats the exercise for the placebo group of mothers who give birth between ages 30 and 38 and thus did not gain additional PTO days from the court ruling. We find no increased return to full-time work among this group. This rules out that treated industries experienced a spurious mother friendly culture or amenity shock that coincided with the PTO expansion.

Across the columns, we show that the results are robust to adding controls. Since we have only a moderate sample here, we lack the power to control for 2-digit industry by year fixed effects that was used in the baseline results above. Column (2) replaces the post dummy with birth year fixed effects, and allows them to vary for core sample firms and the "trailing" firms. Recall that the SIEED data samples workers in two steps, starting with a random panel of firms and then following workers in these firms as they move to other employers. We allow different time effects for the two groups in case the trailing firms have selectively different trends. Columns (3) and (4) add demographic group specific trends for tenure, education and age. Column (5) restricts the sample to only firms that are observed throughout all years of the sample period. All of these specifications show similar effects and point to a positive labor supply effect among mothers of young children, precisely the group that we would expect to value PTO most.

5.3 Wage Effects and Compensating Differentials

We next turn to the incidence of the PTO mandate. The canonical view is that firms will pass on the cost of mandated PTO to workers through wage cuts if workers value PTO (Gruber (1994)). Although such mandates are intended to help workers and workers report a high willingness to pay for PTO, mandates may not actually yield significant benefits if wage reductions offset these costs. Such reduction in wages may, however, be hindered by labor market frictions like wage rigidity and anti-discrimination laws. An additional complication is that the PTO mandate reduces the amount of work hours a firm gets from a given size of the workforce. Firms would need to expand their workforce if they wanted to keep labor inputs constant. Like other restrictions on work hours, PTO thus potentially creates a negative shock to labor supply to the firm that further complicates wage setting. As a result, it is unclear how wages respond in practice.

This section tests the effect of the 2012 court mandate on workers' wages. As a benchmark, we establish what kind of wage cut would imply full compensating differential. Using the results on willingness to pay from (Maestas *et al.*, 2023), workers are willing to accept a 0.5% to 1% pay reduction for each additional PTO day. A pay reduction in that range arguably keeps worker utility constant and implies fully off-setting compensating differentials. An alternative benchmark is the full cost pass-through. Firms can keep the labor cost per day constant by reducing workers' wages by about 0.7%.[7] Either benchmark thus implies a wage cut of around 0.7%.

Figure 6 plots our continuous difference-in-difference estimates of the wage effect over time. Although average wages exhibit some fluctuations in the pre-period, they happen in both treated and placebo age groups and neither series shows any clear pre-trends that suggests the parallel trends assumption are violated. In contrast to the benchmark compensating wage differentials, we find no evidence of a decrease in wages after 2012 among treated firms. In fact, the point estimates for young workers affected by the reform appear to trend upward slightly, whereas the wages of older workers remained relatively stable.

To assess the robustness of our results, Figure 7 plots the average post-treatment effect from a series of alternative specifications with additional controls. The first pair of estimates corresponds to the baseline specification in equation (2) where we just control for firm and year fixed effects. Next, we control for age specific cubic time trends to compare wage growth among workers of similar ages between more and less exposed firms. The third specification compares firms within the same 2-digit industries to account for potential industry-specific time shocks.

The last three specifications test the robustness of our results to various sample restrictions. We show that our results are stable to restricting the sample to only firms that exist throughout the entire study period, further restricting the sample to the baseline panel of firms, and adding person-specific fixed effects so that we are following individuals over time. Across all six specifications, we find small positive or null wage effects for young workers. The placebo test using older workers also shows null results. If anything, the effects in the placebo group are more negative, suggesting that any decreases in wage growth are driven by industry specific trends rather than the new policy. Overall, we can precisely reject even very small negative wage effects. The 95% confidence interval in our baseline specification

[7] Back of the envelope calculations using statistics from Statistisches Bundesamt (Destatis) (2026) imply that non-wage costs are around 56% of worker's earnings. Assuming employees work 5 days per week, minus 30 paid vacation days and 10 public holidays, an additional day of PTO would be equivalent to a 0.7% increase in labor costs (i.e. $\frac{1+0.56}{5*52-30-10}$).

rules out wage decreases beyond -0.05% for each additional day of PTO. This is an order of magnitude below the pass-through needed for full compensated differentials. Looking across all specifications the minimum of all lower bounds implies that wages fell by 1% for each added PTO day. However, in this specification, the lower bound coincides with the point estimate of the placebo test, implying that it largely captures an industry wide trend. Thus, even in this case, the results indicate that the PTO mandates did not result in wage cuts for the young age group. Taken together, these estimates show that employers did not pass the cost of the PTO reform onto workers by reducing wages.

5.4 Employment Effects

The null wage response alone does not necessary contradict models of a competitive labor market. For example, if labor supply is highly elastic or if workers do not value PTO days, then the cost of the mandate will primarily be offset by a reduction in employment rather than wages (Summers, 1989). To test for the employment impact of the 2012 court mandate, we aggregate the SIEED data to the firm level and estimate the firm level difference-in-difference specified in equation (3). The outcome y_{ft} is the number of young workers in firm f at time t, divided by the total number of workers in the firm. This variable allows us to interpret β as a percent change in youth employment, while being able to estimate (3) separately by age group.[8] The rest of the specification follows the same logic as before.

Figure 8 plots the estimates of equation (3) over time. We find that employers more exposed to the court ruling did not decrease employment of young workers, nor did they substitute towards hiring more older workers. The pre-trends were stable prior to the court ruling and there is no trend break afterwards. In the long-run, the estimates even suggest a small rise in employment of young workers, but the broader picture suggests null employment effects.

In Figure 9, we show that our result is robust across a range of specifications. As before, we show estimates for the treated age group and the placebo age group side by side. The first specification shows the baseline results again and finds insignificant effects. The results also show no significant effects in the placebo group. Both groups have point estimates very close to zero and the young age group, if anything, does ever so slightly better than the placebo group. The estimates are robust to adding cubic year trends by firm size quartile (column 2), by firm age (column 3), and by state (column 4). Thus, our estimates are not confounded by contemporaneous shocks that differentially affect firms by broad industry group, size, age,

[8] An alternative approach would be to estimate the outcome in logs, but that would not be defined for age-specific employment if the firm does not hire any workers within that age range.

or geography. Overall, our analysis finds no negative effects for workers as a result of the mandated increase to paid time off. Employment among younger workers remained stable and employers appear to absorb the cost of additional PTO days without adjusting wages or employment.

6 Conclusion

This paper studies the labor market effects of a 2012 German court ruling that eliminated age-based discontinuities in paid vacation days, thereby mandating additional paid time off for younger workers. Leveraging cross-industry variation in pre-reform discontinuities, we combine administrative and survey data to estimate the causal impact of this mandate on wages, employment, worker separations, and selection.

Our findings speak to two central questions in the literature on employer mandates. The first concerns whether competitive markets provide amenities efficiently. We find evidence consistent with adverse selection. In industries more exposed to the mandate, multiple measures of worker quality declined after 2012. These patterns highlight a central problem in the market for paid leave: more generous vacation policies attract a less productive applicant pool. This selection effect raises the cost for a firm to unilaterally offer more vacation time and creates a wedge between the socially optimal level of vacation provision and the market outcome. The mandate, by raising vacation entitlements simultaneously across firms, effectively mitigates this selection channel and addresses an externality that individual firms could not internalize on their own.

We find evidence that workers benefit from these changes. After being offered more PTO, turnover falls and workers become more attached to their positions. Such labor supply responses are also evident in the return of mothers to full-time work after childbirth. Young mothers in highly exposed industries returned to full-time employment more rapidly after childbirth than earlier cohorts, with the gap concentrated two to three years post-birth and no analogous shift for mothers above age 30. This finding suggests that paid vacation can complement parental leave policies by smoothing the transition back to work, and help address persistent child penalties for mothers. The response of mothers also is consistent with the idea that groups with the highest demand for paid time off respond most to these changes, and it illustrates one mechanism through which preferences for PTO and productivity may be correlated within the labor force.

The second question concerns the incidence of mandate costs. The canonical framework predicts that the cost of a valued mandated benefit is passed back to workers through lower wages, while a non-valued benefit reduces employment. Neither prediction holds in our setting. Wage and employment effects are small, statistically insignificant, and confidence intervals rule out the magnitudes implied by full pass-through given workers' estimated willingness to pay for vacation. The cost of the additional PTO days appears to have been absorbed by firms rather than transmitted to workers. Our results are reminiscent of findings from the minimum wage literature, which show small employment responses and significant reductions in separation rates in response to increases in the minimum wage (Dube and Lindner, 2024). This pattern points to monopsony power, productivity benefits of the amenity, or other departures from competitive labor markets as plausible explanations.

Taken together, our results suggest that the standard framework in which markets efficiently price amenities and the cost of mandates is borne by workers poorly fits the German experience with paid vacation. Adverse selection appears to drive a wedge between private and social provision, and mandate costs were absorbed by firms rather than passed back through wages or employment. These findings provide an empirical case for cautious optimism regarding policies that expand paid leave entitlements. However, the normative implications of a mandate depends on both the welfare gains from correcting the under-provision of the benefit, as well as the efficiency loss from reducing assortative matching to productive firms. We believe quantifying these two tradeoffs empirically is a valuable step forward for future research.

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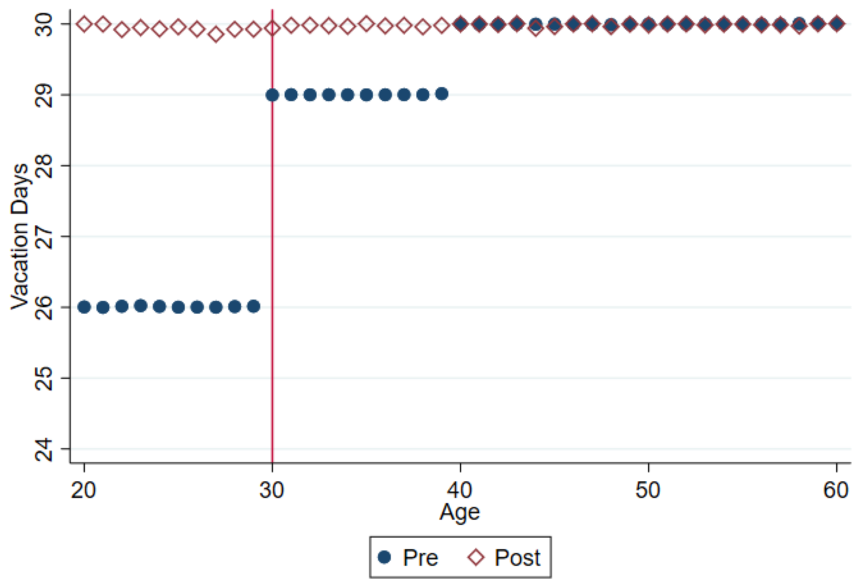
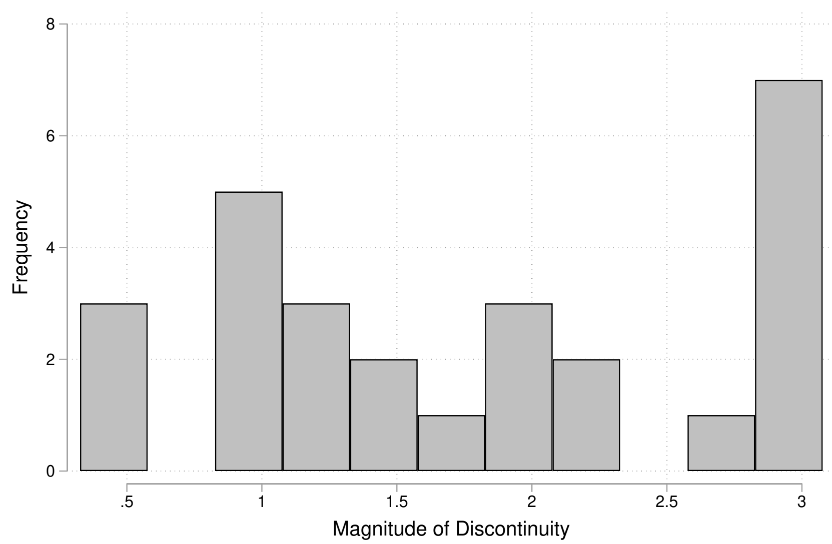
Figure 1: Effect of Court Ruling on Paid Vacation at Age 30

(a) Vacation Days in Education and Public Admin Sector, Before and After 2012

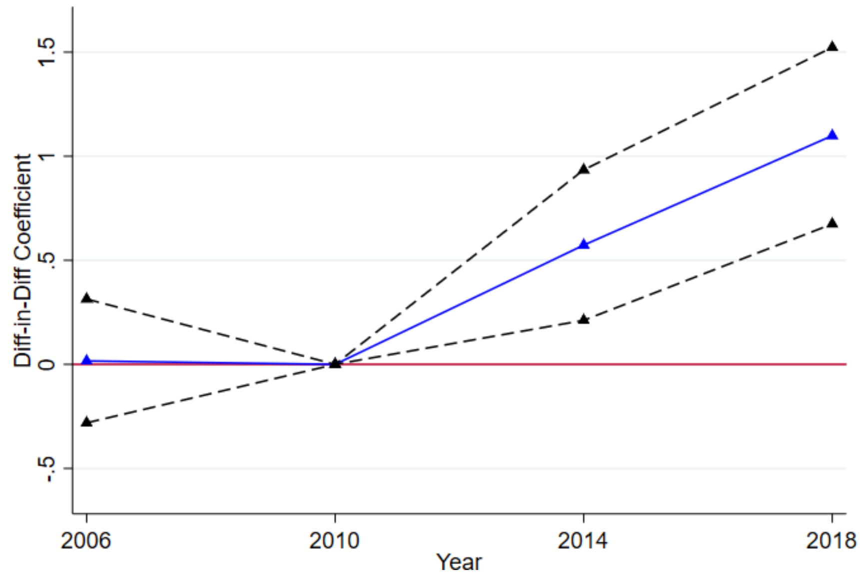


(b) Distribution of RD Estimates Across Treated Industries

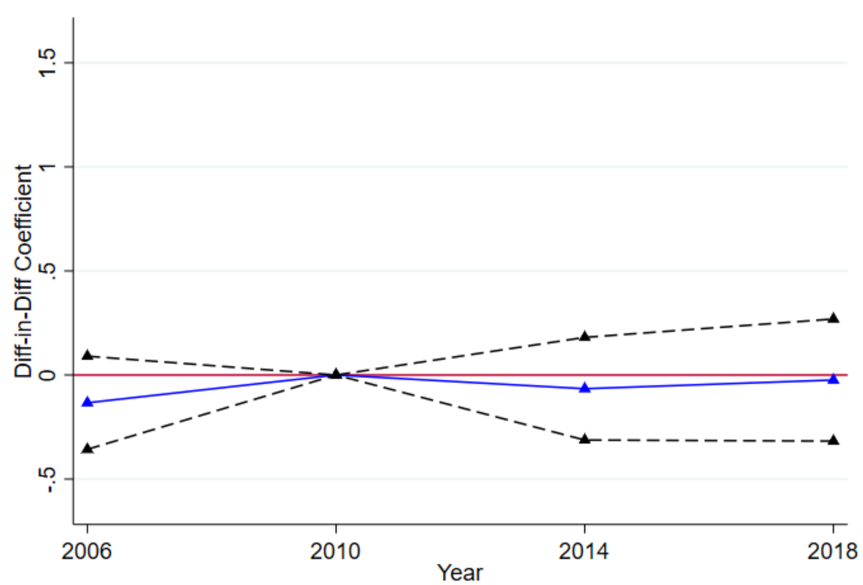


Notes: These figures plot the change in paid vacation days at age 30. Panel (A) illustrates a single case, plotting average paid vacation days against age for workers in the education and public-admin sector, before (circles) and after (diamonds) 2012, using the 2006, 2010, 2014, and 2018 VSE surveys. Panel (B) plots the distribution of RD estimates at age 30 from equation (1), for each 3-digit industry, across all treated industries.

Figure 2: Effect of Court Ruling on Number of Vacation Days



(a) Age 21-29 (treated)

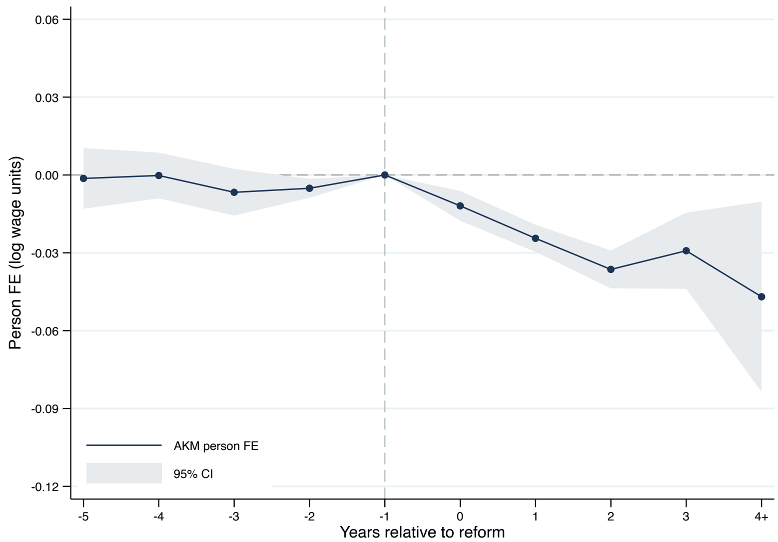


(b) Age 30-39 (placebo)

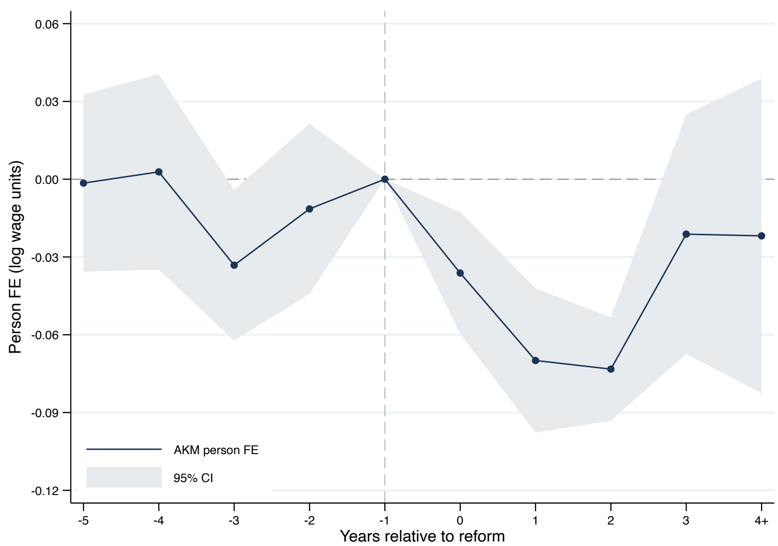
Notes: This figure plots difference-in-difference estimates comparing the number of vacation days between treated and control industries in the VSE data. Due to a data disclosure issue, the most treatment exposed industries are excluded in this output. Panels (a) and (b) restrict the sample to workers of age 21-29 and 30-39 respectively. The number of observations is 481,495 in Panel (a) and 1,005,773 in Panel (b). Standard errors are clustered by firm.

Figure 3: Effect of PTO Mandate on Worker Types (AKM Person FE) at Affected Firms

(a) All workers (Age 21-29)



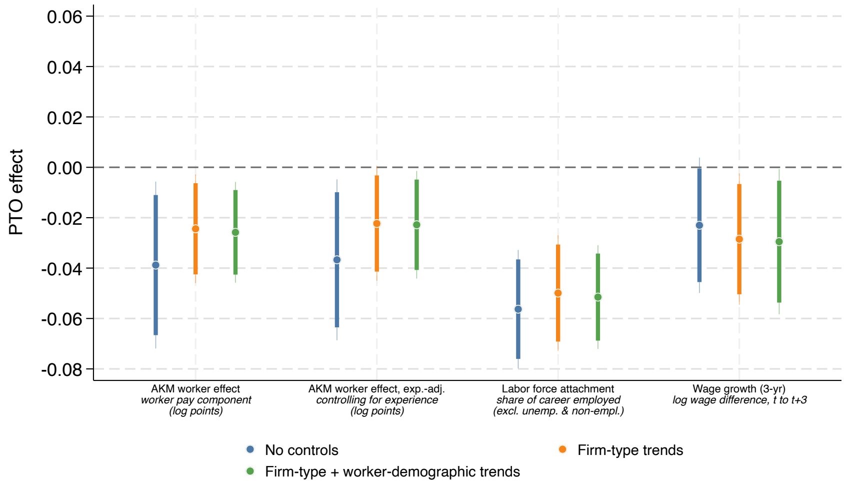
(b) New hires (Age 21-29)



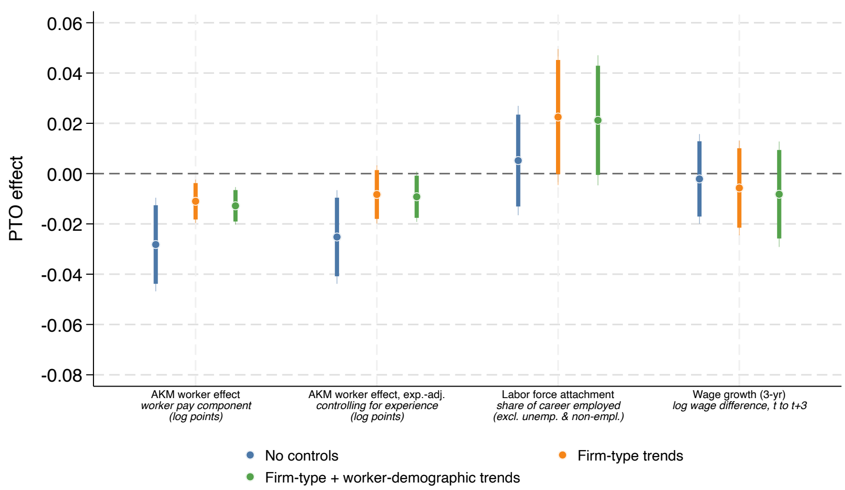
Notes: This figure shows the effect of PTO increases on the types of workers who sort into affected firms. The estimates are based on a continuous difference-in-difference version of equation 2. The measure of worker quality is a workers person AKM FE, estimated in the pre-period (2003-2010) on the full universe of social security records. The DiD estimates come from an event study interacting exposure with event-time dummies, normalized to zero in the year before the court ruling. We group periods t+4 onward into a single period to preserve power as the sample gets thin this far out. All specifications include firm, year, and 2-digit industry by year fixed effects. The sample is restricted to workers aged 21-29 in a balanced panel of establishments; panel (A) includes all such workers (N = 304,963; 207 clusters) and panel (B) restricts to new hires (N = 75,251; 200 clusters). Shaded bands are 95 percent confidence intervals. Standard errors are clustered by 3-digit industry.

Figure 4: Effect of PTO Mandate on New Hire Quality, Alternative Productivity Proxies

(a) Treated Age Group (Age 21-29)

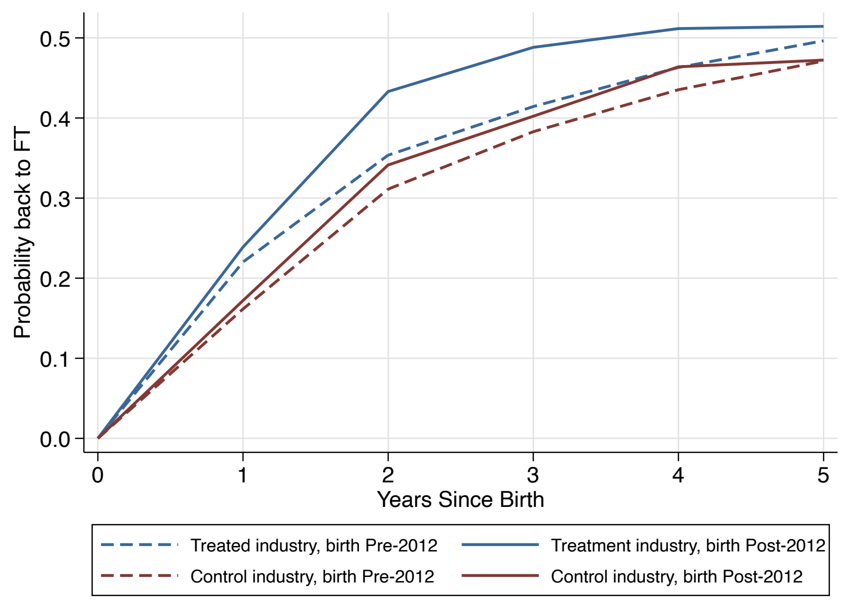


(b) Placebo Age Group (Age 30-38)



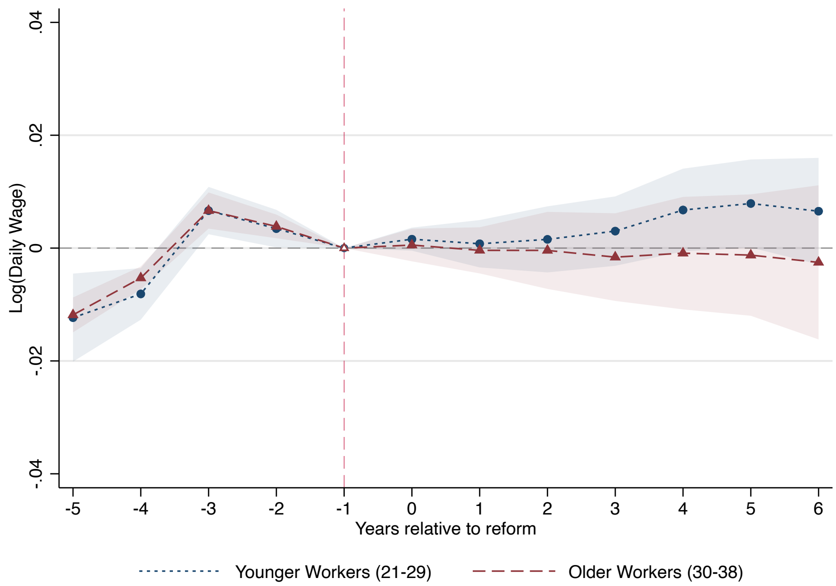
Notes: This figure reports changes in the composition of workers after the 2012 paid time off changes. It uses the dosage difference-in-differences design of equation 2. The outcomes are four measures of worker types: i) AKM person fixed effect estimated with pre-period data, ii) same AKM effects residualized for observable characteristics of Mincer regression, iii) share of days employed since age 18, and iv) wage growth in the three years after being hired (only for stayers). All specifications absorb firm, year, and 2-digit industry by year fixed effects. *No controls* add no additional controls. *Firm-type trends* adds linear year trends for each age group interacted with following establishment characteristics: firm age groups, firm-size quartile, firm skill level group, and firm pay quintile; all are based on pre-period characteristics and firms with missing pre-period data are assigned to an unknown category and have their own trend. *Firm-type + worker-demographic trends* additional adds linear year trends interacted with state and gender. Standard errors are clustered by 3-digit industry.

Figure 5: Mothers' Return to FT Work After Birth, Pre and Post PTO Reform



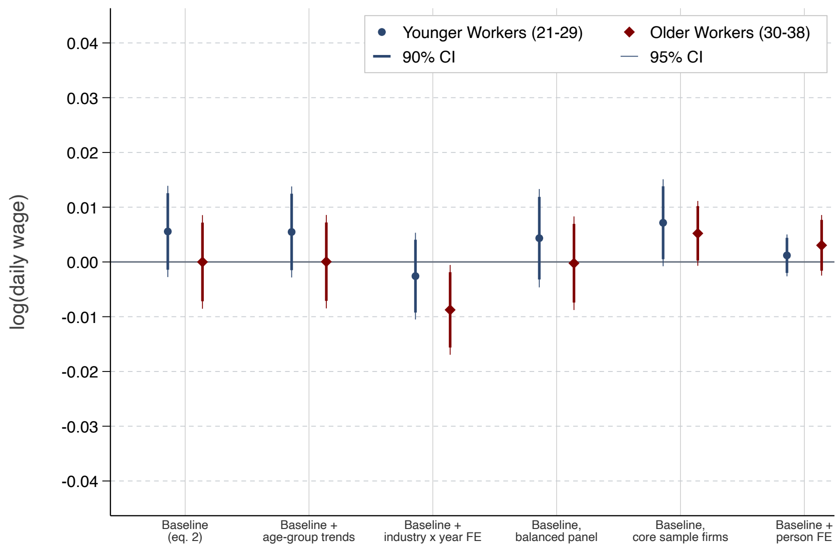
Notes: This figure plots the probability for mothers to return to full time work after giving birth. Full-time status is defined by an indicator that equals one when earnings are at least 80 percent of the individual's pre-leave full-time wage and no part-time status is reported to the social security administration. The sample consists of women aged 21-29 who took parental leave between 2007 and 2016. Blue lines indicate work status of mothers who worked in treated industries and red lines in unaffected industries. The probability of FT work is zero in the year leave begins by construction. The unit of observation is the leave spell. Sample sizes of mothers in this analysis are 840 (Treatment, Pre-2012), 896 (Treatment, Post-2012), 1,870 (Control, Pre-2012), and 1,714 (Control, Post-2012) observations at the time of leave.

Figure 6: Effect of PTO Mandate on Wages



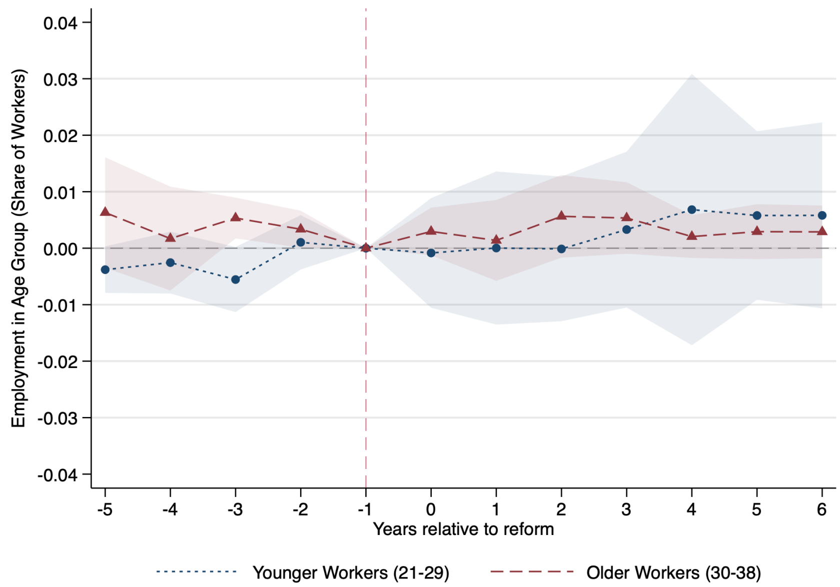
Notes: This figure reports the effect of mandated PTO days on wages of affected workers and the placebo age group. It uses the dynamic dosage difference-in-differences design of equation 2. The outcome is workers' log real daily wage, computed by deflating nominal daily wages to 2015 prices. The reform effects are plotted separately for the age group affected by the reform (aged 21-29) and the placebo age group (aged 30-38). Coefficients are measured relative to 2011, the year before the reform, which is normalized to 0. All specifications include year and firm fixed effects. Shaded bands are 95 percent confidence intervals, and standard errors are clustered by 3-digit industry. (4,448,293 worker-year observations; 211 clusters)

Figure 7: Wage Effects of PTO Mandate: Robustness to Alternative Specifications



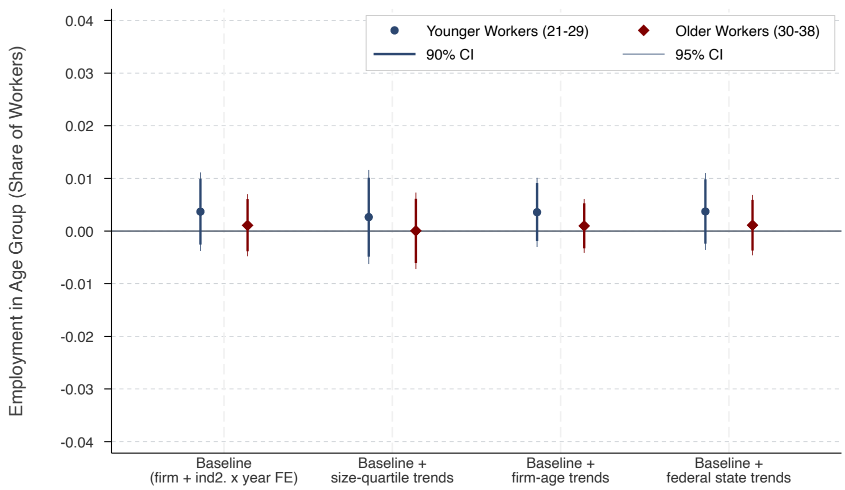
Notes: This figure reports robustness tests for the effect of mandated PTO days on wages of affected workers and the placebo age group. It uses the dosage difference-in-differences design of equation 2. The outcome is workers' log real daily wage, with workers aged 21-38 in years 2007-2018. The sample consists of full-time, full-year workers in regular employment, and the effect is plotted separately for the age group affected by the reform (aged 21-29) and the placebo age group (aged 30-38). Column 1 is the baseline specification as specified in equation (2), with establishment and panel-establishment by year fixed effects. Columns 2 adds age-group-specific cubic time trends to the baseline and column 3 adds 2-digit-industry-by-year fixed effects in column 3. Columns 4-5 applies sample restrictions: to establishments observed in every year from 2007 to 2018, and to establishments in the core sample. Column 6 adds person fixed effects to the baseline. Observation counts are 4,448,293 person-years in columns 1–3; 2,319,704 in column 4; 790,001 in column 5; and 4,237,538 in column 6. Thick bars are 90 percent confidence intervals; thin bars are 95 percent. Standard errors are clustered by 3-digit industry (211 clusters; 208 in column 6).

Figure 8: Effect of PTO Mandate on Age-Group Employment Share



Notes: This figure reports the effect of mandated PTO days on age group's employment shares. It uses a dynamic version of the dosage difference-in-differences design of equation 3. The outcome is a firm's employment in the relevant age group divided by its total employment. Estimates are shown separately for the age group affected by the reform (aged 21-29) and the placebo group of older workers (aged 30-38), with coefficients normalized to 0 in 2011, the year before the reform. The sample spans 2007-2018 and comprises 376,983 firm-year observations (the observation count is the same for the young and older age group analysis). Regressions are weighted by ln(average pre-period employment + 1) and include firm and 2-digit industry $\times$ year fixed effects. Shaded bands are 95 percent confidence intervals based on standard errors clustered at the 3-digit industry level (222 clusters).

Figure 9: Employment Effect of PTO: Robustness to Alternative Specifications



Notes: The figure reports robustness tests for the effect of mandated PTO days on firms' age-group employment shares. It uses the dosage difference-in-differences design of equation 3. The outcome is a firm's employment in the relevant age group divided by its total employment. Estimates are shown separately for the age group affected by the reform (aged 21-29) and the placebo group of older workers (aged 30-38). Column 1 is the baseline; columns 2–4 add cubic time trends interacted with employment quartile (column 2), firm–age group (column 3), and federal state (column 4), always allowing separate effects for the younger (main) and older (placebo) groups. The sample spans 2007–2018 and pools young and older age groups for a total sample of 763,956. Regressions are weighted by ln(average pre-period employment + 1) and include firm and 2-digit industry $\times$ year fixed effects. Thick bars are 90 percent confidence intervals; thin bars are 95 percent. Standard errors are clustered at 3-digit industry level (222 clusters).

Table 1: Summary Statistics: SIEED Matched Employer-Employee Data

	Workers (Age 21-38)			Hires		
	N	Mean	SD	N	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Worker Panel						
Outcome Variables						
Daily wage	4,448,293	101.02	41.46			
AKM worker fixed effect				213,751	4.650	0.354
AKM worker FE, controlling for experience				211,925	−0.032	0.334
Labor force attachment				422,427	9.572	1.043
Log wage growth				186,840	0.377	0.766
Worker characteristics						
Age	4,448,293	31.28	4.506	422,427	28.30	4.621
Female	4,448,293	0.355	0.479	422,427	0.471	0.499
Education	4,409,459	2.499	0.867	400,413	2.598	0.950
Number of days in job	4,448,293	1,558	1,205	422,427	0.00	0.00
Number of days in establishment	4,448,293	1,745	1,334	422,427	0.00	0.00
New to 3-digit industry	4,448,293	0.242	0.429	422,427	0.791	0.407
Treatment						
Dosage	4,448,293	0.375	0.845	422,427	0.594	1.048
Post-2012 (share)	4,448,293	0.571	0.495	422,427	0.619	0.486
Firm characteristics						
Panel establishment	4,448,293	0.178	0.382	422,427	1.00	0.00
Firm size	4,442,939	958.54	3,628	422,072	1,054	2,624
Firm-size group (pre-period)	4,448,293	3.682	0.863	422,427	3.854	0.679
Firm skill level (pre-period)	4,448,293	2.143	0.746	422,427	2.159	0.837
Firm age group (pre-period)	4,448,293	3.052	1.091	422,427	3.083	1.152
Firm pay group (pre-period)	4,448,293	4.275	1.068	422,427	3.733	1.428
Unique workers	1,073,202			381,375		

Panel B: Firm Panel	N	Mean	SD
Worker exit rate	292,945	0.263	0.343
Employment in age group	292,945	5.503	36.33
Total firm employment	292,945	11.42	71.61
Dosage	292,945	0.298	0.711
Post-2012 (share)	292,945	0.495	0.500
Pre-period size quartile	292,945	2.761	1.085
Firm age group	292,945	2.527	0.972
Distinct firms in sample	26,146		

Note: This table presents the summary statistics from the SIEED matched employer–employee data, pooling workers aged 21–38 over 2007–2018. The unit of observation is the person-year in Panel A and the firm-year in Panel B. Both panels are unbalanced. Workers and firms appear only in the years in which they meet the sample restrictions. N is the number of observations; "Unique workers" and "Distinct firms" count distinct individuals and establishments.
Panel A. Columns (1)–(3) is the sample used in the wage analysis; columns (4)–(6) the sample used for the analysis of worker-quality among new hires. Daily wages are deflated to 2015 euros using the CPI. The AKM worker fixed effect and its experience-adjusted counterpart (a Mincer wage residual net of experience) are estimated on pre-reform 2003–2010 data. Labor force attachment is the share of days employed since age 18, multiplied by 10. Log wage growth is the change in log daily wage over the three years following the hire, measured within the hiring firm (for stayers), which requires a three-year follow-up and so is observed only for 2007–2015 hires. Firm size and the firm characteristics that follow are measured at the person-year level. Categorical variables are summarized by the mean of their ordinal code.
Firm groups. All firm-group variables are fixed at pre-reform values and do not vary over time: pre-period firm-size group (quartiles of 2007–2011 employment), firm pay group (quintiles of the 2007–2011 log wage), firm skill level (2007–2011 mean education), and firm age in 2011 grouped (<5, 5–14, 15–24, 25+ years). Each variable adds an "unknown" category for firms with missing pre-period information (the highest categorical value).
Panel B. "Employment in age group" is the number of workers at the firm in the relevant age band (21–29 for the main sample or 30–38 for the placebo) in a given year, and "ln(cell size + 1)" is the natural log of this same count plus one, used as the regression weight. The worker exit rate is the number of age-group workers of year t who separate the next year (present in t but not $t+1$) divided by the firm's age-group headcount in $t-1$. Firm size and firm age are defined as in Panel A.

Table 2: Effect of PTO on Worker Separation Rates

	(1)	(2)	(3)	(4)
Panel A. Younger workers (age 21–29)				
PTO effect	-0.0037^{*}	-0.0160^{***}	-0.0151^{***}	-0.0166^{***}
	(0.0021)	(0.0047)	(0.0046)	(0.0049)
Firms	13,205	13,203	13,203	13,203
Observations	139,419	139,388	139,388	139,388
Panel B. Older workers (age 30–38)				
PTO effect	0.0021	-0.0010	0.0004	-0.0013
	(0.0020)	(0.0043)	(0.0046)	(0.0043)
Firms	14,686	14,684	14,684	14,684
Observations	153,411	153,388	153,388	153,388
Firm fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Industry (2-digit) × Year FE		✓	✓	✓
Pre-period size quartile × Year FE			✓	
Pre-period firm-age quintile × Year FE				✓

Notes: This table reports the effect of mandated PTO days on separation rates, respectively for the affected and placebo age groups. It uses the dosage difference-in-differences design of equation 3. The estimation sample covers 2008-2018 (omitting 2007 when separations aren't defined). Estimates are reported separately for the age group affected by the reform (Panel A, aged 21-29) and the placebo age group (Panel B, aged 30-38). Column 1 reports the baseline estimation with firm and year fixed effects. Column 2 adds two-digit industry by year fixed effects. Columns 3 and 4 control for year-specific employment and age quintiles, respectively. The pre-period runs through 2011 (base year) and the post-period is 2012 onward; the estimation sample covers 2008–2018. Regressions are weighted by $\ln(\text{cell size} + 1)$. Standard errors in parentheses are clustered at the industry (3-digit) level. Observations are firm-year counts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

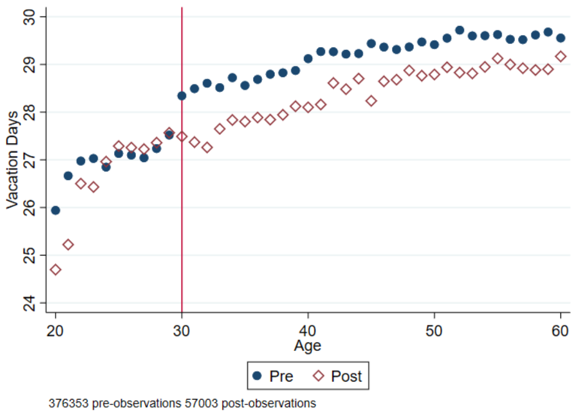
Table 3: Effect of PTO on Full-Time Work Among Mothers

	(1)	(2)	(3)	(4)	(5)
Panel A: Full Time Work among Mothers with Maternal ages 21–29					
PTO Effect on Mothers with:					
× Infant (0-1 year old)	0.0220	0.0211	0.0156	0.0211	0.0174
	(0.0264)	(0.0275)	(0.0284)	(0.0275)	(0.0277)
× Toddler (2-3 years old)	0.0608**	0.0598**	0.0534*	0.0598**	0.0531*
	(0.0265)	(0.0273)	(0.0284)	(0.0273)	(0.0299)
× Young child (4-5 years old)	0.0439	0.0429	0.0367	0.0429	0.0359
	(0.0285)	(0.0287)	(0.0298)	(0.0288)	(0.0317)
Observations	28,706	28,706	28,443	28,706	12,207
Panel B: Placebo test with Maternal ages 30–38					
PTO Effect on Mothers with:					
× Infant (0-1 year old)	−0.0173	−0.0178	−0.0173	−0.0178	−0.0328
	(0.0228)	(0.0245)	(0.0250)	(0.0245)	(0.0320)
× Toddler (2-3 years old)	−0.0082	−0.0086	−0.0083	−0.0086	−0.0238
	(0.0226)	(0.0248)	(0.0256)	(0.0248)	(0.0302)
× Young child (4-5 years old)	−0.0087	−0.0093	−0.0092	−0.0093	−0.0253
	(0.0220)	(0.0242)	(0.0250)	(0.0242)	(0.0288)
Observations	29,613	29,613	29,467	29,613	14,204
Firm fixed effects	✓	✓	✓	✓	✓
Year FE × core firms		✓	✓	✓	✓
Tenure & education trend			✓		
Age-group cubic trend				✓	
Firm sample	All	All	All	All	Balanced

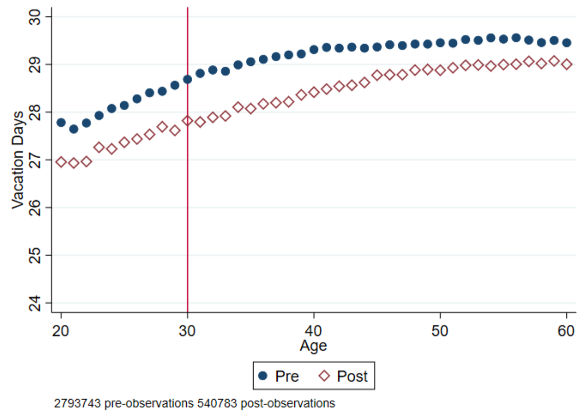
Notes: This table reports the effect of mandated PTO days on the return of mothers to FT work for various ages of the child. It uses the difference-in-differences design of equation 4. Panel A shows results for mothers with maternal ages 21-29 and Panel B for mothers with maternal ages 30-38. The coefficients measure the effect of an additional paid-leave day on the probability of returning to full-time work after child birth. Workers are defined as full-time if both the full-time flag is equal to one and earnings are at least 80% of pre-leave earnings. Ticks indicate the fixed effects/trends absorbed in each column (trends are cubic in birth year); "Firm sample" is the estimation sample of firms, where Balanced restricts to firms continuously observed 2007–2018. Standard errors, clustered by 3-digit industry, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A. Supplementary figures and tables

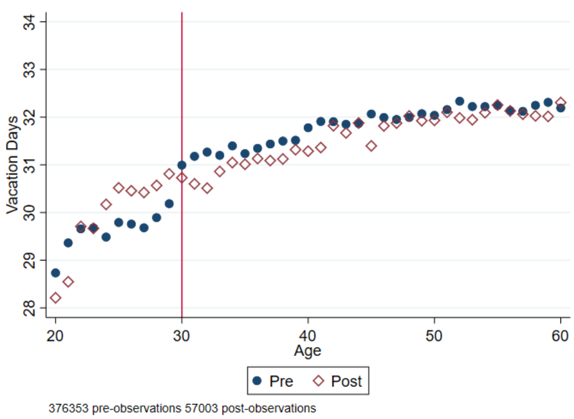
Appendix Figure 1: Vacation Days by Age and Industry in Private Business, pre/post 2012



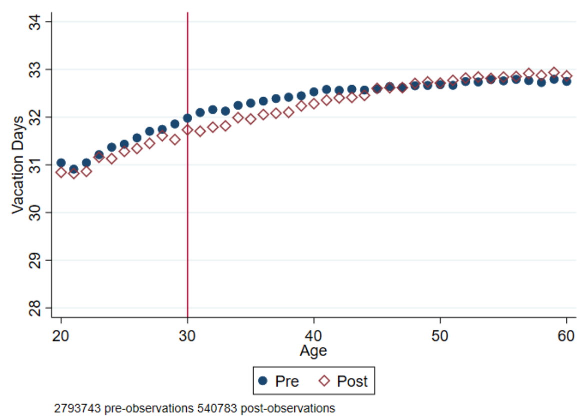
(a) Treated Industries



(b) Control Industries



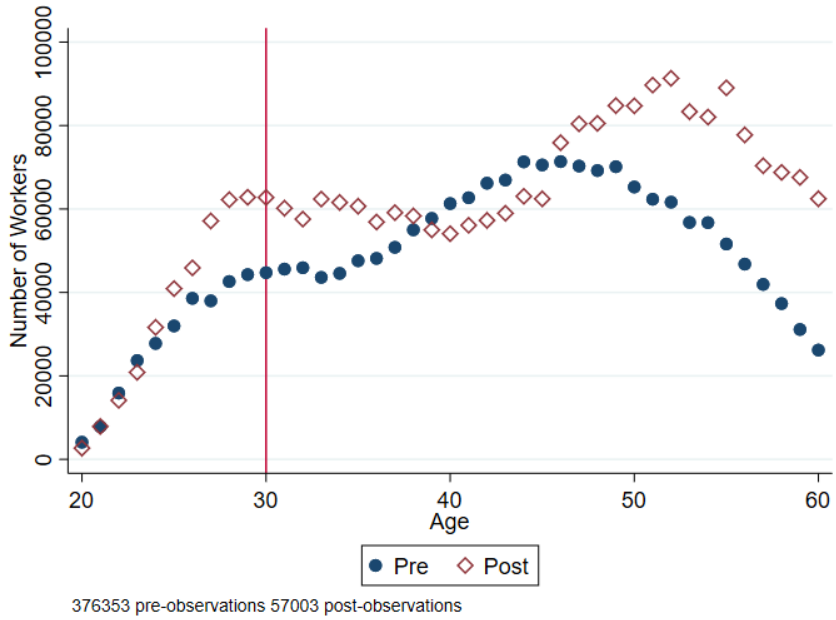
(c) Treated, Within 1-Digit Industry



(d) Control, Within 1-Digit Industry

Notes: This figure plots the average number of paid vacation days as a function of workers' age, separately for before and after 2012. Panels (a) and (b) restrict the sample to firms in the treated and control industries, respectively, as defined in section 4. Panels (c) and (d) residualize PTO days by 1-digit industry fixed effects and worker tenure.

Appendix Figure 2: Age Distribution of Workers, Before and After 2012



Notes: This figure plots the weighted employment count along the age distribution, before and after 2012, computed using the VSE data.

Appendix Table 1: Summary Statistics of VSE

	2006	2010	2014	2018
	(1)	(2)	(3)	(4)
PANEL A: Main Sample				
Share of workers in private sector	1.0000	1.0000	1.0000	1.0000
Share of part-time workers	0.0000	0.0000	0.0000	0.0000
Average age	41.73	42.69	43.76	44.29
Average number of paid holidays	29.26	29.02	28.67	28.24
Average monthly income	3151.54	3355.09	3636.31	4006.74
Average monthly hours	167.53	167.55	169.50	168.63
Share of workers in agriculture	0.0000	0.0000	0.0000	0.0000
Share of workers in manufacturing	0.4491	0.4241	0.3952	0.3820
Share of workers in retail and restaurants	0.2295	0.2888	0.2961	0.2977
Share of workers in high-skilled services	0.2182	0.1611	0.1722	0.1825
Share of workers in education and health	0.0743	0.1034	0.1139	0.1165
Share of workers in other	0.0290	0.0226	0.0225	0.0213
Weighted number of workers	10,027,607	10,222,085	12,003,872	12,787,548
Unweighted number of workers	2,519,225	815,037	329,381	312,639
Weighted number of firms	267,215	311,397	438,473	477,400
Unweighted number of firms	25,076	26,880	30,533	30,004
PANEL B: Raw Data				
Share of workers in private sector	0.9181	0.7594	0.8220	0.8362
Share of part-time workers	0.0053	0.0744	0.0506	0.0465
Average age	40.52	41.82	43.14	43.53
Average number of paid holidays	24.64	24.29	21.97	21.70
Average monthly income	2394.99	2543.18	2458.62	2719.65
Average monthly hours	144.25	142.93	133.49	133.49
Share of workers in agriculture	0.0000	0.1653	0.1268	0.119
Share of workers in manufacturing	0.3297	0.2535	0.2095	0.2015
Share of workers in retail and restaurants	0.2355	0.2489	0.2924	0.2938
Share of workers in high-skilled services	0.2533	0.1356	0.1443	0.1490
Share of workers in education and health	0.1362	0.1664	0.1850	0.1953
Share of workers in other	0.0453	0.0303	0.0420	0.0414
Weighted number of workers	18,622,118	24,221,190	34,134,212	37,572,176
Unweighted number of workers	4,484,220	1,896,775	1,034,053	1,013,690
Weighted number of firms	291,200	348,419	2,090,798	2,271,656
Unweighted number of firms	27,898	32,225	70,727	71,005

Note: This table reports summary statistics from the VSE data. Panel A reports averages of the full dataset. Panel B reports averages for the restricted data after dropping the agricultural industry and small firms.

Appendix Table 2: Largest Treated and Control Industries

PANEL A: Treated Industries	β	*se*	workers	firms
Hospitals [861]	2.16	0.20	7,281	455
Banking [641]	0.56	0.13	7,197	462
R & D [721]	1.01	0.30	3,286	311
Metal Repair [331]	0.48	0.21	3,076	272
Postal Services [532]	1.86	0.38	2,634	369
PANEL B: Control Industries	β	*se*	workers	firms
Programming [620]	-0.15	0.12	11,068	811
Trade (support activities) [522]	-0.09	0.26	6,462	569
Car Manufacturing [291]	-0.08	0.11	6,279	59
Business Admin [701]	0.04	0.21	5,309	547
Manufacture of parts [293]	-0.09	0.13	5,063	238

Note: This table reports the largest treated and control industries in the VSE data, along with the coefficient and standard error of the RD estimate.