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Pay Frequency and Employment Stability

Anthony Lepinteur
University of Luxembourg
and IZA@LISER

Adrián Nieto
Lund University
and IZA@LISER

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Abstract

We study whether pay timing shapes employment stability using staggered state reforms that allow employers to pay less frequently. Combining rich longitudinal labor-market records with multiple complementary data sources, we show that deregulation reduces pay frequency and produces a sharp but temporary increase in employment-to-unemployment transitions, concentrated among workers in the lowest prior-income quartile. The employment separations arise through layoffs, while quits remain unchanged. We provide detailed evidence on the mechanisms underlying this asymmetry. Less frequent pay tightens workers' financial constraints, reduces their use of medical and professional services, worsens their health-care affordability and functional health, and lowers the time they spend working and interacting at work, thereby weakening employment relationships and contributing to employer-initiated separations. To explain the absence of quits, we examine responses on both sides of the labor market. Workers do not increase on-the-job search or interviewing, while employers neither compensate for less frequent pay by increasing earnings nor broadly reduce job openings.

JEL classification

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pay frequency, payday regulation, unemployment transitions, layoffs, quits

Corresponding author

Adrián Nieto

adrian.nieto_castro@nek.lu.se

1 Introduction

A defining feature of modern labor markets is that employment relationships are mediated by adjustment frictions: jobs are formed through time-consuming matching processes, while the dissolution of an existing match destroys relationship-specific surplus and requires workers and firms to return to costly search (Mortensen and Pissarides, 1994). In this environment, research has traditionally placed wage determination at the center of match surplus and employment stability (Hall and Milgrom, 2008; Topel and Ward, 1992). Yet compensation is multi-dimensional. A central but comparatively understudied margin is the timing of pay: The frequency with which workers receive earnings.

Pay timing may affect employment stability through two distinct separation margins: employer-initiated layoffs and worker-initiated quits. On the layoff margin, less frequent pay may tighten workers' liquidity and restrict access to services that support work performance, including health care and time-saving services. The resulting financial, health, and time pressures may weaken employment relationships and lead employers to terminate existing matches. On the quit margin, less frequent pay may make jobs less attractive, prompting workers to search for alternative employment or leave. Whether quits increase may also depend on compensating earnings adjustments by current employers and the availability of outside job opportunities. Establishing whether policies governing pay frequency affect employment stability, and whether any resulting changes reflect involuntary job loss or voluntary worker reallocation, is essential for understanding how compensation design shapes the durability of job matches and the margins through which labor markets adjust.

This paper provides novel evidence on these questions by exploiting the staggered timing of state deregulations of payday requirements, which expanded employers' ability to pay workers less frequently, and generated plausibly exogenous variation in pay frequency across states and over time. Using rich longitudinal microdata from the Current Population Survey (CPS), we follow workers across consecutive months to measure employment transitions and distinguish between employer initiated layoffs and worker initiated quits. We also draw jointly on the American Time Use Survey (ATUS), the Behavioral Risk Factor Surveillance System (BRFSS), and the Job Openings and Labor Turnover Survey (JOLTS) to provide detailed evidence on the channels through which pay timing may shape employer-initiated layoffs and worker-initiated quits.

Our first set of results shows that the reforms operate along the intended margin: weekly payment becomes less prevalent and biweekly payment more common, while monthly payment remains largely unchanged. Employment stability subsequently deteriorates. The probability of transitioning from employment to unemployment rises by 0.9 percentage points in the first year after deregulation and by 0.5 percentage points in the second, relative to an average transition probability of 4 percent. The effects then attenuate, indicating that the deterioration is concentrated in the initial post-reform period. We find no corresponding change in direct transitions between employers and no statistically detectable effect on transitions from unemployment to employment. The response therefore reflects the disruption of existing jobs rather than broader reallocation across firms or a deterioration in unemployed individuals' job-finding prospects. Moreover, the increase in employment-to-unemployment transitions is concentrated among workers in the lowest quartile of the prior income distribution, precisely the group with the least capacity to bridge longer intervals between paychecks using savings or affordable credit.

Our second set of results identifies which side of the employment relationship initiates these separations. When we separate employment-to-unemployment transitions into employer-initiated layoffs and worker-initiated quits, most of the baseline increase is accounted for by layoffs, while quits remain unchanged. Layoffs rise most strongly during the first two years after deregulation and then attenuate, closely mirroring the dynamic pattern of the overall decline in employment stability. This asymmetry is important for interpreting the underlying mechanisms. Workers do not respond to less frequent payment by leaving jobs that have become less attractive; instead, existing employment relationships become more likely to end through an employer-initiated separation. The evidence therefore points away from greater voluntary mobility and toward a weakening of job matches that ultimately affects employers' separation decisions.

Our third set of results examines how the financial pressures generated by less frequent payment contribute to the weakening of employment relationships and increase in layoffs. Using ATUS and BRFSS data, we find that deregulation reduces employed workers' use of medical services, increases the probability that cost prevents them from seeing a doctor, and raises health-related limitations in daily activities. Deregulation also reduces the use of market-provided professional services, including services that allow households to substitute money for their own time. These responses identify two complementary pressures: reduced access to medical care can impair workers' health and capacity to meet job demands, while lower use of professional services can increase the time they must devote to activities outside the workplace. Consistent with these channels, employed individuals spend less time working and socializing at work after deregulation. Because these outcomes are measured among workers who remain employed, they do not arise mechanically from the income loss associated with unemployment. Taken together, the evidence indicates that less frequent access to otherwise unchanged earnings tightens liquidity, worsens health-care affordability, and increases time constraints, reducing effective labour input and weakening workers' connection to their jobs. Together, these pressures weaken existing employment relationships and ultimately lead to more layoffs.

Finally, we investigate the mechanisms underlying the absence of a quit response on both sides of the employment relationship. On the worker side, the null effect could reflect employees not searching for alternative jobs. On the employer side, current employers may increase earnings to retain workers, while outside employers may reduce the availability of job openings. Using ATUS data, we find no statistically detectable change in on-the-job search or interviewing following deregulation, providing no evidence that workers increase their efforts to leave their current employers. We then use CPS data to show that employers do not increase earnings to compensate workers for less frequent pay. Finally, using JOLTS data, we find that state-level job openings remain broadly unchanged. Taken together, these findings indicate that the absence of quits reflects limited worker-initiated reallocation rather than compensating earnings adjustments or a contraction in outside employment opportunities.

A growing literature shows that the timing of income receipt matters for household behavior. When workers face self-control problems or limited savings capacity, less frequent pay can generate pronounced consumption fluctuations between paychecks and impede consumption smoothing (Parsons and Van Wesep, 2013). Consistent with this idea, prior work shows that payment frequency affects consumption, borrowing, and financial stress (Baugh and Correia, 2022), while more frequent payments improve households' ability to smooth consumption over the payment cycle (Aguila et al., 2017; Stephens Jr and Unayama, 2011). Related evidence shows that preferences themselves, such as measured risk aversion, can vary systematically over the payday cycle, particularly among low-income individuals (Akesaka et al., 2023). At the same time, less frequent payments may facilitate the accumulation of resources for lumpy expenditures, increasing investment take-up (Brune and Kerwin, 2019), while the timing of income receipt can also affect the relative consumption of temptation goods (White and Basu, 2016). Complementary evidence from credit markets further highlights the importance of short-term liquidity: payday loans can increase economic hardship (Melzer, 2011), while the timing of repayment obligations affects borrowing behavior and financial outcomes (Carter et al., 2022).

Beyond consumption and financial outcomes, prior research shows that the timing of income receipt can also have important health consequences. Several studies document that income receipt leads to short-run increases in adverse health events and mortality (Andersson et al., 2015; Evans and Moore, 2011, 2012), as well as increases in urgent medical-care utilization (Gross and Tobacman, 2014), drug use and hospitalization (Dobkin and Puller, 2007), ambulance transport incidents (Ibuka and Hamaaki, 2025), and prescription fills (Gross et al., 2022). Other evidence points in the opposite direction for specific transfer programs: food-stamp receipt reduces alcohol-related accidents (Cotti et al., 2016) and emergency-room visits (Cotti et al., 2020). Using administrative registry data from South Korea, Jeon et al. (2025) similarly show that pension income receipt increases hospital admissions but reduces short-run mortality, highlighting that the health effects of income receipt need not operate uniformly across outcomes. More broadly, these effects may reflect pronounced intra-month responses to liquidity, as income receipt shifts expenditures and consumption (Goldin et al., 2022; Hastings and Washington, 2010; Mastrobuoni and Weinberg, 2009; Stephens Jr, 2003, 2006), caloric intake (Shapiro, 2005), and diet quality (Kuhn, 2018).

Beyond its direct effects on health and household finances, the timing of income receipt can have broader consequences for individual behavior and economic outcomes. Predictable liquidity cycles have been shown to affect high-stakes outcomes such as academic performance (Bond et al., 2022) and crime (Carr and Packham, 2019; Foley, 2011). More directly related to the labor market, recent work shows that greater flexibility in accessing earned income increases work time (Chen et al., 2025), while income receipt increases worker productivity by alleviating short-term financial concerns (Kaur et al., 2025).

Our paper contributes to these literatures in three main ways. First, we bring pay timing into the study of employment dynamics. By showing that less frequent access to otherwise unchanged earnings affects transitions into unemployment, we establish the temporal structure of compensation as a consequential contractual margin and extend the literature on income timing to labor-market flows. Second, we demonstrate that the consequences of pay-frequency regulation are highly uneven. The employment effects are concentrated among lower-income workers, who have the least capacity to bridge longer intervals between paychecks. Third, we connect household financial constraints to firm behavior. By distinguishing worker-initiated from employer-initiated adjustment, we show how pressures experienced by workers can enter match surplus and influence firms' decisions to maintain employment relationships. Fourth, we provide a detailed mechanism analysis that allows us to distinguish among competing explanations for changes in employment flows and develop a coherent account of how pay timing affects match stability. Taken together, these contributions recast payday requirements as labor-market institutions that meaningfully shape employment dynamics, rather than as merely administrative rules governing payroll.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 presents the datasets and construction of the estimation samples, and Section 4 outlines the empirical strategy. Section 5 examines the effects of payday deregulation on workers' pay schedules and employment stability, with particular attention to their distributional incidence. Section 6 distinguishes between employer-initiated layoffs and voluntary quits and investigates the mechanisms underlying each response. Section 7 concludes.

2 Institutional Background

2.1 Pay Frequency in the United States

Pay frequency in the United States is governed primarily by state wage-payment laws. The federal Fair Labor Standards Act regulates minimum wages, overtime compensation, and certain recordkeeping practices, but it does not require employers to pay workers weekly, biweekly, or at any other uniform frequency. Federal regulations generally require overtime compensation to be paid on the regular payday for the period in which it was earned, but they do not establish a nationwide pay schedule. Consequently, state law determines the maximum permissible interval between regular wage payments for most workers.

The four principal pay schedules are weekly, biweekly, semimonthly, and monthly. Weekly workers ordinarily receive 52 regular paychecks per year, while biweekly workers are paid every other week and receive 26. Semimonthly workers are paid twice per calendar month, generally on predetermined dates, and receive 24 paychecks. Monthly workers receive 12 regular paychecks per year.

All four schedules are common in the United States. In 2023, 27.0 percent of private establishments covered by the Bureau of Labor Statistics measure paid weekly, 62.8 percent paid biweekly or semimonthly, and 10.3 percent paid monthly (U.S. Bureau of Labor Statistics, 2023).

These patterns reflect both technological and economic considerations. Running payroll entails fixed administrative costs, including calculating hours and deductions, withholding and remitting taxes, maintaining records, and transmitting payments. Less frequent payroll therefore reduces the number of payroll runs. At the same time, employers may face adjustment costs from changing payroll systems or providing legally required notice. A relaxation of a state requirement therefore creates an option to pay less frequently, rather than requiring every employer to adopt the newly permissible schedule.

2.2 State Payday Requirements

State payday statutes typically establish a minimum payment frequency or, equivalently, a maximum permissible interval between regular wage payments. They do not generally prevent an employer from paying more frequently. For example, a statute allowing biweekly payment permits an employer to retain a weekly schedule.

The requirements of these statutes vary substantially across states. Some impose a generally applicable biweekly rule, while others prescribe weekly or monthly schedules. Other states do not specify a general frequency and instead require employers to establish and provide notice of a regular payday.

We construct the policy series using the annual State Payday Requirements tables assembled by the Wage and Hour Division of the U.S. Department of Labor. The Department of Labor tables provide useful standardized information of state requirements.

During the 2003–2024 period, nine state-level reforms relaxed payday requirements. These reforms reduced minimum payment frequencies and allowed employers adopting longer pay cycles. Their staggered adoption across states and years generates multiple treatment episodes and variation between treated, not-yet-treated, and never-treated states. The deregulation reforms expand employers' discretion rather than requiring a change in pay frequency, so their effects depend on employer take-up. For example, an employer may continue paying weekly after biweekly payment becomes permissible. The reform simply makes a previously prohibited schedule legally available. Accordingly, we interpret these reforms as payday deregulations because they expand employers' feasible payroll schedules. The estimated effect of the legislation is therefore a reduced-form effect of relaxing the legal constraint.

Pay-frequency requirements are enforceable provisions of state wage-payment law. Enforcement mechanisms vary across states and may include investigations by state labor departments, administrative wage claims, orders to pay late or unpaid wages, and civil penalties.

When adopting less frequent pay schedules, employers reduce the number of payroll runs, while workers receive their salary in fewer installments and wait longer to access accrued wages. These longer intervals can make it harder to smooth expenditures, particularly for households with limited liquid savings or access to affordable credit. These households may postpone purchases, medical care, and other services until their next paycheck. This financial strain thus provides a channel through which the timing of unchanged earnings can affect individual behavior and employment stability.

3 Data Sources

3.1 Current Population Survey Data

Our primary source of labor-market data is the monthly Current Population Survey (CPS), a nationally representative survey of the U.S. civilian noninstitutional population conducted by the U.S. Census Bureau for the Bureau of Labor Statistics. The CPS is the source of the official U.S. unemployment rate and is specifically designed to measure employment, unemployment, and labor-force participation.

The CPS interviews approximately 60,000 households and collects information on roughly 100,000 individuals each month. For every respondent aged 16 or older, the survey determines labor-force status during a precisely defined reference week. It distinguishes individuals who are employed and at work, employed but temporarily absent, unemployed, and out of the labor force. The survey further records respondents' state of residence, allowing us to assign the payday regulations in effect in each state and month. Its large sample, nationwide coverage, and frequency are particularly valuable in our setting: they provide sufficient variation across states and over time while allowing us to examine whether labor-market effects appear following deregulation and how they evolve thereafter.

For unemployed respondents, the CPS records the reason for unemployment. The latter distinguishes layoffs, voluntary job leavers, workers whose temporary jobs ended, and other job losers. These categories are central to our analysis because they allow us to determine whether changes in unemployment transitions following deregulation reflect primarily employer- or worker-initiated separations.

The CPS rotating-panel design provides an additional advantage. A sampled housing unit is interviewed for four consecutive months, leaves the sample for eight months, and then returns for four additional interviews. We link respondents across consecutive interviews to construct individual-level transitions among employment, unemployment, and nonparticipation. Among respondents who remain employed, the CPS also records whether they continue to work for the same employer and in the same job as in the preceding month. We use this information to distinguish continued employment relationships from job-to-job transitions. The longitudinal structure therefore allows us to move beyond changes in the stock of unemployed workers and identify the labor-market flows responsible for those changes.

In their fourth and eighth months in the survey, which are the outgoing rotation groups, employed wage-and-salary workers answer an additional set of questions about their main job. These questions cover usual earnings, and the receipt of overtime pay. Respondents are also asked to identify the period in which it is easiest to report their total earnings before taxes and other deductions. The available responses are hourly, weekly, biweekly, twice monthly, monthly, annually, and another period. We use this earnings-reporting periodicity as a proxy for pay frequency and examine changes in the probability of reporting each of the principal pay intervals: weekly, biweekly or twice monthly, and monthly. The reporting intervals map directly onto standard payroll cycles, and workers naturally report earnings over the period in which they receive them. We therefore interpret the reported interval as a meaningful measure of pay frequency.

We use monthly CPS data from 2004 through 2023. The long sample period spans multiple state reforms, while the CPS data allows us to trace labor-market responses around their implementation with considerable precision. We restrict the sample to individuals aged 16 to 64, thereby focusing on the working-age population most directly exposed to employers' pay practices. Our main sample consists of 2 million observations.

Appendix Table A.1 reports summary statistics for the two CPS samples used in the analysis. In the employment-stability sample, monthly transitions from employment to unemployment occur for about 4.0 percent of observations, while employer changes occur for 4.3 percent. Layoffs account for 2.6 percent of observations, whereas quits and the ending of temporary contracts are considerably less frequent. In the pay-frequency sample, weekly payment is the most common way of reporting earnings, accounting for 56.4 percent of observations, followed by biweekly payment at 30.0 percent and monthly payment at 13.6 percent. Roughly one quarter of observations in both samples are located in states that eventually deregulate payday requirements over our sample period. The demographic composition of the two samples is also very similar, with an average age of about 42, an approximately even gender split, and comparable shares of White and university-educated respondents. Overall, the close similarity in observable characteristics across the two samples suggests that the outgoing-rotation-group restriction underlying the pay-frequency analysis does not substantially alter the composition of the CPS population used in our main labor-market analysis.

3.2 American Time Use Survey Data

To investigate the behavioral mechanisms underlying our labor-market results, we use the American Time Use Survey (ATUS), sponsored by the Bureau of Labor Statistics and conducted by the U.S. Census Bureau. The ATUS is a nationally representative time-diary survey drawn from households completing their final Current Population Survey interview. From each household selected for the ATUS, one member aged 15 or older is randomly chosen for interview. We use data from 2004, the first year of the survey, through 2023.

Each respondent reports the complete sequence of primary activities undertaken during a single 24-hour period, from 4 a.m. on the diary day to 4 a.m. on the interview day. For each activity, the ATUS records its start and end times, duration in minutes, location, and, for most activities, who was present. Activities are classified using detailed six-digit activity codes. The diary format captures behavior on a specific day rather than respondents' perceptions of their usual time use, allowing us to measure both whether an activity occurred and the number of minutes devoted to it.

This level of detail makes the ATUS particularly useful for examining mechanisms. It identifies time spent searching and interviewing for jobs, including search undertaken by individuals who remain employed. It also measures activities that may respond to short-run liquidity and time constraints, including consumer purchases; medical and care services; professional and personal services; and household services. We can therefore examine whether less frequent pay alters on-the-job search, the use of market-provided services, and the allocation of time to multiple market and non-market activities. These measures capture time devoted to obtaining or using services rather than expenditures, providing direct evidence on behavioral engagement with these activities.

The ATUS reports the exact date and day of the week covered by each diary, while its link to the CPS provides respondents' state of residence and detailed demographic and labor-market characteristics. These include age, gender, race and ethnicity, education, marital status, household composition, presence and ages of children, employment status, hours, earnings, industry, and occupation. The combination of state and diary date allows us to assign the payday regulations in effect when each respondent's activities occurred and to compare time allocation around the implementation of state reforms.

As in the CPS analysis, we restrict the sample to individuals younger than 65 years old. We additionally focus on respondents who are employed at the time of the ATUS interview. These workers are directly exposed to employers' payment practices, and their behavior is informative about mechanisms operating within ongoing employment relationships, including job search before a separation and adjustments in consumption-related activities. The resulting sample contains 127,348 respondent-day observations.

Appendix Table A.2 reports summary statistics for the ATUS analysis sample. Respondents spend, on average, about 256 minutes per day working. Regarding the use of market-provided services, 2 percent of respondents use medical services and 6.6 percent use professional services on the diary day, while the table also reports participation in childcare, cleaning, meal-preparation, and repair services. About 0.4 percent of respondents report engaging in job search and 0.1 percent in job interviewing. Around 25 percent of respondents live in states that deregulate payday requirements during the sample period. The sample has an average age of approximately 42 years, is roughly evenly split by gender, and 42 percent of respondents have completed university education.

3.3 Behavioral Risk Factor Surveillance System (BRFSS)

To examine whether payday deregulation affects health-care access and daily functioning, we use the Behavioral Risk Factor Surveillance System (BRFSS). Coordinated by the Centers for Disease Control and Prevention and administered by state health departments, the BRFSS is a large, state-based, repeated cross-sectional telephone survey of the U.S. adult population. Interviews are conducted continuously throughout the year using a standardized questionnaire. We use data from 2005 through 2023.

The BRFSS provides several outcomes that complement our labor-market and time-use evidence. Respondents report whether, during the preceding 12 months, they needed to see a doctor but could not because of cost, providing a direct measure of unmet medical care attributable to financial constraints. The survey also records whether a physical, mental, or emotional condition makes it difficult to perform errands alone, allowing us to examine effects on daily functioning. More generally, the BRFSS helps us determine whether the consequences of less frequent pay extend beyond labor-market outcomes to financial barriers to medical care, functional limitations, and health-related behavior.

The BRFSS reports respondents' state of residence and interview date, allowing us to assign the payday regulations in effect when each respondent was interviewed. It also provides detailed demographic and socioeconomic characteristics, including age, gender, race and ethnicity, education, marital status, employment status, income, and household composition.

We restrict the sample to respondents younger than 65 years old. Because the BRFSS surveys adults aged 18 and older, the resulting sample consists of individuals aged 18 to 64 and closely corresponds to the working-age population examined in our CPS and ATUS analyses. Our pooled sample contains 3 million respondent observations. Its size and extensive geographic and temporal coverage provide substantial precision for detecting changes in outcomes that occur among a relatively small share of the population. Because the availability of individual questions varies across survey years, the estimation sample for each outcome is restricted to the years and states in which the relevant question was administered.

Appendix Table A.3 reports summary statistics for the BRFSS analysis sample. Approximately 11 percent of respondents report having foregone a doctor visit because of cost during the previous 12 months, while about 2 percent report that health problems limit their ability to perform errands independently. Around 17 percent of respondents live in states that deregulate payday requirements during the sample period. The average respondent is about 45 years old, and 54 percent of the sample is female. Approximately 76 percent of respondents are White, 44 percent have completed university education, and 42 percent have a child living at home.

3.4 Job Openings and Labor Turnover Survey Data

We complement the individual-level data with the Job Openings and Labor Turnover Survey (JOLTS), a monthly establishment survey conducted by the Bureau of Labor Statistics. JOLTS covers private nonfarm establishments and federal, state, and local government entities. Its sample is drawn from the Quarterly Census of Employment and Wages and contains approximately 21,000 establishments. Employers report employment, job openings, hires, and separations. A position is classified as open when work is available, the position could begin within 30 days, and the employer is actively recruiting externally. Job openings therefore provide a direct employer-side measure of new labour demand. The Bureau of Labor Statistics aggregates the JOLTS information and publishes it at the state-month level. We use these state-level job-openings information from 2004 through 2023 and construct an annual measure by averaging the monthly series within each state and year.

4 Empirical Strategy

We identify the effects of payday deregulation using nine state-level reforms implemented at different points between 2003 and 2024. The staggered timing allows us to compare changes in labour-market outcomes within reforming states with contemporaneous changes in states that have not yet deregulated or remain untreated throughout the sample period. Identification therefore comes from deviations in a state's outcome trajectory around implementation, rather than from permanent differences between states. The presence of multiple reforms also reduces reliance on the experience of any single state or legislative episode. These reforms regulate the timing of wage disbursement rather than wage rates, employment protection, or eligibility for unemployment benefits. This institutional separation strengthens the interpretation of changes observed around implementation as consequences of greater employer discretion over pay frequency.

We estimate the following event-study specification:

$$Y_{ist} = \sum_{k \neq -1} \beta_k D_{st}^k + \gamma X_{ist} + \mu_s + \lambda_t + \varepsilon_{ist}, \tag{1}$$

where Y_{ist} denotes an outcome for individual i residing in state s in year t. Our primary outcome is an indicator for employment to unemployment transitions. The variable D_{st}^k equals one for individuals observed k years before or after payday deregulation in their state of residence. Event time $k = 1$ denotes the year after implementation, and $k = -1$, the year immediately preceding implementation, is omitted as the reference period. The coefficients β_k measure differences in the outcome relative to the year before deregulation. Coefficients for $k < 0$ capture pre-reform differences between reforming and comparison states, whereas coefficients for $k \geq 0$ trace the timing and persistence of the post-reform response.

The vector X_{ist} contains individual characteristics. The individual controls include gender, age, and age squared. These characteristics predict labour-market outcomes and account for changes in the demographic composition of the sample over time. Moreover, μ_s and λ_t denote state and year fixed effects. State fixed effects absorb permanent differences across states, including persistent variation in labour-market institutions and regulatory environments. Year fixed effects absorb aggregate shocks and nationwide labour-market developments, such as recessions, that affect all sample in a given year. Because payday regulations vary at the state level, we cluster standard errors at the state level.

The central identifying assumption is that, absent payday deregulation, labour-market outcomes in reforming states would have evolved in parallel with those in states that had not yet deregulated or never deregulated during the sample period. This assumption permits permanent differences in outcome levels across states and differences in the demographic composition of their populations, both of which are accounted for in the specification. A causal interpretation also requires that employers and workers do not substantially adjust their behaviour before implementation.

The event-study structure provides a direct assessment of the identifying assumption. Estimates of β_k for the years preceding implementation reveal whether outcomes in reforming states were already moving differently from those in comparison states or began changing in anticipation of the reform. Small and statistically indistinguishable pre-reform coefficients would support the parallel-trends and no-anticipation assumptions. Moreover, the absence of a systematic pattern would reduce concerns that reforms were adopted in response to pre-existing changes in labour-market conditions. The post-reform coefficients then measure the reduced-form effect of relaxing payday requirements, incorporating the fact that the legislation expands employers' payroll options without requiring them to adopt less frequent payment.

5 Results

5.1 Payday Requirements and Pay Frequency

We first examine whether the relaxation of state payday requirements is reflected in workers' reported earnings periodicity, which we use as a proxy for pay frequency. This evidence is important because deregulation expands employers' payroll options without requiring them to alter their existing practices. The CPS asks respondents to identify the interval over which they can most naturally report their usual earnings, offering categories that coincide with the principal U.S. payroll schedules: weekly, biweekly, twice monthly, and monthly. This close correspondence makes reported earnings periodicity a natural and informative proxy for the frequency with which earnings are received. We use 836,703 employed CPS respondents for whom this information is available. The sample is smaller because the earnings-periodicity question forms part of the CPS earnings module, which is administered only to eligible wage-and-salary workers in outgoing rotation groups—that is, during their fourth and eighth CPS interviews. Given this more limited coverage, we use a parsimonious static specification to estimate whether deregulation is accompanied by an average change in reported earnings periodicity.

Table 1 reports average marginal effects from a multinomial logit model in which the dependent variable identifies whether a worker is paid weekly, biweekly, or monthly.[1] Modeling the three outcomes jointly accounts for the fact that payment schedules are mutually exclusive, so an increase in the probability of one schedule must be offset by reductions in the others. The model includes the deregulation indicator, individual controls, state fixed effects, and year fixed effects, with standard errors clustered at the state level. The reported marginal effects measure the average post-reform change in the probability of each payment schedule relative to contemporaneous changes in states that have not deregulated.

Deregulation reduces the probability of weekly payment by 2.2 percentage points and increases the probability of biweekly payment by 2.7 percentage points. The weekly estimate is statistically significant at the 5 percent level, while the biweekly estimate is significant

[1] We classify semimonthly schedules as biweekly because both represent approximately two pay periods per month. The biweekly category therefore includes workers reporting either every-other-week or twice-monthly pay.

at the 1 percent level. By contrast, the estimated effect on monthly payment is small, at -0.4 percentage points, and statistically indistinguishable from zero. The decline in weekly payment is therefore mirrored closely by an increase in biweekly payment, with little evidence of adjustment along the monthly-payment margin.

These results align closely with the institutional structure of U.S. payroll practices. Weekly and biweekly payments are the most common schedules among private establishments, making it a natural alternative when employers gain flexibility to move away from weekly to biweekly payroll (U.S. Bureau of Labor Statistics, 2023). Moving from weekly to biweekly payment reduces the number of payroll runs from approximately 52 to 26 per year. Monthly payment remains less common and is often legally available primarily for workers whose payment schedules are less likely to be affected by reforms governing more frequently paid workers. The absence of an effect on monthly payment is therefore consistent with employers responding at the most practical and institutionally relevant margin. For affected workers, the shift from weekly to biweekly payment leaves total earnings unchanged but doubles the usual interval between paydays, producing a meaningful reduction in the frequency with which they can access accrued wages.

5.2 Payday Deregulation and Employment Stability

Employment stability is important because the termination of an existing job interrupts earnings, limits the accumulation of employer-specific experience, and commonly exposes workers to the financial and nonfinancial costs of unemployment. Examining departures from workers' current employers allows us to distinguish between two forms of labour-market adjustment. Workers may transition from employment to unemployment, or they may remain employed while moving directly to another employer. The former indicates a disruption of the existing employment relationship, while the latter is more consistent with labour-market reallocation. We examine these possibilities using matched CPS respondents observed across consecutive interviews and condition on employment at the beginning of each transition.

Figure 1 presents the results. Panel A reports event-study estimates for transitions from employment to unemployment. The coefficients preceding deregulation are small and statistically indistinguishable from zero, with no systematic pattern as implementation approaches.

Employment-to-unemployment transitions in reforming and comparison states therefore followed similar trends before the policy change, supporting the parallel-trends assumption.

The probability of transitioning from employment to unemployment increases by 0.9 percentage points in the first year after deregulation. This effect is statistically significant and economically substantial relative to the average transition probability of 4 percent, representing an increase of approximately 23 percent. The effect remains statistically significant in the second year, when it equals 0.5 percentage points, or approximately 13 percent of the sample mean. The subsequent estimates remain positive and range from 0.2 to 0.4 percentage points, corresponding to between 5 and 10 percent of the average transition probability, but they are smaller and not consistently distinguishable from zero. The evidence is therefore strongest during the first two post-reform years and points to a marked, relatively concentrated deterioration in employment stability.

A liquidity-based mechanism provides one explanation for this pattern. Receiving the same earnings in fewer installments increases the time workers must wait to access accrued wages. Workers with limited savings or access to affordable credit may respond by postponing purchases of essential services such as medical care and by reducing their use of professional and personal services, potentially requiring them to devote more of their own time to these activities. These adjustments can affect workers' well-being, time allocation, and ability to manage day-to-day demands, placing additional strain on existing employment relationships. The increase in employment-to-unemployment transitions is consistent with such pressures making some matches more difficult to sustain, although the transition estimates alone do not establish the underlying mechanism.

Panel B examines the probability that an employed worker moves directly to another employer. The estimates are close to zero throughout the event window and are statistically insignificant both before and after implementation. The post-reform coefficients display no systematic pattern and never exceed approximately 0.1 percentage points in absolute value. Payday deregulation therefore does not appear to generate greater mobility between employers.

The contrast between the two panels clarifies the nature of the employment response. Deregulation increases transitions from employment to unemployment but does not increase direct moves between employers. The resulting pattern is more consistent with the disruption of existing employment relationships than with workers reallocating toward other jobs. These estimates do not identify whether separations are initiated by workers or employers, but they show that affected employment relationships are more likely to end without an immediate transition to another employer.

Appendix B complements the main results by examining transitions from unemployment to employment. The pre-reform coefficients are small and statistically insignificant. Following deregulation, the estimate is approximately zero in the first year and becomes positive thereafter, reaching 0.6 and 0.7 percentage points in the second and third years. The later estimates also remain positive. However, the confidence intervals are wide and the estimates not statistically significant throughout the post-reform period, making this evidence suggestive rather than conclusive. The appendix results provide no indication that deregulation reduces job-finding among unemployed workers. If anything, the positive point estimates suggest that the additional transitions into unemployment do not lead to persistent detachment from employment, although the imprecision of the estimates prevents a firm conclusion.

Taken together, the results indicate that payday deregulation reduces employment stability by increasing transitions from employment to unemployment, particularly during the first two years after implementation. The absence of a corresponding increase in employer-to-employer transitions shows that this response does not reflect broader job reallocation, while the appendix evidence provides no indication of a deterioration in re-employment prospects.

5.3 Robustness Tests

The event-study estimates establish that payday deregulation increases transitions from employment to unemployment, with the largest effects concentrated in the first two years after implementation. We next summarize this dynamic pattern using a more parsimonious specification that replaces the full set of event-time indicators with two separate indicators for the short-run and longer-run post-reform periods. The short-run indicator covers the first two years after deregulation, while the long-run indicator captures subsequent years. Pooling adjacent event years improves precision and provides a common specification that can be applied consistently across the multiple robustness tests. This framework is also valuable for the heterogeneity analyses, which divide the CPS sample into smaller subsamples, and for the mechanism analyses that rely on alternative datasets with smaller estimation samples. In both cases, a fully dynamic specification would produce less precise estimates.

Table 2 examines the robustness of the estimated effect on transitions from employment to unemployment. Column (1) reports the baseline specification and provides the benchmark for the remaining columns. In this specification, payday deregulation increases the probability of transitioning from employment to unemployment by approximately 0.5 percentage points during the first two post-reform years. Relative to the average transition probability of 4.0 percent, this estimate represents an increase of approximately 12.5 percent. The long-run coefficient is considerably smaller and statistically indistinguishable from zero, consistent with the event-study evidence that the effect is concentrated in the period immediately following deregulation.

Column (2) removes all individual-level covariates. This specification addresses the possibility that the estimates depend on changes in the observable composition of workers across states and over time. The similarity of the coefficient to the baseline estimate shows that identification is driven by the timing of the reforms rather than by the inclusion of demographic controls.

Column (3) replaces the quadratic control for age with a complete set of age-year fixed effects. Labour-market transitions vary substantially over the life cycle, and a second-order polynomial may not capture this relationship fully. Allowing the effect of each age to enter flexibly produces an almost unchanged estimate, indicating that the results are not sensitive to the functional form used to account for workers' age profiles.

Column (4) adds time-varying state characteristics, including state population. Population growth may be correlated with changes in labour-market composition, economic activity, and the likelihood that states revise their employment regulations. Controlling for these changes reduces concern that the estimates capture evolving state characteristics that coincide with payday deregulation.

Columns (5) and (6) introduce progressively more flexible controls to account for seasonality and national labour-market shocks. Column (5) adds calendar-month fixed effects, which absorb recurring seasonal variation in employment transitions. Column (6) instead includes year-by-month fixed effects, allowing nationwide shocks to vary in every calendar month. This specification accounts flexibly for high-frequency aggregate developments, including changes in national labour-market conditions that cannot be captured by annual fixed effects alone.

Column (7) includes state-by-calendar-month fixed effects. Seasonal employment patterns can differ substantially across states because of differences in climate, tourism, agriculture, school calendars, and industrial composition. These fixed effects absorb recurring state-specific seasonality and ensure that identification does not come from systematic differences in the months during which workers in reforming states are observed.

Column (8) restricts the sample to individuals who do not change their state of residence between matched CPS interviews. Because treatment is assigned according to state of residence, interstate migration can create measurement error in reform exposure and change the composition of the population observed in treated states. The non-mover restriction ensures that individuals remain subject to the same state's regulatory environment throughout the period over which their employment transition is measured.

The estimated short-run effect remains positive, statistically significant, and similar in magnitude across all specifications. The long-run estimates remain substantially smaller and statistically indistinguishable from zero. Overall, the robustness tests reinforce the conclusion that payday deregulation produces a sizeable but concentrated increase in transitions from employment to unemployment.

Finally, Appendix C re-estimates the dynamic effects using the Callaway and Sant'Anna (2021) estimator, which accommodates treatment-effect heterogeneity under staggered adoption. The estimates closely mirror the baseline results: there is no evidence of differential pre-trends, transitions from employment to unemployment increase during the first two post-reform years, and the effects subsequently attenuate. This similarity confirms that our central findings are not an artifact of heterogeneous treatment effects.

5.4 Heterogeneity

The average effect of payday deregulation may conceal important differences in workers' ability to absorb longer intervals between paychecks. Lower-income workers are more likely to have limited liquid savings and restricted access to affordable credit, making the timing of wage receipt particularly consequential even when total earnings remain unchanged. We therefore divide workers into quartiles based on income reported in the preceding CPS wave and re-estimate the parsimonious specification separately for each group. Measuring income before the employment transition avoids classifying workers according to earnings that may have already been affected by a subsequent transition into unemployment. Figure 2 reports the estimated short-run effect for each quartile.

The increase in employment-to-unemployment transitions is concentrated among workers in the lowest income quartile. For this group, deregulation raises the transition probability by approximately 2.0 percentage points. The estimate is statistically significant and four times the 0.5-percentage-point effect for the full sample. It is also equivalent to 50 percent of the average transition probability of 4 percent. The estimate declines to approximately 1.0 percentage points in the second quartile and is statistically insignificant, while the effects in the third and fourth quartiles are close to zero. The results therefore reveal a pronounced income gradient, with the deterioration in employment stability concentrated among workers with the fewest economic resources.[2] Because the confidence intervals overlap, however, we do not interpret every pairwise difference across quartiles as statistically significant.

Taken together, the estimates presented in this section are consistent with liquidity constraints amplifying the consequences of less frequent payment. Workers with greater financial buffers can use savings or affordable credit to bridge longer intervals between paychecks, while lower-income workers have less capacity to smooth consumption and absorb unexpected expenses. The resulting financial and time pressures may affect workers' well-being and place strain on existing employment relationships. In the remainder of the paper, we provide extensive evidence on the mechanisms underlying the baseline results.

[2] To assess whether the effects vary more broadly with workers' demographic characteristics and household responsibilities, we further investigate heterogeneity by gender, educational attainment, and the presence of a child in the household in Appendix D. The estimated effects are similar across subgroups within each dimension, and none of the between-group differences is statistically significant.

6 Mechanisms

6.1 Employer- or Worker-Initiated Separations

The increase in employment-to-unemployment transitions may reflect decisions made by either side of the employment relationship. Workers may voluntarily leave because less frequent payment reduces the value of the job or makes the employment arrangement harder to sustain. Alternatively, employers may terminate the relationship because the match becomes less productive or more costly to maintain. Distinguishing between these margins is central to understanding whether the decline in employment stability operates primarily through worker labour supply or through employer separation decisions. We therefore classify employment-to-unemployment transitions according to whether they result from a layoff or a quit and re-estimate the baseline event-study specification for each outcome.

Figure 3 presents the results. Panel A examines layoffs. The pre-reform coefficients are small and statistically indistinguishable from zero, with no indication that layoffs were already increasing before deregulation. Following implementation, the probability of being laid off rises by 0.7 percentage points in the first year and by 0.3 percentage points in the second year. Both estimates are statistically significant. The subsequent coefficients remain generally positive, ranging from 0.1 to 0.4 percentage points, but are less precisely estimated and are not consistently distinguishable from zero. The magnitude and timing of the layoff estimates closely track the baseline effects on employment-to-unemployment transitions. This correspondence indicates that employer-initiated separations explain most of the deterioration in employment stability following deregulation.

Panel B examines voluntary quits. The estimates are tightly concentrated around zero throughout the event window and are statistically insignificant both before and after implementation. The largest post-reform estimate is approximately 0.1 percentage points in the first year, and the remaining coefficients are effectively zero. There is therefore no evidence that workers respond to payday deregulation by voluntarily leaving their jobs. This result complements the absence of an effect on direct employer-to-employer transitions: deregulation neither induces workers to quit into unemployment nor generates greater movement toward alternative employers.

Taken together, the increase in layoffs and the absence of additional quits indicate a weakening of existing employment relationships rather than greater voluntary worker mobility.[3] The separation decision is more likely to originate with the employer, while workers do not become more likely to leave on their own. This asymmetry motivates the mechanism analyses that follow. We first investigate the channels through which less frequent payment may strain workers and contribute to the increase in layoffs, before turning to the absence of a quit response by examining worker job-search behavior and employer-side adjustments through earnings and job openings.

6.2 Mechanisms supporting layoffs

6.2.1 Work Time and Workplace Interaction

One potential explanation for the increase in layoffs is that less frequent access to earnings creates financial and time pressures that make existing employment relationships harder to sustain. Workers facing tighter liquidity may postpone health-related expenditures or economize on time-saving services, with potential consequences for their health and allocation of time. These adjustments may reduce the time and energy available for work and workplace interaction, thereby weakening workers' connection to their jobs. We next examine whether payday deregulation changes the time workers spend working and socializing at the workplace.

Table 3 presents estimates using the ATUS sample of 127,348 employed respondents. We focus on employed individuals because they are directly exposed to employers' payment practices and allow us to examine time allocation within ongoing employment relationships. The dependent variables are the logarithm of one plus the number of minutes spent working and socializing at work during the diary day. This transformation reduces the influence of extreme values while retaining workers who report no working time in the diary date. We estimate the parsimonious specification distinguishing between the first two post-reform

[3] We also examine whether the increase in employment-to-unemployment transitions reflects the expiration of temporary contracts rather than the termination of ongoing employment relationships. Appendix E shows that the estimated effects on the probability that employment ends because a temporary contract expires are small and statistically indistinguishable from zero throughout the event window. The increase in separations is therefore not driven by the mechanical completion of fixed-term employment, further supporting the interpretation that deregulation increases employer-initiated terminations of existing matches.

years and the longer-run period.

In the short run, payday deregulation reduces the logarithm of working time by 0.116 log points. The estimate is statistically significant at the 10 percent level and corresponds to a 9 percent decline in minutes worked. Deregulation also reduces time spent socializing at work by 0.005 log points. This estimate is smaller in magnitude but precisely estimated and statistically significant at the 1 percent level. Because time spent socializing at work is typically limited and frequently zero, the estimated decline is meaningful relative to the low baseline prevalence of this activity.

The long-run coefficients are smaller, and neither is statistically distinguishable from zero. The concentration of the effects in the first two post-reform years mirrors the timing of the increase in layoffs. The results therefore show that deregulation is accompanied by a short-run decline in both time devoted to work and interaction within the workplace. Their joint decline is consistent with a temporary weakening of workplace engagement that may make some employment relationships more difficult to sustain.

6.2.2 Financial Constraints and Health

The shift toward less frequent payment can tighten short-run liquidity even when workers' total earnings remain unchanged. This constraint may be especially consequential for expenditures that require immediate payment and cannot easily be postponed without cost. Medical care is an important example: workers may delay appointments, treatment, or other health-related services when available funds are low, potentially allowing manageable health problems to become more disruptive. Deteriorating health or difficulty performing routine activities may then reduce workers' capacity to meet job demands and contribute to the decline in working time documented above. Financial pressure can also reduce access to time-saving services outside the workplace, leaving workers with less time available for work. These outcomes provide a direct way to assess whether the reforms relaxing payday requirements are accompanied by financial pressures that make existing employment relationships harder to sustain.

Table 4 combines evidence using data from the ATUS and BRFSS. The ATUS measures whether respondents spend any time obtaining or using medical services on a randomly selected diary day. The BRFSS records whether respondents were unable to see a doctor because of cost and whether a health condition limits their ability to perform errands alone. We restrict every analysis to employed individuals. This restriction focuses attention on financial and health difficulties arising within ongoing employment relationships and reduces the possibility that the results mechanically reflect the income or insurance loss caused by becoming unemployed. We estimate the parsimonious specification separately for each outcome, distinguishing the first two post-reform years from the subsequent period. Differences in sample sizes across columns reflect the availability of each question and dataset.

Column (1) shows that payday deregulation reduces the probability of using medical services on the diary day by 0.6 percentage points in the short run. The estimate is statistically significant at the 5 percent level. Relative to an untreated mean of 2 percent, it represents a 30 percent decline in daily engagement with medical services. The absolute change is therefore substantial given the low probability that medical care occurs on any particular diary day. The long-run estimate is effectively zero and statistically insignificant, indicating that the reduction is concentrated in the first two years after deregulation.

Column (2) provides more direct evidence on affordability by examining whether employed BRFSS respondents report being unable to see a doctor because of cost. Deregulation increases this probability by 0.4 percentage points in the short run. The estimate is statistically significant at the 10 percent level and corresponds to an increase of approximately 3.6 percent relative to the untreated mean of 11 percent. The long-run effect is similar in magnitude, at 0.5 percentage points, or approximately 4.5 percent of the untreated mean, and remains statistically significant at the 10 percent level. The persistence of the affordability effect indicates that some employed workers continue to face difficulty financing medical care beyond the initial adjustment period.

Column (3) examines whether these constraints are accompanied by changes in functional health. Payday deregulation increases the probability that respondents report health-related difficulty performing errands alone by 0.2 percentage points in the short run. This estimate is statistically significant at the 5 percent level and represents an increase of approximately 10 percent relative to the untreated mean of 2.1 percent. The long-run coefficient declines to 0.1 percentage points and is statistically insignificant. The short-run increase therefore indicates that the effects extend beyond health-care use and affordability to a dimension of health that directly captures individuals' ability to perform routine activities.

The results across the two datasets provide a coherent pattern. Following deregulation, employed workers become less likely to use medical services, more likely to report that cost prevented them from seeing a doctor, and more likely to experience health-related limitations in daily activities. The ATUS and BRFSS outcomes capture different margins and reference periods, so their dynamics need not coincide exactly. Taken together, the estimates are consistent with less frequent access to earnings tightening workers' financial constraints and interfering with health-care access. The accompanying deterioration in functional health provides a potential link to reduced working time and to the weakening of employment relationships that culminates in greater layoffs. The analysis that follows examines whether similar financial pressures also limit workers' use of market-provided services that can alleviate time constraints.

6.2.3 Financial Constraints and Professional Services Usage

Financial pressure can also affect employment stability by changing how workers allocate time outside the workplace. Market-provided services such as childcare, cleaning, meal preparation, and household repairs allow individuals to substitute money for time. When access to earnings becomes less frequent, liquidity-constrained workers may reduce or postpone these services and perform the associated activities themselves. The resulting increase in demands on their own time may leave less time and energy available for work, providing a potential link between financial constraints, the decline in working time, and the increase in layoffs documented above.

Table 5 examines this channel using 127,348 employed ATUS respondents. Restricting the sample to employed individuals allows us to study service use within ongoing employment relationships rather than changes arising mechanically after job loss. The outcomes indicate whether respondents used professional services during the diary day. Because the individual activities are uncommon on any particular day, the aggregate measure provides the most informative summary, while the component outcomes help characterize the response.

Deregulation reduces the probability of using professional services by 1.47 percentage points in the short run. The estimate is statistically significant at the 1 percent level and represents a 22 percent decline relative to the untreated mean of 6.65 percent. The long-run coefficient is small and statistically insignificant, concentrating the reduction in the same period in which working time declines and layoffs increase. The component estimates provide no evidence of reduced childcare or cleaning-service use. Instead, meal-preparation services decline by 0.01 percentage points in the short run, an estimate that is statistically significant at the 5 percent level and sizeable relative to the activity's low untreated incidence. Repair-service use falls by 0.27 percentage points, compared with an untreated mean of 0.28 percent. This effect is statistically significant at the 5 percent level and remains negative in the long run, when the estimated decline is 0.22 percentage points.

Taken together, the results show that payday deregulation reduces employed workers' use of market-provided services, especially during the first two post-reform years. The aggregate decline is not shared equally across all service categories but is particularly visible for meal preparation and repairs. These services allow households to substitute expenditures for their own time, so reduced utilization may increase the amount of time workers must devote to activities outside the workplace. The findings therefore provide a plausible connection between tighter liquidity, greater time pressure, reduced working time, and the weakening of employment relationships.

6.3 Mechanisms Underlying the Absence of a Quit Response

Having examined the mechanisms underlying the increase in layoffs, we turn to the absence of a corresponding effect on quits. On the worker side, less frequent payment will generate voluntary mobility only if employees respond to the reduced attractiveness of their current jobs by searching for alternative employment and progressing sufficiently far in the search process to obtain interviews. The absence of additional quits may therefore reflect limited worker-initiated efforts to leave existing employment relationships. Employer-side responses provide a separate set of explanations. Current employers may increase earnings to compensate workers for the less favorable timing of pay and thereby reduce their incentives to leave. Alternatively, outside employers may reduce the number of available vacancies, limiting workers' opportunities to move even if they wish to do so. The null effect on quits could therefore originate from workers' search behavior, from employer adjustments through earnings or job openings, or from responses on both sides of the market. We investigate each of these possibilities below.

6.3.1 Worker Job-Search Behavior

We next examine whether payday deregulation changes employed workers' job-search behavior. If less frequent payment reduces the value of the current job sufficiently to induce voluntary mobility, an initial response should be an increase in efforts to identify and secure alternative employment. We measure this response using ATUS information on whether employed individuals engaged in job-search activities or job interviewing during the diary day. Job-search activity captures the initial effort to identify alternative employment, while interviewing reflects progression to a later stage of the search process. Restricting the sample to employed respondents ensures that these outcomes measure on-the-job search before a separation occurs.

Columns (1) and (2) of Table 6 report the results for 127,348 employed ATUS respondents using the same parsimonious specification applied in the preceding mechanism analyses. Column (1) shows a small and statistically insignificant effect on job-search activity, providing no evidence that employed workers become more likely to seek alternative jobs following deregulation. Column (2) yields the same conclusion for job interviews. The consistency of the estimates across these two stages of the search process indicates that the absence of additional quits is accompanied by limited worker-initiated efforts to move away from current employers.

6.3.2 Employer Responses: Earnings Adjustments and Job Opportunities

Having found no change in workers' job-search behavior, we next consider whether employer-side adjustments help explain the absence of additional quits. We examine two margins. First, current employers may increase earnings to compensate workers for less frequent pay and retain them in existing employment relationships. Second, outside employers may reduce the availability of alternative jobs, constraining workers' ability to move across employers.

We begin by examining whether current employers adjust earnings. If workers regard less frequent access to earnings as costly, an increase in compensation could offset this disadvantage and allow employers to adopt longer pay cycles without inducing additional quits. Columns (1) and (2) of Table 7 report estimates for the logarithm of total personal income and the logarithm of earnings from the respondent's main job among employed CPS respondents. Main-job earnings provide the more direct test of a compensating response by current employers, while total personal income indicates whether workers experience a broader change in available economic resources. Neither outcome changes significantly in the short or long run. We therefore find no evidence that the absence of additional quits is explained by employers compensating workers for less frequent pay through higher earnings. These results also reinforce the interpretation that deregulation affects the timing of workers' access to earnings rather than the amount they receive.

We then examine whether the absence of quits reflects a deterioration in outside employment opportunities. Column (3) of Table 7 reports estimates of the effect of deregulation on the logarithm of state-level job openings using 1,020 JOLTS observations. Because the JOLTS data are measured at the state-year level, the specification includes state and year fixed effects as well as time-varying state controls. The estimated effects are small and statistically indistinguishable from zero in both the short and long run. We therefore find no statistically detectable evidence that payday deregulation produces a broad contraction in state-level labor demand that restricts workers' opportunities to move to other jobs. The absence of additional quits consequently does not appear to reflect a reform-induced disappearance of outside employment opportunities.

Taken together, the evidence provides no support for either compensating earnings adjustments or a contraction in outside opportunities as explanations for the absence of quits. Employed workers do not become more likely to search or interview for alternative jobs, despite the absence of a detectable increase in earnings from their current employment relationships and the continued availability of job openings. The null effect on quits therefore appears to reflect limited worker-initiated reallocation rather than employer-side adjustments that either discourage or restrict mobility. Combined with the earlier evidence on layoffs and reduced working time, as well as tighter financial constraints reflected in lower health-care affordability and professional-service use, these findings point to financial and time pressures weakening existing employment relationships and ultimately contributing to employer-initiated separations.

7 Conclusion

The timing of wage payments is a basic but understudied feature of the employment relationship. Standard measures of compensation typically focus on earnings over a given period, leaving the economic importance of when workers gain access to those earnings less well understood. This paper shows that payment timing matters for employment stability even when statutory reforms do not alter wage rates or total earnings. We exploit staggered state reforms implemented between 2003 and 2024 that relaxed restrictions on the maximum interval between paydays. Because these reforms expand employers' payroll options without requiring them to change payment schedules, our estimates capture the consequences of increasing employer discretion over pay frequency.

Using rich longitudinal microdata from the Current Population Survey, we first establish that this legal flexibility changes employers' payment practices. Following deregulation, weekly payment becomes less common and biweekly payment more prevalent, while monthly payment remains largely unchanged. Employers therefore adjust along the most common and institutionally relevant margin, moving some workers from weekly to biweekly schedules and lengthening the interval over which they must wait to receive accrued earnings. Employment stability subsequently deteriorates: transitions from employment to unemployment increase sharply during the first two years after deregulation and then attenuate. The reforms do not generate a corresponding increase in direct transitions between employers, indicating that the response reflects the disruption of existing jobs rather than greater reallocation across firms. The increase is also concentrated among workers in the lowest income quartile, consistent with limited savings and access to affordable credit making longer intervals between paychecks especially consequential.

Decomposing the additional transitions into unemployment shows that they arise primarily through layoffs, while voluntary quits remain unchanged. This asymmetry indicates that the deterioration in employment stability is not driven by workers choosing to leave jobs that have become less attractive under less frequent payment. Instead, existing employment relationships become more likely to end through an employer-initiated separation.

Using ATUS and BRFSS data, we investigate plausible mechanisms behind the increase in layoffs. We find that deregulation reduces the time employed individuals spend working and socializing at work, lowers their use of medical and professional services, increases cost-related barriers to medical care, and raises health-related limitations in daily activities. These findings point to two complementary channels. Reduced access to medical care may weaken workers' health and capacity to meet job demands, while lower use of professional services may require them to devote more of their own time to activities outside the workplace. The resulting financial and time pressures can reduce workers' effective labour input and weaken their connection to the workplace, making existing employment relationships more difficult to sustain.

We also investigate the mechanisms underlying the absence of an effect on voluntary quits. The null response could originate on the worker side if employed workers do not search for alternative jobs, on the employer side if current employers offset less favorable pay timing by raising earnings or outside employers reduce available job openings, or through a combination of worker and employer responses. We examine the worker side using ATUS measures of on-the-job search and interviewing and the employer side using CPS measures of earnings and JOLTS data on state-level job openings. We find no statistically detectable change in job-search activity or interviewing among employed workers. We also find no increase in total personal income or earnings from the main job, providing no evidence that current employers compensate workers for less frequent pay through higher earnings. Job openings remain broadly unchanged, providing no evidence that outside employers reduce the availability of alternative employment opportunities. Taken together, these findings suggest that the absence of additional quits reflects limited worker-initiated reallocation rather than compensating earnings adjustments by current employers or diminished opportunities offered by outside employers.

An important implication of our findings is that financial pressures generated by the timing of compensation—even when total wages remain unchanged—extend beyond household spending decisions and shape the stability of employment relationships. Receiving the same earnings in fewer installments limits workers' ability to finance medical care and purchase market services that save time, affecting their health, time allocation, and capacity to sustain work. These constraints spill over into the workplace by reducing effective labour input and weakening the durability of existing employment relationships, ultimately contributing to employer-initiated separations. Payroll practices that allow for a lower pay frequency therefore impose consequential costs on workers through less stable employment relationships, especially among those possessing the fewest financial resources.

From a policy perspective, payday-frequency requirements should be viewed as substantive worker protections rather than purely administrative restrictions. Allowing longer pay cycles gives employers greater flexibility, but it also transfers liquidity risk to workers, with the largest consequences borne by those least able to smooth expenditures between paydays. Employers may not fully internalize the resulting costs associated with reduced health-care access, greater time pressure, and less stable employment relationships, creating an economic rationale for regulating the timing of wage payments. Our estimates imply that evaluations of payday deregulation should account for its effects on workers' well-being and employment stability. Policies that preserve minimum payment-frequency protections, require employee consent before extending pay cycles, or provide low-cost access to already-earned wages could retain some employer flexibility while limiting workers' exposure to these financial pressures.

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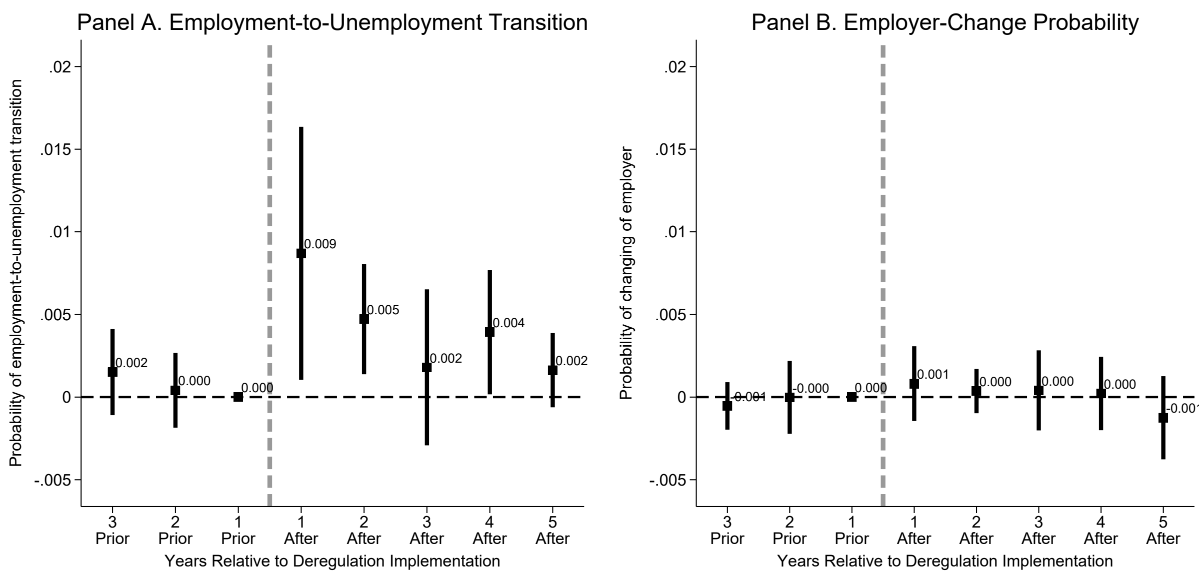
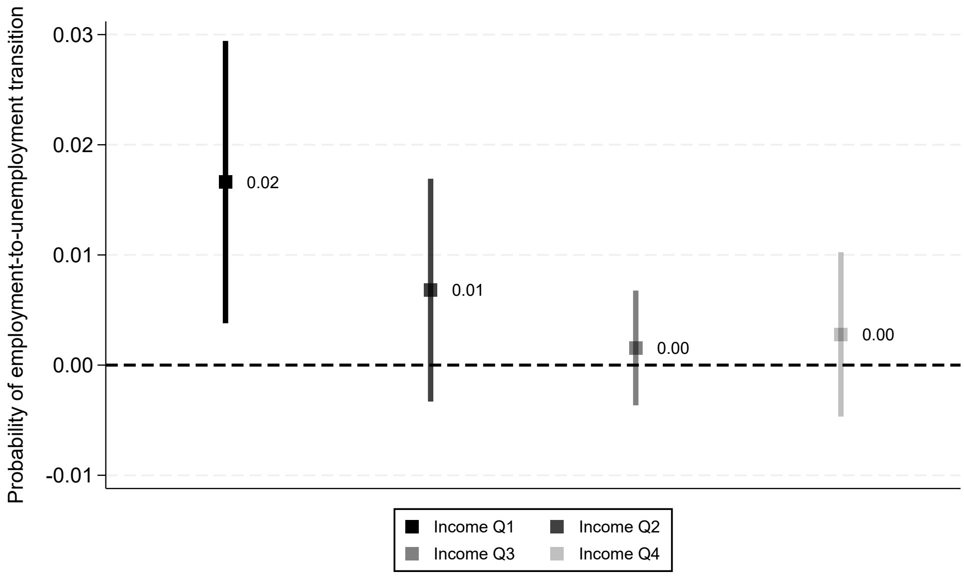
Figures

Figure 1: Employment stability



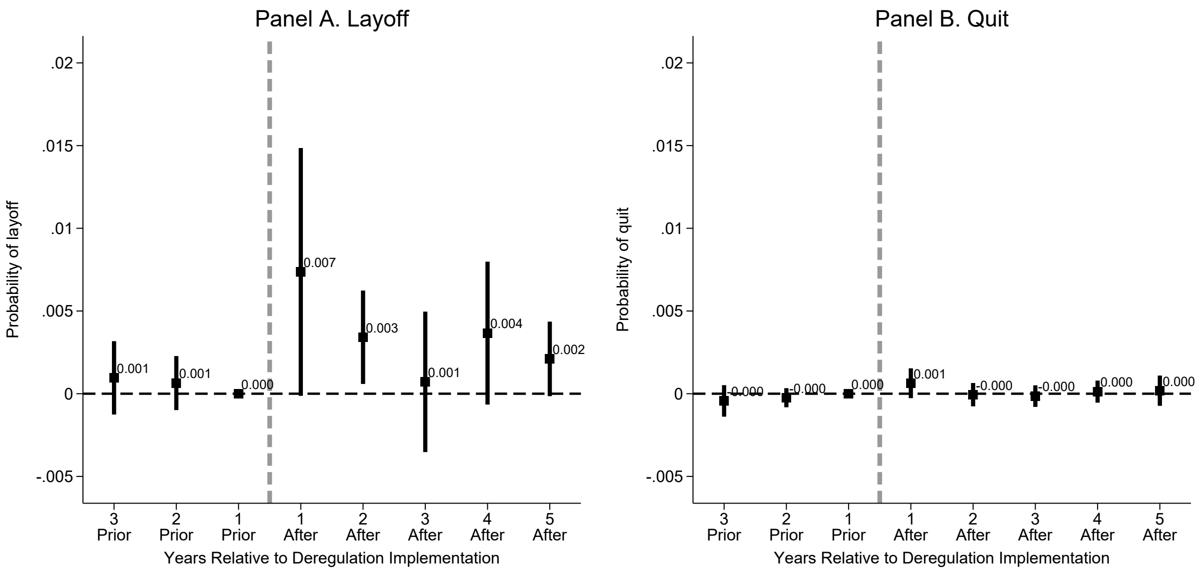
Notes: This figure reports event-study estimates using matched CPS respondents aged 16–64. Panel A examines the probability of transitioning from employment to unemployment by the subsequent interview. Panel B examines the probability of moving directly to a different employer. Event-time indicators identify years relative to payday deregulation, with the year immediately preceding implementation omitted as the reference period. All specifications control for gender, age, and age squared and include state and year fixed effects. The markers report point estimates, and the vertical bars report 95 percent confidence intervals based on standard errors clustered by state. The vertical dashed line denotes reform implementation.

Figure 2: The importance of initial financial constraints



Notes: This figure reports the estimated short-run effect of payday deregulation on employment-to-unemployment transitions by prior-income quartile. The sample consists of matched CPS respondents aged 16–64. Income quartiles are constructed using income reported in the preceding CPS wave, with Q1 denoting the lowest-income quartile and Q4 the highest. The short-run indicator equals one during the first two post-reform years and zero otherwise. The specification is estimated separately for each quartile and controls for gender, age, and age squared, together with state and year fixed effects. The markers report point estimates, and the vertical bars report 95 percent confidence intervals based on standard errors clustered by state.

Figure 3: Layoffs and quits



Notes: This figure reports event-study estimates using matched CPS respondents aged 16–64. Panel A examines the probability of transitioning from employment to unemployment through an employer-initiated layoff. Panel B examines the probability of making the same transition through a worker-initiated quit. Event-time indicators identify years relative to payday deregulation, with the year immediately preceding implementation omitted as the reference period. All specifications control for gender, age, and age squared and include state and year fixed effects. The markers report point estimates, and the vertical bars report 95 percent confidence intervals based on standard errors clustered by state. The vertical dashed line denotes reform implementation.

Tables

Table 1: Payment frequency

	Weekly	Bi-Weekly	Monthly
Deregulation policy	-0.022**	0.027***	-0.004
	(0.009)	(0.008)	(0.003)
Individual controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Average (untreated)	0.562	0.297	0.141
Observations	836,703	836,703	836,703

Notes: This table reports average marginal effects from a multinomial logit model estimated using employed wage-and-salary CPS respondents aged 16–64 in the outgoing rotation groups. Payment frequency is inferred from the period over which respondents report their earnings, and the columns show the probability of weekly, biweekly, and monthly payment. *Deregulation policy* equals one when payday-frequency requirements have been relaxed in the respondent's state by the time of the survey interview and zero otherwise. All specifications control for gender, age, and age squared and include state and year fixed effects, which absorb permanent state differences and common annual shocks. Standard errors, reported in parentheses, are clustered at the state level. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 2: Robustness tests

	Probability of Employment-to-Unemployment Transition							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deregulation policy - Short term	0.005**	0.005**	0.006**	0.005**	0.005**	0.005**	0.005**	0.005**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Deregulation policy - Long term	0.001	0.001	0.002	0.001	0.001	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
State controls	No	No	No	Yes	No	No	No	No
Month FE	No	No	No	No	Yes	Yes	Yes	No
Year-by-Month FE	No	No	No	No	No	Yes	No	No
State-by-Month FE	No	No	No	No	No	No	Yes	No
Average (untreated)	0.040	0.040	0.040	0.040	0.040	0.040	0.040	0.040
Observations	2089713	2089713	2089713	2089713	2089713	2089713	2089713	2089713

Notes: This table reports linear probability estimates using matched CPS respondents aged 16–64. The dependent variable equals one if the individual transitions from employment to unemployment by the next interview and zero otherwise. The short-run indicator of the deregulation policy equals one during the first two post-reform years, while the long-run indicator equals one in subsequent years; both equal zero otherwise. Column (1) presents the baseline specification, which includes gender, age, age squared, and state and year fixed effects. Column (2) omits individual controls. Column (3) replaces the quadratic age controls with age fixed effects. Column (4) adds state population. Columns (5), (6), and (7) add calendar-month, year-by-month, and state-by-calendar-month fixed effects, respectively. Column (8) restricts the sample to individuals who do not change state between interviews. Standard errors, reported in parentheses, are clustered by state. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 3: Time spent working and socializing at work

	Log of minutes spent working + 1	Log of minutes spent socializing at work + 1
Deregulation policy - Short term	-0.116**	-0.005***
	(0.049)	(0.001)
Deregulation policy - Long term	-0.063	-0.000
	(0.070)	(0.002)
Individual controls	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Average (untreated)	3.416	0.004
Observations	127348	127348

Notes: This table reports OLS estimates using employed ATUS respondents younger than 65. The dependent variables are the logarithm of one plus the number of minutes spent working during the diary day and the logarithm of one plus the number of minutes spent socializing at work. The transformation retains observations reporting zero minutes. The short-run indicator equals one during the first two post-reform years, while the long-run indicator equals one in subsequent years; both equal zero otherwise. All specifications control for gender, age, and age squared and include state and year fixed effects. Standard errors, reported in parentheses, are clustered by state. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 4: Medical affordability and health

	Probability using medical services	Probability not seeing doctor due to cost	Probability health limits doing errands
Deregulation policy - Short term	-0.006**	0.004*	0.002**
	(0.003)	(0.002)	(0.001)
Deregulation policy - Long term	0.000	0.005*	0.001
	(0.002)	(0.003)	(0.002)
Individual controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Average (untreated)	0.020	0.110	0.021
Observations	127348	3192997	1773964

Notes: This table reports linear probability estimates for employed respondents aged 16–64. Column (1) uses ATUS data. The outcome is the probability that the respondent spent any time obtaining or using medical services during the diary day, and zero otherwise. Columns (2) and (3) use BRFSS data. The outcome in Column (2) equals one if the respondent reports being unable to see a doctor because of cost during the preceding 12 months and zero otherwise. The outcome in Column (3) equals one if a physical, mental, or emotional condition limits the respondent's ability to perform errands alone and zero otherwise. The short-run indicator covers the first two post-reform years, while the long-run indicator covers subsequent years; both equal zero otherwise. All specifications control for gender, age, and age squared and include state and year fixed effects. Standard errors, reported in parentheses, are clustered by state. Differences in the number of observations reflect data and question availability. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 5: Utilization of professional services

	Probability of using any professional service	Probability of using childcare service	Probability of using cleaning service	Probability of using meal prep service	Probability of using repair service
Deregulation policy - Short term	-0.0147***	0.0004	0.0005	-0.0001**	-0.0027**
	(0.0032)	(0.0007)	(0.0003)	(0.0001)	(0.0013)
Deregulation policy - Long term	0.0014	0.0009	-0.0000	-0.0000	-0.0022**
	(0.0024)	(0.0006)	(0.0002)	(0.0000)	(0.0009)
Individual controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Average (untreated)	0.0665	0.0024	0.0002	0.0001	0.0028
Observations	127348	127348	127348	127348	127348

Notes: This table reports linear probability estimates using employed ATUS respondents aged 16–64. The outcomes are indicator variables equal to one if the respondent spent any time using the specified market-provided service on the diary day, and zero otherwise. Column (1) indicates the use of any professional service, while Columns (2)–(5) separately examine childcare, cleaning, meal-preparation, and repair services. The short-run indicator of the deregulation policy covers the first two post-reform years, while the long-run indicator covers subsequent years; both equal zero otherwise. All specifications control for gender, age, and age squared and include state and year fixed effects. Standard errors, reported in parentheses, are clustered by state. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 6: Job search and job openings

	Workers	
	Probability job search	Probability job interviewing
Deregulation policy - Short term	0.0006	0.0001
	(0.0010)	(0.0003)
Deregulation policy - Long term	0.0009	0.0004
	(0.0008)	(0.0003)
Individual controls	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Average (untreated)	0.0042	0.0007
Observations	127348	127348

Notes: Columns (1) and (2) report linear probability estimates using employed ATUS respondents aged 16–64. The outcomes are indicator variables equal to one if the respondent spent any time searching for a job or interviewing for a job, respectively, during the diary day. The short-run indicator of the deregulation policy covers the first two post-reform years, while the long-run indicator covers subsequent years; both equal zero otherwise. All specifications control for gender, age, and age squared, and include state and year fixed effects, with standard errors clustered by state. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 7: Earnings

	Log total personal income + 1	Log earnings + 1 main job	Log of number of job openings
Deregulation policy - Short term	0.034	0.014	-0.0137
	(0.022)	(0.024)	(0.0124)
Deregulation policy - Long term	0.002	-0.005	-0.0096
	(0.035)	(0.036)	(0.0168)
Time-varying controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Average (untreated)	10.098	9.802	11.0693
Observations	1183019	1182662	1020

Notes: This table reports estimates using employed CPS respondents aged 16–64. The dependent variables are the logarithms of the total personal income from all sources plus one in Column (1) and of the earnings from the respondent's main job plus one in Column (2), as recorded in the CPS. Column (3) uses state-year JOLTS data; its outcome is the logarithm of the annual average number of total nonfarm job openings in each state. The short-run indicator of the deregulation policy equals one during the first two post-reform years, while the long-run indicator equals one in subsequent years; both equal zero otherwise. The CPS specifications control for gender, age, and age squared. The JOLTS specification includes state population as a time-varying state control. All specifications include state and year fixed effects. Standard errors, reported in parentheses, are clustered by state. "Average (untreated)" denotes the outcome mean among untreated observations. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Appendix

A Summary Statistics

Table A.1: Summary Statistics CPS

	Employment stability analysis sample		Pay frequency analysis sample	
	Mean	SD	Mean	SD
Transition Employment to Unemployment	0.040	0.196	0.016	0.125
Probability Change Employer	0.043	0.202	0.045	0.208
Layoff	0.026	0.158	0.011	0.103
Quit	0.005	0.068	0.002	0.041
End Temporary Contract	0.004	0.065	0.002	0.039
Individual total income			51513	60565
Main job earnings			69947	1516135
Weekly salary payment			0.564	0.496
Bi-weekly salary payment			0.300	0.458
Monthly salary payment			0.136	0.343
Lives in State that Deregulates	0.254	0.435	0.253	0.435
Age	42.404	12.490	41.935	12.244
Female	0.478	0.500	0.479	0.500
White	0.825	0.380	0.811	0.392
University education	0.343	0.475	0.372	0.483
Has Child	0.485	0.500	0.487	0.500
Number observations	2089713		836703	

Notes: This table reports the mean and standard deviation (SD) of the main variables used in the analysis. Columns (1)–(2) present summary statistics for the employment stability analysis sample, while columns (3)–(4) present corresponding statistics for the pay frequency analysis sample, which is restricted to observations for which pay frequency is observed. The data are drawn from the Current Population Survey (CPS). The employment stability sample contains 2,089,713 observations, and the pay frequency sample contains 836,703 observations.

Table A.2: Summary Statistics ATUS

	Mean	SD
Time spent working	255.564	263.988
Probability using medical services	0.020	0.138
Probability using professional services	0.066	0.247
Probability using childcare services	0.002	0.048
Probability using cleaning services	0.000	0.015
Probability using meal preparation services	0.000	0.007
Probability using repair services	0.003	0.053
Probability job search	0.004	0.065
Probability job interviewing	0.001	0.026
Lives in State that Deregulates	0.254	0.435
Female	0.507	0.500
White	0.812	0.391
Age	42.274	11.804
University education	0.421	0.494
Has child	0.517	0.500
Number observations	127348	

Notes: This table reports the mean and standard deviation (SD) of the main variables used in the analysis based on the American Time Use Survey (ATUS). The sample consists of respondents included in the ATUS analysis sample for whom the relevant time-use and demographic information is available. The table summarizes both time-allocation outcomes and individual characteristics used in the empirical analysis. The final sample contains 127,348 observations.

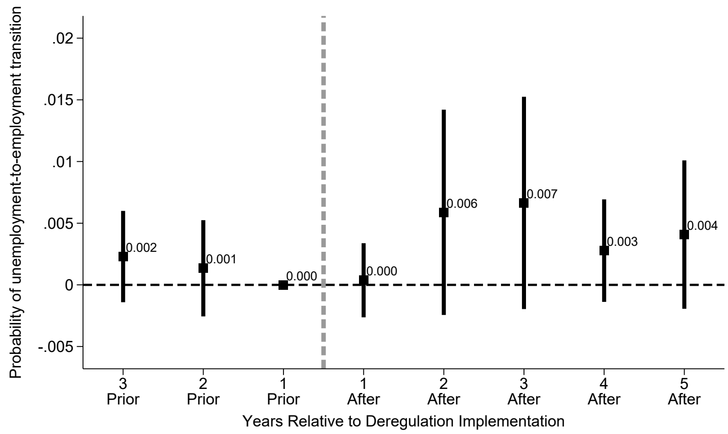
Table A.3: Summary Statistics BRFSS

	Mean	SD
Probability not seeing doctor due to cost	0.110	0.313
Probability health limits doing errands	0.021	0.143
Lives in State that Deregulates	0.173	0.379
Age	44.625	12.245
Female	0.541	0.498
White	0.763	0.425
University education	0.443	0.497
Has Child	0.420	0.494
Number observations	3196809	

Notes: This table reports the mean and standard deviation (SD) of the main variables used in the analysis based on the Behavioral Risk Factor Surveillance System (BRFSS). The sample consists of respondents included in the BRFSS analysis sample for whom the relevant health-access outcomes and demographic characteristics are available. The final sample contains 3,196,809 observations.

B Unemployment-to-employment transitions

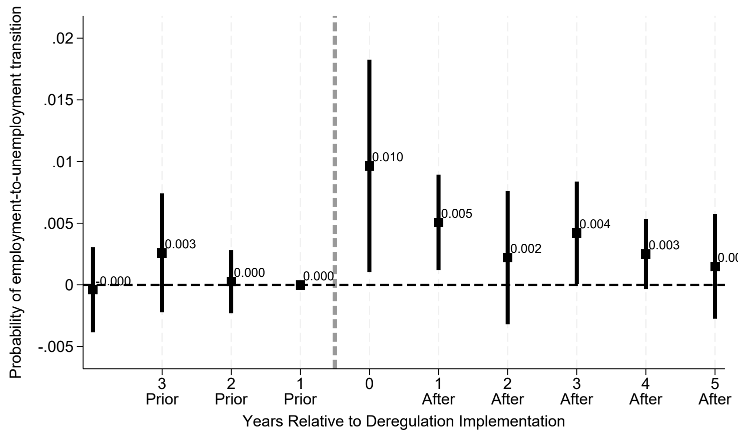
Figure B.1: Unemployment-to-employment transition



Notes: This figure reports event-study estimates using matched CPS respondents aged 16–64. The dependent variable equals one if the individual transitions from unemployment to employment by the subsequent interview and zero otherwise. Event-time indicators identify years relative to payday deregulation, with the year immediately preceding implementation omitted as the reference period. The specification controls for gender, age, and age squared and includes state and year fixed effects. The markers report point estimates, and the vertical bars report 95 percent confidence intervals based on standard errors clustered by state. The vertical dashed line denotes reform implementation.

C Accounting for treatment effect heterogeneity

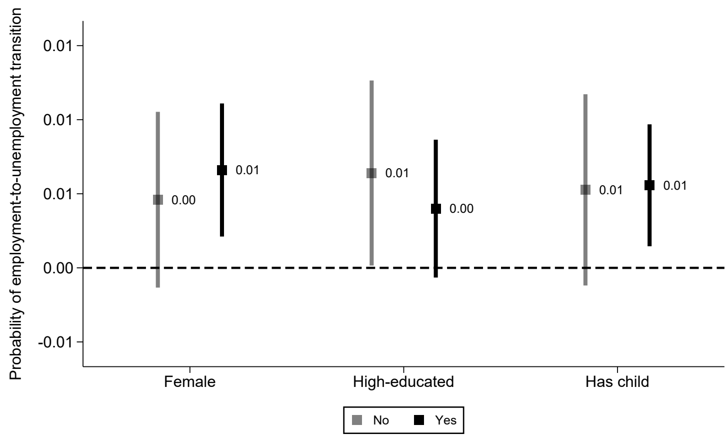
Figure C.1: Accounting for treatment effect heterogeneity



Notes: This figure reports dynamic treatment effects estimated using the Callaway and Sant'Anna (2021) estimator, which accommodates treatment-effect heterogeneity across reform cohorts and over time. The sample consists of matched CPS respondents aged 16–64, and the outcome equals one if the individual transitions from employment to unemployment by the subsequent interview. The estimator constructs cohort- and period-specific treatment effects and aggregates them by event time. The year immediately preceding implementation is the reference period. Markers report point estimates, and vertical bars report 95 percent confidence intervals based on standard errors clustered by state. The vertical dashed line denotes reform implementation.

D Further heterogeneity

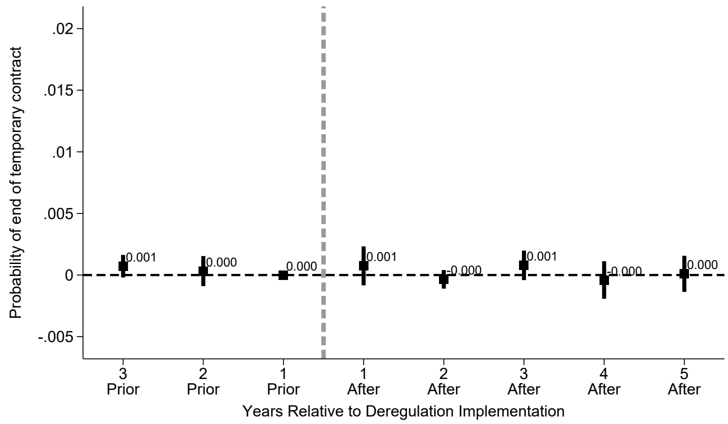
Figure D.1: Further heterogeneity



Notes: This figure reports the estimated short-run effect of payday deregulation on employment-to-unemployment transitions across additional individual subgroups. The sample consists of matched CPS respondents aged 16–64. Estimates are reported separately by gender, high-education status, and the presence of a child in the household. For each characteristic, grey markers represent workers without the indicated characteristic and black markers represent those with it. The short-run indicator of the deregulation policy equals one during the first two post-reform years and zero otherwise. Each specification includes the baseline individual controls, where applicable, together with state and year fixed effects. Markers report point estimates, and vertical bars report 95 percent confidence intervals based on standard errors clustered by state.

E Probability of end of temporary contract

Figure E.1: Probability of end of temporary contract



Notes: This figure reports event-study estimates using matched CPS respondents aged 16–64. The dependent variable equals one if the individual transitions from employment to unemployment by the subsequent interview because a temporary contract or temporary job ended, and zero otherwise. Event-time indicators identify years relative to payday deregulation, with the year immediately preceding implementation omitted as the reference period. The specification controls for gender, age, and age squared and includes state and year fixed effects. Markers report point estimates, and vertical bars report 95 percent confidence intervals based on standard errors clustered by state. The vertical dashed line denotes reform implementation.