Temporary Shocks, Permanent Impacts: The Effects of Liquidity on Job Search and Reemployment *

Niklas Flamang — Nova SBE
Sreeraahul Kancherla — UC Berkeley

(**Job Market Paper**)

This version: December 7, 2023.
Click here for most recent version.

Abstract

How do differences in job seekers' liquidity during unemployment affect their reemployment and long-term earnings trajectories? We examine a novel source of variation: delays in unemployment insurance (UI) benefit payments. These delays create high-frequency variation in claimants' cash-on-hand by only shifting timing rather than the total amount of benefits received. We leverage plausibly random delays during a 2013 California UI system glitch that temporarily froze benefit payments for a subset of active claimants. To minimize residual differences between delayed and non-delayed claimants, our research design further matches on a rich set of demographics, earnings histories, and pre-outage claim histories. The mean claimant affected by the glitch had a total of $825 in UI benefits delayed, and waited an average of 34 days before all benefits were eventually paid. In the short run, claimants with delayed payments exit UI earlier, are employed faster, and find better worker-firm matches at subsequent firms. These effects are highly persistent over time: five years after the system glitch, delayed claimants have 2.5 percentage points higher likelihood of employment, $500 higher labor earnings each quarter, and 5% higher earnings conditional on employment. We find that these effects are largest among claimants affected early into their unemployment spell. Our results are consistent with a model of job search incorporating duration dependence: UI benefits increase the duration of unemployment spells, reducing reemployment rates and future wages.

*Corresponding author: Sreeraahul Kancherla (skancherla@berkeley.edu).
All findings, opinions, and errors are those of the authors alone and do not represent the opinions of the California Employment Development Department (EDD). We are indebted to Jesse Rothstein, Emmanuel Saez, and Danny Yagan for their detailed feedback and guidance throughout this project. We additionally thank Youssef Benzarti, David Card, Stefano DellaVigna, Alex Gelber, Peter Ganong, Hilary Hoynes, Maxim Massenkoff, Emi Nakamura, Pascal Noel, Daniel Reck, Heather Royer, Geoff Schnorr, Alisa Tazhitdinova, and Till von Wachter; seminar audiences at UC Santa Barbara, UC Berkeley, and the California Policy Lab; and conference participants at the 2023 All-California Labor Conference for helpful comments and conversations. Special thanks are due to Till von Wachter and the California Policy Lab for administrative support. We also thank Julia Cheung for excellent research assistance using aggregated, publicly-available data. We gratefully acknowledge the California Employment Development Department's Labor Market Information Division for their partnership in producing this analysis. Sree additionally gratefully acknowledges financial support from the National Science Foundation's Graduate Fellowship Research Program (Grant No. 1752814), the Center for Equitable Growth, and the California Policy Lab. Any remaining errors are our own.

1 Introduction

Unemployment insurance (UI) is the primary policy instrument in the United States to support unemployed workers during job search. The program, which disburses temporary cash benefits to claimants during unemployment, provides meaningful consumption insurance against earnings losses (e.g., Gruber, 1997; Ganong and Noel, 2019) but also prolongs the length of time spent unemployed (e.g., Katz and Meyer, 1990; Card and Levine, 2000). These extended durations partly reflect extended job search for liquidity-constrained households, who can now *afford* to spend more time looking for jobs as a result of receiving UI benefits (Chetty, 2008; Landais, 2015).

However, a key unsettled question is the extent to which UI's subsidy to job search actually improves claimants' reemployment outcomes. Prior work on job quality, overwhelmingly focused on short-run outcomes, finds mixed effects on workers' reemployment wages (e.g., Lalive, 2007; Schmieder et al., 2016; Nekoei and Weber, 2017). Existing research designs also leverage variation mostly affecting the long-term unemployed, a negatively selected group for which additional time spent searching may be less productive. To what extent do differences in job seekers' liquidity during unemployment affect reemployment outcomes and long-term labor market trajectories throughout the UI spell?

In this paper, we use confidential UI administrative records from California's Employment Development Department (CA EDD) to analyze the role of liquidity in job search, reemployment, and long-run labor market outcomes for a broad sample of claimants. We leverage delays in UI payments, which create wealth-constant, high frequency variation in claimants' liquidity during the unemployment spell. These delays, which occur frequently in the UI system, temporarily reduce claimants' cash on hand until benefits are actually paid.[1] Since UI payment delays can be driven by non-random factors, we exploit plausibly random variation caused by a partial system glitch at the California UI system in 2013.[2] The outage, which was caused by contractor errors during an IT infrastructure upgrade, froze UI benefit payments for approximately 211,000 claimants, but did not affect an observably-similar set of other active claimants.[3] This rich setting allows us to examine the effects of a pure liquidity shock for workers at different points in their unemployment spell, including UI claimants who have just begun receiving benefits. Our main research design compares outage-delayed claimants to a comparable set of unaffected claimants after matching on pre-glitch observables to remove potential residual differences. Within the matched sample, the average delayed claimant had 2.6 weeks of UI benefits delayed ($825), and waited an average of 34

[1] Between 1999 and 2018, about 15-20% of UI payments in California (and 7-10% of UI payments nationwide) were delayed each month. Figure 1 shows the frequency of payment delays between 1999 and 2018 using public data from the United States Department of Labor. These delays received particular attention during the early Covid-19 pandemic, in which most state systems were overwhelmed by UI applications and many claimants faced long wait times to receive benefit payments. Our paper highlights the fact that this is a more general feature of the UI system, and exploits a random shock that is not contemporaneous with pandemic-related changes in the labor market. See Section 2.2 for more details on historical patterns and how we define a payment delay in our setting.

[2] For example, payments may be delayed due to claimant-driven issues, such as incomplete paperwork or claimant eligibility concerns. We discuss these factors and additional reasons for payment delays in Section 3.

[3] Using additional data on claimant home addresses and residential moves, we verify that the system glitch affected claimants throughout California, rather than affecting particular geographic areas.

days before all benefits were eventually paid.

We find that these liquidity shocks had large effects on UI claimants' unemployment duration, reemployment outcomes, and long-term labor market trajectories. Delayed claimants ended their UI spell 2.4 weeks sooner, driven by differential exit rates at the beginning of the system glitch, and were about 7 percentage points (22.4%) less likely to exhaust UI. These higher UI exit rates reflect higher job-finding rates, rather than simply leaving the UI system due to discouragement. Delayed claimants were reemployed 5.6 weeks earlier, and about 3 percentage points more likely to ever be reemployed within 25 quarters following the outage.[4] We find that these faster reemployment rates did not result in delayed claimants taking worse jobs: Delayed claimants' first post-UI firms appear to be higher quality, with 5.1% higher average coworker wages and 1.1% larger firm-level wage premia. In addition, these jobs represent better idiosyncratic worker-firm matches: workers are more likely to be reemployed in their pre-UI industry, have shorter commuting distances, and stay at their new employer for 0.56 more quarters. These improved labor market effects are also highly persistent over time: five years after the glitch, delayed claimants have 2.5 percentage points higher likelihood of employment, $500 higher labor earnings each quarter, and 5% higher earnings conditional on employment. Taken together, our results suggest that UI benefit provision has large and persistent costs in terms of future job quality.

A unique feature of our variation is that the system glitch affects the UI system at a single point in calendar time, so that claimants are affected at different points into their respective spells. We are thus able to estimate heterogeneous effects of liquidity shocks at any point of the UI spell, unlike prior work relying on discontinuities in the potential UI duration schedule (Lalive 2007; Nekoei and Weber 2017; Schmieder et al. 2016) or discrete changes in the UI regime over time (Johnston and Mas 2018; Lindner and Reizer 2020). We find that claimants early in their UI spell are most responsive to the liquidity shock: Short-term employment and earnings effects are around twice as large for claimants under 15 weeks into the UI spell as for claimants who had been on UI for 25-50 weeks prior to the shock.[5] We then show that these short-term differences are also persistent over time, with the newest UI entrants exhibiting the largest long-term effects.[6] This novel heterogeneity in treatment effects by time into the spell highlights the importance of examining the entire pool of UI claimants to understand the broad labor market effects of the program.

[4]While we interpret delays as temporary liquidity shocks, claimants may have (erroneously) interpreted a delayed UI benefit payment as a signal of ineligibility, leading to a perceived permanent benefit cut. This is unlikely to be the case for two reasons. First, delays are common in the UI system; about 20% of payments are delayed each week, and so about 42.3% of claimants had already faced delays before the outage started. Second, we broadly examine agency and news sources from this period to better understand the information that claimants would have seen during the outage. We show that both EDD website notices and public reporting emphasized the temporary nature of the outage and the fact that it was system-initiated. It is thus reasonable to expect that claimants following these sources of information would not have interpreted the outage as a reduction in their UI benefits.

[5]This treatment effect profile over the spell is consistent with recent work on the macroeconomic effects of UI extensions, where the treatment effects of potential benefit duration on aggregate unemployment are biggest for shorter baseline durations (Acosta et al., 2023).

[6]This treatment effect heterogeneity does not appear to be driven by observable changes in demographics over the spell; re-weighting claimants to balance the covariate distribution over the observed weeks into the UI spell at the time of the outage results in somewhat *larger* differences between claimants who are only a few weeks into their UI spell and claimants who have been on UI for substantial amounts of time.

An empirical challenge is that our administrative data do not identify causes of payment delays, and so we cannot directly observe which claimants' delays are due to the glitch. Intuitively, the pool of UI claimants delayed during the system error can be thought of as composing both "compliers", whose benefits are delayed as a result of the system outage, and "always-delayed" claimants, who would have experienced a delay regardless of the system outage, for example due to errors in their UI application or other factors. If always-delayed claimants are negatively selected, for example, then naive estimates comparing delayed and non-delayed claimants will be negatively biased. We proceed in two steps to isolate the causal effects of a payment delay. First, we leverage detailed institutional context for the system error and impose targeted restrictions to identify claimants whose delays were likely due to the random glitch. As a validation check, we show that these restrictions alone produce samples of delayed and non-delayed claimants that appear very similar along observable characteristics.[7] To minimize any remaining pre-treatment differences, we next use a two-step propensity score matching design that compares claimants delayed during the outage to a comparable set of unaffected claimants that are matched on a rich set of demographics, earnings histories, and pre-outage UI claim characteristics. As a validation of this approach, we show that pre-outage characteristics unused in the matching process balance almost exactly between the two groups.

Our finding that liquidity shocks induce both shorter non-employment durations and *higher* earnings is consistent with recent models of job search incorporating duration dependence, a negative causal effect of unemployment on future labor market outcomes (Schmieder et al., 2016; Nekoei and Weber, 2017).[8] Liquidity provision from UI generates a trade-off for job search outcomes: while increased cash on hand allows the unemployed to encounter a broader pool of potential jobs, duration dependence also implies that the job seeker draws from a worse job offer distribution over time. Given that the delayed benefits (which cause earlier exits from the UI spell) *improve* labor market outcomes, the duration dependence channel is likely to dominate in our setting.[9] Using our delay variation, we present suggestive evidence for this mechanism by testing the extent to which treatment effects on nonemployment duration explains treatment effects on workers' future labor market outcomes across different subgroups. Our results are consistent with duration dependence: larger declines in UI duration predict lower short-run employment rates and quarterly earnings, with particularly strong responses for claimants early into their UI spells. The effects of unemployment duration on subsequent employment and earnings are present at least five years

[7]Using glitch-induced variation is important as delayed claimants are non-random in general. We test the nature of this selection by repeating the same sample restrictions for placebo shocks before and after the true system outage. We find large differences in covariates for the placebo periods, suggesting that our context-driven restrictions work well at capturing delayed outage compliers induced by the glitch. See Section 3.5 for more details.

[8]Duration dependence may represent employer statistical discrimination against the long-term unemployed, workers' human capital depreciation over the unemployment spell, or a combination of both factors (Dinerstein et al., 2022; Cohen et al., 2023).

[9]The fact that claimants stay unemployed "too long" may reflect behavioral biases, potentially due to overoptimism about job-finding from unemployment (Mueller et al., 2021), lack of knowledge about duration dependence, or other factors. At the same time, it can also arise in alternate rational expectation models, for example if agents discount future wages in favor of current consumption (Nekoei and Weber, 2017).

after the system glitch, which suggests that duration dependence matters even for long-run outcomes. These long-run patterns imply that longer spells without employment may put workers on a *permanently* lower earnings trajectory. This novel result suggests large potential labor market benefits of recent proposals to "front-load" UI benefits, which would provide larger benefits upfront to ease liquidity constraints, but reduce benefits later in the spell to disencentivize extended UI durations (e.g., Pavoni and Violante, 2007; Lindner and Reizer, 2020; Ganong et al., 2022).

An important component of our variation is that we can examine the net effects of the trade-off between improved job quality and duration dependence effects at every point of the spell, instead of focusing on the long-term unemployed as in prior work. This is key because existing evidence suggests that the effects of duration dependence are concentrated early in the unemployment spell. In audit studies, for example, reductions in job posting callback rates occur over the early part of the spell (Kroft et al., 2013; Farber et al., 2016, 2019). However, existing work arguing that duration dependence drives negative reemployment effects of increased UI typically uses sharp increases in potential duration that occur far into the UI spell (30-40+ weeks), for whom we would expect duration dependence to be least costly (Schmieder et al., 2016; Nekoei and Weber, 2017).[10] We find that observed heterogeneous responses in earnings and employment outcomes for early-spell claimants are mirrored by effects on nonemployment duration. A back-of-the-envelope calculation provides suggestive evidence that the negative effects of non-employment duration on wages are large and constant for the first 26 weeks of the UI spell and greatly reduced at longer durations. This novel finding again highlights the importance of examining the entire claimant population to understand the labor market effects of the UI program.

Our paper first contributes to a broad literature on the effects of UI receipt on unemployment duration and reemployment outcomes (Card et al., 2007a; Lalive, 2007; Schmieder et al., 2016; Nekoei and Weber, 2017). These papers, generally using discrete increases in maximum UI duration, usually finds zero or small negative effects on workers' next wage. To our knowledge, we are the first paper to highlight broad longer-run effects of UI on labor market dynamics beyond the worker's first post-UI firm. We find substantial persistence in labor market effects; even five years after the outage, UI claimants have higher employment, earnings, and conditional earnings relative to non-delayed claimants. Since our variation affects a representative snapshot of active claimants, we are also able to identify the effects of UI at each point in the UI spell. This richer setting reveals important heterogeneity, as newer entrants to UI exhibit much larger effects than the long-term unemployed usually studied in previous work. Our detailed administrative data also captures many previously under-explored margins of firm and match quality, highlighting the role of both factors in shaping claimants' long-run labor market trajectories. We estimate a job

[10] In principle, forward-looking UI claimants may also adjust their job search efforts early on in the spell if they place nonzero probability on remaining unemployed until a UI duration extension. In practice, responses are concentrated in the months just before benefit expiration (see for example Schmieder et al., 2016). This pattern suggests that search effort for the long-term unemployed is most responsive to an impending change in benefits, possibly reflecting existing evidence on job-seekers' myopia and overoptimism about job finding (Ganong and Noel, 2019; Landais and Spinnewijn, 2021; DellaVigna et al., 2022). Since most UI spells are relatively short—under 14 weeks in the United States—these patterns may not reflect more general job search behavior.

ladder model of earnings to construct measures of firm and worker-specific pay premia,[11] showing that increased worker wages over time are partially driven by shifts into higher-quality firms with larger pay premia. We also explore worker-firm match quality in new ways by constructing workers' commuting distances to their first post-UI firm using the universe of California firms' establishment locations.[12] We find that delayed workers have shorter commuting distances, reflecting better idiosyncratic worker-firm matches along this margin. Taken together, our results suggest both that UI benefit provision has large and persistent costs in terms of future job quality, and that examining the entire pool of claimants is key for understanding the broad labor market effects of the program.

Our paper also connects to a more general literature in labor economics on the costs of duration dependence(Kroft et al., 2013; Farber et al., 2019; Dinerstein et al., 2022). Duration dependence is an important channel in our setting, and it rationalizes why liquidity shocks can have *positive* impacts on reemployment and future job outcomes. In contrast to recent work arguing that observed duration dependence reflects changing worker heterogeneity, or dynamic selection over the UI spell (e.g., Mueller et al., 2021), we find strong evidence for a causal effect of duration dependence. Since UI payment delays affects claimants at every point into the UI spell, our setting provides unique tests for disentangling these two effects. When examining heterogeneous effects by spell age at the time of the outage, we find that new UI entrants' nonemployment durations are much more responsive to liquidity shocks. Cross-sectionally, these effects do not come at any cost to reemployment outcomes as faster job finding rates go along with higher reemployment wages and better worker-firm matches. These patterns are unlikely to be solely explained by dynamic selection. By reweighting the observed covariate distribution of claimants across the UI spell, we find that treatment effects on labor market outcomes are not explained by negative selection on observables over the spell. Moreover, the strong long-run persistence of delayed workers' labor market outcomes is inconsistent with pure dynamic selection, where delay shocks would simply retime workers' entry into the labor market.

We also contribute to existing work on the effects of liquidity during job search, which generally attempts to understand the moral hazard and search components of increases in unemployment durations (e.g., Card et al., 2007a; Chetty, 2008). This literature typically uses lump-sum severance payouts at job separation, which constitute sizable shifts in both claimants' income and liquidity.[13] In the United States, severance payments are relatively rare and likely correlated with working at higher-quality firms. In contrast, our delay instrument constitutes a novel, high-frequency, pure liquidity shock that is highly common in the UI system: over 42% of all claimants in our sample faced a delay even before the system outage. To our knowledge, we are the first to estimate the effects of this variation, and use it to uncover substantial effects on job search and reemployment

[11] These pay premia measures are often referred to as AKM firm and worker effects, after Abowd et al., 1999.

[12] A small recent literature has examined the effect of job loss on workers' commuting distance to future employment (e.g., Huttunen et al., 2018; Le Barbanchon et al., 2020; Duan et al., 2022). However, these papers do not identify the effect of UI benefits at shaping these effects.

[13] For example, Card et al. (2007a) study the effects of receiving severance pay equal to two months of earnings.

outcomes.

The rest of the paper is structured as follows. Section 2 describes the institutional context, including the California UI system, the timing of UI benefit payments, and the system outage that we exploit for identification. Section 3 describes our data sources, sample construction, and matching design. Section 4 presents reduced-form results on delayed claimants' UI spell, reemployment, and long-term labor market outcomes. Section 5 examines the extent to which our estimates for labor market outcomes reflect duration dependence. Section 6 concludes.

2 Institutional Details

2.1 The California Unemployment Insurance System in 2013

The California UI system, administered by the state's Employment Development Department (EDD), provides weekly benefits for unemployed workers who lost their jobs through no fault of their own. These benefits are generally targeted to replace about 50% of pre-UI income, with both benefit levels and the duration of benefits generally increasing with the level of previous earnings. In California, benefits are capped at $450 per week and claimants may receive full benefits for a maximum potential benefit duration (PBD) of 26 weeks as part of the regular UI program.[14]

Maximum benefit durations are often extended in recessions to allow unemployed workers additional resources in job search. This was especially true in the aftermath of the Great Recession, where a combination of federal and state policies provided for substantially increased PBD. First, California's state-level Extended Benefits program supplemented base durations by an additional 20 weeks. In addition, federal policy also supplemented PBD through the Emergency Unemployment Compensation (EUC) program in June 2008, allowing for a maximum of 99 weeks of benefits from a combination of regular UI and the two duration extension programs.[15] EUC was reauthorized by Congress until January 2014, whereupon all existing claimants receiving extended benefits could no longer receive benefits.[16] In our analysis, we retain claimants who had been receiving UI benefits for under 52 weeks at the time of the outage in September 2013.[17] This pool of UI beneficiaries were eligible for extended benefits through the end of 2013 at latest, implying an ex-post

[14] In practice, the UI system actually calculates a weekly benefit amount and maximum total amount of benefits (maximum benefit amount, or MBA) for each UI claimant, then simply reports the implied duration (MBA divided by weekly benefits) to the claimant. While these two systems are isomorphic for the vast majority of UI claimants, in some cases claimants receive incomplete or "partial" payments that implicitly extend potential duration. We return to this point when discussing partial payments in the next subsection, and again when constructing duration measures for claimants in Section 3.2.

[15] These state and federal extensions differed across states and over time; Rothstein (2011) leverages changing eligibility for benefits to study job search during the Great Recession.

[16] The expiration of EUC at the end of 2013 created a "benefit cliff", removing a large pool of beneficiaries from the UI program. Existing work by Farber et al. (2015) studies this phaseout in detail using data from the Current Population Survey, finding little effect on additional job-finding as a result of the policy lapse.

[17] The ordering of extended benefit programs is important, since program eligibility and authorization was highly time-specific. The EB program automatically "triggers" on and off depending on measured state-level unemployment rates, while EUC was highly dependent on Congressional approval and reauthorizations. UI claimants first receive benefits from the regular benefit program, then EUC, then EB.

benefit duration of at most 73 full benefit weeks for our sample.

2.2 Recertification and the Timing of UI Benefit Payments

As part of the process for receiving benefits, UI claimants must "recertify" on a biweekly basis that they are still unemployed and actively searching for a job. This process starts two weeks after the first benefit payment is received. Recertification is retroactive, covering job search activity for the previous two weeks, and benefit payments are only authorized after the information is received and processed. As part of the process, claimants state if they searched for a job each week, whether they started working in any capacity (e.g., temporary side jobs), as well as questions relating to their availability for work (e.g., too sick to work, attending school or training programs, or refusing work). These responses are used in the calculation of actual benefit payments, as weekly benefit amounts can be reduced (generating a "partial payment") if the claimant reports side earnings, having been too sick to work, or injured and unable to work.[18] Claimants are highly incentivized to report earnings from side jobs, as these employment arrangements are required to be third-party reported to the EDD by employers.[19] The EDD mails out recertification forms every two weeks to be returned via mail, but claimants can also certify for benefits online or via telephone call. At the time of the outage, the EDD processed about 450,000 recertifications per week: about 70% of recertifications were processed on paper forms via mail, 20% were processed online, and the remaining 10% were conducted via a telephone self-service option. Figure A2 shows a screenshot from the paper form, highlighting the biweekly reporting structure and the exact questions that claimants fill out as part of the recertification process.[20]

After the EDD receives the recertification form, computer systems automatically scan and homogenize most information across the different filing methods. Paper forms are especially subject to processing errors, however; at the time of the outage, about 26% of forms needed to be manually reviewed by caseworkers. This process involves validating that the form is scanned and signed properly, that all required information is provided, and that the form matches other existing claim information. Caseworkers can contact claimants for follow-up interviews or to request a new form submission if they find errors or inconsistencies on the initial submission. After the form is accepted, UI benefits for that two-week period are then disbursed to claimants.[21] In California, ben-

[18] Since the UI system calculates and keeps track of a total amount of benefits (MBA) for which a claimant is eligible, rather than the number of weeks, these partial payments can change the total number of weeks for which a claimant receives UI. For example, if a worker reports being sick for 3/7 of a benefit week, only 3/7 of the benefit check is actually disbursed in that week. However, the worker is still able to draw upon the residual amount in a future week, allowing for an "extra" claimed benefit week in which the remaining 4/7 of a benefit payment is disbursed. We return to this point in Section 3.2, while discussing construction of our duration measures.

[19] All new hires in California are required to be reported to a new hire state registry within 10 days. This reporting requirement covers employees of all ages, even those who work less than a full day or part time, and even those who are not retained after their temporary work. For more information, see the EDD's information to firms about new hire reporting in California: https://edd.ca.gov/en/payroll_taxes/new_hire_reporting.

[20] The full paper form can be found here: https://edd.ca.gov/siteassets/files/pdf_pub_ctr/de4581cto.pdf.

[21] For the vast majority of payments, these follow the general biweekly structure with both weeks' payments disbursed at the same time. Sometimes the first and second benefit week's payments can be disbursed on different dates if, for example, the claimant recertified separately for each week or one of the two payments had various issues. In our data,

efits are directly loaded onto a debit card sent to claimants with their first UI payment, so there is no lag time between the EDD's benefit disbursal and benefit receipt by claimants.

As our primary measure observed in the data, we focus on the *payment time lapse*, which is the number of days between the end of a benefit week and the date the corresponding payment was made.[22] This measure is commonly reported in the UI system, as the Department of Labor uses the percentage of payments with time lapses over 14 days as a common metric for UI system performance.[23] Using public data from the Department of Labor, we similarly discretize delays as payment time lapses of over 14 days to characterize the frequency of delayed payments across the US. Figure 1 plots the weekly proportion of continuing claim payments with a delay for California (blue), all US states (red), and individual states (light grey) between 1999 and 2019. In general, we find that payment delays in the UI system are quite common; between 15-20% of payments are delayed each week in California, as well as 8-10% of payments across all US states.[24] The system outage in California, marked on the figure using the red dashed line, is clearly visible as a large and short-term increase in delayed payments (California represents a substantial portion of US UI claims, and so the outage moves the national delay rate as well). We now discuss the institutional details surrounding the outage in more detail.

2.3 The 2013 System Outage

Our empirical strategy in this paper relies on exploiting variation in the timing of UI benefit receipt that resulted from an EDD systems upgrade glitch in September 2013.[25] In the aftermath of the Great Recession, in which the EDD's core IT infrastructure struggled to handle increased claims volume and programmatic changes to state and federal UI policies, a decision was made to redesign and upgrade its IT systems. A key consideration in this effort was that 26% of paper recertifications required manual processing by employees at EDD, a cumbersome and labor-intensive process made worse by the majority of claimants submitting paper recertification forms. The system upgrade, which was carried out by an outside contractor, was broadly intended to shift towards

we observe the exact disbursal date at the benefit week level.

[22]For example, suppose claimants are recertifying for the two week period between August 1st to August 14th, and the payment is made on August 20th. We compute the two payments' respective time lapses as 13 (the first benefit week ends on August 7th) and 6 days, respectively. This timeline is also depicted in Figure A3, which shows each step of the recertification cycle.

[23]Note that this is an imperfect measure since payments are made biweekly; a 14-day time lapse for the second week claimed is a much longer time lapse than 14 days for the first week, since the first week will always have a 7 day gap before being certified. We correct this issue and adjust for the biweekly payment structure when reproducing this graph in the California claims microdata for Figure A1, as described in the next section.

[24]As we describe when discussing the recertification process, these delays are not necessarily random; payment time lapses can arise because of tardy recertifications, incomplete form submissions, or other claimant-side issues. As a result, delayed claimants are plausibly negatively selected relative to the general UI population. We avoid these issues by considering delays during the 2013 system outage, where system-side variation is plausibly uncorrelated with claimant characteristics. We return to this point when discussing the design and data construction.

[25]This upgrade glitch was highly publicized and reported on in the state and local media for having affected a large amount of UI claimants. Background information in this section on the system outage summarizes publicly-available information from a 2013 report by the California State Assembly, following hearings to determine causes and the extent of benefit payment delays for claimants. No confidential or internal documents were used in producing the material in this section.

automatic processing of recertifications without caseworker intervention and free up time for more detailed cases.

The EDD deployed the new UI IT infrastructure to process continuing claims on September 2, 2013. In the process of doing so, it had to transfer and reconvert several years of benefit and wage records between IT systems. This conversion process led to three distinct system glitches that affected payment timing on a subset of UI claims. First, paper recertification forms were not mailed to 110,000 UI claimants. This error was exacerbated by a second glitch in which payments were automatically stopped for claimants who switched recertification methods (between the paper, online, and phone recertification methods). Consequently, claimants who did not receive paper recertification forms and tried to recertify online or by phone had their claim put on hold. A third error meant that current claimants with old (but resolved) issues on their current or any prior claim also saw their claims frozen, an issue that affected around 101,000 claims. Together, this meant that payments to around 211,000 UI claimants were abruptly paused.[26] While these issues were resolved quickly for a subset of claimants, many others went without any benefits for multiple weeks. In our data, we find that the average delayed claimant had $825 in UI benefits, representing 2.6 benefit weeks. The mean delay for any one payment was 25 days, and claimants' delayed payments were fully paid after an average of 34 days. Importantly, all payments were made eventually even if the worker left the UI system and stopped claiming or found a job. As a result, the delayed benefit payments we see in the data are not mechanically related to claimants' job search outcomes. It is worth noting that this sort of temporary outage is not atypical or specific to California; many states attempted to modernize their state UI systems following the Great Recession and were impacted by short-term glitches and payment delays (e.g., Massachusetts, Florida, and Pennsylvania, among others).

An economic interpretation of payment delays is that they represent high-frequency, wealth-constant variation in UI claimants' liquidity during the spell. A payment delay shifts the timing of benefit payment—in our case, over a relatively short horizon—but not the level of benefits, since benefits are eventually received. As a result, these temporary shocks do not induce income effects. An important caveat is that while delays constitute ex-post temporary liquidity shocks, claimants could have perceived them as ex-ante benefit cuts, perhaps erroneously believing that a delayed payment implies future benefit ineligiblity. We see this as unlikely; given the magnitude of the system outage, UI claimants saw frequent coverage of delays from both the EDD and local media sources. In addition, the EDD consistently emphasized that additional processing time was due to a system issue instead of potential claimant-side eligibility issues. Indeed, a claimant seeking information from the EDD's website on September 15, 2013 would have read:[27]

[26] In our data, we can only observe whether a payment was delayed but not the underlying cause (such as switches in claim filing method, old issues with claims, or other claimant-side issues). Our empirical strategy, described in more detail in Sections 3.3 and 3.4, is to use context-driven restrictions to generate a subsample in which the non-mailing of paper forms is likely to be the underlying issue, and then match delayed and non-delayed claims on pre-treatment characteristics.

[27] Quotes taken from the Internet Archive (`https://archive.org/`), which archives historical versions of the Employment Development Department website. Screenshots of these quotes as well as more details are in Appendix D.

"A small amount of certifications will require some one-time manual intervention to be processed. Enhancements will be installed over the weekend [...] *it is not necessary to call the EDD on this issue."*

This was reiterated in further guidance on September 24th:

"The EDD continues to work through a subset of certifications for ongoing unemployment benefits that will require some lengthier, one-time manual processing."

and again on October 7, when the EDD specificially suggested that claimants otherwise continue typical recertification behavior:

"The EDD is working [...] *as quickly as possible. Claimants* [...] *can help us expedite this effort by continuing to submit certifications for continuing benefits."*

A more detailed description of contemporaneous guidance by EDD, including screenshots of EDD updates as well as information on contemporaneous news reporting on the IT infrastructure issues can be found in Appendix D.

3 Data and Sample Construction

3.1 Data Sources

Our empirical analysis links together confidential, individual-level administrative records housed at the California Employment Development Department (CA EDD), the state agency that runs the California unemployment insurance system. We use three distinct datasets: EDD UI claims microdata from 2000-2019; the EDD's Base Wage File, which comprises quarterly linked employer-employee data from 1995-2019; and quarterly firm-level characteristics from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). Next, we discuss each of these datasets in turn.

The UI claims microdata broadly covers most data collected by the EDD over the course of the UI claim. Claim-level data begins with the initial claim filing, which contain detailed, self-reported demographic data collected on an application for UI benefits. These filings comprise the bulk of our demographic data on claimants, and includes information such as race, sex, citizen status, educational attainment, occupation prior to separation, and place of residence. The claims microdata also contain weekly data on processed payments over the course of the spell. For each benefit week, the data includes the claim's weekly eligible benefit amount (the maximum amount a claimant can receive in a particular week), the amount actually disbursed to claimants[28], any reported side job earnings or income tax withholding, and the date the payment is actually disbursed to claimants. This claims data is censored in the sense that workers can exit the UI system because they find a

[28] As described in the previous section, claimants may receive less than the full benefit amount in a particular week if they report side job earnings, injury, or sickness.

job, because they exhaust their benefits without finding a job, or because they fail to recertify for UI benefits even though they would be eligible to receive them (Card et al., 2007b).

To measure labor market outcomes for claimants, we further link the UI microdata to the EDD's Base Wage File, a confidential database which contains quarterly labor earnings information for all UI-covered California wage employees between 1995 and 2021.[29] These linked employer-employee records comprise the near-universe of labor earnings in California, only missing earnings from self-employment, informal work, and some government and nonprofit employers.[30] These records are submitted directly to the EDD by employers, and are validated against other employer-submitted information (for example, the number of employees and total wages paid). Earnings are uncapped and can include wages, salaries, bonuses, tips, vacation pay, and other standard components of labor earnings. All earnings are expressed in real 2019 dollars, inflating using the CPI-RS.

Finally, our data includes California employer information from the Quarterly Census of Employment and Wages (QCEW), which comprises the near-universe of establishments for firms operating in California.[31] QCEW data contain earnings, employment, industry, and address information at the establishment-quarter level, which we aggregate to the firm level (summing across establishments in California) before linking to the earnings data. As a result, we can observe extremely detailed characteristics of a claimant's sequence of pre-and post-UI firms within California.

3.2 Construction of Key Variables

Identifying delayed payments. One downside of our data is that we only observe the benefit week and payment disbursal date for a particular payment, but there are no internal data flags designating a payment as having been delayed. For each payment, we first define the *payment time lapse*, which is the number of days between the end of a benefit week and the date the corresponding payment was made. We discretize this measure, defining a payment as having been delayed if UI benefits are disbursed more than 10 days after the ending biweek for which the claimant is recertifying for benefits.[32] This 10-day cutoff is selected to concord with the EDD's recertification

[29] One potential concern with this data is that we do not observe earnings outside California, and so workers who moved out of the state might be incorrectly imputed as having zero reemployment. This problem is common to all papers that use earnings data from a single state (eg, Jacobson et al. (1993); Couch and Placzek (2010)). To understand the extent to which this is a problem in our data, we link claimants to residential mobility history data from Infutor Data Solutions. We find that a relatively low share of UI claimants ever leave California, and this share does not change across delayed and non-delayed claimants. See Table A2. We return to this point in Section 4.3 when discussing our empirical findings.

[30] For any one employee-quarter, there will be as many observations as that employee held jobs in that quarter. For example, if a worker worked for Firm A in April and May of a given year and then switched jobs to work at Firm B in June, the data will include worker-firm pairs for Firm A and Firm B for the second quarter of that year. In general, we aggregate earnings and employment measures taking into account all jobs, but assign firm-level characteristics using the worker's primary job (the worker-firm pair with the highest total pay for that quarter).

[31] The full QCEW data is maintained by the United States Bureau of Labor Statistics (BLS) and covers all reported establishments in the United States. The EDD has access to this data for California firms by agreement with the BLS.

[32] This measure is intended to capture the fact that in California, claimants recertify for benefits on a biweekly basis. We identify the first payment of the biweek as delayed if its payment time lapse is over 17 days, while the second payment is delayed if its time lapse is over 10 days. This cutoff is similar in spirit to the 14-day cutoff used by the Department of Labor to track payment promptness as a measure of system performance, but adjusts for the biweekly payment cycle.

form, where payments are identified as being made within 10 days (see a screenshot of the form in Figure A2).

Duration of the unemployment spell. To capture claimants' immediate labor supply responses to benefit payment delays, we leverage the weekly payments data to measure the duration of claimants' unemployment spell. Our primary measure is the *current unemployment spell duration*, which we define as the number of calendar weeks between the start of the UI spell and the first post-outage gap in claiming benefits after the start of the system outage. This definition adapts the typical initial spell duration measure used in other work – the number of calendar weeks between the start of the claim and the *first* two-week claiming gap – to our context by allowing claimants to vary unemployment claiming behavior before the outage starts (O'Leary et al., 1995; Card et al., 2015; Landais, 2015; Bell et al., 2023). In practice, the two definitions are nearly the same for all claimants, and we assess robustness to this choice by showing results for alternate duration definitions in Appendix A.[33] As a secondary measure, we also compute whether a UI claimant has exhausted their total benefits. We define a claimant as having exhausted their UI claim if they are unable to apply for UI benefits in the following week, either because they have reached their total amount of benefits available on the claim, duration extension programs have ended in that week, or a combination of the two.

Nonemployment duration. We also use the earnings data to compute UI claimants' nonemployment duration, defined as the number of consecutive quarters without any labor earnings, starting with the quarter *following* the outage. This measure will mechanically undercount the true nonemployment duration by ignoring any within-quarter employment responses for short UI spells, but will also ensure that pre-UI employment is not erroneously counted as a new employer. We define the worker's first post-UI firm as the highest-paying employer in the first reemployment quarter.

Other outcomes. To better understand the change in post-UI reemployment outcomes for delayed claimants, we construct additional measures of firm quality and worker-firm match quality. While most of our outcomes are relatively standard, two novel measures merit additional description. First, we estimate firm-specific wage premia—often called the firm's AKM firm effect, after the pioneering work by Abowd et al. (1999)—to understand the extent to which claimants are moving to better (higher paying) firms. Details of our estimation procedure, as well as standard specification and robustness checks, can be found in Appendix B.2. Next, we also estimate the distance in miles between workers' home address (at the time of the UI claim) and their place of work for their first post-UI firm. This calculation involves imputing establishment (available through the QCEW

[33]Specifically, we compute the following three alternate duration measures: 1) total spell duration, or the calendar weeks between claim start and the last benefit week; 2) claimed week duration, or the total number of weeks that a claimant certified for benefits; and 3) paid week duration, or the total number of weeks that a claimant certified and was disbursed benefits. The total spell duration differs from current spell duration because UI claimants do not have to claim continuously for UI in every week; they can exit temporarily and reopen a UI claim if they still have remaining unused benefits. The total spell duration and claimed weeks duration differ for the same reason, since gaps will be included in the former but not the latter measure. Lastly, claimed and paid weeks durations can differ since workers can receive $0 benefit payouts when they have earned too much from side jobs, report being injured or sick for a given week, or some combination of the two.

data) to each rehired UI claimant based on reported zip codes, and calculating zip pair distances. More details on this imputation and calculation procedure can be found in Appendix B.1.

3.3 Sample Construction

When constructing our analysis sample, we start by extracting the population of active UI claims with a UI payment scheduled for payment in September 2013. We make three primary sample restrictions to define our analysis ”risk set” of UI claimants potentially delayed during the outage. First, given our interest in the effects of liquidity shocks on job search, we drop claimants on special UI programs (for example, workers receiving Trade Adjustment Act payments), expecting a definite recall, or otherwise without job search requirements.[34] Next, we restrict to claimants who report ages between 18 and 70 years old at the time of filing their UI claim. Lastly, we drop claimants who are over 52 weeks into their unemployment insurance spell because data limitations make it hard to differentiate between very long spells or recurring spells for these claimants (O'Leary et al., 1995). Given existing benefit durations in effect at the time—described in more detail in Section 2—all claims in our sample have benefit eligibility at least through the end of 2013. These initial sample restrictions leave us with a starting set of 316,720 UI claimants at risk of having a delayed payment during the system outage.

While we can identify payment delays using the UI claims data, we do not know the reason for which payments were delayed. Intuitively, the pool of UI claimants delayed in September 2013 can be thought of as composing both "always-delayed" claimants, who would have a delay regardless of the system outage, and "compliers" whose benefits are delayed as a result of the system outage.

These always-delayed claims can be due to claimant-side issues, such as tardy recertifications, incomplete form submissions, or ongoing eligibility disputes. Since we cannot directly separate these two pools of UI claimants in the data, our broad strategy is to restrict the sample to claimants whose delays are most likely to be random, and employ a matching design within the remaining pool of UI claimants. As we show, our restrictions alone produce a broadly comparable sample of delayed and non-delayed claimants at the time of the outage, with the matching design removing minor pre-treatment differences between the two groups.[35]

We restrict the sample as follows (these steps, and resulting changes to sample composition, are summarized in Table A1). First, we retain workers for whom we can identify a separating firm with sufficient earnings history to be monetarily eligible for UI. This restriction removes 600 claimants for whom the Base Wage data inadequately covers their earnings history, and reduces our sample to 316,120 claimants. Since workers with serious and ongoing certification issues have a high proportion of their payments delayed, we next remove all claimants for whom *all* UI payments before the outage were delayed. This leaves us with 289,603 claimants. We next want to remove claimants

[34] For example, workers with a job offer or definite recall date within the next 30 days are exempt from job search requirements and can continue to claim benefits until their next job begins. As a result, workers are highly incentivized to report recalls or stored offers to the UI system.

[35] One concern is that these "always-delayed" claimants in our sample are positively selected, which could lead to an upwards bias in our estimates. We discuss this possibility in more depth in Section 3.5.

who have UI eligibility issues, which can be caused by their separating firm disputing the reason for a work separation. This often causes claimants to have delays in their first UI payment, and so we further restrict to claims where the first payment was made on time. Next, recall from Section 2 that some delays during the glitch because previously resolved issues on any ongoing or previous claim were erroneously re-flagged for caseworker review. One non-eligibility based reason for these prior issues can arise when workers report side job earnings on UI, as sometimes caseworkers interpret excessive reported side job earnings as representing a full-time hire and initiate an eligibility review that delays payments. For this reason, our last restriction imposes that the claimant has not reported side job earnings in any benefit week prior to the outage. As expected, this reduces a greater share of delayed than non-delayed claimants. Taken together, these restrictions retain a total of 194,052 spells (about 60% of the initial risk set), of which 68,379 experience at least one delayed payment in September 2013 and 125,673 do not experience any payment delays in that time period. As discussed in Section 2, the California Assembly report providing institutional context for our setting suggests that the total pool of claimants delayed as a result of system upgrade glitches is about 210,000 claims: 110,000 claims affected by the plausibly random and unobserved recertification form mailing issues, and 100,000 claims delayed due to prior issues). Despite imposing relatively strict restrictions to remove the latter group, we still retain a substantial share of claimants affected by the outage.

Cumulatively, our approach to this point has imposed ex-ante restrictions (given the institutional context) that attempt to isolate a pool of UI claimants for whom delays are plausibly random. While we will use a matching design to further eliminate pre-treatment differences (since we cannot definitively say that the shock is random within the remaining claimants), it is instructive to examine raw summary statistics to compare differences between the two groups. Table 1 shows that in general, delayed and non-delayed claimants are very similar along almost all observable characteristics: delayed claimants are slightly more likely to be female, younger, non-Hispanic white, and more educated.[36] These differences are relatively minimal, however, and importantly the two groups exhibit very similar pre-separation labor market experience: they have similar tenure at their separating firm, similar pre-UI earnings, and a similar worker-specific wage premium. Claimants are also quite similar in terms of many UI claim-level observables, such as weekly benefit amounts, potential benefit durations[37], or predicted reemployment scores.[38] While average weeks into spell at outage start is equal in both groups, Figure A5 shows that this masks important heterogeneity; delayed claims are slightly more likely to be both newer and older than non-delayed claims.[39]

[36]While we further match on these characteristics in the next section, we include flexible controls for gender, age, education, and race in all specifications.

[37]These potential benefit durations are calculated ex-post, and include regular programs as well as the total number of extension weeks eventually available based on the week the claim was accepted.

[38]Predicted reemployment scores are calculated internally by the EDD to assess whether claimants are given various additional mandated job search assistance and training requirements.

[39]Note that spell age at outage—i.e. claimants' number of weeks into the UI spell—can be directly mapped to the claim's start date.

One potential concern with the design is that these delays may simply represent a regional shock, perhaps because one or two UI system processing offices in particular were particularly affected by the glitch. While this does not follow from the institutional context, we can show directly that delayed and non-delayed claims are also very geographically similar. In Appendix Figure A4, we show the geographical distribution of delayed claims across counties. There is little evidence of geographical clustering of delays: While the urban centers of California see somewhat higher shares of delayed claims, there is little other evidence of geographical clustering.

Comparing raw summary statistics for the two groups suggests, on balance, that claimants with and without payment delays during the UI system glitch appear broadly comparable. However, Table 1 demonstrates one important difference between non-delayed and delayed claimants: delayed claimants are more likely to have suffered a payment delay even *before* the system outage (52% vs. 37%).[40] This statistic is consistent with the fact that one mechanism for delayed payments was that *old* claim issues were applied to current claims, and we can only imperfectly observe these issues to remove them from the sample. We therefore next construct a matched sample of delayed and non-delayed claimants to account for these observable imbalances between claimants who experienced payment delays during the outage and those who did not.

3.4 Matching

To account for the imbalances in pre-outage delayed payments and claimant start dates, we employ a two-step matching procedure to generate a matched sample of delayed and non-delayed claimants. The general approach is to first exactly match claimants on discrete covariate cells, then retain the closest nearest neighbor non-delayed match (based on an estimated propensity score) to each worker delayed during the outage. This algorithm broadly follows similar work in administrative datasets where the set of control matches is large and a relatively rich set of covariates are available for matching the two groups (Smith et al., 2019; Jäger and Heining, 2022; Schmieder et al., 2023; Arnold, 2023).

We begin by starting with the pool of all claimants that were delayed during the outage. For each claimant, we first identify the set of all exact-cell matches on 1) an indicator for having experienced any payment delay prior to the outage; and 2) for the claim's calendar start week.[41] In the second step, we then identify the nearest non-delayed neighbor (by propensity score) within the exactly-matched cell to the focal delayed worker. The propensity score is calculated as a logistic regression of outage payment delays on prior labor market history (measured between six

[40] Earlier, when looking at weekly payment-level aggregates, we saw that about 20% of payments in a particular week are delayed. The claim-level statistic that 37% of control claimants faced a payment delay before the outage shows that this is not entirely driven by a few claims with consistent issues, but rather that delays are actually quite common in the UI system.

[41] This latter cell is motivated by two considerations. As we noted before, Appendix Figure A5 shows that the distribution of the starting week of UI claims is similar, but not identical between delayed and non-delayed claimants prior to matching. By exact matching on start dates, we can better ensure that duration estimates post-outage only reflect differences that result from the outage. A second key consideration is that we want to examine heterogeneity in treatment effects for claimants affected early on in their unemployment spell versus later on, and so we want to enforce balance on spell timing.

and fourteen quarters pre-outage), pre-outage claim variables (the number of claim filing gaps, delayed payments, and partial payments pre-outage), demographics (race, education, sex, age at filing), and separating firm characteristics (prior industry, tenure, and binned firm size). We allow for sampling with replacement, so that a control worker can be used as the nearest neighbor for multiple treated workers. We are able to find a control match for 68,348 of the 68,379 delayed claimants used in the matching process. Figure A6 shows the distribution of estimated propensity scores for the entire treatment and control samples. The overlap between the two groups is fairly high for most of the score's support. Almost all units have an estimated propensity score of over 0.1 and under 1, reflecting the fact that even within our cleaned subsample of claims, claimants have a nontrivial probability of having a delay.

When we restrict our sample to UI spells for which we can find delayed and non-delayed matches, we are left with a total of 110,621 unique spells, or 35% of the original at-risk of delay sample. In Column 2 of Table 1, we can see that our matching procedure is very successful at eliminating any residual differences between delayed and non-delayed claimants, even among characteristics that are not included in the propensity score. For example, we do not include information about the claimant's estimated worker wage premium, weekly benefit level, or potential benefit duration, or some of the separating firm's characteristics (average coworker pay, firm wage premium). To illustrate the effects of matching in our sample, Figure 2 plots mean employment (Panel A) and quarterly earnings (Panel B) relative to the outage for delayed (red), matched non-delayed (blue), and full-sample non-delayed claimants (semi-transparent blue). Note again that our propensity score only matches on lagged outcomes between 6 and 14 quarters pre-outage, leaving both 15 to 20 quarters and 1 to 5 quarters pre-outage as natural placebo checks. Figure 2 highlights that, even before matching in the two samples, mean employment and quarterly earnings are fairly similar in the full sample of delayed and non-delayed claimants. Both groups exhibit very labor market trajectories, with delayed claimants having slightly higher employment a year before the outage. Our matching algorithm, which incorporates labor market histories between 6 and 14 quarters pre-outage, is intended to correct for this pre-outage difference. In both panels, we find that the matching process does quite well, as the two series nearly exactly overlap for both placebo sets of pre-periods.

3.5 Why Use Outage Delays?

As we outlined in the previous section, administrative reasons for delays are unobserved in our data and so we cannot directly isolate claimants who are delayed due to the system glitch. Therefore, our research design makes context-driven restrictions to remove "always-delayed" claimants contaminating our sample, and then finding nearest-neighbor control matches to the remaining delayed pool. One concern with this strategy is that we cannot be certain that our restrictions remove all always-delayed claimants. If these claimants are positively selected on unobservables, it is possible that random delays have no effect and any resulting positive estimates of delays on labor market outcomes could be driven by selection.

This possibility is ex-ante unlikely given UI institutional context for delays. Delays often happen due to eligibility issues, such as failing to certify on time, or incorrectly filling out paperwork. We would find it reasonable that always-delayed claimants are ex-ante plausibly negatively selected, which would negatively bias our estimates of future labor market outcomes for delayed claimants. Given that we find positive overall effects of delays, it is unlikely that the mixture of claimants in our treated pool explains our main result.

Next, note that given our matching design, always-delayed claimants would have to be positively selected on unobserved characteristics. When constructing the propensity score to measure claimant similarity, we use a very rich covariate specification that captures workers' preemployment history, pre-outage claim history, and demographics. Indeed, we observe almost all information available to the UI system, and so system-originated delays are likely to be captured by observable characteristics.

As a last empirical check, we next construct samples for placebo system glitches in September 2012 and September 2014. If all delays are purely random, we would expect very similar balance across covariates to the actual system upgrade in September 2013. If, however, delays are usually *nonrandom*, then we we would expect starker differences between delayed and non-delayed claimants during these placebo system upgrades. In constructing our placebo system upgrade samples, we perform exactly the same sample restrictions as those described in Section 3.3. Just like for the actual system upgrade, we define delayed claimants as those claimants in the sample who had any UI benefit payment in September 2012 (2014) delayed for more than 10 days. We then run a regression of demographic characteristics on an indicator for being delayed in September of a given year (the placebo system upgrades 2012 and 2014 or the actual system upgrade in 2013). We report the estimated coefficients in Table A3, with Columns (1) and (2) presenting coefficients for the placebo system upgrades in 2012 and 2014 and Column (3) presenting coefficients for the actual system upgrade in 2013. We can see that for the placebo system upgrades, delayed claimants appear a bit positively selected based on observables: They are more likely to be Asian or White, but less likely to be Black or Hispanic. The are substantially more likely to be college graduates or citizens, and a bit more than a year younger than non-delayed claimants. By contrast, these differences are substantially smaller for claimants delayed during the true system outage, indicating a substantially more random component to this cohort.

At the same time, we find mixed evidence on workers' estimated wage premia, which represent a time-invariant measure of worker productivity that should better reflect key unobservable selection. Under this measure, the 2012 placebo group appears to be negatively selected while the 2014 placebo group is positively selected. By contrast, the true 2013 delayed cohort is balanced within delayed and non-delayed claimants on worker wage premia. Our conclusion from this placebo exercise is that, outside the system upgrade shock that we study in the paper, delays are likely to be non-random and potentially positively selected on observables. For the *actual* system upgrade, we find much less evidence of positive selection. There are larger imbalances in the share of college graduates and small imbalances with respect to race, but we employ a matching design to flexibly handle these observable differences . Overall, this suggests that the delays at the time of the system outage are substantially random and provide a unique opportunity to study the effects of liquidity shocks during unemployment spells.

4 Results

4.1 How Large Were Liquidity Shocks During the Outage?

We begin our analysis by characterizing the liquidity shocks that claimants faced during the system glitch in Figure 3. Panel (a) shows the distribution of delayed payments (benefit weeks) during the outage, with delayed claimants facing mean liquidity shocks of 2.6 delayed benefit weeks (median: 2 payments). Reflecting the biweekly UI benefit payment cycle, over 80% of claimants had either two or four benefit weeks delayed.[42] Notably, a substantial fraction of claimants (around 30 percent) saw four or five delayed UI payments. Panel (b) shows the distribution of total delayed payment amounts (in dollars) during the outage, showing that the mean claimant had about \$825 in UI benefits delayed during the system glitch (median: \$760). Spikes in the delayed payment distribution occur at multiples of \$450 dollars, the maximum (and most common) weekly benefit amount in California, and reflect mass points in Figure 3, panel (a). Since the average weekly benefit amount for claimants with delayed payments is \$322, a useful intuitive benchmark is to think of a minimum wage worker seeing their paycheck delayed by two to five weeks.[43]

Next, we quantify these shocks by the amount of time that claimants waited before receiving benefits. Importantly, these delay lengths were not known at the start of the outage; as we describe in Section 2.3, communications from EDD suggested that the outage was temporary and would be resolved relatively quickly. As a result, these time-to-payment measures better reflect ex-post characterizations of claimants' liquidity shocks. Panels (c) and (d) of Figure 3 display distributions of average payment delay and the time for all payments to be fully compensated (the amount of time from the first delayed payment until the payout of the last delayed payment). In both cases, the mean and median time lapses are relatively short; the mean delayed payment took about 25 days to arrive, with claimants' receiving full backpay in 34 days on average. Taken together, these results suggest that payment delays were meaningful in the short term, but not large or long-lasting enough to have serious wealth effects for affected claimants.

4.2 Unemployment Spell Effects

Having established that payment delays were economically meaningful, we turn to investigating how UI claimants reacted to these delays. To explore the effect of UI payment delays on job

[42] Claimants could have received an odd number of delayed benefit forms for several reasons, such as filing for only one week on a recertification form, sending in benefit week recertifications on separate forms that were processed independently, or reporting excessive side job earnings or inability to work in those weeks.

[43] The minimum wage in California was 8 dollars per hour in 2013. A full-time minimum wage worker would therefore earn 320 dollars a week, or 99.4% of the mean weekly benefit amount for delayed claimants.

search behavior, we begin by presenting graphical evidence for the rate at which delayed and non-delayed claimants' remain active on UI over the spell. Figure 4, Panel A presents the post-outage UI survival curve, where the horizontal axis is calendar time in weeks since the payment delay shock, and the vertical axis is the share of UI claimants who are still active on the program. As discussed in Section 3.2, we follow the literature and use our preferred definition of current spell duration to represent active claimants: a UI spell is still active until the worker's first two-week gap in claiming UI after the outage, which we interpret as an exit.[44] These two week gaps are somewhat costly, as workers must formally reopen the claim with the EDD, which can involve reassessment of eligibility and lead to additional interim processing delays. In the context of the system outage, these exits could theoretically represent a mixture of true exits from UI, short-term side jobs, or discouragement from continued UI recertifications as a result of the delay. We therefore supplement our measure of unemployment duration with *nonemployment durations*, or the time that it takes before a UI claimant is observed as working at a new job. While a coarser measure—this data comes from quarterly earnings records, and can only be measured at the quarterly level—we use changes in nonemployment duration to distinguish whether claimants are simply leaving UI or getting new jobs.[45]

First, we can see that delays in UI payments result in much faster exits from the UI system. Four weeks after the delay shock, around 20% of delayed claimants have exited UI while only 10% of non-delayed claimants have done so. The large gap in survival probabilities persists and is roughly parallel over time for the next 15 weeks, indicating that most exits induced by the delay occur within a few weeks after the outage. At around 19 weeks, corresponding to the end of 2013, both groups exhibit large drops in survival due to the sharp end of extended benefits at that time. Only claimants who were very early in their spells at the time of the outage remained on regular benefits at the end of the year, and these claimants almost entirely account for the small continued survival after this point. The longer right tail in both curves is driven by partial payments, which can implicitly extend duration (see Section 3.2 for a detailed description). While our preferred measure of UI duration is relatively standard in this literature (O'Leary et al., 1995; Card et al., 2015; Landais, 2015; Bell et al., 2023), we assess robustness in Figure A7 by presenting several alternate definitions and find qualitatively similar results.

One potential story for the gap in UI survival is that claimants were simply discouraged by the glitch and chose not to continue recertifying. We therefore repeat our analysis by presenting a survival curve for nonemployment, or the time between the outage and the claimant's first post-UI job, in Figure 4 Panel B. While the data is more coarsely defined, it shows a similar qualitative pattern: in the quarter following the glitch, about 50% of delayed claimants entered employment compared to 40% of non-delayed claimants. While this gap shrinks in the following quarter, it is

[44] O'Leary et al. (1995); Card et al. (2015); Landais (2015) and Bell et al. (2023) use the first such two-week gap, and denote this measure as the *initial* spell duration. We amend the definition slightly to incorporate the fact that all claimants are active as of the beginning of the outage, and so we need to look for the first post-outage gap in claiming.

[45] This distinction is meaningful in the UI context. In Austrian data, Card et al. (2007b) find that UI exit spikes at UI benefit exhaustion reflect the fact that claimants are no longer claiming benefits rather than taking up new jobs.

remarkably persistent over time; even after 25 quarters, delayed claimants are about 3 percentage points more likely to have been ever reemployed. Figure A8 further presents the distribution of post-outage spell weeks and nonemployment durations after the outage, highlighting the fact that a substantial mass of spells ended much earlier for delayed claimants.

For a more parametric description of these short-run responses, we estimate the following regression for a broad set of short-run outcomes:

$$y_i = \alpha \text{Delayed}_i + \mathbf{X}_i'\gamma + \epsilon_i \tag{1}$$

where i indexes UI claimants, y_i is the outcome of interest and $\mathbf{X}_i$ is a vector of controls (age, race, and fixed effects for educational attainment, pre-UI industry, and the first calendar week of the UI spell). Delayed_i is an indicator of whether UI claimant i had at least one UI payment delayed in September 2013. The identifying assumption is that for our matched sample, there are no unobserved differences driving the propensity to have payments delayed at the time of the system glitch.

We start by presenting evidence on the effect of payment delays on UI durations, the likelihood to exhaust UI, the duration of non-employment, and the propensity to ever become reemployed in the future in Table 2. We can see that claimants who face delays during the outage are about 7 percentage points (22.4%) less likely to exhaust UI, and have UI spells that are 2.4 weeks shorter (implying a 18.3% drop in the post-spell duration).[46] These shorter UI durations also translate into shorter *non-employment* durations: Claimants with delayed payments are 4.6 percentage points more likely to be reemployed within four quarters, are 3 percentage points more likely to *ever* be reemployed, and have non-employment durations that are 0.44 quarters shorter on average.[47] Recall that the difference between non-employment durations and UI durations is that the former measures time to a first job: the fact that claimants also have shorter non-employment spells demonstrates that reduced UI durations reflect faster reemployment rather than mere discouragement following a delay.

Next, we attempt to rule out alternate mechanisms that could be driving our reemployment results. One possibility is that differential state-level mobility of delayed and non-delayed claimants could be driving our results; if non-delayed claimants are more likely to leave the California labor market (and thus the covered employment we observe in our data), this could mechanically generate the differential reemployment rates we observe in the data. However, ex-ante it is not clear why non-delayed claimants, who have not suffered a negative shock relative to the status quo of receiving benefits, would suddenly change their mobility over time. We can more directly test this mechanism by matching UI claimants to residential moves data from Infutor Data Solutions and present estimates for mobility over different horizons in Table A2. Column 1 shows that

[46] We reestimate this effect using alternate measures of UI duration in Table A4 and find qualitatively similar estimates.

[47] An important note here is that we do not restrict or top code non-employment durations in any way. When computing non-employment durations, the only restriction we impose is that claimants are reemployed at some point after the UI payment delay shock. Since the outage happens in the third quarter of 2013, and our earnings data follow claimants until the end of 2019, this implies that the maximum non-employment duration in our setting is 25 quarters.

while Infutor data coverage is incomplete in our sample—we can link about 65% of claimants to an identified residential history—coverage is not different across the delayed and non-delayed samples. In addition, claimants in both groups are characterized by an extremely high rate of staying within California: about 92% of claimants stay in California through the end of 2019, and it does not appear that either group has differentially higher rates of mobility out of the state.[48]

4.3 Reemployment Effects

How should we interpret these substantial changes in duration? There are at least two potential explanations. First, in a standard model of job search under extreme liquidity constraints, UI delays may cause workers to take any job they can find to meet current consumption needs. However, extreme liquidity constraints are not necessary to rationalize this effect; in behavioral models of job search with reference dependence (e.g., DellaVigna et al., 2017, DellaVigna et al., 2022), workers exert increased search effort when the gap between reference-point consumption and actual consumption is high. To begin to disentangle these two mechanisms, we look at the characteristics of the jobs workers find after their UI spell. If workers are simply taking the first job they can find, we would expect that delayed claimants' first post-UI hiring firm to be of lesser quality than non-delayed claimants. In Table 3, we reestimate Eq. 1 for a broad range of firm-level characteristics. We focus on firm-level characteristics due to the fact that we only observe post-UI labor market outcomes at the quarterly level; since the shock happens in the last month of 2013q3, small changes in the timing of reemployment within quarter can materially affect a worker's reported earnings.

Looking at the first job following the UI spell, we find that claimants who experienced payment delays work at broadly *higher* quality firms. First, we find that average coworker pay at next hiring firms is about 5.1% higher. This effect mixes together having higher quality coworkers and a firm-specific quality measure. To disentangle these two components, we estimate a job ladder model of earnings to construct firm-specific pay premia (often referred to as AKM effects, after initial work by Abowd et al., 1999). These pay premia can be interpreted as a firm-specific component of worker earnings received by all workers at the firm independent of specific workers' productivity.[49] We find that increased coworker pay is mostly driven by higher-quality coworkers at the new firm, as we estimate that only 1.1% of the increase in wages is attributable to the hiring firm's pay premium. Next, we estimate these effects in changes relative to the claimant's pre-UI separating employer. First, note that the control means for both outcomes highlights the fact that, on average, workers appear to move to worse quality firms after a spell of unemployment (for example, Column 3 shows that non-delayed claimants' rehire firms have 23% lower average coworker wages relative to their separating firm). Our estimates show that delayed claimants exhibit *smaller* losses in firm

[48]One concern with this approach is that the computed Infutor coverage may precisely select claimants for whom residential out of state mobility is low. Reassuringly, we find very similar percentages of Californians always staying within the state when using alternate data sources, such as credit bureau records.

[49]More details on this estimation process, including broader interpretation and standard specification checks, can be found in Section B.2.

quality, as measured by the change in average coworker pay and firm premia.[50] In Table A7, we also estimate effects on the hiring firm's size, a standard alternate metric for firm quality (motivated by the idea that large firms are more likely to pay wage premia), and find that delayed claimants are slightly less likely to work at single-worker firms and more likely to work at 50+ person firms. However, these results on firm size are very small and economically insignificant. In general, we find that fast job finding rates for delayed claimants do not appear to be a result of delayed claimants taking jobs at worse firms.

Another possibility is that delayed claimants accept jobs that are worse along other important dimensions. For example, maybe they accept very long commutes or start working in other industries, making their new job a worse fit for their skill set. In Table 4, we report regression results for a set of measures for worker-firm specific match quality. Consistent with the firm quality evidence, we find that delayed claimants find jobs at firms that are *better* matches. First, Columns 1 and 2 indicate that the worker-firm specific estimated pay premium is also higher, indicating higher surplus from the hire. This is also reflected in Column 3, which shows that delayed workers stay at their first next hiring firm for 0.56 more quarters. These effects suggest that even if delays induced some claimants to take short-term jobs to meet liquidity needs, this effect is outweighed by others taking jobs that last longer and are of better quality. One counterargument is that pay differences may generally reflect compensating differentials, in which workers are being paid higher wages to counteract nonpecuniary costs of the firm. To address this question, we next examine various non-wage components of match quality. Delayed claimants have shorter commutes after reemployment, by about 2.8 miles or 3.8%. Delayed claimants are 3.6 percentage points (7.1%) less likely to switch industries, 11.2 percentage points more likely to return to any prior firm, and 4.9 percentage points more likely to return to their separating firm. Taken together, these results are in line with increased search effort and productive search, rather than UI payment delays translating into claimants scrambling for jobs and taking any offer they can find.

Given the large effects on delayed claimants returning to their separating firm, we investigate whether delayed claimants who *do not* return to their previous employer also find jobs more quickly while having no worse reemployment outcomes than non-delayed claimants. In particular, one might be concerned that we are picking up workers who had more intact matches to their prior employer which may have caused payment delays and also drives reemployment outcomes.[51] In Appendix Table A8, we can see that our duration results are hardly affected by restricting the sample to claimants who do not return to their prior employer: All coefficients of similar magnitude to our full-sample estimates. Point estimates for the likelihood of exhausting UI and UI duration in weeks are very close to their full-sample counterparts. Estimates for the effect on measures of nonemployment shrink by eight to 20 percent, but they are still of substantial magnitude and highly

[50] This within-claimant comparison procedure has the additional benefit that it differences out pre-treatment differences (Schmieder et al., 2016).

[51] One potential mechanism here is that claimants may already do some temporary work for their prior employer. If earnings exceed certain thresholds or UI claimants fail to correctly report this work to EDD, continuing claims may take longer to process, resulting in an observed "payment delay" that is actually a true UI claim issue.

statistically significant. Another important question is whether the higher job finding rates come at the cost of worse reemployment characteristics for the subset of claimants who are not returning to their separating firm. As we can see in Appendix Table A10, delayed claimants still work for firms with higher worker-firm-specific wage premiums, are less likely to switch industries, and are more likely to return to a former employer *other* than their separating firm. However, the effects on commuting distances mostly go away and the effect on switching industries is much smaller than for the full sample. Nevertheless, there is no indication that claimants with delayed payments who do not return to their separating firm find jobs that are worse matches than those found by claimants without delayed payments. Taken together, these results suggest that returning to the separating firm is one important mechanism through which claimants react to delayed UI payments, but returning to the separating firm is far from fully explaining our results on job finding.

4.4 Long-Term Effects & Persistence

An important question is whether our short-term estimates translate into permanently higher rates of employment and wages, or whether effects dissipate or even reverse over time. While UI claimants hit with a payment delay manage to find new jobs quickly, these jobs could potentially be worse in terms of career progression or unobservable amenities. In this case, the positive short-run effects might mask substantial long-run costs of payment delays where delayed claimants see much worse outcomes years after the delay. In order to tackle these questions, we begin by examining raw mean employment and quarterly earnings for delayed and non-delayed claimants in Figure 5. As we saw earlier in Figure 2, which presented these means for delayed, all non-delayed, and matched non-delayed claimants, the two groups' pre-outage labor market trajectories are very similar. This pattern is partially due to our matching design, as we include employment and quarterly earnings between 6 and 14 quarters pre-outage while estimating the propensity score (marked by green dashed vertical lines). However, we see no differential trends in other pre-periods (both 15-20 quarters pre-outage and 1-5 quarters pre-outage) that were not included in matching, which provides a reassuring check that the matching process works well for identifying observably similar claimants.

Figure 5 also shows that both groups suffer large (and similar) drops in employment and quarterly earnings in the quarters immediately before the outage. Since all claimants are receiving UI benefits at the start of the outage, this pattern reflects the fact that they have recently entered unemployment. These labor market losses are persistent over time in both groups: it takes five years for quarterly earnings to approach pre-unemployment levels, and employment trajectories never fully recover in either group.[52] However, both panels reveal a striking pattern: while both groups suffer similar employment and earnings losses relative to the quarters pre-outage, delayed claimants suffer relatively *smaller* losses in the short run.

[52]This finding is consistent with work studying labor market trajectories around unemployment in particular (e.g., (Jacobson et al., 1993; Couch and Placzek, 2010; Schmieder et al., 2023)). These papers typically find a large, permanent effect of job loss that lasts for many quarters over time.

To better understand causal impacts of the delay on these trajectories, we estimate the following dynamic difference-in-difference model for our matched sample:

$$y_{i,t} = \alpha_i + \gamma_{I(i),t} + X_{i,t} + \sum_{k=-12}^{25} D_{i,t}^{k}\delta_k + \varepsilon_{i,t}, \tag{2}$$

where i indexes individuals, t indexes calendar time in quarters, $y_{i,t}$ is the outcome of interest (for example, quarterly earnings or employment), α_i is a person fixed effect, $\gamma_{I(i),t}$ is a calendar-quarter-by-education-by-displacement-industry fixed effect, $X_{i,t}$ are separate quadratic age profiles by gender, race, and education, and $D_{i,t}^{k}$ are leads and lags for an indicator variable that is one for claimants who were hit with a payment delay shock as part of the IT infrastructure upgrade. Our flexible controls for industry and education are motivated by the fact that we saw pre-matching differences in education and the distribution of benefit start dates, and that industry-specific layoff trends vary differentially over the year. In practice, given that we match on these characteristics, the additional fixed effects do not materially change our estimates.

This regression compares workers in the matched sample with and without payment delays who were laid off from the same industry, with the same level of education, at the same point in time. Our identifying assumption is one of conditional independence: For a our propensity-score matched sample, experiencing a delay shock in September 2013 is as good as randomly assigned. Importantly, the delay shock happens at the same point in calendar time for all workers, so our estimation is not subject to the concerns raised in the literature on staggered event study designs (Borusyak et al., 2021, Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020, Sun and Abraham, 2020).

We begin by presenting estimated effects on quarterly earnings in Figure 6. First, we can see that the evolution of earnings prior to the delay shock is very similar. Recall that we included the claimant's earnings history between 6 and 14 quarters prior to the delay in the propensity score, so estimates between these two points are mechanically close to zero. However, there is little evidence of a differential earnings trajectory between treated or control claimants between either 15 and 20 quarters prior to the outage, or the previous five quarters before the outage. In the quarter of the delay, delayed claimants have around 450 dollars more in earnings than non-delayed claimants, likely driven by the fact that the outage happened towards the end of 2013q3, and delayed claimants exited earlier than non-delayed claimants. Looking at the first quarter post-outage, we estimate a $1,100 effect on earnings that slowly dissipates and stabilizes at around 500 dollars one year after the delay shock. From then on, the earnings effect of the delay shock remains strikingly stable with the earnings difference between delayed on non-delayed claimants hovering around 500 dollars from one year to eight years after the delay shock. This result implies that delayed claimants are not simply scrambling for a job that leaves them worse off in the long run. Rather, the higher UI exit rates and shorter non-employment durations translate into permanently higher earnings.

Next, in Figure 7, we decompose these earnings effects into the extensive and intensive margin responses of delayed claimants. In Panel (a), we can see that delayed claimants are around 11 percentage points more likely to have labor earnings in the quarter of the delay shock. The effect of delays on employment is similarly large one quarter after and then rapidly declines in the next two quarters. Thereafter the effect of delays on employment gradually declines, but it never falls below an effect size of around 2.5 percentage points higher employment among delayed vs. non-delayed claimants. This estimate is roughly consistent with our previous estimate of delays increasing the probability of being ever-reemployed by about 3 percentage points. Next, we try to understand the extent to which these additional employment effects come at a cost to conditional earnings and firm quality).[53] Panel (b) shows these effects for conditional quarterly earnings. At the onset of the shock, conditional earnings are the same between delayed and non-delayed claimants. In the quarter after the system outage, conditional earnings for delayed claimants are 1,000 dollars higher than for non-delayed claimants. The effect of delays on conditional earnings then declines rapidly, but it remains positive throughout our sample period with delayed claimants earning 250 to 500 dollars more conditional on being employed even five to six years after the delay shock. These broad patterns also hold when examining log earnings in Panel (c): the two groups of claimants are very similar pre-outage, surge in the quarter after the outage starts, and stabilizes within a year to about 5% higher wages. We also find similar (but noisier) results when examining the firm-level wage premium in Panel (d), with about 1% of higher wages attributable to higher paying firms. Taken together, we find that delays improve claimants' long run labor market trajectories across a number of margins.

4.5 Heterogeneity by Spell Age at Outage

A unique and important feature of our setting is that—since the outage happens at a fixed point in calendar time—claimants experience delays at different points in their unemployment spell. For example, some claimants were affected right at the beginning of their spell, while other claimants were already receiving UI benefits for a substantial amount of time when the system glitch began. As a result, this variation in claimants' time into the unemployment spell at the outage (which we call "spell age") allows us to identify the effect of payment delays at *every* point of the UI spell. This variation in treatment effects across spell age are important distinguishing moments for modern job search models with liquidity constraints (DellaVigna et al., 2022). For example, if workers are homogeneous across spell age and workers spend down savings over the course of the spell, we might reasonably expect that older spell ages (claimants further into their spell at the start of the outage) will be more responsive to liquidity shocks.[54] Under alternate models incorporating worker heterogeneity in search costs, young spell ages (relatively new entrants to UI at the start of

[53]Since all workers face a period of unemployment over the spell, we estimate regressions on an unbalanced panel of workers who were employed in a particular quarter, following similar approaches in estimating effects on firm conditional outcomes (Lachowska et al., 2020).

[54]This dis-saving behavior is a standard prediction of buffer stock savings models (Deaton, 1991; Carroll, 1997). Ganong and Noel (2019) use de-identified bank account data to show that households dis-save over the course of their unemployment spell.

the outage) are positively selected and would be more responsive to delayed payments (Paserman, 2008).

By contrast, prior work examining discontinuities in maximum benefit duration (Lalive, 2007; Card et al., 2007a; Schmieder et al., 2016; Nekoei and Weber, 2017) or changes to UI system policies (Johnston and Mas, 2018; Lindner and Reizer, 2020) are unable to test the extent to which effects differ over the spell. For example, both Schmieder et al. (2016) and Nekoei and Weber (2017) study age-based discontinuities that extend potential benefit durations from 12 to 18 months and 30 to 39 weeks, respectively, and find very different results on workers' reemployment wages. Schmieder et al. (2016) specifically cites this difference in the timing of variation within claimants' spells as a potential factor reconciling their disparate results, noting that younger spell ages are likely more responsive, but are unable to test this mechanism directly. Our rich variation allows us to nest these approaches in our data and precisely examine the extent to which responses differ over spell age.

In order to examine these heterogeneous effects, we group claimants into five-week spell age bins and estimate the effect of UI payment delays on employment and quarterly earnings for each bin separately. We perform this exercise over two outcome horizons: the short-run response, which averages over the first four quarters post-outage; and the long-run response, which averages between ten and twenty quarters post-outage. As we can see in Panel (a) of Figure 8, short-run employment responses are much stronger for young spells than old spells. Within the first four quarters post delay, delayed claimants in the first three spell-age bins have around 10 percentage point higher employment rates than their non-delayed counterparts. Effects are substantially smaller for claimants in the extended benefit programs (spell ages 25 and over) with employment effects ranging from 2.5 to 5 percentage points for spell ages 25 to 45, and no discernible effect for the oldest spells. We can see very similar patterns for short-term earnings, where young spell ages have much larger earnings effects than older ones. In both cases, these short-term effects show signs of substantial persistence: The long-run employment and earnings effects for the youngest spell ages are around twice as large as the effects for spell ages 25 and over. These findings may also speak to the question why our results are large relative to the existing evidence on the effects of UI on non-employment durations and reemployment wages: Existing evidence mostly relies on discontinuities in potential benefit durations that only apply to very long unemployment spells (examples include Card et al., 2007a; Schmieder et al., 2012, 2016; Nekoei and Weber, 2017) or program features that still mostly apply to long spells (for example, Card and Levine, 2000 study the Extended Benefit Program in New Jersey and Johnston and Mas, 2018 consider a benefit cut from 73 weeks to 57 weeks).

4.6 Heterogeneity by Demographics

Given our estimates that liquidity shocks improve labor market outcomes on average, we next explore the extent to these effects may differ across different demographic groups. Note that relative ordering of effect sizes across groups are somewhat ambiguous ex-ante; while highly creditconstrained groups are more likely affected by liquidity shocks, if these groups are disadvantaged in the labor market then they might not be able to find better jobs at the same rate. In order to explore heterogeneity in ex-ante identifiable groups, we re-estimate Eq. 1 to understand the effects of payment delays on three summary measures: UI duration, short-run employment, and short-run log earnings for various demographic groups. In this context, we define short-run responses as the average response between one to four quarters after the shock (designed to match the patterns we saw in the dynamic difference-in-difference estimates).

These estimates are plotted in Figure 9. While treatment effects are broadly similar across overall, there are some interesting patterns. We can see that women see larger reductions in UI durations than men and they also have larger treatment effects for employment rates four quarters after the shock. On the other hand, Black UI claimants see smaller reductions in initial UI spells than all other racial and ethnic groups, and effects on employment rates are approximately half of that of the other groups. Similarly, young claimants appear to be the least moved by UI payment delays: They see the smallest reductions in initial UI durations and smallest effects on employment rates four quarters after the system upgrade. An interesting case is splitting the sample by below- and above-median pre-separation earnings. While claimants with below-median earnings have much smaller reductions in initial UI durations, employment rates one year after the outage are indistinguishable from those of claimants with above-median pre-separation earnings.

We can see that treatment effects for UI duration and log earnings effects are closely aligned: Demographic groups that have smaller reductions in the number of weeks on UI also have smaller treatment effects for employment four quarters after the delay shock. This pattern is consistent with duration dependence representing an important mechanism for our results: claimants who are on UI longer face worse job or wage offers so that longer UI durations translate into lower long-term employment rates, lower unconditional earnings, and *conditional* earnings.

5 Why Do UI Payment Delays Improve Claimant Outcomes?

In the benchmark job search model, more generous UI benefits increase benefit duration by making recipients more selective in accepting job offers, since available liquidity allows them flexibility in selecting the best offers they encounter. Intuitively, this selectivity channel should increase reemployment wages for workers with additional access to UI. This is starkly at odds with our results, where UI claimants who experienced payment delays find jobs more quickly but *also* earn higher wages than UI claimants who do not experience any payment delays. However, our finding is also consistent with existing research: Schmieder et al. (2012, 2016); Lalive (2007, 2008); Card et al. (2007a); Johnston and Mas (2018) and Lindner and Reizer (2020) all find negative impacts of increased UI duration on future wages. These findings raise the question: why does unemployment insurance appear to prolong unemployment spells while also *worsening* post-UI reemployment outcomes?

One candidate for this pattern is duration dependence, or a negative causal effect of increased unemployment on reemployment outcomes. This effect is usually microfounded through increased statistical discrimination by employers against workers with longer unemployment spells (Kroft et al., 2013; Farber et al., 2016, 2019), or skill depreciation over time spent unemployed (Dinerstein et al., 2022; Cohen et al., 2023). Observed duration dependence is one of the most robust findings in labor economics, with numerous papers identifying that callbacks to job postings, hiring, and offered wages that claimants encounter decrease over the unemployment spell. To our knowledge, Nekoei and Weber (2017) were the first to argue that disparate findings on the effect of unemployment insurance on reemployment wages could be explained by duration dependence: UI induces longer unemployment spells, and—in turn—these reduce wage offers and reemployment rates. If this mechanism is at play in our sample, we would expect larger UI duration and non-employment duration effects to translate into larger long-term employment and wage effects.[55]

The ideal experiment in this setting would leverage variation in workers' nonemployment durations that is unrelated to workers' actual employment offers. Workers' time to next job and reemployment characteristics are typically jointly determined, however, and so this is a restrictive condition that is unlikely to be satisfied in settings outside audit experiments (e.g., Kroft et al., 2013; Farber et al., 2016). We therefore investigate this relationship through a mediation analysis: we leverage treatment effect heterogeneity to better to examine the relationship between treatment effects on claimants' nonemployment duration and future labor market outcomes.

We present three pieces of evidence. First, we test this relationship between nonemployment duration and reemployment characteristics within subgroups of our sample, finding a highly robust negative relationship between nonemployment durations and future labor market outcomes. Next, we extend and interpret our earlier spell age results to characterize a causal channel for implied duration dependence in our sample. Lastly, we conduct a meta-analysis to show a similarly stark negative relationship between the UI effect on non-employment duration and its effect on reemployment wages across studies, with our estimates lying within the confidence interval predicted out of sample using other studies' estimates. We discuss each of these findings in turn.

5.1 Heterogeneity by Predicted Responses

In the previous section, we investigated treatment effect heterogeneity within context-driven subgroups for which we expected effects to differ *ex-ante*. An alternate, data-driven approach is to split subgroups based on claimants' individual nonemployment duration treatment effects more directly. We operationalize this idea through the machine-learning approach of predicting

[55]Using job-seekers' beliefs about job-finding probabilities, Mueller et al. (2021) argue that almost all *observed* duration dependence in unemployment reflects changing worker heterogeneity over the spell, and not a *causal* effect of unemployment on reemployment outcomes. However, this would be inconsistent with the observed persistence of labor market effects for delayed claimants at a particular spell age. Suppose instead that there is zero causal impact of unemployment on reemployment outcomes, and that all differences in observed duration dependence are instead simply dynamic selection. In that case, liquidity shocks may still change short-run labor market outcomes since affected claimants are exiting nonemployment faster. However, these faster exits should not affect *long-run* labor market trajectories since increased time without employment has no direct effect on future reemployment outcomes. But this is not what we find; Figure 8 shows that employment and earnings effects of delays exhibit persistence across almost all spell age bins. We conclude that, at least in our setting, causal duration dependence must be qualitatively important.

claimants' treatment effects using a causal random forest (Athey and Imbens, 2016; Wager and Athey, 2018; Athey and Wager, 2021). We first leverage a rich vector of covariates for prediction, including workers' employment history, demographics, and pre-outage claim information. We ensure that our estimates are "honest" by using ten disjoint subsets (often referred to as folds) of the data to train the model and forming out-of-sample predictions. In other words, the predicted treatment effects for claimants in fold j are formed using the nine other folds j'. We then rank all claimants into ventiles of their predicted treatment effect on nonemployment duration, and estimate labor market effects within each ventile. The results of this exercise are in Figure 10. Panel (a) shows that our procedure has substantial explanatory power for explaining the observed heterogeneity of nonemployment duration treatment effects; the actual nonemployment duration treatment effect within each ventile is roughly increasing and strongly correlated with the group's predicted treatment effect. Panels (b) and (d) plot each ventile's short-run employment and earnings effects against their predicted nonemployment treatment effects. We see a stark negative relationship between the two treatment effects: subgroups with larger predicted decreases (increases) changes in nonemployment duration also have larger predicted increases (decreases) in short-run labor market outcomes, with estimates very tightly clustered around the estimates' line of best fit. Panels (c) and (e), plotting long-run employment and earnings effects, show that these treatment effects are persistent over time.

5.2 Heterogeneity by Spell Age at Outage

Next, we further explore the extent to which duration dependence explains these patterns by again using the fact that individuals are treated at various spell ages. While Figure 8 displays important *heterogeneity* in treatment effects across spell age, it is not necessarily informative about the extent to which extended time spent unemployed is causing treatment effects due to two key confounding factors. First, we saw in Figure 9 that subgroups with higher UI duration responses also exhibit larger labor market responses. This is partially mechanical: if claimants are reemployed faster, they should also have higher accumulated earnings. Following this logic, without knowing the UI duration responses, it is unclear whether employment responses are driven by causal effects across spell age or heterogeneity in duration responses. Secondly, note that the composition of UI claimants changes over the course of the spell. For example, in job search models where workers have heterogeneity in the costs of job search, high search cost claimants stay on UI for longer periods of time since it is more difficult for them to find a job (Ganong and Noel, 2019). Past work generally finds that long-term unemployed claimants are negatively *dynamically selected* (Schmieder et al., 2016), and so it is possible that older spell claimants (say, at 45 weeks) would have better outcomes if they had the average characteristics of younger spell age claimants (say, at 14 weeks).

To separate response heterogeneity and dynamic selection from our estimated responses over spell age, we perform the following mechanical exercise. First, we repeat our estimation in Figure 8, but now condition all outcomes on ever being reemployed.[56] Furthermore, we reweight the

[56] We do this so that we can measure nonemployment durations at every point in the spell.

covariate distribution for each spell age to match the covariate distribution of claimants who are 14 weeks into the spell using inverse propensity weighting.[57] While this is imperfect—older spells can also be dynamically selected along unobservables—we see this as a tractable approach to reduce the effects of dynamic selection. To remove the effects of duration response heterogeneity across spells, we will rescale our estimated labor market responses (employment, earnings) for each bin by the estimated change in nonemployment duration.

Figure A10 shows the result of conditioning on reemployment and reweighting across spells. Focusing on the effects on non-employment duration first, we can see that the pattern is very similar to our unweighted estimates: Young spell ages have large treatment effects while effects are close to zero and large statistically insignificant for spell ages 25 to 50.[58] Unsurprisingly, this translates into much larger short-run employment and earnings effects for young spell ages with employment effects for spell age bins 0 to 15 being on the order of 7.5 percentage points while employment effects for spell age bins 25 to 45 are between 2.5 and 4 percentage points. Short-run earnings effect differences are less stark, but they are roughly twice as large for spell age bins 0 to 15 as they are for spell age bins 25 to 45. As would be expected, conditioning on reemployment attenuates the long-run differences between old and young spell ages. Nevertheless, long-run treatment effects on quarterly employment are larger and significantly different from zero for young spell ages while they are mostly indistinguishable from zero for older spell-age bins. A similar pattern arises for earnings where the difference between young and old spell ages does not lie so much in the size of the effects but their precision: Long run earnings effects are on the magnitude of 500-700 dollars for young spell ages while they fall between 200 and 700 dollars for older spell ages. However, most of these latter estimates not statistically significant.

Next, we proceed by using our reweighted spell-age estimates to normalize our labor market outcomes for each bin by the corresponding nonemployment duration response. In Figure 12, we show the scaled effect one additional quarter of non-employment duration has on long-run attachment to the labor market and earnings.[59] We emphasize that this is a mechanical exercise: we are examining the extent to which heterogeneous labor market effects over spell age can be explained by observable selection and nonemployment duration responses. We interpret remaining heterogeneity across spell age—which would come from differing effects of extended nonemployment at different points in the UI spell—as suggestive evidence for how duration dependence changes over the spell. To line up with Farber et al. (2019), who find that callback rates to job postings are rel-

[57]14 weeks is selected as the median spell age at outage in our sample. To form the weights, we first estimate a propensity score using a logistic regression of a claimant being 14 weeks into the spell at the start of the outage on a rich set of covariates, including race, education, sex, pre-outage labor market history, and separating firm characteristics (industry and binned firm size). Since we are reweighting other spell ages to the 14 week spell age covariate distribution, we generate the spell-age balancing weights as follows. For a predicted propensity score p_i, all spell age claimants receive a weight $w_{i,ipw}$ of 1, while claimants at other ages receive a weight $w_{i,ipw} = p_i/(1-p_i)$. To incorporate these with our initial matching weights, we simply multiply the two weights together (note that our matching weights were exactly balanced on spell age by construction, and the new weights do not undo the balance).

[58]Figure A11 shows this more explicitly for nonemployment duration and employment. The treatment effect profiles for unweighted and reweighted samples overlap for every binned spell age.

[59]Because the effect of payment delays on non-employment duration for old spell ages is close to zero in many cases, we can only do this exercise for claimants who were still on "regular" UI at the time of the payment delay shock.

atively stable for resumes with unemployment durations between one and six months, we might reasonably expect that labor market effects are also constant across spell age. We find evidence supporting this pattern. Looking first at the employment effects in Panel (a), an additional quarter of nonemployment decreases long-run employment by about 7.5 percentage points for claimants less than 5 weeks into the spell, decreasing to about 5 percentage points for claimants between 20-25 weeks into the spell. Despite this slight decrease, we cannot reject the null of constant duration dependence over the course of the regular unemployment spell. Similarly, the estimated effect of one additional quarter of non-employment on long-run earnings is quite stable across spell ages, with estimates ranging from quarterly earnings losses of 1,000 to 1,300 dollars. In general, our results support an interpretation that the costs of duration dependence are quite stable for regular UI spell claimants.

5.3 Meta-Analysis Across Existing Studies

Having shown suggestive evidence that nonemployment duration responses are driving labor market responses *within* our sample, we next show that this these relationships also true across the literature on UI and reemployment job quality more broadly. This point is also made in Nekoei and Weber (2017), which presents a meta-analysis to show the correlation between the UI effect on non-employment duration and the UI effect on reemployment wages in existing studies. In Figure 13, we update this exercise: we begin with the Nekoei and Weber (2017) set of papers, and add more recent estimates from Johnston and Mas (2018), Lindner and Reizer (2020), and the present paper.[60] Two things clearly stand out from the figure. First, there is a stark negative relationship between the UI effect on non-employment duration and its effect on wages. Declines in non-employment duration go along with substantially *higher* reemployment wages while longer non-employment durations go hand-in-hand with reductions in reemployment wages. This is consistent with the argument by Nekoei and Weber (2017) that the effect of UI on wages will be a combination of the effect of UI on reservation wages and the willingness of UI recipients to hold out for better job offers on the one hand and the effect of longer non-employment durations on the wage offer distribution UI claimants are drawing from on the other hand. A second important takeaway from the figure is that, given the observed pattern of duration and wage effects across papers, our estimates are roughly line with previous work. While estimates in Johnston and Mas (2018), Lindner and Reizer (2020) and our setting are larger than in the other papers, one potential explanation for this is that all three papers include negative shocks to UI: Johnston and Mas (2018) uses a permanent cut in benefits, while our paper and Lindner and Reizer (2020) include ex-post transitory changes to the timing of benefits. Nevertheless, these results also show that our estimates are in line with the existing literature, despite being at the upper end of previously estimated effect sizes.

[60] While we immediately follow Nekoei and Weber (2017) for all the papers included in their study, we have to make some assumptions when translating US-based studies to their setting, mostly due to the quarterly nature of state-level UI wage records. Details are in Section C.

6 Conclusion

How do differences in cash-on-hand during unemployment affect reemployment and long-term earnings trajectories for job seekers? We tackle this question by examining high frequency, wealth constant variation in liquidity coming from delayed UI benefit payments during a 2013 system outage in California. We find that delays increased UI spell exits in the short run, decreased claimants' time-to-first-hire, improved firm and match quality at next hiring firms, and ultimately improved long-run labor market outcomes for affected claimants. We interpret these results through a model of job search in which increased job selectivity comes at the cost of a declining job offer distribution due to duration dependence. We find support for this model in our data, as subgroups with the largest decreases in UI duration also exhibit the largest increases in short-term labor market outcomes. Using variation in claimants' time into the UI spell at the start of the outage, we also find evidence that the costs of duration dependence are relatively large and constant for workers in the first six months of their spell.

Our findings have meaningful implications for the optimal UI benefit path. Several recent papers have suggested front-loading UI benefits by providing a higher level of benefits early in the spell and cutting them later in the spell (e.g., Pavoni and Violante, 2007; Lindner and Reizer, 2020). The broad idea in this system is that workers receive necessary consumption insurance and liquidity, but also have reduced incentives to remain on UI. Our evidence highlights the substantial benefits to incentivizing faster reemployment: shortened UI spells translate into better job quality and improved labor market outcomes, especially for claimants early into their spell.

References

Abowd, J. M., Creecy, R. H., and Kramarz, F. (2002). Computing Person and Firm Effects Using Linked Longitudinal Employer-Employee Data. Technical report, Center for Economic Studies, US Census Bureau.

Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High Wage Workers and High Wage Firms. *Econometrica*, 67(2):251–333.

Acosta, M., Mueller, A. I., Nakamura, E., and Steinsson, J. (2023). Macroeconomic Effects of UI Extensions at Short and Long Durations. Working Paper 31784, National Bureau of Economic Research.

Andrews, M. J., Gill, L., Schank, T., and Upward, R. (2008). High Wage Workers and Low Wage Firms: Negative Assortative Matching or Limited Mobility Bias? *Journal of the Royal Statistical Society Series A: Statistics in Society*, 171(3):673–697.

Arnold, D. (2023). Mergers and Acquisitions, Local Labor Market Concentration, and Worker Outcomes. Working Paper.

Athey, S. and Imbens, G. (2016). Recursive Partitioning for Heterogeneous Causal Effects. *Proceedings of the National Academy of Sciences*, 113(27):7353–7360.

Athey, S. and Wager, S. (2021). Policy Learning With Observational Data. *Econometrica*, 89(1):133–161.

Bell, A., Hedin, T., Schnorr, G. C., and von Wachter, T. (2023). UI Benefit Generosity and Labor Supply from 2002-2020: Evidence from California UI Records.

Bonhomme, S., Holzheu, K., Lamadon, T., Manresa, E., Mogstad, M., and Setzler, B. (2023). How Much Should We Trust Estimates of Firm Effects and Worker Sorting? *Journal of Labor Economics*, 41(2):291–322.

Borusyak, K., Jaravel, X., and Spiess, J. (2021). Revisiting Event Study Designs: Robust and Efficient Estimation. *arXiv preprint arXiv:2108.12419*.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics*, 225(2):200–230.

Card, D., Cardoso, A. R., and Kline, P. (2016). Bargaining, Sorting, and the Gender Wage Gap: Quantifying the Impact of Firms on the Relative Pay of Women. *The Quarterly Journal of Economics*, 131(2):633–686.

Card, D., Chetty, R., and Weber, A. (2007a). Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market. *The Quarterly Journal of Economics*, 122(4):1511–1560.

Card, D., Chetty, R., and Weber, A. (2007b). The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job? *American Economic Review*, 97(2):113–118.

Card, D., Heining, J., and Kline, P. (2013). Workplace Heterogeneity and the Rise of West German Wage Inequality. *The Quarterly Journal of Economics*, 128(3):967–1015.

Card, D., Johnston, A., Leung, P., Mas, A., and Pei, Z. (2015). The Effect of Unemployment Benefits on the Duration of Unemployment Insurance Receipt: New Evidence from a Regression Kink Design in Missouri, 2003-2013. *American Economic Review*, 105(5):126–30.

Card, D. and Levine, P. B. (2000). Extended Benefits and the Duration of UI Spells: Evidence From the New Jersey Extended Benefit Program. *Journal of Public Economics*, 78(1-2):107–138.

Carroll, C. D. (1997). Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis. *The Quarterly Journal of Economics*, 112(1):1–55.

Chetty, R. (2008). Moral Hazard Versus Liquidity and Optimal Unemployment Insurance. *Journal of Political Economy*, 116(2):173–234.

Cohen, J. P., Johnston, A. C., and Lindner, A. S. (2023). Skill Depreciation during Unemployment: Evidence from Panel Data. Working Paper 31120, National Bureau of Economic Research.

Couch, K. A. and Placzek, D. W. (2010). Earnings Losses of Displaced Workers Revisited. *American Economic Review*, 100(1):572–89.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.

Deaton, A. (1991). Saving and Liquidity Constraints. *Econometrica*, 59(5):1221–1248.

DellaVigna, S., Heining, J., Schmieder, J. F., and Trenkle, S. (2022). Evidence on Job Search Models from a Survey of Unemployed Workers in Germany. *The Quarterly Journal of Economics*, 137(2):1181–1232.

DellaVigna, S., Lindner, A., Reizer, B., and Schmieder, J. F. (2017). Reference-Dependent Job Search: Evidence from Hungary. *The Quarterly Journal of Economics*, 132(4):1969–2018.

Dinerstein, M., Megalokonomou, R., and Yannelis, C. (2022). Human Capital Depreciation and Returns to Experience. *American Economic Review*, 112(11):3725–3762.

Duan, Y., Jost, O., and Jost, R. (2022). Beyond Lost Earnings: the Long-Term Impact of Job Displacement on Workers' Commuting Behavior.

Farber, H. S., Herbst, C. M., Silverman, D., and Von Wachter, T. (2019). Whom Do Employers Want? The Role of Recent Employment and Unemployment Status and Age. *Journal of Labor Economics*, 37(2):323–349.

Farber, H. S., Rothstein, J., and Valletta, R. G. (2015). The Effect of Extended Unemployment Insurance Benefits: Evidence from the 2012–2013 Phase-Out. *American Economic Review*, 105(5):171–176.

Farber, H. S., Silverman, D., and Von Wachter, T. (2016). Determinants of Callbacks to Job Applications: An Audit Study. *American Economic Review*, 106(5):314–318.

Ganong, P., Greig, F. E., Noel, P. J., Sullivan, D. M., and Vavra, J. S. (2022). Spending and Job-Finding Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data. Working Paper 30315, National Bureau of Economic Research.

Ganong, P. and Noel, P. (2019). Consumer Spending During Unemployment: Positive and Normative Implications. *American Economic Review*, 109(7):2383–2424.

Gruber, J. (1997). The Consumption Smoothing Benefits of Unemployment Insurance. *American Economic Review*, 87(1):192–205.

Huttunen, K., Møen, J., and Salvanes, K. G. (2018). Job Loss and Regional Mobility. *Journal of Labor Economics*, 36(2):479–509.

Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings Losses of Displaced Workers. *The American Economic Review*, pages 685–709.

Johnston, A. C. and Mas, A. (2018). Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut. *Journal of Political Economy*, 126(6):2480–2522.

Jäger, S. and Heining, J. (2022). How Substitutable Are Workers? Evidence from Worker Deaths. Working Paper 30629, National Bureau of Economic Research.

Katz, L. F. and Meyer, B. D. (1990). The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment. *Journal of Public Economics*, 41(1):45–72.

Kline, P., Saggio, R., and Sølvsten, M. (2020). Leave-Out Estimation of Variance Components. *Econometrica*, 88(5):1859–1898.

Kroft, K., Lange, F., and Notowidigdo, M. J. (2013). Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment. *The Quarterly Journal of Economics*, 128(3):1123–1167.

Lachowska, M., Mas, A., and Woodbury, S. A. (2020). Sources of Displaced Workers' Long-Term Earnings Losses. *American Economic Review*, 110(10):3231–3266.

Lalive, R. (2007). Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach. *American Economic Review*, 97(2):108–112.

Lalive, R. (2008). How do Extended Benefits Affect Unemployment Duration? A Regression Discontinuity Approach. *Journal of Econometrics*, 142(2):785–806.

Landais, C. (2015). Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design. *American Economic Journal: Economic Policy*, 7(4):243–78.

Landais, C. and Spinnewijn, J. (2021). The Value of Unemployment Insurance. *The Review of Economic Studies*, 88(6):3041–3085.

Le Barbanchon, T., Rathelot, R., and Roulet, A. (2020). Gender Differences in Job Search: Trading off Commute against Wage. *The Quarterly Journal of Economics*, 136(1):381–426.

Lindner, A. and Reizer, B. (2020). Front-Loading the Unemployment Benefit: An Empirical Assessment. *American Economic Journal: Applied Economics*, 12(3):140–74.

Mueller, A. I., Spinnewijn, J., and Topa, G. (2021). Job Seekers' Perceptions and Employment Prospects: Heterogeneity, Duration Dependence, and Bias. *American Economic Review*, 111(1):324–63.

Nekoei, A. and Weber, A. (2017). Does Extending Unemployment Benefits Improve Job Quality? *American Economic Review*, 107(2):527–561.

O'Leary, C. J., Spiegelman, R. G., and Kline, K. J. (1995). Do Bonus Offers Shorten Unemployment Insurance Spells? Results from the Washington Experiment. *Journal of Policy Analysis and Management*, 14(2):245–269.

Paserman, M. D. (2008). Job Search and Hyperbolic Discounting: Structural Estimation and Policy Evaluation. *The Economic Journal*, 118(531):1418–1452.

Pavoni, N. and Violante, G. L. (2007). Optimal Welfare-to-Work Programs. *The Review of Economic Studies*, 74(1):283–318.

Rothstein, J. (2011). Unemployment Insurance and Job Search in the Great Recession. *Brookings Papers on Economic Activity*.

Schmieder, J. F., von Wachter, T., and Bender, S. (2012). The Long-Term Effects of UI Extensions on Employment. *American Economic Review*, 102(3):514–19.

Schmieder, J. F., von Wachter, T., and Bender, S. (2016). The Effect of Unemployment Benefits and Nonemployment Durations on Wages. *American Economic Review*, 106(3):739–777.

Schmieder, J. F., von Wachter, T., and Heining, J. (2023). The Costs of Job Displacement over the Business Cycle and Its Sources: Evidence from Germany. *American Economic Review*, 113(5):1208–54.

Smith, M., Yagan, D., Zidar, O., and Zwick, E. (2019). Capitalists in the Twenty-First Century. *The Quarterly Journal of Economics*, 134(4):1675–1745.

Song, J., Price, D. J., Guvenen, F., Bloom, N., and von Wachter, T. (2018). Firming Up Inequality. *The Quarterly Journal of Economics*, 134(1):1–50.

Sun, L. and Abraham, S. (2020). Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *Journal of Econometrics*.

Wager, S. and Athey, S. (2018). Estimation and Inference of Heterogeneous Treatment Effects using Random Forests. *Journal of the American Statistical Association*, 113(523):1228–1242.

7 Tables and Figures

Table 1: Summary Statistics

	Delayed	**Not Delayed (All)**	**Not Delayed (Matched)**
Female	.46	.46	.46
Citizen	.88	.88	.88
Age at Filing	42	43	42
Race			
White	.37	.36	.37
Black	.072	.082	.073
Asian	.19	.18	.19
Hispanic	.36	.36	.36
Education			
HS Grad or Less	.43	.44	.43
Some College/Associate's	.3	.32	.3
College Graduate or More	.24	.21	.24
Labor Market Experience			
Labor Market Experience (Quarters)	45	45	45
Estimated Worker Wage Premium	.31	.31	.3
UI Claim			
Weekly Benefits	322	328	320
Potential Benefit Duration	28	29	28
Predicted Reemployment Score	.26	.29	.28
1(Had Pre-Outage Delay)	.52	.37	.52
Weeks Into Spell At Outage	17	17	17
Separating Firm			
Tenure at Separating Firm (Quarters)	17	17	17
Mean Pre-UI Quarterly Earnings	9,351	9,248	9,253
Avg Coworker Pay	12,104	12,261	12,041
Avg Firm Wage Premium	.24	.25	.24
Avg Worker-Firm Match Wage Premium	-.01	-.014	-.02
Pre-UI Earnings			
Wages 6 Quarters Before Filing	8,485	8,324	8,363
Wages 5 Quarters Before Filing	8,350	8,281	8,206
Wages 4 Quarters Before Filing	8,129	8,329	8,062
Wages 3 Quarters Before Filing	8,748	8,754	8,608
Wages 2 Quarters Before Filing	8,258	8,346	8,170
Wages 1 Quarter Before Filing	6,471	6,262	6,407
Number of Claims	68,348	125,640	42,273

Notes: Table presents summary statistics for our analysis sample, after imposing data cleaning restrictions (see Table A1 for more details). For each UI claimant delayed during the outage, we match them with the closest (by propensity score) non-delayed claimant within exactly matched cells of claim begin date and an indicator for previous payment delays (see Section 3.4 in the main text for more details). Column 1 presents summary statistics for all delayed selected matches. Column 2 presents summary statistics for all non-delayed potential matches. Column 3 presents summary statistics for all non-delayed selected matches.

Table 2: Effects on Unemployment Spell Duration

	(1) 1(Exhaust UI)	(2) UI Current Spell Duration (Weeks)	(3) 1(Reemp. Within 4 Quarters)	(4) Ever Reemp.	(5) Nonemp. Dur. (Quarters)
Delayed	-.0695***	-2.38***	.0463***	.0302***	-.438***
	(.0061)	(.12)	(.0032)	(.0024)	(.031)
Control Mean	.31	13	.72	.85	5.8
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	110,621	110,621	110,621	110,621	95,761

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1. 1(Exhaust UI) is an indicator for exhausting UI benefits. Current Spell Duration is the number of weeks until the first two-week lapse in recertification. 1(Reemp. Within 4 Quarters) is an indicator for being reemployed within four quarters from the system outage. Ever Reemp. is an indicator for ever being reemployed as measured by non-zero labor earnings in the California UI Base Wage File. Nonemp. Dur. (Quarters) is the non-employment duration in quarters from the time of the system outage, conditional on ever being reemployed. Standard errors are clustered at the claimant level. See Section 4.3 for more details.

Table 3: Next Firm Outcomes

	(1) First Comp. Qtr. Log Reemp. Wage	(2) Log Avg. Coworker Pay	(3) Firm-Specific Pay Premium	(4) Chg. Log Avg. Coworker Pay	(5) Chg. Firm-Specific Pay Premium
Delayed	.0462***	.0512***	.0111***	.059***	.0154***
	(.0106)	(.00738)	(.00288)	(.00638)	(.00245)
Control Mean	8.6	8.8	.15	-.23	-.084
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	72,470	93,661	94,788	92,052	94,395

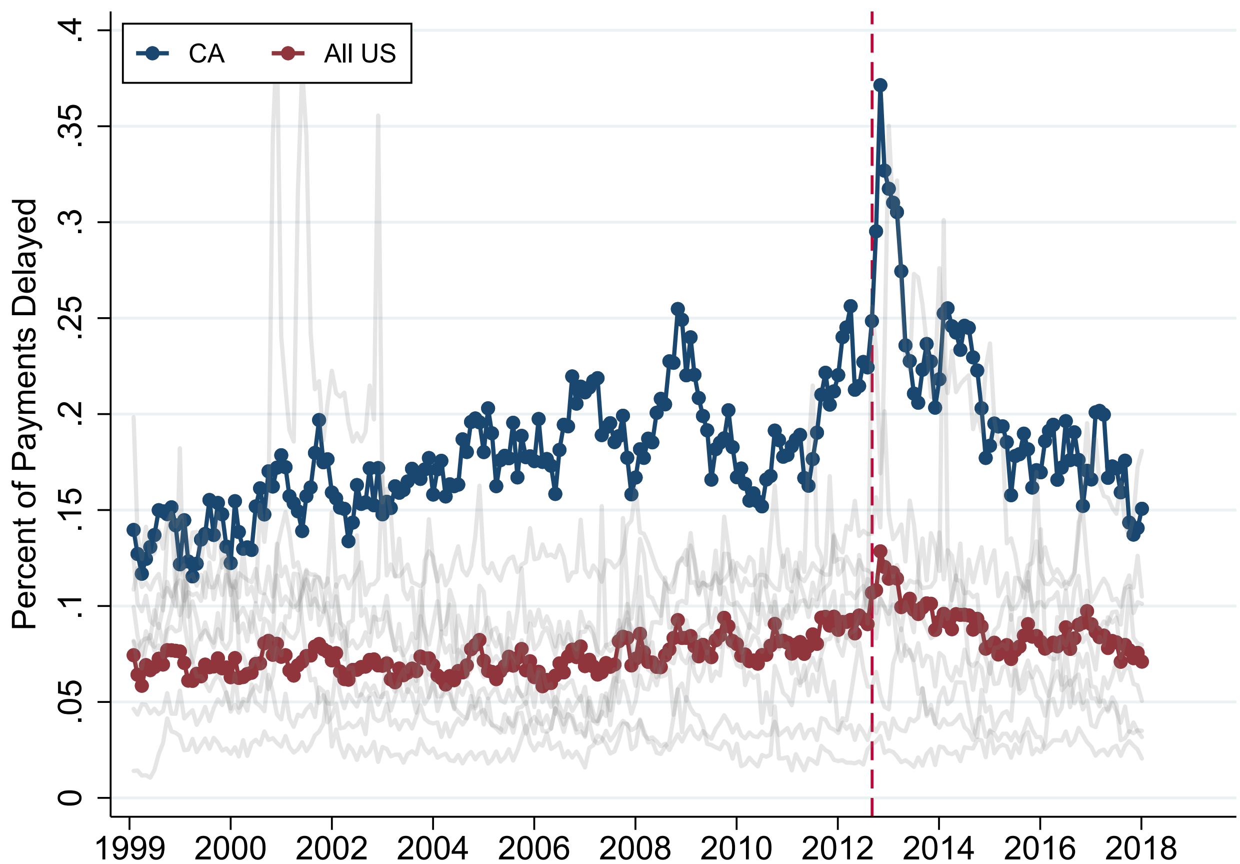
Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1. First Comp. Qtr. Log Reemp. Wage is the natural logarithm of earnings in the first complete quarter after the UI spell. Log. Avg. Coworker Pay is the natural logarithm of the average quarterly pay at the first reemployment firm. The Firm-Specific Pay Premium is estimated as laid out in Section B.2. If a claimant received earnings from multiple firms in their first reemployment quarter, we define the next firm as the firm from which the worker receives the highest quarterly pay. Changes in variables are defined as the change from the separating firm to the first reemployment firm. Standard errors are clustered at the claimant level. See Section 4.3 for more details.

Table 4: Next Firm Match Quality

	(1) Match Wage Premium	(2) Chg Match Wage Premium	(3) Tenure	(4) Distance	(5) Log Distance	(6) Switched Industry	(7) Any Previous Firm Return	(8) Return to Separating Firm
Delayed	.0149***	.0106**	.558***	-2.77***	-.0383*	-.0364***	.112***	.0486***
	(.0037)	(.0046)	(.074)	(.78)	(.019)	(.0044)	(.0062)	(.0056)
Control Mean	-.11	-.1	5.4	49	2.1	.51	.4	.23
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N Spells	71,045	69,357	95,761	92,526	92,526	95,761	95,761	95,761

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1. Match Wage Premium is the worker-firm specific pay premium as laid out in Section B.2. Tenure is the number of quarters a worker remains at their first reemployment firm. Distance is the centroid-to-centroid distance between the claimant's home zip code as reported on their UI claim and the zip code of the closest establishment of their first reemployment firm. Switched industry is an indicator for the first reemployment firm being in a different industry than the separating firm. Any Previous Firm is an indicator for the first reemployment firm being a firm the worker has worked at any point prior to the system outage. Return to Separating Firm is an Indicator for returning to the claimants' separating firm. If a claimant received earnings from multiple firms in their first reemployment quarter, we define the next firm as the firm from which the worker receives the highest quarterly pay. Changes in variables are defined as the change from the separating firm to the first reemployment firm. Standard errors are clustered at the claimant level. See Section 4.3 for more details.

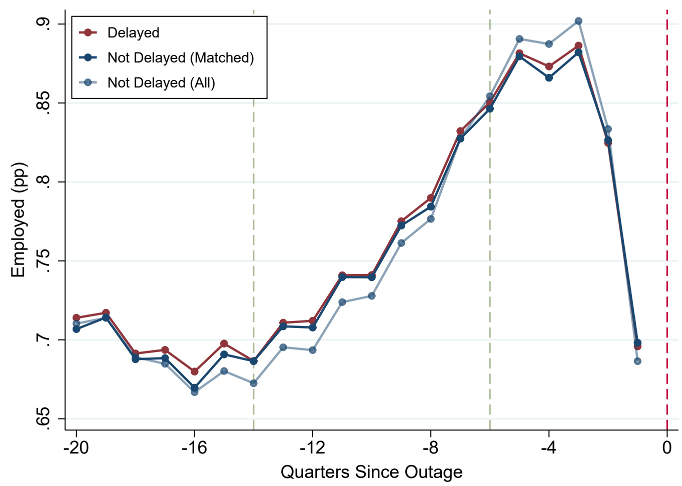
Figure 1: Frequency of Payment Delays, 1999-2019



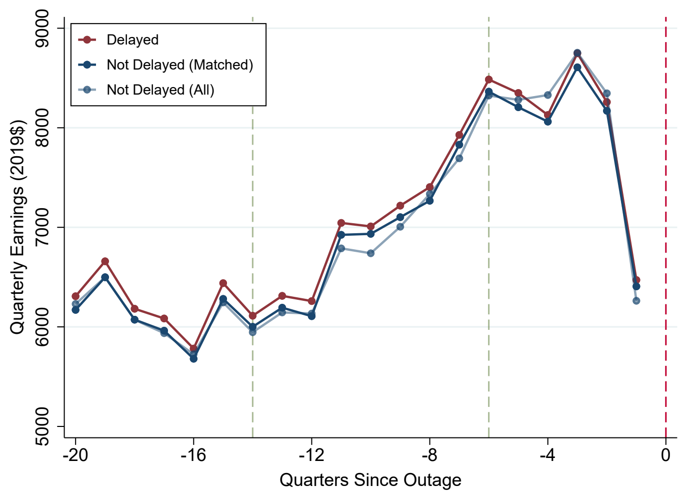
Notes: This figure shows the percent of monthly continuing claim UI payments delayed in California (blue), all states combined (red), and all states separately, using public data aggregates from the Department of Labor's ETA 9051 Report (Continued Weeks Compensated Time Lapse). Delayed payment is defined as a lapse of over 14 days between the end of the benefit week and when payment is disbursed to claimants. Red dashed line identifies September 2013 system glitch in the California UI system.

Figure 2: Mean Outcomes, Before and After Matching

(**a**) Employment



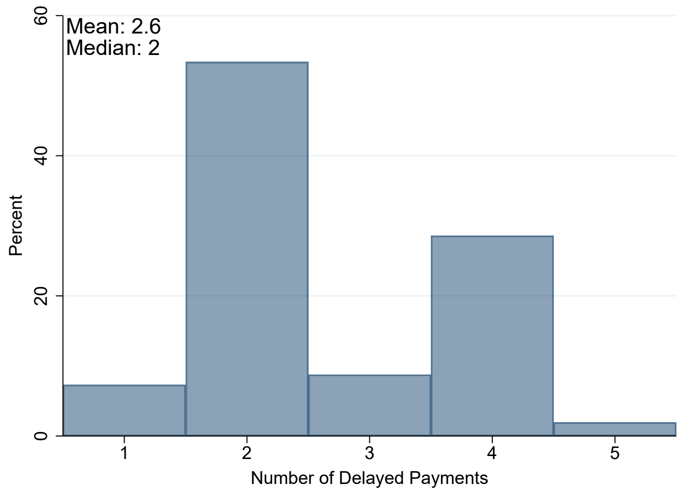
(**b**) Quarterly Earnings



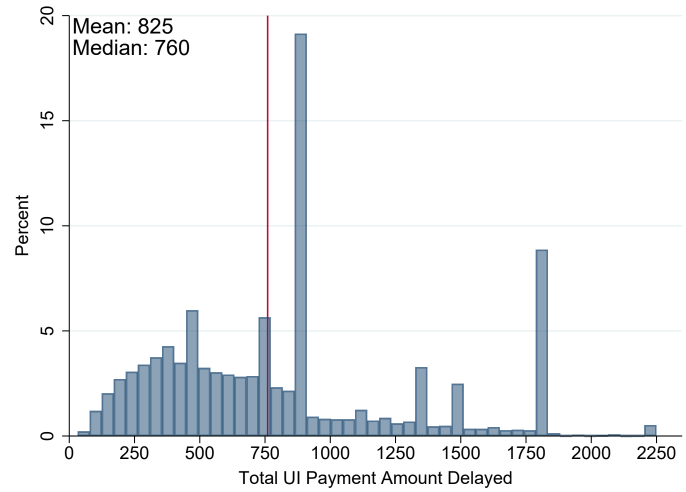
Notes: This figure shows raw outcome means for quarterly employment (Panel (a)) and earnings (Panel (b)) after using a two-step algorithm to match non-delayed claimants to their delayed counterparts on pre-treatment observables. Importantly, our matching design only uses lagged labor market histories between 6 and 14 before the outage (marked by green dashed lines on figures above), permitting other pre-treatment periods (-20 to -15 and -5 to -1 on figures above) to be used as unmatched placebos. For more details on the motivation for and implementation of our matching strategy, see Sections 3.3 and 3.4.

Figure 3: Characterizing Liquidity Shocks

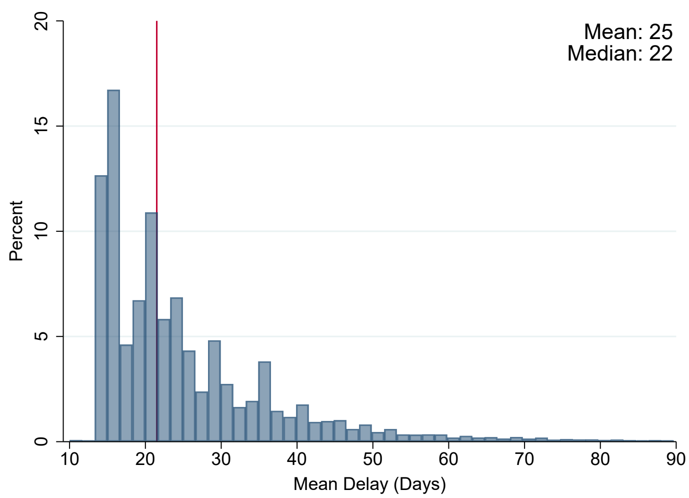
(**a**) Number of Delayed UI Payments



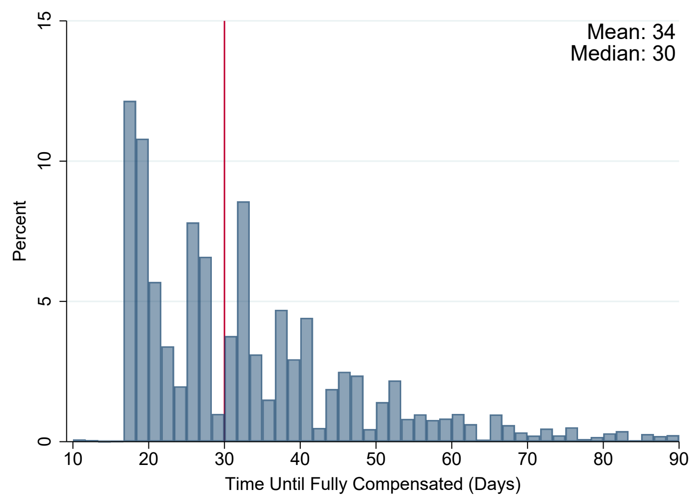
(**b**) Total Delayed Payment Amounts



(**c**) Mean Payment Delay



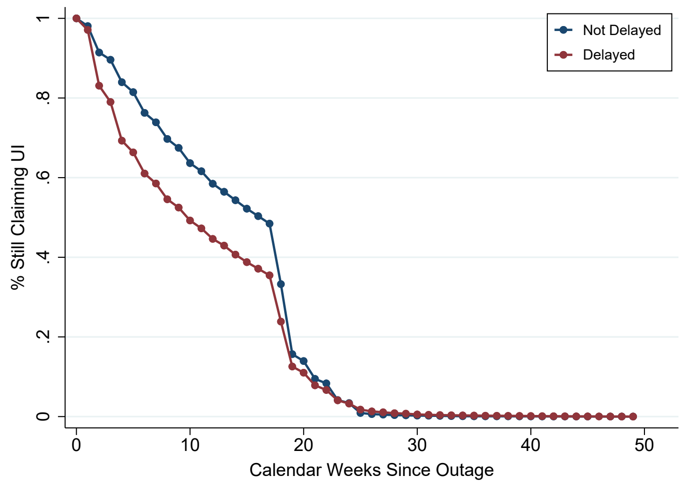
(**d**) Time Until Claimant Fully Compensated



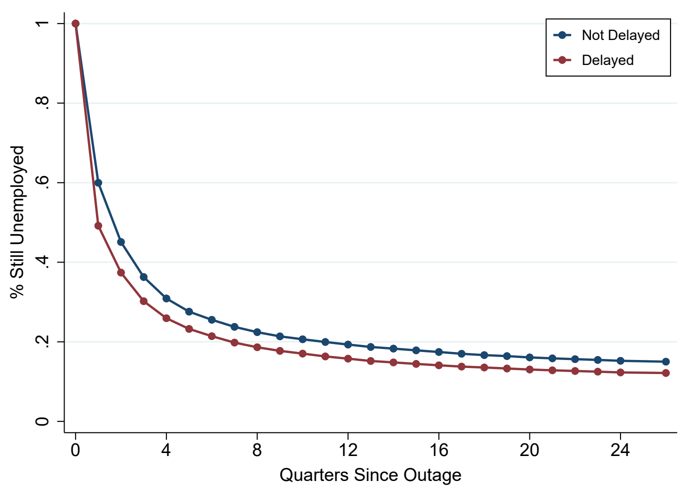
Notes: This figure shows the distribution of payment delays for affected claimants. Panel (a) shows the distribution of delayed UI payments, where payments refer to compensated weeks. Panel (b) shows the distribution of delayed payment amounts. Spikes are at multiples of \$450, which is the maximum weekly benefit amount in the California UI system. Panel (c) shows the distribution of each delayed payment's underlying time lapse in days. Panel (d) shows the claimant-level distribution of time to being fully compensated, which we define as the amount of time from the first delayed payment until the payout of the last delayed payment. The underlying sample are the delayed claimants described in column (3) of Table 1.

Figure 4: Survival Curves

(**a**) Current UI Spell Duration



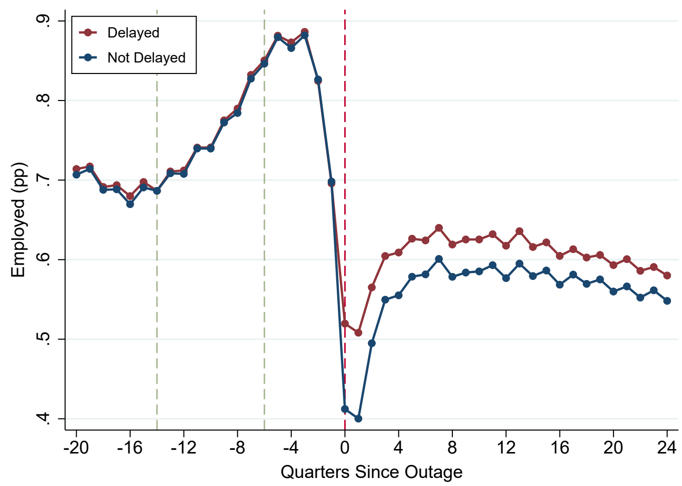
(**b**) Nonemployment Duration



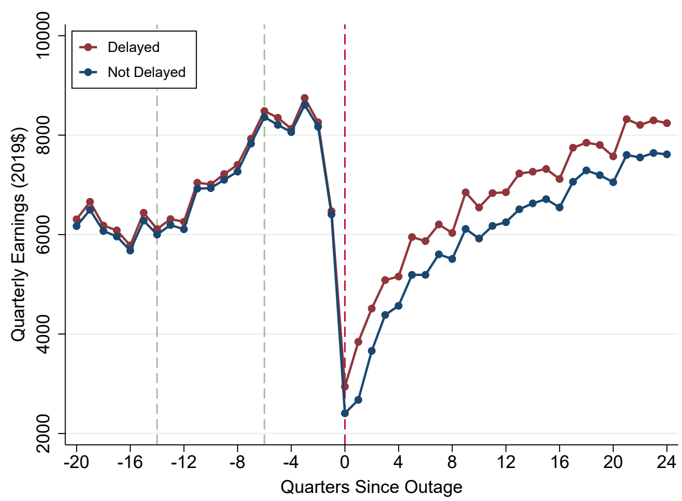
Notes: This figure shows separate survival curves for delayed and non-delayed claimants, examining UI survival (Panel A) and nonemployment survival (Panel B). UI survival is measured as continuing to certify at least once every two weeks after the outage, as required by the UI system, and time is measured in calendar weeks elapsed since the delay shock. Spikes in benefit weeks claimed at 18 and 19 weeks are the result of end of the Emergency Unemployment Compensation program at the end of 2013 (see Section 2 for more details.) Nonemployment duration is measured as the number of post-UI quarters before the claimant is observed receiving positive earnings (see Section 3.2 for more discussion of nonemployment). The underlying sample is the matched sample of columns (2) and (3) of Table 1.

Figure 5: Outcome Means After Matching

(**a**) Employment

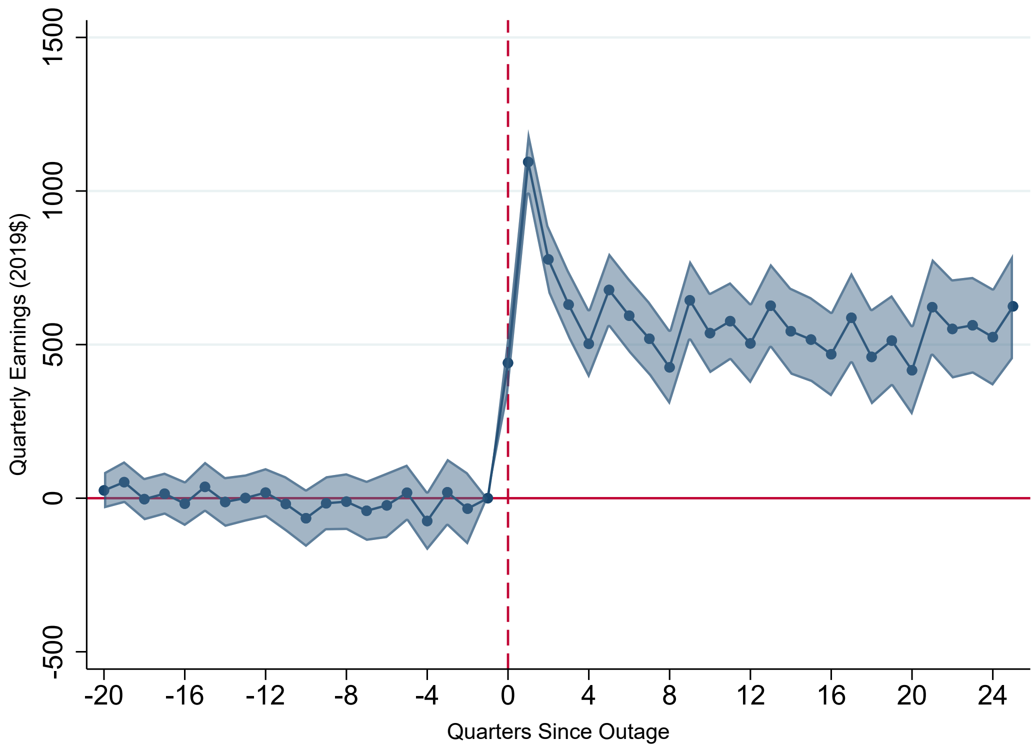


(**b**) Quarterly Earnings



Notes: This figure shows raw outcome means for quarterly employment (Panel (a)) and earnings (Panel (b)) after using a two-step algorithm to match non-delayed claimants to their delayed counterparts on pre-treatment observables. Our matching design only uses lagged labor market histories between 6 and 14 before the outage (marked by green dashed lines on figures above), permitting other pre-treatment periods (-20 to -15 and -5 to -1 on figures above) to be used as unmatched placebos. For more details on the motivation for and implementation of our matching strategy, see Sections 3.3 and 3.4.

Figure 6: Long-Term Dynamic Effects on Quarterly Earnings

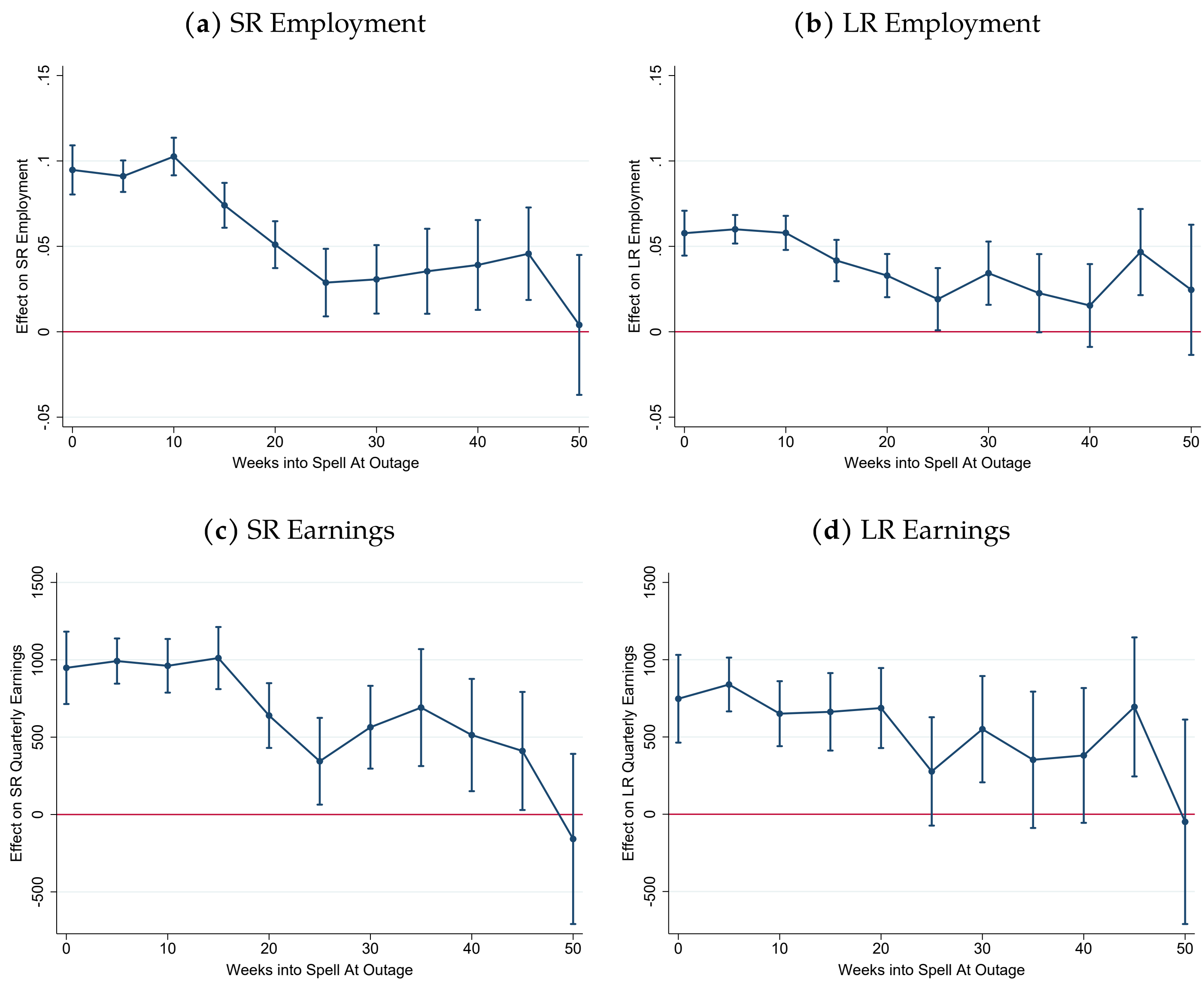


Notes: This figure plots the dynamic difference-in-difference coefficients δ_k from Equation 2, estimated for our matched sample. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California. Shaded areas are 95-percent confidence intervals allowing for arbitrary clustering of errors at the claimant level.

Figure 7: Decomposing the Long-Term Earnings Effect

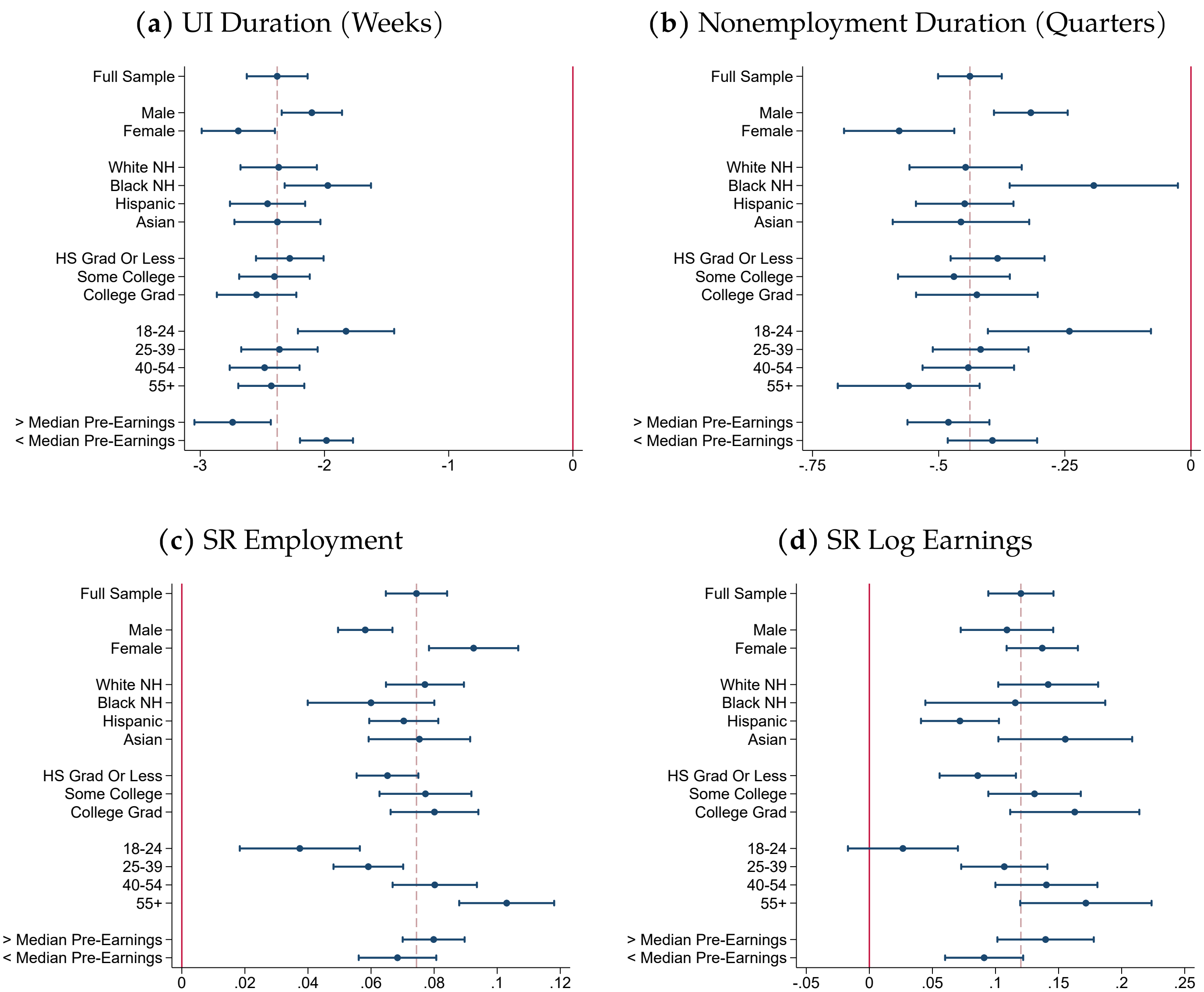
Notes: This figure plots the dynamic difference-in-difference coefficients δ_k from Equation 2, estimated for our matched sample. Panel (a) displays coefficients for being employed in quarter q. Panel (b) displays coefficients for earnings in quarter q conditional on having non-zero earnings. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California. Shaded areas are 95-percent confidence intervals allowing for arbitrary clustering of errors at the claimant level.

Figure 8: Heterogeneity by Spell Age



Notes: This figure plots the treatment effects of being exposed to a UI payment delay shock in September 2013 for employment and quarterly earnings for 5-week bins of spell age. The estimating equation is Equation 1. "SR" refers to short-run outcomes and is the average outcome over the first four quarters after the system glitch. "LR" refers to long-run outcomes and is the average outcome ten to twenty quarters after the system glitch. Employment is an indicator for having nonzero earnings in the California UI Base Wage File. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California.

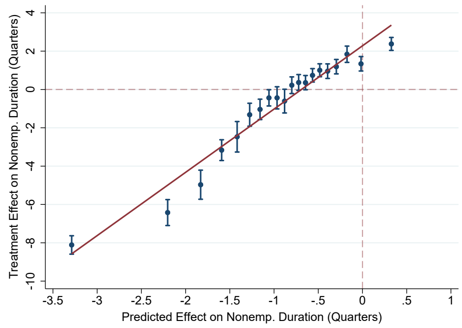
Figure 9: Treatment Effect Heterogeneity by Demographics



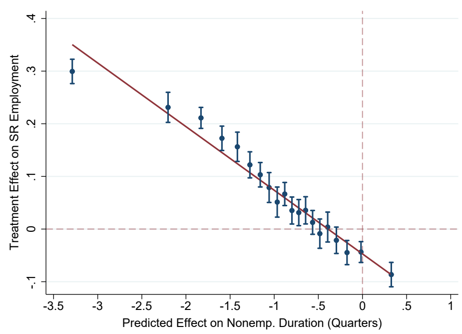
Notes: This figure plots treatment effects on UI duration, short-run employment, and short-run log earnings across demographic groups, as estimated by a regression like that of Equation 1. Short-run outcomes are defined as the average outcome between 1 and 4 quarters after the system glitch. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California. Horizontal lines are 95-percent confidence intervals and standard errors are clustered at the claimant level.

Figure 10: Predicted Nonemployment Duration Versus Labor Market Effects

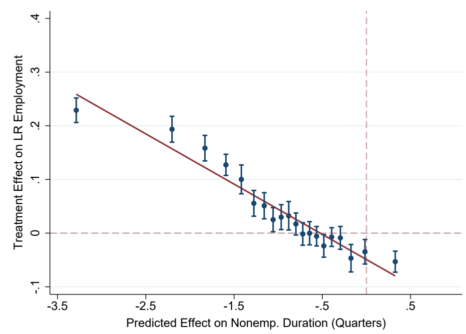
(**a**) Nonemployment Duration



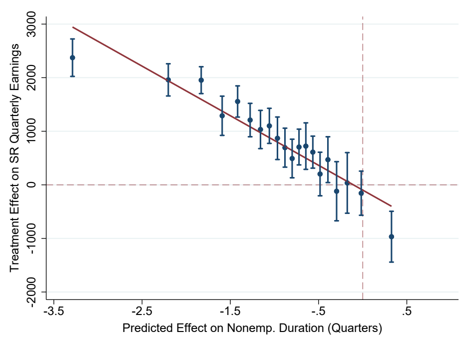
(**b**) Short-Run Employment



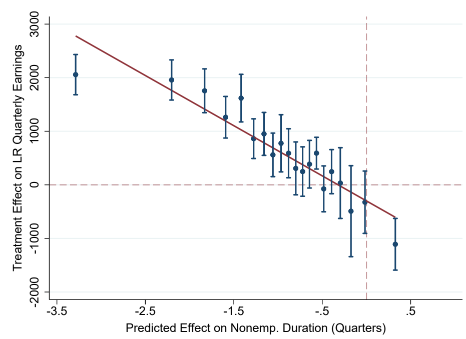
(**c**) Long-Run Employment



(**d**) Short-Run Earnings



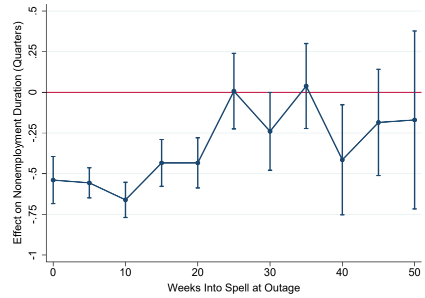
(**e**) Long-Run Earnings



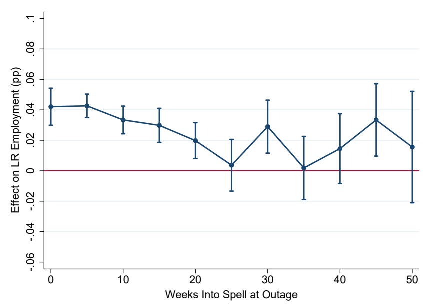
Notes: This figure presents scatterplots of heterogeneous treatment effects on labor market outcomes by bins of predicted treatment effects for nonemployment duration. Bins correspond to ventiles of the predicted nonemployment duration treatment effect for each UI claimant, where predictions are estimated using a causal random forest. See section 5 for more details on estimation method. Panels plot treatment effects within bins for actual nonemployment duration (Panel (a)), short-run employment (Panel (b)), long-run employment (Panel (c)), short-run quarterly earnings (Panel (d)), and long-run quarterly earnings (Panel (d)). Short-run refers to outcomes averaged between 1 and 4 quarters after the outage; long-run refers to outcomes averaged between 10 and 20 quarters after the outage.

Figure 11: Heterogeneity by Spell Age, Reweighted and Reemployed Sample

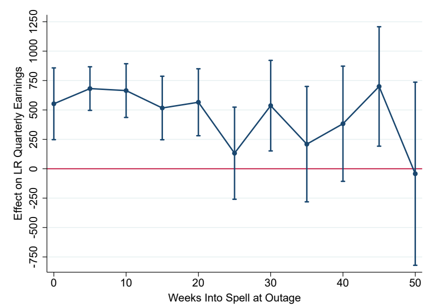
(**a**) Nonemployment Duration



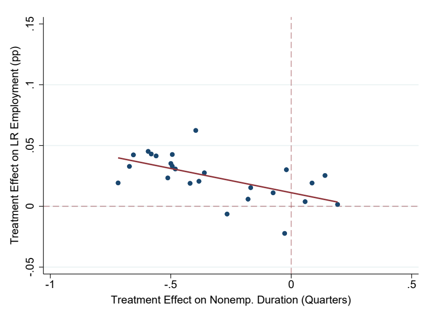
(**b**) LR Employment



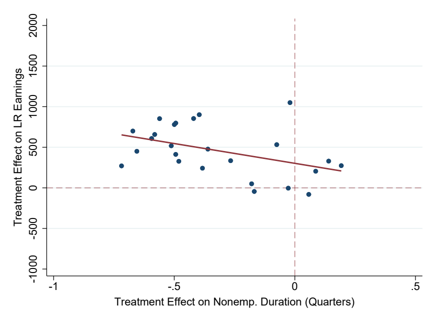
(**c**) LR Earnings



(**d**) Scatter: LR Employment



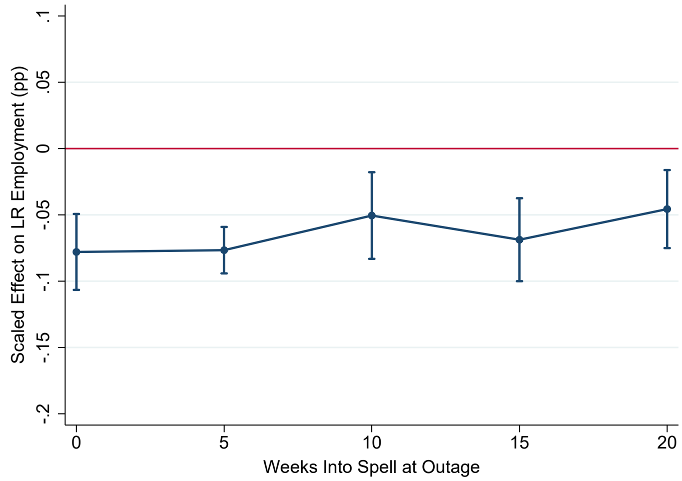
(**e**) Scatter: LR Earnings



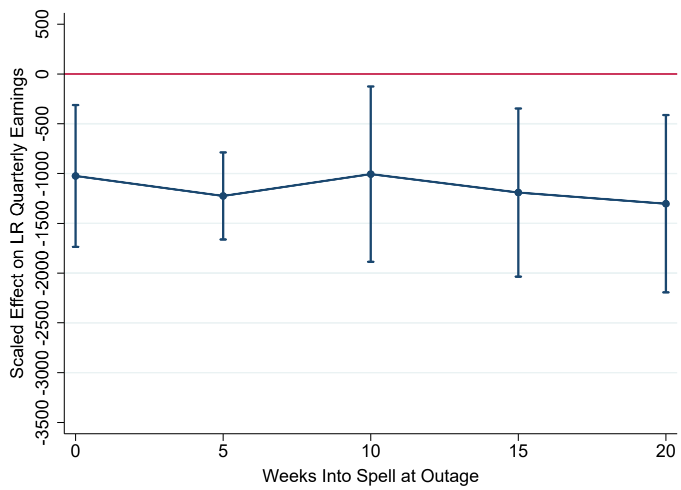
Notes: Panels (a)-(c) of this figure plots the treatment effects of being exposed to a UI payment delay shock in September 2013 for employment and quarterly earnings for 5-week bins of spell age, restricting to claimants who are later reemployed and reweighting to balance the covariate distribution over spell age (see Section 5.2 for more details). Panels (d) and (e) present scatterplots of long-run labor market effects against nonemployment duration effects for finer 2-week bins of spell age. The estimating equation is Equation 1. "LR" refers to long-run outcomes and is the average outcome ten to twenty quarters after the system glitch. Employment is an indicator for having nonzero earnings in the California UI Base Wage File. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California.

Figure 12: Effects of Nonemployment Duration Throughout The Spell

(**a**) LR Employment

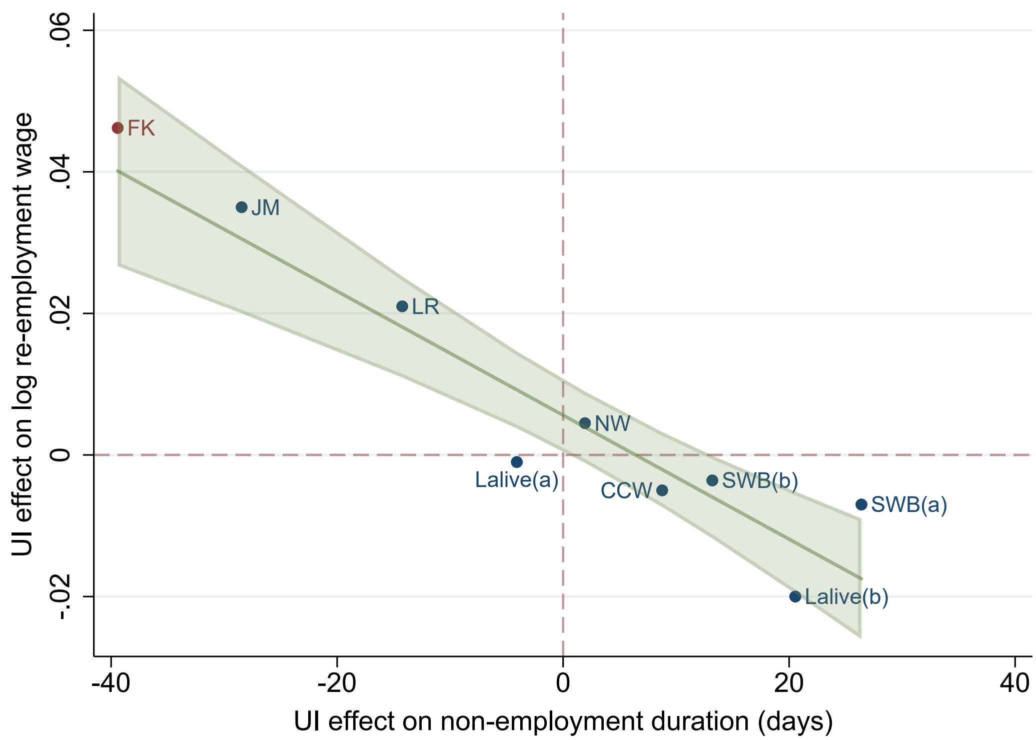


(**b**) LR Earnings



Notes: This figure plots the estimated treatment effects of one additional quarter of non-employment on long-term employment and long-term earnings for 5-week bins of spell age, restricting to claimants on the regular state UI program at the time of the system glitch who are later reemployed and reweighting to balance the covariate distribution over spell age (see Section 5.2 for more details). Long-run outcomes are the average outcome ten to twenty quarters after the system glitch. Employment is an indicator for having nonzero earnings in the California UI Base Wage File. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California.

Figure 13: Meta-Analysis of Duration and Wage Estimates Across Studies



Notes: This figure compares different estimates of UI's effect on nonemployment duration and reemployment wages. The green line (with associated 95% confidence interval for predictions) depicts the result of a regression of reemployment wage treatment effects on nonemployment duration treatment effects, omitting this paper (in red at top left) from the sample. Details on how we obtain UI effects on non-employment duration and log reemployment wages can be found in Section C.

A Additional Results

A.1 Supplementary Figures and Tables

Table A1: Effect of Sample Restrictions

	Number of Claims by Group:			
Sample Restriction	Delayed	Not Delayed	Total	Percentage of all Claims
All Claims in Risk Set	145,210	171,510	316,720	1
+ Can Identify Separating Firm	144,883	171,237	316,120	1
+ Claim Not Always-Delayed Pre-Outage	123,338	166,265	289,603	.91
+ No Claims With Delayed First Payments	100,245	135,957	236,202	.75
+ No Reported Earnings While on UI	68,379	125,673	194,052	.61
+ Only Claims With Control-Treatment Matches	68,348	42,273	110,621	.35

Notes: This table presents the cumulative effect of our sample restrictions. The baseline risk set is defined as the set of UI claimants who have a had scheduled in September 2013, who were subject to work search requirements, who were between 18 and 70 years old, and who were at most 52 calendar weeks into their UI spell. For details of our sample construction procedure, see Section 3.3.

Table A2: Cross-State Mobility

	(1) In Infutor	(2) Ever Outside CA Post	(3) Outside CA 1+ Years Post	(4) Outside CA 2+ Years Post	(5) Outside CA 5+ Years Post	(6) N Quarters Lived Outside CA
Delayed	-.00609	-.00294	-.00456*	-.00429	-.00454	-.164*
	(.0039)	(.0029)	(.0027)	(.0027)	(.0029)	(.09)
Control Mean	.65	.086	.078	.072	.057	2.2
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes
N Spells	110,621	71,241	71,241	71,241	71,241	71,241

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1. Mobility data is from Infutor Data Solutions. Standard errors are clustered at the claimants level. See Section 4.3 for more details.

Table A3: Placebo Delay Shocks

	(1) Placebo Cohort: September 2012	(2) Placebo Cohort: September 2014	(3) True Delay Cohort: September 2013
Worker FE	-0.024	0.024	0.000
	(0.003)	(0.005)	(0.004)
Black	-0.011	-0.014	-0.010
	(0.001)	(0.002)	(0.001)
Hispanic	-0.032	-0.066	-0.006
	(0.002)	(0.003)	(0.002)
White	0.018	0.055	0.012
	(0.002)	(0.003)	(0.002)
Asian	0.025	0.026	0.005
	(0.002)	(0.002)	(0.002)
College Grad	0.061	0.086	0.036
	(0.002)	(0.032)	(0.002)
Citizen	0.014	0.032	0.002
	(0.001)	(0.002)	(0.002)
Female	0.018	0.002	0.003
	(0.001)	(0.003)	(0.002)
Age at Filing	-1.366	-1.127	-0.578
	(0.058)	(0.080)	(0.061)
Claims	277,144	166,851	193,514

Notes: This table reports the coefficients on an indicator for being delayed in September 2012 (Column 1), September 2014 (Column 2) or during the California UI System Upgrade in September 2013 (Column 3) for various demographic characteristics. The sample restrictions for each period are the same as laid out in Section 3.3, adapted to each respective period. Robust standard errors in parentheses.

Table A4: Effects on Unemployment Spell Duration Outcomes

	(1) Post-Outage UI Current Spell Length	(2) Post-Outage UI Claimed Weeks	(3) Post-Outage UI Paid Weeks	(4) Post-Outage UI Initial Spell Length	(5) Post-Outage UI Total Spell Length
Delayed	-2.38***	-1.77***	-2.09***	-2.68***	-.548***
	(.12)	(.081)	(.091)	(.12)	(.12)
Control Mean	13	14	14	13	15
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	110,621	110,621	110,621	88,139	110,621

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1 for additional measures of unemployment duration. Current Spell Length is our preferred measure of UI spell duration and captures the number of calendar weeks until the first two-week post-outage lapse in recertification for benefits. Claimed weeks is the number of benefit weeks for which a claimant filed for UI. Paid weeks is the number of *compensated* weeks. Initial spell length is the number of calendar weeks between the claim's start date and the first two-week lapse in recertification for benefits. Total spell length is the number of calendar weeks between the system glitch and the last claimed week. All measures start from the time of the system glitch (the first week of September). See Section 4.3 for more details.

Table A5: Effects on Post-Outage UI-Reported Earnings

	(1) 1(Side Job On UI Post)	(2) Num Weeks Side Job Post	(3) Side Job Total Earnings Post	(4) Avg Side Job Earnings Post	(5) Avg Side Job Earnings Post (Cond.)
Delayed	.118***	.675***	293***	27.9***	20.3***
	(.0057)	(.049)	(20)	(1.4)	(5.9)
Control Mean	.13	.45	170	11	429
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	110,621	110,621	110,621	110,621	21,939

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1 for various measures of side job earnings while receiving UI benefits. 1(Side Job On UI) is an indicator for reporting any earnings while on UI. Num Weeks Side Job is the number of weeks on UI for which a claimant reports earnings. Side Job Total Earnings are the total earnings from work while on UI in dollars. Avg Side Job Earnings are the average reported earnings while on UI. Avg Side Job Earnings (Cond.) are the average reported earnings while on UI conditional on having positive earnings while on UI. All measures start from the time of the system glitch (the first week of September). See Section 4.3 for more details.

Table A6: Effects on UI Spell Week Types

	(1) 1(Partial Weeks Post)	(2) Num. Partial Weeks Post	(3) 1(Fully-Comp Weeks Post)	(4) Num. Fully-Comp Weeks Post
Delayed	.0465***	-.051	.0175***	-1.72***
	(.0044)	(.067)	(.0027)	(.097)
Control Mean	.63	5.4	.77	9.1
Spell FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes
N Spells	110,621	110,621	110,621	110,621

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1 for various measures of weekly claiming behavior for the UI spell. 1(Partial Weeks Post) is an indicator for the paid-out benefit amount being below the weekly benefit amount at any point after the system glitch. Num. Partial Weeks is the number of weeks for which the paid-out UI benefit is lower than the weekly benefit amount. 1(Fully Comp Weeks) is an indicator for the paid out benefit amount being equal to the weekly benefit amount at any point after the system glitch. Num. Fully-Comp Weeks is the number of weeks for which the paid-out UI benefit is equal to the weekly benefit amount. All measures start from the time of the system glitch (the first week of September). See Section 4.3 for more details.

Table A7: Next Firm Size

	(1) Log Firm Size	(2) Firm Size: 1 Person	(3) Firm Size: 10+ People	(4) Firm Size: 50+ People	(5) Num. Of Estabs.
Delayed	.0178	-.00233**	.006**	.00661**	.926
	(.021)	(.0011)	(.0026)	(.0031)	(2.3)
Control Mean	5.8	.023	.89	.73	49
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	95,761	95,761	95,761	95,761	95,761

Notes: This table reports the coefficients on an indicator for being delayed during the California UI System Upgrade in September 2013 from a regression defined by Eq. 1 for for various firm-level characteristics associated with the first post-system glitch firm. Log Firm Size is the size of the firm in logs, Firm Size: 1 Person is an indicator for the firm having just one employee, Firm Size: 10+ People is an indicator for the firm having at least 10 employees, Firm Size: 50+ People is an indicator for the firm having at least 50 employees. Num. Of Estabs. is the number of establishments of the firm. If a worker is associated with multiple firms in their first quarter of reemployment, we define the next firm as the firm for which the claimant has the highest quarterly earnings in that quarter. See Section 4.3 for more details.

Table A8: Unemployment Spell Duration, No Return to Separating Firm

	(1) 1(Exhaust UI)	(2) UI Duration (Weeks)	(3) 1(Reemp. Within 4 Quarters)	(4) Nonemp. Dur. (Quarters)	(5) Ever Reemp.
Delayed	-.0654***	-2.22***	.037***	-.364***	.0278***
	(.0064)	(.11)	(.0035)	(.037)	(.0028)
Control Mean	.34	13	.67	7.1	.81
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	85,929	85,929	85,929	71,069	85,929

Notes: This table reports coefficients where models in Table 2 are reestimated, restricting to claimants who do not return to their separating employer. Outcomes are regressed on an indicator for being delayed during the California UI system upgrade in September 2013 from a regression defined by Eq. 1. 1(Exhaust UI) is an indicator for exhausting UI benefits. Current Spell Duration is the number of weeks until the first two-week lapse in recertification. 1(Reemp. Within 4 Quarters) is an indicator for being reemployed within four quarters from the system outage. Ever Reemp. is an indicator for ever being reemployed as measured by non-zero labor earnings in the California UI Base Wage File. Nonemp. Dur. (Quarters) is the non-employment duration in quarters from the time of the system outage, conditional on ever being reemployed. Standard errors are clustered at the claimant level. See Section 4.3 for more details.

Table A9: Next Firm Quality, No Return to Separating Firm

	(1) First Comp. Qtr. Log Reemp. Wage	(2) Log Avg. Coworker Pay	(3) Firm-Specific Pay Premium	(4) Chg. Log Avg. Coworker Pay	(5) Chg. Firm-Specific Pay Premium
Delayed	.0509***	.0575***	.0173***	.0579***	.0154***
	(.0115)	(.00891)	(.00282)	(.00868)	(.00326)
Control Mean	8.6	8.8	.15	-.29	-.11
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	54,374	69,396	70,211	67,886	69,818

Notes: This table reports coefficients where models in Table 3 are reestimated, restricting to claimants who do not return to their separating employer. Outcomes are regressed on an indicator for being delayed during the California UI system upgrade in September 2013 from a regression defined by Eq. 1. First Comp. Qtr. Log Reemp. Wage is the natural logarithm of earnings in the first complete quarter after the UI spell. Log. Avg. Coworker Pay is the natural logarithm of the average quarterly pay at the first reemployment firm. The Firm-Specific Pay Premium is estimated as laid out in Section B.2. If a claimant received earnings from multiple firms in their first reemployment quarter, we define the next firm as the firm from which the worker receives the highest quarterly pay. Changes in variables are defined as the change from the separating firm to the first reemployment firm. Standard errors are clustered at the claimant level. See Section 4.3 for more details.

Table A10: Next Firm Match Quality, No Return to Separating Firm

	(1) Match Wage Premium	(2) Chg Match Wage Premium	(3) Distance	(4) Log Distance	(5) Switched Industry	(6) Any Previous Firm Return
Delayed	.0189***	.00804	-1.09	.0179	-.00925**	.104***
	(.0051)	(.0065)	(.9)	(.016)	(.0036)	(.0054)
Control Mean	-.13	-.15	50	2.2	.67	.22
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes
N Spells	46,790	45,102	68,566	68,566	71,069	71,069

Notes: This table reports coefficients equivalent to those reported in Table 4, restricting to claimants who do not return to their separating employer. See Section 4.3 for more details.

Figure A1: Weekly Percent of Payments Delayed, 2012-2014

Notes: This figure shows the weekly fraction of payments delayed in EDD UI claims microdata. A payment delay is defined as a time lapse of over 14 days to match Figure 1.

Figure A2: EDD Recertification Form

ALLOW 10 DAYS FOR DELIVERY OF CHECK. DETACH THIS STUB FOR YOUR RECORD

CONTINUED CLAIM

ANSWER ALL QUESTIONS. SEE SECTION A. ON BACK FOR EXAMPLES OF HOW TO COMPLETE YOUR ANSWERS. Each question is explained in your booklet, A Guide to Benefits and Employment Services.

COMPLETE AND MAIL THIS FORM ON

	IST WEEK Begins Ends YES	NO	2ND WEEK Begins Ends YES	NO
1. Were you too sick or injured to work? ....>	☐	☐	☐	☐
If yes, enter the number of days (1 through 7) you were unable to work. ....>		(I - 7)		(I - 7)
2. Was there any reason (other than sickness or injury) that you could not have accepted full-time work each workday? ....>	☐	☐	☐	☐
3. Did you look for work? ....> ☐ <— **IF MARKED 'X'**, YOU MUST COMPLETE SEC. B., WORK-SEARCH RECORD, ON REVERSE.	☐	☐	☐	☐
4. Did you refuse any work? ....>	☐	☐	☐	☐
5. Did you **begin** attending any kind of school or training? ....>	☐	☐	☐	☐
6. Did you work **or** earn any money, **WHETHER YOU WERE PAID OR NOT?** ....>	☐	☐	☐	☐

(**If yes,** you MUST COMPLETE items a. and b. below.)

a. Enter earnings before deductions here. ....> $ $

b. Report employment or 'source' of earnings information below:

	DATE LAST WORKED	TOTAL HOURS WORKED	EMPLOYER NAME AND MAILING ADDRESS - INCLUDE ZIP CODE	REASON NO LONGER WORKING (OR WRITE "STILL WORKING")
1ST WEEK				
2ND WEEK				

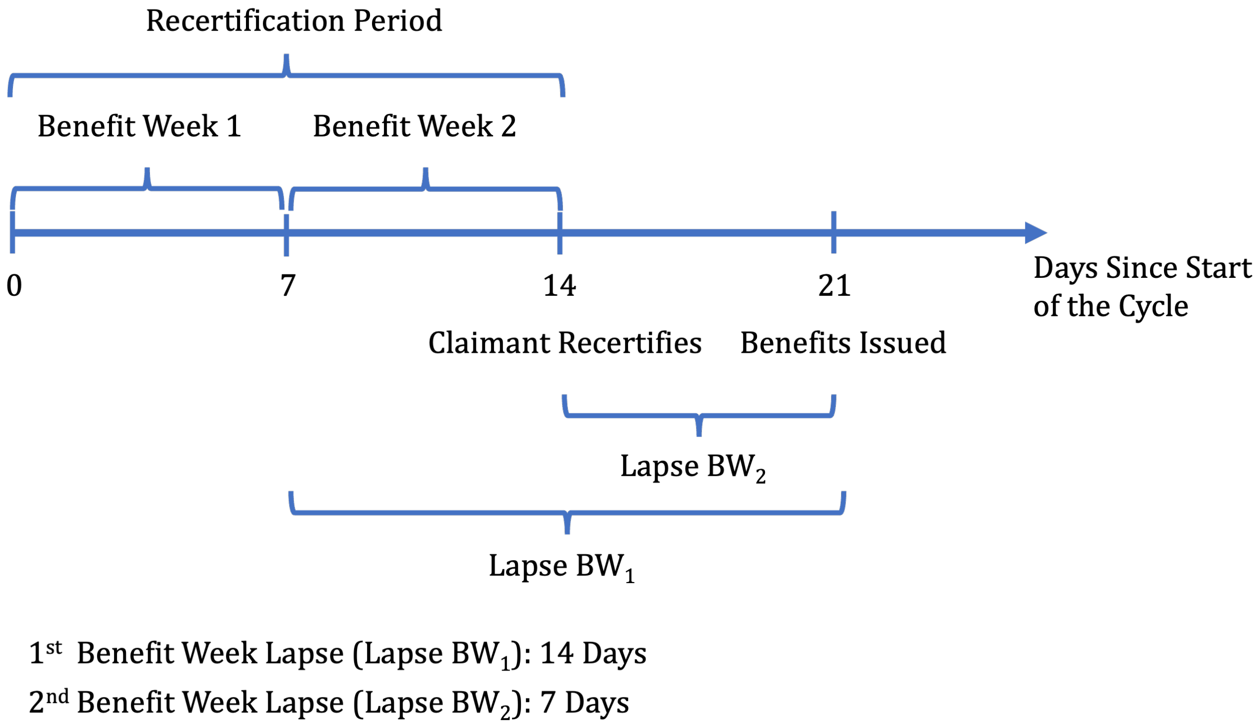
7. If you want federal income tax withheld for the week(s) shown above, mark this block. ....> ☐
8. If you had a change of mailing address or phone number, **mark this block and complete Sec. D on reverse.** ....> ☐

I understand the questions on this form. I know the law provides penalties if I make false statements or withhold facts to receive benefits; my answers are true and correct. I declare under penalty of perjury that I am a U.S. citizen or national; or an alien in satisfactory immigration status and permitted to work by USCIS. I signed this form after the latest date for which I am claiming benefits.

X
(your signature is required)

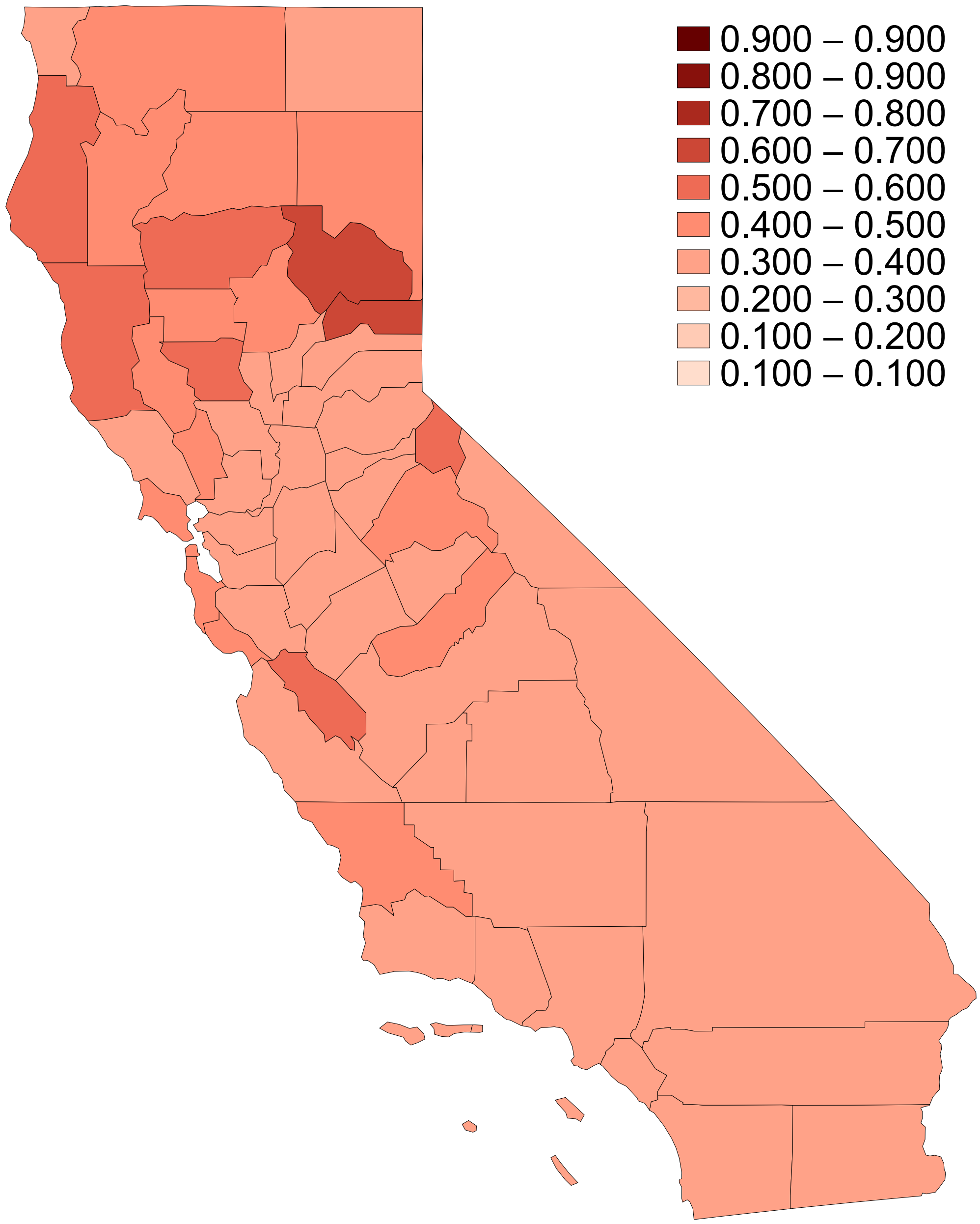
Notes: This figure shows an example paper recertification form that claimants can submit by mail to file for benefits every two weeks. See Section 2 for more details.

Figure A3: Benefit Week Timing



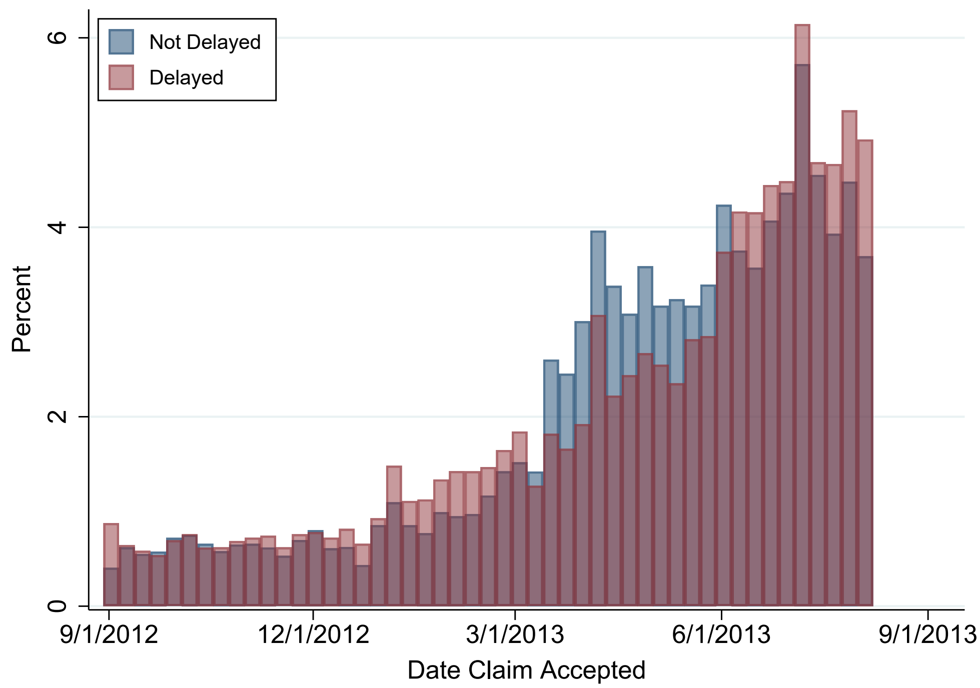
Notes: This diagram shows the timing of UI benefit payments for a two-week recertification period. UI claimants search for jobs over two separate benefit weeks, marked by the intervals 0-7 and 7-14 respectively. After the end of the second benefit week (day 14 in the figure), the claimant is required to recertify their eligibility for benefits by filling out information on work search activity over the previous two weeks (see Figure A2 for screenshot of the actual questions asked). Claimants are typically issued benefits within a week of recertification; the payment date is marked here as day 21. Our main measure of a payment delay is based on the benefit week-specific *payment time lapse*, or the number of days between the end of the benefit week and the date that benefits are issued. The EDD instructs UI claimants that benefits are paid within 10 days, and so we define the second benefit week as being delayed if Lapse $BW_2 > 10$. To account for the mechanical seven day lapse for BW_1 in the biweekly recertification system, we denote the first benefit week as delayed if Lapse $BW_1 > 17$. See Section 2 for more details.

Figure A4: County-Level Geographic Distribution of Delayed Claims During the Outage



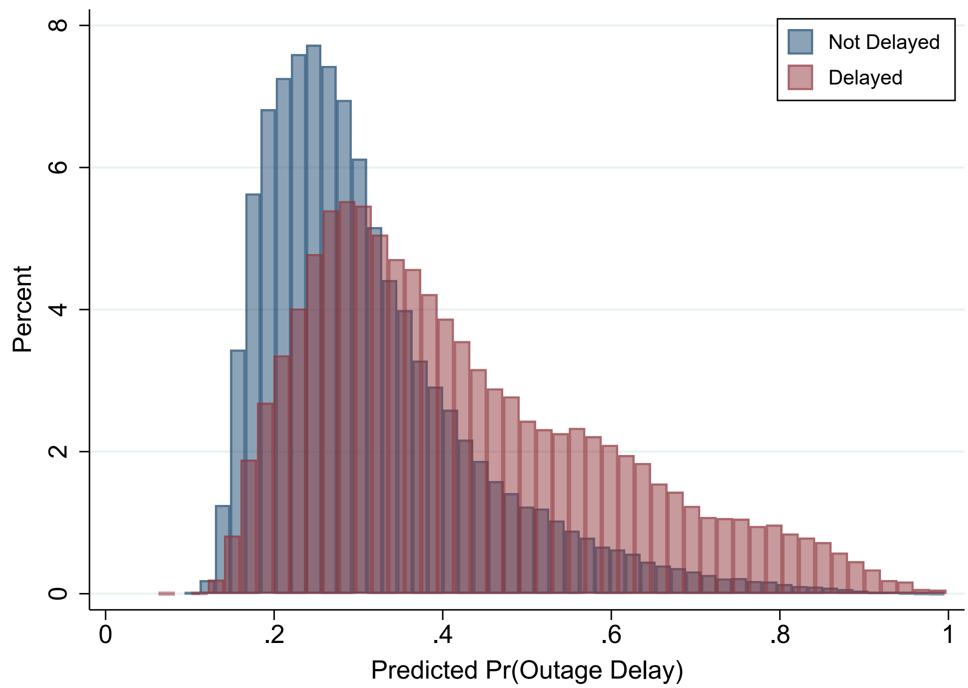
Notes: This figure shows the county-level fraction of claimants who were delayed during the outage. All counties include at least 20 UI claimants. See Section 2 for more details.

Figure A5: Distribution of Claim Start Dates At Outage



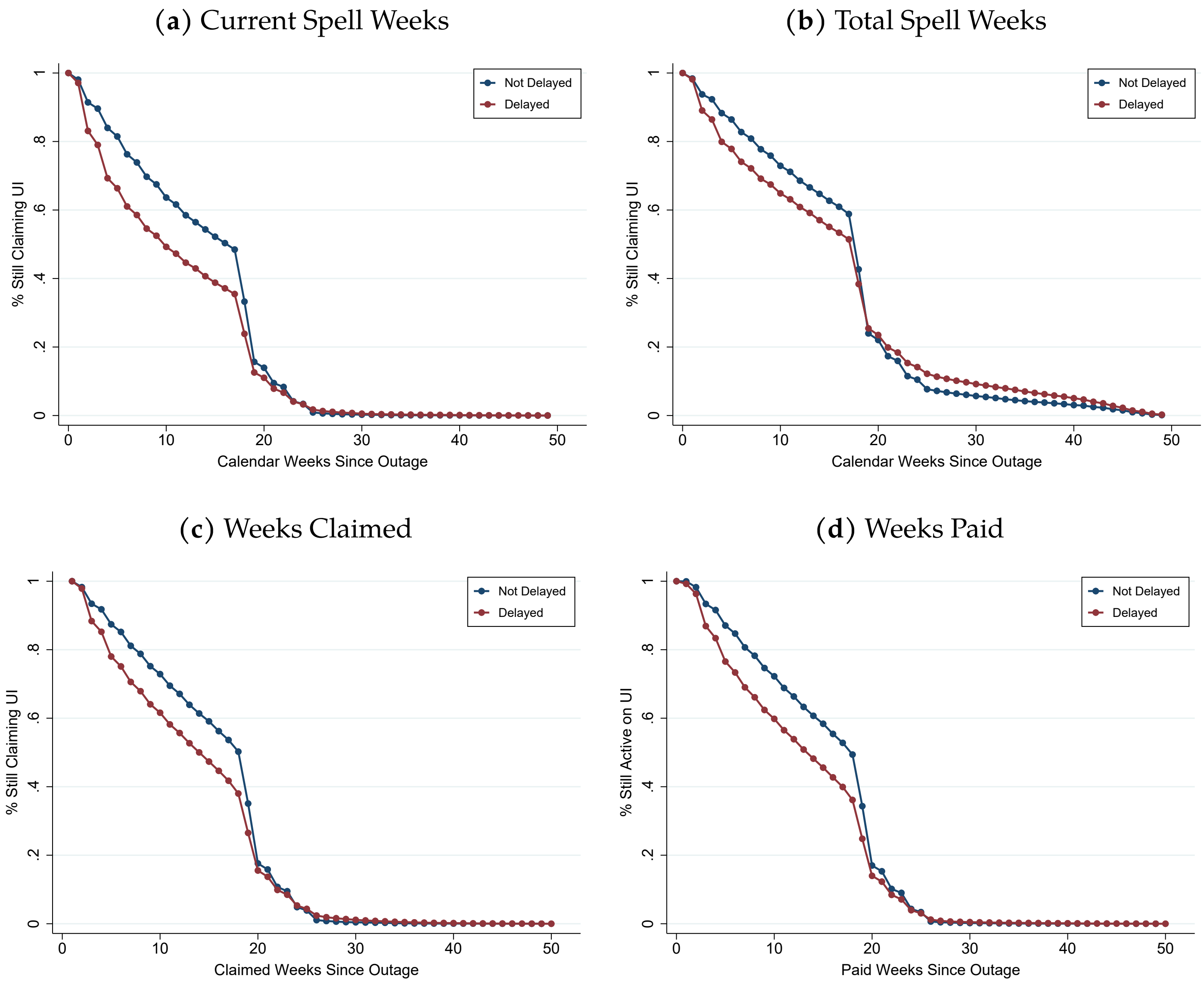
Notes: This figure shows the distribution of claim start dates for claimants in in our risk set as described in Section 3, broken out by delayed and non-delayed claims.

Figure A6: Overlap Between Delayed, Non-Delayed Propensity Scores



Notes: This figure shows the distribution of propensity scores for delayed and non-delayed claimants in in our risk set. See Sections 3.3 and 3.4 for more details.

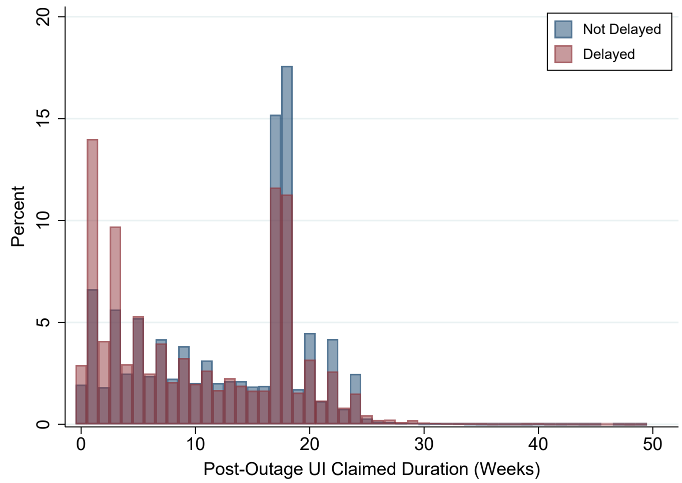
Figure A7: Survival Curves, Varying Definitions of UI Duration



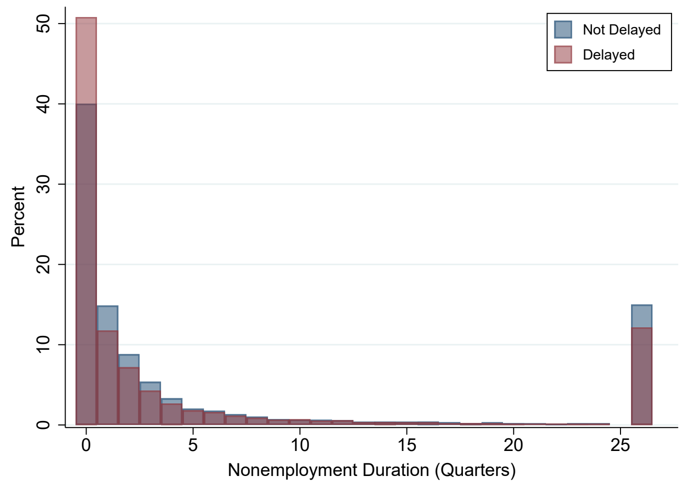
Notes: This figure compares survival curves across different definitions of UI durations. Current Spell Length is our preferred measure of UI spell duration and captures the number of calendar weeks until the first two-week lapse in recertification for benefits. Claimed weeks is the number of benefit weeks for which a claimant filed for UI. Paid weeks is the number of *compensated* weeks. Total spell length is the number of calendar weeks between the system glitch and the last claimed week. Survival is based on continued activity starting from the first week of September (the onset of the system glitch). See Section 4.3 for more details.

Figure A8: Distribution of Post-Outage Unemployment Spells

(**a**) Post-Outage Current Spell Weeks

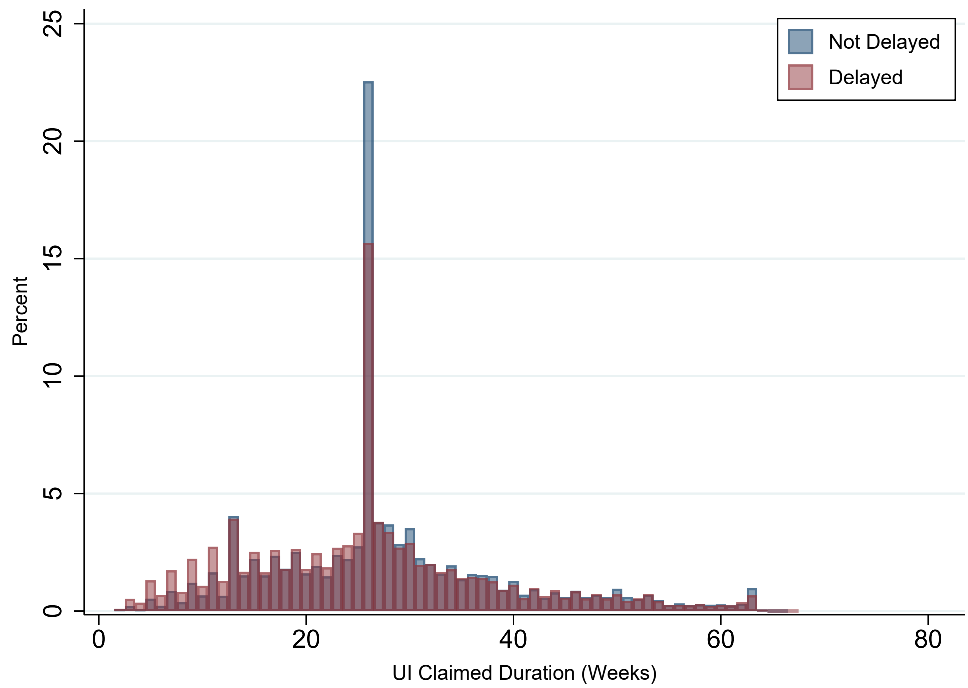


(**b**) Nonemployment Duration



Notes: This figure shows the distribution of post-outage UI benefit weeks claimed and nonemployment duration, separately for delayed and non-delayed claimants. Durations are defined as starting from the time of the system glitch. Spikes in benefit weeks claimed at 18 and 19 weeks are the result of end of the Emergency Unemployment Compensation program at the end of 2013 (see Section 2 for more details).

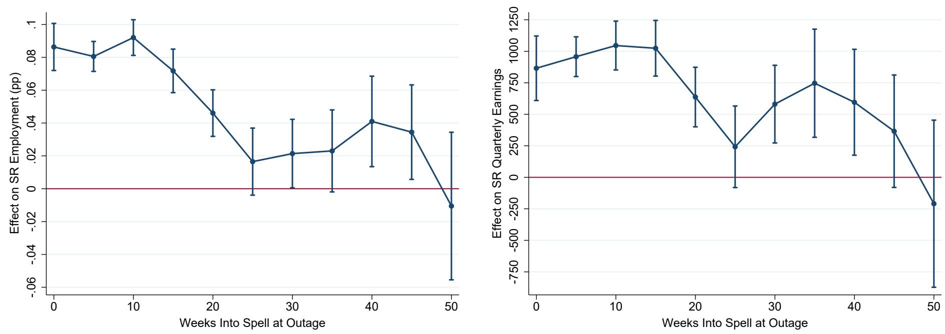
Figure A9: Distribution of UI Spell Durations



Notes: This figure shows the distribution of total UI spell durations for claimants in in our risk set as described in Section 3.2, broken out by delayed and non-delayed claimants. Total spell durations start at the beginning of the claim rather than at the beginning of the outage. See Section 4.3 for more details.

Figure A10: Heterogeneity by Spell Age, Reweighted and Reemployed Sample

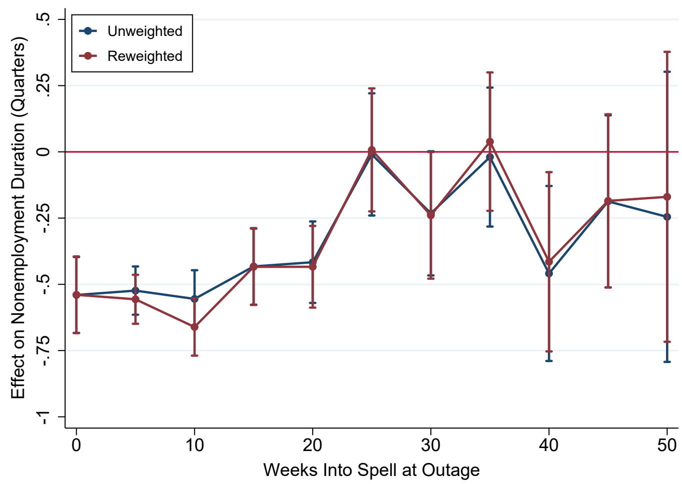
(**a**) SR Employment (**b**) SR Earnings



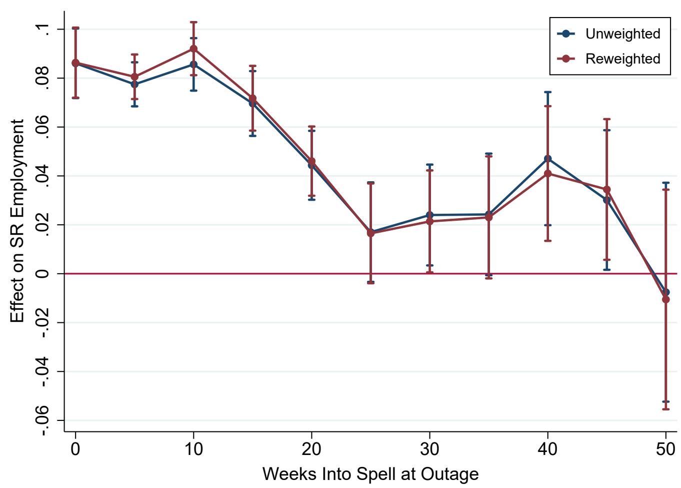
Notes: This figure plots the treatment effects of being exposed to a UI payment delay shock in September 2013 for employment and quarterly earnings for 5-week bins of spell age, restricting to claimants who are later reemployed and reweighting to balance the covariate distribution over spell age (see Section 5.2 for more details). The estimating equation is Equation 1. "SR" refers to short-run outcomes and is the average outcome over the first four quarters after the system glitch. Employment is an indicator for having nonzero earnings in the California UI Base Wage File. Quarterly earnings are deflated by the CPI-RS and capture all labor earnings in California.

Figure A11: Treatment Effects by Spell Age, Unweighted vs Reweighted

(a) Nonemployment Duration



(b) Short-Run Employment



Notes: This figure plots treatment effects of UI payment delays on labor market outcomes for 5-week bins of spell age, both unweighted (blue) and reweighted (red) to balance the covariate distribution over spell age (see Section 5.2 for more details). The estimating equation is Equation 1. "SR" refers to short-run outcomes and is the average outcome over the first four quarters after the system glitch. Nonemployment duration (Panel (a)) measures the number of quarters between the outage and the worker's first post-UI employment. Employment is an indicator for having nonzero earnings in the California UI Base Wage File.

B Data Appendix

B.1 Computing UI claimants' distance to hiring establishments

In Section 4.3, we analyze how delayed UI benefit payments affect the (worker-firm specific) match quality of the claimant's next firm. One measure of match quality presented in Table 4 is the distance between a worker's home address and actual place of work.[61] In this section, we further detail our construction process for this outcome.

To compute distance, we need to identify a place of work by assigning each UI claimant to a particular establishment. This linkage is not directly identified in our data; Base Wage records only link together workers and firms at the UI employer account number (EAN) level, an aggregated firm identifier that can be linked to multiple establishments.[62] We "impute down" from the EAN to establishments by assuming that the worker works at the establishment that is closest to their home address, following a similar procedure used by the Census Bureau in assigning workers to establishments in the LEHD. We obtain the worker's home address zip from their UI initial claim filing. For each hiring firm EAN, we construct a set of candidate hiring establishments and corresponding zips from the QCEW. Our measure of distance is thus between these two zips, using the NBER's ZIP Code Distance Database.[63]

Overall, this assignment process identifies establishments (and corresponding distances) for 96.6% of claimants who are ever reemployed in our data. Residual non-matches are either missing a zip code on their UI claim or all potential establishment matches are missing a zip code in the QCEW. This process is exact for 65% of workers employed at a firm with only one location, leaving 35% for whom we are imputing establishments based on distance. One remaining issue is that we observe a few very large distances between UI claim zip code and establishment zip, often across the state (e.g., between Los Angeles and San Francisco). A likely possibility is that these workers are working remotely or only intermittently commute to a specific workplace. To deal with this situation, we topcode all commuting distances at 750 miles (approximately twice the maximum distance within the Los Angeles-Riversize-San Bernadino commuting zone, the largest commuting zone within the state). This is relatively rare, affecting 1.4% of workers.

[61] One caveat is that workers likely have direct preferences over *commuting time* rather than distance. For instance, locations that are relatively close to one another may be difficult to reach due to public transit inaccessibility or other geographic features (e.g., having to cross a river in between). Due to data quality, we cannot directly compute the exact commuting distance that a worker faces, and we therefore view distance as an rough proxy for the true commute.

[62] More generally, there are three separate levels of employer aggregation in our data. The most granular measure, an establishment, identifies a particular location of work and is the object of interest in the present discussion. A collection of establishments can be linked together under a UI employer account number (EAN), which is the level at which a firm pays UI payroll taxes and is primarily identified in the UI system. Lastly, a collection of EANs can be further uniquely linked together under a single employer identification number (EIN), a identifier assigned by the IRS for federal tax form filing.

[63] Zip code distances are calculated as great-circle distances between central internal points for each zip code; see the project's link (`https://www.nber.org/research/data/zip-code-distance-database`) for more details.

B.2 Estimating the Worker-Firm Job Ladder Model

Setup. In Section 4.3, we analyzed how UI benefit payment delays affect the quality of workers' future employers, as measured by their future firms' average firm-specific wage premia. These premia are estimated as the firm fixed effects in a standard job ladder model of earnings, following Abowd et al. (1999, hereafter AKM), Card et al. (2013); Song et al. (2018), and other similar work. In this section, we describe our estimation procedure and present standard specification checks to assess model validity.

We estimate a statistical model in which log earnings y_{it} of a worker i at firm $\mathbf{J}(i,t)$ in time t are the sum of worker fixed effects α_i, firm fixed effects $\psi_{\mathbf{J}(i,t)}$, time-varying covariates $\mathbf{X}'_{it}\beta$, and a residual error term ε_{it}:

$$y_{it} = \alpha_i + \psi_{\mathbf{J}(i,t)} + \mathbf{X}'_{it}\beta + \varepsilon_{it} \tag{3}$$

In this setup, we interpret the worker fixed effect α_i as the worker-specific component of earnings, capturing time-invariant factors such as ability or general skills that are equally valued across employers. Similarly, the firm fixed-effect $\psi_{\mathbf{J}(i,t)}$ is the firm-specific component of earnings, representing firm-level pay premia that are received by all workers at the firm. This is our main object of interest in this analysis, and is used as an input to the analysis in the main text. The time-varying covariates $\mathbf{X}'_{it}\beta$ additionally measure aggregate and life-cycle components of earnings, such as year effects and varying returns to experience.[64] Lastly, the residual ε_{it} captures both transitory earnings variation and idiosyncratic worker-firm match effects.

Data Construction. Our data comes from the California EDD's Base Wage File, a confidential database which contains quarterly labor earnings information for all California wage employees between 1995 and 2021. These records are submitted directly to the EDD by employers, and are validated against other employer-submitted information (e.g., number of employees, total wages paid). Earnings are uncapped and can include wages, salaries, bonuses, tips, vacation pay, and other standard components of labor earnings. As in the main text, all earnings are expressed in real 2019 dollars. To begin, each observation represents a worker's earnings at a specific firm in a particular quarter.

We follow other work in this literature and impose three key data cleaning steps. First, we aggregate the data to the worker-year level[65] by assigning a main firm: we aggregate earnings across employers and quarters within the year, and assigning these earnings to the firm at which the worker had the most earnings. Next, we remove minimally attached workers by dropping worker-years in which total earnings were less than $4,000 (approximately full-time work at the minimum wage for one quarter). Lastly, we drop worker-years in which the worker is only employed by the firm for one consecutive year.

While we have a relatively large and long panel of worker earnings, the data do not contain demographic information (e.g., age and gender) for each worker. Indeed, all demographic information for the sample of UI claimants described in the main text comes directly from UI claims records. As a result, controlling for age directly as part of the lifecycle controls in $\mathbf{X}'_{it}\beta$ is infeasible in our setting. It is nonetheless important to control for these lifecycle factors since they constitute an important portion of earnings variation. To make progress on this issue, note that for any worker i, age can be written as the sum of the (unobserved, time-invariant) age at which a worker

[64] In Card et al. (2013) and other work, this lifecycle component is captured by a quadratic in age (recentered around age 40 where age-earnings curves are assumed to be approximately linear). Due to data availability in our setting, we employ a slightly different approach as described in the next section.

[65] While in principle we can repeat this analysis at the worker-quarter level, for computational reasons we are only able to estimate the model at the worker-year level.

began work in California, plus the (observed, time-varying) number of years since the worker first worked in California. We replace age with this last term, which is conceptually similar to a measure of observed labor market experience but crucially does not directly condition on workers' extensive margin employment choices over time. Since this measure differs from age only by a person-specific constant– namely, the age at which a worker begins work in California– the only differences from the typical age specification arise due to 1) the unobserved California labor market entry age effect being absorbed by the person effects α_i and 2) a shift in the higher order terms. In practice, the latter effect is minimal since estimated age quadratic effects are typically extremely close to 0 (this is also the case for our estimates). To adjust for the fact that our approach would assign the same years-since-started-working value to workers at the very start of our panel in 1995 (largely workers who entered the labor market in a previous unobserved year), and workers who enter the data in 2005 (more likely to be new labor market entrants), we make the following adjustment. We leave the first two years of wage data (1995 and 1996) out of the estimation sample, and interact years-since-started-working with an indicator for the worker's first observed start year being in 1995 or 1996. In essence, we allow the years-since-started-working lifecycle effects to differ for workers who enter at the very start of our data. Taken together, we view our estimated firm fixed effects $\psi_{\mathbf{J}(i,t)}$ – the main object of interest in this analysis – as broadly comparable to an alternate infeasible estimation approach based on age.

Estimation. As noted by Abowd et al. (2002); Card et al. (2013), and others, Equation 3 is only identified within a "connected set" of workers moving between firms, and estimates within different connected sets are not directly comparable to one another without additional structure. We therefore restrict our estimation to the largest connected set, which captures almost all workers and worker-years given the size and length of our worker earnings panel. Table B1 shows a comparison between the largest connected set and the full eligible sample, showing that the largest connected set contains 99.1% of worker-years, 98.99% of workers, and 77.4% of firms. We estimate the model using the zig-zag method. Without loss of generality, we also recenter the worker and firm fixed effects around 0 for ease of interpretation. The distributions of firm and worker fixed effects are presented in Figure B1.[66]

Next, we present two specification checks on the main design. The main identification assumption is that workers do not move across firms based on the transitory component of wages, for example if mobility is driven by a drop in wages.[67] To investigate this possibility, Figure B2 shows an event study of mean log earnings for movers grouped by the quartile of coworkers' wages at the origin and destination firm.[68] For ease of presentation, we only display moves where a worker moved from a firm in the top (4th) or bottom (1st) quartile. For most sets of movers, workers' wage profiles exhibit very little difference in the year before a move. One exception is for moves between

[66] A recent literature (Andrews et al., 2008; Kline et al., 2020; Bonhomme et al., 2023) has studied the extent to which estimates of the firm fixed-effects may be biased due to a limited number of moves for some firms within the connected set. Note that while this is a potentially problematic issue for variance decomposition—the main object of interest for those papers, as well as Card et al. (2013); Song et al. (2018) and others—this is not an issue for estimation of the firm effects themselves since the OLS estimator for each of the $\psi_{\mathbf{J}(i,t)}$ is unbiased.

[67] An excellent extended discussion of the model's identification assumptions is available in Card et al. (2013); we defer the interested reader to that paper's exposition.

[68] We construct the sample as follows. First, we consider all rolling 4-year windows in which a worker 1) is employed in every year; 2) is at a firm with over 1 worker in every year; and is employed by an origin firm for two consecutive years before moving to a destination firm for two consecutive years. By construction, a worker may qualify as a mover several times over their employment history, but no single move is counted twice. We next construct mean coworker earnings for each worker and compute the distribution of coworker earnings within each year. Movers are assigned to quartiles of coworker earnings based on the year immediately before and after the move.

the 1st and 4th quartiles: workers moving from a 4th quartile origin firm to a 1st quartile destination firm exhibit small drops in earnings before the move, which could be problematic for our design. However, note that a higher proportion of moves from 4th quartile firms are likely involuntary separations (e.g., mass layoffs) that induce periods of unemployment in the pre-move year, mechanically inducing slightly lower wages in the prior year.[69] One additional takeaway from the event study is that earnings profiles exhibit remarkable symmetry: losses for workers moving from the 4th to 1st quartile are approximately matched by gains for workers moving from the 1st to 4th quartile. We interpret this as evidence supporting separability in the worker and firm components.

Next, we investigate the model that workers in the same firm receive the same firm-level wage premium. This could be violated if, for example, firms pay larger premiums to high productivity workers. To examine this assumption, we plot the mean residual within deciles of the firm and worker effect distribution in Figure B3, Panel A. We find find that the mean residuals are almost always close to zero, and particularly so for high worker effect - high firm effect firms where one might intuitively imagine violations. The one exception is for low worker effect - low firm effect firms. While this pattern also holds in other contexts–potentially due to the statutory minimum wage restricting the floor of low wages–the mean residual is only about 0.02 and so we interpret this as a relatively small departure from model assumptions[70] The joint distribution of worker and firm effects in California (Panel B) is highly bimodal, with most worker-firm observations pairing together low (high) firm effect firms with low (high) worker effect workers.

[69] In ongoing companion work, we are studying the extent to which these pre-move changes are driven by differences between involuntary and voluntary moves.

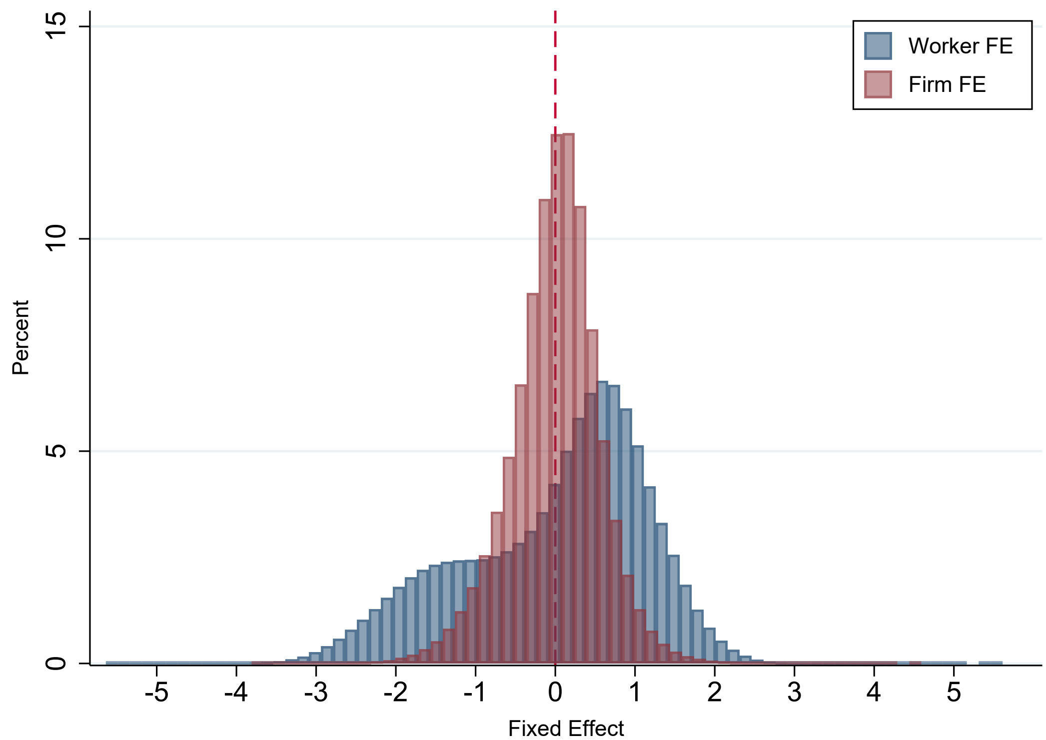
[70] Card et al. (2013, 2016); Song et al. (2018) also find similarly sized deviations when producing similar plots.

Table B1: Coverage in Full Sample vs. Connected Set

Level	**Full Sample**	**Largest Connected Set**
Worker-Years	370,850,484	367,142,756
Percent of Full Sample	1	.99
Workers	34,364,488	34,045,561
Percent of Full Sample	1	.99
Firms	3,265,590	2,500,268
Percent of Full Sample	1	.77

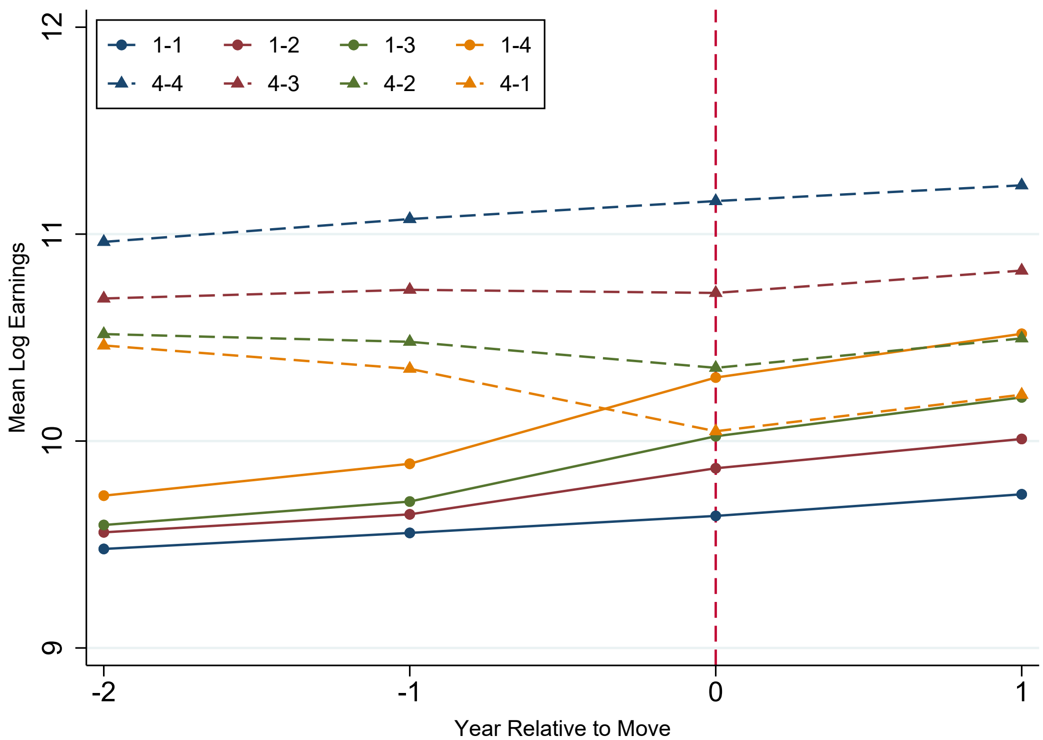
Notes: Table shows match statistics for the estimation connected set.

Figure B1: Distribution of Estimated Worker and Firm FE



Notes: Figure shows the distribution of estimated worker and firm fixed effects estimated from the job ladder model of earnings. See main text for more details.

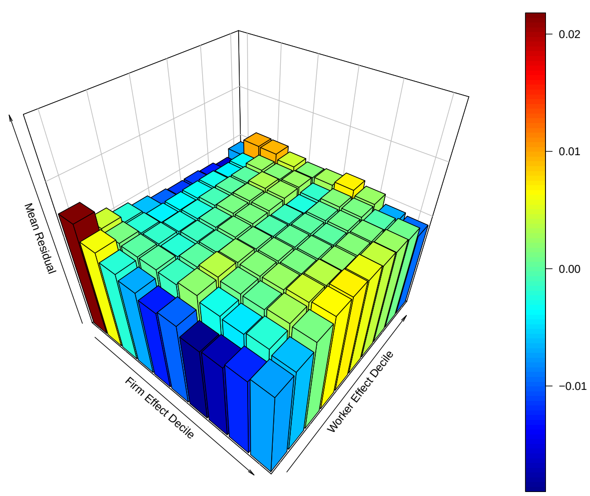
Figure B2: Mean Log Earnings of Job Switchers, by Coworker Wage Quartile



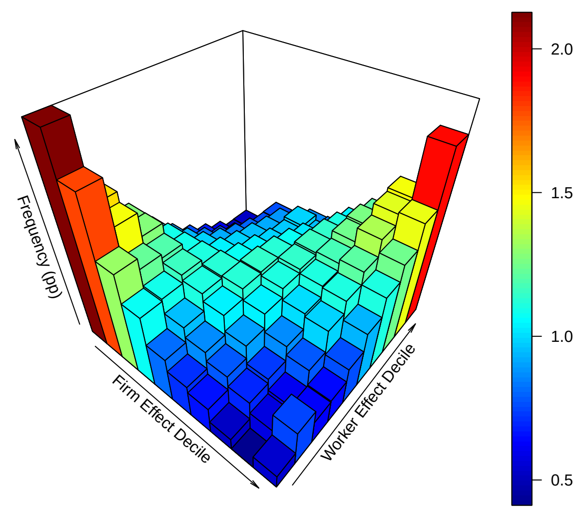
Notes:

Figure B3: Distributions by Deciles of Worker, Firm FE

(**a**) Mean Residuals



(**b**) Frequencies



Notes:

Table B2: Effect of Payment Delays on AKM Coverage

	(1) 1(Worker Wage Premium)	(2) 1(Separating Firm Wage Premium)	(3) 1(Next Firm Wage Premium)	(4) 1(Separating Match Wage Premium)	(5) 1(Next Match Wage Premium)
Delayed	.000555	.000873	.0315***	.0029**	.0494***
	(.00065)	(.00053)	(.0025)	(.0014)	(.0041)
Control Mean	.99	.99	.84	.95	.61
Spell FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes
N Spells	110,621	110,621	110,621	110,621	110,621

Notes: Notes.

C Data Sources and Transformations for Meta Analysis

In Section 5.3, we compare our own estimates to those of the prior literature. Here, we discuss some more details about the individual papers. We follow Nekoei and Weber (2017) and information on the effect of UI on nonemployment duration and reemployment wages directly from their replication file.[71]

We supplement this information with the estimates from Johnston and Mas (2018)and Lindner and Reizer (2020). Lindner and Reizer (2020) report effects on non-employment duration and reemployment duration wages in Table 2. Their preferred specifications are Panel (a) of Column (2) (for non-employment duration) and Panel (c) of Column (2) (for log reemployment wages scaled by baseline UI earnings). Non-employment duration is capped at 360 days and reemployment wages are conditional on obtaining employment within 360 days of the beginning of the UI spell.

Johnston and Mas (2018) report the effect of the benefit cut on log reemployment wages in Column (5) of Table 4. Because their data is similar to ours in that they only have quarterly earnings records, their log wage variable is actually the log of the earnings in the first full earnings quarter post UI (what we call the bracketed wage). Their point estimate is 0.035 with a standard error of 0.037. Johnston and Mas (2018) do not estimate the effect of the benefit cut on nonemployment duration directly. Rather, they make use of relative nonemployment probabilities by quarter to infer the effect of the benefit cut on nonemployment duration. Assuming that job finding rates do not differ between their treatment and control groups once all claimants have exhausted their UI benefits, they sum estimates of the difference in employment rates for the five quarters after the benefit cut (or placebo benefit cut in case of their control group). The find that a 1-month reduction in potential unemployment duration reduces the time in nonemployment by an average of 1.1 week, with a 95 percent confidence interval of (0.75, 1.4). We transform this estimate into a total effect as follows: Since Johnston and Mas (2018) study a 16-week benefit cut, this translates into a cut of UI duration of around 3.7 months (assuming 4.33 weeks per month). We multiply this by their point estimate of 1.1 additional weeks of nonemployment for every additional month of UI and obtain a total reduction in nonemployment duration of 4.06 weeks or 28.5 days (with a confidence interval of 19.4 to 36.3 days).

For our own estimates, we report wage effects for the log bracketed wage, or the log of the earnings of the first quarter for which the UI claimant has non-zero earnings in the preceding and succeeding quarter in the UI base wage file. For non-employment durations, we simply take our estimates of quarterly non-employment duration and multiply it by 90. While this slightly understates the true number of days in a quarter, it is consistent with Nekoei and Weber (2017) who use 30 days per month when translating the estimates in Schmieder et al. (2016) from months into days.

[71] The replication data can be accessed at this url `http://doi.org/10.3886/E113058V1`.

D Contemporaneous Interpretation of Payment Delays

D.1 Guidance Provided by the EDD to Affected Claimants

An important question is how claimants interpreted payment delays at the time of the system upgrade. In order to tackle this question, we make use of the Internet Archive (`https://archive.org/`) to obtain historical snapshots of the Employment Development Department website. From September to December of 2013, the Internet Archive crawled the EDD homepage a total of 60 times (7 times in September, 12 times in October, 17 times in November, and 24 times in December). From these crawled websites, we collect the guidance given by EDD to current UI claimants, and find updates provided on September 15, September 24, September 30, October 2, October 7, October 18, and November 8. In Figures D1, we show the updates provided by EDD over time.

The first update we can identify states "A small amount of certifications will require some one-time manual intervention to be processed. [...] We ask for the patience of our customer as we work through the necessary transition to a new, more efficient payment processing system." The update dated September 24, 2013 conveys similar information while adding that EDD now has a website with specific guidance for affected claimants (`http://www.edd.ca.gov/Unemployment/UI_Updates.htm`), a message that was re-iterated with updates on September 30 and October 2. On October 7, the info box on the EDD homepage says "The EDD is working to complete the transition to a new upgraded payment processing system as quickly as possible." An update from October 18 states "The EDD is working to complete the transition to a new upgraded payment processing system as quickly as possible." This message was re-iterated on November 8.

We can also use scraped website data to analyze the more detailed guidance to affected claimants. The website with specific guidance to claimants with delayed claims was first updated on September 13. Under a Section titled "Some Processing Delays in Transition", it says "[W]we are working through the conversion of old claim data into the new system which is temporarily interrupting payments for a subset of our customers with some more complexity associated with their claims. [...] The EDD is working around the clock to catch up on these associated payments and we are making progress. [...] The staff here at the EDD truly understand just how important unemployment benefits are to our customers – a critical lifeline while they are out of work. We ask for your patience as we work to complete this transition as quickly as possible." An update from September 27 reads "The EDD continues to work around the clock to clear the backlog of certifications that has created undue financial hardship for so many Californians. [...] The EDD is sending out notifications to these individuals experiencing a backlog delay to confirm for them that we have received their certification and are working to quickly clear it through processing." The November 8 update states that "[i]f someone still believes they are due payment dating back to the launch of our new system, they are likely either ineligible, or their case is pending due to more complex challenges which have always existed in the UI program[.]"

Overall, we can see that EDD's guidance throughout is for claimants to recertify again and that payment delays were of temporary nature and did not imply a permanent loss of access to UI benefits. Rather, EDD told claimants that the non-payment of benefits was the result of the systems upgrade or due to complex cases that required manual review. In either case, claimants were encourage to keep on recertifying for benefits.

D.2 Contemporaneous News Reporting

In order to understand claimants interpretation of the temporary non-payment of benefits, we also consulted contemporaneous news reporting. To do so, we analyze reporting from nine of the ten largest daily newspapers by circulation in California as well as AP Statewire: California.[72] For every news source, we keep all articles between August and November 2013 mentioning "unemployment", "system", and "Employment Development Department". We exclude reporting focused on newly released statistics (initial claims or new unemployment rate figures). For the remaining articles, we lemmatize the header or full body of text and generate a word cloud of the resulting set of words. These are presented in Appendix Figure D2. Based on the word clouds, reporting also appears to describe the system upgrade as resulting in a temporary backlog of payments. Prominent words include "delay", "delayed", "glitch", "problem", "backlog", and "backlogged". On the other hand, there is little evidence that contemporaneous reporting described a permanent loss of access to benefits (as evidenced by the absence of terms like "loss", "ineligible", or "end"). While this is only suggestive evidence, combined with the guidance provided by the EDD, we believe that the overwhelming contemporaneous interpretation of the payment issues was that of a temporary lapse of payments, not a permanent cut in benefits or a change in UI policy that deemed thousands of claimants ineligible over night.

[72] The included newspapers are (in order of circulation) Mercury News, Los Angeles Times, Sacramento Bee, Orange County Register, Contra Costa Times, San Francisco Chronicle, Fresno Bee, San Diego Union-Tribune, and The Press-Enterprise. Articles for every paper except the Los Angeles Times are accessed through Access World News. Los Angeles Times articles are accessed through ProQuest.

Figure D1: Contemporaneous Guidance to UI Claimants

(a) September 15, 2013

Update for Benefits Affected by New Upgrades

A small amount of certifications will require some one-time manual intervention to be processed. Further enhancements will be installed over the weekend. We ask for the patience of our customers as we work through the necessary transition to a new, more efficient payment processing system. It is not necessary to call EDD on this issue. We are working to process all certifications received. For more information and updates on the UI System and how it may affect you, visit the New Upgrades for the UI System page.

(b) September 25, 2013

Update for Benefits Affected by New Upgrades

En español

Updated September 24, 2013, 5:30 p.m

The EDD continues to work through a subset of certifications for ongoing unemployment benefits that will require some lengthier, one-time manual processing. We ask for the patience of our customers as we work through the necessary transition to a new, more efficient payment processing system. It is not necessary to call EDD on this issue. We are working to process all certifications received and notices will be sent to customers to confirm that we have received their certification(s). For more information and updates on the new payment processing system and how it may affect you, visit the New Upgrades for the UI System page.

(c) September 30, 2013

Update for Benefits Affected by New Upgrades

En español

Updated September 30, 2013, 4:30 p.m

The EDD continues to work through a subset of certifications for ongoing unemployment benefits that will require some lengthier, one-time manual processing. We ask for the patience of our customers as we work through the necessary transition to a new, more efficient payment processing system. It is not necessary to call EDD on this issue. We are working to process all certifications received and notices will be sent to customers to confirm that we have received their certification(s). For more information and updates on the new payment processing system and how it may affect you, visit the New Upgrades for the UI System page.

(d) October 5, 2013

Update for Benefits Affected by New Upgrades

En español

Updated October 2, 2013, 5:30 p.m

The EDD continues to work through a subset of certifications for ongoing unemployment benefits that will require some lengthier, one-time manual processing. We ask for the patience of our customers as we work through the necessary transition to a new, more efficient payment processing system. It is not necessary to call EDD on this issue. We are working to process all certifications received and notices will be sent to customers to confirm that we have received their certification(s). For more information and updates on the new payment processing system and how it may affect you, visit the New Upgrades for the UI System page.

(e) October 19, 2013

Update for Benefits Affected by New Upgrades

En español

Updated October 18, 2013, 5 p.m.

The EDD is working to complete the transition to a new upgraded payment processing system as quickly as possible. We are supplying continual updates on our progress and are providing recommendations to our claimants, including how they can help us expedite this effort with the submission of their certifications for continuing benefits. For more information and updates on the new payment processing system and how it may affect you, visit the New Upgrades for the UI System page.

(f) November 11, 2013

Update for Benefits Affected by New Upgrades

En español

Updated November 8, 2013, 4:30 p.m.

The EDD is working to complete the transition to a new upgraded payment processing system as quickly as possible. We are supplying continual updates on our progress and are providing recommendations to our claimants, including how they can help us expedite this effort with the submission of their certifications for continuing benefits. For more information and updates on the new payment processing system and how it may affect you, visit the New Upgrades for the UI System page.

Notes: Guidance boxes for current UI claimants on Employment Development Department Website. Websites are obtained through the Internet Archive Project (`https://archive.org/`). Panel titles refer to dates the website was scraped on, so screenshots represent the information available to UI claimants on that specific date. We omit a screenshot of an update from October 7th (scraped on October 12th) that has identical guidance to that provided in the October 18th update.

Figure D2: Word Cloud of Contemporaneous Reporting

(**a**) Headlines

(**b**) Full Text

Notes: Word clouds of contemporaneous news reporting at the time of the outage. Panel (A) is derived from the lemmatized headlines from 28 articles on the system outage from the largest local newspapers in California and AP Statewire. Panel (B) is derived from the lemmatized full articles.