The Effect of Unemployment Benefit Pay Frequency on UI Claimants' Job Search Behaviors

Guangli Zhang

Saint Louis University

Abstract

This paper presents new evidence on how UI (Unemployment Insurance) benefit pay frequencies affect the job search behaviors of UI claimants in the United States. By exploiting quasi-experimental variations in states' benefit pay schedules, I find that switching from biweekly to weekly pay significantly increases UI claimants' unemployment durations. This observed effect can be partly rationalized by the more frequent end-of-the-month positive benefit shocks under weekly pay schedules. I conclude that the previously overlooked policy parameter, *benefit pay frequency*, has important effects on the job search behaviors of UI claimants.

JEL codes: H55, J65
Key words: unemployment insurance, natural experiment, benefit pay frequency

Working Paper 21-03
August, 2021

1. Introduction

The effects of UI (Unemployment Insurance) generosity on workers' unemployment durations have been extensively studied.[1] A robust finding is that higher UI generosity (measured in benefit amount and/or potential durations) lengthens unemployment duration.[2] Consequentially, policy discussions regarding the UI program mostly center on these two 'generosity' parameters. However, non-monetary policy parameters might also have important impacts on individuals' decisions. This paper examines the effect of a previously overlooked policy parameter – *benefit pay frequency* – on UI claimants' search behaviors. Using plausible state-year level policy variations in benefit pay frequency, I find switching from biweekly to a more frequent weekly pay schedule increases UI claimants' unemployment durations (or equivalently, decreases reemployment hazard).[3]

Why does benefit pay frequency matter for households' labor supply decisions? First, several studies of consumption responses to anticipated income note that even unconstrained households exhibit "excess-sensitivity" to anticipated income.[4] These findings suggest that a considerable fraction of many households consume hand-to-mouth. A more frequent pay schedule could potentially reduce households' tendencies to spend excessively by imposing a smoother income flow. The improved consumption smoothing capability would therefore reduce households' urges to find a job quickly. Second, Vellekoop (2018) documents households' intra-monthly budgeting cycles are driven by the end-of-the-month rent/ mortgage payments. Therefore, fluctuations in end-of-the-month cash-on-hand that are generated by variations in benefit pay frequencies could potentially affect households' consumption and labor supply decisions.[5]

Motivated by these recent findings from the household finance literature, benefit pay frequencies could affect UI claimants' search behaviors through a combination of mechanical and behavioral channels. Mechanically, a more frequent weekly benefit pay schedule

[1]See Krueger and Meyer (2002) and Schmieder and Von Wachter (2016) for a summary of past studies.

[2]This effect combines a welfare reducing moral hazard effect and a welfare enhancing liquidity effect (Chetty, 2008). The moral hazard effect occurs when increases in UI generosity reduce UI claimants' net incentives to search. Independently, the liquidity effect occurs when increases in UI generosity enable UI claimants with limited consumption-smoothing capabilities to afford to wait for better jobs.

[3]Throughout the paper, I define the reemployment hazard (h_t) as the likelihood of finding a job at the end of period t, conditional on entering period t unemployed. In addition, one can roughly interpret a 10% increase in reemployment hazard equivalent to a 10% decrease in expected unemployment duration.

[4]Under the Life-Cycle/Permanent Income Hypothesis (Modigliani and Brumberg, 1954, Friedman et al., 1957), the frequency or timing of benefit payments should not matter as forward-looking rational agents' expenditures do not respond to shapes or paths of anticipated inflow of income. See Browning and Lusardi (1996), Browning and Crossley (2001*a*) and Jappelli and Pistaferri (2010) for a summary of past studies.

[5]Note, both mechanisms are operating through the *liquidity effect* as in Chetty (2008).

would increase the occurrences of positive (monthly) liquidity shocks during unemployment. Zhang (2017) who evaluated income on a monthly basis, finds that households with biweekly pay schedule can receive three paychecks (instead of two) once every six months, whereas households with weekly pay schedules can receive five paychecks (instead of four) once every three months. The quasi-experimental variations in the timing and the magnitude of the extra benefit can generate different liquidity shocks to UI claimants being paid under different payment frequencies. Second, in order for these extra benefit to have impacts, households should exhibit excess sensitivities to these anticipated income shocks. Common explanations for this phenomenon include the limited ability to smooth consumption due to liquidity constraints (Browning and Crossley, 2001*b*), quasi-hyperbolic discounting (Ganong and Noel, 2019, Gerard and Naritomi, 2019), illiquid savings (Kaplan, Violante and Weidner, 2014), household's tendency to hold lifetime wealth in cash (Olafsson and Pagel, 2018) and reliance on rules-of-thumb or heuristics (Zhang, 2017). In this paper, I investigate the combined effect from the mechanical and behavioral channels of the pay frequency effect on households.

The UI program in the United States provides an ideal environment to examine the pay frequency effect, as benefit pay frequency varies across states and over time. However, the impact of pay frequency has received little attention partly due to the small monetary differences between weekly and biweekly pay.[6] This paper makes two contributions to the UI literature. First, it quantifies the pay frequency effect in the context of labor supply under unemployment insurance. Second it documents UI claimants' search responses to anticipated fluctuations in the monthly benefit amount, holding the benefit amount constant. To my best knowledge, neither benefit pay frequency nor the timing of extra benefits have been extensively explored in the context of labor supply under social insurance policies.[7] Therefore, I propose two plausible dimensions of heterogeneity for evaluating social benefits.[8]

This paper uses data from the 1985-2007 Survey of Income and Program Participation (SIPP) to provide new evidence of the effects of benefit pay frequencies. SIPP is well suited to this project because it contains information from different states from 1985-2007. The

[6] Apart from Fishman et al. (2003), who collected information on continued UI certification frequency for 8 states and discussed its impact on UI takeup in 2003.

[7] Note, the consumption responses to payment and expenditure timing has been well studied. For example, Castner, Henke et al. (2011) finds food stamp recipients spend a disproportionally large fraction of their SNAP benefits at the start of their benefit month – this is known as the "SNAP cycle". In a recent study, Beatty et al. (2019) finds the SNAP cycle is more pronounced for workers who are paid on a weekly or monthly basis.

[8] In a follow up study, I plan to apply the same type of analysis to Workers Compensation.

panel structure allows me to follow individuals over the course of 2.5 to 4 years and observe the transitions in and out of unemployment, and the weekly employment variable allows me to obtain the precise lengths of unemployment. The detailed measures of individuals' assets prior to unemployment in SIPP's supplemental surveys allow me to control for UI claimants' monetary constraints at the time of unemployment.

The paper is divided into three parts. I start by estimating the effects of different benefit pay frequencies on UI claimants' reemployment hazard. Next, I investigate a potential mechanism by examining the effect from receiving anticipated extra benefit checks on UI claimants' reemployment hazard under different pay frequencies. Lastly, I examine several policy implications relating to the frequency of benefit payments.

Section 2 uses plausible quasi-experimental changes in UI benefit pay frequency to examine its impact on UI claimants' unemployment durations (or reemployment hazards). In my benchmark analysis, I estimate Cox proportional hazard models with state fixed effects and year fixed effects. When I restrict my analysis to New York (1993), Washington (1996) and Massachusetts (2003), I find switching from biweekly to weekly pay frequency increases expected duration by 2-4 weeks (decreases reemployment hazard by 22%). To assess the robustness of the benchmark estimates, I adopt a event study framework to estimate the dynamic impact of pay frequency change on job finding hazard. [9] Interestingly, results from the event study design suggests the main effect seems to only have short run impacts on UI claimants' job finding hazard.

In the presence of households who are hand-to-mouth, I examines a possible channel that can rationalize the pay frequency effect — the frequency and the magnitude of extra benefit checks in Section 3. Variations in the end-of-the-month cash on hand can have important impacts on UI claimants' monthly cash flows as most major expenditures – such as rent, credit card debt, utility bills, mortgage – occur around the end of each month. In this section, I adopt the quasi-experimental variation introduced in Zhang (2017) by estimating UI claimants' responses to the anticipated extra benefit checks under weekly or biweekly pay frequencies.[10] Overall, I find (possibly) receiving an anticipated extra benefit at the end of the month leads to a 34.0% (or 16.3%) decrease in the next month's reemployment hazards for claimants under weekly (or biweekly) pay. The finding seems to suggest that the effect

[9]States with less than 100 observations from 1985-2007 are excluded from the main analysis.

[10]Note, (1) in terms of the *magnitude*: the extra benefit amount is equivalent to a 25% (or 50%) increase in the regular monthly benefit level for weekly (or biweekly) pay states; (2) in terms of the *occurrence frequency*: the extra benefit month occurs 4 times (or 2 times) under weekly (or biweekly) pay.

of extra benefit check on unemployment durations exhibit diminishing marginal returns.[11] Given that UI claimants under weekly pay can experience *twice* as many end-of-the-month benefit shocks as those under biweekly pay, the frequency of extra benefit checks plays a more important role in affecting UI claimants' job search behaviors.

To design a cost-effective social benefit program, policy makers needs to know the potential costs and benefits from switching to a more frequent benefit pay schedule. Therefore I investigate implications from varying pay frequencies on various policy outcomes. First, using data from the Annual Survey of Government Employment and Payroll (ASGEP), I estimate the impact of switching to weekly pay on state governments' annual UI administrative costs. I find close to zero and statistically insignificant effect. Second, using samples from 1985-2007 SIPP, I find switching to weekly pay does not affect UI eligible workers' UI take-up rate. Third, I find suggestive evidence that switching to weekly pay could potentially reduce the liquidity effect (gains from consumption smoothing) from increases in UI benefits.

This paper is closely related to the literature on estimating the consumption smoothing benefit of unemployment insurance. Several papers have estimated the effect from varying the pre-unemployment asset level on unemployment durations and found a considerable liquidity effect (Card, Chetty and Weber, 2007, Chetty, 2008, LaLumia, 2013). All these papers find UI claimants with larger pre-unemployment assets tend to search longer as they are more capable to smooth their consumptions during unemployment. However, studying the effect from varying cash on hand *during* unemployment spell is equally important. A recent strand of literature has examined the optimal path of benefits (Schmieder and von Wachter, 2017, Kolsrud et al., 2018, Ganong and Noel, 2019, Gerard and Naritomi, 2019, Lindner and Reizer, 2020). In particular, most of these studies have relied on temporal variations in benefit extensions to estimate the fiscal cost and consumption smoothing benefits from a step-wised benefit path.[12] On the other hand, this is the first paper that examines the impact of variations in (i) benefit pay frequency and (ii) monthly benefit amount under a constant (weekly) benefit path. Findings from this paper highlight the importance of incorporating the frequency and the timing of income and expenditure streams when evaluating the consumption smoothing benefit of UI.

In addition to the empirical literature on unemployment insurance, this paper also re-

[11] For example, receiving two separate \$500 extra checks lead to larger responses in UI claimants' durations than receiving a one-time \$1000 extra check.

[12] A noticeable exception is Lindner and Reizer (2020), who find that front-loading UI benefit payments leads to shorter unemployment duration and increases in reemployment wage.

lates to the recent household finance literature that examines households' consumption and borrowing responses to the anticipated timing and frequency of income (Aguila, Kapteyn and Perez-Arce, 2017, Zhang, 2017, Berniell, 2018, Olafsson and Pagel, 2018, Baugh and Correia, 2018) or the timing of consumption commitments Vellekoop (2018). In particular, from two closely related works, Leary and Wang (2016) and Baugh, Leary and Wang (2018) find households experience more financial shortfalls when the timing of income and expenditure streams are misaligned. The new evidence documented in this paper indicates that households' imperfect budgeting responses to the anticipated liquidity streams can have significant spillover effects on their labor supply decisions, at least in the context of unemployment insurance.

The rest of the paper is organized as follows. Section 2 studies the impact of pay frequency variations on UI claimants' reemployment hazards. Section 3 explores one potential mechanism. Section 4 discusses related policy implications of switching to weekly benefit pay frequency. Section 5 includes a series of robustness checks for the pay frequency effect estimation and section 6 concludes.

2. Empirical Evidence: Pay Frequency and Reemployment Hazard

This section briefly discusses the UI operations and pay frequency procedures used in the United States when this paper was written. Section 2.2 explains the empirical strategy, Section 2.3 describes the dataset and Section 2.4 presents the first empirical finding.

2.1. UI Benefit Pay Frequency in the United States

The Unemployment Insurance (UI) benefit program, part of the Federal Social Security Act of 1935, is designed to provide periodic economic support for individuals who are laid off involuntarily (Price, 1985). In most states, the program ensures a weekly benefit amount (WBA) for up to 26 weeks determined by the claimant's earnings from the most recent four calendar quarters, i.e. the base period. Eligible individuals file an initial claim in the state they reside in during the first week of unemployment, and may then wait three weeks or longer before the claim is processed. Maximum WBAs, otherwise known as coverage generosity, continued claim certifications, and payment requirements and frequencies vary by state. The map in Figure 1 shows the weekly certification benefits in 2007 for each state.

To be eligible for receiving continued benefits, claimants are required to file certification periodically after the initial claim. The certification process asks claimants to report their earnings, job offers, and job search activities for the past benefit week(s) they are claiming benefits for. Many states impose a minimum amount of weekly job search requirement and only a small fraction of the claimants' search activities were audited.[13] The payday is usually 2-4 days after the certification day depends on the state and most UI benefits were distributed via mailed checks. Most states require weekly certification; some states allow claimants to choose to file either weekly or biweekly and others require claimants to file biweekly.[14]

Prior to the 1980s, many states use a biweekly payment frequency due to the limited capability in filing and payment technology (Blaustein, 1979). Back then, in-person claim and mail claim were the two predominant ways to file a continued UI certification. Starting from the mid 1990s, the introduction of the more advanced telephone and online filing systems as well as the direct deposit payment system induced several states to opted-in for a more frequent (weekly) filing process for continued UI claims. As of today, nine states still require claimants to file for continued certification and receive payments on a biweekly basis.

The Department of Labor (DOL) does not explicitly record the UI pay frequency in the *Significant Provisions of State Unemployment Insurance Laws*, I relied on two complementary information sources to recover the state level benefit pay frequency policies. The primary source comes from the Benefit Accuracy Measurement (BAM) program, administrated by the DOL. For the purpose of auditing, BAM samples around 10 UI claimants every week for each state. Important to this study, BAM contains information on continued benefit filing frequency starting from 1985. For each state, I used the year that the share of alternative claiming method accelerated by the greatest amount and designated it the event year.[15] To complement with the BAM survey, I manually collected the pay frequency information from archived documents on state government websites. Using *Google.com* and *Archive.org*, I was able to verify all UI pay frequency information starting from the mid-1990s.[16] I find several states introduced weekly benefit payment: District of Columbia (2008), Maryland (2014), Massachusetts (2003), Minnesota (2007), Montana (2015), New Hampshire (2003), New Jersey (2014), New Mexico (1999), New York (1993), Oregon (1992), Rhode Island

[13] Only 10 randomly selected UI claimants are chosen to be audited by the Benefit Accuracy Measurement program every week for each state.

[14] Almost all states define a benefit week as a calendar week from Sunday through Saturday; New York is the only state that defines a benefit week as Monday through Sunday.

[15] Apart from Nevada and Ohio, all switcher states changed their pay frequency from Biweekly to Weekly pay.

[16] I have attached the detailed UI pay frequency records along with their document sources in the appendix, see Table A1

(1996), Utah (1994), Virginia (1998), Washington (1996) and Wyoming (2018). Most of these states fully switched to a weekly filing system after 1-5 years. I use Massachusetts, New York and Washington for my main analysis.[17]

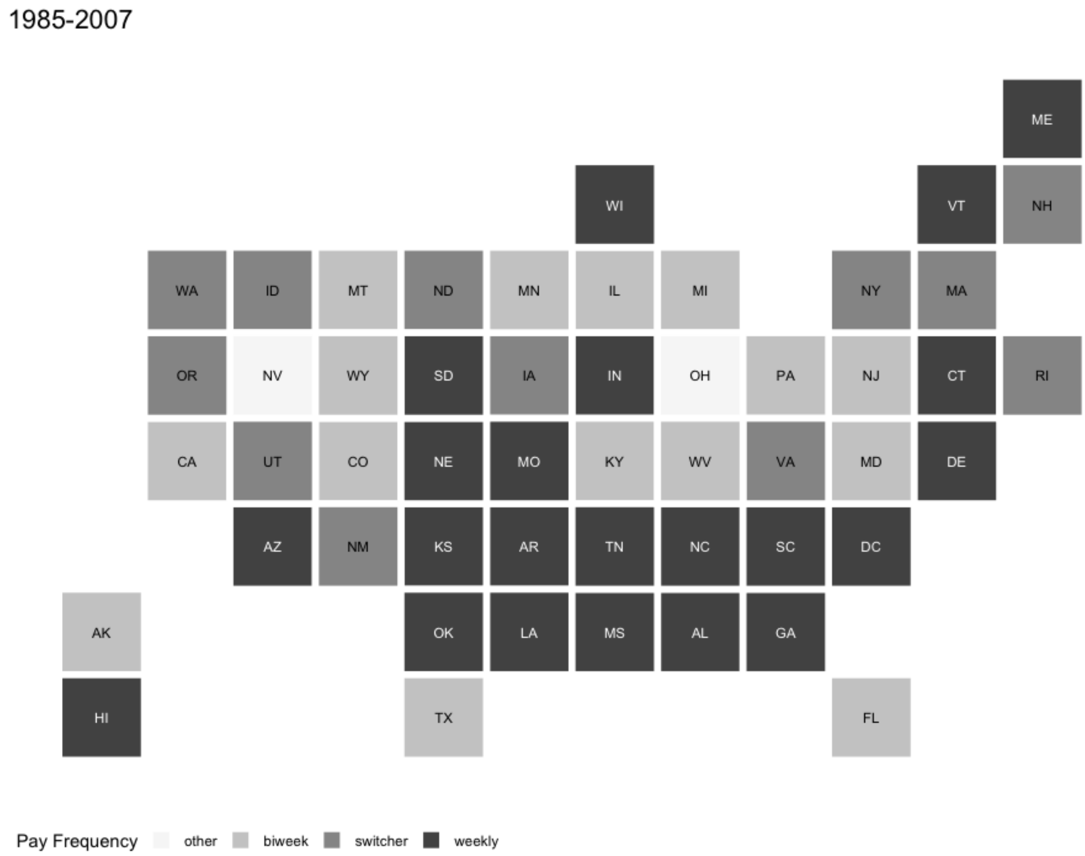


Figure 1: UI Benefit Filing and Pay Frequency Policies

Note: The US map shows the UI benefit pay frequency by state at the time of 2007: States that pays biweekly are in light gray; states that switched pay frequency from biweekly to weekly pay are in darker gray; states that pays weekly are in black. Nevada switched pay frequency two times; Ohio allowed for either weekly or biweekly filing. The pay frequency policy information are collected from a combination of the archived state websites via *archive.org* and survey results from the Benefit Accuracy Measurement Audit.

[17]The rest of the switcher states are excluded from my analyses due to limited observations – concerns with attenuation bias.

2.2. Empirical Strategy

Most states changed their benefit pay frequency to encourage the use of more advanced filing technology by claimants, i.e. mail to telephone, or telephone to internet.[18] Given that the changes in the continued filing technologies are implemented without prior notice, it is unlikely that UI claimants would exhibit anticipatory responses to such policy variations.

To study the effects of switching from biweekly to weekly pay frequencies on UI claimants' unemployment exits, my empirical strategy exploits the state level variations in UI payment frequencies.[19] In particular, I estimate a series of Cox proportional hazard models with the following specification:

$$\log h_{ist} = \alpha_t + \beta_1 \mathbf{1}\{weeklypay_s\} + \mathbf{X}_{ist} \tag{2.1}$$

where h_{ist} is the hazard rate of exiting unemployment for individual i from state s at unemployment week t. α_t is the flexible non-parametric baseline hazard rate at the given week t conditional on surviving. $weeklypay_s$ is a dummy indicates the pay frequency for state s at a given year. Specifically, $weeklypay_s = 1$ if pay frequency is on a weekly basis, and $weeklypay_s = 0$ if pay frequency is on a biweekly basis. $\mathbf{X_{ist}}$ is a set of controls: (1) state level controls that include start-of-the-spell monthly unemployment rate and UI generosity; (2) Industry, occupation fixed effect and (3) individual specific controls such as 10-piece log-linear spline for the claimant's pre-unemployment wage, total wealth, age, education, marital status and being on the seam between interviews to adjust for the seam effect. Lastly, $\mathbf{X_{ist}}$ also includes (4) year fixed effects that capture changes over time that vary uniformly across states and (5) state fixed effects that capture time invariant cross state differences. Standard errors are clustered at the state level.

Because UI benefit is not well measured under the SIPP survey, I use three alternative proxies for claimants' benefits: (i) individual predicted benefit, (ii) state-year level simulated replacement rate and (iii) state-year level maximum benefit. The first proxy – *predicted*

[18]For example, the original communication sent by the New Jersey Department of Labor stated: "The New Jersey Department of Labor and Workforce Development is encouraging all of our unemployment insurance customers to claim their benefits each week by using our Internet application at www.NJUIFILE.net. That's correct! Instead of claiming your benefits every two weeks, you may now claim them each week and receive a benefit payment each week...."

[19]As documented in Anderson and Meyer (1997), UI take up decision is endogenous and can be largely affected by factors such as benefit generosity or potential compensated duration. Controlling for benefit generosity and possible UI duration extensions, I find the elasticity of take up with respect to pay frequency change is insignificant and close to zero, which suggest the endogeneity in take-up decision is unlikely to be affected by the policy change in benefit pay frequency. Similarly, Ebenstein and Stange (2010) finds no impact of UI filing technology on UI takeup.

benefit – follows the two-step approach from Chetty (2008). In the first step, I predict claimants' pre-unemployment log annual wages using their observable characteristics (as included in $\mathbf{X_{ist}}$ in Eq.(2.1)). In the second step, I plug the predicted wages into a UI calculator to obtain claimants' predicted UI benefits.[20] The construction of the second proxy – *simulated replacement rate* – follows the standard two-step procedure (Gruber, 1997, Kroft and Notowidigdo, 2016, East and Kuka, 2015). The idea here is to use to policy change in state-year level UI generosity to proxy for average claimants' UI benefit. In step one, I predict claimants' pre-unemployment log annual wages using observable characteristics. In step two, I use a fixed 1993 national sample to compute the average weekly benefits and UI replacement rate for all state-year combination in the data set. The two-stage simulated replacement rate only depends on observable demographic characteristics and variations from state laws. Lastly, given the fact that approximately 50% of UI claimants receive the maximum benefit Chetty (2008), I also use the *maximum weekly benefit* to proxy for individual claimant's UI benefit.

2.3. Data and Sample

I use unemployment spell data from the Survey of Income and Program Participation (SIPP) from 1985-2007. SIPP is a panel data that contains weekly employment status so I can follow an unemployed worker over time. I closely follow Chetty (2008) and Kroft and Notowidigdo (2016) when constructing my sample for this part of the analysis: I restrict my sample to be prime-age males who (a) report searching for a job, (b) are not on temporary layoff, (c) have at least 3 months of work history in the survey (to compute pre-unemployment earnings), (d) took up UI benefits within the first month of unemployment. Furthermore, to reduce the influence of outliers and restrict my attention to search behavior in the first year after job loss, I censor unemployment duration at 50 weeks. Lastly, All monetary values are adjusted into 1990 dollars using CPI-U.

Apart from the aforementioned sample construction criteria, I make additional restrictions on individuals' wealth measures. For UI claimants in the SIPP data, wealth measures are collected through the topical module - "asset and liquidity" - which only happens 2 to 3 times in a panel. Therefore, about one-half unemployment spells does not contain wealth measures prior to the unemployment. One approach is to use ex-post (post-/ during-unemployment) wealth measures to proxy for ex-ante wealth. However, UI claimant's ex-post wealth level is endogenous to factors such as unemployment duration (Gruber, 2001) and

[20] I used UI calculator program from Kuka (2020).

thus is a noisy indicator to an individual's ability to consumption smooth. Since the pay frequency effect could potentially affect UI claimant's search effort through the liquidity channel, I restrict my sample to those with information on pre-unemployment total (liquid and non-liquid) wealth holding. The aforementioned restrictions leave me 3,406 unemployment spells in the pooled sample.

	Pooled		Switcher		Non-Switcher	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Unemployment Duration (weeks)	18.13	13.70	18.69	14.27	18.03	13.60
Average UI weekly benefit ($)	170.59	29.67	194.21	25.27	166.46	28.43
Maximum UI weekly benefit ($)	235.67	54.51	273.37	37.14	229.07	54.38
Simulated replacement rate	0.51	0.05	0.50	0.02	0.51	0.05
Age	38.75	11.33	39.28	11.58	38.66	11.28
Years of Education	12.32	2.84	12.73	2.78	12.25	2.85
Married	0.63	0.48	0.58	0.49	0.63	0.48
Pre-ue annual wage ($)	21183.49	16122.33	22468.60	15967.98	20958.74	16141.39
Pre-ue liquid wealth ($)	32749.87	96605.55	40208.85	111824.70	31194.76	94078.79
Pre-ue unsecured debt ($)	4889.97	18534.49	6026.81	31496.22	4691.15	15170.78
Pre-ue home equity ($)	35268.72	57448.00	50209.10	74291.17	32655.82	53554.67
# Spells	3,646		507		2,919	

Table 1: Descriptive Statistics for Switcher and Control States, SIPP 1985-2007

Note: The data presented are individual level unemployment spells from 1985-2007 SIPP data. The average and maximum UI weekly benefit amount are obtained from the US Department of Labor. All dollar values are converted into 1990 values. The sample is restricted to male UI claimants only. The pay frequency policy information are collected from archived state websites via *archive.org* and the BAM survey.

2.4. Empirical Result

I begin by providing graphical evidence on the effect of change in pay frequency on duration for UI recipients in the treated states. Then I use regression analyses to complement the graphic analysis. In this part, I split my sample into two subsamples according to the frequency of receiving UI benefits: weekly or biweekly. In particular, prior to the policy change, the switcher state is included in the biweekly group; after the policy change, the switcher state is moved to the weekly group.

Figure 2 is the Kaplan-Meier survival curves for UI claimants under weekly or biweekly benefit pay frequencies. The survival curve for claimants under biweekly pay frequency is slightly lower, indicating a less frequency pay schedule is associated with shorter unemployment duration. Partly due to the limited number of samples for the switcher states, the difference is statistically insignificant under a non-parametric Wilcoxon test for equality with p=0.1598.

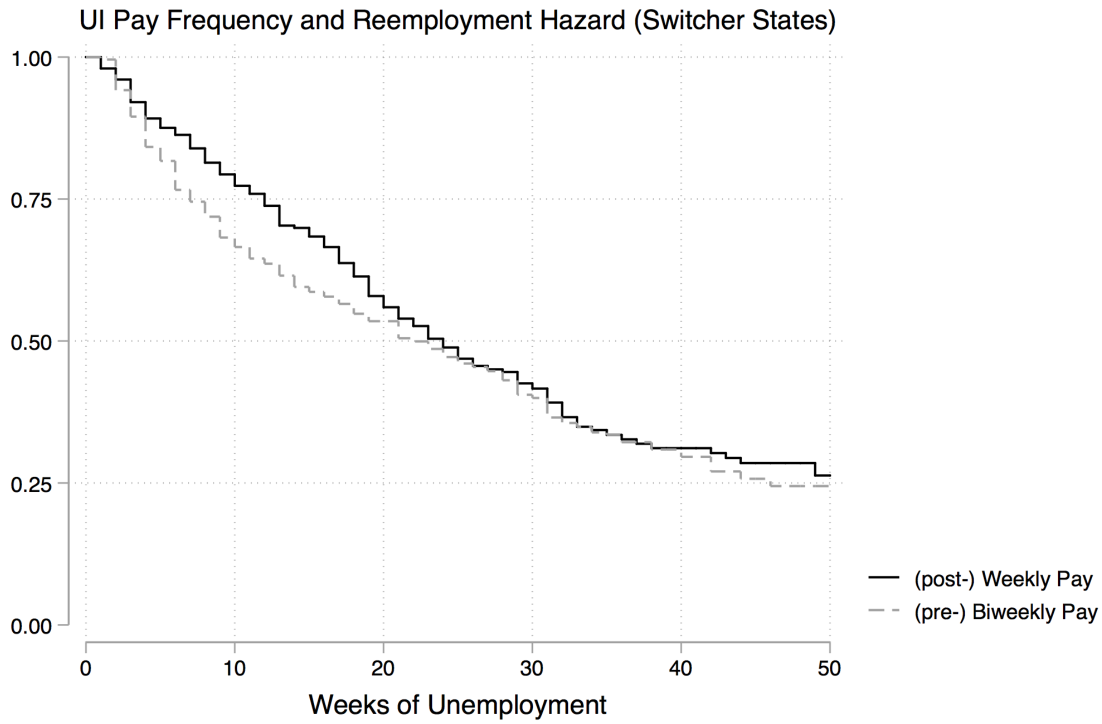


Figure 2: Survival Curves - Comparing biweekly and weekly Pay Frequency

Note: Figure shows individual level unemployment duration from SIPP 1985-2007 for Massachusetts, New York and Washington (the switcher states). The vertical axis indicates the fraction of unemployed sample. The dashed line represents the probability of exiting unemployment for UI claimants from the switcher states prior to the change in pay frequency (distribute benefit payment on a weekly basis); the solid line represents the probability of exiting unemployment for UI claimants from the switcher states post the change (distribute benefit payment on a biweekly basis). Following Chetty (2008), these two Kaplan-Meier survival curves adjusts for the seam effect. The unemployment duration is censored at 50 weeks.

The graphic analysis provide some preliminary evidence that suggests the impact of UI benefits on search duration could be affected by the frequency of benefit payment given a similar benefit replacement rate. However, the result from this simple comparison could potentially be driven by individual or state specific characteristics. To complement the graphic analysis, I run a set of estimations using semi-parametric Cox proportional hazard model (Eq.(2.1)) that includes a rich set of controls. Findings from the regression analysis are consistent with the graphic analysis.

	(1)	(2)	(3)
$\mathbb{1}\{\text{WeeklyPay}\}$ (β_1)	-0.105	-0.116	-0.255
	(0.048)	(0.075)	(0.063)
log(WBA)	-	-0.621	-0.493
		(0.104)	(0.150)
State FE, year FE	×	×	×
Industry FE, occupation FE and seam dummy	×	×	×
Education, marriage and age		×	×
10-piece pre-ue annual wage spline		×	×
State log unemployment rate (at layoff time)		×	×
Pre-unemployment log total wealth		×	
Pre-unemployment log net wealth			×
# Spells	3,383	3,176	1,904

Table 2: Impact of switching from biweekly to weekly pay frequency on UI claimants' reemployment hazard

Note: All columns report result from semi-parametric Cox proportional hazard model from estimating equation (2.1). The key coefficient (β_1) is the change in hazard rate with respect to pay frequency policy changes. Data are individual-level unemployment spells from 1985-2007 SIPP. I include state fixed effects, year fixed effects, industry and occupation fixed effects, a 10-piece linear spline of the pre-unemployment annual wage, pre-/ post-unemployment total wealth, onseam indicator and other individual specific controls such as education and marital status. Standard errors clustered by state are in parentheses.

The main results are presented in Table 2. The reported estimates are hazard coefficients. In column 1, I estimated effect of pay frequency change without controlling for state or individual observable characteristics. In column 2, I include the full sets of controls and restrict my sample to those with pre-unemployment total wealth only. In Column 3, I replace pre-unemployment total wealth with pre-unemployment net wealth. This further reduces the number of samples. Column 3 is my preferred specification as it represents the estimation of Eq. (2.1) using the most stringent set of controls. The key coefficient of interest is the $WeeklyPay$ dummy that varies over time. Under all columns, the estimated hazard coefficient β_1 is negative and significant. In particular, $\beta_1 = -0.255$ (SE 0.063) indicates that switching from biweekly to weekly pay frequency leads to a decrease in the likelihood of exiting unemployment spell by 22% for an average UI claimant.[21]

[21] The percentage change in hazard rate (caused by the change from biweekly to weekly pay frequency) is computed using $exp(\beta_1) - 1$.

Result 1. *For Massachusetts, New York and Washington, switching from biweekly to weekly pay frequency leads to a 10% to 22% decrease in the reemployment hazard. This roughly translates to 2 to 4 weeks of additional unemployment for average UI claimants with mean spell length equal to 18 weeks.*

This result provides a first evidence on the important role of pay frequency in designing UI policy. However, there still exists concerns with both internal and external validity of the estimation. First, states have implemented other concurrent reforms (such as filing technology change) could bias the true causal effect. Second, the treated and control states might be on different outcome trends prior to the treatment. Third, the baseline estimation relies on policy variations from three states only, the results might not be generalized to states with very different demographic or socio-economic characteristics. In response to these concerns, I further assess the validity of the baseline two-way fixed effect research design in Section 5. Overall, results from additional analyses are suggests the benchmark estimation is robust.

3. Mechanism: Pay Frequency and Monthly Benefit Shocks

This section proposes a potential mechanism that could rationalize the pay frequency effect – the frequency of the end-of-the-month extra benefit checks. Section 3.1 introduces the institutional settings of extra benefit checks under the UI system. Sections 3.2 and 3.3 presents the empirical strategy, data and sample. Section 3.4 presents empirical results.

3.1. Institutional Background - Extra Benefit Checks

Standard UI benefit schedule in the US is evaluated under a weekly basis. Typical benefit schedules consist of a predetermined weekly benefit amount (WBA) not exceeding a potential benefit duration (PBD), based on UI claimants' pre-unemployment earnings during the "base period". As noted by Zhang (2017), under either weekly or biweekly pay schedules, in months with five calendar weeks, UI claimants receive an extra payment check. The amount of extra benefit check is equivalent to 25% (or 50%) of monthly benefits under weekly (or biweekly) pay schedules. Under a six-month unemployment duration, claimants receive two (or one) extra benefit months under weekly (or biweekly) pay. Given that many major expenditures – rent, mortgage payment, utility bills – occurs on a monthly basis, receiving this extra benefit check towards the end of the calendar month could have a significant impact on a UI claimant's liquidity in the following month.[22]

[22]These periodic expenditures are sometimes referred as "consumption commitments".

Figures 3 and 4 show examples of the monthly benefit paths under weekly pay and biweekly pay schedules. I assume a UI claimant is entitled to receive the first UI benefit at the beginning of May 2020; the constant weekly benefit amount of $400 paid on Tuesdays terminates at the end of November 2020 (26 weeks). Under the weekly pay schedule, a UI claimant can experience up to two extra benefit shocks, whereas claimants under the biweekly pay schedule can only experience up to one extra benefit shock. Therefore, an average UI claimant under weekly pay schedule are more likely to have extra cash-on-hand at the end of each month during their unemployment spell.

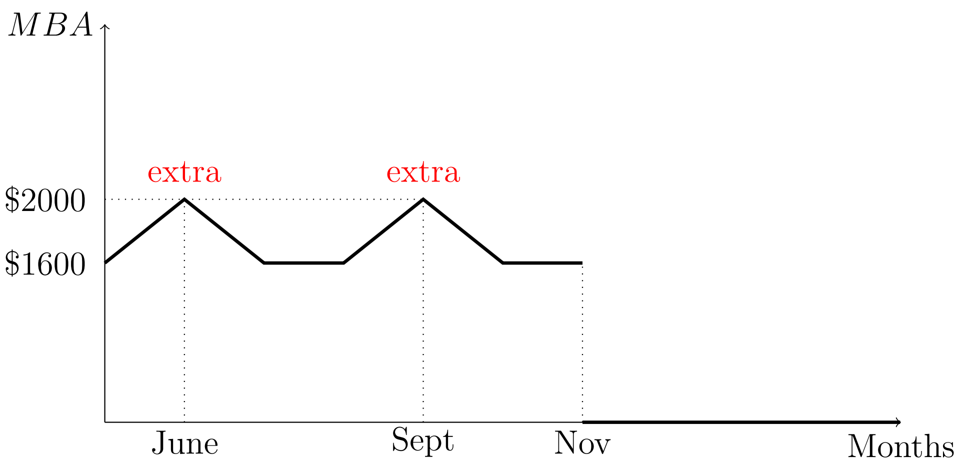
June, 2020

1	**2**	3	4	5	6	7
8	**9**	10	11	12	13	14
15	**16**	17	18	19	20	21
22	**23**	24	25	26	27	28
29	**30**					

Sept, 2020

	1	2	3	4	5	6
7	**8**	9	10	11	12	13
14	**15**	16	17	18	19	20
21	**22**	23	24	25	26	27
28	**29**	30				

(a) Extra Benefit under Weekly Pay



(b) Monthly Benefit Schedule (May to Nov)

Figure 3: Weekly Pay - Extra Benefit Shocks under Monthly Benefit Path

Since there exist variations in both the magnitude and the frequency of extra benefit checks, there should be differential responses by UI claimants to these positive end-of-the-month liquidity shocks among UI claimants under weekly or biweekly pay schedules. In particular, if the effect on unemployment duration exhibit diminishing marginal returns to extra benefit checks, we would expect to observe larger responses under weekly pay schedules. That is, holding the total extra benefit amount constant, the overall duration increases from receiving multiple smaller shocks is expected to be higher than the overall duration increases from receiving a single large shock. On the other hand, since such benefit shocks can be anticipated by forward-looking UI claimants, the impact might be small and insignificant as rational agents should have already internalized this anticipated volatility in their monthly benefit paths.

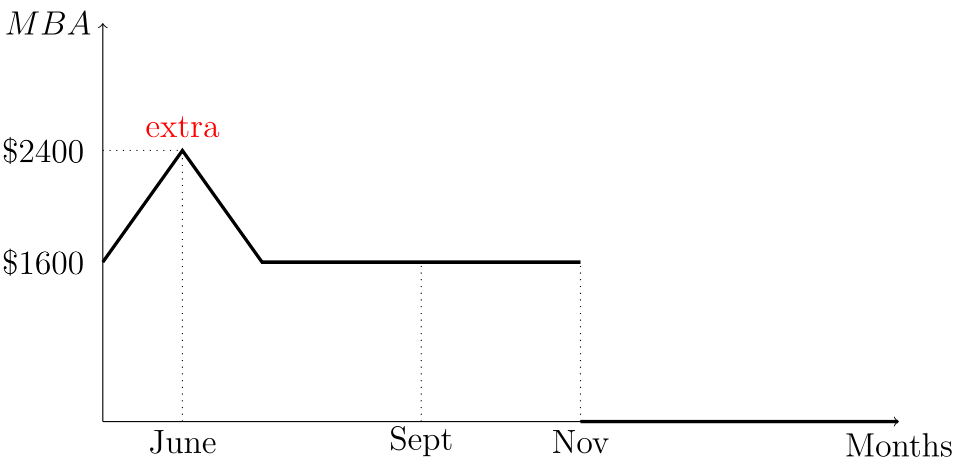
June, 2020

1	**2**	3	4	5	6	7
8	9	10	11	12	13	14
15	**16**	17	18	19	20	21
22	23	24	25	26	27	28
29	**30**					

Sept, 2020

	1	2	3	4	5	6
7	**8**	9	10	11	12	13
14	15	16	17	18	19	20
21	**22**	23	24	25	26	27
28	29	30				

(a) Extra Benefit under Biweekly Pay



(b) Monthly Benefit Schedule (May to Nov)

Figure 4: Biweekly Pay - Extra Benefit Shocks under Monthly Benefit Path

3.2. Empirical Strategy

Next, I analyze the effect of receiving an anticipated end-of-the-month extra benefit check on UI claimants' reemployment hazards in the subsequent month. I exploit quasi-experimental variations in the monthly benefit levels – the variation mainly depends on the claimant's timing of unemployment.

An ideal experiment to study this effect is to compare individuals' reemployment hazards between those who have or have not received the extra benefit checks. However, this comparison requires information on the exact timing of benefit distribution. Due to the SIPP's data limitations, I use the differential probabilities of receiving an extra benefit check in calendar year-months to proxy for UI claimants' the treatment status. In particular, there are months in calendar years where it is never possible to receive extra benefits – as illustrated in Table A2, these months vary from year-to-year. Under this setup, a UI claimant is *not treated* in the t^{th} month of unemployment if the probability of receiving an extra benefit check = 0. Similarly, a UI claimant is (possibly) *treated* in the t^{th} month of unemployment if the probability of receiving an extra benefit check > 0.[23]

To examine whether extra benefit checks affects UI claimants' search behavior in the subsequent calendar month, I estimate a series of Cox proportional hazard models with the following specification:

$$\log h_{ist} = \alpha_t + \beta_1 \mathbf{1}\{PosExtra_{is,t-1}\} + \mathbf{X}_{ist} \tag{3.1}$$

where h_{ist} is the hazard rate of exiting unemployment for individual i from state s at time t. α_t is the flexible non-parametric baseline hazard rate at the given week t conditional on surviving. $\mathbf{1}\{PosExtra_{is,t-1}\}$ is a dummy indicates the status of receiving extra benefit checks from the previous month. Specifically, $\mathbf{1}\{PosExtra_{is,t-1}\} = 1$ if the probably of having received the extra check is > 0, and $\mathbf{1}\{PosExtra_{is,t-1}\} = 0$ if the probably of having received the extra check $= 0$. $\mathbf{X_{ist}}$ is a set of controls: (1) state level controls that include start-of-the-spell monthly unemployment rate and UI generosity; (2) Industry, occupation fixed effect and (3) individual specific controls such as 10-piece log-linear spline for the claimant's pre-unemployment wage, total wealth, age, education, marital status and being on the seam between interviews to adjust for the seam effect. Lastly, $\mathbf{X_{ist}}$ also includes (4) year fixed effects that capture changes over time that vary uniformly across states, (5) state fixed effects that capture time invariant cross state differences. Standard errors are clustered at the state level and (6) calendar month fixed effects that capture the seasonal patterns of reemployment hazard.

In addition to Eq (3.1), I also interacts $\mathbf{1}\{PosExtra_{t-1}\}$ with $\mathbf{1}\{WeeklyPay\}$ in a separate regression. Eq (3.2) allows me to examine the differential extra benefit effects for states before and after they switched from biweekly to weekly pay frequency. If the extra benefit attributes to the observed pay frequency effect, we expect to see a positive and significant estimates for the interaction term.

[23] Given a normal processing and filing time, most UI checks are likely to be distributed towards the end of each week. I use the possibility of receiving extra benefit checks on Wednesday, Thursday and Friday as an alliterative categorization. The results are qualitatively equivalent.

$$\log h_{ist} = \alpha_t + \beta_1 \mathbf{1}\{PosExtra_{is,t-1}\} + \beta_2 \mathbf{1}\{WeeklyPay_s\} + \beta_3 \mathbf{1}\{WeeklyPay_s\} \times \mathbf{1}\{PosExtra_{is,t-1}\} + \mathbf{X}_{ist} \quad (3.2)$$

3.3. Data and Sample

The sample is identical to Section 2. The only difference is that I use monthly (instead of weekly) unemployment status because the variations occur at a monthly basis.

3.4. Empirical Result

	(1) Pooled	(2) Weekly Pay	(3) Biweekly Pay	(4) Interaction
$\mathbb{1}\{\text{PosExtra}_{t-1}\}$ (β_1)	-0.277	-0.428	-0.179	-0.178
	(0.095)	(0.152)	(0.144)	(0.119)
$\mathbb{1}\{\text{WeeklyPay}\}$ (β_2)	-	-	-	-0.112
				(0.079)
$\mathbb{1}\{\text{WeeklyPay}\}$ x $\mathbb{1}\{\text{PosExtra}_{t-1}\}$ (β_3)	-	-	-	-0.182
				(0.094)
log(WBA)	x	x	x	x
State FE, year FE, month FE	x	x	x	x
Industry FE, occupation FE and seam dummy	x	x	x	x
Education, marriage and Age	x	x	x	x
10 piece pre-ue annual wage spline	x	x	x	x
pre-ue net wealth	x	x	x	x
State log unemployment rate (at layoff time)	x	x	x	x
# Spells	1,800	650	1,135	1,680

Table 3: Impact of receiving extra benefit checks on UI claimants' reemployment hazard

Note: All columns report result from semi-parametric Cox proportional hazard model from estimating Eq. (3.1) and (3.2). Data are individual-level unemployment spells from 1985-2007 SIPP. I include state fixed effects, year fixed effects, calendar month fixed effects, industry and occupation fixed effects, a 10-piece linear spline of the pre-unemployment annual wage, pre-unemployment net wealth, onseam indicator and other individual specific controls such as education and marital status. Standard errors clustered by state are in parentheses. I restrict my sample to those who stay unemployed for at least 1 month.

The estimated results are presented in Table 3. The reported estimates are hazard coefficient. For columns (1)–(3), the key coefficient of interest β_1 is negative under both the pooled sample and the two sub-samples. In particular, possibly receiving an extra check in the previous month is estimated to reduce this claimant's reemployment hazard by 23%.

Under the cross sectional comparison (columns (2) and (3)), I find the point estimate is more than two times larger for weekly pay states. In column (4), when I interact pay frequency policy change dummy with the extra benefit dummy, I find that the previously documented pay frequency effect mainly operates through the end-of-the-month extra benefit channel: the effect of receiving extra benefits is significantly stronger after states switched from biweekly to weekly pay!

I note that the magnitude of the extra benefit check is equivalent to a 25% (or 50%) increase in monthly benefit amount under the weekly (or biweekly) pay schedule. UI claimants under weekly pay schedules are twice as likely to experience a positive benefit shock during unemployment. This finding suggests that the frequency of benefit shocks plays a more important role in improving UI claimants' capability to smooth consumption during unemployment.

Result 2. *Possibly receiving an extra benefit check could reduces UI claimants' reemployment hazards for the subsequent month. The estimated effect is larger under weekly pay states.*

The documented larger responses to the anticipated end-of-the-month positive benefit shocks under weekly pay suggest the consumption smoothing gains from receiving extra benefit checks exhibit diminishing returns. That is, holding the total extra benefit amount constant, the effect of receiving multiple smaller benefit on UI claimants' reemployment hazards is estimated to be larger than the effect of receiving a single large shock.

Given that the extra check months occur more frequently under the weekly pay schedule, the results provide some support for the existence of the pay frequency effect. In particular, the weekly pay schedule mechanically leads to more occurrences of extra benefit checks in UI claimants' monthly benefit paths. Relative to the biweekly pay schedule, the greater likelihood of having income shocks aligned with end-of-the-month major expenditures under the weekly pay schedule could significantly increase their cash on hand for the subsequent months. Therefore, holding the weekly benefit and pre-unemployment wealth constant, UI claimants under the weekly pay schedule are more able to smooth consumption during unemployment. As a result, switching from biweekly to weekly pay leads to longer unemployment durations, because UI claimants can afford to wait longer after a switch in pay frequency.

Table 4 presents related results from heterogeneity analysis. In particular, I separately estimated the hazard coefficient for different sub-samples. Overall, I find the extra benefit effect is mainly driven by UI claimants who are not liquidity constrained. In particular, the point estimate is large and statistically significant for UI claimants: (1) with above median pre-unemployment total wealth; (2) married with working spouses and (3) who are homeowners with or without mortgage payments. The result could be interpreted in several ways. First, there might exist some threshold level of extra benefit amount for claimants to become responsive to it; Second, unconstrained households might have stronger consumption commitments and response more intensively to extra benefit shocks; Third, UI claimants who were not liquidity constrained prior to unemployment might be less capable to smooth consumption during unemployment and exhibit higher sensitivity to extra benefit shocks. Due to data limitation, I am not able to tease out these explanations.

	(1) Below Median pre-ue Wealth	(2) Above Median pre-ue Wealth	(3) Spouse Working	(4) Spouse Not Working	(5) Single	(6) Renter	(7) Homeowner w/ Mortgage	(8) Homeowner w/o Mortgage
$\mathbb{1}\{\text{PosExtra}_{t-1}\}(\beta_1)$	-0.055	-0.313	-0.345	-0.044	-0.011	-0.111	-0.225	-0.235
	(0.132)	(0.112)	(0.134)	(0.109)	(0.106)	(0.136)	(0.109)	(0.219)
log(WBA)	x	x	x	x	x	x	x	x
State FE, year FE, month FE	x	x	x	x	x	x	x	x
Education, marriage and Age	x	x	x	x	x	x	x	x
Industry FE, occupation FE and seam dummy	x	x	x	x	x	x	x	x
10 piece pre-ue annual wage spline	x	x	x	x	x	x	x	x
pre-ue total wealth	x	x	x	x	x	x	x	x
State log unemployment rate (at layoff time)	x	x	x	x	x	x	x	x
# Spells	1,601	1,823	1,357	2,067	1,283	1,172	1,766	486

Table 4: Extra benefit checks and reemployment hazard, heterogeneity analysis

Note: All columns report result from semi-parametric Cox proportional hazard model from estimating variants of Eq. (3.1). Data are individual-level unemployment spells from 1985-2007 SIPP. Standard errors clustered by state are in parentheses. I restrict my sample to those who stay unemployed for at least 1 month.

Result 3. *Responses to extra benefit shocks seems to be driven by liquidity unconstrained UI claimants.*

One potential concern with the liquidity-effect based explanation is that UI claimants who anticipates an extra benefit check might *intentionally* delay search effort and capture this "additional" benefit. That is, the response to extra benefit checks might be a result of the incentive-distorting moral hazard effect. However, I argue that moral hazard might be less of a concern. Consider the following example: suppose that a UI claimant receives benefit check every week, and the potential job also makes salary payments every week. Whether this month has five or four paychecks makes absolutely no different to this UI claimant's job finding incentives, because this claimant would get a fifth check whether she find a job or not. This suggests that the potential distortion from moral hazard effect would be quite small.

4. Policy Implications

Findings from the previous sections have shown that switching from biweekly to weekly pay leads to longer unemployment duration as it decreases UI claimants' job finding hazards. In addition, I find evidence suggests that such effect is likely a result of improved consumption smoothing capabilities for UI claimants. In this section, I investigate two policy relevant implications from switching benefit pay frequencies. Specifically, Section 4.1 estimates the impact of varying benefit pay frequency on UI administrative costs and UI Take-up. Section 4.2 studies the interactions between benefit pay frequency and increases in benefit amount (WBA). Overall, I find variations in benefit pay frequency: (1) does not have significant effect on states' UI administrative cost or take-up, (2) and potentially crowds-out the consumption smoothing gains from increases in WBA.

4.1. Impact on UI Administrative Costs and Take-up

Switching to a more frequent certification frequency might occur additional administrative processing cost for state governments, as the weekly volume of benefit certification are likely to be doubled. On the other hand, such variation could also affect the certification cost for UI claimants. To examine these potential policy impacts, I follow Ebenstein and Stange (2010) and estimate a series of two-way FE regression:

$$y_{s,t} = \alpha_0 + \beta_1 \mathbf{1}\{WeeklyPay_{s,t}\} + \beta_2 \mathbf{1}\{PostPhone_{s,t}\} + \beta_2 \mathbf{1}\{PostNet_{s,t}\} + \mathbf{X}_{st} \qquad (4.1)$$

where $y_{s,t}$ is the outcome of interest: {log of Full-Time Employment, log of Payroll, fraction of Employment Part-Time, UI Take-up rate} for state s and calendar year t. $WeeklyPay$ is a dummy that varies with state and calendar year t. $PostPhone$ and $PostNet$ are dummies that indicates whether this state implemented phone or internet claiming for continued certifications.[24] In my sample period, all switcher states changed filing frequency at the same year they adopted phone claiming. Lastly, $X_{i,t}$ includes state FEs, year FEs, max. WBA and state unemployment rate.

Data used in this section are drawn from two separate sources. The UI administrative employment and payroll information is obtained from the Annual Survey of Government Employment and Payroll (ASGEP) dataset from 1992-2007. The data contains state level

[24] The policy event time for technology adoptions are obtained from the BAM survey.

annual expenditure and employment information under the "Social Insurance Administration" item. The UI take-up rate is obtained from the 1985-2007 SIPP data. I first use unemployed individuals' pre-unemployment annual wages to predict their UI eligibility. Then I compute the the UI take-up rate using the number of UI takers divided by the number of UI eligible individuals for each given state year.[25]

	(1) Log(FTE)	(2) log(Payroll)	(3) Frac. PTE	(4) UI Take-Up
$\mathbb{1}\{\text{WeeklyPay}\}$ (β_1)	-0.063	-0.039	0.004	-0.004
	(0.078)	(0.085)	(0.015)	(0.023)
$\mathbb{1}\{\text{PostPhone}\}$ (β_2)	-0.027	-0.044	-0.012	-0.010
	(0.059)	(0.058)	(0.013)	(0.015)
$\mathbb{1}\{\text{PostNet}\}$ (β_3)	0.019	-0.002	-0.007	-0.004
	(0.072)	(0.075)	(0.013)	(0.021)
State FE, year FE	x	x	x	x
State unemployment rate	x	x	x	x
State Max WBA	x	x	x	x
Pre-ue wage, wealth, education, age, marriage	-	-	-	x
Industry FE, Occupation FE	-	-	-	x
Data source		ASGEP 92-07		SIPP 85-07, UI eligible
Observation level		(state x year) 610		(individual) 15,580

Table 5: Benefit pay frequency, UI administrative cost and UI take-up

Note: All columns report result from linear regression model from estimating Eq. (4.1). Columns (1)-(3) are estimated using state x year observations from 1992-2007 ASGEP dataset. Column (4) is estimated using individual-level unemployment spells from 1985-2007 SIPP. I excluded NV and OH due to their non-standard certification frequencies. Standard errors clustered by state are in parentheses.

Table 5 presents results for UI administration costs and UI take-up rates. Columns (1), (2) and (3) are estimated using ASGEP data. The point estimate for β_1 are all negative and close to zero indicating switching to weekly benefits does not seem to have significant impacts on switcher states' employment costs (even after controlling for filing technology). Further, as illustrated from column (3), the insignificant response in employment cost are not driven by increases in part-time employment. Column (4) uses the 1985-2007 SIPP UI eligible unemployed sample to examine the effect on UI take-up rates. The point estimate for β_1 is small and close to zero.

Result 4. *Switching to weekly pay does not seem to affect state's UI administrative cost. In addition, the potential change in the continued certification costs due to pay frequency variation does not seem to affect UI take-up rate.*

[25] I use UI calculator from Kuka (2020) to estimate unemployed workers' UI eligibility.

4.2. Interaction with Variations in WBA

Does switch to week pay *crowd-out* the consumption smoothing gains from increases in UI benefits? This section investigates the interaction between benefit pay frequency and increases in UI benefit amount. In particular, I separately estimates UI claimants' responses to UI benefit increase under different benefit pay frequencies.

	Weekly Pay		Biweekly Pay	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Duration	17.64	13.56	18.16	13.80
Average UI benefit amount ($)	163.36	32.11	174.74	26.40
Maximum UI benefit amount ($)	219.00	59.05	245.91	48.04
Predicted UI benefit ($)	183.34	75.61	186.60	83.78
Simulated replacement rate	0.52	0.04	0.51	0.06
Age	38.59	11.39	38.76	11.31
Years of Education	12.36	2.64	12.28	2.96
Married	0.60	0.49	0.63	0.48
Pre-ue annual wage ($)	20226.46	14017.69	21823.23	17111.16
Pre-ue liquid wealth ($)	32216.64	97894.54	31791.09	88986.34
Pre-ue unsecured debt ($)	5171.25	20627.17	4817.604	16594.31
Pre-ue home equity ($)	31129.15	55169.38	37149.59	57904.9
# Spells	1,334		2,279	

Table 6: Descriptive Statistics for UI recipients, SIPP 1985-2007

Note: The data presented are individual level unemployment spells from 1985-2007 SIPP data. The average and maximum UI weekly benefit amount are obtained from the US Department of Labor. All dollar values are converted into 1990 values. The sample is restricted to male UI claimants only. The pay frequency policy information are collected from archived state websites via *archive.org* and the BAM survey.

The empirical strategy follows earlier literature (Meyer, 1990, Krueger and Meyer, 2002, Chetty, 2008) that exploits state and year variation in the maximum weekly benefit amount. The treatment group is UI claimants with higher earnings that are likely to be affected by the increase in the Max WBA. The control group is UI claimants with lower earnings that are not going to be affected by the change in the Max WBA. The identification assumption requires the two groups to follow parallel trends over time in absence of the Max WBA changes within sub-samples.

The baseline Cox proportional hazard model closely follows Chetty (2008) and Kroft and Notowidigdo (2016):

$$\log h_{it} = \alpha_t + \beta_1 \log b_i + \beta_2 (t \times \log b_i) + \mathbf{X_{it}} \tag{4.2}$$

where h_{it} is the hazard rate of exiting unemployment for individual i at time t. α_t is the flexible non-parametric baseline hazard rate at the given week t conditional on surviving. b_i is the weekly benefit amount that this individual receives. The coefficient β_1 is the elasticity of the hazard rate with respect to UI benefits at $t = 0$. The inclusion of $(t \times \log b_i)$ allows the effect of benefit varying with duration. $\mathbf{X_{it}}$ controls for state and year fixed effect for the purpose of the difference-in-difference design. In addition, $\mathbf{X_{it}}$ also includes for occupation and industry dummies; 10 piece log wage spline for claimant's pre-unemployment wage; log total wealth and other individual specific linear controls (education, age, marital status and being on the seam week between interviews).

I use the identical sample as in Section 2. Table 6 provides a descriptive summary for the subsamples divided into weekly and biweekly pay frequencies. Although UI claimants from biweekly states have longer unemployment duration, receive higher WBA on average and have higher pre-unemployment annual wage, the State-Year level UI generosity measured by simulated replacement rate or predicted benefit amount appears to be similar across the two subsamples.

The main results are reported in Table 7. I report estimations of duration elasticity from the pooled sample, as well as two sub-samples. The variable of interest is duration elasticity – UI claimants' likelihood of find a job at the first week of unemployment in response to increases in UI benefit. I control for state fixed effects and year fixed effects, industry and occupation fixed effects, 10-piece linear spline of pre-unemployment annual wage earnings, pre-unemployment total wealth and other individual specific demographics.

	(1)	(2)	(3)
	Pooled	Weekly Pay	Biweekly Pay
log(WBA)	-0.399	-0.365	-0.509
	(0.180)	(0.323)	(0.025)
State FE, year FE	x	x	x
Industry FE, occupation FE and seam dummy	x	x	x
Education, Marriage, Age	x	x	x
10 piece pre-ue annual wage spline	x	x	x
Pre-ue net wealth	x	x	x
State unemployment rate (at layoff time)	x	x	x
# Spells	1,904	742	1,294

Table 7: Duration Elasticity, by Pay Frequency

Note: All columns report semi-parametric Cox proportional hazard model results from estimating equation (4.2). The reported coefficients are elasticities of hazard rate with respect to UI benefits. Data are individual-level unemployment spells from 1985-2007 SIPP. Standard errors clustered by state are in parentheses.

The results columns (2) to (3) suggests that UI claimants' behavioral responses are slightly stronger under biweekly states compared to the point estimate under the pooled sample. For the subsamples, a 10% increase in benefit is estimated to reduce reemployment hazard by 4% (or 3%) under a Biweekly (or Weekly) pay frequency. Although potentially due to smaller sample size, the point estimate for the Weekly pay sub-sample is statistically insignificant.

Result 5. *Given an identical % increase in UI benefit, UI claimants under the Biweekly pay frequency seems to be more responsive to it, though the difference in estimated hazard elasticity are not statistically significant.*

According to the traditional view (Moffitt, 1985, Meyer, 1990), the difference in the estimated duration elasticities are driven purely by the differences in moral hazard, i.e. the degree of incentive distortion is larger under weekly pay frequencies. Chetty (2008), however, might interpret the observed heterogeneous behavioral responses to changes in UI benefits as differences in the liquidity effect, i.e. UI claimants under weekly pay are more able to smooth consumption during unemployment. Given that the present discounted benefit levels are almost identical under the two pay frequencies, moral hazard is unlikely to be the main driver of this observed difference in duration elasticities.[26] Therefore, I conclude that

[26]Formally, since the government cannot observe agents' search effort (e), moral hazard occurs when agents only consider the private marginal product of work – wage minus benefit (w-b) – and their private costs when choosing search effort. Note that the private marginal product of work is lower than the social marginal product of work (w). Thus, increases in benefit (b) distorts search incentive.

there exists some degree of *substitutability* between the weekly pay schedule and increases in UI benefits. In particular, given that weekly payments potentially improves UI claimants capability to consumption-smoothing, the additional liquidity gains from increases in WBA are less helpful for UI claimants under the weekly pay schemes.

5. Robustness

In this section, I include a series of robustness checks for the estimation of pay frequency effect. First, to eliminate the potential contamination effects from the variations in UI filing technologies, I use a two-way fixed effect framework to examine the impact of filing technology on UI take-up and UI claimants' reemployment hazards independent from changes in pay frequency. Second, I use a event-study framework to visually examine the validity of the parallel-trends assumption for the two-way fixed effect design. Third, I ran a series of permutation tests to compare the baseline estimation to 1,000 randomly generated benchmarks.[27] The main result survives under all these tests.

5.1. Variations in the Continued Filing Method

One concern with the baseline empirical strategy presented in section 2 is that the effect can be confounded by other concurrent policy changes. For all three switcher states in my sample, the pay frequency variation is accompanied by the adoption of telephone filing technology for continuing claims. In this subsection, I restrict my sample to states that only varied continued filing methods to estimate its impact on UI take up and UI claimants' reemployment hazards. The results suggest that the estimated pay frequency effect is likely not driven by variations in continued filing method.

I use the UI claims filing method data collected by the US Department of Labor from 1985 to 2007. The claim filing data is originally collected from the BAM (Benefit Accuracy Measurement) program. The BAM samples around surveys 400 UI claimants in each state-year level. The data contains claiming method information for both initial and continued claims. For the purpose of this project, I restrict my attention to variations in continued claim methods.[28] To date policy changes, I follow Ebenstein and Stange (2010) to look for the sharp changes in claim method usage. In particular, for each state, I infer the time of

[27]In addition to these checks, I adopt a Synthetic Control Method (SCM) to improve the comparability between switcher and non-switcher states prior to the policy change. The SCM results are discussed in detail in Appendix B.

[28]Ebenstein and Stange (2010) use the same BAM dataset to examine the impact of initial claim methods on UI take-up and find no effect.

filing technology change to be the year that the share of claims filed via telephone or internet increased by the greatest amount.

The empirical approach exploit the state-year level variations in UI continued filing methods. I examine the pay frequency effect after the inclusion of technology adoption dummies: $\mathbf{1}\{PostPhone_s\}$ and $\mathbf{1}\{PostNet_s\}$. I use Cox hazard models to estimate the pay frequency effect after accounting for technology adoption changes (Eq. (5.1)). The Cox hazard estimation uses the identical sample as in Section 2. Note that the left-hand side variable $\log h_{ist}$ represents the reemployment hazard for individual i from state s at t^{th} week of unemployment.

$$\log h_{ist} = \alpha_t + \beta_1 \mathbf{1}\{WeeklyPays\} + \beta_2 \mathbf{1}\{PostPhone_s\} + \beta_3 \mathbf{1}\{PostNet_s\} + \mathbf{X}_{ist} \tag{5.1}$$

Results are presented in Table 8. Due to small sample size, the standard errors are quite large. In both cases, I cannot reject the effect of technology adoption is different from zero for states that did not change pay frequencies. This result complements Ashenfelter, Ashmore and Deschênes (2005), who finds increases in continued filing costs – increases in monitoring of job searches – has no effect on the duration of continued claims.

	(1) Baseline	(2) w/ Tech.
$\mathbb{1}\{WeeklyPay\}$ (β_1)	-0.255	-0.232
	(0.063)	(0.082)
$\mathbb{1}\{PostPhone\}$ (β_2)	-	-0.056
		(0.161)
$\mathbb{1}\{PostNet\}$ (β_3)	-	0.070
		(0.126)
log(WBA)	×	×
State FE, year FE	×	×
Education, marriage and age	×	×
Industry FE, occupation FE and seam dummy	×	×
10-piece pre-ue annual wage spline	×	×
pre-ue net wealth	×	×
State log unemployment rate (at layoff time)	×	×
# Spells	1,904	1,904

Table 8: Impact of continued filing technology on UI claimants' reemployment hazard

Note: Columns (1) presents the baseline estimation of the pay frequency effect. Column (2) report result from the Cox proportional hazard model (5.1). Data are from 1985-2007 SIPP. All switcher states in my sample changed pay frequency at the same year as they adopted telephone filing. Standard errors clustered by state are in parentheses.

Result 6. *The pay frequency effect is not likely to be driven by technology adoption.*

5.2. A event study framework

To examine the validity of parallel assumption, I estimate event-study models with leading and lagging treatment dummies, so we can assess the pre-treatment time trends in the hazard coefficient in the following specification:

$$\log h_{ist} = \alpha_t + \sum_{k=-4, k\neq -1}^{4} \delta_k D_s^k + \mathbf{X}_{ist} \tag{5.2}$$

where α_t and $\mathbf{X}_{ist}$ are defined as they were in Eq. (2.1). D_s^k is a dummy variable that equals to 1 after state s changed from biweekly to weekly pay. The endpoints are set to address imbalances in the sample. The endpoints are binned so that $D_s^4 = 1\{event\ time \geq 4\}$ and $D_s^{-4} = 1\{event\ time \leq -4\}$.

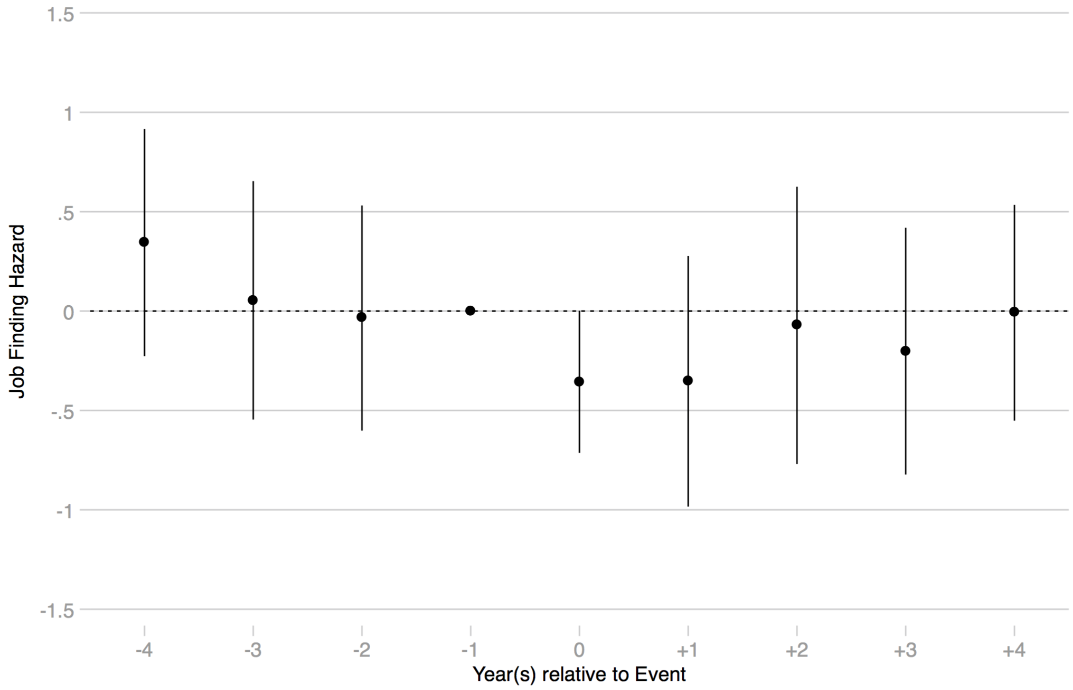


Figure 5: Dynamic effects of weekly pay on UI claimants' job finding hazards

Figure 5 presents the event study plot on the dynamic effects of weekly pay frequency on UI claimants' job finding hazards. I fail to reject the null of having pre-treatment trends. Interestingly, the treatment effect seems to be concentrated in the first two years after the pay frequency switch, suggesting the baseline two-way fixed effect estimates is main driven by short term responses. Overall, the 95% confidence intervals are quite large, potentially due to the small sample size in the SIPP data.

Result 7. *There seems to be no presence of pre-treatment trends. The pay frequency effect seems to be driven by short term responses.*

5.3. Permutation Test

In this section, I implement a non-parametric permutation tests for the purpose of randomization inference: Comparing to a large number of possible random assignments, is my baseline result significantly different from them? How different is it?[29]

[29] I also implement a similar permutation test for the estimated extra benefit effect, see Appendix **??** for more detail.

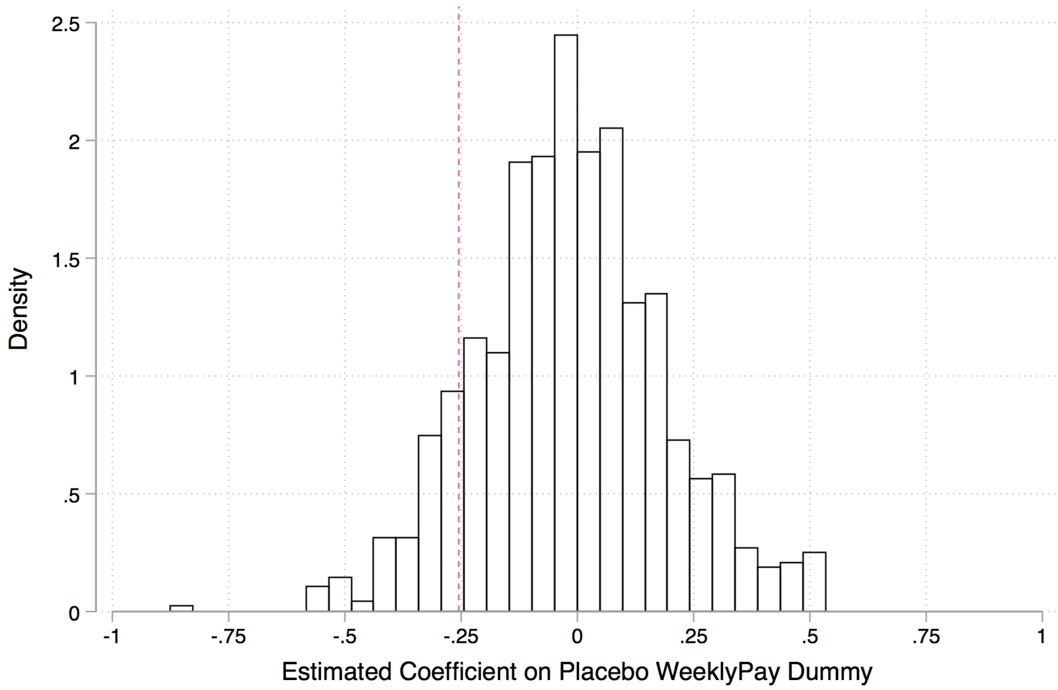


Figure 6: Permutation test for inference of baseline estimation: pay frequency effect

Note: Figures shows the empirical distribution of estimated placebo treatment effects from 1,000 random assignments. Dashed line is the actual treatment effect estimated from Table 2 Column (3). p-value under the permutation test is 0.121

I randomly assign the 3 switcher states in the sample with the event years following the actual pay frequency policy implantation timetable, i.e. 1 state in 1993, 1 state in 1996 and 1 state in 2003.[30] Following random treatment assignments, I re-estimate the placebo pay frequency effect following the baseline specification (Table 2, Column (3)). Then I repeat this process for 1,000 times to obtain a distribution of estimated coefficients. The p-value in this context is defined as the probability that the baseline estimate is obtained purely by chance and is computed by the following expression:

$$p\text{-}value = \frac{\sum_{i=1}^{1000} \mathbb{1}|\beta^{i}_{baseline} \geq \beta_{placebo}|}{1000}$$

Figure C1 plots the empirical distribution of the placebo estimates using 1,000 random treatment assignments. The dashed line is the point estimate from the baseline estimation

[30]Note that the assignment only applies to states that started with biweekly pay frequency at the beginning of the study. See Table A1 for the list of qualified states.

($\beta = -0.255$). Comparing to the estimated placebo treatment effects, the actual effect is not significant (p-value = 0.121). This is potentially driven by the small sample size or the unbalanced dataset. The result from permutation test provides a conservative p-value for the baseline estimation.

6. Conclusion

There is a large literature that studies the impact from receiving unemployment insurance on job search behavior (Krueger and Meyer, 2002, Schmieder and Von Wachter, 2016). Many of these paper evaluated the effects of benefit generosity. However, there has not been many research conducted on the non-monetary aspects of UI policy – in the case of benefit pay frequency and the timing of benefits, there was none.[31]

This paper uses data from the 1985-2007 SIPP to investigate the effect of benefit pay frequency on job search behavior by presenting three pieces of empirical evidence. First, utilizing quasi-experimental changes in benefit pay frequency, this paper finds switching to a more frequent weekly pay schedule reduces UI claimants' job finding hazard. Second, using variations in the timing of the extra benefit checks, the paper finds suggestive evidence that the pay frequency effect is partly due to the more frequent occurrences of the end-of-the-month extra benefit checks under the weekly pay schedule. Third, switching from biweekly to weekly pay does not seem to increase states' UI administrative costs, nor UI eligible workers' UI take-up rate.

Furthermore, I investigate the interactions between benefit pay frequency and benefit amount increases using a standard Difference-in-Difference research design (Meyer, 1990, Krueger and Meyer, 2002, Chetty, 2008, Kroft and Notowidigdo, 2016). I separately estimate the effects of benefit increase on unemployment durations for states under weekly or biweekly pay schedules. Results from the additional analyses imply that the magnitude and the significance of the liquidity effect due to increases in UI benefit amount may vary with benefit pay frequency. Overall, findings from this paper highlight the importance of benefit pay frequency and pay timing when evaluating the consumption smoothing benefit from social insurance policies.

There are several limitations to this paper that future research could address. First, although this paper finds suggestive evidence on the linkage between pay frequency and the liquidity effect, the paper does not directly test this due to data limitations. Future research

[31] See O'Leary (2004) for the recent a summary about the effects of changing continued certification requirements.

could make use of the high frequency transaction data to investigate potential differences in consumption, saving and borrowing behavior across weekly and biweekly pay frequencies. Such a study would provide more concrete evidence on the causal relationship between pay frequency and consumption smoothing. For example, Baugh and Correia (2018) use account aggregator data to investigate the borrowing pattern for employed workers across different pay frequencies. Ganong and Noel (2019) uses JPMCI data to study the consumption patterns for UI claimants.

Second, the idea of evaluating the impact of pay frequency and pay timing variation on consumption and other household behaviors can be easily applied to evaluating different social benefit programs. Future studies could expand this research agenda and investigate related questions. For example, (1) Do we observe a similar pay frequency effect in other settings? (2) How big is the welfare gain if the timing and the frequency of social benefits align with individuals' expenditure streams?

References

Aguila, Emma, Arie Kapteyn, and Francisco Perez-Arce. 2017. "Consumption smoothing and frequency of benefit payments of cash transfer programs." *American Economic Review*, 107(5): 430–35.

Anderson, Patricia M, and Bruce D Meyer. 1997. "Unemployment insurance takeup rates and the after-tax value of benefits." *The quarterly journal of economics*, 112(3): 913–937.

Ashenfelter, Orley, David Ashmore, and Olivier Deschênes. 2005. "Do unemployment insurance recipients actively seek work? Evidence from randomized trials in four US States." *Journal of econometrics*, 125(1-2): 53–75.

Baugh, Brian, and Filipe Correia. 2018. "Does Paycheck Frequency Matter for Households' Decisions? Evidence from Financial Account Data." *working paper.*

Baugh, Brian, Jesse B Leary, and Jialan Wang. 2018. "When Is It Hard to Make Ends Meet?" *RRC Paper No. NB1l7-05. Cambridge, MA: National Bureau of Economic Research.*

Beatty, Timothy KM, Marianne P Bitler, Xinzhe Huang Cheng, and Cynthia Van der Werf. 2019. "SNAP and Paycheck Cycles." National Bureau of Economic Research.

Berniell, Inés. 2018. "Pay cycles: Individual and aggregate effects of paycheck frequency." Documento de Trabajo.

Blaustein, Saul J. 1979. *Insured Unemployment Data.* National Commission on Employment and Unemployment Statistics.

Browning, Martin, and Annamaria Lusardi. 1996. "Household saving: Micro theories and micro facts." *Journal of Economic literature*, 34(4): 1797–1855.

Browning, Martin, and Thomas F Crossley. 2001*a*. "The life-cycle model of consumption and saving." *Journal of Economic Perspectives*, 15(3): 3–22.

Browning, Martin, and Thomas F Crossley. 2001*b*. "Unemployment insurance benefit levels and consumption changes." *Journal of public Economics*, 80(1): 1–23.

Card, David, Raj Chetty, and Andrea Weber. 2007. "Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market." *The Quarterly journal of economics*, 122(4): 1511–1560.

Castner, Laura, Juliette Henke, et al. 2011. "Benefit redemption patterns in the supplemental nutrition assistance program." Mathematica Policy Research.

Chetty, Raj. 2008. "Moral hazard versus liquidity and optimal unemployment insurance." *Journal of political Economy*, 116(2): 173–234.

East, Chloe N, and Elira Kuka. 2015. "Reexamining the consumption smoothing benefits of Unemployment Insurance." *Journal of Public Economics*, 132: 32–50.

Ebenstein, Avraham, and Kevin Stange. 2010. "Does inconvenience explain low take-up? Evidence from unemployment insurance." *Journal of Policy Analysis and management*, 29(1): 111–136.

Fishman, Michael E, Mary Farrell, Karen N Gardiner, Burt Barnow, and John Trutko. 2003. "Unemployment Insurance Non-Monetary Policies and Practices: How Do They Affect Program Participation? A Study of 8 States." U.S. Department of Labor Employment and Training Administration.

Friedman, Milton, et al. 1957. "theory of the consumption function."

Ganong, Peter, and Pascal J Noel. 2019. "Consumer spending during unemployment: Positive and normative implications." National Bureau of Economic Research.

Gerard, François, and Joana Naritomi. 2019. "Job displacement insurance and (the lack of) consumption-smoothing." National Bureau of Economic Research.

Gruber, Jonathan. 1997. "The consumption smoothing benefits of unemployment insurance." *The American Economic Review*, 87(1): 192.

Gruber, Jonathan. 2001. "The wealth of the unemployed." *ILR Review*, 55(1): 79–94.

Jappelli, Tullio, and Luigi Pistaferri. 2010. "The consumption response to income changes."

Kaplan, Greg, Giovanni L Violante, and Justin Weidner. 2014. "The wealthy hand-to-mouth." National Bureau of Economic Research.

Kolsrud, Jonas, Camille Landais, Peter Nilsson, and Johannes Spinnewijn. 2018. "The optimal timing of unemployment benefits: Theory and evidence from sweden." *American Economic Review*, 108(4-5): 985–1033.

Kroft, Kory, and Matthew J Notowidigdo. 2016. "Should unemployment insurance vary with the unemployment rate? Theory and evidence." *The Review of Economic Studies*, 83(3): 1092–1124.

Krueger, Alan B, and Bruce D Meyer. 2002. "Labor supply effects of social insurance." *Handbook of public economics*, 4: 2327–2392.

Kuka, Elira. 2020. "Quantifying the benefits of social insurance: Unemployment insurance and health." *Review of Economics and Statistics*, 102(3): 490–505.

LaLumia, Sara. 2013. "The EITC, tax refunds, and unemployment spells." *American Economic Journal: Economic Policy*, 5(2): 188–221.

Leary, Jesse B, and Jialan Wang. 2016. "Liquidity Constraints and Budgeting Mistakes: Evidence from Social Security Recipients." Working Paper.

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

Meyer, BD. 1990. "Unemployment insurance and unemployment spells." *Econometrica*, 58(4): 757–782.

Modigliani, Franco, and Richard Brumberg. 1954. "Utility analysis and the consumption function: An interpretation of cross-section data." *Franco Modigliani*, 1(1): 388–436.

Moffitt, Robert. 1985. "Unemployment insurance and the distribution of unemployment spells." *Journal of Econometrics*, 28(1): 85–101.

Olafsson, Arna, and Michaela Pagel. 2018. "The liquid hand-to-mouth: Evidence from personal finance management software." *The Review of Financial Studies*, 31(11): 4398–4446.

O'Leary, Christopher J. 2004. "UI work search rules and their effects on employment."

Price, Daniel N. 1985. "Unemployment insurance, then and now, 1935-85." *Soc. Sec. Bull.*, 48: 22.

Schmieder, Johannes F, and Till Von Wachter. 2016. "The effects of unemployment insurance benefits: New evidence and interpretation." *Annual Review of Economics*, 8: 547–581.

Schmieder, Johannes F, and Till von Wachter. 2017. "A context-robust measure of the disincentive cost of unemployment insurance." *American Economic Review*, 107(5): 343–48.

Vellekoop, Nathanael. 2018. "Explaining intra-monthly consumption patterns: The timing of income or the timing of consumption commitments?"

Zhang, Yiwei. 2017. "Consumption responses to pay frequency: Evidence from "extra" paychecks." *ACR North American Advances*.

Appendices

Appendix A Additional Tables

States	Start w/	Switch (year)	States	Start w/	Switch (year)
Alabama	weekly	-	Missouri	weekly	-
Arizona	weekly	-	Nebraska	weekly	-
Arkansas	weekly	-	Nevada*	weekly	*
California	biweekly	-	New Hampshire	biweekly	weekly (2003)
Colorado	biweekly	-	New Jersey	biweekly	weekly (2013)
Connecticut	weekly	-	New Mexico	biweekly	weekly (1999)
Delaware	weekly	-	New York	biweekly	weekly (1993)
District of Columbia	biweekly	weekly (2007)	North Carolina	biweekly	weekly (1997)
Florida	biweekly	-	Ohio	weekly	either (2003)
Georgia	weekly	-	Oklahoma	biweekly	either (2004)
Hawaii	weekly	either (2008)	Oregon	biweekly	weekly (1992)
Illinois	weekly	-	Pennsylvania	biweekly	-
Indiana	weekly	-	Rhode Island	biweekly	weekly (1996)
Kansas	weekly	-	South Carolina	weekly	-
Kentucky	biweekly	-	Tennessee	weekly	-
Louisiana	weekly	-	Texas	biweekly	-
Maryland	biweekly	weekly (2013)	Utah	biweekly	weekly (1994)
Massachusetts	biweekly	weekly (2003)	Virginia	biweekly	weekly (1996)
Michigan	biweekly	-	Washington	biweekly	weekly (1996)
Minnesota	biweekly	weekly (2008)	West Virginia	biweekly	weekly (2014)
Mississippi	weekly	-	Wisconsin	weekly	-

Table A1: State policies on pay frequency, 1985-2016

Note: Data collected from the BAM survey and the archived state government's website via *archive.org*. *Nevada switched from biweekly to weekly in 1994, and switched back to biweekly after 1999.

Year	Month 1	Month 2
1985	Feb	Jun
1986	Feb	Nov
1987	Feb	-
1988	-	-
1989	Feb	Apr
1990	Feb	Sep
1991	Feb	Jun
1992	Feb	-
1993	Feb	-
1994	Feb	-
1995	Feb	-
1996	Jun	-
1997	Feb	Nov
1998	Feb	-
1999	Feb	-
2000	Apr	-
2001	Feb	Sep
2002	Feb	Jun
2003	Feb	Nov
2004	Feb	-
2005	Feb	-
2006	Feb	Apr
2007	Feb	Sep

Table A2: The Timing of No-Extra Check Month From 1985-2007

Note: The table displays the months which it is never possible to have extra benefit checks. From 1985-2007: 74.33% of such months are in February, 10.9% in June, 5.67% in September, 5.43% in November and 3.67% in April. In addition, such months vary from year-to-year.

States	Tel. (year)	Int. (year)	States	Tel. (year)	Int. (year)
Alabama	2002	2007	Missouri	1997	2003
Arizona	2001	2006	Nebraska	2001	2007
Arkansas	2004	2005	Nevada	1999	2003
California	1994	2003	New Hampshire	2000	2003
Colorado	1991	2003	New Jersey	1999	2002
Connecticut	2002	2006	New Mexico	-	2003
Delaware	2002	2010	New York	1999	2003
District of Columbia	2001	2004	North Carolina	2001	2003
Florida	1996	2003	Ohio	1997	2005
Georgia	1990	2011	Oklahoma	2000	2003
Hawaii	2000	2010	Oregon	1994	2004
Illinois	1998	2003	Pennsylvania	1998	2003
Indiana	-	2005	Rhode Island	1997	2003
Kansas	1999	2003	South Carolina	1999	-
Kentucky	2004	2004	Tennessee	2002	2004
Louisiana	2003	2006	Texas	1998	2003
Maryland	1996	2003	Utah	1998	2006
Massachusetts	1996	2013	Virginia	2002	2004
Michigan	2003	2004	Washington	1999	2003
Minnesota	2000	2003	West Virginia	2004	2010
Mississippi	2005	2010	Wisconsin	1994	2003

Table A3: State policies on the adoption of initial claiming technology

Note: Data collected from the BAM survey. Tel. stands for Telephone filing and Int. stands for Internet filing.

States	Tel. (year)	Int. (year)	States	Tel. (year)	Int. (year)
Alabama	1998	2009	Missouri	1995	2003
Arizona	1996	2007	Nebraska	1996	2006
Arkansas	2000	2005	Nevada	1999	2003
California	2005	2012	New Hampshire	2000	2004
Colorado	1998	2006	New Jersey	1997	2009
Connecticut	1996	2006	New Mexico	1999	2006
Delaware	2005	2013	New York	1994	2003
District of Columbia	2004	2005	North Carolina	1997	2003
Florida	1995	2004	Ohio	2003	2005
Georgia	1994	2003	Oklahoma	1996	2008
Hawaii	2001	2011	Oregon	1994	2003
Illinois	1994	2003	Pennsylvania	1996	2003
Indiana	-	2006	Rhode Island	1997	2010
Kansas	1997	2003	South Carolina	1995	-
Kentucky	1997	2004	Tennessee	1994	2004
Louisiana	1996	2003	Texas	1996	2007
Maryland	1996	2003	Utah	1995	2004
Massachusetts	2003	2004	Virginia	1998	2004
Michigan	1996	2010	Washington	1996	2007
Minnesota	1997	2005	West Virginia	2001	2007
Mississippi	2004	2010	Wisconsin	1994	2004

Table A4: State policies on the adoption of continued claiming technology

Note: Data collected from the BAM survey. Tel. stands for Telephone filing and Int. stands for Internet filing. Indiana is a special case where it changed the filing technology from mail filing to internet filing directly. I have therefore excluded Indiana from the analysis.

Appendix B The Effect of Severance Pay under Different UI Pay Frequencies

Severance pay is a lump-sum cash transfer from employers to their employees at the time of layoff. Since such payment does not count against the size of UI benefits, we can interpret the impact from receiving severance pay as a form of liquidity effect. However, as noted in Chetty (2008), severance pay status is not determined at random - its eligibility highly relates to one's job tenure. In fact, most firms have minimum job tenure threshold and the size of severance pay usually increase in job tenure (as a step-function).

To obtain a reasonable estimation of the liquidity effect using severance pay status, it is important to control for UI claimant's job tenure in my sample. Unfortunately, due to the short-panel nature of the SIPP data, over 72% of my sample are left-censored (no information on the exact job starting date). To overcome this limitation, I predict job tenure for each individual using OLS on the Mathematica sample from Chetty (2008). The predictors consists of linear form of pre-unemployment wage, age, education and martial status. I find the results using predicted tenure and the actual tenure are similar under the Mathematica sample. This indicates the predicted tenure is a plausible proxy for the actual tenure in my SIPP 1996-2007 sample.

In the following, I estimate the Cox proportional hazard model regression from Chetty (2008) using sample from SIPP 1996-2007:

$$\log h_{it} = \alpha_t + \beta_1 sev_i + \beta_2 (t \times sev_i) + \mathbf{X_{it}} \tag{B.1}$$

where h_{it} is the hazard rate of exiting unemployment for individual i at time t. α_t is the flexible non-parametric baseline hazard rate at the given week t conditional on surviving. sev_i is an indicator for receiving severance payment at the time of unemployment. $(t \times sev_t)$ allows the effect of severance pay to interact with duration. $\mathbf{X_{it}}$ includes state fixed effects, year fixed effects, industry and occupation fixed effects, state unemployment rate and individual predicted weekly benefit amount and individual demographic controls identical to the previous parts. In addition, since I've argued that tenure plays an important role in determining severance pay eligibility, I control for this by using a 10 piece (predicted) job tenure spline. Assuming that severance pay status is "random" conditional on job tenure, β_1 will identify the liquidity effect on reemployment hazard at the beginning of the unemployment spell.

B.1 Data and Sample

	WEEKLY		BIWEEKLY	
	Severance	No Severance	Severance	No Severance
Unemployment Duration (weeks)	21.99	18.69	24.68	20.33
Age	42.26	39.22	44.18	39.24
Years of education	13.73	12.44	13.70	12.42
$\mathbb{1}$(Married)	0.68	0.57	0.74	0.60
Simulated replacement rate	0.50	0.50	0.50	0.49
State unemployment rate	5.17	5.27	5.30	5.34
Predicted Tenure (weeks)	317.44	289.74	337.93	290.46
Pre-unemployment Annual wage ($)	31,283.78	20,372.10	41,271.39	22,906.31
Liquid Wealth ($)	62,888.79	36,130.25	74,250.97	37,458.53
Unsecured debt ($)	3,954.91	6,211.85	12,541.08	5,301.08
Home Equity ($)	55,725.25	30,362.11	56,873.46	37,487.31
# Spells	78	1,258	104	1,372

Table B1: Descriptive Statistics (Mean) by Pay Frequency and Severance Pay Status, SIPP 1996-2007

Note: The data presented are individual level unemployment spells from 1996-2007 SIPP data. Tenure is predicted using the Mathematica sample from Chetty (2008). All dollar values are converted to 1990 values. The sample restricted to prime age male UI claimants only. The pay frequency policy information are collected from archived state websites via *archive.org*

I use unemployment spell data from SIPP 1996-2007. Starting from the 1996 Panel, SIPP included questions on severance pay recipient status and the amount of severance pay. To facilitate interpretation, I apply the identical sampling restriction as the parts. In particular, I focus on prime age male unemployed worker who (a) report searching for a job, (b) are not on temporary layoff, (c) have at least 3 months of work history in the survey (to compute pre-unemployment earnings), (d) took up UI benefits within the first month of unemployment. Unemployment duration is censored at 50 weeks and all monetary values are in 1990 dollars.

Table B1 provides a descriptive summary for my sample. In this sample, less than 10% of the UI claimants received severance pay - the number is slightly lower compare to the Mathematica sample. Among both "weekly" and "biweekly" states, UI claimants with severance pay at the time of unemployment look significantly different from those without severance pay. In particular, severance pay recipients tend to be older, more educated, more likely to be married, have longer predicted job tenure and higher net wealth.

B.2 Empirical Result

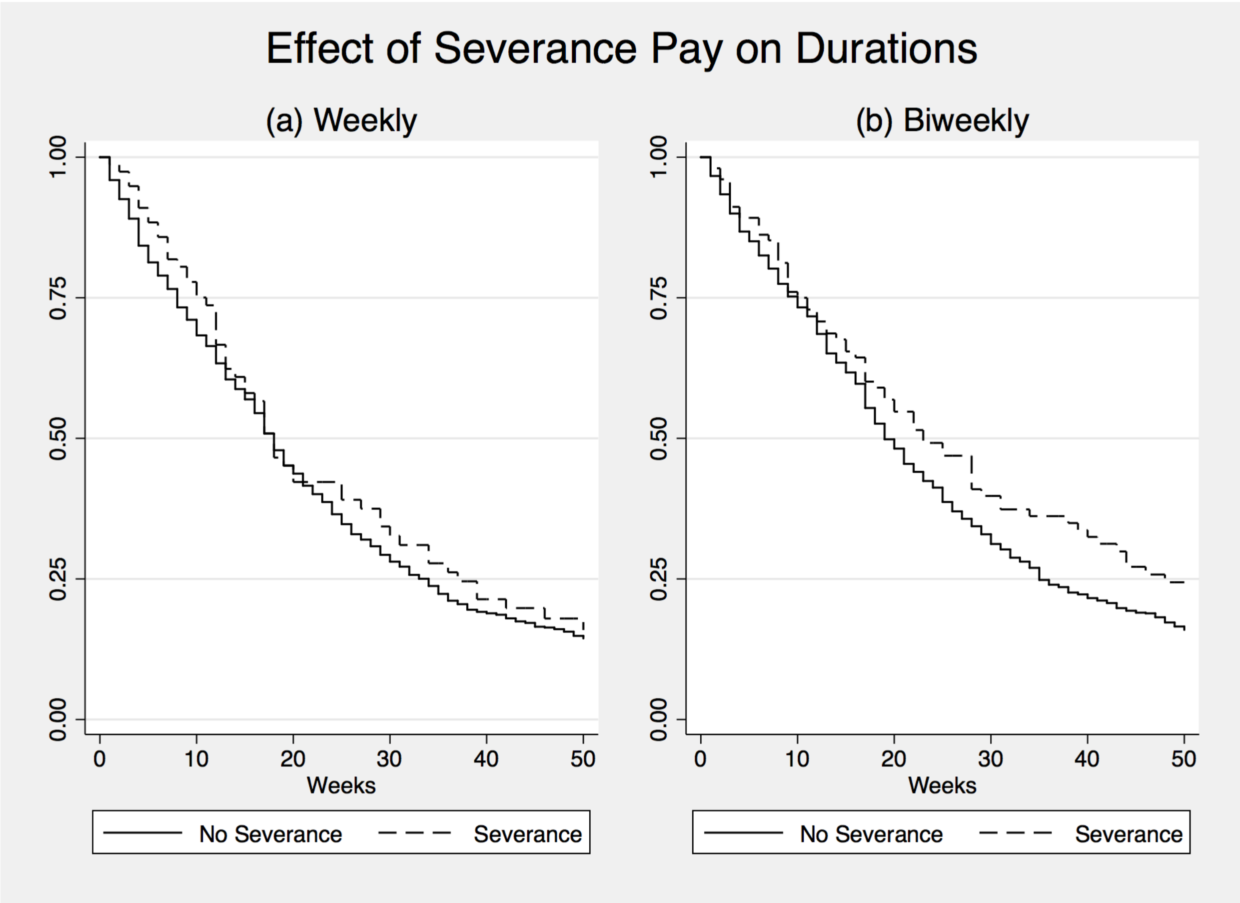


Figure B1: Survival Curve - Effect of Severance Pay on Duration - by Pay Frequency

Note: Figure shows individual level unemployment duration from SIPP 1996-2007 controlling for job tenure. The vertical axes indicates the fraction of unemployed sample. The figure is divided into two panels according to UI benefit pay frequency. For each panel, the solid line represents the hazard of exiting unemployment for UI claimants without severance pay; the dashed line represents the probability of exiting unemployment for UI claimants with severance pay. Following Chetty (2008), the unemployment duration is censored at 50 weeks.

Figure B1 shows the effect of receiving severance pay on unemployment duration for UI claimants under each pay frequency controlling for job tenure. From visual inspection, I find results from both panels resemble the previous finding from Chetty (2008) – receiving severance pay leads to significantly lower reemployment hazard.

	Combined		weekly		biweekly	
	Pooled	Stratified	Pooled	Stratified	Pooled	Stratified
	(1)	(2)	(3)	(4)	(5)	(6)
Severance Pay	-0.232		-0.254		-0.224	
	(0.086)		(0.095)		(0.157)	
Sev., *Ex-ante assets*	**-0.294**		**-0.340**		-0.280	
	(0.123)		(0.141)		(0.209)	
(Tenure<Median)× Sev.		-0.048		-0.091		-0.011
		(0.122)		(0.156)		(0.122)
(Tenure>Median)× Sev.		-0.357		-0.426		**-0.421**
		(0.118)		(0.155)		(0.202)
Equality p-value		0.057		0.166		0.123
# Spells	2,790	2,790	1,306	1,306	1,451	1,451
# Spells, *Ex-ante assets*	1,741		785		932	

Table B2: Effects of Severance Pay, by Pay Frequency

Note: All columns report semi-parametric Cox proportional hazard model results from estimating equation (B.1). The reported coefficients are the percent change in hazard rate with respect to severance pay status. Data are individual-level unemployment spells from 1996-2007 SIPP. For Pooled regression (columns (1) (3) and (5)), I include state fixed effects, year fixed effects, industry and occupation fixed effects, a 10-piece linear spline of the pre-unemployment annual wage, onseam indicator and other individual specific controls - education, age, marital status and total wealth. For stratified regression (columns (2) (4) and (6)), I allow controls to interact with tenure quantiles. The second row controls for household total wealth and restricts the sample to have pre-unemployment assets. The final row display the F-test result comparing coefficients for UI claimants from long or short job tenure quantiles. Standard errors clustered by state are in parentheses.

Table B2 displays a series of regression results. The coefficient of interest is the percent change in hazard rate with respect to severance pay status (β_1). Columns (1) (3) and (5) display results from pooled regression for Combined, weekly and biweekly sample. The estimated hazard coefficient of -0.232 (s.e=0.086) from the combined sample is very close to Chetty (2008)'s estimation of -0.233 (s.e=0.071), who used data from the Mathematica. My estimation from row 1 implies receiving severance pay leads to 20.7% reduction in UI claimant's reemployment hazard. For the "weekly" and "biweekly" sub-samples, the coefficients have similar magnitudes, but is only statistically significant under "weekly" states. In the second row, I further restrict my sample to UI claimants with pre-unemployment asset information - this eliminates about 40% of the sample. Interestingly, the magnitude of the estimated β_1 increases, so does the gap in the hazard coefficient between the two sub-samples.

To further investigate the causal interpretation of the estimated β_1, I run a series of regressions stratified by the relative length of predicted job tenure. Since the size of severance pay increases in job tenure, this regression allow me to test whether an increase in severance pay generosity leads to a bigger liquidity effect (lower reemployment hazard). As shown in Table B2 row 3 and 4, I find the liquidity effect is indeed stronger for severance pay recipients with longer job tenure. The pattern is a consistent across all sub-samples.

Under mean spell length, severance pay amount is equivalent to a 69% increase in UI benefit level under the biweekly pay states and 70% increase in UI benefit level under the weekly pay states. Using my estimated β_1 from Table B2 row 2, a 10% increase in UI benefit level would reduce reemployment hazard through the liquidity channel by 3.5% (insignificant) under the "biweekly" states and 4.1% under the "weekly" states.

Result B1. *Using cross-sectional variation in severance pay status, I find UI claimants respond to the effect of severance pay similarly under the two pay frequencies.*

Appendix C Permutation Test for Extra Benefit Effect

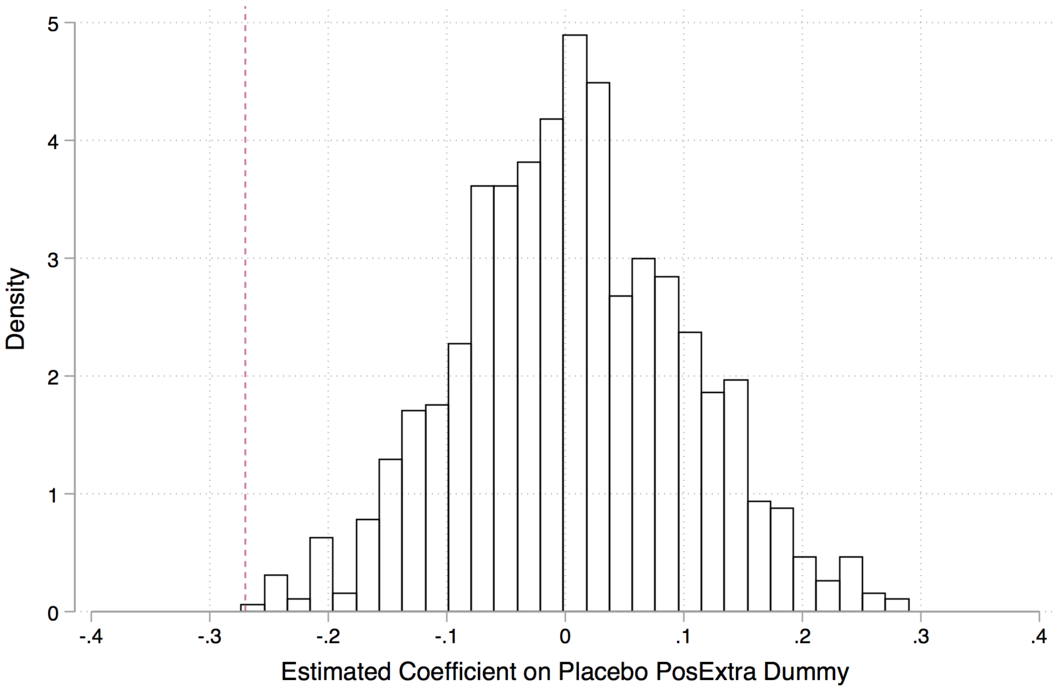


Figure C1: Permutation test for inference of baseline estimation: extra benefit effect

Note: Figures shows the empirical distribution of estimated placebo treatment effects from 1,000 random assignments. Dashed line is the actual treatment effect estimated from Table 3 Column (1). p-value under the permutation test is 0.001

I randomly assign treatment status (*PosExtra*) calendar months following the actual treatment timetable (Table A2).For example, given that 1985 has two *NoExtra* months, I randomly assign two calendar months within 1985 to be *NoExtra* and assign the rest as *PosExtra*. Following random treatment assignments, I re-estimate the placebo pay frequency effect following the specification (Table 3, Column (1)). Then I repeat this process for 1,000 times to obtain a distribution of estimated coefficients. The p-value in this context is defined as the probability that the baseline estimate is obtained purely by chance and is computed by the following expression:

$$p\text{-}value = \frac{\sum_{i=1}^{1000} \mathbb{1}|\beta^i_{baseline} \geq \beta_{placebo}|}{1000}$$

Figure C1 plots the empirical distribution of the placebo estimates using 1,000 random treatment assignments. The dashed line is the point estimate from the baseline estimation

($\beta = -0.277$). Comparing to the estimated placebo treatment effects, the actual effect is statistically significant (p-value = 0.001).