# UI Benefit Generosity and Labor Supply from 2002-2020: Evidence from California UI records[1]

Alex Bell, UCLA

TJ Hedin, UCLA

Geoff Schnorr, CPL UCLA

Till von Wachter, UCLA, CEPR, NBER

**November 2022**

PRELIMINARY DISCUSSION DRAFT

**Abstract:**

This paper provides estimates of the effect of unemployment insurance benefits on labor supply outcomes over the business cycle using 20 years of administrative claims, earnings, and employer data from California. A regression kink design exploiting nonlinear benefit schedules provides experimental estimates of behavioral labor supply responses throughout the unemployment spell that are comparable over time. For a given unemployment duration, the behavioral effect of UI benefit levels on labor supply is unchanged over the business cycle from 2002 to 2019. However, due to increased coverage from extensions in benefit durations , the duration elasticity of UI benefits rises during recessions. The behavioral effect during the start of the COVID-19 pandemic is substantially lower at all weeks of the unemployment spell.

[1]We thank seminar and conference participants at University of British Columbia, David Card's Festschrift, the 2022 Bank of Portugal and Bank of Spain conference, and the 2022 Equitable Growth conference, as well as Janet Currie, Eliza Forsythe, Larry Katz, and Thomas Lemieux for helpful comments. Peter Mannino provided helpful research assistance.

# 1. Introduction

The goal of unemployment insurance is to insure workers against the negative income shock of a job loss while also fostering positive reemployment outcomes for claimants.[2] A large body of literature has identified negative effects of UI generosity on workers' labor supply, highlighting a classic tradeoff between the value of additional insurance and costly behavioral distortions. This tradeoff is particularly important in recessions, when the number of workers relying on UI increases. A key parameter for optimal UI policy, on which the literature has not yet reached consensus, is how the effects of UI on workers' labor supply decisions change over the business cycle.

This paper implements a comparable regression kink design on two decades of California UI claimants' records to determine how labor supply responses to UI generosity change over the business cycle, including during the COVID-19 pandemic. Key to our empirical strategy is to distinguish between labor supply responses at any given point in time during an unemployment spell – measured by changes in the survival curve – and summary measures capturing the effect throughout the entire spell, such as duration elasticities. To interpret our results, we propose a conceptual model in which responses to UI extensions during downturns can be broken down into mechanical and behavioral effects. Our model and empirical findings help to unify existing results on duration elasticities over the business cycle.

Empirically, we find large increases in UI duration elasticity to benefit levels during the Great Recession, but no meaningful changes in responses in UI benefits over the cycle at any point in the survival curve. This result is consistent with a model in which the week-to-week labor supply behavioral responses to UI generosity remain constant throughout the business cycle, while duration elasticities mechanically increase due potential benefit extensions occurring during recessions. Decomposition analyses suggest

[2] An additional set of motives for UI are macroeconomic in nature, focusing on fiscal stabilization.

that this result is not driven by changes in the types of workers who are on UI. In contrast, during the beginning of the COVID-19 pandemic, behavioral responses along the survival curve have been substantially lower at any duration. This reduction does not appear to be driven by large temporary benefit supplements, nor by fluctuating economic conditions in the first year of the pandemic.

Our findings contribute to the literature in several ways. We extend the seminal work applying regression kink designs to administrative data on UI durations in Card, Lee, et al. (2015) and Card, Johnston, et al. (2015) to an analysis of how labor supply effects throughout the unemployment spell varies systematically over time and over the business cycle. An advantage of analyzing survival curves is that they more closely reflect workers' labor supply choices and predictions of theoretical models, while avoiding the problem of dynamic selection that can affect hazard rates over the unemployment spell. Another advantage is that it allows us to clarify how the effect of UI benefit levels on unemployment duration varies with changes in coverage from increased potential benefit durations (PBD) during recessions. Given changes in PBD during recessions are a ubiquitous feature in the US, analyses of UI benefits on labor supply have to take into account the current PBD regime.

Thereby, our results help to clarify currently conflicting results in the literature regarding changes in the effect of UI benefits over the business cycle. While based on an analysis of exit rates, Kroft and Notowidgdo (2015) find that the labor supply responses to UI benefits do not increase in recessions. Using the same design as we do, Card, Johnston, et al. (2015) find a substantial rise in the UI duration elasticity. Card, Kluve, and Weber (2018) also find larger positive impacts of active labor market programs during recessions, perhaps because employers can be more selective when markets are slack. Our findings show that increases in potential benefit duration leads to a rise in the duration elasticity during recessions, even if exit behavior along the survival curve is a-cyclical.

By implementing a comparable, high-quality research design over a long period of time, our study replicates Schmieder, von Wachter, and Bender (2012)'s analysis of UI durations in Germany. As in their case, the use of a comparable research design yields a-cyclical behavioral responses to UI benefits. An advantage of using a fixed policy threshold to changes in UI parameters at the state level is that the latter may themselves be affected by business cycle conditions or that they may estimate the effect for different subgroups over the business cycle.

Last but not least, we further extend the growing evidence on the effect of UI benefits on labor supply summarized in Schmieder and von Wachter (2016). Our estimation strategies identifies the effect of UI benefits holding market-level responses constant, and hence identifies the so-called micro-elasticities that capture the responses of individual job searchers, abstracting from congestion effects among others. Schmieder and von Wachter (2016) reported the median US elasticity to be .38, though there was a wide range across studies from 0.1 to 1.2.[3] Setting the pandemic period aside, relative to existing estimates of UI benefits on labor supply in the literature, our UI duration elasticities range at the upper end from around 0.5 in expansions to 0.8 during the Great Recession. Among others, the difference may derive from

[3] More recent work by Dahl and Knepper (2022) estimates elasticities as large as 1.0, though they recognize these elasticities may also be partially capturing concurrent cuts to income and corporate taxes.

the fact that some of the work based on cross-state comparisons may partly capture market-level responses.

Finally, our analysis of pandemic-era labor supply responses fits into a recent literature studying the effects of the recent expansions of the UI system. Our finding of substantially reduced labor supply elasticities are consistent with other findings indicating that the UI benefit expansions have had little negative distortionary effects on labor supply using cross-state survey data (Finamor and Scott 2021). Recent work using bank account records suggest that the added benefits had large positive impacts on consumer spending relative to their impacts on job search (Bachas et al. 2020; Ganong, Noel, and Vavra 2020; Ganong et al. 2021). Evidence from an online job platform suggests that the added benefits had small negative impacts on job applications, with no discernible effect on vacancy creations (Marinescu, Skandalis, and Zhao 2021). The authors suggest that due to an unusually slack labor market early in the pandemic, the added benefits were welfare-improving because they decreased competition for scarce jobs.

The remainder of this paper is organized as follows. The rest of this section frames our work in relation to selected prior literature on UI generosity. Section 2 details our claim-level data from California as well as our motivation and method for implementing the regression-kink design. Section 3 describes our conceptual model for parsing mechanical and behavioral responses to UI benefit generosity. Section 4 presents our key empirical findings on labor supply prior to and during the pandemic, including an assessment of emergency added benefits during the pandemic on labor supply elasticities. Section 5 summarizes robustness analyses. Section 6 discusses potential welfare implications of our findings, and Section 7 concludes.

# 2. Institutional Background, Data, and Approach

## 2.1 California's Unemployment Benefits Schedule

In the US, the federal government sets a framework for the UI system and the states operate independent UI programs within that framework. In general, the UI system provides unemployed workers who lost their jobs through no fault of their own and who meet a minimum income threshold during their Base Period (BP) with weekly payments that replace a portion of their income (their Weekly Benefit Amount, or WBA) for a certain number of weeks (their Potential Benefit Duration or PBD). Some restrictions in the program are nation-wide; for example, undocumented workers are not eligible for UI in any state. Other aspects of the program vary by state, for example WBA's in each state are determined differently.

In California, before the COVID-19 pandemic, WBAs for all claimants were between $40 and $450 depending on the amount of earnings in each claimant's BP. Specifically, each claimant's WBA is determined by identifying the quarter with the highest earnings during their BP and dividing that number by 26.[4] The result is used as the claimant's WBA up to a maximum of $450; any claimant whose calculated WBA would be over $450 is capped at $450. This leads to a kink in the UI benefit schedule as shown in Figure 1. This maximum benefit value has fluctuated over time based on both state and federal law. The state's statutory maximum was lower than $450 prior to January 2005 and during the Great

[4] First divide the quarterly earnings by 13 to get weekly earnings and then divide by 2 to get half of weekly earnings.

Recession the federal government established the Federal Additional Compensation program which added $25 to all claimants WBAs.

During the COVID-19 pandemic, the federal government made substantially larger changes to each claimant's WBA. Between April and July 2020, the Federal Pandemic Unemployment Compensation (FPUC) program added $600 to each claimants weekly benefit amount making the maximum WBA $1,050 in California (Figure A2). After the FPUC expired, federal policy makers established the Lost Wage Assistance (LWA) program which provided an extra $300 to UI recipients each week between July and September 2020. Finally, between December 2020 and September 2021, the FPUC and then the Pandemic Additional Compensation (PAC) program provided an additional $300 on top of each claimant's regular WBA.

Similar to WBA, a claimant's PBD can vary by state, can change over time within a state, and also varies with workers' Base Period earnings. In California (and in most states), the maximum PBD for the Regular state UI program is 26 weeks. Whether workers receive the maximum PBD or a lower duration is again a function of their Base Period earnings, something we will return to below. The maximum PBD changes over the business cycle for two reasons. First, a joint federal-state program called the "Extended Benefits" (EB) program provides an additional 13 to 20 weeks of UI benefits if the state unemployment rate rises above a certain threshold. Second, federal policy makers have issued additional ad-hoc extensions UI through during downturns, with PEUC being the key federal extension program during the pandemic.

## 2.2 UI Claims and Earnings Data

***Raw Data.*** We combine three administrative datasets maintained by the State of California's Employment Development Department (EDD): Quarterly earnings records (1995-2020), the Quarterly Census of Employment and Wages (QCEW, 2000-2020q3), and UI claims microdata (2000-5/2021). A subset of these data have been used in a series of policy briefs on UI in CA during the pandemic (Bell et al. 2022).

UI claims microdata consists of information collected or produced by EDD in order to process UI claims. The data contains the universe of UI claims filed in CA on or after 1/1/2000 and includes a variety of claim and person-level information. Key information used in our analysis includes the date (start date of claim, or "benefit year begin" date (BYB)) and outcome (eligible or not) of each claim, the date and amount of each payment, and claimant demographics (date of birth, gender, self-reported race/ethnicity).

The quarterly earnings records include total UI-covered earnings in the relevant quarter for each employer-employee (firm) pair. We link each claim to the relevant BP quarterly earnings amounts in order to calculate their HQW---which determines their WBA (as described in Section 2.1) and will serve as the key assignment variable in our research design (as described in Section 2.3). The QCEW data contain earnings, employment, and industry information at the establishment-quarter level, which we aggregate to the firm level (summing across establishments in CA) before linking to the earnings data. This allows us to observe various characteristics of both the firm that a given claimant separates from at the start of their UI spell, and any firm that a claimant moves to after their spell. Both the quarterly earnings data and the QCEW include the universe of UI-covered employment in the state.

Our labor supply results use three separate measures of the duration of each unemployment spell. Our primary measure is the complete duration of an *insured* unemployment spell, which we define as the number of weeks between the first payment and an exit, which we define as two or more unpaid weeks.[5] In several analyses we focus on indicators for whether complete duration exceeded some number of weeks (survival probabilities). Finally, we can use the earnings data to measure the duration of each claimant's *non-employment* spell in quarters (i.e., the number of consecutive quarters with zero earnings). In our sensitivity analyses we use the quarterly earnings and QCEW data to add industry of the main Base Period employer, as well as other employer level characteristics.

***Sample Restrictions.*** Throughout our analysis, we exclude claims from workers who earned too little in their BP to be monetarily eligible for UI. In our main analysis, we also drop claims that have PBD < 26 weeks (to avoid an offsetting but small kink in PBD at the maximum WBA that exists only for these claimants, as described by Card, Johnston, et al. (2015) ); had any disqualifications related to the nature of their job loss (e.g., voluntary quits); had a prior UI claim within 2 years of the claim in question;[6] or had HQW values within $1 of a $1000 multiple (i.e., $999 < HQW < $1001, $1999 < HQW < $2001, etc.). The final restriction is made because substantial "heaping" is observed in the HQW density at these values, an issue known to induce bias in related research designs (Barreca, Lindo, and Waddell 2016). This is further discussed in Section 2.3. Finally, throughout we focus on claims for the regular state UI program, excluding, for example, all claims for the Pandemic Unemployment Assistance (PUA) program as well as claims for other specialized UI programs such as Disaster Unemployment Assistance (DUA).

[5] Following Card, Johnston, et al. (2015), Landais (2015), and Spiegelman, O'Leary and Kline (1993).

[6] In order to avoid potential complications in assigning payments to the correct claim, as described by Leung and O'Leary (2020). Our data contains claim-level identifies which *should* eliminate this concern, but we make this restriction to be conservative.

Table 1 shows summary statistics for our sample and outcomes during the pre-pandemic baseline period. For these cohorts, we walk through our sample restrictions. Starting from a set of nearly seven million claims that were monetarily eligible for UI ("Full Sample", column 1), we drop more than half of these observations when imposing the restrictions described above ("Limit Sample", column 3); for example, 28% of claimants do not have the full 26-week PBD. When we further restrict the sample to those within a $5,000 bandwidth of the kink, we are left with approximately 1.4 million claims for our main analysis (column 4).

## 2.3 Methods

In order to estimate the causal effect of benefit generosity (WBA) on labor supply and reemployment outcomes, we exploit the kinked WBA benefit schedule in a regression kink design. Benefit amounts vary across claimants and are determined by their prior earnings levels (HQW), increasing with prior earnings until the maximum benefit amount $b_{max}$ is reached. Following (Card, Lee, et al. 2015) we model the outcome for claim $c$, $y_c$, as polynomial function of their prior earnings (HQW, the "running variable") $h_c$, allowing the slope of that relationship to differ on either side of the cutoff $h_c = k$:

$$y_c = \alpha + [\sum_{p=1}^{P} \beta_p (h_c - k)^p + \gamma_p (h_c - k)^p \cdot 1\{h_c \geq k\}] + \epsilon_c \quad (1)$$

Here, $\gamma_1$ is the "kink" in the relationship between the outcome and the running variable at the cutoff $k$. An estimate of $\gamma_1$ is causally interpretable under the assumption that any unobserved confounder is smooth through the cutoff, and claimants cannot manipulate their value of $h_c$ around the cutoff. To restate this parameter as the causal effect of an increase in WBA $b_c$, we need to scale by the magnitude of the kink in the benefit schedule. The benefit schedule summarized in Section 2.1 implies that this kink is deterministic. However, in practice non-compliance may be an issue, so we similarly model $b_c$ as:

$$b_c = \theta + [\sum_{p=1}^{P} \mu_p (h_c - k)^p + \eta_p (h_c - k)^p \cdot 1\{h_c \geq k\}] + \nu_c \quad (2)$$

Here, $\eta_1$ is the kink we are exploiting for identification, so that $\gamma_1/\eta_1$ is the causal effect of an additional \$1 in WBA on our outcome $y_c$.

In our preferred specifications we implement a "fuzzy" RKD where $\gamma_1/\eta_1$ is estimated using a Two-Stage Least Squares (2SLS) approach in which $\widehat{b_c}$ is the fitted value from the previous equation, and $\gamma_1/\eta_1$ is estimated as the coefficient on $\widehat{b_c}$ from a second-stage equation which includes $h_c - k$ and a constant. Alternative specifications implement a "sharp" RKD, where $\widehat{\gamma_1}$ is estimated by OLS, $\eta_1$ is assumed to be equal to the deterministic kink in the benefit function, and the standard error of $\widehat{\gamma_1}/\eta_1$ is calculated via the delta-method. Estimates are also presented as elasticities after scaling by one or both of the constant term from a reduced form equation (equal to the mean of the outcome just before the cutoff, since $h_c$ is centered at k) and $b_{max}$.[7]

Recent related methodological work has emphasized the importance of several modeling choices in regression kink and discontinuity designs, including the order of the polynomial $P$, the bandwidth (window around the cutoff determining which observations are included in the regression), and the use of non-parametric regression with triangular kernels that are better suited for boundary estimation (e.g., Cattaneo, Idrobo, and Titiunik 2019; Ganong and Jäger 2018). Our main results use a fixed \$5,000 bandwidth, linear polynomial, and focus on OLS estimation (equivalent to a uniform kernel). In our analysis, we thoroughly evaluate the sensitivity of our results to these choices of bandwidth, functional

[7] Depending on whether the outcome, treatment, or both, are in logs.

form, and calculation of standard errors. We also examine the role of our sample restrictions, including relaxing the restriction on potential benefit duration made in related work.

As mentioned above, the regression kink design delivers causally interpretable estimates under the assumptions that claimants cannot manipulate their HQW value around the cutoff, and that any unobserved confounder is smooth through the cutoff. To provide suggestive evidence in support of the first assumption we plot the density of the running variable in our data in Figure A3. The first panel of FIgure A3 includes the full sample of monetarily eligible UI claimants during the pre-pandemic period (2014-2019). This panel makes clear that abnormally large numbers of claimants appear with “round number” quarterly earnings values. We do not believe that this is related to the WBA schedule in any way, since the HQW cutoff values at which the maximum WBA is attained is never within \$1 of a \$1000 multiple. However, recent work has shown that such “heaping” in the distribution of running variables in regression discontinuity designs can introduce bias, and simply dropping observations at those heaping points has been suggested as a solution (Barreca, Lindo, and Waddell 2016). The second panel in Figure A3 shows the distribution after imposing our preferred sample restrictions, and illustrates the “heaping” of claimants in certain HQW bins has been greatly reduced.

To provide suggestive evidence in support of the second assumption, we estimate regressions analogous to equation 1, with various covariates as the outcome. We implement this test for the following covariates: age, gender, race/ethnicity group indicators, firm size (number of employees and number of establishments, separately), firm average pay, and tenure. Figures A4 and A5 display binned scatter plots of these covariates against the running variable, in each case we see no concerning *visual* evidence of a kink at the cutoff. As shown at the top of each panel, estimated coefficients for slope change the cutoff are statistically significantly different from zero. However, given the size of our data and the small magnitudes of these estimates we do not believe that these results pose a threat to our research design.

# 3 Conceptual Discussion

## 3.1 Implications from Job Search Theory

The classic approach to modeling the effect of unemployment Insurance benefits on labor supply has been job search theory, where unemployed workers sample jobs from a wage distribution every period. In these models, an unemployed individual trades off taking a new job at a given wage versus receiving unemployment insurance benefits and having the option to continue to search for possibly higher paying jobs. Higher unemployment benefits raise the attractiveness of staying unemployed, and hence lead to a reduction in search intensity or an increase in reservation wages. For simplicity, more recent models posit that individuals can directly manipulate the hazard of exit from unemployment (e.g. Card, Chetty, and Weber 2007).

While unemployment is a more important phenomenon in recessions, standard theory is ambiguous as to whether the behavioral effect of unemployment benefits on labor supply increases or falls with labor market conditions (e.g., Schmieder, von Wachter, and Bender 2012, Kroft and Notowidigdo 2015). For example, if search effort is less effective during recessions, when there are fewer jobs available, unemployment benefits could have a weaker effect on labor supply. On the other hand, since job losers typically have lower reemployment wages, and unemployment benefits are usually a fraction of pre-displacement earnings, the benefit replacement rate effectively goes up during recessions. This could lead to stronger labor supply responses to unemployment benefits in recessions.

The labor supply response of an unemployed worker to higher unemployment benefits is sometimes called the 'micro effect' (Landais, Michaillat, and Saez 2018). This can differ from the market-wide effect of an increase in unemployment benefits (the so-called macro effect). Distinguishing between the two is important for optimal UI policy because of spillovers and congestion effects onto other job searchers (Levine 1993; Crépon et al. 2013; Landais, Michaillat, and Saez 2018). These spillovers matter not only for understanding the labor supply distortions of UI, but also for measuring its effectiveness at stabilizing consumption at the macroeconomic level (Gruber 1997; Ganong and Noel 2019). For example, if individuals not receiving UI benefits fill a limited number of jobs as UI beneficiaries reduce their search intensity, the macro effect could be smaller than the micro effect. Alternatively, if the reduction in search intensity by UI beneficiaries increases the cost of vacancy creation, the macro effect could be larger. In this paper, we explicitly seek to focus on the behavioral (micro) response to UI benefits by holding constant the market environment to the left and the right of the benefit kink.[8]

## 3.2 Measuring Behavior Labor Supply Responses

To measure the behavioral effects of unemployment insurance benefits the paper studies the response of survival probabilities as one its primary outcomes. The survival probability measures the fraction of workers still unemployed after a given number of weeks. While the theory suggests the weekly exit hazard (the probability of finding a job among workers that are still unemployed) comes closer to what individuals are able to manipulate directly, by definition hazard rates are calculated from a sample that changes throughout the benefit spell. Insofar as unemployment benefits affect the exit hazard in the first

[8] It is worth noting that the behavioral effect on labor supply that we and most of the literature identify in our empirical work may only partially represent a moral hazard effect. Strictly speaking, we identify the net outcome of a substitution and an income effect (Chetty 2008). The substitution effect captures the reduction in labor supply due to the reduction of relative benefit of working from UI, and is generally considered a potentially costly distortion. Yet, as in classic labor supply theory UI benefits also induce an income effect, in particular if individuals are credit constrained. The size of the income and substitution effects may vary over the business cycle. As most other studies, we are not able to identify these effects separately. In the empirical section, we will show that there is no prima facie evidence of large composition changes that would lead us to expect that workers are more credit constrained in recessions.

(and ensuing) periods, the marginal effect on all remaining hazards is affected by dynamic sample selection bias.

To help understand the effect of UI benefits on the probability of remaining on UI throughout the spell, the survival probability for any given UI duration $t$ can be written as the product of the probability of not exiting in each of the periods up to $t$. Let the probability of finding a job in any given period prior to time $t$ is τ be $s(\tau)$; then the survival curve is

$$S_B(t) = \prod_{\tau=1}^{t} (1 - s(\tau)) \tag{3}$$

If an increase in UI benefits lowers search effort and hence decreases the probability of exit in each week throughout the unemployment spell, the effect on the probability of remaining on UI for a given period will be *cumulative.* Mathematically, $\partial S_B(t)/\partial b$ increases over the unemployment spell. This is immediately clear in the textbook case of a constant exit hazard (i.e., the probability of finding a job does not change over the spell, $s(\tau) = s$). In this case, $S_B(t) = (1 - s)^t$, and $\partial S_B(t)/\partial b = - t(1 - s)^{t-1}(\partial s/\partial b)$ , which increases in t (since $\partial s/\partial b < 0$). This is further explored in the Appendix, which shows simulated survival curves. Note that if we measure the effect in percentage terms as elasticity by dividing by the survival curve, the effect of UI benefits increases even more strongly throughout the spell since the survival curve declines over time. For the constant hazard case, we have $e_{S(t)} = (\partial S_B(t)/\partial b)(b/S_B(t)) = - tb(\partial s/\partial b)/(1 - s)$*, which linearly increases with UI duration.*

A common summary measure of the individual labor supply effects is the unemployment duration elasticity. The UI duration elasticity measures the percent change in UI duration in response to a one percent rise in UI benefits. By expressing the response in percentage terms, the elasticity takes into account that average employment durations vary substantially over the business cycle. This can yield a more meaningful comparison of labor supply responses overtime. However, because the duration elasticity summarizes workers' behavior over the entire unemployment spell, it can change over time even if behavioral responses at any given unemployment duration are constant.

The employment elasticity can be expressed directly as a sum of behavioral responses measured by the survival curve. Let $t$ = weeks, $B$ = duration of unemployment insurance benefits, $P$ = maximum potential duration of UI benefits, and $S_B(t) = Pr[UI\ Benefit\ Spell \geq t]$ is the survival curve of UI duration. Let $e_x = (\partial X/\partial b)(b/X)$ be the elasticity with respect to weekly UI benefits $b$. We then have:

$$B = \sum_{t=1}^{P} S_B(t) \tag{4}$$

$$e_B = \sum_{t=1}^{P} e_{S(t)} w_B(t) \tag{5}$$

with weights $w_B(t) = S_B(t)/B$ (e.g., Schmieder and von Wachter 2016). One implication of this formula is that an increase in the potential duration of unemployment benefits $P$ will lead to a higher employment elasticity in recessions, even if the underlying behavioral responses to UI benefits *at any given duration* are constant over the business cycle. In addition to this *coverage effect*, lower job arrival rates in recessions shift the survival curves out, increasing the weight put on longer duration in the elasticity formula. This *weighting effect* increases the duration elasticity mechanically because the elasticity of the survival curve increases throughout the spell. Overall, the duration elasticity correctly captures an increase in the reduction in labor supply due to unemployment benefits. However, this increase is purely due to an increase in coverage and change in weighting, not due to a change in the behavioral effect at any given point in the spell.

A similar formula holds for the duration for non-employment. Let $q$ = calendar quarter, $D$ = duration of nonemployment, and $S_D(q) = Pr[Nonemployment\ Duration \geq q$ be the survival curve of nonemployment duration. Then we have that

$$D = \sum_{q=1}^{T} S_D(q) \tag{6}$$

$$e_D = \sum_{q=1}^{T} e_{S(q)} \cdot w_D(q) \tag{7}$$

Where $w_D(t) = S_D(t)/D$. Here, the summation is over total potential nonemployment duration $T$. Even thought $T$ does not change, an increase in $P$ leads a greater part of the nonemployment spell to be covered by UI benefits and hence be subject to behavioral labor supply reductions. Hence, a similar mechanical change in the nonemployment duration elasticity occurs with the business cycle, even though the marginal effect on nonemployment at any given point in the spell might be unchanged over the cycle.

Figure 3 shows empirical survival curves for different time periods. During the two expansions in our sample, survival curves drop sharply at 26 weeks, the maximum PBD in California. The survival curves do not drop to zero, because individuals working part-time while unemployed and collecting partial UI benefits can stretch their UI benefits as far as 52 weeks. In the Great Recession, federal benefit expansions brought the maximum PBD to 99 weeks, reflected in a substantial rightward shift in the survival curve. In addition, lower exit rates increase UI durations and hence the survival curve at all durations, clearly visible in the shift below 26 weeks. During the COVID-19 pandemic, benefit extensions increased PBD to a maximum of 99 weeks, again resulting in a rightward shift in the survival curve with respect to the prior expansion. In addition to the coverage effect from PBD increases, these rightward shifts during downturn itself contribute to an increase in the UI duration elasticity through the weighting effect.

# 4 Labor Supply Results

## 4.1 Baseline Results

Figure 4 graphically walks through our main research design for the expansion period prior to the pandemic for our core sample. To the left of the kink, higher earnings (and thus benefits) are associated with higher 8-week survival probabilities, whereas to the right of the kink, higher earnings are associated with lower survival probabilities. This pattern matches that identified by Card, Johnston, et al. (2015) and Landais (2015).[9] The slope of the reduced-form effect of earnings on the survival probability (Equation 1) falls by 0.0000216 after the kink, indicating that the relatively lower benefits decrease survival rates. Given the slope of WBA with earnings (the first stage, Equation 2), we conclude that prior to the

[9] This might be surprising, since to the left of the kink, the fraction of pre-displacement earnings that is replaced before the pandemic is constant at 50% (see Figure A1). Such a pattern could for example arise if earnings losses are larger for workers with pre-displacement earnings, leading to an effective replacement rate that is increasing to the left of the kink. Yet, it is important to keep in mind that away from the kink, the relationship of earnings and survival may be determined by selection or omitted variables, and hence cannot be directly interpreted as causal. For example, workers with higher earnings that are laid off and end up receiving UI might be harder to reemploy, or might search longer for jobs independently of UI benefits.

pandemic, $1 of benefits increased eight-week survival by 0.0869.[10] At the kink point of $450 WBA, this translates to a survival elasticity of approximately 0.39.[11]

While the graphical analysis of Figure 4 is limited to only the eighth week of the survival curve, Figure 5 plots the resulting elasticity estimate at each week of the survival curve. As predicted in Section 3.2, we find that elasticity of survival to UI benefit generosity is larger for later weeks of the survival curve. The elasticity of eight-week survival is just above 0.4 in each year of the pre-pandemic expansion period, rising to nearly 0.7 by the 26th week of the claim. As discussed in Section 3.2 and our Simulation Appendix, this is because the survival curve captures the cumulative effect of lower search effort throughout the spell; in addition, as survival shares fall, the same percentage effect on hazard rates would constitute a larger percentage increase in survival rates.[12]

## 4.2 Changes over the Business Cycle

The extent to which these labor supply responses change across the business cycle is a key input to optimal UI policy, particularly as it relates to other fiscal stabilization tools. Figure 5 shows that the responses to UI benefits throughout the spell have been very similar during the expansion period. Figure 6 extends our analysis of survival curve elasticities to a yearly resolution from 2002 to 2019. As expected, for all years we find higher elasticities at later points in the survival curve. However, despite some moderate fluctuations over time, we do not detect any meaningful changes in survival elasticities during the Great Recession. In fact, the response of survival probabilities to UI benefits fell somewhat

[10] The second-stage difference in slopes is 0.0000216. The first-stage difference in slopes is .5/13 (on the left side of the kink, quarterly benefits increase by $.50 for each $1 of quarterly earnings, but we divide by 13 to convert to weekly benefit amount). Finally we divide through by the mean outcome of .63. (0.0000216/(.5/13))/.63 = 0.00089, or 0.089%.

[11] Multiplying the previous calculation by 439 (the average realized WBA around the kink point).

[12] In the Appendix, we show that the marginal effect of UI benefits on survival curves itself increases over time, so the increase is not purely driven by the decline in average survival rates over time (Figure A6).

throughout the spell at the beginning of the Great Recession, but then quickly recovered during the prolonged recovery.

In contrast to the a-cyclicality of survival elasticities, we find substantially higher *duration* elasticities to WBA during the Great Recession. Figure 7 presents our baseline reduced-form RKD graph using total UI durations rather than fixed-week survival. The average duration elasticity during the pre-pandemic expansion is approximately 0.5 (Table 2).[13] Figure 8 plots these duration elasticities by year, and Table 2 shows analogous results by period. We estimate a duration elasticity of approximately 0.62 prior to the Great Recession and approximately 0.5 in the expansionary period following the recession (2014-2019). However, at the height of the Great Recession (around 2010-2011), we estimate that duration elasticities increased to approximately 0.78.

The conceptual discussion in Section 3.2 helps reconcile the cyclical nature of duration elasticities with the a-cyclicality of survival elasticities. The rise in PBD during the Great Recession raises the UI duration elasticity through a coverage effect even in absence of any change in behavioral responses at any given point in the spell. In addition, the rightward shift in survival curves increases the weight put on higher survival elasticities at longer UI durations. To see the implications of the coverage effect and to isolate shifts in the elasticity due to underlying behavioral changes, one can recalculate the UI duration elasticity by summing only over the first 26 weeks (i.e., simulating a world in which the PBD did not rise during the downturn). In contrast to the actual duration elasticity, the resulting line in Figure 8 is as a-cyclical as the survival elasticities.

[13]The marginal effect is 0.000666. To convert this to an elasticity, we divide by the change in slope of the benefit schedule, which under perfect compliance is (.5/13). We then divide by the average outcome at the kink point (15.81), and multiply by the average WBA near the kink point ($439). This gives .481, where the difference from the quoted result (0.5) arises from some claimants having a WBA slightly different than what the benefit formula would suggest. (This is accounted for when run two stage least squares.)

We do not have particular insights why the behavioral response at any given point in the spell does not seem to vary with economic conditions. One interpretation of the a-cyclicality is that the two countervailing forces of low availability of jobs and higher benefit replacement rates effectively cancel each other out. Alternatively, while responding to UI benefits *on average* as predicted by theory, do not adjust their responses over the cycle.

## 4.3 Non-Employment Elasticities to WBA and to UI Duration

While UI claim duration is a common measure of labor supply responses to UI benefits, it does not capture employment behavior beyond the UI spell. While no weekly measure of nonemployment duration is currently available in the U.S., our data allows us to measure the number of consecutive quarters with zero earnings. As discussed in Section 3.2, the elasticity of nonemployment durations to UI benefits should again be cyclical even if underlying behavior does not change.

The analogue of Figure 8, Figure 9 plots elasticity of non-employment durations to benefit levels. Our first finding is that the elasticity of non-employment duration is lower than the elasticity of claim duration. During expansionary periods, our point estimates for non-employment elasticities fluctuate around 0.2. Standard errors are larger for non-employment durations relative to claims durations due to the coarseness with which we measure the outcome. The second finding is that during the Great Recession, non-employment duration elasticities increased to 0.6, substantially higher than expansionary period non-employment elasticities, but still well below the 0.75 claim duration elasticity of this period. Part of the reduced sensitivity might be due to the fact that the measure is quarterly instead of weekly, something we are working on exploring further. Similarly, the quarterly nature of our employment data does not allow us to compare our nonemployment duration elasticity to granular nonemployment survival rates (as, say, in Schmieder, Bender, and von Wachter 2012).

## 4.4 Labor Supply During the Early Pandemic

Mirroring our analysis of pre-pandemic labor responses, we apply an analogous RKD to the mass of claimants at the start of the pandemic. An important facet of the pandemic policy context is that Congress added large amounts of fixed-level benefits at various points. In this section, we consider only claimants' responses to their statutory WBA (without top-ups). This simplification, which we return to in greater detail in the next section, makes interpretation of the results cleaner since it is not obvious ex-ante whether claimants' job search behavior should be expected to respond to the top-ups that were in force during the particular week, some expectation of future top-ups, or some other behavioral channel. If claimants internalized the added benefits, this would have lowered their elasticities with respect to statutory benefits because $1 of statutory benefits would be a smaller percentage change in the denominator of the elasticity calculation.

Figure 10 shows our RKD during the pandemic with eight-week survival as the outcome. Due to the recency of the data, we focus on analysis of survival curves rather than partially censored durations. In contrast to our pre-pandemic results, we find during the pandemic that survival is decreasing in prior earnings on *both* sides of the kink. In other words, higher-earning workers remained on UI longer, even though their benefits were no more generous. The difference in slope around the reduced-form kink is 0.00000689, which implies that a marginal $1 of benefits decreased the rate of survival by 0.02%.[14] This estimate is somewhat smaller than our pre-pandemic baseline of 0.086%. The implied elasticity for

[14] (0.00000689/(.5/13))/.80=0.00022%

early-pandemic claimants is 0.097, which is lower than our pre-pandemic baseline.[15] (Although the difference in slopes is more subtle than in pre-pandemic years, the percentage difference in benefits around the kink would also be smaller if we take into account the emergency added benefits; a back-of-the-envelope calculation factoring in $600 of added benefits for everyone would bring this elasticity to 0.23.[16])

Figure 11 extends the analysis to each week in the first year of the survival curve for claimants who entered near the start of the pandemic. Although the shape of the survival elasticity (not including supplements) is similar to our pre-pandemic baseline in that survival elasticities are generally increasing with UI duration, the levels everywhere are lower than our baseline. Whether claimants' low responsiveness to statutory benefit generosity during the pandemic can be explained by emergency added benefits is a hypothesis that we turn to next.

## 4.5 Did Benefit Top-Ups Affect Labor Supply During the Pandemic?

The extent to which employment reacted to UI expansions during the pandemic has been debated in the literature (Coombs et al., 2022; Dube, 2020; Finamor and Scott, 2021; Ganong et al., 2021; Holzer, Hubbard, and Strain, 2021; Marinescu, Skandalis, and Zhao, 2021). Whereas we have so far analyzed workers' responses to only statutory benefit levels during the pandemic—implicitly assuming away responses to federal added benefits—in this section we offer evidence from the data on how workers responded to these benefits. Importantly, our data and approach allow us only to examine how workers responded in the short-run to high-frequency changes in benefits.

---

[15] 0.00068575/(1/433)= 0.0970

[16] (0.00000689/(.5/13))/.80/(1/(433+600))

Our approach to isolating claimants' responses to added benefits is as follows. For the large cohort of claimants that entered UI at the start of the pandemic, we calculate these labor supply elasticities two different ways—with and without the federal added benefits that prevailed in that week of the spell—and obtain meaningfully different results. For interpretation, we make use of the additional finding that all survival elasticity estimates we have seen so far have been smoothly increasing functions of spell week. Thus, under the hypothesis that workers responded equally each week to $1 of statutory WBA and $1 of top-up, we would expect to see survival elasticities for the broader measure of benefit levels smoothly increasing in week of spell.

Figure 11 plots these survival elasticities by week of spell for claimants during the pandemic. When we calculate elasticities using claimants' original WBA (i.e., which is capped at $450), we have already seen that we obtain a smooth set of estimates that resembles the shape over the course of the spell of our pre-pandemic estimates, though lower. Using claimants' effective WBA's (which were as high as $1,050 at some points), the elasticity estimates surge during particular weeks, leading to a more jagged pattern. Since we are aware of no changes in the labor market that would have caused changes in labor supply elasticities that so perfectly offset the weekly changes in added benefits, we view these results as suggestive that claimants simply did not respond to weekly changes in added benefits.

Given that claimants evidently did not internalize these level changes in added benefits, the question of why the behavioral response to statutory benefits fell by such an unprecedented rate is all the more puzzling. Leading explanations relate to the situation in the labor market at the start of the pandemic. Both the absence of employment opportunities and the increased health risks would have reduced the importance of UI benefit generosity in workers' decisions to search for a job. At the extreme, in a full lock down the sensitivity to UI benefit extensions should be zero. Liquidity infusions from other government spending programs may also have played a role. Finally, while we do not find that claimants responded to week-to-week changes in UI benefits in a neoclassical way, scope may exist for more behaviorally founded models to explain part of the effects. For instance, if claimants continued to expect the $600 weekly added benefits even after the policy turned off, that could explain some (but not all) of their lower responsiveness to the marginal dollar of benefits.

# 5. Robustness Analysis

## 5.1 Assessing the Role of Composition Changes

To probe whether compositional changes in claimants drive changes in our duration elasticity over time, we re-estimate our results under inverse propensity weights. If one expects that duration elasticities vary across groups with different observable characteristics, then the changes in the relative number of claimants from each group might explain the changes in the duration elasticity over time. Our results suggest this is not the case, and rather the changes in duration elasticities are driven by other factors, such as economic conditions and the availability of extended benefits.

Our reweighting procedure is as follows. For claimants in our core sample, we use a probit model to estimate the probability of each claimant having a BYB in the year 2009, based on their observable characteristics (age, gender, industry, race, education, citizenship, recall expectations, separation reason, tenure, and the characteristics of the separating firm). We then re-estimate the duration elasticity year-by-year, reweighting the claimants in each sub-sample according to this propensity score, so that in each year the composition of the sample is similar to the sample in 2009 (in terms of observables).

Figure 12 shows the results of this inverse-propensity score weighting analysis. We see that the re-weighting has had little effect on the patterns we observed earlier — the elasticities during the Great Recession are still slightly higher than those seen in the 2000s expansion, and remain much higher than those seen in the pre-pandemic expansion. This suggests these higher elasticities are not a result of the “type” of claimant who filed for UI benefits during these years (at least in terms of the observable characteristics described above), but rather a change in other factors, such as the economic environment, or, as indicated by our survival analysis, the availability of extended benefits.

Figure 12 also shows the actual and reweighted duration elasticity for 2020. In contrast to the results for the pandemic shown in Table 2, the 2020 estimates pool all workers starting a UI claim during 2020. Based on the discussion in Section 4.4, to calculate the elasticity, we ignore the Pandemic benefit increases. The resulting elasticity is very similar to what is shown in Table 2 for the early pandemic sample (partly due to the fact that a large share of 2020 claims were filed early on). Using this broader sample, we then recalculate the elasticity based on our reweighting strategy. We see that the reweighted elasticity increases from about 0.17 to about 0.2, indicating that composition changes may have played some role. However, the effects are still substantially smaller than the pre-pandemic elasticities in any year, indicating that factors other than composition changes were responsible for the dramatic decline in the responsiveness to added UI benefits during the pandemic.

## 5.2 Sensitivity of Baseline Results

Our core RKD labor supply results are not particularly sensitive to variations in the bandwidth, specification, or sample definition.  The top half of Table 3 illustrates the sensitivity of our results using total UI duration as an outcome, while the bottom half uses 8-week survival as the outcome. Column 1 reports our main results, while column 2 instead uses a data-driven bandwidth which minimizes the MSE of the local (linear) polynomial point estimator (still estimated under a uniform kernel). We then estimate the model using the ad-hoc bandwidth while including a quadratic term (column 3), before turning to the estimation procedure recommended in Cattaneo et al. (2019). Columns (4)-(6) show the results from using this optimal bandwidth, local linear, triangular kernel estimation method, first with no bias adjustment (column 4), then including an adjustment to the coefficient to account for potential smoothing bias in the linear approximation to the regression function, and finally (column 6) with adjusted standard errors which reflect the uncertainty in estimating this bias. Columns (7)-(9) replicate columns (4)-(6) but include a quadratic term. Overall, the table shows our results are very robust to different bandwidth choices and estimation methods used.[17]

We opted for a common bandwidth for all of our results. To assess this choice, Panels A and B of figure 13 illustrate the sensitivity of our main specification to changes in the bandwidth only. We see that the coefficient is relatively stable for each bandwidth over $3,000, while the variance is much higher for smaller bandwidths. To probe this finding further, Figure 14 illustrates the RKD design for total UI duration in the pre-pandemic period under 3 different bandwidths. We see that both the $5,000 bandwidth (our main result) and $2,500 bandwidth seem to fit the data quite well, while the smaller $1,000 bandwidth appears to provide a misleading estimate of the slope of the underlying function on the left-hand side of the kink, leading to a substantially smaller elasticity. Overall, we conclude that our results are robust to our choice of a somewhat larger bandwidth that allows us to obtain a much more precise estimate than, say, the $2,500 bandwidth.

[17] The table presents estimates for UI durations that were winsorized at the 95th percentile, since otherwise the Cattaneo et al. (2019) approach produces unstable results. Winsorizing does not affect any of the other findings in the table.

Table 4 explores sensitivity to one of our main sample restrictions. As discussed in Section 2, to avoid potential confounding effects from changes in PBD duration at our kink point, our main analysis follows prior work and only includes workers whose potential benefit duration is equal to the maximum benefit, 26 weeks. Hence, our main sample is determined by the kink point, introducing potential sample selection bias. To address this bias, we follow an alternative estimation strategy that combines the variation of both weekly benefit amounts and potential benefit durations at the kink points to estimate the marginal effect of the maximum benefits amount (MBA) available to workers. The MBA is the maximum total benefit amount an individual can receive based on their prior earnings, and yields well-defined variation for workers with and without maximum PBDs. Hence, the kink in MBA can be estimated for the full sample of workers without a PBD restriction.

The results in Table 4 show that this more general estimation strategy confirms our main findings. The table shows the marginal effects and implied elasticities for both the kink in Weekly Benefit Amounts and (WBA) and MBA for the extended sample (column 1) and our main sample (column 2). Note that by design, the results from the WBA kink in column (2) are equal to our main estimates in Table 2. As expected, the marginal effects based on variation in MBA are smaller than WBA effects in column (1), since MBA varies more strongly at the kink point than WBA alone due to variation in PBD. The MBA results for the main sample imply elasticities that are very similar to our main findings, confirming that the focus on workers with maximum PBD in our main sample does not bias our overall findings.

## 6. Potential Welfare Implications

A key question when analyzing the effect of UI benefits on labor supply is what implications the findings have for whether increasing generosity of UI benefit levels are welfare improving relative to not raising benefit levels. In the canonical model used to assess this question (e.g., Bailey 1974, Chetty 2008), workers pay taxes while employed, and receive transfers from the UI system while unemployed. Assuming workers cannot perfectly self-insure, the welfare gain from transferring consumption from the employed to the unemployed state in this setup is offset by short-falls in taxation if UI benefits lead workers to reduce labor supply. Schmieder and von Wachter (2016) show that the change in social welfare ($W$) of additional UI benefit levels b can be expressed as

$$\frac{\partial W}{\partial b}\frac{1}{v'(c_e)} = \underbrace{\underbrace{B}_{\text{Mechanical increase In transfer}} \cdot \underbrace{\frac{u'(c_{u,t\leq P})-v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}}}_{\text{Mechanical transfer to unemployed}} - \underbrace{\left(\frac{\partial B}{\partial b}b + \frac{\partial D}{\partial b}\tau\right)}_{\text{Behavioral cost}} \tag{8}$$

Where $B$ and $D$ are the duration of UI benefit receipt and nonemployment as described in Section 3, and $u(\cdot)$ and $v(\cdot)$ are the utility functions while employed and unemployed, respectively. The welfare gain of a rise in UI benefits (expressed in terms of the marginal value of consumption during employment) consists of the social value of the transfer of income for the covered unemployment spell $B$. The welfare cost is the rise in benefit payments that arises as individuals reduce their search effort ($\partial B/\partial b$) and the decline in tax revenues due to lower employment ($\partial D/\partial b$), valued at the benefit level and the tax rate, respectively.

When comparing the behavioral costs of UI benefits in different labor markets, it is helpful to normalize the total labor supply response by a metric of the total transfer received by unemployed workers. This is particularly useful over the business cycle, because lower exit rates and increasing PBDs lead to large fluctuations of total transfer amounts, such that comparing labor supply estimates alone over time provides a partial picture of the amount of potential distortion in the UI system. Dividing by the total spell duration (i.e., the total mechanical transfer to the unemployed from an additional dollar of UI benefits), one obtains

$$\frac{\partial W}{\partial b}\frac{1}{Bv'(c_e)} = \frac{u'(c_{u,t\leq P})-v'(c_e)}{v'(c_e)} - \frac{1}{B}\left(\frac{\partial B}{\partial b}b + \frac{\partial D}{\partial b}\tau\right) \quad (9)$$

In this formulation, the left hand side measures the welfare gain from an increase in UI benefits per dollar of actual transfer to the unemployed (Schmieder and von Wachter 2017). The social value of each additional dollar of UI benefits is the net gain marginal utility (first term on the right hand side), as before. The last component is the ratio of the behavioral cost (BC) to the mechanical cost (MC) of a one dollar increase in UI benefits ($\$B$). The BC/MC ratio normalizes the behavioral cost by the total transfer, and hence can be more readily compared to over the business cycle. It can also least in principle be entirely measured by administrative data. Figure 15 provides a graphical visualization of the welfare effect of a rise in UI benefit levels when potential benefit durations are changing as well.

Unfortunately, at present our data does not allow us to measure $B$ and $D$ in a comparable fashion. As we are pursuing work arounds, the theory in equation 3 itself provides helpful guidance as to what our estimates imply for the welfare effects of UI benefit increases over the business cycle. To assess the potential response in total nonemployment duration ($B$) for this purpose, it is useful to assume that the effect of UI benefits on nonemployment duration is equal to the effect on UI durations.[18]

To fix ideas and focus solely on what our results imply for the welfare effect of UI benefit increases over the business cycle, consider first the case with constant PBD. As we have seen, survival elasticities are a-cyclical, which implies that the corresponding UI duration elasticity is a-cyclical as well. If the response in nonemployment durations to UI benefits throughout the spell were to follow the same pattern, then the BC

[18] This is sensible and conservative for several reasons. The presumption of a common exit hazard governing UI benefit duration and nonemployment duration is explicit in all theoretical work on UI. In separate ongoing work, we find that nonemployment elasticities with respect to UI benefits tend to be positive after PBD ends. Moreover, work from Germany and other countries suggests that nonemployment durations respond less to UI benefits than UI durations. So if at all, nonemployment duration elasticities should be smaller than UI benefit elasticities.

in equation 4 is a-cyclical. In contrast, due to lower exit rates the average UI duration B, and hence the total MC, increases substantially in recessions. Since it is likely that the marginal value of a transfer stays either constant or increases in recessions, this implies that the marginal welfare gain from additional UI benefits rises in recessions.

In the U.S., PBDs rise in every recession, complicating the theoretical assessment of potential welfare effects of UI benefits somewhat. In addition to the assumption that nonemployment duration elasticities are approximately equal to UI duration elasticities, given that survival elasticities are a-cyclical, it is also safe to assume that the second derivatives of UI benefit effects on welfare with respect to PBD is zero. With this simplification, how the change in the welfare effect of UI benefits over the business cycle is affected by extensions in PBD is captured by two components.

The first is the first derivative of the value of the mechanical transfer in equation (1) with respect to PBD (the total benefit duration B times the marginal value of an additional dollar transferred). Clearly, a rise in PBD covers a larger amount of unemployment spells, and hence leads to an increase in the mechanical transfer B. If one assumes that the marginal value does not decrease as longer unemployment spells are covered, it means that the total value of the mechanical transfer of a change in UI benefits increases with PBD durations. Because unemployment durations increase in recessions, the rise in the value of the mechanical transfer of UI benefit durations under extended PBDs is likely to be larger than in expansions. Hence, both PBD increases and lower exit rates likely raise the welfare gain from UI benefit increases.

The second component is the derivative of the welfare cost in equation 3 with respect to PBD. With second derivatives set to zero, this component is simply the additional labor supply reduction due to higher UI benefits *for the portion of nonemployment spells now covered by the higher PBD.* Formally, this is dS_P/db, i.e., the reduction in exit rates among those workers that previously had exhausted benefits at duration P. This reduction in labor supply leads to higher benefit payments and lower tax revenues. Since the effect of the survival curve was found to be a-cyclical, this component is not expected to be higher in recessions than in expansions. However, given PBD extensions typically occur in recessions, it means that in recession the welfare cost of UI benefit increases is higher.

Given the size of the mechanical transfer compared to the moderate shift in behavioral costs, it is likely that the net overall welfare effect of UI benefits is still positive under PBD extensions, particularly in recession. But without an explicit measure of nonemployment duration that would allow directly measuring changes in welfare costs over the cycle, this is a preliminary hypothesis.

It is important to note that our framework assumes that there are no “spillover” effects of benefit increases on the employment outcomes of workers who do not receive them. This is because the variation in benefit levels that we rely on affects a small number of UI recipients, and is therefore unlikely to produce spillover effects. However, spillover effects might occur in response to benefit changes that affect larger populations of UI recipients. For example, increased job search activity among a large group of unemployed workers whose UI benefits were cut might reduce job-finding probabilities among other workers in the same labor market (Marinescu 2017).

In the presence of such spillover effects the so-called “macroelasticity” of unemployment with respect to UI benefits ($\varepsilon^{Macro}$ , the effect on market-level unemployment) will differ from the “microelasticity” ($\varepsilon^{micro}$ , the effect on individual-level unemployment durations). Landais et. al.{Updating} show that the optimal benefit level in the presence of spillover effects is equal to the standard Baily-Chetty optimal benefit level (i.e., that results from equation (8)), plus a correction term. The correction term is generally increasing in an “elasticity wedge” $1 - (\varepsilon^{Macro}/\varepsilon^{micro})$, so that a larger macro effect relative to micro implies a lower optimal benefit level.

While the literature is mixed, prior work has generally found that this elasticity wedge is small and positive, i.e., that macro effects are smaller than micro. Two exceptions are worth nothing. First, in recent work that was uniquely able to estimate micro and macro effects in the same context Johnston and Mas (2018) find that there was no spillover effect of a large reduction in PBD in Missouri, i.e., that $\varepsilon^{Macro} = \varepsilon^{micro}$ so that Landais et al correction term is zero. Second, Landais et al provide suggestive evidence that the correction term as a whole is procyclical, which would imply that our framework *overestimates* the disincentive cost of benefit increases during recessions.

## 7. Conclusion

Our causal analysis of 20 years of California UI claims data has yielded new insights about how UI benefits affect labor supply choices over the business cycle. Using a regression kink design, we were able to precisely identify labor supply elasticities throughout the entire unemployment spell in different economic contexts. While the labor supply duration response mechanically rises during recessions when the duration of UI benefits are extended, we have found that the behavioral component of the labor supply response at any given point of the unemployment spell is a-cyclical. The behavioral responses for the initial wave of UI claimants during the pandemic – for whom we can assess the role of pandemic supplement payments – were substantially smaller than over the prior 20 years.

Our findings bear potentially salient implications for optimal UI policy, particularly as it relates to the business cycle. Because we find that behavioral distortions to UI benefits levels alone do not rise in recessions, this should push policy toward more generous UI benefits during recessions, when workers need them the most. While this is likely also to be the case when potential benefit durations rise at the same time, additional research is needed to establish this empirically. Our finding that claimants' behavior responded little if at all to large changes in added benefits during the pandemic also points to the power of UI expansions not only to insure workers against job loss, but also to effectively distribute large amounts of fiscal stimulus during downturns with minimal distortions.

In future work, we are continuing to probe open questions raised during our analyses. An important aspect will be to assess the feasibility of obtaining comparable estimates of the duration elasticities for UI and nonemployment duration to implement a more thorough welfare assessment. We will also continue to look into the feasibility of exploring potential explanations for the low behavioral responses during the pandemic, including fluctuations in the strength of the pandemic or in job availability, and availability of funding from other programs.

# References


Angrist, Josh and Guido Imbens. 1995. "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity." *Journal of the American Statistical Association* 90(430): 431-442.

Barreca, Alan I., Jason M. Lindo, and Glen R. Waddell. 2016. "Heaping-Induced Bias in Regression-Discontinuity Designs." *Economic Inquiry* 54 (1): 268–93. https://doi.org/10.1111/ecin.12225.

Bell, Alex, T. J. Hedin, Peter Mannino, Roozbeh Moghadam, Carl Romer, Geoffrey C. Schnorr, and Till von Wachter. 2022. "Estimating the Disparate Cumulative Impact of the Pandemic in Administrative Unemployment Insurance Data." *AEA Papers and Proceedings* 112 (May): 78–84. https://doi.org/10.1257/pandp.20221008.

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): 1511–60. https://doi.org/10.1162/qjec.2007.122.4.1511.

Card, David, Andrew Johnston, Pauline Leung, Alexandre Mas, and Zhuan Pei. 2015. "The Effect of Unemployment Benefits on the Duration of Unemployment Insurance Receipt: New Evidence from a Regression Kink Design in Missouri, 2003-2013." Working Paper 20869. Working Paper Series. National Bureau of Economic Research. https://doi.org/10.3386/w20869.

Card, David, Jochen Kluve, and Andrea Weber. 2018. "What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations." *Journal of the European Economic Association* 16 (3): 894–931. https://doi.org/10.1093/jeea/jvx028.

Card, David, David S. Lee, Zhuan Pei, and Andrea Weber. 2015. "Inference on Causal Effects in a Generalized Regression Kink Design." *Econometrica* 83 (6): 2453–83. https://doi.org/10.3982/ECTA11224.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2019. "A Practical Introduction to Regression Discontinuity Designs: Foundations." *Elements in Quantitative and Computational Methods for the Social Sciences*, November. https://doi.org/10.1017/9781108684606.

Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. "Do Labor Market Policies Have Displacement Effects? Evidence from a Clustered Randomized Experiment *." *The Quarterly Journal of Economics* 128 (2): 531–80. https://doi.org/10.1093/qje/qjt001.

Dahl, Gordon, and Matthew M. Knepper. 2022. "Unemployment Insurance, Starting Salaries, and Jobs." Working Paper. Working Paper Series. National Bureau of Economic Research. https://doi.org/10.3386/w30152.

Ganong, Peter, and Simon Jäger. 2018. "A Permutation Test for the Regression Kink Design." *Journal of the American Statistical Association* 113 (522): 494–504. https://doi.org/10.1080/01621459.2017.1328356.

Ganong, Peter, and Pascal Noel. 2019. "Consumer Spending during Unemployment: Positive and Normative Implications." *American Economic Review* 109 (7): 2383–2424. https://doi.org/10.1257/aer.20170537.

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

Johnston, Andrew C., and Alexandre Mas. 2018. "Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut." *Journal of Political Economy* 126 (6): 2480–2522. https://doi.org/10.1086/699973.

Landais, Camille. 2015. "Assessing the Welfare Effects of Unemployment Benefits Using the Regression

Kink Design." *American Economic Journal: Economic Policy* 7 (4): 243–78. https://doi.org/10.1257/pol.20130248.
Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018. "A Macroeconomic Approach to Optimal Unemployment Insurance: Applications." *American Economic Journal: Economic Policy* 10 (2): 182–216. https://doi.org/10.1257/pol.20160462.
Leung, Pauline, and Christopher O'Leary. 2020. "Unemployment Insurance and Means-Tested Program Interactions: Evidence from Administrative Data." *American Economic Journal: Economic Policy* 12 (2): 159–92. https://doi.org/10.1257/pol.20170262.
Levine, Phillip B. 1993. "Spillover Effects between the Insured and Uninsured Unemployed." *ILR Review* 47 (1): 73–86. https://doi.org/10.1177/001979399304700106.
Marinescu, Ioana. 2017. "The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board." *Journal of Public Economics* 150 (June): 14–29. https://doi.org/10.1016/j.jpubeco.2017.02.012.
O'Leary, Christopher, Robert Spiegelman, and Kenneth Kline. 1993. "Reemployment Incentives for Unemployment Insurance Beneficiaries: Results from the Washington Reemployment Bonus Experiment." *Upjohn Institute Working Papers*, August. https://doi.org/10.17848/wp93-22.

**Table 1: Sample Summary Statistics, 2014-2019**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Full Sample | Full Sample within 5k BW | Limit Sample No Bunching | Limit Sample, No Bunching, 5k BW |
| Age | 40.1 | 40.4 | 41.1 | 40.2 |
| Female | 0.45 | 0.43 | 0.45 | 0.46 |
| Race/Ethnicity | | | | |
| Asian | 0.09 | 0.09 | 0.12 | 0.11 |
| Black | 0.09 | 0.08 | 0.08 | 0.08 |
| Hispanic | 0.42 | 0.45 | 0.36 | 0.41 |
| White | 0.31 | 0.29 | 0.36 | 0.32 |
| Native American/Alaskan Indian | 0.01 | 0.01 | 0.01 | 0.01 |
| Missing Race | 0.08 | 0.08 | 0.07 | 0.07 |
| Educational Attainment | | | | |
| HS or Less | 0.49 | 0.49 | 0.40 | 0.44 |
| Some College/Associate's Deg. | 0.31 | 0.33 | 0.34 | 0.36 |
| Bachelor's or More | 0.19 | 0.16 | 0.25 | 0.19 |
| Missing Educ. | 0.01 | 0.01 | 0.01 | 0.01 |
| Sample/Claim Characteristics | | | | |
| In Limit Sample No Bunch | 0.43 | 0.46 | 1.00 | 1.00 |
| PBD < 26 | 0.28 | 0.22 | 0.00 | 0.00 |
| Claim DQ'd | 0.15 | 0.15 | 0.00 | 0.00 |
| Any Fraud | 0.00 | 0.00 | 0.00 | 0.00 |
| Last Claim Within 2 Years | 0.32 | 0.32 | 0.00 | 0.00 |
| Round Number HQW | 0.01 | 0.01 | 0.00 | 0.00 |
| PBD (No Extensions) | 23.8 | 24.5 | 26.0 | 26.0 |
| Earnings in qtr before claim | 9,712 | 8,184 | 13,462 | 9,133 |
| High Quarter Wage | 12,985 | 10,508 | 17,180 | 10,821 |
| Alt. Base Period | 0.04 | 0.02 | 0.00 | 0.00 |
| N | 6,948,036 | 2,972,360 | 2,962,270 | 1,369,608 |

*Notes:* Summary statistics are presented for our full sample of UI claimants during the pre-pandemic expansion (Column 1), those claimants within the $5,000 bandwidth used for analysis (Column 2), our core sample restriction, (Column 3). and our core sample restriction within the $5,000 bandwidth (Column 4). The "Limit Sample, no Bunching" restriction includes only claimants with a 26 week PBD, no claim disqualifications, no prior claims within 2 years, and not having a HQW that is within $1 of a multiple of $1,000.

**Table 2: Main Estimates of Labor Supply Effects of UI Benefit Increases at Kink in WBA Schedule by Time Period**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2000s Expansion | Great Recession | Pre-Pandemic Expansion | Early Pandemic |
| | | Total UI Duration | | |
| Marg. Effect of $10 WBA Increase | 0.266*** | 0.548*** | 0.179*** | 0.106*** |
| | (0.004) | (0.009) | (0.004) | (0.010) |
| Implied Elasticity | 0.619*** | 0.690*** | 0.497*** | 0.171*** |
| | (0.010) | (0.012) | (0.012) | (0.016) |
| | | 8 Week Survival | | |
| Marg. Effect of $10 WBA Increase | 0.008*** | 0.006*** | 0.006*** | 0.002*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Implied Elasticity | 0.469*** | 0.381*** | 0.404*** | 0.101*** |
| | (0.009) | (0.008) | (0.011) | (0.010) |
| N | 1,899,528 | 1,911,492 | 1,369,607 | 748,463 |

*Notes:* Outcomes are either the number of weeks that the claimant received UI benefits before a gap of 2 or more unpaid weeks (Total UI Duration) or an indicator variable for the claimant continuing to receive UI benefits 8 weeks past the start of their claim (8-week Survival). Each estimate uses the same IV model, where the instrument is the slope-change in the relationship between WBA and HQW at the cutoff. Sample limited to claims with high-quarter wages within 5,000 dollars of the relevant max WBA cutoff (11,674.01 dollars). *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. All models use heteroskedasticity robust standard errors. The "2000s Expansion" period includes claimants with BYB dates (benefit years beginning) between December 2001 and the end of 2007. The Great Recession period includes claimants with BYBs between 2008 and the end of 2013. The Pre-Pandemic Expansion period includes claimants with BYBs between 2014 and the end of 2019. The "Early Pandemic" period includes claimants with BYBs in the last 2 weeks of March 2020.

**Table 3: Sensitivity of Regression Kink Estimates of Labor Supply Effects of Increases in UI Benefits to Bandwidth and Specification Choice**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Ad-hoc BW; Linear IV | Optimal BW; Linear IV | Ad-hoc BW; Quadratic | CIT Conventional Linear | CIT Bias-Corrected Linear | CIT Bias-Corrected Linear + Robust SE | CIT Conventional Quadratic | CIT Bias-Corrected Quadratic | CIT Bias-Corrected Quadratic + Robust SE |
| | | | | Total UI Duration (Winsorized) | | | | | |
| Marginal Effect ($10 WBA) | 0.151*** | 0.137*** | 0.097*** | 0.129*** | 0.118*** | 0.118*** | 0.127*** | 0.112*** | 0.112*** |
| | (0.004) | (0.009) | (0.014) | (0.012) | (0.012) | (0.019) | (0.007) | (0.007) | (0.015) |
| Implied Elasticity | 0.450*** | 0.408*** | 0.289*** | 0.385*** | 0.351*** | 0.351*** | 0.378*** | 0.333*** | 0.333*** |
| | (0.011) | (0.027) | (0.041) | (0.035) | (0.035) | (0.055) | (0.021) | (0.021) | (0.043) |
| Bandwidth | 5,000 | 2,626 | 5,000 | 2,626 | 2,626 | 2,626 | 11,080 | 11,080 | 11,080 |
| N | 1,369,607 | 707,107 | 1,369,607 | 2,369,817 | 2,369,817 | 2,369,817 | 2,369,817 | 2,369,817 | 2,369,817 |
| Effective Obs. | 1,369,607 | 707,107 | 1,369,607 | 707,107 | 707,107 | 707,107 | 2,121,823 | 2,121,823 | 2,121,823 |
| | | | | 8 Week Survival | | | | | |
| Marginal Effect ($10 WBA) | 0.006*** | 0.006*** | 0.004*** | 0.005*** | 0.005*** | 0.005*** | 0.005*** | 0.004*** | 0.004*** |
| | (0.000) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Implied Elasticity | 0.404*** | 0.395*** | 0.264*** | 0.354*** | 0.347*** | 0.347*** | 0.339*** | 0.297*** | 0.297*** |
| | (0.011) | (0.040) | (0.043) | (0.050) | (0.050) | (0.080) | (0.037) | (0.037) | (0.067) |
| Bandwidth | 5,000 | 2,107 | 5,000 | 2,107 | 2,107 | 2,107 | 6,508 | 6,508 | 6,508 |
| N | 1,369,607 | 566,243 | 1,369,607 | 1,895,856 | 1,895,856 | 1,895,856 | 1,895,856 | 1,895,856 | 1,895,856 |
| Effective Obs. | 1,369,607 | 566,243 | 1,369,607 | 566,243 | 566,243 | 566,243 | 1,650,997 | 1,650,997 | 1,650,997 |

*Notes:* This table includes claimants with BYBs in the Pre-Pandemic Expansion Period (2014-2019). Outcomes are either the number of weeks that the claimant received UI benefits before a gap of 2 or more unpaid weeks (Total UI Duration) or an indicator variable for the claimant continuing to receive UI benefits 8 weeks past the start of their claim (8-week Survival). *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. Total UI Duration has been Winsorized at the 95th percentile.

**Table 4: Sensitivity of Regression Kink Estimates of Labor Supply Effects of Increases in UI Benefits to Sample Definition (2014-2019 Claimants)**

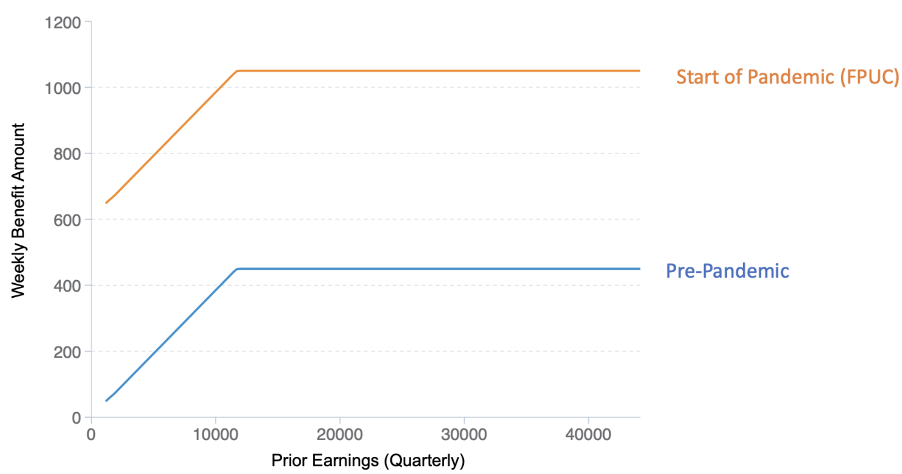
| | (1) | (2) |
|---|---|---|
| | Full Sample (5k BW) | Limit Sample No Bunch (5k BW) |
| | Total UI Duration | |
| Marginal Effect ($10 WBA) | 0.134*** | 0.179*** |
| | (0.003) | (0.004) |
| Implied Elasticity | 0.445*** | 0.497*** |
| | (0.009) | (0.012) |
| Marginal Effect ($100 MBA) | 0.115*** | 0.133*** |
| | (0.002) | (0.003) |
| Implied Elasticity | 0.495*** | 0.493*** |
| | (0.010) | (0.012) |
| | 8 Week Survival | |
| Marginal Effect ($10 WBA) | 0.005*** | 0.006*** |
| | (0.000) | (0.000) |
| Implied Elasticity | 0.419*** | 0.404*** |
| | (0.009) | (0.011) |
| Marginal Effect ($100 MBA) | 0.005*** | 0.004*** |
| | (0.000) | (0.000) |
| Implied Elasticity | 0.467*** | 0.401*** |
| | (0.010) | (0.011) |
| N | 2,972,360 | 1,369,608 |

*Notes:* This table includes claimants with BYBs in the Pre-Pandemic Expansion Period (2014-2019). Outcomes are either the number of weeks that the claimant received UI benefits before a gap of 2 or more unpaid weeks (Total UI Duration) or an indicator variable for the claimant continuing to receive UI benefits 8 weeks past the start of their claim (8-week Survival). *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. Column 1 includes all claimants with BYBs in the time period with a HQW within the $5,000 bandwidth, while column 2 is limited to our preferred sample (described in section 2.2), also with HQWs within $5,000 of the kink point.

**Figure 1: Schedules of Weekly UI Benefit as Function of Highest Quarter Earnings in the Base Period in California in Different Years**

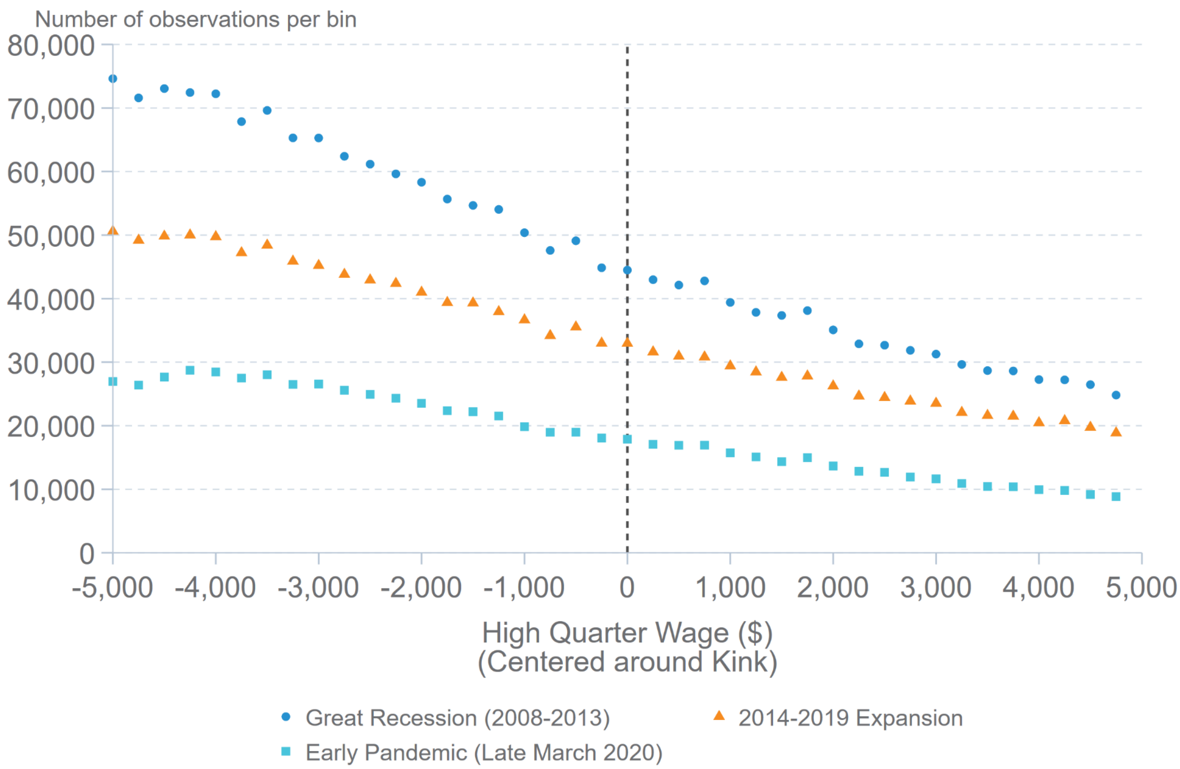
Panel A: Pre-Pandemic


Panel B: During the Pandemic



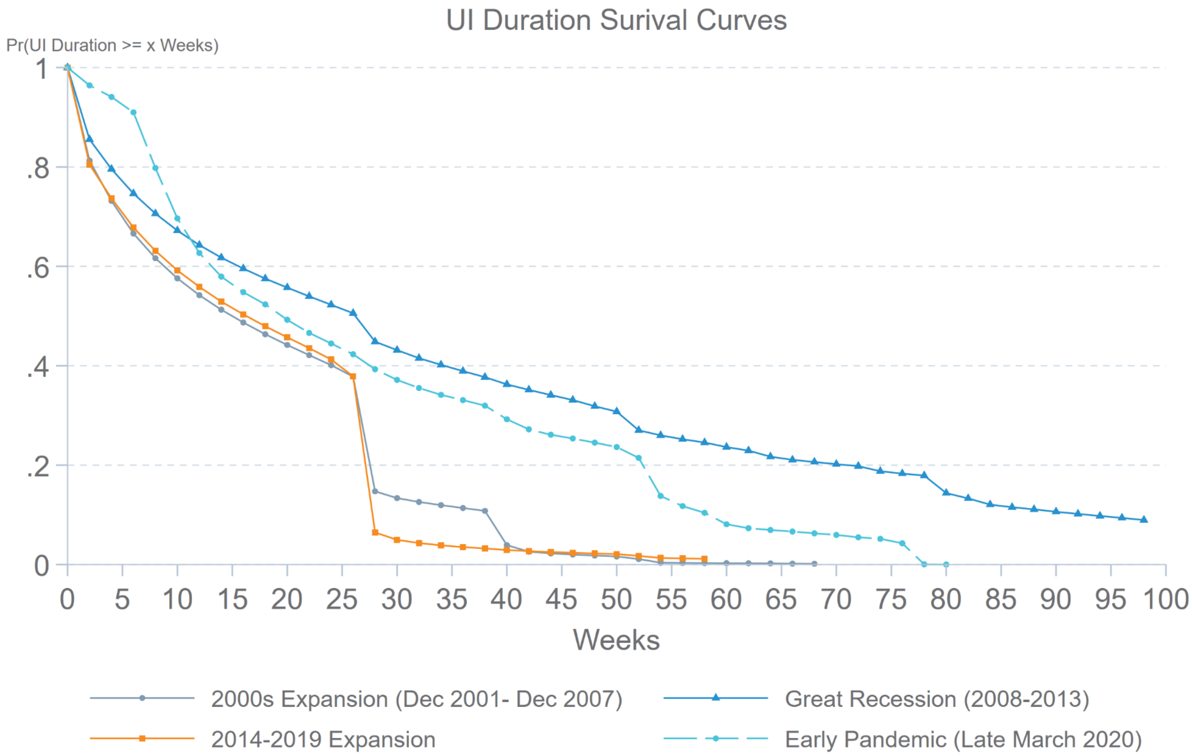

*Notes:* See Section 2 for details on how benefits are calculated. Panel B shows the Weekly benefit amount with and without the $600 FPUC benefits effective at the start of the COVID-19 pandemic.

**Figure 2: Number of Claimants In Wage Bins Above and Below UI Benefit Kink for Different Time Periods**



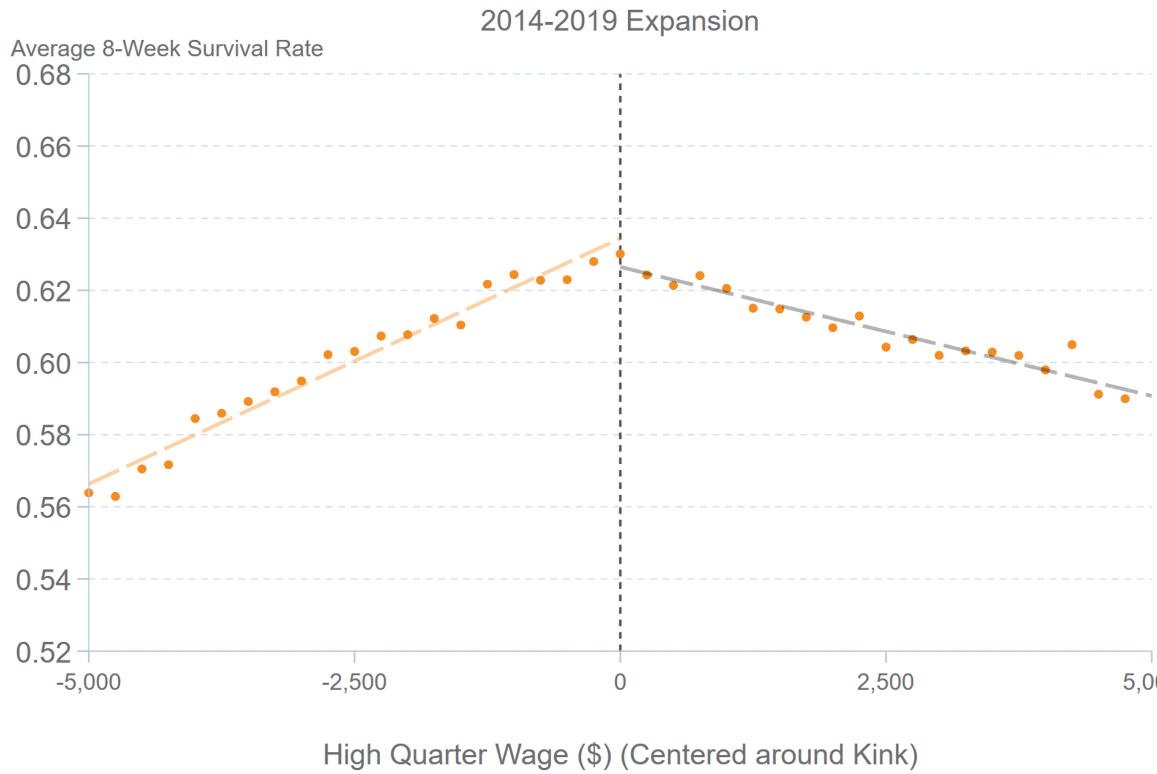

*Notes:* This graph presents a histogram of claimants by Highest Quarter Wage in the Base Period for our core analysis sample.

**Figure 3: Weekly Probability of Remaining After Start of UI Spell (Survival Curve) for Workers Starting New UI Spells in Different Different Time Periods**



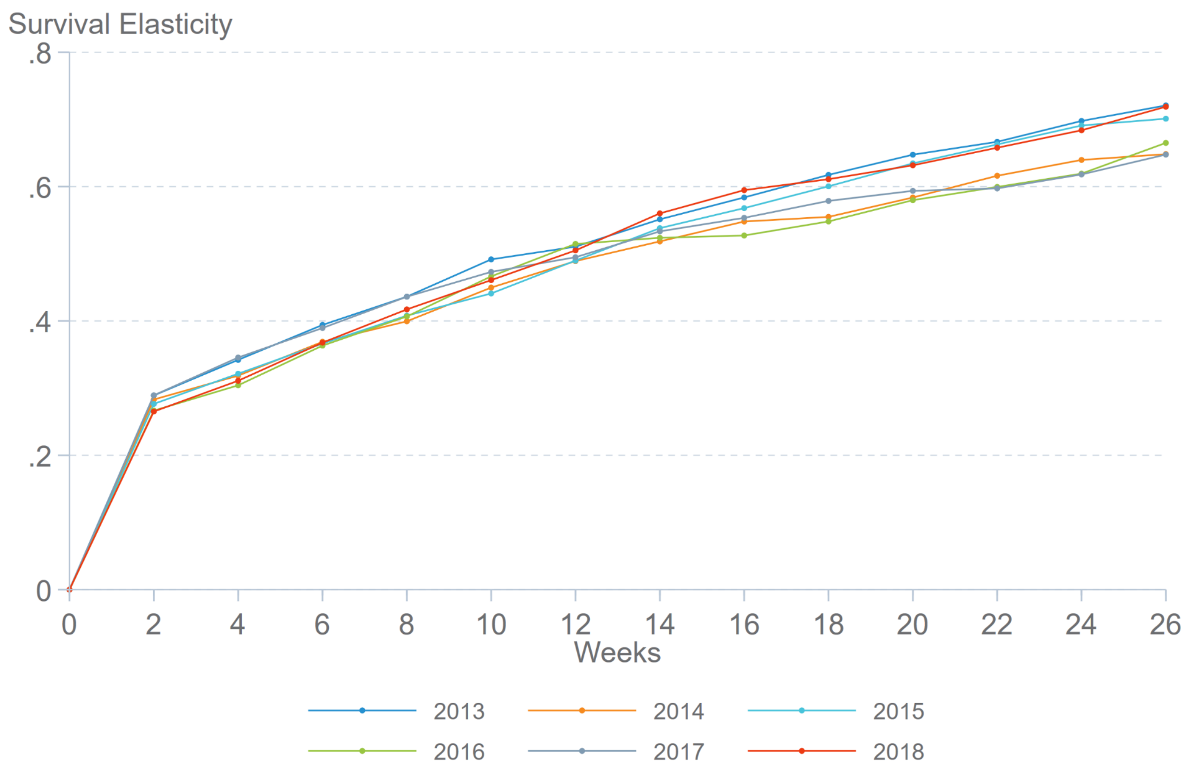

*Notes:* This graph presents survival curves of claimants for our core analysis sample for various BYB ranges.

**Figure 4: Responses in Probability of Remaining on UI 8 Weeks After Start of UI Spell (8-Week Survival Rate) Around the UI Benefit Kink, 2014-2019 Expansion**



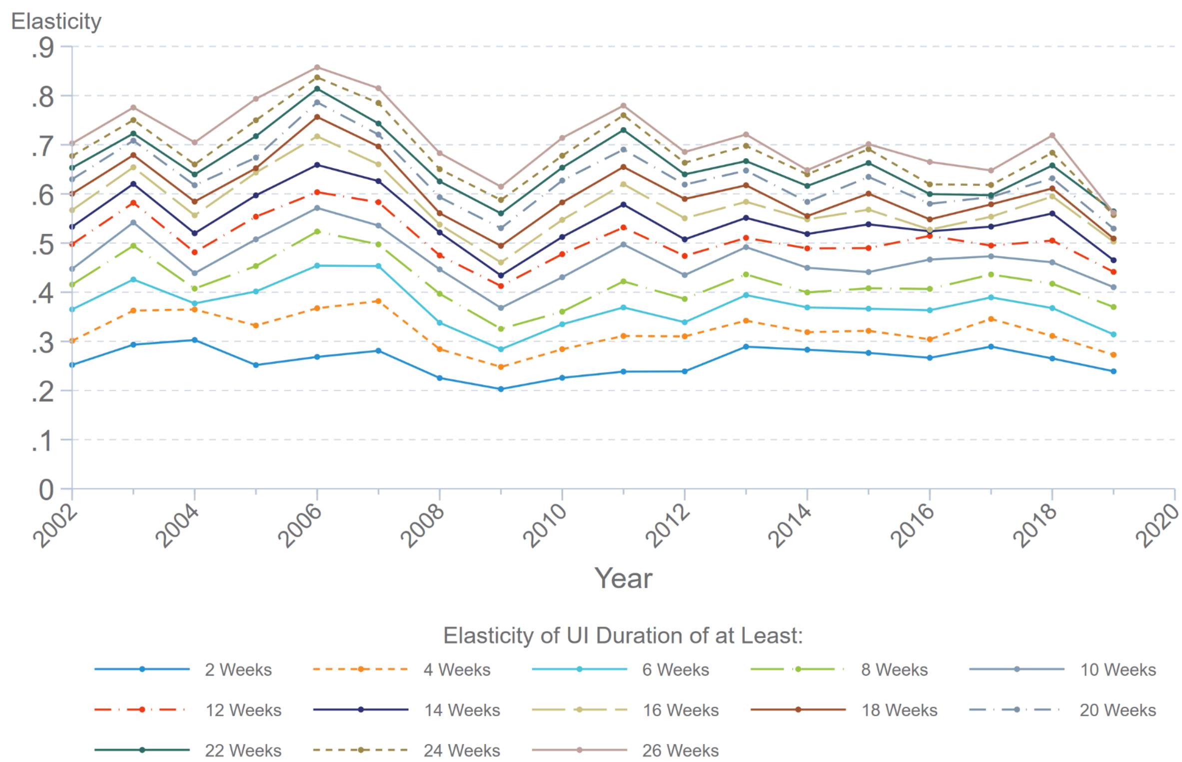

*Notes:* This graph shows eight-week survival as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is -0.0000216. The sample is our core analysis sample restricted to 2014-2019 BYB.

**Figure 5: Percentage Change in the Probability of Remaining on UI by Week of UI Spell Due to a One-Percent Change in UI Benefits Estimated at UI Benefit Kink for Claimants Starting New UI Spells in Different Calendar Years, Expansion Period**



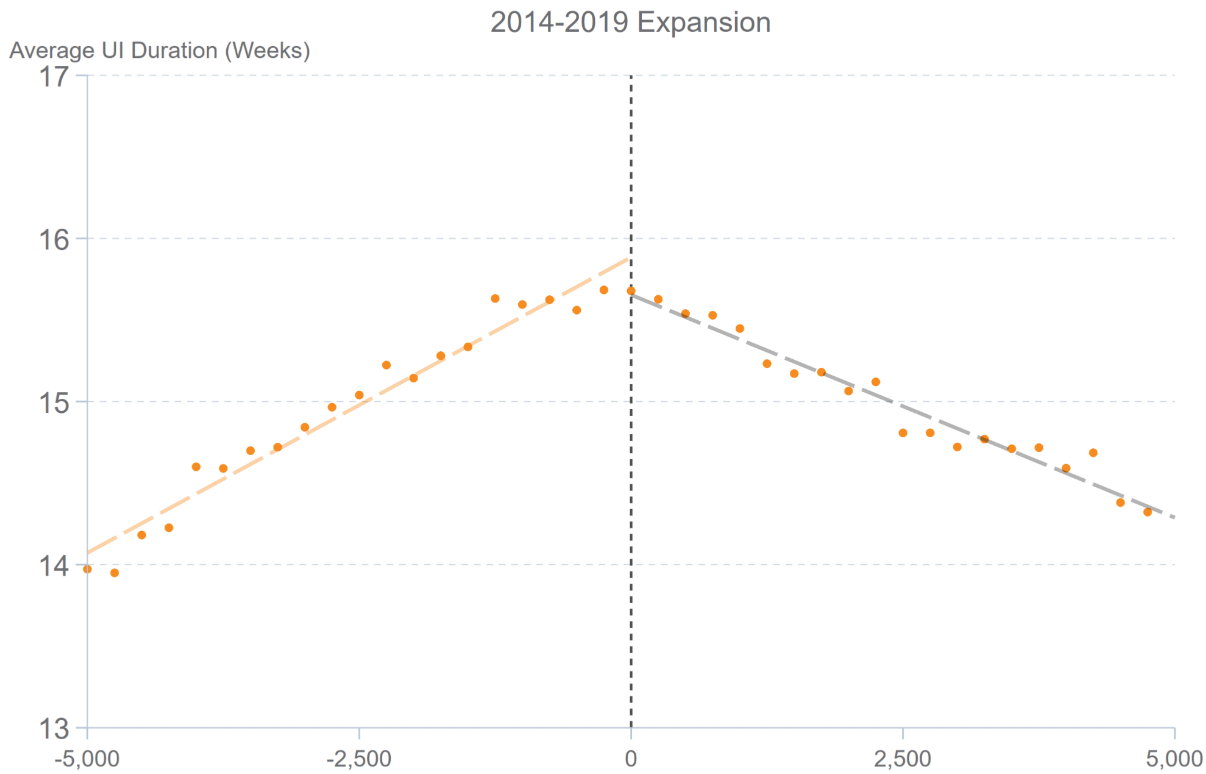

*Notes:* This graph presents survival elasticities by week for our core analysis sample restricted to claimants with a BYB in each year between 2013-2018.

**Figure 6: Percentage Change in the Probability of Remaining on UI by Week of UI Spell Due to a One-Percent Change in UI Benefits Estimated at UI Benefit Kink for Claimants Starting New UI Spells Different Calendar Years, 2002-2019**



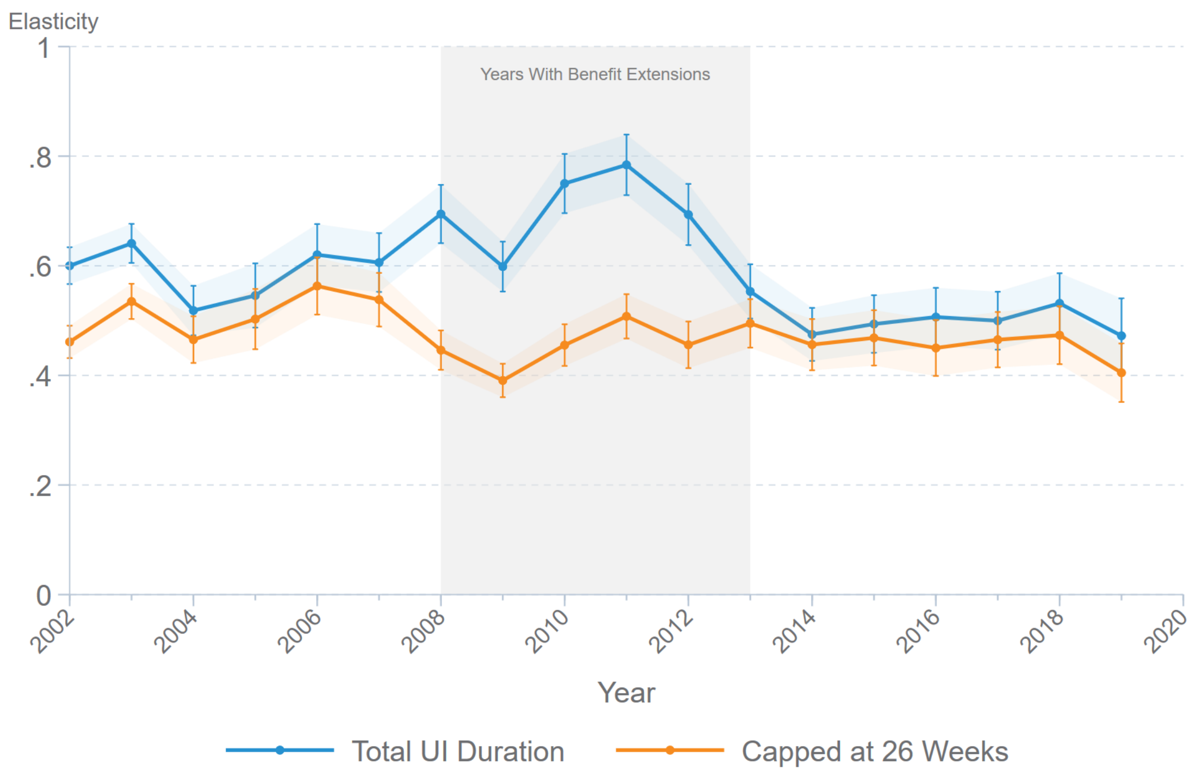

*Notes:* This graph presents survival elasticities by week and year of BYB for our core analysis sample.

**Figure 7: Responses in Average UI Duration in Weeks Around the Kink in Benefit Schedule, 2014-2019 Expansion**



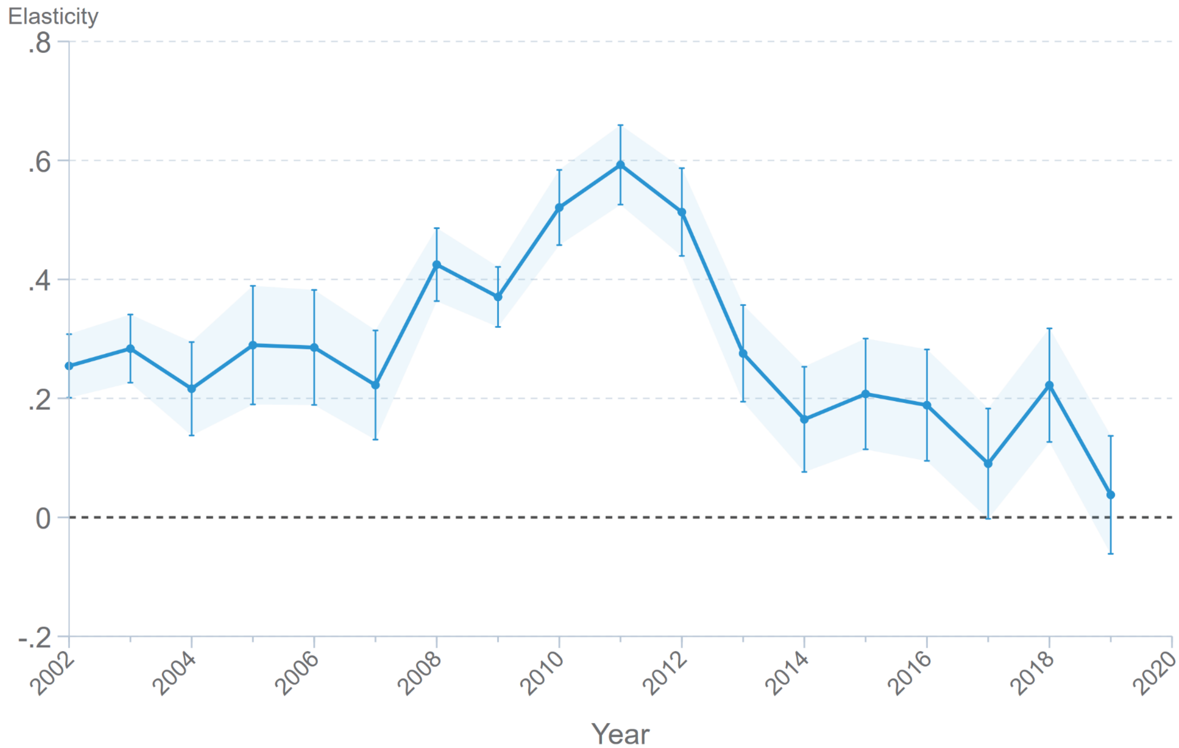

*Notes:* This graph shows average duration as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is -0.00637, which implies an elasticity (with respect to WBA) of 0.497

**Figure 8: Percent Increase in UI Durations in Weeks from a One-Percent Increase in UI Benefits (Elasticity) Estimated at the UI Benefit Kink**



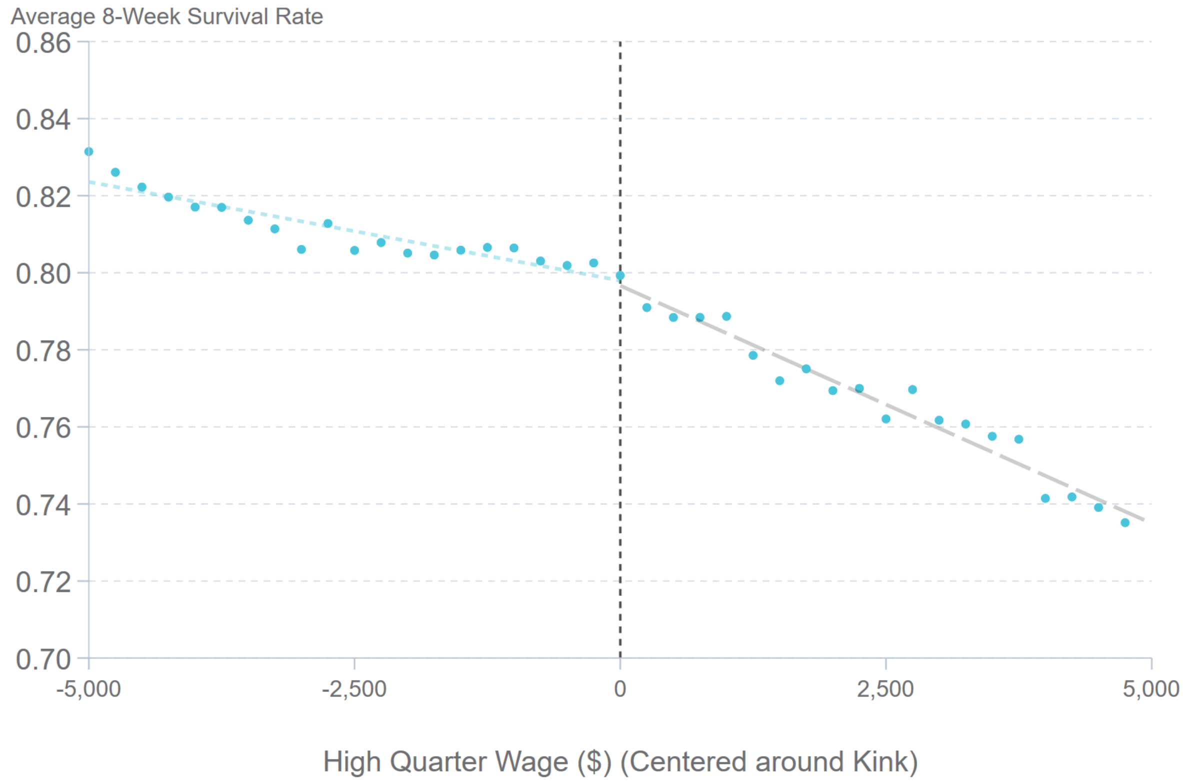

*Notes:* This graph presents duration elasticities by benefit year for our core analysis sample. The orange line shows estimates where total UI duration is capped at 26 weeks, whereas the blue line shows estimates under the actual durations.

**Figure 9: Percent Increase in Duration of Nonemployment Spell in Calendar Quarters from a One-Percent Increase in UI Benefits (Elasticity) Estimated at the UI Benefit Kink**



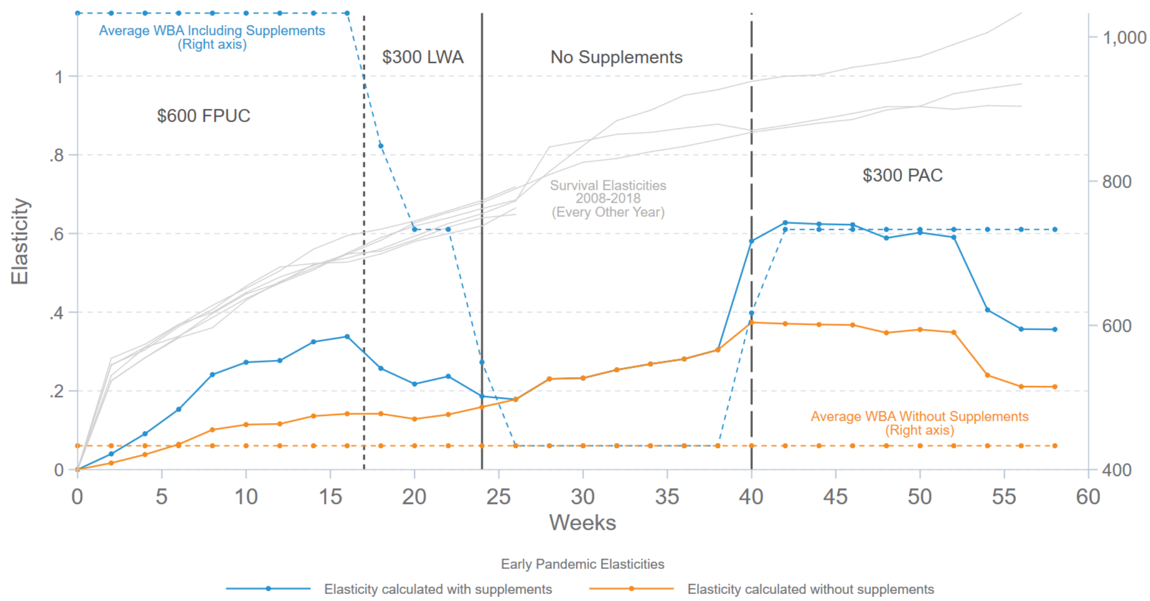

*Notes:* This graph presents non-employment elasticities by benefit year for our core analysis sample. Nonemployment duration has been capped at 4 quarters.

**Figure 10: Responses in Probability of Remaining on UI 8 Weeks After Start of UI Spell (8-Week Survival Rate) Around the UI Benefit Kink, Early Pandemic**



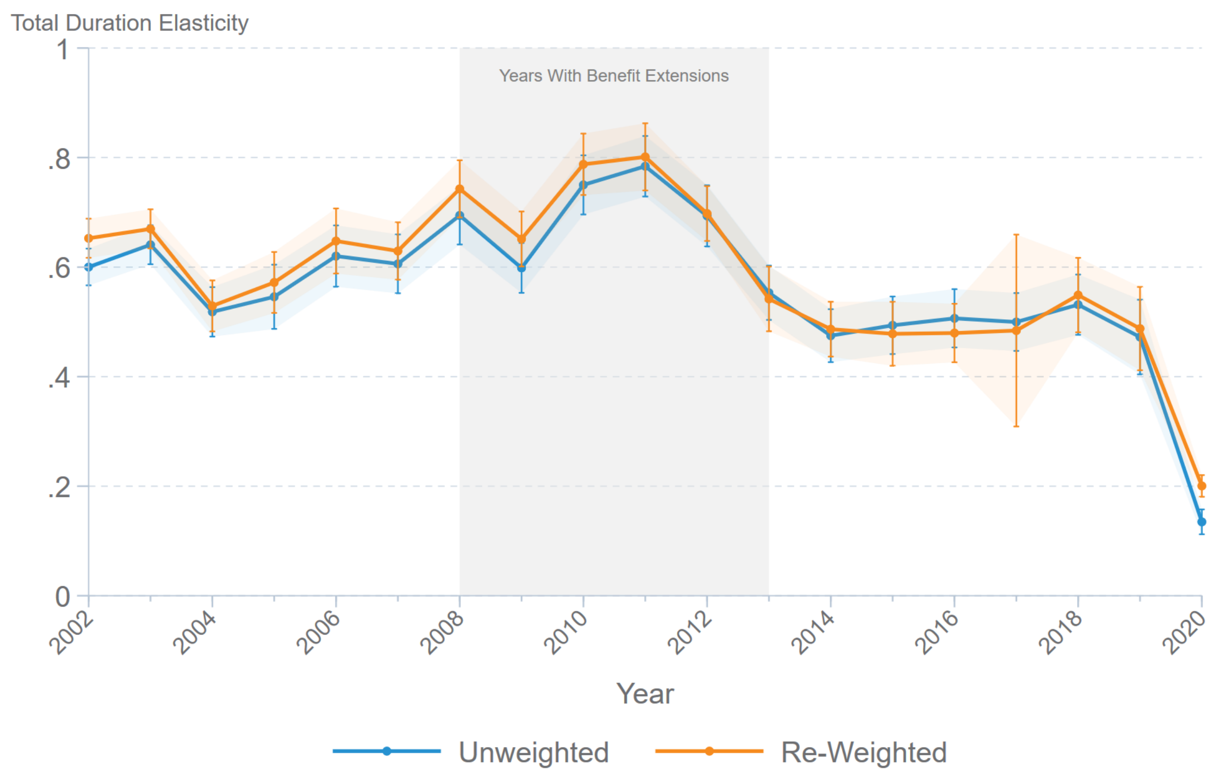

*Notes:* This graph shows 8-week survival rate as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is 0.00000689. The sample is claimants starting a new benefit year in the last two weeks of March of 2020.

**Figure 11: Percentage Change in the Probability of Remaining on UI by Week of UI Spell Due to a One-Percent Change in UI Benefits Estimated at UI Benefit Kink, Early Pandemic, With and Without Added Benefits**




*Notes:* This figure shows *n*-week survival estimates for two early pandemic cohorts (last 2 weeks of March 2020) in our core analysis sample using two different approaches. The solid blue line represents a calculation using the WBA estimate that includes federal supplements that were available to each worker in the given week. The solid orange line represents the same calculation but WBA is calculated without the supplements. Gray lines represent survival elasticities during previous periods. The vertical lines indicate when FPUC turned off for each of the two cohorts., and the dashed blue and orange lines indicate the average WBA of claimants when accounting for supplements in that calendar week and without accounting for supplements.

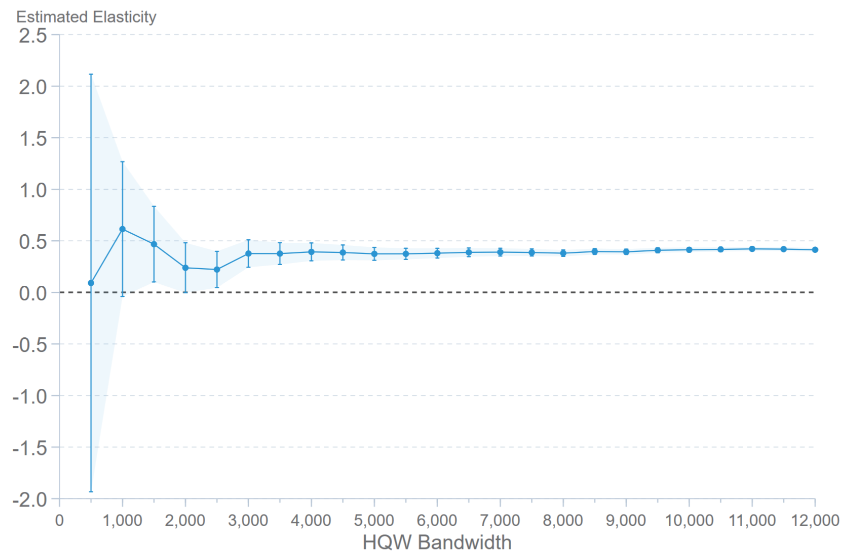
**Figure 12: Inverse Propensity Score Weighted Estimates of the Percent Increase in UI Durations in Weeks from a One-Percent Increase in UI Benefits (Elasticity) Estimated at the UI Benefit Kink**



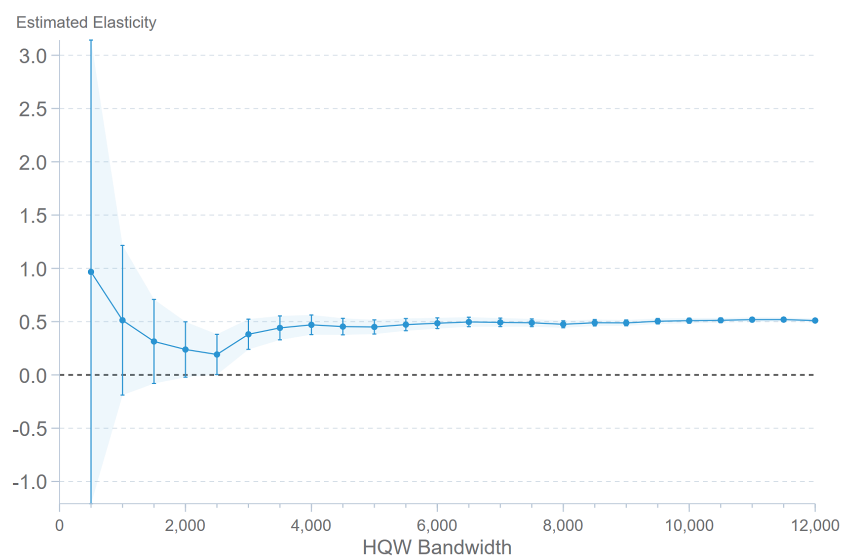

*Notes:* The orange line uses a probit model to estimate the probability of each claimant having a BYB in the year 2009, based on their observable characteristics (age, gender, industry, race, education, citizenship, recall expectations, separation reason, tenure, and the characteristics of the separating firm). We then estimate the duration elasticity year-by-year, re-weighting the claimants in each sub-sample according to their propensity score, so that in each year the composition of the sample is similar to that of the sample in 2009. Total Duration Elasticity refers to the number of weeks that the claimant received UI benefits before a gap of 2 or more unpaid weeks. The difference in the unweighted vs weighted estimate for 2009 arises from the weighted sample only including claimants with non-missing values for each variable used in the probit regression, a restriction not required for the unweighted estimate.

**Figure 13: Robustness of Main Estimates to Varying Bandwidth**

Panel A: Survival Elasticity

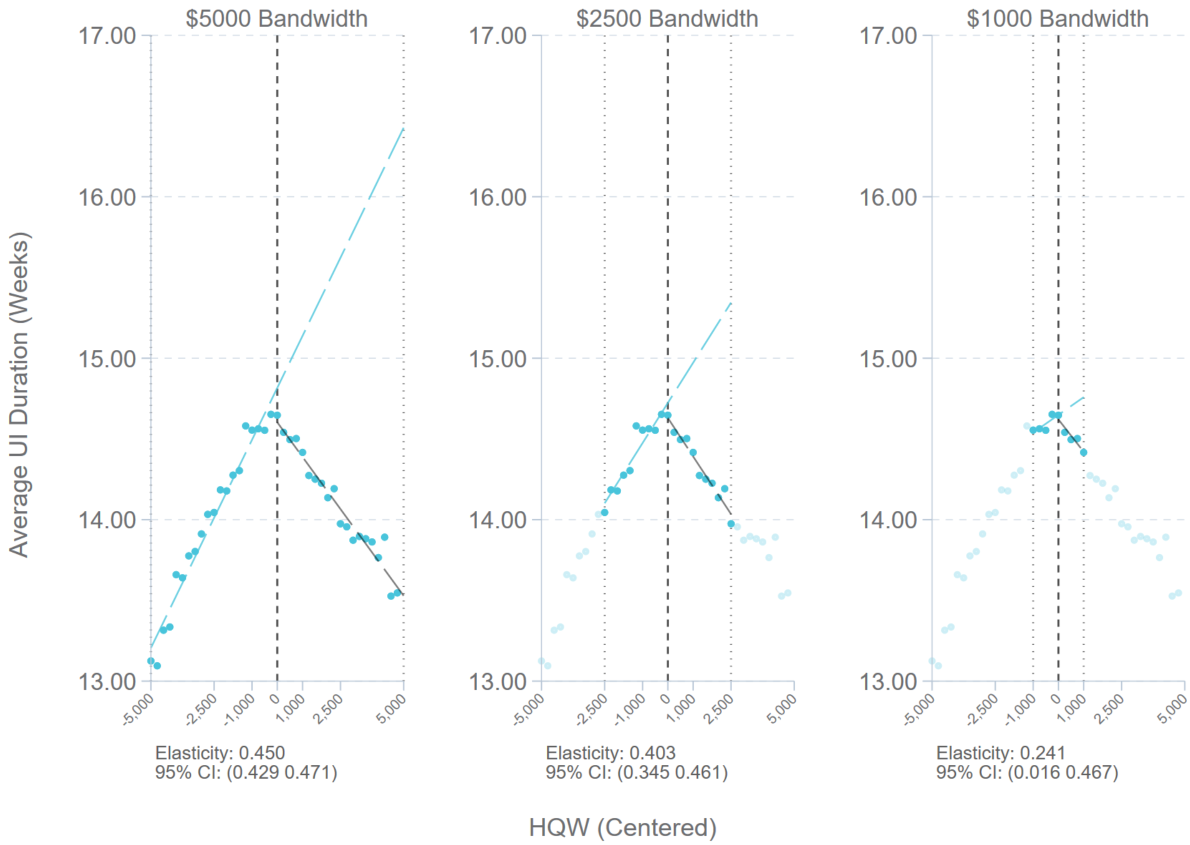



Panel B: Duration Elasticity



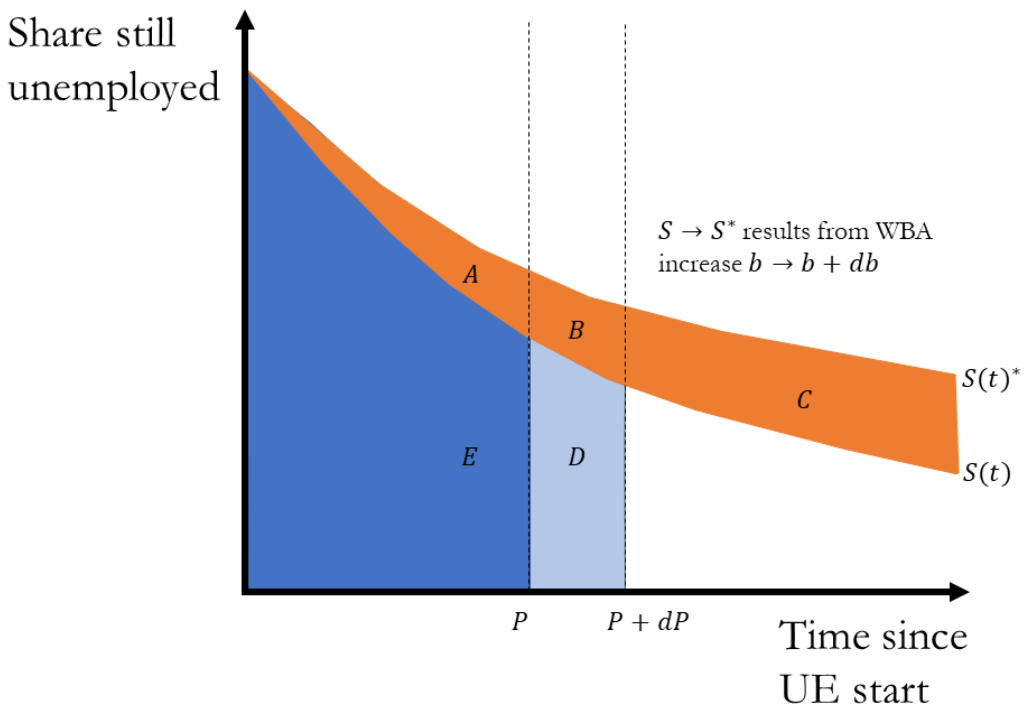

*Notes:* This figure presents elasticity estimates for our core analysis sample restricted to 2014-2019 as a function of the RKD bandwidth. Panel A depicts eight-week survival elasticities, whereas Panel B uses full duration. Bandwidth refers to the distance (in dollars) from the kink point to each edge of the sample.

**Figure 14: Graphical Evaluation of Bandwidth Performance**




*Notes:* This figure illustrates the potential bias-variance trade-off by estimating the RKD under three different bandwidths. While narrower bandwidths are able to reduce any smoothing bias which may arise if the underlying function is nonlinear, by using a smaller sample, they produce noisier estimates. Wider bandwidths tend to have lower variance, but if the underlying function (here, illustrated by the dots representing the average UI duration for all claimants within a $250 HQW bin) is nonlinear, by including data further away from the kink point, they can introduce more bias.

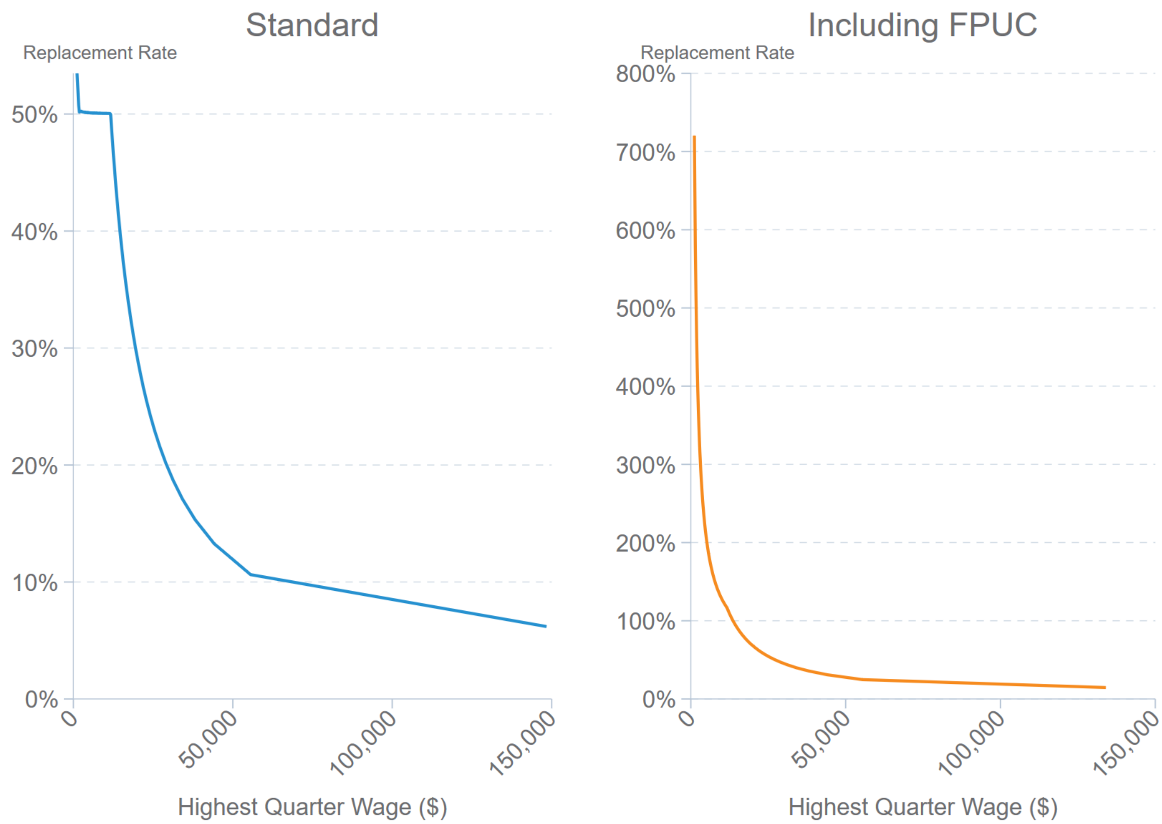
**Figure 15: Illustration of Welfare Effects of Increase in UI Benefits in Presence of Changes in Potential Benefit Durations**




*Notes:* Figure displays two survival curves—one before (S(t)) a benefit level (b) increase (db) and one after (S*(t))---the "behavioral" and "mechanical" cost of this benefit increase, and how those costs vary with PBD (lower PBD, P, vs higher PBD, P + dP). The behavioral cost of db consists of the increase in UI benefits paid due to the moral hazard responses (shaded area in orange to the left of the relevant PBD, scaled by db) plus the lost tax revenue due to longer non-employment spells (entire area shaded orange scaled by the relevant tax revenue per period). The mechanical cost of db is shaded in blue, and represents the $ transfer to the unemployed in the absence of any behavioral response (i.e., if the two survivor curves were identical). While the magnitude of the shift in the survivor curve is shown to be the same regardless of PBD, this may or may not be the case. However the mechanical transfer clearly increases (from E to E + D), and the portion of the behavioral cost that corresponds to additional UI benefits paid will be larger when PBD is higher (A + B when PBD = P + dP, vs. A when PBD = P), unless the shift in the survivor curves there is sufficiently smaller when PBD is high.

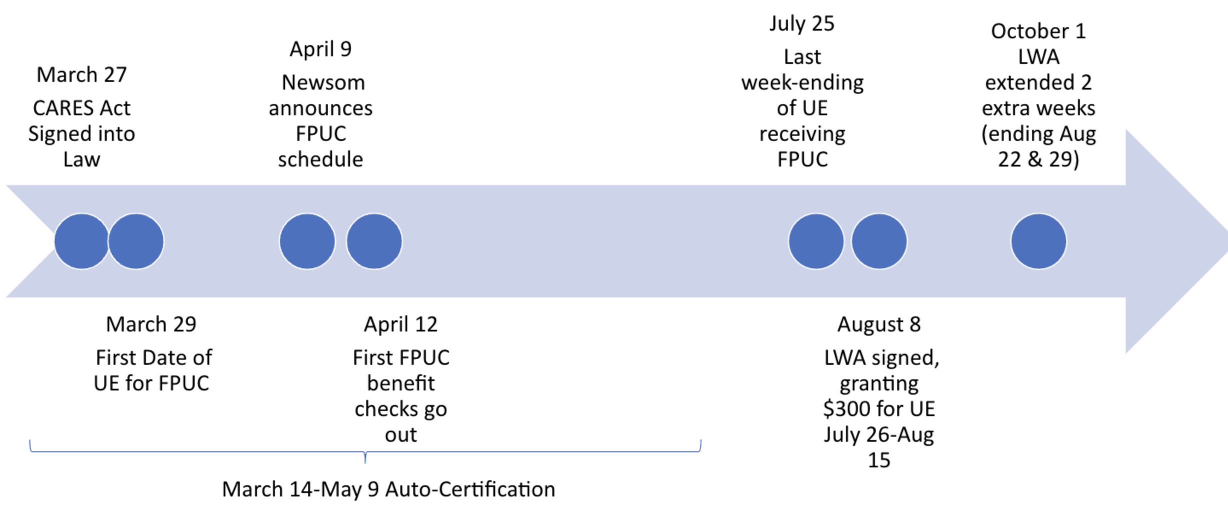
# 1. Appendix Exhibits

## Figure A1: UI Benefit Generosity in California (Replacement Rates)



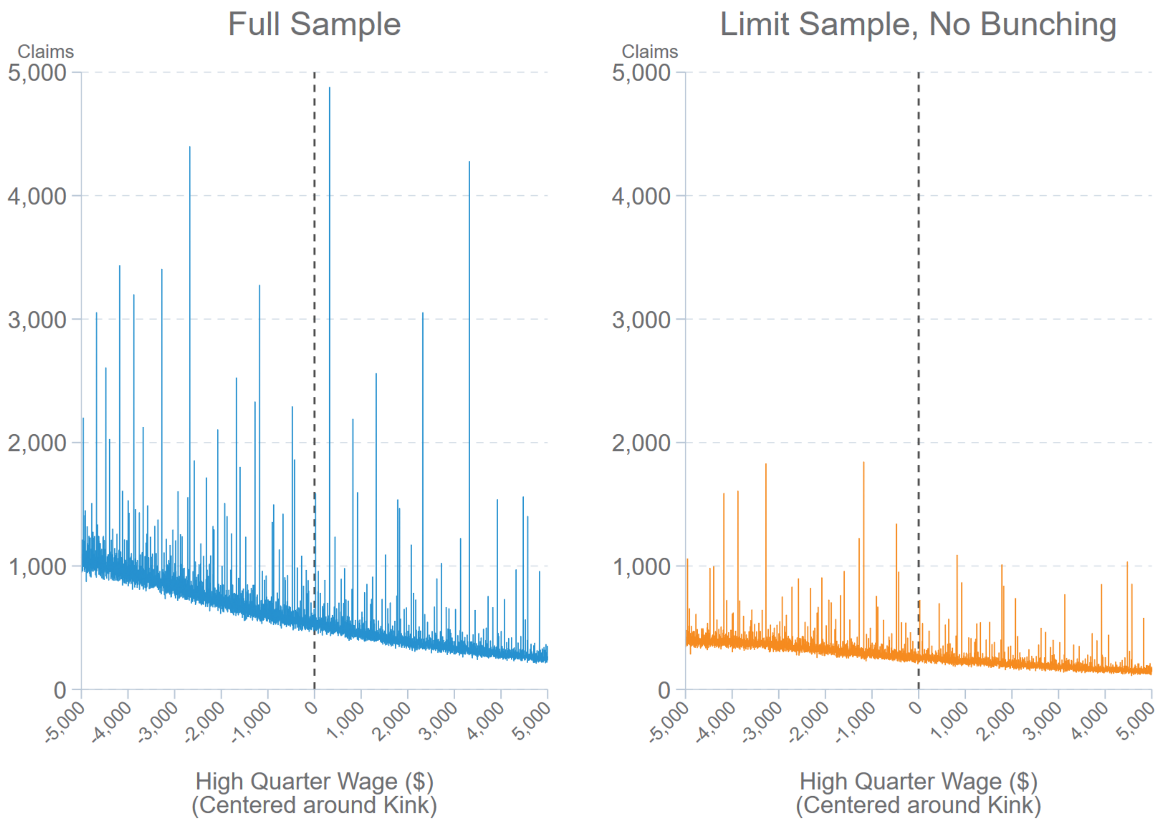

*Notes:* This figure shows the relationship between the Highest Quarter Wage (HQW) in a claimants base period (prior to filing their claim) and their assigned weekly benefit amount (WBA), with and without the $600 pandemic FPUC supplement. From the HQW, we can infer an average weekly pay amount by dividing by 13, and by comparing this to the WBA (with and without FPUC) we compute the Replacement Rate, the share of average weekly earnings that UI benefits replaces.

## Figure A2: Timeline of Early Pandemic UI Expansions in CA



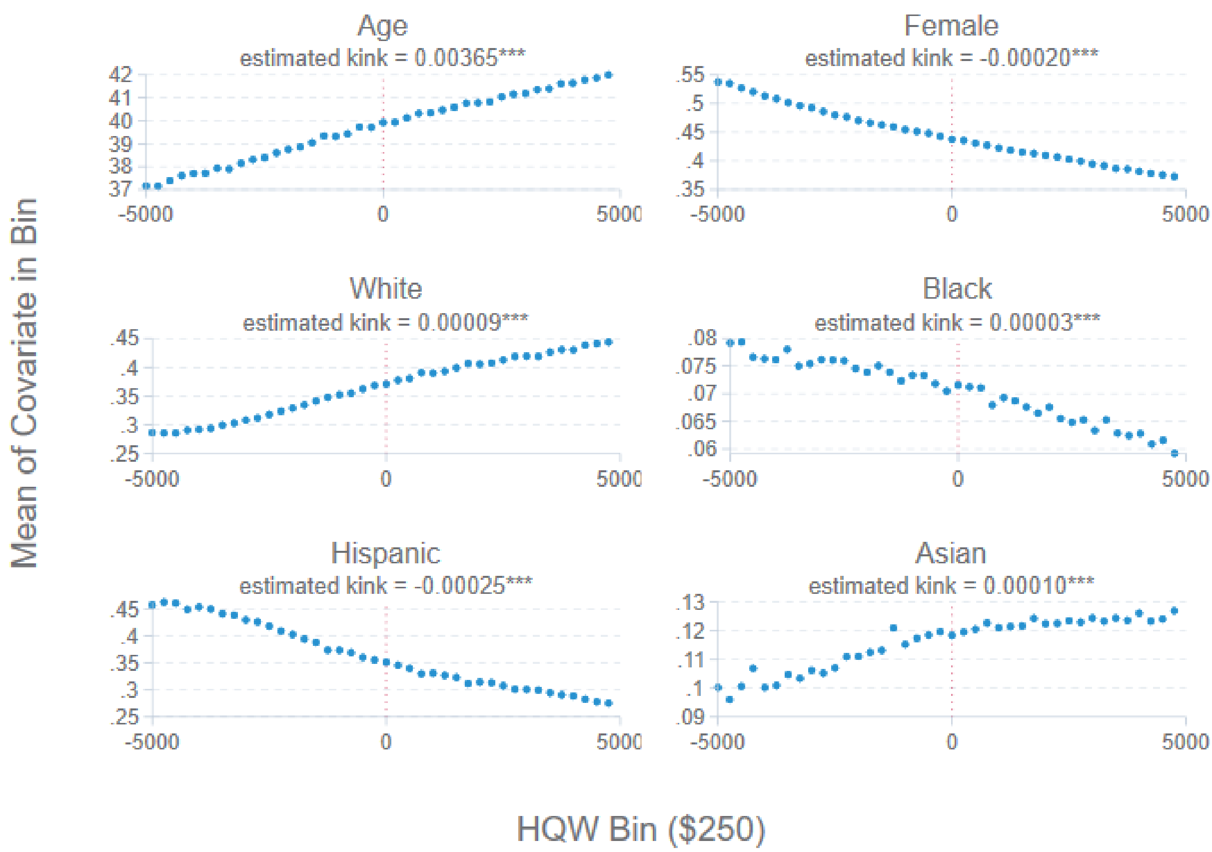

*Notes:* This diagram illustrates the key dates for various pandemic-era (2020) supplement programs.

# Figure A3: Density Around Kink




*Note:* This figure plots the number of claimants in each $2 bin of HQW under the full sample (with no restrictions) and our preferred sample (described in section 2.3).

## Figure A4: Smoothness of Covariates Through Cutoff, Claimant Demographics

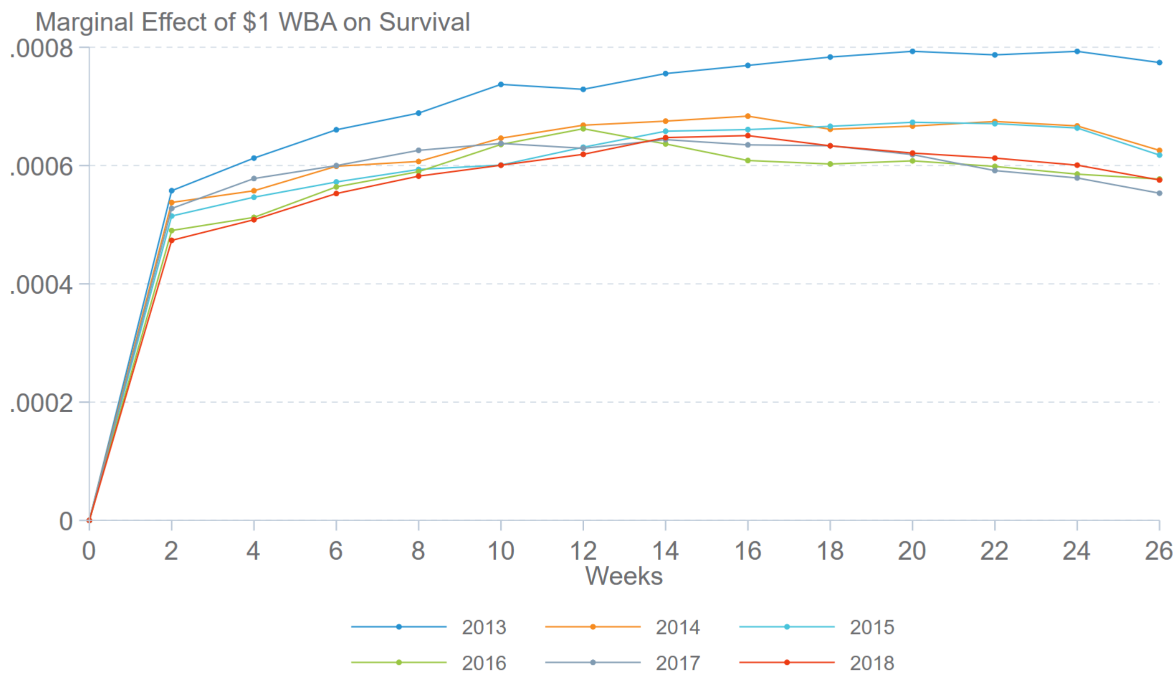



*Notes:* Each panel displays a binned scatter plot of covariate means (y-axis) against the running variable (HQW, high quarter earnings) centered at the cutoff. Subtitles display estimates of the slope change at the cutoff from regressions analogous to our main RKD specification, with the covariate as the outcome. Age, gender, and race/ethnicity are all self-reported by the claimant to EDD when the claim is filed. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A5: Smoothness of Covariates Through Cutoff, Firm Characteristics and Tenure


*Notes:* Each panel displays a binned scatter plot of covariate means (y-axis) against the running variable (HQW, high quarter earnings) centered at the cutoff. Subtitles display estimates of the slope change at the cutoff from regressions analogous to our main RKD specification, with the covariate as the outcome.Firm characteristics are from the QCEW and apply to the separating employer in the quarter of the claimant's BYB. Tenure is calculated from the earnings data and includes all quarters up to and including the quarter of the claimant's BYB in which the claimant had any earnings from the employer. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A6: Percentage Point Change in the Probability of Remaining on UI by Week of UI Spell Due to a One Dollar Increase in UI Benefits Estimated at UI Benefit Kink for Claimants Starting New UI Spells in Different Calendar Years, Expansion Period




*Notes:* This figure shows the RKD estimate of a $1 increase to WBA on the probability of remaining on UI for longer than a given number of weeks, where each line includes a different sample of claimants based on the calendar year their claim began in.

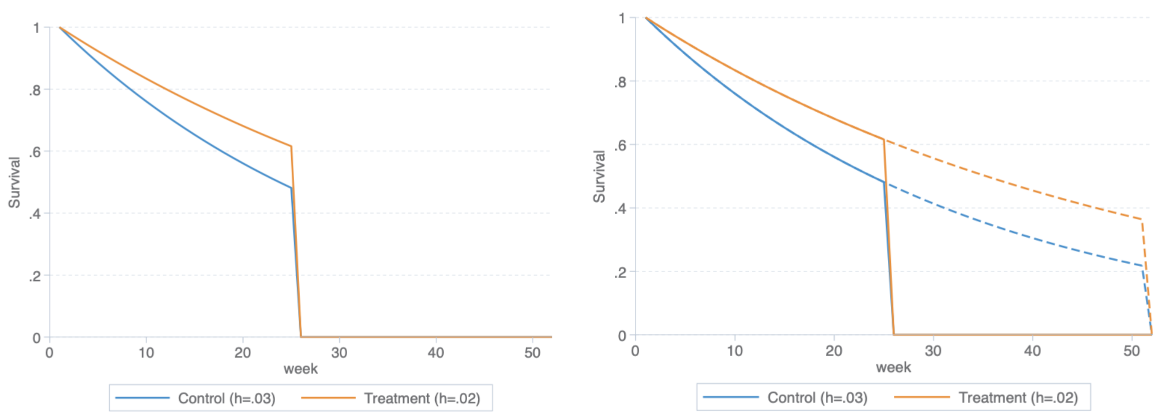
# Simulation Appendix

This appendix shows how a truncated constant hazard model generates (A) $n$-week survival elasticities that are increasing in $n$, and (B) duration elasticities that are increasing in the potential benefit duration. We adopt a truncated constant hazard model because it approximately mirrors the shapes of survival curves we observe in our data. We simulate a binary treatment for simplicity without loss of generality rather than the change in slope induced by the RKD.

Consider a treatment (such as added benefits) that reduces the weekly exit hazard ($h$) from .03 for a control group to 02 for the treated. Each group's survival share diminishes by the relevant hazard for each week from 1 to 25, inclusive. On week 26, each group's exit hazard increases to 1 to simulate exhaustion under a 26-week PBD. The figure below plots survival curves, given by $(1-h)^n$.

Set-Up: Survival Curves

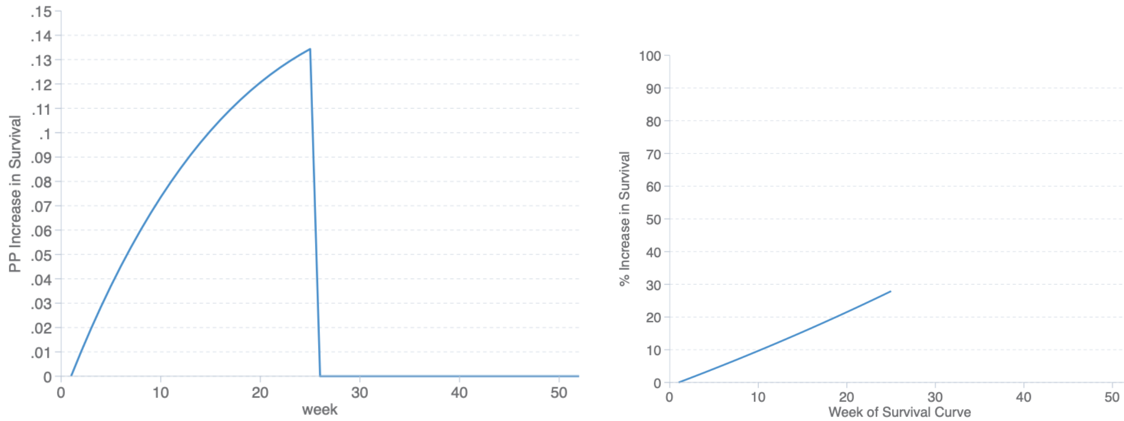
Panel A: 26-Week PBD Panel B: Simulating an extension to 52 Weeks




The first insight from the model, which mirrors our results in the claims data, is that $n$-week survival elasticities are increasing in $n$. This happens because as survival shares (in the denominator) monotonically decrease in $n$, the hazard (affecting the numerator) remains constant. The figure below shows survival elasticities as a function of the week $n$ at which survival is measured in our simulation.

Survival Elasticities Increase by Week

Panel A: PP Increase (Marginal Effect) Panel B: % Increase (Elasticity)




The second insight from the model is that duration elasticities increase in PBD. To show this, instead of fixing PBD at 26 weeks, we perturb the data-generating process by varying the PBD (i.e, the point at which *h*=1) from week 2 through week 52. (As before, prior to the final week, *h* remains the same .02 for the treated and .03 for control.) The figure below shows that as potential benefit durations increase, so does the percent difference in durations between treatment and control groups.

Duration Elasticities Increase by PBD