UI Benefit Generosity and Labor Supply from 2002-2020: Evidence from California UI Records

Alex Bell, *UCLA*

T.J. Hedin, *UCLA*

Geoffrey Schnorr, *California Policy Lab at UCLA, California Employment Development Department*

Till von Wachter, *UCLA, CEPR, NBER*

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Abstract

This paper obtains comparable estimates of the effect of unemployment insurance (UI) benefits on labor supply throughout the unemployment spell and over the business cycle using a regression kink design and 20 years of administrative data from California. For a given unemployment duration, the behavioral effect of UI benefit levels on labor supply does not vary with 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 unemployment durations.

Corresponding author: Till von Wachter tvwachter@econ.ucla.edu.

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1 Introduction

A large body of empirical work has demonstrated that more generous unemployment insurance (UI) benefits lead to longer unemployment spells.[1] However, much less is known about how the labor supply response to UI varies over time, whether it varies business cycle conditions, or how it changed during the COVID-19 pandemic. This question is of interest in its own right to improve our understanding of individual labor supply behavior, but is also an important input into optimal UI policy.

In theoretical models of optimal UI, the labor supply response is the key social cost of additional UI benefits that is traded off against the social benefit of consumption smoothing gains (Baily, 1978; Chetty, 2006). Variation in this moral hazard response over time or across workers would suggest that benefit generosity should differ as well. In fact, in practice in many countries UI benefit generosity varies over time or across workers. A prominent example is the U.S. UI system, where benefit duration is routinely extended during economic downturns. An important motivation for increasing the generosity of UI benefits in recessions is to provide fiscal stimulus. Evidence for how labor supply responses to UI change over time will aid policymakers to better adjust UI policy over the business cycle.

Theoretically, predictions on whether and how the labor supply response to additional UI benefits varies over the business cycle are ambiguous.[2] Empirically, data and institutional constraints make the estimation of the degree of cyclicality difficult—researchers require data that span a full business cycle and a research design which can be applied in each stage of the cycle. A small number of papers have cleared these hurdles, but their results are mixed. Some work has found that the labor supply response is unchanged or smaller during downturns (Schmieder et al., 2012; Kroft and Notowidigdo, 2016), while others find that it is larger (Card et al., 2015a).

This paper provides further evidence on the cyclicality of the labor supply effect of UI benefit levels using a dataset and research design that are uniquely suited to this question. In the U.S., UI benefit levels are set as a constantly increasing function of prior earnings up to some maximum benefit level. This creates a kink in the benefit schedule that we rely on to implement a regression kink design (RKD) in roughly two decades of administrative UI data from California (CA). The size of the CA UI program, length of the time period covered, and

[1] See Schmieder and Von Wachter (2016) for a summary. Early papers based on individual-level variation of UI benefits include Moffitt (1985) and Meyer (1990). Several papers exploit variation and changes in benefit levels over time or across states (e.g., Solon, 1985; Chetty, 2008). Recent papers exploit experimental variation induced by kinks in benefit schedules (e.g., Card et al., 2015a,b; Landais, 2015).

[2] On the one hand, higher job search costs during recessions may dampen responsiveness. On the other hand, lower re-employment earnings (and therefore higher effective replacement rates of UI benefits) may increase responsiveness.

nature of the research design allow us to estimate the causal effect of higher benefit levels on unemployment duration before, during, and after three separate recessions.

A key feature of our empirical strategy is that we 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. Intuitively, UI duration extensions *mechanically* increase UI duration elasticities by no longer truncating claim lengths at 26 weeks, the typical maximum duration. We demonstrate that this distinction is critical in settings like the U.S. where the maximum potential duration of benefits is changing across the business cycle. In this case, the benefit duration elasticity is not a reliable measure of how behavioral responses to UI benefit levels change over the business cycle. We propose a conceptual framework that shows how such duration extensions create a mechanical cyclicality in estimates of the effect of benefit levels on UI duration. Our model and empirical findings help to unify existing results on duration elasticities over the business cycle and provide a useful guide to interpreting our main findings.

Empirically, we estimate that the elasticity of UI duration with respect to benefit level is larger during the Great Recession than surrounding time periods, but find no meaningful cyclicality in responses at any point in the survival curve. This result is consistent with our conceptual model: Week-to-week labor supply behavioral responses to UI generosity remain constant throughout the cycle, but duration elasticities increase mechanically increase due potential benefit extensions occurring during recessions in what we call a "coverage effect." We demonstrate that this result is not driven by changes in the types of workers who are on UI by reweighting our sample so that observable characteristics are constant over time. 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.

We also find that non-employment durations are substantially less responsive to benefit generosity than are claim durations. The difference stems from the fact that regardless of benefit levels, many people leave UI without returning to employment, either because they exhaust benefits or stop claiming for other reasons (for instance, if they are no longer able to search for work). During expansionary periods, non-employment durations are 20-30% as responsive to benefit levels as are UI claims. The ratio rose to 70% during the Great Recession, when program expansions made UI closer to full insurance of the length of spells.

We end the paper by briefly discussing the implications of our results for social welfare.

Borrowing a simple theoretical model from the literature (Schmieder and Von Wachter, 2016), we translate our estimates to a measure of the fiscal externality associated with a $1 increase in the mechanical transfer to the unemployed and show that it is highly countercyclical. Since all responses throughout the spell are relevant for the government's budget, our measure of this fiscal externality includes the elasticity of the entire UI spell duration. We show that this cyclical pattern is likely driven by the mechanical "coverage effect." Despite the lack of cyclicality in underlying behavioral responses at every point in the spell, the total disincentive cost in dollar terms grows substantially during recessions because the duration of UI benefits is extended during recessions.

Recent theoretical and empirical work (e.g., Levine, 1993; Marinescu, 2017; Landais et al., 2018; Johnston and Mas, 2018) has highlighted the potential for UI to influence unemployment durations indirectly through labor market tightness. Such spillover effects mean that the direct effect of a benefit increase on recipient behavior (the so-called "micro-elasticity" that we estimate) differs from the effect on the market as a whole (the "macro-elasticity"). Our design does not allow us to isolate these spillover effects since the variation we exploit affects only a smaller portion of UI recipients. For the same reason the discussion of the welfare implications of our results ignores any potential spillover effects. Empirical evidence on the cyclicality of these spillover effects is limited, but suggests that accounting for them would lessen the cyclicality of our fiscal externality estimates (Landais et al., 2018).

Our findings contribute to the literature in several ways. We extend the seminal work applying regression kink designs to administrative data on UI claims in Card et al. (2015a) and Card et al. (2015b) to an analysis of how labor supply effects of UI benefit levels vary throughout the unemployment spell, 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, our findings show that analyses of UI benefits on labor supply have to take into account the current PBD regime.

Our results help to partly clarify currently conflicting results in the literature regarding changes in the effect of UI benefits over the business cycle. Kroft and Notowidigdo (2016) find that the effect of UI benefit levels on exit rates is lower when local (state) unemployment rates are higher. Using the same design as we do, Card et al. (2015a) estimate that the UI duration elasticity is substantially larger in the Great Recession than in the preceding expansion. Card et al. (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 lead 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 et al. (2012)'s analysis of extensions of potential UI durations in Germany. As in their case, the use of a comparable research design yields a-cyclical behavioral responses to UI benefits. Typically, such a comparable design is not available in the U.S. setting, leading researchers to exploit state-variation in UI benefits (e.g., Chetty, 2008; Kroft and Notowidigdo, 2016; Bell et al., 2022a). One advantage of a fixed policy threshold as we use in this paper is that state-level policy changes can be themselves driven by local economic conditions.

Last but not least, we further extend the large body of evidence on the effect of UI benefits on unemployment duration summarized in Schmieder and Von Wachter (2016) and owing to several foundational papers in this area decades ago (Moffitt, 1985; Solon, 1985; Meyer, 1990). While administrative data from the US UI program has featured prominently in this literature since its beginnings, most prior work uses data from the 1970s and 80s (Meyer, 1990; Moffitt, 1985; Solon, 1985; Landais, 2015), or from narrower time periods in smaller states (Card et al., 2015a; Johnston and Mas, 2018; Leung and O'leary, 2020; Lee et al., 2021). We extend this literature by estimating the moral hazard effect in the most populous U.S. state over a long time period, including each of two of the largest post-war recessions.

Our estimation strategy 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 0.38, though there was a wide range across studies from 0.1 to 1.2. 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 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 extends 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 administrative and cross-state survey data (Bachas et al., 2020; Dube, 2020; Finamor and Scott, 2021; Marinescu et al., 2021; Ganong et al., 2022). An advantage of our pandemic-era estimates is that they are based on a comparable research design and data and hence more directly comparable to pre-pandemic estimates.

The remainder of this paper is organized as follows. 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 over the business cycle prior to the pandemic. Section 5 contains an assessment of emergency added benefits during the early pandemic on labor supply elasticities. Section 6 assesses the role of composition changes. Section 7 contains a brief discussion of the potential implications of our findings for the fiscal costs of UI benefit increases, and Section 8 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 all states, the UI system provides benefits to unemployed workers who lost their jobs through no fault of their own and who meet a minimum income threshold during a one-year period before the claim known as the Base Period (BP). Weekly benefit amounts (WBA) are set to replace a portion of prior income (as measured in the BP) while the claimant remains unemployed. Benefits are time limited, not payable past some maximum potential benefit duration (PBD).

In all states, WBAs are an increasing function of prior earnings up to some maximum WBA. In California, the specific measure of prior earnings used is the highest quarterly earnings amount in the BP (HQW, for high quarter wages) and WBAs are set to replace one-half of weekly pay from that high earning quarter up to a maximum of $450. This maximum WBA 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 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 substantially increased WBAs. Between April and July 2020, the Federal Pandemic Unemployment Compensation (FPUC) program added $600 to each claimant's WBA, so that maximum WBAs reached $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.

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 BP earnings, we will return to the details of this calculation 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., 2022b).

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 4.2) 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.[3] 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 et al. (2015a); 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[4]; or had HQW values within \$1 of a \$1,000 multiple (i.e., \$999 < HQW < \$1,001, \$1,999 < HQW < \$2,001, 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 et al., 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).

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).

[3] Following Card et al. (2015a), Landais (2015), and O'Leary et al. (1993).

[4] 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 identifiers which should eliminate this concern, but we make this restriction to be conservative.

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 et al. (2015a) 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 $h_c = k$:

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

Here γ_1 is the "kink" in the relationship between the outcome and the running variable at the cutoff k. An estimate of γ_1 is causally interpretable under the assumptions 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 as:

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

Here η_1 is the kink we are exploiting for identification so that $\frac{\gamma_1}{\eta_1}$ 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 $\frac{\gamma_1}{\eta_1}$ is estimated using a Two-Stage Least Squares (2SLS) approach in which $\widehat{b}_c$ is the fitted value from the previous equation, and $\frac{\gamma_1}{\eta_1}$ is estimated as the coefficient on b_c from a second-stage equation which includes $h_c - k$ and a constant. Alternative specifications implement a "sharp" RKD, where γ_1 is estimated by OLS, η_1 is assumed to be equal to the deterministic kink in the benefit function, and the standard error of $\frac{\widehat{\gamma}_1}{\eta_1}$ 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}.[5]

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

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., Ganong and Jäger, 2018; Cattaneo et al., 2019). 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 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 2 (separated by the period the claim was filed) and 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 et al., 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 et al., 2007a).

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 et al., 2012; Kroft and Notowidigdo, 2016). 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.[6]

The labor supply response of an unemployed worker to higher unemployment benefits is sometimes called the 'micro effect' (Landais et al., 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 et al., 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

[6] One can cast this analysis in terms of a general version of the search model in Card et al. (2007a), Chetty (2008), and Schmieder et al. (2012) that incorporates individual heterogeneity in benefit responses, differences in reemployment wages, and variation in search effectiveness over the business cycle. Suppose for simplicity that reemployment wages and search effectiveness vary only with the state of the labor market in the year in which individuals file their claim, and that heterogeneity can be captured by average individual-level characteristics of the cohort. For each cohort c of new UI claimants (i.e., BYB), such a model would imply that aggregate search responses to UI benefits depend on a range of factors, including the state of the labor market (through reemployment wages w_c and search effectiveness se_c), the composition of the cohort (X_c), as well as the future path of benefits (BP_c). In other words, the survivor elasticity at any given duration t for a cohort c.

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.[7]

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

Estimates of benefit effects on survivor curves are more robust, because the entire sample is used to estimate the treatment effect at each duration. This is because the survivor function at any given duration is a function of the entire history of each UI claimants' potential outcomes, whether they have exited unemployment earlier in the spell or not. This is fundamentally different from estimating the effect of UI benefits on exit hazards at a given period, because these condition on the realization of the potential outcomes up to this point.

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

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

If an increase in UI benefits lowers search effort and hence decreases the probability of exit in

[7]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.

each week throughout the unemployment spell, the effect on the probability of remaining on UI for a given period will be cumulative. Mathematically, $\frac{\partial S_B}{\partial b}$ 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 $\frac{\partial S_B(t)}{\partial b} = -t(1-s)^{t-1}\left(\frac{\partial s}{\partial b}\right)$. which increases in t (since $\frac{\partial s}{\partial b} < 0$). 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 $e_{S(t)} = \frac{\partial S_B(t)}{\partial b}\frac{b}{S_B(t)} = -tb\left(\frac{\partial s}{\partial b}\right)/(1-s)$, 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 $S_B(t) = P[\text{UI Benefit Spell} \geq t]$ is the survival curve of UI duration. Let $e_X = \frac{\partial X}{\partial b}\frac{b}{X}$ 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 $w_B(t) = \frac{S_B(t)}{B}$. 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 $S_D(q) = P[\text{Nonemployment duration} \geq q]$ be the survival curve of nonemployment duration. Then we have that:

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

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

with weights $w_D(q) = \frac{S_D(q)}{D}$. Here, the summation is over total potential nonemployment duration T. Even though 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 Responses to UI Benefits Over the Business Cycle

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 et al. (2015a) and Landais (2015).[8] The fact that the pattern is downward sloping to the right of the kink—where benefit levels are constant—tells us that in this sample, higher earners generally tend to be positively selected on having shorter UI durations. This means that the naïve regression of survival rates on benefit levels to the right of the kink would understate the causal effect of benefit generosity. That is why to avoid contamination from selection, we compare the change in slope around the kink. 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 pandemic, \$1 of benefits increased eight-week survival by 0.0869.[9] At the kink point of \$450 WBA, this translates to a survival elasticity of approximately 0.39.[10]

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

[8]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 higher 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.

[9]The second-stage difference in slopes is 0.0000216. The first-stage difference in slopes is 0.5/13 (on the left side of the kink, quarterly benefits increase by \$0.50 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. $(0.0000216/(0.5/13))/0.63 = 0.00089$, or 0.089%.

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

percentage increase in survival rates.[11]

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. While policymakers look to provide stimulus during downturns, the potential for UI to dampen work incentives and thereby worsen the downturn can push policy toward less distortionary but also less targeted measures, such as direct stimulus payments to individuals. 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 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).[12] Panel A of 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

[11] 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).

[12] 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 0.481, 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.)

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 Panel A of Figure 8 is as a-cyclical as the survival elasticities.

The simulation in Panel A of Figure 8 relies on the decomposition of the survival curve in Equation (5. To further clarify the mechanisms behind the differences in the actual and simulated duration elasticity, Panel B of Figure 8 shows the key elements of this decomposition. The figure displays the clear difference in survival curves between expansions and recessions, which puts more weight on later duration with higher survival elasticities. It also shows how the survival elasticities in expansions and recessions overlap up until week 26, the maximum benefit duration in expansions. This indicates little difference in the search response up to week 26 of the spell. However, with benefit durations rising up to 99 in the Great Recession, individuals still unemployed after 26 weeks are now also responding to UI benefits—leading to a longer average duration response.

A potential caveat to the interpretation of the differences in survival curves over time as indicating solely responses of individual search behavior to labor market conditions (or absence thereof) is that for forward looking individuals search behavior could also respond to changes in potential benefit durations. By the envelope theorem, small changes in potential benefit durations will not affect the marginal effect of UI benefit levels on labor supply (e.g., Chetty, 2008). It is less obvious how inframarginal changes in maximum benefit durations would affect benefit elasticities. However, to explain the overlap in survival elasticities up to week 26 shown in Panel B of Figure 8, changes in exit rates due to increases in potential benefit durations had to exactly offset effects from worsening labor market conditions, which is highly unlikely.

Why the behavioral response at any given point in the spell does not seem to vary with economic conditions is an important question for future work. One interpretation of the a-cyclicality is that in recessions the two countervailing forces of low effectiveness of job search and higher benefit replacement rates discussed in Section 3.1 cancel each other out. Alternatively, while workers may respond to UI benefits on average as predicted by theory, the marginal benefit increases studied here may not change their search responses to cycle. While several studies analyze the effect of benefit duration on search behavior over the unemployment spell (e.g., Marinescu and Skandalis, 2021; Lichter and Schiprowski, 2021; DellaVigna et al., 2022), we are not aware of a similar study of the effect of UI benefit levels. Similarly, while recent research has studied changes in search behavior over the cycle, there is little research of the effect of UI benefit or duration on job search behavior over the business cycle.[13]

4.3 Non-Employment Elasticities

While UI claim duration is a common measure of labor supply responses to UI benefits, it does not necessarily capture employment behavior beyond the UI spell. While no weekly measure of nonemployment duration is available, 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, again mainly due to a coverage effect.

The analogue of Figure 8, Figure 9 plots elasticity of nonemployment durations to benefit levels. Our first finding is that the elasticity of nonemployment duration is lower than the elasticity of claim duration. During expansionary periods, our point estimates for nonemployment elasticities fluctuate around 0.2. Standard errors are larger for nonemployment durations relative to claims durations, partly because it is an outcome with higher variance in the population, and partly due to the coarseness with which we measure the outcome.[14] One explanation for the lower elasticity of nonemployment durations is that UI benefit durations typically only cover a fraction of the actual nonemployment spell, since many claimants exhaust UI benefits without returning to work. Intuitively, in regular economic times, UI benefits provide only imperfect insurance against nonemployment. Since benefit increases will have the strongest effect on search behavior on the nonemployed while they are receiving benefits , the overall elasticity must be smaller.[15] Another explanation for the lower elasticity of nonemployment durations is that frequently individuals leave UI without returning to work. Among others, this can happen because claimants stop certifying for benefits prematurely, perhaps because they expect to receive a job offer soon (e.g., Lee et al., 2021; Bell et al., 2022b). The higher elasticity of claim durations thus partly reflects the fact that higher benefits reduce the exit rate from UI, rather than reducing the rate of job finding.

Very few U.S.-based studies estimate benefit elasticities for both claim and nonemployment

[13]Since job search activity is a prerequisite for receipt of benefits, presence and variation of UI benefits themselves can affect the study of job search behavior. Using U.S. data, Mukoyama et al. (2018) show that in a recession a higher share of the unemployed report searching for jobs and does so for a longer period. This could be partly related to the increase in coverage and duration of UI benefits during recessions.

[14]To assess to what extent the different frequency in which UI benefits and nonemployment duration are measured affects the comparison between the two elasticities, we replicated our elasticity of claim duration using a quarterly measure of UI claim duration that we obtained by aggregating the weekly series. We found that the marginal effect of a rise in UI benefits is very similar in the weekly and quarterly series, but that as expected, the censoring reduces the average duration. As a result, the quarterly elasticity of claim duration was slightly higher.

[15]In terms of equation (7), for the elasticity of nonemployment durations the weights on the survivor elasticities until potential benefit duration (P) sum to less than one; in contrast for the elasticity of claim duration they sum to one.

duration. The only study using data on (weekly) nonemployment duration, from Washington state (Landais 2015), finds a claim duration elasticity of 0.73 and a nonemployment duration elasticity of 0.21, comparable to our results.[16] Consistent with these findings and the underlying explanation, Schmieder et al. (2012) show that average exit rates from nonemployment in Germany are substantially lower than exits from UI benefits receipt throughout the nonemployment spell. Relatedly, Card et al. (2007b) report evidence from several countries that job finding rates do not spike at benefit exhaustion.[17]

One way to directly see how closely changes in nonemployment durations are tied to UI durations is to consider the elasticity of nonemployment durations to UI durations. This is simply the ratio of the nonemployment elasticity shown in Figure 9 to the UI claim duration elasticity in Figure 8. The ratio can be interpreted as an instrumental variable estimator of the causal effect of an increase in UI duration on nonemployment duration, as long as there is no direct effect of UI benefits on nonemployment other than through a rise in UI benefit durations. This assumption is certainly plausible, but is not necessary for the point made here.[18] In typical search models used in the UI literature (e.g. Chetty, 2008; Schmieder et al., 2016), claim and nonemployment duration are both solely determined by job search effort, and hence the elasticity of nonemployment duration to claim duration should be closer to 1. In regular economic times, we find that the ratio is far away from 1. Figure A8 shows that during expansions, unemployment duration is about 20% as responsive to weekly benefit levels as is UI claim duration, and this ratio rises to about 70% during the UI expansion of the Great Recession. This implies that increases in UI durations in response to UI benefits do not map one-to-one into increases in nonemployment duration, both because UI imperfectly covers nonemployment spells and because many claimants quit UI without immediately returning to employment.

The second finding is that during the Great Recession, nonemployment duration elasticities increased to 0.6, higher than expansionary period nonemployment elasticities, but still well below the 0.75 claim duration elasticity of this period. As in the case of claim duration elastici-

[16]Other studies reviewed in Schmieder and Von Wachter (2016, Table 2) that show estimates for both UI claim and nonemployment duration use estimated hazard rates and survivor functions based on weekly UI claim duration to infer about total unemployment duration past benefit exhaustions (e.g., Marston, 1982; Meyer, 1990; Meyer and Mok, 2007), or Schmieder et al. (2016) impute the implied nonemployment elasticity based on a common constant hazard assumption. However, either approach mechanically leads to larger nonemployment duration elasticities for the same reasons as laid out in Section 3. Alternatively, Card et al. (2015a) present the elasticity of total accumulated claim duration, which is an interesting parameter for policy but does not capture differential exit rates from nonemployment.

[17]As a result, all studies that show both the elasticities of nonemployment and UI claim duration with respect to potential benefit duration reviewed in Schmieder and Von Wachter (2016) show that the nonemployment elasticity is smaller than the claim duration elasticity.

[18]Schmieder et al. (2012) introduced the elasticity of nonemployment durations with respect to UI durations because it accounts for the fact that during recessions, a rise in potential benefit duration reduces UI exhaustion rates without affecting nonemployment durations. Hence, they show it can serve as a single index to measure the welfare cost of increases in potential benefit durations.

ties discussed in the previous section, the rise in the nonemployment duration elasticities again partly occurs due to a UI coverage effect (see Section 3.2). In the notation of equations (6) and (7), during recessions a rise in potential benefit durations P in recessions covers a larger share of potential nonemployment duration. In addition, it might be that during recessions fewer individuals quit UI without finding a job. This could be because claimants expect longer unemployment durations, perhaps because they receive fewer job offers or because unemployment is more salient. As a result, the elasticity of nonemployment duration with respect to actual claim duration moves closer to one, i.e., changes in UI durations are more closely reflected in changes in nonemployment durations during recessions. This is intuitive, since potential benefit durations of close to two years during the Great Recessions implied that UI durations covered much larger proportions of actual nonemployment spells, providing closer to full insurance of unemployment spells.

5 Labor Supply Effects of UI Benefits 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.[19] The

[19]The downward-sloping trend during the pandemic may be consistent with the effects of the fixed-level added benefits, which implied larger percentage increases for lower-income workers. However, it is also consistent with selection-driven stories, namely that the public health nature of the crisis had relatively large impacts on the reemployment prospects of lower-wage workers in the service sector. To better understand the change, we re-

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%.[20] This estimate is somewhat smaller than our pre-pandemic baseline of 0.086%. The implied elasticity for early-pandemic claimants is 0.097, which is lower than our pre-pandemic baseline.[21] (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.[22])

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.

5.1 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 (Dube, 2020; Finamor and Scott, 2021; Holzer et al., 2021; Marinescu et al., 2021; Ganong et al., 2022). 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.

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

weighted the early pandemic sample to match the observable characteristics of the claimants in the pre-pandemic period (2014-2019), and then re-created Figure 10. We found that the slope is still downward-sloping on the left side of the kink, which suggests the pattern is not well explained by a change in the observable characteristics of UI claimants during the pandemic.

[20] (0.00000689/(.5/13))/.80=0.00022%

[21] 0.00068575/(1/433)= 0.0970

[22] (0.00000689/(.5/13))/.80/(1/(433+600))

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.

6 Assessing the Role of Composition Changes

Our core RKD labor supply results are not particularly sensitive to variations in the bandwidth, specification, or sample definition. We show these results in more detail in our Sensitivity Appendix.

To probe whether compositional changes in claimants drive changes in our duration elasticity over time, we re-estimate our results under inverse propensity score 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 re-weighting 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, re-weighting the claimants in each sub-sample according to the inverse of this propensity score, so that in each year the composition of the sample is similar to the sample in 2009 (in terms of observables).[23]

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 re-weighted 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 5, 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 re-weighting strategy. We see that the re-weighted 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.

[23] If we denote the raw probability of a claimant in the sample having a BYB in 2009 as p, and the propensity score (the estimated probability of the BYB date being 2009 based on covariates) as s, we construct a weight w for each observation as $w = [(1-p)/s]$ if the claimant does not actually have a BYB in 2009, and $w = [p/s]$ if the claimant does have a BYB date in 2009. We then estimate the RKD separately for each BYB year, weighting observations with w.

7 Welfare Implications: The Fiscal Cost of Moral Hazard Responses Over the Business Cycle

While our focus on survival probabilities is useful for understanding how workers' labor supply choices respond to UI benefits over the business cycle, the implications of our results for social welfare rely on the cyclicality of duration responses to UI benefits. This is because the increase in nonemployment and benefit durations capture the shortfall in tax revenues and a rise in benefit expenditures caused by increases in UI benefits, respectively. In this section, we show that our finding of countercyclical duration responses to UI benefit increases (Section 4.2) implies that the fiscal externality associated with a \$1 increase in UI benefit levels is strongly countercyclical (higher during recessions). Hence, while mechanically increasing duration elasticities due to PBD extensions (the "coverage effect") do not correspond to changes in underlying behavioral responses made by the unemployed, they are relevant for quantifying the social cost of those responses. These implications hold as long as cyclicality in the micro effect of UI benefit increases (see Section 3) is similar enough to cyclicality in the macro effect that includes indirect spillover effects. As discussed below, the existing research suggests that accounting for such spillover effects would lessen the cyclicality of our fiscal externality estimates.

Following Schmieder et al. (2016), we consider a continuous time job search model where a representative worker becomes unemployed at time $t = 0$. The worker receives UI benefits b while unemployed (which are payable for up to P periods), exerts costly search effort s, and accepts all job offers such that s is also the exit rate from unemployment. The worker has flow utility $u(c_{u,t})$ while unemployed and $v(c_e)$ while employed. While employed the worker receives a fixed wage w and pays a tax τ which finances the UI program. We use B to denote the expected duration of UI benefit receipt.

A social planner chooses b, P, and τ to maximize social welfare—the unemployed person's expected lifetime utility—subject to a government budget constraint. Schmieder et al. (2016) show that in this setup the marginal effect of an increase in b on social welfare per dollar of UI benefit transferred to the unemployed is:

$$\frac{\partial W}{\partial b}\frac{1}{Bv'(c_e)} = \frac{u'(c_{u,t\leq P}) - v'(c_e)}{v'(c_e)} - \frac{1}{B}\left(\frac{\partial B}{\partial b}b + \frac{\partial D}{\partial b}\tau\right) \quad (8)$$

The first term to the right captures the insurance value of transferring \$1 from the employed to the unemployed state. The second term to the right captures the cost of UI benefits, defined as the tax revenue required to finance a \$1 increase in mechanical transfers of UI benefits to the unemployed. This cost can exceed \$1 because a rise in UI benefits may lead to shortfall in tax revenues due to longer nonemployment spells, and a rise in benefit expenditures due to longer benefit durations. These "behavioral costs" in the numerator are scaled by the "mechanical cost" consisting of the total benefit transfer B in the denominator. This normalization accounts for the fact that duration effects in the numerator may increase because benefit availability increases. We follow Schmieder and Von Wachter (2016)) in referring to this second term as the "behavioral cost mechanical cost ratio" (BCMC) (see also Lee et al. (2021)). Some rearranging allows us to express the BCMC as the sum of two components: the elasticity of unemployment duration with respect to b and the elasticity of nonemployment duration with respect to b scaled by . Hence, the duration elasticities partly incorporate the scaling, but alone do not fully reflect the fiscal cost, or how it changes over time or space.

Importantly, nearly all of the parameters in the BCMC ratio can be estimated directly in our data.[24] The fiscal externality associated with transferring an additional \$1 to the unemployed via a WBA increase is higher at the end of the early 2000s recession (0.6) and especially during the Great Recession (0.8) than in expansions (0.5) (See Figure A7). These values are well within a range BCMC ratios from the prior literature ranging from 0.14 to 5.56 with a median of 0.81 as reported by Schmieder and Von Wachter (2016).

The fact that the behavioral cost per dollar of UI benefits transferred is higher in recessions may appear to contradict our main results where we show that the moral hazard response at any point in the spell is a-cyclical. However, the differences are simply because the BCMC ratio relies on the elasticity of the full length of the spell, and not on the search response at any given point in the spell. During recessions, when benefit durations are longer, the same behavioral responses we typically see among those with shorter durations will also occur among those with longer durations—of which there are more and whose spell is now covered. As we have shown, duration elasticities are strongly countercyclical due to this mechanical "coverage" effect of extensions to benefit duration (P) in the US during recessions. In Figure A7 we demonstrate that extensions again explain the countercyclical pattern of the BCMC ratios with an alternative measure that ignores extensions.

We conclude with two caveats. First, our conclusion that the fiscal cost per dollar of UI benefits transferred increases in recessions relies on the assumption that cyclicality in the micro effect that we estimate is similar to cyclicality in the macro effect that takes into account spillover effects of UI benefits on labor market tightness (typically defined as vacancies per search effort).

[24] Following Schmieder and Von Wachter (2016), we assume that the relevant tax rate is 31.47% and that reemployment wages are equal to their prior earnings (which we define as their HQW, the measure of prior earnings used to determine b) so that τ is 0.3147*HQW.

For example, if UI benefits reduce labor market crowding (Marinescu, 2017) the micro effect is larger, but if UI benefits lead to reduction in vacancy creation the micro effect could be smaller than the macro effect (Hagedorn et al., 2013). While a growing literature has focused on the degree of spillover effects, only one paper has focused on the cyclicality of these spillover effects. Landais et al. (2018) present evidence that these spillover effects are cyclical—leading to smaller reductions in social welfare in recessions than expansions. This suggests that accounting for such spillover effects would lessen the cyclicality of our fiscal externality estimates.[25]

Second, these findings do not speak to how the full welfare effect of UI benefit increases varies over the cycle, since we are unable to measure changes in the insurance value of UI. Since the literature has shown that consumption tends to drop throughout the unemployment spell, especially for those exhausting UI benefits, it is likely that the insurance value of UI benefit levels rises in recessions (e.g., Kroft and Notowidigdo, 2016; Rothstein and Valletta, 2017; Ganong and Noel, 2019). Hence, despite the increase in the fiscal cost of UI benefit transfers during recessions we find, the optimal UI benefit amount may still rise in recessions.

8 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 Welfare Appendix provides more commentary on how our empirical results

[25] Papers in this literature typically find that macro effects are smaller than micro effects (e.g., Levine, 1993; Lalive et al., 2015; Landais et al., 2018; Chodorow-Reich et al., 2019; Dieterle et al., 2020), or that they are similar (e.g., Marinescu, 2017; Johnston and Mas, 2018; Boone et al., 2021). However, some find the opposite (e.g., Hagedorn et al., 2013; Karahan et al., 2019; Fredriksson and Söderström, 2020).

shine light on welfare tradeoffs concerning UI benefit generosity over the business cycle. Finally, 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.

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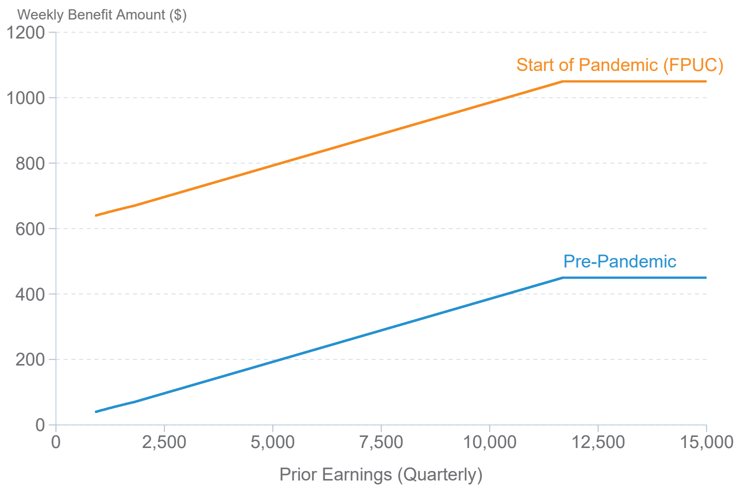
Figures and Tables

Figure 1: Weekly UI Benefit Schedules in California by Time Period

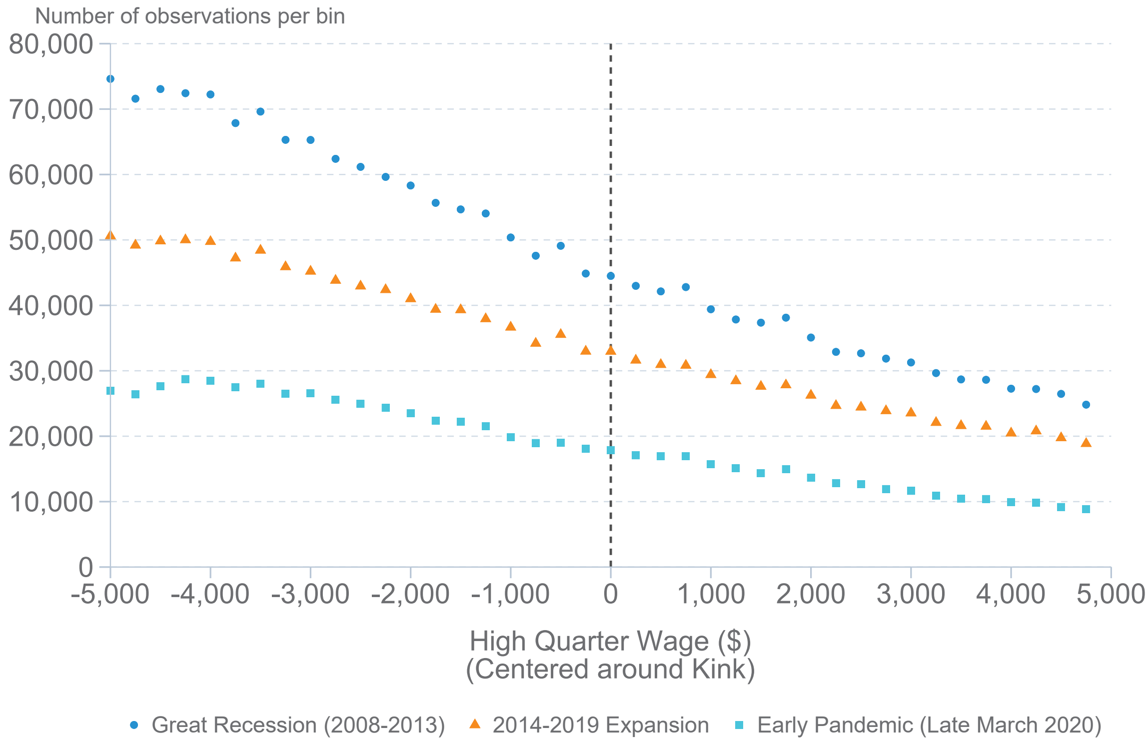
(a) Pre-Pandemic

(b) Pandemic



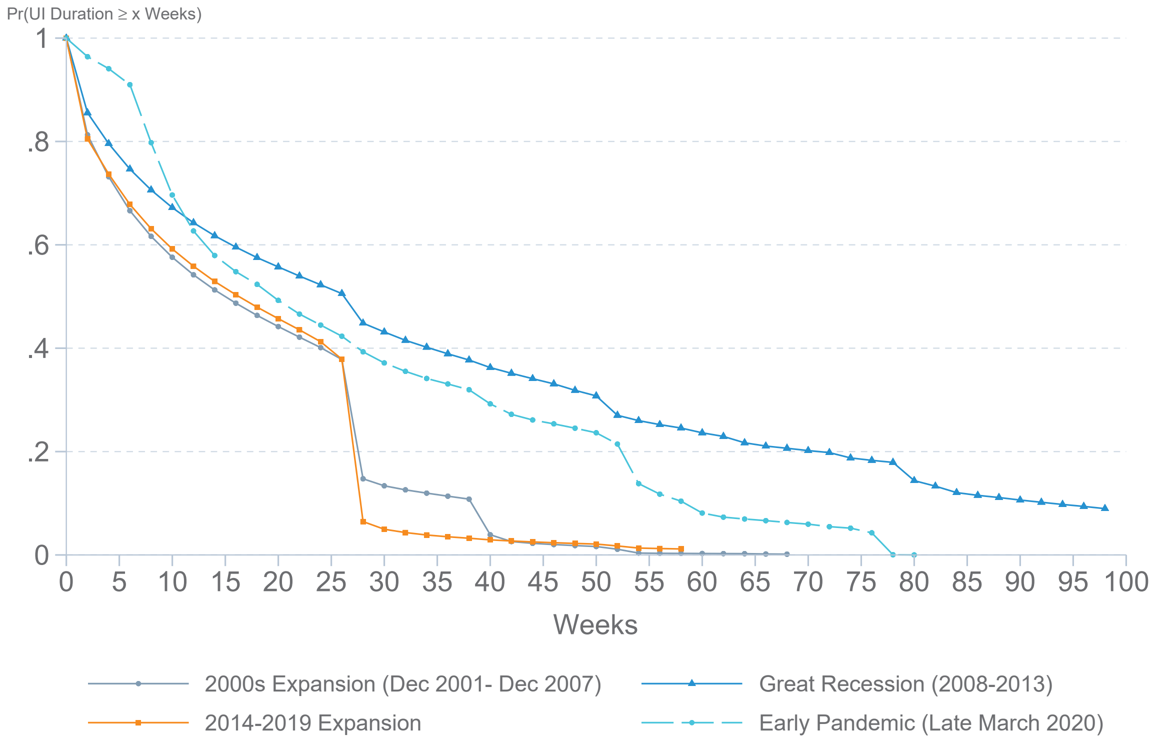
<u>Notes:</u> See Section 2 for details on how benefits are calculated. Panel B shows the Weekly benefit amount with and without the $600 FPUC benefits effective at the start of the COVID-19 pandemic.

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



Notes: 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

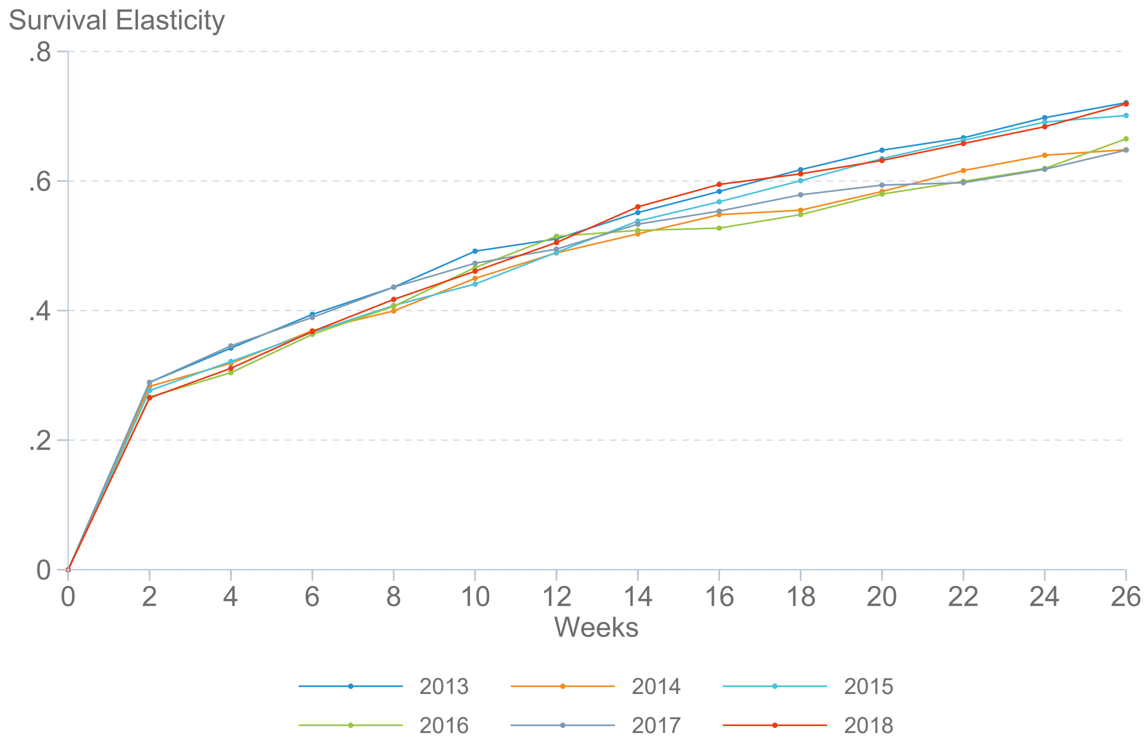


Notes: Survival curves of claimants for our core analysis sample for various BYB ranges.

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

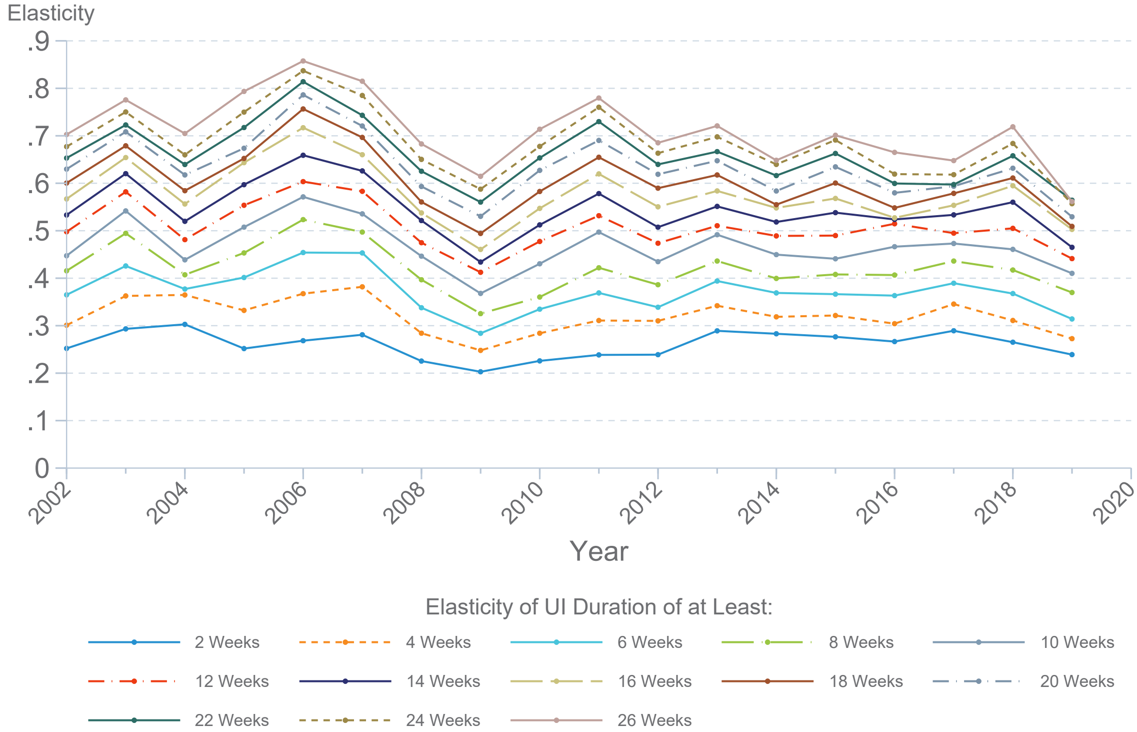
Notes: Eight-week survival as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is -0.0000216. The sample is our core analysis sample restricted to 2014-2019 BYB.

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



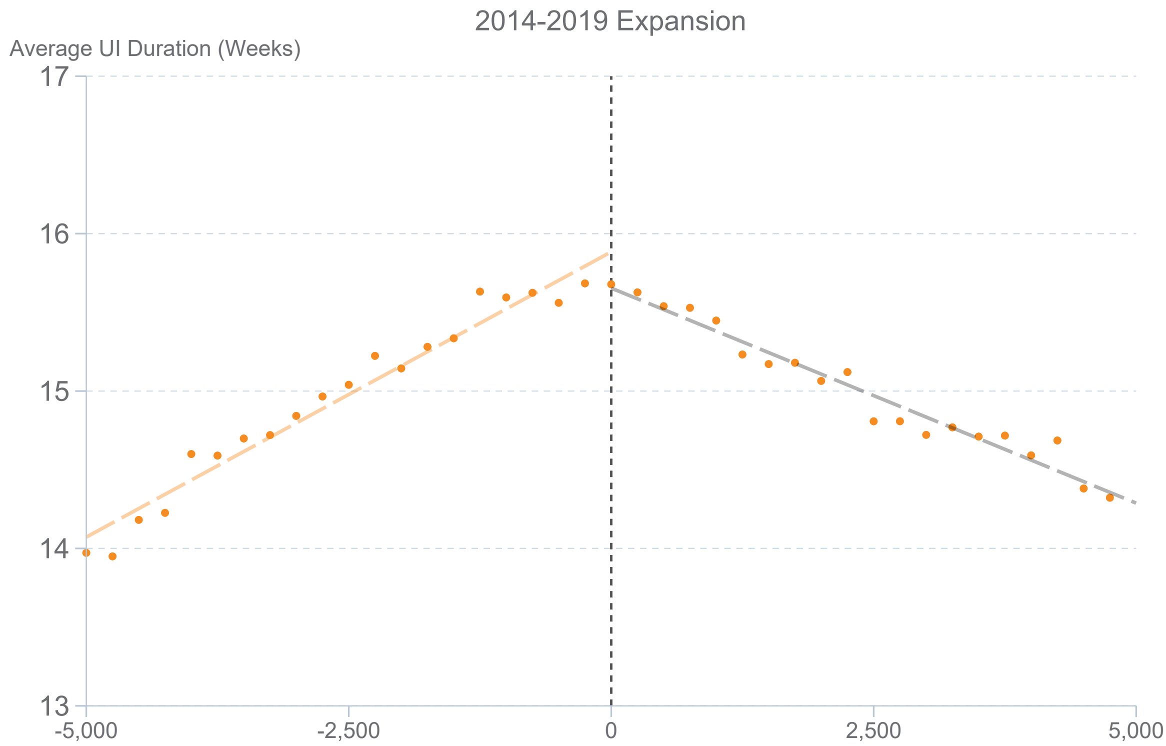
Notes: Survival elasticities by week for our core analysis sample restricted to claimants with a BYB in each year between 2013-2018.

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: Survival elasticities by week and year of BYB for our core analysis sample.

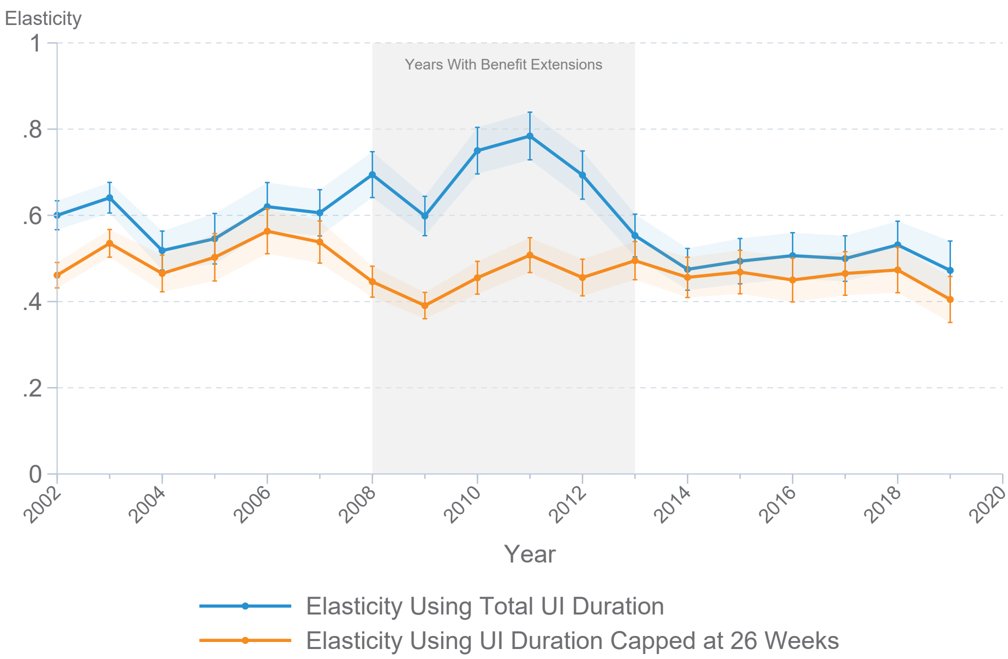
Figure 7: Responses in Average UI Duration in Weeks Around the Kink in Benefit Schedule, 2014-2019 Expansion



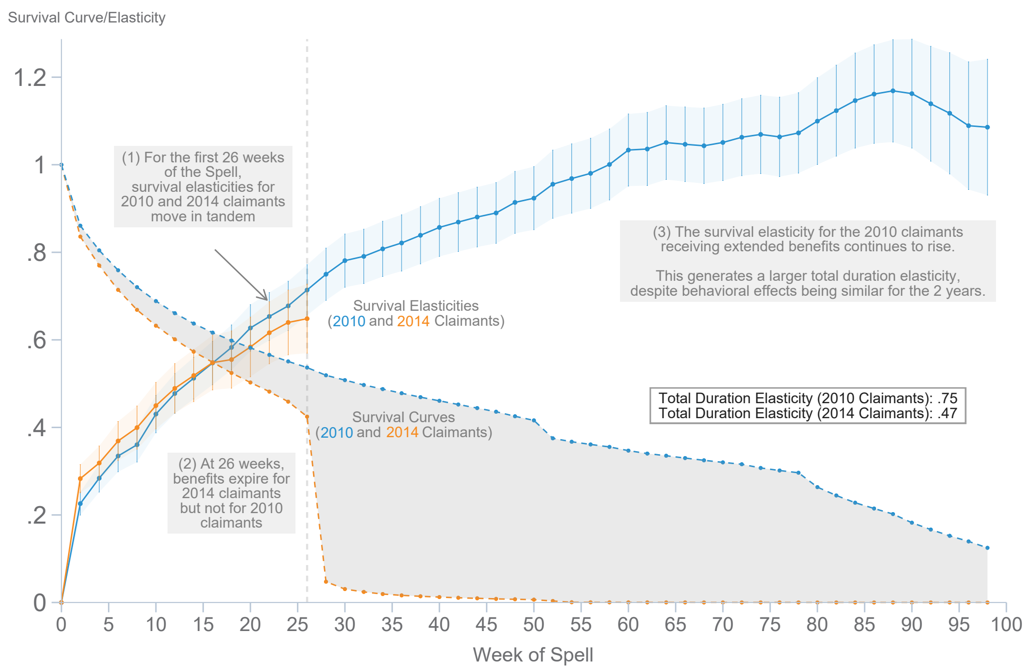
Notes: Average duration as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is -0.00637, which implies an elasticity (with respect to WBA) of 0.497.

Figure 8: The Role of Extensions in the Cyclicality of Duration Elasticities

(a) Elasticity of Truncated UI Duration

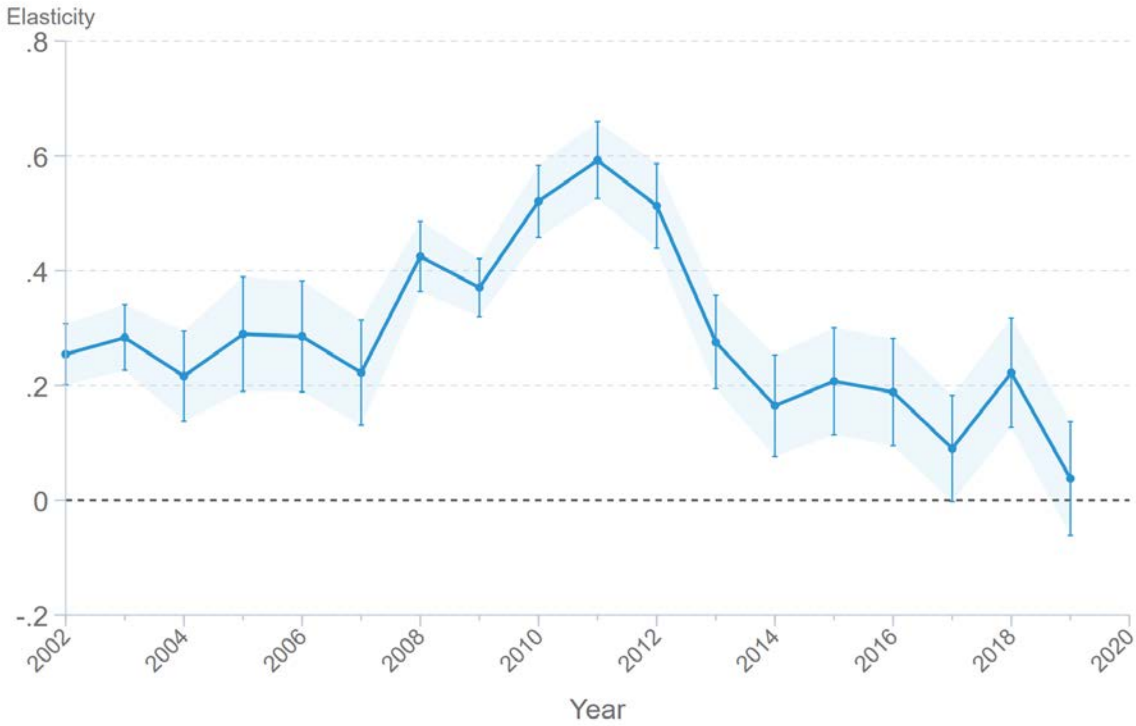


(b) Weekly Survival Elasticities, 2010 vs. 2014



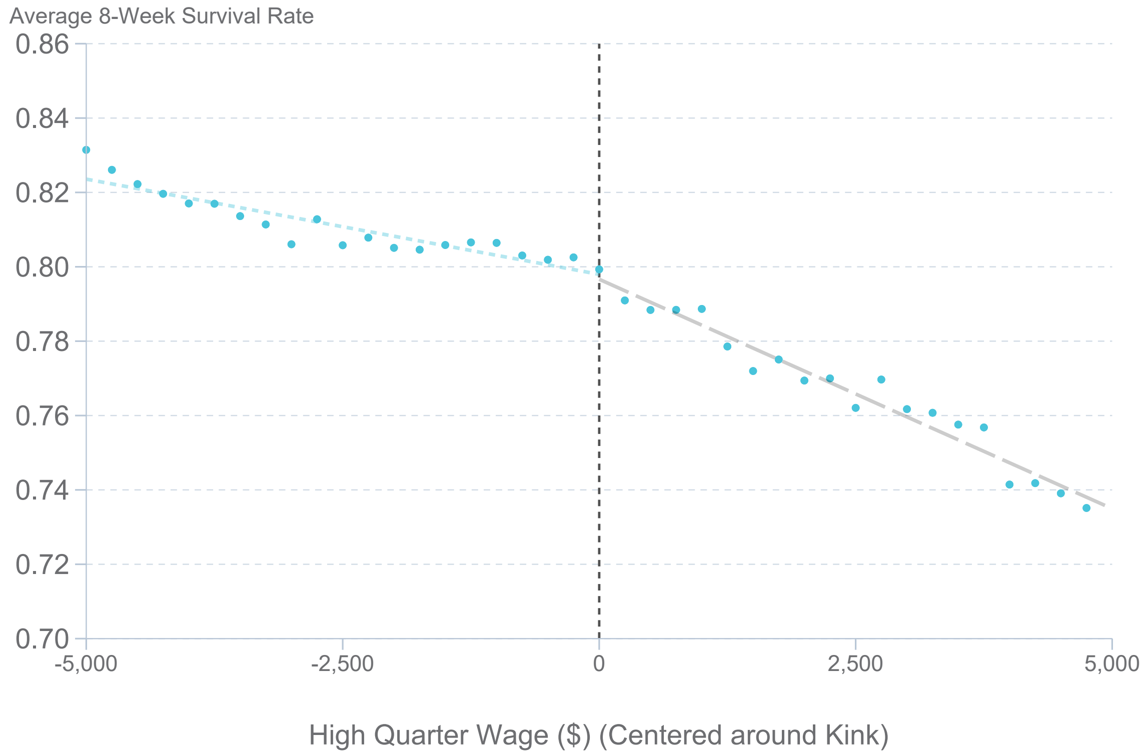
Notes: *Panel (a)* Duration elasticities by benefit year for our core analysis sample. The orange line shows estimates where total UI duration is capped at 26 weeks, whereas the blue line shows estimates under the actual durations. *Panel (b)* Survival elasticities, by week, and survival curves, separately for claims beginning in 2014 and 2019. For 2014 claimants, survival elasticities past 26 weeks are not shown due to insufficient sample sizes.

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



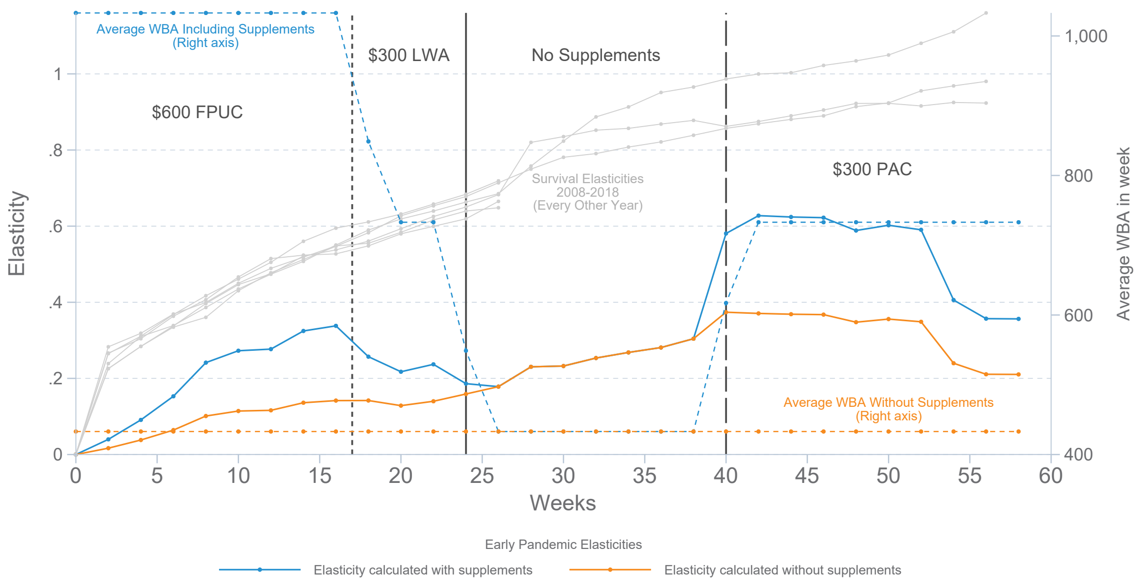
Notes: Non-employment elasticities by benefit year for our core analysis sample. Nonemployment duration has been capped at 4 quarters.

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



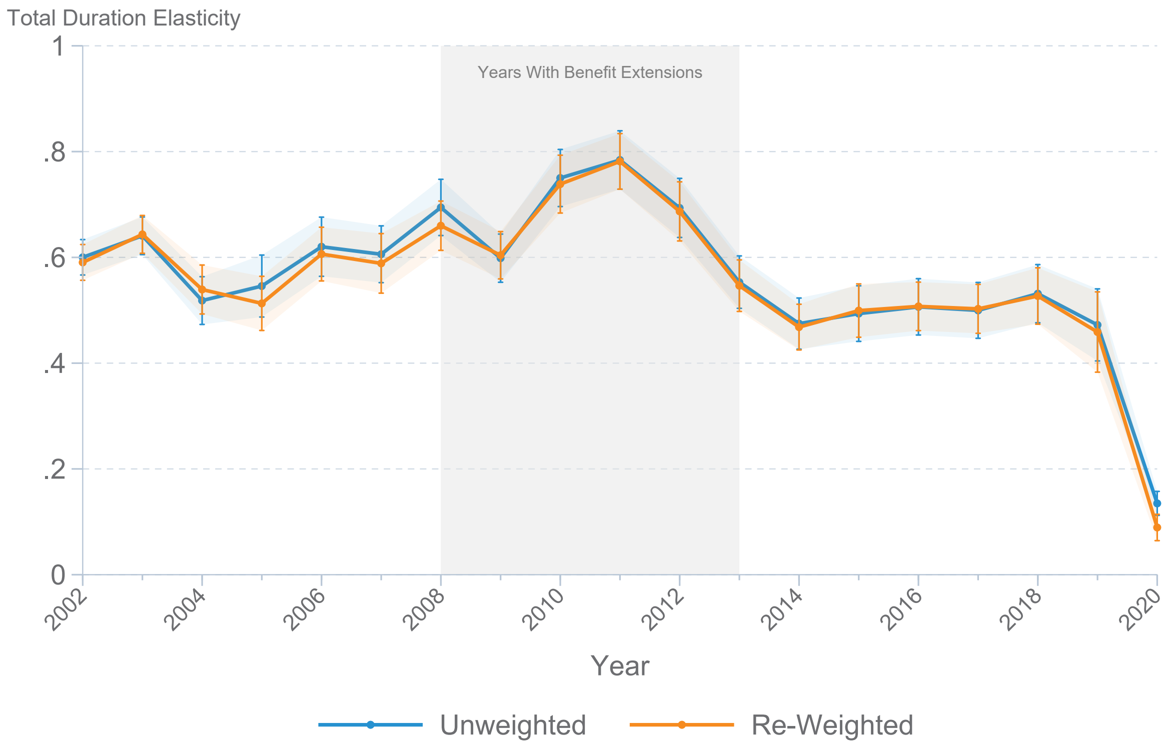
Notes: 8-week survival rate as a function of highest quarter wages, which is the running variable of our design. The difference in slopes is 0.00000689. The sample is claimants starting a new benefit year in the last two weeks of March of 2020.

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



<u>Notes:</u> 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



<u>Notes:</u> 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, 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.

Table 1: Summary Statistics by Sample Definition, 2014-2019

	(1)	(2)	(3)	(4)
	Full Sample	Full Sample within 5k BW	Limit Sample No Bunching	Limit Sample, No Bunching, 5k BW
Female	0.45	0.43	0.45	0.46
Age	40.1	40.4	41.1	40.2
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: Limit Sample No Bunch is defined as having a 26 week PBD, not having a DQ'd claim, not having a prior claim within 2 years, and not having a HQW that is a perfect multiple of 1,000.

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

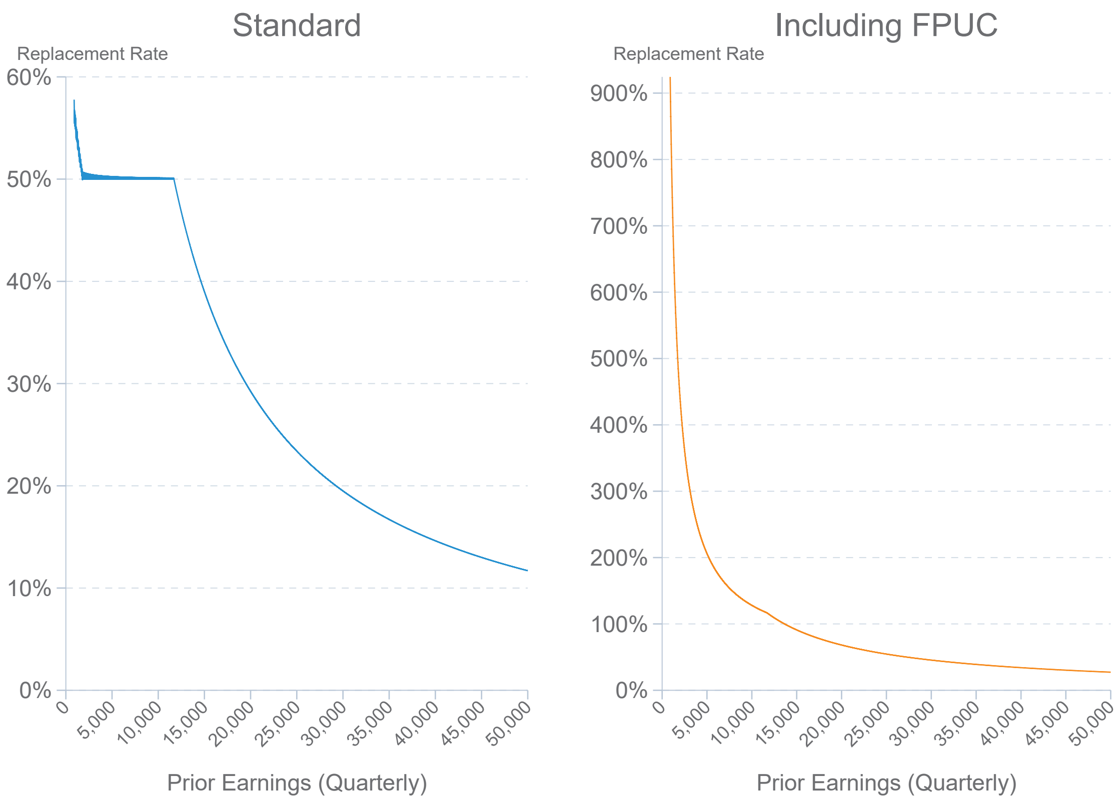
	(1)	(2)	(3)	(4)
	2000s Expansion	Great Recession	Pre-Pandemic Expansion	Early Pandemic
	Total UI Duration			
Marg. Effect of $10 WBA Increase	0.266***	0.548***	0.179***	0.106***
	(0.004)	(0.009)	(0.004)	(0.010)
Implied Elasticity	0.619***	0.690***	0.497***	0.171***
	(0.010)	(0.012)	(0.012)	(0.016)
	8 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.

Appendices

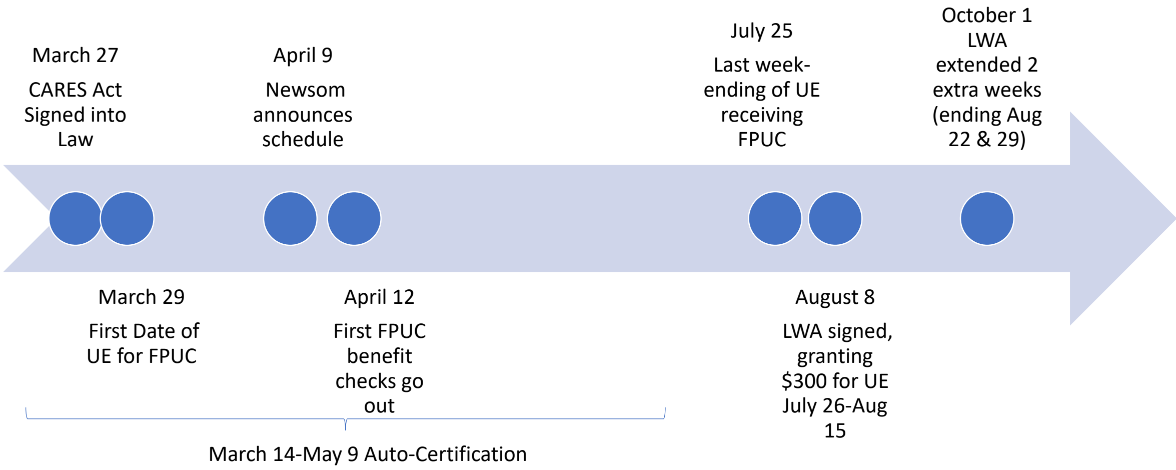
A Appendix Figures and Tables

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



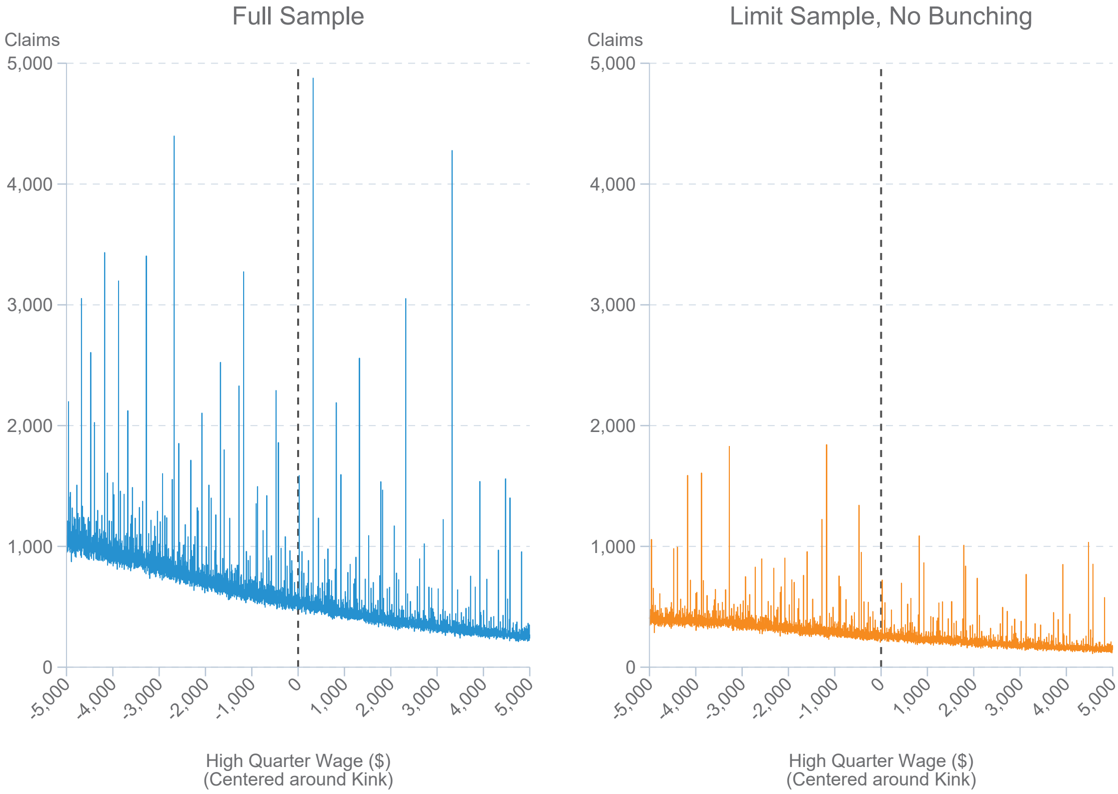
Notes: Replacement rate of UI benefits, with and without FPUC (which adds $600 to the regular UI benefit amount, as explained in Section 2.1. The replacement rate is defined as $\frac{\text{WBA}}{\text{HQW}/13}$, i.e., the proportion of weekly pre-claim earnings in the highest earning quarter of the base period replaced by UI benefits.

Figure A2: Timeline of Early Pandemic UI Expansions in CA



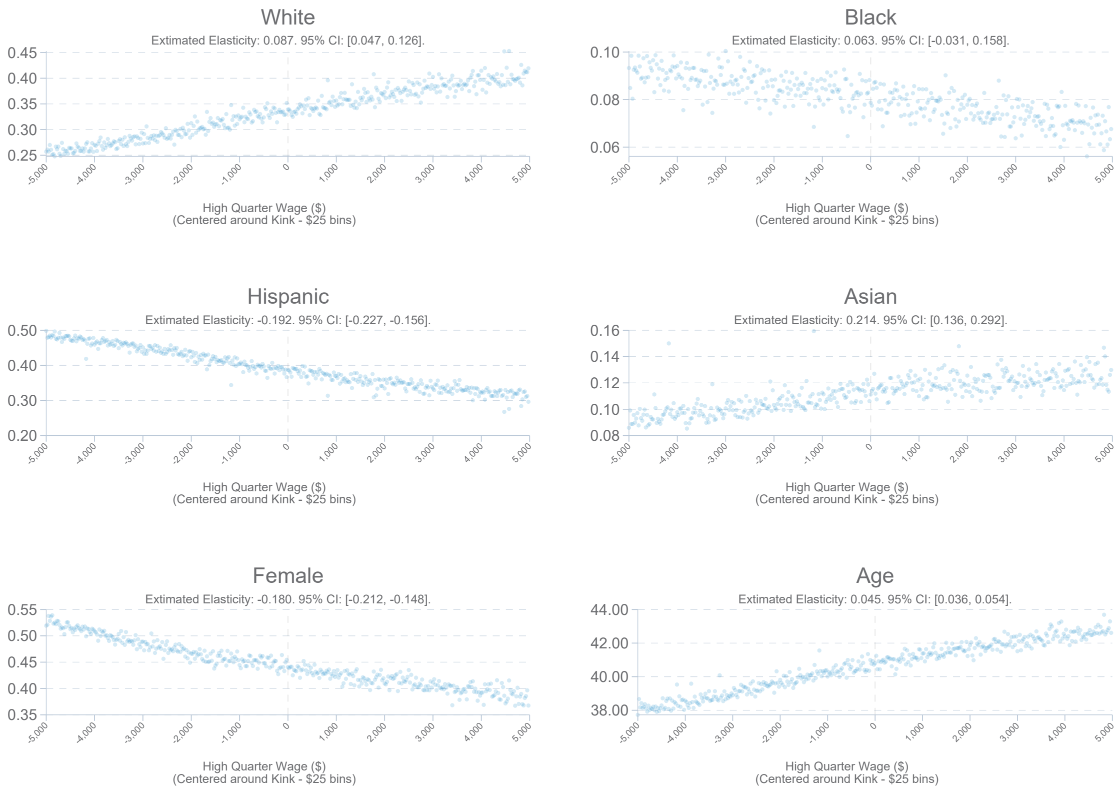
Notes: This diagram illustrates the key dates for various pandemic-era (2020) supplement programs.

Figure A3: Density Around Kink



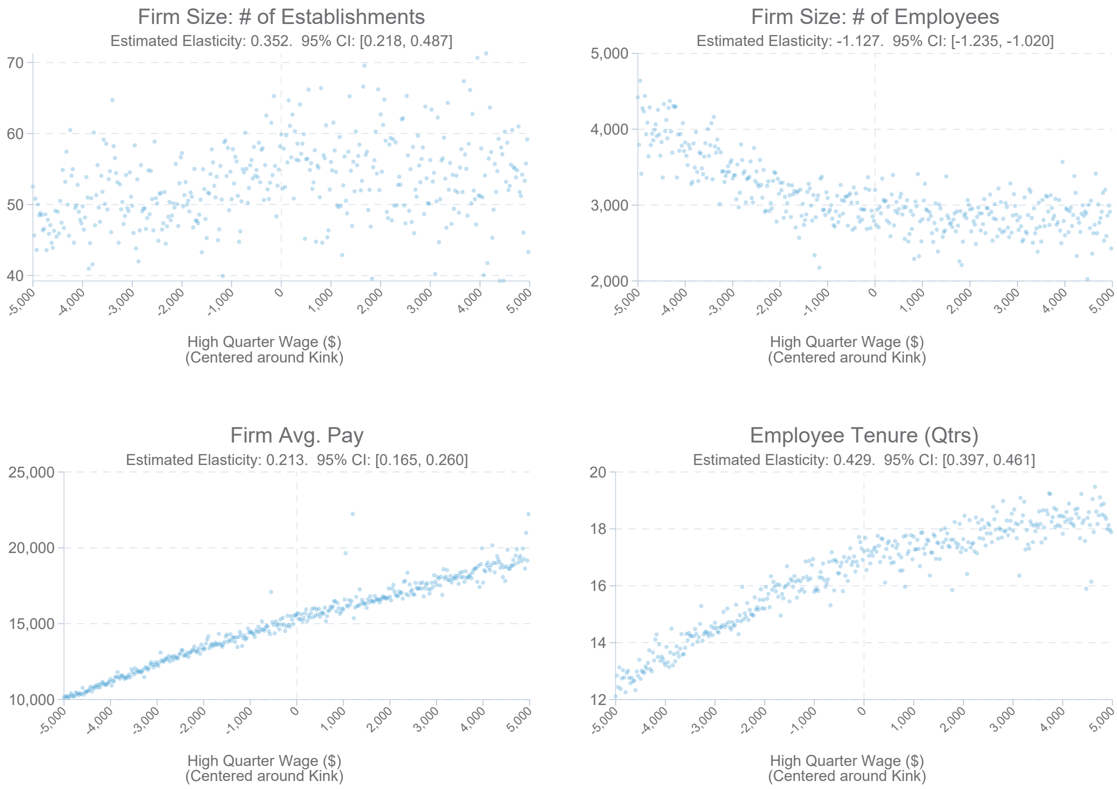
Notes: This figure plots the number of claimants in each $2 bin of HQW under the full sample (with no restrictions) and our preferred sample (described in section 2.3).

Figure A4: Smoothness of Covariates Through Cutoff, Claimant Demographics



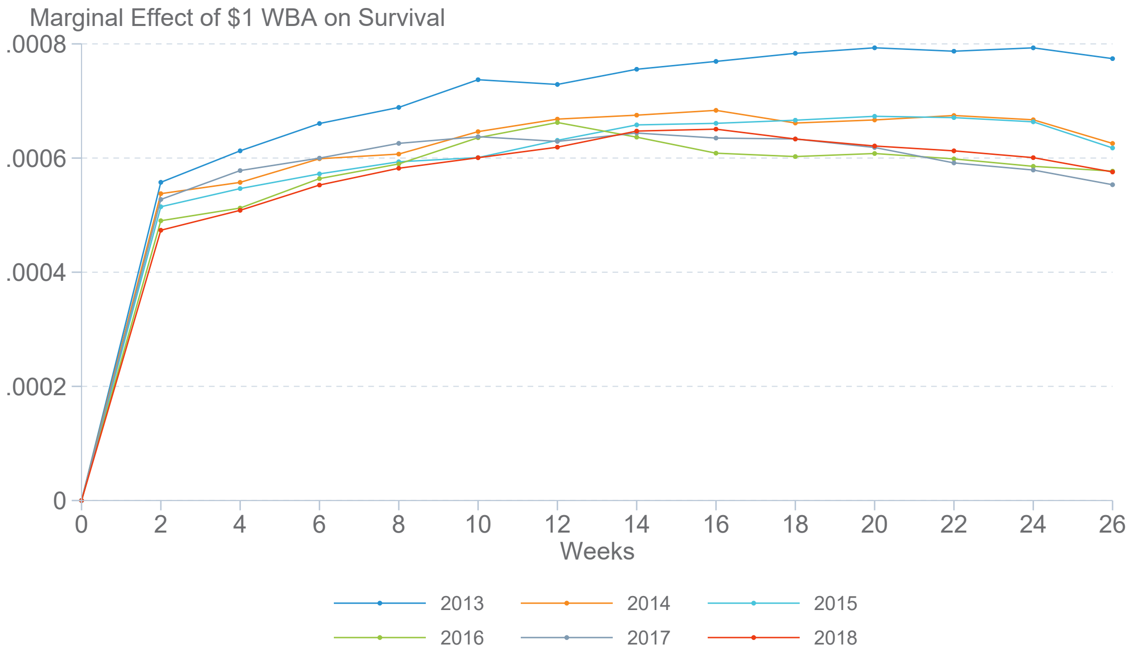
Notes: Each panel displays a binned scatter plot of covariate means (y-axis) against the running variable (HQW, high quarter earnings) centered at the cutoff. Subtitles display estimates of the slope change at the cutoff from regressions analogous to our main RKD specification, with the covariate as the outcome, converted to an elasticity. Age, gender, and race/ethnicity are all self-reported by the claimant to EDD when the claim is filed. Following Ganong and Jager (2018), we also constructed a distribution of placebo estimates by varying the kink location in $25 increments of HQW, then re-estimating the RKD under each (placebo) kink with each predetermined demographic variable as the outcome. The observed estimates using the true kink location are not more extreme than the placebo estimates, and we cannot reject the null hypothesis that the kink in the WBA schedule has no effect on the observable characteristics of claimants in our sample using standard levels for statistical significance.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A5: Smoothness of Covariates Through Cutoff, Claimant Demographics



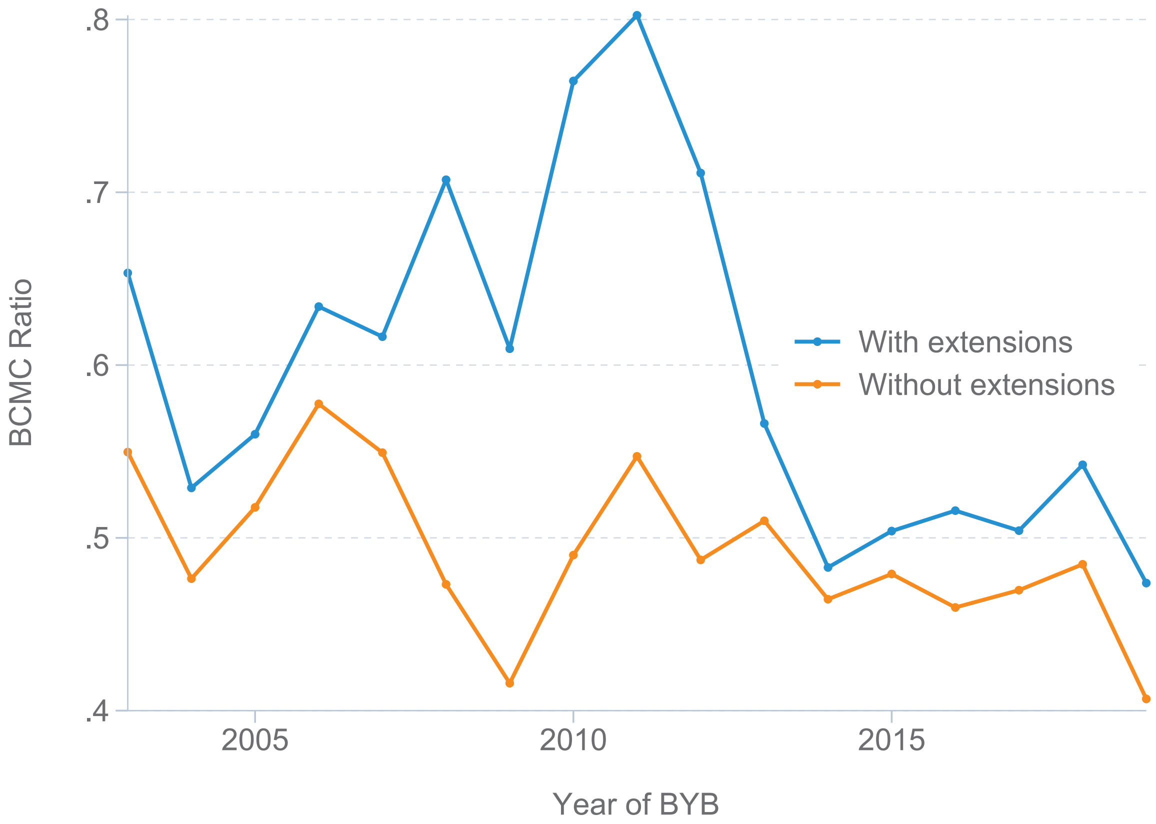
Notes: Each panel displays a binned scatter plot of covariate means (y-axis) against the running variable (HQW, high quarter earnings) centered at the cutoff. Subtitles display estimates of the slope change at the cutoff from regressions analogous to our main RKD specification, with the covariate as the outcome. 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



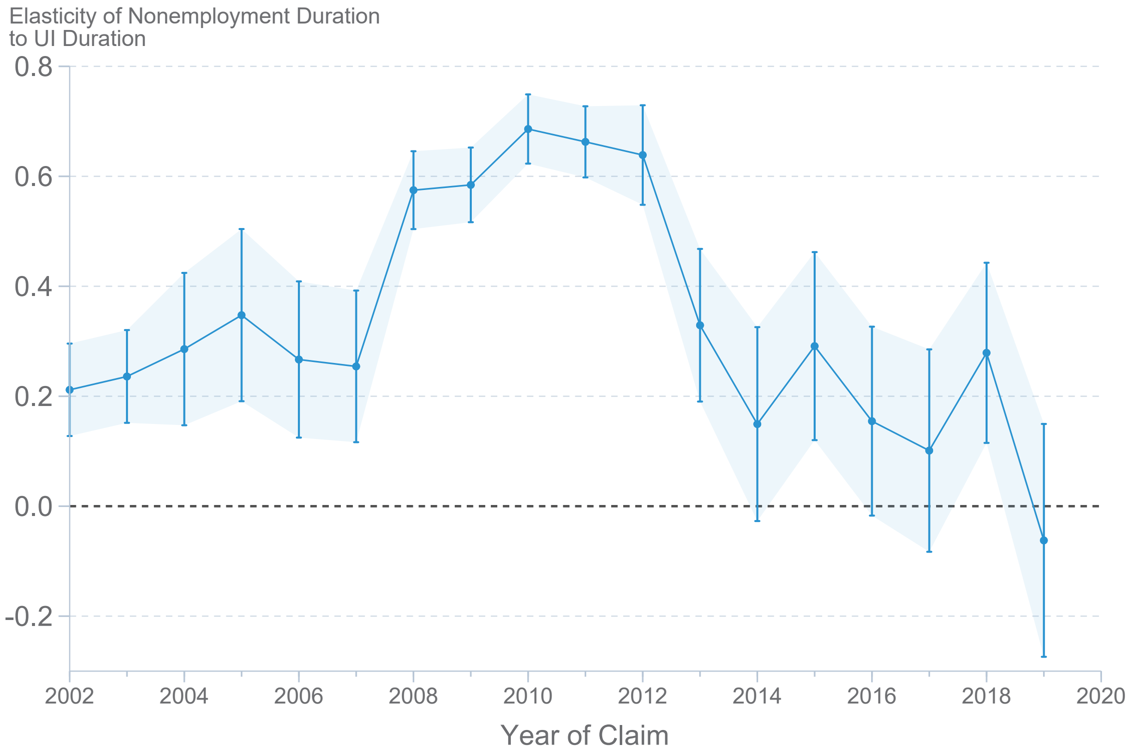
Notes: This figure shows the RKD estimate of a $1 increase to WBA on the probability of remaining on UI for longer than a given number of weeks, where each line includes a different sample of claimants based on the calendar year their claim began in.

Figure A7: Cyclicality of Behavioral Cost Mechanical Cost Ratios



Notes: BCMC ratios as described in Section 7. The blue line displays BCMC ratios considering the full UI spell length observed in the data, including PBD extensions present during recessions. The orange line ignores such PBD extensions when calculating the ratios by capping insured unemployment durations at 26 weeks.

Figure A8: Elasticity of Nonemployment Duration to UI Duration (Ratio of Nonemployment Duration Elasticity to UI Claim Duration Elasticity



Notes: This figure shows the ratio of two sets of estimates: the elasticity of nonemployment duration (measured in quarters) with respect to WBA over the elasticity of UI duration with respect to WBA. The ratio of these elasticities is equivalent to the elasticity of nonemployment duration with respect to UI duration. In order to match the measure of nonemployment duration, the measure of UI duration (typically measured in weeks) has been coarsened so that it is measured in quarters. The standard errors account for the covariance between the estimators for the 2 elasticities via a stacked regression, in which we estimate the two equations jointly and cluster the standard error by individual (similar stacked regressions can be seen in more detail in sec. 4 of Lee et al. (2021), sec. 4.4.2 of Lee and Lemieux (2010) and in sec. 5.3 of Pei et al. (2019)).

B Sensitivity Appendix

Our core RKD labor supply results are not particularly sensitive to variations in the bandwidth, specification, or sample definition. The top half of Table B1 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.

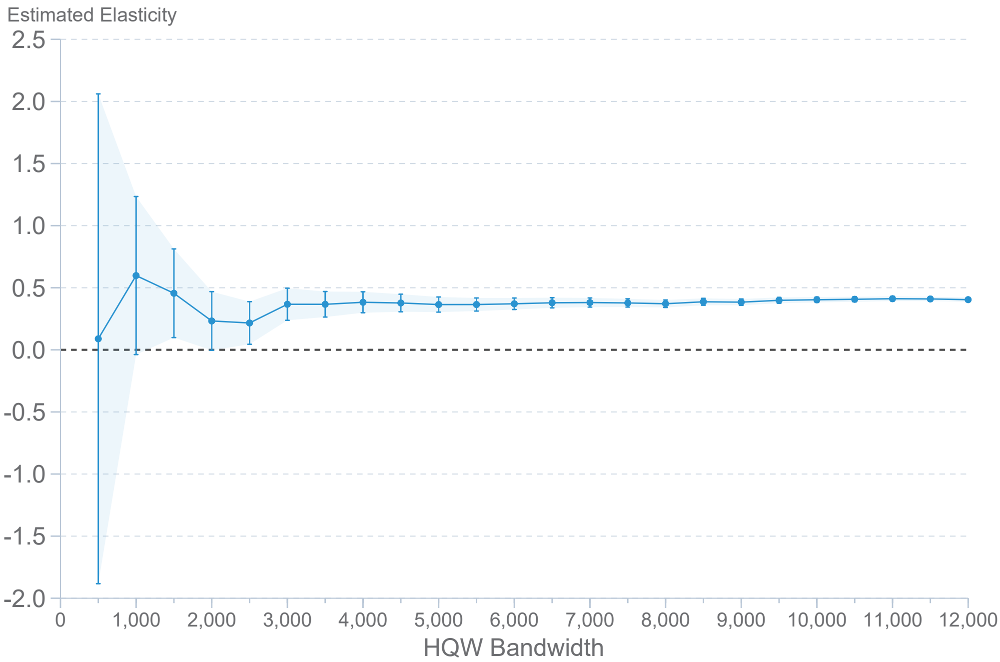
We opted for a common bandwidth for all of our results. To assess this choice, Panels A and B of Figure B1 illustrate the sensitivity of our main specification to changes in the bandwidth only. We see that the coefficient is relatively stable for each bandwidth over \$3,000, while the variance is much higher for smaller bandwidths. To probe this finding further, Figure B2 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.

Table B2 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.

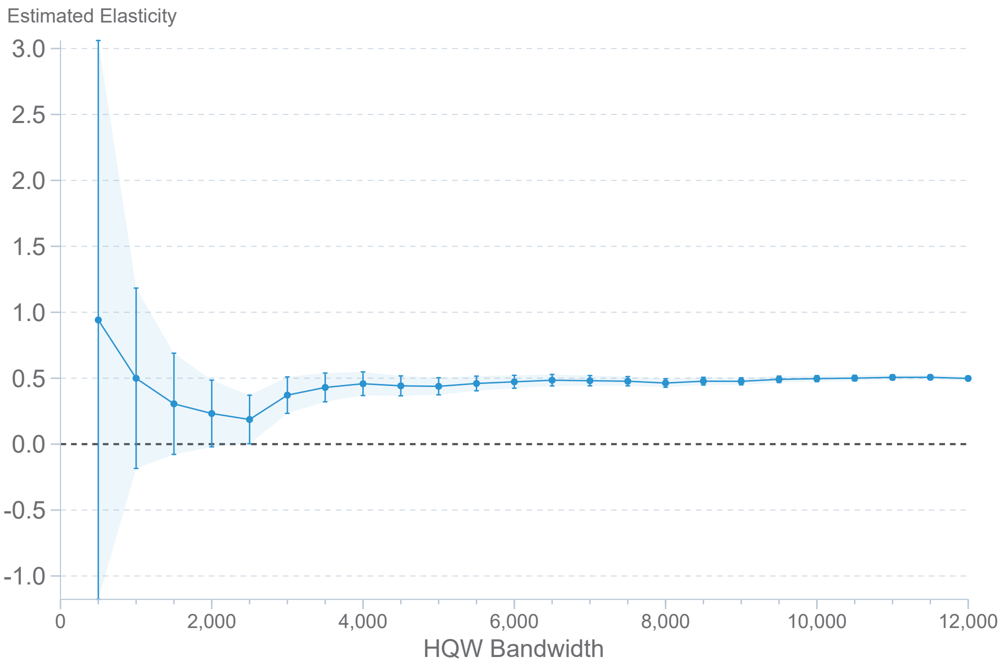
The results in Table B2 show that this more general estimation strategy confirms our main findings. The table shows the marginal effects and implied elasticities for both the kink in Weekly Benefit Amounts and (WBA) and MBA for the extended sample (column 1) and our main sample (column 2). Note that 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.

Figure B1: Robustness of Main Estimates to Varying Bandwidth

(a) 8-Week Survival Elasticity

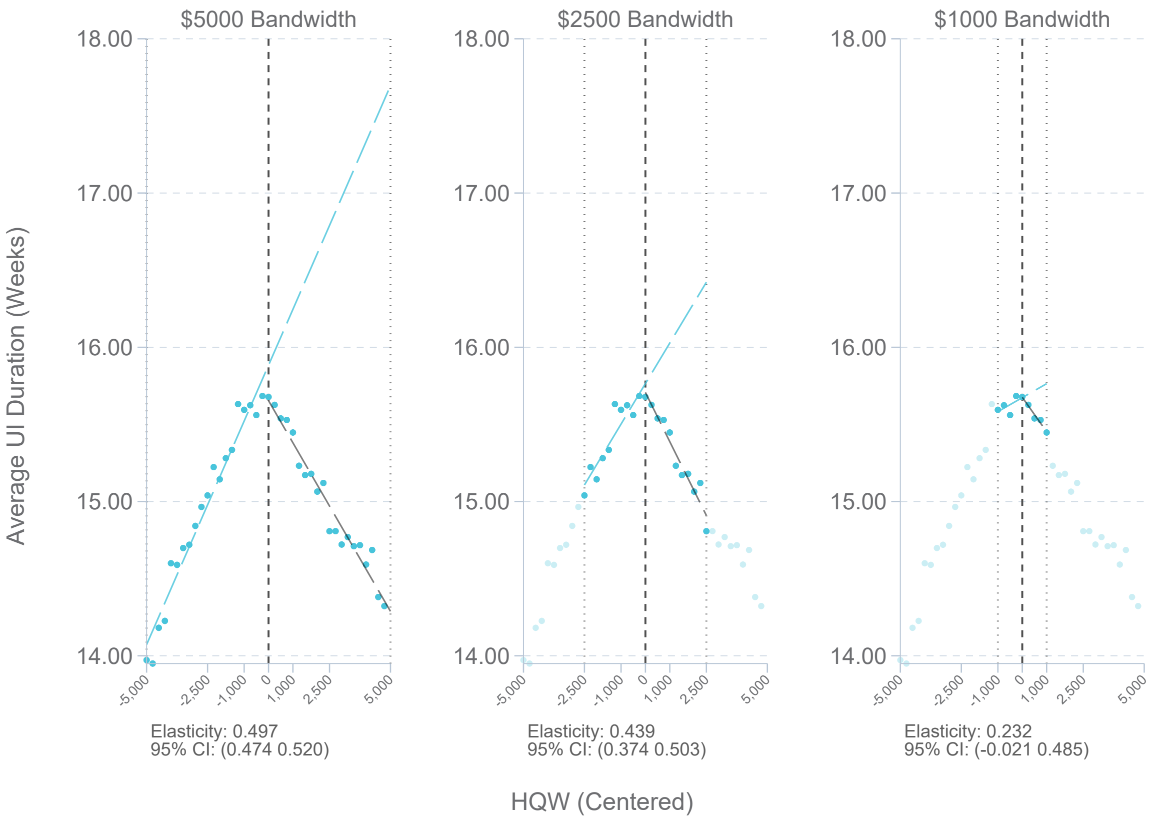


(b) Duration Elasticity



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 B2: Graphical Evaluation of the Bias-Variance Trade-off Associated with Different Bandwidths



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.

Table B1: 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 B2: 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.

C 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.

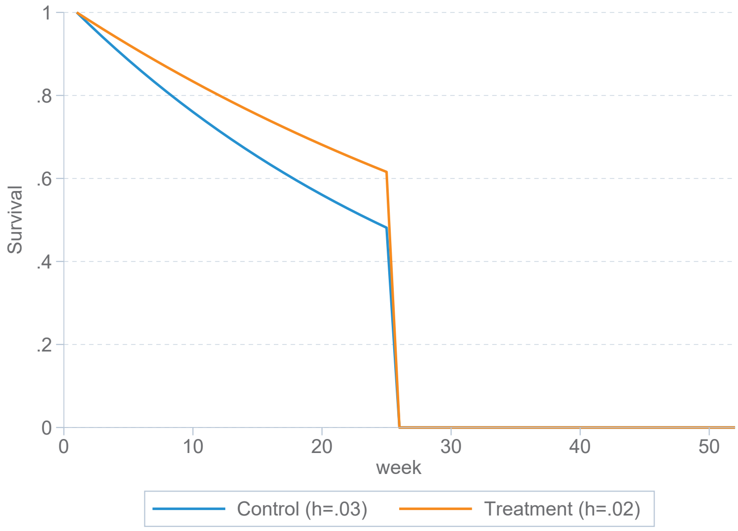
Consider a treatment (such as added benefits) that reduces the weekly exit hazard (h) from 0.03 for a control group to 0.02 for the treated. Each group's survival share diminishes by the relevant hazard for each week from 1 to 25, inclusive. On week 26, each group's exit hazard increases to 1 to simulate exhaustion under a 26-week PBD. Figure C1 plots survival curves, given by $(1 - h)n$.

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. Figure C2 below shows survival elasticities as a function of the week n at which survival is measured in our simulation.

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 0.02 for the treated and 0.03 for control.) Figure C3 shows that as potential benefit durations increase, so does the percent difference in durations between treatment and control groups.

Figure C1: Set-Up: Survival curves

(a) 26-Week PBD



(b) Simulating an Extension to 52 Weeks

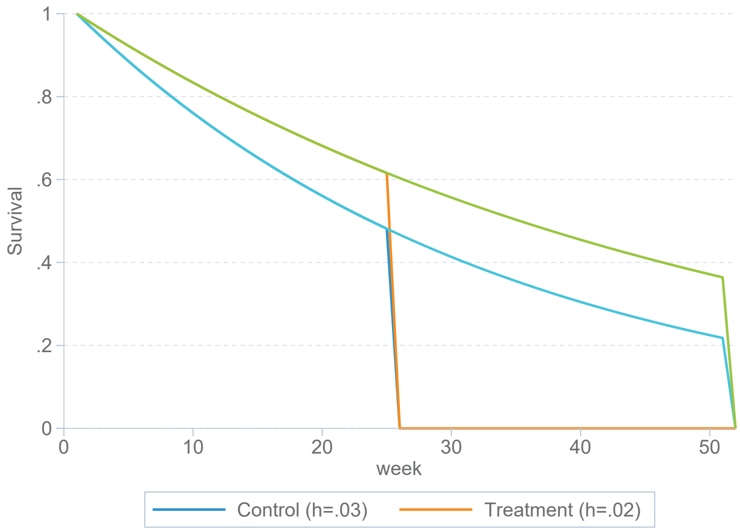
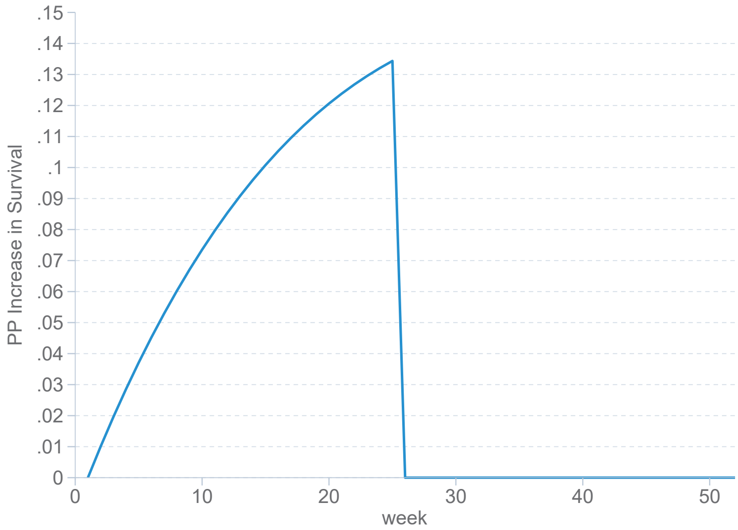


Figure C2: Survival Elasticities Increase by Week

(a) pp Increase (Marginal Effect)



(b) % Increase (Elasticity)

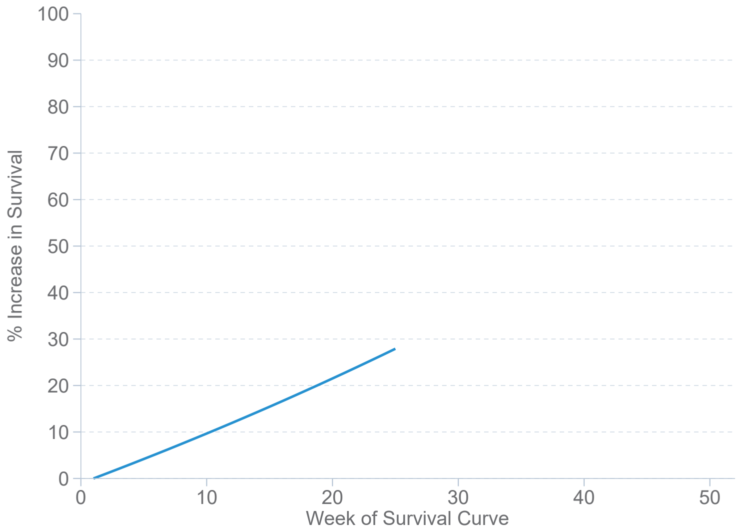


Figure C3: Duration Elasticities Increase by PBD