Claim Timing and Unemployment Insurance Benefit Generosity*

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Abstract

Unemployment Insurance replaces a percentage of prior earnings while a claimant is out of work. To implement the program, policymakers must define a base period from which prior earnings are measured. I analyze two implications of this previously unexamined policy choice. First, for claimants with volatile enough earnings, a commonly used base period structure creates "benefit risk"—a job loss at the wrong time implies lower benefit amounts. Second, since base periods are determined by the claim filing date, claimants can partially avoid the negative effects of this risk by strategically timing their claims. Using several new sources of administrative data from California's Unemployment Insurance program, I make four contributions. First, I demonstrate that exposure to benefit risk is widespread. Of roughly 21 million claimants in my sample, over 8 million are exposed to some level of benefit risk. Second, using a bunching approach I demonstrate that roughly 3% of affected claimants strategically delay their claims after a job loss in order to receive higher benefits. Third, I provide evidence that information frictions are a key barrier preventing more widespread use of this strategic response. Finally, I use a dynamic model of job search and Unemployment Insurance to show that the private welfare costs of benefit risk are large. After accounting for claim-timing responses, the average claimant would trade 4% of their expected Unemployment Insurance benefits to eliminate exposure to benefit risk. This number rises substantially among young and especially low-income claimants.

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1 Introduction

Many social insurance programs replace some percentage of prior earnings while a claimant is away from work during a shock (e.g. unemployment, disability, or the birth of a child). A large literature in economics has studied the optimal design of such programs, focusing on the optimal level of this replacement rate (Baily, 1978; Chetty, 2006). In this paper I analyze the implications of a different program parameter, the "base period" (BP). Largely ignored in the social insurance literature, the BP defines the time period from which prior earnings used to calculate benefits are measured. I show empirically and theoretically that this seemingly innocuous program characteristic can have substantial implications for social insurance claimants.

BPs are important for at least three reasons. First, for claimants with enough earnings volatility, benefit eligibility will vary across BPs. These changes are often dramatic and this implies that program rules expose some claimants to a particular type of variability in their benefits that I refer to as "benefit risk"—if their qualifying event (e.g. job loss) occurs at the "wrong" time they will receive lower benefits. Second, in many programs a claimant's BP is a function of the date on which the claimant chooses to file their claim. This creates a take-up decision on the *intensive* margin. Affected claimants can self-select into more generous benefits by strategically timing their claims. Third, claimants will vary in their exposure to benefit risk and are likely to vary in their ability to respond to it. Since benefit risk is driven by earnings volatility, more exposed claimants are likely to be less advantaged (Hardy and Ziliak, 2014).[1] Within the set of exposed claimants, barriers such as limited knowledge of the relevant program rules and imperfect control over claim-timing (due to, e.g., behavioral factors or liquidity constraints) are likely to prevent some claimants from making claim-timing adjustments.[2] Heterogeneous exposure and heterogeneous claim-timing responses to benefit risk each have the potential to alter the targeting properties of social insurance programs.

In California's Unemployment Insurance (UI) program, the empirical context for this paper, UI benefits are determined by earnings in a BP defined as the first four of the last five completed calendar quarters as of the claim date. It follows that claimants with volatile enough earnings histories will receive different benefits on a claim filed late in quarter q instead of early in quarter $q+1$. Similar BP structures are used in UI programs in every other state, as well as Paid Family Leave

[1] Past research (e.g., Hardy and Ziliak, 2014), has found that income volatility is concentrated at the top and bottom of the income distribution. However, social insurance programs typically cap payments at a maximum, insulating high-income claimants from benefit risk regardless of their earnings volatility.

[2] These barriers are analogous to similar constraints which have been found to explain incomplete take-up of various social programs on the extensive margin (Currie, 2006).

and Temporary Disability Insurance programs in several states. The purpose of this paper is to quantify the magnitude of benefit risk in the context of California's UI program, estimate its causal effect on the timing of UI claims, and determine the extent to which exposure and responsiveness to benefit risk are heterogeneous across different types of claimants. I utilize a new administrative dataset which includes the universe of UI claims filed in California (CA) between 1/1/2000 and 12/31/2019, as well as matched worker-firm quarterly earnings records for the universe of UI covered workers in CA from 1995-2019.[3] This setting is useful because of the size and richness of the data. Over 40 million claims are observed along with detailed information on the claimants and their firms.

My empirical analyses begin by quantifying exposure to benefit risk among UI claimants in CA. To measure benefit risk I compare the UI benefits a claimant would receive if their claim were filed in the quarter of their layoff to the benefits they would receive if their claim were filed in the next quarter. This measure is a useful starting point since it aligns with the claim-timing choice that the claimant faces—a UI claim cannot be filed before the job loss occurs and a job loser is very unlikely to delay their claim more than one full quarter. Benefit risk is found to be extremely common and is often very large. 38% of the relevant claims in the data were filed by workers whose available benefits over the life of the claim would change if they delayed claiming until the quarter after their layoff. Many claimants face dramatically large benefit changes. 3.8% of all claimants would see their benefits increase by $129 or more per week. Another 3.8% would see their benefits decrease by $45 or more. I also demonstrate that claimants more exposed to these claim-timing incentives are broadly less advantaged. This is important because policymakers may be interested in targeting benefits towards such claimants.

Visual evidence of bunching in claim date distributions strongly suggest that *some* of these claims are strategically delayed in order to receive higher benefits. Among claimants with an incentive to delay claiming until the quarter after their layoff (i.e., those whose UI benefits would be more generous if they delayed their claims until the next BP), claim date distributions show missing masses prior to the BP (quarter) change and excess masses immediately at the BP change. This bunching behavior is more pronounced among claimants with larger incentives to delay (larger increases in benefits with the new BP) and among claimants who are laid off closer to the end of a calendar quarter.

[3] This data has been acquired through a partnership between the Employment Development Department (EDD)—the state government agency in CA which administers the UI program—and the California Policy Lab.

I use bunching methods to quantify these responses and "count" claimants who strategically delay their claims. I find that between 2.5% and 5.4% of claimants incentivized to delay their claims to receive more generous benefits do so. While this is a meaningful proportion, it is perhaps more surprising how many claimants do not strategically delay—at least 94.6% of affected claimants *do not* bunch, effectively choosing the lower benefit level.

In order to investigate heterogeneity in these claim-timing responses across different types of UI claimants, and provide additional support for a causal interpretation of the bunching results, I develop an alternative regression-based approach. Specifically, I regress an indicator for whether the claimant delays their claim until the next BP on their incentive to delay (parameterized by the change in benefits between the two BPs) utilizing two separate identification strategies. First, I implement a selection-on-observables approach which sequentially adds sets of controls to the baseline model. Second, I exploit variation in the claim-timing incentives driven solely by large changes to the UI benefit schedule in CA during the early 2000s. These policy changes differentially affected claimants based on their prior earnings histories, were very large, and their effects (on any outcomes) have yet to be estimated in the literature. The two identification strategies produce similar results which are broadly in line with bunching methods—there is a moderately-sized but meaningful claim-timing response to the change in benefit generosity between base periods.

This more flexible approach also demonstrates that these responses are heterogeneous in one key expected dimension: the length of time the claimant needs to wait to receive the new BP. Unsurprisingly, claimants laid off very early in a calendar quarter (who would need to delay their claims several months in order to reach the new BP) are virtually unresponsive to these incentives. I also investigate heterogeneity along several other dimensions including a simple measure of predicted unemployment duration. This is an important dimension because delay is costly, and paying this cost may not be worthwhile for claimants experiencing short unemployment spells. I show that claim-timing responses are stronger in exactly the groups that are more exposed to benefit risk. This suggests that these claimants may be able to effectively undo some negative effects of benefit risk exposure. However, there is no clear pattern of heterogeneity by predicted unemployment duration. I also use similar approaches to show that claimants incentivized to claim sooner—because their benefits decrease with the new BP—do not engage in such strategic behavior.

Strategic claim-timing responses can be thought of as a take-up decision on the intensive margin. Viewed in this light, barriers to take-up are another group of potential explanations for the relatively limited amount of strategic claim-timing observed. Information frictions are one such barrier often found to be important in the wider literature on social programs (e.g., Mastrobuoni, 2011; Chetty et al., 2013; Armour, 2018; Barr and Turner, 2018; Finkelstein and Notowidigdo, 2019). Given the complex program rules involved with the claim-timing decision that I study, similar barriers may be relevant. In fact, the agency which administers UI in CA routinely notifies *some* claimants about upcoming BP changes. Every calendar quarter roughly 8,000 claimants who would have seen their benefits increase (by any amount) had they delayed their claim until the following week are notified of this fact and given the option to revisit their claim-timing decision. Roughly 400,000 claims in my sample received this information and were given the option to switch to the higher-benefit BP ex post. Just under 150,000 of these claims, roughly 39%, were delayed. In other words, among claimants who were incentivized to delay their claim, but failed to do so initially, 39% changed their decision when given the opportunity and made aware of the exact incentives that they faced. This suggests that incomplete information is a key barrier to take-up in this setting.

Finally, I provide a simple back-of-the-envelope calculation of the private welfare costs of benefit risk. I do this by adapting a standard dynamic model of job search and UI from Schmieder and Von Wachter (2016) to include benefit risk. Using the model I compare the expected utility of claimants given the current structure of the UI program (i.e., with benefit risk) to their expected utility in a hypothetical alternative system without benefit risk. I define a risk premium as the percentage reduction in expected UI benefits (in the no benefit risk system) that would make the claimant indifferent between the two systems. Using a combination of observed (e.g., the variation in benefits across base periods) and assumed parameters (e.g., counterfactual layoff dates), I calculate a risk premium for each claim in the data. These calculations can be made with and without allowing for strategic claim-timing responses. Without claim-timing responses the average claimant in the data would trade 6.4% of their expected UI benefits to remove benefit risk. Allowing for claim-timing responses reduces this average risk premium to 4%. While highly stylized, this exercise demonstrates that the differences in benefit generosity between adjacent base periods has meaningful welfare consequences for UI claimants.

This paper makes two important contributions to the literature. First, I demonstrate that the structure of base periods in social insurance programs can have important implications for program claimants. Due to earnings volatility, a commonly used base period structure exposes many less-advantaged claimants to benefit risk. In a simple model, I show that this risk has important welfare implications for UI claimants. A small body of existing work on social programs has called attention to the interaction of program design with earnings volatility. For example, estimating the ability of transfer programs to smooth periods of income instability (Hardy, 2017), and analyzing the interaction between earnings volatility, the design of recertification periods, and program churn (Prell, 2008; Pei, 2017). To my knowledge, this is the first work to highlight the connection between time aggregation in benefit formulas and benefit risk in the context of social insurance programs. This adds to a growing body of evidence on the role of time aggregation in social program benefit determinations more generally (e.g. Prell, 2008; Graves, 2012; Shore-Sheppard, 2014; Pei, 2017; Hong and Mommaerts, 2021) and in tax assessment (e.g. Vickrey, 1939; Milton and Mommaerts, 2020). Second, I demonstrate that claimants exposed to benefit risk in California's UI program strategically time their claims to take-up additional benefits. Although several papers have analyzed the extensive margin take-up decision in UI (whether to claim) from both theoretical and empirical perspectives (Blank and Card, 1991; McCall, 1995; Anderson and Meyer, 1997; Ebenstein and Stange, 2010; Auray et al., 2019), no existing work has identified or analyzed the claim-timing decision that I study.[4] Since the relevant parameters of CA's UI program are broadly similar to those used by UI programs in many other US states (and several other social insurance programs), each of these findings has implications which extend beyond my empirical setting.

The remainder of this paper is structured as follows. Section 2 describes the UI program in CA, with a specific focus on the relevant aspects of benefit schedules. Section 3 summarizes the data sources used in my empirical analyses. Section 4 presents some simple measures of exposure to benefit risk (including the magnitude of this risk and how it varies by certain claimant characteristics), and discusses a simple model which demonstrates the welfare implications of benefit risk. Section 5 demonstrates that some claimants strategically time their claims to avoid the negative effects of benefit risk, and quantifies this behavior. Sections 6 and 7 investigate heterogeneity in this strategic behavior. Section 8 provides some simple back-of-the-envelope calculations of the private welfare cost benefit risk. Finally, Section 9 concludes.

[4] Similar claim-timing decisions in other social insurance programs have also not yet been studied, with one exception. A large related literature on claim-timing in the Social Security program has attempted to explain why many retirees do not time their Social Security claims to maximize benefit receipt (see e.g. Coile et al., 2002; Sass et al., 2013; Henriques, 2018).

2 Institutional Context

2.1 UI Benefit Levels in California

The UI program in the US is run by individual states. States have the discretion to set benefit levels and durations (with some constraints), among other program parameters, but benefits in all states are determined based on prior earnings in a "Base Period" (BP) of four completed calendar quarters. The remainder of this section will describe the specific rules which determine BPs, benefit levels, and benefit durations in the State of California.

As shown in Figure 1 a UI claimant's BP is defined as the first four of the last five completed calendar quarters prior to the beginning date of their claim. The beginning date of the claim is typically set as the Sunday prior to the claim filed date. In other words, if a claim begins in quarter q the BP for that claim is the four quarter period beginning in quarter $q-5$ and ending in quarter $q-2$. If the same claimant instead begins their claim in quarter $q+1$, their BP spans quarters $q-4$ through $q-1$. The generosity of UI benefits are defined by two parameters, the weekly benefit amount (WBA), and the maximum number of weekly payments that can be made during the life of the claim (potential benefit duration, or PBD). The maximum $ amount payable during the life of the claim (maximum benefit amount, or MBA) is defined as the WBA multiplied by the PBD. If a claimant temporarily regains employment before exhausting their UI benefits, the claim can be reopened at any time during the 52-week period following the start date of the claim.

WBAs and PBDs are functions of earnings in the claimant's BP, specifically their total base period earnings, or Base Period Wages (BPW), and their earnings in the highest earning quarter of the base period, or High Quarter Wages (HQW). The specific functions used are:

$$WBA = \min\left\{HQW \cdot \frac{1}{13} \cdot RR,\, WBA_{max}\right\}$$

Where RR is the replacement rate (% of pre-claim earnings being replaced by UI benefits). While these formulas appear complex, they do have straightforward interpretations. The WBA is equal to a proportion, RR, of the average weekly wage in the high earning quarter, up to some WBA_{max}. Since my analyses focus on variation in WBA, additional information on the determination of PBDs is included in Appendix A.

Finally, to be UI eligible claimants must meet minimum earnings requirements ($HQW \geq$ \$1,300, or $HQW \geq \$900$ and $BPW \geq \$1{,}125$).

2.2 Policy Changes

Replacement rates and maximum $WBAs$ in California changed four times during the time period covered in my analyses. As shown in Figure 2 these four changes occurred on 1/1/2002, 1/1/2003, 1/1/2004, and 1/1/2005 and differentially affected workers based on their prior earnings histories. These changes were instituted as part of California's Senate Bill 40, passed on 10/1/2001. In each case, new claims beginning on or after the date of policy change received the new RR and WBA. Claims filed before January 1st of the relevant year received the prior year's WBA schedule. As described in more detail in the Section 2.3, this is helpful for my purposes as it creates claim-timing incentives which differentially affect claimants based on their earnings histories and layoff dates that are driven solely by the policy change.

These policy changes were significant, with maximum WBAs increasing by \$100 (from \$230 to \$330, +43%) on 1/1/2002, and by \$40 in each of the remaining years - to \$370 on 1/1/2003 (+12%), \$410 on 1/1/2004 (+11%), and finally \$450 on 1/1/2005 (+10%). To my knowledge, no existing research has studied the effects of these policy changes (on any outcomes).

2.3 The Claim-Timing Decision

With the formulas from Section 2.1 in mind we can consider some specific examples of earnings histories that would result in a claimant's benefit level and/or duration differing between two adjacent BPs. Table 1 demonstrates how different earnings histories translate into different claim-timing incentives. In each case I consider a claimant laid off in quarter q and show the WBA that the claimant would receive if their claim were filed in q vs $q+1$. The example earnings histories are chosen to highlight the existence of 4 different sources of variation in claim-timing incentives:

1. The magnitude of earnings volatility experienced by the claimant in the 5 quarters preceding the quarter of the job loss

2. The timing of this earnings volatility

3. Whether or not this earnings volatility pushes a claimant across the maximum WBA threshold

4. Whether the layoff occurs in a quarter of a policy change (i.e., the quarter before a new RR and/or WBA_{max} are applied to new claims)

In Table 1, comparing claimant 1 to claimant 2, demonstrates the clearest source of variation in claim-timing incentives. Claimant 1 has no earnings volatility in the relevant quarters so that BP_1 ($q1$-$q4$) provides the same benefits as BP_2. Claimant 2 on the other hand can increase their WBA by waiting for BP_2, since for this claimant $HQW_1 = \$10k < HQW_2 = \$15k$. Comparing claimant 2 to claimant 3 demonstrates the importance of both the magnitude and the timing of earnings volatility within the five relevant quarters. Despite having the same five quarterly earnings amounts as claimant 2, claimant 3 has no claim-timing incentive due to the ordering of those amounts. Defining the magnitude of earnings volatility as the standard deviation of the five quarterly earnings amounts, claimant 4 has earnings volatility identical in magnitude to claimant 2. However, claimant 4's earnings amounts are such that they are always eligible for the maximum WBA. Finally, claimant 6 is an example of a claimant whose claim-timing incentives are driven entirely by a policy change (here the increase in WBA_{max} for claims effective on or after 1/1/2005). Claimant 5 has an identical (relevant) earnings history to claimant 6 but was laid off one quarter earlier, not exposed to the policy change, and has no incentive to delay their claim.

3 Data

In my analyses I utilize administrative data from the UI program in the State of California for the years 1995-2019. Specifically, I combine three administrative datasets maintained by the State of California's Employment Development Department (EDD)[5]: Quarterly earnings records (1995-2019), the Quarterly Census of Employment and Wages (QCEW, 2000-2019), and UI claims microdata (2000-current).

The quarterly earnings records are administrative earnings records which exist in each state and were originally developed to administer the UI program (e.g., to determine benefit eligibility). Earnings are recorded at the firm-employee-quarter level. Each record includes a masked individual identifier, a masked UI Account Number (unique employer-level identifier used by EDD), and the total earnings paid to the employee in the relevant quarter for each UI-covered job in the State of California.

The QCEW data are administrative records of establishment-level earnings and employment which, much like the quarterly earnings records, exist in each state and are used to administer the UI program (e.g., to facilitate calculation of UI payroll tax liability). The QCEW data includes

[5]EDD is the government agency in CA which administers, among other programs, UI, Temporary Disability Insurance, and Paid Family Leave.

information on industry (6-digit NAICS code), size (number of employees), and location for all business establishments in CA with UI-covered employees, and is linkable to the quarterly earnings files via the UI account number.

UI claims microdata consists of a variety of information collected or produced by EDD in order to process UI claims. The UI claims microdata contains the universe of UI claims filed in CA on or after 1/1/2000 and includes a variety of detailed information about each claimant, their work history, their benefit eligibility, and the UI payments that they receive. Key information used in my analyses includes the claimant's self-reported last worked date, the date on which the claim was filed, a masked individual indentifier linkable to the quarterly earnings records, and detailed claimant demographics. In total the data contains roughly 41 million UI claims.

Similar datasets from other states have been used by other researchers.[6] However, the data used in this analysis is unique in two key ways. First, due to both the size of the state of California and the long time period covered, the dataset used in this paper is an order of magnitude larger than those previously used in the literature. Second, the UI claims data used in this analysis is abnormally rich, notably including exact dates of job losses and claims which are needed to study the questions of interest. A subset of these data has been used in a series of policy briefs written by myself and a team of researchers at the California Policy Lab (Bell et al., 2020).

The goal of my empirical analyses is to quantify both exposure to benefit risk and resulting strategic claim-timing behavior in the California UI program. The starting point for my analyses is the UI claims microdata. I exclude claims for which I cannot observe key information necessary to quantify exposure to benefit risk and/or claim-timing responses. Notably, my measures of benefit risk and claim-timing will require me to observe the date that the claimant lost their job and their quarterly earnings amounts in the six calendar quarter period ending with the quarter of their job loss. Therefore, I drop claimants with missing last worked dates, claims filed after 12/31/2019 (whose pre-claim earnings are not fully observed), claims for various special types of UI which follow different benefit rules or are otherwise different from typical UI claimants (e.g., the Disaster Unemployment Assistance program, and the Short Time Compensation program), claims filed by workers who either reside out of state or were denoted by EDD as having out-of-state wages in the relevant pre-claim time period (since out of state wages are used to determine benefit eligibility but

[6]For example, several researchers have used administrative datasets from state UI systems including Florida Johnston (forthcoming), Missouri Card et al. (2015); Johnston and Mas (2018), New York Meyer and Mok (2014), and Ohio Leung and Pei (2020) Several earlier papers also made use of the Continuous Wage and Benefit History data, which combined administrative data from the UI programs of several states during the 1970s and 80s (e.g. Anderson and Meyer, 1997; Landais, 2015).

are not observable in the data I use), and claims which would be monetarily ineligible in either of the two BPs of interest in my analyses. This leaves me with 21.7 million claims,[7] the characteristics of these claims are described in Table 2, Table 3, and Table 4.

4 Benefit Risk

In this section I begin by outlining the measures of benefit risk that I use in my analyses and describing how I calculate those quantities in the data. Next, I demonstrate that exposure to benefit risk in my setting is both substantial and highly concentrated among more disadvantaged groups. Finally, I present a simple framework to contextualize the potential welfare cost of benefit risk for UI claimants.

4.1 Measurement

Linking the UI claims microdata to the quarterly earnings files via the masked identifier allows me to observe complete earnings histories for every UI claimant. I use these earnings histories to calculate benefits that the claimant would be eligible for in two different base periods: the base period if their claim were filed during the quarter in which they reported working last (denoted BP_1), and the base period if their claim were filed in the following quarter (denoted BP_2). I define a simple measure of exposure to these BP-driven changes in benefit generosity as the change in WBA between these two adjacent BPs. The *WBA* if the claim were filed in the quarter *after* the layoff, denoted WBA_2, minus the *WBA* if the claim were filed in the quarter of the layoff WBA_1. I will refer to this measure throughout as ΔWBA.[8] Using the quarterly earnings data, the last worked date, and the benefit formulas described in Section 2.1, I calculate WBA_1 and WBA_2 for each claim in the sample.

ΔWBA will treat an additional $1 in UI benefits equally for all claimants, regardless of their prior income level. In order to provide a second measure scaled by prior earnings I define two replacement rates, RR_1 and RR_2, where again the subscript denotes the base period in which the claim is filed. In each case the numerator of the RR is the WBA in the relevant BP and

[7] In a subset of my analyses, presented in Section 8, I am interested in the variability of benefits over a larger set of *four* (instead of two) BPs, and therefore use a weaker restriction that the claimant is eligible in at least one of those four BPs. That sample includes 24 million claims.

[8] I similarly define $\Delta MBA = MBA_2 - MBA_1$ and $\Delta PBD = PBD_2 - PBD_1$. However, I focus on the WBA based measure in my descriptive analyses. Various results reported below for ΔWBA are also reported for these measures in Appendix C.

the denominator is a measure of earnings *across both* BPs. Specifically, the denominator is the average weekly earnings in the highest earning quarter of the five quarter period spanning both BPs. ΔRR is therefore interpretable as the percentage point change the replacement rate received by the claimant across these two BPs.[9]

As a first step in my analysis I identify all claimants in the full sample of 21.7 million claimants who are exposed to any amount of benefit risk as measured by $\Delta WBA \neq 0$ (or equivalently $\Delta RR \neq 0$). Figure 3 shows the distribution of in this measure of benefit risk in the subsample of 8.3 million claimants (38% of the full sample) who have some change in their WBA (or RR) between the two base periods ($\Delta WBA \neq 0$).[10] The bottom panel of the figure shows the same histogram in terms of ΔRR. Further, the magnitude of this risk is often large for each of these two measures. For example, 10% of the claimants included in the top panel of Figure 3, or 3.8% of the full sample, would see their WBA increase by at least $129 if their claim were delayed until the quarter after their layoff. Each panel also demonstrates that a small but meaningful portion of benefit risk exposure entails the loss of all benefits if a claim is filed "too early" or "too late." In each bin the portion of the bar accounted for by this group is shaded in black. The remainder (in gray) consists of claimants who are eligible in each BP, but for different benefit levels (i.e., each panel of the figure consists of two histograms which are "stacked," not overlaid).

These measures of benefit risk are of interest because they create incentives for claimants to alter the timing of their claims in order to take-up additional benefits. It is worth noting that these measures underestimate benefit risk created by the BP structure in the UI program for two reasons. First, these measures are limited to workers with realized UI claims and to the two potential BPs available to those claimants conditional on the realized date of their job loss. Second, the ΔWBA and ΔRR variables ignore exposure to benefit risk that operates through variation in PBD. These measures therefore also ignore the PBD extensions which occur in CA during downturns and have been studied, for example, by Farber et al. (2015) and Rothstein (2011). By increasing the total amount of benefits at stake, these extensions may amplify benefit risk and the associated claim-timing incentives. I abstract away form these sources of benefit risk for simplicity.

[9] Note that these replacement rates are *not* the replacement rates that are defined by the benefit functions in Section 2.1 (i.e., the slopes for $WBA < WBA_{max}$ in Figure 2), since those replacement rates use the HQW from a single BP as the denominator.

[10] Figure A1 shows similar results for ΔMBA and ΔPBD

4.2 Benefit Risk Exposure is Concentrated in Disadvantaged Groups

We would expect exposure to benefit risk to vary with various claimant characteristics for two reasons. First, benefit risk exposure is primarily driven by earnings volatility. Second, since WBAs are increasing in prior earnings and capped at a maximum amount, the highest earning claimants will remain unexposed to benefit risk even with substantial amounts of earnings volatility. Since prior research has found that income volatility is concentrated at the top and bottom of the income distribution, this implies that benefit risk is likely to be concentrated among the low-income (Hardy and Ziliak, 2014).

To visualize the relationship between benefit risk and claimant characteristics, I take the absolute value of ΔWBA (or ΔRR) and graph the average values of these amounts across groups defined by prior earnings, age, completed education, and race/ethnicity. Results are shown in Figure 4 and Figure 5. In each case, results demonstrate that less advantaged claimants are more exposed to benefit risk. Among the highest earning UI claimants in the sample (roughly the top two deciles of earnings in the 5 pre-claim quarters that makeup BP_1 and BP_2) there is virtually no exposure to benefit risk.[11] In contrast, the average claimant in the bottom two deciles of this prior earnings measure is exposed to a $47 change (in absolute value) in WBA between the two adjacent BPs. Younger and less educated claimants are also differentially exposed to benefit risk, although less dramatically. Considered in terms of RR the differences are even more stark. The highest earning claimants see virtually no change in RR between the two BPs, while the bottom deciles of claimants by prior earnings see a 16pp increase in the proportion of prior earnings replaced by UI.

These results uncover a previously unknown source of inequity in a key social insurance program. UI benefits received by lower-income, younger, less-educated, and minority claimants are volatile. The extent to which UI is able to protect such claimants from the risk of lost earnings during unemployment will depend upon whether their job loss and claim filing dates occur at a better or worse point in their earnings history. Since policy parameters similar to BPs are used in all other wage-replacing social insurance programs, these results also imply that similar inequities are likely to exist in those programs as well.

[11] The small amount of benefit risk exposure at the high end of the earnings distribution is primarily driven by high earners who lose their jobs in the quarter before one of the policy changes described in Section 2.2.

4.3 The private welfare costs of benefit risk

To provide a simple measure of the welfare costs of benefit risk, I adapt a standard dynamic model of job search and UI from Schmieder and Von Wachter (2016) to include benefit risk by making UI benefit levels b stochastic. Using this model, I compare the expected utility of a representative claimant given some level of benefit risk, $\mathbb{E}[U(b)]$, to the utility that that claimant would receive if benefit risk were removed while holding the *expected* benefit level constant, denoted $U(\mathbb{E}[b])$. Under the assumption that the claimant is risk averse, concave utility implies that $U(\mathbb{E}[b]) > \mathbb{E}[U(b)]$. Finally, I define a risk premium rp as the drop in consumption in the no benefit risk case that equalizes these two values: $U(\mathbb{E}[b] - rp) = \mathbb{E}[U(b)]$. This risk premium provides a simple measure of the private welfare cost of benefit risk which I take to the data later in the paper.

The model consists of a representative UI claimant who begins an insured unemployment spell at time t = 0. The model is in discrete time and ends at time T. While unemployed, the worker consumes $c_{u,t} = b_t + A$,where b_t is the unemployment benefit and A is income from other sources. The unemployed worker also exerts job search effort s_t at cost $\psi_t(s_t)$, remaining unemployed at time t with probability S_t. Upon reemployment consumption is $c_e = w - \tau$, where τ is a lump sum tax that finances UI benefits.

Unemployment benefits are set to b until $t = P$, at which point benefits are exhausted, i.e. $b_t = 0$ for $t \geq P$. I adapt the model to accommodate benefit risk by assuming that b is stochastic: $b \sim F(b)$. This distribution is discrete, with up to K possible values occurring with probability q_k, so that $\sum_{k=1}^{K} q_k = 1$ and $\sum_{k=1}^{K} q_k b_k = \mathbb{E}[b]$.

After accounting for benefit risk, the claimant's expected utility is:

$$\mathbb{E}[U] = \sum_{k=1}^{K} q_k \left(\sum_{t=0}^{P} S_t u(b_k + A) + \sum_{t=P+1}^{T} S_t u(A) + \sum_{t=0}^{T} (1 - S_t) u(w - \tau) - \sum_{t=0}^{T} S_t \psi_t(s_t) \right) \quad (1)$$

The claimant's risk premium rp solves:

$$\sum_{t=0}^{P} S_t u(\mathbb{E}[b] + A - rp) + \sum_{t=P+1}^{T} S_t u(A) + \sum_{t=0}^{T} (1 - S_t) u(w - \tau) - \sum_{t=0}^{T} S_t \psi_t(s_t) = \mathbb{E}[U] \quad (2)$$

This framework makes clear that there exists some private welfare cost of benefit risk so long as claimants are risk averse and claimants face some uncertainty over their UI benefit levels (i.e. $\exists k$

s.t. $q_k \neq 0$ and $b_k \neq \mathbb{E}[b]$). Earlier in this Section I quantified exposure to benefit risk using simple measures which directly connect to the claim-timing decisions I will study later in the paper. The risk premium measure presented here serves as a useful complement to those earlier measures, since it allows me to directly analyze the normative implications of benefit risk. In Section 8, I will use this framework to provide some simple back of the envelope calculations of the private welfare cost of benefit risk.

5 Do claimants strategically delay?

In this section I present results which demonstrate that some claimants respond to benefit risk by strategically timing their claims so that they receive more generous benefits. For simplicity, I focus on the effect of ΔRR on a simple measure of claim-timing: an indicator for whether the claim was filed in the first week of BP_2 (i.e. the first week of the quarter after the layoff) and focus on claimants with either no incentive to time their claim ($\Delta RR = 0$) or some incentive to delay ($\Delta RR > 0$).

5.1 Visual evidence

As a starting point, I present descriptive evidence in Figure 6 which shows the fraction of claimants in claim-date bins around BP changes in each of four groups defined by $\Delta RR = RR_2 - RR_1$. Groups are (1) claimants with $\Delta RR = 0$ and (2)-(4) defined by terciles of ΔRR among claimants with $\Delta RR > 0$. In the figure, I bin claim dates at the weekly level and center them around the closest BP change, so that week zero is the first week in which the new BP is effective (i.e., the week beginning with the first Sunday in a quarter). There is clear and substantial bunching at the BP change among claimants with large incentives to wait for the next BP. This is strong evidence that some claimants are aware of and responsive to these incentives. However, these bunching results abstract from a second useful source of variation in the data: the distance between the layoff date and the BP change.

We would expect that these claim-timing responses to benefit risk are decreasing in this distance. To provide visual evidence for this Figure 7 graphs several distributions of the number of weeks between layoff and claim dates. Each panel is limited to claimants laid off some number of weeks before the BP change, and shows these distributions in two groups: claimants with $\Delta RR = \$0$ (no incentive to delay), and claimants with $\Delta RR > 0$ (some incentive to delay). In each panel there is a clear spike in the claim-date distribution for the second group in exactly the week when the BP changes. While this spike is very small for long wait times, it clearly grows as the layoff date moves closer to the BP change.

5.2 How many claimants delay?

The results described in Section 5.1 strongly suggest that claimants are responsive to these incentives in deciding when to file their claim. However, they do not allow me to quantify the response or to do inference. To address these issues, I use a standard bunching approach to estimate the number of claimants with $\Delta RR > 0$ who strategically delay their claims. As described in Kleven (2016) for a more general setting, the approach uses the distribution of claim dates away from the date on which the BP changes to estimate a counterfactual distribution in the absence of any incentive to delay. By comparing the observed distribution of the claim dates to this counterfactual distribution around the "notch," we can quantify the bunching response—effectively counting the number of claims that were strategically delayed until the start of the next BP.

I start by estimating the following regression among claims with $\Delta RR > 0$, using data binned to the day of claim d, where d is centered around the nearest BP change as in Figure 6:

$$claims = \sum_{i=0}^{p} \beta_i^p d + \sum_{k=-14}^{13} \gamma_k \cdot 1\{d = k\} + \sum_{j=1}^{6} \delta_j \cdot 1\{dow(d) = j\} + \epsilon \tag{3}$$

Where *claims* is the number of claims filed on day d, δ_j are coefficients on day-of-week dummies with Sundays the excluded category[12], p is the polynomial order used to fit the distribution, and days -14 to 13 make up the "manipulation" region. I determine the manipulation region in an ad hoc manner by visually inspecting the distribution. While more automated approaches exist and are common in the literature, the combination of diffuse bunching and a relatively discrete running variable in my setting make these approaches difficult to implement.

Next, I estimate the counterfactual distribution as the fitted value from this regression excluding the manipulation region dummies:

[12] Likely due to a combination of claimant behavior and EDD processes, the claim date distribution exhibits substantial day-of-week effects, with relatively few claims filed on weekends. This is akin to the "round number bunching" issue common in tax bunching settings (see e.g., Kleven and Waseem (2013)).

$$\widehat{claims} = \sum_{i=0}^{p} \widehat{\beta}_i^p d + \sum_{j=1}^{6} \widehat{\delta}_j \cdot 1\{dow(d) = j\} \tag{4}$$

Finally, I sum the gaps between the counterfactual distribution and the empirical distribution of claim dates for each claim date above the notch and within the manipulation region. This is the "excess mass," or the number claims that were strategically delayed. Standard errors are calculated via bootstrap.

The key threat to the validity of this approach is that the distribution outside the manipulation region may not serve as an adequate counterfactual. This would occur if, for example, there were some other reason unrelated to the change in benefit generosity for claimants to bunch at day zero. To investigate, and if necessary correct for, this concern we can exploit the group of claims with no incentive to bunch (i.e., $\Delta RR = 0$). First testing for bunching in this group, and second, exploiting variation within the $\Delta RR > 0$ group to demonstrate that the amount of bunching grows with incentives to bunch.

Figure 8 shows, for both the $\Delta RR > 0$ and the $\Delta RR = 0$ groups, the empirical distribution of claim dates, the counterfactual distribution estimated as described above, the bunching estimate (cumulative distance between empirical and counterfactual distributions in bunching region), a bootstrapped standard error for the bunching estimate, and the bunching estimate as a percentage of all claims in the relevant group. In the $\Delta RR > 0$ group, we see a clear excess mass of claims on the right of the cutoff and a clear missing mass on the left much like Figure 6. I estimate that 278,000 claims, or 5.4% of claims with $\Delta RR > 0$, bunch—i.e. are strategically delayed until the BP changes at day zero. However, we see in the bottom panel for the $\Delta RR = 0$ that bunching also occurs in the control group, although it is much less substantial at 2.5% of claims.

One potential explanation for the presence of bunching in the control group is demonstrated in Figure A2, which shows that layoffs are concentrated in the last week of a quarter in my sample (regardless of ΔRR values). Since, as shown in Figure 7, the vast majority of claims are filed in the week after the job loss, this would lead to an excess mass of claims in the first week of a base period in all ΔRR groups. Regardless of the underlying reason, a reassuring feature of the bunching apparent in the "treatment" group is that the magnitude of the bunching behavior grows with incentives to bunch. In Figure A3 I implement the same bunching approach used above in groups defined by terciles of ΔRR values (excluding claims with $\Delta RR = 0$). In the first tercile ($\Delta RR < .06$), 4.1% of claimants bunch. This number grows to 4.8% in the second tercile ($.06 \leq \Delta RR < .29$) and 7.4% in the third ($\Delta RR \geq .29$).

The bunching results provide strong evidence that between 2.8% and 5.4% of claimants with $\Delta RR > 0$ strategically delay their claims. Perhaps more interesting is the implication that at least 94.6% *do not* strategically delay, effectively choosing the lower benefit level. In the remainder of this section I provide some additional evidence to support the bunching results before turning to the question of why some claimants engage in this strategic behavior while many others do not.

5.3 An Alternative Approach

The bunching results from Section 5.2 quantify the number of claimants that strategically delay claiming. However, both the diffuseness of the bunching and the existence of bunching among claimants with no incentive to delay call for additional evidence to support this interpretation. Further, the bunching results abstract away from the dramatic variation in incentives *within* the subset of claimants with $\Delta RR > 0$. Figure 6 suggests that strategic claim delay is very responsive to the magnitude of ΔRR within this group. Finally, Figure 7 clearly shows the importance of a second key dimension of heterogeneity in strategic delay—the distance between the job loss date and the next BP—which is also ignored by the simple bunching approach. To address these issues, I develop an alternative and more flexible approach to quantify strategic claim-timing responses by estimating regressions of the following form:

$$delay_c = \sum_{-12}^{-1} \beta_1^\tau 1\{week_c = \tau\} + \sum_{-12}^{-1} \beta_2^\tau 1\{week_c = \tau\} \cdot \Delta RR_c + \beta_3 \Delta RR_c + X_c' \beta_4 + \gamma_{d(c)} + \psi_{q(c)} + \epsilon_c \quad (5)$$

Subscripts denote claims (c), week of layoff (τ, in event time relative to the BP change[13]), weekday of layoff (d), and quarter of layoff (q). The outcome is some measure of waiting for the quarter *after* the layoff to claim (i.e., proxies for a claimant "choosing" the new BP), X_c is a large vector of covariates (e.g., demographics, employment history, etc) which are interacted with week dummies in all specifications unless otherwise noted, γ_d and ψ_q are weekday and quarter of layoff FEs. The coefficients of interest are β_3 and the β_2^τs.

[13]BPs typically include 13 weeks but are sometimes 14 weeks long. I define τ to range from 1 to 13, where 13 includes claimants laid off 13 or 14 weeks before a BP change and is the omitted category.

For my main results I use an indicator for whether the claim was filed in the first week of the new BP as the outcome. Figure 9 show results for a baseline regression, estimated among all claimants in the analysis sample with $\Delta RR \geq 0$ who are eligible in both BPs (the latter restriction is made to lessen concerns about extensive margin effects). In Figure 9 I start with a simple specification that includes only a control for ΔPBD and then progressively add sets of additional covariates to the model. The sets of controls are:

1. "Baseline": week of layoff dummies, layoff date FEs (quarter-year and day of week), and ΔPBD

2. "Demographics": Completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code[14]

3. "Prior Earnings": Average quarterly earnings in the five completed calendar quarters pre-claim and a measure of the magnitude of "effective" earnings volatility in these quarters (more detail on this below)

4. "Pre-separation Employer": Reason for job loss, indicator for whether the claimant expects a recall to the separating employer, size of separating employer (# employees and # establishments), average earnings of employees at separating employer during quarter of sepration, and sector (two-digit NAICS code) of separating employer[15]

Each claimant has five quarterly earnings amounts which make up BP_1 and BP_2 and therefore influence ΔRR. A simple way to control for earnings volatility would be to include some measure of the dispersion of these five quarterly earnings amounts in equation 5. However, earnings volatility is only relevant for ΔRR if it occurs at earnings levels below those which correspond to maximum benefits. To measure only the volatility in earnings that matters for ΔRR I instead calculate $WBAs$ for each of the four possible combinations of four earnings amounts from this set of five quarters. Two of these WBA values are WBA_1 and WBA_2, the remaining do not correspond to any possible BP (or claim). My measure of "effective" earnings volatility is the standard deviation of these four $WBAs$.

Results are stable across models and suggest that a 10pp increase in ΔRR leads to a 1.4-1.8pp increase in the probability of filing a claim in the first week of the next BP among claimants who

[14] To keep the number of regressors manageable I do not interact the zip code dummies with week dummies.
[15] I do not interact the NAICS sector dummies with week dummies.

lost their jobs 2-4 weeks before the BP change. This effect fades out as the distance between the layoff date and the BP change widens. As shown in Table 3, 7% of all claimants file their claim in the first week of the quarter following their layoff, so the marginal effects for claimants laid off towards the end of a quarter are clearly meaningfully large. While often statistically significantly different from zero, the marginal effects among claimants laid off very early in a quarter are very small (or zero). This makes sense as only those claimants with extremely large ΔRR values should be willing to delay their claim a full calendar quarter. A causal interpretation of these results relies on the assumption that there are no unobserved confounding factors and this assumption is ultimately untestable. However, coefficients are remarkably stable as controls are added and this is reassuring. In the next subsection I briefly describe several related specifications which establish the robustness of these results and support a causal interpretation.

5.4 Robustness & Threats to Identification

5.4.1 Alternative Specifications

To demonstrate robustness, I vary the specifications from Section 5.3 in several dimensions. First, using ΔWBA as the RHS variable of interest instead of ΔRR in Figure A5. Second, in all the aforementioned figures I include claims with $\Delta RR = 0$, but these claimants are very different from claims with $\Delta RR > 0$ and it may be preferable to drop them (e.g., to differentiate this approach from the bunching by focusing on variation in incentives within the $\Delta RR > 0$ group). The (a) panels of Figure A6 and Figure A4 do this for each treatment variable. Coefficients are notably less stable when controls are added, but the general pattern remains.

One potentially concerning pattern in these results is the large drop in the effect of ΔRR on claim delay for claimants laid off in the last week of a BP relative to the second to last week. There are two competing explanations for this pattern. First, as can be seen in Figure 7, the vast majority of claimants file their claims either in the week of their layoff or the week after. This could explain the pattern in claim-timing responses since it implies that the vast majority of claimants laid off in week -1 would have filed their claim in week 0 anyway. Second, the pattern could reflect claimants with larger ΔRR values waiting longer to claim for reasons unrelated to the benefit change between BPs. An alternative outcome, robust to these concerns, is an indicator for whether the claimant waited at least two weeks after their layoff to file their claim. For this outcome we should expect to see a zero effect among claimants laid off in the last week of a BP since they are only incentivized to delay their claim by one week. Results using this alternative claim delay measure as an outcome are shown in Figure A7. These results are less stable as groups of control variables are added and at times produce counterintuitive negative effects of ΔRR on claim delay. However, the broad patterns are the same: claimants laid off early in the quarter show very small to zero responses to these incentives, claimants laid off late in the quarter are substantially more likely to delay their claim if doing so provides higher benefits as measured by ΔRR. Importantly, the estimate for layoffs occurring in week -1 is zero.

5.4.2 Exploiting Policy Driven Variation

As described above, causal interpretations of the results in Section 5.3 rely on selection-on-observables assumptions. An alternative approach is to exploit policy-driven variation in claim-timing incentives shown in Figure 2 and described in Section 2.2. Each policy change consists of a change to the WBA schedule, specifically an increase in the WBA_{max} value and in some cases an increase in the replacement rate. These changes differentially affect claimants based on their HQW values. Claimants with higher prior earnings amounts receive larger replacement rate increases.

To isolate the variation in claim-timing incentives driven by these policy changes I assign each claim to an earnings group based on the HQW of the claimant in each of the two possible BPs[16] and add earnings group level fixed effects to equation 5. In other words, I estimate the following equation:

$$
\begin{aligned}
delay_c = \sum_{-12}^{-1} \beta_1^{\tau} 1\{week_c = \tau\} + \sum_{-12}^{-1} \beta_2^{\tau} 1\{week_c = \tau\} \cdot \Delta RR_c \\
+ \beta_3 \Delta RR_c + X_c' \beta_4 + \gamma_{d(c)} + \psi_{q(c)} + \delta_g + \epsilon_c \quad (6)
\end{aligned}
$$

Where the subscripts denote earnings groups (g) and quarter of layoff (q). For this approach I also limit the sample to claimants who were laid off between 1/1/2000 and 12/31/2007.

Figure 10 shows results from this regression using an indicator for whether the claim was filed in the first week of the next BP as the outcome, and comparable selection-on-observables results from Figure 9, both including the most complete set of controls. Results from the two approaches

[16] The most flexible version of this would require one earnings group for each possible pair of WBAs implied by the HQWs. To make this more tractable I divide each HQW into 26 bins and assign the average change in benefits within the bin.

are broadly similar. Estimates from the diff-in-diff approach imply that an increase of 10pp in ΔRR has moderate effects on claim delay among claimants laid off early in the quarter (a roughly 1.5pp increase in the outcome among claimants laid off in the first 3 weeks of a quarter) that grow to nearly 5pp for claimants laid off 2-3 weeks before the BP change. Results are broadly similar to those in Section 5.3, although effects are generally larger. Potential explanations for larger responses to policy-change-driven incentives to delay include increased salience (as described in Section 2.2 these policy changes where announced several months ahead of time) and differences between the groups of claimants exposed to the two types of variation (earnings volatility driven variation is concentrated among the young and low-income while policy change driven variation is more broadly distributed).

Sensitivity of these results to alternative specifications and tests for differential pre-trends in an event study setup are available in Appendix B.

5.4.3 Endogenous Layoff Dates

Both Figure 6 and Figure 7 consider claim-timing behavior taking the layoff date as given. However, it is also possible for claimants and/or their employers to manipulate layoff dates in response to these incentives. On the claimant side, this could occur through negotiation between the worker and their employer. If the worker is aware of their benefit risk in advance they may be able to influence their employer to alter the timing of the separation. On the employer side, experience rated UI payroll taxes provide an incentive for firms to minimize receipt of UI benefits by their former employees. Experience rating of UI payroll taxes has been shown to influence hiring (Johnston, forthcoming), firm location decisions (Guo, 2019), and employer challenges to former employees' UI benefit eligibility (Anderson and Meyer, 2000; Lachowska et al., 2021). This characteristic of the UI system may also induce employers to systematically time layoffs when benefit eligibility is lowest. As a first pass at investigating this margin of response to benefit risk, I produce a bunching figure analogous to Figure 6 where the X-axis is the layoff date relative to the new BP. Figure A2 provides little evidence for strategic timing of layoff dates around BP changes.

In a complementary approach I also reestimate all regressions described in Section 5.3 in a sample of claims that result from mass layoff events. The logic for this exercise is that employers may have less ability to strategically time layoffs in such circumstances, both because mass layoff events are often thought of as relatively unexpected shocks that firms would not have been able to plan, and because ΔRR is likely to vary substantially across workers suffering from the same mass layoff event—making it difficult for the affected firm to time layoffs accordingly. This mass layoff sample consists of a subset of the full sample of 21.7m claims, which appear likely to have occurred due to a mass layoff event at their separating employer. Specifically, I consider a firm to have experienced a MLO event if at least 20% of the firm's employees (as measured in the first month of the quarter in the QCEW) file a UI claim and report a last worked date that falls in the same one-week period. 3.4m claims are included in the MLO sample. Results using this sample are shown in the (c) panels of Figure A4 and Figure A6.

6 Who strategically times their claim?

In this section I provide a brief narrative overview of dimensions along which we would expect claim-timing responses to vary. I then investigate these potential heterogeneous effects empirically.

6.1 Who should we expect to strategically time their claims?

While the claim-timing decision is clearly complex, there are several key dimensions along which claim-timing responses should be expected to vary. First, and most simply, we would expect that claimants who expect to have longer unemployment spells would be more likely to delay. This is because, relative to claiming immediately, delay involves an initial temporary *decrease* in UI benefits received (from WBA_1 to \$0) while the claimant waits to file their claim, followed by an increase from WBA_1 to WBA_2 for the remainder of the spell. If the spell is too short, delay may cause the claimant to experience a decrease in total benefits received despite the increase in WBA. Second, we might expect claimants to respond differently to different changes in their WBA between BPs. For example, they might ignore small changes while responding strongly to very large ones. Similarly, they may respond asymmetrically so that a positive value of ΔWBA induces claim-delay while a negative value of ΔWBA does not induce claimants to claim sooner. This might occur if, for example, claim-delay is less costly than speeding up the claiming process.

6.2 Heterogeneity in claim-timing responses

To investigate heterogeneity in the strategic behavior described in Section 5, I make three changes to equation 5. First, I limit the sample to job losses occurring in the last 5 weeks of a calendar quarter, since that is the range in which claim-timing responses are concentrated as per, e.g., Figure 9. Second, I drop interactions between τ (the number of weeks between the job loss and

BP change) and ΔRR. This simplifies exposition and allows me to report only one coefficient per subgroup—the average effect of ΔRR. Third, I multiply the estimated coefficient by $\overline{\Delta RR}/\overline{delay}$ so that the reported values are interpretable as elasticities evaluated at the means. This ensures that variation in the effect of ΔRR on claim delay across subgroups is isolated from variation in the distribution of ΔRR and claim delay across subgroups.

The resulting specification is:

$$delay_c = \sum_{\tau=-4}^{-1} \beta_1^{\tau} 1\{week_c = \tau\} + \beta_2 \Delta RR_c + X_c' \delta + \psi_{q(c)} + \epsilon_c \tag{7}$$

This equation is estimated via OLS with cluster robust standard errors at the layoff-quarter level. The coefficient of interest is $\hat{\beta}_2$. Results are shown in Figure 11 which report $\hat{\beta}_2 \cdot \left(\overline{\Delta RR}/\overline{delay}\right)$, with standard errors calculated by the delta method.

Two broad patterns stand out in these plots. First, groups which are more exposed to benefit risk (as shown in Figure 4) are more responsive to the incentive to delay. Since the results in these plots are elasticities, this is not driven by the differing magnitude of those incentives across groups unless the response is nonlinear. This is important because it suggests that the groups most negatively affected by benefit risk may actually be those who are most likely to avoid some negative consequences of this risk by strategically delaying their claims. Second, claimants are much more responsive to these incentives during the time period encompassing the policy changes described in Section 5.4.2, than afterwards,[17] but effects are otherwise relatively consistent over time. This is surprising since we would expect that claimants expecting long unemployment spells—such as those losing their jobs during the Great Recession—would be more responsive to these claim-timing incentives.

To further investigate heterogeneity in claim-timing responses by expected unemployment duration, I produce a simple nonparametric prediction of unemployment duration for each claim in my sample. These predictions exploit information on the number of weekly UI payments received by each claimant. A complication with this proxy for expected unemployment duration is that realized unemployment duration (the outcome that I predict) is differentially censored. Different claimants often have different PBD values, meaning that the maximum number of weekly payments a claimant can receive varies. To deal with this I predict the probability that each claimant receives

[17] This is not surprising in light of the results described in Section 5.4.2

at least 13 weekly payments, since 13 is the minimum PBD in CA throughout the time period covered by my sample. Since this is meant to be suggestive, my predictions correspond to the average value of this outcome in cells defined by claimant age, gender, recall status, industry, tenure at separating employer, and job loss. Finally, I bin claimants into 9 groups based on the values of this predicted probability and estimate the same regression shown above in subgroups defined by these groups. Results shown in Figure 12 are somewhat consistent with the hypothesized effects. Groups with the lowest and highest predicted probabilities of suffering long unemployment spells fit the hypothesized heterogeneity—claimants with short predicted unemployment spells are less responsive and those with long predicted spells are more responsive. However, responses are very similar across the remaining groups.

6.3 Claimants do not "hurry up"

Taken together, the results in Figure 6, Figure 7, Figure 9, and Figure 11 show that claimants with an incentive to delay are more likely to do so if that incentive (e.g., ΔRR) is larger, if the "cost" of delay (e.g., the length of time between the job loss and the new BP where benefits increase) is smaller, if the claimant is low-income, if the claimant has less completed education, if the claimant is young, or if the claimant is Black. This heterogeneity is broadly consistent with a framework where claimants are weighing the costs and benefits of delay and responding accordingly. However, there is one key exception to this pattern.

I have focused thus far in this section on the subset of claimants with an incentive to delay claiming. However, as noted in Section 4, there are also a meaningful number of claimants who have an incentive to speed up their claim—i.e., claimants whose benefits will decrease in the new BP relative to the current one ($\Delta RR < 0$). To investigate this, Figure 13 plots the distribution of weeks elapsed between job loss and claim filing for two groups of claimants, a control group with $\Delta RR = 0$ and a treatment group with $\Delta RR < 0$ (much like Figure 7, with the $\Delta RR < 0$ group replacing the $\Delta RR > 0$ group). The plot is reproduced for several subsamples, each limited to claimants losing their job some number of weeks before the BP change. In these figures there is no evidence that claimants with an incentive to speed up their claims are differentially likely to file their claims before the BP changes and their benefits decrease.

The first finding highlighted in this section is that 5.4% of claimants with $\Delta RR > 0$ strategically delay their claims. Perhaps more interesting, is the implication that 94.6% *do not*, effectively choosing the lower benefit level. This evidence that such strategic behavior does not occur at all in the other direction is important because it helps us to better understand this latter group. While some of these claimants may have simply decided that delay is not worthwhile, this cannot be the only explanation.

7 Information as a Barrier to Strategic Claim-Timing Responses

Information frictions are often found to be important barriers to take-up in the wider literature on social programs (e.g. Mastrobuoni, 2011; Chetty et al., 2013; Armour, 2018; Barr and Turner, 2018; Finkelstein and Notowidigdo, 2019) and are likely to be especially relevant here given the complexity of the choice that claimants face. To study information frictions in this context, I exploit a subset of UI claimants who are explicitly informed about these incentives by the UI agency in California as part of the normal claim-processing steps undertaken by the agency.

Every calendar quarter roughly 8,000 claimants who would have seen their benefits increase (by any amount) had they delayed their claim until the following week are notified of this fact by the agency and given the option to revisit their claim-timing decision. In my sample, these informed claims include every claim filed on day -7 to -1 in the top panel of Figure 8. For the typical claimant, a claim begins on the Sunday prior to the date that they file their claim. Whichever quarter that Sunday falls in determines their BP. An exception is made for claimants who file their claim in the last week of a BP, but would have seen their benefits increase (by any amount) if they had instead waited until the next Sunday where the new BP would become effective. These claimants are notified by the agency that their benefits would have been different if they had delayed their claim one more week, made aware of the exact change in benefits they would have been eligible for, and given the opportunity to revisit their decision (i.e., to delay their claim by one week after it had already been filed). Figure A9 shows what such a claimant would see if they were filing their claim online.

Table 5 presents some simple descriptive statistics on the 398,546 claims in my sample that received this information and option to switch to the higher-benefit BP ex post. Just over 154,000 of these claims, roughly 39%, were delayed. In other words, among claimants who were incentivized to delay their claim, but failed to do so initially, 39% changed their decision when given the opportunity and made aware of the exact incentives that they faced.

8 The Welfare Cost of Benefit Risk

The two key results shown so far in this paper are that earnings volatility exposes some UI claimants to large and costly benefit risk, and that some exposed claimants are able to reduce the negative consequences of benefit risk by strategically timing their claims. This section brings these two results together within the theoretical framework laid out in Section 4.3. This allows me to quantify the private welfare cost of benefit risk accounting for claim-timing responses.

The starting point for this exercise is equation 2, which defines the risk premium in terms of the claimants expected utility in states of the world with and without benefit risk:

$$\sum_{t=0}^{P} S_t u(\mathbb{E}[b] + A - rp) + \sum_{t=P+1}^{T} S_t u(A) + \sum_{t=0}^{T}(1 - S_t)u(w - \tau) - \sum_{t=0}^{T} S_t \psi_t(s_t) = \mathbb{E}[U]$$

Where $\mathbb{E}[U]$ is defined as:

$$\mathbb{E}[U] = \sum_{k=1}^{K} q_k \left(\sum_{t=0}^{P} S_t u(b_k + A) + \sum_{t=P+1}^{T} S_t u(A) + \sum_{t=0}^{T}(1 - S_t)u(w - \tau) - \sum_{t=0}^{T} S_t \psi_t(s_t) \right)$$

In earlier sections I focused on a simple two-BP measure of benefit risk which was directly tied to the claim-timing decision that claimants face. Here I widen that view in order to provide a fuller picture of the welfare costs of benefit risk. Figure 14 visualizes the approach that I take. For each claim in my sample, I start with the observed job loss date from the claims data. First, I allow for variability in the week of the job loss by assuming that the actual week was drawn from an approximately normal distribution centered at the actual week, and bounded above and below by one calendar quarter.[18] Next, I allow for variability in how long each claimant waits after their job loss to file their claim by applying the empirical probability distribution of the number of weeks between job loss and claim filing. In order to account for the role of strategic claim-timing responses, I retrieve this distribution for seven separate groups of claimants based on their ΔRR values: those with $\Delta RR = 0$, three groups defined by terciles of ΔRR if $\Delta RR < 0$, and three groups defined by terciles of ΔRR if $\Delta RR > 0$. Combining the observed layoff week, the probability distribution of

[18] Specifically, I use a binomial distribution with 26 trials and a probability of success of 0.5, since binomial distributions with a probability of success close to 0.5 are approximately normal, and I want to ensure that the counterfactual job loss weeks are no more than one calendar quarter after the actual job loss so that I can observe enough pre-claim earnings history to determine counterfactual benefits.

counterfactual layoff weeks occurring ≤ 1 quarter earlier or later, and the probability distribution of time elapsed between the job loss and the claim, I calculate the probability that each of four potential BPs is realized. (These probabilities correspond to q_k in equation 1 where $k = \{1, 2, 3, 4\}$.)

With these four possible BPs identified for each claim in the data, the goal is to solve for each claimant's risk premium. To do this, I also need information on claimant preferences, other sources of consumption while unemployed (A), the probability of remaining unemployed in each period (S_t), and consumption while employed. I make the following assumptions and simplifications. First, I follow related work and assume that claimants have constant relative risk aversion (CRRA) preferences with coefficients of relative risk aversion equal to 3 (Luttmer and Samwick, 2018; Caldwell et al., 2020). Second, by defining the risk premium as a portion of consumption while the claimant is receiving UI benefits (and not, e.g., a proportion of all period consumption) I have implicitly removed the role S_t and of unemployment duration in general—I return to the implications of this assumption below. Third, I rely on estimates of the change in consumption at UI benefit exhaustion from Ganong and Noel (2019) and Rothstein and Valletta (2017) to back out an estimate of the proportion of consumption while unemployed financed by non-UI sources.[19] Finally, I directly measure benefit eligibility in each of the four potential BPs using the claimant's earning history.

I now am able to plug in each of these values to equation 2 and solve for rp for each claim in the data. To simplify interpretation, I will scale each risk premium by the claimant's expected benefit level $\mathbb{E}[b]$. This implies that the risk premia I report are interpretable as the percent of expected UI benefits that a claimant would trade in order to remove benefit risk.

Figure 15 displays two separate cumulative distributions of risk premia in my sample calculated as described above. In the first, I use the empirical distribution of the number of weeks between job loss and claim-filing among claimants with $\Delta RR = 0$ *for all claimants* (i.e., to calculate risk premia without strategic claim-timing responses). In the second, I allow the claim-timing distribution to vary with ΔRR as described above (risk premia *with* strategic claim-timing responses). Results suggest that the private welfare cost of benefit risk is moderate but meaningful. Without strategic claim-timing responses, the average claimant in my sample would trade 6.4% of their expected UI benefits to eliminate their exposure to benefit risk. With strategic claim-timing responses this number falls to 4%. Both distributions have long right-tails. Unsurprisingly, many claimants

[19]Specifically, I assume that the change in consumption at benefit exhaustion is equal to the level of UI benefits. With this assumption, results from Ganong and Noel (2019) suggest that non-UI sources of consumption are roughly 5.7 times UI benefit levels.

(nearly 60%) have risk premia equal to zero—this is not surprising given that many claimants have no benefit risk exposure, either because their earnings are not volatile or their earnings are always high enough to be eligible for the maximum WBA. On the other hand, there is a relatively small but important group of claimants with *very* large risk premia—the 90th percentile of the risk premium distribution is 16.4% without strategic claim-timing and 8% after allowing for strategic claim-timing. These results make clear that benefit risk exposure has important normative implications for UI claimants, and that, while these welfare costs are reduced by claim-timing responses, they remain important after accounting for such responses.

Several important caveats are worth mentioning. First, as mentioned above this model is only superficially dynamic. This can be reframed either as an assumption that all claimants are exhausting benefits, or as scaling the risk premia by the amount of UI benefits each claimant expects to receive. Second, I have implicitly assumed that strategic claim-timing adjustments are costless in these calculations. This is very unlikely to be true, and implies that these risk premia estimates are likely *underestimates* for any claimants who take advantage of the option to strategically delay their claims. Third, claimants are assumed to be identical in all dimensions except for ΔRR. This is important since substantial heterogeneity is likely to exist in other dimensions (such as non-UI sources of consumption during unemployment, or unemployment duration), and this heterogeneity is likely to be strongly correlated with exposure to benefit risk (e.g., the broadly more disadvantaged group exposed to benefit risk is likely to have less access non-UI sources of consumption during unemployment and longer unemployment spells, both implying larger welfare costs from benefit risk).

9 Conclusions

I demonstrate that base periods, a parameter previously ignored by the literature on social insurance programs, can have substantial implications for program claimants. Base periods define the pre-claim time period from which earnings are measured in order to calculate benefit eligibility. Using data from California's UI program, a commonly used base period structure is shown to expose many claimants to a previously unexamined form of risk, which I call "benefit risk." Benefit generosity varies, sometimes dramatically, for such claimants based on when their job loss occurs. Claimants exposed to benefit risk are broadly less-advantaged than those who are not. I show theoretically that benefit risk reduces the value of UI and empirically that claimants engage in strategic claim-timing behavior in order to partially reduce the negative effects of this risk.

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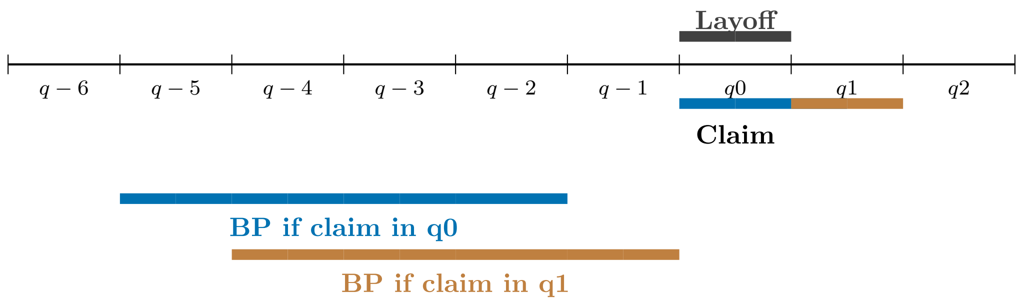
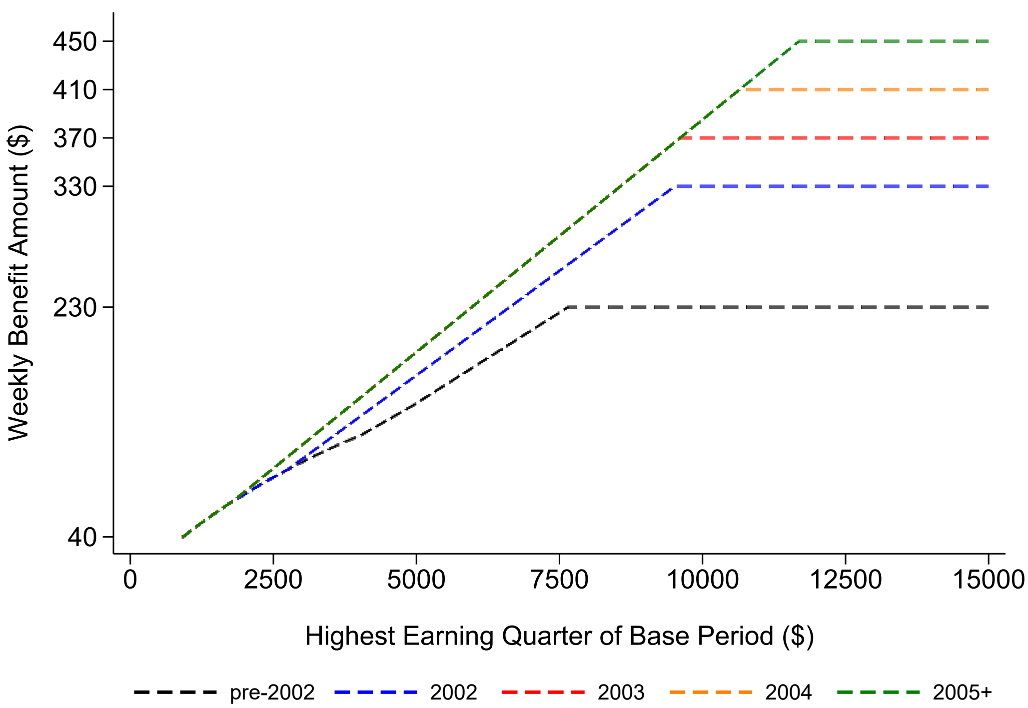
10 Figures and Tables

Figure 1: Base Periods by Quarter of Claim



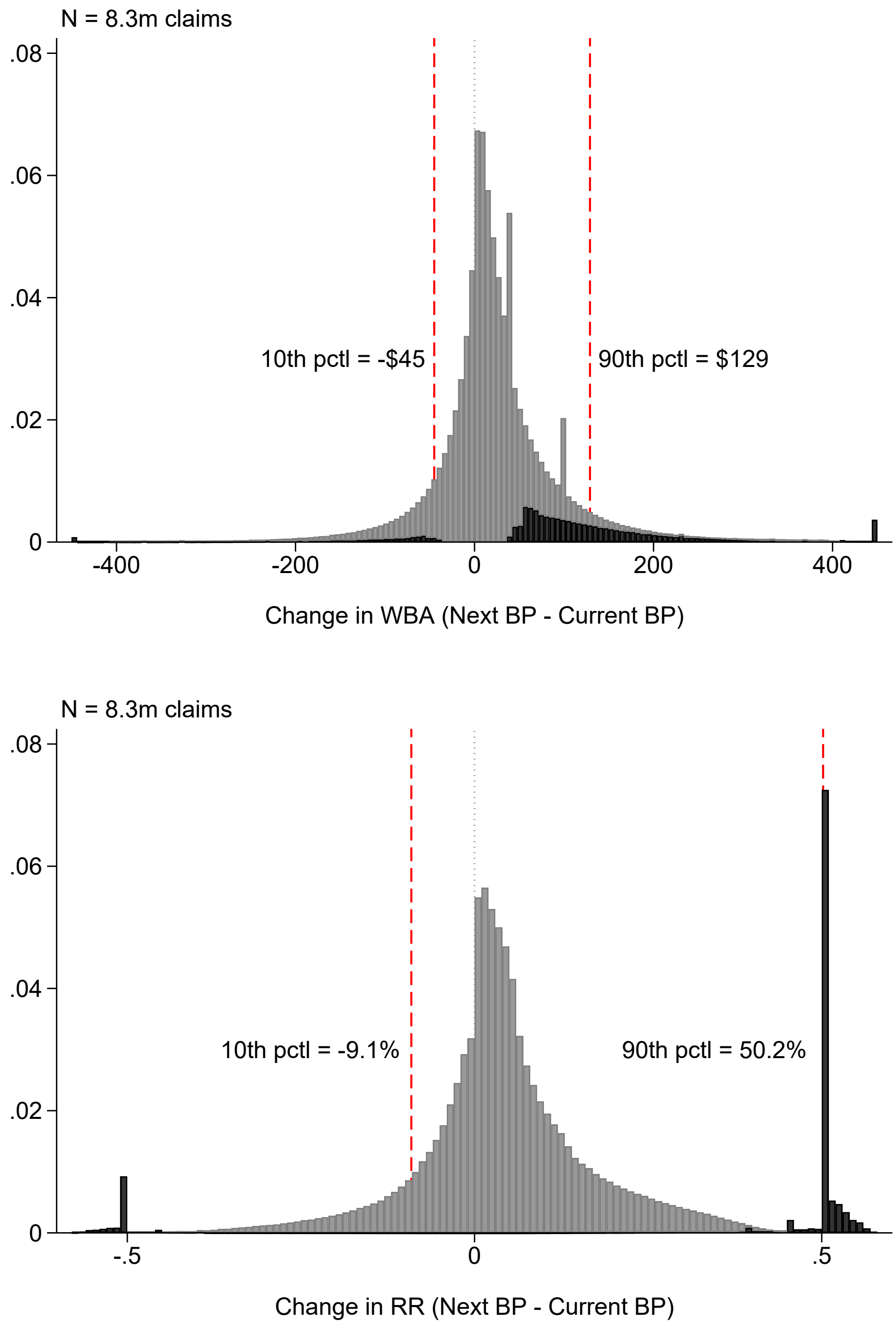
Notes: WBAs and PBDs are calculated as functions of earnings in the base period (BP). Base periods are continuous periods of four completed calendar quarters, determined based on the claim date as shown.

Figure 2: WBA Schedule in California



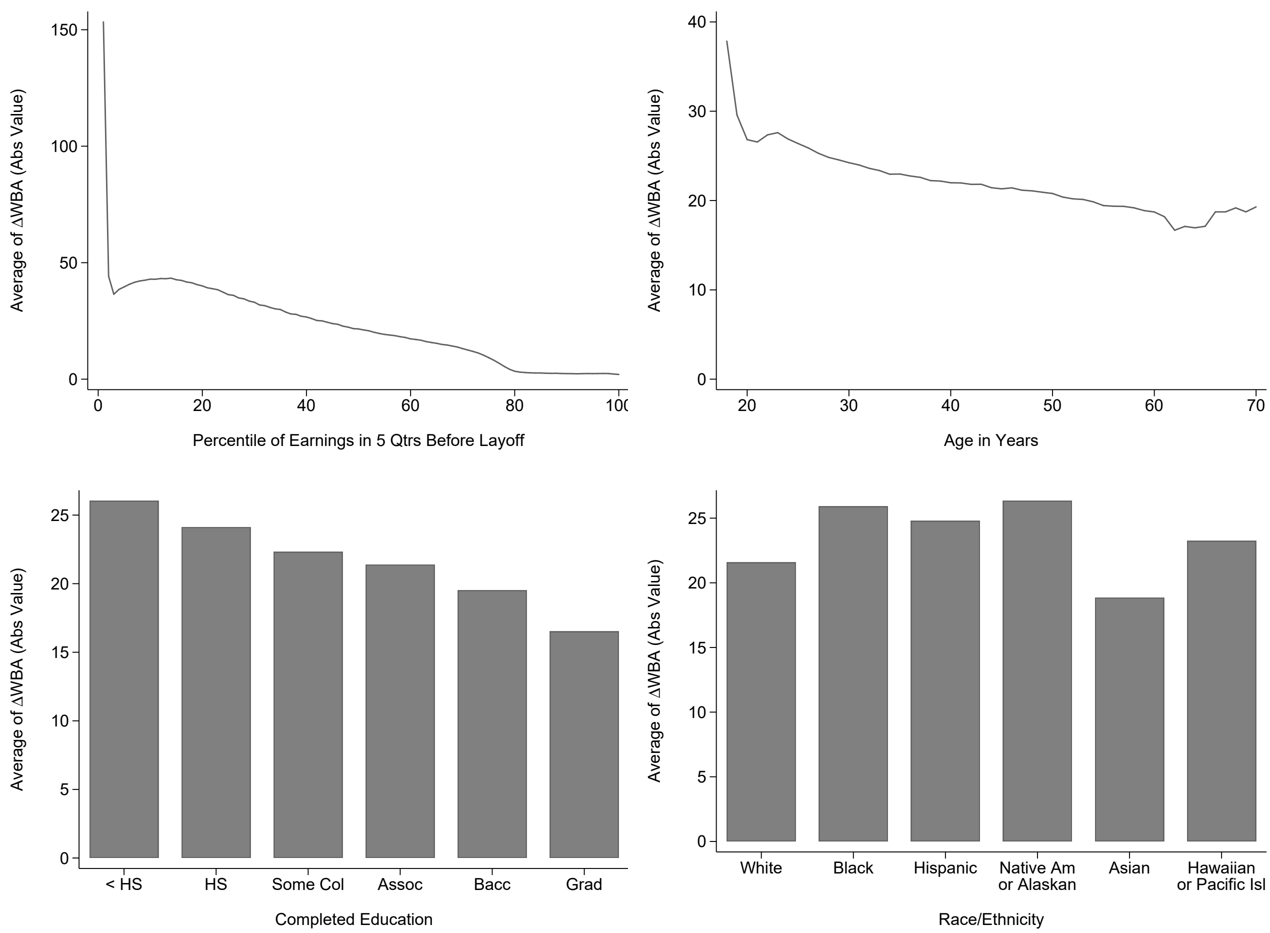
Notes: Beginning with the 2002 schedule, each WBA schedule is effective for new claims made on or after January 1st of the relevant year.

Figure 3: Distribution of WBA Change With New Base Period



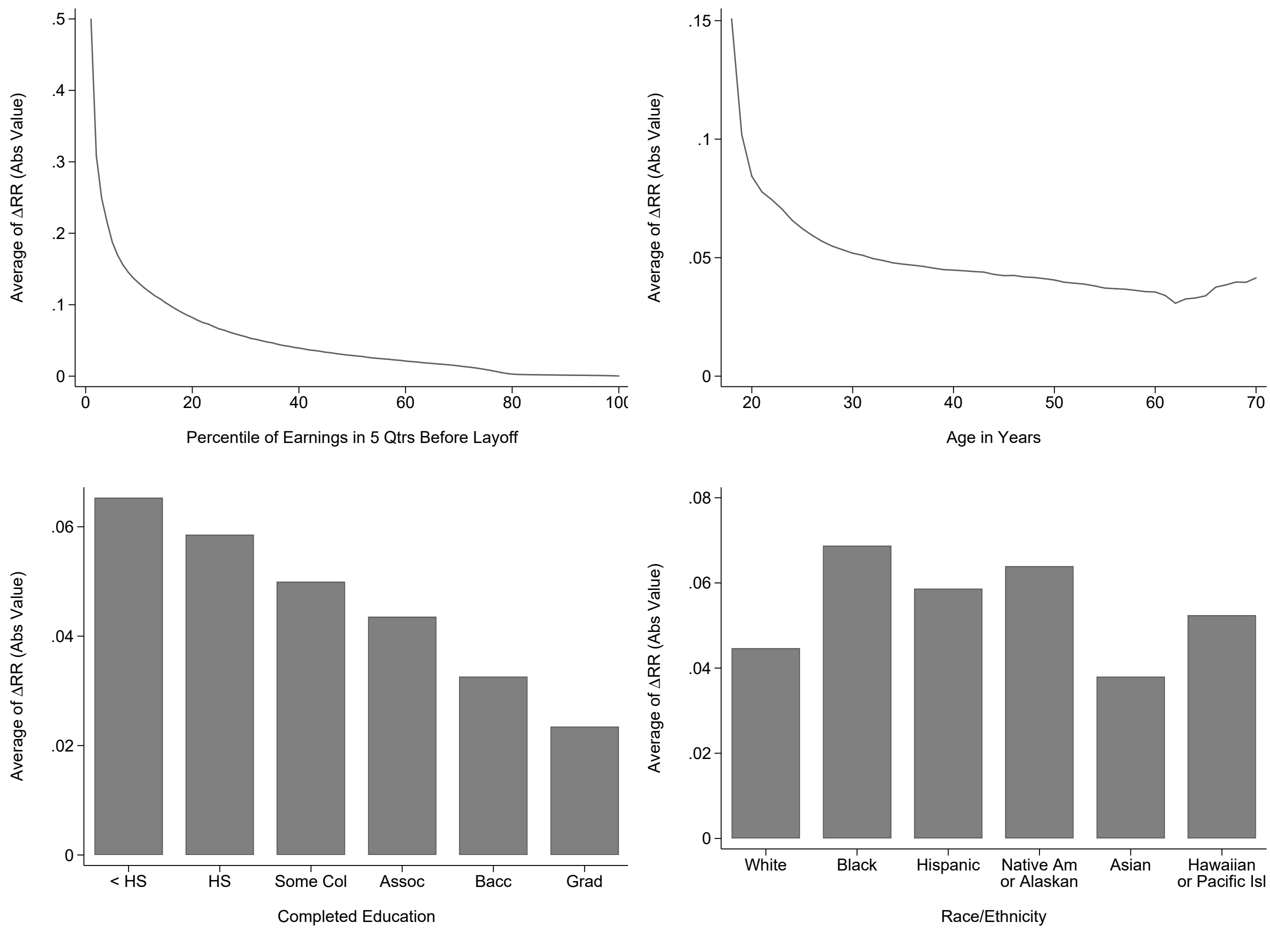
Notes: Histograms of $\Delta WBA = WBA_2 - WBA_1$ and $\Delta RR = RR_2 - RR_1$. Where subscripts denote the BP if the claim is filed in the same quarter as the layoff (1) and the quarter after the layoff (2). Analysis sample limited to claimants with ΔWBA (or RR) $\neq 0$. In each bin, claimants that lose eligibility entirely in one of the two BPs are shaded black while claimants who are eligible in both BPs, but for different benefit amounts, are shaded gray (i.e., the histograms are "stacked" and not overlaid).

Figure 4: Heterogeneous Exposure to Benefit Risk



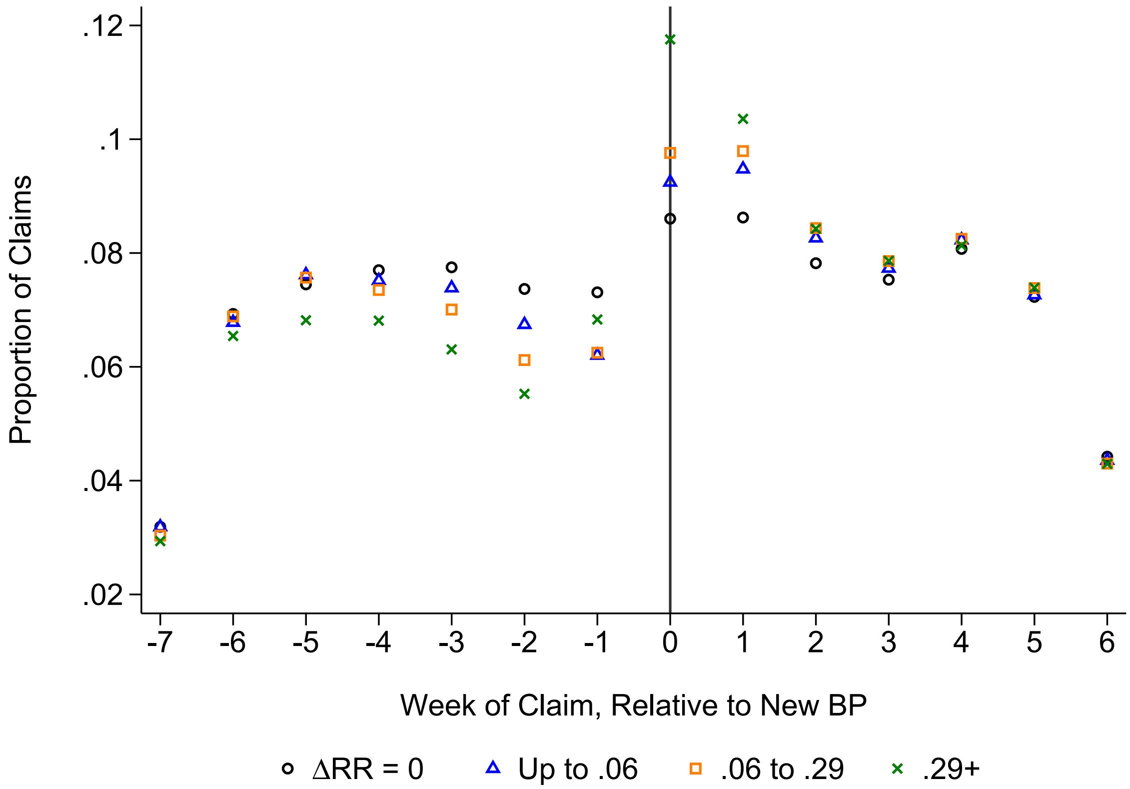
Notes: Each panel displays the mean value of $abs(\Delta WBA)$ in bins defined by the x-axis in the full sample of 21.7 million claims. Completed education, ethnicity, and date of birth are self-reported by the claimant to EDD when the claim is filed. Age is calculated as of the date of the layoff.

Figure 5: Heterogeneous Exposure to Benefit Risk



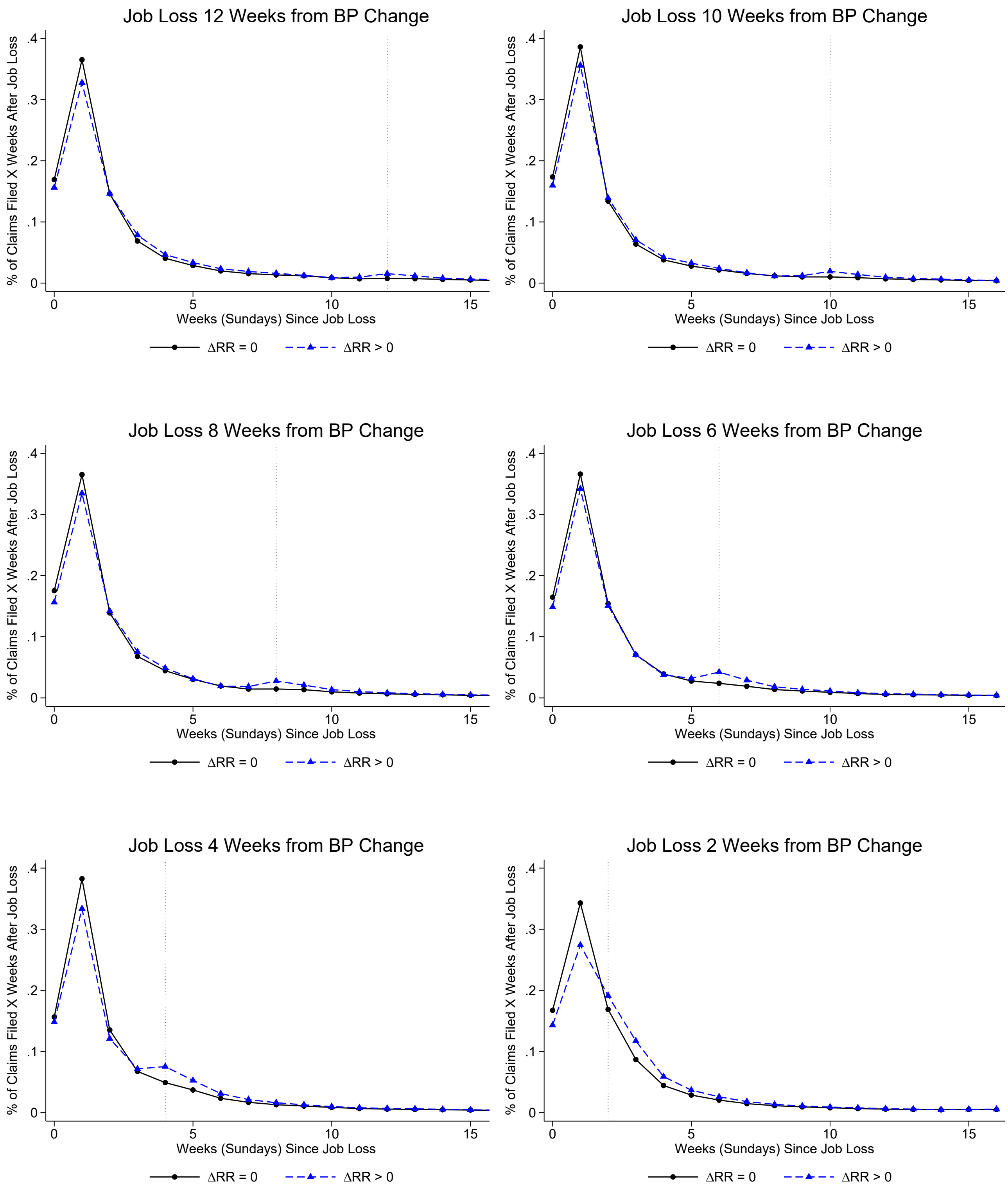
Notes: Each panel displays the mean value of $abs(\Delta RR)$ in bins defined by the x-axis in the full sample of 21.7 million claims. Completed education, ethnicity, and date of birth are self-reported by the claimant to EDD when the claim is filed. Age is calculated as of the date of the layoff.

Figure 6: Bunching at preferred claim dates



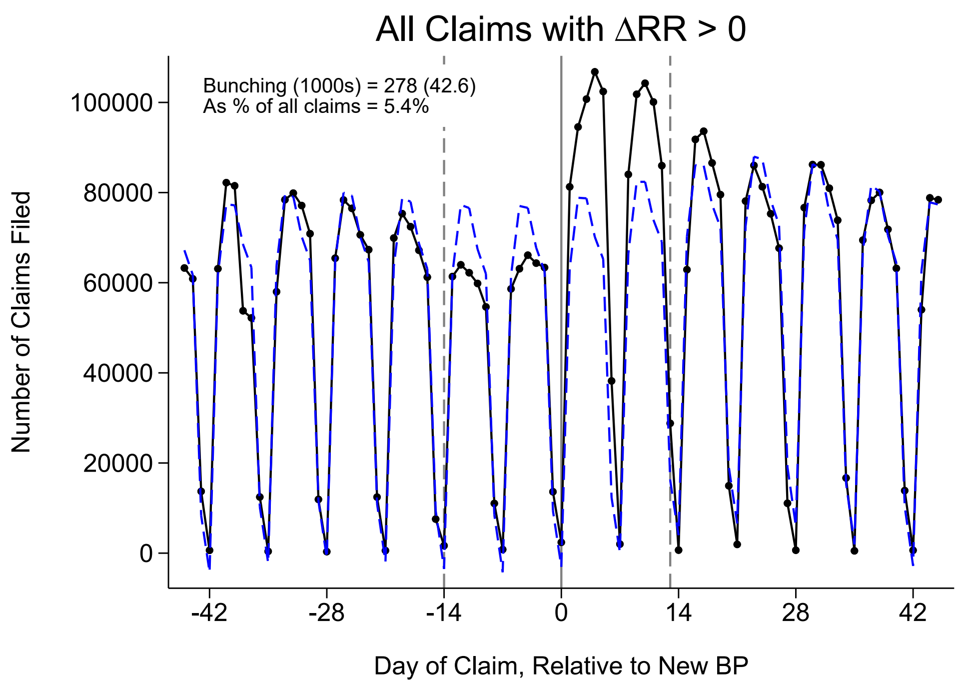
Notes: Bins are claim dates, relative to the closest BP change. For example, week -1 is the week before the BP changes (week ending on the Saturday before the first Sunday of a quarter).

Figure 7: Bunching at preferred claim dates, by layoff date relative to BP change

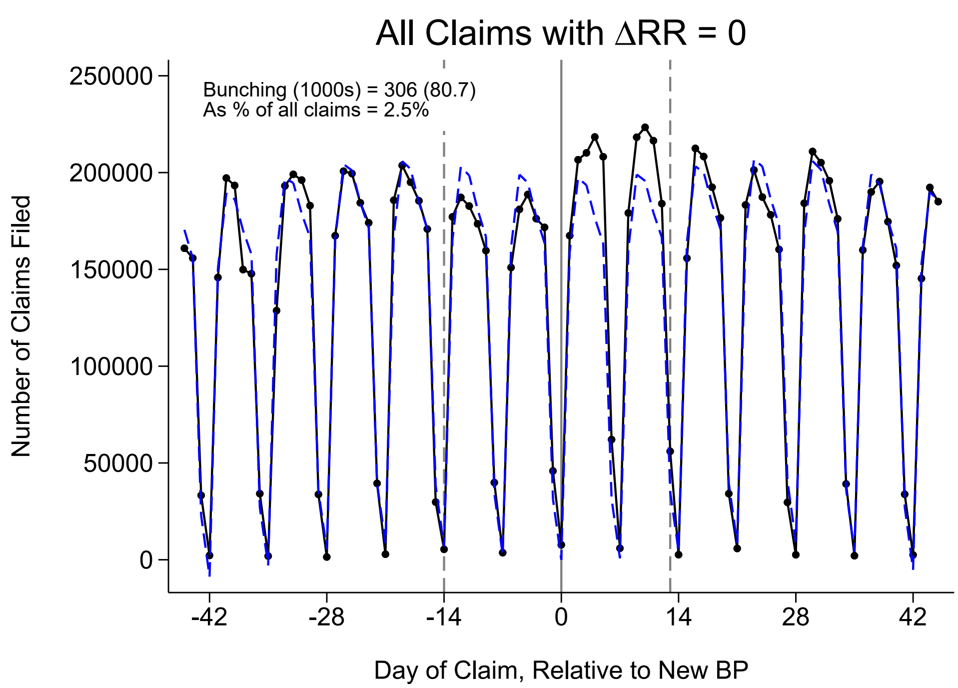


Notes: Each panel shows distributions of the number of weeks between layoff and claim-filing with (blue) and without (black) incentives to delay their claim until the BP change.

Figure 8: Bunching Estimates



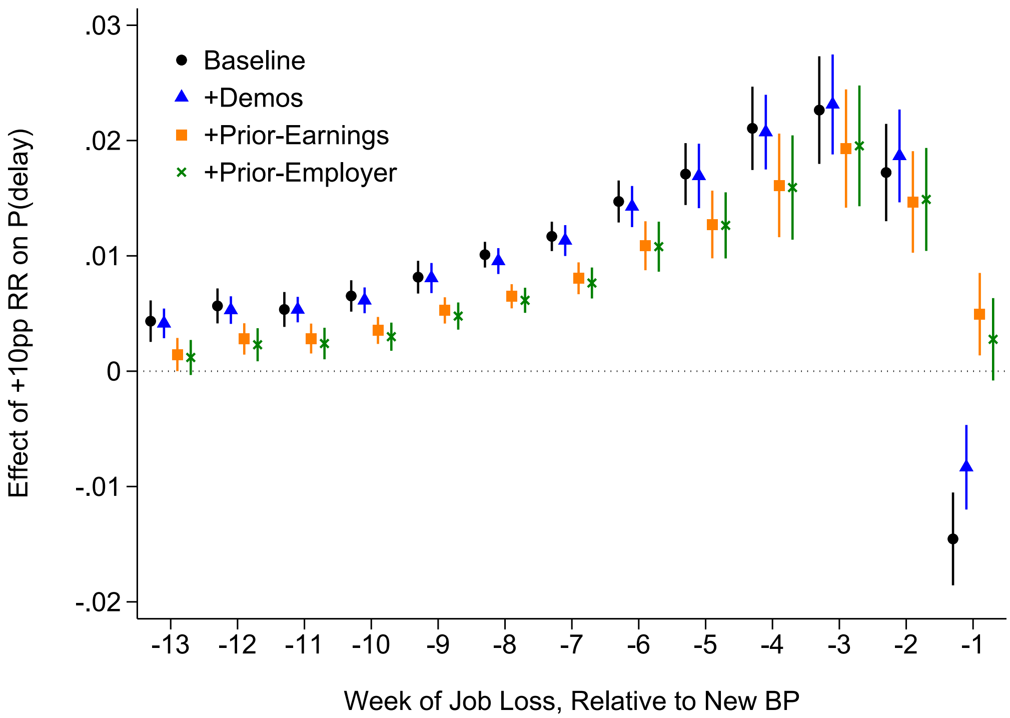
(a)



(b)

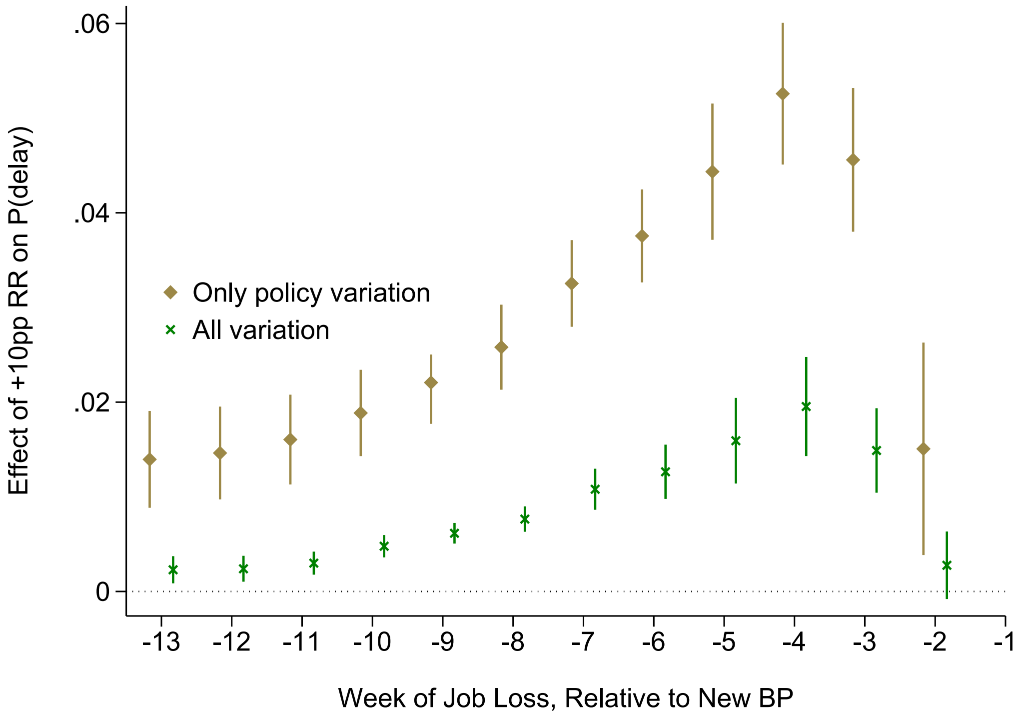
Notes: Panels show distributions of claim dates, centered at the first day of the "new" BP (first Sunday of the quarter after the claimant's last worked date). Black solid lines represent actual distributions, blue dashed lines represent counterfactual distributions which are estimated as described in Section 5.2. Vertical dashed lines represent the excluded region used to estimate the counterfactual distribution. Estimates of the number (with bootstrapped standard errors) and percent of claims that are delayed are shown in the top left of the figure.

Figure 9: Effects of ΔRR on P(claim filed in first week of next qtr)



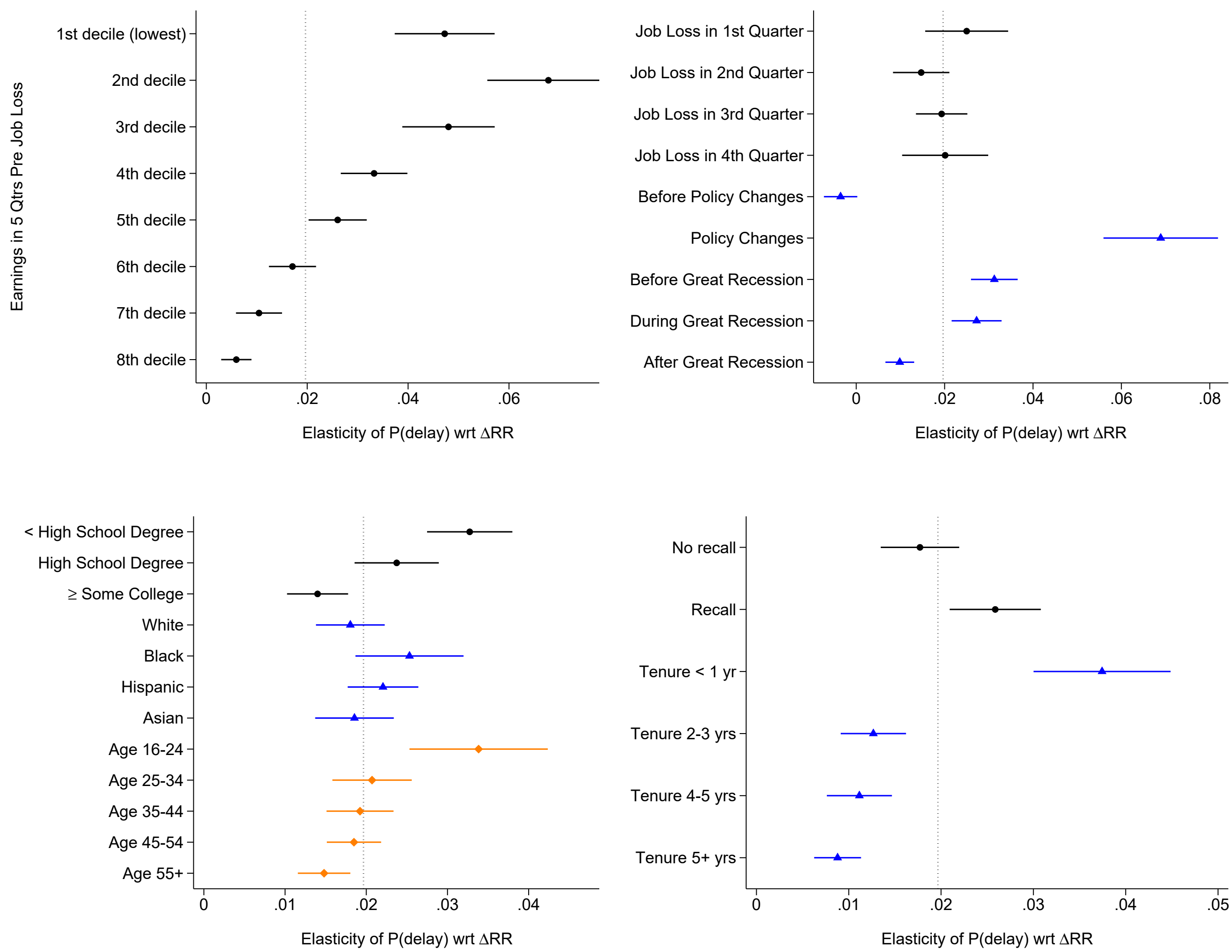
Notes: Estimates of the marginal effect of an extra 10pp in ΔRR on the probability of filing a claim in the first week of the quarter after the layoff. (i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equation 5.) Colors denote separate models, estimated with sequentially more complete sets of controls. Each model is estimated in a randomly selected sample of 5 million claims via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes quarter and weekday of layoff FEs, week of layoff (relative to the BP change) dummies, and a control for ΔPBD. "Demos" includes completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code. "Earnings" includes average quarterly earnings totals in the 5 calendar quarters that span the two possible BPs and a measure of "effective" earnings volatility in the same period as described in Section 5.3 "Pre-separation Employer" refers to the separating employer and includes the reason for job loss, an indicator for whether a recall is expected, firm size, average quarterly earnings of coworkers during the layoff quarter, and sector (two-digit NAICS).

Figure 10: Effects of ΔRR on P(claim filed in first week of next qtr), generalized difference-in-difference vs. selection on observables



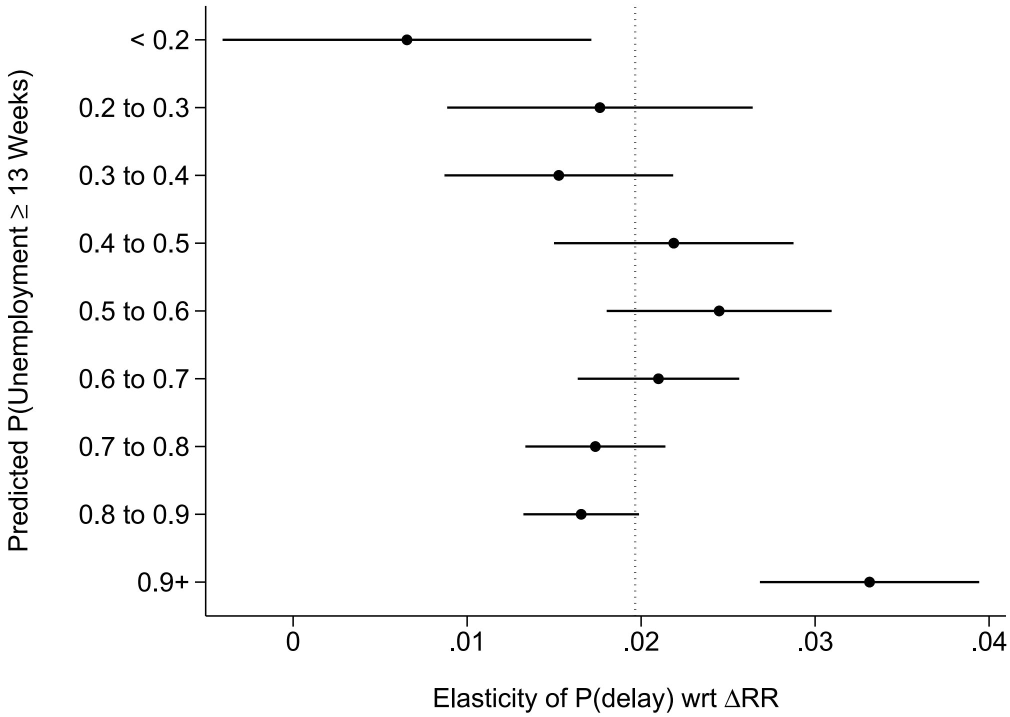
Notes: Estimates of the marginal effect of an extra 10pp in ΔRR on the probability of filing a claim in the first week of the quarter after the layoff. i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equations 5 (selection-on-observables) and 6 (difference-in-differences). The difference-in-difference model is estimated in the subset of claims filed by workers laid off between 1/1/2000 and 12/31/2006. The selection-on-observables model is estimated in a randomly selected sample of 5 million claims from the full sample. Both models are estimated via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes the most complete set of controls.

Figure 11: Heterogeneity in the Effect of ΔRR on P(claim filed in first week of next qtr)



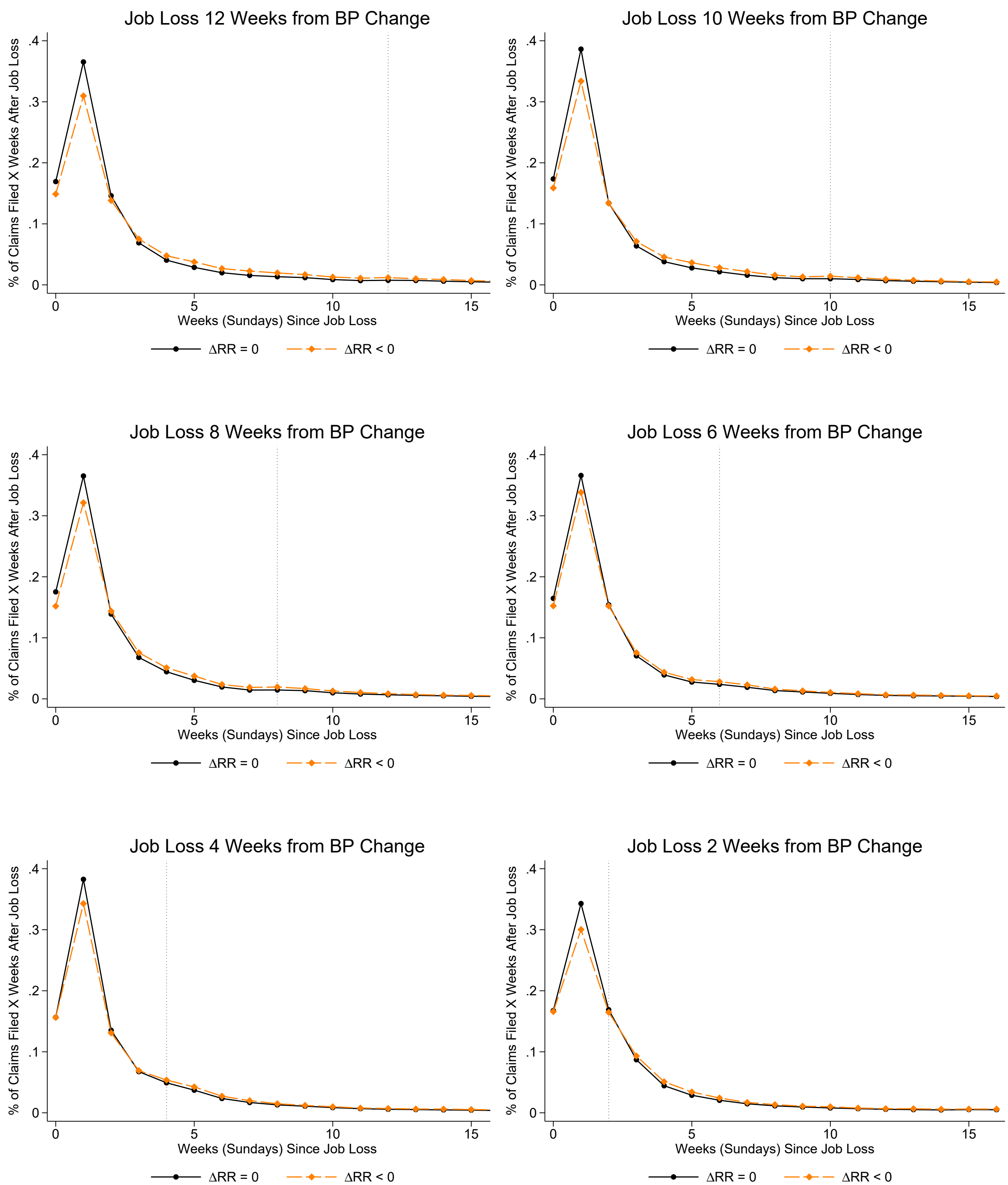
Notes: Estimates of the marginal effect of the elasticity of claim delay (here measured as the probability of filing a claim in the first week of the quarter after the layoff) with respect to Δ RR. The elasticity is calculated as $\hat{\beta}_2$ from equation 7 multiplied by by $\overline{\Delta RR}/\overline{delay}$ so that the reported values are interpretable as elasticities evaluated at the means. This ensures that variation in the effect of ΔRR on claim delay across subgroups is isolated from variation in the distribution of ΔRR and claim delay across subgroups. Standard errors are calculated via the delta method. Each row is coefficient from single model, limited to a subsample of claimants. All models are limited to claimants with $\Delta RR \geq 0$ and job losses occurring in the last 5 weeks of a BP. For reference, each panel includes a dotted vertical line which corresponds to the point estimate of the same elasticity among all claimants with $\Delta RR \geq 0$ and job losses occurring in the last 5 weeks of a BP.

Figure 12: Heterogeneity in the Effect of ΔRR on P(claim filed in first week of next qtr) by Predicted Unemployment Duration



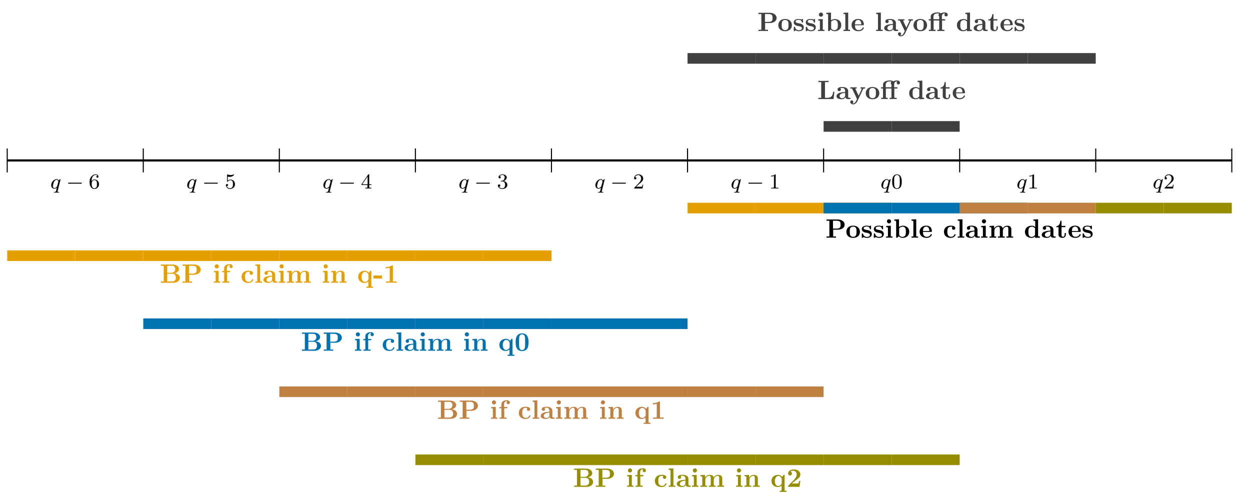
Notes: Estimates of the marginal effect of the elasticity of claim delay (here measured as the probability of filing a claim in the first week of the quarter after the layoff) with respect to Δ RR. The elasticity is calculated as $\hat{\beta}_2$ from equation 7 multiplied by by $\overline{\Delta RR}/\overline{delay}$ so that the reported values are interpretable as elasticities evaluated at the means. This ensures that variation in the effect of ΔRR on claim delay across subgroups is isolated from variation in the distribution of ΔRR and claim delay across subgroups. Standard errors are calculated via the delta method. Each row is coefficient from single model, limited to a subsample of claimants defined by predicted unemployment duration (probability of a 13+ week spell), predicted as described in Section 6.2. All models are limited to claimants with $\Delta RR \geq 0$ and job losses occurring in the last 5 weeks of a BP. For reference, each panel includes a dotted vertical line which corresponds to the point estimate of the same elasticity among all claimants with $\Delta RR \geq 0$ and job losses occurring in the last 5 weeks of a BP.

Figure 13: Claimants do not "hurry up"



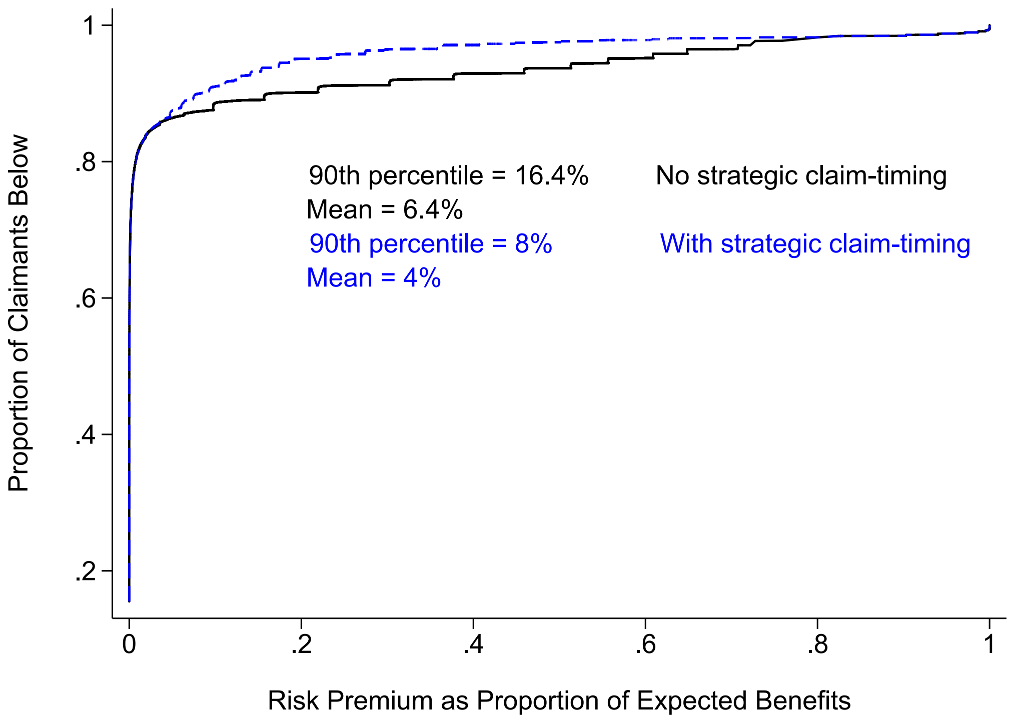
Notes: Each panel shows distributions of the number of weeks between layoff and claim-filing with (blue) and without (black) incentives to expedite their claim until the BP change.

Figure 14: Illustration of Potential Base Periods Considered in Risk Premia Calculations



Notes: Each claimant's risk premium is based on benefit eligibility under four possible base periods, and the probability that each of those base periods occurs. This figure shows an example of those four base periods using a timeline. The starting point is a claimant's actual layoff date ($q0$). I then allow for variability in week of layoff using an assumed distribution. (Binomial w/ 26 trials and prob success = 0.5 to ensure that the possible layoff weeks are +/- 1 qtr from the actual and roughly normally distributed). Next, to determine the probability that the claimant waits X weeks before claiming, I use the empirical distribution for full sample. Together these two distributions imply 4 potential BPs as shown. Finally, I calculate benefit eligibility in each BP, filling in missing earnings info in the layoff quarter by extrapolation (assume avg weekly earnings in qtr paid for remainder of qtr).

Figure 15: Cumulative Distribution of Risk Premia



Notes: Figure displays cumulative distributions of risk premia calculated as described in Section 8. Blue distribution (dashed line) is calculated with strategic claim-timing by allowing the distribution of time between job loss and claiming to vary with ΔRR. Black distribution (solid line) is calculated without strategic claim-timing.

Table 1: Some examples of earnings volatility driven variation in benefit risk

Claimant	$q-5$	$q-4$	$q-3$	$q-2$	$q-1$	q (layoff qtr)	WBA_1	WBA_2
1	\$10k	\$10k	\$10k	\$10k	\$10k	2005q3	\$385	\$385
2	\$10k	\$10k	\$10k	\$10k	\$15k	2005q3	\$385	\$450
3	\$10k	\$10k	\$10k	\$15k	\$10k	2005q3	\$450	\$450
4	\$15k	\$15k	\$15k	\$15k	\$20k	2005q3	\$450	\$450
5	\$15k	\$15k	\$15k	\$15k	\$15k	2004q3	\$410	\$410
6	\$15k	\$15k	\$15k	\$15k	\$15k	2004q4	\$410	\$450

Notes: Table displays hypothetical earnings histories chosen to demonstrate various sources of claim-timing incentives. For each claimant the table shows 5 quarterly earnings amounts for the 5 *completed* calendar quarters preceding the quarter of the layoff, WBA_1 (the WBA received by the claimant if the claim is filed in the layoff quarter), WBA_2 (WBA if the claim is filed in the quarter after the layoff).

Table 2: Claimant demographics

	All mean/sd	$\Delta WBA = 0$ mean/sd	$\Delta WBA > 0$ mean/sd	$\Delta WBA < 0$ mean/sd
Age	39.22	40.54	36.82	39.50
	(12.88)	(12.58)	(12.90)	(13.21)
Female	0.43	0.42	0.45	0.47
	(0.50)	(0.49)	(0.50)	(0.50)
White	0.37	0.41	0.33	0.31
	(0.48)	(0.49)	(0.47)	(0.46)
Black	0.08	0.07	0.09	0.08
	(0.27)	(0.26)	(0.29)	(0.27)
Hispanic	0.37	0.32	0.42	0.44
	(0.48)	(0.47)	(0.49)	(0.50)
Completed Educ:				
< HS diploma	0.21	0.16	0.26	0.28
	(0.41)	(0.37)	(0.44)	(0.45)
HS diploma	0.31	0.29	0.34	0.34
	(0.46)	(0.46)	(0.47)	(0.48)
Some college	0.26	0.27	0.25	0.24
	(0.44)	(0.44)	(0.43)	(0.43)
Associates	0.05	0.06	0.05	0.04
	(0.22)	(0.23)	(0.21)	(0.21)
Bachelors	0.12	0.16	0.09	0.07
	(0.33)	(0.36)	(0.28)	(0.26)
> Bachelors	0.04	0.06	0.02	0.02
	(0.20)	(0.23)	(0.14)	(0.13)
N	21,731,869	11,851,730	6,876,553	3,003,586

Notes: ΔWBA is defined as $WBA_2 - WBA_1$, where subscripts denote BP if claim is filed in quarter of layoff (1) or quarter after layoff (2). Completed education, ethnicity, and date of birth are self-reported by the claimant to EDD when the claim is filed. Age is calculated as of the date of the layoff.

Table 3: Claimant earnings, benefits, and claim timing

	All mean/sd	$\Delta WBA = 0$ mean/sd	$\Delta WBA > 0$ mean/sd	$\Delta WBA < 0$ mean/sd
In 5 completed qtrs pre-layoff:				
Avg. qtrly earnings	7970.55	10924.67	4505.83	4246.40
	(53147.95)	(71426.92)	(9899.07)	(2556.12)
SD qtrly earnings	2848.14	3298.99	2353.94	2200.65
	(117010.54)	(157711.35)	(19947.72)	(2188.79)
If claim filed in qtr of layoff:				
Max benefit amount (MBA)	6962.15	8375.37	4851.10	6218.95
	(3449.54)	(3218.41)	(2787.91)	(2965.25)
Wkly benefit amount (WBA)	276.60	322.93	207.23	252.56
	(128.01)	(122.83)	(106.49)	(114.02)
Benefit duration (PBD)	24.71	25.86	22.82	24.50
	(3.27)	(1.22)	(4.62)	(3.01)
Prior job:				
Quit	0.04	0.04	0.04	0.03
	(0.19)	(0.19)	(0.18)	(0.18)
Fired	0.11	0.12	0.11	0.09
	(0.32)	(0.32)	(0.31)	(0.29)
Prior employer:				
# Employees	3559.63	3645.82	3478.08	3398.31
	(11110.08)	(11417.61)	(10713.61)	(10752.84)
# Establishments	48.40	51.28	44.91	44.91
	(225.89)	(237.09)	(212.90)	(207.88)
Avg. Quarterly Pay	12104.72	14299.58	9660.15	8888.87
	(23630.91)	(27685.83)	(18671.11)	(12369.56)
Claim-timing:				
Claimed in next qtr	0.24	0.23	0.26	0.25
	(0.43)	(0.42)	(0.44)	(0.43)
Claimed in 1st wk of next qtr	0.07	0.07	0.07	0.06
	(0.25)	(0.25)	(0.26)	(0.24)
Days btwn layoff and claim	29.24	27.57	30.74	32.40
	(58.51)	(58.68)	(57.50)	(59.89)
N	21,731,869	11,851,730	6,876,553	3,003,586

Notes: ΔWBA is defined as $WBA_2 - WBA_1$, where subscripts denote BP if claim is filed in quarter of layoff (1) or quarter after layoff (2).

Table 4: Claimant prior job industry by change in MBA

	All	$\Delta WBA = 0$	$\Delta WBA > 0$	$\Delta WBA < 0$
	mean	mean	mean	mean
Ag, Forestry, Fishing, Hunting	0.07	0.03	0.11	0.13
Mining, Quarrying, Oil/Gas Extr	0.00	0.00	0.00	0.00
Utilities	0.00	0.00	0.00	0.00
Construction	0.11	0.13	0.10	0.09
Manufacturing	0.11	0.12	0.09	0.09
Wholesale Trade	0.04	0.05	0.04	0.03
Retail Trade	0.10	0.10	0.11	0.11
Transportation, Warehousing	0.03	0.03	0.03	0.03
Information	0.05	0.06	0.03	0.02
Finance, Insurance	0.03	0.04	0.03	0.02
Real Estate, Rental/Leasing	0.02	0.02	0.02	0.01
Professional, Sci, Tech	0.07	0.09	0.05	0.05
Mgmt of Companies/Enterprises	0.01	0.01	0.01	0.01
Admin Support, Waste Mgmt, Remed	0.11	0.09	0.13	0.13
Educational Services	0.05	0.05	0.04	0.04
Health Care, Social Assistance	0.07	0.07	0.07	0.07
Arts, Entertainment, Recreation	0.02	0.02	0.02	0.02
Accomodation, Food Services	0.06	0.05	0.07	0.07
Other Services	0.04	0.03	0.04	0.04
Public Admin	0.01	0.01	0.01	0.01
N	20,298,973	11,122,151	6,550,684	2,626,138

Notes: ΔWBA is defined as $WBA_2 - WBA_1$, where subscripts denote BP if claim is filed in quarter of layoff (1) or quarter after layoff (2). Industry groups are equivalent to 2-digit NAICS codes of the separating employer.

Table 5: Quarter Change Option Take-up

	Offered mean/sd	Did not delay mean/sd	Delayed mean/sd
Female	0.47	0.47	0.47
	(0.50)	(0.50)	(0.50)
Age (on layoff date)	36.36	35.87	37.14
	(12.94)	(12.91)	(12.95)
White	0.32	0.32	0.32
	(0.47)	(0.47)	(0.47)
Completed Education:			
$<$ HS	0.22	0.22	0.23
	(0.42)	(0.41)	(0.42)
HS	0.33	0.32	0.33
	(0.47)	(0.47)	(0.47)
$>$ HS	0.41	0.41	0.40
	(0.49)	(0.49)	(0.49)
Tenure at separating employer:			
$\leq$ 1 yr	0.49	0.48	0.52
	(0.50)	(0.50)	(0.50)
2-3 yrs	0.24	0.25	0.23
	(0.43)	(0.43)	(0.42)
4-5 yrs	0.08	0.09	0.08
	(0.28)	(0.28)	(0.27)
$>$ 5 yrs	0.14	0.14	0.14
	(0.34)	(0.34)	(0.35)
WBA difference ($)	42.22	31.45	59.27
	(59.28)	(50.38)	(67.71)
RR difference	0.08	0.06	0.10
	(0.09)	(0.08)	(0.10)
N	398,546	244,265	154,281

Notes: Sample limited to claimants who file their claim in the last week of a BP, but would have seen their benefits increase (by any amount) if they had instead waited just one more week. These claimants are notified by the agency that their benefits would have been different if they had delayed their claim one more week, made aware of the exact change in benefits they would have been eligible for, and given the opportunity to revisit their decision (i.e. to delay their claim by one week after it had already been filed). The second column includes on those claimants who did not accept this option, the third column includes only those claimants who did not.

Appendices

A Additional Institutional Context

PBDs are determined by the following formula:

$$PBD = \begin{cases} 26 & \text{if } WBA \cdot 26 \leq BPW \cdot \frac{1}{2} \\ \frac{BPW \cdot \frac{1}{2}}{WBA} & \text{if } WBA \cdot 26 > BPW \cdot \frac{1}{2} \end{cases}$$

The PBD never exceeds 26 weeks, and the MBA never exceeds one half of the BPW. To further clarify the PBD formula, we can plug in the WBA formula *for the case where the* WBA *is less than the maximum.* This allows the PBD formula to be expressed as a function of only the HQW and the BPW:

$$PBD = \begin{cases} 26 & \text{if } 4 \cdot RR \leq \frac{BPW}{HQW} \\ \frac{BPW \cdot 13}{HQW \cdot 2 \cdot RR} & \text{if } 4 \cdot RR > \frac{BPW}{HQW} \end{cases}$$

Again, this appears complex, but it makes clear that the PBD is a function of the ratio $\frac{BPW}{HQW}$ and that (since this ratio is bounded below by 1) the PBD is bounded below by $\frac{13}{2 \cdot RR}$ (e.g. 13 weeks if the RR is 0.5). This formulation also makes clear that a PBD below 26 weeks is only assigned to claimants whose HQW accounts for too large a proportion of their total BP earnings.

B Generalized Difference-in-Differences and Event Study Specifications

Several factors could explain the differences between the results in Section 5.4.2 and Section 5.3. First, the sample is different—limited to the years around the policy changes in the diff-in-diff approach and spanning all 18 years of the full analysis sample in the selection on observables approach. Second, the differences may reflect remaining unobserved confounders that are biasing the estimates in Figure 9. Third, the discrepancies may reflect differences in the characteristics of the claimants who have large ΔWBA values. Regressions in Figure 9 rely on variation in ΔWBA driven by earnings volatility conditional on a series of controls. As shown in e.g. Table 2 and Table 3, claimants with large ΔWBA values due to earnings volatility tend to be broadly less advantaged than those with ΔWBA values close to zero. On the other hand, as shown in Table A1 (and explained further in the next subsection), many of the claimants with policy-driven variation in ΔWBA (which is the variation isolated by the diff-in-diff estimates in Figure 10) have little to no earnings volatility and are therefore likely a more advantaged group. These claimants may have an easier time responding to these incentives for a variety of reasons. Investigating this potential source of heterogeneity in the effects of ΔWBA on claim-timing decisions will be an important next step in this analysis.

The key assumption underlying specifications in Section 5.4.2 is that outcomes in differentially exposed earnings groups would have evolved in parallel in the absence of the policy change. To provide suggestive evidence I test for pre-trends using event study specifications estimated separately for each of the four policy changes:

$$delay_{gq} = \sum_{q \in Q} \beta^q 1\{qtr = q\} \cdot \Delta WBA_g^Q + \gamma_g + \psi_q + \epsilon_{gq} \tag{8}$$

Where Q denotes a set of quarters around the policy change of interest, and ΔWBA_g^Q is the "treatment" received by group g in the relevant policy change quarter. As an example consider the policy change which occurs in 2001q4. The set of quarters Q included in the regression are 2000q1-2002q3 (the data begins in 2000q1, a second policy change occurs in 2002q4), the omitted quarter in the set of interactions with ΔWBA_g^Q is 2001q3, the policy change quarter of interest is 2001q4, and ΔWBA_g^Q is defined as the value of ΔWBA for the claimant's earnings group g in 2001q4 (e.g., if $HQW_1 = HQW_2 = \$10,000$, $\Delta WBA_g^Q = \$100$ as per Table A1).

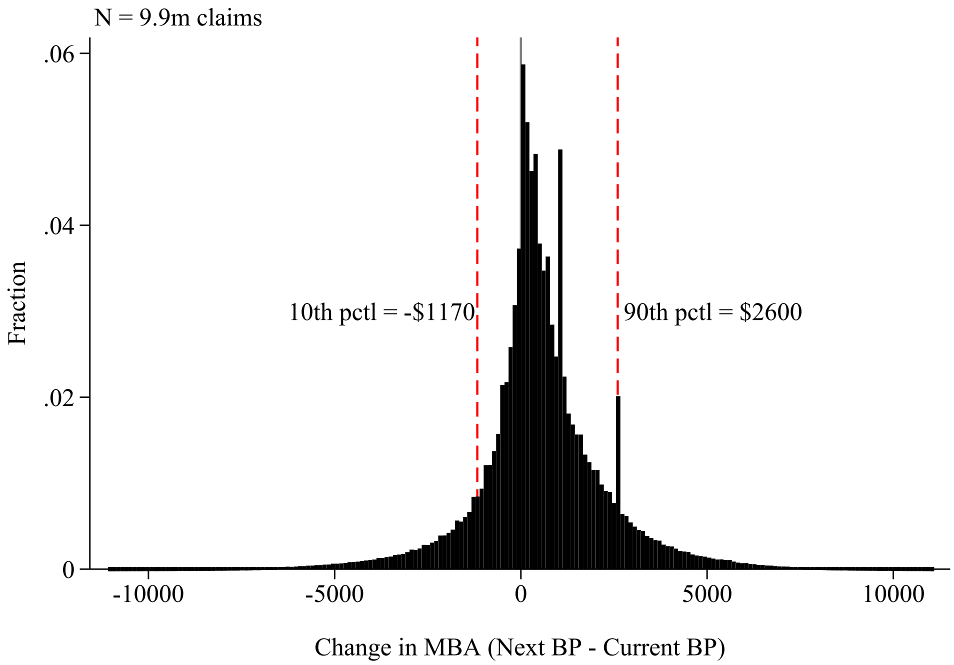
Several characteristics of this setting complicate these specifications. First, this is an event study setting with multiple treatments per treated unit. Second, the effects of the policy changes on claim-timing incentives are not permanent. Table A1 demonstrates that for some claimants the effect of the policy change persists after the policy change quarter but at a different level (Claimants 2 and 3 in the table). This occurs for claimants who would have claim-timing incentives without the policy change. In the quarter of the policy change these incentives are altered because waiting to claim implies a new WBA schedule. In the quarter after the policy change these incentives are altered again because the new WBA schedule is applied to both potential BPs. For other claimants (like Claimant 1 in the table) the policy change is effective only within the policy change quarter and then immediately "turns off" in the quarter after. This is because these claimants have earnings histories such that they do not typically have an incentive to delay or speed up claiming. The incentive only exists in the policy change quarter because waiting for the next BP leads to a change in the effective WBA schedule.

These two different types of claimants need to be handled differently by the event study specifications. For claimants 2 and 3, it would not make sense to expect to observe no pre-trends in the event studies for the latter policy changes. This is because the "pre" period for the 2nd-4th policy changes are also the "post" period for the preceding policy change. Another complication for claimants 2 and 3 is that it does not make sense to interact ΔWBA_g^Q with "post" policy change quarters since a different policy-driven ΔWBA is effective for these claimants in those quarters. In the case of claimant 1, the uniqueness of the setting is actually helpful. Since the policy change immediately turns off in the quarter following the policy change, the existence of multiple treatments per treated unit does not complicate testing for pre-trends. Instead, for these claimants I can provide supporting evidence for the parallel trends assumption not only by testing for pre-trends but also by testing post-trends. For these reasons I limit the event-study estimates to claimants like claimant 1, that is, claimants with no "relevant" earnings volatility in the five quarters that span the two BPs of interest.

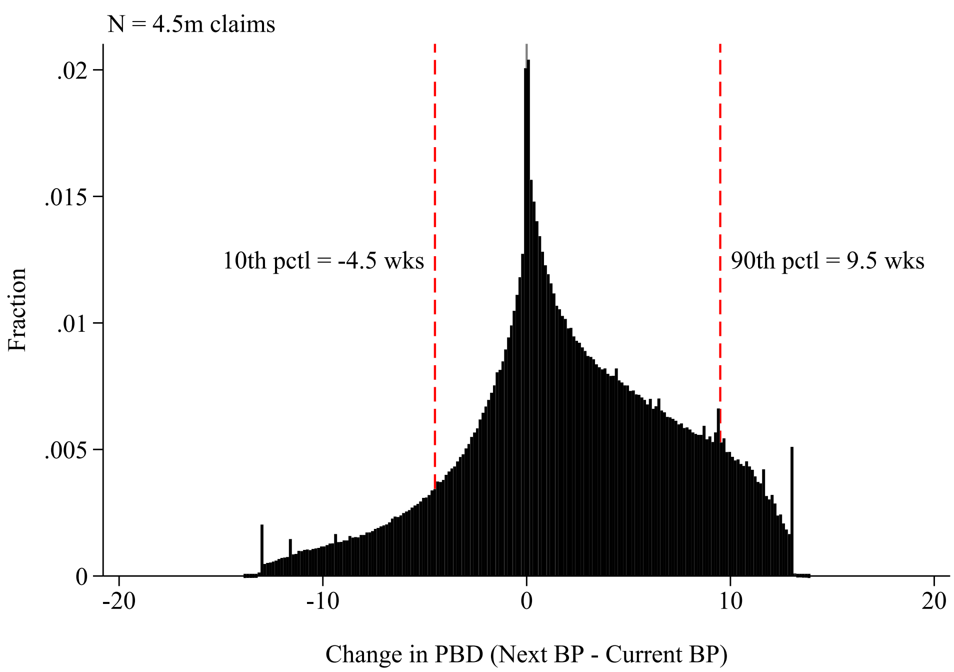
Results are shown in Figure A8 and broadly consistent with a conclusion that there are no pre-trends, with a few exceptions. First, there are several quarters in the first pre-period where earnings groups who will later be more affected by the policy change are more likely to delay their claim. Second, this differential delay is also apparent in several quarters after the 2004q1 policy change and one quarter after the last (2005q1) policy change. This is somewhat concerning for the assumptions underlying the generalized difference-in-differences specifications presented in the prior subsection, as it suggests the possibility that parallel trends assumption does not hold in this setting. However, in each specification the coefficient in the policy change quarter is dramatically larger than the others implying that this may not be a major concern. Future work will need to investigate this further, perhaps via placebo tests in non-policy change quarters or by testing the robustness of the basic event study specifications (e.g. to adding controls).

C Appendix Figures and Tables

Figure A1: Distribution of MBA and PBD Change With New Base Period



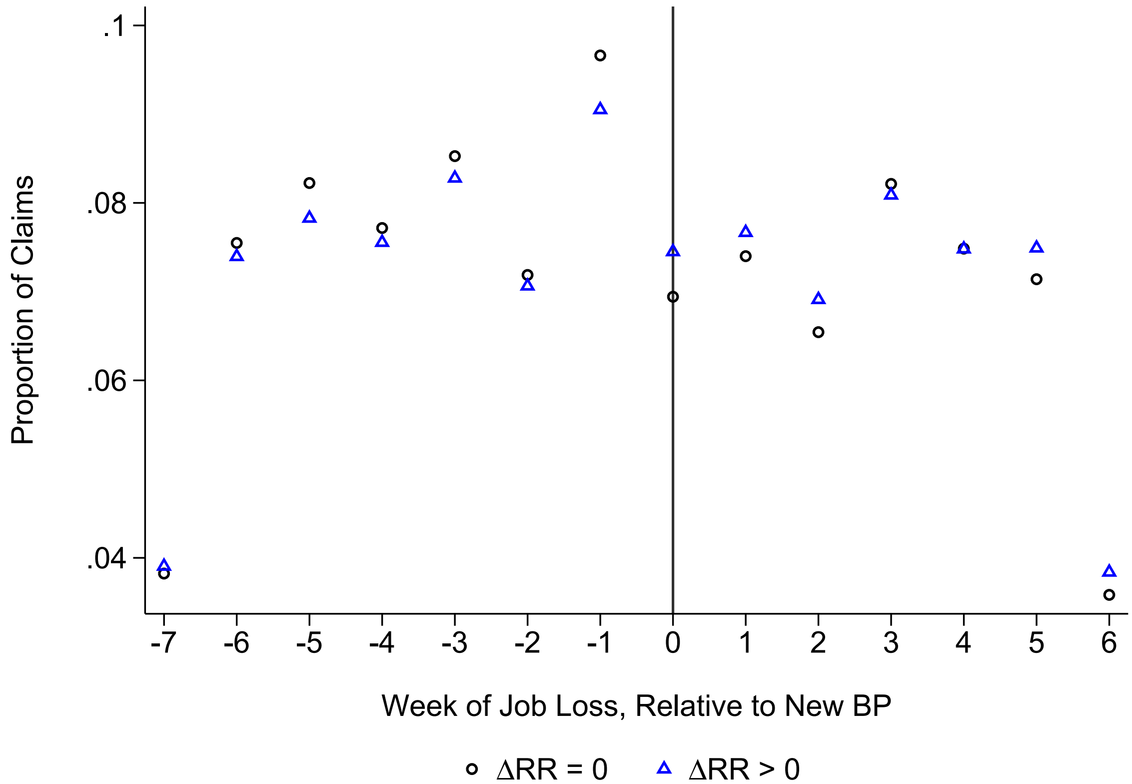
(a)



(b)

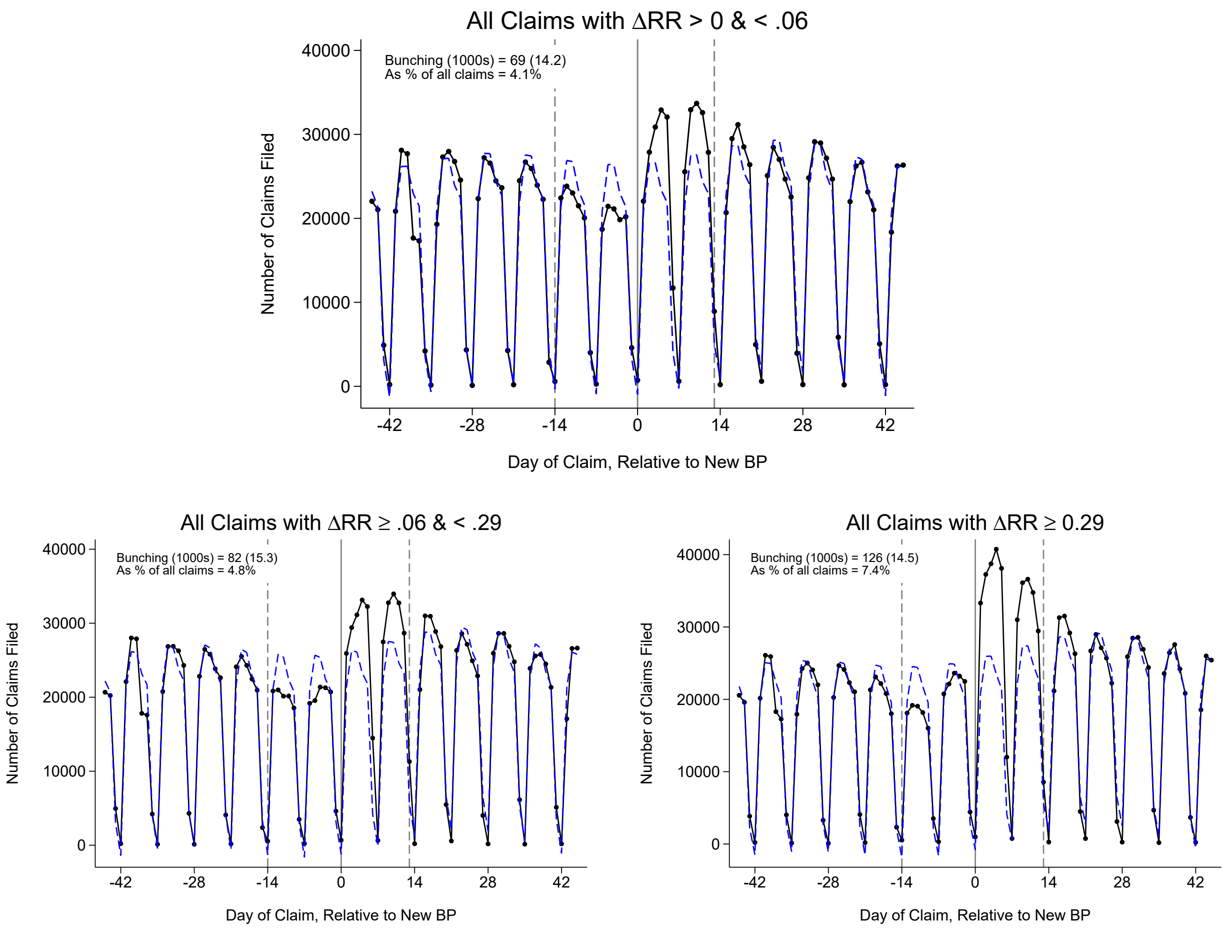
Notes: Histograms of $\Delta MBA = MBA_2 - MBA_1$ and $\Delta PBD = PBD_2 - PBD_1$. Where subscripts denote the BP if the claim is filed in the same quarter as the layoff (1) and the quarter after the layoff (2).

Figure A2: (non) Bunching in layoff date distributions



Notes: Bins are layoff dates, relative to the closest BP change. For example, week -1 is the week before the BP changes (week ending on the Saturday before the first Sunday of a quarter).

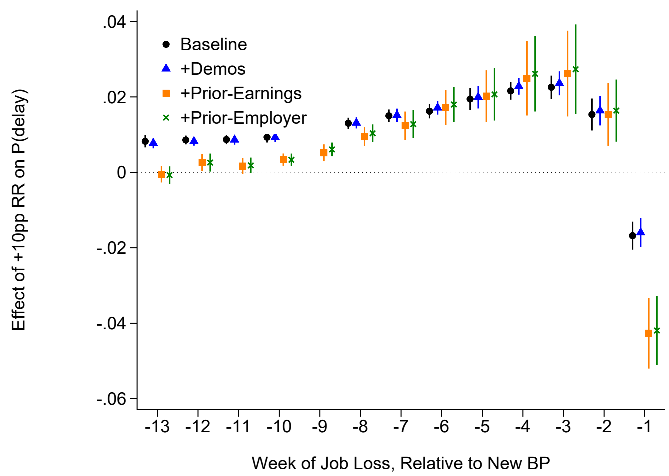
Figure A3: Amount of bunching increases with incentives to bunch



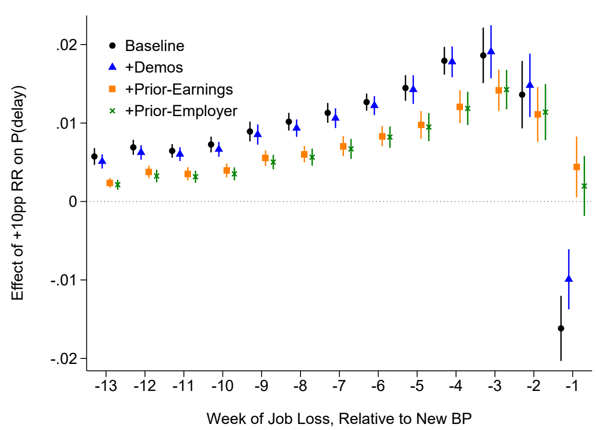
Notes: Panels show distributions of claim dates, centered at the first day of the "new" BP (first Sunday of the quarter after the claimant's last worked date). Black solid lines represent actual distributions, blue dashed lines represent counterfactual distributions which are estimated as described in Section 5.2. Vertical dashed lines represent the excluded region used to estimate the counterfactual distribution. Estimates of the number (with bootstrapped standard errors) and percent of claims that are delayed are shown in the top left of the figure.

Figure A4: Effects of ΔRR on P(claim filed in first week of next qtr): Alternate Samples

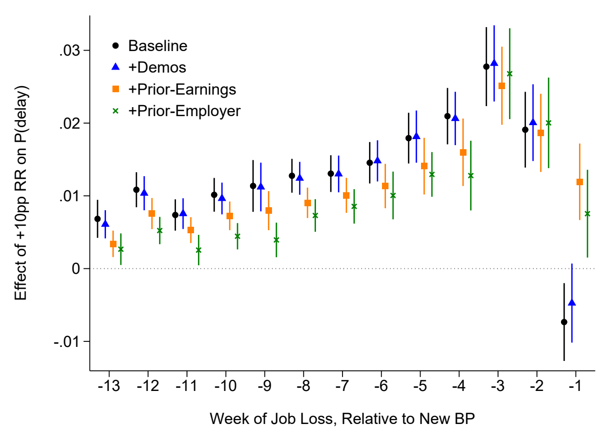
(a) ΔWBA $= 0$ Claims Excluded



(b) Post Policy Changes

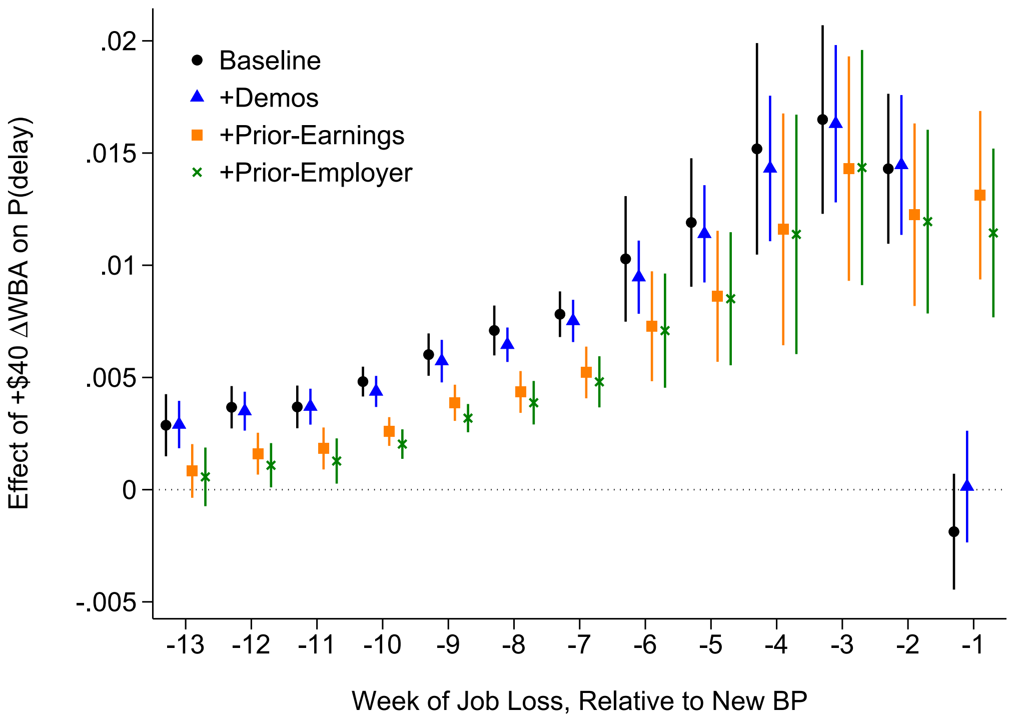


(c) Mass Layoff Sample



Notes: Estimates of the marginal effect of an extra 10pp in ΔRR on the probability of filing a claim in the first week of the quarter after the layoff. (i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equation 5.) Colors denote separate models, estimated with sequentially more complete sets of controls. Each model is estimated in a randomly selected sample of 5 million claims via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes quarter and weekday of layoff FEs, week of layoff (relative to the BP change) dummies, and a control for ΔPBD. "Demos" includes completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code. "Earnings" includes average quarterly earnings totals in the 5 calendar quarters that span the two possible BPs and a measure of "effective" earnings volatility in the same period as described in Section 5.3 "Pre-separation Employer" refers to the separating employer and includes the reason for job loss, an indicator for whether a recall is expected, firm size, average quarterly earnings of coworkers during the layoff quarter, and sector (two-digit NAICS).

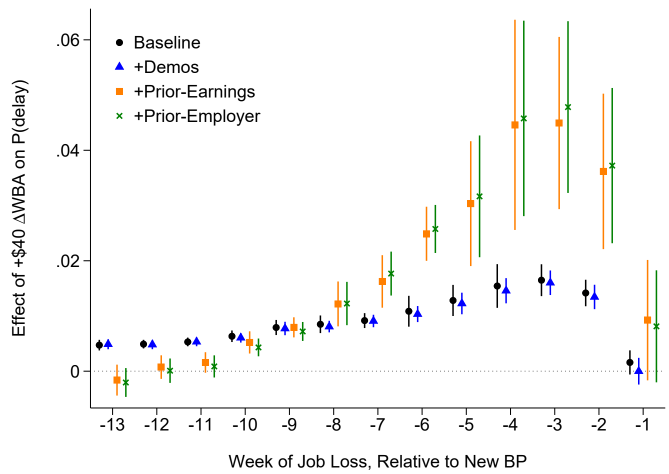
Figure A5: Effects of ΔWBA on P(claim filed in first week of next qtr)



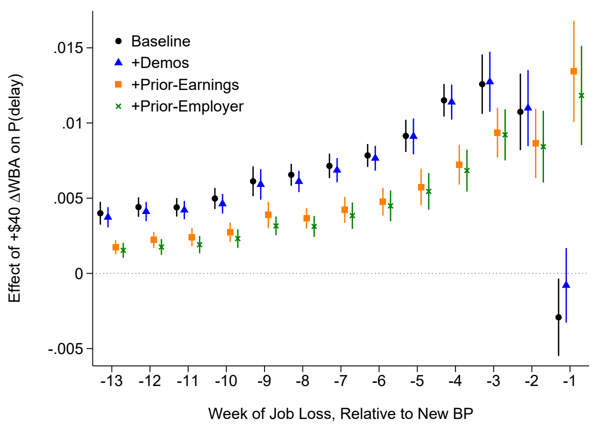
Notes: Estimates of the marginal effect of an extra \$40 in ΔWBA on the probability of filing a claim in the first week of the quarter after the layoff. (i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equation 5.) Colors denote separate models, estimated with sequentially more complete sets of controls. Each model is estimated in a randomly selected sample of 5 million claims via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes quarter and weekday of layoff FEs, week of layoff (relative to the BP change) dummies, and a control for ΔPBD. "Demos" includes completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code. "Earnings" includes average quarterly earnings totals in the 5 calendar quarters that span the two possible BPs and a measure of "effective" earnings volatility in the same period as described in Section 5.3 "Pre-separation Employer" refers to the separating employer and includes the reason for job loss, an indicator for whether a recall is expected, firm size, average quarterly earnings of coworkers during the layoff quarter, and sector (two-digit NAICS).

Figure A6: Effects of ΔWBA on P(claim filed in first week of next qtr): Alternate Samples

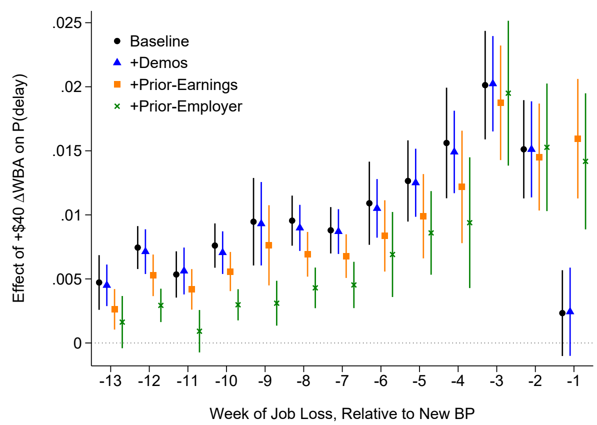
(a) ΔWBA = 0 Claims Excluded



(b) Post Policy Changes

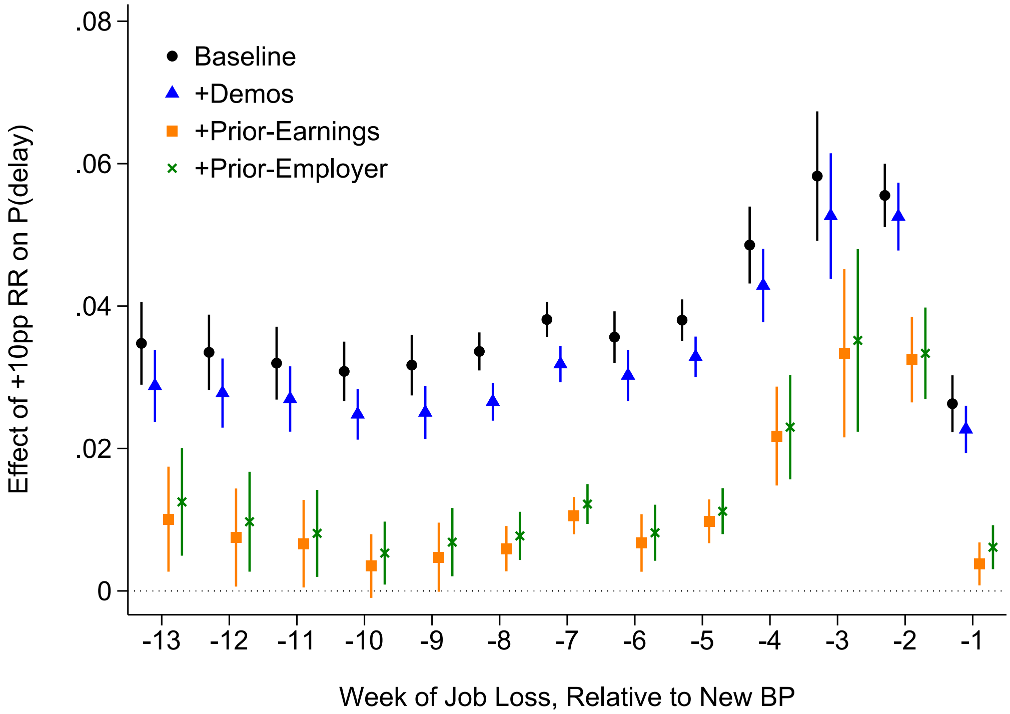


(c) Mass Layoff Sample



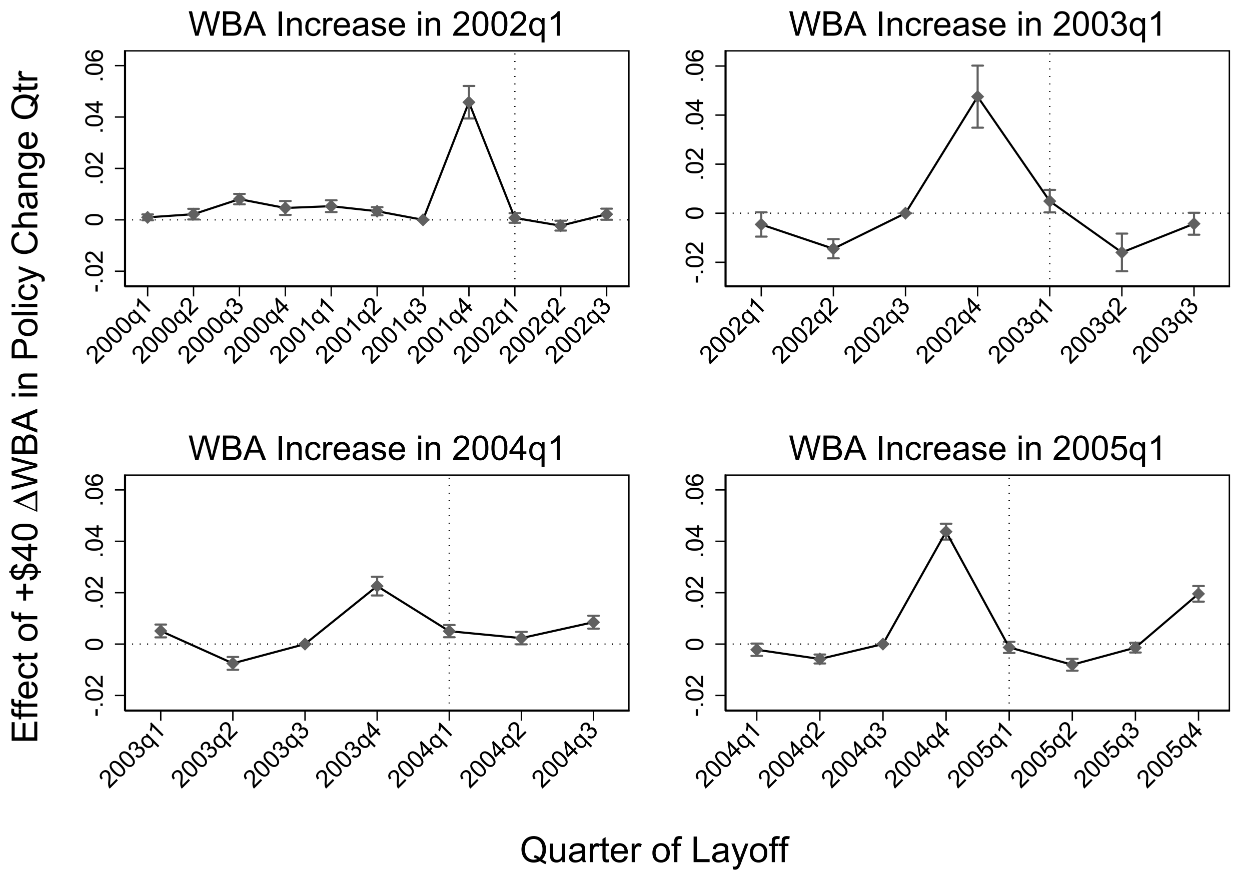
Notes: Estimates of the marginal effect of an extra \$40 in ΔWBA on the probability of filing a claim in the first week of the quarter after the layoff. (i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equation 5.) Colors denote separate models, estimated with sequentially more complete sets of controls. Each model is estimated in a randomly selected sample of 5 million claims via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes quarter and weekday of layoff FEs, week of layoff (relative to the BP change) dummies, and a control for ΔPBD. "Demos" includes completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code. "Earnings" includes average quarterly earnings totals in the 5 calendar quarters that span the two possible BPs and a measure of "effective" earnings volatility in the same period as described in Section 5.3 "Pre-separation Employer" refers to the separating employer and includes the reason for job loss, an indicator for whether a recall is expected, firm size, average quarterly earnings of coworkers during the layoff quarter, and sector (two-digit NAICS).

Figure A7: Effects of ΔWBA on P(claim filed 2+ weeks after layoff)



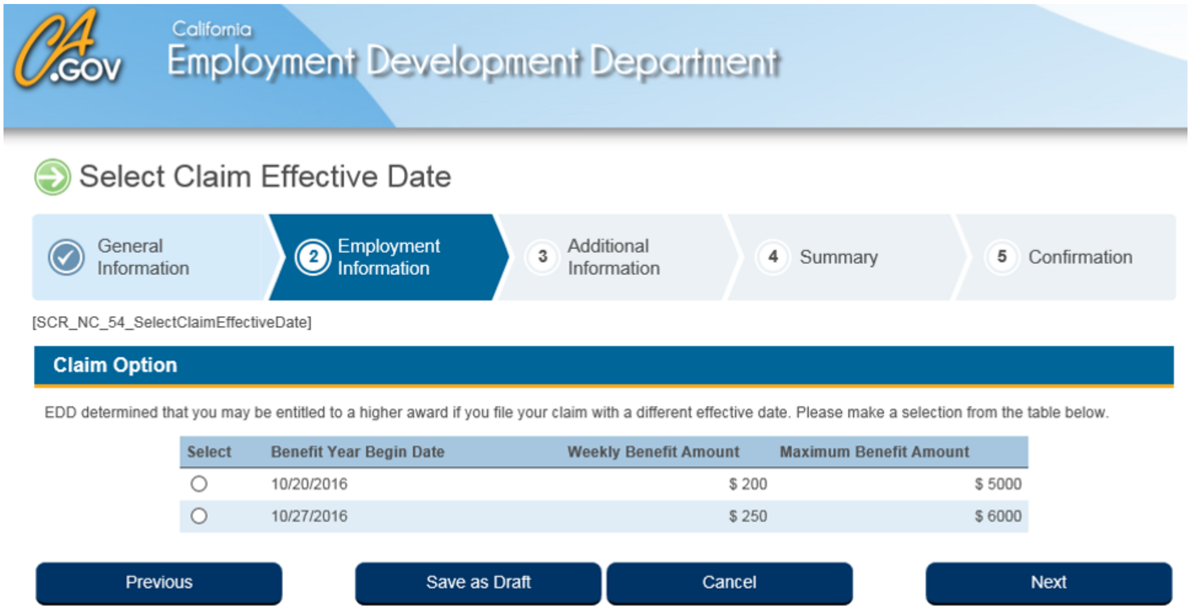
Notes: Estimates of the marginal effect of an extra \$40 in ΔWBA on the probability of filing a claim two or more weeks after the layoff. (i.e. $\hat{\beta}_2^{\tau} + \hat{\beta}_3$ from equation 5.) Colors denote separate models, estimated with sequentially more complete sets of controls. Each model is estimated in a randomly selected sample of 5 million claims via OLS with cluster-robust standard errors at the layoff-quarter level. Each model includes quarter and weekday of layoff FEs, week of layoff (relative to the BP change) dummies, and a control for ΔPBD. "Demos" includes completed education, gender, age, ethnicity, citizenship status, and 3-digit zip code. "Earnings" includes average quarterly earnings totals in the 5 calendar quarters that span the two possible BPs and a measure of "effective" earnings volatility in the same period as described in Section 5.3 "Pre-separation Employer" refers to the separating employer and includes the reason for job loss, an indicator for whether a recall is expected, firm size, average quarterly earnings of coworkers during the layoff quarter, and sector (two-digit NAICS).

Figure A8: Effects of ΔWBA on P(claim filed in first week of next qtr), event study specification



Notes: Figures plot $\hat{\beta}^q$ estimates from equation 8 around each of the four policy changes shown in Figure 2. Each panel represents a separate regression including all claims filed by claimants laid off in the quarters shown with $HQW_1 = HQW_2$ (i.e. claimants with no relevant earnings volatility). The "treatment" variable is defined as the average change within the earnings group in ΔWBA in the relevant policy change quarter. This treatment is interacted with indicators for layoff quarters in a regression with time (layoff quarter) and group (earnings group) FEs. The coefficients on the "treatment" $\times$ layoff quarter dummies and their 95% CIs are graphed. The sample in each regression is limited to claims filed by individuals laid off in the quarters shown. Standard errors are cluster robust at the earnings group level.

Figure A9: EDD's Quarter Change Option



Notes: Example screenshot from EDD's online claim-filing system, demonstrating what a claimant would see if they were offered the quarter change option. Note that this information is simulated and does not apply to any actual claimants. This example claimant has filed their claim at the end of a BP, but would have received higher benefits (an additional $50 in WBA and $1,000 in MBA) if they had delayed their claim by one week. This information is explicitly shown to them and they are given the option to delay their BYB in order to receive the higher benefit amounts.

Table A1: Some examples of policy change driven variation in benefit risk

Claimant	HQW_1	HQW_2	Layoff Quarter	WBA_1	WBA_2
1	$10k	$10k	2001q3	$230	$230
1	$10k	$10k	2001q4	$230	$330
1	$10k	$10k	2002q1	$330	$330
2	$5k	$10k	2001q3	$130	$230
2	$5k	$10k	2001q4	$130	$330
2	$5k	$10k	2002q1	$160	$330
3	$10k	$5k	2001q3	$230	$130
3	$10k	$5k	2001q4	$230	$160
3	$10k	$5k	2002q1	$330	$160

Notes: HQW_1 = HQW if claim is filed in layoff quarter, HQW_2 = HQW if claim is filed in quarter after layoff. Orange rows highlight effects of the 2001q4 policy change in the quarter of the policy change. Magenta rows highlight effects of the policy change in the quarter after the policy change.