Efficiency Costs of Unemployment Insurance Denial: Evidence from Randomly Assigned Examiners

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Abstract

Approximately 10% of Unemployment Insurance (UI) claimants in the US are denied benefits after being deemed at-fault for their job loss by a government examiner. Using administrative data from California and an examiner leniency design, we estimate the causal effects of extending eligibility to marginally at-fault claimants—whose separation-reason would have been considered eligible by a different examiner group. Approving a marginally at-fault claimant increases UI benefits paid by over $3,000 and lengthens the nonemployment spell by just under 2 weeks, but it does not decrease total earnings. Combining these estimates to calculate the total fiscal externality of expanding eligibility on this margin, we find that these social costs are 16% of the total cost of the eligibility expansion. Using different research designs in the same data, we show that other types of UI benefit expansions have significantly higher efficiency costs. We provide suggestive evidence that lower efficiency costs for the at-fault eligibility expansion are driven by smaller responses among lower-income claimants who are disproportionately affected by at-fault eligibility criteria.

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1 Introduction

Many unemployed workers are denied unemployment insurance benefits after being deemed "at fault" for their job loss. According to the Department of Labor (DOL), 11% of Unemployment Insurance (UI) claims filed in 2019 were denied for this reason. While concerns about ex ante moral hazard (Chiu and Karni, 1998; Ejrnæs and Hochguertel, 2013; Lusher et al., 2022) necessitate *some* restriction of UI eligibility on this margin, the subjectivity of assigning fault presents other difficult questions: How should policymakers determine fault? Which claims should be denied?

In the US, states are tasked with defining circumstances under which a worker who voluntarily quit or was fired for cause remains UI eligible. States vary substantially in terms of which circumstances confer eligibility[1] and in the proportion of claims which are denied for these reasons—for example, South Carolina had over three times as many of these denials as Hawaii in 2019. Despite clear importance, empirical evidence that can inform these policy choices is extremely limited.[2]

In this paper, we combine administrative data from the State of California's (CA) UI program and variation across claim processing offices in their propensity to approve separation-based eligibility issues to estimate the causal effect of eligibility on nonemployment duration among "marginally at-fault" job losers. These responses impact social welfare through the fiscal externality that they impose on the government's budget. Therefore, a complete accounting of the social cost of the expansion must account for *all* responses that affect net government transfers (see, e.g., Lawson, 2017; Schmieder and von Wachter, 2017; Lee et al., 2021). To make progress in this direction, we also estimate the effects of eligibility on tax revenues, future UI receipt (during subsequent additional nonemployment spells), and the receipt of State Disability Insurance (SDI) benefits. Adopting a framework from Schmieder and von Wachter (2017) and Lee et al. (2021) we combine these estimates to calculate an easily interpretable scaled fiscal externality measure. For every $1 *mechanically* transferred to the unemployed by the expansion, this measure represents the additional cost to the government created by resulting changes in claimant behavior. Finally, we use the framework to compare this fiscal externality to those of two other UI benefit expansions (increases in benefit levels and durations)—estimated in the same data with separate research designs.

[1] For example, as per the Department of Labor's Comparison of State UI Laws, moving for a spouse's job opportunity is considered an eligible quit in 20 states and illness or injury preventing the worker from performing their job duties is considered is considered an eligible quit in 48 states.

[2] Lachowska et al. (2021) highlight the role of employers in "contesting" claims and the role this plays in low UI take-up rates. Skandalis et al. (2022) and Lachowska and Woodbury (2022) highlight implications of these criteria for racial and gender inequality in UI receipt. Venator (2022) demonstrates that extending UI eligibility to voluntary quitters who move with a spouse increases wages among female long-distance movers.

Our main finding is that the fiscal externality from extending eligibility to marginally at-fault claimants is substantially smaller than that of the other expansions we study. Each $1 mechanically transferred to the unemployed via an expansion in at-fault eligibility leads to an additional $0.19 cost due to behavioral responses. Corresponding values are $0.67 and $0.96 for benefit amount and benefit duration increases, respectively. These values are made up of several different claimant responses that we also estimate individually. Expanding eligibility to marginally at-fault claimants moderately lengthens nonemployment spells (+0.14 quarters), has small positive (but statistically insignificant) effects on earnings (+$68 per quarter), substantially increases the probability of receiving UI benefits during a *future* spell of unemployment (+6pp), and has no effect on SDI receipt.

We also show that the effect of at-fault eligibility on nonemployment duration is smallest among claimants with low pre-claim earnings levels. We provide suggestive evidence that this heterogeneity by pre-claim earnings is a key driver of the gap in fiscal externalities between the at-fault eligibility and benefit level expansions.

Understanding how claimants are affected by benefit eligibility is more demanding than simply comparing the characteristics of different claimant groups. A central concern is that there are unobserved differences affecting both eligibility approval and subsequent employment. Suppose an employer initiates a job separation, but it is in dispute whether the worker who claims UI was laid off for lack of work or fired for misconduct. Prior misconduct could directly hurt the worker's future employment prospects through word-of-mouth, and the types of workers who engage in misconduct egregious enough to be denied UI are likely less attractive job candidates.

Our identification strategy leverages variation across UI claim processing offices in their propensity to approve separation-based eligibility issues. Because claims with separation-based eligibility issues are quasi-randomly assigned to these offices for adjudication, any office-level differences in approval rates can be attributed to the causal effect of assignment to the office itself. After confirming the existence of these office-level differences and their ability to recover the local average treatment effect (LATE) of UI eligibility in an instrumental variables framework, we estimate a statistically significant 0.14 quarter increase in nonemployment duration, a small and imprecise increase in subsequent earnings, a substantial increase in future UI receipt, and no effect on SDI receipt. Our results are similar in a complementary research design based on the quasi-random assignment of claims with separation-based eligibility issues to individual government examiners within offices.

We contextualize the social cost of these behavioral responses as relatively small with an apples-to-apples comparison between several key margins of UI generosity. Informally, we measure the per-dollar cost to the government of different types of UI benefit expansions. Formally, the object of interest is the ratio of behavioral costs to the government's budget compared to the transfer absent any behavioral response. This has been used in previous work to compare different intensive margins of UI generosity (Schmieder and von Wachter, 2016; Lee et al., 2021), and we generalize its empirical implementation to study the extensive margin of benefit eligibility. Projecting employment effects from our main IV estimates onto the tax code delivers the behavioral costs, estimating the effect of eligibility on total government costs reveals the total cost of the expansions, and the difference between the two delivers the mechanical transfer.[3] By directly estimating the quantities of interest—as opposed to routing auxiliary estimates through a behavioral model—we take an approach similar to Lee et al. (2021).

Finally, we highlight implications for UI policy using a simple Baily-Chetty optimal UI framework. In lieu of observing consumption-smoothing benefits to estimate an optimal extensive margin UI eligibility threshold, we make a bounding argument. Under standard regularity conditions, those on the margin of extensive margin eligibility have larger marginal utility decreases when denied due to their lower earnings and larger percentage income drops. On efficiency grounds, the estimated lower disincentive costs and these larger consumption-smoothing benefits point to the desirability of reallocating UI benefit generosity away from the intensive margin towards the extensive margin.

In practice, however, there are a mix of other costs and benefits that such a reallocation would potentially incur, such as an increase in job separations (Ragan, 1984; Solon, 1984; Lusher et al., 2022) or a decrease in administrative costs. We notably provide suggestive evidence that inflow responses via separations or claim filing behavior are small (via estimates of spillovers onto former coworkers of marginally at-fault claimants) and that the potential administrative cost savings from expanding separation-based eligibility for UI are large (using aggregate data on appeals and the cost of appeals administration from the DOL).

This paper's primary contribution is estimating the employment effects and corresponding fiscal externality of a previously unstudied UI policy. In contrast to the large literature on the employment effects of intensive margin UI benefit generosity, Leung and O'Leary (2020) is the only existing paper on the extensive margin.[4] Their monetary eligibility variation due to the minimum earnings

[3] For some margins a slight variant of this procedure retrieves the mechanical transfer from a projection of unemployment potential outcomes from an instrumental variables complier analysis.

[4] A recent systematic review of causal estimates of UI disincentive effects contained 39 intensive margin estimates:

threshold is local to workers with very low earnings and labor force attachment. Because this monetary eligibility variation applies to a very specific population, it is *a priori* unclear how it would extrapolate to the much larger and more economically diverse population at risk of separation-based eligibility denial.

In providing a theoretically-motivated benchmark for the effects of separation-based eligibility relative to several other UI policy margins, we also provide the most comprehensive comparison of UI policy margins in a given institutional context. The most closely related paper in this respect is Lee et al. (2021), which estimates the per-dollar transfer costs of increasing WBA and decreasing partial benefit marginal taxes in the State of Washington.[5] We make a unified comparison across the two most commonly studied UI benefit intensive margins—WBA and PBD—in addition to our novel at-fault eligibility margin that has received little attention in the literature. In doing so, we can more plausibly abstract from other institutional factors in reconciling effect size differences. Pertinent to policy, we provide suggestive evidence on a welfare-enhancing budget-neutral reform and highlight the additional causal effects necessary to understand its net impact.

This paper is the first application of a leniency design to UI benefit receipt. The most closely related leniency applications include disability insurance (Autor et al., 2019; Maestas et al., 2013; French and Song, 2014); retraining benefits (Hyman, 2018); job search assistance (Schiprowski, 2020; Schmieder and Trenkle, 2020; Humlum et al., 2023); and food stamps (Cook and East, 2023). A recent survey of the applied microeconomics literature finds over 70 studies employing a judges design, where the application span criminal justice; finance; health; patents; and various government programs at the federal, state, and local level (Chyn et al., 2022).

A final contribution based on our identification approach new to the UI literature is estimating treatment effect heterogeneity of UI benefits across a wide range of observable characteristics. Our UI benefit individual-level variation is vast and does not directly depend on prior income or age. With this power, we find that responses to UI are the lowest for lowest-income claimants; if anything, their unemployment duration decreases. Heterogeneity in responses by prior earnings levels can also fully explain the gap in fiscal externalities that we calculate between the at-fault eligibility and benefit amount expansions. In contrast, the existing UI literature relies on either

18 pertaining to weekly benefit amount and 21 pertaining to potential benefit duration (Schmieder and von Wachter, 2016).

[5] Two other related papers estimate the per-dollar transfer costs of disability insurance policies (Haller et al., 2020) and pension policies (Haller, 2022). This quantity is also the fiscal externality term in the denominator of the Marginal Value of Public Funds (MVPF), which Hendren and Sprung-Keyser (2020) evaluate for a wide range of government transfer programs in the United States.

interacting state-level panel variation in UI generosity with observable demographics (e.g., Chetty, 2008), calibrating a lifecycle labor supply model (e.g., Michelacci and Ruffo, 2015), or estimating regression discontinuity designs at separate policy cutoffs across an age range of several years (e.g., Schmieder et al., 2012).

2 Institutional Details

In this section, we highlight general UI eligibility rules. First, we discuss separation-based eligibility adjudication, which provides identification for our causal research design in Section 4. We also outline general UI benefit parameters, or the monetary implications of benefit denial. We focus on California because that is the setting of our causal analysis, but we conclude by comparing California's rules to those in other states. There is significant across-state variation in separation-based eligibility policy design, but California's rules are broadly similar to those in most states.

2.1 Overall Eligibility Criteria for UI

Eligibility criteria fall into two broad categories: monetary eligibility and nonmonetary eligibility.[6] Within each of these two criteria categories, there are conditions related to the prior employment spell and conditions related to the claimant's current actions. In order to receive any UI benefits, workers must establish initial eligibility. After doing so, they must continue certifying their eligibility each week in order to receive that week's benefit payment.

Monetary eligibility criteria pertain to prior or current income, and they are relatively easy for the state to verify using administrative earnings records. In order to receive any benefits at all, the worker must have accrued a minimum amount of earnings during approximately the year preceding the claim. The dollar requirement is low enough that nearly all newly unemployed formal workers meet it.[7] After the claimant establishes initial eligibility, any income the claimant receives can reduce that benefit week's payment, potentially to $0.

Nonmonetary eligibility encompasses all other eligibility criteria, and it includes the focus of our project: separation-based eligibility. Separation-based eligibility criteria require that the worker lost their job through no fault of their own. Unlike monetary eligibility criteria, which are objectively verifiable based on administrative records, separation-based eligibility criteria hinge on a

[6]See the Department of Labor's Comparison of State UI Laws for a thorough discussion of UI policy parameters.

[7]For more detail on the minimum earnings requirement, see Appendix C where we use it in a separate research design.

subjective investigation into claimant and employer reports. And unlike other nonmonetary eligibility criteria—such as availability to start work, active job searching, and truthful reporting—that relate to the claimant's current actions, separation-based eligibility is exclusively retrospective.

2.2 Separation-based Eligibility Criteria and Detection

Rather than denying benefits to all workers who quit or were fired, separation-based eligibility criteria deny quits without "good cause" and firings with "misconduct". The California Employment Development Department (EDD) maintains a list of circumstances surrounding job loss that is intended to be exhaustive. These are broadly separated into quits and firings, and each detailed subcategory delineates how circumstantial details should affect the UI eligibility determination.[8] These detailed subcategories pertain both to work conduct and personal conduct.

Whether a given quit or firing is deemed no-fault is subjective, creating scope for claimants with similar circumstances to receive different eligibility determinations. The subjective criteria for firings hinge on the type, intention, and degree of employee misconduct, while those for quits highlight how "involuntary" the employee's decision was. EDD's UI Benefit Determination Guide contains a section titled "Weighing the Facts" that instructs adjudicating examiners to "imagine the Scales of Justice with both sides equally weighted" and consider whether the claimant's or employer's evidence carries more weight (California Employment Development Department, 2023). One instructive example is the guideline for adjudicating quits due to commute difficulty. The guidelines state that "because travel time is subjective, depending upon the claimant's situation and labor market area, there is no hard-and-fast answer".

Due to this subjectivity, states detect separation-based eligibility issues using attestations from the claimant and the previous employer. Figure 1 summarizes the eligibility issue detection process. All claimants select a separation reason upon claim filing; if they contend the reason was a layoff, the previous employer can dispute that contention. This process is starkly different from verifying monetary eligibility, which the state automatically checks upon claim filing using administrative earnings records.

Subject to some federal regulation, each state has the discretion to decide separation-based

[8] The quit subcategories are attendance at school or training, conscientious objection, voluntary leaving, travel difficulty, domestic circumstances, health and safety considerations, the irresistible compulsion to use intoxicants, leaves of absence, personal affairs, leaving for other work, union relations, and wages and time. The firing subcategories are attendance, attitude toward the employer, dishonesty, health or physical condition, insubordination, use of intoxicants and drug testing, manner of performing work, neglect of duty, off-the-job-conduct, relations with coworkers and customers, union relations, and violation of employer rules.

eligibility parameters. This is clearest for voluntary quits, as state law can explicitly designate qualifying circumstances. For example, in terms of provisions granting eligibility for specific circumstances, 27 states lack any related to joining the military, 9 lack any related to family member illness, 6 states lack any related to domestic violence, and 2 states lack any related to sexual harassment at work.[9] Apart from the presence of these general provisions, different states can require different types of documentation or apply a different burden of proof.[10]

2.3 Quasi-Random Assignment of Eligibility Issues to Offices and Examiners

A key feature in our California data that influences the eligibility determination is the quasi-random assignment of claims for examination. This occurs in two stages: first to an office and then to an individual examiner who generally works within the office. Our research designs use this assignment mechanism to isolate quasi-random variation in initial eligibility denials and benefit receipt, and our primary research design focuses on office assignment.

Once a claimant's self-report or employer's dispute triggers an eligibility investigation, it is assigned to a single office almost entirely based on the last two digits of the claimant's Social Security Number (SSN). The limited exceptions include claimants with special communication needs or employment from another state. Importantly for our causal research design, the last two SSN digits are quasi-randomly assigned by the federal government (see, e.g., Parker et al. (2013) or `https://www.ssa.gov/history/ssn/geocard.html`).

The mapping of the one-hundred final SSN digits to offices changes over time for two workload management reasons. First, the number of offices gradually increases from nine to fourteen. Second, even when the number of offices remains the same, EDD periodically reassigns SSN groupings from relatively understaffed offices to relatively overstaffed offices. Taken together, there are 19 distinct assignment regimes throughout our entire sample period.

After being assigned to an office, claims are then assigned to a single examiner for investigation and determination. While all claims are quasi-randomly assigned to offices, only a subset of claims is also *a priori* quasi-randomly assigned to examiners. Quasi-random assignment to examiners arises from a scheduling queue that sequentially matches pending claims to available examiners in the assigned office who speak the claimant's language. Around 40% of claims are assigned to

[9] California has provisions for all of these circumstances except for joining the military. See Chapter V of the Comparison of State UI Laws.

[10] These rules also vary internationally. For example, many countries allow UI claimants to retain eligibility even after a quit or a firing but impose lower benefit levels, as in Thailand, or a several-month-long waiting period, as in Germany and Japan (Schmieder and von Wachter, 2016).

examiners in this manner. These assignments are useful for our supplementary examiner research design, as unobserved examiner characteristics are conditionally independent of unobserved claim characteristics. The other 60% of claims are taken up while awaiting automatic scheduling on an ad hoc basis by an examiner with unexpected availability.

2.4 Monetary Implications of UI Eligibility

Initial eligibility approval makes you likelier—but not perfectly so—to replace lost wages with UI. Broadly speaking, there are three reasons why some ineligible claimants do not receive UI and some eligible claimants do: incomplete take-up, delayed eligibility issue detection, and subsequent appeals.

First, claimants with eligible initial determinations may never receive benefits if they do not subsequently submit a weekly certification. Examples include becoming quickly reemployed, not satisfying ongoing nonmonetary eligibility because of insufficient job search activities, or simply failing to complete the certification form. This is common for claims both with and without any separation-based eligibility issues, occurring 17% and 23% of the time, respectively.

Second, claimants may receive some UI benefits before a separation-based eligibility issue arises. This could be triggered either by periodic government audits or delayed employer disputes. Because initial claimants must satisfy a 1-week waiting period before they can receive benefits and employers must dispute a claim within 10 days of receiving government notice about the claim, this is uncommon.

Third, successful appeals to Administrative Law Judges in the California Unemployment Insurance Appeals Board can reverse the initial eligibility determination. Claimants who successfully appeal an ineligible determination can receive backdated benefits as long as they certify continuing unemployment for those weeks. Conversely, employers who successfully appeal eligible determinations prevent any further benefit payments; the government may claw back previously paid benefits occurs if the initial determination was due to claimant misreporting.

We combine Department of Labor aggregate data on appeal propensities and success rates with our microdata that indicate other reasons for benefit receipt to demonstrate the quantitative importance of each channel. Figure 2 illustrates the mapping of eligibility approval to UI benefit receipt by reason for those with separation-based eligibility issues in California, where bar widths are proportional to sample counts. Claimant appeals are both common and often successful, while employer appeals are both rare and often unsuccessful. Take-up is the most common reason for benefit nonreceipt among eligible claimants.

The benefits that eligible claimants can receive as they remain unemployed are economically meaningful. State benefits replace approximately 50% of prior weekly earnings and usually have a maximum duration of 26 weeks. Replacement rates are relatively lower for workers with higher and more stable earnings, as the weekly benefit amount (WBA) is a function of the highest quarterly earnings during the base period and is capped at $450 per week for most of our sample period. Potential benefit duration (PBD) increases during recessions through federally-funded extensions, reaching 52 weeks in 2002 and 99 weeks from 2009 to 2012. The PBD can also vary across claimants, as those with variable quarterly earnings can have lower PBD.[11]

3 Data and Descriptive Analysis: Who is Affected by Separation-Based UI Eligibility?

This section describes administrative data from California's UI program that we use in our analyses. The data includes linked employer-employee quarterly earnings, UI claims, and UI payments. Notably, we observe information on the adjudication of eligibility issues. We use this information to show that claimants with separation-based eligibility issues and denials are disproportionately from lower socioeconomic groups.

3.1 Data Sources

Our primary analysis links individual-level administrative datasets on UI claims (2000-2019) and quarterly earnings (1995-2019), both maintained by the State of California's Employment Development Department (EDD).[12] We exclude years before 2002 due to missing data on processing office assignments and years after 2019 due to a vastly different policy regime during the COVID-19 pandemic.

UI claims microdata contain nearly all of the information collected or produced by EDD while administering claims. The claims data comprise three categories: claimant self-reports at initial filing, benefit payments throughout the claim, and agency documentation of how the claim was processed. Claimant self-reports provide most of the demographics that we use. Benefit payments

[11] California's rules are comparable to those in other states. Its $450 maximum WBA is approximately the median amount across states during 2019, where the lowest is $221 in Louisiana and the highest is $844 in Washington State. Nearly every state's usual PBD is 26, though there is some variation from 14 weeks in Alabama to 30 weeks in Massachusetts. For additional detail about how WBA and PBD are calculated in California, see Appendix C.

[12] Subsets of these data have been used in a series of policy briefs on UI in California during the pandemic (Bell et al., 2022a).

are used as a measure of UI receipt—including detailed measures of payment timing, since we observe both the payment disbursal date, and compensated unemployment week. Finally, agency documentation includes any eligibility issues that were detected along with the timing and outcome of the resulting investigations. We also observe the processing office and examiner that handled the eligibility determination which is key for our research design.

We measure employment using the near-universe of California quarterly earnings records. These linked employer-employee data include all UI-covered earnings, which does not capture some government, nonprofit, and informal employment. We use these data for two purposes. First, to measure quarterly earnings after claim-filing. Second, we define nonemployment duration as consecutive quarters without any earnings, starting with the quarter *following* the initial claim.[13] This necessarily undercounts nonemployment duration by ignoring any within-quarter periods of nonemployment immediately following filing and immediately preceding reemployment. While this could plausibly bias our estimated nonemployment duration response downwards, we show in Section 5 that it also generates an upward bias for our welfare-relevant measure of eligibility expansion efficiency costs. Reassuringly, conclusions drawn from both estimates are consistent with each other.

Separation-based eligibility determinations are a unique aspect of the data that we use. For each determination, we observe the issue type (quit or firing), de-identified but time-invariant identifiers for the processing office and examiner handling the determination, the determination date, and the initial eligibility determination. Additionally, since 2009, we observe the claimant's self-reported separation reason: quit, firing, or layoff. Following Lachowska et al. (2021) we use the presence of a separation-based eligibility determination among claimants who self-reported being laid off as a proxy for employer contestation of the claim. Eligibility determinations among claimants who self-report a quit or firing are assumed to have arisen from that claimant self-report.

Since 2017 we observe two inputs which are used to assign determinations to *examiners*: the claimant's spoken language, and an indicator for whether examiner assignment was made by a conditionally-random algorithm. Since interviews are conducted by phone, the claimant's spoken language limits the set of potential examiners. The default is then for a scheduling algorithm to assign the next available examiner, so that the only claimant characteristic affecting assignment is

[13] We use quarterly nonemployment duration rather than weekly insured unemployment duration because the latter is unobserved for unpaid claims. For the purpose of defining unemployment duration, we right-censor at 8 quarters following the quarter of the initial claim. Both of these choices follow existing work on UI eligibility based on monetary eligibility criteria (Leung and O'Leary, 2020).

spoken language. When workload is high, some assignments are instead made through an ad hoc process which involves manager discretion.

We also observe claims and payments for California's State Disability Insurance (SDI) and Paid Family Leave (PFL) programs. SDI provides wage-replacement benefits to workers who require time off to recover from non-work-related illness or injury. PFL provides wage-replacement benefits to workers who require time off to bond with a new child or care for an ill family member. Both programs provide substantially higher benefit levels than UI, and SDI often provides benefits for a substantially longer time period than UI. Claimants must meet prior-earnings requirements similar to those of UI, and provide either a medial certification of the disability/illness (theirs for SDI, the person they are caring for for PFL) or proof of birth (PFL).[14] Going forward we use "SDI" as shorthand for the combination of both programs.

In descriptive analyses we use the SDI data to help characterize UI claimants affected by separation eligibility issues. As described further in Section 5.2, our welfare analysis is concerned with the total effect of changes to UI policy parameters on government spending—which includes potential spillovers on other government programs like SDI.

Finally, we link a subset of our main analysis sample ($\approx$ 3.9m claimants) to a large panel of person-quarter level addresses from Infutor. Infutor is a private data aggregator which specializes in Consumer Identity Management—providing person-level data with persistent person IDs to other private companies for marketing purposes. This data has been used, for example, by Bernstein et al. (2022) who show that it is broadly representative of the adult US population. This linkage allows us to observe the zip code of the claimants primary address in the quarters following their claim. This allows us to test for out-of-state migration responses, which could bias our employment and earnings results (since out-of-state earnings are not included in our CA data).

3.2 Characteristics of Claimants with Separation-Based Eligibility Issues

Table 1 shows that separation-based eligibility issues and denials are extremely common, and that this is especially true in various disadvantaged groups. Each column contains sample sizes and average characteristics for a given subset of claimants. Among monetarily eligible claims, nearly one-third had at least one separation-based eligibility determination and 40% of these were denied. Younger, non-white, female, and lower-income claimants are likelier to have a separation-based

[14] SDI and PFL currently replaces roughly 60-70% of base period earnings up to a maximum of $1,620. Maximum SDI PBD reaching 52 weeks and each claim's realized PBD determined by the severity of their disability. PFL benefits are paid for 8 weeks.

eligibility issue and for that issue to result in benefit denial.

A substantial portion of UI claimants with separation issues also appear to transit onto the UI from the SDI program—14% of claimants with separation issues receive some SDI benefits in the year preceding their UI claim, much higher than the 9% of all monetarily eligible UI clams with recent SDI receipt. This suggests that health issues are a common factor precipitating separation eligibility issues in the UI program. This SDI receipt is concentrated among voluntary quit claimants, roughly 20% of whom receive SDI in the quarter before their UI claim.[15]

Frequent flows between the two programs also suggest that program substitution may be important in this context. Some UI claimants whose separation eligibility is marginally denied may see increased SDI receipt. We test for these responses in Section 4 and incorporate them in our welfare analyses in Section 5.2.

Even among eligible claims, a separation-based eligibility investigation may delay payments. As a reference point, Column 1 of Table 2 shows the fraction receiving any UI payments, and typical payment delays for claimants without eligibility issues. A mandatory 1-week waiting period means that the first covered week of unemployment usually occurs 10 days after claim filing, and processing logistics mean that claimants usually wait another week until receiving the payment for first that covered week. Because eligibility investigations require interviewing the claimant and former employer, Column 2 shows that the time to first payment is slightly delayed for claimants who are investigated but ultimately approved.[16] Finally, Column 3 shows that a successfully appealed separation-based eligibility denial delays payment receipt by over two months as the case proceeds through the California Unemployment Insurance Appeals Board.

While the above patterns are evidence of disparate impact, it is important to note that we cannot determine the underlying cause. Socioeconomically disadvantaged people may have more separation eligibility issues because of the nature of their jobs, their personal circumstances, or their preferences. Similarly, among claimants with eligibility issues, we cannot say whether lower-income claimants are likelier to have separation circumstances befitting denial or are likelier to be denied conditional on those circumstances.

[15]While we use"SDI" as shorthand for the combination of the SDI and PFL programs, only a small portion of recent SDI receipt among UI claimants with separation issues is accounted for by the PFL program.

[16]Both of these delay measures are well under the Department of Labor's definitions of UI agency timeliness.

4 Causal Results: What are the Employment Impacts of Separation-Based UI Eligibility?

In this section, we present our main result that UI benefit eligibility has only modest effects on a claimant's subsequent employment outcomes. We focus on a design leveraging variation in the processing office to which claimants from 2002 to 2019 were assigned, and show in Appendix B that results are similar using a more granular source of variation over a shorter time period.

4.1 Design: Processing Office Assignment as UI Policy Variation

Our research design leverages quasi-random assignment of claims with separation-based eligibility issues to processing offices for adjudication, as described in Section 2.3. By comparing subsequent employment outcomes of claimants assigned to relatively lenient offices and those assigned to relatively strict offices, we approximate an experiment that approves claimants marginally denied due to separation-based eligibility issues.

Treating the initial eligibility determination as the endogenous treatment of interest—and estimating its effect on marginally eligible claimants—corresponds to a policy reform that increases leniency when adjudicating separation-based eligibility issues. We argue this is more policy-relevant than the actual receipt of payments, as the initial eligibility determination is the policy lever at the UI agency's disposal. Nevertheless, we sometimes scale by measures capturing the monetary consequence of benefit eligibility, such as eligibility's effect on benefit receipt or the total potential benefit duration.

4.2 Implementation: Econometric Framework and Identification Assumptions

We first lay out the main estimating equation for the design based on assignment to processing offices. We then present and validate the identifying assumptions necessary to interpret the coefficient of interest as a causal effect of UI eligibility.

4.2.1 Estimating Equation

We construct our instruments to isolate the quasi-random assignment of a given eligibility determination to a processing office. We ensure that identifying variation in eligibility approval stems from the SSN-based assignment regime, and we allow eligibility approval leniency within a given processing office to vary across quits vs. firings.

From communication with the data provider, we know that claims have been assigned to processing offices via the last two digits of the claimant's SSN for the entirety of our sample. However, we only observe the specific rules which assign claims to processing offices for 11 of the years in our sample, so we instead infer these rules from the data in all years. To do so, we define the intended processing office for a given combination of the final 2 SSN digits and claim filing month as the office that processed at least 95% of those SSN digits' claims in the month. This measure is comprehensive: 97% of digit-month combinations have a defined intended office, and it is missing mostly when the SSN-based assignment regime changes in the middle of a month. It is also externally validated: for the 11 years with written agency documentation of the SSN-based assignment regime, it agrees for more than 99% of digit-months.

At a given point in time, the mapping of claims to intended offices is quasi-random. However, because both office assignment propensities and potential outcomes change over time, we additionally include time controls. Office assignment propensities change over time as office openings shift the number of SSN digits for which each office is responsible. Potential outcomes change with macroeconomic conditions that affect the composition of UI claimants and their job search prospects.

Our instruments additionally interact the processing offices dummies implied by the SSN-based assignment regime with the separation-based issue type (i.e., discharge for misconduct vs. voluntary quit). Allowing for flexibility in leniency appears to be empirically relevant, as Figure A1 shows that office-level leniency is uncorrelated across issue types, even though there is statistically and economically significant variation across offices within each issue type.[17] Our specifications control for issue type to partial out level differences in eligibility across issue types.

This strategy requires us to assume that the issue type itself is unaffected by the processing office. It would be a problem if there is often ambiguity in the proximate job separation reason and processing offices differ in classification tendencies.[18] In practice, this is rare. Only 7% of cases have claimant-reported separation reasons that do not match the final determination's issue type, and many of these are plausibly due to claimant transcription error during claim filing. Figure A2

[17] One potential explanation for this—that we cannot empirically verify or rule out—is that offices differ in both their propensity to approve claimants vs. employers and their propensity to rule in favor of the party with vs. without the burden of proof (employers have the burden of proof for misconduct determinations, while claimants do for voluntary quits). The variation in propensities would have to be similar in magnitude, but they could have an arbitrary correlation.

[18] Existing UI case law does contain a handful of precedents where this was the case, such as withdrawn resignations, mutual misunderstandings, and pressured resignations. See California Unemployment Insurance Appeals Board precedent summaries at https://cuiab.ca.gov/precedent-decisions-a-d/.

shows that the minimal variation (1.3 percentage points) in the composition of separation-based eligibility issue types across offices is uncorrelated with issue-specific eligibility approval rates.

Consider the following system of equations for claimant i who files an initial claim in month t:

$$Y_{it(s)} = \beta D_{it} + \mathbf{X}'_{it(s)}\psi + e_{it} \tag{1}$$

$$D_{it(s)} = \mathbf{Z}'_{it(s)}\gamma + \mathbf{X}'_{it(s)}\mu + \varepsilon_{it} \tag{2}$$

where $Y_{it(s)}$ is an outcome of interest (e.g., UI payment amount, subsequent earnings, nonemployment duration, etc.), D_{it} is an indicator for eligibility approval at the initial separation-based eligibility determination, $\mathbf{Z}_{it}$ is a vector of indicator variables corresponding to the interaction of assigned processing office and separation-based eligibility issue type, and $\mathbf{X}_{it(s)}$ is the fully interacted set of claiming filing month-by-issue type dummies that serve as control variables. The coefficient of interest is β, and the equation is overidentified because the excluded instrument $\mathbf{Z}_{it}$ is a vector.

Our preferred estimator for Equations 2 and 1 is the two-step estimator unbiased jackknife IV (UJIVE) (Kolesár et al., 2013). The UJIVE estimator extends the logic of jacknife IV (JIVE) to accommodate covariates (Angrist et al., 1999). To do so, it leaves out the own observation in the first-stage given by Equation 2 both when partialling out the covariate fixed effects and when projecting the vector of office-by-issue instruments onto the endogenous treatment. Results are qualitatively similar when we employ alternative estimators like 2SLS and LIML.

Following a design-based approach to inference due to our individual-level variation, all specifications cluster standard errors by claimant (Abadie et al., 2022). We do so because SSN-based randomization is persistent for individual i making multiple claims in time t and t'. There are indeed individuals with multiple claims that have separation-based eligibility issues in the sample period: our sample includes 6.9 million initial claims with eligibility issues from 5.5 million unique individuals.

4.2.2 Validating the Instrument

The coefficient β in Equation 1 identifies a local average treatment effect as long as four broadly defined conditions hold: first-stage relevance, independence, exclusion, and monotonicity. We present supporting evidence of each in the form of testable implications and institutional details.

First-stage relevance. The first identification assumption—first-stage relevance—requires that office assignment be predictive of separation-based eligibility. We directly test this by estimating the first-stage regression equation Equation 2 and testing the joint significance of the office-by-issue dummies.[19] Column 1 of Table 3 shows the first-stage F-statistic for this regression is 405, which is well above the threshold of 104.7 that ensures 95% coverage without any adjustment to standard errors (Lee et al., 2022).[20] Figure 3 plots the coefficients from the first-stage regression; variation in eligibility approval propensity is evenly spread across office-by-issue pairs.

Independence. The second identification assumption—independence—is bolstered in part by the SSN-based assignment of claims to processing offices, and we present evidence in favor of it in the third through sixth columns of Table 3. As before, we test the joint significance of the office-by-issue instruments in Equation 2. In each regression, the outcome variable is a pre-existing claimant characteristic. For several different claimant characteristics, the range across office-by-issue is narrow, and we fail to find statistically significant differences across office-by-issue pairs.

Excludability. The third identification assumption—excludability—requires that the only effect processing offices have on the endogenous outcome Y is through the initial eligibility determination. The primary excludability concerns relate to the other administrative processing duties that offices handle, as there are economically small but statistically significant differences in these measures. The last two columns of Table 3 show that the expediency of making an eligibility determination and the propensity to find continuing claim issues differs across offices.[21] Both of the F-statistics are statistically significant, but the range across office-by-issue pairs is only 4 days and 3 percentage points, respectively. Reassuringly, Figure A3 shows that these minor differences are uncorrelated with eligibility approval propensity. Accordingly, in Appendix B, we show that results for the design based on examiner assignment are similar when including or excluding office-level variation.

While processing offices handle several administrative duties, it is important to note that they are not responsible for job search assistance or training. Job postings are provided on a centralized online board, CalJOBS$^{\text{SM}}$, for which all UI claimants are required to register. Similarly, personalized workforce services are accessible based on geographic proximity—rather than assigned based on SSN digits—through American Jobs Centers of California$^{\text{SM}}$.

Our results primarily focus on eligibility approval as the endogenous treatment of interest, as

[19] Testing the joint significance of the office-by-issue dummies $\mathbf{Z}_{it}$ rather than a constructed scalar leniency correctly accounts for the degrees of freedom in the overidentified setup (Hull, 2017).

[20] When the first-stage F-statistic falls below this level in specifications with limited subsamples, we inflate standard errors following the tF-adjustment of Lee et al. (2022).

[21] Continuing claim issues include failure to engage in work search, failure to be available for work, and irregular reporting. These most often results in denying benefits for only the relevant week.

it is a clear policy level of the government. However, we also contextualize magnitudes using other measures of UI benefit receipt, such as by scaling the effect of eligibility on receiving UI payments. When doing so, positive bias from plausible exclusion restriction violation makes our main findings of small employment responses even starker. The likely source of an exclusion restriction violation is UI benefit timing. Conditional on receiving UI benefits, a claimant assigned to a relatively lenient office is likelier to receive those benefits through an initial eligible determination rather than a successful appeal following an initial ineligible determination. Based on the third row of Table 2, this means the claimant waits, on average, over 2 fewer months to receive the first payment. If anything, the increased liquidity afforded by more expedient payments leads to a longer unemployment duration.

Monotonicity. Independence, excludability, and first-stage relevance are sufficient in the constant effects setup in Equations 1 and 2. However, if we allow for heterogeneous treatment effects so that the parameter of interest in Equation 1 is β_i, then we require some form of a monotonicity assumption. Monotonicity assumptions place restrictions on the first-stage relationship in Equation 1, and they ensure that the resulting estimand of interest in Equation 1 can be interpreted as a local average treatment effect (LATE) by weighting individual treatment effects with weakly positive weights that sum to 1 (Imbens and Angrist, 1994).

Broadly speaking, there are two types of monotonicity concerns stemming from Equation 1: the assumed homogeneity of γ and the included control variables $\mathbf{X}_{it(s)}$. The first type of concern yields testable implications, and we provide evidence in favor of it. The second type of concern motivates specific robustness checks, which we explore in Appendix A.

The first type of monotonicity concern is that compared to another office-by-issue pair, a given office-by-issue pair may be relatively more lenient with one type of claimant but relatively less lenient with another type. This applies even under unconditional random assignment; for simplicity, suppose that is the case so that Equation 2 omits the fixed effect controls. A sufficient monotonicity condition to recover a LATE would then be *average monotonicity* (Frandsen et al., 2023; Chan et al., 2022). Informally, this requires that office-by-issue-specific eligibility approval and overall eligibility approval are positively correlated for each claimant. Formally, the average monotonicity condition is:

$$\sum_{j \in J} \lambda_j \left(p_j - p\right)\left(D_i(j) - \overline{D}_i\right) \geq 0 \quad \forall i \tag{3}$$

where J is the set of office-by-issue pairs, J_i is the office-by-issue pair corresponding to claimant

i, $\lambda_j := Pr(J_i = j)$ is the probability a claim is assigned to j, p_j is the approval probability for claims in office-by-issue pair j, $p := \sum_{j \in J} \lambda_j p_j$ is the average approval probability across all offices and issues, $D_i(j)$ is the counterfactual determination if claimant i were assigned to j, and $\overline{D}_i := \frac{1}{|J|} \sum_{j \in J}$ is claimant i's overall expected approval probability.

A test to assuage the first type of monotonicity concern is showing that the same overall first-stage relationship holds within various subsamples (Frandsen et al., 2023). A useful auxiliary object for this test is the predicted first-stage from Equation 2. To calculate it, we manually implement the residualization and leave-one-out procedure. Specifically, let $A_{it(os)}$ be an indicator eligibility approval of claimant i's claim filed in month t with issue types s and an SSN that implies they should be assigned to office o. We residualize this by fully-interacted office-by-issue controls in Equation 2:

$$A^*_{it(os)} = A_{it(os)} - \mathbf{X}'_{it(s)} \mu \tag{4}$$

We then calculate the scalar leave-one-out mean of this residualized leniency measure at the office-by-issue level, which is indexed by os:

$$\tilde{Z}^{os}_{it} = \left(\frac{1}{n_{os} - 1} \right) \left(\sum_{k(os)} \sum_{t(oj)} A^*_{kt(os)} - A^*_{it(os)} \right) \tag{5}$$

where n_{os} denotes the total number of algorithmically scheduled separation-based eligibility issues of type s assigned to office o.

In support of the monotonicity assumption, Figure 4 shows that this leniency measure is positively correlated with the claimant's own eligibility approval decision within various subsamples. This holds for claimant demographics and prior employment. Along these lines, Section A shows that even when we allow leniency to vary at various socioeconomic levels or over time, these group-specific or period-specific approval probabilities are highly correlated for a given office-by-issue pair.

The second type of monotonicity concern is related to the *conditional* quasi-random assignment that motivates including fixed effects. There are two conceptually distinct issues that stem from including controls in the specification, but both can be addressed in a specification that fully saturates Equations 1 and 2 with interactions between the office-by-issue instruments and the fixed effect controls.[22]

We confirm that our results are not sensitive to the second type of monotonicity concern. In particular, we run separate regressions in each of the 480 month-by-issue cells and manually aggregate the estimates in proportion to cell size. Each separate regression is within a given month-by-issue cell, so it does not require additional controls. This permits flexibility in the first and second-stage equations for each month-by-issue pair, so none of them is susceptible to the second type of monotonicity concern. Reassuringly, as shown in Table A1, all of the manually reweighted estimates fall within the confidence intervals from our main specification.

4.3 Results: Eligibility Affects UI Receipt More than Employment

Unsurprisingly, initial eligibility approval increases UI benefit receipt. Columns 1 through 3 of Table 4 show estimates of Equations 1 and 2 using UJIVE, where the endogenous treatment D is eligibility approval and the outcome Y is some measure of benefit receipt. The average value of the outcome Y among those who were on the margin of eligibility approval but received an ineligible determination is also shown for all outcomes in this table.[23]

For those on the margin of eligibility approval, eligibility approval doubles various measures of benefit receipt. For example, Column 1 shows the probability of receiving any UI payments increases from 31% to 63%. Panel (a) of Figure A5 illustrates this doubling of payment receipt by presenting the "visual IV", where the causal effect is represented by the positive relationship between the office-by-issue-level reduced-form differences in payment receipt vs. first-stage differences in eligibility approval (Angrist, 1990). Interestingly, while the 31% receipt rate for marginally denied claimants roughly coincides with the 26% average benefit receipt rate among all separation-based eligibility denials given in the final column of Table 1, the 63% receipt rate for marginally approved claimants is somewhat lower than the receipt rates among either all claims without eligibility issues (77%) or all claims with separation-based that were nevertheless approved (83%), as shown in Table 2.

Despite doubling UI benefit receipt, separation-based eligibility approval has virtually no effect

[22] One possible issue is if unmodeled heterogeneity in the first-stage effects γ in Equation 2 is correlated with the included fixed effects. Goldsmith-Pinkham et al. (2022) show this would induce *contamination bias*, meaning that the estimated office-by-issue effect in one entry of γ would depend on the true office-by-issue effect in another entry of γ. Another concern stems from treatment effect heterogeneity in the second-stage equation: β in Equation 1. In this case, Blandhol et al. (2022) show it is possible for a specification that does not include full interactions between the instruments and covariates to deliver negative weights.

[23] Following Frandsen et al. (2023), we estimate the untreated complier mean $\bar{Y}^0$ by interacting Y with an indicator for eligibility *denial* $(1 - D)$ in Equation 1, replacing the indicator for eligibility approval D with an indicator for eligibility denial $1 - D$, and estimating the system. We can also estimate the treated complier mean by interacting Y with an indicator for eligibility *approval* D in Equation 1.

on subsequent earnings and only modest effects on subsequent unemployment. Columns 4-6 have the same structure as Columns 1 through 3, except the outcome Y is some measure of subsequent employment. All outcomes in Columns 4-6 are measured over the 12 quarter period beginning with the quarter after the initial claim. Column 6 reports the small, positive, and statistically insignificant impact on average subsequent quarterly earnings over the 12 quarters following the initial claim. The point estimate is 68.1, indicating that UI eligibility *raises* subsequent total earnings across all 12 quarters by just over $800, but imprecise.

An indicator for any employment in a quarter is much more precise outcome than total earnings in the quarter, and examining this outcome reveals decreases caused by UI eligibility. Column 5 shows that approved claimants are 4 percentage points less likely to have any employment in the quarter following the initial claim. This difference is just under one-tenth the untreated complier mean employment rate in that quarter (0.49), and it is highly statistically significant.

We also find increases in nonemployment duration when we translate the lack of employment in a given quarter into a nonemployment duration. Column 6 shows that eligibility lengthens nonemployment by 0.14 additional quarters, while the effect is not statistically significant, theory, prior work on moral hazard responses to UI benefits, and the related outcome in Column 5 suggest to us that this is a real response. This less than 2-week increase in nonemployment duration is small relative to the more than 10-week increase in unemployment benefit receipt. However, because our quarterly measure is an underestimate of the underlying weekly measure as discussed in Section 3.1, the estimated treatment effect will be an underestimate in absolute value.

Columns 7-8 show that marginally eligible claimants are substantially more likely to receive UI benefits on a future claim occurring after an intervening spell of employment, again measured over the 12 quarter period beginning with the quarter after the original claim. Specifically, the probability of receiving any payments on such future claims increases by just under 6pp, which is more than 25% of the untreated complier mean. This corresponds to a $697 increase in UI benefits received, above and beyond the additional $2,547 increase on the original spell, shown in Column 3.

Despite relatively high SDI use in this sample over the 12 quarters following the UI claim (e.g, an untreated complier mean of $1,052), we find no evidence of program substitution among marginally approved claimants. While the point estimate implies that SDI receipt decreases slightly in response to UI eligibility, the decrease is both economically small and imprecise.

Dynamics. We next show the dynamics behind our main results in Table 4. To do so, we run separate regressions of the form in Equation 1 and Equation 2, where the outcome $Y(q)$ is now a benefit or employment outcome measure q quarters relative to the initial claim filing. Figure 5 displays these dynamic treatment effects, Figure A7 provides additional context which illuminates mechanisms for some of our main results, and Figure A6 further decomposes (some of) those treatment effects into potential outcomes for marginally approved and denied claimants.

We do not find any evidence of employment hysteresis, as any impacts on contemporaneous employment or earnings are statistically insignificant and typically roughly zero 7-12 quarters after the initial claim. Panels (a) and (b) of Figure 5 plot treatment effects for employment and earnings, respectively, in a 4-year window centered around claim filing. There is an immediate decrease in the probability of any employment, but the effect diminishes over time and disappears at the end of the window. There is never a statistically significant effect of eligibility approval on average quarterly earnings. A majority of point estimates following claim filing are actually positive, especially later in the followup window, and all are quite close to 0.

Panel (c) UI receipt on future claims is substantially increased by eligibility in all quarters. We assign payments to claims (as opposed to people) based on claim "benefit year begin" dates (BYBs) available in the data. In this way we can identify benefit receipt that occurs on a future UI claim (i.e., not the claim that led to the separation-based eligibility determination in question). We see that future benefit receipt is substantially elevated in all quarters in the followup window. This effect peaks 6 quarters after the original claim and then begins to dissipate.

Panel (d) shows that the null result for SDI benefit receipt from Table 4 holds in nearly all quarters. Estimates are imprecise in all quarters, starting off positive before turning negative and converging to zero by quarter 12.

Figure A7 shows that the increase in future spell UI receipt shown in panel (c) of Figure 5 is likely not driven by an increase the number of unemployment spells. Unemployment spells are not directly observable in our data unless they lead to a UI claim, so we test for this response by observing separations in the earnings data—which occur in quarter q when a worker's main (highest paying) employer in quarter q is no longer their employer in quarter $q+1$. This outcome is defined only among the employed, so we focus on quarters 5-12 after the original claim (which avoids endogenous sample selection issues since the effect of eligibility on employment is no longer present at that point). Panel (a) of Figure A7 shows no effect of eligibility on future separations.

Figure A7 also shows that the employment results in Table 4 and Figure 5 are not driven by migration responses. Since we only observe earnings within CA, any effect of eligibility on out-of-state migration would show up as a decrease in employment. As described in Section 3.1 a subset of our data can be linked to data from Infutor which contains a panel of current addresses for many US adults. Panel (b) of Figure A7 shows that there is no effect of eligibility on out-of-state migration.

Figure A6 contextualizes the magnitude of treatment effects relative to baseline outcome outcomes by separately plotting potential outcomes for marginally approved and denied claimants. The dissipation in treatment effects on employment probability is driven by an increase in employment among the approved rather than a decrease in employment among the denied. For both marginally approved and marginally denied claimants, there is a persistent decrease in any employment and total earnings of around one-third. This is similar to unemployment earnings losses documented in previous work (Davis and von Wachter, 2011; Jacobson et al., 1993). This is evidence against an interpretation that separation-based eligibility's relatively small effect is driven by labor force exit irrespective of UI eligibility.

Heterogeneity. Relative to the wider literature on the moral hazard costs of UI benefits, our identifying variation is unique in that varies across individual claims and has a first stage that is similarly strong among a wide variety of claimant subgroups. This makes us well-positioned to consider heterogeneous responses across different types of UI claimants. Table A2 separately re-estimates Equations 1 and 2 within specific subsamples.

Several notable sources of heterogeneity are apparent. Perhaps most stark are differences by recent earnings levels, employment, and SDI receipt. Although estimates are imprecise, the lowest earnings group, UI claimants who were not employed (had zero earnings) in the quarter *before* their UI claim,[24] and UI claimants who moved from the SDI program to the UI program, all see their nonemployment durations *decrease* in response to UI eligibility.

There is also meaningful heterogeneity by demographic characteristics—with old, young, and white claimants demonstrating small nonemployment responses relative to middle-aged and non-white clamiants—and calendar time—with larger nonemployment responses in the early 2000s and the Great Recession. The specific nature of the eligibility issue in question also appears important, as claimants who self-reported a layoff (i.e., whose separation eligibility issue was likely driven by employer contestation) and to a lesser extent voluntary quitters have smaller nonemployment

[24] This is possible, if, for example, a worker delays UI claim-filing for a meaningful period of time after their job loss, or if a worker moved from SDI to UI.

responses.

Heterogeneous responses across subgroups defined by prior SDI use, self-reported separation reason, and eligibility issue have potentially important implications for policymakers interested in expanding separation-based eligibility. One approach for such policymakers would be to target a relaxation of eligibility criteria towards claimants with smaller moral hazard responses. Both the underlying reason for the separation and whether the issue was detected via employer contestation or claimant self-report are easily observable to the UI agency, making these factors candidates for such an approach. If we consider prior SDI use to be a proxy for separations related to health issues, this further suggests that more detailed information on separation reasons (beyond simply whether the job loss was a layoff, firing, or voluntary quit) would be similarly useful.

Several of the findings in Table A2 also contribute to the relatively limited literature on heterogeneity in the moral hazard response to UI benefits. The lack of response to eligibility for the youngest and oldest workers support findings from theoretical lifecycle models that moral hazard responses should be lowest for the youngest workers (Michelacci and Ruffo, 2015). The lack of response among the lowest-income claimants is, to our knowledge, a novel finding in the literature. Both findings relate to ongoing policy discussions about potential justifications for so-called "differentiated" UI programs that provide different benefit generosities to different types of claimants (Spinnewijn, 2020).

To test whether heterogeneity in the effect of eligibility on UI receipt is a likely mediator of heterogeneity in employment responses, Table A3 shows that this heterogeneity in the nonemployment duration effects of eligibility is not driven by heterogeneity in the effects of eligibility on UI receipt. With the exception of subgroups defined by the claimant's self-reported reason for the job loss, we find little heterogeneity in this outcome.[25]

5 How Do the Effects of Separation-based Eligibility Compare to Other UI Policies?

This section shows that the claimant responses to separation-based eligibility estimated in Section 4 correspond to relatively low efficiency costs. We accomplish this by producing similar estimates for two other types of UI program expansions—increases in weekly benefit amounts (WBA) and

[25] Among claimants who self-report a job loss, and whose eligibility issue was therefore likely driven by employer contestation, eligibility has no effect on UI receipt. This may suggest that the issue detection process takes longer in such cases, so that claimants receive at least one payment before the determination process begins, or that the complier claimants within this subgroup are especially likely to successfully appeal ineligible decisions.

potential benefit duration (PBD)—in the same data and context. We then translate the estimates for all three policy changes to a scaled fiscal externality that can be compared across policy changes. Finally, we provide suggestive evidence that the relative efficiency of expanding UI on the at-fault eligibility margin is explained by heterogeneity across income groups. Lower earners have smaller responses to benefit generosity and are over-represented in the marginally at-fault claimant group.

5.1 Illustration of Per-Dollar Transfer Costs for All UI Policy Margins

We use a simple quantity to compare the social costs of different UI program expansions: the cost to the government per dollar *mechanically* transferred to the unemployed by the expansion. Following Schmieder and von Wachter (2017), we refer to this quantity as the "behavioral cost/mechanical cost" ratio (BCMC). In Appendix C.1 we derive this ratio formally and demonstrate its welfare-relevance in a stylized optimal UI framework. Here we provide a simple visual illustration, highlight the advantages of the approach, and explain how we implement it empirically.

Panel (a) in Figure 7 demonstrates a hypothetical change in non-employment duration due to an increase weekly UI payments from $\$b$ to $\$b + db$.[26] As a result of this program expansion the probability that a representative job-loser remains unemployed at time t, S_t, shifts upwards by some arbitrary amount. The fiscal externality from this "behavioral" response is simply the area between the two survivor curves (shaded red), in terms of the government's budget (e.g., additional UI benefits paid, tax dollars foregone, etc.). The mechanical transfer that we use to scale this fiscal externality is the total \$ amount that would have been transferred to the unemployed *in the absence of any behavioral response* (blue region). This is simply the expected number of UI benefit payments that the unemployed person would have received in the absence of the expansion, which we denote $\bar{B}_0$, multiplied by $\$db$.

Panel (b) illustrates the same quantities for an increase UI benefit duration from P weeks to $P + dP$ weeks. Here benefits are only transferred mechanically to individuals who remain unemployed until at least week P. Denoting the share of individuals who remain unemployed until week P as S_P, the mechanical transfer in this case is $S_P \cdot \$b$.

5.2 Empirical Implementation

Following Lee et al. (2021), we calculate BCMC ratios for each policy change with a simple accounting identity: total costs = mechanical costs + behavioral costs. Appendix C.1 derives this

[26] Note that this could represent either an intensive margin benefit increase ($\$b > 0$) or an eligibility expansion ($\$b = 0$).

identity, but its intuition is simple. Even without any behavioral response by claimants (e.g., longer spells) an expansion has some cost to the government's budget (additional UI benefits are paid out *mechanically*). Any additional costs can be seen as the consequence of some behavioral response on the part of the claimant (or others who are indirectly exposed to the consequences of the reform).

A key advantage of this approach in our setting is that we observe (or can construct reasonable proxies for) a large portion of the components of an expansion's total cost. This allows us to directly estimate the BCMC ratio and avoid the need to route lower-level estimates (e.g., effects of a policy change on nonemployment duration) through a behavioral model of job search (as in, e.g., Schmieder and von Wachter, 2017). We directly observe UI benefits paid, SDI benefits paid, and earnings. Earnings are transformed into tax revenue using a 31.47% tax rate to translate labor income to taxes paid.[27] Total costs are defined as the sum of UI benefits, SDI benefits, and (negative) tax revenue, each measured over the 12 calendar quarters beginning with the quarter after the claim.

With this data and accounting identity, we can calculate the BCMC ratio for a given expansion in three steps. First, the change in net government expenditure (total costs) of the reform can be directly estimated via the relevant research design. Second, we estimate *either* the behavioral costs or the mechanical costs of the expansion. Behavioral costs have three components: additional UI benefits paid on the current claim as a consequence of longer nonemployment spells, the change in UI benefits paid on future claims, and the change in SDI benefits paid. The latter two components can be estimated directly, the former can be estimated as the effect on nonemployment (or insured unemployment) duration, multiplied by the per-period benefit amount the claimant is eligible for, scaled to account for incomplete take-up (e.g., not all eligibles receive payments), and capped at the claimants potential benefit duration.[28] Mechanical costs can be estimated similarly via non or unemployment durations for untreated compliers. Third, we subtract our behavioral cost or mechanical cost estimate from our total cost estimate to retrieve the remaining component.

A few caveats are worth mentioning. Our definition of government expenditures as UI and SDI benefits minus implied tax revenue does not account for effects on other government transfers (Leung and O'Leary, 2020) or spillover effects onto other people who are indirectly exposed to the

[27] We prefer this value, which is the US total tax wedge in 2015 according to the OECD, to maintain comparability with Schmieder and von Wachter (2016). It is similar to the 34.3% marginal tax rate for federal, state, and payroll taxes estimated by NBER TAXSIM for a single California worker in 2015 with our separation-based analysis sample's average income.

[28] For extensive margin reforms, insured unemployment duration (e.g., number of UI payments received) is not available, since untreated claimants are ineligible.

expansions.[29] Our sources of exogenous variation are all at the individual-level, and dependent on features of UI claim applications, so we always condition on the realized sample of UI claimants. This ignores any effect a UI policy reform could have on the composition of UI claimants as outlined in Figure 1: either selection into unemployment, selection into claim filing conditional on unemployment, or, for the case of separation-based eligibility, selection into an eligibility determination.

Below we outline the implementation of this strategy for each policy change. Appendices C.2 and C.3 provide more detail on sample construction, estimating equations, and identification assumptions.

Separation-based eligibility. For this expansion, we first use our office-by-issue instrumental variables (IV) design to estimate the effect of eligibility on net government expenditure (total costs). Next we use the same design to estimate the behavioral costs of the reform, defined as the UI benefit dollar amount implied by the effect of eligibility on non-employment duration, plus the estimated effects on SDI benefits received, tax revenue, and future UI benefit receipt. Finally, the mechanical cost is calculated as the difference between the total and behavioral costs.

To translate the estimated effect on non-employment duration to UI benefit costs, we proportionately allocate benefits starting in the quarter following the claim. For example, benefits in the quarter immediately following the initial claim are 0 if the claimant had any employment in that quarter and $WBA \cdot \min\{PBD, 13\}$ if they did not. More generally, counterfactual benefits k quarters following the initial claim quantity are $WBA \cdot \min\{\max\{PBD - 13k, 0\}, 13\}$.

We also account for the fact that eligibility approval does not map one-to-one to benefit receipt. To do so, we scale down benefits by the causal effect of eligibility approval on the probability of receiving any UI benefits. Intuitively, the total causal effect on UI benefit dollars we estimate includes the imperfect mapping between eligibility approval and benefit receipt; subtracting off the mechanical transfer to recover the behavioral cost requires us to do the same.

Our strategy for translating nonemployment durations to dollars in UI benefits received implies two sources of measurement error. Inferring non-employment duration based on the absence of any employment in the entire quarter will understate the degree of actual nonemployment, which generates a negative bias in the mechanical transfer (and thus a positive bias in the BCMC). Moreover, if there is a nonzero correlation between heterogeneity in the degree of nonemployment

[29]Such indirect effects might include added worker effects, search congestion effects (if a large number of unemployed workers in a given market receive a benefit increase, their reduced search activity may increase returns to search for others not directly affected by the reform), or job creation effects (if employers respond to changes in job-search activity by adjusting wages or job posting0.

and heterogeneity in effect of eligibility on nonemployment duration, then scaling by the average effect of eligibility on benefit receipt will introduce bias. A positive correlation generates an positive bias, while a negative correlation generates a negative bias.

An alternative approach that follows Lee et al. (2021) would instead treat the behavioral cost as the residual (difference between total and mechanical costs) and estimate the mechanical cost from a similar translation using untreated potential outcomes for compliers. We do not favor this approach because it produces an estimate of the UI benefit portion of the behavioral cost that is too small to be consistent with our IV estimate of approval on nonemployment duration. This is also conservative for our purposes, since we will show that the BCMC ratio is meaningfully smaller for the at-fault eligibility margin.

Benefit amount. To estimate the causal effects of an increase in benefit amounts we implement a regression kink design (RKD). WBAs are set as constantly increasing functions of prior earnings up to some maximum. This maximum creates a kink in the benefit schedule that we rely on for identification, as shown in panel (a) of Figure 8.[30]

We model our outcomes of interest as polynomial functions of the prior earnings measure which determines WBAs (the "running variable," HQW, see Section 2.4), allowing the slope of that relationship to change at the cutoff value where the maximum WBA is reached. The estimated slope-change is causally interpretable so long as any unobserved confounders are smooth through the cutoff, and claimants do not manipulate their HQW values around the cutoff. We refer the reader to Bell et al. (2022b) for evidence in support of these assumptions. To interpret these estimates as the causal effect of a \$1 increase in WBA, we scale these reduced form kinks by the *estimated* first stage kink in WBA at the cutoff (in order to account for potential noncompliance).

The total cost of a WBA increase is then easily estimated using the RKD via 2SLS, where the outcome is net government revenue, the endogenous treatment is the WBA, and the instrument is the change in the slope of WBA with respect to HQW at the cutoff.

The mechanical transfer estimate is derived as the outcome level for those at the HQW threshold, i.e., it is the total number of UI benefit weeks claimed among claimants at the kink. Intuitively, the mechanical transfer of a \$1 WBA increase is simply the number of times the claimant receives a benefit payment.

[30]This strategy follows Bell et al. (2022b), who estimate and decompose the heterogeneous impacts of WBA expansions over different time periods in the same dataset that we use, Lee et al. (2021) who implement a similar research design in data from Washington State, Card et al. (2015) who implement a similar research design in from Missouri, and Landais (2015) who implements a similar design in data from 5 US states in the 1970s and 80s.

For this margin the behavioral cost is treated as the difference between the total and mechanical costs, following Lee et al. (2021) who calculate the BCMC ratio using a similar design in similar data from Washington state. As for the at-fault eligibility margin, we could alternatively estimate the behavioral cost itself and subtract it from the total cost estimate to retrieve the mechanical cost. We note that our preferred approach is conservative for our purposes as it produces a smaller BCMC ratio estimate than the alternative.

Additional details on this research design are in Appendix C.2.

Benefit duration. To estimate the causal effects of an increase in benefit durations we implement a regression kink design (RKD). PBDs are set as constantly increasing functions of prior earnings up to some maximum. This maximum creates a kink in the benefit schedule that we rely on for identification, as shown in panel (b) of Figure 8.[31]

The total government cost is estimated exactly as in the WBA RKD with net government expenditures as the outcome, except with PBD as the endogenous treatment and $\frac{HQW}{BPW}$ as the running variable. The mechanical transfer estimate is the fraction of claimants at the kink exhausting benefits scaled by their WBA. In words, it is the additional week of benefits for claimants who otherwise remain unemployed through benefit expiry. Here again the behavioral cost is treated as the difference between the total and mechanical costs. For this expansion we cannot directly estimate the behavioral cost, since one of the outcomes (insured unemployment duration) that we would use for this purpose is censored by PBD which of course changes at the cutoff.

Additional detail on this design is in Appendix C.3, results supporting the identification assumptions are in Figure C1 (smoothness of density and covariates through the cutoff).

5.3 Results

Table 6 summarizes BCMC ratios for each type of UI benefit expansion. For the RKDs, binscatters of the relevant underlying outcomes as a function of the running variable are shown in Figure 9 and Figure 10. For at-fault eligibility, estimates are based on 2SLS specifications with a scalar (residualized, leave-out mean) instrument as described in Section 4 (Equation 5).[32] For all expansions, standard errors are estimated via stacked regressions, where we estimate the effects of the reform

[31] This strategy follows Landais (2015) who implements a similar design in data from 5 US states in the 1970s and 80s.

[32] We use the scalar Z approach here to simplify implementation of the stacked regressions. Analogous UJIVE estimates for individual components are in 4. These analogous UJIVE estimates are not identical to the scalar Z estimates used here, but differences are minor and conservative for purposes—leading to a larger BCMC estimate for at-fault eligibility than the UJIVE estimates would produce.

on all components of total and behavioral or mechanical costs jointly, cluster standard errors at the claim level, and use the delta method calculate standard errors on various functions of component estimates.

The last column displays our main result: the BCMC ratio for at-fault eligibility is much smaller than that of a benefit level or duration increases. Each \$1 mechanically transferred to the unemployed via an expansion in at-fault eligibility imposes an additional \$0.19 cost to the government's budget due to behavioral responses. Corresponding values are \$0.67 and \$0.96 for benefit amount and benefit duration increases, respectively.

What components of the behavioral cost drive these differences? Columns 1-4 show that these differences are driven by the moral hazard effect of more generous benefits on the length of the initial spell. BCMC ratios ignoring the other components of total cost would be 0.04 for at-fault eligibility, 0.3 for benefit amount, and 0.38 for benefit duration.[33]

5.4 Why are At-Fault Eligibility Expansions Less Socially Costly?

While our finding that fiscal externalities of at-fault eligibility expansions are substantially smaller than those of benefit amount or duration increases is important on its own, an understanding of *why* this is the case could be especially useful for policymakers. Both status quo UI policy parameters and the population of UI claimants differ substantially across states. Further, while we have estimated fiscal externalities for three especially important UI policy parameters, this is not an exhaustive list and policymakers may be interested in a different set of potential program expansions. Understanding what drives the lower fiscal externality for at-fault eligibility would help policymakers bridge the gap between our results and the specific context and choices that they face.

One candidate explanation is the combination of heterogeneity in responses to benefit generosity, and differences in the characteristics of claimants affected by these different policy changes. If claimants affected by one policy change differ meaningfully from those affected by another, and those differences are also correlated with heterogeneity in behavioral responses to benefit generosity, this could explain part of the difference in fiscal externalities. Notably, we have shown that meaningful heterogeneity exists for the nonemployment effects of at-fault eligibility in Section 4.

[33] Ignoring the other components implies total cost = column 2. The other components are also purely behavioral cost, so we can subtract them from column 6 to get an alternative BC value. The MC value for at-fault eligibility is then the difference between this alternative total cost and behavioral cost. The MC values for benefit amount and duration are unchanged.

We have also shown in Section 3.1 that claimants exposed to at-fault eligibility determinations are generally less-advantaged than the population of otherwise-eligible claimants. We can take this further by characterizing the compliers for all three of the policy changes that we study in Section 5.2. Table 5 displays sample and complier means for the three research designs described in Section 5.2. For the at-fault eligibility IV, complier means are estimated following Frandsen et al. (2023).[34] For the RKDs, complier means for some characteristic X are estimated as the constant in a reduced form regression of X on the centered running variable, and the interaction of the centered running variable with the cutoff dummy. Complier means are typically relatively similar to sample means within each design, but very different across designs.

We take two approaches towards investigating the role of these observable characteristics empirically. First, we use entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013) to reweight the two RKD samples so that the RKD sample means for the covariates in Table 5 match the at-fault IV complier means. We prefer entropy balancing to more commonly used inverse propensity score weighting approaches because it allows us to directly target moments of complier characteristics. (This is not possible with propensity score reweighting because compliers are not identifiable.) The primary flaw with reweighting in this context is that the three samples have very incomplete overlap in one key dimension—prior earnings. This is especially the case for the WBA RKD, since the cutoff is in terms of a prior earnings measure and therefore the LATE estimated by the RKD is necessarily local to that (relatively high) earnings amount. This motivates our second approach, which re-estimates BCMCs in subsamples that are meant to be more comparable across policy changes. For the at-fault eligibility expansion, we re-estimate the BCMC in subsamples defined by deciles of prior earnings. For the RKDs, we re-estimate the BCMCs among the subset of RKD sample claims that had separation-based eligibility determinations (all of which were deemed eligible, since ineligible claims are removed from the RKD samples, as described in Appendix C).

Figure 11 presents the results of these exercises. Panel (a) displays the baseline BCMC estimates for each margin along with (i) reweighted RKD BCMC estimates (gray diamonds for RKDs), and (ii) reweighted RKD BCMC estimates among claims with eligible separation-based eligibility determinations (blue squares for RKDs). Reweighting on its own explains very little of the original BCMC differences—8% of the \$0.48 gap between the baseline WBA and at-fault BCMC estimates, and -5% of the PBD and at-fault gap (reweighting widens the gap between PBD and at-fault).

[34] Frandsen et al. (2023) show that the complier mean of some X can be estimated via IV with $X * (1 - D)$ as the outcome, $(1 - D)$ as the endogenous treatment, and the same Z (here, office-issue dummies) used in the main design.

Re-weighting and restricting the sample leads explains a much more meaningful portion of the gap—64% of the at-fault vs. WBA gap, and 22% of the at-fault vs PBD gap.

As previously mentioned, the key flaw with the approaches in panel (a) is that cannot account for the drastically different prior earnings amounts between the three samples. Panel (b) addresses this issue by re-estimating the at-fault BCMC in subsamples defined by deciles of prior earnings (quarterly averages in the two years pre-claim). Results demonstrate that at-fault BCMC ratios are clearly increasing in prior earnings. This heterogeneity is substantial to fully explain the difference in baseline BCMCs between the (full sample) at-fault expansion and the WBA increase—the at-fault BCMC estimate for the decile of prior earnings which includes the WBA RKD cutoff prior earnings is slightly large than the baseline WBA BCMC, and the at-fault BCMCs for the lowest deciles of prior earnings are actually negative.

As shown in Table 6 prior earnings levels for the PBD sample are actually substantially *lower* than the at-fault sample, implying that this heterogeneity by prior earnings cannot explain the large gap between the PBD and at-fault (full sample) BCMC ratios. One potential explanation for this puzzle is that the mechanical transfer for a PBD increase is fundamentally different than that of a benefit amount or eligibility increase. As shown in Figure 7, the mechanical transfer of a PBD increase is $0 for all claimants that would not exhaust their benefits in the absence of the expansion. This could drive up the PBD BCMC ratio if claimants who would not have exhausted nonetheless respond to the PBD increase by lengthening their spells, which seems plausible.

The important role of pre-claim earnings levels in explaining the different BCMC ratios for at-fault eligibility and benefit amount expansions can help policymakers to understand whether our results are likely to extend to their setting. If prior income levels in the group of claimants they are considering extending eligibility is similar to prior income levels among our marginally at-fault claimants, then our results are more likely to hold in their setting. Our results also *suggest* that targeting *other* benefit expansions towards low-income claimants may be especially efficient. For example, the benefit amount policy change that we study is a $1 increase to the maximum benefit—which only affects relatively high earners—but there is no reason that policymakers cannot instead increase benefit levels for lower earning claimants only (e.g., via a replacement rate increase for a certain range of prior earnings). However, especially in light of our relatively high BCMC ratios among low-income claimants exposed to the PBD increase that we study, more work is needed to determine whether behavioral responses to other policy changes are smaller among lower-income claimants.

Our lower BCMC ratio estimates for benefit amount and duration increases among eligible claimants with separation-based eligibility determinations suggest that there may also be something specific to the experience of voluntary separation that dampens behavioral responses to UI. For example, the circumstances surrounding a voluntary quit or a firing for cause might be more likely to lead the claimant to look for a job that is meaningfully different than the one they lost. Similarly, quitting or being fired for cause may complicate the job search process if claimants are unable to secure good references from their prior employer. Finally, the separation-based eligibility process itself could lengthen unemployment spells.

5.5 Other Welfare Considerations

We have shown in Appendix C.1 that the BCMC ratio is a sufficient statistic for the efficiency cost of a UI benefit expansion and in Section 5.2 that the BCMC ratio for separation-based eligibility is lower than those for other UI policy margins. However, we cannot fully characterize the welfare implications of these different policy changes because we do not measure their social benefits. Further, while we believe that our BCMC ratio estimates are comprehensive relative to the existing literature, the nature of our identifying variation and data require us to make several simplifying assumptions in our measurement of efficiency costs. The following brief discussion argues that a more complete analysis not subject to these caveats would still very likely favor UI expansions via at-fault eligibility. A more complete discussion on this topic is in Appendix D.

There are several other ways in which an at-fault eligibility expansion could induce other costly behavioral responses by either directly or indirectly affected workers. Perhaps most importantly, this could occur if more lenient at-fault eligibility criteria induce more workers to voluntarily separate and/or file claims conditional on separating voluntarily. Earlier empirical evidence on this question is mixed (Ragan, 1984; Solon, 1984; Jäger et al., 2022; Venator, 2022). However, we stress that these concerns—and the existing empirical evidence—deal with larger non-marginal changes to at-fault eligibility, e.g., by shifting large categories of quits or firings from ineligible to eligible (such as the "trailing spouse" criteria studied by Venator (2022)). A policy change more closely tied to our identification strategy would instead extend eligibility to a relative small group of marginally at-fault job losers.

We believe that, in general, marginal changes to at-fault eligibility are unlikely to be salient to the vast majority of indirectly affected workers. This substantially limits the scope for such a change to lead to increases in UI inflows. Two leading examples of indirect inflow responses that do seem plausible are increases in future UI receipt among claimants directly exposed to more lenient rules on an earlier claim (an outcome included in our analysis and BCMC ratio calculations), and spillovers onto peers—e.g., family members, friends, or coworkers of the directly affected workers in our sample who become aware of the marginal change in leniency via word of mouth. We test this later hypothesis by constructing a sample of indirectly affected coworkers. We limit our main sample to single-establishment firm-quarters that have only one claim in our main sample (the "index" claim). The limitation to single-establishment firms ensures that the claimants work together in the same location (necessary because our earnings data includes only employer and not establishment IDs). The limitation to separating firm-quarters with single index claims simplifies the definition of the instrument and endogenous treatment, allowing us to rely on the same specification used in our main results (Equations 2 and 1. We then focus on all coworkers of the index claimant who were present at the separating firm in the quarter of the index claim and the quarter prior (the "peers"). Finally, we implement our office-issue design for various measures of peer utilization.

Figure 12 presents results from this exercise and shows no evidence of an increase in peer UI claiming behavior or payment receipt. This result holds when we look at all types of UI claims, and when we look specifically at claims with separation-based eligibility issues. This exercise is limited by our relative crude definition of peers (e.g., we cannot identify family members, or narrow down the set of coworkers to those that know each other well), but we view these results as suggestive evidence that any inflow effects of extending UI eligibility marginally at-fault claimants is small.

As discussed for example, in Appendix C.1, the social benefits of an increase in UI benefit generosity are represented by the utility gain from improved consumption smoothing. We believe that these consumption smoothing benefits are likely to be larger for at-fault eligibility expansions than the other margins we study for two reasons. First, diminishing marginal utility implies that the first dollar of insurance provides the greatest consumption smoothing value, so that an expansion on the extensive margin has a higher social benefit. Second, we have shown in **??** that marginally at-fault claimants have lower incomes than other UI claimants. Appendix D discusses two potential concerns with these arguments—that at-fault claimants may have very small consumption drops at unemployment, and that society may attach lower welfare weights to the consumption of (even marginally) at-fault job losers. Notably, we show that unemployed workers in the Panel Study of Income Dynamics (PSID) who quit their last job experience meaningful consumption drops at unemployment.

One unique aspect of at-fault eligibility relative to other UI policy parameters is that its implementation is costly. The government must fund the technological infrastructure and staff time necessary to conduct initial interviews and appeals. A marginal change at-fault eligibility criteria would not influence administrative costs for initial interviews, but it would avoid claimant appeals, which are common and expensive to process. In Appendix D we use state-year aggregate data on appeals and appeals processing costs to provide a back-of-the-envelop estimate of administrative cost savings via avoided appeals from shifting one marginally at-fault claimant to eligibility. We arrive at an estimate of just under $50 in administrative cost savings. While small, this is a meaningful portion of the behavioral costs for the at-fault eligibility expansion shown in Table 6.

6 Conclusion

Whether a UI claim is initially approved or denied on separation-based eligibility grounds greatly matters for subsequent UI benefit receipt but much less so for subsequent employment. We provide the first evidence of these causal effects using a design leveraging variation in eligibility approval rates across offices to which claimants are randomly assigned. Our individual-level variation using data on the universe of UI claims in California for almost two decades provides significant power, and we use this to show that any decreases in employment due to separation-based eligibility are the smallest for low-income claimants.

We show these employment responses are particularly small in the context of a theoretically-motivated measure of the efficiency costs of benefit expansions. This holds both relative to estimates from the existing UI literature and relative to replications of those research designs within our own data.

Does this mean UI agencies should relax their separation-based eligibility criteria, or at least reallocate benefit generosity? Our theoretical framework highlights other considerations we do not measure, and we highlight the role future research can play in filling these gaps.

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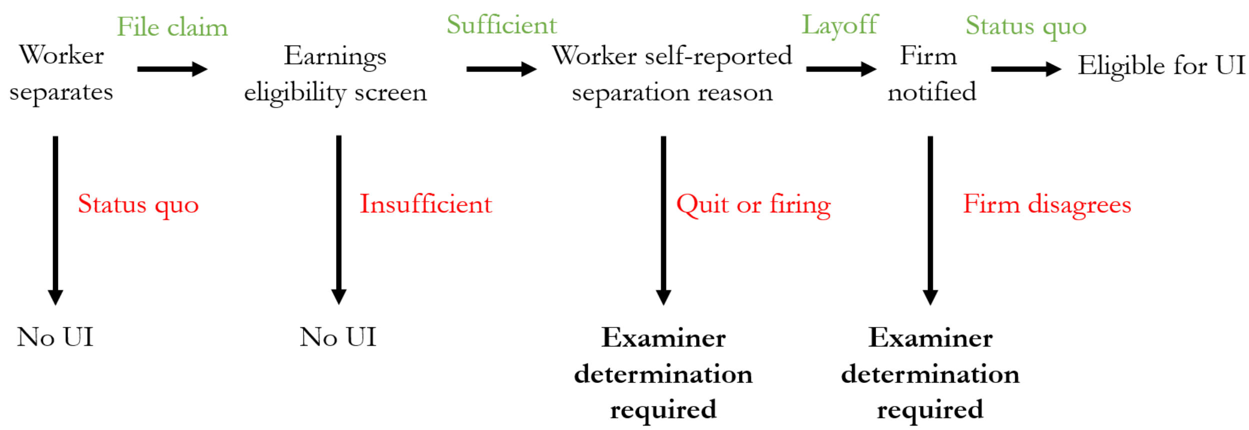
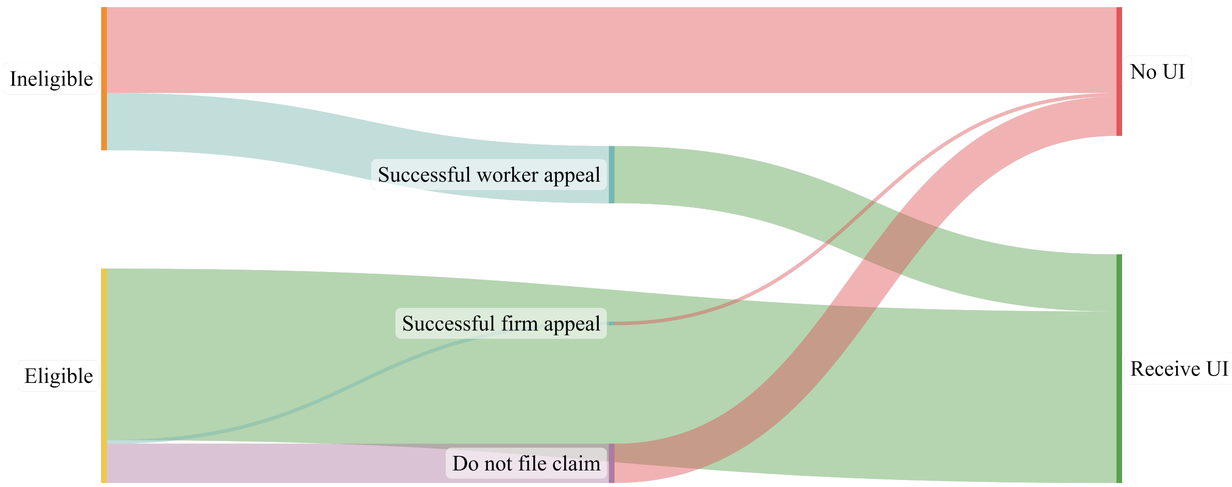
Figures and Tables

Figure 1: Selection Into Separation-Based Eligibility Determinations



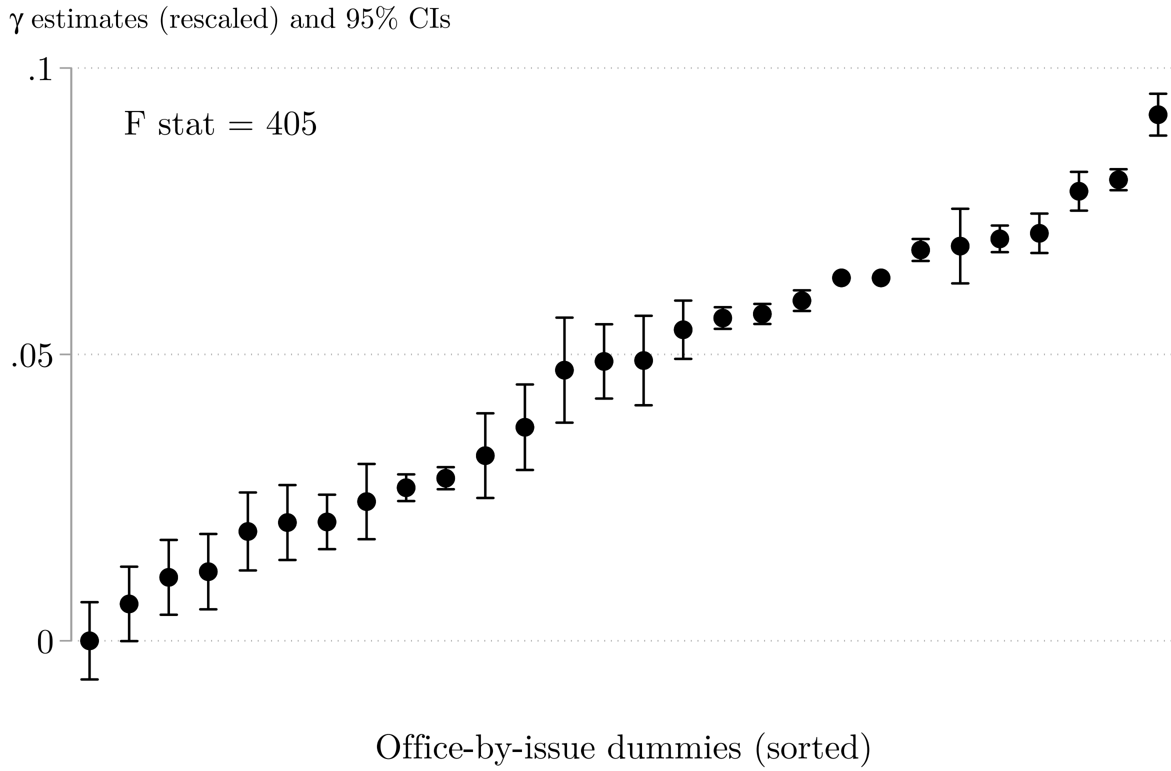
Notes: This chronological flowchart describes how unemployed workers can end up in our separation-based eligibility sample. Arrows in blue indicate steps leading to UI benefit receipt, while arrows in red indicate steps that put UI benefit receipt in jeopardy.

Figure 2: Reasons for UI Benefit (Non-)Receipt by Eligibility Approval Status



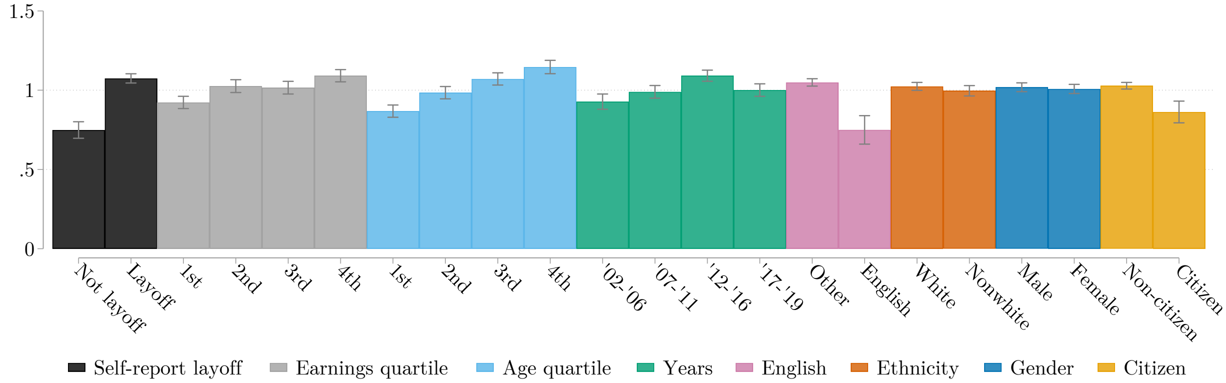
Notes: This Sankey diagram describes the mapping between separation-based eligibility and UI payment receipt in our sample of separation-based eligibility issues from 2002 to 2019. Bar thickness is proportional to the relevant percentage of claims. The rates of successful worker and firm appeals come from aggregate statistics in DOL ETA 5130 Benefit Appeals Report. All other quantities come from our EDD microdata.

Figure 3: Variation in Eligibility Approval Propensities Across Office-by-Issue Pairs



Notes: This figure plots office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval. Coefficients are sorted along the x-axis in ascending order and normalized so that the smallest fixed effect is 0. Standard errors are cluster robust at the claimant level. The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019.

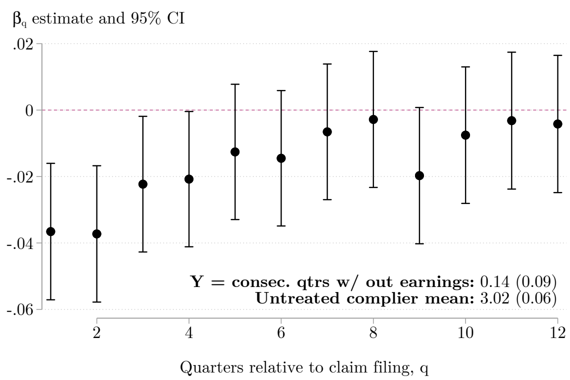
Figure 4: Consistency of Office-by-Issue Leniency Across Claimant Subgroups



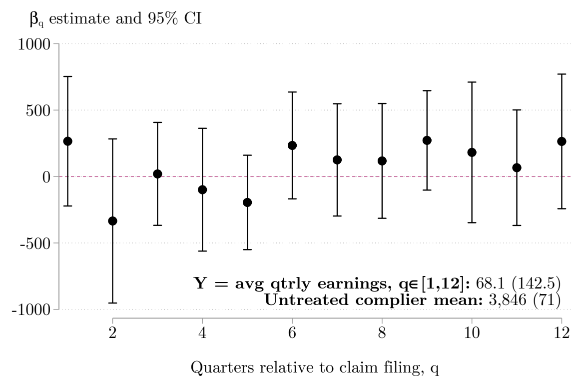
Notes: Each bar represents a separate regression of the claimant's own eligibility decision D_{it} on their assigned office-by-issue's overall leave-one-out residualized eligibility leniency $\tilde{Z}^j_{it}$ within a given subsample. Each color represents a different categorical variable, and separate bars refer to separate levels of that categorical variable. Lower quartiles correspond to lower levels of the variable. Robust standard errors are clustered by claimant and error bars provide 95% confidence intervals.

Figure 5: Dynamic Effects of At-Fault Eligibility on Employment, Earnings, and Benefit Receipt

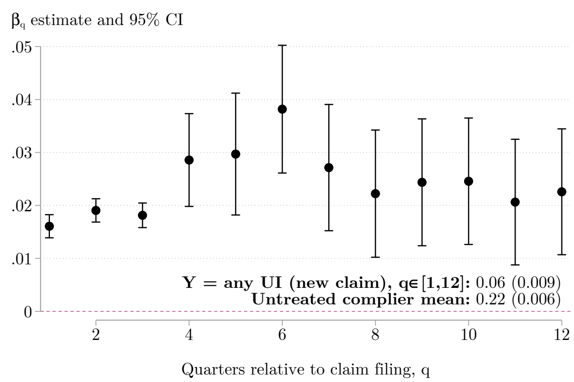
(a) Employment (> \$0 earnings)



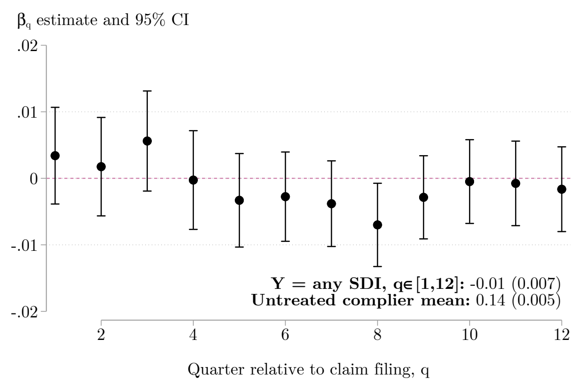
(b) Total Earnings



(c) Future UI Receipt (> \$0 on new claim)

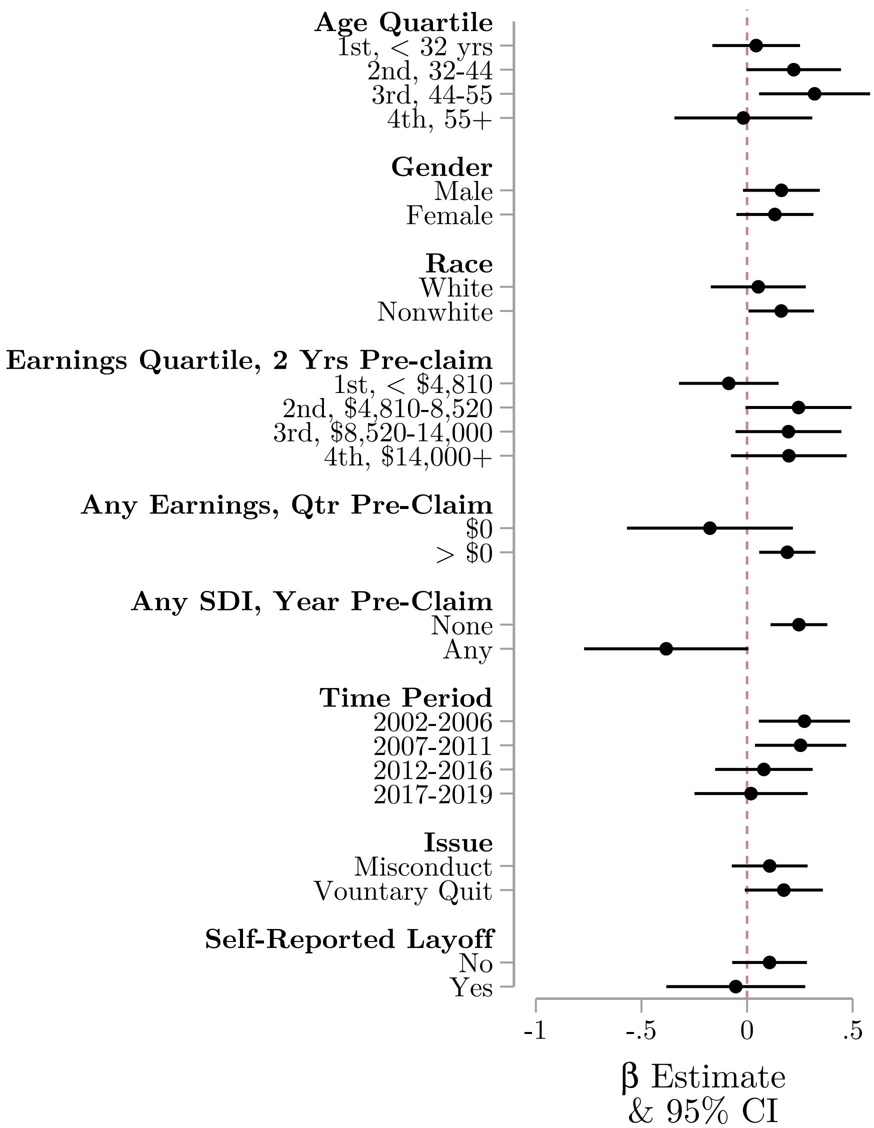


(d) SDI Receipt (> \$0)



Notes: $\widehat{\beta_q}$ estimates from separate quarterly level IV specifications where office-by-issue dummies instrument for eligibility (see Equations 1 and 2). The q subscript denotes the quarter in which the outcome is measured. Estimation is via UJIVE and standard errors are cluster-robust at the claimant level. For reference, each panel also includes estimates for corresponding outcomes that are pooled across quarters, and their untreated complier means estimated following Frandsen et al. (2023). The outcomes in panel (a) are indicators for any employment in each quarter and the total duration of the nonemployment spell. The outcomes in panel (b) are total earnings by quarter, and avg. quarterly earnings across the 12 quarters. The outcomes in panel (c) are indicators for any *future* UI receipt (on a new claim) by quarter, and an indicator for future UI receipt in any of the 12 quarters. The outcomes in panel (d) are indicators for any SDI receipt by quarter, and an indicator for SDI receipt in any of the 12 quarters. Each sample includes all regular initial claims with separation-based eligibility issues between 2002 to 2019.

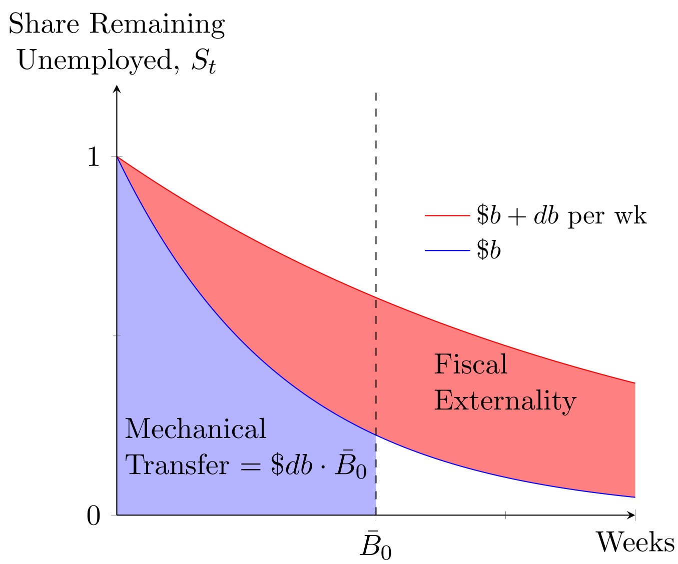
Figure 6: Heterogeneity in Nonemployment Duration Effects of At-Fault Eligibility



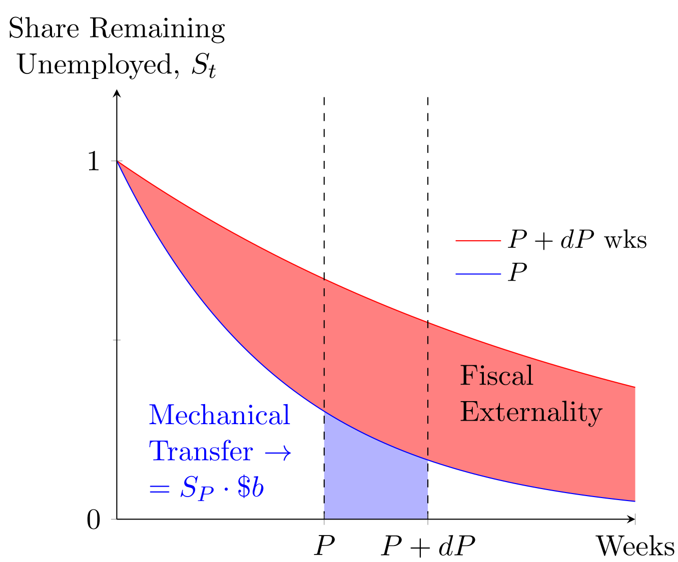
Notes: $\widehat{\beta}$ estimates from separate quarterly level IV specifications where office-by-issue dummies instrument for eligibility (see Equations 1 and 2). Each estimate is from a subset of our main analysis sample. Estimation is via UJIVE and standard errors are cluster-robust at the claimant level.

Figure 7: Graphical Illustration of BCMC Ratio for Different UI Program Expansions

(a) Eligibility (initial \$$b = 0$) or Benefit Level (initial \$$b > 0$) Expansion

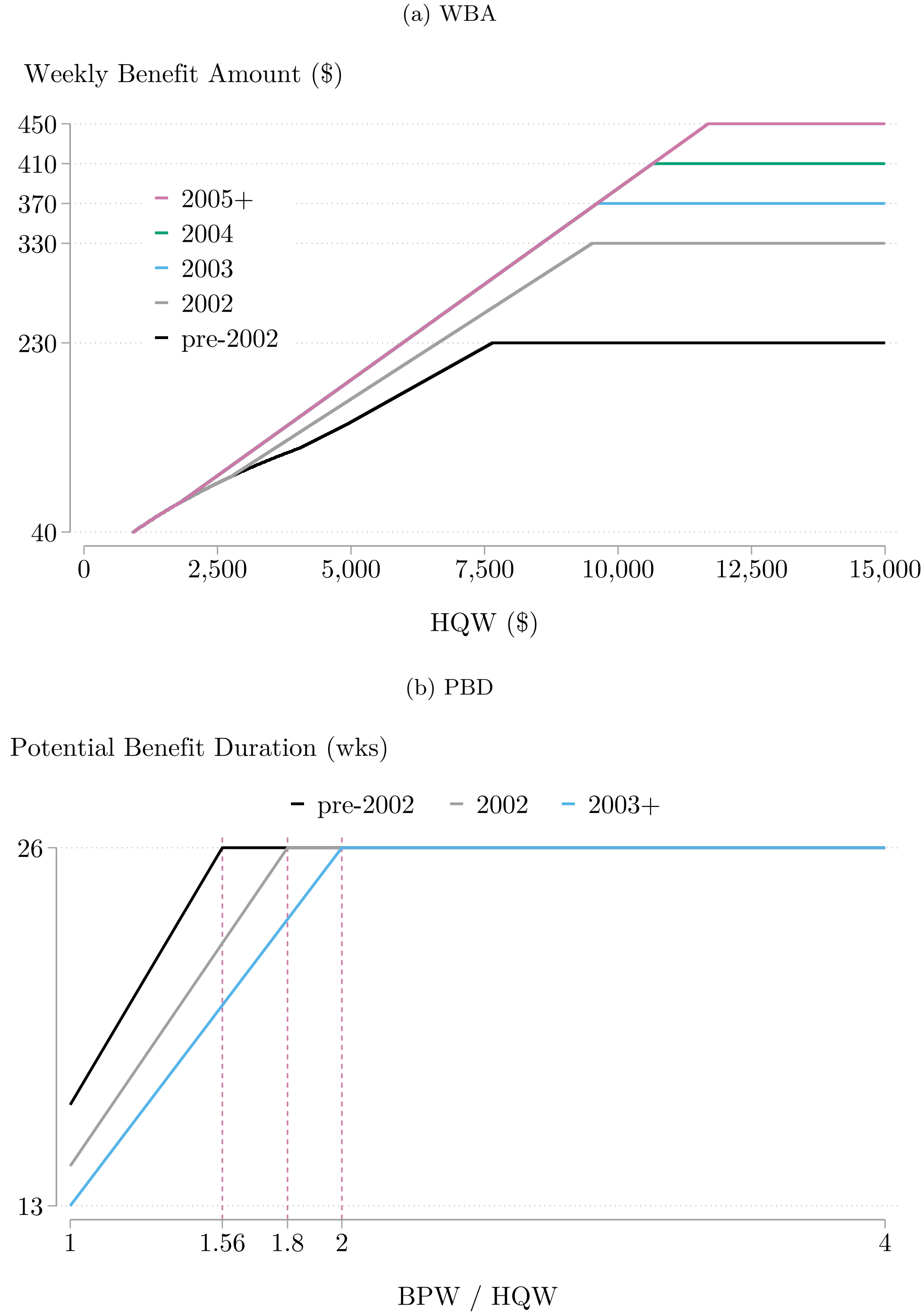


(b) Benefit Duration Expansion



Notes: The panels illustrate calculations of behavioral and mechanical costs to the government of different types of UI benefit expansions. Each panel displays hypothetical survivor curves (S_t) with and without the benefit expansion for a homogeneous population, where the vertical distance between survivor curves represents an increase in nonemployment duration in response to benefit expansions. Under the status quo policy, the weekly benefit amount is b, the potential benefit duration is P, and the expected insured unemployment duration is $\bar{B}_0$. The mechanical cost is the portion of benefit expansions claimants received if nonemployment duration were held fixed. The behavioral cost is the sum of additional benefits and foregone tax revenues due to the increase in nonemployment duration.

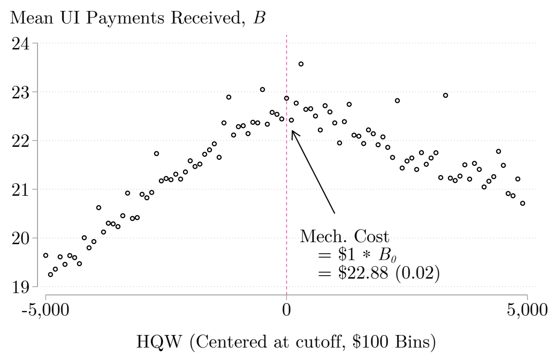
Figure 8: WBA and PBD Benefit Schedules



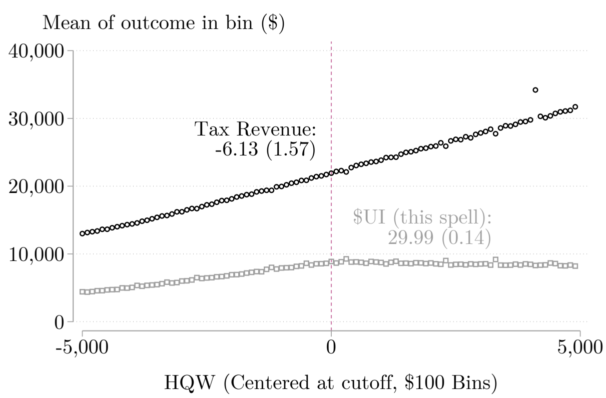
Notes: The top panel (a) displays the functions which determine weekly benefit amounts (WBA) in California during the years covered by our sample. In each time period, WBAs are set to replace some portion of earnings in the highest earning quarter of a claimant's base period ("High Quarter Wages" or HQW) up to a maximum. Claimants whose HQW values imply WBA < $40 are monetarily ineligible. The bottom panel (b) displays similar information for potential benefit duration (PBD). PBDs are set as increasing functions of BPW/HQW where BPW is total earnings in the base period. PBDs are always capped at 26 weeks, and BPW/HQW is ≥ 1 by definition.

Figure 9: WBA RKD BCMC Components

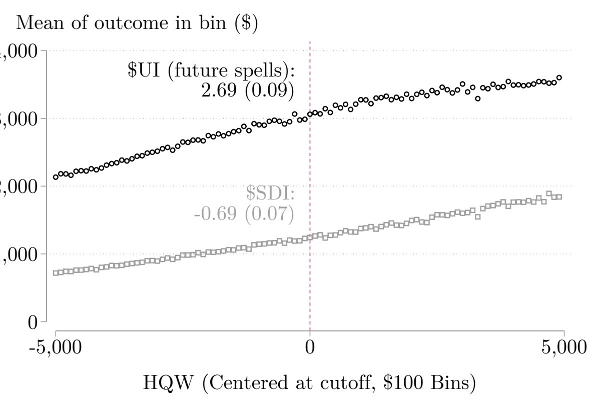
(a) Mechanical cost



(b) Tax Rev. and Current Spell UI



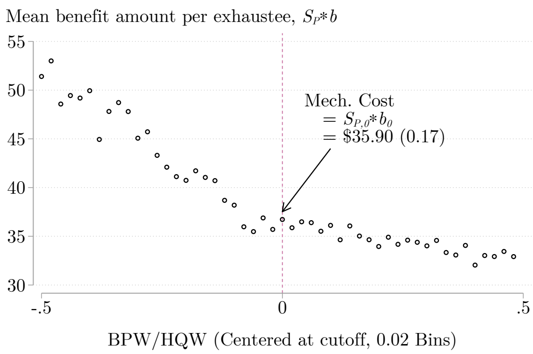
(c) SDI and Future Spell UI



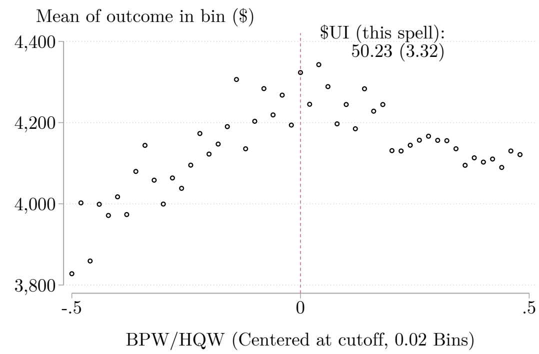
Notes: Each panel is a binned scatterplot of the weekly benefit amount analysis sample described in Table 5 and Appendix C.2. The running variable is high-quarter wages relative to the year-specific kink and the bin width is \$100. The outcome for panel (a) is the number of UI payments received, which is used to calculate the mechanical cost of a WBA increase. The outcomes for panels (b) and (c) are the components of net government expenditures used to calculate the total cost of a WBA increase, as described in Section 5.2.

Figure 10: PBD RKD BCMC Components

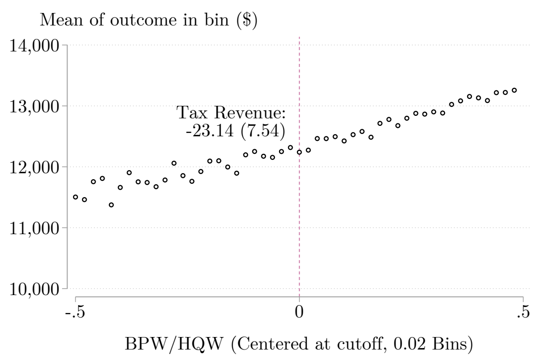
(a) Mechanical cost



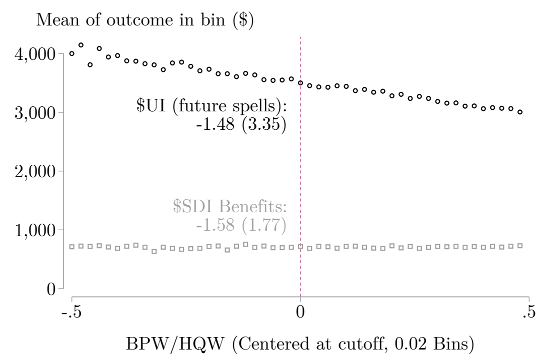
(b) Current Spell UI



(c) Tax Revenue



(d) SDI and Future Spell UI

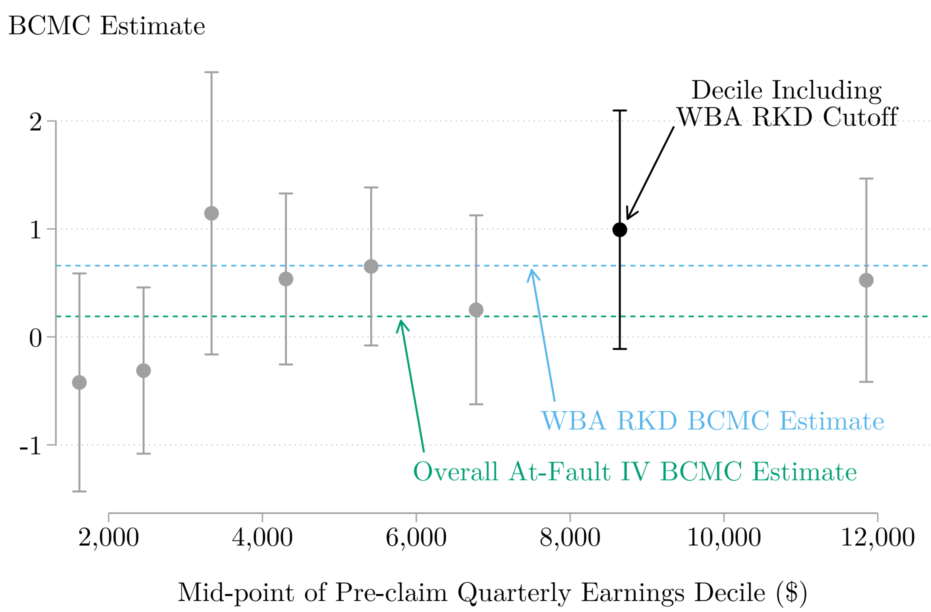


Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in Table 5 and Appendix C.3. The running variable is the ratio of base period wages to high-quarter wages relative to the year-specific kink and the bin width is 0.02. The outcome for panel (a) is weekly benefits received per exhaustee, which is used to calculate the mechanical cost of a PBD increase. The outcomes for panels (b), (c), and (d) are the components of net government expenditures used to calculate the total cost of a WBA increase, as described in Section 5.2.

Figure 11: Explaining BCMC Differences Across Margins

(a) Reweighting and/or limiting RKD samples

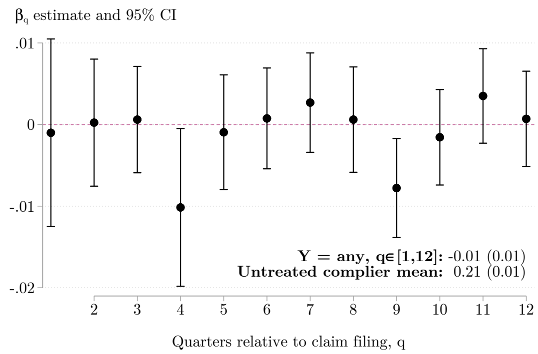
(b) At-Fault IV BCMC estimates by pre-claim earnings decile



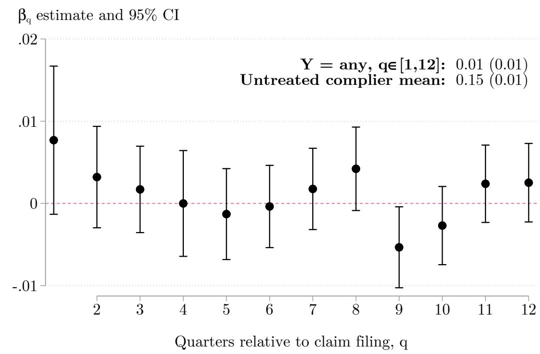
Notes: The top panel displays our baseline BCMC estimates for each expansion in type of program expansion (black circles), reweighted WBA and PBD BCMC estimates (where entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013) is used to reweight the RKD samples so that means match the complier means for the at-fault IV shown in Table 5), and similarly reweighted WBA and PBD BCMC estimates among eligible claims with separation-based eligibility determinations. The bottom panel displays at-fault IV BCMC estimates for subsamples of claims defined by deciles of pre-claim earnings. The decile which includes WBA RKD cutoff value is highlighted, and the baseline full sample) BCMC estimates for at-fault eligibility and WBA expansions are shown as horizontal lines for reference. The first and last deciles are excluded from panel (b) because their estimates are substantially less precise.

Figure 12: Dynamic Effects of Eligibility on Peer Claim Filing and Benefit Receipt

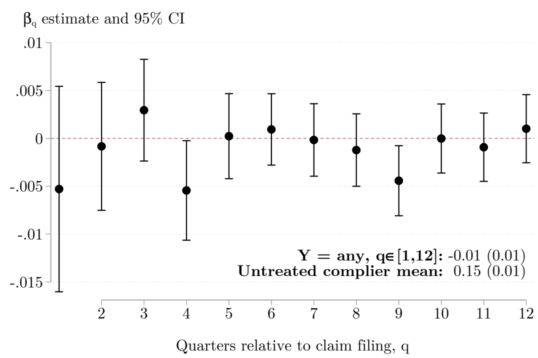
(a) Claims per Peer



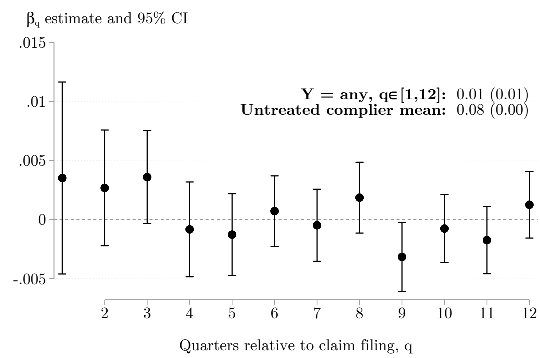
(b) Paid Claims per Peer



(c) Claims Following Quit/Firing per Peer



(d) Paid Claims Following Quit/Firing per Peer



Notes: $\widehat{\beta_q}$ estimates from separate quarterly level IV specifications where office-by-issue dummies instrument for eligibility (see Equations 1 and 2). The q subscript denotes the quarter in which the outcome is measured. Estimation is via UJIVE and standard errors are cluster-robust at the claimant level. Samples are limited to the 1.7m single-establishment "index" firm-quarters with a single claim in our main analysis sample. The index claim is filed in quarter zero, and the outcomes are measures of claim-filing behavior of peers in later quarters. Peers are all coworkers present at the separating firm in both the quarter of the index claim and the quarter prior. Outcomes are scaled by the number of peers. For reference, each panel also includes estimates for corresponding outcomes that are pooled across quarters, and their untreated complier means estimated following Frandsen et al. (2023). The outcomes in panel (a) are indicators for any employment in each quarter and the total duration of the nonemployment spell. The outcomes in panel (b) are total earnings by quarter, and avg. quarterly earnings across the 12 quarters. The outcomes in panel (c) are indicators for any *future* UI receipt (on a new claim) by quarter, and an indicator for future UI receipt in any of the 12 quarters. The outcomes in panel (d) are indicators for any SDI receipt by quarter, and an indicator for SDI receipt in any of the 12 quarters.

Table 1: UI Receipt and Ineligibility Among the Unemployed

	(1)	(2)	(3)	(4)	(5)
	All Claims	Monetarily Eligible	+ Separation Issue	+ Initial Denial	+ No Payments
N (millions)	26.8	22.9	6.9	2.9	2.1
Share of previous column		86%	30%	42%	74%
Avg. prior quarterly earnings ($)	6,960	7,881	6,912	5,615	5,241
Weekly benefit amount ($)		288	274	251	239
Age	39	40	36	34	33
Female	0.45	0.45	0.49	0.50	0.51
Nonwhite	0.65	0.65	0.63	0.67	0.67
English-speaking	0.88	0.87	0.94	0.94	0.94
Any SDI, yr before UI claim	0.08	0.09	0.14	0.12	0.11
Claimant reports layoff	0.72	0.73	0.23	0.21	0.11
Share misconduct (vs. quit)			0.62	0.44	0.42
UE duration (qtrs)	2.2	1.9	2.4	2.3	2.4

Notes: Column 1 includes all regular UI initial claims filed in California from 2002 to 2019. Each subsequent column adds an additional restriction to those applied in the previous columns. Column 2 restricts to initial claims satisfying the minimum earnings eligibility threshold. Column 3 further restricts to initial claims with any investigation of a separation-based eligibility issue. Column 4 restricts to separation-based eligibility issues whose initial determination is a denial. Column 5 restricts to separation-based eligibility denials that have no evidence of a successful appeal in the form of claimed benefit payments.

Table 2: Payment Timing Implications of Eligibility Investigations and Appeals

	(1)	(2)	(3)
	No eligibility issues	Separation issue but approved	Separation denial but paid
Any payments	0.77	0.83	1.00
Median days to 1^{st} payment	16	21	93
Median days to 1^{st} covered week	10	10	12
N (millions)	16.0	4.0	0.8

Notes: Columns represent mutually exclusive groups of regular UI initial claims filed in California from 2002 to 2019. Column 1 includes only claims without an initial eligibility issue. Column 2 includes only claims with separation-based eligibility issues that were initially approved. Column 3 includes only paid claims with separation-based eligibility issues that were initially deemed disqualifying, which is evidence of a successful claimant appeal.

Table 3: Validating Instrumental Variables Assumptions in the Office Research Design

	Relevance		Independence				Exclusion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Eligibility	Any payments	Prior avg. qtrly. earn.	Age	Female	Nonwhite	Days to det.	Other issue if paid
Range	0.09	0.05	176	0.14	0.01	0.01	3.51	0.03
Mean	0.59	0.59	6,911	36	0.49	0.63	28.94	0.17
Joint F-statistic	405	77	1	1	1	1	35	24
Joint F p-value	0.00	0.00	0.30	0.21	0.11	0.42	0.00	0.00

Notes: Statistics in each column come from first-stage coefficients γ in Equation 2 using all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. For each variable listed at the top of the column, the table reports the range of first-stage coefficients when that variable is the outcome, the overall sample mean, and the joint F-statistic and corresponding p-value when testing all first-stage coefficients. Columns 1 and 2 test the first-stage relevance assumption using endogenous UI treatments, Columns 3 through 6 test the independence assumption using claimant demographics, and Columns 7 and 8 test the exclusion restriction using other claim processing outcomes.

Table 4: Effects of Initial Eligibility Approval

	UI Benefits (this spell)			Non-employment Duration	
	(1)	(2)	(3)	(4)	(5)
	Any payments	Payments (weeks)	Payments ($)	Any earnings 1 qtr. after	Consecutive qtrs. w/o earnings
IV	0.32	10.3	2,547	-0.04	0.14
SE	(0.01)	(0.43)	(146)	(0.01)	(0.09)
OLS	0.55	16.9	4,935	-0.06	0.30
SE	(0.00)	(0.02)	(5.37)	(0.00)	(0.00)
$\bar{Y}^0$	0.31	7.85	2,324	0.49	3.02
	Earnings	Future UI Receipt		SDI	
	(6)	(7)	(8)	(9)	(10)
	Avg. $ in qtrly. earnings (w/ 0's)	Any Payments	Payments ($)	Any Payments	Payments ($)
IV	68.1	0.06	697	-0.01	-18.3
SE	(143)	(0.01)	(102)	(0.01)	(97.7)
OLS	1,074	0.03	354	0.02	299
SE	(4.95)	(0.00)	(3.87)	(0.00)	(3.71)
$\bar{Y}^0$	3,846	0.22	1,632	0.14	1,052
F	405				
Unique N	6.9m				

Notes: The instrumental variables estimate is β in Equation 1 where the endogenous treatment D is initial eligibility approval. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. $\bar{Y}^0$ is the untreated complier mean estimated following Frandsen et al. (2023). All robust standard errors are at the 95% confidence level and are clustered by claimant.

Table 5: Sample and Complier Characteristics by Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	At-Fault IV		Benefit Level RKD		Benefit Duration RKD	
	Sample	Compliers	Sample	Compliers	Sample	Compliers
Female	0.49	0.51	0.43	0.40	0.50	0.49
	(0.50)	(0.01)	(0.49)	(0.00)	(0.50)	(0.00)
Nonwhite	0.63	0.65	0.64	0.61	0.70	0.70
	(0.48)	(0.01)	(0.48)	(0.00)	(0.46)	(0.00)
Age	35.6	34.3	39.4	39.7	35.1	35.1
	(12.5)	(0.17)	(12.4)	(0.01)	(13.5)	(0.03)
Citizen	0.92	0.92	0.87	0.88	0.85	0.85
	(0.28)	(0.00)	(0.33)	(0.00)	(0.36)	(0.00)
Any Earn, qtr pre-claim	0.87	0.86	0.95	0.95	0.89	0.88
	(0.33)	(0.01)	(0.22)	(0.00)	(0.31)	(0.00)
Any SDI, qtr pre-claim	0.07	0.07	0.04	0.04	0.04	0.04
	(0.25)	(0.00)	(0.21)	(0.00)	(0.20)	(0.00)
Avg Earn, 2 yrs pre-claim	$6,914	$6,145	$7,969	$8,489	$3,293	$3,078
	($12,216)	($92.8)	($4,015)	($1.55)	($1,909)	($3.39)
N:	6.8m		8.5m		1.2m	
Running variable:			HQW		BPW/HQW	
Bandwidth:			[$4,504.45, $16,674.01)		[1.06, 2.5)	

Notes: Complier means for separation IV are untreated complier means estimated following Frandsen et al. (2023). Complier means for RKDs are For the RKDs, complier means for some characterstic X are estimated as the constant in a reduced form regression of X on the centered running variable, and the interaction of the centered running variable with the cutoff dummy. HQW = high quarter wages, the highest quarterly earnings amount in the claimant's base period. BPW = total earnings in the base period.

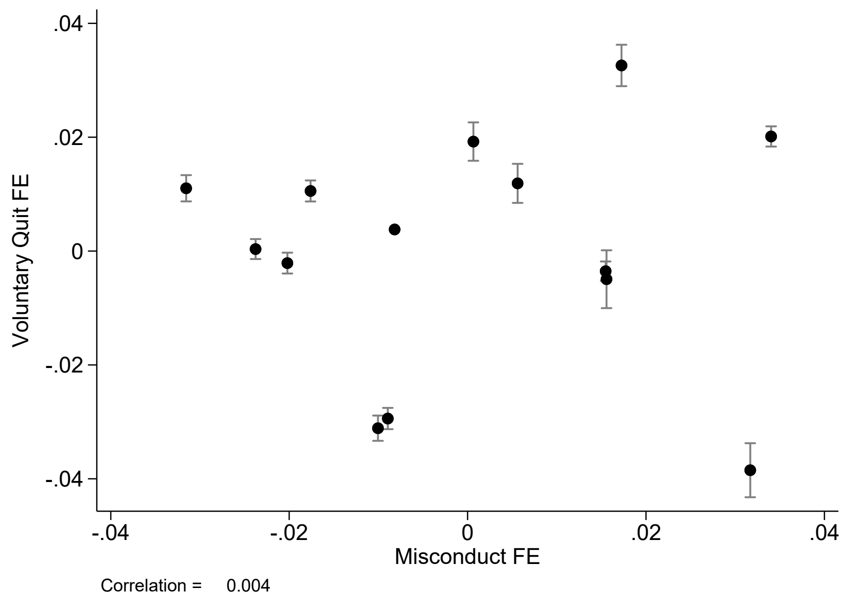
Table 6: Decomposition of Benefit Expansion Costs for Different UI Policy Margins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Components of Total Cost				Total cost	BC	MC	$\frac{\text{BC}}{\text{MC}}$
	Tax	UI (this spell)	SDI	Future UI				
Separation-based eligibility	-328	2417	-1	658	2747	431	2316	.19
	(527)	(142)	(95)	(100)	(564)	(569)	(136)	(.25)
Benefit amount (+ $1)	6	30	-1	3	38	15	23	.67
	(2)	(< 1)	(< 1)	(< 1)	(2)	(2)	(< 1)	(.07)
Benefit duration (+ 1 week)	23	50	-2	-1	70	34	36	.96
	(8)	(3)	(2)	(3)	(9)	(9)	(< 1)	(.25)

Notes: Each row demonstrates various effects of a different UI expansion on the government's budget, estimated within our California data. The coefficients in Columns 1 through 4 are the individual components of net government transfers. The sum of these columns is the total effect of the expansion on net government transfers (total cost), which is in Column 5. The behavioral cost is the sum of Columns 1, 3, 4 and the portion of column 2 that results from longer nonemployment (at-fault expansion) or unemployment (other expansions) durations. The mechanical cost is the increase in UI benefits paid out for the current spell in the absence of any behavioral response, shown in Column 7. The BCMC ratio in Column 8 is Column 6 divided by Column 7. Further details on sample construction and estimation are in Appendix C and Section 5.2.

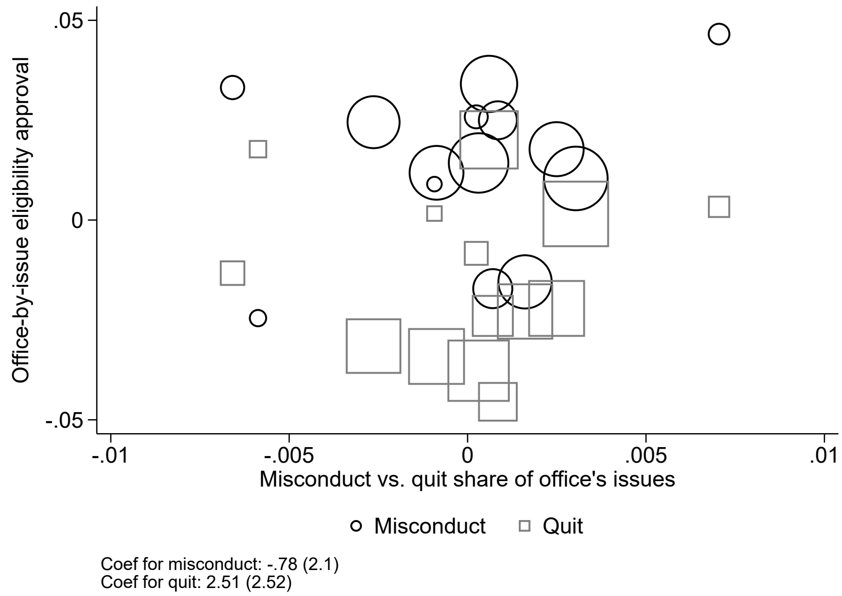
A Additional Tables and Figures for Office Assignment Research Design

Figure A1: Office-Level Leniency is Uncorrelated Across Issue Type



Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is a processing office. The coordinates are office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval; coefficients are normalized so that the across-office average fixed effect for each issue type is 0. The figure's regression coefficient and robust standard error are from a weighted OLS regression of the discharge fixed effects on quit fixed effects at the office level, where each observation is weighted by the total number of separation-based eligibility issues adjudicated by that office.

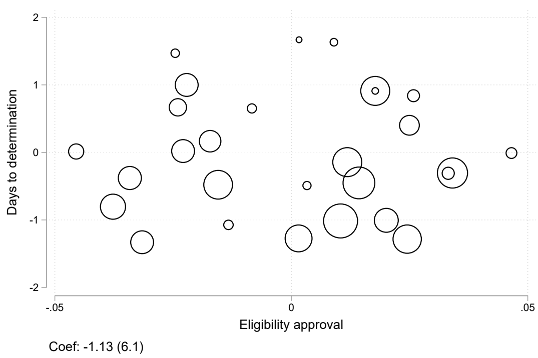
Figure A2: Minimal Variation in Office-level Issue Type Share is Uncorrelated with Issue-specific Approval Rates



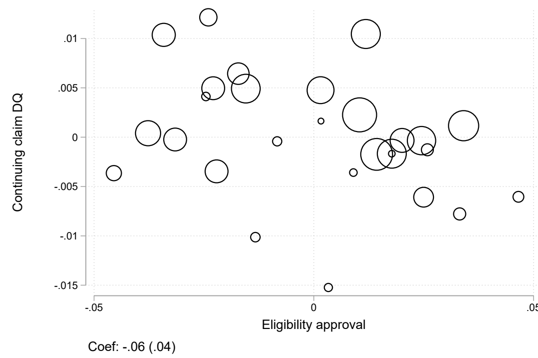
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is a office-by-issue pair. The y-coordinates are office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval; coefficients are normalized so that the across-office average fixed effect for each issue type is 0. The x-coordinates are analogous coefficients on the overall offices estimated from a form of Equation 2 where the outcome D is the misconduct share at the office and $\mathbf{Z}$ omits the issue interactions. The figure's regression coefficient and robust standard error are from a weighted OLS regression of the office-by-issue eligibility fixed effects on the office issue type fixed effects, where each observation is weighted by the total number of separation-based eligibility issues adjudicated by that office.

Figure A3: Other Office-level Processing Differences are Unrelated to Eligibility Approval Propensity

(a) Initial Claim Adjudication

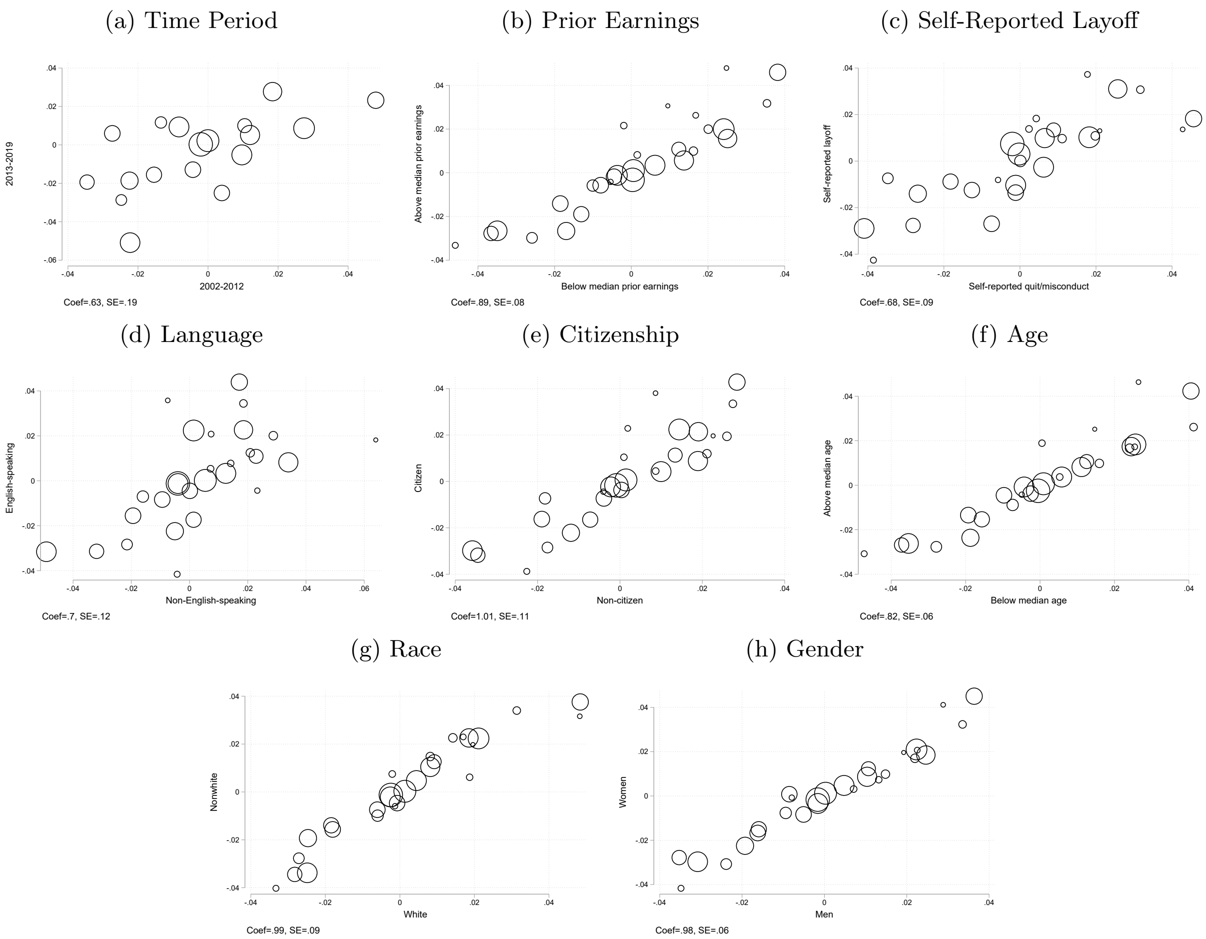


(b) Continuing Claim Adjudication



Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates. The x-axis indexes office-by-issue coefficients in γ estimated in Equation 2 where the outcome D is eligibility approval; coefficients are normalized so that each within-issue across-office average fixed effect is 0. The y-axis in the left panel is the number of days between the claim filing date and the recorded eligibility determination date. The outcome in the right panel is an indicator for any disqualification related to continuing claims. The figure's regression coefficient and robust standard error come from a weighted OLS regression of the outcome fixed effects on eligibility approval fixed effects at the office-by-issue level, where each observation is weighted by the number of separation-based eligibility issues in that office during the sample period.

Figure A4: Consistency of Approval Rates Across Demographics and Within Office-by-Issue

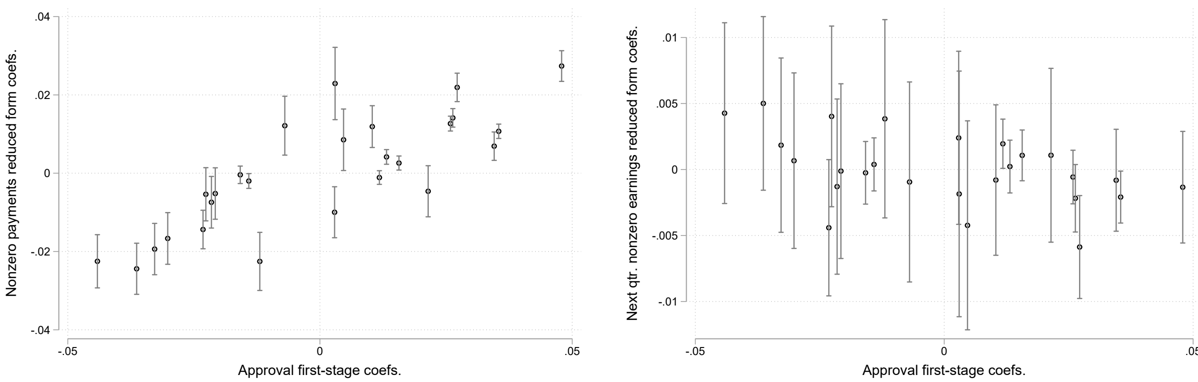


Notes: Each panel displays group-specific separation-based eligibility approval propensities at the office-by-issue level. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates and the coordinates are eligibility approval rates for groups of claimants labeled on the axes. The coefficient is from a regression of the approval rate for the y-axis group on the approval rate for the x-axis group at the office-by-issue level weighted by the number of eligibility issues in the office-by-issue pair. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019.

Figure A5: Visual IV Representation of First-Stage and Reduced-Form Effects

(a) Any payments

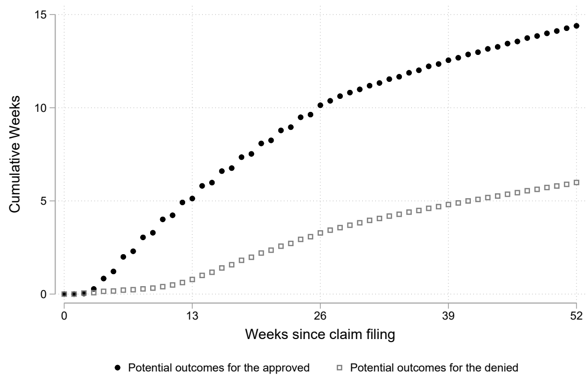
(b) Any earnings 1 qtr. after



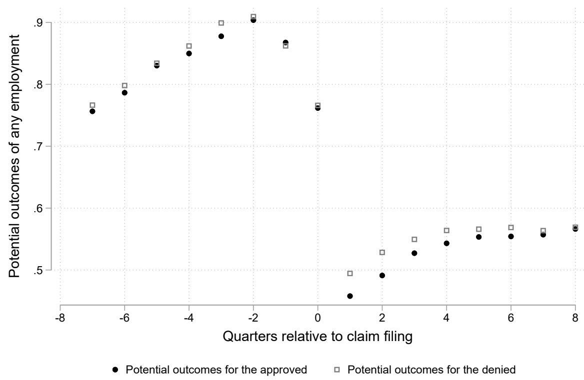
Notes: Each panel presents reduced-form coefficients for office-by-issue pairs on the y-axis and first-stage coefficients for office-by-issue pairs on the x-axis. Both sets of coefficients come from Equation 2, where the outcome is eligibility approval for the first-form and the variable in the graph title for the reduced-form. The sample is of all regular initial claims with separation-based eligibility issues between 2002 to 2019.

Figure A6: Dynamic Potential Outcomes by Treatment Status

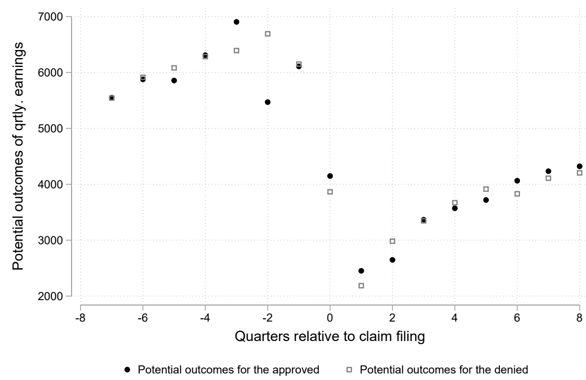
(a) Paid Week



(b) Any Earnings



(c) Avg. Earnings Incl. 0's

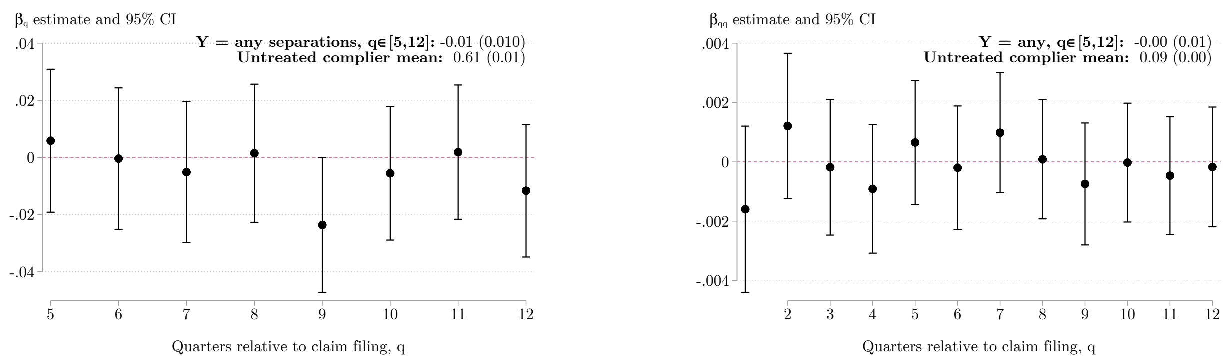


Notes: Each panel decomposes treatment effects of at-fault eligibility into complier potential outcomes for approved and denied claimants. Treated potential outcomes are estimated by interacting Y with an indicator for eligibility *approval* D in Equation 1. Untreated potential outcomes are estimated by interacting Y with an indicator for eligibility *denial* $(1 - D)$, replacing the indicator for eligibility approval D with an indicator for eligibility denial $1 - D$, and estimating the system. Panels (b) and (c) include 95% confidence intervals while Panel (a) omits them. The sample is of all regular initial claims with separation-based eligibility issues between 2002 to 2019.

Figure A7: Dynamic Effects of Eligibility on Future Separations and Migration

(a) Separate from Main Employer

(b) Move to Non-CA Zip Code



Notes: $\widehat{\beta_q}$ estimates from separate quarterly level IV specifications where office-by-issue dummies instrument for eligibility (see Equations 1 and 2). The q subscript denotes the quarter in which the outcome is measured. Estimation is via UJIVE and standard errors are cluster-robust at the claimant level. For reference, each panel also includes estimates for corresponding outcomes that are pooled across quarters, and their untreated complier means estimated following Frandsen et al. (2023). In panel (a), the sample is limited to claimants from the main sample who are employed in the relevant quarter (3.99m-4.06m in relevant quarters) and the outcome is an indicator for whether or not the claimant separates from their highest paying employer in this quarter (separation = zero earnings from this employer in the following quarter). In panel (b), N = 3.9m claimants from main sample who are included in the Infutor data (allowing us to observe the zip code of their current address) and the outcome is an indicator for whether or not the claimant's zip code switches from a CA to a non-CA address in the following quarter.

Table A1: Robustness of Main Results to a Fully Saturated Specification

	(1)	(2)	(3)	(4)
	Any Payments	Avg. $ in qtrly. earnings (w/ 0's)	Any earnings 1 qtr. after	Consecutive qtrs w/o earnings
Baseline IV	0.32	17	-0.04	0.14
SE	(0.01)	(142)	(0.01)	(0.07)
Saturate and Weight IV	0.37	-14	-0.05	0.05
Untreated complier mean	0.26	3,530	0.49	2.37
First-stage F	405			
Unique N	5.5m			

Notes: The first two rows and the last three rows replicate results in Table 4. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The baseline IV results are estimated from Equations 1 and 2 using UJIVE where the endogenous treatment D is initial eligibility approval. These include fixed effects for month and issue type. The third row labeled "Saturate and Weight" separately estimates the office assignment IV design within the 480 month-by-issue cells. Each specification, therefore, does not include controls. We aggregate these estimates proportional to the sample size within each month-by-issue cell.

Table A2: Heterogeneous Effects of Initial Eligibility Approval on Nonemployment Duration

	Age Quartile				Gender		Race	
	1^{st}	2^{nd}	3^{rd}	4^{th}	Male	Female	White	Nonwhite
IV	0.04	0.22	0.32	-0.02	0.16	0.13	0.05	0.16
SE	(0.11)	(0.11)	(0.13)	(0.17)	(0.09)	(0.09)	(0.11)	(0.08)
tf SE	[0.11]	[0.11]	[0.13]	[0.17]	[0.09]	[0.09]	[0.11]	[0.08]
F	117	121	102	86	196	214	151	261
N	1.51m	1.86m	1.81m	1.73m	3.49m	3.41m	2.54m	4.38m
	Prior Earnings Quartile				Employed Qtr. Before Claim		SDI $\rightarrow$ UI	
	1^{st}	2^{nd}	3^{rd}	4^{th}	No	Yes	No	Yes
IV	-0.09	0.24	0.20	0.20	-0.18	0.19	0.25	-0.38
SE	(0.12)	(0.13)	(0.13)	(0.14)	(0.20)	(0.07)	(0.07)	(0.20)
tf SE	[0.12]	[0.13]	[0.13]	[0.14]	[0.22]	[0.07]	[0.07]	[0.22]
F	127	100	101	103	50	362	371	51
N	1.73m	1.73m	1.73m	1.73m	0.88m	6.04m	6.08m	0.83m
	Time Period				Issue		Self-Reported Layoff	
	2002-6	2007-11	2012-16	2017-19	Misconduct	Quit	No	Yes
IV	0.27	0.25	0.08	0.02	0.11	0.17	0.11	-0.05
SE	(0.11)	(0.11)	(0.12)	(0.14)	(0.09)	(0.09)	(0.09)	(0.17)
tf SE	[0.11]	[0.11]	[0.12]	[0.14]	[0.09]	[0.09]	[0.09]	[0.19]
F	241	244	118	92	415	385	231	48
N	1.77m	2.17m	1.87m	1.10m	4.28m	2.64m	3.25m	0.99m

Notes: The overall sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The IV results are estimated from Equations 1 and 2 using UJIVE where the endogenous treatment D is initial eligibility approval and the outcome Y is consecutive quarters without earnings following the initial claim. Each column represents a separate model estimated on a given subsample. Earnings quartiles are constructed in the entire sample based on the average quarterly earnings in 7 quarters prior to the claim. Lower quartiles correspond to lower levels of the variable. The 1^{st} time period is 2002-2006, the 2^{nd} is 2007-2011, the 3^{rd} is 2012-2016, and the 4^{th} is 2017-2019. The tF adjustment affects subsamples with an F-statistic below 104.7 and uses a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

Table A3: Heterogeneous Effects of Initial Eligibility Approval on UI Receipt

	Age Quartile				Gender		Race	
	1st	2nd	3rd	4th	Male	Female	White	Nonwhite
IV	0.33	0.37	0.35	0.26	0.32	0.33	0.32	0.33
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
tf SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.01]
F	112	121	102	86	196	214	151	261
N	1.51m	1.86m	1.81m	1.73m	3.50m	3.42m	2.55m	4.39m
	Prior Earnings Quartile				Employed Qtr. Before Claim		SDI $\rightarrow$ UI	
	1st	2nd	3rd	4th	No	Yes	No	Yes
IV	0.28	0.33	0.36	0.39	0.18	0.35	0.34	0.27
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)
tf SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.01]	[0.01]	[0.03]
F	127	100	101	103	50	362	371	51
N	1.73m	1.73m	1.73m	1.73m	0.88m	6.06m	6.11m	0.83m
	Time Period				Issue		Self-Reported Layoff	
	2002-6	2007-11	2012-16	2017-19	Misconduct	Quit	No	Yes
IV	0.41	0.30	0.34	0.43	0.33	0.32	0.40	-0.03
SE	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
tf SE	[0.01]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.03]
F	241	243	118	92	417	383	231	48
N	1.77m	2.17m	1.87m	1.10m	4.29m	2.65m	3.25m	0.99m

Notes: The overall sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. The IV results are estimated from Equations 1 and 2 using UJIVE where the endogenous treatment D is initial eligibility approval and the outcome Y is an indicator for receipt of any UI payments following the initial claim. Each column represents a separate model estimated on a given subsample. Earnings quartiles are constructed in the entire sample based on the average quarterly earnings in 7 quarters prior to the claim. Lower quartiles correspond to lower levels of the variable. The 1st time period is 2002-2006, the 2nd is 2007-2011, the 3rd is 2012-2016, and the 4th is 2017-2019. The tF adjustment affects subsamples with an F-statistic below 104.7 and uses a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

B Separation-Based Eligibility Effects using Examiner Assignment

This section supplements our primary instrumental variables research design based on processing office assignment during 2002-2019 with a complementary instrument variables research design based on examiner assignment during 2017-2019. Its structure of it mirrors that of Section 4. The primary advantage of the design based on examiner assignment is that the more granular source of variation addresses potential monotonicity and excludability concerns. The primary disadvantage is that data availability limits the time period, which complicates making comparisons across policies, weakens the first-stage relationship, and precludes heterogeneity analyses.

B.1 Estimating Equation

Consider the following system of equations for claimant i speaking language l who files an initial claim in month t handled by an examiner in office o:

$$Y_{it(los)} = \beta D_{it} + \mathbf{X}'_{it(los)}\psi + e_{it} \tag{B.1}$$

$$D_{it(los)} = \mathbf{Z}'_{it}\gamma + \mathbf{X}'_{it(los)}\mu + \varepsilon_{it} \tag{B.2}$$

where $Y_{it(los)}$ is the endogenous outcome of interest; D_{it} is the endogenous UI treatment of interest; $\mathbf{Z}_{it}$ is a vector of indicator variables corresponding to a full interaction between the assigned examiner and separation-based issue type; and $\mathbf{X}_{it(los)}$ is a vector of control variables (i.e., fully-interacted dummies for language, assigned office, separation-based issue type, and claim filing month). The equation is overidentified because the excluded instrument $\mathbf{Z}_{it}$ of assigned examiners is a vector.

The sample used to estimate Equations B.2 and B.1 consists of only algorithmically scheduled claims between 2017 to 2019 in order to isolate the quasi-random assignment of examiners. Following the existing literature, we mitigate weak instruments concerns by limiting to examiners who handled a sufficient number of claims (Bhuller et al., 2020). We choose 200 as the threshold, as it retains approximately 90% of the sample.

Including language, office, issue, and time fixed effects is motivated by the assignment mechanism discussed in Section 2.3. First, while language fixed effects are most important to our identification strategy, they are unlikely to be quantitatively important. In particular, even though language certainly affects both a claimant's examiner assignment probabilities and plausibly affects subsequent employment outcomes, Table 1 shows that 94% of the sample speaks English. Second, while the assigned office is quasi-randomly assigned based on SSN, we include assigned office fixed effects to strengthen the exclusion restriction. This controls for any potential effect the office can have on claimants apart from the eligibility decision. Third, issue type fixed effects are necessary due to the instrument including an interaction with issue type. Finally, month-of-claim fixed effects are included due to changing macroeconomic conditions. California's unemployment rate gradually fell from 5% to 4% between 2017 and 2019, but claimants later on in the sample period faced the severe Covid-19 recession several quarters after their claim. If the pool of examiners remained fixed during this time period, then increased precision would be the sole benefit of these time fixed effects. However, due to some examiner hiring and attrition, these also address a potential confound due to changes in examiner composition over time.

B.2 Validating the Instrument

Like the research design based on examiner assignment, the identification assumptions to interpret β in Equation 1 as a partial equilibrium LATE are independence, excludability, first-stage relevance, and monotonicity.

A useful auxiliary object for testing these assumptions is the predicted first-stage from Equation B.2. To calculate it, we manually implement the residualization and leave-one-out procedure. Specifically, let $A_{it(jlos)}$ be an indicator eligibility approval of claimant i's claim filed in month t that is assigned to examiner j and residualize this by the fully-interacted language l, office o, issue type s, time t fixed effects in B.2:

$$A^{*}_{it(jlos)} = A_{it(jlos)} - \mathbf{X}'_{it(los)}\mu - \eta_t \tag{B.3}$$

We then calculate the scalar leave-one-out mean of this residualized leniency measure at the examiner-level:

$$\tilde{Z}^{js}_{it} = \left(\frac{1}{n_{js}-1}\right)\left(\sum_{k(js)}\sum_{t(js)} A^{*}_{kt(jlos)} - A^{*}_{it(jlos)}\right) \tag{B.4}$$

where n_{js} denotes the total number of algorithmically scheduled separation-based eligibility issues of type s handled by examiner j.

The first identification assumption—independence—requires that examiners be independent of potential outcomes, and it is conceptually supported by the quasi-random assignment of examiners among examiners assigned algorithmically. Table B1 empirically supports. Specifically, we calculate the residualized leave-one-out approval rate $\tilde{Z}_{it}^{j}$ from Equation B.4 among the sample of algorithmically scheduled claims and split claims by those with above vs. below median values of $\tilde{Z}_{it}^{j}$. Reassuringly, these two groups of claimants have strikingly similar pre-existing characteristics. The only statistically significant difference across the two groups—self-reported layoff and eligibility issue type—are economically small in magnitude.

Table B1: Claimant Balance Across Examiner Leniency

	(1)	(2)	(3)	(4)
	Ad hoc scheduling	Algorithmic scheduling		
		Above-median leniency	Below-median leniency	p-value of diff.
Age	37.5	37.5	37.5	0.60
Nonwhite	0.696	0.675	0.673	0.39
Prior earnings	8,371	8,913	8,921	0.85
Days to decision	27.1	19.4	19.4	0.61
Self-reported layoff	0.270	0.115	0.110	0.00
Share misconduct	0.609	0.715	0.707	0.00
Initially eligible	0.601	0.663	0.574	0.00
Any payments	0.574	0.605	0.553	0.00
N	658,020	222,579	222,579	

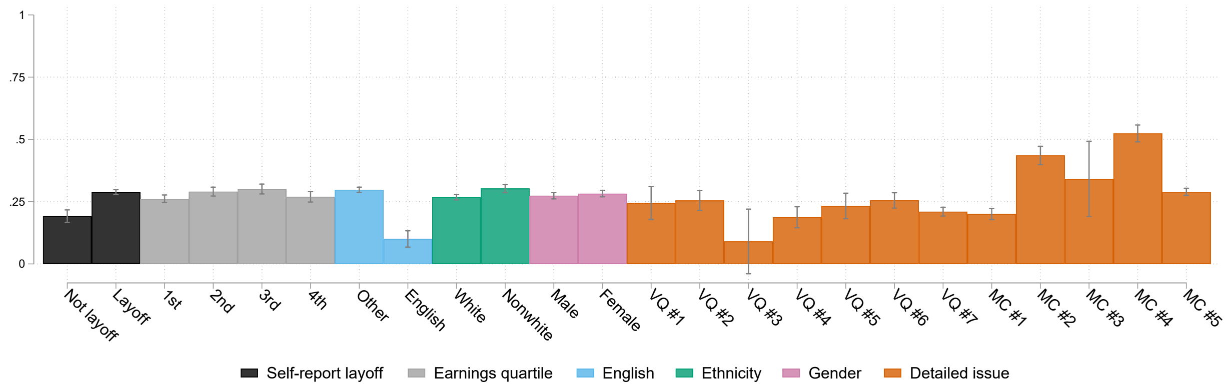
Notes: The overall sample consists of all regular UI initial claimants between 2017 and 2019 with a separation-based eligibility issue. The sample of algorithmically scheduled claims limits to examiners who handled at least 200 such claims. Column 1 reports claimant demographics at the time of claim filing and claim outcomes. Column 2 reports averages for claimants assigned ad hoc to examiners, which is not the analysis sample of interest. Columns 3 and 4 report averages of claimants assigned to examiners with above and below-median leniency, respectively, as measured by the leave-one-out residualized eligibility approval rate. Column 5 reports p-values calculated from separate regressions of each row variable on an indicator for being assigned to an examiner with above median leniency. These regressions also include language-by-assigned office fixed effects and standard errors are clustered by claimant.

The second identification assumption—excludability—requires that the only effect that examiners have on the endogenous outcome Y is through the endogenous treatment D. This is the chief benefit of the design based on examiner assignment. The design based on examiner assigned includes office fixed effects, so it leverages only *within-office* variation in eligibility approval propensities. In other words, in addition to the case for exclusion in the office-based design, the examiners-based design account for any other administrative effects of the office. The only role of the examiner is to make the eligibility determination. They do not handle other administrative duties related to the claim.

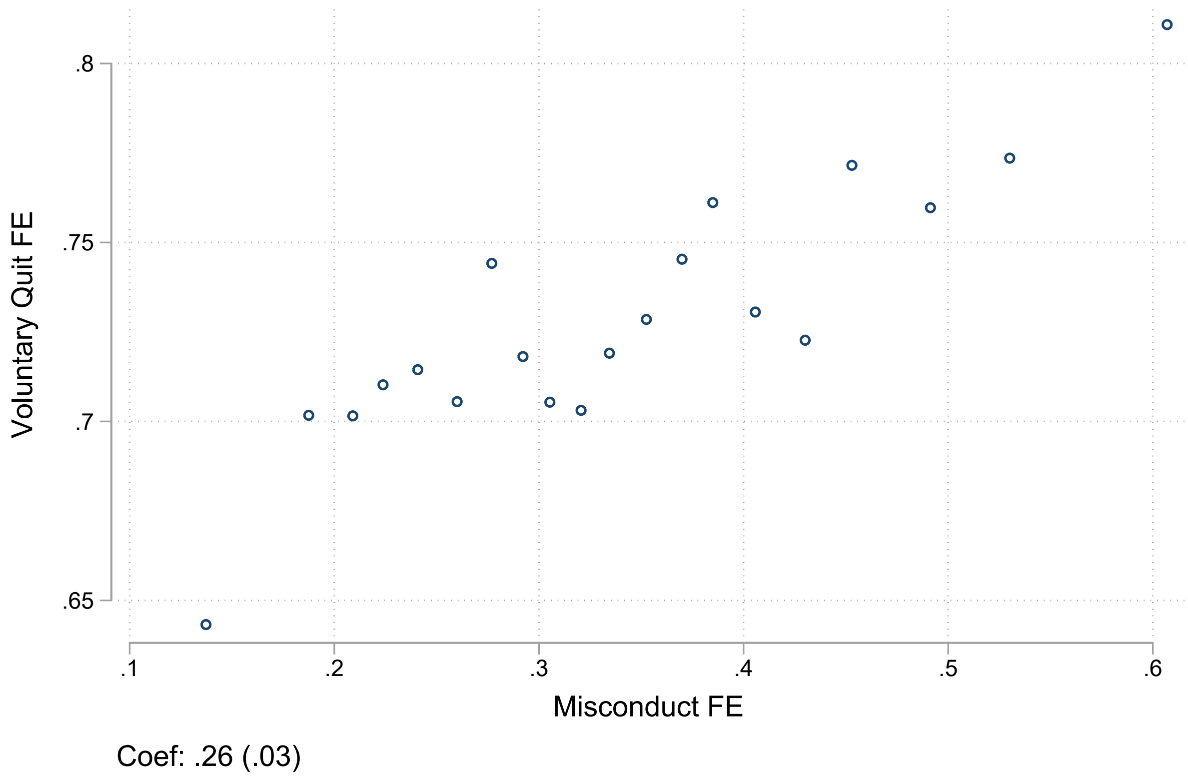
The third identification assumption—first-stage relevance—requires that the examiner assignment be predictive of the endogenous treatment. We directly test this by estimating the first-stage regression equation Equation B.2 and testing the joint significance of the examiner dummies. The first-stage F-statistic of 8 for the binary endogenous treatment of initial eligibility approval is just conventional thresholds for instrument relevance (Stock and Yogo, 2005). To ensure proper 95% coverage, we employ the tF confidence interval adjustment suggested by Lee et al. (2022). Given our first-stage F-statistics of 8 for the endogenous treatment of eligibility approval (any UI payment receipt), this inflates the second-stage confidence interval by 94%.

The final identification assumption—monotonicity—requires that an examiner-by-issue pair that is relatively more lenient with one type of claimant cannot be relatively less lenient with another type of claimant. Following the test implemented in Section 4, Figure B1a confirms the first-stage relationship is positive within various claimant subsamples, which is a testable implication of average monotonicity.

Figure B1: Consistency of Examiner Leniency Measures



(a) Positive First-Stage within Claimant Subsamples



(b) Positive Correlation between Issue-Specific Leniency

Notes: In the top panel, each bar represents a separate regression of the claimant's own eligibility decision D_{it} on their assigned examiner's overall leave-one-out residualized eligibility leniency $\tilde{Z}_{it}^{j}$ within a given subsample. Each color represents a different categorical variable, and separate bars refer to separate levels of that categorical variable. Lower quartiles correspond to lower levels of the variable. Detailed issue types refer to subcategories within misconduct (MC) and voluntary quit (VQ). Robust standard errors are clustered by claimant and error bars provide 95% confidence intervals. The bottom panel is a binned scatterplot of the average quit-specific examiner-level leniency by ventiles of misconduct-specific examiner-level leniency. Each ventile contains approximately 50 unique examiners.

B.3 Results

As in the office-based research design, the examiners-based research design finds that meaningfully increases benefit receipt. The decrease in any employment the quarter following the claim is larger by approximately 3 percentage points, and there is also a statistically significant decrease in earnings. Table B2 shows the treatment effect on receiving any payments, having any employment in the subsequent quarter, and average earnings in the subsequent quarter. Due to the weaker first-stage relationship, the tF-adjustment is now nontrivial; this almost doubles the confidence intervals. Even so, the eligibility effects are highly statistically significant.

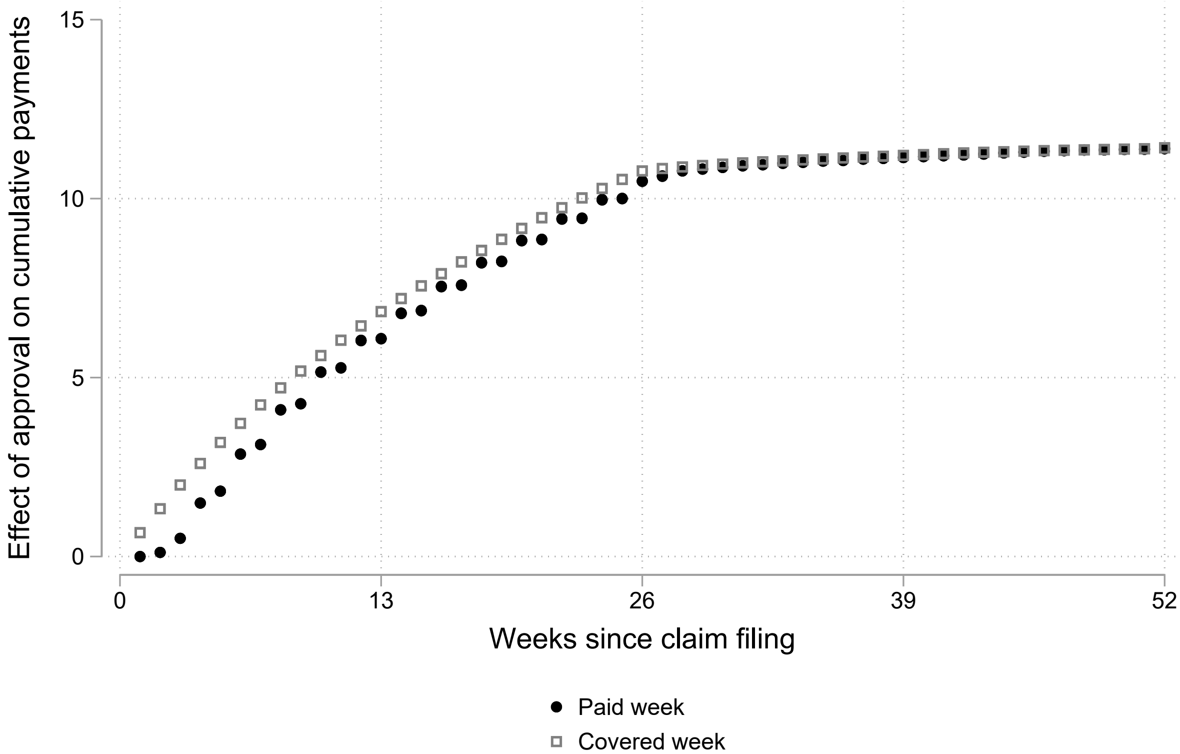
Table B2: Effects of Initial Eligibility Approval

	UI Benefits (this spell)			Non-employment Duration	
	(1)	(2)	(3)	(4)	(5)
	Any payments	Payments (weeks)	Payments ($)	Any earnings 1 qtr. after	Consecutive qtrs. w/o earnings
IV	0.56	12.7	3,807	-0.08	0.29
SE	(0.01)	(0.25)	(90.2)	(0.01)	(0.08)
tf SE	[0.01]	[0.50]	[184.3]	[0.02]	[0.16]
OLS	0.60	13.4	4,424	-0.06	0.26
SE	(0.00)	(0.04)	(14.3)	(0.00)	(0.01)
$\bar{Y}^0$	0.26	4.87	1,551	0.56	2.51
	Earnings	Future UI Receipt		SDI	
	(6)	(7)	(8)	(9)	(10)
	Avg. $ in qtrly. earnings (w/ 0's)	Any Payments	Payments ($)	Any Payments	Payments ($)
IV	-332	0.02	3.15	-0.01	44.5
SE	(166)	(0.01)	(120)	(0.01)	(107)
tf SE	[340]	[0.02]	[245]	[0.01]	[218]
OLS	2,031	0.00	151	0.01	321
SE	(26.9)	(0.00)	(21.0)	(0.00)	(18.3)
$\bar{Y}^0$	5,408	0.45	4,060	0.14	1,301
F	8				
Unique N	445k				

Notes: The instrumental variables estimate is β in Equation B.1. The sample includes all separation-based eligibility issues from regular initial claims between 2017 and 2019 that were assigned to examiners through the scheduled queue. The tF adjustment uses our first-stage F of 8.4 and conservatively derives the inflation factor of 1.944 using a linear interpolation between Table 3A values in Lee et al. (2022). $\bar{Y}^0$ is the untreated complier mean estimated following Frandsen et al. (2023). All robust standard errors are at the 95% confidence level and are clustered by claimant.

The dynamic effects of payment receipt mirror those in the office-based design, as Figure B2 replicates Figure A4 using examiner assignment as eligibility approval variation. The cumulative effect of eligibility approval on payment receipt is just over 10 weeks of benefit payments.

Figure B2: Dynamic Impacts of Eligibility Approval on Cumulative Benefit Receipt

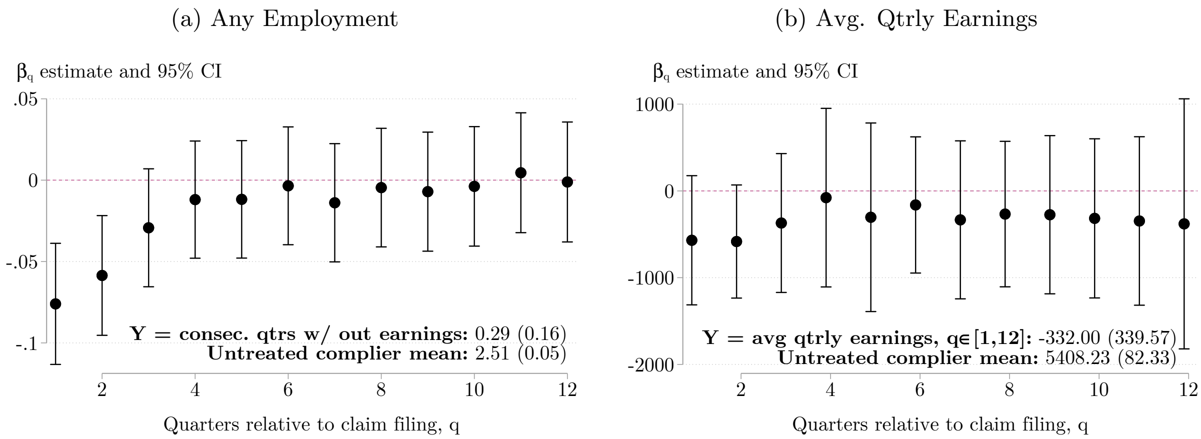


Notes: This figure displays coefficients from separate regressions of the form in Equations B.1 and B.2, where the outcome is a measure of cumulative payments as of that week. A paid week refers to the calendar week the payment is made, while a covered week refers to the week of unemployment to which that payment pertains.

Figure B3 displays the dynamic employment effects of eligibility. The patterns are consistent with those in Figure 5, as any negative employment effect dissipates two years after the claim. The primary difference is that the point estimates for impacts on average total earnings are negative and only marginally insignificant.

Within each panel, there are two series. One series includes office fixed effects while the other excludes office fixed effects. The similarity between the two series suggests that the downstream impacts of office-level eligibility variation are consistent with those of examiner-level eligibility variation, which assuages excludability concerns in the design based on office assignment.

Figure B3: Dynamic Impacts of Eligibility Approval on Employment



Notes: Both panels display coefficients from separate UJIVE IV regressions of the form in Equations B.1 and B.2, where the outcome Y is a measure of contemporaneous quarterly employment and the endogenous treatment D is initial eligibility approval. Within each panel, one series includes office fixed effects while the other excludes office fixed effects. The sample includes all separation-based eligibility issues from regular initial claims between 2017 and 2019 that were assigned to examiners through the scheduled queue. The tF adjustment uses our first-stage F of 8.42 and conservatively derives the inflation factor of 1.944 using a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

C Additional Context for Comparisons of BCMC Ratios Across Different Program Expansions

This section contains additional information regarding the theoretical justification and empirical estimation of BCMC ratios in the California data.

C.1 Theoretical Motivation for BCMC Ratio

The following discussion closely follows the derivations in Schmieder and von Wachter (2017) and Lee et al. (2021). Consider a vector of UI policy rules θ (e.g., separation-based eligibility approval probability, WBA, or PBD), a scalar earnings tax rate τ, and an optimizing representative worker's resulting set of choices $\mathbf{Y}(\theta, \tau)$ (e.g., search effort while unemployed, diligence while employed, etc.). Given government policy, these choices imply UI benefits $B(\mathbf{Y}(\theta,\tau);\theta)$ and taxes paid $T(\mathbf{Y}(\theta,\tau);\tau)$. The government's budget G is net transfers $B(\mathbf{Y}(\theta,\tau);\theta) - T(\mathbf{Y}(\theta,\tau);\tau)$. Consequently, the total effect of a UI policy change $\mathrm{d}\theta_j$ along a policy margin j on the government budget can be decomposed into an indirect effect due to behavioral responses and a direct effect due to the mechanical benefit transfer:

$$\underbrace{\frac{\mathrm{d}G}{\mathrm{d}\theta_j}}_{\text{total}} = \frac{\mathrm{d}B(\mathbf{Y}(\theta,\tau);\theta) - T(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\mathrm{d}\theta_j} = \underbrace{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta_j}}_{\text{behavioral}} + \underbrace{\frac{\partial B}{\partial \theta_j}}_{\text{mechanical}} \tag{C.1}$$

The ratio of the second terms and third terms in Equation C.1 is the BCMC ratio, and we next show it features prominently in the welfare effects of UI policy reforms. For the worker, let $U(\cdot,\cdot,\cdot)$ be the representative worker's utility function with arguments B (i.e., consumption while unemployed), T (i.e., consumption while employed), and $\mathbf{Y}$ (i.e., other choices). The worker treats government policy θ and τ as fixed. Denote the worker's resulting indirect utility function by $V(\theta,\tau)$. For the government, its balanced budget constraint $G(B,T) = 0$ implicitly defines a function $\tau(\theta)$:

$$\frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} = -\frac{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial \theta_j}}{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial \tau}} = -\frac{\frac{\mathrm{d}G}{\mathrm{d}\theta}}{\frac{\mathrm{d}G}{\mathrm{d}\tau}} \tag{C.2}$$

The welfare change due to a UI policy reform $\mathrm{d}\theta_j$ in utility terms is:

$$\begin{aligned} \frac{\mathrm{d}V}{\mathrm{d}\theta_j} &= U_1 \cdot \frac{\partial B}{\partial \theta_j} + U_2 \cdot \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} \\ &= U_1 \cdot \frac{\partial B}{\partial \theta_j} - U_2 \cdot \phi \cdot \frac{\mathrm{d}G}{\mathrm{d}\theta_j} \end{aligned} \tag{C.3}$$

where the first line comes from applying the envelope theorem to the worker's problem while respecting the government budget constraint while the second line comes from substituting in Equation C.2 and denoting $\phi := \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}G}{\mathrm{d}\tau}$ as the mechanical share of total deficit reduction following a tax increase. Intuitively, first-order welfare changes come only from changes to the tax and transfer system. The mechanical UI transfer $\frac{\partial B}{\partial \theta_j}$ matters in proportion to the marginal utility while unemployed U_1. The resulting total effect on the government budget $\frac{\mathrm{d}G}{\mathrm{d}\theta_j}$ requires changing taxes while employed. ϕ is the share of this tax change that has a first-order welfare impact, and this matters in proportion to the marginal utility while employed U_2.

To aid interpretation of Equation C.3, we substitute in the total government cost decomposition from Equation C.1 and normalize the entire equation. The normalization divides through by $U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}$. The U_2 rescaling translates the utility welfare change into a money-metric, the $\frac{\partial B}{\partial \theta_j}$ rescaling translates UI policies of different magnitudes into a common unit of "welfare gain per dollar that provides first-order welfare gain", and rescaling by ϕ focuses attention on the welfare costs depending on the choice of UI policy θ. The resulting equation is:

$$\frac{\mathrm{d}W}{\mathrm{d}\theta_j} = \frac{U_1}{U_2 \cdot \phi} - 1 - \frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta_j}} \tag{C.4}$$

where $\frac{\mathrm{d}W}{\mathrm{d}\theta_j} := \frac{\frac{\mathrm{d}V}{\mathrm{d}\theta_j}}{U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}}$ is the money-metric unit welfare change.

In the parlance of the Baily-Chetty formula for optimal UI, $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is the "benefit-side" and $\frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta}}$ is the "cost-side" (Baily, 1978; Chetty, 2006). For a UI benefit expansion, the former captures the (first-order) welfare gain from transferring consumption across employment states, while the latter represents the (first-order) welfare loss from raising additional revenue beyond the mechanical transfer of money across employment states. In this sense, the BCMC ratio is a sufficient statistic for the efficiency costs of different UI benefit reforms $\mathrm{d}\theta_j$: for a given gap in marginal utilities across employment states, a lower BCMC ratio indicates that a UI benefit reform is likelier to be welfare-increasing.[35]

While the above model is quite general, one plausible generalization worth noting is allowing for effects of UI policy reforms $\mathrm{d}\theta_j$ independent of effects on taxes and transfers. For the worker,

[35] The general Baily-Chetty optimal UI framework can be seen as a specific application of the MVPF framework (Hendren and Sprung-Keyser, 2020). Through the lens of the MVPF, the Baily-Chetty framework essentially separately calculates (1) the MVPF of a UI benefit increase *without* a tax adjustment and (2) the MVPF of the tax increase necessary to finance such a UI benefit increase. The BCMC ratio is the fiscal externality term corresponding to the MVPF of a UI benefit increase.

administrative hassles could directly affect utility. If so, the left-hand side of Equation C.4 would have an additional summand capturing the direct effect of the reform ($\propto \frac{\partial U}{\partial \theta_j}$). For the government, administrative costs could directly affect the government budget independent of net transfers $B-T$. If so, the numerator of the BCMC ratio would have an additional term capturing the UI policy reform's direct effect on administrative costs ($\frac{\partial G}{\partial \theta_j}$).

The representative worker framework generates immediate policy implications based on the BCMC ratio. For example, suppose $\text{BCMC}_j < \text{BCMC}_k$ for different UI policies θ_j and θ_k. The first term $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ depends on the final transfer of consumption across states rather than the source of this transfer, so the money-metric unit welfare change is higher for the UI policy margin j than k ($\frac{\mathrm{d}W}{\mathrm{d}\theta_j} > \frac{\mathrm{d}W}{\mathrm{d}\theta_k}$). Consequently, a budget balanced policy reallocation away from k towards j ($\mathrm{d}\theta_k < 0 < \mathrm{d}\theta_j$) raises total welfare.

Different UI policy margins tend to apply to different types of workers, and this can have important implications for making welfare comparisons across policy reforms. Without a representative consumer, social welfare is an aggregation of individual utilities, and the aggregation weights may depend on individual characteristics or marginal utility itself (Saez and Stantcheva, 2016). As a concrete example, the monetary eligibility minimum earnings threshold θ_j affects UI benefits for relatively low-earning claimants who either receive some UI benefits or none at all, while the maximum WBA threshold θ_k affects UI benefits for relatively high-earning claimants who are already eligible for UI benefits. The relative gap in marginal utilities across employment states $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is plausibly larger for the monetary eligibility earnings threshold θ_j than the maximum WBA threshold θ_k, as diminishing marginal utility implies that the first dollar of insurance is more valuable than additional dollars of insurance. Moreover, utilitarian preferences for redistribution would imply that those affected by monetary eligibility have higher generalized social marginal welfare weights, as they have higher marginal utility due to lower baseline income. While a balanced budget policy reform affecting different types of workers would no longer be a Pareto improvement due to distributional consequences, it could in theory be combined with reforms along existing non-UI policy dimensions to generate one (Hendren, 2020).

C.2 Weekly Benefit Amount Research Design

Institutional Details. The maximum WBA in California has been \$450 since January 2005 and increased four times during 2000: from \$230 to \$370 in January 2002, to \$410 in January 2003, and to \$450 in January 2005. In each time period the WBA is set to replace some proportion of average weekly wages in the highest earning quarter of the base period ($HQW/13$). The target replacement rate was 0.39 prior to 2002, 0.45 in 2003, and 0.5 thereafter. Therefore WBA is:

$$WBA(t) = \min\left(\frac{HQW}{13} \cdot RR(t), WBA^{max}(t)\right) \tag{C.5}$$

where $RR(t)$ is the target replacement rate and $WBA^{max}(t)$ the maximum WBA at time t.

There are two temporary WBA supplements during our sample period. First, the Federal Additional Compensation program added \$25 to all WBAs from February 2009 through December 2010 due to the Great Recession. Second, the Federal Pandemic Unemployment Compensation program added \$600 to all WBAs between April and June 2020 due to the Covid-19 pandemic.

Sample Restrictions. As we will expand on in the following discussion of potential benefit duration program rules, there is an offsetting kink in WBA among claimants with $PBD < 26$. Therefore, following Card et al. (2015) and Bell et al. (2022b), we restrict to claimants with the full regular potential benefit duration of 26 weeks. This excludes those with especially variable earnings across quarters.

We define recentered high-quarter wages HQW^* as follows:

$$HQW^*(t) = HQW - \frac{13}{RR(t)} \cdot WBA^{max}(t) \tag{C.6}$$

where $WBA^{max}(t)$ is the maximum WBA at time t. We restrict to claims with $HQW^* \in [-5000, 5000)$.

Estimating Equation. We estimate 2SLS linear regressions. The reduced-form and first-stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{C.7}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{C.8}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the recorded WBA; and $T_i = \mathbf{1}\left[HQW_i^* \geq 0\right]$. The fuzzy RKD estimator for the causal effect of an additional \$1 of WBA, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on the interaction term $HQW_i^* \cdot T_i$, $\hat{\beta} = \hat{\beta_2}/\hat{\alpha_2}$. It is a fuzzy RKD because the actual WBA awarded to claimants may differ if they appeal with non-recorded UI-eligible wages.

We use the constant β_0 to calculate the mechanical transfer with Y_i as the number of paid weeks.

C.3 Potential Benefit Duration Research Design

Institutional Details. Once WBA is determined as described above, a formula determines the regular maximum benefit amount (MBA). In words, aims for no more than a 50% replacement based both on WBA and total base period wages. Formally, it is:

$$MBA = \min\left(\frac{1}{2} \cdot BPW, WBA \cdot 26\right) \tag{C.9}$$

Regular PBD is then defined as the number of weeks a claimant can receive their WBA before exhausting their regular MBA. Rearranging Equation C.9:

$$PBD = \begin{cases} 26 & \text{if } WBA \cdot 26 \leq \frac{1}{2} \cdot BPW \\ \frac{\frac{1}{2}}{BPW} & \text{if } WBA \cdot 26 > \frac{1}{2} \cdot BPW \end{cases} \tag{C.10}$$

Substituting in the case when $WBA < WBA^{max}$ from Equation C.5 into Equation C.10:

$$PBD = \begin{cases} 26 & \text{if } 4 \cdot RR(t) \leq \frac{BPW}{HQW} \\ \frac{13}{2 \cdot RR(t)} \cdot \frac{BPW}{HQW} & \text{if } 4 \cdot RR(t) > \frac{BPW}{HQW} \end{cases} \tag{C.11}$$

Equation C.11 demonstrates the kink in the regular PBD formula with respect to $\frac{BPW}{HQW}$ we exploit.

There are several benefit extensions during our sample period. These benefit extensions increase the total PBD at a given point in calendar time in proportion to the regular PBD.[36] We define total PBD as the total number of continuous weeks of full regular and extended benefits the claimant could receive if they remained continuously unemployed.[37]

Sample Restrictions. To avoid the offsetting kink due to WBA^{max} kink, we exclude claims with $WBA = WBA^{max}$. Additionally, since the ratio $\frac{BPW}{HQW}$ is by definition bounded above by 2—which is usually the location of the PBD kink—we further restrict to claims with earnings in every quarter

[36] Temporary Extended Unemployment Compensation increased the maximum PBD from March 2002 through December 2003 by at least an additional 13 weeks, Emergency Unemployment Compensation increased the maximum PBD from July 2008 through December 2013 by up to an additional 73 weeks, and Pandemic Emergency Unemployment Compensation increased the maximum PBD by up to an additional 73 weeks starting in March 2020. See Bell et al. (2022b) and Chodorow-Reich et al. (2019) for more detail.

[37] We ignore several instances of 1-week gaps when extended benefits temporarily expired.

of the base period.

We define the recentered ratio of base period wages to high-quarter wages $\frac{BPW}{HQW}^*$ as follows:

$$\frac{BPW}{HQW}^*(t) = HQW - \frac{BPW}{HQW} \cdot 4RR(t) \tag{C.12}$$

We further restrict to claims with $\frac{BPW}{HQW}^*(t) \in [-0.5, 0.5)$.

Estimating Equation. We estimate 2SLS linear regressions. The reduced-form and first-stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 \frac{BPW_i}{HQW_i}^* + \beta_2 \frac{BPW_i}{HQW_i}^* \cdot T_i + \varepsilon_i \tag{C.13}$$

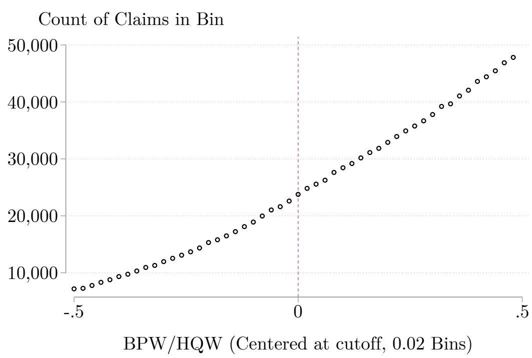
$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 \frac{BPW_i}{HQW_i}^* i + \alpha_2 \frac{BPW_i}{HQW_i}^* \cdot T_i + u_i \tag{C.14}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the total PBD; and $T_i = \mathbf{1}\left[\frac{BPW_i}{HQW_i}^* \geq 0\right]$. The fuzzy RKD estimator for the causal effect of an additional week of benefits, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on the interaction term $\frac{BPW_i}{HQW_i}^* \cdot T_i$, $\hat{\beta} = \hat{\beta}_2 / \hat{\alpha_2}$. It is a fuzzy RKD due to measurement error in apportioning extended benefits in addition to the aforementioned possibility of benefit recomputation. We use the constant β_0 to calculate the mechanical transfer with Y_i as the product of WBA and an indicator for exhausting total benefits.

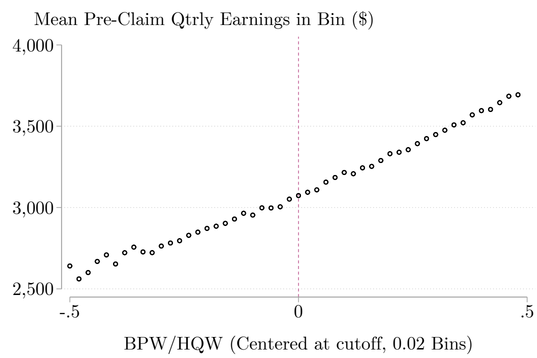
C.4 Additional Tables and Figures for Comparisons of BCMC Ratios Across Different Program Expansions

Figure C1: PBD RKD Density and Covariate Smoothness through Cutoff

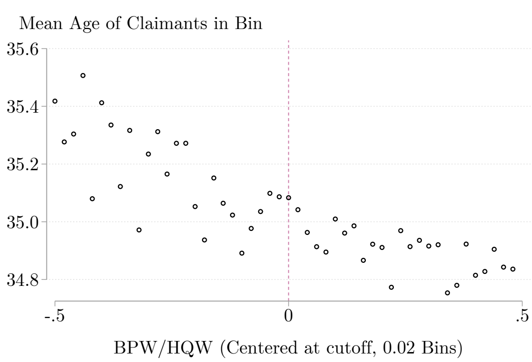
(a) Running Variable Density



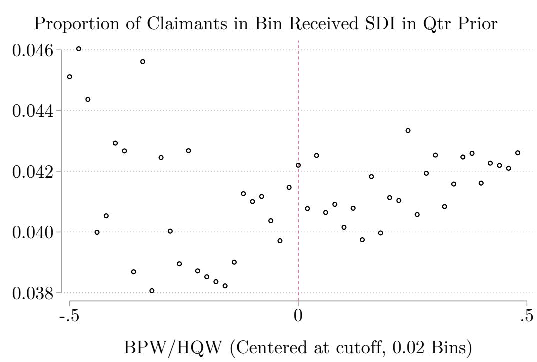
(b) Smoothness of Prior Earnings



(c) Smoothness of Age



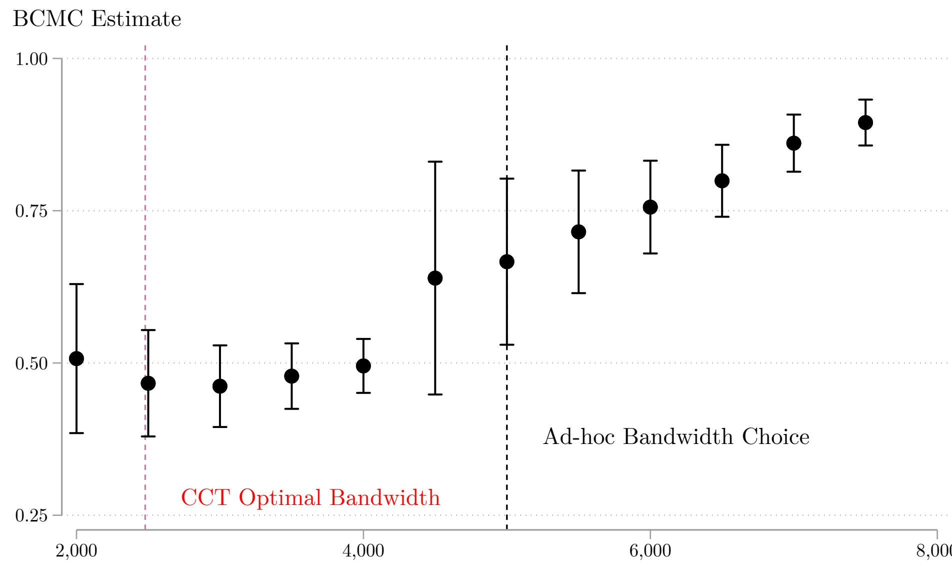
(d) Smoothness of Pre-UI SDI Receipt



Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in Table 5. The running variable is the ratio of base period wages to high-quarter wages relative to the year-specific kink and the bin width is 0.02. Panel (a) shows that the density of the running variable is smooth through the cutoff. Panels (b), (c), and (d) show that three example covariates are smooth through the cutoff.

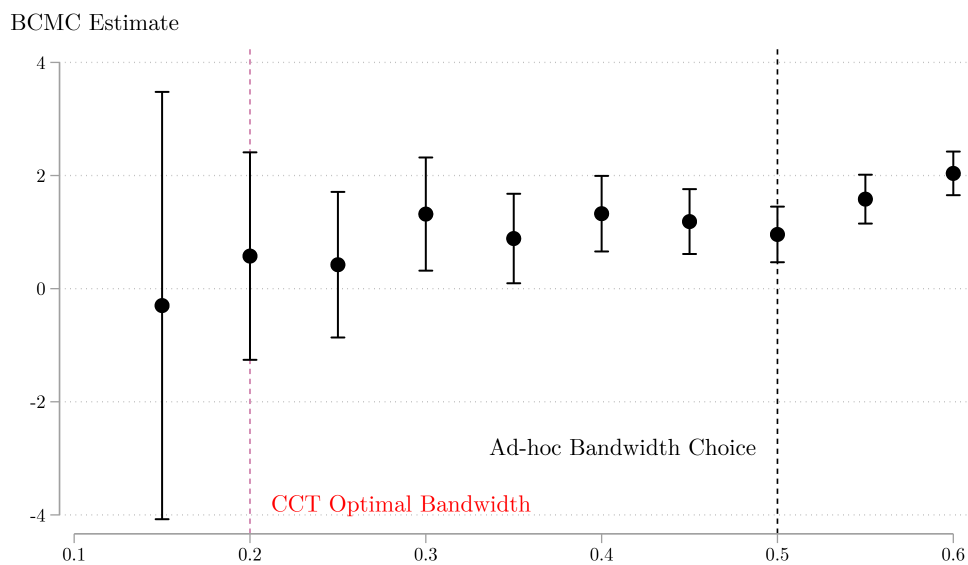
Figure C2: RKD Sensitivity to Bandwidth Choice

(a) WBA



Bandwidth (HQW Range Around Cutoff, $)

(b) PBD



Bandwidth (BPW/HQW Range Around Cutoff)

Notes: BCMC estimates for benefit amount and benefit duration increases with different RKD bandwidths. CCT optimal bandwidth is from Calonico et al. (2014).

D Other Welfare Considerations

D.1 Consumption-smoothing benefits

Unobserved consumption-smoothing benefits are likely to increase the welfare benefits of separation-based eligibility expansions for two reasons. As equation C.4 demonstrates, the gap in marginal utilities across employment statuses captures the consumption-smoothing benefits of the benefit expansion. One reason, as shown in Tables 1 and 5, is that claimants on the margin of separation-based or monetary eligibility have relatively lower incomes than other UI claimants. With diminishing marginal utility of consumption, this suggests a given consumption drop due to unemployment—relative to pre-unemployment consumption—implies a larger gap in marginal utilities. Diminishing marginal utility of consumption also implies that the first dollar of insurance provides the greatest consumption-smoothing value. Unobserved consumption-smoothing benefits might decrease the welfare benefits of separation-based eligibility expansions if claimants who quit or were fired for cause have *very* small declines in consumption at unemployment (relative to claimants who were laid off). For fired workers, we think this is unlikely to be true, given that such a separation is clearly still an unexpected shock for the worker. For voluntary quits, we acknowledge that this is less certain. To shed light on the magnitude of consumption declines at unemployment for unemployed workers who quit, we follow the literature on the consumption smoothing benefits of UI and estimate the consumption drop at unemployment in the Panel Study of Income Dynamics (PSID). Table D1 shows estimates of a specification common in the consumption smoothing literature, where the outcome is the change in consumption between periods, and the coefficient of interest is a separation dummy. The first column replicates the consumption drop at unemployment reported by Hendren (2017), which is for all separations to unemployment. The second limits this sample to voluntary quitters. While somewhat imprecise due to the small sample size, we find that consumption declines by 3.7% at unemployment among quitters. This is smaller than the consumption decline in the full sample of separators (7.2%), but still meaningfully large.

D.2 Administrative costs

The separation-based eligibility determination process involves both monetary and utility costs, and eligibility expansions could avoid those. Investigating cases has fixed and variable costs in the form of technological infrastructure and examiner wages, respectively. As mentioned in the discussion of Figure 1, the rate of claimant appeals following an initial denial is much higher than the rate of employer appeals following an initial approval. Therefore it is likely that increasing the approval rate will decrease the total number of appeals of an initial determination.

To provide empirical evidence on the magnitude of these administrative costs we turn to DOL data from its Resource Justification Model (RJM). As described in Lachowska et al. (2022), the RJM consists of publicly available state reports to DOL of costs incurred by the state UI agency per various workload units, such as claims processed or eligiblity determinations completed. Importantly, these reports include state-by-fiscal-year estimates of the *marginal staffing cost* associated with each nonmonetary eligibility determination and each appeal of eligibility determinations.

We note first that the aggregate annual cost of administering separation-based eligibility rules is substantial in California. For example, in FY2019, staffing costs associated with nonmonetary eligibility determinations was reported to DOL as nearly $50 million dollars. For appeals, FY2019 costs were just above $50 million dollars. As per DOL ETA reports 207 and 5159 for the same time period, 55% of nonmonetary determinations and 21% of appeals in CA were tied to separation-based eligibility decisions.

Since the RJM data provides these costs at the unit level, we can also provide back-of-the-envelope estimates of the cost savings from shifting one denied separation-based eligibility determination to approved. This quantity is driven entirely by avoided appeals, is positive if claimant appeals are more common than employer appeals (since claimants appeal denials and employers appeal approvals), and is closely tied to our research design.

We calculate the costs savings from approving one denied claim as the associated change in the probability that that determination is appealed—i.e., the probability that a claimant appeals (which occurs only if the claim is denied) minus the probability that the employer appeals (occurs only if the claim is approved)—multiplied by the unit cost of appeals as per the RJM. We take the simple average of the unit cost values for CA in the RJM over the period covered by our sample, which is $238. The proportions of denied and approved separation determinations that are appealed are calculated for the same time period via DOL ETA reports 2017 and 5159. These values are 23% (claimant appeals) and 3% (employer appeals).

These values imply that approving one denied separation-based eligibility determination would provide just under $50 in administrative cost savings. If we were to take this estimate at face value and include it in our BCMC calculation it would offset slightly more than 100% of the *behavioral cost* associated with expanding UI eligibility on the separation eligibility margin. It is important to note that the various state-year level averages underlying this calculation may not align directly with the complier group from our research design. For example, employer appeals may be more common and claimant appeals may be less common for the compliers in our design than overall. Nonetheless, we believe this back of the envelope estimate provides added support for our main finding that relaxing separation-eligibility is an especially low-cost approach to expanding the UI program.

Similar approaches could be used to calculate the cost savings from avoiding one eligibility determination entirely. This would include the costs of both the eligibility determination itself ($45 in the same time period) and the appeals processing costs mentioned above (scaled differently to incorporate the costs of avoided claimant *and* employer appeals). We do not provide such a calculation here, since it is not directly tied to our research design, and is likely subject to offsetting employee and employer behaviors. (Additional claim contestations on the part of employers, or quits on the part of employees.) However, given that cost savings from avoiding determinations entirely are likely to be large, some policymakers may wish to consider relaxing separation-based eligibility criteria in this way. This could be achieved, for example, by only initiating eligibility interviews if an employer contests eligibility. (So that a claimant's self-report of a quit or firing alone would not trigger an eligibility determination.)

D.3 Welfare weights

Different welfare frameworks likely imply different impacts of incorporating generalized marginal social welfare weights. Related to utilitarian considerations, claimants on the separation-based eligibility margin are relatively low-income. Preferences for redistribution provide an independent reason for transfers to this group, though it is unclear whether this is desirable beyond standard redistribution through the tax code (Atkinson and Stiglitz, 1976; Akerlof, 1978). Non-utilitarian considerations could include the relative undesirability of "false positives" (i.e., approving a claimant who technically does not satisfy eligibility) vs. "false negatives" (i.e., denying a claimant who technically does satisfy eligibility). One estimate of the current levels of "ground-truth" eligibility comes from periodic Department of Labor audits through the Benefit Accuracy Measure (BAM) Program. These aggregate data estimate that approximately one-fourth of separation-based eligibility denials should've been approved while one-twentieth of approved claims should have been denied on separation-based eligibility grounds.

D.4 Other behavioral responses

An expansion in separation-based eligibility could induce other general equilibrium behavioral responses not captured by our partial equilibrium approach. One plausible channel is an increase in job separations through increased employee quits.[38] While quasi-experimental work documents that quits increase in response to benefit extensions, panel variation in separation-based eligibility criteria across states finds mixed evidence on quits (Jäger et al., 2022; Ragan, 1984; Solon, 1984).

It is also worth noting that most approaches to expanding separation-based eligibility would maintain two key barriers to an ex ante quit response. First, claimants who voluntarily quit their job must prove that they had good cause to do so. Even if the criteria that constitute good cause were expanded substantially, or if UI agencies adopted more lax standards for determining whether a given claimant meets those criteria, this requirement can protect against claimants opting to quit their job solely to collect UI benefits. Second, the input of the separating employer provides an additional screen against this behavior.

Another plausible channel is a change in the composition of employers' offered jobs and desired candidates. For example, employers may be less willing to employ workers they deem to be at risk of quitting due to personal circumstances or committing misconduct.

We believe either of these general equilibrium responses is likelier to be important if the policy change is non-marginal and clear to outside parties, such as adding an entire category of UI-eligible quits. On the other hand, they are less likely to be important if it is a marginal shift in the probability of eligibility approval.

[38] However, experience-rated employers may be less likely to fire workers if they are likelier to receive UI benefits.

D.5 Tables and Figures for Other Welfare Considerations

Table D1: Consumption Drop at Unemployment by Separation Reason (PSID)

	All Separations	Quits Only
Change in log food consumption at unemployment	-0.072***	-0.037
	[-0.092,-0.053]	[-0.082,0.008]
Mean of outcome	-0.005	-0.004
Observations	65,808	63,894
Households	9,562	9,423
Separations	1,614	348

Notes: Table presents estimates from a regression of the change in log food consumption (relative to the prior survey wave) on a dummy for whether a job separation occurred in this period. Controls include dummies for the year of the survey and cubic polynomial in age. Following Hendren (2017), the sample is limited to stably employed household heads between the ages of 25 and 65. Food consumption is measured as the sum of food expenditure in the home, food expenditure out of the home, and the value of food stamps received. Estimates in the left-hand column correspond to the $t = 0$ estimate from Figure 4 in Hendren (2017). Estimates in the right-hand column limit the same from the left-hand column to include only those separators who report quitting their jobs.