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Efficiency costs of unemployment insurance denial: Evidence from randomly assigned examiners

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

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ABSTRACT

Approximately 10 percent of Unemployment Insurance (UI) claimants in the United States 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—those whose job separation reason would be deemed UI-eligible by some examiners but UI-ineligible by others. Approving a marginally at-fault claimant increases UI benefits paid by over \$3,000 and lengthens the nonemployment spell by just under two weeks, but it does not decrease labor income. We combine these estimates and other relevant claimant responses to calculate the fiscal externality of expanding eligibility on this margin and find that it accounts for 16 percent of the expansion's total cost. Using two regression kink designs in the same data, we show that other more commonly studied UI benefit expansions have significantly larger fiscal externalities. 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.

JEL Classification Codes: H76, J64, J65, J68

Key Words: unemployment insurance, separation-based eligibility, UI claimants, UI expansions, unemployment duration

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Upjohn Institute working papers are meant to stimulate discussion and criticism among the policy research community. Content and opinions are the sole responsibility of the author.

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 percent of Unemployment Insurance (UI) claims filed in 2019 were denied for this reason. The subjectivity of what caused a job separation raises difficult questions: How should policymakers determine fault? Which claims should be denied?

In the United States, states define 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 eligibility¹ 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 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. 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.

Our main finding is that the fiscal externality from extending eligibility to marginally at-fault

¹For 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 an eligible quit in 48 states.

²Lachowska et al. (2023) 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.

claimants is much 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—compared to \$0.67 for benefit amount increases and \$0.96 for benefit duration increases. 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.

Our identification strategy relies on variation across UI claim processing offices in their propensity to approve separation-based eligibility issues. Because claims are quasi-randomly assigned to offices, office-level differences in approval rates can be interpreted as the causal effect of assignment to the office—and are not confounded by unobservable differences between claimants assigned to those offices. 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 (IV) 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 show that the social costs resulting from these behavioral responses are relatively small by making an apples-to-apples comparison across several key margins of UI generosity. Specifically, we translate estimates of behavioral responses (e.g., longer nonemployment spells) to measures of the total government cost per dollar transferred to the unemployed. 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 the approach to study the extensive margin of benefit eligibility. Similar to Lee et al. (2021), our empirical implementation directly estimates the effect of policy changes on the government’s budget. This requires fewer assumptions than the typical approach of estimating the effect of the policy change on nonemployment duration and using a parametric model to determine the implications of that response on net government spending.

Using this measure we show that the social costs of expanding at-fault eligibility (estimated via our eligibility IV) are much lower than those resulting from increases in UI benefit levels or durations (estimated from regression kink designs in the same California data). This points to the desirability of reallocating UI benefits away from the intensive margin and toward marginally at-fault claimants.

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; Ahammer et al., 2023), a decrease in administrative costs, or a change in consumption-smoothing benefits. We 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 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). Diminishing marginal utility suggests that a reallocation from benefit levels or durations toward marginally at-fault claimants would increase the consumption-smoothing benefits of the UI program on net, since the first dollar of insurance is most valuable.

Our primary contribution is to estimate 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.³ Their monetary eligibility variation due to the minimum earnings threshold is local to workers with very low earnings and labor force attachment. It is *a priori* unclear how the impacts of monetary eligibility 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 context. The most closely related paper in this respect is Lee et al. (2021), which estimates the per-dollar transfer costs of increasing weekly benefit amount and decreasing partial benefit marginal taxes in Washington State.⁴ We compare the two most commonly studied UI benefit intensive margins—weekly benefit amount (WBA) and potential

³A recent systematic review of causal estimates of UI disincentive effects contained 89 intensive margin estimates: 40 pertaining to weekly benefit amount and 49 pertaining to potential benefit duration (Cohen and Ganong, 2024).

⁴Two other related papers estimate the per-dollar transfer costs of disability insurance policies (Haller et al., 2024) 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.

benefit duration (PBD)—to the novel at-fault eligibility margin that has received little attention in the literature.

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, 2024). A recent survey of the applied microeconomics literature finds over 70 studies employing a judges design, where the applications span criminal justice; finance; health; patents; and various government programs at the federal, state, and local level (Chyn et al., 2024).

Finally, our novel research design is uniquely able to characterize heterogeneity in the nonemployment effect of UI benefits. Our identifying variation is at the individual level and does not directly depend on prior income or age. This allows us to show 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 at-fault eligibility and benefit amount expansions. In contrast, the existing UI literature relies on either interacting state-level panel variation in UI generosity with observable demographics (e.g., Chetty, 2008), calibrating a life-cycle 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 discuss eligibility criteria for UI in California and how their implementation facilitates our causal research design. We also compare California’s rules to those in other states, which are broadly similar.

Eligibility criteria fall into two broad categories: monetary eligibility and nonmonetary eligibility.⁵ Monetary eligibility requires a minimum amount of earnings during approximately the year preceding the claim. In California the dollar requirement is low enough that nearly all newly unemployed formal workers meet it. Nonmonetary eligibility encompasses all other eligibility criteria that relate to the claimant’s job search activity (e.g., that they are able and available to begin work while receiving UI benefits) and the focus of our project: separation-based eligibility.

⁵See the Department of Labor’s [Comparison of State UI Laws](#) for a thorough discussion of UI policy parameters.

2.1 Separation-based Eligibility Criteria and Detection

Separation-based eligibility criteria deny benefits when claimants quit without “good cause” or were fired for “misconduct.” The presence of misconduct depends on the type, intention, and degree of an employee’s disregard for their duty to the employer. The criteria for quits highlight how “involuntary” the employee’s decision was. The California Employment Development Department (EDD) maintains a list of circumstances surrounding quits and firings which describes what would constitute good cause or misconduct in each circumstance (California Employment Development Department: Anaheim, CA, 2023).⁶

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.

Fault is inherently subjective, creating scope for claimants with similar circumstances to receive different eligibility determinations. This is made clear, for example, by the EDD’s UI Benefit Determination Guide, which 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: Anaheim, CA, 2023). Another instructive example is the guideline for adjudicating quits due to commute difficulty. This guideline states that “because travel time is subjective, depending upon the claimant’s situation and labor market area, there is no hard-and-fast answer.”

States have relatively broad discretion to decide separation-based eligibility parameters. For example, good cause includes joining the military in 23 states, caring for an ill family member in 41, fleeing domestic violence in 44 states, and sexual harassment at work in 48 states. Different states can also require different types of documentation or apply a different burden of proof. Rules also vary internationally. Notably, some countries allow UI claimants to retain eligibility after a quit or a firing but impose lower benefits or long waiting periods (e.g., in Germany and Japan) (Schmieder and von Wachter, 2016).

⁶The 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.

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

In California, UI claims are assigned quasi-randomly to examiners who make eligibility decisions. 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 to isolate quasi-random variation in initial eligibility denials and benefit receipt.

When a claim is filed it is assigned to a single office based almost entirely 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 100 final SSN digits to offices changes over time for two workload management reasons. First, the number of offices gradually increases from 9 to 14. 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.

When an eligibility issue is detected, that investigation is assigned to an examiner within the office. Around 40 percent of claims are assigned to examiners by a scheduling queue that sequentially matches pending claims to available examiners in the assigned office who speak the claimant’s language. These assignments are useful for our supplementary examiner research design, as unobserved examiner characteristics are conditionally independent of unobserved claim characteristics. The other 60 percent of claims are taken up while awaiting automatic scheduling on an ad hoc basis by an examiner with unexpected availability.

2.3 Monetary Implications of UI Eligibility

Some claimants who are initially deemed eligible do not go on to receive UI benefits, and some claimants who are initially deemed ineligible do. **Figure 2** quantifies the relative role of three reasons for this. First, some claimants successfully appeal ineligible decisions. Second, some claimants initially deemed eligible never go on to receive benefits—this could occur, for example, if job search requirements are not met, or if the claimant quickly becomes reemployed. Third, some employers successfully appeal decisions when their former employees are initially deemed eligible. The first two examples are relatively common and the third is rare.

Claimants may also receive some UI benefits before a separation-based eligibility issue arises. However, since employers must dispute a claim within 10 days of receiving government notice about the claim, this is rare and not shown in [Figure 2](#).

UI benefits typically replace around 50 percent of prior weekly earnings for 26 weeks. Replacement rates are relatively lower for workers with higher earnings, as the WBA is capped at \$450 per week for most of our sample period. Potential benefit duration increases during recessions through federally funded extensions (reaching 52 weeks in 2002 and 99 weeks from 2009 to 2012) and can also vary across claimants, as those with variable quarterly earnings can have lower PBD. For additional detail on WBA and PBD calculations, see [Appendix D](#).

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

In this section we describe administrative data from the California UI program that we use in our analyses. We then show that separation-based eligibility issues are common—affecting 30 percent of the relevant claims in our data—but disproportionately affect claimants with lower socioeconomic status.

3.1 Data Sources

Our primary analysis links individual-level administrative datasets on UI claims (2000–2019), quarterly earnings (1995–2022), and SDI claims (2000–2022), all maintained by the state of California’s Employment Development Department (EDD). Related data have been used in a series of policy briefs on UI in California during the pandemic (Bell et al., 2022). The claims datasets include all UI and SDI claims filed in California. The earnings data include all UI-covered employment in the state (non-covered employment includes some government, nonprofit, and informal employment).

UI claims. UI claims microdata contain claimant demographics and UI payment receipt (amounts and dates), and the presence and outcome of eligibility issues on the claim. We exclude years before 2002 due to missing data on processing office assignments and years after 2019 due to a different policy regime during the COVID-19 pandemic.

For each eligibility determination, we observe the issue type (i.e., quit or firing), de-identified and time-invariant identifiers for the processing office and examiner handling the determination, the determination date, and the eligibility determination. Since 2009, we observe the claimant’s self-

reported separation reason: quit, firing, or layoff. Following Lachowska et al. (2023), we infer that the previous employer contested the claim when there is a separation-based eligibility determination for a claimant who self-reported a layoff. Eligibility determinations among claimants who self-report a quit or firing are assumed to have arisen from that claimant self-report.

From 2017 onward we observe two inputs to *examiner* assignment: the claimant’s spoken language and an indicator for whether a conditionally random algorithm assigned the examiner. The claimant’s spoken language limits the set of potential examiners. A scheduling algorithm typically assigns the next available examiner who speaks that language. When workload is high, some assignments are instead made through an ad hoc process which involves manager discretion.

Quarterly Earnings. Since the number of UI payments received (the typical spell duration measure in the literature on the U.S. UI program) is unobserved for unpaid claims, we use the earnings records to measure quarterly earnings and nonemployment duration. We define nonemployment duration as the number of consecutive quarters with \$0 earnings starting with the quarter *following* the initial claim (i.e., for a claim filed in quarter q , $> \$0$ earnings in quarter $q + 1$ implies nonemployment duration = 0 quarters). We right-censor at 12 quarters following the quarter of the initial claim. 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 downward, we show in Section 6 that it also generates an upward bias for our welfare-relevant measure of eligibility expansion efficiency costs.

SDI and PFL Claims. 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 higher benefits levels than UI (60–70 percent replacement rates up to a max of \$1,620 per week), SDI often provides benefits for longer than UI (≤ 52 weeks), and PFL lasts up to 8 weeks. Claimants must meet prior-earnings requirements similar to those of UI, and provide either a medical certification of the disability/illness (theirs for SDI, the person they are caring for for PFL) or proof of birth (PFL). Since only a small portion of recent SDI receipt among UI claimants with separation issues is accounted for by the PFL program, we use “SDI” in the rest of the paper as shorthand for both programs.

We use the SDI data for two purposes. First, we descriptively characterize UI claimants affected by separation eligibility issues. Second, as described further in Section 6.2, we estimate the causal effect of changes to UI policy parameters on government spending—which includes potential spillovers on other government programs like SDI.

Infutor. We link a subset of our sample (≈ 3.9 m claimants) to a panel of person-quarter level addresses from Infutor. Infutor is a private data aggregator that specializes in providing data with persistent person IDs to other private companies for marketing purposes. This linkage allows us to observe changes to claimants who migrate out-of-state migration, which could bias results that are limited to California data. These data have been used by, e.g., Bernstein et al. (2022), who show that it is broadly representative of the adult U.S. population.

3.2 Characteristics of Claimants with Separation-Based Eligibility Issues

Table 1 shows that 30 percent of all monetarily eligible claims had a separation-based eligibility determination and that 42 percent of these were denied. Younger, nonwhite, female, and lower-income claimants are likelier to have a separation-based eligibility issue and for that issue to result in a denial. Fourteen percent of claimants with a separation issue receive some SDI benefits in the year before their UI claim, much higher than the 9 percent among all monetarily eligible UI claims. SDI receipt is concentrated among voluntary quit claimants, 22 percent of whom receive SDI in the year before their UI claim. Frequent flows between the UI and SDI programs also suggest that program substitution may be important—denied UI claimants may turn to SDI as a substitute. We test for these responses in Section 5 and incorporate them in our welfare analyses in Section 6.2.

Even among eligible claims, a separation-based eligibility investigation may delay payments. Column 1 of Table 2 shows mean time to first payment for claimants with and without eligibility issues. Column 2 shows that the time to first payment is slightly delayed for claimants who are investigated but ultimately approved, likely due to required examiner interviews with the claimant and employer. Column 3 shows that a successfully appealed separation-based eligibility denial delays payment receipt by over two months relative to an initially approved claim.

While these patterns are evidence of disparate impact, we cannot determine the underlying cause. Socioeconomically disadvantaged people may have more separation eligibility issues and/or denials because of the nature of their jobs, their personal circumstances, or their preferences.

4 Research Design

In this section, we first describe the instruments and main estimating equation used for our design based on assignment to processing offices. We then discuss and validate the identifying assumptions necessary to interpret the coefficient of interest as a causal effect of UI eligibility.

Treating the initial eligibility determination as the endogenous treatment of interest 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 initial eligibility is the policy lever at the UI agency’s disposal.

4.1 Construction of Instruments

From the data provider we know that claims have been assigned to offices via the claimant’s two-digit SSN group since 2002. We use this information to construct our instruments in three steps. First, for each two-digit SSN group in each month we define the intended office as the office that processed at least 95 percent of claims. This measure is non-missing in 97 percent of digit-months and matches written agency documentation in 99 percent of digit-months where it is available (documentation is available for 11 of the 18 years in the data). The 3 percent of excluded digit-months in which the most common office accounts for < 95 percent of claims in the group occur almost entirely when SSN-based assignment rules change mid-month. Second, we include time controls in all specifications since office assignment and potential outcomes change over time (office openings shift the number of SSN digits per office, macroeconomic conditions affect the composition of UI claimants). Third, since more lenient offices for quits are not always more lenient for firings (as shown in [Figure A1](#)),⁷ we 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). We control for issue type to partial out level differences in eligibility across issues and show that issue type is unlikely to be affected by office assignment.⁸

⁷One 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; the burden of proof is on employers for misconduct but claimants for voluntary quits. The variation in propensities would have to be similar in magnitude, but they could have an arbitrary correlation.

⁸Ninety-three percent of determination issues match the claimant self-reported separation reason (quits lead to voluntary quit issues and firings lead to misconduct issues), and the minimal variation in the composition of separation-based eligibility issue types across offices (1.3 percentage points) is uncorrelated with issue-specific eligibility approval rates (see [Figure A2](#)).

4.2 Estimating Equation

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

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

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

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 1 and 2 is the two-step estimator unbiased jackknife IV (UJIVE) (Kolesár et al., 2013). The UJIVE estimator extends the logic of jackknife 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 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.

Since SSN-based randomization is persistent for individual i making multiple claims in time t and t' , all specifications cluster standard errors by claimant (Abadie et al., 2022). Our sample includes 6.9 million initial claims with eligibility issues from 5.5 million unique individuals.

4.3 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. First-stage relevance requires that office assignment be predictive of separation-based eligibility. We estimate the first-stage regression Equation 2 and test the joint significance of the office-by-issue dummies instead of using a constructed scalar leniency measure in order to correctly account for the degrees of freedom in the overidentified setup (Hull, 2017). 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 percent coverage without any adjustment to standard errors (Lee et al., 2022). [Figure 3](#) plots the coefficients from the first-stage regression.

Independence. Independence is implied 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](#). We test the joint significance of the office-by-issue instruments by estimating Equation 2 with several pre-existing claimant characteristics as outcomes, and we fail to find statistically significant differences across office-by-issue pairs.

Excludability. Excludability requires that processing offices affect the outcome Y only through the initial eligibility determination. Offices handle other administrative processing duties that could threaten this assumption. We rely on measures of office-level performance for some of these other processing duties and our complementary examiner-level design to alleviate this concern.

[Table 3](#) shows that that some offices are slower than others to make an eligibility determination and that some offices are more likely than others to detect future continuing claim eligibility issues unrelated to separation eligibility.⁹ However, the range across office-by-issue pairs in these measures is only four days and 3 percentage points, respectively. Further, [Figure A3](#) shows that these minor differences are uncorrelated with eligibility approval propensity.

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, 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.

Since examiners are assigned to investigations of individual eligibility issues, and not claims, our complementary examiner-level design in [Appendix C](#) is much less subject to these concerns.

Monotonicity. 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. 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 monotonicity concern is that an office-by-issue pair may be more lenient with one type of claimant and less lenient with another type. (Frandsen et al., 2023; Chan et al., 2022)

⁹Continuing claim eligibility issues include failure to engage in work search, failure to be available for work, and irregular reporting. These most often result in denying benefits for only the relevant week.

show that *average monotonicity* is sufficient to recover a LATE. Average monotonicity requires that office-by-issue-specific eligibility approval and overall eligibility approval are positively correlated for each claimant (see Appendix B for a formal definition).

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). In support of the monotonicity assumption, Figure 4 uses a scalar leniency measure in place of our office-by-issue dummy instruments¹⁰ to show that office-by-issue leniency 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, Figure A4 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. Two conceptually distinct issues stem from including controls in the specification. First, unmodeled heterogeneity in the first stage may be correlated with the included fixed effects, which can introduce “contamination bias” in the estimated treatment effect (Goldsmith-Pinkham et al., 2024). Second, heterogeneity in the second-stage equation can cause an unsaturated specification to deliver negative weights (Blandhol et al., 2022).

We provide a more detailed description of these issues in Appendix B but note here that both can be addressed by saturating Equations 1 and 2 with interactions between the office-by-issue instruments and time fixed effects. 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. In Table A1 we run separate regressions in each of the 480 month-by-issue cells and manually aggregate the estimates in proportion to cell size. All manually reweighted estimates fall within the confidence intervals from our main specification.

5 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 C

¹⁰A scalar version of our instruments facilitates simple comparisons of the first stage across subgroups. We follow standard practice in the leniency design literature and calculate the leave-out mean of a residualized eligibility indicator (by fully interacted office-by-issue controls). The construction of this scalar instrument is described in detail in Appendix B.

that results are similar using a more granular source of variation over a shorter time period.

5.1 Employment, Earnings, SDI Receipt, and Future UI Receipt

[Table 4](#) shows various estimates of Equations [1](#) and [2](#) using UJIVE, where the endogenous treatment D is eligibility approval. Columns 4–10 are measured over the 12-quarter period beginning with the quarter after the initial claim. The table includes the average value of each outcome Y among marginally ineligible determinations (untreated complier mean, \bar{Y}^0). We estimate \bar{Y}^0 in a system of equations analogous to Equations [1](#) and [2](#) with $Y \cdot (1 - D)$ as the outcome and $1 - D$ as the endogenous treatment following Frandsen et al. (2023). Outcomes for the remaining columns in [Table 4](#) are measured over the 12-quarter period beginning with the quarter after the initial claim.

Columns 1–3 show that eligibility doubles UI benefits received. The probability of receiving any UI payments increases from 31 percent to 63 percent (column 1), and the number and total dollar amount of payments increase similarly (columns 2–3). Panel (a) of [Figure A5](#) illustrates this visually, 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).

Despite doubling UI benefit receipt, separation-based eligibility approval has only modest effects on subsequent unemployment and virtually no effect on subsequent earnings. Column 4 shows that approved claimants are four 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. Column 5 shows that eligibility lengthens nonemployment by 0.14 additional quarters, and while the effect is not statistically significant, theory, prior work on moral hazard responses to UI benefits, and the related outcome in Column 4 suggest 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. 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 is imprecise.

Columns 7–8 show that marginally eligible claimants are substantially more likely to receive

UI benefits on a future claim occurring after reemployment, 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 six percentage points, which is more than 25 percent 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.

5.2 Dynamics

We next show the dynamics behind our main results in Table 4. To do so, we estimate Equation 1 and Equation 2 with outcomes $Y(q)$ measured q quarters after initial claim filing. Figure 5 displays these dynamic treatment effects, Figure A7 provides additional context that illuminates mechanisms for some of our main results, and Figure A6 further decomposes those treatment effects into potential outcomes for marginally approved and denied claimants.

Panels (a) and (b) of Figure 5 plot treatment effects for employment and earnings, respectively. 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 (panel (a)). There is never a statistically significant effect of eligibility approval on average quarterly earnings (panel (b)), and estimated effects of eligibility on earnings are often positive, especially later in the followup window.

Panel (c) shows that UI receipt on future claims is substantially increased by eligibility in all quarters. We define a future claim as a claim that is in a different 52-week benefit year. We see that future benefit receipt is substantially elevated in all quarters in the follow-up window. The impact on future UI claim receipt is smaller in magnitude during the first 3 quarters, as those periods are usually in the same benefit year as the claim with a separation-based eligibility determination. The effect peaks 6 quarters after the original claim, decreases in magnitude, but remains statistically significant at two percentage points 12 quarters post claim.

Panel (d) shows that the null result for SDI benefit receipt from Table 4 holds in nearly all quarters. All estimates are also insignificant economically.

Figure A7 shows that the increase in future 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. We define a separation as occurring in quarter q when a worker’s highest paying employer in quarter q no longer employs them 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 responses are not driven by migration responses. Since we only observe earnings within California, 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 contain a panel of current addresses for many U.S. adults. Panel (b) of [Figure A7](#) shows that there is no effect of eligibility on out-of-state migration.

[Figure A6](#) compares treatment effects to baseline outcomes by 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, similar to unemployment and earnings losses documented in previous work (Davis and von Wachter, 2011; Jacobson et al., 1993). This suggests that at-fault eligibility’s relatively small effect on nonemployment is not explained by a high propensity for affected claimants to exit the labor force.

5.3 Heterogeneity

Our identifying variation is unique in the literature on the disincentive effects of UI in that it varies across individual claims and has a relatively homogeneous first stage. This allows us to use [Equations 1](#) and [2](#) to estimate heterogeneous responses across different subsamples of UI claimants in [Table A2](#).

Heterogeneity is most notable by recent earnings levels, employment, and SDI receipt. Although estimates are imprecise, several subgroups see their nonemployment durations *decrease* in response to UI eligibility: the lowest earnings group, those who had no earnings in the quarter immediately before their UI claim (possible, if, for example, a worker delays claim-filing), and UI claimants who received SDI benefits in the quarter before their UI claim.

There is also meaningful heterogeneity by demographic characteristics—old, young, and white

claimants have small nonemployment responses relative to middle-aged and nonwhite claimants—and calendar time—larger nonemployment responses in the early 2000s and the Great Recession. The specific nature of the eligibility issue in question also appears important. There are smaller nonemployment responses for 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.

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 toward 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.

The findings in [Table A2](#) contribute to the relatively limited literature on heterogeneity in the moral hazard response to UI benefits. The lack of responses to eligibility for the youngest and oldest workers support findings from theoretical life-cycle 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. Both findings relate to ongoing policy discussions about potential justifications for so-called differentiated UI programs that provide different benefit generosity to different types of claimants (Spinnewijn, 2020).

[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 the effect of eligibility on UI receipt. Eligibility has no effect on UI receipt among claimants who report being laid off. The issue detection process may take longer in such cases, so that some claimants receive payments before the determination process begins. Complier claimants within this subgroup may also be likely to successfully appeal ineligible decisions.

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

In this section we take two steps to show that the impacts of separation-based eligibility estimated in Section 5 correspond to relatively low efficiency costs. First, we show how these responses can be combined to construct a comparable measure of the efficiency costs of any program expansion. Second, we construct this measure for two additional UI expansions commonly studied in the literature in the same data and compare the efficiency costs of the three types of expansions.

6.1 Illustration of Approach

To compare social costs across UI program expansions, we measure the government cost per dollar *mechanically* transferred to the unemployed. Following Schmieder and von Wachter (2017), we refer to this quantity as the “behavioral cost/mechanical cost” ratio (BCMC). In Appendix D.1 we derive this ratio formally and demonstrate its welfare-relevance in an optimal UI framework. Here we provide a simple visual illustration and explain our empirical implementation.

Panel (a) in Figure 7 demonstrates a hypothetical change in nonemployment duration due to an increase in weekly UI payments from $\$b$ to $\$b + db$. Note that this could represent either an intensive margin benefit increase ($\$b > 0$) or an eligibility expansion ($\$b = 0$). As a result of this program expansion, the probability that a representative job loser remains unemployed at time t , S_t , shifts upward by some arbitrary amount. The shaded red area between the two survivor curves represents the behavioral response (more time spent nonemployed), and the implications of this response for the government’s budget (e.g., additional UI benefits paid, tax dollars foregone, etc.) is the fiscal externality or efficiency cost of the expansion. The mechanical transfer that we use to scale this fiscal externality is the total dollar 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 in 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 $S_P \cdot \$b$.

6.2 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 D.1 derives this 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 (nonmechanical) costs can be seen as the consequence of some behavioral response.

A key advantage of this approach in our setting is that we estimate many components contributing to 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 parametric 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 percent tax rate to translate labor income to taxes paid.¹¹ 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.

We 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: the change in UI benefits paid on future claims, the change in SDI benefits paid, and additional UI benefits paid on the current claim as a consequence of longer nonemployment spells. The first two components are observed in the data so the corresponding causal effects are estimated directly. The third component is 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 claimant’s potential benefit duration. Mechanical costs are estimated as the mean nonemployment 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

¹¹We prefer this value, which is the U.S. 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 percent 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.

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 expansions.¹² Our sources of exogenous variation are all at the individual level, so we always condition on the realized sample of UI claimants. This ignores any effect that 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 implementation for each policy change. Appendices [D.2](#), [D.3](#), and [D.4](#) provide more detail on sample construction, estimating equations, and identification assumptions.

Separation-based eligibility. We begin by using our office-by-issue 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 nonemployment 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.

We translate the estimated effect on nonemployment duration to UI benefit costs in three steps. First, we multiply the estimated increase in nonemployment duration by $13 \cdot WBA$ (moving from quarters to weeks to dollars). Second, we cap this amount at $WBA \cdot PBD$. Third, we scale by the estimated effect of eligibility on benefit receipt. WBA and PBD are measured at the claim level. Estimated effects of eligibility on nonemployment duration and benefit receipt are from [Table 4](#). Additional details are in [Appendix D.2](#).

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. We estimate the causal effects of an increase in benefit amounts using a regres-

¹²Such 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 posting).

sion kink design (RKD). WBAs increase with prior earnings up to a maximum, creating a kink in the benefit schedule (shown in panel (a) of [Figure 8](#)).¹³

We model our outcomes of interest as polynomial functions of the prior earnings measure, which determines WBAs (the “running variable,” HQW for high quarter wages; see [Appendix D.3](#) for details), 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. (2024) 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* (in order to account for potential noncompliance) first-stage kink in WBA at the cutoff.

We estimate the total cost of a WBA increase 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 the average number of weekly UI payments made to claims at the HQW threshold. Intuitively, the mechanical transfer of a \$1 WBA increase is the number of times the claimant receives a benefit payment. The behavioral cost is treated as the difference between the total and mechanical costs. 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 D.3](#).

Benefit duration. To estimate the causal effects of an increase in benefit duration, we implement a second RKD. PBDs also increase with a function of prior earnings up to a maximum (specifically, $\frac{HQW}{BPW}$, where BPW is earnings in a four-quarter period pre-claim, described further in [Appendix D.4](#)), creating a kink in the benefit schedule (shown in panel (b) of [Figure 8](#)).¹⁴

As in the WBA RKD, total cost is estimated via 2SLS with net government expenditures as the outcome and PBD as the endogenous treatment. The mechanical transfer is the additional week of benefits for claimants who otherwise remain unemployed through benefit expiry. We estimate this

¹³This strategy follows Bell et al. (2024), 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 U.S. states in the 1970s and 80s.

¹⁴This strategy follows Landais (2015), who implements a similar design in data from five U.S. states in the 1970s and '80s.

as the fraction of claimants at the kink exhausting benefits, scaled by their WBA. The behavioral cost is treated as the difference between the total and mechanical costs. 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 D.4, results supporting the identification assumptions are in Figure D1 (smoothness of density and covariates through the cutoff).

6.3 Results

Table 6 summarizes BCMC ratios for each type of UI benefit expansion. For the RKDs, binned scatterplots 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 5 (Equation B.3).¹⁵ For all expansions, standard errors are estimated via stacked regressions, where we estimate the effects of the reform on all components of total and behavioral or mechanical costs jointly, cluster standard errors at the claim level, and use the delta method to 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.¹⁶

¹⁵We use the scalar Z approach here to simplify implementation of the stacked regressions necessary to calculate standard errors. Analogous UJIVE estimates for individual components are in Section 5. These analogous UJIVE estimates are not identical to the scalar Z estimates used here, but differences are minor and conservative for our purposes, leading to a larger BCMC estimate for at-fault eligibility than the UJIVE estimates would produce.

¹⁶Ignoring the other components implies total cost = column 2. The other components are also purely behavioral costs, 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.

6.4 Other Welfare Considerations

We cannot fully characterize the welfare implications of these different policy changes because we do not measure their social benefits and do not observe all behavioral responses that could impact the government’s budget. In this section we argue that a more complete analysis not subject to these caveats would still favor UI expansions via separation-based eligibility. Additional discussion on this topic is in Appendix E.

Other behavioral responses. More lenient separation-based eligibility criteria could induce more workers to voluntarily separate and/or file claims. Earlier empirical evidence on this question is mixed (Ragan, 1984; Solon, 1984; Venator, 2022). However, we stress that these concerns—and the existing empirical evidence—primarily deal with large nonmarginal changes to eligibility, such as by shifting large categories of quits or firings from ineligible to eligible as in Venator (2022). We believe that a marginal change to separation-based eligibility, like the one we study, is unlikely to be salient to the vast majority of indirectly affected workers.

Two plausible inflow responses to low-profile policy changes are increases in future UI receipt among claimants directly exposed to more lenient rules on an earlier claim and spillovers onto peers (e.g., friends or coworkers of the directly affected claimants who learn about the expansion via word of mouth). The former response is included in our efficiency cost estimates.

We test the second hypothesis by constructing a sample of indirectly affected coworkers and implementing our office-issue design for various measures of peer utilization. We focus on a subsample of firm-quarters that have only one claim in our main sample and where the firm has a single establishment. The construction of this sample is described in more detail in Appendix E.1. Figure 12 presents results from this exercise and shows no evidence of an increase in peer UI claiming behavior or payment receipt. We view these results as suggestive evidence that any inflow effects of extending UI eligibility marginally at-fault claimants is small.

Consumption-smoothing benefits. The social benefit of a UI expansion is the utility gain from improved consumption smoothing. We believe that consumption-smoothing benefits are likely to be larger for separation-based eligibility expansions than other expansions we study for two reasons. First, diminishing marginal utility implies that the first dollar of insurance provides the greatest consumption-smoothing value. Second, we have shown in Table 1 that marginally at-fault claimants have lower incomes than other UI claimants. Appendices E.2 and E.3 address two 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 marginally at-fault job losers. Notably, we show that unemployed workers in the Panel Study of Income Dynamics (PSID) who quit their last job have meaningful consumption drops.

Administrative costs. The government must fund the technological infrastructure and staff time necessary to conduct separation-based eligibility 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 E.4 we use state-year aggregate data on appeals and appeals processing costs to provide a back-of-the-envelope 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. This is a meaningful portion of the behavioral costs shown in Table 6.

6.5 Why Are At-Fault Eligibility Expansions Less Socially Costly?

If claimants affected by one policy change differ 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 separation-based eligibility in Section 5.

We have also shown in Section 3.1 that claimants exposed to separation-based 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 6.2. Table 5 displays sample and complier means for the three research designs described in Section 6.2. For the eligibility IV, complier means are estimated following Frandsen et al. (2023) as in Section 5. 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 toward 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 means for the covariates in Table 5 match the eligibility 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. (Propensity score reweighting cannot accomplish this 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 running variable is a prior earnings measure and the LATE estimated by the RKD is local to the relatively high earnings cutoff. This motivates our second approach, which reestimates BCMCs in subsamples that are meant to be more comparable across policy changes. For the eligibility expansion, we reestimate the BCMC in subsamples defined by deciles of prior earnings. For the RKDs, we reestimate 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 D).

Panel (a) of Figure 11 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 percent of the \$0.48 gap between the baseline WBA and at-fault BCMC estimates, and -5 percent of the PBD and at-fault gap (reweighting widens the gap between PBD and at-fault). Reweighting and restricting the sample explains a much more meaningful portion of the gap—64 percent of the at-fault vs. WBA gap, and 22 percent of the at-fault vs PBD gap.

Panel (b) addresses differences in prior earnings amounts between three complier groups by reestimating the at-fault BCMC within prior earnings deciles (quarterly averages in the two years pre-claim). BCMC ratios are increasing in prior earnings, and this can 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 lowest deciles are negative and the estimate for the decile which includes the WBA RKD cutoff prior earnings is slightly larger than the baseline WBA BCMC.

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 who 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 relative efficiency of an at-

fault eligibility expansion can help policymakers to understand whether our results are likely to extend to their setting. If they are considering extending eligibility to claimants with income levels similar to our marginally at-fault claimants, then our results are more likely to hold. Our results also support targeting other expansions toward low-income claimants. For example, rather than increasing maximum benefit amounts (as in our benefit amount RKD)—which only affects relatively high earners—policymakers could increase benefit levels for lower earning claimants only (e.g., via a replacement rate increase for a certain range of prior earnings).

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 determination process itself could lengthen unemployment spells.

7 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 that these employment responses are particularly small in the context of a theoretically motivated measure of the efficiency costs of benefit expansions. This holds relative both to estimates from the existing UI literature and 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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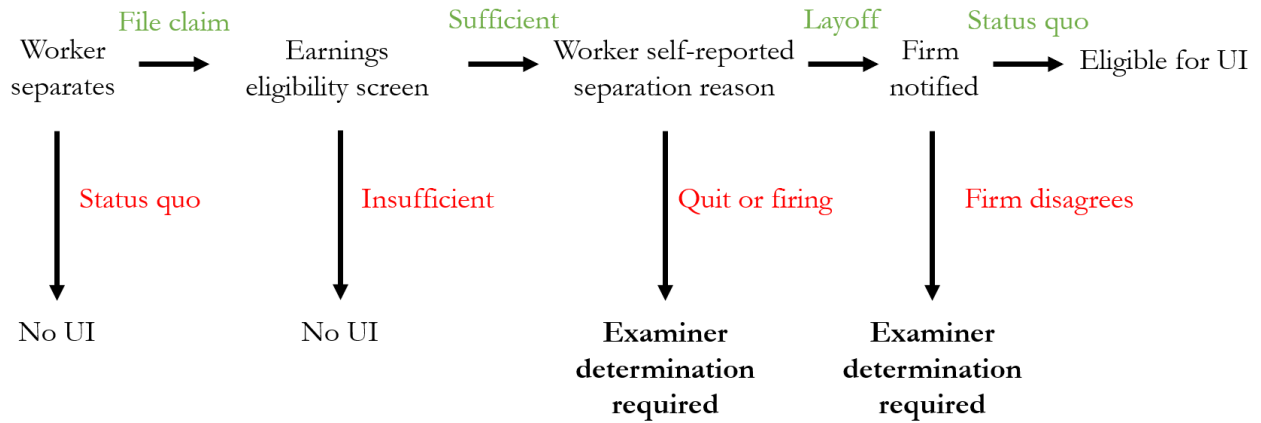
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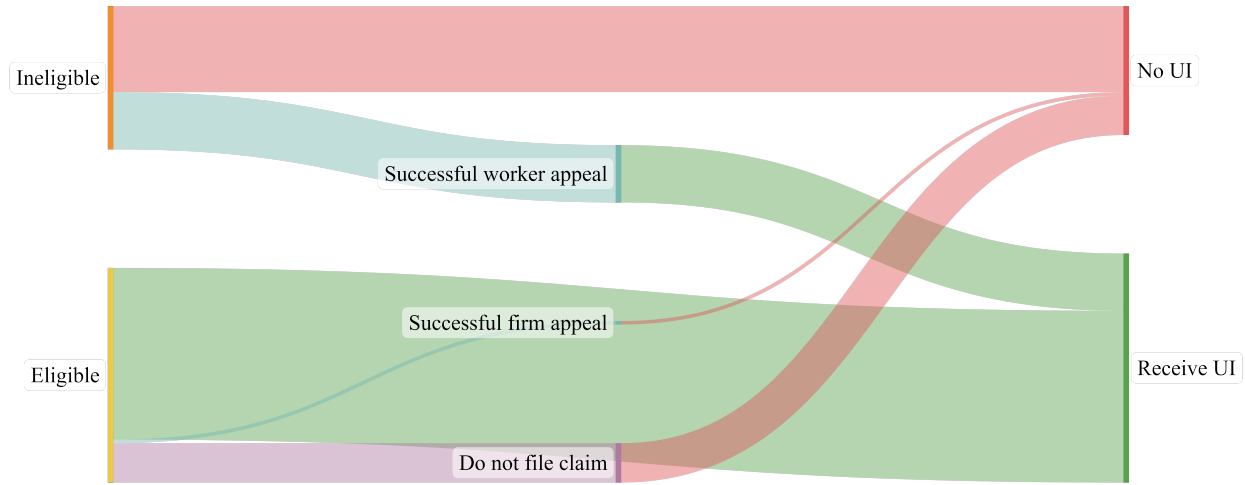
Figures and Tables

Figure 1: Selection Into Separation-Based Eligibility Determinations



Notes: This chronological flowchart describes how and when at-fault eligibility issues arise on UI claims. Arrows labeled with green text indicate steps leading to UI benefit receipt, while arrows labeled with red text indicate steps that put UI benefit receipt in jeopardy.

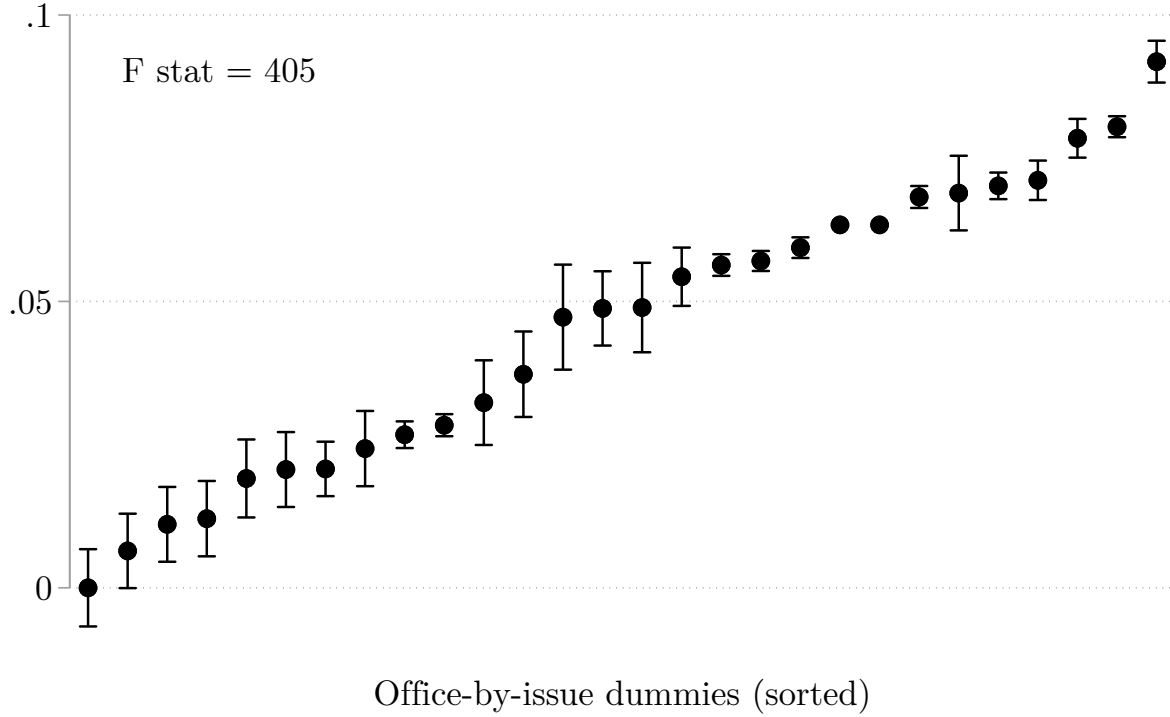
Figure 2: Reasons for UI Benefit (Non-)Receipt by Eligibility Approval Status



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 California UI microdata.

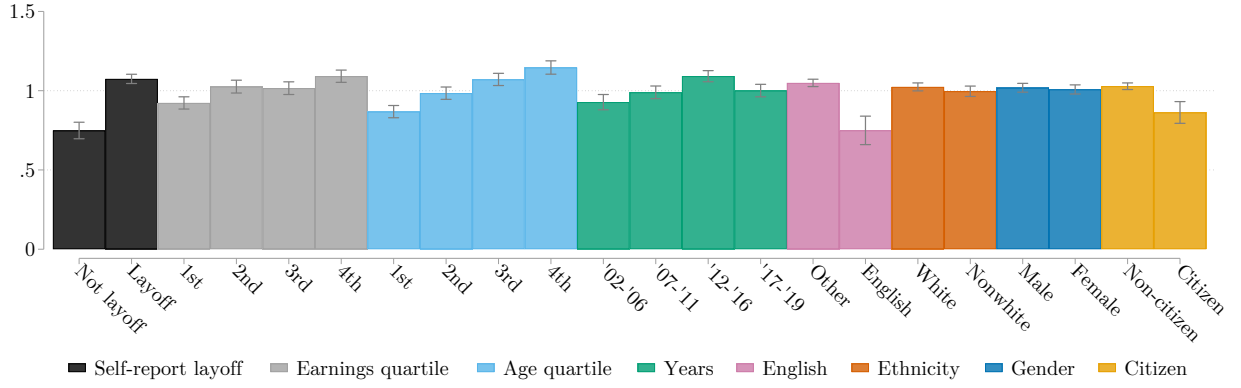
Figure 3: Variation in Eligibility Approval Propensities across Office-by-Issue Pairs

γ estimates (rescaled) and 95% CIs



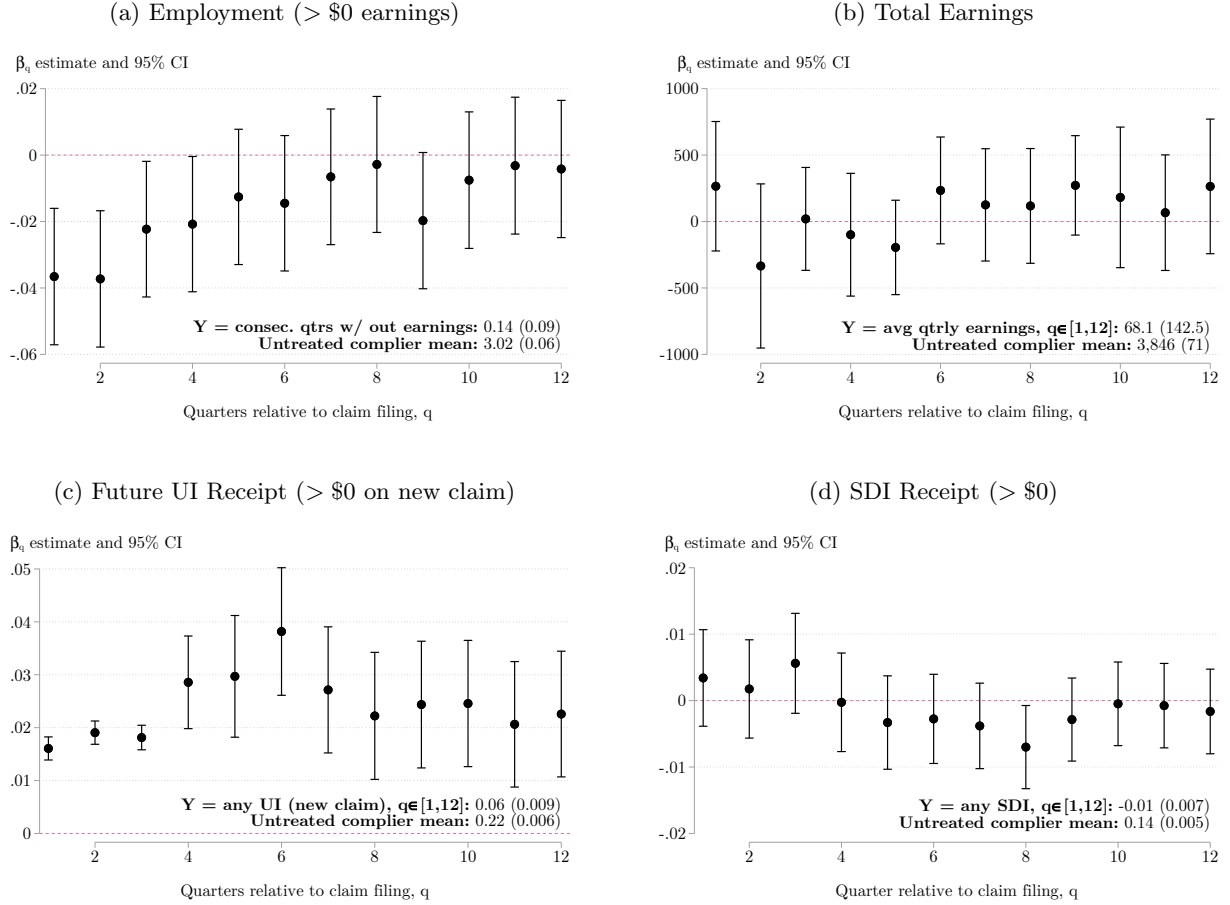
Notes: This figure plots office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval. Coefficients are sorted along the x -axis in ascending order and normalized so that the smallest fixed effect is 0. Standard errors are cluster robust at the claimant level. 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



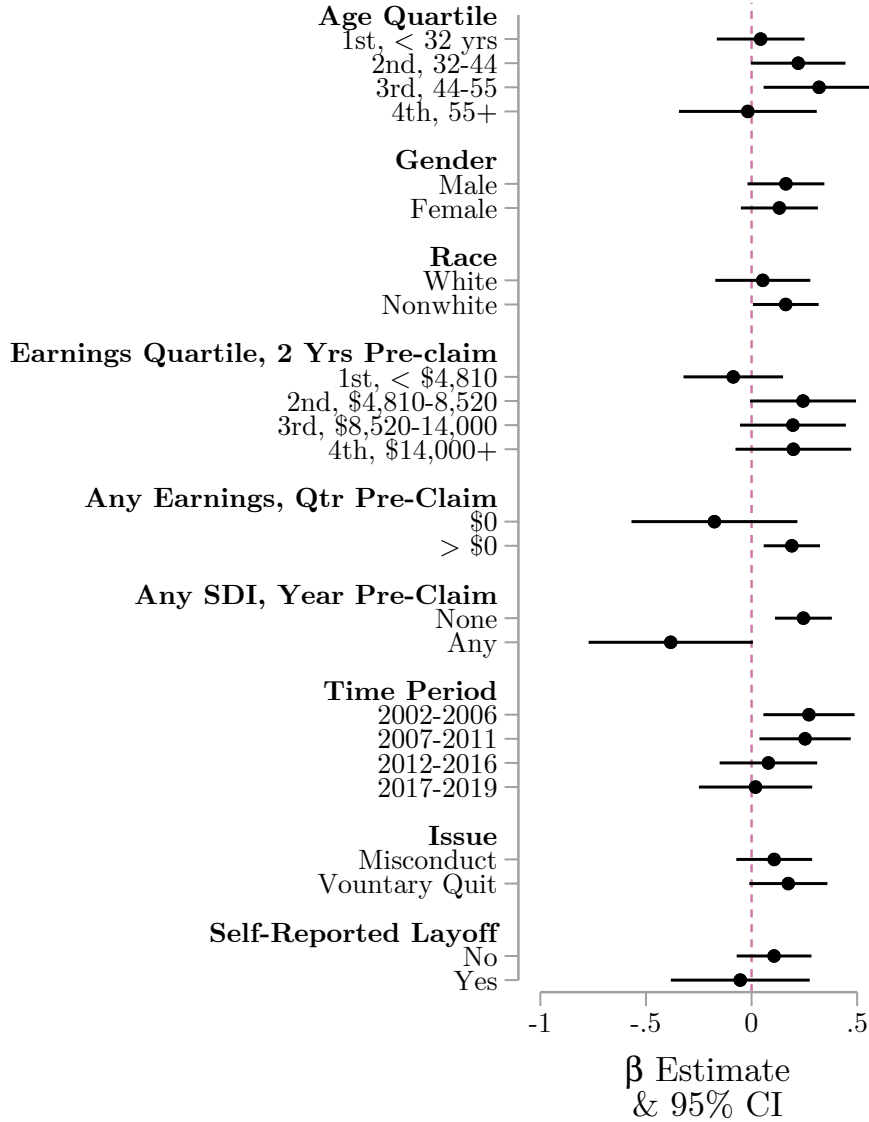
Notes: Each bar represents a separate regression of the claimant's own eligibility decision D_{it} on their assigned office-by-issue's overall leave-one-out residualized eligibility leniency \tilde{Z}_{it}^j (described in more detail in Appendix B) within a given subsample. Each color represents a different 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 percent confidence intervals.

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



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.

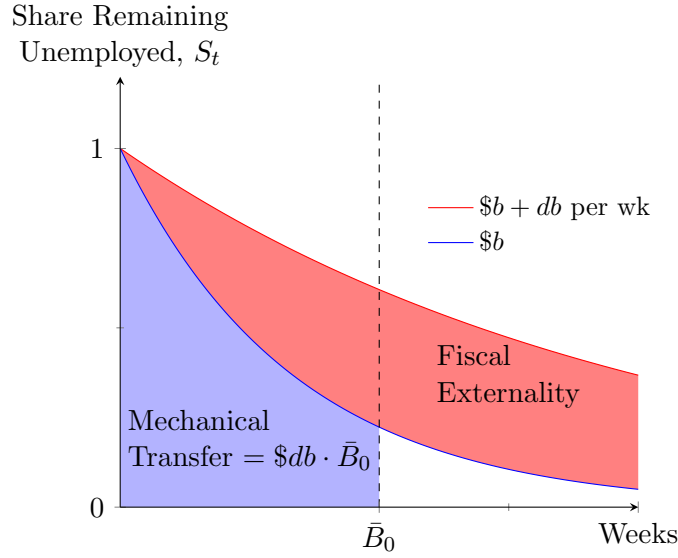
Figure 6: Heterogeneity in Nonemployment Duration Effects of At-Fault Eligibility



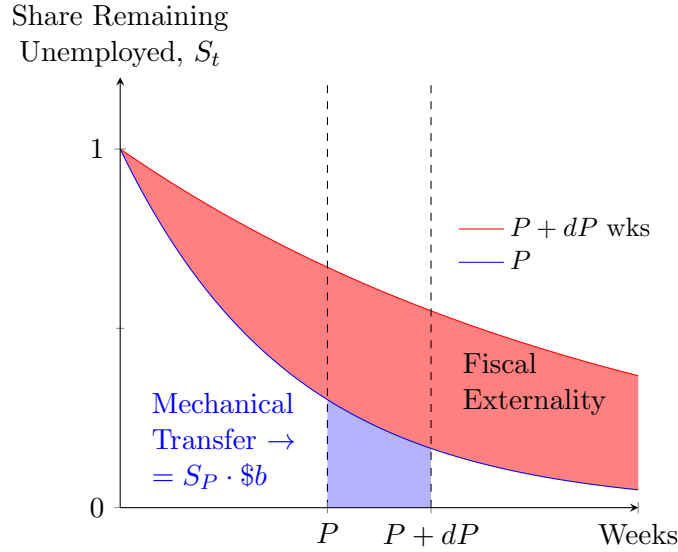
Notes: $\hat{\beta}$ estimates from separate quarterly level IV specifications where office-by-issue dummies instrument for eligibility (see Equations 1 and 2). Each estimate is from a subset of our main analysis sample. Estimation is via UJIVE and standard errors are cluster-robust at the claimant level.

Figure 7: Graphical Illustration of BC/MC Ratio for Different UI Program Expansions

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

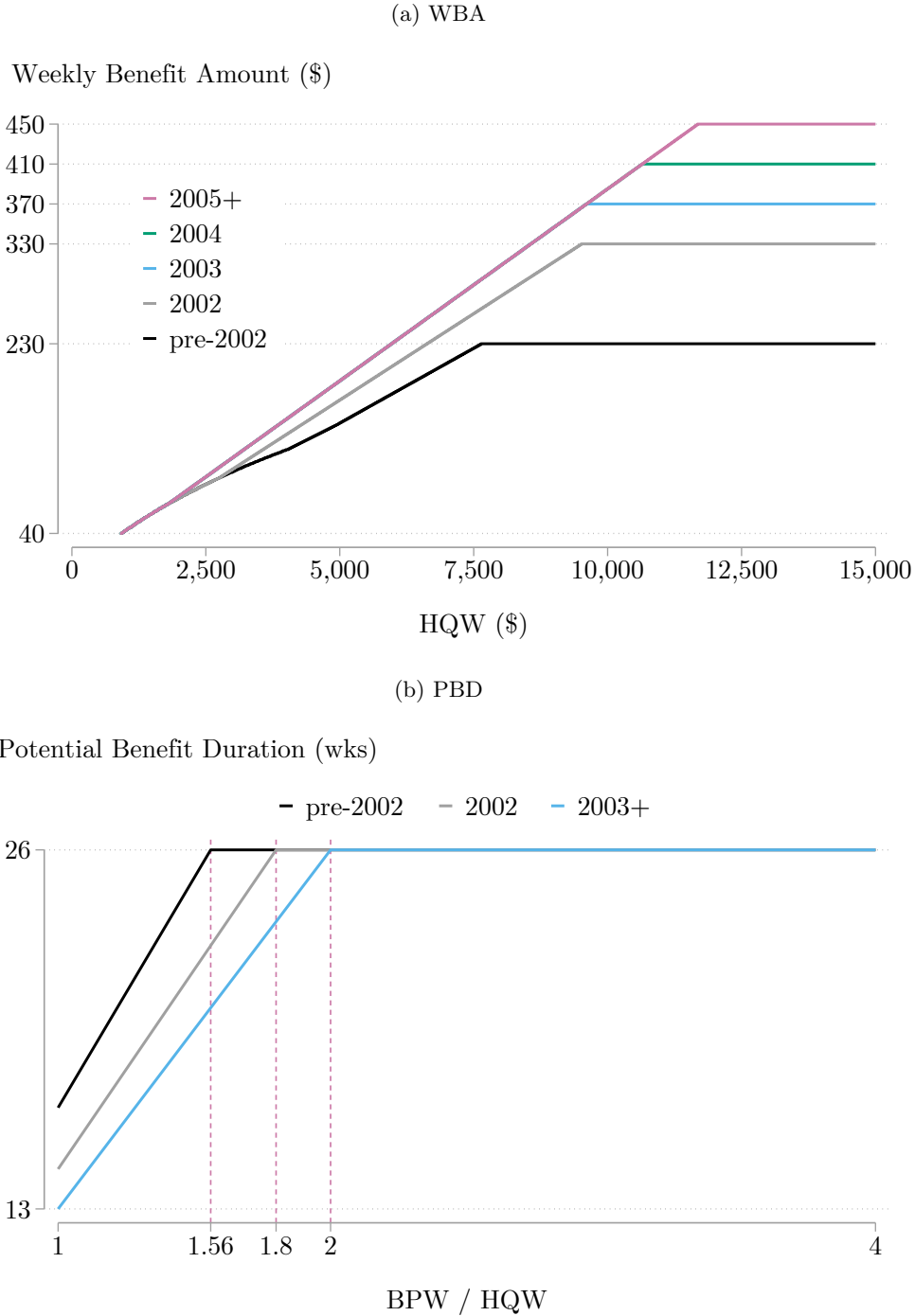


(b) Benefit Duration Expansion



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.

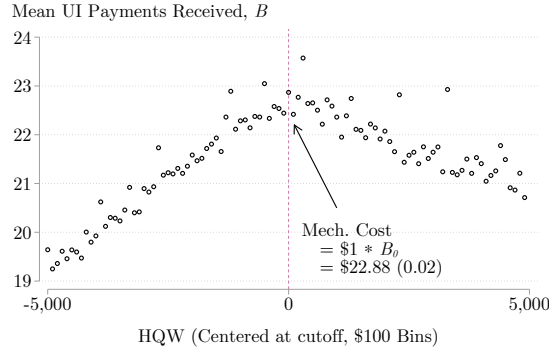
Figure 8: WBA and PBD Benefit Schedules



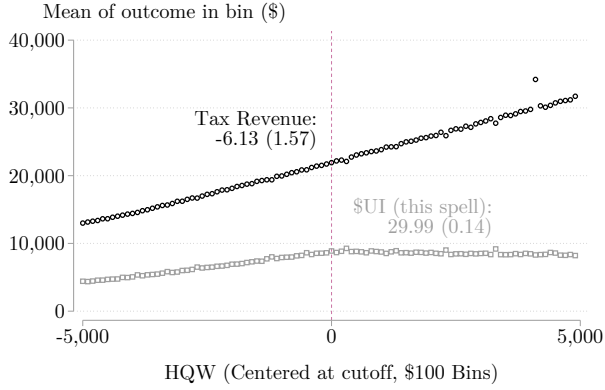
Notes: The top panel (a) displays the functions which determine weekly benefit amounts (WBAs) in California during the years covered by our sample. In each time period, WBAs are set to replace some portion of earnings in the highest earning quarter of a claimant's base period ("High Quarter Wages" or 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

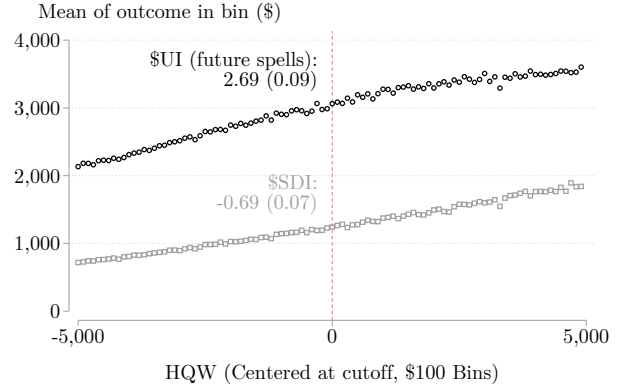
(a) Mechanical Cost



(b) Tax Rev. and Current Spell UI

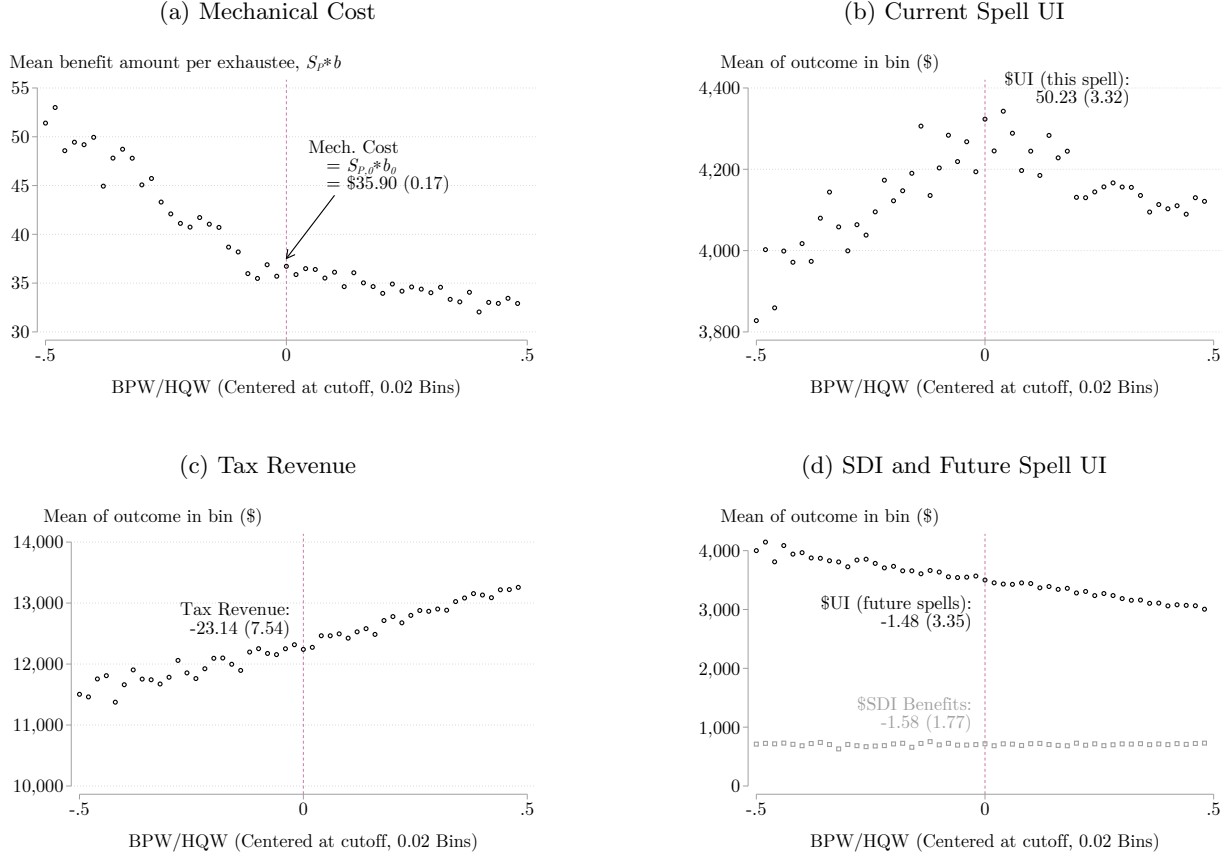


(c) SDI and Future Spell UI



Notes: Each panel is a binned scatterplot of the weekly benefit amount analysis sample described in [Table 5](#) and [Appendix D.3](#). 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 6.2](#).

Figure 10: PBD RKD BCMC Components



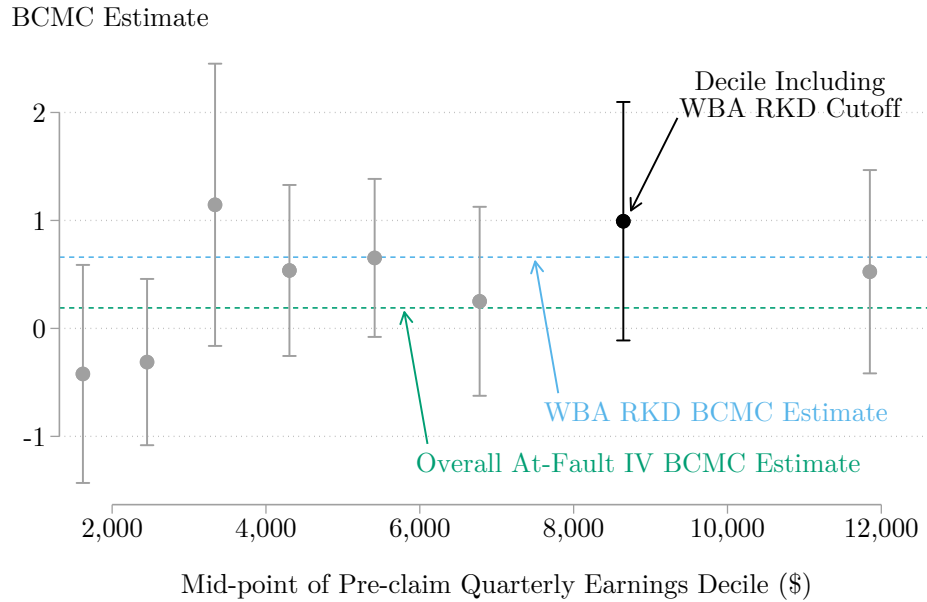
Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in [Table 5](#) and [Appendix D.4](#). The running variable is the ratio of base period wages to high-quarter wages relative to the year-specific kink and the bin width is 0.02. The outcome for panel (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 6.2](#).

Figure 11: Explaining BCMC Differences across Margins

(a) Reweighting and/or limiting RKD samples

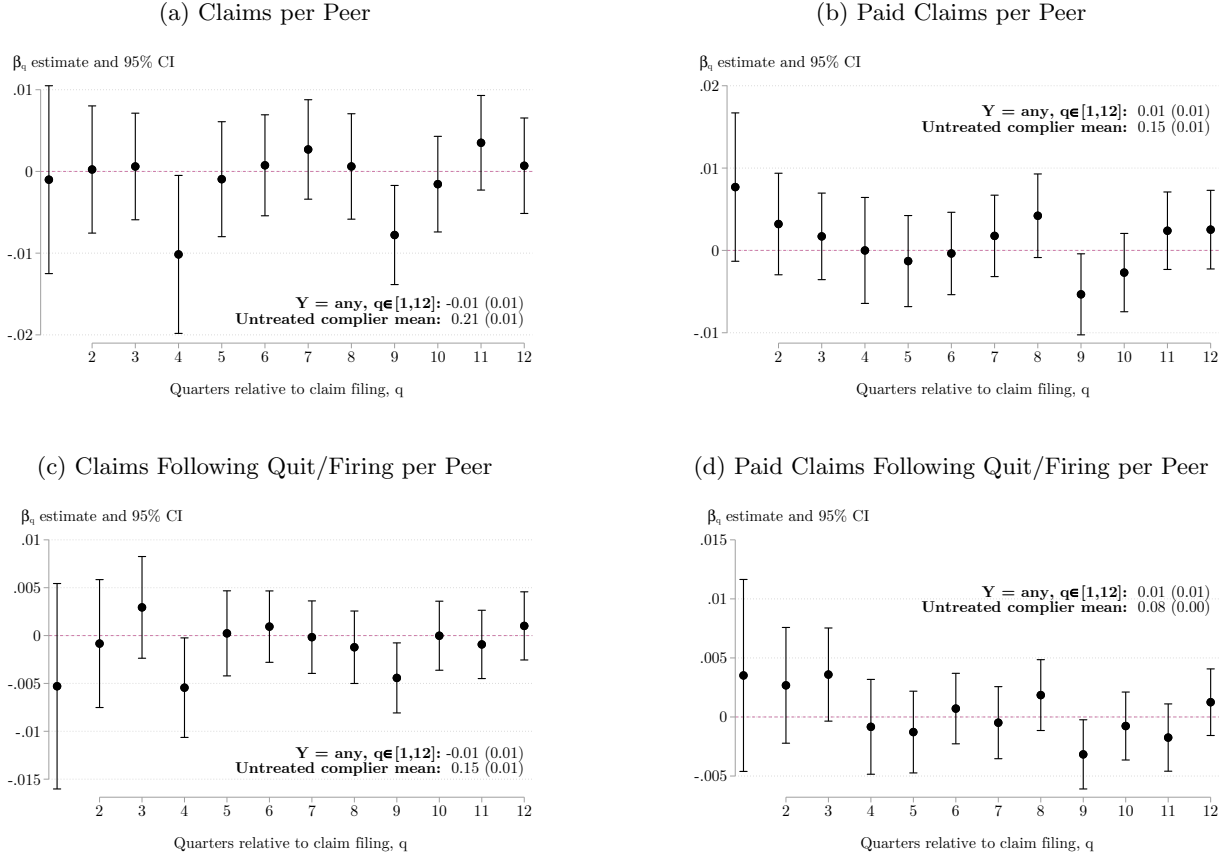


(b) At-Fault IV BCMC estimates by pre-claim earnings decile



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 the 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



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).

Table 1: UI Receipt and Ineligibility among the Unemployed

	(1)	(2)	(3)	(4)	(5)
	All Claims	Monetarily Eligible	+ Separation Issue	+ Initial Denial	+ No Payments
<i>N</i> (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
<i>N</i> (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)			Nonemployment 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 regular initial claims with separation-based eligibility issues 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
Nonwhite	0.63	0.65	0.64	0.61	0.70	0.70
Citizen	0.92	0.92	0.87	0.88	0.85	0.85
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)
Any Earn, qtr pre-claim	0.87	0.86	0.95	0.95	0.89	0.88
Any SDI, qtr pre-claim	0.07	0.07	0.04	0.04	0.04	0.04
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 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. HQW = high quarter wages, the highest quarterly earnings amount in the claimant's base period. BPW = total earnings in the base period.

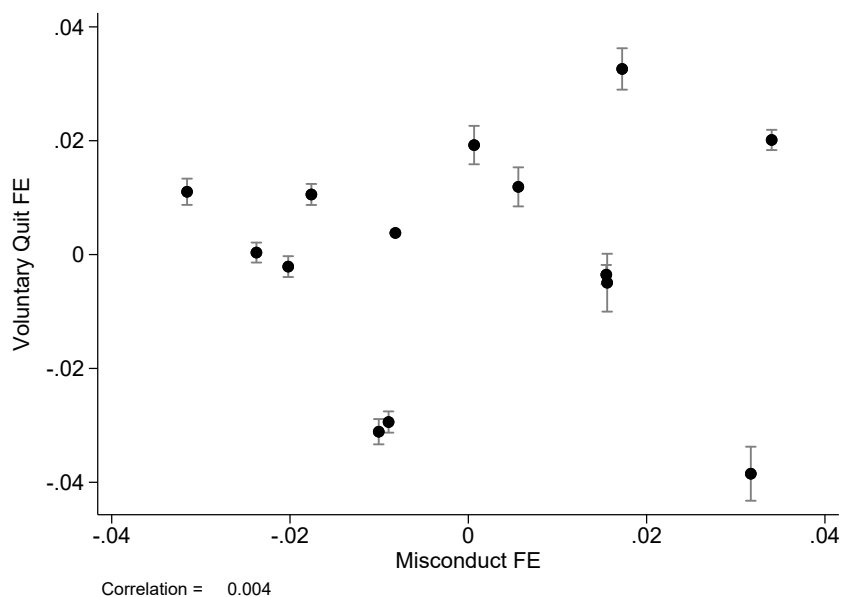
Table 6: Decomposition of Benefit Expansion Costs for Different UI Policy Margins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Components of Total Cost				Total cost	BC	MC	$\frac{BC}{MC}$
	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 CA 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 D and Section 6.2.

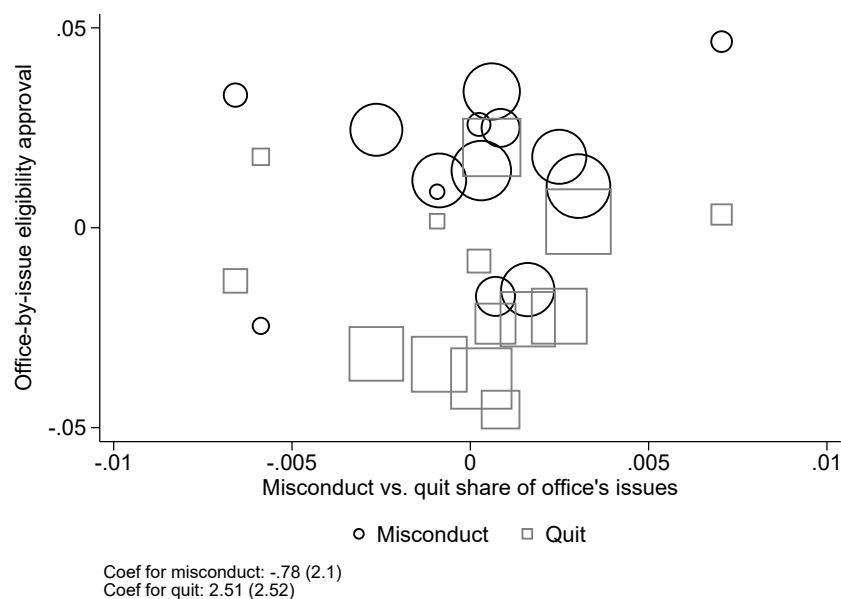
A Additional Tables and Figures for Office Assignment Research Design

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



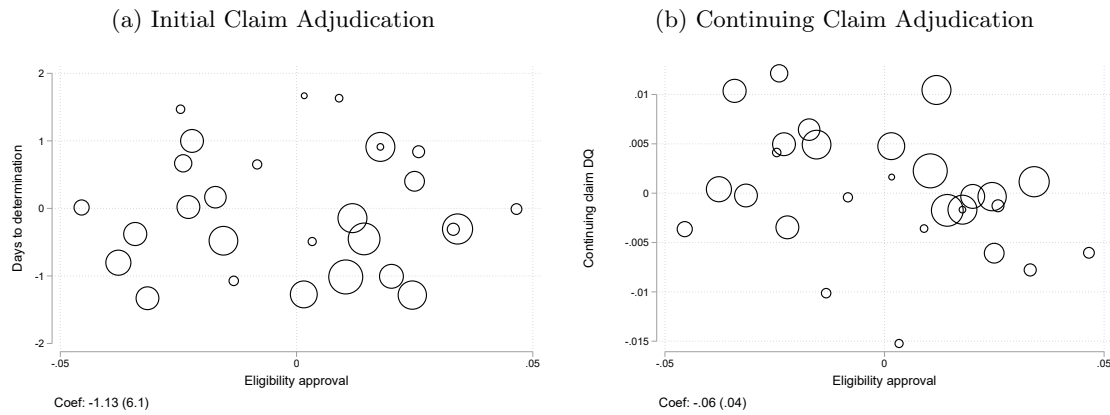
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is a processing office. The coordinates are office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval; coefficients are normalized 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



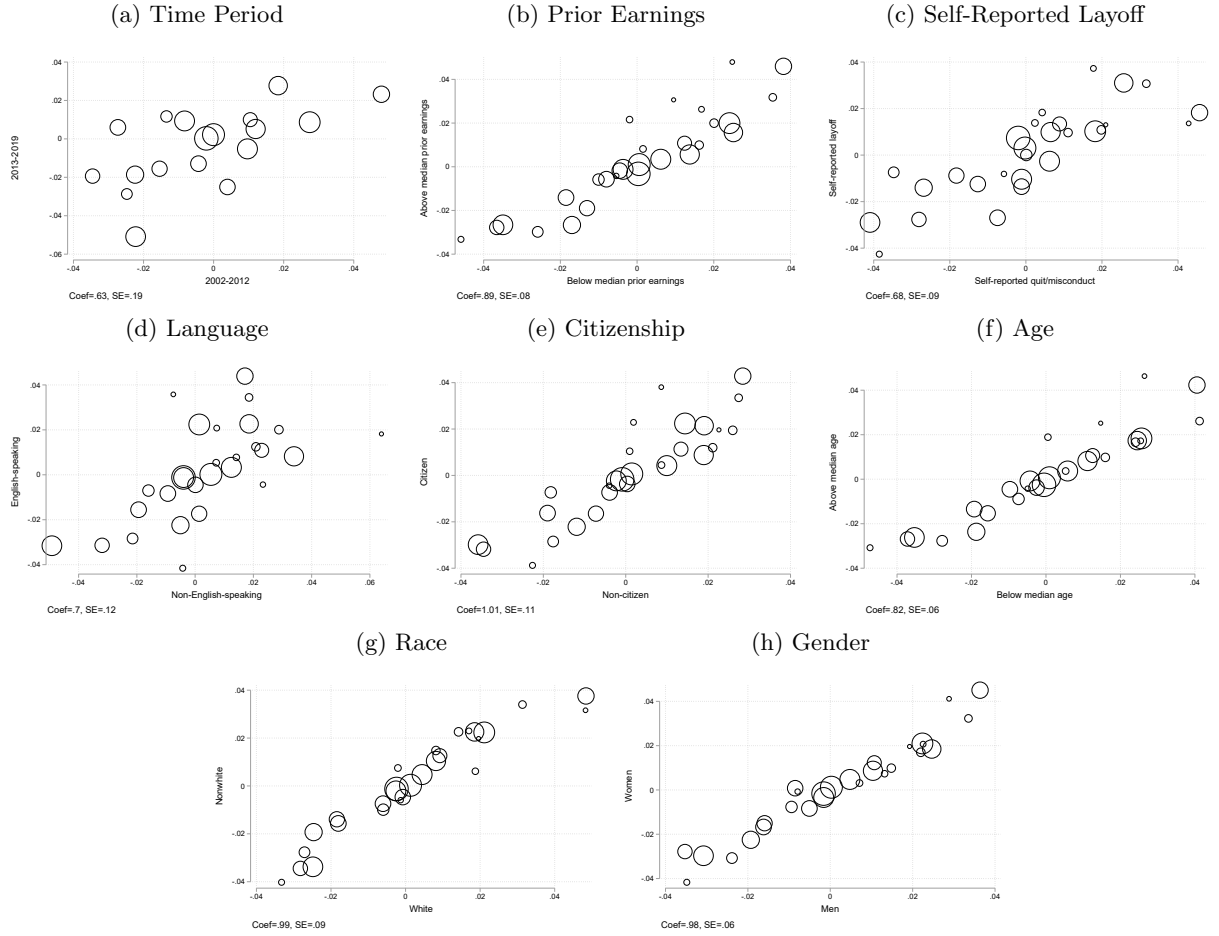
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each marker is a office-by-issue pair. The y -coordinates are office-by-issue coefficients γ estimated from Equation 2 where the outcome D is eligibility approval; coefficients are 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



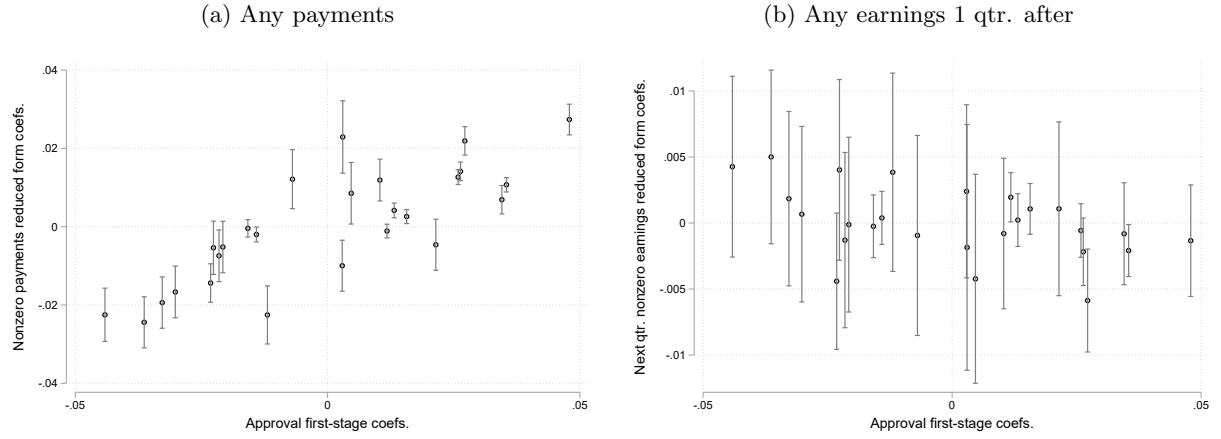
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates. The x -axis indexes office-by-issue coefficients 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



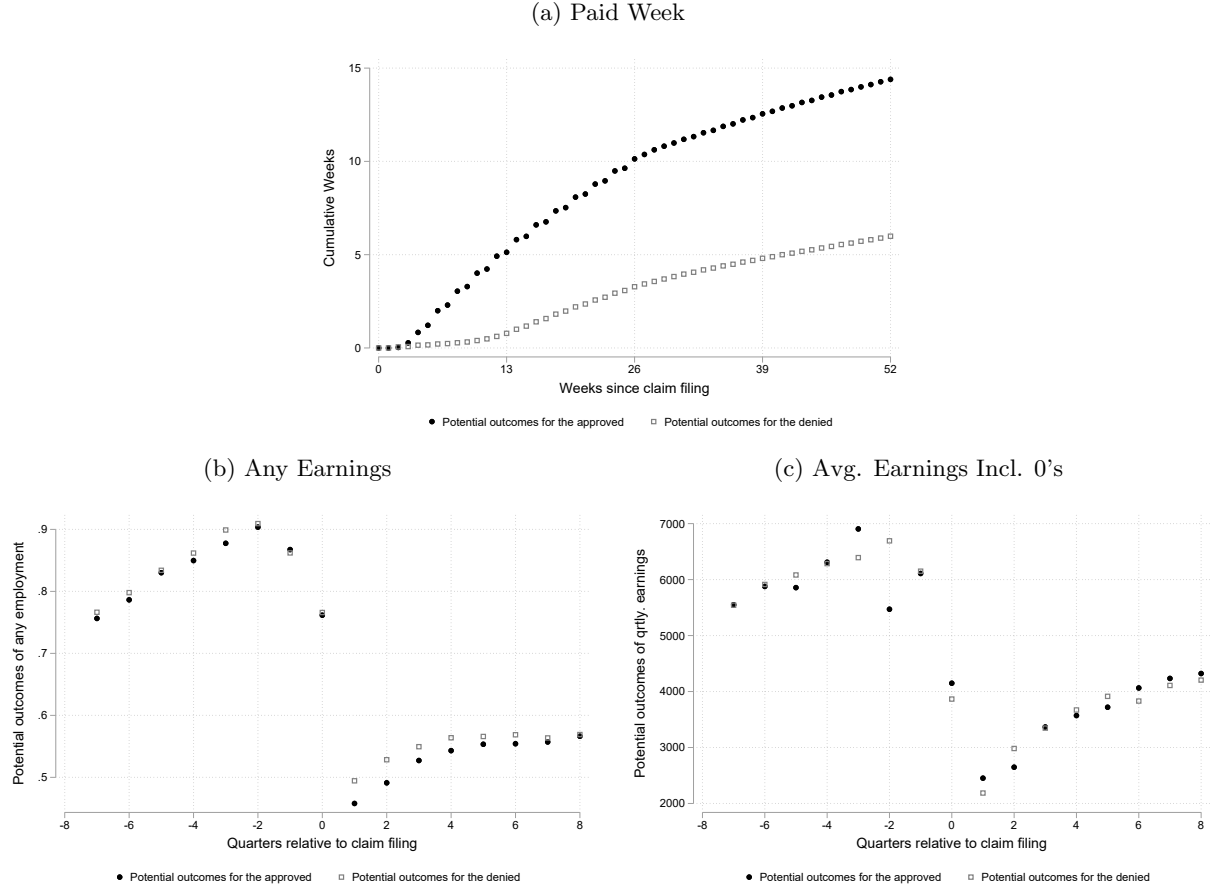
Notes: Each panel displays group-specific separation-based eligibility approval propensities at the office-by-issue level. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates and the coordinates are eligibility approval 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



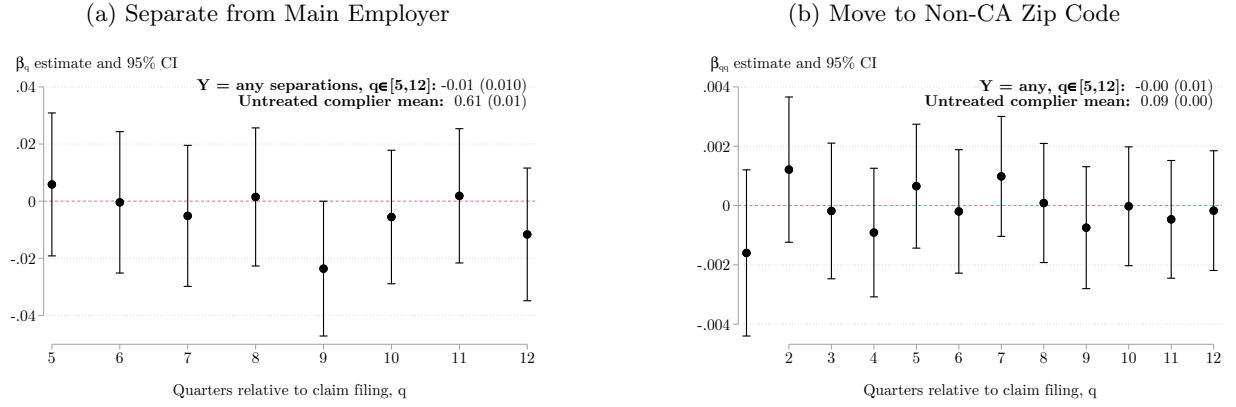
Notes: Each panel presents reduced-form coefficients for office-by-issue pairs on the y -axis and first-stage coefficients for office-by-issue pairs on the x -axis. Both sets of coefficients come from Equation 2, where the outcome is eligibility approval for the first-form and the variable in the graph 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



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



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), $N = 3.9$ m 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)
<i>tf</i> SE	[0.11]	[0.11]	[0.13]	[0.17]	[0.09]	[0.09]	[0.11]	[0.08]
<i>F</i>	117	121	102	86	196	214	151	261
<i>N</i>	1.51m	1.86m	1.81m	1.73m	3.49m	3.41m	2.54m	4.38m
	Prior Earnings Quartile				Employed Qtr. Before Claim		SDI → 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)
<i>tf</i> SE	[0.12]	[0.13]	[0.13]	[0.14]	[0.22]	[0.07]	[0.07]	[0.22]
<i>F</i>	127	100	101	103	50	362	371	51
<i>N</i>	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)
<i>tf</i> SE	[0.11]	[0.11]	[0.12]	[0.14]	[0.09]	[0.09]	[0.09]	[0.19]
<i>F</i>	241	244	118	92	415	385	231	48
<i>N</i>	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 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.

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

	Age Quartile				Gender		Race	
	1 st	2 nd	3 rd	4 th	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)
<i>tf</i> SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.01]
<i>F</i>	112	121	102	86	196	214	151	261
<i>N</i>	1.51m	1.86m	1.81m	1.73m	3.50m	3.42m	2.55m	4.39m
	Prior Earnings Quartile				Employed Qtr. before Claim		SDI → UI	
	1 st	2 nd	3 rd	4 th	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)
<i>tf</i> SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.01]	[0.01]	[0.03]
<i>F</i>	127	100	101	103	50	362	371	51
<i>N</i>	1.73m	1.73m	1.73m	1.73m	0.88m	6.06m	6.11m	0.83m
	Time Period				Issue		Self-Reported Layoff	
	2002–06	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)
<i>tf</i> SE	[0.01]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.03]
<i>F</i>	241	243	118	92	417	383	231	48
<i>N</i>	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 Monotonicity and Construction of a Residualized Leniency Measure

The first-stage Equation 1 used in our at-fault eligibility IV design presents two monotonicity concerns: the assumed homogeneity of γ and the included control variables $\mathbf{X}_{it(s)}$.

The assumed homogeneity of the γ s could result in a violation of the monotonicity assumption if a given office-by-issue pair is more lenient than another office-by-issue pair with one type of claimant but relatively less lenient with another type. Frandsen et al. (2023) and Chan et al. (2022) show that *average monotonicity* is a sufficient monotonicity condition to recover a LATE in an IV design. Informally, this requires that office-by-issue-specific eligibility approval and overall eligibility approval are positively correlated for each claimant. To formally define average monotonicity, suppose, for simplicity, that we have unconditional random assignment so that we can omit the fixed effect controls from Equation 2. Formally, the average monotonicity condition is:

$$\sum_{j \in J} \lambda_j (p_j - p) (D_i(j) - \bar{D}_i) \geq 0 \quad \forall i \quad (\text{B.1})$$

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 $\bar{D}_i := \frac{1}{|J|} \sum_{j \in J}$ 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:

$$A_{it(os)}^* = A_{it(os)} - \mathbf{X}_{it(s)}' \mu \quad (\text{B.2})$$

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 :

$$\tilde{Z}_{it}^{os} = \left(\frac{1}{n_{os} - 1} \right) \left(\sum_{k(os)} \sum_{t(oj)} A_{kt(os)}^* - A_{it(os)}^* \right) \quad (\text{B.3})$$

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

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. 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. (2024) 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. Both of these issues can be addressed with a fully saturated specification, as discussed in Section 4.

C Separation-Based Eligibility Effects Using Examiner Assignment

This section supplements our primary IV research design based on processing office assignment between 2002 and 2019 with a complementary instrument variables research design based on examiner assignment between 2017 and 2019. Its structure of it mirrors that of Section 5. 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.

C.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 :

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

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

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 C.2 and C.1 consists of only algorithmically scheduled claims between 2017 and 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 percent of the sample.

Including language, office, issue, and time fixed effects is motivated by the assignment mechanism discussed in Section 2.2. 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 percent 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 percent to 4 percent 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.

C.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 C.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 C.2:

$$A_{it(jlos)}^* = A_{it(jlos)} - \mathbf{X}_{it(los)}' \mu - \eta_t \quad (\text{C.3})$$

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

$$\tilde{Z}_{it}^{js} = \left(\frac{1}{n_{js} - 1} \right) \left(\sum_{k(j,s)} \sum_{t(j,s)} A_{kt(jlos)}^* - A_{it(jlos)}^* \right) \quad (\text{C.4})$$

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 C1 empirically supports. Specifically, we

calculate the residualized leave-one-out approval rate \tilde{Z}_{it}^j from Equation C.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 preexisting characteristics. The only statistically significant difference across the two groups—self-reported layoff and eligibility issue type—are economically small in magnitude.

Table C1: Claimant Balance across Examiner Leniency

	(1)	(2)	(3)	(4)
	Ad hoc		Algorithmic	
	scheduling		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
<i>N</i>	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.

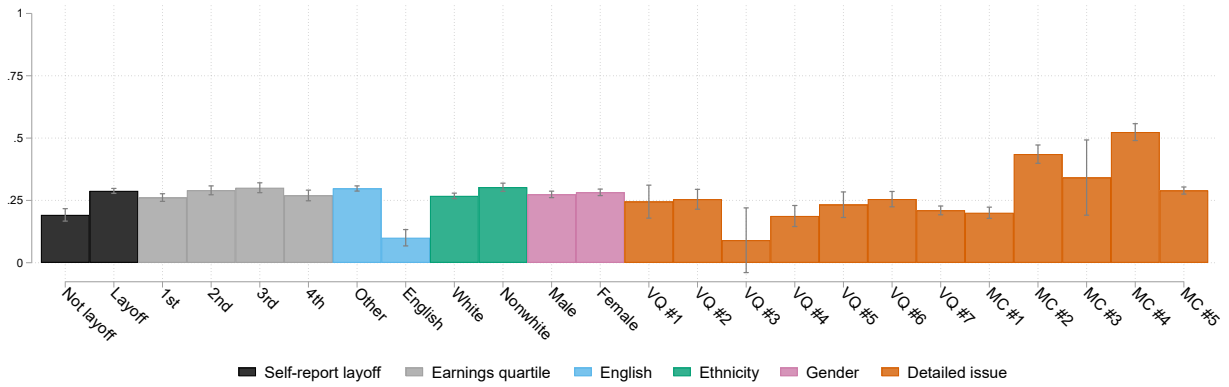
The second identification assumption—excludability—requires that the only effect that exam-

iners 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.

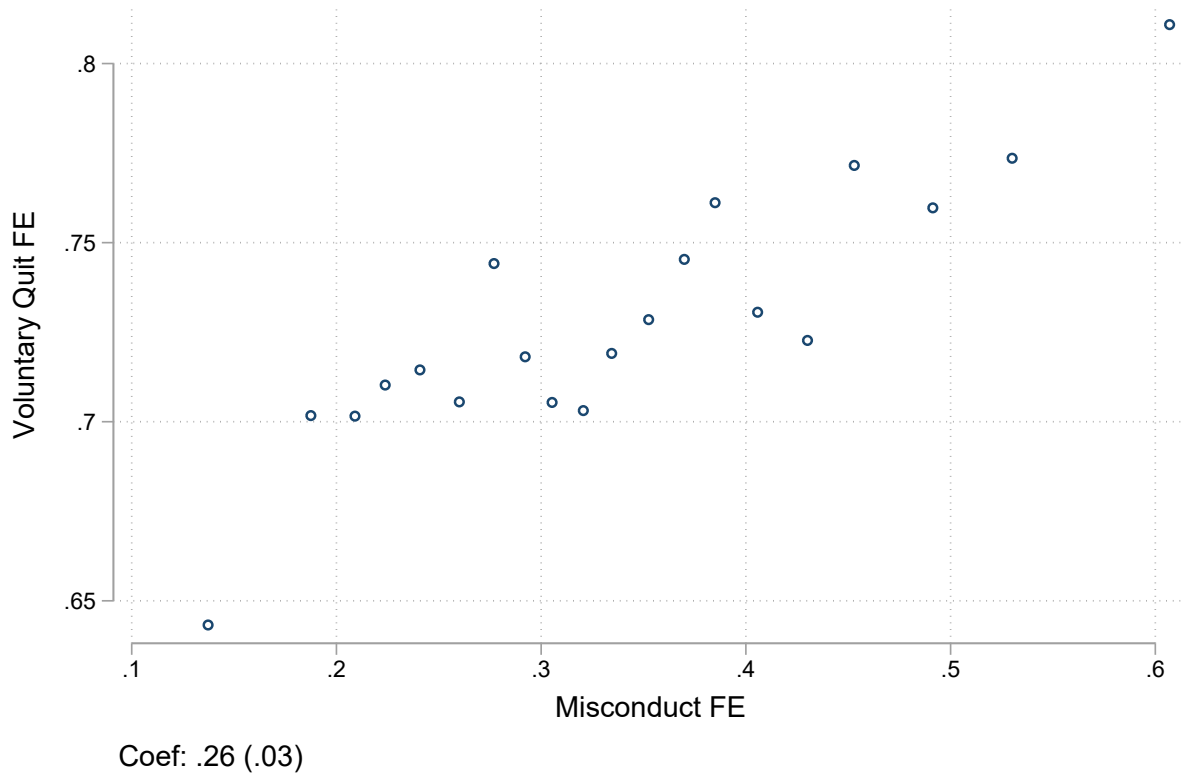
The third identification assumption—first-stage relevance—requires that the examiner assignment be predictive of the endogenous treatment. We directly test this by estimating the first-stage regression equation Equation C.2 and testing the joint significance of the examiner dummies. The first-stage F -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 percent 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 percent.

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 5, Figure C1a confirms the first-stage relationship is positive within various claimant subsamples, which is a testable implication of average monotonicity.

Figure C1: 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 \hat{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.

C.3 Results

As in the office-based research design, the examiners-based research design finds that eligibility 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 C2](#) shows the treatment effect on receiving any payments, having any employment in the subsequent quarter, and average earnings in the subsequent quarter. The tF -adjustment we employ due to the weaker first-stage relationship is nontrivial; it almost doubles the confidence intervals. Even so, the eligibility effects are highly statistically significant.

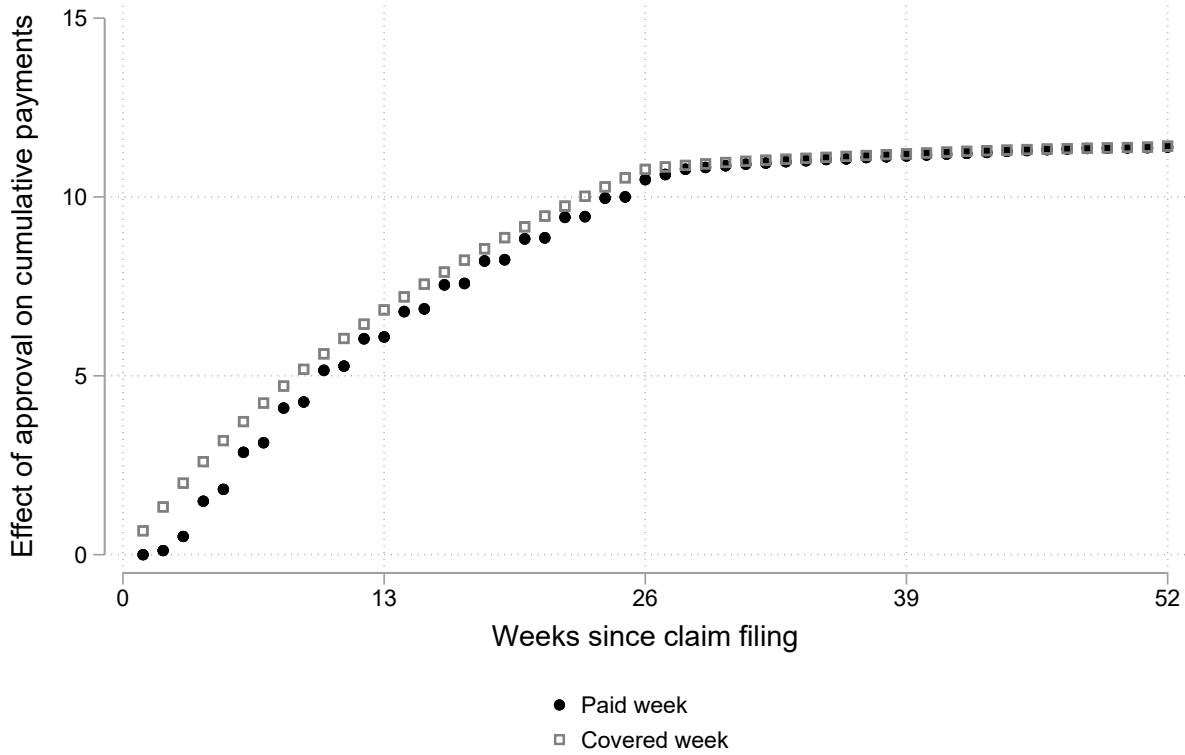
Table C2: Effects of Initial Eligibility Approval

	UI Benefits (this spell)			Nonemployment 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)
<i>tf</i> 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)
<i>tf</i> 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 C.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 C2 replicates Figure A6 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 C2: Dynamic Impacts of Eligibility Approval on Cumulative Benefit Receipt

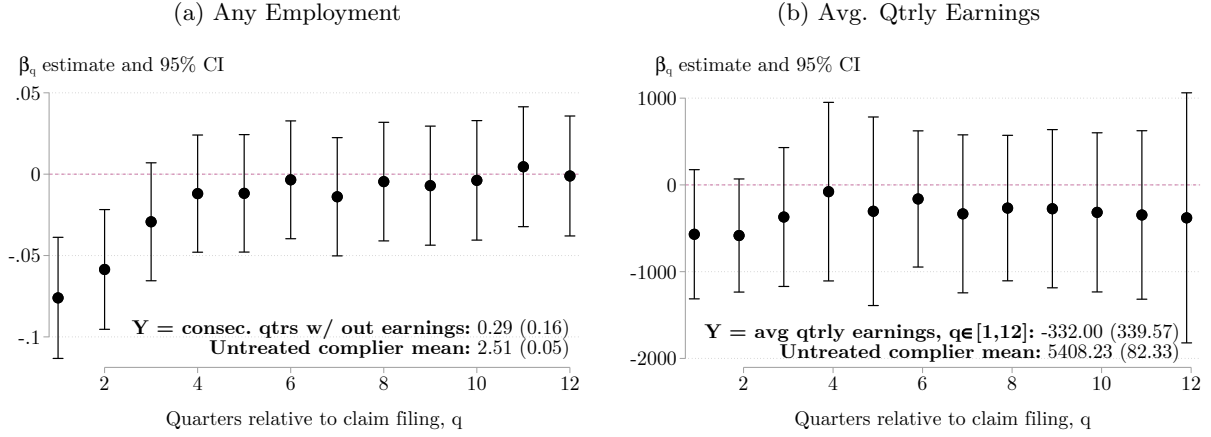


Notes: This figure displays coefficients from separate regressions of the form in Equations C.1 and C.2, where the outcome is a measure of cumulative payments as of that week. A paid week refers to the calendar week the payment is made, while a covered week refers to the week of unemployment to which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

Figure C3 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 C3: Dynamic Impacts of Eligibility Approval on Employment



Notes: Both panels display coefficients from separate UJIVE IV regressions of the form in Equations C.1 and C.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.

D 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. We begin with a theoretical derivation of the BCMC ratio used in Section 6, which closely follows Schmieder and von Wachter (2017) and Lee et al. (2021). We then move to additional details on the research designs used to estimate BCMC ratios for each of the three UI expansions that we study.

D.1 Theoretical Motivation for BCMC Ratio

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 $d\theta_j$ 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:

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

The ratio of the second and third terms in Equation D.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 $\tau(\theta)$:

$$\frac{d\tau}{d\theta_j} = - \frac{\frac{\partial G(\mathbf{Y}(\theta, \tau); \theta, \tau)}{\partial \theta_j}}{\frac{\partial G(\mathbf{Y}(\theta, \tau); \theta, \tau)}{\partial \tau}} = - \frac{\frac{dG}{d\theta}}{\frac{dG}{d\tau}} \quad (\text{D.2})$$

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

$$\begin{aligned}\frac{dV}{d\theta_j} &= U_1 \cdot \frac{\partial B}{\partial \theta_j} + U_2 \cdot \frac{\partial T}{\partial \tau} \cdot \frac{d\tau}{d\theta_j} \\ &= U_1 \cdot \frac{\partial B}{\partial \theta_j} - U_2 \cdot \phi \cdot \frac{dG}{d\theta_j}\end{aligned}\tag{D.3}$$

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 D.2 and denoting $\phi := \frac{\partial T}{\partial \tau} \cdot \frac{d\tau}{d\theta_j}$ 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 $\frac{dG}{d\theta_j}$ 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 D.3, we substitute in the total government cost decomposition from Equation D.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:

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

where $\frac{dW}{d\theta_j} := \frac{\frac{dV}{d\theta_j}}{U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}}$ 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 $\frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta_j}}$ 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 $d\theta_j$: 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.¹⁷

While the above model is quite general, one plausible generalization worth noting is allowing for effects of UI policy reforms $d\theta_j$ independent of effects on taxes and transfers. For the worker, administrative hassles could directly affect utility. If so, the left-hand side of Equation D.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 ($\frac{dW}{d\theta_j} > \frac{dW}{d\theta_k}$). Consequently, a budget balanced policy reallocation away from k toward j ($d\theta_k < 0 < d\theta_j$) 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

¹⁷The 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.

non-UI policy dimensions to generate one (Hendren, 2020).

D.2 Separation-Based Eligibility

Estimating UI costs implied by nonemployment effect of eligibility. To translate the estimated effect on nonemployment 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.

Potential sources of measurement error. Our strategy for translating nonemployment durations to dollars in UI benefits received implies two sources of measurement error. Inferring nonemployment 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 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 a positive bias, while a negative correlation generates a negative bias.

D.3 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 base period is the pre-claim time period from which earnings are used to determine eligibility—typically the first four of the last five completed calendar quarters as of the start date of the claim. The target replacement rate was 0.39 prior to 2002, 0.45 in 2003, and 0.5 thereafter. Therefore WBA is:

$$WBA(t) = \min \left(\frac{HQP}{13} \cdot RR(t), WBA^{max}(t) \right) \quad (D.5)$$

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. (2024), 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 HQP^* as follows:

$$HQP^*(t) = HQP - \frac{13}{RR(t)} \cdot WBA^{max}(t) \quad (D.6)$$

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

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

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQP_i^* + \beta_2 HQP_i^* \cdot T_i + \varepsilon_i \quad (D.7)$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQP_i^* + \alpha_2 HQP_i^* \cdot T_i + u_i \quad (D.8)$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the recorded WBA; and $T_i = \mathbf{1}[HQP_i^* \geq 0]$. 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 $HQP_i^* \cdot T_i$, $\hat{\beta} = \hat{\beta}_2 / \hat{\alpha}_2$. It is a fuzzy RKD because the actual WBA awarded to claimants may differ if they appeal with nonrecorded UI-eligible wages. We use the constant β_0 to calculate the mechanical transfer, with Y_i as the number of paid weeks.

D.4 Potential Benefit Duration Research Design

Institutional details. Once WBA is determined as described above, a separate formula determines the regular maximum benefit amount (MBA). MBAs are set to ensure that total UI benefits paid do not exceed either 50 percent of the claimants total base period earnings or 26 weekly payments at the claimant’s WBA. Formally, the formula is:

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

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

$$PBD = \begin{cases} 26 & \text{if } WBA \cdot 26 \leq \frac{1}{2} \cdot BPW \\ \frac{\frac{1}{2}}{BPW} & \text{if } WBA \cdot 26 > \frac{1}{2} \cdot BPW \end{cases} \quad (D.10)$$

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

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

Equation D.11 demonstrates the kink in the regular PBD formula with respect to $\frac{BPW}{HQRW}$ 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.¹⁸ 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.¹⁹

Sample Restrictions. To avoid the offsetting kink due to WBA^{max} kink, we exclude claims with $WBA = WBA^{max}$. Additionally, since the ratio $\frac{BPW}{HQRW}$ 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 of the base period.

¹⁸Temporary 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. (2024) and Chodorow-Reich et al. (2019) for more detail.

¹⁹We ignore several instances of one-week gaps when extended benefits temporarily expired.

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

$$\frac{BPW}{HQPW}^*(t) = HQW - \frac{BPW}{HQPW} \cdot 4RR(t) \quad (D.12)$$

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

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

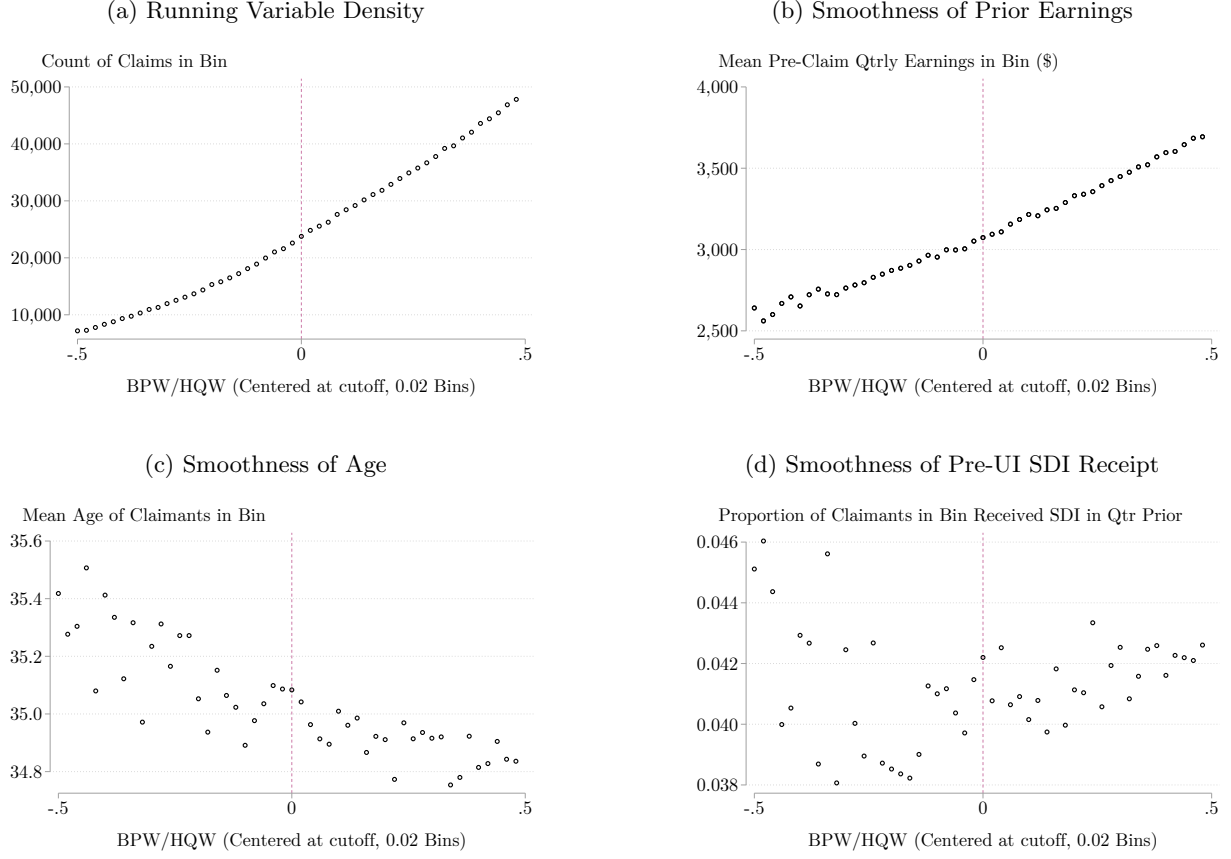
$$Y_i = \beta_0 + \tau_y T_i + \beta_1 \frac{BPW_i}{HQPW_i} + \beta_2 \frac{BPW_i}{HQPW_i} \cdot T_i + \varepsilon_i \quad (D.13)$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 \frac{BPW_i}{HQPW_i} + \alpha_2 \frac{BPW_i}{HQPW_i} \cdot T_i + u_i \quad (D.14)$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the total PBD; and $T_i = \mathbf{1} \left[\frac{BPW_i}{HQPW_i} \geq 0 \right]$. 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 $\frac{BPW_i}{HQPW_i} \cdot T_i$, $\hat{\beta} = \hat{\beta}_2 / \hat{\alpha}_2$. 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.

D.5 Additional Tables and Figures for Comparisons of BCMC Ratios across Different Program Expansions

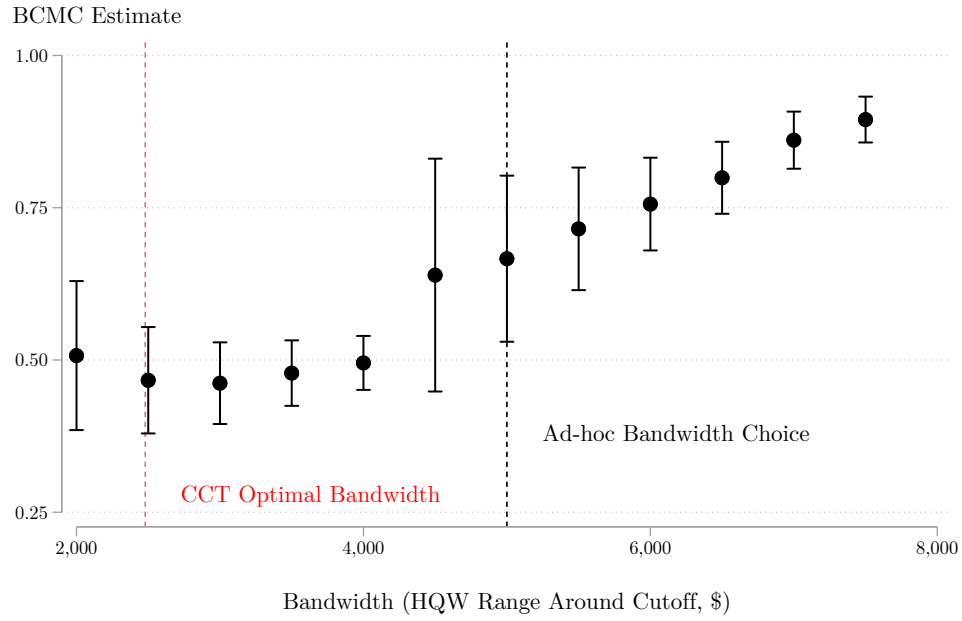
Figure D1: PBD RKD Density and Covariate Smoothness through Cutoff



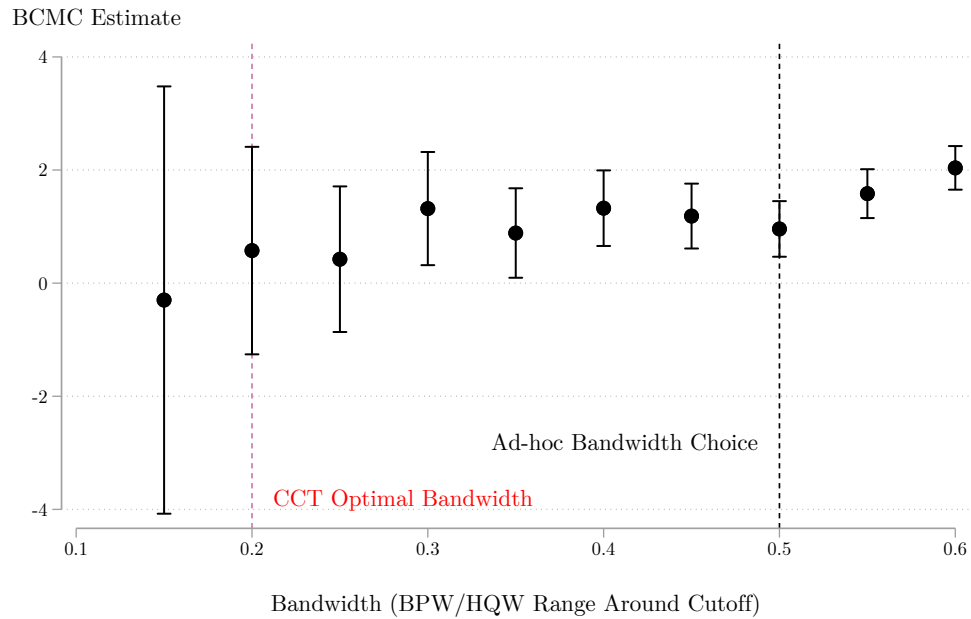
Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in [Table 5](#). The running variable is the ratio of base period wages to high-quarter wages relative to the year-specific kink, and the bin width is 0.02. Panel (a) shows that the density of the running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

Figure D2: RKD Sensitivity to Bandwidth Choice

(a) WBA



(b) PBD



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

E Other Welfare Considerations

E.1 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.²⁰ 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). A separate plausible behavioral response 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 nonmarginal 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.

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. Similarly, when a claimant was fired for cause by a former employer that employer must prove that misconduct occurred. Even if the criteria that constitute good cause or misconduct were altered substantially, or if UI agencies adopted more or less lax standards for determining whether a given claimant meets those criteria, these requirements can protect against claimants opting to separate from their job solely to collect UI benefits. Second, the input of the separating employer more generally provides an additional screen against this behavior.

While we think it is unlikely for most workers to be aware of the type of marginal change to separation-based eligibility that we study, some workers may learn about such marginal changes from their peers. We test this 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; this is necessary because our earnings data include only employer and not establishment IDs. The limitation to separating firm-quarters with single

²⁰However, experience-rated employers may be less likely to fire workers if they are likelier to receive UI benefits.

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 1 and 2). 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. 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 who 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.

E.2 Consumption-Smoothing Benefits

Unobserved consumption-smoothing benefits are likely to increase the welfare benefits of separation-based eligibility expansions for two reasons. As Equation D.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—and 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 attenuate 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 E1 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 percent

at unemployment among quitters. This is smaller than the consumption decline in the full sample of separators (7.2 percent) but still meaningfully large.

E.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.

E.4 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 eligibility 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 percent of nonmonetary determinations and 21 percent of appeals in California were tied to separation-based eligibility decisions.

Since the RJM data provide 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 California 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 percent (claimant appeals) and 3 percent (employer appeals).

These values imply that approving one denied separation-based eligibility determination would provide just under \$50 in administrative cost savings. This would offset slightly more than 10 percent 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.

E.5 Tables and Figures for Other Welfare Considerations

Table E1: Consumption Drop at Unemployment by Separation Reason (PSID)

	All Separations	Quits Only
Change in log food consumption at unemployment	-0.072*** [-0.092,-0.053]	-0.037 [-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.