No-Fault Job Loss? Less Moral Hazard *

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Abstract

Unemployment insurance (UI) eligibility requires a claimant to have lost their job through no fault of their own. Approximately 10% of claims are deemed ineligible solely based on the job separation reason. Using the systematic variation in separation-based eligibility approval rates across UI claim processing offices and examiners in California from 2002 to 2019, we show that receiving any UI benefits causes approximately 2 additional weeks of nonemployment. These effects are the smallest for lower-income claimants. By replicating existing research designs for other UI policy margins within the California data, we conclude the efficiency costs of UI benefit expansions through separation-based eligibility criteria are lower compared to those of expansions through monetary eligibility, weekly benefit amount, or potential benefit duration.

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1 Introduction

Unemployment insurance (UI) mitigates the financial consequences of involuntary job loss. However, at any given point in time over the past several decades, nearly two-thirds of the unemployed in the United States do not receive UI benefits. Table 1 decomposes the proximate reason for UI non-receipt among all unemployed people at a given point in time and among those who applied for UI over the course of their unemployment spell. One reason is the *intensive* margin of UI benefit generosity: there is a maximum limit on benefit duration for those who ever receive benefits. But another significant reason—larger by a factor of 9 when looking at the unemployed at a given point in time—is the *extensive* margin of UI benefit generosity: one needs sufficient prior earnings and a qualifying job loss reason in order to ever receive benefits. Ineligible job loss reasons drive almost two-thirds of this UI non-receipt.

Table 1: UI Receipt and Ineligibility Among the Unemployed

	(1)	(2)	(3)	(4)
	Receive UI	**No UI Receipt due to Insufficient Prior Earnings**	**No UI Receipt due to Ineligible Job Loss Reason**	**Received UI but Exhausted Benefits**
Share of the unemployed	35%	38%	10%	6%
Share of filed UI applications	63%	13%	8%	7%

Notes: The first row pertains to all unemployed individuals at a given point in time and breaks down reasons for currently (not) receiving UI benefits. It reproduces from Table 1 in Auray et al. (2019), and the population is the entire US from 1989 to 2012. The authors infer receipt and ineligibility reasons using the March Supplement of the Current Population Survey. Ineligibility due to insufficient earnings is inferred from prior earnings applied to state eligibility rules. Ineligibility due to job loss is limited to self-reported quits. The implied residual percentage is due to incomplete take-up among eligible claimants. The second row pertains to initial UI claims and breaks down reasons for ever (not) receiving UI benefits. The population is initial UI claims from 2002 to 2019, which we measure using our administrative data. Ineligibility is directly observed from agency records and excludes initially ineligible claimants who received UI following successful appeals.

The extensive margin of UI benefit generosity—also referred to as benefit eligibility criteria—affects many people, and its leniency is a policy choice. This is clear for the objective formula applied to administrative earnings records to determine monetary eligibility. But it also matters for the subjective determination of whether the claimant lost their job through no fault of their own, or what we refer to as separation-based eligibility. States explicitly vary in whether certain circumstances—such as quitting for family care responsibilities or getting fired for substance use—qualify as eligible job loss. For example, for each initial claim filed in the respective states with varying criteria, South Carolina has over three times as many of these eligibility denials as Hawaii. The federal government made this particularly salient during the Covid-19 pandemic, as the CARES Act temporarily extended pandemic-related UI benefits to anyone who quit as a "direct result of Covid-19" (Department of Labor, 2020).

Despite its policy relevance and the vast UI literature, there is scant evidence on the causal employment effects of UI extensive margin eligibility. There is robust evidence that intensive margin benefit amounts—either potential benefit duration (PBD) or weekly benefit amount (WBA)—increase unemployment duration, an effect often referred to as moral hazard (Schmieder and Von Wachter, 2016). It may be natural to think that the extensive margin would have qualitatively similar employment effects, but each margin's relative efficiency is unclear.

In this paper, we document the types of UI claimants affected by extensive margin eligibility criteria, identify the employment consequences of any UI benefit receipt, and derive the relative efficiency of various intensive and extensive margins of UI benefit generosity. We obtain causal estimates of separation-based UI eligibility on employment using a leniency-based research design with the universe of California UI records from 2002 to 2019. We show that socioeconomically disadvantaged groups are likelier to have benefits denied due to separation-based eligibility. Separation-based eligibility causally increases the duration of nonemployment by approximately two weeks. In comparison to the causal employment effects of other margins of UI benefit generosity that we also estimate in our data, we show the lowest UI benefit expansion efficiency costs correspond to separation-based eligibility benefit expansions.

For the descriptive analysis, our microdata allow us to show that disadvantaged claimants are likelier both to be investigated and to be denied due to separation-based eligibility. This is in line with a recent investigation of separation-based eligibility denials in Georgia during the first year of the Covid-19 pandemic (Donnan et al., 2021). And while Lachowska et al. (2021) show with coarser data from Washington State that employers are likelier to dispute claims for Black and less educated claimants, our ability to directly observe each step along the eligibility process allows us to demonstrate that issues and denials due to the claimant's self-report on their UI claim are just as common, particularly among socioeconomically disadvantaged groups.

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

Our identification strategy leverages variation across UI claim processing offices in their propensity to approve separation-based eligibility issues. Because claims with separation-based eligibility issues are quasi-randomly assigned to these offices for adjudication, any office-level differences in approval rates can be attributed to the causal effect of assignment to the office itself. After confirming the existence of these office-level differences and their ability to recover the local average treatment effect (LATE) of UI eligibility in an instrumental variables framework, we estimate a statistically significant 2-week increase in nonemployment duration and an imprecise null effect on subsequent earnings. 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.

To situate these effects in the existing UI literature, we first interpret separation-based eligibility as changing the total weeks of PBD (from 0 to the claimant's PBD). By doing so, we can easily compare our effect on nonemployment both to the one existing extensive margin study on monetary eligibility and the many existing intensive margin studies on PBD. In this light, our nonemployment duration effect is half the existing one for monetary eligibility and around the 25$^{\text{th}}$ percentile of PBD estimates.

We contextualize these employment effect magnitudes as relatively small with an apples-to-apples comparison between all margins of UI generosity. Informally, we measure the per-dollar cost to the government of different types of UI benefit expansions. Formally, the object of interest is the ratio of behavioral costs to the government's budget compared to the transfer absent any behavioral response. This has been used in previous work to compare different intensive margins of UI generosity (Schmieder and Von Wachter, 2016), and we generalize its empirical implementation to study the extensive margin of benefit eligibility. Projecting employment effects from our main IV estimates onto the tax code delivers the behavioral costs, while projecting unemployment potential outcomes from an instrumental variables compliers analysis onto the tax code delivers the mechanical transfer. By directly estimating the quantities of interest—as opposed to routing auxiliary estimates through a behavioral model—we take an approach similar to Lee et al. (2021).

We highlight implications for UI policy using a simple Baily-Chetty optimal UI framework. In lieu of observing consumption-smoothing benefits to estimate an optimal extensive margin UI eligibility threshold, we make a bounding argument. Under standard regularity conditions, those on the margin of extensive margin eligibility have larger marginal utility decreases when denied due to their lower earnings and larger percentage income drops. On efficiency grounds, the estimated lower disincentive costs and these larger consumption-smoothing benefits point to the desirability of reallocating UI benefit generosity away from the intensive margin towards the extensive margin.[1]

This paper's primary contribution is estimating the employment effects 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.[2] Their monetary eligibility variation due to the minimum earnings threshold is local to workers with very low earnings and labor force attachment. Because this monetary eligibility variation applies to a very specific population, it is *a priori* unclear how it would extrapolate to the much larger and more economically diverse population at risk of separation-based eligibility denial.

In providing a theoretically-motivated benchmark for the effects of separation-based eli-

[1] In practice, however, there is a mix of other costs that such a reallocation would potentially incur, such as an increase in job separations or a decrease in administrative costs (Lusher et al., forthcoming; Ragan, 1984; Solon, 1984). We discuss this and other welfare-relevant considerations in subsubsection 5.2.4.

[2] A recent systematic review of causal estimates of UI disincentive effects contained 39 intensive margin estimates: 18 pertaining to weekly benefit amount and 21 pertaining to potential benefit duration (Schmieder and Von Wachter, 2016).

gibility relative to several other UI policy margins, we also provide the most comprehensive comparison of UI policy margins in a given institutional context. The most closely related paper in this respect is Lee et al. (2021), which estimates the per-dollar transfer costs of increasing WBA and decreasing partial benefit marginal taxes based on variation within Washington State in 1995.[3] One other paper that studies the employment effects of multiple UI margins in the same setting is Landais (2015), doing so for WBA and PBD within 5 US states in the late 1970s. We make a unified comparison across the two most commonly studied UI benefit intensive margins—WBA and PBD—in addition to two UI benefit extensive margins of eligibility that have received little attention in the literature. In doing so, we can more plausibly abstract from other institutional factors in reconciling effect size differences. Pertinent to policy, we provide suggestive evidence on a welfare-enhancing budget-neutral reform and highlight the additional causal effects necessary to understand its net impact.

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

A final contribution based on our identification approach new to the UI literature is estimating treatment effect heterogeneity of UI benefits across a wide range of observable characteristics. Our UI benefit individual-level variation is vast and does not directly depend on prior income or age. With this power, we find that responses to UI are the lowest for lowest-income claimants; if anything, their unemployment duration decreases. We also find small effects for the top and bottom quartile age groups. 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 lifecycle labor supply model

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

(e.g., Michelacci and Ruffo, 2015), or estimating regression discontinuity designs at separate policy cutoffs across an age range of several years (e.g., Schmieder et al., 2012).

2 Institutional Details

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

2.1 Overall Eligibility Criteria for UI

Eligibility criteria fall into two broad categories: monetary eligibility and nonmonetary eligibility.[4] Within each of these two criteria categories, there are conditions related to the prior employment spell and conditions related to the claimant's current actions. In order to receive any UI benefits, workers must establish initial eligibility. After doing so, they must continue certifying their eligibility each week in order to receive that week's benefit payment.

Monetary eligibility criteria pertain to prior or current income, and they are relatively easy for the state to verify using administrative earnings records. In order to receive any benefits at all, the worker must have accrued a minimum amount of earnings during approximately the year preceding the claim. The dollar requirement is low enough that nearly all newly unemployed formal workers meet it.[5] After the claimant establishes initial eligibility, any income the claimant receives can reduce that benefit week's payment, potentially to $0.

Nonmonetary eligibility encompasses all other eligibility criteria, and it includes the focus of our project: separation-based eligibility. Separation-based eligibility criteria require that the worker lost their job through no fault of their own. Unlike monetary eligibility criteria,

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

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

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

2.2 Separation-based Eligibility Criteria and Detection

Rather than denying benefits to all workers who quit or were fired, separation-based eligibility criteria deny quits without "good cause" and firings with "misconduct". The California Employment Development Department (EDD) maintains a list of circumstances surrounding job loss that is intended to be exhaustive. These are broadly separated into quits and firings, and each detailed subcategory delineates how circumstantial details should affect the UI eligibility determination.[6] These detailed subcategories pertain both to work conduct and personal conduct.

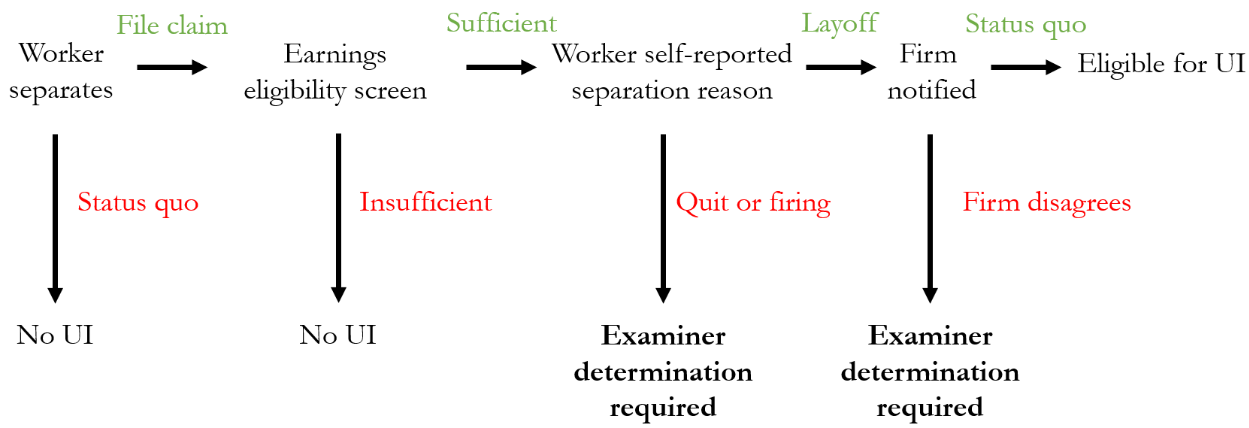
Whether a given quit or firing is deemed no-fault is subjective, creating scope for claimants with similar circumstances to receive different eligibility determinations. The subjective criteria for firings hinge on the type, intention, and degree of employee misconduct, while those for quits highlight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

Due to this subjectivity, states detect separation-based eligibility issues using attestations

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

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

Figure 1: Selection Into Separation-Based Eligibility Determinations



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

Subject to some federal regulation, each state has the discretion to decide separation-based eligibility parameters. This is clearest for voluntary quits, as state law can explicitly designate qualifying circumstances. For example, in terms of provisions granting eligibility for specific circumstances, 27 states lack any related to joining the military, 9 lack any related to family member illness, 6 states lack any related to domestic violence, and 2 states lack any related to sexual harassment at work.[7] Apart from the presence of these general provisions, different states can require different types of documentation or apply a different burden of proof.[8]

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

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

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

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

Once a claimant's self-report or employer's dispute triggers an eligibility investigation, it is assigned to a single office almost entirely based on the last two digits of the claimant's Social Security Number (SSN). The limited exceptions include claimants with special communication needs or employment from another state. Importantly for our causal research design, the last two SSN digits are quasi-randomly assigned by the federal government (Social Security Administration, 2199).

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

After being assigned to an office, claims are then assigned to a single examiner for investigation and determination. While all claims are quasi-randomly assigned to offices, only a subset of claims is also *a priori* quasi-randomly assigned to examiners. Quasi-random assignment to examiners arises from a scheduling queue that sequentially matches pending claims to available examiners in the assigned office who speak the claimant's language. Around 40% of claims are assigned to examiners in this manner. These assignments are useful for our supplementary examiner research design, as unobserved examiner characteristics are conditionally independent of unobserved claim characteristics. The other 60% of claims are taken up while awaiting automatic scheduling on an ad hoc basis by an examiner with unexpected availability.

2.4 Monetary Implications of UI Eligibility

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

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

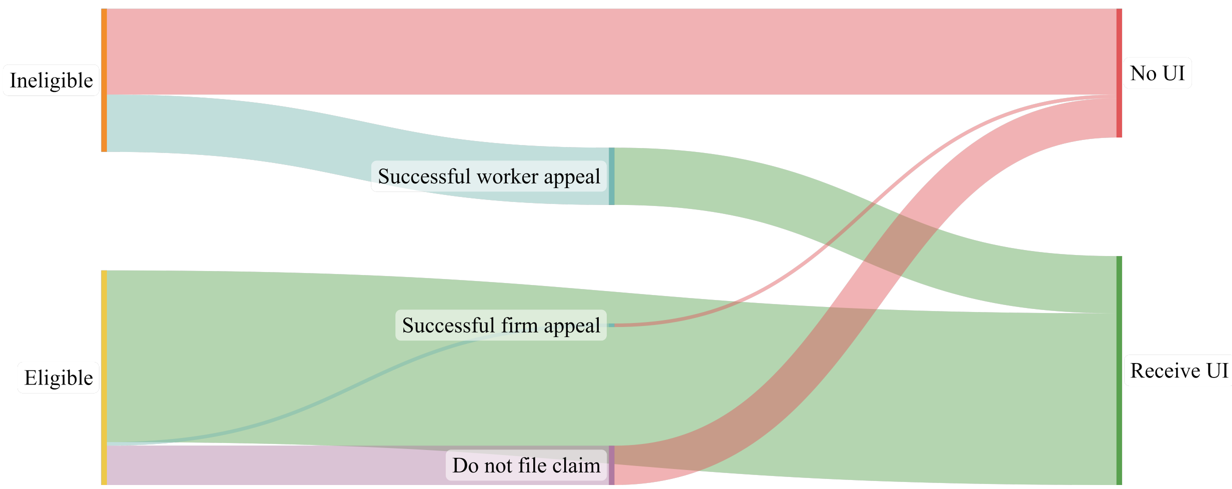
Second, claimants may receive some UI benefits before a separation-based eligibility issue arises. This could be triggered either by periodic government audits or delayed employer disputes. Because initial claimants must satisfy a 1-week waiting period before they can receive benefits and employers must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

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

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

Take-up is the most common reason for benefit nonreceipt among eligibile claimants.

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



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

The benefits that eligible claimants can receive as they remain unemployed are economically meaningful. State benefits replace approximately 50% of prior weekly earnings and usually have a maximum duration of 26 weeks. Replacement rates are relatively lower for workers with higher and more stable earnings, as the weekly benefit amount (WBA) is a function of the highest quarterly earnings during the base period and is capped at $450 per week for most of our sample period. Potential benefit duration (PBD) increases during recessions through federally-funded extensions, reaching 52 weeks in 2002 and 99 weeks from 2009 to 2012. The PBD can also vary across claimants, as those with variable quarterly earnings can have lower PBD.[9]

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

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

This section describes the administrative UI data from California. In addition to commonly used linked employer-employee quarterly earnings and paid UI benefits, we observe information on how the state adjudicates eligibility issues. Using this, we show that claimants with eligibility issues and denials are disproportionately from lower socioeconomic groups.

3.1 Data Sources

Our primary analysis links individual-level administrative datasets on UI claims and quarterly earnings from 2002 through 2019. Both datasets are maintained by the State of California's Employment Development Department (EDD): UI claims microdata since 2000 and quarterly earnings records since 1995.[10] We exclude years before 2002 due to missing data on processing office assignments and years after 2019 due to a vastly different policy regime during the COVID-19 pandemic.

UI claims microdata contain nearly all of the information that EDD collects from claimants or produces itself while administering claims. The claims data comprise three categories: claimant self-reports at initial filing, benefit payments throughout the claim, and agency documentation of how the claim was processed. The claimant's self-reports constitute most of the demographics we use. The benefit payments are primarily used as a measure for receiving any UI. Still, we can also observe detailed timing using each individual weekly payment's dollar amount, payment disbursal date, and compensated unemployment week. Finally, the agency documentation includes any eligibility issues that were detected along with the timing and outcome of the resulting investigations. Importantly for our research design leveraging leniency variation, we observe the processing office and examiner that handled the eligibility determination.

We measure employment using the near-universe of California quarterly earnings records. These linked employer-employee data include all UI-covered earnings, which does not capture some government, nonprofit, and informal employment. We use these data for two

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

purposes. Earnings measures following the claim are our primary outcomes of interest. Our definition of nonemployment duration is consecutive quarters without any earnings, starting with the quarter following the initial claim.[11] This necessarily undercounts nonemployment duration by ignoring any within-quarter periods of nonemployment immediately following filing and immediately preceding reemployment. While this could plausibly bias our estimated nonemployment duration response downwards, we show in Section subsection 5.2 that it also generates an upward bias for our welfare-relevant measure of eligibility expansion efficiency costs. Reassuringly, conclusions drawn from both estimates are consistent with each other.

Detailed UI eligibility determination data is a unique aspect of our UI records. For all determinations, we observe the separation-based eligibility issue type (e.g., quit or firing), de-identified but time-invariant identifiers for the processing office and examiner handling the determination, the determination date, and the initial eligibility determination. Additionally, since 2009, we observe the claimant's self-reported separation reason: quit, firing, or layoff.

Apart from the detailed eligibility data and realized processing office assignment we observe for all determinations, we observe data pertinent to the assignment of claims to examiners since 2017. The primary variable is the claimant's spoken language determines which examiners a claim *could* be sent to. Because the examiner interview is conducted verbally over the phone, the assigned examiner must be able to communicate with the claimant. Additionally, we can observe an indicator of whether the interview was scheduled algorithmically via the queue or via the ad hoc scheduling process. Claims scheduled through the queue do not involve any manager discretion, so the only claimant characteristic affecting assignment is spoken language.

3.2 Claimants with Separation-Based Eligibility Issues are Lower-Income

Table 2 shows that disadvantaged groups are disproportionately likely to have a separation-based eligibility issue and denial. Each column contains sample sizes and average characteristics for a given subset of claimants, where each column is a strict subset of the adjacent

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

column to the left. Among monetarily eligible claims, nearly one-third had at least one separation-based eligibility determination and 40% of these were denied. Comparing average characteristics across these groups of claims demonstrates that younger, non-white, female, and lower-income claimants are likelier to have a separation-based eligibility issue and for that issue to result in benefit denial.

Table 2: UI Receipt and Ineligibility Among the Unemployed

	(1)	(2)	(3)	(4)	(5)
	All Claims	Monetarily Eligible	+ Separation Issue	+ Initial Denial	+ No Payments
N (millions)	26.8	22.9	6.9	2.9	2.1
Share of previous column		86%	30%	41%	75%
Avg. prior quarterly earnings ($)	6,960	7,881	6,911	5,615	5,240
Weekly benefit amount ($)		283	271	246	233
Age	39	40	36	34	33
Female	0.45	0.45	0.49	0.50	0.51
Nonwhite	0.64	0.65	0.63	0.67	0.67
English-speaking	0.88	0.87	0.94	0.94	0.94
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.

Even when claims are eventually paid, a separation-based eligibility investigation or initial denial delays payments. Theoretically, there are two primary channels why such payment delays could affect a claimant's job search: decreased liquidity and increased benefit receipt uncertainty. Even conditional on the eventual receipt of benefit payments, both channels plausibly decrease unemployment duration. This is relevant to interpret the initial eligibility determination as a single policy lever that can impact claimants in multiple ways.

Table 3: Payment Timing Implications of Eligibility Investigations and Appeals

	(1)	(2)	(3)
	No eligibility issues	Separation issue but approved	Separation denial but paid
Avg. prior quarterly earnings	8,327	7,837	6,708
Any payments	0.77	0.83	1.00
Median days to 1^{st} payment	16	21	93
Median days to 1^{st} covered week	10	10	12
N (millions)	16.0	4.0	0.8

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

Empirically, eligibility investigations and denials disproportionately impact lower-income claimants. As a reference point, Column 1 of Table 3 shows the average prior quarterly earnings, fraction receiving any UI payments, and typical payment delays for claimants without eligibility issues. The mandatory 1-week waiting period means that the first covered week of unemployment usually occurs 10 days after claim filing, and processing logistics mean that claimants usually wait another week until receiving the payment itself. Because eligibility investigations require interviewing the claimant and former employer, Column 2 shows that the time to first payment is slightly delayed for claimants who are investigated but ultimately approved.[12] Because this delay is usually only 1 week, it likely does not have a meaningful impact on claimants outcomes. Finally, Column 3 shows that a successfully appealed separation-based eligibility denial delays payment receipt by over two months as the case proceeds through the California Unemployment Insurance Appeals Board.

While the above patterns are evidence of disparate impact, it is important to note that we cannot determine the underlying cause. For example, when it comes to selection into having an eligibility dispute, it is possible this is because socioeconomically disadvantaged people work in jobs with a higher rate of quits and firings compared to layoffs. On the other

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

hand, it is possible that employers are likelier to dispute the claims filed by socioeconomically disadvantaged people.[13] Similarly, when it comes to selection into an eligibility denial conditional on an eligibility issue, we cannot say whether lower-income claimants are likelier to have had separation circumstances befitting denial or are likelier to be denied conditional on their separation circumstances.

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

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

4.1 Design: Processing Office Assignment as UI Policy Variation

Our research design leverages the fact, as discussed in Section 2.3, that claims with separation-based eligibility issues are quasi-randomly to processing offices for adjudication. By comparing subsequent employment outcomes of claimants assigned to relatively lenient offices and those assigned to relatively strict offices, we aim to approximate an experiment that approves the marginal claimants with separation-based eligibility issues who were barely denied (or vice versa).

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

[13] Lachowska et al. (2021) employ a movers design with workers who file UI with different employers in different years and find a blend of these explanations: firms that pay all of their workers lower wage premia also are likelier to dispute UI claim eligibility.

4.2 Implementation: Econometric Framework and Identification Assumptions

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

4.2.1 Estimating Equation

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

We directly observe the SSN-based assignment regime of claims to processing offices for only 11 years, so we infer this from the data. To do so, we define the intended processing office for a given combination of the final 2 SSN digits and claim filing month as the office that processed at least 95% of those SSN digits' claims in the month. This measure is comprehensive: 97% of digit-month combinations have a defined intended office, and it is missing mostly when the SSN-based assignment regime changes in the middle of a month. It is also externally validated: for the 11 years with written agency documentation of the SSN-based assignment regime, it agrees for more than 99% of digit-months.

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

Our final instruments additionally interacted 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). Importantly, we include issue type as a control to partial out level differences in eligibility across issue types. Allowing for flexibility in leniency appears to be empirically relevant, as Figure A1 shows that office-level leniency is uncorrelated across issue types, even though there is statistically and economically significant variation across offices within each issue type.[14]

This strategy hinges on the issue type itself being a characteristic unaffected by the processing office. It would be a problem if there is often ambiguity in the proximate job separation reason and processing offices differ in classification tendencies.[15] In practice, this is rare. Only 7% of cases have claimant-reported separation reasons that do not match the final determination's issue type, and many of these are plausibly due to claimant transcription error during claim filing. Figure A2 shows that the minimal variation (1.3 percentage points) in the composition of separation-based eligibility issue types across offices is uncorrelated with issue-specific eligibility approval rates.

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

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

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

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

Our preferred estimator for Equations 2 and 1 is the two-step estimator unbiased jackknife IV (UJIVE) (Kolesár et al., 2013). Intuitively, this estimator extends the logic of jacknife IV (JIVE) to accommodate covariates (Angrist et al., 1999). To do so, it leaves

[14]One potential explanation—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 variation in propensities would have to be similar in magnitude, but they could have an arbitrary correlation.

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

out the own observation in the first-stage given by Equation 2 both when partialling out the covariate fixed effects and when projecting the vector of office-by-issue instruments onto the endogenous treatment. Results are qualitatively similar when we employ alternative estimators like 2SLS and LIML.

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

4.2.2 Validating the Instrument

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

First-stage relevance. The first identification assumption—first-stage relevance—requires that office assignment be predictive of separation-based eligibility. We directly test this by estimating the first-stage regression equation Equation 2 and testing the joint significance of the office-by-issue dummies.[16] Column 1 of Table 4 shows the first-stage F-statistic for this regression is 405, which is well above the threshold of 104.7 that ensures 95% coverage without any adjustment to standard errors (Lee et al., 2022).[17] Figure A4 plots the coefficients from the first-stage regression; variation in eligibility approval propensity is evenly spread across office-by-issue pairs.

Independence. The second identification assumption—independence—is bolstered in part by the SSN-based assignment of claims to processing offices, and we present evidence in favor of

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

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

it in the third through sixth columns of Table 4. As before, we test the joint significance of the office-by-issue instruments in Equation 2. In each regression, the outcome variable is a pre-existing claimant characteristic. For several different claimant characteristics, the range across office-by-issue we fail to find statistically significant differences across office-by-issue pairs.

Table 4: 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.

Excludability The third identification assumption—excludability—requires that the only effect processing offices have on the endogenous outcome Y is through the initial eligibility determination. The primary excludability concerns relate to the other administrative processing duties that offices handle, as there are economically small but statistically significant differences in these measures. The last two columns of Table 4 show that the expediency of making an eligibility determination and the propensity to find continuing claim issues differs across offices.[18] Both of the F-statistics are statistically significant, but the range

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

across office-by-issue pairs is only 4 days and 3 percentage points, respectively. Reassuringly, Figure A3 shows that these minor differences are uncorrelated with eligibility approval propensity. Accordingly, in Appendix B, we show that results for the design based on examiner assignment are similar when including or excluding office-level variation.

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

Our results primarily focus on eligibility approval as the endogenous treatment of interest, as it is a clear policy level of the government. However, we also contextualize magnitudes using other measures of UI benefit receipt, such as by scaling the effect of eligibility on receiving UI payments. When doing so, positive bias from plausible exclusion restriction violation makes our main findings of small employment responses even starker. The likely source of an exclusion restriction violation is UI benefit timing. Conditional on receiving UI benefits, a claimant assigned to a relatively lenient office is likelier to receive those benefits through an initial eligible determination rather than a successful appeal following an initial ineligible determination. Based on the third row of Table 3, this means the claimant waits, on average, over 2 fewer months to receive the first payment. If anything, the increased liquidity afforded by more expedient payments leads to a longer unemployment duration.

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

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

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

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

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

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

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

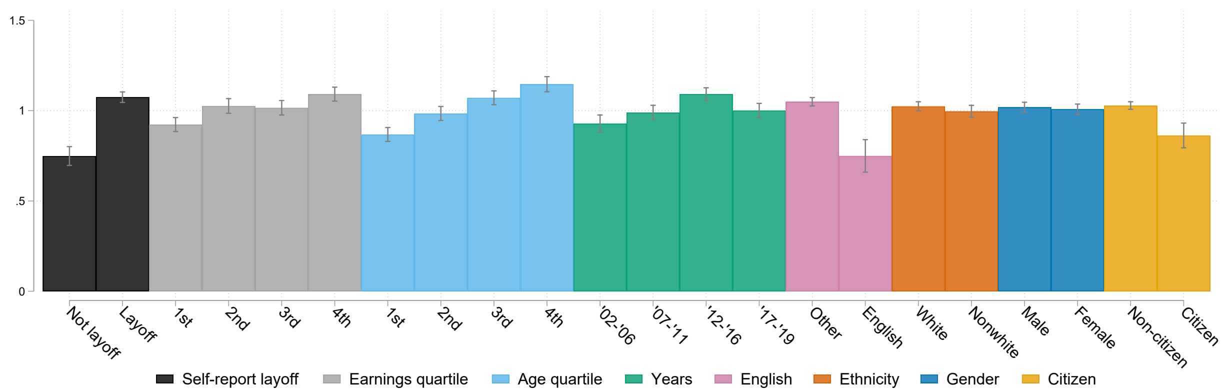
We then calculate the scalar leave-one-out mean of this residualized leniency measure at the office-by-issue level, which is indexed by os:

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

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

In support of the monotonicity assumption, Figure 3 shows that it 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, Appendix A shows that even when we allow leniency to vary at various socioeconomic levels or over time, these group-specific or period-specific approval probabilities are highly correlated for a given office-by-issue pair.

Figure 3: Consistency of Office-by-Issue Leniency Measures



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 $\tilde{Z}_{it}^{j}$ within a given subsample. Each color represents a different categorical variable, and separate bars refer to separate levels of that categorical variable. Lower quartiles correspond to lower levels of the variable. Robust standard errors are clustered by claimant and error bars provide 95% confidence intervals.

The second type of monotonicity concern is related to the *conditional* quasi-random assignment that motivates including fixed effects. There are two conceptually distinct issues that stem from including controls in the specification, but both can be addressed in a specification that fully saturates Equation 1 and Equation 2 with interactions between the office-by-issue instruments and the fixed effect controls.[19]

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

4.3 Results: Eligibility Affects UI Receipt More than Employment

Unsurprisingly, initial eligibility approval increases UI benefit receipt. The top panel of Columns 1 through 3 of Table 5 show estimates of Equations 1 and 2 using UJIVE, where the endogenous treatment D is eligibility approval and the outcome Y is some measure of benefit receipt. The bottom panel estimates the average value of the outcome Y among those who were on the margin of eligibility approval but received an ineligible determination.[20]

For those on the margin of eligibility approval, eligibility approval doubles various measures of benefit receipt. For example, Column 1 shows the probability of receiving any UI payments increases from 31% to 63%. Panel (a) of Figure A6 illustrates the doubling of payment receipt by presenting the "visual IV", where the causal effect is represented by the positive relationship between the office-by-issue-level reduced-form differences in payment receipt vs. first-stage differences in eligibility approval Angrist (1990). Interestingly, while the 31% receipt rate for marginally denied claimants roughly coincides with the 26% average benefit receipt rate among all separation-based eligibility denials given in the final column

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

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

of Table 2, the 63% receipt rate for marginally approved claimants is somewhat lower than the receipt rates among either all claims without eligibility issues (77%) or all claims with separation-based that were nevertheless approved (83%), as shown in Table 3.

Table 5: Effects of Initial Eligibility Approval

	UI Benefits			Subsequent Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any payments	Payments (weeks)	Payments ($)	Avg. $ in qtrly. earnings (w/ 0's)	Any earnings 1 qtr. after	Consecutive qtrs. w/o earnings
IV	0.32	10.3	2,547	17	-0.04	0.14
SE	(0.01)	(0.43)	(146)	(142)	(0.01)	(0.07)
tf SE	[0.01]	[0.43]	[146]	[142]	[0.01]	[0.07]
OLS	0.55	16.9	4,935	979	[-0.06]	0.30
SE	(0.00)	(0.02)	(5.37)	(5)	(0.00)	0.00
Y^0	0.31	6.9	2,323	3,530	0.49	2.37
F	405					
Unique N	5.5m					

Notes: The instrumental variables estimate is β in Equation 1 where the endogenous treatment D is initial eligibility approval. The sample includes all separation-based eligibility issues from regular initial claims between 2002 and 2019. Y^0 is the untreated complier mean estimated following Frandsen et al. (2023). The tF adjustment does not change standard errors because the F-statistic is above 104.7 Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

Our main result is that, despite doubling UI benefit receipt, separation-based eligibility approval has virtually no effect on subsequent earnings and only modest effects on subsequent unemployment. Columns 4 through 6 has the same structure as Columns 1 through 3, except the outcome Y is some measure of subsequent employment. Column 4 reports the near-zero, statistically insignificant impact on average subsequent quarterly earnings over the 7 quarters following the initial claim. The point estimate is 17, indicating that UI eligibility *raises* subsequent total earnings by just over \$100. However, uncertainty is large: the standard error is an order of magnitude larger than the point estimate.

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

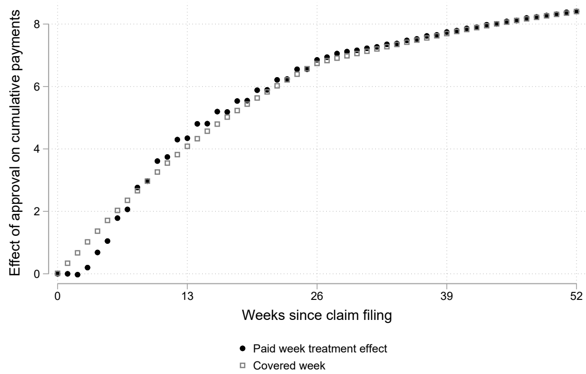
We also find increases in nonemployment duration when we translate the lack of employment in a given quarter into a nonemployment duration. Column 6 shows that eligibility lengthens nonemployment by 0.14 additional quarters, and the effect is marginally statistically significant. This less than 2-week 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.

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

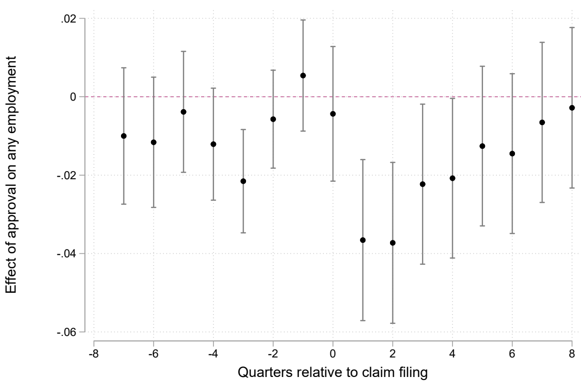
Eligibility approval has the largest impact on payment receipt approximately 1-3 months after claim filing, but its effect on cumulative benefit receipt continues to grow throughout the subsequent year. Panel (a) of Figure 4 shows the dynamic treatment effects of eligibility approval on both paid and covered weeks, where the former is the calendar week the UI benefit is paid and the latter is the week of unemployment for which the payment compensated. The moderate gap between paid and covered week treatment effects during the first month reflects retroactive benefit payments made to cover certified weeks while the eligibility determination process was ongoing.

Figure 4: Dynamic Impacts of Eligibility Approval

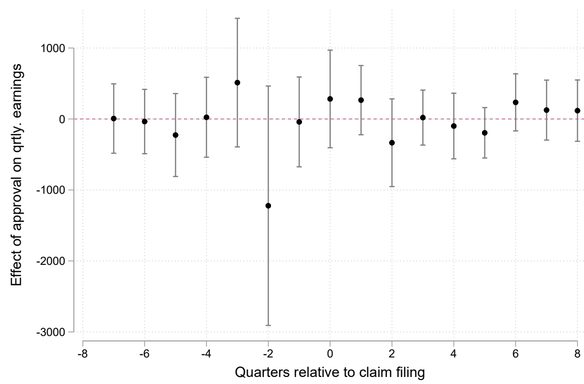
(a) Paid and Covered Weeks



(b) Any Earnings



(c) Avg. Earnings Incl. 0's



Notes: Each panel displays coefficients from separate regressions of the form in Equation 1 and Equation 2. Outcomes in panel (a) are cumulative measurements of payments as of each of the 52 weeks following claim filing. 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. Outcomes in Panels (b) and (c) are contemporaneous measures for the quarters before, during, and after claim filing. 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.

We do not find any evidence of employment hysteresis, as any impacts on contemporaneous employment or earnings are statistically insignificant and roughly zero 8 quarters after the initial claim. Panels (b) and (c) of Figure 4 plot treatment effects for employment and earnings, respectively, in a 4-year window centered around claim filing. Eligibility approval estimates using employment outcomes are consistently statistically insignificant prior to claim filing, which further bolsters the independence assumption of our research design. 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. In contrast, there is never a statistically significant effect of eligibility approval on average quarterly earnings. A majority of point estimates following claim filing are actually positive, and all are quite close to 0.

Figure A7 contextualizes the magnitude of treatment effects relative to baseline outcome outcomes by separately plotting potential outcomes for marginally approved and denied claimants. The dissipation in treatment effects on employment probability is driven by an increase in employment among the approved rather than a decrease in employment among the denied. For both marginally approved and marginally denied claimants, there is a persistent decrease in any employment and total earnings of around one-third. This is similar to unemployment earnings losses documented in previous work (Davis and von Wachter, 2011; Jacobson et al., 1993). This is evidence against an interpretation that separation-based eligibility's relatively small effect is driven by labor force exit irrespective of UI eligibility.

Heterogeneity. Individual-level variation with a very strong first-stage allows us to show that employment effects are the smallest for those with the lowest prior earnings. Table 6 separately re-estimates Equation 1 and Equation 2 within specific subsamples. One notable source of heterogeneity is that the point estimate for any earnings in the quarter following the UI claim is 0 among claimants in the lowest quartile of prior earnings. Pairwise differences across earnings groups are marginally statistically insignificant, though the negative effects on employment are consistent for the other earnings groups.

Another suggestive pattern of heterogeneity is the lack of response to eligibility for the youngest and oldest workers. While we cannot observe schooling or retirement decisions, it is plausible that these other labor force categories particularly relevant to these groups are mediating factors. These findings are independently pertinent to the academic literature. First, they support findings from theoretical lifecycle models that moral hazard responses should be lowest for the youngest workers (Michelacci and Ruffo, 2015). Second, they suggest one should use caution when extrapolating from a UI policy local average treatment effects (LATEs) based on an age cutoff (Schmieder et al., 2012; Lalive, 2008; Centeno and Novo, 2009; Caliendo et al., 2013).

Table 6: Heterogeneous Effects of Initial Eligibility Approval on Subsequent Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prior Earnings Quartile				Age Quartile			
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
IV	0.00	-0.04	-0.05	-0.05	-0.02	-0.04	-0.07	-0.01
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
tF-SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
F	127	100	101	103	171	121	102	86
N	1.5m	1.5m	1.5m	1.5m	1.5m	1.5m	1.5m	1.5m

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 any earnings in the quarter 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 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.

We do not find any patterns in heterogeneity by time period, gender, or race in Table A2. Notably, even though the maximum potential benefit duration is much higher during and soon after the Great Recession, we find the employment effects of eligibility are smallest during and soon after this period.

To test whether heterogeneity in the effect of UI receipt is a likely mediator of heterogeneity in employment responses, Table A3 runs the subsample regressions with the outcome of receiving any UI payments. Indeed, the patterns of heterogeneity in employment responses across income groups and time periods is mirrored by heterogeneous effects on UI receipt. For example, eligibility increases UI receipt by 28 percentage points for the lowest-earners but 39 percentage points for the highest earners. Nevertheless, the heterogeneity in payment effects across prior income cannot explain the heterogeneity in employment effects for two reasons: the former increase monotonically while the latter is different for only the lowest earners. However, heterogeneity in payment effects does seem to explain much of the heterogeneity in employment effects across time periods. This complements Bell et al. (2022b), who find that the elasticity of employment with respect to WBA rises during recessions due to longer PBDs and baseline unemployment durations during those time periods.

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

This section contextualizes the employment effects estimated in Section 4 to show that the employment effects of separation-based eligibility imply lower efficiency costs than those for other UI policy margins. The other UI policy margins we consider are extensive margin monetary eligibility and intensive margin benefit amounts through potential benefit duration (PBD) and weekly benefit amount (WBA). In addition to drawing on existing estimates from the literature for the other UI policy margins, we replicate those designs in our California data to make consistent comparisons within the same institutional context and dataset. Appendix C further details the sample construction, identification assumptions, and estimation procedures underlying these replication estimates.

5.1 Comparing Nonemployment Responses to Benefit Weeks for Some UI Policy Margins

We begin by showing that, in terms of additional weeks of nonemployment per week of benefit eligibility, our separation-based eligibility estimates based on office assignment are small relative to the existing literature. While we will show in Section 5.2 that this is not the welfare-relevant metric for measuring efficiency costs—as it does not scale by the mechanical transfer to claimants—it is a helpful benchmark for understanding the absolute magnitude of the behavioral response to UI benefits.

PBD shifts benefit weeks on the intensive margin, while monetary and separation-based eligibility do so on the extensive margin. For PBD, we draw on the 21 available estimates in Schmieder and Von Wachter (2016) Table 1 Column 5; for monetary eligibility, we use the single existing estimate in Leung and O'Leary (2020); and for separation-based eligibility, we show our main estimate based on office assignment in section 4.

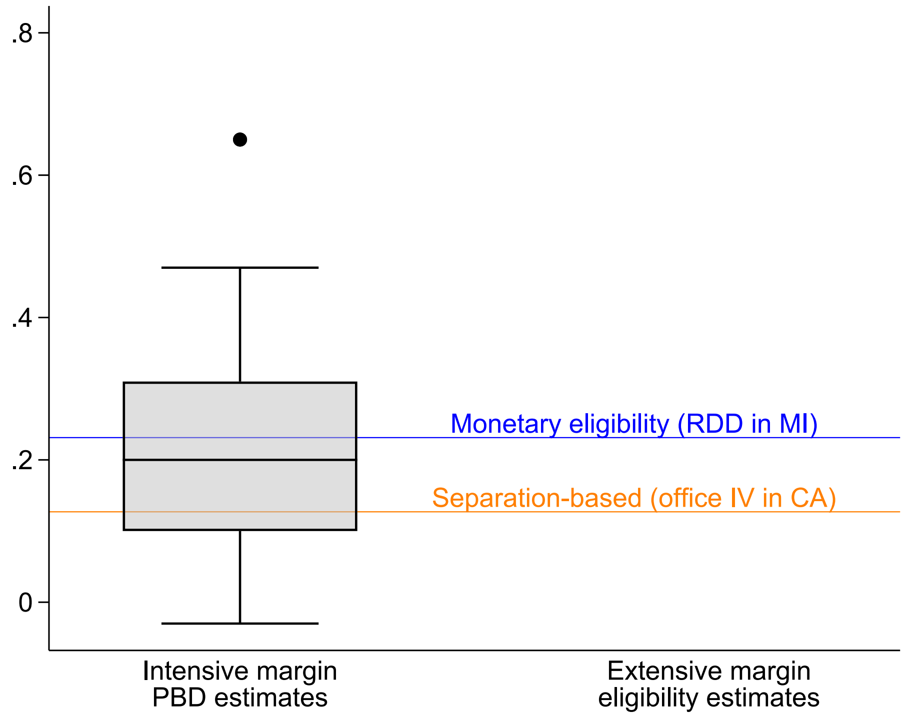
We employ two scaling procedures to make the extensive margin directly comparable to the intensive margin. First, we account for the fact that extensive margin eligibility only partially translates to benefit receipt, as discussed in Section 2.4. We scale the causal effect of eligibility on consecutive quarters of nonemployment by the causal effect of eligibility on UI benefit receipt.[21] Second, we translate quarters of nonemployment into weeks of unemployment and UI eligibility into benefit weeks. The former is a factor of 13, and the latter is a factor of total PBD including extended benefits. Table C1 shows that the average total PBD is 45 weeks for the office-assignment design's sample from 2002 to 2019 that includes the Great Recession, while Table 1 Column 2 in Leung and O'Leary (2020) shows this is 32.1 weeks for their analysis sample.

Figure 5 plots the effects of an additional week of potential benefits on weeks of consecutive nonemployment. All of the extensive margin estimates are within the interquartile range of the intensive margin estimates, but our separation-based eligibility estimate is around the literature's 25$^{\text{th}}$ percentile estimate for PBD and below the literature's existing estimate for monetary eligibility.

There are two drawbacks to this comparison of nonemployment responses: comprehensiveness and welfare-relevance. First, it does not facilitate comparisons with WBA expansions. Second, as we demonstrate in the next section, welfare-relevant costs of benefit expansions require additionally (i) translating this behavioral response to a fiscal externality on the government budget and (ii) scaling by the mechanical benefit transfer to claimants if their behavior were held fixed.

[21] For monetary eligibility, this is the fuzzy RDD estimate reported in Table A1 Column 2 in Leung and O'Leary (2020): receiving UI benefits increases nonemployment quarters by 0.571. For separation-based eligibility, this is the ratio of two overidentified instrumental variables estimates; we obtain similar results directly using UI benefit receipt as the endogenous treatment in the instrumental variables design.

Figure 5: Nonemployment Duration Effects per Week of Potential Benefit Duration



Notes: The graph plots values of the partial effect of an additional week of UI benefits on nonemployment duration. The box-and-whisker chart is from 21 benefit duration estimates surveyed in Schmieder and Von Wachter (2016). The separation-based eligibility estimate using our main office-assignment-based IV design in California from 2002 to 2019 is in orange and the monetary eligibility estimate from Leung and O'Leary (2020) using a minimum earnings threshold-based RD design in Michigan from 2005 to 2010 is in blue. Eligibility estimates are derived from the causal effects of eligibility on consecutive quarters of subsequent unemployment. These causal effects are scaled by (1) the effect of eligibility on benefit receipt, (2) the average number of total potential benefit weeks for the relevant sample, and (3) the thirteen weeks in a quarter of nonemployment.

5.2 Comparing Per-Dollar Transfer Costs for All UI Policy Margins

Our preferred metric for comparing different UI policies is the per-dollar cost to the government of a given UI benefit expansion, or the ratio of behavioral costs to mechanical costs (BCMC ratio). We proceed by first deriving the welfare-relevance of this quantity in a stylized optimal UI framework, which highlights its theoretical advantages relative to the quantity in the previous section. We then map it to our main results on the employment effects of separation-based eligibility. Finally, we compare it to estimates corresponding to other UI policy, both from the existing academic and from replications in our California data. We find the BCMC ratio is lowest for separation-based eligibility.

5.2.1 Theoretical Motivation

Before demonstrating the BCMC ratio's theoretical relevance, we first define it formally.[22] Consider a vector of UI policy rules θ (e.g., separation-based eligibility approval probability, monetary earnings threshold, 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 government of a UI policy change $\mathrm{d}\theta_j$ along a policy margin j can be decomposed into an indirect effect due to behavioral responses and a direct effect due to the mechanical benefit transfer:

$$\underbrace{\frac{\mathrm{d}G}{\mathrm{d}\theta_j}}_{\text{total}} = \frac{\mathrm{d}B(\mathbf{Y}(\theta,\tau);\theta) - T(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\mathrm{d}\theta_j} = \underbrace{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta_j}}_{\text{behavioral}} + \underbrace{\frac{\partial B}{\partial \theta_j}}_{\text{mechanical}} \tag{6}$$

The ratio of the second terms and third terms in Equation 6 is the BCMC ratio, and we next show it features prominently in the welfare effects of UI policy reforms. For the worker, let $U(\cdot, \cdot, \cdot)$ be the representative worker's utility function with arguments B (i.e., consumption while unemployed), T (i.e., consumption while employed), and $\mathbf{Y}$ (i.e., other choices). The worker treats government policy θ and τ as fixed. Denote the worker's resulting indirect utility function by $V(\theta, \tau)$. For the government, its balanced budget constraint $G(B, T) = 0$ implicitly defines a function $\tau(\theta)$:

$$\frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} = -\frac{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial \theta_j}}{\frac{\partial G(\mathbf{Y}(\theta,\tau);\theta,\tau)}{\partial \tau}} = -\frac{\frac{\mathrm{d}G}{\mathrm{d}\theta}}{\frac{\mathrm{d}G}{\mathrm{d}\tau}} \tag{7}$$

[22] The following discussion closely follows the derivations in Schmieder and von Wachter (2017) and Lee et al. (2021).

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

$$\begin{aligned} \frac{\mathrm{d}V}{\mathrm{d}\theta_j} &= U_1 \cdot \frac{\partial B}{\partial \theta_j} + U_2 \cdot \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}\tau}{\mathrm{d}\theta_j} \\ &= U_1 \cdot \frac{\partial B}{\partial \theta_j} - U_2 \cdot \phi \cdot \frac{\mathrm{d}G}{\mathrm{d}\theta_j} \end{aligned} \tag{8}$$

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 7 and denoting $\phi := \frac{\partial T}{\partial \tau} \cdot \frac{\mathrm{d}G}{\mathrm{d}\tau}$ as the mechanical share of total deficit reduction following a tax increase. Intuitively, first-order welfare changes come only from changes to the tax and transfer system. The mechanical UI transfer $\frac{\partial B}{\partial \theta_j}$ matters in proportion to the marginal utility while unemployed U_1. The resulting total effect on the government budget $\frac{\mathrm{d}G}{\mathrm{d}\theta_j}$ requires changing taxes while employed. ϕ is the share of this tax change that has a first-order welfare impact, and this matters in proportion to the marginal utility while employed U_2.

To aid interpretation of Equation 8, we substitute in the total government cost decomposition from Equation 6 and normalize the entire equation. The normalization divides through by $U_2 \cdot \phi \cdot \frac{\partial B}{\partial \theta_j}$. The U_2 rescaling translates the utility welfare change into a money-metric, the $\frac{\partial B}{\partial \theta_j}$ rescaling translates UI policies of different magnitudes into a common unit of "welfare gain per dollar that provides first-order welfare gain", and rescaling by ϕ focuses attention on the welfare costs depending on the choice of UI policy θ. The resulting equation is:

$$\frac{\mathrm{d}W}{\mathrm{d}\theta_j} = \frac{U_1}{U_2 \cdot \phi} - 1 - \frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta_j}} \tag{9}$$

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

In the parlance of the Baily-Chetty formula for optimal UI, $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is the "benefit-side" and $\frac{\left(\frac{\partial B}{\partial \mathbf{Y}} - \frac{\partial T}{\partial \mathbf{Y}}\right) \cdot \frac{\partial \mathbf{Y}}{\partial \theta}}{\frac{\partial B}{\partial \theta}}$ is the "cost-side" (Baily, 1978; Chetty, 2006). For a UI benefit expansion, the former captures the (first-order) welfare gain from transferring consumption across employment states, while the latter represents the (first-order) welfare loss from raising additional revenue beyond the mechanical transfer of money across employment states. In this sense, the BCMC ratio is a sufficient statistic for the efficiency costs of different UI benefit reforms $\mathrm{d}\theta_j$: for a given gap in marginal utilities across employment states, a lower BCMC ratio indicates that a UI benefit reform is likelier to be welfare-increasing.[23]

While the above model is quite general, one plausible generalization worth noting is allowing for effects of UI policy reforms $\mathrm{d}\theta_j$ independent of effects on taxes and transfers. For the worker, administrative hassles could directly affect utility. If so, the right-hand side of Equation 9 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 $\mathrm{BCMC}_j < \mathrm{BCMC}_k$ for different UI policies θ_j and θ_k. The first term $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ depends on the final transfer of consumption across states rather than the source of this transfer, so the money-metric unit welfare change is higher for the UI policy margin j than k ($\frac{\mathrm{d}W}{\mathrm{d}\theta_j} > \frac{\mathrm{d}W}{\mathrm{d}\theta_k}$). Consequently, a budget balanced policy reallocation away from k towards j ($\mathrm{d}\theta_k < 0 < \mathrm{d}\theta_j$) raises total welfare.

Different UI policy margins tend to apply to different types of workers, and this can have important implications for making welfare comparisons across policy reforms. Without a representative consumer, social welfare is an aggregation of individual utilities, and the aggregation weights may depend on individual characteristics or marginal utility itself (Saez and Stantcheva, 2016). As a concrete example, the monetary eligibility minimum earnings threshold θ_j affects UI benefits for relatively low-earning claimants who either receive some UI benefits or none at all, while the maximum WBA threshold θ_k affects UI benefits for relatively high-earning claimants who are already eligible for UI benefits. The relative gap in marginal utilities across employment states $\left(\frac{U_1}{U_2 \cdot \phi} - 1\right)$ is plausibly larger for the monetary eligibility earnings threshold θ_j than the maximum WBA threshold θ_k, as diminishing

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

marginal utility implies that the first dollar of insurance is more valuable than additional dollars of insurance. Moreover, utilitarian preferences for redistribution would imply that those affected by monetary eligibility have higher generalized social marginal welfare weights, as they have higher marginal utility due to lower baseline income. While a balanced budget policy reform affecting different types of workers would no longer be a Pareto improvement due to distributional consequences, it could in theory be combined with reforms along existing non-UI policy dimensions to generate one (Hendren, 2020).

5.2.2 Empirical Implementation

Our mapping of the theoretical framework to empirical estimates for each UI policy θ_j focuses on estimating BCMC_j. For simplicity, we assume as in the baseline setup that the UI policy reform $d\theta_j$ affects the government budget only through net transfers (i.e., $\frac{\partial G}{\partial \theta_j} = 0$). Section 5.2.4 explores plausible generalizations.

Behavioral costs are estimated as causal effects on government expenditures, while the mechanical transfer is estimated by applying the policy reform to counterfactual behavior in the absence of the reform. For measuring government expenditure, we directly observe only UI benefits and UI-covered labor income. We consider 8 quarters of earnings starting with the quarter of UI claim filing. To maintain comparability with Schmieder and Von Wachter (2016), we apply a 31.47% tax rate to translate labor income to taxes paid.[24] Our definition of government expenditures as UI benefits minus implied tax revenue thus does not account for effects on other government transfers (Leung and O'Leary, 2020). For studying policy reforms, our exogenous variation is individual-level rather than aggregate. This individual-level variation depends on features of UI claim applications, so we always condition on the realized sample of UI claimants. This ignores any effect a UI policy reform could have on the composition of UI claimants as outlined in Figure 1: either selection into unemployment, selection into claim filing conditional on unemployment, or, for the case of separation-based eligibility, selection into an eligibility determination.

We next outline the general strategies for estimating the BCMC for each margin. The

[24] This is the US total tax wedge in 2015 according to the OECD. It is similar to the 34.3% marginal tax rate for federal, state, and payroll taxes estimated by NBER TAXSIM for a single California worker in 2015 with our separation-based analysis sample's average income.

total cost is always the causal effect of policy variation on net transfers, while the estimation strategy for isolating the mechanical transfer depends on the specific UI policy margin. Appendix C details sample construction, estimating equations, and identification assumptions. It also provides graphical intuition for these calculations based on hypothetical employment responses to different types of UI benefit expansions.

Separation-based eligibility. Available policy variation for the extensive margin of separation-based eligibility is the probability of eligibility approval for a given UI claimant. Holding fixed the composition of UI claimants with separation-based eligibility issues, the total government cost of approving the marginal claimant is estimable using our instrumental variables (IV) research design.

To build intuition for the mechanical transfer, suppose that claimants receive UI benefits if and only if they are deemed eligible. The total government cost causal effect is estimated off of marginal claimants, so the mechanical transfer is the counterfactual benefit amount implied by a marginally denied claimant's monetary entitlement and subsequent nonemployment duration. This is an untreated potential outcome for compliers, which our IV design recovers.

To measure counterfactual benefits, we proportionately allocate benefits starting in the quarter following the claim. For example, counterfactual 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 $WBA \cdot \min\{\max\{PBD - 13k, 0\}, 13\}$.

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

Our strategy for inferring counterfactual benefits 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 an positive bias, while a negative correlation generates a negative bias.

Monetary eligibility. Available policy variation for the extensive margin of monetary eligibility is the minimum earnings threshold. Because this criterion also determines whether claimants receive any benefits or none at all, our measurement approach mirrors that for recovering the separation-based eligibility's BCMC.

The primary distinction in estimation is that our identifying variation comes from a minimum earnings threshold—specifically, the level of high-quarter wages—that facilitates a regression discontinuity design (RDD). The same intuition for identifying causal effects carries through, as a local RD can be interpreted as an IV with monetary eligibility as the endogenous treatment and (recentered) high-quarter wages. In terms of treatment effects, the total cost to the government of monetary eligibility is the RDD estimate on net government transfers. This is the size of the discontinuity in net transfers across the monetary eligibility threshold. In terms of counterfactual outcomes, the mechanical transfer is counterfactual benefit receipt for marginally denied claimants. This is the level of counterfactual benefits for those who fall just below the minimum earnings threshold.

Finally, is worth noting that, by definition, claimants on the margin of monetary eligibility have very low formal earnings. Table C1 shows the average prior quarterly earnings for those around the threshold is only $1,267, which is less than one-fifth the value for the sample of claimants with separation-based eligibility issues.

Weekly benefit amount. Available policy variation for the intensive margin of WBA is the kink in the mapping of prior earnings—specifically, the level of high-quarter wages (HQW)—to WBA around the maximum WBA. Our causal research design is therefore a regression kink design (RKD).[25]

[25]This strategy follows Bell et al. (2022b), who estimate and decompose the heterogeneous impacts of

The total government cost estimate follows the same IV intuition as above, where the endogenous treatment is WBA. In words, just as the above monetary eligibility RDD scales the reduced-form discontinuity in net transfer *levels* by the first-stage discontinuity in eligibility *levels*, the WBA RKD scales the reduced-form discontinuity in net transfer *slopes* with respect to HQW by the first-stage discontinuity in WBA *slopes* with respect to HQW.

The mechanical transfer estimate is derived similarly to the one for monetary eligibility: an outcome level for those at the HQW threshold. It is the total number of UI benefit weeks claimed among claimants at the kink. Intuitively, the mechanical transfer of a $1 WBA increase is simply the number of times the claimant receives a benefit payment.

Potential benefit duration Available policy variation for the intensive margin of PBD is the kink in the mapping of prior earnings—specifically the ratio of high-quarter wages to base period wages ($\frac{HQW}{BPW}$)—to PBD around the maximum (regular) PBD. Our causal research design is therefore once again a regression kink design (RKD).

The total government cost is estimated exactly as above with net transfers as the outcome, except with (total) PBD as the endogenous treatment and $\frac{HQW}{BPW}$ as the running variable. The mechanical transfer estimate is the fraction of claimants at the kink exhausting benefits scaled by their WBA. In words, it is the additional benefit week only for claimants who otherwise remain unemployed through benefit expiry.

5.2.3 Results

Table 7 summarizes the cost decomposition for each type of UI benefit expansion. For each benefit expansion type, the unit of individual-level treatment is in parentheses adjacent to the benefit expansion type. Column 1 reports the treatment effects for each benefit expansion type on net government transfers, which we measure UI benefit dollars paid minus tax revenues collected. Because each benefit expansion type has different units, treatment effects are not directly comparable with each other. All of the point estimates are positive and highly statistically significant, indicating that no type of benefit expansion is self-financing.

WBA expansions over different time periods in the same dataset that we use.

Table 7: Decomposition of Benefit Expansion Costs for Different UI Policy Margins

	(1)	(2)	(3)	(4)	(5)
	Total cost	Total tax cost	Total benefit cost	Mechanical cost	$\frac{\text{Behavioral cost}}{\text{Mechanical cost}}$
Separation-based (eligibility)	2468	-78	2546	2902	-0.15
	(400)	(367)	(145)	(92)	
Monetary eligibility (eligibility)	591	294	297	449	0.32
	(21)	(21)	(3)	(2)	
Weekly benefit amount ($)	36	6	30	23	0.57
	(2)	(2)	(< 1)	(< 1)	
Potential benefit duration (weeks)	66	16	50	35	0.88
	(6)	(5)	(3)	(< 1)	

Notes: Each row is a different UI policy treatment margin estimated within our California data. The coefficient in Column 1 is the causal effect of treatment on net government transfers. It is the sum of those in Columns 2 and 3, which are causal effects on total UI benefit dollars and total income taxes, respectively. The mechanical cost is the counterfactual benefit dollars untreated claimants would receive were they to receive treatment but not change their behavior. Column 5 is the BCMC ratio, which is calculated by subtracting the coefficient in Column 3 from the coefficient in Column 1 and dividing this difference by the coefficient in Column 3. Robust standard errors are in parentheses. Further details on sample construction and estimation are in Appendix C.

This treatment effect on benefits paid to claimants minus taxes paid by the claimants can be further decomposed into treatment effects on each component, which is presented in Columns 2 and 3. Monetary eligibility, PBD, and WBA all decrease tax revenues, as the positive coefficients indicate an increase in tax costs to the government; these effects are all highly statistically significant. The striking difference is that separation-based eligibility does not decrease tax revenues. This mirrors our earlier finding in 5 on average quarterly earnings following the claim, where the magnitude differences in Table 7 are due to including earnings in the quarter of claim filing, applying a tax rate, and summing over quarters rather than averaging.

The decomposition relevant to efficiency costs, however, is separating the total government cost into (i) benefits mechanically provided to claimants in the absence of behavioral responses and (ii) benefits paid and taxes lost due to claimants changing their behavior.

Column 4 isolates the mechanical transfer of UI benefits, which we estimate in different regression specifications depending on the benefit expansion type. By definition, expanding benefit generosity mechanically increases benefits paid to claimants. Accordingly, all of the coefficients are positive and highly statistically significant.

The share of an increase in UI benefits mechanically due to program rule changes can be calculated as the ratio of the coefficient in Column 4 over the coefficient in Column 3. In theory, as long as there is no offsetting behavioral response that is a force towards decreased benefit receipt—such as a decrease in unemployment duration or takeup in response to a benefit expansion—100% is an upper bound for this share. In practice, the mechanical benefit transfer we estimate for both extensive margins of eligibility exceeds its margin's total effect on benefits received. Because we do not find any direct evidence of offsetting behavior, it is likely that this discrepancy is driven by the previously discussed measurement error in inferring counterfactual benefits for eligibility margins.[26] Nevertheless, we interpret this as evidence that the vast majority of the increase in UI benefits due to eligibility is mechanical. On the other hand, for the intensive margins of WBA and PBD, we find that only approximately two-thirds of the benefit increase is mechanical.

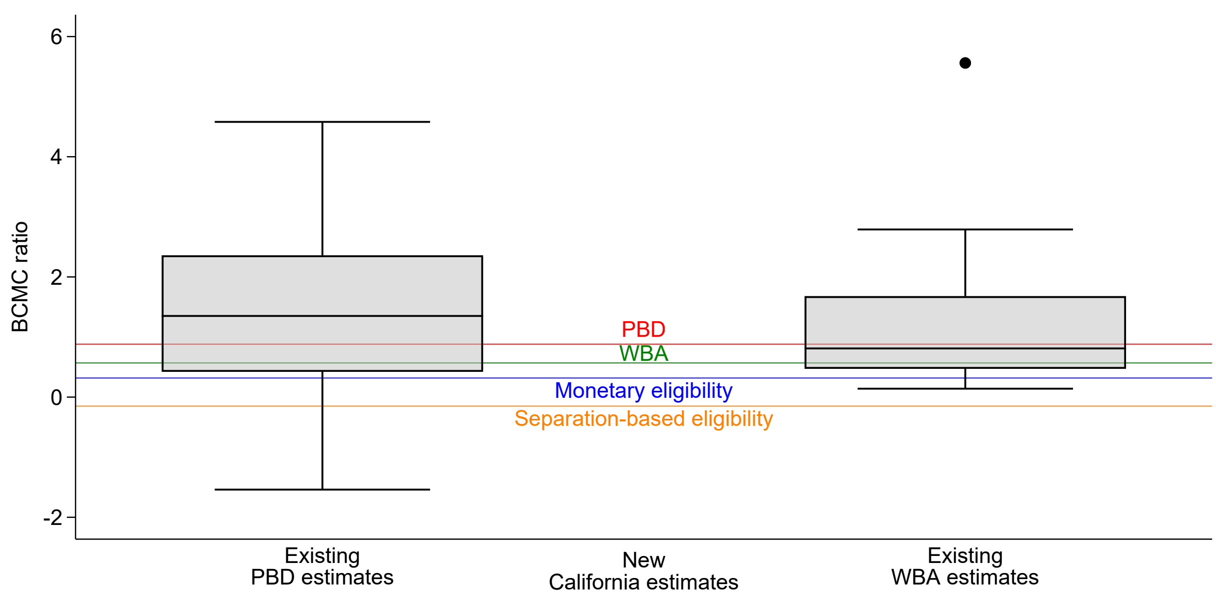
Column 5 puts the previous pieces together to calculate the welfare-relevant object of interest: the ratio of behavioral costs to mechanical costs. The mechanical cost is the coefficient in Column 4 and the behavioral cost is the increase in total government costs after accounting for the mechanical transfer (i.e., the coefficient in Column 1 minus the coefficient in Column 4). Taking our estimates at face value, we find that the behavioral response to separation-based eligibility *increases* tax revenue, which leads to a *negative* behavioral costs and thus a negative BCMC ratio. However, given the previously discussed measurement concerns and our focus on making relative comparisons policy margins, we direct to the robustness of the relatively low separation-based eligibility BCMC. For example, even if we take the upper bound of the revenue change confidence interval (so that there is $641 revenue decrease rather than a $78 revenue increase) and assume the mechanical transfer is only 90% of the total benefits increase (which is well outside the confidence interval), we

[26] In line with the tax cost of monetary eligibility shown in Column 2 Row 2 of Table 7, the RDD estimate of monetary eligibility on any employment in the subsequent quarter is -0.03 (SE=0.008) and on quarters of nonemployment duration is 0.20 (SE=0.05).

still find a separation-based eligibility's BCMC ratio of 0.39. This is similar to monetary eligibility's BCMC ratio of 0.32 and still well WBA's of 0.57 and PBD's of 0.88. Moreover, if we apply the same robustness procedure to monetary eligibility's BCMC ratio, its BCMC ratio increases to 1.36.

Finally, we situate these California-specific BCMC estimates within the wider UI literature. Analogous to Figure 5, Figure 6 plots our BCMC estimates against those from papers surveyed by Schmieder and Von Wachter (2016). Just as we find a larger BCMC for PBD than WBA in California, the literature's median BCMC estimate for PBD is larger than its median BCMC estimate for WBA. Our WBA and PBD estimates both lie within the interquartile ranges of corresponding literature estimates, indicating that our institutional context is comparable to others around the world.

Figure 6: Comparison of Estimated BCMC Ratios with the Literature



Notes: The graph plots values of the BCMC ratio, which is defined as the ratio of behavioral costs to mechanical costs for a UI benefit expansion. The vertical coordinate of each horizontal line corresponds to the BCMC ratio of the adjacent UI policy margin written in the same color as the horizontal line. The left-hand side box-and-whisker chart represents 17 BCMC estimates from Schmieder and Von Wachter (2016), and the right-hand side box-and-whisker chart represents 17 BCMC estimates from the same review. We exclude the two BCMC estimates for PBD from Schmieder and Von Wachter (2016) that are multiple orders of magnitude larger than those from the other studies.

The key takeaway from Figure 6 is that the separation-based eligibility's BCMC is well below those for other margins in various contexts. Even the aforementioned "robustness" value of 0.39 for separation-based eligibility's BCMC would be the fourth-lowest BCMC (i.e., below the 25$^{\text{th}}$ percentile) within either the set of WBA estimates or PBD estimates from the literature.

5.2.4 Other Considerations

While we show in 5.2.1 that the BCMC is a sufficient statistic for the efficiency cost of a UI benefit expansion and in 5.2.2 that the BCMC for separation-based eligibility is lower than those for other UI policy margins, it is important to revisit the caveats mentioned in those sections when considering the policy implications of our findings. The following brief discussion focuses on consumption-smoothing benefits, administrative costs, social welfare weights, and other behavioral responses we cannot observe in our data. We believe the first two reasons are likely to increase the total welfare effect of separation-based benefit expansions, while the last reason is likely to decrease it.

First, unobserved consumption-smoothing benefits are likely to increase the welfare benefits of separation-based eligibility expansions for two reasons. As Equation 9 demonstrates, the gap in marginal utilities across employment statuses captures the consumption-smoothing benefits of the benefit expansion. One reason, as shown in Tables 2 and C1, 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 implies a larger gap in marginal utilities. Another reason is that those impacted by intensive margin benefit expansions were already receiving inframarginal UI dollars, while those impacted by extensive margin benefit expansions were not receiving any UI. Again, with diminishing marginal utility of consumption, the first dollar of insurance provides the greatest consumption-smoothing value.

Second, 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.

Third, 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.

Finally, 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.[27] 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). Another plausible channel is a change in the composition of employers' offered jobs and desired candidates. For example, employers may be less willing to employ workers they deem to be at risk of quitting due to personal circumstances or committing misconduct. We believe either of these general equilibrium responses is likelier to be important if the policy change is non-marginal and clear to outside parties, such as adding an entire category of UI-eligible quits. On the other hand, they are less likely to be important if it is a marginal shift in the probability of eligibility approval.

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

6 Conclusion

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

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

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

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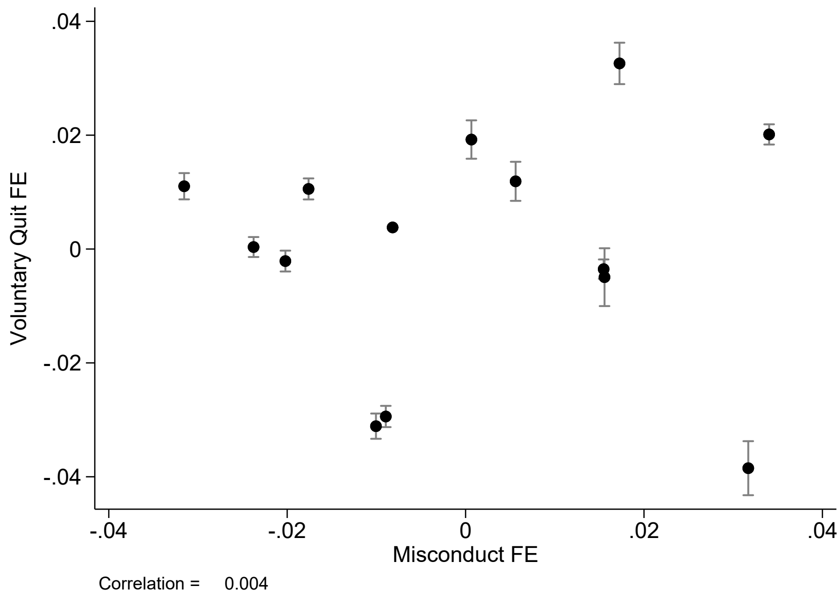
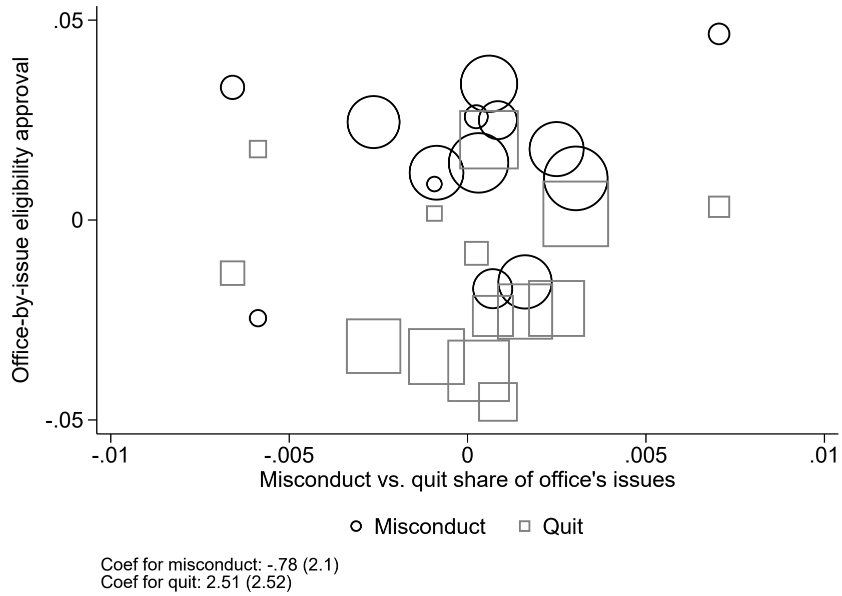
A Additional Tables and Figures for Office Assignment Research Design

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



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

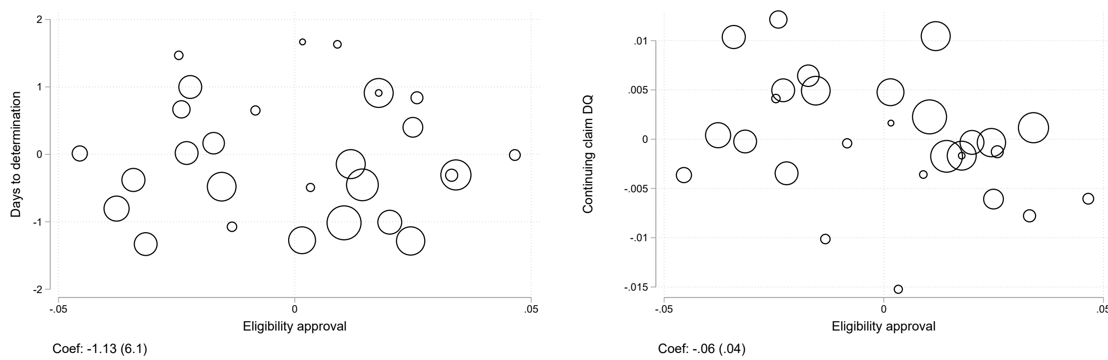
Figure A2: Minimal Variation in Office-level Issue Type Share is Uncorrelated with Issue-specific Approval Rates



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

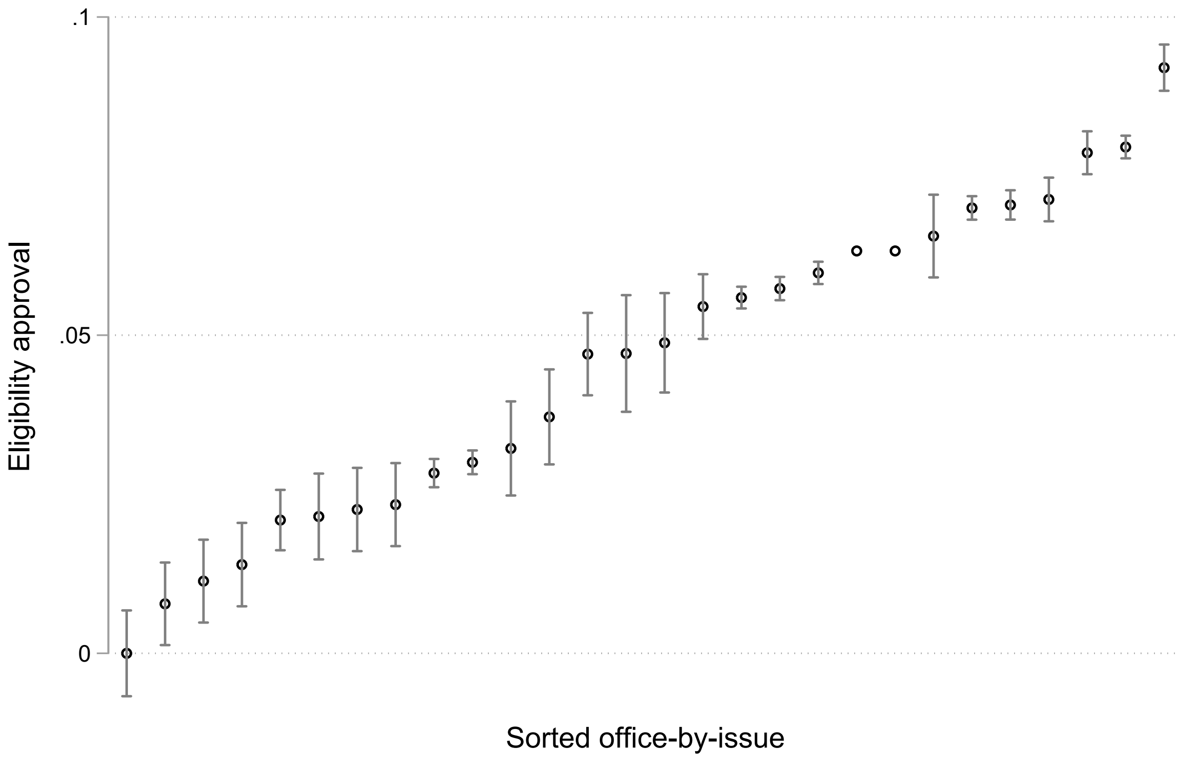
Figure A3: Other Office-level Processing Differences are Unrelated to Eligibility Approval Propensity

(a) Initial Claim Adjudication (b) Continuing Claim Adjudication



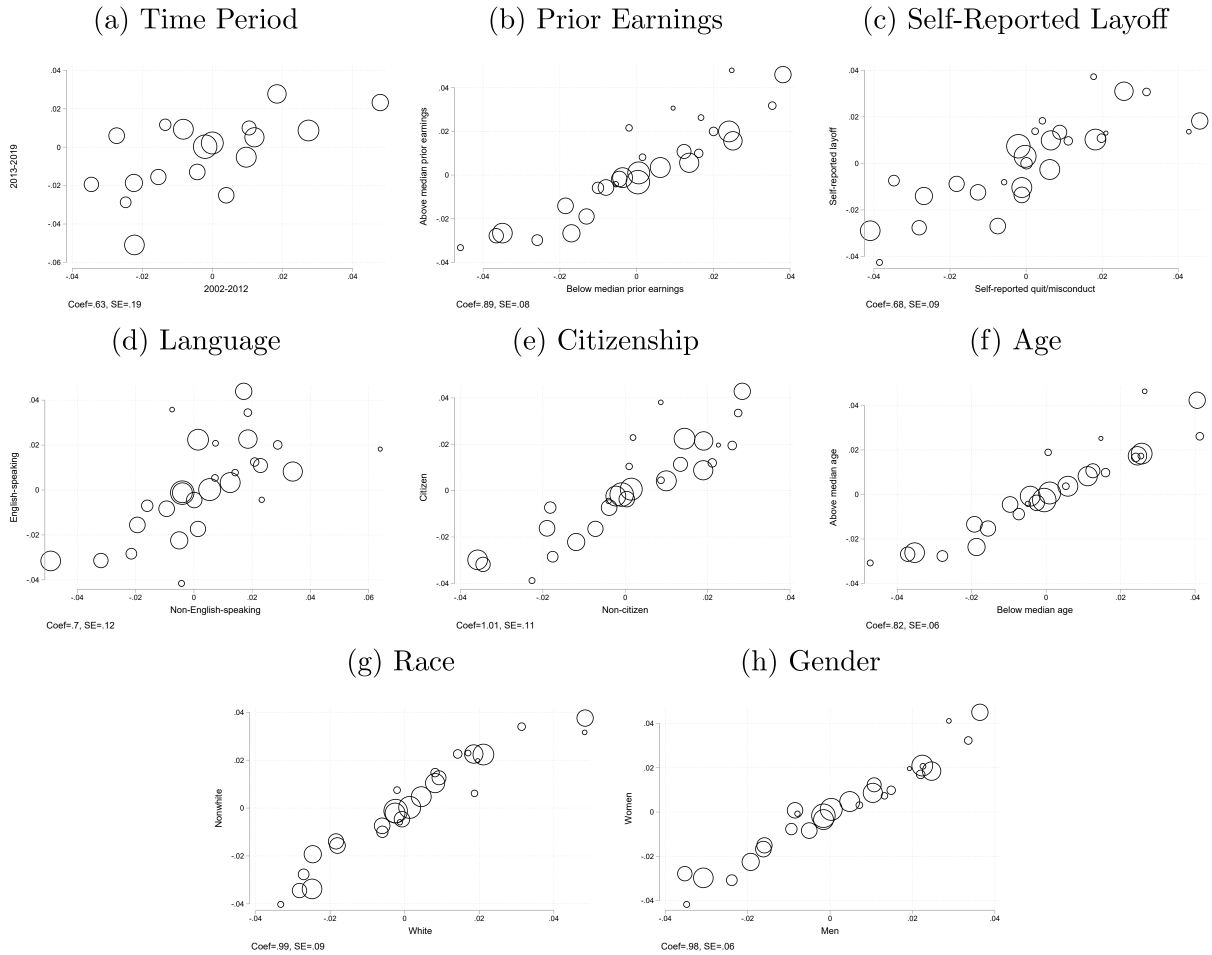
Notes: The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019. Each bubble is an office-by-issue pair. Bubble size is proportional to the number of eligibility issues the office-by-issue pair adjudicates. The x-axis indexes office-by-issue coefficients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

Figure A4: Variation in Eligibility Approval Propensities Across Office-by-Issue Pairs



Notes: The 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. The sample includes all regular initial UI claims with separation-based eligibility issues between 2002 and 2019.

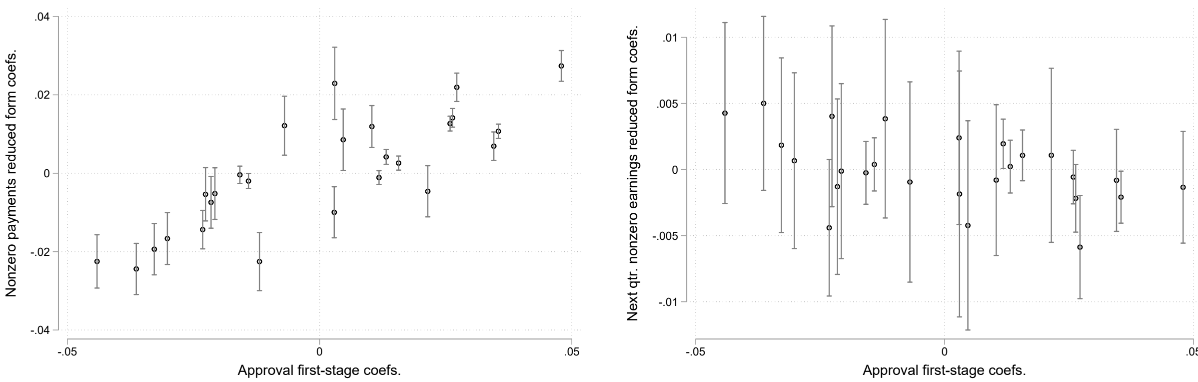
Figure A5: Consistency of Approval Rates Across Demographics and Within Office-by-Issue



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 A6: Visual IV Representation of First-Stage and Reduced-Form Effects

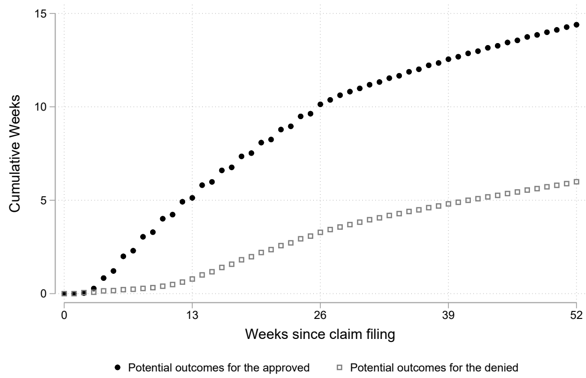
(a) Any payments (b) Any earnings 1 qtr. after



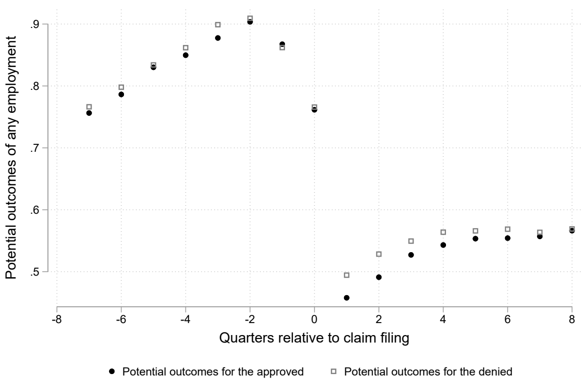
Notes: Each panel presents reduced-form coefficients for office-by-issue pairs on the y-axis and first-stage coefficients for office-by-issue pairs on the x-axis. Both sets of coefficients come from Equation 2, where the outcome is eligibility approval for the first-form and the variable in the graph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

Figure A7: Dynamic Potential Outcomes by Treatment Status

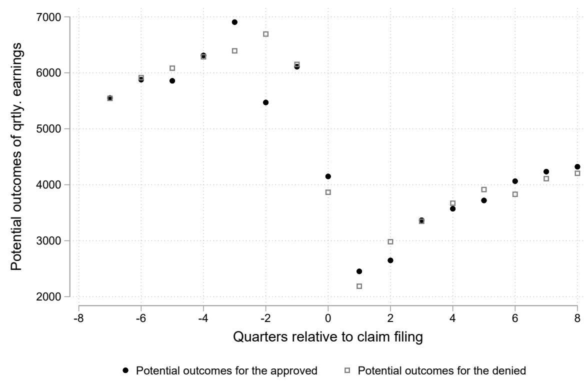
(a) Paid Week



(b) Any Earnings



(c) Avg. Earnings Incl. 0's



Notes: Each panel decomposes the treatment effects in Figure 4 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.

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 5. 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 Subsequent Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Gender		Race		Time Period			
	Male	Female	White	Nonwhite	'02-'06	'07-'11	'12-'16	'17-'19
IV	-0.03	-0.04	-0.03	-0.04	-0.08	-0.03	-0.03	-0.05
SE	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
tF-SE	[0.02]	[0.02]	[0.02]	[0.01]	[0.02]	[0.02]	[0.02]	[0.02]
F	214	196	151	261	241	243	118	92
N	2.7m	2.6m	2.1m	3.4m	1.4m	1.8m	1.5m	0.9m

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 any earnings in the quarter following the initial claim. Each column represents a separate model estimated on a given subsample. 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Prior Earnings Quartile				Gender		Race		Age Quartile				Time Period			
	1^{st}	2^{nd}	3^{rd}	4^{th}	Male	Female	White	Nonwhite	1^{st}	2^{nd}	3^{rd}	4^{th}	1^{st}	2^{nd}	3^{rd}	4^{th}
IV	0.28	0.33	0.36	0.39	0.32	0.33	0.32	0.33	0.33	0.37	0.35	0.27	0.41	0.30	0.34	0.43
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
tF-SE	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]	[0.01]	[0.01]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]	[0.02]	[0.02]
F	127	100	101	103	214	196	151	261	171	121	102	86	241	243	118	92
N	1.5m	1.5m	1.5m	1.5m	2.7m	2.6m	2.1m	3.4m	1.5m	1.5m	1.5m	1.5m	1.4m	1.8m	1.5m	0.9m

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 receipt of any UI benefits. 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. The 1^{st} time period is 2002-2006, the 2^{nd} is 2007-2011, the 3^{rd} is 2012-2016, and the 4^{th} is 2017-2019. The tF adjustment affects subsamples with an F-statistic below 104.7 and uses a linear interpolation between Table 3A values in Lee et al. (2022). All robust standard errors are at the 95% confidence level and are clustered by claimant.

B Separation-Based Eligibility Effects using Examiner Assignment

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

B.1 Estimating Equation

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

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

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

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

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

Including language, office, issue, and time fixed effects is motivated by the assignment mechanism discussed in Section 2.3. First, while language fixed effects are most important to our identification strategy, they are unlikely to be quantitatively important. In particular, even though language certainly affects both a claimant's examiner assignment probabilities and plausibly affects subsequent employment outcomes, Table 2 shows that 94% of the sample speaks English. Second, while the assigned office is quasi-randomly assigned based on SSN, we include assigned office fixed effects to strengthen the exclusion restriction. This controls for any potential effect the office can have on claimants apart from the eligibility decision. Third, issue type fixed effects are necessary due to the instrument including an interaction with issue type. Finally, month-of-claim fixed effects are included due to changing macroeconomic conditions. California's unemployment rate gradually fell from 5% to 4% between 2017 and 2019, but claimants later on in the sample period faced the severe Covid-19 recession several quarters after their claim. If the pool of examiners remained fixed during this time period, then increased precision would be the sole benefit of these time fixed effects. However, due to some examiner hiring and attrition, these also address a potential confound due to changes in examiner composition over time.

B.2 Validating the Instrument

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

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

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

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

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

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

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

Table B1: Claimant Balance Across Examiner Leniency

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

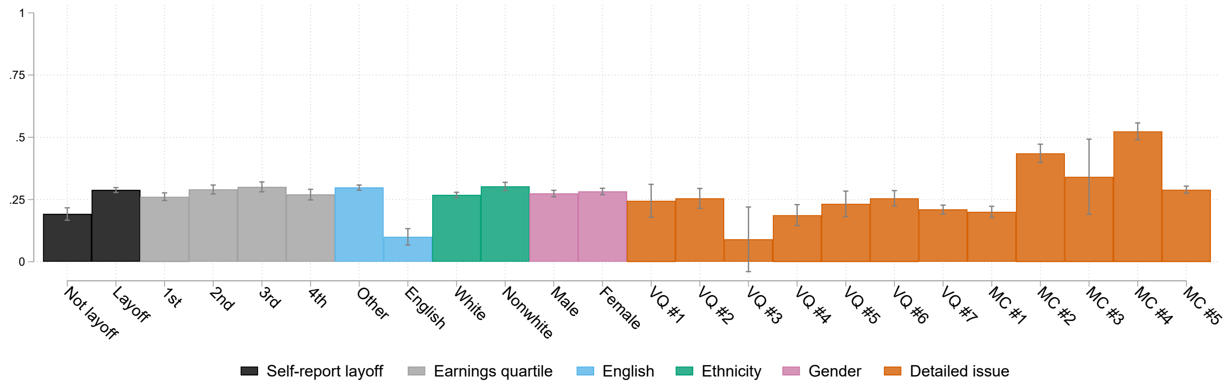
Notes: The overall sample consists of all regular UI initial claimants between 2017 and 2019 with a separation-based eligibility issue. The sample of algorithmically scheduled claims limits to examiners who handled at least 200 such claims. Column 1 reports claimant demographics at the time of claim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

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

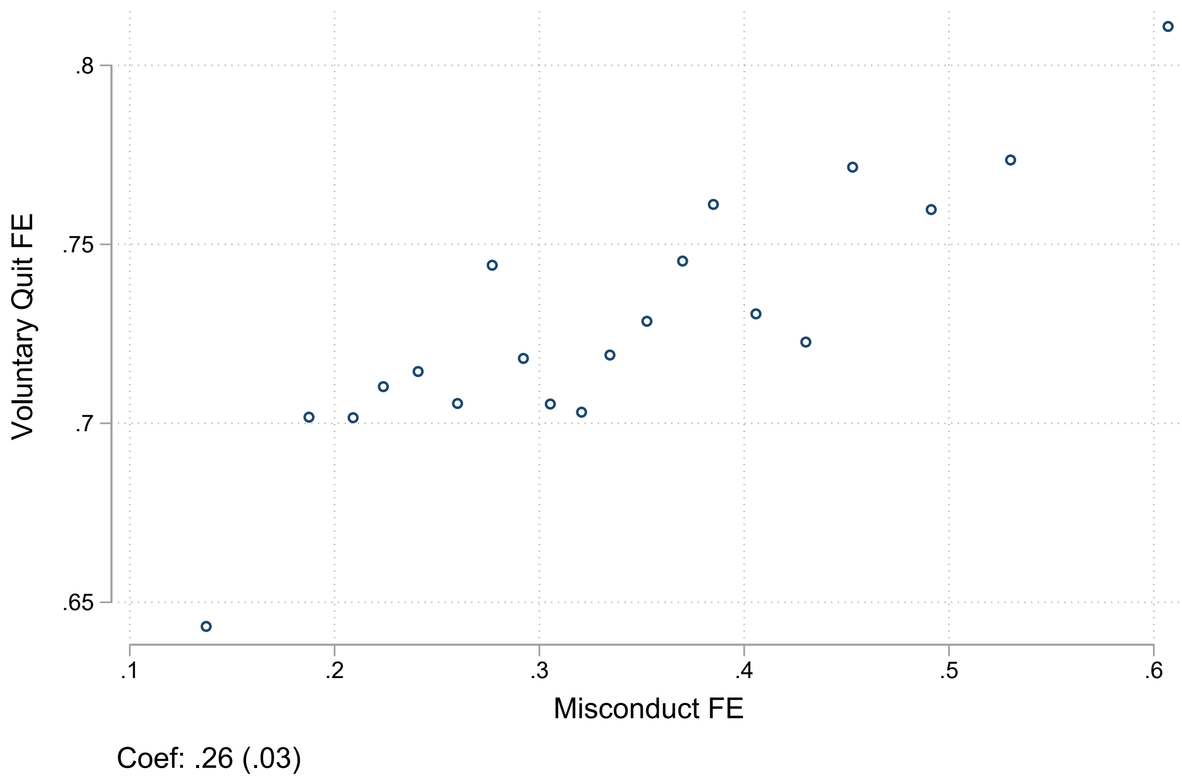
The third identification assumption—first-stage relevance—requires that the examiner assignment be predictive of the endogenous treatment. We directly test this by estimating the first-stage regression equation Equation B.2 and testing the joint significance of the examiner dummies. The first-stage F-statistic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

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

Figure B1: Consistency of Examiner Leniency Measures



(a) Positive First-Stage within Claimant Subsamples



(b) Positive Correlation between Issue-Specific Leniency

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

B.3 Results

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

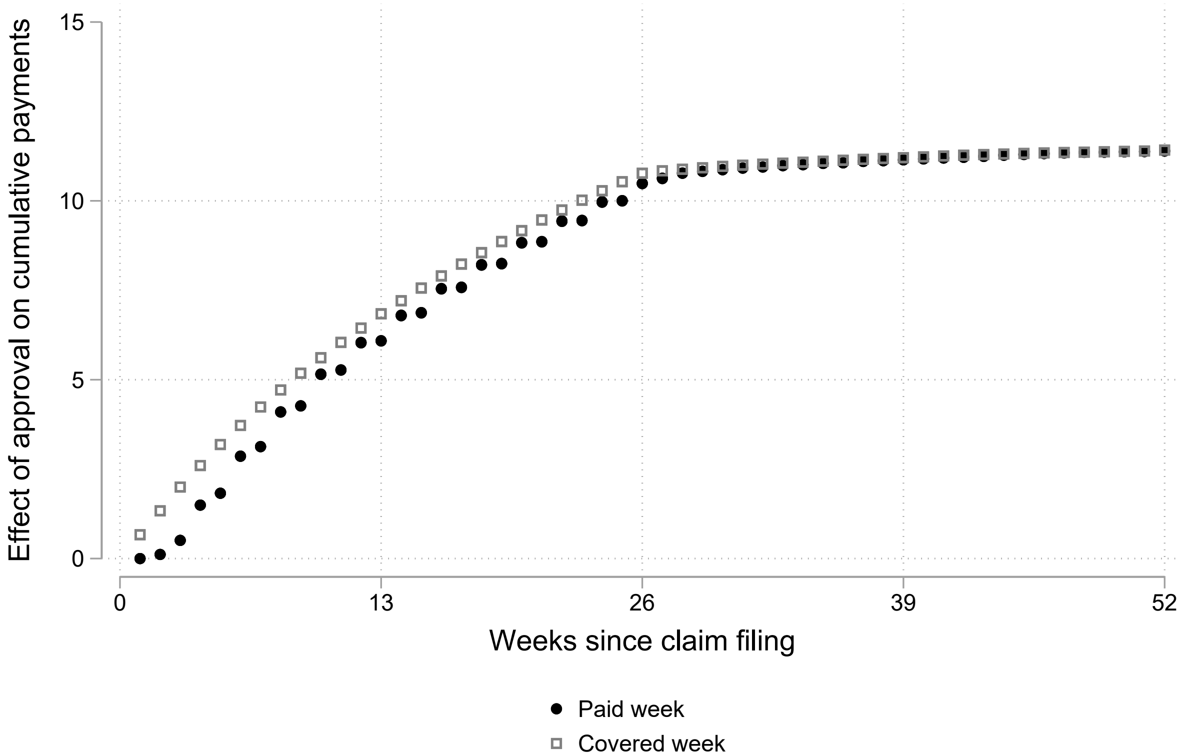
Table B2: Effects of Initial Eligibility Approval

	(1)	(2)	(3)	(4)
	Any Payments	Earnings 1 qtr. after	Emp. 1 qtr. after	Consecutive qtrs w/o earnings
IV	0.55	-653	-0.08	0.25
SE	(0.01)	(183)	(0.01)	(0.05)
tf-adjusted SE	[0.01]	[357]	[128]	[0.10]
OLS	0.02	973	-0.06	0.20
SE	(0.00)	(30)	(0.00)	(0.01)
Untreated complier mean	0.26	3,293	0.56	1.94
First-stage F	8			
Unique N	396k			

Notes: 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 IV results are estimated from Equations B.1 and B.2 using UJIVE where the endogenous treatment D is initial eligibility approval. 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.

The dynamic effects of payment receipt mirror those in the office-based design, as Figure B2 replicates Figure A5 using examiner assignment as eligibility approval variation. The cumulative effect of eligibility approval on payment receipt is just over 10 weeks of benefit payments.

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



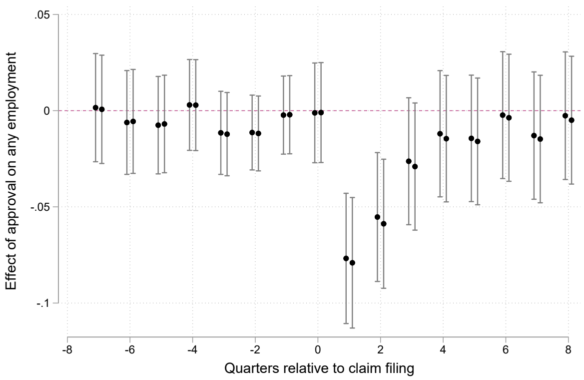
Notes: This figure displays coefficients from separate regressions of the form in Equation B.1 and Equation B.2, where the outcome is a measure of cumulative payments as of that week. A paid week refers to the calendar week the payment is made, while a covered week refers to the week of unemployment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

Figure B3 displays the dynamic employment effects of eligibility. The patterns are consistent with those in Figure 4, as any negative employment effect dissipates two years after the claim. The primary difference is that the point estimates for impacts on average total earnings are negative and only marginally insignificant.

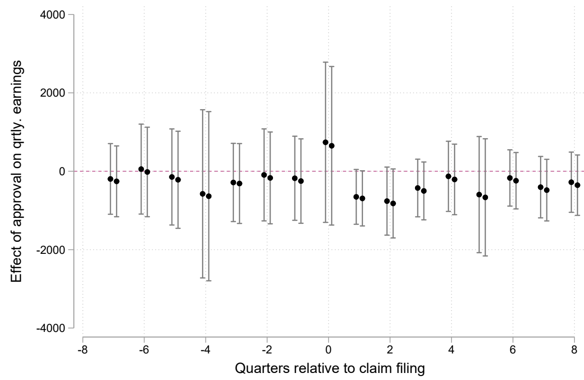
Within each panel, there are two series. One series includes office fixed effects while the other excludes office fixed effects. The similarity between the two series suggests that the downstream impacts of office-level eligibility variation are consistent with those of examiner-level eligibility variation,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

Figure B3: Dynamic Impacts of Eligibility Approval on Employment, Both Including and Excluding Office Fixed Effects

(a) Any Employment



(b) Avg. Qtrly Earnings



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

C Replicating Other UI Research Designs in California Data

This section contains supporting details regarding the institutional details and necessary sample restrictions for each of the non-separation-based UI policies we estimate in the California data. The research design for monetary eligibility (weekly benefit amount, potential benefit duration) uses a discontinuous change in the level (slope) in the policy at a cutoff for some continuous measure of prior earnings; the difference in levels (slopes) of an outcome across the cutoff is the reduced-form effect, the difference in levels (slopes) of the UI policy across the cutoff is the first-stage effect; and the ratio of the reduced-form effect to first-stage effect is the local average treatment effect of the UI policy for those around the cutoff (Cattaneo et al., 2020).

The primary threat to identification for all the RD/RK designs is manipulation that leads claimants locally above the cutoff to be different than those locally below the cutoff for reasons other than the UI policy treatment of interest. This could be due to strategic filing behavior by the claimant (e.g., knowing one's own earnings history along with UI eligibility rules and declining to file a claim if monetarily eligible) or incomplete record-keeping (e.g., some records of claims denied on monetary eligibility grounds are not retained). We test for this in two ways. First, we implement two distinct tests for discontinuities in the sample density around the cutoff (McCrary, 2008; Cattaneo et al., 2020). Second, we examine the conditional expectation of an auxiliary measure of earnings that is not directly related to the earnings measure used to determine UI benefits.

C.1 Monetary Eligibility

Institutional Details The base period is defined as the set of quarters used to determine monetary entitlement. The standard base period is defined as the earliest 4 of the 5 completed quarters preceding the UI claim, and the alternative base period is defined as the 4 completed quarters preceding the UI claim. Base period wages are defined as the sum of all earnings during the base period, and high quarter wages are defined as the highest quarter of earnings during the base period.

There are two ways claimants can be monetarily eligible: (i) high-quarter wages at least

\$1,300 or (ii) high-quarter wages at least \$900 and a ratio of base period wages to high-quarter wages of at 1.25. Monetary entitlement for claims filed prior to April 2012 is determined solely by the standard base period. For claims filed after April 2012, if the claimant is monetarily ineligible using the standard base period, monetary entitlement is then checked using the alternative base period. In all time periods, claimants can appeal a monetary eligibility denial by producing evidence of UI-covered earnings that were erroneously excluded from administrative records.

Sample Restrictions We restrict to claims filed prior to April 2012, as the alternative base period qualification almost entirely removes the discontinuity in monetary eligibility at $HQW^* = 0$.

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

$$HQW^*(BPW) = \begin{cases} HQW - 900 & \text{if } BPW < 1.25 \\ HQW - 1300 & \text{if } BPW \geq 1.25 \end{cases} \tag{C.1}$$

We also consider only claims with $HQW^* \in (-900, 2500)$.

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

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{C.2}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{C.3}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is an indicator for being recorded as monetarily eligible; and $T_i = \mathbf{1}[HQW_i^* \geq 0]$. The fuzzy RD estimator for the causal effect of monetary eligibility, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on T_i, $\hat{\tau} = \hat{\tau}_y / \hat{\tau}_d$. We use the constant β_0 to calculate the mechanical transfer with Y_i as counterfactual benefits.

Manipulation and First-Stage Tests Panel (a) of Figure C2 shows a clear discontinuity in the sample density across the monetary eligibility cutoff. The density is approximately 10%

lower just below the cutoff, and this missing mass is consistent with either strategic claim filing or incomplete record-keeping. Panel (b) shows that average prior earnings are very slightly but discontinuously lower just above the cutoff. Finally, Panel (c) shows a clear discontinuous increase in UI benefit receipt across the cutoff.

C.2 Weekly Benefit Amount

Institutional Details Within a given year, WBA is determined solely by high-quarter wages. 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. The target replacement rate was 0.39 prior to 2002, 0.45 in 2003, and 0.5 thereafter. Therefore WBA is:

$$WBA(t) = \min\left(\frac{HQW}{13} \cdot RR(t), WBA^{max}(t)\right) \tag{C.4}$$

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 Bell et al. (2022b), we restrict to claimants with the full regular potential benefit duration of 26 weeks. This excludes those with especially variable earnings across quarters.

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

$$HQW^*(t) = HQW - \frac{13}{RR(t)} \cdot WBA^{max}(t) \tag{C.5}$$

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

$(-5000, 5000)$. *Estimating Equation* We estimate 2SLS linear regressions. The reduced-form and first-stage equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{C.6}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{C.7}$$

where Y_i is an outcome measure (e.g., total UI benefits received); D_i is the recorded WBA; and $T_i = \mathbf{1}\left[HQW_i^* \geq 0\right]$. The fuzzy RKD estimator for the causal effect of an additional \$1 of WBA, which we use for calculating the total behavioral cost, is the ratio of the reduced-form and first-stage coefficients on the interaction term $HQW_i^* \cdot T_i$, $\hat{\beta} = \hat{\beta}_2 / \hat{\beta}_2$. It is a fuzzy RKD because the actual WBA awarded to claimants may differ if they appeal with non-recorded UI-eligible wages. We use the constant β_0 to calculate the mechanical transfer with Y_i as the number of paid weeks.

Manipulation and First-Stage Tests Panel (a) of Figure C3 shows a visually smooth sample density across the WBA kink. Though the density appears to be mostly smooth across the entire support in Panel (a), both density manipulation tests reject smoothness across the the kink. As discussed in Bell et al. (2022b), this is due to bunching of high-quarter wages in even increments of \$1,000. This is not a problem as long as the "bunchers" are not unobservably different than nearby "non-bunchers" (Barreca et al., 2016). Reassuringly, Panel (b) shows average prior quarterly earnings are smooth through the kink. Finally, Panel (c) shows a clear discontinuous change in WBA slope across the kink in line with program rules.

C.3 Potential Benefit Duration

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

$$MBA = \min\left(\frac{1}{2} \cdot BPW, WBA \cdot 26\right) \tag{C.8}$$

Regular PBD is then defined as the number of weeks a claimant can receive their WBA before exhausting their regular MBA. Rearranging Equation C.8:

$$PBD = \begin{cases} 26 & \text{if } WBA \cdot 26 \leq \frac{1}{2} BPW \\ \frac{\frac{1}{2} \cdot BPW}{WBA} & \text{if } WBA \cdot 26 > \frac{1}{2} \cdot BPW \end{cases} \tag{C.9}$$

Substituting in the case when $WBA < WBA^{max}$ from Equation C.4 into Equation C.9:

$$PBD = \begin{cases} 26 & \text{if } 4 \cdot RR(t) \leq \frac{BPW}{HQW} \\ \frac{13}{2 \cdot RR(t)} \cdot \frac{BPW}{HQW} & \text{if } 4 \cdot RR(t) > \frac{BPW}{HQW} \end{cases} \tag{C.10}$$

Equation C.10 demonstrates the kink in the regular PBD formula with respect to $\frac{BPW}{HQW}$ we exploit.

There are several benefit extensions during our sample period. These benefit extensions increase the total PBD at a given point in calendar time in proportion to the regular PBD.[28] 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.[29]

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

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

$$\frac{BPW}{HQW}^*(t) = HQW - \frac{BPW}{HQW} \cdot 4RR(t) \tag{C.11}$$

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

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

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

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

equations are:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 HQW_i^* + \beta_2 HQW_i^* \cdot T_i + \varepsilon_i \tag{C.12}$$

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 HQW_i^* + \alpha_2 HQW_i^* \cdot T_i + u_i \tag{C.13}$$

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

Manipulation and First-Stage Tests Panel (a) of Figure C4 shows a visually smooth sample density across the PBD kink. The the density is smooth across the entire support in Panel (a), and both manipulation tests do not come close to rejecting the null of density continuity across the kink. Additinally, Panel (b) shows average prior quarterly earnings are smooth through the cutoff. Finally, Panel (c) shows a noisy but clear discontinuous change in PBD slope across the kink in line with program rules.

C.4 Additional Tables and Figures for Comparing UI Policy Margins

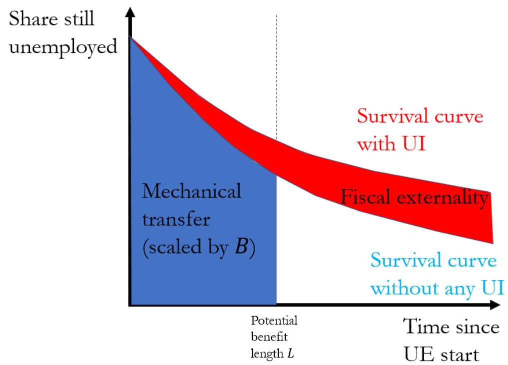
Table C1: Summary Statistics of Each UI Policy Analysis Sample

	(1)	(2)	(3)	(4)
UI Policy	**Separation eligibility**	**Monetary eligibility**	**Weekly benefit amount**	**Potential benefit duration**
Research Design	**IV**	**RDD**	**RKD**	**RKD**
Avg. prior qtrly earnings ($)	6,893	1,267	7,565	3,258
WBA ($)	274	76	341	221
Regular PBD (weeks)	24.4	20.0	26	25.3
Total PBD (weeks)	45.0	45.1	48.5	48.8
Any UI benefit receipt	0.59	0.49	0.78	0.72
P(Exhaust UI | any UI)	0.18	0.10	0.13	0.10
Quarters of UE	2.4	2.4	1.8	1.5
Years	'02-'19	'02-'12	'00-'19	'00-'19
N	7.1m	2.9m	8.7m	1.2m

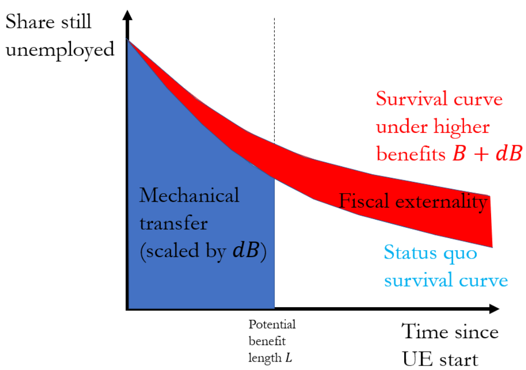
Notes: This table contains average characteristics for the relevant samples in each research design using the California microdata. For separation-based eligibility, the office-based IV sample is our main analysis sample of all separation-based eligibility issues from regular initial claims between 2002 and 2019. For monetary eligibility, the RDD sample restricts to those with high quarter wages fewer than $900 below the threshold and or $2,500 above the threshold, where the threshold is usually $900, during the pre-2012 time period without alternative base period qualification. For weekly benefit amount, the RKD sample restricts to those with high quarter wages within $5,000 of the kink and at the maximum potential benefit duration. For potential benefit duration, the RKD sample restricts to those with a ratio of base period wages relative to high quarter wages within 50 percentage points of the kink and below the maximum weekly benefit amount.

Figure C1: Graphical Illustration of the BCMC Ratio

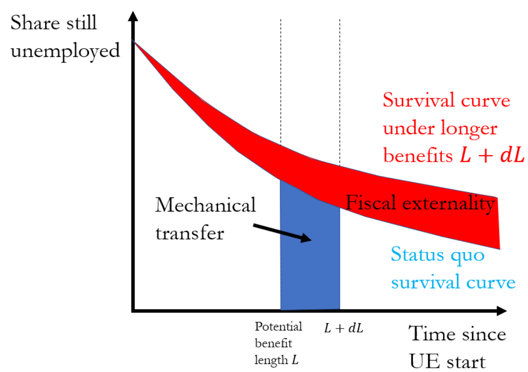
(a) Separation-based/Monetary Eligibility Expansion



(b) Weekly Benefit Amount Expansion



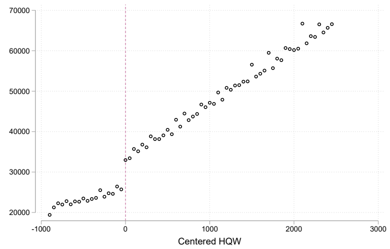
(c) Potential Benefit Duration Expansion



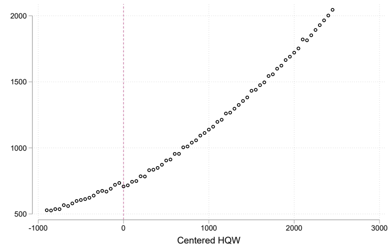
Notes: The panels illustrate calculating behavioral and mechanical costs to the government of different types of UI benefit expansions. Each panel displays hypothetical survivor curves with and without the benefit expansion for a homogeneous population, where the vertical distance between survivor curves represents an increase in nonemployment duration in response to benefit expansions. Under the status quo policy, the weekly benefit amount is B and the potential benefit duration is L. 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 C2: Monetary Eligibility Regression Discontinuity Design

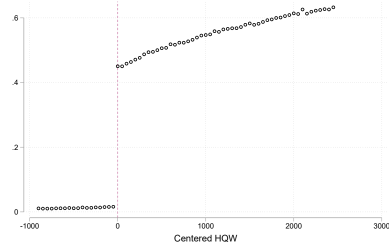
(a) Sample Counts



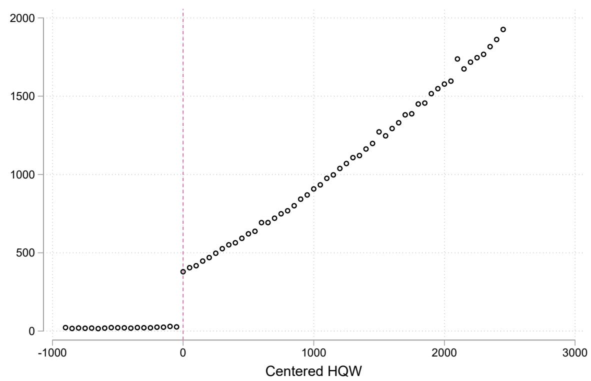
(b) Avg. Prior Earnings



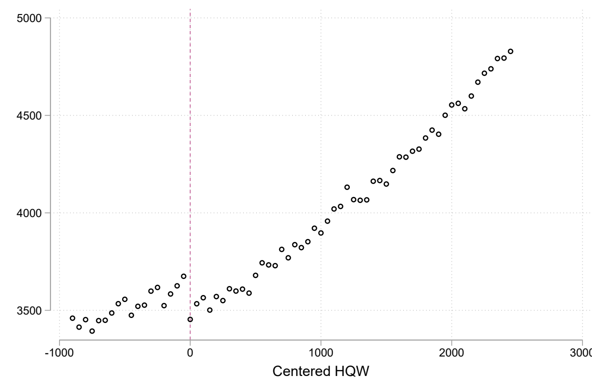
(c) Any Payments



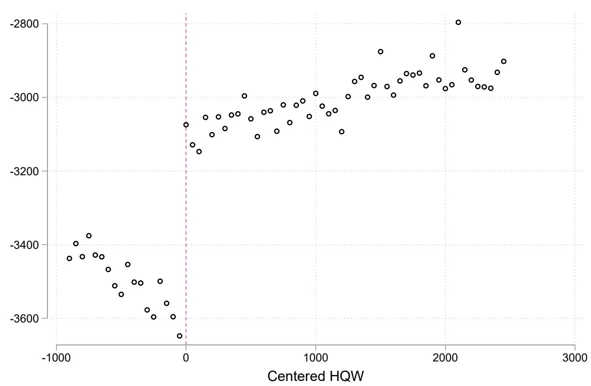
(d) UI Benefits



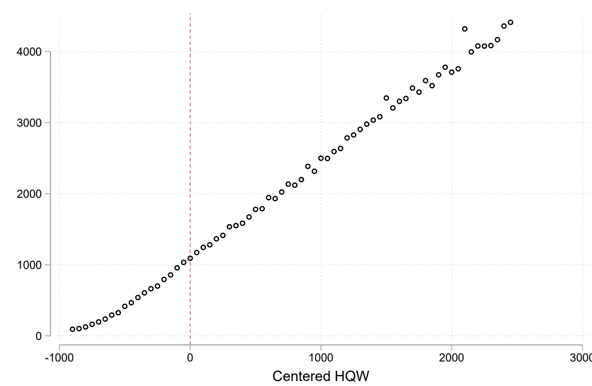
(e) Taxes



(f) Net Govt. Revenue

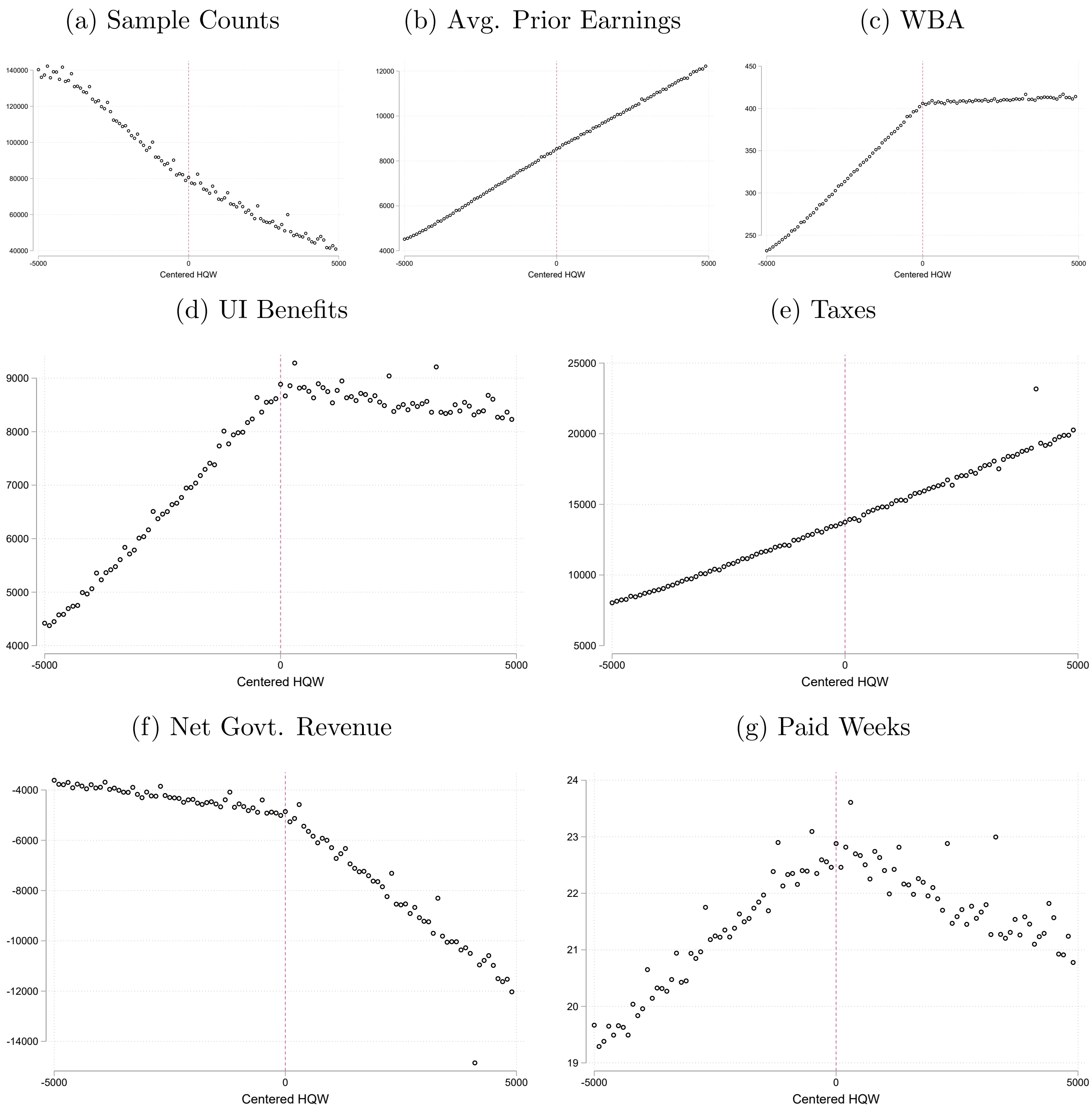


(g) Counterfactual Benefits



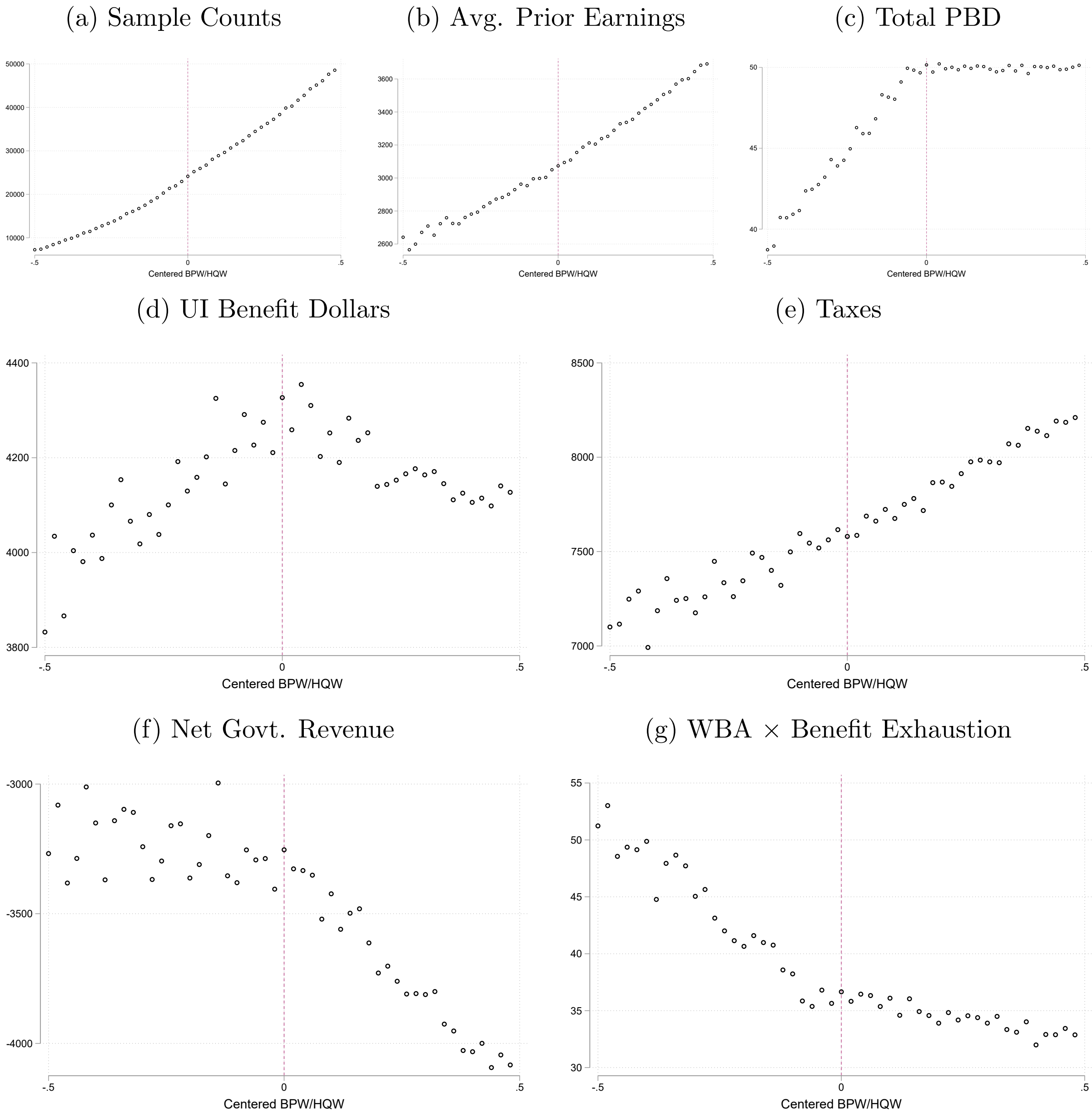
Notes: Each panel is a binned scatterplot of the monetary eligibility analysis sample described in Table C1. The running variable is high-quarter wages relative to the year-specific cutoff and the bin width is \$50. Panel (a) are counts in the sample. *p*-values for the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are both 0. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is an indicator for receiving any UI benefit payments. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is unscaled counterfactual benefits, whose cutoff value we scale by monetary eligibility's effect on payment receipt (0.52) to calculate the mechanical transfer.

Figure C3: Weekly Benefit Amount Regression Kink Design



Notes: Each panel is a binned scatterplot of the weekly benefit amount analysis sample described in Table C1. The running variable is high-quarter wages relative to the year-specific kink and the bin width is \$100. Panel (a) are counts in the sample. p-values for the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are 0 and 0.02, respectively. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is the actual WBA awarded to the claimant. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is the number of paid benefit weeks, whose value at the kink is the mechanical transfer.

Figure C4: Potential Benefit Duration Regression Kink Design



Notes: Each panel is a binned scatterplot of the potential benefit duration analysis sample described in Table C1. 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) are counts in the sample. p-values for both the McCrary (2008) test and Cattaneo et al. (2020) test of a discontinuity in density are 0.14 and 0.79, respectively. Panel (b) is a placebo outcome of average quarterly earnings in the 7 quarters prior to the initial claim. Panel (c) is the total PBD awarded to the claimant, which is the sum of regular PBD and any benefit extensions. Panel (f) is taxes paid in Panel (e) minus benefits received in Panel (d). Panel (g) is the number of paid benefit weeks, whose value at the kink is the mechanical transfer.