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The Effect of Unemployment Insurance Eligibility in Equilibrium

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Abstract: In the United States, workers whose past earnings were below a threshold are generally ineligible for unemployment insurance (UI), creating a discontinuous jump in the value of being unemployed. Using a regression discontinuity design with administrative panel data, we estimate a sizable local effect from UI eligibility on earnings in the next employer, around 10 percent per quarter. This evidence, however, understates UI's causal effect because of endogenous non-compliance. It also does not distinguish between underlying reasons for higher re-employment earnings, a higher share of production, or more productive matches. These are addressed through a quantitative model. The underlying causal effect is 50 percent higher than the empirical estimates, and nearly all of the effect comes from workers getting a larger share.

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Key words: unemployment insurance, directed search, earnings

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1 Introduction

Unemployment insurance (UI) protects workers against the risk of job loss by replacing income. While it helps smooth consumption over this labor shock, UI could also affect the recipients' job search behavior and, in equilibrium, the job offers they receive from firms. Studying this impact is difficult because receiving UI is not random. People who get UI might already differ from those who don't in ways that influence re-employment outcomes and therefore, simply comparing outcomes between UI recipients and non-recipients does not isolate the causal effect of UI.

To answer how UI causally affects earnings and re-employment outcomes, we exploit a unique feature of the UI system in the United States that creates a discontinuity in UI eligibility. Specifically, there is a near-universal criterion that workers' pre-separation earnings exceed a lower bound. We first take an empirical approach by using a regression discontinuity design (RDD) at this monetary eligibility threshold for eligibility. To see workers experiencing unemployment on both sides of this threshold, we use data from a large administrative dataset to estimate the local, causal effect of UI eligibility. We then introduce a model with frictional labor markets and a detailed UI program and take-up decision. This helps interpret and decompose the forces driving the empirical treatment effect, lets us extrapolate the local effects and contextualizes the result to inform search models broadly.

Our empirical estimate for the effect of UI eligibility, the RDD, compares earnings at re-employment for individuals just above and below their state's monetary eligibility thresholds. These eligibility criteria require a minimum amount of earnings in the year prior to separation. Nearly everyone below is ineligible, whereas those above it are mostly eligible. To precisely measure earnings prior to separation, we use an administrative panel, the Longitudinal Employer-Household Dynamics (LEHD) dataset, for workers' earnings and employment histories. This data is constructed using the actual UI offices' data, so we can calculate eligibility without measurement error. With this empirical strategy and dataset, we find a significant effect of UI eligibility on re-employment outcomes. Having earnings put just over the eligibility threshold increases their earnings at their next employer by \$276.91 to \$318.92 in their next full quarter of employment, roughly a 10% increase.

We show observable characteristics are quite smooth across the threshold, both for workers and firms. Further, there is no detectable manipulation in the density of earnings prior to separation. We also run placebo experiments, arbitrarily moving the presumed threshold and testing for discontinuities, and find no effects.

This estimate is notable as new evidence on how UI impacts re-employment outcomes using a new source of exogenous variation. Prima facie, it is quasi-experimental evidence that increasing UI benefits raises re-employment earnings with an elasticity of 0.23. If compliance were perfect, so everyone above the discontinuity received UI and all below did not, then this would directly estimate the local causal effect of changes in a worker's outside option on their next employment earnings. However, a substantial share of the unemployed above the threshold are ineligible for other reasons, and others (endogenously) do not claim UI although they are eligible. Both aspects of unobserved heterogeneity mean that our estimate likely understates the impact of UI on re-employment outcomes if we directly observed claims and receipt—and the reasons for these.

With this in mind, we construct a frictional model of the labor market to estimate and better understand the underlying treatment effect. We build on the Moen (1997) and Acemoglu and Shimer (1999) frameworks, incorporating a detailed UI system and match effects into an equilibrium directed search model. In the model, workers search for jobs posted by firms that offer a piece-rate wage of a match's productivity. Once employed, a firm must pay an operating cost to continue the match, and matches are subject to idiosyncratic taste and productivity shocks. As a result, a worker may choose to quit when they receive a negative taste shock, or a firm may choose to fire a worker when the productivity of a match is too low. In the model as in the data, UI eligibility is affected by the reason for a separation and a worker's base period earnings prior to separation. Claiming is a choice influenced by the worker's state and transitory shocks. Hence, the key features clouding our interpretation of the data are present in the model, and we can use this to quantify the "true" impact of UI.

To use the model as a quantitative tool, we calibrate it to match our RDD estimates, features of the UI system, and basic labor market flow data. This allows us to go from our 10% empirical RDD estimate to a "true" treatment estimate of 15.32% by comparing re-employment earnings of those with monetary eligibility who claim and receive UI to those without monetary eligibility who do not claim and do not receive UI. This is almost like a treatment-on-treated estimate, except that it would be contaminated by the endogenous choice to be treated, which we are addressing using the model. Although our empirical estimate was sizable, our model suggests it drastically understated the effect of UI for recipients.

We find that UI increases earnings primarily because it increases a worker's average piece rate. At the threshold, the treatment effect on the piece-rate increases it by an average of 7.93%, while match productivity increases only negligibly (0.23%). The key feature driving this dichotomy in the model is that the tradeoff between unemployment duration and piece rate is relatively flat, so workers are quite responsive in this aspect. Whereas, movements in the domain of acceptable productivity jobs only censor the lower tail and minimally increase the mean.

Even though match productivity does not change much, the total value of the match does increase. It does so by decreasing worker quitting and thereby increasing the match's expected duration. So, even though increasing the piece rate transfers value from the firm to the worker, firms are nearly indifferent, while workers see the very large earnings effects. We could have seen large earnings effects because worker bargaining was near zero, so their earnings closely follow their outside option, but instead their implied bargaining power is about 60% and the effect is a combination of the increased value of the match and the transfer to them.

These same forces affecting workers at the true threshold can be inferred throughout the distribution of prior earnings. Essentially, we can use the model to extrapolate from the local treatment effect to understand UI more globally. We find that near the current cut-off, UI creates the largest discontinuity between receivers and non-receivers. This is because lowering the monetary eligibility rapidly reduces the benefit amount toward a level commensurate with a worker's outside option, reducing their incentive to undertake costly claiming activity. Above the current threshold, the impact is muted by the presence of a benefits cap, again driving outside options above and below the threshold together. Accounting for these differences in outside options and the sources of these differences is crucial for understanding the true impact of UI on the labor market.

Our work primarily relates to two strands of the literature, one empirical and one quantitative. Empirically, we build on a growing body of empirical research documenting the effects of unemployment insurance (UI) on labor market outcomes, particularly the heterogeneous impact of UI policies across different worker groups (Skandalis et al., 2022). A number of papers analyze the impact of UI on unemployment durations by exploiting a natural experiment for identification. Marinescu and Skandalis (2021) examines the impact of UI on job search behavior and finds that UI causes negative duration dependence in target wages. Their focus is on the dynamics of job search and complements our results on the job-finding rate. Marinescu (2017) examines the impact of UI extensions during the Great Recession and finds that a 10 percent increase in UI benefit duration reduced state-level job applications by 1 percent. Lalive (2008) uses RDD to analyze a UI extension tied to work history thresholds, identifying a causal effect of extended benefits on unemployment duration. Lalive and Zweimüller (2004) examines a unique policy change in Austria that extended UI duration from 30 to 209 weeks, finding a 17 percent decline in the transition rate after accounting for endogeneity. Similarly, Card et al. (2007) studies a sharp discontinuity in Austria's UI and severance pay eligibility, and finds a 5 to 9 percent reduction in the job-finding rate due to UI extensions. Only a small fraction of the literature examines the effect of UI on broader labor market outcomes, such as match quality and post-unemployment wages, and the findings remain mixed. Addison and Blackburn (2000) uses the Displaced Worker Survey (1983–1990) and finds little evidence of a positive UI effect on wages, although their findings are limited to UI claimants and do not provide a causal estimate. More recently, Schmieder et al. (2016) leverages a quasi-experiment of UI policy changes in Germany to estimate the causal effect of extended UI duration on wage offers and found a small negative effect of UI extensions on post-unemployment wages. However, due to the nature of their research design, their sample is restricted to long-term unemployed workers and hence a different local effect is estimated. Nekoei and Weber (2017) finds that UI improves re-employment firm quality and attenuates the wage loss associated with unemployment. We focus on different aspects of the UI system for identification and provide complementary evidence that suggests much stronger effects for workers with high marginal utility. Griffy (2021) uses data from the Survey of Income and Program Participation (SIPP) and between-state variation in replacement rates over time and finds a positive effect on re-employment earnings and a negative effect on hazard rates.

The closest empirical design to our work is Leung and O'Leary (2020), which is another RDD estimate on eligibility. They use a slightly different eligibility criterion, highest quarterly earnings, and look at a slightly different set of outcomes. Notably, they see longer unemployment duration, which reduces earnings in the quarter immediately after separation. This is qualitatively consistent with our findings, which see higher earnings *upon re-employment*, which the model predicts would coincide with mildly longer durations. Quantitatively, our duration effects are much smaller, which could be due to differences in sample and design.

Our paper is also closely related to the UI literature that develops equilibrium models in which workers make both UI take-up and employment decisions, allowing for the quantification of UI effects in a general equilibrium setting. Validating its inclusion in our model, Auray et al. (2019) demonstrates that endogenous UI take-up is essential when constructing an equilibrium model to assess UI's effects. Birinci and See (2023) highlights the importance of accounting for worker heterogeneity, as differences in individual responses to UI can significantly shape observed labor market dynamics. Similarly, Chao (2025) shows that UI eligibility considerations are important in constructing policy and have heterogeneous effects for different groups of workers. Our paper contributes by incorporating endogenous separation decisions from both workers and firms, addressing a key yet often overlooked component of UI eligibility: the separation requirement. In combination, our novel identification approach provides credible estimates for a group likely to be affected by UI, and our quantitative approach allows us to further understand our empirical estimates.

The rest of the paper is organized as follows. In section 2, we discuss the structure of UI policy in the United States, and our data sources. In section 3, we conduct our primary empirical analysis. In section 4, we construct a quantitative labor search model. In section 5, we calibrate our model and use it to understand our empirical findings. section 6 concludes.

2 Data

We begin by describing our data sources and their unique features that enable our empirical approach. We construct a panel of state-level UI laws, which includes eligibility requirements. We combine this panel with administrative data from the Longitudinal Employer-Household Dynamics (LEHD).

2.1 Unemployment Insurance Eligibility Requirements

Unemployment insurance (UI) is a progressive, conditional transfer program designed to provide consumption insurance for workers who lose their jobs involuntarily. For recipients, UI replaces a fraction of previous income (typically around 50 percent) up to a maximum weekly amount. However, not all separated workers are eligible. To qualify, an applicant must have experienced a no-fault job loss and earned a minimum amount in qualified employment during the "base period," which typically refers to the four quarters before job loss. While base-period earnings are not the only requirement for eligibility, they are usually necessary and, therefore, serve as the basis of our analysis. Other earnings and non-earnings requirements exist, but in nearly all states, workers with base-period earnings below the threshold are ineligible for UI, while those above it may qualify.

Despite relatively low earnings requirements, income eligibility remains a significant factor for many potential claimants. While this threshold represents a small fraction of the overall earnings distribution, it is considerably higher among job losers. During our sample period, about one in five separated workers had base-period earnings below this threshold. Yet, many workers deemed "monetarily ineligible" still file claims, and monetary eligibility requirements account for about half of all rejected initial claims.[1] Additionally, among ineligible claimants, a substantial number still receive UI benefits, often due to imperfect enforcement and variability in state laws over time.

While UI is federally mandated, states have flexibility in setting their own eligibility criteria and benefit provisions. Replacement rates and maximum benefit levels vary across states, and some states impose additional eligibility conditions, such as a minimum earnings

[1] The majority of the remaining rejections result from failing to meet the separation requirement.

threshold for the highest-earning quarter within the base period. However, regardless of additional criteria, all states impose a minimum earnings requirement over the base period.[2]

There is substantial variation in the minimum earnings thresholds across states. The average is $2,621, but the range is considerable. We plot the distribution of these thresholds in Appendix Figure A8. While some of these differences reflect variations in local wage levels, the dispersion of UI thresholds across states is far greater than the variation in state-level wages.

We use these earning eligibility cutoffs to estimate the effect of UI eligibility on re-employment outcomes. In our baseline analysis, we normalize the running variable as the percent deviation from the earnings eligibility threshold in a separator's state. Since percent deviation from the threshold is correlated with base-period earnings, one concern is that our estimates may capture non-linear effects of base-period earnings rather than the true threshold effect.

To address this, we also condition on prior earnings and still find a significant threshold effect. This approach leverages variation across states, as different UI thresholds mean that the same base-period earnings may place workers on different sides of the eligibility cutoff. These analyses are made possible by combining state-level variation in UI thresholds with highly accurate administrative earnings data, allowing for a more precise identification of UI's impact. Appendix A.2 has more information on the UI criteria and state-determined minimums.

2.2 Data on Workers' Earnings History

To track each worker's earning history prior to separation and after re-employment, we use data from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The LEHD is administrative data on covered earnings collected by the states and used in their unemployment insurance systems to determine eligibility. This is crucial for our application: as it is administrative data, it mitigates many of the measurement error concerns to which we would be subject in survey data. And because it includes *all* covered employment, we are able to very precisely determine whether an individual in monetarily eligible when they separate. In addition to earnings, it includes important job and individual characteristics, like state of employment, industry, occupation, tenure, sex, age and imputed education, and race. These features make it a nearly ideal dataset to study the impact of UI eligibility.

Despite its advantages, the LEHD has some shortcomings. Ironically, although it is the data used in state UI systems, it does not include data on UI receipt or application. In addition, it is constructed from quarterly data, which limits our ability to track employment transitions at the same frequency as some available surveys. While these are both noteworthy limitations, the very accurate earnings data observed at just the correct frequency allows us to estimate our effects where public data would misclassify too many workers to reliably observe the threshold effect. In general, if misclassification still exists, it is likely to bias our findings downward because some of the actually eligible will be counted as ineligible and

[2]In most states, the base period is defined as the first four of the past five quarters, excluding the quarter of separation.

vice-versa. This will likely understate the size of any effect.

We follow standard restrictions when constructing our LEHD sample. We create a panel following individuals in 17 states over the period of 1997-2014. [3] From this super-sample that represents approximately 40% of the U.S. labor force over this period, we draw a random 2% sample of individuals, maintaining the panel dimension for these individuals. The panel dimension allows us to identify separations and the resulting unemployment spells, using the approach from Gregory et al. (2021). This approach identifies a separation any time we observe one of three joint earnings and employment outcomes: first, if there is a full quarter of non-employment; second, if two employers abut but without a quarter in which both pay simultaneously; and third, if two employers abut with a quarter of overlapping pay, but which is lower than the minimum of the two adjacent quarters. The first case is unambiguously a separation into unemployment, whereas the latter two attempt to separate job-to-job transitions from transitions through unemployment.

We use the state laws collected in Section 2.1 to calculate base-period earnings exactly as they would be calculated by state UI systems. Although the quarterly frequency of the LEHD seems like it could potentially inhibit our ability to accurately calculate earnings over the year before separation, the structure of this data is actually perfect for calculating eligibility because all states determine UI monetary eligibility by calculating income over *completed* quarters prior to separation. State UI systems calculate base-period earnings by adding up the earnings in all covered employment over the year before the last complete quarter of employment. Though the LEHD does not include some earnings from employment that is not covered by UI, e.g. at the Federal government, the structure of state UI systems again assists our approach: any earnings in non-covered employment also should not be included in base-period earnings calculations.

3 Empirical Evidence on the Effect of UI Eligibility

In this section, we provide quasi-experimental evidence on the impact of unemployment insurance eligibility on workers' search behavior. We exploit a discrete cut-off in UI eligibility created by base period earning requirements as a source of variation for a regression discontinuity design (RDD), a technique pioneered in Chen and van der Klaauw (2008). We use this RDD to document three key facts: First, UI eligible workers experience a 10% increase in earnings upon re-employment. Second, there is little or no difference in the subsequent firms' average wage between eligible and ineligible workers. Third, exposure to UI eligibility appears to be random near the threshold of eligibility. We start by describing our data, the longitudinal employer-household dynamics (LEHD) dataset from the Census Bureau. Then, we discuss our research design and our findings. Last, we describe the implications of our findings for models of labor market search.

[3] The 17 states are California, Colorado, Hawaii, Idaho, Illinois, Indiana, Kansas, Maine, Maryland, Missouri, Montana, Nevada, North Dakota, Tennessee, Texas, Virginia, and Washington.

3.1 Discontinuity-based Evidence on the Earnings Effect of UI Eligibility

With the earnings data from the LEHD, we create a running variable in the RDD estimate. To normalize across states and years, we convert base-period earnings into a percent deviation from the state- and year-specific threshold. Let base-period earnings be $B_{i,t}$ for individual i in quarter t, which is the quarter or the separation. The threshold is given by $\underline{B}_{s(i,t),y(i,t)}$, indexed by the state s in which i resides during quarter t and year y, which corresponds to quarter t. Then we define the percent of the threshold as $\frac{B_{i,t}-\underline{B}_{s(i,t),y(i,t)}}{\underline{B}_{s(i,t),y(i,t)}}$. Most of our analysis will focus on 25% deviation, $-0.25 \leq \frac{B_{i,t}-\underline{B}_{s(i,t),y(i,t)}}{\underline{B}_{s(i,t),y(i,t)}} \leq 0.25$. On the left side of the cutoff, that domain includes about 132,000 observations and the right side includes 101,000 observations.

As the dependent variable, define $y_{i,t}$ as the earnings in the first full quarter of re-employment. Note, t again refers to the quarter of the separation, although these earnings occur at some date in the future. This earnings concept corresponds to the Census' "full quarter employment," requiring that the households also have positive earnings in the following quarter. This is because of time aggregation: we only observe earnings by quarter so if the worker is not employed in the next quarter after reemployment, they likely lost the job during the first quarter of reemployment meaning that our earnings will reflect the length of time before they lost that job, rather than the level of pay.

Figure 1 provides graphical evidence of the threshold effect. Along with the bin-scatter, to help visualize the discontinuity, we include estimates of a 4th-order polynomial on either side of the threshold estimated over a domain of 25% above and below. The open circles are the binned scatter, showing average re-employment earnings within prior-earnings bins chosen by the methods of Calonico et al. (2019).

Notice that the trend on either side of the threshold in Figure 1 is fairly flat. This actually hides the trend that earnings upon re-employment increase quite uniformly with earnings pre-separation. However, the running variable on the horizontal axis is $\frac{B_{i,t}-\underline{B}_{s(i,t),y(i,t)}}{\underline{B}_{s(i,t),y(i,t)}}$, the prior earnings relative to a threshold that varies over states and therefore mixes earnings levels across the horizontal axis and flattens the slope.

In our main specification, Equation 1, the coefficient of interest is that of the dummy for base-period earnings above the threshold. On either side, the regression has separate local polynomial regressions on $\frac{B_{i,t}-\underline{B}_{s(i,t),y(i,t)}}{\underline{B}_{s(i,t),y(i,t)}}$ characterized by vectors of parameters ψ_L, ψ_R for negative and positive values. We also include dummies for the state of separation and the period t. Because the threshold represents different values of the base period earnings, we can also include $B_{i,t}$ as a separate covariate.

$$\begin{aligned} y_{i,t} =& \mathbb{I}(B_t \geq \underline{B}_{s,y}) f\left(\frac{B_t-\underline{B}_{s,y}}{\underline{B}_{s,y}}, \gamma_R\right) + \mathbb{I}(B_t \leq \underline{B}_{s,y}) f\left(\frac{B_t-\underline{B}_{s,y}}{\underline{B}_{s,y}}, \gamma_L\right) \\ &+ \beta B_{i,t} + D_y + D_s + \epsilon_{i,t} \end{aligned} \tag{1}$$

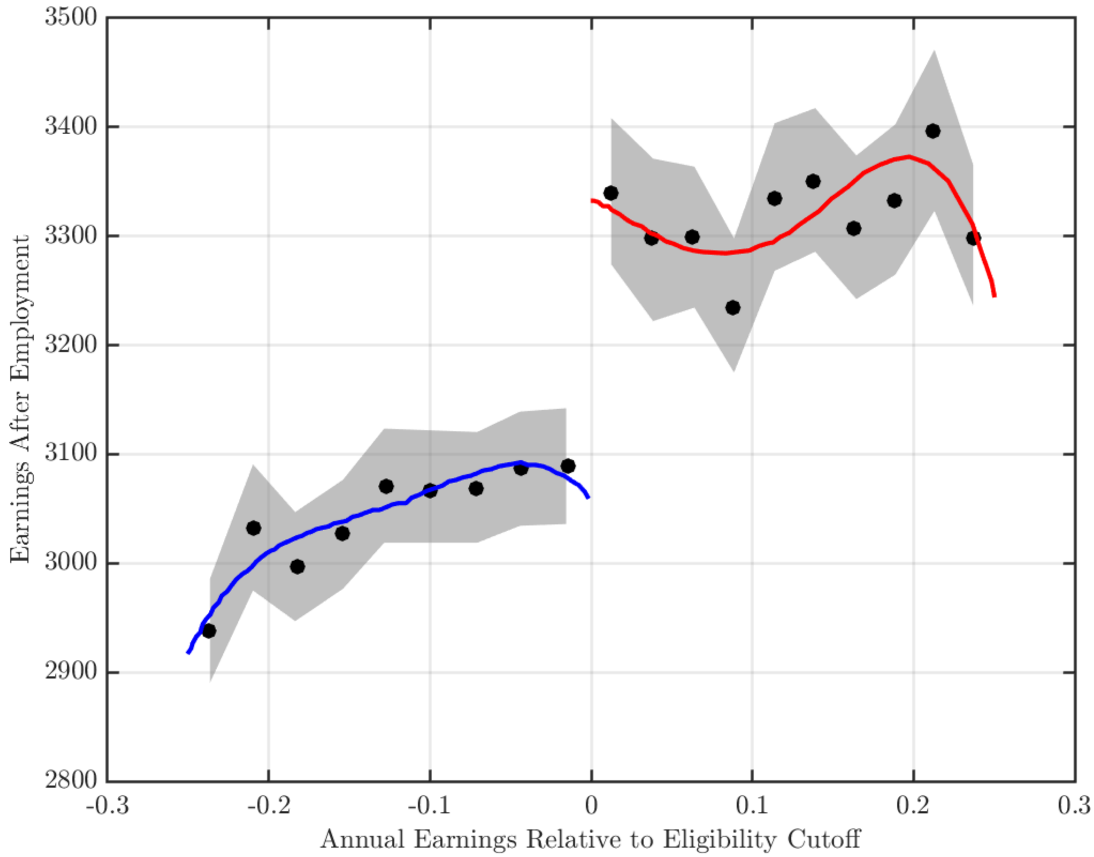


Figure 1: Annual earnings prior to separation as a percent deviation from the state eligibility cutoff against earnings in the next job. Binned scatter and 4th-order polynomial fit.

The jump we observe is given by

$$\gamma = \lim_{B_t \to^{+} \underline{B}_{s,y}} E\left[f\left(\frac{B_t - \underline{B}_{s,y}}{\underline{B}_{s,y}}, \gamma_R \right) |\cdot \right] - \lim_{B_t \to^{-} \underline{B}_{s,y}} E\left[f\left(\frac{B_t - \underline{B}_{s,y}}{\underline{B}_{s,y}}, \gamma_L \right) |\cdot \right]$$

Table 1 shows the estimates for our treatment effect γ, which is just over \$300 in 2013 US dollars. The bandwidths of the kernels are chosen independently on the left-hand and right-hand side, following the data-driven procedures of Imbens and Kalyanaraman (2012). To correct the truncation of these kernels as we approach the cutoff, we use the bias-correction methods presented in Calonico et al. (2014). The first row presents the non-parametric bias-corrected estimator for the treatment effect with classical errors, while the second estimator that combines the bias correction with robust standard errors. In the first and third columns, we estimate without controls for the income level. In the second and fourth, we control for income, which is feasible because the eligibility cutoff differs across states.

The jump is quite consistent across specifications. Households make about 10% more per quarter on the right side of the threshold, eligible for the UI. Notice that the effect is also approximately 10% of the threshold because, on average, the threshold is about \$3K.

Dependent	$y_{i,t}$		$\frac{y_{i,t}}{\underline{B}_{s,y}}$	
	(1)	(2)	(3)	(4)
Bias-Corrected	318.92	276.913	0.102	0.0970
	(67.47)	(69.22)	(0.0351)	(0.0328)
Robust	318.92	276.913	0.102	0.0970
	(80.81)	(82.71)	(0.0415)	(0.0393
With B_t control		X		X

Table 1: Regression discontinuity effect of UI receipt in 2013$ or as a fraction of cutoff. Standard errors in parenthesis

So, when we use as a dependent variable post-employment earnings as a percentage of the base-period threshold, in the third and fourth specifications, we see a very consistent set of estimates. These are helpful however, because they are somewhat scale-free and hence will be useful later as calibration targets.

In specifications with base-period earnings as a control, the marginal effects are slightly smaller. Including the base-period earnings level in the estimator reduces the jump, which could have a number of explanations. It could be because reemployment earnings trend upwards with prior earnings, so if we do not remove that trend, then some of it will appear in the jump–essentially the within-state variation due purely to this cross-employment spell earnings correlation. The estimator is designed, however, to avoid that effect. The control could also reduce the point estimate because states with higher thresholds have a larger effect. This might be opaque not because the true treatment is getting larger at higher levels of earnings, but rather, uptake is higher, and so the right side of the threshold has relatively more actual recipients.

To gauge whether this effect is valid or plausible, the rest of this section will present further statistical evidence. But to pause briefly on the household situation can also add clarity. The earnings thresholds are quite low in many states, which might lead one to believe that these are tertiary earners with very weak labor market attachment, perhaps they flit in and out of employment without much weight to their actions. While it is true that these are low earnings individually, they also tend to be from quite low-earning households. On average, workers who separate with earnings near the threshold have a total annual household income of around $14,000, which is a bit above the 10th percentile for that year. We compute this in the SIPP and show it in Appendix C.

Although our empirical findings credibly establish that UI affects employment outcomes, it is an intent-to-treat regression discontinuity, and so further interpretation is needed. Specifically, the effect we are interested in is how UI receipt changes outcomes. But this is not precisely observed on the right side of our discontinuity because base-period earnings above the eligibility cutoff do not guarantee receipt, and so the earnings of the eligible are a mixture of eligible receivers and nonreceivers. The eligible might not receive benefits for two reasons: they are ineligible for other reasons, or they choose not to claim their benefits. Both sources of non-compliance are potentially endogenous, and so we will explore further in the model how to interpret them.

3.2 Discontinuity-based Evidence on the Source of the UI Eligibility Effect

The earnings discontinuity could be caused either by differences in employment rates after re-employment or by differences in wages, and the two have potentially different economic interpretations. Relatedly, the very low earnings of workers near the eligibility threshold could be because of low base-period employment rates or low base-period wages. In this subsection, we disentangle this using evidence from the LEHD.

Our first bit of suggestive evidence looks at whether unemployment insurance gets workers to stay employed longer, which is related to the productivity of the next match. We find that it does only very slightly, as graphically shown in subsection A.1, Figure A7. We looked for a break in re-employment at the eligibility cutoff, fitting local linear fits independently on either side of the threshold. The jump after re-employment is only about 0.4pp: a minimal increase in employment at the threshold that is statistically indistinguishable from zero and does not suggest that the productivity of the next match was particularly better due to UI.

The exercise also helps to understand why some workers have such low earnings before displacement: they have low employment rates in the pre-period. For example, those whose base-period earnings were two times the state eligibility threshold were employed for about three of four quarters in the base-period and employed for nearly 90% of quarters in the year after re-employment.

To go further, we present two more regression discontinuities using the same formulation as when we presented the next-employer earnings jump, Equation 1. Instead of looking at earnings, we look at the realized tenure in the next employer match and the average wage among the employees of the next employer. The first measure is expected to be related to match productivity in much of the empirical literature, such as Topel and Ward (1992), and in many models such as Menzio et al. (2016). If idiosyncratic match quality is observable and UI allows a worker to wait longer for the arrival of a higher value, then workers with access to UI should generally have longer subsequent matches. The latter measure measures quite directly whether the worker found a more productive firm by measuring if the firm generally pays more. Again, if UI allows the worker to wait longer for the arrival of an offer from a better firm, then the average wage at that firm should be higher. If, however, UI just raises the bargaining prowess of that worker, then it should be neutral on the rest of the workers at the firm, and firms' average wages should be no higher. This is to say, workers match at nearly identical firms upon receiving UI, so the firm component of productivity is not increasing, despite the workers' higher earnings.

Table A8 shows that the discontinuity effects for both job tenure and firm wages are statistically zero. If we could reject that these estimators were zero, then it would be strong evidence for a productivity effect from the UI treatment. In fact, many of the point estimates go in the opposite direction of what we would expect if the UI increased productivity. While this is not definitive proof, especially because match tenure is only an indirect measure of productivity, it is suggestive that the primary mechanism driving our earnings effect comes from rents, workers getting a larger *share* of the productivity of the match.

3.3 Observable Characteristics and Continuity at the Cutoff

Of course, these estimators all rely upon continuity across the cutoff and, that workers are not endogenously choosing to be above or below. This amounts to testing for manipulation and bunching of the distribution of the running variable.

To begin addressing these concerns, Table 2 shows several characteristics and their standard errors for a window of 2% in the running variable above and below the cutoff. Along most of the demographic dimensions that we can observe in the LEHD, there is little economically meaningful difference between those above and below the threshold. Of particular interest is the tenure variable, which is calculated as the number of quarters their prior job lasted. Somewhat surprisingly, those under the threshold have slightly longer tenures. If the threshold were selecting "worse" workers below it, we would expect the opposite relationship.

	Born	Tenure	College	Female	Non-white	Employment
$B_t < \underline{B}_{s,y}$	1973.63	12.85	0.49	0.54	0.37	0.54
	(0.058)	(0.099)	(0.002)	(0.002)	(0.002)	(0.0015)
$B_t > \underline{B}_{s,y}$	1973.06	12.48	0.49	0.53	0.36	0.51
	(0.065)	(0.112)	(0.002)	(0.002)	(0.002)	(0.0017)

Table 2: Characteristics within 2% of $B_{i,t} = \underline{B}_{s,y}$. Standard errors in parentheses.

It is probably not surprising that groups on either side of this threshold are quite similar. Of course, UI receivers tend to be observably quite different from the total unemployed population, but this selection does not appear at the somewhat arbitrary monetary eligibility threshold.

3.4 Validation: Placebo and Manipulation Test

To assess the robustness of our main findings, we first conduct a placebo test by shifting the cutoff threshold used in our regression discontinuity design. Specifically, we estimate the treatment effect at multiple artificial cutoffs relative to the actual threshold, both above and below the true cutoff. If our estimated effects are truly capturing the causal impact of treatment, we would expect to see significant results only at the actual threshold, while estimates at placebo cutoffs should be close to zero and statistically insignificant. Figure 2 presents the estimated treatment effects at various placebo cutoffs, with the solid black line representing point estimates and the shaded region indicating the corresponding confidence intervals. As seen in the figure, the treatment effects fluctuate around zero at placebo cutoffs, with wide confidence intervals that include zero, indicating a lack of statistical significance. In contrast, the estimated effect at the true cutoff stands out as the only statistically significant result.

Several observations to note about why this is a particularly strong result. Just visually, one can see that none of the placebo thresholds include our estimated effect within their confidence intervals. Further, the false threshold estimates are not consistently positive. One might be concerned that with earnings correlated between the base-period and reemployment, there would be an upward trend, and this can appear as positive breaks with incorrect or restrictive functional forms on either side of the break. We do not see this both because the running variable is less correlated to the reemployment earnings, because we make it a function of the threshold too, and also because the amount of data we have allows for a very flexible functional form on either side of the threshold. This placebo test confirms that our findings are not driven by arbitrary cutoff choices. Instead, the significant treatment effect at the actual cutoff reinforces the causal interpretation of our results.

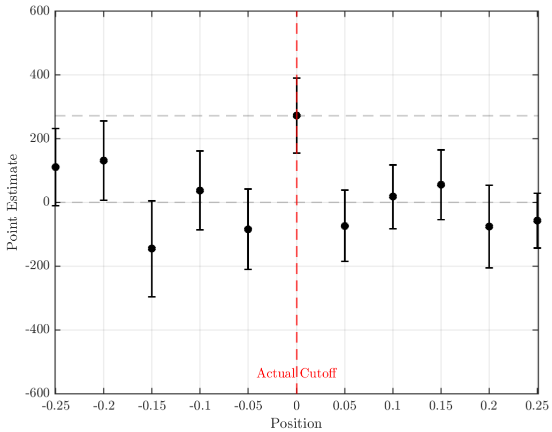
To further validate our estimates, we also conduct a manipulation test to examine whether there is a discontinuity in the density of observations around the cutoff. If individuals could precisely manipulate their assignment variable, we would expect an excess mass of observations just above or below the threshold, which could bias our estimates because workers would no longer be randomly assigned to receive the treatment, UI eligibility. Intuitively, we would be concerned that more "able" workers could both manipulate themselves above the threshold and were also more desirable workers when they were reemployed.

We think that the structure of the UI system makes this difficult for several reasons. First, base period earnings are generally determined by earnings over the year in the completed quarter prior to separation rather than at separation. Hence, the worker cannot quit when their earnings pass the threshold: they would have to wait until the end of the quarter. Further, many of these workers have very unstable employment relationships and so their hours worked are essentially random processes.

Modern statistical tests can validate this intuition, and so we use the set of manipulation tests proposed in Matias D. Cattaneo and Ma (2020). Fundamentally, these are estimating kernel densities on either side of our threshold, just like our local polynomials for the regression discontinuity estimator itself. The estimators then look for abrupt changes in density on either side, with the idea being that if workers could choose to get UI, we would see excess density bunching just above the threshold.

The null hypothesis is that the density on either side of our threshold is different, and a low p-value would reject this, essentially finding that the density is different. Figure 2 shows that we cannot reject that the density estimated from the left of the threshold is the same as the density from the right. This is actually a very high power test because of how many observations we have, and so the high p-values imply that the density is quite smooth in the area of interest.

Figure 2 reports the test statistics using both side-specific and common bandwidths. The side-specific bandwidth allows different bandwidths on each side, accommodating potential asymmetries in density, while the common bandwidth applies a symmetric bandwidth around the cutoff. The estimated test statistic and p-values indicate that there is no significant discontinuity in density at the cutoff, suggesting that manipulation is unlikely. This supports the validity of our estimate by confirming that individuals on either side of the threshold are comparable and that our causal estimates are not driven by sorting behavior.



Bandwidth	Test Statistic	S.E	P-value
Side-specific	-0.8252	0.0525	0.4092
Common	-0.5472	0.0563	0.5842

Figure 2: Validity tests for the main result confirm our finding. Placebo tests for the discontinuity (LEFT) and test for manipulation of the running variable density (RIGHT) show our threshold has a unique effect and that the density does not show bunching suggestive of endogeneity around the threshold.

4 Quantitative Model

4.1 Environment

Our economy is populated with a continuum of infinitely-lived workers of measure one, and firms with positive measure. Time in our economy is discrete and continues forever, and both firms and workers discount future value at an identical rate, β. Workers and firms are ex-ante homogeneous, but workers become ex-post heterogeneous as a result of their income history, μ. Workers may be employed or unemployed and receiving UI, or unable to receive UI. Upon separating, workers choose whether or not to claim UI, which is a stochastic process that depends on their income history (μ) and whether they separated to unemployment by quitting, $q = 1$, or being fired, $q = 0$. Unemployed workers of either UI status are able to direct their search to vacancies posted by firms in different submarkets, which are indexed by $(\mu, w) \in \mathbb{R}_+ \times \mathbb{R}_+$, the income history, and piece-rate.

Matched firms produce using a linear technology, z, where z is a stochastic productivity process composed of an idiosyncratic and persistent component. At the beginning of the period, an idiosyncratic shock realizes with probability p_0, and the match produces a trivial amount $\underline{z}$, reflecting a period where the firm does not require output and the worker is not paid. With complementary probability, the match is productive and z follows an AR(1) process: $z' = \rho_z z + \epsilon_z$, where $\epsilon_z \sim N(0, \sigma_\epsilon)$. Firms pay piece-rate wages w, which yields a wage bill of wz, and are subject to a stochastic fixed operating cost χ. After observing the productivity and hours shocks, the firm decides whether to fire its worker, which we denote by the indicator $d_f(w, z, \mu, \chi)$. Matches may also dissolve because workers quit, which depends on a time cost shock, γ, that workers realize each period. For some values of γ, workers prefer to quit and enter unemployment. This yields an indicator function

$D(w,z,\mu) = \max\{d_f(w,z,\mu,\chi), d_q(w,z,\mu)\}$, the expectation of which is the probability that a match dissolves between periods. We assume that a firm's decision to fire a worker occurs before the worker's decision to quit, should both realize.

Workers are risk-averse with utility $u'(c) \geq 0$, $u'(0) = \infty$, and do not have access to savings technology. While they are employed, their income history updates according to $\mu' = (1 - \frac{1}{T})\mu + \frac{1}{T}wz$, where T is the "look-back" period, over which previous income is calculated for eligibility and level of benefits (52 weeks in our calibration). After producing, the quit shock realizes. If a worker separates, they choose whether or not to claim UI.

The likelihood of UI recipiency depends on two factors: whether the worker was fired and whether their income history falls above a monetary eligibility threshold, $\bar{\mu}$. While neither factor unilaterally precludes a worker from receiving UI, quitting or having income below the threshold hampers their likelihood of receipt. If a worker fails to meet either the separation or base period earning requirement, they face a likelihood ξ_l of being deemed eligible if they claim. If they meet both criteria, they have a probability ξ_h of receipt should they claim. Claiming UI entails a cost, $\epsilon \sim$ Gumbel as well as a fixed cost η, both of which linearly decrease utility. If they are successful, they receive $b_{UI} = \max\{b_{RR}\mu, b_{RR}\bar{\omega}\}$ in UI benefits. They face a probability λ_0 of exogenously losing benefits, and may only receive benefits for at most T_b consecutive periods. If they are not receiving UI or have exhausted their benefits, they receive $b_n\mu < b_{RR}(\bar{\omega})\mu$.[4]

Firms post vacancies at marginal cost κ. Vacancies are one-firm to one-worker contracts that specify a piece rate to which the firm can commit for the duration of the contract. In each submarket, there exists a constant return to scale (CRTS) matching technology, $M(u,v)$, where u is the number of unemployed in the submarket, and v is the vacancies. We define the market tightness θ as $\frac{u}{v}$. We define the job-finding rate as $\frac{M(u,v)}{u} = p(\theta)$ and the job-filling rate $\frac{M(u,v)}{v} = q(\theta) = \frac{p(\theta)}{\theta}$. p is a strictly increasing and concave function such that $p(0) = 0$, and $p'(0) > 0$, and q is a strictly decreasing and convex function such that $q(0) = 1$, $q'(0) < 0$, and further the composite function $p(q^{-1})$ is concave. We assume that the free entry condition holds in any open submarket.

The aggregate state of this economy is given by a tuple (y, e, u), the aggregate productivity, and measures of employed and unemployed, respectively. The equilibrium is stationary and block recursive, so we suppress this notation for ease of exposition.

4.2 Timing of The Model

Each period in the model consists of two sub-periods: search and production. At the beginning of each period, an employed worker first decides whether to quit their job. This decision depends on their current wage, outside options, and UI eligibility status. If the worker chooses to quit, they enter the unemployed pool and may apply for UI benefits. For workers who remain employed, the firm then draws a productivity shock (z), an employment shock (p_0), and a match destruction shock (δ). The productivity shock z determines the worker's output within the firm. The employment shock p_0 determines whether the match produces zero output, meaning the worker remains employed but does not receive earnings

[4] This is an approximation, where μ proxies for their wealth and this implies that their resources from which to consume are some fraction of this wealth.

during the period. The match destruction shock δ determines whether the match is automatically dissolved due to external factors. Given these realizations, the firm decides whether to fire the worker. If the firm fires the worker, the worker enters the unemployed pool and may apply for UI. If the firm retains the worker, production continues, and the worker receives their wage.

Following the separation phase, the search phase begins. Firms post vacancies and unemployed workers search for jobs, directing their search based on their reservation wage and job market conditions. Workers who have been separated, either voluntarily or involuntarily, may apply for UI benefits, with eligibility determined by the state's earnings and separation requirements. If eligible, they receive UI benefits with probability ξ. At the end of the period, employed workers carry over their match into the next period, while unemployed workers either continue searching for jobs or receive UI benefits if eligible. The process then repeats in the next period, with workers facing the same sequence of quit, search, and employment decisions.

4.3 Worker's Problem

We first describe the problems solved by employed and unemployed agents. Unemployed agents may be in one of four discrete states: they may be receiving, eligible to receive, ineligible but not rejected, or rejected and ineligible to receive UI. We first describe the quit decision and subsequent production phase for the employed worker.

4.3.1 Production and Quit Decision

Each period, an employed worker is subject to a cost of time shock, γ, that in concert with their productivity shock, z, determines whether or not they choose to quit. If they choose not to quit, they may be fired by the firm, which is determined in the firm's problem, but happens exogenously with probability δ. An employed worker has state $s_E = (w, z, \mu)$, and faces $s'_E = (w, z', \mu')$, $s^0_U = (\mu', d_q = 0)$, $s^1_U = (\mu', d_q = 1)$. Such a worker, one who has already decided not to quit, faces the following problem during the production phase

$$U_E(s_E) - \gamma = (1 - d_f(s_E))\{u(c) + \beta E\left[U_E(s'_E)\right]\} + d_f(s_E)U_C(s^0_U) - \phi - \gamma$$

$$\text{s.t. } c = \begin{cases} wz & z > \underline{z} \\ b_n\mu & z = \underline{z} \end{cases} \tag{2}$$

$$\mu' = \left(1 - \frac{1}{T}\right)\mu + \frac{1}{T}wz \tag{3}$$

$$z' = \begin{cases} \underline{z}, & w/\ prob.\ p_0(z) \\ z' = \rho z + \epsilon_z, & w/\ prob.\ 1 - p_0(z) \end{cases} \tag{4}$$

They receive income wz, where z is realized before the period. They consume their income, $c = wz$.[5].

[5]Because our focus is on workers barely eligible or ineligible for UI, unlikely to be able to self-insure much, we abstract from a savings decision.

Before production, the worker chooses whether or not to quit, probabilistically. The probability of not quitting is given by

$$\Pr(U_C(s_U^1) > U_E(s'_E)) = \frac{\exp\{(U_C(s_U^1) - U_E(s'_E)) / \sigma_\gamma\}}{1 + \exp\{(U_C(s_U^1) - U_E(s'_E)) / \sigma_\gamma\}} \tag{5}$$

which is because the shock, γ, is Gumbel distributed.

4.3.2 UI Take-Up and Receipt

Each period, an unemployed worker who is still eligible chooses whether to apply for UI benefits. He makes this decision based on the probability of acceptance, $\xi(\mu, Q)$, which depends on income eligibility and quit status ($Q \in \{0, 1\}$). Should he choose to apply for UI benefits, he pays a fixed cost η and a stochastic utility cost $\epsilon \sim$ *Gumbel*. If he is rejected for UI, he becomes ineligible. If he is successful, he receives $b_{UI} = \max\{\min\{b_{RR}\mu', b_{MAX}\}, b_{RR}\bar{\omega}\}$ and has τ periods remaining of receipt. Hence, the states for workers who can claim, $s_C = (\mu, Q)$, are currently receiving, $s_R = (\mu, b_{UI}, \tau)$ or are ineligible $s_X = (\mu, b_n)$ His value function is

$$\begin{aligned} U_C(s_U) = \max_{\ell \in \{0,1\}} \; & u(b_n\mu) + \beta E[\mathbb{I}_{\{\ell=1\}}\{\xi(s_U)R_R(s'_R) \\ & + (1 - \xi(s_U))R_X(s'_X) - \eta - \epsilon\} + \mathbb{I}_{\{\ell=0\}}R_C(s'_U)] \qquad (6) \\ s.t. \;\; \mu' = {} & (1 - \frac{1}{T})\mu \qquad (7) \\ \xi = {} & \begin{cases} \xi_h e^{\xi_Q} & \text{if } \mu \geq \bar{\omega} \\ \xi_l e^{\xi_Q} & \text{if } \mu < \bar{\omega} \end{cases} \qquad (8) \end{aligned}$$

where R_R, R_X, and R_C are the values of searching for receivers, ineligible, and potential claimants, respectively, during the search subperiod. Because ϵ is realized after applying, potential claimants apply with probability

$$\begin{aligned} & \Pr(E_{z'|z}\{\xi R_R + (1 - \xi)R_X - \epsilon - \eta\} > E_{z'|z}[R_X]) \\ & \quad = \frac{\exp\{(\xi R_R + (1 - \xi)R_X - \eta - R_X) / \sigma_\epsilon\}}{1 + \exp\{(\xi R_R + (1 - \xi)R_X - \eta - R_X) / \sigma_\epsilon\}} \, , \end{aligned}$$

which is increasing in the likelihood of acceptance (ξ) and decreasing in costs ϵ and η. Notably, ϵ can take values less than zero, which can cause workers to claim even if they are ineligible and unlikely to receive UI.

An unemployed worker who is receiving UI has the value function

$$U_R(s_R) = u(b_{UI}) + \beta E[(1 - \lambda(\tau))R_R(s'_R) + \lambda(\tau)R_X(s'_X)].$$

$$\begin{aligned} s.t. \;\; \mu' &= \left(1 - \frac{1}{T}\right)\mu \\ \lambda(\tau) &= \begin{cases} \lambda_0 & \tau > 0 \\ 1 & \tau = 0 \end{cases} \end{aligned}$$

where λ determines whether he becomes ineligible for UI after the search subperiod. While he still has periods of eligibility ($\tau > 0$), he faces a probability λ_0 of losing UI, reflecting the probability that his receipt is discontinued.[6] Once he has exhausted his UI, he no longer receives UI after the search subperiod ($\lambda = 1$).

An ineligible worker faces a similar problem, with zero probability of regaining UI without first finding employment. His value function is

$$U_X(s_X) = u(b_n \mu) + \beta E[R_X(s'_X)].$$

$$s.t. \;\; \mu' = \left(1 - \frac{1}{T}\right)\mu$$

4.3.3 Job Search

After producing, separating, and resolving the claims decision, an unemployed worker searches for a job. This defines three component values, R_R, R_X, and R_C for receivers, ineligible, and potential claimants, respectively. The value function is

$$\begin{aligned}
R_R(\mu, b_{UI}, \tau) &= \max_{w,\theta} p(\theta) \int \max\left\{U_E(w, z, \mu), U_R(\mu, b_{UI}, \tau)\right\} d\Phi(z) + [1 - p(\theta)]U_R(\mu, b_{UI}, \tau) \\
R_X(\mu) &= \max_{w,\theta} p(\theta) \int \max\left\{U_E(w, z, \mu), U_X(\mu)\right\} d\Phi(z) + [1 - p(\theta)]U_X(\mu) \\
R_C(\mu, \mathcal{Q}) &= \max_{w,\theta} p(\theta) \int \max\left\{U_E(w, z, \mu), U_C(\mu, \mathcal{Q})\right\} d\Phi(z) + [1 - p(\theta)]U_C(\mu, \mathcal{Q})
\end{aligned}$$

where b_{UI} and τ can be suppressed for ineligible or potential claimants and $\Phi(z)$ is the ergodic distribution of z implied by the AR(1) process described above.

4.4 Firms's Problem

In our model, firms may be matched with a single worker or remain unmatched. Matched firms produce and choose whether or not to continue the match. Unmatched firms choose whether or not to post a vacancy.

4.4.1 Production and Firing

A matched firm produces z units of output each period and pays wz in income. It also pays a fixed cost ψ associated with operating the firm. Productivity, z, is stochastic and realizes prior to the separation decision (D). It also faces a risk that its employee may quit prior to production. Should this not occur, a matched firm in the production stage faces the following problem:

[6] Claims may be discontinued for violations of the receipt agreement, like not actively searching for a job.

$$
\begin{aligned}
J(w,z,\mu) &= \max_{d_f\in\{0,1\}} (1-d_f)\{(A-w)z-\chi+\beta E\{(1-d_q(w,z',\mu'))J(w,z',\mu')\} \\
\mu' &= \left(1-\frac{1}{T}\right)\mu+\frac{1}{T}wz \\
z' &= \begin{cases} \underline{z}, & w/\ prob.\ p_0(z) \\ z'=\rho z+\epsilon_z, & w/\ prob.\ 1-p_0(z) \end{cases}
\end{aligned}
$$

where we have imposed the equilibrium free entry condition that $E[V(w,\tilde{z})]=0$ in the interest of brevity. A firm fires workers, $d_f(w,z,\mu)=1$, if the value of continued employment falls below the value of searching for a new worker, $J(w,z)<E[V(w,\tilde{z})]=0$, a rate governed by χ. If the firm chooses not to fire the worker, the worker may quit with probability $d_q(w,z',\mu')\geq\delta$ before the firm gets the chance to choose their firing choice next period. Because χ is Gumbel-distributed, the probability that the firm fires a worker is given by

$$
\Pr(J(w,z,\mu)>0)=\frac{\exp\{J(w,z,\mu)/\sigma_\chi\}}{1+\exp\{J(w,z,\mu)/\sigma_\chi\}} \tag{9}
$$

4.4.2 Vacancy Creation and Free Entry

An unmatched firm can post a vacancy at cost κ that specifies a wage w. With probability $q(\theta)$, it contacts a worker during the following week and draws an idiosyncratic productivity, z. An unmatched firm has the value function

$$
V(w)=-\kappa+\beta q(\theta(\theta)))E_{z'}[(1-D(w,z'))J(w,z')]. \tag{10}
$$

We assume that the free entry condition holds in equilibrium, which yields the following worker contact rates

$$
q(\theta(w))=\frac{\kappa}{\beta E_{z'}[(1-D(w,z'))J(w,z')]} \tag{11}
$$

in a submarket.

4.5 Equilibrium

A *Block Recursive Equilibrium* (Shi (2009) and Menzio and Shi (2010)) in this model economy is a set of policy functions for workers, $\{\ell,w\}$, value functions for workers U,R, value functions for firms with filled jobs, J, and unfilled jobs, V, as well as a market tightness function $\theta(w)$. These functions satisfy the following:

1. The policy functions $\{\ell,w\}$ solve the workers problems, U, R.
2. $\theta(w)$ satisfies the free entry condition for all submarkets (w).
3. The aggregate law of motion is consistent with all policy functions.

As in the prior literature, the equilibrium is "Block" Recursive in that the first two blocks of the equilibrium, i.e. the individual decision rules, can be solved without conditioning upon the aggregate distribution of agents across states, i.e. the third block of the equilibrium. In our context, this has implications for how we interpret the RDD because firms know they are getting either treated or untreated workers and the equilibrium finding rate reflects the firms' internalization of the workers' outside option.

5 Calibration and Quantitative Results

In this section, we calibrate the model and use it to quantify the role of UI in re-employment outcomes. We first use simulated method of moments to discipline key features of our model. Our moments can be broken into three sets: standard search and matching model targets, UI-specific structural features, and the re-employment earnings jump estimated in section 3. By aligning the RDD-estimated treatment effect, we can infer typically unobservable parameters that determine workers' share of the surplus. This approach enables us to assess the fundamental treatment effect driving our quasi-experimental results.

Then, we present the key findings from our analysis. First, we use our calibrated model to interpret the empirical results from the regression discontinuity design in subsection 5.3. Here, we extract the "true" treatment effect, pulling away the effects of endogenous noncompliance. Then we decompose the earnings effect of UI eligibility into its underlying mechanisms, distinguishing between changes in match productivity and changes in the worker's share of the surplus. This allows us to quantify the extent to which UI eligibility influences re-employment wages by garnering a larger share of production rather than productivity gains.

Last, we use the model to understand the result. First, we answer why productivity seems to move so little. Then we use the model to answer two big questions and policy experiments. Getting at some of the fundamentals of labor search, we use the exercise to isolate workers' bargaining power. Then, we assess the broader labor market implications of UI eligibility by generating global treatment effects. Finally, we conduct counterfactual policy exercises for how relaxing UI eligibility requirements, the base period earnings requirement and the separation requirement, impact key labor market outcomes, including employment rates, UI claim rates, and average wages.

5.1 Externally Calibrated Parameters and Functional Forms

We adopt several standard assumptions for functional forms and externally calibrate a subset of parameters. We assume that workers exhibit constant relative risk aversion (CRRA) utility, given by $u(c) = \frac{c^{1-\sigma}}{1-\sigma}, \sigma \neq 0$, which allows unemployment insurance to have a nonlinear effect on marginal utility. Risk aversion is set to 2. The matching function is specified as $M(u,v) = \frac{uv}{(u^{n_1}+v^{n_1})^{\frac{1}{n_1}}}$, which ensures that matching probabilities remain bounded between 0 and 1. n_1 controls the elasticity of the matching function.

Table 3 presents the externally calibrated parameters. We calibrate the model to a weekly frequency, setting $\beta = 0.99$, which implies an annual interest rate of approximately 5%, closely aligning with the average during this period. Following Fujita and Ramey (2012), we assume that filling a vacancy requires roughly 6.7 hours per week, leading us to set $\kappa = 0.2$, a value consistent with other studies in the literature, such as Petrongolo and Pissarides (2001). To calibrate the unemployment insurance (UI) system, we use administrative data from the U.S. Department of Labor. For our sample period and states, the average statutory replacement rate is $b_{RR} = 0.55$. The "look-back" period T, over which prior income is calculated to determine UI eligibility and benefit levels, is 52 weeks. Also, the probability of losing UI λ_0 is set to 0.0385, which matches the regular UI duration of 26 weeks.

Parameter	Value	Comment
β	0.99	Discount rate
n_0	1	Matching efficiency
κ	0.20	Vacancy creation cost
b_{RR}	0.55	UI replacement rate
λ_0	0.0385	Probability of losing UI
σ	2	Risk aversion
A	1	Normalization
T	52	Base period lookback

Table 3: Externally Calibrated Parameters

5.2 Estimated Parameters and Targeted Moments

The remaining parameters are set by matching simulated moments. The left panel of Table 4 presents the estimated parameters, while the right panel of Table 4 compares the moments estimated from the data with those generated by the model.

Three sets of moments are crucial to the success of this study: the discontinuity in earnings observed in the data, labor market transitions, and the distribution of UI status. We rely on several data sources to be able to measure these variables near the earnings threshold, where possible, specifically the LEHD and Survey of Income and Program Participation (SIPP). See Appendix C for more details on the SIPP and its construction. The moments related to UI status are estimated using the Non-Monetary Determinations Activity reports and the Benefit Rights and Experience reports from the Employment and Training Administration (ETA).[7]

These moments are jointly disciplined by three sets of parameters: those common in equilibrium search models, parameters unique to our UI structure, and parameters related to endogenous separation decisions by both firms and workers.

The first set of parameters includes the matching elasticity n_1 and outside subsistence income b_n. The outside subsistence income b_n directly influences how much an unemployed worker values UI benefits. The matching elasticity plays a key role in determining how much of a worker's earnings stem from productivity versus how much is attributed to outside

[7]ETA reports are sourced from the National Office database, which compiles data from the 50 states, Washington, D.C., Puerto Rico, and the Virgin Islands. The data in the Non-Monetary Determinations Activity reports are used by the U.S. Department of Labor for budget projections and to assess disqualification processes. The Benefit Rights and Experience reports are used to evaluate state benefit formulas.

options. These two parameters are critical for capturing the 10% earnings difference at the cutoff.

The second set of parameters captures UI eligibility and the process for productivity. Specifically, these include the probability of receiving UI conditional on earnings eligibility, denoted by ξ_h and ξ_l, the quitting penalty φ, the fixed application cost η, earnings eligibility threshold $\bar{\omega}$ and the slack probability p_0. The parameters ξ_h, ξ_l, and φ are novel to our paper and are crucial to how we model the UI eligibility determination process. These parameters incorporate both the base period earnings and separation requirements for UI eligibility: if a worker's past earnings do not meet the base period earnings requirement, they receive UI with probability ξ_l. If a worker voluntarily quits their previous job, their probability of receiving UI is reduced by φ. These adjustments account for the observed non-trivial rejection rates related to eligibility requirements in the data. Specifically, ξ_h, ξ_l, and φ are calibrated to match the monetary ineligibility rejection rate, the separation rejection rate, and the eligible receiving rate. From Table 4, we estimate that meeting the base period earnings requirement increases an applicant's probability of receiving UI by approximately fourfold. Additionally, quitting carries a 12.5% penalty on the probability of UI receipt.

The parameters η and $\bar{\omega}$ further refine the model by regulating two key moments: the claiming rate and the proportion of monetarily ineligible earners, respectively. The fixed application cost η captures the administrative burden of applying for UI, reflecting the requirement for applicants to visit UI offices weekly to report their status and recertify eligibility. The parameter $\bar{\omega}$ serves as the model counterpart to the base period earnings requirement.

In our income process, p_0, is unique to our model and quite important to achieve very low earnings prior to separation. We define p_0 as the probability that a job match experiences an idiosyncratic shock, preventing production and resulting in the worker receiving no earnings. This parameter is included because, according to SIPP data, approximately 26% of employed workers experience periods without earnings despite remaining employed. Additionally, p_0 is instrumental in generating the observed 20% of separations that do not meet the base period earnings requirement. We calibrate p_0 to match the 26% no-work rate reported in SIPP.

We set the exogenous separation rate, δ, the variance of the firing cost, γ, and the variance of the quitting cost, χ, by targeting the average separation rate (16%), the proportion of separations resulting from firings (33.4%), voluntary quits (19.5%) estimated in SIPP and the employment population ratio. Details of our SIPP estimation are in Appendix C. As shown in Table 4, quitting is associated with a substantial cost, approximately equivalent to two and a half years of accumulated subsistence income for an unemployed worker.

For the baseline productivity, we calibrate the parameters ρ_z and σ_z governing the productivity (z) process to match empirical moments related to the earning process. Specifically, ρ_z and σ_z are chosen so that the simulated model reproduces the first-order autocorrelation and the variance of innovations in the log earnings process, as estimated from the SIPP data at the wave (four-month) frequency. This strategy ensures that the model's earning dynamics are consistent with the observed persistence and volatility of earnings in the data. The empirical estimates are described in Appendix C.

Overall, our calibration results suggest that our model captures the key mechanisms present in the data, including the re-employment earnings jump attributable to maintaining base period earnings that confer monetary eligibility. It also accurately replicates employment transition dynamics and aligns well with observed UI claim, rejection, and ineligibility rates.

Parameter	Comment	Value
Labor market parameters		
n_1	Matching elasticity	0.745
b_n	Outside subsistence income	0.134
p_0	Probability of no hours	0.111
δ	Separation probability	0.149
σ_γ	Worker quitting cost	1.879
σ_χ	Firm firing cost	0.927
ϕ	Worker fixed quitting cost	19.266
UI receipt probability		
ξ_h	Monetarily eligible	1.000
ξ_l	Monetarily ineligible	0.565
φ	Quitting penalty	-0.112
UI program parameters		
η	Application cost	1.523
$\bar{\omega}$	Eligibility threshold	0.378
Productivity Process		
ρ_z	Auto-corr. of z shock	0.964
σ_z	SD of z shock	0.046

Moments	Data	Model
Main RD estimate		
Earnings discontinuity	**0.100**	0.099
Employment Transition		
UE rate	**0.412**	0.395
Quitting rate	**0.334**	0.330
Firing rate	**0.195**	0.192
Emp rate	**0.600**	0.639
EU rate	**0.160**	0.126
No-work-rate	**0.262**	0.174
UI status		
Claiming rate	**0.734**	0.746
Percent monetarily ineligible	**0.200**	0.208
Separation rejections	**0.125**	0.135
Monetary ineligible rejections	**0.074**	0.067
Ineligible receiving rate	**0.100**	0.136
Earning Process		
Auto correlation	**0.245**	0.245
Variance of shock	**0.402**	0.383

Table 4: Parameter values and estimated moments

5.3 The Underlying Effects of UI on Re-Employment Wages

In the data, the effect we estimate from the regression discontinuity is an amalgam of different effects. The post-treatment average wage is a weighted average across different types of workers whose earnings change differently at the threshold. While our baseline results strongly suggest that UI receipt leads to higher re-employment earnings, we cannot directly observe claim and receipt status in the data, so we cannot directly observe a "true" causal effect, like a treatment on the treated. To address this limitation, we begin by estimating the underlying effect of UI receipt on re-employment earnings for a hypothetical eligible *and* claiming unemployed worker.

We utilize our model to construct an appropriate counterfactual and estimate the causal effect of UI on re-employment wages for eligible workers. Unemployed workers can be in one of three categories, *Non-Quit*, *Quit*, and *Exhausted* based on their separation status before unemployment. Among non-quitters, we specifically focus on those who claim UI both above and below the eligibility threshold and refer to them as *Non-quitters (Claim)*, which imposes perfect compliance. Unlike other groups that may exhibit endogenous selection into UI, the compliers provide a clean estimate of the true causal effect of UI by allowing us to isolate the impact of eligibility on re-employment wages without the confounding influence of differential take-up behavior.

Then, for each of these types, we decompose the overall treatment effect on post-unemployment quarterly earnings (*Treatment*) into two components: the effect on productivity and the effect on piece-rates. Treatment is measured by simulating the model and measuring wage, piece-rate, and productivity within a window of 2% of the earnings cutoff. In the simulation, we can identify workers who would claim and those who would not, and split them into non-quitters, quitters, and those who are ineligible.

To simplify this decomposition, we first approximate each individual's re-employment earnings as $y_{it} = s_{it} z_{it} w_{it}$, where s_{it} denotes the probability the worker is not slack in a period (i.e., the p_0 shock), z_{it} is the productivity conditional on not being slack, and w_{it} is the piece-rate. Then effective productivity as the product $\tilde{z}_{it} = s_{it} z_{it}$. This allows us to write average earnings as $\bar{y} = \bar{\tilde{z}}\,\bar{w}$. Now, the log difference can be additively approximated by

$$\log(y_R) - log(y_L) = [\log(\bar{\tilde{z}}_R) - \log(\bar{\tilde{z}}_L)] + [\log(\bar{w}_R) - \log(\bar{w}_L)] + residuals.$$

This is only an approximation of earnings, but it is convenient because it is log-separable; however, the treatment effect with this approximation is slightly different.

In Table 5, we report this as a two-component decomposition: the z effect, which combines the slack probability and conditional productivity, and the w effect. Any residual captures interaction terms and approximation error. This provides a clear and interpretable breakdown of how UI eligibility affects re-employment earnings. More interestingly, Table 5 shows how the estimated 10% treatment effect from the RDD is confounded by the presence of heterogeneous worker types: essentially, the overall treatment is a weighted average of these other treatments. Those who exhausted the UI exhibit minimal earnings increases near the cut-off. The reason we see any effect among the exhausted is because some of these workers will change status within the quarter, and therefore their quarterly treatment effect is just a relic of time-aggregation.[8]

To get a sense of the "true effect," the treatment on the treated, we also estimate the true effect of UI focusing on the perfect compliers who claim UI and are above the threshold for μ. The comparison group below is a bit odd in reality, but we can compute it in our simulation as those below the threshold who claim anyway, to be as similar as possible to those treated above the threshold. As shown in Table 5, this group exhibits significantly higher estimated treatment effects compared to the broader empirical counterpart, particularly in re-employment earnings and wages (w). The true treatment effect on post-unemployment earnings is estimated to be 15.32% at the cutoff.

Decomposing the treatment, we find that the increase in re-employment earnings above the cutoff is mostly the improved outside option increasing the fraction of production going to workers, rather than higher worker productivity. That is, workers who gain access to UI benefits strengthen their bargaining position, allowing them to search for jobs with higher wages. They mostly do not become pickier about the productivity of the job they will accept. This aligns with our empirical findings that suggested the rise in post-unemployment wages primarily reflects workers capturing a larger share of the match surplus. We dive into the economics of the model for why this happens more in subsection 5.4.

[8] Appendix D provides a deeper look into the heterogeneity of treatment effects across different worker categories, distinguishing between weekly and quarterly frequency.

Approximate Earnings $\approx$ $[log(s_R) + log(z_R) + log(w_R)] - [log(s_L) + log(z_L) + log(w_L)]$					
Group	Overall	z	w	Residual	Mass
All Workers	0.0842	0.0066	0.0776	0.0000	1.0000
Non-Quitters	0.0887	0.0059	0.0829	0.0000	0.5385
Non-Quitters (Claim)	0.1380	0.0043	0.1337	0.0000	0.4009
Quitters	0.0675	0.0084	0.0591	-0.0000	0.2757
Exhausted	0.0128	0.0107	0.0020	0.0000	0.1857
Actual Earnings = $[log(y_R)] - [log(y_L)]$					
Group	Overall	z	w	Residual	
All Workers	0.0947	0.0066	0.0776	0.0105	
Non-Quitters	0.0998	0.0059	0.0829	0.0111	
Non-Quitters (Claim)	0.1532	0.0043	0.1337	0.0152	
Quitters	0.0759	0.0084	0.0591	0.0084	
Exhausted	0.0482	0.0107	0.0020	0.0354	

Table 5: Decomposition of the empirical treatment effect within the model. The first panel uses our log-separable approximation and the second uses the actual earnings. The columns labeled z and w represent the decomposition into productivity and piece-rate components, respectively.

To summarize these results, access to UI benefits raises a worker's reservation wage by strengthening their outside option, which then translates more than one-for-one into higher earnings. This effect is most directly measured in the "Non-Quitter (claim)" group, the model's analog to the treatment on treated. For these workers, increasing benefits from subsistence levels ($b_n = 0.145$) to the minimum UI level ($b_{UI} = 0.21$), calculated as the eligibility threshold (0.377) multiplied by the replacement rate (0.555), results in a 15.32% increase in re-employment earnings. This corresponds to an elasticity of approximately 0.34.

5.4 Why z Accounts for so Little of the Treatment

To understand why productivity (z) accounts for only a small portion of the treatment effect, we examine Figure 3. This figure plots the reservation productivity (z_s) as a function of base period earnings for different types of job searchers. The reservation z_s values are computed from the values of R_s at optimal wages.

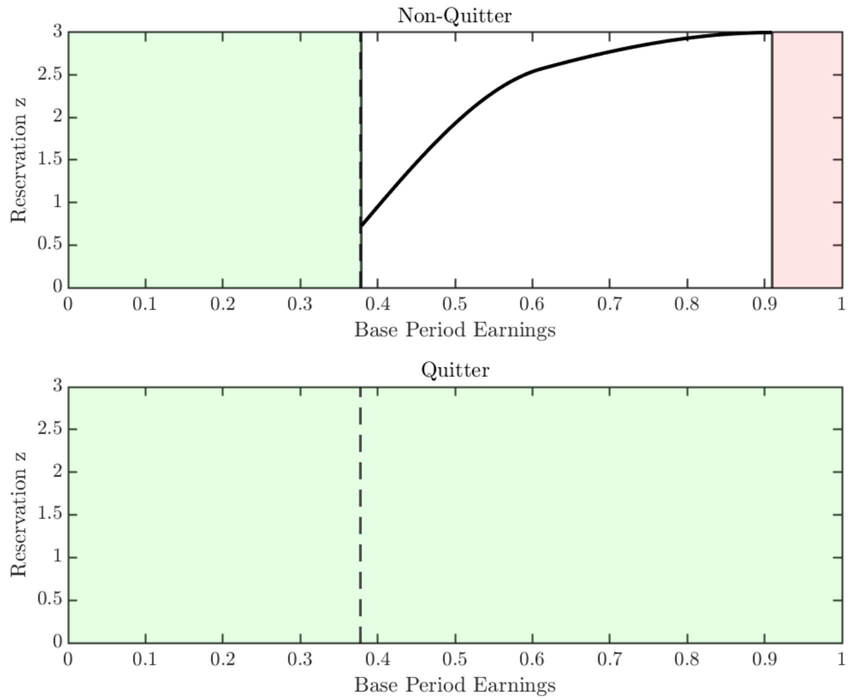


Figure 3: Reservation z

In Figure 3, the green region represents the values of μ where employment is consistently more beneficial than unemployment, whereas the red region denotes values where remaining unemployed is more advantageous. The white region illustrates the variation in the reservation z_s across different levels of μ.

At the UI eligibility cutoff (dashed line), the impact on individuals who previously quit their jobs is negligible. As expected, most individuals who quit still find re-employment to be the preferable option. For non-quitters, although there is a visible discontinuity at the threshold, the magnitude of this difference remains relatively small. Specifically, only 4% of non-quitters just below the base period earnings threshold have a productivity level below the reservation z, so very few offers are rejected.

This result makes sense when we think about the productivity process. It is quite volatile, both because z has a relatively low persistence at this portion of the earnings distribution and because of the non-productive-period shocks that lower it temporarily. The agents have little incentive to select strongly on z, which can easily change through the course of their match.

5.5 The effect on unemployment duration at the threshold

While the small change in reservation productivity at the threshold implies a minimal effect on unemployment duration, the model does have a direct relationship between the piece rate and market tightness, which does change considerably at the threshold. So, it is natural to look for unemployment duration effects. To assess this mechanism of the effect of UI eligibility on the duration of unemployment, we estimate the elasticity of the job-finding rate, the jump in Figure 4. Given that a 43.8% increase in subsistence benefits leads to an average decrease of 6.81% in job-finding rates, our estimated elasticity is approximately 0.16. This means that a 1% increase in UI benefits reduces the job-finding rate by 0.16%.

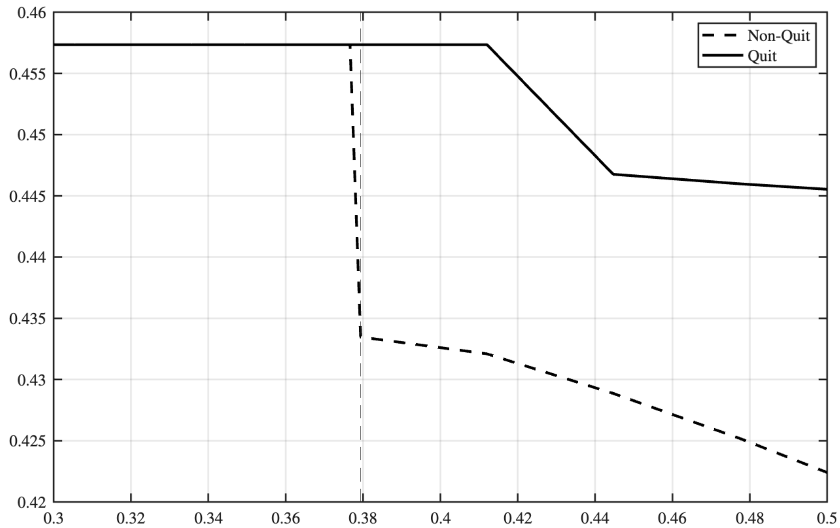


Figure 4: Offer arrival rates

This estimate aligns with Leung and O'Leary (2020), or the existing literature summarized in Schmieder and von Wachter (2016) Table 2, where duration elasticities in U.S. studies range from 0.1 to 2, with a median of 0.38. Compared to these estimates, our finding is at the smaller end of the range, suggesting that UI eligibility moderately prolongs unemployment duration but does not lead to excessively long jobless spells.

Taken together with the findings in subsection 5.3, we observe that the elasticity of re-employment wages is significantly larger than that of job-finding rates. This suggests that UI primarily influences wages rather than substantially extending unemployment duration.[9]

Examining the θ, w trade-off derived from Equation 11, we observe from Figure 5 that labor market tightness (θ) is relatively insensitive to changes in wages (w). This is likely because our sample focuses on workers near the UI eligibility threshold, who tend to be financially constrained and risk-averse. As a result, they exhibit a high degree of precaution, making them less willing to prolong their job search in pursuit of higher wages.

5.6 Worker bargaining power

From the analysis above, we relied on an experimental approach where UI eligibility significantly increased workers' outside options, and measured how this changed their job search direction and employment outcomes. A natural follow-up question is how this same experiment in UI eligibility could inform us about workers' share of the surplus. Essentially, we

[9] Seen within the context of our LEHD data, duration is measured very coarsely while earnings are measured very precisely, so this validates why we would miss effects on duration while seeing important and large effects on earnings.

see a discrete change in the worker's outside option and so its pass-through could be new evidence of the worker's bargaining power.

Our model operates under a directed search framework where wage determination occurs without explicit bargaining, so this is inferred from outcomes rather than assumed. While there is no bargaining process in equilibrium, we can approximate an ad hoc measure of bargaining power by computing workers' share of the measured surplus from a match. Specifically, for each individual, we compute their value of unemployment, U_C, before transitioning from unemployment to employment. We then calculate the match surplus, $U_E + J - U_C$, for the new match to estimate the worker's share of the surplus. Next, we compute the average surplus on either side of the threshold, along with the worker's share, to evaluate how UI eligibility affects surplus division. Table 6 reports the average worker share of the match surplus just below and just above the monetary eligibility threshold. Consistent with estimates in the literature, the share is slightly above one-half and exhibits a discrete jump of about 1.76% at the threshold. This result is expected, as the share should remain mostly unchanged across the threshold because UI eligibility primarily affects U_C rather than the fundamental determinants of surplus division. The estimated worker's share is close to the calibrated matching function elasticity, which aligns with our intuition from competitive search theory.[10]

Despite this non-trivial bargaining share, we found a very high estimated pass-through from UI benefits to worker earnings. In the simplest of models, this high elasticity of outside options to earnings would have only been consistent if workers had zero bargaining power. With zero bargaining power, workers are paid their outside option and so movements in UI translate directly into earnings.

Our model is different, however, because the surplus is not invariant to the change in UI benefit. In our model, while $\frac{\Delta J}{\Delta \text{UI benefit}}$ is close to zero, $\frac{\Delta U_E}{\Delta \text{UI benefit}}$ grows considerably, as does $\frac{\Delta U_C}{\Delta \text{UI benefit}}$ just mechanically. The firms' relative indifference is contrasted with workers being better off in both states. These values are affected by two contravening effects. First, the change in piece-rate makes the relationship between a firm's value and UI benefits negative, and U_E's relationship is positive. This is the classic transfer from one party to the other, where higher UI benefits lead workers to demand a higher piece rate, reducing the firm's surplus. Quantitatively, however, Figure 5 shows a very flat relationship between wage and market tightness. So this negative effect is small. This piece-rate effect is partially offset by another small positive impact coming from a larger total output over the course of the match. This is because the higher piece rate actually makes workers quit less, and near the threshold, this effect dominates the slightly higher rate at which firms fire workers. So even though z is not meaningfully increasing, the size of the surplus does rise. Combined, the forces mean that nearly all of the increase in UI benefits translates directly into higher earnings.

[10]There are several aspects of our model that mean the bargaining share is not precisely the matching function elasticity. Most notably, workers actually have two dimensions of selection that they can use to manipulate the value of their match, rather than just the piece rate.

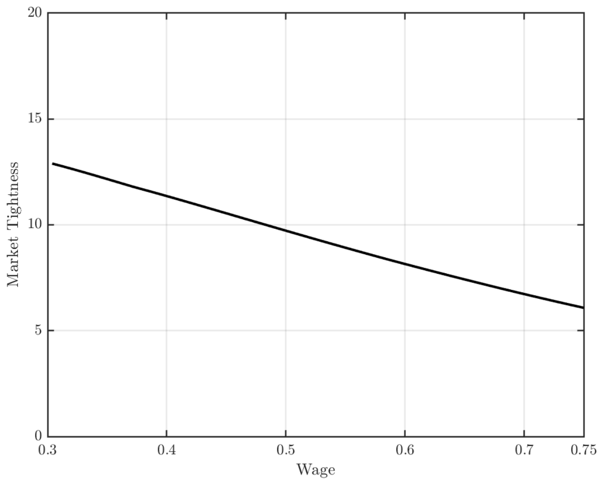
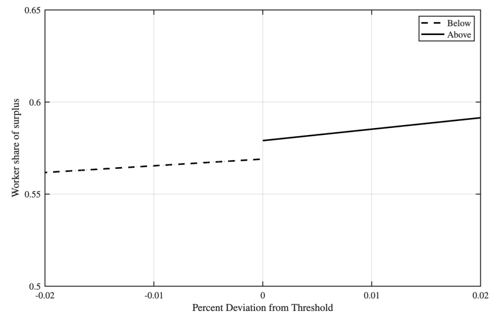


Figure 5: θ-w trade-off



Base Period Earnings	**Value of Share**
Below	0.569
Above	0.579

Table 6: Worker Share of Match Surplus Around the Base Period Earnings Threshold

5.7 Global treatment effects

An interesting extension of the model would be to quantify how the treatment effect changes with variations in base period earnings (μ). In the analysis below, we evaluate the treatment effects across the μ distribution.

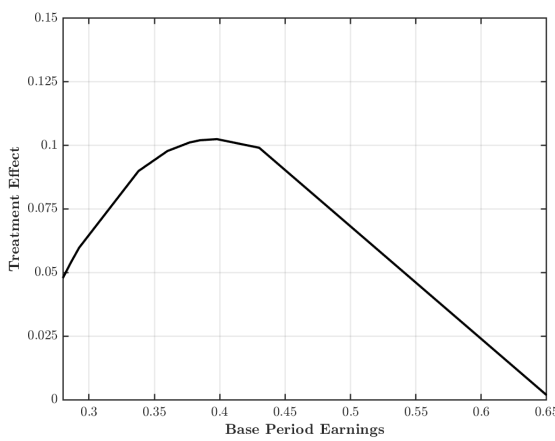


Figure 6: Effect over μ

Figure 6 presents the global treatment effect, illustrating how the impact of UI eligibility on post-unemployment earnings evolves as the UI earnings requirement varies. The treatment effect is measured as the percent difference in post-unemployment earnings at the cutoff, and it initially increases as the requirement rises before declining at higher thresholds. This pattern reflects the interaction between UI eligibility, benefit levels, and workers' share of the surplus.

When looking at lower base-period earnings thresholds, going from our initial local $\mu = 0.358$ to $\mu = 0.3$, the treatment effect declines. This occurs because newly eligible workers have lower base-period earnings, and since the UI benefit replacement rate is a constant share of past earnings, their UI benefits are also lower. Because the treatment effect is measured as a percent difference, the increase in post-unemployment wages from UI eligibility is smaller at lower thresholds, leading to a reduced treatment effect.

As the UI earnings requirement increases (e.g., from 0.358 to 0.4), the treatment effect initially rises. This is because higher base-period earnings translate into larger UI benefits, increasing the workers' share of the surplus. Since UI benefits serve as the worker's outside option if they are eligible, this results in a larger difference in post-unemployment wages at the cutoff. At these intermediate eligibility thresholds, UI has its strongest effect on post-unemployment earnings.

However, as the UI earnings requirement continues to increase (e.g., from 0.5 to 0.6), the treatment effect starts to decline. This is primarily due to the UI maximum benefit cap, which limits how much UI benefits can increase beyond a certain level of base-period earnings. When workers reach this cap, additional pre-unemployment earnings no longer lead to higher UI benefits, meaning firms no longer increase posted wages as much in response to UI eligibility. Additionally, high-earning workers already receive a larger share of the match surplus, meaning UI eligibility has a smaller marginal effect on their post-unemployment earnings. Consequently, the treatment effect declines at very high eligibility thresholds, completing the hump-shaped pattern observed in Figure 6.

Overall, these findings indicate that UI eligibility has the strongest impact on wages at intermediate earnings thresholds. At lower thresholds, UI benefits are too small to generate large wage differences, while at higher thresholds, the maximum benefit cap and firms' pre-existing wage-setting behavior limit the effect of UI eligibility. This suggests that UI policy changes that adjust earnings requirements can have nonlinear effects on labor market outcomes, with the largest wage effects occurring when eligibility is expanded to middle-tier earners rather than to very low or very high earners.

5.8 The Impact of Eligibility Requirements

In this section, we analyze how changes in UI eligibility requirements affect labor market outcomes. Specifically, we examine the consequences of reducing the base period earnings requirement by 10%, lowering the separation requirement by 10%, and relaxing both requirements simultaneously by 10% each.

The first policy experiment, lowering the base period earnings requirement, mirrors the temporary eligibility expansions implemented during the CARES Act in response to the COVID-19 pandemic. While prior studies have explored the effects of removing this requirement during the pandemic and normal times, our focus extends beyond special events, analyzing its equilibrium effects under normal economic conditions. Additionally, we examine the separation requirement, a relatively unexplored aspect of UI eligibility. While this requirement remains in place in the U.S., several other countries, such as Argentina, allow UI access to voluntary quitters. Understanding the labor market effects of relaxing both requirements provides valuable insights for UI policy design.

We report the percent change in key labor market outcomes relative to the baseline in

Table 7, including employment rates, UI claiming and receipt rates, average wages, and quitting rates.

Percentage changes w.r.t baseline	$\bar{\omega}$	φ	*Both*
Employment rate	-0.0202	-0.0038	-0.0227
Receiving rate	0.0753	0.0156	0.0890
Average wage	0.0603	0.0111	0.0704
Quitting rate	0.0878	0.0247	0.1022

Table 7: Comparison of outcomes when base period earning and separation requirements are relaxed.

As expected, loosening UI eligibility requirements reduces employment, particularly when both requirements are simultaneously relaxed. This occurs because more workers qualify for UI, reducing their urgency to accept job offers. The increase in UI receipt confirms this mechanism. When eligibility expands, more unemployed workers apply for and receive benefits, which prolongs the unemployment duration.

Quitting rates increase in all counterfactual scenarios, but the effect is especially pronounced when the base period earnings requirement is relaxed. This suggests that some workers remain employed primarily to maintain UI eligibility, implying that strict earnings thresholds serve as an implicit employment incentive. Moreover, relaxing the base period earnings requirement significantly increases average wages, with a 9 percent rise when adjusted alone and an 11 percent increase when combined with separation requirement relaxation. This reflects a precautionary response, where workers who now qualify for UI have stronger outside options, allowing them to secure higher post-unemployment wages. In contrast, reducing the separation requirement has a smaller effect on wages. This suggests that monetary eligibility constraints play a more dominant role in shaping UI's effect on post-unemployment earnings.

The findings highlight the importance of UI eligibility criteria in shaping job search behavior and employment transitions. The base period earnings requirement has a stronger influence on both wages and employment rate than the separation requirement. From a policy perspective, these results suggest that relaxing the base period earnings requirement expands UI access but also raises reservation wages, leading to higher post-unemployment wages and longer unemployment spells. On the other hand, removing the separation requirement primarily affects quitting behavior without substantially altering wage-setting mechanisms. This distinction is critical for policymakers considering UI expansions, as it highlights trade-offs between UI accessibility and employment incentives.

6 Conclusion

This paper provides new evidence on the impact of Unemployment Insurance (UI) eligibility on labor market outcomes, combining quasi-experimental estimates with a structural equilibrium model. Exploiting a regression discontinuity design using administrative data, we find that UI eligibility increases post-unemployment earnings by approximately 10% at the eligibility threshold. However, our model reveals that this local treatment effect significantly understates the true causal effect due to endogenous non-compliance. When accounting for the heterogeneity in UI status, we estimate that the actual earnings effect of UI is approximately 7 times larger than the empirical estimate. Moreover, our findings suggest that UI eligibility primarily affects workers' outside option rather than improving match quality. Workers who qualify for UI benefits secure higher wages, not because they find more productive matches, but because their improved outside options enable them to extract a larger share of the surplus.

In addition, we explore the broader implications of UI transfers, extrapolating the local estimated treatment effect. We vary the earning requirement in the model and see treatment effects that are nonlinear across prior earnings. The strongest wage increases occur at intermediate eligibility thresholds, while effects weaken when eligibility is extended to very low or very high earners. The program does not provide much income for the very low earners and has a declining replacement rate at the top because of benefit caps, meaning that its largest effect on search occurs among workers with only slightly higher earnings than the data's cutoff.

Beyond the intensive-margin effects of UI eligibility, we explore the broader implications of policy changes: relaxing the earning requirement and the separation requirement. By varying UI earnings and separation requirements in counterfactual experiments, we show that expanding UI eligibility increases average wages and UI take-up rates, but it can also lead to longer unemployment durations and modest declines in employment rates. In addition, relaxing requirements have an effect on workers' quitting behavior.

Overall, our study provides a more comprehensive understanding of UI's role in shaping labor market dynamics. These findings have important policy implications, suggesting that UI expansions should carefully consider the distributional effects across workers and the trade-offs between wage gains, job-finding rates, and the heterogeneity in different workers' types. As UI systems continue to evolve in response to economic fluctuations, understanding these dynamics remains critical for designing the unemployment insurance program.

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A Additional figures for empirical results

A.1 Employment Effect of UI Eligibility

In this appendix section, we examine whether unemployment insurance (UI) encourages workers to stay employed longer, which is relevant to the productivity of the next match. The findings suggest that it does not, as shown in Appendix Figure A7.

The graph presents employment rates over time, as a function of re-employment and the base period. We assess whether there is a noticeable break in re-employment rates at the UI eligibility threshold, employing local linear fits separately on either side of the cutoff. The results show that the increase in employment at the threshold is only about 0.4 percentage points, a small and statistically indistinguishable change. This lack of a significant jump suggests that UI eligibility does not meaningfully improve the productivity of the next match, as it does not lead to notably longer employment durations after re-employment.

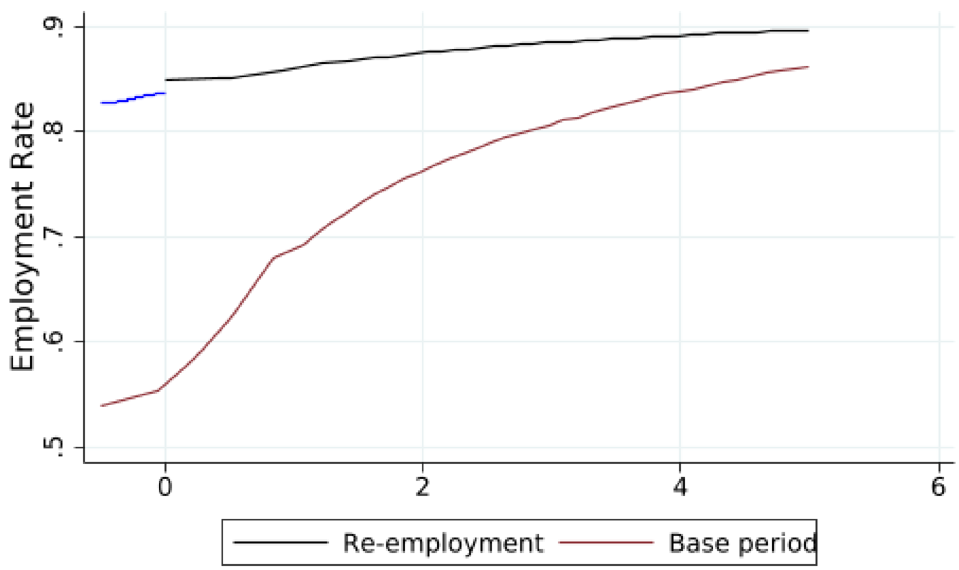

Figure A7: Employment rates prior to and after the unemployment spell as a function of base-period earnings relative to state eligibility cutoffs.

In Table Table A8 we show the regression discontinuity estimated in Equation (1) but for empirical proxies of productivity. The easiest to interpret is the second column in which the UI eligibility discontinuity does not increase the average firm wage at re-employment. This is akin to showing that the firm component of productivity is not increasing.

The first column shows that average tenure at the next firm is not increasing, through an improvement in z would make for longer matches. Note that this nonresult about re-employment tenure seems at odds with our model's important role of reduced quitting as the piece-rate w rises. This is actually for several reasons. The most mechanical is just time aggregation, which is that employment tenure can be mostly unaffected even if some very quick separations occur. The second is more nuanced, which is that the decline of the quit rate may mostly affect the higher productivity, longer tenure workers while the increase of the firing rate might mostly affect the shorter tenure workers, essentially canceling out effects on the overall tenure but increasing the size of the overall surplus. So whereas the change in the piece-rate has an ambiguous effect on tenure a change in the z overall productivity would unambiguously increase tenure, which we do not see.

Dependent	Re-employment Tenure	Next Firms' Average Wage
Bias-Corrected	-0.0193	-205.5
	(0.192)	(119.5)
Robust	-0.193	-205.5
	(0.225)	(139.5)
With B_t control	X	X

Table A8: Average tenure (quarters) and firm average wage ($) have insignificant jumps at the earnings eligibility cutoff, suggesting the productivity does not improve.

A.2 Unemployment Insurance eligibility criteria and state variation

The Unemployment Insurance (UI) program is a joint federal and state initiative designed to provide financial assistance to workers who lose their jobs through no fault of their own and are actively seeking employment. While each state administers its own UI system, they must adhere to federal guidelines, resulting in variations in eligibility requirements across states. To qualify for UI benefits, applicants typically need to meet three key criteria: earnings history, reason for job separation, and job search activity.

First, applicants must satisfy the Base Period Earnings Requirement, which ensures they have earned sufficient wages over a defined period. Most states define this as the first four of the last five completed calendar quarters, though some use an alternative base period to better account for recent employment history. Second, the Non-Quit Requirement disqualifies workers who voluntarily leave their jobs unless they have a valid reason recognized by state law, such as unsafe working conditions or employer misconduct. While some states allow broader personal reasons, others restrict eligibility strictly to job-related circumstances. Third, the Job Search Requirement mandates that claimants must be capable of working, ready to accept suitable job offers, and actively looking for employment. They are expected to accept jobs that match their skills and prevailing wages, and refusing a job without a valid reason can lead to disqualification. Additionally, claimants must submit regular job search reports, and failure to do so may result in benefit denial.

Although UI operates under federal guidelines, individual states have the authority to set their own eligibility thresholds, leading to significant differences in the minimum earnings required to qualify for benefits. As illustrated in Figure A8, the minimum earnings threshold for UI eligibility varied across U.S. states in 2013, with an average threshold of approximately $2,621. This variation reflects the joint nature of the UI program, where both federal and state governments play a role in shaping policy and determining eligibility criteria.

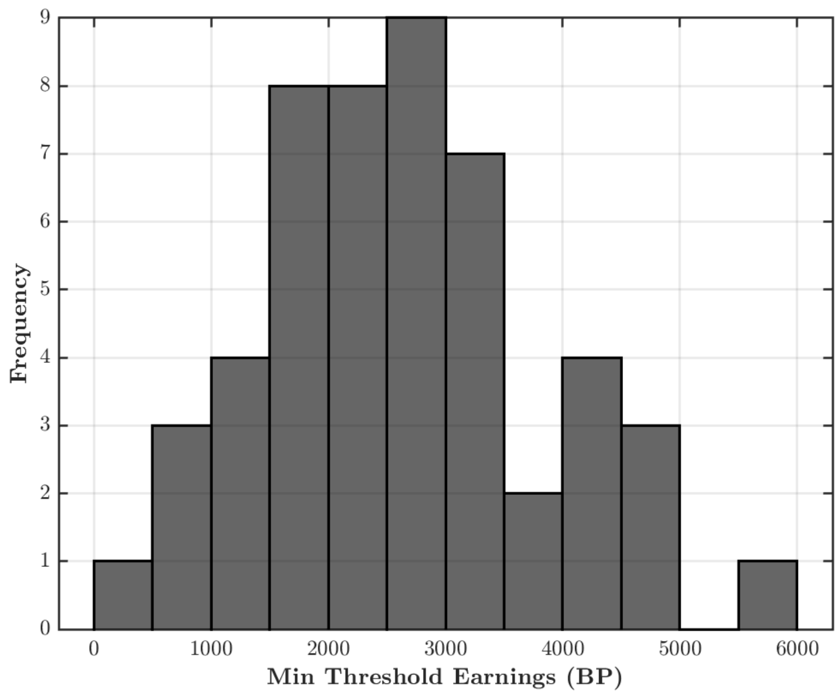

Figure A8: Distribution of state-year base period thresholds.

B Policy functions

The policy functions in Figure B9 and Figure B10 illustrate how workers direct their job search and how separation decisions, including quitting and firing, respond to changes in base-period earnings (μ).

Figure B9 presents the wage policy function, which shows how the direction of job search changes with base-period earnings. Posted wages increase with base-period earnings, but a discontinuity appears at the UI eligibility threshold. This reflects the fact that UI eligibility improves workers' outside options, influencing their job search behavior. Workers with UI eligibility are more selective, directing their search toward higher-paying jobs, while ineligible workers tend to accept lower wages more quickly. This is consistent with empirical evidence where UI receivers generally have longer unemployment duration.

Figure B10 displays the quitting and firing policy function, which reveals distinct patterns in separation behavior. The probability of quitting increases around the UI eligibility threshold, reflecting the fact that UI access reduces the opportunity cost of leaving a job. Meanwhile, firing probabilities exhibit a smoother pattern, as firms internalize the effects of UI eligibility on worker search behavior and separation decisions. The differences in quitting and firing responses highlight the role of UI eligibility in shaping labor market transitions, reinforcing the importance of accounting for both voluntary and involuntary separations when assessing the effects of UI policy.

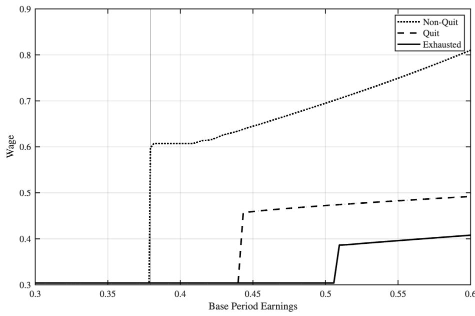

Figure B9: Wage policy

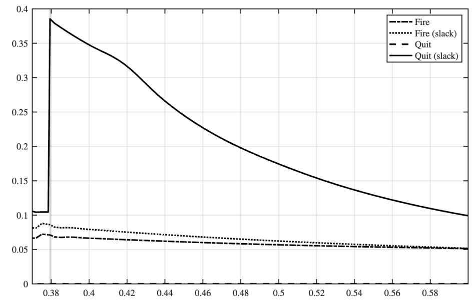

Figure B10: Quitting and firing policy

C SIPP construction and estimation

In this section, we detail our SIPP sample and how it leads to our model targets. The basic data set uses the 1990-2008 waves of the SIPP. It connects workers across waves to create a panel of labor force status and earnings. Because much of what we want to observe uses transitions and earnings, the SIPP is perfectly suited and we need to ensure both are cleanly measured. Our cleaning criteria follow Carrillo-Tudela et al. (2022), which basically strips out outliers in earnings changes and levels. It also forces transitions to be balanced, so that every transition out of employment coincides with a transition back into employment. We do not require active search during the entirety of this nonemployment spell, only that the worker returns to employment after the separation. This clears out some of the potentially spurious transitions, like between unemployment and non-employment. It is also more consistent with the sorts of transitions that we would see in the LEHD where only non-employment is defined but we refine by requiring matching transitions. The cleaned sample size is 2,215,679 individuals with observations during three recessions.

In general, however, the transition rate in the SIPP is somewhat lower than the CPS, largely because of this long panel and retrospective design. Because of the well-known seam bias associated with transitions clustering at survey response, it leads to "lumps" in the time series of transitions. Hence, we measure everything at the wave frequency, the time between survey responses. To measure whether the worker quit or was fired, we use the variable `ersend`, "main reason stopped working for employer." We categorize quitters as those who

- "Retirement or old age"
- "Quit to take another job"
- "Unsatisfactory work arrangements (hours, pay, etc)"
- "Quit for some other reason"

while firings are those who were

- "On layoff"
- "Discharged/fired"

- "Slack work or business conditions"

and the other category takes a whole range of options like health conditions or that the employer closed.

Finally, we limit our sample in the estimation of earnings and job finding and job separation rates to those who have ever experienced a unemployment spell and whose individual annual earnings are below $10,000, so as to be the population most similar to the job losers near the regression discontinuity. These workers are not necessarily in households making so little, but generally, they are not merely ancillary earners: on average, the household income is about $1,000 more than their individual income. This smaller sample contains 138,669 wave-individual observations.

We estimate EU and UE rates at the wave frequency. An EU transition means that the prior wave had employment for at least one week in each month of the wave, and then a separation with at least a month of unemployment in the next wave. A UE transition is the opposite transition.

To estimate the process for z, we estimate the variance of earnings and the covariance with one-wave lagged earnings. We require workers to be employed in both these waves and take log of the earnings. The variance is then 0.47 and autocovariance is 0.10, implying a persistence of 0.22. This is quite low, but we are conditioning on a very low portion of the earnings distribution and requiring that the individual has a recent unemployment spell.

The SIPP also allows us to see household details that are not visible in the worker-level data of the LEHD. Most notably, we can see total household income for individuals whose income is near the eligibility threshold. This answers the question of whether these individuals are merely ancillary earners. As noted earlier, these low-earning job losers are very frequently in low-income households.

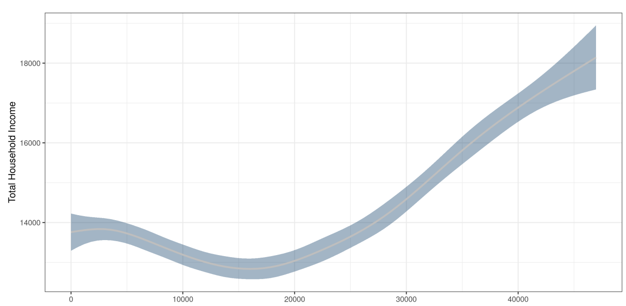

Figure C11: SIPP-estimate household income as a function of pre-seperation earnings near the monetary eligibility threshold

D Decomposing Treatment Effects

In this section, we describe in detail how we categorize different types of workers and decompose the overall treatment effects. Each individual is classified into one of several worker types based on their employment status immediately before their unemployment-to-employment transition. A worker is categorized as a *Non-Quitter* if their preceding unemployment spell is labeled as both Non-Quitter and Non-Exhausted. Similarly, a worker is assigned to the *Quitter* category if their unemployment spell is labeled as both Quitter and Non-Exhausted. If the individual has exhausted their benefit entitlement or has been rejected due to ineligibility, then they are classified as *Exhausted*, regardless of whether they were previously a quitter or not. Lastly, a worker is considered a *Claimant* if their unemployment spell in the previous quarter is associated with UI claims.

To compute the treatment effect on outcomes at the base period earnings threshold, we use a bandwidth of 2 percentage points around the threshold and compare the average outcome just above and just below the cutoff. Outcomes are measured as forward-looking averages of quarterly earnings over the next quarter; if the frequency is weekly, the outcome is simply the next week's earnings. Within the bandwidth, individuals are split into two groups: those whose base period earnings exceed the threshold (*above*) and those whose base period earnings fall below it (*below*). For each worker type, we compute the average outcome above and below the threshold and define treatment effects at both the weekly (Table D9) and quarterly frequencies (Table D10).

To analyze percentage differences in average earnings, presented in Table D9 and Table D10, we use a log difference decomposition. Specifically, the log difference is defined as:

$$\Delta \log y = \log(\bar{y}_R) - \log(\bar{y}_L).$$

Using the group means, the log of average earnings can be approximated by the sum of the logs of its components:

$$\log(\bar{y}_R) \approx \log(\bar{z}_R) + \log(\bar{w}_R), \quad \log(\bar{y}_L) \approx \log(\bar{z}_L) + \log(\bar{w}_L).$$

Therefore, the log difference can be additively decomposed as:

$$\Delta \log y = [\log(\bar{z}_R) - \log(\bar{z}_L)] + [\log(\bar{w}_R) - \log(\bar{w}_L)] + \text{interaction terms}.$$

In the log decomposition tables, the first (upper) panel reports the sum of individual log differences in slack probability, productivity, and wages:

$$[\log(\bar{z}_R) + \log(\bar{w}_R)] - [\log(\bar{z}_L) + \log(\bar{w}_L)].$$

The second (lower) panel shows the direct log difference in total earnings,

$$\log(\bar{y}_R) - \log(\bar{y}_L),$$

where the residual in the second (lower) panel captures the approximation error and interaction terms not accounted for by the additive component-wise log differences.

This approach allows us to assess heterogeneity in treatment effects across different subpopulations based on their UI status, such as non-quitters, quitters, and exhausted workers, while keeping the measurement window consistent across groups. As shown in Table 5, non-quitters who claim UI only when eligible exhibit the largest treatment effect on earnings. Non-quitters who claim UI regardless of eligibility also experience a positive effect, albeit smaller.

These findings highlight the limitations of reduced-form estimates in fully capturing the equilibrium effects of UI eligibility. While the regression discontinuity design provides a credible local estimate of the treatment effect, the observed heterogeneity across worker types underscores the importance of developing a quantitative model. Such a structural model allows us to evaluate the true causal impact of UI eligibility by incorporating dynamic employment transitions, endogenous UI take-up, and equilibrium search behavior, thereby complementing our empirical findings and providing a more comprehensive understanding of how UI affects labor market outcomes beyond the local discontinuity.

Table D9: Log Decomposition: Weekly

Overall = $[log(z_R) + log(w_R)] - [log(z_L) + log(w_L)]$				
Group	Overall	z	w	Residual
All Workers	0.0948	0.0232	0.0716	0.0000
Non-Quitters	0.1022	0.0225	0.0797	0.0000
Non-Quitters (Claim)	0.2397	0.0425	0.1973	0.0000
Quitters	0.0727	0.0306	0.0421	-0.0000
Exhausted	-0.0410	-0.0410	0.0000	0.0000
Overall = $[log(y_R)] - [log(y_L)]$				
Group	Overall	z	w	Residual
All Workers	0.0966	0.0232	0.0716	0.0019
Non-Quitters	0.1046	0.0225	0.0797	0.0023
Non-Quitters (Claim)	0.2438	0.0425	0.1973	0.0041
Quitters	0.0732	0.0306	0.0421	0.0005
Exhausted	-0.0410	-0.0410	0.0000	-0.0000

Table D10: Log Decomposition: Quarterly

Overall = $[\log(z_R) + \log(w_R)] - [\log(z_L) + \log(w_L)]$				
Group	Overall	z	w	Residual
All Workers	0.0842	0.0066	0.0776	0.0000
Non-Quitters	0.0887	0.0059	0.0829	0.0000
Non-Quitters (Claim)	0.1380	0.0043	0.1337	0.0000
Quitters	0.0675	0.0084	0.0591	-0.0000
Exhausted	0.0128	0.0107	0.0020	0.0000
Overall = $[\log(y_R)] - [\log(y_L)]$				
Group	Overall	z	w	Residual
All Workers	0.0947	0.0066	0.0776	0.0105
Non-Quitters	0.0998	0.0059	0.0829	0.0111
Non-Quitters (Claim)	0.1532	0.0043	0.1337	0.0152
Quitters	0.0759	0.0084	0.0591	0.0084
Exhausted	0.0482	0.0107	0.0020	0.0354