The Effect of Unemployment Insurance Eligibility in Equilibrium *

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Abstract

In the U.S., workers whose past earnings were below a threshold are ineligible to receive unemployment insurance (UI). This creates a discontinuous jump in their value of being unemployed. Exploiting this in a regression discontinuity design using administrative panel data, we estimate a sizable local effect from UI eligibility on earnings in the next employer, around \$300 or roughly 10% of quarterly earnings. This evidence of a UI treatment effect on re-employment outcomes, however, understates UI's causal effect because of endogenous non-compliance and it does not distinguish between underlying reasons: either a higher share of production or more productive matches. We interpret the quasi-experimental estimates through a tractable equilibrium model and a calibrated quantitative one. The empirical estimates understate the true causal effect by 9.8% and most of the effect comes from worker getting a larger share of production.

Keywords: Unemployment Insurance, Directed Search, Earnings
JEL: E24, E30, J62, J63, J64.

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1 Introduction

Unemployment spells are among the largest economic risks households face. Earnings are scarred, often permanently, human capital is lost, and workers may only regain employment at a less desirable occupation. Unemployment insurance (UI) is intended to mitigate these risks and, for many workers, UI offers vital income replacement. However, a significant fraction of the unemployment are ineligible and enter unemployment ineligible for this buffer. Those ineligible workers are often among the most vulnerable to consumption risk because their ineligibility stems from earnings that were too *low* prior to separation. One in five workers who separate from covered employment are ineligible because their annual earnings fall below a minimum threshold. With a limited ability to self-insure by drawing on savings, these low-income workers are exposed to significant consumption risk because of they are ineligible for UI.

In this paper, we address a crucial question: does UI improve earnings and employment outcomes? To answer this question, we exploit eligibility thresholds by income in a large administrative dataset to estimate the local, causal effect of UI eligibility. Then we construct an equilibrium directed search model with a detailed UI system to account for workers who are eligible, but never claim UI. In tandem, we show that eligibility has a sizable effect on earnings, particularly for workers at the margin.

Our empirical approach uses administrative data from the Longitudinal Employer-Household Dynamics (LEHD) dataset and a regression discontinuity design (RDD) to provide quasi-experimental evidence on the lost future earnings due to ineligibility. This dataset offers highly detailed earnings and employment data across 17 states from 1997-2014. Using the exact earnings criteria for eligibility, we look at re-employment earnings just below and just above state-level cut-offs. This identification strategy contrasts with other RDD-based estimates such as Nekoei and Weber (2017) or Schmieder et al. (2016) in that the treatment is on the extensive margin and the local effect is among workers with a higher marginal utility who are likely to exhibit a large response from UI receipt.

Our findings show a robustly significant effect from UI: we find a discontinuous jump of about $300 in income during the next full quarter of employment. This $300 is an increase of nearly 10% of quarterly re-employment earnings at the eligibility threshold. Our estimate is a conservative observation of the underlying causal effect of UI because we do not directly observe UI receipt and those who do not claim potentially do not experience a causal effect from above-threshold earnings, but average into the estimates. That this estimator is a "fuzzy" RD means that we likely understate the full effect of UI eligibility, though the extent to which depends on a hypothetical potential response.

We construct a frictional model of the labor market to address this attenuation. We build on a canonical Acemoglu and Shimer (1999) or Menzio and Shi (2010) framework, incorporating a detailed UI system into an equilibrium directed search model with self-insurance and match effects. In the model, workers search for jobs posted by firms. These jobs offer a fixed hourly wage, but subsequently hours vary due to idiosyncratic shocks. As a result, workers with identical wages may receive different earnings. These difference in earnings may affect the workers eligibility for UI. Production in this economy is subject to idiosyncratic shocks, and firms must pay a fixed operating cost which is distinct from the wage each period. This causes firms to occasionally fire workers when productivity drops too low. Workers may also quit when employment is less valuable than returning to unemployment and searching for a new job. These features allow us to account for both key dimensions of UI eligibility: income and separation without cause.

2 Related Literature

This paper relates to the ample of empirical papers that document the treatment effect of unemployment insurance on workers' labor market outcomes and especially recent work on how the system can affect people differently, e.g. Skandalis et al. (2022). To get clean identification of the effect of UI policy, several exploit these differences in the form of natural experiments using regression discontinuity design. In the 1990s, Card and Levine (2000) utilized a discontinuity in the UI policy in New Jersey, USA. of a six-month extended benefit in 1996 and found that the program has a very modest effect on the UI claimants. Lalive and Zweimüller (2004) found a negative effect on transition rate (17%) after accounting for endogeneity of a unique policy change in Austria that prolonged UI duration from 30 weeks to 209 weeks. Similarly using policy design in Austria, Card et al. (2007) studied sharp discontinuity in eligibility for severance pay and extended unemployment insurance and found a negative effect on the job-finding rate (5-9%) of UI extension.

However, only a small fraction of the literature considers the effect of UI on other labor market outcomes such as match quality and post unemployment wage. Furthermore, the findings of these papers are mixed. Centeno (2004) showed that the more generous UI is, the longer job tenure is (i.e., the higher match quality is). Ehrenberg and Oaxaca (1976) uses National Longitudinal Survey (NLS) and found a positive effect of UI benefit level on average wage using cross-sectional variation in replacement rate. However, their result cannot be generalized due to the limitation of the data. Griffy (2021) reaches a similar conclusion using more recent data from the Survey of Income and Program Participation (SIPP) and between-state variation in replacement rates over time. He finds a positive effect on re-employment earnings and a negative effect on hazard rates, but lacks a natural experiment and faces the same endogeneity concerns addressed in this paper. Addison and Blackburn (2000) acknowledged the lack of research on the effect of UI on post unemployment wage outcomes and aimed to provide new estimates using Displaced Worker Survey in the period of 1983-1990. They found little evidence of a positive effect of UI on wage. However the data only includes UI claimants and so it is not a causal estimate.

Recently, Schmieder et al. (2013) uses quasi-experiment of UI policy changes in Germany to estimate the causal effect of extended UI duration on wage offers.

Their estimate suggests a small and negative effect of UI extension on post unemployment wage. Nevertheless, by the nature of the design, the sample is limited to those who are at the longer end of the UI duration.

Among those taking a structural approach to interpreting the role of UI, Birinci and See (2023) find that it is crucial to consider heterogeneity when accounting for differences in observed responses to UI. We restrict our focus to a single group that is likely to be responsive to the consumption insurance value UI provides: those near the monetary eligibility threshold.

3 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 LEHD.

3.0.1 Unemployment Insurance eligibility requirements

Unemployment insurance is a progressive, conditional transfer program intended to provide consumption insurance for workers who lose their job. For recipients, UI replaces a fraction of previous income (typically around 50%) up to a maximum weekly amount. Not all workers who separate are eligible, however. To be eligible, a prospective applicant must have experienced a no-fault job loss and have earned a minimum amount in qualified employment during the "base period," which is typically the 4 quarters before job loss. While base-period earnings are not the only requirement to be eligible, it is usually necessary and therefore will be the basis of our analysis. There are other earnings and non-earnings requirements but, except in a few states with "alternative" minimum earnings thresholds, for nearly all job losers if their base period earnings were too low they cannot get UI and above the threshold they may be able to.

Despite relatively low requirements, income eligibility is a relevant consideration for a large number of potential claimants. While the this threshold is very small in the overall distribution of earnings, among job losers it is considerably higher. On average in our sample period, about $\frac{1}{5}$ of the separations earned less than this amount during the base period. Yet, many workers deemed "monetarily ineligible," having earned less than the minimum, still claim and monetary eligibility requirements account for about half of the rejections of initial claims.[1] And among those who are ineligible and claim, many are still successful and receive UI. The reasons for this include a imperfect enforcement and variability in laws over time and between states.

While UI is federally mandated, states are allowed to set their own rules for eligibility and provisions for generosity. Replacement rates and maximum benefits vary, and many states include additional eligibility requirements, like a minimum for the highest earning quarter during base period. Crucially, while

[1] The majority of the remaining rejections fail to meet the no-fault requirement.

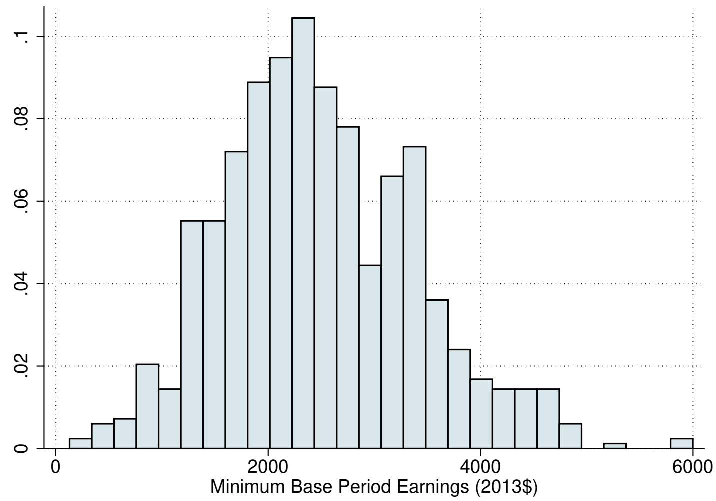

Figure 1: Distribution of state-year base period thresholds.

some states do create additional eligibility requirements, all create a threshold for minimum earnings over the "base-period." [2]

There is still variation between states in minimum levels of income for eligibility. We plot the distribution of these in Figure 1. While these differences reflect local wage levels to some extent, their dispersion is far larger than that of the state-level wages.

We use these eligibility cut-offs to determine the effect of UI eligibility on re-employment outcomes. In our baseline analysis, we will normalize our running variable to be percent deviations from the income eligibility threshold in a separator's state. Because percent deviation from the threshold is correlated with base-period earnings, one might worry that we are only picking up non-linear effects of base-period earnings, instead of the threshold effect. To address this, we also condition on prior earnings and still find a significant threshold effect. That exercises uses variation across states, because different thresholds imply that the same base-period earnings may be on different sides of the eligibility threshold. These exercises are all made possible by using these cut-offs in concert with highly accurate administrative earnings data.

3.0.2 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

[2] In almost all states, this is defined as the first four of the past five quarters so that it doesn't include the quarter of the separation.

collected by the states and used in their unemployment insurance systems to determine eligibility. This is crucial for our application: because it is administrative data, it abrogates 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 short-comings. 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, our highly accurate earnings data along with the quarterly structure of state UI systems lend credence to the validity of our results. And as we discuss, any mis-classification of treated and un-treated groups is likely to bias our findings downward, meaning we are likely to understate the size of any effect.

We follow standard restrictions when constructing out 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 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

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

employment also should not included in base-period earnings calculations.

3.1 Benefit Accuracy Measurement Data

The Unemployment Insurance Benefit Accuracy Measurement (BAM) is a survey conducted by the department of labor to provide a comprehensive assessment of the accuracy of the unemployment insurance, assess improvements in program accuracy and integrity, and encourage more efficient administration of the program. Based on the survey designed of BAM, the finding of BAM should be consistent with official rules and written policies of the Federal and State Workforce Agency (SWA). Each week, each state is required to provide a weekly representative sample of paid claims (PCA), incorrect payments (Error), and disqualifying determination (DCA). Then, each provided individual is surveyed. As a result, with BAM, we will be able to observe invaluable information on surveyed individuals regarding their past earnings at base-period before and after the investigation (thus, improper payments), demographic characteristics (gender, age, occupation, education), employment history (job before application, employer before applications), job search behavior, and rejection reasons (Monetary, separation, and non-separation reasons).

4 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 minimum previous income requirements as a source of variation for a regression discontinuity design (RDD). 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 subsequent employment duration or firms' average wage among 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.

4.1 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 2 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, average re-employment earnings in a bit optimally chosen by the methods of Calonico et al. (2019).

Notice that the trend on either side of the threshold in Figure 2 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.

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

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

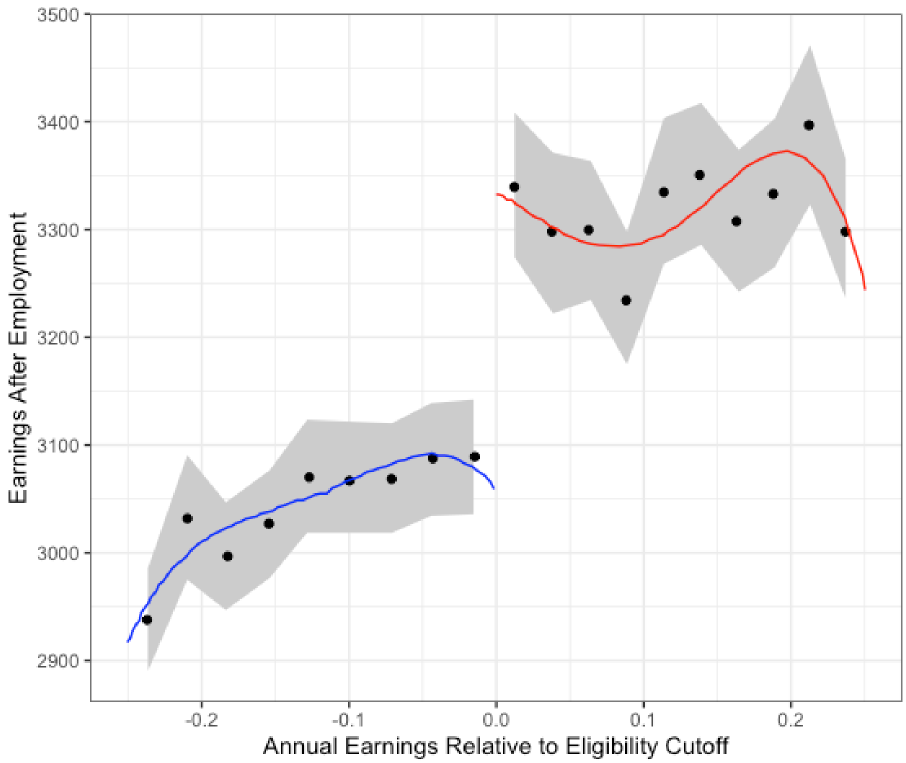

Figure 2: Annual earnings prior to separation as a percent deviation from the state eligibility cutoff against earnings in the next job. Binned scatter and 4th order polynomial fit. z

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 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. 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

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: Causal effect of UI receipt in 2013$ or as a fraction of cutoff. Standard errors in parenthesis

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, which could be consistent with the higher thresholds affecting more households.

To further assess whether these effects are spurious, we perform a series of placebo tests. We estimate our main specification but moving the threshold throughout the domain of base-period earnings. Specifically, we move it evenly between -25% and 25% of the true base-period. In Appendix A we present these results showing that in no other location do we see a significant treatment effect. The treatment effects of different base-period earnings levels bounce around zero, neither systematically positive nor negative and always insignificant.

Next we test for manipulation around our threshold. This concern is that households may choose earnings above the threshold because there are clear welfare benefits of being eligible for UI. 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. This would invalidate our experimental design because the characteristic that impelled the worker to chose to be above the threshold might also affect their future earnings.

Instead, these manipulation tests validate our assumption that workers are essentially randomly allocated above and below 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. But we cannot reject that the density estimated from the left of the threshold is the same as the density from the right. Again, 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. More details of the estimation with several assumptions and bandwidth selection techniques are given in Appendix A.

4.2 Discontinuity-based evidence on the employment effect of UI eligibility

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 and from the BAM.

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. Figure 3 shows the LEHD-derived employment rates prior to and after the unemployment spell as a function of base-period earnings. 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 interpret this figure, note it orders people by base-period earnings, which will be closely related to employment, and hence the red line almost has to be increasing sharply. That the re-employment line is far flatter partly because of mean reversion and partly because it only begins counting employment after the new job is found. The figure allows a break in re-employment earnings 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.

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

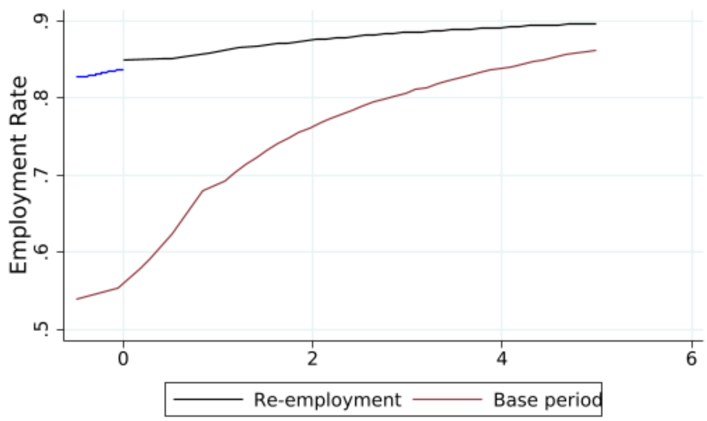

Figure 3: employment rates prior to and after the unemployment spell as a function of base-period earnings relative to state eligibility cutoffs.

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.

Table 2 shows 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.

Dependent	Re-employment Tenure		Next Firms' Average Wage	
	(1)	(2)	(3)	(4)
Bias-Corrected	-0.004	-0.009	-5.67	13.8
	(0.038)	(0.033)	(59.66)	(65.6)
Robust	-0.004	-0.009	-5.67	13.8
	(0.040)	(0.044)	(69.82)	(77.3)
With B_t control		X		X

Table 2: Average tenure (quarters) and firm average wage ($) have insignificant jumps at the earnings eligibility cutoff, suggesting the productivity does not improve.

4.3 Observable characteristics and continuity at the cutoff

Of course, these estimators all rely upon continuity across the 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 3 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 actually 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 3: Characteristics within 2% of $B_{i,t} = \underline{B}_{s,y}$. Standard errors in parentheses.

The goal of this regression discontinuity design is to estimate the treatment effect of UI on the employment outcomes of unemployed workers. The regression discontinuity design has several advantages over looking at more aggregated data, like differences in outcomes for UI recipients and non-recipients, because at the threshold we can see exogenous variation in access to the program rather than the endogenous decision to take up the payments or not. However, the estimators can not be interpreted directly as the pure treatment effect of the UI program for several reasons. In this section, we discuss them and foreshadow how the model in Section **??** can be used to address them.

4.4 Interpreting the estimated treatment effects

While 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 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 non-receivers. The eligible might not receive benefits for two reasons, first that are ineligible for other reasons and second because 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.

First, however, we can try to bound the potential treatment effect by assuming that non-compliance is purely random. In that case, the eligible who do not receive UI would have behaved exactly the same as the receivers, were UI given to them. Hence, an upper bound on the true treatment effect will divide our earlier RD estimate by the rate of non-compliance.

Unfortunately, neither source of noncompliance can be seen directly at the individual-level in the LEHD. Thus, we use the SIPP to estimate the the probability of ineligibility for reasons other than base-period earnings and of the probability of non-claiming. The main reason a worker would be ineligible despite sufficient base-period earnings is that the separation was voluntary. Hence, we condition on workers who separated and whose self-reported base-period earnings were near the eligibility threshold and compute the fraction who reported being displaced relative to those who quit. The majority of these separations would have the worker ineligible for UI: about 60%. Then, we estimate the fraction of unemployed workers who reported receiving any unemployment benefits, an upper bound on the fraction who claimed given that many would have been rejected. Again, the majority of workers do not claim: only about 43% of unemployed report actually receiving benefits. These two factors create an upper bound of the treatment effect of $946.20 or 30.3% of the base-period earnings threshold.

5 Tractable model of take-up and match quality

In this section, we describe an analytically tractable economy with UI eligibility, endogenous take-up, and heterogenous match quality. We use the model to show that i) the effect of UI may differ for non-compliers, and thus bias our empirical results; ii) UI can affect both the share of rents accrued by the worker as well as the average quality of a match. We also use this model to motivate our auxiliary model in Section 7.

Consider a one-period economy in which workers begin unemployed, and firms post vacancies to hire them. Before making decisions regarding the search, the workers will choose to apply for unemployment insurance or not. An application costs ϕ and will be successful with probability ξ. They receive flow utility b if the application is successful and η if it is unsuccessful or they do not claim.

If a match forms, it produces $z \in (0, 1]$, distributed according to $F(z)$, and pays the worker wz. Matches are formed in a frictional labor market with tightness θ and posting cost $z\kappa$ workers and firms face finding rates $p(\theta) = \theta^{1-\alpha}, q(\theta) = \theta^{-\alpha}$, respectively. Search is directed. Workers choose a submarket indexed by w, θ. After a vacancy and a worker match, z is revealed to workers, so they also must choose a lower threshold for acceptable match qualities, $\check{z}$.

We will explore two scenarios, one in which ϕ differs across workers and the other in which η does. These two distributions will be G_ϕ, G_b. The workers' problem can be described in

$$U(\phi,\eta) = \max_{\ell\in\{0,1\}} \ell \Bigg\{ \xi(\max_{w,\check{z}} p(\theta)w \int_{\check{z}}^{1} zdF(z) + (1-p(\theta)(1-F(\check{z})))b) \tag{2}$$

$$(1-\xi)(\max_{w,\check{z}} p(\theta)w \int_{\check{z}}^{1} zdF(z) + (1-p(\theta)(1-F(\check{z})))\eta) - \phi \Bigg\} \tag{3}$$

$$+(1-\ell)\left\{\max_{w,\check{z}} p(\theta)w \int_{\check{z}}^{1} zdF(z) + (1-p(\theta)(1-F(\check{z})))\eta\right\}. \tag{4}$$

Now, turning to the firm-side problem establishes the equilibrium relationship between w and $p(\theta)$. Firms post vacancies in submarkets that are specific to receipt or non-receipt and are z-, $\phi-$, and η-specific, which we index by s. The value of such a vacancy is

$$V(s) = z(-\kappa + q(\theta(s))(1-w(s))), \tag{5}$$

Because of free-entry, the value of any of these vacancies is 0, which implies a functional relationship between p and w that workers face for any ϕ, η and receipt or non-receipt, as in

$$1 - p(\theta(s))^{\frac{\alpha}{1-\alpha}}\kappa = w. \tag{6}$$

Going back to the solution to the workers' problem, we denote the wage, finding rate and $z-$threshold choices of a worker who receives UI as $w_R(\phi,\eta), p_R(\phi,\eta), \check{z}_R(\phi,\eta)$ and the wage and $z-$threshold choices of a non-receiver as $w_N(\phi,\eta), p_N(\phi,\eta), \check{z}_N(\phi,\eta)$. The optimal decisions can be expressed as

$$w_R(\phi,\eta) = \alpha + (1-\alpha)\frac{b}{\tilde{z}_R(\phi,\eta)} \tag{7}$$

$$p_R(\phi,\eta) = \left(\frac{1-\alpha}{\kappa}\left(1 - \frac{b}{R(\tilde{\phi},\eta)}\right)\right)^{\frac{1-\alpha}{\alpha}} \tag{8}$$

$$\check{z}_R(\phi,\eta) = \frac{b}{w_R(\phi,\eta)} \tag{9}$$

$$w_N(\phi,\eta) = \alpha + (1-\alpha)\frac{\eta}{\tilde{z}_R(\phi,\eta)} \tag{10}$$

$$p_N(\phi,\eta) = \left(\frac{1-\alpha}{\kappa}\left(1 - \frac{\eta}{N(\tilde{\phi},\eta)}\right)\right)^{\frac{1-\alpha}{\alpha}} \tag{11}$$

where $\tilde{z}_x(\phi,\eta) = \int_{\check{z}_x(\phi,\eta)}^{1} tdF(t)/(1-F(\check{z}_x(\phi,\eta)))$.

Finally, with the inner problems solved, we consider whether the household claims, $\ell = 1$, or not. Denote $U_R(\phi,\eta)$ as the indirect utility of a receiver, $U_R(\phi,\eta) = \max_{w,\check{z}} p(\theta)w \int_{\check{z}}^{1} zdF(z) + (1-p(\theta)(1-F(\check{z})))b$ and $U_N(\phi,\eta)$ as the indirect utility of a non-receiver, $U_N(\phi,\eta) = \max_{w,\check{z}} p(\theta)w\int_{\check{z}}^{1} zdF(z) + (1 - p(\theta)(1 - F(\check{z})))\eta$

A worker claims UI ($\ell = 1$) if the net value of receiving UI is greater or equal to the value of not receiving UI, as in

$$U_R(\phi, b) - \frac{\phi}{\xi} \geq U_N(\phi, \eta) \tag{12}$$

From (12), we show that the heterogeneity can drive the difference between the true and observed treatment effect in ϕ or η. Moreover, the true treatment will only be inflated by non-compliance through G_ϕ and not G_η. In the first of the two scenarios where $\phi \sim G_\phi$, the observed treatment can be expressed as

$$\widehat{\Delta w} = \int_\phi (w_R(\phi) - w_N)\mathbb{I}_{U_R(\phi)\geq U_N} dG_\phi(\phi) \ , \tag{13}$$

and the true treatment can be expressed as equation

$$\Delta w = \frac{\int_\phi (w_R(\phi) - w_N)\mathbb{I}_{U_R(\phi)\geq U_N} dG_\phi(\phi)}{\int_\phi \mathbb{I}_{U_R(\phi)\geq U_N} dG_\phi(\phi)}. \tag{14}$$

In the second scenario where $\eta \sim G_\eta$, the observed and the true treatment can be expressed as

$$\widehat{\Delta w} = \int_\phi (w_R - w_N(b_N))\mathbb{I}_{U_R\geq U_N(b_N)} dG_b(b_N). \tag{15}$$

Observing higher post-unemployment wage in the data can be translated into two things: the match quality and the surplus of the match is higher, thus a higher payoff. Or, workers have higher outside options (having access to UI) to extract more of the surplus. One of the key advantages of having a model is to further decompose the causal effect of UI take-up on post-unemployment wage into better match quality and higher rents. In

$$w_x(\phi,\eta)\tilde{z}_x(\phi,\eta) = \alpha\tilde{z}_x(\phi,\eta) + (1-\alpha)b_x \quad , x = \{R, N\}, kdkl \tag{16}$$

the wage received by matched workers $w_x\tilde{z}_x$ can be expressed as a fraction of $\tilde{z}_x$, and worker's outside options b or η, with the fraction as α. One can think of α as the competitive search analog of Nash bargaining weight in random search. As bargaining weight α goes to zero, the change in wage is almost all caused by the change in outside option. (i.e. As $\alpha \to 0$, $\frac{\partial w}{\partial b}\frac{b}{w} \to 1$)

6 Quantitative model

6.1 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 cost of operating, χ. After observing the productivity and hours shocks, the firm decides whether to fire their worker, which we denote with the indicator $d_f(w, z, \mu, \chi)$. Matches may also dissolve because workers quit, which depends on a time cost shock, γ, realized by workers 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 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 or not their income history falls above or below a monetary eligibility threshold, $\bar{\mu}$. While neither factor unilaterally precludes a worker from receiving UI, quitting or having income below the threshold hamper their likelihood of receipt. If a worker fails to meet either the separation or monetary eligibility 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 exausted their benefits, they receive $b_n < b_{RR}(\bar{\omega})$.

Firms post vacancies at a cost κ. Vacancies are one-firm 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.

6.2 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.

6.2.1 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 firms' 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

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

$$\text{s.t. } c = \begin{cases} wz & z > \underline{z} \\ b_n & z = \underline{z} \end{cases} \tag{17}$$

$$\mu' = \left(1 - \frac{1}{T}\right)\mu + \frac{1}{T}wz \tag{18}$$

$$z' = \begin{cases} \underline{z}, & w/\ prob.\ p_0(z) \\ z' = \rho z + \epsilon_z, & w/\ prob.\ 1 - p_0(z) \end{cases} \tag{19}$$

They receive income wz, where z is realized before the period. They consume their income, $c = wz$.[4].

[4]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.

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

$$\Pr(U_C(s_U^1) > U_E(s_E')) = \frac{\exp\{\left(U_C(s_U^1) - U_E(s_E')\right)/\sigma_\gamma\}}{1 + \exp\{(U_C(s_U^1) - U_E(s_E'))/\sigma_\gamma\}} \tag{20}$$

which is a standard result when the shock, γ, is Gumbel distributed.

6.2.2 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 chooses 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

$$\begin{aligned} U_C(s_U) = \max_{\ell \in \{0,1\}} u(b_n) + \beta E[\mathbb{I}_{\{\ell=1\}}\{\xi(s_U)R_R(s_R') \\ + (1 - \xi(s_U))R_X(s_X') - \eta - \epsilon\} + \mathbb{I}_{\{\ell=0\}}R_C(s_U')] \end{aligned} \tag{21}$$

$$s.t. \;\; \mu' = (1 - \frac{1}{T})\mu \tag{22}$$

$$\xi = \begin{cases} \xi_h e^{\xi_Q} & \text{if } \mu \geq \bar{\omega} \\ \xi_l e^{\xi_Q} & \text{if } \mu < \bar{\omega} \end{cases} \tag{23}$$

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 prior to 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

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

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

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.[5] 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

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

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

6.2.3 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

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

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

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

6.3 Firms's Problem

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

6.3.1 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:

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

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

$$
\Pr(J(w, z, \mu) > 0) = \frac{\exp\{J(w, z, \mu)/\sigma_\chi\}}{1 + \exp\{J(w, z, \mu)/\sigma_\chi\}} \tag{24}
$$

6.3.2 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 draw an idiosyncratic productivity, z. An unmatched firm has the value function

$$
V(w) = -\kappa + \beta q(\theta(\theta)))E_{z'}[(1 - D(w, z'))J(w, z')]. \tag{25}
$$

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

$$
q(\theta(w)) = \frac{\kappa}{\beta E_{z'}[(1 - D(w, z'))J(w, z')]} \tag{26}
$$

in a submarket.

6.4 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.

7 Calibration

We discipline our model using simulated method of moments, incorporating our empirical results in section 4 as well as insights from our static model in section 5. In subsection 7.1, we preset parameters that are externally estimable or available from closely related work, and then define our functional forms. In subsection 7.2, we describe our auxiliary model and use our static model to show how these moments discipline our key structural parameters.

To draw quantitative conclusions, we calibrate the model to match three sets of targets: standard search and matching model targets, UI-specific features, and the re-employment earnings jump. We focus on earnings and employment dynamics, alongside the estimated treatment effect, to mirror the incomplete UI exposure that we observe in the data. This allows us to assess the underlying treatment effect driving our quasi-experimental results. By matching the RDD estimated treatment, we can infer typically hard-to-observe parameters that determine the workers' share of the surplus.

7.1 Preset Parameters and Functional Forms

We start by making several standard functional form assumption and externally calibrating a subset of parameters. We assume that workers have CRRA utility, $u(c) = \frac{c^{1-\sigma}}{1-\sigma}, \sigma \neq 0$, which allows unemployment insurance to have a non-linear effect on marginal utility. We assume that the matching function $M(u, v) = n_0 \frac{uv}{(u^{n_1}+v^{n_1})^{\frac{1}{n_1}}}$, following den Haan et al. (2000), which ensures that match probabilities remain bounded between 0 and 1.

Table 4 shows the fixed parameters calibrated outside of the model. We calibrate the model to a weekly frequency. We set $\beta = 0.998$ implying an annual interest rate of roughly 5%, close to the average during this period. We follow Fujita and Ramey (2012) and assume that it takes roughly 6.7 hours per week to fill a vacancy, so we set $\kappa = 0.2$, roughly in line with others in the literature as in Petrongolo and Pissarides (2001). Next, we use US Department of Labor data to calibrate the UI system in our model. For our sample period and states, the average statutory replacement rate is $b_{RR} = 0.56$. Then, the parameters regarding worker's productivity process is directly estimated from SIPP, with auto-correlation 0.8 and standard deviation 0.2. TFP is normalized to one, and the value of risk aversion is 2. Lastly, T is the "look-back" period, over which previous income is calculated for eligibility and level of benefits. T is about 52 weeks in normal times.

Parameter	Value	Comment
β	0.998	Discount rate
n_0	1	Matching efficiency
κ	0.20	Vacancy creation cost
b_{RR}	0.555	UI replacement rate
ρ_z	0.8	Auto-corr. of z shock
σ_z	0.2	SD of z shock
σ	2	Risk aversion
A	1.00	Normalization
T	52	Base period lookback

Table 4: Fixed Parameter

7.2 Targeted Moments

The free parameters are estimated by matching simulated moments. Table 5 shows the estimated free parameters and Table 6 shows the comparison between the moments estimated from the data, and the ones generated from the model. The the jump in earnings at the cutoff and data moments on employment transitions are estimated from LEHD and SIPP respectively. The data moments of the UI status are estimated from the Non-monetary Determinations Activity reports and the Benefit Rights and Experience reports from the Employment and Training Administration (ETA).[6] From Table 6, we show that this model calibrated to match the 10% jump in post-unemployment earnings that we estimated in section 4 using RDD. Although this model underestimate the difference by 0.7 ppt, this moment is the key to success for our decomposition in the later section.

There are three sets of moments crucial to the success of this paper: the jump in earnings observed in the data, labor market transitions, and the UI

[6]ETA reports are from the National Office database that is populated by collecting data from the 50 States, Washington DC, Puerto Rico, and the Virgin Islands. The data in the Non-monetary Determinations Activity reports are used by the U.S Department of Labor to project budgets and to assess the disqualification processes. The Benefit Rights and Experience reports are used to evaluate state benefit formulas

status distribution. Correspondingly, these moments are disciplined by three sets of parameters: those common in equilibrium search models, parameters unique to our UI structure, and parameters related to endogenous separation on both the firm and worker sides.

The first set of parameters is the value of the matching elasticity n_1 and outside subsistence income b_n. Outside subsistence income b_n directly affects how much unemployed worker values unemployment insurance benefits. The matching elasticity, as we derived in Section 5, plays a key role in determining how much earnings is coming from the productivity of a worker and how much of that is from workers' outside options b_n. These two parameters are keys to capture the 10% earning difference at the cut-off. Moreover, in Section 5, we demonstrate that n_1 (or the corresponding $1-\alpha$ in the tractable model) is a crucial parameter indicating how much of the match surplus is passed through to the worker relative to the firm in a Nash bargaining setting, or how much of the change in wage is attributable to changes in outside options. We find that this value exceeds 62%.

The second set of parameters are parameters related to our unique UI eligibility status and productivity process. First, we have the probability of receiving UI conditional on the earnings eligibility status ξ_h, ξ_l, quitting penalty φ and the slack probability p_0. ξ_h, ξ_l and φ are novel and specific to how we model UI eligibility determination process. These three parameters allow us to consider both eligibility requirement in UI: earning requirement, quitting requirement. If a worker does not have sufficient past earning to qualify earning requirement, he receives UI with probability xi_l. If a worker quit in his previous job, his probability of receiving UI will be discounted by φ. These are to take into account that in the data, we observe non-negative rejections regarding both eligibility requirements and ξ_h, ξ_l and φ allows the model to capture that. Specifically, ξ_h, ξ_l and φ is calibrated to capture monetary ineligible rejection rate, separation rejection rate and eligible receiving rate. From Table 5, we estimated that having sufficient earnings to qualify for the monetary eligibility requirement gives the applicant around four times higher probability of receiving UI. In addition, there is a 16% penalty on UI receiving rate for being a quitter. Second, we have fixed application cost η and $\bar{\omega}$. Intuitively, they discipline two moments: the claiming rate and the percentage of monetarily ineligible earners, respectively. Third, p_0 is another parameter that is specific to our model. We define p_0 as an idiosyncratic shock where, with probability p_0, the match produces a trivial amount, resulting in the firm not producing and the worker not being paid. We include p_0 because, according to SIPP data, around 26% of employed workers experience this type of shock, where for a period, workers do not receive any earnings while being employed. Moreover, this is an important parameter that allow us to generate 20% separations that does not have sufficient earnings to meet the earning requirement. We estimate p_0 by matching 26% no-work-rate observed from SIPP. Lastly, we have λ_0, the exogenous probability of losing UI benefits. This parameter is calibrated to discipline the UI exhaustion rate in the model.

Lastly, we discuss firing cost γ and quitting cost χ. To estimate these two parameters, we directly target the percentage of separation that was resulting from firing (33.4%) and quitting (17.2%) from SIPP. As Table 5 shown, it is quite costly to quit. The quitting cost is about two and a half years of accumulated subsistence income for an unemployed worker. Overall, this model does quite well matching several key moments, including the re-employment earnings jump from maintaining base period earnings that imply monetary eligibility and employment transitions. It also matches the claim, rejection and ineligible receipt rates well.

Parameter	Comment	Value
n_1	Matching elasticity	0.622
b_n	Outside subsistence income	0.146
ξ_h	Monetarily eligible UI receipt probability	0.997
ξ_l	Monetarily ineligible UI receipt probability	0.191
φ	Quitting UI receipt probability penalty	-0.164
η	Fixed application cost	0.034
$\bar{\omega}$	Eligibility threshold	0.377
p_0	Probability of no hours	0.261
λ_0	Exogenous probability of losing UI	0.001
δ	Exogenous separation probability	0.040
γ	Worker quitting cost	19.576
χ	Firm firing cost	0.010

Table 5: Parameter values

Moments	Data	Model
UI cliff	**0.100**	0.093
Employment Transition		
No-work-rate	**0.262**	0.257
EU rate	**0.031**	0.038
Quitting rate	**0.334**	0.332
Firing rate	**0.172**	0.131
UE rate	**0.560**	0.552
UI status		
Claiming rate	**0.734**	0.774
Percentage of monetary ineligible earners	**0.200**	0.214
Separation rejection rate	**0.125**	0.080
Monetary ineligible rejection rate	**0.074**	0.090
Ineligible receiving rate	**0.100**	0.100
Exhaustion rate	**0.380**	0.409

Table 6: Estimated Moments.

8 Findings

In this section, we use our calibrated model to interpret our empirical findings in subsection 4.1. First, in subsection 8.1, we decompose the total treatment effect and quantify the underlying true treatment effect by comparing the baseline model with other alternatives that groups workers by their eligibility status. Then, in subsection 8.2, we quantify the impact of both monetary eligibility requirement and no-quit requirement by performing counterfactual policy exercises.

8.1 The Underlying Effect of UI on Re-Employment Wages

In the data, we can only observed the weighted average wage from various types of worker. However, the effect of UI is different depending on the worker's state and the observed effect mixes across them. While our baseline result strongly suggests that UI receipt leads to higher re-employment earnings, we are unable to observe claim and receipt status in the data, which could affect the magnitudes of the underlying true effect. We start by exploring the true effect of UI receipt on re-employment earnings.

Here, we utilize our model to construct an appropriate counterfactual and estimate the causal effect of UI on re-employment wages. Specifically, we first group unemployed workers into categories -*NonQuit*, *Quit*, and *Exhausted*-based on their previous separation status. Furthermore, considering that a worker can switch status within a quarter, we further separate each group into conditional or unconditional, depending on whether the worker switches status in the observed quarter. For example, if a quitter later on finds a job, then gets fired within a quarter, he is included in *Quit* but not in *Quit*(conditional). Moreover, we further decompose the overall treatment effect on post-unemployment quarterly earnings into effects on productivity and effects on wages. Treatment is measured by the percent difference in outcome at the cut-off with 2% bandwidth.

Table 7 shows the treatments on worker's quarterly re-employment earnings, productivity (z) and piece wage (w) conditional on base period earnings and UI eligibility status. categorized as *Quit* and *Exhausted* show minimal earnings increases near the cutoff. This suggests that the estimated 10% treatment effect from RDD is indeed confounded by the impact on heterogeneous workers. The true treatment effect on the treated among those eligible for UI is approximately 3.6 times stronger than initially estimated. Moreover, the higher estimated re-employment earnings above the cutoff are not due to increased worker productivity, but rather because those above the cutoff have access to UI benefits, thereby enhancing their outside options and reservation wage. Combined with our findings in Section 5, this suggests that the higher post-unemployment wage observed in the data is mainly due to workers receiving a higher surplus from the match, driven by their enhanced outside options and resulting in a higher payoff, rather than an increase in match quality.

Subset	Treatment	z-Treatment	w-Treatment	Mass
Empirical Counterpart	0.093	0.001	0.450	1.000
Non-Quit	0.432	-0.001	0.536	0.201
Non-Quit (conditional)	0.561	0.000	0.536	0.104
Quit	0.065	0.004	0.000	0.009
Quit (conditional)	0.000	0.002	0.000	0.003
Exhausted/Rejected	0.064	0.002	0.000	0.790
Exhausted/Rejected (conditional)	0.000	0.001	0.000	0.024

Table 7: Decomposing the empirical treatment effect within the model by comparing the underlying treatment of compliers and the various types of non-complier. The columns with $z-$ and $w-$ treatment break down the treatment into productivity and piece-rate components.

In summary, these above results strongly suggest that the addition of UI changes a worker's reservation wage by changing their outside option. An increase from subsistence benefits, $b_n = 0.146$, to the minimum level of UI, $b_{UI} = 0.21$ (the threshold, 0.375 times the replacement rate, 0.555), produces a 43.2% increase in re-employment earnings, producing an elasticity of roughly 0.986.

8.2 The Impact of Eligibility Requirements

In this section, we analyze the relaxation of each eligibility requirement. Firstly, we reduce the base period earnings requirement by 10%. Next, we decrease the no-quit requirement by 10%. Finally, we explore the combined impact of reducing both requirements by 10% each. In reality, the relaxation of the first requirement was implemented during the pandemic through the CARES Act. While few papers have discussed the effect of removing this requirement during the pandemic and in pre-pandemic steady states (Chao 2024), the exercises below allow us to move beyond that special event and focus solely on normal times. Additionally, we address the relaxation of the quitting requirement, which has not been previously discussed. Although the no-quit requirement has not been removed in the United States, there are countries where it is not a prerequisite for UI eligibility. For instance, in Argentina, workers can apply for UI even if they voluntarily quit their previous job. Therefore, understanding the effects of these variations is non-trivial.

We start by considering the role of the base period earnings requirement. We focus on quantities that characterize much of the behavior of the economy, employment rate, claiming rate, quitting rate, UI receiving rate and average wages. We report each of these quantities relative to our baseline and present our results in Table 8.

Percent change (%) w.r.t baseline	$\bar{\omega}$	φ	*Both*
Employment rate	-0.009	-0.003	-0.014
Claiming rate	0.017	0.002	0.021
Receiving rate	0.005	0.003	0.009
Average wage	0.089	0.011	0.110
Quitting rate	0.119	0.050	0.173

Table 8: Comparison of outcomes when earning and no-quitting requirements are relaxed.

As expected, relaxing the requirements has a negative effect on employment, especially when both requirements are waived. There is also a significant positive effect on the average wage when the base period earnings requirement is relaxed, increasing the average wage by up to 9%. As before, this indicates a strong precautionary response. In both counterfactuals, there is a substantial increase in UI receipt among the unemployed when each requirement is relaxed. Predictably, after the no-quit requirement is relaxed, more people quit their jobs. This is because the value of being unemployed has increased for quitters. Interestingly, when the base period earnings requirement is relaxed, the quitting rate not only increases but does so to a greater extent.

Finally, with the relaxation of both requirements, we observe increased UI claims and receipts. This result is expected, as higher likelihood of receiving UI benefits encourages more unemployed workers to apply.

9 Conclusion

In this paper, we first present a robust empirical evidence using regression discontinuity design to identify the local effect of UI. We show that an unemployed worker, who is UI eligible, receives a $300 or roughly 10% increase in their post unemployment quarterly earnings. This provides robust evidence of a non-zero treatment effect of UI on unemployment outcomes, however, it understates UI's causal effect and does not distinguish between a higher share of production or more productive matches as the underlying reason.

Then in order to pick up the true treatment effect of UI, we decompose the total effect by using a tractable equilibrium directed search model with endogenous match quality and take-up. With the model, we are able to show that almost half of the increase in the post unemployment earnings is due to the increase in match quality. Last, we perform counterfactual exercises to quantify the impact of removing both the monetary eligibility requirement and the no-quit requirement. We find that removing the no-quit requirement do not have significant impact on the workers because quitters are not a big portion of the workers who are around the earning threshold. On the other hand, removing monetary eligibility requirement has a more sizeable impact, especially on workers' average earnings and employment rate.

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A Appendix: Placebo and manipulation tests

B Appendix: Tractable model

C Firm Side:

$$1 - p(w)^{\frac{\alpha}{1-\alpha}}\kappa = w \tag{27}$$

$$V = 0 = -\kappa + (1-w)q(\theta(w)) \tag{28}$$

$$\kappa = (1-w)q(\theta(w)) \tag{29}$$

$$\frac{\kappa}{q} = 1 - w \tag{30}$$

$$p^{\frac{\alpha}{1-\alpha}} = \frac{1}{q} \tag{31}$$

$$(\theta^{1-\alpha})^{\frac{\alpha}{1-\alpha}} = \frac{1}{q} \tag{32}$$

$$p(w)^{\frac{\alpha}{1-\alpha}} = \frac{1-w}{\kappa} \tag{33}$$

$$p = [\frac{1-w}{\kappa}]^{\frac{1-\alpha}{\alpha}} \tag{34}$$

D w_b FOC:

For this derivation we utilize the following two substitutions:

$$p'(w_b) = \frac{\delta}{\delta w}([\frac{1-w}{\kappa}]^{\frac{1-\alpha}{\alpha}}) = \frac{\alpha-1}{\alpha}p(w)\frac{\kappa}{1-w}\frac{-1}{\kappa} = \frac{\alpha-1}{\alpha}p(w_b)(\frac{1}{1-w_b}) \tag{35}$$

$$\bar{z_b} = \tilde{z_b}(1 - F(\tilde{z_b})) \tag{36}$$

Now we begin the FOC:

$$p(w_b)\bar{z_b} + p'(w_b)w_b\bar{z_b} = p'(w_b)(1 - F(\bar{z_b}))b \tag{37}$$

$$p(w_b)\bar{z_b} + \frac{\alpha-1}{\alpha}p(w_b)(\frac{1}{1-w_b})w_b\bar{z_b} = \frac{\alpha-1}{\alpha}p(w_b)\frac{1}{1-w_b}(1 - F(\bar{z_b}))b \tag{38}$$

$$\bar{z_b} + \frac{\alpha-1}{\alpha}(\frac{1}{1-w_b})w_b\bar{z_b} = \frac{\alpha-1}{\alpha}\frac{1}{1-w_b}(1 - F(\bar{z_b}))b \tag{39}$$

$$\frac{\alpha}{\alpha-1}(1-w_b)\tilde{z_b}(1 - F(\tilde{z_b})) + w_b\tilde{z_b}(1 - F(\tilde{z_b})) = (1 - F(\tilde{z_b}))b \tag{40}$$

$$\frac{\alpha}{\alpha-1}(1-w_b)\tilde{z_b} + w_b\tilde{z_b} = b \tag{41}$$

$$\frac{\alpha}{\alpha-1}\tilde{z_b} - \frac{\alpha}{\alpha-1}w_b\tilde{z_b} + w_b\tilde{z_b} = b \tag{42}$$

$$\frac{\alpha}{\alpha-1} - \frac{\alpha}{\alpha-1}w_b + w_b = \frac{b}{\tilde{z_b}} \tag{43}$$

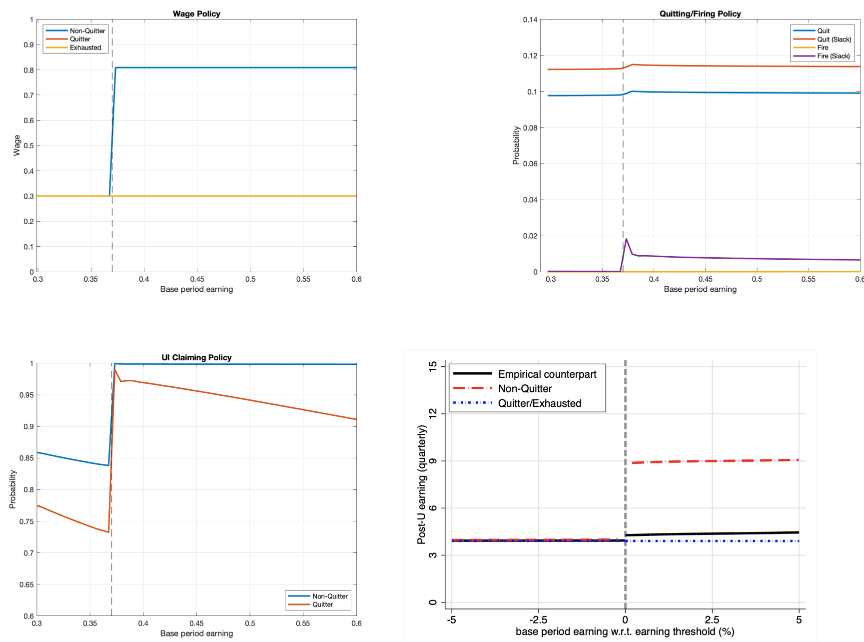
$$w_b(1 - \frac{\alpha}{\alpha-1}) = \frac{b}{\tilde{z_b}} - \frac{\alpha}{\alpha-1} \tag{44}$$

$$w_b(\frac{-1}{\alpha-1}) = \frac{b}{\tilde{z_b}} - \frac{\alpha}{\alpha-1} \tag{45}$$

$$w_b = (1-\alpha)\frac{b}{\tilde{z_b}} + \alpha \tag{46}$$

E Appendix: Policy functions

[Updated policy here]



In this section, we show some relevant policy functions. In the following figures, we plot the wage policy and the corresponding market tightness over past earnings (or past earnings at take-up for receiver) conditional on UI status. **??** shows the wage policy and the corresponding market tightness of UI non-receivers. And **??** shows that of UI receivers.

These figures show that once a worker has past earning past the monetary eligibility threshold, the value of applying for UI increases as past earning increases. As a result, worker search for a higher wage since their outside options increase. Moreover, since non-quitters have a higher probability of getting accepted with UI, their targeted wage is even higher than quitters.

For a worker who is already receiving UI, his UI benefit is a function of the past earnings at the time when they decide to apply. Therefore, the wage they search for is a function of that past earnings. Moreover, since receiver can only receive UI for at most 26 weeks under regular state program, given a same past earnings, optimal wage decreases as τ increases.This is due to the fact that for workers who almost reach the end of the maximum UI duration, they value the expected value of getting back to employment much more than staying unemployed since being employed again means another opportunity to renew UI.

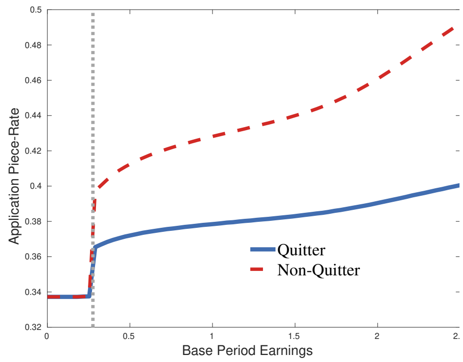


Figure 4: By quit status.

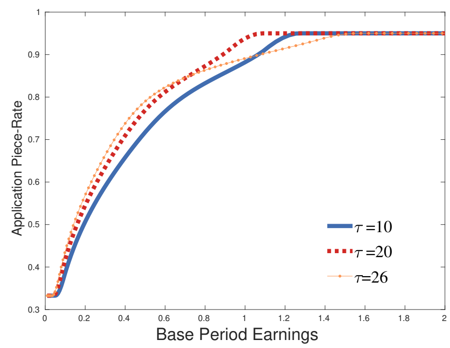


Figure 5: UI receivers.

F timing