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Should UI Eligibility Be Expanded to Low-Earning Workers? Evidence on Employment, Transfer Receipt, and Income from Administrative Data

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ABSTRACT

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JEL Classification Codes: J65, I38, J68

Key Words: Unemployment insurance, Temporary Assistance to Needy Families, TANF, Supplemental Nutrition Assistance Program, SNAP, Medicaid, welfare, public assistance, unemployment, social safety net

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ABSTRACT

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1 INTRODUCTION

Over the past several years, there has been increasing recognition among policymakers that the Unemployment Insurance (UI) program does not serve as an adequate safety net for many vulnerable workers. In particular, all state UI programs require that workers have sufficient labor force attachment in order to receive benefits. This requirement, which typically takes the form of a minimum earnings level prior to job loss, excludes low-earning workers. The Government Accountability Office estimates that low-wage workers are more than twice as likely to be out of work relative to high-wage workers but only half as likely to receive benefits, attributing a part of this disparity to the minimum earnings criteria for UI (GAO 2000, 2007). Furthermore, due to the growth of the low-skilled service sector (e.g. Autor and Dorn, 2013) and the entry of former welfare recipients into low-wage jobs (e.g., Bernstein, 2004), UI eligibility requirements are likely to have affected an increasingly large proportion of workers in recent decades.

In this paper, we examine the effect of UI eligibility on various economic outcomes among low-earning workers. These estimates are crucial for evaluating the welfare impacts of a policy such as the UI modernization provision of the American Reinvestment and Recovery Act of 2009, which allotted $7 billion to incentivize states to adopt more liberal UI eligibility rules.[1] Despite its policy importance, the consequences of UI eligibility have long been neglected in the literature, which has overwhelmingly focused on the effects of increasing UI replacement rates or extending benefit durations for UI-eligible workers. As we discuss below, however, these existing estimates are uninformative for understanding the impacts of expanding UI eligibility, as workers at the

[1] Specific policies included accounting for more recent earnings in eligibility determinations and allowing workers who seek part-time jobs or who quit their jobs for good cause to be eligible for benefits. Lindner and Nichols (2012) and O'Leary (2011) have shown that these policies will indeed expand UI access, particularly among low earners.

eligibility margin differ in several key dimensions from the typical UI recipient.

Since the primary purpose of UI is to maintain consumption following job loss (see Gruber, 1997 for a discussion), a first order question is whether UI eligibility has a meaningful impact on workers' incomes. While it may seem tautological that UI eligibility raises the disposable incomes of eligible workers, the overall impact may be dampened (or even reversed) if ineligible workers are more likely to receive other sources of income that trade off with UI. The first source of income that we consider is earnings. It is well documented, both theoretically and empirically (e.g., Mortensen, 1977 and Krueger and Meyer, 2002), that increased UI generosity reduces reemployment, and hence earnings, though existing estimates cannot easily be extrapolated to examine the effects of expanding UI eligibility. In particular, low-earning workers are likely liquidity constrained and may therefore be more sensitive to UI (e.g., Chetty, 2008 and Centeno and Novo, 2014). In the extreme case, ineligible workers may have no choice but to become immediately reemployed while eligible workers hold off reemployment until after UI benefits are exhausted. It is thus possible that, among this low-earning group, UI-eligible workers could even have lower incomes than ineligibles, since UI benefits replace only half of previous earnings.[2] On the other hand, if UI eligibility has a negligible effect on reemployment, expanding eligibility may have significant income smoothing effects.

Because our focus is on low-earning workers, it is also important to consider the effect of UI eligibility on benefits from means-tested social programs in addition to wage income. These programs provide a consumption floor, and may simply be displaced by UI benefits when eligibility is expanded. Mechanically, this occurs because UI benefits are counted as income in these programs' eligibility and benefit level determinations.

[2] Of course, eligible workers are not necessarily worse off, since they enjoy more leisure.

On top of the potential mechanical effects, if there are significant costs to participating in means-tested programs, as summarized in Currie (2006), workers may also choose not to take up welfare benefits when they receive UI. If UI eligibility reduces benefits from other programs, expanding eligibility may not only have a smaller than expected net impact on consumption, it will also have budgetary spillover effects on other areas of public spending.

The first major obstacle to uncovering the effects of UI eligibility on various sources of income is the lack of high-quality data. As noted in a recent study by Meyer et al. (2015), household survey data sets are increasingly plagued by nonresponse bias and measurement error. They find, for example, that about half of transfer dollars from two of the three outcome programs that we consider (cash and food assistance) are missing in several major survey data sets when compared with administrative counterparts. Since our focus is on low-earning workers, for whom transfers are likely to comprise a significant portion of total income, the use of survey data is particularly problematic. On the other hand, using survey data is often a researcher's only choice when studying program interactions, as they are the only source of information on multiple program use. In this paper, we utilize a unique data set from the state of Michigan, which has the advantage of being both an administrative data set and one that spans multiple programs. Specifically, the data cover the universe of UI claimants from 2005 to 2010, and contain information on earnings and transfers from three major means-tested programs: Temporary Assistance to Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program), and Medicaid.

A second challenge in isolating the effects of UI eligibility lies in the fact that previous work history is used to determine eligibility. Eligible workers, who have higher prior year earnings and possibly higher unobserved future earnings potential, are there-

fore systematically different from ineligible workers.[3] To identify a causal effect of UI eligibility, we use a fuzzy regression discontinuity design that exploits a minimum earnings rule for UI eligibility. We show that being above the minimum earnings threshold is a strong predictor of ultimately being eligible for UI among claimants whose prior year earnings are near this cutoff. We then compare the future employment, means-tested program usage, and incomes of unemployed workers who fall just above and below the threshold, and attribute the differences to the availability of UI.

Our analysis yields three main findings. First, UI eligibility increases jobless durations by 4.1 (13 percent) to 8.9 (25 percent) weeks, which roughly translates to a 5-14 percent drop in earnings over the next two and a half years. The larger estimates correspond to the Great Recession period, when the potential duration of UI was increased dramatically. Second, we find that eligibility for UI also lowers TANF participation by 42-63 percent immediately after job loss, but has no effect on usage of either SNAP or Medicaid programs. Furthermore, the reduction in TANF participation is only temporary, which suggests that some UI recipients sequentially participate in TANF after UI exhaustion. Finally, and most importantly, we find that despite statistically significant reductions in both wage and means-tested transfer income, the net impact of UI eligibility on total income is positive and large: In the quarter following job loss, UI eligibility increases income by 46 and 61 percent, during the prerecession and Great Recession periods, respectively. To the extent that the observed income differences translate to consumption differences, these results indicate that there are large consumption smoothing benefits in expanding UI access to low-earning workers.[4]

[3] In nonadministrative data, eligibility is often not observed. Comparing recipients and nonrecipients is problematic for the same reasons as above, and in addition, nonrecipients could also differ if only those who anticipate being unemployed longer apply for UI benefits.

[4] One of the main arguments against using income to measure consumption for the low income population is that government transfers are widely underreported (Meyer and Sullivan, 2008). However, since we use administrative data that cover a number of transfer programs, this is much less of a concern here.

Finally, we use our empirical findings to assess the social welfare implications of UI eligibility expansions. As highlighted by Baily (1978), the decreased rate of reemployment associated with UI reflects a moral hazard risk of social insurance: UI recipients do not take into account the social costs of prolonged unemployment, in terms of lost tax contributions. Expanding UI eligibility could therefore be socially costly, even if it improves individual workers' welfare through increased income and consumption. In the final part of the paper, we examine the social welfare impact of a policy that lowers the UI eligibility threshold using a simple model in the spirit of Baily (1978) and Chetty (2008). Calibrating the model using observed empirical quantities and realistic values of relative risk aversion suggests that recent efforts to increase UI access by reducing eligibility requirements for low-earning workers may be socially beneficial.

The rest of the paper will be organized as follows. In Section 2, we discuss our study's contribution to the previous literature. Section 3 contains relevant institutional details about Michigan's UI and means-tested programs. We then describe our empirical strategy and data in Section 4. Section 5 contains our empirical results as well as an analysis of the social welfare impact associated with changing the UI eligibility threshold. Finally, Section 6 concludes with a summary of our findings and policy implications.

2 PREVIOUS LITERATURE

Our study connects to three major strands of literature. First, it is related to several studies that quantify the benefits of UI by measuring its impact on consumption and income. Gruber (1997) and Browning and Crossley (2001) directly estimate the effect of UI benefits on measures of household consumption. Gruber (1997) finds that workers experience a drop in food consumption during unemployment of 6.8 percent, but that the drop would be three times larger in the absence of UI benefits, at 22.2 percent. While Browning and Crossley (2001) finds smaller effects, they note that the effects are hetero-

geneous and largest for those with fewest assets. A more recent literature seeks to break down the consumption impact by looking at *which* components of income, including income from social programs, respond to unemployment and UI benefits. Most related to our study is one by Rothstein and Valletta (2014), who use data from the Survey of Income and Program Participation and find that when workers *lose* UI at benefit exhaustion, wage and social assistance income increase significantly, but only offsets the loss of UI by approximately 31 percent. Kawano and LaLumia (2015) also study the effects of UI on wage and various types of nonwage income, and do so using high quality tax return data.[5] They find that more generous UI benefits (as measured by state-level maximum benefit levels) are associated with less wage income and retirement savings withdrawals. In contrast to these two studies, our focus is on the causal impact of being eligible to receive UI benefits on various income sources.

Second, this study contributes to a large literature that examines the employment effects of UI. Prior studies focus on the effects of increasing the UI wage replacement rate or benefit duration for workers who *already receive* UI, and generally find that increasing benefit generosity lengthens durations of unemployment, though estimates of the magnitude cover a broad range (see surveys by Krueger and Meyer, 2002 and Tatsiramos and Van Ours, 2014). In contrast, we estimate the effect of *UI eligibility* rather than marginal increases in benefits, which may differ if UI-ineligible workers are more likely to find alternative sources of income.

Finally, our study adds to a developing literature on the interaction between UI and other social safety net programs. Most of the recent work in this area has concentrated on whether more generous UI benefits affect disability insurance caseloads, which has so

[5] Although they are able to analyze the effect of UI on many sources of nonwage income, like retirement, disability insurance, and self-employment, they are unable to observe other social assistance income that are not taxable.

far yielded mixed evidence (Lindner, forthcoming; Rutledge, 2012; Mueller et al., 2013; and Inderbitzin et al., 2013). In terms of the interaction between unemployment benefits and means-tested programs, the study that is most closely related to ours is Browning et al. (1995), who use the 1997 Canadian Out of Employment Panel data set and find that when a Canadian reform disqualified a group of unemployed workers (voluntary quitters) from UI, take-up of welfare increased by an offsetting amount. Whelan (2010) uses the same data as Browning et al. (1995) to further examine this interaction among all job separators, and finds more generally that lowering the potential duration of UI increases usage of means-tested social assistance. Within the U.S. context, the effect of UI on means-tested program participation remains largely unexplored. [6]

3 INSTITUTIONAL BACKGROUND

Unemployment Insurance

The unemployment insurance system in the United States provides temporary partial wage replacement to workers who involuntarily lose their jobs. Because UI is administered jointly by federal and state governments, the general program features are similar across states but differ in terms of specific eligibility requirements, benefit levels, and the potential duration of benefits. Since we use only data from the state of Michigan, we describe in detail the specific eligibility rules and benefit entitlements for this particular state. These eligibility rules are central to our identification strategy.

To be eligible for UI benefits, an unemployed UI applicant must satisfy both "monetary" and "nonmonetary" eligibility criteria. Monetary eligibility requires that a claimant demonstrate labor force attachment by having sufficient recent earnings. The specific

[6] Exceptions include O'Leary and Kline (2008), who look at how UI impacts return to welfare among former TANF participants, and O'Leary and Kline (2014), who document higher SNAP receipt among ineligible UI claimants.

requirement in Michigan is that during the first four of the previous five completed calendar quarters ("standard base period") or the most recent four completed quarters ("alternative base period"), a worker must have earned at least $2,871 in the highest earning quarter.[7] A second criteria for monetary eligibility is that earnings in the entire base period must be greater than 1.5 times the high quarter earnings.[8] Finally, UI applicants must have had an involuntary job separation, meaning that they may not have quit or been fired for cause ("nonmonetary eligibility").

In normal economic times, a UI-eligible claimant near the monetary eligibility threshold is entitled to a weekly benefit amount equal to 4.1 percent of the highest quarterly earnings in her base period, a wage replacement rate of 53 percent, for no more than 26 weeks.[9] During periods of high unemployment, the potential duration of benefits may be extended beyond 26 weeks by either the permanent federal-state Extended Benefits (EB) program, or through a discretionary federal extension program. During our sample period, which covers the Great Recession, extensions under the EB and Emergency Unemployment Compensation (EUC08) programs were in place at various points (see Farber and Valletta, 2013 and Rothstein, 2011 for a history of the haphazard roll-out of the program). Michigan UI recipients who exhausted their regular UI benefits between June 2008 and early 2012 received between 13 and 73 additional weeks of benefits. Furthermore, the American Recovery and Reinvestment Act of 2009 increased benefit levels by a flat $25 for all recipients from March 2009 through May 2010, increasing the

[7]The high quarter earnings requirement was (nominally) $1,998 from 2005 through mid 2007, $2,697 through the end of 2007, $2,774 in 2008, and $2,871 after 2008.

[8]It is also possible to be monetarily eligible for UI if a worker earns more than 20 times the state average weekly wage (SAWW) and have earnings in two quarters of the base period. In 2010, the SAWW in MI was $828.73, which meant that workers must have had base period earnings of $16,575. No claimants in our sample who did not meet the usual criteria qualified using this criteria.

[9]The exact duration entitlement is $0.43 \times \frac{\text{Base Period Earnings}}{\text{Weekly Benefit Amount}}$, with a minimum of 14 weeks, and a maximum of 26 weeks. Since the weekly benefit is a piecewise linear function of the high quarter earnings, the duration entitlement is a function of how spread out earnings are over the base period. That is, the more concentrated earnings are in one quarter of the base period, the shorter the duration entitlement.

replacement rate to approximately 64 percent for workers at the eligibility threshold.

Means-Tested Programs

We focus on participation in three means-tested programs as our outcomes of interest: Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP), and Medicaid. To receive TANF, SNAP, or Medicaid benefits, participants must have income below certain thresholds. Importantly, since UI is counted in the means test of all three programs, eligibility for UI may lower the amount of means-tested benefits (or disqualify the worker altogether).

Temporary Assistance for Needy Families (TANF)

TANF is a program that provides cash assistance to needy families. In Michigan, the maximum benefit is approximately equal to 35 percent of the federal poverty level, and is subject to a benefit reduction rate of 80 percent for earned income over $200 and 100 percent for unearned income (e.g., UI). To understand how UI eligibility may mechanically affect TANF benefits, consider a TANF-eligible family of three. Without any other sources of income, the family has a standard monthly TANF payment of $492. If the family receives UI payments of $472 a month (i.e., the minimum benefit in Michigan) but no other income, the TANF benefit for the month would equal to $20 because UI is taxed away 100 percent.

In addition to the income test, TANF participants are also subject to other requirements, including an asset test, work requirements, and time limits. Specifically, adult enrollees in Michigan must have less than $3,000 in assets, work or participate in training, and are limited to a lifetime total of 48 months of aid.[10] Perhaps because of the

[10] Asset limits, work requirements, and time limits are common features in all state TANF programs, but the specific requirements differ. For example, the lifetime limit in MI (48 months) is lower than required by federal law.

latter two requirements, which increase costs and diminish the value of being on the program, the take-up rate of TANF is low, estimated to be around 36 percent of eligibles nationally in 2007 (Loprest, 2012).

Supplemental Nutrition Assistance Program (SNAP)

SNAP, formerly the Food Stamp Program, provides individuals and families with a dollar amount on an electronic benefit transfer (EBT) card that can be spent on food items. The income limit for SNAP in Michigan is 200 percent of the federal poverty line. Benefit levels are approximately 30 percent of the federal poverty line and are subject to a 30 percent benefit reduction rate (after deductions). As in TANF, benefits may therefore be mechanically lowered by UI eligibility. For example, a family of three who has no net income receives $497 of food stamp benefits per month.[11] UI eligibility would raise the family's net income by up to $472, which would lower the SNAP benefit to $355 per month.

Apart from income requirements, SNAP recipients are also required to work or engage in job search-related activities, though certain groups (e.g., the elderly or disabled) are exempt.[12] Take-up of SNAP benefits is much higher than for TANF: Nationally, the take-up rate is estimated to be at approximately 65 percent in 2005, and up to 80 percent in 2010 (Ganong and Liebman, 2013).

Medicaid

Medicaid is a federal-state program that provides health insurance to certain groups, including low-income families, children, pregnant women, the elderly, and the disabled. During the period studied in Michigan, there were 40 ways individuals could have poten-

[11] Net income is total income less deductions for earned income and some living expenses.

[12] There was no asset test during the study period in Michigan.

tially qualified for some type of Medicaid coverage, and each sub-program had different eligibility requirements (Center for Healthcare Research and Transformation 2012). We focus here on programs available to healthy adults, since we are interested in program participation among UI claimants. Generally, adults only qualify for Medicaid coverage if they have incomes under approximately 35 percent of the federal poverty line.[13] Several categories of adults have higher income limits: Pregnant women under 185 percent of the poverty line are covered during and a few months after pregnancy, and those under age 19 at less than 150 percent of poverty are also eligible. Finally, parents and those under age 21 could potentially also qualify for Medicaid even if they exceed income limits but have high medical expenses ("medically needy").

The value of Medicaid can be approximated by per-enrollee spending, which was $3,073 for adults in Michigan during 2011 (Kaiser Family Foundation 2015). Unlike TANF or SNAP, eligibility for UI does not diminish the size of the benefit, unless income exceeds the eligibility limit. Sommers and Epstein (2010) estimates the take-up rate of adult Medicaid to be about 76 percent in Michigan, compared to a national average of 62 percent.[14]

4 EMPIRICAL STRATEGY AND DATA

Empirical Strategy: Regression Discontinuity Design

The goal of this study is to understand how eligibility for UI affects workers' incomes from various sources, including employment and other social assistance. In most nonadministrative data sets, the researcher typically observes only whether a worker receives

[13]The Medicaid program is called "Low Income Families" (LIF) in MI and comes automatically with TANF enrollment, though TANF enrollment is not necessary. If the family is no longer eligible for LIF due to income changes, a program called "Transitional Medical Assistance" grants continuing coverage for 12 months. Adults without children receive less comprehensive benefits.

[14]This take-up rate includes adults with disabilities.

UI benefits rather than eligibility. If we simply regress outcomes on whether or not a worker receives UI, we would be concerned that observed receipt is correlated with earnings ability or other unobserved characteristics that have independent effects on worker decisions to work or participate in other programs. We overcome this potential bias by using a regression discontinuity (RD) design and exploiting the rule that monetary eligibility for UI requires a claimant's past earnings exceed a specific threshold. As is well-established in the RD literature (Imbens and Lemieux, 2008; DiNardo and Lee, 2011), under certain continuity assumptions, the design allows us to identify the causal effect of UI eligibility for the subpopulation of claimants with earnings near the earnings cutoff.

In practice, we follow the literature and estimate local linear regressions with a uniform kernel:

$$Y_i = \beta_0 + \tau_y T_i + \beta_1 (X_i - c) + \beta_2 T_i \cdot (X_i - c) + \varepsilon_i \tag{1}$$

and

$$D_i = \alpha_0 + \tau_d T_i + \alpha_1 (X_i - c) + \alpha_2 T_i \cdot (X_i - c) + u_i \tag{2}$$

for $c - h \leq X_i \leq c + h$, where Y_i is an outcome measure (e.g., jobless duration), D_i is an indicator for being UI-eligible, X_i is the claimant's high quarter earnings, c is the monetary eligibility threshold, and $T_i = \mathbf{1}_{[X_i \geq c]}$. The fuzzy RD estimator is the ratio of the reduced form and first stage coefficients on T_i, $\hat{\tau} = \frac{\hat{\tau}_y}{\hat{\tau}_d}$. As noted by Hahn et al. (2001), this is numerically equivalent to an estimate of τ in the following TSLS regression

$$Y_i = \delta_0 + \tau D_i + \delta_1 (X_i - c) + \delta_2 T_i \cdot (X_i - c) + \nu_i \tag{3}$$

for $c - h \leq X_i \leq c + h$, where T_i is used as an instrument for D_i.

Our main outcomes of interest are measures of employment, participation in each

of the three means-tested programs, and income from each source. As is noted in the RD literature, estimates tend to be sensitive to the choice of bandwidth h. Intuitively, a bandwidth that is too large may yield biased estimates, while a bandwidth that is too small excludes too many observations and yields imprecise estimates. Therefore, we estimate Equation (1), (2), and (3) using a variety of bandwidths h. In our main tables, we report the estimates corresponding to a bandwidth of $700, which appears reasonable for most outcomes by visual inspection.

In the Appendix tables, we also report two additional estimates and confidence intervals. We report the estimates using a bandwidth proposed by Imbens and Kalyanaraman (2012) and bias-corrected confidence intervals as suggested by Calonico et al. (2014). The Imbens and Kalyanaraman (2012) and Calonico et al. (2014) bandwidths and confidence intervals are implemented following the algorithm in Appendix B.2 of Card et al. (2015b).[15]

Data

The data used in this paper are administrative records from the state of Michigan. From the state's Unemployment Insurance Agency, we have the universe of all UI claims made from 2005 to 2010, which we can match to quarterly earnings from 1997 Q2 - 2012 Q2. The claims data contain detailed information on when a claim for UI benefits was made, whether it was deemed ineligible for various reasons, the weekly benefit amount, "regular" potential duration (i.e., before extensions), total "regular" benefits received, and some basic demographic information. The quarterly earnings data contain earnings in each calendar quarter reported by all private UI-covered employers, as well as each employer's NAICS industry code. From the state's Department of Human Services, we have data on whether each UI claimant received benefits from TANF, SNAP, or

[15] We thank Zhuan Pei for sharing programs that implement the fuzzy analogue of both estimators.

Medicaid, and the amounts received (for TANF and SNAP) each month.

In this paper, we are interested in comparing the outcomes of claimants who were deemed eligible or ineligible for UI based on their earnings at claim filing. The data includes two types of claims that we wish to exclude from the analysis. The first type of claim is those that are filed on the one-year anniversary of a previous claim. Since claims are only valid for 52 weeks after the filing date, workers who were still unemployed or on a subsequent spell of unemployment at the end of the benefit year filed new claims to continue benefits. During the Great Recession, claimants must file again after 52 weeks to continue EB or EUC benefits from the previous benefit year. For these reasons, we observe in the data a mass of claims filed on or shortly after the one-year anniversary of a previous claim. Because these new claims are unlikely to correspond to the beginning of a new layoff spell, we exclude them from the analysis.

Second, in mid 2010, the Unemployment Compensation Extension Act of 2010 allowed a newly eligible claimant who had a previous higher, unexhausted EUC claim to receive the same weekly benefit as the unexhausted claim.[16] Therefore, after this law was in place, it is possible that eligible claimants received a much higher benefit relative to eligible claimants from before the law change. Since we cannot observe which claimants have unexhausted benefits, we make the admittedly arbitrary restriction to exclude all claims that occur within two years after another claim, under the assumption that after two years, claimants are unlikely have previously unexhausted benefits. This restriction also eliminates the problematic "anniversary" claims described above.

From this group of "new" claims, we select our analysis sample of claimants with earnings near the UI monetary eligibility threshold. Specifically, we calculate each

[16]The exact rule is that if the new weekly benefit is at least either $100 or 25 percent less than the benefit from the old claim, states may continue payment of the old EUC benefits and defer the new claim, or supplement the regular benefit with the difference.

claimant's earnings in the highest earning quarter of the previous five quarters (to account for eligibility via standard or alternative base periods) and keep only claims that are within $1,500 of the cutoff. We also exclude claimants with wage outliers – that is, those who earn more than half million dollars in any single quarter. Our final sample consists of 286,113 claims, of which 276,165 are unique individuals.

By definition, our analysis sample contains only low-earning UI claimants. The earnings at the UI eligibility cutoff is approximately that of workers employed at the minimum wage and working for about 30 hours per week during their highest earning quarter. Relative to all other new UI claimants, workers at the eligibility cutoff are at about the tenth percentile of the earnings distribution. Table 1 presents summary statistics for all new claims (column 1) and our analysis sample (column 2). Compared to the full sample of claimants, the workers in our analysis sample are younger, less educated, more likely to be nonwhite, and have shorter job tenures.[17] In our sample, workers earned on average $7,696 in the previous year. Many claimants have had a history of receiving welfare benefits: 15.2 percent are observed to have received TANF benefits before the claim, 45.6 percent received SNAP benefits, and 41.6 percent were enrolled in Medicaid.

Since our analysis sample covers periods both before and during the Great Recession, when UI was much more generous, the "treatment" of UI eligibility differed significantly depending on the timing of the claim. We explore these differences by estimating the effects separately for two subsamples. The first subsample consists of claims made before any extensions were in effect (i.e., before January 1, 2008), and therefore had a maximum potential duration of 26 weeks (two quarters). The second subsample consists of claims that potentially qualified for 92-99 weeks (at least seven quarters) of UI bene-

[17]The dependents measure in the UI data correspond to the number of dependents claimed for tax withholding purposes.

fits.[18] We say "potentially qualified" because only claimants whose earnings are spread out enough in the base period to qualify full 26 weeks of regular benefits were eligible for the full 99 weeks. Because only 57 percent of eligible claims in the sample had a potential duration of 26 weeks, we are likely to underestimate the effect for those who qualified for the full 26 or 99 weeks. The short duration subsample consists of 118,523 claims, while the long duration subsample consists of 101,958 claims.

Validity of Identifying Assumptions

A primary concern with any RD design is the ability of agents to manipulate the running variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In this specific case, we worry that claimants (or employers) are aware that they must reach the requisite amount of earnings in the base period to qualify for UI, resulting in sorting above (or below) the threshold. We attempt to dispel this concern by examining the density of claims, as well as the distribution of predetermined observable characteristics, around the threshold. We follow the standard practice of first graphically examining the density of claims and the distribution of observable characteristics around the relevant threshold, followed by formal regression analyses and tests for smoothness.

Figure 1 shows the distribution of claims around the high quarter earnings threshold by plotting the number of claims in $15 intervals around the minimum earnings amount, which is normalized to zero. Visually, there does not appear to be a jump in the number of claims around the earnings threshold, suggesting that selection into UI eligibility is unlikely. McCrary (2008) proposes a formal test of the smoothness assumption, which we implement. Using his automatic bin size and bandwidth selector, undersmoothing as

[18] Specifically, the sample contains claims made between September 19, 2008 and May 15, 2009. The potential durations are calculated assuming workers were eligible for 26 weeks of regular benefits plus extended benefits that were in effect at the exhaustion of each benefit tier (e.g, regular, EUC, or EB). We cannot stratify claims using actual potential duration because we do not know which base period (standard or alternative) *would* have been used to calculate durations for those who were ineligible.

suggested, we fail to reject the null hypothesis of no discontinuity in the density at the 5 percent level. The lack of sorting around the threshold suggests that either claimants and employers do not have precise knowledge of the location of the eligibility threshold, or that it is difficult for them to exactly control the amount of high quarter earnings.

Another method that is commonly used for assessing the validity of an RD is to examine the pattern of predetermined covariates around the eligibility threshold. We create two summary measures of covariates by linearly predicting outcomes (the probability of being employed and the probability of receiving any means-tested benefits in the quarter after the claim) using the following baseline covariates: gender, industry (19 dummies), number of dependents (5 dummies), education level (4 dummies), race (4 dummies), age deciles (9 dummies), tenure deciles (9 dummies), county (82 dummies), deciles for earnings in the previous 10 quarters (9 dummies), and year (5 dummies). Since each predicted outcome is simply a linear combination of predetermined covariates, we would expect them to evolve smoothly across the threshold if the RD were valid. Figure 2 plots the binned averages of these predicted outcomes, and shows no perceptible discontinuities in either covariate index. The first two rows of Table 2 quantify the size of these discontinuities by estimating Equation (1), with each outcome Y_i being a different covariate index, for a variety of bandwidths. The estimates roughly confirm the visual evidence that there are no discontinuities in the predetermined covariates, especially at smaller bandwidths. The next few rows of Table 2 break down the covariate index into some of its main components, which tell a similar story.

One predetermined covariate that we did not include in the covariate index is the probability that a claimant has received welfare benefits (from any of the three programs) in the observable past. As one might expect, whether a claimant has ever participated in a means-tested program is highly correlated with program participation after job loss,

and when included in the second covariate index, greatly diminishes the importance of all other covariates. We plot the conditional probability of past welfare receipt in Appendix Figure 1 and argue that it passes a simple eye test for smoothness, especially when compared to the visually striking discontinuities in our main outcomes discussed in Section 5. The last row of Table 2 shows the estimated discontinuity, which is statistically significant at the 5 percent level for some bandwidths. However, note that the estimated discontinuity is positive: Workers above the UI eligibility threshold, if anything, are *more* likely to have participated in means-tested programs in the past. To the extent that past program participation is positively correlated with future program participation, and UI eligibility has a negative effect on program participation, the estimated outcome discontinuities will be biased towards zero.

UI Eligibility (First Stage)

In this section, we show that crossing the high quarter earnings threshold has an impact on UI eligibility. A claimant is eligible for UI if she is monetarily eligible, and not disqualified due to a firing or quit. Because our sample consists only of UI claimants, the "treatment" in all the analyses below is eligibility for the minimum weekly benefit at the beginning of the unemployment spell, conditional on filing a UI claim. In Figure 3, we plot the probability of eligibility against the claimants' (normalized) highest quarter earnings in the previous five quarters. The graph shows a large increase in eligibility by 45 percentage points once a claimant's high quarter earnings exceed the threshold.

The probability of UI eligibility does not reach one to the right of the threshold for two reasons. First, it is possible for a worker to have high quarter earnings that exceed the minimum requirement but be monetarily ineligible because base period earnings are less than 1.5 times the high quarter earnings. Second, even if a claimant is monetarily eligible, she could be disqualified if she quits her job or was fired for cause. We find

that, of the claimants above the threshold who are ineligible in our sample, 20 percent failed to meet the spread requirement for monetary eligibility and 59 percent quit or were fired from their previous jobs. The rest of the excess eligibility to the left of the cutoff and ineligibility to the right of the cutoff (about 13 percent of claims) may be due to misreporting of wages by employers. For example, if a claimant's earnings were underreported in the wage records, she can ask that her earnings be verified with her employers in UI covered sectors. Additional earnings that are validated would be added to the base period earnings and could result in monetary eligibility, but the quarterly wage records would not necessarily be corrected.

Estimates of the first stage discontinuities are presented in the first column of Table 3, Panel A, using a bandwidth of $700. In Appendix Table 1, we show that the 45 percentage point increase in UI eligibility is robust to different bandwidth choices. Finally, because we will be focusing on the short and long duration subsamples in the analysis below, we also report the first stages for these subsamples in the second and third columns of Table 3.

5 RESULTS

Employment Effects

In this section, we present graphical evidence as well as formal estimates of the effect of UI eligibility on employment. To begin, Figure 4 plots binned averages of the duration of initial nonemployment spells (in quarters) against high quarter earnings categories. The initial nonemployment duration is defined as the number of consecutive calendar quarters after the filing of the UI claim in which no earnings are reported, up to 10 quarters. We find a clear discontinuity at the UI eligibility threshold, with workers who fall just above working approximately three fewer weeks (0.23 quarters) than those who

fall just below the cutoff. In the first column of Table 3, Panel B, we report the RD estimate of the duration effect using a local linear regression with a bandwidth of $700. We find that eligibility to receive UI increases the duration of nonemployment by about half a quarter, or 20 percent.

As previously mentioned, the "treatment" of UI eligibility varied over the analysis period due to legislated increases in the potential duration of benefits during the Great Recession: UI-eligible workers who separated from their jobs prior to 2008 had potential durations of up to 26 weeks, while those who separated during the recession had potential durations of up to 99 weeks. To see how the impact of UI eligibility varied in the two periods, we analyze the effects separately for the short and long duration subsamples described in data section above. The RD estimates for these subsamples are presented in the second and third columns of Table 3. For the short duration subsample, UI eligibility increases nonemployment durations by roughly 13 percent, whereas for the long duration subsample, the effect is closer to 25 percent. The last row of Panel B translates this into an average loss of $444 (5 percent) and $1,224 (14 percent) over the subsequent two and half years, for the short and long duration subsamples, respectively. In Appendix Tables 1, 2, and 3, we present these estimates using a variety of bandwidths. Looking across the columns in Appendix Tables 1-3, we see that estimates and significance levels are relatively robust to bandwidth choice. The two bandwidth selectors and robust confidence intervals suggest that in the pooled sample, duration effects are approximately 0.54-0.62, and earnings losses are $875-$922, with both ranges slightly higher but similar to the point estimates in Table 1.

Thus far, we have shown that UI eligibility increases nonemployment durations, and that the effects are stronger when there are longer potential UI durations. We now explore how the effects evolve over the jobless spell, focusing on the periods before and

after UI exhaustion. In Figure 5, we plot the impact of UI eligibility on the probability of being employed in each quarter relative to the UI claim for each of the two subsamples. Each point on the graph represents RD estimate $\hat{\tau}$ from Equation (3), where the outcomes are indicators for having positive earnings in a certain quarter, and the vertical dashed line indicates the quarter at which UI is exhausted for claimants with 26 weeks of regular benefits. The bandwidth used for estimating each of these estimates is \$700.[19] For short duration claims (Panel A), we find that UI eligibility only has a significant negative effect on employment in the first quarter after the claim.[20] In contrast, Panel B shows that the employment effects for long duration claims are larger and persist even after UI is exhausted.

The first two columns of Table 4 present the above results numerically, along with the estimated intercept right below the threshold for comparison. For short duration claims, UI eligibility results in a 4.9 percentage point difference (9 percent decrease) in the probability of being employed in the first quarter. For the long duration subsample, we find that UI eligibility decreases the probability of employment for at least 10 quarters. In each of the first 9 quarters, there is approximately a 7 percentage point decrease (13-14 percent) in employment. The last two columns present these in effects in terms of earnings differences: UI eligibility decreases earnings by \$13.92 (insignificant) and \$169.48 (18 percent) in the first quarter after the UI claim, for the short and long duration subsamples, respectively.

Though it is tempting to interpret the difference in results between the short and long duration subsamples as the causal impact of potential UI durations, one should exercise caution in doing so. It is important to note that a major difference between the two

[19] Appendix Figure 2 presents the discontinuities visually for several time periods.

[20] Although there also appear to be significantly negative effects on employment even after benefit exhaustion, the coefficients are not jointly significant.

subsamples is that claimants in the long duration subsample lost their jobs during the Great Recession. To the extent that the responsiveness to UI benefits varies with economic conditions, the difference between the two samples encompasses both the effect of potential UI durations, as well as the effects of the business cycle. Recent evidence on how the responsiveness to UI changes during downturns has been mixed: Kroft and Notowidigdo (2014) find that durations are less responsive; Card et al. (2015a) find that durations are more responsive; and Schmieder et al. (2012) find that the responsiveness remains roughly stable over the cycle.[21]

Finally, we attempt to relate our estimates of the duration effects to those of the previous literature. As mentioned above, the focus of numerous studies conducted over the last few decades has been on marginal increases in benefit levels or potential durations, rather than on UI eligibility itself. Although it is difficult to directly compare our results to this literature, as a way to gauge the size of our estimates, we can think of UI eligibility as either a large increase in the benefit level or potential duration. If the thought experiment is that ineligibles receive a zero percent replacement rate for the usual duration of benefits, our estimates indicate that a 10 percentage point increase in replacement rates leads to a 0.8 (short duration sample) to a 1.4 week (long duration sample) increase in jobless durations. If instead the thought experiment is that ineligible workers receive the same nonzero replacement rate as eligibles for zero weeks, our estimates suggest that a one-week increase in potential benefit duration increases jobless durations by 0.09 (long duration sample) to 0.16 weeks (short duration sample). Although the employment effect of UI eligibility is likely a combination of benefit level and duration effects (i.e., Lalive et al., 2006), both ranges of estimates are surprisingly consistent with those found in the recent literature. In terms of the benefit level effects,

[21] Note, however, that the outcome in Card et al. (2015a) is UI durations rather than nonemployment durations.

the most widely cited estimate is that of Meyer (1990), who finds that UI durations increase by 1.5 weeks for every 10 percentage point increase in the replacement rate. Recent reexamination of this effect using the same data as in Meyer (1990) concludes that by better controlling for past earnings, the estimate is closer to 0.9 weeks (Landais, forthcoming). In terms of potential duration effects, the lower range of our estimates, 0.09, is quite consistent with quasi-experimental evidence from the United States (Card and Levine, 2000 find an effect of 0.07), Austria (Card et al., 2007 find an effect of 0.1), and Germany (Schmieder et al., 2012 find an effect of 0.1).

Program Interaction Effects

We now turn to the effects of UI eligibility on participation in means-tested programs. As noted in the introduction, workers who are affected by policies that expand UI eligibility tend to be low-earning workers, who may be more likely to rely on several social safety net programs. Therefore, in addition to examining the effects of UI on employment, we also look at how workers who are eligible for UI differ in terms of means-tested benefit receipt.

We begin by examining the effects of UI eligibility on participation in means-tested programs in the quarter after UI claim filing. In Figures 6, 7, and 8, we plot the fraction of UI claimants receiving benefits from each program for each high quarter earnings bin. Figure 6 shows a clear drop of about 2 percentage points in the probability of TANF receipt for workers above the eligibility threshold. In contrast, the analogous plots for SNAP and Medicaid show no difference in benefit receipt from these two programs between those above and below the threshold.

As in the previous results for employment, we also examine the effect of UI eligibility on program participation at various points relative to job loss. In particular, we are interested in the longer run effects of UI eligibility: If UI has a negative effect on

TANF participation right after job loss, does it persist once UI benefits are exhausted? If so, there are potentially long term benefits associated with UI eligibility from lowering dependence on cash assistance. On the other hand, it is possible that UI simply delays workers' enrollment in TANF.

To examine the effects of UI eligibility over time, we estimate again Equation (3), but with outcome Y_i being an indicator for whether individual i received benefits from a specific welfare program in a certain quarter relative to the UI claim, separately for our short and long duration subsamples. Figures 9, 10, and, 11 plot the RD estimates $\hat{\tau}$ against the quarter relative to the UI claim quarter.[22] Panel A of Figure 9 shows that in the short potential duration subsample, UI eligibles are significantly less likely to receive benefits from TANF in the first quarter, but the gap disappears after UI is exhausted (denoted by the dashed vertical line). Panel B shows that when UI durations are longer, the gap in TANF participation persists for approximately as long as the UI potential duration, suggesting that the availability of UI simply delays the receipt of TANF for the duration of UI benefits, rather than permanently deters participants from welfare. Analogous figures for SNAP and Medicaid are shown in Figures 10 and 11, respectively. In general, we do not see similarly strong patterns in SNAP and Medicaid enrollment corresponding to the beginning and expiration of UI benefits.

The first six columns of Table 5 present the numerical estimates corresponding to these graphs, as well as the estimated intercepts right below the threshold ($\hat{\beta}_0$) for comparison. Panel A reports the results for the short duration subsample and Panel B reports the results for the long duration subsample. In the short duration subsample, being above the UI eligibility threshold decreases the probability of participating in TANF by 3.9 percentage points (42 percent) in the first quarter after layoff, 2.1 percentage points

[22] Appendix Figures 3, 4, and 5 show the discontinuities for several time periods.

(24 percent) in the second, and insignificantly after that. For claims with a potential duration of roughly eight quarters, a gap of roughly 4 percentage points (62 percent) persists for about eight quarters, after which the difference is insignificant.

The last four columns of Table 5 presents these results in terms of dollars of benefits received from TANF and SNAP per quarter. In the short duration subsample, the effect of UI on TANF benefits in the first quarter is approximately $50, and is significant for two quarters, while for the long duration subsample, the initial effect is a roughly similar magnitude at $53, but persists for eight quarters. Effects for SNAP are slightly smaller in magnitude, with an initial drop of approximately $35 and $44, for short and long duration subsamples, respectively. However, the estimates are much more imprecise than those for TANF. The magnitudes indicate that although there is a large impact of UI on TANF, very few UI claimants participate in TANF in the first place (roughly 8 percent), resulting in relatively small overall effects. Though a much larger proportion of claimants enroll in SNAP and Medicaid (roughly 30 to 40 percent) in this sample, UI does not significantly affect participation.

The above results suggest that any effects of UI on means-tested programs all but disappear within two years. To summarize our program interaction results, Panel C of Table 3 shows the estimated effects of UI eligibility on dollars of TANF benefits, dollars of SNAP benefits, and months of Medicaid benefits received over the subsequent two years. Pooling together claims of all potential durations, UI eligibility results in a $230 (40 percent) drop in TANF benefits over two years, with no significant difference in SNAP benefits and Medicaid months. In columns (2) and (3), we break the estimates down by UI potential duration, and find nearly all of the TANF benefit difference is driven by the long duration subsample. Panel C of Appendix Tables 1-3 shows that these effects are generally robust to bandwidth choice.

Since welfare programs are historically targeted at single mothers (especially TANF), we examine the effects by gender in Appendix Table 4.[23] We find that the entire effect on TANF is driven by females, which is consistent with the fact that TANF generally aids single mothers. However, we still do not find significant effects for either Medicaid or SNAP.

Because TANF, SNAP, and Medicaid differ significantly in the types of benefits provided, coverage populations, and program rules, it is difficult to fully pinpoint the reasons for the different patterns of results. However, we highlight several key differences between each program that may explain why we find strong effects of UI on TANF relative to the other two programs. First, since unearned income in TANF is subject to a 100 percent reduction rate, TANF benefits are more significantly reduced by UI eligibility than SNAP benefits (30 percent reduction) and Medicaid (not reduced). To the extent that participation in means-tested programs is costly (Currie, 2006), the low benefit level may reduce take-up of TANF among UI eligibles. Second, TANF has strict lifetime time limits while the other programs do not, which can further reduce TANF participation among UI eligibles, as it might not be worthwhile for workers to "use up" a month of TANF when benefits are offset by UI. This behavior would be consistent with Grogger (2002) and Grogger and Michalopoulos (2003), who show that welfare recipients conserve months of benefits in response to lifetime limits. A third complementary reason is that more stringent work requirements for TANF make the program more onerous and costly than SNAP and Medicaid. Finally, it is possible that since TANF provides cash assistance rather than in-kind benefits, workers may consider it more as a substitute for UI payments than the benefits from other programs.

[23] Although we observe number of dependents, it is likely understated, as only individuals who claim dependents for tax withholding purposes are observed to have dependents. Estimates of effects for the subset of claims who are observed to have dependents are in the Appendix Table 5. Estimates for claimants without dependents were not very different from estimates on the whole sample.

Effects of UI Eligibility on Total Income

The results of the previous section show when workers are eligible for UI, income from other sources (employment and other social assistance) decrease. In this section, we gauge the relative size of these reductions by comparing the total incomes of workers above and below the UI eligibility threshold, where total income includes labor market earnings, TANF and SNAP benefits, and UI payments. To the extent that this measure of income captures differences in consumption, this exercise sheds light on the consumption smoothing benefits of UI eligibility.

We construct a quarterly measure of total income as the sum of earnings, benefits from TANF and SNAP, and UI, adding each one at a time to get a sense of the relative contributions of each type of income. For each income measure and each quarter, we estimate Equation (1) using a bandwidth of $700. In Figures 12 and 13, we plot the point estimates of the intercept terms, $\hat{\beta}_0$ and $\hat{\beta}_0 + \hat{\tau}_y$, against time relative to the claim date. The lower intercept ($\hat{\beta}_0$) represents the income for claimants who just miss the threshold while the upper intercept ($\hat{\beta}_0 + \hat{\tau}_y$) represents the income of those just above the threshold. We conduct the analyses separately for the two duration subsamples. These plots represent the same information as in Tables 4 and 5, but give a better sense of the how large the RD effects are relative to each other and to preseparation earnings.

For both subsamples, because we do not observe the exact timing of UI payments within the benefit year, we assume in this exercise that claimants receive the weekly benefit amount every week after filing a claim, up to the observed total benefits received. Furthermore, as detailed in the Appendix, in the long duration subsample, we impute the UI income beyond the second quarter after layoff because we do not observe the total amount of EB or EUC benefits received. Since we are most interested in the immediate consumption smoothing effects of UI, we emphasize the (nonimputed) results from the

first and second quarters after layoff in the analysis below.

The upper plot of Figure 12 shows that earnings drop from about $1,750 one quarter before layoff by approximately $650 (36 percent) one quarter after layoff, both for workers above and below the UI threshold. This initial drop in earnings associated with job loss is similar to that found in the seminal work by Jacobson et al. (1993). After the layoff, although there is a significant difference in employment in the first quarter between the two groups, there is no difference in overall earnings. Earnings appear to catch up to pre-layoff levels within six quarters for both groups, which is much faster than documented in other studies, potentially due to our focus on low earners, who are younger than previously studied displaced workers.[24] In the middle panel, we add income from TANF and SNAP. The drop in income from one quarter before to one quarter after layoff is mitigated somewhat, to about 28 percent, but not differentially for the two groups. Finally, when we add UI income (bottom panel), we find that workers above the threshold experience about a 5 percent drop in income in the first quarter post layoff relative to preseparation, whereas workers below the threshold experience a 20 percent drop, a difference of approximately $300.

In Figure 13, we similarly analyze the incomes of long potential duration claimants. The upper panel shows that for workers above the UI eligibility threshold, earnings drop by approximately 61 percent in the quarter after layoff. The analogous drop for workers just below the threshold is 58 percent. While those below the threshold earn more in all periods after layoff, the difference is small when compared to the magnitude of the initial loss in earnings. For both groups, earnings do not reach pre-layoff levels even 10 quarters later. The relatively large initial drop and slow rebound for these workers,

[24] In Jacobson et al. (1993) and Couch and Placzek (2010) earnings do not reach predisplacement levels even six years post-displacement. Kawano and LaLumia (2015) find that earnings rebound slightly faster using tax return data.

compared to those in Figure 12, is consistent with Davis and von Wachter (2011), who find that workers who are laid off during recessions experience larger earnings losses. The middle panel shows that TANF and SNAP mitigates the income drop to about 48 and 43 percent for those above and below the threshold, respectively. By comparing the top and middle graphs, we find that although there are significant differences in TANF receipt between UI eligibles and ineligibles, they are barely perceptible when considered relative to other income. Finally, in the bottom panel, when we include UI payments (which are imputed as described above after the second quarter), we find that claimants above the UI threshold experience a 14 percent drop in income in the first quarter, whereas those below the threshold experience a 34 percent drop.

When we break down the incomes into their different components, we find that the "gain" of $621 in UI income for those just above the threshold is offset by only $129 (21 percent) in reduced earnings and other social assistance in the long duration subsample.[25] Although this estimate corresponds to the effect of UI at the beginning of the unemployment spell, it is only slightly smaller than the finding in Rothstein and Valletta (2014), where a loss of UI benefits at UI exhaustion is offset by 31 percent through increases in earnings and social assistance payments.

We close this section with a discussion of how income differences between UI eligibles and ineligibles are informative of the *consumption* impact of UI eligibility. One possibility is that while ineligibles experience larger negative income shocks, they are able to smooth consumption by drawing down savings or borrowing. However, as shown by Meyer and Sullivan (2006) for a similar population (low-educated mothers), low-earning workers are unlikely have substantial assets or access to credit. Consistent with this, Browning and Crossley (2001) find that consumption smoothing benefits of UI

[25]For the short duration subsample, the UI gain of $347 is offset by $40 (12 percent) lost from other income sources, though the estimate is statistically insignificant.

are largest for those without liquid assets. Another possibility is intra-household consumption smoothing. Although our data do not allow us to observe households, some recent studies using tax return (Kawano and LaLumia, 2015) and survey (Rothstein and Valletta, 2014) data find that earnings from other household members do not respond appreciably with UI benefits, while others suggest that responses might differ depending on which spouse becomes unemployed (Kawano and LaLumia, 2014 and Cullen and Gruber, 2000). Finally, other income sources we do not observe include disability insurance benefits, social security, and early retirement income, though evidence on interactions with UI are mixed or nonexistent (Lindner, forthcoming, Rutledge, 2012, and Mueller et al., 2013).

Social Welfare Impact of Expanding UI Eligibility

In this section, we consider the social welfare impacts of a policy that expands UI eligibility to workers by lowering the earnings threshold. As in an optimal UI analysis (i.e., Baily, 1978 and Chetty, 2008), we argue that the costs of such a policy come from increased durations of joblessness, which must be balanced with consumption smoothing benefits. One difference between our model and the previously mentioned optimal UI studies is that we assume workers' consumption during unemployment comes from means-tested programs as well as UI, which some studies have begun to do as well (Saporta-Eksten, 2014 and Haan and Prowse, 2015). As in Chetty (2008), we use empirically estimated quantities that capture the costs and benefits of shifting the eligibility threshold to determine whether it is locally optimal.

In the Appendix, we present a framework that formalizes the above points. In the model, workers are initially unemployed with exogenously assigned previous earnings that determine whether they qualify for UI. Conditional on UI eligibility, workers choose the duration of unemployment for the subsequent period. All workers receive the same

reemployment wage and pay a lump sum tax upon reemployment that finances all social benefits. Since the value of unemployment is higher for UI-eligible workers, they have longer unemployment spells. A social planner selects the UI eligibility threshold to maximize the expected utility of the unemployed, subject to a balanced budget constraint. The first order condition of the planner problem reveals that the social benefit of marginally expanding eligibility from its current level is quantified by the difference in the consumption utility between the UI-eligible and ineligible. The social cost is quantified by the difference in benefits paid out to the UI eligible versus ineligible, as well as the difference in taxes received due to longer unemployment spells.

We calibrate the model using a CRRA utility function with varying levels of relative risk aversion and observed empirical quantities from our data. Our main assumption is that earnings, UI, and welfare benefits are the only sources of income, and that workers consume all of their income. Although we do not observe households in our data, we also calibrate the model under the assumption that individuals live in two-worker households, where both workers earn the same amount. We find that for a coefficient of relative risk aversion $\gamma \geq 1.02$, it is welfare-enhancing to lower the UI threshold. Chetty and Szeidl (2007) provide evidence that the relevant value of risk aversion for unemployment shocks can be as high as $\gamma = 4$ in a model with consumption commitments. Therefore, based on this simple calibration exercise, we conclude that lowering the eligibility threshold would be socially beneficial.

There are several important aspects of the eligibility expansion problem that we ignore in our simple analysis. First, we assume that workers and employers are not able to manipulate earnings or timing of layoffs in order gain or avoid UI eligibility. While we do not find evidence of these behaviors in our data (i.e., bunching at the threshold), Baker and Rea Jr. (1998) has documented increased employment hazards when workers

meet UI eligibility requirements in Canada.

A second important caveat is that we abstract from the effects of changing the UI eligibility threshold on UI take-up. In our model, we assume that all unemployed workers apply for UI benefits, when in reality, the filing rate for UI is quite low: Wandner and Stettner (2000) find that the percent of unemployed who applied for UI was approximately 46 percent in 1993. They further find that among the nonfilers, approximately 33 percent did not apply because of perceived ineligibility due to not having worked enough. Therefore, it is likely that loosening eligibility requirements will increase the number of people who file for UI, especially among those with relatively low earnings.

Finally, we have abstracted from important aspects of TANF and SNAP that may be consequential for welfare analysis. First, we have assumed that they perfectly substitute for UI benefits when this is not the case in reality. SNAP benefits, of course, can only be used for food consumption. TANF benefits are different from UI in that there are lifetime time limits and stringent work requirements associated with participation. Therefore, we are likely understating the utility difference between UI eligibles and ineligibles. Second, in our analysis, we have completely ignored the fact that welfare programs are only available to certain groups of individuals, assuming instead that the average welfare benefit is consumed by all workers. However, if only some workers are able to access cash or food assistance, there are potential social benefits from equalizing consumption across groups when the UI eligibility threshold is lowered. A final point is that we assume a single government entity provides all social benefits, when in fact assistance programs are financed differently and administered by several levels of government. Investigating the potential redistributive consequences (across workers and across levels of government) of this policy is beyond the scope of this paper and left for future study.

6 CONCLUSION

This paper explores the impact of UI eligibility on the incomes of low-earning workers, accounting for changes in earnings and other social assistance. Using a regression discontinuity design around the UI earnings threshold, we find that UI eligibility increases jobless durations by about 4 weeks (13 percent increase) and 8.9 weeks (25 percent increase) before and during the Great Recession, respectively. We also find that UI eligibility lowered benefits from TANF, but not from SNAP and Medicaid. Our preferred estimate suggests that UI eligibility lowers the probability of TANF receipt by approximately 42-63 percent immediately after layoff, but that the effect is likely temporary.

Although we find significant effects of UI eligibility on employment and income from means-tested programs, the overall impact of UI on workers' incomes is positive and large. Accounting for the components of income that we observe (earnings, UI, and means-tested benefits), those who fall just above the UI eligibility threshold have approximately $500 more income following job separation. This result points to a significant "hole" in the social safety net for workers who are ineligible for UI benefits, as only a small portion of the difference between UI eligibles and ineligibles is made up for by other forms of social assistance.

Finally, returning to our motivating question, in a simple calibration exercise, we considered the welfare impacts of a policy in which the UI eligibility threshold is shifted downward to cover more workers. We find that, balancing the consumption smoothing benefits with the potential cost of longer jobless durations, the policy is welfare enhancing for realistic values of risk aversion. The results from this exercise suggests that policies of state UI programs such as those incentivized in the UI modernization provisions of the ARRA will yield net positive benefits to society.

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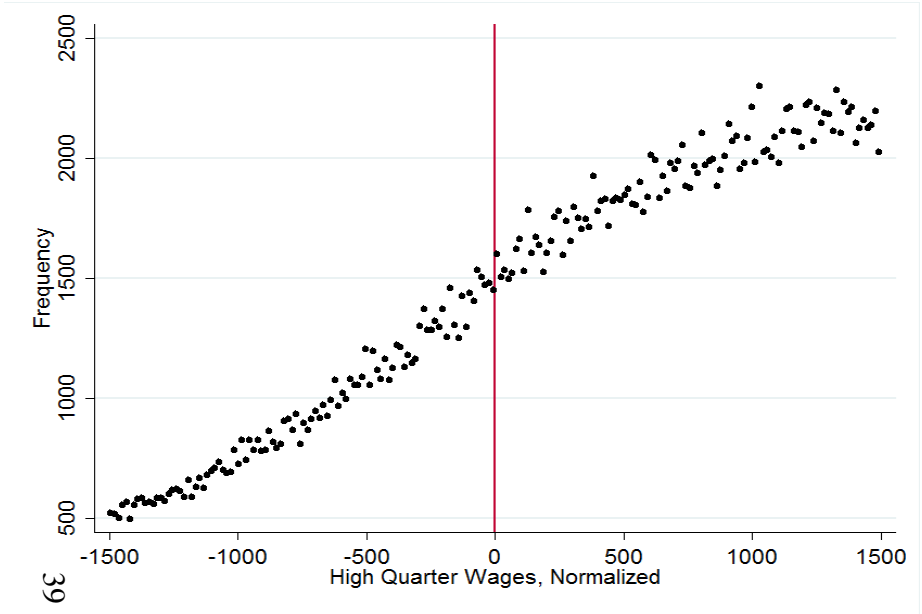
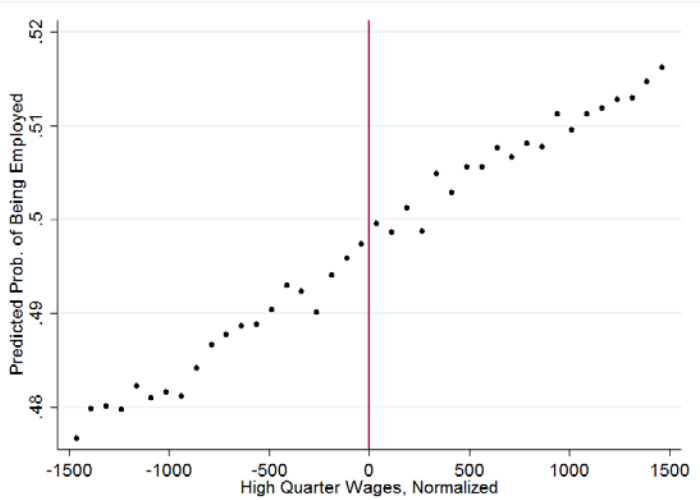
Figure 1: Density of Claims Around Eligibility Threshold



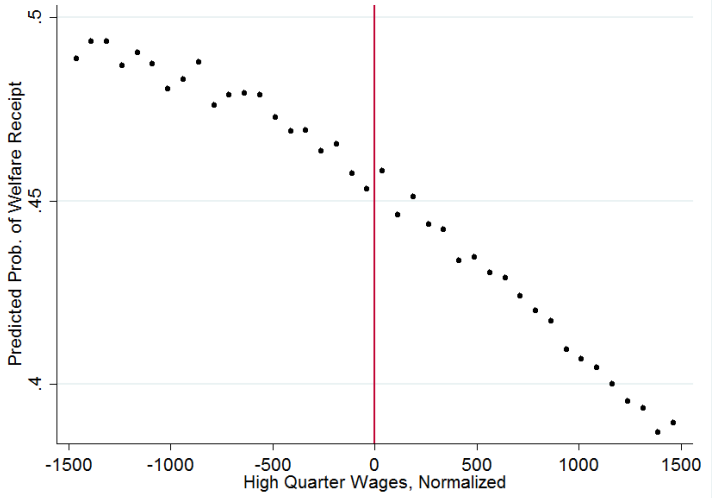
Notes: This figure plots the number of UI claimants in each non-overlapping $15 interval of (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

Figure 2: Predicted Outcomes Using Predetermined Covariates

A. Predicted Probability of Employment in First Quarter

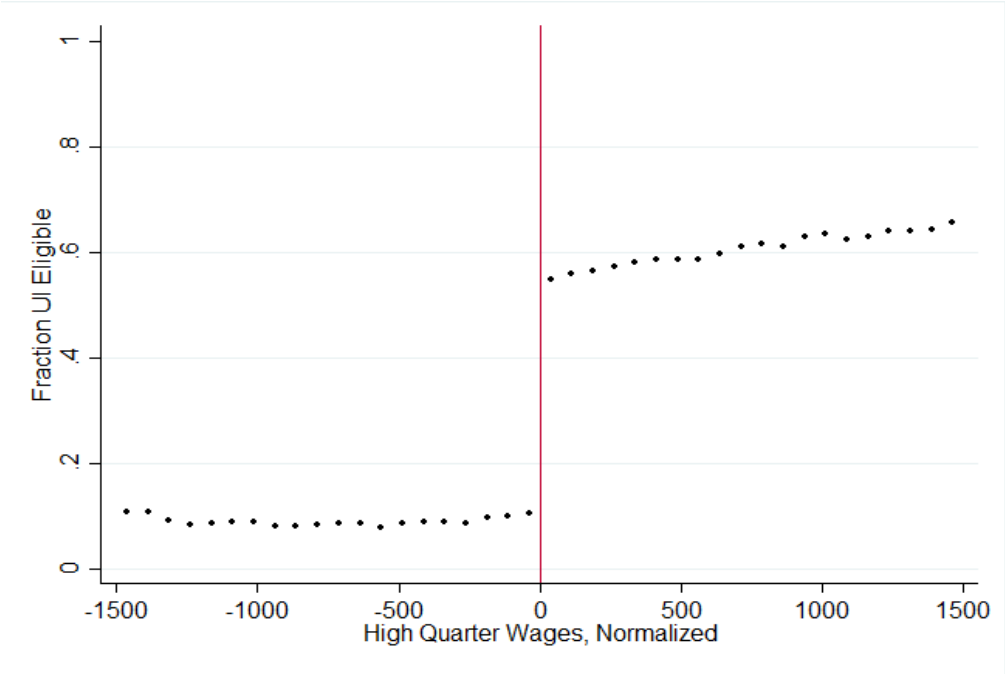


B. Predicted Probability of TANF, SNAP or Medicaid Receipt in First Quarter



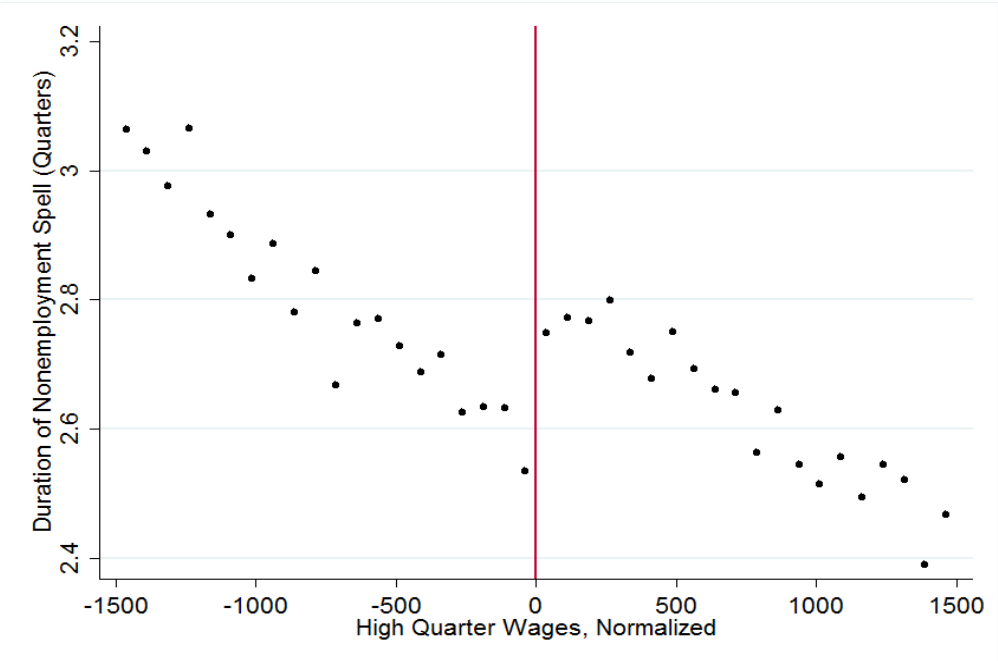
Notes: These figures summarize the relationship between observable predetermined characteristics and high quarter earnings of UI claimants. The graphs are constructed by first predicting the probability of being employed or of receiving benefits from any means-tested program in the first quarter after layoff using linear probability models with gender, industry, number of dependents, education, race, age, tenure, county, previous earnings, and year as predictors. For each outcome, the mean predicted value is plotted against non-overlapping high quarter earnings categories. The vertical line denotes the minimum earnings threshold.

Figure 3: Probability of UI Eligibility (First Stage)



Notes: This figure plots the proportion of UI claimants eligible to receive UI payments in each non-overlapping $75 interval of the (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

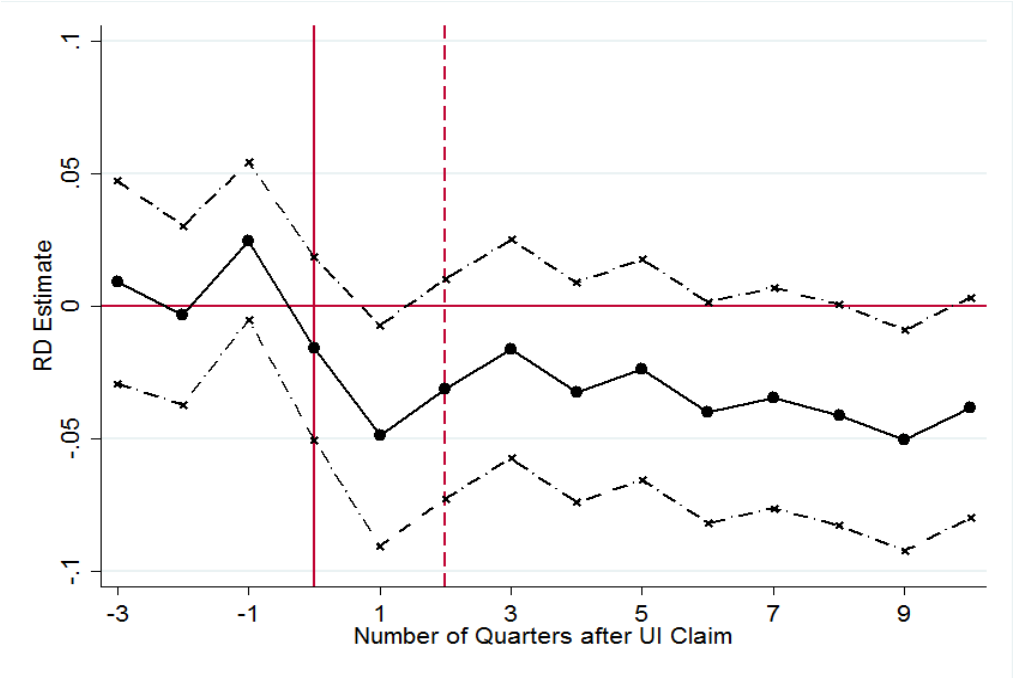
Figure 4: Duration of Initial Nonemployment Spell



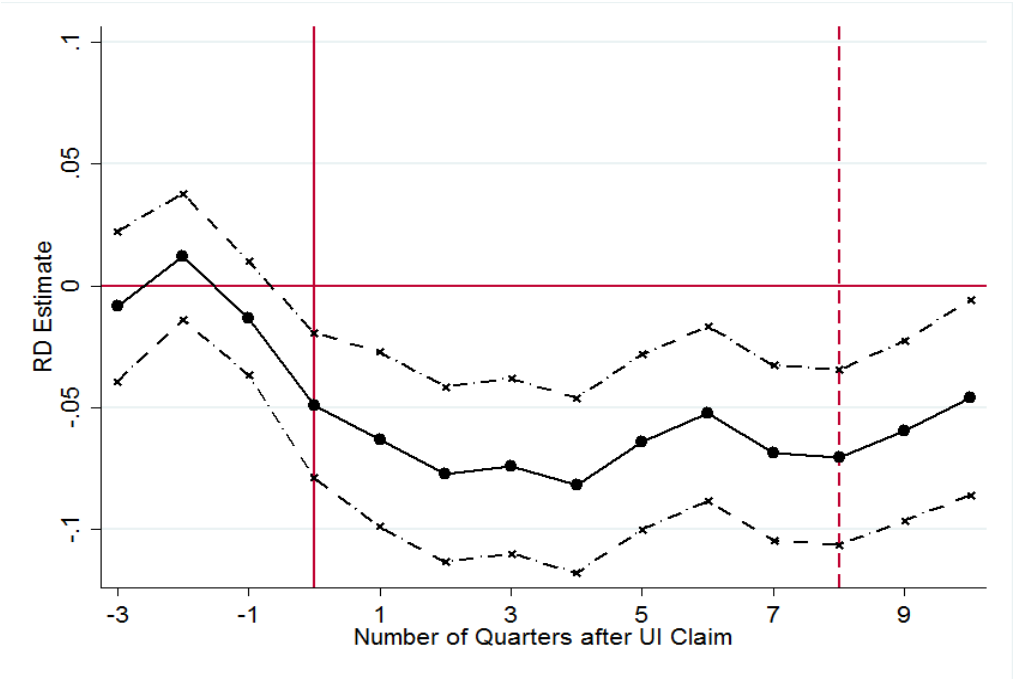
Notes: This figure plots the mean number of consecutive quarters in which UI claimants received no earnings by each category of high quarter earnings. The vertical line denotes the minimum earnings threshold.

Figure 5: RD Estimates of Employment Effects Over Time

A. Short Potential Duration

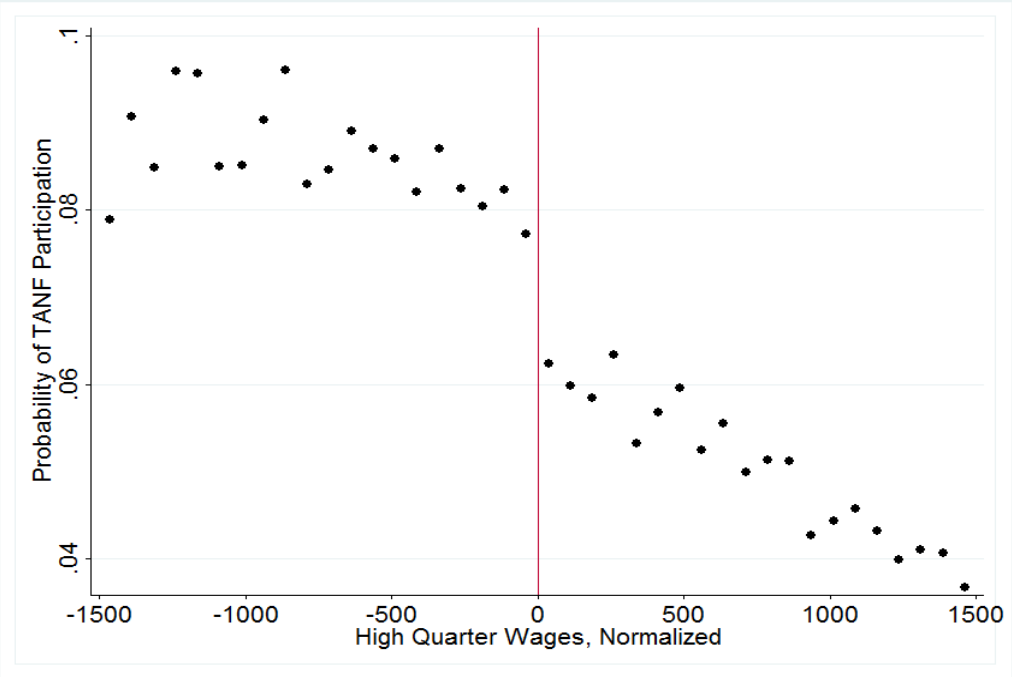


B. Long Potential Duration



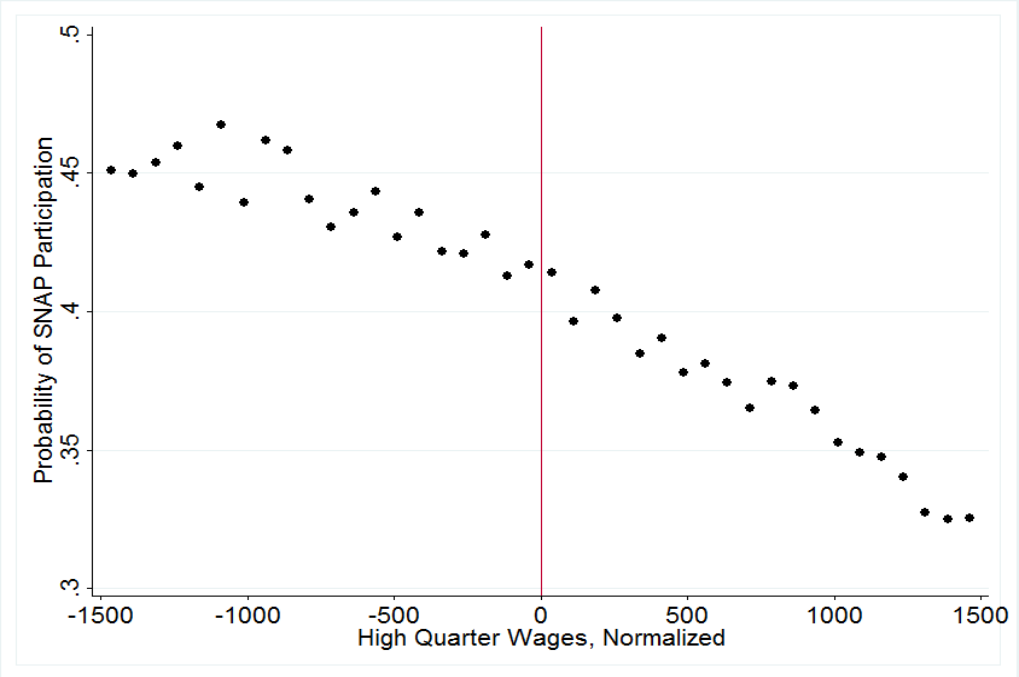
Notes: This figure plots fuzzy RD estimates of the effect of UI eligibility on the probability of being employed in a certain quarter relative to the claim quarter. Each RD estimate is obtained using the local linear regression specification in Equation (3), with a bandwidth of $700. The upper plot is restricted to the short potential duration subsample, and the bottom panel is restricted to the long potential duration subsample. See text for details. The dash-dot lines denotes 95% confidence intervals, and the vertical dashed line indicates UI exhaustion.

Figure 6: TANF Participation in the First Quarter After Layoff



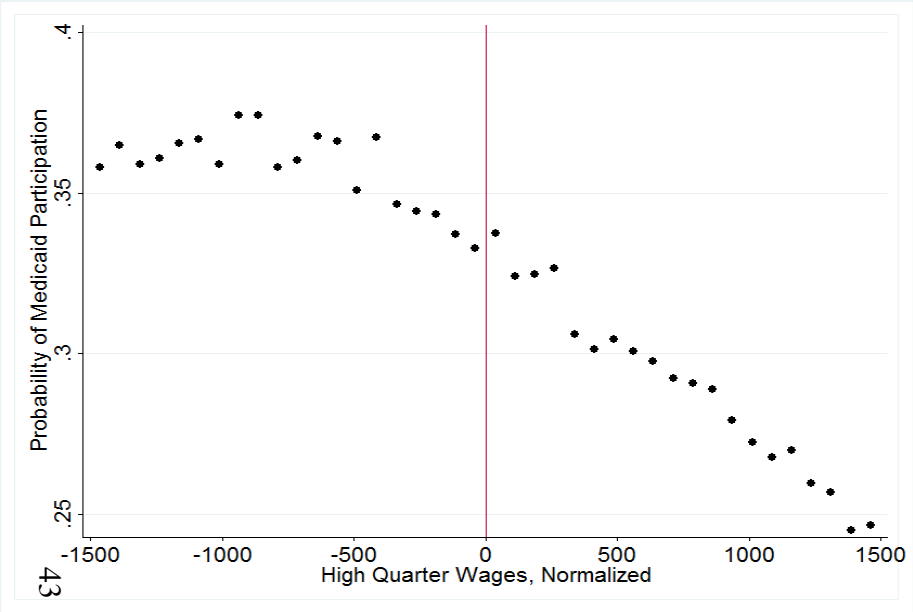
Notes: This figure plots the fraction of UI claimants who received TANF benefits in the first quarter after the UI claim for each high quarter earnings category. The vertical line denotes the minimum earnings threshold.

Figure 7: SNAP Participation in the First Quarter After Layoff



Notes: This figure plots the fraction of UI claimants who received SNAP benefits in the first quarter after the UI claim for each high quarter earnings category. The vertical line denotes the minimum earnings threshold.

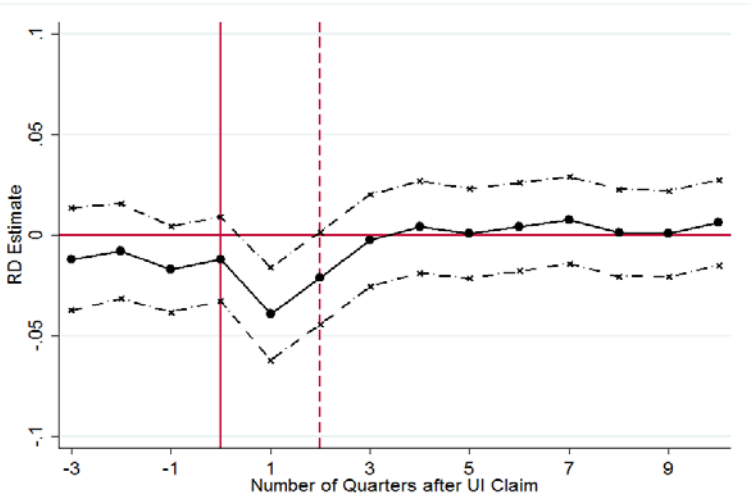
Figure 8: Medicaid Participation in the First Quarter After Layoff



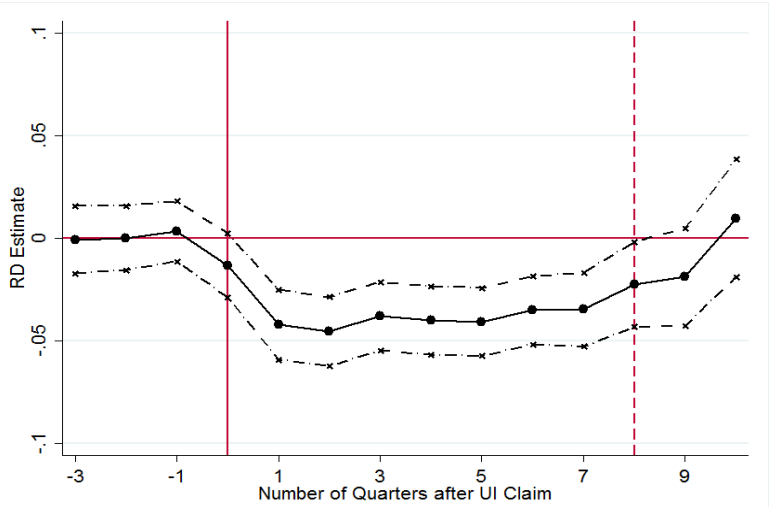
Notes: This figure plots the fraction of UI claimants who were enrolled in Medicaid in the first quarter after the UI claim for each high quarter earnings category. The vertical line denotes the minimum earnings threshold.

Figure 9: RD Estimates of TANF Participation Effects Over Time

A. Short Potential Duration

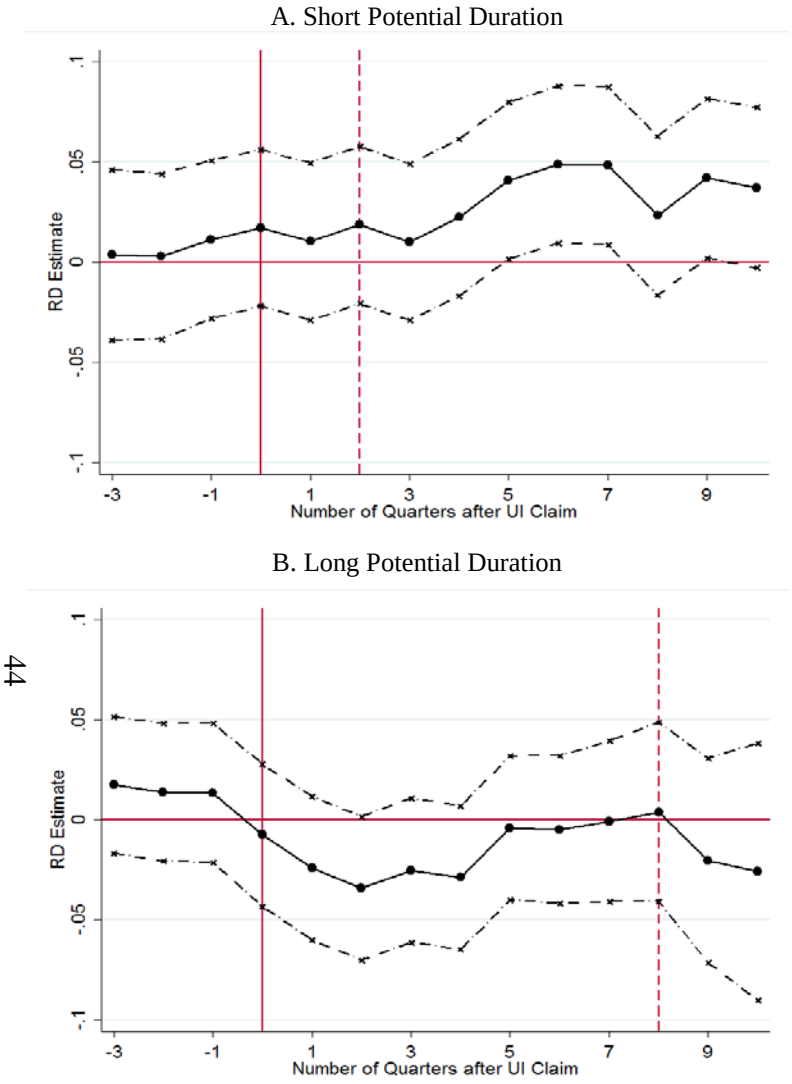


B. Long Potential Duration



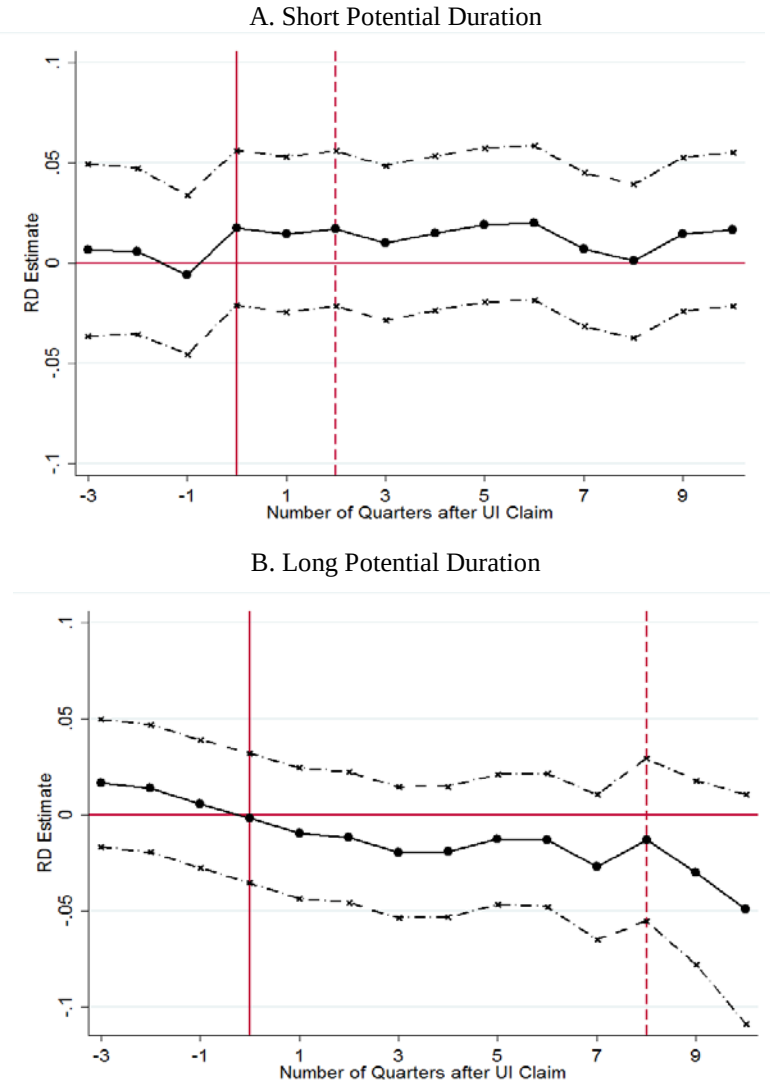
Notes: This figure plots fuzzy RD estimates of the effect of UI eligibility on probability of TANF benefit receipt in a certain quarter relative to the claim quarter. Each RD estimate is obtained using the local linear regression specification in Equation (3), with a bandwidth of $700. The upper plot is restricted to the short potential duration subsample, and the bottom panel is restricted to the long potential duration subsample. See text for details. The dash-dot lines denotes 95% confidence intervals, and the vertical dashed line indicates UI exhaustion.

Figure 10: RD Estimates of SNAP Participation Effects Over Time



Notes: This figure plots fuzzy RD estimates of the effect of UI eligibility on probability of SNAP benefit receipt in a certain quarter relative to the claim quarter. Each RD estimate is obtained using the local linear regression specification in Equation (3), with a bandwidth of $700. The upper plot is restricted to the short potential duration subsample, and the bottom panel is restricted to the long potential duration subsample. See text for details. The dash-dot lines denotes 95% confidence intervals, and the vertical dashed line indicates UI exhaustion.

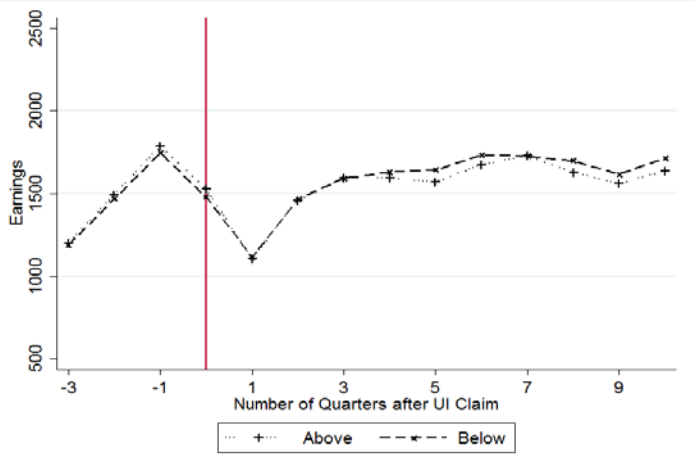
Figure 11: RD Estimates of Medicaid Participation Effects Over Time



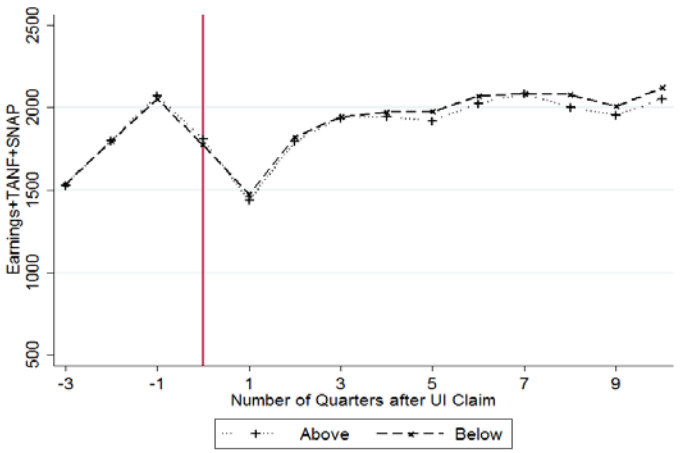
Notes: This figure plots fuzzy RD estimates of the effect of UI eligibility on probability of Medicaid participation in a certain quarter relative to the claim quarter. Each RD estimate is obtained using the local linear regression specification in Equation (3), with a bandwidth of $700. The upper plot is restricted to the short potential duration subsample, and the bottom panel is restricted to the long potential duration subsample. See text for details. The dash-dot lines denotes 95% confidence intervals, and the vertical dashed line indicates UI exhaustion.

Figure 12: Income Over Time (Short Potential UI Duration)

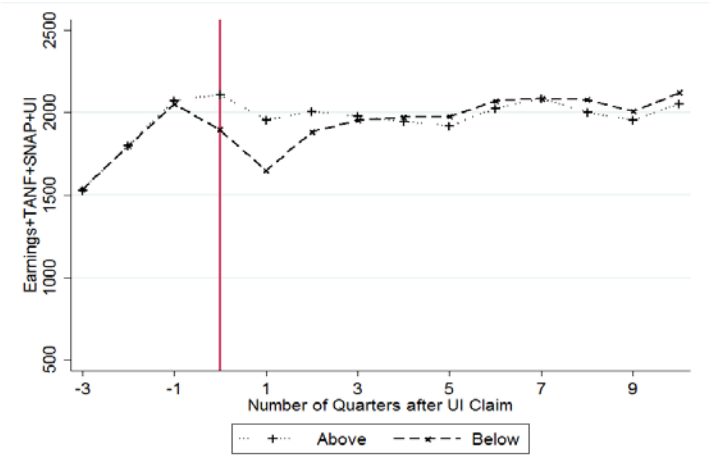
A. Earnings Only



B. Earnings, TANF, and SNAP Benefits



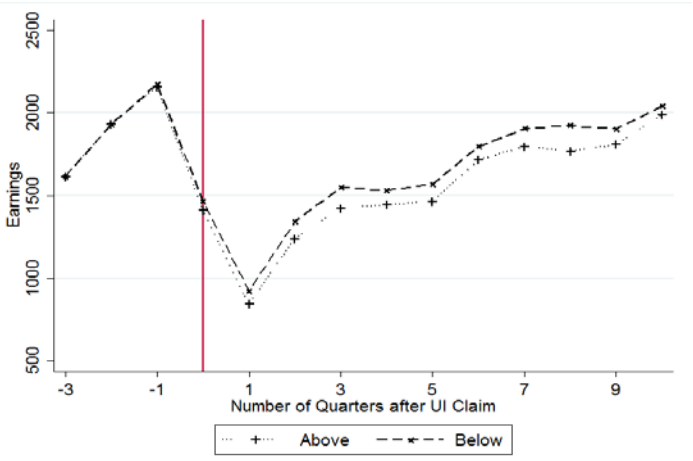
C. Earnings, TANF, SNAP, and UI Benefits



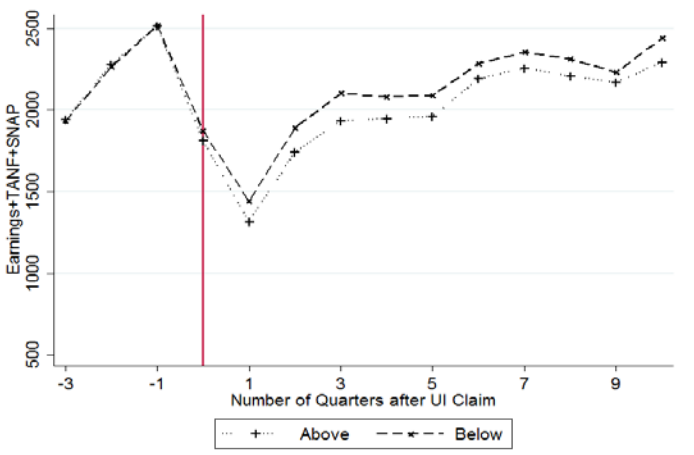
Notes: The graphs in this figure show point estimates of β_0 and $\beta_0+\tau_y$ from Equation (1), where Y_i are different measures of income in each quarter. Each local linear regression uses a bandwidth of $700. In Panel A, Y_i include earnings; in Panel B, TANF and the cash-equivalent of SNAP benefits are added; and in Panel C, UI benefits are added.

Figure 13: Income Over Time (Long Potential UI Duration)

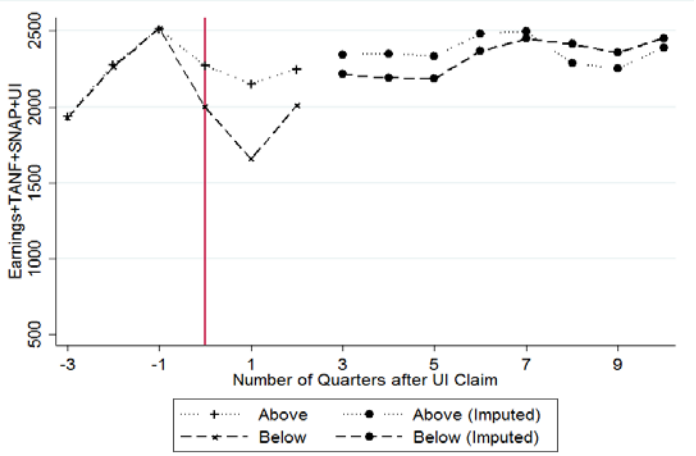
A. Earnings Only



B. Earnings, TANF, and SNAP Benefits



C. Earnings, TANF, SNAP, and UI Benefits



Notes: The graphs in this figure show point estimates of β_0 and $\beta_0+\tau_y$ from Equation (1), where Y_i are different measures of income in each quarter. Each local linear regression uses a bandwidth of $700. In Panel A, Y_i include earnings; in Panel B, TANF and the cash-equivalent of SNAP benefits are added; and in Panel C, UI benefits are added.

Table 1: Descriptive Statistics

	All (1)	RD Sample (2)
Demographic Characteristics		
Female	40.7%	57.4%
Age	39.1	33.1
Education		
Less Than HS	11.0%	18.5%
High School	49.7%	50.3%
Some College	25.6%	24.2%
Bachelor Degree	7.9%	3.7%
Adv. Degree	5.4%	2.6%
Race		
White	61.9%	50.9%
Black	13.1%	19.7%
Asian	1.1%	0.7%
Native American/Alaskan	1.1%	1.5%
Has Dependents	24.9%	25.6%
Employment and Program Participation Before Layoff		
Prev. Job Tenure (Months)	66.80	22.58
	[92.09]	[39.86]
Previous Industry		
Manufacturing	22.8%	5.2%
Retail	11.0%	22.2%
Support/Waste	10.0%	15.0%
Accomodations and Food Svcs.	7.2%	24.3%
Previous Year Earnings	36847.61	7696.36
[std. dev.]	[33316.47]	[4103.81]
Ever Claimed UI (since 2001)	27.3%	24.9%
Ever Received Benefits from…		
TANF	5.2%	15.2%
SNAP/Food Stamps	19.4%	45.6%
Medicaid	16.6%	41.6%
Employment and Program Participation After Layoff		
Num. of Jobless Quarters	2.06	2.66
[std. dev.]	[3.47]	[3.72]
UI Recipient	76.2%	43.9%
Received benefits one quarter after layoff		
TANF	2.1%	6.1%
SNAP/Food Stamps	17.4%	38.9%
Medicaid	12.6%	30.9%
Number of months on… (Over Next 2 Years)		
TANF	0.40	1.14
[std. dev.]	[2.30]	[3.88]
SNAP/Food Stamps	3.84	8.20
[std. dev.]	[7.34]	[9.38]
Medicaid	2.80	6.50
[std. dev.]	[6.69]	[9.21]
(Conditional on receipt) Quarterly income received from …		
TANF	961.09	970.51
[std. dev.]	[689.52]	[682.86]
SNAP/Food Stamps	785.14	847.91
[std. dev.]	[604.21]	[597.37]
N	2,001,462	286,113

Notes: Data are administrative records from Michigan, 2005-2010. The sample for column (1) consists of all "new" UI claims (i.e., who have not claimed UI for the past two years). Column (2) contains only those within $1500 of the eligibility threshold. Standard deviations are in brackets. All monetary amounts are in 2012 dollars.

Table 2: Local Linear Estimates of Discontinuities in Pre-determined Covariates

	Bandwidth				
	$1,500	$1,000	$700	$500	$300
	(1)	(2)	(3)	(4)	(5)
Predicted Prob.	0.002 ***	0.001	0.001	0.000	0.000
of Employment in 1st Qtr	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Predicted Prob. Of Means-	-0.001	0.001	0.002	0.003	0.004
Tested Program in 1st Qtr	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Female	-0.002	0.003	0.006	0.006	0.009
	(0.004)	(0.004)	(0.005)	(0.006)	(0.008)
Manufacturing	-0.004 **	-0.001	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Retail	0.003	0.000	-0.001	0.000	-0.005
	(0.003)	(0.004)	(0.005)	(0.005)	(0.007)
Accomodation	0.002	0.004	-0.001	-0.003	0.009
and Food Services	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
Less than HS	-0.001	-0.003	-0.001	0.003	0.010
	(0.003)	(0.004)	(0.004)	(0.005)	(0.006)
White	-0.003	-0.006	-0.006	-0.005	-0.008
	(0.004)	(0.005)	(0.005)	(0.006)	(0.008)
Age	0.068	0.060	0.059	0.108	0.091
	(0.104)	(0.126)	(0.149)	(0.175)	(0.226)
Prev. Job Tenure (Months)	0.397	0.775 **	0.650	0.797	0.981
	(0.294)	(0.354)	(0.415)	(0.490)	(0.625)
Has Received Means-Tested	0.002	0.005	0.011 **	0.013 **	0.007
Program Benefits in Past	(0.004)	(0.005)	(0.005)	(0.006)	(0.008)

Notes: Each table entry corresponds to an estimate of τ_y of Equation (1) from separate regressions, using bandwidths of $1500, $1000, $700, $500, and $300. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Table 3: Local Linear Estimates of First Stage and Main Outcome Discontinuities: By UI Duration

		By UI Potential Duration	
	All	Short	Long
	(1)	(2)	(3)
A. First Stage			
UI Eligible	0.449 ***	0.403 ***	0.484 ***
	(0.004)	(0.007)	(0.007)
Value Right Below Thr.	0.102	0.109	0.100
B. Fuzzy RD Estimates: Employment			
Initial Nonemployment Duration (Qtrs)	0.514 ***	0.322 **	0.682 ***
	(0.090)	(0.161)	(0.140)
Value Right Below Thr.	2.566	2.529	2.753
Percent Impact	20%	13%	25%
Earnings (Over 2.5 Years)	-834.82 ***	-444.13	-1224.66 ***
	(272.14)	(532.82)	(378.65)
Value Right Below Thr.	8950.78	9179.03	8715.79
Percent Impact	-9%	-5%	-14%
C.Fuzzy RD Estimates: Program Participation (Over Next 2 Years)			
Dollars of TANF Benefits	-229.74 ***	-42.29	-345.76 ***
	(46.72)	(95.82)	(62.58)
Value Right Below Thr.	571.75	705.91	481.14
Percent Impact	-40%	-6%	-72%
Dollars of SNAP Benefits	-114.91	-59.49	-210.68
	(91.64)	(143.83)	(157.23)
Value Right Below Thr.	2679.88	2126.07	3385.95
Percent Impact	-4%	-3%	-6%
Months of Medicaid Benefits	-0.048	0.281	-0.430
	(0.227)	(0.422)	(0.347)
Value Right Below Thr.	7.023	7.339	7.275
Percent Impact	-1%	4%	-6%
N	137,708	55,435	50,250

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). All regressions use a bandwidth of $700. See text for descriptions of the subsamples for columns (2) and (3). Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Table 4: Fuzzy RD Estimates of UI Eligibility on Employment and Earnings Over Time

Panel A: Short Potential Duration Subsample (N=55,435)

	Prob. Employed			Earnings		
Qtrs After Claim	*Value at Thr.* (1)	*RD Est.* (2)		*Value at Thr.* (3)	*RD Est.* (4)	
1	0.528	-0.049	**	1113.93	-13.92	
		(0.021)			(103.95)	
2	0.554	-0.031		1467.53	-21.60	
		(0.021)			(110.80)	
3	0.560	-0.016		1593.83	-0.25	
		(0.021)			(107.03)	
4	0.555	-0.032		1627.84	-81.92	
		(0.021)			(117.81)	
5	0.525	-0.024		1642.01	-177.98	
		(0.021)			(132.03)	
6	0.529	-0.040	*	1733.91	-148.46	
		(0.021)			(127.89)	
7	0.524	-0.034		1726.23	6.63	
		(0.021)			(125.83)	
8	0.504	-0.041	*	1698.03	-176.48	
		(0.021)			(120.32)	
9	0.486	-0.050	**	1617.48	-143.80	
		(0.021)			(118.82)	
10	0.487	-0.038	*	1714.57	-189.14	
		(0.021)			(128.10)	

Panel B: Long Potential Duration Subsample (N=50,250)

	Prob. Employed			Earnings		
Qtrs After Claim	*Value at Thr.* (1)	*RD Est.* (2)		*Value at Thr.* (3)	*RD Est.* (4)	
1	0.472	-0.063	***	923.21	-169.48	***
		(0.018)			(55.74)	
2	0.516	-0.077	***	1343.81	-218.67	***
		(0.018)			(71.08)	
3	0.534	-0.074	***	1549.04	-261.55	***
		(0.018)			(79.89)	
4	0.536	-0.082	***	1533.10	-185.67	**
		(0.018)			(81.09)	
5	0.518	-0.064	***	1569.51	-220.32	***
		(0.018)			(84.88)	
6	0.541	-0.052	***	1797.13	-168.96	*
		(0.018)			(95.38)	
7	0.552	-0.069	***	1905.80	-222.53	**
		(0.018)			(97.56)	
8	0.551	-0.070	***	1923.44	-317.87	***
		(0.018)			(100.28)	
9	0.538	-0.059	***	1903.79	-197.81	*
		(0.019)			(103.86)	
10	0.547	-0.046	**	2043.07	-117.50	
		(0.020)			(118.10)	

Notes: RD Est. are estimates of τ in Equation (3) and Value at Thr. is an estimate of β_0 in Equation (1), all from separate regressions (bandwidth \$700). The outcome variable is an indicator for any positive earnings (cols. (1)-(2)) or earnings (cols. (3)-(4)). See text for details on subsamples in each panel. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Table 5: Fuzzy RD Estimates of UI Eligibility on Means-Tested Program Use Over Time

Panel A: Short Potential Duration Subsample (N=55,435)

	Probability of Receiving…								Income From…					
	TANF		SNAP		Medicaid		TANF		SNAP			
Qtrs After Claim	*Value at Thr.* (1)	*RD Est.* (2)		*Value at Thr.* (3)	*RD Est.* (4)		*Value at Thr.* (5)	*RD Est.* (6)	*Value at Thr.* (7)	*RD Est.* (8)		*Value at Thr.* (9)	*RD Est.* (10)	
1	0.093	-0.039	***	0.334	0.010		0.319	0.015	100.18	-50.26	***	263.01	-34.70	*
		(0.012)			(0.020)			(0.020)		(14.86)			(19.68)	
2	0.089	-0.021	*	0.330	0.019		0.316	0.017	98.23	-34.34	**	258.46	-20.18	
		(0.012)			(0.020)			(0.020)		(15.02)			(19.69)	
3	0.087	-0.003		0.326	0.010		0.316	0.010	93.66	-2.62		258.15	-16.25	
		(0.012)			(0.020)			(0.020)		(15.17)			(19.67)	
4	0.085	0.004		0.326	0.022		0.312	0.015	89.06	15.50		256.30	-4.28	
		(0.012)			(0.020)			(0.020)		(15.05)			(19.74)	
5	0.080	0.001		0.319	0.041	**	0.308	0.019	83.38	19.76		251.27	15.78	
		(0.011)			(0.020)			(0.020)		(15.00)			(19.87)	
6	0.075	0.004		0.321	0.049	**	0.306	0.020	80.12	10.56		259.09	17.87	
		(0.011)			(0.020)			(0.020)		(14.45)			(20.67)	
7	0.072	0.007		0.324	0.048	**	0.307	0.007	81.18	-0.53		278.57	-3.55	
		(0.011)			(0.020)			(0.020)		(14.57)			(21.65)	
8	0.074	0.001		0.340	0.023		0.310	0.001	80.09	-0.36		301.23	-14.20	
		(0.011)			(0.020)			(0.020)		(14.37)			(22.79)	
9	0.073	0.001		0.344	0.042	**	0.306	0.015	79.68	1.74		312.70	1.59	
		(0.011)			(0.020)			(0.020)		(14.50)			(23.49)	
10	0.071	0.006		0.355	0.037	*	0.303	0.017	76.53	10.53		329.61	9.05	
		(0.011)			(0.020)			(0.020)		(14.18)			(24.47)	

Panel B: Long Potential Duration Subsample (N=50,250)

	Probability of Receiving…								Income From…					
	TANF		SNAP		Medicaid		TANF		SNAP			
Qtrs After Claim	*Value at Thr.* (1)	*RD Est.* (2)		*Value at Thr.* (3)	*RD Est.* (4)		*Value at Thr.* (5)	*RD Est.* (6)	*Value at Thr.* (7)	*RD Est.* (8)		*Value at Thr.* (9)	*RD Est.* (10)	
1	0.067	-0.042	***	0.470	-0.024		0.334	-0.010	68.19	-52.99	***	452.09	-44.29	*
		(0.009)			(0.018)			(0.017)		(10.61)			(23.57)	
2	0.068	-0.045	***	0.484	-0.034	*	0.339	-0.012	68.12	-45.99	***	479.07	-49.31	**
		(0.009)			(0.018)			(0.017)		(10.81)			(24.24)	
3	0.064	-0.038	***	0.485	-0.025		0.342	-0.020	70.29	-50.22	***	482.78	-41.30	*
		(0.009)			(0.018)			(0.017)		(10.95)			(24.35)	
4	0.065	-0.040	***	0.491	-0.029		0.342	-0.019	69.31	-51.45	***	478.10	-40.63	*
		(0.009)			(0.018)			(0.017)		(10.61)			(24.23)	
5	0.065	-0.041	***	0.479	-0.004		0.338	-0.013	64.06	-45.36	***	455.29	-4.26	
		(0.008)			(0.018)			(0.017)		(10.19)			(23.75)	
6	0.059	-0.035	***	0.475	-0.005		0.330	-0.013	58.35	-43.37	***	437.27	-1.23	
		(0.008)			(0.019)			(0.018)		(10.02)			(23.70)	
7	0.060	-0.035	***	0.473	-0.001		0.336	-0.027	60.03	-43.25	***	431.46	-1.33	
		(0.009)			(0.020)			(0.019)		(10.92)			(25.34)	
8	0.061	-0.022	**	0.481	0.004		0.338	-0.013	57.38	-30.36	**	423.76	15.91	
		(0.011)			(0.023)			(0.022)		(12.00)			(27.79)	
9	0.058	-0.019		0.476	-0.020		0.332	-0.030	52.21	-16.20		403.00	-8.79	
		(0.012)			(0.026)			(0.024)		(13.48)			(30.50)	
10	0.044	0.010		0.468	-0.026		0.323	-0.049	38.53	5.54		371.60	-20.28	
		(0.015)			(0.033)			(0.030)		(15.60)			(36.07)	

Notes: RD Est. are estimates of τ in Equation (3) and Value at Thr. is an estimate of β_0 in Equation (1), all from separate regressions (bandwidth $700). The outcomes are indicators for participating in (cols. (1)-(6)) or the amount received from a certain program (cols. (7)-(10)). See text for details on subsamples in each panel. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

APPENDIX

Imputing UI Income

In this section, we describe the rationale and method for imputing UI income for the exercise in the final part Section 5 of the main text.

First, although we are able to observe when each UI spell began, the weekly benefit amount, and the total amount of regular UI benefits received over a one year period, we do not observe the exact timing of UI payments. Therefore, to calculate the amount of UI received per quarter, we assume that workers are eligible for the full amount of UI benefits each week with no gaps in payments beginning with the claim date (i.e., 13 weeks times the weekly benefit amount per quarter), up to the observed total benefits received.

Second, although we observe for each UI claimant the total amount of *regular* benefits received, we do not observe the total amount of UI benefits (i.e., including benefits from EB or EUC programs) when an extended benefit period is in effect. Specifically, even though the data contains a variable that should be the total weeks of extended benefits received, since each claim is only valid for one year, we do not observe workers receiving more than 52 weeks of benefits in total, even when we know that claimants could have collected up to 99 weeks of benefits.[1] This would not be a problem for the short duration subsample since no extended benefits were available. For the long

[1] Furthermore, even if the duration is less than 52 weeks, we believe the extended benefits variable is understated for unknown reasons: For example, for claims made between October 2008 and August 2009, when federal EUC benefits were being rolled in, we observe that most claimants only received six weeks of extended UI, even though they would have been eligible for at least the first tier of EUC, which extends benefits by 13 weeks.

duration subsample, however, the observed UI received after the second quarter (when regular benefits are exhausted) would be greatly understated. For illustrative purposes, we impute the UI income in the third and later quarters for the long duration sample using a method described below. In both imputed and nonimputed measures of UI income, we do not incorporate payments from future UI claims. Since this is likely to understate UI income in later quarters, this is yet another reason to focus on the quarters immediately following layoff.

To obtain an approximate measure of UI income beyond the second quarter in the long duration subsample, we use the fact that workers who are not employed (i.e., with no earnings) are likely to be collecting extended benefits. Specifically, we calculate UI income using the following method. We first approximate each worker's potential duration of benefits by using the observed regular potential duration and multiplying it by $\frac{99}{26}$. This is because the extended potential duration for any individual claimant is proportional to their regular potential duration. For example, a worker who has a regular potential duration of 26 weeks may receive up to 99 weeks of benefits, while a worker with 20 weeks of regular potential duration may receive up to $20 \cdot \frac{99}{26} = 76$ weeks of benefits. In each quarter without earnings, we assume that the worker receives the total possible quarterly UI payments (i.e., the weekly benefit amount multiplied by 13 weeks) until they exhaust their benefits. Finally, since it is also possible that some workers with no earnings have exited the labor force (to pursue further education, to retire, or to collect disability insurance, for example), we further adjust UI receipt downward by a factor of 0.764, based on the finding in Krueger et al. (2014) that 33.7 percent of workers who are unemployed for over 27 weeks exit the labor force 15 months later.

Optimal UI Eligibility: Framework

In this section, we present a simple model that can be used to assess the welfare benefits of decreasing the UI eligibility threshold. It is similar in spirit to the models of optimal unemployment insurance, e.g., Baily (1978) and Chetty (2008), wherein the social planner balances the consumption smoothing benefits of providing UI with the cost of reduced job search effort.

Suppose that the utility of the worker is given by a state independent utility function $u(c)$, where c is their consumption level, and $u(\cdot)$ is an increasing, concave function. Workers are all initially unemployed, and some workers are eligible for UI while others are not. Suppose that the distribution of worker's previous earnings are given by p.d.f. $f(\cdot)$, and that the probability of being eligible for UI differs over an earnings threshold, G. Specifically, the probabilities of eligibility are P_A and P_B above and below the threshold, respectively. Workers who are eligible for UI consume c^u_{UI} when unemployed, and those who are not consume c^u_{-UI}. The consumption of the unemployed will consist not only of UI payments, but benefits from all other social assistance programs as well. When workers are reemployed, they receive a wage w and pay a lump sum tax τ, so that they consume $c^e = w - \tau$.

Workers allocate their one unit of time between (re)employment and unemployment, and incur a cost of working that is given by a strictly convex cost function $\psi(\cdot)$. An unemployed worker who is eligible for UI allocates $s_{UI} \in (0,1)$ to employment and spends $1 - s_{UI}$ of time unemployed. Therefore, this worker's utility is given by

$$\max_{s_{UI}} (1 - s_{UI}) u(c^u_{UI}) + s_{UI} u(c^e) - \psi(s_{UI})$$

Similarly, the worker who is ineligible for UI spends s_{-UI} and $1 - s_{-UI}$ of the period

employed and unemployed, respectively. This worker's utility is

$$\max_{s_{-UI}}(1-s_{-UI})u(c^u_{-UI})+s_{-UI}u(c^e)-\psi(s_{-UI})$$

Since UI-ineligible workers consume less while unemployed, their optimal job search intensity will be higher than the eligible ($s_{UI} < s_{-UI}$). Note that we assume neither the consumption while employed nor unemployed depend on the value of the previous earnings, except to determine whether or not the worker is eligible for UI, which is true within a neighborhood of the UI threshold.

Let $P_{UI}(G) \equiv F(G)P_B + [1-F(G)]P_A$ be the proportion of workers who are eligible for UI. The utilitarian planner's objective is to maximize expected utility by choosing the eligibility threshold G such that benefits from all social programs equal the tax collected:

$$\begin{aligned}\max_G W(G) \quad = \quad & [1-P_{UI}(G)]\left[(1-s_{-UI})u(c^u_{-UI})+s_{-UI}u(c^e)-\psi(s_{-UI})\right] \\ & +P_{UI}(G)\left[(1-s_{UI})u(c^u_{UI})+s_{UI}u(c^e)-\psi(s_{UI})\right]\end{aligned}$$

$$\begin{aligned}\text{s.t.} \qquad & [1-P_{UI}(G)](1-s_{-UI})b_{-UI}+P_{UI}(G)(1-s_{UI})b_{UI} \\ & =\tau\left([1-P_{UI}(G)]s_{-UI}+P_{UI}(G)s_{UI}\right)\end{aligned}$$

where b_{UI} are benefits paid by the government while the worker is eligible for UI, and b_{-UI} are benefits paid while the worker is not.

We will assume that workers do not change pre-layoff employment behavior in response to changes in the eligibility threshold. Our data support this assumption, in that we do not observe bunching near the UI threshold. The marginal welfare increase of

lowering the eligibility threshold is thus given by

$$\begin{aligned} -\frac{dW}{dG} &= f(G)\left[P_A - P_B\right] \cdot \\ &\quad \left[(1-s_{UI})u(c^u_{UI}) + s_{UI}u(c^e) - \psi(s_{UI}) - (1-s_{-UI})u(c^u_{-UI}) - s_{-UI}u(c^e) + \psi(s_{-UI})\right] \\ &\quad - \left\{\left[1 - P_{UI}(G)\right]s_{-UI} + P_{UI}(G)s_{UI}\right\}u'(c^e)\frac{d\tau}{dG} \end{aligned} \tag{1}$$

The first term of Equation (1) reflects the benefits of giving UI eligibility to the marginal worker at the UI threshold, while the second term reflects tax costs. Using the workers' optimality conditions and the mean value theorem, it can be shown that the first term is greater than $f(G)\left[P_A - P_B\right](1-s_{-UI})\left[u(c^u_{UI}) - u(c^u_{-UI})\right]$.[2]

Note that the government budget constraint can be written as

$$\tau = \frac{\left[1 - P_{UI}(G)\right](1-s_{-UI})b_{-UI} + P_{UI}(G)(1-s_{UI})b_{UI}}{\left[1 - P_{UI}(G)\right]s_{-UI} + P_{UI}(G)s_{UI}}$$

which yields

$$\begin{aligned} \frac{d\tau}{dG} &= f(G)\left[P_A - P_B\right]\frac{(1-s_{-UI})b_{-UI} - (1-s_{UI})b_{UI}}{\left[1 - P_{UI}(G)\right]s_{-UI} + P_{UI}(G)s_{UI}} \\ &\quad - f(G)\left[P_A - P_B\right]\left\{s_{-UI} - s_{UI}\right\}\frac{\tau}{\left[1 - P_{UI}(G)\right]s_{-UI} + P_{UI}(G)s_{UI}} \end{aligned}$$

Plugging this expression back into (1) yields

$$\begin{aligned} -\frac{dW}{dG} &> f(G)\left[P_A - P_B\right]\left\{(1-s_{-UI})\left[u(c^u_{UI}) - u(c^u_{-UI})\right]\right. \\ &\quad \left. -u'(c^e)\left[(1-s_{UI})b_{UI} - (1-s_{-UI})b_{-UI} + (s_{-UI} - s_{UI})\,\tau\right]\right\} \end{aligned} \tag{2}$$

We can use the right hand side of (2) to assess whether it will be welfare enhancing to increase or decrease the eligibility threshold. The first term within the braces reflects

[2] Worker optimality conditions are $u(c^e) - u(c^u_{UI}) = \psi'(s_{UI})$ and $u(c^e) - u(c^u_{-UI}) = \psi'(s_{-UI})$.

consumption smoothing benefits of lowering the threshold. The second term within the braces reflects the cost: Expanding eligibility means that more benefits must be paid out, and since workers spend a longer time unemployed, tax revenues also decrease. A positive value of the right-hand side indicates that UI eligibility has net positive benefits from consumption smoothing and that it will be optimal to lower the eligibility threshold. A negative value indicates that lowering the eligibility threshold *may* have net positive benefits due to shorter durations of unemployment, but the welfare effects would be ambiguous. Note that since $f(G)\left[P_A - P_B\right]$ is positive, we only need the sign of the term in braces.

Optimal UI Eligibility: Model Calibration

Now, we take our empirically estimated quantities to calibrate the above model. For this exercise, we use estimates from the long duration subsample, though the conclusions are generally the same for the short duration subsample. We assume that worker's utility is given by a CRRA utility function: $u(c) = \frac{c^{1-\gamma}}{1-\gamma}$, where $\gamma \in [1,4]$, as in Gruber (1997). For $F(G)$, we use the approximate proportion of "new" claimants below the UI threshold, 0.1. The probabilities of receiving UI above and below the threshold, P_A and P_B, are given by the estimated first stage intercepts from Equation (2): To the left of the threshold, the probability of eligibility is $\hat{\alpha}_0 = 0.10$, whereas the probability to the right of the threshold is $\hat{\alpha}_0 + \hat{\tau}_d = 0.58$.

The remaining empirical quantities (income, jobless durations, and benefit levels) need to be obtained for the UI eligible and ineligible. Since the RD design generates exogenous variation in UI eligibility at the minimum earnings threshold, we use intercept estimates of Equations (1), (2), and (3), where the dependent variables are measures of income, durations, and benefits. For each outcome, the quantities $\hat{\beta}_0 + \hat{\tau}_y + (1 - \hat{\alpha}_0 - \hat{\tau}_d)\hat{\tau}$ and $\hat{\beta}_0 - \hat{\alpha}_0\hat{\tau}$ correspond to the UI-eligible and ineligible, respectively. Note that

these are the upper and lower reduced form intercepts, $\hat{\beta}_0 + \hat{\tau}_y$ and $\hat{\beta}_0$, with an adjustment factor. The adjustment factors are necessary because not every claimant above the threshold receives UI and not every claimant below the threshold is a nonrecipient. The terms $(1 - \hat{\alpha}_0 - \hat{\tau}_d)\hat{\tau}$ and $-\hat{\alpha}_0\hat{\tau}$ mechanically "sharpen" the first stage, so that the difference is exactly the fuzzy RD estimate.

As a proxy for consumption while employed, c^e, we use total observed income (i.e., earnings, UI, and means-tested benefits) in the quarter right before layoff. As shown in Panel C of Figure 13, the estimated intercepts are quite similar for those who do and do not receive UI. In the calibration, we use an average of the two: $c^e = 2512.30$. For consumption while unemployed, c^u_{-UI} and c^u_{UI}, we assume that the only income workers get is transfer income from UI and means-tested benefit programs, and use the combined UI and means-tested program benefits in the quarter right after layoff. For UI eligibles, $c^u_{UI} = 1802.12$ and for ineligibles , $c^u_{-UI} = 616.24$. Since the government pays for both UI and means-tested programs, $b_{-UI} = c^u_{-UI}$ and $b_{UI} = c^u_{UI}$.

Finally, we calculate the time spent unemployed, $1 - s_{-UI}$ and $1 - s_{UI}$, using the estimated durations of initial nonemployment spells, as measured by the number of consecutive quarters without earnings. In the data, we find that the average duration for UI-ineligible workers is 2.68 quarters, whereas the average duration for eligible workers is 3.37 quarters. Since $1 - s_{UI}$ and $1 - s_{-UI}$ are expressed as proportions of time spent unemployed, we divide each of these by 10 quarters, which is where we truncate durations, so that $1 - s_{UI} = 0.34$ and $1 - s_{-UI} = 0.27$. The proportions of time spent employed, $s_{UI} = 0.66$ and $s_{-UI} = 0.73$ roughly translate to 21 months of employment, which is close to the observed preseparation job tenure among workers near the RD threshold (Table 1).

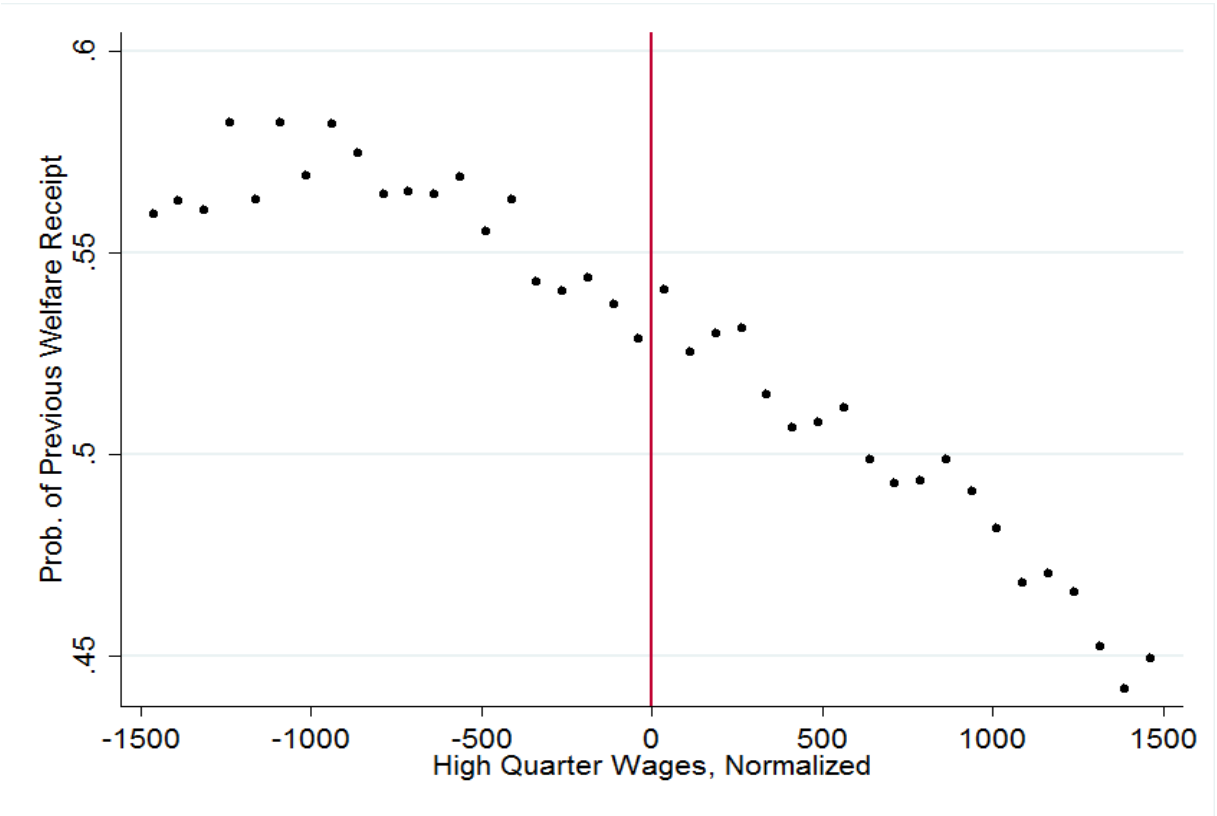
Plugging these empirical quantities into expression (2), we find that for $\gamma \in [1,4]$, the

term in braces is always positive. This implies that the consumption smoothing benefits from lowering the UI eligibility threshold exceeds the cost of increased benefits and foregone tax revenue.

Our measures of consumption may be understated if *household* income is what ultimately matters for consumption. Using individual income may overstate the drop in consumption during unemployment, which will overstate the consumption smoothing benefits of UI. While we do not observe households in the administrative data, we can nevertheless attempt to ballpark the consumption smoothing benefits of UI within households. To do this, we assume that households comprise of two workers, and that they both make the same amount when employed. If consumption is equal to the income of the two earners, means-tested benefits, and UI, consumption when employed is $c^e = 4672.74$. When one household member becomes unemployed, income drops to $c^u_{UI} = 3943.58$ or $c^u_{-UI} = 2795.65$. Social benefits remain the same at $b_{UI} = 1802.12$ and $b_{-UI} = 616.24$. This exercise assumes that spousal earnings do not respond when one member of the household becomes unemployed, which approximates the negligible to small effects found in the literature (e.g., Rothstein and Valletta (2014)); and that spousal earnings do not respond to UI eligibility, which is more contentious, as Cullen and Gruber (2000) find that wives have large labor supply response to husbands' UI benefits, but Kawano and LaLumia (2014) do not. When we plug these quantities into expression (2), we still find that for values of risk aversion $\gamma \geq 1.02$, lowering the eligibility threshold is welfare-enhancing.[3]

[3]For the short duration subsample, lowering the threshold is optimal for $\gamma = 1$ as well.

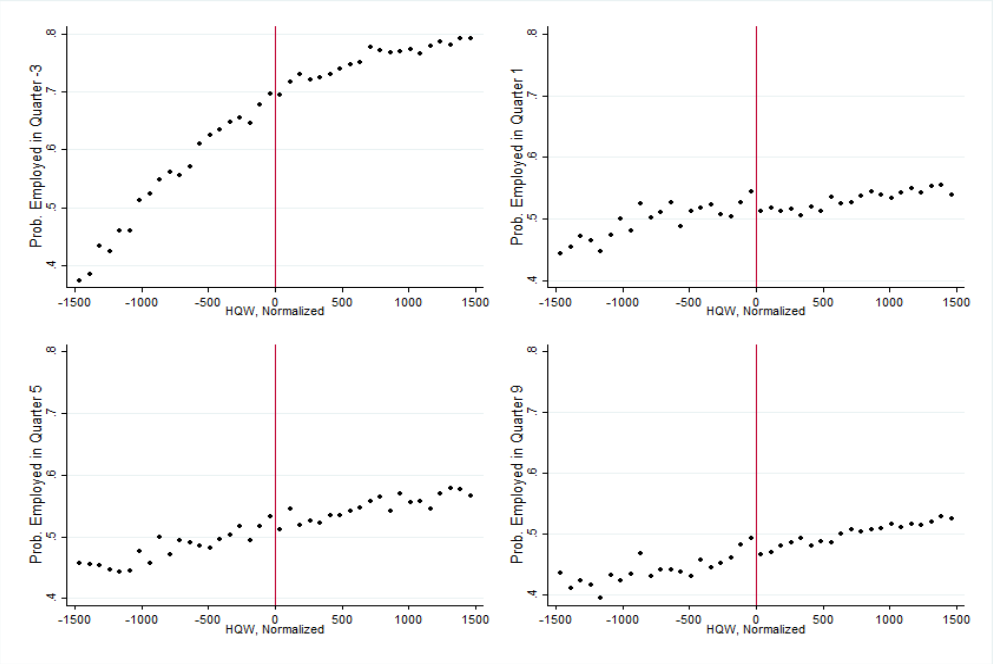
Appendix Figure 1: Probability of Previous TANF, SNAP, or Medicaid Receipt



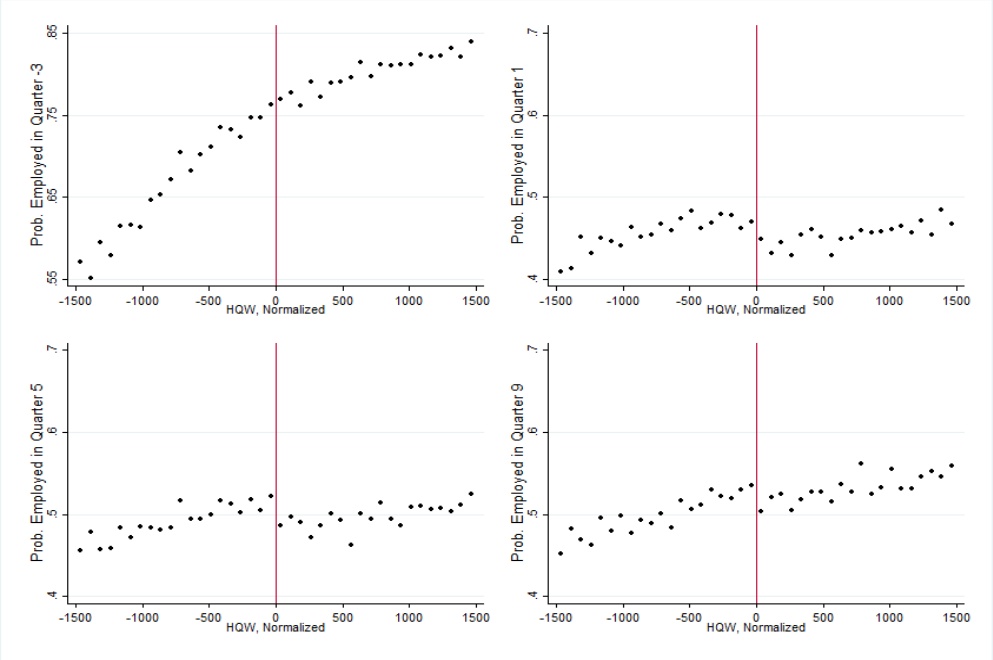
Notes: This figure plots the proportion of UI claimants who are observed to have received benefits from any of the three means-tested programs prior to claiming UI in each non-overlapping $75 interval of the (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

Appendix Figure 2: Employment in Various Quarters Relative to Claim Filing

A. Short Potential Duration



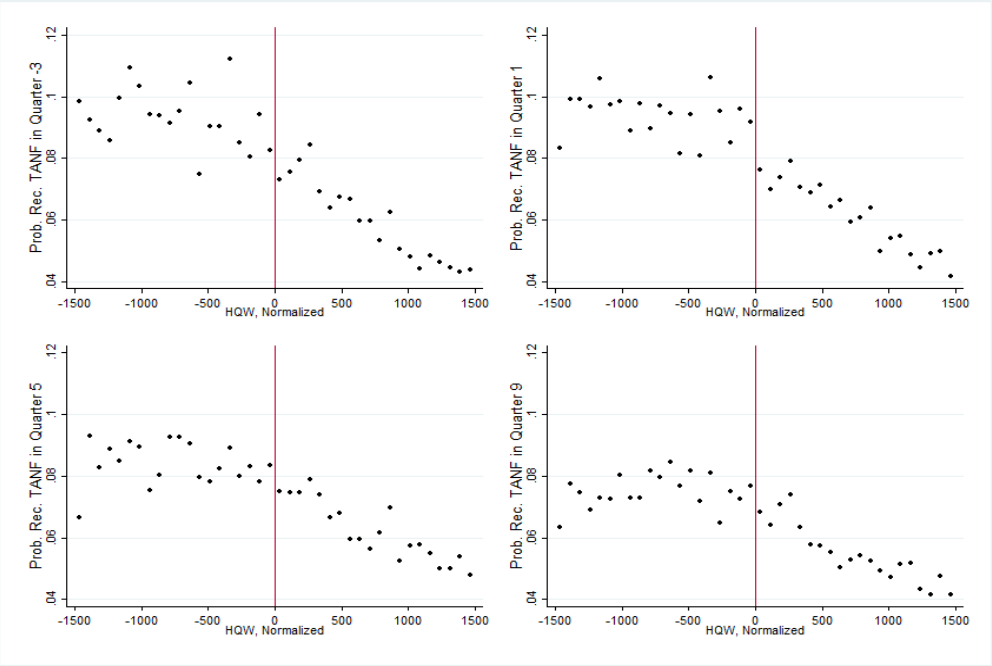
B. Long Potential Duration



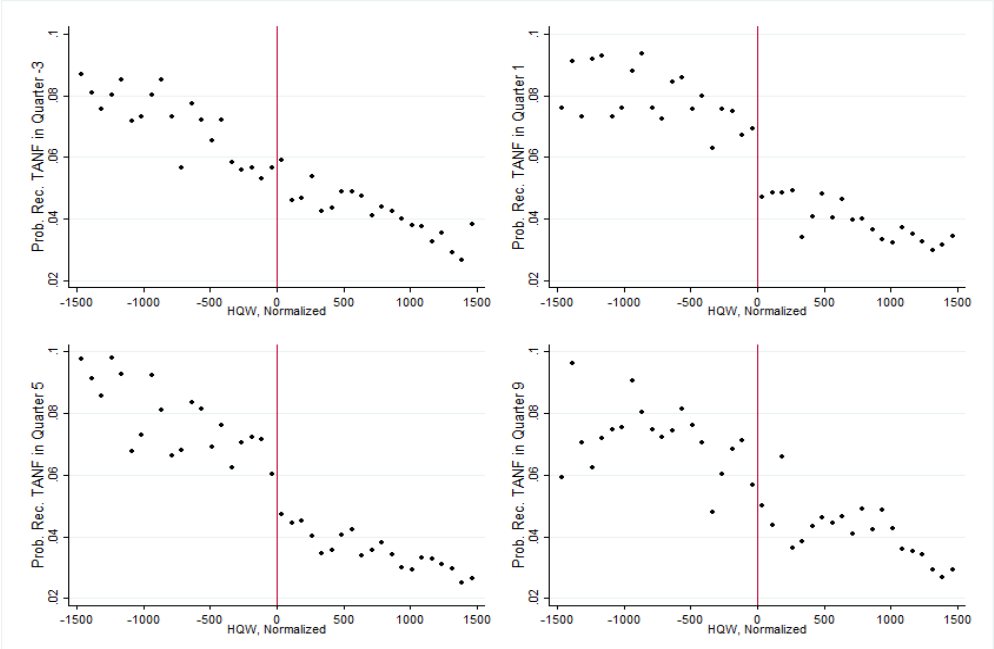
Notes: This figure plots the fraction of UI claimants who were employed during a certain quarter relative to the UI claim filing in each category of high quarter earnings. The vertical lines denote the minimum earnings threshold.

Appendix Figure 3: TANF Participation in Various Quarters Relative to Claim Filing

A. Short Potential Duration



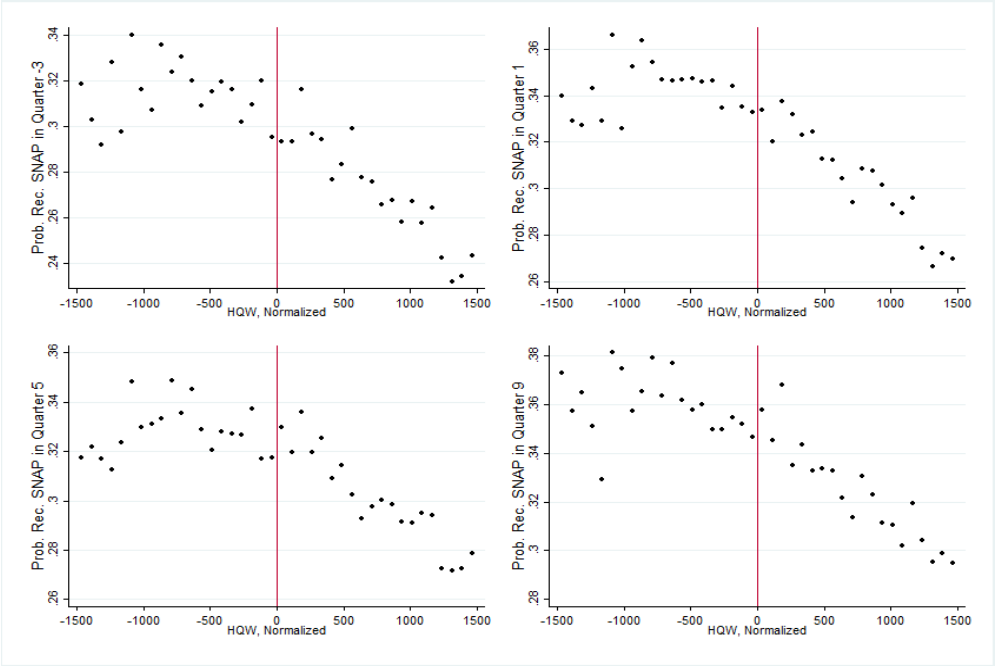
B. Long Potential Duration



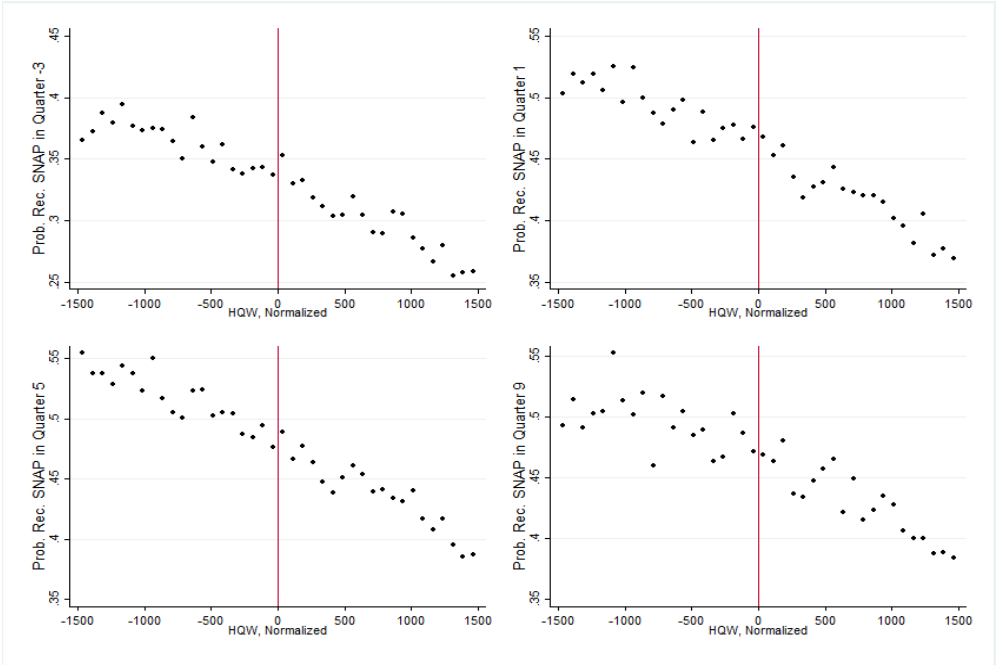
Notes: This figure plots the fraction of UI claimants who received TANF benefits during a certain quarter relative to the UI claim filing in each category of high quarter earnings. The vertical lines denote the minimum earnings threshold.

Appendix Figure 4: SNAP Participation in Various Quarters Relative to Claim Filing

A. Short Potential Duration



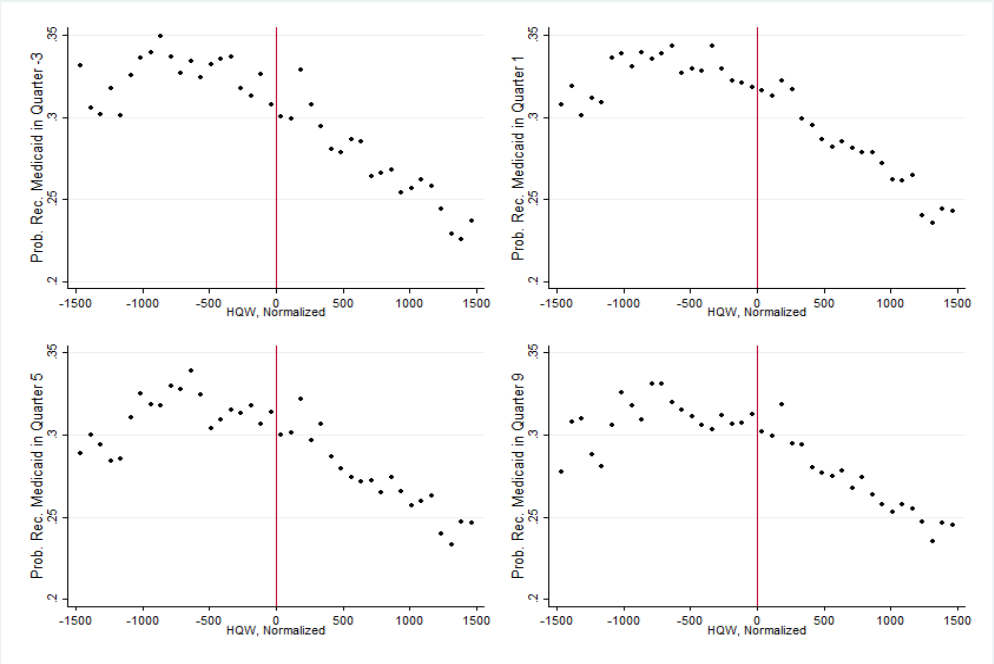
B. Long Potential Duration



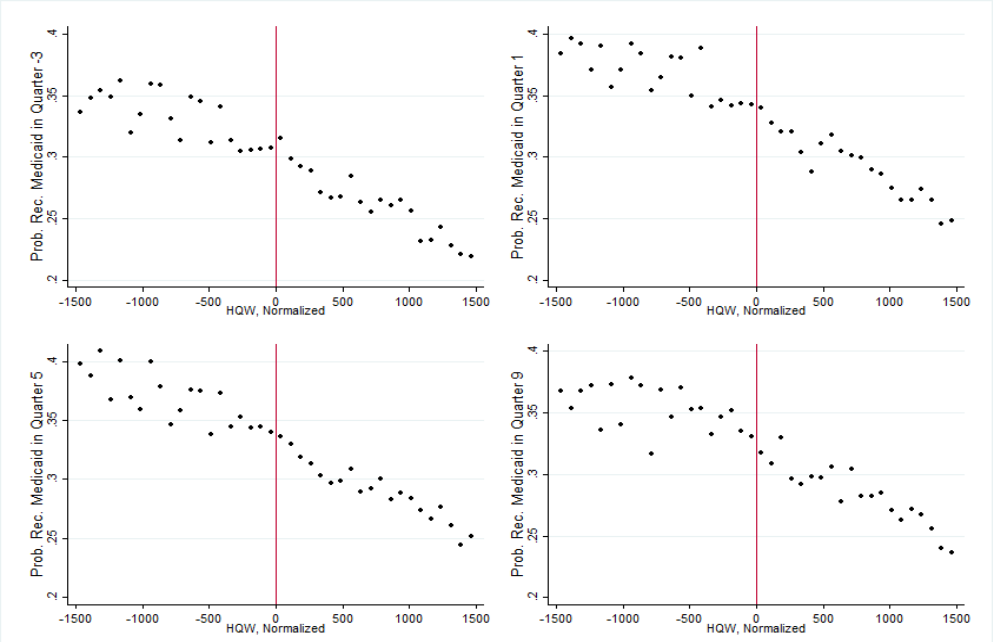
Notes: This figure plots the fraction of UI claimants who received SNAP benefits during a certain quarter relative to the UI claim filing in each category of high quarter earnings. The vertical lines denote the minimum earnings threshold.

Appendix Figure 5: Medicaid Participation in Various Quarters Relative to Claim Filing

A. Short Potential Duration



B. Long Potential Duration



Notes: This figure plots the fraction of UI claimants who were enrolled in Medicaid during a certain quarter relative to the UI claim filing in each category of high quarter earnings. The vertical lines denote the minimum earnings threshold.

Appendix Table 1: Local Linear Estimates of First Stage and Main Outcome Discontinuities: Varying Bandwidths

	Bandwidth					IK		CCT	
	$1,500	$1,000	$700	$500	$300	*bw*	*est.*	*bw*	*CI*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. First Stage									
UI Eligible	0.461 ***	0.448 ***	0.449 ***	0.443 ***	0.439 ***				
	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)				
Value Right Below Thr.	0.092	0.099	0.102	0.104	0.107				
B. Fuzzy RD Estimates: Employment									
Initial Nonemployment Duration (Qtrs)	0.563 ***	0.584 ***	0.514 ***	0.608 ***	0.549 ***	666	0.615 ***	1637	[0.367,0.719]
	(0.061)	(0.076)	(0.090)	(0.107)	(0.138)		(0.11)		
Value Right Below Thr.	2.554	2.559	2.566	2.536	2.532				
Earnings (Over 2.5 Years)	-903.18 ***	-983.13 ***	-834.82 ***	-916.41 ***	-766.69 *	778	-874.50 ***	1910	[-1462.01,-382.77]
	(188.02)	(230.68)	(272.14)	(324.29)	(417.96)		(251.37)		
Value Right Below Thr.	8894.06	8936.02	8950.78	9021.40	8986.93				
C.Fuzzy RD Estimates: Program Participation									
Dollars of TANF Benefits	-250.06 ***	-254.78 ***	-229.74 ***	-192.05 ***	-257.19 ***	1059	-256.11 ***	918	[-300.48,-52.54]
	(32.12)	(39.52)	(46.72)	(55.87)	(72.20)		(42.18)		
Value Right Below Thr.	579.19	581.18	571.75	558.09	572.28				
Dollars of SNAP Benefits	-142.36 **	-161.18 **	-114.91	-57.29	-226.58	936	-94.02	1132	[-344.24,83.46]
	(62.18)	(77.18)	(91.64)	(109.38)	(142.61)		(89.30)		
Value Right Below Thr.	2705.93	2678.05	2679.88	2679.47	2712.72				
Months of Medicaid Benefits	-0.413 ***	-0.211	-0.048	0.070	-0.187	1118	-0.142	899	[-0.459,0.739]
	(0.154)	(0.191)	(0.227)	(0.270)	(0.352)		(0.212)		
Value Right Below Thr.	7.179	7.052	7.023	7.033	7.084				
N	286,113	194,385	137,708	99,065	59,906				

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). Each column corresponds to a different bandwidth. Cols. (6)-(7) report the Imbens and Kalyanaraman (2012) bandwidth and corresponding estimate. Col. (8) reports the bandwidth using Calonico et al. (2015)'s bandwidth selector and col. (9) reports the bias-corrected confidence intervals. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Appendix Table 2: Local Linear Estimates of First Stage and Main Outcome Discontinuities: Varying Bandwidths, Short Duration Sample

	Bandwidth					IK		CCT	
	$1,500	$1,000	$700	$500	$300	*bw*	*est.*	*bw*	*CI*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. First Stage									
UI Eligible	0.421 ***	0.404 ***	0.403 ***	0.396 ***	0.391 ***				
	(0.005)	(0.006)	(0.007)	(0.008)	(0.011)				
Value Right Below Thr.	0.099	0.107	0.109	0.110	0.117				
B. Fuzzy RD Estimates: Employment									
Initial Nonemployment Duration (Qtrs)	0.326 ***	0.348 **	0.322 **	0.488 **	0.523 **	677	0.309 *	589	[-0.034,0.999]
	(0.109)	(0.136)	(0.161)	(0.193)	(0.250)		(0.160)		
Value Right Below Thr.	2.519	2.529	2.529	2.452	2.407				
Earnings (Over 2.5 Years)	-169.49	-308.12	-444.13	-690.07	-657.32	633	-255.44	618	[-2700.26,821.52]
	(361.00)	(449.31)	(532.82)	(632.19)	(823.56)		(418.35)		
Value Right Below Thr.	9145.91	9158.88	9179.03	9405.07	9342.37				
C.Fuzzy RD Estimates: Program Participation									
Dollars of TANF Benefits	-125.45 *	-112.39	-42.29	-44.04	-235.03	850	-97.41	1329	[-270.93,127.71]
	(64.30)	(80.60)	(95.82)	(114.94)	(149.83)		(76.41)		
Value Right Below Thr.	711.23	718.33	705.91	689.78	714.28				
Dollars of SNAP Benefits	-128.11	-76.17	-59.49	-81.33	-336.09	613	-36.38	601	[-611.93,314.65]
	(96.01)	(121.36)	(143.83)	(172.07)	(223.69)		(136.12)		
Value Right Below Thr.	2140.82	2116.59	2126.07	2122.80	2139.23				
Months of Medicaid Benefits	-0.516 *	0.038	0.281	0.247	-0.414	662	-0.516 *	571	[-1.719,1.058]
	(0.281)	(0.355)	(0.422)	(0.503)	(0.655)		(0.276)		
Value Right Below Thr.	7.594	7.366	7.339	7.364	7.358				
N	118,523	78,626	55,435	39,780	23,847				

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). Each column corresponds to a different bandwidth. Cols. (6)-(7) report the Imbens and Kalyanaraman (2012) bandwidth and corresponding estimate. Col. (8) reports the bandwidth using Calonico et al. (2015)'s bandwidth selector and col. (9) reports the bias-corrected confidence intervals. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Appendix Table 3: Local Linear Estimates of First Stage and Main Outcome Discontinuities: Varying Bandwidths, Long Duration Sample

	Bandwidth					IK		CCT	
	$1,500	$1,000	$700	$500	$300	*bw*	*est.*	*bw*	*CI*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. First Stage									
UI Eligible	0.497 ***	0.485 ***	0.484 ***	0.485 ***	0.476 ***				
	(0.005)	(0.006)	(0.007)	(0.008)	(0.011)				
Value Right Below Thr.	0.092	0.097	0.100	0.102	0.104				
B. Fuzzy RD Estimates: Employment									
Initial Nonemployment Duration (Qtrs)	0.745 ***	0.758 ***	0.682 ***	0.776 ***	0.658 ***	1057	0.730 ***	1905	[0.451,1.003]
	(0.095)	(0.118)	(0.140)	(0.164)	(0.214)		(0.130)		
Value Right Below Thr.	2.743	2.737	2.753	2.761	2.748				
Earnings (Over 2.5 Years)	-1378.56 ***	-1326.47 ***	-1224.66 ***	-1043.23 **	-930.05	3411	-944.82 **	1157	[-2413.46,-69.86]
	(262.38)	(321.35)	(378.65)	(453.57)	(582.16)		(395.73)		
Value Right Below Thr.	8604.98	8655.97	8715.79	8620.60	8757.75				
C.Fuzzy RD Estimates: Program Participation									
Dollars of TANF Benefits	-343.27 ***	-360.61 ***	-345.76 ***	-282.64 ***	-261.36 ***	580	-268.74 ***	680	[-444.95,-67.65]
	(44.07)	(53.30)	(62.58)	(73.68)	(93.74)		(79.60)		
Value Right Below Thr.	496.41	495.27	481.14	467.48	456.38				
Dollars of SNAP Benefits	-162.96	-253.91 *	-210.68	-35.51	-188.81	1009	-110.26	761	[-343.69,571.07]
	(107.20)	(132.59)	(157.23)	(185.66)	(243.09)		(174.61)		
Value Right Below Thr.	3411.64	3376.66	3385.95	3372.46	3417.11				
Months of Medicaid Benefits	-0.519 **	-0.573 **	-0.430	-0.150	-0.046	809	-0.402	809	[-0.583,1.415]
	(0.236)	(0.292)	(0.347)	(0.409)	(0.536)		(0.340)		
Value Right Below Thr.	7.377	7.297	7.275	7.277	7.287				
N	101,985	70,523	50,250	36,039	21,986				

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). Each column corresponds to a different bandwidth. Cols. (6)-(7) report the Imbens and Kalyanaraman (2012) bandwidth and corresponding estimate. Col. (8) reports the bandwidth using Calonico et al. (2015)'s bandwidth selector and col. (9) reports the bias-corrected confidence intervals. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Appendix Table 4: Local Linear Estimates of First Stage and Main Outcome Discontinuities: Gender

		By Gender	
	All	Female	Male
	(1)	(2)	(3)
A. First Stage			
UI Eligible	0.449 ***	0.495 ***	0.389 ***
	(0.004)	(0.005)	(0.007)
Value Right Below Thr.	0.102	0.090	0.120
B. Fuzzy RD Estimates: Employment			
Initial Nonemployment Duration (Qtrs)	0.514 ***	0.417 ***	0.659 ***
	(0.090)	(0.105)	(0.161)
Value Right Below Thr.	2.566	2.514	2.560
Percent Impact	20%	17%	26%
Earnings (Over 2.5 Years)	-834.82 ***	-673.49 **	-1028.47 *
	(272.14)	(287.13)	(559.02)
Value Right Below Thr.	8950.78	8417.06	9816.19
Percent Impact	-9%	-8%	-10%
C.Fuzzy RD Estimates: Program Participation (Over Next 2 Years)			
Dollars of TANF Benefits	-229.74 ***	-372.70 ***	6.16
	(46.72)	(68.64)	(31.28)
Value Right Below Thr.	571.75	917.17	87.30
Percent Impact	-40%	-41%	7%
Dollars of SNAP Benefits	-114.91	-74.79	-235.17 *
	(91.64)	(117.60)	(136.33)
Value Right Below Thr.	2679.88	3221.57	1948.61
Percent Impact	-4%	-2%	-12%
Months of Medicaid Benefits	-0.048	-0.057	-0.238
	(0.227)	(0.287)	(0.316)
Value Right Below Thr.	7.023	9.293	3.893
Percent Impact	-1%	-1%	-6%
N	137,708	80,419	56,429

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). All regressions use a bandwidth of $700. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.

Appendix Table 5: Local Linear Estimates of First Stage and Main Outcome Discontinuities: Dependents

	All (1)	Have Dependents (2)	Females w/ Dependents (3)
A. First Stage			
UI Eligible	0.449 ***	0.469 ***	0.512 ***
	(0.004)	(0.009)	(0.010)
Value Right Below Thr.	0.102	0.099	0.085
C. Fuzzy RD Estimates: Employment			
Consecutive Jobless Qtrs	0.514 ***	0.518 ***	0.287
	(0.090)	(0.168)	(0.190)
Value Right Below Thr.	2.566	2.51	2.50
Percent Impact	20%	21%	12%
Earnings (Over 2.5 Years)	-834.82 ***	-521.58	-585.83
	(272.14)	(476.84)	(482.61)
Value Right Below Thr.	8950.78	8642.57	8267.40
Percent Impact	-9%	-6%	-7%
C.Fuzzy RD Estimates: Program Participation (Over Next 2 Years)			
Dollars of TANF Benefits	-229.74 ***	-282.55 ***	-467.79 ***
	(46.72)	(107.58)	(144.65)
Value Right Below Thr.	571.75	845.44	1251.27
Percent Impact	-40%	-33%	-37%
Dollars of SNAP Benefits	-114.91	-371.828 *	-313.36
	(91.64)	(198.148)	(234.83)
Value Right Below Thr.	2679.88	3516.02	4092.99
Percent Impact	-4%	-11%	-8%
Months of Medicaid Benefits	-0.048	0.141	0.339
	(0.227)	(0.461)	(0.539)
Value Right Below Thr.	7.023	8.928	11.101
Percent Impact	-1%	2%	3%
N	137,708	34,973	22,599

Notes: Panel A reports estimates of τ_d from Equation (2) and panels B and C report RD estimates τ from Equation (3), where each entry is obtained using separate regressions. The values right below the threshold are point estimates of intercept terms β_0 from Equation (1). All regressions use a bandwidth of $700. Standard errors are in parentheses and clustered at the person level. *p<0.10, **p<0.05, ***p<0.01.