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Unemployment Insurance and Means-Tested Program Interactions: Evidence from Administrative Data†

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We study the ways in which unemployment insurance (UI) benefits interact with other elements of the social safety net around job losses. We exploit a cutoff for UI eligibility, based on a workers' highest quarterly earnings in the past year, to generate quasi-experimental variation in UI receipt. We find that UI receipt cuts welfare (TANF) receipt by half among low-earning UI applicants but has no impact on SNAP or Medicaid usage. However, because welfare participation is low in this population, overall crowdout is small. In the quarter following layoff, UI increases total income by 55 percent (including labor earnings and transfers) (*JEL* E24, H53, I18, I38, J64, J65).

The US social safety net is comprised of a network of government programs that provides income and consumption support to disadvantaged individuals and families. Although only the Unemployment Insurance (UI) program explicitly targets resources to the unemployed, other programs, such as those that are means-tested, have also been shown to boost incomes during economic downturns (Moffitt 2013, Bitler and Hoynes 2016). Given this overlap in transfer programs, specific UI policies may impact not only the UI program itself but also the benefits and takeup of other government programs. Accounting for these potential program interactions may alter the implied costs and social welfare impact of a policy, as noted recently by Lawson (2015); Inderbitzin, Staubli, and Zweimüller (2013); and Schmieder and von Wachter (2016).

This paper focuses on the effects of a particular UI policy—a minimum recent work requirement—on other program participation and income sources during job loss for a population of low-earning workers. Minimum recent work policies, which exist in all state UI programs, are meant to ensure that workers have sufficient labor force attachment and typically require a minimum level of earnings over a specific

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period prior to layoff. As such, low-earning workers may be excluded from UI. We examine the extent to which workers participate in other programs targeted at low-income populations, including Temporary Assistance for Needy Families (TANF), the Supplemental Nutritional Assistance Program (SNAP), and Medicaid, in response to UI payments.

Our study uses administrative data from Michigan's Unemployment Insurance Agency containing the universe of UI claimants from 2005 to 2010 and their pre- and post-layoff quarterly earnings. For each claimant, we also observe monthly enrollment and benefits received from TANF, SNAP, and Medicaid using data from the state's Department of Human Services. The availability of administrative data is crucial for this study as household survey data are increasingly plagued by nonresponse bias and measurement error. For example, Meyer, Mok, and Sullivan (2015) find 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 datasets when compared with administrative counterparts.

A key challenge in estimating a causal effect of UI payments on other outcomes is the fact that, as noted above, such payments correlate mechanically with earnings and likely other unobserved characteristics. Therefore, differences in means-tested program use may reflect differences in worker characteristics and earnings potential rather than the availability of UI benefits. We overcome this potential bias by employing a fuzzy regression discontinuity (RD) design that exploits an eligibility rule requiring a worker's highest quarterly earnings in the previous year exceed a specific threshold. By comparing workers who are barely eligible and barely ineligible for UI based on their earnings relative to this threshold, we can identify a causal impact.

We find that UI benefits cut the probability of TANF participation by roughly half and have no impact on SNAP and Medicaid. In order to interpret this effect, though, it is important to note that TANF participation among UI claimants near the earnings threshold is so low that this implies just a \$0.04 reduction in TANF per dollar of UI benefits spent over the next two years, though the effects are larger for some subpopulations. In particular, we find that women, African Americans, and workers who have previously participated in means-tested programs are more likely to substitute UI for TANF. Given the relatively small response of means-tested programs to UI eligibility, we document large differences in total income (labor earnings plus transfers) post-layoff between workers receiving UI benefits and those who are not. Total income differences are somewhat muted by the fact that workers without benefits are more quickly reemployed, a result that is consistent with the large literature on the moral hazard and liquidity effects of UI. Overall, we find that at the eligibility threshold, UI-eligible workers have 55 percent higher total incomes immediately following layoff.

Having established that UI eligibility requirements and benefit receipt create large differences in total income among low-earning workers, in the final part of the paper, we consider the social welfare impacts of changing UI eligibility policy. We demonstrate that shifting the eligibility threshold downward will trade off the benefits of increased consumption during unemployment for previously ineligible workers with the costs of increased social safety net spending and reduced tax revenues (from reduced employment). Importantly, we show that the costs of the policy are dependent on whether we account for non-UI safety net spending and the degree of program substitution, as previously noted by Lawson (2015, 2017) in the case of optimal UI benefit levels. Calibration of the model using observed quantities reveals that for some realistic values of risk aversion, it is socially beneficial to expand UI eligibility, and it is even more so when taking benefit substitution into effect. This finding suggests that policies such as the UI Modernization provisions of the American Reinvestment and Recovery Act of 2009, which allotted \$7 billion to incentivize states to adopt more liberal UI eligibility rules, may have improved the welfare of unemployed workers.[1]

This paper contributes to the literature in three ways. First, these findings illuminate the sources of income for the unemployed. Our findings are most closely related to those of Rothstein and Valletta (2017), who examine the sources of income, including benefits from means-tested programs, that replace UI at benefit exhaustion. Using data from the Survey of Income and Program Participation, they find that social assistance benefits (e.g., from SNAP) increase slightly when UI runs out, replacing approximately a tenth of UI income. Our paper differs from Rothstein and Valletta (2017) in two ways. We focus on the impact of UI availability at the beginning of the unemployment spell rather than at benefit exhaustion, which only affects workers who are still unemployed after several months. Additionally, we study workers at the margin of UI eligibility who have lower earnings than the average UI recipient, and who may therefore rely more on means-tested programs.[2] Similarly, Kawano and LaLumia (2015), who use tax data to examine income patterns of the unemployed, do not focus on a population of likely participants in means-tested programs.

Second, our study contributes to a growing literature on the interaction between UI and other social safety net programs. Most of the recent work in this area has concentrated on whether UI benefits affect disability insurance caseloads, which has so far yielded mixed evidence (Lindner 2016; Rutledge 2012; Mueller, Rothstein, and von Wachter 2013; and Inderbitzin, Staubli, and Zweimüller 2013). In terms of the interaction between unemployment benefits and means-tested programs, the study that is most closely related to ours is Browning, Jones, and Kuhn (1995), who find that welfare program use is higher among voluntary quitters after a 1993 Canadian reform rendered them ineligible for UI, though the prewelfare reform policy environment differs from our setting.[3] Within the US context, our paper is the first to document a causal relationship between UI and means-tested program participation.

Finally, this study relates to a large literature on behavioral responses to UI and its implications for optimal policy. Recent advances in quasi-experimental research

[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 expand UI access, particularly among low earners.

[2] For example, O'Leary and Kline (2014) document descriptively that ineligible UI claimants are more likely to participate in SNAP.

[3] Whelan (2010) uses the same data as Browning, Jones, and Kuhn (1995) and finds more generally that lowering the potential duration of UI increases usage of means-tested social assistance.

designs and the availability of large administrative datasets have paved the way for a growing number of studies that estimate the causal effects of UI benefit generosity using variation from reforms and nonsmooth benefit formulas (see Krueger and Meyer 2002 and Schmieder and von Wachter 2016 for reviews). Unlike most papers in this literature, which focus on marginally changing benefit levels or potential durations for those who already receive UI benefits, we examine the extensive margin impact of granting a worker UI benefits. Since those without UI are more likely to be more liquidity constrained, behavioral responses at this margin may differ from those traditionally considered and generate different optimal policy responses (Chetty 2008, Centeno and Novo 2014). To the extent that policymakers have the flexibility to design UI entry requirements, this paper fills an important gap in UI policy research.

The rest of the paper is organized as follows. In Section I, we describe relevant institutional details about Michigan's UI and means-tested programs. Section II gives an overview of the data and empirical strategy. Section III contains our main empirical results, while Section IV discusses how the effects differ across subgroups. Section V considers the social welfare impacts of changing UI eligibility policies. Finally, Section VI concludes with a summary of our findings and policy implications.

I. Institutional Background

A. *Unemployment Insurance*

The US unemployment insurance (UI) system provides temporary partial wage replacement to workers who involuntarily lose their jobs. To be eligible for benefits, a UI claimant must satisfy both "monetary" and "nonmonetary" eligibility criteria. Monetary eligibility requires that the worker demonstrate labor force attachment by having sufficient earnings in "base period," a one-year period preceding the claim. In Michigan, the base period is either the first four of the previous five completed calendar quarters ("standard") or the most recent four completed quarters ("alternative"). During the base period, a Michigan worker must have earned a minimum of \$2,871 in the highest earning quarter and at least 1.5 times the high quarter earnings in the entire base period.[4,5] Compared to other states, these requirements are strict. Panel A of online Appendix Figure A.1 shows the distribution of a monetary eligibility measure for all US states and the District of Columbia in 2010, where the vertical line denotes Michigan. This measure, which is the proportion of a nationally representative sample of workers who are monetarily eligible in each state, is unrelated to average earnings (panel B) or minimum wages of the state (not shown).[6] In addition

[4] The high quarter earnings requirement, which is indexed to the state minimum wage, 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.

[5] 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 has earnings in two quarters of the base period. In 2010, the SAWW in Michigan was \$828.73, which meant that workers must have had base period earnings of \$16,575. No claimants in our main sample who did not meet the usual criteria qualified using this criteria.

[6] See online Appendix Section B.B4 for details on the construction of this measure.

to monetary eligibility, workers must have involuntarily lost their jobs, meaning that they may not have quit or been fired for cause ("nonmonetary eligibility").

In normal economic times, Michigan's "regular" UI program entitles a UI-eligible claimant near the minimum earnings threshold to a weekly benefit that replaces 53 percent of earnings in the highest quarter for 14 to 26 weeks, as long as she remains unemployed.[7] During our sample period, which covers the Great Recession, some workers were able to receive up to 73 additional weeks of benefits through the Extended Benefits (EB) and Emergency Unemployment Compensation (EUC08) programs. Benefit extensions for both programs depended on the state's unemployment rate. Over the study period, the unemployment rate in Michigan exceeded the national rate by roughly 2.7 percentage points on average, though the gap was as large as 5.4 percentage points during the downturn, as shown in panel A of online Appendix Figure A.2. As a result, Michigan was eligible for the maximum extension during most of the Great Recession. Panel B of online Appendix Figure A.2 shows the statutory maximum number of weeks of extended benefits available to Michigan workers compared to the US average. Finally, the American Recovery and Reinvestment Act of 2009 increased weekly benefit levels by a flat $25 for all recipients from March 2009 through May 2010, increasing the replacement rate to approximately 64 percent for workers at the eligibility threshold.

B. *Means-Tested Programs*

We focus on participation in three means-tested programs as our outcomes of interest: TANF, 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 (for TANF or SNAP) or disqualify the worker altogether. In this section, we describe how the various programs may interact with the UI program. Online Appendix Section A provides more details on the eligibility requirements, benefits, and take-up rates of each means-tested program.

Temporary Assistance for Needy Families.—TANF provides cash assistance to low-income families. To understand how UI and TANF interact, note that TANF benefits are calculated by taking a maximum benefit guarantee, which is set at approximately 35 percent of the federal poverty level, and subtracting any other household income. Unearned income is taxed at a 100 percent rate. Therefore, if a family of three without any income has a monthly TANF guarantee of $492, being eligible for UI payments of $472 per month (i.e., the minimum benefit in Michigan in 2009) would reduce the monthly TANF benefit to $20. Panel A of online Appendix

[7] Michigan determines the weekly benefit amount (WBA) using the following formula: $WBA = \min(0.041 \cdot HQ + 6 \cdot \min(Dep, 5), 362)$, where HQ denotes high quarter earnings in the base period and Dep denotes the number of dependents. The potential duration of regular benefits (in weeks) is given by $0.43 \times (BP/WBA)$ with a minimum of 14 weeks and a maximum of 26 weeks, where BP denotes base period earnings. Since the WBA is proportional to high quarter earnings near the eligibility threshold, the potential duration is a function of BP/HQ. This implies that the more concentrated earnings are in one quarter of the base period, the shorter the potential duration is.

Figure A.3 shows the relationship between the TANF guarantee and the minimum UI payment in each state. While Michigan's TANF benefit is close to the median, its high minimum earnings criteria for UI makes it one of a few states in which the minimum UI benefit is roughly equal to the TANF benefit (equality is denoted by the solid line). In most other states, the minimum UI benefit is lower than the TANF benefit. Therefore, relative to other states, there is more scope for a mechanical interaction between UI and TANF in Michigan.

Supplemental Nutrition Assistance Program.—SNAP, formerly the Food Stamp Program, provides individuals and families with a benefit voucher that can be used to purchase food. The value of the voucher is equal to the maximum benefit guarantee, set at approximately 30 percent of the federal poverty line, minus 30 percent of household income (after deductions). As in TANF, benefits may be mechanically lowered by UI eligibility. For example, the maximum SNAP benefit guarantee for a family of three is $463 per month (in 2009). UI payments of $472 would lower the SNAP benefit to $321 per month. Note that since SNAP benefits are set nationally, there is no variation in benefit levels across states (and hence, there is no SNAP analogue to online Appendix Figure A.3). For most states, the minimum UI benefit is lower than the SNAP benefit guarantee for a family of three, though for Michigan, the two are of comparable size.

Medicaid.—Medicaid provides health insurance to low-income families, children, pregnant women, the elderly, and the disabled. The income limit for low-income families is approximately 35 percent of the federal poverty line. As detailed in online Appendix Section A, several narrowly defined demographic groups (e.g., pregnant women) may qualify for Medicaid programs with higher income limits, though the majority of workers in our sample who are on Medicaid are in the program for low-income families. Panel B of online Appendix Figure A.3 shows how the Medicaid income threshold for jobless parents varies across states and correlates with the minimum UI benefit amount. While for most states, the Medicaid income limit exceeds the minimum UI benefit, the combination of a low Medicaid eligibility limit and high minimum UI benefit in Michigan makes it one of a few states where the thresholds are roughly equal. This may make it more likely that UI eligibility impacts Medicaid participation (mechanically) more in Michigan than in other states.

II. Empirical Strategy and Data

A. *Regression Discontinuity Design*

To estimate the causal impact of UI eligibility, we use a regression discontinuity (RD) design that exploits the monetary eligibility rule requiring a worker's high quarter earnings exceed a certain threshold. By comparing the means-tested program benefits of workers who have similar (high quarter) earnings, but who barely meet or barely miss the eligibility criteria, we can attribute differences to the availability of UI benefits (Imbens and Lemieux 2008, DiNardo and Lee 2011).

We follow the literature and estimate local linear regressions with a uniform kernel. In practice, we estimate the following reduced-form and first-stage equations,

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

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

using observations that are within bandwidth h of the threshold. The variable Y_i is an outcome measure (e.g., participation in a means-tested program); D_i is an indicator for being UI eligible or a dollar amount of UI that a worker is eligible to receive; R_i is the claimant's high quarter earnings; c is the minimum earnings threshold; and $T_i = \mathbf{1}[R_i \geq c]$. The fuzzy RD estimator is the ratio of the reduced-form and first-stage coefficients on T_i, $\hat{\tau} = \hat{\tau}_y / \hat{\tau}_d$.[8]

Our main outcomes of interest are participation in and income from each of the three means-tested programs, and labor earnings. 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. For our main estimates, we implement the bandwidth selector of Calonico, Cattaneo, and Titiunik (2014) and show that results are robust to a variety of bandwidths. All estimates are bias corrected following Calonico, Cattaneo, and Titiunik (2014), and standard errors are adjusted to account for the bias correction.[9,10]

Although this design allows us to estimate a "local average treatment effect (LATE)" that is only relevant for a population of claimants near the threshold, we argue that such a population is a policy-relevant one for studying how different social safety net programs interact. A related question is the extent to which the causal effects of UI eligibility are applicable outside of Michigan, which has stringent monetary eligibility requirements (online Appendix Figure A.1) and a particularly high unemployment rate (online Appendix Figure A.2) over our sample period. To understand whether these findings apply to other states, we explore whether results vary by local area unemployment rates within Michigan and at a different threshold for UI eligibility in Section IV.

[8] As noted by Hahn, Todd, and Van der Klaauw (2001), this is numerically equivalent to an estimate of τ in the TSLS regression $Y_i = \delta_0 + \tau D_i + \delta_1 (R_i - c) + \delta_2 T_i \cdot (R_i - c) + \nu_i$, where T_i is used as an instrument for D_i, using only data where $c - h \leq R_i \leq c + h$.

[9] We use the 2017 version of "rdrobust" package developed by Calonico, Cattaneo, and Titiunik (2014) for all our main estimates. In online Appendix Table A.2, we also report the estimates of our main results using Calonico, Cattaneo, and Titiunik (2014) and Imbens and Kalyanaraman (2012) without regularization terms as implemented by online Appendix B.2 in Card et al. (2015). We thank Zhuan Pei for sharing programs that implement these estimators.

[10] We find that some estimates of the optimal bandwidth vary slightly (typically less than 1 percent) with different iterations of our code. However, the variation in resulting estimates is minor and does not change the overall narrative of our study.

B. *Data*

The data used in this paper are from administrative records from the state of Michigan. From the state's Unemployment Insurance Agency, we have the universe of all regular UI claims made from 2005 to 2010, which we can match to quarterly earnings from the second quarter of 1997 through the second quarter of 2012. 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 benefit duration (i.e., before extensions), and basic demographic information. Since we do not observe extended benefits in the data, we calculate potential benefit durations assuming that workers who exhaust regular UI benefits while an extended benefit period was in effect will receive the extension, as detailed in online Appendix Section B.B3. The quarterly earnings data contain total wages—including bonuses, stock options, severance pay, and tips and gratuities—in each calendar quarter 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 Medicaid and the amounts received (for TANF and SNAP) each month from 2005 through the second quarter of 2011.[11]

To arrive at our main analysis sample, we make several sample restrictions. First, for workers who make multiple claims over our sample period, we only include the first observed claim and claims that are made more than two years after a previous claim. As detailed in online Appendix Section B.B1, the primary reason for this restriction is that the claims made soon after a previous claim may not reflect a new unemployment spell due to the requirement that a worker must reapply for benefits each year. From this group of "new" claims, we select our analysis sample by keeping claimants whose highest quarter earnings of the previous five quarters (to account for eligibility via standard or alternative base periods) are within \$1,500 of the minimum earnings threshold. Finally, to focus on a sample for which the high quarter earnings threshold is relevant, we eliminate claims that do not meet the second eligibility requirement, that earnings in the base period exceed 1.5 times the high quarter earnings. Our final sample consists of 252,616 claims, of which 244,516 are unique individuals.

By definition, our analysis sample contains only low-earning UI claimants. Workers at the cutoff are at approximately the tenth percentile of the earnings distribution and making approximately the equivalent of a minimum wage worker employed 30 hours per week in their highest quarter. Table 1 presents summary statistics for all new claims (column 1) and our analysis sample (column 2).[12] 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. In the RD sample, workers earned on average \$8,278 in the previous year. Many

[11] In online Appendix Section B.B2, we discuss how we adjust our estimates for a short follow-up period in the program data for some workers.

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

Table 1—Descriptive Statistics

	All (1)	RD sample (2)
Demographic characteristics		
Female	40.7%	58.8%
Age	39.0	33.2
Education		
Less than high school	11.0%	18.3%
High school	49.7%	50.5%
Some college	25.6%	24.4%
Bachelor's degree	7.9%	3.7%
Advanced degree	5.4%	2.6%
Race		
White	61.9%	51.7%
Black	13.1%	19.5%
Asian	1.1%	0.7%
Native American/Alaskan	1.1%	1.5%
Has dependents	25.0%	25.9%
Program participation and employment before layoff		
Ever claimed UI (since 2001)	27.3%	24.8%
Ever received benefits from (since 2005)…		
TANF	5.2%	15.0%
SNAP/food stamps	19.4%	45.4%
Medicaid	16.6%	41.5%
Previous job tenure (months)	66.21	24.03
	[91.90]	[40.73]
Previous industry		
Manufacturing	22.8%	4.7%
Retail trade	11.0%	22.9%
Administration, support, and waste management	10.0%	13.9%
Accommodation and food services	7.2%	25.6%
Health care and social assistance	7.4%	8.1%
Previous year earnings	36,847.62	8,277.72
	[33,316.47]	[3,960.09]
Program participation and employment after layoff		
UI eligible	76.2%	47.6%
(Conditional on UI eligibility)		
Replacement rate (median)	44.8%	53.2%
Potential benefit duration in weeks	54.9	56.6
	[33.3]	[32.1]
Received benefits one quarter after layoff from...		
TANF	2.1%	5.9%
SNAP/food stamps	17.4%	38.8%
Medicaid	12.6%	30.9%
(Conditional on receipt) benefit amount one quarter after layoff from…		
TANF	1,083.78	1,071.15
	[659.82]	[655.86]
SNAP/food stamps	785.14	847.51
	[604.21]	[597.94]
Number of months on… (over next two years)		
TANF	0.40	1.11
	[2.30]	[3.83]
SNAP/food stamps	3.84	8.21
	[7.34]	[9.39]
Medicaid	2.80	6.52
	[6.69]	[9.22]
Number of consecutive jobless quarters	2.06	2.63
	[3.47]	[3.71]
Observations	2,001,462	252,616

Notes: The sample for column 1 consists of all "new" UI claimants (i.e., who have not claimed UI for the past two years) in Michigan, 2005–2010. The sample for column 2 is a subsample of column 1, containing only claimants who have high quarter earnings within $1,500 of the UI eligibility threshold and whose base period earnings are more than 1.5 times the high quarter earnings. Standard deviations are in brackets. All monetary amounts are in 2012 dollars.

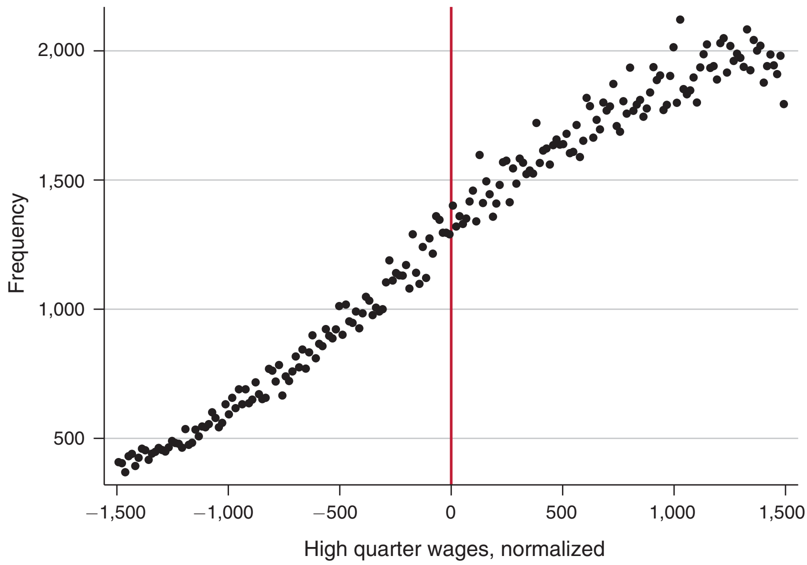

Figure 1. Density of Claims around Eligibility Threshold

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

claimants have had a history of receiving welfare benefits: 15 percent are observed to have received TANF benefits before the claim, 45 percent received SNAP benefits, and 42 percent were enrolled in Medicaid.

C. *Validity of Identifying Assumptions*

The primary concern with any RD design is that agents above or below the threshold are systematically different from one another (Imbens and Lemieux 2008, Lee and Lemieux 2010). In our application, differences between workers on either side of the threshold may arise if workers or employers are knowledgeable about the eligibility rules and time the end of employment so that workers would be just eligible or just ineligible for benefits.[13] The ideal data for the density plot would contain all laid off workers with earnings near the threshold. However, we only have the earning distribution of workers who apply for benefits, which we show in Figure 1. A jump at the threshold in this sample does not necessarily imply that workers manipulated their pre-layoff earnings; rather, it shows that workers may simply be aware of the rules. Perhaps surprisingly though, we find no jump in the density of claims, which suggests that both strategic timing of layoff and perfect knowledge of eligibility rules are unlikely. We implement a formal test of smoothness proposed by McCrary (2008) and fail to reject the null hypothesis of no discontinuity in the density at the 5 percent level using his automatic bin size and bandwidth selector.

[13] For example, Baker and Rea (1998) find evidence that in Canada, when UI eligibility depends on the number of weeks worked, workers are likely to work just enough to qualify for UI.

Another method that is commonly used to assess the validity of an RD is to examine whether the predetermined covariates are smooth across the eligibility threshold. We create two summary measures of covariates by linearly predicting outcomes (probability of receiving any means-tested benefits and probability of being employed in the quarter after the claim) using the following baseline covariates: gender, industry (19 dummies), number of dependents (5 dummies), education level (5 dummies), race (5 dummies), age deciles (10 dummies), tenure deciles (10 dummies), county (83 dummies), deciles for earnings in the previous 10 quarters (9 dummies), and year of claim (5 dummies). Since each predicted outcome is simply a linear combination of predetermined covariates, we would expect them to evolve smoothly across the threshold in a valid RD. The first two panels of Figure 2 plot the binned averages of these predicted outcomes and show no perceptible discontinuities in either covariate index. The first two columns of Table 2 quantify the size of these discontinuities by estimating equation (1), with each outcome Y_i as a different covariate index, for a variety of bandwidths. The estimates confirm the visual evidence that there are likely no discontinuities in the predetermined covariates, especially at smaller bandwidths. The next few columns of Table 2 present estimated discontinuities of several individual covariates that make up the covariate index. Using the Calonico, Cattaneo, and Titiunik (2014) bandwidth, the only covariate that exhibits a statistically significant jump is tenure (1.5 months more tenure for workers above the threshold), which is economically small. With larger bandwidths, there appears to be an increase in the proportion female of about 1.7 to 2 percentage points above the threshold. Since women are more likely to be on means-tested programs, the jump in proportion female will, if anything, bias against finding a reduction in program use among UI eligibles.

One predetermined covariate that we do not include in the covariate index is the probability that a claimant has received means-tested benefits prior to layoff. Since program participation history is highly correlated with program participation after job loss, inclusion of this covariate greatly diminishes the importance of all other covariates in the index. We plot the conditional probability of past welfare receipt in panel C of Figure 2 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 III. The last column of Table 2 shows that the estimated discontinuity is statistically significant at the 5 percent level for a larger bandwidth, but economically small (approximately 1.7 percentage point higher above the threshold). As with the proportion female, however, any increase in the proportion of workers with prior program participation will bias against finding a negative impact of UI eligibility on means-tested program receipt.

D. *UI Eligibility (First Stage)*

Figure 3 plots the probability of eligibility against the claimants' (normalized) high quarter earnings. A claimant is eligible for UI if she is monetarily eligible and not disqualified due to having been fired or having quit. The graph shows a large increase in eligibility of about 50 percentage points once a claimant's high quarter earnings exceed the threshold.

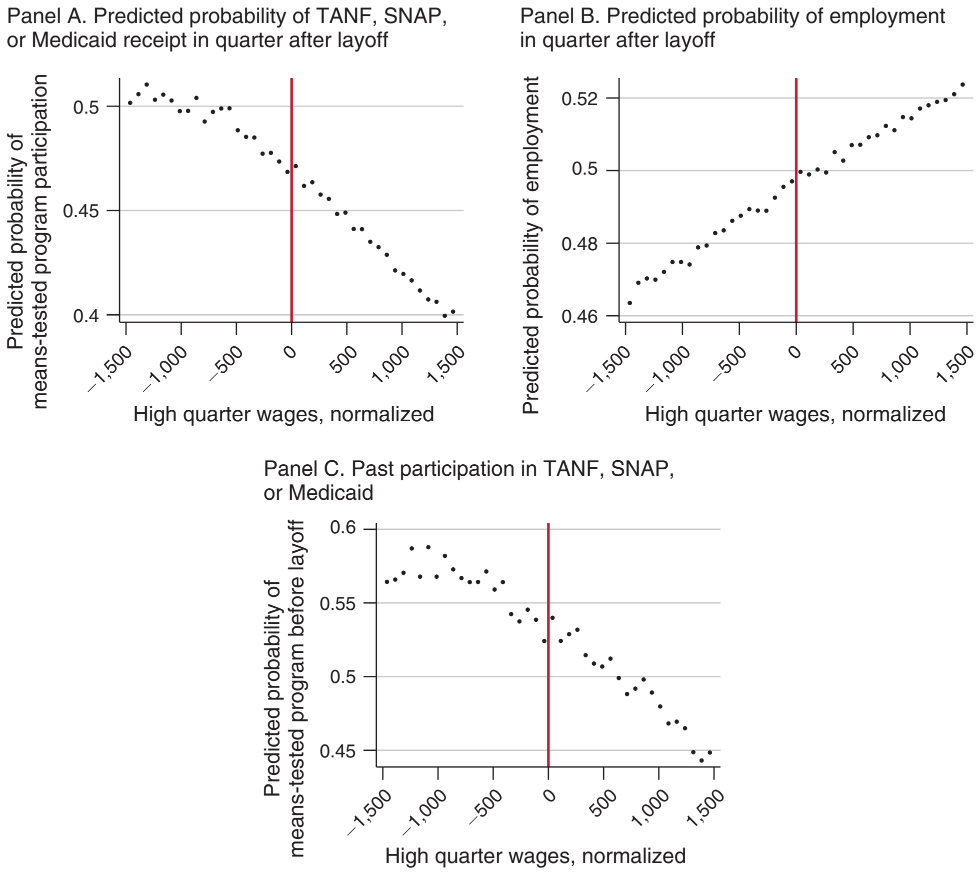

Figure 2. Covariate Balance around Eligibility Threshold

Notes: These figures summarize the relationship between observable predetermined characteristics and high quarter earnings of UI claimants. The summary index in panel A (B) is constructed by predicting the probability of receiving benefits from any means-tested program (of being employed) in the first quarter after layoff, using a linear probability model with gender, industry, number of dependents, education, race, age deciles, tenure deciles, county, previous earnings deciles, and year of claim as predictors. For each summary index, the mean value is plotted against nonoverlapping $75 intervals of (normalized) high quarter earnings categories. Panel C plots the proportion of claimants in each high quarter earnings interval who participated in a means-tested program prior to layoff. The vertical line denotes the minimum earnings threshold.

The probability of UI eligibility does not reach one to the right of the threshold because workers may be ineligible if they quit their jobs or were fired for cause.[14] Of the claimants above the threshold who are ineligible, about 72 percent were due to quits or being 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 9 percent of claims) may be due to misreporting of wages by employers. For example, if a

[14] Workers may receive benefits if they quit for "good cause." Since there is some ambiguity in the rule and workers or employers can appeal the eligibility determination, workers may be incentivized to apply even if they were fired or quit. Furthermore, Michigan's TANF and Medicaid programs require applicants to pursue all benefits for which they might be eligible (including UI), which may induce some workers who appear to be ineligible to apply.

Table 2—Local Linear Estimates of Discontinuities in Predetermined Covariates

					Education			Race	
	Pred. prob. on MT prog. (1)	Pred. prob. emp. (2)	Female (3)	Age (4)	< HS (5)	High school (6)	> HS (7)	White (8)	Black (9)
Panel A. Main estimates									
Estimate	0.003	0.000	0.019	−0.018	0.012	−0.004	0.000	−0.006	0.010
	(0.004)	(0.002)	(0.010)	(0.282)	(0.008)	(0.008)	(0.009)	(0.008)	(0.007)
Bandwidth	373	303	328	307	292	440	353	487	354
Panel B. Estimates using alternative bandwidths									
Bandwidth = 1,000									
Estimate	0.005	0.001	**0.017**	−0.009	0.001	−0.009	0.008	−0.009	0.007
	(0.003)	(0.001)	(**0.007**)	(0.200)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
Bandwidth = 700									
Estimate	0.004	0.001	**0.018**	−0.018	0.007	−0.008	0.001	−0.009	0.013
	(0.004)	(0.002)	(**0.008**)	(0.238)	(0.007)	(0.009)	(0.008)	(0.009)	(0.007)
Bandwidth = 500									
Estimate	0.005	0.000	**0.021**	0.021	0.008	−0.008	0.000	−0.010	0.011
	(0.004)	(0.002)	(**0.010**)	(0.280)	(0.008)	(0.010)	(0.009)	(0.010)	(0.008)

				Prior industry					
	Has dep. (10)	Prev. UI claim (11)	Prev. job tenure (months) (12)	Manu. (13)	Retail (14)	Admin support, waste (15)	Accom. and food (16)	Health and soc. services (17)	Prev. on MT prog. (18)
Panel A. Main estimates									
Estimate	0.005	−0.007	**1.460**	0.002	0.003	0.000	0.018	−0.003	0.016
	(0.008)	(0.008)	(**0.680**)	(0.003)	(0.007)	(0.006)	(0.009)	(0.004)	(0.008)
Bandwidth	338	314	374	390	459	377	283	487	434
Panel B. Estimates using alternative bandwidths									
Bandwidth = 1,000									
Estimate	0.005	0.002	0.679	0.002	0.000	0.003	0.000	−0.001	**0.017**
	(0.006)	(0.006)	(0.566)	(0.003)	(0.006)	(0.005)	(0.007)	(0.004)	(**0.007**)
Bandwidth = 700									
Estimate	0.005	0.002	1.251	0.002	0.000	0.003	0.005	−0.005	0.015
	(0.007)	(0.007)	(0.667)	(0.003)	(0.007)	(0.006)	(0.008)	(0.004)	(0.008)
Bandwidth = 500									
Estimate	0.006	−0.004	**1.752**	0.004	−0.004	0.001	0.014	−0.002	0.013
	(0.009)	(0.009)	(**0.784**)	(0.004)	(0.009)	(0.007)	(0.009)	(0.005)	(0.010)

Notes: Each table entry corresponds to a local regression of the covariate listed in the column heading on a constant, an indicator for being above the threshold, normalized high quarter earnings, and the interaction of being above the threshold and normalized high quarter earnings, using observation within the bandwidth indicated. The estimated coefficient on being above the threshold is reported. The outcome in column 1 (2) is the predicted value from regressing an indicator for enrollment in any means-tested program (employment) in the quarter after layoff on gender, industry, number of dependents, education, race, age deciles, tenure deciles, county, previous earnings deciles, and year of claim. Panel A reports the results using the Calonico, Cattaneo, and Titiunik (2014) bandwidth and panel B reports the results using bandwidths of 1,000, 700, and 500. All estimates are bias corrected, and robust standard errors are in parentheses. Bolded coefficients are significant at the 5 percent level.

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 eligibility, but the quarterly wage records would not necessarily be corrected.

Throughout this paper, we are interested in the impact of UI eligibility or the dollars of UI benefits a worker is entitled to during the unemployment spell. However, although a worker may be eligible for a certain amount of UI benefits, she may not necessarily receive all the benefits to which she is entitled if she returns to

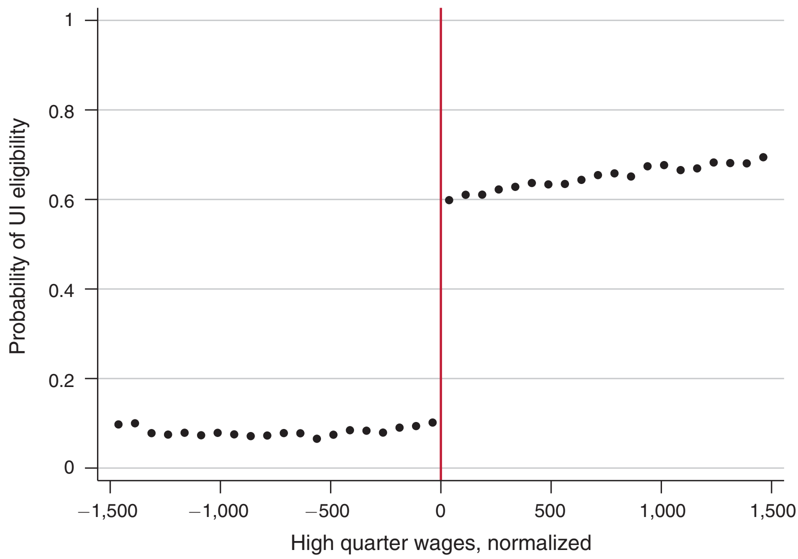

Figure 3. Probability of UI Eligibility (First Stage)

Notes: This figure plots the proportion of UI claimants eligible for UI in each nonoverlapping $75 interval of (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

employment before benefits are exhausted. Therefore, one may alternatively be interested in how actual dollars of UI received affect means-tested program benefits. If the only impact of UI eligibility on means-tested program use occurs through the actual amount of UI benefits received, then actual UI benefits received may be the more appropriate treatment of interest. However, we note that since it is possible for *potential* UI benefits to affect job search behavior and expected durations of unemployment, which in turn may impact decisions to apply for and receive means-tested benefits, we will use UI eligibility as our main treatment of interest, as that fully captures all effects of UI. For the interested reader, in addition to the first-stage impacts being above the minimum earnings threshold on UI eligibility or UI entitlement in dollars, we report the alternative first-stage impacts on the probability of receiving any UI benefits or the dollars of UI benefits received. We find that at the threshold, there is a 45 percentage point increase in the probability of receiving at least one week of UI benefits, implying a 92 percent takeup among those who apply.

Estimates of the first-stage discontinuities are presented in the first two columns of Table 3 using a local linear regression with the Calonico, Cattaneo, and Titiunik (2014) bandwidth (panel A) and other bandwidths (panel B). The sixth and seventh columns of Table 3 express these first stages in terms of dollars of UI benefits that a worker is entitled to or received during the unemployment spell. Over our sample period, which contains the Great Recession, workers just above the eligibility threshold are entitled to draw an additional $2,991 over the next two years.[15] In

[15] This first stage corresponds to the benefit entitlement over two years for most UI claimants in our sample. For some later claimants where we do not observe the full follow-up period, this first stage corresponds to the benefit entitlement over the period we observe their outcome data. See online Appendix Section B.B2 for details.

Table 3—Local Linear Estimates of First-Stage and Means-Tested Program Discontinuities

	Effects in the first quarter after layoff				
	First stage		RD estimates: Effect of UI eligibility		
	UI eligible (1)	Received UI (2)	Received TANF (3)	Received SNAP (4)	Enrolled in Medicaid (5)
Panel A. Main estimates					
Estimate	0.491	0.452	−0.040	0.011	0.023
	(0.008)	(0.007)	(0.010)	(0.018)	(0.017)
Value right below threshold	0.106	0.096	0.077	0.412	0.330
Bandwidth	288	369	293	336	331
Panel B. Estimates using alternative bandwidths					
Bandwidth = 1,000					
Estimate	0.498	0.457	−0.038	0.002	0.019
	(0.006)	(0.006)	(0.007)	(0.014)	(0.013)
Value right below threshold	0.095	0.090	0.079	0.413	0.335
Bandwidth = 700					
Estimate	0.490	0.452	−0.036	0.007	0.028
	(0.007)	(0.007)	(0.009)	(0.017)	(0.016)
Value right below threshold	0.099	0.094	0.078	0.413	0.331
Bandwidth = 500					
Estimate	0.491	0.452	−0.032	0.001	0.027
	(0.008)	(0.008)	(0.010)	(0.020)	(0.019)
Value right below threshold	0.101	0.095	0.078	0.412	0.330

(*continued*)

terms of actual benefits received, workers above the eligibility threshold gain $2,083 in UI benefits.

III. Empirical Results

A. *Evidence of Program Interactions*

In this section, we present graphical evidence as well as formal estimates of the effect of UI eligibility on participation in TANF, SNAP, and Medicaid. We begin by examining how UI eligibility impacts the probability of participating in means-tested programs immediately after filing a UI claim. In Figure 4, we plot the fraction of UI claimants receiving benefits from each program in the quarter following a UI claim within each high quarter earnings bin. Panel A of Figure 4 shows a clear drop of about 2 percentage points in the probability of TANF receipt among workers just above the threshold. In contrast, the analogous plots for SNAP and Medicaid in panels B and C show no difference in program participation between those above and below the threshold. Column 3 of Table 3 confirms that UI eligibility reduces the probability of TANF receipt in the quarter following the claim by 4 percentage points after accounting for the first stage. Also consistent with the visual evidence, there are no statistically significant estimates for SNAP and Medicaid participation (columns 4 and 5). It is worth noting that although there is a large impact of UI on TANF, few UI claimants participate in TANF (roughly 8 percent). On the other hand, a much larger proportion

Table 3—Local Linear Estimates of First-Stage and Means-Tested Program Discontinuities (*continued*)

	Crowdout estimates over two years after layoff					
	First stage		RD estimates: Effect of $1 of UI entitlement			
	UI entitlement ($)	UI received ($)	Dollars of TANF	Dollars of SNAP	Months of Medicaid (x300)	Total income
	(6)	(7)	(8)	(9)	(10)	(11)
Panel A. Main estimates						
Estimate	2,990.93	2,082.77	−0.042	−0.009	−0.001	0.317
	(72.94)	(54.30)	(0.013)	(0.024)	(0.018)	(0.077)
Value right below threshold	882.60	687.76	612.17	2,688.46	2,119.20	12,808.24
Bandwidth	395	422	354	366	354	322
Panel B. Estimates using alternative bandwidths						
Bandwidth = 1,000						
Estimate	3,031.86	2,099.69	−0.038	−0.009	0.005	0.365
	(59.62)	(47.05)	(0.009)	(0.018)	(0.013)	(0.055)
Value right below threshold	838.35	674.23	624.26	2,681.00	2,119.24	12,585.21
Bandwidth = 700						
Estimate	2,993.99	2,063.15	−0.037	0.001	0.008	0.358
	(70.59)	(55.58)	(0.011)	(0.022)	(0.016)	(0.065)
Value right below threshold	877.37	697.31	615.44	2,679.61	2,108.74	12,708.18
Bandwidth = 500						
Estimate	2,933.82	2,013.04	−0.043	−0.010	0.004	0.321
	(83.28)	(65.40)	(0.013)	(0.025)	(0.019)	(0.076)
Value right below threshold	879.12	694.71	609.73	2,672.51	2,111.38	12,764.23

Notes: "First-stage" estimates (columns 1–2, 6–7) are obtained by regressing the outcome indicated by the column heading on a constant, an indicator for being above the threshold, normalized high quarter earnings, and the interaction of being above the threshold and normalized high quarter earnings, using observations within the bandwidth indicated; the coefficient on being above the threshold is reported. "RD estimates" (columns 3–5, 8–11) are obtained by regressing the outcome indicated by the column heading on the same regressors using observations within the bandwidth indicated; the ratio of the coefficient on being above the threshold to the analogous first-stage coefficient (using the same bandwidth) is reported. For columns 3–5, the first stage is UI eligibility; for columns 8–11, the first stage is UI entitlement. "Total income" includes earnings, UI, TANF, and SNAP benefits starting in the quarter after the layoff quarter. Panel A reports the results using the Calonico, Cattaneo, and Titiunik (2014) bandwidth and panel B reports the results using bandwidths of 1,000, 700, and 500. All estimates are bias corrected, and robust standard errors are in parentheses. "Value right below threshold" is the estimated intercept in the reduced-form equation.

of claimants enrolls in SNAP and Medicaid (roughly 41 and 33 percent, respectively), though UI does not significantly affect participation.

Figure 5 shows how impacts of UI on means-tested program participation evolve over time. In each panel, we estimate equations (1) and (2) with outcome Y_i being an indicator for whether individual i received benefits from a means-tested program in a certain quarter relative to the UI claim, and plot RD estimates $\hat{\tau}$. Panel A shows that the reduction in TANF participation persists two years after the initial UI claim, which matches the potential duration of UI benefits for a large fraction of claimants in our sample.[16] The initial difference in TANF participation between UI eligibles

[16] Online Appendix Figure A.4 presents the same graphs focusing on claimants with short and long potential durations separately, showing that the persistent negative effects on TANF participation are largely driven by the claimants with eight quarters of potential UI benefits (panel B). Section IV contains more discussion on how impacts differ by potential duration and other subgroups.

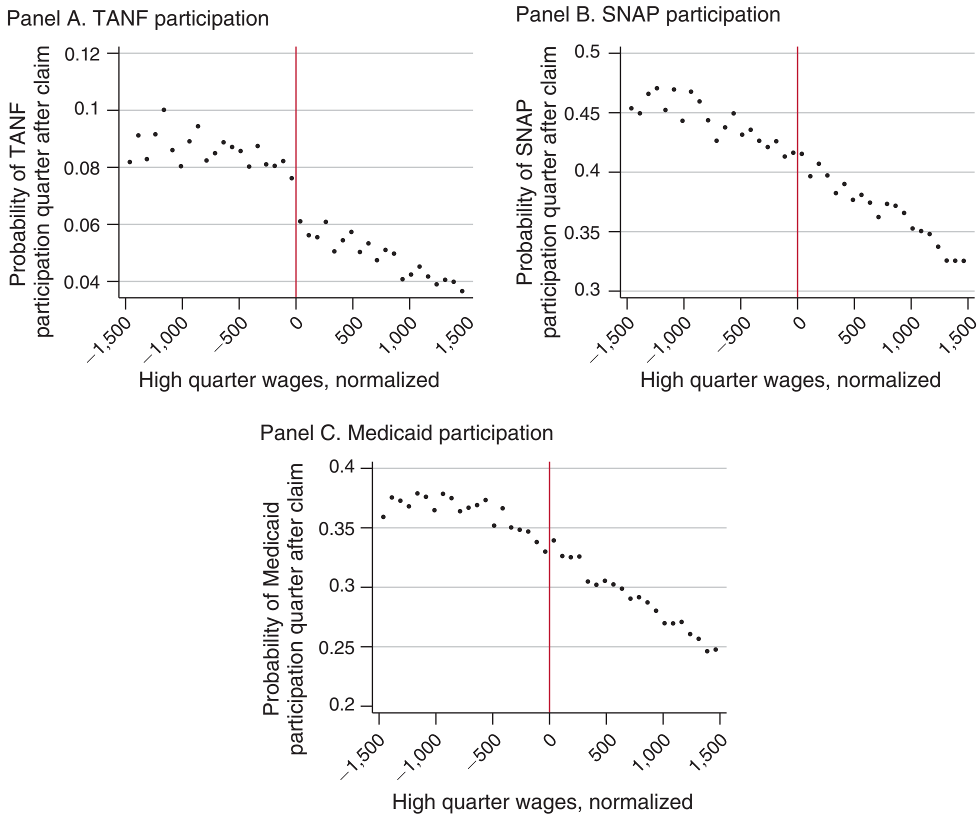

Figure 4. Program Participation in the First Quarter after Layoff

Notes: These figures plot the fraction of UI claimants who participated in each means-tested program in the first quarter after applying for UI in each nonoverlapping $75 interval of (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

and ineligibles, followed by an insignificant difference after UI benefits expire, suggests that UI benefits delay enrollment in TANF but do not permanently deter workers from the program. Panels B and C of Figure 5 show that UI does not similarly reduce participation in SNAP and Medicaid over the unemployment spell.[17]

In terms of dollars of TANF and SNAP benefits received over time, UI-eligible workers receive approximately $43 less in TANF benefits in the quarter immediately following the UI claim relative to an average of about $87 for those just left of the eligibility cutoff (see full results in online Appendix Table A.3). These effects persist to the ninth quarter post-claim, though they are smaller in the third through sixth quarters. The quarterly differences in SNAP benefits are smaller than the

[17] Although there appears to be positive impact on SNAP participation in the fifth through seventh quarters after the UI claim, the positive effects are not robust to specifications with different bandwidths. Online Appendix Figure A.6 shows the reduced-form graphs for several time periods, which exhibit no striking discontinuities. Online Appendix Figures A.5 and A.7 show the analogous graphs for TANF and Medicaid.

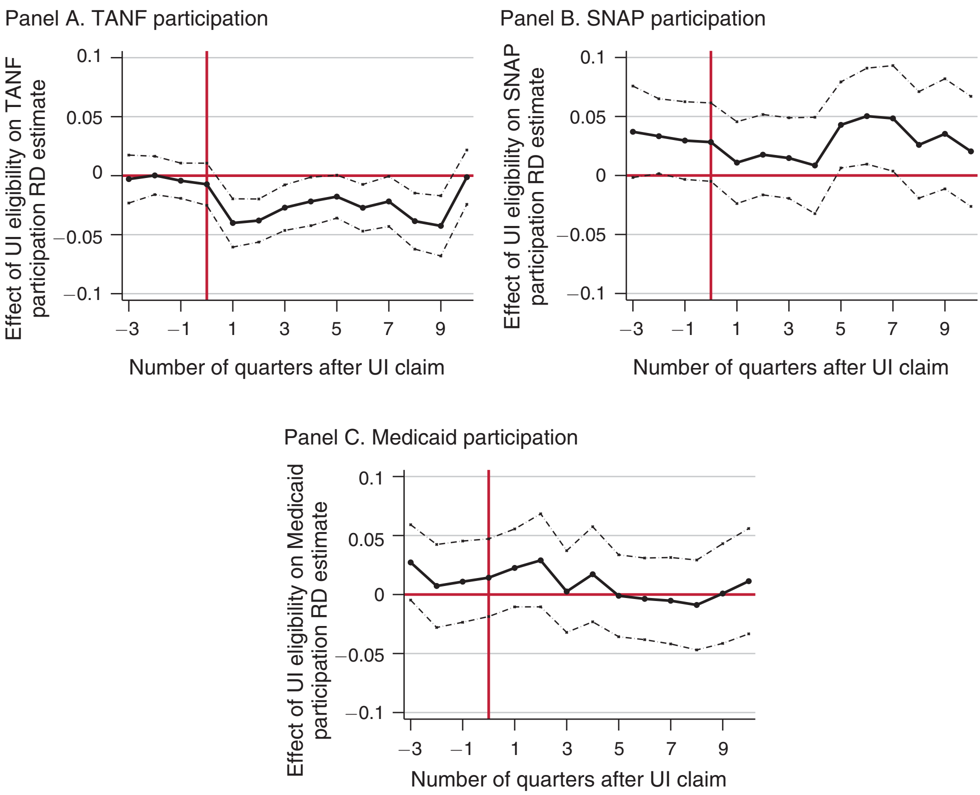

Figure 5. RD Estimates of Program Participation Effects over Time

Notes: These figures plot fuzzy RD estimates of the effect of UI eligibility on probability of participation in a means-tested program in a certain quarter relative to the initial UI claim quarter. Each RD estimate is the ratio of reduced-form and first-stage coefficients on an indicator for being above the minimum earnings threshold. The first-stage (reduced-form) specifications regress an indicator for UI eligibility (probability of program participation in a certain quarter) on a constant, normalized high quarter earnings, an indicator for being above the minimum earnings threshold, and their interaction, using observations within the Calonico, Cattaneo, and Titiunik (2014) bandwidth. All estimates are bias corrected. The dash-dot lines denote robust 95 percent confidence intervals.

corresponding effects on TANF throughout the entire follow-up period, and none of the estimates are statistically significant.

We summarize the impact of UI eligibility on means-tested program use over a two-year period in columns 8–10 of Table 3. In each column, we estimate equations (1) and (2) with D_i as the total dollar amount of UI benefits a worker is entitled to over two years and Y_i as the dollar amount of means-tested program benefits received (or number of months enrolled in Medicaid times \$300) over the same period.[18] The coefficients can be interpreted as the dollar difference in means-tested benefit per dollar of UI available or a "crowdout" measure. We find that although UI eligibility cuts the proportion of workers on TANF by more than half, so few workers are on TANF that the degree of crowdout is small: every dollar of UI benefits reduces TANF

[18] We scale the months of Medicaid by the per month spending on medical expenditures found in the Oregon Health Experiment to obtain a dollar value of Medicaid (Finkelstein, Hendren, and Luttmer 2015).

by a statistically significant \$0.04 and does not reduce SNAP benefits or months of Medicaid enrollment. Panel B shows that these effects are robust to bandwidth choice. This is similar to (though smaller than) the finding in Rothstein and Valletta (2017) that at benefit exhaustion, only a tenth of lost UI income is replaced by public assistance benefits. However, in contrast to our study, Rothstein and Valletta (2017) finds that SNAP and Medicaid respond to UI benefit exhaustion.

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 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.

B. *Effects of UI Eligibility on Total Income*

Labor Earnings.—Given the relatively small response of means-tested programs to UI eligibility, we now turn to its impact on labor market behavior. Since it is well-documented that UI benefit extensions and increases in replacement rates prolong unemployment durations (see Krueger and Meyer 2002 and Schmieder and von Wachter 2016 for reviews), we explore whether the same is true of UI eligibility. To the extent that UI eligibility also reduces labor earnings, the differences in total income (i.e., labor earnings and transfers) between UI-eligible and ineligible workers may be muted.

In Figure 6, we plot binned averages of the initial nonemployment spell duration (in quarters) against high quarter earnings categories.[19] The initial nonemployment duration is defined as the number of consecutive calendar quarters after the UI claim in which no earnings are reported, up to ten quarters. We find a clear discontinuity at the UI eligibility threshold, with workers just above the cutoff working approximately three fewer weeks than those just below the cutoff. The second column of

[19] We use the term "nonemployment" to distinguish from "unemployment" because we do not observe work search in our data.

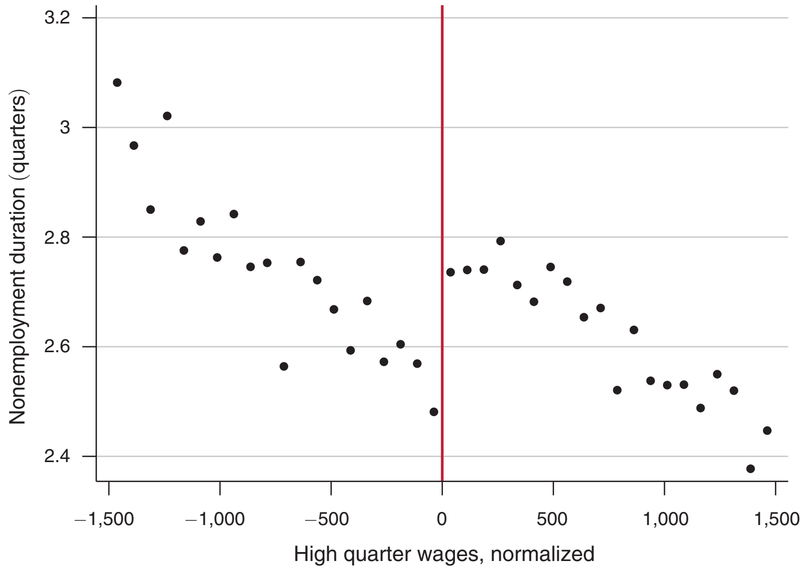

Figure 6. Duration of Initial Nonemployment Spell

Notes: This figure plots the mean number of consecutive quarters in which UI claimants received no earnings for each nonoverlapping $75 interval of (normalized) high quarter earnings. The vertical line denotes the minimum earnings threshold.

online Appendix Table A.1 shows that after scaling by the first stage, UI eligibility increases the duration of nonemployment by about 0.57 quarters (or 1.7 months, a 23 percent increase). These effects on nonemployment durations are large. Among the best-identified papers on the effect of UI on durations, this estimate is closest to, but still larger than, Schmieder and von Wachter's (2016) finding that a six-month increase in potential benefits increased nonemployment by a month.

The dynamics of this impact are shown in online Appendix Figure A.8: The upper panel shows that UI eligibility reduces the probability of employment by approximately 3 to 8 percentage points over the entire follow-up period, while the lower panel shows that it significantly lowers the nonemployment hazard in the first quarter after claim filing and increases the hazard in the tenth quarter. Since UI benefits for a large fraction of eligible workers lasted about two years, this pattern of results is consistent with one in which some UI-eligible workers reduced search effort while collecting UI and increased search effort near benefit exhaustion.

In online Appendix Table A.3 (column 3), we show that UI eligibility reduces (pretax) quarterly earnings by approximately $150 per quarter for 10 quarters post-claim. In terms of crowdout, we find that every dollar of UI benefits reduced labor market earnings by $0.21 two years post–UI claim (column 6 of online Appendix Table A.1).[20] An interesting question is whether the negative impact on earnings (including zeros) masks an effect on reemployment wages as documented

[20] The first stage of dollars of UI entitlement is different in this Appendix Table than in our main Table 3 because we have a longer follow-up period for the earnings data, necessitating less artificial attenuation in the first stage.

recently by Nekoei and Weber (2015) and Schmieder, von Wachter, and Bender (2016). We explore this by defining the pre-layoff wage as the highest quarter earnings in the most recent five quarters before the claim, the reemployment wage as the highest quarter earnings in the five quarters after becoming reemployed, and the wage change as the difference in the log wages. As reported in column 3 of online Appendix Table A.1, we find no effect of UI eligibility on reemployment wages, though estimates are not precise. Furthermore, a statistically significant difference of 3.6 percentage points in surviving nonemployment to the end of our observation period (column 4) makes interpretation of the wage effect difficult, as there are differential rates of observing reemployment wages across the threshold.

Total Income.—The results of the previous section show when workers are eligible for UI, income from other sources (employment and other social assistance) decreases. We now 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 earnings and TANF, SNAP, and UI benefits.

We construct a quarterly income measure that adds labor earnings, TANF and SNAP, and UI one at a time to understand the relative contributions of each type of income.[21] For each income measure and each quarter, we estimate the reduced-form equation (1). In Figure 7, 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. While these plots of reduced-form estimates give a sense of how large the effects are relative to each other and to pre-layoff earnings, online Appendix Table A.3 presents the same information in terms of numerical RD estimates.

Panel A of Figure 7 shows that labor earnings (net of taxes) drop from about \$1,791 one quarter before layoff by approximately \$981 (54 percent) and \$858 (48 percent) one quarter after layoff for workers above and below the UI threshold, respectively. These initial drops in earnings associated with job loss are similar to that found in the seminal work by Jacobson, LaLonde, and Sullivan (1993) for long-tenured workers and consistent with research showing that workers experience larger drops during downturns (Couch and Placzek 2010 and Davis and von Wachter 2011). While those below the threshold earn more in all periods after layoff, the difference is small compared to the magnitude of the initial loss in earnings. For both groups, earnings do not reach pre-layoff levels even ten quarters later, which is also consistent with the literature on the persistence of earnings losses due to layoffs (Jacobson, LaLonde, and Sullivan 1993 and Couch and Placzek 2010).

In Panel B, we add income from TANF and SNAP. The drop in income from one quarter before to one quarter after layoff is mitigated to about 41 and 35 percent for those above and below the threshold, respectively. By comparing panel A and

[21] Because we do not observe the exact timing of UI payments or the amount of extended benefits received, we assume in this exercise that eligible claimants receive the weekly benefit amount every week after filing a claim until they exhaust benefits or become reemployed. We describe the details of our UI calculations in online Appendix Section B.

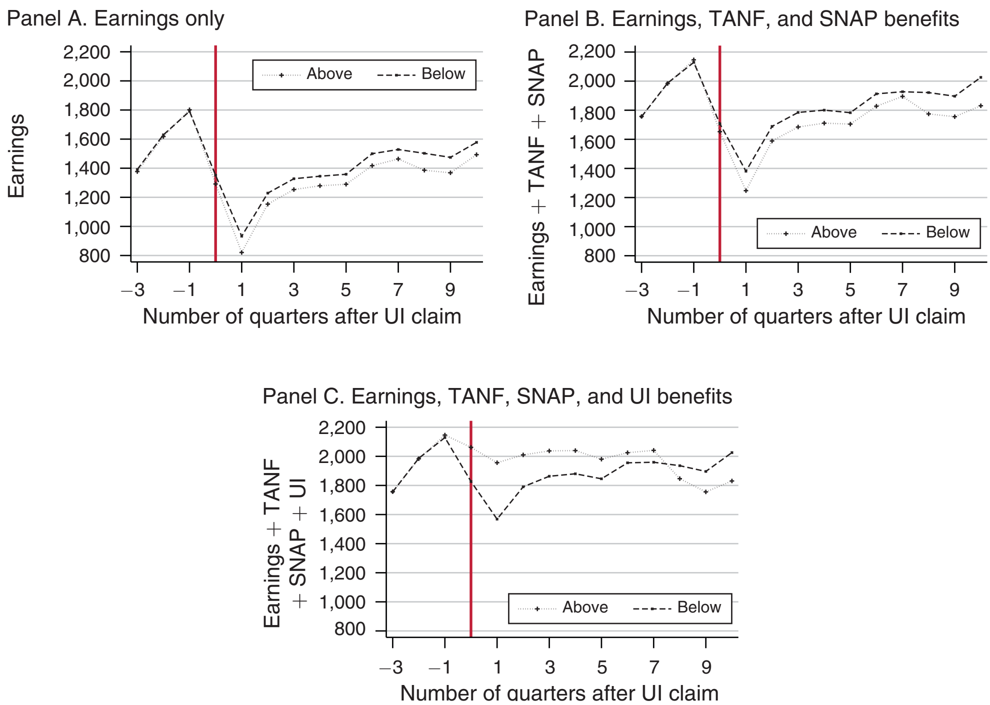

Figure 7. Income from Various Sources over Time

Notes: The graphs in this figure are constructed by regressing an income measure in a certain quarter relative to the UI claim quarter on a constant, normalized high quarter earnings, an indicator for being above the threshold, and their interaction. In panel A, the income measure includes earnings; in panel B, TANF and the cash-equivalent of SNAP benefits are added; and in panel C, (posttax) UI benefits are added. In each graph, "below" denotes the estimated constant and "above" is the estimated constant plus the coefficient on the indicator for being above the threshold. Each regression uses observations within the Calonico, Cattaneo, and Titiunik (2014) bandwidth and is bias corrected.

panel B, we find that although there are statistically significant differences in TANF benefits between UI eligibles and ineligibles, they are barely perceptible when considered relative to the large changes in income across time. Finally, when we add UI income (Panel C), we find that workers above the threshold experience about a 9 percent drop in total income in the first quarter post-layoff relative to pre-layoff, whereas workers below the threshold experience a 26 percent drop, a difference of approximately $387.

Column 5 of online Appendix Table A.3, which presents the total income impacts after accounting for the first stage, shows that in the quarter following layoff, UI-eligible workers have 55 percent higher incomes. By breaking down the different income components, we see that the "gain" of $1,105 of UI income for eligible workers is offset by only $220 (20 percent) in reduced labor earnings and other social assistance. For comparison, Rothstein and Valletta (2017) finds that a loss of UI benefits at exhaustion is offset by 41 percent through increases in labor earnings and social assistance payments. We find that a dollar of UI benefit entitlement increases income by $0.32 over a two-year period (column 11 of Table 3). We conclude that even when alternative safety net programs exist, the impact of UI eligibility on total income is large and significant.

TABLE 4—FUZZY RD ESTIMATES OF UI ELIGIBILITY ON PROGRAM PARTICIPATION: SUBGROUP ANALYSIS

	Effect of UI eligibility on means-tested program use in first quarter after layoff					
	TANF		SNAP		Medicaid	
	Value at threshold	RD estimate	Value at threshold	RD estimate	Value at threshold	RD estimate
	(1)		(2)		(3)	
Panel A. Main estimates						
All	0.077	−0.040	0.412	0.011	0.330	0.023
		(0.010)		(0.018)		(0.017)
Panel B. By potential duration						
Short duration	0.096	−0.060	0.331	−0.016	0.315	−0.006
		(0.022)		(0.037)		(0.037)
Long duration	0.071	−0.040	0.472	0.012	0.340	0.023
		(0.012)		(0.027)		(0.025)
Panel C. Likely program participants						
Female	0.110	−0.054	0.467	0.007	0.428	0.010
		(0.014)		(0.023)		(0.023)
Has dependents	0.112	−0.064	0.498	−0.045	0.427	−0.013
		(0.022)		(0.034)		(0.036)
Black	0.148	−0.076	0.528	0.023	0.436	0.015
		(0.033)		(0.047)		(0.037)
Past program participant	0.141	−0.074	0.683	−0.008	0.578	0.030
		(0.015)		(0.025)		(0.023)
Panel D. Industry and job characteristics						
Accom. and food services industry	0.089	−0.027	0.473	−0.016	0.376	0.069
		(0.019)		(0.032)		(0.032)
Retail industry	0.076	−0.044	0.413	0.036	0.344	0.011
		(0.027)		(0.052)		(0.049)
Admin. support, waste industry	0.089	−0.045	0.443	−0.034	0.329	0.034
		(0.026)		(0.050)		(0.048)
Manufacturing industry	0.047	−0.037	0.325	−0.001	0.263	−0.125
		(0.045)		(0.097)		(0.080)
Tenure $<$ 1 year	0.082	−0.041	0.425	0.018	0.341	0.046
		(0.017)		(0.029)		(0.031)

Notes: This table reports program-interaction results within different subgroups. Each estimate is obtained by regressing an indicator for participating in a means-tested program in the first quarter post-layoff on a constant, an indicator for being above the threshold, normalized high quarter earnings, and the interaction of being above the threshold and normalized high quarter earnings, using observations within the Calonico, Cattaneo, and Titiunik (2014) bandwidth; the ratio of coefficient on being above the threshold to the analogous first-stage coefficient on UI eligibility (using the same bandwidth) is reported. All estimates are bias corrected, and robust standard errors are in parentheses. "Value at threshold" is the estimated intercept in the reduced-form equation.

IV. Heterogeneity Analysis

A. *Subgroup Analysis*

Table 4 reports impact of UI eligibility on program participation in the first quarter post-layoff for various subgroups. For comparison, the first row contains the estimates from Table 3 for our main analysis sample.

First, we stratify by UI potential duration (panel B). As mentioned in Section I, benefits were extended to an unprecedented 99 weeks during the Great Recession from the usual 26 weeks. Therefore, the "treatment" of UI eligibility differed significantly depending on the timing of the claim. We explore how results differ by potential duration by estimating the effects separately for two subsamples. We report the impact of UI eligibility for claims made before any extensions were in effect (i.e., before January 1, 2008) and therefore had a maximum potential duration of 26 weeks ("short duration"). In column 3, we report the analogous results for a subsample of claims that were made between September 21, 2008 and May 15, 2010 when eligible claimants may have qualified for 92–99 weeks of UI benefits ("long duration").[22] It is important to note that labor market conditions across the two subsamples were quite different.[23] We find that TANF participation is reduced in the first quarter for both subgroups, though the effect is larger in the short duration subsample. However, as shown in online Appendix Figure A.4, the reduction in TANF receipt persists much longer in the long duration subsample.

Panel C of Table 4 explores whether effects are stronger among groups that have been traditionally served by or would likely have knowledge of means-tested social programs. In particular, TANF, and to a lesser extent Medicaid and SNAP, was limited for much of its history to single mothers. Accordingly, we find that the reduction in TANF receipt is driven by women and is larger among those who have at least one dependent.[24] Furthermore, since TANF caseloads have been disproportionately African American, we find a larger TANF response in that group. Finally, we find evidence that prior recent experience with any means-tested program (as observed in the program data prior to layoff) results in larger TANF responses but no SNAP or Medicaid responses.[25]

In panel D of Table 4, we examine whether there are systematic differences in program substitution effects by those past job characteristics. In particular, we explore the extent to which those in relatively low-wage, higher turnover sectors are more likely to substitute toward other safety net programs in the absence of UI. We focus on three low-wage sectors that make up the majority of our sample (accommodation and food services, retail, and administrative support and waste management) and one relatively higher wage sector (manufacturing). Although none of the effects are statistically significant, we see that UI reduces TANF more among retail workers and those in administrative support and waste management and less so in manufacturing and accommodation and food services. Finally, since industry is a rather coarse way of categorizing job characteristics, in the final row of Table 4, we examine how program substitution varies by job tenure. We find similar effects on TANF participation among those with less than one year of job tenure relative to the whole sample.

[22] Only claimants who had past earnings that were sufficiently evenly distributed over the base period were eligible for the full 26 weeks of regular benefits and full 73 weeks of extended benefits.

[23] In online Appendix Figure A.9, we explore more fully how effects may differ by labor market conditions within our sample by looking across cities over time. In each graph, we plot the RD estimates for each major metropolitan area (i.e., counties with a labor force of more than 200,000 in 2005) in each year against the county's annual unemployment rate. Overall, we find no relationship between program substitution effects and the unemployment rate, though these effects are noisily estimated.

[24] We find no statistically significant impacts for men.

[25] The stronger effects for those who have a history of past program participation are consistent with O'Leary and Kline's (2008) finding that former TANF participants who received UI were less likely to return to TANF.

B. *Alternative Thresholds*

In this section, we explore the impact of UI on program participation at different points of the earnings distribution by exploiting other features of the UI eligibility and benefit determination process. As mentioned in Section I, to be monetarily eligible for UI benefits in Michigan, workers must meet both a minimum high quarter earnings requirement as well as an earnings distribution requirement. That is, workers may be ineligible if their base period earnings are less than 1.5 times their high quarter earnings, even if their high quarter earnings exceed the minimum required. In panel A of Table 5, we report the effects of UI eligibility at this alternative threshold, again using an RD design. The running variable in this case is the ratio of base period to high quarter earnings, normalized to the 1.5 threshold.[26] To show that this RD design is valid, we conduct a McCrary density test, which confirms that the difference in the number of claims at the threshold is not statistically significant at the 5 percent level. We also check that there is no evidence of a discontinuity in a covariate index constructed as before by predicting the probability of program participation immediately following layoff using fixed pre-layoff worker characteristics (reported in column 11 of Table 5). We find that the first-stage impact of having just enough earnings to meet the distribution requirement is associated with a 38 percentage point increase in being UI eligible (column 1). We do not find any statistically significant program interaction effects in this sample. It is notable, however, that the workers at this threshold have lower rates of program participation relative to workers at the minimum earnings threshold: the participation rates in TANF, SNAP, and Medicaid immediately after claiming are 4 percent, 28 percent, and 20 percent at the earnings distribution threshold, while the corresponding rates are 8 percent, 41 percent, and 33 percent, respectively, at the minimum earnings threshold.

Next, we turn to the impact of UI generosity at a higher point in the earnings distribution by exploiting a kink in the weekly benefit formula. Specifically, eligible workers are entitled to receive a weekly benefit amount (WBA) that is 4.1 percent of their high quarter earnings, up to a maximum benefit of \$362 per week.[27] Therefore, we can use a fuzzy Regression Kink Design (RKD) to identify the effect of the UI benefit level by examining how rates of program participation change as function of high quarter earnings at the kink point (located at \$362/0.041 $\approx$ \$8,829 in high quarter earnings). In panel B of Table 5, we report the RKD estimates.[28] We estimate that first-stage slope change in the weekly benefit level at the kink threshold is -2.7 percent, which is smaller than the statutory change of -4.1 percent and suggests that there is incompliance with the benefit formula for a proportion of the sample.[29] Columns 3–5 and 7–9 in Table 5 show no robust relationships between UI benefits and participation in

[26] See online Appendix Section B for a description of the samples used in this section.
[27] The formula differs slightly for workers with dependents. See Section I for details.
[28] The last column of Table 5 confirms the validity of the RKD design by showing that there is no kink in a covariate index.
[29] As in our main RD analysis, this "fuzziness" could be due to inaccurate wages in the UI records for some workers.

Table 5—Estimates of Program-Substitution Effects at Alternative Thresholds

	Effect of UI in the first quarter after layoff				
	First stage		RD estimates of effect of UI eligibility/RKD estimates of a $1 increase in weekly benefit level		
	UI eligible (1)	UI weekly benefit amount (2)	Received TANF (3)	Received SNAP (4)	Enrolled in Medicaid (5)
Panel A. Earnings distribution threshold					
Estimate	0.376		−0.007	−0.039	0.006
	(0.013)		(0.012)	(0.032)	(0.027)
Value right below threshold	0.257		0.042	0.275	0.203
Bandwidth	0.10		0.15	0.11	0.13
Panel B. Maximum benefit threshold					
Estimate		−0.027	0.0001	0.0004	0.0003
		(0.003)	(0.0001)	(0.0002)	(0.0003)
Value right below threshold		349.76	0.0072	0.1066	0.0659
Bandwidth		473	676	887	573

(*continued*)

means-tested programs at the kink threshold.[30] We note again that participation in means-tested programs is lower at the kink threshold relative to our main analysis sample—less than 1 percent, 10.7 percent, and 7 percent of workers are on TANF, SNAP, and Medicaid, respectively.

What explains the different estimates of program interactions at each point of the earnings distribution? As we highlight above, workers at the alternative thresholds are less likely to participate in means-tested programs. In online Appendix Table A.4, we show the demographic, program participation, and labor market characteristics of the workers who are close to each threshold. First, unsurprisingly, by comparing the first and third columns, we see that workers near the kink threshold are quite different from those at the minimum earnings eligibility threshold: they are more likely to be male, older, white, more educated, and have longer tenures and higher earnings. The most common industry represented at the kink threshold is manufacturing, whereas those at the minimum earnings threshold are more likely in accommodation and food services and retail trade. Characteristics of workers near the earnings distribution threshold are shown in the second column. In many respects, these workers are similar to workers at the minimum earnings threshold. Prior year earnings, age, educational attainment, and racial composition are similar, but at the earnings distribution threshold, workers are disproportionately male and are more likely to work in administrative support and waste management, manufacturing, and construction (11.96 percent, not shown).

We assess the extent to which observable characteristics can explain the lack of TANF response at the earnings distribution threshold by conducting an Oaxaca-Blinder decomposition. Suppose that for claimants at each threshold

[30] The only impact that is statistically significant at the 5 percent level is the number of months of Medicaid, though the estimates and statistical significance are sensitive to alternative bandwidth specifications.

TABLE 5—ESTIMATES OF PROGRAM-SUBSTITUTION EFFECTS AT ALTERNATIVE THRESHOLDS (*continued*)

	Crowdout estimates over two years post-layoff					
	First stage	RD/RKD estimates effect of $1 of UI entitlement				Validity check
	UI entitlement ($) (6)	Dollars of TANF (7)	Dollar of SNAP (8)	Months of Medicaid (×300) (9)	Total income (10)	Pred. prob. on MT prog. (11)
Panel A. Earnings distribution threshold						
Estimate	2,485.27	0.011	−0.023	−0.011	0.298	−0.004
	(178.05)	(0.019)	(0.032)	(0.027)	(0.206)	(0.005)
Value right below threshold	2,238.76	279.98	1,661.85	1,265.91	17,999.28	0.320
Bandwidth	0.10	0.10	0.13	0.11	0.11	0.16
Panel B. Maximum benefit threshold						
Estimate	−1.616	0.004	0.046	0.051	0.241	−0.000003
	(0.288)	(0.007)	(0.028)	(0.024)	(0.198)	(0.000004)
Value right below threshold	17,848.39	68.12	625.82	498.16	38,394.50	0.127
Bandwidth	952	1,133	988	888	1,129	584

Notes: Panel A reports results for the alternative RD at the minimum earnings distribution discontinuity. "First-stage" and "Validity-check" estimates are obtained by regressing the UI eligibility (column 1), dollars of UI entitlement (column 6), or predicted program use (column 11) on a constant, an indicator for being above the earnings distribution threshold, the normalized ratio between base period and high quarter earnings, and the interaction of being above the threshold and the normalized ratio, using observations within the bandwidth indicated; the coefficient on being above the threshold is reported. "RD estimates" (columns 3–5, 7–10) are obtained by regressing the outcome indicated by the column heading on the same regressors, using the bandwidth indicated; the ratio of coefficient on being above the threshold to the analogous first-stage coefficient (using the same bandwidth) is reported. Panel B reports results for the RKD at the maximum benefit kink. "First-stage" and "Validity-check" estimates are obtained by regressing the weekly benefit amount (column 2), dollars of UI entitlement (column 6), or predicted program use (column 11) on a constant, an indicator for being above the kink threshold, the normalized high quarter earnings, and the interaction of being above the threshold and the normalized high quarter earnings, using observations within the bandwidth indicated; the coefficient on the interaction is reported. "RKD estimates" (columns 3–5, 7–10) are obtained by regressing the outcome indicated by the column heading on the same regressors, using observations within the bandwidth indicated; the ratio of coefficient on the interaction to the analogous first-stage coefficient (using the same bandwidth) is reported. The outcome in column 11 is constructed as in Table 2. "Total income" includes earnings, UI, TANF, and SNAP benefits starting in the quarter after the layoff quarter. All estimates are obtained using the Calonico, Cattaneo, and Titiunik (2014) bandwidth and are bias corrected. Robust standard errors are in parentheses. "Value right below threshold" is the estimated intercept in the reduced-form equation.

$k \in \{min, dist\}$, which denote the minimum earnings or earnings distribution thresholds, respectively,

$$(3) \qquad E_k\big[Y_i|X_i, D_i\big] = \delta_k + X_i'\rho_k + \omega_k D_i + D_i X_i'\gamma_k,$$

where Y_i is an indicator for participation in TANF, X_i is a vector of demographic and past job characteristics, and D_i is an indicator for UI eligibility.[31] If τ_{min} is the average causal effect of UI eligibility at the minimum earnings threshold and τ_{dist} is the corresponding effect at the earnings distribution threshold, then

$$(4) \qquad \tau_{dist} - \tau_{min} = \big(\omega_{dist} - \omega_{min}\big) + \bar{X}_{dist}'\big(\gamma_{dist} - \gamma_{min}\big) + \big(\bar{X}_{dist} - \bar{X}_{min}\big)'\gamma_{min},$$

[31] Since we have a fuzzy RD that only identifies the effect of UI eligibility for a "complier" population, all definitions should be understood to be conditional on being a complier at each threshold. To be precise, let the potential treatment D_{1i} be an indicator for whether worker i is UI eligible when her earnings are above the relevant threshold. Similarly, let D_{0i} be an indicator for whether worker i is UI eligible when her earnings are below the relevant threshold. The "compliers" at each threshold are given by the population with $D_{1i} > D_{0i}$, and equation (3) is shorthand for $E_k\big[Y_i|X_i, D_i, D_{1i} > D_{0i}\big]$.

where $\bar{X}_{min}$ and $\bar{X}_{dist}$ are the average characteristics of workers at each threshold. Equation (4) shows that the difference in causal effects identified at the two thresholds can be decomposed into a portion that is "explained" by differential observable characteristics (third term) and a portion that is due to different effects of UI eligibility at the two thresholds (first two terms).

To operationalize (4), we first obtain the eligibility interaction effects γ_{min} from equation (3) by estimating

$$(5) \qquad Y_i \;=\; \delta_0 + \omega D_i + D_i X_i' \gamma + X_i' \rho + \delta_1 \big(R_i - c\big) + \delta_2 T_i \cdot \big(R_i - c\big) + \nu_i,$$

where we instrument D_i and $D_i X_i'$ with T_i, an indicator for whether the minimum earnings are above the threshold, and $T_i X_i'$. The vector X_i contains a series of indicators for demographic and worker characteristics. Equation (5) differs from our main specification by allowing each subgroup to have a different overall level of Y_i and treatment effect. We use only observations that are within \$350 of the threshold, roughly the average optimal bandwidth across subgroups in Table 4. The first column of online Appendix Table A.5 reports the estimated γs. We find that, as in Table 4, women, African Americans, and those who have participated in means-tested programs in the past are more likely to reduce TANF participation when UI eligible. The next columns of online Appendix Table A.5 report the mean characteristics of near each eligibility threshold, as well as a weighted "complier" mean using the weighting method of Abadie (2003).[32] We find that the differences in observable characteristics (i.e., the third term in equation (4)) account for 68 percent of the differences in eligibility effects at the two thresholds (using either the raw means or the complier means).

Although our data are restricted to one state, the analysis in this section offers some lessons for external validity. The reconciliation of different results at alternative thresholds suggests that a minimum requirement for the findings to hold in other settings is that worker characteristics are similar across settings. Since Michigan has a stringent monetary eligibility requirement (online Appendix Figure A.1), it is possible program interactions may differ in other states if different types of worker are affected by eligibility requirements in other states. We show in online Appendix Figure A.10, however, that demographic and industry characteristics, as measured by predicted program use, of ineligible workers in other states appear to not vary systematically with the stringency of eligibility requirements.[33] Another reason for findings to differ across states may be the relative size of UI benefits and means-tested benefits and income thresholds. As mentioned in Section I, Michigan's UI and TANF benefits are more similar than in other states, leaving more scope for a mechanical interaction between the two programs. Our findings within Michigan do not shed much light on whether our results may hold when the UI benefit is much

[32] For the minimum earnings threshold, we estimate the means for workers within \$350 of the threshold, and for the earnings distribution threshold, we estimate means for workers within base period to high quarter earnings ratios within 0.15 of the threshold. For the complier-weighted means, we treat the workers as if they are randomly assigned to be above the threshold within \$350 or 0.15 of each respective threshold.

[33] See online Appendix Section B.B5 for details on the construction of this graph. We also overlay actual program participation (gray markers) to show how well worker characteristics predict actual program use.

smaller than the TANF benefit, as workers at both eligibility thresholds have similar earnings and therefore similar UI and TANF benefits, though it might be reasonable to assume that program interactions in other states may be even smaller than in Michigan's case absent further research.

V. Social Welfare Impact of Expanding UI Eligibility

In this section, we evaluate the social costs and benefits of UI eligibility given the existence of other safety net programs. To illustrate the main points, we consider a policy that expands UI eligibility by lowering the minimum earnings threshold. We show that the costs of this policy change come from increased benefit outlays and longer average durations of unemployment, while the benefits are from increased consumption during unemployment. In many ways, our analysis is similar to a classic optimal UI analysis (i.e., Baily 1978 and Chetty 2008), though with several key differences. First, we consider the effects of offering a marginal worker UI rather than a marginal change in the benefit level for a worker who is already eligible for UI. Second, we account for the fact that there exist social programs other than UI that are paid for by the government through tax revenues.[34] Finally, as in Lawson (2015), we explicitly allow for UI to interact with other social programs. We then use empirically estimated quantities that capture the social costs and benefits to determine whether the eligibility threshold is locally optimal.

In our model, which is described in detail in online Appendix Section C, workers are initially unemployed with exogenously assigned previous earnings that affect their UI eligibility. As in our empirical setting, workers who have prior earnings above a threshold have an increased probability of being UI eligible. UI-eligible workers receive a benefit b from the UI program and a benefit g_{UI} from non-UI programs during unemployment. UI-ineligible workers receive only a benefit g_{-UI} during unemployment from non-UI programs. Given their incomes during unemployment, workers choose the duration of unemployment. 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 workers, 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 increased consumption utility of UI-eligible workers relative to the ineligible. The social cost is quantified by the increased benefits paid out to UI-eligible workers relative to the ineligible and the reduced tax revenue due to longer unemployment spells.

An important aspect of our model is that we do not assume that UI is the only social program that provides benefits during unemployment. This impacts the optimal UI calculation in two ways. First, as noted by Lawson (2017), when government spending consists of both UI and non-UI spending, any tax revenue lost due to longer unemployment durations will have larger social costs than when UI is

[34] Several recent studies (e.g., Saporta-Eksten 2014 and Haan and Prowse 2015) have also considered optimal UI when other government programs provide consumption floors.

the only source of government spending. Second, by allowing non-UI spending to differ for UI-eligible and ineligible workers, costs of expanding UI eligibility will be lower than when we do not account for such benefit substitution. In online Appendix Section C, we show the welfare costs under two alternative scenarios. First, we show that when the social planner incorrectly assumes that non-UI spending remains constant at g_{-UI} when UI eligibility is expanded, she will overstate the cost by the difference in non-UI benefit outlays between the UI eligible and ineligible plus a term that reflects a larger cost of increased unemployment durations. The latter effect is due to the fact that the welfare impact of longer durations is proportional to overall government spending. The second case we consider assumes that the social planner ignores the existence of non-UI programs in their budget. Relative to the case where all non-UI benefits and benefit substitution are taken into account, the change in social costs is ambiguous. On the one hand, costs would be overstated because we do not account for program substitution as before. On the other hand, by ignoring all social programs, the government budget is much smaller, reducing the costs of search distortions.

We calibrate the model using the average labor earnings, benefits, and unemployment durations among just eligible and just ineligible workers in our sample and report the results in Table 6.[35] Panel A reports the empirical quantities we use to calibrate the model, and panel B reports the social welfare benefits and costs at different levels of risk aversion. We first assume that the labor earnings, UI, TANF, and SNAP are the only sources of income that workers receive 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 greater than 1.33, it is welfare enhancing to expand UI eligibility. Furthermore, we show that accounting for even the small amount of benefit substitution found in our empirical setting can affect the welfare calculation: if instead the social planner ignores benefit substitution, social costs increase by approximately 6 percent, and the threshold level of risk aversion for which expanding UI is welfare enhancing is 1.58. If the government ignores all non-UI programs, the costs are lower than the "full accounting" case.

In the last two columns in panel B of Table 6, we loosen the assumption that workers consume all their incomes and instead calculate, for each value of risk aversion, the minimum marginal propensity to consume (MPC) out of social benefit income that would make it welfare enhancing to expand UI eligibility. For a coefficient of relative risk aversion of 4, the minimum MPC is 0.39 if we assume that workers only consume out of their own income and 0.61 if we account for a second earner in the household. Chetty and Szeidl (2007) provides evidence that the relevant value of risk aversion for unemployment shocks can be as high as $R = 4$ in a model with consumption commitments, while Ganong and Noel (2017) shows that

[35] More details on the calibration exercise can be found in online Appendix Section C.

Table 6—Welfare Calibration

Statistic	Value UI-eligible	Value UI-ineligible	Description
Panel A. Empirical quantities			
c^e	1,795.14	1,795.14	Earnings in the quarter before layoff (posttax)
c^e (HH)	3,590.28	3,590.28	Earnings in the quarter before layoff (two-earner household, posttax)
b	1,066.05	0.00	UI benefits in the first quarter after layoff (posttax)
g	469.13	538.47	TANF and SNAP benefits in the first quarter after layoff
c^u	1,535.19	538.47	Income in the first quarter after layoff ($b + g$)
c^u (HH)	3,330.33	2,333.61	Income in the first quarter after layoff (two-earner household, $b + g + c^e$)
s	0.70	0.76	Proportion of the 2.5 years after layoff spent with earnings
τ	424.86	424.86	Budget balancing lump-sum tax

Risk aversion R	Social benefits: Cons. smoothing, CRRA (1)	Social benefits: Cons. smoothing, suff. stat. (2)	Social benefits: Cons. smoothing, suff. stat. (HH) (3)	Social costs: Full costs (4)	Social costs: No benefit substitution (5)	Social costs: No non-UI spending (6)	Minimum MPC: Base (7)	Minimum MPC: HH (8)
Panel B. Welfare calibration								
1	452.97	408.11	324.08	351.98	373.52	331.28	0.86	1.09
2	935.83	576.16	408.11	351.98	373.52	331.28	0.61	0.86
3	2,107.07	744.21	492.14	351.98	373.52	331.28	0.47	0.72
4	5,109.58	912.27	576.16	351.98	373.52	331.28	0.39	0.61

Notes: Panel A shows the values used to calibrate the model described in online Appendix Section C (see online Appendix Section C.1 for details on how each number is derived). Panel B columns 1–3 contain estimates of social benefits assuming a CRRA utility function, using the simplified approximation in equation (C6), and assuming that individuals live in two-worker households, respectively. Panel B columns 4–6 contain estimates of social costs accounting for all programs, assuming no social benefit substitution, and ignoring all non-UI programs, respectively. Columns 7 and 8 are estimates of the smallest value of marginal propensity to consume (ρ) for which it is socially beneficial to expand eligibility. "HH" denotes household.

the MPC out of UI benefits is about 38 cents per dollar in a sample representative of all unemployed workers.[36]

Although welfare conclusions are somewhat dependent on assumptions regarding risk aversion, the marginal propensity to consume, and the worker's other household income, it is notable that we find a positive welfare gain to expanding UI eligibility in some cases despite a fairly large negative impact on work. This contrasts with recent findings that a marginal increase in UI replacement rates may have negative welfare consequences (Kolsrud et al. 2015, Kroft and Notowidigdo 2016, Ganong and Noel 2017). In two of these studies, however, the welfare impact of extending potential durations is positive *because* there are larger drops in consumption as the duration of unemployment increases (Kolsrud et al. 2015 and Ganong and Noel 2017). Since UI eligibility is associated with a large negative drop in income at the beginning of unemployment for a low-earning group of workers, it therefore may not be surprising that the consumption gains are also substantial for this margin.

[36] Ganong and Noel's (2017) MPC is only for nondurable expenditures and may be lower than the overall MPC for both nondurables and durables. Parker et al. (2013) show, for example, that the MPC out of tax rebates for total consumption is more than two times higher than for nondurables. Furthermore, Ganong and Noel's (2017) MPC applies to unemployed workers already receiving UI, which may understate the actual MPC for low earners.

Finally, we note that our welfare calculations are partial equilibrium in the sense that we do not model changes in claiming behavior in response to shifting eligibility thresholds. Although we do not find increased claiming right above the eligibility threshold, it is possible that workers are more likely to apply for UI after the policy change. This is consistent with analyses by Wandner and Stettner (2000) and Vroman (2009), who find that among unemployed nonclaimants, approximately 30 percent cite perceived ineligibility due to insufficient work history as the primary reason for not applying to UI. Despite this limitation, however, we view the welfare exercise as a useful way to summarize the relative magnitudes of each effect and serve as a starting point assessing potential costs and benefits of expanding a part of the social safety net.

VI. Conclusion

This paper explores the impact of UI eligibility on participation in means-tested programs among low-earning workers. Using an RD design, we find that UI eligibility cuts participation in TANF by half but does not impact SNAP or Medicaid participation. However, since most UI applicants do not treat TANF and UI as substitutes, we find that, on net, UI has a large positive impact on workers' total incomes: after accounting for employment responses and benefit substitution, we find that UI-eligible workers have 55 percent more total income following job separation than ineligible workers. These results echo the recent findings in Bitler and Hoynes (2016) that unemployment insurance is a critical and responsive part of the social safety net during downturns.

Our results point to a potentially significant gap 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. In Section V of the paper, we consider a policy that changes one part of the safety net to offer more protection for workers. On the whole, we find that expanding UI eligibility is likely to be beneficial for low-earning workers.

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