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DISCUSSION PAPER SERIES

IZA DP No. 17095

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ABSTRACT

Optimal Unemployment Insurance with Program Interactions*

We study the interaction between unemployment insurance (UI) and other social transfers exploiting state-level changes to UI generosity and data from the Survey of Income and Program Participation (1990-2013). We find that more generous UI leads to a reduction in the receipt of other public transfers. This results from a short-term decrease in the probability of receiving means-tested programs, like the Supplemental Nutrition Assistance Program, and a long-term reduction in the probability of receiving Social Security benefits. Accounting for these interactions, the optimal UI replacement rate may be 17 percentage points higher than the current rate in the US.

JEL Classification: J01, J08, J65

Keywords: unemployment insurance, social security, welfare analysis

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1 Introduction

Job losses generate large and persistent negative effects for dismissed workers and their households. Most estimates indicate an earnings drop at job loss between 10 and 25%, with labor income not returning to its pre-layoff levels even several years after dismissal (Couch and Placzek, 2010; Jacobson et al., 1993; Farber, 1997). Since most laid-off individuals have low levels of wealth, income losses often translate into large reductions in living standards (Chetty, 2008). In the United States (US), estimates of the consumption drop at job loss range from 6-15% in expansions to 20-27% in recessions (Schmieder and von Wachter, 2016). Studies on developing economies find even larger consumption drops, potentially due to gaps in credit markets (Gerard and Naritomi, 2021; Liepmann and Pignatti, 2024).

This points to the importance of providing some form of income protection when job loss occurs. In most countries, this comes in the form of publicly provided unemployment insurance (UI). The literature has identified the optimal UI levels as balancing the trade-off between the insurance value from consumption smoothing and the efficiency costs arising due to longer unemployment spells (Baily, 1978; Chetty, 2008). However, UI also interacts with other welfare programs in an *a priori* unknown direction (Rothstein and Valletta, 2017). By providing income support to laid-off individuals, UI receipt might reduce the demand for other income transfers (e.g. means-tested programs or disability insurance). Alternatively, by creating disincentives to re-employment, UI receipt might induce individuals to rely more on different types of public transfers (e.g. cash welfare or retirement benefits).

The purpose of this study is to provide a full account of the interactions between UI and other contributory and non-contributory programs. This is important given that individuals increase the take-up of many other benefits (i.e. other than UI) after job loss, as we will document. Specifically, we study the effects of UI generosity on the probability of receiving other types of benefits and the amount of benefits received. We thus aim to understand if job losers react to more generous UI by either increasing or decreasing their consumption of other transfer types. This will allow to take a broader perspective to the estimation of both the insurance value (i.e. net of any change in the receipt of other transfers) and the efficiency costs of UI (i.e. net of any spillover on other expenditure types).[1]

Our empirical strategy follows recent contributions on the impact of UI generosity

[1] In countries where UI and non-UI programs are financed by different entities (e.g. in the US, UI is financed by the state budget while other public transfers are financed by the federal budget), this also helps to study if savings from one government entity increase expenditures for another part of the government.

on home foreclosure (Hsu et al., 2018), health insurance (Kuka, 2020) and family stability (Lindo et al., 2023) in the US. Specifically, we use data from the Survey of Income and Program Participation (SIPP) from 1990 to 2013 and exploit variations in UI generosity across states and over time. The SIPP follows respondents at the monthly level for between 30 to 64 months. Our main sample consists of individuals who lose their job after at least three consecutive months of employment. Our treatment indicator corresponds to the maximum amount of UI in a given state and year. In our baseline specification, we include individual fixed effects and follow job losers from 12 months before layoff to 24 months after. Our main identifying assumption is that state-level changes in UI generosity are independent of factors that might otherwise affect the take-up of other non-UI benefits, conditional on state and time fixed effects as well as a rich set of state- and individual-level covariates. We provide evidence supporting our identification assumption in a series of robustness and placebo tests.

Our findings reveal strong interactions between UI and other types of transfer programs. First, we confirm the results from previous studies finding that more generous UI is associated with longer unemployment spells. This translates into higher UI transfers for the first 12-18 months following job loss.[2] However, we also find that more generous UI substitutes for the receipt of other transfers. Specifically, a $1,000 increase in maximum monthly UI benefits decreases the probability of receiving any other public benefit by around 1.1 percentage points. This corresponds to approximately 16% of the estimated increase in the probability of receiving other public transfers after layoff. When looking at the effects in terms of benefit amounts, we find that 20% of the increased costs associated with more generous UI benefits is compensated for by lower transfers from other programs.

Which programs are behind these effects? We find very different patterns across program types. Specifically, more generous UI levels reduce the probability of receiving Social Security for retirement and disability. These effects are large in magnitude and persist even after the end of UI eligibility. We find program substitution effects also for some means-tested programs. However, these are more limited in both magnitude and duration. Specifically, we find that a more generous UI reduces the probability of receiving the Supplemental Nutrition Assistance Program (SNAP), while instead there is no substitution with participation in Temporary Assistance for Needy Families (TANF) or the Supplemental Security Income (SSI). Additionally, even the effects on SNAP disappear around one year after layoff.

We find that labor market responses at both the individual and household levels

[2] The maximum UI duration is generally equal to six months in the US. However, the state or federal governments can approve emergency extensions in response to specific conditions.

lie behind these results. Specifically, the decrease in Social Security receipt is driven by a lasting reduction in the probability of being inactive: the more generous UI increases the opportunity costs of leaving the labor force (i.e. partially counterbalancing the increase in inactivity that takes place after job loss for all dismissed individuals irrespective of UI generosity) and this leads to higher employment rates in the long-run.[3] To interpret our results on means-tested programs, we note that household income is not affected by UI benefit generosity after job loss.[4] This means that eligibility to means-tested programs is unchanged: the little substitution effect that we find (e.g. for SNAP) is primarily driven by lower take-up among otherwise eligible individuals.

To summarise, more generous UI levels (i) increase UI receipt in the short-run, but (ii) reduce the probability to receive means-tested programs in the short-run and (iii) have a long-lasting negative effect on the probability of claiming pension and disability benefits. In the first 12-18 months after job loss, the increase in UI transfers outweighs the savings from lower non-UI transfers. However, the effect on UI transfers disappears with the end of UI eligibility, while the decrease in Social Security transfers remains in place. As a result, we find that, starting around 18 months after job loss, households where the laid-off individual received a *more* generous UI are now receiving *less* public transfers.

These findings have implications in terms of the optimal level of UI. In order to quantify these effects, we adapt the standard framework for UI welfare analysis by Baily (1978) and Chetty (2008) to allow for interactions between UI and other programs, as in Lindner (2016). This assumes that laid-off individuals, while receiving UI, can also apply to non-UI benefits at a cost that is a decreasing with the application's success probability. The individual's optimization problem now includes also the elasticity of UI benefit levels to non-UI application probability, which we have shown being negative. Using this framework, we find that the current UI replacement rate in the US is close to optimal for reasonable values of the coefficient of relative risk aversion, if we were not taking into account program interactions.[5] When savings on non-UI spending are instead also considered, we find that

[3]We still find a negative short-term effect on employment probabilities, in line with the extensive literature that has examined the labor supply responses to UI generosity (Schmieder and von Wachter, 2016). However, the employment effect becomes positive after the end of UI eligibility (i.e. around 18 months after layoff). This is because the positive effect of UI generosity on the probability of being unemployed goes to zero at that point in time, while the negative effect of UI generosity on inactivity persists.

[4]Individuals who receive more generous UI are less likely to be employed in the short term, therefore reporting a decrease in their labor income. However, this is compensated by (i) the higher income from UI, and (ii) increased labor force participation by other household members (i.e. so-called added worker effect). As a result, household total income is barely affected by UI generosity around the time of job loss.

[5]Given that there is considerable debate in the UI literature on what constitutes a reasonable value of

the UI replacement rate should be around 17 percentage points higher.

Our study contributes to different debates at the intersection between public and labor economics. To start with, the paper adds to a literature that examines the interactions between UI and other forms of social protection.[6] Most of the evidence in this area comes from studies that examined the interactions between UI and either disability insurance (Lindner, 2016; Mueller et al., 2016; Rutledge, 2013) or early retirement (Inderbitzin et al., 2016; Kyyrä and Ollikainen, 2008; Lammers et al., 2013). The evidence on the interaction with disability insurance suggests very weak substitution between programs, possibly because there is little overlap in the samples of eligible individuals. Studies on the interactions with early retirement show that longer UI entitlements increase the probability that old-age individuals transition to retirement. Compared to these studies, we consider the interaction between UI and a larger set interventions. These are relevant to a higher share of UI recipients and might interplay differently with the functioning of UI.[7]

A more recent wave of studies has looked at the interaction between UI and a broad range of programs. Leung and O'Leary (2020) exploit a discontinuity in UI eligibility to study the effects of UI on receipt of means-tested programs in Michigan between 2005 and 2010. They find that UI reduces the probability of receiving TANF, but has no effect on SNAP or Medicaid. Compared to this study, we use data for all US states and across several decades and examine the interactions between UI and both means-tested and non means-tested programs. The latter focus allows us to isolate the behavioral from the mechanical effects behind the reduction in non-UI receipt.[8] Rothstein and Valletta (2017) use data from the SIPP to analyze the evolution of household income after UI exhaustion. They find that benefit exhaustion leads to a large drop in income, which is only partially compensated by

the coefficient of relative risk aversion, we will present results for a range of values from one to five, in line with the approach of other papers (Chetty and Finkelstein, 2013; Kolsrud et al., 2018; Schmieder and von Wachter, 2016). However, we note that recent contributions have shown that values in the upper half of this range are more realistic for UI welfare analysis (Landais, 2015).

[6]This is part of a broader literature on the interaction between different social protection programs. As an example, studies have examined the interaction between disability insurance and retirement (Duggan et al., 2007), workers' compensation (Guo et al., 2012) and various social programs (Borghans et al., 2014).

[7]Additionally, most studies on early retirement look at the effects of an expansion in maximum benefit duration (e.g. the Austrian reform in the 1990s), which can generate an unemployment tunnel until retirement. This is different from what we study here, which is the effect of an increase in benefits levels holding duration constant. This might provide incentives for individuals to remain in the labour market.

[8]Another difference compared to Leung and O'Leary (2020) is that we look at the intensive margin of UI receipt (i.e. the effects of a more generous UI benefit), rather than focusing on the extensive margin of benefit receipt. This has important implications for identification, if we believe that the fact of receiving UI has, in itself, an effect on the outcomes of interest. This is likely to happen as UI receipt comes with a number of conditionalities (e.g. reporting on job search) which can also affect the demand of other benefits.

an increase in income from other transfers. Compared to this study, we do not restrict our analysis to the period around benefit exhaustion. This is important if, as we document, program substitution takes place mostly at the beginning of the unemployment spell.

By looking at the effects of benefit generosity on re-employment probabilities and the evolution of income around job loss, our study also contributes to a literature on the welfare effects of UI. The increasing availability of administrative data has led to an expansion of studies that estimate the causal effect of UI generosity on benefit receipt and the length of the unemployment spell (for a review, see Schmieder and von Wachter (2016)). The evidence on the insurance value of UI is more limited, but a large body of evidence now points towards the presence of large and persistent drops in both labor income and consumption after layoff (Couch and Placzek, 2010; Farber, 1997; Gruber, 2001; Jacobson et al., 1993; Kolsrud et al., 2018; Liepmann and Pignatti, 2024; Stephens, 2001). Taken together, these studies can be used to compute the optimal level of UI (Baily, 1978; Chetty, 2008). We contribute to this literature by showing that optimal UI levels can be larger, after considering that more generous UI can reduce fiscal spending in other forms of social protection.

2 Institutional background

In this section, we first provide information on the of UI system in the US (Section 2.A). We then discuss several other programs that UI may interact with: Social Security (Section 2.B); means-tested programs such as TANF and SNAP (Section 2.C); Supplemental Security Income, and other transfer programs (Section 2.D). For each program type, we discuss its functioning as well as possible interactions with UI, as these will be later tested empirically.

A. Unemployment insurance

UI is a joint federal-state program that provides temporary cash payments that partially replace wages of dismissed individuals.[9] In order to be eligible to receive UI, individuals need to fulfill two main requirements. First, they need to demonstrate labor market attachment by having sufficient earnings during a base period, which normally corresponds to the year before job loss. Second, individuals need to have involuntarily lost their job, meaning that they are not eligible to UI if they quit their job or were fired for cause.

[9]This section will describe the UI system as currently operating, but it is worth noting that the legislation governing UI has not experience substantial changes over the period under consideration, with the exception of the temporary expansions approved during times of crises that will be mentioned in the text.

The organization of UI follows a common structure at the federal level, but each state has the autonomy to set the policy parameters. Most individuals are eligible to UI for up to 26 weeks. However, during periods of high unemployment, individuals can receive UI benefits for an extended period under the Extended Benefits (EB) program. This is a federal program adopted in 1970, which provides 13 additional weeks of benefits when the state's unemployment rate reaches 5% and it is at least 20% higher than the average over the two previous years.[10] States can also activate additional EB extensions.[11] The federal government can also supplement the EB with other temporary extensions during recessions.[12]

The typical UI benefit replaces 50% of individuals' weekly earnings in the base period (Kroft and Notowidigdo, 2016). However, states can establish minimum and maximum benefit levels, making UI entitlements a non-linear function of earnings. States vary significantly in the frequency and the scale with which they change maximum UI levels (see Section 3 for details). This generates variations in UI generosity across states and over time that we exploit for identification. Instead, changes in UI minimum levels are less frequent and they also affect a smaller share of the unemployed population (Lindner, 2016).

B. Social security

The SIPP provides information on whether individuals receive income from Old-Age, Survivors, and Disability Insurance, commonly known as Social Security in the US. This scheme provides monthly benefits to three groups of potential recipients (i) retired individuals, (ii) disabled workers and their dependents, and (iii) survivors of insured workers. The scheme is financed through payroll taxes up to a maximum yearly earning amount. Employees and employers contribute at the same rate (currently equal to 6.2%), while contributions by the self-employed equal the combined employee-employer contributions. In 2019, around 176 million workers contributed to Social Security (around 93% of the workforce) and benefits were paid for approximately $1.05 trillion (SSA, 2020).[13] As a result, Social

[10]The federal government pays for half of the added costs of the EB programs.

[11]The first optional extension, which applies in 39 states, adds another 13 weeks if the unemployment rate averages at least 6% for 13 weeks. The second optional extension, which applies in 11 states, adds another 13 weeks if the 3-month rolling average of the unemployment rate is at least equal to 6.5% and is at least 10% higher than the rolling average in one of the two previous years (or 20 weeks under stricter conditions).

[12]During the sample period, this happened with the Emergency Compensation Act of 1991, the Temporary Extended Unemployment Compensation Act of 2002 and the Emergency Unemployment Compensation Program of 2008. The federal extensions of 2002 and 2008 supplemented the automatic extensions from the EB program. The 1991 federal extension instead superseded the state-level EB extensions.

[13]Workers who are excluded from Social Security coverage include civilian federal employees hired before 1984, railroad workers as well as domestic workers, farm workers and self-employed with very low incomes.

Security is a particularly large source of expenditure in the federal budget.

The vast majority of Social Security recipients are comprised of retired individuals, who normally become eligible to receiving full retirement benefits from the age of 67.[14] However, individuals can start receiving Social Security from the age of 62 with reduced benefits. Alternatively, they can postpone retirement until the age of 70 with higher benefits. Pension entitlements are defined based on average earnings in the 35 years where the individual has accumulated highest earnings.[15] Pension entitlements are then defined by applying different replacement rates to different portions of the individual average earning (i.e. the first part of the income is replaced at 90%, while the second part at 32% and the third part at 15%).

Workers with work-limiting health conditions can apply to receive disability benefits from five months after the onset of the disability, provided that they are not working but have a sufficient amount of total and recent work history.[16] The Disability Determination Service in each state collects information from medical providers and decides on the request, based on whether the individual's health condition is sufficiently severe and on the List of Impairments as well as based on whether the applicant can do the same job as before or an alternative job. Around one third of applications are approved. In 2022, 5.4% of the US population between 18 to 64 received Social Security disability payments. The method for computing the monthly benefits is the same as for old-age retirement.

Social Security benefits are not means tested. That is, their receipt will depend on meeting certain eligibility criteria (e.g. reaching retirement age), but not on individual or household income. Additionally, income from UI is not counted as earnings for determining Social Security benefits. This means that any interaction between UI and Social Security will emerge only if the receipt of a more generous UI generates a behavioral response that also affects Social Security receipt. The direction of this interaction is *a priori* unclear. For instance, a more generous UI can increase the incentives to remain out of employment for longer and then directly apply to Social Security (Inderbitzin et al., 2016). At the same time, higher UI levels will increase the opportunity costs of leaving the labor force, and this might decrease the probability of claiming Social Security for either pension or disability.

[14] There were no major changes in retirement age during the period we study.

[15] These average earnings are indexed against the national average wage two years before retirement, in order to obtain an estimate expressed in current wage levels.

[16] Individuals need to have earned at least two credits per year since they turned 21 and have accumulated at least 20 credits in the last year. A credit is earned for every amount of earnings (changing over time, and equal to $1,730 in 2024), but no more than four credits can be earned in the same year.

C. Means-tested programs

We focus on three means-tested programs: SNAP, TANF and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).[17] To receive benefits from any of these programs, participants need to report an income below a certain value. Income from UI is counted to determine eligibility to these programs. This means that, in addition to any behavioral response affecting eligibility, there will be a mechanical effect, whereby a more generous UI either disqualifies individuals from receiving means-tested programs or reduces the amount of benefits received. The mechanical and behavioral effects can potentially work in opposite directions. A more generous UI will mechanically reduce eligibility to other means-tested programs. However, individuals are likely to react to the more generous UI by remaining unemployed for longer (thus reducing their earned income, which increases their eligibility to other means-tested programs). However, earned and unearned income are discounted differently, and the behavior of other household members also needs to be taken into account (i.e. eligibility is generally defined at the household level).

SNAP, previously known as the Food Stamp program, provides families with resources that can only be spent on food items. In order to be eligible, household income needs to be below a certain share of the federal poverty line (which depends on household size and other characteristics, but it is generally around 130% of the poverty line). All income sources count in determining eligibility (i.e. including from Social Security or UI). Benefit levels are set nationally and vary depending on household size and income. Specifically, the maximum benefit level is equal to the cost of a Thrifty Food Plan, which is calculated by the US Department of Agriculture and represents the lowest cost nutritious diet.[18] Household income is deducted from this maximum amount at a 30% rate.[19] SNAP take-up has fluctuated over time, but it was around 80% in 2010 (Ganong and Liebman, 2013).

One of TANF's functions is to provide cash assistance to low-income families with dependent children. To be eligible, households need to report income and assets below certain thresholds and adults need to work or attend training. All these parameters are set

[17]The decision to focus on these three programs is related to (i) their relative importance in terms of spending compared to other means-tested interventions, and (ii) our understanding on how they could interact with UI. Other means-tested programs whose receipt is reported in the SIPP are in any case included in the analysis as part of a miscellaneous category of policies (see below in this section for the full list).

[18]This roughly corresponds to 30% of the federal poverty line.

[19]The income that generates benefit deductions is referred to as net income, and is obtained by considering all sources of household income and subtracting a fixed deduction that varies by family size, a 20% deduction on earnings as well as some other costs (e.g. childcare, medical and shelter costs).

at the state level and there is large variation in their stringency (Leung and O'Leary, 2020). All states also have lifetime time limits to the maximum duration of TANF receipt (e.g. 48 or 60 months of lifetime monthly transfers). The amount of the benefit depends on family income and size. The maximum benefit is around 35% of the federal poverty line, and any income is subtracted to this. Income deduction rates are set by states, but they are generally higher than for SNAP (Hanson and Andrews, 2009). Take-up of TANF is lower than for SNAP, at around 36% of eligible individuals but declining over time (Loprest, 2012).

WIC provides "supplemental foods, nutrition education, and referrals to healthcare and other social services" to low-income mothers, infants and children below the age of five (Hodges et al., 2024). Eligible individuals have their income below a certain threshold, which is defined at the state level and varies between 100 and 185% of the federal poverty line.[20] Additionally, individuals must be at nutrition risk as determined by a health professional.[21] The main benefit provided with WIC corresponds to a food package containing foods that are high in nutrients and beneficial to mothers and their infants. WIC also offers nutrition education (e.g. promoting changes in dietary or physical habits) as well as referral to medical centers or other social services. Benefits are not deducted with household income, provided that individuals remain eligible. Take-up is around 50% of the eligible population.

D. Supplemental Security Income and other programs

We also study the interaction between UI and the receipt of Supplemental Security Income (SSI) and other programs. SSI provides benefits to the same categories of individuals covered by Social Security (i.e. mostly old-age individuals and people with disabilities), but covers individuals with an insufficient history of contributions to be be eligible to Social Security. Additionally, SSI recipients need to have low income levels (below $1,971 per month in 2023, considering both earned and unearned income but after deductions) and low assets (i.e. less than $2,000 for an individual and $3,000 for couples). As for Social Security, benefits correspond to cash transfers disbursed monthly. However, being a means-tested program, benefit levels decrease with household income. SSI is financed by the US Treasury via income taxes, corporate taxes and other taxes.

We also include other income support programs in a miscellaneous category. These programs cover only a small share of the population or provide only low transfer levels. These

[20]Individuals who receive SNAP, TANF or Medicaid are automatically considered eligible to WIC.

[21]The list of nutrition risk criteria varies by state, but federal guidelines note that this should include either medical-based conditions (e.g. anemia) or dietary-based conditions (e.g. poor diet).

include foster child care support, child support payments, alimony, veterans' and workers' compensations, sickness or accident insurance payments, general assistance and relief, receipt of other types of pensions (e.g. from company or the military), receipt of other transfers (e.g. casual earnings or income from relatives) and any other transfer programs.

3 Data

A. SIPP description and sample definition

Our main data sources are the Survey of Income and Program Participants (SIPP) and state-level records of UI benefit generosity. The SIPP is a longitudinal household survey that provides detailed information on household and personal income, labor market status and job characteristics, participation in public programs and a series of other individual and household characteristics. The survey is nationally representative of the civilian non-institutionalized population. We use information from all SIPP panels between 1990 and 2008, which jointly contain information from 1990 to 2013. Each SIPP panel interviews up to 43,500 individuals and follows them for up to four years. Interviews occur every four months, but questions also reference the months between interviews.

The SIPP is very well suited for the purpose of the analysis, as it contains information on employment status, job-search behavior and reason for dismissal, which allow to identify likely UI recipients, together with state and time identifiers, which allow to compute the relevant UI benefit schedule.[22] Additionally, the longitudinal nature of the survey allows us to track individuals before and after job loss, giving us the opportunity to test the plausibility of the parallel trend assumption and to study the evolution of treatment effects after layoff. The main disadvantage of the SIPP relates to standard concerns over the use of survey data to report information on program participation, with evidence showing that survey respondents under-report benefit receipt (Meyer et al., 2015). While later we provide evidence that this is unlikely to affect our results, we note here that classical measurement error in the dependent variable will not necessarily lead to attenuation bias.

We include in the main sample unemployed individuals who are separated from a job after at least three consecutive months in employment, irrespective of the reason for absence

[22]The only exception is represented by the fact that, in SIPP panels before 2004, the state identifier is suppressed for households in states with only few observations. These include the following years and states: Alaska (1990-97), Idaho (1990-95), Iowa (1990-95), Maine (1990-2003), Montana (1990-95), North Dakota (1990-2003), South Dakota (1990-2003); Vermont (1990-2003) and Wyoming (1990-2003).

from employment and the reason for dismissal.[23] The 3-month employment requirement is meant to focus on individuals who are more likely to receive UI (Kuka, 2020; Lindo et al., 2023). However, we also show that increasing this tenure requirement does not change our results. The fact that we do not condition on specific reasons for non-employment is meant to avoid conditioning on post-layoff job-search decisions (Lindo et al., 2023).[24] However, results are very similar even if we use a stricter definition of unemployment as in Hsu et al. (2018). Finally, we do not restrict the sample to individuals reporting specific reasons for dismissals due to concerns of misreporting (Hsu et al., 2018; Lindo et al., 2023). However, results are similar when we look at individuals dismissed for specific reasons as in Kuka (2020). We also conduct a placebo test on individuals who report quitting their job.

B. Descriptive trends from the SIPP

We center the analysis around the month of layoff and follow individuals from the year before separation up to two years after layoff. Appendix Table B1 presents selected descriptive statistics for the sample of unemployed individuals as identified above, measured at the time of job loss as well as 12 months before and 24 months after the layoff event. The data shows that the composition of the sample remains constant around the layoff event for all the selected individual-level characteristics. Half of the sample is constituted by men, more than 80% are White individuals and the average age at layoff is around 36 years. Around half of individuals are married and one third holds at least a college degree.[25]

Looking at the evolution of benefit receipt around the time of job loss, we find that UI benefit receipt is almost zero 12 months before job loss. It then sharply increases around the time of layoff, but it goes back to very low levels 24 months after layoff.[26] This is consistent with the fact that most UI recipients have exhausted their benefits two

[23]As in previous studies, we look at the sample of unemployed individuals rather than the sample of UI recipients. This is because policy take-up is likely endogenous.

[24]In particular, we consider individuals as unemployed if they choose any of the options from 3 to 7 to the SIPP question on employment status (RMESR). This also means that we include in the sample individuals who are temporarily laid-off. While this might seem inconsistent, it follows from the evidence that temporarily laid-off individuals represent a large share of UI recipients (Katz and Meyer, 1990).

[25]The composition of the sample shows an increase in the share of individuals with a college degree after layoff. This is consistent with evidence that laid-off individuals increase educational enrollment (Barr and Turner, 2014), a finding that we also confirm in the paper.

[26]Note that, even at the time of job loss, only 14% of our sample reports receiving UI. While this value is low, it is consistent with other survey evidence. For instance, Kuka and Stuart (2022) report a UI take-up rate equal to 37% among White and 28% among Black individuals, but after imposing more conservative sample restrictions to identify likely UI recipients (i.e. including on the reason for job loss).

years after job loss, even when temporary extensions were in place. We note that the take-up of other benefits, such as Social Security, SNAP, WIC and SSI, also increases around layoff. Differently than for UI, however, receipt of these benefits further increases after layoff. This is consistent with the fact that job losses generate large and persistent earning and consumption losses, and individuals might react by combining participation in different programs (Rothstein and Valletta, 2017). Trends in TANF also follow a similar trajectory, but the magnitude of the increase in take-up is substantially smaller. Appendix Figure A1 shows the share of individuals in our sample receiving UI, Social Security, SNAP, TANF, WIC and SSI; by month before and after the time of job loss.

C. State-level UI data

We match our SIPP sample with state-by-year information on the generosity of UI benefits collected by the US Department of Labor. Following Hsu et al. (2018) and Lindo et al. (2023), we use the maximum weekly UI benefit amount as our measure of UI generosity. This is because there is large heterogeneity in changes in maximum UI levels across states and over time, and these changes affect a large share of UI recipients (Lindner, 2016).

States vary significantly in the frequency and magnitude with which they change maximum weekly UI benefits (Table 1). Between 1990 and 2013, some states updated maximum weekly benefits only rarely and their maximum UI amounts decreased in real terms (e.g. Delaware, Florida and Michigan). In other states, maximum weekly benefits have been updated more frequently, but only to keep them constant in real terms (e.g. Alabama, Mississippi and West Virginia). Instead, certain states implemented frequent increases that have resulted in maximum weekly benefits growth outpacing inflation (e.g. Illinois, Kansas and Oregon). There are also states that have updated maximum UI levels only rarely, but with large increases each time (e.g. California, Maryland and Tennessee).

While there are states with persistently high maximum benefits (e.g. in the North-east) and states with persistently low benefit levels (e.g. in the South), there are also large variations across states and over time. Some of the states that had very low maximum UI benefits in 1990, experienced among the largest increases (e.g. Nebraska and North Dakota) and ended the period with above the median maximum benefit levels. Some of the states that had among the highest UI levels in 1990 implemented instead very small increases and ended the sample period with relatively low maximum benefit amounts (e.g. District of Columbia and Michigan). In 2013, the maximum UI weekly benefit ranged from $265 in

Table 1: State-level variations in UI maximum weekly benefit levels

	1990	2013	Rank 1990	Rank 2013	# changes	% change
Alabama	150	265	48	48	9	0.77
Alaska	260	442	10	24	3	0.70
Arizona	155	240	47	50	6	0.55
Arkansas	215	451	25	21	21	1.10
California	190	450	33	22	6	1.37
Colorado	224	513	21	15	22	1.29
Connecticut	302	666	3	3	21	1.21
Delaware	225	330	19	42	7	0.47
District of Columbia	293	359	4	39	5	0.23
Florida	200	275	28	47	3	0.38
Georgia	175	330	40	42	13	0.89
Hawaii	256	534	12	12	21	1.09
Idaho	200	357	28	40	21	0.79
Illinois	260	562	10	8	19	1.16
Indiana	161	390	44	33	14	1.42
Iowa	222	486	22	16	22	1.19
Kansas	216	456	24	20	22	1.11
Kentucky	186	415	36	30	13	1.23
Louisiana	181	247	37	49	5	0.36
Maine	270	558	9	9	21	1.07
Maryland	205	430	27	26	9	1.10
Massachusetts	408	1011	1	1	21	1.48
Michigan	275	362	8	37	5	0.32
Minnesota	255	610	13	5	22	1.39
Mississippi	145	235	49	51	7	0.62
Missouri	160	320	45	46	10	1.00
Montana	190	446	33	23	23	1.35
Nebraska	134	362	51	37	14	1.70
Nevada	194	402	32	32	21	1.07
New Hampshire	162	427	43	27	13	1.64
New Jersey	279	624	7	4	22	1.24
New Mexico	170	457	41	19	20	1.69
New York	245	405	14	31	5	0.65
North Carolina	236	535	18	11	20	1.27
North Dakota	187	516	35	14	22	1.76
Ohio	291	557	5	10	21	0.91
Oklahoma	197	386	31	34	22	0.96
Oregon	238	524	16	13	23	1.20
Pennsylvania	288	581	6	7	19	1.02
Rhode Island	323	707	2	2	22	1.19
South Carolina	165	326	42	44	15	0.98
South Dakota	140	333	50	41	21	1.38
Tennessee	160	325	45	45	8	1.03
Texas	217	440	23	25	18	1.03
Utah	214	479	26	17	21	1.24
Vermont	178	425	38	28	19	1.39
Virginia	176	378	39	35	14	1.15
Washington	237	604	17	6	17	1.55
West Virginia	245	424	14	29	19	0.73
Wisconsin	225	363	19	36	13	0.61
Wyoming	200	459	28	18	20	1.30
Average US	217.25	446.65			15.69	1.06
Median US	214.00	430.00			19.00	1.10

Notes: The table presents the maximum weekly UI levels in nominal terms in 1990 and 2013 for each US state, as well as the average and median value for the US. The table also reports the state ranking in terms of maximum weekly UI benefits in both 1990 and 2013, the number of changes to UI maximum benefit levels that occurred during the sample period and the per cent increase in maximum benefit levels. Information comes from the yearly reports ”Significant Provisions of “State Unemployment Insurance Laws” conducted by the Employment and Training Administration from the US Department of Labor.

Mississippi to $1,011 in Massachusetts. Instead, variations in minimum benefit levels are significantly less frequent during the sample period (Appendix Table B2).

4 Methodology

Our empirical approach follows closely the one adopted by Hsu et al. (2018), Kuka (2020) and Lindo et al. (2023). In particular, we exploit the longitudinal nature of the SIPP to construct for each job loser a panel than can span from 12 months before to 24 months after job loss. We then conduct a difference-in-differences (DiD) analysis comparing individuals exposed to differential maximum UI benefit levels before and after job loss. In practice, we estimate the following equation:

$$Y_{isqt} = \alpha_i + \beta_1 AfterLayoff_{isqt} + \beta_2 MaxUI_{sqt} * AfterLayoff_{isqt} + \gamma_1 X_{isqt} + \gamma_2 Z_{st} + \nu_{qt} + \lambda_s + \epsilon_{isqt} \quad (1)$$

where Y_{isqt} is the outcome of interest for individual i in state s in quarter q and year t. $MaxUI_{sqt}$ is the maximum UI benefit level in the state at the time of job loss. This is expressed in thousand dollars and in monthly (rather than weekly) amounts, and it is also converted to 2011 real values. $AfterLayoff_{isqt}$ is a dummy that takes the value of one in the 24 months after layoff. We also include individual fixed effects α_i as well as quarter-by-year fixed effects ν_{qt} and state fixed effects λ_s. The vector X_{isqt} includes individual- and household-level controls for the number of children (capped at four), marital status (i.e. married, widowed, divorced or separated and single) and educational attainments (i.e. high school or less, some college but no degree, college degree or more). Z_{st} includes state-level macroeconomic variables (unemployment rate, GDP growth, per-capita income and poverty rates), institutional variables (i.e. minimum wage levels, trade union membership, collective bargaining coverage and party affiliation of the state governor) and controls for the eligibility criteria of other policies (i.e. SNAP, TANF, SSI, worker compensation and the EITC).[27]

Panel weights are used in all specifications and standard errors are clustered at the state level. We also augment equation (1) by running event-study estimates as in Jacobson et al. (1993). This is implemented by creating dummies for all the months before and after

[27]These state-level controls come from the University of Kentucky Center for Poverty Research (for the unemployment rate, per-capita income, poverty rate, minimum wage levels, party affiliation of the governor and stringency of eligibility criteria for the various policies included in the analysis), the Quarterly Census of Employment and Wages (for GDP growth) and the Union Membership and Coverage Database (for trade union membership and collective bargaining coverage).

the layoff event and interacting them with the measure of UI generosity (with the omission of the month before layoff). This more flexible specification allows to estimate the impact of UI generosity also in the months before job loss, where we would expect to find no treatment effects if the parallel trend assumption holds. We also conduct a large set of robustness tests, by, among others, varying the set of controls and fixed effects, modifying the composition of the sample and changing the definition of the treatment indicator.

While researchers have recently identified shortcomings related to the use of two-way fixed effect models (Borusyak et al., 2024; Callaway and Sant'Anna, 2021), it is difficult to adopt any of the proposed alternatives to the present context. This is because of the continuous nature of our treatment indicator as well as the fact that (i) all states experienced at least one increase in UI maximum weekly benefits during the sample period, and (ii) the median state saw 19 yearly increases in UI maximum weekly benefits over a 24 year interval (see Table 1 above for details). This makes very difficult to split the sample between treated and control observations, unless one is willing to make strong assumptions on what constitutes big enough changes to UI levels. However, we lack any theoretical guidance on what these big changes could look like and also ignore if any non-linear relationship exists between the size of the benefit change and its effects on the outcomes of interest. Accordingly, we follow Lindo et al. (2023) and adopt the standard two-way fixed effect DiD framework.[28] However, we will run a robustness test with a sub-set of our sample to show that our results are robust to the use of alternative DiD estimators.

The main coefficient of interest corresponds to β_2, which captures the effect of UI generosity after job loss (i.e. when the individual is eligible to UI). Given that we include individual fixed effects, the analysis exploits only within-individual variations in the outcomes of interest. Additionally, after including year-by-quarter and state fixed effects, the main variation in UI generosity that we use for identification comes from variations in maximum UI levels within states over time. The main identification assumption is that these differences in UI generosity are not correlated with other factors that also affect individual changes in the probability to participate in public programs around the time of job loss (Kuka, 2020).

A possible concern arises if state-level changes in UI generosity are correlated with changes in the business cycle or changes in other policy domains, which are also likely to affect individuals' likelihood to participate in public programs. To rule out this hypothesis, we regress our measure of UI benefit generosity against (i) a series of macro-economic outcomes

[28] This is also similar to the approach in studies on the Earned Income Tax Credit (McInnis et al., forth.).

at the state level, (ii) the stringency of eligibility requirements for other public policies and (iii) other institutional labour market characteristics (e.g. trade union density and minimum wages). We include these characteristics first separately and then jointly, and we control in all specifications for year and state fixed effects. The results show that there is no significant relationship between maximum UI benefit levels and state-level macroeconomic variables (Panel A of Appendix Table B3), the stringency of eligibility conditions of other public programs (Panel B) and other institutional labour market characteristics (Panel C).[29]

Another concern relates to whether the sample of unemployed individuals changes in a way that is correlated with changes in UI benefits. In particular, evidence has shown that more generous UI benefits lead to longer unemployment spells and it can also increase policy take-up (Anderson and Meyer, 1997). If these changes in the sample composition are correlated with unobserved changes that also affect participation in public programs (e.g. individuals who take-up UI when this is more generous are also more likely to rely on other means-tested programs), then our estimates might be biased. To rule out this concern, we test whether changes in UI generosity affect the composition of our sample (Kuka, 2020). The results show that, after controlling for year and state fixed effects, there is no statistically significant relationship between either maximum benefit levels or maximum potential duration and indicators for gender, age, marital status, educational attainments and race (Appendix Table B4). This provides suggestive evidence that there should be no correlation also between unobserved heterogeneity and UI generosity.[30]

5 Results

This section presents the main results of the analysis. We start with our baseline results on receipt of UI and of any other public program jointly considered (Section 5.A). We then explore in details the receipt of which types of non-UI public programs is affected by UI generosity (Section 5.B) as well as the labor market mechanisms that are behind our results (Section 5.C). We end the section by presenting a series of robustness tests (Section 5.D).

5.A Baseline results

Table 2 presents the baseline results of the analysis. There, we look at the effects

[29] This confirms findings in Hsu et al. (2018), Kuka (2020) and Lindo et al. (2023) who also found no significant relationships between UI benefit generosity and similar state-level controls.

[30] To further validate this point, we will show in the robustness tests that results are very similar with and without individual fixed effects, suggesting that unobserved heterogeneity does not drive our findings.

Table 2: Effects of UI benefit generosity on the receipt and amount received of UI and any other programs

	Panel A: Receipt of UI				Panel B: Amount of UI			
Post layoff	0.050***	0.132***	0.132***	0.132***	12.268	102.637***	103.029***	102.464***
	(0.012)	(0.013)	(0.013)	(0.013)	(15.788)	(16.778)	(16.790)	(16.854)
Post layoff*Max UI	0.021***	0.022***	0.022***	0.022***	48.612***	49.961***	49.789***	50.118***
	(0.006)	(0.006)	(0.006)	(0.006)	(8.470)	(8.184)	(8.163)	(8.217)
	Panel C: Receipt of any other program				Panel D: Amount of any other program			
Post layoff	0.086***	0.068***	0.068***	0.067***	125.757***	97.352***	98.979***	98.255***
	(0.005)	(0.005)	(0.005)	(0.005)	(10.617)	(10.534)	(10.725)	(10.877)
Post layoff*Max UI	-0.010***	-0.011***	-0.011***	-0.010***	-9.713**	-11.112**	-11.107**	-10.628**
	(0.002)	(0.002)	(0.002)	(0.002)	(4.543)	(4.835)	(4.916)	(4.988)
State	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year	No	Yes	No	No	No	Yes	No	No
Quarter	No	Yes	No	No	No	Yes	No	No
Year-Quarter	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	No	No	Yes	No	No	No	Yes

Notes: The table reports estimates and standard errors for coefficients β_1 and β_2 in Equation (1). Results are presented separately for the receipt of UI (Panel A), the amount of UI received (Panel B), the receipt of any other program (Panel C) and the amount received from any other program (Panel D). For each outcome, results are presented from different specifications that vary the set of covariates. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

of UI maximum benefit levels on the receipt of UI (Panel A) as well as the amount of UI benefits received (Panel B). We then group all other non-UI public programs for which SIPP reports information in a single category, and study treatment effects on their receipt (Panel C) as well as on the amount of benefit received (Panel D). For all our outcomes of interest, we present two different coefficient estimates. The first one is the one of the dummy variable equal to one in the 24 months after the layoff event (corresponding to β_1 in equation (1)), which captures the effect of job loss. The second one is the interaction between this dummy variable and the measure of benefit generosity (corresponding to β_2 in equation (1)), which captures the effect of UI benefit generosity and is our main regressor of interest.

To start with, the results confirm findings from previous studies that the entitlement to a more generous UI increases the length of benefit receipt. In the most complete specification, an additional $1,000 in maximum UI monthly benefits increases the likelihood of receiving UI by 2.2 percentage points (or approximately 17% of the increase in UI receipt that takes place in our sample after layoff) and increases the amount of UI benefits actually received by around $50 (or approximately 49% of the increase in UI benefit amounts that takes place in our sample after layoff) in the 24 months after layoff.[31] We also note that our

[31] The effect on benefit amounts is larger in relative terms, given that it captures both the intensive margin of longer benefit receipt and the intensive margin of higher UI entitlements.

estimates of treatment effects remain very stable for both outcome variables across different specifications where we vary the set of individual-level controls as well as the fixed effects.

Given that the standard deviation of maximum monthly UI benefits is around \$500, this means that a one standard deviation in maximum UI benefits leads to an increase in the probability of receiving UI by around 1.1 percentage points. In order to benchmark our results to those of earlier studies, we also compute elasticity estimates. To do so, we note that the average maximum UI monthly benefit during our sample period was equal to approximately \$1,850 (in 2011 values). This means that a 10% increase in benefit levels leads to an increase in the probability of UI receipt by around 3%. This is similar to the estimates in Card et al. (2015), Landais (2015) and Meyer and Mok (2007), while other studies in the US found larger estimates (Schmieder and von Wachter, 2016).

At the same time, we also find that more generous UI leads to a reduction in the take-up of other programs. For the time being, we look at the effects on the receipt and amount of transfers from all other non-UI public programs reported in the SIPP (see Section 2 for details). Our results indicate that an additional \$1,000 in maximum monthly UI benefits decreases the likelihood of receiving any other public program by around 1 percentage point in the 24 months after layoff. Given that individuals experience an increase of around 6.8 percentage points in the probability of receiving any non-UI type of welfare after layoff, this corresponds to an offsetting effect of around 15% from the more generous UI. Even in this case, results are very stable across model specifications.

The effect on non-UI benefit amount is very similar to the one on non-UI benefit receipt, when interpreting coefficients in relative terms. In particular, point estimates indicate that an additional \$1,000 in maximum monthly UI benefits reduces transfer amounts of any other non-UI public program by around \$10 in the 24 months after job loss (or approximately 11% of the overall increase in non-UI transfers that individuals experience in the 24 months after layoff). Looking jointly at the treatments effects of UI benefit generosity on both UI and non-UI transfer amounts, this means that around 20% of the cost associated with increasing UI benefit generosity (i.e. which was estimated at \$50, as discussed above) is offset by lower public transfers for other program types.

Figure 1 presents coefficient estimates from the event study analysis, where we substitute the simple dummy taking the value of one in the 24 months after layoff with a full set of monthly dummies before and after job loss (with the dummy for the month before layoff corresponding to the omitted category). This more flexible approach allows to check for parallel trends before layoff, but also to study the evolution of treatment effects over time after job loss. The results reveal that the effect of UI generosity on UI receipt (Panel A) and UI amounts (Panel B) materialize immediately after the layoff event. Treatment effects decrease sharply around 6 months after layoff and then more gradually to reach zero between 18 to 24 months after job loss. This is consistent with the functioning of the program.[32]

The results also indicate that coefficients before the layoff event are statistically significant. However, the magnitude of these pre-trends is small. This has been connected to recall bias with regard to the timing of job loss (i.e. note that most of the pre-trends are accounted for by trends in the 2-3 months before layoff, which can also lie in between interview rounds in the SIPP) (Hendren, 2017; Stephens, 2004).

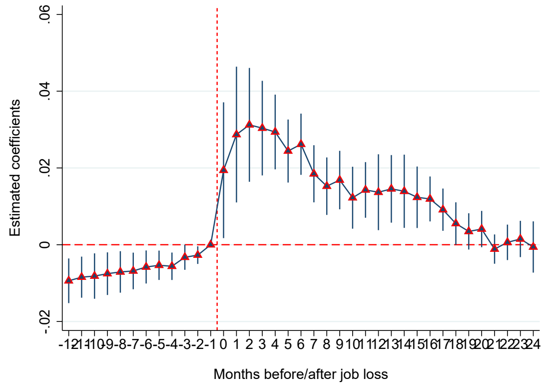
Event study estimates of the treatment effects UI benefit generosity on the probability of receiving any other non-UI program (Panel C) and the amount of non-UI public transfers received (Panel D) follow a similar, but reversed, pattern. Non-UI policy receipt sharply decreases after job loss. Coefficient estimates start closing around 4 months after job loss. However, they do not reach zero, but rather stabilize at 9-12 months after job loss. This means that, 24 months after job loss, while individuals that were granted more generous UI benefits are no longer more likely to be receiving UI, they are still less likely to receive other types of public programs. For these two outcomes, we do not find any evidence of pre-treatment trends. However, certain coefficients are imprecisely estimated, also given that they are smaller in magnitude compared to those analyzed above.

We end this sub-section by presenting some heterogeneous effects (Appendix Table B5). Focusing on treatment effects on the probability of receiving any other type of public program, we start by noting that program substitution materializes for all the groups considered in the analysis. Treatment effects are also very similar for most groups; including between men and women, individuals with and without some college education, individuals with and without children as well as younger and older individuals. We also find that treatment effects are similar if, rather than imposing the restriction that individuals need to be in a job for at least three months before layoff as we do for our baseline results, we extend this to at least six months of previous employment. Instead, we find larger treatment effects for individuals who are not married, compared to those who are married.[33] We also find that, if

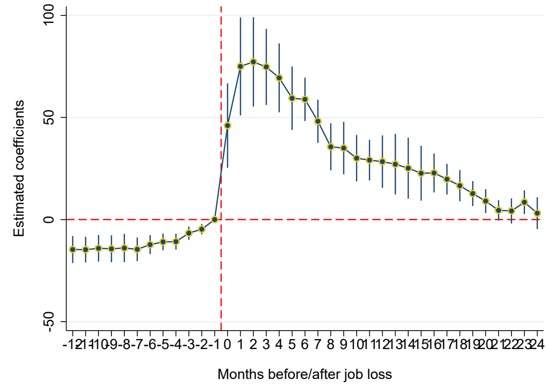
[32]Most individuals in the sample were eligible to UI for six months (i.e. maximum UI duration in most states under normal circumstances), but a series of extensions were introduced during the sample period that raised maximum UI duration up to 99 weeks (see Section 2).

[33]This might be because, for single individuals, income from UI represents a larger share of total household income (compared to married individuals, whose household income also depends on the partner's). As such,

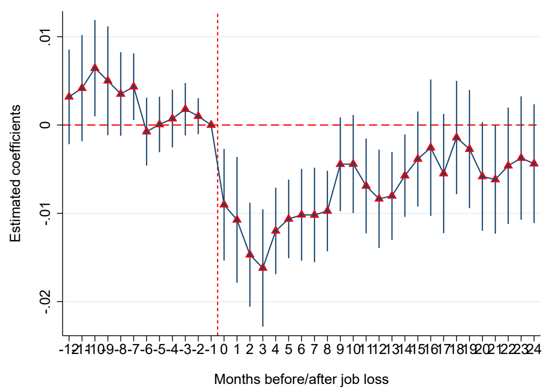
Figure 1: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of UI and any other programs



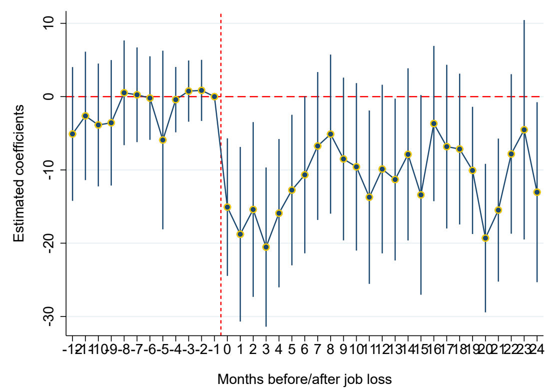
(a) Receipt of UI



(b) Amount of UI



(c) Receipt of any other program



(d) Amount of any other program

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on UI and non-UI benefits, analysing separately the effects on receipt (Panels A and C) and amounts received (Panels B and D).

we restrict the sample to individuals who spend at least six or 12 months in unemployment, program substitution is larger.[34]

5.B Results by program types

The results discussed above have shown that individuals that receive more generous UI are less likely to receive other types of transfers, with this substitution effect potentially lasting even beyond the end of UI eligibility. This section aims to investigate which programs

eligibility to other public programs might more tightly depend on UI generosity for unmarried individuals.

[34] As expected, this is also a group for which we observe a higher take-up of non-UI public programs after layoff, irrespective of the role of UI generosity.

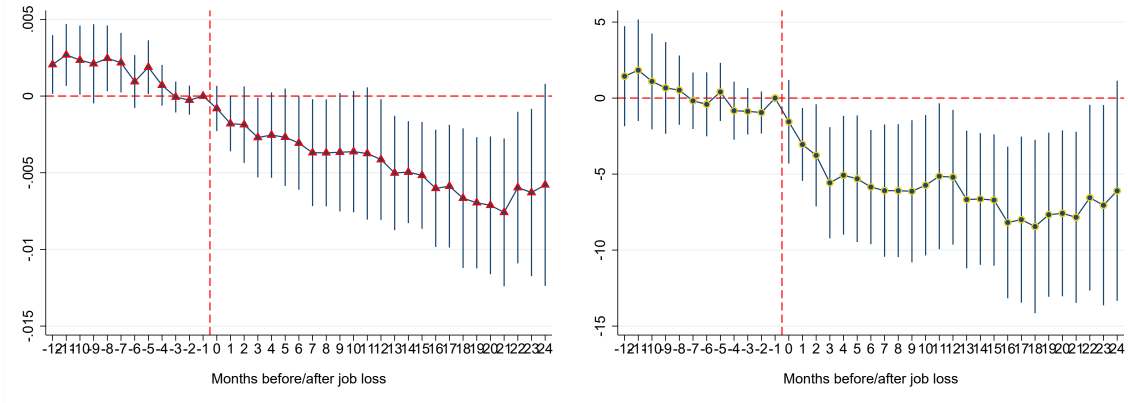
are behind this effect. Specifically, we will be looking at treatment effects on the receipt of Social Security, SNAP, TANF, WIC, SSI and any other programs. This will reveal the extent to which the program substitution effects documented above are driven by non means-tested programs (where only a behavioral mechanism can be at play), or, rather, by means-tested programs (where both mechanical and behavioral responses can be happening).

Figure 2 presents the event study estimates of the effects of UI benefit generosity on Social Security, while Appendix Table B6 presents the DiD estimates for all program types that will be analysed in this sub-section. The results show that the amount of Social Security transfers decreases sharply around the time of job loss for individuals receiving more generous UI (Panel B), in line with an overall lower likelihood of receiving any Social Security transfer (Panel A). For both transfer amount and receipt, the effects increase with time after layoff, until 18-20 months after job loss. At that point, coefficient estimates stabilize. This means that the substitution effect between UI and Social Security is of long-term nature and it outlasts the period of UI eligibility. To provide a sense of the magnitude of these effects, we note that the take-up of Social Security increases sharply after layoff in our sample, irrespective of UI generosity. This is in line with evidence showing that laid-off individuals might leave the labour force to either retirement or to receive disability benefits (Inderbitzin et al., 2016; Lindner, 2016). More generous UI partially counterbalances this effect: an additional $1,000 in maximum monthly UI benefits decrease by around 30% the increase in Social Security receipt and transfer amounts that takes place after job loss.[35]

We now further investigate this result. To start with, the SIPP differentiates between Social Security received for the respondent, or on behalf of a child in the household. Both Social Security types decrease as a result of more generous UI. However, the effect is larger when looking at own Social Security receipt (Appendix Figure A2). The SIPP also reports information for the reasons for Social Security receipt, with the two main options corresponding to retirement and disability. We find that both self-reported reasons for Social Security receipt decline at job loss and that the effect persist even 24 months after layoff (Appendix Figure A3). Given that early retirement decisions seem to be an important factor behind our results, we replicate our results for the population aged 50 and above at the

[35]We also note that for Social Security receipt, coefficients before the time of job loss are significant, indicating some possible pre-trends. This can be connected to recall bias between survey rounds, as discussed above. However, it is also possible that these pre-trends indicate that individuals can anticipate their dismissal and are sufficiently aware of the rules governing UI to modify their behavior in advane (e.g. apply to Social Security before layoff in states where they can expect to receive low UI upon being dismissed) (Light and Omori, 2004; Lusher et al., 2022). Under both scenarios, the implications would be that the results presented in the paper would be conservatively estimated (Lindo et al., 2023).

Figure 2: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of Social Security



(a) Social security receipt

(b) Social security amount

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on overall Social Security, analysing separately the effects on receipt (Panel A) and amounts received (Panel B).

time of job loss (Appendix Figure A4). As expected, treatment effects are much larger in magnitude (around four times than the baseline figures).

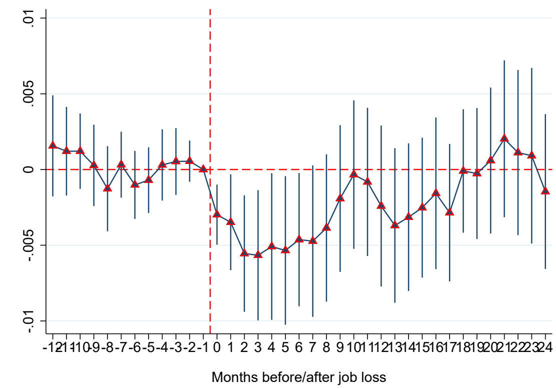
These findings show that more generous UI might generate long-lasting savings in other areas of public spending. These effects partially contradict those from existing studies. Specifically, the literature on the interaction between UI and early retirement has found that longer UI entitlements create incentives for individuals to retire earlier (the so-called unemployment tunnel) (Inderbitzin et al., 2016; Kyyrä and Ollikainen, 2008; Lammers et al., 2013).[36] However, we can reconcile our findings by noting that we study the effects of maximum benefit levels, rather than maximum benefit duration. While longer UI can create incentives to delay labor market re-entry until the time of retirement, higher UI levels, holding duration constant, can increase the opportunity costs of leaving the labor force (i.e. which comes with the loss of UI).[37] We will return to this interpretation in Section 5.C.

We now turn to analysing the results for selected means-tested programs. Starting with SSI, we do not find any notable effect of UI generosity on either the probability of

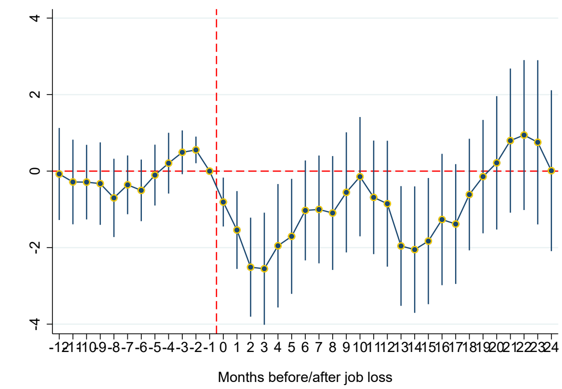
[36]The evidence on the interaction between UI and disability insurance is instead quite mixed, with studies finding only weak evidence of substitution between UI and disability insurance (Lindner, 2016; Mueller et al., 2016; Rutledge, 2013), possibly because the limited overlap in the two populations of eligible individuals.

[37]It is also worth mentioning that most of the previous evidence on the interactions between UI and early retirement comes from European countries, where UI is generally more generous and it might be easier for older individuals to rely on the welfae state until the time of retirement.

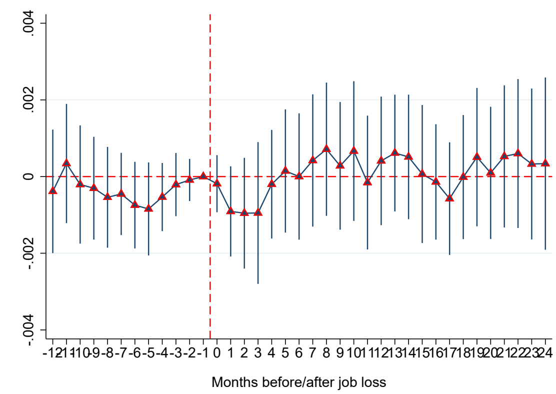
Figure 3: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of means-tested programs



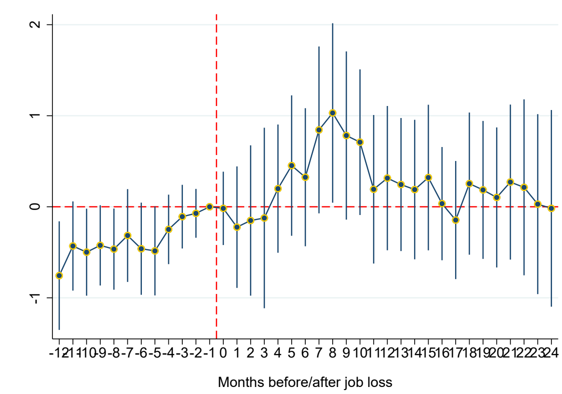
(a) SNAP receipt



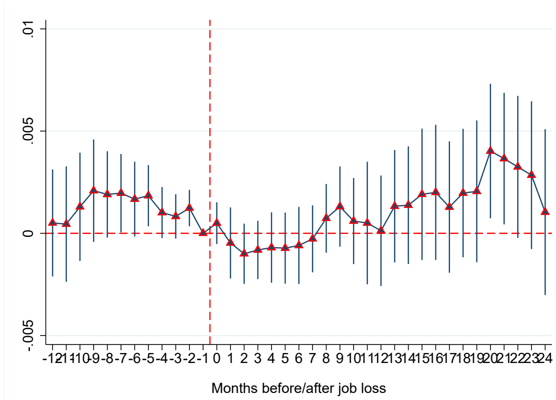
(b) SNAP amount



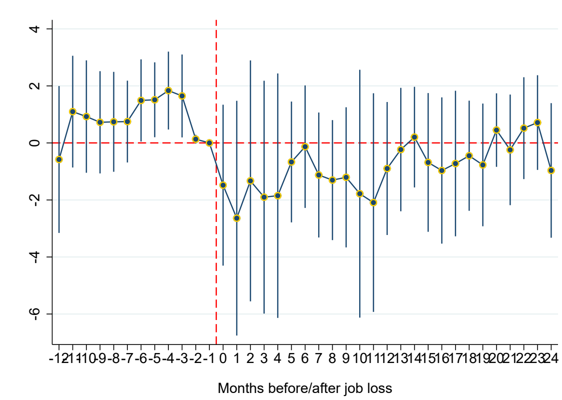
(c) TANF receipt



(d) TANF amount



(e) WIC receipt



(f) WIC amount

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on selected means-tested programs, analysing separately the effects on receipt (Panels A, C and E) and amounts received (Panels B, D and F).

benefit receipt or benefit amount after job loss (Appendix Figure A5). Instead, we find that more generous UI has a small negative effect on SNAP transfer receipt and transfer amounts (Panels A and B of Figure 3). However, these effects disappear between 12 and 18 after job loss, when the effect on UI receipt also ends (see Figure 2). We instead do not find any effect of UI benefit generosity on TANF benefit receipt or transfer amounts (Panels C and D of Figure 3).[38] Finally, for WIC, we see a small, transitory and imprecisely-estimated reduction in transfer amounts among individuals receiving more generous UI (Panel F of Figure 3), but no effects on the probability of transfer receipt (Panel E of Figure 3).[39]

We end this sub-section by looking at the effects of UI generosity on a miscellaneous group of transfers (Figure 4).[40] We see a pattern similar to the one observed for means-tested programs: more generous UI decrease the likelihood of receiving other transfers and the amount of transfers received. However, the effect disappears as soon as eligibility to standard UI ends (i.e. around six months after job loss). We further unpack this result to see which specific transfer types lie behind it. We first analyze participation in other income support programs, and observe a reduction in the probability of receiving income for Child Support and Worker Compensation (Appendix Figure A6). Looking at the effects of UI generosity on the probability of receiving other types of pensions (i.e. different from Social Security), we observe a decline in the probability of receiving pension from the military (Appendix Figure A7). Finally, we look at the effects on other types of transfers (i.e. including non-public transfers). We observe a short-term decrease in the probability of receiving money from relatives as well as having income from casual earnings (Appendix Figure A8).

These results show that more generous UI leads to a short-term decrease in the receipt of certain means-tested programs, most notably SNAP.[41] This could be due to either

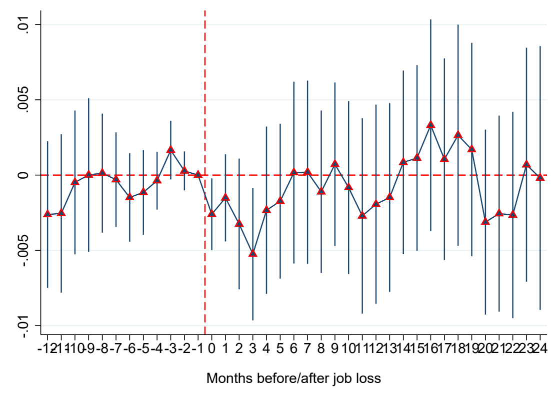
[38]For TANF, we see a small and transitory increase in benefit amounts between 6 and 10 months after job loss. This is not accompanied by a parallel increase in the probability of benefit receipt, so it would imply higher benefit amounts among individuals who are receiving some TANF transfer.

[39]We observe a small increase in the probability of receiving WIC starting from 20 months after job loss, but do not have a clear interpretation of this finding.

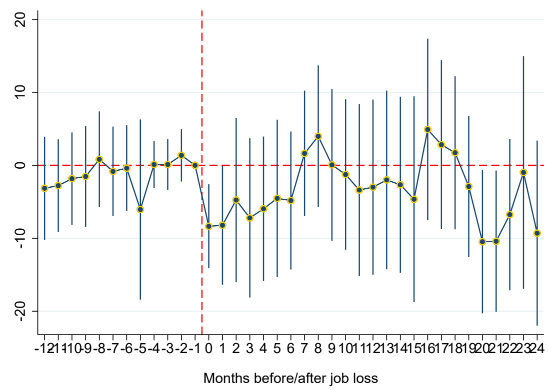
[40]In particular, we include in this miscellaneous group all program types that entered the non-UI category in the baseline results (Table 2 and Figure 1), but have not been separately analyzed so far.

[41]The fact that we find program substitution for SNAP, but not for TANF, is puzzling, also given that income deduction rates (including for income from UI) are higher for TANF compared to SNAP (see Section 2 for details). However, we also note that TANF policy take-up is substantially smaller than SNAP take-up (Ganong and Liebman, 2013; Loprest, 2012). This is also true in our sample, where we observe that the share of the population reporting receiving SNAP is around six times larger than the share of the population reporting TANF receipt, both before and after the time of job loss (see Appendix Table B1). We also see that there is a very tiny overlap between the population receiving UI and individuals who receive TANF. While around 7% of UI recipients in our sample report also receiving SNAP in the same month, this share is only equal to less than 1% for TANF. This might be why we see substitution for SNAP, but not for TANF.

Figure 4: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of other programs



(a) Other programs



(b) Other programs

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on a residual category composed of programs not included elsewhere in the paper, analysing separately the effects on receipt (Panel A) and amounts received (Panel B).

a reduction in the likelihood of meeting the programs' eligibility requirements (e.g. because of the higher income from UI), or a reduction in program take-up among eligible individuals. In the next sub-section, we will provide evidence to disentangle the two hypotheses.

5.C Labor market and income mechanisms

The results presented above have shown that the reduction in non-UI benefits among individuals receiving more generous UI is driven by two main factors: a short-term reduction in the probability of receiving some means-tested programs, and a medium to long-run reduction in the probability of receiving Social Security. But how do these effects materialize? We now turn to the labor market to see how laid-off individuals and their household members react to being eligible to a more generous UI.

The results presented in Figure 1 have already shown that a more generous UI leads individuals to receive UI for longer. This is a standard finding in the literature, even though the elasticity estimate that we find are at the lower end of previous studies for the US. Figure 5 confirms these results.[42] In particular, we find that a more generous UI reduces the probability of re-employment after job loss (Panel A) and increases the likelihood of being in

[42] These results are also available in a simple DiD format in Appendix Table B7.

unemployment (Panel B).[43] However, we also find a negative effect of UI generosity on the probability of being inactive (or outside of the labor force, Panel C). This effect materializes soon after job loss, but it increases over time. The effect of UI generosity on the probability of leaving the labor force is generally overlooked in the literature (e.g. administrative data sources generally do not allow to differentiate between unemployment and inactivity, unless individuals are also receiving UI). The likely mechanism is that more generous UI increases the incentives to remain in the labor market (i.e. in order not to lose eligibility to UI).[44]

The fact that we find a negative and persistent effect of UI generosity on inactivity also has important implications for our employment and unemployment results. Specifically, the UI-induced increase in unemployment documented above disappears starting 18 months after job loss. This matches the findings on UI receipt (see Panel A of Figure 1) and it is consistent with the fact that individuals are no longer eligible to UI at that point in time (i.e. even when federal or state extensions were implemented). Instead, the negative effect on inactivity still persists around that time. This implies that, starting from 20 months after job loss, treatment effects on employment change sign: individuals who received more generous UI are now more likely to hold a job. The same pattern is observed for monthly earnings (Panel D), who first decrease and then increase in response to UI generosity.

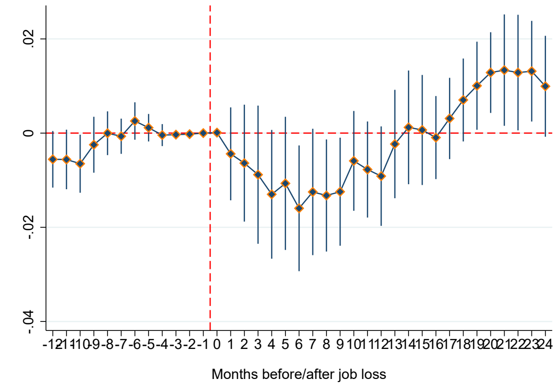
These findings can be interpreted in light of the fact that a more generous UI provide incentives for individuals to remain in the labour market after job loss. This, in turn, can have long-lasting effects: as individuals who receive UI need to look for a job, they are more likely to find one and therefore to remain in the labour market even after the end of UI eligibility. This interpretation is consistent with previous studies that have found that more generous UI increases employment probabilities in the long run, potentially as a result of improved job search (Caliendo et al., 2013; Scrutinio, 2020).

What are the implications of these results in terms of income? How do other

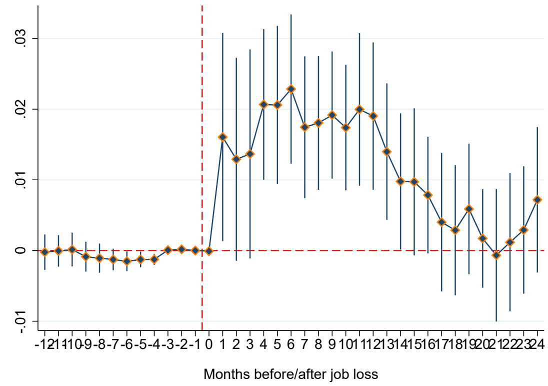
[43] For the labor market results, note that the regressions omit the three months before layoff, as we impose that our sample is continuously in employment during this period (see Section 3).

[44] We also investigate the effect on inactivity by looking at the self-reported reasons that individuals provide for being outside of the labor force (Appendix Figure A9). We find that inactivity due to retirement (Panel A) and disability (Panel B) both decrease, in line with the results discussed in the previous sub-section in terms of Social Security receipt. We also find that individuals receiving a more generous UI are less likely to being inactive due to care responsibilities (Panel C). This might be because the more generous UI increases the implicit cost of leaving the labor market for taking care of these duties, but also because it allows individuals to buy care services in the market. Instead, we find that a more generous UI increases the probability of not being in the labor force because of being enrolled in education (Panel D). This finding is reassuring, as it confirms previous results showing that more generous UI (in the form of longer UI entitlements) increase enrollment in post-secondary education in the US (Barr and Turner, 2014).

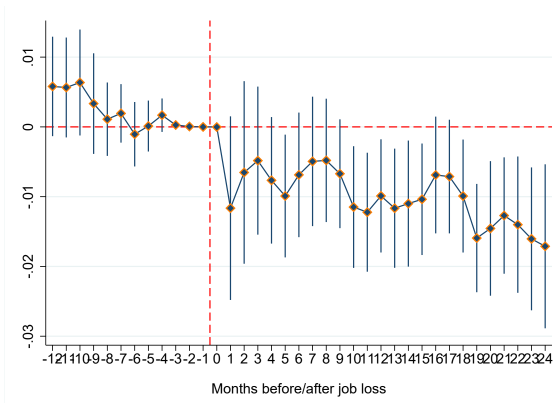
Figure 5: Event study estimates of the effects of UI benefit generosity on labor market status and individual earnings



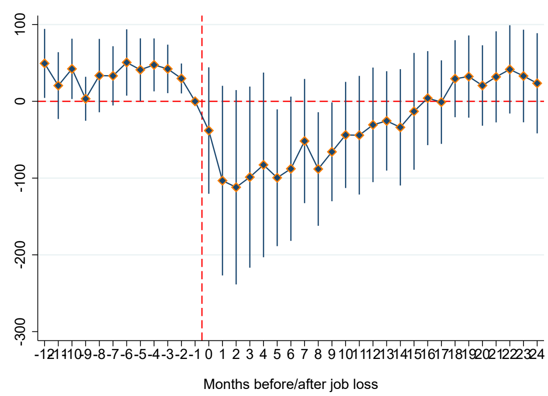
(a) Employment



(b) Unemployment



(c) Inactive



(d) Monthly earnings

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on different dummies for labor market status (Panels A to C) and on individual monthly earnings (Panel D). Individual earnings are set to zero for individuals not in employment.

household members react to the individuals' receipt of a more generous UI, and the associated labor market responses presented above? We answer these questions by presenting results on treatment effects on personal and household income. We use the already-available income variables in the SIPP, according to which total income (for either the individual or the household) is composed of earned income, property income and income from transfers.[45] Figure 6 presents the results on the effects of UI generosity on earned, transfers and total

[45]This corresponds to the sum of UI and non-UI benefits. As a result, the effects on individual income from transfers should roughly match those we obtain by combining the effects on the amount of UI (Panel B of Figure 1) and the amount of any other programs (Panel D of Figure 1). Similarly, results on individual earned income should match those on monthly earnings presented in Panel D of Figure 5.

income, while Appendix Figure A10 shows the results on property income. Appendix Table B8 presents all the results in the simple DiD format. For each income variable, we present in parallel results on individual and household income. This is done to provide suggestive evidence of possible spillover effects on other household members.

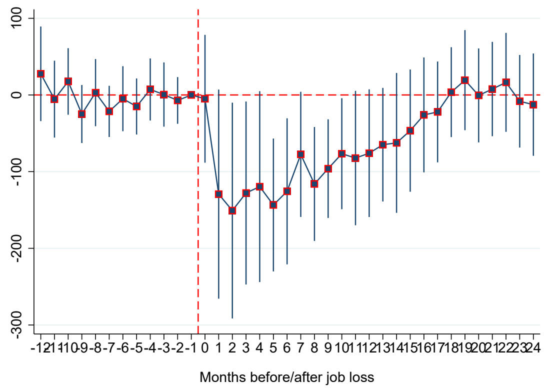
We find that individual income sharply decreases among individuals receiving a more generous UI (Panel A of Figure 6). This is in line with the drop in employment due to the more generous UI, and it matches the estimates on monthly earnings presented above (Panel D of Figure 5). We see that household earned income follows a similar pattern. However, the drop in household earned income is smaller than for individual earned income at job loss, and the recovery is faster (Panel B). This is consistent with the fact that other household members increase their labor supply in response to the longer unemployment spell of the laid-off individual, similar the so-called added worker effect (Lundberg, 1985).[46] As a result, already 12 months after job loss, we do not see any discernible difference in household earned income based on the level of UI generosity to which the laid-off individual was eligible.

Looking at the evolution of income from transfers, we see that this increases for the individual around the time of job loss, before gradually decreasing (Panel C). This trend matches the results discussed above in terms of amount of UI and non-UI benefits (i.e. summing the results in panels B and D of Figure 1). Household income from transfers also follows the same pattern (Panel D). However, the increase in household income from transfers is smaller and the decrease is larger. This might be because other household members find a job, as discussed above, and reduce their reliance on other government transfers. As a result, the effect of UI generosity on household transfer income is negative starting from around 18 months after job loss. This means that, after accounting for spillover effects within the household, a more generous UI decreases the receipt of total (i.e. UI plus non-UI) transfers in the households of eligible members in the medium to long run.

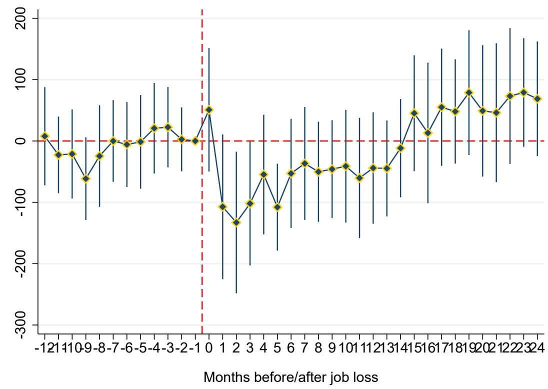
We also find that property income does not vary with UI generosity at either the individual or household level (Appendix Figure A10). The effects on total income thus results from the opposing effects observed between earned and transfers income as well as the different behaviors registered by the laid-off individuals and other household members. Specifically, the drop in earned individual income is partially compensated by the increase in income from transfers for the individual. As a result, total individual income registers a smaller fluctuation around the time of job loss (Panel E), compared to individual earned

[46] The main difference is that, here, we are not documenting a labor supply increase of other household members in response to the layoff event, but, rather, to the longer unemployment spell.

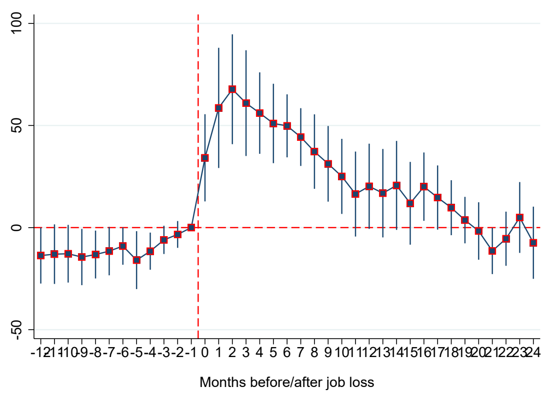
Figure 6: Event study estimates of the effects of UI benefit generosity on personal and household income



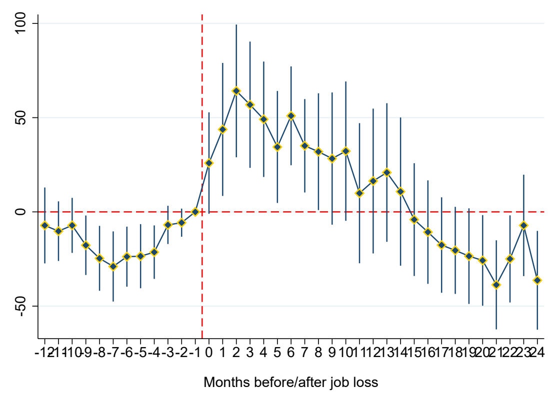
(a) Earned income: Individual



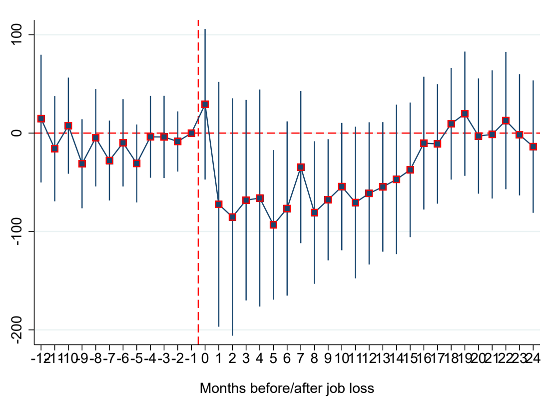
(b) Earned income: Household



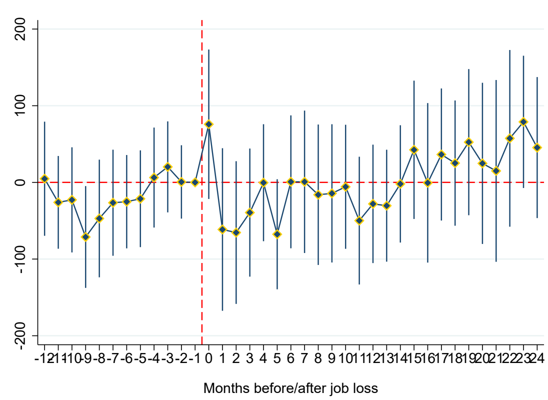
(c) Transfers income: Individual



(d) Transfers income: Household



(e) Total income: Individual



(f) Total income: Household

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted for earned income, transfers income and overall income, measuring these variables at either the individual level (Panels A, C and E) or at the household level (Panels B, D and F).

income. In addition, and given that other household members increase their labor force participation to compensate for the longer unemployment spell of the laid-off individual, total household income is barely affected by UI generosity around job loss (Panel F).

We use the results presented in this sub-section to intepret the finding of the previous sub-section on program type. Specifically, the effects on Social Security receipt are easily interpretable in light of the labor market responses to UI generosity we presented above (most notably, the decrease in inactivity). Given that Social Security is not means-tested, the reduction in Social Security receipt follows entirely from this behavioral response. For the means-tested programs, instead, different mechanisms could be at play. However, the results in this sub-section have shown how household income is barely affected by UI generosity. This means that eligibility to these programs remains the same. The substitution effect that we observe for certain means-tested programs (e.g. SNAP) can therefore be driven only by a reduction in benefit take-up, whereby individuals already receiving generous UI decide not to apply to other programs. This is consistent with evidence on incomplete policy take up of social benefits (Kleven and Kopczuk, 2011; Ko and Moffitt, 2022).

In line with this interpretation, we present evidence on self-reported reasons to apply to certain means-tested programs. Specifically, we look at the effects of UI generosity on the probability of reporting having applied to TANF or SNAP for economic reasons.[47] The results reveal that the reduction in the probability of receiving SNAP documented above is driven by the fact that individuals who receive more generous UI are less likely to having applied to SNAP for economic reasons (Panel A of Appendix Figure A11). Instead, for TANF, we do not find any treatment effects on the reasons to apply (Panel B of Appendix Figure A11), in line with the absence of effects on program participation documented above.

5.D Robustness tests

We now provide a series of robustness tests to confirm the findings presented above (Table 3, where in the first row we reproduce our baseline results as reported in Table 2). For ease of exposition, these tests are presented only for the four main outcomes of interest presented in Section 5.A (i.e. receipt and amount of both UI and non-UI benefits).

To start with, we augment our specification by including state-level controls for macroeconomic variables (i.e. GDP growth, unemployment rate, income per capita and

[47] We group together individuals who reported having applied to these programs (i) because of job losses or reduction in wages, and (ii) because of the reduction in other sources of income.

poverty rates) institutional controls (i.e. minimum wage level, trade union membership rate and collective bargaining coverage) and the generosity of other policies (i.e. TANF, SNAP, EITC, SSI and Worker compensation).[48] This is to check if our measure of UI generosity is instead capturing other factors related to economic conditions, other institutional factors or other policies' generosity. These controls are included first separately (rows 2 to 4) and then jointly (row 5). Effects remain very stable across all specifications.

We then test alternative sample definitions. As mentioned in Section 3, we include in the main sample individuals who are separated from a job after at least three consecutive months in employment. However, we do not condition on the reasons for absence from employment nor on the individual reporting any reason for job loss. This is done in order to limit concerns of measurement error and sample selection (Lindo et al., 2023).[49]

While we believe this approach is the most suitable in the present context, it comes with the risk of including in the sample also individuals who are not dismissed or eligible for UI. To test how much this could affect our results, we restrict the sample to individuals who report at least one reason for job loss (row 6) and to individuals reporting the reasons for job loss selected in Kuka (2020) (row 7).[50] We then impose the same sample restrictions as in Hsu et al. (2018) (row 8).[51] In all these cases, results are slightly larger than our baseline findings, potentially signalling that our baseline results are conservatively estimated.

We further restrict our analysis to individuals who lost their job because of their employer went bankrupt or sold the business (i.e. a subset of the reasons included in Kuka (2020), row 9). Sample size becomes small (around 1.5% of our baseline sample), and, as a result, coefficients are imprecisely estimated. However, results are either very similar in magnitude (for UI receipt and amounts) or, again, larger (for non-UI receipt and transfers) than our baseline estimates. We also conduct a placebo test by looking at the sample of unemployed who report having quit their job, and are thus not eligible for UI (row 10). Reassuringly, we find no effects of UI generosity on either UI or non-UI receipt.

[48]We prefer controlling for other programs in terms of their generosity, rather than spending levels, given that we are interested in holding constant others programs' regulations, rather than spending (which can instead react to changes in UI generosity, as shown in the main analysis).

[49]An additional reason for not imposing any restriction in terms of the reason for job loss is that these variables cannot be reconstructed consistently for all SIPP waves between 1990 and 2008. For this reason, in this robustness test we will focus only on SIPP panels from 1996 to 2008.

[50]Kuka (2020) includes in the sample individuals who reported having stopped working because of "layoff", "employer bankrupt", "employer sold business" or "slack work or business conditions"

[51]This means not including any restriction on the reasons for layoff, but imposing a stricter definition of unemployment status (i.e. RMESR only equal to 6 in the SIPP).

Table 3: Robustness tests for the effects of UI benefit generosity on the receipt and amount received of UI and any other programs

Row		UI receipt	Amount of UB	Receipt of any other benefit	Amount of any other benefits
1	Baseline	0.022***	50.293***	-0.010***	-10.646**
		(0.006)	(8.236)	(0.002)	(4.912)
2	Macro controls	0.022***	50.471***	-0.011***	-10.987**
		(0.006)	(8.153)	(0.002)	(4.862)
3	Institution controls	0.022***	50.028***	-0.011***	-10.892**
		(0.006)	(8.312)	(0.002)	(4.961)
4	Policy controls	0.023***	52.559***	-0.011***	-10.022*
		(0.006)	(8.283)	(0.002)	(5.406)
5	All state-level controls	0.023***	52.828***	-0.012***	-10.301*
		(0.006)	(8.364)	(0.002)	(5.384)
6	Reason for jobloss: Any	0.031***	69.142***	-0.017***	-11.582
		(0.008)	(10.002)	(0.004)	(8.889)
7	Reason for jobloss: as in Kuka (2020)	0.055***	142.852***	-0.024***	-10.664
		(0.019)	(29.465)	(0.005)	(7.804)
8	Sample: as in Hsu et al (2023)	0.036***	88.536***	-0.026***	-15.156
		(0.011)	(15.755)	(0.007)	(9.576)
9	Reason for jobloss: Firm closure	0.020	52.977	-0.030	-25.675
		(0.020)	(33.537)	(0.024)	(23.898)
10	Reason for jobloss: Quit	-0.008	-0.861	0.005	-8.071
		(0.009)	(7.320)	(0.007)	(11.238)
11	1996-2008 panels	0.023***	47.194***	-0.009***	-11.865**
		(0.005)	(7.420)	(0.003)	(5.459)
12	1990-2004 panels	0.022***	48.124***	-0.009***	-8.360
		(0.006)	(6.848)	(0.003)	(7.011)
13	2008 panel	0.018*	49.350***	-0.012***	-13.915*
		(0.010)	(14.297)	(0.004)	(7.019)
14	Low unemployment rates	0.024***	46.087***	-0.012***	-16.979*
		(0.003)	(5.104)	(0.004)	(8.563)
15	High unemployment rates	0.021**	57.865***	-0.006*	-6.228
		(0.010)	(13.717)	(0.003)	(8.042)
16	No individual FEs	0.020***	44.702***	-0.008**	-9.386*
		(0.005)	(7.638)	(0.003)	(5.489)
17	Controlling for benefit duration	0.022***	50.293***	-0.010***	-10.646**
		(0.006)	(8.236)	(0.002)	(4.912)
18	Max dur*Max ben	0.022***	50.293***	-0.010***	-10.646**
		(0.006)	(8.236)	(0.002)	(4.912)
19	Duration as treatment	0.003	7.697**	-0.002	-3.275***
		(0.002)	(2.948)	(0.002)	(0.925)

Notes: The table reports estimates and standard errors for coefficients β_2 in Equation (1). Results are presented separately for the receipt of UI (first column), the amount of UI received (second column), the receipt of any other program (third column) and the amount received from any other program (fourth column). For each outcome, results in each row are from specifications that conduct different robustness tests (see text for details). Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

To account for changes in survey design, we restrict the analysis to the period in which the SIPP survey is more comparable (panels 1996 to 2008, row 11). We also split our sample between the years before and after the global financial crisis (SIPP panels 1990 to 2004 and 2008, rows 12 and 13 respectively), when UI regulations were also changed. Results are in all cases very similar to our baseline findings. Similarly, we split the sample between periods of high and low unemployment (rows 14 and 15), to check if the different composition of the sample of unemployed could affect our results.[52] However, results are once again in line with our baseline estimates.[53]

We then conduct a series of miscellaneous robustness tests. First, we replicate our findings, but without including individual fixed effects (row 16). If unobserved heterogeneity was driving our results, we should see a large difference between our preferred estimates and those without individual fixed effects. Instead, results are virtually identical. We also play with the definition of the treatment indicator (i.e. maximum UI levels). First, we control for maximum UI duration, to check if benefit extensions (rather than levels) were driving our results (row 17). Then, we change our treatment indicator to equal the product between maximum benefit level and duration, as in Hsu et al. (2018) (row 18). Results are again very similar. This is because benefit duration, in itself, does not explain our results (row 19).

We conduct three final tests. First, we add a control group of individuals who remain employed throughout the 36 months of length of our panel. This corresponds to a triple-differences design as in Hsu et al. (2018) and Lindo et al. (2023). This has the benefit of allowing to control for systematic differences between laid-off individuals in different states through the inclusion of state-by-group fixed effects (i.e. one group corresponding to laid-off individuals and the other including the always employed) as well as to control for differential trends across states through the inclusion of state-by-quarter-by-year fixed effects. We present the results of this exercise in Appendix Table B9, where we vary the set of fixed effects included and present the results of the effects of UI generosity on both the unemployed (i.e. our group of interest, corresponding to the triple interaction between (i) after layoff, (ii) maximum UI, and (iii) unemployed) as well as for the employed (i.e. corresponding to the double interaction between (i) after layoff and (ii) maximum UI). The results on the unemployed are almost identical to our baseline results, and vary very little as

[52] These are defined based on whether the unemployment rate was above or below 6 per cent in a given state and year, which roughy splits the sample into two equal parts.

[53] Note that effects on both the receipt and amount of non-UI benefits are smaller in periods of high unemployment. However, this needs to be interpreted in light of the fact that in these models the effect of the layoff event (β_1 in Equation (1)) is also smaller.

we add additional fixed effects. Instead, for the employed, we find very little and generally non-significant coefficient estimates. This is reassuring, as our identification strategy hinges on the assumption that employed individuals are not affected by changes in UI generosity

Second, we aim to address concerns related to measurement error. This is important in light of the evidence that survey respondents mis-report information on benefit receipt (Meyer et al., 2015). Given that information on benefit receipt is used to construct the outcomes variables in the present analysis, this should not lead to attenuation bias of coefficient estimates. We confirm this with our last robustness test. Specifically, we exploit the fact that the SIPP collects information on benefit receipt both in a specific month as well as over the reference period. Given how SIPP interviews are structured, the latter corresponds to the 4-month interval between interview rounds (see Section 3 for details). This last measure assigns the value of one to all the months in the reference period, if the individual reports having received the benefit at least in one of the four months. As such, this outcome of interest will be mis-measured in all those circumstances in which the individual does not receive the benefit throughout the entire 4-month window. We compare results obtained with the monthly measure of benefit receipt to those obtained with this 4-month measure. This 4-month measure is not available for all benefit types, but we are nevertheless able to compare results on UI receipt as well as the main non-UI benefits (i.e. Social Security, SSI and SNAP).[54] We compare results from these two variables in Appendix Figure A12, and find that they are virtually identical for all the selected outcomes of interest.

Third, we address recent concerns raised in the literature over the use of two-way fixed effect models with staggerred DiD (Borusyak et al., 2024; Callaway and Sant'Anna, 2021). As mentioned in Section 4, there is no obvious way of adapting recent advancements in the literature to the present context, given the continuous nature of the treatment indicator as well as the fact that UI benefit changes happened frequently in most states.[55] For these reasons, in the main analysis we use the standard two-way fixed effect estimator as in Lindo et al. (2023), among others. However, for a sub-group of our sample, we can define more easily treated and control observations, construct a binary treatment indicator and compare results from alternative DiD estimators.[56] Using these two groups of states and observations from

[54]The information is available only from the 1996 SIPP panel onwards.

[55]This would likely require defining big policy changes and compare the effects of those to smaller policy changes. However, we lack an understanding of what a big policy change could look like (something that is also likely to be both time- and state-specific) and there is also no clear prediction from the theory on whether a non-linear relationship exists between UI benefit changes and the outcome of interest.

[56]Specifically, we look at a period with relatively few maximum UI benefit changes (2003-2008, roughly corresponding to the years between the Dotcom bubble and the Global financial crisis). During this interval

both before and after the policy change, we run the analysis on the main outcomes of interest using the two-way fixed effect model as well as estimators by Callaway and Sant'Anna (2021) and Borusyak et al. (2024). Sample size is smaller and coefficients should not be necessarily comparable to those from the baseline findings, owing to differences in sample composition and treatment definition. However, the three models deliver results that are consistent with those presented above and also very similar to each other (Appendix Table B10).

6 Implications for Optimal Unemployment Insurance

We use the estimates obtained in the paper to assess the implications of our results in terms of optimal UI levels. Specifically, we follow the standard framework for optimal UI introduced by Baily (1978) and Chetty (2008) and adapt it to allow for the interaction between UI and other public transfers as in Lindner (2016). We start the exposition with the presentation of the standard Baily-Chetty framework, before introducing the interactions between UI and other programs and discussing the implications in terms of welfare.[57]

The model starts with a worker who becomes unemployed at time $t = 0$. The model assumes continuous time, where each period is denoted by t and the individual retires at time T. The worker chooses search effort s_t, which is normalized to equal the rate of arrival of job offers. Job search comes with a search cost $\psi(s_t)$, which is assumed to be increasing and convex in search effort. When the individual is unemployed, she receives UI denoted by b_t (as well as any other source of non-labor income), consumes $c_{u,t}$ and derives utility $u(c_{u,t})$. When the individual is employed, she receives a wage w but needs to pay taxes τ. Consumption when employed is denoted by $c_{e,t}$, which results into utility $v(c_{e,t})$. The trade-off of the worker is given by the fact that higher search effort will increase the likelihood of exiting unemployment, but it comes with a search cost.

The social planner can set UI levels in order to maximize the unemployed persons' lifetime utility subject to a budget constraint, knowing that the unemployed will react to

of time, five states (i.e. Alaska, Florida, Louisiana, New York and Tennessee) did not experience any change in maximum UI levels. Two states, Arizona and the District of Columbia, experienced instead only one change in UI benefits (which took place in 2005 in both states). This policy change is also comparable in size, given that it corresponded to a 17.1% increase in maximum benefit levels in Arizona (from $205 to $240) and a 16.2% increase in the District of Columbia (from $309 to $359).

[57]The discussion in this section will present the model in terms of welfare effects to changes in UI levels, rather than with respect to changes in UI duration, consistently with the empirical focus of the paper. To simplify the notion, presentation of the Baily-Chetty model follows the exposition in Schmieder and von Wachter (2016). Accordingly, we will assume that there is no savings, that wage offers are high enough such that any job offer will be accepted and that there is no discounting.

benefit levels by adjusting search efforts. Specifically, the social planner can set UI benefits b_t to be equal to b for P periods after unemployment (with $P < T$). The unemployed individual will then consume UI and any other non-labor income for the first P periods, and only the non-labor income afterwards. The budget constraint imposes that total tax revenues need to be equal to total spending on UI plus other government spending E. Denoting with B the length of UI receipt (with $B <= P$) and with D the time spent out of employment (with $D >= P$), the budget constraint of the social planner is equal to $(T - D) * \tau = Bb + E$.

Solving the model provides for the following expression capturing the welfare effects of marginally increasing UI levels (Schmieder and von Wachter, 2016):

$$\frac{\partial W}{\partial b}\frac{1}{Bv'(c_e)} = \underbrace{\frac{u'(c_u) - v'(c_e)}{v'(c_e)}}_{Insurance\ Value} - \underbrace{(\eta_{B,b} + \eta_{D,b}\frac{D}{B}\frac{\tau}{b})}_{Efficiency\ \ Costs} \qquad (2)$$

The first term on the right hand side is the insurance value of UI. This corresponds to the gap in marginal utility between UI recipients and the employed. Assuming that $c_{u,t}<c_{e,t}$ and that the utility functions are increasing and concave in consumption, this gap is positive. Intuitively, it measures the welfare gain from transferring resources from the employed to the unemployed, absent any behavioral response to UI benefit receipt. In practice, this first term is proxied with the flow drop in consumption at the time of job loss (denoted with $\frac{\Delta C}{C}$) multiplied by the coefficient of relative risk aversion (denoted with γ).[58]

The second term on the right hand side expresses the efficiency costs of increasing UI levels. This cost has two components. First, individuals will react to higher b by receiving UI for longer. This is captured by $\eta_{B,b}$, which is the elasticity of UI benefit levels to the length of UI receipt ($\eta_{B,b} = \frac{\delta B}{\delta b}\frac{b}{B}$). Second, individuals will also remain out of employment, and therefore will not pay taxes, for longer. This is denoted by $\eta_{D,b}$, which is the elasticity of UI benefit levels to time spent out of employment ($\eta_{D,b} = \frac{\delta D}{\delta b}\frac{b}{D}$). This last term is multiplied by $\frac{D}{B}\frac{\tau}{b}$, which adjusts the costs of non-employment in terms of lost tax revenues.

This framework can be adapted to multiple extensions, including the interaction between UI and non-UI benefits. We follow the approach proposed by Lindner (2016) in the context of disability insurance and apply it to our more general context. This requires assuming that the unemployed individual, while receiving UI, can also apply to other non-

[58]This follows from a Taylor approximation under the assumption that the third order derivatives of the utility functions are small (Chetty, 2008).

UI benefits and receive the non-UI benefit amount g_t.[59] Receipt of non-UI benefits is not automatic, but it requires the individual to apply. Applying to non-UI benefits comes with a cost $\Phi(p_t)$, which is decreasing and convex in the success probability. If the application to non-UI benefits is successful, the individual will consume UI and non-UI benefits (in addition to any other non-labor income). The individual optimization problem includes an additional component, which corresponds to the application probability (p_t).

It can be shown that, once this additional element is considered, the optimization problem becomes the following (Lindner, 2016):

$$\frac{\partial W}{\partial b}\frac{1}{Bv'(c_e)} = \underbrace{\frac{u'(c_u) - v'(c_e)}{v'(c_e)}}_{Insurance\ Value} - (\underbrace{(\eta_{B,b} + \eta_{D,b}\frac{D}{B}\frac{\tau}{b})}_{Efficiency\ \ Costs} + \underbrace{\frac{p_t g_t}{D}\eta_{p,b}}_{Interactions}) \tag{3}$$

Where the last term on the right hand side captures the elasticity of UI benefit levels to the application probability to non-UI benefits ($\eta_{p,b}$). The results presented in the paper have shown that this elasticity is negative (i.e. the higher UI levels, the lower the probability of applying to other programs). This implies that this additional term will decrease the efficiency costs of UI, thereby increasing optimal UI levels.

We now try to quantify the magnitude of this adjustment. This requires bringing some estimates from our empirical discussion and combining them with additional results from other papers as well as some further assumptions.

Starting with the efficiency costs of UI benefits, we use the median estimates of $\eta_{B,b}$ of $\eta_{D,b}$ from previous studies from the US, as reviewed by Schmieder and von Wachter (2016). These correspond to 0.38 and and 0.3, which are broadly in line with the results obtained in the paper (Section 5.A).[60] We obtain estimates of length of UI receipt B and unemployment duration D directly from our analytical sample, capping both quantities at 24, which represents the maximum length of our panel.[61] For comparability, we set the tax rate τ equal to 3% as in Schmieder and von Wachter (2016). The value of benefit level b is expressed in terms of the replacement rate, which we set to be equal to 50% as this is the

[59]The model implicitly assumes that the regulations and generosity of all other non-UI programs are constant. In the analysis, it has been shown that this assumption is not unrealistic, given that our results did not change after controlling for the rules governing other non-UI programs (see Section 5.D).

[60]If anything, our elasticity estimate with respect to the length of receipt of UI benefits is smaller than the one we are using here, meaning that our welfare estimates might be conservative.

[61]Note that this impacts the estimate of D much more than the estimate of B, as very few individuals would be eligible to more than 24 months of UI receipt.

value that applies to standard UI in the US (Kroft and Notowidigdo, 2016).

Moving to the interaction between UI and non-UI (i.e. last term on the right hand side of the equation), we obtain an estimate of $\eta_{p,b}$ from our empirical analysis. Specifically, our results in Section 5.A had shown that a $1,000 in UI benefit levels reduced the probability of applying to non-UI benefits by 15% on average. This translates into an elasticity estimate of UI benefit levels to the application probability to non-UI benefits of approximately 0.28. We further approximate the application probability with the observed share of individuals in our sample receiving non-UI benefits, which is equal to 0.18. We also take the value of non-UI benefit amounts g_t from our sample, obtaining a value of $3,351.9.[62]

The last remaining term corresponds to the insurance value of UI. Here, we use estimates from the pioneering study by Gruber (2001), which allows us to obtain a relationship between the flow drop in consumption at layoff and the UI replacement rate. The only remaining element corresponds to the coefficient of relative risk aversion (γ). Given that there is disagreement in the literature on the appropriate level of γ, we present results based on values ranging from one to five, as also done in most previous papers (Chetty and Finkelstein, 2013; Kolsrud et al., 2018; Landais and Spinnewijn, 2021; Schmieder and von Wachter, 2016). However, recent contributions have found evidence in favor of relatively high values of the coefficient of relative risk aversion.[63] That is, while we present estimates for a range of values of γ from one to five, we believe that results from values in the upper half of this range should be taken in greater consideration.

The results of this exercise are presented in Table 4. If we do not consider the interaction between UI and non-UI, the optimal replacement rate would be lower than the replacement rate for standard UI in the US (equal to 50%) for values of γ lower or equal to three. For a value of γ equal to four, we obtain a replacement rate roughly equal to the UI replacement rate in the US. When taking into account the interaction between UI and non-UI spending, the optimal replacement rate is already equal to 50% for a value of γ equal to two. For γ equal to four, the optimal replacement rate is equal to 67.8%. That is, assuming a value of γ equal to four, our results show that the UI replacement rate in the US

[62] As noted in Lindner (2016), this needs to be expressed in hundreds of dollars. Note also that we are not estimating the present-discounted value of non-UI benefits, but rather take the actual amount of non-UI benefits as observed in the 24 months after job loss. While this is consistent with the fact that we present a simplified version of the model without discounting, this will underestimate the true value of savings

[63] For instance, Landais and Spinnewijn (2021) present results for a range of values of γ between one and four. However, when using a revealed preferences approach, they find evidence for values of γ ranging from 4.5 to 8.7. Similarly, Schmieder and von Wachter (2016) present results for values of γ equal to two and five, but note that a high value is "likely to be more appropriate".

Table 4: Optimal UI replacement rates for different levels of the coefficient of relative risk aversion

	Values of gamma				
	1	2	3	4	5
Without interactions	0	0.187	0.404	0.512	0.577
With interactions	0.199	0.519	0.625	0.678	0.71

Notes: The table reports the optimal UI replacement rate for different values of the coefficient of relative risk aversion (γ). These replacement rates are presented from a model that does not consider the interaction between UI and non-UI programs (first row) as well as from a model where instead these interactions are taken into account (second row). See the main text for details on the theoretical framework that is used as well as the different assumptions imposed to estimate the model.

is optimal when not taking into account the interactions between UI and non-UI spending. When the additional savings from lower non-UI spending are instead also considered, the UI replacement rate should be around 17 percentage points higher than what it currently is. Data from the Department of Labor reported that the average weekly UI benefit was equal to $441.41 in the US in 2023. Assuming that this replaced 50% of previous wages, our results would justify raising the average weekly UI benefit amount to around $600.[64]

7 Conclusions

This paper has investigated the interactions between UI and other public programs. This is a largely overlooked field of study, with most of the previous evidence coming from either descriptive studies or well-identified contributions that however looked at the interaction between UI and specific programs (e.g. disability or retirement benefits) or specific groups in the population (e.g. UI benefit exhaustees). The contribution of this paper is to provide a full account of the interactions between UI and both contributory and non-contributory programs, to study in details the labor market and household mechanisms behind the results we find, and to quantify the implications of our results in terms of optimal UI levels.

Our results indicate strong interactions between UI and other public programs. Specifically, while individuals react to UI generosity by receiving UI benefits for longer, they reduce receipt of other benefits. This effect is driven by a short-term decrease in the

[64] 50% is the standard replacement rate for wages that fall within the range set by minimum and maximum benefit levels. The actual replacement rate is generally lower (around 40% in most years), given the higher number of individuals for whom the upper bound to UI benefits is binding (and for whom the actual replacement rate is lower) compared to those for whom the lower bound to UI benefits is binding (where the opposite applies). This implies that the current system of UI in the US can be considered optimal for a value of relative risk aversion of three (rather than four, as stated in the main text) without considering program interactions. Under this scenario, the necessary increase in replacement rate when taking into account program interactions would be equal to around 22 percentage points (rather than 17).

probability of receiving some means-tested programs, most notably SNAP, among otherwise eligible individuals as well as a long-term reduction in the probability of receiving Social Security for either pension or retirement. This latter result is driven by the fact that laid-off individuals who are eligible to more generous UI are less likely to quit the labor force after dismissal, which translates into a positive employment effect when UI eligibility ends.

These results have important policy implications. To start with, these concern the optimal level of UI. The welfare analysis presented above has shown that, when taking into account the interaction between UI and non-UI spending, optimal UI replacement rates should be higher than what would be predicted by the standard Baily-Chetty framework. Specifically, we find that the current UI replacement rate in the US is close to optimal when interactions between UI and non-UI are not taken into account. When savings on non-UI spending are instead also considered, we find that the UI replacement rate should be around 17 percentage points higher.

Additional policy implications relate to the spillover effects of public spending across different government budget lines. This is particularly important in countries, such as the US, where UI is mostly state financed, while other public programs are primarily funded by the federal government.[65] Our results imply that, in the absence of coordination across government agencies, strategic behavior could lead to an inefficient outcome. This is because each government entity knows that the effects of its benefit reduction will, at least partially, be offset by higher spending from other agencies.

[65] However, similar considerations apply also to countries where the same government entity finances both UI and non-UI spending, given that the source of financing is different in most of the cases.

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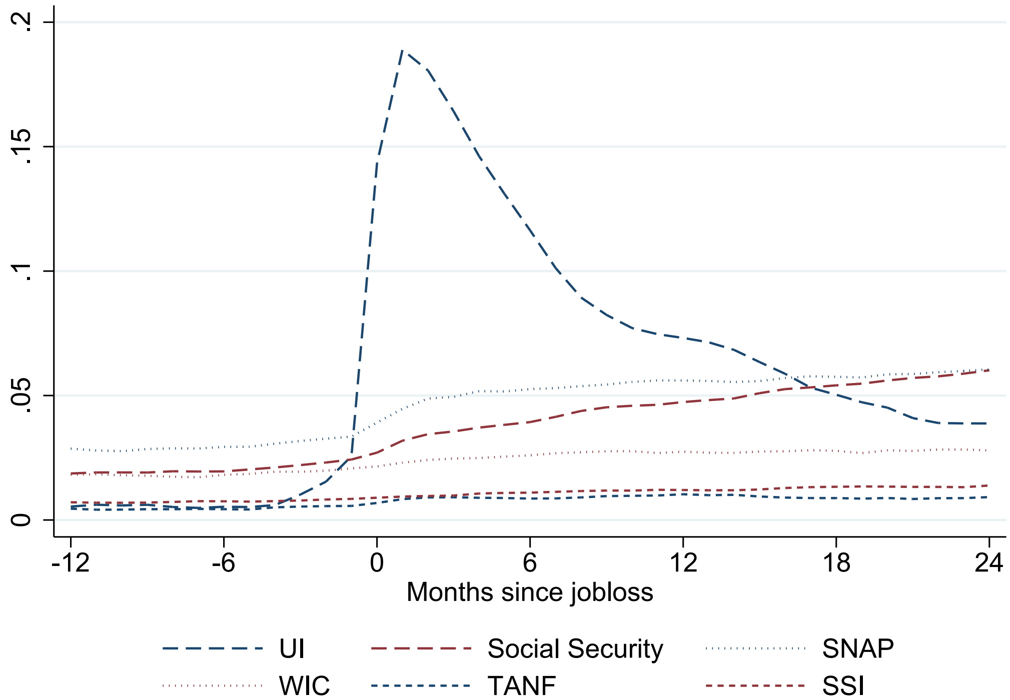
Appendices

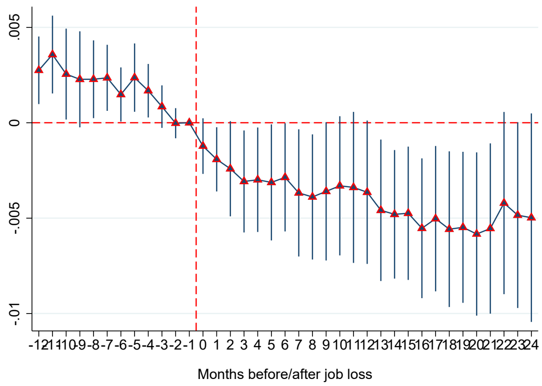
A Appendix: Additional figures

Figure A1: Share of individuals in the SIPP sample receiving selected programs, by month before and after job loss

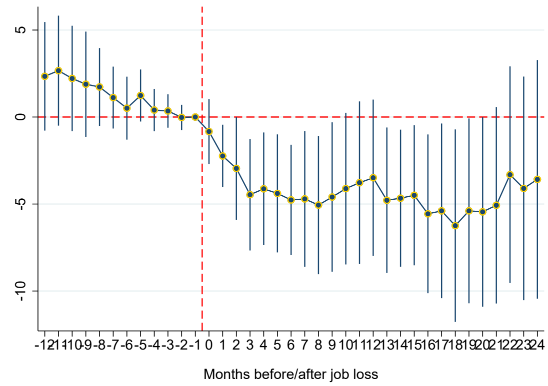


Notes: The figure plots the share of individuals in our SIPP sample receiving selected programs, by month before and after job loss.

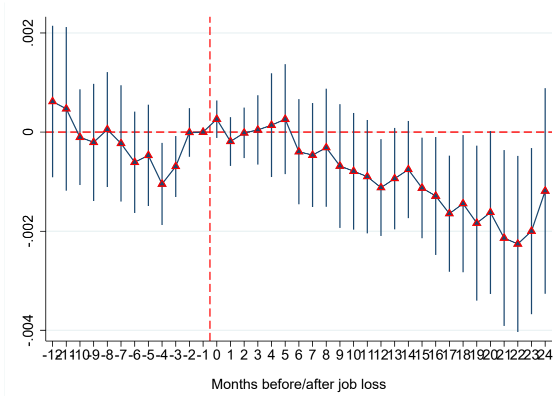
Figure A2: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of social security types



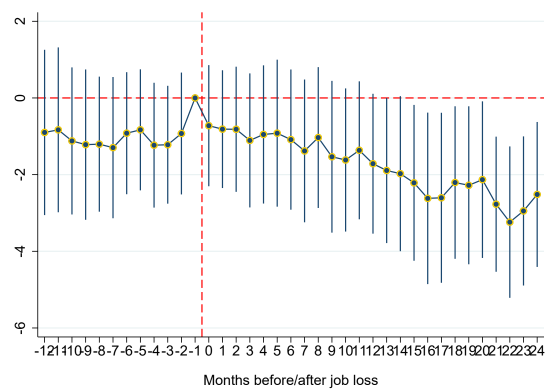
(a) Receipt of social security for adults



(b) Amount of social security for adults



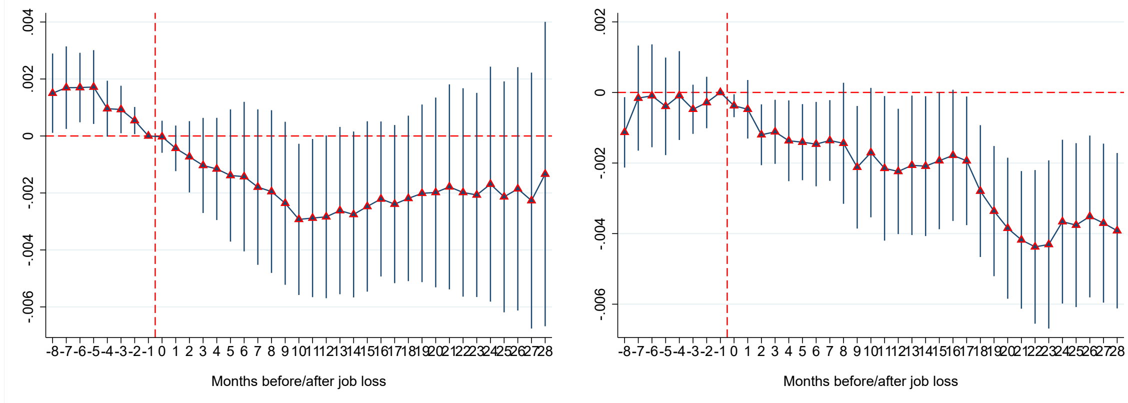
(c) Receipt of social security for children



(d) Amount of social security for children

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on Social Security for adults and child, analysing separately the effects on receipt (Panels A and C) and amounts received (Paneld B and D).

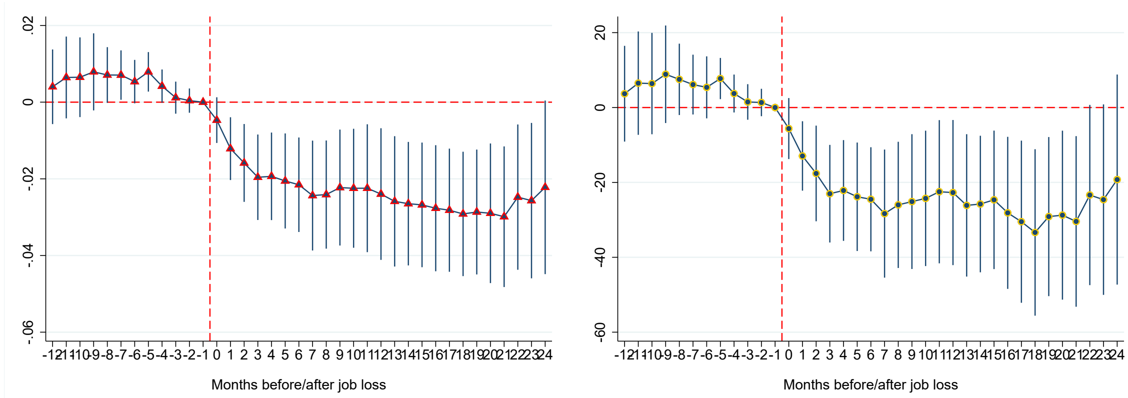
Figure A3: Event study estimates of the effects of UI benefit generosity on the reasons for social security receipt



(a) Retired **(b)** Disability

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis examines the effects on the self-reported reasons for Social Security receipt, differentiating between retirement (Panel A) and disability (Panel B).

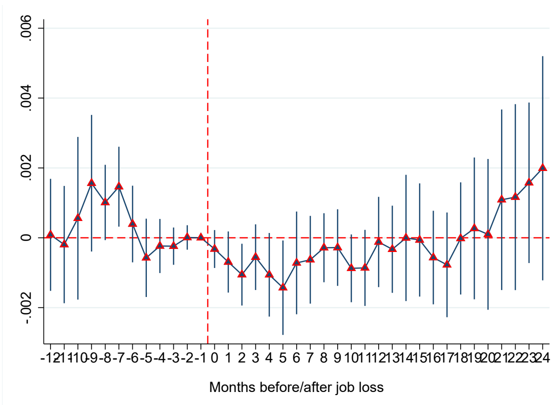
Figure A4: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of Social Security, for the population aged 50 and above



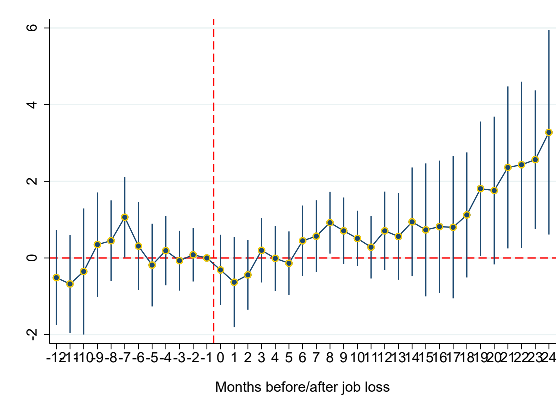
(a) Social security receipt **(b)** Social security amount

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on overall Social Security, analysing separately the effects on receipt (Panel A) and amounts received (Panel B). The sample here is composed of only individuals aged 50 and above.

Figure A5: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of SSI



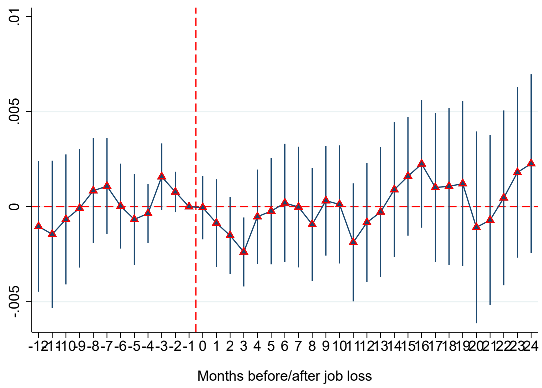
(a) SSI receipt



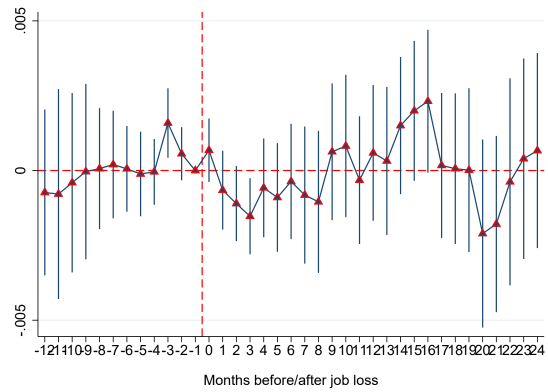
(b) SSI amount

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on SSSI, analysing separately the effects on receipt (Panel A) and amounts received (Panel B).

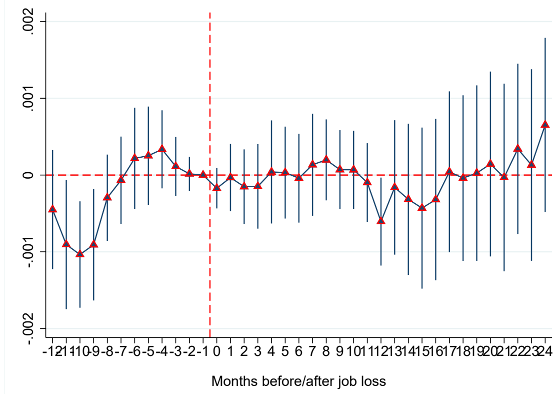
Figure A6: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of other programs



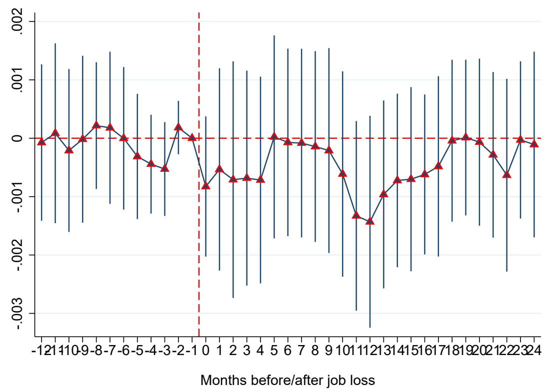
(a) Overall



(b) Child support



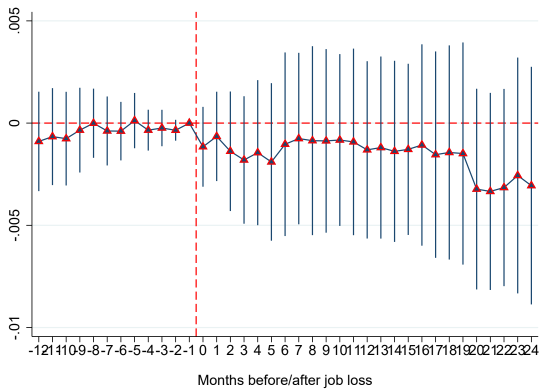
(c) Veteran compensation



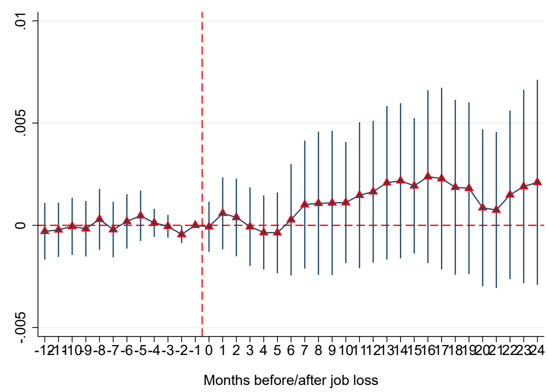
(d) Worker compensation

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on the receipt of a miscellaneous group of programs (from Panels B to D). Panel A presents the results obtained when combining all these programs in a single outcome of interest.

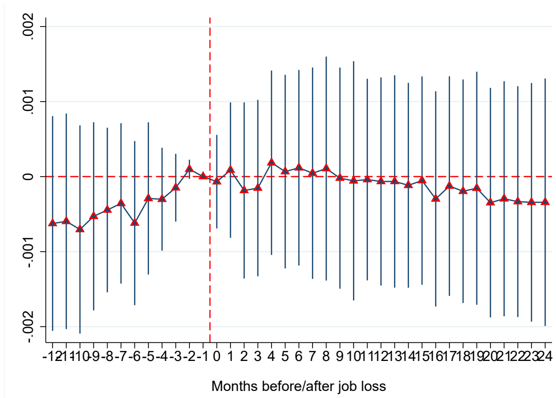
Figure A7: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of other pension types



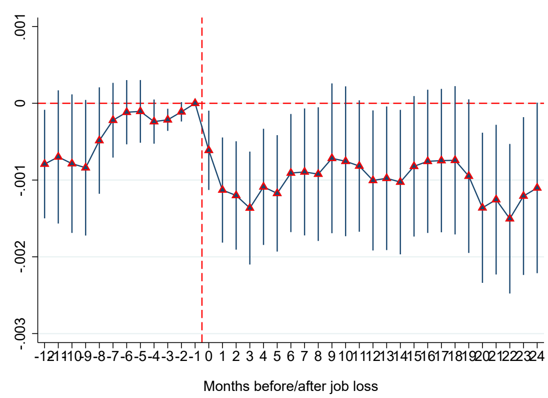
(a) Overall



(b) Pension from company



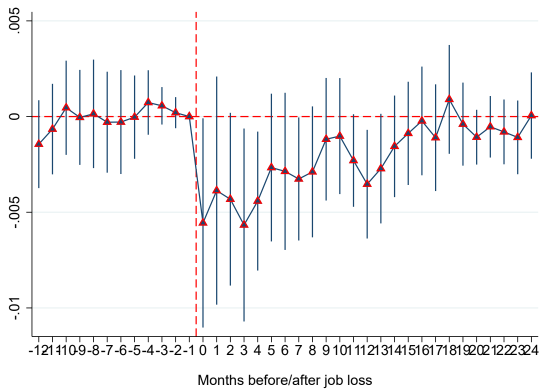
(c) State pension



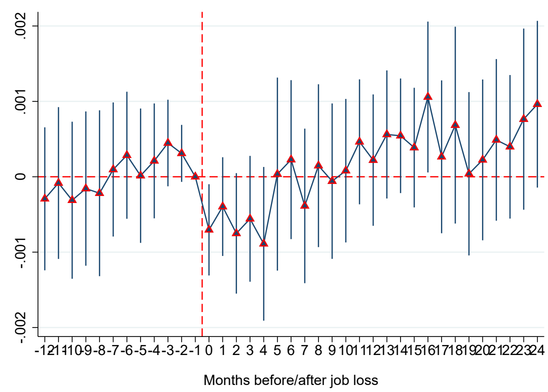
(d) Military pension

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on the receipt of a miscellaneous group of pensions different from Social Security (from Panels B to D). Panel A presents the results obtained when combining all these pensions in a single outcome of interest.

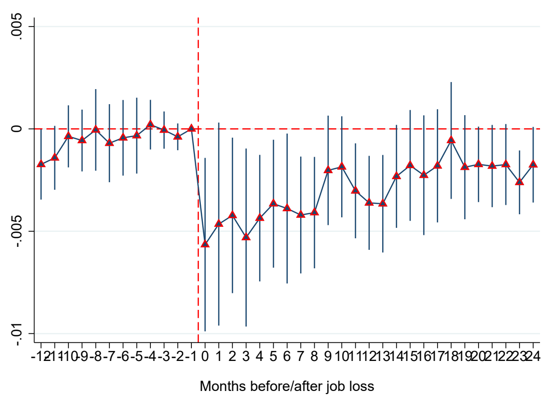
Figure A8: Event study estimates of the effects of UI benefit generosity on the receipt and amount received of other types of transfers



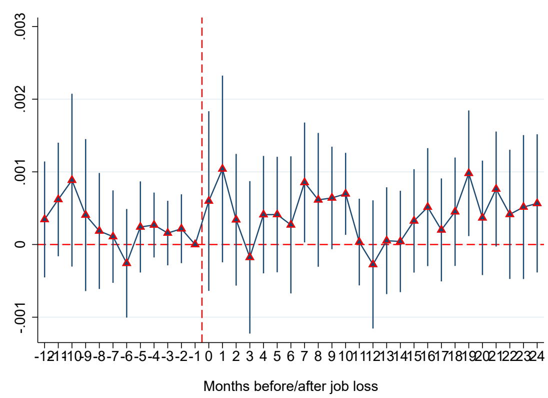
(a) Overall



(b) Money from relatives



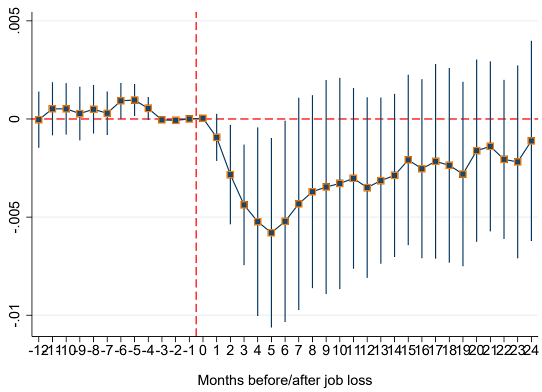
(c) Casual earnings



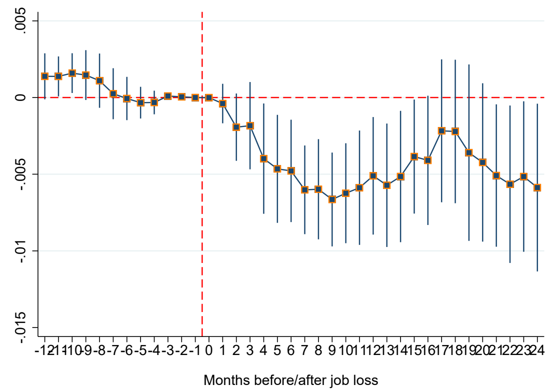
(d) Various income

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on the receipt of a miscellaneous group of income sources (from Panels B to D). Panel A presents the results obtained when combining all these income sources in a single outcome of interest.

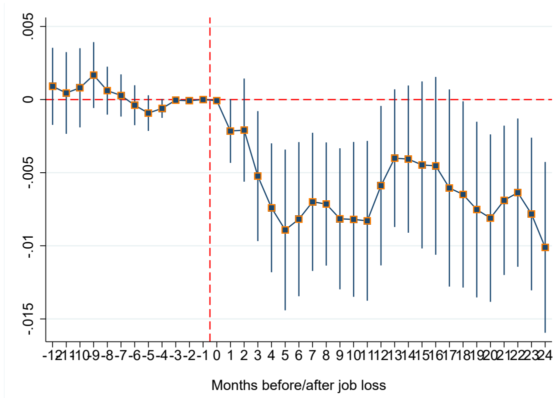
Figure A9: Event study estimates of the effects of UI benefit generosity on the reasons for inactivity



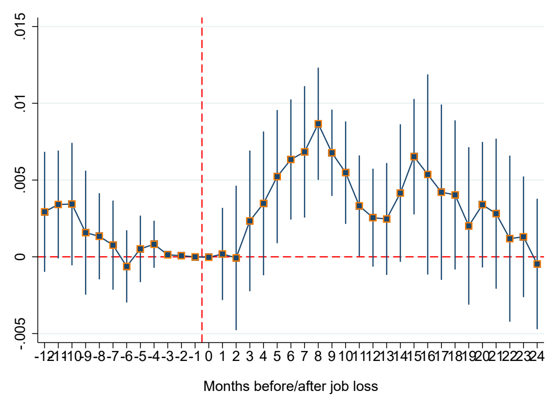
(a) Retired



(b) Unable to work



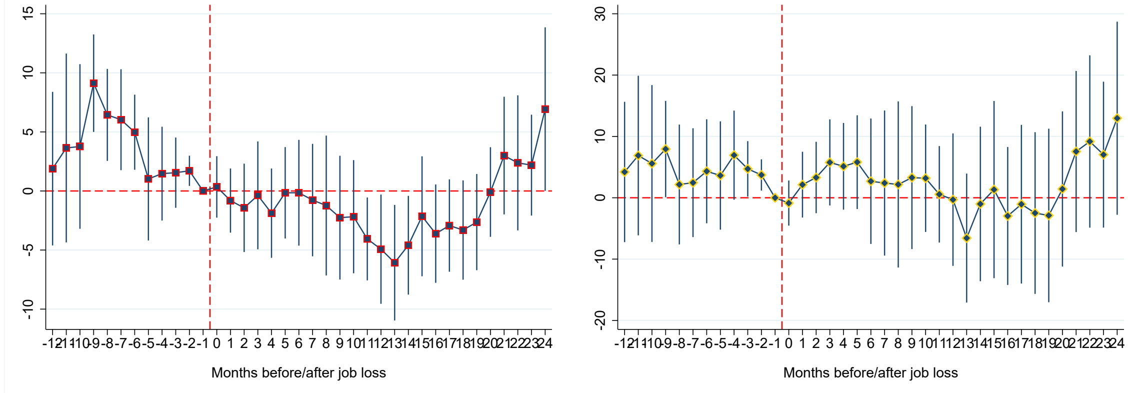
(c) Care responsibilities



(d) Enrolled in education

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on the reasons for inactivity (i.e. retired, unable to work, care responsibilities and education). Reasons for inactivity are set to zero for individuals who are not inactive.

Figure A10: Event study estimates of the effects of UI benefit generosity on property income

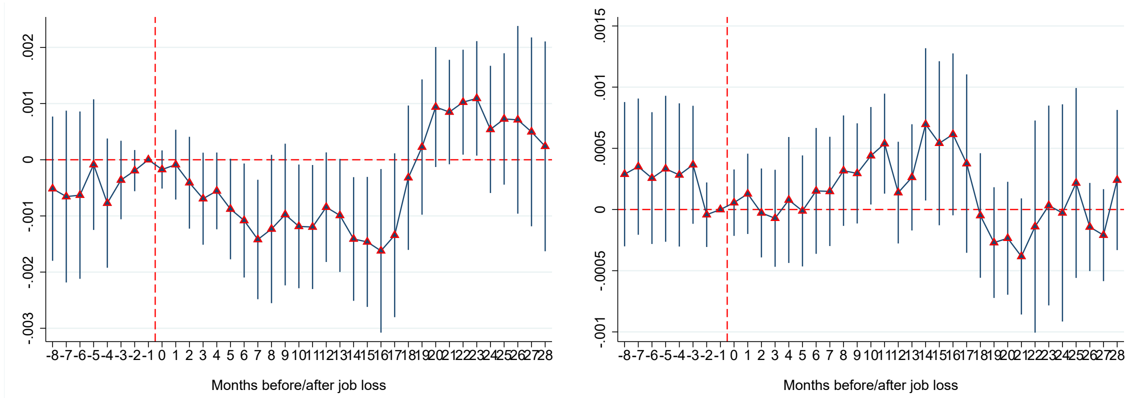


(a) Property

(b) Property

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted for property income, measuring this variables at either the individual level (Panel A) or at the household level (Panel B).

Figure A11: Event study estimates of the effects of UI benefit generosity on SNAP and TANF receipt for economic reasons

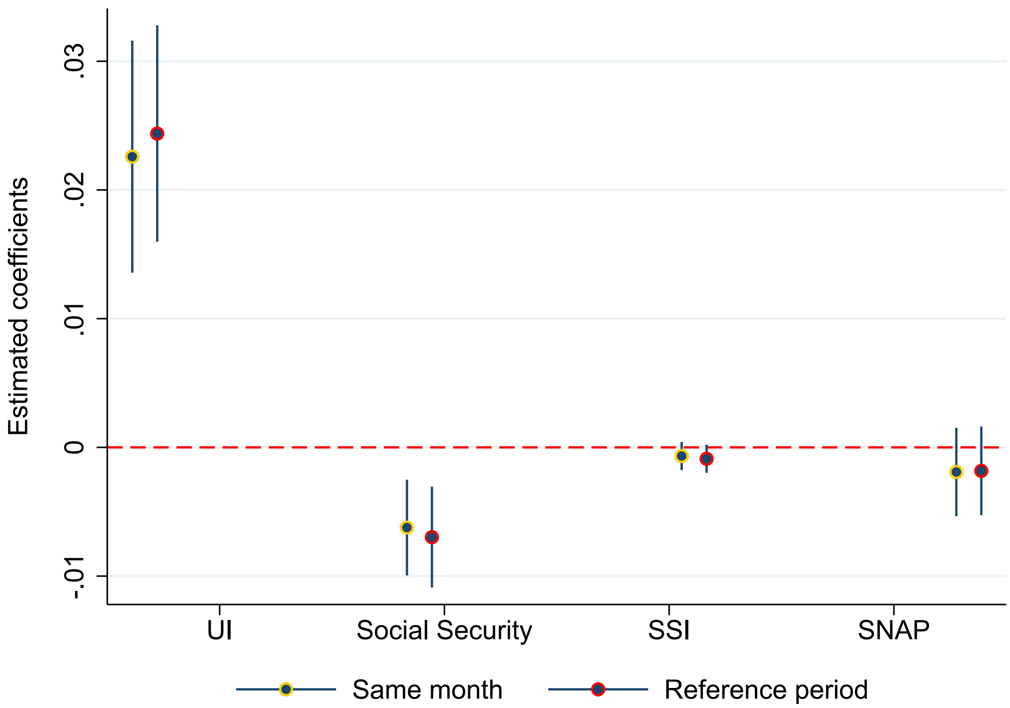


(a) SNAP

(b) AFDC

Notes: The figure plots point estimates and 90% confidence intervals for event-study specifications that expand the simple DiD model presented in Equation (1). The analysis is conducted between 12 months before job loss and 24 months afterwards. The month of job loss is denoted with zero and it corresponds to the dashed vertical red line. The coefficient of the month before job loss is omitted. The analysis is conducted on a dummy equal to one if the individual reports receiving SNAP (Panel A) or AFDC (Panel B) for either (i) job loss or a reduction in wages, or (ii) the reduction in other sources of income. These variables are set to zero among non-SNAP and non-AFDC recipients.

Figure A12: Effects of UI benefit generosity on the receipt of selected programs, with the monthly and 4-month variables



Notes: The figure reports point estimates and confidence intervals for coefficient β_2 in Equation (1). The analysis is run separately for four outcomes of interest: UI receipt, Social Security receipt, SSI receipt and SNAP receipt. For each outcome of interest, the analysis uses information on receipt referring to the exact month (in yellow) or to the reference period (in red). This latter variable takes the value of one if the individual has received the relevant benefit even in one single month in between interviews (see Section 5 in the main text for details).

B Appendix: Additional Tables

Table B1: Descriptive statistics

	12 months before		Jobloss		24 months after	
	mean	sd	mean	sd	mean	sd
Male	0.494	0.500	0.501	0.500	0.496	0.500
Age	36.639	13.179	36.435	13.135	38.322	13.222
White	0.825	0.380	0.821	0.383	0.819	0.385
Black	0.119	0.323	0.124	0.330	0.127	0.333
Native American	0.021	0.143	0.020	0.142	0.021	0.142
Asian	0.035	0.184	0.034	0.182	0.033	0.179
Married	0.509	0.500	0.497	0.500	0.509	0.500
Widowed	0.014	0.119	0.016	0.124	0.018	0.134
Divorced or separated	0.126	0.332	0.128	0.334	0.131	0.338
Single or never married	0.351	0.477	0.359	0.480	0.342	0.474
Number of children in household	0.900	1.179	0.883	1.172	0.835	1.148
Completed high school or less	0.441	0.497	0.455	0.498	0.402	0.490
Some college but no degree	0.222	0.416	0.224	0.417	0.226	0.418
Completed college and above	0.331	0.471	0.319	0.466	0.372	0.483
Receipt of UI	0.005	0.073	0.143	0.350	0.039	0.193
Receipt of social security	0.019	0.135	0.027	0.162	0.060	0.238
Receipt of SNAP	0.029	0.167	0.039	0.194	0.060	0.238
Receipt of WIC	0.018	0.134	0.022	0.145	0.028	0.165
Receipt of TANF	0.005	0.067	0.007	0.083	0.009	0.096
Receipt of SSI	0.007	0.084	0.009	0.094	0.014	0.117
N	24570		43237		19678	

Notes: The table reports means and standard deviations of the main sample included in the analysis, as defined in Section 3 in the main text. These descriptive statistics are captured at the beginning of the sample period (i.e. 12 months before job loss), at the time of job loss, and at the end of the sample period (i.e. 24 months after job loss). The number of observations differs across these different time periods, as the lenght of the panel is not equal across observations in the sample.

Table B2: State-level variations in UI minimum weekly benefit levels

	1990	2013	Ranking 1990	Ranking 2013	# changes	% change
Alabama	22	45	37	29	1	1.05
Alaska	62	128	1	3	4	1.06
Arizona	40	122	16	5	2	2.05
Arkansas	38	81	21	14	23	1.13
California	40	40	16	33	0	0.00
Colorado	25	25	34	44	0	0.00
Connecticut	22	30	37	40	2	0.36
Delaware	20	20	41	47	0	0.00
District of Columbia	13	50	48	26	1	2.85
Florida	10	32	49	38	1	2.20
Georgia	37	44	23	30	4	0.19
Hawaii	5	5	51	51	0	0.00
Idaho	44	72	12	18	4	0.64
Illinois	51	77	8	17	7	0.51
Indiana	40	37	16	35	6	-0.08
Iowa	32	71	30	19	21	1.22
Kansas	54	114	7	8	22	1.11
Kentucky	22	39	37	34	1	0.77
Louisiana	10	10	49	50	2	0.00
Maine	46	97	11	11	18	1.11
Maryland	33	90	28	13	3	1.73
Massachusetts	21	49	40	27	7	1.33
Michigan	59	147	2	1	9	1.49
Minnesota	38	24	21	45	5	-0.37
Mississippi	30	30	31	40	0	0.00
Missouri	33	35	28	36	6	0.06
Montana	47	127	10	4	22	1.70
Nebraska	20	70	41	20	3	2.50
Nevada	16	16	45	48	0	0.00
New Hampshire	35	32	26	38	2	-0.09
New Jersey	59	100	2	10	10	0.69
New Mexico	34	114	27	8	20	2.35
New York	40	64	16	22	1	0.60
North Carolina	20	46	41	28	13	1.30
North Dakota	43	43	13	31	0	0.00
Ohio	42	115	14	7	14	1.74
Oklahoma	16	16	45	48	2	0.00
Oregon	55	122	6	5	23	1.22
Pennsylvania	40	78	16	16	2	0.95
Rhode Island	49	95	9	12	7	0.94
South Carolina	20	42	41	32	1	1.10
South Dakota	28	28	33	42	2	0.00
Tennessee	30	80	31	15	3	1.67
Texas	36	62	24	23	20	0.72
Utah	14	26	47	43	11	0.86
Vermont	25	69	34	21	13	1.76
Virginia	56	54	5	24	8	-0.04
Washington	59	143	2	2	21	1.42
West Virginia	24	24	36	45	0	0.00
Wisconsin	42	54	14	24	13	0.29
Wyoming	36	33	24	37	17	-0.08
Average US	33.98	62.10			7.39	0.82
Median US	35.00	50.00			4.00	0.77

Notes: The table presents the minimum weekly UI levels in nominal terms in 1990 and 2013 for each US state, as well as the average and median value for the US. The table also reports the state ranking in terms of minimum weekly UI benefits in both 1990 and 2013, the number of changes to UI minimum benefit levels that occurred during the sample period and the per cent increase in minimum benefit levels. Information comes from the yearly reports ”Significant Provisions of “State Unemployment Insurance Laws” conducted by the Employment and Training Administration from the US Department of Labor.

Table B3: Relationship between maximum UI benefits and state-level variables

	Panel A: Macroeconomic variables				
Unemployment rate	-0.006				-0.011
	(0.012)				(0.011)
GDP growth		-0.001			-0.002
		(0.004)			(0.004)
Per-capita income			0.001		0.001
			(0.004)		(0.004)
Poverty rate				0.003	0.005
				(0.007)	(0.006)
	Panel B: Institutional variales				
Minimum wage	0.027				0.027
	(0.018)				(0.018)
Trade union membership		0.004			-0.023
		(0.012)			(0.025)
Collective bargaining coverage			0.008		0.023
			(0.011)		(0.019)
Demogratic governor				0.014	0.010
				(0.023)	(0.021)
	Panel C: Policy variables				
AFDC/TANF and Food stamp	0.000				0.000
	(0.000)				(0.000)
WIC recipients		-0.000			-0.000
		(0.000)			(0.000)
Maximum SSI benefit			-0.000		-0.000
			(0.001)		(0.001)
EITC rate				-0.022	-0.017
				(0.047)	(0.047)
N	1,224	1,224	1,122	1,224	1,122

Notes: The table reports coefficient estimates and standard errors from a series of regressions at the state-year level, where the dependent variable is the maximum weekly UI benefits in a given state and year. The coefficients of interest are those of selected macroeconomic variables (Panel A), institutional variables (Panel B) and policy variables (Panel C). See Section 3 in the main text for details of the different data sources. All regressions also include state and year fixed effects. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B4: Relationship between UI indicators and characteristics of the unemployed population

Panel A: Maximum UI levels				
Female	Age	Single	College	White
-0.000	0.000	-0.001	-0.002	0.001
(0.002)	(0.000)	(0.001)	(0.002)	(0.001)
Panel B: Maximum UI duration				
Female	Age	Single	College	White
-0.003	-0.000	-0.005	-0.017*	-0.009
(0.005)	(0.000)	(0.006)	(0.010)	(0.010)

Notes: The table report coefficient estimates and standard errors from a series of regressions that use as dependent variable either the maximum level of UI (Panel A) or the maximum duration of UI (Panel B) in a given state and year. The regressions are run at the individual level, and they differ based on the individual-level controls that are included each time, whose coefficients are reported in the table. All regressions also include state and year fixed effects. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B5: Effects of UI benefit generosity on the receipt and amount received of UI and any other programs: Heterogeneous effects

	Receipt of UI		Amount of UI		Receipt of any other program		Amount of any other program	
	Male	Female	Male	Female	Male	Female	Male	Female
Post layoff	0.149***	0.114***	123.286***	82.159***	0.069***	0.063***	115.356***	78.578***
	(0.016)	(0.012)	(19.766)	(16.910)	(0.006)	(0.007)	(15.522)	(12.306)
Post layoff*Max UI	0.025***	0.019***	60.782***	39.070***	-0.009***	-0.011***	-12.618	-8.957
	(0.007)	(0.006)	(8.607)	(9.351)	(0.003)	(0.003)	(7.648)	(5.903)
	Less than college	Some college	Less than college	Some college	Less than college	Some college	Less than college	Some college
Post layoff	0.138***	0.126***	102.397***	103.588***	0.070***	0.063***	77.304***	114.004***
	(0.014)	(0.014)	(16.840)	(19.259)	(0.010)	(0.008)	(12.891)	(18.031)
Post layoff*Max UI	0.020***	0.025***	40.605***	57.921***	-0.011**	-0.009**	-5.169	-14.993*
	(0.007)	(0.006)	(8.439)	(9.719)	(0.005)	(0.004)	(5.971)	(8.480)
	No children	At least one child	No children	At least one child	No children	At least one child	No children	At least one child
Post layoff	0.141***	0.118***	112.509***	84.154***	0.084***	0.053***	135.971***	66.912***
	(0.013)	(0.016)	(16.844)	(20.370)	(0.008)	(0.007)	(19.452)	(9.676)
Post layoff*Max UI	0.022***	0.024***	52.619***	51.680***	-0.014***	-0.009**	-19.418**	-8.310*
	(0.005)	(0.007)	(7.860)	(10.788)	(0.004)	(0.003)	(9.229)	(4.625)
	Married	Not married	Married	Not married	Married	Not married	Married	Not married
Post layoff	0.146***	0.113***	115.634***	83.849***	0.062***	0.072***	98.801***	89.924***
	(0.015)	(0.013)	(21.618)	(15.110)	(0.007)	(0.007)	(19.841)	(10.621)
Post layoff*Max UI	0.027***	0.018***	62.786***	40.497***	-0.004	-0.017***	1.072	-18.904***
	(0.008)	(0.005)	(11.598)	(6.056)	(0.003)	(0.003)	(9.857)	(5.657)
	<=35 years	>35 years	<=35 years	> 35 years	<=35 years	>35 years	<=35 years	> 35 years
Post layoff	0.097***	0.169***	73.529***	135.395***	0.046***	0.092***	39.596***	161.610***
	(0.012)	(0.016)	(13.157)	(24.505)	(0.005)	(0.009)	(9.552)	(20.932)
Post layoff*Max UI	0.013**	0.030***	28.553***	69.687***	-0.008***	-0.014***	-6.238	-19.679**
	(0.006)	(0.007)	(6.449)	(11.595)	(0.003)	(0.005)	(4.699)	(9.725)
	Tenure: 6mths	Tenure: 12mths	Tenure: 6mths	Tenure: 12mths	Tenure: 6mths	Tenure: 12mths	Tenure: 6mths	Tenure: 12mths
Post layoff	0.136***	0.133***	107.328***	104.678***	0.071***	0.070***	101.180***	103.373***
	(0.013)	(0.016)	(17.965)	(21.402)	(0.005)	(0.006)	(11.686)	(12.435)
Post layoff*Max UI	0.024***	0.029***	54.289***	62.140***	-0.011***	-0.009***	-10.685*	-6.855
	(0.006)	(0.007)	(8.795)	(10.129)	(0.003)	(0.003)	(5.583)	(6.331)
	Unemp: 6mths	Unemp: 12mths	Unemp: 6mths	Unemp: 12mths	Unemp: 6mths	Unemp: 12mths	Unemp: 6mths	Unemp: 12mths
Post layoff	0.556***	0.699***	502.879***	606.734***	0.122***	0.152***	82.824***	78.479**
	(0.024)	(0.037)	(42.242)	(57.504)	(0.014)	(0.026)	(21.395)	(34.893)
Post layoff*Max UI	0.039***	0.048**	144.865***	183.357***	-0.033***	-0.045***	-18.682*	-21.039
	(0.012)	(0.019)	(18.984)	(30.420)	(0.006)	(0.011)	(10.043)	(15.247)

Notes: The table reports estimates and standard errors for coefficients β_1 and β_2 in Equation (1). Results are presented separately for the receipt of UI (Columns 1-2), the amount of UI received (Columns 3-4), the receipt of any other program (Columns 5-6) and the amount received from any other program (Columns 7-8). For each outcome, results are presented separately for groups identified based on their sex (first panel), educational attainments (second panel), presence of children in the household (third panel), marital status (fourth panel), age (fifth panel), tenure in the previous job (sixth panel) and length of unemployment after job loss (seventh panel). Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B6: Effects of UI benefit generosity on the receipt and amount received of main program types

	Panel A : Receipt					
	Social security	SNAP	WIC	TANF	SSI	Other
Post layoff	0.017***	0.018***	0.005**	0.002**	0.002*	0.027***
	(0.004)	(0.004)	(0.002)	(0.001)	(0.001)	(0.005)
Post layoff*Max UI	-0.005**	-0.003	-0.001	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)
	Panel B: Amounts					
	Social security	SNAP	WIC	TANF	SSI	Other
Post layoff	20.036***	6.465***	5.731*	0.328	-0.499	54.876***
	(4.480)	(1.415)	(3.019)	(0.420)	(0.961)	(10.662)
Post layoff*Max UI	-5.655**	-1.054	-1.944	0.555**	0.530	-2.655
	(2.391)	(0.725)	(1.193)	(0.233)	(0.462)	(5.276)

Notes: The table reports estimates and standard errors for coefficients β_1 and β_2 in Equation (1). Results are presented separately for the receipt (Panel A) and the amount (Panel B) of different non-UI programs. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B7: Effects of UI benefit generosity on labor market variables

	Employment	Unemployment	Inactivity	Earnings
Post layoff	-0.702***	0.508***	0.194***	-1,132.480***
	(0.011)	(0.011)	(0.009)	(75.155)
Post layoff*Max UI	-0.001	0.011**	-0.010**	-77.166*
	(0.005)	(0.005)	(0.004)	(38.820)
	Inactive: Retired	Inactive: Unable	Inactive: Care duties	Inactive: Education
Post layoff	0.022***	0.021***	0.033***	0.019***
	-0.004	-0.003	-0.005	-0.003
Post layoff*Max UI	-0.003	-0.004***	-0.006**	0.003**
	-0.002	-0.002	-0.003	-0.001

Notes: The table reports estimates and standard errors for coefficients β_1 and β_2 in Equation (1). Results are presented for the mutually exclusive categories of labor market status (i.e. employment, unemployment and inactivity), labor earnings as well as for the reasons for inactivity (i.e. retired, unable to work, care responsibilities and education). Reasons for inactivity are set to zero for individuals who are not inactive. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B8: Effects of UI benefit generosity on personal and household income

	Panel A : Personal			
	Earned	Property	Transfers	Total
Post layoff	-1,143.249***	11.917***	196.801***	-934.174***
	(81.297)	(3.508)	(23.815)	(68.652)
Post layoff*Max UI	-70.178*	-4.476***	40.981***	-33.710
	(41.495)	(1.379)	(11.416)	(35.879)
	Panel B: Household			
	Earned	Property	Transfers	Total
Post layoff	-1,265.390***	2.295	230.668***	-1,032.427***
	(101.811)	(6.597)	(31.322)	(97.020)
Post layoff*Max UI	-22.369	-1.980	34.764**	10.416
	(48.082)	(3.186)	(14.837)	(45.862)

Notes: The table reports estimates and standard errors for coefficients β_1 and β_2 in Equation (1). Results are presented separately for personal income (Panel A), and household income (Panel B). For both personal and household income, the analysis differentiates based the different income sources as available in the SIPP. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B9: Triple difference approach using employed individuals as a control group

UI receipt	MaxUI*AfterLayoff	0.001**	0.001*	0.001**	0.001**	0.003***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
	MaxUI*AfterLayoff*Unemployed	0.020***	0.020***	0.019***	0.020***	0.019***
		(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Amount of UB	MaxUI*AfterLayoff	0.644	0.469	0.924*	0.739	0.495
		(0.393)	(0.376)	(0.473)	(0.455)	(3.323)
	MaxUI*AfterLayoff*Unemployed	44.667***	45.071***	44.356***	44.755***	46.368***
		(7.867)	(7.784)	(7.801)	(7.720)	(7.731)
Receipt of any other benefit	MaxUI*AfterLayoff	-0.001	-0.001	-0.001	-0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
	MaxUI*AfterLayoff*Unemployed	-0.007*	-0.007*	-0.007*	-0.007*	-0.008**
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Amount of any other benefits	MaxUI*AfterLayoff	-1.472	-1.436	-1.512	-1.474	-2.019
		(1.523)	(1.524)	(1.499)	(1.501)	(4.742)
	MaxUI*AfterLayoff*Unemployed	-8.369	-8.448	-8.291	-8.372	-8.711
		(5.473)	(5.441)	(5.503)	(5.470)	(6.510)
State		Yes	No	Yes	No	No
Quarter		Yes	Yes	No	No	No
Year		Yes	Yes	No	No	No
State*Group		No	Yes	No	Yes	Yes
Quarter*Year		No	No	Yes	Yes	No
Quarter*Year*State		No	No	No	No	Yes

Notes: The table reports estimates ad standard errors from a triple-difference exercise, conducted using the sample of employed individuals as a control group in the analysis (see Section 5 in the main text for details). The reported coefficients are the one of the interaction term between maximum UI levels and the post-layoff dummy and the one of the interaction between maximum UI levels, the post-layoff dummy and the dummy for being part of the unemployed sample. The latter one is the main coefficient of interest in the analysis, as it reports the differential impact of UI generosity for the unemployed (compared to the employed) after layoff (compared to before layoff). Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table B10: Effects of UI benefit generosity on the receipt and amount received of UI and any other programs, using alternative DiD estimators

	UI receipt	Amount of UB	Receipt of any other benefit	Amount of any other benefits
Two-way fixed effects	0.024**	14.694	-0.019	-44.012*
	(0.009)	(11.869)	(0.041)	(21.941)
Callaway and Sant'Anna (2021)	0.017***	18.058**	-0.029***	-49.134***
	(0.006)	(7.632)	(0.003)	(15.54)
Borusyak et al. (2024)	0.024***	15.384	-0.022	-45.456**
	(0.008)	(10.456)	(0.038)	(20.408)

Notes: The table reports estimates and standard errors for coefficients β_2 in Equation (1). Results are presented separately for the receipt of UI (first column), the amount of UI received (second column), the receipt of any other program (third column) and the amount received from any other program (fourth column). For each outcome, results in each row are from specifications that use a different DiD estimator: the standard two-way fixed effect (first row), the estimator proposed by Callaway and Sant'Anna (2021) (second row), and the estimator in Borusyak et al. (2024) (third row). Results presented in this table differ from the baseline findings in several ways. First, the sample is restricted only to observations between 2003 and 2008 and in seven states (i.e. Alaska, Arizona, District of Columbia, Florida, Louisiana, New York and Tennessee). Observations fro Arizona and the District of Columbia form the treatment group, while other observations are in the control group (i.e. the treatment indicator corresponds to a dummy variable). The post-policy period starts in 2005, where both states implemented an increase in maximum weekly UI benefits. All regressions include state, year and quarter fixed effects; while we do not include additional controls at the individual level. In all cases, the regressions are run only on the months after layoff for each individual. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.