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# Reexamining the consumption smoothing benefits of Unemployment Insurance☆

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ABSTRACT

The Great Recession spurred renewed interest in the moral hazard effects of the Unemployment Insurance (UI) program, however little research has focused on determining its benefits. This paper examines the consumption smoothing benefit of the UI program over the last 40 years, finding strong evidence of heterogeneity in this effect over time. In particular, the effects of UI are smaller in the 1990s compared with the 1970s. The 1990s were unique because of the long period of low unemployment rates as well as relatively low UI program generosity, thus we test whether the consumption smoothing effects vary by the state unemployment rate and average program generosity. We find suggestive evidence that the effects are larger when the state unemployment rate and average generosity are high. Together, these two dimensions can explain around 30–46% of the differential effect that we find in the 1990s.



## 1. Introduction

During the Great Recession, roughly 700,000 workers lost their jobs every month,[1] and more than 10 million individuals received Unemployment Insurance (UI) between 2007 and 2010 (Vroman et al., 2011). The size of the recession has generated renewed interest in understanding the moral hazard effects of the UI program (Rothstein, 2011; Farber and Valletta, 2011; Hagedorn et al., 2013) and the relationship between these effects and the optimal level of benefits (Landais et al., 2010; Schmieder et al., 2012; Kroft and Notowidigdo, 2014; Lalive et al., 2013). Most studies focus on providing new estimates of the social costs of UI, however, and ignore its potential benefits, which are also fundamental for the calculation of optimal benefit levels.

Upon job displacement, earnings are estimated to fall by roughly 25% in the first year and this drop remains large for many years afterwards (Stevens, 1997; Jacobson et al., 1993). Moreover, one third of the unemployed do not have enough savings to replace even 10% of their lost earnings (Gruber, 2001). The consumption smoothing effects of UI may therefore play an important role in the efficacy of the safety net. Despite this, the existing literature on the consumption smoothing benefits of UI is limited. Gruber (1997) provided the first such estimates for the U.S. in the 1970s and 1980s, finding that a 10 percentage point increase in UI generosity leads to a 2.8% reduction in the fall in consumption upon job loss. Two other recent papers examine this question, but neither analyzes the magnitude of the consumption smoothing effect for the most recent decades in the U.S.[2]

While new estimates of the moral hazard effects of UI have been generated as a result of the Great Recession, recent estimates of the consumption smoothing effects of UI are not available. Given the long periods of economic expansion in the 1990s and mid 2000s (Zarnowitz, 2000) and the changes to the safety net that have taken place since the late 1980s — both to the UI program itself and other welfare programs (Bitler and Hoynes, 2010) — it is unclear whether the consumption smoothing effects that have been documented previously still hold.

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[1] Current employment statistics in the Office of Employment and Unemployment Statistics at the Bureau of Labor Statistics for years 2008–2009; http://www.bls.gov/opub/mlr/2011/04/art1full.pdf.

[2] First, Browning and Crossley (2001) use Canadian data from the 1990s and find that the average effect of UI on total consumption is statistically insignificant and smaller in magnitude compared to Gruber (a 10 percentage point increase in UI generosity leads to 0.8% reduction in the fall in consumption). Second, Kroft and Notowidigdo (2014) use the same sample as Gruber (1997) to examine how the consumption smoothing effects vary over the business cycle, finding no evidence of heterogeneous effects.

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Six years after the official end of the Great Recession, the efficacy of UI and its optimal level of benefits remains a contentious political issue. Therefore quantifying the benefits of this program is especially important today, and this paper uses the 1968–2011 Panel Study of Income Dynamics (PSID) to provide new estimates of the consumption smoothing effects of UI.

The PSID is well suited for our analysis in several ways. First, it is a panel that follows individuals over time, which allows us to observe transitions into unemployment. Second, information about food consumption is collected annually, which to our knowledge makes this the only data set for which we can observe consumption smoothing at the individual level. Third, it spans more than 40 years, which allows us to examine how the benefits of UI may have changed over time. The main limitation of the PSID is the small sample size — only about 5000 families are interviewed each year. Additionally, we are only able to measure food consumption, rather total consumption. However, we believe that the uniquely detailed individual level data in the PSID outweigh these limitations.

Our core specification focuses on a sample of heads of household who transition from employment to unemployment, and it relates the changes in consumption observed over this transition to the generosity of UI benefits. While in principle we could use the benefit amount that an individual actually receives, we take a different approach by calculating the benefit amount that an individual is eligible for based on past wages, state of residence, year of unemployment, and number of children. This allows us to avoid problems of selection into take-up of UI, which is endogenous and could lead to biased estimates. This methodology was used also by Gruber (1997), and has been used in other contexts to estimate the effects of various safety net programs (Currie and Gruber, 1996a, 1996b; Dahl and Lochner, 2012). We use these eligible benefit amounts to construct our measure of UI generosity — the after-tax replacement rate — which is calculated as the after-tax weekly UI benefits divided by the after-tax weekly pre-unemployment wages. Since wages enter directly into the formula for UI benefits, we implement several checks to ensure that the potential endogeneity of lagged wages are not biasing our results.

Our estimate of the food consumption smoothing effect of UI over the full sample period is small compared to the previous literature — a 10 percentage point increase in UI generosity leads to a statistically insignificant 1.0% reduction in the consumption drop upon unemployment (off an average fall in consumption of 7%). We find that this small effect is driven by the fact that the consumption smoothing effect of UI was heterogeneous across decades, and significantly smaller in the 1990s compared to the 1970s. This result is generally robust to our sample and variable selection choices, as well as accounting for the potential endogeneity of wages. Additionally, we find evidence that the heterogeneity across decades is not explained by changes in the fraction of income that individuals spend on food over time, and we find similarly heterogeneous effects over time when analyzing imputed total consumption, suggesting that our findings may be applicable to total consumption as well.

We explore two key mechanisms that could explain the smaller effect in the 1990s. Since this decade was a period of a long economic expansion, we first analyze whether heterogeneous consumption smoothing benefits with respect to the state unemployment rate may contribute to this smaller effect. We find suggestive evidence that the consumption smoothing effects of UI are concentrated among individuals who are unemployed in states and years with high unemployment rates. These heterogeneous effects may be due to UI benefit extensions, longer durations of unemployment or higher take-up of UI benefits that occur during recessions. Second, we investigate whether the consumption smoothing effects are non-linear with respect to the state average replacement rate. Lower average replacement rates could lead to smaller consumption smoothing effects because of their negative effect on take-up rates (Anderson and Meyer, 1997) or because only replacement rates of a certain level affect consumption smoothing. Indeed, our findings suggest that in states and years with above median UI generosity, the consumption smoothing effects of UI are larger. Once we take into account these two dimensions of heterogeneity, the difference between the effect in the 1990s and the 1970s is reduced by 30–46%.[3]

The rest of the paper proceeds as follows. In Section 2 we provide background information on the UI program and the previous literature that analyzes its effects. In Section 3 we describe our empirical strategy and how we calculate the UI benefits that an individual is eligible for. Section 4 describes the PSID data and Section 5 presents the results. Finally, we conclude in Section 6.

## 2. Background on Unemployment Insurance

UI is a joint federal-state program that provides cash benefits to workers who have been laid off and are searching for work. Each state funds their own program through payroll taxes, except when the state or national unemployment rates become very high, at which point the states can receive supplemental funding from the federal government. As a result, the benefit amount varies by state, and in each state it is computed from formulas that depend on previous earnings and number of children. These formulas are frequently changing across states and over time, and Fig. A.1 provides an example of this variation for several states over time.[4] One might be concerned that changes in these formulas are endogenous and correlated with other state characteristics such as local economic conditions. Hsu et al. (2013) conduct detailed tests of the correlations between UI generosity and states' unemployment rates, GDP growth, house price growth, and average wages, finding that these relationships are very close to zero.

Previous studies have used this type of variation to analyze both the benefits and costs associated with UI. The literature on the costs of UI, specifically the moral hazard effects of lengthening durations of unemployment, is very extensive. See for example Meyer (1990), Katz and Meyer (1990), Lalive et al. (2006) and Card et al. (2007). All of this work finds that more generous benefits, and longer benefit durations, lead to longer unemployment durations. Related to our finding of heterogeneous consumption smoothing benefits with respect to the state unemployment rate, Schmieder et al. (2012) and Kroft and Notowidigdo (2014) find that these moral hazard effects are significantly smaller in recessions than expansions.

On the other hand the literature on the benefits of UI is very limited. Pioneering work was done by Gruber (1997), who used the variation described above to look at the consumption smoothing benefits of UI in the PSID. He constructs benefit eligibility using individuals' characteristics, and finds that a 10 percentage point increase in the replacement rate leads to a 2.8 percent reduction in the fall of food consumption upon job loss (off a mean fall in consumption of 7%). Taking a slightly different approach, Browning and Crossley (2001) use changes in the generosity of Canadian UI benefits at the federal level in the 1990s to examine how differences in the replacement rate affect individual's ability to consumption smooth as measured by total consumption rather than food consumption. Their results of the average consumption smoothing effects are slightly smaller in magnitude relative to Gruber (1997), and not statistically different from zero. In addition, they find that these effects are

[3] Our limited sample size leads to imprecise results, and we cannot rule out the possibility that these dimensions explain as little at 7% or as much as 94% of the differential effect in the 1990s.

[4] The data for this figure come from a simulated replacement rate, which entails using a fixed, national sample of unemployed individuals and assigning it to each state and year consecutively. After each assignment we run the sample through our UI benefit calculator and then collapse to generate an average replacement rate for each state and year. Therefore these state averages are only affected by state laws and not by differences in wages or demographics.

concentrated on individuals without a working spouse and with low levels of assets.

Lastly, Kroft and Notowidigdo (2014) use the PSID to replicate Gruber's result and examine whether this effect varies with the log of the state unemployment rate. Their estimates suggest larger effects when unemployment rates are high, however their standard errors are large so they cannot rule out that the effect is the same in periods of high and low unemployment.

## 3. Empirical strategy

Our empirical strategy exploits state and year variation in UI generosity to analyze how UI benefits affect the drop in consumption experienced upon job loss. More specifically, we estimate the following specification:

$$\Delta C_{ist} = \alpha + \beta_1 EligUI_{ist} + \beta_2 X_{ist} + \beta_3 Z_{st} + \theta_t + \delta_s + \gamma_s * t + \epsilon_{ist}, \tag{1}$$

where $\Delta C$ is the change in log food consumption from period $t-1$, when the head is employed, to period $t$, when he is unemployed. By taking changes in the log of consumption, the dependent variable has the interpretation of being the percent change in consumption.

X is a vector of individual demographic characteristics of the head, which includes controls for sex, marital status, race, the change in the number of household members (to capture any changes in food needs) and pre-unemployment wages. In addition, it contains categorical dummies for number of children, age, education group, major industry and major occupation. $Z$ is a vector of state-level controls, including the state unemployment rate and per capita spending on major U.S. welfare programs (AFDC/TANF, Medicaid, Food Stamps, and SSDI). These state-level controls may be important to include if states are changing UI generosity concurrently with changes in economic conditions or other safety net programs, which might influence individuals' ability to smooth consumption. The specification also includes state fixed effects $\delta_s$ to capture differences across states that are time invariant, and year fixed effects $\theta_t$ to absorb changes over time that vary uniformly across all states. In addition, we include state linear time trends, $\gamma_s * t$, which absorb any linear changes within states over time. This may be important if states that change their UI generosity are experiencing differential trends in the ability of the unemployed to smooth consumption upon unemployment. Lastly, we weight all our regressions by the sample family weights provided in the PSID to account for the low-income oversample as well as attrition, and we cluster our standard errors at the state level.

The main coefficient of interest in this analysis is $\beta_1$, the coefficient on the after-tax UI replacement rate that an individual is eligible for. Instead of using information about the UI benefits that an individual actually receives, we calculate the benefit amount the individual is eligible for based on his pre-unemployment wages, state of residence, year of unemployment, and number of kids. To calculate the after-tax replacement rate (*EligUI*), we follow Anderson and Meyer (1997) and Gruber (1997) and first calculate the weekly benefits that the individual is entitled to. Then we divide these benefits by the weekly wages prior to job loss, and finally we apply the marginal tax rates that these benefits and wages are subject to, where tax rates are calculated using the TAXSIM NBER model (Feenberg and Coutts, 1993). We take the approach of calculating benefit eligibility rather than using the observed receipt of benefits because take-up of UI is endogenous, and if the determinants of take-up are correlated with determinants of changes in consumption, our estimates will be biased. For example, if only individuals who have no personal savings take up UI, we would over-estimate UI's consumption smoothing effects. In addition, through this methodology we are able to quantify the Intent To Treat (ITT) effect, which is the policy relevant effect (Gruber, 1997) because it identifies the effect of a change in generosity on the consumption smoothing of all the unemployed rather than the subset who take-up UI.

By calculating the benefits that an individual is eligible for we are able to compare individuals who are eligible for different benefit amounts by virtue of the state and year they become unemployed, but are otherwise similar. However, since individual wages and number of children enter into the calculation of eligible benefit amounts, it is important to control for these characteristics as flexibly as possible. For this reason the vector $X$ includes dummy variables for the number of children. Moreover, we examine how sensitive our results are to controlling linearly for wages or including a much more flexible control of a five-knot linear spline in wages, as in Cullen and Gruber (2000) and Bronchetti (2012). These papers emphasize that when calculating individual level benefits that depend on individual level wages, controlling for these wages as flexibly as possible is important because otherwise these benefits might be correlated with unobservables. This may be true in our case if individuals' wages are correlated with their ability to consumption smooth, which is plausible as individuals who have higher wages likely also have more forms of private insurance.

Our strategy of exploiting state by year variation in UI benefits to estimate the effects of UI is quite widespread. For example, Cullen and Gruber (2000), Kroft and Notowidigdo (2014), and Hsu et al. (2013) analyze the effects of UI replacement rates on spousal labor supply, unemployment duration, and consumer credit, respectively. However, since individual pre-unemployment wages enter directly into the calculation of the replacement rate, our replacement rates may still be endogenous even after controlling flexibly for wages. Potential reasons for endogenous wages include the fact that higher replacement rate generosity might affect who is selecting into unemployment. In addition, endogeneity issues may arise if components of the wage that are unexplained by observables are correlated with an individual's ability to smooth consumption in the absence of UI. Therefore, as a check on our primary methodology (described above), we calculate replacement rates utilizing a two-stage procedure that was introduced by Chetty (2008).

In the first stage, we first estimate Eq. (2), which estimates the log (yearly or weekly) wages as a function of all the individual demographic characteristics included in X in Eq. (1).[5]

$$\log(w_{ist-1}) = \nu + \gamma X_{ist} + \theta_t + \delta_s + \upsilon_{ist}. \tag{2}$$

We then use the estimated coefficients on these variables to construct a "predicted wage", with which we calculate "predicted wage UI benefits" and a "predicted wage replacement rate" that depends only on observable characteristics and state laws. Finally, we estimate Eq. (1) using this predicted UI eligibility instead of actual UI eligibility.[6] The main advantage of using this two-stage procedure is that, in the presence of endogenous wages due to correlation with unobservables, the predicted wage replacement rate depends only on observable characteristics and state laws, hence is not correlated with the error term in Eq. (1).

[5] As mentioned above, $X$ is a vector of individual demographic characteristics of the head that includes controls for sex, marital status and race, as well categorical indicators for age, education group, number of children, major industry and major occupation groups. The Adjusted R-squared is 0.53 and 0.55 for the yearly and weekly wages estimation, respectively.

[6] In this stage, we also control for a five knot linear spline in after-tax predicted wages rather than actual wages.

## 4. Data

Our analysis uses the 1968–2011 Panel Study of Income Dynamics (PSID), a longitudinal study that began in 1967 with a nationally representative sample of families and an oversample of low-income families. The PSID then follows these roughly 5000 families (18,000 individuals) and their descendants over time. The survey contains detailed income measures, employment histories, and a variety of measures of consumption, but we focus on food consumption since it is measured throughout the full sample period.[7] The sample size of the PSID is a limiting factor in our analysis, however this data set is well suited for our purposes because it allows us to observe both changes in employment status and changes in consumption.

Our baseline sample is constructed using the employment history of the head of household, where a single observation is a head of household who is observed in two consecutive periods: in the first period the head is employed, and in the second the head is unemployed. Heads of household are male unless there is no male adult present and we include all heads regardless of gender. Employment status pertains to the time of the survey (typically in early spring of the survey year). We utilize the longitudinal nature of the data to construct changes in food consumption and for information about pre-unemployment wages, but otherwise information only from the year of unemployment is used. Our sample is composed of unemployment spells of the heads of household because these spells will likely result in the largest shock to family income and consumption (Browning and Crossley, 2001).

We further restrict our sample to contain only heads for whom all the necessary control variables are observed, whose food consumption is not imputed, and who have a smaller than threefold change in food consumption.[8] Beginning in 1976, the PSID differentiates between temporary layoffs and other types of unemployment. From that point forward we do not include workers who are on temporary layoffs, as these workers may be better able to anticipate their unemployment spells and may change consumption in response to UI benefits differently than individuals whose unemployment was unanticipated. All these sample restrictions give us a sample of 3383 observations of heads who transition into unemployment. We explore the sensitivity of our results to all of these sample construction assumptions in the analysis below. Summary statistics of all relevant variables for our sample are shown in Table 1.

After 1997, the PSID only surveys families every other year. We replicate the data construction used for the earlier PSID years as closely as possible, but due to this data constraint we have to examine heads who are employed in one year and then unemployed two years later, and we observe a corresponding two year change in food consumption as well. In order to have the most comparable sample over the full time period, all these individuals would remain employed from year $t-2$ to year $t-1$, but we do not know the employment status in $t-1$ so conditioning on this is not possible.[9] Even if we assume all individuals do remain employed in year $t-1$, food consumption changes from two years prior to unemployment may be different than those from one year prior because income usually falls in advance of a job loss (Stevens, 1997; Jacobson et al., 1993). As a check that this change in survey format does not drive our results, we reconstruct our data set such that the years 1968–1997 mimic the format of the post-1997 PSID and rerun our baseline results.

As mentioned above, our main outcome of interest is the change in food consumption upon job loss. To construct this variable, we add up the four measures that contain information on food expenditures: food consumed at home (excluding food purchased with food stamp benefits), food delivered to the home, food consumed away from home, and food stamp benefits.[10] In most years, the first three questions, henceforth the "non-food-stamp" questions, ask explicitly about consumption in an average week, and we follow the literature and assume individuals respond regarding their consumption in that survey year (Zeldes, 1989; Gruber, 1997; Fisher and Johnson, 2006). In the years in which the "non-food-stamp" questions do not specify consumption in an "average week", the questions instead follow a question about food stamps received in the month prior to the survey. So again we follow the literature and assume that in these years individuals were likely thinking about their current consumption because the preceding question asked about current food stamp receipt.[11]

Households are also asked several questions about their food stamp benefit receipt. In all survey years the households report the amount of food stamp benefits received in the calendar year prior to the survey (and in two calendar years prior to the survey after the change in survey design in 1999) — we refer to this measure as food stamps received "last year". In addition, in the years 1975–1987, 1990–1997, and 2009–2011 households are asked about the amount of food stamp benefits they received in the month prior to the survey. Given that the question about food stamp receipt "last month" is not asked in every survey year, we rely on the measure of food stamp receipt "last year" as our primary measure of food stamps.[12] Since the "last year" measure explicitly refers to the year prior to the survey, we assign the annual amount received "last year" to the employment status of the prior year's survey (or food stamps from two years ago to employment status from two years ago in 1999–2011).[13] The upside of utilizing the "last year" measure of food stamp receipt is that it prevents changes in the survey questionnaire from influencing our results because this variable is available in every survey year (except 2011, as described above). The downside is that the retrospective nature of this question means the time that this measure of food stamps is collected in is not the same as our measures of non-food-stamp food consumption. Therefore, in the robustness checks section we test how sensitive our results are to using food stamp receipt "last month" instead of food

[7] In 1973, 1988, and 1989 the questions about food consumption were not asked, so we do not have 1973–1974 and 1988–1990 in our sample. The other measures of consumption available throughout our sample are rent payments, mortgage payments and home value. However, using these variables to create measures of housing consumption is problematic because it is unclear how to proceed for non-renters (Bronchetti, 2012). Therefore, in our main analysis we focus on food consumption, but we utilize housing consumption information to impute total consumption discussed in more detail below. Additionally, in recent years the PSID has added information about other types of consumption such as luxury items, education, and health care. We have used these other measures of consumption, but the small sample size due to the few years these questions were asked cause these results to be quite imprecise. Therefore we do not include them in our analysis.

[8] Specifically, we drop observations where $ln(C_t/C_{t-1}) < -1.1$ or $ln(C_t/C_{t-1}) > 1.1$.

[9] We have tried to get a sense of how many individuals are employed in $t-1$ in 1999–2011 by tabulating the employment status for individuals observed prior to 1997 who we observe to be employed in $t-2$ and unemployed in $t$. Among these individuals, 70% of them were employed in $t-1$, which suggests that our sample construction does not change drastically over time.

[10] If information on one of the four measures of food is missing, this measure is implicitly set to zero and added to the other food variables. Our results are very similar if we drop observations that have missing information on any of the four food components.

[11] The assumption about the timing of food consumption is important for our analysis. If the variables collected information regarding the calendar year prior to the survey rather than the year of the survey, our outcome variable — change in food consumption — would not always be measuring the drop in consumption upon job loss, and the degree of mismeasurement and bias that would result from this depends on the length of the unemployment spells of the individuals in our sample. We discuss this in more detail in the Appendix A.

[12] Since this is an annual measure, we also annualize food consumed at home, food delivered to the home and food consumed away from home.

[13] For example, if an individual is unemployed in 1970 and employed in 1969, and in 1970 that individual reported that in 1969 they spend $300 on food using food stamps, we assign this $300 to the 1969 employed state. This decision was made after careful consideration and discussion with PSID employees. It is important to accurately measure food stamp receipt in our measure of consumption if unemployed heads rely on food stamps in addition to UI to smooth consumption. In 2011 we do not observe retrospective food stamps so we use the question about last month's food stamp receipt, multiplied by 12, in this year.

**Table 1**
Sample summary statistics.

| | Full sample | By decade | | | |
|---|---|---|---|---|---|
| | 1968–2011 | 1968–1977 | 1978–1987 | 1991–1997 | 1999–2011 |
| Food consumption ($1984) | 3372 | 3945 | 3613 | 3137 | 3088 |
| Age | 38.92 | 36.34 | 36.57 | 38.01 | 42.96 |
| Female | 0.27 | 0.23 | 0.28 | 0.27 | 0.27 |
| Married | 0.45 | 0.60 | 0.51 | 0.37 | 0.39 |
| White | 0.77 | 0.79 | 0.80 | 0.79 | 0.72 |
| Black | 0.19 | 0.13 | 0.17 | 0.17 | 0.24 |
| Years of education | 12.45 | 11.16 | 11.94 | 12.77 | 13.22 |
| Number of kids | 0.86 | 1.19 | 1.00 | 0.76 | 0.66 |
| Change family size | −0.02 | 0.02 | −0.01 | −0.06 | −0.02 |
| Previous weekly wage ($1984) | 274 | 262 | 239 | 264 | 319 |
| After-tax replacement rate | 0.51 | 0.56 | 0.53 | 0.49 | 0.49 |
| Unemployment rate | 0.07 | 0.07 | 0.08 | 0.06 | 0.07 |
| Change log consumption | −0.07 | −0.10 | −0.04 | −0.06 | −0.10 |
| Observations | 3383 | 506 | 1151 | 805 | 921 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. The first column present the summary statistics for the full sample and the remaining columns present the summary statistics for each decade.

stamp receipt “last year”. Despite the limited number of years in which this variable is available, which leads to a small sample size, this robustness check provides valuable evidence that the timing of the food stamp variable does not influence our main results.

The fact that we focus on food consumption is an important caveat to our results. Work by Dynarski et al. (1997) examines how consumption responds to income losses due to unemployment, finding that for each dollar of income lost, food consumption falls by 5.5 cents and total consumption falls by 24 cents. As expected, food consumption is less responsive to job loss than total consumption, but both types of consumption respond significantly. Since food consumption is less sensitive to changes in income than total consumption, the consumption smoothing estimates in our paper will likely be smaller than the consumption smoothing effects on total consumption. We attempt to address this issue by imputing total consumption following Bronchetti (2012), Skinner (1987), Fisher and Johnson (2006) and Blundell et al. (2008), who use food consumption, housing consumption, and other observable characteristics of households to predict total consumption. We discuss this methodology in more detail in the sections below.

After we create our sample and the relevant consumption measures, we calculate the weekly benefit eligibility for each individual using a UI calculator that was constructed with information from the calculators used in Chetty (2008) and Gruber (1997). Moreover, we supplement and update these calculators with information from the Employment and Training Administration, which reports semiannual information on state benefit schedules, as well as other state laws and documents. To calculate these benefits we use the wage data collected in the year in which we observe the head to be employed, and these wages pertain to the year prior to when the head is interviewed. The formula used to calculate benefits varies by state and year, and includes the percent of wages to be replaced by UI as well as a minimum and maximum amount of weekly benefits. Finally, some states have additional benefits depending on the number of children of the unemployed individual, which we incorporate into the calculations.[14]

To this data we merge in data on state economic and safety net conditions based on state of residence and year of unemployment spell. This state information includes the unemployment rate from the Bureau of Labor Statistics (BLS)[15] and state spending per capita on other social safety net programs that might be important for the unemployed — AFDC/TANF, SSDI, Medicaid and Food Stamps.[16]

## 5. Results

### 5.1. Baseline results

Panels A and B of Table 2 present our baseline results for the replacement rates calculated using actual and predicted wages, respectively. In the first column we show the results from a regression that includes a linear control for wages, controls for demographics, and state and year fixed effects. The estimates indicate that a 10 percentage point increase in the actual (predicted) wage replacement rate leads to a 1.36 (1.59) percent reduction in the fall in consumption experienced upon job loss. However, the actual wage estimate is only marginally significant and the predicted wage one is not statistically significant.[17] As discussed previously, since wages enter into the calculation of the replacement rates directly, controlling very flexibly for pre-unemployment wages may be important (Bronchetti, 2012; Gruber, 2001; Gruber and Saez, 2002), hence in the second column we replace the linear control for wages with a 5-knot linear spline. The estimated coefficients remain very similar using both actual and predicted wages, and neither of them is significantly different from zero. In column (3) we add state-level variables, including the state unemployment rate and state spending on safety net programs, to control for changes occurring within states and over time, and again these controls do not affect our estimates. Finally, in column (4) we include state-specific linear time trends, which may be important to include if states that change their UI generosity are experiencing differential trends in the ability of their unemployed to smooth consumption. The addition of the state specific linear time trends causes the estimated coefficients to shrink slightly, without meaningfully changing the standard error estimates.

[14] As of 2014, fourteen states provide additional dependent benefits. These are Arizona, Connecticut, Illinois, Maine, Maryland, Massachusetts, Michigan, New Jersey, New Mexico, Ohio, Pennsylvania, Rhode Island and Tennessee.

[15] We use BLS unemployment rates for the period 1976–2011. State unemployment rates for years 1968–1975 were obtained from Moffitt's welfare benefits file at http://www.econ2.jhu.edu/people/moffitt/datasets.html.

[16] This expenditure data was obtained from Bureau of Economic Analysis Regional Economic Accounts (BEAREA) and from Bitler and Hoynes (2013). State population information comes from the National Cancer Institute SEER data (http://seer.cancer.gov/popdata/download.html).

[17] The standard deviation of the replacement rate is .16. So a 10 percentage point increase in the replacement rate is not unreasonable within our sample.

**Table 2**
Effect of UI on food consumption — baseline results.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A: Actual wages, 1968–2011* | | | | |
| Repl. rate | 0.136* | 0.130 | 0.134 | 0.100 |
| | (0.069) | (0.085) | (0.087) | (0.089) |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 3383 | 3383 | 3383 | 3383 |
| *B: Predicted wages, 1968–2011* | | | | |
| Repl. rate | 0.159 | 0.169 | 0.162 | 0.115 |
| | (0.128) | (0.142) | (0.145) | (0.157) |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 |
| Mean repl. rate | 0.50 | 0.50 | 0.50 | 0.50 |
| Observations | 3383 | 3383 | 3383 | 3383 |
| Demographic controls | X | X | X | X |
| State, year FE | X | X | X | X |
| Wage spline | | X | X | X |
| State controls | | | X | X |
| State trends | | | | X |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. In column (1) we show the results control for demographics, a linear control for wages, and state and year fixed effects. In column (2) we instead control for wages by using a spline, and in columns (3) we add in controls for the state unemployment rate and safety net expenditures. Finally in column (4) we include state linear time trends. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table 3**
Effect of UI on food consumption — comparison to Gruber (1997).

| | 1968–2011 | 1968–1987 | | | Gruber (1997) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Repl. rate | 0.100 | 0.326** | 0.244** | 0.280*** | 0.279*** |
| | (0.089) | (0.138) | (0.115) | (0.097) | (0.105) |
| Mean food change | −0.07 | −0.06 | −0.07 | −0.07 | −0.07 |
| Mean repl. rate | 0.51 | 0.54 | 0.55 | 0.56 | 0.58 |
| Gruber specification | | | X | X | X |
| Gruber outcome | | | | X | X |
| Observations | 3383 | 1657 | 1543 | 1621 | 1604 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Column (1) presents our baseline results with our preferred specification over the period 1968–2011. Columns (2)–(4) restrict the period to 1968–1987 only. Column (2) estimates our preferred specification, in column (3) we change the control variables to replicate the variables in Gruber (1997), and in column (4) we also use same food stamp definition as Gruber. See text for more details about the differences between these models. In the last column we reproduce the result from Gruber (1997). Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

We take the estimates from column (4) as our preferred model, since it controls most flexibly for pre-unemployment wages and it includes additional controls for other changes occurring across states and over time. With our preferred model we find that a 10 percentage point increase in the replacement rate leads to a 1.0% reduction in the fall in consumption when we use actual wages, and to a 1.15% decrease when we use predicted wages. However, these effects are not statistically significant and we cannot rule out large positive or negative effects.[18] Comparing our results to the literature, Gruber (1997) finds that a 10 percentage point increase in the replacement rate leads to a statistically significant 2.8% reduction in the drop of food consumption upon job loss. On the other hand, Browning and Crossley (2001) use a sample of Canadian individuals in the 1990s and find that a 10 percentage point increase in the replacement rate leads to a 0.8% reduction in the drop of total consumption. Since we use the same country and data as Gruber (1997), we next examine whether our smaller point estimates are due to the difference in the time period of analysis or a difference in specification. Additionally, we explore whether these differences are driven by the 1999 change in the PSID design.

#### *5.1.1. Comparison with Gruber (1997)*

There are two primary differences between our specification and the one used in Gruber (1997). First, in order to construct our outcome variable we use food stamp “last year” throughout the sample (as discussed above), whereas Gruber uses food stamps “last month” if available and food stamps “last year” otherwise. Second, Gruber uses slightly different, and less conservative, control variables. In Table 3 we therefore move from our preferred specification and full time period to the time period and specification used in Gruber (1997), focusing on the results based on actual wages in order to be comparable to Gruber. In column (1) we show our estimate from our preferred specification and the full time period, and in column (2) we restrict our sample to only include the survey years 1968–1987, the same sample period as in Gruber (1997). With this time period restriction we find a much larger (and statistically significant) consumption smoothing effect, which is similar to Gruber's original estimate: an increase in the replacement rate of 10 percentage points reduces the decline in consumption by 3.3%. In column (3) we change the control variables to replicate the ones in Gruber (1997),[19] and in column (4) we also change the outcome variable to reflect Gruber's food stamps variable choice. Again, the estimates in these two columns are large, positive and statistically significant, and the estimate in column (4) is very close to the results reported in Gruber (1997), shown in column (5). Therefore, we believe that the difference between our estimated effect and Gruber's is not driven by differences in specification, but perhaps by differences in the time period of analysis.

#### *5.1.2. Do changes in the survey design matter?*

Next, we investigate whether the changes in PSID design that occurred after 1997 contribute to the smaller effect that we estimate over the period 1968–2011 (as compared to 1968–1987). In order to do so, we reconstruct our data set such that the years 1968–1997 mimic the format of the post-1997 data. This essentially means throwing away even numbered years of data, and calculating the change in the food consumption variable as a two-year change. Moreover, we re-calculate the replacement rate based on wage data collected two years before unemployment. We then use this data to re-estimate our baseline results.

[18] These findings suggest that there is a low marginal propensity to consume out of UI benefits, which seems to contradict recent findings that the unemployed are highly liquidity constrained. We believe a possible explanation for this discrepancy is the fact that we observe only food consumption, and that for other types of consumption the marginal propensity to consume is much higher.

[19] Replicating Gruber's controls entails controlling for wages linearly, the age and education of the head linearly, and omitting the state linear time trends and controls for safety net spending and industry and occupation. Additionally, Gruber includes controls for family change in the log of “food needs” and the unemployment rate in the county of residence, which we do not use in our preferred specification because these variables are not available consistently after 1993. “Food needs” is constructed by the PSID using U.S. Department of Agriculture guidelines based on family size and composition. Moreover, Gruber (1997) uses federal tax laws rather than TAXSIM to impute tax rates, hence we use these federal tax laws in this replication. Finally, we do not use the sample weights in our replication as Gruber's choice about this is unclear and we find our estimates to be more similar to his without the weights.

There are several differences to keep in mind about this data construction. As described above, individuals could be employed, unemployed, or out of the labor force in the year between observations, and this may introduce noise into our estimates. Using the 1968–1997 data, we calculate that for individuals observed for three consecutive years who are employed in the first year and unemployed two years after that, 30% of the sample is unemployed or out of the labor force in the second year. Individuals who are unemployed or out of the labor force in this second year may no longer be eligible for UI in the third year, hence we expect the responsiveness of consumption to UI generosity for these individuals to be small or zero, biasing our estimates towards zero. In addition, and as previously described, two year changes in consumption may be different from one year changes, especially if consumption changes in anticipation of a job loss. Finally, this data construction leaves us with a much smaller sample size, which affects the precision of our estimates.

Despite these caveats, the results, shown in Table 4, indicate a similar pattern of larger effects in 1968–1987 relative to 1968–2011. Using actual wages we find that a 10 percentage point increase in the UI replacement rate leads to a 0.8% reduction in the magnitude of the consumption drop for the full period, whereas the same change leads to a 4.9% reduction in the 1968–1987 period. This pattern is similar when using predicted wages. We therefore conclude that changes in survey design are not driving the difference in the effect that we estimate between the two time periods. However, in order to ensure that these survey changes do not affect any of our results, in what follows we include estimates for both the full 1968–2011 sample and the 1968–1997 sample.

### 5.2. Does the effect change over time?

In the previous section we documented that in the full sample period the consumption smoothing effect of UI is small and statistically insignificant. In addition, we found that the small magnitude relative to the previous canonical estimate appears to be explained by the addition of data from the years 1990–2011. Hence we next explore in more detail whether the consumption smoothing benefit of UI is heterogeneous over time.

**Table 4**
Effect of UI on food consumption — baseline results dropping even years.

| | 1968–2011 | 1968–1987 |
|---|---|---|
| | (2) | (3) |
| *A: Actual wages* | | |
| Repl. rate | 0.077 | 0.485* |
| | (0.163) | (0.277) |
| Mean food change | −0.09 | −0.09 |
| Mean repl. rate | 0.52 | 0.55 |
| Observations | 1955 | 722 |
| *B: Predicted wages* | | |
| Repl. rate | −0.260 | 0.231 |
| | (0.192) | (0.287) |
| Mean food change | −0.09 | −0.09 |
| Mean repl. rate | 0.52 | 0.54 |
| Observations | 1955 | 722 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID) after dropping the even years of data. The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$.**$p < 0.05$. *** $p < 0.01$.

**Table 5**
Effect of UI on food consumption — analyzing changes over time.

| | Actual wages | | Predicted wages | |
|---|---|---|---|---|
| | 1968–2011 | 1968–1997 | 1968–2011 | 1968–1997 |
| *A: Linear time trend* | | | | |
| Repl. rate | 0.248* | 0.604*** | 0.297* | 0.637*** |
| | (0.134) | (0.155) | (0.163) | (0.171) |
| Repl. rate ∗ trend | −0.008 | −0.031*** | −0.011 | −0.026** |
| | (0.006) | (0.009) | (0.007) | (0.010) |
| Mean food change | −0.07 | −0.06 | −0.07 | −0.06 |
| Mean repl. rate | 0.51 | 0.52 | 0.50 | 0.51 |
| Observations | 3383 | 2462 | 3383 | 2462 |
| *B: Decade interactions* | | | | |
| Repl. rate | 0.255 | 0.289 | 0.317 | 0.480** |
| | (0.176) | (0.172) | (0.206) | (0.190) |
| Repl. rate ∗ 1978–1987 | −0.040 | −0.062 | −0.145 | −0.178 |
| | (0.219) | (0.203) | (0.225) | (0.210) |
| Repl. rate ∗ 1990–1997 | −0.508*** | −0.460** | −0.521*** | −0.397** |
| | (0.169) | (0.190) | (0.174) | (0.191) |
| Repl. rate ∗ 1999–2011 | −0.146 | | −0.327 | |
| | (0.211) | | (0.216) | |
| p-Value equal decades | 0.02 | 0.04 | 0.04 | 0.12 |
| Mean food change | −0.07 | −0.06 | −0.07 | −0.06 |
| Mean repl. rate | 0.51 | 0.52 | 0.50 | 0.51 |
| Observations | 3383 | 2462 | 3383 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged (actual or predicted) weekly wage, and state linear time trends. In Panel B we display the p-values obtained from testing the null hypothesis that the coefficients on the interactions with each decade are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

We begin by simply analyzing whether there is a linear decline in the consumption smoothing effect of UI over time by interacting the replacement rate with a linear time trend. The results of this exercise are shown in Panel A of Table 5, where we use actual wages in the first two columns and predicted wages in the last two. When we look at the full sample period we find no evidence of a statistically significant linear decline. The estimated coefficient on the trend is substantially larger when we focus on the 1968–1997 period, however, and is statistically different from zero. This suggests that the period between 1987 and 1997 may differ from the later years in ways that are more nuanced than can be captured by the linear trend.

In order to more thoroughly investigate the possibility of treatment effect heterogeneity over time, we next move to a more flexible model, where we interact the replacement rate with dummies indicating the different decades in our sample window: the 1980s, 1990s, and 2000s. These results are shown in Panel B. The point estimates on all three decade interactions are negative, suggesting that the largest effects of UI were in the first decade. In addition, the point estimates indicate that the largest change relative to the first decade occurred in the 1990s. This is also the only decade in which the difference relative to the 1970s is statistically significant. Therefore the difference between our estimates using the full period and the 1968–1987 period is due mostly to a much smaller consumption smoothing effect in the 1990s. We also test whether the coefficients on all three interaction terms are jointly zero, and hence whether we can reject similar effects across the four decades. We find that the coefficients based on actual wages are statistically jointly different from zero, with a p-value of 0.02 in the full period, and 0.04 in 1968–1997. The pattern of effects is similar when using predicted

**Table 6**
Effect of UI on food consumption — robustness checks.

| | Baseline | | Food change | | Food imputation | | Temp. layoffs | | FS last month | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *A: Actual wages, 1968–2011* | | | | | | | | | | |
| Repl. rate | 0.100 | 0.255 | 0.188 | 0.367** | 0.087 | 0.185 | 0.058 | 0.269 | 0.117 | 0.286 |
| | (0.089) | (0.176) | (0.158) | (0.179) | (0.087) | (0.149) | (0.089) | (0.171) | (0.154) | (0.297) |
| Repl. rate * 1978–1987 | | −0.040 | | −0.043 | | 0.003 | | −0.154 | | −0.137 |
| | | (0.219) | | (0.250) | | (0.198) | | (0.163) | | (0.276) |
| Repl. rate * 1988–1997 | | −0.508*** | | −0.589*** | | −0.432*** | | −0.502*** | | −0.347 |
| | | (0.169) | | (0.184) | | (0.149) | | (0.184) | | (0.276) |
| Repl. rate * 1999–2011 | | −0.146 | | −0.197 | | −0.063 | | −0.235 | | 0.105 |
| | | (0.211) | | (0.208) | | (0.189) | | (0.197) | | (0.446) |
| p-Value equal decades | | 0.02 | | 0.00 | | 0.03 | | 0.05 | | 0.46 |
| Mean food change | −0.07 | −0.07 | −0.11 | −0.11 | −0.08 | −0.08 | −0.06 | −0.06 | −0.05 | −0.05 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 3383 | 3383 | 3689 | 3689 | 3563 | 3563 | 4310 | 4310 | 2457 | 2457 |
| *B: Actual wages, 1968–1997* | | | | | | | | | | |
| Repl. rate | 0.176 | 0.289 | 0.346 | 0.467** | 0.161 | 0.229 | 0.095 | 0.269* | 0.196 | 0.384 |
| | (0.131) | (0.172) | (0.221) | (0.197) | (0.125) | (0.155) | (0.102) | (0.160) | (0.155) | (0.284) |
| Repl. rate * 1978–1987 | | −0.062 | | −0.067 | | −0.021 | | −0.157 | | −0.166 |
| | | (0.203) | | (0.256) | | (0.185) | | (0.168) | | (0.268) |
| Repl. rate * 1988–1997 | | −0.460** | | −0.487** | | −0.372** | | −0.462** | | −0.342 |
| | | (0.190) | | (0.219) | | (0.177) | | (0.198) | | (0.300) |
| p-Value equal decades | | 0.04 | | 0.05 | | 0.11 | | 0.07 | | 0.50 |
| Mean food change | −0.06 | −0.06 | −0.10 | −0.10 | −0.07 | −0.07 | −0.05 | −0.05 | −0.04 | −0.04 |
| Mean repl. rate | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 | 0.51 | 0.51 | 0.52 | 0.52 |
| Observations | 2462 | 2462 | 2679 | 2679 | 2641 | 2641 | 3227 | 3227 | 2281 | 2281 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. In columns (3)–(6) we relax the last two restrictions. In columns (7) and (8) we include heads who report being on temporary layoff. In the last two columns we construct our outcome variable using food stamps received “last month”, available only in 1975–1987, 1990–1997, and 2009–2011, rather than food stamps “last year” (or two years prior in 1999–2011). All regressions include controls for state and year fixed effects, demographics, state unemployment rates and safety net expenditures, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across decades, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with each decade are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

wages, however we can no longer reject that the three interaction terms are jointly different from zero at conventional levels in the 1968–1997 period.

#### 5.2.1. Robustness checks

We now explore whether our estimates of the consumption smoothing effects of UI and their heterogeneity across decades are driven by any of our sample restrictions or variable choices. The results of these robustness checks are presented in Table 6, where Panels A and B contain results based on actual wages obtained with the full and the 1968–1997 periods, respectively.[20] In each panel, the first two columns contain our baseline results. The baseline results of the effect of UI in the 1968–1997 period is slightly larger than that for the full sample, but is still small and not statistically different from zero.

Columns (3)–(8) present results where we relax our sample restrictions. First, in columns (3) and (4) we include individuals who experience an absolute change in log food consumption between 1.1 and 2.[21] Then, in columns (5) and (6) we include those with imputed food consumption. As we relax these restrictions, our sample size increases, but the results remain similar. Last, since in the PSID prior to 1976 it is not possible to distinguish between unemployed individuals and those on temporary layoffs, and since in our main specifications we drop individuals who are on temporary layoff for the post-1976 period, we want to test whether this restriction is driving the smaller effects that we document in the 1990s. Therefore, in columns (7) and (8) we present the results obtained when we include individuals on temporary layoffs in the post-1976 period, and we find similar results. We also include the p-values obtained when testing the null that the effects of UI are equal across all decades, and in the full sample we can reject this null at least at the 5% level in all of these checks. In the 1968–1997 sample these p-values are generally larger, however we can generally rule out that the effects are similar across decades at the 10% level.

Finally, in the last two columns we change the way we measure food stamps in order to make the timing of this measure more consistent with that of the other types of food consumption. As discussed in the data section, this means using food stamp receipt “last month” rather than “last year”. This variable is only available in a subset of the survey years (1975–1987, 1990–1997, and 2009–2011), hence our sample size becomes somewhat smaller and the estimates become much more imprecise. When we use this alternative measure we find patterns consistent with our baseline model, with smaller effects in the 1990s compared to the 1970s.[22] However, the standard errors are large and we cannot reject that the effect is the same in the 1990s and the 1970s, or the same across all decades (testing the null that the effects of UI are equal across all decades yields a p-value of 0.46 for the full sample).

To summarize, in this section we found consistent evidence of changes in the magnitude of the effect of UI over time, and in particular a statistically significantly smaller effect in the 1990s as compared to the 1970s. An important caveat to our results is that this change over time is

[20] We also present the same checks using the predicted wages in Appendix Table A.1, finding similar results.

[21] Roughly 100 individuals experience a change in log consumption larger than 2 and the addition of these observations cause our estimates to become very noisy.

[22] Since not all years are included in the sample, the years represented by the decade interactions are slightly different than in our main model.

only measured for food consumption. In the subsections that follow, we explore whether our results differ when using a measure of imputed total consumption as our main outcome variable, and we analyze other dimensions of heterogeneity in the consumption smoothing effect of UI.

Because the results obtained with the actual and predicted wage replacement rates are quantitatively and qualitatively very similar, indicating small biases in the results with actual wages, and because we believe the results with actual wage replacement rates to be more precise, for the rest of our analysis we will use just the actual wage replacement rates.

### *5.3. Are results applicable to food consumption only?*

Our analysis so far has focused on food consumption only. Because food is a necessity and the fraction of income that households spend on food has declined over our sample period (see Appendix Fig. A.2), it is plausible that the responsiveness of food consumption to income shocks may have also changed over time, as job losers may first cut out consumption of other types of goods before reducing their food consumption. If this were the case, our findings of smaller consumption smoothing effects of UI in the 1990s may be applicable to food consumption only.

To examine if changes in the consumption basket over time could be driving our findings, we first analyze whether the effects of UI are heterogeneous with respect to the percent of income that the family spends on food two years prior to their unemployment spell (or five years prior in 1999–2011). We model this heterogeneity as both a linear interaction of the replacement rate with the fraction of income spent on food, and as interactions of the replacement rate with two indicators for the fraction of income spent on food that is above and below the median in our sample. The sample size is smaller than in our main estimates because we require all heads to be observed for three consecutive years instead of two (or six consecutive years in 1999–2011). Moreover, we drop individuals with the top 1% food budget share, who are outliers as they spend more then 150% of their budget on food. Therefore, in addition to testing for this type of heterogeneity we also re-estimate our baseline model with this new subsample. The results, shown in Table 7, indicate that the baseline estimates with this subsample are similar to our main results, confirming that the heads in this sample are similar to our main sample. We find no evidence of heterogeneous effects across food budget shares, suggesting that decreases in the percent of income spent on food are likely not driving the differential effect in the 1990s.

Apart from changes in the consumption basket, changes in the income elasticity of food could also explain our differential findings across decades. Since we are ultimately interested in the effect of UI on total consumption, we follow Bronchetti (2012) and analyze the effect of UI on imputed total household consumption. There are several methods used for imputing total consumption that vary in functional form and the specific variables used for the imputation. We present results for the imputation methodologies used in Skinner (1987), Fisher and Johnson (2006) and Blundell et al. (2008), which are highly cited in the literature. Skinner (1987) and Fisher and Johnson (2006) use the Consumer Expenditure Survey (CEX) data to regress total consumption on measures of food consumption, expenditures on housing and utilities, number of vehicles owned, and demographics in order to estimate the relationship between total consumption and these various measures (the specific variables used vary slightly across the two methods). Blundell et al. (2008) take a slightly different approach and estimate a demand function for food at home, where the components of the function are demographics, other food consumption and total non-durable consumption. We use the coefficients from each of these three methodologies — reported in Fisher and Johnson (2006) — to impute total consumption in our data. Since the PSID does not collect information about number of vehicles or expenditures on utilities in the 1990s, we calculate the mean values of these variables in our sample and assign these to observations in the 1990s. We do the same thing for missing observations of other variables in order to observe imputed total consumption for all observations in our sample.

We present the results using these measures of imputed total consumption in Table 8, with the results for the full period in Panel A and for the 1968–1997 period in Panel B. We show our baseline estimates of the food consumption smoothing effects of UI in columns (1) and (2) for ease of comparison, and the remaining columns (3) to (8) display the total consumption smoothing effects using the different methodologies described above. With all the methodologies we find evidence that the total consumption smoothing effect of UI is similar in magnitude to the food consumption smoothing effect. The only specification where this is not the case is the one utilizing

**Table 7**
Effect of UI on food consumption — heterogeneity by food budget share.

| | 1968–2011 | | | 1968–1997 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Repl. rate | 0.101 | 0.053 | | 0.188 | 0.117 | |
| | (0.129) | (0.164) | | (0.179) | (0.223) | |
| Repl. rate * percent food | | 0.223 | | | 0.324 | |
| | | (0.536) | | | (0.591) | |
| Repl. rate * low food | | | 0.088 | | | 0.208 |
| | | | (0.128) | | | (0.179) |
| Repl. rate * high food | | | 0.116 | | | 0.187 |
| | | | (0.156) | | | (0.202) |
| p-Value food low = high | | | 0.83 | | | 0.89 |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 |
| Observations | 2628 | 2628 | 2628 | 1872 | 1872 | 1872 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Percent spending on food defined two years before the observed spell of unemployment (or five in 1999–2011), hence all heads in our sample must be observed for three surveys in a row. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across size of food budget share, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with "low food" and "high food" are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table 8**
Effect of UI on imputed total consumption.

| | Food consumption | | Imputed total consumption | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | Skinner (1987) | | Fisher and Johnson (2006) | | Blundell et al. (2008) | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *A: 1968–2011* | | | | | | | | |
| Repl. rate | 0.100 | 0.254 | 0.131 | 0.255$^{*}$ | 0.219$^{***}$ | 0.323$^{**}$ | 0.031 | 0.476 |
| | (0.089) | (0.177) | (0.087) | (0.145) | (0.065) | (0.141) | (0.292) | (0.538) |
| Repl. rate ∗ 1978–1987 | | −0.040 | | 0.018 | | −0.050 | | −0.281 |
| | | (0.219) | | (0.167) | | (0.173) | | (0.545) |
| Repl. rate ∗ 1988–1997 | | −0.508$^{***}$ | | −0.316$^{**}$ | | −0.248 | | −1.053$^{**}$ |
| | | (0.169) | | (0.141) | | (0.152) | | (0.511) |
| Repl. rate ∗ 1999–2011 | | −0.146 | | −0.384$^{**}$ | | −0.154 | | −0.502 |
| | | (0.211) | | (0.162) | | (0.153) | | (0.465) |
| p-Value equal decades | | 0.02 | | 0.00 | | 0.04 | | 0.06 |
| Mean Y | −0.07 | −0.07 | −0.08 | −0.08 | −0.06 | −0.06 | −0.15 | −0.15 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 3383 | 3383 | 3375 | 3375 | 3383 | 3383 | 3314 | 3314 |
| *B: 1968–1997* | | | | | | | | |
| Repl. rate | 0.176 | 0.288 | 0.170 | 0.180 | 0.133 | 0.203 | 0.141 | 0.433 |
| | (0.131) | (0.173) | (0.117) | (0.138) | (0.084) | (0.127) | (0.433) | (0.571) |
| Repl. rate ∗ 1978–1987 | | −0.062 | | 0.054 | | −0.035 | | −0.251 |
| | | (0.203) | | (0.178) | | (0.172) | | (0.519) |
| Repl. rate ∗ 1988–1997 | | −0.460$^{**}$ | | −0.287$^{*}$ | | −0.295$^{*}$ | | −0.833 |
| | | (0.190) | | (0.153) | | (0.164) | | (0.520) |
| p-Value equal decades | | 0.04 | | 0.01 | | 0.03 | | 0.12 |
| Mean Y | −0.06 | −0.06 | −0.06 | −0.06 | −0.06 | −0.06 | −0.17 | −0.17 |
| Mean repl. rate | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 |
| Observations | 2462 | 2462 | 2461 | 2461 | 2462 | 2462 | 2412 | 2412 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Imputed total consumption is constructed using the estimates from Fisher and Johnson (2006), and is described in more detail in the text. In columns (3) and (4) with use the imputation procedure developed in Skinner (1987), in columns (5) and (6) we use the Fisher and Johnson (2006) methodology, and finally in columns (7) and (8) we use the Blundell et al. (2008) one. In columns (5)–(6) the imputation procedure directly imputes the log of total consumption. In columns (3)–(4) and (7)–(8) instead, the procedures impute the level of total consumption, which we transform into logs to form our dependent variable. Hence, a few observations with imputed total consumption of zero drop out in these analyses, leading to smaller sample sizes compared to (1)–(2) and (5)–(6). All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across decades, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with each decade are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses. * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

the Fisher and Johnson (2006) method for our full sample period, in which we find a larger and statistically significant consumption smoothing effect: a 10 percentage point increase in the replacement rate reduces the fall in consumption by 2.2% ($p < 0.01$). Additionally, we can still rule out homogeneous effects across decades in most time periods and imputation methodologies. Finally, while the differences between the 1990s and 1970s are not always statistically significant across specifications, the point estimates all indicate large differences in the effect between the two time periods, similar to our results for food.

We take this as suggestive evidence that our finding of heterogeneous effects over time could be broadly applied to total consumption and not just to food consumption. However, it is important to note that there are not large changes in demographic characteristics or the other variables used in the imputation upon job loss. Therefore most of the variation in our measure of changes in imputed total consumption comes from changes in food consumption, so we are cautious when comparing the effect of UI on total consumption to the one on food consumption based solely on this test. Given the similarity in heterogeneous effects across decades between the results obtained with food and imputed total consumption, in what follows we rely on the former as our main measure of consumption.

### *5.4. Differential effects by economic conditions*

In the previous sections we established that the consumption smoothing benefits of UI changed over time, and that most of this change was due to a statistically significantly smaller effect in the 1990s relative to the 1970s. In this section we explore whether the low unemployment rates in the 1990s compared to the other three decades, shown in Fig. 1, might explain some of these differential effects.

The effect of UI may vary with economic conditions for a number of different reasons. First, the characteristics of unemployed individuals may differ during recessions relative to periods of economic expansions. Moreover, UI benefit extensions generally occur when states experience unusually high unemployment rates, and unemployment rates are highly correlated with UI take-up rates (Anderson and Meyer, 1997), as shown by Fig. 2. Benefit extensions and higher take-up rates may both lead to higher average benefits per capita, and thus possibly larger consumption smoothing effects. Lastly, during periods with very high unemployment rates the average duration of unemployment is longer so individuals might draw down private savings and become more reliant on UI, also leading to larger effects. We next explore whether UI has larger consumption smoothing effects during “bad” economic times — as measured by high state unemployment rates.

Columns (1) and (6) of Table 9 present our baseline results for the full and the 1968–1997 periods, respectively. Columns (2), (3), (7) and (8) show results where the effect of the replacement rate is allowed to vary by state's economic conditions. We model these heterogeneous effects in two ways. First, we allow the effect to vary linearly with the state unemployment rate by interacting the replacement rate with the unemployment rate. Second, we construct

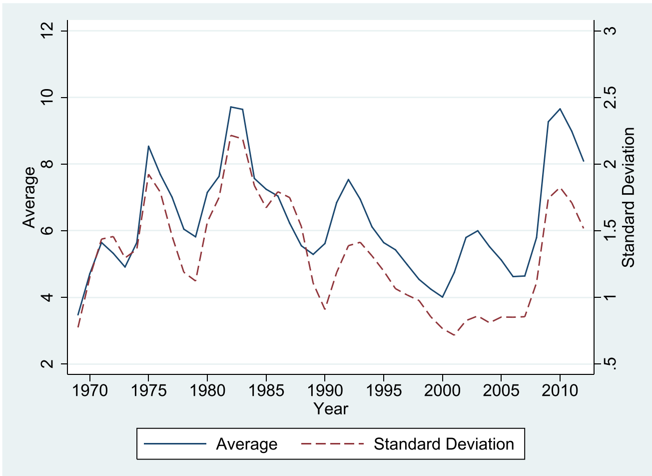


**Fig. 1.** State unemployment rates.
Notes: Data on state unemployment rates from the 1976–2011 period are from the Bureau of Labor Statistics (BLS), and state unemployment rates for 1968–1975 are obtained from Moffitt's welfare benefits file at http://www.econ2.jhu.edu/people/moffitt/datasets.html. National averages and standard deviations are computed using state population weights from the National Cancer Institute SEER data (http://seer.cancer.gov/popdata/download.html).

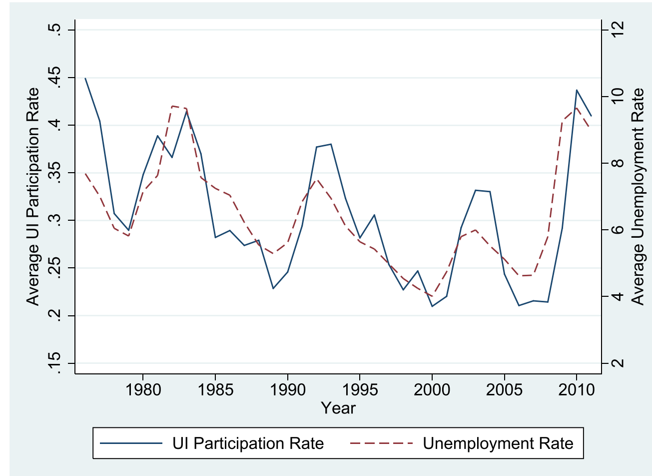


**Fig. 2.** UI participation and state unemployment rates.
Notes: UI participation rates are computed using data from the 1977–2012 March supplement of the Current Population Survey (CPS), which contain data for calendar years 1976–2011. We restrict the sample to include only unemployed heads of household, and then calculate the national yearly (weighted) percent of unemployed population with positive UI earnings. Data on state unemployment rates from the 1976–2011 period are from the Bureau of Labor Statistics (BLS). National averages are computed using state population weights from the National Cancer Institute SEER data (http://seer.cancer.gov/popdata/download.html).

dummy variables that indicate whether an individual is unemployed during a period of low, medium or high state unemployment, defined as periods with unemployment rates below 6%, between 6% and 9.3%, and above 9.3%, respectively. We interact these dummies with the replacement rate.[23] The latter model tests whether the effects are non-linear with respect to the state unemployment rate, which may be an important distinction since benefit extensions are determined non-linearly with respect to the state unemployment rate.

In the full sample, the estimated coefficient on the interaction term with the unemployment rate is positive, as expected, but not statistically different from zero. In the second model, the estimated effects are statistically insignificant in times of low and medium unemployment rates, but large and statistically significant in times of high unemployment rates, indicating that UI plays a large role in smoothing consumption during the worst economic conditions. However, these estimates are imprecise, and a statistical test of equality of the estimated coefficients across economic conditions yields a p-value of 0.20.[24] Hence, we cannot reject that the effects of UI are similar in periods of low, medium and high unemployment rates.

The results for the 1968–1997 period present similar patterns. The interaction with the linear unemployment rate is larger than for the full sample and statistically different from zero. Similarly, in the non-linear model the test for the equality of effects across economics conditions rejects the null hypothesis of equality of effects (p-value = 0.00). We take these results as strongly suggestive that the consumption smoothing effects are larger when the unemployment rate is high, with the caveat that we do not have enough precision in the full period to reject that the effect in the same across economic conditions.

As mentioned earlier, variation in the consumption smoothing effects of UI across the business cycle could be due to differences in take-up rates, benefit extensions, selection into unemployment spells or changes in unemployment duration. We cannot separately identify the role of differences in take-up rates due to the correlation between take-up rates and unemployment rates (see Fig. 2). Similarly, it is hard to analyze the role played by variation in unemployment duration because it is endogenous to the UI system. We can, however, attempt to address the other two possibilities.

We start by constructing an indicator for whether the unemployed individual is observed in a state, month and year during which the maximum weeks of benefits was extended.[25] In columns (4) and (9) of Table 9 we first show whether the effect of the replacement rate depends on the presence of UI extensions or not. As expected, given that extensions lead to higher total yearly benefits, the effect is larger in periods with extensions, and we are able to reject at the 10% significance level that two coefficients are the same (p = 0.08 in both the full sample and in 1968–1997). In columns (5) and (10) we implement a "horse-race" regression to test if extensions can explain all of the heterogeneous effects by the value of the unemployment rate. However, extensions are highly correlated with the unemployment rate, so while the results indicate that extensions alone cannot explain the variation by unemployment rate, these results are only suggestive.

We also check whether the differential effects could be driven by differential selection on observables into unemployment. Following Kroft and Notowidigdo (2014), we add an interaction between the replacement rate and observable demographic characteristics. Recall that these demographics are already controlled for in our models, so overall differences based on these are accounted for, and these

[23] We picked these unemployment rates because they represent the 50th and 90th (population weighted) percentiles of state unemployment rates for the 1968–2011 period. As a sensitivity check, we also use the 80th and 85th unemployment percentiles to define the worst economic conditions. These results are very similar.

[24] This is the p-value obtained from a statistical test where the null hypothesis is that the three coefficients on the interactions between the unemployment rate indicators and the replacement rates are statistically all equal to each other.

[25] We use this indicator variable for the presence of extensions instead of the number of weeks of extensions because data on weeks of Extended and Emergency benefits is not currently available prior to the 1990s. To construct this indicator we use two different data sources. First, we use information on the number of weeks of Extended and Emergency benefits that are available to the newly unemployed in each month between 2003 and 2012 from Mueller et al. (2013). Second, we use Department of Labor (DOL) administrative data on monthly state expenditures on Extended and Emergency benefits for the years 1971–2012 from Bitler and Hoynes (2013). By combining these two data sources we determine the minimum state spending when each of these programs is in place during the 2003–2013 period. Then we create an indicator for the presence of extended benefits if the state spending on these programs was above this minimum in any given year. The advantage of this measure over using just the expenditure data is that it reduces the likelihood of misclassifying periods where there were no extended benefits available to the newly unemployed but there was a small amount of spending on these programs. This misclassification arises following recessions when the newly unemployed are not eligible for extended benefits, but individuals who became unemployed during the recession are still receiving some payments from the extended benefit programs.

**Table 9**
Effect of UI on food consumption — heterogeneity by economic conditions.

| | 1968–2011 | | | | | 1968–1997 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Repl. rate | 0.100 | −0.094 | | | | 0.176 | −0.403 | | | |
| | (0.089) | (0.265) | | | | (0.131) | (0.299) | | | |
| Repl. rate ∗ UR | | 0.026 | | | | | 0.075** | | | |
| | | (0.033) | | | | | (0.035) | | | |
| Repl. rate ∗ UR 0–6% | | | −0.046 | | −0.055 | | | −0.096 | | −0.095 |
| | | | (0.135) | | (0.138) | | | (0.194) | | (0.207) |
| Repl. rate ∗ UR 6–9.3% | | | 0.082 | | 0.028 | | | 0.057 | | 0.039 |
| | | | (0.123) | | (0.152) | | | (0.155) | | (0.196) |
| Repl. rate ∗ UR 9.3+% | | | 0.311** | | 0.229 | | | 0.652*** | | 0.646** |
| | | | (0.139) | | (0.185) | | | (0.171) | | (0.243) |
| Repl. rate ∗ no extensions | | | | −0.011 | | | | | 0.018 | |
| | | | | (0.123) | | | | | (0.186) | |
| Repl. rate ∗ extensions | | | | 0.220** | 0.088 | | | | 0.310** | 0.007 |
| | | | | (0.098) | (0.141) | | | | (0.118) | (0.160) |
| p-Value UR 0–6% = 9.3+% | | | 0.08 | | 0.18 | | | 0.00 | | 0.00 |
| p-Value UR 0–6% = 6–9.3% = 9.3+% | | | 0.20 | | 0.37 | | | 0.00 | | 0.00 |
| p-Value no Ext = Ext | | | | 0.08 | | | | | 0.08 | |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 | 0.52 | 0.52 |
| Observations | 3383 | 3383 | 3383 | 3383 | 3383 | 2462 | 2462 | 2462 | 2462 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. Our main sample excludes individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Unemployment rate categories are at the state by year level and signify the 0–50th percentiles, the 50–90th percentiles, and the 90–100th percentiles defined using BLS unemployment rates and population weights. UI benefit extensions are defined as the presence of extensions when the head is observed to be unemployed. See text for more details on the construction of this variable. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across economic conditions, we display the p–values obtained from several tests: we test the null hypotheses that the effects of UI are equal during times of low and high unemployment rates, and we test the null hypotheses that the coefficients on the interactions with the three unemployment rate categories are equal to each other. In the specifications that allow for differential effects in the presence or absence of benefit extensions, we display the p-values obtained when testing the null that the coefficients on the two interactions are equal to each other. All results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

interactions explore whether there are heterogeneous effects across these demographic attributes. If differential demographics are driving our results, we would expect the estimated coefficient on the interaction between the replacement rate and local economic conditions to weaken when the additional interactions are included. Appendix Table A.2 shows the pattern of heterogeneous effects by economic conditions is stable when we include these interactions. This suggests that the differential effects by state unemployment rate are not driven by observable variation in the demographic characteristics of unemployed individuals.

Our results differ somewhat from Kroft and Notowidigdo (2014), who analyze whether optimal UI benefits should vary with the unemployment rate. Their main focus is on whether the effects of replacement rates on unemployment duration vary with the business cycle, but they also study the impact of the replacement rate on consumption smoothing. In their analysis that is most similar to ours, they use the 1968–1987 PSID to replicate Gruber (1997), and then include an interaction of the replacement rate with the log of the state unemployment rate (scaled by the national unemployment rate). Their point estimates indicate larger effects when the unemployment rate is high, however their estimated standard errors are large and they cannot rule out similar effects across periods of high and low unemployment rates. Importantly, we find further suggestive evidence that the effects vary with the unemployment rate, and that individuals living in states with the highest unemployment rate experience the largest consumption smoothing effects. However, our estimates are not precise enough to reject that the effect is the same across economic conditions.

To summarize, we find suggestive evidence that the consumption smoothing effects of UI kick in when the economy is at its worst, and that UI provides substantive protective power in times of need. We show that these differential effects are not driven by differential selection on observables into unemployment, so they are likely driven by changes in take-up rates, extensions of benefits and changes in durations of unemployment, which are all highly correlated with the business cycle.

### 5.5. *Differential effects by UI generosity*

A second phenomena that might contribute to the smaller consumption smoothing benefit of UI in the 1990s is the falling value of the average after-tax replacement rate that took place over our sample period, shown in Fig. 3. This fall was due to a number of factors, including states' failure to increase benefits to keep up with inflation, and a major change in the tax treatment of UI benefits in 1982. Before 1982, UI benefits were only taxable through social security taxes, but after 1982 they became subject to income taxes, which resulted in a significant decline in their after-tax value.[26]

Changes in the overall generosity of the UI program may affect consumption smoothing for two reasons: first, because the take-up of UI benefits is correlated with the benefit amount, (Anderson and Meyer, 1997) and second, there may be local average treatment effects, implying that for consumption smoothing to occur UI benefits must be above a certain level.[27] Because we are measuring benefits that an individual is eligible for, not the amount that they actually receive, if take-up declines we may observe a decline in the Intent To Treat (ITT) effect (the effect we

[26] In some states individuals have the option to have these taxes taken out of their unemployment check, but this is not automatically done, so the salience of these taxes is unclear. However, evidence from Anderson and Meyer (1997) suggests that the tax rate on benefits negatively affects take-up of benefits.

[27] In order to have consumption smoothing this cutoff does not have to be sharp. For example, it may be that these consumption smoothing benefits slowly increase from zero as UI becomes more generous. We empirically test whether participation is affected by benefit generosity using the Current Population Survey and find that more generous benefits are correlated with higher participation, and that this relationship may be non-linear: as benefits increase the responsiveness of participation becomes even larger.

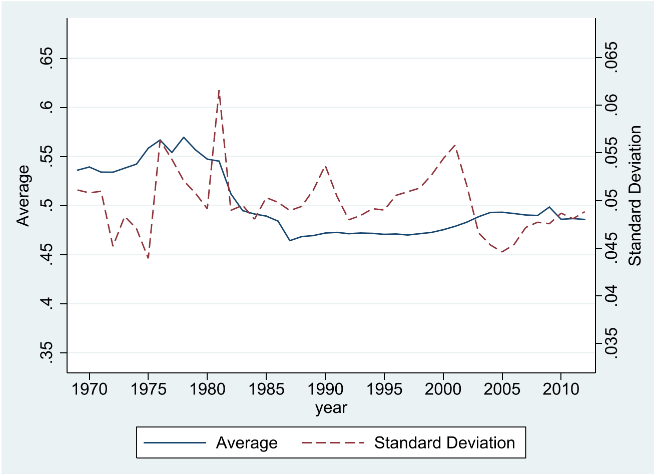


**Fig. 3.** State average simulated replacement rates.
Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. To construct this figure, we use this full sample of unemployed, and assign all observations to every state and every year. We then run this simulated sample through our UI benefits calculator, and then collapse the sample to have an average simulated replacement rate for each state and year. National averages and standard deviations are computed using state population weights from the National Cancer Institute SEER data (http://seer.cancer.gov/popdata/download.html).

are measuring) even if the Treatment on the Treated (TOT) effect (the effect for individuals who actually receive the benefits) stays constant.

To test whether or not there are heterogeneous effects at different average values of the replacement rate, we first create a measure of state average UI generosity—a state by year simulated replacement rate that is constructed by assigning our fixed, national sample of unemployed heads to every state and year consecutively, and calculating the average state replacement rate for that state and year. Hence this measure captures variation in state UI laws only, and is not influenced by individual characteristics in any way. We then model the heterogeneity in two ways. First, we interact the individual replacement rate with the measure of state generosity. Second, we create indicators for high (above median) and low (below median) state generosity, and interact the individual replacement rates with these indicators. These results are presented in columns (2), (3), (5) and (6) of Table 10.

As expected, the effect of UI on consumption smoothing is linearly increasing with state UI generosity, although the coefficient on this interaction term is statistically insignificant. Similarly, when looking at the indicators for high and low generosity, the largest values of state generosity lead to the largest consumption smoothing effects, and for the smallest values the consumption smoothing effects are small and imprecise. The p-values on tests for equality of the two coefficients in the non-linear model show that the effect is statistically significantly different at above and below median generosity at the 10 percent level for the full period, and at the 5 percent level in the 1968–1997 period. Thus these results suggest that the consumption smoothing effects are concentrated among individuals who happen to live in states and years with above median generosity. However, we are unable to differentiate whether this is due to higher take-up in these more generous states, or to there being a local average treatment effect independent of take-up.

We also explore two alternative explanations that might contribute to the smaller consumption smoothing benefit of UI in the 1990s relative to the 1970s: changes in the demographics of our sample over time, and changes in the generosity of other safety net programs during the 1990s. We therefore test whether the consumption smoothing effect of UI differs by observable demographics or state safety net spending, finding no evidence of heterogeneity across these dimensions. We conclude that these changes cannot significantly contribute to the smaller effect of UI in the 1990s. These results are discussed in detail in Appendix A.

### 5.6. Explaining the small effect in the 1990s

Our next step is to test whether the heterogeneous effects by economic conditions and UI generosity drive any of the smaller effects that we estimate for the 1990s. Therefore we conduct “horse-race” regressions in which we include the interactions of the replacement rate with indicators for the different decades in our sample together with: 1) the replacement rate interacted with the indicators for economic conditions, or 2) replacement rate interacted with the indicators for high and low state UI generosity. If either of these two heterogeneous effects explains at least part of the decline in the 1990s, the coefficient on the interaction with the third decade should decrease.

Columns (2) and (6) of Table 11 suggest that including the interactions with high, medium or low unemployment rates can account for a portion of the decline. For the full period, the coefficient of the interaction with the third decade falls by around 13%, from −0.508 to −0.444, while for the 1968–1997 this fall is even larger.[28] In columns (3) and (7) we include the interactions with high and low UI generosity to test whether the changing generosity of the replacement rate over time is part of the reason we observe smaller consumption smoothing effects in the 1990s. As expected, once we take account of the changing value of the replacement rate the coefficient on the interaction with the third decade falls by around 16% in both periods.

Since both changes in the economy and the average state replacement rate explain part of the decline in the consumption smoothing effects of UI in the 1990s, we also analyze how much of it they can explain jointly. Hence in the remaining columns we include both the interactions with the different average values of the replacement rate and the interactions with the low, middle and high state unemployment rates. These results suggest that these two explanations can account for roughly 30 to 46% of the smaller effects found in the 1990s.[29] Additionally, accounting for these heterogeneous effects causes the coefficients on the 1980s and 2000s interaction terms to become much smaller and closer to zero. However, since the estimated coefficients on these interaction terms are never statistically different from the effect in the 1970s, we cannot determine precisely whether there were changes in the effect in these decades or whether the economy and program generosity explain any differences in these decades. The coefficients on the interactions with the above median replacement rate and the unemployment rate categories are jointly statistically significant ($p=0.03$ in the full period and $p=0.00$ in 1968–1997), indicating that there are still heterogeneous effects on these dimensions after accounting for changes over time. Finally, once we account for these heterogeneous effects, the effect of UI is only marginally statistically heterogeneous across decades in the full period and is not statistically heterogeneous across decades in 1968–1997 ($p = 0.06$ in the full period and $p = 0.22$ in 1968–1997).

## 6. Conclusion

The Great Recession generated new interest in the UI program and its impact on job search, along with its implications for the level of optimal UI benefits (Rothstein, 2011; Landais et al., 2010; Kroft and Notowidigdo,

[28] We calculate these percentages by first taking the difference in the coefficient on the 1990s interaction term from column (1) to (2), and (5) to (6), and then dividing these differences by the coefficient on the 1990s interaction term in columns (1) and (5), respectively.

[29] We calculate these percentages by first taking the difference in the coefficient on the 1990s interaction term from column (1) to (4), and (5) to (8), and then dividing these differences by the coefficient on the 1990s interaction term in columns (1) and (5), respectively. Bootstrapping gives us the 95% confidence intervals for the estimates of how much the changing economic conditions and generosity of UI explain the decline. These confidence intervals are 7% to 75% in the full sample period and 18% to 94% in 1968–1997.

**Table 10**
Effect of UI on food consumption — heterogeneity by average UI generosity.

| | 1968–2011 | | | 1968–1997 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Repl. rate | 0.100 | −0.246 | | 0.176 | −0.257 | |
| | (0.089) | (0.611) | | (0.131) | (0.519) | |
| Repl. rate * st. repl. rate | | 0.632 | | | 0.764 | |
| | | (1.104) | | | (0.903) | |
| Repl. rate * st. repl. rate 0–50% | | | −0.023 | | | −0.129 |
| | | | (0.184) | | | (0.218) |
| Repl. rate * st. repl. rate 50+% | | | 0.416*** | | | 0.449*** |
| | | | (0.147) | | | (0.155) |
| p-Value RR low = high | | | 0.09 | | | 0.02 |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 |
| Observations | 3383 | 3383 | 3383 | 2462 | 2462 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Replacement rate categories are defined at the state by year level as above and below median state average simulated replacement rate (population weighted). All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects by average UI generosity, we display the p-values obtained when testing the null hypotheses that the coefficients on the interactions with above and below median generosity are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table 11**
Effect of UI on food consumption — explaining the smaller effects in the 1990s.

| | 1968–2011 | | | | 1968–1997 | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Repl. rate | 0.255 | | | | 0.289 | | | |
| | (0.176) | | | | (0.172) | | | |
| Repl. rate * 1978–1987 | −0.040 | −0.041 | −0.012 | −0.009 | −0.062 | −0.072 | −0.006 | −0.005 |
| | (0.219) | (0.212) | (0.233) | (0.227) | (0.203) | (0.184) | (0.212) | (0.193) |
| Repl. rate * 1988–1997 | −0.508*** | −0.444*** | −0.428** | −0.356** | −0.460** | −0.337** | −0.386** | −0.249 |
| | (0.169) | (0.164) | (0.171) | (0.167) | (0.190) | (0.167) | (0.192) | (0.169) |
| Repl. rate * 1999–2011 | −0.146 | −0.089 | −0.042 | 0.023 | | | | |
| | (0.211) | (0.211) | (0.215) | (0.213) | | | | |
| Repl. rate * UR 0–6% | | 0.103 | | −0.062 | | 0.027 | | −0.406 |
| | | (0.211) | | (0.303) | | (0.214) | | (0.296) |
| Repl. rate * UR 6–9.3% | | 0.226 | | 0.061 | | 0.167 | | −0.249 |
| | | (0.189) | | (0.319) | | (0.190) | | (0.275) |
| Repl. rate * UR 9.3+% | | 0.392** | | 0.238 | | 0.707*** | | 0.300 |
| | | (0.195) | | (0.301) | | (0.202) | | (0.279) |
| Repl. rate * st. repl. rate 0–50% | | | 0.111 | | | | −0.049 | |
| | | | (0.292) | | | | (0.275) | |
| Repl. rate * st. repl. rate 50+% | | | 0.491** | 0.410* | | | 0.491** | 0.631*** |
| | | | (0.207) | (0.244) | | | (0.192) | (0.225) |
| p-Value equal decades | 0.02 | 0.03 | 0.03 | 0.06 | 0.04 | 0.12 | 0.07 | 0.22 |
| p-Value UR 0–6% = 6–9.3% = 9.3+% | | 0.30 | | 0.26 | | 0.00 | | 0.00 |
| p-Value RR low = high | | | 0.12 | | | | 0.03 | |
| p-Value UR, RR jointly significant | | | | 0.03 | | | | 0.00 |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 | 0.52 |
| Observations | 3383 | 3383 | 3383 | 3383 | 2462 | 2462 | 2462 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Replacement rate categories are defined at the state by year level as above and below median state average simulated replacement rate (population weighted). Unemployment rate categories are at the state by year level and signify the 0–50th percentiles, the 50–90th percentiles, and the 90–100th percentiles defined using BLS unemployment rates and population weights. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across decades, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with each decade are equal to each other. In the specifications that allow for differential effects across economic conditions, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with the three unemployment rate categories are equal to each other. In the specifications that allow for differential effects by average UI generosity, we display the p-values obtained when testing the null hypotheses that the coefficients on the interactions with above and below median generosity are equal to each other. Finally the p-values "UR, RR Jointly Significant" are obtained from testing the null hypothesis that the coefficients on the interactions with above median generosity and the three unemployment rate categories are jointly significant. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses. * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

2014), however very little research has focused on determining the benefits of the program. Given that the main objective of the UI program is to provide insurance against the risk of unemployment, and as such to make people better off during periods of unemployment, knowing the magnitude of these benefits is key to designing optimal policy.

We use data from 1968–2011 to provide a new estimate of the consumption smoothing benefit of UI, and we find our estimate to be small relative to the ones previously found in the literature. We attribute this small overall estimate to heterogeneity in the effect over time, and in particular to a very small effect in the 1990s compared to the 1970s, when the effect of UI is more similar to the existing estimates. An important caveat to this result is that it relates to food consumption only. Hence we explore the possibility that the effects of UI may be different for total consumption, finding no consistent evidence of this.

We then explore possible mechanisms that might explain the smaller effects estimated in the 1990s. First, since the 1990s were characterized by relatively low unemployment rates, we analyze whether the consumption smoothing effect of UI is heterogeneous across economic conditions. We find evidence that the effect is larger when the state unemployment rate is very high, however we do not have the statistical power to estimate this heterogeneity with certainty. Second, we test whether the effect of UI is smaller when overall program generosity is lower, as it was in the 1990s. We do this by analyzing whether the consumption smoothing effect is larger in states and years with higher replacement rates, and we find suggestive evidence in support of this hypothesis. Once we account for changes in overall program generosity and changes in economic conditions, the difference in the consumption smoothing effect in the 1990s compared to the 1970s declines by about 30–46%. Hence some of the differential effect remains unexplained.

Our findings have important implications for the literature on the optimal benefits of UI, suggesting that to calculate optimal UI benefits new estimates of the moral hazard effects of UI should be accompanied by new estimates of its benefits. Moreover, our findings also inform the recent literature on the optimal level on UI benefits across economic conditions (Landais et al., 2010; Schmieder et al., 2012; Kroft and Notowidigdo, 2014; Lalive et al., 2013), which finds that the moral hazard effects are UI are smaller in periods characterized by high unemployment rates. Combing our results with this literature suggests that the optimal level of benefits should be positively related to unemployment rates.

## Appendix A

### *A.1. Timing of food consumption variable*

The assumption about the timing of food consumption is important for our analysis. We follow the literature and assume that the food consumption questions refer to the current survey year (Zeldes, 1989; Gruber, 1997; Fisher and Johnson, 2006). However, since in the PSID the questions about income receipt generally collect information regarding the calendar year prior to the survey, it is possible that households confuse the time periods and provide information about food consumption in the calendar year prior to the survey. If this were the case, our outcome variable — change in food consumption — would not be measuring the drop in consumption upon job loss. This would lead to some bias in our estimates of the effect of UI on food consumption, and the magnitude and direction of this bias would depend on the length of the unemployment spells of the individuals in our sample.

If unemployment spells are short, then in the year prior to the survey (t-1) the individual will be employed for most or all of the year. Thus consumption collected in survey year t would capture consumption in year t-1, when the individual was employed, and the change in consumption would not measure the fall in consumption upon unemployment. In this case the estimated effect may be biased, although the direction of this bias is unclear and we next consider several specific cases. First, if year-to-year changes in consumption while individuals remain employed are uncorrelated with UI generosity, then this would bias our estimated coefficient towards zero due to measurement error. On the other hand, if these consumption changes are related to UI generosity, for example if individuals anticipate job loss and know the amount of UI benefits they will receive, then the bias could be positive or negative. If more generous UI benefits cause consumption to remain more similar in years prior to the job loss because individuals anticipate a smaller drop in income upon job loss, then we would estimate a positive consumption smoothing effect, even though we would not be measuring the drop in consumption upon unemployment. Finally, if instead more generous benefits cause the individual to reduce consumption by more than they would have otherwise done prior to the job loss, perhaps due to the expectation of longer unemployment spell duration, then this would cause the estimated consumption smoothing effect to be negative.

Considering the case of long unemployment spells, in which individuals may spend a large part of t-1 unemployed, our measure of consumption changes will still measure the drop in consumption upon job loss, and the exact timing of the consumption variables would be less of an issue for our estimates.

### *A.2. Other explanations of smaller effect in 1990s*

While changing economic conditions and the generosity of UI can explain part of the decline in consumption smoothing in the 1990s relative to the 1970s, they do not explain all of it. We thus explore alternative explanations for the heterogeneous effects over time.

First, we examine whether there were changes in the observable characteristics of our sample, which is a concern if there are large changes over time in what determines the replacement rate (number of kids and wages), or in observables that might be correlated with individuals' ability to smooth consumption (for example, wealth, which we proxy with education). As shown in Table 1, the sample of unemployed heads is indeed aging, becoming more educated, and is more likely to be nonwhite, married, and with fewer children over time. Therefore, we test whether the effect of UI is heterogeneous across these dimensions by allowing the effect of the replacement rate to vary across these observables.[30] The results, shown in Appendix Table A.3, provide no evidence of such heterogeneous effects, with the exception of mixed results for heterogeneity across educational achievement. As expected, when analyzing whether UI differentially affects heads with at least some college attendance compared to heads with at most a high school degree, we find smaller effects for more educated individuals. However, these results are statistically significantly different by education only for the full 1968–2011 period, and not for 1968–1997, which leads us to believe that changing educational achievement could not be driving the smaller effects found in the 1990s.

Our next concern is the many changes to the US safety net that have occurred since the 1980s, which might affect the consumption smoothing effects of UI. Through welfare reform, which strengthened work requirements and time limits for receipt of TANF, and expansions of the EITC, low income people have a much larger fraction of their income made up of a combination of in-work tax credits and earnings rather than AFDC/TANF (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Eissa and Hoynes, 2006). In addition the eligibility requirements for food stamps have become less strict, leading to more working poor receiving food stamps (Bitler and Hoynes, 2010). To test whether changes in the social safety net can explain the estimated changes in consumption smoothing effects over time, we utilize state by year data on food stamps and AFDC/TANF spending per capita to test whether states that spend relatively more on these programs have larger UI consumption smoothing effects. Appendix Table A.4 shows that we do not find any evidence of such heterogeneity.

[30] These demographics are already controlled for in our models, so overall differences based on these characteristics are already accounted for. Therefore these interactions simply test for heterogeneous effects by these demographics.

**Table A.1**
Effect of UI on food consumption — robustness checks with predicted wages.

| | Baseline | | Food change | | Food imputation | | Temp. layoffs | | FS last month | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *A: Predicted wages, 1968–2011* | | | | | | | | | | |
| Repl. rate | 0.115 | 0.317 | 0.047 | 0.297 | 0.129 | 0.255 | 0.078 | 0.314* | 0.124 | 0.286 |
| | (0.157) | (0.206) | (0.236) | (0.285) | (0.156) | (0.203) | (0.131) | (0.171) | (0.135) | (0.287) |
| Repl. rate ∗ 1978–1987 | | −0.145 | | −0.233 | | −0.063 | | −0.207 | | −0.154 |
| | | (0.225) | | (0.289) | | (0.218) | | (0.189) | | (0.265) |
| Repl. rate ∗ 1988–1997 | | −0.521*** | | −0.559** | | −0.424** | | −0.475*** | | −0.326 |
| | | (0.174) | | (0.261) | | (0.195) | | (0.142) | | (0.298) |
| Repl. rate ∗ 1999–2011 | | −0.327 | | −0.292 | | −0.244 | | −0.435** | | 0.460 |
| | | (0.216) | | (0.254) | | (0.234) | | (0.193) | | (0.459) |
| p-Value equal decades | | 0.04 | | 0.16 | | 0.20 | | 0.01 | | 0.25 |
| Mean food change | −0.07 | −0.07 | −0.11 | −0.11 | −0.08 | −0.08 | −0.06 | −0.06 | −0.05 | −0.05 |
| Mean repl. rate | 0.50 | 0.50 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 3383 | 3383 | 3689 | 3689 | 3563 | 3563 | 4310 | 4310 | 2457 | 2457 |
| *B: Predicted wages, 1968–1997* | | | | | | | | | | |
| Repl. rate | 0.312** | 0.480** | 0.345 | 0.535* | 0.306** | 0.412** | 0.234* | 0.419*** | 0.229* | 0.409 |
| | (0.145) | (0.190) | (0.227) | (0.275) | (0.144) | (0.186) | (0.119) | (0.146) | (0.121) | (0.261) |
| Repl. rate ∗ 1978–1987 | | −0.178 | | −0.249 | | −0.106 | | −0.202 | | −0.198 |
| | | (0.210) | | (0.300) | | (0.208) | | (0.175) | | (0.245) |
| Repl. rate ∗ 1988–1997 | | −0.397** | | −0.212 | | −0.309 | | −0.336* | | −0.189 |
| | | (0.191) | | (0.281) | | (0.221) | | (0.173) | | (0.338) |
| p-Value equal decades | | 0.12 | | 0.69 | | 0.38 | | 0.13 | | 0.72 |
| Mean food change | −0.06 | −0.06 | −0.10 | −0.10 | −0.07 | −0.07 | −0.05 | −0.05 | −0.04 | −0.04 |
| Mean repl. rate | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 | 0.52 | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 2462 | 2462 | 2679 | 2679 | 2641 | 2641 | 3227 | 3227 | 2281 | 2281 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. In columns (3)–(6) we relax the last two restrictions. In columns (7) and (8) we include heads who report being on temporary layoff. In the last two columns we construct our outcome variable using food stamps received "last month", available only in 1975–1987, 1990–1997, and 2009–2011, rather than food stamps "last year" (or two years prior in 1999–2011). All regressions include controls for state and year fixed effects, demographics, state unemployment rates and safety net expenditures, a linear spline of the lagged predicted weekly wage, and state linear time trends. In the specifications that allow for differential effects across decades, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with each decade are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table A.2**
Effect of UI on food consumption — heterogeneity by economic conditions and demographics of head.

| | 1968–2011 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Repl. rate ∗ R 0–6% | −0.046 | −0.074 | −0.031 | 0.015 | −0.033 | 0.099 | 0.035 | 0.281 |
| | (0.135) | (0.189) | (0.145) | (0.147) | (0.143) | (0.163) | (0.145) | (0.245) |
| Repl. rate ∗ UR 6–9.3% | 0.082 | 0.058 | 0.096 | 0.146 | 0.090 | 0.198 | 0.145 | 0.348 |
| | (0.123) | (0.167) | (0.130) | (0.181) | (0.129) | (0.145) | (0.136) | (0.255) |
| Repl. rate ∗ UR 9.3% | 0.311** | 0.282* | 0.322** | 0.379* | 0.319** | 0.423*** | 0.387** | 0.580** |
| | (0.139) | (0.161) | (0.141) | (0.200) | (0.137) | (0.146) | (0.160) | (0.243) |
| Repl. rate ∗ ages 31–45 | | 0.084 | | | | | | 0.210 |
| | | (0.184) | | | | | | (0.192) |
| Repl. rate ∗ ages 46–93 | | −0.031 | | | | | | 0.052 |
| | | (0.171) | | | | | | (0.201) |
| Repl. rate ∗ female | | | −0.061 | | | | | −0.163 |
| | | | (0.215) | | | | | (0.296) |
| Repl. rate ∗ married | | | | −0.117 | | | | −0.140 |
| | | | | (0.195) | | | | (0.275) |
| Repl. rate ∗ black | | | | | −0.062 | | | −0.143 |
| | | | | | (0.173) | | | (0.197) |
| Repl. rate ∗ some college | | | | | | −0.306** | | −0.375** |
| | | | | | | (0.126) | | (0.144) |
| Repl. rate ∗ num. kids | | | | | | | −0.059 | −0.085** |
| | | | | | | | (0.049) | (0.041) |
| p-Value UR 0–6% = 6–9.3% = 9.3+% | 0.20 | 0.20 | 0.22 | 0.20 | 0.22 | 0.23 | 0.21 | 0.27 |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 | 0.51 |
| Observations | 3383 | 3383 | 3383 | 3383 | 3383 | 3383 | 3383 | 3383 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. Unemployment rate categories are at the state by year level and signify the 0–50th percentiles, the 50–90th percentiles, and the 90–100th percentiles defined using BLS unemployment rates and population weights. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects across economic conditions, we display the p-values obtained from testing the null hypotheses that the coefficients on the interactions with the three unemployment rate categories are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table A.3**
Effect of UI on food consumption — heterogeneity by demographics.

| | 1968–2011 | | | | | | 1968–1997 | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Repl. rate | 0.100 | 0.064 | 0.151 | 0.114 | 0.234* | 0.176 | 0.176 | 0.183 | 0.213 | 0.206 | 0.219 | 0.271* |
| | (0.089) | (0.145) | (0.142) | (0.095) | (0.119) | (0.106) | (0.131) | (0.159) | (0.148) | (0.138) | (0.152) | (0.137) |
| Repl. rate * age 31–45 | | 0.095 | | | | | | 0.043 | | | | |
| | | (0.181) | | | | | | (0.159) | | | | |
| Repl. rate * ages 46–93 | | −0.012 | | | | | | −0.136 | | | | |
| | | (0.175) | | | | | | (0.153) | | | | |
| Repl. rate * married | | | −0.093 | | | | | | −0.067 | | | |
| | | | (0.192) | | | | | | (0.143) | | | |
| Repl. rate * black | | | | −0.089 | | | | | | −0.192 | | |
| | | | | (0.171) | | | | | | (0.203) | | |
| Repl. rate * some college | | | | | −0.326** | | | | | | −0.140 | |
| | | | | | (0.124) | | | | | | (0.167) | |
| Repl. rate * num. kids | | | | | | −0.062 | | | | | | −0.075 |
| | | | | | | (0.050) | | | | | | (0.048) |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.53 | 0.53 | 0.53 | 0.53 | 0.53 | 0.53 | 0.54 | 0.54 | 0.54 | 0.54 | 0.54 | 0.54 |
| Observations | 3383 | 3383 | 3383 | 3383 | 3383 | 3383 | 2462 | 2462 | 2462 | 2462 | 2462 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

**Table A.4**
Effect of UI on food consumption — heterogeneity by safety net spending.

| | 1968–2011 | | | 1968–1997 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Repl. rate | 0.100 | | | 0.176 | | |
| | (0.089) | | | (0.131) | | |
| Repl. rate * low FS | | 0.118 | | | 0.215 | |
| | | (0.102) | | | (0.140) | |
| Repl. rate * high FS | | 0.069 | | | 0.074 | |
| | | (0.096) | | | (0.146) | |
| Repl. rate * low ADFC | | | 0.064 | | | 0.074 |
| | | | (0.103) | | | (0.149) |
| Repl. rate * high ADFC | | | 0.144 | | | 0.263 |
| | | | (0.134) | | | (0.167) |
| p-Value low gen = high gen | | 0.62 | 0.60 | | 0.27 | 0.28 |
| Mean food change | −0.07 | −0.07 | −0.07 | −0.06 | −0.06 | −0.06 |
| Mean repl. rate | 0.51 | 0.51 | 0.51 | 0.52 | 0.52 | 0.52 |
| Observations | 3383 | 3383 | 3383 | 2462 | 2462 | 2462 |

Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. All regressions include controls for state and year fixed effects, state unemployment rates and safety net expenditures, demographics, a linear spline of the lagged actual weekly wage, and state linear time trends. In the specifications that allow for differential effects by safety net generosity, we display the p-values obtained when testing the null hypotheses that the coefficients on the interactions with above and below median generosity are equal to each other. The results are weighted using the PSID provided family weights. Standard errors are clustered by state and shown in parentheses.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

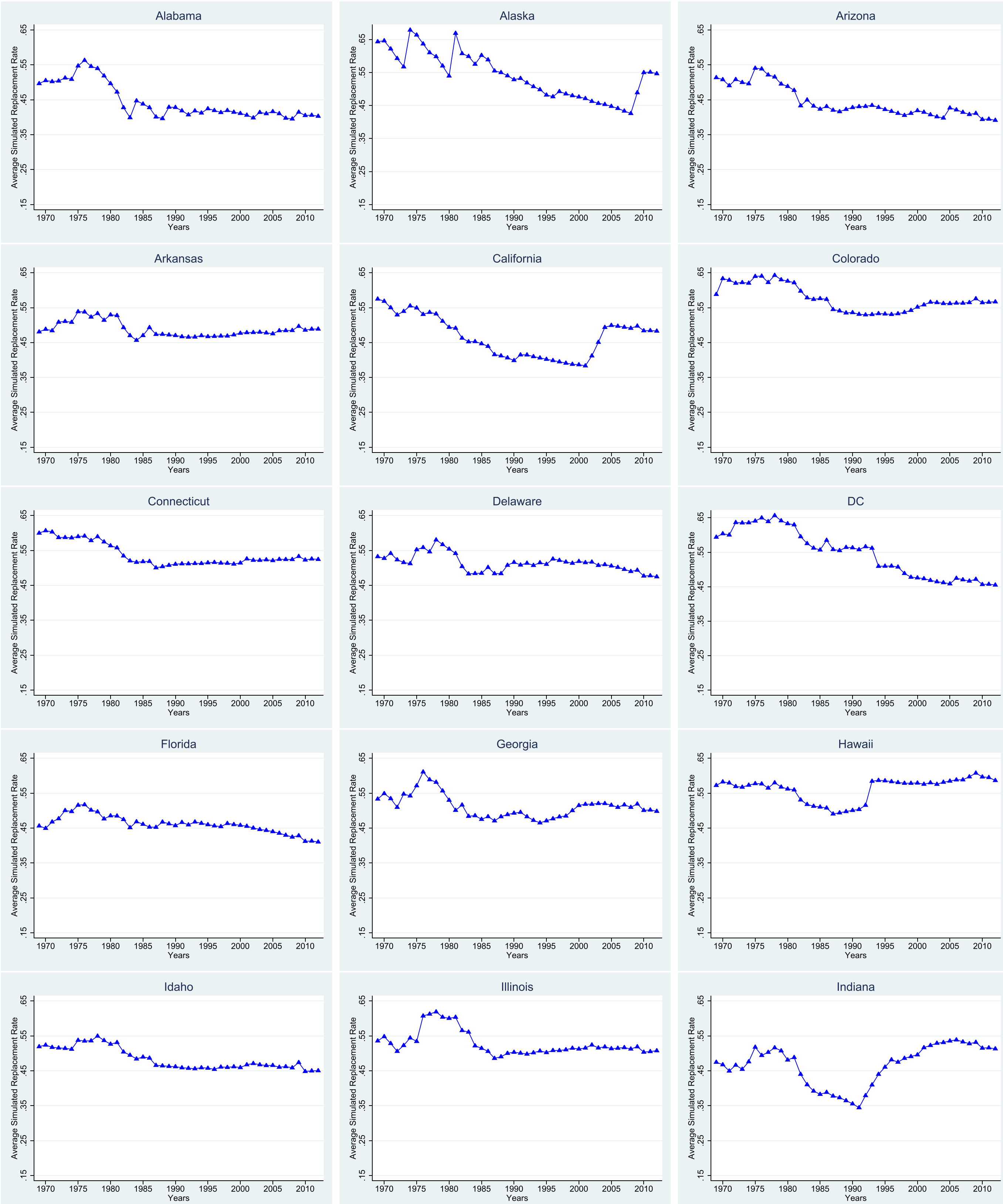


**Fig. A.1.** State average simulated replacement rates.
Notes: Data are from the 1968–2011 Panel Study of Income Dynamics (PSID). The sample includes all heads of household who are employed in one survey year and unemployed in the following one. We exclude individuals with missing demographics, those with changes in food consumption larger than threefold, and individuals with imputed food consumption. To construct these figures, we use this full sample of unemployed individuals, and assign all observations to every state and every year. We then run this simulated sample through our UI benefits calculator, and then collapse the sample to have an average simulated replacement rate for each state and year.

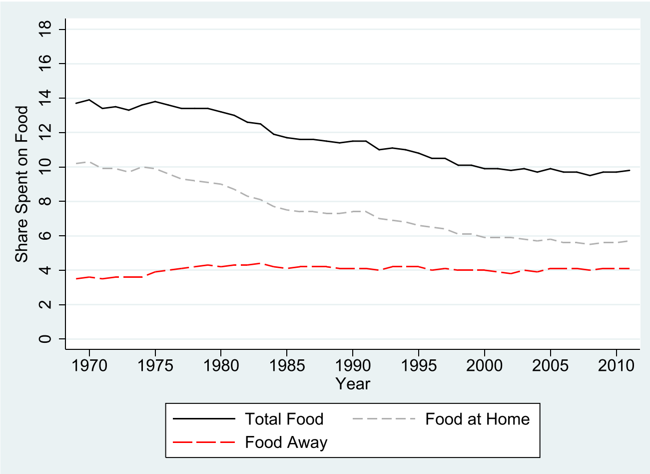


**Fig. A.2.** Food consumption as a share of disposable personal income. Source: USDA ERS Food Expenditure Series (http://www.ers.usda.gov/data-products/food-expenditures.aspx.UuR68WTTksl).

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