The Consumption Smoothing Benefits of Unemployment Insurance

By Jonathan Gruber*

Over the past 25 years, spending on social insurance programs in the United States has grown rapidly. Perhaps as a result, there also has been considerable growth in empirical research on the effects of these programs on the behavior of economic agents. This research has focused primarily on estimating the distorting effects of social insurance programs on individual and firm decision-making. Yet, for government policy makers considering the design of social insurance programs, this type of evidence provides only half the puzzle. The presence of these programs often is justified by the failure of private insurance markets along some dimension. This justification implies that increasing program generosity may lead not only to increased distortions to behavior, but to increased consumption smoothing across states of the world as well. These consumption smoothing gains must be weighed against the carefully documented set of behavioral distortions in assessing optimal program size. To date, however, there has been little empirical work on the benefits from increased social insurance generosity.

This gap is exemplified by the case of unemployment insurance (UI). The cost of this program is a set of well-documented distortions to worker search behavior and firm layoff behavior. A number of empirical studies not only have shown that these distortions exist, but that they are sizeable. The primary benefit of UI is the ability of the government to smooth consumption during unemployment spells. Private unemployment insurance could provide the same function, but, due to problems such as adverse selection, private consumption insurance markets for spells of unemployment may not exist.1 Individuals can save for unemployment, but this is less efficient than pooling unemployment risk through insurance, since those who do not end up losing their jobs are inefficiently reducing today’s consumption. Furthermore, there are potential capital market constraints faced by the worker trying to smooth his consumption across unemployment spells.2 Thus, the provision of public unemployment insurance may raise welfare by filling the missing market for a state-contingent payment. But there is little empirical evidence on either the nature or the magnitude of the benefits from UI.

The goal of this paper is to assess the benefits of UI by measuring the effect of this program on consumption smoothing during periods of joblessness. I do so through direct reduced form estimation of the relationship

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1 Other potential market failures include the incentive problem in implicit contract models with asymmetric information (Murray Brown and Elmar Wolfstetter, 1988) and the problem of moral hazard in search behavior; however, government provision of insurance may only exacerbate the latter distortion.

2 See Stephen P. Zeldes (1989) for evidence of the existence of such borrowing constraints. Such constraints are likely to be important, as the wealth holdings of most workers are sufficiently low that they could not finance an unemployment spell without borrowing. The median 25-64-year-old worker has gross financial assets equivalent to less than three weeks of income, and the average unemployment spell for those becoming unemployed lasts approximately 13.1 weeks (Eric Engen and Gruber, 1995).
between consumption changes upon unemployment and the level of UI generosity. The resulting regression model is identified by using the differences in UI benefits eligibility across individuals which arise from the substantial variation in the generosity of this state-administered program across states and over time within states. This variation has been exploited before to estimate the effect of UI on firm layoff behavior and worker search behavior, but has not been applied to assess the benefits of UI.

To do so, I build a detailed simulation program of the structure of the UI system in each state since 1968. I then apply this program to data from the Panel Study of Income Dynamics (PSID), a longitudinal data set which contains annual observations on food consumption for the period 1968–1987. I consider both the effect of UI on all unemployed and heterogeneity of this effect across different types of unemployed. Finally, I combine my estimates with those of the search distortion from UI to evaluate a simple model of optimal UI benefit levels.

I. Data and Specification

The data for assessing the consumption smoothing effects of UI are from the Panel Study of Income Dynamics. This longitudinal survey has been carried out continuously since 1968, following the same sample of families and their “split offs” over time. The original sample consisted of a nationally representative cross section and a subsample of families in poverty; in the analysis below I use both samples in order to increase the precision of the estimates.

The PSID is the only longitudinal data set available with both information on prime-age workers and a measure of consumption. Each year, individuals are asked how much they “usually” spend on food at home and food away from home, as well as the amount of food paid for by food stamps. The restriction to food consumption has the disadvantage that total consumption may not respond in the same way as food consumption to changes in UI generosity. This disadvantage, however, is outweighed by the uniqueness of having repeated observations on both food consumption and a variety of socioeconomic characteristics. Furthermore, the careful studies of Jerry L. Kingston et al. (1978) and Paul L. Burgess et al. (1981) showed that the response of food consumption and other consumption to the event of unemployment was quite similar. This suggests that food is a reasonable proxy for total consumption for my purposes.

Following Zeldes (1989), I deflate each component of food consumption by the consumer price index for that component in the month of the interview, and then sum the real components (including food stamps). I exclude observations where any element of food consumption is imputed, rather than reported directly by the respondent, and observations with more than a threefold change in total food consumption. Although the frame of reference for the food consumption measures is not entirely clear, Zeldes (1989) convincingly argues that it refers to the point of the interview (rather than the previous year). I therefore use employment status at the time of the interview as my key independent variable in the tests below.

The sample consists of all heads of household who are employed at interview date \( t - 1 \) and unemployed at interview date \( t \). There is one observation for each such person/year from 1968–1987. Individuals are defined as unemployed if they are looking for a new job, but are not on temporary layoff. Temporary layoffs are excluded because the information about probabilities of both layoff and recall to the same job may be quite different for this population; I provide evidence on the consumption response of this group below.

For this sample, I run regressions of the form:

\[
\Delta C_t = \alpha + \beta_1 X_t + \beta_2 UI_t + \epsilon_t.
\]

That is, where \( \log(C_{t-1}/C_t) > 1.1 \) or \( < -1.1 \), following Zeldes. The results are stronger if these outliers are not excluded, but specification checks such as median regression yield results very similar to those reported below. I therefore pursue this more robust approach.

Consumption data were not gathered in 1973, so that the values for consumption change are missing for 1973 and 1974.
$\Delta C$ is the change in (log) consumption when the individual becomes unemployed, $X$ is a vector of individual characteristics which may affect the consumption change, and $UI$ measures the replacement rate (ratio of benefits to wages) for which an individual is eligible. A consumption smoothing effect of $UI$ would be represented by a finding of $\beta_2 > 0$.

The set of exogenous control variables used in the basic specification includes the age, sex, marital status, race, and education of the individual, and the change in the log of the “food needs” of the family, which is a combination of family size and age (calculated by the PSID). In some specifications, I also control for the unemployment rate in both the respondent’s county and state of residence in the interview year; the former is obtained by the PSID from county UI offices, while the latter was collected by Olivier Blanchard and Lawrence Katz (1992). Finally, I include separate dummies for each year to capture time trends in consumption changes.

The key regressor is the UI benefits for which an individual is eligible. To create this variable, I have built a simulation program which models each state’s UI system for the period 1968–1987. The basis for this program is the Employment and Training Administration (various years), which reports semianual information on state benefit schedules; in addition, it was augmented by information from a number of states and from Phillip B. Levine (1990). UI benefits are calculated as a function of average weekly earnings in the year preceding the $t-1$ (employed) observation. UI benefits are then divided by this weekly earnings variable to calculate the replacement rate, where both the benefits and the wage rate are expressed in after-tax terms. After-tax real wages also are included as control variables to capture any spurious correlation between individual characteristics, UI benefit levels, and consumption changes.

The output of the UI benefit simulation model is reported in Gruber (1994). There is a secular decline in benefit generosity, although the decline varies across states: Arkansas saw a decline of 33 percentage points in the average replacement rate from 1969–1987, while the replacement rate actually rose in Minnesota, North Dakota, Oklahoma, and Oregon.

My use of UI benefit eligibility, in place of actual UI benefits received, is dictated by three considerations. First, receipt of unemployment insurance, and the amount of UI received, is endogenous. Rebecca Blank and David Card (1991) document that the take-up of UI is only 67 percent among those eligible for benefits. If the factors determining UI take-up are correlated with consumption changes from job loss, estimates of equation (1) using actual UI benefits received would not be valid for predicting the response to future changes in legislated UI generosity. Second, the data on actual UI receipt are very noisy. This is one reason that most previous studies of the behavioral effects of UI using survey data sets

5 The formulation in terms of changes in log consumption can be derived from an underlying constant relative risk aversion utility function; see Jerry A. Hausman and Lynn Paquette (1987). It also is most appropriate for evaluating the optimal benefits formula described below. Correcting the error term for the fact that there are repeated consumption change observations for some workers, along the lines of Halbert White (1984) and Zeldes (1989), yields almost exactly the same inferences as the results presented below.

6 It is important to account for the tax treatment of UI benefits relative to earnings, which changed substantially over this period; see Gruber (1994) for details of the tax correction used here. In order to qualify for UI, individuals must have a (state-specific) minimum level of earnings. To approximate this eligibility criterion, I drop any observation where weekly wages were below the weekly minimum UI benefit for that state and year. UI benefits are calculated as a function of the highest earnings quarter in the “base period” (generally the first four of the five quarters preceding the unemployment spell). Thus, my use of average earnings over the previous year will lead to some imprecision in the calculation of benefits for those with variable earnings streams. But restricting my sample to those with at least 48 weeks of employment in the previous year does not change the basic result.

7 For example, if there is some stigma associated with receipt of UI, then individuals who face only a short expected layoff, and who may only see a small proportionate change in their consumption, will not take up UI benefits. This would lead to a downward bias to the estimate of $\beta_1$, if UI were measured as actual benefits receipt.
II. Results

A. Basic Results

Table 1 presents the basic regression results. In the first column, I report a model for the change in consumption excluding the UI measure. The drop in consumption is larger for those with higher after-tax real wages. This may reflect the fact that UI is replacing a lower fraction of their income, since UI benefits schedules are progressive. The drop also is larger for older workers, and smaller for female workers and white and black workers (relative to other nonwhites). Consumption rises with the change in food needs.

In the second column, I include the after-tax UI replacement rate. It enters positively and highly significantly, indicating that a 10-percentage-point rise in the replacement rate reduces the fall in consumption upon unemployment by 2.65 percent. Furthermore, I find that the fall in consumption for those becoming unemployed, at a replacement rate of zero, is 22.2 percent. That is, in the absence of UI, becoming unemployed would be associated with a fall in consumption over three times as large as the 6.8-percent average drop noted above. But, at a replacement rate above 84 percent, UI fully smooths consumption across the unemployment spell.10

The results, therefore, decisively reject the notion that there are complete private consumption insurance markets for unemployment spells, since variation in publicly provided UI generosity is a significant

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9 For example, in the PSID data used here, approximately one-quarter of the observations with reported unemployment insurance income have no reported weeks of unemployment.

9 As a result, the estimated coefficient in the reduced form results below will underestimate the effect on those actually receiving UI. In my sample, a marginal one-dollar increase in UI benefits eligibility leads to a 48-cent increase in UI benefits receipt. This implies that the effect on those actually receiving UI is approximately twice as large as the effect for the full sample of unemployed. This is an upper bound, however, since the provision of UI benefits may have an insurance aspect for those who do not receive benefits as well. If there is stigma associated with the receipt of benefits, then those who expect short spells may not initially take up benefits, but their consumption may fall less because they know that benefits are available should their spell turn out to be more severe than expected. See Julie B. Cullen and Gruber (1996) for a more detailed argument along these lines.

10 These predictions are not grossly out of sample; 90 percent of the sample has replacement rates within the range of 20 percent to 80 percent. In terms of dollars, my findings imply that, given the average level of UI, food consumption falls by $243 (in 1984 dollars), or only $4.67 per week. In the absence of UI, this fall would be $792, or $15.23 per week. Food consumption is approximately 18 percent of the total consumption bundle, according to tabulations from the 1980–1990 Consumer Expenditure Surveys. If total consumption responds to unemployment, and UI, proportionally to food consumption (as the results of Kingston et al. [1978] and Burgess et al. [1981] suggest), then the fall in total consumption is $1,350 given average UI benefit levels, but would be $4,400, or $84.62 per week, in the absence of UI.
Table I—Basic Regression Results

<table>
<thead>
<tr>
<th>Dependent variable is change in log real consumption</th>
<th>(1) OLS</th>
<th>(2) OLS</th>
<th>(3) IV</th>
<th>(4) OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>After-tax replacement rate</td>
<td>0.265</td>
<td>0.325</td>
<td>0.280</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.091)</td>
<td>(0.180)</td>
<td>(0.105)</td>
<td></td>
</tr>
<tr>
<td>Implied consumption fall at 0 replacement rate</td>
<td>−0.222</td>
<td>−0.257</td>
<td>−0.231</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.249)</td>
<td>(0.337)</td>
<td>(0.385)</td>
<td></td>
</tr>
<tr>
<td>After-tax real wage (×10¹)</td>
<td>−0.798</td>
<td>−0.136</td>
<td>0.015</td>
<td>−0.177</td>
</tr>
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<td></td>
<td>(0.249)</td>
<td>(0.337)</td>
<td>(0.516)</td>
<td>(0.385)</td>
</tr>
<tr>
<td>Age</td>
<td>−0.002</td>
<td>−0.002</td>
<td>−0.001</td>
<td>−0.002</td>
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<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Female</td>
<td>0.066</td>
<td>0.069</td>
<td>0.070</td>
<td>0.067</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.034)</td>
<td>(0.034)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>Married</td>
<td>0.024</td>
<td>0.024</td>
<td>0.024</td>
<td>0.024</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.031)</td>
<td>(0.031)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>White</td>
<td>0.110</td>
<td>0.107</td>
<td>0.106</td>
<td>0.114</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.051)</td>
<td>(0.051)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Black</td>
<td>0.078</td>
<td>0.077</td>
<td>0.077</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.052)</td>
<td>(0.052)</td>
<td>(0.055)</td>
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<tr>
<td>Years of education</td>
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<td>0.005</td>
<td>0.006</td>
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<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
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<tr>
<td>Change in log food needs</td>
<td>0.216</td>
<td>0.215</td>
<td>0.214</td>
<td>0.200</td>
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<tr>
<td></td>
<td>(0.073)</td>
<td>(0.073)</td>
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<tr>
<td>Number of children</td>
<td>0.016</td>
<td>0.016</td>
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<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
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<tr>
<td>County unemployment rate</td>
<td></td>
<td></td>
<td></td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.005)</td>
</tr>
<tr>
<td>State unemployment rate</td>
<td>−0.888</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.985)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State effects</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>1,604</td>
<td>1,604</td>
<td>1,604</td>
<td>1,604</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Sample is those becoming unemployed from period \(t - 1\) to period \(t\). All regressions include 16-year dummies. In column (3), replacement rate is instrumented by simulated replacement rate.

determinant of individual consumption behavior. The wage coefficient is now insignificant, supporting the interpretation offered above.

B. Specification Checks

In this section, I run the reduced form estimates from equation (1) through a series of specification checks designed to address three potential problems with my empirical approach. The first, discussed in Bruce D. Meyer (1989), is that the UI replacement rate for a given individual is a function not only of the legislative environment in the individual’s state/year, but also of individual characteristics, such as wages. While wages are included linearly in the regression, they determine UI benefits in a very nonlinear way. If consump-
tion changes depend on these variables in a corresponding nonlinear fashion, then there could be a spurious correlation between UI benefits and consumption changes.

In order to separate the component of UI variation which is a function of the legislative environment only, I instrument UI benefit eligibility with "simulated" UI eligibility. I take the full sample of individuals who become unemployed in each year, and assign that same sample to every state in that year. I then run this simulated sample through my benefit imputation program, and take the average of the resulting after-tax replacement rates estimated for each state and year. In this way, I have created a state/year replacement rate which is independent of the characteristics of the actual individuals in that state/year; rather, it is only a function of the legislated benefits in that state/year, applied to a uniform set of characteristics nationwide. I then instrument UI with this simulated replacement ratio within the regression framework (1).11

The results of this exercise are presented in column (3) of Table 1. Both the estimated fall in consumption from unemployment, and the estimated consumption smoothing effect of UI, rise slightly. The UI effect is significant at the 7-percent level, and indicates that a 10-percentage-point increase in the replacement rate is associated with a 3.25-percent rise in consumption. The coefficients are not very different from those in column (2), however, indicating little bias from the fact that benefits are a function of individual characteristics.

The second potential problem is that I may not be controlling for the omitted characteristics of the states and years in which individuals live. For example, rich states may feature high unemployment benefits and small drops in consumption among the unemployed. Alternatively, a "legislative endogeneity" explanation for my findings might suggest that I am just capturing the fact that, when times are good, states may increase UI benefits, and at these same times a worker who loses his job may see consumption fall less (i.e., because his spouse is able to find a job).

I deal with this potential problem in two ways. First, in the final column of Table 1, I include both fixed state effects, and the county and state unemployment rates. In this way, I control for both time-invariant state omitted variables, and the most likely form of legislative endogeneity, which is that benefits setting at the state level (or "administrative hassle" at the county level) is related to the level of unemployment.12 Including these controls has no effect on the coefficient of interest. Neither of the unemployment rate measures are significant, nor are the state effects (jointly) significant.

Second, I control for more general state/year-specific omitted variables by making use of a within-state control group: those who remain employed from t − 1 to t. There will be some "true" effect of UI on those who were unemployed between interview date t − 1 and t (but are employed again at t), as well as a correlation through the effect of UI on savings behavior and, thus, on consumption growth rates (Engen and Gruber, 1995). But these effects should be small relative to the consumption smoothing effects on the unemployed. Thus, if the estimated effect truly reflects exogenous consumption smoothing benefits of UI, then the coefficient on the after-tax replacement rate for this control group should be close to zero. If, instead, I am simply measuring a spurious correlation between within-state changes in consumption opportunities and changes in UI benefits setting, then it should be reflected in a large coefficient on the replacement rate for the employed.

The consumption change regression for those remaining employed is presented in column (1) of Table 2; I present the coefficient of

11 See Janet Currie and Gruber (1996) for an application of this type of approach. This is similar to the procedure used in Levine (1993), instrumenting replacement rates with the maximum benefit level; however, my methodology exploits the structure of the entire benefits schedule, rather than just the level of the maximum.

12 There is one clear dimension of endogeneity which this approach will capture: extended unemployment benefits (for 13 weeks beyond the 26-week basic benefit duration) in a state are "triggered" by a sufficiently high state unemployment rate.
interest from a regression such as those in Table 1. The coefficient on the after-tax replacement rate is significant at the 15-percent level, but it is quite small. Thus, this check supports the contention that the effect of UI is causal.13

Finally, I consider the potential criticism of sample selection bias that arises from my focus on individuals who become unemployed only. Feldstein (1978) and Topel (1983) find that the probability that an individual is laid off is a function of the replacement rate, although Patricia M. Anderson and Meyer (1994) find an inconsistent relationship between layoffs and benefits. And a number of studies, most notably Meyer (1990), have found that the duration of unemployment spells is a function of benefits generosity. This is potentially problematic for my approach, if the marginal individuals who are laid off, or who lengthen their spells, when replacement rates rise are systematically different from the average layoff.14 This sample selection problem should be less severe in my data, since I exclude individuals on temporary layoff; this is the margin of response analyzed by both Feldstein and Topel.

Nevertheless, I can assess the potential for such bias in my sample of nontemporary layoffs by directly measuring the correlation between losing a job and UI generosity. In column (2) of Table 2, I present a probit model of the likelihood of becoming unemployed as a function of the level of UI generosity. There is a small and totally insignificant effect of benefits on unemployment; a 10-percentage-point increase in the replacement rate raises the likelihood of becoming unemployed by only 0.02 percent (compared to a sample mean of 3.3 percent). This suggests that there is little problem of selection bias arising from benefit generosity causing layoff increases within my sample.15

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13 In Gruber (1994), I consider two other control groups that are not eligible for UI: those who quit and those who retire. For the former group, there is a large UI effect (although it is not significant). This may reflect poor measurement of the reason for separation, however, as the quit rate in my sample is much higher than that from the Current Population Survey (CPS) reported in Topel (1983), and the UI receipt rate for quitters (which should be zero) is one-half that of job losers. The retirement rate in my sample matches that from the CPS reported in Gruber and Brigitte Madrian (1995), however, and there is no UI effect on this more precisely measured control group.

14 For example, if rising UI benefits lead to more short-term layoffs, and such layoffs feature small consumption drops, then there will be a spurious positive correlation between benefit generosity and consumption changes.

15 An alternative means of controlling for the potential problem of “length based sampling” which arises from

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<table>
<thead>
<tr>
<th>Table 2—Specification Checks and Extension</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Employed</td>
</tr>
<tr>
<td>(2) Become unemployed? (Probit)</td>
</tr>
<tr>
<td>(3) Temporary layoff or previous unemployment</td>
</tr>
<tr>
<td>After-tax replacement rate</td>
</tr>
<tr>
<td>Implied consumption fall at 0 replacement rate</td>
</tr>
<tr>
<td>Effect of 10-percentage-point replacement rate increase</td>
</tr>
<tr>
<td>Number of observations</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. All regressions include the full sample of regressors from Table 2, including 16-year dummies. Sample is those remaining employed from period $t - 1$ to period $t$ in column (1), all workers who are employed at $t - 1$ in column (2), and those who are on temporary layoff or who have experienced at least one week of unemployment in each of the previous two years in column (3). Regression in column (2) is a probit.
C. Effects of UI After the Unemployment Spell

The regressions thus far have considered only the change in consumption when workers move from employment in period $t - 1$ to unemployment in $t$. In Table 3, I extend the analysis to examine the effect of UI on changes in consumption from period $t$ to period $t + 1$. In the top panel, I examine the response of consumption for the full sample of individuals becoming unemployed between $t - 1$ and $t$ who have data on consumption for $t + 1$. The first column replicates the previous regression from Table 1, examining the effects of UI on consumption changes from $t - 1$ to $t$ (employed) to $t$ (unemployed). The second column shows the effect of UI on the consumption change from period $t$ to period $t + 1$. The third column shows the two-period consumption smoothing effect of UI. All regressions include the set of control variables from Table 1, but only the coefficients of interest are reported.

The results illustrate the explicit short-run consumption smoothing effects of UI. The effect of UI from period $t - 1$ to period $t$ is similar for this subsample to the full sample results reported in Table 1—a consumption fall with unemployment that is mitigated by increased UI generosity. In the next period, the results are reversed. At a replacement rate of zero, there is a moderate consumption rise in the period after unemployment, although it is smaller than the consumption fall upon entering unemployment. However, this consumption rise is tempered by reduced consumption growth for higher replacement rates. That is, for those who receive no UI, consumption falls substantially upon unemployment, then increases again the next period. But for those receiving generous UI, there is little change in consumption over the two-year period; UI serves to smooth consumption during their jobless spell, but has no permanent effect on their consumption prospects. This is illustrated in the third column, which examines the two-period consumption change from $t - 1$ to $t + 1$: there is an overall negative effect of becoming unemployed in period $t$ of 9.1 percent, but there is only an insignificant consumption smoothing effect of 5.7 percent from having more generous UI.

In the bottom panel of the table, I split the sample into those who become reemployed at $t + 1$, and those who remain nonemployed (including both those who are unemployed and those who leave the labor force). For those becoming reemployed, there is a smaller consumption smoothing effect of UI from period $t - 1$ to period $t$. But there is the same pattern of reversal in the next period, and the two-period effect of UI is negligible. For those who will remain unemployed, there is a much larger consumption smoothing effect of UI initially, and this is counteracted by a large negative effect from $t$ to $t + 1$ as their UI benefits run out. This group sees a larger negative fall over two periods, but once again there is little benefit beyond one period from increased UI generosity. Thus, UI is serving as a short-run consumption smoothing device, reducing consumption falls during initial unemployment, but having little effect beyond one year.

The much larger response for the group which remains nonemployed at period $t + 1$ most likely reflects the fact that those workers are in the midst of more serious unemployment spells at period $t$. In fact, for individuals who remain nonemployed at interview $t + 1$, the duration of their ongoing spells at period $t$ is 28 percent longer than for those who will find a job by period $t + 1$, and they spend over twice as many months unemployed from period $t$ to $t + 1$. A more serious unemployment shock will raise the consumption smoothing effects of UI if individuals have other sources of consumption smoothing, such as own savings, but these other sources are somewhat limited. In that case, for short spells, individuals will be able to run down their savings to finance consumption, so that increases in UI simply will crowd

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endogenous unemployment durations is to include in my model the duration of the unemployment spell. This is difficult to do with precision, since individuals are asked only about their weeks of unemployment over the previous year. But monthly unemployment timing is available for the 1984–1987 interviews. Including either weeks of unemployment over the previous calendar year for my full sample, or the duration of the current unemployment spell for the 1984–1987 sample, has no effect on the basic result, which is consistent with my contention of no sample selection bias.
out own savings as a consumption smoothing mechanism. But, for longer spells, savings will have run out and UI will provide the only source of consumption smoothing. Indeed, PSID data show that while roughly one-half of individuals who lose their jobs have savings before job loss, only 18.6 percent have savings of more than two months of income. Thus, spells that last more than a few months cannot be financed by own savings, inducing a larger measured consumption smoothing effect for UI.

D. Layoff Anticipation and Consumption Response

As demonstrated theoretically in Gruber (1994), the extent to which consumption responds to unemployment and to UI generosity will be a function of the likelihood of layoff. With perfectly certain layoff (and nonmyopic workers), consumption will not fall upon unemployment and UI will have no effect. However, as the probability of layoff is smaller, then the event of layoff will lead to a larger consumption fall, and UI will play a larger role, since own savings provides a less-perfect consumption smoothing device. While data on unemployment expectations is not available in the PSID, I can exploit information on the nature of the current unemployment spell, as well as on past unemployment histories, to assess the extent to which workers may have anticipated their current spell. I create a measure of "anticipation" by combining two groups of unemployed. The first group represents those on temporary layoffs, who are much more likely to be recalled to their former employers, indicating that their unemployment spell may be a (regular) seasonal one, or part of some other ex ante layoff arrangement with employers. The second group represents those who

\^\text{16} Of individuals who are temporarily laid off and then reemployed, over 50 percent return to their former employer. On the other hand, of individuals who become otherwise unemployed and then reemployed, less than 20 percent return to their former employer (author’s calculations using PSID data for 1981–1983). Of this anticipation sample, 69 percent are those on temporary layoff, and the remaining 31 percent are those with previous unemployment spells; the results are similar if estimated separately for these two groups. As noted earlier, there is a potential sample selection problem, since temporary layoffs are a function of the level of UI generosity. Thus, these results should be viewed with some caution.
have experienced at least one week of unemployment in each of the two years preceding interview date \( t - 1 \) (as well as being unemployed at interview \( t \)); these individuals presumably have more information about their unemployment prospects in the year of observation than the average laid-off worker.

As the final column of Table 2 illustrates, for this group of “anticipators,” there is in fact no consumption smoothing benefit of UI; the coefficient on the replacement rate is wrong-signed and completely insignificant. While these results are only illustrative, they suggest that the benefits of UI may be much more limited for those who anticipate their layoff spells.

### III. Application: Optimal UI Benefits

My finding that the fall in consumption from unemployment is less than one-third as large as it would have been in the absence of public UI implies that there are potentially large welfare gains from the existence of this program. In order to interpret the magnitude of these gains, however, it is necessary to contrast them to the distortions that UI causes to individual behavior. I do so in this section, using the optimal benefits model of Martin Neil Baily (1978). Baily’s two-period model considers individuals who make consumption and savings decisions in the first period, subject to a level of UI taxes and an exogenous probability of layoff, and make consumption and duration of unemployment decisions in the second period, subject to a level of UI benefits. The individual maximizes:

\[
V = U[y(1 - t) - s] \\
+ \alpha U[y(1 - t) + s] \\
+ (1 - \alpha) U[y_t + s],
\]

where \( \alpha \) is the probability of remaining employed, so that \((1 - \alpha)\) is the probability of losing the original job. The net amount of savings is \( s \). Earnings on the original job are \( y \), and earnings if the individual becomes unemployed for some fraction of the second year are:

\[
y_t = (1 - \beta)(b - c) + \beta y_n(1 - t),
\]

where \((1 - \beta)\) is the fraction of the year unemployed, \( b \) is the unemployment insurance benefit, \( c \) is search costs, and \( y_n \) is the earnings on the new job. Unemployment insurance is financed by a tax \( t \). The model is closed by a UI budget constraint which sets the aggregate level of benefits and taxes equal:

\[
y_t + \alpha y + (1 - \alpha) \beta y_n t = (1 - \alpha)(1 - \beta)b.
\]

Gruber (1994) demonstrates that this model predicts the type of response to unemployment insurance generosity increases found in the empirical work; it also predicts that this response will be greatest among those for whom \((1 - \alpha)\) is the smallest, as demonstrated in the final column of Table 2.

Baily uses his framework, with an endogenous unemployment duration that responds to UI generosity, to derive an expression for the optimal level of UI benefits:

\[
\Delta C / C_r [R(C_\theta)] = E^*_\theta,
\]

where \( C_r \) and \( C_\theta \) are consumption when employed and unemployed, respectively, \( \Delta C = C_r - C_\theta, R(C_\theta) \) is the coefficient of relative risk aversion (evaluated at the level of consumption when unemployed), and \( E^*_\theta \) is the elasticity of the duration of unemployment with respect to balanced-budget increases in UI benefits and taxes. Thus, the optimal level of benefits trades off the gains from consumption smoothing against the costs of search distortion.

The parameter estimates needed to evaluate this expression are the elasticity of unemployment duration with respect to UI benefits, and the percentage change in consumption from unemployment, as a function of the level of UI benefits. For the former, I use Meyer’s (1990) estimate of an elasticity of 0.9. For the latter, I initially use my estimates for the effect of the replacement rate on consumption smoothing
The problem with this approach is that Meyer’s elasticity is estimated from a sample that included temporary layoffs, while my consumption smoothing effect is smaller if temporary layoffs are included. Thus, I also use estimates from a model which includes both permanent and temporary layoffs to compute optimal benefits.17

The results for the level of optimal UI benefits are reported in Table 4, for the range of values of the degree of relative risk aversion estimated in the previous literature (Zeldes, 1989). I find that optimal benefits are zero for a risk aversion coefficient below 2, and even at very high degrees of risk aversion, the replacement rate is below 0.5. In 1987, the average replacement rate for individuals who became unemployed in my data was 42.6 percent. These findings suggest that such a replacement rate is optimal only at very high levels of risk aversion. Thus, despite large consumption smoothing effects, the distortions of UI to search behavior are so large that the optimal benefit level is fairly low.

In the next column, I include temporary layoffs in the consumption smoothing calculation. This substantially reduces the optimal benefit level at all degrees of risk aversion, since UI offers no benefits for those who anticipate their unemployment spells. This finding implies that program efficiency potentially could be improved by having separate benefits schedules for temporary and nonpermanent layoffs. In the third column, I reduce the duration elasticity from 0.9 to 0.6. This substantially raises the estimated optimal replacement rate, so that at a level of risk aversion between 2 and 2.5 the optimal rate is equal to current levels. The sensitivity of these results highlights the value of confirming the precise magnitudes of these key parameters. Nevertheless, in almost no case is the optimal replacement rate much higher than current levels, and it is generally lower.

While the Baily model provides a tractable and intuitive formulation for computing the optimal benefit level, there are at least five reasons why the nature of this formulation understates the net benefits of increasing the UI replacement rate. First, he assumes that unemployment durations are endogenous, but certain; when he introduces duration uncertainty into the model, the optimal replacement rate is found to rise. Second, in a nonfull employment economy, the costs of UI in terms of increased unemployment duration may be mitigated by spillover effects onto those unemployed not receiving UI (Levine, 1993). Third, Baily does not consider the effects of a heterogeneous workforce with differential responsiveness to UI generosity. If the insurance gains accrue largely to low-income workers while the efficiency cost is economy-wide, and the social welfare function is redistributive, then the optimal level of benefits will rise. Fourth, the model ignores the benefits from subsidizing search to both the worker and to others (through potential search externalities).18 Finally, Baily does not model the benefits of UI in terms of increasing the accumulation of human capital by workers facing uncertain demand for specialized labor services, as in Eleanor Brown and Howard Kaufold (1988).

On the other hand, there are at least two important reasons why Baily’s formulation overstates the net benefits of more generous UI. First, he does not incorporate the valuation of leisure; implicitly, he assumes that the disutility of work and job search are equal. Optimal benefits would be lower if there were a net increment to leisure while unemployed (and leisure is not very complementary with consumption). Second, he assumes that the probability of layoff, \( \alpha \), is exogenous. As was discussed above, a number of studies have found that, given the imperfect experience rat-

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17 The coefficient from this model, which is estimated on the 2,273 observations which are on both permanent and temporary layoff, is 0.175 (0.074). Note that Meyer’s estimate applies to a sample of UI recipients only; for comparison to the consumption smoothing findings, I multiply his elasticity by the take-up elasticity of UI benefits in my sample, 0.48.

18 Evidence that there are benefits from subsidizing search is mixed; see Ronald G. Ehrenberg and Ronald L. Oaxaca (1976) and Meyer (1989) for examples.
riage of the UI system, more generous benefits lead to a higher level of temporary layoff unemployment.

IV. Conclusions

As a large and growing share of government budgets worldwide, social insurance programs have increasingly attracted the attention of empirical economists. However, this attention has been directed primarily towards estimating the disincentive effects of these programs. The purpose of this paper was to provide, within the context of a specific social insurance program, estimates of the benefits of government intervention in insurance markets. I find strong evidence that unemployment insurance does smooth individual consumption, and that the magnitude of such consumption smoothing is nontrivial. In the absence of UI, I estimate that the consumption of the unemployed would fall by 22 percent—over three times the average fall in the presence of this public program.

Given the very large estimated distortions from this program, however, the results suggest that current levels of UI replacement rates are optimal only at fairly high coefficients of relative risk aversion. Moreover, while this consumption smoothing effect is large, I find that each dollar of UI is translated to much less than one dollar of increased consumption, suggesting that UI is to some extent crowding out other forms of insurance against unemployment spells. Engen and Gruber (1995) and Cullen and Gruber (1996) investigate two of these other forms of insurance—own savings and spousal labor supply—and find significant crowd out in each case.

I have argued that the reduced form strategy pursued in this paper was the most efficient way to derive the parameters of interest for measuring the optimal UI replacement rate. This strategy also could be potentially useful for addressing other design questions within the UI program. For example, information on the effects of benefits levels and duration on the time pattern of consumption during unemployment could be used to help infer the optimal time structure of benefits. This approach clearly can be extended to other social insurance programs as well. The most fruitful applications will be in cases, such as workers’ compensation insurance or cash welfare under Aid to Families with Dependent Children, where there is cross-state variation in benefit generosity.

However, this reduced form strategy runs up against two fundamental limitations. First, I am unable to model the behavior that underlies my findings: does this consumption smoothing effect derive primarily from myopic behavior, the presence of liquidity constraints, or failures of private unemployment insurance markets? Second, this strategy is useless for evaluating alternatives which deviate substantially from past policies. There are a number of interesting alternatives for UI reform which have not been adopted in a
widespread fashion, such as paying job-findings bonuses to workers or firms. Stephen Woodbury and Robert Speigelman (1987) used evidence from an experimental trial to estimate that reemployment bonuses to workers substantially decreased their search time, but the effects of such a policy on consumption smoothing are not obvious. It would be useful if, when future experiments of the type evaluated by Woodbury and Speigelman are run, information were collected not just on the effect of the program on search behavior, but on consumption behavior as well.

Finally, an obvious shortcoming of this exercise arises from the limited nature of the optimal benefits formula employed. It would be quite useful to develop the theory of optimal benefit determination further, accounting for worker heterogeneity, the valuation of leisure, and an explicit modelling of firm decision-making.

REFERENCES


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