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THE CONSUMPTION SMOOTHING BENEFITS OF UNEMPLOYMENT INSURANCE

Jonathan Gruber

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1050 Massachusetts Avenue
Cambridge, MA 02138
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ABSTRACT

Previous research on unemployment insurance (UI) has focused on the *costs* of the program, in terms of the distorting effects of generous UI benefits on worker and firm behavior. For assessing the optimal size of an unemployment insurance program, however, it is also important to gauge the *benefits* of increased UI generosity, in terms of smoothing consumption across periods of joblessness. I do so through a reduced form approach which directly measures the effect of legislated variations in UI benefits on consumption changes among individuals becoming unemployed. I use annual observations on food consumption expenditures for 1968-1987 from the Panel Study of Income Dynamics, matched to information on the UI benefits for which unemployed persons were eligible in each state and year. I estimate that a 10 percentage point increase in the UI replacement rate leads to a consumption fall upon unemployment which is 2.7% smaller. Over this period, the average fall in consumption for the unemployed was 7%; my results imply that, in the absence of unemployment insurance, this fall would have been over three times as large. I also find that the positive effect of UI only extends for one period, smoothing consumption during initial job loss but having no permanent effect on consumption levels; that individuals who anticipate layoff see a smaller consumption smoothing effect; and that UI appears to somewhat crowd out other forms of public consumption insurance. Despite the substantial estimated consumption smoothing effect, however, my results imply that the optimal UI benefit level is within the range of current replacement rates only at fairly high levels of risk aversion.

Jonathan Gruber
Department of Economics
M.I.T.
50 Memorial Drive
Cambridge, MA 02139
and NBER

Over the past twenty-five years, spending on social insurance programs in the U.S. has grown rapidly.[1] Perhaps as a result, there has also 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.[2] 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 is often 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 have not only 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 by completing missing private consumption insurance markets. But there is

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[1]Federal spending on Medicare, Social Security, and Other Health (primarily Medicaid) has grown from 13.5% of the budget in 1965 to 36.8% today; data from *Economic Report of the President*.

[2]Classic examples include studies of the effect of unemployment insurance on firm and worker layoff and search behavior by Feldstein (1978), Topel (1983), and Meyer (1989, 1990); studies of the distortion to savings and retirement behavior under social security by Feldstein (1974), Burtless (1978), and Diamond and Hausman (1984); studies of the effect of workers compensation benefits on injury frequency and duration by Meyer et al. (1990) and Krueger (1990a,b); and studies of the effect of disability insurance on labor force withdrawal by Parsons (1980) and Bound (1989).

little empirical evidence on either the nature or the magnitude of these benefits. As Baily (1978) states in his theoretical study of optimal UI benefit levels:

> The empirical studies [of the labor market distortions from UI] have certainly been useful . . but they do not go directly to the policy issue. They do not tell us whether or not the value of the existing UI program outweighs its costs. Nor do they give direct guidance as to the optimal benefit level and means of payment, if it turns out to be desirable to have a program (p. 380).

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. One approach to doing so would be to specify a structural model which incorporated features such as the consumption behavior of the unemployed, the process by which they accumulate information on their future job prospects, and the extent to which they face capital market constraints in carrying out their planned consumption path. The comparative statics of this model would yield the effect of changes in UI generosity on the consumption of the unemployed. Such a model, however, would not only require a number of assumptions to be tractable, but its calibration would involve a variety of parameters which have not been empirically estimated to date.

I propose an alternative reduced form strategy, which is to directly estimate the relationship between consumption changes upon unemployment and the level of UI generosity. The resulting regression model is identified by using differences in UI benefits *eligibility* across individuals. Differences in the benefits available to the unemployed 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, a longitudinal data set which contains

annual observations on consumption for the period 1968-1987.

To summarize, I find a sizeable and significant role for UI in smoothing consumption. I estimate that a 10 percentage point increase in the replacement rate leads to a consumption fall upon unemployment which is 2.7% smaller. Over this period, the average fall in consumption for the unemployed was 7%; my results imply that, in the absence of unemployment insurance, this fall would have been over three times as large. This basic finding is robust to a variety of specification checks. I also find that the positive effect of UI only extends for one period, smoothing consumption during initial job loss but having no permanent effect on consumption levels; that individuals who anticipate layoff see a smaller consumption smoothing effect; and that UI appears to somewhat crowd out other forms of public consumption insurance. In the final section of the paper, I apply a simple model of optimal UI benefit determination to assess what my results imply for program design. Despite my large estimated consumption smoothing effects, the results suggest a fairly low optimal benefit level; the optimal UI benefit is within the range of current replacement rates only at fairly high levels of risk aversion.

I begin, in Part I, by modelling the consumption smoothing properties of unemployment insurance benefits; in this context, I discuss previous research on the consumption response to unemployment. In Part II I describe the data and estimation strategy. In Part III I estimate the consumption smoothing effects of UI in the PSID, and consider a number of extensions of the basic results. In Part IV, I apply this result to estimate the optimal level of UI benefits. Part V concludes with a discussion of directions for future research.

I. The Consumption Smoothing Effects of UI

Why should we provide public unemployment insurance? A number of justifications have been offered, such as the benefits from allowing workers who are constrained by capital market imperfections to increase their search duration. Perhaps the most traditional justification, however, is the consumption insurance aspect of UI (Baily, 1977, 1978; Stafford, 1977). Public unemployment insurance provides a means of smoothing individual consumption across the risky event of unemployment. 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.[3] Individuals can save for unemployment, but this is less efficient than insuring against such a risky event, since individuals who do not end up losing their job are inefficiently reducing today's consumption. Insurance markets overcome this inefficiency by pooling the risk across individuals who do and do not lose their jobs. Thus, the provision of public unemployment insurance may raise welfare by filling the missing market for a state-contingent payment. The optimal size of the program will trade off these consumption smoothing gains against the induced distortions to individual and firm behavior.

This argument is readily illustrated by an application of the optimal benefits model of 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:

$$(1) \qquad V = U[y(1-t) - s] + \alpha^{*}U[y(1-t) + s] + (1-\alpha)^{*}U[y_1 + s]$$

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

where α 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:

(2) $\quad y_1 = (1-\beta)(b-c) + \beta y_a(1-t)$

where $(1-\beta)$ is the fraction of the year unemployed, b is the unemployment insurance benefit, c is search costs, and y_a 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:

(3) $\quad yt + (1-\alpha)yt + \alpha\beta y_a t = \alpha(1-\beta)b$

This model can readily be used to illustrate the consumption smoothing gains from introducing a public UI system in the absence of private unemployment insurance. To do so, I consider the simplified case of Cobb-Douglas utility, exogenous search behavior with no search costs, and a post-unemployment wage equal to the pre-unemployment wage ($y_a=y$).[4] I first solve the model for the optimal level of savings s^*, given the UI replacement rate. I then use s^* to calculate the effect of increasing UI generosity on consumption smoothing, defined as the percentage change in consumption from period 1 to period 2 for an individual who becomes unemployed for β weeks in period 2.

The results of this calculation are expressed graphically in Figure 1, for probabilities of layoff of 1%, 25%, and 50%. Each point in Figure 1 represents the percentage fall in consumption for the individual becoming unemployed. The generosity of the UI system is measured on the X-axis

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[4]The fixed unemployment duration (β) is assumed to be 14.4 weeks, the average in my PSID sample for those who spend at least one week unemployed. Allowing duration to be endogenous to the level of benefit mitigates the consumption smoothing result presented below, since longer duration leads to a lower total income in the second period, reducing the extent of consumption smoothing.

by the replacement rate, the extent to which UI replaces pre-unemployment wages. For each layoff probability, there is a sizeable drop in consumption with no unemployment insurance; for a probability of 1%, the drop is approximately 20%. However, introducing public unemployment insurance mitigates this fall. This consumption smoothing effect increases with the generosity of UI, until consumption is unchanged at a 100% replacement rate. This figure yields the key prediction that will be tested below: *consumption will fall with unemployment in the absence of UI, but this fall will be mitigated as UI generosity increases.*

There is another interesting features of Figure 1: as the probability of layoff rises, the initial consumption fall is reduced, and UI has a smaller consumption smoothing effect. This is because the efficiency of the savings mechanism increases as unemployment becomes a more likely event. With perfect certainty, savings is fully efficient and UI has no effect; more generous UI simply crowds out savings for the unemployment spell. I return to this point in the extensions section below.

The magnitude of the consumption smoothing gains from UI will be a function of a number of factors beyond those considered in this simple model. One such factor is the extent to which private unemployment insurance arrangements exist, through employers, other social insurance programs, or the labor supply of family members.[5] Another is the nature of the preferences of unemployed workers. For example, if such workers are myopic, the consumption smoothing effects of UI will be larger than those depicted in a model with savings for unemployment spells. On the other hand, with forward looking workers as the number of periods of employment rises relative to

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[5]Only 5% of workers worked for employers which offered supplemental unemployment benefits in 1989 (Employee Benefits Research Institute, 1992). Lundberg (1985) finds only a very small "added worker effect" when the primary earner becomes unemployed; furthermore, pooling labor supply among a small set of family members is once again inefficient relative to pooling the risk of unemployment across a large number of workers.

the number of periods unemployed, savings becomes a more efficient smoothing mechanism, and UI will have a smaller effect. Even in the limit, however, smoothing across the (relatively) small number of periods of life is inferior to smoothing across a large pool of workers. Furthermore, even with forward looking workers, there may be considerable capital market constraints faced by the worker carrying out her planned lifetime consumption path.[6] Given our imperfect understanding of each of these factors, and their interaction, the consumption smoothing effect of UI is ultimately an empirical question.

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:

(4) $\Delta C/C_e * [R(C_u)] = E_b^*$

where C_e and C_u are consumption when employed and unemployed, respectively, $\Delta C = C_e - C_u$, $R(C_u)$ is the coefficient of relative risk aversion (evaluated at the level of consumption when unemployed), and E_b^* 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,

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[6]Consider an individual who faces a known probability of unemployment in one period of a many period life, but the timing of this spell is unknown. If the individual can borrow, she can smooth this small consumption fall over her entire lifetime; UI will only have a very small consumption smoothing effect through risk-sharing. If she cannot borrow, however, then she may not have accumulated sufficient assets to smooth her consumption during the unemployment spell, and UI may have a much bigger effect. There is some limited evidence on the savings behavior of the unemployed in the PSID, which periodically collected information on individual savings. Among individuals who lose their job, only 56% had any savings before the job loss, and only 23% had savings of more than 2 months income. The comparable figures for those not losing their jobs were 84% and 52%, respectively.

as a function of the level of UI benefits. Evidence on the former was available at the time that Baily wrote his articles, and improved estimates have subsequently been provided by Meyer (1990). But there is no evidence on the relationship between UI generosity and the consumption change of the unemployed. I will estimate this relationship in Part III; in Part IV, I use my estimates to simulate the optimal level of UI benefits.

b) Consumption and Unemployment - Evidence

Past evidence suggests that workers are not fully insured against falls in consumption during unemployment, so that there is a potential consumption smoothing role for public UI; both Dynarski and Shefrin (1987) and Cochrane (1991) demonstrate in PSID data that consumption falls significantly during spells of unemployment. However, neither test provides a definitive rejection of the null of "full consumption insurance" posed by Cochrane, for two reasons. First, leisure may rise during spells of unemployment, and if leisure and consumption are substitutes, then consumption will fall even if there is full insurance. Second, utility may be state-dependent; that is, even if leisure is unchanged, the marginal utility of a given level of consumption may be lower when unemployed.[7] No test of full consumption insurance which compares consumption across two different states can convincingly surmount these fundamental identification problems. Thus, whether there is scope for consumption smoothing by UI remains an empirical question.

On the other hand, the analysis performed here provides a stronger test for the presence of full consumption insurance against unemployment spells. If such insurance exists, then we should

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[7]See Viscusi and Evans (1990) for evidence of the dependence of marginal utility on health states. Cochrane notes that these are potential problems with his approach; he assumes that the social planner possesses some extra-market institution through which leisure can be transferred, and that there is not state dependence.

see no effect of increased UI generosity on consumption smoothing among those losing their jobs; UI increases should simply crowd out other sources of consumption insurance. By examining the effect of changes in UI generosity among workers in the same state (unemployed), this test controls for state dependence in assessing the extent of consumption insurance.[8]

Direct evidence on the role of UI is provided by Hamermesh (1982), who examines whether the marginal propensity to consume out of UI benefits is equal to the marginal propensity to consume out of other income. He takes his rejection of this null as evidence that individuals are liquidity constrained during unemployment spells, justifying the provision of public unemployment insurance. His cross-sectional test, however, is performed by comparing individuals who receive UI to those who do not. A problem with this approach is that individuals receiving UI may differ in a number of ways from those not receiving UI, not the least of which is the fact that they are unemployed. A stronger test would be to compare two individuals, both of whom are unemployed, and assess whether increasing the UI available to one, relative to the other, increases her relative consumption. This is the spirit of the test carried out below.[9]

II. Data and Specification

a) Data

My data for assessing the consumption smoothing effects of UI are the Panel Study of Income

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[8]A test of this nature is carried out by Hausman and Paquette (1987), who measure the consumption change of involuntary early retirees relative to that of voluntary retirees. If involuntary retirement is truly exogenous, then they surmount the state dependence problem by focusing only on individuals who have retired. They find a strong relative fall in the consumption of involuntary retirees, rejecting full consumption insurance along this dimension.

[9]Dynarski and Shefrin also include UI benefits received in their model, and they find a positive but insignificant effect of benefits on consumption changes. However, as described below, this is not a convincing test of the consumption smoothing role of UI, since takeup of UI is endogenous.

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 sub-sample 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.[10] This disadvantage, however, is outweighed by the uniqueness of having repeated observations on both food consumption and a variety of socioeconomic characteristics. As a result, these data have been widely used in tests of the applicability of the life-cycle model of consumption (ie. Hall and Mishkin, 1982) and the presence of liquidity constraints (Zeldes, 1989; Engelhardt, 1993), as well as in the papers described above by Dynarski and Shefrin (1987) and Cochrane (1991). Following Zeldes, I deflate each component of food consumption by the CPI 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.[11]

One key question with these data is the frame of reference for the food consumption measure. Individuals are predominantly interviewed in the spring, and some survey questions explicitly refer

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[10]I consider this point in more detail in interpreting my findings in Part IV.

[11]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.

to the point of the interview (ie. family structure), while others explicitly refer to the previous year (ie. labor earnings). However, the reference period for the food question is ambiguous, and past researchers have assumed both that it refers to the past year (Hall and Mishkin, 1982; Dynarski and Shefrin, 1987) and to the point of the interview (Zeldes, 1989). Zeldes argues convincingly, however, that the data refer to the point of the interview, since this question is asked following another question about current consumption (of food stamps), and since this is the stated intention in the 1972 interviewers manual.[12] 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 were employed at the previous interview. There is one observation for each such person/year from 1968-1987.[13] Individuals are defined as unemployed if they are looking for a new job, but are not on temporary layoff.[14] Temporary layoffs are excluded because the information about both 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.

b) Specification

The basic specification is run on individuals who are employed at interview date t-1 and

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[12]One exception is food expenditures paid for using food stamps. Before 1977, this is measured using average food stamp expenditure last year; from 1977 onwards, food stamp expenditure in the month of the survey is used. The fact that individuals are asked about "usual" consumption may cause them to somewhat smooth their response over spells of employment and unemployment, which would bias against finding a smoothing effect of UI.

[13]Consumption data were not gathered in 1973, so that the values for consumption change are missing for 1973 and 1974.

[14]Before 1976, it is not possible to distinguish those on temporary layoff from the employed. My results are unchanged if I include those on temporary layoff in the employed sample from 1976 onwards as well.

unemployed at interview date t. Selecting the sample in this way leads to a potential sample selection bias; I discuss this in the results section. For these individuals, I run regressions of the form:

$$(5) \qquad \Delta C_i = \alpha + \beta_1 X_i + \beta_2 UI_i + \epsilon_i$$

ΔC 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*.[15] 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 head, 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 respondents 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 Blanchard and Katz (1992). Finally, I include separate dummies for each year to capture time trends in consumption changes.[16]

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 Employment and Training Administration (various years), which

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[15]The formulation in terms of changes in log consumption can be derived from an underlying constant relative risk aversion utility function; see Hausman and Paquette (1987). It also is most appropriate for evaluating the optimal benefits formula given by equation (4). Correcting the error term for the fact that there are repeated consumption change observations for most heads, along the lines of White (1984) and Zeldes (1989), yields almost exactly the same inferences as the results presented below.

[16]Other natural candidates for control variables include wealth and the labor supply of other family members. However, these other forms of consumption insurance are potentially endogenous to UI benefit levels.

reports semi-annual information on state benefit schedules; in addition, it was augmented by information from a number of states and from Levine (1990). UI benefits are generally calculated as a function of the individual's highest quarter of earnings in some base period (ie. last five quarters), along with information on the number of dependents (in some states). Since quarterly earnings information is not available, I use the 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. After tax real wages and the number of children are also included as control variables to capture any spurious correlation between individual characteristics, UI benefit levels, and consumption changes.[17]

The appropriate measure of the extent of wage replacement must account for the fact that UI benefits are subsidized by the U.S. tax system. Before 1979, benefits were not taxed under either the income tax or Social Security tax, while wages were taxed under both, so I multiply the replacement rate by: $\frac{1}{(1-\tau-\tau_{ss})}$, where τ is the income tax rate, and τ_{ss} is the social security tax rate. From 1979 through 1986, benefits were subject to income tax above certain income cutoffs,[18] so that the replacement rate is only multiplied by the Social Security tax adjustment; this was true at all income levels for 1987 onward. Finally, for those above the Social Security taxable maximum in any given year, there is no Social Security adjustment.

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[17]The results are unchanged if nonlinear functions of the wage are included as well, such as squared or cubed terms or wage splines. Below, I discuss an alternative strategy for dealing with the fact that UI benefits are a non-linear function of wages. 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 wages were below the minimum UI benefit for that state and year; this is somewhat less restrictive than the rules used by most states over this period.

[18]The cutoffs were $20,000 for singles and $25,000 for marrieds from 1979-81, and $12,000 for singles and $18,000 for marrieds from 1982-86.

The PSID reports marginal income tax rates for respondents from 1976 onwards. From 1970-75, the PSID does not report tax rates, but does report taxes paid; following Zeldes (1989), I use the tax tables for each of those years, along with information on marital status, to calculate the marginal rate. For 1968 and 1969, I use information on taxable income, number of dependents, and marital status to calculate the tax rate.

The output of the UI benefit simulation model is reported in Table A1, which reports after-tax replacement rates for each state for 1969, 1979, and 1987, as well as the change from 1969 to 1987 and from 1979 to 1987. A number of states are missing in the early years, owing to the smaller sample size of the original PSID cohort. There is a large amount of variation in UI replacement rates across states and over time.[19] There is a strong secular decline in benefit generosity over this period, 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. Blank and Card (1991) document that the takeup of UI only 67% among those eligible for benefits, and that takeup is a function of potential benefits eligibility. If the factors determining UI takeup are correlated with consumption changes from job loss, estimates of (5) using actual UI benefits received would not be valid for predicting the response to future changes in legislated UI generosity. For example, if there is some stigma associated with receipt of UI, then individuals who face only a short expected layoff, and thus may only see a small proportionate change in their

[19]This variation reflects differences in both individual characteristics and state/year legislative environments. I will present an instrumental variables strategy below for separating these two components of replacement rate variation.

consumption, will not takeup UI benefits. This would lead to a downward bias to the estimate of β_2.

Second, the data on actual UI receipt are very noisy. This is one reason that most previous studies of the behavioral effects of UI (ie. Feldstein, 1978; Topel, 1983; Levine, 1993) have used information on benefit eligibility, rather than benefit receipt.[20] Feldstein (1978) notes that aggregating nationally representative individual survey data results in a figure for UI benefits receipt which is one-half of the administratively-reported total of benefits paid out. Noisy data on UI receipt are a problem in my sample as well; for approximately 1/4 of the observations where individuals report receiving UI, they report no weeks of unemployment. Furthermore, UI receipt is reported for the previous year, while consumption is based on the point of the interview, and UI benefits received are only reported for 1977 onwards; before that point, they are grouped with workers compensation benefits.

Of course, an alternative means of circumventing the problems of an endogenous and noisy measure of actual UI receipt is to instrument receipt with benefits eligibility. However, the third reason for running regressions of the form of (5) is that they yield the reduced form coefficient of policy interest, the effect of legislated changes in the UI replacement rate on consumption smoothing. In contrast, this instrumental variables regression would report the effect of changes in UI *receipt* on consumption smoothing. Since the government cannot directly control UI receipt, but can control UI benefit eligibility, this instrumental variables estimate is of less interest in terms of policy design.[21]

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[20]A notable exception is Meyer (1989,1990); however, he uses administrative data, in which this measurement error problem is presumably greatly attenuated.

[21]As a result, the estimated coefficient in the reduced form results below will understate the effect on those actually receiving UI. In my sample, a one dollar increase in UI benefits eligibility leads to a 48 cent increase in UI benefits receipt; this is from a regression of benefits receipt on benefits eligibility, where benefits received are averaged over the years preceding interviews t and t+1 (to

The means and standard deviations of the data set are presented in Table 1, for the full sample of individuals employed at t-1, those who remain employed, and those who become unemployed. Those who become unemployed have a much lower real wage and consumption level in the previous period, are more likely to be female and black, and are less likely to be highly educated or married. Most importantly from the perspective of this paper, unemployment is associated with a drop in food consumption of 6.8% on average, while remaining employed has a very small positive effect.

III. Results

a) Basic Results

Table 2 presents the basic regression results, for the sample of individuals who are employed at period t-1 and unemployed at t. 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. The drop is also larger for older heads, and smaller for female heads and white and black heads (relative to other non-whites). 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% rise in the replacement rate reduces the fall in consumption

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account for the timing problem) and set to be missing before 1977 for workers missing more than 2 weeks of work due to illness (and therefore assumed to have received workers compensation). 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 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.

upon unemployment by 2.65%. Furthermore, I find that the fall in consumption for those becoming unemployed, at a replacement rate of zero, is 22.2%. 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% average drop reported in Table 1. But, at a replacement rate above 84%, UI fully smooths consumption across the unemployment spell.[22]

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 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 (5) through a series of specification checks designed to address three potential problems with my empirical approach. The first, discussed in Meyer (1989), is that the UI replacement rate for a given individual is a function not only of the legislative environment in their state/year, but also of their individual characteristics, such as their wage, tax rate, and number of children. While these variables are included linearly in the regression, they determine UI benefits in a very non-linear way. If consumption changes depend on these variables in a corresponding non-linear fashion, then there could be a spurious correlation

[22]These predictions are not grossly out of sample; 90% of the sample has replacement rates within the range of 20% to 80%. 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% of the total consumption bundle, according to tabulations from the 1980-90 Consumer Expenditure Surveys. If total consumption responds to unemployment, and UI, proportionally to food consumption (an issue discussed below), then the fall in total consumption is $1350 given average UI benefit levels, but would be $4400, or $84.62 per week, in the absence of UI.

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 (5).[23]

The results of this exercise are presented in column (5) of Table 2. 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% level, and indicates that a 10% increase in the replacement rate is associated with a 3.25% 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

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[23]See Currie and Gruber (1994) 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.

that, when times are good, states may increase UI benefits, and at these same times workers who lose their job may see consumption fall less (ie. because their spouse is able to find a job).

I deal with this potential problem in two ways. First, in the final column of Table (2), 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.[24] 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 *within-state control groups*. That is, I rerun my regression for three other populations which should not see first order consumption smoothing effects of variations in UI generosity. If my estimated effect truly reflects exogenous consumption smoothing benefits of UI, then the coefficient on the after-tax replacement rate for these control groups 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 large coefficients on the replacement rate for other groups.

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[24]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. In this paper I only model the effect of benefit levels, and not benefit duration, on consumption smoothing. This is because any variation in benefit duration across workers is primarily a function of either worker characteristics (those with short past employment spells are entitled to shorter spells of UI receipt), state unemployment rates (through extended benefits programs), or federal unemployment rates (through federal supplemental benefits programs which extend benefits yet further). All of these factors are likely to be otherwise correlated with the consumption changes of the unemployed, so that it would be difficult to separately identify the consumption smoothing effect of benefit duration increases.

The first control group is 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 consumption growth rates. But these effects should be small relative to the consumption smoothing effects on the unemployed measured earlier. The consumption change regression for those remaining employed are presented in column (1) of Table 3. The coefficient on the after-tax replacement rate is significant at the 15% level, but it is quite small. Thus, this check supports the contention that the effect of UI is causal.

Another natural within-state control group is those who quit their jobs. The results thus far have included quitters in the sample of the unemployed. However, in most states during this period, quitters were technically ineligible for UI. In the second column of Table 3, I examine separately the sample of quitters. Surprisingly, the estimated consumption smoothing effect for quitters is of a similar magnitude to that for all job losers, although it is insignificant due to a much smaller sample size.[25]

However, there may be a fundamental problem with differentiating quitters from job losers using this self-reported data. Topel (1983) reports that only 10% of separations arose from quits over the 1968-1979 period; from 1969-1979 in the PSID data, over 28% of job leavers report themselves as quitting. Furthermore, over 20% of those who report having quit their jobs still collect unemployment insurance; this is approximately one-half of the rate for other reported job losers. And the drop in consumption for quitters at a zero replacement rate is of a similar magnitude as for others becoming unemployed, which would not be expected if quits were planned and layoffs

[25]This is not due to the fact that quitters are eligible for benefits in a limited set of state/years; removing these state/years from the sample has no effect.

were exogenous negative shocks. Thus, the distinction between quitters and other job leavers in this data does not seem to be a useful one. If quitters are excluded from the unemployed sample, the basic results are unchanged, although they are estimated somewhat less precisely.

This potentially troubling result motivates the use of a third within state control in column (3): retirees. This is a group which is also voluntarily leaving their job, and for whom UI should have no effect. Retirement is measured much more precisely in the PSID than is quitting: the reported retirement rate for 55-64 year olds from 1980-1987, 7.3%, is almost exactly the same as that measured in the Current Population Survey over this period (Gruber and Madrian, 1993), and few retirees report receiving UI benefits. For this sample, there is once again a zero correlation between consumption changes and benefit generosity.[26] Thus, this more precisely measured control group confirms the hypothesis that my findings do not reflect spurious correlations between benefits setting and within-state consumption opportunities.

Finally, I consider the potential criticism of sample selection bias that arises from my focus on unemployed individuals only. Feldstein (1978) and Topel (1983) have shown that the probability that an individual is laid off is a function of the replacement rate. This is potentially problematic for my approach, if the marginal individuals who are laid off when replacement rates rise are systematically different from the average layoff. 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. 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 non-temporary layoffs

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[26]The results are similar if all labor force leavers, rather than just retirees, are used.

by directly measuring the correlation between losing a job and UI generosity. In column (4) of Table 3, 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% increase in the replacement rate raises the likelihood of becoming unemployed by only 0.02% (compared to a sample mean of 3.3%). This suggests that there is little problem of selection bias in my sample.

This finding does not contradict the conclusions of the earlier literature, due to the exclusion of temporary layoffs from the sample. If similar regressions are estimated for the probability of temporary layoff, I find a sizeable and significant positive effect of UI generosity.

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 4, 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 2, examining the effects of UI on consumption changes from t-1 (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 2, 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 2: a consumption fall with unemployment that it 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 consumption 2 period change from t-1 to t+1: there is an overall negative effect of becoming unemployed in period t of 9.1%, but there is only an insignificant consumption smoothing effect of 5.7% 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 non-employed. 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 unemployment spells but having little effect beyond one year.

d) Layoff Anticipation and Consumption Response

As was illustrated in Figure 1, the extent to which consumption responds to unemployment

will be a function of the likelihood of layoff. With perfectly certain layoff and non-myopic workers, consumption will not fall upon unemployment and UI will have no effect. This type of result may be driving the finding in Table 4 that the consumption smoothing effect of UI from period t-1 to period t is much smaller than for those who eventually become reemployed than for those who do not; those becoming reemployed are more likely to have anticipated their unemployment spell.[27] 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 is 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.[28] The second is those who 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 first column of Table 5 illustrates, for this group of "anticipators", there is in fact no

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[27]This finding could also be somewhat driven by the fact that unemployment durations are about 25% longer (over the two year period surrounding interview date t) for those remaining non-employed. In this paper, I do not use weeks unemployed as my measure of unemployment for assessing the UI consumption smoothing effect. This is because (a) weeks of unemployment are measured for the previous year, so that it is difficult to compare their effect to point-of-interview consumption data, and (b) weeks of unemployment are endogenous to UI benefit generosity, leading to the type of sample selection problem described above.

[28]Of individuals who are temporarily laid off and then reemployed, over 50% return to their former employer. On the other hand, of individuals who become otherwise unemployed and then reemployed, less than 20% return to their former employer (author's calculations using PSID data for 1981-83). 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.

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.

e) Other Consumption Insurance and Types of Consumption

One interesting further question is the extent to which UI crowds out other kinds of consumption insurance during spells of unemployment. The analysis thus far has ignored the fact that UI is not provided in a vacuum, but rather as part of a larger social welfare net. The model of Part I could readily be extended to allow for additional types of public insurance, which could potentially be crowded out by UI. The extent of crowdout will be a function of the relative generosity of UI and other programs, and the nature of substitutability across these programs (ie. whether there is differential stigma attached to their receipt). The net public cost of smoothing the consumption of the unemployed will be a function of the combined effect of these different public programs.

One other public insurance program which is readily available to the unemployed is food stamps, a means-tested program which subsidizes the purchase of food. The consumption measure used thus far incorporates consumption paid for by food stamps. By comparing results with and without food stamps, we can assess the extent to which food stamps interact with UI in smoothing consumption. Columns (2) and (3) of Table 5 examine the response of home food consumption to unemployment and UI generosity, with and without food stamps. In fact, the consumption smoothing effect of UI is much larger if consumption paid for by food stamps is excluded. This suggests that, as UI becomes more generous, unemployed workers make less use of food stamps in order to smooth consumption. This type of interaction is important to consider in assessing the budgetary

implications of increasing UI generosity.

One hypothesis for the estimated consumption smoothing role of UI could be that individuals are switching from restaurant meals to home meals when unemployed, lowering their expenditure on food but maintaining their total level of consumption, and that more generous UI simply mitigates this compositional shift. If this is the case, then the response of food away from home to unemployment and UI generosity should be much larger than the response of food at home. The results for food away from home are presented in the final column of Table 5. The drop in food away at a zero replacement rate is similar to the drop if food at home without food stamps; it is much larger than food at home with food stamps. However, the consumption smoothing effect of UI is smaller for food away, although it is imprecisely estimated. Thus, it does not appear that more generous UI simply shifts the composition of consumption.

IV: Application: Optimal UI Benefits

My finding that the fall in consumption from unemployment is less than 1/3 as large as it would have been in the absence of public insurance 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 this program causes to individual behavior. I do so in this section, using the optimal benefits model of Baily (1978), equation (4) above. I evaluate Baily's model using my estimates for the effect of the replacement rate on consumption smoothing (column (2) of Table 2). The other parameter needed to evaluate equation (4) is the elasticity of unemployment duration with respect to the replacement rate; I use Meyer's

(1990) estimate of 0.9.[29]

The results for the level of optimal UI benefits are reported in Table 6, 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% 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.

This low level of the optimal replacement rate may reflect shortcomings in both Baily's formulation and my application of it. One obvious weakness of my application is that I only have data on food consumption smoothing. The bias which this imparts to my estimates is unclear ex ante. Traditional consumer budget studies show that food is a necessity, suggesting that it will respond less to unemployment shocks than total consumption; Hausman et al. (1988) estimate an expenditure elasticity for food of approximately 0.7. Thus, using food consumption only could potentially understate the consumption smoothing effects of UI. On the other hand, I am aware of no studies of how the consumption bundle responds to high frequency income variation of the type represented by unemployment spells; recall that the average duration of unemployment spells in the sample is only 14.4 weeks. It is at least possible that food responds more to short run income

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[29]Meyer's estimate applies to a sample of UI recipients only; for comparison to the consumption smoothing findings, I multiply his elasticity by the takeup elasticity of UI benefits in my sample, 0.48. Note that one weakness with my 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. This suggests that the optimal benefit level would be lower than that shown below if benefits were uniform for all unemployed, but that program efficiency could potentially be improved by having separate benefits schedules for temporary and non-temporary layoffs.

shocks, since other forms of consumption cannot be easily adjusted over this time period.[30]

The next two columns of Table 6 explore the effect of varying the consumption smoothing estimate to account for the bias from using food only. In the second column, I increase both the estimates of the consumption smoothing effect of UI by a factor of 1/0.7, following the expenditure elasticity estimate of Hausman et al (1988). This increases the optimal replacement rate dramatically at lower levels of relative risk aversion, but only by 25% at the highest level. In the third column, I reduce my estimates by a factor of 0.7. This lowers the optimal replacement rate, which is now zero below a coefficient of risk aversion of 2.8, and reaches a maximum of only 0.275.

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.[31] 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 non-full 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) estimates that increases in UI generosity have no effect on the aggregate unemployment rate. 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).

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[30]Also at issue is the relative complementarity of food and other types of consumption with leisure; see Meghir and Weber (1993).

[31]Flemming (1978) provides a formulation which is somewhat richer, but is much less empirically tractable and still does not address a number of the concerns raised below.

Evidence that at least the former benefit is not important, however, is provided by Meyer's (1989) finding that the ultimate wage received by workers who stay out of work longer due to more generous UI benefits is no higher than the ultimate wage received by similar workers under less generous schemes who return more quickly. 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 Brown and 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. The first is that 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 was a net increment to leisure while unemployed (and leisure is not very complementary with consumption). The second is that he assumes that the probability of layoff, α, is exogenous. As was discussed above, a number of studies have found that, given the imperfect experience rating of the UI system, more generous benefits lead to a higher level of temporary layoff unemployment. A richer model of optimal benefit determination would endogenize the layoff process, building in the experience rating side of unemployment insurance discussed in Topel (1983).

V: Conclusions

As a large and growing share of government budgets in the worldwide, social insurance programs have increasingly attracted the attention of empirical economists. However, this attention has primarily been directed 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 non-trivial. In the absence of UI, I estimate that the consumption of the unemployed would fall by 22%, 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.

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 could also 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 can clearly 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. Woodbury and 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.

There are at least two further interesting extensions of this approach. First, I can model the relationship between UI generosity and the use of other forms of consumption insurance for unemployment spells, such as savings, borrowing, the work effort of family members, and other social insurance programs. Second, I can extend the results on the response of consumption over several periods to assess the effects of worker displacement on short and long run consumption changes. There is a large literature that documents non-trivial costs of worker displacement in terms of subsequent wage growth and employment opportunities.[32] A natural extension of this literature is to consider the effect of displacement on consumption levels 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 and spillovers to the uninsured unemployed.

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[32]See Hamermesh (1989) for a review, and Ruhm (1991) and Jacobson, LaLonde, and Sullivan (1993) for recent results.

Table 1: Means of the PSID Data

	Full Sample	Remain Employed	Become Unemployed
Food Consumption$_{t-1}$ (1984 dollars)	4012 [2054]	4027 [2058]	3567 [1894]
Age	37.9 [11.8]	38.0 [11.8]	34.0 [10.9]
Female (%)	18.7	18.1	25.1
Married (%)	71.2	71.7	55.5
White (%)	64.6	65.2	48.7
Black (%)	31.7	31.2	46.6
Years of Education	12.2 [3.17]	12.2 [3.18]	11.3 [2.82]
Number of Kids	1.26 [1.37]	1.26 [1.37]	1.23 [1.38]
ΔLog Family Needs	-0.0046 [0.121]	-0.0046 [0.119]	-0.0033 [0.150]
Previous Real Weekly Wage (1984 dollars)	420.9 [317.8]	424.8 [314.7]	292.6 [217.7]
Δ in Employment (%)	3.34	0	100
After-Tax Calculated Replacement Rate	0.515 [0.190]	0.513 [0.191]	0.575 [0.163]
County Unemployment Rate	0.068 [0.027]	0.068 [0.027]	0.075 [0.028]
State Unemployment Rate	0.073 [0.021]	0.073 [0.022]	0.079 [0.022]
Δ Log Consumption$_{t-1,t}$	0.0053 [0.349]	0.0079 [0.346]	-0.068 [0.424]
Number of Observations	47953	46349	1604

Notes: Based on author's calculations using PSID. See text for variable descriptions.

Table 2: Basic Regression Results
Dependent Variable is Change in Log Real Consumption

	(1) OLS	(2) OLS	(3) IV	(4) OLS
After-Tax Replacement Rate		0.265	0.325	0.280
		(0.091)	(0.180)	(0.105)
Implied Consumption Fall at 0 RR		-0.222	-0.257	-0.231
After-Tax Real Wage ($*10^3$)	-0.798	-0.136	0.015	-0.177
	(0.249)	(0.337)	(0.516)	(0.385)
Age	-0.002	-0.002	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.066	0.069	0.070	0.067
	(0.034)	(0.034)	(0.034)	(0.034)
Married	0.024	0.024	0.024	0.024
	(0.031)	(0.031)	(0.031)	(0.032)
White	0.110	0.107	0.106	0.114
	(0.052)	(0.051)	(0.051)	(0.055)
Black	0.078	0.077	0.077	0.063
	(0.052)	(0.052)	(0.052)	(0.055)
Years of Education	0.005	0.005	0.005	0.006
	(0.004)	(0.004)	(0.004)	(0.004)
Change in Log Food Needs	0.216	0.215	0.214	0.200
	(0.073)	(0.073)	(0.073)	(0.074)
Number of Kids	0.016	0.016	0.016	0.017
	(0.009)	(0.009)	(0.009)	(0.009)
County Unemployment Rate				0.003
				(0.005)
State Unemployment Rate				-0.888
				(0.985)
State Effects	No	No	No	Yes
Number of Obs	1604	1604	1604	1604

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.

Table 3: Specification Checks				
	Within-State Control Groups			Sample Selection
	(1) Employed Consumption Change	(2) Quitters Consumption Change	(3) Retirees Consumption Change	(4) Become Unemployed? (Probit)
After-Tax Replacement Rate	0.017 (0.012)	0.334 (0.218)	0.004 (0.105)	0.021 (0.100)
Implied Consumption Fall at 0 RR	-0.0005	-0.216	-0.038	
Effect of 10% RR Increase				0.0002
After-Tax Real Wage ($*10^5$)	-0.117 (0.041)	-0.243 (0.788)	0.001 (0.253)	-0.002 (0.0004)
Age	-0.001 (0.001)	0.004 (0.002)	-0.004 (0.002)	-0.014 (0.001)
Female	0.027 (0.006)	0.044 (0.072)	0.148 (0.056)	-0.167 (0.039)
Married	0.032 (0.005)	-0.074 (0.072)	0.114 (0.050)	-0.343 (0.035)
White	0.005 (0.009)	-0.010 (0.125)	0.072 (0.088)	-0.118 (0.057)
Black	-0.013 (0.009)	-0.105 (0.127)	0.051 (0.091)	-0.011 (0.057)
Years of Education	0.002 (0.001)	-0.008 (0.009)	0.003 (0.004)	-0.045 (0.004)
Change in Log Food Needs	0.252 (0.014)	0.199 (0.163)	0.149 (0.132)	0.077 (0.091)
Number of Kids	0.004 (0.001)	0.043 (0.022)	0.041 (0.005)	0.015 (0.009)
Number of Obs	46349	377	751	751

Notes: Standard errors in parentheses. Sample is those remaining employed from period t-1 to period t in column 1; those employed at t-1 and quit at t in column 2; and those employed at t-1 and retired at t in column 3. In column 4, sample is all those employed at t-1, and dependent variable is a dummy for becoming unemployed at period t (relative to remaining employed); regression is run as a probit. All regressions include 16 year dummies.

Table 4: The Effects of UI on Consumption Smoothing Over Time						
Consump. Change	t-1 to t		t to t+1		t-1 to t+1	
After-Tax Replacement Rate	0.218 (0.097)		-0.152 (0.093)		0.057 (0.118)	
Implied Consumption Change at 0 RR	-0.200		0.107		-0.091	
	Empl - Unemp - Empl			Empl - Unemp - Non-Empl		
Consump. Change	t-1,t	t,t+1	t-1,t+1	t-1,t	t,t+1	t-1,t+1
After-Tax Replacement Rate	0.092 (0.116)	-0.076 (0.122)	0.022 (0.148)	0.470 (0.180)	-0.274 (0.159)	0.051 (0.206)
Implied Consumption Change at 0 RR	-0.121	0.092	-0.045	-0.354	0.118	-0.141

Notes: Standard errors in parentheses. Dependent variable in each column is change in log consumption from the first to the second period noted. Top panel reports results for all those who are employed in period t-1 and unemployed in period t; bottom panel breaks the results up into those who are employed at t+1 and those who are not employed at period t+1. Regressions include full set of controls listed in Table 2, including 16 year dummies.

Table 5: Further Extensions				
Specification	(1) Temp Layoff or Previous Unemployment	(2) Food at Home, with Food Stamps	(3) Food at Home, without Food Stamps	(4) Food Away
After-Tax Replacement Rate	-0.038 (0.112)	0.214 (0.104)	0.415 (0.150)	0.154 (0.281)
Implied Consump Change at 0 RR	-0.003	-0.156	-0.377	-0.298
Number of Obs	964	1572	1477	979

Notes: Standard errors in parentheses. In column (1), dependent variable is change in log total food consumption; in columns (2)-(4), dependent variable uses different components of food consumption. Sample in column (1) is those who are on temporary layoff or who have experienced at least one week of unemployment in each of previous two years; in columns (2)-(4), sample is all those becoming unemployed between t-1 and t. Regressions include full set of controls listed in Table 2, including 16 year dummies.

Table 6: Optimal Benefit Calculations			
Rel. Risk Aversion	Base Case	Increase Smoothing by 1/0.7	Decrease Smoothing by 0.7
1	0	0	0
1.5	0	0.091	0
2	0.035	0.286	0
2.5	0.202	0.403	0
3	0.314	0.481	0.076
3.5	0.394	0.537	0.190
4	0.453	0.579	0.275

Notes: Table presents evaluation of equations (4), as described in text. Base case uses estimates from column (2) of Table 2, and elasticity of benefits duration of 0.9*0.478 = 0.43. In second column, consumption smoothing estimates are increased by 1/0.7; in third column, estimates are decreased by a factor of 0.7.

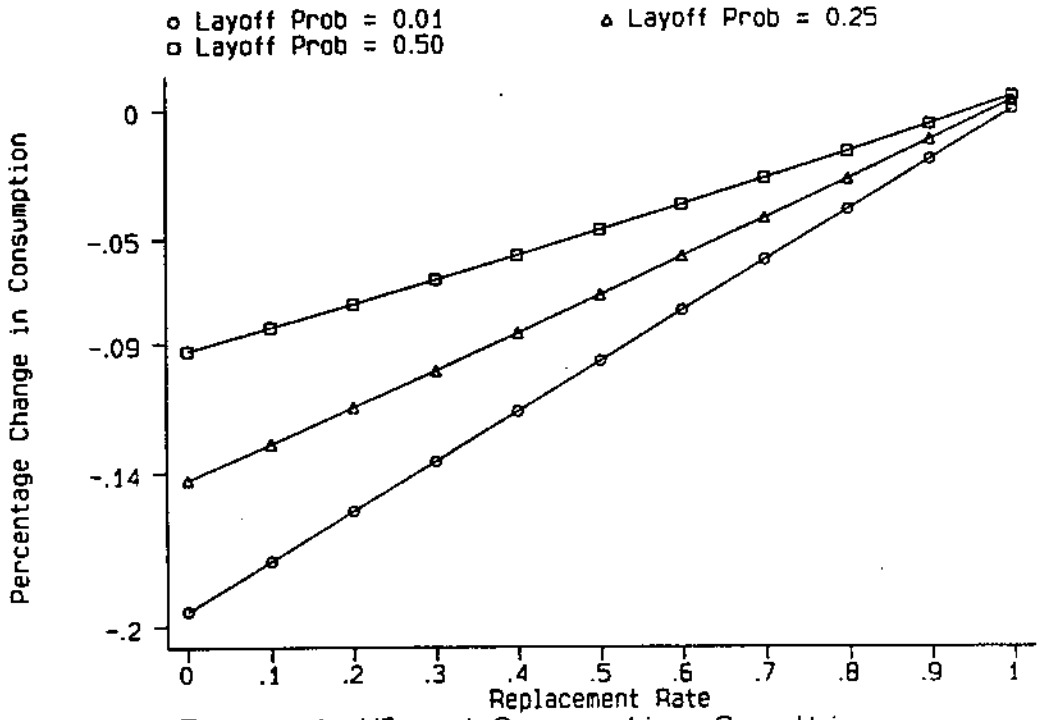

Figure 1: UI and Consumption Smoothing

Table A1: Replacement Rates Across States and Over Time

St	1969	1979	1987	Change, 69-87	Change, 79-87
AL	.686	.563	.357	-.329	-.205
AK		.353	.721		.368
AZ	.510	.468	.341	-.169	-.127
AR	.476	.547	.456	-.019	-.091
CA	.586	.486	.352	-.233	-.134
CO	.644	.682	.486	-.157	-.196
CT	.651	.563	.369	-.283	-.194
DE		.681	.534		-.147
DC	.672	.714	.582	-.090	-.131
FL	.509	.470	.408	-.101	-.062
GA	.555	.540	.397	-.158	-.143
HI		.590	.424		-.166
ID		.315	.361		.046
IL	.573	.560	.390	-.182	-.170
IN	.465	.453	.310	-.156	-.143
IA	.550	.600	.447	-.102	-.153
KS	.528	.580	.494	-.033	-.085
KY	.494	.526	.409	-.085	-.117
LA	.696	.716	.572	-.124	-.144
ME	.478	.553	.434	-.044	-.118
MD	.616	.599	.483	-.133	-.116
MA	.567	.518	.412	-.155	-.105
MI	.619	.480	.442	-.177	-.037
MN	.424	.492	.429	.005	-.063
MS	.580	.534	.451	-.128	-.083
MO	.557	.455	.401	-.156	-.054
MT			.528		
NE	.624	.684	.407	-.217	-.277
NV		.514	.449		-.065

NH		.517	.283		-.233
NJ	.564	.466	.456	-.109	-.010
NM		.599	.342		-.257
NY	.558	.532	.377	-.181	-.155
NC	.599	.616	.486	-.113	-.130
ND	.379	.620	.629	.250	.008
OH	.506	.566	.410	-.096	-.155
OK	.417	.500	.457	.040	-.044
OR	.469	.488	.483	. 014	-.005
PA	.594	.650	.515	-.079	-.135
RI		.610	.411		-.198
SC	.568	.592	.411	-.157	-.180
SD	.488	.493	.347	-.141	-.146
TN	.534	.540	.352	-.182	-.487
TX	.548	.496	.455	-.093	-.041
UT	.493	.582	.415	-.078	-.168
VT		.719			
VA	.545	.557	.392	-.153	-.165
WA	.417	.501	.389	-.029	-.112
WV	.623	.621	.420	-.203	-.201
WI	.575	.612	.426	-.149	-.187
WY		.530	.427		-.103

Note: Each cell reports the average after-tax replacement rate for that state/year, calculated using the PSID data and the simulation program described in the text

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