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DEPARTMENT OF ECONOMICS

Unemployment Insurance Benefit Levels and Consumption Changes.

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Abstract:

A principal motivation for Unemployment Insurance (UI) schemes is to support consumption smoothing. The magnitude of the consumption smoothing benefits of UI depend on the extent to which households are liquidity constrained. We use a survey of unemployed people to examine how a job loss impacts on household expenditures. The principal focus is on the effect of the level of income replacement provided by UI. We restrict attention to a sub-sample of respondents who are still in their first spell of unemployment after six months. For this group we find large consumption falls, averaging about 14% of total expenditure. The actual fall depends on a variety of factors of which the most important is the pre-job loss ratio of the respondent's income to household income.

These consumption falls confound the ‘permanent income’ or wealth shock of job loss, costs of working and, potentially, a liquidity constraint or ‘transitory income’ effect. The effects of varying the replacement ratio - which isolates the transitory effect - are relatively small. We only find effects for those who did not have assets at the job loss. Even for them, the elasticity of total expenditure with respect to benefit is small. We conclude that for most of our sample, small changes in the benefit level will have almost no effect on living standards within the household and hence little impact on other facets of behavior such as job search, unemployment duration and the quality of any new job taken. The data are consistent, however, with some households being liquidity constrained.

1. Introduction

Should governments provide unemployment insurance and, if so, how generous should the provisions be? Unemployment insurance (UI) systems may have a number of benefits, including a redistributive function and a role as an automatic stabilizer. Primarily, however, they are designed to provide insurance against income loss and support consumption smoothing. Against these benefits are the potential distortions to firms' layoff decisions and workers' search and labor supply behavior. There have been numerous studies of the costs of UI in recent years but fewer attempts to measure the benefits. In this paper, we examine the impact of benefit levels on short run living standards within a household with an unemployed member.

Changes in benefit levels may impact on short run living standards within a household with an unemployed member and hence on job search, the duration of unemployment and the quality of any new job taken. If, however, households can adequately smooth living standards over an unemployment spell then the impact of marginal changes in UI benefit levels on any of these outcomes will be largely mitigated. In this case, the major welfare impact of a decrease in benefits will be that households have to hold higher precautionary saving. Although this lack of full insurance is a real welfare loss, it is of only second order importance. Indeed, if households are not liquidity constrained then even this loss will be much reduced since households can always smooth by borrowing (that is, they can self-insure).

Thus the primary case for using changes in the benefit level to affect outcomes rests on whether agents are liquidity constrained at the current benefit level. The question of whether households are liquidity constrained has been the central focus of a great number of studies over the past 15 years.[1] This debate has been remarkably inconclusive, with about as many studies that do not find evidence of excess sensitivity (correlation between expenditure changes and predicted income changes) as do. One of the motives for this paper is the belief that the data that is typically used to test for liquidity constraints is not very likely to lead to powerful tests.[2] If any

[1] See Browning and Lusardi (1995) who list about 25 such studies.

[2] Typical year to year income shocks in, say, the PSID are not very dramatic and the income measure is quite noisy. Thus auxiliary equations that estimate income growth to isolate expected and unexpected components have very low R^2's (of the order of 1%). Surely this substantially underestimates how well agents themselves predict changes. Thus the expected income change variable that is included in consumption Euler equations is a poor proxy and the tests of excess sensitivity have low power. Moreover, a finding of excess sensitivity is itself no sure sign of liquidity constraints; as Carroll (1993) demonstrates, many of the excess sensitivity findings are consistent with a model with no liquidity constraints but with a significant precautionary motive.

segment of the population is likely to be liquidity constrained it is the unemployed. The income replacement typically provided by UI benefits is likely to lead to current income that is lower than 'permanent' income even when the latter is adjusted downwards to reflect the shock (or permanent effect) of a job loss. Thus to attain 'desired' (or 'permanent') utility levels households must either run down assets or borrow. Very many people have very little in the way of liquid assets (see Browning and Lusardi (1995), section 3) and anecdotal evidence suggests that borrowing is likely to be difficult for the unemployed since the first thing that banks and other lenders consider is the labour force status of the applicant. This reasoning suggests three things. First, the role of UI benefits in consumption smoothing remains an important empirical question. Second, the sample we have offers a potentially more powerful test for liquidity constraints than tests on conventional data. Finally, if, as seem likely, only a subset of households are liquidity constrained, then only that subset of households should respond to variations in benefit generosity.

In this paper we examine *changes* in total expenditure as one member of a household moves from employment to unemployment. The expenditure change with unemployment confounds three things: the costs of working (changes due to the non-separability of consumption from labor supply), a response to the ‘permanent income’ shock of job loss, and, a response to ‘transitory income’. Only the last reflects liquidity constraints and should be mitigated by UI benefits. The form of our test will be to see if differences in the UI replacement ratio across our sample make a difference to expenditure changes from before the unemployment spell. There is no variation in labour force status *across* our sample. Consequently, to isolate a ‘transitory response’ the main econometric problem we have is that in our sample the UI replacement ratio is plausibly correlated with the permanent shock from a job loss as well as the temporary income loss. To overcome this we use rich controls for the permanent shock and also instruments for the replacement ratio. Our instruments are based on two sets of legislative changes to the Canadian UI system which are captured by our data. These instruments are correlated with the temporary loss of income but not with the permanent shock.

The data we employ is a large survey of Canadians who experienced a job separation in early 1993 or 1995. Our data merges (panel) survey information, administrative information and tax data for the respondent and his or her spouse (if married). Thus these data give an unusually detailed glimpse into the circumstances of households that contain someone who has had a job separation.

As well as examining the sensitivity of expenditure changes to benefit levels we also provide a description of what happens to *total* expenditure consequent on a job separation. This is first data of which we are aware that allows such an investigation. We describe the impact of a six month unemployment spell and its correlation with a wide variety of household and labor market factors.

The average fall in expenditure is about 14%; this is quite a dramatic fall. Moreover, the change is very different for different households. For example, if the income from the lost job was the only source of income for the household then the fall is about eight percentage points higher than if the lost job income only accounted for half of household income. Although the change in total expenditures is large it should not be inferred that the change in living standards is so dramatic. First, as discussed above, the expenditure change with unemployment will include the impact of non-separabilities between consumption and leisure. If there are costs of going to work or if households substitute home production for market purchases when a member is unemployed then living standards may remain constant even if total expenditures fall. A second break in the link between expenditures and living standards is because total expenditure is for both non-durables and durables. We believe the latter to be important and pursue that idea in a companion paper (Browning and Crossley, 1998). We also take up this point again in our conclusions.

On the specific question of the effect of benefit levels, our conclusion is that differences in the replacement ratio have relatively small effects on changes in household total expenditures. For example, a 10 percentage point cut in benefit levels (from 60% to 50% replacement, for example) would lead to an average fall of only 0.8% in total expenditure. In a more focused investigation we find that there is no effect for households that had some liquid assets at the job separation. Amongst the 'no assets' group the largest impacts are for married households in which the respondent's spouse was not employed at the job separation and who are not eligible for Social Assistance (that is, income support or 'welfare'). Even for this group, however, the elasticity is relatively modest. Thus, on the narrow question of the impact of marginal changes in UI benefit levels on expenditures we find that there are significant but very small effects for some groups. On the wider question of the presence of liquidity constraints, we conclude that there is strong evidence that some households are liquidity constrained.

Two recent studies have also examined the effects of UI benefits on expenditures. In the work most similar to that reported here, Gruber (1997) examines the impact of UI benefits on food expenditure changes using the PSID. Our analysis differs from Gruber's study in several important ways. First, our data allow the examination of changes in total expenditure, rather than food. We also employ a different source of variation in UI benefits to identify the transitory response. Finally, recognizing that not all households are likely to be constrained, we allow the effect of benefit levels to differ across groups of households. Gruber's basic finding is that there is a significant but small impact of variations in the replacement rate on *food* expenditures. The estimates suggest that a decrease of ten percentage points in the UI replacement rate would lead to an average fall of about 2.5% in food expenditures . These estimates imply larger responses than we observe, and potentially much larger depending on the relationship between food and total expenditure responses. We reserve a further discussion of this issue until the concluding section of the paper.

We can trace the impact of benefits to household welfare through the chain: UI benefits → personal income → household income → household expenditures → household utility level. Some of these links have been explored. For example, the link between personal income and household income depends on the reaction of the earnings of other household members (see, for example, Kell and Wright (1990), Gruber and Cullen (1996) and Antecol and Crossley (1998)). This paper and Gruber (1997) are mainly concerned with the overall link from benefits to expenditures. In the second recent study of the effects of UI benefits on expenditures, Slesnick and Hamermesh (1995) present a more ambitious study of the impact of having some benefit on household welfare levels. Although the principal focus of the Slesnick and Hamermesh paper is on the latter link they also provide an empirical analysis of some of the other links based on the US CEX from 1980 to 1992, albeit without taking into account the link between the timing of durables purchases and household utility that we believe to be important. The benefit variable used is receipt of UI benefits sometime in the previous 12 months. They conclude that “demographically identical households that receive benefits achieve the same level of economic welfare as demographically identical households that do not receive benefits". Note, however, that 'UI recipient' households at the time of the survey may not contain any unemployed members and 'non-UI' households may have unemployed members who do not receive benefits.

The rest of this paper proceeds as follows. In Section 2 we derive a version of our estimating equation and discuss several issues in its implication. In Section 3 we introduce the data and institutional setting. Section 4 presents an informal analysis of the distribution of expenditures changes and correlates of expenditure changes. As well as being of independent interest, these statistics reassure us that our somewhat novel data is of reasonable quality. In Section 5 we report the results of our investigation of the effect of marginal changes in the benefit level. Section 6 concludes.

2. Theory

2.1 The basic model

We begin by considering changes in expenditure around a job separation for an agent who lives alone. We consider a discrete period, single non-durable good model in which new information is only revealed at the end of each period. Consumption in any period takes place before the end of period information is known but after the current period income is revealed. We are considering an agent who suffers a job loss, so let period *t* be the period before the job separation and period *t+1* the period after; thus the job separation takes place at the end of period *t* and may not be fully anticipated in period *t*.

Let λ_t be the marginal utility of expenditure in period *t*; the condition for optimal intertemporal allocation between *t* and *t+1* gives that

$$\lambda_{t+1} = \lambda_t - u_{t+1} \tag{1}$$

where u_{t+1} is a 'surprise' error term[3] which is orthogonal to the information set in time t; it includes the shock from the job loss (and hence is likely to be negative) as well as any other new

[3]The Euler equation is written with a 'minus' in front of the surprise term so that negative shocks lead to a rise in the marginal utility of expenditure.

information that arrives at the end of period t. Thus:

$$E(\, u_{t+1} \mid I_{\,t}) = 0 \qquad (2)$$

(where I_t is the information available at time t). However, our sample below takes only those who experienced a job loss. If this separation is partially unexpected (with respect to the information set in time t) then we have that the expectation of u_{t+1} conditional on I_t and a separation occurring between t and t+1 is negative and may be correlated with past information.

To formalize this, let the realized shock be $\Delta_L + \epsilon_{\,t+1}$ if the agent loses their job between periods t and $t+1$ and $\Delta_E + \epsilon_{\,t+1}$ otherwise. Thus Δ_L represents the 'permanent' shock (the revision to the marginal utility of expenditure) from a job loss. The residual term $\epsilon_{\,t+1}$ captures all of the impact of news except for that concerning any job loss. Thus $E(\,\epsilon_{\,t+1} \mid I_{\,t}) = 0$. If π_t is the probability of a job loss between t and $t+1$, given the information available at time t, then the latter equation and equation (2) give $\pi_t \Delta_L + (1-\pi_t)\Delta_E = 0$. This relationship has a number of implications. First, both of the 'job' shocks are zero if there is no uncertainty concerning the job loss (that is, π_t=0 or 1). Second, the two shocks have opposite signs if there is some uncertainty (presumably, Δ_L is negative). Third, the less expected the job separation is, the greater the negative shock associated with a job loss with respect to the shock of keeping the job. Finally, we have the revised version of equation (2):

$$E(\, u_{t+1} \mid I_{\,t},\, Job\ Loss) = \Delta_L \qquad (3)$$

This captures the important feature of our stochastic specification which is that the Euler equation shock for a selected sample is not necessarily uncorrelated with past information.

There are two sets of correlates for Δ_L . First we have variables that reflect the permanent shock from the job loss; denote these Z_t where the t subscript emphasizes that these variables are known at time t (and some may even be permanent). Examples include the occupation in the lost job, earnings and tenure on that job, the race, age and family situation of the agent and local labour market conditions.

The second set of correlates with Δ_L arise from the possibility of the agent being liquidity constrained. If this is the case then the job loss has a negative impact over and above the permanent shock. More importantly for the purposes at hand, in this case the UI benefit level will enter directly into the determination of Δ_L and hence into the level of consumption in period $t+1$. Denote any UI benefit received by the agent as B_{t+1} and pre-separation earnings as Y_t and define the 'replacement ratio' as $a_{t+1} = (B_{t+1}/Y_t)$. As well as the replacement ratio, the level of assets that the agent carries forward from period t to period $t+1$ will also affect the level of the liquidity constraint Lagrange multiplier, which we denote μ. Denoting asset levels at the beginning of period $t+1$ by A_{t+1} we have:

$$\Delta_L = f(Z_t,\, \mu(A_{t+1}, a_{t+1})) \qquad (4)$$

The non-negative constraint function $\mu(.)$ is non-increasing in A_{t+1} since higher assets at the beginning of the period reduce the probability of being constrained. It is also non-increasing in a_{t+1}. More specifically, if a_{t+1} is close to zero then the constraint is likely to be binding but as a_{t+1} rises, at some point the constraint no longer binds and μ is zero for all higher values of a_{t+1}. We term the relationship between total expenditure and the replacement rate, conditional on permanent variables, the 'benefit effect'. This reflects the impact of *transitory* changes in income on expenditure.

The job loss function $f(Z_t, \mu(A_{t+1},a_{t+1}))$ in equation (4) is decreasing in μ; this reflects that liquidity constraints make bad shocks even worse. Thus, if the agent is constrained, then increasing the benefit level (and hence the replacement rate) makes the job loss shock less negative. Combining (1), (3) and (4) we have the revised Euler equation:

$$\Delta\lambda_{t+1} = - f(Z_t, \mu(A_{t+1},a_{t+1})) - \epsilon_{t+1} \tag{5}$$

If we parameterize preferences (that is, define λ in terms of consumption) and the functions f(.) and μ(.) then this gives us an equation for consumption changes that can be estimated from the data. Note that this implicitly invokes the usual Euler equation orthogonality conditions. One major concern in doing this is that we have only two periods. The 'Chamberlain critique' points out that the Euler equation orthogonality conditions apply across time and not across agents which invalidates the use of short panels if there are common macro shocks which impact differently on different agents (see Chamberlain (1984) and Browning and Lusardi (1996) for a recent discussion). Here, however, we are conditioning on past levels of *Z*, so that we only need to invoke the much weaker assumption that, conditional on the *Z* variables, any common macro shock has the same effect on all agents.

It is important to control for the permanent shock variables Z_t. To illustrate the biases that would arise from just regressing consumption changes on the replacement rate, consider two variables: 'earnings on the lost job' and the agent's 'attachment to the labor force'. It is plausible that the job loss shock is negatively correlated with earnings on the lost job. Earnings are also negatively correlated with the replacement rate (details will be given below). Thus any correlation between consumption changes and the replacement rate partly reflects this effect. The bias from 'attachment to the labor force' has the opposite sign since attachment is negatively correlated with the replacement rate but positively correlated with the job loss shock since 'low attachment' workers do not experience much of a negative job shock loss. These two examples illustrate that the bias from ignoring the permanent variables cannot be signed a priori .

Effectively, then, we identify the benefit effect by assuming that the replacement rate is uncorrelated with the error term in equation (5) . Even though this is much weaker than assuming that it is uncorrelated with the permanent shock, it is still important to be able to test for the validity of this assumption. To do this involves testing for the exogeneity of the liquidity constraint variables once we have conditioned on the permanent shock variables Z_t . For this we need variables that affect the replacement rate but not the permanent shock; that is instruments for the replacement rate. Since these instruments depend on changes to the Canadian UI system, we leave the details for the empirical section but we emphasize here that we do test the identifying assumption.

To complete the model we need to parameterize preferences and the functions in equation (5). Denoting consumption in period t as C_t we take the following (Frisch or λ -constant) equation for log consumption in period t:

$$\ln C_t = \beta_t + \delta\lambda_t \tag{6}$$

where the variable β_t captures the effects of the discounted price level, demographics and discount factors and δ is negative so that higher lifetime wealth agents (who have a lower marginal utility of expenditure) have a higher consumption level, all else being equal.[4] Note that both β_t and λ_t are known at time t.

Finally we assume that the job loss shock is linear in the permanent variables Z and a_{t+1}:

$$\ln C_{t+1} - \ln C_t = (\beta_{t+1} - \beta_t) + Z_t'\alpha + \eta a_{t+1} + \epsilon_{t+1} \tag{8}$$

In this equation the first term $(\beta_{t+1} - \beta_t)$ captures the effects of the real interest rate, discount rates and changes in the factors that affect preferences. For example, since the agent is employed in period t and unemployed in period $t+1$ one element of this term allows for the cost of going to work which has a negative impact on desired consumption growth. Since there is no variation in this in our data this is absorbed into the constant. The coefficient eta on the replacement may be a function of other variables (in particular, the level of liquid assets at the job loss); we shall return to that in the empirical work below.

2.2 Extensions to the basic model.

Before turning to the empirical analysis we consider two refinements of the theoretical discussion that will be important for the econometric implementation. The first of these is to allow for the availability of other sources of income support during a 'low income' spell. Specifically, suppose that Social Assistance (or 'welfare') provides a transfer to households that have low

[4]The utility function associated with (6) is:

$$\nu^{t}(C_{t}) = \delta^{-1}(C_t(\ln C_t - 1 - \beta_t)) \tag{7}$$

Although this is different from the widely used iso-elastic form it is a good deal more convenient here. On important feature to note is that the marginal utility of expenditure is convex in consumption so that these preferences display 'prudence'.

income.[5] For households in receipt of Social Assistance, as UI benefits fall, so Social Assistance benefits rise on a one-for-one basis. Thus, a reduction in UI benefits for those who receive Social Assistance will have no effect on current income and we should not observe any benefit effect for the poorest households. We shall allow for this in our empirical specification below.

The second issue we need to deal with is that most of our sample of unemployed persons (hereinafter, 'respondents') live with other people. The impact of the job separation on household consumption depends on what proportion of pre-separation household income was from the respondent's earnings. Fairly obviously, if the job lost only accounted for a small fraction of household income we should not expect much of an impact, whatever the replacement ratio. From the data we have we can construct the proportion of pre-separation household income that was accounted for by the respondent's earnings. From this we construct a variable that measures the proportional change in household income consequent on the job loss assuming that all other income sources stay the same. Note that this may not be equal to the actual household income change if, for example, other household members change their income as a result of the job loss. Formally this variable ρ is defined by:

$$\rho = (\, a_{t+1} - 1) * (\textit{ importance of respondent's pre-separation earnings}) \tag{9}$$

(where, as before, a_{t+1} is the UI replacement ratio). We shall refer to this as the 'importance adjusted replacement ratio' or the 'adjusted replacement ratio'. It is zero if the replacement ratio is unity (in which case no income is lost) or if the respondent's pre-separation earnings were zero. It is bounded below by minus unity which represents the case where the respondent's earnings was the only source of income and this is not replaced at all.

3. The Institutional Setting and Data.

3.1 The Canadian Out of Employment Panel.

To understand why and how the data were collected we first present some details of the Canadian UI system. Before April 1993 workers who had a minimum number of weeks of work in the year before a job separation were entitled to UI benefits for a period that could be as long as one year. The exact number of weeks to qualify for UI and the weeks of entitlement depended on the local unemployment rate and ranged from 10 to 20 weeks of work to qualify and between 30 and 52 weeks of entitlement. The benefit paid was 60% of pre-separation earnings up to a maximum insurable earnings of $745 per week (for a maximum benefit of $447 per week.)[6] All

[5]In Canada all low income households are eligible for Social Assistance irrespective of the composition of the household. Thus a large proportion of the Social Assistance case load is single people with no dependents.

[6]To convert (approximately) to U.S., divide by 1.3; for U.K. Pounds, divide by two; for French Francs multiply by 4.

those who had a job separation and who met the entitlement qualification were eligible for UI but 'quitters' were penalized by a 7 to 12 week waiting period.

On April 1993 changes were made to the UI system (Bill C113). The most important of these were that the replacement ratio was reduced from 60% to 57% (and the maximum benefit was reduced commensurately to $425) and 'quitters' were dis-entitled. To evaluate the impact of these changes, a survey of about 11,000 people who had a job separation in February and May of 1993 was conducted by Human Resources Development Canada (HRDC). This survey is known as the Canadian Out of Employment Panel (COEP). Each respondent was interviewed three times, at about 26, 39 and 60 weeks after the job separation. Each interview was conducted over the telephone and took an average of 25 minutes. Although it would have been desirable to have the first interview at a date closer to the job separation this was not possible since the administrative records that form the sampling frame do not become available until some months after the job separation. This long interval between the job separation and the first interview is the price we have to pay if we wish to sample only those who started an unemployment spell.

Bill C17, which came into effect in two stages, April and July of 1994, brought a second set of changes. First, the statutory rate was further reduced from 57% to 55%. Second, the minimum number of weeks worked in the previous year increased from 10 to 12 weeks in high unemployment regions. Third, the duration of benefits was decreased for any given number of weeks of work in the year prior to the job loss. Finally, individuals with dependents and income below $340 per weeks will receive a 60% statutory replacement rate.

HRDC then commissioned a second survey of about 8,000 individuals who separated from a job in February and May of 1995. The survey instrument was refined (and slightly expanded) for this second survey but care was taken to insure backwards comparability. In addition, the third interview was dropped. Together, the 1993 and 1995 COEP surveys capture substantial legislative variation in the parameters of the Canadian UI system. In addition, there was some administrative variation in the system in the time period captured by the data. In particular, maximum insurable earnings were increased annual by a formula that was substantial outpacing inflation. For individuals above the maximum insurable earnings, these increases more than offset the cuts to the statutory replacement rate so that the real value of benefits increased.

The period of 1993 to 1995 was one of slowly improving labor market conditions in Canada (for example, the aggregate unemployment rate fell from 11.2 to 9.5%). In this context our data capture changes in the UI system that for some individuals increase its generosity: the increase in the maximum insurable earnings and the introduction of the ‘dependents’ rate. For other individuals the program became less generous due to the cuts to the standard statutory rate, the disentitlement of quitters; the decreased weeks of benefit for weeks of work and the increased in the weeks of work required to qualify for benefits.

In this paper we use only information from the first interviews. In this first wave a wide range of questions were asked including questions on the pre-separation job; labor market activity in the period between the job separation and the interview; job search details; the activities of other household members; income; expenditure and assets. As well as the survey data we also use tax (for the respondent and their spouse, if any, for the two years prior to the survey year) and UI administrative data for each respondent for up to 5 years prior to the survey year.

3.2. Expenditure Questions.

For the purposes of this paper the most important set of variables are those concerning expenditures. Two sets of questions were asked. The first was a set of levels questions concerning expenditures in the past week or month on a range of goods including housing; food at home; food outside the home; clothing and total expenditures in a month. The latter seems to be the first time such a comprehensive question has been asked so we present the text of the question in full here:

> *About how much did you and your household spend on everything in the past month? Please think about all bills such as rent, mortgage loan payments, utility and other bills, as well as all expenses such as food, clothing, transportation, entertainment and any other expenses you and your household may have.*

Although the answers to this question are somewhat noisy (and there is a good deal of rounding) it seems from subsidiary analysis (not presented here) that the answers are largely sensible. For example, the ratios of total expenditures to income and to food expenditures are about what we observe in a detailed expenditure survey. We denote the answer to this question by X_{t+1}.

As well as questions concerning the levels of a range of specific goods we also asked about the change in total monthly expenditure from before the job separation to the first interview. From the answers to these questions we construct a variable that gives the proportional change in total expenditure from before the job separation to the interview date almost half a year after. Specifically, we construct a proportional change variable $\Delta\, ln\, X_{t+1}$ by dividing the change in total expenditure by the level in period $t+1$:

$$\Delta \ln X_{t+1} = \frac{C_{t+1} - C_t}{C_{t+1}}$$

Note that, this is slightly different from the usual construction which divides by the lagged level. This variable is the 'left hand side' variable of this paper.

3.3. Sample.

As to sample selection, we began by restricting attention to respondents between the ages of 20 and 60 who lost their job either because of a 'shortage of work' ('laid of') or because they quit (other than to take another job) or were 'dismissed with cause’. The main exclusions here are those who left due to illness and those on maternity leave. We also excluded unmarried respondents living with a parent or with unrelated adults. Preliminary analysis indicated that the quality of the responses to the household income and expenditure questions was very poor for these individuals. From this group we identified a sample of 5,318 respondents who were unemployed at the first interview. The next selection is on still being in the first spell of unemployment; this left 3,327 respondents. Thus all those in our sample have been continuously unemployed for about half a year. We then dropped 487 respondents for whom the change in expenditure variable was missing. Then we excluded those who had changes in total expenditure that indicated that their current total expenditure was more than double or less than one half of their previous total expenditure. This left a working sub-sample of 2,617 people. Even for this group some variables are missing so that the sample sizes for some of the analysis below is somewhat less than this total.

4. An informal analysis of expenditure changes.

4.1 The distribution of proportional expenditure changes.

In this sub-section we concentrate on the 'proportional change in total expenditure' variable, ΔlnX_{t+1} and its correlation with other variables. In our sample, the mean change in monthly expenditures is $183, and the mean proportional change 13.7%. This is a substantial fall, but it is much smaller than the income loss of the respondents. Figure 1 compares the dollar expenditure changes of the respondents to their income loss (minus the earnings in the job that ended plus UI benefits). The mean income loss is some $1325 (median $998) and the average replacement rate 48%.

The proportional change in total expenditure is highly variable within the sample; the distribution is given in Figure 2. The principal features of this distribution are clear. First, there is a good deal of 'piling up' at some points; for example, there are obvious mass points are at 0, -100 and -50. Second, the majority of respondents report a fall in expenditure (51.3% , with 31.9% reporting 'no change').

The presence of so many zeros may have many causes. First, there may be significant rounding to zero for those who have only small changes. That this is the case is apparent from a detailed examination of the distribution around zero (not presented here). To address this we develop an estimator that allows for rounding to zero. To do this, we assume that households randomly report zero even when there is a change. The probability of doing this is assumed to be proportional to the absolute value of the true change with a zero-centered Normal distribution. The mean of the distribution taking into account this rounding is -16% and the predicted number of zeros is 18%. Thus, conditional on our rounding assumption, some, but not all of the mass at zero can be attributed to rounding.

As for the remaining 14%(=22%-18%) of the sample who are at zero, other possible reasons why they might be there are that there is actually a mass of agents who did not change their behavior or that the question was misunderstood (and the answer given related to the change from after the job separation). Since we have no way of controlling for the latter we simply assume that this misinterpretation is randomly distributed and is uncorrelated with all our other variables.

4.2 The correlation of expenditure changes with non-benefit variables.

In Tables 1 and 2 we present some correlations of the proportional changes in total expenditure with a variety of other variables.[7] Table 1 gives means for different discrete groups and t-values for the differences between these means. Table 2 presents univariate regressions for continuous variables. For the moment we do not include variables that are associated with UI benefits. These two Tables serve a number of purposes. First, they indicate to the reader the range of possible controls we have in the data and how many non-missing values we have for each. Second, we hope they will indicate to the reader that the expenditure change variable is sensible and is probably measuring expenditure changes, albeit with a good deal of noise and rounding. Finally, these Tables give us a first look at the possible correlates for expenditure changes during a protracted unemployment spell. Of course, in univariate analysis it may well be that some variables appear 'significant' simply because they are proxying for other variables. Consequently, in Table 3 we present multivariate regressions.

Turning to the first row of Table 1, we see that expenditure falls are significantly greater for men than for women. This effect disappears once we control for other variables; in none of the subsequent analysis do we find gender important once we control for family type and other sources of income. As to the other groups, we would draw the reader's particular attention to 'seasonal', 'single' and 'more than high school education'. These illustrate different aspects of the analysis. Seasonal workers have significantly lower expenditure falls than non-seasonal workers. We believe that this reflects the fact that for seasonal workers the loss of a job reveals little new information about 'permanent income'. The larger fall for the 'more than high school' group is simply the converse of this: the loss of a job for someone with higher education represents a larger permanent shock, probably because it is a less common event. Finally, we note that the 'singles' group has a much larger fall than do married couples, particularly if the latter had an employed spouse at the time of the job separation. This reflects the greater importance of the respondent's earnings in the household; as we shall see, these effects are attenuated once we control for the latter.

We also present mean expenditure changes across different regions of Canada (Atlantic, Quebec, Ontario, Prairies and British Columbia). The only significant difference is for the Atlantic region for which the average fall is about eight percentage points lower than for the rest of Canada. This is a very large difference which persists throughout the following analysis in which we control for many of the possible sources of differences between the Atlantic region and the rest of Canada. We have no explanation for this except to note that the Atlantic provinces are generally the most economically depressed provinces in Canada and generally have higher saving

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[7]Details of the variable construction are given in the Appendix. The only one that may need explanation is ' of income committed'. This is the ratio of mortgage payments or rent to current household income; it represents a somewhat imperfect measure of 'fixed outgoings'.

rates than the latter. However, controlling for the local unemployment rate and/or having assets does not take out the Atlantic effect. Quite how households in the Atlantic provinces manage to insulate themselves so effectively from the income loss following a job loss remains something of a mystery.

Of the continuous variables in Table 2, we discuss age; past earnings (either in the month before the job separation or in the previous calendar year) and the importance of the respondent's pre-job loss earnings for the household. Older workers and low wage workers have smaller falls in total expenditure. This is consistent with the view that a job loss constitutes a smaller permanent shock for an older worker or for a low paid one. There are, however, alternative explanations. For example, older households typically have higher resources and may be able to smooth over a spell of income loss more easily than younger workers. In the multivariate analysis, we shall control for this by including controls for having liquid assets at the job separation or for having some home equity. Equally, the income effect could be because household expenditure levels do depend on benefit levels and the replacement ratio is lower for high income respondents because of the 'maximum benefit' rule. This is one of the principal themes of the next section.

Of all the variables in this analysis, one of the most 'significant' is the measure of importance of the respondent's income for the household. As can be seen from row 5 of Table 2, households in which the respondent's earnings were the only source of income before the job separation ('importance' equals unity) have a expenditure fall that is 14 percentage points higher than households in which the respondent's income was unimportant. The fact that we get such a strong and intuitively sensible response suggests to us that our expenditure change variable is meaningful.

Turning to the multivariate analysis we include most of the variables in Tables 1 and 2 and also the square and cube of log(earnings on the lost job). Specification 1 in Table 3 presents the first set of estimates. As we would expect, given the collinearity between most of our variables, many variables become insignificant. Conversely, some variables that were not correlated with the expenditure change variable are now significant. In particular, the ' of income committed' variable has a positive sign. This indicates that households that have high fixed outgoings reduce expenditures by less than those that have, for example, paid off their mortgage.

The estimator that takes into account rounding gives very similar parameter estimates. These are not reported here; in the subsequent analysis we shall only report the OLS results. This concludes our informal discussion of the data. We turn now to a more structural analysis of the impact of different replacement ratios on the change in expenditures following a job separation.

5. Benefit levels and expenditure changes.

5.1 The replacement ratio and expenditure changes

In the last section we presented results on the covariance between the change in log total expenditure and various 'non-benefit' variables. Some of these variables capture the permanent effect of the job loss and some may also reflect the effect of the temporary loss of income due to the job loss and continuing unemployment. As discussed in section 2, to estimate the effects of changes in UI benefits on expenditures we need to control for permanent effects and isolate the temporary effect. The benefit variable we use is the 'importance adjusted replacement ratio' variable defined in equation (9). The set of permanent controls we use are given in Table 3. As we shall see below this seems to constitute an adequate set for controlling for permanent effects.

In the definition of the adjusted replacement ratio, the replacement ratio used is the ratio of UI benefits currently received relative to the self-reported net-of-tax earnings on the lost job. The UI benefit is derived from administrative records and is believed to be very accurate. Since UI benefits are taxable it is adjusted to a net figure using the respondent's average UI tax rate for the survey year. The self-reported earnings figure is subject to noise, both because of reporting errors and because the reporting period (hourly, weekly etc.) is not completely clear in every case. We cannot use the administrative record of earnings (which is presumably more accurate) since it is capped at the maximum insurable earnings. In Figure 3 we plot the distribution of the replacement ratio. As can be seen this is bi-modal with modes at zero and at the administrative value of about 60%. The actual replacement ratio can exceed the latter if, for example, workers earn less per week in the period just before the job separation (to which the earnings question refers) than the average in the 20 weeks before the job loss that are used to calculate the administrative entitlement. Differential tax treatment of benefits and income can also lead to an actual replacement rate which exceeds the statutory value.

In column 2 of Table 3 we present OLS estimates from the regression of the proportional change in expenditures on the 'importance' variable and two benefit measures. The first of these is our adjusted replacement ratio. The second is the importance variable multiplied by a dummy for having no benefit. This allows for a discontinuity at zero. The coefficient on the 'no benefit' term reflects a permanent effect since those with a positive benefit but a low replacement ratio had high earnings and hence a big permanent effect. Those with no benefit almost all have less weeks of work in the 52 weeks before the job separation than the statutory requirement for being entitled for a benefit. Consequently they have low attachment to the work force and the job loss represents a relatively smaller permanent shock. When we add controls for the job loss shock, the significance of the 'no benefit' variable is a check on whether we have controlled adequately for the permanent shock since the (transitory) benefit effect should be continuous at zero.

If taken at face value, the coefficient estimates in column 2 of Table 3 indicate that for a household in which the respondent's earnings were the only source of income ('importance' = 1) and in which the replacement ratio was 60%, expenditures fall by -4.2+10.3*1*(0.6-1)=-8.3%. If the household has a replacement ratio of 50% the fall is -4.2+10.3*1*(0.5-1)=-9.4%. This further 1.1% fall in expenditures is a very small effect for such a large change in benefits; since a cut in the replacement rate from 60% to 50% represents a cut in the benefit paid of 17% this gives an elasticity of consumption with respect to benefit of (1.1/17)= 0.06.

If the household did not receive benefit because of being disentitled then the predicted fall is -4.2+10.3*1*(-1)+4.5= -10.0%. Once again we emphasize that the non-linearity at zero replacement rate partially reflects the permanent shock; this should *not* be interpreted to mean that if a household receiving benefit with a replacement rate of 60% lost its benefit altogether then its expenditure would only fall a further 1.7%. The other implication of the estimates given is that if the respondent's earnings were zero (which implies that 'importance' equals zero) then the constant gives a measure of the costs of going to work - in this case about 4% of total expenditure.

The estimates in column 2 of Table 3 do not have any controls for permanent variables other than 'importance'. In specifications 3 and 4 of Table 3 we give estimates controlling for permanent variables. In column 3 we add controls for demographics, household type, regional and seasonal variation, non-wage characteristics of the job that ended, and measures of the household's financial resources prior to the job loss. Column 4 adds to these variables a polynomial in the natural logarithm of monthly earnings in the lost job and values of log (monthly) earnings from previous years. The most important feature of these estimates is that the coefficient on the 'no benefit dummy' variable is much reduced (and its standard error is increased). This is what we would expect if the benefit variables now reflect only transitory changes since then there should not be any discontinuity at zero benefit. In column 5 and all that follows we drop the zero benefit variable. Column 5 is then our best estimate of the benefit effect with no allowance for different effects across different groups. We regard allowance for the latter as important and we shall present pursue that below. Relative to column 2 the coefficient on the adjusted replacement ratio is somewhat diminished in both size and statistical significance. It has a t-statistic of 1.8 and implies that a cut in the replacement rate from 60 to 50% would induce a further 0.8% fall in expenditures.

As discussed above, we expect a benefit effect only if households are liquidity constrained, and that at most a subset of households are so constrained. Therefore, in the next section we focus on households which are more likely to be liquidity constrained and investigate whether the benefit effect varies significantly across different groups. In the following sub-section we investigate the robustness of our findings.

5.2 The Liquidity Constrained.

Above we simply included the adjusted replacement ratio on the right hand side of the consumption change equation. The estimated coefficient indicates to us that the average household is not liquidity constrained, and thus unemployment insurance benefits largely crowd out private - or other public - smoothing mechanisms. This is not surprising. Our data reveal that between a third and a half of our households hold liquid assets at separation. Also, as discussed in section 2.2, some of our households would be able to replace benefit cuts one for one with social assistance (welfare) benefits. It remains possible some agents are liquidity constrained and their current consumption depends on the current benefit level. In this sub-section we investigate this possibility. To do this, we first identify different groups who are more likely to be liquidity constrained and then cross the adjusted replacement ratio variable with dummies for these groups.

The results of this exercise are presented in Table 4.[8] For convenience, our best estimate of the “average” effect - specification 5 from Table 3 - is presented as well. In specification 6 we target the obvious candidates: those with no liquid assets at the job separation.[9] To do this we cross the importance adjusted replacement with a dummy for this inclusion in this group. When we augment specification 5 with this variable we find that the crossed term is quite significant. The original benefit variable becomes wholly insignificant. Thus it appears that the benefit effect reported in Table 3 is entirely driven by households with no liquid assets at job separation - consistent with the prediction that benefits levels should only affect the behavior of liquidity constrained households. In specification 7 we drop the basic original benefit variable and estimate the benefit effect for this group. This focused effect is larger than the “average” effect captured in Table 3 and strongly significant. However, it is still an economically small effect. The implication is that, for these households, a cut in the replacement rate from 60 to 50% would induce a further 1.3% fall in expenditures.

We also investigated a number of other groups are likely to be constrained. We arrived at two (non-exclusive) groups that are sensitive to benefit fluctuations.[10] The first of these are those who do not have any liquid assets at the job separation. The second are households in which the respondent is married, the spouse was not in employment at the job separation and the household has enough current income (mostly UI benefit) to not be eligible for Social Assistance. This follows from the interaction of UI benefits with Social Assistance ('income support') raised in section 2. As discussed there, households that have low UI benefits may be eligible for income support. If this is the case, then changes in the benefit level should have no effect on consumption since there is a one-for-one replacement of UI benefit with Social Assistance. The level of Social Assistance that a household is eligible for depends on the household composition and the province of residence. From the latter and Social Assistance administrative records we can determine the level of potential support for each household in our sample. We then construct a 'not eligible for Social Assistance' dummy that equals unity if the household's self-reported net income is above

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[8]For parsimony, the full set of results are not reported; they are available on request.

[9]In fact, we are identifying those who reported no investment income in the previous year. The 1995 COEP contains a question about liquid assets prior to the job loss, but the 1993 data does not. An analysis of 1995 data indicates that investment income in the previous year is a very good predictor of liquid assets at job loss of at least 2 months earnings.

[10]As predictors of liquidity constraints we also considered age, regular UI use, renters (versus home owners), singles and those with a spouse not employed at separation date. In households were the spouse was employed, it is likely that the household could borrow against the spouses income. It should be noted that as our final specification (specification 8) is the outcome of such a specification search, the t-values are certainly overstated.

the Social Assistance level.[11]

The final column of Table 4 (specification 8) gives our preferred specification, allowing for separate benefit effects among these two groups. These benefit effects are statistically significant, and larger than any others we report. The largest predicted benefit effect is thus for households in the intersection of these two groups - which comprises just over 8% of our sample. Once again we consider the case where the respondent is the sole bread-winner ('importance' = 1) and a cut in the replacement rate from 60 to 50 (which constitutes a cut of 17% in the actual benefit paid). The predicted further change in total expenditures is (13.0+15.7)*1*(-0.1) = -2.9%, for an elasticity of -2.9/17= -0.17. Evaluated at the means (for households in the intersection of these two groups), this would be a monthly cut in benefits of about $180 and in total expenditures of about $45. Even for these households, our estimates do not imply dollar-for-dollar cuts in expenditures with cuts to benefits. Discussion of these results is postponed until the concluding section which follows an investigation of the robustness of our results.

5.3 Robustness Checks.

In this sub-section we present variants on our preferred specification to check whether the results are sensitive to changes in some of the modeling choices. The results are summarized in Table 5. For convenience, the first row presents the preferred estimates from the final column in Table 4. Here we also check the sensitivity of the parameters of interest to changes in the sample using DFBETA statistics (see Chaterjee and Hadi (1988), for example). These statistics are computed for each observation; they show by how much the parameter in question is 'influenced' by the observation. Thus a large negative value means that if this observation is removed then the parameter value would be become more positive. Similarly, a value close to zero indicates that this observation could be removed without changing the coefficient value. The second row of Table 5 indicates that the largest (in absolute value) DFBETA statistics for the coefficients on the replacement ratio variables is less than 0.4. Thus we conclude that our results are not being driven by a small number of outliers.[12]

We turn now to a number of restrictions on the estimating sample. The first experiment concentrates on the 'importance' variable. It is motivated by the fact that for many of the respondents the 'importance' variable is greater than unity. Unless the household had negative net

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[11]There is also an asset disqualification rule in most provinces. We have not built this into our eligibility variable.

[12] A plot of the DFBETA statistics against the proportional change in total expenditure (not reported) further reveals that the vast majority of observations have very little influence on the parameter estimate (this is typical of empirical work on micro data); effectively the results are dominated by those with large negative falls in expenditure. Amongst these, it is , of course, those who have the smallest replacement ratio who make the parameter estimate 'more positive' and conversely for those with a high replacement ratio.

income for other sources of income than the respondent's earnings (which is possible) then this must be because of the measurement error in the lagged net income and earnings variables that are used to construct the importance measure. We drop respondents who had 'importance' greater than unity; as can be seen the parameter estimates are virtually unchanged.

The next two experiments address concerns about the variation in the replacement ratio. As can be seen from Figure 3 we have a substantial number of respondents with a replacement ratio of above the statutory rate of around 60% and also a number who do not receive benefit. As explained above, the former is partly the result of the differential tax treatment of benefits and earnings, but in the case of very high replacement rates is likely a consequence of our measure of past earnings being different from 'insurable earnings' and measured with error. Could this be biasing our results? To check this, we simply exclude those with a replacement ratio of above unity. As the third row of Table 5 reports, the results actually become slightly stronger.

The other concern about the replacement rate is that our results might be driven solely by the variation between those who have no benefit and those who do. The drop in significance on the 'no benefit' dummy recorded between columns 2 and 3 of Table 3 suggests that this is not the case but just to be sure, in experiment 4 we present the parameter estimates for the sub-sample who have positive benefit (using the 'preferred' replacement rate). Here, we find that our results become stronger again. The “zero benefit” group in our sample comprises both individuals who have exhausted their benefits and those who do not take up, including the ineligible. However, conditional on our rich controls we find no difference between these groups. We interpret this strengthening of the response when excluding “zeroes” as indicating a nonlinear “s-shaped” relationship between benefit levels and expenditure changes. Certainly, as the replacement rate approaches one the ‘transitory income’ effect must diminish - presumably to nothing at full replacement. The strengthening of our results with the exclusion of high replacement rates in experiment 2 are consistent with this. It also seems plausible that cuts to very low replacement rates are likely to have less effect. If households’ expenditures have fallen to a subsistence level, then they may access resources that they would otherwise be unable or unwilling to utilize. For example, borrowing from family and close friends may be contingent on (no more than) subsistence level expenditures. If mechanisms such as these do operate, then cuts in the replacement rate from, for example, 10% to zero should be expected to have a much smaller impact on expenditures than cuts from 60 to 50%. Unfortunately, the distribution of replacement rates in our sample is not sufficiently disperse (see Figure 2) to allow nonparametric estimates of the benefit response. We simply note that our (linear) estimates should of the response should be interpreted as applying in the neighborhood of the mean replacement rate in the data (48%).

Next we consider quits. The disentitlement of quitters in 1993 provides an additional source of variation in benefits but to the extent that quits are more voluntary than other job separations one might be concerned that their presence in the sample is endogenously determined by their entitlement. However, as row 5 (experiment 4) of Table 5 illustrates, excluding quits from our sample has a negligible impact on the results.

The next set of robustness checks are somewhat different. It is plausible that those who are most sensitive to benefit variations are those who experience the largest fall in expenditure. To check this, we ran three quantile regressions - for the median, first quartile and first decile respectively. As can be seen from Table 5, expenditure for the three percentiles fall by 3.1%, 25% and 50% respectively. The parameter estimates given speak for themselves. The effect of the benefit variables is insignificant for the median group whereas it is much larger for those who experience a large fall. Indeed, taking the parameter estimate for the first decile, (and again, assuming 'importance' equal to one and focusing on the households in the intersection of our two “sensitive” groups) a reduction in the replacement rate from 60% to 50% would increase the expenditure fall by almost 8 percentage points. Our interpretation of these quantile regression results is that some agents are liquidity constrained; they have large falls in expenditure and show a large sensitivity to benefit changes whereas most other agents are not affected by marginal changes in the benefit level and have smaller falls in expenditure.

Finally, we consider issues of endogeneity and sample selection. The validity of our interpretation of the coefficients on the benefit variables rests on the latter reflecting only transitory changes and being uncorrelated with the permanent shock of job loss. Furthermore, our sample is selected on having a unemployment spell of at least 6 months. We have controlled for various correlates of the permanent shock associated with a job loss, such as being a seasonal worker or having high income. In addition to absorbing the permanent shock, this rich set of covariates should also control for many of the differences between our selected sample and separations as a whole. We tested the adequacy of these controls with regard to these concerns. To do this, we estimated auxiliary regressions for the benefit variable and for selection into our sample. We then 'stuffed' the residuals from these auxiliary regressions into our basic specification (4) for the proportional change in total expenditure. Tests for the exclusion of these residuals are tests for the endogeneity of the benefit variable and sample selectivity.[13]

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[13]This is an alternative form of the familiar Durbin-Wu-Hausman test. Our two auxiliary equations are identified by two instruments. The first is the statutory replacement rate, which takes on values 0.6, 0.57, 0.55 and 0 (for the ineligible). The variation in this variable is driven entirely by the legislative reforms captured by our data (including changes in eligibility requirements). Our second instrument is the weeks elapsed between the separation and the interview date. This varies between 15 and 45 weeks, (with 90% between 24 and 40 weeks) due to the time required to conduct a survey of this size. Under the null of full smoothing, and assuming that the permanent shock of the unemployment spell is fully revealed in the first 15 weeks, this variable can be excluded from the expenditure change regression. Beginning with the auxiliary equations we see that our instruments are significant determinants, even conditional on our full set of covariates, of both the benefit variable and inclusion in our sample. Interestingly, only the weeks elapsed appears to have a significant effect on inclusion in the sample. Thus we do not detect here the impact of benefit levels on spell duration reported in the literature. Complete results are available from the authors.

In neither case was the exclusion rejected by the data. We take this to indicate that our rich set of covariates adequately control both for the potential correlation of benefits with the permanent shock of job loss and for sample selection.

The bottom line for these robustness checks is that the basic result (see specification 8 in Table 4) seems to be robust to many changes in the empirical specification. Our results are not being driven by a small number of outliers and they seem to be robust to changes in the specification of the importance variable and the replacement rate. On the other hand, there is considerable evidence that the mean effect given seems to be the result of a large effect for a few people rather than a smaller effect for everyone.

6. Conclusions.

We have presented results based on a survey of workers who became unemployed in early 1993 or 1995. The surveys and the associated administrative and tax data give us an unprecedented glimpse into the circumstances of households in which one member suffers a job loss. In this paper we have concentrated on the impact of a six month spell of unemployment on household expenditures. In section 3 we presented some descriptive statistics for the impact of a job loss on total expenditure. As seen from Figure 2 there is considerable variation in the impact. Some households halved their total expenditures while others actually reported an increase. The modal response was zero but much of this represents rounding to zero. The mean fall was about 14%. A main finding regarding these changes in expenditure is that households in which the income from the lost job was a major source of household income have significantly larger falls in consumption. We also saw that households that had higher fixed outgoings for rent or mortgage had lower expenditure falls. There were also other significant differences across the sample (see Table 3). Some of these differences reflect the permanent shock of the job loss and the consequent revision downward of lifetime income. They may also, however, be driven by the transitory loss of income that follows a job loss. A major factor in the latter is the level of income replacement that UI benefits represent.

The impact of UI provisions on living standards during a spell of unemployment is a critical factor in the design of a UI system. As well as the direct concern with living standards, a large component of the effect of UI benefits on job search, unemployment spell duration and the quality of a new job run through this channel. We have presented an attempt to identify the size of this effect. As we have seen, the empirical analysis is complicated by the fact that cross-section differences in the replacement rate confound differences in the transitory impact of an income loss and the permanent shock from a job loss. To deal with this confounding we controlled for correlates of the permanent shock and tested for the validity of our identifying assumption using variation generated by legislative changes to the UI. Our results indicate that we do indeed have an adequate set of permanent controls. Our findings suggest only very modest impacts of benefit levels on household expenditures and that for only some households. In particular, it seems that households that had no liquid assets at the job loss were sensitive to variations in transitory income. Thus these findings support the hypothesis that some households are liquidity constrained.

In the work similar to that reported here, Gruber (1997) examines the impact of UI benefits on food expenditure changes using the PSID. Gruber's estimates suggest that a 10 % point cut in the UI replacement rate would lead to an average fall of about 2.5% in food expenditures. Our findings suggest that the same cut would result in average fall of less than 1% in total expenditure. The interpretation of Gruber's results - and the comparison of those with the results reported here - depends heavily on the relationship between changes in food expenditure and changes in total expenditure. Gruber argues that the response of food and total expenditure should be the same. However, budget studies reject the idea that preferences are homothetic. Food, a necessity, is routinely estimated to have an income (total expenditure) elasticity of less than 0.5. Hamermesh (1982) does in fact report that unemployment affects the structure of demands with households cutting back more on luxuries (goods with income elasticities greater than one) during an unemployment spell.

In a companion paper (Browning and Crossley (1998)) we develop the idea that agents have access to 'internal capital markets' by postponing the purchase of durables during an unemployment spell. Although there is a welfare cost from not replacing a (functioning) durable at the optimal time,[14] this is of second order importance. For example, the service flow from an old undamaged winter coat is almost as great as that from a new one. If this is the case, then large changes in durable expenditures may not be reflected in large changes in service flows and hence welfare. One corollary of this 'internal capital markets' hypothesis is that durables expenditures will be much more volatile over the business cycle than non-durable expenditures, which is exactly what we observe in the aggregate data. Because most small durables are luxuries, this 'internal capital markets' hypothesis has qualitative predictions that are quite similar to the Hamermesh thesis. The rationale of the two hypotheses are, however, quite different. Furthermore, the 'internal capital markets' hypothesis suggests that the responses of expenditure on durables to the income loss of unemployment will be *larger* than implied by income elasticities estimated on samples of households experiencing 'normal times'. In Browning and Crossley (1998) we present strong evidence for this contention. This further suggests that changes in food expenditure may be an even smaller fraction of changes in total expenditure than typical food-income elasticities would suggest.

Gruber (1997) finds in the PSID that households experience a 6% fall in food expenditures with unemployment. If the elasticity of food with respect to total expenditure is about 0.4 (as suggested by budget studies) then this suggests about a 15% fall in total expenditure which agrees very closely with our estimate. Turning to the benefit response, if one accepts Gruber's hypothesis that food and total expenditure responses are roughly similar, then the response that Gruber estimates is 2.5 times as large as the one we find. However, if one rejects this notion and scales the Gruber's estimated food response by a typical food-total expenditure elasticity then one gets a total expenditure response that is more than 6 times as large as the response we find. If the

[14] That is, optimal for an agent who is not liquidity constrained.

‘internal capital markets hypothesis’ is correct and household maintain food expenditures by delaying the replacement of small durables, the disparity may be even greater.

Our analysis differs from Gruber’s study in several additional respects. We employ a different source of variation in UI benefits to identify the transitory response. To address the question of the impact of UI benefits, Gruber uses state level provisions which vary over both time and across states. In Canada, UI benefits are set nationally, and vary only through time. However, our data capture two major sets of legislative changes to the Canadian UI system, as well as some administrative changes. While not as extensive as the variation captured by Gruber’s data, the variation in the Canadian program parameters is substantial and transparent. One concern with Gruber’s approach is that it identifies the (temporary) 'benefit' effect only if state level variables are uncorrelated with permanent shocks from a job separation. For example, if states have to balance their UI accounts then benefit levels will be lower the worse is the unemployment situation. If the latter is also correlated with a larger negative shock from a job loss then part of the effect in Gruber’s base estimates could be due to the negative correlation between benefit levels and the permanent shock. If in a regression of expenditure changes on benefit levels there is a positive coefficient on the latter then this may be partly due to the direct benefit effect (due to liquidity constraints) and partly due to the negative correlation between job separation shocks and expenditure changes. However, Gruber’s results seem robust to attempts to deal with this issue, including conditioning on state fixed effects.

It is also possible that the differences in our results arise because of differences in the population of unemployed between Canada and the US, or in differences in the smoothing mechanisms available to the unemployed in the two countries. Given the expenditures on unemployment insurance in western economies, and the relative lack of empirical research on the *benefits* of unemployment insurance, estimates on various samples and using alternative sources of variation in benefit levels would seem important. More research will be required to further determine when - and for whom - unemployment insurance is beneficial.

This raises another new element of the current study - the specific focus on households which are likely to be constrained. Compared to our average response, we do find a larger effect of UI benefits for those households without liquid assets at the separation date, and no response for those with assets. This supports the hypothesis that some but not all households are liquidity constrained, and thus some but not all households are sensitive to the level of income replacement provided by unemployment insurance. This is an important result. For example, Gruber combines his estimates with the framework of Bailey (1978) to calculate optimal levels of UI provision under alternative assumptions about risk aversion. However, Bailey’s framework does not accommodate the type of heterogeneity we find strong evidence for. If the benefits of unemployment insurance differ across households then the social benefit of the program depends on the weights society ascribes to different households.

Our principal focus has been on expenditures. The link between this and living standards (here taken to mean the purchases of non-durables and services, the service flow from durables and housing) is a complicated one. For example, the mean 14% fall in expenditures seen in our sample could all be for durables, clothing and costs of working. If small durables and clothing depreciate only slowly, then households will maintain living standards (or 'smooth consumption') even over a relatively long unemployment spell. If this is the case, then it seems that marginal changes in UI benefit levels may have effects on living standards during an unemployment spell that are even smaller than the total expenditure responses reported here. In that case, the unemployment insurance must be justified on the basis of other potential benefits, for example, a reduction in precautionary saving or a redistributive function.

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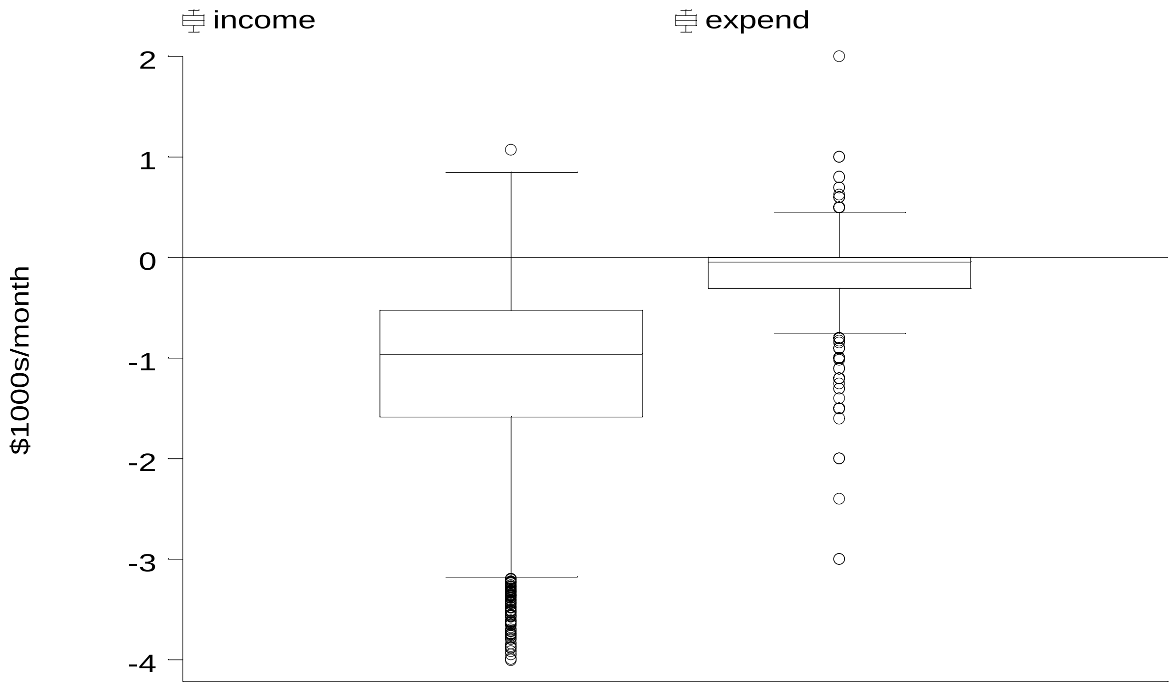

Figure1: Income Loss and Expenditure Change

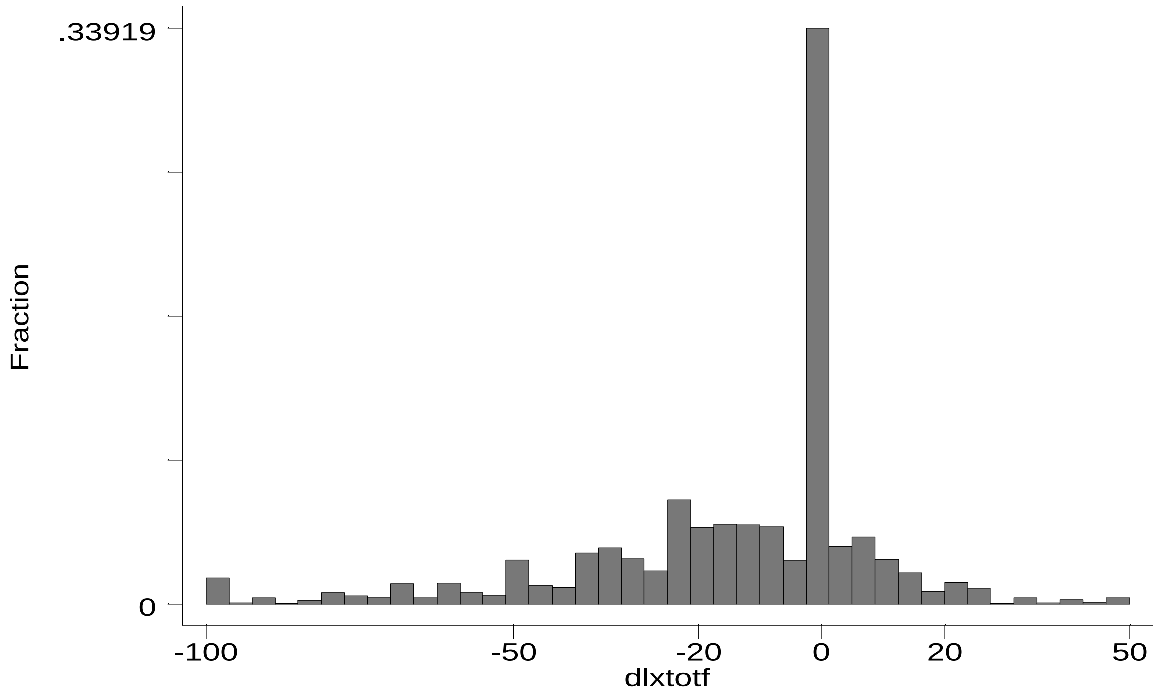

Figure 2: Proportional Change in Expenditure

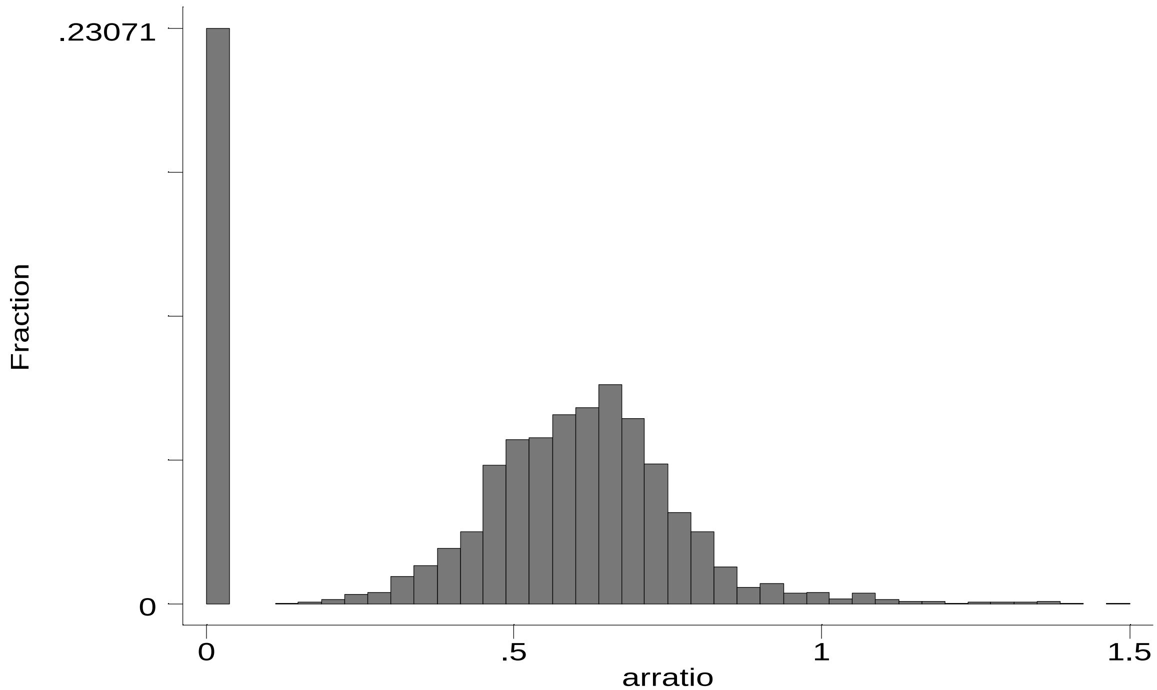

Figure 3: Ratio of Benefit to Past Earnings

TABLE 1: Means of $\frac{C_{t+1} - C_t}{C_{t+1}}$ for Discrete Variables

Variable	Number Non Missing	Means	T-Value
Female	1336	-12.2	-----
Male	1282	-15.2	-3.1
Irregular UI User	1025	-15.8	-----
Regular UI User	1448	-11.7	4.2
Non-seasonal Job	2219	-14.5	-----
Seasonal Job	399	-9.2	4.0
Short Tenure Job	1172	-11.7	-----
Long Tenure Job	1444	-15.3	-3.7
Renter	1148	-16.7	-----
Home Owner	1470	-11.3	5.6
Married, Spouse Employed	1252	-10.7	-----
Married Spouse Not Employed	585	-14.1	-2.7
Single	486	-19.3	-6.6
Other Household	251	-17.4	-4.0
No Assets At Job Loss	1653	-13.5	-----
Some Assets At Job Loss	835	-13.5	-0.1
Job Loss Unexpected	1194	-14.7	-----
Job Loss Expected	1424	-12.8	2.0
White Collar	1062	-12.8	-----
Blue Collar	747	-12.4	0.3
Manager	597	-17.3	-3.6
Less Than High School	575	-11.9	-----
High School	1010	-13.0	-0.9
More Than High School	755	-16.6	-3.8
Atlantic Region	286	- 6.2	5.1
Quebec	751	-14.9	-0.2
Ontario	891	-14.6	-----
Prairies	356	-13.7	-0.6
British Columbia	288	-15.9	-0.8

TABLE 2: Univariate Regressions of $\frac{C_{t+1} - C_t}{C_{t+1}}$ on Continuous Variables

Variable	Number Non Missing	Coefficient [T-Value]
Change in Spouse's Hours	2533	-0.015 [-0.3]
Local Unemployment Rate	2618	29.6 [2.5]
Ln(Earnings on Lost Job)	2618	-5.5 [-6.3]
Ln(Earnings Last Year)	2618	-5.3 [-7.0]
Importance of Income for Household	2547	-14.4 [-8.8]
Age (In Decades)	2618	0.85 [1.7]
% Of Income Committed	2215	-2.0 [-0.9]
Ln(Household Size)	2618	4.7 [5.4]
Weeks of Work In Year Before Job Loss	2612	-1.7 [-6.3]

TABLE 3: Multivariate Regressions					
Specification	1	2	3	4	5
Variable	Coeff. [t-stat]	Coeff. [t-stat]	Coeff. [t-stat]	Coeff. [t-stat]	Coeff. [t-stat]
Importance of income for household	-10.86 [-3.59]	-10.29 [-4.11]	-11.35 [-3.11]	-10.56 [-2.76]	-10.64 [-3.52]
Importance X Adjusted replacement rate		10.17 [2.21]	11.03 [2.08]	8.15 [1.23]	7.99 [1.84]
Importance X (replacement rate = 0)		4.50 [1.30]	1.13 [0.26]	0.15 [0.03]	
Highschool	-0.26 [-0.19]		-0.24 [-0.18]	-0.13 [-0.09]	-0.13 [-0.09]
College	-0.40 [-0.24]		-0.72 [-0.44]	-0.39 [-0.24]	-0.39 [-0.24]
Age	2.03 [3.10]		1.88 [2.90]	2.10 [3.18]	2.10 [3.19]
Ln(household size)	-0.81 [-0.43]		-1.20 [-0.64]	-1.10 [-0.59]	-1.10 [-0.59]
Male	-0.21 [-0.15]		-0.70 [-0.50]	-0.03 [-0.02]	-0.03 [-0.02]
Married, spouse not employed	-2.41 [-1.58]		-1.79 [-1.17]	-2.03 [-1.31]	-2.03 [-1.32]
Single	-4.27 [-1.48]		-2.84 [-0.99]	-3.49 [-1.19]	-3.49 [-1.19]
Other household	-5.19 [-2.43]		-4.31 [-2.02]	-4.69 [-2.18]	-4.68 [-2.18]
Atlantic	6.66 [3.11]		7.24 [3.43]	6.95 [3.24]	6.95 [3.25]
Quebec	0.57 [0.37]		0.94 [0.61]	0.72 [0.46]	0.72 [0.46]
Prairies	1.88 [1.04]		2.24 [1.25]	2.14 [1.19]	2.14 [1.19]
BC	-1.22 [-0.60]		-1.03 [-0.51]	-1.11 [-0.54]	-1.10 [-0.54]
Investment income in previous tax year	0.18 [0.14]		-0.02 [-0.02]	0.23 [0.18]	0.23 [0.18]
Home owner	3.35 [2.45]		2.78 [2.04]	3.12 [2.27]	3.12 [2.27]

TABLE 3: Multivariate Regressions

% of household income committed	7.36		8.77	8.41	8.42
	[2.92]		[3.45]	[3.25]	[3.26]
Expected job loss	1.09		1.34	1.24	1.24
	[0.88]		[1.08]	[1.00]	[1.00]
Seasonal job	1.39		1.69	1.40	1.40
	[0.79]		[0.97]	[0.80]	[0.80]
Long tenure Job	-0.24		-0.78	-0.08	-0.07
	[-0.17]		[-0.59]	[-0.05]	[-0.05]
Manager	-2.03		-2.53	-2.13	-2.13
	[-1.34]		[-1.70]	[-1.40]	[-1.40]
Blue collar	1.14		1.01	1.16	1.16
	[0.73]		[0.65]	[0.75]	[0.75]
UI use in 1 of 2 previous tax years	1.69		1.77	1.46	1.45
	[1.29]		[1.36]	[1.10]	[1.10]
Weeks of work in year before job loss	-0.09		-0.10	-0.09	-0.09
	[-2.09]		[-2.48]	[-2.19]	[-2.20]
Local unemployment rate	20.90		20.79	18.77	18.75
	[1.25]		[1.24]	[1.12]	[1.12]
Ln(earnings last year)	0.69			0.16	0.16
	[0.40]			[0.09]	[0.09]
Ln(earnings 2 years previous)	-2.36			-2.45	-2.45
	[-1.74]			[-1.80]	[-1.80]
Ln(earnings on lost job)	-0.71			0.71	0.70
	[-0.35]			[0.32]	[0.32]
Ln(earnings on lost job) squared	-0.83			-0.25	-0.26
	[-0.68]			[-0.20]	[-0.21]
Ln(earnings on lost job) cubed	-0.68			-0.63	-0.63
	[-0.88]			[-0.81]	[-0.81]
Constant	-7.33	-4.18	-5.60	-5.22	-5.20
	[-1.79]	[-3.41]	[-1.38]	[-1.22]	[-1.22]
observations	1805	2547	1805	1805	1805
R- squared	0.09	0.03	0.08	0.09	0.09

Estimated equations are of the form: $$\frac{c_{t+1} - c_t}{c_{t+1}} = Xb + (a_{t+1} - 1) * impyq + e$$

where X are controls a_{t+1} is the actual replacement rate and *impy* is the importance of the respondents pre-separation earnings to household income.

TABLE 4: Multivariate Regressions - Alternative Specifications				
Specification	5	6	7	8
Variable	Coeff. [t-stat]	Coeff. [t-stat]	Coeff. [t-stat]	Coeff. [t-stat]
Adjusted Replacement Ratio	7.989 [1.844]	-0.948 [-0.158]		
Adjusted Replacement Rate Crossed With No Assets at Job Loss		13.396 [2.142]	12.711 [2.824]	12.999 [2.892]
Adjusted Replacement Ratio Crossed With Not Eligible for Social Assistance Crossed With Spouse Not Employed				15.723 [2.671]
N R - square	1805 0.09	1805 0.09	1805 0.09	1805 0.09
Implied % change in total expenditure from a 10 point cut in the replacement rate for *impy = 1*.	-0.8%	-1.3%	-1.3%	-2.9%

Estimated equations are of the form:

$$\frac{c_{t+1} - c_t}{c_{t+1}} = Xb + (a_{t+1} - 1) * impy * Zq + e$$

where *X* are controls a_{t+1} is the actual replacement rate, *impy* is the importance of the respondents pre-separation earnings to household income, and Z are indicators of potential liquidity constraints. Only the parameters θ are reported. Full results are available from the authors. The implied changes in total expenditure are calculated as for the most affected group: for those without assets in specifications 6 and 7, and for those with nonworking spouses, without assets at job loss and ineligible for social assistance. In each case the calculations are made under the assumption that the lost earnings were 100% of household earnings (*impy = 1*).

TABLE 5: Multivariate Regressions - Robustness Checks			
Experiment	Size	Coefficient [t-stat]	
		Adjusted Replacement Rate	
		X No Assets at Separation	X Non Working Spouse X Not Eligible For Social Assistance
Preferred. (Specification 8, Table 5)	1805	12.999 [2.892]	15.723 [2.671]
Max(abs(DFbeta))		0.38	0.25
1. Importance ≤1.	1423	12.176 [2.544]	14.749 [2.243]
2. Replacement Ratio ≤1.	1772	16.510 [3.487]	15.692 [2.621]
3. Receiving UI benefit.	1407	17.506 [2.708]	22.393 [2.983]
4. Exclude Quits	1490	13.209 [2.670]	14.597 [2.302]
5. Median Regression. (Median =--3.1%)	1805	5.269 [0.952]	4.533 [0.562]
6. 25% Regression. (First Quartile = -25%)	1805	15.860 [2.211]	26.851 [1.929]
7. 10% Regression. (First Decile = -50%)	1805	34.758 [2.750]	44.129 [2.784]
Alternate estimates of specification 8 in Table 4. Rows 1 through 4 differ in the estimating sample. Rows 5 through 7 report quantile, rather than least squares regressions.			

TABLE A1: Variable Means, Ranges and Notes			
Variable	Mean	Range	Notes
Highschool College	0.39 0.29	0,1 0,1	Less than highschool is the omitted category
Age	-0.23	0,1	In decades from 40.
Ln(household size)	0.94	0,1	
Male	0.49	0,1	
Married, spouse not employed Single Other household	0.23 0.19 0.10	0,1 0,1 0,1	Married, spouse employed at job loss is the omitted category.
Atlantic Quebec Prairies BC	0.11 0.29 0.14 0.11	0,1 0,1 0,1 0,1	Ontario is the omitted category
Assets at Job Loss	0.34	0,1	Investment income in previous tax year
Home owner	0.56	0,1	
% of household income committed	0.32	0,2	(mortgage payment or rent)/ household income at interview
Expected job loss	0.54	0,1	
Seasonal job	0.15	0,1	
Long tenure Job	0.55	0,1	
Manager Blue collar	0.25 0.31	0,1 0,1	White collar, non-manager is the omitted category
UI use in 1 of 2 previous years	0.59	0,1	
Weeks of work in year before job loss	30.5	1,52	
Local unemployment rate	0.11	0,0.27	
Ln(earnings last year) Ln(earnings 2 years previous)	1.05 0.97	-0.05,3.6 -0.03,3.2	\$1000s/month
Ln(earnings on lost job)	0.42	-2.8,2.2	\$1000/month

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