Unemployment Insurance Benefit Levels and Consumption Changes.

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Abstract

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 Unemployment Insurance.

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 16% 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.

The effects of varying the replacement ratio are relatively small. We only find effects for those who did not have assets at the job loss and 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 no effect on living standards within the household and hence on other facets of behaviour such as job search, unemployment duration and the quality of any new job taken.

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1. Introduction

Unemployment Insurance (UI) systems have a number of functions. They redistribute lifetime income from people who rarely experience an unemployment spell to those who have frequent spells. They may also act as automatic stabilisers if the scheme is funded in such a way that net payments are positive in a recession and negative in a boom. Primarily, however, they are designed to provide insurance against the income loss that results from a job separation. This is desirable for at least two reasons. First, it obviates the need for excessive precautionary saving by workers faced with the otherwise uninsurable risk of a job loss. Second, if unemployed agents are liquidity constrained during an unemployment spell then they may curtail search and take a job which has an inferior match relative to the socially optimal match. The design of any UI system has to reflect these different goals and also the well known incentive costs that the existence of a UI scheme gives rise to.

One of the most important parameters in any UI system is the level of benefits paid. 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 marginal changes in UI benefit levels will have no impact on any of these outcomes. In this case, the only welfare impact of a decrease in benefits will be that households will 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 (see Browning and Lusardi (1995) who list about 25 such studies). This debate has been remarkably inconclusive. There are just about as many studies that find no evidence of excess sensitivity (that is, a correlation between expenditure changes and predicted income changes) as do. 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.

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. Typically income shocks from year to year in, say, the PSID are not very dramatic so that it is difficult for the econometrician to predict them, particularly given the noisiness of the income measure. Thus auxiliary equations that estimate income growth to isolate expected and unexpected components have very low R^2's (of the order of 1%) which surely underestimates dramatically how well agents themselves predict changes. Thus the expected income variable that is included in consumption Euler equations is a poor proxy for actual expected income changes and the tests of excess sensitivity have low power.

In this paper we use a sample of about 11,000 Canadians who experienced a job separation in early 1993. The reasoning is that if any segment of the population is likely to be liquidity constrained it is the unemployed. There are two strands to this reasoning. First, 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. The second strand is that 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. If this reasoning is correct, then the sample we have offers a potentially more powerful test for liquidity constraints than tests on conventional data.

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. For example, the administrative data gives the exact benefit and entitlement period for every respondent; if these are measured at all in surveys they are usually measured with a good deal of error. As another example, the tax data allow us to control for the labour supply and income of the spouse which is typically missing from administrative data. Finally, the survey provides information concerning expenditures that is never observed in administrative or tax data. Moreover, we often have two or three independent measures of the same thing from our three sources (an example is past earnings)

which allows us the potential to correct for measurement error.

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. The main econometric problem we have to address 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 controls for the permanent shock and also instruments for the replacement ratio. These instruments are correlated with the temporary loss of income but not with the permanent shock. The richness of our data set provides us with both permanent controls and instruments for our examination of the impact of short run income losses on expenditures.

Two recent studies have also examined the effects of UI benefits on expenditures. Gruber (1994) presents a study of the impact of UI benefits on food expenditure changes using the PSID. On the 'raw' differences, he finds that households experience a 6% fall in food expenditures. 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 is very close to our estimates presented below. To address the question of the impact of UI benefits, Gruber uses state level provisions which vary over both time and across states. The 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. This identifies the (temporary) 'benefit' effect if state level variables are uncorrelated with permanent shocks from a job separation. This seems unlikely. 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 that Gruber estimates is 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.

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)

and Lundberg (1985)). This paper and Gruber (1995) are mainly concerned with the overall link from benefits to 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. Their conclusion is unequivocal: "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.

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. Specifically, in section 4 we provide a description of the impact of a six month unemployment spell and its correlation with a wide variety of household and labour market factors. The average fall in expenditure is about 16%; 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. There are two reasons why expenditure changes around a job loss may not reflect changes in living standards. The first is that we are comparing two different labour supply regimes. 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. The second break in the link between expenditures and living standards is because total expenditure is for both durables and non-durables. In a companion paper (Browning and Crossley (1995)) 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[1] this is of second order importance. For example, the service flow from an old undamaged

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

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.

In this paper we concentrate on *changes* in total expenditure; thus we do not address the validity of the 'internal capital markets' hypothesis. It should be kept in mind, however, that we must not equate changes in expenditure with changes in welfare levels. It would be most desirable to be able to examine the changes in different components of expenditure such as housing; food at home; food outside the home and clothing. Although we have information on the levels of these expenditures on components of total expenditure we do not have measures of the changes. In the companion paper referred to above we use indirect methods to infer the effect of benefit levels on the components of expenditure.

This 'internal capital markets' hypothesis is similar to the Hamermesh thesis (see Hamermesh (1982)) that unemployment affects the structure of demands but with the crucial difference that it is durables and semi-durables that are cut back on during an unemployment spell rather than luxuries. Of course, since durables and semi-durables tend to be luxuries (in the sense of having income elasticities. above one) the two hypotheses will lead to similar predictions concerning demand patterns during an unemployment spell. The rationale of the two hypotheses are. however, quite different.

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 5% cut in benefit levels would lead to an average fall of only 0.3% 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 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.

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

$$\lambda_{t+1} = \lambda_t - u_{t+1} \tag{2.1}$$

where u_{t+1} is a 'surprise' error term[2] 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 information that arrives at the end of period t. Thus:

$$E\left(u_{t+1} \mid I_t\right) = 0 \tag{2.2}$$

(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 formalise this, let the realised shock be $\Delta_L + \varepsilon_{t+1}$ if the agent loses their job between periods t and $t+1$ and $\Delta_E + \varepsilon_{t+1}$ otherwise. Thus Δ_L represents the 'permanent' shock (the revision to the marginal utility of expenditure) from a job loss. The residual term ε_{t+1} captures all of the impact of news except for that concerning any job loss. Thus $E\left(\varepsilon_{t+1} \mid I_t\right) = 0$. If π_t is the probability of a job loss between t and $t+1$, given the information available at time t, then latter equation and equation 2.2 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, $\pi_t = 0$ or 1). Second, the two shocks have opposite

[2]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.

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 (2.2):

$$E\left(u_{t+1} \mid I_t, \text{Job Loss}\right) = \Delta_L \tag{2.3}$$

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 $\mathbf{Z}_t$ where the t subscript emphasises 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 $a_{t+1} = \frac{B_{t+1}}{Y_t}$. 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:

$$\Delta_L = f\left(\mathbf{Z}_t, \mu\left(A_{t+1}, a_{t+1}\right)\right) \tag{2.4}$$

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 $f(\mathbf{Z}, \mu)$ in equation 2.4 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 2.1 , 2.3 and 2.4 we have the revised Euler equation:

$$\Delta\lambda_{t+1} = -f\left(\mathbf{Z}_t, \mu\left(A_{t+1}, a_{t+1}\right)\right) - \varepsilon_{t+1} \tag{2.5}$$

If we parameterise preferences (that is, define λ in terms of consumption) and the functions $f(.)$ and $\mu(.)$ 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 (1995) for a recent discussion). Here, however, we are conditioning on past levels of $\mathbf{Z}$, so that we only need to invoke the much weaker assumption that, conditional on the $\mathbf{Z}$ variables, any common macro shock has the same effect on all agents.

It is important to control for the permanent shock variables $\mathbf{Z}_t$. 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 labour 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 labour 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 2.5. 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 $\mathbf{Z}_t$. 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 features of the Canadian UI system, we leave the details for the empirical section but we emphasise

here that we do test the identifying assumption.

To complete the model we need to parameterise preferences and the functions in equation 2.5. Denoting consumption in period t as C_t we take the following (Frisch or λ-constant) equation for log consumption in period t:

$$\ln C_t = \beta_t + \delta\lambda_t \tag{2.6}$$

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[3]. 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 $\mathbf{Z}$ and a_{t+1}[4]:

$$\ln C_{t+1} - \ln C_t = (\beta_{t+1} - \beta_t) + \mathbf{Z}_t'\alpha + \eta a_{t+1} + \varepsilon_{t+1} \tag{2.8}$$

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 η 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

[3]The utility function asscoiated with 2.6 is:

$$\nu^t(C_t) = \frac{1}{\delta}(C_t(\ln C_t - 1 - \beta_t)) \tag{2.7}$$

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'.

[4]In our empirical work we experimented with using a spline function for the replacement ratio to capture the fact that the effect of this variable is likely to be zero if the replacement ratio is close to unity. Although this gave slightly 'sharper' results the qualitative results were very similar to those using the linear form (since very few agents had a replacement rate close to unity). For the sake of presentational simplicity we choose to just present only the linear form.

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 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. Thus 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:

$$\rho = (a_{t+1} - 1) * (\text{importance of respondent's pre-separation earnings}) \qquad (2.9)$$

(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.

In our empirical work we shall take account of several other institutional features but these are best presented after we have looked at the data. It is to this that we now turn.

[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.

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 benefit of $447 per week[6]. All those who had a job separation and who met the entitlement qualification were eligible for UI but 'quitters' were penalised by a 7 to 12 week waiting period.

On April 4 1993 changes were made to the UI system. 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 disentitled. 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. 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.

In this paper we use only information from the first interview. In this first wave a wide range of questions were asked including questions on the pre-separation job; labour 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 1992 and 1993) and UI administrative data for each respondent for 1987 to 1993.

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

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 $t + 1$[7]. This variable is the 'left hand side' variable of this paper.

As to sample selection, we began with a sample of 5,163 respondents who were unemployed at the first interview. The next selection is on still being in the first spell of unemployment; this left 3,473 respondents. Thus all those in our sample have been continuously unemployed for about half a year. We then selected those 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 quit to take another job and those on maternity leave. This left 3,290 respondents. We also drop respondents

[7]This is slightly different from the usual construction which divides by the lagged level.

who had an outstanding UI claim from an earlier spell of unemployment since their 'reference' job loss may not be the one that selected them into our sample. This left a sample of 2,709. We then dropped 628 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 (16 and 30 observations respectively). Finally we excluded two respondents for whom we did not have information on their current UI benefit and one 'outlier'[8]. This left a working sub-sample of 2,032 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, $\Delta \ln X_{t+1}$ and its correlation with other variables. The distribution of the proportional change in total expenditure is given in Figure 1. 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 (54.8% , with 29.9% reporting 'no change'); the mean fall is -15.4%.

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-centred Normal distribution. The mean of the distribution taking into account this rounding is -16.0% and the predicted number of zeros is 17.7%. Thus, conditional on our rounding assumption, some, but not all of the mass at zero can be attributed to rounding.

[8]The 'outlier' was a married respondent with a child who had been the sole breadwinner in his household. He reported a *doubling* of expenditures despite the fact that he did not receive any UI benefit and the household had zero current income.

As for the remaining 12.2% (= 29.9% − 17.7%) 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 behaviour 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[9]. 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

[9]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'.

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 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 and subsequent analysis, far and away 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 17 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 colinearity 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 (2.9). 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 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. Because of the errors in the self-reported earnings figure a small number of respondents (12 out of 2043) were imputed replacement ratios of over 150%. We set these to their administrative replacement ratios (60% or 57% depending on the date of the job separation). In Figure 2 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.

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 $-16.4 + 10.2 * (-0.4) = -20.5\%$. If the household has a replacement ratio of 50% the fall is -21.5%. This is a very small effect for such a large change; since a cut in the replacement rate from 60% to 50% represents a cut in the benefit paid of 18.3% this gives an elasticity of consumption with respect to benefit of 0.06.

If the household did not receive benefit because of being disentitled then the predicted fall is -20.3%. Once again we emphasise 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 be unchanged. 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 3% of total expenditure.

The estimates in column 2 of Table 3 do not have any controls for permanent variables other than 'importance'. In specification 3 of Table 3 we give the estimates controlling for permanent variables. 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 all that follows we drop the zero benefit variable. The other feature of these estimates is that the coefficient on the adjusted replacement ratio is virtually unchanged.

In specification 4 of Table 3 we drop the 'no benefit' variable and give our best estimate of the benefit effect with no allowance for different effects across different groups. We regard allowance for the latter as most important and we shall present these below. Once again, the parameter estimate indicates a small effect. We now consider two further extensions. The first is to test for the exogeneity of the benefit variable. The second is to see whether it varies significantly across different groups. Following these sub-sections we investigate the robustness of our findings.

5.2. Testing for exogeneity.

The validity of our interpretation of the coefficient on the benefit variable rests on the latter reflecting only transitory changes. 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. We now turn to testing for whether these controls are adequate by instrumenting the adjusted replacement ratio variable and testing for the endogeneity of this variable. To do this we employ two sets of instruments. The first set is derived from the quasi-experimental survey design. The second set exploits non-linearities in the administration of the UI program.

As we noted above, the survey was designed to have half of the sample coming from before a change in the UI program (cohort 1) and half from after (cohort 2). The change reduced the replacement ratio and disentitled quitters. Thus we

construct dummies for 'cohort 1 quitter', 'cohort 2 quitter' and 'cohort 2 non-quitter'. These should be correlated with the replacement ratio; the identifying assumption is that they are uncorrelated with permanent effects once we control for other permanent effects.

The first of the second set of instruments (the 'administrative' set)is a dummy for being eligible for UI benefits. This is constructed from the UI administrative data. Eligibility depends on the number of weeks worked in the year before the job loss and the local unemployment rate. Since the variables in the regression include measures of labour force attachment (the number of weeks of work in the year before the job separation; the seasonal job dummy; a 'long tenure' dummy for having had the lost job for more than 52 weeks and a dummy for use of the UI program for three years in the past five) and the local unemployment rate the exact administrative eligibility variable should not be correlated with the permanent job loss shock but it, of course, correlated with the replacement ratio.

The other administrative instrument we use is a dummy variable for having pre-job loss monthly earnings of more than $3,203. This is the 'maximum insurable earnings' so that respondents above this rate have a replacement ratio below the administrative rate but those below have a rate equal to the administrative rate. Thus this dummy variable is certainly correlated with the replacement rate used in our analysis. It should also be uncorrelated with the job loss shock since we include the log, log squared and log cubed of the lost job earnings (and the log of the previous year's earnings). Given the latter controls, we have no reason to believe that being above or below a particular administrative level of earnings should be correlated with the job loss shock.

One immediate concern is that since the instruments we have are related to the included permanent variables they do not add much to the prediction of the adjusted replacement ratio in the auxiliary equation. This is not the case. The quasi-experimental instruments have an $F(3, 1462)$ statistic of 2.8 and the two administrative instruments have an $F(2, 1462)$ statistic of 50.8. Thus the instruments are satisfactory in the sense of having some (partial) correlation with the possibly endogenous variable; this is particularly the case for the administrative instruments. The $\chi^2(4)$ statistic for the over-identifying restrictions is 5.3 (probability = 25%). Thus the instruments can be excluded from the consumption change equation. Taking this to indicate that the instruments we have are valid, we then tested for the exogeneity of the adjusted replacement ratio variable by adding the residual from the auxiliary equation to the consumption change

equation[10]. The t-value on this residual is 0.80 which suggests that the adjusted replacement ratio can be taken to be exogenous; that is, the benefit variable reflects only a transitory effect. In all that follows we take the adjusted replacement ratio to be exogenous and we revert to using OLS.

5.3. Who is constrained?

Above we simply included the adjusted replacement ratio on the right hand side of the consumption change equation. The significance of this variable indicates to us that some agents are liquidity constrained and their current consumption depends on the current benefit level. In this sub-section we identify more precisely who is constrained. 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 first group are the obvious candidates: those with no liquid assets at the job separation. The next group are 'renters'; the rationale here is that homeowners can perhaps borrow against their home equity[11]. The next group of candidate variables concern household structure and the employment status of other household members. We take as the reference group those who are married and whose spouse was employed at the time of the job separation. The idea here is that this group may be able to borrow against the spouse's earnings. The three groups who might be constrained are: 'married with spouse not employed at job separation'; 'single' and 'other'[12]. Finally, we crossed 'age' with the benefit variable. There is strong evidence that older households have higher wealth so that it may be that older households are less likely to be constrained. Note that the levels of all these variables are also included in the regression.

We also used one other variable to capture the possibility that fluctuations in the benefit level have permanent effects for those who use the UI system on a regular basis. If future UI receipts are a significant proportion of lifetime wealth then variations in benefit levels may induce changes in consumption even if the household is not liquidity constrained. To check for this we cross the benefit variable with the dummy for being a regular UI user.

[10]This is an alternative form of the familiar Durbin-Wu-Hausman test.

[11]We also constructed a dummy variable for 'not having significant home equity' from answers to questions concerning the value of owned homes and the outstanding mortgage. The results were very similar to those using the 'renter' dummy.

[12]Unfortunately we have too few observations to make a separate study of lone parents.

The results of this exercise are presented in Table 4[13]. From specification 5 we see that of all the crossed variables, only 'no assets' and 'married with spouse not employed at the job separation' are significant. In specification 6 we present the results with just these two crossed variables. That is, we drop all the other crossed terms and the level of benefit variable itself; the latter is wholly insignificant if added to specification 6.

Finally, we come back to the interaction 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 the Social Assistance level[14].

The final two columns of Table 4 present the results of crossing the 'not eligible for Social Assistance' dummy variable with the adjusted replacement ratio variables. The final column (specification 8) gives our preferred specification. From this we see that two (non-exclusive) groups are sensitive to benefit fluctuations. 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.

Although these benefit effects are statistically significant, they are not very large. Once again we consider the case where the respondent is the sole breadwinner ('importance' = 1) and a cut in the replacement rate from 60% to 50% (which constitutes a cut of 17% in the actual benefit paid). For those with no assets at the job loss, we predict a decrease in total expenditure of about 2.1%. The effect for the other group is about the same if they have some assets; if they had no assets at the job separation then the fall is about in total expenditure is about 4%. Discussion of these results is postponed until the concluding section which follows an investigation of the robustness of our results.

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

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

5.4. 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 modelling choices. The first check is on the sensitivity of the parameter 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.

In figure 3 we plot the DFBETA statistics for the coefficients on the 'no assets' replacement ratio variable (which has a value of 21.35) against the proportional change in total expenditure. The plot symbol give the replacement ratio (in % terms) for the observation. Thus figure 3 incorporates information from figures 1 and 2 to focus very precisely on the influence of individual observations on the parameter of primary interest. The most important information in figure 3 is that there is no very high positive values so that the positive coefficient observed for the parameter of interest is not being driven by a small number of outliers. It is also of interest to note 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 (in particular, see the two large negative observations with rates of over 100%). We shall see some of this evidence repeated in the checks below.

In Table 5 we present the estimates of the parameters of interest with various modifications to the specification. For convenience, the first row presents the preferred estimates from the final column in Table 4. The first two variants (experiments 1 and 2) concentrate on the 'importance' variable. They are motivated by the fact that for many of the respondents the 'importance' variable is greater than unity. Unless the household had negative net 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. In experiment 1 we drop the 198 respondents who had 'importance' greater than unity; as can be seen the parameter estimates are virtually unchanged. In the second experiment we

replace the importance variable used (which is based on the respondent's report of household net income and earnings) with a measure based on the 1992 tax data. For unmarried respondents, the alternative measure of importance is defined as the ratio of the respondent's gross earnings to gross income. For married couples it is defined as the ratio of the respondent's gross earnings to the gross income of the couple. This is an imperfect measure of 'importance' since it is based on gross values and ignores the earnings of other members of the household but it has the great virtue of having a measurement error that is uncorrelated with the measurement error in our preferred measure of importance. As can be seen from Table 5, this alternative measure gives somewhat different parameter values but the qualitative results are unchanged.

The next three experiments address concerns about the variation in the replacement ratio. As can be seen from figure 2 we have a substantial number of respondents with a replacement ratio of above the statutory rates of around 60% and also a number who do not receive benefit. As explained above, the former phenomenon comes about since our measure of past earnings is different to 'insurable earnings'. Could this be biasing our results? To check this, we try two variants. In the first, we simply exclude those with a replacement ratio of above unity. In the second, we replace the replacement ratio by the statutory rate whenever the previous 'insurable earnings' are below the maximum level. That is, for those with insurable earnings below the maximum level we use the insurable earnings as the 'previous earnings' measure (which gives these respondents a replacement rate of exactly 57% or 60%) and for those with insurable earnings at the maximum level (remember, insurable earnings are top coded at the maximum level) we use the respondent's reported earnings. The distribution of this revised replacement rate measure is given in figure 4. Comparing this with figure 2 it will be clear that this revised measure removes much of the variation in the rate. As can be seen from Table 5, however, neither of these changes makes any significant difference to the parameter estimates. Note that taking out those with a replacement ratio of above 100% increases the coefficient on the 'no assets' variable, exactly as we would expect from figure 4.

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 5 we present the parameter estimates for the sub-sample who have positive benefit (using the 'preferred' replacement rate). Once again the results

are virtually unchanged.

The final 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 falls by 7.5%, 25% and 50% respectively. The parameter estimates given speak for themselves. The effect of not having assets at the job separation is insignificant for the median group whereas it is very large for those who experience a large fall. Indeed, taking the parameter estimate for the first decile, a reduction in the replacement rate from 60% to 50% (which equals a 17% fall in the benefit level) would increase the expenditure fall by 7.8 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.

The bottom line for these robustness checks is that the basic result (see specification 7 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 large 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. The survey 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 1 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 16%. The 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, all of the effects on job search, 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 two sets of instruments. Our results indicate that we do indeed have an adequate set of permanent controls. Our findings suggest only 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.

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 and the service flow from durables and housing) is a complicated one. For example, the mean 16% fall in expenditures seen in our sample could all be for durables and clothing. If these depreciate only slowly, then households will maintain living standards (or 'smooth consumption') even over a relatively long unemployment spell. Equally, the small effect of differences in the replacement rate may translate into almost negligible falls in real living standards. If this is the case, then it seems that marginal changes in UI benefit levels may have only very small effects on living standards during an unemployment spell. This in turn implies that variations in the benefit rate are unlikely to have any noticeable impact on search behaviour, the duration of unemployment or the quality of any new job taken.

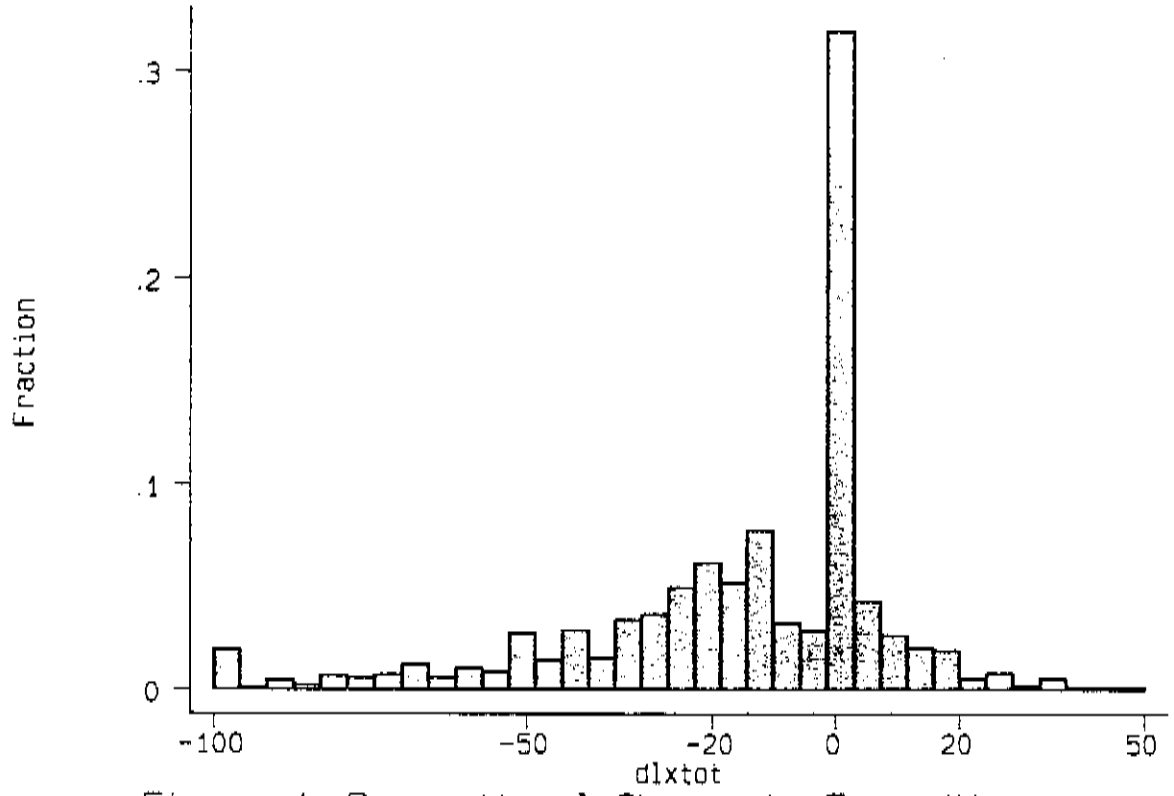

Figure 1: Proportional Change in Expenditure

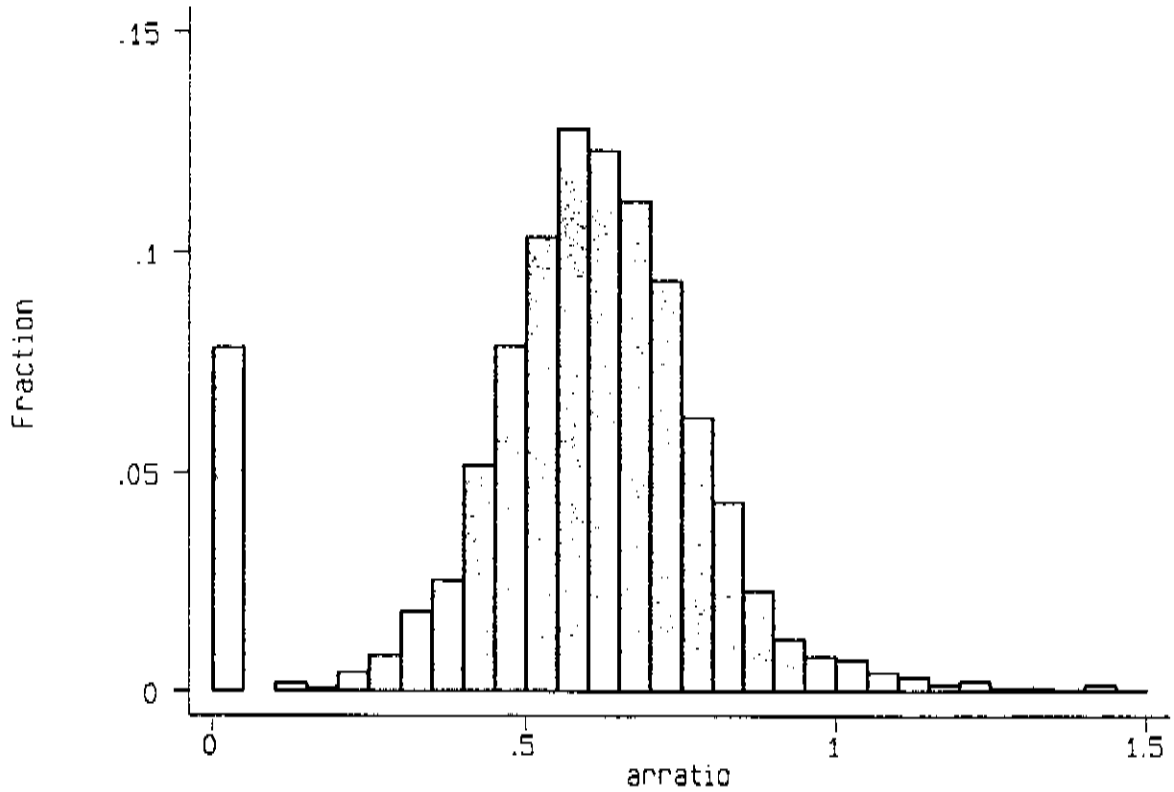

Figure 2. Ratio of Benefit to Past Earnings

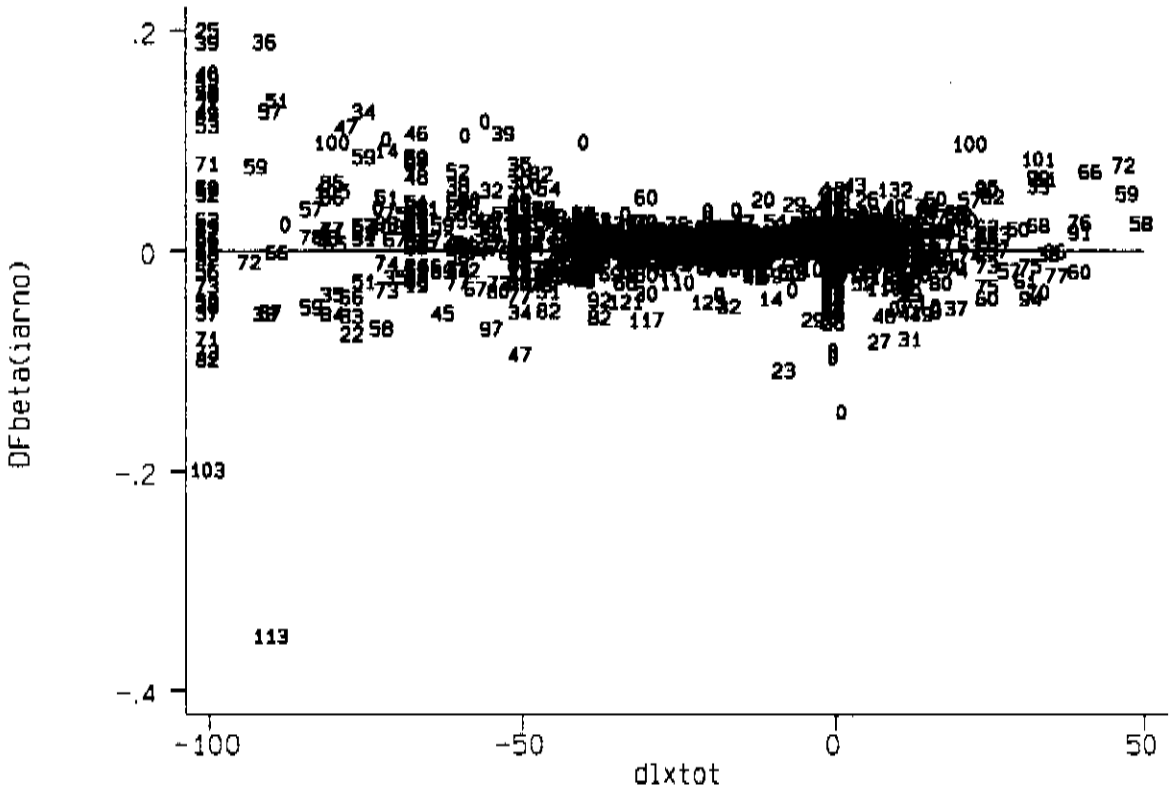

Figure 3. Dfbeta for 'no assets' parameter

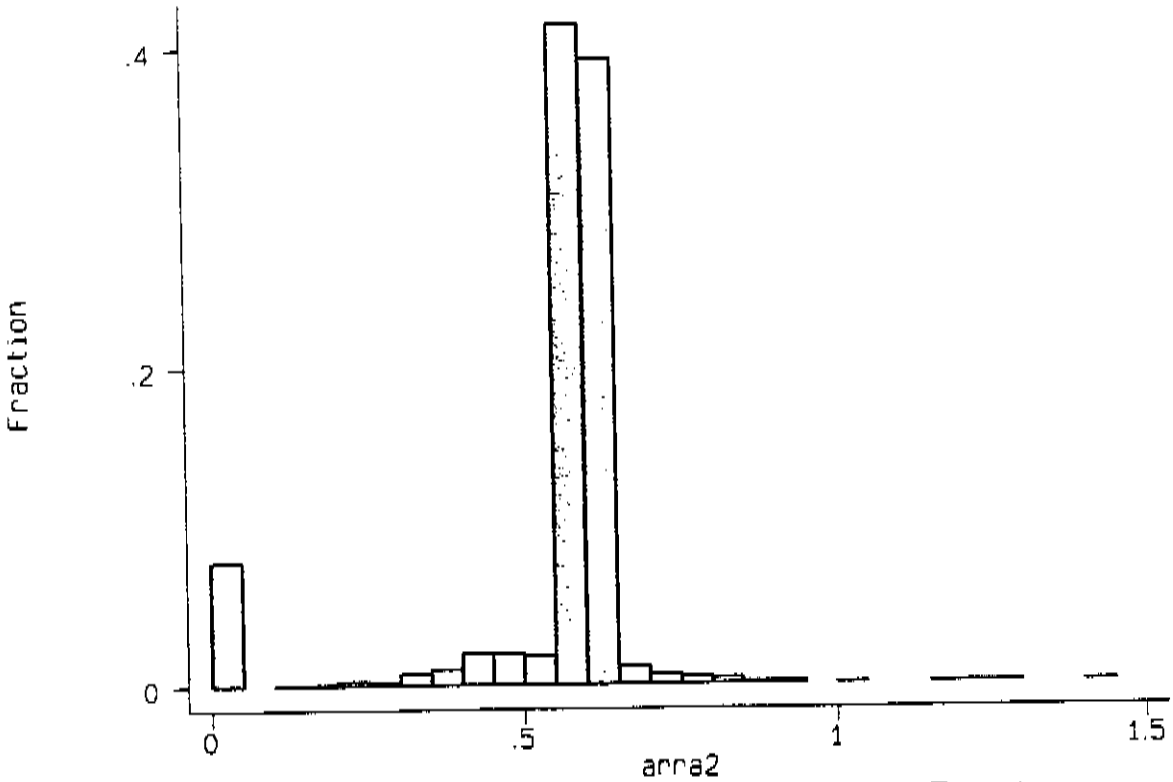

Figure 4. Ratio of Benefit to Insurable Earnings

TABLE 1
MEANS OF DLXTOT FOR DISCRETE VARIABLES

VARIABLE	NUMBER	MEANS	T-VALUE
FEMALE	1055	-13.4	
MALE	976	-17.5	3.6
IRREGULAR UI USER	1245	-16.1	
REGULAR UI USER	634	-12.9	2.6
NON-SEASONAL JOB	1749	-16.1	
SEASONAL JOB	283	-10.7	3.3
SHORT TENURE JOB	954	-13.2	
LONG TENURE JOB	1078	-17.3	3.7
RENTER	960	-17.7	
HOMEOWNER	1072	-13.3	4.0
MARRIED, SPOUSE EMPLOYED	806	-11.5	
MARRIED, SP. NOT EMPLOYED	396	-16.2	3.0
SINGLE	335	-21.3	6.0
OTHER HOUSEHOLD	495	-16.9	3.7
NO ASSETS AT JOB LOSS	1151	-15.6	
SOME ASSETS AT JOB LOSS	881	-15.1	0.3
JOB LOSS UNEXPECTED	972	-16.8	
JOB LOSS EXPECTED	1060	-14.1	2.4
WHITE COLLAR	908	-14.5	
BLUE COLLAR	480	-14.6	0.1
MANAGER	445	-18.8	2.9
LESS THAN HIGH SCHOOL	650	-13.5	
HIGH SCHOOL	755	-14.6	0.8
MORE THAN HIGH SCHOOL	627	-18.1	3.2
NOT VISIBLE MINORITY	1645	-16.0	
VISIBLE MINORITY	387	-12.5	2.4
ATLANTIC REGION	180	-8.48	3.6
QUEBEC	476	-16.13	0.0
ONTARIO	776	-16.14	-
PRAIRIES	313	-15.45	0.4
BRITISH COLUMBIA	239	-17.54	0.7

TABLE 2
UNIVARIATE REGRESSIONS ON CONTINUOUS VARIABLES

VARIABLE	NUMBER NON-MISSING	COEFFICIENT (T-VALUE)
CHANGE IN SPOUSE'S HOURS	1988	0.08 (0.9)
LOCAL UNEMPLOYMENT RATE	2020	0.41 (2.5)
LN(EARNINGS ON LOST JOB)	1863	-5.95 (5.6)
LN(EARNINGS LAST YEAR)	1916	-6.03 (5.8)
IMPORTANCE OF INCOME FOR HHOLD	2022	-17.24 (8.9)
AGE (IN DECADES)	2032	1.12 (2.2)
% OF INCOME COMMITTED	1832	0.30 (0.2)
LN(HOUSEHOLD SIZE)	2032	4.81 (4.5)
WEEKS OF WORK IN YEAR BEFORE JOB LOSS	2029	-0.21 (5.6)

TABLE 3
MULTIVARIATE REGRESSIONS

SPECIFICATION	1	2	3	4
VARIABLE	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)
MALE	-2.37 (1.5)	-	-2.41 (1.5)	-2.45 (1.5)
REGULAR UI USER	2.02 (1.5)	-	1.88 (1.5)	1.91 (1.5)
SEASONAL	2.21 (2.1)	-	2.25 (2.1)	2.26 (2.1)
LONG TENURE	-1.19 (1.6)	-	-0.98 (1.6)	-1.03 (1.6)
WEEKS WORKED IN YEAR BEFORE JOB LOSS	-0.11 0.1	-	-0.13 (0.1)	-0.12 (0.1)
LOCAL UNEMPLOYMENT RATE	0.02 (0.2)	-	0.01 (0.2)	0.01 (0.2)
HOMEOWNER	2.34 (1.5)	-	2.15 (1.5)	2.17 (1.5)
SPOUSE NOT EMPLOYED	-1.83 (1.9)	-	-1.61 (1.9)	-1.62 (1.9)
SINGLE	-4.34 (3.3)	-	-3.91 (3.3)	-3.87 (3.3)
OTHER HOUSEHOLD	-2.53 (1.9)	-	-2.43 (1.9)	-2.44 (1.9)
SOME ASSETS	1.51 (1.4)	-	1.49 (1.4)	1.48 (1.4)
EXPECTED JOB LOSS	1.26 (1.4)	-	1.30 (1.4)	1.28 (1.4)
BLUE COLLAR	2.53 (1.8)	-	2.43 (1.8)	2.42 (1.7)
MANAGER	-1.97 (1.7)	-	-2.03 (1.7)	-2.08 (1.7)
HIGH SCHOOL	1.00 (1.1)	-	1.01 (1.6)	1.00 (1.6)
COLLEGE	-2.18 (1.8)	-	-2.30 (1.8)	-2.32 (1.8)
VISIBLE MINORITY	3.73 (1.7)	-	3.69 (1.7)	3.69 (1.7)

TABLE 3 (CONTINUED)				
SPECIFICATION	1	2	3	4
VARIABLE	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)
AGE (DECADES)	1.97 (0.7)	-	2.04 (0.7)	2.04 (0.7)
LN(HHOLD SIZE)	-2.22 (2.1)	-	-2.47 (2.1)	-2.49 (2.1)
ATLANTIC REGION	7.10 (2.6)	-	7.13 (2.6)	7.19 (2.6)
QUEBEC	1.70 (1.9)	-	1.57 (1.9)	1.56 (1.6)
PRAIRIES	1.00 (1.9)	-	1.12 (1.9)	1.13 (1.9)
B.C.	-0.79 (2.2)	-	-0.74 (2.2)	-0.73 (2.2)
LN(EARNINGS ON LOST JOB)	2.70 (2.3)	-	4.17 (2.6)	4.33 (2.5)
SQUARE OF ABOVE	-0.49 (2.2)	-	0.39 (2.3)	0.52 (2.3)
CUBE OF ABOVE	-2.07 (1.5)	-	-2.03 (1.5)	-2.06 (1.5)
LN(EARNINGS LAST YEAR)	1.34 (2.4)	-	0.61 (2.5)	0.46 (2.4)
% OF INCOME COMMITTED	10.21 (2.4)	-	11.27 (2.5)	11.22 (2.5)
IMPORTANCE OF EARNINGS	-18.11 (3.5)	-13.5 (2.5)	-16.91 (3.8)	-16.53 (3.6)
ADJUSTED REPLACEMENT RATE	-	10.2 (4.3)	9.48 (7.0)	10.56 (5.9)
IMPORTANCE*NO BENEFIT	-	6.3 (4.6)	-2.00 (6.9)	-
CONSTANT	-2.87 (5.1)	-2.9 (1.5)	-0.59 (5.3)	-0.81 (5.2)
SAMPLE SIZE	1497	2022	1497	1497
R^2	0.096	0.04	0.098	0.098

TABLE 4
MULTIVARIATE REGRESSIONS

SPECIFICATION	5	6	7	8
VARIABLE	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)	COEFF. (SE)
ADJUSTED REPLACEMENT RATIO	-1.62 (9.4)	-	-	-
ADJUSTED REPLACEMENT RATIO CROSSED WITH:				
NO ASSETS AT JOB LOSS	20.21 (7.9)	17.28 (6.1)	21.54 (6.8)	21.35 (6.1)
RENTER	-5.33 (8.2)	-	-	-
AGE	-3.32 (3.7)	-	-	-
SPOUSE NOT EMPLOYED	24.06 (9.8)	21.29 (8.4)	10.01 (9.6)	-
SINGLE	-0.21 (11.9)	-	-	-
OTHER HOUSEHOLD	0.39 (11.0)	-	-	-
REGULAR UI USER	-5.03 (8.2)	-	-	-
(ADJUSTED REPLACEMENT RATIO* NOT ELIGIBLE FOR SOCIAL ASSISTANCE) CROSSED WITH:				
NO ASSETS AT JOB LOSS	-	-	-3.83 (6.4)	-
SPOUSE NOT EMPLOYED	-	-	19.79 (8.5)	22.92 (7.3)
SAMPLE SIZE	1497	1497	1494	1494
R^2	0.108	0.107	0.111	0.110

Notes: All regressions contain the set of variables given in Table 3, specification 4 (except for the adjusted replacement ratio).

TABLE 5
ROBUSTNESS CHECKS

Experiment	Size	Coefficient (se) var1*	Coefficient (se) var2*
0. Preferred.	1494	21.4	22.9
		(6.1)	(7.3)
1. Importance ≤ 1.	1296	20.6	21.8
		(6.5)	(7.1)
2. Alternative importance variable.	1464	31.6	17.4
		(7.6)	(7.8)
3. Replacement ratio ≤ 1.	1461	25.2	22.1
		(6.6)	(7.4)
4. Statutory replacement rate for those below maximum benefit.	1494	19.8	19.6
		(8.0)	(7.7)
5. Receiving UI benefit.	1397	25.5	21.0
		(6.7)	(7.7)
6. Median regression. (Median = -7.5%)	1494	9.4	14.8
		(6.3)	(12.4)
7. 25% regression. (First quartile = -25%)	1494	28.3	35.5
		(11.7)	(13.5)
8. 10% regression (First decile = -50%)	1494	78.1	34.6
		(17.7)	(23.0)

*((1 − adjusted ratio) × importance) multiplied by (no assets) and (not eligible for SA and non-working spouse) respectively.

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