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McMASTER UNIVERSITY

Department of Economics
Kenneth Taylor Hall 426
1280 Main Street West
Hamilton, Ontario, Canada
L8S 4M4

http://socserv.socsci.mcmaster.ca/~econ/

Shocks, stocks and socks: smoothing consumption over a temporary income loss.

Martin Browning[a] and Thomas F. Crossley[b]

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Abstract: Recent research has demonstrated that some households cut back on expenditures in an unemployment spell. Moreover, some of these households respond to variation in the transitory income provided by unemployment insurance benefits. This suggests that these households are constrained in the sense that they respond to variations in current income even if these do not have any permanent impact. In this paper we take up the question of *how* households in temporarily straitened circumstances cut back and how they spend marginal dollars of transfer income. Our theoretical and empirical analysis emphasises the importance of allowing for the fact that households buy durable as well as non-durable goods. The theoretical analysis shows that in the short run households can significantly cut back on total expenditures without a significant fall in welfare if they concentrate their budget reductions on durables. We present an empirical analysis based on a Canadian survey of workers who experienced a job separation. Exploiting changes in the unemployment insurance system over our sample period we show that cuts in UI benefits lead to reductions in total expenditure with a stronger impact on clothing than on food expenditures. These effects are particularly strong for households with no liquid assets and/or households in which the lost income was 'important' for the household. These findings are in precise agreement with the theoretical predictions.

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[a] CAM, Institute of Economics, University of Copenhagen, Studiestraede 6, DK-1455 Copenhagen K., Denmark. Martin.Browning@econ.ku.dk
[b]Department of Economics, McMaster University, Hamilton, Canada. crossle@mcmaster.ca.

I. Introduction

How do agents in a temporarily difficult financial situation – for example an unemployment spell – adjust to these circumstances? Research by Gruber (1997) and by us (Browning and Crossley, 2001) has demonstrated that households cut back on expenditures in an unemployment spell after a job loss. Moreover, some of these households respond to variation in the transitory income provided by unemployment insurance benefits. This suggests that these households are constrained in the sense that they respond to variations in current income even if these do not have any permanent impact. In this paper we take up the question of *how* households in temporarily straitened circumstances cut back and how they spend marginal dollars of transfer income. Our theoretical and empirical analysis emphasises the importance of allowing for the fact that households buy durable as well as non-durable goods.

Hamermesh (1982) and Parker (1999) discuss how changes in transitory income affect demands for individual goods. On the face of it they seem to present different effects. Hamermesh notes that if agents cut back on total expenditure then there will be a bigger proportional impact on luxuries; this is the standard uncompensated response. Parker, on the other hand, suggests that agents who are temporarily constrained will cut back more on goods that exhibit high intertemporal substitution since the utility cost of fluctuations in these is lower than for goods which are not substitutable over time. In Browning and Crossley (2000) we show formally that the Hamermesh and Parker (H-P) effects are identical (if within period preferences are additive); that is, luxuries have a high intertemporal substitution elasticity. This is an exact form of Pigou's Law (see Deaton (1974)). Although the H-P point is valid we show in our empirical analysis that the impact of temporary short-falls in income differ too dramatically

across goods for the H-P effect to be the main driving force behind the adjustments in expenditure patterns.

In this paper we examine a mechanism that is quite distinct from the H-P effect and which emphasises the durability of many of the goods that households purchase. Our theoretical analysis extends Chah, Ramey and Starr (1995) and Alessie, Devereux and Weber (1997). In the next section we present a neo-classical (discrete or continuous) durables model with irreversibility. We show that for liquidity constrained agents modest falls in transitory income are absorbed almost completely by cutting back on expenditures on durables and leaving non-durable expenditures almost unchanged. We also show that this leads to small falls in current welfare. We then introduce an irreversibility constraint on durables (no second hand markets and, consequently, no collateral borrowing). We show that if income falls sharply enough so that the agent hits these constraints then the situation changes dramatically. Now all additional cuts in total expenditure must be met by cutting non-durable expenditures. This leads to much larger falls in current welfare. Thus our analysis suggests that irreversible durable goods play a critical role in consumption smoothing mechanisms. The sorts of goods that formally fit our model are 'small durables' such as socks, coats, pillows, plates etc. Budget studies indicate that these goods account for about twenty percent of total (non-housing) expenditures in 'normal' times.[1] Our analysis formalises the idea that agents who have to cut back temporarily on total expenditure will choose to postpone replacing a worn but serviceable winter coat rather than go hungry.

In the theoretical section we derive qualitative predictions for the structure of demands and the impact on these of current income and financial assets. We also present a calibration exercise to explore the potential size of the effects we describe.

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[1] Our analysis can also be extended to goods for which there are imperfect capital markets and which are partially collateralisable, such as white goods and electronic goods.

In our empirical work we examine whether these implications hold, with a particular emphasis on the role of Unemployment Insurance benefits. Among the unemployed, variation in UI benefits gives a source of variation in transitory income. Because the unemployed are a group who are most likely to be in straitened circumstances, this should enhance the power of our test. Our data capture a series of reforms to the Canadian Unemployment Insurance system, which provide plausibly exogenous variation in benefits. We find considerable support in the data for the implications of our theoretical model.

The outline of the rest of the paper is as follows. The next section lays out the theory, and presents our qualitative and quantitative analysis. Section III describes the data we use in our empirical work, and presents some preliminary statistics on the budget allocations of employed and unemployed workers. Section IV discusses a number of econometric issues that must be resolved in order to give a proper assessment of our theoretical predictions. Section V reports our empirical results and Section VI concludes.

II. Theory

II.1 Allocation with irreversibility and liquidity constraints.

We present a theoretical model for the many goods case in which at least one good is non-durable (consumption equals current purchases) but others may be durable. Our assumptions concerning the environment are motivated by our primary concern with `small durables' so that we impose irreversibility (no second hand markets) and no collateral for all goods. We emphasise, however, that we could allow for partial reversibility and some collateral and still have the same qualitative implications. We adopt a neo-classical framework, so that the stock of durable k in period t, S_{kt}, evolves according to:

$$S_{kt} = (1-\delta_k)S_{k,t-1} + d_{kt}$$

where d_{kt} is the addition to the stock in period t, δ_k is the depreciation of the durable, with $\delta_{kt} \leq 1$. The irreversibility constraint is imposed by requiring $d_{kt} \geq 0$. At the end of this section we discuss the effects of transaction costs and discreteness. We take the first good to be non-durable: $\delta_1 = 1$. The stock evolution equation can be written more succinctly in vector form as:

$$S_t = (1-\delta).*S_{t-1} + d_t$$

where $.*$ denotes term by term multiplication. The agent starts each period with assets A_t and receives non-capital earnings (which include transfer income) of Y_t. Cash-on-hand, $X_t = A_t + Y_t$, is then divided between expenditure on goods and saving. Since we are not primarily interested in price effects we shall simply set all relative prices to unity and assume that the real interest rate is constant at the value r. Assets evolve according to:

$$A_{t+1} = (1+r)(X_t - e'd_t)$$

where e is the unit vector. When we have durables, the precise definition of a liquidity constraint depends on whether the agent can borrow against the stocks of durables (see Alessie, Devereaux and Weber (1997) and Chah, Ramey and Starr (1995)). For the case of `small durables' we impose that no collateral borrowing is possible which gives the liquidity constraint $X_t \geq e'd_t$. The state variables are cash-on-hand, X_t, and the stocks of the durable in the last period, S_{t-1}.[2] Taking an infinite horizon stationary program and denoting the value function at time t by $V(X_t, S_{t-1})$ and the within period utility function by $\upsilon(S)$, the Bellman equation is:

$$V(X_t, S_{t-1}) = \max_d \left\{ \begin{array}{l} \upsilon\big((1-\delta).*S_{t-1} + d\big) + \\ \beta E_t\Big[V\big((1+r)(X_t - e'd) + Y_{t+1}, (1-\delta).*S_{t-1} + d\big)\Big] \end{array} \right\} \qquad (2.1)$$

[2] If we did not have the irreversibility constraint then we could write the program with just one state variable, total assets (financial assets plus the value of the stocks carried forward).

subject to the liquidity and irreversibility constraints. The parameter β is the discount factor and $E_t[.]$ is the expectations operator conditional on the information set at time t. Assuming Inada conditions so that any non-durable consumption and the stock are positive, the first order conditions for the program in (2.1) are:

$$\begin{aligned} \upsilon_1^t &= \beta(1+r)E_t\left[V_X^{t+1}\right] + \mu_t \\ \upsilon_k^t &= \beta(1+r)E_t\left[V_X^{t+1}\right] - \beta E_t\left[V_k^{t+1}\right] - \theta_{kt} + \mu_t \quad \text{for } k>1 \end{aligned} \tag{2.2}$$

where υ_k^t is the partial of $\upsilon(.)$ with respect to S_{kt} evaluated at $\hat{S}_t = (1-\delta).*S_{t-1} + \hat{d}$ and similarly for the partials of the value function. The variables μ_t and θ_{kt} are the (non-negative) Lagrange multipliers associated with the liquidity constraint and the irreversibility constraints respectively. Note that since we have taken the first good to be non-durable the irreversibility multiplier θ_{1t} is always zero. The envelope conditions are:

$$\begin{aligned} V_X^t &= \beta(1+r)E_t\left[V_X^{t+1}\right] + \mu_t \\ V_k^t &= (1-\delta)\left(\upsilon_k^t + \beta E_t\left[V_X^{t+1}\right]\right) \quad \text{for } k>1 \end{aligned} \tag{2.3}$$

Multiplying the first order conditions for $k>1$ by $(1-\delta_k)$ and substituting, we have:

$$V_k^t = (1-\delta_k)\left(V_X^t - \theta_{kt}\right) \tag{2.4}$$

Taking leads and expectations this yields:

$$E_t\left[V_k^{t+1}\right] = (1-\delta_k)\left(E_t\left[V_X^{t+1}\right] - E_t\left[\theta_{kt+1}\right]\right) \tag{2.5}$$

Collecting everything together we have the following expression for the marginal rate of substitution (*mrs*) between good k and good 1 in period t:

$$\frac{\upsilon_k^t}{\upsilon_1^t} = \frac{(\delta_k + r)}{(1+r)} + \frac{(1-\delta_k)}{(1+r)}\frac{\mu_t}{V_X^t} - \frac{\theta_{kt}}{V_X^t} + \frac{\beta(1-\delta_k)E_t\left[\theta_{kt+1}\right]}{V_X^t} \tag{2.6}$$

If good k is non-durable then $\delta_k = 1$ and $\theta_{kt} = 0$ for all t (from the assumed Inada condition), so that the within period *mrs* between non-durables is unity (the relative price) and is consequently independent of whether or not the liquidity constraint holds. Meghir and Weber (1996) exploit this condition in a test for liquidity constraints. For a durable ($\delta_k < 1$) the first term on the right hand side is the user cost; if there are no constraints then this is the usual *mrs* condition for a neo-classical durables model.

In all that follows we assume that:

$$\frac{\upsilon_k(S)}{\upsilon_1(S)} > \frac{\upsilon_k(S^*)}{\upsilon_1(S^*)} \Rightarrow \frac{S_k}{S_1} < \frac{S_k^*}{S_1^*} \tag{2.7}$$

This is equivalent to assuming enough so that in an environment with no irreversibility or liquidity constraints, a rise in the real rate (which increases the user cost for all durables) would lead to a fall in *all* stocks relative to the non-durable consumption. A sufficient condition for this is the utility function being additive with each sub-utility function being strictly concave, but weaker conditions will also give the condition. Essentially we need to rule out strong complementarities between the first (non-durable) good and the other goods.

We now consider the other three terms in equation (2.6) in turn (assuming $\delta_k < 1$). Suppose first that there are no irreversibility constraints ($\theta_{kt} = 0$ for all t) and that the liquidity constraint binds ($\mu_t > 0$). Then

$$\frac{\upsilon_k^t}{\upsilon_1^t} = \frac{(\delta_k + r)}{(1+r)} + \frac{(1-\delta_k)}{(1+r)}\frac{\mu_t}{V_X^t} > \frac{(\delta_k + r)}{(1+r)} \tag{2.8}$$

so that the *mrs* of any stock relative to the non-durable is higher than in the unconstrained case. That is, a binding liquidity constraint causes agents to cut back more on *all* durable expenditures than on non-durables. This is because the future value of current additions to stocks are

discounted more heavily than in the unconstrained case. Moreover, agents cut back proportionately more on durables with a low depreciation rate. For example, Bils and Klenow (1998) report high depreciation rates for shoes and curtains but low depreciation rates for books and china.

Suppose now that the agent is not liquidity constrained but finds that the desired stock of good k is lower than the stock brought in from the last period. This might follow if, for example, there was a fall in 'permanent income' (so that lower stocks are desired) but agents had liquid assets at the beginning of the period. In this case $\mu_t = 0$ and $\theta_{kt} > 0$. Suppose further that the $t+1$ realisations are such that $\theta_{kt+1} = 0$ for all states of the world. In this case, we have:

$$\frac{\upsilon_k^t}{\upsilon_1^t} = \frac{(\delta_k + r)}{(1+r)} - \frac{\theta_{kt}}{V_X^t} < \frac{(\delta_k + r)}{(1+r)} \tag{2.9}$$

so that the agent starts the period with too much of durable k and, of course, does not buy any of this durable. Examining this equation we see that this effect would be more likely for durables that depreciate slowly and/or have a high lifetime wealth elasticity.

Finally we examine the case in which the agent is not liquidity constrained and does purchase some of durable k in period t (so that $\theta_{kt} = 0$). If there is any period $t+1$ state of the world in which the stock carried forward is too high then $E_t[\theta_{kt+1}] > 0$ and we have:

$$\frac{\upsilon_k^t}{\upsilon_1^t} = \frac{(\delta_k + r)}{(1+r)} + \frac{\beta(1-\delta_k)E_t[\theta_{kt+1}]}{V_X^t} > \frac{(\delta_k + r)}{(1+r)} \tag{2.10}$$

so that the agent holds less stock in period t than would be justified by the user cost. This follows since there is some chance that the stock next period would be too high and consequently current additions to stocks are less valuable.

II.2 Quantitative effects.

The qualitative results given above indicate that agents will cut back disproportionately on durables (as compared to non-durables) if they are liquidity constrained but they do not give much hint on how strong this effect will be. Nor do these results give any clear indication on the interactions between liquidity constraints and irreversibility. To assess these we present a calibration exercise. To keep things manageable, we consider the case of one non-durable and one durable good. We take a model in which the planning period is three months and we set the interest rate equal to the discount rate. We consider a model with no uncertainty, constant earnings of unity each quarter and we assume that the agent does not have any financial assets. In this case the agent sets expenditure on non-durables and durables equal to earnings in each period, keeps consumption constant from period to period and sets durables expenditures equal to depreciation so that the stock is constant. We set the quarterly real rate to 1% and the depreciation rate to 0.1 (an annual depreciation rate of 0.34). We take an additive log utility function

$$\nu\left(S_{1t}, S_{2t}\right) = \ln\left(S_{1t}\right) + \gamma \ln\left(S_{2t}\right) \tag{2.11}$$

with a weight on the second sub-utility function so that in the steady state the agent sets consumption expenditure equal to 0.8 and non-durable expenditures equal to 0.2 (values suggested by budget studies). We intentionally impose within period additivity and homotheticity for the preferences over the two stocks to assume away complementarity and H-P ('luxury') effects.

To introduce a constrained program, assume that in one period earnings are set to less than unity and agents are not allowed to borrow. We assume that the agent did not anticipate any earnings fall so that the stock in the previous period is equal to the steady state value and we set

earnings in the subsequent period so that the agent returns to the steady state values for consumption and the stock of durables.[3] In this case we have that both non-durable and durable expenditures are lower in the low income period than in the steady state. Figure 1 presents the graph of expenditures on the two goods against current earnings (the figure is to be read 'right to left' with small income losses on the right). There are two important features to this figure. First, for falls in earnings of less than about 20% the effect on non-durables is negligible and the effect on durables expenditures is almost one for one. That is, a cut in earnings of 0.2 leads to a cut of one percent in non-durables expenditures and 95% for durables. The second important feature of the figure is that if earnings are low enough (in the case considered here, below 0.79) then the desired stock exceeds the stock inherited from the last period and because of the irreversibility constraint the agent sets durables expenditures equal to zero. In this case all of the impact of further earnings cuts is forced onto non-durables. Thus there is a distinct shift in responses at a critical value of earnings at about the budget share of non-durables.

To emphasise our point, in Figure 2 we plot the current marginal utility of money against earnings. As can be seen, modest cuts in earnings do not cause the marginal utility to rise very much. However, once the irreversibility constraint begins to bind, the effect of earnings cuts on the marginal utility of money is much more dramatic. Another important feature of Figure 2 is that the *mue* is convex which is usually taken to indicate prudence. As is well known, adding a liquidity constraint to a program with only a single (non-durable) good and no uncertainty leads to convexity in the *mue* (if we are in the HARA class). Here the kink in the *mue* occurs not at

[3] Assuming that the earnings fall was totally unexpected gives simple analytics. Simulation with stochastic earnings (with iid earnings with a very high probability of unity and a very low probability of earnings less than unity) gives almost identical results to those presented here.

unit earnings but at the value at which ‘total wealth’ (financial assets plus excess durables stock) is zero.

The implication of this analysis is that agents absorb most of a modest earnings cut by cutting back dramatically on durables and leaving non-durables almost untouched. Thus the welfare impact of such an earnings cut is much less than we would anticipate in an environment with no durable goods. For large earnings falls, however, agents have to start cutting back on non-durables and this has a much more immediate impact on welfare. Effectively it is as though the financial constraint is unimportant until the irreversibility constraint binds.

Two other facets of durables models that are often emphasised are transactions costs and indivisibilities. For `small durables' transactions costs are unlikely to be a significant factor. Discreteness is a different matter since most durables come in discrete units. Finding analytic results is usually impossible for models with discreteness, irreversibility and stochastic earnings, so we conducted some simulations using a simple replacement model. For this we took a non-durable and a durable which is held in unit quantity. The utility derived from the durable falls as it ages so that periodic replacement is required. We do not report the details since they are quite involved and the qualitative results are much the same as for the continuous case. In such a model, impatient agents in ‘unconstrained' periods keep the marginal utility of the non-durable constant and accumulate assets to finance the periodic replacement of the durable. In periods of temporarily low earnings agents do not replace the durable but concentrate their expenditures on non-durables, even to the extent of running down assets that were being saved for durable replacement. Thus assets serve two roles in the discrete case: as saving toward the replacement of a non-collateralisable durable and as a financial buffer stock. These two functions are complementary in that savings accumulated to replace the durable can be used to buffer non-

durables in the event of a transitory negative income shock. The important feature of the discete model is that in low income periods, the probability of a durables purchase is low relative to non-durable expenditures.

III. A First Look at the Data

We now consider testing some of the empirical implications of the model developed above. The source of temporary negative income shocks is the loss of a job with the consequent replacement of earnings by Unemployment Insurance benefits. The data source we use is the Canadian Out of Employment Panel (COEP). The COEP is a sample of Canadians who had a job separation in one of four windows - two in early 1993 and two in early 1995. We refer to respondents drawn from each of these four windows as belonging to cohorts one through four. Respondents were initially interviewed some 14 to 44 weeks after the reference separation. At this first interview they were asked a broad set of question regarding employment prior to the reference separation, subsequent job search and employment, household demographics, finances and expenditures. These data can then be matched to several kinds of administrative records, including those from the Unemployment Insurance (UI) system to provide an extremely detailed picture of these households in the period before and after a job loss. One or two subsequent interviews were conducted so that the households can be followed for about two years.

These data offer several important advantages. First, all respondents had a job separation so we have relatively large sample sizes of households experiencing unemployment. Second, we have exact details of any UI benefit payments (from the administrative data). Third, we have expenditure measures on food at home, clothing and housing and also a total expenditure measure (an advantage over the PSID, for example). Finally, the data span two reforms of the

Canadian UI system (between the first and second and between the second and third cohorts). As we discuss below this provides a quasi-experimental source of variation in transitory income.

In this paper we focus on expenditure information from the first interview and benefit records for the same period. We also focus on respondents who are unemployed at the first interview as the unemployed are the group who are likely to have current earnings below `permanent' earnings and for whom UI benefits provide a good measure of current income. By focusing on the first interview we maximize the fraction of respondents who are unemployed. The sample we study comprises singles, couples and couples with children where the respondent is between the ages of 20 and 60. We also exclude some types of separations which were sampled in the 1993 cohorts but not in 1995. Our final sample has 1,959 observations.

In addition to this sample of unemployed individuals we also construct a `control' sample of 1198 workers who report that they are back in a steady job, at least as good as the one that they separated from. In our empirical work, we will use this latter group for an important specification test (described in the next section).

To evaluate the theory developed in the previous section with our data, a key issue is the size of the income shocks experienced by our respondents' households. The theory predicts a sharp change in behaviour when the income shock exceeds the budget share of small durables. Budget studies suggest this number might be on the order of 20%. For a subset of our sample (those respondents who separated from jobs in 1995), we have information on the change in monthly, take-home household income between the month just prior to the job separation and the month prior to the interview. The mean percentage change for unemployed respondents is - 21% (median -19%). The modest size of income shocks associated with unemployment (a complete loss of earnings) reflects several factors. The UI system in Canada is fairly generous, with

statutory replacement rates over 50% and benefits lasting up to a year.[4] Second, Canada also has a second tier of income support: a means tested social assistance program that would be available to those who are ineligible for benefits, or whose benefits expire.[5] Finally, workers live in households and those households often have other earners.[6] Further details on the data and sample selection are provided in the Data Appendix.

Table 1 presents summary statistics on expenditure levels and patterns for our two samples, at the first interview. All expenditures are reported in 1993 Canadian Dollars (C$); the Canadian Dollar was worth about 0.75 U.S. Dollars at the time. The numbers are striking. Those who are back in steady and satisfactory jobs have much higher per capita total expenditures than those who are still unemployed, but almost identical per capita food expenditures. Consequently, their food shares are significantly lower. Conversely their shares of expenditures on clothing (a small durable) are significantly larger, as is their probability of having a positive expenditure on clothing. These differences in the structure of demand are summarized graphically in Figures 3 and 4, which display nonparametric Engel curves for food and clothing, for the two groups. These numbers and pictures are obviously strongly suggestive of our theoretical predications. Unfortunately, they are not entirely convincing, for a number of reasons.

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[4] Moreover, because the Canadian income tax system is progressive, the actual (after-tax) replacement rate is often higher than the statutory rate. Against that, workers losing jobs with earnings above the maximum insurable earnings will have an effective replacement rate below the statutory rate

[5] Social Assistance can also top up unemployment insurance benefits where those benefits are below the cutoff of the means test.

[6] Quite mechanically, if a worker provides 50% of household income prior to job loss, and faces a 60% actual replacement rate, then the job loss represents a shock to personal income of – 40% but to household income it is a shock of -20%. In some of our analysis below, we will examine variation in the `importance' to the household of the lost job as a source of variation in the size of income shocks.

An obvious place to start is the issue of heterogeneity. Those back in work may be different from those still unemployed, and this may explain some of the differences in observed expenditure patterns. Beyond that, it is important to recognize that unemployment likely has three broad impacts on expenditures. First, if there are costs of going to work then we would expect to see total expenditure fall and also to see a fall in such specific work related items as transport and clothing. More generally, if preferences over goods are not separable from labour supply (see Browning and Meghir (1991)) then a change in labour force status will induce changes in total expenditure and also in the structure of demands conditional on that total.

Second, job loss is often an unpleasant shock and can be expected to lower desired lifetime consumption. This shock impacts on both durable and non-durable expenditures. Agents will typically wish to run down stocks of durables by letting them depreciate so that we should expect to see lower levels of purchases of durables (or more zeros) after a job loss. There will also be a corresponding fall in nondurable expenditures. Conversely, finding a new job may be a pleasant shock with corresponding effects. Together these effects can be thought of as the 'permanent shock' effects of job loss and reemployment. These effects will obviously differ between the employed and unemployed samples.

Finally, there is the temporary loss of income due to being out of work. Our theoretical analysis presented above is concerned with responses to this "transitory shock". However, to assess such responses we must isolate this impact of unemployment from the others just noted, and control for heterogeneity. In the next section we outline an empirical framework which allows us to do so by exploiting the quasi-experimental nature of our data.

IV. Econometric Issues

Our empirical strategy is to estimate equations for food expenditures (e_i^f), clothing expenditures (e_i^c), and total expenditures (e_i^t), on our sample of unemployed respondents. The explanatory variable of interest is unemployment benefits (b_i). We also include other variables X_i that control for heterogeneity in tastes, for the current marginal utility of wealth ('permanent income', including the impact of the recent separation from a job) and for the process of selection into unemployment (more on this below). Thus our empirical framework can be summarized as:

$$
\begin{aligned}
f^f(e_i^f) &= \alpha^f(b_i) + X_i\beta^f + \varepsilon_i^f \\
f^c(e_i^c) &= \alpha^c(b_i) + X\beta^c + \varepsilon_i^c \\
f^t(e_i^t) &= \alpha^t(b_i) + X\beta^t + \varepsilon_i^t
\end{aligned}
$$

Among the unemployed, variation in UI benefits gives a source of variation in transitory income. We use simple and convenient functional forms for the $f()$ and $\alpha()$ functions, and focus on using the quasi-experimental nature of our data to derive 2SLS estimates of the effect of benefits on the level and composition of expenditures. In particular, we follow Gruber (1997) and instrument actual benefit paid with 'potential benefit'. Potential benefits are calculated as a function of past earnings, local unemployment rates, and weeks worked in the reference job. Because the UI system is federal in Canada, we cannot use the cross-state variation in benefits formulae that is the basis of Gruber's study. Instead we use the fact that parameters of the Canadian formula varied over the sample period with both legislative (the 1993 and 1994 reforms) and administrative changes. Because our regression controls (X_i) include past earnings, local unemployment rates, and weeks worked in the reference job, identification is coming from changes in the program parameters and also from nonlinearities in the benefit formula. The

available variation in the statutory rate is small relative to cross-state differences in the US. Against this, our rich controls and exact measurement of benefits means there is less noise from which to extract the signal. Furthermore the source of the variation we are using is transparent: a series of legislative cuts to the UI system designed to reduce program expenditures against the backdrop of a very slowly improving labour market.[7] Full details of the program changes are given in the appendix. However, we note here that while most of the changes in this period made the program less generous, there were two with the opposite effect. One was the introduction of a "dependency rate" which allowed for higher benefits for low income individuals with dependents. The other was the significant real growth in the maximum insurable earnings over the period, which offset the cuts in the legislative replacement rate. This meant that for individuals above the maximum insurable benefits the *actual* replacement rate did not decline.

This empirical strategy has a number of advantages. First, by focusing just on the unemployed, we eliminate the variation in labour supply, which confounds comparisons of the employed and unemployed if there are costs of working or non-separabilities between leisure and consumption (as discussed in the previous section). Second, the quasi-experimental nature of the data provides a fairly transparent source of variation in transitory income (benefits), and using potential benefits as our instrument allows us to capture all of the variation generated by the program changes.

Against these, it may be that our simple functional forms may be mis-specified. It is also certainly the case that respondents who are out of work at the first interview are a selected sample. We have several ways of addressing these concerns. First, with respect to functional form, we can and do subject our estimates to a variety of standard specification tests. With

[7]The unemployment rate in Canada drifted down from 11.3% in 1992 to 9.5% in 1995.

respect to selection, we note that the data provide us both with a very rich set of controls (X_i), and with quasi-experimental instruments for our variable of interest. All that is required is that our instruments are uncorrelated with the error terms in the expenditure equations conditional on selection and our controls (X_i).

Most importantly, however, the data provide us with a very natural way to test for a range of potential problems, including those just mentioned. In particular, we can estimate reduced forms of the expenditure equations (that is, with potential benefits in place of actual benefits) on the sample of respondents who are back in good jobs. Because these respondents were not receiving benefits, the potential benefits they would have received had they been unemployed should not affect their expenditures. This is perhaps most easily thought of as at test of instrument exogeneity. However, it is in fact an omnibus test for mis-specification and other problems, including those noted above. Intuitively, if the instrumented benefit variables are picking up mis-specifications in our simple functional forms, this should be apparent in the employed sample as well. Similarly, if the instruments are not exogenous conditional on the selection process into employment and unemployment, then this should be apparent in the employed sample.

For the $f()$ functions we use the inverse-hyperbolic sine (*ihs*) proposed by Burbidge, Magee and Robb (1988). The *ihs* is an alternative to the logarithm that admits zero values (it is linear through the origin), but which is very similar to the logarithm for larger values.[8] Expenditures are measured in dollars. Benefits (the $\alpha()$ functions) are entered linearly, and measured in hundreds of dollars. To aid in interpreting the estimates we calculate the marginal

[8] For food at home and total expenditure, which are always positive and measured in dollars per month, the *ihs* and the logarithm have a correlation of 1.00.

effect of 100 dollars of additional monthly benefits on dollars of monthly expenditure for each observation, and average over the estimation sample.[9]

Our controls include the size and composition of the household; the age, education and gender of the respondent; regional and seasonal dummies; characteristics of the lost job and local labour market; dummies for homeownership and investment income in the previous year; measure of the importance of the lost job in household income; and a polynomial in the earnings in the lost job. Further details are provided in the Data Appendix.

The average level of benefits in our sample of 1959 respondents who are unemployed at the first interview is C$ 770 per month. Average calculated potential benefits for this group were C$ 1104 per month. For the sample of 1198 respondents back in a good job, calculated potential benefits (had they not been employed) were C$ 1126 per month. An important question is the power of our instrument (potential benefits) to explain benefits, conditional on our other controls. Using the unemployed sample, we regressed actual benefits received on potential benefits and all our other controls. The estimated coefficient on potential benefits was 0.588 with a t-statistic (based on a robust standard error) of 27.0. Thus the reforms to the Unemployment Insurance system captured by our data provide substantial variation in benefits.

V. Quasi-Experimental Estimates

Our basic results are presented in Table 2, which contains three sets of estimates in 3 panels. For each good, in each panel, we report four quantities: the unconditional mean of

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[9] The *ihs* of e is $\sinh^{-1}(\theta e)/\theta$ where θ is a parameter. We use a value of 1 for θ; preliminary investigation suggested that our results were insensitive to this choice. The derivate of the *ihs* (with $\theta = 1$) is $(1+e^2)^{-\frac{1}{2}}$, so that the coefficient on benefits is transformed into a marginal propensity to spend by multiplying by $(1+e^2)^{\frac{1}{2}}$.

expenditure in the estimation sample; the estimated coefficient on the variable of interest (benefits, or potential benefits), the t-statistic for this estimate, and the average implied impact of 100 dollars of additional benefits on dollars of expenditure.

The first panel reports estimation of reduced form relationships – the linear regression of the *ihs* of expenditures on our instrument (potential benefits) and other controls. As Gruber (1997) points out, the response to potential benefits is often of most interest to policy makers, as it is potential benefits (rather than actual benefits) over which they have direct control.[10] Potential benefits have statistically significant effects on food, clothing and total expenditures. However, the effect on clothing is twice as large in absolute terms as the effect on food (averaging 4.5 dollars per 100 dollars of benefits against 2.2 for food). Because these households spend more on average on food than clothing (362 dollars against 102), the difference in the *relative* effects is even greater.

We have subjected these reduced form estimates to a standard battery of specification tests. None of these tests suggested any problem. For example, for all three equations, RESET tests for omitted variables could not reject the null hypothesis of no omitted variables. We also calculated DFBETA influence statistics (see Chaterjee and Hadi, 1988) for each observation for the coefficients of interest. These calculations did not reveal any influential observations. Full details of these robustness checks are available from the authors.

We next consider reduced form estimates for a control sample of respondents back in a good job. As discussed in the previous section, these estimates provide a test of the exogeneity of our instruments and of the adequacy of our specification. In fact we cannot use the food equation

[10]Gruber also notes that actual UI receipts are very badly measured in the PSID. That is not a problem with our data. We have exact administrative records of UI receipt. Thus our main results are for actual benefit receipt.

for this test, because this sample was used to calibrate food expenditures across a change in the food expenditure reporting period between the 1993 and 1995 survey.[11] However, the clothing and total expenditure questions were the same in both surveys, so they are informative. The results demonstrate that potential benefits are not a significant determinant of either clothing expenditures or total expenditures among those back in a good job.

The final panel of Table 2 reports 2SLS estimates. The variable of interest is now *actual* benefits received, which is instrumented with potential benefits. Again we find statistically significant effects for food, clothing and total expenditures. The effects of actual benefits are, unsurprisingly, larger than the effects of potential benefits. Benefits have an economically significant effect on total expenditures, although the marginal propensity to consume benefit income is less than 1, with 100 dollars of additional benefits raising total expenditures by 22 dollars on average. The key finding, however, is that once again the effect on clothing expenditures is much larger (both absolutely and relatively) than the effect on food expenditures.

In Table 3, we report 2SLS estimation on subsamples of the unemployed. For readability, we focus on the food and clothing equations, which are our key comparison.

The numerical simulations in Section 2 assumed that households cannot borrow, and have no liquid financial assets to draw down. This is not likely true of all of the households in our sample. Thus, the first split of the data we consider is to divide households into those with and without liquid assets. Households without liquid assets are more likely to be liquidity constrained.[12] The top panel of Table 3 shows that unemployment benefits only have a

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[11] Full details are in the Data appendix

[12] This strategy of splitting the sample by financial assets, with those with low assets most likely constrained, follows Zeldes (1989), McCarthy (1995) and Browning and Crossley (2001). Households were classified according to their responses to the question: *Do you or someone in your household have any assets that you could draw on if it was really necessary? For example,*

statistically significant impact on the expenditures of households without assets. This mirrors the findings of Browning and Crossley (2001). However, the pattern of larger effects on clothing than on food remains.

We also split the sample on the basis of whether the lost job had provided more or less than 60% of household income (about the median value in the data). Unsurprisingly, we find that benefit effects are limited to households in which the lost job was important (by this definition). Presumably households with other incomes can smooth consumption in a variety of ways (for example, by borrowing against the incomes of other earners). Once again, however, we find larger effects of benefits on clothing than on food.

To summarize then, we find that marginal dollars of unemployment benefit income have statistically significant, but economically small effects on food, clothing and total expenditures. The effect of marginal dollars of benefits on clothing expenditures is twice as large in absolute terms (dollars) as the effect on food expenditures despite the fact the households in our sample spend a much larger fraction of their budget on food. We find that benefit effects are much stronger for households without liquid assets, and for households in which the lost job represented a significant fraction of household income. However, wherever we find benefit effects, we find that they are much larger for clothing, a small durable, than for food. These findings are consistent with the theory developed in the first half of this paper, which suggested

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money in the bank, savings bonds or RRSPs that are cashable, or insurance policies, etc. Please do not include fixed assets such as house, cars, boats, etc. An RRSP is a tax-favoured retirement savings account similar to a 401(k). Cash withdrawn from an RRSP is counted as taxable income in the year of the withdrawal. Because holding positive liquid assets at the interview date is surely endogenous, we also tried splitting the sample on the basis of whether the household reported investment income in the previous tax year. This led to similar, albeit less sharp, results. Liquid asset holdings prior to the job separation were recorded in the 1995 survey but unfortunately not in 1993.

that households in temporarily straitened circumstances would cut back primarily on durables (see Figure 1 in particular).

The final question we address is: could our finding simply reflect the mechanism discussed by Hamermesh and Parker? While our simulations assumed homothetic preferences in order to abstract from H-P effects, the households in our sample certainly have non-homothetic preferences, with clothing having a greater income (total expenditure) elasticity than food. Nevertheless, we do not think that H-P effects can explain our results. The (absolute dollar) benefit effect on clothing is about twice the effect on food, while food expenditures are 3 to 4 times greater than clothing expenditures in our sample. Thus if these effects were generated by the benefit effect on total expenditure operating through different income elasticities, clothing would have to be 7 times as income elastic as food. Budget studies (see for example Bils and Klenow, 1998) suggest that the ratio of clothing to food income elasticities is more in the range of 2 or 2.5.

To investigate this directly with our data we switch from modelling expenditures on food and clothing and instead model the effect of unemployment benefits on the structure of demand (conditional on total expenditure). To model demands, we use the simple and familiar Working-Leser form (budget shares linear in the logarithm of total expenditures and other controls). We include benefits (linearly) as an explanatory variable, and continue to instrument with potential benefits. These estimates are reported in Table 4. As expected, food and clothing have different income elasticities. Food is a necessity, with an income elasticity of about 0.5, while clothing has an income elasticity of just over 1. These numbers are typical of what is found in budget studies. However, even controlling for total expenditure, marginal dollars of benefit income have an

impact on the structure of demand. In particular, they increase the budget share of clothing. This result cannot be explained by H-P effects.

VI. Conclusion

In this paper we consider the question of *how* households in temporarily straitened circumstances cut back and how they spend marginal dollars of transfer income. Both our theoretical and empirical analysis emphasizes the importance of allowing for the fact that households buy durable as well as non-durable goods.

In the theoretical section we develop a neo-classical (discrete or continuous) durables model with irreversibility. In this model liquidity constrained agents absorb the impact of modest falls in transitory income almost completely by cutting back on expenditures on durables. Expenditures on non-durables are almost unchanged and we show that that this leads to small falls in current welfare. However, if income falls sharply enough so that the agent hits the irreversibility constraint, then the situation changes dramatically. Now all additional cuts in total expenditure must be met by cutting non-durable expenditures, and this leads to much larger falls in current welfare.

In our empirical work we exploit reforms in the Canadian UI system as a source of variation in transitory income among the unemployed – many of whom are in temporarily straitened circumstances. The data provide considerable support for the implications of our theoretical model. We find effects of marginal dollars of unemployment benefit income in food, clothing and total expenditures. These effects are much stronger for households without liquid assets, and for households in which the lost job represented a significant fraction of household income. Most importantly, the effect of marginal dollars of benefit on clothing expenditures is twice as large in

absolute terms (dollars) as the effect on food expenditure despite the fact the households in our sample spend a much larger fraction of their budget on food.

Our empirical results are consistent with the idea that our theoretical model formalizes: agents who have to cut back temporarily on total expenditure will choose to postpone replacing worn but serviceable clothing (socks, for example) rather than go hungry. The reason that they will do so, of course, is that worn but serviceable socks continue to provide a flow of services. For durables – even small durables – consumption is not equal to expenditure. Thus, for agents who own socks (and shirts, coats, pillows, sheets, towels, plates), a failure to smooth expenditures need not imply a failure to smooth consumption, especially in the short run.

Acknowledgments

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TABLES AND FIGURES

FIGURE 1:

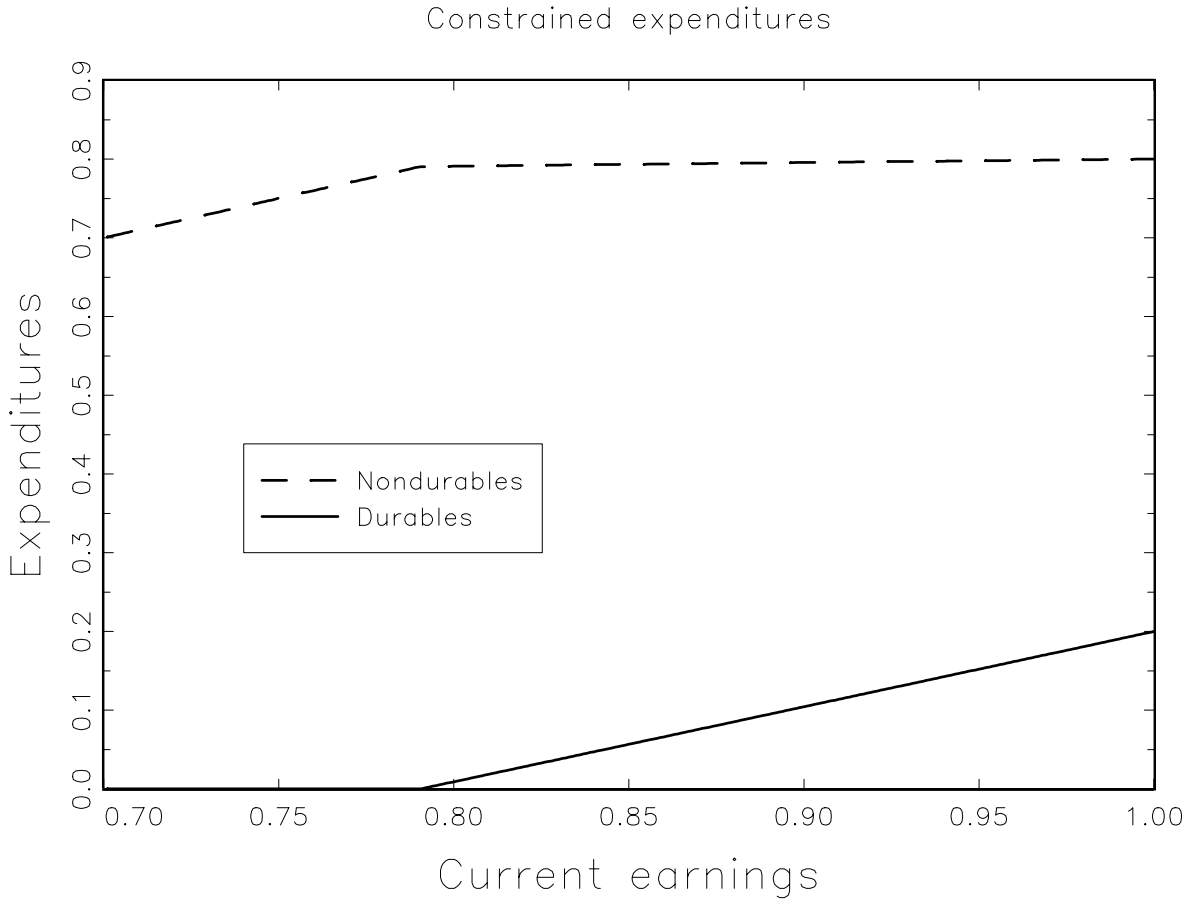

FIGURE 2:

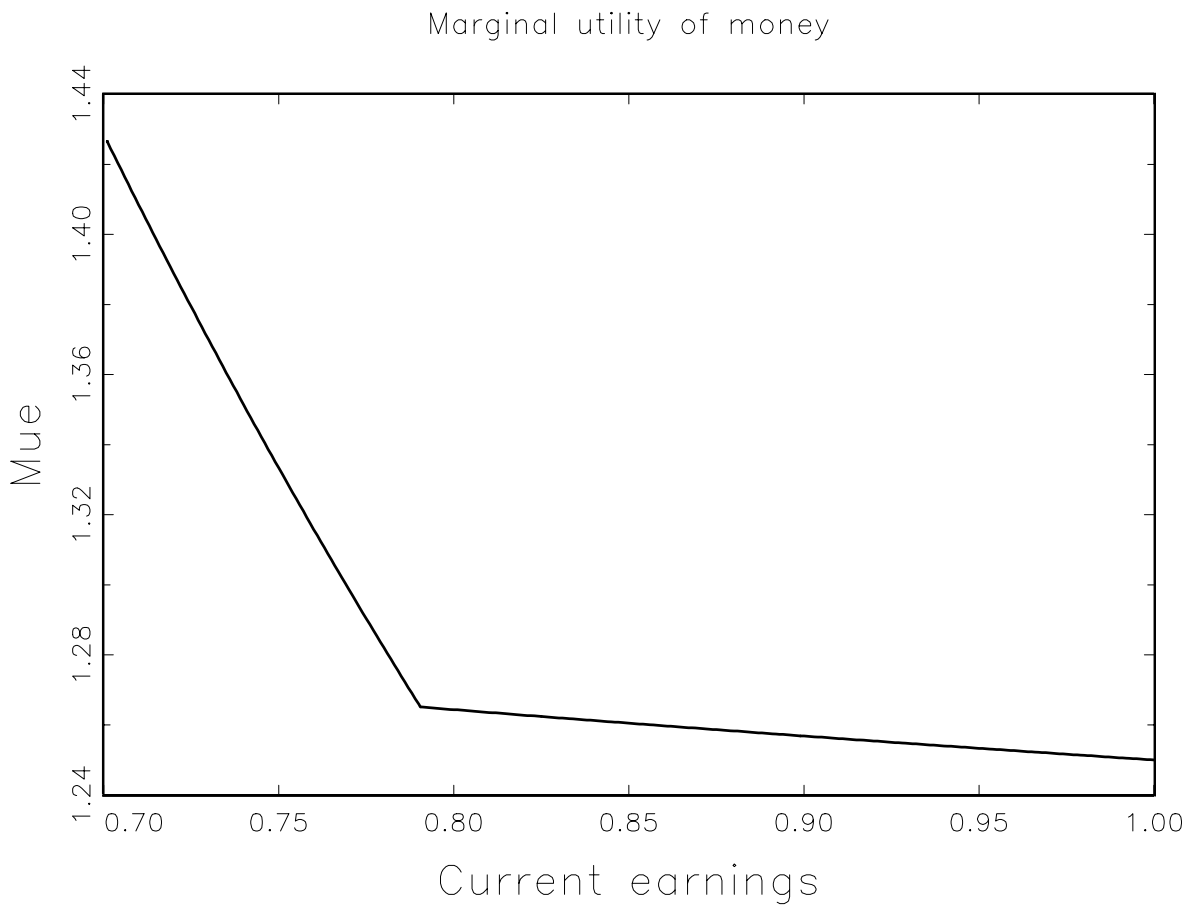
Marginal utility of money
Mue
1.24
1.28
1.32
1.36
1.40
1.44
0.70
0.75
0.80
0.85
0.90
0.95
1.00
Current earnings

TABLE 1: Summary Statistics for Expenditures

		Mean, unemployed n = 1959	Mean, employed n = 1198	T-test for equal means
Food at home	$/month per capita	143	144	0.37
	Budget share	0.24	0.22	-4.22
Clothing	$/month per capita	37	63	8.35
	Budget share	0.06	0.08	7.31
	Dummy for +ve expenditure	0.64	0.79	8.93
Total expenditure	$/month per capita	696	793	5.40

FIGURE 3: Engel Curves for Food

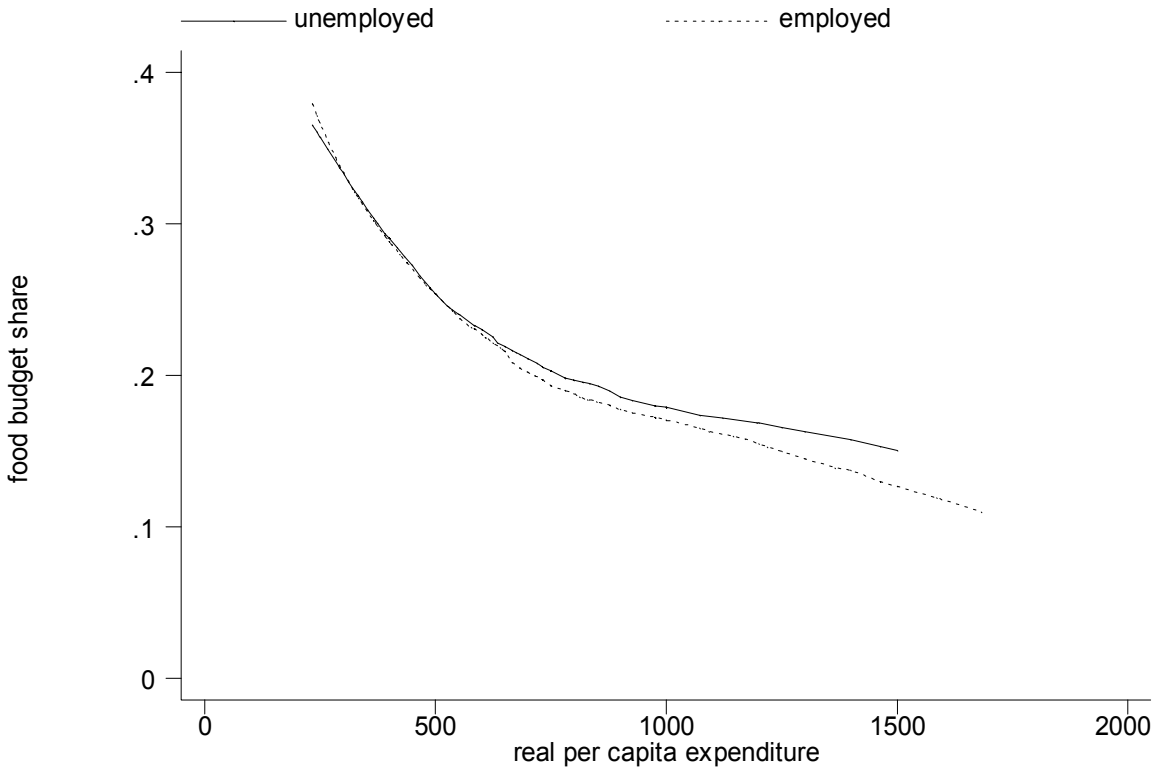

FIGURE 4: Engel Curves for Clothing

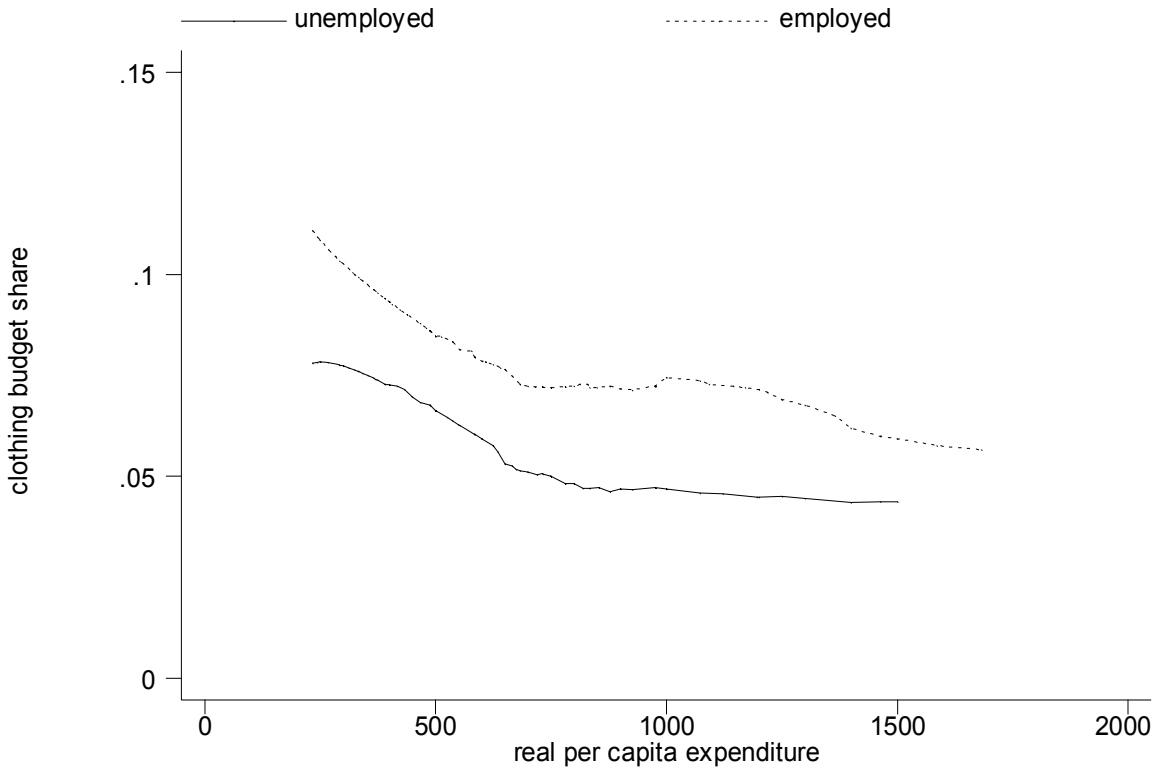

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TABLE 2: Quasi-experimental Estimates: Effects of UI Benefits on Expenditures
(Instrument = Potential Benefits)

	Food at Home	Clothing	Total Expenditure
Reduced Forms, Unemployed Sample (n=1959)			
Unconditional Mean of Expenditures ($)	362	102	1675
Estimated Coefficient on *Potential* Benefits	0.0060	0.037	0.011
T-statistic	[2.56]	[2.61]	[4.56]
Average Implied Marginal Propensity To Spend ($ per $100 of additional benefits)	2.2	4.5	12.9
Reduced Forms, Employed Sample (Omnibus Specification Test; n=1198)			
Unconditional Mean of Expenditures ($)	373	150	1872
Estimated Coefficient	-0.001	0.010	0.005
T-statistic	[-0.29]	[0.60]	[1.64]
Average Implied Marginal Propensity To Spend ($ per $100 of additional benefits)	-0.4	1.5	5.3
2SLS, Unemployed Sample (n=1959)			
Unconditional Mean of Expenditures ($)	362	102	1675
Estimated Coefficient on *Actual* Benefits	0.010	0.074	0.013
T-statistic	[2.55]	[2.83]	[2.89]
Average Implied Marginal Propensity To Spend ($ per $100 of additional benefits)	3.7	7.6	22.0

Notes:

1. T-Statistics based on robust standard errors.
2. Additional controls include the size and composition of the household; the age, education and gender of the respondent; regional and seasonal dummies; characteristics of the lost job and local labour market; dummies for homeownership and investment income in the previous year; measure of the importance of the lost job in household income; and a polynomial in the earnings in the lost job. Further details are provided in the Data Appendix, and complete results are available from the authors.

TABLE 3: Effects of UI Benefits on Expenditures – Subsample Analysis
(2SLS on Unemployed Respondents)

	Food at Home	Clothing	Food at Home	Clothing .
Liquid Assets at Interview (Household)	No (n=1278)		Yes (n=681)	
Unconditional Mean of Expenditures ($)	353	93	378	102
Estimated Coefficient on *Actual* Benefits	0.013	0.060	0.005	0.037
T-statistic	[2.65]	[2.16]	[0.90]	[0.91]
Average Implied Marginal Propensity To Spend ($ per $100 of additional benefits)	4.7	6.1	2.0	4.5
Lost job provided <=60% of household income	No (n=1018)		Yes (n=941)	
Unconditional Mean of Expenditures ($)	321	83	406	123
Estimated Coefficient on *Actual* Benefits	0.012	0.081	0.005	0.013
T-statistic	[2.34]	[2.86]	[0.82]	[0.29]
Average Implied Marginal Propensity To Spend ($ per $100 of additional benefits)	3.7	6.8	2.1	1.6

Notes:

1. T-statistics based on robust standard errors.
2. Additional controls include the size and composition of the household; the age, education and gender of the respondent; regional and seasonal dummies; characteristics of the lost job and local labour market; dummies for homeownership and investment income in the previous year; measure of the importance of the lost job in household income; and a polynomial in the earnings in the lost job. Further details are provided in the Data Appendix, and complete results are available from the authors.

TABLE 4: Effects of UI Benefits on the Structure of Demands

(2SLS on 1959 Unemployed Respondents, Selected Coefficients)

	Budget share of Food (at Home) Expenditures	Budget share of Clothing Expenditures
Estimated Effect of Log of Monthly Total Expenditure (C$)		
Coefficient	-0.115	0.0052
T – Statistic	[-8.44]	[0.44]
Implied Total Expenditure Elasticity	0.52	1.09
Estimated Effect of C$ 100 of *Actual* Monthly Benefits		
Coefficient	0.0006	0.0020
T-Statistic	[0.67]	[2.59]

Notes:

1. With the Working-Leser form (budget share linear in the logarithm of total expenditures) the total expenditure elasticity is $1+\beta/w$, where β is the coefficient on the logarithm of total expenditure and w is the budget share of the good in question. Because we observe zeros for clothing we calculate the elasticity at the mean budget share.
2. T-statistics based on robust standard errors.
3. Additional controls in the demand equation include the size and composition of the household; the age, education and gender of the respondent; regional and seasonal dummies; characteristics of the lost job and local labour market; dummies for homeownership and investment income in the previous year; measure of the importance of the lost job in household income; and a polynomial in the earnings in the lost job. Lagged incomes are excluded from the demand equations. Further details on control variables are provided in the Data Appendix, and complete results are available from the authors.

DATA APPENDIX

A. Reforms to the Canadian Unemployment Insurance System, 1993-4

The Canadian Unemployment Insurance (UI) program provides earnings related benefits of limited duration[1] to unemployed workers who qualify by having worked at least the minimum required number of weeks in the previous year. In recent years, the minimum number of weeks worked required to qualify has depended on local unemployment rates and ranged from 10 to 20 weeks. The duration of benefits has depended on both local unemployment rates and the number of weeks worked in the year prior to the unemployment spell and could be up to one year. Benefits are a fixed fraction (the *statutory replacement rate*) of earnings in the 20 weeks prior to the unemployment spell up to the *maximum insurable earnings.* For example, in 1992 the statutory rate was 60% and the maximum insurable earnings were $710/week, so that the maximum weekly benefits were $426. The system is financed by payroll taxes.

In our data period there were two sets of important legislative changes to the Canadian UI system[2], in 1993 and in 1994. These changes were introduced and enacted as Canada came out of the 1991 recession. Broadly, the two Acts were intended to finance a cut in payroll taxes (as a job creation strategy) while keeping the program's budget under control. The 1993 changes cut the statutory replacement rate from 60 to 57 percent of insurable earnings and disentitled individuals who, according to Human Resources Development Canada (HRDC)[3], either voluntarily quit their jobs or were dismissed with cause. Prior to this, 'quitters' were penalized by a 12 week waiting period and had their statutory replacement rate cut from 60 to 50%.

Four further changes to the system were introduced in 1994. This reform raised the minimum entrance requirement in high unemployment regions from 10 to 12 weeks (effective July, 1994). It contained a further cut in the statutory replacement rate (from 57% to 55%, also effective July, 1994). There was also a revision in the mapping from weeks of work and unemployment rates into weeks of benefit entitlement (effective April 1994). Finally, a new "dependency rate" was introduced. Individuals with dependent children and low insurable earnings (less than $390 per week) became eligible for a statutory replacement rate of 60%. This change was intended to shield poor families from the 1993/4 cuts in the general statutory rate. It also represented somewhat of a change in the philosophy of unemployment insurance in Canada since previously benefits had been tied strictly to contributions and not to need. Table A1 summarizes the sources of variation in potential UI benefits, in the period captured by our data.

B. The Canadian Out of Employment Panels

To evaluate the impacts of the 1993 reform, HRDC commissioned a panel survey of individuals who separated from jobs in windows before and after the reform came into force on

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[1] Canadians are also eligible for social assistance benefits, which are of unlimited duration. These benefits depend on family type and other measures of need rather than past earnings or contributions. There is a means test (on assets) and a high implicit tax on earnings.

[2]The previous legislative changes were in 1989. A subsequent reform in 1996 renamed Unemployment Insurance "Employment Insurance" and moved the system from a weeks worked basis to an hours worked basis.

[3]The federal department responsible for unemployment insurance.

April 3rd, 1993. Each window was about 6 weeks long. To avoid issues of strategic filing, the first window ended about one month before the effective date of the bill and the second window began about one month after the effective date of the bill. This survey has come to be called the 1993 Canadian Out of Employment Panel (COEP). Respondents from the first sampling window constitute "cohort 1" and those from the second window constitute "cohort 2". Because the cohorts were separated by the policy change, and subject to different UI rules, the data have a "quasi-experimental" structure.

In Canada, employers are required to submit a Record of Employment (ROE) whenever a job separation occurs. Approximately 6 million such forms are issued each year. The sampling frame for the COEP is the population of individuals receiving an ROE form in one of the two window periods, and having a Social Insurance number that ends with a particular digit. We refer to the job whose end led to an individual's inclusion in our sampling frame as "the reference job". The ROE form contains a reason for separation. All reasons were sampled except for participation in a Work Sharing program, apprenticeship, and retirement at age 65. Approximately 6,000 separations were sampled in each window.

Each respondent was interviewed by phone three times, at about 26, 39 and 60 weeks after the reference job separation. The average interview length was 25 minutes for the first interview. Subsequent interviews were shorter. The long lag to the first interview is imposed by the time it takes all the administrative records that form the sampling frame to become available. This lag means that only a selected sample of respondents are observed in unemployment; we discuss this issue in the text.

This survey information is then merged with UI administrative information from HRDC and with administrative earnings data from the current and previous years for the respondent and his or her spouse (if married). The picture of the circumstances of households that contain someone who has separated from a job which is provided by these data is unprecedented in its detail. For example, the administrative data give the exact benefit and entitlement period for every respondent; these are usually very badly measured in surveys (or imputed from state level averages). Conversely the earnings data allow us to control, for example, for the labour supply and income of the spouse which is typically missing from administrative data. Finally, the survey provides information concerning expenditures, search measures, demographics, and other variables that are never observed in administrative data. Moreover, there are often two or three independent measures of the same quantity in the multiple data sources (an example is past earnings) which allows for the correction of measurement error.

In 1995, HRDC commissioned a second survey of individuals separating from jobs. The 1995 COEP sampled approximately 4000 ROE's in each of two windows, timed roughly to correspond to the 1993 sampling windows. We refer to these samples as cohorts 3 and 4. There were no policy changes between cohorts 3 and 4, so they provide a seasonal control for cohorts 1 and 2. In addition, cohorts 2 and 4 provide a before and after framework for the evaluation of the 1994 policy changes. In the 1995 COEP, respondents were only interviewed twice, at approximately 36 and 60 weeks. Sampling of separation reasons was more restricted than in 1993 with further minor categories excluded; the only groups sampled had separations because of 'short work', 'voluntary quit', 'dismissal', 'illness' and 'other'. The survey questionnaire was revised somewhat in light of the experience with the 1993 COEP, but considerable care was taken to ensure backwards comparability.

C. Samples

While the 1993 and 1995 COEP together comprise some 20,384 respondents (12,490 in cohorts 1 and 2 (1993) and 7,894 in cohorts 3 and 4 (1995)), we work with a sample which is restricted in several important ways. First, we restrict the sample to separation reasons "short work" (about 50% of separations), "voluntary departures" or “quits” (almost 20%), “dismissals” (some 5%) and the approximately 20 percent labelled "other". These last represent the second largest single category of separations. Discussions with HRDC staff suggest that this group is similar to the “short work” group; our investigations support this and we commonly pool them. This leaves us with 11,228 observations from cohorts 1 and 2 and 7,573 observations from cohorts 3 and 4.

Second, we focus on respondents between the ages of 20 and 60. This reduces the 1993 sample to 10,528 and the 1995 sample to 7195. In addition we select respondents from three family types: singles, couples and couples with children and/or others. Single parents and young individuals living with parents or unrelated adults are the primary groups excluded. Though these latter groups are not unimportant, we found in preliminary analysis that it was difficult to capture adequately the heterogeneity of responses in a pooled sample. Furthermore, the quality of responses to questions about household income and expenditure were very poor among respondents living with parents or unrelated adults.[4] The family types we do consider comprise 6,750 respondents in 1993 and 5,676 in 1995.

Finally, we focus on those individuals who are unemployed[5] at the time of the first interview: this is the group who are likely to have current earnings below “permanent” earnings, and for whom UI benefits (if any) provide a good measure of current ‘earnings’. Of course, because the first interview occurs some time after the separation date this cuts down our sample size: we have 3,132 respondents in 1993 and 1,557 respondents in 1995. This selection introduces a possibility that our results will be biased by sample selection; we discuss this in the text.

In addition to these sample selections, our estimating sample is further reduced because we are forced to discard observations for which the expenditure information is missing or inconsistent and observations which do not have a complete set of information. This leaves us with a final sample of 1,959 respondents (1162 in 1993 and 797 in 1995). The incidence of incomplete records is quite high, but this reflects the fact that we are merging data from four sources (the survey responses, plus three different HRDC administrative files). We consider it the acceptable cost of the very rich set of information we are able to use.

In addition to this sample of unemployed individuals we also construct a `control’ sample of 1198 workers who report that they are back in a steady job at least as good as the one that they separated from. In our empirical work, we will use this latter group for two purposes. First, we use them to correct for a survey design flaw; see the next section. Second, we use them to test for the exogeneity of our instruments. Table A2 presents summary statistics on both our unemployed and employed (`control’) samples

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[4] See also the evidence in Browning, Crossley and Weber, 2003, particularly Table 2 and the associated discussion

[5] That is, we exclude both the employed and those who report withdrawal from the labour market.

D. The Expenditure Questions

One novel feature of the survey information is that we ask questions concerning expenditures. In 1993 we ask about housing costs (weekly or monthly), food at home (weekly), food outside the home (weekly), clothing (monthly) and total expenditure on everything (monthly). In 1995 the survey period for food inside the home and outside was changed to monthly. The food at home, clothing and total expenditure variables constitute our 'left hand side' variables. In particular we are interested in how the level of total expenditure and the structure of demand varies with the UI benefit level. Since the use of expenditure questions outside expenditure surveys is relatively rare there is a concern about the reliability of the responses. To address this we investigated the expenditure patterns for households in which the COEP respondent is back in what is self-reported to be steady employment at a job at least as good as the old one. These responses were compared to data drawn from the Canadian Family Expenditure Survey (FAMEX) which provides high quality information on household income and expenditures. The conclusion from this analysis is that we are recording something like the 'true' values (albeit with noise and considerable rounding).[6] For example, for the COEP we find that even when we control for current and lagged household income and a wide range of other variables, family size has a highly significant effect on both total expenditure and food expenditures. Moreover this effect is very similar to the effect found in the FAMEX data. In our empirical work, we find additional evidence that our consumption information seems quite reliable.

We also have to address a serious problem in our survey design. As noted above, the survey period for food at home is 'weekly' in 1993 and 'monthly' in 1995. It is tempting to take the 1993 figure and simply multiply it by 4.2 to make it comparable to the 1995 figure. However, there is evidence that increasing the survey period for self-reported expenditures lowers the figure reported once the responses are scaled to a common period; see Deaton and Grosh (2000). This is a problem for us since we use the policy variation between 1993 and 1995 to help in the identification of the effects of changing benefit levels. Since this may be confounded with spurious changes in reported expenditures on food at home, using the unadjusted figures could lead to serious bias. To overcome this, we conducted an analysis of the 'food at home' demand pattern for those back in a 'good' job (the group referred to at the end of the previous sub-section). The results indicate that the 1993 food at home figure needs to be scaled down by about 13% to compensate for the change in survey design; we make this correction everywhere in our empirical work.

[6] Some of this analysis is reported in Browning, Crossley and Weber (2003).

Additional References

Browning, M., T.F. Crossley and G. Weber, (2003). "Asking Consumption Questions in General Purpose Surveys," *The Economic Journal*, 113: F540-F567.

Deaton, A. and Grosh, M. (2000). 'Chapter 15: Consumption' in Glewwe, P. And Grosh, M., (eds.) *Designing Household Survey Questionnaires for Developing Countries: Lessons from Fifteen Years of the Living Standards Measurement Study,* Vol. 1, Washington, D.C.: The World Bank.

TABLE A1: Variation in Potential Benefit Variables.

1. Potential Benefit Formula.

$$\mathbf{I1_t(iw_{it,},lur_{it})*I2_t(iw_{it,},lur_{it})*RR_t*max[ie_{it}, mie_t]}$$

Individual Characteristics:
$\boldsymbol{iw_{it,}}$: insured weeks in the year prior to separation.
$\boldsymbol{ie_{it}}$: insured earnings
$\boldsymbol{lur_{it}}$: local unemployment rate

Potential benefits are a highly nonlinear function of these individual characteristics. In our expenditure regressions we condition on $iw_{it,}$, lur_{it} and a polynomial in ie_{it} in order to capture correlations of these variables with `permanent income'. The residual variation in the instrument (potential benefits) comes from the nonlinearity of the potential benefit formula, and more importantly, the changes to that formula outlined below.

Policy Functions and Parameters:
$I1_t$: indicator function =1 if eligible, 0 otherwise.
$I2_t$: indicator function = 1 if weeks of entitlement exceed time between separation and interview.
RR_t: statutory replacement rate.
MIE_t: maximum insurable earnings.

2. Changes in Potential Benefit Formula

RR_t: Statutory rate cut from 60% to 57% between cohort 1 and 2 and to 55% between cohorts 3 and 4. In cohorts 3 and 4 individuals with dependents and low incomes were entitled to a replacement rate of 60%.
MIE_t: The maximum insurable earnings rose by 10% in real terms over the period covered by the data. This completely offset the fall in the statutory rate for those with earnings above the insurable maximum.
$I1_t$:$I2_t$: The mappings from insured weeks and local unemployment rates in qualification and entitlement were reformed.

TABLE A2: Summary Statistics		
Variable	Mean, Employed n = 1198	Mean, Unemployed n = 1959
dummy for high school graduate	0.43	0.40
dummy for tertiary education	0.32	0.29
age (years/40+0.5)	1.40	1.44
age squared also included in regressions		
log of household size	0.92	0.90
dummy for children	0.47	0.48
dummy for single female	0.07	0.08
dummy for male with spouse employed at separation	0.17	0.18
dummy for female with spouse employed at separation	0.04	0.08
dummy for male with spouse not employed at separation	0.30	0.22
dummy for female with spouse not employed at separation	0.30	0.30
(omitted category: single male)		
Region dummies (omitted category: Ontario)		
Atlantic	0.10	0.13
Quebec	0.30	0.27
Prairies	0.14	0.16
B.C.	0.10	0.11
Local unemployment rate at job separation	0.10	0.11
month dummies (to control for seasonality)		
September	0.11	0.19
October	0.13	0.19
November	0.33	0.30
December	0.09	0.07
January	0.20	0.13
February	0.09	0.04
(weeks elapsed between separation and interview)/52	0.66	0.58

TABLE A2: Summary Statistics (Cont'd)		
dummy for home ownership	0.64	0.61
dummy for some employment between separation and interview (dropped from regressions on employed sample)	1.00	0.36
fraction of household income provided by ROE job, prior to separation	0.71	0.60
dummy for separation expected	0.14	0.16
dummy for seasonal job	0.63	0.52
dummy for job tenure > 1yr	0.27	0.23
dummy for managerial occupation	0.44	0.39
dummy for bluecollar occupation	0.58	0.54
(insurable weeks)/52		
dummy for UI use in previous 2 years	0.60	0.62
respondent's income lagged one year	1.25	1.05
respondent's income lagged two years	1.17	0.99
household income lagged one year	1.68	1.49
household income lagged two years	1.56	1.42
respondent's earnings in lost job	0.43	0.46
higher order polynomials in these variables also included		

Notes:
1. All monetary amounts (expenditures, incomes and benefits) are measured in 1000s of 1993 Canadian dollars per month.