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The Long Run Costs of Job Loss as Measured by Consumption Changes.

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VERY PRELIMINARY: Please do not quote without permission of the authors; comments very welcome.

Abstract: The costs of involuntary job loss are an object of substantial research and policy interest. We consider the measurement of the costs of job displacement with household expenditure data. We explicitly derive a "difference-in-difference" estimator from a structural life cycle model. This exercise emphasizes questions about the appropriate counter-factual and control group, about the parameter of interest in the presence of heterogeneity and about identifying conditions. We argue that studies based on earnings or wages suffer from similar problems. In the empirical portion of the paper, we use a relatively new Canadian survey of individuals who experienced a job separation to examine consumption growth across different kinds of job separations. Our preliminary findings are that permanent layoffs have consumption growth that lags one or two percentage points behind temporary layoffs, but that this gap is not strongly correlated with individual or household characteristics.

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1. Introduction.

What are the costs of permanent involuntary job loss? This question has spawned an extensive literature. "Displaced" workers are of interest because they bear the costs of labour reallocation in a dynamic economy. Equity and political economy considerations dictate that we be concerned with their experiences. These workers are the target of considerable public policy including adjustment assistance (job search counselling, retraining and income replacement) and advanced notice requirements.

Previous studies of the costs of permanent involuntary job loss have focussed on wages, earnings, and duration of joblessness. In this paper we approach this question from a the point of view of a life-cycle model of consumption. This leads to two innovations.

The first contribution is that we consider an alternative measure of the costs of job loss: changes in the level of household expenditures with separation. If agents are forward looking, current expenditures summarize information they have about future earnings prospects and thus changes in expenditures associated with job loss may provide a measure of the "full" economic cost of displacement from a short data series. Of course, the corollary of this is that the problem of losses which occur before the beginning of the data series is exacerbated by the fact that, in the case of expenditures, the losses only need be anticipated (not realized) prior to the beginning of the data series to be missed. However, our data do contain information on advanced notice and expectation of layoff, which we can exploit to investigate the importance of this issue. A second advantage of examining expenditures is that expenditures are closer than income to the object of ultimate interest: household welfare. Consumption will more directly reflect household welfare particularly where other earners are present in the household or where there are opportunities for inter-temporal smoothing. Of course, this comes at a cost as well. Because expenditures reflect optimizing behaviour by households, differences in expenditure changes across households represent not only heterogeneity in changes in expected wealth, but also heterogeneity in preferences (for example, individuals discount rates). However, it is important to note that this should be a concern in studies that measure the costs of displacement by wages and earnings as well.

If wages are determined by human capital considerations, and labour markets operate as frictionless spot markets, then indeed, wages will reflect only individuals' productive characteristics and wage changes will reflect only changes in human capital. However, such a model is demonstrably inadequate: it is not consistent with the substantial spells of unemployment experienced by displaced workers. However in model which is consistent with unemployment - a search model - *wages* reflect optimizing behaviour by households. Thus differences in wage changes, like expenditure changes, will reflect heterogeneity in preferences (for example, discount rates) as well as heterogeneity in the loss of productive capacity. In our empirical work, we will provide some evidence that wage changes reflect preferences as well as productivity changes.

The second contribution that flows from beginning from a structural behavioural model is the focus which that procedure brings to questions regarding the counterfactual and parameters of interest, and identification issues. In particular we explicitly derive from the structural model a "difference-in-difference" estimator and thus are completely transparent about the assumptions required for identification. In that sense our work is in the same spirit as Blundell, Duncan and Meghir (1998), who in a study of British tax reforms derive a difference-in-difference estimator from a structural model of labour supply.

The current paper is a contribution to at least three literatures. First, it is a natural complement to earlier work by ourselves (1999a,b) and Gruber (1997) on the short run costs of job loss and benefits of Unemployment Insurance. That work is concerned with transitory changes in income and welfare, and with liquidity constraints, where as the current paper is concerned with the long run effects of job loss. The second relevant literature is the extensive literature on job displacement and displaced workers, as surveyed by Kletzer (1998) and Fallick (1996). Finally, the current paper is related to tests of full insurance and consumption growth around idiosyncratic shocks such job loss, unemployment, illness and disability. Examples of such work are Cochrane (1991) and more recently Stephens (1999).

The next section presents our theoretical framework. Section 3 introduces our data. Results are presented in Section 4 and Section 5 concludes.

2. Theory.

The sample we use in our empirical work consists of a group of workers who had a job separation in one of four specified time windows. Some of these separations were quits to take other jobs, some were temporary layoffs and some were a permanent lay-off. Workers who separated for reasons other than these (for example, retirement or return to school) are excluded from our analysis (precise details of the definitions will be given below). We shall be using changes in expenditures between the month before the job separation (period t) and a month over one year later (period $t+s$) to infer the 'long run economic impact' of a permanent lay-off relative to the other outcomes. The purpose of this theory section is to present with as little formality as possible the identification assumptions we use and the possible problems with them. Our theory analysis proceeds in three parts. First, we discuss the circumstances under which we can identify the 'long run' impact with changes in the marginal utility of expenditure (*mue*). Then we discuss how we can implement a differences in differences estimator to identify the long run losses from a permanent lay-off relative to other outcomes and how this varies with observables such as age and tenure in the lost job. Finally, we examine the link between the (unobservable) change in the mue and the (observable) change in total expenditure.

As we shall see, our identification of the long run losses is fraught with hazard so that a reader might conclude that it would be best to simply abandon this approach and rely on a conventional approach that looks at changes in wages, labour supply and/or earnings. The first problem with the conventional approach is that to estimate the long run consequences from point in time estimates of changes we need a model for the dynamics of wages and employment; the great virtue of the consumption approach is that (under given assumptions) current consumption is a sufficient statistic for beliefs about everything in the future. Second, we need a model to relate individual earnings to the welfare of individuals living in households. Finally, we shall argue at the end of this section that even though there are real problems in using consumption changes to infer long run effects, the use of these other measures such as wages and earnings is subject to many of the same problems.

Although the use of the change in the marginal utility of expenditure as a measure of the long run economic impact of a permanent lay-off is natural in a life-cycle framework it does have its problems. First, the pre-separation *mue* already accounts for some of the job-loss shock if the separation is partially or wholly anticipated. Since we are interested in the full impact of a separation, simply looking at the change in the *mue* from immediately before the job loss may cause us to underestimate the long run loss. In our empirical work we shall attempt to deal with this by using measures in the data that describe how expected the job loss was. There are also problems with using the post-separation *mue*. In particular, if agents are liquidity constrained in the post-separation period then the period *(t+s) mue* is higher than the expected future *mue*. This could come about for two reasons. First, if the agent is still unemployed, then current income may be considerably below 'permanent' income and agents with no assets or access to credit may simply have to reduce consumption. We deal with this problem by only considering agents who are more than a year away from the separation and all of whom have a new job. Although this helps, some of this group may be also constrained. Suppose, for example, that the effect of a lay-off is to move a worker on to a temporarily lower wage path which will later 'catch up' again with their pre-separation wage trajectory. An unconstrained worker would then set period *(t+s)* consumption higher than an otherwise identical worker who cannot draw on savings nor borrow to exploit the expected future recovery. Thus the effects of liquidity constraints in the post-separation period will be to bias our estimates of the job loss shock downwards; we shall return to this in the empirical section.

We now develop some theory in a simplified context with only two labour market outcomes; below we shall extend this framework to the four regimes we observe. The usual Euler equation for an individual agent *h* between periods *t* and *t+1* is given by:

$$\lambda_{h,t+1} = \lambda_{h,t} + \epsilon_{h,t+1} \qquad \textit{with} \quad E(\epsilon_{h,t+1} \mid \Omega_{h,t}) = 0 \tag{1}$$

where $\lambda_{h,t}$ is the marginal utility of money at time t and $E(.|\ \Omega_{h,t})$ is the expectations operator conditional on the information set at time *t*. We shall now put more structure on the surprise term $\epsilon_{h,t+1}$; this extra structure is tailored to our primary interest in the long run effect for a given worker of a firm demand shock. Let the firm demand shock be denoted d_h, and let all other uncertainty be captured by a set of random variables η_h which includes both macro shocks and idiosyncratic shocks. For simplicity we assume that d_h is a binary random variable with values zero, leading to a lay-off, or unity, leading to continuing employment. The variables $(d_h,\ \eta_h)$ have some joint distribution; we do not assume independence. In general a particular realisation of

$(d_h,\ \eta_h)$ will lead to a revision to the *mue:* $\epsilon_{h,t+1}\ =\ G^{\,h}(d_h,\ \eta_h)$. The exact mapping depends on many things including preference parameters (for example, how risk averse and prudent the agent is); the impact of the new information on beliefs about future outcomes and the agent's current beliefs.[1] For the question "what is the effect of a demand shock on the *mue*" to be meaningful we have to have the following separability between the two random variables:

$$G^{\,h}(d_h,\ \eta_h)\ =\ \phi^h(d_h)\ +\ g^{\,h}(\eta_h) \tag{2}$$

so that the effect of a demand shock is independent of the realisation of the other shocks. Given this we have, taking into account that d_h is binary:

$$\epsilon_{h,t+1}\ =\ (1-d_h)\Gamma_h^0\ +\ d_h\Gamma_h^1\ +\ g^{\,h}(\eta_h) \tag{3}$$

where Γ_h^J is the revision to the mue induced solely by the demand shock *J*. The size and determinants of these "demand shock revisions" are the principal focus of this paper. Below we shall discuss how we identify the shocks associated with various outcomes and how these vary with observables such as age, local labour market characteristics and tenure on the time *t* job.

To proceed we need to take iterated expectations so we introduce some more notation. The operator $E^{\,\eta}(X)$ denotes the expectation of *X* over the marginal distribution of η and the operator $E^{\,\eta|d}(X|\ d{=}J)$ denotes the expectation of *X* over the conditional distribution of η, given *d=J*, and the operator $E^{\,\eta,d}(X)$ denotes the expectation over the joint distribution. Note that in this

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[1] In particular, a realisation $(d_h,\ \eta_h)$ that gives $G^{\,h}(d_h,\ \eta_h)\ =\ 0$ is said to be *anticipated* by agent *h*. That is, the realisation leaves the *mue* unchanged so that an agent would not want to go back and change the time *t* action given the time *t+1* information. Note that an anticipated realisation is different from the mathematical expectation (unless we have *certainty equivalence*).

notation the Euler equation orthogonality condition is written as: $E^{d,\eta}(\epsilon_{t+1}) = 0$. Taking means conditional on demand shocks through equation (4) we have (dropping the h subscripts for the moment):

$$E^{\eta|d}(\epsilon_{t+1}|\ d{=}J) = \Gamma^{J} + E^{\eta|d}(g(\eta)|\ d{=}J) = \Gamma^{J} + \mu^{J} \quad for\ J = 0,1 \tag{4}$$

If we now take the mean of this over the outcomes *d* we have, using iterated expectations:

$$\begin{aligned} E^{d,\eta}(\epsilon_{t+1}) &= E^{d}(E^{\eta|d}(\epsilon_{t+1}|\ d)) \\ &= E^{d}(d(\Gamma^{1} + \mu^{1}) + (1-d)(\Gamma^{0} + \mu^{0})) \\ &= \pi(\Gamma^{1} + \mu^{1}) + (1-\pi)(\Gamma^{0} + \mu^{0}) \end{aligned} \tag{5}$$

where π is the probability of $d = 1$ given time t information. Thus the Euler equation condition implies that for a given individual the expectation of $(\Gamma^{J} + \mu^{J})$ is zero:

$$E^{d,\eta}(\epsilon_{t+1}) = = \pi(\Gamma^{1} + \mu^{1}) + (1-\pi)(\Gamma^{0} + \mu^{0}) = 0$$

Note that one implication of the Euler equation is that “no news is good news”. So long as a worker faces an *ex ante* risk of layoff ($\pi \neq 1$), the realization of continued employment must result in a positive revision to the *mue*.

All of this relates to one individual. We now go on to account for heterogeneity and derive the implications for the population and for various sub-samples. We assume that there are some time t observable characteristics $Z_{h,t}$ and write the demand shock revisions in the following random coefficients model form:

$$\Gamma_{h}^{J} = \gamma_{0}^{h,J} + Z_{h,t}^{/}\gamma^{h,J} \quad for\ J = 0,1 \tag{7}$$

Given this formulation, we can consider means over different samples. For example, the population mean conditional on Z is given by:

$$\begin{aligned} E^{h|Z}(\Gamma_h^J|\ Z) &= E^{h|Z}(\ \gamma_0^{h,J}\ +\ Z^{/}\gamma^{h,J}|\ Z) \\ &= E^{h|Z}(\gamma_0^{h,J}|\ Z)\ +\ Z^{/}E^{h|Z}(\gamma^{h,J}|\ Z) \end{aligned} \quad \textbf{(8)}$$

As has been extensively discussed in the treatments literature, this is not a very interesting estimate since it takes no account of the probability of experiencing outcome J: why should we be interested in the mean over a population including some who will never experience the outcome? Instead, we focus attention on those who actually experience the outcome J:

$$\begin{aligned} E^{h|Z,d}(\Gamma_h^0|\ Z,\ d=0) &= E^{h|Z,d}(\ \gamma_0^{h,0}\ +\ Z^{/}\gamma^{h,0}|\ Z,\ d=0) \\ &= E^{h|d}(\gamma_0^{h,0}|\ d=0)\ +\ Z^{/}E^{h|d}(\gamma^{h,0}|\ d=0) \\ &= \gamma_0^0\ +\ Z^{/}\gamma^0 \end{aligned} \quad \textbf{(9)}$$

(and similarly for outcome $J=1$). That is, we take as our 'parameters of interest' the mean impact of experiencing an outcome for those who actually experienced that outcome, here parametrised as γ_0^J and γ^J for outcome J. Generally the means in equations (7) and (8) will differ. One conventional set of conditions that makes them equal is 'no heterogeneity in the slope parameters' ($\gamma^{h,J} = \gamma^J$) and no 'sample selection bias through the error term' ($E^{h|d}(\gamma_0^{h,J}|\ d=0) = E^{h|d}(\gamma_0^{h,J}|\ d=1)$) but these (strong) assumptions will not be needed below.

Combining equations (1), (3) and (8) we have (for outcome $d=0$):

$$E^{h|Z,d}(\Delta\lambda_{h,t+1}|\ Z_{h,t},\ d_h=0)\ =\ \gamma_0^0\ +\ Z_{h,t}^{/}\gamma^0\ +\ E^{h|Z,d}(g^{\,h}(\eta_h)|\ Z_{h,t},\ d_h=0) \quad \textbf{(10)}$$

Before presenting our identification assumptions it is convenient to first present the link to consumption.

The link between changes in the marginal utility of expenditure and changes in expenditures is mediated by several factors. First, if preferences between consumption and labour supply are non-additive (because of the costs of going to work or the possibility of substituting home production for market purchases) then we cannot directly infer what is happening to the *mue* by observing consumption. We deal with this in some specifications by only considering agents who are employed in the pre-separation and post-separation period and then controlling for labour supply. Second, there will be consumption growth (or contraction) if the (after tax) real interest rate diverges from the discount rate. Third, if there are changes in the composition of the household then this will lead to changes in consumption, even if the *mue* is held constant. Fourth, we shall be considering total expenditure and not consumption; in particular, the link between the purchase of durables and the *mue* is not immediate. More specifically, if agents run down stocks of durables during an unemployment spell (see Browning and Crossley (1999b)) and then 'stock up' again when they return to work then recently re-employed agents may have unusually high expenditures. Fifth, preferences may not be additive over time. If agents have habits then they will 'rationally' adjust consumption downwards slowly following a sudden and large negative shock. Note, however, that we are not using contiguous months but pairs of months that are over a year apart so that much of the adjustment may have been made. Finally (and closely related to the last point), there may be fixed outgoings for the household that may be subject to slow adjustment. To control for this in our empirical work we control for the percentage of expenditures that are 'fixed'.

Denote log consumption by household *h* at time *t* by $c_{h,t}$. We assume[2]:

[2] The following assumption that log consumption is a linear function of demographics and the mue can be given a formal utility theoretic justification. See Browning and Crossley (1999a).

$$\Delta c_{h,t+1} = \alpha_h + \Delta Z^{\prime}_{h,t+1}\beta + \Delta(\lambda_{h,t+1}) \qquad \textbf{(11)}$$

where the α_h term allows for different households having, for example, different discount rates. Note that this heterogeneity may be related to Z. The second term allows that changes in demographics may lead to changes in consumption even if the marginal utility of money is held constant. Combining equations (9) and (10) we have our basic estimation equation:

$$\begin{aligned} E^{h|Z,d}(\Delta c_{h,t+1}|\ Z_{h,t},\ d_h{=}0) = \gamma_0^0 + Z^{\prime}_{h,t}\gamma^0 + E^{h|Z,d}(\Delta Z_{h,t+1}|\ Z_{h,t},\ d_h{=}0)^{\prime}\beta\ + \\ E^{h|Z,d}(\alpha_h|\ Z_{h,t},\ d_h{=}0) + E^{h|Z,d}(g^{h}(\eta_h)|\ Z_{h,t},\ d_h{=}0) \end{aligned} \qquad \textbf{(12)}$$

The final two terms on the right hand side may not have mean zero and may depend on time t demographics or the realisation d_h. For example, the impact of the "non-demand shock" $g^{h}(\eta_h)$ may be depend on $Z_{h,t}$ because there is a macro shock and different agents have different reactions to this macro shock. In a long panel we could assume that these shocks have mean zero but since we have only four time windows we cannot rely on this source of identification. Another source of dependence would arise if the non-demand shock is correlated with the realisation of d_h ("sample selection"). Or the consumption growth term α_h may depend on demographics (such as age). Because of this we cannot use a “difference” estimator and simply regress log consumption changes on $Z_{h,t}$ and $\Delta Z_{h,t+1}$ (with the implicit assumption that the latter was perfectly anticipated and uncorrelated with the realisation) to recover estimates of the parameters of interest $(\gamma_0^0,\ \gamma^0)$. With a “difference-in-difference estimator” we may be able to identify the differences in the effects of the demand shock $(\gamma_0^1-\gamma_0^0,\ \gamma^1-\gamma^0)$. Note again that the Euler equation implies that the effect of a demand shock is not zero for those that are not laid off. From equation (11) it will be clear that the supplementary identifying assumptions we need for this are that the final three terms in that equation do not depend on the realisation. If we further assume that this dependence on the demographics is linear and that the changes in the demographics, $\Delta Z_{h,t+1}$, were perfectly anticipated we have:

$$E^{h|Z,d}(\Delta c_{h,t+1}|\ Z_{h,t},\ d_h = J) \ = \ \gamma_0^J \ + \ Z_{h,t}^{\prime}\gamma^J \ + \ \Delta Z_{h,t+1}^{\prime}\beta \ + \ \delta_0 \ + \ Z_{h,t}^{\prime}\delta \tag{13}$$

Substituting for the outcomes and rearranging this we have:

$$\begin{aligned} E^{h|Z}(\Delta c_{h,t+1}|\ Z_{h,t},\ d_h = J) \ = \ d*[(\gamma_0^1 - \gamma_0^0) \ + \ Z_{h,t}^{\prime}(\gamma^1 - \gamma^0)] \ + \\ \Delta Z_{h,t+1}^{\prime}\beta \ + \ (\delta_0 + \gamma_0^0 \ + \ Z_{h,t}^{\prime}(\delta + \gamma^0) \end{aligned} \tag{14}$$

Thus a simple dummy variable transformation allows us to consistently estimate $(\gamma_0^1 - \gamma_0^0,\ \gamma^1 - \gamma^0)$.

In our empirical work below we consider four groups of workers who all experienced a job separation: "quits to take another job"; "temporary lay-offs with a definite recall date"; "temporary lay-offs with no definite recall date" and "permanent lay-offs". To accommodate this we allow that the demand shock above is actually a composite of new information about the state of demand in the existing firm and about outside opportunities. The two types of information may be correlated; for example, an industry wide shock may lead to a negative shock for the current firm and a fall in outside offers. The probability of quitting to take another job is correlated with both the outside offer and the firm specific demand shock; if a bad demand shock is received then the worker may be more likely to quit to take another job. Thus some of the group who record that the job separation was due to a quit to take another job also experienced a bad demand shock that would have lead to a permanent lay-off. Ideally we would like to identify the long run effects of a bad demand shock but that does not appear possible with the information to hand[3]. Instead, we shall have to be satisfied with identifying the long run effects associated with particular reasons for a job separation; we shall return to this issue below.

Thus our empirical strategy is to regress log consumption changes on levels and differences of demographics and to cross the levels with dummies for the separation reason. Using a trivial renaming in equation (13) we have:

$$\Delta c_{h,t+1} = \delta_0 + Z_{h,t}^{/}\delta + \Delta Z_{h,t+1}^{/}\beta + \sum_{J=1}^{3} d^{J} * [(\gamma_0^J - \gamma_0^0) + Z_{h,t}^{/}(\gamma^J - \gamma^0)] + \epsilon_h \qquad \textbf{(15)}$$

where $E^{h}(\epsilon_h) = 0$. This allows us to identify the relative demand shock revisions.

Are Wage Changes Uncorrelated with Preferences?

The above discussion suggests that consumption changes with lay-off need to be interpreted with considerable care. A frame work is required to separate out changes resulting from shocks to the marginal utility of wealth from preference factors, including changes in demographics, changes in labour forces status and expected consumption growth. Further more, heterogeneity in preferences, as well as in forecast errors could be correlated with the same characteristics (Z) which determine the magnitude of the shock to the marginal utility of wealth. Does this constitute an argument for measuring the costs of lay-off with earnings or wages instead of consumption?

If wages are determined by human capital considerations, and labour markets operate as

[3] The information required - the continuing state of the firm at time t - is recorded for workers who experience a lay-off but it is not recorded for workers who respond that they quit to take another job.

frictionless spot markets, then indeed, wages will reflect only individuals' productive characteristics and wage changes will reflect only changes in human capital. However, such a model is demonstrably inadequate: it is not consistent with the substantial spells of unemployment experienced by some job losers. However in a model which is consistent with unemployment - a search model - *wages* reflect optimizing behaviour by households. For example in a simple, continuous time search model (Mortensen, 1986; Devine and Kiefer,) with no recall, infinite horizons, and a Poisson offer arrival process, the optimal search strategy is a reservation wage, w^r which solves

$$\frac{l}{r}\int_{w^r}^{\infty}[w-w^r]\ \ dF(w;m(Z)) \ = \ w^r - b$$

Where λ is the offer arrival rate, r is the interest rate, w is the wage (with cumulative distribution *F(w)*). We have written the wage offer distribution *F()* as having a mean parameter *μ* which depends on individual characteristics, *Z*.[4] Critically b is the alternative value of time and this value will surely reflect many of the usual preference shifters, such as demographics, which are among the individual characteristics, *Z*. Thus the reservation wage is a function:

$$w^r = w^r(r,\lambda,\mu(Z),b(Z))$$

and the expected wage is:

$$E[w] = \int_{w^r(r,l,m(Z),b(Z))}^{\infty} w dF(w;m(Z)) = w(r,l,m(Z),b(Z))$$

Wage changes, like expenditure changes, will reflect heterogeneity in preferences (for example,

[4] Of course, we could allow other parameters, such as the variance, of the distribution of *F()*, or the arrival rate λ to depend on individual characteristics, *Z* as well.

for leisure) as well as heterogeneity in the loss of productive capacity. From wage data alone, it will be very difficult to separate the impact of individual characteristics via the post-lay-off wage offer distribution, from the correlation between wages and individual characteristics that arises from the dependence of preferences *b*, on individual characteristics, *Z*.

In the above model, the simple dependence of the reservation wage on the interest rate, r, reflects the assumption that the worker is risk neutral and unconstrained in capital markets. This assumption is common in the search literature, on account of its convenience, but not necessarily convincing, in the case of an unemployed worker searching for a job. If the assumption is relaxed, then the expected re-employment wage will depend on other preference parameters, such as the difference between the worker's discount rate and the interest rate.[5]

Other models of the labour market which are richer than a simple human capital formulation will also generate a correlation between wage changes with displacement and preference heterogeneity. For example, if workers self select into occupations with different wage profiles, then post -displacement wages will be correlated with discount rates.

In our empirical work, we will provide some evidence that wage changes reflect preferences as well as productivity changes, by showing that wage changes depend on a range of individual characteristics which are typically included as taste shifters in consumption studies but typically not included as productive characteristics in wage regressions.

Similar arguments can be generated about the impact of macro shocks. If aggregate shocks have differential impacts on the wages of different groups (as seems likely) then the differential impact of job loss can only be discriminated from the differential impact of macro economic conditions if a sufficiently long data series exists to fully control for the interaction of

[5] These assumptions are relaxed by Danforth, (1979).

macroeconomic conditions with individual characteristics.

Finally, in the previous section we pointed out that if leisure and consumption are non-separable, then the endogeneity of employment status presents a difficulty in the empirical modelling of expenditure changes. Here we simply note that the same is true of wages. Wage changes can only be calculated for those whom are re-employed in the course of the survey. This represents a selected sample and correction for the resulting sample selection bias requires a good instrument for re-employment that can be excluded from a wage equation. In a search model, expected wages and expected unemployment durations are jointly determined by the reservation wage, making the such an instrument essentially an impossibility.

Thus our conclusion is that the issues investigated in the first part of this section are not an argument for measuring the costs of displacement with earnings or wages instead of consumption. Rather, they suggest that consumption changes with displacement need to be carefully interpreted in the context of an adequate model, as do earnings changes.

3. Data.

3.1 The Canadian Out of Employment Panels.

The data for this paper are drawn from a relatively new survey on Canadians who separate from a job: the Canadian Out of Employment Panel (COEP). The survey was conducted by Human Resources Development Canada (HRDC) to evaluate the effects of a series of changes in the Canadian Unemployment system in the mid- 1990s. Approximately 11,000 people who had a job separation in February or May of 1993 were interviewed three times, at about 26, 39 and 60 weeks after the job separation. In Canada, when a job separation occurs, the employer is obliged to file a "Record of Employment" (ROE) with HRDC. These reports are compiled into the database from which the sampling frame was constructed. We refer to the job separation that led to inclusion in the sample as the “reference separation”. Because the administrative records that form the sampling frame are not complete until some months after the job separation, it was not possible to have the first interview closer to the separation date. Thus survey information about the periods just before and after the job separation are asked retrospectively from a point some 6 months on. This long interval between the job separation and the first interview is the price of a sample of only those who experience a job separation; this price is somewhat mitigated by the availability of complimentary administrative data which is collected continuously. Interviews were conducted over the telephone and took an average of 25 minutes.

A second sample of some 8,000 individuals who separated from a job in February or May of 1995 was subsequently drawn. The survey instrument was refined (and slightly expanded) for this second survey but care was taken to insure backwards comparability. In addition, the third interview was dropped. Together, the 1993 and 1995 COEP surveys provide a large sample of individuals who separated from a job. The period of 1993 to 1995 was one of slowly improving labour market conditions in Canada (for example, the aggregate unemployment rate fell from 11.2 to 9.5%).

A feature of the data is the wide range of questions were asked including questions on the pre-separation job and reason for separation; labour market activity; job search details; the activities of other household members; income; expenditure and assets. The availability of expenditure data in a survey of this type is somewhat unique; further details on these questions are given below. In addition to the extensive information available from the survey it is possible to match the survey data to several types of administrative records from HRDC, which have been collected over a long period.[6]

In this paper our primary focus is on information about expenditures in the period prior to the job separation (collected retrospectively at the first interview) and at the last opportunity we have to observe the respondents (the third interview for respondents in the 1993 sample and the second interview for respondents in the 1995 sample). The timing of the interviews was adjusted between the 1993 and 1995 samples so that the timing (relative to the job separation) of the third interview for the former sample corresponds roughly to the timing of the second interview for the latter sample. The details of interview timing are presented in Table 1.

As discussed in the previous section, the reason that we focus on the last point at which respondents are observed is that we wish to examine the change in the marginal utility of wealth ("permanent income") across a job loss. At earlier interviews, as smaller fraction of respondents are back in some employment and a greater fraction of the sample will be liquidity constrained. Where respondents are liquidity constrained our analysis of the permanent shock is confounded.

3.2 Sample Selection.

With regard to sample selection we begin considering only respondents between the ages of 20 and 60, and exclude single adults living with parents or unrelated adults. Extensive experience with the data (as well as common sense) suggests that the latter group return expenditure information which is of particularly poor quality. We also exclude workers who held multiple jobs at the separation date, one of which was ongoing.

Next we limit the sample to workers whose "reference" job had a duration of 6 months

[6] Another important feature of the data is that it captures substantial legislative and administrative variation in the parameters of the Canadian UI system. While this variation in UI program parameters in not a focus of the current paper, it does provide the basis for some of our other work. See for example, Browning and Crossley (1999a) and (1999b).

or more. This corresponds to the notion that a job loss presumes some attachment to the job. In fact, many studies have defined displaced workers as having “established work histories” (Kletzer, 1998) and some studies have limited their analysis to workers who lost jobs in which they had rather considerable tenure (for example, Jacobson, Lalonde and Sullivan (1993)). In our empirical analysis differences across workers with different levels of pre-separation tenure will be an important focus.

We use self reported (survey) information to divide the resulting sample into layoffs and quits. The data also contain and administrative reason for separation (from the ROE form). These correlate reasonably well with the self reported reasons, but have the drawback of a very large “other” catergory. We then limit the quit group to those who self reported that they quit to take another job. We have 402 such individuals and they form one group for our empirical analysis.

Among the layoffs, we distinguish types of layoffs on the basis of a series of survey questions about the *ex ante* (at time of layoff) expectation of recall. We define workers to have had a strong expectation of recall if they expected to be recalled on a specific date. We also refer to this group as “temporary layoffs”. Those workers who reported no expectation of recall are our “permanent layoffs” and this is the principal group of interest for this study. Note that this *ex ante* definition of job loss or “displacement” differs from much of the displaced worker literature in which “displacement” is defined in terms of *ex poste* realizations. However, conditioning on “time 0" information is much more natural in the consumption growth framework develped in the previous section. We also have a group of workers who expected recall but reported that they did not have a particular date by which they expected to be recalled. We refer to these workers as having “some expectation of recall”.

Our data contain 3028 “permanent layoffs” (no expectation of recall), 1094 “temporary layoffs” (strong expectation of recall) and 1419 workers with some expectation of recall. The large number of temporary layoffs may be surprising to readers from outside North America, but the important role of temporary layoffs in unemployment in North American labour markets is well documented (see for example, Feldstein, 1976). Its worth noting that the Canadian UI system (unlike the U.S. system) has no experience rating of firms.

The previous theory section emphasized that to implement a “difference-in-difference” estimator, we require a control group that, once we control for observable household and individual characteristics, is similar to the permanent layoffs in terms of anticipated consumption growth and realizations of macro and other (non-job-loss) shocks. The theory also emphasized that, even under the above assumptions, the “difference-in-difference” estimator only identifies the difference in the revisions to the *mue* that arise from different demand shock outcomes. To identify the revision due to permanent job loss, one requires a control group whose *mue* is not revised in the period in question. This would not be true, for example, of workers who experienced continuing employment if those workers had faced an ex ante risk of permanent layoff (because then continuing employment would be a positive surprise).

Our data do not contain workers who did not experience a separation of some type. With regard to the different types of separations outlined above, among workers who voluntarily moved to (presumably better) jobs, one would expect that the shock of the job separation is, if anything, positive (the *mue* falls). However, if such moves are largely anticipated, then the economic benefits of the change in job are likely largely incorporated in expenditures at *t*. In that case revision to *mue* associated with actual separation might be negligible. Thus this group may provide a useful upper bound on “zero” (keeping in mind that to be a useful control they must be similar to the permanent layoffs in terms of anticipated consumption growth and realizations of macro and other (non-job-loss) shocks.)

With respect to temporary layoffs, the shock associated with their separations might be negative (because a temporary layoff does involve some income loss, and may reveal negative information about the future prospects of the firm - though many companies such as car manufacturers have temporary layoffs year after year). It might also be positive for the same reason that continuing employment represents a positive: temporary layoff, like continuing employment is associated with the positive news of *not* being permanently laid off. Temporary layoffs are our most natural control group. They are more likely than the quits to match the to the permanent layoffs in terms of anticipated consumption growth and realizations of macro and other (non-job-loss) shocks. As we shall see, the quits are younger, and better educated than either group of layoffs. If the shock of temporary layoff is positive, then it is almost surely no more positive than that of continuing employment. It may be that the shock of temporary layoff is negative, but it remains an interesting question whether permanent layoff is a substantially worse outcome than temporary layoff. Finally, we note that temporary layoffs are very often preannounced, in which case the revision to the *mue* associated with actual separation might be negligible.

Before investigating the consumption growth around a job separation for each of our groups we begin, in Tables 2 through 4, by documenting their demographic and economic characteristics. The first panel of Table 2 reports demographic characteristics. The most dramatic differences - in terms of age, education, and local labour market conditions - are between the quits and layoffs. The second panel of this Table reports economic characteristics prior to the reference separation. Relative to all layoffs, the quits have much shorter tenures on average. Comparing the temporary and permanent layoffs we note that the temporary layoffs are more likely to be unionized and have higher tenures. Note also that more than 80% of them expected the layoff. This supports the notion that for this group, the shock associated with actual separation may be small, and thus that they may provide an appropriate “zero” or control group.

In Table 3 we document the outcomes for these groups as of the first interview. There is important attrition in our sample between the first and last interviews (see the first few rows of Table 2). In Table 3 we report the same first interview information for all first interview respondents (in the top panel) and for the sub sample that subsequently responded to the second interview (in the bottom panel). Comparing the top and bottom panels we note that the numbers are very simple. Thus this very simple exercise does not reveal any evidence that the attrition was nonrandom.

In terms of the actual outcomes we note that re-employment is much higher among temporary layoffs and quits than permanent layoffs. A small number of *ex ante* permanent layoffs do return to their former firm, while some ex ante temporary layoffs take work else where. If not re-employed, a permanent layoff is more likely to be actively searching than a temporary layoff. Workers with "some expectation of recall" exhibit outcomes which lie somewhere between the permanent and temporary layoff groups.

In Table 4 we summarize the labour market outcomes for these groups at the final interview. Interestingly, the employment rate among temporary layoffs fall from the first to final interview. This may be because the final interview is in the fifth quarter after the reference separation, and temporary layoffs are often seasonal in nature (even in non-seasonal manufacturing industries). By this point some 15% (26% of the 57% employed) of *ex ante* permanent layoffs have returned to their former firm, while have almost 20% (29% of the 66% employed) *ex ante* temporary layoffs are working at a new firm.

Before concluding our discussion of our sample and sub samples we note that our regression samples vary due to missing co-variates. This is a trade off that comes with having a very rich data set with information drawn from several sources.

3.3 Expenditure Questions.

For the purposes of this paper the most important set of variables are those concerning expenditures. Two sets of questions were asked at each interview. 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 second set comprised a single question regarding the change in total expenditures relative to the month prior to the ROE (separation) date. In this paper our focus in on total expenditures, principally because this is the only (expenditure) quantity for which we have pre-separation information. Since these questions are somewhat unusual in a survey of this type, we present the full text of the questions here. At each interview, the respondent was asked

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.

And:

Has the amount you spend on everything decreased since <ROE>?

By what amount monthly?

Has the amount you spend on everything increased since <ROE>?

By what amount monthly?

Although the answers to this question are undoubtedly noisy, we have several reasons to believe that they contain significant information about the levels and changes in household expenditures. First, we note that in each survey the expenditure questions are asked before income questions, so that we think it is less likely that the respondents just report incomes in response to expenditure questions. Second in other work (Browning and Crossley, 1998a,b) and in unreported subsidiary analysis, we have amassed considerable internal evidence of the validity of the expenditure responses in the COEP. In particular income elasticities and demographic effects can be precisely estimated with this data (which would not be the case if the data were simply noise.) Finally, in unreported subsidiary analysis, we have found good external evidence of the validity of the expenditure responses in the COEP, via a series of budget share and Engel curve comparisons with the FAMEX, a Canadian household budget survey thought to be of excellent quality.

We also note that at every interview the expenditure change question is posed relative to the month prior to the job separation, and that this, combined with the levels information, provides multiple measures of the change in expenditures between that month (denoted “0" in what follows) and the final interview (denoted “2"):

$$\Delta C_{2,0} = \Delta C_{1,0} + C_2 - C_1$$

In the next section (subsection 4.3), we present a simple measurement error model which allows us to take advantage of this feature of the survey to generate more precise estimates of the parameters of interest. We also present an over-identification test which allows us to test our specification, including our assumptions about the structure of measurement error.

4.4 Job Loss in Different Countries.

How informative are our results, based on a rather unique Canadian data set, about the costs of job loss in other countries? Relative to the US, Canada has higher unemployment, more generous unemployment insurance, and greater unionization. Several recent papers have used comparable data sets to compare post-displacement wage and employment outcomes in Canada and the US. Gray and Grenier (1998) focus on postdisplacement jobless durations which they report are somewhat longer in Canada (28.5 versus 22.5 weeks at the median). Their analysis suggests that the difference is largely explained by the differences in the characteristics of displaced workers across the two countries (particularly unionization in the predisplacement job and local unemployment rates) and not by differences in the effects of these characteristics. One possibility is that the more generous UI system in Canada induces workers to search longer and thus avoid some of the wage losses experienced by their US counterparts. Storer and van Audenrode (1998) investigate this possibility and do indeed find some evidence that displaced workers in Canada experience less severe wage losses. Interestingly however, this gap does not appear to be limited to the UI eligible. On balance then, workers who experience permanent involuntary job loss in Canada have outcomes that are roughly comparable to their US counterparts, with slightly longer spells of joblessness and slightly smaller wage losses. Thus we conclude that a study of Canadian consumption data is likely to be informative about North American labour markets generally. On the other hand, it appears that the effect of displacement in other countries, especially continental Europe may be quite different, and this is the subject of considerable ongoing research (see for example Albaeck, Browning and van Audenrode, 1998, and Bender, Dustman, Margolis and Meghir, 1998). Finally we note that the wage information in our data has been used by Kuhn and Sweetman (1998), who argue that it potentially informative about job displacement in the U.S. as well as Canada.

4 Empirical Analysis.

4.1 Distribution of Earnings and Consumption Growth.

We begin our analysis by examining the distribution of earnings and consumption growth from the month just before a job separation until a month in the fifth quarter after job loss, across different separation categories. Figure 1 presents box and whisker plots of proportional consumption and earnings changes for layoffs with strong expectation of recall (ie., a recall date), some expectation of recall, and no expectation of recall (permanent layoffs) as well as quits. In each case the left hand box reflects earnings growth and the right hand box consumption growth. A number of statistics corresponding to these pictures are presented in Table 5a. The contrast in earnings growth is stark. Five quarters out, the median individual who quit to take another job experienced substantial earnings growth (9%) while the median permanent layoff has earnings almost 50% below their pre-separation level. Both parametric tests of common means and nonparametric rank tests suggest that the distribution of proportional earnings of permanent layoffs is strongly statistically different from that of the other groups.

In contrast to earnings, the differences in consumption growth are not so visually striking. In every category the median change in consumption is zero. Nevertheless, those who quit to take another job do appear - in both the figure and in the mean - to experience stronger consumption growth than the other groups. The differences among the other groups are difficult to discern from the box and whisker plots, but both the statistical test reported in the bottom panel of Table 5a suggest that the permanent layoffs are different for each of the other groups. Temporary layoffs (strong expectation of recall) experience stronger consumption growth than those with some expectation of recall, who in turn experience more consumption growth than permanent layoffs (no expectation of recall). As noted in the introduction, there are a number of reasons to expect that any proportional change in individual earnings translates into a rather smaller change in household consumption (the earnings loss may be transitory, the individual may be providing only a fraction of household income). Nevertheless, the striking differences in earnings and consumption data, combined with the way the consumption data are collected may suggest to some readers that the consumption data is simply noise. However, the statistically significant differences across groups, and the strong consumption growth of those who quit to take another job refutes that position. More evidence that the consumption data contains significant information will be reported below.

As first reported in Table 1, the weeks elapsed between separation from the reference job and the final interview varies between approximately 54 and 64 weeks in our sample. The bottom row of Table 5a reports that the mean is between 58 and 59 weeks (about 9/8 of a year) for each of our separation type groups. Thus variation in elapsed time does not seem to have played any role in the heterogeneity in earnings and consumption growth across groups. Notice also that the data underlying both the figures and tables is nominal.[7] This was a relatively low inflation period in Canada. The respondents to our sample experienced proportional changes in the CPI which ranged from -0.0018 to 0.027 (inflation of -0.1 to 2.7%). The bottom row of Table 5 reports that there was some difference in the inflation experienced across groups, with in particular the permanent layoffs experiencing on average one percentage point less inflation. This is a very small component of the differences in nominal consumption and earnings changes.

Figure 2 repeats the analysis of Figure 1, but with the sample limited to those who report being back in employment at the last interview. The corresponding statistics are reported in Table

[7]We chose to report the nominal amounts (that is the respondents actual responses) rather than to convert to real amounts and then to also report inflation for reasons of transparency. In particular, converting to real earnings and consumption growth would mask the large number of reported zero changes.

5b. Several features of the data bear notice. First, the differences in earnings growth across layoff groups largely disappear (in the means and figures - the rank tests still suggest statistically different distributions). Furthermore the median earnings change in each layoff group is non-negative. This suggests that among our sample the earnings losses associated with job separations are all associated with nonemployment (and not with wage changes). This is inconsistent with studies of job displacement which have focussed on highly attached workers (for example Ruhm, 1991) which find that both wage and employment changes play a role, but it is consistent with studies such as (Polsky, 1999) which examine job losers of a broad range of labour force attachment. We investigate this point further in Figures 3 and 4, which present box and whisker plots of earnings and consumption growth across tenure categories for permanent layoffs only. Figure 3, which presents calculations for the full sample of permanent layoffs, reveals that, as expected, earning losses increases with tenure (except for the very lowest tenure group). Figure 4, which focuses on the subsample of permanent layoffs who were back in employment at the final interview, exhibits earnings changes that are decreasing in tenure and median earnings changes which are in fact losses (not gains) for the highest tenure category. Thus in our data, as in most other studies, high tenure workers experience wage losses on re-employment. It is also interesting to note in both Figures 3 and 4 that tenure appears to be a determinant of consumption growth of permanent layoffs, and that many workers in the highest tenure category appear to have experienced negative consumption growth. A fuller empirical investigation of the determinants of consumption growth follows below.

Turning back to Figure 2 and the associated Table 5b, we conclude this section by noting that among those back in employment at the final interview, the statistical difference in the distributions of consumption growth between those with no expectation of recall (permanent layoffs) and those with some expectation of recall is quite weak. However, even conditional on re-employment, permanent layoffs do appear to exhibit consumption growth that differs from that of temporary layoffs (strong expectation of recall) and from those who quit to take another job.

In the next subsection we report consumption growth regressions. The interpretation of those regressions is guided by the theoretical discussion of Section 2. The results of this section confirm our suspicion that those respondents who quit to take another job are really very different from those experiencing a layoff. They are unlike to be a useful control group in the sense outlined in Section 2. We have also found in this section that those with some expectation of recall appear to be a middle group with outcomes somewhere between permanent and temporary layoffs. We therefore focus the next section of the determinants of consumption growth among permanent layoffs and a comparison of (ex ante) permanent and temporary layoffs.

4.2 Consumption Growth Regressions.

Table 6a reports regressions of the proportional change in consumption (approximate log consumption growth) on individual and household characteristics for final interview respondents in both employment and nonemployment. The regression coefficients and associated tests in the first column are for the sample of permanent layoffs. These results further confirm that the consumption data contain information despite being noisy. The regression is statistically significant and several coefficients - particularly those on age and tenure - are tightly estimated. Consumption growth across a job loss declines in age (but a declining rate) and is much lower for high tenure workers). Both of these results are consistent with the theoretical ideas about the cost of job loss. High tenure workers may have large amounts of firm specific capital or be very well matched. Similarly, age may be proxying for experience, and workers with high labour force experience might be expected to be well matched to their jobs, or to have particular difficulty finding new employment after and involuntary job loss. Nevertheless, as Section 2 emphasized, the coeffficients of this “difference” regression may reflects several things besides the determinants of the long run costs of job loss. In particular they might confound determinants of anticipated consumption growth and correlates of the impact of macro and other (non-jobloss) shocks. One hint of this is in the very strong estimated time effects.

The next column reports the results of a consumption growth regression for temporary layoffs. This regression is also statistically significant. As with the permanent layoffs, age and tenure are significant determinants of consumption growth.

As outlined in Section 2, the temporary layoffs can be employed as a control group in a “difference in difference” estimate of the costs of involuntary permanent job loss If one accepts that:

(1) The change in the marginal utility associated with a temporary layoff is neither very positive (the risk of permanent layoff for these workers was quite small, so the realization of a temporary layoff - and not a permanent layoff - is not an important positive shock) nor very negative (either the risk of temporary layoff was very large - so that the realization not much of a shock - or the cost of a temporary layoff - both in terms of lost earnings during the layoff and what it reveals about future prospects - is very small).

(2) Conditional on the individual and household characteristics we can control for the temporary layoffs are very like the permanent layoffs in their anticipated consumption growth and in their experience of macro and other non-jobloss shocks.

The third column of Table 6a implements the “difference-in-difference” estimator. Under the (admittedly extreme) assumptions that the temporary layoffs experience no change in the marginal utility of expenditure (*mue*) and have the same anticipated consumption growth and macro (and other) shocks as the permanent layoffs as the permanent layoffs, the coefficients in column 2 capture the common anticipated consumption growth and impacts of other shocks, while the *differences* in the coefficients across columns 1 and 2 captures the determinants of changes in the *mue* and hence the long run costs of job loss. Put another way, under these assumptions, any additional correlation between characteristics and consumption growth among the permanent layoffs - beyond that which we observe among the temporary layoffs - reflects the costs of job loss.

As noted in the theory section, one testable implication of the assumptions of the difference -in-difference estimator is time invariance. That is, there should not be time effects in the difference between the control and treatment groups. This is confirmed in the third column of Table 6a, and particularly in the joint test of time effects reported in the last row of this table: there is no statistically significant difference in the time effects experienced by permanent and temporary layoffs (or equivalently in this linear model, the difference between the groups is time invariant).

However, the time effects are not the only thing that is insignificant in column 3 of Table 6a. We noted above that the pattern of coefficients in columns 1 and 2 is very similar. The test reported in the penultimate row of column 3 indicates that data do not reject common coefficients for the two groups. Of course, this F test is certain not to reject because of the many coefficients in each of the individual regressions (columns 1 and 2) which are not precisely estimated. Nevertheless, an inspection of the individual t-tests in column 3 reveals that there are no significant differences across groups in the coefficients that are precisely estimated in the separate regressions (such as age). If one regarded temporary layoffs as an adequate control group (as defined above) then this result would indicate that all the correlations of consumption growth with observable individual and household characteristics among the permanent layoffs (column 1) reflect anticipated consumption growth and non-job-loss shocks. If one estimates a intercept shift model on the pooled permanent and temporary layoffs (ie., common slopes) one gets an estimate of nominal consumption growth that is 2.7 percentage points lower for the permanent layoffs, conditional on observable characteristics.

Table 6b repeats the analysis of Table 6a, but for the restricted sample of permanent and temporary layoffs who are back in employment at the final interview. The results are very similar to those for the full sample. Consumption growth appears to be determined by age and tenure for both temporary and permanent layoffs, conditioning on re-employment. Each of the regressions is statistically significant, suggesting that the consumption responses are predictable. The data do not reject the pooling of the data across the two groups.

4.3 Improving Precision.

While the results of the previous section suggest that consumption data certainly do contain information, there is no denying that the data are noisy, and a great many of the coefficients are insignificant. In this section we report a preliminary investigation into how we might improve the precision of our estimates, focussing for now on the permanent layoff sample. The first approach we take simply concerns the construction of our "left hand side" variable. In the results reported above, we approximate log consumption growth with the usual "lagged" percentage change variable - that is, consumption change divided by its lagged value: $\frac{\Delta C_{2,0}}{C_0}$.

However, as noted in Subsection 3.3. we do not actually observe the lagged value and must construct it from the current value and reported change. The variable actually is $\frac{\Delta C_{2,0}}{C_2 - \Delta C_{2,0}}$.

The reported change is likely very noisy, and this procedure of forming a ratio in the reported change may exacerbate the measurement error. An alternative is to approximate log consumption growth with the “forward “ proportional change: $\frac{\Delta C_{2,0}}{C_2}$.

In the first column of Table 7 we report again - for comparison -the results first reported in column 1 of Table 6a. That is, this is the consumption growth regression for all (employed and non-employed) permanent layoffs, using the lagged proportional change as the independent variable. In the second column, we estimate the same regression but use the forward proportional change as the independent variable. The effect of some variables, including education and household size, appears stronger, though the overall significance of the regression is not improved.

The second strategy we pursue exploits the redundancy in the consumption information first noted in section 3.3. We wish to explore the correlates of changes in total expenditure (from before the job separation until the last point we observe the survey respondents) in a regression framework. Denote the “true” measure of consumption growth with a star:

$$\Delta C_{2,0}^{*} = \alpha z + \epsilon; \tag{18}$$

$$E[\epsilon^2] = \sigma_\epsilon^2;\ E[z\epsilon] = E[\epsilon] = 0;$$

As discussed in subsection 3.3, we can construct expenditure changes from a direct question, or from a combination of responses to questions about the level of expenditures at different interviews as well as a question about the change in total expenditure from the month prior to job loss to the first interview. Each of these quantities is surely reported with error:

$$\Delta C_{2,0} = \Delta C_{2,0}^{*} + \mu_2.$$

$$\Delta C_{1,0} = \Delta C_{1,0}^{*} + \mu_1.$$

$$C_2 = C_2^{*} + \nu_2.$$

$$C_1 = C_1^{*} + \nu_1.$$

We assume that all measurement error is uncorrelated with the covariates of interest (z):

$$E[z\mu_i] = E[z\nu_j] = 0.$$

It is difficult to place an *a priori* restrictions on the covariance structure of the measurement errors. For example, it may be that if respondents over-report the level at a point in time, they also over- or under-report the change since the month prior to the job loss (depending on the sign of the change), so that:

$$E[\mu_i\nu_j] \neq 0.$$

It may also be the case that if respondents over-report a level or change at one interview they are likely to do so again at another interview, so that:

$$E[\mu_1\mu_2] \neq 0, E[\nu_1\nu_2] \neq 0.$$

That is, errors could be correlated within surveys, or across surveys within types of questions.

Fortunately, under the assumption that each of the measurement errors are uncorrelated with *z*, the measurement errors will not bias estimates of the conditional mean.[8] This does not require any restriction on the covariance structure of the measurement errors.[9] We can estimate the parameter of interest, α, by OLS, with either measure of the change in expenditures. Note that:

$$\Delta C_{2,0} = \alpha z + (\epsilon + \mu_2). \qquad \textbf{(19)}$$

and:

$$\Delta C_{1,0} + C_2 - C_1 = \alpha z + (\epsilon + \mu_1 + \nu_2 - \nu_1). \qquad \textbf{(20)}$$

so that:

$$a_1 = (z'z)^{-1}z'(\Delta C_{2,0}).$$

[8] They will of course bias (upward) estimates of dispersion.

[9] Or for that matter, on the correlation between any of the measurement errors and the regression disturbance, ϵ.

and

$$a_2 = (z'z)^{-1}z'(\Delta C_{1,0} + C_2 - C_1).$$

While (under our assumptions) the measurement errors in the two measures of expenditure change will not bias estimates of α, the measurement errors do add noise to the regression disturbances (compare (20) or (19) to (18)) and hence reduce the precision of the estimates of α. However, we can combine both estimates to achieve an efficient estimate given the data. To do so, we do not need to know the covariance structure of the measurement errors, only the covariance structure of the composite regression disturbances $e = (\epsilon + \mu_2)$ and $u = (\epsilon + \mu_1 + \nu_2 - \nu_1)$, or equivalently the covariance matrix for a_1 and a_2. These are estimable quantities without further assumptions. Given a_1 and a_2, their variances and covariances, the optimal estimate a, of α is the solution to the equation:

$$[v(a_1)-2c(a_1,a_2)+v(a_2)]a = [v(a_2)-c(a_1,a_2)]a_1 + [v(a_1)-c(a_1,a_2)]a_2. \qquad \textbf{(21)}$$

We implement this by estimating equations (5.2) and (5.3) as SUR system with cross equation restrictions ($a_1=a_2$). In addition, a test of the validity of the cross equation restrictions gives us a test of our statistical specification, including the assumption that the measurement errors are uncorrelated with the conditioning variables, z.[10]

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[10] Of course, because the two linear regression equations (5.2 and 5.3) condition on the same variables (z) the estimates a_1 and a_2 are numerically invariant to whether we estimate by single equation or system methods. However, the system estimation provides the covariance of a_1 and a_2 conveniently. Also, we could estimate the two equation SUR system without the cross equation restrictions and impose them via a minimum χ^2 step, where the minimized distance would provide our test statistic. Alternatively, since (5.4) is linear and has a trivial closed form solution, we could calculate both the estimates a and the test statistic directly. There are a number of equivalent ways to proceed and our choices have been dictated solely by convenience.

The test of the cross-equation restrictions can also be interpreted as an over-identification test, where the over-identification arises from covariance restrictions (all the measurement errors uncorrelated with z), rather than from exclusions restrictions (that latter being the more familiar case). Note that the test does not have power against all alternatives; in particular, it has no power against the alternative hypothesis that measurement errors are

The results of this exercise are reported in columns 3 and 4 of Table 7, for “lagged” and “forward” measures of proportional consumption growth respectively. In each case we restrict the coefficients across the two equations two be the same (as suggested above) except for the intercepts (so that we are allowing from measurement error that is uncorrelated with *Z* but not necessarily mean zero). The effect of some variables appears to be strengthened. At the bottom of these two columns we report a test of the equality of coefficients (which is the overidentification test outlined above) which passes in both cases (though perhaps marginally). We also test for a common intercept which is not rejected in case of the “lagged” measure but is strongly rejected in column 4 where we are modelling the “forward” measure. We can also construct R^2 statistics for each of the equations. The first R^2 is for the direct measure of consumption change (the measure used in all the previous tables). Comparing this with the R^2 measures reported in columns 1 and 2 suggests that the measurement model fits the direct measure of consumption change about as well as the single equation regressions. The second R^2 suggests that the constructed consumption change measure is not so well explained.

Before concluding this section we noted that the challenges we are facing here are endemic to modelling differenced variables in panel data. We illustrate the point in two ways. First, in Table 8 we compare regressions for earnings and consumption changes on the same sample of re-employed permanent layoffs. In fact, there is no evidence that earnings changes are easier to model statistically than consumption changes. Second, in Table 9, we use our basic specification to model the *level* of consumption at the final interview. Unsurprisingly, we do much better at modelling consumption levels, with an R^2 of 0.27 and many precisely estimated and statistically significant coefficients. We conclude that it is not the fact that we are modelling consumption data

correlated with *Z*, but are exactly consistent across both surveys and questions $(\mu_2 = \mu_1 + \nu_2 - \nu_1)$ for all respondents.

- but rather the fact that we are modelling changes, which is our difficulty. It is almost always easier to model levels than changes, but this is of little comfort when it is the changes which are of interest.

5 (Tentative) Conclusions.

The (very) preliminary results of this study are that permanent layoffs experience consumption growth across a job separation that lags on average several percentage points behind temporary layoffs. This gap does not appear to be strongly correlated with individual or household characteristics. Only very high tenure workers appear to experience consumption falls.

The “costs of job loss” is not a particularly well defined notion, and the theory section of this paper highlights the considerable care that must be taken in interpreting the numbers reported here (and in other studies) as estimates of any particular parameter of interest.

One might (very) tentatively conclude that the case for permanent job loss (or “job displacement”) as a particular policy concern - meriting attention distinct from unemployment generally or income distribution generally - has not been strengthened by this analysis.

We have considerable work still to do, including continuing to explore ways of improving the precision of our estimates, and more detailed explorations of the effects off job loss expectation, and group versus individual layoffs. Research in both the U.S. (Gibbons and Katz, 1991) and the Canada (Doiron, 1995) has shown that workers laid off in plant closures have shorter unemployment spells and smaller wage losses than other permanent layoffs. This is a consistent with a model in which, when a firm has discretion over whom to layoff, a layoff is treated as a signal of low ability by other firms. Thus it would be interesting to explore differences in consumption growth across these subgroups of permanent layoffs.

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TABLES AND FIGURES

TABLE 1: Interview Timing, 1993 and 1995 COEP
(Weeks since Reference Separation; Inter-quartile Range)

	1993 Cohort 1	1993 Cohort 2	1995 Cohort 1	1995 Cohort 2
Reference Job Separation	Feb. - Mar.	Apr.	Jan.-Mar.	Apr.-June
Interview1	27-29	24-25	36-40	33-38
Interview 2	40-43	37-40	60-63	54-57
Interview 3	61-64	55-59	X	X

TABLE 2: Descriptive Statistics: Pre - Reference Separation Information

	Layoffs			Quits
	No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
1st Interview Obs.	3023	1417	1094	402
COEP 1995 (%)	845 (28%)	1122 (79%)	794 (73%)	344 (86%)
Last Interview Obs. (%)	2199 (73%)	1127 (80%)	890 (81%)	315 (78%)
Demographics				
highschool	0.37	0.42	0.44	0.42
college	0.33	0.21	0.27	0.43
age	38.0	37.8	39.0	32.7
ln (household size)	0.94	0.95	1.03	089
male	0.53	0.61	0.48	0.60
Atlantic	0.08	0.13	0.11	0.09
Quebec	0.27	0.40	0.31	0.22
prairies	0.15	0.12	0.08	0.19
BC	0.10	0.10	0.06	0.12
local unemployment rate	10.5%	10.6%	10.1%	9.2%

TABLE 2: Descriptive Statistics: Pre - Reference Separation Information (Cont´d)				
	Layoffs			Quit
	No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
Reference Separation Job				
manager	0.28	0.18	0.28	0.30
blue collar	0.33	0.61	0.46	0.29
union	0.27	0.42	0.47	0.15
seasonal	0.10	0.28	0.33	0*
expected loss	0.45	0.71	0.81	1*
Job Tenure (Months)	65.2	80.4	89.7	44.5
Monthly Earnings	1.89	1.76	1.65	1.76
Program Use				
UI in at least 1 of past 2 years	0.55	0.80	0.74	0.40

TABLE 3: Descriptive Statistics: First Interview Information				
	Layoffs			Quit
	No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
All First Interview Respondents				
Employed	0.44	0.60	0.80	0.79
Of Employed: Back at reference Employer	0.13	0.75	0.90	0.08
Job as good as reference job	0.82	0.89	0.90	0.96
Of Non-Employed: Still in First UE Spell	0.77	0.53	0.49	0.26
Searched in Last 4 weeks	0.82	0.72	0.59	0.59
Participation Rate	0.84	0.85	0.89	0.89
Last Interview Respondents Only				
Employed	0.43	0.61	0.80	0.79
Of Employed: Back at reference Employer	0.12	0.76	0.90	0.08
Job as good as reference job	0.83	0.89	0.90	0.96
Of Non-Employed: Still in First Spell	0.77	0.52	0.46	0.28
Searched in Last 4 weeks	0.81	0.72	0.61	0.56
Participation Rate	0.84	0.85	0.89	0.89

TABLE 4: Descriptive Statistics: Last Interview Information				
	Layoffs			Quit
	No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
Employed	0.57	0.59	0.66	0.80
Of Employed: Back at reference Employer	0.26	0.58	0.71	0.31
Job as good as reference job	0.79	0.84	0.88	0.94
Of Non-Employed: Still in first UE spell	0.44	0.27	0.20	0.25
Searched in Last 4 weeks	0.68	0.60	0.54	0.33
Participation Rate	0.83	0.71	0.74	0.82
Regression Sample (Last Interview Respondent, Employed at last interview, complete and consistent expenditure and earnings information:)	971	527	464	214

TABLE 5a: Descriptive Statistics: Earnings and Expenditure Changes
Pre- reference separation to last interview
Proportional Changes in nominal monthly Amounts
All Final Interview Respondents

		Layoffs			Quit
		No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
Earnings	q1	-1	-1	-1	-0.40
	q2	-0.47	-0.19	0	0.09
	q3	0.016	0.025	0.025	0.04
	mean	-0.44	-0.39	-0.31	-0.013
Difference of mean from no expectation group, [t-stat]			0.044 [1.9]	0.13 [5.1]	0.42 [11.1]
Kruskal-Wallis rank test of common distribution with no expectation group: $\chi^2_{(1)}$ (p-value)			8.6 (0.003)	36.5 (<0.001)	109.2 (<0.001)
Total Expenditure	q1	0	0	0	0
	q2	0	0	0	0
	q3	0.053	0.047	0.067	0.13
	mean	0.0083	0.029	0.051	0.10
Difference of mean from no expectation group, [t-stat]			0.021 [2.5]	0.043 [4.9]	0.095 [7.3]
Kruskal-Wallis rank test of common distribution with no expectation group: $\chi^2_{(1)}$ (p-value)			11.8 (<0.001)	30.0 (<0.001)	38.1 (<0.001)
CPI	mean	0.0058	0.014	0.013	0.015
Weeks elapsed	mean	59	58	58	59

TABLE 5b: Descriptive Statistics: Earnings and Expenditure Changes
Pre- reference separation to last interview
Proportional changes in nominal monthly amounts
Employed at Last Interview

		Layoffs			Quit
		No Expectation of Recall	Some Expectation of Recall	Strong Expectation of Recall	
Earnings	q1	-0.25	-0.045	0.0062	0.025
	q2	0	0.025	0.025	0.19
	q3	0.20	0.097	0.10	0.45
	mean	0.032	0.033	0.071	0.25
	difference of mean from no expectation group, [t-stat]		0.00018 [0.0]	0.038 [1.8]	0.22 [8.0]
	Kruskal-Wallis rank test of common distribution with no expectation group: $\chi^2_{(1)}$ (p-value)		6.9 (0.008)	19.3 (<0.001)	63.4 (<0.001)
Total Expenditure	q1	0	0	0	0
	q2	0	0	0	0.015
	q3	0.067	0.059	0.071	0.16
	mean	0.036	0.049	0.068	0.13
	difference of mean from no expectation group, [t-stat]		0.013 [1.2]	0.031 [2.8]	0.091 [6.0]
	Kruskal-Wallis rank test of common distribution with no expectation group: $\chi^2_{(1)}$ (p-value)		2.9 (0.09)	12.1 (<0.001)	24.3 (<0.001)
CPI	mean	0.0057	0.014	0.013	0.015
Weeks elapsed	mean	59	58	59	59

TABLE 6a: Determinants of $\Delta\ln C_t$, Regression Estimates. All Final Interview Respondents			
Specification	RawRegression	Raw Regression	Difference in Difference Regression
Separation Category	Layoff, No Expectation of Recall	Layoff, Strong Expectation of Recall	Layoff, No Expectation of Recall
Comparison Group	-----	-----	Layoff, Strong Expectation of Recall
Sample	all	all	all
Dependent variable	$\Delta\ln C_{2,0}$ (lagged)	$\Delta\ln C_{2,0}$ (lagged)	$\Delta\ln C_{2,0}$ (lagged)
completed highschool	-0.022 [-1.63]	0.015 [.79]	-0.037 [-1.55]
completed tertiary	0.001 [.05]	-0.003 [-.13]	0.004 [.14]
age	-0.027 [-4.6]	-0.021 [-2.3]	-0.006 [-.51]
age squared	0.017 [3.03]	0.019 [2.29]	-0.002 [-.23]
ln(household size)	0.023 [1.24]	-0.032 [-1.21]	0.055 [1.67]
male	-0.015 [-1.17]	0.009 [.47]	-0.024 [-1.01]
married, spouse not employed	0.003 [.21]	-0.015 [-.72]	0.018 [.7]
single	0.011 [.41]	-0.067 [-1.64]	0.077 [1.57]
other household	0.007 [.34]	0.006 [.2]	0.001 [.02]

atlantic	0.002 [.11]	-0.035 [-1.32]	0.037 [1.11]
quebec	-0.011 [-.78]	-0.048 [-2.65]	0.037 [1.59]
prairies	-0.003 [-.18]	-0.030 [-.97]	0.027 [.76]
bc	0.044 [2.24]	0.007 [.21]	0.037 [.91]
manager, reference job	-0.003 [-.24]	0.017 [.77]	-0.020 [-.76]
blue collar, reference job	0.009 [.57]	-0.015 [-.67]	0.024 [.85]
unionized, reference job	0.003 [.21]	0.034 [2.13]	-0.031 [-1.5]
tenure 3-10 years, reference job	-0.004 [-.29]	-0.040 [-2.22]	0.036 [1.63]
tenure 10+ years, reference job	-0.036 [-2.32]	-0.028 [-1.29]	-0.008 [-.28]
importance of reference job	-0.026 [-1.15]	-0.052 [-1.39]	0.027 [.59]
93 second window	0.012 [.97]	0.026 [.83]	-0.014 [-.39]
95 first window	0.057 [3.35]	0.032 [1.17]	0.025 [.76]
95 second window	0.059 [3.73]	0.018 [.65]	0.041 [1.28]
constant	-0.028 [-.89]	0.099 [1.86]	-0.127 [-2.01]
N R - square	1497 0.05	671 0.06	2168 0.07
Regression F-Test	$F_{22, 1474} = 3.87$ ($P < 0.001$)	$F_{22, 648} = 2.03$ ($P = 0.0038$)	$F_{45, 2122} = 3.35$ ($P < 0.001$)
Ramsey OV test	$F_{3, 1471} = 1.94$ ($P = 0.12$)	$F_{3, 645} = 0.81$ ($P = 0.49$)	
Test of pooling groups			$F_{23, 2122} = 1.31$ ($P = 0.15$)
Joint test of time effects			$F_{3, 2122} = 1.39$ ($P = 0.24$)

Notes:
t stats in square parenthesis.

TABLE 6b: Determinants of $\Delta\ln C_t$, Regression Estimates. Employed at Last Interview.			
Specification	RawRegression	Raw Regression	Difference in Difference Regression
Separation Category	Layoff, No Expectation of Recall	Layoff, Strong Expectation of Recall	Layoff, No Expectation of Recall
Comparison Group	-----	-----	Layoff, Strong Expectation of Recall
Sample	employed at last interview	employed at last interview	employed at last interview
Dependent variable	$\Delta\ln C_{2,0}$ (lagged)	$\Delta\ln C_{2,0}$ (lagged)	$\Delta\ln C_{2,0}$ (lagged)
completed highschool	-0.015	-0.017	0.002
	[-.77]	[-.68]	[.06]
completed tertiary	0.035	-0.010	0.045
	[1.65]	[-.31]	[1.15]
age	-0.023	-0.038	0.015
	[-2.64]	[-3.05]	[.98]
age squared	0.018	0.027	-0.009
	[2.21]	[2.39]	[-.62]
ln(household size)	0.027	-0.029	0.055
	[1.07]	[-.86]	[1.3]
male	-0.008	0.017	-0.025
	[-.46]	[.69]	[-.81]
married, spouse not employed	-0.008	-0.022	0.014
	[-.4]	[-.79]	[.41]
single	-0.001	-0.020	0.020
	[-.02]	[-.38]	[.3]
other household	0.022	0.018	0.004
	[.78]	[.46]	[.07]

atlantic	-0.027 [-.99]	-0.097 [-2.6]	0.071 [1.49]
quebec	0.013 [.66]	-0.052 [-2.26]	0.065 [2.09]
prairies	-0.031 [-1.48]	-0.018 [-.51]	-0.013 [-.3]
bc	0.053 [2.04]	0.000 [0]	0.053 [.95]
manager, reference job	-0.002 [-.11]	0.019 [.65]	-0.021 [-.58]
blue collar, reference job	0.004 [.2]	-0.004 [-.13]	0.008 [.21]
unionized, reference job	0.022 [1.25]	0.025 [1.23]	-0.003 [-.11]
tenure 3-10 years, reference job	-0.006 [-.36]	-0.057 [-2.53]	0.051 [1.78]
tenure 10+ years, reference job	-0.059 [-2.74]	-0.052 [-1.8]	-0.007 [-.2]
importance of reference job	0.008 [.28]	-0.088 [-1.84]	0.096 [1.65]
93 second window	0.004 [.23]	0.050 [1.28]	-0.046 [-1.03]
95 first window	0.041 [1.82]	0.057 [1.8]	-0.016 [-.41]
95 second window	0.070 [3.13]	0.041 [1.26]	0.029 [.71]
constant	-0.034 [-.78]	0.123 [1.97]	-0.157 [-2]
N	812	411	1223
R - square	0.08	0.11	0.10
Regression F test	$F_{22, 789} = 3.17$ ($P < 0.001$)	$F_{22, 388} = 2.28$ ($P = 0.001$)	$F_{45, 1177} = 2.76$ ($P < 0.001$)
Ramsey OV test	$F_{3, 786} = 2.22$ ($P = 0.085$)	$F_{3, 385} = 1.83$ ($P = 0.14$)	
Test of pooling groups			$F_{23, 1177} = 1.05$ ($P=0.39$)
Joint test of time effects			$F_{3, 1177} = 1.27$ ($P = 0.28$)

Notes:
t stats in square parenthesis.

TABLE 7: Determinants of $\Delta \ln C_t$, Improving Precision.				
Specification	Raw Regression	Raw Regression	Measurement Error Model	Measurement Error Model
Separation Category	Layoff, No Expectation of Recall	Layoff, No Expectation of Recall	Layoff, No Expectation of Recall	Layoff, No Expectation of Recall
Comparison Group	-----	-----	-------	-------
Sample	all	all	all	all
Dependent variable	$\Delta \ln C_{2,0}$ (lagged)	$\Delta \ln C_{2,0}$ (forward)	$\Delta \ln C_{2,0}$ (lagged)	$\Delta \ln C_{2,0}$ (forward)
completed highschool	-0.022	-0.045	-0.025	-0.043
	[-1.63]	[-2.6]	[-1.73]	[-2.34]
completed tertiary	0.001	-0.037	0.002	-0.033
	[.05]	[-1.94]	[.13]	[-1.61]
age	-0.027	-0.028	-0.030	-0.033
	[-4.6]	[-3.79]	[-4.67]	[-4.08]
age squared	0.017	0.015	0.022	0.018
	[3.03]	[2.05]	[3.65]	[2.35]
ln(household size)	0.023	0.052	0.036	0.069
	[1.24]	[2.25]	[1.85]	[2.85]
male	-0.015	-0.022	-0.005	-0.005
	[-1.17]	[-1.36]	[-.39]	[-.3]
married, spouse not employed	0.003	-0.002	-0.005	-0.007
	[.21]	[-.11]	[-.35]	[-.39]
single	0.011	0.032	0.032	0.067
	[.41]	[.97]	[1.17]	[1.94]
other household	0.007	-0.006	0.015	0.002
	[.34]	[-.22]	[.69]	[.09]

atlantic	0.002 [.11]	0.014 [.57]	0.002 [.12]	0.012 [.49]
quebec	-0.011 [-.78]	-0.028 [-1.56]	-0.009 [-.59]	-0.026 [-1.41]
prairies	-0.003 [-.18]	-0.016 [-.81]	-0.005 [-.31]	-0.023 [-1.13]
bc	0.044 [2.24]	0.041 [1.67]	0.038 [1.91]	0.042 [1.66]
manager, reference job	-0.003 [-.24]	-0.007 [-.42]	0.002 [.17]	0.001 [.08]
blue collar, reference job	0.009 [.57]	0.011 [.59]	0.012 [.74]	0.008 [.39]
unionized, reference job	0.003 [.21]	0.007 [.43]	0.001 [.1]	0.002 [.09]
tenure 3-10 years, reference job	-0.004 [-.29]	-0.004 [-.24]	-0.004 [-.3]	0.000 [.01]
tenure 10+ years, reference job	-0.036 [-2.32]	-0.050 [-2.53]	-0.029 [-1.75]	-0.040 [-1.94]
importance of reference job	-0.026 [-1.15]	-0.038 [-1.34]	-0.016 [-.65]	-0.039 [-1.31]
93 second window	0.012 [.97]	0.017 [1.07]	0.006 [.45]	0.014 [.83]
95 first window	0.057 [3.35]	0.076 [3.51]	0.052 [2.92]	0.070 [3.14]
95 second window	0.059 [3.73]	0.074 [3.71]	0.069 [4.17]	0.082 [3.91]
constant(1)	-0.028 [-.89]	-0.066 [-1.68]	-0.060 [-1.8]	-0.103 [-2.48]
constant(2)			-0.049 [-1.42]	-0.247 [-5.55]
N	1497	1497	1315	1315
R - square(1)	0.05	0.05	0.05	0.05
R - square(2)			0.03	0.02
Regression F-test	$F_{22, 1474} = 3.87$ ($P < 0.001$)	$F_{22, 1474}=3.48$ ($P < 0.001$)		
Over-identification Test (common slopes)	-------	--------	$\chi^2_{(22)} =30.8$ ($P = 0.10$)	$\chi^2_{(22)} =29.18$ ($P = 0.14$)
Test of common intercepts	-------	-------	$\chi^2_{(1)} = 0.80$ ($P = 0.37$)	$\chi^2_{(1)} =73.17$ ($P < 0.001$)

Notes:
t stats in square parentheses.

TABLE 8: Determinants of $\Delta \ln C_t$, Regression Estimates.		
Specification	Raw Regression	Raw Regression
Separation Category	Layoff, No Expectation of Recall	Layoff, No Expectation of Recall
Comparison Group	-----	-----
Sample	Employed at Last Interview	Employed at Last Interview
Dependent variable	$\Delta \ln Y_{2,0}$ (lagged)	$\Delta \ln C_{2,0}$ (lagged)
completed highschool	-0.018 [-.3]	-0.015 [-.77]
completed tertiary	0.010 [.14]	0.035 [1.65]
age	-0.014 [-.52]	-0.023 [-2.64]
age squared	-0.025 [-.96]	0.018 [2.21]
ln(household size)	0.051 [.65]	0.027 [1.07]
male	0.004 [.08]	-0.008 [-.46]
married, spouse not employed	-0.022 [-.37]	-0.008 [-.4]
single	0.056 [.51]	-0.001 [-.02]
other household	-0.068 [-.77]	0.022 [.78]

atlantic	0.105 [1.26]	-0.027 [-.99]
quebec	0.038 [.63]	0.013 [.66]
prairies	-0.057 [-.88]	-0.031 [-1.48]
bc	0.073 [.9]	0.053 [2.04]
manager, reference job	0.005 [.09]	-0.002 [-.11]
blue collar, reference job	0.073 [1.15]	0.004 [.2]
unionized, reference job	-0.039 [-.72]	0.022 [1.25]
tenure 3-10 years, reference job	0.033 [.63]	-0.006 [-.36]
tenure 10+ years, reference job	-0.201 [-3.01]	-0.059 [-2.74]
importance of reference job	-0.044 [-.47]	0.008 [.28]
93 second window	-0.069 [-1.29]	0.004 [.23]
95 first window	-0.115 [-1.65]	0.041 [1.82]
95 second window	-0.004 [-.06]	0.070 [3.13]
constant	-0.130 [-.97]	-0.034 [-.78]
N	812	812
R - square	0.04	0.08
Regression F-test	$F_{22, 789} = 1.35$ ($P = 0.13$)	$F_{22, 789} = 3.17$ ($P < 0.001$)

Notes:
t stats in square parenthesis.

TABLE 9: Determinants of ln C_2, (levels) Regression Estimates.		
Specification	Raw Regression	
Separation Category	Layoff, No Expectation of Recall	
Comparison Group	-----	
Sample	All Last Interview Respondents	
Dependent variable	ln C_2	
completed highschool	0.083	[2.6]
completed tertiary	0.180	[5.09]
age	-0.045	[-3.34]
age squared	-0.059	[-4.49]
ln(household size)	0.229	[5.35]
male	0.037	[1.25]
married, spouse not employed	-0.111	[-3.36]
single	-0.272	[-4.5]
other household	-0.215	[-4.39]
atlantic	-0.212	[-4.81]
quebec	-0.172	[-5.23]
prairies	-0.074	[-2.04]
bc	0.023	[.5]
manager, reference job	0.109	[3.47]
blue collar, reference job	-0.026	[-.73]
unionized, reference job	0.032	[1.06]
tenure 3-10 years, reference job	0.050	[1.75]
tenure 10+ years, reference job	0.021	[.58]
importance of reference job	0.033	[.63]
93 second window	-0.014	[-.46]
95 first window	0.153	[3.86]
95 second window	0.170	[4.61]
constant	0.146	[2.01]
N	1494	
R - square	0.27	
Regression F-test	$F_{22,1471} = 25.23$ ($P < 0.001$)	

Notes:
t stats in square parenthesis.

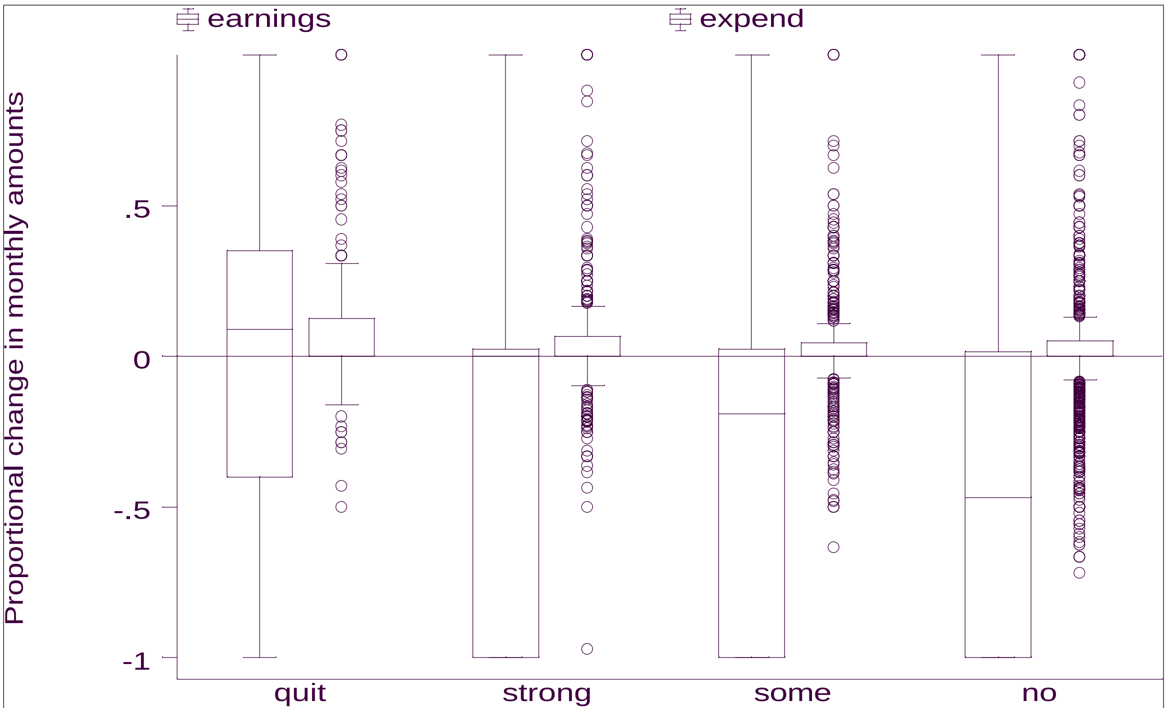

Fig 1: Changes in Earnings, Expenditure

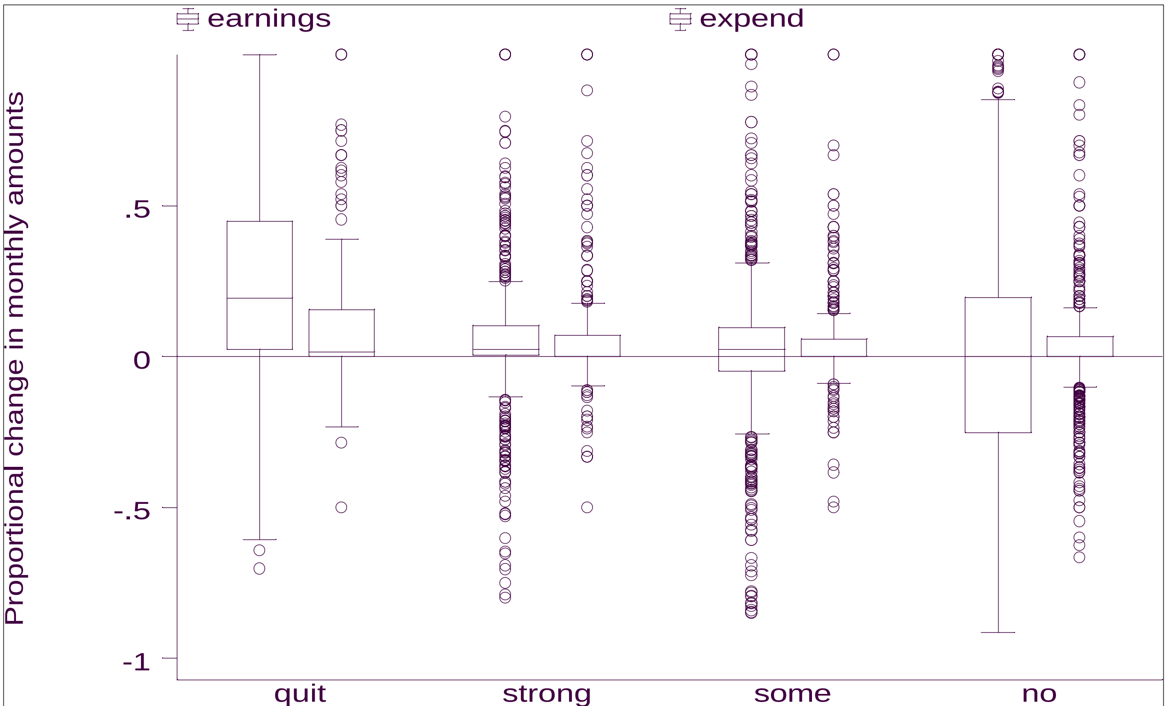

Fig. 2: Changes, Employed Sample

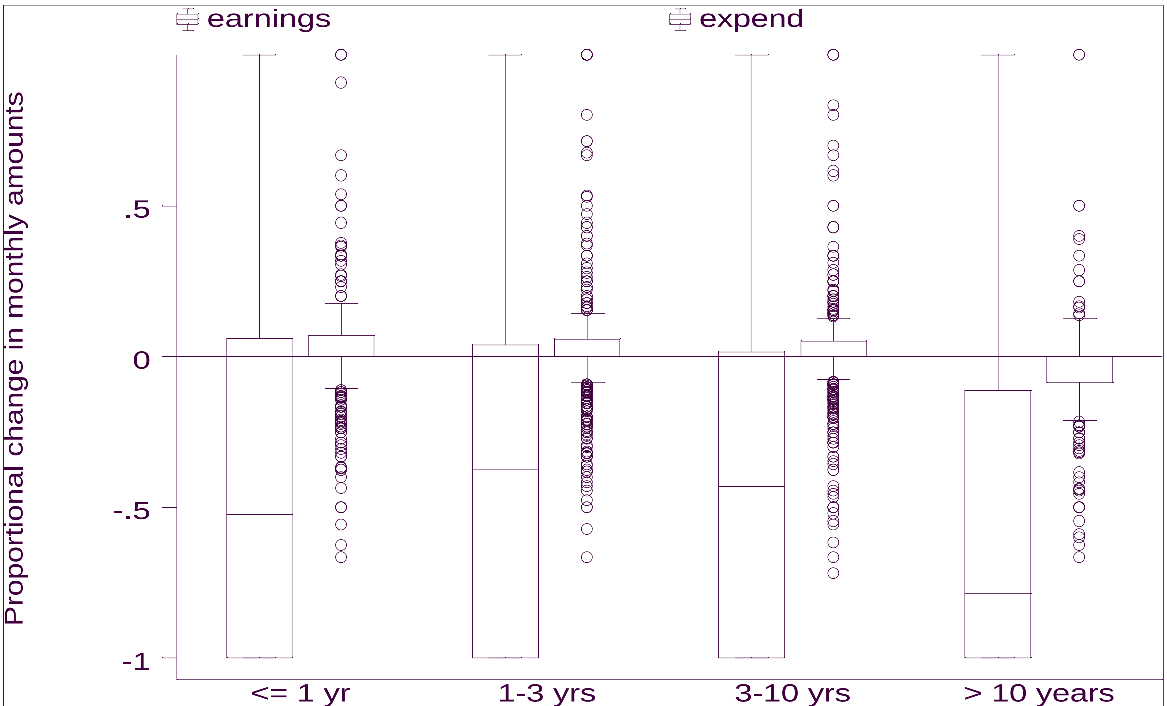

Fig 3: Changes by Tenure

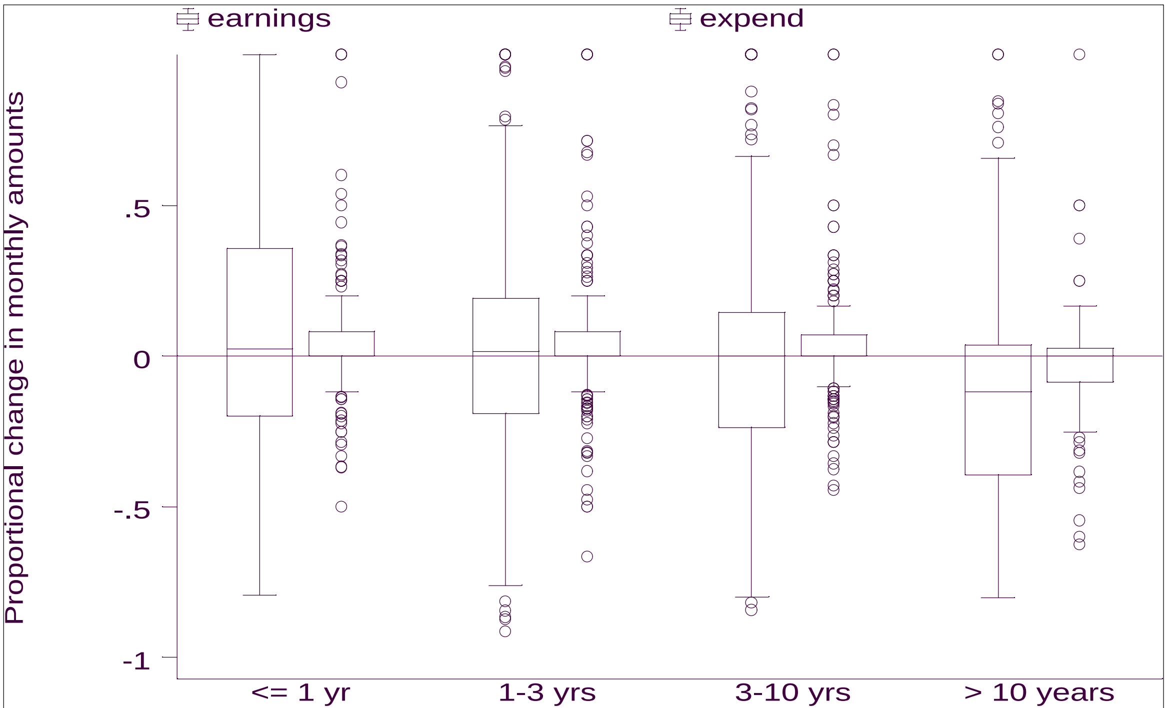

Fig. 4: Changes, Employed Sample, by Tenure