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Unemployment Insurance Evaluation Series

Human Resources Development Canada (HRDC), in its policies and programs, is committed to assisting all Canadians in their efforts to live contributing and rewarding lives and to promote a fair and safe workplace, a competitive labour market with equitable access to work, and a strong learning culture.

To ensure that public money is well spent in pursuit of this mission, HRDC rigorously evaluates the extent to which its programs are achieving their objectives. To do this, the Department systematically collects information to evaluate the continuing rationale, net impacts and effects, and alternatives for publicly-funded activities. Such knowledge provides a basis for measuring performance and the retrospective lessons learned for strategic policy and planning purposes.

As part of this program of evaluative research, the Department has developed a major series of studies contributing to an overall evaluation of UI Regular Benefits. These studies involved the best available subject-matter experts from seven Canadian universities, the private sector and Departmental evaluation staff. Although each study represented a stand alone analysis examining specific UI topics, they are all rooted in a common analytical framework. The collective wisdom provides the single most important source of evaluation research on unemployment insurance ever undertaken in Canada and constitutes a major reference.

The Unemployment Insurance Evaluation Series makes the findings of these studies available to inform public discussion on an important part of Canada's social security system.

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Abstract

In April 1993, Bill C-113 introduced two important changes to Canada's regular Unemployment Insurance system: (1) Voluntary quitters without just cause were **disentitled** from UI receipt, and (2) the benefit replacement rate was cut from 60 to 57 percent for UI recipients. Obviously both changes could affect the material well-being of those individuals who have experienced a job separation since that time. This study examines the relationship between the level of benefit and income and living standards. The analysis covers only those individuals who were still in their first spell of unemployment at the first interview of the COEP Survey, i.e., approximately six months between the pre and post Bill C-113 period.

The information for the empirical work is based on data collected by the Canadian Out of Employment Panel (COEP) Survey. Although the data set records primarily the pre-and-post Bill C-113 experiences of unemployed individuals, the study goes beyond the evaluation of the impacts of Bill C-113. It attempts to shed some light on the implications of UI benefit rate reduction on UI recipients' livelihood under more general circumstances.

The empirical evidence confirms that UI benefit rates significantly influence net personal incomes of UI recipients. At the 60 percent benefit rate, net personal incomes of UI recipients are 68 percent and 61 percent of their pre-unemployment earnings for women and men respectively. At the 57 percent rate, the corresponding net personal incomes become 63 percent for women and 59 percent for men. This conclusion remains valid when the data are subject to econometric testing for statistical significance.

By gender, the impact of a UI benefit cut is harder on women than on men. Men generally have higher "other" sources of income and have higher marginal tax rates, which tend to cushion the blow of UI benefit reduction on personal and household incomes. The significance of a UI benefit cut on UI recipients can be further illuminated by using the estimated equations to perform other calculations. A 5 percent cut in benefits (e.g., the benefit replacement rate is cut from 60 to 57 percent) would lead to approximately a 3.5 percent reduction in the ratio of "current household income/pre-unemployment merit income".

For consumption, the empirical evidence shows that UI recipients' consumption generally falls with a reduction in UI benefit rate. The study estimates that a cut in the benefit rate from 60 percent to 50 percent would lead to a fall in household consumption between 3 to 6 percent. This reduction applies across the three categories of goods expenditures (food at home, food outside the home, and clothing) as well as to total consumption.

As expected, the impacts of UI benefit reduction vary across different households. Repeat users (e.g., three claims in the last five years) would have suffered more in their standards of living than those who do not claim UI benefits regularly. Amongst households that have at least one member who is unemployed for six months or more the following are the most vulnerable to UI benefit cuts: (i) households in which the unemployed person is the "major bread winner", and (ii) households in which the unemployed person is a repeat user.

The empirical evidence confirms that UI benefit rates significantly influence net personal incomes of UI recipients.



Introduction

In April 1993, Bill C-113 introduced two important changes to Canada's Unemployment Insurance (UI) system:

- Those who quit their jobs voluntarily, without cause, were no longer entitled to UI benefits; and,
- The ratio of benefits to insurable earnings, otherwise known as the replacement rate, was cut from 60 percent to 57 percent for UI recipients.

The purpose of this report is to examine the impact of those changes on the incomes and material well-being of individuals who became unemployed after April 1993. The information for the empirical work is based on data collected by the Canadian Out of Employment Panel Survey (COEP) and data from administrative records.

This analysis deals only with those individuals who were still in their first spell of unemployment at the time of the first COEP interview. The sample is divided into two cohorts: those who became unemployed between January 31 and March 13, 1993, prior to the amendments; and those who became unemployed between April 25 and June 5, 1993, who were subject to the revised UI system.

The study keeps the distinction between income and material well-being. Its rationale is that a short-run decrease in an individual's income does not necessarily translate into a drop in the material living standard for the whole household. In the short-run, a household may be able to run down its financial resources or to borrow money to maintain its living standard. Alternatively some other members of the household may increase their income either by taking new jobs or by working longer hours in existing jobs. Even if the household finds that it must reduce its total expenditure in the short-run, this may not lead to a meaningful fall in material well-being. The household may reallocate its total expenditure among goods and services to keep material welfare nearly constant.

The COEP is a survey of individuals separating from jobs and was designed to supplement existing administrative data. It is of particular relevance to the evaluation of Bill C-113 because it consists of two cohorts, one experiencing a job separation between January 31 and March 13, 1993, and the other between April 25 and June 5, 1993. This affords us a data set of quasi-experimental nature that can be used to gauge the impacts of the policy changes.

In addition to the COEP data, we augment the income information with the tax information of gross earnings in the 1992 tax year and the insured earnings of the administrative records. From the COEP data, we extract the income information from the answers to the survey's questions: "What was your total personal income in the past month after deductions?" and "What was the total income of your household this past month after deduction?" For consumption, we obtain the data from the answers to COEP'S small number of tightly focused questions on consumption. The questions were designed to allow the construction of a measure of household expenditure on all items including housing. Subsidiary questions were also asked about housing expenditures to allow the construction of non-housing total expenditure. Questions concerning assets and debts and how the level of total consumption has changed since last job separation were also asked.

The study keeps the distinction between income and material well-being. Its rationale is that a short-run decrease in an individual's income does not necessarily translate into a drop in the material living standard for the whole household.

The study attempts to establish the relationship between UI benefit and income, and the relationship between UI benefit and the material living standard of the unemployed. This requires the study to classify the unemployed individuals of the COEP survey into three groups:

- A “no UI benefit” group,
- The “reference” group who received UI with a replacement rate of 60%; and,
- The “low benefit” group who received benefits with a replacement rate of 57%. The analysis covers personal income, household income, household total consumption and consumption-allocation patterns.

The study uses descriptive statistics and regression analysis (including ordinary least squares and probit analysis). The specifications of the econometric equations pay special attention to their conceptual and logical consistency. This is particularly important to the empirical work on “Living Standards During Unemployment”. The COEP survey data cover only “food at home”, “food outside the home”, “clothing” and “total consumption”. Since the three consumption components are integral parts but do not add up to “total consumption”, this presents a technical challenge. The report has an extensive discussion on the merits and demerits of three feasible approaches to the empirical work on this topic. They are:

- The direct method, which involves the use of regression methods (including the standard regression technique and probit analysis) to estimate the impacts of UI legislative changes on the three consumption components and total consumption directly;
- The factor analytic method, which applies a specific technique of “factor analysis” to the available consumption data to construct a measure of “predicted total expenditures on consumer goods and services”: and,
- The structural method, which integrates methods(1) and (2).

Section 1 of this report lays some of the groundwork for research and discusses some of the theories that must be taken into consideration in the work. It also outlines some characteristics of the sample used for analyses.

Section 2 examines how personal and household incomes are affected by changes in benefit levels and entitlements, such as those that came into effect in April, 1993, and Section 3 focuses on how UI legislative changes might affect household expenditures.

Section 4 presents conclusions about the impacts on the incomes and material well-being of unemployed people in Canada, if UI benefits are reduced, or if they receive no UI benefits at all. Appendices A and B describe the research methods in more technical detail.

1. How Unemployment Affects Incomes and Living Standards



In this study we examine how a period of unemployment affects personal and household incomes and material living standards. In doing so, we take into consideration:

- **A household's ability to maintain a certain level of material well-being.**
This depends largely on the household's assets, debts, current income and access to various types of credit.
- **A household's actual level of material well-being.**
This depends on what the household already had in the way of housing and possessions before a member became unemployed, and on what it purchased after the job loss, especially non-durable goods such as food.

It is critical to differentiate between these two concepts. A significant but temporary cut in a person's income does not necessarily result in a significant reduction in a household's material welfare, if that household has enough financial savings it can draw on, if it can borrow what it needs, or if some other member of the household can increase his or her income by taking a new job or by working longer hours in an existing job.

Even if a household must lower its expenses for a while, it may be able to keep its material well-being nearly constant by spending less on durable goods such as clothing and furniture, and concentrating available resources on more immediate needs. For example, by postponing the purchase of a new winter coat, a person may spend less than he or she had originally planned, yet experience little short-term impact on material well-being if the old coat provides almost as much warmth. or "consumption service," as would a new one.

Characteristics of Survey Respondents

We took a sample of 3,229 respondents who were still in their first spell of unemployment at the time they were interviewed. Depending on what we wanted to find out, we then analyzed the sample along several different lines, including sex, family type, whether or not each respondent was receiving UI benefits, and groups or "cohorts," which are defined in more detail below.

Sex

Our initial analyses suggested that changes in UI benefits and entitlement affect women and men differently.

Family Type

Analyzing the data by family type enabled us to learn how the loss of a job affects personal incomes and household incomes. Obviously, in single-person households, the personal and household incomes are the same. Table 1 below shows that men and women in our sample have similar living arrangement patterns: about 60 percent live with a spouse, with or without children.

A significant but temporary cut in a person's income does not necessarily result in a significant reduction in a household's material welfare.

Table 1
Demographic Characteristics of Survey Respondents

Household type	Percent	
	Female	Male
Single person	12.3	18.8
Married couple	20.1	16.8
Married couple with children	39.5	34.6
Other	28.1	28.8
Total	100.0	100.0

Receiving/Not Receiving UI Benefits

In an effort to learn more about the general impact of UI benefits on the unemployed, we divided the sample group according to whether or not each respondent was receiving UI benefits at the time of the first interview. We then used descriptive statistics and econometric methods to compare those with UI benefits and those without UI benefits.

Cohort

The respondents are divided into two major groups, or cohorts.

Cohort 1:

Respondents who became unemployed between January 31 and March 13, 1993

Cohort 1 includes those who qualified for UI benefits before the changes to the Unemployment Insurance Act. They were potentially entitled to a maximum replacement rate of 60 percent of their insurable earnings. It also includes some unemployed individuals who were not receiving UI benefits.

Cohort 2:

Respondents who became unemployed between April 25 and June 5, 1993

Cohort 2 includes respondents who qualified for benefits after the April 13 changes; their maximum replacement rate is 57 percent of their insurable earnings. It also includes some unemployed individuals who were not receiving UI benefits.

This division by cohort allowed us to examine the impact of the cut in benefits introduced by BillC-113 on personal and household incomes and material well-being.

Table 2 shows that there were slightly more Cohort 2 respondents than Cohort 1 respondents and slightly more men than women in our sample.

Table 2
Breakdown by Sex and Cohort

Cohort	Percent		Total
	Female	Male	
1. Became unemployed before April 13,1993	21.8	25.9	47.7
2. Became unemployed after April 13,1993	24.3	26.0	52.3

Tables 3 and 4 show that a higher proportion of men and women in Cohort 2 received benefits than did men and women in Cohort 1. That is, relatively fewer men and women who were eligible for a 60 percent replacement rate received UI benefits compared to women and men who were eligible for only a 57 percent replacement rate. This is an unexpected finding since Cohort 2 respondents were not entitled to benefits if they quit their jobs voluntarily.

Table 3
Women Receiving/Not Receiving UI Benefit by Cohort

	Percent	
	Cohort 1	Cohort 2
Receiving UI	22.7	34.4
Not receiving UI	22.8	20.1

Table 4
Men Receiving/Not Receiving UI Benefit by Cohort

	Percent	
	Cohort 1	Cohort 2
Receiving UI	24.8	30.4
Not receiving UI	21.6	23.1

Data and Related Methodological Issues

Ideally we would like to treat the effects of the 1993 changes to the Unemployment Insurance Act as a pure experiment. But because we did not randomly assign people to the before and after groups, we had to allow for the fact that there may be systematic differences in socio-economic factors and personal attributes associated with members of Cohorts 1 and 2. Our preliminary data analysis suggests that, in the econometric work, it is critical to control for pre-unemployment differences in income and consumption.

Specifically, the data analysis examines the influences of the following factors:

- Three separate measures of income before unemployment:
- Work patterns of other household members before the respondent became unemployed: and,
- Pre-unemployment consumption, that is, the amount spent on food, housing, clothing and other household expenses.

This analysis is described in detail below:

Respondents' Incomes Before Unemployment

We used three measures of respondents' income before they became unemployed:

- **Survey information** on respondents' weekly earnings immediately before the unemployment date;
- **Administrative data** on respondents' insurable earnings for the 20 weeks before unemployment; and,
- **Tax information** on gross earnings in 1992.

Each of the three income measures has disadvantages. The **survey measure** is for a short period only and may not reflect "normal" income over the recent past. Also, the survey amounts are likely to be less accurate than the amounts derived from the administrative and tax record measures. Table 5 indicates that for respondents who are receiving UI benefits, the survey measure has more missing values than either of the other two variables.¹ It should be noted, however, that the 90 percent response rate is impressively high for an income-related question.

The **administrative measure** is available only for those who have claimed benefits. It only measures insurable earnings, not net income; because it ignores earnings that exceed the maximum insurable amount, it underestimates the incomes of high earners.

The **tax measure** of gross earnings in 1992 is likely the most accurate measure of past incomes. However, it covers a period some time before the date of unemployment. It also has the disadvantage of being a pre-tax measure and so is not directly relevant to household decisions. Finally, the tax measure includes some zero incomes and very low incomes that are difficult to believe.

Columns two, three, four and five of Table 5 show the numbers of negative, zero, positive and missing values for each of our three income variables. We did not use all of these measures for all respondents.

Our analysis shows that whether or not the respondent is receiving UI benefits has a significant impact on income. The sex of the respondent has much less of an impact. Of those who were receiving UI benefits, the pre-unemployment incomes of Cohort 1 respondents were significantly lower than those of Cohort 2. Of those who were *not* receiving UI benefits, Cohort 1 respondents had *higher* incomes than did those in Cohort 2.² This indicated that we had to control for pre-unemployment incomes in some way.

There tends to be a high correlation — between (.6 and (.8 — between the three income measures we used. We used the survey measure of pre-unemployment earnings where this information was available, and used the administrative and tax measures, as well as demographic information, to impute values for

1 In the construction of this variable, some implausibly high values — over \$10,000 per week — were set to zero.

2 We are not suggesting a cause-and-effect relationship between the cohort variable and employment income.

10 percent of the sample for which survey data was missing. This was accomplished by regression analysis. Since the regression fit for the other 90 percent of the sample group is very high (the R^2 is 0.6) we were confident that it gave us a fairly reliable characterization.

Table 5
Pre-Unemployment Income Data for All Respondents

Variable	Receiving UI	Negative Income	Zero Income	Received Some Income	Missing Data	Total
Survey data — weekly earnings prior to unemployment	No	0	0	725	19	604
	Yes	0	0	2,172	253	2,425
Administrative data —20weeks average insurable earnings	No	0	10	0	794	604
	Yes	0	0	2,425	0	2,425
Tax data — 1992 gross earnings	No	0	26	727	51	604
	Yes	0	42	2,260	123	2,425
Survey data— spouse's hours of work (married only) at unemployment date	No	0	167	314	15	496
	Yes	0	465	915	66	1,468

Note: *Unweighted data*

In all that follows, this corrected measure serves as our control for pre-unemployment income. Row four of Table 6A shows the effect of cohort on this corrected measure for those who are receiving UI benefits: row four of Table 6B shows the effect for respondents who are not receiving UI benefits. As we expected, it is very similar to the effect of cohort on the uncorrected measure.

Table 6A
Pre-Unemployment Income: Cohort Effects for Respondents Receiving UI Benefits

Variable (log)	Sex	Coefficient on Cohort Dummy (x 100)	t-value
Survey data —weekly earnings in pre-unemployment job	Female	9.1	3.1
	Male	10.6	3.4
Administrative data 20 weeks average of insurable earnings	Female	10.5	4.1
	Male	6.9	3.8
Tax data: 1992 gross earnings	Female	4.8	0.9
	Male	12.3	2.0
Weekly earnings Imputed from survey data	Female	9.4	3.4
	Male	10.8	3.7

Table 6B
Pre-Unemployment Income: Cohort Effects for Respondents Not Receiving UI Benefits

Variable (log)	Sex	Coefficient on Cohort Dummy (x 100)	t-value
Survey data — weekly earnings in pre-unemployment job	Female	-4.0	0.6
	Male	-8.8	1.3
Tax data: 1992 gross earnings	Female	-19.0	1.9
	Male	-44.5	3.5
Weakly earnings imputed from survey data	Female	-4.4	0.7
	Male	-8.2	1.2

Work Patterns of Other Household Members Before the Respondent Became Unemployed

When looking at household incomes, we considered that we might need to control for the income earned by other members of the respondent's household in the pre-unemployment period. The income of a married person's spouse is of particular importance in determining household income. Consequently, we asked the married respondents how many hours their spouse was working at the date of unemployment. Their responses are shown in row two of Table 7. Although there are some missing values, most of the respondents were willing and able to answer this question.

Table 7 also presents some summary statistics on how many spouses were employed and, of those, how many hours they were working at the time the respondents became unemployed.³ As expected, we found that the female respondents were more likely than the male respondents to have a working spouse. Also, the spouses of the female respondents tended to work more hours. Our analysis showed no significant correlations between cohort and the spouse's pre-unemployment work pattern.

Table 7
Spouse's Work Patterns Prior to Unemployment

Variable	Receiving UI	Sex	Coefficient on Proportion/ Cohort Dummy		t-value
			Average	(x 100)	
Spouses involved in work force at unemployment date (Probit equation)	No	Female	77.0	-0.02	0.1
		Male	51.0	-0.04	0.2
	Yes	Female	75.0	-0.06	0.6
		Male	54.0	0.06	0.8
Hours of market work per week (Regressions for positive hours only)	No	Female	41.3	0.11	0.1
		Male	35.8	-0.64	0.4
	Yes	Female	41.6	-0.64	1.0
		Male	34.5	0.50	0.5

³ To estimate the probability of a spouse being employed, we ran a probit on the cohort dummy. The cohort dummy is equal to 0 if the individual belongs to cohort 1, and equal to 1 if the individual is a member of cohort 2. With respect to hours of market work in Table 7, the regressions are for positive hours only.

Pre-Unemployment Consumption

The only information we have on pre-unemployment consumption was obtained from the survey, which asked respondents about their current total consumption and whether their consumption changed after they became unemployed. In our opinion, the responses we received cannot be relied upon for accuracy.

Table 8 shows negative or zero consumption for some respondents. The greatest cause for concern, however, is that values are missing for 22 percent of the sample group. Table 9 presents the estimated cohort effects for those who recorded positive consumption levels. The results mirror the income estimates. However, the coefficients are generally lower and less significant. This presumably indicates that the data are less reliable. Although we will make some use of these data in the analysis below, we prefer to use the measure of pre-unemployment income to control for consumption as well as for income.

Table 8
Pre-Unemployment Consumption Data for All Respondents

Variable	Receiving	Consumption			Missing Data	Total
	UI	Negative	Zero	Positive		
Survey data:	No	0	5	627	172	604
total consumption at unemployment date	Yes	2	3	1,674	546	2,425

Note: Data is unweighted.

Table 9
Pre-Unemployment Consumption: Cohort Effects

Variable (log)	Receiving	Sex	Coefficient on Cohort Dummy	t-value
	UI		(x 100)	
Survey data: total consumption at unemployment date	Yes	Female	3.5	0.9
		Male	5.3	1.3
	No	Female	-4.9	0.8
		Male	-13.7	1.6

Our investigations revealed that in general, there is a strong relationship between pre-unemployment income and the cohort effect. This suggests that we cannot treat the cohort effect as a pure experimental effect. Instead, we shall have to control for pre-unemployment differences in income in all of our analysis below.



2. How Changes in UI Benefits and Entitlement Affect Incomes

By evaluating econometrically the differences between the responses of those with benefits and those with no benefits in Cohorts 1 and 2, we were able to infer how the personal incomes of unemployed men and women are affected by a cut in benefits or the total absence of benefits.

Personal Income

During the first interview, the survey asked respondents their total personal income after deductions in the past month from all sources, including UI benefits and Social Assistance. From this information, we assessed each respondent's total personal income relative to UI benefits and relative to pre-unemployment earnings. By evaluating econometrically the differences between the responses of those with benefits and those with no benefits in Cohorts 1 and 2, we were able to infer how the personal incomes of unemployed men and women are affected by a cut in benefits or the total absence of benefits.

All but 6 percent of the respondents answered the survey question on personal income, the results of which are shown in Table 10 below. However, some of the answers are suspect, especially for the 7 percent who reported having no income.

Table 10
Personal Income of Respondents

Variable	Receive UI	Negative Income	Zero Income	Positive Income	Missing Data	Total
Personal income	No	0	153	957	54	604
(survey data)	Yes	0	80	2,198	147	2,425

Note: Data is unweighed.

Row one of Table 11 — the administrative data — shows that the benefits received by Cohort 2 women and men are 5.4 percent and 3.9 percent higher respectively than the benefits received by Cohort 1 women and men. On the surface this may appear somewhat surprising because the 1993 policy changes were supposed to produce the opposite effect. The answer to this apparent puzzle lies in the fact that Cohort 2 respondents had higher pre-unemployment incomes. On examination, we found that the ratio of benefits to insurable earnings was almost exactly 60 percent for Cohort 1 respondents and 57 percent for Cohort 2 respondents. This indicated once again that we had to control for pre-unemployment differences in income.

Table 11
Benefits and Incomes: Cohort Effects

Variable	Sex	Coefficient on Cohort Dummy (x 100)	t-value
Log (weekly UI benefit) (administrative data)	Female	5.4	2.1
	Male	3.9	1.7
Ratio of UI benefit to imputed pre-unemployment earnings	Female	-2.2	1.6
	Male	-6.3	3.3
Ratio without outliers removed	Female	-3.1	2.9
	Male	-3.4	2.7

Note: UI recipients only.

Row two of Table 11 shows the estimated impact of changes in UI on the incomes of women and men in Cohort 2 compared to Cohort 1. Women and men receive 2.2 percent and 6.3 percent less income respectively. These results are consistent with the results derived algebraically.⁴ It should be noted, however, that the differences shown in Table 11 may be affected by a few outliers, that is, factors so far removed from the norm that they can distort “the overall picture. Consequently, for a few of the respondents, the ratio of benefits to pre-unemployment earnings is above three. When we drop the outliers, the income of women in Cohort 2 falls by 3.1 percent and the income of men in Cohort 2 falls by 3.4 percent. We consider this to be a more reliable estimate of the relationship between benefit cuts and pre-unemployment income. The results with this restricted sample are shown in row three of Table 11.

Before studying the interaction between cohort and personal income at the interview date, we examined the reliability of the survey measure of personal income. To do this, we first looked at the administrative data to find out precisely what UI benefit each respondent was receiving. In theory, this benefit should form the bulk of a recipient’s personal income. In particular, it should represent a lower limit on gross personal income, though not necessarily on net income because the survey specifically asked respondents about their income after deductions.

We multiplied weekly UI benefits by 4 to make them monthly, then figured out the ratio of reported monthly income to monthly benefits. Next we divided those figures into categories. As Table 12 reveals, about three-quarters of all respondents reported a personal income that was between 75 percent and 125 percent of their UI benefit. For most, the ratio was less than 100 percent. This suggests that the survey measure of personal income is fairly reliable for all but those who reported a very low income.

Since personal income includes UI benefits, there should be a slight drop for Cohort 2 recipients but no change for non-recipients. Cohort 1 and Cohort 2 respondents may have had different personal incomes simply because their pre-unemployment earnings are different. Therefore, we could not just look at mean figures to determine the effect of UI benefits on personal income. Table 13 gives a clearer picture of the impact of UI benefits on the ratio of first interview personal incomes to pre-unemployment earnings. Once again, we have restricted the sample to minimize the influence of outliers. In this case, we only consider ratios less than two. This seems to restrict the sample of unemployed people reasonably enough.

4 Calculation of the ratio of benefits to weekly pre-unemployment earnings:

Let i refer to cohort and let:

E_i = insurable earnings

Y_i = earnings above E_i ($E_i + Y_i$ = pre-unemployment earnings)

B_i = UI benefits

R_i = ratio of benefits to ($E_i + Y_i$)

The administrative data indicates that:

$B_1 = 0.60E_1$ and $B_2 = 0.57E_2$

Rows one and four of Table 6A indicates that:

$E_2 = 1.105E_1$ and $(E_2 + Y_2) = 1.094(E_1 + Y_1)$ for females: and,

$E_2 = 1.089E_1$ and $(E_2 + Y_2) = 1.108(E_1 + Y_1)$ for males

Given these numbers, $R_2 = 0.96R_1$ for females and $R_2 = 0.93$, for males. This translates into a fall of 4 percent and 7 percent for females and males respectively.

Table 12
Total Personal Income of All UI Recipients as a Percentage of UI Benefits

	Percentage						
	0	0 - 75	75-100	100	100-125	125-300	More than 300
Female	3.5	7.8	53.7	5.6	11.8	9.8	7.7
Male	3.3	10.8	65.4	2.3	6.7	5.5	6.1

Table 13
**Personal Income Relative to Pre-Unemployment Earnings:
 Effects of Cohort and Receipt of UI Benefits**

	Intercept	Cohort 1 No Benefit	Cohort 2 With Benefit	Cohort 2 No Benefit
Female	66.6	-20.4 (8.11)	-4.7 (2.1)	-18.9 (7.3)
Male	60.6	-10.8 (4.6)	-3.7 (1.7)	-13.5 (6.0)

*Note: Reference group: cohort 1 with UI benefits
 All coefficients multiplied by 100; t-values in brackets.*

The results presented in Table 13 are quite striking. They clearly show that changes in personal income mirror changes in the amount of UI benefits a person receives. Of the Cohort 2 respondents who received benefits — shown in column three of Table 13 — women’s incomes dropped by 4.7 percent, while men’s incomes dropped by 3.7 percent. This suggests that a 3.3 percent cut in benefits results in a slightly higher cut in personal income. The slightly lower drop for men may reflect the fact that the men may have had a higher marginal tax rate.

Columns two and four of Table 13 show that there is no significant difference between the personal incomes of Cohort 1 and Cohort 2 respondents who are not receiving UI benefits.⁵

When comparing the incomes of those with benefits and those with no benefits from the figures in Table 13, it is important to note that much depends on pre-unemployment earnings. As a group, Cohort 2 recipients have significantly higher pre-unemployment earnings than Cohort 1 recipients: the exact opposite is true for the groups of non-recipients. Also, the selection into the benefit/no-benefit groups is more likely to be endogenous than the selection into cohort. Therefore, we have to use caution when interpreting the results. Nevertheless, we found a smaller difference than we had expected between the personal incomes of those receiving benefits and those not receiving benefits.

Table 10 showed almost 20 percent of the no-benefit group reported zero income, compared with 3.5 percent of the group with benefits. Despite this, the ratios of the no-benefit group’s mean personal incomes to their pre-unemployment earnings are only 20 percent and 12 percent lower for women and men respectively than they are for the group with benefits. Somehow those with no benefits manage to

⁵ Formal F-tests confirm this

insulate themselves to a certain extent from a large drop in income when they become unemployed. Although we are curious about how they do it, we will have to let future studies find the answers.

Household Income

We have shown that the cut in benefits introduced by Bill C-113 appears to reduce a woman's personal income by 4.7 percent and a man's by 3.7 percent. Here we will examine how these changes affect household income, and in turn, how they affect a household's expenditures and material well-being. We are particularly interested in examining how the availability and level of UI benefits influence households when other family members, especially spouses, are also earning an income.

As with our earlier analyses, we once again divided Cohorts 1 and 2 by sex and benefit category. Our major focus here is on men and women who are receiving benefits, although we have also included those who are not.

The survey asked the sample group, "What was the total income of your household this past month after deductions?" For single-person households, we set the respondent's personal income as the household income. Table 14 shows the group's responses.

Table 14
Household Income at Time of First Interview

Variable	Receiving UI	Negative Income	Zero Income	Positive Income	Missing Data	Total
Household income	No	0	43	588	193	804
	Yes	0	42	1,802	581	2,425

There are considerably more missing answers for household income than for personal income. This may reflect some respondents' reluctance to discuss anything other than their own personal situation. Another 1.5 percent of respondents reported personal incomes that were higher than their household incomes. Since household incomes include personal incomes, these responses were clearly inaccurate, so we eliminated them from our analysis. We also dropped respondents with a missing or zero household income, as well as some other outliers, including anyone whose personal income was greater than his or her pre-unemployment earnings. In addition, we left out some observations if the ratio between a respondent's household income and pre-unemployment earnings was greater than 4.

Results

Of the remaining sample, 33 percent of women and 47 percent of men reported that their personal income was the same as their household income. The equality between the two incomes suggests that these people were either living alone or they were the sole income earners in a multi-person household. Table 15 indicates that, of the multi-person households, 20.3 percent of women and 29.7 percent of men reported equal personal and household incomes. Our earlier conclusions with respect to personal incomes also apply to the household incomes of these respondents. We now look at the remaining households in which the respondent is not the only income earner.

The cut in benefits introduced by Bill C-113 appears to reduce a woman's personal income by 4.7 percent and a man's by 3.7 percent.

Table 15
Personal Income Relative to Household Income

Household Gender Composition	Percent		
	No Personal Income	Personal Income Less than Household Income	Personal Income = Household Income
All households			
Female	8.9	66.3	32.7
Male	6.0	41.1	46.9
Multi-person households			
Female	10.6	69.1	20.3
Male	7.9	62.4	29.7

Table 16 shows some regression results for household income relative to pre-unemployment earnings. Since the sample selection for this group is much greater than the sample selection in the previous section, we are also presenting some results relating to personal income. The results in Table 13 and those in Table 16 are very similar with respect to the effect of the cohort variable on benefit recipients. In Table 16, however, the drop for men is somewhat smaller.

The second row of Table 16 indicates that the household incomes of female UI recipients in Cohort 2 dropped by 11.5 percent, whereas the household incomes of male UI recipients only dropped by an insignificant 3.2 percent. We ran several checks to make sure that the result for women was not being influenced by outliers. Those checks confirmed that the data were valid. The only possible explanation for the large difference is that household incomes for Cohort 1 are higher because of higher incomes from other household members. In that regard, either Cohort 1 household members had higher pre-unemployment incomes to begin with, or their pre-unemployment incomes started off on a par with those of Cohort 2, but changed between the date of unemployment and the date of the first interview.

A change in income can occur if another household member increases his or her earnings to make up for the respondent's lost income. Alternatively, another household member may lose his or her job at the same time. Although the latter event is not likely to happen, it is possible given that members of a household live in the same labour market and may even have been working at the same place prior to unemployment. It is not feasible to study all the work patterns and income possibilities for every member of a household. We can, however, examine the work patterns of the spouses of married respondents. This analysis is restricted to respondents who are receiving benefits.

We found no difference between the cohorts in the pre-unemployment work patterns of spouses. Row three of Table 16, showing our estimate of the probability of a spouse's participation, suggests that there is no significant difference for men or women. We can thus tentatively conclude that the difference in household income between women in Cohort 1 and Cohort 2 are not caused by differences in their spouses' behaviour.

Table 16
Household and Personal Income: Cohort and Benefit Impacts

	Sex	Intercept	Cohort 1 No Benefit	Cohort 2 With Benefit	Cohort 2 No Benefit
Personal income relative to pre- unemployment earnings	Female	67.7	-16.0 (5.6)	-4.5 (-1.8)	-16.9 (5.7)
	Male	61.0	-5.4 (2.2)	-2.3 (1.0)	-5.6 (2.3)
Household income relative to pre- unemployment earnings	Female	152.8	-17.3 (2.3)	-11.5 (1.7)	-1.1 (0.11)
	Male	97.0	-10.6 (2.2)	-3.2 (0.7)	-6.6 (1.3)
Spouse involved in work force (UI recipients only) (Probit equation)	Female	—	—	-6.4 (0.6)	—
	Male	—	—	-0.4 (0.04)	—
Household income relative to pre- unemployment household income	Female	80.3	-27.0 (19.6)	-3.1 (2.5)	-24.4 (17.0)
	Male	73.4	-30.7 (21.0)	-3.7 (2.8)	-30.0 (20.6)

Note: Reference group: cohort/with UI benefits.
All coefficients multiplied by 100; t-values in brackets.

The household's pre-unemployment income is simply its income at the time of the interview minus the respondent's loss in earnings.⁶ This measure of pre-unemployment income seemed to be quite consistent when we checked it. The final row of Table 16 shows the level of current household income relative to this constructed measure of pre-unemployment income. When benefits are cut by 5 percent, we estimate that the household income drops about 3.5 percent below the pre-unemployment household income.

⁶ To find out if the spouses' pre-unemployment incomes differ, we developed the following algebraic analysis. For the purpose of this analysis, let:

- P = respondent's personal income,
- R = respondent's pre-unemployment earnings,
- H = interview I household income.
- B = UI benefit, and
- Y = pre-unemployment household income.

The total incomes of other household members at the pre-unemployment and interview I dates are determined by $(Y-R-(P-B))$ and $(H-P)$ respectively. If the earnings of other household members do not change, we can estimate the household's pre-unemployment income as follows: $Y = H+(R-B)$.



3. *How Changes in Income Affect Household Expenditures*

The principal trade-off in the design is between how accurately expenditure on a particular item is likely to be reported, how important that item is in the household budget and how income elastic the demand for that good is.

Measuring Consumption

The survey asked a number of tightly focused questions that were designed to measure household spending on all items including housing, and on all items excluding housing. Additional questions asked about assets and debts, while others asked respondents to explain how their total consumption had changed from the date of unemployment. In this section, we examine the reliability of the consumption data. This is necessary because we have never before tried to measure consumption from just a few questions. We also discuss here how we can use the answers to the consumption questions.

Most countries now run surveys of family expenditures. Some examples are Statistics Canada's *Survey on Families' Expenditures* (FAMEX) and the *Consumption Experience Survey* (CEX) in the United States. These surveys generally provide very detailed information on the consumption of a wide and inclusive range of goods. However, they are usually very time-consuming since household members must keep detailed diaries of what they buy and how much they spend, or experienced interviewers must collect recalled information in a number of interviews on what respondents consumed in the past. Neither method was feasible for our purposes.

Instead the survey asked four principal questions. These four questions were designed to allow us to construct a measure of total expenditure by means of a post-survey structural analysis. The design of the questions was based partly on a prior analysis of FAMEX data, partly on consumption questions asked in other surveys (especially the "food" question in the PSID), and partly on what we knew to be possible in post-survey analysis. The principal trade-off in the design is between how accurately expenditure on a particular item is likely to be reported, how important that item is in the household budget and how income elastic the demand for that good is.

Specifically, the four principal questions are:

- About how much did you and your household spend last week on **food** that you use at home?
- ∨ About how much did you and your household spend last week **eating out** (excluding any meals at work or school)?
- ∨ About how much did you and your household spend on **clothing** in the past month? and
- ∨ About how much did you and your household spend on **everything** in the past month'?

The last question asked respondents to consider all bills, including rent, mortgage payments, utility and other bills, as well expenses such as food, clothing, transportation, entertainment and any other expenses the household may have had.

The first three questions are sub-components of question four, but they do not account for the total amount: from the 1986 FAMEX⁷ we estimate that they only account for about 28 percent of a typical household's total expenses.

The responses for the four questions are given in Table-17. To "clean" the data, we considered responses to be missing if they showed more than \$10,000 in total expenses, more than \$2,000 in clothing expenses and more than \$1,000 spent on food, either at home or in restaurants. Appendix A gives a detailed explanation of available econometric methods for analyzing the data.

Table 17
Consumption at Time of First Interview

Variable	Negative	Zero	Some	Missing Data	Total
Spent weekly on food at home	0	31	2,857	341	3,229
Spent weekly on eating out	0	2,036	1,033	155	3,229
Spent monthly on clothing	0	1,144	1,785	300	3229
Total spent monthly	0	10	2123	496	3,229

Note: Based on unweighted data.

Results

The response rate was quite high: in all, 77 percent of the sub-sample answered all four questions. However, some of the answers are not consistent. For example, of those who answered all four questions, 7.9 percent indicated that they had spent more on clothing and food combined than they had spent overall. It is impossible to determine whether these anomalies are due to errors in coding and reporting.

The zero responses for total expenditures present another problem because it is difficult to believe that a household didn't buy anything. However, we have to take the zero responses for the three sub-components seriously. It is entirely believable that a household did not eat out in the past week or buy clothes in the past month. Even the zero responses relating to food at home may be credible since the question only asks about *expenditures* in a given week, not actual *consumption*. We could use a structural model of infrequency of purchase to deal with the zeros for eating out and clothing. This model is based on the premise that all households spend something on these items over the course of the year, even though they may not in any particular week or month. This premise is supported by FAMEX

⁷ We used the 1986 FAMEX because it is the last national one: the 1990 and 1992 surveys are restricted to large cities.

⁸ To express this algebraically, $4(XF + XR) + XC > XT$ (the 4 is to convert weekly figures to monthly figures), where:

XF = money spent on food eaten at home

XR = money spent eating out, excluding meals at work or school

XC = money spent on clothing

XT = total household expenses,

data, which indicate that only 1 percent of households report zero annual spending on clothing, and only 3 percent report zero annual spending on eating out.

The Impact of Unemployment on Consumption

Here we examine the impact of the April 1993 legislative changes on a household's consumption in the four categories listed earlier, namely food in the home, eating out, clothing and total consumption. As with other areas of our study, we carry out the analysis for males and females separately.

Before presenting our results, we describe some of the methods and controls we used to achieve those results. Formulae describing some aspects of our study algebraically are presented in Appendix B. We also indicate what cautions are needed in interpreting our results.

Methods, Controls and Cautions

In this study we used only the direct approach described in Appendix A. We controlled for pre-unemployment differences by dividing the consumption measures by our constructed measure of pre-unemployment income. This is not completely satisfactory; it would have been better if we could have used pre-unemployment consumption data. We decided, however, that the only consumption measure we could construct was too inaccurate to be worth using. As it stands, the results using pre-unemployment income must be treated with some caution.⁹

Typically, the ratio of a household's pre-unemployment consumption to its pre-unemployment income is less than 100 percent. Since the exact value depended on each household's saving habits, it was impossible to control for this in any way. Therefore, as shown in the mathematical model in Appendix B, we simply noted that the coefficient on the cohort dummies in our "total expenditure" results underestimated the true effect. Similarly, the ratio of consumption of food and clothing to pre-unemployment income is clearly less than 100 percent, hence these effects have also been underestimated. Given these assumptions, we have scaled both the coefficient on the cohort dummy and its standard error in the same way so that the recorded t-value should be the same as the t-value for the corrected model.

We were concerned that because members of Cohort 1 and Cohort 2 were interviewed at different times of the year, there might be seasonal differences in expenditure between the two cohorts that is independent of any differences in benefit levels. To control for this, we included the sub-sample without benefits in our analysis on the premise that any significant seasonal effects would be revealed by differences in consumption between the no-benefit groups in Cohort 1 and Cohort 2. Since no significant differences were evident, we tentatively concluded that seasonal effects were not an issue.

Table 18
Consumption: Cohort and Benefit Effects

Variable	Sex	Intercept	Cohort 1 No Benefit	Cohort 2 With Benefit	Cohort 2 No Benefit
Total consumption relative to pre-unemployment household income	Female	70.2	-23.6 (8.3)	-3.3 11.3)	-221 (7.4)
	Male	66.1	-27.9 (9.4)	-6.7 (2.5)	-24.3 (8.3)
Log changes in consumption from date of unemployment	Female	-10.7	12 (0.7)	-0.4 (0.3)	2.4 (1.2)
	Male	-11.4	-0.1 (0.1)	-1.0 (0.6)	4.9 (8.3)
Food at home relative to pre-unemployment household income	Female	22.2	-6.3 (5.6)	-20 (20)	-6.1 (5.2)
	Male	23.4	-11.1 (7.4)	-23 (1.7)	-7.5 (5.0)
Log (eating out relative to pre-unemployment household income) (Positive values only)	Female	—	-45.4 (3.5)	17.0 (1.6)	-225 (3.5)
	Male	—	-76.0 (6.1)	2.0 (0.2)	-43.7 (3.4)
Log (clothing relative to pre-unemployment household incomes) (positive values only)	Female	—	45.0 (3.8)	-14.4 (1.3)	-37.4 (3.1)
	Male	—	-45.5 (4.1)	-17.6 (1.7)	-54.6 (4.7)

Note: Reference group: cohort 1 with U/benefits.
All coefficients multiplied by 100; t-values in brackets.

Regression Analysis

We used an infrequency estimator to take account of zero responses with respect to “eating out” and “clothing”. This was a two-stage procedure. By running a probit for positive values, we first predicted the probability of a positive value for each observation, then multiplied the actual expenditure to income ratio by this predicted probability. We then regressed the log of this constructed variable on the cohort/benefit dummies, using positive valued observations only. The latter results are presented in rows five and six of Table 18. We included constants in the regression, but do not report them since they are difficult to interpret.

The results presented in Table 18 tend to be imprecise when it comes to comparing the differences between cohorts for those who receive UI benefits. To increase the precision of our results, we have to make some supplementary assumptions. These assumptions take advantage of the fact that, in Table 18, all of the values for those without benefits are significantly negative.

Assumption one:

Our first assumption is that *a change in the replacement ratio has a linear effect on consumption levels*; that is, a cut in benefits from 60 percent to 57 percent of pre-unemployment income has the same effect as a cut from 57 percent to 54 percent. Using this assumption, we can pool the results for those with benefits and those without. Our sample has three replacement ratios:

- Zero for all respondents without benefits;
- 57 percent for Cohort 2 respondents with benefits: and,
- 60 percent for Cohort 1 respondents with benefits.

The assumption of linearity is a strong one since it implies that the effect of disentanglement (a cut from 60 percent to zero) is twenty times the effect of a cut from 60 percent to 57 percent. Fortunately, we can test for this restriction so that it acts more as a means of reducing variances than as a way to identify any particular characteristic.

Assumption two:

The second assumption we make is that *an individual's cohort membership and UI claim, or lack of it, are based on factors unrelated to consumption decisions*. This is potentially testable, but for now we simply assume it.

Assumption three:

Finally, we assume that *if income does not change after unemployment, then neither does consumption*. This is by no means assured; if people become more pessimistic about the future as a result of becoming unemployed, then they may cut back expenses even if they do not suffer any income loss during the unemployment spell.¹⁰

Results

Table 18 shows the findings of our consumption models. without the assumption of linearity mentioned above, on the impact of unemployment on a household's consumption. These findings are broken down by the sex of the respondent.

To increase the accuracy of our results, we removed some outliers; specifically, we treated the variable for the ratio of expenditure to pre-unemployment household income as missing if it was greater than 6 for total consumption, 2 for food consumed at home, 0.5 for eating out and 1 for clothing. The estimated values for eating out and clothing include the log of pre-unemployment household income to control partially for pre-unemployment differences.

As shown in row one of Table 18. men's total consumption seems to have fallen significantly when their incomes were cut. Women's consumption also appears to have been affected. but to a somewhat lesser degree: for women, the estimated fall is broadly comparable to the fall of 3.4 percent in household income. As mentioned earlier, the true effect of income loss on total consumption was probably even higher than we estimated here for both sexes.

¹⁰ Appendix B shows algebraically the derivation of the linear-assumption model.

According to row two of Table 18, the total consumption of those with benefits does not appear to have been significantly affected by Bill C-113. This is perhaps not too surprising given that this measure used past data that were likely inaccurate. On the other hand, the intercepts for this regression are of considerable interest because they show the mean reported fall in total consumption from the date of unemployment to the date of the interview. This is our only direct evidence of what happens to households in which one member experiences a long (six month) spell of unemployment. The values given suggest that households cut consumption by about 11 percent when at least one member was in a long spell of unemployment.

The values recorded for the consumption of food at home were difficult to interpret since we were comparing current expenditures with past income and not with past expenditures. Nonetheless, we can reasonably conclude that consumption did fall for women and men because the values for "total consumption" and "food at home" are both lower for Cohort 2.

With respect to spending on eating out and on clothing, we cannot easily interpret the actual values since we were using logs; therefore, the results are not as clear as they were for spending on food at home and total expenditures. The t-values are the most important indicators of change. Judging from them, expenditures for eating out seemed to increase slightly, if anything. Clothing expenditures seemed to go down to some extent in Cohort 2 households of men with benefits. These changes are very minor, however, so we conclude that a drop in benefits did not greatly affect expenditures in these two areas.

Table 19 shows the results based on the linear assumption model described above.

The first notable feature of these results is that now all of the effects are significant (see column four). Furthermore, the results rejected none of the restrictions at the 2 percent level from the model with individual cohort and benefit dummies. The implied budget shares for total expenditure were also quite reasonable: pre-unemployment total consumption was estimated to be about 80 percent of household income. On the other hand, the figure for food at home is rather high at about 25 percent, while the figures for eating out and for clothing seem rather low. In future work we shall incorporate information on these quantities from the FAMEX, which should lead to even sharper results.

Even now, however, the results are striking. It seems that a cut of 10 points in the ratio of benefits to pre-unemployment income led to a reduction of from three to six percent in expenditures for all goods. Both sexes reduced their spending, but men generally cut back more than women. This may be because gender was substituting for some other variable. This is quite possible because in our analysis so far, we were mixing dissimilar groups who may have had different reactions. In Table 20 we have broken the sample down into several categories and by sex, to explore differences and similarities in total consumption. Specifically we examined how respondents' consumption was influenced by how regularly they made use of UI, the level of their assets and how important their income was to the household.

It seems that a cut of 10 points in the ratio of benefits to pre-unemployment income led to a reduction of from three to six percent in expenditures for all goods. Both sexes reduced their spending, but men generally cut back more than women.

Table 19
Linear Consumption Effects

Variable	Sex	Sample Size	F (2,#) Test [Prob]	Slope Coefficient (t-value)	Implied Budget Share (%)	Effect of 10% fell in replacement ratio
Total consumption relative to pre- unemployment household income	Female	1,011	0.53 [59.0%]	36.2 (10.7)	63.0	-4.3%
	Male	1,055	2.63 [5.9%]	38.7 (11.0)	79.0	-4.9%
Food at home relative to pre- unemployment income	Female	963	1.44 [24.0%]	8.6 (6.6)	25.0	-3.6%
	Male	993	3.62 [27%]	13.8 (7.8)	28.0	-5.0%
Log (eating out relative to pre- unemployment household income — positive values only)	Female	322	3.12 [4.5%]	78.8 (5.3)	1.7	-5.4%
	Male	391	3.30 [3.8%]	105.2 (7.0)	2.5	-6.5%
Log (clothing relative to pre- unemployment household income — positive values only)	Female	604	0.89 [41.0%]	56.9 (4.1)	3.8	-4.3%
	Male	629	69.6 [5.1%]	69.6 (5.1)	3.9	-5.0%

Regularity of UI Use

We deemed a respondent to be a “regular user” if the administrative data revealed that he or she had received UI benefit income in any three of the five years between 1987 and 1991. As can be seen from Table 20, men who claimed UI regularly reduced their consumption more than those who did not use UI regularly (5.3 percent and 4.7 percent respectively). For women, it was the non-regular users who tended to cut back more. Among the non-regular users, men and women appear to have cut back by approximately the same amount. Overall, the changes for both sexes were very modest: thus it is reasonable to say that there was no significant difference in the impact of benefit cuts on consumption between regular and non-regular users.

Assets

The survey asked for information on the respondents’ household assets to determine how the level of assets affected household consumption when a member became unemployed. Note that “assets” here refer only to liquid assets and do not include equity in an owned home.

From our research, we concluded that regular UI users, especially men, were more likely to have low assets. Both high and low asset households reported very similar drops in consumption when the benefit replacement ratio is lowered by 10 percent.

Table 20
Total Consumption Relative to Pre-Unemployment Income

Group	Sex	Effect of a 10% fall in Replacement Ratio
Regular UI claimants	Female	-3.4%
	Male	-5.3%
Non-regular UI claimants	Female	-4.7%
	Male	4.7%
Low-asset respondents	Female	4.1%
	Male	-5.0%
High-asset respondents	Female	-4.7%
	Male	-4.7%
Low income relative to household	Female	-3.8%
	Male	-2.1%
High income relative to household	Female	-4.7%
	Male	-5.3%

Importance of the Respondent's Income to the Household

In this study, we examined how much each respondent contributed to the household income before becoming unemployed. We considered a person to have had a high relative income if his or her pre-unemployment earnings represented more than half of the household's pre-unemployment earnings. In theory, the higher the relative income, the more important it was likely to be to a household's financial well-being, and the more likely it would have been missed if it were suddenly cut. Table 20 supports this theory to some extent. It indicates that when a man lost his job, household consumption dropped by 5.3 percent if the man had been the major income earner, whereas it only dropped by 2.1 percent if the man had a relatively low income. The differences for women were much less marked.



4. Conclusion

We would have to conduct many more studies to fully understand what happens to income and living standards during a spell of unemployment. However, we have been able to draw several conclusions from the work we have done to date.

Impact on Personal Income

For some unknown reason, the lack of UI benefits has surprisingly little impact on personal income. The personal income of women with no benefits is only about 16 percent lower than the personal incomes of those with benefits. For men, there is even less of a difference — only about 5.5 percent — between the personal incomes of those with benefits and those without benefits.

In both Cohorts 1 and 2, UI benefits made up the largest part of the recipients' income. In the Cohort 1 group with benefits, the net personal incomes amounted to approximately 68 percent of pre-unemployment earning levels for women and 61 percent for men. In the Cohort 2 group with benefits, those percentages dropped somewhat to about 63 percent for women and 59 percent for men.

A 5 percent cut in benefits lowered net personal incomes by 4.5 percent for women and 2.3 percent for men.

Impact on Household Income

For men with benefits in Cohort 2, the cut in household income is slightly *higher* than the cut in personal income when compared to the personal and household incomes of their counterparts in Cohort 1. For women with benefits, the cut in household income is slightly *lower* than the cut in personal income. Overall, however, the impact is very similar for both sexes: a 5 percent cut in benefits leads to approximately a 3 percent drop in the household incomes of both men and women.

For those who do not receive UI benefits, unemployment leads to a drop of between 24 percent and 30 percent in household incomes.

Impact on Other Household Members

Other members of the household do not appear to change their work patterns because of a benefit cut. The work patterns of other household members seem to be the same in both Cohorts 1 and 2.

Impact on Consumption

Consumption falls when benefit levels are cut. The effect varies depending on the household and on the gender of the unemployed person. However, we estimate from Table 19 that if the ratio of UI benefits to insurable earnings falls from 60 percent to 50 percent, consumption drops by between 3 percent and 6 percent for all three categories of goods we examined. The more important a claimant's income is to the household, the greater the fall in consumption.

What would happen if the **UI** benefit ratio dropped to 50 percent?

By applying what we have learned, we can make a number of predictions about what would happen if the replacement ratio of UI benefits to insurable earnings dropped from 60 percent to 50 percent:

- As a matter of mathematics, gross UI benefits would fall by 17 percent.
- Of those who receive UI benefits, such a cut would lead to a 14 percent drop in women's net personal income and a 7 percent drop in men's net personal income. (It is not yet clear how men manage to insulate themselves so effectively from a benefit cut, but it may have something to do with the fact that they generally report higher "other" sources of income and have higher marginal tax rates.)
- The cuts in net personal income would reduce the net household income of both men and women by about 9 percent.
- This reduction in household income would lead to a fall in household consumption of from 3 to 6 percent; the exact impact on consumption would depend on how important the unemployed person's income is to the household.

The bottom line is that a 17 percent cut in benefit levels would probably lead to about a 5 percent reduction in consumption.

The bottom line is that a 17percent cut in benefit levels would probably lead to about a 5 percent reduction in consumption.



Appendix A: Econometric Methods for Analyzing the Data: I

There are three broad approaches we can use to examine material living standards in the household based on the data we obtained:

- **The direct method** uses the responses to each of the four questions independently. It uses regression methods to determine total expenditure and the total spent on food at home, and a combination of probit and regression results to estimate the total spent on clothing and on eating out.
- **The factor analytic method** involves the construction of a measure to predict total expenditures in each of the four areas.
- **The structural method** integrates the other two measures into a structural model.

To elaborate the factor analytic and structural methods, let:

Y_1 = reported expenditure on food at home last week,
 Y_2 = reported expenditure on eating out last week,
 Y_3 = reported expenditure on clothing in the last month,
 x = reported total expenditure in the last month.

The full assumed measurement model is:

$$Y_i = \alpha_i(z) + \lambda_i(z)H + e_i, \quad i = 1, 2, 3$$

$$X = H + e_4$$

Where H is "true" monthly expenditure and z is a vector of household characteristics. For convenience, drop the z 's and work with deviations about means. Thus the measurement model is:

$$Y_1 = \lambda_1 \eta + e_1$$

$$Y_2 = \lambda_2 \eta + e_2$$

$$Y_3 = \lambda_3 \eta + e_3$$

$$x = \eta + e_4$$

The observable are the 10 non-redundant elements of the variance/covariance matrix of (Y_1, Y_2, Y_3, x) . The **unobservable** are the 10 elements of $E(ee')$, the three coefficients $(\lambda_1, \lambda_2, \lambda_3)$ and the variance of η . Thus the system is under-identified. The usual identification assumption is to let the measurement errors (e_1, e_2, e_3, e_4) be uncorrelated. This gives 8 unobservable and two degrees of over-identification. Given this, we can estimate the model and construct a prediction of total expenditure for each household.

The problem with this approach is that the measurement errors will be correlated if respondents give x as a sum of elements, including Y_1 to Y_3 , for example. The assumption of uncorrelated errors can be tested to a limited extent by testing the over-identifying restrictions, but this test has to maintain that at least two of the errors are uncorrelated. An attractive alternative is to take estimates of $(\lambda_1, \lambda_2, \lambda_3)$ from the estimates from the FAMEX. This will be attempted later. Here we adopt the usual orthogonal error assumption.

The structural model approach includes elements of both approaches. The measurement model for this approach is given by:

$$\begin{aligned} Y_1 &= \eta_1 + e_1 \\ Y_2 &= \eta_2 + e_2 \\ Y_3 &= \eta_3 + e_3 \\ x &= \eta + e_4 \end{aligned}$$

where the η_i 's are the true values of the corresponding expenditures. Let d be the cohort 2 dummy. The structural model is:

$$\begin{aligned} \eta_i &= f^i(z, d, \eta) + u_i & i = 1, 2, 3. \\ \eta &= g(z, d) + u_4 \end{aligned}$$

Thus each “true” demand depends on demographics, the true level of total consumption and the cohort dummy. This model is also known as the allocation system model. In the conventional model, changes in benefit should only lead to changes in total expenditure (\sim) and not to changes in the allocation of the total, except for the usual income effects. To illustrate, suppose that clothing is a luxury with an income elasticity of 2. We would thus predict that if a benefit cut leads to a 5 percent cut in total spending, it should lead to a 10 percent cut in spending on clothing. However, if the benefit cut changes the allocation of spending, leading to a cut of, say, 20 percent in clothing expenditures, then the cohort dummy will be significant in the allocation equations (that is, the $f^i(\cdot)$ above)

We can estimate the structural model by known latent variable techniques. We can also reframe the structural approach to give the direct approach. To do so, we substitute the measurement equation for x (the level of total consumption reported in the survey) into the structural model for η (the unobserved true level of consumption):

$$x = g(z, d) + (u_4 + e_4)$$

Under the usual assumptions about the measurement error e_4 , this can be estimated by OLS. The problem is that x is likely an exaggerated indicator of η , thus the variances of e_4 , and hence of $(e_4 + u_4)$ might be quite large. With the simple sizes we have, this might make us more inclined to believe that d has no effect. This is why the factor analytic and structural approaches might be preferable even though they require many more computations.

Another direct approach is to substitute into the “food at home” equation and then estimate that:

$$\begin{aligned} y_1 &= \eta_1 + e_1 = f^1(z, \eta) + u_1 + e_1 \\ &= f^1(z, g(z, d) + u_4) + u_1 + e_1 \\ &= g^1(z, d) + (u_4 + u_1 + e_1) \end{aligned}$$

if we take $f^i(\cdot)$ to be linear in η . This will be better or worse than the first direct approach depending on the relative variances of $(e_4 + u_4)$ and $(u_4 + u_1 + e_1)$.



Appendix B: Econometric Methods for Analyzing the Data II

The results using pre-unemployment income must be treated with some caution. To demonstrate this further, let 0 refer to the date of unemployment and 1 refer to the interview 1 date. Let C_t be total consumption in period t and let Y_t be household income in period t . Ideally, we should examine the ratio of consumption at the time of the interview to pre-unemployment consumption, C_1/C_0 . All we observe, however, is Y_0 , Y_1 and C_1 .

Let us suppose that the true consumption ratio is a linear function of the cohort dummy d

$$C_1/C_0 = \alpha + \beta*d.$$

The unobservable ratio of interest can be written:

$$C_1/C_0 = C_1/Y_0 * Y_0/C_0.$$

We can construct the ratio C_1/Y_0 from the data but Y_0/C_0 is not observed. To overcome this, we make the assumption that this latter ratio is not correlated with the cohort dummy d . This is not necessarily inconsistent with our earlier finding that Y_0 is correlated with the cohort dummy. Denoting Y_0/C_0 by p we have:

$$C_1/Y_0 = (\alpha + \beta*d)p = \alpha + \beta d$$

We can estimate the parameters α and β and given an estimate of p , we can recover an estimate of the parameter of interest β .

We assume that the selection into the two cohorts and into the group with or without benefits is exogenous for the consumption decision. This is potentially testable, but for now we simply assume it. We also assume that if the replacement rate is 100 percent then there is no effect on consumption after unemployment as compared with consumption before. This is by no means assured. The mere fact of becoming unemployed tells people something about their future prospects. If they become more pessimistic about the future as a result of becoming unemployed then they may reduce consumption even if they do not suffer any income loss during the current unemployment spell.

With these assumptions, we can be much more precise about the effects of cuts in unemployment benefits on consumption. To see this, we again use the notation above.

From the linear assumption:

$$C_1/Y_0 = \alpha + \beta R$$

where R is the replacement ratio for UI benefits (that is, 0.60 for cohort 1 people who receive benefits). All of the variables here are observable so that we can estimate α and β . Given these estimates, from the assumption that a replacement ratio of unity would not have any effect we can estimate pre-unemployment consumption relative to pre-unemployment household income as:

$$C_1/Y_0 = \alpha + \beta.$$

This will provide a useful check on the methods used here since these budget shares are interpretable. We can then estimate the proportional impact of any change from:

$$(C_1 - C_0)/C_0 = [(\alpha + \beta R)/(\alpha + \beta)] - 1$$

Note that if R is unity, then this change is zero. In this case, the effect of a reduction of ten points in the benefit rate, (for example, a reduction from 0.60 to 0.50) is to reduce consumption by $10\beta/(\alpha + \beta)\%$.

Since we model the log of the consumption-to-income ratio for eating out and clothing, the formulae here are slightly different:

$$C_1/Y_0 = \exp(\alpha + \beta) \text{ and,} \\ (C_1 - C_0)/C_0 = \exp(-\beta) - 1$$

These formulae are used in Table 18 for these two goods.



List of UI Evaluation Technical Reports

Unemployment Insurance Evaluation

In the spring of 1993, a major evaluation of UI Regular Benefits was initiated. This evaluation consists of a number of separate studies, conducted by academics, departmental evaluators, and outside agencies such as Statistics Canada. Many of these studies are now completed and the department is in the process of preparing a comprehensive evaluation report-

Listed below are the full technical reports. Briefs of the full reports are also available separately. Copies can be obtained from:

Human Resources Development Canada
Enquiries Centre
140 Promenade du Portage
Phase IV, Level O
Hull, Quebec KIA OJ9 Fax: (819) 953-7260

UI Impacts on Employer Behaviour

- **Unemployment Insurance, Temporary Layoffs and Recall Expectations**
M. Corak, Business and Labour Market Analysis Division, Statistics Canada, 1995. (*Evaluation Brief #8*)
- **Firms, Industries, and Cross-Subsidies: Patterns in the Distribution of UI Benefits and Taxes**
M. Corak and W. Pyper, Business and Labour Market Analysis Division, Statistics Canada, 1995. (*Evaluation Brief #16*)
- **Employer Responses to UI Experience Rating: Evidence from Canadian and American Establishments**
G. Betcherman and N. Leckie, Ekes Research Associates, 1995. (*Evaluation Brief #21*)

UI Impacts on Worker Behaviour

- **Qualifying for Unemployment Insurance: An Empirical Analysis of Canada**
D. Green and C. Riddell, Economics Department, University of British Columbia, 1995. (*Evaluation Brief #1*)
- **Unemployment Insurance and Employment Durations: Seasonal and Non-Seasonal Jobs**
D. Green and T. Sargent, Economics Department, University of British Columbia, 1995. (*Evaluation Brief #19*)
- **Employment Patterns and Unemployment Insurance**
L. Christofides and C. McKenna, Economics Department, University of Guelph, 1995. (*Evaluation Brief #7,1*)

•State Dependence and Unemployment Insurance

T. Lemieux and B. MacLeod, Centre de Recherche et Développement en Economique, University de Montreal, 1995. (*Evaluation Brief #4*)

•Unemployment Insurance Regional Extended Benefits and Employment Duration

C. Riddell and D. Green, Economics Department, University of British Columbia, 1995. (*To be released when available*)

•Seasonal Employment and the Repeat Use of Unemployment Insurance

L. Wesa, Insurance Programs Directorate, HRDC, 1995. (*Evaluation Brief #24*)

UI Macroeconomic Stabilization

•The UI System as an Automatic Stabiliser in Canada

P. Dungan and S. Murphy, Policy and Economic Analysis Program, University of Toronto, 1995. (*Evaluation Brief #5*)

•Canada's Unemployment Insurance Program as an Economic Stabiliser

E. Stokes, WEFA Canada, 1995. (*Evaluation Brief #6*)

UI and the Labour Market

• Unemployment Insurance and Labour Market Transitions

S. Jones, Economics Department. McMaster University, 1995. (*Evaluation Brief #22*)

• Unemployment Insurance and Job Search Productivity

P.-Y. Crémieux, P. Fortin, P. Storer and M. Van Audenrode, Département des Sciences économiques, Université du Quebec à Montreal, 1995. (*Evaluation Brief #3*)

• Effects of Benefit Rate Reduction and Changes in Entitlement (Bill C-113) on Unemployment, Job Search Behaviour and New Job Quality

S. Jones, Economics Department, McMaster University, 1995. (*Evaluation Brief #20*)

• Jobs Excluded from the Unemployment Insurance System in Canada: An Empirical Investigation

Z. Lin, Insurance Programs Directorate, HRDC, 1995. (*Evaluation Brief #15*)

• Effects of Bill C-113 on UI Take-up Rates

P. Kuhn, Economics Department. McMaster University, 1995. (*Evaluation Brief #17*)

• Implications of Extending Unemployment Insurance Coverage to Self-Employment and Short Hours Work Week: A Micro-Simulation Approach

L. Osberg, S. Phipps and S. Erksoy, Economics Department, Dalhousie University, 1995. (*Evaluation Brief #25*)

•The Impact of Unemployment Insurance on Wages, Search Intensity and the Probability of Re-employment

P.-Y. Crémieux, P. Fortin, P. Storer and M. Van Audenrode, Département des Sciences économiques, Université du Québec à Montréal, 1995. (*Evacuation Brief #27*)

UI and Social Assistance

•The Interaction of Unemployment Insurance and Social Assistance

G. Barrett, D. Doiron, D. Green and C. Riddell, Economics Department, University of British Columbia, 1995. (*Evaluation Brief #18*)

•Job Separations and the Passage to Unemployment and Welfare Benefits

G. Wong, Insurance Programs Directorate, HRDC, 1995. (*Evaluation Brief #9*)

•InterProvincial Labour Mobility in Canada: The Role of Unemployment Insurance, Social Assistance and Training

Z. Lin, Insurance Programs Directorate, HRDC, 1995. (*Evaluation Brief #26*)

UI, Income Distribution and Living Standards

•The Distributional Implications of Unemployment Insurance: A Micro-Simulation Analysis

S. Erksoy, L. Osberg and S. Phipps, Economics Department, Dalhousie University, 1995. (*Evaluation Brief #2*)

•Income and Living Standards During Unemployment

M. Browning, Economics Department, McMaster University, 1995. (*Evaluation Brief #14*)

•Income Distributional Implications of Unemployment Insurance and Social Assistance in the 1990s: A Micro-Simulation Approach

L. Osberg and S. Phipps, Economics Department, Dalhousie University, 1995. (*Evaluation Brief #28*)

•Studies of the Interaction of UI and Welfare using the COEP Dataset

M. Browning, P. Kuhn and S. Jones, Economics Department, McMaster University, 1995.

Final Report

•Evaluation of Canada's Unemployment Insurance System: Final Report

G. Wong, Insurance Programs Directorate, HRDC, 1995.