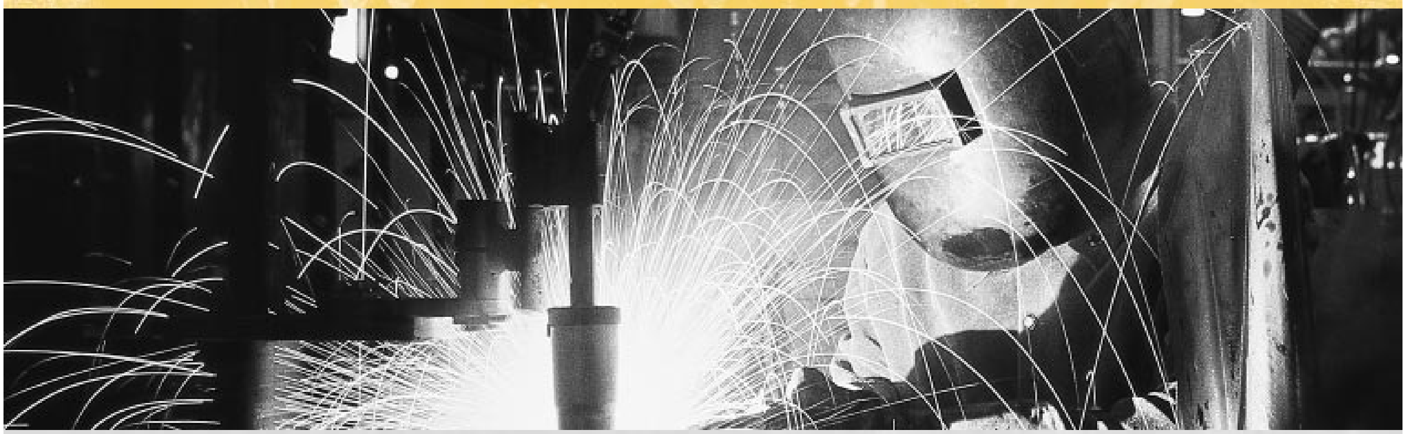
UI

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

by Peter Kuhn



Human Resources Development Canada
Développement des ressources humaines Canada

UI and the Labour Market

Canada

August 1995

Publication également disponible en français.

IN-AH-215E-08-95

UI

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

by Peter Kuhn
McMaster University

UI and the Labour Market

Acknowledgements

This is the sixteenth in a series of papers being sponsored by Human Resources Development Canada (HRDC). The excellent research assistance of Tom Crossley and Arthur Sweetman are acknowledged with thanks. The views expressed in this study are the personal views of the author and not necessarily those of Human Resources Development Canada.

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.

I.H. Midgley
Director General
Evaluation Branch

Ging Wong
Director
Insurance Programs

Table of Contents

List of Figures

List of Tables

Abstract

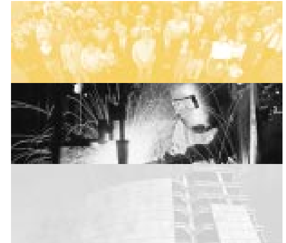
Effective the beginning of April 1993, Bill C-113 made two changes to Canada's regular unemployment insurance system. For most individuals, benefits were cut from 60 to 57 percent of insurable earnings. However, individuals who either voluntarily quit their jobs without just cause or were dismissed from their jobs had their benefits cut to zero. This study of Bill C-113's effects attempts to answer the following questions:

1. What were its effects on the UI take-up rates of job quitters? Did it drastically reduce UI take-up rates and thus disentitle a lot of quitters from benefits, or (because of the "just cause" exemption) did it have only a small effect on the take-up?

2. By what mechanisms did Bill C-113 reduce the claim rates of job quitters? Did fewer quitters apply for UI? Were more of the applications by quitters rejected by HRDC and/or did the disentitled workers become re-employed more rapidly than before?

In this paper we develop an analytical framework for decomposing the decline in UI take-up into a set of components, each associated with a different mechanism of adjustment to the disentitlement. These mechanisms are a "discouragement effect" (fewer quitters applying for UI), a "rejection effect" (more UI applications by quitters rejected by HRDC), and an "incentive effect" (disentitled individuals induced to become re-employed more quickly as a result of the benefit cut).

Our findings indicate that by far the most important factor contributing to the decline in UI take-up by job quitters was a "discouragement effect," accounting for about 78 percent of the total drop in UI take-up. This drop in take-up was mainly observed among workers who had no re-employment success following their separation: the proportion of this group applying for UI fell from 73 to 49 percent. Further evidence of a discouragement effect is that the proportion of non-applying quitters who said they did not apply because they "believed they were ineligible for benefits" rose from 40 to 54 percent. A much smaller portion of the decrease in take-up was due to a "rejection effect". We find no evidence of any positive incentive effect of the UI benefit cut on the re-employment rates of workers affected.



Introduction

Effective at the beginning of April 1993, Bill C-113 made two changes to Canada's regular unemployment insurance system. For most individuals, benefits were cut from 60 to 57 percent of insurable earnings. However, individuals who, according to HRDC, either voluntarily quit (labelled "VQ's") their jobs or were dismissed had their benefits cut to zero. Since, overall, these policy changes made UI receipt less remunerative, one might expect them to have had some effect on the number of individuals who start UI spells.

…overall, these policy changes made UI receipt less remunerative, one might expect them to have had some effect on the number of individuals who start UI spells.

The goal of this report is to ascertain whether, indeed, Bill C-113 had a significant effect on the UI take-up rate in Canada, and if so, how that effect occurred. For example, one way in which Bill C-113 could reduce the number of people starting UI claims is by reducing the fraction of voluntary quitters who claim UI. We call this the "VQ disentitlement effect," and note that it can have a number of components. In particular, the fraction of VQ's claiming UI can fall either because fewer VQ's apply for UI (the "discouragement effect"), because more of those who apply for benefits are rejected (the "rejection effect"), or because fewer workers remain unemployed long enough to claim UI (the "incentive effect"). Indeed, it is possible for some of these effects to cause UI claims to increase: for example, if local UI officers are unwilling to impose total disentitlements, the rejection effect of Bill C-113 on UI take-up could lead to a potential rise in UI take-up.

The VQ disentitlement effect and its components are not the only possible effects of a measure like Bill C-113. For example, the Bill might also affect the fraction of non-VQ's claiming UI, since this group's benefits were also cut; this is the "non-VQ disentitlement effect." As well, a UI cut, either from 60 to 57 percent for laid-off workers or especially from 60 percent to zero for quitters, might also reduce the number of people separating from their jobs. We call this the "inhibition effect." Or, rather than affecting the number of job separations, a UI cut such as Bill C-113's, which falls much more heavily on quitters than other separations, could lead simply to a change in the composition of separations, i.e., to the label assigned a particular separation by workers and firms, in order to avoid disentitlement. We call this the "relabelling effect." Figure 1, which provides the basic framework for the analysis in this report, shows that the total effect of any policy change like Bill C-113 on UI take-up depends on a combination of the inhibition, relabelling, discouragement, rejection, and incentive effects. Understanding its effects thus requires an understanding of all these component effects as well.

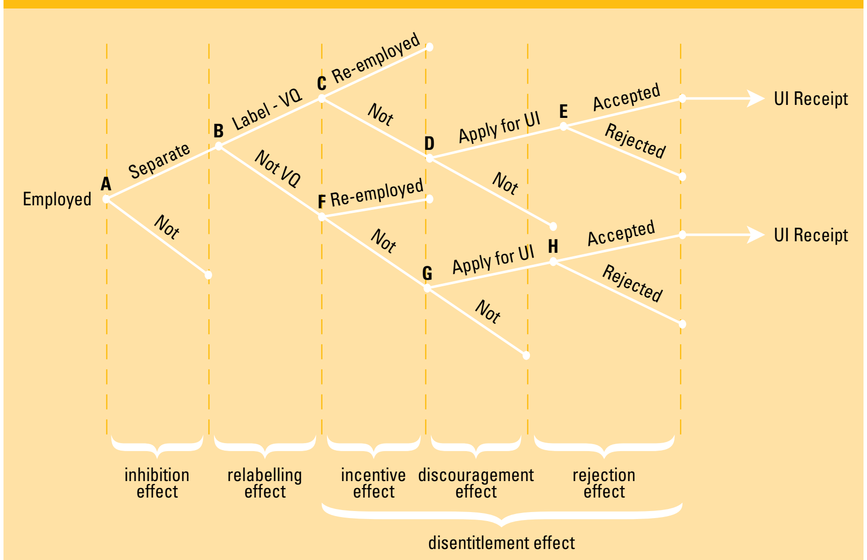
There are at least two reasons for our interest in the effects of Bill C-113 on UI take-up. First, we wish to know to what extent the changes really made it harder for separating workers to get UI, and how much, if at all, this reduced the expenditures of the UI system[1]. Second, isolating the mechanism by which any such changes occurred can tell us a lot about the functioning of the UI system and how it is likely to respond to future policy changes. For example, if workers' (and

1 Another very important aspect of these changes, namely, the amount of hardship these changes imposed on workers, is dealt with in detail in a companion paper by Martin Browning: *Income and Living Standards During Unemployment.*

firms') responses to benefit cuts for a specific separation reason consist largely of relabelling separations, the cuts will have much smaller effects on workers and on the costs of the UI program than if these responses genuinely disentitle workers. Or, one might feel differently about UI cuts if workers respond to such cuts by becoming re-employed more quickly rather than just being discouraged from applying for benefits.

In this report, we shall focus on the magnitude of Bill C-113's effects on take-up rates and program expenditures and the precise mechanisms through which this occurred. We shall use aggregate time-series data and micro-panel data. Because the panel data at our disposal covers only workers separating from employment in two specific months (February and May 1993), our analysis of the medium- and long-run effects of Bill C-113 on UI take-up in section II is based mainly on two sets of time series – one based on HRDC's administrative Record of Employment (ROE) data and the other based on Statistics Canada's Labour Force Survey (LFS). In addressing this question, it is both necessary and useful to also model the effects of a similar policy change that was implemented in November 1990: the extension, in Bill C-21, of the benefit waiting period for voluntary quits from 3-6 weeks to 7-12 weeks. However, because neither of our time-series data sets contain very detailed information on the precise mechanics of UI disentitlement, in section III we use the very detailed microdata in HRDC's "Canadian Out of Employment Panel" (COEP) data set to explore in greater detail the mechanisms through which Bill C-113 affected take-up rates and program expenditures.

Figure 1
The Path from Employment to UI and the Possible Effects of Bill C-113 on UI Take-up



1. Effects on UI Take-up of Penalizing Quitters

This section estimates the effects of two recent policy changes, in November 1990 (Bill C-21) and April 1993 (Bill C-113), on the UI benefit take-up rate, and provides some detail regarding the mechanism by which any take-up changes occurred. We use time series data from two sources: HRDC and Labour Canada's administrative Record of Employment (ROE) data and some special tabulations supplied to us from Statistics Canada's monthly Labour Force Survey (LFS).[2]

The main advantages of the ROE data, relative to the LFS, are that they contain information on UI take-up rates and they categorize individuals according to the reasons for separation that are used in administering the UI system. The main advantages of the LFS are that it provides complete data over a longer time period, especially after the institution of Bill C-113; it allows us to measure separations into unemployment, which are of more direct interest here than total separations; and it allows us to ascertain whether the Bill affected the actual rate of job separation or just the labelling of separations (because it provides data on total employment as well.)

Record-of-Employment Data

The data used in this section consist of a monthly time series of the number of ROEs issued by HRDC and Labour Canada. An ROE should be issued every time a worker separates from an employer in Canada, and it is a prerequisite for receiving UI benefits. The time series begins in January 1985 and ends in December 1993. However, since the ROE file typically takes about six months to become complete, we end our analysis in June 1993.[3]

In addition to counting the total number of ROEs issued in each month, the data set used here disaggregates them by the reason for separation reported on the ROE form. While the list of possible reasons is quite detailed, in this section we only look at two exhaustive categories: "voluntary quits" (VQ's) and "all others."[4] The data also indicate the fraction of ROEs issued in each month that gave rise to a UI claim within the next four months. This is the basic measure of UI take-up we use here.

2 The authors would like to thank Garnett Picot and Debbie Tobalt of Statistics Canada for producing these series and generously making them available to us.

3 A comparison of the December 1993 ROE file (used here) with the September and May files of the same year reveals that after six months all the variables used here are at least 95 percent complete. Most of the analysis in this section was also performed on a data set that adjusts for incomplete reporting using completion rates estimated from these previous updates. The results were very similar.

4 Since dismissed workers were also penalized by Bills C-21 and C-113, it might be of interest to consider this category of separations separately. We do not do so here for two reasons. First, dismissals were not listed as a distinct reason for separation until 1990. Assuming dismissals were categorized as "other" before 1990, the only consistent time series of the composition of separations throughout our sample period is one that aggregates dismissals and "other" separations together. Second, dismissals are a much smaller fraction of separations than quits. If there were any major effects of legislation on take-up and labelling of separations, it will be more visible in the data on quits than dismissals.

…the ROE data allow us to decompose the effects of Bills C-21 and C-113 on UI take-up into three components: the total VQ disentitlement effect; the relabelling effect; and the non-VQ disentitlement effect…

Analytical Framework

In terms of Figure 1, the ROE time series data give us information on two aspects of the path from employment to UI receipt. The first, conditional on separating from a job, is the label that is assigned to that separation (node B). The second, conditional on the label, is whether UI receipt occurs; it does not include any information on the intervening steps between labelling and UI receipt (re-employment, application, and acceptance/rejection; i.e., nodes C, D, and E collapsed together, and nodes F, G, and H collapsed together). Thus we can only estimate relabelling effects and total disentitlement effects from these data; the precise manner by which we do so is outlined below.

In any given period, the fraction of separating workers who start a UI claim at any time within four months of separation (the claim rate, or CR) can be expressed as:

$$CR = VQ(CRV) + (1 - VQ)(CRO), \tag{1}$$

where *CRV* is the fraction of voluntary quitters who claim within four months, *CRO* is the fraction of other separations who claim within four months, and *VQ* is the share of separations labelled as voluntary quits.

Taking the total differential of (1) yields:

$$dCR = VQ\ dCRV + (1 - VQ)\ dCRO + (CRV - CRO)dVQ, \tag{2}$$

which expresses any small change in CR, dCR, as the sum of three components. These are, in turn, the effect due to a change in the claim rate of VQ's (*VQ dCRV*), the effect due to a change in the claim rate of other separations (*(1–VQ)dCRO*), and the effect due to a change in the composition of separations (*(CRV–CRO)dVQ*).

In principle, then, the ROE data thus allow us to decompose the effects of Bills C-21 and C-113 on UI take-up into three components: the total VQ disentitlement effect, representing the reduction in take-up among the disentitled group; the relabelling effect, due to a potential relabelling of quits as separations in response to the policy change; and the non-VQ disentitlement effect, reflecting any induced changes in the claim rate of non-VQ separations. It is worth noting that, in addition to reflecting actual disentitlement of non-VQ's, the last effect would also occur if the claim rate for the VQ's who are labelled as non-VQ's is different from that of those previously labelled as non-VQ's. Disaggregating these total disentitlement effects into the components due to re-employment, applications, and denial of applications is only possible with the COEP survey data. Identifying the inhibition effects is only possible with the LFS data.

For finite changes in claim rates such as we encounter in the data here, the decomposition in (2) can be expressed (using first-period weights on the disentitlement and spillover effects) as:

$$\begin{aligned} CR_2 - CR_1 = {} & VQ_1(CRV_2 - CRV_1) + (1 - VQ_1)(CRO_2 - CRO_1) + \\ & (CRV_2 - CRO_2)(VQ_2 - VQ_1), \end{aligned} \tag{3}$$

or, using second–period weights on these effects, as:

$$\boldsymbol{CR_2 - CR_1 = VQ_2(CRV_2 - CRV_1) + (1-VQ_2)(CRO_2 - CRO_1)} \\ \boldsymbol{(CRV_1 - CRO_1)\ (VQ_2 - VQ_1).} \tag{4}$$

In what follows we shall report results for both decompositions (3) and (4).

In contrast with Anderson and Meyer (1994), who estimate a structural econometric model derived from economic theory, we adopt a more agnostic, reduced-form approach, taking advantage of the quasi-experimental nature of the passage of Bill C-113. There are two main reasons for this. First, in our view, the main benefit of a structural approach is that it allows one to make predictions about the effects of policy changes that are different from the ones that actually occur in the data. Since the main purpose of the current report is to analyze the effects of Bill C-113 itself, this benefit does not apply here. Clearly the best "natural experiment" from which to estimate the effects of Bill C-113, which is a fairly multidimensional policy change, is Bill C-113 itself. Second, the main cost of using a tightly specified structural model is that its predictions are correct only if the assumptions embodied in that model, including the functional form and distribution assumptions, are correct. Since, unlike the benefit, this cost remains with us here, we choose not to use such a model. Instead, we simply note that, to the extent that the imposition of Bill C-113 was orthogonal to other factors affecting UI take-up, an unbiased estimate of its effect is available simply from a comparison of UI take-up rates before and after its imposition. To account for any factors that might not be orthogonal to Bill C-113, yet might affect UI take-up, we simply control for these factors in a regression context and interpret the remaining effect of a dummy variable for Bill C-113 as the true effect of that Bill.[5]

Time Series Regression Analysis

In order to isolate the effects of UI policy changes associated with Bills C-21 and C-113 on the four series in the above decompositions (*CR*, *CRS*, *CRV*, and *VQ*), we consider time series regressions of each series on seasonal dummies, measures of aggregate business conditions, and dummy variables for the different policy regimes affecting UI eligibility. These regimes are the period before Bill C-21, during which the waiting period for UI benefits of voluntary quitters was three to six weeks; the Bill C-21 period, during which the waiting period was 7-12 weeks; and the last three months of our sample, the Bill C-113 period, during which voluntary quitters were ineligible for UI.

The main results of these regressions are reported in Table 1. They clearly indicate, first, that the probability that a worker who separates from a job will start a UI claim is strongly increasing in the unemployment rate. This is true of all separations as a group, as well as of VQ and non-VQ separations separately. Almost surely, this is because workers are re-employed more slowly, and are therefore

5 An alternative to using a simple policy dummy would be to allow for changes in the non-policy parameters before and after the policy change, thus estimating completely separate equations before and after. Given the small number of observations – especially in the time series analysis here – this is not practical. Since we can think of no strong reason for the effects of, for example, seasonal factors on the composition of separations to differ before and after Bill C-113, we do not view this as a serious drawback of the current data.

Table 1
Time Series Regression Coefficients for UI Claim Rates and Labelling of Separations

	Dependent Variable			
	CR (Overall UI claim rate)	*CRO* (non-VQ claim rate)	*CRV* (VQ claim rate)	*VQ* (VQ share in separations)
Independent Variable				
Unemployment Rate	0.013* (0.002)	0.006* (0.002)	0.012* (0.001)	-0.022* (0.002)
Period 2 (November 90 – March 93)	0.010 (0.006)	0.005 (0.006)	–0.029* (0.004)	–0.047* (0.005)
Period 3 (April – June 93)	0.016 (0.013)	0.023 (0.012)	–0.086* (0.009)	–0.060* (0.011)
Number of observations	102	102	102	102
Adjusted R^2	0.920	0.650	0.940	0.930

Notes: All regressions contain a complete set of month dummies plus a constant term. Standard errors are in parentheses.
** Significantly different from zero at 99 percent.*

more likely to need UI, in recessionary times. Second, the share of job separations that are quits (VQ's) drops substantially when the unemployment rate rises. This could be either because workers are less likely to quit into unemployment or because fewer employed workers are receiving job offers from other employers. Third, the Bill C-21 and Bill C-113 policy regimes have a positive but insignificant effect on both the overall UI claim rate and the UI claim rate of non-VQ separations. The sources of this effect are unclear, but it is important to remember that they are net of any cyclical factors as measured by the unemployment rate. Fourth, both Bills C-21 and C-113 appear to have had the expected negative effect on the claim rate among VQ's: the former appears to have lowered the VQ claim rate by about 3 percentage points and the latter by a further amount of almost 6 percentage points. Finally, it also appears – although it is not apparent from the preceding figures – that both policy changes had a significant effect on the labelling of separations: during the C-21 period the fraction of separations labelled as VQ's was 5 percentage points below what one would have expected in the absence of a policy change, given the levels of unemployment prevailing at that time. Bill C-113 appears to have caused a further reduction of about 1 percentage point. Thus it appears that individuals have been able to mitigate the effects of both Bills C-21 and Bill C-113 by relabelling their separations as something other than quits. This effect appears to have been fairly substantial at the time Bill C-21 was introduced, but rather minimal at the introduction of Bill C- 113.

Decomposition of the Total Effect of Bills C-21 and C-113 on UI Claim Rates

Despite the fact that both Bills C-21 and C-113 reduced UI claim rates among individuals who quit their jobs, they appear to have had no detectable effect on the overall rate at which people separating from jobs claim UI. In fact, both Bills appear to be associated with an insignificant increase in the overall UI claim rate.

How are these findings to be reconciled? Some insights into this question are provided by applying the decompositions in equations (3) and (4) to the overall change in UI take-up to quantify the effects of the various components (VQ disentitlement, non-VQ disentitlement, and relabelling) on the overall change in UI take-up.

Table 2 reports these decompositions for the effects of Bill C-21, using cyclically and seasonally adjusted data on *CR*, *CRS*, *CRV* and *VQ*.[6] In the top panel we present means of these series for the entire periods January 1985-October 1990 (before the passage of the Bill) and November 1990-March 1993 (after its passage). All series are adjusted to correspond to a mean level of unemployment for the entire sample period and to be representative of an entire year rather than any single month. As noted earlier, the policy change appears to be associated with a small increase in the total UI claim rate of 0.8 percentage points. According to Table 2, this small rise was the net result of three factors. First, the UI claim rate of VQ's fell by 1.2 percentage points (the VQ disentitlement effect). Had none of the other variables changed, this would have reduced the UI claim rate by 0.3 percentage points. Second, the share of separations labelled as VQ's fell by 2.2 percentage points. On its own, this would raise the overall claim rate by about half a percentage point (0.5 or 0.6 percentage points, depending on the calculation method), since non-VQ's are more likely to claim UI than VQ's, due to the relabelling effect. This relabelling effect more than offsets the disentitlement effect, leading to a small net rise in take-up. The remainder of the rise in total take-up is explained by a rise of 0.6 percentage points in the non-VQ

Table 2
Estimated Effects of Bill C-21 on UI Take-up Rates: Cyclically/Seasonally Adjusted Time Series

Variable	Before	After	Net Change
Total Claim Rate (*CR*)	0.382	0.390	+0.008
VQ Claim Rate (*CRV*)	0.197	0.185	–0.012
Non-VQ Claim Rate (*CRO*)	0.442	0.449	+0.006
Share of VQ's in Total Separations (*VQ*)	0.246	0.224	–0.022

Contribution of each variable to net change in total *CR*

	Method 1 (1st period weights on change in *CRV*)	Method 2 (2nd period weights on change in *CRV*)
Portion of Change Due To:		
VQ Disentitlement (change in *CRV*)	–0.003	–0.003
Non-VQ Disentitlement (change in *CRO*)	+0.005	+0.006
Relabelling (change in *VQ*)	+0.006	+0.005
Total	+0.008	+0.008

6 This adjustment was carried out first by regressing each time trend on a set of month dummies and the Canadian unemployment rate, then using the coefficients from these regressions to remove the cyclical and seasonal components from the series. Experimentation with different functional forms and lag structures for the cyclical variable – the unemployment rate – yielded broadly similar results.

take-up rate, which does not appear to be explained by either seasonal or macroeconomic effects.

However, even if we exclude this rise in the non-VQ take-up rate, it appears that the penalties imposed by Bill C-21 on quitters had no discernible net effect on UI take-up – according to these estimates, the induced relabelling effect more than offset the direct disentitlement effect of the policy change.

In Table 3 we present the same exercise for the effects of Bill C-113. The top panel consists of means of the cyclically and seasonally adjusted series for the periods November 1990-March 1993 (before the passage of the Bill) and April-June 1993 (after its passage). As was the case with Bill C-21, the policy change appears to be associated with a small increase in the total UI claim rate, this time of 0.3 percentage points. According to Table 3, this small rise was the net result of three factors. First, the UI claim rate of VQ's fell by 3.2 percentage points. Had none of the other variables changed, this disentitlement effect would have reduced the UI claim rate by 0.7 percentage points. Second, the share of separations labelled as VQ's this time fell very slightly, by only 0.2 percentage points.

Table 3
Estimated Effects of Bill C-113 on UI Take-up Rates: Cyclically/Seasonally Adjusted Time Series

Variable	Before	After	Net Change
Total Claim Rate (*CR*)	0.390	0.393	+0.003
VQ Claim Rate (*CRV*)	0.185	0.153	–0.032
Non-VQ Claim Rate (*CRS*)	0.449	0.461	+0.012
Share of VQ's in Total Separations	0.224	0.222	–0.002

Contribution of each variable to net change in total *CR*

	Method 1 (1st period weights on change in *CRV*)	Method 2 (2nd period weights on change in *CRV*)
Portion of Change Due To:		
VQ Disentitlement (change in *CRV*)	–0.007	–0.007
Non-VQ Disentitlement (change in *CRO*)	+0.009	+0.009
Relabelling (change in *VQ*)	+0.001	+0.001
Total	+0.003	+0.003

On its own, this relabelling effect would raise the overall claim rate by one-tenth of a percentage point. The remainder of the small rise in total take-up is again explained by a rise of 1.2 percentage points in the non-VQ take-up rate, which does not appear to be explained by either seasonal or macroeconomic effects. Thus, as in 1990, an unexplained rise in the UI take-up rate among non-VQ's plays a large role in the changes in take-up associated with a legislative change primarily affecting VQ's. If we net out this effect and consider the VQ disentitlement and relabelling effects only, then our estimates indicate that Bill C-113 reduced the total UI take-up rate by at most six-tenths of 1 percent.

Intuitively, the reasons for such a small effect are twofold. First, the apparent effect of Bill C-113 on UI take-up by VQ's is not large, perhaps because a considerable fraction of voluntary quits, even under the old rules, were for reasons HRDC considered justified. Second, even if the decline were larger, it could not have had a large effect on the overall UI claim rate because (a) the VQ claim rate before Bill C-113 was already quite low, and (b) VQ's are a fairly small share of total separations.

Finally, to check on the robustness of these estimates to the statistical techniques used, we take advantage of a fortuitous feature of the timing of Bill C-113 to present some very simple nonparametric estimates of its effects. To do so, we simply perform the above decomposition using mean levels of each of the above series before and after the change, eliminating seasonal and cyclical effects using a judicious choice of sample periods. To eliminate seasonal effects, we use data for the months of April-June only, since these are the only months for which relatively complete information about the situation after the passage of Bill C-113 is available. Second, the only year we use before the change is 1992, because macroeconomic conditions in the second quarters of 1992 and 1993, unlike 1991, were actually very similar (with unemployment rates of 11.2 and 11.3 percent, respectively).[7] While this restricts the sample to only three months before and after the passage of Bill C-113, it is worth recalling that the samples on which these numbers are measured in each month are very large (one-tenth of the entire population of separations in Canada), so even then the numbers will be fairly precise.

The results of this comparison are presented in Table 4. Overall, the trends shown in Table 4 are somewhat larger in magnitude, but have exactly the same pattern as the parametric estimates shown in Table 3. According to Table 4, the total UI

Table 4
Estimated Effects of Bill C-113 on UI Take-up Rates
(Results from Nonparametric Comparison of April-June 1992 versus April-June 1993)

Variable	Before	After	Net Change
Total Claim Rate (*CR*)	0.390	0.403	+0.013
VQ Claim Rate (*CRV*)	0.190	0.135	–0.055
Non-VQ Claim Rate (*CRO*)	0.439	0.465	+0.026
Share of VQ's in Separations (*VQ*)	0.199	0.188	–0.012

Contribution of each variable to net change in total *CR*

	Method 1 (1st period weights on change in *CRV*)	Method 2 (2nd period weights on change in *CRV*)
Portion of Change Due To:		
VQ Disentitlement (change in *CRV*)	–0.011	–0.011
Non–VQ Disentitlement (change in *CRO*)	+0.020	+0.021
Relabelling (change in *VQ*)	+0.004	+0.003
Total	+0.013	+0.013

7 Unfortunately, such a neat comparison is not possible for the periods before and after Bill C-21, so there is no alternative to regression techniques to eliminate the cyclical component in the variables.

…our analysis of ROE time series data indicates quite conclusively that the benefit cuts in Bill C-113 had at most a very small effect on overall UI take-up rates…

claim rate rose by 1.3 percentage points, from 39.0 to 40.3 percent. As in Table 3, this was the net result of a decrease (of 5.5 percentage points) in the VQ claim rate, an increase (of 2.6 percentage points) in the non-VQ claim rate, and a decrease in the share of VQ's in separations of 1.2 percentage points. Considering the effects of the VQ disentitlement and relabelling only (and thus ignoring the unexplained rise in the non-VQ claim rate) the most by which Bill C-113 could have reduced the overall UI claim rate is now calculated to be eight-tenths of 1 percentage point.

Overall, our analysis of ROE time series data indicates quite conclusively (but perhaps not surprisingly) that the benefit cuts in Bill C-113 had at most a very small effect on overall UI take-up rates – in the order of less than 1 percentage point. The main reasons for this small effect are, first, the measured decrease in UI claims by the group most directly affected by the Bill – VQ's – was actually fairly small, perhaps because many of those workers, even under the old rules, would be considered "justified" quits anyway. Second, even if the decline in take-up among VQ's had been bigger, it still could not have had a major effect on overall UI take-up because the initial VQ take-up rate was already very small, and VQ's are a fairly small share of total separations. Third, part of the direct disentitlement effect of the legislation was counterbalanced by a small relabelling of VQ separations as non-VQ separations, which would tend to raise claim rates, because non-VQ's have higher UI claim rates than do VQ's.

The analysis of ROE data in this section could be made more precise in several ways. First, despite the well-known problems with province identification in the ROE data (due to the large amount of missing information and to the "head-office problem" – ROEs are sometimes identified with a company's head office location rather than where the individual actually worked),[8] it may be worth using this information to improve our controls for cyclical effects here. Specifically, better estimates of cyclical effects on all the endogenous variables considered here could probably be obtained by using the asynchronous component of the within-province variation in unemployment rates to identify these effects. Second, some more demographic information, such as sex and age, could probably be incorporated by merging the ROE file with information from the SIN file. Third, more advanced time-series regression techniques would sharpen up the current estimates. Finally, it is crucial that the current estimates be updated with more recent versions of the ROE file as soon as they become available, since information about the situation after the passage of Bill C-113 is still very skimpy on that file.

Labour Force Survey Data

One limitation of the ROE time series data is that it is, by nature, limited to the population of separations. Thus, while giving us information on the take-up rate of UI benefits by separating workers as well as the composition of the pool of separations, it cannot identify another possible effect of Bill C-113: its effect on

8 Note that, in the analysis of UI take-up issues, this problem cannot be circumvented by using geographic information from the Status Vector file, because it is available for claimants only.

the separation rate itself. In particular, concerns have been expressed that, by penalizing quits, Bill C-113 may have forced people to stay in jobs that they otherwise would have, perhaps justifiably, quit. Since this kind of reduction in separations would appear in the ROE data as relabelling, we need a data set with information on the overall separation rate as a function of employment, to ascertain whether what appears to be relabelling in the ROE time series really is relabelling. In order to address this issue, as well as to corroborate our conclusions in the previous section with evidence from a completely different data source, we consider data from the monthly Labour Force Survey (LFS).

In contrast with the ROE data, the LFS is a monthly sample of the Canadian population rather than a sample of job separations.

The LFS data used here consists of monthly counts of individuals, in various labour force states, from January 1980 to December 1993. In contrast with the ROE data, the LFS is a monthly sample of the Canadian population rather than a sample of job separations. A close approximation to the number of separations into unemployment, however, can be constructed from the LFS data by counting the number of currently unemployed individuals who last worked less than four weeks ago.[9] Since the LFS asks currently unemployed workers why they separated from their last job, these separations can be classified by reason. It is also worth noting that the LFS reasons for separation are reported by workers, not employers, and unlike the ROE they are not used in the administration of the UI system in any way. To that extent they may be less contaminated by the financial incentives related to qualifying for UI. Finally, unlike the ROE, the LFS provides monthly information on the stock of employed workers.

Using the number of employed workers in the previous month as a base allows us to calculate the fraction of employed workers separating from their jobs into unemployment. This allows us to identify any inhibition effects associated with Bill C-21 and Bill C-113, which is not possible with any of the other data sets used here.

Several groupings of the LFS “reason for separation” categories were attempted. The most useful, for the purposes of this study, is to label as voluntary quitters those who cited personal/family responsibilities, no specific reason, or dissatisfied. This voluntary quitter group is the focus of the remainder of the section. Further disaggregation is made, however, on the basis of age, because of the considerable and well-known difference between the separation behaviour of young and old workers. The sample is divided into two groups according to age – 15-24 year-olds and 25-54 year-olds. Unemployment, separation, and quit rates are calculated for each group separately. The older group is much larger than the younger; their average employment over the period was 8,051,733 and 2,321,516 people, respectively. The mean numbers of separations into unemployment per month are, however, much more similar. For the 25-54 and 15-24 age groups, respectively, the mean monthly voluntary quits are 24,386 and 21,292 and other separations are 168,961 and 129,470. As a result, the fractions of quits and sepa-

9 We experimented with different cutoff levels, including eight and 16 weeks. Since not many extra observations on reasons for separation were gained with these longer windows, and since four weeks comes closer to the measure of inflow into unemployment we want to measure, four weeks was chosen.

rations are much higher for the younger age group. The smaller sample size of the younger age group is partly responsible for its higher variance, as will be seen in the regressions below.

Analytical Framework

In terms of Figure 1, the LFS provides information on what happens at nodes *A* and *B*, with no information on UI take-up at all. Its main advantage, relative to the ROE data, is that it provides independent evidence on the changes in composition of separations as reported by workers (node *B*) and (because it provides data on employment rates) the only evidence available on the separation rate itself (node *A*). In particular, LFS data tell us whether the changes in the composition of separations found both in the LFS and in the ROE are primarily relabelling or inhibition effects.

The fraction of voluntary quits (*Frac VQ*) is equal to the number of voluntary quits divided by the number of separations, or alternatively, the quit rate (*Qrate*) divided by the separation rate (*Srate*) as in (1):

$$\textbf{\textit{Frac VQ = VQ/SEPS = (VQ/EMPL) / (SEPS/EMPL) = Qrate / Srate.}} \quad (1)$$

Taking logs:

$$\textbf{\textit{ln(Frac VQ) = ln(Qrate) - ln(Srate),}} \quad (2)$$

the quit and separation rates (*Qrate and Srate*) are the monthly fractions of individuals who make transitions into unemployment as measured by the LFS. Voluntary quits are defined above, and separations is the total for all reasons. Derivatives, with respect to the policy change, can be taken from this identity; they are estimated by the regression coefficients presented below.[10] To distinguish between inhibition and relabelling, the concomitant change in the *Srate* can be used. If the reduction in the *Qrate* is entirely attributable to relabelling, then the *Srate*, which is the sum of all types of separations, will remain constant. At the other extreme, if there is no relabelling, then the *Srate* will fall by the amount that the *Qrate* fell, multiplied by the fraction of separations that are voluntary quits (VQ's). The fraction of separations which are VQ's will fall in both cases. While it will fall (very) slightly more for relabelling, it cannot be reliably used to distinguish between the inhibition and relabelling effects. The *Frac VQ* is included primarily to allow a comparison with the ROE data.

Time Series Regression Analysis

As for the ROE data, time series regression is used to isolate the effects of policy regimes on the main series in question. Regressions are run with two dummy variables representing the policy periods; the unemployment rate and national help wanted index are included to capture cyclical effects; and a linear time trend is used to compensate for any underlying changes over the period that are unrelated to the policy changes of interest. Selected coefficient estimates are presented in Tables 5 through 7.

10 Given the properties of least squares (LS) regression, the coefficients on each variable in the three regressions must "add up" in accord with the identity in (2). The coefficients presented below are, however, not from LSs but were obtained using the Cochrane-Orcutt GLS technique which, while preferable in the presence of autoregressive errors, does not provide strict "adding up."

None of the policy variables are significantly different from zero for the 25-54 age group. The 15-24 age group's coefficients on the period 2 (following Bill C-21) dummy variable are also never significant, but its period 3 coefficient is statistically significant in the *Frac VQ* and *Qrate* regressions. In both significant cases the coefficients are negative. The -0.148 coefficient in the *Frac VQ* equation implies a 13.7 percent drop in the VQ rate of the 15-24 age group that coincides with the Bill C-113 policy change.[11] We get a rough idea of the magnitude of the change by using the pre–policy mean of the fraction, 0.146 of separations that are voluntary with a 13.7 percent drop to 0.126. This number should only be used to get an idea of the magnitudes involved; there are obviously a number of cyclical and seasonal factors operating simultaneously. A similar calculation for the quit rate has it dropping from 0.065 before Bill C- 113 to 0.055 after it, a decrease of 14.4 percent. It thus appears that Bill C-21 had no significant impact on the fraction of workers who label themselves as "voluntary quitters." Bill C-113, on the other hand, caused a modest reduction in these ratios.

Bill C–21 had no significant impact on the fraction of workers who label themselves as "voluntary quitters." Bill C-113, on the other hand, caused a modest reduction in these ratios.

Table 5
Voluntary Quitters Share of Separations in Unemployment by Age
Coefficients from Log (Fraction VQ) Equation (LFS Data)

Variable	15-24	25-54	15-54
Help Wanted Index	0.0003 (0.0006)	0.003* (0.0005)	0.002* (0.0005)
Unemployment Rate	-0.070* (0.009)	-0.048* (0.015)	-0.046* (0.007)
Period 2	0.067 (0.049)	0.011 (0.052)	0.031 (0.038)
Period 3	-0.148* (0.060)	-0.077 (0.065)	-0.091 (0.047)
Adjusted R^2	0.740	0.740	0.820

Notes:
(a) Results are from the Cochrane-Orcutt GLS regression technique.
(b) Time trend and month coefficients are not shown.
(c) Data are 168 observations from Jan. 1980 to Dec. 1993.
(d) Standard errors are in parentheses.
*(e) * indicates that the coefficient is significant at the 5 percent level.*

The *Srate* regression addresses the "inhibition" versus "relabelling" question about the reduction in quits. Its policy variable coefficients are uniformly insignificant, and the estimates are positive or very close to zero. An inhibition effect requires that the total separation rate drop. This has not occurred. From identity (2), it is clear that since the total number of separations has not been significantly affected by the policy, the reduction in the fraction of quitters is entirely due to relabelling, or at least insignificantly different from it.

11 Since this is a dummy variable in a semi–logarithmic equation, the percent effect of the change in the dummy variable on the dependent variable is: *[exp(b) - 1] * 100%*.

Table 6
Voluntary Quit Rate into Unemployment by Age
Coefficients from Log (*Qrate*) Equation (LFS Data)

Variable	15-24	25-54	15-54
Help Wanted Index	0.0007 (0.0007)	0.003* (0.0005)	0.002* (0.0005)
Unemployment Rate	-0.027* (0.011)	0.021* (0.015)	-0.009* (0.008)
Period 2	0.073 (0.059)	0.058 (0.053)	0.056 (0.042)
Period 3	-0.155* (0.073)	-0.086 (0.066)	-0.111* (0.051)
Adjusted R^2	0.460	0.740	0.660

Notes:
(a) Results are from the Cochrane-Orcutt GLS regression technique.
(b) Time trend and month coefficients are not shown.
(c) Data are 168 observations from January 1980 to December 1993.
(d) Standard errors are in parentheses.
*(e) * indicates that the coefficient is significant at the 5 percent level.*

Table 7
Total Separation Rate into Unemployment by Age
Coefficients from Log (*Srate*) Equation (LFS Data)

Variable	15-24	25-54	15-54
Help Wanted Index	0.001* (0.0004)	0.003* (0.0005)	0.0003 (0.0003)
Unemployment Rate	0.057* (0.006)	0.185* (0.017)	0.041* (0.005)
Period 2	0.014 (0.033)	0.001 (0.043)	0.026 (0.026)
Period 3	0.003 (0.041)	0.073 (0.059)	-0.002 (0.032)
Adjusted R^2	0.880	0.800	0.890

Notes:
(a) Results are from the Cochrane-Orcutt GLS regression technique.
(b) Time trend and month coefficients are not shown.
(c) Data are 168 observations from January 1980 to December 1993.
(d) Standard errors are in parentheses.
*(e) * indicates that the coefficient is significant at the 5 percent level.*

As is usually the case, the coefficient estimates are affected by the specification employed. For the regressions presented here, a time trend was included. If it is removed, then the period 3 variable grows in magnitude for the *Srate* and *Qrate* regressions and becomes more strongly significant for the 15-24 age group. The parallel coefficients for the 25-54 age group also grow more negative and become significant. All of the period 2 coefficients remain statistically insignificant, but they become more negative. The *Srate* policy coefficients remain insignificant and near zero for all groups. The result that the reduction in quits is entirely attributable to relabelling, with no significant inhibition effect, remains stable, but the amount of relabelling potentially increases. A residual analysis suggests that including the trend is, however, the preferred specification. There appears to be a trend over time in both the separation rate and the labelling of separations that is unrelated to the policy changes under study. Other specifications were also explored to ensure the robustness of the results. The main result holds throughout; a small drop in the fraction of voluntary quits into unemployment is associated with the introduction of Bill C-113, but the effect appears to have its origin in the relabelling, rather than the inhibition, of separations. The Bill C-21 effect, as reflected in the period 2 dummy variable coefficients, is not found to differ significantly from zero.

Overall, our analysis of the LFS time series data tells us three things. First, the changes in the composition of separations by reason, which we observe both here and in the ROE data when Bills C-21 and C-113 were introduced, appear to be driven entirely by the relabelling of a given number of separations rather than by the "inhibition" of separations by these legislative changes. Our evidence for this is the fact that the rate of quits into unemployment, but not the rate of total separations into unemployment, changes with the introduction of these Bills. Second, we now know something about the segment of the population in which most of this relabelling occurs: it is primarily young workers aged 15-24. Third, in contrast with the ROE time series data, there is stronger evidence in the LFS of relabelling due to Bill C-113 than to Bill C-21. Further analysis should explore this discrepancy, in particular whether it is simply due to the fact that the LFS time series covering the period after Bill C-113 is longer. Further analysis should also use provincially, or at least regionally, disaggregated statistics and more advanced time-series econometric techniques.

Cost Savings from Bill C-113

By definition, the total amount of UI benefits paid out in a given year, *TB*, can be expressed as:

$$TB = CR*BR, \tag{5}$$

where the claim rate, *CR*, is the total number of claims made, and the benefit rate, *BR*, is the average benefit paid per claim. Differentiating both sides of (5) with respect to a policy variable, *P*, and rearranging it yields:

$$(1/TB)*(dTB/dP) = (1/CR)*(dCR/dP) + 1/BR*(dBR/dP). \tag{6}$$

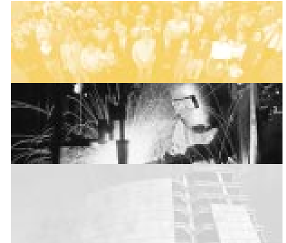
In words, for small changes in *TB*, the percentage change in total benefits paid out (the left-hand side) is a simple sum of the percentage change in the claim rate (*CR*) and the percentage change in the benefit rate (*BR*).

Because Bill C-113 had provisions that were likely to affect the total claim rate as well as the average benefits paid, equation (6) provides a convenient way of aggregating both of these effects into a total effect. Regarding the benefit rate, *BR*, Bill C-113 cut weekly benefits for all UI beneficiaries from 60 to 57 percent of insurable earnings, a 5 percent reduction. Stephen Jones, in a companion piece, finds that Bill C-113 did not reduce benefit durations, and our best estimate of the percentage decline in benefits per claim due to that Bill is also 5 percent. According to Table 4, which provides our best estimates of Bill C-113's total disentitlement effects, Bill C-113 caused an approximately 0.75 percentage point decline in the overall UI claim rate (taking the sum of the VQ disentitlement effect and the relabelling effect and averaging across methods 1 and 2).

…we estimate a rough expenditure reduction of 6.9 percent due to Bill C-113, of which only 1.9 percent is due to the reduction in claim rates resulting from the VQ disentitlement.

Since overall UI claim rates per separation are around 40 percent in these data, this translates into a (0.75/0.4), or approximately 1.9 percent, decline in the overall UI claim rate. Together, we therefore estimate a rough expenditure reduction of 6.9 percent (or, given a 1992 regular UI expenditure of $15.3 billion, about $1.06 billion) due to Bill C-113, of which only 1.9 percent, or $290 million, is due to the reduction in claim rates resulting from the VQ disentitlement. Interestingly, this total reduction corresponds quite closely with the actual UI expenditure reduction of about $1 billion between 1992 and 1993 (Human Resources Development Canada, 1993).[12] Also of interest, if we had ignored the possibility of relabelling of separations in our analysis (thus considering the VQ disentitlement effect only), our prediction of the cost reduction would have been (5 + (1.1)/0.4) 7.75 percent, or $1.15 billion. Estimates of cost savings from program cuts can thus be biased upward if they do not take into account changes due to actions by individuals and firms to soften the blow of those cuts, such as relabelling separations. While this bias is not extremely large in the case of Bill C-113, this may not always be the case, as indicated by our much larger estimates of the relabelling for Bill C-21 (see Table 2).

12 One reason for this close match is undoubtedly the fact that aggregate macroeconomic conditions in the two years were quite similar, making adjustements for these factors unnecessary.



2. The Mechanics Of UI Disentitlement under Bill C-113

Just how did UI disentitlement under Bill C-113 reduce the total UI claim rate of workers who separated from their jobs? Did fewer workers apply for benefits, or were more applicants simply denied benefits? Which workers' application rates changed, if any? Did workers claim less because they become re-employed more quickly, or was there no change in re-employment rates? Unlike the aggregate ROE and LFS data, all this information is available in the "Canadian Out of Employment Panel" (COEP) data set. However, since this panel only covers workers separating immediately before and after the imposition of Bill C-113 (February and May 1993), it can only capture its short-run effects.

The COEP is a survey of individuals separating from their jobs and is designed to supplement existing administrative data in the analysis of Canada's regular Unemployment Insurance system.

The "Canadian Out of Employment Panel" (COEP) Data Set

The COEP is a survey of individuals separating from their jobs. It is designed to supplement existing administrative data in the analysis of Canada's regular unemployment insurance system. The survey was commissioned by HRDC, designed with extensive involvement by the author of this report, and administered by Ekos Research Associates. It is of particular relevance to the evaluation of Bill C-113 because it consists of two cohorts experiencing job separations, one between January 1 and March 13, and the other between April 25 and June 5, 1993. Thus it was possible to exploit the quasi-experimental nature of the policy change occurring at the start of April in evaluating its effects.

The sampling frame for the COEP is the population of individuals receiving an ROE form in one of the two window periods and having a social insurance number ending in five. Aside from ROEs due to participation in a work sharing program, apprenticeship, and retirement at age 65, ROEs issued for all separation reasons were sampled. Individuals were interviewed twice, first at 23-29 weeks after their job separation and then again at 34-45 weeks after. The response rates for the first interview were 70-75 percent of the functional sample, and the overall retention rate from the first to the second interview was 80 percent. Based on information available for the whole population from HRDC administrative data, sampling weights were calculated by Ekos Research Associates that adjust for nonresponse, sample attrition, and for the deliberate oversampling of UI claimants in the survey. A third interview was completed a little more than a year after the initial separation, but was not available for this analysis.

As was documented in a preliminary report to HRDC by Browning, Jones and Kuhn on these data, the COEP appears to be a representative sample of the approximately six million job separations occurring in each year for which ROEs are issued. Of these separations, about half are because of "short work" as reported by the employer, about 15 percent are "voluntary departures," or quits, while the remainder consist of a wide variety of codes, including 18 percent labelled "other."

COEP and ROE Time-Series Data Compared

Comparing changes in UI take-up between February and May 1993 from the Canadian Out of Employment Panel survey to those from ROE time series data gives us an indication of the representativeness of the COEP survey data. We also use the time series data to compare the changes in February-May 1993 in take-up related variables with those in earlier years. This gives us an indication of whether the changes in 1993 were substantially different from earlier ones.

The means of take-up-related variables from the ROE time series data and the COEP data are presented in panels 1 and 2 of Table 8. The four variables that are available in both sources are very similar. The overall take-up rate of UI claims fell by 6-7 percentage points between February and May 1993. The claim rates fell for both VQ separations and non-VQ separations, with a slightly larger fall for the non-VQ group. Both data sources also indicate a rise in the share of VQ's in total separations of between 4 and 5 percent. Finally, Panel 3 of Table 8 shows the means of the three most common ROE codes: short work (*A*); "other" (*K*), and VQ's (*E*). This allows us to provide more precise results on the cases of greatest interest with regard to the regular UI system. Also, since individuals who quit their job simply to take another one are of little relevance in the UI system, workers reporting it are excluded from the COEP analysis sample in this section. As expected, these sample restrictions raise the fraction of VQ's claiming UI and reduce the share of VQ's in the sample. The magnitudes of the February-May changes in all four variables also change, but the same overall pattern remains: Claim rates fall for both VQ's and non-VQ's, but substantially more for VQ's. This is as expected, since one would expect little change in UI claims among workers who quit to take another job, who are now excluded from the analysis. The VQ share in separations still rises, but much less than before. This suggests that most, but not all, of the February-May rise in the VQ share is due to an increase in job-to-job mobility rather than to quits into unemployment.

While the changes reported in Table 8 measure the variables of interest just before and after the policy change mandated by Bill C-113, it would be a mistake to conclude that they measure the effects of that policy change. As the preceding analysis has shown, all the variables are highly seasonal, and the changes in February-May changes might just represent typical seasonal effects. To check for this possibility, ROE time series data are used to compare the changes in all these variables in February-May of 1993 with those in previous years (Table 9). The results of this exercise are striking: in many ways 1993, rather than being obviously different due to the introduction of Bill C-113, was an absolutely typical year. In particular, we conclude first, that the overall UI take-up rate typically falls from February to May by an average of 6.5 percent. The 6.1 percent fall that occurred in 1993 was absolutely typical in this respect, indicating once again that Bill C-113 had no detectable effect on overall UI take-up. Second, the UI take-up rate by non-VQ's also typically falls from February to May, by an average of just over 6 percentage points. The fall in this variable in 1993 seems to be somewhat less than in previous years, but it is unclear how significant this relatively small difference is. Third, the only variable for which 1993 is clearly atypical is the take-up rate for VQ's, which fell more sharply between February and May of 1993 than in any preceding year for which we have data. Indeed, the difference

Table 8
Percentage Change in UI Take-up and Labelling of Separations, February-May (Various Data Sources)

	February 1993	May 1993	February – May Change
1. ROE Data			
CR (overall claim rate within four months)	42.1	36.0	–6.1
CRV (VQ claim rate)	18.7	12.3	–6.4
CRO (non-VQ claim rate)	47.3	43.0	–4.3
VQ (share of VQ's in separation)	18.2	22.6	+4.4
2. COEP Data (No Sample Restrictions)			
CR (overall claim rate within 12 weeks)	41.2 (6,117)	34.5 (6,046)	–6.7
CRV (VQ claim rate)	18.0 (1,091)	11.3 (950)	–6.7
CRO (non-VQ claim rate)	46.3 (5,026)	41.3 (5,096)	–5.0
VQ (share of VQ's in separation)	17.7 (6,257)	22.0 (6,194)	+4.3
3. COEP Data (Sample for Analysis*)			
CR (overall claim rate within 12 weeks)	41.9 (4,769)	38.2 (4,940)	–3.7
CRV (VQ claim rate)	23.4 (803)	16.2 (627)	–7.2
CRO (non-VQ claim rate)	45.5 (3,966)	42.9 (4,313)	–2.6
VQ (share of VQ's in separation)	16.2 (4,888)	17.3 (5,074)	+1.1

Notes:
* *Restricted to ROE codes "A" (short work), "E" (VQ) and "K" ("other"), excluding individuals who reported they "quit to take another job."*
(a) Number of valid observations for each variable is in parentheses.
(b) All calculations use the sampling weights provided by Ekos Research Inc.

between 1993 and the 1985-92 average for *CRV* yields an estimated effect of Bill C-113 on UI take-up by VQ's of -3.5 percentage points, almost identical to that reported in Tables 1 and 3. Finally, the proportion of separations labelled as VQ's typically rises between February and May by an average of 5 percentage points. The 1993 rise in this variable of 4.4 percent is essentially indistinguishable from earlier experience.

Overall, Table 9 and the preceding time series analyses suggest that the only robustly measured effect of Bill C-113 in the ROE data is a fairly modest decrease in the proportion of VQ's claiming UI. In effect, the decreases in overall UI take-up between the February and May cohorts of the COEP survey, as well as the decrease in take-up by non-VQ's, rather than being effects of Bill C-113, appear to be driven by seasonal factors. This is also true of part of the decline in UI take-up by VQ's observed in the survey data, but not all of it, since

Table 9
Changes in UI Take-up and Labelling in Context, February-May 1993 Versus 1985-92

	February–May Percentage Change in:			
	CR (overall claim rate)	*CRV* (VQ claim rate)	*CRO* (non–VQ claim rate)	*VQ* (share of VQ's in separations)
1985	–8.7	–3.7	–8.8	+5.7
1986	–5.7	–2.5	–5.8	+3.7
1987	–6.9	–3.4	–6.7	+5.1
1988	–6.4	–3.2	–6.1	+5.6
1989	–6.6	–3.7	–6.7	+4.2
1990	–3.6	–1.5	–3.5	+3.1
1991	–7.3	–2.2	–6.4	+6.5
1992	–6.9	–3.2	–6.0	+5.9
Average, 1985–92	–6.5	–2.9	–6.3	+5.0
1993	–6.1	–6.4	–4.4	+4.4

the 1993 decline in this variable is truly atypical of previous years. For this reason, we now examine this decline in VQ take-up using the COEP data. While the overall change is likely an overestimate of the effects of Bill C-113, understanding the mechanics of how this change occurred provides useful information about the main effect of Bill C-113 on UI take-up – the UI claim behaviour of workers who quit their jobs.

UI Take-up in the COEP Data

Analytical Framework

In general, whether workers who quit their jobs receive UI within a certain period depends on: first, whether or not they have become re-employed in the interval since separation; second, whether or not they actually apply for UI; and third, whether or not that application is granted by HRDC. Unlike the ROE and LFS time series data, the COEP survey provides information on each of these points, allowing us to document the mechanics of how workers were disentitled from UI by Bill C-113. In terms of Figure 1, we can now study separately the components of the disentitlement effect for VQ's, i.e., the events at nodes *C*, *D*, and *E*.

More formally, the probability of claiming UI can be expressed as:

Prob(Claim) = Prob(Claim | Apply and Re-employed) x Prob(Apply | Re-employed) x Prob(Re-employed) + Prob(Claim | Apply and Not Re-employed) x Prob(Apply | Not Re-employed) x Prob(Not Re-employed). (7)

Expression (7) assumes that one cannot claim UI without applying; it does, however, allow for individuals who are re-employed within a certain interval after separation to claim UI in that interval, since they may claim either before re-employment or after a short employment spell. In this respect it generalizes the simpler conceptual framework set out in Figure 1. In more compact notation, (7) can be written as:

$$C = CRA \times RA \times R + CNA \times NA \times (1-R), \tag{8}$$

where *CRA* is the claim rate of re-employed applicants, *RA* is re-employed workers' application rate, *R* is the fraction of workers who are re-employed, etc.

Taking the total differential of (8) yields:

$$\begin{aligned} dC = \; & RA(R)dCRA + NA(1-R)dCNA + CRA(R)dRA + \\ & CNA(1-R)dNA + (CRA\,RA - CNA\,NA)dR. \end{aligned} \tag{9}$$

The total change in the claim rate can thus be decomposed into five terms. The first two give the change in *C* that is attributable to changes in the claim initiation rate for individuals who have applied for UI, the first for individuals who have become re-employed in the interval since separation and the second for those who have not. Together, these are what we have termed the rejection effect. The next two terms give the change in *C* due to changes in application rates for benefits, holding fixed the fraction of claims accepted and workers' re-employment success. This is what we have termed the discouragement effect. The last term gives the effect of the change in the fraction of workers who are re-employed, the incentive effect.

For finite changes in claim rates such as we shall encounter in the data here, the decomposition in (9) can be expressed as:

$$\begin{aligned} C_2 - C_1 = \; & (CRA_2 - CRA_1)\, RA_2\, R_2 + (CNA_2 - CNA_1)\, NA_2 \\ & (1-R_2) + (RA_2 - RA_1)\, CRA_1\, R_2 + (NA_2 - NA_1)\, CNA_1 \\ & (1-R_2) + (R_2 - R_1)\, (CRA_1 RA_1 - CNA_1 NA_1) \end{aligned} \tag{10}$$

Depending on whether first- or second-period weights are used in various parts of (10) there are now three other ways of expressing this decomposition. In the analysis that follows, results from all four methods are presented.

In order to implement the decomposition in (10), empirical counterparts to the concepts measured there are required. In particular, since there was a variable amount of time between the ROE and survey date for individuals in the sample, and since it varied systematically between the two cohorts, it would not be correct simply to compare total UI applications, re-employment, and take-up by the survey date. To that end, we chose a 12-week window period from each person's ROE date, and determined whether or not each person was re-employed, applied for UI, and/or started UI within that 12-week period. Twelve weeks was chosen because it was the maximum waiting period under the rules prevailing before April 1993. In doing so, we noted that the data included a number of individuals who started UI claims even though said they did not apply for benefits. We decided that, by definition, they must have applied, and modified the data accordingly.

Basic means of variables measuring re-employment, UI applications, and UI take-up are presented for both VQ's and non-VQ's in Table 10. They show that, overall, VQ's worked more weeks out of the first 12 since separation than non-VQ's, even when we exclude individuals who quit just to take another job.

38 percent of job leavers did not apply for UI benefits because they believed they were ineligible, suggesting that a considerable amount of self-screening by workers does occur.

This probably reflects the fact that workers are more likely to quit voluntarily when their re-employment prospects are good, although the difference is not large (about half a week). Interestingly, re-employment actually falls between February and May, which is not what one would expect from an "incentive" effect from Bill C-113. The proportion of VQ's applying for UI is much lower than that of non-VQ's, and falls, as expected, between February and May; however, it falls for non-VQ's as well. Finally, as we saw earlier, the overall UI claim rate, which represents a combination of all the above factors, falls for both VQ's and non-VQ's, but more for the former.

The reasons given by survey respondents who did not apply for UI benefits for their choice are presented in Table 11. Overall, the most frequent reason for not applying, 38 percent of the total, is that the applicants believed they were ineligible for benefits. This is especially the case for workers who quit their jobs, suggesting that a considerable amount of self-screening by workers does occur before they decide to apply for benefits. Interestingly, the fraction of VQ's not applying for this reason rose considerably from February to May, from 40 to 54 percent. This suggests that a discouragement effect may play a role in the decrease in UI claims by VQ's over that period. Aside from "other reasons" and missing data, the next most frequent reason for not applying for UI benefits, at 15 percent of the total, is rapid re-employment. The fraction of VQ's reporting this reason did not change between February and May, however, suggesting that an incentive effect does not play a big role in the decline in VQ take-up. The considerable rise in nonapplicant non-VQ's reporting this reason is somewhat of a mystery, although seasonal variation in re-employment prospects may play a role. Overall, 9 percent of workers did not apply because they were already on UI – suggesting that repeat UI use and interrupted claims are an important phenomenon – and 6 percent waited more than 12 weeks before applying for UI.

Table 10
Means of Take-up Related Variables, by Cohort and Reason for Separation, COEP Data

	VQ's		Non-VQ's	
	February	May	February	May
Weeks Worked of First 12 Since ROE	2.85 (603)	2.74 (479)	2.39 (2,995)	2.16 (3,367)
Proportion Applying for UI in First 12 weeks	53.7% (770)	42.0% (619)	81.1% (3,818)	73.2% (4,200)
Proportion of Applicants Claiming UI in first 12 weeks	41.4% (468)	34.7% (332)	54.8% (3,210)	57.2% (3,401)
Overall Claim Rate in First 12 Weeks	23.4% (801)	16.2% (626)	45.5% (3,943)	42.9% (4,280)

Notes:

(a) Data are from the COEP survey. The sample contains ROE codes A (short work), E(VQ) and K("other") only; it excludes those who "quit to take another job."

(b) Number of valid observations in parentheses. All calculations use sample weights provided by Ekos Research Inc.

Table 11
Frequency Distribution of Reasons Given by VQ's and Non-VQ's for Not Applying for UI Within 12 weeks of Separation, COEP Data

	VQ's		Non-VQ's		Total
	Percentage				
	February	May	February	May	Both cohorts
Believed not Eligible	40.2	54.4	30.5	36.6	38.0
Applied after 12 weeks	5.2	3.5	7.8	6.0	6.1
Already on UI	2.1	4.2	7.3	14.5	9.0
Found Another Job Right Away	18.3	18.4	7.9	18.0	14.9
Don't believe in UI; Have Enough Money	9.8	9.5	4.7	5.6	6.5
Other – no reason – missing	24.4	10.0	41.8	19.3	25.6
	100.0	100.0	100.0	100.0	100.0

Finally, it is interesting to note that overall, 6.5 percent of non-applying workers (and almost 10 percent of non-applying quitters) made this choice, apparently on principle, because they "don't believe in UI," or "have enough money."

The main results of our decomposition are presented in Tables 12 and 13. The samples differ somewhat from those used previously since they exclude individuals for whom information was missing for any of the take-up-related variables analyzed here: employment since ROE, UI application rates, and UI take-up rates. The decline in UI take-up of 9.6 percent by VQ's shown in Table 5 is somewhat higher than in the more comprehensive sample, but of the same order of magnitude. Since this is the highest estimate of this number from any of our estimating procedures, and it does not net out the usual seasonal decline in VQ take-up between February and May, we view it as an upper bound on the effect of Bill C-113 on UI take-up. The rest of this subsection is devoted to partitioning this decline into a number of component effects.

According to Table 12, the decline in UI take-up by VQ's between February and May of 1993 was accompanied by a sizable drop in the number of UI applications made by re-employed workers that actually led to UI claims. This suggests that UI officers were more likely to reject UI claims made by re-employed VQ's after Bill C-113 than before it; perhaps evidence of a rejection effect. Interestingly, this does not appear to be the case for applications made by VQ's who had no re-employment success: the fraction of such applications resulting in UI claims within 12 weeks remained essentially unchanged between February and May.

The drop in VQ take-up between February and May was also accompanied by a drop in the fraction of workers applying for UI benefits within 12 weeks of separation. This time, however, the drop (from 73 to 49 percent) was much greater among workers who were not re-employed than those who were. Thus there is also some evidence of a discouragement effect in the COEP data. Finally, rather than rising as predicted by the incentive effect, the proportion of voluntary quitters (who did not quit specifically to take another job)who were re-employed within 12 weeks actually fell between February and May of 1993. Thus there is no evidence of an incentive effect of the VQ disentitlement here.

…by far the largest factor explaining the 9.6 percent drop in UI take-up among VQ's between February and May 1993 is the decrease in applications for UI among job quitters who experienced no re-employment in the 12 weeks following separation.

While very suggestive, the figures in Table 12 do not tell us to what extent each of the effects isolated above actually contributed to the total fall in UI claims among VQ's. This is done in Table 13, which gives results for each of the four possible decompositions of the total change in VQ take-up. Method 1 corresponds exactly to equation (10); the other methods represent different combinations of the period subscripts on the weights attached to each of the five change terms in (8). The main message of Table 13 is clear: by far the largest factor explaining the 9.6 percent drop in UI take-up among VQ's between February and May 1993 is the decrease in applications for UI among job quitters who experienced no re-employment in the 12 weeks following separation. This "discouragement effect" alone explains about 7.5 percentage points of the total 9.6 percentage point change in VQ take-up. Virtually negligible in importance are the discouragement effect for workers with some re-employment success, the rejection effect for workers with no re-employment success, and the "wrong-signed" incentive effect. The only other factor apparently making a substantial contribution to the decline in VQ take-up is the increase in the fraction of applications made by re-employed quitters that are apparently rejected by HRDC.

Table 12
Estimated Effects of Bill C-113 on Take-up Related Variables for Voluntary Quitters (VQ's), COEP Data

	Before	After	Net Change
		Percentage	
Total Claim Rate (*C*) (within 12 weeks of ROE)	28.3	18.6	–9.6
Claim Rate of Re-employed Applicants (*CRA*)	32.5	20.7	–11.7
Claim Rate of Non Re-employed Applicants (*CNA*)	51.7	51.0	–0.7
Proportion of Re-employed Workers Applying for Benefits(*RA*) (within 12 weeks of ROE)	51.3	46.0	–5.3
Proportion of Non Re-employed Workers Applying for Benefits (*NA*)	73.0	49.3	–23.7
Proportion of Workers with any Re-employment within 12 weeks of ROE (*R*)	44.8	41.7	–3.1

Notes:
(a) Totals may not add exactly due to rounding.
(b All calculations use sample weights provided by Ekos Research Inc.
(c) Sample differs somewhat from Table 10 because observations with missing data on any of the variables are now excluded from sample.

Probit Analysis of Applications, Rejections, and Re-employment

One possible concern with the above conclusions is that the data they draw on do not adjust for possible differences across cohorts in worker characteristics that might affect re-employment. While this should not be a problem in a true experimental design where workers are randomly assigned to treatments (i.e., covered by Bill C-113 or not), it may be a problem here if workers who separate in May are systematically different from those who separate in February in ways that might affect their UI take-up behaviour.

To check for this possibility, we estimated a series of linear probability models and probits of all the variables in Table 13 (*CRA*, *CNA*, *RA*, *NA*, *R* and *C*) on a dummy variable for belonging to cohort 2, and a series of demographic and economic variables which might plausibly affect UI take-up. To ensure a consistent estimation sample across specifications, the data used for these models exclude observations with missing values for any of the independent variables used in the regressions. The linear probability model coefficients for the cohort 2 dummy variable in a variety of specifications are reported in Table 14 and the probits in Table 15. As the results shown in Tables 14 and 15 are quite similar, we confine our discussion in what follows to the linear probability models. This is convenient since, unlike logit or probit models, the magnitude of the cohort 2 effect on the probability of observing the outcome in question, in percentage points, is just equal to the linear probability coefficient on the cohort 2 dummy.

Table 13
Contribution of Various Factors to the Change in UI Take-up by VQ's, February – May 1993[13]

	Method 1	Method 2	Method 3	Method 4
	Percentage			
Rejection Effects (Effect of Change in Fraction of UI Applicants with given Re-employment Experience starting UI claims)				
(a) For workers with some re-employment (change in *CRA*)	–2.2	–2.4	–2.5	–2.7
(b) For workers with no re-employment (change in *CNA*)	–0.2	–0.2	–0.3	–0.3
(c) Total	–2.4	–2.6	–2.8	–3.0
Discouragement Effects (Effect of Change in Fraction of Individuals with given Re-employment Experience applying for UI)				
(a) For workers with some Re-employment (change in *RA*)	–0.7	–0.8	–0.5	–0.5
(b) For workers with no Re-employment (change in *NA*)	–7.1	–6.7	–7.0	–6.7
(c) Total	–7.8	–7.5	–7.5	–7.2
Incentive effect (Effect of change in Fraction of Individuals with some Re-employment) (Change in *R*)	+0.7	+0.5	+0.7	+0.5
Total Change in UI Take-up by VQ's	–9.6	–9.6	–9.6	9.6

Note: Totals may not add exactly due to rounding.

With no covariates except the cohort dummy, the coefficients in column 1 of Table 14 simply equal the mean difference between the two cohorts on the variable in question. These differ from the "net change" column in Table 12 only because of the more restrictive sample. While some of the smaller numbers (like

13 As we do not explicitly attempt to adjust for sample selection into, for example, the sample of UI applicants or the sample of re-employed workers, the models in Table 13 implicitly assume that the error terms in the equations for application rates, claim rates, and re-employment are independent. Given our fairly exhaustive set of control variables and the "natural experiment" nature of the Bill C-113 policy change, this may not be an unreasonable assumption. Still, in future work we plan to explore the possibility of correlated errors by invoking some exclusion restrictions on variables in the various selection equations.

Table 14
Components of Take-up by VQ's (Cohort #2 Coefficient, Linear Probability Models)

	(1)	(2)	(3)	(4)
Claim Rate of Re-employed Applicants (*CRA*)	–0.1330* (0.0654)	–0.0992 (0.0683)	–0.0770 (0.0644)	–0.0483 (0.0651)
Claim Rate of Non Re-employed Applicants (*CNA*)	0.0241 (0.0636)	0.0330 (0.0615)	0.0426 (0.0580)	0.0478 (0.0581)
Application Rate of Re-employed Workers (*RA*)	0.0112 (0.0632)	0.0339 (0.0627)	0.0452 (0.0616)	0.0531 (0.0598)
Application Rate of Non Re-employed Workers (*NA*)	–0.1470* (0.0536)	–0.1440* (0.0540)	–0.1350* (0.0509)	–0.1010* (0.0486)
Re-employment Rate (*R*)	0.0212 (0.0415)	0.0129 (0.0412)	0.0123 (0.0409)	0.0090 (0.0409)

Specifications
(1) No covariates.
(2) Age and sex controls.
(3) Age, sex, visible minority, spouse present, education, and province.
(4) Age, sex, visible minority, spouse present, spouse labour supply, province, pre-ROE wages and tenure, UI eligibility, previous UI experience.

Notes:
(a) Heteroskedasticity consistent standard errors are in parentheses.
** Indicates significant at a 5 percent level.*
(b) All calculations use the sampling weights provided by Ekos Research Inc.

Table 15
Components of Take–up by VQ's (Cohort #2 Coefficient, Probit Models)

	(1)	(2)	(3)	(4)
Claim Rate of Re-employed Applicants (*CRA*)	–0.3800* (0.1965)	–0.3124 (0.2056)	–0.2667 (0.2342)	–0.1253 (0.2519)
Claim Rate of Non Re-employed Applicants (*CNA*)	0.0604 (0.1322)	0.0868 (0.1371)	0.1317 (0.1450)	0.1526 (0.1490)
Application Rate of Re-employed Workers (*RA*)	–0.0281 (0.1417)	0.0899 (0.0627)	0.1236 (0.1574)	0.1575 (0.1631)
Application Rate of Non Re-employed Workers (*NA*)	–0.4115* (0.1208)	–0.4117* (0.1227)	–0.3900* (0.1289)	–0.3360* (0.1360)
Re-employment Rate (*R*)	0.0534 (0.0895)	0.0330 (0.0913)	0.0267 (0.0942)	0.0146 (0.0954)

Specifications:
(1) No covariates.
(2) Age and sex controls.
(3) Age, sex, visible minority, spouse present, education, and province.
(4) Age, sex, visible minority, spouse present, spouse labour supply, province, pre-ROE wages and tenure, UI eligibility, previous UI experience.

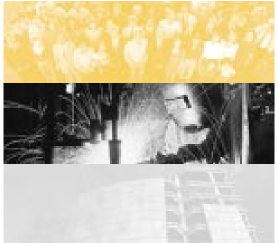
Notes:
(a) Asymptotic standard errors in parentheses.
** Indicates significant at a 5 percent level.*
(b) All calculations use the sampling weights provided by Ekos Research Inc.

the tiny change in *CNA*) change sign, the major effects are the same, although the magnitude of the change in *NA*, the application rate of workers who are not re-employed, is lower. The only two coefficients that are statistically significant are those for *CRA* and *NA*, which were the largest differences in Table 12 as well. Looking across columns 2-4 now gives an idea of the effects of adding covariates on these raw cohort differences, of which there are three. First, the small, insignificant effects (*CNA*, the rejection effect for workers who are not re-employed; *RA*, the discouragement effect for workers who are re-employed; and R, the incentive effect) remain small and insignificant. Second, the rejection effect for re-employed applicants (*CRA*), which was large and significant without controls, falls considerably in sign and becomes insignificant as tighter controls are added. The major effect identified in the Tables 12 and 13, i.e., the discouragement effect for non-reemployed workers, remains large in magnitude and statistically significant. Interestingly, the decline in its magnitude in column 4 is a result, at least in part, of including an estimated UI eligibility variable, which equals one if the individual had enough pre-ROE weeks to qualify for benefits. Thus some, but by no means all, of the discouragement effect identified in Table 13 is a spurious consequence of differences in pre-ROE weeks of employment across cohorts, which caused a systematic difference in eligibility. Overall, however, we found that the main vehicle by which Bill C-113 reduced UI claims was through a discouragement effect among workers who were not re-employed: after the change, these workers were significantly less likely to apply for UI than before, even when a long list of economic and demographic variables are controlled for.

…we found that the main vehicle by which Bill C-113 reduced UI claims was through a discouragement effect among workers who were not re-employed: after the change, these workers were significantly less likely to apply for UI than before, even when a long list of economic and demographic variables are controlled for.

Since one would not have expected Bill C-113 to have a major effect on the take-up rates of non-VQ's, and since the change between February and May in take-up by non-VQ's does appear to be indistinguishable from the usual seasonal effects operating between February and May, our main focus in this section has been to study the "mechanics" of the abnormal decline in UI take-up by VQ's between February and May of 1993. A considerable part of this decline is likely due to the introduction of Bill C-113 in April of 1993.

Our main findings on this matter are as follows. First, incentive effects, i.e., quicker re-employment in response to the elimination of UI benefits for VQ's, do not appear to play a role in explaining the decline in VQ take-up between February and May. In fact, they operate in the "wrong" direction: re-employment rates went down slightly, tending to raise the claim rate. Second, the major factor at work appears to have been a "discouragement effect," particularly among VQ's who were relatively unsuccessful in finding re-employment. After April 1993 a considerably larger fraction of these workers simply decided not to apply for UI benefits because they believed (possibly quite correctly) that they were ineligible for them. Third, the only other factor that might play a significant role is a "rejection effect," operating among workers who had some success in finding re-employment shortly after they quit their jobs. Such an effect would be important if officials at HRDC were more willing to deny UI applications by individuals in these situations after April 1993 than before; however, estimates of this effect are not robust to the addition of control variables for observed differences between workers separating in February and May.

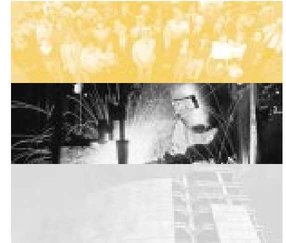


3. Conclusion

…the benefit cuts in Bill C-113 had at most a very small effect on overall UI take-up rates, probably in the order of less than 1 percentage point.

Overall, the analysis in this report shows quite conclusively (perhaps not surprisingly) that the benefit cuts in Bill C-113 had at most a very small effect on overall UI take-up rates, probably in the order of less than 1 percentage point. There are several reasons for this. First, the measured decrease in UI claims by the group most directly affected by Bill C-113, VQ's, was actually fairly modest, at the very most 9.5 percentage points, from 28 to 18.5 percent. Second, even if the decline in take-up among VQ's had been bigger, it still could not have had a major effect on overall UI take-up because the initial VQ take-up rate was already very small, and VQ's are a fairly small share of total separations. Third, some of the direct disentitlement effect of the legislation was counterbalanced by a relabelling of VQ separations as non-VQ separations, which would tend to raise claim rates because non-VQ's have higher UI claim rates than VQ's. Interestingly, however, the estimated magnitude of this relabelling effect differs between data sources; it is actually higher in the self-reported data on reasons for separation in the Labour Force Survey data (which have no implications for UI eligibility) than in the employer-reported ROE data (which do have such implications).

Our analysis also produced some findings on the "mechanics" by which the drop in UI claim rates by voluntary quitters occurred. First, we find no evidence of an inhibition effect, i.e., that Bill C-113 induced people to stay in jobs they otherwise would have quit. Second, neither do we find any evidence of incentive effects of Bill C113, i.e., quicker re-employment in response to the elimination of UI benefits among VQ's. If anything, re-employment immediately after separation actually appears to have fallen after the Bill was implemented. Third, the major factor appears to have been a discouragement effect, particularly among VQ's who were relatively unsuccessful in finding re-employment: after April 1993 a considerably larger fraction of these workers simply decided not to apply for UI benefits because they believed (possibly quite correctly) that they were ineligible for them. Finally, the only other major contributing factor might be a "rejection effect," operating for workers who had some success in finding re-employment shortly after they quit their jobs. Although this effect is not robustly estimated, it may be that officials in HRDC were more likely to deny UI applications made by individuals in these situations after April 1993 than before.



Bibliography

Anderson, P. and B. Meyer (1994), "Unemployment Insurance Benefits and Take-up Rates," Working Paper no. 4787, National Bureau of Economic Research.

Canada, Human Resources Development, Financial Research Directorate, "Comparison of UI Benefits in 1992 and 1993," and "Estimated C-113 Cost Savings," June 1, 1994.

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 0
Hull, Quebec
K1A 0J9 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, Ekos 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)*

- **State Dependence and Unemployment Insurance**
  T. Lemieux and B. MacLeod, Centre de recherche et développement en économique, Université de Montréal, 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 Stabilizer 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 Stabilizer**
  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 Québec à Montréal, 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)*

- **Implication of Extending Unemployment Insurance Coverage to Self–Employment and Short Hours Work Week: A Microsimulation 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. *(Evaluation 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 Microsimulation 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 Microsimulation 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.