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

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UI and the Labour Market

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Unemployment Insurance Evaluation Series

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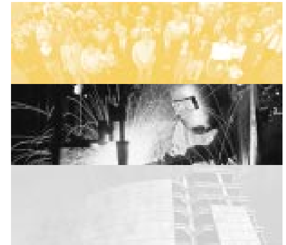

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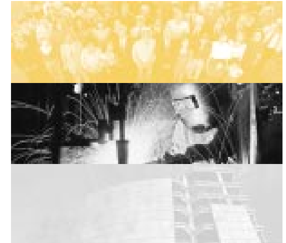
Abstract

This paper investigates the effects of Bill C-113 (which came into effect on April 1, 1993) on the durations of unemployment spells, the job-search behaviour of the unemployed, and the quality of new jobs (as measured by wages and hours worked) found after an unemployment spell. The study focuses primarily on a comparison of two groups — individuals who were subject to the legislation in effect prior to Bill C-113 (the "Before" sample); and those who had to cope with the Bill's less generous unemployment insurance (UI) provisions (the "After" sample).

The conclusions for unemployment durations can be summarized as follows. When the experiences of the two cohorts are compared, it is found that the members of the Before group suffer less unemployment than those of the After group. This conclusion holds true for the proportions of individuals in each cohort who remained unemployed (evaluated at various durations), for the proportions of those who were employed when the Before and After interviews were conducted, and for a variety of econometric and statistical specifications used to control for the influence of other variables that could differ between the two cohorts or that could vary over the course of unemployment spells. Thus the Before group, which enjoyed the more generous UI provisions, typically performed somewhat better than the After group, which faced a lower replacement rate and was subject to the complete disqualification of workers who left their jobs without justification (as defined in Bill C-113). While the interpretation of these findings is somewhat problematic, given that they are probably the reverse of what intuition might suggest, it can safely be stated that there is no evidence that the April 1993 reduction in UI benefits resulted in shorter unemployment durations.

With regard to job search, there is little evidence that either search inputs (number of search hours or expenses) or reservation wages vary systematically by cohort. Interestingly, controlling for these search inputs (which can vary over time) in the analysis of unemployment durations does not reverse the conclusions about the two cohorts' unemployment experiences.

Finally, neither of the objective measures of new-job quality displays a significant difference across the two cohorts, once relevant control variables are introduced. The subjective measure of overall job satisfaction does exhibit some cohort effects, with the members of the After group tending to be less satisfied with their post-unemployment jobs than their Before C-113 counterparts. Although there are reasons to be cautious in assessing the importance attached to this indicator — in particular with respect to the unusual distribution of rankings, in which so few workers report a decline in satisfaction, relative to the reference "Record of Employment" job — these results may strengthen the cohort effect found for unemployment: the members of the After group were prepared to take less desirable jobs more quickly than their Before counterparts.



Introduction

It is difficult to disentangle the direct *effects of UI benefits on the behaviour of both workers and the unemployed, and the* indirect *effects of all the factors that influence past earnings.*

This paper forms part of the final report on the "Studies of Unemployment Insurance Based on the Canadian Out of Employment Panel Survey." Its objective is to assess the effects of Bill C-113 on the durations of unemployment spells, on the job-search behaviour of the unemployed, and on the quality of the new job (as measured by wages and hours worked) found after an unemployment spell. Two features of the bill (which came into effect on April 1, 1993) are specifically examined in the study: 1) the lowering of the wage replacement rate from 60 percent to 57 percent of insurable earnings; and 2) the disentitlement of "unjustified separations" (i.e. voluntary quits and dismissals) from UI benefits. A brief discussion of the scope of the investigation is followed by a detailed analysis of the data and of the patterns observed in the results.

A number of researchers have noted that there is no exogenous variation in the parameters of the unemployment insurance (UI) programs of many countries. In the U.S. context, for example, Welch (1977) points out that the level of weekly UI benefits within a particular state is determined by the level of previous earnings (subject to possible minimum and maximum benefit levels). This implies that it is difficult to disentangle the *direct* effects of UI benefits on the behaviour of both workers and the unemployed, and the *indirect* effects of all the factors that influence past earnings (see also Meyer 1992). The estimated effects on unemployment durations or on wage gains or losses in a subsequent job could thus be biased, although it is difficult to determine the direction of the bias.

Applied to the Canadian context, this type of criticism has greater force since, with the exception of the regional aspects of benefit eligibility and the variable entrance requirement (VER) for such eligibility, the program's parameters are national in scope. As a result, there is no equivalent in Canada of the state-level variation exploited in some U.S. research. The authors of previous studies of the Canadian UI system (as surveyed, for example, by Corak 1993) therefore had to rely, in large part, on a strong belief in the precise statistical structure imposed on the data to identify the effects of different benefit levels.[1] To the extent that other researchers or policy makers may have misgivings about the ability of the data to distinguish between the direct effects of UI and the influence of all the other variables that affect UI eligibility and recipiency, the conclusions drawn from these past studies should be treated with some caution.

Data drawn from various UI administrative files were used in some of the most influential work on the Canadian unemployment insurance program in the past.[2] However, these files suffer from deficiencies in such areas as demographic information and the behaviour of claimants after the termination of their benefit period. The present project seeks to compensate for some of these shortcomings by using a dataset created as part of the Canadian Out of Employment Panel

1 Green and Riddell (1993) is one recent exception that exploits a legislative "fluke" that temporarily altered the variable entrance requirement.

2 Ham and Rea (1987) is an important study using the Status Vector administrative data.

(COEP) study undertaken by Human Resources Development Canada (HRDC) in 1993. In addition to supplementing the deficiencies of the UI administrative files, the COEP data can be used to assess the changes that were enacted in Bill C-113.

The COEP data were drawn from two six-week sampling periods bracketing the April 1993 legislative change: people who became unemployed between January 31 and March 7 represented the "Before" sample, or cohort 1; those who became unemployed between April 25 and June 5 formed the "After" sample, or cohort 2.[3] The subjects were selected at random, with social-insurance numbers ending with the numeral 5 being used as the criterion, among people with a Record of Employment (ROE) in the two sampling periods. (Canadian employers are required to issue a Record of Employment form whenever a job separation occurs.) Aside from ROEs issued for participation in a work sharing program, apprenticeship, or retirement at age 65, ROEs 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) in order to determine whether they had undergone a change in status either between that moment and the first interview or between the two interviews.

The focus of the present study is on four groups within the two samples, defined by the ROE reason for separation and distributed as follows, based on ROE files between July 1992 and June 1993:

Voluntary quits (VQs)	16 percent
Dismissals	4 percent
Shortage of work (SWs)	49 percent
Other reasons	18 percent.

The remaining 13 percent of separations were due to a variety of particular reasons not otherwise accounted for (such as labour disputes, maternity, retirement, and return to school). The first two groups may have been disentitled altogether by the April 1993 changes — and even if they were not disentitled, they would certainly have been affected by the replacement rate change — and so in some of the subsequent analysis they are grouped together. Members of the last two categories were only affected by the change in UI benefit rates; again, they are grouped in some of the analytical work.

In practice, it should be noted that the assignment of individuals to the two cohorts is not completely unambiguous, in part because these data (like all real datasets) can be assumed to contain measurement and coding errors. Three groupings into cohorts may potentially be employed; Tables A.1 and A.2 illustrate the issues involved. First, the data grouped individuals by cohort. These are the cohort groups corresponding to the rows in the tables. Second, the replacement ratio received by each individual UI beneficiary was constructed from the UI administrative data, with members of cohort 1 receiving 60 percent of past insurable

3 The use of two survey periods bracketing the April 1993 changes made it possible to exploit the quasi-experimental nature of the UI data: since the changes may be considered to be exogenous from the perspective of individual claimants, the results should not be too dependent on particular assumptions about how behaviour is modelled. For influential U.S. work that exploits a similar natural experiment associated with changes in maxima of weekly benefit amounts across a number of states, see Meyer (1992).

earnings up to the maximum, and members of cohort 2 receiving 57 percent. This gives the alternative (and quite distinct) cohort groupings represented in the columns of Tables A.1 and A.2.[4] Finally, one could also group the individuals by the week of the separation, as documented in the relevant ROE. Because of possible measurement errors, however, this type of data might not line up exactly with the cohort variables from the COEP.

The first part of Table A.1 shows the major difference between the original cohorts and the benefit rate-based cohorts — namely, that while the "diagonal" elements are always very large (so that, for example, 4,845 individuals from the COEP cohort 1 are in benefit-based cohort 1), there is a substantial group of 620 individuals in the COEP cohort 1 who received the 57 percent replacement rate and hence find themselves in benefit-based cohort 2. Investigation shows that this group is largely made up of people who were late in filing for UI benefits and who ended up receiving the lower wage replacement rate because their benefit period began after the legislative change. The origin of the other "off-diagonal" group — the 13 persons in the COEP cohort 2 who appear to have received a 60 percent replacement rate — can probably be attributed to measurement error.

The remainder of Table A.1 details the breakdown of these anomalous subcohorts by gender and by reason for separation. The substantial off-diagonal group is equally split by gender, and all four reasons for separation give rise to the anomaly, although it clearly larger, proportionately, for the SW and Other groups. Table A.2 also supplies the breakdown of these results for the 12 key ROE weeks in the periods targeted by the interviews. (There are also some observations with reported ROE weeks outside these intervals, probably as a result of measurement or coding error.) The numbers in column 2 — those receiving the 57 percent replacement rate, who are in the COEP cohort 1 — rise as the ROE week moves closer to the date of the legislative change, which is consistent with the late-filing interpretation. Again, the small numbers of individuals in benefit cohort 1 but with an ROE date on the COEP cohort 2 are puzzling, as are all entries other than 0 in the rows labelled 1 in the lower half of the table.

Given these various definitions of the cohorts, a study of the labour market effects of the legislative changes was undertaken, using either the COEP cohort variables or the benefit-based cohort measures. This section of the report presents results for the benefit-based groupings, since search behaviour during an unemployment spell and job-acceptance behaviour at the end of the spell are more likely to be influenced by the actual UI recipiency patterns than by the prospective replacement rate an individual might have envisaged around the date of separation. In any event, the use of the alternative COEP cohort variable does not substantially alter any of the conclusions on labour market behaviour.

Three further issues should be mentioned briefly before turning to the results. A first concern in the use of quasi-experimental data of this type pertains to strategic filing — that is, to the possibility that some individuals foresaw the legislative

4 In the case of those who did not receive benefits during the two periods, the default definition of the cohorts was adopted; 59 percent of the sample have the value of the cohort dummy variable determined by their observed replacement ratio along the lines described in the text, while for the remaining 41 percent the value determined from the cohort variable implied in the COEP is employed.

change on April 1, 1993 and altered their behaviour accordingly in order to become eligible for the higher 60 percent replacement rate of the previous legislation. Specifically, an individual who anticipated a long spell of unemployment could have acted to influence the timing of a job separation so as to initiate a claim before the date of the change. A worker who anticipated only a short unemployment duration might not have bothered to manipulate claim timing in this way. If this scenario holds true, it would clearly generate a positive correlation between the replacement rate and unemployment duration, caused by strategic filing behaviour rather than in response to the change in the UI replacement rate.

To address this concern, note that the three-week period around the date of the change was purposely excluded from the sample. If individuals did file UI claims with a strategic view of their timing, this would be assumed to take place shortly before the April 1 legislative change, and the people concerned would therefore be excluded from the relevant sample in the COEP data. Second, it is also clear from the discussion about cohort grouping that many individuals seem, in fact, to have filed late, behaving in a way that was both non-strategic and unfortunate for them. Third, a back-of-the-envelope calculation suggests that the present study should be little affected by these timing issues. Suppose the relevant choice is between filing a claim on April 1 and filing early enough both to qualify for the higher replacement rate *and* to fall into the COEP sample. This amounts to a three-week minimum gap: hence, for a strategic filer whose claim is established immediately following the separation, three weeks of wages that must be sacrificed. The gain from a claim filed before April is 3 percent of wages (60 vs. 57 percent) for each week of UI benefits. As a simple first approximation, therefore, one would require a claim of about 100 weeks for this benefit/cost calculation to break even. Adding discounting and utility considerations to the equation would complicate the picture (probably in opposite directions), but I suspect that any reasonable model would predict little or no strategic filing in the sample selected for this study.

A second source of concern with these quasi-experimental data is precisely that there may *not* be an exact experiment because other factors may differentiate the two cohorts. They may have different attributes prior to the ROE date (for example, if different types of people tend to have job separations in February and March than in May) or their members may experience different labour market conditions subsequent to their individual ROE dates — something that may be seasonal (e.g., if the local labour market behaves differently in April than in July) or that may be part of a larger (secular) macroeconomic trend. In the work below, an attempt is made to control for these factors by allowing a variety of control variables to affect labour market outcomes when looking for a cohort effect. Duration modelling techniques are also employed to allow for changing labour market conditions in each month of an unemployment spell as a further means of addressing the problem of non-experimental differences across cohorts subsequent to the ROE date.

Third, it should be mentioned that the COEP data are constructed out of data from two sets of interviews, matched to the appropriate administrative data files of the UI program. A problem in many panel surveys of this type is attrition, whereby individuals covered in interview 1 cannot be contacted for interview 2 or refuse to submit to it. In the COEP data, the attrition rate is 19.9 percent of the interview 1 sample, a figure that is substantially higher than that associated with some other Canadian surveys (such as the Labour Market Activity Survey). To the extent that attrition is random and uncorrelated with behaviour, it does not pose a problem. However, when it is the result of particular patterns of behaviour (such as moving to take a new job, which makes tracking and re-interviewing harder to achieve), non-random attrition may bias the analysis. Few studies take detailed account of non-random attrition, although clearly it is something that could be addressed in future work with the COEP dataset, especially when the analysis of the data gathered in interview 3 is complete. In the analysis reported below, some sensitivity testing for the effects of attrition was conducted, and it was concluded that attrition does not seem to drive the main patterns of the results.

Finally, it should be mentioned that except where explicitly noted, the estimates are based on weighted data. The COEP used a stratified sampling scheme, and the sample weights are used to control for this. In practice, however, the main conclusions are not sensitive to the use of either weighted or unweighted data.

1. Re-employment at the Two Interview Dates

The first COEP interview took place 23 to 29 weeks after the ROE date, while the second was held 34 to 45 weeks after that date.[5] (The results of a third interview, conducted in April 1994, have not been considered here.) The first set of results concerns the proportions of people in each sample who reported a current job and wage at the two interview dates. Note that this was not necessarily the first job held after the ROE separation, a point to which I shall return shortly. Rather, this snapshot gives a summary of the overall effects of the legislative change without detailing the intermediate processes (such as transitional jobholding) that may lie behind the final result. Table A.3 provides the proportions, by cohort and by gender and age, of those in the VQ/Dismissals and SW/Other groups who had a job at interview 1. Table A.4 reports similar results at interview 2.

For both the VQ/Dismissals and SW/Other groups, the proportion reporting a current wage declines from cohort 1 to cohort 2 at both interviews. Proportionately, the difference is somewhat greater for the VQ/Dismissals group, but it is clearly present for both. Across the two interview dates, each group experiences a rise in the proportion reporting a current wage, although cohort 2's SW/Other group only reaches the 40 percent mark at interview 2 — a level the VQ/Dismissals group had attained by interview 1. By gender, the decline in the proportion with a current wage is relatively small for female SW/Other group members but larger for women in the VQ/Dismissals group. For men, the reverse is true, with SW/Other individuals recording the larger drop from cohort 1 to cohort 2. Finally, by age group (young, prime-age, and older), the proportion reporting a current wage declines for both separation categories and for each of the three age groups. In some cases, the sample sizes are not large, but the pattern is unmistakable.

A probit model of binary choice was then used to assess the determinants of this employment probability. While a large number of alternative specifications were investigated, only one set of results is reported in Table A.5, where the model is estimated for the four separation groups, first pooled and then divided into VQ/Dismissals and SW/Other groups. The explanatory variables in this particular specification include a cohort dummy variable (taking the value 1 when then replacement rate is 57 percent or, if the replacement rate cannot be calculated, when the COEP cohort variable is cohort 2), a set of demographic variables, a set of regional controls (Ontario being omitted), and some limited information on the ROE reference job (the separation from which led to each individual's inclusion in this sample). The demographic variables are: a quadratic in age, dummy variables for visible minority status and marital status ("never married" being omitted), and a set of educational dummy variables ("completed high school" being omitted).

The results are quite striking in their regularity. In each case, without the other

5 There is a small difference between the cohorts in the timing of the interviews, with cohort 1 having interview 1 in weeks 23–26 and cohort 2 having this interview in weeks 26–29 (and 34–43 and 38–45 for interview 2). The evaluation of job-holding and other point-in-time variables at the earliest consistent date in each case (for example, at the 23-week point for all interview 1 variables) does not alter the conclusions on the comparison between cohorts.

controls and except for the VQ/Dismissals group with controls, the coefficient on the cohort 2 dummy variable is negative, which is consistent with the proportions data in the previous tables. That is, being in cohort 2 tends to lower the probability of reporting a current job and wage at interview 1, even allowing for a large set of controls.[6] Without the other explanatory variables, the effect is strong and negative for all groups, and the cohort effect remains significant in the full specification. Other variables have the expected sign, with "visible minority status" lowering the probability of a current wage and with "more education" tending to raise this probability, although many of the provincial controls are not significant in this respect. Table A.6 reports the coefficients and standard errors on the cohort 2 variable when the specification from Table A.5 is estimated separately by gender. Without controls, the cohort 2 dummy variable is negative and significant for each gender and each separation group while, with the controls as in the preceding table, the cohort 2 variable remains negative and significant for VQ/Dismissals females and for SW/Other males. For VQ/Dismissals for men, the point estimate is positive but with a *t*-ratio of only 1, while for SW/Other for women, the point estimate is negative but also insignificant.

The members of cohort 2 do worse in terms of having a job at each interview date than do their counterparts in cohort 1.

Analogous probit estimation results are given in Tables A.7 and A.8 for the probability of reporting a current wage at interview 2. The results are very similar, with significantly negative cohort 2 effects without controls for all groups and with controls overall, and for the SW/Other separation group (the VQ/Dismissals group point estimate remains negative but insignificant). When they are disaggregated by gender, the coefficients reported in Table A.8 largely echo those in Table A.6, with all significant estimates being negative and significant, except those for women in the VQ/Dismissals group.

Overall, this pattern of results is fairly clear in suggesting that the members of cohort 2 do worse in terms of having a job at each interview date than do their counterparts in cohort 1. This result holds at each point in time that is observed in the COEP data and thus cannot be considered an artefact of attrition-inducing bias in the interview 2 sample. It also seems to hold both unconditionally and with allowance for a variety of other control variables (demographic, regional and ROE-job related) that could otherwise potentially act to undermine the experimental nature of the comparison of the two cohorts. Finally, though the effect does differ, it holds for men and women and it holds for both the VQ/Dismissals and the SW/Other separation groups.

6 The addition of other controls such as the presence of children in the household, estimated UI eligibility, and spouse's ability to work does not alter this conclusion.

2. *Durations of First Post-ROE Unemployment Spells*

Duration analysis is based on notion of "hazard": the theoretical hazard is the probability that a given spell will end within a specified time period, provided it has not ended prior to that period.

In addition to the point-in-time information examined in the preceding section, it is important to study the durations of unemployment experienced after the ROE reference separation, because these data contain important information for analysis and policy evaluation. For example, it will clearly matter for welfare analysis to know whether an individual was unemployed four weeks or 20 weeks (or zero weeks), in addition to knowing whether he or she held a job at interview 1. Moreover, understanding the processes that determine unemployment duration may provide a better guide to policy making. Finally, consideration of unemployment durations enables the researcher to adopt the tools of duration and transition analysis. In the present context, these have two particular advantages. First, duration analysis makes appropriate allowance for unemployment spells that are still in progress (i.e., that are "censored") at the time of a survey, as will be the case with a good part of the COEP sample. Second, duration methods also make it possible to use explanatory variables (such as indicators of local labour market conditions) that may vary during the course of an unemployment spell. Such time-varying covariates may be important controls in a proper assessment of cohort effects — a topic that will be explored below.

Duration analysis is based on notion of "hazard": the theoretical hazard is the probability that a given spell will end within a specified time period, provided it has not ended prior to that period. Here, the Kaplan-Meier empirical hazard for the post-ROE duration is represented graphically for various groups in the sample.[7] For any particular week, this is calculated as the number of spells that end during that week, divided by the size of the total population at risk (termed the "risk set") at the start of the week.[8] Figures 1 to 8 present these empirical hazards, together with 95 percent confidence bands, for a variety of subsamples of the COEP data.

Overall, Figure 1 shows the pooled empirical hazard for both cohorts and for all separation groups. The hazard declines sharply in the first few weeks after the ROE separation and then remains relatively constant to a point beyond 20 weeks. Around 27 or 28 weeks, however, the hazard shows some sign of a rise, followed by a quite sharp decline; it remains at this lower level until somewhere after 40 weeks. In the final few weeks of the sample to date (interview 3 will extend these durations substantially), the empirical hazard begins to rise, although the confidence bands also widen, reflecting the declining sample size.[9] The 27–30 weeks point is close to the date of interview 1, and behaviour here may, to some extent, reflect

7 See, for example, Meyer (1990) for a related discussion of empirical hazards using some U.S. administrative data.

8 If $h(t)$ is the empirical hazard for week t, $f(t)$ the number of failures in week t, $c(t)$ the number of observations that are censored at the start of week t, and $r(t)$ the number of people with spells neither ended nor censored at the start of week t,

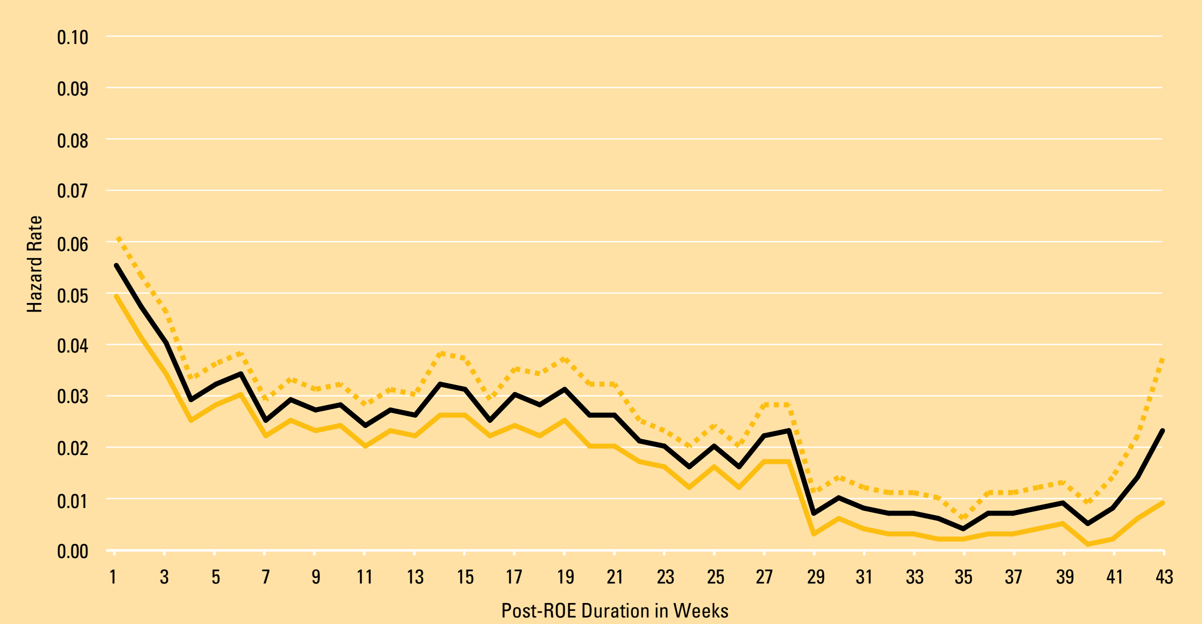
$$h(t) = f(t)/r(t)$$

where the censoring count obeys

$$c(t) = r(t\text{-}1)\text{-}f(t\text{-}1)\text{-}r(t).$$

9 In all of these empirical-hazard graphs, the sample is limited to 43 weeks to avoid the huge error bands at the longest durations forcing an uninformative vertical scale for the hazard rate. The econometric empirical work uses all of the data, however.

Figure 1
Empirical Hazard for Both Cohorts — VQ, SW, OTHER and DISMISSALS — 95% Confidence Bands Shown



non-random attrition, in the sense that the later sample with the lower hazard is disproportionately composed of individuals who did not suffer attrition (as a result, perhaps, of a move into employment).

Figures 2 and 3 provide the breakdown, by cohort, of these empirical hazards and error bands. For cohort 1, the initial decline in the hazard is quite marked, and the rise and fall around week 28 is clear, though the 95 percent confidence bands are naturally wider for this smaller sample. For cohort 2, in contrast, the initial decline over the first few weeks is relatively limited, the hazard never rising above 0.05, and there is a jagged pattern of decline from 18 to 29 weeks. Both cohorts display a sharp rise after 40 weeks, though again this is based on small sample sizes.

For clarity and for purposes of comparison, the two empirical hazards by cohort are graphed together in Figure 4 (but without error bands). The first month after the ROE separation has a much higher hazard for cohort 1 than for cohort 2; with the exception of weeks 5 and 6, this differential is maintained until about week 15. For a short period thereafter, cohort 2 has the higher hazard but the cohort 1 rise around 28 weeks again reverses the ranking. Beyond 30 weeks, there is some alternation in the ranking, with both empirical hazards rising at the end of the sample. Overall, this hazard pattern means that the proportions employed at various points in time and various measures of these durations will tend to favour cohort 1; having the higher hazard in the first 15 weeks or so, when the risk set is large, must

Figure 2
Empirical Hazard for Cohort 1— VQ, SW, OTHER and DISMISSALS — 95% Confidence Bands Shown

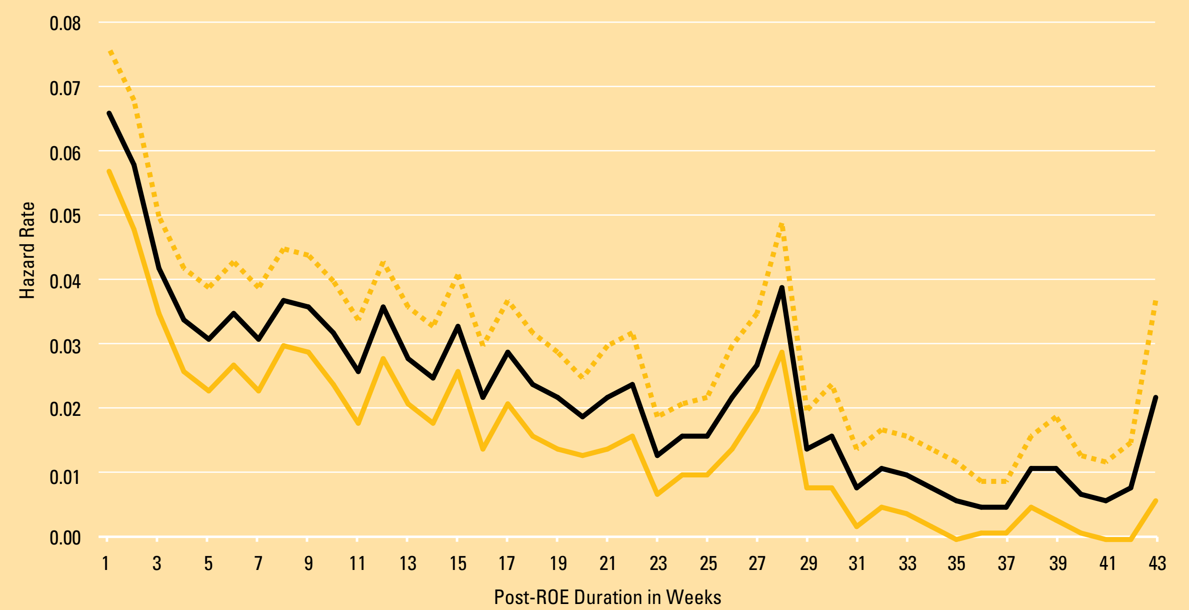


Figure 3
Empirical Hazard for Cohort 2 — VQ, SW, OTHER and DISMISSALS — 95% Confidence Bands Shown

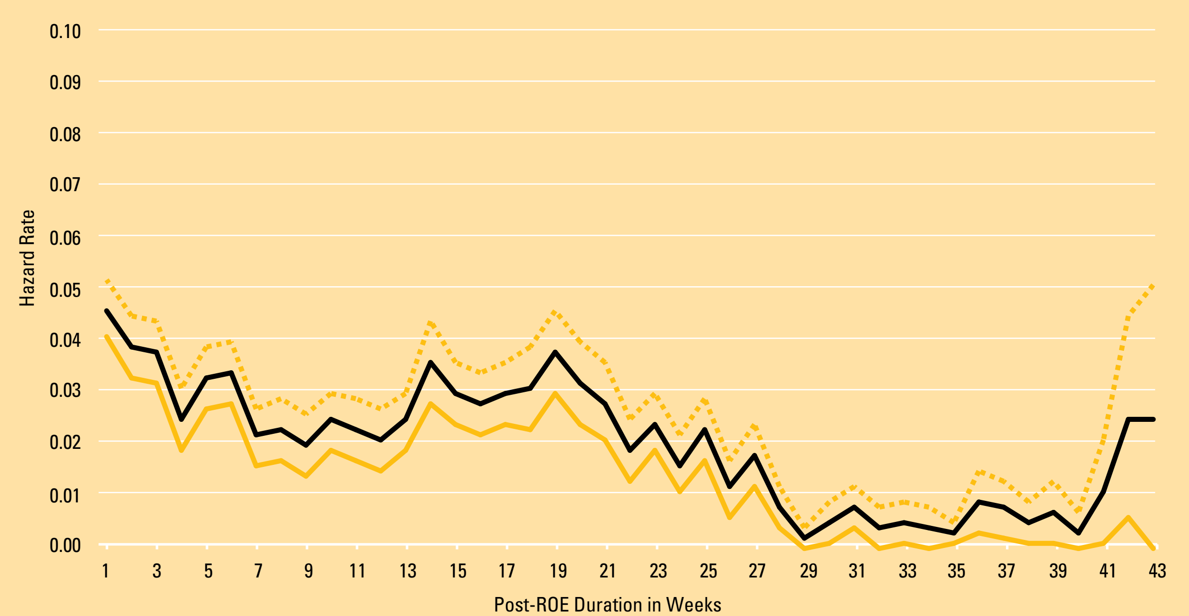
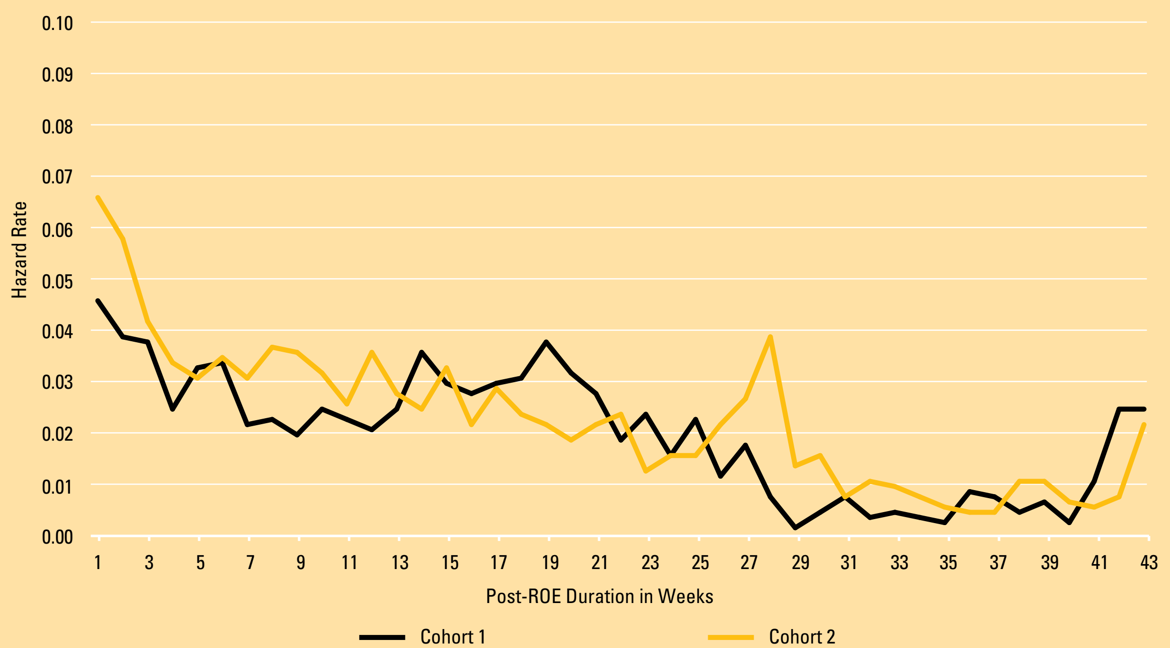


Figure 4
Empirical Hazard for Cohort 1 and 2 — VQ, SW, OTHER and DISMISSALS



outweigh the effects of cohort 2's higher empirical hazard at weeks 18–21.

These empirical hazards were also examined by separation group and graphed for both cohorts in Figures 5 to 8.[10] For the VQ group alone (Figure 5), there is a precipitous decline in the initial period, followed by a relatively constant hazard thereafter. Many VQ individuals experience little or no unemployment and have behaviour that may be quite distinct from other types of job separations. The Dismissals group's empirical hazard (Figure 6) also displays some decline but, beyond that, the sample size does not permit much useful analysis. For SW individuals (Figure 7), there is a broad pattern of decline in the first 10 weeks, although the empirical hazard actually rises initially, and there is relative constancy out to 26 weeks, at which point the hazard peaks and then falls. It remains flat at a lower level until it rises again after 40 weeks. Finally, the Other group (Figure 8) shows some decline in the hazard initially, with rises around 18 and 27 weeks and a flat pattern beyond 28 weeks and no sign of a rise at the end of the sample period. Overall, this breakdown of the empirical hazards suggests that it may be important to investigate distinct treatment of at least some of the separation

10 All of the vertical scales on these separation group figures are consistent to enable comparison across graphs, with the exception of that for Figure 5 for the VQ group, who have a very high initial empirical hazard.

Figure 5
Empirical Hazard for Both Cohorts — VQ Only — 95% Confidence Bands Shown

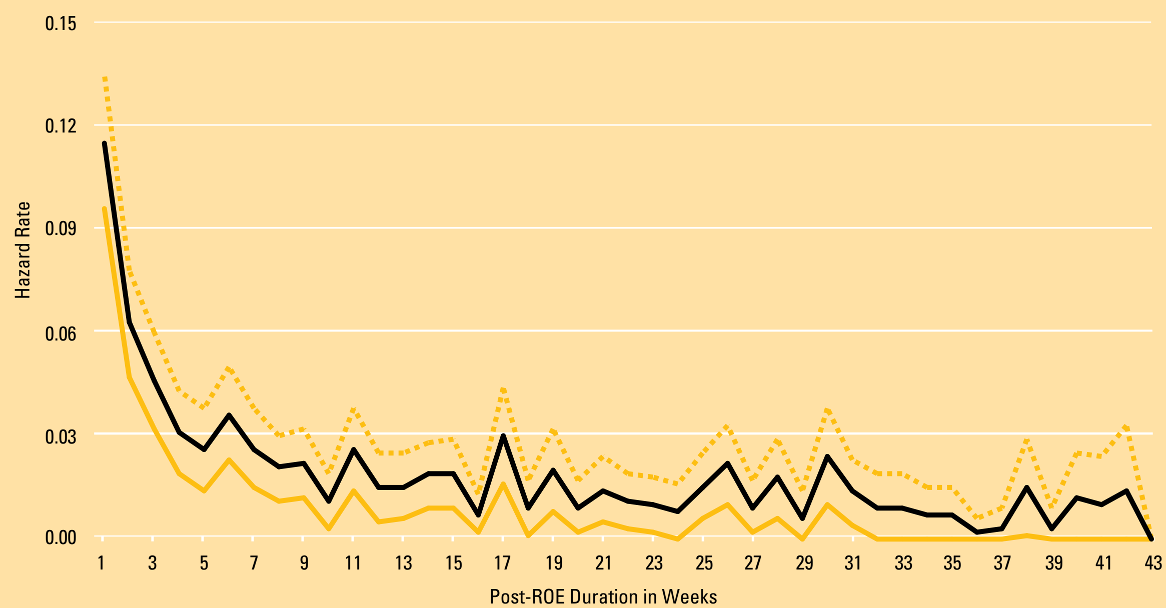


Figure 6
Empirical Hazard for Both Cohorts — DISMISSALS only — 95% Confidence Bands Shown

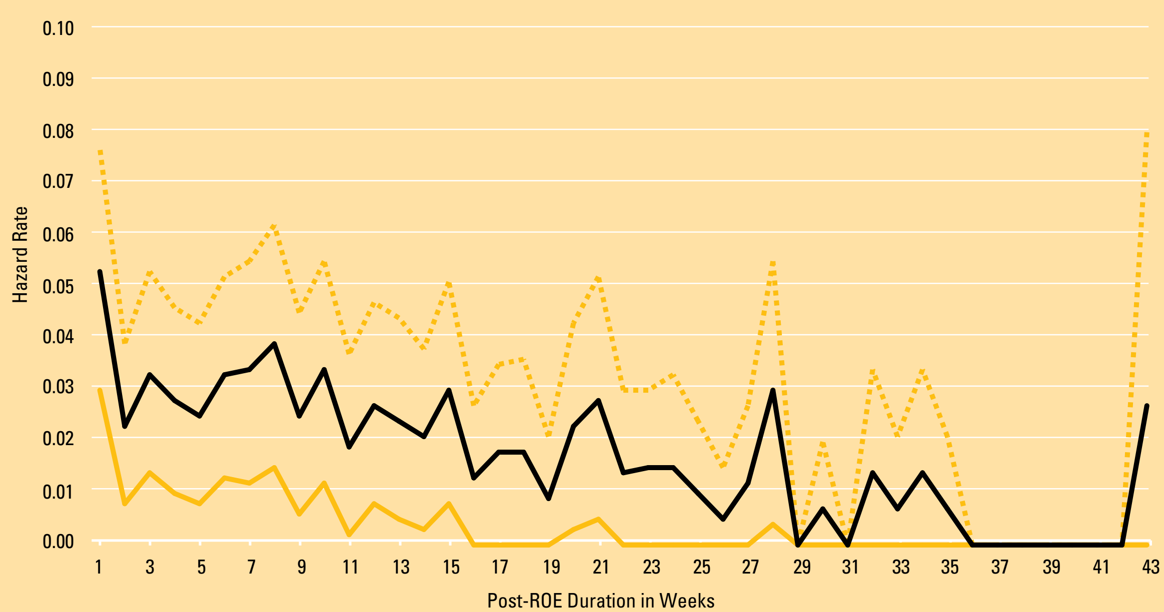


Figure 7
Empirical Hazard for Both Cohorts — SW only — 95% Confidence Bands Shown

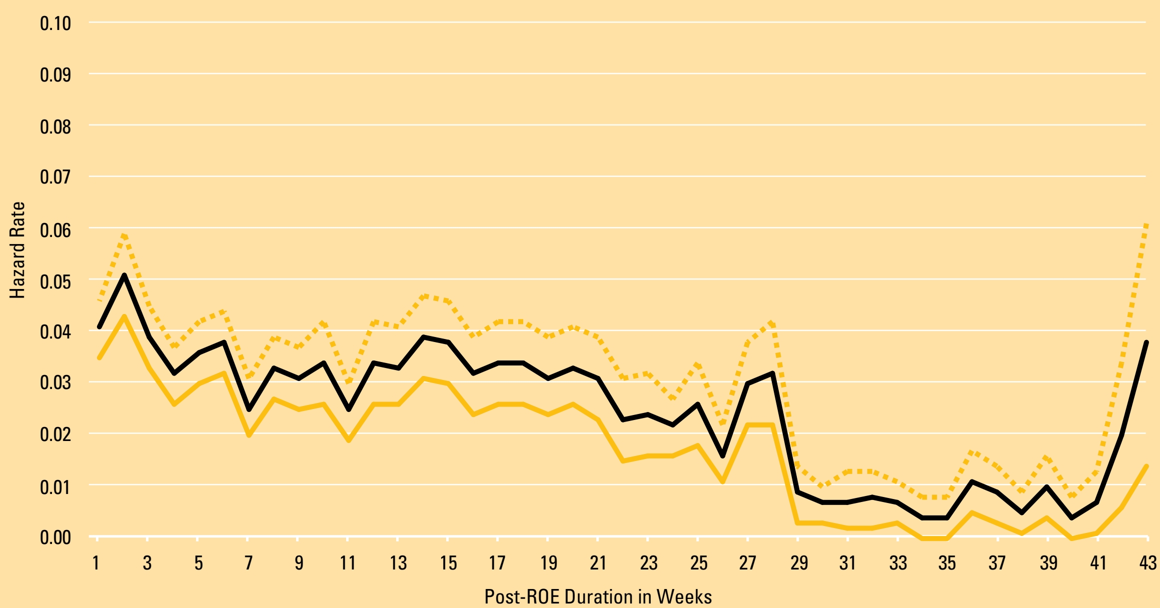
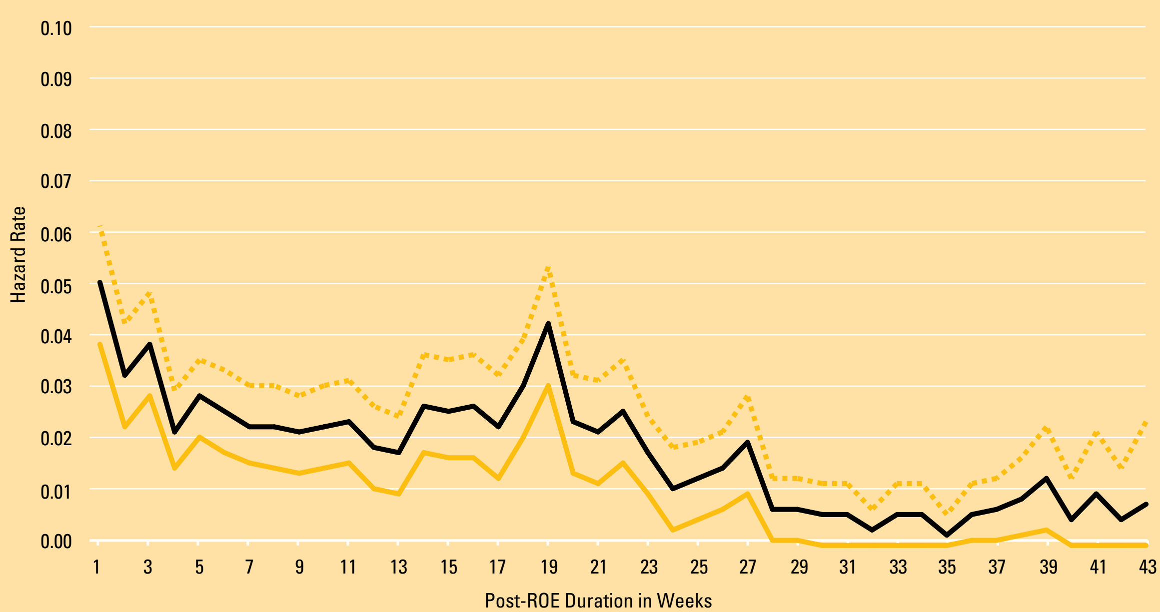


Figure 8
Empirical Hazard for Both Cohorts — OTHER only — 95% Confidence Bands Shown



groups in the econometric duration analysis.

Although these graphs of the Kaplan-Meier empirical hazard are useful guides to analysis, particularly with regard to an assessment of the cohort effect in these data, further analysis is required to assess the robustness of these conclusions. In particular, it should be noted that there is a natural tendency for individuals with good employment prospects (and with observed variables, such as education, that produce such good prospects) to exit the duration sample in the early weeks, leaving a group with systematically poorer job-finding characteristics in the later weeks of the graphs. Accordingly, it is useful to assess whether controlling for this observed heterogeneity in the data (variables such as schooling and region) will alter conclusions about cohort effect. In addition, duration analysis makes it possible to control for changing labour market conditions over the course of a (potentially long) spell — an element that is ignored in a graphical comparison of the two cohorts' empirical hazards. Finally, duration modelling may allow for unobserved heterogeneity, an example being "dynamism" that might be observed by an employer but is not recorded in the data available to the researcher. To the extent that dynamism (or some such factor) is present, the average degree of dynamism in the population remaining unemployed will decline as durations lengthen, producing a bias towards finding a hazard that declines with increasing duration. Moreover, such a bias from neglect of unobserved heterogeneity will also in general contaminate the other coefficient estimates, although the sign of this bias will depend on the specifics of the case in question.

3. *Proportional-Hazards Models of Unemployment Duration*

Duration analysis begins with a particular version of the hazard framework — namely, the proportional-hazards (or partial-likelihood) model developed by Cox (1972). In this approach, the baseline hazard $b(t, 0)$ represents the exit probability at time t when all explanatory variables are set at 0. This baseline is allowed to assume any shape and is factored out of the likelihood equation so that it is not estimated. The effect of explanatory variables is restricted to act proportionally on this baseline, which yields an overall hazard of

$$h(t,X(t)) = b(t,0)e^{X(t)'ß} \qquad \textbf{(1)}$$

where $X(t)$ is a vector of explanatory variables (some or all of which may depend on time) and $ß$ is a vector of coefficients.

A base set of results using this approach is presented in Table A.9, both overall and for the VQ/Dismissals and SW/Other separation groups. With no other controls, the cohort 2 dummy variable has a significant negative sign in each case, and each point estimate would imply a hazard ratio of about 0.9.[11] This effect can also be assessed by including a large number of other controls for demographics, education, province, the full-time and/or unionized status of the ROE reference job, the expected return to the ROE job, previous weeks of UI benefits claimed since 1971 (entered both as a dummy for 0 and linearly thereafter), and separation group (in the full sample). These results, which appear in the second column under the "All," "VQ/Dismissals," and "SW/Other" headings of Table A.9, act to remove the significantly negative cohort 2 effect, although the point estimate of this cohort dummy coefficient remains negative, both overall and for the SW/Other group. Other variables have largely sensible effects, with being male, married, and educated acting to raise the hazard (and hence lower the durations), while minority status, "having had a full-time ROE job," and "having had no previous weeks of UI" all act to lower the hazard.

To allow for changing labour market conditions both between and within cohorts as individual unemployment spells evolve, the CEC (Canada Employment Centre) region for each individual in the dataset was matched to the appropriate regional unemployment rate, which was available in seasonally adjusted form as a three-month average for each of the 62 regions — as used to determine the VER and the length of the regional extended UI benefit periods.[12] These unemployment rates vary each month, and this variation was matched with the ongoing durations in the dataset to generate a set of person-specific time-varying covariates $X(t)$, representing the evolution of unemployment rates in the appropriate CEC region at the appropriate point in the unemployment spell. In addition, the use of benefit-exhaustion dummy variables (only for recipients, based on the benefit period termination date) was assessed, these taking the value 1 in

11 This hazard ratio gives the effect of being in cohort 2 on the hazard, relative to being in cohort 1, and is calculated as *exp*(-0.1) using equation (1).

12 A possible improvement might be to use the actual unemployment rate for each region (rather than the three-month average), as well as seasonally unadjusted data. Results using these data are reported in various notes below.

weeks 1 to 3, 4 to 6, or 7 to 12 after the week of benefit exhaustion. The results of either or both sets of time-varying covariates are given in Table A.10.

In each case, the model was estimated for the whole sample, including dummy variables for the separation group (SW being the excluded group). Column 1 of Table A.10 reports the results, using only the local unemployment rate as a time-varying control (together with the time-invariant controls from Table A.9), while column 2 includes the three benefit-exhaustion dummies and column 3 gives the results when all of these variables are included. In each case, the cohort dummy variable is negative and significant, suggesting that, if anything, the failure to find significance for the cohort variable in Table A.9 may have been the result of *not* controlling appropriately for changing economic conditions across the two cohorts in the months following the ROE separations. In column 1, the local unemployment rate has a negative point estimate, so that as a region's economic conditions worsen, the hazard declines, and unemployment durations tend to lengthen. Clearly, this makes sense. Most of the other explanatory variables have the same effects as in Table A.9 without the time-varying control, although the cohort effect is a clear exception. In column 2, only the "7–12 weeks until expiration" category is significant for the benefit-exhaustion dummy variables (perhaps reflecting the very small number of individuals close to exhaustion in the data so far), and it has a large negative coefficient; the benefit-expiration dummy variables for 1–3 and 4–6 weeks remaining have positive but insignificant coefficients. Finally, the combined model in column 3 of Table A.10 has the usual cohort effect, a negative but insignificant local unemployment rate coefficient, and the same pattern of benefit-expiration variables, while the effect of the other (time-invariant) explanatory variables is largely unchanged from the earlier columns (and from Table A.9).[13]

For assessment of robustness, a similar estimation was also performed with a more limited set of explanatory variables, still including these two sets of time-varying covariates. The results for the cohort effect and the time-varying explanatory variables are given in Table A.11. In each case, the cohort effect remains clearly negative and significant, while the local unemployment variable has a small (but just insignificant) negative effect on the hazard, and the benefit-exhaustion variables go from negative to positive as benefit exhaustion nears, though they often lose statistical significance.[14]

Finally, as a further check on the potential effects of non-random attrition between interviews 1 and 2, these models were re-estimated without and with the time-varying covariates, with the sample being truncated at the first interview. All unemployment spells still in progress after that interview date were thus treated as right-censored at that point, so that the analysis proceeds as if interview 2 had never been conducted. Summary results for the cohort effects are given, using two specifications, in Tables A.12 and A.13. In the simplest model (Table A.12),

13 Using the raw monthly unemployment data for 61 regions (excluding the Northwest Territories and Yukon) as a time-varying control, the three respective cohort effects in Table 10 become -0.070 (0.092), -0.077 (0.094) and -0.077 (0.094), in every case being negative but insignificantly different from zero.

14 Using the monthly unemployment rates as time-varying controls, the matching results to those in Table 11 are -0.03 (0.09) for each case without other controls and -0.06 (0.09) for each case with controls, results that are uniformly negative but insignificant.

the estimated cohort effect is always negative, though it is insignificantly different from zero for the VQ/Dismissals group and is only significant overall and for the SW/Other group without other controls. This matches the results of Table A.9. Allowing for the two sets of time-varying controls in Table A.13 produces results analogous to those in Table A.10, with a negative estimate of the coefficient on the cohort dummy variable of about -0.1. This estimate is almost always significant and robust across specifications.[15]

Overall, the results of this analysis of the Cox partial-likelihood proportional-hazards model are fairly uniform across specifications and sample groups; they suggest the presence of a clear cohort effect that acts to lower the hazard for members of cohort 2. While some models can be found where this effect is insignificant or where the point estimate on the cohort effect itself is even positive, in no case is there a significantly positive effect of the cohort 2 dummy variable on the hazard.

15 Using the monthly unemployment rate by UI region as the time-varying control, the analogous results to Table 13 are always negative and insignificantly different from zero, with values ranging from -0.021 (0.094) to -0.081 (0.096).

4. Parametric Modelling of Unemployment Duration

An alternative framework for modelling unemployment durations in the COEP data is to specify a particular parametric structure for the hazard and then estimate the coefficient vector conditional on this functional form. Some exploration of this approach was carried out, and the results for a Weibull specification are reported here. This distributional assumption permits positive or negative duration dependence (so that the hazard for an individual may rise or fall over the course of a spell), although the hazard must be monotonic since it is given by:

$$h(t) = \lambda p(\lambda t)^{p-1} \qquad \textbf{(2)}$$

which nests the constant hazard exponential as a special case when $p = 1$. As well, the possibility of allowing for unobserved heterogeneity in this estimation was examined, under the assumption that such heterogeneity follows a gamma distribution. In this case, the survivor function, which gives the probability that a spell lasts (survives) at least to a given point in time, is specified as:

$$S(t \mid v) = v e^{(-\lambda t)^{p}}, \qquad \textbf{(3)}$$

where v represents the unobserved heterogeneity component that follows a gamma distribution with density:

$$f(v) = [k^{R}/\Gamma(R)]e^{-kv}v^{R-1}. \qquad \textbf{(4)}$$

The associated hazard is just:

$$h(t) = \lambda p(\lambda t)^{p-1}[1 + ((\lambda t)^{p})/k]^{-k}. \qquad \textbf{(5)}$$

The results of these investigations are presented in Tables A.14, A.15, and A.16 (where θ and σ are the reciprocals of k and p, respectively, in the preceding equations). Many of the results in these models are similar to those in the Cox partial-likelihood estimation (see Table A.9) and will not be discussed in detail here. But it is important to note that the significantly negative coefficient on the cohort 2 variable persists across a variety of these specifications, with and without unobserved heterogeneity and with and without control for past UI receipt.

The estimates on the full sample in Table A.14 show negative and significant cohort effects that are largely unchanged by the inclusion of two variables measuring past UI receipt. These cohort effects rise somewhat when allowance is made in the final two columns for gamma unobserved heterogeneity. In each case, the estimates of σ are significantly greater than unity (which is the value for the benchmark case of an exponential hazard that is constant), implying a declining hazard.[16] The heterogeneity parameter is significantly greater than zero (zero would collapse the model back to the Weibull specification without heterogeneity) in both of the final two columns of the table. Other estimated effects largely parallel those of the proportional-hazards models discussed above, one notable feature here being the large magnitude of many of the estimated coefficients.

16 Note that the test of whether σ is significantly greater than zero is not interesting; the key issue for the type of duration dependence is whether it exceeds or falls short of 1.

Estimates for three separation groups — VQ, SW, and Other — are reported in Tables A.15 and A.16, without and with allowance for unobserved heterogeneity, respectively. In each of the six possible cases, the cohort effect is significant and negative: members of cohort 2 have lower hazards and hence longer durations with this Weibull specification. There is a tendency for the point estimates on this cohort dummy variable to rise in the gamma heterogeneity model, as there was in Table A.14, and the heterogeneity parameter is significantly greater than zero for two of the three groups. (For the Other group, it also has a positive point estimate, but the standard error is large enough to make the value insignificant.) For each group, the duration dependence parameter σ implies a declining hazard as spells lengthen, though this partial effect tends to be smaller when heterogeneity can adjust in Table A.16 for the imposition of a monotonic hazard that is implied by the Weibull specification.

Overall, the results of this parametric estimation of models of unemployment duration strongly support the earlier conclusions regarding the effect of cohort dummy variable. A significantly negative coefficient is found in each case that was examined, reinforcing the finding that the cohort 2 members have a lower hazard, controlling for the other explanatory variables, and hence tend to experience longer unemployment durations.

5. Search Inputs and the Job Search Process

In addition to contributing knowledge on the effect of UI on unemployment durations and re-employment probabilities, the COEP data makes it possible to analyse the process of job search and to understand how it may vary over the course of an unemployment spell. This section summarizes the COEP data on job search and presents some econometric investigation of the possible effects of alternative levels of search inputs on labour market outcomes.

The discussion of search inputs is focused on two main indicators: an hours-per-week measure of the total time spent in looking for work; and an expense measure covering the weekly dollar expenses associated with that search. Table A.17 summarizes these inputs for the two cohorts in the COEP dataset. For all individuals, information is available about the two inputs in the first post-ROE spell of unemployment; for those still unemployed at interview 1, this measure covers the entire period from the ROE date to that interview. In addition, for this latter group of individuals still in the first unemployment spell at interview 2, there is a measure of their job-search hours and expenses between interviews 1 and 2.

The discussion of search inputs is focused on two main indicators: an hours-per-week measure of the total time spent in looking for work; and an expense measure covering the weekly dollar expenses associated with that search.

With respect to the time input, there is no difference between cohorts for the VQ/Dismissals group members in the first spell, while there is a slight decline in cohort 2 for the SW/Other group. Mean levels drop in three of the four cells as one moves to the period between the two interviews, although none of these changes are large or significant. Relative to other studies, the amount of time reported in these COEP data seems large. As for the monetary measure of search inputs, the cohort effect is negative for both the VQ/Dismissals group and the SW/Other group, although again the sample standard deviations are large. Finally, the cohort effect is again negative for the between-interviews period, with quite large differences between cohort 1 and cohort 2 for both separation groupings.

Some additional detail is provided in Table A.18, which includes summary data on hours in the first unemployment spell, broken down by gender and age for each of the VQ/Dismissals and SW/Other groups and for each of the two COEP cohorts. The male inputs are somewhat higher than the female, and for each the VQ/Dismissals inputs are higher than those for the SW/Other group, but again these differences are not large, on average. By age, there are few differences in the mean levels of search time.

Table A.19 presents similar breakdowns for monetary search inputs in the first unemployment spell. For both VQ/Dismissals and SW/Other, the female figures display a mean decline in cohort 2 relative to cohort 1; in the male group, this holds true only for the SW/Other group. Also, men tend to incur more expenses in looking for work, especially those in the SW/Other group. By age, the prime-age group usually has the highest monetary search inputs and, in this group (though not for the young or the old), there is a decline for both VQ/Dismissals and SW/Other as one moves from cohort 1 to cohort 2.

Detailed information on the use of search time and money according to the methods employed is also available for a small subsample of the overall COEP dataset (a group that was randomly selected to answer the long questionnaire). Summary information from these questions is contained in Table A.20, where each cell reports the proportion of the sample that

1) Used a particular method in looking for work; and,

2) Contacted an employer by use of that method.

The methods summarized are: to ask friends, to answer ads, to make direct application to employers, and other methods (excluding application to a provincial employment agency). There are some differences in the use of methods and in their apparent success rates, at least measured by employer contacts (which may amount to an unsuccessful interview, of course), but there is no clear indication of any systematic difference by cohort.

A further aspect of the search process — one that is often unobserved — is the reservation wage of the individual job searcher.

A further aspect of the search process — one that is often unobserved (or thought to be unobservable) — is the reservation wage of the individual job searcher. In the COEP survey, respondents were asked in interview 1 about the reservation wage both at the time of the ROE separation and at the time of that interview[17]; the same question was asked of those who remained unemployed at interview 2. The reservation wage was then compared with the wage on the ROE reference job, all wages being converted to an hourly basis. This fraction is termed the reservation wage ratio (RWR).

Table A.21 presents a summary of the RWRs reported in the COEP data for those three points in time, separated by cohort and by separation-reason group. All RWRs have a mean value in excess of 1 at the ROE date, with those for the VQ/Dismissals group being about 10 percent higher than those for the SW/Other members (whose RWR is very close to 1), and neither displays a cohort effect. This pattern largely holds at interview 1; by interview 2, however, the members of the VQ/Dismissals group have higher RWRs, as do the SW/Other group in cohort 1. Although one might expect individual reservation wages to decline as the unemployment spell lengthens, people with higher reservation wages may tend to remain unemployed longer. The latter effect is probably dominant by interview 2.

By gender, these RWRs display some interesting patterns (Table A.22). At the ROE date, female ratios are lower than male ratios within each cohort and within each VQ/Dismissals and SW/Other category; this differential holds up at the time of interview 1 as well. By interview 2, however, the female VQ/Dismissals group has a higher RWR than does the corresponding male group; sample sizes here are rather small, however. The results display no evident differentiation between cohorts.

The COEP data also contain some information on expected wages in a new job,[18] evaluated at the date of each interview. The figures are naturally higher than the corresponding reservation wage values (since the expected wage is treated as

17 For the current level of reservation wages, the wording of the question was as follows:

> Many people feel that the level of wages is very important in considering a new job opening. If a similar job to the job you left on [ROE date] were offered today, what is the lowest take-home pay you would accept?

18 The wording was:

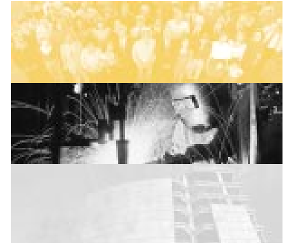
> What is the take-home pay you expect to be earning in your next job?

conditional on acceptance of the job — that is, on the wage being no less than the reservation value). Table A.23 provides a summary of the average responses, expressed as a fraction of the individual's ROE reference-job hourly wage. The VQ/Dismissals group have higher mean values than the SW/Other group at interview 1, the former expecting a 23 percent gain on the ROE wage and the latter, a 10 percent gain. Neither group displays a cohort effect. At interview 2, the VQ/Dismissals group in cohort 1 has moderated the mean expected gain to 16 percent (the same figure as the SW/Other group for this cohort at this time), but the VQ/Dismissals cohort 2 group now expects a 27 percent gain. These figures are based on small sample sizes, however.

Finally, a variety of econometric investigations were performed to assess the explanatory power of these search variables for unemployment durations. Typical results are summarized in Table A.24, where the results of Cox partial-likelihood proportional-hazard estimation are presented. In addition to a set of time-invariant controls (similar to those in Table A.9) and to the time-varying covariates (CEC region unemployment rates and benefit-expiration dummy variables) employed earlier in the analysis, three of the search variables were entered as time-varying explanatory variables in this Cox specification — i.e., the RWR and the measures of job-search hours and expenses. Of course, as the preceding summary tables made clear, these variables are only observed at one, two, or three points — rather than continuously over the course of the unemployment spell — so that the analysis proceeds on the assumption that these variables follow step functions (changing at interview 1 and interview 2, as appropriate).

The results in Table A.24 with or without other controls and with or without the earlier time-varying covariates lend only very weak support to these search measures as determinants of duration. The RWR variable always has a positive coefficient but is insignificantly different from zero, while the measure of money spent on job search has small coefficients that are always positive but only significant in one case. The time input variable displays no consistent pattern in its results. Finally, note that, although disappointing as determinants of unemployment duration, the inclusion of these search measures does not alter the nature of the cohort variable point estimates, which remain uniformly negative, although in this case the estimates are not statistically significant.[19]

19 If the monthly unemployment rate is used as a time-varying control, rather than the three-month averages, the cohort effects in Table A.24 remain insignificantly different from zero, although the point estimates become small and positive in each case.



6. New Job Quality: Wages

In addition to examining unemployment durations in the assessment of the effects of the UI program, it is important to consider the effects that UI (and changes in UI) may have on various measures of the quality of the new job found after an unemployment spell. This study begins by comparing the hourly wage received in the first job after the ROE reference separation to the wage in the ROE job itself. The mean values in the COEP data are given in Table A.25, broken down in the usual way by demographic groups, ROE reason for separation, and the UI cohort in question.

It is important to consider the effects that UI (and changes in UI) may have on various measures of the quality of the new job found after an unemployment spell.

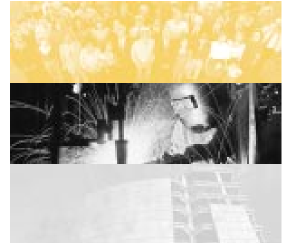
The log wage differential is measured as the difference between the logarithm of the ROE wage and that of the first wage, so that a negative number indicates an increase in wages and a positive number indicates a decline. The overall pattern reveals increases for the VQ/Dismissals group in both cohorts and for the SW/Other group a slight decline for cohort 1 and a slight increase for cohort 2. When all separation groups are taken together, there is a slight increase for members of cohort 1 and a larger increase for those of cohort 2. By gender, this pattern repeats itself, but the largest log wage drop is for cohort 1 men in the SW/Other group. Decomposed by age, SW/Other prime-age men in cohort 1 take the largest loss — about twice the size of the loss for all men in this cohort/separation group cell. Overall, there is usually some indication that a cohort 2 group does better than the corresponding cohort 1 group, but the differences are not large. However, this could be interpreted as compensating the members of this cohort for the longer unemployment durations found in the earlier sections.

These average log wage losses or gains are also reported for the status — full-time (FT) or part-time (PT) — of the two jobs involved. (The wages themselves are measured on an hourly basis in both cases.) For FT to FT job changes, the VQ/Dismissals group has a tiny gain in cohort 1 and a tiny decline in cohort 2; the SW/Other group records the reverse mean change, with a drop for cohort 1 and a small advance for cohort 2. For PT to PT changes, all four cohort/separation group cells register a log wage gain, though the samples involved are small. There are more moves from FT to PT than from PT to FT, especially among the SW/Other group, but interestingly the FT to PT movers have log wage gains for each cohort and for both separation groups, while the PT to FT movers in the SW/Other group take a substantial hourly wage loss.

Finally, with regard to these means, the average log wage drop is reported for each of the four separation groups in Table A.27, since clearly there could be a difference between VQs and those dismissed from the ROE job. It turns out that the VQ group records a log wage gain that is slightly larger in cohort 2, while the dismissal group have a loss in cohort 1 and a gain in cohort 2. The SW group alone has a drop in cohort 1 that turns into a mean gain by cohort 2, while the Other group has a log wage drop for both cohorts.

The next step is to estimate the determinants of these log wages and of the log wage drop between the ROE job and the first post-ROE job. A relatively standard wage equation has demographic controls (including a large set of educational dummy variables), controls for the ROE job tenure and for union status (ROE or first post-ROE job), and regional identifiers (Table A.28). While many of the results here are standard — such as the significant quadratic in age, the strong role of the educational variables (where "high school completed" is the omitted category) and the mostly significant provincial dummy variables (Ontario being omitted) — the main focus is on the coefficient of the cohort 2 dummy variable. In both the ROE and the first post-ROE job wage equations, this coefficient is insignificantly different from zero, suggesting that when these other variables are controlled for, being in one or the other cohort does not materially affect the log wage. Most importantly, the coefficient on the cohort 2 dummy variables displays similar insignificance in the log wage drop equation in the last column of Table A.28. This suggests that there is no strong cohort effect on the wage change from the ROE job to the first post-ROE job, once the other factors are controlled for.[20]

20 A sample selection model of the determination of this wage drop was also estimated, controlling for the fact that the group with re-employment wages is not a random sample of the population as a whole. Using dummy variables for the presence of children, the spouse being employed and a measure of UI eligibility as instruments in the first stage probit, overall, for the VQ/Dismissals group and for the SW/Other group, the estimated coefficient on the cohort dummy variable in the second stage equation is never significantly different from zero.



7. New Job Quality: Hours Worked

Some investigation of the hours reported in the ROE job and the first post-ROE job has also been done as another means of assessing job quality. If hours and wages are bundled together as a package by an employer, the hours worked by the individual may be a useful supplemental indicator of overall quality of the new job, relative to the reference ROE job. Results analogous to those for log wages are presented in the various tables, although the discussion will be more brief.

If hours and wages are bundled together as a package by an employer, the hours worked by the individual may be a useful supplemental indicator of overall quality of the new job, relative to the reference ROE job.

Table A.30 provides mean values for hours worked, in the ROE job and the first post-ROE job, by separation group and by cohort. The cohort 2 sample has slightly lower ROE job hours for both the VQ/Dismissals and the SW/Other groupings; this pattern persists in the first post-ROE job, the VQ/Dismissals group being almost two hours lower in cohort 2 and the SW/Others being an hour and a half lower. The average hourly drop from the ROE job to the first job is given in Table A.31. Overall, both the VQ/Dismissals and the SW/Other groups have a decline that is clearly larger for cohort 2 than for cohort 1. The change for those going from FT to FT job is small but is always a drop — and is always larger for the cohort 2 members — while the change for the PT to PT groups is larger and is again higher for cohort 2. The FT to PT and PT to FT changes are naturally very large in magnitude but are based on small samples.

Finally, a simple model of the determination of changes in hours worked was investigated, using the same explanatory variables as for the log wage drop equations reported in Table A.28. The cohort dummy variable coefficients are recorded in Table A.32, with and without these controls, separately for the two VQ/Dismissals and SW/Other groups. Overall, the cohort variable has a significantly positive effect on the hours drop (so that being in cohort 2 raises the drop in hours from the ROE job to the first post-ROE job) when no other controls are used, but this effect becomes small and insignificantly different from 0, depending on the other controls. Neither estimated effect is significant for the VQ/Dismissals group, though both point estimates are positive, while the SW/Other group has a significantly positive effect without controls that becomes just negative and clearly insignificant when the controls are also added to the specification. Allowing for the other factors, then, the conclusion is that there is no clear and significant evidence of a cohort effect on changes in hours worked, although there is some indication of a decline as workers move from the ROE job to the first post-ROE job.

8. New Job Quality: Measures of Job Satisfaction

... very small percentages of each group report a subjective decline in overall job satisfaction.

The study also included an examination of a broader measure of job satisfaction associated with individuals' own subjective evaluations of post-ROE jobs, relative to the reference job itself. The question was worded as follows:

> Compared to the job that ended on ROE date, how satisfied are you in your first/current job? Please rate your answer on a 7-point scale where 1 means you are much less satisfied in your current job, 7 means you are much more satisfied and the midpoint 4 means about the same.

Since responses to this type of question are probably much more reliable for the *ranking* of answers ("7" is better than "5") than for their relative *magnitude* (is "7" better than "5" more or less than "4" is better than "2"?), an ordered probit model of the determinants of this ordinal variable was estimated. This approach estimates a score as a linear function of a set of control variables, together with a set of cutoff values that divide the scores into the ordinal variable recorded in the "1" to "7" answers.

The sample responses and some estimation results are presented in Tables A.33 and A.34 for the first job and the current (interview 1) job respectively. In Table A.33, the sample responses are strikingly grouped at the higher end of the subjective range for evaluation, with 26 percent overall and fully 41 percent of the VQ/Dismissals group reporting an evaluation in the maximum possible group. For the SW/Other group, this figure is still 22 percent. On the down side, very small percentages of each group report a subjective decline in overall job satisfaction. For the SW/Other individuals, for example, only 15 percent report a figure below 4 in response to the above question. In Table A.34, the results are quite similar, with slightly higher proportions in the top group in each case.

The results of estimating this ordered probit with the cohort dummy variable as the only control are given in the next line in the two tables. In each of the six cases studied, the point estimated on the cohort 2 dummy is negative; it is significantly so for five of the six cases, the exception being the SW/Other group for the first job evaluation in the final column of Table A.33. With control variables in addition to the cohort dummy variable, the negative estimated coefficient remains in each case; it is significantly different from zero for both the first and the current job when the two separation categories are pooled. For the first job, the estimates for the VQ/Dismissals and the SW/Other groups are insignificant as the standard errors rise with a smaller overall sample size, though for the current job the cohort coefficient remains significantly negative for each of these two groups.

To compare these results with those for the log wage change, and to evaluate further the role of the four distinct separation categories, this ordered probit model of total job satisfaction in the first post-ROE job was also estimated for each of the four groups (VQ, Dismissals, SW, and Other). These results for the cohort variable, together with those from the corresponding estimation of a log wage drop equation, are reported in Table A.29. All of the ordered probits have negative point estimates of the coefficient on the cohort dummy variable, although none of them are individually significant. In the log wage drop equations, all groups but Dismissals record a negative estimate, though again the standard errors are too large for significant conclusions to be drawn.

Finally, a related measure of job satisfaction that may usefully supplement the ordinal variable analysed above is whether or not an individual reports that he or she is “still looking” around for another job, having found a first post-ROE job. Presumably, individuals who find a match that is much better than their ROE reference job are less likely to keep searching for employment than those who feel that their new job is a step down relative to their past job. Table A.35 gives the proportions who report “still looking” by cohort and for each of the four separation groups; it also provides a breakdown for those who came from a FT ROE job and have moved to FT or PT status in subsequent employment. Overall, the proportion still looking rises from cohort 1 to cohort 2 for VQs, Dismissals, and SWs, though not for the Other separation category. For the FT to FT moves, this positive cohort effect again holds for these three groups although the sample sizes are too small for the FT to PT moves to say much about such effects. Not surprisingly, the FT to PT group has a much higher proportion of people who are still looking. Table A.36 presents estimates from a binary probit on this “still looking” variable, without and with controls. It shows that being in cohort 2 raises the probability of being “still looking” for the whole sample, for the VQs and for the SWs without controls, but these effects become insignificant (though usually retaining a positive point estimate) when the other control variables are added to the probit specification.

A related measure of job satisfaction that may usefully supplement the ordinal variable analysed above is whether or not an individual reports that he or she is “still looking” around for another job, having found a first post-ROE job.

9. Conclusion

The "Before" group, which has benefited from the more generous UI provisions, typically performs somewhat better than the "After" group, which must cope with a lower wage replacement rate and the complete disqualification of workers with "unjustified separations."

The conclusions for unemployment that emerge from this study of UI using the COEP data can be summarized quite concisely. When the unemployment experiences of the two cohorts are compared, members of cohort 1 usually do better than those of cohort 2. This conclusion holds for the unconditional empirical hazards, for the proportions of the sample employed at each interview date, and for a variety of econometric and statistical specifications that seek to control for the influence of other variables that may differ between cohorts or that may vary over the course of the unemployment spells. Thus the "Before" group, which has benefited from the more generous UI provisions, typically performs somewhat better than the "After" group, which must cope with a lower wage replacement rate and the complete disqualification of workers with "unjustified separations."

Exactly how these results on unemployment experiences should be interpreted is less clear, however. Corak's recent survey (1993) of the effects of UI on the Canadian labour market concludes that the past Canadian microeconometric studies have found that benefit rates have little effect on the duration of UI claim periods, especially for men, although some effect (from greater generosity to longer UI durations) may be present for women claimants. One view of the present results is that they agree with the weak behavioral effects found previously, the addition here being that these results are derived from a dataset with an exogenous benefit rate change that may lend greater credibility to the conclusions. One could even go beyond this and claim that these results suggest some sort of general-equilibrium effect from UI benefits to unemployment durations, as suggested in the abstract model of Albrecht and Axell (1984); in this case, the more generous UI program before the change could indeed have been associated with shorter durations. Alternatively, one might conclude from the present results that the controls for seasonality or for secular change between the two cohorts and during unemployment spells may be deficient or that the sample size or the magnitude of the benefit rate change are not large enough to pin down a subtle overall effect. While further investigation is clearly warranted, the present investigation must conclude that there is no evidence that the April 1993 change in UI generosity reduced unemployment durations.

With regard to the other two aspects of labour market behaviour addressed in this section of the report — job search and new-job quality — there is little evidence that reported search inputs vary systematically by cohort, although such inputs (hours and expenses) do vary substantially across individuals in the sample. Self-reported reservation wages at various points in the unemployment spell also display no significant cohort effect; interestingly, controlling for these time-varying search inputs in the duration analysis does not reverse the conclusions on unemployment experiences by cohort. Second, there are various measures of new-job quality but neither of the objective measures (wages and hours worked) displays a significant difference across the two cohorts, once relevant control variables are introduced. The subjective measure of overall job satisfaction does exhibit some cohort effects when analysed appropriately as an ordinal measure, with members of cohort 2 tending to be less satisfied with their post-unemployment job than members of cohort 1. Although there are reasons to weigh carefully the importance attached to this measure — in particular, the unusual distribution of rankings that has so few workers reporting a decline in satisfaction, relative to the reference ROE job — these results may strengthen the cohort effect found for unemployment, with the members of cohort 2 being prepared to take less desirable jobs more quickly than their counterparts in cohort 1.

Appendix A: Tables

Table A.1
Cohort Definitions and Counts (unweighted)

	Cohort based on Replacement Ratio		
	Cohort 1	Cohort 2	Total
Cohort from COEP			
Cohort 1	4,845	620	5,465
Cohort 2	13	5,681	5,694
Total	4,858	6,301	11,159
Female			
1	2,109	301	
2	4	2,838	
Male			
1	2,722	319	
2	9	2,828	
Separation reason			
VQ			
1	1,022	78	
2	3	942	
Dismissals			
1	262	27	
2	0	234	
SW			
1	2,626	370	
2	8	3,070	
Other			
1	935	145	
2	2	1,435	

Table A.2
Cohort Definitions, by ROE Week (unweighted)

	Benefit Cohort (defined by replacement ratio)	
By ROE (key weeks)	Cohort 1	Cohort 2
1727		
1	735	57
2	0	2
1728		
1	677	57
2	0	0
1729		
1	750	68
2	0	1
1730		
1	893	106
2	0	0
1731		
1	859	119
2	0	0
1732		
1	836	185
2	0	1
ROE week		
1739		
1	6	3
2	5	1,337
1740		
1	1	2
2	2	787
1741		
1	0	0
2	2	856
1742		
1	1	0
2	1	738
1743		
1	0	2
2	1	932
1744		
1	1	2
2	2	940

Notes: ROE weeks begin on January 1, 1960 so that week 1727, for example, is the week beginning January 31, 1993. Rows within each week give the COEP cohort variable, while columns give the benefit-based cohort variable.

Table A.3
Proportions Employed at Interview 1

All	Cohort 1	Cohort 2
VQ/Dismissals	0.309	0.213
	(0.462)	(0.410)
	1,287	1,281
SW/Other	0.404	0.331
	(0.491)	(0.471)
	3,571	5,020
Females		
VQ/Dismissals	0.313	0.166
	(0.464)	(0.373)
	685	663
SW/Other	0.353	0.318
	(0.478)	(0.466)
	1,428	2,476
Males		
VQ/Dismissals	0.304	0.253
	(0.460)	(0.435)
	599	613
SW/Other	0.437	0.341
	(0.496)	(0.474)
	2,132	2,534
Young (≤25)		
VQ/Dismissals	0.349	0.260
	(0.477)	(0.439)
	377	439
SW/Other	0.431	0.348
	(0.496)	(0.477)
	533	729
Prime Age (>25, ≤45)		
VQ/Dismissals	0.307	0.184
	(0.462)	(0.387)
	769	684
SW/Other	0.418	0.346
	(0.493)	(0.476)
	2,225	3,062
Older (>45)		
VQ/Dismissals	0.164	0.162
	(0.371)	(0.369)
	141	158
SW/Other	0.342	0.278
	(0.475)	(0.448)
	813	1,229

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.4
Proportions Employed at Interview 2

All	Cohort 1	Cohort 2
VQ/Dismissals	0.366	0.277
	(0.482)	(0.448)
	1,018	1,034
SW/Other	0.462	0.405
	(0.499)	(0.491)
	2,866	4,044
Females		
VQ/Dismissals	0.367	0.258
	(0.483)	(0.438)
	555	547
SW/Other	0.441	0.432
	(0.497)	(0.496)
	1,155	2,052
Males		
VQ/Dismissals	0.366	0.292
	(0.482)	(0.455)
	462	483
SW/Other	0.476	0.386
	(0.500)	(0.487)
	1,703	1,986
Young (≤25)		
VQ/Dismissals	0.399	0.303
	(0.490)	(0.460)
	290	345
SW/Other	0.496	0.408
	(0.501)	(0.492)
	412	570
Prime Age (>25,≤45)		
VQ/Dismissals	0.372	0.269
	(0.483)	(0.444)
	614	573
SW/Other	0.474	0.422
	(0.499)	(0.494)
	1,805	2,499
Older (>45)		
VQ/Dismissals	0.214	0.207
	(0.412)	(0.407)
	114	116
SW/Other	0.401	0.358
	(0.490)	(0.480)
	649	975

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.5
Determinants of the Probability of Employment at Interview 1

	All		VQ/Dismissals		SW/Other	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 2	-0.218* (0.025)	-0.138* (0.038)	-0.296* (0.054)	-0.104 (0.090)	-0.193* (0.028)	-0.158* (0.042)
Age		0.004 (0.012)		-0.045 (0.031)		0.022 (0.013)
Age^2		-0.000 (000)		0.000 (0.000)		-0.000* (000)
Visible minority		-0.276* (0.055)		-0.334* (129)		-0.247* (062)
Married		0.089 (0.049)		0.209 (117)		0.060 (054)
Widowed, separated, divorced		-0.073 (0.072)		-0.137 (0.178)		-0.042 (0.080)
Elementary school		-0.025 (0.100)		0.287 (0.774)		-0.059 (0.107)
Some high school		-0.047 (0.052)		-0.216 (129)		-0.018 (0.058)
Trade college		0.133 (0.080)		0.112 (0.200)		0.123 (0.088)
Some college		0.121 (0.069)		0.262 (0.156)		0.090 (0.078)
College		0.174* (0.068)		0.279 (0.149)		0.132 (0.077)
Some university		0.182* (0.089)		0.326 (0.187)		0.167 (0.103)
Undergraduate		0.181* (0.081)		0.395* (0.188)		0.141 (0.089)
Professional certification		0.090 (0.174)		-0.143 (0.461)		0.143 (0.191)
Postgraduate		0.288* (0.133)		-0.849 (0.556)		0.424* (0.141)
ROE job tenure		0.000 (0.000)		0.000 (0.000)		0.000 (0.000)
ROE job union		0.197* (0.042)		-0.283* (0.135)		0.248* (045)

Table A.5 *(continued)*
Determinants of the Probability of Employment at Interview 1

	All		VQ/Dismissals		SW/Other	
Newfoundland		-0.206		-10.359*		-0.122
		(0.139)		(0.602)		(0.145)
Prince Edward Island		0.070		0.282		0.077
		(0.201)		(10.062)		(0.203)
Nova Scotia		-0.008		-0.219		0.017
		(0.100)		(0.249)		(0.109)
New Brunswick		0.228*		0.405		0.149
		(0.113)		(0.254)		(0.128)
Quebec		0.041		-0.018		0.048
		(0.051)		(0.127)		(0.057)
Manitoba		0.034		-0.281		0.152
		(0.118)		(0.254)		(0.136)
Saskatchewan		0.057		0.051		0.071
		(0.108)		(0.251)		(0.120)
Alberta		0.020		-0.035		0.048
		(0.067)		(0.149)		(075)
British Columbia		0.073		0.133		0.049
		(0.063)		(0.136)		(0.072)
Northwest Territories and Yukon		-0.048		-0.093		0.023
		(0.771)		(0.872)		(0.423)
Constant	-0.309*	-0.153	-0.500*	0.801	-0.243*	-0.513*
	(018)	(0.211)	(0.039)	(0.511)	(0.020)	(267)
N	11,159	4,712	2,568	889	8,591	3,823

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
Columns 1, 3 and 5 are specific to cohort 2 only. Columns 2, 4 and 6 include demographic and labour market controls.

Table A.6
Cohort Effects on the Probability of Employment at Interview 1, by Gender

	VQ/Dismissals		SW/Other	
	Female	Male	Female	Male
No other controls				
Cohort	-0.481*	-0.154*	-0.096*	-0.250*
	(0.076)	(0.078)	(0.042)	(0.037)
With other controls				
Cohort	-0.337*	0.143	-0.107	-0.157*
	(0.128)	(0.143)	(0.063)	(0.058)

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The other controls are as in Table A.5.

Table A.7
Determinants of the Probability of Employment at Interview 2

	All		VQ/Dismissals		SW/Other	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 2	-0.170* (0.027)	-0.097* (0.042)	-0.253* (0.058)	-0.047 (0.102)	-0.141* (0.030)	-0.128* (0.046)
Age		0.010 (0.013)		-0.070* (0.035)		0.034* (0.015)
Age^2		-0.000 (0.000)		0.001 (0.000)		-0.000* (0.000)
Visible minority		-0.235* (0.062)		-0.324* (0.147)		-0.217* (0.069)
Married		-0.067 (0.054)		0.119 (0.132)		-0.042 (0.061)
Widowed, separated, divorced		-0.081 (0.080)		-0.209 (0.208)		-0.029 (0.088)
Elementary school		0.061 (0.112)		-0.561 (0.583)		0.089 (0.115)
Some high school		-0.017 (0.059)		-0.392* (0.151)		0.066 (0.065)
Trade college		-0.002 (0.089)		0.174 (0.218)		-0.034 (0.097)
Some college		0.272* (0.076)		0.544* (0.179)		0.236* (0.084)
College		0.337* (0.075)		0.398* (0.164)		0.321* (0.086)
Some university		0.171 (0.098)		0.257 (0.209)		0.145 (0.113)
Undergraduate		0.316* (0.088)		0.335 (0.205)		0.340* (0.098)
Professional certification		0.264 (0.184)		-0.291 (0.567)		0.398 (0.203)
Postgraduate		0.349* (0.141)		-0.529 (0.505)		0.469* (0.150)
ROE job tenure		0.000 (0.000)		0.001 (0.000)		-0.000 (0.000)
ROE job union		0.026 (0.047)		-0.334* (0.154)		0.055 (0.050)

Table A.7 *(continued)*
Determinants of the Probability of Employment at Interview 2

	All		VQ/Dismissals		SW/Other	
Newfoundland		-0.266 (0.147)		0.403 (0.482)		-0.383* (0.157)
Prince Edward Island		0.285 (0.243)		— —		0.306 (0.243)
Nova Scotia		0.313* (0.109)		-0.051 (0.277)		0.367* (0.120)
New Brunswick		0.022 (0.124)		-0.642 (0.286)		0.196 (0.141)
Quebec		0.061 (0.056)		-0.272 (0.147)		0.112 (0.061)
Manitoba		0.091 (0.133)		-0.129 (0.281)		0.212 (0.154)
Saskatchewan		0.193 (0.121)		0.105 (0.276)		0.240 (0.137)
Alberta		0.048 (0.073)		-0.159 (0.164)		0.115 (0.083)
British Columbia		0.073 (0.071)		-0.139 (0.162)		0.115 (0.081)
Northwest Territories and Yukon		0.005 (0.396)		-0.569 (0.879)		0.184 (0.457)
Constant	-0.159 (0.020)	-0.186 (0.238)	-0.341* (0.043)	10.422* (0.588)	-0.096* (0.022)	-0.656* (0.277)
N	8,943	3,786	2,047	697	6,896	3,088

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
Columns 1, 3 and 5 are specific to cohort 2 only. Columns 2, 4 and 6 include demographic and labour market controls.

Table A.8
Cohort Effects on the Probability of Employment at Interview 2, by Gender

	VQ/Dismissals		SW/Other	
	Female	Male	Female	Male
No other controls				
Cohort	-0.311* (0.079)	-0.205* (0.086)	-0.021 (0.044)	-0.229* (0.041)
With other controls				
Cohort	-0.154 (0.143)	0.125 (0.164)	-0.071 (0.068)	-0.158* (0.065)

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The other controls are as in Table A.7.

Table A.9
Cox Proportional-Hazards Estimates of the Determinants of Unemployment Duration

	All		VQ/Dismissals		SW/Other	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort 2	-0.102*	-0.045	-0.125	0.008	-0.097*	-0.059
	(0.034)	(0.035)	(0.078)	(0.082)	(0.038)	(0.039)
Male		0.239*		0.323*		0.202*
		(0.038)		(0.085)		(0.042)
ROE job FT		-0.223*		-0.229*		-0.183*
		(0.050)		(0.102)		(0.059)
Married		0.184*		0.309*		0.169*
		(0.040)		(0.097)		(0.045)
Age		0.013		-0.060*		0.032*
		(0.011)		(0.029)		(0.013)
Age^2		-0.000*		0.000		-0.001*
		(0.000)		(0.000)		(0.000)
Visible minority		-0.369*		-0.339*		-0.376*
		(0.054)		(0.120)		(0.061)
Elementary		-0.178		-0.129		0.220*
		(0.096)		(0.403)		(0.099)
Some high school		-0.030		-0.219		0.019
		(0.050)		(0.124)		(0.055)
Trade school		0.246*		373*		0.204*
		(0.071)		(0.071)		(0.078)
Some college		0.243*		0.368*		0.198*
		(0.063)		(0.140)		(0.072)
College		0.209*		0.460*		0.130
		(0.063)		(0.132)		(0.073)
Some university		0.229*		0.231		0.193
		(0.084)		(0.170)		(0.099)
Undergraduate		0.197*		0.273		0.153
		(0.073)		(0.175)		(0.081)
Professional certification		0.077		-0.175		0.143
		(0.150)		(0.508)		(0.156)
Postgraduate		0.217		-0.040		0.185
		(0.120)		(0.500)		(0.123)

Table A.9 *(continued)*
Cox Proportional-Hazards Estimates of the Determinants of Unemployment Duration

	All		VQ/Dismissals		SW/Other	
	(1)	(2)	(3)	(4)	(5)	(6)
Newfoundland		-0.461* (0.133)		-10.004* (0.485)		-0.380* (0.139)
Prince Edward Island		0.298 (0.187)		0.315 (0.543)		0.272 (0.199)
Nova Scotia		-0.111 (0.093)		-252 (0.220)		-0.060 (0.103)
New Brunswick		0.021 (0.100)		-0.108 (0.265)		0.058 (0.109)
Quebec		-0.006 (0.049)		0.071 (0.119)		-0.034 (0.054)
Manitoba		0.108 (0.108)		0.285 (0.243)		0.075 (0.122)
Saskatchewan		0.240* (0.097)		122 (0.240)		0.267* (0.106)
Alberta		0.190* (0.059)		0.106 (0.129)		0.225* (068)
British Columbia		0.085 (0.059)		-0.050 (0.131)		0.128 (0.066)
Northwest Territories and Yukon		0.188 (0.326)		-10.331 (10.020)		0.464 (0.343)
Young children present		-0.184* (0.031)		-0.228* (0.087)		-0.169* (0.033)
ROE union job		0.308* (0.039)		-0.056 (0.131)		0.366* (0.042)
Expected to return to old job		0.261* (0.040)		0.339* (0.169)		0.268* (0.041)
VQ		0.141* (0.053)		—		—
Other		-0.157* (0.045)		—		—
Dismissals		-0.232* (0.086)		—		—
Previous weeks of UI		0.000 (0.000)		0.002 (0.001)		-0.000 (000)
No previous weeks of UI (dummy variable)		-0.116* (0.048)		0.106 (0.115)		-0.210* (0.055)
N	5,605	5,077	1,175	977	4,429	4,099

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
Columns 1, 3 and 5 are specific to cohort 2 only. Columns 2, 4 and 6 include demographic and labour market controls.

Table A.10
Cox Proportional-Hazards Estimates of the Determinants of Unemployment Duration with Time-Varying Covariates

	Local Unemployment Rate as Time Control Variable	Benefit Exhaustion Variables Added	Full Specification
Cohort 2	-0.122*	-0.175*	-0.176*
	(0.045)	(0.046)	(0.046)
CEIC region unemployment rate	-0.017*		-0.013
	(0.008)		(0.008)
Benefits expire in 1–3 weeks		0.954	0.384
		(0.632)	(0.622)
Benefits expire in 4–6 weeks		10.622	0.571
		(0.638)	(0.659)
Benefits expire in 7–12 weeks		-10.526*	-10.507*
		(0.273)	(0.273)
Male	0.117*	0.119*	0.112*
	(0.048)	(0.048)	(0.048)
ROE job full-time	-0.168*	-0.163*	-0.157*
	(0.069)	(0.070)	(0.070)
Married	0.183*	0.180*	0.183*
	(0.051)	(0.052)	(0.052)
Age	0.006	0.008	0.007
	(0.015)	(0.015)	(0.015)
Age^2	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Visible minority	-0.201*	-0.203*	-0.202*
	(0.069)	(0.069)	(0.070)
Elementary school	0.079	0.060	0.062
	(0.115)	(0.115)	(0.116)
Some high school	0.029	0.028	0.034
	(0.063)	(0.064)	(0.064)
Trade college	0.130	0.122	0.122
	(0.088)	(0.089)	(0.089)
Some college	0.131	0.124	0.120
	(0.081)	(0.083)	(0.083)
College	0.178*	0.178*	0.180*
	(0.079)	(0.079)	(0.079)
Some university	-0.054	-0.081	-0.088
	(0.118)	(0.120)	(0.121)
Undergraduate	0.156	0.150	0.153
	(0.099)	(0.100)	(0.100)
Professional certfication	0.123	0.103	0.097
	(0.209)	(0.215)	(0.215)
Postgraduate	0.179	0.130	0.128
	(0.158)	(0.163)	(0.162)

Table A.10 *(continued)*
Cox Proportional-Hazards Estimates of the Determinants of Unemployment Duration with Time-Varying Covariates

	Local Unemployment Rate as Time Control Variable	Benefit Exhaustion Variables Added	Full Specification
Newfoundland	-0.087	-0.225	-0.118
	(0.155)	(0.141)	(0.156)
Prince Edward Island	0.568*	0.475	0.550*
	(0.250)	(0.259)	(0.269)
Nova Scotia	0.173	0.130	0.171
	(0.114)	(0.112)	(0.114)
New Brunswick	0.148	0.119	0.142
	(0.117)	(0.117)	(0.118)
Quebec	0.030	-0.011	0.018
	(0.063)	(0.061)	(0.064)
Manitoba	0.125	0.092	0.096
	(0.128)	(0.130)	(0.130)
Saskatchewan	0.254*	0.277*	0.256*
	(0.122)	(0.121)	(0.122)
Alberta	0.221*	0.239*	0.230
	(0.082)	(0.082)	(0.083)
British Columbia	0.123	0.129	0.121
	(0.086)	(0.086)	(0.086)
Young children in household	-0.109*	-0.108*	-0.107*
	(0.037)	(0.038)	(0.038)
ROE job union	0.232*	0.244*	0.243*
	(0.050)	(0.050)	(0.051)
Expected to return to old job	0.204*	0.196*	0.200*
	(0.049)	(0.050)	(0.050)
Voluntary quit	-0.208*	-0.194*	-0.193
	(0.090)	(0.092)	(0.092)
Other reason for separation	-0.130*	-0.110	-0.111
	(0.057)	(0.058)	(0.058)
Dismissals	-0.284*	-0.334*	-0.326*
	(0.130)	(0.135)	(0.134)
Previous weeks ofactual UI receipt	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Dummy for no previous weeks of UI benefits	-0.125*	-0.136*	-0.136*
	(0.063)	(0.064)	(0.064)

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The time-varying covariates are the CEIC regional unemployment rate and the benefit-expiration dummy variables.

Table A.11
Cox Proportional-Hazards Model With Time-Varying Covariates

	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification
Cohort 2	-0.139* (0.044)	-0.197* (0.044)	-0.196* (0.044)	-0.122* (0.045)	-0.177* (0.045)	-0.174* (0.045)
CEIC region unemployment rate	-0.012 (0.006)		-0.011 (0.006)	-0.009 (0.008)		-0.006 (0.008)
Benefits expire in 1–3 weeks		0.214 (0.616)	0.213 (0.616)		10.184 (0.637)	10.136 (0.638)
Benefits expire in 4–6 weeks		0.675 (0.618)	0.664 (0.619)		0.568 (0.657)	10.475* (0.634)
Benefits expire in 7–12 weeks		-10.441* (0.242)	-10.456* (0.242)		-10.566* (0.273)	-10.574* (0.273)
Controls	N	N	N	Y	Y	Y

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *. The other controls included in the estimation are male, married, ROE job FT status, age, age-squared, visible minority, and education and province dummy variables.*

Table A.12
Cohort Effects in the Cox Proportional-Hazards Model — (evaluated at interview 1)

	All		VQ/Dismissals		SW/Other	
Cohort	-0.091* (0.032)	-0.050 (0.032)	-0.130 (0.076)	-0.025 (0.075)	-0.083* (0.036)	-0.059 (0.036)
Controls	N	Y	N	Y	N	Y

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *. The other controls included in the estimation are male, married, ROE job FT status, age, age-squared, visible minority, and education and province dummy variables.*

Table A.13
Cohort Effects in the Cox Proportional-Hazards Model with Time-Varying Covariates — (evaluated at interview 1)

	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification
Cohort 2	-0.114* (0.045)	-0.117* (0.045)	-0.114* (0.045)	-0.090* (0.046)	-0.093* (0.046)	-0.090 (0.046)
Local unemployment rate	Y	N	Y	Y	N	Y
Benefit-expiration dummy variables	N	Y	Y	N	Y	Y
Controls	N	N	N	Y	Y	Y

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *. The other controls included in the estimation are male, married, ROE job FT status, age, age-squared, visible minority, and education and province dummy variables.*

Table A.14
Weibull Estimates of the Determinants of Unemployment Duration — (without and with gamma unobserved heterogeneity)

			With Gamma Unobserved Heterogeneity	
	No Measure of Past UI Receipt	UI Receipt Measured	No Measure of Past UI Receipt	UI Receipt Measured
Cohort	-0.181*	-0.176*	-0.211*	-0.207*
	(0.044)	(0.044)	(0.048)	(0.048)
Male	0.306*	0.299*	0.325*	0.317*
	(0.047)	(0.047)	(0.051)	(0.051)
ROE job FT	-0.254*	-0.269*	-0.264*	-0.274*
	(0.064)	(0.065)	(0.072)	(0.072)
Married	0.295*	0.296*	0.306*	0.305*
	(0.051)	(0.051)	(0.056)	(0.055)
Age	0.022	0.005	0.023	0.006
	(0.014)	(0.014)	(0.015)	(0.015)
Age^2	-0.0006*	-0.0004*	-0.0007*	-0.0004*
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Visible minority	-0.475*	-0.457*	-0.493*	-0.479*
	(0.065)	(0.066)	(0.068)	(0.068)
Elementary	0.108	0.085	0.082	0.065
	(0.118)	(0.118)	(0.126)	(0.126)
Some high school	0.056	0.045	0.048	0.040
	(0.062)	(0.062)	(0.067)	(0.066)
Trade school	0.341*	0.343*	0.336*	0.369*
	(0.087)	(0.087)	(0.099)	(0.099)
Some college	0.300*	0.314*	0.311*	0.327*
	(0.079)	(0.079)	(0.088)	(0.088)
College	0.347*	0.360*	0.349*	0.364*
	(0.079)	(0.079)	(0.087)	(0.087)
Some university	0.244*	0.276*	0.275*	0.308*
	(0.107)	(0.107)	(0.118)	(0.118)
Undergraduate	0.301*	0.350*	0.300*	0.348*
	(0.095)	(0.096)	(0.106)	(0.106)
Professional certification	0.070	0.111	0.128	0.169
	(0.189)	(0.189)	(0.205)	(0.205)
Postgraduate	0.352*	0.416*	0.350*	0.411*
	(0.157)	(0.158)	(0.174)	(0.175)

Table A.14 *(continued)*
Weibull Estimates of the Determinants of Unemployment Duration — (without and with gamma unobserved heterogeneity)

	No Measure of Past UI Receipt	UI Receipt Measured	With Gamma Unobserved Heterogeneity: No Measure of Past UI Receipt	With Gamma Unobserved Heterogeneity: UI Receipt Measured
Newfoundland	-0.432*	-0.472*	-0.480*	-0.503*
	(0.152)	(0.155)	(0.159)	(0.163)
Prince Edward Island	0.474*	0.425	0.490	0.461*
	(0.243)	(0.241)	(0.274)	(0.273)
Nova Scotia	0.047	0.018	-0.008	-0.027
	(0.117)	(0.117)	(0.127)	(0.128)
New Brunswick	0.139	0.088	0.137	0.098
	(0.120)	(0.123)	(0.132)	(0.135)
Quebec	0.043	0.018	0.029	0.010
	(0.059)	(0.061)	(0.065)	(0.066)
Manitoba	0.028	0.006	0.018	0.009
	(0.132)	(0.130)	(0.144)	(0.142)
Saskatchewan	0.432*	0.411*	0.390*	0.376*
	(0.127)	(0.127)	(0.146)	(0.145)
Alberta	0.295*	0.290*	0.284*	0.277*
	(0.077)	(0.077)	(0.085)	(0.085)
British Columbia	0.285*	0.263*	0.258*	0.245*
	(0.074)	(0.074)	(0.082)	(0.082)
Northwest Territories	0.748	0.721	0.692	0.682
and Yukon	(0.424)	(0.410)	(0.498)	(0.496)
Young children present	-0.233*	-0.251*	-0.241*	-0.260*
	(0.038)	(0.038)	(0.040)	(0.040)
ROE union	0.468*	0.454*	0.507*	0.492*
	(0.049)	(0.049)	(0.056)	(0.056)
Expected to return to old job	0.461*	0.446*	0.467*	0.450*
	(0.050)	(0.051)	(0.057)	(0.057)
VQ	0.082	0.089	0.182*	0.191*
	(0.065)	(0.065)	(0.071)	(0.070)
Other	-0.253*	-0.238*	-0.244*	-0.225*
	(0.057)	(0.056)	(0.061)	(0.061)
Dismissals	-0.215*	-0.207*	-0.177*	-0.166*
	(0.104)	(0.103)	(0.110)	(0.110)
Previous weeks of UI benefits		0.0001		-0.0001
		(0.0005)		(0.0005)
No previous weeks of		-0.298*		-0.334*
UI benefits		(0.061)		(0.066)
Constant	-30.865*	-30.444*	-30.538*	-30.090*
	(0.264)	(0.274)	(0.294)	(0.304)
θ (heterogeneity)			0.506*	0.501*
			(0.106)	(0.105)
σ (duration dependence)	10.377*	10.374*	10.224*	10.222*
	(0.022)	(0.021)	(0.035)	(0.035)
Log likelihood	-101,570.37	-101,410.79	-101,410.87	-101,260.62
N	6,541	6,541	6,541	6,541

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The estimates are based on unweighted data.

Table A.15
Weibull Estimates of the Determinants of Unemployment Duration by Separation Group (without unobserved heterogeneity)

	VQ	SW	Other
Cohort	-0.317*	-0.102*	-0.313*
	(0.161)	(0.051)	(0.097)
Male	0.636*	0.294*	0.026
	(0.162)	(0.057)	(0.101)
ROE job FT	-0.463*	-0.182*	-0.315
	(0.200)	(0.086)	(0.129)
Married	0.495*	0.291*	0.163
	(0.180)	(0.060)	(0.109)
Age	-0.081	0.031	0.049
	(0.057)	(0.017)	(0.033)
Age^2	0.0005	-0.0006*	-0.0011*
	(0.0008)	(0.0002)	(0.0004)
Visible minority	-0.278	-0.450*	-0.570*
	(0.220)	(0.078)	(0.143)
Elementary	-0.506	-0.001	0.431
	(0.608)	(0.122)	(0.304)
Some high school	-0.160	0.042	0.101
	(0.224)	(0.070)	(0.147)
Trade school	0.832*	0.245*	0.169
	(0.350)	(0.093)	(0.218)
Some college	0.455	0.219*	0.425*
	(0.266)	(0.094)	(0.172)
College	0.782*	0.179	0.588*
	(0.244)	(0.101)	(0.167)
Some university	10.123*	-0.087	0.611*
	(0.362)	(0.140)	0.229
Undergraduate	0.720*	0.146	0.630*
	(0.325)	(0.127)	(0.184)
Professional certification	0.358	0.131	0.066
	(0.815)	(0.234)	(0.366)
Postgraduate	-0.166*	0.075	0.754*
	(0.849)	(0.247)	(0.239)

Table A.15 *(continued)*
Weibull Estimates of the Determinants of Unemployment Duration by Separation Group (without unobserved heterogeneity)

	VQ	SW	Other
Newfoundland	-10.925*	-0.396*	0.002
	(0.907)	(0.161)	(0.366)
Prince Edward Island	20.923	0.321	0.045
	(10.024)	(0.269)	(0.583)
Nova Scotia	0.227	0.035	-0.004
	(0.454)	(0.139)	(0.257)
New Brunswick	0.028	0.076	-0.006
	(0.550)	(0.132)	(0.299)
Quebec	0.296	-0.001	-0.049
	(0.221)	(0.069)	(0.136)
Manitoba	0.035	0.176	-0.319
	(0.455)	(0.158)	(0.259)
Saskatchewan	0.579	0.372*	0.359
	(0.470)	(0.141)	(0.335)
Alberta	0.294	0.354*	0.127
	(0.254)	(0.094)	(0.162)
British Columbia	0.237	0.351*	0.126
	(0.239)	(0.089)	(0.169)
Northwest Territories and Yukon	0.503	0.706	0.813
	(10.489)	(0.621)	(10.245)
Young children present	-0.393*	-0.173*	-0.331*
	(0.138)	(0.042)	(0.090)
ROE union	-0.109	0.554*	0.263*
	(0.244)	(0.054)	(0.109)
Expected to return to old job	0.274	0.417*	0.512*
	(0.362)	(0.053)	(0.116)
Previous weeks of UI benefits	0.005*	-0.0007	0.001
	(0.002)	(0.0005)	(0.001)
No previous weeks of UI benefits	-0.051	-0.542*	0.063
	(0.214)	(0.078)	(0.132)
Constant	-20.072*	-40.063*	-40.129*
	(0.994)	(0.334)	(0.632)
θ (heterogeneity)			
σ (duration dependence)	10.797*	10.242*	10.384*
	(0.085)	(0.024)	(0.047)
Log likelihood	-15,730.654	-56,420.632	-22,990.540
N	935	3,687	1,580

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The estimates are based on unweighted data.

Table A.16
Weibull Estimates of the Determinants of Unemployment Duration by Separation Group (with unobserved heterogeneity)

	VQ	SW	Other
Cohort	-0.451*	-0.110*	-0.363*
	(0.183	(0.056)	(0.103)
Male	0.753*	0.294*	0.046
	(0.186)	(0.061)	(0.107)
ROE job FT	-0.482*	-0.174	-0.330*
	(0.244)	(0.096)	(0.140)
Married	0.512*	0.300*	0.171
	(0.208)	(0.066)	(0.116)
Age	-0.072	0.032	0.053*
	(0.062)	(0.018)	(0.035)
Age^2	0.0004	-0.0007*	-0.0012
	(0.0008)	(0.0002)	(0.0004)
Visible minority	-0.288	-0.465*	-0.598*
	(0.241)	(0.081)	(0.148)
Elementary	-0.129	-0.038	0.470
	(0.590)	(0.131)	(0.312)
Some high school	-0.205	0.040	0.104
	(0.248)	(0.076)	(0.152)
Trade school	0.726	0.284*	0.153
	(0.465)	(0.107)	(0.236)
Some college	0.568*	0.236*	0.407*
	(0.308)	(0.106)	(0.183)
College	10.078*	0.138	0.585*
	(0.289)	(0.111)	(0.179)
Some university	0.953*	-0.043	0.605*
	(0.470)	(0.147)	(0.244)
Undergraduate	0.953*	0.116	0.606*
	(0.380)	(0.137)	(0.199)
Professional certification	0.451	0.151	0.160
	(0.875)	(0.261)	(0.393)
Postgraduate	-0.322	0.029	0.772*
	(0.887)	(0.272)	(0.257)

Table A.16 *(continued)*
Weibull Estimates of the Determinants of Unemployment Duration by Separation Group (with unobserved heterogeneity)

	VQ	SW	Other
Newfoundland	-20.172*	-0.406*	-0.008
	(0.904)	(0.169)	(0.392)
Prince Edward Island	20.728	0.318	0.156
	(10.540)	(0.306)	(0.596)
Nova Scotia	-0.144	-0.015	-0.010
	(0.528)	(0.149)	(0.274)
New Brunswick	0.056	0.108	0.025
	(0.627)	(0.146)	(0.316)
Quebec	0.375	-0.013	-0.070
	(0.252)	(0.076)	(0.142)
Manitoba	-0.079	0.198	-0.302
	(0.490)	(0.179)	(0.274)
Saskatchewan	0.312	0.377*	0.269
	(0.592)	(0.164)	(0.361)
Alberta	0.195	0.334*	0.132
	(0.297)	(0.105)	(0.176)
British Columbia	0.106	0.335*	0.105
	(0.278)	(0.099)	(0.181)
Northwest Territories	-0.071	0.627	0.681
and Yukon	(10.856)	(0.668)	(10.201)
Young children present	-0.412*	-0.167*	-0.353*
	(0.148)	(0.045)	(0.095)
ROE union	-0.136	0.606*	0.272*
	(0.286)	(0.061)	(0.116)
Expected to return to old job	0.341	0.422*	0.522*
	(0.431)	(0.059)	(0.125)
Previous weeks of UI	0.005	-0.001*	0.001
	(0.003)	(0.0006)	(0.001)
No previous weeks of UI	0.170	-0.567*	0.063
	(0.247)	(0.083)	(0.138)
Constant	-10.245*	-30.754*	-30.918*
	(10.136)	(0.368)	(0.676)
θ (heterogeneity)	10.262*	0.496*	0.350
	(0.406)	(0.118)	(0.228)
σ (duration dependence)	10.373*	10.091*	10.287*
	(0.140)	(0.0377)	(0.078)
Log likelihood	-15,660.621	-56,300.785	-22,980.470
N	935	3,687	1,580

*Note: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The estimates are based on unweighted data.

Table A.17
Search Inputs by Group and Cohort

	Cohort 1	Cohort 2
Hours/week at spell 1		
VQ/Dismissals	160.1	160.2
	(110.8)	(110.8)
	703	662
SW/Other	150.4	140.5
	(100.6)	(110.2)
	2,197	3,332
Hours/week between interviews 1 and 2		
VQ/Dismissals	150.3	140.7
	(120.1)	(110.7)
	215	188
SW/Other	140.8	140.6
	(100.7)	(110.1)
	607	938
Expenses spell 1 ($/week)		
VQ/Dismissals	380.12	370.85
	(800.08)	(530.40)
	722	630
SW/Other	460.00	420.39
	(990.42)	(1,040.38)
	2,272	3,148
Expenses between interviews 1 and 2		
VQ/Dismissals	390.61	290.92
	(850.10)	(370.18)
	209	179
SW/Other	420.73	340.97
	(1,260.12)	(1,100.09)
	585	875

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.18
Search Hours/Week by Group (First Unemployment Spell)

	Cohort 1	Cohort 2
Female		
VQ/Dismissals	150.0 (110.0) 370	140.3 (110.3) 321
SW/Other	140.7 (100.8) 850	130.1 (100.0) 1,622
Male		
VQ/Dismissals	170.2 (120.5) 331	170.3 (110.8) 338
SW/Other	150.9 (100.5) 1,340	150.6 (120.0) 1,704
Young (≤25)		
VQ/Dismissals	150.7 (120.6) 215	160.3 (110.5) 222
SW/Other	150.7 (100.9) 364	130.5 (90.9) 504
Prime age (>25–≤45)		
VQ/Dismissals	160.5 (110.5) 410	160.2 (120.1) 351
SW/Other	150.7 (100.5) 1,399	140.9 (110.3) 2,074
Older (>45)		
VQ/Dismissals	150.3 (90.8) 78	150.8 (110.7) 89
SW/Other	140.2 (100.5) 434	140.2 (120.2) 754

Note: Each cell gives the mean, the sample standard deviation, and the sample size. Age groupings are as in Table 3.

Table A.19
Search Expenses ($/Week) by Group (First Unemployment Spell)

	Cohort 1	Cohort 2
Female		
VQ/Dismissals	320.70	270.68
	(480.24)	(300.95)
	374	307
SW/Other	350.54	330.39
	(650.35)	(980.63)
	866	1,056
Male		
VQ/Dismissals	430.45	450.51
	(1,010.67)	(630.38)
	347	320
SW/Other	520.37	490.01
	(1,150.11)	(1,080.17)
	1,399	1,636
Young (≤25)		
VQ/Dismissals	320.91	410.22
	(400.33)	(500.04)
	218	220
SW/Other	390.82	380.21
	(740.62)	(970.49)
	392	488
Prime age (>25–≤45)		
VQ/Dismissals	410.62	360.01
	(1,000.79)	(590.18)
	427	325
SW/Other	510.01	430.14
	(1,140.76)	(950.61)
	1,431	1,955
Older (>45)		
VQ/Dismissals	380.33	320.50
	(580.32)	(390.33)
	77	85
SW/Other	360.16	430.74
	(590.43)	(1,300.77)
	449	705

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.20
Job-Search Methods: Use and Success

	Cohort 1	Cohort 2
Friends — use		
VQ/Dismissals	0.80	0.85
	(0.40)	(0.35)
	164	133
SW/Other	0.86	0.85
	(0.35)	(0.36)
	559	776
Friends — contacts		
VQ/Dismissals	0.51	0.52
	(0.50)	(0.50)
	135	113
SW/Other	0.43	0.41
	(0.50)	(0.49)
	480	649
Answered ads — use		
VQ/Dismissals	0.74	0.75
	(0.44)	(0.44)
	170	133
SW/Other	0.59	0.66
	(0.49)	(0.48)
	571	776
Answered ads — contacts		
VQ/Dismissals	0.67	0.53
	(0.47)	(0.50)
	124	101
SW/Other	0.52	0.46
	(0.50)	(0.50)
	330	512
Employer applications — use		
VQ/Dismissals	0.78	0.89
	(0.41)	(0.32)
	173	132
SW/Other	0.79	0.79
	(0.41)	(0.41)
	582	776

Table A.20 *(continued)*
Job-Search Methods: Use and Success

	Cohort 1	Cohort 2
Employer applications — contacts		
VQ/Dismissals	0.57 (0.50) 133	0.59 (0.49) 116
SW/Other	0.53 (0.50) 466	0.49 (0.50) 610
Other (not from agency) — use		
VQ/Dismissals	0.34 (0.48) 183	0.24 (0.43) 133
SW/Other	0.25 (0.43) 634	0.27 (0.44) 784
Other — contacts		
VQ/Dismissals	0.56 (0.50) 57	0.67 (0.48) 38
SW/Other	0.38 (0.49) 154	0.43 (0.50) 209

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.21
Reservation Wage Ratios

	Cohort 1	Cohort 2
At ROE date		
VQ/Dismissals	10.11	10.10
	(0.75)	(0.50)
	783	691
SW/Other	10.01	10.02
	(0.78)	(10.27)
	2,752	3,782
At interview 1		
VQ/Dismissals	10.12	10.09
	(0.95)	(0.39)
	322	290
SW/Other	10.00	0.99
	(0.36)	(0.40)
	1,084	1,670
At interview 2		
VQ/Dismissals	10.13	10.18
	(0.44)	(0.58)
	208	184
SW/Other	10.10	10.03
	(0.72)	(0.42)
	597	899

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.22
Reservation Wage Ratios by Gender

	Cohort 1	Cohort 2
At ROE date		
Female		
VQ/Dismissals	10.06	10.06
	(0.51)	(0.48)
	407	349
SW/Other	0.97	0.96
	(0.37)	(0.55)
	1,095	1,848
Male		
VQ/Dismissals	10.16	10.14
	(0.93)	(0.51)
	375	340
SW/Other	10.03	10.06
	(0.96)	(10.61)
	1,648	1,930
At interview 1		
Female		
VQ/Dismissals	10.09	10.06
	(0.44)	(0.37)
	177	167
SW/Other	10.00	0.94
	(0.33)	(0.35)
	500	906
Male		
VQ/Dismissals	10.15	10.11
	(10.30)	(0.41)
	145	122
SW/Other	10.01	10.03
	(0.39)	(0.44)
	582	763
At interview 2		
Female		
VQ/Dismissals	10.16	10.16
	(0.49)	(0.45)
	124	117
SW/Other	10.06	0.99
	(0.38)	(0.35)
	281	486
Male		
VQ/Dismissals	10.09	10.23
	(0.37)	(0.72)
	84	66
SW/Other	10.14	10.07
	(0.91)	(0.47)
	315	413

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.23
Expected Wages by Group and Interview Date

	Cohort 1	Cohort 2
At interview 1		
VQ/Dismissals	10.23	10.23
	(10.04)	(0.68)
	301	258
SW/Other	10.11	10.09
	(0.62)	(0.54)
	994	1,517
At interview 2		
VQ/Dismissals	10.16	10.27
	(0.50)	(0.72)
	188	169
SW/Other	10.16	10.11
	(0.62)	(0.49)
	554	853

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.24
Proportional-Hazards Models with Job-Search Time-Varying Covariates

	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification	Local Unemployment Rate as Time Varying Variable	Benefit Exhaust Added	Full Specification
Cohort 2	-0.069	-0.111	-0.110	-0.077	-0.115	-0.114
	(0.058)	(0.059)	(0.059)	(0.059)	(0.060)	(0.060)
RWR	0.013	0.018	0.014	0.020	0.023	0.021
	(0.052)	(0.053)	(0.053)	(0.052)	(0.052)	(0.053)
Expenses	0.000	0.001*	0.001	0.001	0.000	0.000
	(0.000)	(000)	(0.000)	(0.000)	(0.000)	(0.000)
Hours	0.000	-0.001	-0.001	0.000	-0.001	-0.000
	(003)	(003)	(0.003)	(0.003)	(0.003)	(0.003)
Local unemployment	Y	N	Y	Y	N	Y
Benefit variables	N	Y	Y	N	Y	Y
Controls	N	N	N	Y	Y	Y

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The controls included in the estimation are the same as in Table 9.

Table A.25
Log Wage Drop from ROE Job to First Post-ROE Job

All	Cohort 1	Cohort 2
VQ/Dismissals	-0.032 (0.517) 289	-0.050 (0.432) 234
SW/Other	0.007 (0.339) 1,184	-0.008 (0.401) 1,428
Both groups	-0.001 (0.381) 1,473	-0.016 (0.407) 1,662
Females		
VQ/Dismissals	-0.049 (519) 153	-0.082 (0.429) 106
SW/Other	0.004 (0.353) 449	-0.017 (0.417) 666
Males		
VQ/Dismissals	-0.015 (0.516) 135	-0.031 0.434 128
SW/Other	0.010 (0.331) 730	-0.002 (0.390) 762
Young (≤25)		
VQ/Dismissals	0.053 (0.588) 93	-0.089 (0.406) 96
SW/Other	-0.064 (0.337) 170	-0.045 (0.458) 209
Prime Age (>25,≤45)		
VQ/Dismissals	-0.070 (0.453) 175	-0.025 (0.460) 116
SW/Other	0.020 (0.353) 758	0.005 0.356 908
Older (>45)		
VQ/Dismissals	-0.209 (0.515) 21	0.078 (0.423) 22
SW/Other	0.025 (289) 256	-0.020 (482) 311

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.26
Log Wage Drop from the ROE Job to the First Post-ROE Job by Part- or Full-Time Status

Both full-time	Cohort 1	Cohort 2
VQ/Dismissals	-0.002	0.002
	(0.347)	(0.350)
	183	137
SW/Other	0.003	-0.005
	(0.287)	(0.345)
	926	997
Both part-time		
VQ/Dismissals	-0.091	-0.294
	(0.890)	(0.560)
	22	17
SW/Other	-0.019	-0.067
	(0.359)	(0.397)
	74	162
FT to PT		
VQ/Dismissals	-0.178	-0.092
	(0.731)	(0.468)
	49	47
SW/Other	-0.029	-0.060
	(0.523)	(571)
	113	173
PT to FT		
VQ/Dismissals	0.082	-0.079
	(0.456)	(0.553)
	31	30
SW/Other	0.193	0.214
	(0.564)	(0.666)
	47	52

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.27
Log Wage Drop by Detailed Separation Group

	Cohort 1	Cohort 2
VQ	-0.052	-0.060
	(0.541)	(0.405)
	223	158
Dismissals	0.035	-0.267
	(0.417)	(0.491)
	66	76
SW	0.001	-0.022
	(0.317)	(0.373)
	906	1,053
Other	0.025	0.033
	(0.401)	(0.469)
	278	375

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.28
Determinants of Wages

	Log ROE wage	Log first job wage	Log ROE wage — log first wage
Cohort 2	0.001 (0.015)	0.038 (0.021)	-0.024 (0.021)
Age	0.044* (0.004)	0.035* (0.007)	0.015* (0.007)
Age^2	-0.000* (0.000)	-0.000* (0.000)	-0.000* (0.000)
Visible minority	-0.042 (0.021)	-0.047 (0.035)	-0.002 (0.036)
Married	0.047* (0.019)	0.066* (0.018)	-0.019 (0.028)
Widowed, separated, divorced	-0.067* (0.028)	-0.096* (0.042)	0.006 (0.042)
Elementary school	-0.219* (039)	-0.042 (0.056)	-0.091 (0.055)
Some high school	-0.084* (0.021)	-0.005 (0.030)	-0.044 (0.030)
Trade college	0.137* (0.032)	0.169* (0.044)	0.004 (045)
Some college	0.113* (0.027)	0.140* (0.038)	-0.074* (0.037)
College	0.099* (0.027)	0.134* (0.038)	-0.094* (037)
Some university	0.138* (0.035)	0.099* (0.051)	0.031 (0.051)
Undergraduate	0.322 (0.031)	0.404* (0.046)	-0.061 (0.047)
Professional certification	0.247* (0.065)	0.257* (0.096)	-0.045 (0.101)
Postgraduate	0.379* (0.050)	0.403* (0.078)	-0.065 (0.077)

Table A.28 *(continued)*
Determinants of Wages

	Log ROE wage	Log first job wage	Log ROE wage — log first wage
ROE job tenure	0.000*	0.000	0.000*
	(0.000)	(0.000)	(0.000)
ROE job union	0.325*	—	0.108*
	(0.017)		(031)
First post-ROE job union	—	0.319	-0.151*
		(0.023)	(0.031)
Newfoundland	-0.214*	-0.181*	-0.091
	(0.051)	(0.072)	(0.075)
Prince Edward Island	-244*	-0.094	-0.049
	(0.085)	(0.122)	(0.127)
Nova Scotia	-0.162*	-0.104	-0.013
	(0.038)	(0.055)	(0.055)
New Brunswick	-0.144*	0.135*	-0.054
	(0.043)	(0.063)	(0.061)
Quebec	-0.119*	-0.102*	-0.020
	(0.020)	(0.028)	(0.028)
Manitoba	-215*	-0.119	-0.083
	(0.050)	(0.067)	(0.074)
Saskatchewan	-0.060	-0.077	0.074
	(0.043)	(0.054)	(0.054)
Alberta	-0.061*	-0.026	-019
	(0.026)	(0.038)	(0.039)
British Columbia	0.009	0.063	-0.043
	(0.025)	(0.038)	(0.038)
Northwest Territories and Yukon	0.025	0.244	-0.255
	(0.138)	(0.171)	(0.173)
Constant	10.200*	10.383*	-0.227
	(0.083)	(0.125)	0.124
$\bar{R}^2$	0.278	0.240	0.021
N	3,423	1,606	1,500

*Note: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*

Table A.29
Cohort Effects on Job Changes by Detailed Separation Group

	Log wage drop from ROE job to first post-ROE job	Ordered probit on change from ROE job to first post-ROE job
VQ	-0.014 (0.076) 178	-0.217 (0.163) 219
Dismissals	0.111 (0.133) 75	-0.049 (0.315) 91
SW	-0.024 (0.024) 935	-0.101 (0.067) 1,089
Other	-0.026 (0.053) 312	-0.011 (0.115) 381
Other controls	Y	Y

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The controls included in the estimation are as in Table A.28.

Table A.30
Hours Worked: Means and Standard Deviations

ROE job	Cohort 1	Cohort 2
VQ/Dismissals	360.3 (130.4) 1,004	360.1 (130.8) 1,018
SW/Other	390.0 (110.8) 2,823	380.0 (120.5) 3,954
First post-ROE job		
VQ/Dismissals	350.9 (130.6) 358	340.1 (140.9) 295
SW/Other	380.1 (120.4) 1,399	360.5 (130.5) 1,683

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.31
Hours Worked: Drop from the ROE Job to the First Post-ROE Job

	Cohort 1	Cohort 2
All		
VQ/Dismissals	10.19	30.11
	(140.94)	(170.76)
	357	293
SW/Other	10.43	20.37
	(110.42)	(130.50)
	1,385	1,660
FT both jobs		
VQ/Dismissals	0.11	0.72
	(90.85)	(100.47)
	229	167
SW/Other	0.06	0.47
	(80.14)	(90.76)
	1,609	1,208
PT both jobs		
VQ/Dismissals	10.56	10.98
	(60.66)	(70.42)
	26	27
SW/Other	0.58	10.87
	(50.56)	(60.50)
	84	183
FT to PT		
VQ/Dismissals	220.83	250.91
	(100.26)	(100.81)
	61	70
SW/Other	220.76	240.99
	(90.36)	(120.07)
	136	205
PT to FT		
VQ/Dismissals	-190.32	-230.41
	(60.53)	(100.85)
	41	33
SW/Other	-180.97	-210.41
	(70.61)	(100.65)
	56	64

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.32
Cohort Effects on Changes in Hours Worked

	All	VQ/Dismissals	SW/Other
Cohort 2 coefficient	10.134*	10.920	0.941*
(no controls)	(0.441)	(10.288)	(0.455)
Cohort 2 coefficient	0.162	0.938	-0.072
(with controls)	(0.644)	(10.778)	(0.685)

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The controls included in the latter specification are as in Table A.28.

Table A.33
Determinants of Job Satisfaction in First Post-ROE Job (ordered probits)

	All	VQ/Dismissals	SW/Other
Ranking			
1 (lowest)	0.064	0.074	0.062
2	0.036	0.047	0.033
3	0.053	0.052	0.053
4 (same as ROE)	0.379	0.158	0.436
5	0.088	0.080	0.090
6	0.124	0.178	0.111
7 (highest)	0.255	0.412	0.215
Cohort dummy	-0.083*	-0.186*	-0.054
(no controls)	(0.035)	(0.085)	(0.039)
Cohort (with controls)	-0.108*	-0.160	-0.084
	(0.051)	(0.135)	(0.057)
Sample (with controls)	1,780	310	1,470

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The controls included in the estimation are as in Table A.28.

Table A.34
Determinants of Job Satisfaction in Current Interview 1 Job (ordered probits)

	All	VQ/Dismissals	SW/Other
Ranking			
1 (lowest)	0.054	0.050	0.055
2	0.035	0.024	0.038
3	0.055	0.053	0.056
4 (same as ROE job)	0.347	0.173	0.395
5	0.086	0.068	0.092
6	0.139	0.211	0.119
7 (highest)	0.284	0.423	0.245
Cohort (no controls)	-0.112*	-0.200*	-0.087*
	(0.034)	(0.076)	(0.038)
Cohort (with controls)	-0.158*	-0.245*	-0.113*
	(0.050)	(0.122)	(0.055)
Sample (with controls)	1,915	368	1,547

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *.*
The controls included in the estimation are as in Table A.28.

Table A.35
Proportions "Still Looking" in First Job after ROE

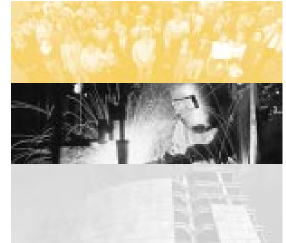
	Cohort 1	Cohort 2
VQ	0.475	0.551
	(0.500)	(0.499)
	283	212
Dismissals	0.553	0.563
	(0.500)	(0.499)
	85	93
SW	0.440	0.513
	(0.497)	(0.500)
	1,089	1,307
Other	0.540	0.539
	(0.499)	(0.499)
	353	452
ROE FT to first FT		
VQ	0.301	0.446
	(0.460)	(0.502)
	125	530
Dismissals	0.330	0.415
	(0.476)	(0.500)
	39	35
SW	0.279	0.311
	(0.448)	(0.463)
	610	550
Other	0.358	0.316
	(0.481)	(0.466)
	151	180
ROE FT to first PT		
VQ	0.558	0.384
	(0.506)	(0.495)
	26	28
Dismissals	0.539	0.388
	(0.546)	(0.526)
	6	7
SW	0.622	0.605
	(0.490)	(0.492)
	45	70
Other	0.717	0.735
	(0.456)	(0.445)
	38	51

Note: Each cell gives the mean, the sample standard deviation, and the sample size.

Table A.36
Probit on "Still Looking" in Full-time Job

	Cohort Coefficient (no controls)	Cohort Coefficient (with controls)
All	0.140*	0.072
	(0.040)	(0.059)
VQ	0.228*	0.016
	(0.098)	(0.173)
Dismissals	0.076	-0.203
	(0.181)	(0.316)
SW	0.103*	0.045
	(0.049)	(0.072)
Other	0.038	0.192
	(0.078)	(0.121)

*Notes: The coefficients that are significantly different from 0 at a 95 percent significance level are denoted by an *. The controls included in the estimation are age, age-squared, visible minority, married, widowed/separated/divorced, tenure on ROE job, union status in ROE job, and schooling and province dummy variables.*



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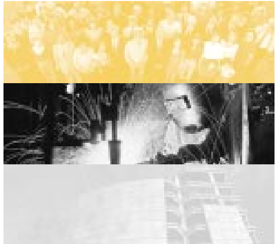

List of UI Evaluation Technical Reports

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

- **Canada's Unemployment Insurance Program as an Economic Stabiliser**
  E. Stokes, WEFA Canada, 1995. *(Evaluation Brief #6)*

UI and the Labour Market

- **Unemployment Insurance and Labour Market Transitions**
  S. Jones, Economics Department, McMaster University, 1995. *(Evaluation Brief #22)*

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