Do Labels Matter? Unemployment Insurance and Quits in Canada

Final Report

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Executive Summary

Recent unemployment insurance (UI) legislative changes that penalise those who quit their jobs allow us to discriminate between alternative labour market theories of worker-firm separations. Further, the implications of behavioural responses by workers on the cost of operating the UI system and the substantial differences in the responses by different age and sex groups are investigated. In response to the legislation, firms and workers might relabel separations that would have been called quits in terms of UI-eligible labels; alternatively, workers might be inhibited from quitting if relabelling does not occur. We find no evidence of relabelling, but we find that women and young men are inhibited from quitting, whereas prime-age males seem unaffected by the large increase in the cost of quitting imposed by the changes. This suggests that the cost savings to the system are actually larger than the reduction in the claim rate of quitters, since those inhibited from quitting who would have claimed benefits also contribute to the savings. Further, it suggests that simple "efficient separations" models do not adequately characterise the labour market and that labels matter for reasons apart from pure UI eligibility. Moreover, one simple characterisation need not comprehend the diversity of responses to a policy change; clearly in this case different groups' responses were not identical.

1. Introduction

Unemployment insurance (UI) systems, as is well known, are complex social programs with a wide variety of potential effects on economic behaviour. Aspects of UI that have been examined include the effects of: benefit replacement rates on unemployment durations (Meyer 1990), UI qualifying periods on employment durations (Green and Riddell 1993), experience rating on the amplitude of seasonal employment fluctuations (Anderson 1993), overall benefit generosity on employers' choices between layoffs and hours reductions (Burdett and Wright 1989), and differential generosity on interregional labour mobility (Day 1992). Interestingly, however, one aspect of UI that has received almost no attention to date is that in many systems, including Canada's, entitlement to benefits depends critically on the reason why one left one's last job. Most systems, for example, impose substantial penalties on workers who quit or were dismissed from their last job, relative to workers separating for other reasons.[1]

Recent Unemployment Insurance (UI) legislative changes — Bills C-21 and C-113 — increased the UI penalties for those who voluntarily quit their jobs.

In this paper we study the effect of non-neutralities in UI related to separation reasons on the separation behaviour of Canadian workers, using two recent legislative changes (Bills C-21 and C-113) that dramatically increased the UI penalties for quitting one's job or being dismissed for cause. These changes to the UI legislation first partially and then completely disentitled those who quit without just cause and those dismissed for cause from receiving UI benefits. We argue that the responses of Canadian workers to these changes are of interest for two distinct reasons. First, in the period preceding the legislative changes being studied, voluntary quits accounted for between 19 % (prime age males) and 37 % (young females) of all separations. To the extent that quit penalties are actually effective in reducing overall UI claim rates, they have the potential to generate substantial cost savings for the UI system as a whole. The size of the savings are, of course, of direct interest to policymakers.

To the extent that quit penalties are actually effective in reducing overall UI claim rates, they have the potential to generate substantial cost savings for the UI system.

A second reason for examining the natural experiments provided by the disentitlement of Canadian job quitters is that they may provide useful information on the empirical relevance of some simple theoretical models of job separations. For example, in the extreme case of a pure "efficient separations" model (e.g. Becker et al. 1977; McLaughlin 1991) labels attached to separations have no market consequences apart from their implications for UI eligibility. As a result, any distinction between the UI eligibility of quitters

1. The one exception we are aware of is Ragan (1984), who examines the effects of a general tightening up of the eligibility requirements for those who quit or were dismissed for cause in the United States in the 1970s. Ragan used aggregate manufacturing data for a subset of states in the four even-numbered years from 1972 to 1980, excluding 1976. Like our findings for prime-age males, but unlike our findings for other groups, he did not find a significant effect of UI quit penalties on the quit rate.

Some firms and workers may "relabel" their separations to avoid the UI quit penalty; other workers may be "inhibited" from quitting.

and other job losers should lead firms and workers to "relabel" all UI-eligible quits as layoffs, with no associated change in either the overall separation rate or the UI claim rate. Under such a "pure relabelling" scenario, UI quit penalties would be totally ineffective in generating cost savings for the UI system. This is not the case for some other models of separations, including ones in which wage rigidities (Hashimoto and Yu 1980) or asymmetric information (Gibbons and Katz 1991) play a role. In these models, firms may be reluctant to acquiesce in the relabelling of separations, or workers may not wish to be relabelled because of the signalling value of labels. In such models, the introduction of UI quit penalties should reduce the total number of separations and impose real costs on workers, leading to the potential for substantial cost savings from the introduction of UI quit penalties. An examination of Canadians' responses to these legislative changes is, therefore, also of broader relevance to our conceptualisation of how labour markets function.

The empirical analysis in this paper is based on Human Resources Development Canada's (HRDC) administrative files, together with background variables from Statistics Canada. The administrative data provide monthly counts of job separations by reason, but because of a reporting change for dismissals in the midst of the period in question, our focus is primarily on voluntary quitters. In our analysis we first establish that both the legislative changes examined here did indeed make it harder for workers who were reported to have quit a job to claim UI: the fraction of voluntary quitters and those dismissed for cause who actually receive UI benefits does fall substantially following the legislative changes, particularly the complete disentitlement. Despite this, however, we find no evidence of a change in the reported frequency of quits in response to the first, less severe legislated increase in quit penalties. While this may reflect our inability to separate signal from noise, it also indicates that any changes in quit rates resulting from the legislation were not very large, since the standard errors of the estimates are sufficiently small that any economically large change would be observed. With respect to the second, more drastic policy change, we do find a significant decrease in the measured quit rate for all demographic groups except prime-age males. Finally, and perhaps most importantly, in no case do we find evidence that penalty-induced decreases in the quit rate are accompanied by concomitant increases in the separation rate for non-penalised reasons, such as layoffs. Indeed, most of the appropriate point estimates are negative, indicating that the observed reductions in quits correspond to reductions in total separations, rather than a relabelling thereof.

Although the analysis in this paper could be improved by access to more detailed microdata on separations over time, and while our aggregate analysis might as a result fail to identify certain population subgroups (e.g. frequent users) who might have been able to respond to the quit penalties by relabelling their separations, we conclude from our analysis here that simple relabelling of separations in response to the UI disentitlement of quitters was not an important phenomenon for the Canadian labour market as a whole. Instead, it appears that the legislation either had no effect on workers' separation behaviour (the case of prime-age men) or worked not by relabelling separations but by "inhibiting" workers from quitting. As a consequence, we are led to question the empirical relevance of simple "efficient separations" models of the dissolution of firm-worker matches: labels *do* seem to matter for reasons apart from pure UI eligibility. Another consequence is that the savings to the UI system resulting from UI quit penalties, rather than being negligible, might in fact have been larger than what is implied by the drop in the claim rate of those who quit. This is because the decreased claim rate of quitters does not incorporate the unclaimed UI benefits by those who were inhibited from leaving their jobs, which are also a cost savings to the system.

Section 2 sketches the relevant institutional and policy background. Section 3 contrasts some alternative theories in the literature and what they imply for the expected effects of these policy changes. Section 4 discusses important identification issues, while the data and some descriptive statistics are presented in Section 5. The statistical methodology is outlined in Section 6, and the results are presented and discussed in Section 7. Section 8 concludes.

2. Policy and Institutional Background

Two distinct policy changes in the Canadian UI system are the focus of this paper. Prior to November 1990, the *Unemployment Insurance Act* disentitled claimants from receiving benefits for a *maximum* of 6 weeks if they quit without just cause or were dismissed for cause, although there was no requirement that the case worker impose any penalty at all.[2] Bill C-21, which became effective in November 1990, increased the disentitlement period to between 7 and 12 weeks. Then, in April 1993, Bill C-113 completely eliminated benefits for these two groups. These three intervals, the baseline (period 1), the partial disentitlement (period 2), and the total disentitlement (period 3), are contrasted throughout the analysis.

In order to comprehend the nature and limits of any relabelling, it is important to understand the process whereby labels are assigned to separations in the Canadian UI system. For official UI purposes, the label, subject to revision, is appended by the firm in filling out the Record of Employment (ROE) form at or shortly after the time of separation. A copy of the form is given to the worker, and another is sent directly to the UI district office by the firm. This form is a prerequisite for a claim, but it must be sent to the UI office whether or not a claim is established. Workers, of course, can protest the label, and the UI regulations require that the benefit of the doubt be given to the worker in cases of equality in the balance of evidence. Bill C-21, along with partially disentitling quitters, also introduced a penalty for false reporting. Given that (1) unlike the United States, there is no UI experience rating of firms in Canada, (2) labels are inherently difficult to verify, and (3) enforcement of the penalty for falsely reporting the reason for separation is very expensive, it appears plausible that a firm might sometimes apply UI-eligible labels, such as “other” or “short work,” instead of “voluntary quit” or “dismissal,” on the ROE form. This would effectively relabel the worker. Of course not all voluntary quitters are disentitled, only those who quit without “just cause.” This is a somewhat loosely defined concept; at present there are 14 justified categories, the last of which is “such other reasonable circumstances as are prescribed” (HRDC, 1996, Bill C-21, p. 12).

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[2] A “disentitlement” of X weeks in the current context means the individual has to wait that many weeks without benefits before he or she can apply for and begin to receive UI benefits.

3. *Theoretical Considerations*

In our empirical analysis below, we assess the consistency of the evidence with three simple accounts of what quit penalties do to separation behaviour: a "pure relabelling" scenario in which the penalties have no real effects but change the reported reasons for separation; an "inhibition" scenario in which these penalties reduce the total number of separations; and a naive, "no-response" model in which both the total volume and composition of separations is unchanged, with an attendant decrease in UI receipt by workers who quit or are dismissed from their jobs. In the remainder of this section, we discuss theoretical models of the separation process that might give rise to each of these three possible scenarios in turn.

A. Models Generating Pure Relabelling

A common characterisation of the efficient turnover view of labour markets (e.g. Becker et al. 1977; Burdett 1978; Jovanovic 1979; Mortensen 1988) is that there is no meaningful distinction between quits and layoffs. McLaughlin's (1991) model is perhaps the most noteworthy of this class, since it builds on earlier work. It interprets the quit or layoff label as summarising the labour market environment of a particular separation but posits that the label is economically inconsequential. Labour turnover is efficient and joint wealth maximising in the face of economic shocks, since wage revisions or side payments can always be used to prevent inefficient separations. A quit occurs when an upward revision of the wage requested by a worker on the basis of an outside offer is not met by the employer. Similarly, a layoff results when an employer initiated downward revision is refused by a worker.

Interestingly, in all of these models there is no financial cost or gain to either party attached to the relabelling of separations: in the absence of UI, a firm or worker that chooses to change the label publicly attached to a separation incurs no costs or benefits. If these models are taken literally, the introduction of a financial gain to relabelling, via the UI system, should cause the complete elimination of quits from the economy.[3] Further, there should be no change in the actual separation behaviour of firms and workers in response to a UI quit

[3] McLaughlin (1991, p 11) obviously does not intend his model to be taken quite so literally, but in addressing the implications of efficient turnover for UI he posits an incentive for quits to be relabelled so as to make them UI eligible. Side payments, or arrangements of some type, are proposed as mechanisms by which the worker could entice the firm to agree to relabelling.

penalty.[4] Pure relabelling models also predict that UI quit penalties will have no effects on government UI expenditures, since those who would have quit will continue to claim as "layoffs" after the policy change.

B. Models Generating an Inhibition of Separations

Understanding, where possible, differences in responses to the policy changes across genders and ages is central to the study.

In any model where workers' ability to have their separations relabelled is limited, UI quit penalties will raise the costs of employee-initiated separations, and should lead to a reduction in total separations from the firm. In this subsection we make note of three broad classes of factors that might limit the extent of such relabelling. One of these is the fact that, if asymmetric information is important in labour markets, separation labels might convey useful information to other firms or workers in the labour market and in this sense be valuable in themselves. An example of this class of models is Gibbons and Katz's (1991) "layoffs and lemons" hypothesis, where a worker would prefer to be identified as separating as a result of a mass layoff than an individual layoff, because of the negative signal of worker quality embodied in the individual layoff category. Analogous to Gibbons and Katz, one can easily conceive of a signalling model in which workers prefer other firms to know that they quit their former jobs and were not laid off (because it signals greater ability). Similarly, firms might not want to develop a reputation for laying workers off repeatedly. In both these cases, the parties may have an interest in preventing the relabelling of quits as layoffs, even though there is a UI-related financial gain associated with doing so.[5]

A second model in which relabelling might be limited is one with some *ex post* wage rigidities (as in Hashimoto and Yu 1980; Hall and Lazear 1984), combined with some limitations on firms' and workers' abilities to make contractual commitments. For example, suppose that firms and workers agree, *ex ante*, to a real wage of *w*, despite the existence of uncertainty in the worker's future internal and external productivity (θ^i and θ^e, respectively). Focus on a worker whose best outside option includes at least the possibility of an

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4 More precisely, comparing a world in which all separations are treated equally by the UI system to one where only layoffs are UI eligible, efficient labour contracts should generate identical patterns of separations in the two worlds. The only difference between the two worlds will be that in the second, all separations will be labelled as quits, since it is jointly inefficient (from the firm and worker's joint perspective) to forsake UI benefits. It may also be worth noting that *both worlds will have more separations than a world with no UI at all.*

5 Of course, in some environments, the argument might run in the opposite direction: workers might be reluctant to have their separations labelled as quits because it signals less expected labour force attachment. The main point of the argument is that, with signalling, there are factors other than UI eligibility affecting the optimal labelling of separations and that the presence of these other considerations is likely to dampen the "extreme" amount of relabelling predicted by the pure efficient separations model.

unemployment spell, let $\theta^e + UI$ be her outside option if she is laid off, and let θ^e be her outside option if she quits (quitters are ineligible for UI). *Ex post*, suppose it happens that $\theta^e + UI > \theta^i > w > \theta^e$. In this situation, firm-plus-worker wealth is maximised by dissolving the firm-worker match and calling the separation a layoff. Firms faced with this situation, however, will be reluctant to lay the worker off unless they can be guaranteed a sufficiently large share of the value of the UI payment to cover the loss of $(\theta^i - w)$ and any transaction costs. In the absence of such a side payment, the firm can make positive profits by retaining the worker at the wage *w*, and the worker cannot profitably quit, given this wage and the UI quit penalty. Thus, unless firms can credibly precommit to relabel all separations as layoffs, the introduction of UI quit penalties will cause some workers who would otherwise leave to remain with their former employers.

Workers or firms may not want to relabel separations because of the signalling value of the label.

Finally, the economic theories discussed to this point include only two types of players: firms and workers. The environment under consideration, of course, has other important players, notably the ministry that runs the UI system and the Canadian legislature, which originated the Bills being studied. A stated purpose of each piece of legislation is to reduce the cost of operating the UI system. These additional players, therefore, have an incentive to ensure that massive relabelling does not occur. What efficient turnover models euphemistically call side payments, are, in some of their incarnations, labelled as bribes by those who drafted the legislation. While verification of relabelling is difficult, given that penalties were introduced in Bill C-21 and HRDC employs a number of inspectors to guard against, among other things, false reporting, there is at least the threat of penalties being sufficient to stop relabelling.

C. Models Generating No Response

While it is hard to think of a plausible economic model in which a large change in the UI eligibility of quitters has no effect at all on the number of separations labelled as quits, a number of factors might act to limit firms' and workers' responses to the legislation to the point where it cannot be detected in national separation rates. Aside from the factors mentioned in the previous section, these might include the fact that quitters are more likely than other workers to move directly from one job to another without an intervening unemployment spell. To the extent that some quitters are certain that no such spell will occur, this should make them immune to the effects of UI quit penalties, rendering relabelling unnecessary. Another would be a combination of good enforcement of HRDC's "false reporting" provisions and a relatively small number of workers who are on the margin between quitting and staying with the firm. Thus, despite the fact that such behaviour may be hard to rationalise, it is important to take note of this possibility in our analysis of the data.

4. Identification Issues

Since we never see the same separation twice, changes must be inferred from the counterfactual.

While the notions of inhibition and relabelling are at first glance very intuitive, it is useful at this point to give a somewhat more formal definition. In this paper, we shall say that relabelling away from quits has occurred in response to a UI policy change that penalises quits if after the policy change there are some layoffs which *would* have been labelled as quits had the policy change not occurred. This definition points out two important aspects of relabelling. First, since we never actually see the same separation twice with different labels attached to it, nor do we observe any "true" label for separations, relabelling must be defined by a counterfactual. (Similarly, inhibition is defined as the number of retentions — workers who do not separate — who *would* have quit under the old regime.) Second, notice that the quit rate (i.e. the ratio of separations labelled as quits to employment in the previous period) can fall because of relabelling and/or inhibition.

To see how we might distinguish relabelling and inhibition effects empirically, it is instructive to consider a simple formal model. To that end, consider the population of employed persons at the start of a given month, E_t in number, and suppose E_t is determined by a dynamic process of the form:

$$\boldsymbol{E_t = h(X_t, u_t)} \tag{1}$$

where X_t is a vector of macroeconomic and seasonal variables, and u_t is a random shock. Suppose further that each member, *i*, of this population has a vector of personal characteristics x_{it}.

Consider also the mapping g: $(x_{it}, X_t, P_t, e_t) \rightarrow \{R, VQ, SW\}$ where the outcomes are: (R) the worker remains with his or her employer until the end of the month; (VQ) the worker voluntarily quits; or (SW) the worker is laid off for economic reasons, that is, "short work." Note that, in addition to aggregate macroeconomic and seasonal variables, this mapping can also be shifted by a UI policy variable (P_t), and an error term e_t, which may be correlated with u_t.[6]

Next, note that between any two periods, and for any given value of x, the function g can either assign the same label to that value of x in both periods, or change the label assigned to it in one of six possible ways (R to VQ, VQ to R, R to SW, SW to R, VQ to SW, and SW to VQ). Now, holding *X* fixed at a particular value, and changing the level of *P*, denote the expected net fraction of the employed population switching among the three states as follows:

[6] Note also that e_t might not be independently and identically distributed. For example, it might be autocorrelated.

Net movement from:	Concept:	Notation:
VQ to SW	"Relabelling to SW"	D_s
SW to VQ	"Relabelling to VQ"	D_v
SW to R	"Inhibition of SW's"	I_s
VQ to R	"Inhibition of VQ's"	I_v

where D_s, D_v, I_s and I_v are all defined on the positive reals.[7] Thus, in principle, a UI policy change can cause separating workers and/or firms to relabel their separations, and it can induce some workers who would have quit to remain employed. The policy changes may have opposing effects for different values of x_{it}, but it is only the net change that can be observed.

Next, suppose that the functions g and h outlined above are such that the fraction of the employed population assigned each of the three labels in a given month can be written:

$$SW_t/E_t = a_1 + b_1X_t + d_1P_t + v_{1t} \tag{2}$$
$$VQ_t/E_t = a_2 + b_2X_t + d_2P_t + v_{2t} \tag{3}$$
$$R_t/E_t = a_3 + b_3X_t + d_3P_t + v_{3t} \tag{4}$$

where v's are error terms. Note that, by the identity R + VQ + SW = E, equation (4) is redundant. The coefficients d_1 and d_2 are related to the relabelling and inhibition effects as follows:

$$d_1 = D_s - D_v - I_s, \text{ and} \tag{5}$$
$$d_2 = -(D_s - D_v) - I_v \tag{6}$$

Equations (5) and (6) imply the following. First, if in addition to the estimated parameters d_1 and d_2, we have no other prior information on the magnitude of I_s, I_v, D_s or D_v, then none of these effects are identified. Some identification can be obtained, however, if we assume that I_s and D_v are equal to zero. This seems plausible. We can think of no obvious reason why the changes to the UI benefits of voluntary quitters under study would inhibit firms from laying off workers.[8] Also, there is no obvious reason why laid-off workers would agree to be labelled as quits and lose their UI eligibility. Given these restrictions, the different scenarios discussed in the previous section have distinct implications for d_1 and d_2, some of which can be tested against one another. They are, in turn:

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[7] It is also possible to have "encouragement of VQs" and "encouragement of SWs." We restrict these to be zero, since the changes in question cannot be interpreted to promote separations.

[8] Bill C-113 reduced the benefit replacement rate for non-VQs from 60 to 57%. This could have had a negative effect on the UI take-up rate of non-VQs, but the firm-induced layoff rate is not likely to be affected.

A. Naive No-Response Model:

There is no behavioural response to the policy changes: $d_1 = d_2 = 0$.

B. A "Pure Relabelling" or Efficient Separations Model:

If there is no inhibition of quits (i.e. $I_v = 0$), equations (5) and (6) imply we should see $d_1 = -d_2 > 0$. Layoffs should increase, and the increase should be by the same amount that quits decrease in response to the policy change.

C. A "Pure Inhibition" Model:

If there is no net relabelling (i.e. $D_s = 0$), the coefficient on the policy dummy in the layoff equation, d_1, equals zero, and the coefficient on the policy dummy in the quits equation, d_2, equals the negative of the inhibition of quits.

D. Mixed Inhibition and Relabelling:

If both relabelling and inhibition of quits occur, then d_1 identifies relabelling, and $d_1 + d_2$ is a measure of inhibition.

5. Data and Descriptive Statistics

We find clear evidence that voluntary quitters, and those dismissed for cause, were in many cases disentitled from benefits by the legislation.

Monthly time series of counts of separations by type from HRDC's administrative ROE data are employed in this study.[9] The main advantages of the ROE data for this exercise are that it is collected as a 10% random sample of the complete inflow of job separations and that it categorises individuals according to the same reasons for separation that are used in administering the UI system. Over the period, the categories on the ROE form have been: shortage of work, strike or lockout, return to school, illness or injury, quit, pregnancy or adoption, retirement, work sharing, apprentice training, age 65, dismissal, leave of absence, and other. An ROE should be issued every time a worker separates from an employer, and it is a prerequisite for receiving UI benefits. Since dismissed workers were also penalised by Bills C-21 and C-113, it might also be of interest to consider this category of separations. We can do so here only in a very limited way, since dismissals were not listed as a distinct reason for separation until 1990.[10] Previously they had been included in the "other" category. To account for some of the heterogeneity in the labour market, the counts are disaggregated by sex and into 15- to 24- and 25- to 54-year age groupings. Unfortunately, no distinctions can be made on the basis of temporary versus permanent layoffs, industry, or region in the data extract available to us. The ROE is also matched with the UI administrative Status Vector (SV) file to determine the fraction of separations that result in a UI claim. None of the series were deseasonalised prior to our analysis.

Basic descriptive statistics for the ROE data are presented in Tables 1 and 2 by UI policy period: (1) in the baseline period before Bill C-21, from January 1980 to October 1990; (2) during the partial disentitlement while C-21 was in effect, from November 1990 to March 1993; and (3) following the total disentitlement, under Bill C-113, starting in April 1993 and extending 9 and 15 months for the SV rate and ROE data, respectively.[11] Descriptive statistics are of the total separation rate (S/E), which includes all 13 reason-for-separation categories, the voluntary quit rate (VQ/E), a composite group of the other and dismissed categories (O-D), and the rate of layoffs (SW/E). Employment (E) is the age-specific count measured by Statistics Canada's Labour Force Survey (LFS).

9 The authors would like to thank Ging Wong, Carol Guest, and Anne Routhier of HRDC for producing the administrative data and generously making it available to us.

10 Looking at the data, starting in 1990, it appears that it took close to a year from the time dismissals were first reported until they reached a stable level.

11 While the data file actually extended into 1995 when our sample was taken, the files take a long time to "fill up" because of lags in collecting and inputting the ROE data. The matched UI claim data take even longer to become reliable. Since it would be easy for lags in the data collection process to be spuriously interpreted as drops in the count of separations or claims, we end the series at very conservative dates.

While we find little or no effects of the partial disentitlement on separation behaviour, we do find that the subsequent complete elimination of benefits for (unjustified) quitters reduces the propensity of 15- to 24-year-old males and both 15- to 24-, and 25- to 54-year-old females to voluntarily leave their job.

From Table 1 it is clear that the separation rate of young male workers is almost twice that of prime-age men, with, on average, total separations equal to just over 7% of employment occurring each month. For females the gap is not quite as large, and the level is slightly lower, with the 15- to 24- and 25- to 54-year age groups' separation rates being 5.5 and 3.5%, respectively. It is difficult to draw inferences about how the levels change across the periods from the gross numbers in this table because the beginning of period 2 coincides quite closely with the onset of the 1991–92 recession, and the different periods cover different months of the year. These levels are therefore contaminated by cyclical and seasonal fluctuations which are quite large. They do, however, give us some sense of the magnitude of the flows in question. About 1.5 to 2% of employed 15- to 24-year-old workers quit their job each month, compared with 0.5 to 0.8% of the 25- to 54-year age group for both sexes. A similar, but not as pronounced, difference across the age groups is observed for the "other and dismissed" group; about 1.4 to 1.5% of the younger group is laid off each month, but the same number for the older groups is only about 0.7 to 0.8%. In contrast to the "quit" and the "other and dismissed" reasons for separation discussed thus far, where there have been much greater differences across age groups than between the sexes, the greatest differences in the layoff ("short work" or SW) rate is by sex. Men have much higher layoff rates than women. Those for women are in the 1.3 to 1.4% range, whereas men experience a rate of between 2 and 3%. This likely reflects differences in the industries in which men and women are employed. Standard errors of the series of 15- to 24-year-old are also uniformly larger than those for the 25- to 54-year age group for both sexes. This appears to be attributable to both the smaller underlying sample size and the greater intrinsic volatility of the labour force behaviour of the younger workers.

Matched to the ROEs are monthly counts of the UI claims by reason for separation, where the month associated with the claim is the month of the separation. Our definition of the UI claim rate is simply the fraction of ROEs that are used to establish claims.[12] Table 2 presents a summary of the VQ claim rates by period. The claim rate is seen to decline substantially across the periods, but as with the separation rates seasonal and cyclical factors confound inference. In contrast to the separation rates the largest difference in claim rates is by age, not sex, with the younger quitters having a lower claim rate.

[12] For a discussion of different definitions of the claim rate, see Storer and Van Audenrode (1995). A related issue is that several ROEs may be combined to initiate a single claim: that is, a claim may be based on multiple distinct jobs. If a claim is based on, say, two ROEs, then the numerator and denominator of our claim rate are both incremented by two; our measure is the fraction of job separations (ROEs) that are used to establish any claim.

6. Statistical Framework

To evaluate the policy effects and net out the possible confounding effects of cyclical and seasonal factors on the composition of separations, we developed a set of time series regressions, using the following as right hand side variables: seasonal dummies, measures of aggregate business conditions, and dummy variables for the different policy regimes affecting UI eligibility. In developing these models, we noted that the business cycle measures that we have are (potentially) closely related to the dependent variable. (Unemployment rates, for example, are clearly affected by the size and composition of separations.) For this reason, only lagged business cycle variables, rather than contemporaneous ones, were used to avoid simultaneity bias. The measures employed as independent variables are Statistics Canada's Help Wanted Index (HWI), always divided by 10, and the age-specific unemployment rate derived from the Labour Force Survey.

The analysis attempts to control for changes in the business cycle and seasonal effects that might be confounded with the effect of the policy change.

All of the times series used were tested for zero frequency unit roots, using augmented Dickey-Fuller tests with Phillips-Perron provisions for autocorrelation. A set of monthly dummy variables to account for the seasonality was also included. Following Davidson and MacKinnon (1993, p. 705), since these monthly dummy variables are non-stochastic and of the same order as the constant, their inclusion does not change the asymptotic distribution of the test statistics. Most of the series strongly reject the null of a unit root, although one of the ROE series has a test statistic that is very close to the critical value at the 10% level but which cannot reject the null hypothesis. This is not unexpected, given the large number of series tested. All of the series are taken to be stationary.[13]

In order to allow individuals to respond less than instantaneously to business cycle conditions and UI policy changes and to account for autocorrelation in the residuals, we included lagged dependent variables which can be interpreted as partial adjustment effects. Given all these considerations, we selected an autoregressive distributed lag model of the form:

$$y_t = a + \sum_{i=1}^{p} b_i \, y_{t-i} + \sum_{j=12}^{q} b_j \, y_{t-j} + \sum_{k=1}^{r} c_j \, x_{t-k} + \sum_{l=12}^{s} c_k \, x_{t-l} + \sum_{n=1}^{11} m_n \qquad (7)$$

$$+ \; d_1 t + d_2 P_2 + d_3 P_3 + u_t,$$

$$where: \; u_t \,\_\, IID(0,\sigma^2)$$

[13] This is consistent with Lee and Siklos (1991), who find no unit roots in the raw Canadian quarterly unemployment rate at either the zero or seasonal frequencies.

as the base from which to test down to the final model. In addition to t- and F-tests, the Akaike (1973) and Schwarz (1978) information criteria are used in model selection. We never tested for *p* or *r* larger than 3 and *q* or *s* larger than 15 for the lagged dependent (*y*) or business cycle (*x*) variables. The use of dummy variables, as opposed to seasonal differencing, is discussed in Harvey (1981). The policy variable approach is what Mills (1990, chaps. 12 and 13) terms intervention analysis; each policy indicator is set to 1 in its policy interval and is zero elsewhere. The summations of *j* and *l*, starting at 12, in equation (7) capture the monthly nature of our data and permit the most flexibility in reducing the parameters set.[14] In contrast, to facilitate comparisons, we wanted to use the same specification across similar equations where feasible and, therefore, include business cycle variables beyond the minimum number in some of the regressions.

A linear time trend (*t*) is retained in all of the regressions. As might be expected, given the potentially high partial correlation between a step function and a time trend, its inclusion affects the policy variables. While the Akaike and Schwarz criteria reject the trend for some of the series, it is maintained in all of the regressions to facilitate comparability.

Substantial testing for autocorrelation was carried out since; in the presence of lagged dependent variables, inconsistency would result had it been present. A variety of Breusch (1978) - Godfrey (1978) type tests using artificial regressions were conducted; those for residuals lagged once and 12 times are presented for each equation. Breusch and Pagan's (1980) Lemieux-MacLeod (LM) tests for autocorrelation (not shown) of various orders were also checked. While a small number of these test statistics are significant at the 10% level, this is considered to be normal, given that 24 lags were tested for each regression. We are satisfied that autocorrelation is not a problem. Testing, however, did reveal heteroskedasticity in many of the regressions. Heteroskedasticity-consistent estimates of the error terms have therefore been used throughout the paper, since the series are sufficiently short to make us reluctant to model the heteroskedasticity directly.

In equations with lagged dependent variables, the independent variables can be interpreted as having both long- and short-run effects (e.g. Johnston 1984, p. 350). The short-run effect is the coefficient on the variable. Because of the autoregressive nature of the equation, the long-run equilibrium is approached asymptotically. The specification is, however, restrictive in that all of the right-hand-side variables are forced to have the same adjustment process. Further,

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[14] A common parsimonious specification of the lag structure on either the dependent or independent variable is: $(1\text{-}B)^p(1\text{-}B^{12})$, where B is the lag operator. Our specification captures this intuition, but allows us to drop higher order lags (the interactions of the lags from the first term with B^{12}) if they are rejected by our information criteria. Conserving degrees of freedom is important in this application, since our series are relatively short.

the policy variables affect relatively few observations and are less numerous and less significant than the business cycle variables. The magnitude of the long-run effects of the policy variables should, therefore, only be viewed with caution.

7. Results

A. Claim Rates of Those Who Quit Voluntarily

In order to establish that the legislation had an impact on voluntary quitters' propensity to claim UI and therefore on their costs, we first examine the claim rates of that group. All four regressions are clearly autoregressive, but the specification criteria reject the annual lags. As can be seen from the Breusch-Godfrey tests, autocorrelation of the residuals is not a problem. Interestingly, there appears to be a decreasing trend in the claim rate independent of the policy changes for all but the prime-age male group. In the second policy period, all four groups experience a moderate but significant decrease in their UI take-up rates. In contrast, the decrease in the third period, relative to the first, is quite large, as might be expected, given the much more severe nature of the UI change. These latter decreases are quite large, given the levels seen in Table 2, but, as noted above, voluntary quitters are far from completely disentitled. Although the two older groups start from a higher claim rate in period 1, the coefficient on the period 3 dummy variables for the two 25- to 54-year age groups indicates a reduction of just over 7%, which is very large. Recall that these are only the short-run effects and that the long-run effects are much larger (equal to the period-specific coefficients divided by the sum of the coefficients on the lagged dependent variables).

B. Separation Rate Regressions by Reason for Separation

Tables 4 (for men) and 5 (for women) present the separation rate regressions. A more complex autoregressive pattern is required for these data than was required for the claim rates to remove the autocorrelation of the residuals. Four regressions are presented for each age-sex group. The first is the voluntary quit rate (VQ/E), which is central to the analysis. Next are the 2 groups in which relabelling is expected to occur. First is the combined "other and dismissed" category, which obviously is a problematic grouping, since it contains a disentitled label as well as the label into which we most expect any relabelling to occur. This combination is an unfortunate necessity since, as mentioned earlier, these groups only became distinct in 1990. Still, one effect might dominate, or relabelling might occur only from the voluntary quit and not the dismissed group; thus, the regression is included. More attention is given to this issue in the next subsection. The category of the short work (layoff) reason for separation follows in the next column; relabelling might also be expected for this group. Recall that both temporary and permanent layoffs are subsumed under this group, since they cannot be separately identified in our data. Finally, the total separation rate (S/E) regression is presented; it includes separations for all reasons.

Looking at the policy variable's coefficients, the 25- to 54-year-old men, in Table 4, exhibit no response to the legislative changes. The coefficients are not only insignificant, but the point estimates are quite small. For the 15- to 24-year-old males, the period 3 dummy in the voluntary quit regression is marginally significant ($P = 0.75$) and of a moderate size, suggesting that their quit propensity is reduced by the legislation. None of the policy coefficients in this group's other regressions are significant though. However, the point estimates in the total separation regression (S/E) are large and negative, as would be expected if there is a reduction in the quit rate with no relabelling. But the standard error is also quite large, so little can be said with confidence.

Both female age groups, in Table 5, have strongly significant period 3 dummy variable coefficients in the VQ regressions that are of an appreciable economic size. Further, the overall separation rate (S/E) for the young females has a negative period 3 coefficient that is economically large and statistically significant. This suggests that inhibition is very likely for this group.

For none of the three out of the four groups that exhibit a reduction in their quit rates is there any evidence of an increase in the layoff or the "other and dismissed" rates, as would be required for relabelling to be credible. In fact, the point estimates are not even positive, but small and insignificantly negative. Overall, the naive no-response model seems to work fairly well for the prime-age males, whereas both female age groups, as well as the younger males, appear to be inhibited from quitting by the total disentitlement.

C. "Dismissed" and "Other" Reasons for Separation

Prior to 1990, the "dismissal" category was included with the "other" one. Their division, unfortunately, coincides too closely with the introduction of the partial disentitlement for any meaningful analysis of it to be done for that policy change. A simple before-after comparison is, however, possible around the total disentitlement. The separation rates are shown in Table 6, and the claim rates are shown in Table 7. We have a strong desire to use all 15 months of data that are available after the change for the separation rates. Seasonal and cyclical factors are, however, quite important. Fortunately, business cycle conditions before and after the total disentitlement were remarkably stable. Seasonality is addressed by matching months, that is, ensuring that there are the same number from each month in the "before" and "after" groups. Two possible "before" samples are feasible; both will obviously contain the period

April 1992 to March 1993. The question is how to match the second April-to-June sequence and hence the total 15 months required. One alternative (Before 1) is to use April to June in 1992; the other is to count those months in 1993 twice (Before 2). The former has the advantage of not introducing the same error terms twice, while the latter is a period with more business cycle conditions similar to those of the period after the change. Notice how similar the unemployment rates and HWI are across the periods. Whichever specification might be preferred, each gives very similar results for the "dismissed" and "other" groups. None of the differences in the separation levels across the periods are significant; in fact, there is almost no change at all for either reason-for-separation group.

Unlike the separation rates, there are only 9 months of claim data in period 3, so only the matching months prior to the Bill are used. Both age groups exhibit a reduction in the claim rate for those who are dismissed, as evidenced in Table 7. This suggests that the financial incentives to motivate a behavioural change exist, although 34 and 16% of the 25- to 54- and 15- to 24-year groups continue to receive benefits following total disentitlement. In contrast, the claim rate of the "other" group is remarkably constant across the periods 2 and 3. These tables provide evidence against relabelling away from the UI disentitled group "dismissed for cause," who clearly suffer a financial loss because of the UI legislation. Either the label has value in itself that outweighs the costs of relabelling or the institutional constraints are sufficient to prevent side deals. The stability of the "other" group further strengthens the conclusion, suggested in the previous subsection, that the voluntary quit group is not relabelling to the "other" category following the complete disentitlement of UI eligibility (i.e. in period 3) where significant declines in the quit rate are observed. This is a strong point, since if firms are concerned about possible penalties, the "other" rather than the "short work" category is arguably the less problematic category into which workers might be relabelled.

8. Summary

This paper attempts to use recent legislative changes that reduced (Bill C-21), and then eliminated (Bill C-113) the UI eligibility of voluntary quitters and those dismissed for cause to shed some light on economic theories of job separations. We are also interested in measuring the impact of the legislation on the total expenditures of the UI system. We find clear evidence that voluntary quitters and those dismissed for cause were in many cases disentitled from benefits by the legislation. While we find little or no effects of the initial, partial disentitlement on separation behaviour, we do find that the subsequent complete elimination of benefits for (unjustified) quitters reduces the propensity of 15- to 24-year-old males and both 15- to 24-, and 25- to 54-year-old females to voluntarily leave their job.[15] In no case, however, can we detect a concomitant increase in the number of separations for other (UI-eligible) reasons, which should occur if workers and firms simply relabelled their separations in response to this legislation.

In all cases where a response is observed, it is an inhibition of quits. There is no evidence of re-labelling ... . Because total separations seem to be reduced by this legislation, the total cost savings to the UI system are likely greater than what is indicated by the reduction in the claim rate of quitters alone.

We conclude that the response of workers and firms to the introduction of very substantial asymmetries in the UI treatment of quits and dismissals versus separations is adequately described for prime-age men by a naive no-response model, in which separation behaviour remains unchanged and quitters experience real losses in UI benefits. For other demographic groups, a pure inhibition model, in which some workers remained with their former employer instead of separating and no relabelling occurred, is sufficient to account for the response patterns we observe. Overall, we see our results as providing little support for the efficient separations view of labour markets: in contrast, the fact that the labelling of separations appears to be unaffected, despite dramatic changes in financial rewards associated with those labels, suggests that for some reason other than UI, labels really *do* matter for labour market behaviour. Regarding the direct policy impacts of the legislative changes studied in this paper, we conclude that because total separations seem to be reduced by this legislation, the total cost savings to the UI system are likely greater than what is indicated by the reduction in the claim rate of quitters alone. This is because of the increased propensity of those who would have quit in the baseline period to remain in their jobs. Of particular note is that men and women and older and younger workers had very different responses to the legislation, and the policy cannot be said to have had one “singular” effect. Understanding the diverse impacts of legislative changes across the population is clearly an important issue.

[15] This contrasts with Ragan (1984), whose small sample size may have prevented him from separating the signal from the noise in his data.

Of particular note is that men and women and older and younger workers had very different responses to the legislation, and the policy cannot be said to have had one "singular" effect. Understanding the diverse impacts of legislative changes across the population is clearly an important issue.

Future work looking at the longer run effects of these legislative changes would be valuable, although difficult, given that subsequent changes to the UI system occurred shortly after the end of the period under study. In particular, evaluating the role and timing of any learning that might occur would be important. As well, while we do not observe any evidence of relabelling for the entire population, it is possible that it might occur in more depressed regions of the country, where there is both more knowledge of, and reliance on, UI. A study using more tightly defined economic regions would, therefore, be worthwhile; unfortunately, administrative data are not amenable to identifying the geographic region of separations that do not terminate in a claim.

Table 1
Means and Standard Errors by Period for the ROE Data

Variable/Period		Females 15-24	Females 25-54	Males 15-24	Males 25-54	Obs/ Period
VQ/E						
(80.01-90.10)	1	2.12 (.070)	.88 (.021)	2.09 (.073)	.73 (.020)	130
(90.11-93.03)	2	1.79 (.088)	.63 (.024)	1.62 (.079)	.51 (.020)	29
(93.04-94.06)	3	1.63 (.098)	.52 (.021)	1.57 (.090)	.49 (.020)	15
Oth & Dismiss/E						
	1	1.41 (.046)	.80 (.019)	1.56 (.041)	.70 (.012)	130
	2	1.46 (.094)	.84 (.037)	1.58 (.077)	.74 (.012)	29
	3	1.40 (.135)	.84 (.065)	1.47 (.121)	.67 (.026)	15
SW/E						
	1	1.44 (.043)	1.29 (.037)	3.06 (.097)	2.11 (.056)	130
	2	1.42 (.090)	1.44 (.088)	3.18 (.208)	2.55 (.148)	29
	3	1.31 (.131)	1.36 (.149)	2.58 (.275)	2.05 (.179)	15
S/E						
	1	5.69 (.181)	3.49 (.067)	7.45 (.195)	3.78 (0.59)	130
	2	5.42 (.372)	3.49 (.133)	7.15 (.403)	4.11 (.157)	29
	3	5.02 (.479)	3.24 (.226)	6.28 (.560)	3.42 (.191)	15

Table 2
Means and Standard Errors by Period for the Claim Rates

Variable / Period		Females		Males		Obs/ Period
CR of VQs		15-24	25-54	15-24	25-54	
(81.07-90.10)	1	24.53 (.681)	34.87 (.383)	27.58 (.727)	31.73 (.453)	112
(90.11-93.03)	2	17.33 (.437)	30.44 (.354)	19.70 (.447)	27.97 (.573)	29
(93.04-93.12)	3	10.59 (.450)	21.43 (.822)	12.18 (.477)	19.51 (.704)	9

Table 3
Claim Rate Regressions for Voluntary Quitters

	Females		Males	
	15-24	25-54	15-24	25-54
Y_{-1}	.29*** (.092)	.33*** (.074)	.16** (.074)	.40*** (.088)
Y_{-2}	.08 (.087)	.18** (.080)	.37*** (.088)	.15** (.072)
Trend	-.08*** (.017)	-.03** (.014)	-.04** (.016)	-.000 (.014)
UR_{-1}	-.45* (.258)	-.52 (.435)	-.11 (.169)	-.21 (.515)
UR_{-12}	-.02 (.245)	.51 (.311)	-.07 (.176)	.52 (.373)
HWI_{-1}	-.58*** (.110)	-.33*** (.086)	-.34*** (.126)	-.50*** (.160)
HWI_{-12}	.08 (.145)	.14 (.122)	.04 (.153)	.42** (.220)
P2	-1.88** (.878)	-1.71* (.930)	-1.99* (1.193)	-2.30** (1.189)
P3	-4.69*** (1.452)	-7.05*** (1.735)	-4.80*** (1.861)	-7.37*** (2.093)
R^2	.93	.91	.97	.84
σ	1.98	1.68	1.33	1.71
B-G (2) (*P* value)	1.14 (.565)	1.33 (.513)	2.58 (.274)	1.67 (.433)

Note: Standard errors in parentheses. One, two, and three asterisks represent significance at the 10, 5, and 1% level, respectively. There are 138 observations in each regression. Heteroskedasticity-consistent standard errors are employed. Eleven-month dummies and constant not shown.

Table 4
Male Separation Rates Based on ROE Data

	15-24				25-54			
	VQ/E	**O-D/E**	**SW/E**	**S/E**	**VQ/E**	**O-D/E**	**SW/E**	**S/E**
Y_{-1}	.40***	.20**	.36***	.23***	.18**	.01	.26***	.12*
	(.087)	(.082)	(.092)	(.089)	(.084)	(.072)	(.077)	(.074)
Y_{-2}	.13	.24***	.15**	.23***	.17**	.03	.18**	.21***
	(.081)	(.068)	(.074)	(.088)	(.069)	(.064)	(.089)	(.080)
Y_{-12}	.35***	.16	.29***	.28**	.31***	-.02	.23**	.17**
	(.084)	(.143)	(.082)	(.133)	(.078)	(.079)	(.089)	(.076)
Y_{-13}	-.23	-.17*	-.17***	-.21**	-.17**	-.18**	-.19**	-.23***
	(.082)	(.092)	(.077)	(.092)	(.077)	(.086)	(.077)	(.071)
Trend	.02**	-.002*	-.01	.05	.003	.001	-.002	-.01
	(.011)	(.001)	(.016)	(.042)	(.003)	(.004)	(.015)	(.022)
UR_{-1}	-.001	-.01	-.03	-.05	-.003	-.01	-.03	-.04
	(.012)	(.014)	(.019)	(.048)	(.004)	(.009)	(.027)	(.034)
UR_{-12}	-.03**	.02	.05**	.11**	.007*	.02**	.07***	.15***
	(.014)	(.012)	(.024)	(.059)	(.004)	(.009)	(.027)	(.039)
HWI_{-1}	.05***	.01	-.06***	-.01	.02***	-.01	-.05***	-.05***
	(.013)	(.010)	(.021)	(.044)	(.004)	(.004)	(.011)	(.015)
HWI_{-12}	.01	.01	.06**	.09*	-.003	.02***	.05***	.09***
	(.013)	(.011)	(.026)	(.050)	(.004)	(.005)	(.016)	(.023)
P2	-.13	-.05	-.08	-.32	-.01	.04	.005	.07
	(.118)	(.010)	(.158)	(.444)	(.029)	(.033)	(.112)	(.158)
P3	-.24*	-.15	-.20	-.77	-.04	-.02	-.16	-.28
	(.135)	(.119)	(.205)	(.559)	(.034)	(.043)	(.148)	(.212)
R^2	.96	.87	.95	.92	.95	.60	.94	.87
σ	.18	.17	.28	.70	.05	.08	.18	.29
B-G (2)	.47	2.98	2.16	1.66	4.19	0.43	2.41	2.99
(*P* value)	(.791)	(.225)	(.340)	(.435)	(.123)	(.806)	(.299)	(.224)

Note: Standard errors in parentheses. One, two, and three asterisks represent significance at the 10, 5, and 1% level, respectively. There are 161 observations in each regression. Heteroskedasticity-consistent standard errors are employed. Eleven-month dummies and constant not shown.

Table 5
Female Separation Rates Based on ROE Data

	15-24				25-54			
	VQ/E	**O-D/E**	**SW/E**	**S/E**	**VQ/E**	**O-D/E**	**SW/E**	**S/E**
Y_{-1}	.22*** (.087)	.10 (.082)	.22*** (.075)	.14* (.086)	.13* (.079)	.02 (.071)	.24*** (.084)	.09 (.073)
Y_{-2}	.21*** (.083)	.20*** (.071)	.21*** (.070)	.20** (.079)	.23*** (.063)	.10 (.076)	.04 (.067)	.13** (.068)
Y_{-12}	.33*** (.104)	.28 (.223)	.35*** (.118)	.38** (.192)	.40*** (.076)	.27*** (.087)	.41*** (.096)	27*** (.082)
Y_{-13}	-.17** (.079)	-.12 (.095)	-.23*** (.067)	-.17** (.086)	-.17** (.086)	-.31*** (.072)	-.33*** (.074)	-.32*** (.070)
Trend	.04*** (.011)	-.003*** (.001)	.01 (.007)	.08** (.034)	-.01 (.064)	.001* (.0004)	.002 (.007)	.01 (.014)
UR_{-1}	.01 (.021)	.02 (.022)	-.01 (.016)	.02 (.071)	-.01 (.006)	.01 (.009)	-.01 (.016)	.01 (.032)
UR_{-12}	.07*** (.021)	.05*** (.019)	.06*** (.020)	.21*** (.071)	.02*** (.006)	.03*** (.010)	.05*** (.016)	.14 (.035)
HWI_{-1}	.04*** (.013)	.01 (.011)	-.02** (.009)	.02 (.035)	.02*** (.004)	.01* (.003)	-.02*** (.005)	.003 (.010)
HWI_{-12}	.02 (.014)	.02* (.011)	.03** (.011)	.09** (.038)	-.001 (.004)	.01*** (.004)	.02*** (.007)	.05*** (.014)
P2	-.18 (.127)	-.05 (.105)	-.02 (.077)	-.36 (.360)	-.03 (.031)	.03 (.040)	.01 (.064)	.08 (.139)
P3	-.39*** (.149)	-.19 (.117)	-.13 (.099)	-.87** (.445)	-.11*** (.041)	-.03 (.050)	-.07 (.086)	-.24 (.181)
R^2	.94	.89	.90	.92	.95	.90	.94	.91
σ	.20	.18	.16	.65	.06	.07	.12	.26
B-G (2) (*P* value)	3.92 (.141)	2.34 (.309)	7.22 (.027)	2.82 (.243)	.83 (.660)	0.13 (.938)	2.82 (.243)	.45 (.799)

Note: Standard errors in parentheses. One, two, and three asterisks represent significance at the 10, 5, and 1% level, respectively. There are 161 observations in each regression. Heteroskedasticity-consistent standard errors are employed. Eleven-month dummies and constant not shown.

Table 6
Means (Standard Errors) of "Other" and "Dismissed" Categories' Separation Rates Before and After Bill C-113

	15-24			25-54		
	After	**Before 1**	**Before 2**	**After**	**Before 1**	**Before 2**
Females						
Other/E	1.12 (.132)	1.14 (.123)	1.14 (.123)	.76 (.064)	.79 (.066)	.79 (.067)
Dismissed/E	.28 (.009)	.29 (.012)	.29 (.010)	.35 (.010)	.37 (.017)	.37 (.017)
UR	15.04 (.246)	14.81 (.278)	15.10 (.263)	9.48 (.159)	9.36 (.168)	9.43 (.141)
HWI	89.2 (.857)	89.2 (1.559)	86.3 (.300)	89.2 (.857)	89.2 (1.559)	86.3 (.300)
Males						
Other/E	1.08 (.119)	1.11 (.106)	1.11 (.107)	.56 (.026)	.59 (.024)	.59 (.027)
Dismissed/E	.39 (.008)	.42 (.016)	.40 (.014)	.58 (.018)	.59 (.021)	.58 (.020)
UR	20.01 (.529)	20.10 (.266)	20.42 (.401)	10.16 (.267)	10.34 (.274)	10.62 (.240)
HWI	89.2 (.857)	89.2 (1.559)	86.3 (.300)	89.2 (.857)	89.2 (1.559)	86.3 (.300)

Note: Standard errors in parentheses. Each cell represents the average of 15 months of data. The periods are defined in the text.

Table 7
"Other" and "Dismissed" Categories' UI Claim Rates Before and After Bill C-113

	15-24		25-54	
	After	**Before 1**	**After**	**Before 1**
Females				
CR Other	25.67	25.86	50.44	49.62
	(2.118)	(2.125)	(1.226)	(1.247)
CR Dismissed	16.37	21.63	36.87	41.81
	(.771)	(1.260)	(.524)	(.867)
UR	14.88	15.15	9.65	9.43
	(.359)	(.364)	(.203)	(.145)
HWI	87.11	85.78	87.11	85.78
	(.455)	(.278)	(.455)	(.278)
Males				
CR Other	25.99	27.18	43.42	45.40
	(1.931)	(2.243)	(.931)	(1.509)
CR Dismissed	15.53	23.80	32.64	41.94
	(.407)	(1.491)	(.756)	(.663)
UR	19.44	19.62	9.88	10.24
	(.590)	(.452)	(.248)	(.269)
HWI	87.11	85.78	87.11	85.78
	(.455)	(.278)	(.455)	(.278)

Note: Standard errors in parentheses. Each cell represents the average of 9 months of data. The periods are defined in the text.

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