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ABSTRACT

This paper presents results from a 1971 natural experiment carried out by the Canadian government on the unemployment insurance system. At that time, they dramatically increased the generosity of the system. We find that the propensity to collect UI increases with a first time exposure to the system. Hence as more individuals experience unemployment their lifetime use of the system increases. This supply side hysterisis effect may explain why unemployment has steadily increased over the 1972 - 1992 period, even though the generosity of unemployment insurance did not.

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1 Introduction

A recurrent theme in policy debates regarding social welfare programs is the relationship between benefits and the disincentives to work (Moffitt (1992)). In the case of unemployment insurance, there is a great deal of evidence suggesting that it tends to increase both the duration of unemployment and the probability of becoming unemployed.[1] Moreover, work by Katz and Meyer (1990), Corak (1993) and Meyer and Resenbaum (1995) find evidence that workers adjust their labor supply so that unemployment insurance may subsidize part year work. Despite this micro-econometric evidence, there does not seem to be a direct relationship between unemployment insurance benefits and the recent secular rise in the unemployed in the OECD[2]. Lindbeck (1995) has pointed to social norms and the sluggish response of individual labor supply to changes in incentives as a potential source of "supply side hysteresis" that may help explain this secular trend.

The goal of this paper is to build upon this idea, and see whether recent trends in the use of UI in Canada can be explained using a simple adaptive learning model. In a standard labor supply framework one supposes that changes in worker alternatives results in an immediate behavioral response. Moreover, whether or not the individual has had experience with these alternatives is irrelevant to his or her choice. However, there is a large body of evidence demonstrating that experience context does matter for human decision making.

This implies for example that a worker, even one who knows and understands the parameters of unemployment insurance, is likely to behave very differently from an individual who has received unemployment insurance in the past, and hence has had direct experience with the state of unemployment. This distinction is hi-lighted in psychological models of learning where laboratory evidence suggests that a person's behavior in choice situations changes with repeated experience.[3] With sufficient experience behavior converges to what can be identified as the utility maximizing choice.

It is well recognized in the economics literature that it takes time for individuals to find an optimal response, and hence short run supply elasticities

[1]See for example Topel (1983), Meyer (1990) for the United States, and Ham and Rea (1987) and Green and Riddell (1993) for Canada.

[2]See for example Layard, Nickell and Jackman (1991).

[3]The learning literature is very large and so a complete citation is not possible. See Wickens (1992) for a useful overview. Bandura (1986) is classic reference on social learning.

are likely to be smaller than long run elasticities.[4] This issue that we wish to address in the study is the importance of this lagged adjustment in the case of labor supply responses to changes in the unemployment insurance (UI) parameters. A number of studies have shown that individuals adjust their labor supply as a function of the parameters of the system in the predicted direction. However, in the case of the Canadian unemployment system, UI use and unemployment increased steadily from 1971 until 1990, though during this period benefit level were constant or falling (see figures 1 and 2).

The hypothesis we wish to explore is that workers did not immediately respond to the large increase in benefits that occurred in 1971. Rather, when workers experienced unemployment for the first time, due to natural turnover or a recession, this would expose them to the UI insurance and cause them to begin exploring ways to use the UI system as a subsidy to part year work. This was possible due to a number of the rule changes that occurred in 1971. First, coverage of the unemployment insurance system was expanded from 68% to 96% of the work-force. The number of weeks of work needed to qualify for benefits was reduced from 30 weeks in a two year period to 8 weeks in a single year. The maximum number of weeks during which benefits could be received was increased to 28 or 44 weeks, depending on the regional unemployment rate (in high unemployment regions benefits are more generous). The replacement rate was increased from 57% of previous earnings to 66% (or 75% if claimant had dependents). The effect is summarized by the subsidy rate (replacement rate*number of weeks of benefits/number of weeks needed to qualify), and is illustrated in Figure 1 for the period 1946 to 1992.

Given the much shorter qualification period, this creates an incentive to for individuals to tailor their behavior to the parameters of the system, resulting in what many Canadians affectionately called the *lotto 10/42.* Work 10 weeks and win 42 weeks of paid holidays. The immediate impact of these changes are readily visible in Figure 2, illustrating the evolution of use and cost of the UI system. In 1972 there was a dramatic increase in both the number of recipients of UI and the total cost of the program. However both of these indexes continued to increase over the 1972-1992 period, with the fluctuations in outlay explained by the business cycle (for reference the unem-

[4] See Alchian (1950) for a nice dicussion of adjustment to utility maximizing behavior. The fact that costly adjustment results in different long run and short run elasticities of response is an old idea that goes back to at least Marshall (1948).

ployment rate is also illustrated in Figure 2). The cost of the unemployment insurance system as a fraction of total labor income increased from about 3% in 1972 to 5% in 1991, a 66% increase. This occurred even though the disincentives for use were either constant or decreased.[5]

To see how conditioning may help explain this observation consider a cohort of workers that were working full–year in 1971, the date of the large scale change to UI. Over time more and more workers from this cohort will have experience unemployment and possibly received some UI. We find that the probability that an individual will receive UI increases when he or she has had experience with the system in the past, implying that the fraction of workers receiving UI should also increase over time, even though the parameters of the system are unchanged.[6] This also creates a hysteresis effect during recessions. A recession increases the number of workers who leave full–year employment and experience unemployment and UI. The conditioning or learning effect that we identify implies that at the end of the recession the equilibrium number of workers who are unemployed should be greater, and hence the economy does not return to its pre-recession equilibrium level of employment. This may account for the rising trend in the unemployment rate illustrated in Figure 2.

To ensure that we are identifying a behavioral change rather than a structural change in the economy we follow the behavior of individual workers using a large administrative data set. In addition to the usual controls, we are able to control for individual effects, year effects and industry effects. Using a random coefficients probit we find that first time treatment with the unemployment insurance system permanently increases future use. In the next section we present a discussion of the model. Section 3 presents that data, while section 4 provides some simple results using a difference-in-differences approach. Section 5 discusses the estimation strategy, while the results are discussed and summarized in sections 6 and 7.

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[5]The earnings replacement rate of UI was reduced to 60% in 1978 (from 66% in 1971), 57% in 1990, and 55% in 1993; the minimum qualifying period was extented to 10 weeks in 1978 (from 8 weeks in 1971), and to 12 weeks in 1990.

[6]Heckman and Borjas (1980) call this effect "occurence dependence".

2 The Effect of Unemployment Insurance on Recipiency

For purposes of exposition it is useful to present a simple formal model that captures many of the incentive effects of UI. Suppose that at time t all workers are completely characterized by their base productivity denoted θ_t, and the value of home production denoted by u_t. The base productivity of a person is a composite variable representing the market value of education, occupation choice and innate skills. Since this variable represents a market value, it will vary over time due to on–the–job training, technical change etc.

In addition to a worker's base productivity, the wage of a worker is also affected by business cycle shocks, including seasonal shocks. Letting η_t denote the size of this shock in period t, suppose that the wage of a worker is given by:

$$w_t = \theta_t + \eta_t. \tag{1}$$

Abstracting away for the time required for search, individuals choose employment if and only if the wage is greater than the value of home production or $w_t \geq u_t$. Let $E(\eta) = \{(\theta, u) \mid \theta + \eta \geq u\}$ denote the set of worker characteristics that would result in full time employment in the absence of an unemployment insurance system, while $O(\eta)$ denotes the set of worker characteristics who are out of the labor force.

Workers who are receiving UI are supposed to be actively searching for work, and hence should respond positively to the question "Are you searching for a job". Now suppose that search costs are negligible so that individuals can find work immediately, then the introduction of UI can result in an increase in measured unemployment as follows. To simplify matters as much as is possible suppose that once an individual has x weeks of insured earnings, she or he is eligible for y weeks of benefits equal to a fraction α of the previous wage. An individual with characteristics (θ, u) considers one of the following three options (with the t subscripts dropped for convenience):

1. Work full–year at a wage of $w = \theta + \eta$.
2. Exit the labor force to receive a benefit of u.
3. Work the number of periods necessary collect UI and then collect the

benefits until exhaustion before beginning to work again[7]. Letting $\delta = x/(x+y)$ be the fraction of time the worker must be employed to earn y weeks of benefits we have that the return to the individual is given by $ui = \delta \cdot w + (1-\delta)(u + \alpha \cdot w) = (\delta + (1-\delta)\alpha)w + (1-\delta)u$. Let us call a worker that follows this strategy a part year worker.

Individuals choosing to work part year are from the set of characteristics:

$$U(\eta, \delta, \alpha) = \{(\theta, \eta, u) \mid ui \geq \max\{w, u\}\}. \tag{2}$$

Notice that individuals with these characteristics are taken from both the set E and O, hence the addition of a UI system creates an incentive for some full time employees to work part year, while some individuals who are out of the labor market, enter to work part year. Thus we have the following observation.

Observation If workers receiving unemployment insurance report that they are looking for work, then the addition of an unemployment insurance system increases both measured unemployment and labor force participation.

This observation is consistent with the finding of Card and Ridell (1993) that though unemployment grew in Canada during the 1980's, so did labor force participation, particularly by women. An actual example is employment in the arts. In Canada there a great deal of sectorial employment, such as summer theater companies, that permits the entry of businesses that survive because its employees are able to receive UI during the winter months. The implications of parameter changes are summarized in the following proposition.

Proposition 1 *Decreasing the entry requirement, δ, or increasing the replacement rate, α, has the following effects:*

1. *Participation in the labor force increases.*

[7]Given the linearity of the system, it is not difficult to show that if agents choose to cycle in and out of UI then they will only work the minimum number of periods needed to qualify. Exactly the same form of behavior is optimal with the more complex system one observes in practice, though with fluctuating labor demand they may work for more periods, to qualify for a greater number of periods of benefits.

2. *Number of individuals receiving unemployment insurance per year increases.*

Proof. This result follows immediately from the observation that for $\delta \leq \delta'$ and $\alpha \geq \alpha'$ then $U(\eta, \delta, \alpha) \subseteq U(\eta, \delta', \alpha')$, with strict membership if $(\delta, \alpha) \neq (\delta', \alpha')$. ∎

Our data are a panel of individuals, and hence we cannot follow changes in employer behavior. It is worth emphasizing that the supply side behavior described here must also be consistent with changes in demand side behavior. Firms, particularly those employing seasonal workers, are also learning about and adapting to changes in UI parameters. The model illustrates that an increased generosity of UI decreases the cost of seasonal labor supply, and hence increases the number of seasonal jobs. Due to data limitations we are not able to look at this question, however our analysis is not inconsistent with changes in employment pattern by firms in response to UI changes.

2.1 Hysteresis

When the major change to the unemployment insurance system occurred in 1971, this increased the incentives for individuals to subsidize part–year employment with UI. Figure 2 shows that the rate of use of UI increased sharply between 1971 and 1972. It then followed an upward trend between 1972 and 1992 even though the underlying incentives (subsidy rate) were constant or declining. In this paper we present evidence that individuals permanently changed their behavior after a bout of experience receiving UI.

While the immediate impact effect of the change in rules is consistent with the standard economic model of incentives, the fact that use of the system increased over time while benefits if anything decreases is not. The foundations for that model are based upon Savage (1972)'s theory of decision making where it is assumed that each agent understands the consequences of each action. As both Knight (1921) and Simon (1956) have emphasized, individuals are not able in general to explore all possibilities before making a decision, but rather consider the consequence of actions that they perceive as salient for the current the decision.

The two most important mechanism for learning are experience and social learning. In the many laboratory studies of human behavior we find that individuals adjust their behavior in the direction of increased rewards, but this response is not immediate. Rather individuals modify their behavior with repeated trials with a given situation.[8] In the context of the unemployment insurance system, this implies that the possibility of cycling in and out of the unemployment insurance system is not salient for the decision making of individuals who work full time. However, individuals who lose their jobs, for what ever reason, would then apply for UI and become aware of the parameters of the system, and hence adjust their behavior appropriately.

In a recent study sponsored by Human Resources Canada, Bloom, Fink, Lui-Gurr, Bancroft and Tattrie (1997) find evidence that even in 1995, displaced workers who had been working full time for many years had less knowledge of the parameters of the UI system than did repeat users.[9] This study of the consequence of a re-employment subsidy for worker behavior finds that the re-employment bonus had little impact for repeat UI users, while there was some preliminary evidence that displaced workers might alter their behavior as a consequence of the supplement. Together these results suggest that even in 1995, there were significant differences in the knowledge and response rates of first time UI users compared to repeat users.

In our study of workers from 1971 until 1992, we look for evidence of a hysteresis effect. That is we explore the extent to which displaced workers adapt their behavior as a consequence of experience with UI, and as a consequence of this experience are more likely to become a repeat UI users. One reason that the behavior of displaced workers and repeat UI workers is different may be due to selection effects. We deal with this problem by exploiting the panel aspect of the data, combined with the large sample size to correct for individual differences in unobserved characteristics.

If experience with the system does indeed alter one's behavior, then after the initial increase in benefits, we would expect the equilibrium unemployment rate to increase over time as more people experience unemployment. The effect would be particularly evident during a recession, where we would expect that the equilibrium level of unemployment to ratchet up after each downturn. This is what we observe in figure 2.

As Bandura (1986) has emphasized, social learning has an important impact on behavior. In the context of UI, we would expect the impact of individual learning to be lower for those social groups that have high levels of UI recipiency. In those groups individuals learn about UI from their friends

[8]This literature is too vast adequately cite here. One textbook that provides a useful overview of the literature is Wickens (1992). See MacLeod (1998) for a formal economic model of this phenomena.

[9]Chapter 7.

and spouses, and hence are able to adapt their behavior given full knowledge of the alternatives. An implication is that if we can identify coherent social groups with significant UI recipiency, then the treatment effect from the first spell of unemployment for a member of such a group should be smaller. One implication for our data set is that the learning or treatment effect should be smaller is areas with high repeat use, such as the Maritime provinces, compared to low unemployment provinces such as Ontario or Alberta.

The next section describes the administrative data set used in this study. Section 4 uses a difference–in–differences approach to see if previous experience has an effect on the probability of use. Section 5 describes a random effects probit model in which the probability of using UI is modelled as a function of a dummy variable indicating if the worker has experienced at least one spell of UI in the past. If the coefficient of this parameter is positive, then this implies that the probability of receiving UI has permanently increased after this experience.

3 Data and Descriptive Statistics

We analyze the dynamics of UI recipiency in Canada using a large longitudinal data set for the years 1972 to 1992. To create this data set, we combine the "Status Vector File" of Employment Immigration Canada (EIC) from 1971 to 1993 with income tax data from the "T4 Supplementary File" of EIC from 1972 to 1991.

These two data sets are complementary. The Status Vector File contains data pertaining to all unemployment insurance claims established by each claimant whose Social Insurance Number (SIN) ends in the digit '5' (10 percent of the population). It also contains some demographic information such as the age and sex of the claimant as well as the UI region in which the claim was filed. The drawback of this file is that it has very little information on what happens to claimants before and after their UI claims.

By contrast, the T4 Supplementary File provides no demographic information on workers but it contains records of all sources of T4 income for workers whose SIN ends in the digit '5'.[10] It also provides information on the location and industry of the employer that issued the T4. This file can be used to identify whether a UI claimant received some labor income before and after each UI spell. By combining the two files, it is thus possible to

[10] The T4 tax form is the Canadian counterpart to the U.S. W2 form.

reconstruct a detailed longitudinal history of UI and labor income recipiency from 1972 to 1991 for a large sample of workers. Note, however, that the sample only includes individuals who established at least one UI claim between 1971 and 1993.[11] This is a potential source of selection biases that we address in the empirical analysis.

More precisely, we extract from the Status Vector File all claims that eventually led to the payment of regular UI benefits in the first week of payment. We exclude workers filing claims for special benefits (seasonal, sickness, maternity, etc.) from the analysis. We use the benefit period commencement of each claim to identify the year in which the UI spell started. Once we have identified all the years from 1972 to 1992 in which at least one spell started, we merge this information to the information contained in the T4 Supplementary File. From this file we know when a person first received T4 income. This enables us to identify a "year of entry" in the sampling universe for each UI claimant.

Table 1 indicates that for close to half of the male UI claimants (slightly less for women), the year of entry is simply the year in which the T4 file starts, that is 1972. For most of these workers, the year of entry is really a year of entry in the sample as opposed to a year of entry in the work-force. For the rest of the sample of claimants, what we call the "year of entry" may either be a true year of entry in the work-force or the year of "re-entry" for people who earned some T4 income before 1972 but no T4 income in 1972. Table 1 nevertheless indicates that the age of entry of half of the claimants (age at which T4 income is first recorded) is 20 or less. This suggests that most of the 50.7 percent of men and 48.6 percent of women whose year of entry is 1973 or later are not re-entrants in the work-force.

Why is it so important to know when a claimant first "entered the work-force"? The answer is that if we want to find out how previous use of the system affects how long it takes before the person receives UI again, we need to know how long it took before the person used UI for the first time. Since different people join the work-force at different times, we need to have some idea of when the person entered the workforce to compute the duration before the first UI spell. Our measure of entry is imperfect since some students earn T4 income during summer jobs even if they have not made a "permanent"

[11] A comparison between our administrative data set and the 1981 Canadian Census indicates that over 50 percent of the population established at least one clain between 1971 and 1993. This fraction is as high as 75 percent for some younger cohort of males (those born in 1951).

transition to the work force. We nevertheless feel this is the best we can do with the available data. We will discuss these issues again in Section IV.

We also use information from the T4 Supplementary File to compute a coarse measure of eligibility to UI. The point is that someone who has not worked at any time during year t and year t-1 can never qualify for a new UI benefit period starting during year t. This UI eligibility variable can thus be used to correct for potential estimation biases likely to arise when people exit the workforce temporarily or permanently because of early retirement, illness, etc.

Once the year of entry has been identified in the T4 File, this information is merged to the information about demographic characteristics and UI spells from the Status Vector File. The two files are combined into a yearly panel data file. There is one observation per person in the panel for each year (from the year of entry to 1992). For each observation we know whether the individual received some T4 income and whether he or she initiated a UI spell during the year. Note that we do not keep the observation in the sample when the person is under 15 or over 65 years old. We also exclude people born before 1912 or after 1972. The resulting sample contains 10,253,535 observations for 618,911 men who have started a UI spell at least once in the years 1972 to 1992. The comparable sample of women contains 8,074,326 observations for 494,697 women.

A few statistics on the composition of the men's sample are reported in Table 2. The average age in the sample is slightly under 35. The regional composition of the sample more or less reflects the relative weight of each province in the national population. Note however, that Quebec and especially the Maritimes are over-represented. This simply reflects the fact that a larger fraction of workforce have received UI at least once in these provinces than in provinces west of Quebec.

The table also shows that men in the sample received at least some T4 income in four years out of five and started a UI spell in one year out of five. These proportions are slightly smaller for women. The probability of receiving a UI claim is disaggregated by provinces and by year in the second column of Table 2. Once again, there are important East–West differences as people in Quebec and the Maritimes are more likely to start a UI spell than people in other provinces. Not surprisingly, the probability of starting a spell of UI is also counter-cyclical.

4 Longitudinal Analysis: Difference-in-Differences Estimates

The descriptive statistics reported in Table 2 do not exploit the longitudinal aspect of the data, nor do they give any indication on how, for example, the past history of UI recipiency is related with the current probability of starting a UI spell. In what follows we present some descriptive statistics that highlight the dynamic aspects of UI recipiency.

One advantage of working with a large data set like ours is that it is easy to control for observed characteristics by dividing the sample in homogeneous groups of people and doing the analysis separately for each group. In what follows we select three cohort of men and three cohorts of women to present some descriptive evidence focusing on the longitudinal aspect of the data. The six cohorts consist of men born in 1931, 1941, and 1951 respectively. The three particular birth years are selected so that people are old enough to be in the workforce in 1972 and young enough to still be in the workforce in 1992.

If learning effects are important, a given experience with the UI system should have a larger impact on the future probability of receiving UI for people who had no previous experience with the UI system than for people who had some previous experience. One simple measure of the magnitude of learning effects is thus obtained by comparing the evolution in the probability of UI recipiency of one group of workers that have no previous UI experience with an otherwise comparable group of workers who have had some previous experience.

More concretely, consider a fixed cohort of workers at the beginning of the 1981–83 recession. Some of these workers have received UI in the past while some others have not. Focusing on the 1981–83 period is an interesting "natural experiment" since it "exposed" many workers to unemployment and UI recipiency for the first time in their careers. If learning is important, the post-recession probability (e.g. 1984-86) of these workers to receive UI should be higher than the probability that would have prevailed if they had never been exposed to UI. Although this hypothetical probability cannot be directly observed, a control group of workers that were exposed to UI before the recession can be used to calculate the change in the probability of receiving UI between the recession (1981–83) and the post–recession period (1984–86) that would prevail in the absence of learning effects. The point is that since these workers have already been exposed to the system, a new exposure during the recession should not have any additional effect on the future probability of receiving UI. The change in probability for workers that have been exposed before is thus net of learning effects.

This suggests a simple "difference–in–differences" estimator of the effect of learning on the probability of using UI. Panel A of Table 3 reports separate difference–in-differences estimates of the effect of learning for the cohorts of men and women born in 1931, 1941, and 1951. Column 1 to 3 indicate the probability of receiving UI at least one during the periods 1981-83, 1984-86, and 1987-89, respectively. These probabilities are simple empirical frequencies for all individuals of the relevant cohorts in the administrative data. While this probability decreases sharply in the post-recession years for workers who had been exposed to UI before the 1981-83 recession (rows 1b, 2b, and 3b), it remains relatively stable, at least for the 1984-86 period, for workers who had never received UI before the recession. Relatively speaking, a first exposure to UI during the recessions increases the probability of receiving UI in the future. These results suggest that part of the upward trend in the use of UI is due to the fact that exposure to the system permanently increases the probability of future use.

One potential problem with this exercise is that, by definition, all individuals in our administrative data set have to receive UI at least once between 1972 and 1992. By definition, individuals who had not experienced UI before 1981 have to do so between 1981 and 1992. Otherwise, they would not be included in the sample. One way to think about this problem is that we are missing observations for individuals who never collect UI. This problem is easily fixed since the size of this "residual" category is the difference between the total size of the corresponding population and the number of individuals included in the administrative data. For instance the number of men born in 1931 included in our data set represent about a half of the total population of men born in 1931 enumerated in the 1981 Canadian Census.[12] We can thus recompute the probabilities of receiving UI for individuals with no previous experience by incorporating the information from the Census in the calculations.

These "corrected probabilities" are reported in Panel B of Table 3. The

[12]Of course, these caculations take account of the fact that the public use files of the 1981 Census are a 2 percent sample of the population, while the administrative data set is a 10 percent sample.

corresponding difference-in-differences estimates suggest that a first exposure to UI permanently increase the probability of future use in a three years period by 7 to 11 percentage points, which is quite significant from an economic point of view.

5 Estimation by Random Effect Probit

In order to look more formally at the dynamics of UI recipiency, consider the following model for the probability that individual i starts a spell of UI in period t:

$$\Pr(U_{it} = 1 | U_{it-1}, x_{it}, L_{it}) = F(\alpha_i + \delta_t + \gamma U_{it-1} + x'_{it}\beta + \theta_0 L_{it} + (x'_{it}\theta_1) L_{it}), \quad (3)$$

where $i = 1, .., N$, $t = 1, .., T$, F(.) is a cumulative distribution function. In this paper, we simply assume that F(.) is a unit normal. The cumulative distribution function F(.) is increasing in its arguments. An increase in arguments such as α_i or $x'_{it}\beta$ will thus increase the probability that individual i starts a spell of UI in period t. The arguments in the function F(.) are listed below:

- U_{it}: dummy variable equal to one if individual i starts a UI spell during year t,
- α_i: time invariant random effect,
- δ_t: aggregate time effect,
- x_{it}: vector of covariates including the age of person i, the parameters of the UI system in individual i's region at time t and industry dummies.
- L_{it}: a variable indicating whether or not individual i has "learned" how to use the UI system at time t. In the simplest version of the learning model, this variable is one if the individual has received unemployment insurance in the past, and zero otherwise.

In what follows, we refer to L_{it} as a learning variable although, more generally, it can simply be viewed as a variable indicating whether the person has ever collected UI in the past. The parameter θ_0 relates the learning variable to the probability of receiving UI, while the vector of parameters θ_1

indicates whether variables in the vector x_{it} (such as the replacement rate of UI) have a different impact on UI recipiency for people who have learned than for people who have not learned. In other words, θ_1 captures possible interactions between learning effects and variables such as the parameters of the UI system.

To understand why learning effects can be interpreted as "hysteresis" effects in the use of UI, consider the simple case in which θ_1 is equal to zero. From the definition of the learning variable L_{it}, it is clear that receiving UI for the first time switches the learning variable L_{it} from 0 to 1 and thus permanently increases the probability of receiving UI, provided that θ_0 is positive. This basic property of learning effects remains when θ_1 is different from zero except that the size of the "hysteresis" effect then depends on the value of variables such as replacement and subsidy rates of the UI system.

One difficulty in isolating the importance of learning effects is that many other factors may explain why the history of UI recipiency of a given person i, $(U_{i1}, ..., U_{it-1})$, may help predict whether the person will receive UI in period t. To see this, note that except for the learning term $\theta_0 L_{it} + (x'_{it}\theta_1)L_{it}$, equation (3) is a standard statistical model for a binary variable with panel data (see Chamberlain (1980), Heckman (1978), and Heckman (1981a)). In such models, there are two reasons why the history of UI recipiency of a given person i, $(U_{i1}, ..., U_{it-1})$, may help predict whether the person will receive UI in period t. First, certain individuals may be more likely to be unemployed and to receive UI because they are less–skilled and/or they have a high marginal valuation of leisure. These factors are summarized by the random person effect α_i. Since this random effect is by definition fixed for a given person i over time, it increases the probability that the person will receive UI in any time period. As a result, previous use of UI will be strongly correlated with present use of UI since some people are always likely to receive UI (high α_i), while some people are not (low α_i). This could give the misleading impression that previous use of UI is a *cause* of the present use of UI. This is called the problem of *unobserved heterogeneity*.

The history of UI recipiency of a given person i may also help predict whether the person will receive UI in period t because of the presence of the lagged dependent variable U_{it-1} in equation (3). Note that in the estimation we consider models that include further lags of U_{it-1}. We call this particular form of state dependence an *adjustment lag*. It is natural to expect an adjustment lag in the data for a variety of reasons. For instance, it is well known that the rate of job separation is higher in the first year on a job than in subsequent years (see ?). In other words, a job separation is more likely to occur at time t if there was also a separation at time $t-1$ than otherwise. Since UI recipiency is positively correlated with job separations, a UI spell is more likely to be observed in year t if $U_{it-1}=1$ than if $U_{it-1}=0$. Alternatively, workers who have lost some specific human capital because of permanent job displacement may be more likely to be unemployed than if they still had that specific human capital. A UI spell due to permanent job displacement may thus increase the future probability of receiving UI. They key difference between an adjustment lag and learning is that the adjustment lag only temporarily affects the probability of receiving UI, while learning effects are permanent.

It should thus be clear that the mere fact that the history of UI recipiency $(U_{i1},...,U_{it-1})$ may help predict whether the person will receive UI in period t is not a proof of the existence of learning effects. The econometric challenge consists in isolating learning effects from the presence of unobserved heterogeneity and adjustment lags. We discuss the econometric strategy in detail below.

The increase in use of UI may also be due to structural change both over time and by industry. One explanation for the changes may be due to a shift from industries employing workers full–year, to industries that encourage part–year work. To deal with this potential explanation we include a full set of time and industry controls. Thus if a worker is unemployed for the first time, and then moves into an industry that has a great deal of part–year workers, then even though the use of UI goes up, the estimated learning parameter in this case would be zero.

It is not possible to rule out learning on the part of the firm and worker together. Since UI is not experience rated in Canada, it is possible that firms have an incentive to learn along with the worker the best way to use the UI system to subsidize part–year work. However, if this effect is occurring across all firms over time, then the time controls would tend to eliminate any measured learning effects. Given this set of controls, any positive learning effect is evidence that experiencing a spell of UI has the effect of increasing future use. Given that we do not observe all the factors that affect a worker's decision, our results can be expected to underestimate the importance of learning and slow behavioral change.

One final remark is that the variable L_{it} is only a crude measure of learning. People may also learn how to use the UI system through friends and family. This yields the interesting prediction that the relative role of past

UI experience in learning how to use the system should be less important in regions and/or industries in which the use of UI is widespread. One testable implication of this learning model is thus that the coefficient should be lower in high UI regions such as the Maritimes or Quebec than in low UI regions such as Ontario or Alberta.

5.1 Estimation methods

Under the assumption that F(.) is a unit normal, the probability that individual i will start a spell of UI in period t can be rewritten as:

$$\text{Prob}(U_{it} = 1 | U_{it-1}, L_{it}, x_{it}, \alpha_i) = \Phi(\alpha_i + \delta_t + z'_{it}\omega), \tag{4}$$

where:

$$z'_{it}\omega = \gamma U_{it-1} + x'_{it}\beta + \theta_0 L_{it} + (x'_{it}\theta_1)L_{it}. \tag{5}$$

The probability of observing a sequence $(U_{i1}, ..., U_{iT})$ of UI spells is thus equal to:

$$\prod_{t=1}^{T} \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{(1-U_{it})}(1 - \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{U_{it}}. \tag{6}$$

This probability is the essential building block of the likelihood function to be maximized. There are two important issues, however, that need to be addressed before the model can be estimated. First, the probability in equation (6) is conditional on a particular value of the random effect α_i. Since the random effect is not observed, we need to integrate over its distribution to obtain an unconditional probability of observing the sequence $(U_{i1}, ..., U_{iT})$:

$$\int \prod_{t=1}^{T} \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{(1-U_{it})}(1 - \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{U_{it}} dG(\alpha_i), \tag{7}$$

where G(.) is the cumulative distribution function of the random effect α_i.

The log-likelihood function of the model is then obtained by summing up the log of (7) over all the observations:

$$\sum_{i=1}^{N} \log \left(\int \prod_{t=1}^{T} \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{(1-U_{it})}(1 - \Phi(\alpha_i + \delta_t + z'_{it}\omega)^{U_{it}} dG(\alpha_i) \right). \tag{8}$$

Since we have already assumed that the cumulative distribution function F(.) was normal, it seems natural to follow authors like Heckman (1981b) and assume that G(.) is also normal. In general, evaluating the log-likelihood function (8) requires some numerical integration, which is computationally burdensome in a large panel data set like the one used here. We thus follow the initial suggestion of Lehman and Manski (1981) of randomly drawing values of α_i to evaluate the likelihood function. See also Gourieroux and Monfort (1993) for a recent survey of simulation-based estimation methods.

To see the basic idea of the simulated maximum likelihood (SML) method, first rewrite the random effect α_i as $\alpha_i = \alpha + \sigma_\alpha u_i$, where u_i is a standard normal random variable and σ_α is the standard deviation of α_i. The log-likelihood function (8) can then be approximated by randomly drawing K values $\widetilde{u}_i^1, ..., \widetilde{u}_i^K$ of u_i:

$$\sum_{i=1}^{N} \log[\frac{1}{K} \sum_{j=1}^{K} (\prod_{t=1}^{T} \Phi(\alpha+\delta_t+z_{it}'\omega+\sigma_\alpha \widetilde{u}^j)^{(1-U_{it})}(1-\Phi(\alpha+\delta_t+z_{it}'\omega+\sigma_\alpha \widetilde{u}^j)^{U_{it}})]. \tag{9}$$

We will refer to the estimates obtained by maximizing equation (9) as SML estimates. It is well known (Lehman and Manski (1981), Gourieroux and Monfort (1993)) that SML estimates are only consistent when the number of draws K goes to infinity. We thus use K=20 in the empirical analysis presented below.[13]

The second important estimation issue arise because of the nature of the administrative files that we used to construct the data set used for the estimation. Since the Status Vector only contains information on workers who file a UI claim at least one, we have no demographic information on workers who never filed a claim. We thus have to correct for the potential sample selection biases that could result from the way the final sample is constructed.

One way of handling this selection problem would be to use the parameters of the model to compute the probability that a worker experiences at least one spell of UI during the sample period. A resulting conditional log-likelihood function could then be maximized. It would yield consistent

[13]We noticed in several empirical experiments that there were only small differences between the estimates obtained with 5, 10, 20 or 100 random draws (results from these experiments are available on request).We are thus conservative in using a K as large as 20.

estimates of the parameters.[14]

The simpler correction for sample selection we use here consists in including people who never received UI in the sample. Although this cannot be done directly because of the limitations of the administrative data files, some external data sources can be used to estimate the fraction of people who never received UI. More precisely, we use the 1981 Canadian Census to compute the total number of individuals who earned some labor income in 1980 by age, sex, and province of residence. We also use our merged T4-Status Vector file to calculate the corresponding number of individuals who worked in 1980 and received UI at least once between 1972 and 1992. We then use the two sets of numbers to compute an estimate of the fraction of all wage earners in 1980 who received UI at least once between 1972 and 1992. We finally use these estimated fractions to generate a random sample of non-UI recipients who look exactly like UI recipients except that we set their U_{it}'s to zero in all periods. We then maximize the log-likelihood function (9) over a sample composed of the subsample of UI-recipients who earned some wage income in 1980 and the "artificial" subsample of non-UI recipients who also earned some wage income in 1980.[15]

A further advantage of the SML method is that it is straightforward to incorporate heterogeneity in other parameters than the intercept α. It seems natural to introduce heterogeneity in the learning parameter θ_0 since a first experience with UI may have different effects on different workers. To introduce heterogeneity in the learning parameter, write:

$$\theta_{0i} = \theta_0 + \sigma_\theta v_i,$$

where v_i is a standard normal variable and σ_θ is the standard deviation of θ_{0i}. The log-likelihood function can now be approximated by randomly drawing K values $\widetilde{u}_i^1, ..., \widetilde{u}_i^K$ of u_i and K values $\widetilde{v}_i^1, ..., \widetilde{v}_i^K$ of v_i :

$$\begin{aligned}\sum_{i=1}^{N} \log[\tfrac{1}{K} \sum_{j=1} (\textstyle\prod_{t=1}^{T} \Phi(\alpha + \delta_t + z_{it}'\omega + \sigma_\alpha \widetilde{u}^j + \sigma_\theta \widetilde{v}^j L_{it})^{(1-U_{it})} \\ (1 - \Phi(\alpha + \delta_t + z_{it}'\omega + \sigma_\alpha \widetilde{u}^j + \sigma_\theta \widetilde{v}^j L_{it})^{U_{it}})].\end{aligned} \tag{10}$$

[14]We used this approach in a previous version of this paper and found results that are very similar to those reported here.

[15]This procedure is still potentially biased since people must have worked in 1980 to enter the sample. The evidence presented in the previous version of this paper suggests, however, that this type of selection bias has only a small effect on the estimates.

Note that u_i and v_i will be positively correlated if the learning effect and the probability of a first experience with UI are both larger some workers than others. Following Gourieroux and Monfort (1993), the correlation between the heterogeneity in the learning effect and in the intercept is introduced by redefining θ_{0i} as

$$\theta_{0i} = \theta_0 + r_1 u_i + r_2 v_i,$$

where $r_1 = \rho\sigma_\theta/\sigma_\alpha$, $r_2 = \sqrt{\sigma_\theta^2 - \rho\sigma_\theta/\sigma_\alpha}$, ρ is the correlation between the heterogeneity in the learning effect and in the intercept, and the error component v_i is now defined as the part of the heterogeneity in the learning effect which is uncorrelated with the heterogeneity in the intercept. This model with correlated heterogeneity in the intercept and in the learning effect is easily estimated by replacing $\sigma_\alpha \widetilde{u}^j + \sigma_\theta \widetilde{v}^j L_{it}$ with $\sigma_\alpha \widetilde{u}^j + (r_1 \widetilde{u}^j + r_2 \widetilde{v}^j) L_{it}$ in equation (10), drawing K independent values for both $\widetilde{u}^j$ and $\widetilde{v}^j$, and numerically maximizing the resulting log-likelihood function.

Though our model is a random effect model, it is important to point out that it takes implicitly into account of the correlation between the person effect α_i and the explanatory variables related to previous use of UI (U_{it-1} and L_{it}) since we are jointly modelling the probability of receiving UI in all sample years.

5.2 Results

Given the numerical burden associated with maximizing the log–likelihood function, we only perform the estimation over a randomly selected subsample of the main sample . In order to obtain estimates precise enough for several demographic groups in each province, we randomly select a 1–in–5 sample for Newfoundland, Nova Scotia, New Brunswick, and Saskatchewan, a 1–in–6 sample for Manitoba, a 1–in–8 sample for Alberta, a 1–in–20 sample for British Columbia, and a 1–in–50 sample for Quebec and Ontario. Prince Edward Island, Yukon and Northwest Territories are excluded from the estimation since these regions cannot be identified separately in the public use release of the 1981 Census micro data.

For both men and women in each province, we further divide the sample in three subsamples based on the year of birth. The first demographic subsample includes individuals born before 1946 who were all old enough to be in the labor force in 1972. The second sample is a sample of "baby–boomers" born from 1946 to 1955 while the third sample of individuals born after 1955 were unlikely to have entered the workforce in 1972. We also limit our analysis to observations that satisfy the "eligibility" rule of having received some T4 income during the current or the previous year. Using this selection rule limits potential biases caused by people who permanently exit the labor force for various reasons. We have also estimated our models without this selection rule and found very similar results.

We first estimate separate models for each of the six demographic groups (two genders and three cohorts) in each province. In each of the 54 random effect probit models, we include the learning variable, the first four lags of the dependent variable (U_{it-1} to U_{it-4}), a full set of year dummies, industry dummies, age and age squared. We decided to include four lags of the dependent variable after observing that the estimated effect of further lags was rarely statistically different from zero. Table 4 reports estimates from models in which unobserved heterogeneity is only included in the intercept. Unobserved heterogeneity is introduced in both the intercept and the learning coefficient in Table 5. We do not include any interactions between the learning variable and other variables in these simple models. The parameter θ_1 is thus implicitly set to zero.

Estimates of the learning parameter θ_0 are reported in Table 4A. While the estimated effect is on average positive, some interesting patterns seem to emerge from the Table. A first pattern is that learning effects tend to be large and positive for men born before 1946 but much smaller and often negative for women and younger men.[16] In addition, learning effects are largest in Ontario, Saskatchewan, Alberta, and British Columbia, four provinces in which the use of UI is less pervasive than in the rest of the country.

These two patterns of results are consistent with the role of social versus individual learning mentioned earlier in the text. The more widespread the use of UI is in a region at a point of time, the less previous experience with UI will affect the propensity to use UI. The point is simply that when "everybody else" uses the system, a first experience with the system will not teach a person anything he or she did not already know through family or friends. The results reported in Table 4A thus support the view that younger

[16]Interestingly, there was a substantial decline in the employment rate of older men in Canada relative to the United States during the 1970s and 1980s. From the mid-1970s to the late 1980s, the employment rate of men age 55 to 64 declined by 7 percentage points in the United States but by 12 percentage points in Canada. This difference is consistent with learning effects being larger for older men than for other groups in the workforce.

cohorts of men and women living in areas where the use of UI is more wide spread already knew how the system worked before receiving UI for the first time. It is hard to see how other theories of occurrence dependence such as models of "addiction" or other sources of "vicious circles" could explain the pattern of results reported in Table 4A. For example, if people get addicted to UI in the way they get addicted to cigarette smoking, there is no reason why the effect of first time use of UI would vary across cohorts and regions. By contrast, the substitutability between individual and social learning provides a simple rationalization for the patterns observed in the data.

It is important to point out, however, that there is a lot of persistence in the propensity to use UI that has little to do with learning. The four lagged values of the dependent variable are positive and statistically significant for all demographic groups in all provinces. To give an idea of the magnitude of the effects, we report the sum of the estimated coefficients on the four lags of the dependent variable in Table 4B. The sum of these four coefficients is on average much larger than the size of the estimated learning effects. This suggests that labor market shocks can have relatively large effects on the propensity to use UI that will persist over several years.

The average learning effect becomes substantially larger when unobserved heterogeneity is also introduced in the learning effect in Table 5. In Table 5A, the average learning effect is positive for all provinces and all age groups. For all practical purpose, the estimated learning effect is positive for each individual demographic group in Ontario and in Western Canada. The results reported in Table 5A thus reinforce our previous conclusion about the substitutability between individual and social learning. Note also that the sum of the coefficients on the four lags of the dependent variable is smaller in Table 5B than in Table 4B. There is also some variation in this sum across provinces and demographic groups, suggesting there may be problems in identifying the learning effect separately from the effect of the lags of the dependent variable.

We have thus re–estimated a more constrained version of the model in which the lagged dependent variables as well as age and year dummies are constrained to have the same effect in the nine provinces. For each of the three groups of men and women, this constrained model is estimated on a pooled sample of the nine provincial samples used in Table 4. We also include a set of province dummies to allow for differences in the intercept in each province.

One further advantage of working with a pooled sample is that we can exploit the variation of the parameters of the UI system over regions and over time to estimate the effect of these parameters on the propensity to use UI. We combine these UI parameters into a single "subsidy rate" of UI defined as the replacement rate multiplied by the ratio of the maximum number of weeks of eligibility of someone who has only worked the minimum number of weeks required to qualify over the minimum number of weeks to qualify. An increase in the subsidy rate tends to increase the fraction workers who work part-year and regularly collect UI (Section 2). It should thus have a positive effect on the probability of receiving UI. One interesting hypothesis we can also test in this setting is whether the subsidy rate has a larger effect on people who had some previous experience with the UI system than on people who never had such experience. In terms of equation (3), this means that the component of the vector of parameters θ_1 corresponding to the subsidy rate (one of the element of x_{it}) should be positive. To insure that the estimated value of this parameter does not simply reflect omitted trends or regional differences in the size of the learning effect, we also interact the learning variable with the full set of year and province dummies.

The random effect probit estimates of the pooled models for men are reported in Table 6. The results for women are reported in Table 7. In both cases, we estimate models with unobserved heterogeneity in both the intercept and the learning coefficient. Given the large number of parameter estimates reported in Tables 6 and 7, we only discuss few broad patterns in the results. The main conclusions that emerge from these Tables are:

1. With few exceptions, there is no longer much of a tendency for the learning effect to be smaller in the Maritimes and in Quebec than in other provinces. This means that the pattern found in Tables 5A and 6A may be a spurious consequence of the fact that it is hard to separately identify the learning effect from the effect of lags of the dependent variable. An alternative interpretation is that it is inappropriate to restrict the effect of lags of the dependent variable to be constant across provinces.

2. The subsidy rate always has a positive and significant effect on the probability of receiving UI. These effects are quite small, however, in economic terms. According to the parameter estimates, the impact of a 100 percent increase in the subsidy rate on the probability of receiving UI is 2 to 3 percentage points for most of the estimated specifications.

The effect of the subsidy rate does not tend to be systematically larger, however, for individuals who have been exposed to UI in the past than for individuals who have not been exposed.

3. There is no systematic pattern in the learning effect over time except for the cohort born in 1946-55 for which it trends upward.

4. The variance of the heterogeneity components is significantly larger than zero in most of the specifications. Furthermore, the error components reflecting heterogeneity in the intercept and in the learning effect are positively correlated for men. This is consistent with the simple model of employment and unemployment presented in Section 2 (the probability of receiving UI for a first time and the effect of a first exposure are both larger for marginal than for non-marginal workers). This correlation is negative, however, for older cohorts of women but tends to become more positive for younger cohorts. We thus conjecture that the negative correlation is due to differences in labor force participation behavior of older cohorts of men and women.

Taken together, the results reported in Tables 4 to 7 suggest that one's first experience with UI has a permanent effect on the future probability of receiving UI. This is especially clear for older cohorts of men. This pattern of results is partly consistent with the idea that individual or social learning about the parameters of the UI system has an impact on employment and unemployment behavior. It could also be consistent, however, with other sources of "hysteresis" coming from the supply side of the market.

The main results are very robust to the choice of estimation procedure. For example, we have re-estimated the main specifications using a linear probability model with fixed effects. To eliminate the fixed effect, we take first differences and instrument the lagged dependent variables and the learning variables with further lags of the UI variable.[17] The results reported in Appendix Table 1 are very similar to the SML estimates of the random effect

[17] A simplified version of the linear probability model is $U_{it} = \gamma U_{it-1} + x'_{it}\beta + \theta_0 L_{it} + \alpha_i + \epsilon_{it}$. The fixed effect α_i is eliminated by taking first differences: $\Delta U_{it} = \gamma \Delta U_{it-1} + \Delta x'_{it}\beta + \theta_0 \Delta L_{it} + \Delta\epsilon_{it}$.

Since $\Delta\epsilon_{it}$ is correlated with U_{it-1}(by construction), both ΔU_{it-1} and ΔL_{it} can be correlated with it. Consistent estimates can nevertheless be obtained by instrumenting ΔU_{it-1} and ΔL_{it} with further lags of the UI variables (U_{it-2}, U_{it-3}, etc.).

probit model. In particular, the learning effect tends to be larger for older cohorts than for younger ones.

5.3 Summary of the Findings: How Big Are the Learning Effects?

Both the difference–in–differences approach and the random effect probit model suggest that learning plays a significant role in the probability of receiving UI. Simple calculations suggest that learning effects are large enough to explain a large fraction of the 2–2.5 percentage point gap in unemployment rates between Canada and the United States that emerged in the early 1980s.

To see this, first notice that both the difference–in–differences and the random effect probit estimates indicate that a first exposure to UI increases, on average, the future probability of receiving UI by 3–4 percentage points a year. This is easily seen for the difference–in–differences models for which a first exposure increases the probability of future use by around 10 percentage points over a 3 years period (Table 3), or at least 3–4 percentage points a year.[18].

In the case of the random effect probit model, the effect of learning on the probability of receiving UI depends on workers' type (unobserved heterogeneity) as well as on the size of the learning parameter. Consider a learning parameter of 0.2 (average of the parameters in Table 5) and a variance of unobserved heterogeneity in the intercept of 0.27, which is the average estimated variance for women. In this case, the effect of learning on the future probability of receiving UI varies from 5 percentage points for less–skilled workers (5th percentile of the distribution of unobserved heterogeneity) to 1 percent for highly–skilled workers (95th percentile of the distribution of unobserved heterogeneity). The average effect is around 3 percentage points, which is comparable to the difference-in–differences estimates.[19]

Since over 50 percent of individuals in the sample period eventually receive UI at least once, learning effects can potentially explain a 1.5-2 per-

[18]Since many workers receive UI more than once over a 3 years period, the yearly probability exceeds a third of the probability over a 3 years period.

[19]Using a variance of unobserved heterogeneity smaller than 0.27 yields the same average effect of learning. It simply reduces the effect for less–skilled workers and increases the effect for highly–skilled workers, leaving the average effect virtually unchanged.

centage points increase in the probability of receiving UI.[20]. If there was a one–to–one mapping between UI and unemployment, as Figure 2 suggests, this would mean that learning effects account for most Canada–U.S. unemployment rate gap that emerged in the 1980s.[21].

6 Concluding Remarks

We find that first time use of the unemployment insurance system in Canada increases the probability of future use. This effect is a potential explanation for the increasing share of UI spells accounted by repeated users[22]. As workers are exposed to UI for the first time for a variety of reasons, they learn about the functioning of the system and adjust their behavior accordingly. For highly-skilled workers, this first exposure to the system may not affect their behavior very much since it is not profitable for them to work part–year except perhaps during a short adjustment period. By contrast, for less-skilled workers working part–year may be an attractive long run option. For these workers, a first experience may thus have a large effect on the future probability of receiving UI.

We also find some support for the view that the effect of first time use is a learning effect since the estimated effect tends to be lower for people who are more likely to know how the UI system operates (young workers and people living in high unemployment regions). We also find that adjustment lags over several period account for an important part of the dynamics in UI recipiency.

In this framework, the evolution of UI recipiency over time depends jointly on the evolution of the employment and income process and on the evolution

[20] The 50 percent figure is obtained by comparing the number of people in the sample to total population counts in the 1981 Canadian Census.

[21] There is a small slippage in these calculations since the yearly probability of starting a spell of UI (the variable we analyze) is not the same as the weekly probability of collecting UI which corresponds more closely to the unemployment rate. This means that our estimate of 1.5-2 percentage points is biased up since one is more likely to collect UI during a year than during a given week. On the other hand, learning may also increase the duration of UI spells, which would bias the results in the other direction. A further bias is the fact that our crude measure of learning may well underestimate the full learning effects because of social learning, etc.

[22] See Lemieux and MacLeod (1995) for evidence on the distribution of UI recipients by frequency of use.

of the probability of receiving UI conditional on being eligible (the take–up rate). In future research, it would interesting to see how the dynamics of UI recipiency can be broken up into these two components.

Though we have only reduced form results they do have some potentially important implications of the design of social welfare programs. First, the behavior that we have observed is consistent with individuals responding to the incentives provided by the system. The fact that experience is required for a change in behavior is consistent with laboratory studies of learning. It also suggests that studies based on cross-section estimates of supply responses underestimate the long term impact of the disincentive effects of social welfare programs.

In Canada's case, the increased divergence of the Canadian and American unemployment rates has long been a source of concern. The study of Card and Ridell (1993) is unable to identify the source of the difference based upon a standard supply and demand analysis. The lagged adjustment effects identified in this study may provide a coherent explanation for this effect. Given the size of the 1971 changes to the UI system, even as benefits decreased in the subsequent 20 years, workers unfamiliar with the system may still modify their behavior after a spell of unemployment several years after the change because they had never considered part year work as an option. If subsequent research supports this conjecture, this has potentially important implications for the design of social welfare programs. Specifically it would imply that the feedback between a policy change, and its ultimate impact on the economy may be very slow, and hence it suggests that it may be very costly to learn about and correct policy errors. Moreover, once individuals have adjusted their behavior to the system, one is likely to face a similar lagged adjustment in the reverse direction as individuals take time to learn and respond to the new parameters. Canada has recently tightened significantly its UI rules. It will be interesting to see how the Canada-US unemployment rate gap responds as a consequence.

Secondly, it highlights the importance of coverage in determining the impact of changes in a program. Rule changes that effect current recipients can be expected to have an immediate impact because the individuals *experience* the rule change. However for program rule changes that involve an increase in the target population our results suggest that it may take some time before the new individuals at risk respond fully to the new incentives.[23]

[23]Another example of this effect occurs for the foodstamp program in the US where

In summary these results suggest that great care must be taken if we are to properly interpret the relationship between changes in incentives at the individual level, and the subsequent impact on the economy as a whole.

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Figure 1

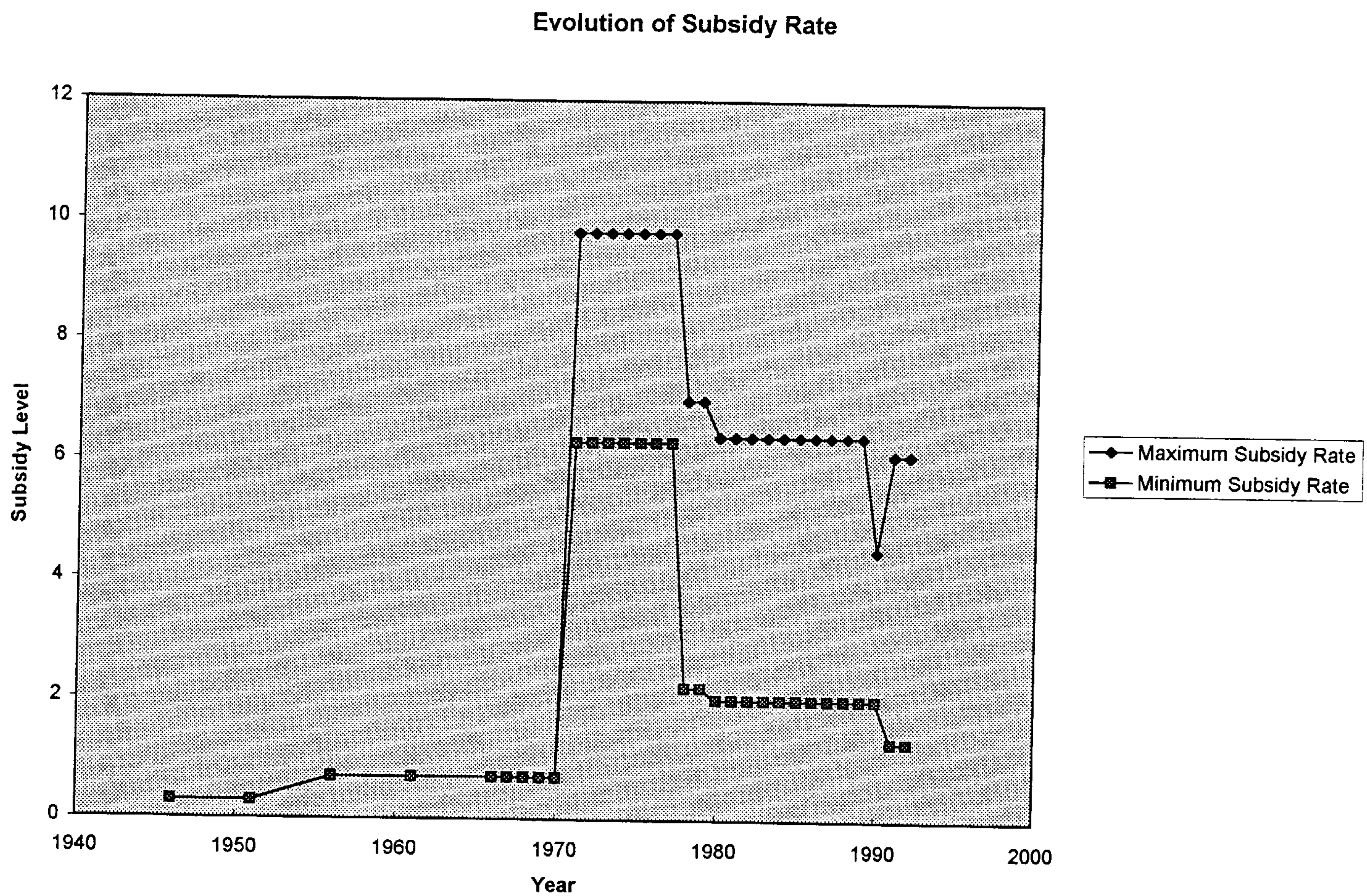
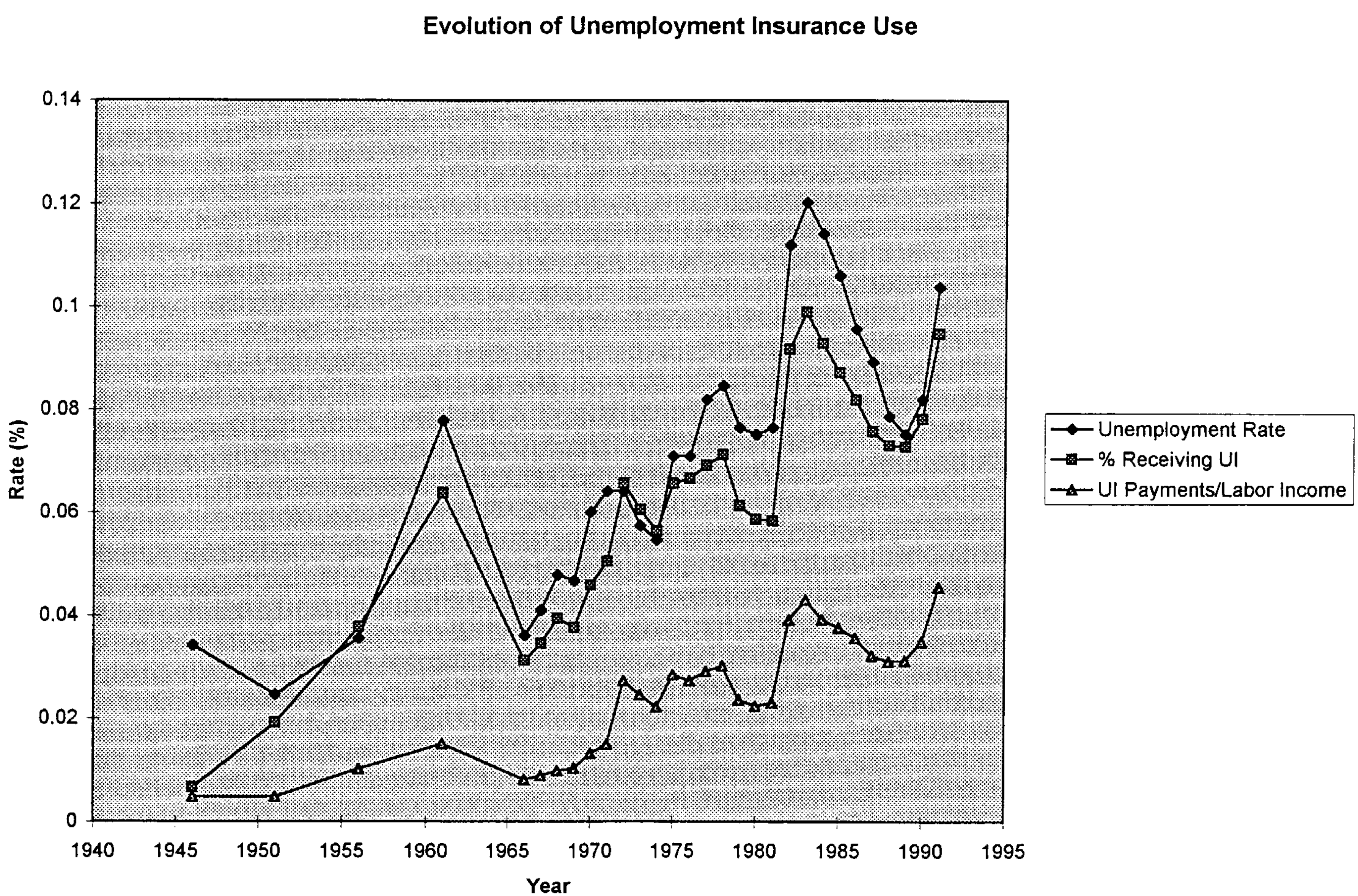

Evolution of Subsidy Rate
Subsidy Level
12
10
8
6
4
2
0
1940
1950
1960
1970
1980
1990
2000
Year
Maximum Subsidy Rate
Minimum Subsidy Rate

Figure 2



Evolution of Unemployment Insurance Use
Rate (%)
0.14
0.12
0.1
0.08
0.06
0.04
0.02
0
1940
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990
1995
Year
Unemployment Rate
% Receiving UI
UI Payments/Labor Income

Table 1: Distribution of Age and Year of Entry in the Sample

A. MEN

Age	Age of Entry Frequency	Cumul. freq.
15	0.105	0.105
16	0.124	0.229
17	0.110	0.339
18	0.079	0.418
19	0.049	0.466
20	0.035	0.501
21	0.029	0.531
22	0.026	0.557
23	0.025	0.582
24	0.024	0.606
25	0.023	0.629
26	0.020	0.649
27	0.019	0.668
28	0.018	0.685
29	0.017	0.702
30-34	0.069	0.771
35-39	0.057	0.828
40-44	0.055	0.883
45-49	0.049	0.932
50-54	0.038	0.970
55-59	0.025	0.995
60-64	0.005	1.000

Year	Year of Entry Frequency	Cumul. freq
1972	0.493	0.493
1973	0.061	0.553
1974	0.049	0.602
1975	0.038	0.640
1976	0.035	0.675
1977	0.034	0.709
1978	0.033	0.742
1979	0.034	0.776
1980	0.031	0.807
1981	0.030	0.837
1982	0.020	0.857
1983	0.022	0.879
1984	0.023	0.902
1985	0.023	0.925
1986	0.021	0.946
1987	0.021	0.967
1988	0.013	0.981
1989	0.010	0.991
1990	0.006	0.996
1991	0.004	1.000

Note: Based on a sample of 618,911 men aged 15 to 65. A person "enters" the sample the first time he receives T4 income between 1972 and 1991.

Table 1 (continuation)

B. WOMEN

Age	Age of Entry Frequency	Cumul. freq.	Year	Year of Entry Frequency	Cumul. freq
15	0.083	0.083	1972	0.384	0.384
16	0.107	0.191	1973	0.079	0.463
17	0.111	0.301	1974	0.066	0.529
18	0.089	0.390	1975	0.053	0.582
19	0.057	0.447	1976	0.047	0.628
20	0.039	0.486	1977	0.042	0.670
21	0.032	0.519	1978	0.041	0.712
22	0.028	0.546	1979	0.041	0.753
23	0.025	0.572	1980	0.039	0.792
24	0.024	0.596	1981	0.036	0.828
25	0.023	0.619	1982	0.024	0.852
26	0.020	0.639	1983	0.025	0.877
27	0.019	0.658	1984	0.026	0.903
28	0.018	0.675	1985	0.024	0.927
29	0.017	0.692	1986	0.021	0.948
30-34	0.078	0.770	1987	0.019	0.967
35-39	0.069	0.839	1988	0.014	0.981
40-44	0.059	0.898	1989	0.010	0.991
45-49	0.048	0.946	1990	0.006	0.997
50-54	0.033	0.979	1991	0.003	1.000
55-59	0.018	0.997			
60-64	0.003	1.000			

Note: Based on a sample of 494,697 women aged 15 to 65. A person "enters" the sample the first time she receives T4 income between 1972 and 1991.

Table 2: Summary Statistics of the Sample

	Mean		Proportion starting a UI claim	
	Men	Women	Men	Women
Age:	34.759	34.493	---	
Employed during the year:	0.796	0.731	---	
UI claim:	0.210	0.160	---	
Province:				
Newfounland	0.025	0.023	0.375	0.300
PEI	0.005	0.006	0.349	0.279
Nova Scotia	0.036	0.035	0.259	0.191
New Brunswick	0.031	0.029	0.321	0.247
Quebec	0.286	0.285	0.234	0.182
Ontario	0.350	0.360	0.179	0.138
Manitoba	0.037	0.037	0.185	0.133
Saskatchewan	0.029	0.029	0.196	0.126
Alberta	0.087	0.082	0.176	0.124
British Columbia	0.115	0.113	0.194	0.153
Year:				
1972	0.030	0.024	0.234	0.205
1973	0.033	0.028	0.205	0.178
1974	0.036	0.032	0.204	0.171
1975	0.038	0.036	0.238	0.180
1976	0.041	0.038	0.216	0.164
1977	0.043	0.041	0.221	0.161
1978	0.044	0.043	0.215	0.155
1979	0.046	0.046	0.180	0.128
1980	0.048	0.048	0.183	0.122
1981	0.049	0.050	0.198	0.134
1982	0.050	0.051	0.267	0.169
1983	0.051	0.053	0.230	0.159
1984	0.052	0.054	0.225	0.168
1985	0.053	0.055	0.204	0.160
1986	0.054	0.056	0.199	0.159
1987	0.055	0.057	0.183	0.156
1988	0.055	0.057	0.182	0.157
1989	0.055	0.058	0.190	0.159
1990	0.055	0.058	0.215	0.170
1991	0.055	0.057	0.222	0.173
1992	0.054	0.057	0.214	0.172

Note: Samples of 10,253,535 observations for men age 15 to 65 and 8,074,326 observations for women aged 15 to 65 from the years 1972 to 1992 who have earned some T4 income at least once since 1972.

Table 3: Difference-in-Differences Estimates of the Effect of Learning on the Future Probability of Receiving Unemployment Insurance

	Probability of Receiving UI in:			Difference between (1) and (2)	Difference-in-Differences
	1981-83	1984-86	1987-89		
	(1)	(2)	(3)	(4)	(5)
A. Not Adjusted for Selection					
1. Men born in 1931					
1.a. Had no previous UI experience	0.355	0.373	0.345	0.019	0.123
1.b. Had some previous UI experience	0.432	0.328	0.260	-0.104	
2. Men born in 1941					
2.a. Had no previous UI experience	0.406	0.365	0.330	-0.041	0.042
2.b. Had some previous UI experience	0.436	0.353	0.295	-0.083	
3. Men born in 1951					
3.a. Had no previous UI experience	0.421	0.395	0.325	-0.026	0.056
3.b. Had some previous UI experience	0.412	0.330	0.269	-0.082	
4. Women born in 1931					
4.a. Had no previous UI experience	0.323	0.375	0.357	0.052	0.108
4.b. Had some previous UI experience	0.292	0.236	0.187	-0.056	
5. Women born in 1941					
5.a. Had no previous UI experience	0.324	0.379	0.382	0.055	0.088
5.b. Had some previous UI experience	0.320	0.287	0.247	-0.033	
6. Men born in 1951					
6.a. Had no previous UI experience	0.300	0.375	0.390	0.075	0.089
6.b. Had some previous UI experience	0.259	0.245	0.236	-0.014	

Table 3: Continuation

	Probability of Receiving UI in: 1981-83	1984-86	1987-89	Difference between (1) and (2)	Difference-in-Differences
	(1)	(2)	(3)	(4)	(5)
B. Adjusted for selection					
1. Men born in 1931					
1.a. Had no previous UI experience	0.104	0.109	0.101	0.006	0.109
1.b. Had some previous UI experience	0.432	0.328	0.260	-0.104	
2. Men born in 1941					
2.a. Had no previous UI experience	0.117	0.105	0.095	-0.012	0.071
2.b. Had some previous UI experience	0.436	0.353	0.295	-0.083	
3. Men born in 1951					
3.a. Had no previous UI experience	0.168	0.158	0.130	-0.010	0.072
3.b. Had some previous UI experience	0.412	0.330	0.269	-0.082	
4. Women born in 1931					
4.a. Had no previous UI experience	0.094	0.094	0.086	0.000	0.104
4.b. Had some previous UI experience	0.432	0.328	0.260	-0.104	
5. Women born in 1941					
5.a. Had no previous UI experience	0.101	0.112	0.110	0.010	0.093
5.b. Had some previous UI experience	0.436	0.353	0.295	-0.083	
6. Men born in 1951					
6.a. Had no previous UI experience	0.121	0.146	0.146	0.025	0.108
6.b. Had some previous UI experience	0.412	0.330	0.269	-0.082	

Table 4A: Random Effect Probit Estimates of the Learning Effect by Demographic Group and by Province, 1972-1992:

Unobserved (normal) heterogeneity in intercept only

Province	Born before 1946 Men	Born before 1946 Women	Born from 1946 to 1955 Men	Born from 1946 to 1955 Women	Born after 1955 Men	Born after 1955 Women	Average
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Newfoundland	0.580	0.087	0.045	-0.198	-0.422	-0.460	-0.061
	(0.045)	(0.076)	(0.051)	(0.060)	(0.053)	(0.059)	
Nova Scotia	0.481	0.090	0.084	-0.031	-0.293	-0.259	0.012
	(0.038)	(0.054)	(0.042)	(0.052)	(0.040)	(0.050)	
New Brunswick	0.518	0.010	-0.071	-0.212	-0.300	-0.307	-0.060
	(0.042)	(0.064)	(0.044)	(0.059)	(0.044)	(0.052)	
Quebec	0.214	0.143	-0.074	0.093	-0.246	-0.094	0.006
	(0.046)	(0.053)	(0.046)	(0.052)	(0.046)	(0.052)	
Ontario	0.421	0.136	0.034	-0.001	-0.068	0.005	0.088
	(0.035)	(0.039)	(0.040)	(0.047)	(0.038)	(0.044)	
Manitoba	0.396	0.109	-0.048	-0.071	-0.063	-0.021	0.050
	(0.037)	(0.044)	(0.042)	(0.051)	(0.039)	(0.050)	
Saskatchewan	0.511	0.122	0.145	-0.065	0.041	0.051	0.134
	(0.036)	(0.054)	(0.047)	(0.060)	(0.035)	(0.046)	
Alberta	0.505	0.245	0.385	0.241	0.390	0.354	0.353
	(0.044)	(0.052)	(0.047)	(0.057)	(0.038)	(0.043)	
British Columbia	0.454	0.156	0.173	0.196	-0.018	-0.026	0.156
	(0.038)	(0.050)	(0.045)	(0.053)	(0.041)	(0.051)	
Average	0.453	0.122	0.075	-0.005	-0.109	-0.084	0.075

Note: Standard errors are in parentheses. All models also include a full set of year effects, four lagged values of the dependent variable, age and its squared. Unobserved heterogeneity is accounted for by including a standard normal component in the intercept and estimating the model using simulated maximum likelihood (20 draws).

Table 4B: Sum of the Estimated Coefficients on the Four Lags of the Dependent Variable by Demographic Group and by Province.

Province	Born before 1946		Born from 1946 to 1955		Born after 1955		
	Men	Women	Men	Women	Men	Women	Average
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Newfoundland	1.345	1.415	1.400	1.618	1.580	1.555	1.441
Nova Scotia	1.572	1.634	1.436	1.483	1.355	1.383	1.454
New Brunswick	1.728	1.798	1.591	1.631	1.349	1.390	1.556
Quebec	1.390	1.396	1.296	1.252	1.388	1.306	1.358
Ontario	1.484	1.622	1.182	1.284	1.188	1.166	1.285
Manitoba	1.765	1.809	1.334	1.366	1.203	1.122	1.434
Saskatchewan	1.927	1.946	1.444	1.373	1.305	1.146	1.559
Alberta	1.500	1.503	1.348	1.422	1.101	0.995	1.317
British Columbia	1.399	1.469	1.261	1.324	0.988	0.970	1.216
Average	1.568	1.621	1.366	1.417	1.273	1.226	1.402

Note: See the note at the bottom of Table 4A for more information on the estimated models.

Table 4C: Number of Observations and Persons Used in the Estimation

Province	Born before 1946 Men	Born before 1946 Women	Born from 1946 to 1955 Men	Born from 1946 to 1955 Women	Born after 1955 Men	Born after 1955 Women
	(1)	(2)	(3)	(4)	(5)	(6)
Newfoundland	19047	8606	16843	9592	11788	8383
	(1087)	(544)	(846)	(530)	(723)	(548)
Nova Scotia	34682	18293	24056	15847	18742	15252
	(2018)	(1161)	(1233)	(879)	(1175)	(1024)
New Brunswick	25466	14061	19236	12576	15588	11845
	(1488)	(893)	(976)	(691)	(957)	(789)
Quebec	27120	15250	18678	13008	13972	11098
	(1558)	(962)	(961)	(712)	(859)	(735)
Ontario	39187	26842	25005	19600	20226	18039
	(2282)	(1623)	(1291)	(1066)	(1251)	(548)
Manitoba	32750	23373	22553	17383	20101	17245
	(1950)	(1462)	(1166)	(947)	(1265)	(1131)
Saskatchewan	28322	23248	22793	15783	23305	18441
	(1714)	(1437)	(1188)	(850)	(1471)	(1208)
Alberta	36200	23558	31299	21736	29090	23714
	(2123)	(1448)	(1645)	(1191)	(1789)	(1532)
British Columbia	30826	20017	21232	15903	16869	13995
	(1795)	(1244)	(1114)	(885)	(1057)	(924)

Note: The number of indiduals in each subsample is in parentheses.

Table 5A: Random Effect Probit Estimates of the Learning Effect by Demographic Group and by Province, 1972-1992:
Unobserved (normal) heterogeneity in intercept and learning coefficient

Province	Born before 1946 Men	Born before 1946 Women	Born from 1946 to 1955 Men	Born from 1946 to 1955 Women	Born after 1955 Men	Born after 1955 Women	Average
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Newfoundland	0.553	0.144	0.151	-0.373	-0.216	-0.233	0.004
	(0.058)	(0.079)	(0.067)	(0.092)	(0.066)	(0.073)	
Nova Scotia	0.737	0.467	0.262	-0.056	-0.104	-0.235	0.178
	(0.041)	(0.062)	(0.048)	(0.067)	(0.048)	(0.054)	
New Brunswick	0.750	-0.030	0.069	-0.350	-0.216	-0.136	0.014
	(0.050)	(0.064)	(0.055)	(0.066)	(0.056)	(0.065)	
Quebec	0.633	0.125	0.068	0.138	-0.124	-0.121	0.120
	(0.043)	(0.059)	(0.054)	(0.072)	(0.060)	(0.070)	
Ontario	0.604	0.268	0.158	0.105	0.042	0.115	0.215
	(0.036)	(0.041)	(0.046)	(0.057)	(0.047)	(0.046)	
Manitoba	0.562	0.338	0.188	0.119	0.201	0.214	0.270
	(0.045)	(0.058)	(0.044)	(0.055)	(0.043)	(0.055)	
Saskatchewan	0.590	0.184	0.367	-0.001	0.034	0.235	0.235
	(0.052)	(0.061)	(0.050)	(0.072)	(0.045)	(0.054)	
Alberta	0.730	0.245	0.622	0.634	0.521	0.491	0.540
	(0.055)	(0.062)	(0.045)	(0.086)	(0.044)	(0.064)	
British Columbia	0.725	0.405	0.346	0.459	0.047	0.189	0.362
	(0.044)	(0.059)	(0.054)	(0.059)	(0.055)	(0.051)	
Average	0.654	0.238	0.248	0.075	0.021	0.057	0.215

Note: Standard errors are in parentheses. All models also include a full set of year effects, four lagged values of the dependent variable, age and its squared. Unobserved heterogeneity is accounted for by including a standard normal component in the intercept and the learning coefficient and estimating the model using simulated maximum likelihood (20 draws).

Table 5B: Sum of the Estimated Coefficients on the Four Lags of the Dependent Variable by Demographic Group and by Province.

Province	Born before 1946		Born from 1946 to 1955		Born after 1955		
	Men	Women	Men	Women	Men	Women	Average
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Newfoundland	1.391	1.476	1.285	1.570	1.325	1.244	1.333
Nova Scotia	1.504	1.535	1.262	1.421	1.126	1.262	1.297
New Brunswick	1.760	1.935	1.349	1.522	1.224	1.220	1.445
Quebec	1.294	1.363	1.108	1.142	1.201	1.220	1.201
Ontario	1.220	1.388	1.035	1.175	1.010	0.973	1.089
Manitoba	1.491	1.478	1.154	1.181	0.950	0.845	1.198
Saskatchewan	1.686	1.804	1.337	1.311	1.241	1.038	1.421
Alberta	1.113	1.263	1.157	1.206	0.900	0.762	1.057
British Columbia	1.019	1.134	0.968	1.041	0.843	0.795	0.944
Average	1.387	1.486	1.184	1.285	1.091	1.040	1.221

Note: See the note at the bottom of Table 5A for more information on the estimated models.

Table 5C: Number of Observations and Persons Used in the Estimation

Province	Born before 1946		Born from 1946 to 1955		Born after 1955	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Newfoundland	19047 (1087)	8606 (544)	16843 (846)	9592 (530)	11788 (723)	8383 (548)
Nova Scotia	34682 (2018)	18293 (1161)	24056 (1233)	15847 (879)	18742 (1175)	15252 (1024)
New Brunswick	25466 (1488)	14061 (893)	19236 (976)	12576 (691)	15588 (957)	11845 (789)
Quebec	27120 (1558)	15250 (962)	18678 (961)	13008 (712)	13972 (859)	11098 (735)
Ontario	39187 (2282)	26842 (1623)	25005 (1291)	19600 (1066)	20226 (1251)	18039 (548)
Manitoba	32750 (1950)	23373 (1462)	22553 (1166)	17383 (947)	20101 (1265)	17245 (1131)
Saskatchewan	28322 (1714)	23248 (1437)	22793 (1188)	15783 (850)	23305 (1471)	18441 (1208)
Alberta	36200 (2123)	23558 (1448)	31299 (1645)	21736 (1191)	29090 (1789)	23714 (1532)
British Columbia	30826 (1795)	20017 (1244)	21232 (1114)	15903 (885)	16869 (1057)	13995 (924)

Note: The number of indiduals in each subsample is in parentheses.

Table 6: Random Effect Probit Estimates for Each of the three Groups of Men for all Provinces, 1972-1992.

	Men born before 1946		Men born in 1946-55		Men born after 1955	
	Main Effect	Interact. with Learning	Main Effect	Interact. with Learning	Main Effect	Interact. with Learning
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Intercept	-1.247 (0.090)	0.714 (0.128)	-1.524 (0.119)	0.289 (0.149)	-1.220 (0.135)	0.106 (0.232)
First Lag	0.479 (0.013)	---	0.416 (0.011)	---	0.416 (0.012)	---
Second Lag	0.469 (0.013)	---	0.418 (0.011)	---	0.407 (0.011)	---
Third Lag	0.292 (0.013)	---	0.243 (0.011)	---	0.235 (0.011)	---
Fourth Lag	0.205 (0.013)	---	0.198 (0.012)	---	0.143 (0.011)	---
Age	-0.040 (0.017)	---	-0.352 (0.019)	---	-0.499 (0.027)	---
Age squared	0.010 (0.005)	---	0.121 (0.012)	---	-0.401 (0.020)	---
Subsidy Rate	0.099 (0.005)	0.048 (0.036)	0.114 (0.033)	-0.009 (0.042)	0.108 (0.033)	0.010 (0.043)
Province Dummies:						
Nova Scotia	-0.234 (0.030)	0.005 (0.041)	0.008 (0.039)	-0.184 (0.047)	-0.230 (0.040)	0.062 (0.045)
New Brunswick	-0.193 (0.026)	0.064 (0.038)	0.077 (0.032)	-0.114 (0.040)	-0.272 (0.036)	0.199 (0.041)
Quebec	-0.141 (0.028)	-0.181 (0.041)	0.054 (0.036)	-0.278 (0.045)	-0.312 (0.039)	0.040 (0.046)
Ontario	-0.199 (0.039)	-0.203 (0.054)	-0.002 (0.052)	-0.354 (0.062)	-0.542 (0.050)	0.154 (0.061)
Manitoba	-0.216 (0.039)	-0.163 (0.053)	-0.010 (0.051)	-0.339 (0.062)	-0.581 (0.050)	0.250 (0.059)

Table 6 (continuation)

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Saskatchewan	-0.281	-0.093	-0.253	-0.204	-0.609	0.272
	(0.041)	(0.057)	(0.053)	(0.065)	(0.052)	(0.061)
Alberta	-0.382	-0.085	-0.460	-0.011	-0.970	0.542
	(0.039)	(0.054)	(0.051)	(0.062)	(0.050)	(0.058)
British Columbia	-0.222	-0.147	-0.203	-0.109	-0.598	0.288
	(0.033)	(0.046)	(0.043)	(0.052)	(0.043)	(0.050)
Year Dummies						
Year 1973	-0.382	---	-0.183	---	-0.069	---
	0.023		0.025		0.084	
Year 1974	-0.622	0.043	-0.300	0.065	0.093	-0.154
	0.027	0.053	0.028	0.051	0.078	0.204
Year 1975	-0.491	-0.179	-0.192	-0.053	0.312	-0.053
	0.025	0.051	0.029	0.051	0.076	0.194
Year 1976	-0.665	-0.072	-0.294	0.034	0.237	-0.157
	0.028	0.052	0.032	0.052	0.077	0.193
Year 1977	-0.595	-0.147	-0.226	-0.009	0.343	-0.355
	0.027	0.051	0.033	0.053	0.076	0.190
Year 1978	-0.609	-0.160	-0.211	-0.012	0.394	-0.452
	0.028	0.052	0.036	0.054	0.076	0.189
Year 1979	-0.687	-0.007	-0.308	0.061	0.249	-0.354
	0.037	0.060	0.049	0.065	0.080	0.191
Year 1980	-0.583	-0.035	-0.296	0.171	0.271	-0.345
	0.035	0.059	0.050	0.065	0.080	0.192
Year 1981	-0.515	-0.152	-0.246	0.178	0.399	-0.423
	0.036	0.060	0.052	0.068	0.081	0.191
Year 1982	-0.235	-0.213	0.064	0.170	0.688	-0.557
	0.029	0.052	0.044	0.057	0.078	0.189
Year 1983	-0.425	-0.204	-0.191	0.260	0.421	-0.415
	0.029	0.051	0.047	0.058	0.078	0.188
Year 1984	-0.387	-0.273	-0.291	0.359	0.318	-0.345
	0.030	0.052	0.052	0.062	0.080	0.189

Table 6 (continuation)

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Year 1985	-0.579	-0.126	-0.364	0.345	0.302	-0.424
	(0.034)	(0.055)	(0.056)	(0.065)	(0.082)	(0.190)
Year 1986	-0.685	-0.086	-0.326	0.424	0.245	-0.335
	(0.039)	(0.059)	(0.058)	(0.066)	(0.084)	(0.190)
Year 1987	-0.700	-0.080	-0.459	0.428	0.098	-0.289
	(0.040)	(0.060)	(0.064)	(0.071)	(0.087)	(0.191)
Year 1988	-0.722	-0.061	-0.570	0.564	0.047	-0.194
	(0.044)	(0.064)	(0.073)	(0.080)	(0.090)	(0.193)
Year 1989	-0.652	-0.086	-0.353	0.417	-0.012	-0.073
	(0.044)	(0.064)	(0.067)	(0.074)	(0.093)	(0.195)
Year 1990	-0.573	-0.113	-0.250	0.350	0.133	-0.142
	(0.044)	(0.065)	(0.069)	(0.074)	(0.093)	(0.194)
Year 1991	-0.530	-0.098	-0.377	0.571	0.249	-0.191
	(0.046)	(0.066)	(0.075)	(0.079)	(0.094)	(0.195)
Year 1992	-0.475	-0.193	-0.263	0.430	0.212	-0.083
	(0.045)	(0.066)	(0.074)	(0.076)	(0.097)	(0.196)
Heterogeneity parameters						
Variance of Intercept	0.0190		0.0505		0.0483	
	(0.0052)		(0.0081)		(0.0070)	
Variance of Learning Coeff.	0.0252		0.0893		0.0299	
	(0.0124)		(0.0120)		(0.0072)	
Covariance	0.0219		0.0672		0.0380	
	(0.0048)		(0.0072)		(0.0050)	
Observations:	273600		201695		169681	
(persons)	(16015)		(10420)		(10547)	
Log-likelihood:	-71640.8		-68800.0		-73834.3	

Note: The subsidy rate is the UI replacement rate multiplied by the maximum number of weeks of eligibility and divided by the minimum number of weeks to qualify. Unobserved heterogeneity in the intercept and the learning coefficient is modelled as a bivariate standard normal distribution. The model is estimated by simulated maximum likelihood (20 draws).

Table 7: Random Effect Probit Estimates for Each of the three Groups of Women for all Provinces, 1972-1992.

	Women born before 1946		Women born in 1946-55		Women born after 195	
	Main Effect	Interact. with Learning	Main Effect	Interact. with Learning	Main Effect	Interact. with Learning
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Intercept	-1.358 (0.128)	-0.093 (0.184)	-1.585 (0.139)	0.126 (0.188)	-2.307 (0.184)	0.423 (0.445)
First Lag	0.626 (0.016)	---	0.532 (0.016)	---	0.432 (0.016)	---
Second Lag	0.578 (0.017)	---	0.491 (0.016)	---	0.438 (0.016)	---
Third Lag	0.335 (0.017)	---	0.327 (0.016)	---	0.272 (0.015)	---
Fourth Lag	0.243 (0.017)	---	0.226 (0.017)	---	0.132 (0.016)	---
Age	-0.108 (0.026)	---	-0.066 (0.022)	---	-0.420 (0.032)	---
Age squared	0.011 (0.007)	---	0.005 (0.015)	---	-0.350 (0.026)	---
Subsidy Rate	0.105 (0.036)	0.140 (0.052)	0.178 (0.039)	-0.071 (0.052)	0.281 (0.035)	-0.162 (0.054)
Province Dummies:						
Nova Scotia	-0.124 (0.048)	-0.057 (0.059)	-0.109 (0.050)	-0.104 (0.058)	-0.363 (0.045)	0.146 (0.050)
New Brunswick	-0.083 (0.044)	-0.018 (0.053)	-0.076 (0.043)	-0.029 (0.050)	-0.273 (0.043)	0.166 (0.047)
Quebec	-0.073 (0.047)	-0.111 (0.058)	-0.111 (0.048)	-0.080 (0.058)	-0.281 (0.046)	0.000 (0.053)
Ontario	-0.193 (0.059)	-0.078 (0.076)	-0.138 (0.063)	-0.186 (0.076)	-0.462 (0.057)	0.001 (0.071)
Manitoba	-0.233 (0.058)	-0.061 (0.075)	-0.165 (0.063)	-0.198 (0.076)	-0.476 (0.057)	0.006 (0.069)

Table 7 (continuation)

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Saskatchewan	-0.405	0.036	-0.218	-0.277	-0.534	0.031
	(0.061)	(0.079)	(0.066)	(0.080)	(0.059)	(0.072)
Alberta	-0.428	-0.009	-0.518	0.024	-0.829	0.291
	(0.059)	(0.077)	(0.063)	(0.078)	(0.055)	(0.067)
British Columbia	-0.269	-0.004	-0.366	0.040	-0.467	0.051
	(0.052)	(0.064)	(0.055)	(0.066)	(0.049)	(0.057)
Year Dummies						
Year 1973	-0.181	---	-0.115	---	0.253	---
	0.034		0.035		0.151	
Year 1974	-0.317	-0.125	-0.169	-0.022	0.275	0.398
	0.036	0.079	0.037	0.083	0.144	0.453
Year 1975	-0.392	-0.073	-0.098	0.044	0.450	-0.008
	0.038	0.076	0.038	0.083	0.140	0.435
Year 1976	-0.376	-0.134	-0.132	0.030	0.499	-0.072
	0.038	0.076	0.041	0.080	0.139	0.413
Year 1977	-0.342	-0.157	-0.202	0.115	0.582	-0.077
	0.038	0.075	0.044	0.081	0.139	0.417
Year 1978	-0.342	-0.268	-0.154	0.047	0.664	-0.155
	0.039	0.075	0.046	0.081	0.139	0.415
Year 1979	-0.361	-0.021	-0.110	-0.008	0.727	-0.325
	0.048	0.086	0.057	0.092	0.141	0.417
Year 1980	-0.273	0.027	-0.087	0.050	0.683	-0.256
	0.048	0.084	0.058	0.091	0.142	0.416
Year 1981	-0.242	-0.019	-0.129	0.134	0.815	-0.337
	0.050	0.086	0.062	0.093	0.142	0.417
Year 1982	-0.123	-0.060	-0.040	0.192	0.940	-0.383
	0.044	0.077	0.056	0.085	0.140	0.415
Year 1983	-0.171	-0.102	-0.124	0.243	0.784	-0.290
	0.043	0.074	0.057	0.084	0.140	0.415
Year 1984	-0.111	-0.134	-0.134	0.259	0.833	-0.256
	0.045	0.077	0.061	0.086	0.141	0.415