

Webb, Matthew; Sweetman, Arthur; Warman, Casey

**Working Paper**

## How targeted is targeted tax relief? Evidence from the unemployment insurance youth hires program

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# How Targeted is Targeted Tax Relief? Evidence from the Unemployment Insurance Youth Hires Program

Matthew Webb  
Queen's University

Arthur Sweetman  
McMaster University

Casey Warman  
Dalhousie University

Department of Economics  
Queen's University  
94 University Avenue  
Kingston, Ontario, Canada  
K7L 3N6

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# How Targeted is Targeted Tax Relief?

## Evidence from the Unemployment Insurance Youth Hires Program<sup>1</sup>

Matthew D. Webb,

Department of Economics, Queen's University

Arthur Sweetman,

Department of Economics, McMaster University

Casey Warman,

Department of Economics, Dalhousie University

### Abstract

Targeted employment subsidy programs are commonly employed by governments. This study examines one such initiative that rebated unemployment insurance premiums for net new insurable employment for youth aged 18 to 24. Using microdata from two datasets to estimate the labour market impacts of this program, in each we find statistically and economically significant impacts of various measures of employment for the targeted age group relative to older age groups. Neither dataset exhibits a concurrent change in unemployment; rather a reduction in those not in the labor force is observed. Oddly, no program impacts are observed for females; all of the effects are for males. Notably, we find evidence of displacement – substitution away from slightly older non-subsidized workers towards the younger subsidized group. These spillovers suggest that the aggregate impact of the program is less than that observed for the targeted group.

JEL: J64, J65, J68, C12

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<sup>1</sup> The authors would like to thank Statistics Canada's research data center program for access to the data, as well as Shawn de Raaf, participants at the Canadian Economics Association Annual meetings, and Jonah Gelbach and James MacKinnon for discussions regarding inference. While the research and analysis are based on data from Statistics Canada, the opinions expressed do not represent the views of Statistics Canada.

## Introduction

In the wake of a recession governments are often interested in stimulating employment growth with programs targeting particular industrial sectors or demographic groups. One approach of potential interest in developed economies involves tax rebates or social insurance premium rebates. The Canadian government pursued two targeted initiatives involving unemployment insurance (UI - renamed employment insurance or EI since 1996) following the recession of the 1990s and is currently pursuing a similar strategy.<sup>2</sup> This paper examines the labour market impacts of a short term program that targeted youth unemployment. In 1999 and 2000 this program -- “Youth Hires” -- rebated any increase in aggregate UI premiums paid by firms for workers aged 18 to 24 that were in excess of the 1998 premiums they paid for that age group. While most economists believe that the relative inelasticity of the labour supply curve implies that changes in payroll taxes are passed on to workers through adjustments in wage rates in the long run, with minimal ensuing employment effects, there may be some scope for a short term program to affect employment levels during a period of slack labour demand. However, a program that targets a particular identifiable group may induce substitution towards the subsidized workers (i.e., displacement of close substitutes to the subsidized workers) and the program’s aggregate impact may differ from that experienced by the targeted group. We look for evidence of such effects.

A large research literature looks at optimal UI benefit rates (e.g., Chetty 2006, 2008), and the labour supply effects of UI, especially with respect to benefit duration (e.g., Meyer, 1990; Jurajda and Tannery 2003; Card, Chetty, and Weber 2007a, 2007b; for an overview see Krueger and Meyer 2002). Also, much research addresses how workers and firms tailor their behavior to

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<sup>2</sup> The current program is suitably titled the Hiring Credit for Small Business (HCSB – see <http://www.cra-arc.gc.ca/tx/bsnss/tpcs/pyrll/hwpyrllwrks/stps/hrng/hcsb-2012-eng.html>, accessed July 2012).

the parameters of the UI system, which is a particularly important phenomenon in Canada where UI is not experience rated (see, e.g., Green and Riddell 1997; Green and Sargent 1998; Kuhn and Sweetman 1998). Kuhn and Riddell (2010) also find appreciable long run responses to UI's parameters in their contrast of adjacent regions in the US and Canada.

However, we are aware of relatively few studies looking at targeted employment stimulus programs using social program premium rebates such as the Youth Hires program. A notable exception is Kugler, Jimeno, Hernanz (2003) who examine a reform that occurred in the Spanish economy in 1997. They examine a reduction in payroll taxes and in dismissal costs for permanent contract employees using the Spanish Labour Force Survey, which has a similar structure to the data used in this paper. Spain's reforms reduced dismissal costs by 25%, and payroll taxes by 40%. As in this paper, the authors exploit differences in tax reductions for different age groups. They compare the outcomes of those 20-29 against those 30-39, and find significant increases in the probability of being employed amongst the young treated population.

Similar historical US programs were the New Jobs Tax Credit (NJTC -- Perloff and Wachter 1979; Bishop and Haveman 1979; and Bishop 1981), and the Targeted Jobs Tax Credit (Hollenbeck and Willke, 1991). Bishop examines the impact of the NJTC that offered a tax credit of 50% on wages for the first \$4200 per employee for increases in employment in excess of 2%, with a cap of \$100,000 per firm that effectively favored smaller firms. He suggests that the program was responsible for between 150,000 – 670,000 jobs created between mid 1977-mid 1978 in the retail and construction industries. About \$2.4 billion in credits were claimed in 1977, and \$4.5 billion in 1978. Overall it is estimated that 28% of employers claimed a credit through the program in 1978. A broadly similar new proposal has been put forward by Bartik and Bishop (2009). In contrast to these US examples, the program under study is smaller on a per

capita basis and the Canadian approach did not require vouchers nor did it target individuals, other than by age, so there is no stigmatization. In fact, the administration of the Canadian model was through employers and the workers in question need not even have been aware of the UI premium rebate.

Unlike Bishop's work, we do not attempt to estimate the impact of the Youth Hires program on aggregate job creation. Rather, using a difference-in-differences framework we focus on the precursors to this by attempting to ascertain if there are any impacts on the targeted age group, and any displacement effects on those a little older. Classic work on displacement effects is Davidson and Woodbury's (1993) exploration of the UI bonus experiments. Their study showed the power of formal modeling and found that displacement undid a modest but nontrivial proportion of the program's benefits, however it also relied on particular economic theories and specific functional form assumptions. Dahlberg and Forslund (2005) is a more recent examination of displacement from wage subsidies and training exploiting variation across municipalities in Sweden, and they find substantial displacement effects from subsidies. Understanding the magnitude of any displacement effects in different contexts is fundamental to the evaluation of many labor market interventions.

Additionally, in our estimation context, there are well known problems of inference given that the source of randomization, the policy change, is at the aggregate level. We explore the sensitivity of our results to alternative methods for dealing with this issue, and for most of the analysis employ Cameron, Gelbach and Miller's (2008) approach to avoid over rejecting the null hypothesis. Overall, we observe a modest although discernible impact of Youth Hires in that it increases employment for the targeted 18-24 age group, but we also find what appears to be a concurrent employment decrease for those 25 to 29 suggesting that some substitution or

displacement is occurring.

The paper is structured so that the next section provides the institutional background, and then the third section describes the two independent data sets employed, defines two comparison groups that have different strengths and weaknesses, and presents descriptive statistics and an initial graphical analysis. Following that, the fourth section addresses the econometric methodology with a focus on issues of interpretation and inference that are relevant in this context, the fifth presents the empirical results where similar findings from both datasets add to our confidence in the analysis, and the final section concludes.

### **Institutional Background Regarding UI and Youth Hires Program**

Legally, the incidence of Canadian UI premiums is partitioned across employers and employees with employers paying 1.4 times the employee rate, which varies from year to year. This system is notably different than the American program in that it operates nationally, and premiums are set by the federal government and are not experience rated against the history of either the employer or the employee.<sup>3</sup> Premiums are invariant across regions and, since the premium rebate affected employers in all regions equally, we estimate the impact at the national level.

Youth Hires was announced in the federal budget on February 24, 1998 and was described as being a temporary measure in place in 1999 and 2000 to address high youth unemployment rates. For workers who were aged 18-24 at any point during each calendar year, any premiums paid in 1999 and 2000 in excess of the firm's 1998 premiums were refunded to the employer. Employer premium rates in 1998, 1999 and 2000 were respectively 3.78%, 3.57%

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<sup>3</sup> For a short period starting in 1997 the benefit rate was experience rated on the employee side. It decreased as the number of weeks of benefit receipt in the previous five years increased.

and 3.36% of insurable earnings with the maximum insurable earnings fixed at \$39,000 in nominal terms. The maximum rebate – for a net new employee aged 18-24 earning \$39,000 or more – was therefore \$1,392.30 in 1999.

One idiosyncrasy is that the premium rate decline implied that a firm's aggregate UI insurable payroll for those in the relevant age group had to increase by, for example, 0.21% in 1999 before the firm was entitled to the first dollar of rebate. Further, although the intention of the program was to increase youth employment by using premiums paid as the benchmark, employers had several margins on which they could adjust. Employers were eligible for the credit if they increased insurable earnings sufficiently by any combination of increasing wages (including regular annual increases), the number of young workers employed, and/or hours per year for existing young employees. However, firms received no credit towards the rebate for any individual worker's annual earnings that were in excess of the maximum insurable \$39,000 limit. For more information on this program see Canada Employment Insurance Commission (1999, 2000, 2001).

Employers' information set is an extremely important determinant of any behavioral change they might undertake. If they are unaware of the program, then it only operates through easing the budget constraint on expanding firms and not through the behavioral change required to target youth; this also affects the timing of any effect. Clearly, in this case the government was interested in behavioral change since the goal of the program was to target unemployment among a specific age group. In addition to discussions of the program in the media and mailings to human resource departments or others in firms paying UI premiums, this program had the advantage of following on the heels of a similar program, the New Hires Program, that operated in 1997 and 1998. The earlier program refunded UI premiums associated with net job growth in



small businesses.<sup>4</sup> One of the criticisms of the earlier program was that many small firms were not aware of its existence. Also, it required an application to receive the refund that many small businesses found difficult (Canada Employment Insurance Commission, 2000). By contrast, Youth Hires was more broadly known and the premium rebate was presented as being automatic and without administrative burden thereby making it more attractive. The program refunded over \$400 million in premiums to approximately 295,000 firms (Canada Employment Insurance Commission, 2002).<sup>5</sup>

An important limitation to our analysis is the very substantial reform associated with the move from the UI to EI system, which was phased in during the six months ending January 1, 1997. This limits our ‘before’ period to two years for difference-in-differences analyses, and also limits any ‘falsification’ exercises in the pre-program period. One particularly relevant element of the reform for youth is that prior to the reform UI did not cover part-time jobs, defined as below both 15 hours per week and an earnings threshold, whereas EI premiums are paid from the first hour of work. Friesen (2002) finds a modest shift away from part-time, and towards full-time, employment following the move to EI and the associated introduction of EI premiums for part-time employment.<sup>6</sup>

Given the nature of the Youth Hires program, we would not necessarily expect its introduction and termination to have equal and opposite impacts. If firms react to the incentive and hire new young workers they must incur at least some training and other fixed hiring costs and, therefore, may continue to employ these workers after the rebate period expires. Of course,

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<sup>4</sup> The program entitled firms with EI premiums of up to \$60,000 to a full rebate on additional hires in 1997. It is broadly similar to the current program. Unfortunately, we are unable to examine the New Hires Program due to data limitations.

<sup>5</sup> Given the roughly 10:1 ratio between the sizes of the Canadian and US economies and the exchange rates in effect at that time, this would have been equivalent to total US program expenditures of approximately \$US5.9 billion across the two years in \$1999.

<sup>6</sup> Workers with very low annual earnings – far too low to qualify for benefits – have the employee share of their premiums refunded through the tax system. However, no such refunds are made to employers.

job mobility rates are quite high for young workers; thus while any impact may continue beyond the program's life it will attenuate over time. In this vein, one group that will need special attention are those who are age 24 in the first year of the program but too old to be subsidized in its second year. We address this last group in the empirical specification.

## **Data and Descriptive Statistics**

We analyze individuals residing in Canadian provinces using the masterfiles of Statistics Canada's Survey of Labour and Income Dynamics (SLID) and Labour Force Survey (LFS). The SLID is a rotating panel that contains roughly 60,000 individuals in each wave, with overlapping waves starting every three years and lasting for six years; each individual's annual labor market outcomes are detailed. In contrast, the LFS is comparable to the US Current Population Survey and interviews roughly 54,000 households comprising about 100,000 individuals and capturing labour market information on the week that contains the 15th of each month. For both datasets survey weights are used throughout.

Given that the EI reforms make it difficult to use data before 1997, and that hiring and training costs suggest the effects of the program are likely to continue beyond its termination, the bulk of the analysis focuses on 1997-2000, that is, the two years before, and the two years of, the program's implementation.

Two comparison groups, with different strengths, are employed. The data for analysis are initially restricted to those aged 18-30, but the age range is then expanded to those aged 18-35. The advantage of having a comparison group aged 25 to 30 is that this age group operates in labor markets that are more similar to those for 18 to 24-year-olds and this make a useful

comparison group. But, the same characteristics that make them a good comparison group also means that they are reasonably close substitutes in hiring/employment for the targeted younger workers, and may be negatively affected by Youth Hires with displacement occurring. In contrast, 18-24 year olds are less substitutable for those aged 30-35, which is beneficial in the latter group serving as a comparison group, but the older group also is less likely to have a common trend in employment in the absence of the program, making it a slightly less satisfactory comparison group. Using both allows two perspectives on the policy change. The possibility of including individuals younger than age 18 was explored, but not pursued given the very large share still in high school, making them less comparable since they have different labour market dynamics.

Any significant impact of Youth Hires could affect variables for individuals treated such as the likelihood of being employed, or hours or weeks worked. However, as seen for minimum wages (e.g., Landon, 1997; Neumark and Wascher, 1996, 2001, 2004), while government policy may be motivated by unemployed youth aged 18-24 who are out of school, post-secondary (or even high school) attendance may also be affected for this age group, so we also investigate that outcome.

### **Summary Statistics**

Table 1 contains mean values and sample sizes of dependent variables to be used in similarly specified regressions. These are presented by age group for the two years prior to, and the two years of, the Youth Hires program. In the upper panel the first three variables are from the SLID and are counts of annual weeks of employment, unemployment and not in the labor force status. These variables are mutually exclusive and sum to the number of weeks in the year. Next are

three annual indicator (0/1) variables that are not mutually exclusive. The first is equal to one if the individual was employed at any point in the year, and zero otherwise. In a different vein, the second variable of this set measures the fraction of individuals who were not employed in the year although they sought employment (or were unemployed) at some point in the year. Similarly, the ‘not in the labor’ force indicator is set to one if the person is out of the labor market at any point in the year. Total hours worked at all jobs in the year is next, and following it are the natural logarithm of total annual income and the hours-weighted average hourly wage across all jobs. Both of the earnings measures are deflated to \$1999. The new job variable indicates whether an individual started with a new employer in the reference year, and the full time indicator is set to one if an individual’s primary job was full time. If the person was a full-time student at some point in the year the student variable is set to one.

**{{Place Table 1 about here}}**

In the lower panel of Table 1 the same statistics are presented for variables from the LFS. All variables in the LFS refer to the reference week. The LFS binary variables for employed, unemployed and not in the labour force are mutually exclusive and exhaustive. Total weekly hours worked is for all jobs in the reference week. Weekly income includes all income earned in those jobs, and is converted to the natural log of 1999 dollars; the hourly wage is likewise converted and is a weighted average for all jobs worked. New job is defined only for those who are currently working and is set to 1 if an individual started a new job in the reference week. Finally, student is a variable which indicates whether the individual was a full time student in the reference week.

## **Graphical Analysis**

Plots for three different variables from two datasets are provided to illustrate the time trend in relevant dependent variables in the years of, and surrounding, the Youth Hires program.

**{{Place Figure 1 about here}}**

For various age groups in the SLID, Figure 1 shows the trends in annual total weeks employed, which is a central variable given the aims of the program. In the first year of the program, 1999, there are opposite effects for those treated by and those excluded from the program. In 1999 we can see a sharp year over year increase in weeks employed by those aged 22-24. This contrasts with a slight decline by those 25-27 and 28-30. At the same time the weeks worked by those 18-21 increased in line with a trend experienced throughout 1997-2002. The sharp increase for those 22-24, coupled with the slight decline for those 25-27, is what one would expect to see if the program was effective in stimulating employment for the targeted group and simultaneously generating a modest amount of substitution/displacement. However, in contrast to this effect seen in the first year the program, there is no obvious “bump” in the second year.

**{{Place Figure 2 about here}}**

Weeks not in the labor force, also from the SLID, is presented in Figure 2 and a conceptually similar pattern is obvious. Of particular note, especially in the first year of the program, is the increase in weeks not in the labour force for those 25-27 coincident with a decrease in the weeks out of the labour force for those aged 22-24. Recalling that employers were eligible for the credits if they hired those 18-24 in 1999 or 2000, it appears as though

workers of the younger age group were brought into the labor force in 1999 while those just excluded from (too old for) the program were slightly displaced. But, although there may be some ongoing effect, no additional increment is apparent for the second year of the program.<sup>7</sup>

**{{Place Figure 3 about here}}**

Figure 3 uses LFS data to plot the employment rate over time for the various age groups, in which we see a comparatively large increase in the employment rate for those aged 18-21 in the first year of the program. Here the other age groups also see increases, though not as large, in their employment rates which is to be expected as general economic conditions were improving. Although we do not want to draw too many conclusions at this stage of the analysis, these graphs support the idea that employers were preferentially hiring those subsidized by the program. Moreover, although noticeable, the magnitude of the aggregate affect is modest in all three graphs. Clearly, there are a large number of employers who are increasing the size of their workforce as a result of macroeconomic trends and for whom this rebate is a windfall gain.

### **Econometric Approach**

We employ a framework that, in terms of the equations estimated, looks like a difference-in-differences (DID) specification. However, for the initial set of models estimated (and perhaps for all of them) the results do not have the usual interpretation as the “causal” impact of the

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<sup>7</sup> In discussions with stakeholders regarding Youth Hires it was suggested that some employers were initially drawn to the rebate, but then realized that the rebate was not sufficient given the productivity differences across the age groups in question. However, this is purely speculative.

treatment on the treated since both theory and the graphical analysis suggest that the common trend assumption required to identify such a parameter is not satisfied (see, e.g., DiNardo and Lee, 2010) given that the program potentially has both direct causal impacts on the targeted age groups, and indirect causal impacts on workers slightly older than the program's maximum age. That is, it seems plausible that the 25 to 30 age group, which is too old for Youth Hires, is displaced by the program. In this situation, the DID coefficient is perhaps best interpreted as the causal change in the gap between the treatment and comparison groups across the policy periods assuming that they would otherwise have a common trend, and not as the impact of the policy change on the treatment group.

Beyond identification, inference using the DID specification with a policy change at the aggregate level can be problematic as demonstrated by Bertrand, Duflo, and Mullainathan (2004) who find, among other issues, that the standard approach which relies on the asymptotic properties of the cluster-robust variance estimator does not function well when there are a small number of clusters. In this case there are only 16 clusters since we take each annual birth cohort as the basic unit affected by the policy change.

Cameron, Gelbach, and Miller (2008 - CGM hereafter) based on both econometric theory and Monte Carlo evidence argue that wild cluster bootstrap-t techniques work well even when the number of clusters is small; we employ this approach. In accord with Donald and Lang (2007), CGM's Monte-Carlo simulations also suggest that over rejection is less severe if we assume that the t-statistics follow a distribution with  $G-2$  degrees of freedom, with  $G$  being the number of clusters and 2 being the number of within-cluster parameters estimated. Initially, we explore alternative approaches to inference and observe some variation. However, for the vast majority of the analysis we present only results from our preferred method of inference, which is

to generate bootstrap samples using the wild cluster bootstrap-t technique with the null hypothesis imposed. This allows us to generate a distribution of t-statistics (which allows for asymptotic refinement since the t-statistic is asymptotically pivotal) from which a p-value for the test statistic can be obtained.<sup>8</sup>

The first specification we estimate employs data from 1997-2000 and uses those aged 18-24 as the treated group, with 25-30 year-olds as the comparison group, and is specified in equation (1).

$$(1) \quad Y_{it} = \beta_o + \beta_{YH}YH_{it} + \beta_{99}Only1999_{it} + \beta_A Age_{it} + \beta_B YearBorn_i + \beta_Y Year_t + [\beta_C Controls_{it}] + \varepsilon_{it}$$

In this equation  $Y_{it}$  represents a labor market variable of interest;  $YH$  is the Youth Hires indicator which is set to one if individual  $i$  is of an age to be affected by the program in a year,  $t$ , when it is operating; and  $Only1999$  is an indicator set equal to one for individuals who qualify for the subsidy in the first year of the program, but not the second.  $Age$ ,  $YearBorn$  and  $Year$  are all vectors comprising full sets of indicator variables that respectively represent the individual's age (measured in years as of year  $t$ ) and birth year, and the calendar year in question. This represents an effort to flexibly control for any background effects that may influence the coefficient of interest. The vector of variables identified as  $Controls$  are in brackets to indicate they are included in some, but not all, specifications. For both datasets the control variables are province of residence as well as an indicator for urban residence, and the SLID regressions additionally control for race and immigrant status. Models estimated using the LFS, but not the SLID, also include a full set of months indicators. The  $\beta$ s are vectors of coefficients to be estimated.

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<sup>8</sup> We thank Cameron, Gelbach and Miller for making their code available. A downside is that this approach bypasses the generation of standard errors, which Angrist and Pischke (2009) argue some economists like to observe.



In all cases the equations are estimated using ordinary least squares (OLS), hence some specifications are linear probability models. Clustering is on the individual's birth year since that allows a longitudinal dimension;  $\varepsilon_{it}$  is allowed to be arbitrarily correlated within clusters, but is assumed to be independent across birth cohorts. In some specifications employing the SLID data, the error term is decomposed to include an individual fixed effect recognizing that individuals are in the data for (up to, since there is some attrition) six years.

The coefficient  $\beta_{YH}$  is the DID variable of interest and as mentioned its estimate will conflate any positive impact on those in the treatment group with any negative impact those in the comparison group may experience in the years when the program is operating. We are agnostic as to the expected sign of  $\beta_{99}$  since it will hinge on the impact of the program in 1999 and the degree of labor market attachment in the subsequent year. We do not report the coefficients for  $\beta_{99}$  in the text, though in general the coefficients are of the same sign, smaller in magnitude and of lesser significance than the coefficients for  $\beta_{YH}$  on a regression by regression basis.

A subsequent specification is estimated using data on individuals aged 18-35. It is not shown since it is remarkably similar to equation (1) except that the YH indicator is interacted with a set of indicators for those in the 18-21, 22-24, 25-27, and 28-30 age groups. Individuals aged 31 to 35 serve as the omitted comparison group. Plausibly, this comparison group is not (or is extremely minimally) affected by the Youth Hires program, so substitution/displacement is minimized. However, it is less credible that this older age group would have a similar trajectory across time as that of the treated age groups in the absence of the policy change. That is, the common trend assumption is less credible given the larger gap in age and the well-known differences across the business cycle in rates of unemployment, job turnover and the like with

age. Nevertheless, any difference in trends may not be that large over a short period and this model is estimated since it allows heterogeneity across age groups during the time of the program to be observed. Additionally, the estimates for this group are shown using only the 1998-1999 data to capture the primary impact of the policy. The pattern of results was generally the same when estimated using the 1997-2000 data though these results are not presented in the paper.

An attempt was made to conduct a three period analysis of the program, trying to determine the outcomes of the targeted group before, during and after the program. However, this was frustrated by the lack of a clear comparison group in the “after” period. The issue is that the individuals treated in 1999 and 2000 would be those aged 20-26 in 2002, but that age range would contain both treated and untreated individuals in the year 2000.

To further test the robustness of our research design we conduct a series of falsification exercises (see DiNardo and Lee, 2010, regarding the benefits of falsification tests) using data from 2002 to 2005 (i.e., leaving a two-year gap after the end of the program in case there are any “knock on” effects, but being reluctant to extend too far from the policy change given the possibility of other age-specific changes derived from the education system). In general it would be preferable to conduct a falsification exercise using a period prior to the program. Unfortunately, as discussed, the significant reforms in 1996-1997 render any pre-period analysis problematic.

### **Regression Analysis**

Various approaches to inference for equation (1) using three key dependent variables, all

measures of employment which is the central policy variable for Youth Hires, are presented in Table 2. The first two regressions are from the SLID, and the third is based on the LFS. For each dependent variable there are two OLS specifications – one with a minimal set of covariates, and the other with a full set of controls. For the data from the SLID there is also a specification including both individual fixed effects and a full set of controls. In all cases, but particularly for the OLS regressions which are less time-consuming to bootstrap, an extremely large number of bootstrap replications are employed to increase the precision with which we can estimate p-values.

**{{Place Table 2 about here}}**

### **Comparison Group Aged 25-30**

Coefficients are presented in the first line and show sensible modest increases among the targeted group, relative to the slightly older one, associated with the program: approximately two to two and a half extra weeks of employment in the year, and an increase of about 3.5% to 4% in the employment rate as measured in the SLID or just over 1% as measured using the LFS. When clustering is ignored, but the standard errors are adjusted for heteroscedasticity of unknown form, the p-values in the SLID are larger than those observed in any other test, but when individual fixed-effects are employed (with the associated clustering on the individual) the p-value seems inappropriately small. In the subsequent rows a series of p-values are presented using Stata's "cluster" command, but choosing different degrees of freedom. Given the 16 clusters in this dataset the degrees of freedom adjustment makes a difference, but it is relatively modest. Two implementations of the wild cluster bootstrap-t are also undertaken – the first without and the second with the null hypothesis imposed. For the OLS models the p-values

increase slightly, but still mostly indicate statistical significance at conventional levels. For the fixed effect model, the p-values actually decreased slightly. In accord with the evidence in CGM and Davidson and MacKinnon (1999), we take the wild bootstrap with the null imposed as our preferred approach to inference. It is reassuring, however, to see that there are not enormous differences in inference across the last three approaches, which are arguably superior to the others. Although we do not present the information to save space, we observe similar patterns for the other dependent variables that we tested where the coefficients were statistically significant.

In terms of the substantive results, they almost everywhere indicate statistical significance at conventional levels with the least statistically significant p-value being 12%. This provides robust evidence – based on alternative approaches to inference and three variables from two datasets – that Youth Hires had a causal effect increasing employment for the targeted age group relative to those slightly older.

Results from the specification in equation (1) for a wide range of dependent variables are presented in Table 3. In the upper panel are those from the SLID, while those from the LFS are in the lower one. Among the dependent variables from each dataset those at the top of each panel are alternative measures related to employment, unemployment and out of the labor market status. The SLID provides two measures of each, whereas the LFS only has one. Employment is the only variable for which there is a strong prior expectation regarding the sign of the coefficient if the program is functioning as intended. Although some government planners might also have expected unemployment to decrease, it is well known by labor economists that the unemployment rate may increase in the early part of the expansionary period of a business cycle as the economy improves since discouraged workers shift their labor market status from out of the labor market to unemployed as they begin active job search. Depending upon whether this

phenomenon occurs, the unemployment and or ‘not in the labor force’ coefficients could be negative, positive or close to zero.

**{{Place Table 3 about here}}**

Those dependent variables in the lower half of each panel represent ancillary features of the labor market that may be affected by the policy change, but we are agnostic regarding the expected sign of the coefficient since theory suggests that there might be opposing effects in operation. For example, average hours of work could increase if any additional employment results from increasing the hours of part-time workers, potentially making them full-time, or could decrease if additional part-time youth are added to the labor force. Additionally, as with minimum-wage legislation, the additional opportunities for employment could draw youth out of school so that the percentage who are full-time students might decline, but there need not be an effect on this dimension. These dependent variables are included to improve our understanding of the program’s impact.

The models in this table the data are also divided along gender lines, and this highlights a very interesting finding. Essentially none of the coefficients, in both datasets, are statistically significant for females. For some reason the entire policy response to the Youth Hires program appears to be concentrated among the males. Or, alternatively, the response is more muted for females and is statistically insignificant given the limited precision feasible with only 16 birth cohorts (degrees of freedom).

Looking first at the coefficient estimates in the upper half of the table for each dataset, the positive effect on employment seen in Table 2 is repeated in Table 3.<sup>9</sup> Moreover, both

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<sup>9</sup> It is important to note the differences in the p-values for the employment regressions in Table 2 and Table 3 result from the Table 2 values being generated using 9999 bootstraps, while the Table 3 values are generated using 1499 bootstraps. Even with 1499 bootstraps the estimated p-values are not yet stable, which is particularly remarkable as

datasets are consistent in finding that there is no statistically significant change in the unemployment rate associated with the program; rather, there is a reduction in the various measures of ‘not in the labour force’. Overall, and this is a central finding of the analysis of this program, there is consistent evidence that the program served to increase the relative employment rate of the targeted group compared to those slightly older, but the aggregate effect was to draw youth out of the not in the labor force state.

Hours of work are not statistically significantly affected by the policy change in either dataset, and the estimated coefficients are of opposite signs across the datasets. Wages and/or annual earnings also appear to be largely unaffected, although the point estimates are mostly negative and one of them is statistically significant. Similarly, the results are mixed for the incidence of new jobs, but there appears to be a small decrease in the LFS, and also a small decrease in the incidence of full-time employment for youth. Finally, there is no evidence that this policy change is inducing students to leave their studies using this comparison group.

### **Comparison Group Aged 31-35**

Those aged 31-35 serve as the comparison group in the regressions in Table 4 where they are compared to those subsidized by the premium rebate, as well as two those aged 25-30 who were previously used as the comparison group. Further, those 18-24 and 25-30 are each subdivided into two smaller age groups to highlight any patterns across age. The very first row, looking at annual weeks employed in the SLID, tells an interesting story. Relative to the 31-35 age group, the younger two groups have point estimates that show appreciable relative increases in their weeks of work, with that for the youngest group being statistically significant. In contrast, the

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1499 is regarded to be quite a large number of bootstraps; see Davidson and MacKinnon (2000).

coefficients for the two age groups just outside the age cut off for the EI premium rebate have negative coefficients, with that for the older of these two being statistically significant. Akin to the informal analysis of Figure 1, this suggests displacement/substitution is likely to have occurred as a result of Youth Hires. For the LFS, the coefficients on employment tell a similar story, but the pattern is not quite as extreme in that neither of the coefficients for the two older age groups is statistically significant and one of them is actually positive although close to zero. Although the coefficients bounce around a little, the remainder of the coefficients on labor force status variables largely support an interpretation suggesting that the policy change increased employment among the targeted age groups, but some of their increased employment came at the expense of those just above the subsidy cut-off. Clearly, understanding the spillovers from this program is important for understanding its aggregate impact. In terms of the magnitude of the effects, they are appreciable, but not enormous, which accords with the magnitude of the subsidy associated with Youth Hires.

**{{Place Table 4 about here}}**

By breaking the subsidy-eligible group into an older and younger half, Table 4 also makes obvious the finding that the effects of the program appear to be larger for the 18-21 age group than the 22-24 one, which differs slightly from the informal graphical analysis. Also, some of the coefficients in the bottom half of each dataset's panel that were not statistically significant in Table 3 are significant in Table 4. In particular, there is some evidence in this specification that students were drawn out of school as a result of the subsidy to employment targeted at their age group.

## **Falsification Tests**

Although the timing of the data for the falsification tests presented in Table 5 is not ideal, the results provide some support for the analysis. Of the 63 regression coefficients estimated, seven of them are statistically significant at the 10% level, with most of these being significant between 5% and 10%. This is well within the range of what one would expect if random numbers were being regressed to generate the coefficients in question. (Of course, it also reminds us that results in the earlier tables are subject to both type I and type II errors; although the earlier tables have, proportionately, far more coefficients that are statistically significant and typically with smaller p-values.) Importantly, all of the employment related coefficients have point estimates very close to zero and none of them are statistically significant. Also, unlike the earlier tables, the significant coefficients in Table 5 do not accord with a pattern that is easy to interpret as being consistent with an alternative interpretation of the program's impacts. Overall, there does not appear to be evidence to undermine the conclusions in this analysis.

**{{Place Table 5 about here}}**

## **Discussion and Conclusion**

In this paper we examined the effectiveness of a stimulus program in Canada that was designed to temporarily target youth unemployment. The Youth Hires program subsidized employers to hire youth between the ages of 18 and 24 by rebating unemployment insurance premiums for net new insured employment. Overall, we believe the evidence supports the conclusion that this program serves to increase employment among the subsidized population. However, Youth Hires also appears to have had an impact, albeit smaller, in reducing the labor market outcomes of those slightly too old to be eligible for the EI premium rebate. This substitution/displacement



points to the trade-offs ubiquitous in social policy development, especially when programs seek to assist targeted groups. Of course, an analysis such as this cannot answer the broader general equilibrium question about the number of jobs produced in the economy as a whole as a result of the program, but it lays the foundation for such an analysis. Oddly, it appears that the effect of the program was predominantly, if not entirely, experienced by males. For some reason females appear not to have been much affected.

In interpreting these results it is worth remembering that the value of the annual rebate for net new youth employment was relatively modest – only around 3.5% of earnings below \$39,000 per worker/year – so an impact larger than that observed is not expected and a substantial percentage of the total subsidy payment can be thought of as a windfall gain for employers. Any future cost-benefit analysis of the program needs to take into account the likelihood that some of the costs are borne by those aged 25 to 30 (and perhaps others) via substitution/displacement.

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**Table 1: Variable Means and Sample Sizes 1997-98 and 1999-2000**

	18-24 (Treated)				25-30 (Comparison)			
	Before (97-98)		During (99-00)		Before (97-98)		During (99-00)	
	Mean	N	Mean	N	Mean	N	Mean	N
<b>SLID</b>								
Annual Weeks Employed	30.01	14,643	31.90	12,970	39.90	11,848	40.67	10,298
Annual Weeks Unemployed	4.36	14,643	3.68	12,970	4.35	11,848	3.16	10,298
Annual Wks Not Lbr Force	18.63	14,643	17.43	12,970	8.74	11,848	9.17	10,298
E Any Time in Year	0.78	14,643	0.80	12,970	0.85	11,848	0.86	10,298
U Any Time in Year	0.07	14,643	0.05	12,970	0.05	11,848	0.03	10,298
N Any Time in Year	0.54	14,643	0.53	12,970	0.25	11,848	0.27	10,298
Total Annual Hours	909.04	14,643	968.31	12,970	1507.81	11,848	1546.24	10,298
ln(Annual Income)	8.89	11,801	8.96	10,593	9.78	10,065	9.89	8,499
ln(Average Wage)	2.16	11,801	2.19	10,593	2.53	10,065	2.60	8,499
New Job	0.52	12,024	0.54	10,789	0.26	10,466	0.30	8,965
Mostly Full-time work	0.64	12,018	0.64	10,788	0.85	10,454	0.87	8,960
Full-time Student in Year	0.66	14,622	0.65	12,946	0.21	11,832	0.22	10,277
<b>LFS</b>								
Employed	0.63	274,993	0.66	264,605	0.78	237,961	0.80	220,944
Unemployed	0.10	274,993	0.09	264,605	0.08	237,961	0.06	220,944
Not in Labor Force	0.27	274,993	0.26	264,605	0.14	237,961	0.14	220,944
Total Weekly Hours Worked	30.29	168,306	30.83	170,900	35.83	180,862	36.19	172,578
ln(Weekly Income)	5.49	158,980	5.55	162,725	6.16	161,197	6.20	155,402
ln(Wage)	2.20	158,980	2.22	162,725	2.59	161,197	2.62	155,402
New Job	0.04	168,829	0.08	171,700	0.02	181,090	0.03	172,841
Student	0.37	276,730	0.37	266,192	0.09	240,447	0.09	223,081

**Table 2: Difference-in-Differences Employment Regressions**

	SLID						LFS	
	Weeks Employed			Employed			Employed	
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS
Coefficient	2.409	2.320	1.949	0.044	0.036	0.037	0.012	0.012
Hetero. consistent std err	1.307	1.274	0.610	0.025	0.024	0.012	0.005	0.005
Clustered std err	0.730	0.784	0.965	0.015	0.016	0.017	0.005	0.005
t-stat hetero	1.844	1.821	3.195	1.748	1.487	3.041	2.426	2.405
t-stat cluster	3.299	2.957	2.021	2.970	2.246	2.188	2.432	2.368
p-value hetero df=N-k	0.065	0.069	0.001	0.081	0.137	0.002	0.015	0.016
p-value cluster df=N-k	0.001	0.003	0.043	0.003	0.025	0.029	0.015	0.018
p-value cluster df=G-1	0.005	0.010	0.062	0.010	0.040	0.045	0.028	0.032
p-value cluster df=G-2	0.005	0.010	0.063	0.010	0.041	0.046	0.029	0.033
p-value wild bootstrap	0.020	0.052	0.029	0.049	0.120	0.033	0.057	0.065
p-value wild bootstrap null imposed	0.009	0.026	0.056	0.020	0.092	0.019	0.077	0.061
Bootstrap replications	9999	9999	1499	9999	9999	1499	9999	9999
Number of clusters	16	16	16	16	16	16	16	16
Observations	49,759	49,759	49,759	49,759	49,759	49,759	985,148	985,148
Min cluster size	928	928	928	928	928	928	20,452	20,452
Average cluster size	3,445	3,445	3,445	3,445	3,445	3,445	69,165	69,165
Full set of controls	No	Yes	Yes	No	Yes	Yes	No	Yes

Notes: Variables included in the regressions with controls are: province of residence and urban residence for both datasets, visible minority and immigrant status for SLID. The 'hetero. consistent std error' for the FE regressions and the associated t-stat and p-value are estimated clustering on the individual, whereas the estimates for the 'hetero consistent std err' off the OLS regressions are not clustered.

**Table 3: Difference-in-Differences Estimates for 18-24 Year Olds, 25-30 Comparison Group**

	All		Female		Male	
	Coeff	Wild p-value	Coeff	Wild p-value	Coeff	Wild p-value
<b>SLID</b>						
Annual Weeks Employed	2.320	0.019	1.028	0.504	3.602	0.007
Annual Weeks Unemployed	0.059	0.851	0.714	0.209	-0.562	0.545
Annual Wks Not Lbr Force	-2.379	0.008	-1.742	0.355	-3.040	0.007
E Any Time in Year	0.036	0.113	0.026	0.559	0.048	0.031
U Any Time in Year	-0.005	0.736	0.024	0.248	-0.034	0.171
N Any Time in Year	-0.055	0.016	-0.011	0.677	-0.100	0.037
Total Annual Hours	22.888	0.480	14.974	0.713	34.657	0.345
ln(Annual Income)	-0.073	0.108	-0.060	0.481	-0.090	0.056
ln(Average Wage)	-0.038	0.017	-0.049	0.185	-0.027	0.337
New Job	-0.006	0.880	0.050	0.292	-0.049	0.393
Mostly Full-time work	-0.042	0.021	-0.002	0.972	-0.077	0.024
Full-time Student in Year	-0.028	0.248	-0.013	0.591	-0.044	0.152
<b>LFS</b>						
Employed	0.012	0.056	0.002	0.847	0.022	0.056
Unemployed	0.004	0.440	0.005	0.539	0.002	0.795
Not in Labor Force	-0.015	0.007	-0.007	0.524	-0.024	0.011
Total Weekly Hours Worked	-0.113	0.605	-0.309	0.269	0.109	0.807
ln(Weekly Income)	-0.003	0.701	0.004	0.787	-0.008	0.557
ln(Wage)	-0.002	0.759	0.001	0.916	-0.004	0.788
New Job	-0.028	0.007	-0.033	0.001	-0.023	0.100
Student	0.005	0.520	0.013	0.184	-0.002	0.916

Notes: Wild p-values based on 1499 bootstrap replications. All regressions have the full set of control variables listed in Table 2.

**Table 4: Difference-in-Differences Coefficient Estimates with 31-35 Year Olds as the Comparison Group**

	18-21		22-24		25-27		28-30	
	Coeff	Wild p-value	Coeff	Wild p-value	Coeff	Wild p-value	Coeff	Wild p-value
<b>SLID</b>								
Annual Weeks Employed	3.196	0.020	2.953	0.476	-0.619	0.349	-1.696	0.080
Annual Weeks Unemployed	1.256	0.160	0.437	0.175	0.274	0.167	0.233	0.696
Annual Wks Not Lbr Force	-4.452	0.008	-3.390	0.440	0.345	0.600	1.463	0.008
E Any Time in Year	0.053	0.117	0.023	0.472	-0.019	0.241	-0.023	0.404
U Any Time in Year	-0.019	0.253	0.018	0.009	0.000	0.849	0.001	0.943
N Any Time in Year	-0.059	0.009	-0.081	0.272	0.021	0.203	0.048	0.009
Total Annual Hours	130.932	0.027	105.237	0.333	-0.493	0.967	-64.506	0.063
ln(Annual Income)	0.212	0.008	0.131	0.196	0.078	0.291	0.014	0.820
ln(Average Wage)	0.021	0.348	0.030	0.268	0.064	0.111	0.028	0.475
New Job	-0.019	0.325	-0.078	0.148	-0.056	0.319	0.020	0.025
Mostly Full-time work	0.078	0.032	0.018	0.707	0.026	0.239	0.016	0.471
Full-time Student in Year	-0.095	0.007	-0.104	0.035	-0.021	0.292	0.020	0.281
<b>LFS</b>								
Employed	0.061	0.008	0.034	0.008	0.010	0.171	-0.003	0.705
Unemployed	-0.007	0.372	0.000	0.691	-0.008	0.044	-0.001	0.323
Not in Labor Force	-0.053	0.008	-0.033	0.024	-0.002	0.719	0.004	0.703
Total Weekly Hours Worked	3.106	0.005	2.090	0.004	0.495	0.132	0.269	0.325
ln(Weekly Income)	0.225	0.016	0.148	0.001	0.073	0.015	0.027	0.184
ln(Wage)	0.078	0.004	0.065	0.035	0.055	0.035	0.028	0.097
New Job	-0.020	0.009	-0.009	0.431	-0.004	0.511	0.001	0.076
Student	-0.087	0.011	-0.052	0.023	-0.015	0.044	-0.004	0.439

Notes: Wild p-values based on 1499 bootstrap replications. All regressions have the full set of control variables listed in Table 2.

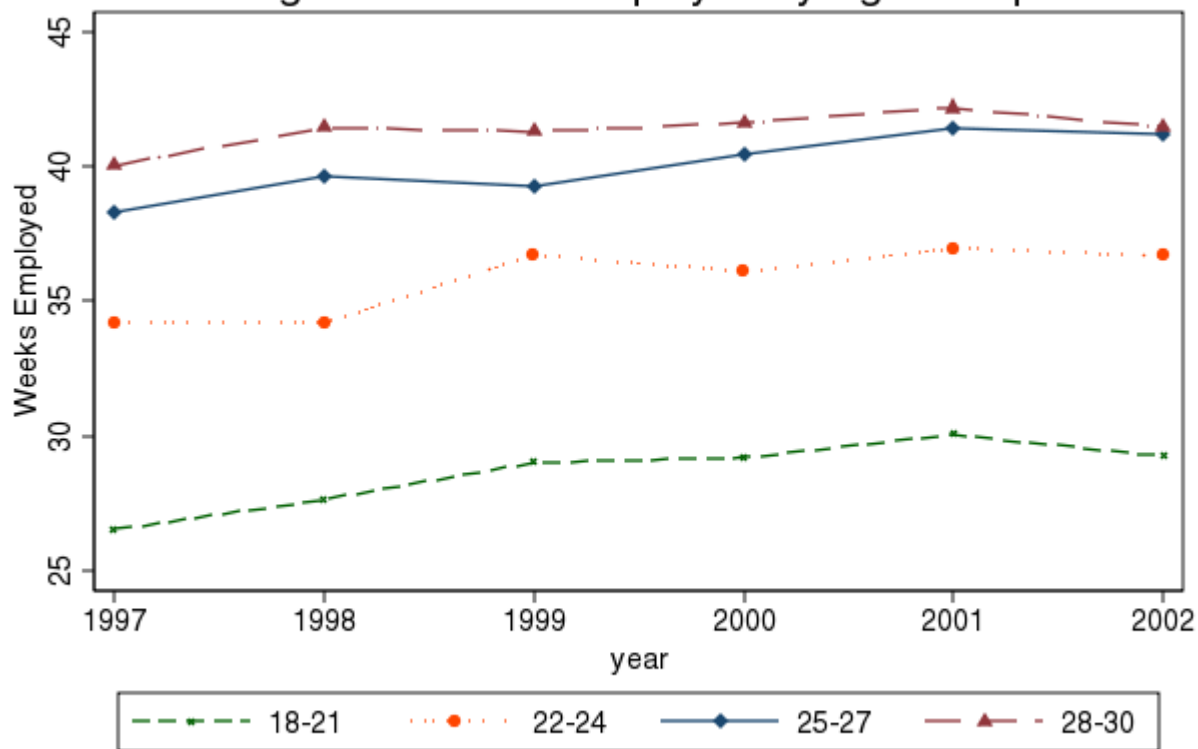
**Table 5: Falsification Test Using Data From 2002-2005**

	All			Female			Male		
	Coeff	Hetero p-value	Wild p-value	Coeff	Hetero p-value	Wild p-value	Coeff	Hetero p-value	Wild p-value
<b>SLID</b>									
Annual Weeks Employed	-0.090	0.945	0.979	1.186	0.520	0.388	-1.336	0.450	0.439
Annual Weeks Unemployed	0.526	0.423	0.329	0.895	0.264	0.335	0.111	0.914	0.855
Annual Wks Not Lbr Force	-0.437	0.713	0.687	-2.082	0.236	0.156	1.224	0.435	0.363
E Any Time in Year	-0.022	0.360	0.387	-0.003	0.922	0.951	-0.038	0.231	0.241
U Any Time in Year	0.021	0.126	0.051	0.015	0.409	0.547	0.026	0.205	0.167
N Any Time in Year	-0.025	0.386	0.585	-0.078	0.061	0.093	0.030	0.450	0.487
Total Annual Hours	9.816	0.851	0.759	34.133	0.634	0.383	-14.198	0.851	0.803
ln(Annual Income)	0.021	0.742	0.772	0.016	0.867	0.836	0.032	0.718	0.721
ln(Average Wage)	-0.007	0.787	0.693	0.002	0.955	0.957	-0.015	0.672	0.592
New Job	-0.035	0.281	0.299	-0.042	0.356	0.271	-0.029	0.525	0.583
Mostly Full-time work	-0.004	0.870	0.813	-0.030	0.469	0.152	0.022	0.525	0.431
Full-time Student in Year	0.008	0.777	0.757	0.038	0.321	0.073	-0.022	0.585	0.557
<b>LFS</b>									
Employed	0.003	0.634	0.805	-0.013	0.101	0.137	0.016	0.029	0.239
Unemployed	-0.009	0.010	0.088	0.001	0.748	0.789	-0.018	0.000	0.011
Not in Labor Force	0.006	0.198	0.368	0.011	0.111	0.083	0.002	0.765	0.843
Total Weekly Hours Worked	0.233	0.275	0.488	0.839	0.006	0.112	-0.337	0.256	0.507
ln(Weekly Income)	-0.007	0.458	0.684	0.012	0.373	0.588	-0.026	0.031	0.107
ln(Wage)	-0.016	0.003	0.208	-0.013	0.075	0.353	-0.019	0.009	0.131
New Job	0.004	0.233	0.560	-0.002	0.730	0.837	0.009	0.048	0.339
Student	0.015	0.003	0.105	0.003	0.672	0.756	0.026	0.000	0.023

Notes: Wild p-values based on 1499 bootstrap replications. All regressions have the full set of control variables listed in Table 2.



Figure 1: Weeks Employed by Age Group



Source: 1997-2002 SLID microfile data. Individuals aged 18-30.

