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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*

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Abstract

Targeted employment subsidy programs are commonly employed by governments. This study examines one such initiative that rebated unemployment insurance premiums to employers with net increases in insurable earnings for youth aged 18 to 24. In each of two datasets, statistically and economically significant impacts on employment are observed for the targeted age group relative to older age groups. However, neither dataset exhibits a concurrent change in aggregate unemployment; instead there is a reduction in those not in the labour force. Oddly, no program impacts are observed for females and all of the effects involve only males. Notably, evidence of displacement – substitution away from slightly older non-subsidized workers towards the younger subsidized group – is observed. Although modest, these spillovers suggest that the aggregate impact of the program is less than that observed for the targeted group.

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1 Introduction

In the wake of recessions, governments are interested in stimulating employment with general programs as well as those targeting particular locations, industrial sectors or demographic groups. Hiring credits for employers, especially temporary credits, are an important tool for this purpose when recessionary unemployment is thought to result primarily from deficient labour demand. The Organization for Economic Cooperation and Development (OECD 2010) provides a non-exhaustive international list of such initiatives following the recent Great Recession, and this class of policies has been subject to academic (e.g., Neumark 2013, Neumark and Grijalva 2013, Cahuc, Carcillo and Le Barbanchon 2014), policy (e.g., Bartik and Bishop 2009), and popular (e.g., Blinder 2013) attention. Testifying before the US Senate Committee on the Budget, Elmendorf (Congressional Budget Office, 2011) argued that programs which subsidize employers as a function of payroll growth have the greatest effects on employment per dollar expended among a range of policies they considered.

Employer hiring credits frequently take the form of tax, or social insurance premium, rebates. The US Hiring Incentives to Restore Employment (HIRE) Act of 2010 is one such effort. It exempted employers from paying their share of Social Security, that is Old-Age, Survivors, and Disability Insurance (OASDI) taxes for new hires who were unemployed or underemployed. Since employers and employees in 2012 each paid 6.2% of the employee's annual earnings below \$110,100, this amounted to a substantial subsidy for employers to hire unemployed or underemployed persons. Employers were also eligible for a \$1000 retention credit for each of those new workers retained for at least one year. Beyond this national strategy, Neumark and Grijalva (2013) study various US state-level hiring credits and discuss the implications of alternative program designs. Another targeted US federal employment subsidy, the Empowerment Zone program is studied by Busso, Gregory and Kline (2013). This program targeted on the basis of geographic "place" rather than unemployment status. Despite fears that this program would distort economic markets as a result of geographic displacement by firms and workers in response to the subsidy, they find positive benefits with only modest distortions.

France similarly initiated a temporary program in 2008 that provided social contribution rebates to small firms hiring low wage workers that is studied by Cahuc, Carcillo and Le Barbanchon (2014). They observe that the surprise introduction of the credit began to have an effect quite quickly. Importantly, many economists fear that versions of these programs that subsidize all (i.e., gross) new hires, as opposed to net new hires, incentivize distortionary churning as firms let existing workers go in favour of new subsidized workers. However, in France this is not observed. They attribute firms not laying off higher priced incumbents in favour of subsidized new hires to France's high level of existing churn given that most new hires are on temporary contracts. Further, despite the substantial windfall gain to employers, which Cahuc, Carcillo and Le Barbanchon estimate at 84% of relevant new hires, they find that the net costs of the program are about zero. Currently, Canada's federal government is pursuing a broadly similar and suitably titled Hiring Credit for Small Business that aims to encourage job creation using an employer side payroll tax reduction for net new employment insured under the employment insurance system (EI called unemployment insurance or UI prior to 1996).[1]

In the same vein, Canada pursued two initiatives involving EI following the recession of the 1990s, with the first targeting small firms and the second, which is the focus of this study, youth employment. In 1999 and 2000 this program – "Youth Hires" – rebated any increase in aggregate EI 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 scope for a short-term program to affect employment levels during a period of slack labour demand.[2] Neumark (2013) discusses the economics of these schemes at length, so we do not do so here. However, it is worth noting three aspects of the program under study. First, unlike the program in France studied by Cahuc, Carcillo and Le Barbanchon (2014) the Canadian program is a subsidy for net (not gross) increases in premiums paid. Second, a program that targets a particular identifiable group, in this case youth, may induce substitution towards the subsidized workers (i.e., displacement of close substitutes) and the program's aggregate impact may differ from that experienced by the targeted group. We look for evidence of such effects. Finally, although it was implemented late in the relevant part of the business cycle, Youth Hires is a post-recession program given the extended length of that recession, especially for youth, in Canada. Impacts from similar programs operated during different phases of the business cycle may be quite different. A related initiative is the 1997 reduction in Spanish payroll taxes and dismissal costs for permanent contract employees examined by Hernanz, Jimeno and Kugler (2003). Spain's reforms reduced dismissal costs, and the authors exploit differences in tax reductions for different age groups. They compare 20-29 year olds to those aged 30-39 and find significant increases in the probability of being employed amongst the young treated population.

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., Card, Chetty and Weber, 2007a,b). Of particular relevance to this study, much research addresses how workers and firms tailor their behavior to the parameters of the UI/EI system, which is an important phenomenon in Canada where EI was not experience rated (see, e.g., Green and Riddell 1997; Green and Sargent 1998; Kuhn and Sweetman 1998). Kuhn and Riddell (2010) find appreciable long run responses to UI's parameters in the US and Canada in their contrast of adjacent regions in both nations.

A related literature examines active labour market programs. Andersen and Svarer (2012) review a series of randomized experiments which occurred in Denmark. These largely took the form of increased job counseling and had different target groups, including one targeting youth unemployment. The authors conclude that many of the interventions pass a cost benefit analysis, as the interventions often increased the employment rates of the targeted groups. A large scale program aimed at reducing youth unemployment was introduced in the UK in 1998. This *New Deal for Young People* is studied by De Giorgi (2005) and

[1] See `http://www.craarc.gc.ca/tx/bsnss/tpcs/pyrll/hwpyrllwrks/stps/hrng/hcsb-2012-eng.html`, accessed November 2012.

[2] Owyang et al. (2013) presents evidence that government spending multipliers are much higher during periods of high unemployment than in periods of low unemployment.

others. The program was aimed at individuals 18 to 24 years of age who had been unemployed for six months or more. De Giorgi uses a regression discontinuity design, testing the impact of the program for those just below the age cutoff versus those just above the age cutoff. The intervention, which in this case was national in scope, is estimated to increase the likelihood of being employed 18 months later by 6-7%.

Crepon et al. (2012) argues that these types of studies do not consider general equilibrium effects. These authors study a set of randomized experiments in France, which aimed to reduce unemployment among educated youths, and show that there are significant displacement effects which are generally more significant for males than females. Similarly, Khan and Lehrer (2013) find evidence of displacement effects in a randomized experiment aimed at increasing the size of individuals' social networks. Both Andersen and Svarer (2012) and De Giorgi (2005) mention the possibility of the control groups being disadvantaged by the policy changes, but their estimation strategy does not separately identify displacement effects.

The long tradition of, sometimes post-recession, targeted subsidies and credits is evidenced by the US New Jobs Tax Credit (NJTC) studied by Perloff and Wachter (1979), Bishop and Haveman (1979), and Bishop (1981), and the Targeted Jobs Tax Credit examined by Hollenbeck and Willke (1991). More broadly, O'Leary, Decke and Wandner (2005) study bonuses for UI recipients. They observe that only targeted bonuses are cost effective in the context of the US UI system and illustrate the importance of the choice of the target population for cost-effectiveness. In contrast to these US examples, the Canadian approach did not require vouchers nor did it target individuals, other than by age, so there was no stigmatization. In fact, subsidized employees need not even have been aware of the program. Despite the prevalence of targeted programs, we are aware of relatively few studies providing evidence on *post-recession stimulus on the employer-side for net employment growth* as does Youth Hires.

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 attempt to determine if there are any impacts on the targeted age group, or any displacement effects on older age groups. Classic work on displacement effects is the exploration of the UI bonus experiments by Davidson and Woodbury (1993). Their study showed the importance of formal modelling in identifying general equilibrium effects, and found that displacement offset a modest, but non-trivial, proportion of the program's benefits. Identification, however, relied on specific economic theories and functional form assumptions. Dahlberg and Forslund (2005) examines displacement from wage subsidies and training, exploiting variation across municipalities in Sweden. They find substantial displacement effects from subsidies. Understanding the magnitude of any displacement effects is fundamental to the evaluation of labour market interventions.

There are well known problems for inference in our estimation context, given that the source of randomization in this analysis, the policy change, varies at the aggregate level. We explore the sensitivity of our results to alternative methods for dealing with inference with few clusters. Most of the analysis employs the wild cluster bootstrap of Cameron, Gelbach and Miller (2008). This is a markedly different inferential approach than that employed by

Cahuc, Carcillo and Le Barbanchon (2014) who address a national program. [3] Overall, we observe modest, although discernible, impacts of Youth Hires in that it increases employment for the targeted 18-24 age group, but we also find concurrent employment decreases for those 25 to 29, suggesting limited displacement is occurring.

The next section of the paper provides the institutional background. Section 3 describes the two independent data sets analyzed, defines two comparison groups that have different strengths and weaknesses, and presents descriptive statistics and an initial graphical analysis. Section 4 addresses the econometric methodology with a focus on issues of interpretation and inference that are relevant in this context, and Section 5 presents the empirical results where similar findings from both datasets add to our confidence in the analysis. The final section summarizes and interprets the findings.

2 Institutional Background Regarding EI and Youth Hires Program

Legally, the incidence of Canadian EI premiums is partitioned across employers and employees with employers paying 1.4 times the employee rate. This system is also notably different than the American program in that it operates nationally, and premiums are set annually by the federal government, and are not experience rated for either the employer or the employee.[4] 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 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% and 3.36% of insurable earnings with the maximum insurable earnings fixed at $39,000 in nominal terms.

The declining premium rate implies that a firm's aggregate EI insurable payroll for those in the relevant age group had to increase by 0.21% in 1999 before the firm was entitled to the first dollar of rebate. Although the intention of the program was to increase youth employment, employers had several margins on which they could adjust to increase premiums paid above the benchmark. Employers were eligible for the credit if they sufficiently increased any combination of wages, the number of young workers employed, or hours per year for existing young employees. However, firms received no rebate for wage increases to any individual worker whose annual earnings exceeded the maximum insurable limit.[5]

The salience of this program will obviously impact its effectiveness. If employers are

[3] There is some ambiguity in the econometrics literature on the appropriate approach to inference in such problems. See, for example, the discussion by Imbens and Wooldridge (2009), esp. sect. 6.5.3.

[4] 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. Also, in 2013 a quasi-experience rating element was added to the system whereby claimants' job search and new job acceptance criteria were made a function of previous claim history.

[5] For more information on this program see Canada Employment Insurance Commission (1999, 2000, 2001).

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, the government was interested in behavioral change since the goal of the program was to target unemployment among a specific age group. Awareness of this program was encouraged by the government through discussions of the program in the media and mailings to human resource departments in firms paying EI premiums. Additionally, 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 EI premiums associated with net job growth in small businesses.[6]

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 administratively burdensome (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, 1999 to 2002).[7]

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[8] 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.[9]

Given the nature of the Youth Hires program, we would not necessarily expect its introduction and termination to have opposing 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, job mobility rates are quite high for young workers. Therefore, while any impact may continue beyond the program's horizon 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 group in the empirical specification.

[6]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.

[7]By comparison, 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 \$5.9 billion across the two years in \$1999 US dollars.

[8]Part time was defined as below both 15 hours per week and an earnings threshold.

[9]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.

3 Data and Descriptive Statistics

We analyze individuals residing in Canadian provinces using the master files 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 labour market outcomes are detailed. In contrast, the LFS interviews roughly 54,000 households comprising about 100,000 individuals and capturing labour market information on the week that contains the 15^{th} of each month. For both datasets survey weights are used throughout.

The bulk of the analysis focuses on 1997-2000, that is, the two years before, and the two years of, the program's existence. The EI reforms make it difficult to use data before 1997, and hiring and training costs suggest the effects of the program are likely to continue beyond its termination. Two comparison groups, with different strengths, are employed. A comparison group close in age will likely compete in the same labour market, which makes it a good/similar comparison group but also makes it susceptible to displacement. A slightly older group is less likely to compete in the same labour markets, and thus is less liable to be displaced, but it is also probably somewhat less similar to the treated group. We use both comparison groups at different points in the analysis. The data for analysis are initially restricted to those aged 18-30, with the comparison group aged 25-30. Later, data on individuals 18-35 is used, with those aged 30-35 as a 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.

Any significant impact of Youth Hires could affect variables for individuals treated such as the likelihood of being employed, or hours or weeks worked. While government policy may be motivated by unemployed youth who are out of school, post-secondary or high school attendance may also be affected for this age group, so we also investigate that outcome. This is similar to analysis on educational decisions in response to changes in the minimum wage, e.g., Landon (1997), Neumark and Wascher (2004), and Campolieti, Fang and Gunderson (2003).

Summary Statistics

Table 1 contains mean values and sample sizes of dependent variables used in the regression analysis. 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 labour 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. 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 labour force' indicator is set to one if the person is out of the labour market at any point in the year. Total hours worked at all jobs in the year is next, followed by 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 dollars. 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.

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, while the hourly wage is a weighted average for all jobs worked. Both income and wages are converted to 1999 dollars prior to taking the natural log. 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 are provided to illustrate the time trend in relevant dependent variables in the years of, and surrounding, the Youth Hires program. For various age groups in the SLID, Figure 1 shows the trends in annual total weeks employed. 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. In contrast to the effects seen in the first year of the program, there is no obvious jump in the second year.

Weeks not in the labour force, also from the SLID, is presented in Figure 2 and a conceptually similar pattern is evident. 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 labour force in 1999 while those just excluded from (too old for) the program were slightly displaced. Although there may be some ongoing effect, no additional effect is apparent for the second year of the program.[10]

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

[10] 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.

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, 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.

4 Econometric Approach

We employ a framework that, in terms of the equations estimated, is similar to a linear difference-in-differences (DiD) specification. However, the results do not have the usual interpretation as the causal impact of the treatment on the treated. 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, 2011) given that the program potentially has both direct causal impacts on the targeted age groups, and indirect causal impacts on slightly older workers. 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 a DiD specification with a policy change at the aggregate level can be problematic as demonstrated by Bertrand, Duflo and Mullainathan (2004). The cluster-robust variance estimator yields unreliable inference when there is 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) argue that wild cluster bootstrap-t technique works well even when the number of clusters is small; we employ this approach.[11] 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 p-values using the wild cluster bootstrap-t technique with the null hypothesis imposed.[12] Note that this approach bypasses the generation of standard errors, which Angrist and Pischke (2008) argue some economists like to observe.

The first specification we estimate employs data from 1997-2000 and regards those aged 18-24 as the treated group, and those aged 25-30 as the comparison group, as is specified in

[11] This procedure is refined for very few clusters in Webb (2013), and argued for in the case of unbalanced clusters in MacKinnon and Webb (2014).

[12] We thank Cameron, Gelbach and Miller for making their code available.

equation (1).

$$
\begin{aligned}
Y_{it} = \beta_0 + \beta_{YH} YH_{it} + \beta_{99}\text{Only1999}_{it} + \beta_A \text{Age}_{it} \\
+ \beta_B \text{YearBorn}_i + \beta_Y \text{Year}_t + [\beta_c \text{Controls}_{it}] + \epsilon_{it}
\end{aligned} \tag{1}
$$

In this equation Y_{it} represents a labour market variable of interest; YH is the Youth Hires indicator which is set to one if individual i is of an age targeted 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 indicators for province of residence and an indicator for urban residence, while the SLID regressions additionally control for race and immigrant status. The monthly nature of LFS, allows for the inclusion of a full set of months indicators. The βs are vectors of coefficients to be estimated.

In all cases, the equations are estimated using ordinary least squares (OLS). Some specifications are linear probability models. Standard errors are clustered on the individual's birth year since we view the program as having differing impacts across cohorts. This approach allows ϵ_{it} to be arbitrarily correlated within clusters, but assumes zero correlation 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 upwards of six years.

The coefficient β_{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 on those in the comparison group in the years when the program is operating. We are agnostic as to the expected sign of β_{99} since it will hinge on the impact of the program in 1999 and the degree of labour market attachment in the subsequent year. We do not report the coefficients for β_{99} in the text, though in general, the coefficients are of the same sign, smaller in magnitude and of lesser statistical significance than the coefficients for β_{YH}.

A subsequent specification is estimated using data on individuals aged 18-35. It is not shown since it is 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 comparison group. Plausibly, this comparison group is not (or is minimally) affected by the Youth Hires program, so treatment effects can be estimated separately for the targeted and potentially displaced groups. 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, et cetera with age. The estimates for this group are shown using only the 1998-1999 data, to capture the impact of the policy in its first year. The pattern of results was generally the same when estimated using the 1997-2000 data.

An attempt was made to conduct a three period analysis of the program, with the aim of determining 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. Individuals treated in 1999 and 2000 would be 20-26 years old in 2002, but that age range would consist of 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 using data from 2002 to 2005.[13] It would be preferable to conduct a falsification exercise using a period prior to the program, unfortunately, significant EI reforms in 1996-1997 render this infeasible.

5 Regression Analysis

Table 2 compares various approaches to inference for equation (1) using three key dependent variables – all measures of employment. The first two regressions use SLID data, and the third uses LFS data. 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, a large number of bootstrap replications are employed to increase the precision of the estimated p-values.

Comparison Group Aged 25-30

Coefficients are presented in the first line of Table 2 and, in a key result, all show sensible modest increases among the targeted group, relative to the slightly older one, associated with the program. Weeks of employment increased by approximately 2-2.5 weeks a year, and the employment rate increased by 3.5% to 4% as measured in the SLID or just over 1% as measured in the LFS. The heteroscedasticity robust p-values in the SLID are larger than those observed in any other test, but when individual fixed-effects are employed (with the standard errors clustered 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 modest difference. Two implementations of the wild cluster bootstrap-t are also undertaken – the first without, and the second with imposing the null hypothesis. 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 excessive differences in inference across the last three approaches, which are arguably superior to the others. We observe

[13] This time period leaves a two-year gap after the end of the program in case there are any "knock on" effects, without extending too far from the policy change given the possibility of other age-specific changes derived from the education system. See DiNardo and Lee (2011) regarding the benefits of falsification tests.

similar patterns for the other dependent variables although to save space we do not present these results.

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. The upper panel presents results from the SLID, while results from the LFS are presented in the lower panel. 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 labour 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 labour economists that the unemployment rate may increase when demand increases following a period of high unemployment since discouraged workers re-enter the labour market. This effect is common in the early part of the expansionary phase of a business cycle.

The dependent variables in the lower half of each panel represent ancillary features of the labour market that may be affected by the policy change, but we are agnostic regarding the expected sign of the coefficients 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, or could decrease if additional part-time youth are added to the labour force. Inline with impacts from changes to minimum-wage legislation, the additional opportunities for employment could potentially draw youth out of school so that the percentage who are full-time students might decline. These dependent variables are included to improve our understanding of the program's impact.

The equations estimated in this table are also estimated separately by gender, and this highlights a very interesting finding. Essentially none of the coefficients, in either dataset, is statistically significant for females. The entire policy response to the Youth Hires program appears to be concentrated among males. Or, alternatively, the response is more muted for females and is statistically insignificant given the limited precision in estimating the coefficients that is 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.[14] Moreover, both 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

[14]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, and are less precise.

employment rate of the targeted group compared to those slightly older, but the aggregate effect was to draw youth into the labour force.

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 to 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 the coefficient for the youngest group being statistically significant. In contrast, the coefficients for the two age groups just outside the age cutoff for the EI premium rebate have negative coefficients, with the coefficient 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. Clearly, understanding the spillovers from this program is important for understanding its aggregate impact. 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. Spillovers appear to be modest. Although the coefficients change slightly, the remainder of the coefficients on labour force status variables largely support an interpretation suggesting that the policy change increased employment among the targeted age groups. 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.

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. To compare our findings with those of Crepon et al. (2012) that the displacement effects are more significant for men than for women, we estimated a version of the model presented in Table 4 separately for each gender. The estimates have been omitted in the interest of space but they also find that the displacement effects are significant for men, but not for women. This is consistent with the results in Table 3 where essentially none of the impacts is significant for women.

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 (or 11%) 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 given the level of the test.[15] Importantly, all of the employment related coefficients have point estimates very close to zero and none of them is statistically significant. Also, 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 impacts. Overall, there does not appear to be evidence to undermine the conclusions in this analysis.

6 Discussion and Conclusion

We examine the effectiveness of a Canadian stimulus program designed to temporarily combat high youth unemployment. The Youth Hires program subsidized employers to hire youth between the ages of 18 and 24 by rebating EI premiums for net new insured employment. Overall, we believe the evidence supports the conclusion that this program served to increase employment among the subsidized population by about one or two weeks per year on average, or one or two percentage points, relative to older individuals. Interestingly, it appears that the effect of the program was predominantly, if not entirely, experienced by males. Female employment appears 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 around 3.5% of earnings below $39,000 per worker/year, so a substantial percentage of the total subsidy payment can be thought of as a windfall gain for employers who were expanding.

The Youth Hires program also appears to have had an impact, albeit a modest one, in reducing the labour market outcomes of those slightly too old to be eligible for the EI premium rebate. In some ways our results are similar to those found by Busso, Gregory and Kline (2013). There is likely some distortion resulting from the policy, but not enough to overturn the intended impact of the program. This substitution/displacement effect points to the trade-offs ubiquitous in social policy development, especially when programs seek to assist targeted groups. 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. Of course, an analysis such as this cannot answer the broader general equilibrium question about the number of jobs produced in, or the benefits accruing to, the economy as a whole as a result of the program.

[15] 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.

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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**
SLID								
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	14,643	968	12,970	1508	11,848	1546	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
LFS								
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 Labour 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 as Comparison Group

	All		**Female**		**Male**	
		Wild		**Wild**		**Wild**
	Coeff	**p-value**	**Coeff**	**p-value**	**Coeff**	**p-value**
SLID						
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
LFS						
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 Labour 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**
SLID								
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
LFS								
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 Labour 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

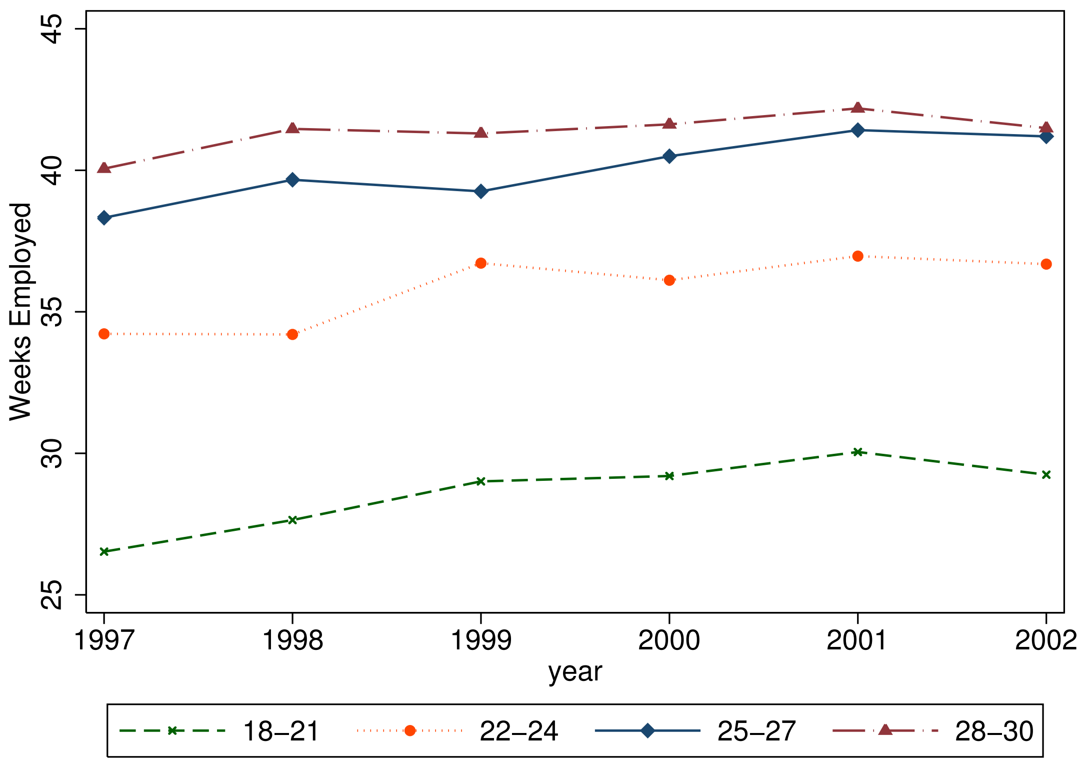
Notes: Wild p-values based on 1499 bootstrap replications. All regressions have the full set of control variables listed in Table 2.

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**
SLID									
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
LFS									
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 Labour 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

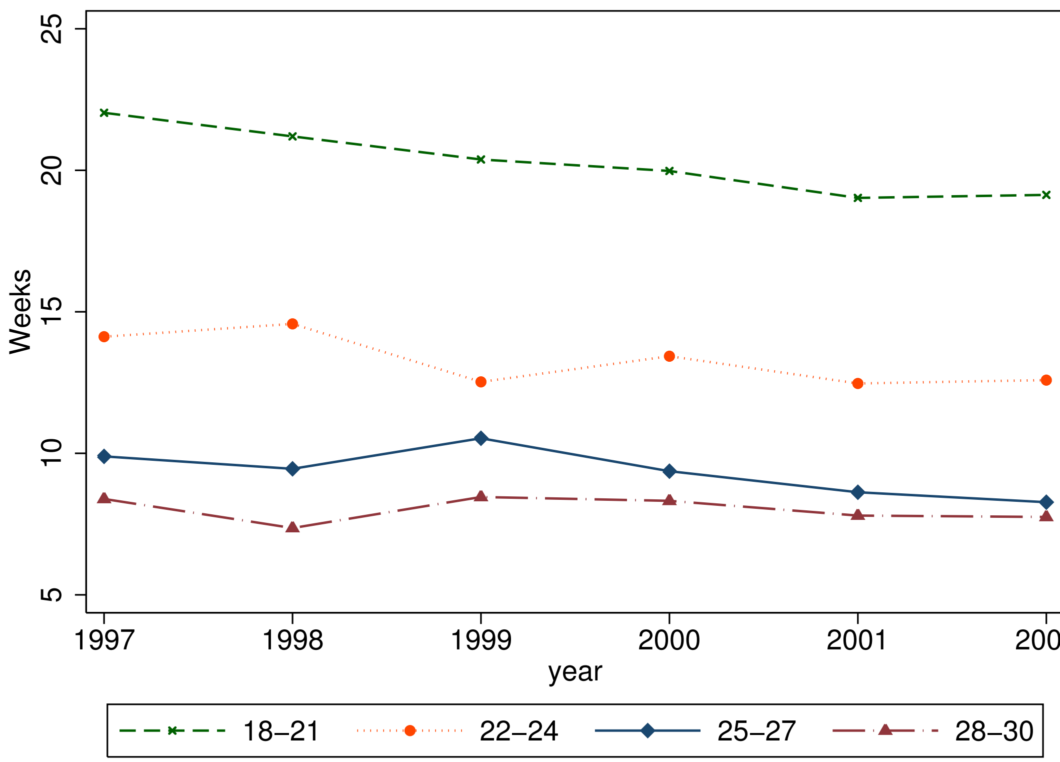
Notes: Wild p-values based on 1499 bootstrap replications. All regressions have the full set of control variables listed in Table 2.

Figure 1: Weeks Employed by Age Group



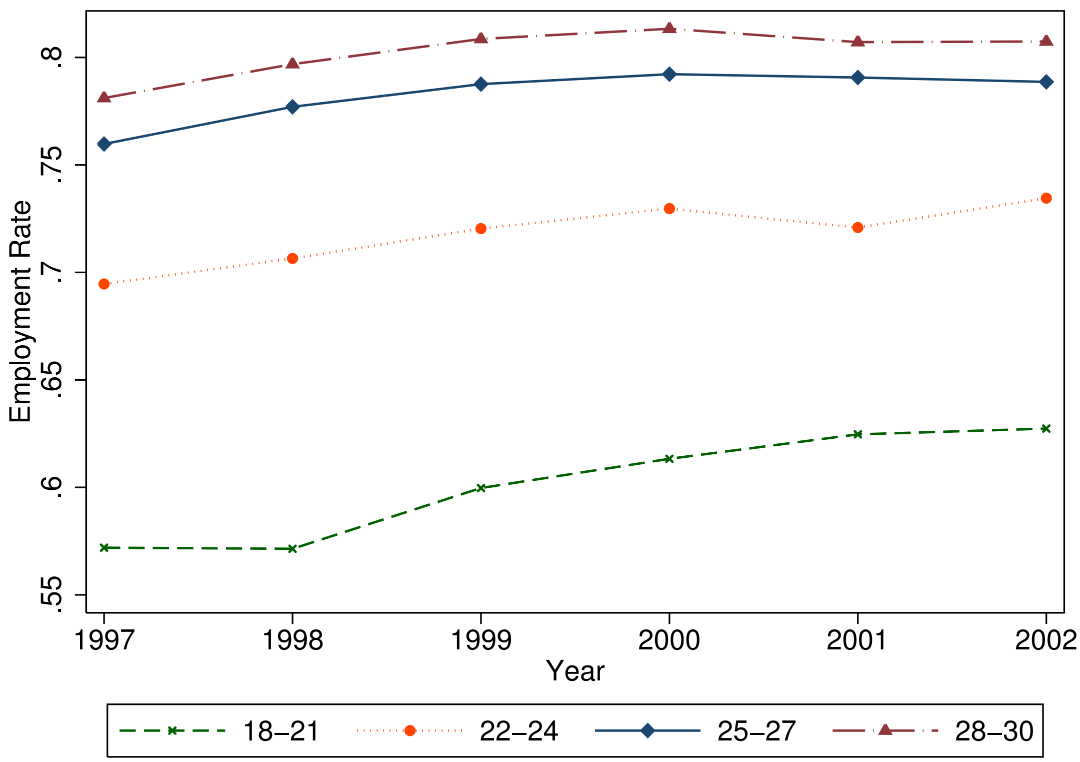
Source: 1997-2002 SLID microfile data. Individuals aged 18-30.

Figure 2: Weeks Not in the Labour Force by Age Group



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

Figure 3: Employment Rate by Age Group



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