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# Education, earnings, and the 'Canadian G.I. Bill'

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*Abstract.* Canadian Second World War veterans benefited from an extensive educational program similar to the U.S. G.I. Bill. Because of differences in military enlistment rates, however, a much lower fraction of Quebec men were eligible for these benefits than men from other provinces. Building on this fact, we analyse inter-cohort patterns of education and earnings for English-speaking men from Ontario, using French-speaking men from Quebec as a control group. We find that the instrumental variables estimates of the return to schooling are typically as big or bigger than the corresponding OLS estimates. JEL Classification: J24, I21

*Education, revenus, et le 'G.I. Bill canadien.'* Les anciens combattants canadiens de la deuxième guerre mondiale ont bénéficié d'un programme d'aide aux études similaire au G.I. Bill américain. Relativement peu de Québécois ont cependant pu bénéficier de ce programme en raison de la faible proportion de ces derniers qui ont fait leur service militaire durant cette période. Sur la base de cette observation, nous analysons les différences intergénérationelles dans la scolarisation et les revenus des Ontariens anglophones en se servant des Québécois francophones comme groupe témoin. Nos résultats indiquent que les estimés du taux de rendements de l'éducation obtenus à l'aide de la méthode des variables instrumentales sont comparables sinon plus élevés que ceux obtenus par moindres carrés ordinaires.

## 1. Introduction

Despite the nearly universal finding that better-educated workers earn higher wages,<sup>1</sup> the interpretation of this correlation remains controversial. At issue is a very basic

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1 See Cohn and Addison (1997) and Psacharopoulos (1985, 1994) for surveys of U.S. and international evidence and Katz and Autor (1999) for a survey of recent research on schooling and earnings inequality.

question: Are the higher earnings received by better-educated workers *caused* by their extra schooling, or do they reflect inherent differences in abilities among those who choose to acquire more schooling? This question has become especially relevant in Canada in recent years, with the dramatic rise in college and university enrolment rates of young men and women (Statistics Canada 1997, 14). Will these massive investments in higher education pay off in the future by raising the earnings of the post-baby-boom cohorts? Or is the true return to education substantially overstated by the observed earnings differences between those with more or less schooling?

In a series of recent studies attempts have been made to estimate the true return to education by measuring the earnings differentials associated with differences in education caused by institutional features of the education system. Angrist and Krueger (1991) argued that compulsory schooling laws create a disparity in average schooling between people born earlier and later in the year that can be used to identify the causal effect of education. Other studies have proposed the use of college proximity (Kane and Rouse 1993; Card 1995b; Conneely and Uusitalo 1997) and birth cohort (Harmon and Walker 1995; Ichino and Ebmer-Winter 1998) as exogenous sources of variation in schooling outcomes. A surprising finding in this literature is that the estimated returns to schooling are typically as big (or even bigger) than conventional estimates based on the cross-sectional association between earnings and schooling (see Card 1995a, 1999 for reviews of this literature). Taken at face value, this pattern suggests that ability differences between those who obtain different levels of education are relatively small or are offset by other sources of bias. Nevertheless, the assumptions underlying these studies are controversial (e.g., Bound and Jaeger 1996). Moreover, most of them rely on the behaviour of a narrow subgroup – typically individuals with low levels of schooling – to identify the causal effect of education. To the extent that returns to education vary by the level of schooling or by family background or ability, estimates in the recent literature may provide limited guidance for current policy questions.<sup>2</sup>

These considerations underscore the value of studying alternative ‘quasi’ or ‘natural experiments’ that affected the education choices of a larger group of individuals. Perhaps the biggest and best known of such ‘experiments’ is the American G.I. Bill (the Serviceman’s Readjustment Act of 1944). This law provided financial aid and institutional support for servicemen (and women) returning from World War II (WW II) to attend college. The G.I. Bill is widely credited with opening up college to the middle class and stimulating the rise in educational attainment between pre-war and post-war cohorts (see Olson 1974 for a history of the bill). While the G.I. Bill seems like an ideal ‘natural experiment’ in terms of its comprehensive nature and relevance for policies targeted to post-secondary education, it suffers from the absence of a credible control group. Over 70 per cent of all American men born between 1920 and 1928 were veterans of World War II and were eligible for benefits

2 The return to education estimated in Angrist and Krueger’s (1991) study, for example, is based on schooling and earnings data for children who were likely to drop out of high school.

under the G.I. Bill.<sup>3</sup> With such a high rate of participation it is not surprising that recent studies find important selection biases between veterans and non-veterans (Angrist and Krueger 1994). Moreover, rates of military service remained high after World War II, and later veterans were also eligible for G.I. Bill benefits. Thus, the education and earnings outcomes of *later* cohorts cannot be used to form simple inferences about the effect of the G.I. Bill on WW-II-eligible cohorts.

Canada, like the United States, established a comprehensive program of benefits for returning WW II veterans to further their education and ease their transition to civilian life. In contrast to the effect in the United States, however, there were large differences in the impact of the program across provinces, arising from differences in rates of military participation and the absence of a comprehensive national draft. In particular, fewer than 20 per cent of French-speaking men in Quebec who were in their late teens or early twenties during the war years served in the military and were eligible for benefits under the Veteran's Rehabilitation Act (VRA) – the Canadian 'G.I. Bill.' Moreover, the Quebec university system made virtually no accommodation for those returning veterans who were potentially eligible for benefits. In Ontario, by comparison, over one-half of young men served in the military, and the universities adopted an 'open door' policy that included remedial programs for under-prepared veterans. Thus, in Ontario, the VRA had the same dramatic impacts as the American G.I. Bill, whereas in Quebec the VRA had virtually no effect on university attendance rates.

In this paper we use these unique experiences to identify the effect of a large-scale educational program on education and earnings outcomes. Our primary strategy is to use 1971 Census data to compare inter-cohort differences in the education and earnings of English-speaking men from Ontario relative to French-speaking men from Quebec. We focus on the contrast between Ontario and Quebec because these are two largest provinces in Canada – comprising over 60 per cent of Canada's population – and because the two provinces are economically integrated and have comparable industrial structures. Since rates of military service in Canada before *and* after World War II were negligible, older and younger cohorts of men provide natural 'control groups' for the cohort that was most likely to serve in the war. What is important, we compare the education and earnings for all men in a given province and cohort – not just the veterans, who are presumably a non-random subset of the population.

One drawback of this comparison is that it confounds any direct impact of WW II service on earnings (i.e., any pure veteran effect) with the induced impact arising through veteran's higher education. To the extent that military service in the war had a negative impact on veterans' earnings (Angrist and Krueger 1994), our

3 Since there were so few female veterans in World War II, it may be possible to use females in the same age cohorts as a control group for men. A preliminary examination of data from the 1970 Census for men and women born from 1900 to 1945 shows some correlation between the fraction of male veterans in a cohort and the relative rate of university education among men and women in the same birth year.

TABLE 1  
Number of men in the armed forces during WW II in Canada and the United States

	Canada		United States	
	Number (millions)	Percentage of male population age 18–45 in 1945	Number (millions)	Percentage of male population age 18–45 in 1945
Total population in 1941 (1940 for the U.S.)	11.507	–	131.67	–
Population of men age 18–45 in 1945	2.658	100.0	30.49	100.0
Total veterans	1.013	38.1	16.13	52.9
Volunteers	0.913	34.4	6.11	20.0
Conscripts	0.099	3.7	10.02	32.9
Casualties	0.042	1.6	0.29	1.0

SOURCES: Canadian population: 1941 Census of Canada. U.S. population: 1940 Census of the United States. Canadian veterans: Stacey (1970). U.S. veterans: 1989 *Statistical Abstract of the United States*

comparisons will *understate* the causal effect of VRA-induced education on earnings. To examine this issue further, we use data from the 1973 Job Mobility Survey (JMS) on the veteran status and parental backgrounds of men in the WW II cohorts. Because of the limited sample sizes in this survey, we do not try to compare education and earnings outcomes by province and cohort. Rather, we propose an alternative estimation strategy based on the hypothesis that VRA benefits had a bigger impact on the education outcomes of men from relatively disadvantaged family backgrounds.<sup>4</sup> We use the interaction of family background and veteran status as an instrumental variable for education, allowing both variables to have independent effects on earnings outcomes.

## 2. Military service in Canada during the Second World War

Canada entered World War II in September 1939, a few days after Britain and France declared war against Germany. As shown in the third row of table 1, about 1 million men served in the armed forces between 1939 and 1945, or about 38 per cent of all 18–45-year-old Canadian men. This rate is below the fraction of American men from the same age range who participated in military service (53 per cent), although it is interesting that the casualty rate was higher for Canadians, reflecting a high rate of active service among Canadian soldiers.

4 Like its U.S. counterpart, the VRA relaxed university admission standards for veterans and provided short-term courses for veterans to prepare for university. These features presumably had larger effects on the university enrolment decisions of young men from disadvantaged backgrounds.

TABLE 2  
Veteran intake rates and other characteristics by province

	Percentages of male population <sup>a</sup> age 18–45 in 1945		Percentages of overall population <sup>b</sup>			
	Intake	Conscripts	French-speaking	Immig. from U.K.	Immig. not U.K.	Voted yes in plebiscite <sup>c</sup>
P.E.I.	43.8	1.8	11.2	0.7	1.6	82.3
Nova Scotia	44.1	1.9	7.2	2.3	2.6	77.8
New Brunswick	43.2	3.5	34.5	2.0	2.3	69.1
Quebec	22.7	5.7	81.6	2.6	4.0	27.1
Ontario	45.8	2.7	7.6	11.5	7.5	83.0
Manitoba	44.6	3.5	7.1	11.2	15.1	79.0
Saskatchewan	38.2	3.8	4.9	8.1	18.5	71.1
Alberta	41.0	3.2	4.0	10.7	21.6	70.4
British Columbia	49.0	3.0	1.4	21.4	15.0	79.1
Total for Canada	38.1	3.7	29.2	8.3	9.2	–

a SOURCE: Stacey (1970) and 1941 Census of Canada

b SOURCE: 1941 Census of Canada and Comeau (1982)

c The question asked in the 1942 Plebiscite was whether the federal government should be relieved from its previous commitment not to use conscription for military service overseas.

The table also shows, however, that compulsory military service – referred to as conscription in Canada and the draft in the United States – played a very different role in the two countries. While most American servicemen were drafted, Canada relied almost exclusively on volunteers to fight the war, with conscripts representing only 10 per cent of enlistments. This difference is crucial to our analysis: while compulsory service tends to equalize the fractions of men from different regions and backgrounds who serve in the armed forces, large differences can emerge in an army of volunteers.

Table 2 shows, indeed, that there were very significant interprovincial differences in the fraction of men who served in WW II. Men from Quebec were only one-half as likely to serve as men from Ontario or other Canadian provinces. Although conscription rates were higher in Quebec than in other provinces, conscription reduced the gap in overall military service rates between Ontario and Quebec only by about 3 percentage points. The differences in enlistment rates shown in table 2 are even bigger when we consider French-speakers in Quebec. Assuming that non-French-speakers in Quebec had about the same military service rate as men from Ontario, we estimate that roughly 17 per cent of French-speaking men from Quebec served in WW II, versus about 46 per cent of men from Ontario.

The results in tables 1 and 2 are derived from aggregate Census and military records.<sup>5</sup> Even stronger conclusions emerge from a comparison of individual veteran service rates using the 1973 Job Mobility Survey (JMS). According to JMS

5 It is interesting that the 1971 Census did not ask about military service.

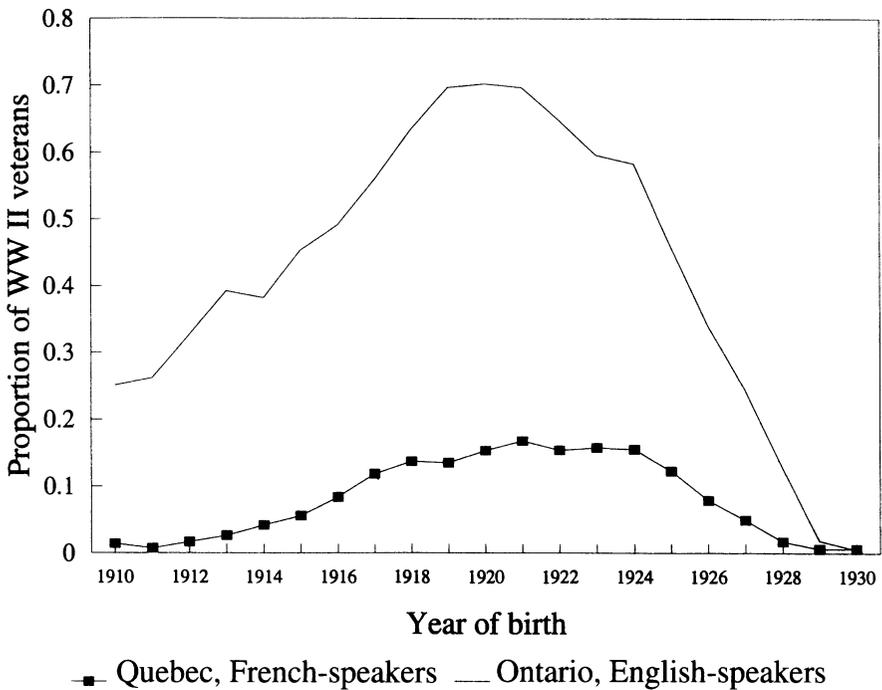


FIGURE 1 Proportion of men who served in WW II by year of birth (five-year moving average)

data, 11 per cent of Quebec French-speakers age 18–45 in 1945 served in WW II, versus 43 per cent of Quebec English-speakers and 46 per cent of men from Ontario.<sup>6</sup> Figure 1 illustrates the dramatic differences in military service rates by age cohort for French-speakers from Quebec and English-speakers from Ontario.<sup>7</sup> Over 60 per cent of English-speaking Ontario men born in the early 1920s served in the war, compared with a rate of about 15 per cent for French-speaking Quebec men. This contrast suggests that French-speaking men from Quebec might be usable as a ‘control group’ for evaluating the impact of the Canadian G.I. Bill on English-speakers from Ontario. Before we conduct this comparison, however, it would be useful to document some of the factors that lie behind the remarkable interprovincial patterns in table 2 and figure 1.

Perhaps the most obvious factor is that, with the exception of some French-speaking infantry regiments, the Canadian armed forces were unilingual English. Since three-quarters of the French-speaking population of Quebec did not speak English, the infantry was the only option for most French-speaking volunteers. In

6 These figures exclude immigrant men, to ensure that the group was eligible for service in the Canadian armed forces during WW II.

7 In view of the small sample sizes in the JMS we elected to smooth the veteran rates by using five-year moving averages.

the light of the high casualty rate among Canadian infantrymen during World War I (WW I), the absence of other alternatives may have discouraged many French-speaking men from volunteering.<sup>8</sup>

A second and potentially more important explanation is the strength of family ties to Europe. Although many English-speaking Canadians had close links to Britain, most French Canadians had very weak ties to France. As shown in table 2, for example, over 10 per cent of the residents of Ontario and 20 per cent of the residents of British Columbia were immigrants from the United Kingdom. A much higher fraction were the children or grandchildren of U.K. immigrants. By contrast, French immigration to Quebec had stopped in the mid-eighteenth century, and political and economic ties to France were very limited. A comparison of the military intake rates by province confirms that intake rates were higher in provinces with more U.K. immigrants and lower in provinces with more non-U.K. immigrants.<sup>9</sup> Just as second- and third-generation English Canadians were less likely to volunteer than recent British immigrants, French Canadians – who were many generations removed from the 'old country' – may have felt less compelled to join a European war.<sup>10</sup>

While these factors explain some of the regional differences in the fraction of men who *volunteered* for military service, an equally important factor was the absence of a comprehensive conscription program for military service. Throughout the war a relatively small number of men were drafted, with the stipulation that they could perform their military service in Canada. The reluctance of the Liberal government to adopt a general conscription policy reflected the bitter lesson of World War I, when a conscription law passed by a Conservative government led to civil uprisings in Quebec and a massive voter reaction against the Conservative party.<sup>11</sup>

A 1942 plebiscite on the use of conscripts for overseas military service revealed the depth of feeling against conscription in Quebec and confirmed the political risks for any party supporting it. As shown in the right-hand column of table 2, 70–80 per cent of voters in other provinces favoured relieving the government from its promise not to send conscripts overseas. In Quebec, on the other hand, fewer than 30 per cent of voters supported the plebiscite. The liberal leader, Mackenzie King, was

8 Angrist (1991) shows that many men who were 'at risk' to be drafted into the army during the Vietnam War volunteered to serve in other branches of the armed forces to avoid the infantry. Although the context of World War II in Canada was quite different, the reputation of the Canadian infantry was problematic in the aftermath of WW I.

9 For example, British Columbia had the highest fraction of U.K. immigrants and the highest intake rates, whereas Alberta and Saskatchewan had the highest fractions of non-U.K. immigrants and the lowest intake rates (apart from Quebec).

10 We used the 1973 JMS to examine veteran rates by ethnicity. We find that men born in the United Kingdom are more likely to have served in WW II than Canadian-born men whose parents were British immigrants, who themselves are more likely to have served than other Canadian-born English-speakers.

11 In the 1917 election the Conservatives lost all support in the province of Quebec, which at the time represented one-third of the seats in Parliament. Thanks in large part to their base of support in Quebec, the Liberal party remained in power in Canada for all but twelve of the following sixty years.

determined not to alienate Quebec voters and steadily resisted the use of conscription throughout the war.<sup>12</sup>

### 3. Education systems in Quebec and Ontario and the impact of the VRA

#### 3.1. *The Veterans Rehabilitation Act*

The Veteran's Rehabilitation Act (VRA), signed into law in 1944, created a series of programs to ease the return to civilian life for honorably discharged veterans of WW II. One important group of veterans who were ineligible for these programs were conscripts.<sup>13</sup> Since a disproportionate fraction of conscripts were from Quebec, their ineligibility reinforced the differential impact of the VRA on English-speaking men from Ontario relative to French-speaking men from Quebec. Nevertheless, given the small fraction of conscripts in the Canadian armed forces during WW II, we essentially ignore them in the remainder of our analysis.<sup>14</sup>

Under the VRA qualified veterans had the option of choosing between vocational training programs and university programs. In either case, veterans received tuition expenses and a living allowance of \$60 per month (equivalent to about \$500 per month in current dollars), with supplemental payments for those with dependants. Vocational training was targeted at older and less academically oriented veterans and typically lasted under a year. VRA funding for university programs was available for up to four years for 'veterans whose university careers were interrupted by enlistment, or who were not able to commence such courses; also those who need refresher courses to assist in re-establishment' (Department of Veterans' Affairs 1947). These criteria suggest that younger veterans probably benefited more from VRA university programs than those beyond traditional university age at the time of their enlistment. In addition to subsidizing the tuition and living expenses of students, the Department of Veterans' Affairs provided an annual grant of \$150 per veteran to universities to aid in handling the large influx of new students.<sup>15</sup>

A total of 50,000 veterans (about 5 per cent of the population who served in WW II) received university allowances under the VRA, while a somewhat larger number (70,000) received vocational training allowances (Department of Veterans' Affairs, 1950). Since university benefits were available for a much longer period than vocational subsidies, they presumably had a bigger impact on the educational attainment

12 King only agreed to send 15,000 conscripts to Europe at the end of 1944 as reinforcements, following unexpectedly large casualties in Normandy and Belgium. He managed to retain political support in Quebec by arguing that this was an exceptional measure. See Stacey (1970, part VII).

13 The 15,000 conscripts who were sent as reinforcements to Europe at the end of 1944 were eligible for VRA benefits.

14 We had initially thought that similar fractions of men from Quebec and Ontario had served in the armed forces but that the majority of soldiers from Quebec were conscripts and were therefore ineligible for VRA benefits. As table 2 and figure 1 show, this impression, which was also shared by two veterans, was inaccurate.

15 These grants represented over 10 per cent of the operating budget of Canadian universities in 1946 (Dominion Bureau of Statistics 1946). Universities later asked the federal government to provide them with further funding as the number of veterans enrolled (and the associated federal grants) started to fall while regular enrolments were swelling. The VRA program opened the way for federal involvement in a sector that was traditionally the domain of the provinces (Cameron 1991).

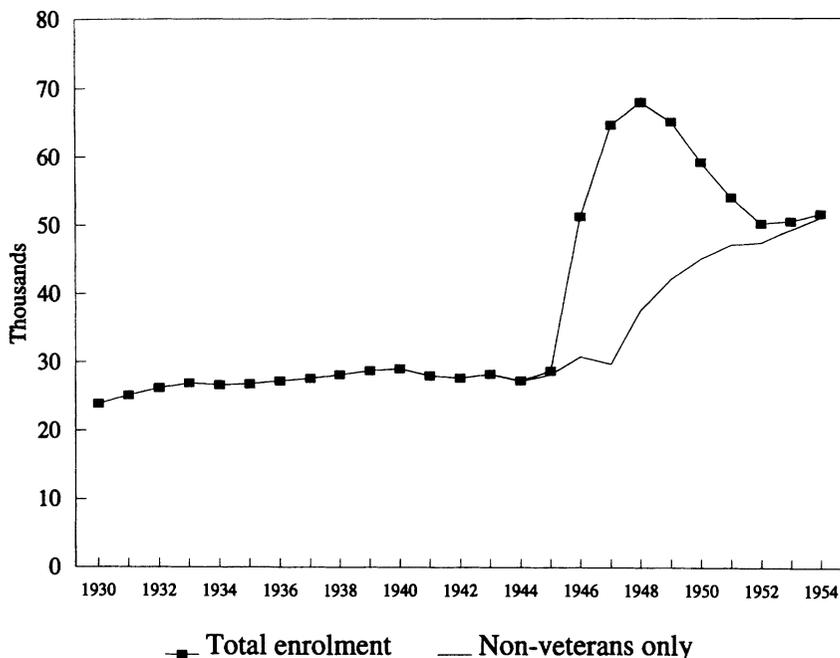


FIGURE 2 Full-time male university enrolment

of veterans. Figure 2 shows that the influx of veterans into Canadian universities substantially affected enrolments in these institutions. Total enrolment, which had remained relatively stable at about 35,000 students per year between 1930 and 1945, more than doubled in 1947.

Two other interesting patterns emerge from figure 2. First, there was only a minor drop in university enrolment during the war years. The decline was much smaller than the ensuing increase in enrolments between 1945 and 1947, suggesting that most veterans who entered university after the war would not have enrolled in the absence of the VRA. There was also a sharp rise in regular (i.e., non-veteran) enrolment starting in 1947. It appears that the opening up of Canadian universities to the returning WW II veterans had a 'spillover effect' that raised the demand for university education among groups who previously would not have attended university.

### 3.2. The education systems in Quebec and Ontario in the early 1940s

In the 1940s Ontario had a comprehensive system of public elementary and secondary schools, with compulsory schooling to age 16.<sup>16</sup> Four years of high school

16 The Ontario education system includes publicly funded Catholic schools. The basic structure of the Ontario education system has remained relatively unchanged over the past century.

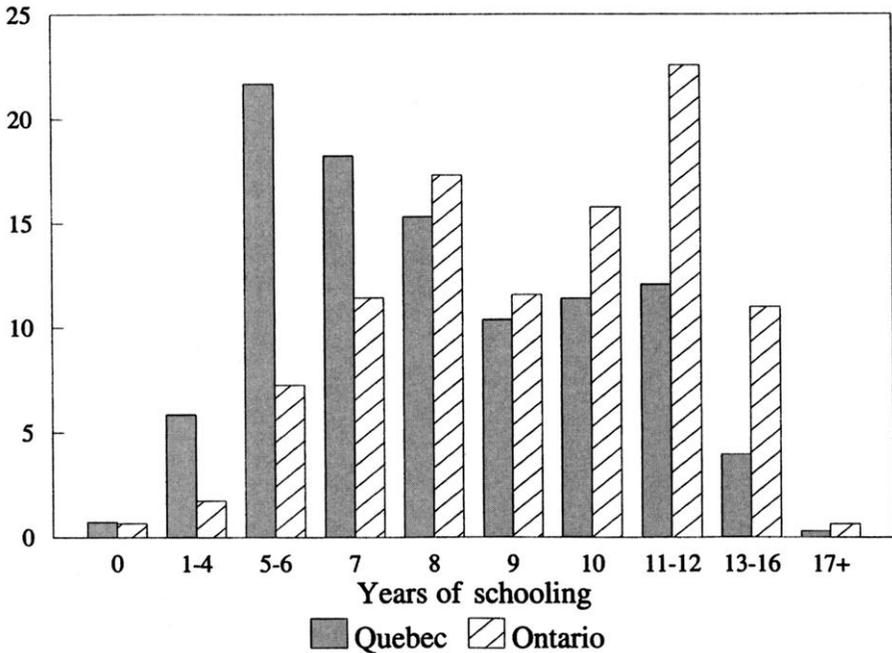


FIGURE 3 Frequency distribution of years of schooling, men and women age 20–24, 1941 Census

were sufficient to obtain a high school diploma, although students who wanted to attend university were required to attend a fifth year (grade 13). All Ontario universities were publicly funded: a Bachelor's degree was normally awarded after three or four years of attendance. Thanks to its comprehensive public education system, Ontario had the most highly educated population in Canada at the outbreak of WW II. Figure 3 shows the frequency distribution of completed schooling for Ontarians age 20–24 in the 1941 Census. About one-third of young adults in the province had eleven or more years of education. This level of schooling compares favourably to most other countries in the world at the time, and is only slightly below the level in the United States (see, e.g., Goldin and Katz 1997).

By contrast, the education system for French-speakers in Quebec in the early 1940s was quite backward.<sup>17</sup> There was *no* compulsory schooling age until 1943, and many young people had very low levels of education. Although French-speaking elementary schools were publicly funded and open to all, there was no comprehensive system of public high schools. The small number of publicly funded

17 The English language branch of the Quebec education system was more similar to the system of Ontario. School boards in the province were formally divided on the basis of religion, although almost all Protestant school boards were English speaking while most Catholic school boards were French speaking.

French secondary schools were mainly vocational high schools (*écoles primaires supérieures*) that offered programs in home economics, agriculture, and various trades.<sup>18</sup>

French-speaking students who wanted to obtain university-level training had to attend private (mainly church-run) academies known as *collèges classiques*. Each of the approximately fifty *collèges classiques* was affiliated with one of the two church-run French-speaking universities in the province (Laval and Montréal). The *collèges classiques* offered an eight-year program: four years of college preparatory work, and four years of college-level work, culminating in a Bachelor of Arts degree. The upper-level programs concentrated on traditional subjects such as philosophy and humanities. More specialized training in law, medicine, and engineering was offered at the universities. Because of the *collèges classiques* system, there is some ambiguity in the meaning of a university degree in Quebec prior to the education reforms of the 1960s. In principle, a student who completed eight years at a *collège classique* held a BA degree, although the standards for this degree probably fell short of the standards for a Bachelor's degree from an Ontario or U.S. university.

The lack of public high schools and compulsory school attendance laws, combined with an elitist post-secondary education system, resulted in relatively low levels of education in Quebec. Indeed, figure 3 shows that the modal level of education in Quebec was 5–6 years of schooling, compared with 11–12 years in Ontario. About half of young Quebecers had completed only elementary school (grade 7) or less.

### *3.3. The differential impact of the VRA in Quebec and Ontario*

The institutional circumstances under which VRA benefits were made available to returning veterans in Ontario and Quebec thus were quite different, and also were somewhat different from those south of the border, where the G.I. Bill provided similar benefits for returning servicemen. In particular, although returning veterans to Ontario were well educated by Canadian standards, many had not completed the full five years of high school necessary to enter Ontario universities.<sup>19</sup> In an effort to make VRA university benefits available to as many veterans as possible, Ontario set up a number of special schools that offered pre-matriculation and refresher courses for those who lacked the necessary high school credentials.<sup>20</sup>

Among French-speakers in Quebec, on the other hand, levels of education were substantially lower, creating a greater barrier for veterans to enter university. Moreover, to the best of our knowledge, no special schools were available to French-speaking veterans. Those who wanted to use VRA benefits to obtain a university degree would have had to pursue the traditional eight-year *collège classique* pro-

18 The Dominion Bureau of Statistics 1958–59 *Survey of Elementary and Secondary Education* contains an extensive description of the complex secondary education system in Quebec.

19 By contrast, by 1940 high school completion had become the modal level of education for recent cohorts in the United States (Goldin and Katz 1997).

20 The entry requirements for these special schools were apparently quite liberal. One veteran told us of a friend who had completed only grade school but went to university after one year in a special school.

TABLE 3  
Selected statistics on university enrolment in Canada

	Quebec			Canada
	French <sup>a</sup>	English	Ontario	
1. Total university enrolment				
1941	13,251	3,648	16,615	44,224
1945	12,311	3,648	12,983	47,346
1946	11,448	6,907	21,618	42,443
1947	11,352	9,303	29,174	65,704
1948	12,379	8,672	30,575	82,154
1949	12,335	8,626	25,161	75,833
1950	12,165	8,286	23,598	70,208
1945-46 Change	-863	3,259	8,635	23,261
1945-49 Change	24	4,978	12,178	33,390
2. Number of veterans enrolled <sup>b</sup>				
1946	2,790	7,460	20,500	
1949	2,910	9,240	23,100	
3. Male enrolment in selected universities				
	Montréal <sup>c</sup>	McGill	Toronto	
1945	2,032	2,062	3,577	
1946	2,063	3,933	9,788	
1947	2,600	5,385	10,739	
1948	2,847	5,403	11,435	

*a* Includes students in BA programs the 'collèges classiques.'

*b* The estimates for Quebec and Ontario are obtained by multiplying the total number of veterans enrolled in universities in Canada by the fraction of federal grants received by universities in each respective province. These grants were proportional to the number of veterans enrolled.

*c* Includes affiliated professional schools (HEC and Polytechnique) but excludes affiliated collèges classiques.

SOURCES: *Survey of Higher Education*, 1944-46, 1946-48, 1948-50, and 1952-1954, Statistics Canada (Dominion Bureau of Statistics), Cat. no. 81-402.

gram prior to entering Laval or Université de Montréal.<sup>21</sup> These requirements were probably an insurmountable barrier for all but a handful of French-speaking veterans who wanted to take advantage of the VRA to attend university.<sup>22</sup>

Table 3 presents a variety of statistics on university enrolments in Quebec and Ontario that illustrate the different reactions of the university systems in the two

21 As far as we have been able to determine, the only exception to the policy of requiring a BA degree prior to entering a French-speaking university was the engineering program at the Université de Montréal, which began admitting students without a BA in 1939.

22 We were told of one French-speaking veteran who could not attend university because his English was not good enough to enter one of the special schools that offered catch-up training for returning veterans. This anecdote suggests that attending an English-speaking university was the only option for French-speaking veterans who did not hold a BA degree.

provinces after WW II. The first panel of the table shows that while enrolments roughly doubled in Ontario and in the English-speaking universities in Quebec between 1945 and 1947, they remained virtually constant in Quebec's French-speaking universities and in the college-level programs of the *collèges classiques*. The second panel shows that the number of veterans enrolled in 1946 in the two provinces corresponds very closely to the total jump in enrolment between 1945 and 1946. Finally, the third panel shows a similar pattern of relative enrolment changes in the largest universities in Quebec (Montréal and McGill) and Ontario (Toronto).

We conclude that a combination of two powerful forces led to very different impacts of WW II on the education levels of English-speaking men in Ontario and French-speaking men in Quebec. First, a much smaller fraction of French-speaking men served in the military and were therefore eligible for VRA benefits. Second, the rigid and elitist education system in Quebec made it difficult for French-speaking men who qualified for VRA benefits to actually gain admittance to a French-speaking university. These two complementary forces led the VRA program to have *virtually no effect* on higher-education attainment among French-speakers in Quebec, compared with a strong positive effect on higher-education attainment among English-speakers in Ontario.

#### 4. Measuring the effect of the VRA on education and earnings

##### 4.1. Census and job mobility survey data

With this background we now turn to our analysis of the effects of the VRA on the education and earnings of Canadian men born in the mid-1920s. We begin by examining data from the 1971 Census data. The public use file of the 1971 Census is a 1 per cent sample of the population that provides basic demographic data, information on annual earnings in the previous year (1970), and categorical information on weeks worked and hours per week in the previous year. Education is recorded in a series of broad categories: we construct a conventional measure of years of education by assigning the mid-points of the intervals to people who fall in each category.<sup>23</sup>

Our primary sample consists of 21,241 Canadian-born French-speaking men from Quebec and English-speaking men from Ontario who were between the ages of 25 and 65 in 1971.<sup>24</sup> Summary statistics for this sample are reported in columns 1 and 2 of table 4. We use age at the time of the Census (June 1971) to identify two groups of men potentially eligible for VRA benefits: a narrower group of men

23 The discussion in section 3 suggests that men from Quebec and Ontario who went to university typically had 11 and 13 years of primary and secondary education, respectively. This is more or less confirmed by the 1973 JMS (see below) in which the average years of completed education of people with exactly a high school diploma is 11.34 and 12.75 in Quebec and Ontario, respectively. We use the latter numbers to compute years of education in the two provinces.

24 We identify province by 1971 location, rather than by province of birth. Some limited experimentation suggests that using province of birth gives very similar results.

TABLE 4  
 Sample means: English-speaking Ontario men and French-speaking Quebec men in three samples

	1971 Census		1973 Job Mobility Survey	
	Ontario	Quebec	Ontario	Quebec
<i>A. All men age 25–65</i>				
Mean age	42.5	41.6	41.7	42.4
Pct. 18–21 in 1945	10.3	10.0	8.8	10.9
Pct. 18–24 in 1945	18.2	17.0	16.4	17.7
Percent WWII vets	–	–	19.1	4.6
Education (years)	10.6	8.7	11.8	9.3
Pct. some university	15.1	10.8	20.0	9.7
Pct. worked last year	95.3	91.7	89.2	85.1
Annual earnings	8,742	6,858	10,227	7,435
Sample size	11,163	10,078	1,986	2,131
<i>B. Age 18–21 in 1945 only</i>				
Mean age	45.5	45.5	47.6	47.6
Percent WWII vets	–	–	50.9	12.5
Education (years)	10.5	8.4	10.6	8.3
Pct. some university	17.0	8.0	12.4	5.6
Pct. worked last year	96.1	93.8	89.7	84.4
Annual earnings	10,145	7,506	10,137	7,500
Sample size	1,156	1,004	175	232
<i>C. Age 18–24 in 1945 only</i>				
Mean age	47.0	46.9	49.1	48.8
Percent WWII vets	–	–	58.0	14.3
Education (years)	10.2	8.2	10.8	8.1
Pct. some university	14.6	8.0	16.0	5.3
Pct. worked last year	95.8	93.7	90.5	84.1
Annual earnings	9,643	7,430	10,558	7,352
Sample size	2,033	1,718	326	377

NOTES: Samples include French-speaking Canadian-born men in Quebec and English-speaking Canadian-born men in Ontario age 25–65. 1973 Job Mobility sample is restricted to men who report valid education data for themselves and both parents. Annual earnings are mean annual earnings among those who report positive earnings and weeks worked last year.

age 18–21 in June 1945, who were close to normal university matriculation age at the end of the war; and a broader group of men age 18–24 in June 1945. Means for these two subsamples are reported in panels B and C of table 4. About 10 per cent of the overall 1971 sample fall in the narrower age group, while 17–18 per cent fall in the wider group.

Comparisons of the Ontario and Quebec men in the overall 1971 Census sample and in the two WW II cohorts reveal a number of expected patterns. In particular, Ontario men were better educated (with about two more years of education on average), had higher employment rates over the previous year, and earned 25–30

per cent higher wages. Note that the WW II cohorts in both provinces had employment rates of between 94 and 96 per cent in 1970.

For purposes of evaluating the long-run effects of the VRA on education and earnings the timing of the 1971 Census is close to ideal, since the men who were potentially affected by the VRA were in their late forties and therefore in the 'flat' segments of their life-cycle age-earnings profiles.<sup>25</sup> This feature minimizes the impact of any systematic mis-measurement of labour market experience between veterans and non-veterans, or between individuals with different levels of schooling. However, the advantageous timing of the 1971 Census must be weighed against two disadvantages of the data set. First, education is measured imperfectly: there is little information on post-secondary vocational education or on university education beyond the Bachelor's level. Second, the Census contains no information on military service.

The other data set we use in this paper, the Job Mobility Survey (JMS), addresses some of these deficiencies, although it also has important limitations. This survey was conducted as a supplement to the July 1973 Labour Force Survey and consequently is much smaller than the Census sample. Another limitation of the JMS is the fact that labour market outcomes (annual earnings, weeks of work, and usual hours per week in the previous year) were collected in categorical form, with no information on self-employment earnings.<sup>26</sup> On the plus side, however, the JMS collected a variety of detailed background information, including parental education, years of actual work experience, periods of military service, and data on the number of years of school attended, as well as highest qualification or degree.

A comparison of the 1971 Census sample and the JMS is particularly informative, since these two data sets were collected only two years apart. As can be seen in table 4, the age structures of the 1971 Census sample and the JMS sample are similar. The means of education by province for the two WW II cohorts are fairly similar in the 1971 Census and the JMS. For the broader samples of 25–65-year-olds, however, the two data sets give somewhat different estimates of average education in the two provinces.<sup>27</sup> Closer inspection reveals that for younger workers the 1971 Census education question seems to be downward biased relative to the JMS question, with a bigger relative bias for Ontario men. Part of this reflects the fact that Ontario men in a given education range (e.g., 'some high school') tend to have more years of schooling than Quebec men. At the upper end of the education distribution it also seems that a relatively higher fraction of Ontario men have some university education but no degree and are coded as only have completed high school in the 1971 Census question.

25 See Mincer (1974) for a thorough discussion of the characteristic shape of age-earnings profiles.

26 In our Census samples we construct an employment indicator for individuals who report positive weeks of work and positive earnings, including wage and salary and self-employment earnings. In the JMS we have no information on self-employed earnings, so a comparable indicator of employment is lower.

27 Formal statistical tests indicate that there is a significant difference between average years of education in the Census and the JMS for the broader samples, but not for the narrower samples (except for Ontario men age 18 to 24 in 1945).

The veteran information recorded in the JMS suggests that 50–60 per cent of native-born Ontario men in their late teens and early twenties at the end of WW II served in the military, compared with 12–14 per cent of native-born Quebec men. These rates are similar to the rates estimated from administrative data in table 2, although the Ontario rate from the JMS microdata is higher and the Quebec rate is lower. The discrepancies in veteran service rates between table 2 and table 4 are explainable by several factors. In particular, the age ranges are different, and our JMS samples are limited to English-speakers in Ontario and French-speakers in Quebec.<sup>28</sup> Moreover, the JMS questionnaire instructed individuals who served in ‘reserve’ or ‘militia’ units not to report themselves as serving in the military. This may have led some men who were conscripted and served in reserve units in Canada to ignore their military service.

#### *4.2. Descriptive analysis: 1971 Census*

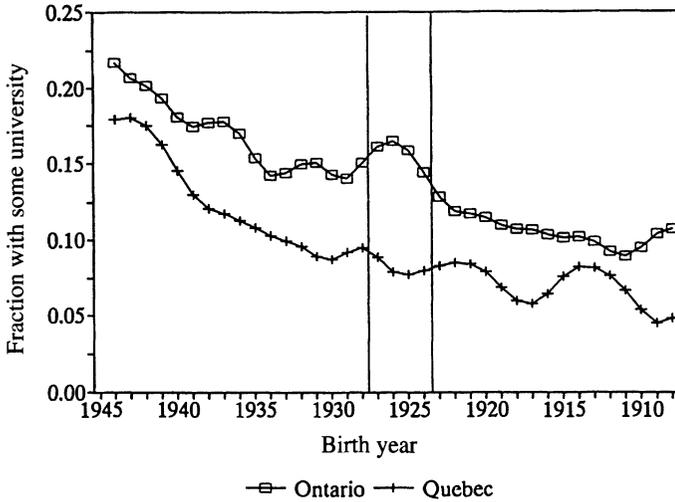
Before we proceed to model the effects of the VRA on education and earnings, it is worth verifying that the inter-cohort patterns of educational attainment and earnings for men in Ontario and Quebec show evidence of a ‘VRA effect.’ Panel A of figure 4 shows that there was a clear surge in the fraction of men from Ontario born in the mid-1920s who went to university, relative to either younger or older men in this province. By contrast, the data for Quebec show no indication of such a shift. The figure is thus consistent with the enrolment data in table 3, which indicate a rapid rise in university enrolment in Ontario at the end of the war, with no change for French universities in Quebec. As a check on the inference that the rise in university attendance was attributable to the VRA we present the same figure for women in panel B.<sup>29</sup> In contrast to the figures on men, the percentage of women from Ontario born in the mid-1920s who attended university is similar to the percentages among slightly younger and older women. Moreover, there is no indication of a systematic change for Ontario women relative to women in Quebec.

Figure 5 shows the age profiles of employment probabilities (panel A) and mean log annual earnings (panel B) for Ontario and Quebec men. While the employment rates of Quebec men are somewhat irregular, there is no indication of either positive or negative selectivity in the relative employment rate of Ontario men born in the WW II cohort. It is interesting to note that the earnings profile for Ontario men peaks for exactly the same age group (those born in 1926) that shows an abnormally high rate of university education in figure 4. Taken together, these two figures strongly suggest that the VRA program affected veteran’s education and subsequent earnings.

28 If we examine all men who were age 14–40 in 1941 in the JMS (excluding immigrants who arrived after 1940) we estimate a 40 per cent veteran rate for Ontario men and a 13 per cent rate for Quebec.

29 Women who served in the armed forces overseas (e.g., nurses) were eligible for the same programs as men. However, the number of women who served in the armed forces is too small to be detected in our Census data.

a. Men



b. Women

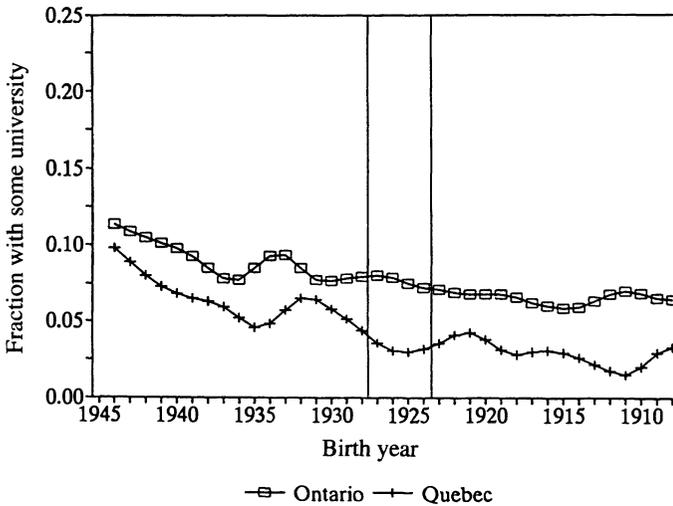
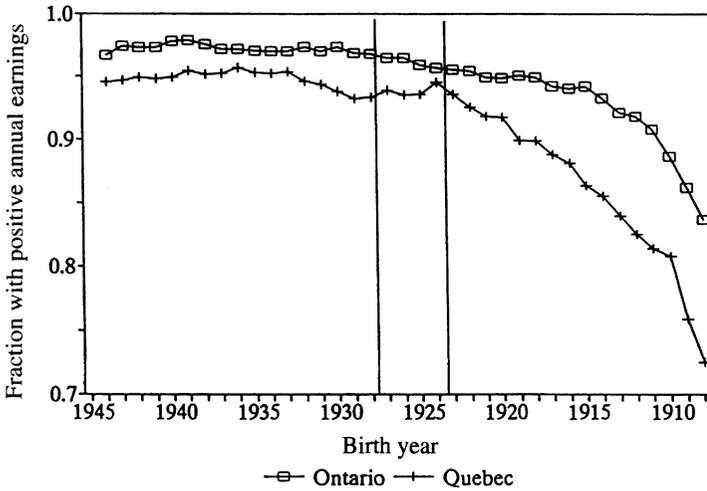


FIGURE 4 Fraction of men and women with some university, 1971 Census (five-year moving average)

4.3. Interpretation of OLS and IV Models

We now turn to estimates of the rate of return to education for men in our 1971 Census sample. We compare ordinary least squares (OLS) estimates, which implicitly treat education as orthogonal to any unobserved components of earnings determination, to instrumental variables (IV) estimates that use potential eligibility for

a. Employment rates



b. Mean log annual earnings

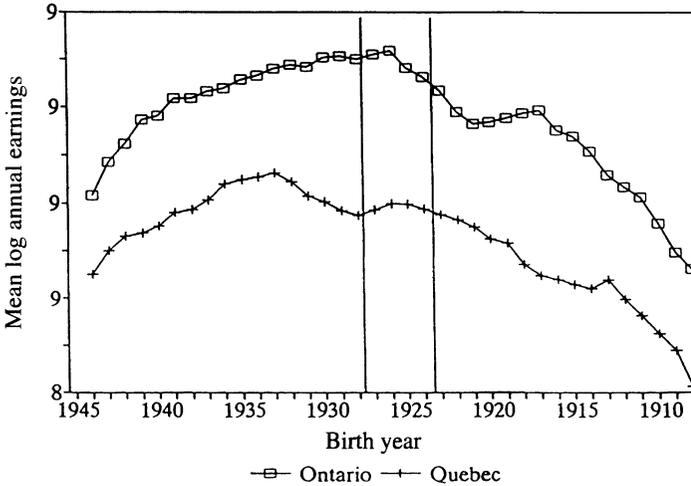


FIGURE 5 Labour market outcomes of men, 1971 Census (five-year moving average)

VRA benefits as an exogenous determinant of education. To illustrate the issues underlying a comparison of these estimators, consider a simple, two-equation model of individual schooling ( $S_i$ ) and earnings ( $y_i$ ) determination:

$$S_i = X_i \alpha + u_i \tag{1}$$

$$y_i = S_i \beta + X_i \gamma + v_i, \tag{2}$$

where  $X_i$  is a vector of observed covariates (such as age or experience, location, etc.), and  $(u_i, v_i)$  are the residual or unobserved components of schooling and earnings, respectively. The coefficient  $\beta$  represents the true causal effect of education: it is the expected increase in average earnings if an additional unit of schooling was assigned to a random sample of the population.

One widely discussed source of bias in OLS estimates of the return to education is a positive correlation between  $u_i$  and  $v_i$ , arising from a tendency for people who would earn higher wages at any level of schooling to acquire more schooling (Griliches, 1977). Such tendencies lead to a positive 'ability bias' in OLS estimates of  $\beta$ . An opposing source of bias that may be particularly relevant for this study is measurement error in observed schooling, arising from survey errors or/and from mis-measurement of the quality of schooling in the relatively heterogeneous Ontario and Quebec education systems.<sup>30</sup> Finally, a more subtle bias arises if the true return to education varies across individuals and if individuals with higher returns to schooling attend school longer. As illustrated in Card (1999), this will lead to an upward bias in the conventionally estimated return to education relative to the average return to education in the population.

A standard solution to the problems of ability bias and measurement error bias is the method of instrumental variables. Consider an observed covariate  $Z_i$  that affects schooling but has no direct effect on earnings. The IV estimate of the return to schooling ( $\beta_{iv}$ ) is a simple function of the coefficients of the unrestricted reduced form models that relate schooling and earnings outcomes to the observed  $X$ s and the instrument  $Z$ :

$$S_i = X_i \pi_{sx} + Z_i \pi_{sz} + \eta_i, \quad (3)$$

$$y_i = X_i \pi_{yx} + Z_i \pi_{yz} + \epsilon_i. \quad (4)$$

Specifically,  $\beta_{iv} = \pi_{yz}/\pi_{sz}$ . If the true underlying model of earnings has a constant return to education for all individuals than the assumptions  $\pi_{sz} \neq 0$  and  $E[Z_i v_i] = 0$  are sufficient to ensure that  $\beta_{iv}$  is consistent for the true return to education. More generally, if the return to education varies across individuals, the IV estimator may or may not provide a consistent estimate of the average return to education in the population as a whole (see, e.g., Wooldridge 1997; Angrist and Imbens 1995; Angrist, Imbens, and Rubin 1996). In general, if the instrument  $Z_i$  is dichotomous and has a uniformly positive (or at least non-negative) effect on schooling outcomes, the IV estimator provides a consistent estimate of an average marginal return to education among the subgroup of individuals whose schooling is affected by the instrument (Angrist and Imbens 1995).

In this paper we use *potential eligibility* for VRA benefits as an instrument for schooling outcomes. Specifically, in the light of the discussion and results of the

30 If observed schooling differs from true schooling by an additive white noise error, OLS estimation of equation (2) using observed schooling will lead to a downward-biased estimate of  $\beta$ .

previous sections, we use an indicator for the cohort of Ontario men born in the early 1920s as an instrument for education. In principle, this estimation strategy is implementable on a sample that includes only Ontario men, provided that any independent effect of age (or experience) on earnings is sufficiently smooth. For example, if earnings depend on education and a low-order polynomial of experience, then one can use a cohort indicator as an instrument for schooling. Such a strategy is analogous to the so-called regression discontinuity method described by Thistlethwaite and Campbell (1960) and employed in recent studies by Angrist and Lavy (1999) and Van der Klaauw (1997).

The assumption that the independent effect of age (or experience) on earnings is smooth can be relaxed by pooling data for Ontario men and Quebec men and including unrestricted age or experience effects in the earnings models. Provided that the *difference* in the earnings effects of age or experience in Ontario relative to Quebec is smooth, it is still possible to identify the increments to schooling and earnings associated with an Ontario-specific cohort dummy. Underlying this specification is the assumption that all cohorts of French-speaking Quebec men were essentially ineligible for VRA benefits. As noted earlier, we believe this is a valid assumption, since only a small fraction of French-speaking Quebec men served in WW II, and since the Quebec university system made no effort to admit returning veterans.

The use of potential eligibility for VRA benefits as an instrument for schooling presumes that this variable has no independent effect on earnings. If veteran status affects earnings, however, then this assumption is false, since Ontario men born in the early 1920s had relatively high military service rates – higher than earlier or later cohorts of Ontario men or any cohorts of French-speaking Quebec men (see figure 1). In this case, an IV estimate of the return to education based on a dummy for the 1920s cohort of Ontario men will contain an asymptotic bias equal to the true earnings effect of veteran service, multiplied by the difference between the fraction of veterans among Ontario men from the early 1920s cohort and the fraction of veterans in ‘surrounding’ cohorts of Ontario men.<sup>31</sup>

We are unaware of any previous studies of the effect of veteran status on the earnings of Canadian men. Angrist and Krueger's (1994) study of WW II veterans in the United States suggests that military service had a modest negative effect on annual earnings in both 1969 and 1979.<sup>32</sup> Similarly, Angrist (1990) found a negative effect of military service in the Vietnam War. Our own analysis of the JMS data

31 Formally, this requires that the second-stage earnings model allows for smooth differences in the age/experience profiles of Ontario and Quebec men, so that the dummy for Ontario men born in the 1920s identifies differences in earnings relative to slightly older and slightly younger cohorts of Ontario men. The asymptotic bias due to the omission of veteran status from the regression is obtained using the standard omitted variable formula.

32 In theory, the effect of military service could be either positive (e.g., because of preferential treatment of veterans) or negative (e.g., lost human capital). Angrist and Krueger's estimates for the effect of WW II service on 1969 earnings range from -4 per cent to +2 per cent. Their estimates of the effect in 1979 are more negative and range from -14 per cent to 0. These estimates are obtained using IV methods that control for the ‘ability bias’ that plagued earlier studies, such as Rosen and Taubman (1982), which found positive effects of WW II military service using OLS.

(see below) also finds a negative effect of veteran service on earnings. Assuming that the true (selection-corrected) military service effect is negative for Canadian veterans, IV estimates based on an indicator for the cohort of Ontario men born in the early 1920s will be *downward biased*.

One interesting aspect of our instrumentation strategy is that the VRA program may have been large enough to have general equilibrium effects on the return to education in Ontario, especially among men born in the 1920s. Standard cross-sectional estimates may overstate the return to education in general equilibrium if the increase in the supply of educated workers is large enough to depress skill prices. Unfortunately, it is generally impossible to separate these general equilibrium effects from time effects in single cross-section. If different cohorts are imperfect substitutes in production, however, a large increase in the supply of education of a specific cohort (like Ontario men born in the 1920s) will depress the return to education for this cohort relative to other cohorts (Card and Lemieux 2001), and our estimates would capture this 'general equilibrium cohort effect.' How much of a general equilibrium effects our IV estimates will capture depends on the elasticity of substitution across cohorts.<sup>33</sup>

#### 4.4. OLS and IV estimates of the return to education: 1971 Census

Table 5 presents a series of OLS and IV estimates of the return to education using 1971 Census data for English-speaking men from Ontario and French-speaking men from Quebec. The dependent variable used in all models is log annual earnings in 1970 (including wage and salary and self-employment income). The specifications in rows 1 to 4 include controls for potential experience (age-education-6), while those in rows 5 and 6 include controls for age. In the presence of severe endogeneity biases in education (or substantial measurement errors) the latter specifications may be preferred, since potential experience is constructed from observed education and therefore 'inherits' any endogeneity components or measurement errors from observed education. On the other hand, there is much evidence that earnings are better described as a function of education and experience than as a function of education and age (e.g., Mincer 1974; Murphy and Welch 1990).

Each column of the table corresponds to a different specification for the earnings model. The model in column 1 includes a linear education term, a quartic function of experience or age, and a single dummy variable for Quebec (as well as a full set of interactions of the five categories for weeks worked in the previous year with a full-time work indicator). These specifications therefore assume that the experience or age profiles of English-speaking Ontario men and French-speaking Quebec men

33 If there is no substitutability across cohorts, our IV estimates capture the full general equilibrium, since an increase in the supply of education for one cohort only depresses the return to education for that specific cohort. If cohorts are perfect substitutes, our IV estimates do not capture any general equilibrium effects, since the VRA-induced increase in supply affects men in all cohorts equally. Card and Lemieux (2001) find that different cohorts are strong (elasticity of substitution of 5) but not perfect substitutes. This suggests that our IV estimates capture some, but not all, general equilibrium effects.

TABLE 5  
 OLS and IV estimates of return to education using 1970 earnings

	(1)	(2)	(3)
<i>Models controlling for experience</i>			
1. OLS education coefficient	0.070 (0.002)	0.070 (0.002)	0.070 (0.002)
2. IV Using Ontario * age 18–21 in 1945			
a. Reduced-form education	0.425 (0.093)	0.320 (0.096)	0.465 (0.101)
b. Reduced-form earnings	0.060 (0.021)	0.056 (0.022)	0.073 (0.023)
c. IV estimate	0.141 (0.050)	0.175 (0.073)	0.157 (0.051)
3. IV Using Ontario * age 18–24 in 1945			
a. Reduced-form education	0.303 (0.076)	0.172 (0.082)	0.442 (0.088)
b. Reduced-form earnings	0.025 (0.017)	0.021 (0.019)	0.035 (0.020)
c. IV estimate	0.081 (0.055)	0.125 (0.107)	0.080 (0.044)
4. IV estimates for women using Ontario * Age 18–21 in 1945			
a. Reduced-form education	-0.048 (0.110)	-0.221 (0.114)	-0.080 (0.122)
b. Reduced-form earnings	0.017 (0.033)	-0.002 (0.034)	0.009 (0.037)
c. IV estimate	-0.360 (1.189)	0.007 (0.153)	-0.111 (0.524)
<i>Models controlling for age</i>			
5. OLS education coefficient	0.062 (0.001)	0.062 (0.001)	0.062 (0.001)
6. IV Using Ontario * age 18–21 in 1945			
a. Reduced-form education	0.341 (0.111)	0.350 (0.120)	0.252 (0.177)
b. Reduced-form earnings	0.050 (0.022)	0.044 (0.024)	0.037 (0.035)
c. IV estimate	0.145 (0.067)	0.126 (0.068)	0.148 (0.145)

NOTES: Standard errors are in parentheses. Controls include weeks and hours per week plus: column 1: quartic in experience (or age) and dummy for Quebec; column 2: quartic in experience (or age), dummy for Quebec, and interaction of Quebec dummy with quartic in experience; column 3: full set of experience (or age) dummies, dummy for Quebec, and interaction of Quebec dummy with quartic in experience

are parallel. The model in column 2 relaxes this assumption by allowing smooth (quartic) province-specific experience or age profiles. Finally, the model in column 3 includes an unrestricted set of experience or age dummies, as well as an interaction between a Quebec dummy and a quartic function of experience or age. This model allows for a completely non-parametric relationship between earnings and experience or age in the pooled Ontario-Quebec labour market, while assuming that

differences in the experience or age profiles across provinces are a smooth function of experience or age.

The first row of the table reports OLS estimates of the return to education for each specification. The return to education is estimated precisely at 7 per cent. Examination of these estimates confirms that the estimated returns to education are unaffected by relaxing restrictions on the returns to experience across provinces.

Rows 2a and 2b report the unrestricted reduced form coefficients of a dummy variable for Ontario men who were age 18–21 at the end of WW II in models for education and earnings, respectively, while row 2c reports the corresponding IV estimator. A parallel set of estimates, using a broader age cohort (age 18–24 at the end of the war), is contained in rows 3a–3c. Given the targeting of VRA-related programs, we believe that the narrower age cohort includes the group of veterans most likely to have used VRA benefits to attend university, whereas the broader age cohort includes a larger fraction of men who were 'too old' to resume their studies in 1946 or 1947.<sup>34</sup> Finally, as a check on our inference that the relative surge in education and earnings among the WW II cohort of Ontario men is attributable to their eligibility for VRA benefits, we perform a similar analysis using data on women in rows 4a–4c.

A number of conclusions emerge from these various estimates. First, the first stage estimates of the extra education acquired by Ontario men in the WW II cohort are between 0.2 and 0.4 years, and are generally statistically significant.<sup>35</sup> A second important conclusion from table 5 is that although the IV estimates are somewhat imprecise, they exceed the corresponding OLS estimates. This is consistent with other recent studies in the literature that compare OLS and IV estimates (see Card 1999 for a summary).<sup>36</sup> There are several explanations for this pattern. First, OLS estimates presumably are downward-biased by measurement error, whereas the IV estimates are not. One might expect measurement error to account for a 10–20 per cent positive gap between IV and OLS, other things being equal (see Card 1999). Second, if rates of return to education vary in the population, and if the Ontario men who went to university because of the VRA program had relatively high marginal returns, this will also lead the IV estimates in table 5 to exceed the OLS

34 Note that although the war ended in the summer of 1945, our data on university enrolments suggest that veterans began entering university only in the fall of 1946, with a peak in 1947. Men who were 21 in June 1945 were therefore 22–24 by the time they could potentially enter university.

35 Using administrative data on the number of men who received VRA benefits, together with population counts, we estimate that about one in ten Ontario men who were in their late teens or early twenties at the end of the war received VRA assistance. If we assume that the average recipient acquired two to three years of additional university education, this calculation suggests that potential eligibility for VRA benefits may have raised schooling by 0.2–0.3 years. It is interesting that Bound and Turner (1999) find comparable effects for the U.S. GI Bill on educational attainment.

36 In a previous version of this paper, we estimated similar models using the 1981 Census. The key disadvantage of the 1981 Census is that by the time it was collected, men who were potentially eligible to receive VRA benefits were at or nearing retirement age. In general, the 1981 results show many of the same patterns as the 1971 estimates, although the larger sample sizes in 1981 yield slightly more precise estimates. The 1981 IV estimates also tend to be more tightly clustered around the corresponding OLS estimates than the 1971 estimates.

estimates. The possibility that men affected by the VRA incentives had relatively high marginal returns to schooling, yet would not have attended university in the absence of the program, is plausible if most of these men were from relatively poor family backgrounds and needed the VRA benefits to afford university.<sup>37</sup>

Although all the estimates in table 5 are obtained from samples that pool Ontario and Quebec men, the different specifications use the inter-cohort patterns of education and earnings for the two provinces in different ways to identify the return to education. The IV specifications in column 2 are essentially identified by comparisons *within* Ontario. To see this, note that the reduced-form models include unrestricted province-specific experience profiles and a dummy for Ontario men in the WW II cohort. Apart from the fact that the hours control variables are restricted to have the same coefficients in Ontario and Quebec, one would obtain the same coefficients on the WW II cohort dummy if the sample were restricted to only observations from Ontario. Indeed, models fitted to the subset of Ontario observations yield IV estimates that are virtually identical to those in the column 2.

By comparison, the specifications in column 3 of table 5 use information on the inter-cohort differences in education and earnings within Ontario relative to the same differences in Quebec to identify the return to education. By including unrestricted experience dummies, these models abstract from inter-cohort differences within provinces and instead focus on the interprovincial differences across cohorts. The similarity of the estimates in columns 2 and 3 suggests that the two types of comparisons yield comparable inferences about the return to education.

Another aspect of table 5 is the set of estimates for women (row 4). These are uniformly ill behaved, with insignificant first-stage effects for the instrumental variable and large nominal standard errors for the IV estimates. These negative findings for women – who were essentially unaffected by the VRA program – give more confidence that the findings for men are a reflection of the program itself, rather than of some artefact of the inter-cohort comparisons.

Finally, the results for the models with controls for age instead of potential experience are reported in rows 5 and 6. As expected, the OLS estimates reported in row 5 are smaller than they are when potential experience is used as a regressor. The IV estimates reported in row 6c are also smaller than the corresponding IV estimates obtained with controls for potential experience instead of age (row 2c). The difference is not statistically significant, however, because of the imprecision of the IV estimates. The results suggest, nonetheless, that the (potential) endogeneity of the potential experience variable does not affect the estimated return to education very much.

A more direct test of endogeneity of potential experience consists of instrumenting both education *and* potential experience using potential VRA eligibility and polynomials in age as IV. The resulting estimate of the return to education for the model of column 1 is 0.099 (standard error of 0.052), which is not statistically different from the IV models in row 2c, where potential experience is presumed

37 The return to a year of university may also be larger than the return to a year of primary or secondary school.

exogenous. We infer that the treatment of labour market experience as exogenous does not introduce a significant bias in the IV estimates of the return to education.

### **5. An alternative estimation strategy**

As we noted earlier, the IV estimates in the previous section will confound any direct effect of WW II service on earnings, with the indirect impact arising through veteran's higher education. Although we suspect that any direct veteran effect on earnings is small and negative, we decided to investigate an alternative estimation scheme that allows us to identify the direct 'veteran' earnings effect while still using the VRA as a source of exogenous variation in education outcomes among the WW II cohort. Specifically, we hypothesize that VRA benefits had a bigger impact on the education outcomes of men from relatively disadvantaged family backgrounds. We therefore use the interaction of family background and veteran status as an instrument for education and allow veteran status to have an independent effect on earnings outcomes. This method requires data on family background and veteran status for samples of men who potentially served in WW II. Such data are available in the 1973 JMS.

Summary statistics for our JMS sample are reported in table 4. As with the Census samples, we include only native-born English-speaking Ontario men and French-speaking Quebec men who were 25–65 at the time of the Survey. The JMS asked about a variety of potential family background variables, including mother's education and father's education and occupation. For simplicity, however, we use father's education as a one-dimensional index of family background.<sup>38</sup> This variable was reported in categorical form: we transform it to a years of education measure by assigning to each category the mean years of education for men in our sample who report themselves in the same education categories. The means of father's education for the Ontario and Quebec men in the overall sample are 8.6 and 7.1 years, respectively.

#### *5.1. OLS and IV results*

Table 6 reports a variety of OLS and IV estimates of earnings models fit to the pooled Ontario-Quebec sample from the JMS. The dependent variable in these models is the log of average hourly earnings from wage and salary employment last year.<sup>39</sup> In fitting these models we use actual years of work experience as reported in the JMS (rather than potential experience), although the estimates are fairly similar if we use the more conventional experience measure. Column 1 reports a simple

38 Similar results are obtained when both father's and mother's education are used as measures of family background.

39 Annual earnings for 1972 are reported in the JMS in nineteen categories. Information on weeks worked and usual hours per week during 1972 is also reported in six and three categories, respectively. We construct an hourly wage rate using the mid-points of the earnings, weeks, and hours variables. Ham and Hsiao (1984) examine various econometric methods for modelling the distribution of hours of work in the 1971 Canadian Census and conclude that a midpoint assignment algorithm is reasonably accurate.

TABLE 6

OLS and IV estimates of hourly earnings equations for French-speaking Quebec men and English-speaking Ontario men age 25–65, from the 1973 Job Mobility Survey

	OLS	Reduced forms		Instrumental Variables		
	Log wage (1)	Log wage (2)	Educ (3)	Log wage (4)	Log wage (5)	Log wage (6)
a. Years of education	0.065 (0.003)	–	–	0.140 (0.091)	0.151 (0.036)	0.084 (0.007)
b. Served in World War II	0.076 (0.030)	1.785 (0.519)	0.261 (0.085)	0.012 (0.084)	–	0.059 (0.031)
c. Father's education	0.009 (0.003)	0.477 (0.022)	0.041 (0.004)	–0.026 (0.042)	–0.031 (0.017)	–
d. Interaction: Father's educ * WW II vet	–	–0.112 (0.058)	–0.016 (0.009)	–	–	–
Instruments	None	None	None	d	b,d	c,d
P-value for exclusion of instruments in first-stage	–	–	–	0.055	0.000	0.000
P-value for over-identification	–	–	–	–	0.894	0.498

NOTES: Standard errors in parentheses. Based on sample of 3,196 observations for French-speakers in Quebec and English-speakers in Ontario who report valid education data for themselves and both parents. All models include a dummy variable for Quebec and controls for actual experience and its square.

OLS specification that includes a WW II veteran dummy, a dummy for Quebec residence, father's education, as well as education and a quadratic in work experience. Despite differences in the variable definitions, the estimated rate of return to education is very close to the corresponding OLS estimate obtained from the 1971 Census sample. The estimated WW II veteran effect is positive and marginally significant, with a magnitude fairly similar to one obtained in 1970 Census samples for the United States (see Angrist and Krueger 1994). Finally, as is typically found in the U.S. literature, the estimated coefficient of father's education is small but statistically significant. While not shown in the table, a comparable model that excludes father's education yields a slightly higher return to education (0.068).<sup>40</sup>

Our first strategy for identifying the causal effect of education on earnings is to use the interaction of veteran status and father's education as an instrumental variable for education, controlling for veteran status and father's education in the wage equation. The IV estimates corresponding to this basic specification are reported in column 4 of table 6. The corresponding reduced forms are presented in columns 2 and 3. Consistent with our hypothesis that VRA benefits had a bigger effect on educational attainment for young men from relatively disadvantaged backgrounds, the reduced-form effect of WW II veteran status on education is large and positive, while the coefficient on the interaction of veteran status and father's education is negative and marginally significant ( $t = 1.9$ ).<sup>41</sup> The IV estimate of the return to education is larger than the OLS estimate but relatively imprecise. The veteran status coefficient in the IV model is essentially zero while the estimated coefficient of father's education is weakly negative.

In an attempt to improve the precision of the IV estimate, we present a variety of more restrictive specifications in the remaining columns of table 6. In column 5 we exclude both veteran status and its interaction with father's education from the wage equation and use both variables as instruments for education. This specification is valid if veteran status has no direct effect on earnings, as is implicitly assumed in our cohort analysis of the Census data. Although this change does not much affect the point estimates, it greatly improves the precision of the IV estimates. The estimated return to education is about 15 per cent, which is comparable to many of the IV estimates in table 5. The IV estimate is also statistically different from the OLS estimate under a Hausman test. Note that the model easily passes a standard over-identification test (see the last row of table 6).

An alternative exclusion restriction is to assume that father's education has no direct effect on earnings and use father's education and its interaction with veteran status as instruments for education. This specification is presented in column 6 of

40 As discussed in Card (1999), in the presence of measurement error in schooling one would expect the addition of father's education to lower the estimated own-schooling coefficient, even if father's education has no direct effect on earnings. The magnitude of the drop in the schooling coefficients in the models that exclude and include father's education in the JMS is about what would be expected because of measurement error.

41 One reason for using other instrumentation strategies in columns 5 and 6 is that there may be a problem of weak instruments in column 4, since the  $t$ -statistic is only 1.9.

table 6 and yields an even more precise estimate of the rate of return to education, which is about 25 per cent above the corresponding OLS estimate.

One problem with our primary identification strategy based on the interaction between father's education and veteran status is that this interaction is highly correlated with the interaction between father's education and province, and it is possible that father's education should enter the earnings model directly with province-specific coefficients. This is particularly a concern if the Quebec education system (and/or other features of the Quebec economy) induced a higher intergenerational correlation between fathers and sons than existed in Ontario.

Another problem with this estimation strategy is that it ignores potential biases in the estimated veteran service effect (and possibly in the interaction between veteran service and father's education) arising from the non-random selection of veterans. If veterans have unobserved characteristics, such as good health or higher ability, that would tend to lead to higher earnings, then the veteran coefficient in the models in table 6 will be upward biased (leading to a bias in all the other coefficients). Moreover, except under restrictive conditions, the interaction of veteran service and parental education also may be correlated with unobserved determinants of earnings, violating the necessary conditions for a valid instrumental variable.

In the light of these difficulties, we estimate a final set of earnings models that treat both education *and* veteran status as endogenous variables. The specification of these models is motivated by the observation that children of U.K. immigrants were more likely to serve in WW II than other (native-born) men. If men of U.K. ancestry had higher veteran rates, and those of U.K. ancestry with less educated parents were more likely to augment their education because of the VRA program, then one can potentially use U.K. ancestry, and the interaction of U.K. ancestry with father's education, as instruments for veteran status and education. Alternatively, if we ignore any concerns about the potential endogeneity of education, U.K. ancestry can be used as a potential instrument for veteran status.

Our findings from this analysis are reported in table 7. To abstract from differences in the effect of family background by province, we constructed a sample of native-born English-speaking men in provinces other than Quebec or Newfoundland.<sup>42</sup> Column 1 of table 7 reports OLS estimates of an earnings model for this sample. We include education, a quadratic in experience, veteran status, father's education, an indicator for an immigrant father, a dummy variable for U.K. parentage and the interaction of this variable with father's education, as well as dummies for different provincial labour markets. The estimates are very similar to those reported in column 1 of table 6.

Column 5 of table 7 reports IV estimates from a specification that treats both veteran status and education as endogenous, using U.K. ancestry and its interaction with parental education as instruments. The underlying reduced form models are reported in columns 2–4. Consistent with the hypothesis that VRA benefits increased

42 Newfoundland was not part of Canada until 1949.

TABLE 7  
 OLS and IV estimates of hourly earnings equations for English-speaking men outside Quebec and Newfoundland

	OLS	Reduced forms			Instrumental variables
	Log wage (1)	Educ (2)	Veteran status (3)	Log wage (4)	Log wage (5)
a. Years of education	0.063 (0.003)	–	–	–	0.166 (0.100)
b. Served in World War II	0.055 (0.022)	–	–	–	–0.459 (0.439)
c. Father's education	0.009 (0.003)	0.418 (0.017)	0.009 (0.002)	0.036 (0.003)	–0.030 (0.038)
d. Immigrant father	0.046 (0.022)	0.265 (0.115)	0.014 (0.014)	0.064 (0.023)	0.026 (0.034)
e. Father from U.K.	0.066 (0.085)	1.292 (0.459)	0.130 (0.055)	0.155 (0.090)	–
f. Father from U.K. * father's educ	–0.010 (0.009)	–0.112 (0.047)	–0.003 (0.006)	–0.017 (0.009)	–

NOTES: Standard errors in parentheses. Based on sample of 4,880 English-speaking native-born men age 25–65 outside Newfoundland and Quebec who report valid education data for themselves and both parents. In column 5 veteran status and education are treated as endogenous and 'father from U.K.' and interaction of 'father from U.K.' and father's education are used as instruments. All models include province dummies and controls for actual experience and its square.

veteran's education, with a bigger gain for veterans from less educated families, we find that children of U.K. immigrants have higher education than other men, with an effect that declines with father's education. (In contrast, children of less educated U.K.-born fathers are no more or less likely to have served in WW II). The implied IV estimate of the return to education is 0.17 (standard error 0.10), while the IV estimate of the veteran effect is  $-0.46$  (standard error 0.44). Although rather imprecise, these point estimates suggest that, consistent with Angrist and Krueger's (1994) results for the United States, Canadian veterans of WW II suffered earnings losses because of their military service and that the returns to the extra education induced by the VRA program were above the conventionally estimated rates of return for Canadian men.

## 6. Summary and conclusions

In this paper, we use the unique experience of Canada in the aftermath of World War II to identify the effects of a large scale education program – the Veteran's Rehabilitation Act – on educational attainment and earnings of Canadian men. Because of interprovincial differences in military enlistment rates and education systems, the VRA had a much larger impact in Ontario than in Quebec. Simple plots of data from the 1971 Canadian Census confirm that the VRA had a strong impact on the educational attainment and earnings of Ontario men who were in the late teens and early twenties at the end of the war relative to older and younger cohorts of men from this province. By contrast, the VRA had no discernable impact on the education and earnings of Quebec men from the same generation.

Building on this fact, we estimate the rate of return to schooling using potential eligibility for VRA benefits, as measured by an indicator for the cohort of Ontario men born in the early 1920s, as an exogenous determinant of schooling. Data from the 1971 Census indicate that Ontario men in this cohort acquired 0.2 to 0.4 years of additional education and earned significantly higher wages relative to what would have happened in the absence of the VRA. The relative rises in education and earnings are consistent with a 15 per cent rate of return to education (the IV estimate). This is substantially higher than conventional OLS estimates of the return to education (7 per cent) for the same sample. The fact that the IV estimates are larger than the OLS is consistent with a variety of explanations, including measurement error in the education variable and some heterogeneity in the return to education across individuals.

One potential shortfall of the estimates obtained from the 1971 Census is that they confound any direct effect of WW II service on earnings, with the indirect impact arising through veteran's higher education. Since veteran status information is not available in the Census, we use information from the 1973 Canadian Job Mobility Survey and find that, if anything, the direct effect of WW II service on earnings is *negative*. This suggests that the IV results obtained using the Census understate the true return to education.

We also explore an alternative identification strategy that utilizes information on family background available in the 1973 Canadian Job Mobility Survey. We hypothesize that veterans from relatively disadvantaged family backgrounds were more likely to be affected by the VRA's incentives than were those from other backgrounds. Using the interaction of veteran status and family background as an exogenous determinant of schooling, we find rates of return to education comparable to those from our analysis of the 1971 Census.

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