THREE ESSAYS IN EMPIRICAL LABOUR ECONOMICS

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ABSTRACT

This thesis is a collection of three essays that use what have arguably become the three most common empirical strategies found in the labour economics literature. The first essay uses *descriptive analyses* to document and explain a long-term secular increase in on-the-job search (OJS) in the U.S. and Canada between the mid-1970s and mid-1990s. Based on observed concomitant trends in job-to-job transition rates and returns to wage changing, the OJS increase appears most consistent with a long-term reduction in the costs of searching while employed. Moving beyond descriptive analyses, the second essay takes an *instrumental variables* approach to estimating the effectiveness of internet job search in reducing unemployment durations. Although raw means indicate shorter unemployment spells among internet searchers, the evidence from a more complete model that controls for observable characteristics suggests no effect of using the internet on unemployment durations. Finally, the third essay employs a *differences-in-differences* strategy to infer the employment and hours of work effects of Sunday shopping deregulation. The results suggest that deregulation led to a long-run increase in labour demand that was disproportionately satisfied through an increase in the employment level. In conclusion the thesis offers some insights into the relative advantages and disadvantages of these three identification strategies.

TO MONA
FOR ALL HER LOVE AND LAUGHTER
AND
FOR TRYING SO HARD
TO BE INTERESTED

PREFACE

This thesis has been influenced throughout by the generous comments of my thesis supervisor, Peter Kuhn. Despite his decision to move to the University of California, Santa Barbara in July 1999, Peter has remained committed over the past three years to providing me with prompt feedback whenever it was requested I am sincerely grateful to him for the more than 284 email messages he sent since July 1999 (few if any have been deleted) and for the wonderful hospitability he provided during my visits to Santa Barbara in the summers of 2000 and 2001. I would also like to thank John Burbidge for his regular advice, Lonnie Magee for always having an open door to respond to pressing statistical enquiries, and the Social Sciences and Humanities Research Council of Canada for financial support.

My desire to learn and continue my studies to the Ph.D. level is primarily due to the influence of my first teacher and mother, Andrina. In his speech on my wedding day in January 1999, my father, Erik, recalled thinking as the father of a primary school boy "at least he's strong; he'll make a good workman." My thanks to my mother for all she has taught me and to my father for the way that his surprise at my school success has always, in some strange way, encouraged me.

The second chapter of this thesis was prepared with the intention of joint publication with Peter Kuhn. I had primary responsibility for the data preparation and empirical analysis, and played a role in the writing of the paper.

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INTRODUCTION

Following a gradual decline in the importance of empirical analysis in the labour economics literature between the late-1960s and early-1980s, the literature appears to have experienced a definite reversal of this trend over the past decade and a half. In his contribution to the first volume of the *Handbook of Labor Economics*, Stafford (1986) reports that the percentage of labour economics articles appearing in major U.S. journals that contain some empirical work declined from 86 between 1965 and 1969 to 71 between 1980 and 1983 (see Table 7.2). Updating these data in their contribution to the most recent volume of the *Handbook*, Angrist and Krueger (1999) find that between 1994 and 1997 the comparable statistic had rebounded to nearly 80 percent. In addition, over the same period they find that 66 percent of studies from all fields of economics contained some empirical work (see Table 1). Not only is empirical analysis becoming increasingly common within the labour economics field, labour economists appear also to be regaining a clear distinction within the broader economics community for their use of data. Perhaps as a consequence of their reliance on data they have, as Angrist and Krueger remark, come to be distinguished by their development of econometric and statistical methods.

This thesis is a collection of three essays that use what have arguably become the three most common empirical methods found in the labour economics literature: chapter 1 uses a *descriptive analysis* to explain a long-term secular

increase in on-the-job search; chapter 2 uses *instrumental variables analysis* to examine the effectiveness of internet job search in reducing unemployment spells; and chapter 3 uses *differences-in-differences analysis* to study the labour demand effects of Sunday shopping deregulation. In their discussion of these empirical techniques in the literature, Angrist and Krueger distinguish research using descriptive techniques from research concerned with causal inference. Examples of causal inference include studies using instrumental variables techniques, such as Card's (1995) research on the effect of schooling on earnings, and studies using differences-in-differences techniques, such Angrist's (1990) study of the effect of military service on future civilian earnings. However, it is also possible to use what are essentially descriptive statistics to document an empirical regularity and infer causation. In addition to the first chapter of this thesis, examples of research employing this strategy include the Berman et al. (1994) study on trends in skill-biased technological change and Riddell's (1993) investigation of the divergence in union density rates in the U.S. and Canada through the 1970s and 1980s. The collection of essays presented in this thesis offers some useful insights into the relative advantages and disadvantages of using each of these three empirical strategies to infer causation. This discussion is presented in the conclusion to the thesis.

The first chapter of this thesis represents the first study to examine how the incidence of job search among employed workers has been changing over time. The motivation for this research is the popular perception that today's young

workers are less committed and loyal to their employers than their parents or grandparents were when they were the same age.[1] Using a variety of data sources with information on employed workers' search activities, evidence of a long-term secular increase in on-the-job search (OJS) rates is found in both the U.S. and Canada between the mid-1970s and mid-1990s. The remainder of the essay is concerned with explaining this trend.

The results from decomposition analyses suggest that an important part of the OJS increase is not simply an artifact of compositional effects, including cohort effects, but reflects a true behavioural change among today's employees. Using a variety of descriptive statistics from various data sources, the role of concomitant trends in sectoral reallocation and residual wage inequality, improved search efficiency, and a possible long-term decline in mobility and search costs are evaluated. Following a strategy of ruling out explanations based on inconsistencies with the data, the essay concludes that the upward trend in OJS appears most consistent with a long-term reduction in the costs of employed job search. Since there is no evidence that the relative use of any particular search technology changed over the period, the data are entirely consistent with a reduction in the costs associated with foregoing feelings of loyalty to an employer while searching on-the-job.

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[1] This perception was recently expressed by *The Economist* in their special *Survey of Youth* supplement (December 23, 2000) and in a front-page article of *The Globe and Mail* entitled "Are workers loyal? Not likely" (August 30,2001). Interestingly, the latter article refers to a study by private management consulting firm Towers Perrin, whose study based on a cross-section of 6,000 North American workers leads its authors to conclude that employee internet access and the weakening disloyalty stigma associated with mobility has created a "potential time bomb" for employers.

The second essay is the first research to examine the effect of internet job search on the unemployment durations of jobless workers. This research is made possible by two special supplements to the Current Population Survey (CPS) conducted in December 1998 and August 2000 that asked all respondents whether they regularly use the internet for job search. Using the sample of unemployed workers, matched with job search outcomes from subsequent CPS files, this research goes beyond the descriptive techniques of the first essay by applying instrumental variables to the estimation of a duration model with a fully flexible baseline hazard. Two different sets of instruments are applied to identify exogenous variation in the incidence of internet job search: (i) indicators of whether somebody in the respondent's household uses the internet at a location away from home; and (ii) indicators of state-level internet access rates and costs of access. In addition to the issue of an endogenous causal variable, the empirical strategy addresses two other key econometric issues: (i) large gaps in the measured unemployment durations of CPS respondents due to the eight months between the two CPS rotation groups; and (ii) length-biased sampling of unemployment spells induced by sampling from the stock of unemployed workers.

Descriptive statistics indicate that internet searchers are significantly more likely than other unemployed workers to be employed when observed one year later. However, we find that this difference is entirely accounted for by differences in observable characteristics, such as age and education. When our

analysis is extended to account for gaps in the measured unemployment durations and length-biased sampling, our estimates suggest a *counterproductive* effect of internet job search on unemployment durations. While an instrumental-variables technique attributes this apparent counterproductive effect to negative selection on unobservables, none of our estimates show a statistically significant, beneficial effect of internet job search on the length of unemployment spells.

The final essay is concerned with the effect of Sunday shopping deregulation on the employment level and average weekly hours of work in the retail trade industry. A differences-in-differences approach to inferring causation is possible because in Canada the jurisdiction for determining the legality of Sunday shopping lies at the provincial level and at different times between 1980 and 1998 all provinces in some way relaxed their restrictions. A major complication of this analysis is to first determine whether the provincial deregulation dates correspond to significant increases in Sunday store openings. This essay uses a unique trading-day regression technique to identify these provinces and then uses data from these provinces to estimate two different differences-in-differences specifications: (i) a fixed effects specification using data from the wholesale trade industry; and (ii) a structural specification based on a simple dynamic labour demand model that allows employment and hours to be imperfect substitutes in production. Just as in the first specification, which is more widely recognized as the differences-in-differences approach, the second

specification controls for common unexplained movements in the employment and hours data between provinces.

The results suggest that deregulation led to a long-run increase in labour demand that was disproportionately satisfied through an increase in the employment level. There is however no evidence that these new hires were any more likely to be hired on a part-time basis than their existing counterparts. Comparison of the estimates between the two data sets used suggests that the employment and hours gains were larger among general merchandise stores than among more specialized retail establishments. In addition, despite evidence of an immediate shortfall in the employment level below the long run optimal level, the results suggest that firms were unable to compensate by temporarily increasing the hours of their existing employees. Although there is no direct evidence on the hours of store owners and managers, the evidence is consistent with retail employers temporarily working Sunday shifts until new employees with weak preferences for Sunday leisure can be recruited.

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CHAPTER ONE

ARE TODAY'S EMPLOYEES LESS LOYAL? EXPLAINING THE INCREASE IN ON-THE-JOB SEARCH[1]

1. Introduction

There is a popular perception that today's workers are less inhibited by a sense of company commitment or loyalty than were their counterparts of the 1960s and 1970s. In the language of Albert Hirschman, the belief is that workers are increasingly likely to respond to job dissatisfaction with exit instead of voice or loyalty.[2] This trend has been attributed to at least two economic developments. First, workers perceive that the culture of lifetime jobs and joint employer-employee commitment was sacrificed during the recessions and widely reported corporate downsizing of the early 1980s and 1990s. Second, increasing education levels suggest that the relative importance of general human capital, as opposed to firm specific skills, may have grown significantly over the past three decades. In both cases, today's workers in their twenties and thirties are seen as behaving more like "free agents" than "company men," identifying themselves by their

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[1] The research and analysis are based, in part, on data from Statistics Canada and the opinions expressed do not represent the views of Statistics Canada. I would like to thank Deborah Sunter and Jacques Ouellet for facilitating this access. I would also like to thank John Baldwin for providing unpublished data from the Annual Survey of Manufacturers and José Campa for providing the industry trade data. This paper has also benefited from valuable comments from Peter Kuhn, Lonnie Magee, John Burbidge, and seminar participants at the University of California - Santa Barbara, McGill University, Simon Fraser University, University of Alberta, Carleton University, and Windsor University.

[2] See Albert Hirschman (1971), *Exit, Voice and Loyalty*, Cambridge, Mass.: Harvard University Press.

portable skills instead of the companies they work for.[3]

Despite the pervasiveness of these perceptions in both the popular press and human resources literature, the economics literature has struggled to find evidence of long-term changes in workers' job attachment. This is true whether it has focused on changes in job tenure distributions or directly on the incidence of either voluntary or involuntary job separations. In a recent edited volume examining changes in the employment relationship during the 1990s, Neumark (2000) concludes that although there is some evidence of weakened employer-employee bonds during the 1990s, the magnitude of these changes has not been large enough to infer long-term trends. Similarly, the Canadian evidence using the regular job tenure data in the Labour Force Survey (LFS) between 1976 and 1995 does not suggest a general increase in job instability for workers across the tenure distribution. Instead, there appears to have been a hollowing out of the middle of the tenure distribution with increased probabilities of both relatively short and long-term jobs (Green and Riddell 1996, Heisz 1996).

This paper provides new evidence on a long-term change in employee loyalty to firms by focusing on what is perhaps a more robust indicator of a trend – the percentage of employed workers looking for other jobs. Using data from the Current Population Survey (CPS), Panel Study of Income Dynamics (PSID), and two cohorts of the National Longitudinal Survey (NLS), I construct on-the-job search (OJS) rates for wage and salary workers between 1976 and 1995. The

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[3] This perception was recently expressed in *The Economist*'s "Survey of Youth", December 23, 2000.

resulting data suggests that employees were slightly more than twice as likely to be searching for other jobs in the mid-1990s than they were in the mid-1970s. Unfortunately, the U.S. time-series are patchy and suffer from small sample sizes. Given the similarity of U.S. and Canadian labor markets, this paper assumes that the Canadian experience is reflective of the U.S. and makes extensive use of the more reliable OJS data available from the Canadian Labour Force Survey (LFS). Not surprisingly, given this assumption, the LFS reveals a remarkably similar upward trend in Canadian OJS rates over this twenty-year period. Decomposition analysis suggests that an important part of the trend in both countries cannot be explained by compositional shifts, including cohort effects. Rather, the increases in OJS appear to be true period effects experienced by all types of workers. These period effects seem to have occurred independently of rising job insecurity due to sector-specific demand shocks and concomitant increases in the dispersion of log wage residuals. From consideration of changes in employer-to-employer transition rates and the resulting wage returns to job changing over the period, the data appear most consistent with a long-term decrease in search costs.

There now exist three separate economic literatures concerned with OJS. The first is a surprisingly small empirical literature on the incidence and determinants of OJS. In the U.S. this includes a study by Rosenfeld (1977) using a special supplement to the CPS in May 1976, Black (1981) using a sample of household heads drawn from the 1972 PSID, and Meisenheimer and Ilg (2000) who use the contingent worker supplements to the CPS asked in February 1995,

1997 and 1999. For the U.K. it includes Pissarides and Wadsworth (1994), who use the 1984 British Labour Force Survey. The Canadian LFS microdata on OJS used in this paper have not been examined elsewhere.

There is also an empirical literature concerned with the relative effectiveness of employed and unemployed job search. Blau and Robins (1990) and Holzer (1987) estimate reduced-form models of job search. An important difficulty in these studies is determining how much of the estimated differences are driven by individual unobserved heterogeneity in search abilities. In an attempt to correct for this heterogeneity, Jones and Kuhn (1996) focus on a sample of workers receiving different amounts of advance notice of layoff. Similarly, Burgess and Low (1992) examine whether preunemployment search intensity is increased by advance notice of layoff and Gottschalk and Maloney (1985) consider whether the choice of employed and unemployed job search affects job-finding probabilities for workers receiving advance notice. Finally, Belzil (1996) estimates a representative agent model of job search.

The third literature is the theoretical literature that has tried to incorporate OJS into models of unemployed job search. The challenge in this area has been to generate models with endogenous search strategies and both employed and unemployed job search in equilibrium. If one of these search strategies dominates in generating more wage offers or more favorable offers, such an equilibrium will obviously not exist. The original advances in this literature are the partial equilibrium models by Burdett (1978), Jovanovic (1979) and Mortensen (1986)

that assume not all firms offer the same wage in any given period and both employed and unemployed workers are able to draw wages from the same nondegenerate wage distribution that describes these offers. The problem with this approach is that it ignores the demand side of labour markets. Burdett and Mortensen (1980) and Pissarides (1994) offer two quite different approaches to modeling search equilibrium with OJS.

The remainder of the paper is organized as follows. Section 2 discusses the data and trends in U.S. and Canadian OJS. Section 3 examines the role of compositional effects, including higher average educational achievement, increased incidence of contingent jobs, and cohort effects. In sections 4 and 5 industry-level analyses are used to explore the importance of sectoral reallocation trends and rising residual wage inequality respectively. Finally, in order to reconcile the results with the existing literature on trends in job instability and to provide some evidence on the relative importance of declining search costs, section 6 considers concomitant trends in job-to-transition rates and the resulting returns to job changing. The results are summarized in section 7.

2. Data

The data used to construct OJS time-series for the U.S. and Canada are presented in Table 1. Evidence of an upward trend in the U.S. comes from five different series. First, in every year from 1969 to 1975 the PSID asked all employed household heads: "Have you been thinking about getting a new job, or will you keep the job you have now?" This question was dropped from the 1976,

1977 and 1978 surveys, but was then reintroduced in 1979 and kept until 1987 with *identical* wording and placement in the question ordering of the employment section of the PSID survey as it had been in 1975. There is therefore no reason to believe that the 1969-1975 and 1979-1987 are not comparable. In addition, in every year from 1979 to 1987 the PSID asked all respondents that were thinking about getting a new job the follow-up question: "Have you been doing anything in particular about it?" Responses to these two questions provide information on trends in OJS through the 1970s and first half of the 1980s.

In 1988 two important changes were made that affected the OJS information contained in the PSID. First, the two-part question was dropped in favor of the single question with a reference period: "Have you been looking for another job during the past 4 weeks?" Second, the sample was extended to include the wives of household heads. Although the latter change has greatly enhanced the value of the OJS data in the PSID by substantially raising sample sizes, the former served to break the continuous 1969-1987 series. Fortunately, in both May 1976 and May 1977 the CPS contained special supplements that asked all employed respondents with at least 4 weeks job tenure an essentially *identical* question to the new PSID question: "During the past 4 weeks, have you looked for another job?" To the extent that the PSID sample has maintained its national representativeness since its creation in 1968, a common sample of household heads and wives can be taken from the CPS to produce comparable OJS rates for 1976-1977 and post-1987. Using the CPS estimates as benchmarks, Fitzgerald,

Gottschalk and Moffitt (1998) find that despite a 50 percent sample loss from the original 1968 PSID sample, there is no strong evidence that sample attrition has distorted the representativeness of the original PSID sample through 1989. The CPS and PSID data should therefore provide some additional evidence on trends in OJS over the past two decades.

Selected annual surveys from two cohorts of the NLS also contain OJS questions with a four-week reference period. The 1984 survey of the National Longitudinal Survey of Youth (NLSY) asked all employed workers: "Have you been looking for other work in the last 4 weeks?" Similarly, the 1985 and 1987 Surveys of Work Experience of Young Women (NLSYW) asked employed workers: "Have you been looking for other work during the past 4 weeks?" Again, comparable samples are taken from the 1976-1977 CPS and compared to the OJS rates from these NLS samples from the mid-1980s.[4] However, unlike the PSID comparison, we should expect the NLS rates to exceed those based on the CPS question, ceteris paribus, since the former questions refer to "other work" while the latter ask about "another job." Arguably, workers looking for *additional*, as opposed to *new*, jobs are more likely to respond affirmatively to the former. Due to this contrast in question wording the OJS data from the NLS are not used in the subsequent analysis.

The resulting OJS rates for the U.S. are plotted in Figure 1. I use the entire

[4] The NLSY actually included the OJS question in every year between 1979 and 1984. However, this is a longitudinal data set of a particular cohort born between 1957 and 1965, so changes in the incidence of OJS over these six samples will include age effects in addition to any time-series trends

PSID and NLSY samples, including the low-income supplemental samples, and the CPS, PSID, and NLS weights throughout. I do however exclude the military supplemental sample from the NLSY since there is no comparable sample in the CPS. All rates are based on the sample of employed workers aged 16 and over excluding workers on temporary layoff. The search decision problem is likely to be very different for the self-employed, so the analysis also limits the sample from the outset to wage and salary workers. Finally, where the question specifies a four-week reference period, the sample is restricted to employees with at least four weeks of job tenure to insure the question is not capturing unemployed search activity. Despite the use of these substantially different data sources, all the series suggest the incidence of OJS roughly doubled between the mid-1970s and mid-1980s. The percentage "thinking about getting a new job" increased from about 12 in 1975 to 24 in 1985, while the percentage "looking for another job" went from about 4 to 8 between 1976 and 1988. As expected, responses to the "looked for other work" questions in the NLS seem to overstate the OJS increase found using the original two-part PSID question that refers specifically to search for new jobs. Taken together Panels 1 and 2 provide assurance that the increase in OJS reflects search for replacement jobs that involved actual search effort. It is also worth noting that the upward trend appears to predate the culture of downsizing that is typically linked to the recessions of the early 1980s and 1990s. This raises doubt about the role of corporate responses to these economic downturns in undermining company loyalty.

While the above evidence is clearly suggestive, these U.S. series are imperfect in terms of their breaks and the relatively small PSID and NLS samples. The Canadian series presented in Figure 2 is a substantial improvement over the U.S. in both respects. Given the similarity of the two economies, evidence of an upward trend in Canada of equal magnitude should go far in relieving any suspicions of the U.S. evidence. Like the CPS, the LFS is a monthly nationally representative survey of the Canadian population providing sample sizes that exceed 25,000 in every month between 1976 and 1995 (mean sample sizes for all series are shown in Table 1). Unlike the CPS, the basic monthly LFS contained a question on OJS that was asked in every month and remained unchanged throughout this twenty-year period. Furthermore, the OJS question was asked of *all* employed respondents with positive hours of work during the reference week. This study is the first to explore these data.[5] Using the March files of the LFS and the common sample of employed wage and salary workers, aged 16 and over, with at least 1 month tenure, these data reveal a remarkably persistent secular upward trend in Canadian OJS. Moreover, the Canadian series also suggest OJS rates approximately doubled between the mid-1970s and mid-1980s. Specifically, the rate increased from just over 2 percent in 1976 to slightly more than 4 percent in 1985. Although the rate continued to increase through the latter half of the 1980s and into the 1990s, there was a definite deceleration in its growth. So

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[5] Due to confidentiality concerns, the OJS variable was never included in the LFS public-use files between 1976 and 1995. Unfortunately, Statistics Canada ceased to collect *any* information on the job search activity of employed workers with the LFS redesign in 1995

between 1985 and 1995 the rate changed by a single percentage point compared to the more than two percentage point increase in the previous decade. This also accords with the U.S. evidence using the considerably smaller sample size of the PSID. It is interesting that in all comparable years U.S. employees appear to be twice as likely to be looking for other jobs as Canadians. It turns out that decomposition analysis using information on respondents' industry, occupation, part-time status and various demographic characteristics is unable to explain any of this difference. This paper will focus on explaining trends and will leave the cross-country difference to future research.

Having established common upward trends in OJS in the U.S. and Canada between the mid-1970s and mid-1990s, it is worth considering how these increases in search activity varied between some demographic groups that can be consistently defined in the U.S. and Canadian data. Table 2 presents OJS rates and percentage point changes in these rates by gender/age group.[6] Consistent with the findings of Pissarides and Wadsworth (1994) and Meisenheimer and Ilg (2000), there is very little difference in search rates between men and women. There are some large differences between young men and young women in the U.S., but these disappear if the sample is not restricted to the PSID sample of household heads and wives. All three data sources also show higher OJS rates among younger workers. This is also consistent with other research and is typically

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[6] The 1993, instead of 1995, PSID is used because the former is the most recent Final Release (Public Release II) file available. A number of variables used in the analysis here, such as industry codes, are not available in the Early Release files.

explained in one of two ways: (i) younger workers have more years to collect the benefits of job changes or (ii) the quality of job matches tends to rise over the life-cycle as workers refine their skills and become more effective in collecting and evaluating job offers. On the other hand, the differences may simply reflect cohort effects as workers born in later years have more portable skills or a different sense of company loyalty. Table 2 also reveals that increases in OJS occurred across all age groups for both men and women. Measured as percentage point changes the increases tend to decline with age, while the growth rates of the rates are rising with age. Unfortunately, with the exception of women aged 40-49 these changes are not statistically significant in the U.S. data. This reflects the small size of the 1993 PSID sample, rather than differences in the magnitude of the changes between the two countries. When the relatively large samples of the LFS are used all the changes are statistically significant at the 1% level. These results suggest that changes in the age distribution of the labor force cannot explain the trends, although they are of course entirely consistent with other compositional shifts, including cohort effects.

3. Compositional Effects

3.1. Decomposition Analysis

The simplest way to examine the role of other compositional shifts is to construct counterfactual changes in OJS rates by holding the distribution of

observable characteristics constant in either the mid-1970s or mid-1990s.[7] To the extent that the size of the estimated increases in OJS rates between two years remain similar to the actual increases in Figures 1 and 2, the role of compositional shifts can be ruled out. Differences between three different pairs of OJS rates are decomposed using this technique: (i) the 1975 and 1985 U.S. rates taken from panel 1 of Figure 1, (ii) the 1976 and 1993 U.S. rates taken from panel 3 of Figure 1, and (iii) the 1976 and 1995 Canadian rates taken from Figure 2. Since each data source provides different demographic and labour market information, performing the decomposition in three ways improves our ability to explain the trends.

The first stage in performing these decompositions involves estimating probit models that predict either the early or late year OJS rate separately for each of the three series. The results from using the early year cross-sections are presented in Table 3. Data from both countries show younger, more educated, part-time employees are significantly more likely to be searching for another job.[8] In addition, both the CPS and LFS suggest married women are significantly less likely to report OJS than single men. Some other noteworthy results are obtained from variables unique to one of the data sources. The PSID contains a series of questions on annual hours preferences. The results in column 1 reveal that

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[7] This general technique for decomposing predicted changes has been credited to Oaxaca (1973) and Blinder (1973), although they never used it in the context of a binary dependent variable model. For examples of decompositions in a probit model context see Even and MacPherson (1993) and Doiron and Riddell (1994).

[8] Note the LFS defines part-time work as usual weekly hours less than 30, as opposed to less than 35 in the CPS. Where the involuntary/voluntary distinction is made I am forced to use the LFS definition. Otherwise part-time rates throughout the paper are defined as less than 35 hours per week.

employees with preferences for additional hours are significantly more likely to be looking for a new job than workers that are content with their current hours. This is consistent with the Altonji and Paxson (1988) finding that underemployed workers are relatively more likely to quit their jobs unless compensated for their short hours. Using information on reason for part-time employment the Canadian data also implies much higher probabilities of OJS for employees that feel under-worked. However, the PSID data suggest that demographically comparable workers that feel overworked are not significantly more likely to be searching for a new job. Perhaps surprisingly, workers paid on an hourly basis are significantly more likely to be searching, while union members appear, if anything, only marginally less likely to search. The former result probably reflects, at least in part, the strong tenure effects reported in column 3 that are not available from the U.S. data. These tenure effects are a well established result that Pissarides and Wadsworth (1994) attribute to low tenure workers having not yet ascertained the non-pecuniary characteristics of their jobs and having invested less in job-specific skills.

Having estimated probit models using each of the early year cross-sections, it is possible to predict OJS probabilities for each observation in these samples. The average predicted probability of OJS for year t, $\bar{p}_t$, is then simply:

$$\bar{p}_t = \frac{1}{N_t} \sum_i \Phi(X_{it} \beta_t) \qquad (1)$$

where N_t is the year t sample size, Φ is the normal cumulative density function, X_{it} is a vector of independent variables (all dummy variables) for observation i in year t, and β_t is the estimated coefficient vector from the probit model using the year t data. Since $\bar{p}_t$ equals the actual OJS rate in year t, the actual increase in OJS rates is $(\bar{p}_l - \bar{p}_e)$ where l and e index the late and early year respectively. This change can be decomposed into a part due to changes in characteristics, X_{it}, and a part due to changes in the returns to those characteristics, β_t. This is done by predicting the counterfactual rate:

$$\bar{p}_l^0 = \frac{1}{N_l} \sum_i \Phi(X_{il}\beta_e) \tag{2}$$

and decomposing the increase in rates between the early and later year:

$$\bar{p}_l - \bar{p}_e = (\bar{p}_l - \bar{p}_l^0) + (\bar{p}_l^0 - \bar{p}_e) \tag{3}$$

where the first and second terms are usually referred to as the unexplained and explained parts of the difference respectively. Since it is equally legitimate to base this decomposition on probit estimates using the later year cross-sections, there are of course two ways of doing this analysis.[9] The results in part A of Table 4 present estimates from both approaches. The results from the three data sources are remarkably similar. The upper bound U.S. and Canadian estimates using the mid-1970s and mid-1990s data are in fact identical. In each column the estimates

[9] With the Canadian data it is also possible to use years other than 1976 and 1995. Although not reported, separate decompositions of the 1976 rate and every year between 1977 and 1995 were performed. The results were remarkably similar between specifications, emphasizing that there is little to gain from focusing on shorter periods within the twenty-year series.

suggest that compositional shifts can, at most, explain one-third of the increase in OJS experienced in the two countries.

It is possible to further decompose the explained part of the trend into the change due to each of the independent variables. This is done by allowing only the elements of β associated with a particular variable to change when calculating the counterfactual rate. The complication here is that in a nonlinear model, like the probit, the calculated changes due to each variable will not, in general, add-up to the total explained change. In order for the effects of the individual variables to add-up, it is necessary to linearize the probit function in some way. Following Even and MacPherson (1993), the approach used here is to attribute:

$$(\bar{p}_l^0 - \bar{p}_c)_j = (\bar{p}_l^0 - \bar{p}_c) \cdot \frac{(\bar{X}_{jl} - \bar{X}_{jc})\beta_{jc}}{(\bar{X}_l - \bar{X}_c)\beta_c} \tag{4}$$

to the variable j set of dummy variables. The first term on the RHS is the total explained change, while the second term is the proportion of the total index change due to the variable j.[10]

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[10] Doiron and Riddell (1993) point out that a weakness of this technique is that it ignores the nonlinearity of the probit function. They argue that a preferred approach is to predict at the means of X_{il} and take a first order Taylor series approximation of the probit function. The explained gap can then be approximated by:

$$\Phi(\bar{X}_l \beta_c) - \Phi(\bar{X}_c \beta_c) \approx \frac{\partial \Phi(\psi)}{\partial \psi} \cdot (\bar{X}_l - \bar{X}_c) \cdot \beta_c \tag{5}$$

where the first term on the RHS is the derivative of the probit function evaluated at an arbitrary location ψ. And the contribution of variable j to this change is given by:

$$\frac{\partial \Phi(\psi)}{\partial \psi} \cdot (\bar{X}_{jl} - \bar{X}_{jc}) \cdot \beta_{jc} . \tag{6}$$

Doiron and Riddell (1994), and more recently Morrisette and Drolet (2001), present results from both techniques. However, it is unclear that there is a substantive difference. Expressed as percentage point changes, the two approaches will produce different results, but expressed as the

The results from using the late year parameter estimates are presented in part B of Table 4. Again the three data sources provide remarkably similar results. In all cases, the most important compositional change was the shift towards a more educated labor force. In all years, employees with post-high school education are significantly more likely to be searching and between the mid-1970s and mid-1990s their share increased from about 35 to 51 percent in the U.S. and 34 to 48 percent in Canada.[11] This provides some support for the hypothesis that the acquisition of more portable skills has resulted in weakened company loyalty. However, the data in columns 2 and 3 suggest that this shift can account for no more than a sixth and a third of the U.S. and Canadian trend respectively. In addition to educational changes, shifts to part-time, nonunion, service sector jobs appear to have played a modest role in raising OJS.[12] Perhaps surprisingly, shifts in the tenure distribution appear to have reduced, not increased, search activity among employed workers. The explanation is that there was a bifurcation of the tenure distribution but, at least when the tenure distribution is truncated at 1 month, the increase in the probability of being in a long job dominates the

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percentage of the total explained change due to each variable j, they produce *identical* estimates The reason is that the derivative of the probit function is a constant so that (6) divided by the RHS of (5) produces exactly the second term on the RHS of (4). An alternative and simpler approach, taken by Morrisette and Drolet (2001) and Hamermesh (2001), that produces different results is to linearize by estimating a linear probability model The results in part B of Table 4 are not substantively different from those obtained from a linear probability model.

[11] In January 1989 the LFS coding of the education variable changed from one based on years completed to highest level attained. The consequences of this change are discussed extensively in Bar-Or et al. (1995) and I follow their approach to coding.

[12] Shifts to part-time work are considerably more important in explaining the Canadian data The reason is simply that the U.S. has not experienced a comparable increase in part-time rates over this period. Similarly, decreasing union density rates is important in explaining the U S increase in OJS, but not the Canadian Again, the reason is simply that Canada did not experience a similar reduction in union density over this period.

increased probability of being in a short job. Finally, both U.S. data sources imply that shifts in the age distribution and marriage rate served to increase OJS rates. These results are a consequence of the sample restriction to household heads and household heads and wives. By excluding the single dependents, whom have low OJS probabilities relative to single heads, the estimated marital effect becomes negative and significant. This combined with an increase in the single share gives us the estimated effects. The unexpected age effect is more complicated but reflects estimated age effects that decline less smoothly than when the sample is not restricted to household heads and wives.

3.2. Contingent Jobs

If shifts to a more educated labor force and more part-time, nonunion, service sector jobs can explain only a third of the upward trends in OJS, then what explains the residual trend? A weakness of the decomposition analysis above is that it cannot account for compositional shifts that are unobserved or OJS effects that are not measured. Of particular concern is the absence of information on contingent work status in the data. There is a widespread belief and some limited evidence that the incidence of temporary and contract jobs has increased significantly over the past two decades (see Polivka (1996) and Segal and Sullivan (1997)). Since workers in contingent jobs are more likely to anticipate their terminations, we expect them to exhibit significantly higher OJS probabilities than similar workers in less flexible work arrangements. Since

contingent worker rates may be only weakly correlated with the industry, occupation, weekly hours and demographic variables observed, the Section 3 estimation will attribute too little to compositional shifts. We do however expect the incidence of contingent work to be highly correlated with the tenure data available in the LFS. This is confirmed by estimates from the Canadian Survey of Work Arrangements, a special supplement to the LFS in December 1995. Among employees with less than one year of job tenure, slightly more than 30 percent claimed to be in a job that was in some respect not permanent. The comparable rate for workers with more than ten years tenure is less than 2 percent. Changes in OJS rates among high tenure workers should therefore shed some light on importance of shifts to contingent work.

Table 5 presents OJS rates for a group of workers we expect to have strong job attachment – high tenure men with usual weekly hours of 40 or more. In order to boost the annual sample sizes the twenty March LFS files between 1976 and 1995 are pooled into four periods. The estimates reveal increasing OJS across the tenure distribution. Although the percentage point changes are monotonically decreasing in tenure, measured as proportions of the first-period rates these changes are considerably greater among the high tenure employees. Assuming a simple binomial distribution with variance given by $p(1-p)/N$, these increases are all highly statistically significant. It is telling that even men with more than 10 years job tenure were more than twice as likely to be looking for a different job in the mid-1990s than in the mid-1970s. Clearly, an important part of

the upward trend in OJS is not explained by shifts to contingent work. More generally, distributional shifts to other types of low-quality jobs with high turnover rates, whether explicitly fixed-term or not, are unable to explain the residual trend.

3.3. Cohort effects

It is still possible that no individual worker's commitment fell. Although there is only weak evidence of cohort effects based on workers' perceptions of corporate commitment to employees and higher educational attainment, they may still exist for different reasons. Perhaps workers born in the liberal political culture of the 1960s acquired a different sense of company loyalty than workers born before 1940. Table 6 presents OJS rates for four different cohorts in the U.S.. In order to raise sample sizes, the two CPS samples from the 1970s and the six PSID samples from 1988-1993 are pooled separately. The first feature to note from Table 6 is that the OJS rates are monotonically increasing in cohort. So even without any change in individual worker behavior we should find increasing OJS rates when we compare cross-sections from the 1970s and 1990s. What happens to the trends when we look within cohorts? Consistent with a cohort effect explanation, the large increases in OJS seem to disappear. With the exception of the youngest cohort, all the period-to-period changes are statistically insignificant

despite all the cell samples exceeding 3,500.[13] These results come across even more convincingly in the LFS data. Figure 3 plots the Canadian OJS series for the same four cohorts. Again, OJS rates are increasing in year of birth and the upward trends disappear when we look within cohorts. With the exception of a modest increase in the early period for the youngest cohort, the within-cohort profiles are remarkably flat relative to the total trend in Figure 2. On the surface, these results seem to suggest that cohort effects can account for all of the upward trends in OJS.

The problem is, of course, that these results follow directly from the two main results in Table 2 - OJS rates are increasing over time and decreasing in age. With year and age effects of equal magnitude, within-cohort profiles must be flat. It is well known that since the year, age and cohort variables are linearly dependent (cohort = year – age), it is impossible to correctly identify all three without knowing something about the form of at least one of them. Fortunately, there is good reason to believe that independent negative age effects exist. In the context of the standard search model with OJS (see Burdett (1978) or Mortensen (1986)), if employed workers are able to draw wages from a nondegenerate wage distribution in every period of their lifetimes, the probability of drawing a wage that exceeds their current wage, their best draw to date, must decrease as the number of draws or periods increase. The probability that optimizing agents will

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[13] Note that in the period 1976-1977 the youngest cohort group includes only 16 and 17 year-olds so these sample are relatively small in both the U.S and Canadian data This explains the different patterns for this cohort

absorb the costs of search must therefore decline as the number of periods, or their age, increases. Further, where agents are finite-lived the expected benefits of OJS will fall simply because the present value of any wage improving draw declines with age. If we accept that independent negative wage effects exist, we should then expect the within-cohort profiles in Figure 3 to be declining. What explains the fact that these profiles tend to be flat? Clearly, there must have been period effects of equal and opposite magnitude. Since we believe the age effects must be quite large, the period effects must similarly be important. This suggests that compositional shifts, including popular perceptions of cohort effects, cannot explain an important part of the upward trends in OJS rates. Rather, the residual trend appears to be true period effects, experienced by workers across the age and cohort distributions.

4. Sectoral Reallocation

In an effort to determine the role of two well-established labour market trends that are plausibly related to the secular trend in OJS, in this section and the next I exploit between-industry variation in the growth of OJS. In this section the role of sectoral reallocation trends is examined with particular interest in the impact of the deindustrialization process experienced in the U.S. and Canada through the 1980s and 1990s. As argued by Lilien (1982) in his explanation of cyclical unemployment, shifts of employment demand between sectors of the economy necessitates job reallocation and therefore variation in the incidence of

job search. Following this idea, if workers in declining industries anticipate their layoffs and workers employed in expanding industries realize new job opportunities, we might expect within-industry employment changes over a period to be correlated with the level of OJS. More formally, the probability that individual i employed in industry j searches for a new job in period t might be determined by:

$$\Pr(OJS_{ijt}) = f\left(X_i, \left|\Delta E_{jt}\right|\right) \tag{7}$$

where X_i is a vector of person-specific characteristics, E_{jt} is the employment level in industry j in period t, and $f_2 > 0$. This implies that:

$$\Delta \Pr(OJS_{ijt}) = g\left(\left|\Delta E_{j,t+1}\right| - \left|\Delta E_{j,t}\right|\right) \tag{8}$$

assuming the terms in (7) are additively separable. Evidence on the role of sectoral reallocation can then be obtained by considering whether there is a positive correlation between within-industry changes in OJS and within-industry differences-in-differences employment changes.

A complication in this analysis is knowing how far into the past workers look when making search decisions based on employment level changes. Ideally a number of different time horizons could be explored, but unfortunately the analysis is limited by the available OJS data. Using the Canadian LFS data, which offer the more complete OJS time-series, I construct absolute annual employment level changes at the two-digit industry level from March-to-March of every year between 1976 and 1995. To the extent that (7) explains OJS behaviour, these employment level changes should be correlated with OJS rates observed in the

March LFS files of each year. A consequence of the level of industrial detail is that some of the cell sizes, particularly among the manufacturing industries, are worryingly small. In response I focus on mean OJS rates and employment changes within four five-year periods: 1976-1980, 1981-1985, 1986-1990 and 1991-1995. As long as f_{22} is close to 0 over the range of employment changes in the data, this aggregation is innocuous. In addition, in all cases the OLS estimates of the correlations are supplemented with estimates from a weighted least squares (WLS) procedure, which serves to weight observations according to the size of the samples used to generate the industry-specific rates. The details of this estimation are given in Appendix B.

Before examining the correlations, it is worth comparing the changes in OJS experienced within Canadian industries over the four five-year periods. These rates for 45 two-digit industries are presented in Table A1. At least two noteworthy results emerge from these data. First, the OJS increases are remarkably pervasive, as no industry appears to have experienced a net decline in search activity between the late-1970s and early-1990s. Second, the increases are on average larger for service-producing industries than goods-producing industries. Given the shift away from manufacturing employment over this period, this casts some doubt on the role of heightened job insecurity in raising OJS.

Figure 4 presents the correlations between changes in OJS and differences-in-differences employment changes. Given the contrasting employment experiences of the manufacturing and service sectors over this

period, the panels of this figure show this relation separately for goods and service-producing industries. Clearly the results do not support the hypothesis that sectors experiencing relatively large increases in employment instability should see the largest increases in OJS. This casts doubt on the importance of sectoral reallocation trends in explaining the rising OJS rates. However, perhaps the search decision in (7) is based on lagged values OJS_{ijt} and longer-term employment changes. We should then see a u-shaped relationship between first-difference employment changes and increasing OJS rates. Figure 5 considers this correlation using both the Canadian and U.S. data. In order to make the Canadian results comparable to the U.S., I focus on the period from the mid-1970s to the late-1980s when aggregate OJS rates appear to have roughly doubled. The OJS rates for 37 two-digit industries in the U.S. are shown in Table A2. Similar to the Canadian within-industry OJS changes, these data reveal remarkably pervasive increases that are on average larger among service than goods-producing industries. When plotted against first-difference employment changes in Figure 5, there now appears to be some role for sector-specific demand shocks in explaining the OJS increases. Although the results for goods-producing industries are all statistically insignificant, the service-producing estimates from both countries now suggest a marginally significant positive correlation. Arguably, since there are relatively few industries that experienced employment contractions over this period, it is not surprising that a negative correlation does not appear.[14]

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[14] As plots in Figure 5 suggest, pooling the goods and service-producing industries and adding a

However, closer scrutiny of these results reconfirms our doubt of the role of sectoral reallocation trends. First, despite the positive correlations in Figure 5, the estimated relationships imply rising OJS even where employment levels are stable. As Figure 6 reveals, much of the positive correlation that is observed can, at least for Canada, be explained by increasing part-time and contingent employment rates in the service sector. Second, despite experiencing widespread negative demand shocks through the recession of the early 1990s, data from the Canadian manufacturing sector over this period also does not suggest a role for rising job insecurity. The first three panels of Figure 7 consider changes in production worker employment levels using the Annual Survey of Manufacturers (ASM). The annual employment loss rates, plotted in the third panel, are calculated for each industry as the loss in employment from all plant contractions and closings between two years as a function of the total employment in the first year.[15] Regardless of the measure used, both the OLS and WLS estimates are now statistically insignificant despite all the industries, except rubber and plastics, experiencing negative employment changes over this period. Further, changes in the mean production worker wage appear to be, if anything, *positively* correlated with OJS increases. Finally, the fifth and sixth panels of Figure 7 consider the level and changes in the ratio of imports to domestic consumption by industry.[16]

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quadratic term produces no evidence of a negative correlation over the range of negative log employment changes.

[15] These data were generously provided by John Baldwin from the Micro-Economics Analysis Division at Statistics Canada.

[16] These ratios are constructed using the import data by commodity in the System of National

Again, regardless of the measure used the estimates are highly insignificant. There is clearly no evidence that heightened job insecurity due to increasing import competition or any other negative demand shocks served to raise OJS. Taken together these results suggest that something other than sectoral reallocation trends is responsible for the observed change in search behaviour among U.S. and Canadian employees.

5. Firm Wage Effects

An alternative explanation of the period effects found in Section 3 comes from consideration of within-industry earnings inequality. Consider the log earnings equation:

$$\log w_{ijt} = X_{ijt}\,\beta_{jt} + u_{ijt} \qquad (5)$$

where w_{ijt} is the weekly earnings of individual i, in industry j, in year t, X_{ijt} is a set of observable characteristics, including human capital characteristics, and u_{ijt} is an iid random error term. In addition to weekly hours of work and the unobserved ability of individual i, u_{ijt} should capture firm wage effects. These include noncompetitive factors, such as unionization, monopoly status in the labor market and discrimination, efficiency wages, and varying returns to unobserved ability between firms. Regardless of the source of these firm wage effects, it is conceivable that a change in their distribution within an industry results in increased OJS as workers seek wage improvements through within-industry job

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Accounts and the assumption that an industry's lost production due to trade is equivalent to its domestic production share of each imported commodity.

changes. As evidence that firm wage effects are an important determinant of OJS behaviour, Bhaskar et al. (2002) show that workers in higher-paid jobs are less likely to search for other jobs and search less intensively when they do than otherwise similar workers in lower-paid jobs. Increased dispersion of industry-level weekly hours could also serve to raise search activity, although this is likely to be less important given the focus on full-time workers. Yet, in either case OJS trends driven by firm wage effects should be captured by changes in the distribution of u_{ijt}. In contrast, changes in the distribution of unobserved ability, due to more unequal schooling quality for example, should not raise OJS, although it may affect the distribution of u_{ijt}. Although we cannot separately identify the firm and hours effects component of u_{ijt} from the unobserved ability component, evidence of a positive correlation between within-industry residual wage inequality and within-industry increases in OJS is certainly suggestive of firm wage effects.

Using the March files of the CPS from 1977-1989 and the Survey of Consumer Finance (SCF) individual files from 1981 to 1996, I estimate equation (5) separately for 36 and 13 industries in the U.S. and Canada respectively.[17] Both of these surveys provide retrospective information on work activity and earnings in the previous calendar year, including total earnings from wages and salaries and total weeks worked. These data are used, along with CPI indexes from each

[17] Unfortunately, the most detailed industry code available in the Survey of Consumer Finances identifies only 13
industries

country, to construct real weekly earnings.[18] The samples are restricted in all cases to full-time, full-year workers aged 16 to 65 with positive wage and salary earnings in the reference year. In an effort to avoid self-employment income as much as possible, I drop the agricultural industry and limit the U.S. data to workers whose longest job in the reference year was a wage and salary job and the Canadian data to workers whose major source of income was wages and salaries.[19] The vector of observable characteristics includes information on employees' education, labor market experience, region/province, sex, marital status, urban/rural residence, and occupation.[20] In addition, in the U.S. it includes dummies for black and Hispanic and in Canada for immigrant status and French as a mother tongue. Finally, in order to avoid complications that result from outliers and the topcoding of the earnings data in the CPS, equation (5) is estimated by median regression (i.e. least absolute deviations) and the 90th minus 10th percentile of the absolute residuals is used as a measure of within-industry dispersion of firm wage effects.[21]

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[18] The U.S. CPI series are annual, seasonally unadjusted, all items, base year 1982-1984. The Canadian series are annual, seasonally unadjusted, all items, base year 1992

[19] It turns out the results are quite sensitive to the inclusion of the agricultural industry. In particular, in both countries the agricultural industry experienced an unusually large reduction in the dispersion of firm wage effects (note that with the exception of the personal services industry in the U.S., the agricultural industry had the highest average level of firm wage effects dispersion in both countries over the periods examined). Coupled with very mild OJS increases in both countries, the inclusion of this industry suggests a weak positive correlation between changes in the dispersion of firm wage effects and increases in OJS. As Figure 9 reveals, the relationship is negative when agriculture is excluded. Given the difficulty of interpreting firm wage effects in this industry, the decision was made to exclude agriculture

[20] Three occupation dummies are constructed in both the U.S. and Canadian data: (i)white-collar (managerial-administrative-professional), (ii) pink-collar (clerical-sales-service); and (iii) blue-collar.

[21] Alternative measures have been estimated using the SCF data, which has earnings data that are not topcoded. These include taking the variance of the residual from an OLS regression and taking

The aggregate U.S. and Canadian trends in these measures of dispersion in firm wage effects are plotted in Figure 8. They indicate remarkably similar magnitudes and upward trends during the early 1980s. Further, the U.S. data reveal that the trend predates the recessions of the early 1980s, which is hopeful for attempts to explain the OJS trends in Figures 1 and 2. However, these trends appear to diverge in the mid-1980s as the Canadian residual inequality seems to fall during the second-half of the 1980s. The absence of an upward trend in Canada through the 1980s raises some doubt about the ability of firm wage effects to explain the data.

Before attempting to explain the trends in OJS, the two panels on the left hand-side of Figure 9 correlate the average within-industry level of OJS with the average level of within-industry residual wage dispersion over the period of interest. The U.S. and Canadian results are again remarkably similar, both suggesting an important role for firm wage effects in explaining OJS behaviour. Clearly, employees in industries with higher levels of residual wage inequality, such as personal services in both countries, are more likely to search for new jobs than employees in industries with lower levels of residual wage inequality, such as the durable manufacturing industries in each country. Since it appears that OJS decisions are, at least in part, motivated by firm wage effects, it seems reasonable to expect upward trends in residual wage inequality to explain the observed increase in OJS. The two right-hand panels of Figure 9 plot this correlation.

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the variance, mean and median of the absolute residual from a median regression. In all cases, the main results are unaffected by the choice of measure.

Although the sign of the point estimates from the two countries contrast, all the estimates are statistically insignificant. It seems that increased OJS has occurred even where the dispersion of firm wage effects has been declining. This suggests that firm wage effects are also unable to account for the widespread increases in OJS documented in Section 3.

6. Employer-to-Employer Transitions

The finding that heightened OJS activity appears to have occurred independently of broader labour market trends that should have changed the relative benefits of changing jobs, raises the important question of whether increased OJS has actually resulted in more employer-to-employer transitions. It is of course possible that employees are becoming increasingly fastidious about the jobs they accept at the same time as they search more. Although I am aware of no theoretical model with such a prediction, it seems plausible that falling search costs could raise the incidence of search *and* reservation wages so that OJS rates increase with little or no change in employer-to-employer transition rates.[22] Indeed, as noted above, the large literature concerned with changes in job instability over the past two decades has struggled to find evidence of long-term trends. The problem is that, with only two exceptions, this literature has focused

[22] The reason that this theoretical prediction does not exist is that it requires a model with both a search decision (as in Burdett (1978)) and a job acceptance decision (as in Hey and McKenna (1979)). The theoretical OJS literature has focused on either search or mobility costs, so that a model with both decisions does not exist. In the absence of search costs optimizing agents will always choose to search and in the absence of mobility costs the reservation wage is always equal to the current wage and therefore independent of search costs.

exclusively on tenure distributions and overall job separation rates.[23] Interestingly, both papers that estimate employer-to-employer transition rates do find evidence of increasing instability. First, using the PSID from 1981 to 1992, Gottschalk and Moffitt (2000) compute separation rates conditional on exit destination and find some evidence of increasing transitions to non-self-employment through the 1980s. Second, using the March CPS from 1976 through 2001, Stewart (2002) identifies a dramatic long-term increase in employment-to-employment transition rates (job changes with two or fewer weeks of intervening unemployment) of about 50 percent. Interestingly, both papers also find similar increases for men and women and an offsetting decline in transitions to non-employment. The former result is consistent with OJS trends, while the latter reconciles these results with the extensive evidence of long-term stability in overall tenure distributions and separation rates.

Evidence of an upward trend in employer-to-employer transition rates and information on how the wage returns to job switching have changed provides useful insights into the cause of increased OJS. Unfortunately, neither the PSID nor CPS can be used to construct these data between the mid-1970s and late-1980s when OJS rates roughly doubled.[24] Following Monks and Pizer (1998) and

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[23] See Table 1.1 in Neumark (2000) for a useful summary of the measures and findings of this literature

[24] The PSID depends on tenure data to identify job changes, but the tenure questions have changed over time making rates from the 1980s incomparable to those from the 1970s (see Polsky (1999) for a summary of these changes). The March CPS, on the other hand, depends on retrospective questions about job changes in the past year. Wages on the old job are not observed and would probably be subject to tremendous measurement error if respondents were asked to recall these wages

Bernhardt et al. (2000), who focus on two-year total job separation rates, I use two separate cohorts of the NLS to compare one-year transition rates by destination between the mid-1970s and late-1980s. This is possible because the National Longitudinal Survey of Young Men (NLSYM) and the National Longitudinal Survey of Youth (NLSY) cohorts were aged 23 to 31 in 1975 and 1988 respectively. Besides providing information on wages in both the old and new jobs, the NLS are a preferred data source because they contain unique employer identification codes, which are used to identify transitions. Brown and Light (1992) find that these employer codes are the best source of employer identification both within the NLS and when compared to other longitudinal data sets.

Figure 10 presents one-year transition rates using the sample of employed men from each cohort that are wage and salary workers in their main jobs. The poor white and military supplemental samples were excluded from the NLSY cohort, since there are no comparable supplemental samples available in the NLSYM. The weights from both cohorts are used throughout so as to produce representative samples of the 23-31 age group in each of the two years. Consistent with the results based on the PSID and CPS, the bottom-left panel of Figure 10 reveals an increase of almost 50 percent in the probability of making a job change despite a relatively small increase in the overall job separation rate (shown in the top-left panel). Again, the contrast is explained by an offsetting decline in the probability of making a transition to non-employment (top-right panel).

Interestingly, there is no indication that the increase in employer-to-employer transitions was disproportionately voluntary (bottom-right panel).

Given the potentially important compositional differences between the NLSYM and NLSY samples of young adult men, it seems important to adjust for these differences before implying a behavioural change. Table 7 presents the results from predicting the probability of experiencing an employer-to-employer transition by probit conditional on being observed in 1988, instead of 1975, and a set of characteristics whose means may have changed between these years. Regardless of the set of controls used a statistically significant increase in the incidence of making an employer change is found. As some assurance of the meaningfulness of these transitions, the estimates also suggest higher rates of employer change among younger, single workers and the wider the window between interviews. Together with the CPS and PSID results, there is strong evidence that rising OJS has resulted in more employer-to-employer transitions. However, the increase in OJS does not appear to be equivalent to the increase in job changing. This is entirely consistent with an increase in reservation wages motivated by a long-term decline in search costs.

Although the evidence in Section 5 suggests that the OJS increase was not caused by increasing dispersion of firm wage effects, it is still possible that workers have seen growth in the wage returns to job changing. The explanation based on falling search costs and rising reservation wages implies this result. Figure 11 presents kernel density estimates of the distribution of one-year real log

wage changes experienced by employer-stayers and switchers separately for the 1975 and 1988 samples. As expected there is noticeably less dispersion of wage changes among the samples of employer-stayers than switchers. Although less clear, the results also indicate a higher probability of experiencing a positive wage change among the voluntary, than involuntary, switchers. However, reconfirming the irrelevance of wages in Section 5, there is no indication of a change in either the distribution of wage returns to job changing itself or its location. Consistent with the evidence on firm wage effects, the OJS increase does not appear to have been driven by workers anticipating improved wage returns to job switching. The results are also at odds with the explanation based on falling search costs and rising reservation wages. Yet, it is still entirely possible that workers have become increasingly fastidious in response to falling search costs. As emphasized by Blau (1991) in his empirical rejection of the reservation wage property, the search decision is more accurately based on a reservation utility decision. Surely workers also value the nonwage attributes of jobs such as hours of work (Blau's focus), fringe benefits, and working conditions. This suggests reservation utilities might have been rising in response to declining search costs. Since workers' utilities are not directly observed, obtaining evidence of this behavioural change is not straightforward.

Additional support for the hypothesis of declining search costs and rising reservation utilities becomes apparent when we recognize that the OJS rates in Figures 1 and 2 are based on stock samples of employed workers. This means that

the computed rates will be sensitive to increases in both the incidence *and* duration of OJS spells. The fact that employer-to-employer transition rates appear to have increased implies that at least part of the OJS trend reflects a higher incidence of OJS spells. Longer OJS spells resulting from lower search costs and rising reservation utilities offers an explanation for why stock sample OJS rates increased more than transition rates. In contrast, explanations based on declining mobility costs, such as more portable pensions or higher marital separation rates, imply lower reservation wages and therefore shorter durations and *lower* stock sample OJS rates (see Hey and McKenna (1979) for this prediction). Improved search efficiency, modeled as an increase in the offer arrival rate, due to improved communication technology or the introduction of private employment agencies should similarly reduce search durations and therefore stock sample OJS rates. Given these contrasting effects on stock sample OJS rates, the evidence appears most consistent with a long-term decline in the cost of OJS.

Unfortunately, it is difficult to think of explanations of why search costs have fallen in such a smooth way over the period from the mid-1970s to the mid-1990s. Since there is no foregone income cost of OJS, the most obvious explanation is that the use of a particular search technology has become more affordable. Between 1976 and 1981, the LFS also asked respondents reporting OJS to report their method of search. Table 8 considers changes in OJS rates by method over these six years. Although most of the OJS trend reflects increases in direct employer contact, the rates suggest that the use of public employment

agencies, ads and other methods for OJS were also becoming more common. It is hard to believe that scanning newspaper ads became gradually less costly in the second-half of the 1970s. This suggests that something other than a change in the cost of using particular search technologies has been driving the OJS increase. Although there is no direct evidence of weakening company loyalty, the data are entirely consistent with decreasing OJS costs associated with foregoing loyalty to a current employer. These costs may be psychic, but need not be. Similar to the cost of shirking in the Shapiro and Stiglitz (1984) efficiency wage model, perhaps workers face a positive probability of being caught searching, which may be punished through lower future wage gains, loss of promotion possibilities or in the extreme case through job termination. The upward trend in OJS then implies that either the probability or the cost of being caught searching have gradually declined since the mid-1970s.

7. Summary

This paper is the first to examine long-term trends in U.S. and Canadian OJS rates. The nationally representative data examined reveal that the percentage of employed workers looking for other jobs more than doubled in both countries between the mid-1970s and mid-1990s. Analysis suggests that an important part of these trends cannot be explained by compositional effects, including cohort effects. Rather, the upward trends appear to reflect true period effects, in the sense that increased search activity occurred among all types of employees. This result

is at odds with the popular perception that today's younger generations have a weaker sense of commitment or loyalty to their employers than their parents or grandparents did. Instead, their parents' loyalty seems to have fallen as much as their own.

To determine the cause of these period effects variation in within-industry growth of OJS rates is correlated with variation in within-industry changes in employment, wages, import penetration ratios and earnings inequality. The results suggest that the period effects occurred independently of rising job insecurity due to sector-specific demand shocks and concomitant increases in the dispersion of firm wage effects. From consideration of changes in employer-to-employer transition rates and the resulting wage returns to job changing over the period, the data appear most consistent with a long-term decrease in search costs. Although there is no direct evidence of the source of the decline in search costs, the data are entirely consistent with a reduction in the psychic costs of foregoing loyalty to a current employer or in the probability or cost of being caught searching while employed.

Table 1: Data series.

Survey	Date	Sample (mean sample size)	Question
U.S.			
Panel Study of Income Dynamics (PSID)	1969-1975 and 1979-1987	Household heads that are wage and salary workers (3,023).	"Have you been thinking about getting a new job, or will you keep the job you have now?"
Panel Study of Income Dynamics (PSID)	1979-1987	Household heads that are wage and salary workers (3,153).	"Have you been doing anything in particular about it?"
Current Population Survey (CPS)	May 1976 and 1977	Wage and salary workers with tenure > 1 month (39,618).	"During the past 4 weeks, have you looked for another job?"
Panel Study of Income Dynamics (PSID)	1988-1995	Household heads and wives that are wage and salary workers with tenure > 1 month (3,999).	"Have you been looking for another job during the past 4 weeks?"
National Longitudinal Survey of Youth – (NLSY)	1984	Wage and salary workers aged 19 to 26 with tenure > 1 month (5,972).	"Have you been looking for other work in the last 4 weeks?"
National Longitudinal Survey – Young Women (NLSYW)	1985 and 1987	Women aged 33 to 41 that are wage and salary workers with tenure > 1 month (1,883).	"Have you been looking for other work during the past 4 weeks?"
Canada			
Labour Force Survey (LFS)	March 1976-1995	Wage and salary workers with tenure > 1 month (45,372).	"In the past 4 weeks, have you looked for another job?"

Table 2: On-the-job search rates by age and gender, U.S. and Canada.

	U.S.			Canada		
	1976	1993	Change	1976	1995	Change
Men						
16-19	0.161	0.222	0.061	0.046	0.109	0.063*
20-24	0.082	0.235	0.153	0.037	0.101	0.064*
25-29	0.067	0.136	0.069	0.033	0.078	0.045*
30-39	0.033	0.096	0.063	0.019	0.050	0.031*
40-49	0.021	0.086	0.065	0.010	0.030	0.020*
50 and over	0.012	0.026	0.014	0.005	0.018	0.013*
Women						
16-19	0.083	0.226	0.143	0.047	0.113	0.066*
20-24	0.082	0.158	0.076	0.043	0.112	0.069*
25-29	0.050	0.128	0.078	0.033	0.067	0.034*
30-39	0.042	0.107	0.065	0.013	0.048	0.035*
40-49	0.018	0.113	0.095**	0.012	0.038	0.026*
50 and over	0.012	0.023	0.011	0.005	0.025	0.020*

Note: The U.S. sample is household heads and wives, whereas the Canadian sample is all wage and salary workers. *,** indicate if changes are statistically significant at the 1 percent and 10 percent level respectively.
Source: May 1976 Current Population Survey and 1993 Panel Study of Income Dynamics for the U.S.. March 1976-1995 Labour Force Survey for Canada.

Table 3: Probit estimates of the probability of on-the-job search.

	1975 PSID[a]		1976 CPS[b]		1976 LFS[c]	
Age 16-19	1.503*	(0.275)	1.388*	(0.130)	0.573*	(0.176)
Age 20-24	1.115*	(0.201)	1.156*	(0.095)	0.637*	(0.172)
Age 25-29	0.819*	(0.200)	0.996*	(0.094)	0.631*	(0.172)
Age 30-39	0.625*	(0.203)	0.774*	(0.093)	0.445*	(0.171)
Age 40-49	0.499*	(0.201)	0.561*	(0.095)	0.389*	(0.176)
Age 50-59	-0.029	(0.216)	0.373*	(0.098)	0.219	(0.186)
Black	0.164	(0.108)	0.018	(0.049)		
Hispanic			-0.110	(0.078)		
Female	0.054	(0.118)	-0.012	(0.061)	-0.013	(0.060)
Married	-0.012	(0.125)	-0.116*	(0.050)	0.074	(0.062)
Female and married			-0.314*	(0.069)	-0.233*	(0.081)
Wife works	0.025	(0.088)			-0.074	(0.055)
Have children	0.033	(0.085)				
High school	0.170	(0.152)				
Some post high	0.369*	(0.158)				
College degree	0.558*	(0.182)				
High incomplete			0.114	(0.071)		
High graduate			0.088	(0.066)		
Post high school			0.301*	(0.070)		
High school					0.071	(0.071)
Some post high					0.211*	(0.085)
Post-high diploma					0.190*	(0.086)
University degree					0.291*	(0.093)
Reside in house	-0.008	(0.084)	-0.073	(0.057)		
Rent residence	0.074	(0.082)				
Union member	-0.093	(0.084)				
Prefer more hours	0.241*	(0.079)				
Prefer fewer hours	0.150	(0.150)				
Weekly hours 0-14	0.099	(0.318)				
Weekly hours 15-29	0.361*	(0.175)				
Weekly hours 30-40	0.035	(0.074)				
Part-time			0.331*	(0.044)		
Involuntary part-time					1.218*	(0.093)
Voluntary part-time					0.122*	(0.059)

Paid by the hour			0.143*	(0.034)		
Tenure 2-6 months					1.496*	(0.288)
Tenure 7-12 months					1.178*	(0.290)
Tenure 1-2 years					1.133*	(0.289)
Tenure 3-5 years					1.022*	(0.288)
Tenure 6-10 years					0.810*	(0.290)
Tenure 11-20 years					0.474	(0.302)
Constant	-1.44*	(0.625)	-2.53*	(0.233)	-4.20*	(0.350)
Pseudo R^2		0.138		0.093		0.142
Number of obs.		2,901		33,167		26,797

Note: Standard errors are in parentheses. * indicates significance at the 5% level. The regressions also include region/province dummies and industry and occupation dummies.

[a] Sample is employed household heads that are wage and salary workers. Omitted categories are "Northeast," "Age 60 and over," "Primary school," "Weekly hours 40 and over," "Construction industries," and "Farming occupations."

[b] Sample is employed household heads and wives that are wage and salary workers. Omitted categories are "Northeast," "Age 60 and over," "Primary school," "Construction industries," and "Farming occupations."

[c] Sample is employed wage and salary workers. Omitted categories are "Quebec," "Age 60 and over," "Primary school," "Job tenure 20 years and over," "Construction industries," and "Construction occupations."

Table 4: Decomposition of upward trends in on-the-job search.

A. Overall decomposition						
	PSID[a]		CPS/PSID[b]		LFS[c]	
	Actual	Pred.	Actual	Pred.	Actual	Pred.
Early year	0.123	0.123	0.035	0.035	0.023	0.023
Late year	0.240	0.240	0.097	0.097	0.054	0.054
Change		0.117		0.062		0.031
Expected in early year		0.210		0.079		0.045
Due to characteristics		0.030		0.018		0.009
(%)		(26)		(29)		(29)
Due to returns		0.087		0.044		0.022
(%)		(74)		(71)		(71)
Expected in late year		0.136		0.042		0.029
Due to characteristics		0.013		0.007		0.006
(%)		(11)		(11)		(19)
Due to returns		0.104		0.055		0.025
(%)		(89)		(89)		(81)
B. Contribution of each variable to upward trend						
Province/region		0.001		-0.000		0.001
Age		0.007		0.005		-0.001
Female		-0.001		-0.000		-0.001
Married		0.005		0.004		-0.000
Female and married				-0.001		-0.001
Spouse employed		0.000				0.001
Children		-0.001				
Education		0.009		0.007		0.011
Black		0.001		0.000		
Hispanic				-0.000		
Paid by the hour				0.001		
Reside in house		0.000		0.001		
Rent residence		0.000				
Tenure						-0.007
Usual weekly hours		0.001				
Part-time status				0.000		0.006
Hours preference		0.001				
Union member		0.006				
Industry		-0.001		0.004		0.001
Occupation		0.001		-0.002		-0.000
Total		0.030		0.018		0.009

[a] Early and late years are 1975 and 1985 respectively. Sample is employed household heads that are wage and salary workers.
[b] Early and late years are 1976 and 1993 respectively. Sample is employed household heads and wives that are wage and salary workers.
[c] Early and late years are 1976 and 1995 respectively. Sample is employed wage and salary workers.

Table 5: On-the-job search rates by tenure, full-time men, Canada.

Job Tenure (years)	1976-1980	1981-1985	1986-1990	1991-1995
0 – 1	0.049	0.058	0.061	0.072
1 – 5	0.022	0.030	0.042	0.043
5 – 10	0.011	0.015	0.024	0.026
10 – 15	0.004	0.006	0.014	0.016
> 15	0.001	0.003	0.006	0.006

Note: The sample is employed male wage and salary workers with usual weekly hours at least 40. All rates are based on samples of at least 10,000 observations.
Source: March 1976-1995 Labour Force Survey.

Table 6: Within-cohort on-the-job search rates, U.S..

Year of birth	1976-1977	1988-1993	Change
Before 1940	0.018	0.029	0.011
1940 – 1949	0.047	0.072	0.025
1950 – 1959	0.079	0.093	0.015
After 1959	0.211	0.141	-0.070*

Note: * indicates if change in rates is statistically significant at the 5 percent level.
Source: May 1976 and 1997 Current Population Survey and 1988-1993 Panel Study of Income Dynamics. The sample is employed household heads and wives that are wage and salary workers.

Table 7: Probit estimates of the probability of an employer change.

	(1)	(2)	(3)
Year 1988 dummy	0.2187*	0.2671*	0.1984*
	(0.0415)	(0.0594)	(0.0614)
Weeks since last interview		0.0150*	0.0151*
		(0.0040)	(0.0041)
Local unemployment rate		-0.0000	0.0035
		(0.0132)	(0.0134)
Age			-0.0685*
			(0.0093)
Married			-0.2209*
			(0.0451)
Black			0.0305
			(0.0823)
Some or completed college			-0.0851
			(0.0439)
Enrolled in school			0.1205
			(0.0736)
No. of observations	4,405	4,363	4,363

Note: Standard errors are in parentheses. * indicates significance at the 5% level.
Source: Sample of employed men aged 23-31 that are wage and salary workers in their main jobs in 1975 NLSYM and 1988 NLSY.

Table 8 On-the-job search methods, Canada, 1976-1981.

	1976	1977	1978	1979	1980	1981
Contacted employer	0.016	0.018	0.020	0.023	0.023	0.024
Contacted public agency	0.007	0.008	0 008	0.008	0.008	0.008
Looked at ads	0.006	0.007	0.008	0.009	0.009	0.010
Other	0.010	0.011	0.010	0.011	0.011	0 012
On-the-job search rate	0.026	0.028	0.030	0.034	0.034	0.036
Average number of methods	1.54	1.57	1.56	1.50	1.52	1.50

Note: Sample is *all* employed workers, including the self-employed.
Source: The Canadian data are from Statistics Canada, *The Labour Force*, Catalogue no. 71-001, monthly 1976-1995.

Figure 1: On-the-job search rates, U.S., 1969-1995.

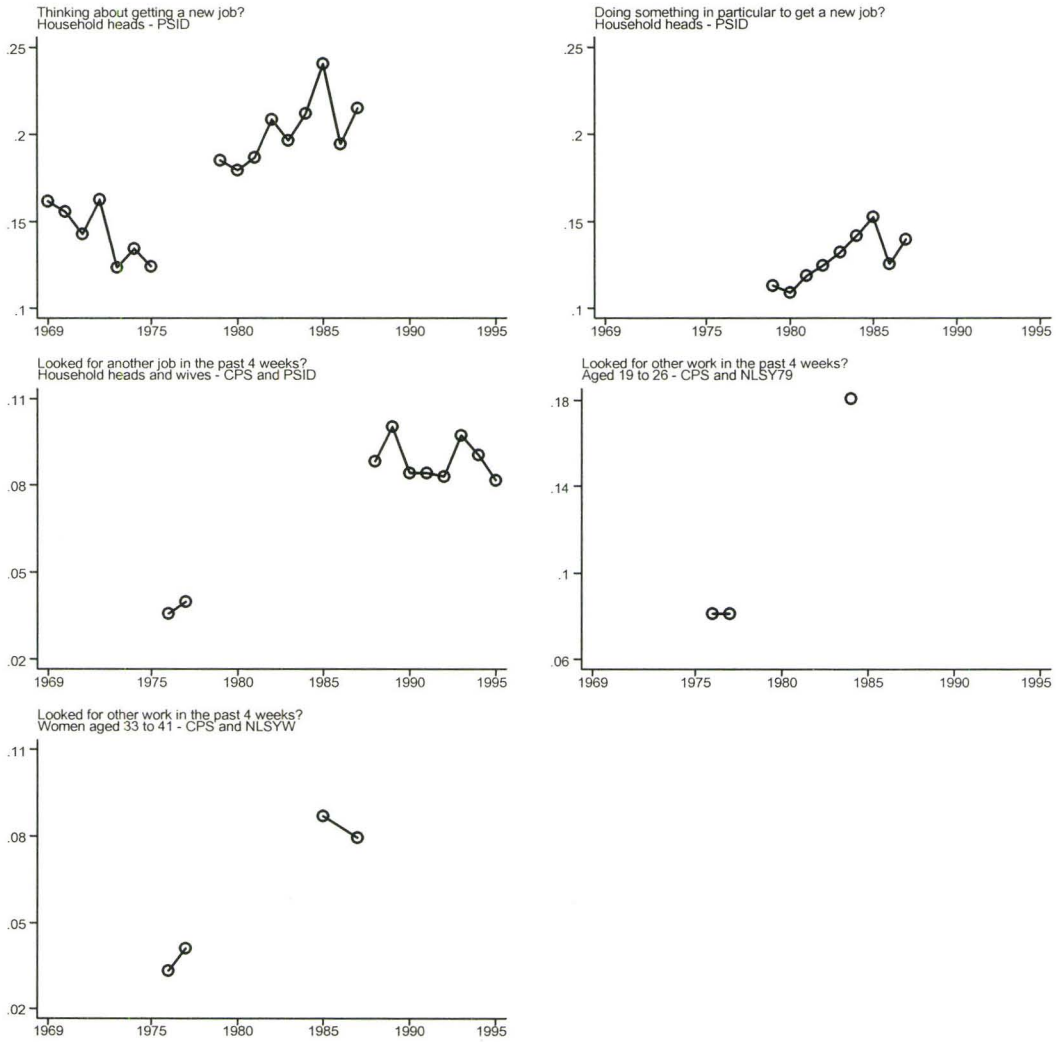


Figure 2: On-the-job search rates, Canada, 1976-1995.

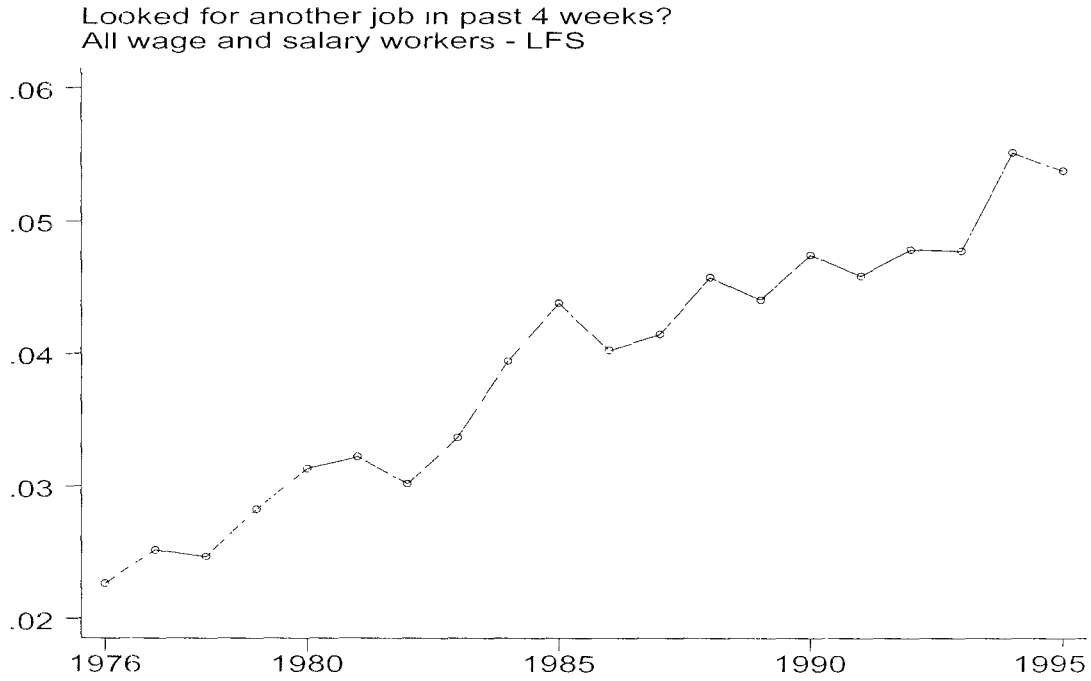


Figure 3: Within-cohort on-the-job search rates, Canada.

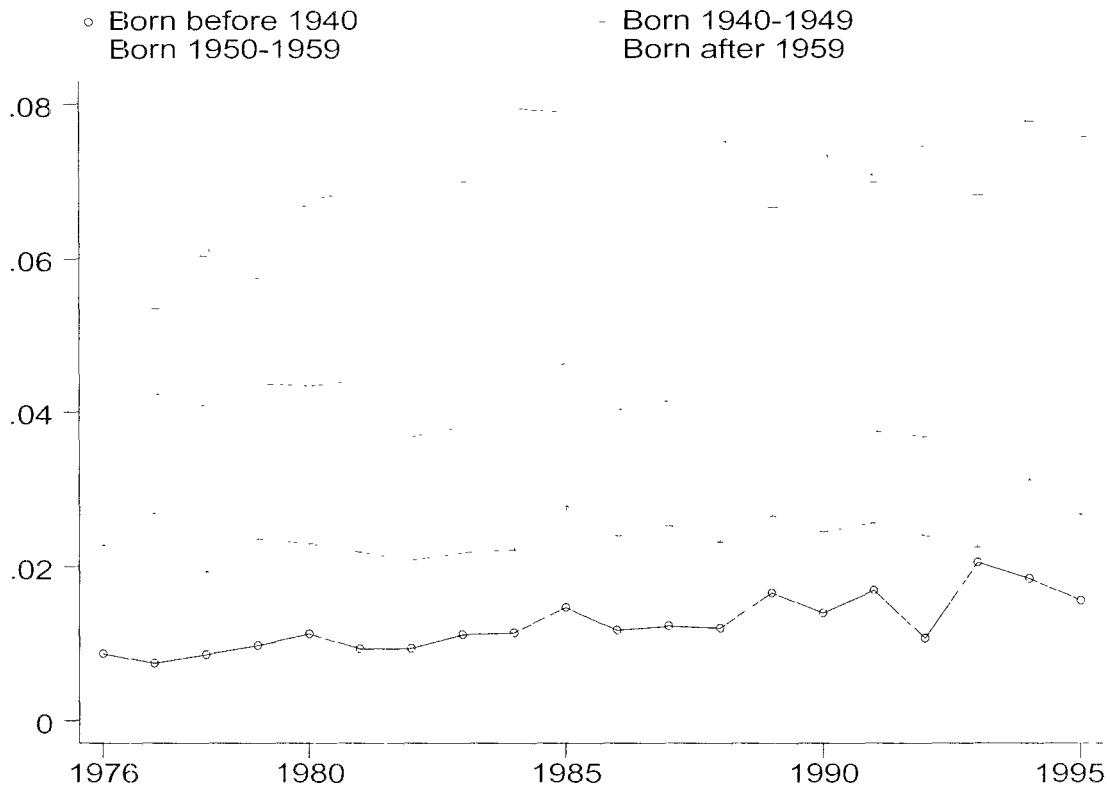
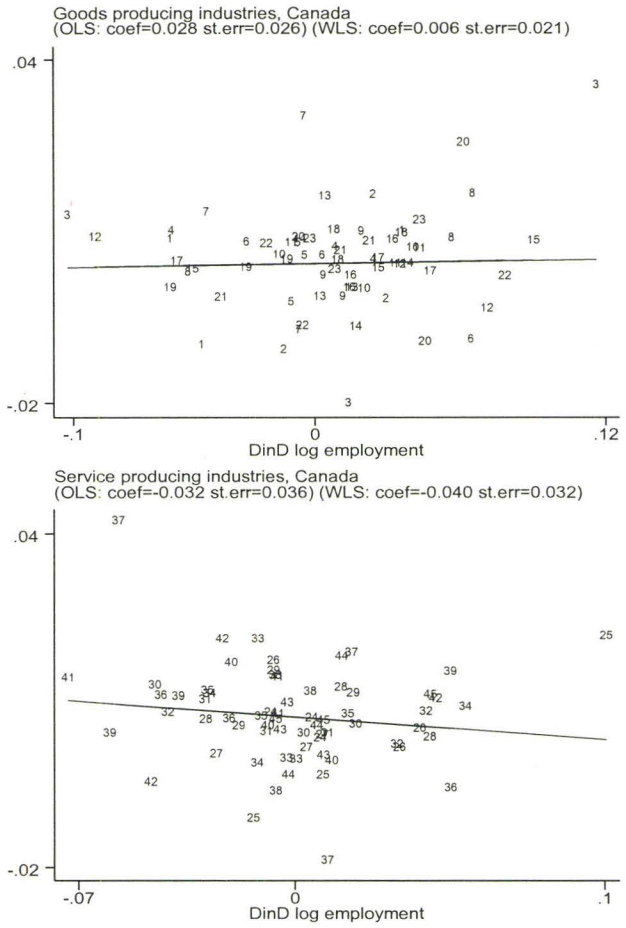
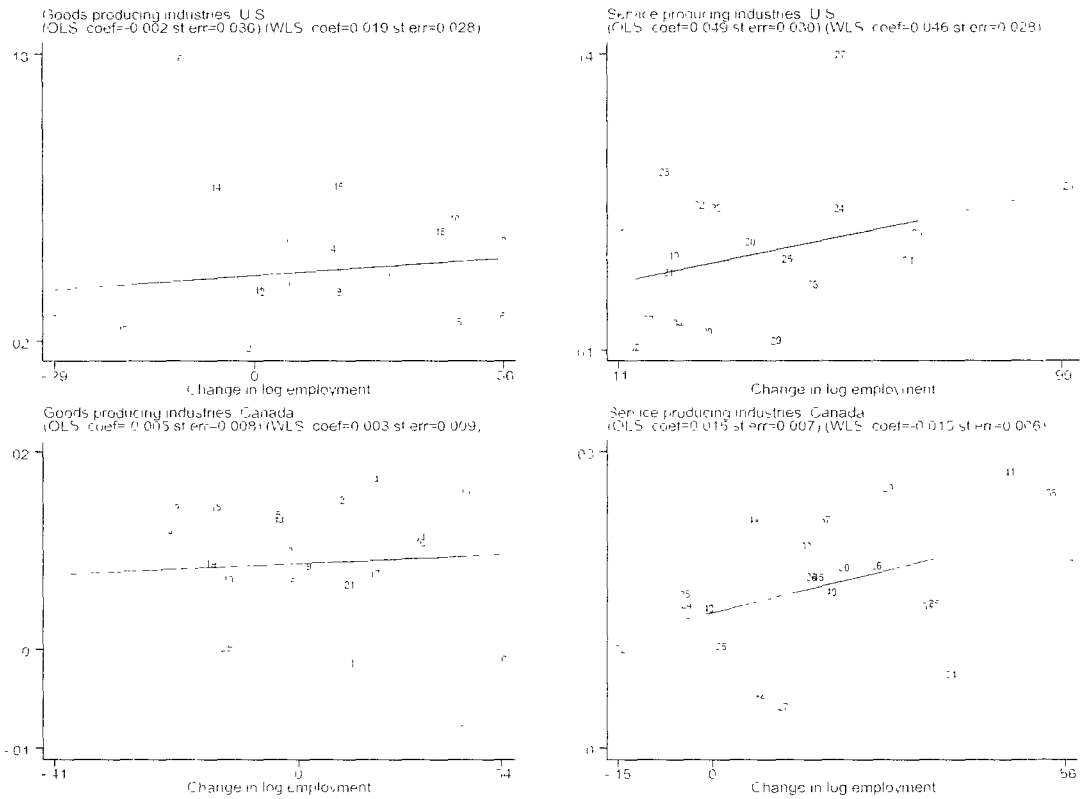


Figure 4: Changes in on-the-job search rates and differences-in-differences log employment.



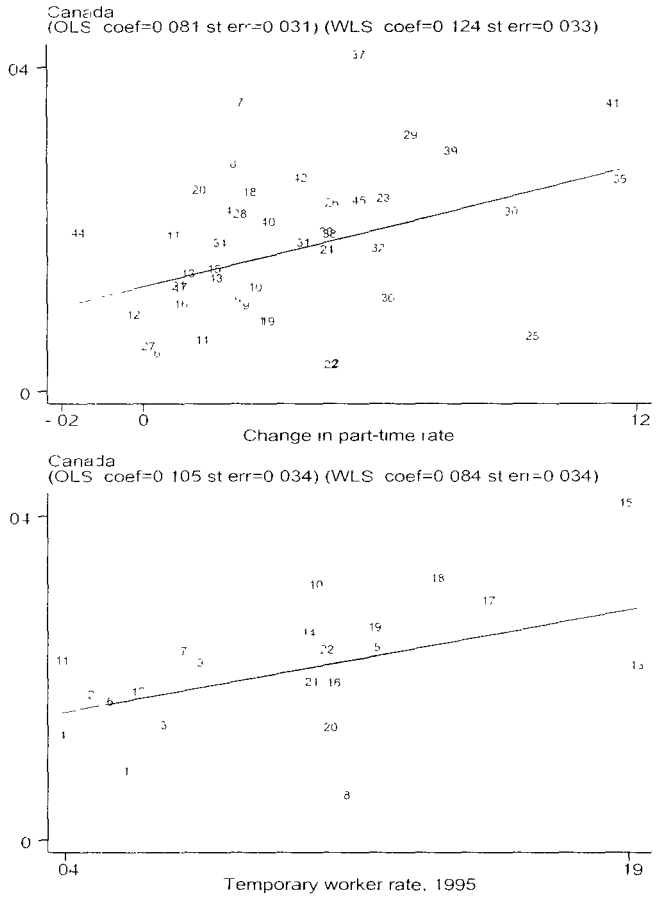
Note: Vertical axes measure percentage point changes in the OJS rate. The regression line is from the WLS estimates. Industry codes are in Table A1. Differences-in-differences log employment is the difference in the mean annual absolute log employment change between two five-year periods. Data on four five-year periods are plotted: 1976-1980, 1981-1985, 1986-1990 and 1991-1995. Source: Change in Canadian OJS rates and log employment levels are from March 1976-1995 Canadian Labour Force Surveys.

Figure 5: Changes in on-the-job search rates and log employment by industry, mid-1970s to late-1980s.



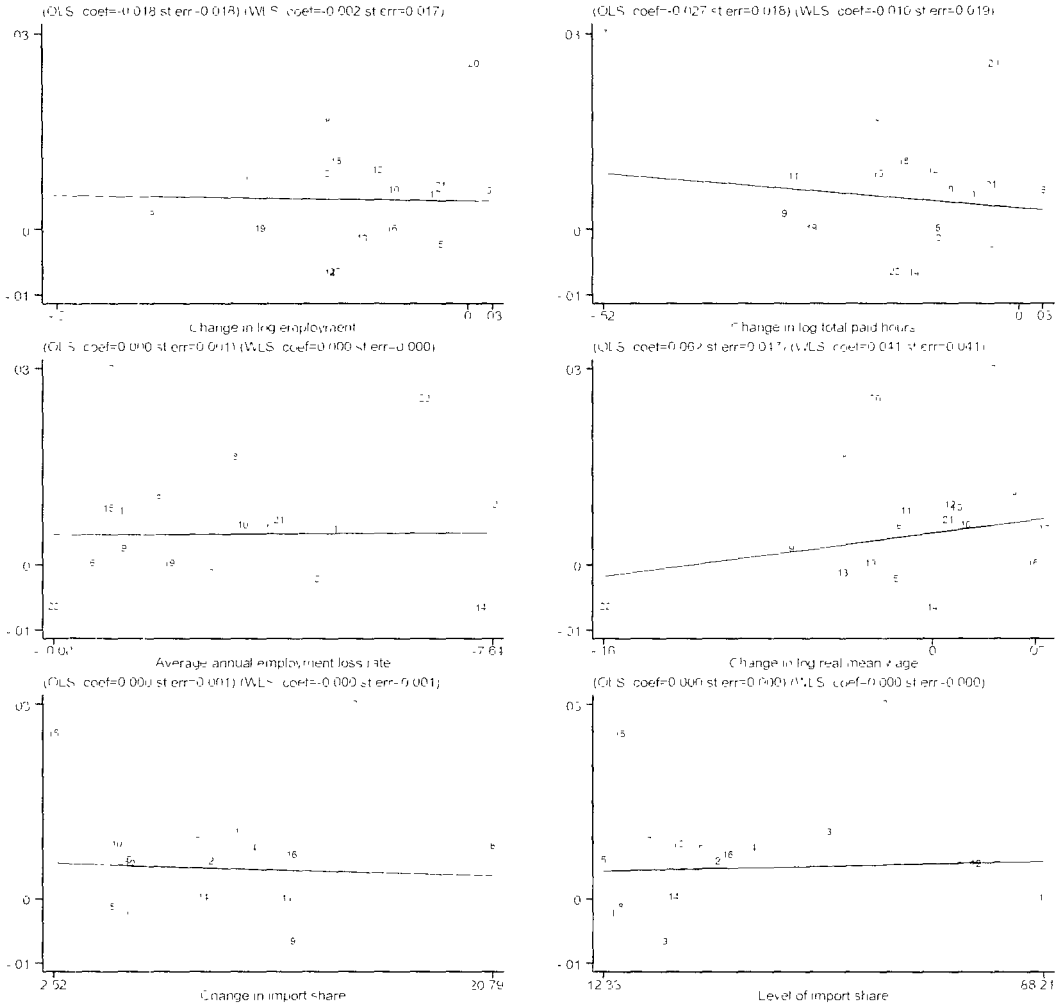
Note: Industry codes for the Canada and the U.S. are in Tables A1 and A2 respectively. Vertical axes measure percentage point changes in the OJS rate. The regression line is from the WLS estimates. Source: Change in U.S. OJS rates are from May 1976 and 1977 Current Population Survey and 1988-1993 Panel Study of Income Dynamics. Change in U.S. log employment levels is from the 1976 and 1988 basic monthly Current Population Survey files. Change in Canadian OJS rates and log employment levels are from March 1976-1980 and 1986-1990 March Labour Force Surveys.

Figure 6: Changes in on-the-job search rates and part-time/contingent worker rates.



Note: Vertical axes measure percentage point changes in the OJS rate. The regression line is from the WLS estimates. Industry codes for panel 1 are in Table A1. Industry 3 (Fishing and trapping) is an outlier in panel 1 and is dropped from the graph (change in part-time rate = 0.239 and change in OJS rate = 0.028). Industry codes for panel 2 are: 1. Agriculture 2. Primary industries 3. Non-durable manufacturing 4. Durable manufacturing 5. Construction 6. Transportation and storage 7. Communications 8. Utilities 9. Wholesale trade 10. Retail trade 11. Finance industries 12. Insurance and real estate 13. Education and related services 14. Health and welfare services 15. Amusement and recreation 16. Services to business management 17. Personal services 18. Accommodation and food services 19. Miscellaneous services 20. Federal administration 21. Provincial administration 22. Local administration.
Source: Change in OJS and part-time rates are from March 1976-1980 and 1991-1995 March Labour Force Surveys. Temporary worker rate is from the Survey of Work Arrangements supplement to the November 1995 Labour Force Survey.

Figure 7: Changes in on-the-job search rates and production worker employment/wages and industry trade, late-1980s to early 1990s, Canadian manufacturing industries.

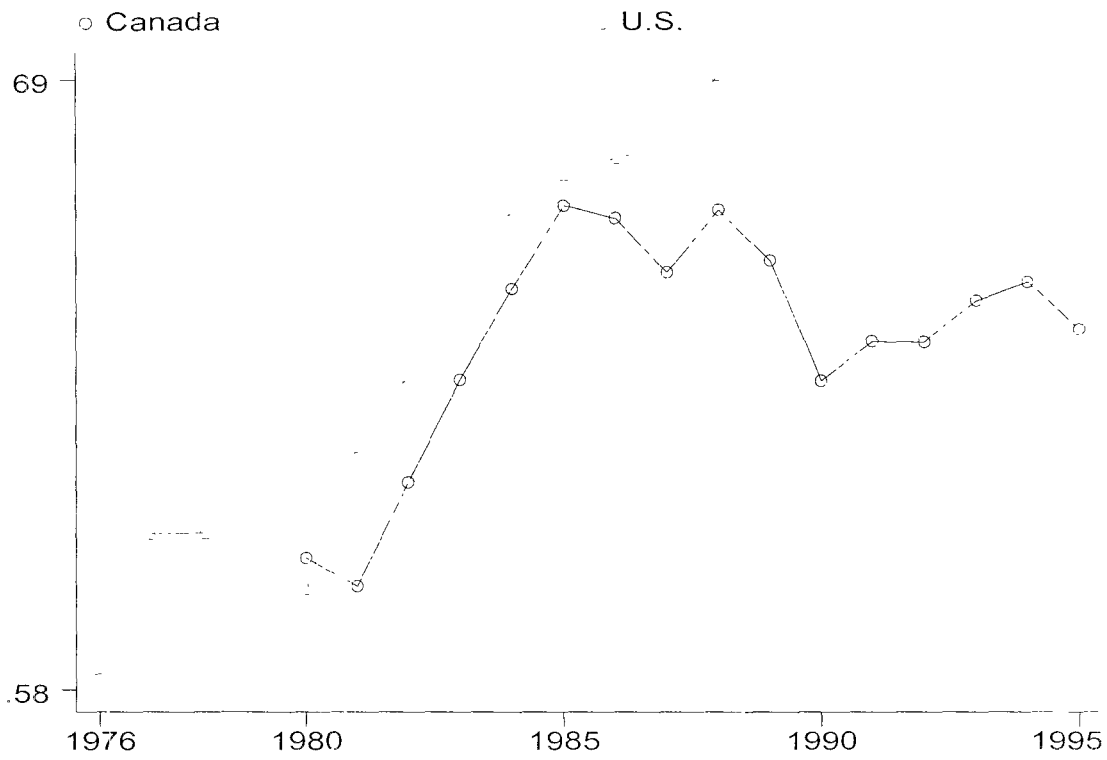


Note: Vertical axes measure percentage point changes in the OJS rate. The regression line is from the WLS estimates. Industry codes for the first four panels are in Table A1. Industry codes for the fifth and sixth panel are: 1. Food, beverage, tobacco 2. Rubber and plastic 3. Leather 4. Textiles and clothing 5. Wood 6. Furniture and fixtures 7. Paper and allied 8. Printing and publishing 9. Primary metal 10. Fabricated metal 11. Machinery 12. Transportation equipment 13. Electrical machinery 14. Non-Metallic mineral products 15. Petroleum and coal 16.Chemical.

Source: Change in Canadian OJS from March 1986-1990 and 1991-1995 March Labour Force Surveys. Employment and wage changes in panels 1-4 from Annual Survey of Manufacturers 1986-1990 and 1991-1995. Employment loss rate in

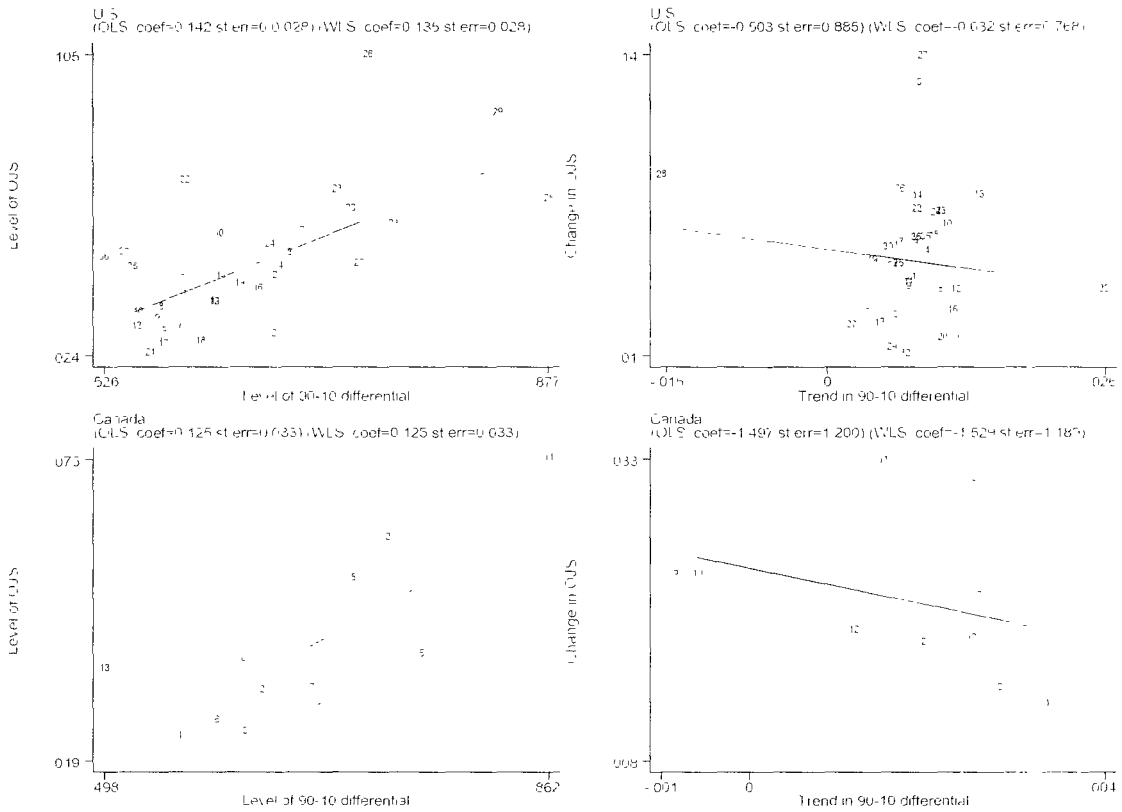
panel 4 from Annual Survey of Manufacturers 1976-1995. This series is calculated as the loss in employment from all plant contractions and closings between two years as a function of total employment in the first year. Trade data are from are from the System of National Accounts (Statistics Canada, *The Input-Output Structure of the Canadian Economy*, 1986-1993).

Figure 8: 90-10 differentials of absolute within-industry log wage residuals.



Note: National differentials are created by taking a weighted average of the industry differentials, where the weights are the industry employment shares in each year.
Source: March Current Population Survey 1977-1989 for the U.S. and Survey of Consumer Finances 1981-1996 for Canada.

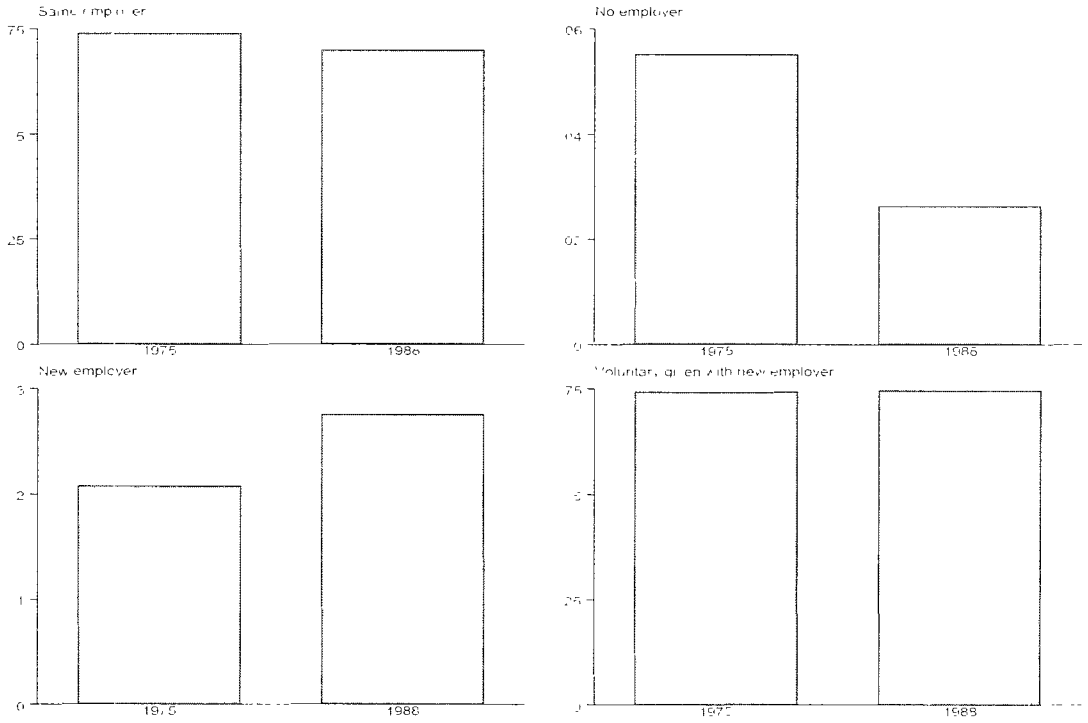
Figure 9: Level of and change in on-the-job search rates and levels and trends in 90-10 differentials of absolute log wage residuals, non-agricultural industries.



Source: Change in U.S. OJS rates are from May 1976 and 1977 Current Population Survey and 1988-1993 Panel Study of Income Dynamics. U.S. wage dispersion data from March Current Population Survey 1977-1989. Change in Canadian OJS rates are from March 1976-1980 and 1991-1995 March Labour Force Surveys. Canadian wage dispersion measures are from the Survey of Consumer Finances 1981-1996.

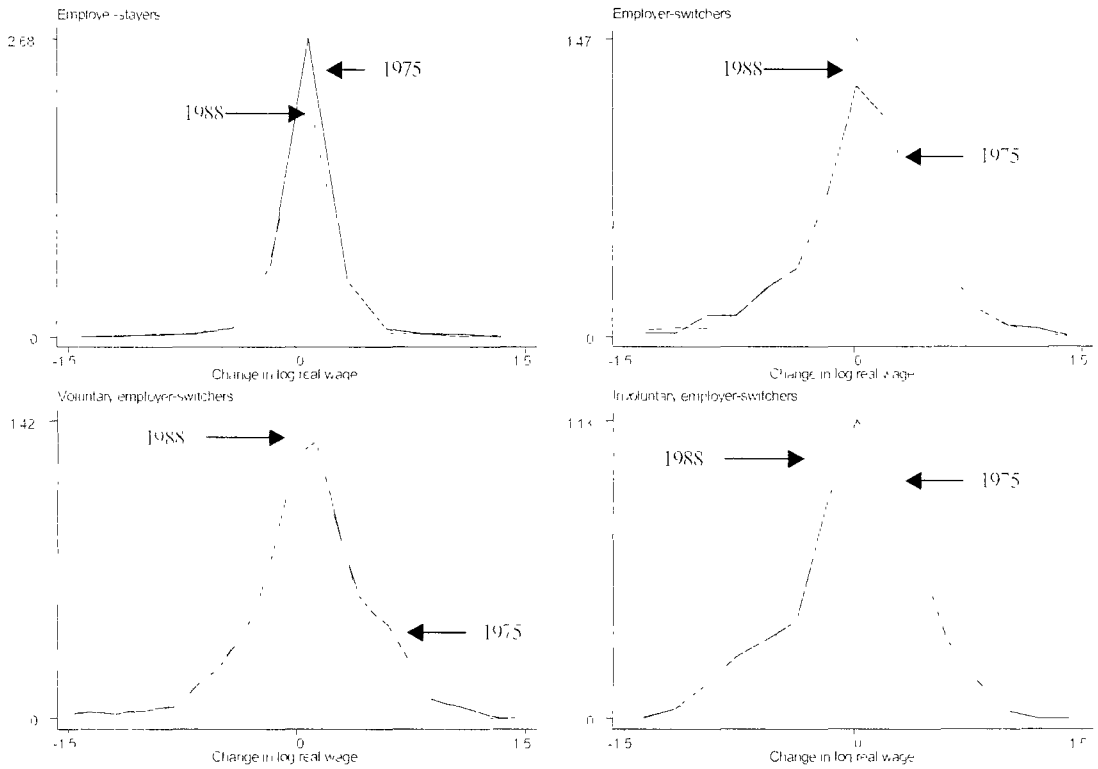
Note: Industry codes for the U.S. are in Table 8. Separate wage data for industry 34 (postal service) is not available. Industry codes for Canada: 1. Agriculture 2. Primary industries 3. Non-durable manufacturing 4. Durable manufacturing 5. Construction 6. Transportation, communications and other utilities 7. Wholesale trade 8. Retail trade 9. Finance, insurance and real estate 10. Community services 11. Personal services 12. Business and miscellaneous services 13. Public administration. The regression line is from the WLS estimates.

Figure 10: One-year transition rates, employed male wage and salary workers aged 23-31.



Source: Sample of employed men aged 23-31 that are wage and salary workers in their main jobs in 1975 NLSYM and 1988 NLSY.

Figure 11: Distributions of one-year real log wage changes.



Note: The distributions are estimated using a Epanechnikov kernel density function. The bandwidth of the kernel is calculated as $h = 0.9m / n^{1/5}$ where n is the number of observations and m is the standard deviation of x.

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Appendix A: Period-to-period changes in OJS rates, by industry.

Table A1: On-the-job search rates by industry, Canada.

	1976-1980	1981-1985	1986-1990	1991-1995
Goods producing industries				
1 Agriculture	0.033	0.042	0.032	0.042
2. Forestry	0.034	0.051	0.049	0.038
3. Fishing and trapping	0.025	0.038	0.018	0.053
4. Mining, quarries and oil wells	0.020	0.027	0.037	0.042
5. Food, beverage, tobacco	0.019	0.024	0.032	0.030
6. Rubber and plastics	0.026	0.016	0.024	0.030
7 Leather	0.016	0.029	0.022	0.051
8 Textiles	0.010	0.018	0.021	0.037
9 Clothing	0.014	0.012	0.022	0.024
10 Wood	0.027	0.026	0.033	0.039
11 Furniture and fixtures	0.019	0.023	0.030	0.038
12. Paper and allied	0.009	0.013	0.010	0.018
13. Printing, publishing and allied	0.037	0.037	0.052	0.051
14 Primary metal	0.009	0.013	0.022	0.015
15. Fabricated metal	0.022	0.026	0.029	0.037
16. Machinery	0.020	0.022	0.030	0.030
17 Transportation equipment	0.011	0.014	0.019	0.024
18. Electrical products	0.019	0.023	0.033	0.043
19. Non-metallic mineral products	0.020	0.025	0.029	0.029
20. Petroleum and coal	0.009	0.017	0.008	0.033
21 Chemical	0.015	0.023	0.021	0.027
22. Miscellaneous	0.018	0.026	0.028	0.021
23. Construction industries	0.027	0.039	0.042	0.051
Service producing industries				
24. Transportation	0.021	0.028	0.036	0.039
25. Storage	0.018	0.039	0.028	0.024
26. Communications	0.013	0.015	0.032	0.036
27. Electric power, gas and water utilities	0.014	0.014	0.018	0.019
28. Wholesale trade	0.022	0.034	0.038	0.044
29. Retail trade	0.035	0.050	0.061	0.066
30. Finance	0.013	0.026	0.031	0.035
31. Insurance and real estate	0.024	0.033	0.038	0.042
32. Elementary and secondary schools	0.018	0.020	0.028	0.036
33 Universities and colleges	0.043	0.064	0.064	0.063
34. Other education	0.044	0.052	0.051	0.062
35. Health and welfare services	0.023	0.030	0.038	0.049
36. Religious organizations	0.013	0.024	0.030	0.024
37 Amusement and recreation services	0.058	0.099	0.080	0.099
38. Services to business management	0.041	0.056	0.067	0.060
39. Personal services	0.027	0.042	0.046	0.056
40. Accommodation services	0.059	0.075	0.074	0.080
41 Food services	0.060	0.074	0.088	0.095
42 Miscellaneous services	0.063	0.058	0.068	0.089

43. Federal administration	0.033	0 037	0.047	0.047
44. Provincial administration	0 025	0.030	0 048	0 044
45. Local and other government services	0 018	0.025	0 035	0 042

Source: March 1976-1995 Labour Force Survey The sample is employed wage and salary workers.

Table A2: On-the-job search rates by industry, U.S..

	1976-1977	1988-1993
Goods producing industries		
1 Agriculture	0.039	0 084
2. Other primary	0.027	0 043
3 Construction	0 038	0.096
4. Lumber and wood products	0.035	0.090
5 Furniture and fixtures	0.035	0.061
6 Stone, clay and glass	0 025	0 153
7. Metal industries	0.023	0.051
8. Machinery	0.028	0 066
9. Electrical machinery, equipment and supplies	0 024	0 063
10 Transportation	0 021	0 087
11 Miscellaneous manufacturing	0.030	0.072
12. Food, beverage, tobacco	0.030	0 068
13. Textile mill products	0.030	0 054
14 Apparel	0.024	0 102
15 Paper and allied products	0.019	0.098
16. Printing and publishing	0 028	0.057
17. Chemical, Petroleum and coal products	0.018	0 076
18. Rubber and plastics	0.017	0.078
Service producing industries		
19 Transportation services	0.030	0.081
20 Communications	0.024	0.041
21. Utilities and sanitary services	0.015	0 058
22. Wholesale trade	0.029	0 102
23. Retail trade	0 047	0 118
24. Finance	0.032	0 103
25 Insurance and real estate	0.040	0 088
26. Business services	0 065	0.146
27. Repair services	0.022	0.161
28. Personal services	0.045	0 132
29. Entertainment and recreation	0.088	0.101
30. Health services	0.035	0.091
31. Welfare and religious services	0.040	0.088
32. Education	0.065	0 075
33 Other professional services	0 054	0.092
34. Postal service	0.011	0 031
35 Federal administration	0 027	0 087
36 State administration	0 032	0.093
37. Local administration	0 041	0 063

Source May 1976 and 1997 Current Population Survey and 1988-1993 Panel Study of Income Dynamics The sample is employed household heads and wives that are wage and salary workers.

Appendix B: Weighted Least Squares Estimator

The weighted least squares (WLS) estimates supplement the OLS estimates by weighting observations based on larger cell sizes more heavily. We are interested in estimating the equation:

$$\Delta p_i = \alpha + \beta x_i + v_i, \tag{1}$$

where Δp_i is the true change in OJS rates experienced by industry i, x_i is the magnitude of some other change experienced by industry i over the same period, and v_i is a random error with expected value 0 and a uniform variance σ^2. The problem is, of course, that we do not observe Δp_i. Instead, we must estimate:

$$\Delta \hat{p}_i = \alpha + \beta x_i + e_i \tag{2}$$

where $\Delta \hat{p}_i$ is the estimated change in OJS rates experienced by industry i, and β are the OLS estimates presented in Figures 4, 5, 6, 8 and 9. The error term in (2), e_i, does not have a uniform variance. Rather,

$$Var(e_i) = Var(p_{il}) + Var(p_{ie}) + \sigma^2 \tag{3}$$

where p_{il} and p_{ie} are the OJS rates for industry i from the early and late periods respectively. To correct for this heteroscedasticity, the estimate of β is instead obtained from:

$$\frac{\Delta \hat{p}_i}{\sqrt{w_i}} = \frac{\alpha}{\sqrt{w_i}} + \frac{\beta x_i}{\sqrt{w_i}} + \frac{e_i}{\sqrt{w_i}} \tag{4}$$

where w_i is given by the right-hand-side of (3) and an estimate of σ^2 is obtained from:

$$\hat{\sigma}^2 = \frac{\sum \hat{e}_i^2}{n-1} - \frac{\sum (Var(\hat{p}_{il}) + Var(\hat{p}_{ie}))}{n} \tag{5}$$

CHAPTER TWO

DOES INTERNET JOB SEARCH SHORTEN UNEMPLOYMENT SPELLS?

"Using CareerBuilder® to find a job is like driving in the carpool lane."

-half-page ad for an internet job site in the Los Angeles Times, Friday March 1, 2002. (p. C5)

1. Introduction

By August 2000, just over one quarter of U.S. unemployed jobseekers, and over one in ten employed workers, reported that they regularly used the internet to look for jobs. The use of internet job and recruiting sites is generally free of cost for workers and much cheaper for firms than traditional print advertisements. In addition, these services give firms and workers instant access to a much larger number of potential matches than traditional channels, and offer the potential for the exchange of much more detailed information about both worker and job attributes.[1]

Not surprisingly, economists have begun to speculate on the potential effects of the above developments on various aspects of labor market equilibrium.

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[1] For example, at firms' request WebHire will check the following worker credentials: social security numbers; current and previous addresses, references; education; criminal, civil and bankruptcy court records; driving and credit reports; and workers' compensation claims. Also offered are on-line skills and personality testing. The combination of internet application procedures and traditional database management software also dramatically simplifies the process of searching through submitted resumes for appropriate matches. Workers can now gain much more information about working conditions and job requirements from job boards as well as company websites.

For example, commentators have argued that the higher contact rate, lower cost, and greater information content provided by this technology could lead to lower frictional unemployment (Mortenson 2000), higher average match quality (Krueger 2000), a reduction of noncompetitive wage differentials (Autor 2001), and an amplification of ability-related wage differentials (Kuhn 2000). If even some of these claims are correct, the advent of internet job search will have important implications for both labor- and macroeconomic policy.[2]

The goal of this paper is to ascertain whether, in recent U.S. labor markets, persons who use the internet to look for jobs have better search outcomes than otherwise-identical persons who do not. Our interest in this question derives from the fact that such a differential is a necessary condition for the spread of internet job search technology to have *any* general-equilibrium effects on unemployment rates, wages, and other outcomes such as the NAIRU. While –depending on the model-- such equilibrium effects could be either beneficial or perverse[3], all search models of which we are aware share the following feature: Consider a given search equilibrium in which (for arguably exogenous reasons) some individuals have access to internet job search technology and others do not. Then if internet job search truly represents a technological improvement in the individual's job-search "production function", those persons who use the technology will have

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[2] One potentially relevant aspect of labor market policy is the rationale for government-provided job matching services such as the states' Employment Services. Macro policy implications could follow from any change in the NAIRU caused by internet job search technology.

[3] For example, Lang (2000) has suggested an asymmetric-information model in which a reduction in the costs of applying to jobs can be Pareto-worsening, in part by reducing the average match quality in every firm's applicant pool.

better outcomes (i.e. shorter unemployment durations and better job matches) than those who do not.[4]

In order to answer our question we use measures of internet search derived from the December 1998 and August 2000 CPS Computer and Internet Use Supplements, matched with job search outcomes from all subsequent CPS files that contain some of the same survey respondents. Throughout our analysis we focus on the search outcomes of unemployed persons only. This is because the regular monthly CPS does not collect data on non-internet search methods used by employed persons.[5] Thus, for employed persons, CPS data does not allow one to distinguish the effect of looking for work on line from choosing to look for work at all. We also restrict our attention to one particular outcome of the job search process— jobless duration. In part, this is driven by data considerations: in the CPS, job quality (i.e. wage) information is not available for a sufficient sample of jobseekers.[6] For many policy purposes, however, unemployment durations are the outcome of most direct interest, justifying our focus here.

This paper contributes to an emerging literature on the effects of internet technology on product market performance (e.g. Brown and Goolsbee 2002 in life insurance markets; Brynjolfsson and Smith 2000 on book and CD markets, and

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[4] Under certain conditions workers may adjust to an internet-induced increase in the offer arrival rate by raising their reservation wages so much that unemployment durations rise (Burdett and Ondrich 1985)

[5] See Skuterud (2002) for a recent analysis of trends in on-the-job search using the occasional CPS surveys that do collect this information.

[6] CPS wage information is of course only available for persons who find new jobs, and who are in the outgoing rotation groups. Further, a credible analysis of re-employment wages also requires controls for *pre-unemployment* wages, a restriction which reduces the sample to non-useful levels

Carlton and Chevalier 2001 on various consumer durables); to our knowledge ours is the only study of the effects of internet technology on the functioning of the labor market. The current paper also contributes to an older literature on the relative effectiveness of different job search methods. For example, Holzer (1987, 1988), Bortnick and Ports (1992), Osberg (1993) and Addison and Portugal (2001) compare the job-finding rates of unemployed workers using a variety of search methods. Thomas (1997) focuses specifically on the effectiveness of public employment agencies.

In our data, simple means indicate that internet job searchers are more likely to be employed one year after their search methods are observed than are other unemployed workers. However we find that this difference is entirely accounted for by differences in observable characteristics. Further adjusting our estimates (a) to incorporate all the available information in our sample on unemployment durations, and (b) for length-biased sampling (Lancaster 1979), yields estimates of internet job search effects that are *counterproductive*, i.e. internet job search appears to *lengthen* unemployment spells. Finally, we develop and add to the above model an instrumental-variables-type technique to adjust for endogenous selection into internet search. While this model attributes this apparent counterproductive effect to negative selection on unobservables, none of our estimates show a statistically significant, beneficial effect of internet job search on the length of unemployment spells.

2. Data and Descriptive Statistics

As noted, our data on internet job search come from the December 1998 and August 2000 Computer and Internet Use Supplements to the Current Population Survey. These supplements included the following question: "Do(es) (you) (any one) REGULARLY use the Internet ... to search for jobs?". As always, the regular monthly CPS survey in these months also asked unemployed individuals which out of a list of nine "traditional" job search methods they used.

Internet job search rates in these two surveys, classified by labor force status, are shown in Table 1. As already noted, the fraction of unemployed jobseekers[7] looking for work on line was 25.5 percent in August 2000, up from 15.0 percent in November 1998, less than two years earlier. As Table 1 also shows, much of this increase was associated with a large rise in home internet access among unemployed persons (from 22.3 to 39.4 percent), but internet use for job search conditional on internet access also rose over this period. By August 2000, *regular* internet job search was also surprisingly common among the employed (around 11 percent) and among labor force nonparticipants, at least those who were neither retired nor disabled (around 6 percent).[8]

In order to measure the job-finding success of internet versus other job searchers, we matched observations in the December 1998 supplement with the same persons in the ten subsequent CPS regular monthly surveys (January-March

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[7] All unemployed workers not expecting to be recalled to their former employer are classified by the BLS as "jobseekers".

[8] Kuhn and Skuterud (2000) compare these recent rates of on-the-job *internet* job search (IJS) to historical measures of on-the-job search (OJS) via any method. They are significantly higher, suggesting that the internet may have contributed to an increase in total OJS.

1999, September 1999 through March 2000) in which some of the same individuals were re-interviewed. Similarly the August 2000 survey was matched with September-November 2000, and May through November 2001. Matching was done using established methods (see for example Madrian and Lefgren 1999); some details about our procedure are provided in Appendix A.

To be in our sample, a person had to be unemployed according to the official Bureau of Labor Statistics definition in a Computer/Internet supplement month (December 1998 or August 2000), yielding a sample of 4139 persons.[9] To be considered unemployed, the individual had to be not working, and *either* "on layoff" from a job to which he/she expected to be recalled, *or* searching for work using at least one of nine recognized "active" methods.[10] These methods are listed in Table 2; note that they could themselves involve internet use (for example "sending resumes" could include sending resumes via email). The role of our internet supplement variable is to distinguish persons who incorporated the internet into their job search strategy from those who did not, holding other dimensions of this strategy fixed.

Sample means of all the variables used in the regression analyses below are presented in Table 2 separately for unemployed persons who searched for a new job on the internet and those who did not. In most cases, unemployed

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[9] This includes a small group of persons who were never matched with an observation after those dates. While these observations contribute no information on unemployment durations, they do contribute information on the determinants of internet search, and are retained in some of our analysis for that reason.

[10] We also conducted some analyses that excluded workers expecting recall, as well as some analyses that included marginally-attached workers (nonparticipants who engaged in passive job search only). In neither case were the results substantially different.

workers who look for jobs on line have observable characteristics that are usually associated with greater job search success than other unemployed workers. For example, in the Computer/Internet Supplement month, the average unemployed internet searcher had already been unemployed for 3.44 months, somewhat less than the 3.75-month "retrospective duration" of the non-internet searchers. Internet searchers resided in states with somewhat lower unemployment rates than other unemployed workers, and had previously worked in occupations with considerably lower unemployment rates. They were more likely to have been employed prior to the current unemployment spell, were much better educated, and were more likely to be in their "prime" working ages (26-55) (versus under 26 or over 55). Internet job searchers were less likely to be black, Hispanic or immigrant and more likely to be homeowners than other unemployed persons. Finally, on average, unemployed workers who looked for work on line were *more* likely, not less likely, to use "traditional" job search methods than other unemployed workers. In all, they used an average of 2.17 "traditional" search methods, compared to 1.67 for other unemployed workers, suggesting an overall greater investment in search.[11]

Table 2 also reports rates of internet use outside the home among the members of respondents' households. These rates differ between internet job searchers and others, with the spouses and "other" household members (excluding spouses, parents and children) of internet job searchers being more likely to use

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[11] This apparent "complementarity" between internet and other job search methods is examined in more detail in Kuhn and Skuterud (2000)

the internet outside the home. Finally, Table 2 shows that internet job searchers live in states with higher mean overall internet access rates, and where a smaller share of households must make a long-distance call to access the internet. There is no significant difference in state mean internet access fees between internet searchers and other unemployed persons.

By construction, no one in our sample was working in the month in which we observe whether or not their job search strategy incorporated the internet (December 1998 or August 2000). The fraction of our sample observed in employment at various points after these dates are reported in Table 3. For example, among those individuals whose labor market status was observed one month after the Supplement date (i.e. in January 1999 or September 2000), 29.1 percent were employed. Two months after the supplement date, 37.5 percent were employed, and a year later 55.9 percent were employed. If we pool all individuals who were re-interviewed at least once after the date in which we observe their internet search activity, the same share, 55.9 percent, were seen in re-employment at some time after the Supplement date.

Comparing internet job searchers with other unemployed workers, essentially no difference in employment rates is evident one or two months after an individual's internet job search activity is observed. A year later, however, 64.6 percent of unemployed internet searchers are re-employed, compared to 53.3 percent of other unemployed workers. This difference, like the difference in re-employment at *any* time after the Supplement date (in row 4 of the Table), is

statistically significant. On the surface, Table 3 thus seems to suggest that internet search facilitates re-employment, at least if one allows a few months to elapse for this method to yield results.

3. Re-employment Probits

A first step in ascertaining whether the differences found in Table 3 are truly causal effects of internet search is to see whether they are simply artifacts of differences between internet searchers and other unemployed persons in observable characteristics, such as education, local labor market conditions, and the use of non-internet job search methods. To this end, Table 4 presents probit estimates of the probability an unemployed individual is re-employed 12 months after we observe their internet job search activity in the CPS Computer/Internet Supplement. We focus on 12 months because this is where the largest apparent internet effect was observed in Table 3.[12]

Of course, re-employment probabilities in the above probits will likely depend on how long an individual had already been unemployed when we observe whether or not he/she uses the internet for job search, i.e. at the Supplement date. As is well known, there are at least two distinct reasons for this: duration dependence (long unemployment spells may have a causal effect on

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[12] Similar analyses were performed for re-employment within a month, within two months, or at any time after internet search activity is observed. (In the latter specification, we added a control for the number of months in which the individual is observed after the Supplement month) In all cases, the results were similar to those in Table 4: whenever even a relatively parsimonious set of demographic controls are used, the internet search coefficient is either insignificant or negative.

subsequent exit rates from unemployment), and unobserved heterogeneity (individuals who have been unemployed a long time are disproportionately less "employable" on unobserved dimensions). Up to and including Section 5 of this paper we handle both these possibilities in a simplistic manner: we simply include "retrospective" (pre-Supplement date) unemployment durations as a regressor in our models. Sections 6 and 7 will handle both these issues more formally.

Column 1 of Table 4 reproduces the significant difference in Table 3, where only internet search is included a regressor. Column 2 adds a control for home internet access, which reduces the value of the coefficient. Thus, part of the apparent effect of internet use on re-employment rates in Table 3 is an artifact of different re-employment rates between individuals who have internet access and those who do not. This should not be surprising since these individuals are, on average, better educated and probably more familiar with computer technology, itself a potentially valuable job skill.

A similar, but stronger message emerges when additional controls for observable characteristics are added in the remaining columns of Table 4. Columns 3 and 4 add controls for labor market conditions –local and occupational unemployment rates-- and for various characteristics of the unemployment spell. The latter include how long the spell had been in progress by the Supplement month, whether the individual was "on layoff" and therefore expecting recall, what activity (school, public sector employment, private sector employment, self employment, school) preceded the unemployment spell, and the reasons for

leaving any previous job ("lost job" and "temporary job", with quits as the omitted category). We also include a fixed effect for the 2000 survey to capture any changes in macroeconomic conditions between the surveys. As for columns 1 and 2, we present one specification with and one without a home internet access control. The apparent effect of internet search on re-employment remains positive, but is again smaller and becomes statistically insignificant in the presence of a home access control.

Effects of the "control" variables in columns 3 and 4 are generally in line with expectations. For example, although the coefficient is not quite significant at conventional levels, we see that individuals with high retrospective durations are less likely to be re-employed -- a result that mirrors the common finding of declining re-employment hazards in duration studies. A high occupational unemployment rate depresses job-finding rates, and individuals who worked or went to school immediately before the onset of their current unemployment spell are much more likely to be re-employed than those who did neither. Persons whose last job was in the private sector fared better in re-employment than those whose last job was in the public sector or in self-employment, or who did not work just prior to the current unemployment spell.[13]

Columns 5 and 6 add controls for demographic characteristics. They show, as expected, that younger workers are re-employed more quickly, and less-

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[13] Note that in a substantial number of cases the individual's last job preceded a spell of nonparticipation, so that these "sector" indicators do not simply subdivide the group who entered unemployment directly from a job.

educated and black workers more slowly. Single men are less likely to be re-employed than single women, but married men are more likely to be re-employed than married women, possibly reflecting greater geographical search constraints among married women (Crossley, Jones and Kuhn, 1994). The internet effect on re-employment now becomes highly insignificant in both specifications.

The last two columns of Table 4 add controls for the use of other, "traditional" job search methods. Interestingly, we detect significant positive effects on re-employment for three of these methods: direct employer contact, "sent resumes" and public employment agencies, which incidentally are also the search methods most commonly used by unemployed persons in our data. For the remaining methods, no statistically-significant effects on the job-finding rate are found. Likewise, adding the internet to one's job search strategy appears not to increase re-employment rates. In sum, when we control for observed characteristics of unemployed workers and their unemployment spells, those who look for work on line are not more likely to be re-employed in the near future.

4. Econometric Issues

While the results in columns 7 and 8 of Table 4 certainly suggest that incorporating the internet into one's job search strategy is ineffective in reducing jobless durations, there are at least three reasons why this conclusion may be premature. In this section we describe these reasons, and outline our strategy for dealing with them in the remainder of the paper.

A first reason why the re-employment probits summarized in Table 4 might fail to reveal a true, beneficial effect of internet job search is simply an inefficiency in the estimation procedure. Indeed, any probit focusing on a worker's labor force status at only a single date (in the above case 12 months after his/her search activity is observed) discards a considerable amount of information on the actual duration of unemployment. To address this issue, in what follows we estimate duration models that incorporate all the available information about a worker's jobless spell following the Supplement date. Of course, the information available to us on durations in the CPS is highly discrete: at best, we only know the month in which re-employment occurred; in some cases (the gap between the two four-month CPS observation "windows"), we only know that re-employment occurred during an eight-month period. This makes continuous-time duration models (such as, for example, the Cox partial likelihood model) highly inappropriate. For this reason we develop and estimate a discrete-time hazard model that takes into account the particular features (potentially large failure "windows" whose structure varies across observations) of CPS duration data, while still allowing for a fully flexible form of the baseline hazard function.[14]

A second reason why the estimates in Table 4 might disguise a true, beneficial effect of internet search on jobless durations results from the fact that our data is sampled at random from the *stock* of workers who were unemployed in the month of the Computer and Internet supplement. As a result, the probability of

[14] Existing discrete-time hazard models, such as that used by Meyer (1990) require the structure of intervals to be the same across observations

being in this sample is directly proportional to the dependent variable – i.e. the length of an individual's completed unemployment spell -- a property sometimes referred to as *length-biased sampling*. Since, in the simplest case, such a systematic undersampling of short spells will bias our internet search coefficients towards zero[15], addressing this issue is also essential to ruling out a true, beneficial effect of internet search on jobless durations. In what follows, we will augment our duration model using a technique introduced by Lancaster (1979) to address this issue. Essentially we will condition each observation's contribution to the likelihood function on the fact that it lasted long enough to be observed in our sample.

The remaining potential source of bias in Table 4 concerns the endogeneity of the internet job search variable. For example, one might be concerned that individuals who look for work on line might be a positively-selected sample, in the sense that they are more motivated and able to find a new job than observationally equivalent non-internet searchers. If that is the case, then the estimates in Table 4 *exaggerate* the benefits of internet job search, thus strengthening the case that internet job search does not reduce unemployment durations. Indeed, such positive selection seems particularly likely in our specifications that do not control for home internet access: persons with access

[15] Suppose that (aside from a constant term) internet search was the only regressor in a simple OLS regression model, and that its true effect was to reduce unemployment durations. Then the systematic undersampling of short durations induced by stock sampling will induce a positive correlation between the level of internet search and the error term. Failing to account for this will bias the (negative) coefficient on internet search upward, i.e. towards zero.

may be more re-employable than those without, in part because they are more computer-literate.

But what of the possibility of negative selection into internet search on unobservables? While this was not our prior when we started this research, and while it runs counter to the positive selection on *observables* evident in Tables 2 through 4, a quick reading of the trade literature on internet search and recruiting reveals that this is both a real possibility, and a real concern among firms perusing on-line resumes. Especially once we are able to hold constant an individual's access to the internet from home, it is possible that those unemployed persons who choose to search for work on line are in fact less able or motivated to find work than those who do not. One possible reason for this is private information about re-employment prospects: controlling for observables, individuals using a larger number of search methods (including the internet) may do so *in anticipation of* having a particularly difficult time finding work.[16]

Another possible source of negative selection is a lack of access to other, less formal and anonymous methods for finding work, such as social networks. Circumstantial evidence of this arose when one of us described our early results from this project to a newspaper reporter. The reporter, who teaches minority college students at night, remarked that a large majority of her students looked for work on line, and attributed this to the fact that very few of them had informal

[16] There is in fact evidence of this from the CPS itself, though not specifically for internet search. In a previous version of this paper (November 2000) we present counts of the number of "traditional" search methods used during the course of workers' unemployment spells. Workers with (ex post) long unemployment durations use a larger number of methods throughout their unemployment spell.

contacts in the world of white-collar work. Relatedly, Holzer (1987) attributes slower job-finding rates among minority youth to poorer informal networks; the same could be true of those who consult internet job boards.[17] Finally, a recruiting executive quoted in Autor (2001) observed that internet job boards are populated with four types of resumes: "the unhappy (and thus probably not a desirable employee); the curious (and therefore likely to be a 'job-hopper'; the unpromotable (probably for a reason); and the unemployed (probably for a worse reason)". Together, these comments suggest at least the possibility that negative selection into internet search (especially *among* those with home internet access) could be obscuring what is otherwise a true, unemployment duration-reducing effect of internet search in our sample.

In order to address the endogeneity issue, we need to do two things: one is to identify some instrumental variables that affect internet use but are unlikely to be correlated with idiosyncratic variation in individual workers' "re-employability". The second is to develop a means of incorporating these instruments (which are essentially exclusion restrictions) into a duration model that both handles the peculiarities of the CPS duration data and accounts for the length-biased sampling problem discussed above. Regarding the latter issue, we shall proceed by jointly modelling the process of selection into internet job search among the unemployed and the duration of search. By adapting a technique first

[17] Of course, for poorer networks to explain our result, they must not already be captured by our "contacted friends and relatives" variable. This is quite possible: informal networks often yield job leads without any worker initiatives (Granovetter 1995); Skuterud (2002) finds that a large fraction of new job starts are not preceded by any worker search at all.

used by Han and Hausman (1990), we are able to allow the idiosyncratic, unobserved determinants of both these outcomes to be correlated, and to estimate the degree of correlation empirically.

Regarding instruments, we propose two sets. The first is a set of indicators of internet use by members of the respondent's household outside the home. While, as noted, one might suspect that having home internet access oneself might be positively correlated with one's unobservable labor market prospects, this seems less likely to be the case for having a household member who uses the internet outside the home (especially holding own access constant). Further, the presence of such a person in the household should reduce any costs of becoming familiar with on-line job search sites. While not a perfect instrument, non-home internet use by household members should be less correlated with an individual's unobserved re-employability than are measures of his/her own internet access; further, to the extent they are still correlated with own re-employability we would expect this correlation to be positive. Thus, if anything, they should bias our results *towards* finding a beneficial, causal effect of internet search.

Our alternative set of instruments comprises three variables measuring mean internet access costs and internet diffusion at the state level. These are the mean level of access fees paid by internet users in the respondent's state, the share of households in the respondent's state who need to make a long-distance telephone call to access the internet, and simply the state mean home internet access rate. Because these are state means, they should be purged of any

individual idiosyncracies in re-employability (or reservation wages). Also, since (at least in two of the three cases) they focus on the costs rather than benefits of internet job search, they should not be contaminated by unobserved private information regarding expected unemployment durations, either across individuals or states. Finally, note that if these instruments are biased in any way, we would again expect them to be biased *towards* estimating a beneficial, causal effect of internet search. The main reason is that, as a consequence of unobserved demand shocks, states with high mean levels of internet access may have tighter labor markets on dimensions other than those which, like state unemployment rates, are held constant in our regressions.

The analysis in the remainder of the paper proceeds as follows. Section 5 develops and estimates a duration model that uses all the available information on durations and takes account of the peculiar structure of CPS duration data. Section 6 then develops an extension of this model that incorporates both length-biased sampling and endogenous selection into internet search. Section 7 discusses the results of estimating this model, which addresses all three of the above econometric concerns simultaneously.

5. A Univariate Duration Model

We begin, as is common, by assuming the hazard rate into re-employment, $\lambda(\tau)$, is separable into a baseline component that depends on elapsed duration

$\lambda_0(\tau)$, and a component that depends on a linear combination of observed characteristics X_i and estimated parameters β:

$$\lambda(\tau) = \lambda_0(\tau) \cdot \exp(-X_i\beta) \tag{1}$$

From assumption (1) it follows that (see Kiefer 1988, pp. 664-665):

$$\log \Lambda_0(t_i) = X_i\beta + \mu_i \tag{2}$$

where the random variable $\Lambda_0(t_i)$ is the integrated baseline hazard up to each observation's realized duration, i.e.:

$$\Lambda_0(t_i) = \int_0^{t_i} \lambda_0(\tau) d\tau \tag{3}$$

and where μ_i follows a type-1 extreme-value distribution.[18] Thus the transformed duration variable, $\log \Lambda_0(t_i)$,--which is monotonically increasing in t_i-- can be thought of as the dependent variable in a linear regression.

Suppose now that a particular unemployment spell is known to have ended between two dates, $t_a < t_b$. Defining $\delta_a \equiv \log \Lambda_0(t_a)$ and $\delta_b \equiv \log \Lambda_0(t_b)$, the likelihood of such a spell is just:

$$F(\delta_a - X_i\beta) - F(\delta_b - X_i\beta), \tag{4}$$

where F(A) is the cdf of μ_i. Durations known only to have ended after, say, t_a (i.e. right-censored durations) have a likelihood of $1 - F(\delta_a - X_i\beta)$; durations

[18] The cdf for the extreme-value distribution is given by $F(\mu_i) = \exp(-\exp(-\mu_i))$

known to have ended between $t=0$ and, say, t_b, have a likelihood of $F(\delta_b - X_i\beta)$.[19]

In our data, job searchers are observed no more frequently than once per month. Recognizing this discreteness, we divide the set of possible jobless durations into disjoint intervals.[20] Denote the number of such intervals by T+1; in the results reported in Table 5 (which focus on post-Supplement durations only), we used eight intervals: 0-1, 1-2, 2-3, 3-10, 10-11, 11-12, 12-13 and more than 13 months. For some of our observations (for example those persons observed as unemployed in one month and employed the next), we know in exactly which of these intervals their unemployment spell ended. Others are right-censored, due to attrition or rotation out of the sample. For yet others (including, but not limited to, persons who were not matched in a period before they are first observed in employment) we know only that they became employed at some point within a set of adjacent intervals.

To allow for the latter types of observations, define $\underline{V_i}$ as a 1xT vector of "lower bound" dummy variables (think of these as applying, in order, to each of the T+1 intervals defined above except the highest one). Set $\underline{V_i}$ equal to zero for all intervals except the one *preceding* the interval in which worker i's

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[19] Unlike observed durations which must be positive, note that the transformed durations and the error term μ_i occupy the entire real line.

[20] Appendix B describes how we constructed unemployment durations from the matched CPS files.

unemployment spell is known to have ended.[21] Define $\overline{V}_i$ as a 1xT vector of upper bound dummy variables, equal to zero for all intervals except the one *during which* we knew the unemployment spell ended.[22] Finally, let δ be a Tx1 coefficient vector corresponding to the "cut points" between the above intervals. Because the elements of δ correspond to the log of the integrated baseline hazard at the upper end of each interval, and because δ is estimated, this procedure allows for an unrestricted baseline hazard function.

Putting all the above together, the log likelihood for the entire sample can be expressed as:

$$\begin{aligned} \log L = & \sum_{Cens=L} \log\left[F(\overline{V}_i\delta - X_i\beta)\right] + \\ & \sum_{Cens=0} \log\left[F(\overline{V}_i\delta - X_i\beta) - F(\underline{V}_i\delta - X_i\beta)\right] + \\ & \sum_{Cens=R} \log\left[1 - F(\underline{V}_i\delta - X_i\beta)\right]. \end{aligned} \tag{5}$$

where $Cens$ = L, 0 and R indicates the observation is left-censored, not censored, or right-censored, respectively. (Note that we refer to observations that became re-employed in the first month of their unemployment spell as left-censored because the transformed duration variable, $\log \Lambda_0(t_i)$, has no lower bound for this group).

While the derivation leading to (5) relies on F having an extreme value distribution, in Table 5 we present estimation results based on a normal

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[21] If the observation is right-censored this is the interval before it became right-censored; if the observation became re-employed during the first interval $\underline{V}_i$ is a vector of zeroes

[22] If the observation is right-censored, $\overline{V}_i$ is a vector of zeroes.

distribution for F as well. This ordered probit-type specification does not follow directly from the proportional-hazards specification in (1), but yields predicted durations (both with and without internet search) that are very similar to those obtained from the extreme-value specification.[23] The value of the ordered-probit specification is that it allows us to model correlation between the disturbance term (μ_i) in our unemployment duration equation (2), and unobserved characteristics that induce unemployed individuals to look for work on line in the following section.

As in Table 4, Table 5 presents specifications with and without controls for home internet access. (Note that because the index $X_i\beta$ enters equation (1) negatively, a positive coefficient in Table 5 indicates that the variable in question reduces the hazard rate, i.e. it increases expected unemployment duration). Recalling also that our estimation framework so far continues to treat pre-Supplement unemployment duration as an exogenous covariate, Table 5 shows that persons who are far into their unemployment spells (i.e. with high retrospective durations in the Supplement month) have longer remaining unemployment durations after that date. Durations were lower in the 2000 Supplement, reflecting the tighter aggregate labor market conditions prevailing around the time of that survey. High state unemployment rates raise

[23] Predicted survivor curves for all four specifications in Table 4 are available from the authors. The normal and extreme-value based curves are essentially indistinguishable from each other. The likely reason why functional form is of so little consequence is that our specification allows for an unrestricted baseline hazard: moving the "cut-points" for the two distributions gives us a large number of degrees of freedom with which to fit observed transition patterns.

unemployment durations. As in Table 4, younger workers have shorter unemployment durations and older workers remain unemployed longer. One interesting difference from Table 4 is that persons with home internet access now have significantly shorter jobless durations. The most surprising finding from Table 5, however, is that according to the coefficient estimates, internet job search now appears to be not simply ineffective, but in fact significantly *counterproductive*. In other words, holding constant observable characteristics of the person and the previous duration of the unemployment spell, persons who searched for work on line actually entered re-employment more slowly than those who did not, during the period after we observe whether they search on line. While not proving that internet search is in fact counterproductive, these results certainly present a strong preliminary case against the argument that internet job search reduces unemployment durations. Of course, these results are also consistent with negative selection into internet search on unobservables, especially in the specification where internet access is held constant, where the estimated counterproductive effect is the strongest.

6. Length-Biased Sampling and Endogeneity of Internet Search: Methods

6.1. Length-Biased Sampling

We begin again with the linear-regression representation of the proportional hazard model in (2), but now re-interpret the duration variable t_i, as total spell duration *including* the retrospective component, t_i^R. Consider again a

spell observed in our data (thus it is known to have lasted at least t^R months), known to have ended between two dates, $t_a < t_b$ (where both t_a and t_b must exceed t^R). Thus the information provided by our data is that, conditional on lasting at least t^R months, this spell lasted between t_a and t_b months. Once again defining $\delta_a \equiv \log \Lambda_0(t_a)$ and $\delta_b \equiv \log \Lambda_0(t_b)$, and now $\delta^R \equiv \log \Lambda_0(t^R)$, the likelihood of such a spell is[24]:

$$\frac{F(\delta_a - X_i\beta) - F(\delta_b - X_i\beta)}{1 - F(\delta^R - X_i\beta)}. \tag{6}$$

Next, we divide up the set of possible *total* durations (including the retrospective portion) into T+1 intervals and define the 1xT lower- and upper-bound vectors $\underline{V_i}$ and $\overline{V}$ as before for these spell durations.[25] Lastly, define the 1xT vector V_i^R as equal to zero for all intervals except the one *preceding* the supplement month. (Thus, for example, with month-long intervals, a worker who became unemployed in September 1998 has the third --November 1998-- element of V_i^R set to one and the rest to zero).

Parallel to (6), the log likelihood for the entire sample, corrected for length-biased sampling, can now be written:

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[24] This assumes a constant inflow rate into unemployment before the Supplement month.

[25] In the results reported here we used 22 intervals for these total durations. With the exception of months 7 and 8 (which were combined due to small sample sizes) these comprised individual months up to 16. Beyond that, the categories were 16-22, 23-26, 27, 28, 29-37, and over 38 months.

$$\log L = \sum_{Cens=L} \log\left[F(\overline{V}_i\delta - X_i\beta)\right] + \sum_{Cens=0} \log\left[\frac{F(\overline{V}_i\delta - X_i\beta) - F(\underline{V}_i\delta - X_i\beta)}{1 - F(V_i^R\delta - X_i\beta)}\right] + \sum_{Cens=R} \log\left[\frac{1 - F(\underline{V}_i\delta - X_i\beta)}{1 - F(V_i^R\delta - X_i\beta)}\right]. \quad (7)$$

Note that because all "left-censored" spells in this context are new spells, no length-biased sampling correction applies to them.

Of course, parameter estimates in (7) have a different interpretation than in (5): they refer to the effect of each covariate on *total* unemployment durations (including the retrospective portion) rather than on post-Supplement durations. In the interpretation of both (5) and (7), note again that –given the absence of internet search data at any dates other than the Supplement month—we treat and interpret internet search, like all our other covariates, as a non-time-varying characteristic of the unemployment spell.[26]

6.2. Endogeneity

Finally, to incorporate the possible endogeneity of internet job search into our model, we rewrite (2) as:

$$\log \Lambda_0(t_i) = X_i\beta + y_i\gamma + \mu_i \quad (8)$$

[26] Any other treatment of internet search would require data on internet search activity at more than one point during an unemployment spell, which is currently not available

where y_t (previously included in X_t) is the internet search dummy, with coefficient γ.

Now define the difference between the marginal benefit and marginal cost of internet job search as the latent variable:

$$y_t^* = W_t\theta + \varepsilon_t \ . \qquad (9)$$

where W_t is a vector of exogenous, non-time varying covariates, X_t, plus a set of instrumental variables excluded from X_t. The latent variable y_t^* is not observed, but instead we observe:

$$y_t = \begin{cases} 1 & if \ \ y_t^* > 0 \\ 0 & otherwise \end{cases} .$$

Our concern is that ε_t in (9) may be correlated with μ_t in (8). This leads to bias, e.g., in the estimate of γ in (8) because the endogenous variable, y_t, is correlated with the error term μ_t.

Unfortunately, we are aware of no widely-accepted technique for estimating hazard models with an endogenous covariate. The difficulty, essentially, is modeling the joint distribution of ε_t and μ_t when μ_t is non-normal.[27] Our approach is therefore to extend the "ordered-probit" version of (7) to the bivariate case, where standard bivariate normal results can be used to model the joint

[27] One possibility would be to follow Heckman and Singer's (1984) treatment and model unobserved heterogeneity in both the unemployment and search equations as following a discrete distribution (with, say, four mass points). We prefer our continuous formulation because it does not impose a discrete – which in practice generally amounts to two-point— distribution on the unobserved heterogeneity that is correlated between the two equations.

distribution of ε_i and μ_i. With the exception of the interval nature of our duration measure and the correction for length-biased sampling, our approach is similar to Greene's (1998) bivariate probit model with an endogenous dummy variable.[28]

To extend the model in (7) to the case where ε_i and μ_i have a joint normal distribution with (potentially) non-zero correlation, note first that an observation will be in our sample iff:

$$t_i > t^R$$

or, $$\log \Lambda_0(t_i) > \delta^R$$

or, $$\mu_i > \delta^R - X_i\beta - y_i\gamma$$

or, $$\mu_i > \delta^R - X_i\beta - I(W_i\theta + \varepsilon_i)\gamma \qquad (10)$$

where the indicator function, $I()$, returns 1 if $y_i^* > 0$ and 0 otherwise. Thus (for the bivariate normal case) the likelihood of being in the sample is given by:

$$\Phi_2(z_0, -z_{11}, \rho) + \Phi_2(-z_0, -z_{10}, -\rho) \qquad (11)$$

where:

$$z_0 = W_i\theta$$

$$z_{11} = V_i^R\delta - X_i\beta - \gamma$$

$$z_{10} = V_i^R\delta - X_i\beta$$

[28] See also Greene (2000), pp. 852-856

and Φ_2 is the standard bivariate normal cdf with correlation ρ. By adjusting the denominator in (7) to reflect this new condition, and the numerator in (7) to account for the joint distribution of μ_i and ε_i, we can obtain an unbiased estimate of γ. To express the bivariate likelihood function, first define:

$$q = 2y - 1$$

(thus $q = 1$ when $y = 1$, and $q = -1$ when $y = 0$). The complete likelihood function for this model can then be written:

$$L = \sum_{Cens=1} \log\left[\Phi_2(q z_0, z_3, -q\rho)\right]$$
$$\sum_{Cens=0} \log\left[\frac{\Phi_2(q z_0, z_3, -q\rho) - \Phi_2(q z_0, z_2, -q\rho)}{\Phi_2(z_0, -z_{11}, \rho) + \Phi_2(-z_0, -z_{10}, -\rho)}\right] + \qquad (12)$$
$$\sum_{Cens=R} \log\left[\frac{\Phi_2(q z_0, -z_2, q\rho)}{\Phi_2(z_0, -z_{11}, \rho) + \Phi_2(-z_0, -z_{10}, -\rho)}\right].$$

where:

$z_2 = \underline{V}_i\delta - X_i\beta - y_i\gamma$, and

$z_3 = \overline{V}_i\delta - X_i\beta - y_i\gamma$.

6. Endogeneity of Internet Search and Length-Biased Sampling: Results

Results from maximizing the likelihood function in equation (12) are presented in Table 6. In all, six specifications are reported, each of which contains two equations: a "search" equation for the determinants of internet search, and an unemployment duration equation. The first two columns present estimates of the entire model, but with the correlation (ρ) between the search and duration error

terms (μ_i and ε_i) constrained to equal zero. Essentially, this amounts to introducing the correction for length-biased sampling introduced in Section 5(a), but not that for endogenous internet search in Section 5(b). As in previous tables, we present a version that includes controls for home internet access (in both the search and duration equations) and one that does not. To economize on space we report only the internet search and internet access coefficients in the duration equation, and the estimated correlation between the error terms, ρ.[29]

The main message of columns 1 and 2 of Table 6 is that length-biased sampling alone cannot account for our estimated "counterproductive" search effects. While the internet search effect on durations is now insignificant in the absence of an internet access control (see column 1), column 2 shows that this is due to two offsetting effects: (1) unemployed individuals with home internet access have shorter unemployment durations, whether they search on line or not; and (2) among the unemployed with access, those who use the internet to look for work actually have longer unemployment durations than those who do not. Whether these longer durations are a perverse causal effect of internet search or an artifact of selection cannot be determined from columns 1 and 2; to address that question we must turn to the estimates in the remainder of the Table.

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[29] Complete results for the four specifications with ρ unrestricted are provided in Appendix C, results for the "control" variables in the two remaining specifications are very similar. Note also that when ρ=0, the instruments used in the search equation should not, and do not affect our estimates of the duration equation. Thus Table 6 does not present separate estimates for the two instrument sets when ρ=0.

As noted, columns 3 through 6 of Table 6 relax the $\rho=0$ constraint. Columns 3 and 4 present estimates of the duration equation when instrument set 1 –non-home internet use by other household members—is used; columns 5 and 6 use state means of internet access costs and internet diffusion as instruments. Three features are noteworthy: first, all the positive estimated effects of internet search on durations disappear; thus internet job search no longer appears to be counterproductive. Instead (point two), the model prefers to attribute the positive partial correlation between internet search and unemployment durations to what we have been calling negative selection: all our estimates of ρ are positive, indicating that unemployed individuals who have a higher unexplained disposition to look for work on line tend to have longer unemployment durations. Third, however, neither the estimate of ρ nor the estimated effect of internet search on unemployment durations is significantly different from zero. Recalling the remaining expected bias of both our instrument sets *towards* finding a beneficial effect of internet search, we read these results as reaffirming the notion, already suggested by the simple probit models of Table 4, that internet job search is ineffective in reducing unemployment durations.

7. Summary

This paper has examined the effects of internet job search on the job-finding rates of unemployed US workers. After accounting for both observable *and* unobservable differences between internet searchers and other unemployed

workers, for the large gaps in measured unemployment durations induced by the CPS rotation structure, and for the length-biased sampling problem inherent in data collected from the stock of unemployed workers at a point in time, our estimates suggest the following. First, unemployed internet searchers are positively selected on *observables*: i.e. compared to other unemployed workers, they have measured characteristics such as education and internet access which are associated with faster re-employment whether or not the internet is used. Second, internet searchers appear to be negatively selected on *unobservables*, i.e. they have unobservable characteristics (such as, for example, private information about their re-employability) that are associated with longer unemployment spells. Finally, we are unable to detect any statistically significant beneficial causal effect of internet job search on re-employment rates.

If internet job search does not reduce unemployed workers' jobless durations, what factors might explain the ineffectiveness of such a highly-touted technical innovation in labor markets? One possibility, of course, is that workers do benefit from the new technology, but that they "consume" the entire benefit of this technological advance in the form of better job matches. While we cannot directly test this hypothesis here, it strikes us as unlikely. For example, if it were true, one would not expect better-educated workers (who have a better set of job offers to choose from) to have shorter unemployment durations than less-educated workers; yet this relationship is very well documented (see for example Mincer 1991). If educated workers "consume" at least some of their expanded budget set

in the form of shorter unemployment durations (rather than just better jobs alone) one might reasonably expect workers who look for work on line to do the same, if internet search truly offers an advantage.

Alternatively, internet matching technology may have great potential for labor markets, but given the difficulty of communicating some key dimensions of match quality (including applicant personality and corporate culture, factors which many recruiters cite as a key dimension of applicant "fit") on line, those benefits may not yet have been realized. Finally, perhaps the internet does not even offer the potential for a major change in job search outcomes. One of its major advantages, a substantial reduction in use costs to both employers and workers, may not be that important if the *initial* cost of these services was small in magnitude. The other major advantage --easier access to information about jobs in other cities---may simply not be relevant to most workers. Still, while the jury is still out on the long-run effects of internet technology on labor market matching, the assertion that the US "employment miracle" of the late 1990's (e.g. Katz and Krueger 1999) and early 2000's might in part explained by the "wiring of the labor market", seems highly premature based on our results here.

Table 1: Fraction of persons with internet access and engaging in internet job search, by labor force status, December 1998 and August 2000.

	Fraction with home internet access		Fraction looking for work on line		Fraction looking for work on line, given home internet access[1]	
	1998	2000	1998	2000	1998	2000
Employed						
- at work	.347	.521	0.071	0.113	0.159	0.183
- absent	.339	.611	0.070	0.105	0.166	0.151
Unemployed						
- on layoff	.165	.396	0.048	0.103	0.176	0.207
- jobseeker	.223	.394	0.150	0.255	0.495	0.541
Not in LF						
- retired	.122	.238	0.003	0.005	0.023	0.021
- disabled	.105	.204	0.014	0.022	0.104	0.097
- other	.319	.465	0.038	0.063	0.090	0.117
Total	.294	.457	0.055	0.089	0.146	0.165

Note: 1. Does not equal the ratio of previous columns because some individuals without home internet access search on line.

Table 2: Sample means by internet search activity.

	Internet Search		Total
	Yes	No	
Retrospective duration	3.440	3.749	3.684*
2000 supplement	0.637	0.477	0.510*
On layoff	0.107	0.093	0.096
State unemployment rate	4.312	4.370	4.358
Occupational unemployment rate	3.681	4.723	4.506*
Worked prior to unemployment	0.619	0.507	0.530*
School prior to unemployment	0.208	0.215	0.213
Lost job	0.323	0.240	0.258*
Temporary job	0.115	0.117	0.117
Private sector	0.792	0.794	0.794
Public sector	0.115	0.070	0.079*
Self-employed	0.047	0.034	0.036
Age 16-25	0.302	0.408	0.386*
Age 26-35	0.240	0.211	0.217
Age 36-45	0.219	0.199	0.203
Age 46-55	0.180	0.108	0.123*
Male	0.484	0.498	0.495
Married	0.421	0.302	0.326*
Male and married	0.203	0.135	0.150*
Spouse employed	0.307	0.213	0.233*
Primary school	0.006	0.072	0.058*
Incomplete high school	0.098	0.296	0.255*
Completed high school	0.241	0.368	0.342*
Incomplete college	0.234	0.139	0.158*
Associate degree	0.084	0.039	0.048*
Black	0.117	0.210	0.191*
Hispanic	0.079	0.168	0.149*
Home owner	0.602	0.515	0.533*
Immigrant	0.100	0.133	0.126*
Contacted employer directly	0.653	0.643	0.645
Contacted public employment agency	0.250	0.191	0.203*
Contacted private employment agency	0.116	0.057	0.069*
Contacted friends or relatives	0.151	0.128	0.133
Contacted school employment center	0.044	0.022	0.027*
Sent resumes / filled applications	0.603	0.456	0.487*

Checked union/professional registers	0.033	0.018	0.021*
Placed or answered ads	0.221	0.120	0.141*
Other active search method	0.099	0.038	0.051*
Number of traditional search methods	2.171	1.674	1.777*
Spouse uses internet away from home	0.131	0.044	0.062*
Child uses internet away from home	0.064	0.058	0.059
Parent uses internet away from home	0.067	0.066	0.066
Sibling uses internet away from home	0.045	0.053	0.052
Other uses internet away from home	0.098	0.045	0.056*
State-level mean access rate	0.469	0.430	0.438*
State-level mean access cost	17.855	17.874	17.870
State-level long-distance incidence	0.043	0.045	0.045*
Internet access at home	0.801	0.202	0.326*
Number of months observed	2.805	2.611	2.651*

Note: * indicates if means are statistically different at a 5% significance level which is obtained by regressing each variable on a constant and the internet search dummy variable. Sample sizes are 860 internet searchers and 3279 non-internet searchers.

Table 3: Percent of unemployed sample observed in employment in subsequent months by internet search activity.

	Internet Search		Total
	Yes	No	
Employed in the month following the Computer/Internet Supplement (share of persons observed at that date)	0.298	0.289	0.291
Employed 2 months after Computer/Internet Supplement (share of persons observed at that date)	0.413	0.365	0.375
Employed 12 months after Computer/Internet Supplement (share of persons observed at that date)	0.646	0.533	0.559*
Observed in Employment, in any post-supplement month (share of all observations)	0.614	0.545	0.559*

Notes: Overall sample size is 860 internet searchers and 3279 non-internet searchers. * indicates if means are statistically different at a 5% significance level. This is obtained by regressing each variable on a constant and the internet search dummy variable.

Table 4: Probit estimates of the probability of being employed in 12 months.

	Home Internet Access Control							
	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net search	0.291*	0.230*	0.193*	0.140	0.062	0.035	0.031	-0.005
	(0.083)	(0.099)	(0.089)	(0.103)	(0.095)	(0.107)	(0.097)	(0.109)
Retrospective duration			-0.030*	-0.030*	-0.027*	-0.027*	-0.029*	-0.029*
			(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
2000 supplement			-0.111	-0.119	-0.100	-0.105	-0.108	-0.115
			(0.074)	(0.074)	(0.075)	(0.076)	(0.076)	(0.077)
On layoff			0.337*	0.336*	0.328*	0.328*	0.316*	0.315*
			(0.133)	(0.133)	(0.135)	(0.135)	(0.138)	(0.137)
State ur			0.036	0.038	0.040	0.041	0.034	0.034
			(0.037)	(0.038)	(0.039)	(0.039)	(0.039)	(0.040)
Occupation ur			-0.064*	-0.062*	-0.044*	-0.044*	-0.044*	-0.043*
			(0.019)	(0.019)	(0.021)	(0.021)	(0.021)	(0.021)
Worked before u			0.418*	0.420*	0.414*	0.415*	0.418*	0.420*
			(0.119)	(0.119)	(0.122)	(0.122)	(0.123)	(0.123)
School before u			0.261*	0.250*	0.271*	0.266*	0.258*	0.251*
			(0.108)	(0.108)	(0.121)	(0.121)	(0.122)	(0.122)
Lost job			0.019	0.018	0.027	0.027	-0.010	-0.010
			(0.120)	(0.121)	(0.123)	(0.123)	(0.125)	(0.125)
Temporary job			-0.220	-0.215	-0.195	-0.191	-0.246	-0.241
			(0.140)	(0.140)	(0.142)	(0.142)	(0.145)	(0.145)
Private sector			0.494*	0.489*	0.422*	0.419*	0.440*	0.437*
			(0.142)	(0.142)	(0.148)	(0.148)	(0.149)	(0.149)

Public sector	0.188	0.181	0.089	0.087	0.132	0.130
	(0.186)	(0.186)	(0.195)	(0.195)	(0.196)	(0.196)
Self-employed	0.199	0.188	0.153	0.151	0.219	0.216
	(0.231)	(0.232)	(0.241)	(0.241)	(0.242)	(0.243)
Age 16-25			0.571*	0.572*	0.584*	0.585*
			(0.160)	(0.160)	(0.161)	(0.161)
Age 26-35			0.466*	0.471*	0.443*	0.450*
			(0.153)	(0.154)	(0.155)	(0.156)
Age 36-45			0.507*	0.511*	0.511*	0.516*
			(0.149)	(0.149)	(0.151)	(0.151)
Age 46-55			0.242	0.243	0.241	0.243
			(0.155)	(0.155)	(0.157)	(0.157)
Male			-0.187*	-0.190*	-0.175	-0.179
			(0.094)	(0.095)	(0.095)	(0.095)
Married			0.032	0.027	0.031	0.024
			(0.151)	(0.151)	(0.153)	(0.153)
Married male			0.336*	0.339*	0.319*	0.322*
			(0.154)	(0.154)	(0.157)	(0.157)
Spouse employed			-0.081	-0.082	-0.077	-0.079
			(0.135)	(0.135)	(0.137)	(0.137)
Primary school			-0.295	-0.286	-0.264	-0.251
			(0.204)	(0.205)	(0.207)	(0.208)
Incomplete high			-0.443*	-0.439*	-0.414*	-0.407*
			(0.144)	(0.144)	(0.147)	(0.147)

Complete high	-0.200	-0.190	-0.170	-0.160
	(0.126)	(0.127)	(0.129)	(0.130)
Incomplete college	-0.087	-0.083	-0.061	-0.055
	(0.136)	(0.136)	(0.138)	(0.138)
Associate degree	0.153	0.157	0.171	0.177
	(0.187)	(0.187)	(0.190)	(0.190)
Black	-0.267*	-0.260*	-0.280*	-0.272*
	(0.095)	(0.095)	(0.096)	(0.097)
Hispanic	0.008	0.016	0.007	0.017
	(0.114)	(0.115)	(0.116)	(0.117)
Home owner	0.071	0.067	0.077	0.071
	(0.078)	(0.079)	(0.079)	(0.080)
Immigrant	0.011	0.008	0.057	0.051
	(0.117)	(0.117)	(0.118)	(0.118)
Contact employer			0.159*	0.160*
			(0.079)	(0.079)
Contact public ea			0.257*	0.262*
			(0.097)	(0.098)
Contact private ea			0.247	0.249
			(0.153)	(0.153)
Contact friend/relative			-0.146	-0.143
			(0.111)	(0.111)
Contact school ec			-0.245	-0.243
			(0.220)	(0.220)
Sent resumes			0.220*	0.220*
			(0.077)	(0.077)

Check union							-0.139	-0.142
							(0.292)	(0.292)
Used ads							-0.158	-0.157
							(0.108)	(0.108)
Other active							0.281	0.284
							(0.179)	(0.180)
Constant	0.083*	0.064	-0.297	-0.326	-0.517	-0.531	-0.744*	-0.766*
	(0.039)	(0.043)	(0.244)	(0.246)	(0.308)	(0.309)	(0.320)	(0.322)
Net access		0.099		0.092		0.052		0.069
		(0.087)		(0.090)		(0.096)		(0.097)
Log likelihood	-916.12	-915.47	-862.26	-861.74	-840.15	-840.00	-827.22	-826.97

Note: Standard errors are in parentheses. * indicates significance at the 5% level. The sample size for all specifications is 1344.

Table 5: Ordered Extreme-Value and Ordered-Probit Models of Unemployment Duration.[1]

	Ordered Extreme-Value		Ordered Probit	
Access Control	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Internet search	0.170*	0.309*	0.144*	0.269*
	(0.074)	(0.086)	(0.067)	(0.077)
Retrospective duration	0.030*	0.030*	0.030*	0.030*
	(0.005)	(0.005)	(0.005)	(0.005)
2000 Supplement	-0.310*	-0.280*	-0.244*	-0.215*
	(0.061)	(0.062)	(0.054)	(0.055)
On layoff	-0.186	-0.183	-0.155	-0.154
	(0.097)	(0.097)	(0.087)	(0.087)
State ur	0.083*	0.088*	0.077*	0.082*
	(0.031)	(0.031)	(0.028)	(0.028)
Occupation ur	0.005	0.002	0.009	0.007
	(0.016)	(0.016)	(0.015)	(0.015)
Worked before u	0.005	-0.014	0.022	0.007
	(0.099)	(0.099)	(0.086)	(0.086)
School before u	0.097	0.093	0.079	0.082
	(0.102)	(0.102)	(0.091)	(0.091)
Lost job	0.200*	0.212*	0.174*	0.180*
	(0.093)	(0.093)	(0.083)	(0.083)
Temporary job	0.026	0.021	0.031	0.026
	(0.112)	(0.113)	(0.098)	(0.098)
Private sector	-0.090	-0.067	-0.112	-0.087
	(0.134)	(0.134)	(0.121)	(0.122)
Public sector	-0.360*	-0.343*	-0.319*	-0.301*
	(0.170)	(0.171)	(0.151)	(0.152)
Self-employed	-0.355	-0.327	-0.396*	-0.372*
	(0.207)	(0.207)	(0.180)	(0.181)
Age 16-25	-0.374*	-0.359*	-0.347*	-0.337*
	(0.135)	(0.135)	(0.123)	(0.124)
Age 26-35	-0.306*	-0.312*	-0.291*	-0.302*
	(0.130)	(0.130)	(0.119)	(0.120)
Age 36-45	-0.199	-0.200	-0.184	-0.188
	(0.128)	(0.128)	(0.117)	(0.118)
Age 46-55	-0.008	-0.014	0.000	-0.004
	(0.132)	(0.132)	(0.123)	(0.123)

Male	0.028	0.052	0.025	0.041
	(0.074)	(0.075)	(0.066)	(0.067)
Married	0.044	0.063	0.052	0.067
	(0.124)	(0.124)	(0.110)	(0.110)
Married male	-0.169	-0.205	-0.153	-0.173
	(0.124)	(0.124)	(0.111)	(0.111)
Spouse employed	0.036	0.050	0.019	0.028
	(0.110)	(0.111)	(0.098)	(0.098)
Primary school	-0.005	-0.071	0.009	-0.052
	(0.169)	(0.170)	(0.149)	(0.151)
Incomplete high	0.092	0.041	0.082	0.036
	(0.115)	(0.116)	(0.104)	(0.105)
Complete high	0.003	-0.055	-0.002	-0.050
	(0.098)	(0.100)	(0.088)	(0.089)
Incomplete college	-0.014	-0.050	-0.012	-0.041
	(0.107)	(0.108)	(0.095)	(0.096)
Associate degree	0.019	-0.029	-0.004	-0.049
	(0.143)	(0.144)	(0.128)	(0.129)
Black	0.143	0.114	0.155*	0.126
	(0.080)	(0.080)	(0.073)	(0.073)
Hispanic	0.181	0.149	0.153	0.116
	(0.100)	(0.101)	(0.088)	(0.089)
Home owner	-0.027	0.004	-0.014	0.012
	(0.062)	(0.062)	(0.055)	(0.056)
Immigrant	-0.229*	-0.217*	-0.236*	-0.223*
	(0.109)	(0.110)	(0.094)	(0.094)
Contact employer	-0.002	-0.004	-0.016	-0.018
	(0.063)	(0.063)	(0.057)	(0.057)
Contact public ea	0.188*	0.166*	0.144*	0.127*
	(0.070)	(0.071)	(0.065)	(0.065)
Contact private ea	-0.085	-0.095	-0.052	-0.059
	(0.109)	(0.109)	(0.097)	(0.097)
Contact friend/relative	0.149	0.137	0.148	0.136
	(0.083)	(0.083)	(0.077)	(0.077)
Contact school ec	0.070	0.069	0.015	0.018
	(0.170)	(0.170)	(0.151)	(0.151)
Sent resumes	0.143*	0.138*	0.101*	0.097*
	(0.061)	(0.061)	(0.054)	(0.054)
Check union	0.395*	0.398*	0.322*	0.317
	(0.171)	(0.172)	(0.164)	(0.165)
Used ads	0.049	0.050	0.041	0.042
	(0.080)	(0.080)	(0.074)	(0.074)

Other active	-0.074	-0.062	-0.104	-0.087
	(0.137)	(0.137)	(0.119)	(0.119)
Net access		-0.263*		-0.239*
		(0.082)		(0.071)
Log likelihood	-2026.83	-2021.58	-2024.45	-2018.82

Notes: 1. Following the specification in equations (1) and (2), and in contrast to Table 4, a positive coefficient now means the variable in question is associated with a *longer* duration of unemployment. The sample size for all specifications is 4139.

Table 6: Bivariate lognormal duration models with length-biased sampling correction.

	No endogeneity correction (ρ=0)		With endogeneity correction (ρ unconstrained) Instrument Set 1		Instrument Set 2	
	(1)	(2)	(3)	(4)	(5)	(6)
Net search	0.051	0.184*	-0.303	-0.036	-0.147	-0.309
	(0.050)	(0.057)	(0.426)	(0.466)	(0.696)	(0.425)
Net access		-0.202*		-0.118		-0.004
		(0.083)		(0.202)		(0.201)
Rho	0	0	0.207	0.127	0.111	0.282
			(0.240)	(0.262)	(0.401)	(0.223)
Log likelihood	-3784.89	-3463.61	-3784.65	-3463.49	-3795.74	-3477.02

Note: Standard errors are in parentheses. * indicates significance at the 5% level.
The sample size for all specifications is 4139.

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Appendix

A. Construction of Matched CPS Sample

To capture as much information as possible, each of post-Supplement file was matched separately with its corresponding Supplement file. Below we describe the four steps involved in matching the December 1998 Supplement with the ten subsequent CPS files that contain common survey respondents. The procedure for the August 2000 file was identical.

1. Construct the *master data set* by extracting the 3 merging variables: (i) the household identifier (HRHHID), (ii) the individual line number with in the household (PULINENO) and (iii) the household number (HUHHNUM), the labor force status variable (PEMLR) and a set of non-time varying covariates for each observation from the raw December 1998 CPS file. Also, create the set of instrumental variables by using HRHHID, PULINENO, spouse line number (PESPOUSE) and parent line number (PEPARENT) to reshape the data set and identify relationships within households. The resulting master data set contains 122,935 observations.

2. Create the 10 *matching data sets* by extracting the 3 merging variables, in addition to the labor force status, sex (PESEX), age (PRTAGE), race (PERACE) and person type (PRPERTYP) variables from the January 1999, February 1999, March 1999, September 1999, October 1999, November 1999, December 1999, January 2000, February 2000 and March 2000 raw CPS files. For each using data set use the month-in-sample variable (HRMIS) to limit the sample to those observations that potentially appear in the December 1998 file. The resulting "matching" data sets contain the following sample sizes:

January 1999	93,129
February 1999	61,979
March 1999	30,917
September 1999	14,961
October 1999	30,541
November 1999	46,538
December 1999	61,831
January 2000	47,208
February 2000	31,569
March 2000	15,710

3. Merge the master data set with each of the 10 matching data sets using the 3 merging variables discarding all observations that appear only in the matching data set. This produces 10 files each containing 122,935

observations. Then merge these 10 files producing a single *merged data set* containing 122,935 observations.

4. Following the algorithm proposed by Madrian and Lefgren (1999), identify matches in the merged data set for which any of the following are true: (i) a month-to-month change in sex; (ii) a month-to-month change in race; (iii) the difference in age from month-to-month is less than −1 or greater than 3; or (iv) the individual went from being classified as an adult civilian to a child or as an adult, armed forces member to a child. In particular, we identify the survey month in which the implausible match was made. In creating our duration data we discard observations where an implausible transition occurs before an observation makes a transition to employment or out of the labor force (see Section B for the frequencies of these discards).

The percent of potential matches made from merging the master and matching data sets are tabulated below:

Month	Match Rate (%)
January 1999	91.7
February 1999	87.6
March 1999	83.2
September 1999	68.1
October 1999	67.1
November 1999	66.3
December 1999	64.9
January 2000	64.3
February 2000	62.9
March 2000	61.2

Only 10.4 percent of observations were not matched at least once after the Supplement date. The match rate for internet searchers and others were very similar. For example, in January 1999 the match rate for internet searchers is 93.6 compared to 91.5 for those not reporting internet search in the previous month. In order to more directly assess the possibility that our results might be driven by internet searchers who were not matched because they moved to take jobs, we replicated our entire analysis treating all individuals whose spells were censored due to a failure to match as becoming re-employed in the month following the censoring. There was very little change.

B. Construction of Duration Data

The data set containing the unemployment duration information was constructed in the following seven steps (again the description focuses for concreteness on the December 1998 supplement; procedures for August 2000 were identical).

1. Extract the sample of observations from the merged data set described in Section A that were unemployed and actively searching in December 1998. The resulting data set contains 2,027 observations.

2. Create variables ETRANSIT and NTRANSIT which indicate the months that the first employment or out of the labor force transition respectively was observed. Observations with missing values for both of these variables were never observed making a transition from the unemployed state. For these observations create the variable LASTU indicating the last month in which the observation was observed in the unemployed state. These 3 variables are tabulated below:

Month	ETRANSIT	NTRANSIT	LASTU
December 1998	-	-	434
January 1999	325	403	156
February 1999	97	78	87
March 1999	31	17	36
September 1999	95	54	3
October 1999	61	23	0
November 1999	37	20	2
December 1999	29	13	4
January 2000	4	6	6
February 2000	2	1	0
March 2000	0	0	3
TOTAL	681	615	731

3. Construct the variable t1, which is the retrospective unemployment spell reported in December 1998. In the raw data this variable is reported in weeks so the following transformation is made:

   t1 = RAW VARIABLE / 4.

4. Construct the variable t2, which gives the lower bound of the post-December 1998 unemployment spell. For all the observations it is calculated by determining how many months after December 1998 the

observation was last seen unemployed. This number is added to 0.25, to allow us to take logarithms of observations that are never observed after December 1998, to produce t2.

5. Construct the variable t3, which gives the upper bound of the post-December 1998 unemployment spell. Since all the NTRANSIT and LASTU observations are right-censored, t3 only contains values for the ETRANSIT observations. It is calculated simply as the number of months after December 1998 that the employment transition is observed.

6. Construct the variables t21 and t31, which give the lower and upper bounds of the complete unemployment spells. They are calculated as:

   $$t21 = t2 + t1$$
   $$t31 = t3 + t1$$

7. For observations with a LASTU value of September 1999 or later the reported retrospective spell in the final month the observation is seen is used to insure that spells were continuous through the longitudinal gaps. If the retrospective spell is shorter than t2, the lower bound spell is adjusted by subtracting the retrospective spell plus 0.25 from t2. This adjustment is made to 14 observations. The same adjustment to t2 is made to observations with an NTRANSIT value of October, 1999 or later and an observed retrospective spell in the previous month. This adjustment is made to 18 observations. Finally, for observations with an ETRANSIT value of September, 1999 or later the reported retrospective spell in the previous month, if it is observed, is used to check for continuous unemployment spells. If the retrospective spell is shorter than t2, the lower bound spell is adjusted as above and t3 is changed to missing. This change was made to 14 observations.

C: Bivariate lognormal duration models with length-biased sampling correction, full results (ρ unconstrained).

	Internet search equation				Duration equation			
	No Access controls		Access controls		No Access controls		Access controls	
Instrument set:	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Net search					-0.303	-0.147	-0.036	-0.309
					(0.426)	(0.696)	(0.466)	(0.425)
2000 supplement	0.427*	0.296*	0.170*	0.231*	-0.409*	-0.429*	-0.418*	-0.405*
	(0.053)	(0.101)	(0.059)	(0.107)	(0.091)	(0.110)	(0.074)	(0.075)
On layoff	-0.103	-0.097	-0.109	-0.098	-0.186	-0.181	-0.176	-0.182
	(0.091)	(0.090)	(0.100)	(0.100)	(0.109)	(0.111)	(0.110)	(0.109)
State ur	0.030	0.040	0.010	0.030	0.100*	0.100*	0.100*	0.100*
	(0.027)	(0.028)	(0.030)	(0.031)	(0.035)	(0.036)	(0.035)	(0.035)
Occupation ur	-0.086*	-0.086*	-0.063*	-0.060*	0.005	0.008	0.008	0.005
	(0.016)	(0.016)	(0.017)	(0.017)	(0.020)	(0.023)	(0.019)	(0.019)
Worked before u	0.169*	0.180*	0.232*	0.232*	-0.047	-0.053	-0.059	-0.047
	(0.083)	(0.083)	(0.092)	(0.092)	(0.111)	(0.114)	(0.112)	(0.112)
School before u	0.330*	0.347*	0.294*	0.301*	0.168	0.156	0.154	0.161
	(0.087)	(0.087)	(0.094)	(0.093)	(0.122)	(0.131)	(0.119)	(0.118)
Lost job	0.079	0.070	0.078	0.085	0.247*	0.245*	0.245*	0.245*
	(0.081)	(0.080)	(0.091)	(0.091)	(0.106)	(0.107)	(0.106)	(0.106)
Temporary job	-0.053	-0.047	-0.010	-0.003	0.111	0.111	0.106	0.106
	(0.098)	(0.097)	(0.106)	(0.106)	(0.121)	(0.122)	(0.121)	(0.121)
Private sector	0.288*	0.284*	0.248*	0.259*	-0.156	-0.168	-0.158	-0.147
	(0.114)	(0.114)	(0.123)	(0.124)	(0.165)	(0.169)	(0.163)	(0.163)
Public sector	0.333*	0.320*	0.307*	0.307*	-0.316	-0.330	-0.324	-0.312
	(0.139)	(0.139)	(0.150)	(0.150)	(0.204)	(0.208)	(0.204)	(0.203)

Self-employed	0.392*	0.370*	0.410*	0.406*	-0.561*	-0.580*	-0.548*	-0.520*
	(0.167)	(0.169)	(0.181)	(0.183)	(0.248)	(0.252)	(0.247)	(0.247)
Age 16-25	0.494*	0.582*	0.411*	0.463*	-0.197	-0.222	-0.222	-0.197
	(0.125)	(0.123)	(0.137)	(0.136)	(0.176)	(0.190)	(0.169)	(0.167)
Age 26-35	0.432*	0.456*	0.431*	0.448*	-0.181	-0.200	-0.210	-0.180
	(0.118)	(0.118)	(0.130)	(0.131)	(0.168)	(0.180)	(0.167)	(0.166)
Age 36-45	0.291*	0.323*	0.252*	0.290*	-0.097	-0.110	-0.117	-0.098
	(0.118)	(0.116)	(0.129)	(0.130)	(0.164)	(0.170)	(0.164)	(0.162)
Age 46-55	0.365*	0.373*	0.345*	0.356*	0.194	0.181	0.176	0.195
	(0.122)	(0.120)	(0.133)	(0.134)	(0.169)	(0.176)	(0.170)	(0.168)
Male	-0.054	-0.047	-0.177*	-0.170*	0.018	0.019	0.026	0.023
	(0.065)	(0.064)	(0.070)	(0.070)	(0.083)	(0.084)	(0.084)	(0.084)
Married	0.166	0.155	-0.062	-0.072	0.086	0.078	0.086	0.089
	(0.107)	(0.105)	(0.119)	(0.118)	(0.143)	(0.145)	(0.142)	(0.141)
Married male	0.191	0.188	0.316*	0.311*	-0.143	-0.151	-0.163	-0.147
	(0.108)	(0.107)	(0.119)	(0.120)	(0.145)	(0.148)	(0.146)	(0.145)
Spouse employed	-0.107	-0.076	-0.097	-0.078	-0.069	-0.066	-0.065	-0.069
	(0.097)	(0.094)	(0.108)	(0.105)	(0.127)	(0.128)	(0.127)	(0.126)
Primary school	-1.580*	-1.629*	-1.078*	-1.175*	-0.146	-0.084	-0.105	-0.164
	(0.221)	(0.233)	(0.240)	(0.239)	(0.261)	(0.333)	(0.221)	(0.219)
Incomplete high school	-1.146*	-1.183*	-0.791*	-0.865*	-0.029	0.028	0.014	-0.045
	(0.100)	(0.110)	(0.108)	(0.107)	(0.206)	(0.283)	(0.167)	(0.162)
Complete high	-0.837*	-0.873*	-0.509*	-0.551*	-0.165	-0.119	-0.135	-0.177
	(0.079)	(0.082)	(0.087)	(0.086)	(0.172)	(0.239)	(0.137)	(0.133)

Incomplete college	-0.335*	-0.361*	-0.162*	-0.195*	-0.129	-0.107	-0.127	-0.148
	(0.081)	(0.081)	(0.089)	(0.088)	(0.136)	(0.161)	(0.129)	(0.126)
Associate degree	-0.261*	-0.272*	0.003	-0.004	-0.143	-0.124	-0.159	-0.167
	(0.111)	(0.111)	(0.124)	(0.123)	(0.178)	(0.191)	(0.175)	(0.173)
Black	-0.257*	-0.255*	0.042	0.029	0.221*	0.233*	0.220*	0.222*
	(0.072)	(0.074)	(0.080)	(0.082)	(0.101)	(0.107)	(0.097)	(0.097)
Hispanic	-0.206*	-0.218*	0.085	0.095	0.128	0.136	0.122	0.126
	(0.090)	(0.090)	(0.099)	(0.099)	(0.117)	(0.119)	(0.117)	(0.116)
Home owner	0.081	0.078	-0.155*	-0.184*	-0.034	-0.037	-0.025	-0.034
	(0.054)	(0.053)	(0.061)	(0.060)	(0.071)	(0.073)	(0.073)	(0.073)
Immigrant	-0.036	-0.020	-0.123	-0.088	-0.301*	-0.303*	-0.300*	-0.297*
	(0.091)	(0.091)	(0.099)	(0.099)	(0.128)	(0.129)	(0.129)	(0.129)
Con. employer	0.098	0.096	0.096	0.089	-0.032	-0.039	-0.0400	-0.033
	(0.054)	(0.054)	(0.059)	(0.059)	(0.075)	(0.077)	(0.074)	(0.074)
Con. public ea	0.199*	0.192*	0.349*	0.326*	0.172*	0.163	0.149	0.163
	(0.063)	(0.063)	(0.070)	(0.070)	(0.086)	(0.090)	(0.089)	(0.087)
Con private ea	0.296*	0.287*	0.294*	0.290*	0.089	0.074	0.066	0.083
	(0.092)	(0.092)	(0.102)	(0.101)	(0.124)	(0.133)	(0.124)	(0.121)
Con. friend/rel.	0.019	0.032	0.065	0.079	0.177	0.175	0.163	0.168
	(0.076)	(0.075)	(0.083)	(0.083)	(0.102)	(0.103)	(0.103)	(0.102)
Con. school cc	0.032	0.044	0.059	0.081	0.039	0.032	0.038	0.049
	(0.143)	(0.142)	(0.160)	(0.159)	(0.192)	(0.194)	(0.194)	(0.192)
Sent resumes	0.359*	0.358*	0.367*	0.374*	0.082	0.067	0.056	0.077
	(0.052)	(0.053)	(0.058)	(0.057)	(0.080)	(0.094)	(0.078)	(0.077)
Check union	0.063	0.062	0.032	0.052	0.278	0.277	0.274	0.273
	(0.168)	(0.168)	(0.187)	(0.186)	(0.211)	(0.213)	(0.212)	(0.211)

Used ads	0.297*	0.299*	0.336*	0.351*	0.128	0.115	0.115	0.139
	(0.068)	(0.069)	(0.076)	(0.076)	(0.102)	(0.114)	(0.104)	(0.102)
Other active	0.426*	0.429*	0.333*	0.356*	-0.039	-0.060	-0.065	-0.039
	(0.103)	(0.103)	(0.113)	(0.112)	(0.161)	(0.178)	(0.157)	(0.156)
Spouse use	0.243*		0.162					
	(0.101)		(0.112)					
Child use	-0.063		0.145					
	(0.109)		(0.117)					
Parent use	0.183		-0.089					
	(0.109)		(0.116)					
Sibling use	0.156		0.271*					
	(0.118)		(0.124)					
Other use	0.500*		0.547*					
	(0.098)		(0.106)					
State access rate		0.891*		-0.155				
		(0.453)		(0.495)				
State mean cost		-0.058*		-0.063*				
		(0.022)		(0.024)				
State long-distance		0.251		-0.527				
		(1.326)		(1.423)				
Constant	-1.498*	-0.788	-2.225*	-1.052*				
	(0.231)	(0.484)	(0.256)	(0.523)				
Net access			1.491*	1.490*			-0.118	-0.004
			(0.071)	(0.068)			(0.202)	(0.201)

Rho	0.207	0.111	0.127	0.282
	(0.240)	(0.401)	(0.262)	(0.223)
Log likelihood	-3784.65	-3795.74	-3463.49	-3477.02

Note: Standard errors are in parentheses. * indicates significance at the 5% level. The sample size for all specifications is 4139.

CHAPTER THREE

THE IMPACT OF SUNDAY SHOPPING DEREGULATION ON EMPLOYMENT AND HOURS OF WORK IN THE RETAIL INDUSTRY: EVIDENCE FROM CANADA[1]

1. Introduction

Over the past forty years a number of countries in the Western world have witnessed the dismantling of legislation that has historically, in some cases for hundreds of years, restricted business activity on the Christian day of Sabbath. The international trend toward Sunday shopping deregulation has been most extensive in North America, but is more recently showing signs of gaining momentum in Western Europe. In the United States a steady decline in the number of states that impose a general ban on all Sunday business activity began in the early 1960s so that by 1985 only 22 states still had general bans compared to 35 in 1961.[2] A similar decline began in Canada in the early 1980s and continued until 1998, when Newfoundland became the last province in the country to pass some form of deregulating legislation. In contrast, in Europe only Belgium, Luxembourg, Sweden and Spain had taken any formal steps to

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[2] The research by Burda and Weil (2001) on Sunday shopping restrictions, or "blue-laws" as they are known in the US, is the most recent complete collection of state legislation throughout the US. Using state legislative records they track regulatory regimes in all the US states between 1969 and 1993. Laband and Heinbuch (1987) contains a similar collection of state legislation, but their time-line ends in 1985. Price and Yandle (1987) focus exclusively variation in blue laws between US states in the years 1970 and 1984.

deregulate Sunday retail activity prior to the 1990s.[3] However, over the following decade England and Wales, the Netherlands and then Finland opted to relax their restrictions on Sunday shopping.[4] Furthermore, there is indication that France and Italy are similarly moving in the direction of deregulation.[5]

Reference to the popular press of these countries reveals that legislative changes have taken place amid contentious political and public debates about the costs and benefits of Sunday shopping. In fact, in some political jurisdictions opposition to Sunday shopping following a deregulating initiative has actually been strong enough to reinstate restrictions.[6] A common concern in all these debates is the expected labour demand impact of Sunday shopping. In particular, will retail firms satisfy their need for Sunday employment by increasing the weekly hours of existing employees or by hiring new workers? Or is it possible that deregulation has neither an hours nor an employment impact as labour demand is reduced during the rest of the week? Opponents and proponents of deregulation have often based their arguments on their expectations of these

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[3] Kajalo (1997) has done extensive research to collect information on the legality of Sunday retail business across Europe. This information is available from his website at http://www.hkkk.fi/talsos/internat.htm.

[4] England and Wales deregulated all Sunday shopping in 1994, while the Netherlands and Finland opted to permit Sunday shopping during part of the year in 1996 and 1997 respectively

[5] For the French case see "Government grasps Sunday law nettle" *International Management*, October 1991, pp.20-21 and for the Italian see "Open up!" *The Economist*, March 14, 1998, p.59

[6] In 1994 Spain moved from complete deregulation to only eight Sundays per year of unrestricted Sunday shopping (Kajalo 1997). Nova Scotia, Canada opted to entirely repeal their legislation following a three month experiment which proved unpopular with retailers (see "Sunday store openings not worth it: retailers" *Halifax Chronicle Herald*, November 26, 1993, p.B6). More recently, Norway has repealed the exemption of large supermarkets from its strict Sunday shopping legislation (see "Norway's Sunday best" *The Economist*, August 1, 1998, p 43) and Kajalo (1997) reports that in Finland there is evidence of growing support among citizens for renewed regulation.

labour demand effects.[7]

Despite the widespread debate in the popular press there is a dearth of empirical research examining the labour demand effects of deregulation.[8] In an early attempt to evaluate the economic impact of Sunday shopping, Kay et al. (1985) use consumer surveys to predict what impact deregulation will have on retail sales in England. This estimate is then combined with an assumed labour demand elasticity with respect to sales to predict a short run increase in overall retail employment of 5 percent.[9] More recently, Gradus (1991) estimates a model of retail behaviour for the Netherlands and simulates the employment impact of deregulating store opening hours using evidence from the Swedish experience with deregulation. His estimates suggest that increasing opening hours by 10 will

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[7] Opponents of deregulation, of which Christian churches and trade unions are typically the most vocal, argue that deregulation will force some existing employees to work on Sundays against their will. They also emphasize possible undesirable effects on religious and family oriented activities. In response, proponents point to the presence of workers with low preferences for Sunday leisure, such as students, who will gain employment opportunities as a result of Sunday shopping. Further, they argue that legislators can easily provide retail employees with the legal right to refuse Sunday work. Opponents criticize that these protections are weak as they are unable to protect potential new hires from discrimination.

[8] There is, however, a relatively large theoretical literature concerned with the efficiency of Sunday shopping regulations. Kay and Morris (1987) and De Meza (1994) present theoretical models in which deregulation leads to inefficient equilibria by raising operating costs and thereby consumer prices, whereas Clemenz (1990) shows that in a market where consumers have imperfect price information, liberalization of trading hours is likely to improve consumer welfare. Both Morrison and Newman (1983) and Ferris (1991) provide theoretical and empirical evidence that deregulation impacts retail market structure by shifting sales from small to large stores. Lanoie et al. (1994) and Tanguay et al. (1995) study the effect of deregulation on retail prices, while Ingene (1986) considers the effect on average household expenditures.

[9] In the long run, Kay et al. (1984) actually predict a slight decline of around 1 percent in full-time equivalent retail jobs. Their argument is that extended opening hours increases the sales capacity of the retail sector without actually increasing sales. The resulting overcapacity of the industry implies that smaller, less efficient stores will be unable to compete with larger, retailing firms that may operate multiple stores. In the long run this restructuring of the industry will result in overall job losses.

lead to a 1.6 percent increase in Dutch retail jobs.[10] The obvious problem with these estimates is that both are simulations based on data from countries that have yet to experience deregulation. In contrast, Laband and Heinbuch (1987) compare raw averages of employment and hours of work in US states with general bans on all Sunday commercial activity to states with no restrictions, while Tanguay et al. (1995) consider changes in average overtime hours before and after deregulation in Quebec, Canada. The problem with these studies is that both depend on a single source of variation in legal regimes to identify a Sunday shopping effect (i.e. first-difference estimates). By tracking legal changes within states, Burda and Weil (2001) fully exploit the decentralized US legal structure and extend the Laband and Heinbuch (1987) study to a differences-in-differences analysis. They however ignore the source of their estimated positive employment effect of deregulation and the important issue of whether their blue-law indicator variables actually capture significant differences in Sunday retail opening hours within states.

The Canadian experience offers a similar ideal setting to examine the consequences of Sunday shopping as the legislation is provincial and was introduced at different times. As a result, a "natural experiment" exists in which common movements in the retail industry data between provinces can be controlled for so as to isolate the deregulation effect. This paper exploits this setting to identify how retail employers that choose to open on Sundays following

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[10] Note that the Gradus (1996) estimate is of full-time equivalent jobs whereas Kay et al. (1984) distinguish between full-time and part-time workers. Using the existing full-time/part-time mix in English retailing, Kay et al. predict a 3.3 percent increase in full-time equivalent jobs.

deregulation adjust their employment level and weekly hours of work. A complication of the analysis arises because there is reason to suspect that the provincial deregulation dates may, in some cases, not be concomitant with significant increases in Sunday store openings. Using monthly sales data and a unique trading-day regression approach that exploits the fact that some months have five Sundays while other have only four, I begin by first determining in which provinces the "treatment" was actually received. I then limit my analysis to these provinces and estimate a simple dynamic labour demand model that allows employment and hours to be imperfect substitutes in production. The resulting estimates indicate that deregulation led to a long run increase in labour demand that was disproportionately satisfied through an increase in the employment level. There is also evidence that the job gains experienced were larger among general merchandise stores than more specialized retail establishments. Despite short run rigidities in the employment level the estimates suggest that employers were unable to compensate by temporarily increasing the weekly hours of either new or existing employees.

The remainder of the paper is organized as follows. Section 2 presents the Canadian legal experience with Sunday shopping deregulation, examines in which provinces deregulation resulted in a significant increase in Sunday opening hours, and considers some simple differences-in-differences estimates of the employment and hours effects of Sunday shopping deregulation. The theoretical

model, empirical specification and data used to estimate the structural model are presented in Section 3. In Section 4 the results are examined.

2. The Canadian Experience

2.1. The deregulation process

The Canadian process of Sunday shopping deregulation began in 1985 when the Supreme Court of Canada found the federal *Lord's Day Act*, which had designated Sunday as a weekly day of rest since its adoption in 1907, to be unconstitutional.[11] The immediate consequence of this ruling was that the ten provinces became responsible for determining the legality of Sunday shopping within their own jurisdictions. At that time Newfoundland already had legislation in place restricting retail business on Sundays. British Columbia and Ontario had also opted out of federal control before 1985, but had passed legislation providing municipalities with exclusive autonomy to regulate retail business hours.[12] By 1993 all provinces had passed legislation either restricting Sunday shopping (Newfoundland, Prince Edward Island, Nova Scotia, and New Brunswick), providing municipal autonomy (Saskatchewan, Alberta, and British Columbia), or permitting wide-open Sunday shopping (Manitoba, Quebec, and Ontario).[13] Of

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[11] The ruling was based on the logic that the *Act* violated the guarantee of religious freedom enshrined in the *Charter of Rights and Freedoms* of 1982.

[12] Ontario passed legislation in 1975 and British Columbia in 1980. In Ontario by 1985, 13 of 45 cities had adopted early closing by-laws (Ferris 1991). These by-laws were primarily intended to regulate non-Sunday shopping hours so that Ontario cities without municipal by-laws before 1985 continued to acquiesce to the Federal legislation banning Sunday shopping.

[13] Beginning in December 1992, Manitoba conducted a 10-month province-wide experiment with

the provinces that originally regulated business hours all have now either experimented with Sunday shopping (Nova Scotia), permitted it during part of the year (Prince Edward Island and New Brunswick), or entirely deregulated (Newfoundland). The result is a patchwork of legislation that is not only complicated by municipal jurisdiction in some provinces, but also by season and type of retail establishment.

An attempt to empirically evaluate the labour demand consequences of Sunday shopping deregulation using provincial time-series data requires the use of an indicator variable for each province that captures the dates of legal change. Combining information obtained from periodicals, government publications, and personal contact with government offices, Table 1 presents a unique historical record of provincial Sunday shopping deregulation in Canada between 1980 and 2001. The final column of the Table defines periods in which the provincial government had in some way deregulated Sunday shopping. Unfortunately, an accurate dataset of provincial deregulation dates is not sufficient to identify the employment-hours response of retail firms. Since we are interested in the behaviour of retail establishments that chose to respond to deregulation by opening on Sundays, rather than the impact of the provincial legislation itself, it is critical that our legal change indicator variables capture significant increases in Sunday store openings within a province. This point is worth emphasizing because in all provinces there is reason to believe that provincial deregulation

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Sunday shopping. This experiment was followed by legislation providing exclusive municipal autonomy. Winnipeg, the largest city in the province, has chosen not to introduce restrictions.

may not have resulted in an immediate and significant increase in Sunday openings.

First, and most obviously, in Western Canada provincial governments deregulated by downloading legal responsibility onto municipalities. Heterogeneity in preferences between city councils and between residents in different communities is likely to have produced contrasting decisions following provincial deregulation. An attempt was made to collect legal information from cities across British Columbia. The information obtained reveals that the transition from the *Lord's Day Act* to deregulation has been a piecemeal process and in the case of some cities changes in the legality of Sunday shopping are quite ambiguous.[14] As a result, not only does the task of constructing an accurate indicator variable require judgment, there is no guarantee that a single date corresponding to a significant increase in provincial Sunday store openings even exists. Second, many types of establishments, such as variety stores and retailers in the tourism industry, were never constrained by the *Lord's Day Act*. Clearly, there is no reason to expect the Sunday hours of these establishments to have increased following provincial deregulation. Finally, some retailers, such as those in small, rural and religious communities, may choose to remain closed on Sundays following deregulation. To the extent that these establishments are prevalent, any structural breaks at the provincial deregulation date are unlikely to

[14] Richmond, Victoria, Vancouver and Coquitlam deregulated Sunday retail hours in 1981, Maple Ridge in 1985 and Chilliwack in 1990. Interestingly, Langley and Abbotsford have no restrictions on Sunday shopping despite the absence of a by-law formally deregulating it.

show up in the provincial aggregate data. Given these measurement problems, it is important to limit the employment-hours analysis to provinces where there is evidence that provincial deregulation resulted in a significant increase in Sunday opening hours.

2.2. Trading-day regressions

In the absence of data on daily opening hours of retail establishments, it is not obvious how to identify which provincial deregulation dates can be used as a "treatment" to identify how retail firms satisfy their need for Sunday labour following deregulation. The approach taken here is to test for structural breaks at provincial deregulation dates in the trading-day regression given by:

$$\begin{aligned} Q_{it} = \alpha_i + \beta_{1i} SUN_t + \beta_{2i} MON_t + \beta_{3i} TUE_t + \beta_{4i} WED_t + \beta_{5i} THU_t + \\ \beta_{6i} FRI_t + \beta_{7i} SAT_t + \gamma_1 u_{it} + \gamma_2 Y_{it} + \gamma_3 r_t + \gamma_{4i} T_t + \gamma_{5i} T_t^2 + \varepsilon_{it} \end{aligned} \quad (1)$$

where Q_{it} is real, per capita retail sales, u_{it} is the unemployment rate, Y_{it} is real, per capita labour income, r_t is the national consumer loan rate, T_t is a simple linear trend and SUN_t to SAT_t are variables that take on values of 4 or 5 depending on the number of instances of that particular day in month t. The question of interest is for which provinces is the estimate of β_{1i} significantly different before and after the provincial deregulation date defined in the last column of Table 1? A significant increase in the estimate of β_{1i} suggests that provincial deregulation resulted in an immediate increase in Sunday retail activity, and by implication in

Sunday opening hours.[15] In order to sharpen the results, two sources of retail sales data that are disaggregated by type of establishment are used. The first are department store type merchandise (DSTM) sales, which are calculated as total retail sales minus food sales, all sales related to motor and recreational vehicles, and establishments selling alcoholic beverages. The DSTM sales data typically comprise about one-third of total retail sales.[16] The second are total department store (TDS) sales, which are compiled from monthly surveys of all department stores in Canada and typically comprise slightly less than one-tenth of total retail sales.[17] All the series run from January 1981 to December 2001.

Since the days of the week variables are correlated with the month and a set of month dummies will be correlated with sales, the estimates will be biased if the sales data are not first seasonally adjusted.[18] A consequence of this adjustment is that the sales data are purged of the effect of some months having more days than others. As a result, the effect of adding an additional Sunday to a month must be to reduce the incidence of some other day in that month. The estimates of β_{1i} from (1) then tend to be negative when intuition tells us $\beta_{1i} = 0$ in the absence of Sunday shopping and $\beta_{1i} > 0$ following deregulation. The complication is that it is

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[15] At least theoretically, it is possible that Sunday store hours increase significantly without stores experiencing a similar significant increase in Sunday sales. However, such an equilibrium is unlikely to be stable so that
including such provinces in the labour demand analysis identifying long-run effects is inappropriate.

[16] The DSTM data are published monthly in *Retail Trade*, Statistics Canada, Catalogue no. 63-005.

[17] The TDS data are published monthly in *Department store sales and stocks*, Statistics Canada, Catalogue no. 63-002.

[18] The intuition for the former correlation is that months with 31 days are more likely to have a fifth Sunday or any other day of the week.

unclear which day is lost when we add an additional Sunday. The solution is to weight the estimates of β_{ji}, $j = 1, \ldots, 7$, by the vector $c = c_{5SUN} - c_{4SUN}$ where c_{kSUN} is a 7-element vector containing the means of SUN_t to SAT_t in months with k Sundays. The effect of adding an additional Sunday is then given by $c'\beta_i$ which has a variance of $c'Var(\beta_i)c$. If the variables SUN_t to SAT_t in (1) are also interacted with the provincial deregulation indicator variables, defined in Table 1, another vector δ_i is obtained. The estimate $c'(\beta_i + \delta_i)$ then gives the effect of adding an additional Sunday after Sunday shopping is legal. The Wald statistic given by:

$$\frac{(c'\delta_i)^2}{c'Var(\delta_i)c} \sim \chi_1^2 \qquad (2)$$

provides a test of whether the before and after point estimates are statistically different. Since c depends on whether there are 30 or 31 days in a month, there are two separate cases to consider.

The results from the trading-day regression analysis are presented in Tables 2 and 3. British Columbia is excluded from the estimation because there is no provincial deregulation in the period 1981 to 2001, but the province was experiencing deregulation at the municipal level during the period. The TDS estimates also exclude Newfoundland and PEI as these sales series were dropped by Statistics Canada in July 1995. The difference between the before and after point estimates suggest significant increases in Sunday sales following deregulation in New Brunswick, Ontario, Manitoba, and Alberta when using the DSTM data and in Ontario and Alberta when using the TDS data. These are also

the only four provinces in which none of the point estimates suggest a *decrease* in Sunday sales following deregulation. The identification of these four provinces from this analysis is remarkably consistent with the evidence available from newspapers.[19] Interestingly, Nova Scotia is the only province that shows a significant decrease in Sunday sales following deregulation. It is perhaps not surprising then that the government opted to reintroduce regulations following their experiments in 1990 and 1993 and that Nova Scotia is currently the only province in the country with a province-wide ban.

Since retail firms are expected to respond quite differently to deregulation if it is only seasonal, the decision was made to restrict the analysis to those provinces that show both a significant increase in Sunday retail activity in Table 2

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[19] In Ontario, New Brunswick and Manitoba the legal changes were province-wide (for the Ontario case see "Shopping on Sunday wide open" *Globe and Mail*, July 4, 1990, pA6; for New Brunswick see "N.B. retailers fight back with Sunday shopping" *Marketing*, November 19, 1991, p1; for the Manitoba case see "Sunday shopping opens up" *Winnipeg Free Press*, November 20, 1992, pA1). In Alberta, there was a concerted decision by retailers in the two major cities, Calgary and Edmonton, to open stores in November 1984 (see "Floodgates open on Sunday shopping" *Calgary Herald*, October 26, 1984, pA1 and "Sunday shopping blooms for now in Alberta" *Toronto Star*, November 25, 1984, pA19). The weak results for Saskatchewan were expected since provincial deregulation has been a piecemeal process with Regina deregulating in June, 1989 and Saskatoon in October, 1991 (this information was obtained through personal communication with Randy Markewich, City Clerk with the City of Regina, and Crystal Lowe, Records Administrator in the City Clerk's Office of Saskatoon). The Nova Scotia results were also expected as there is strong evidence that the province's Sunday shopping experiments were unpopular among consumers and retailers (see "Government retreats on Sunday shopping" *Halifax Chronicle Herald*, January 29, 1991, p1, "Sunday store openings not worth it – retailers" *Halifax Chronicle Herald*, November 26, 1993, pB6 and "Sunday shopping shot down" *Halifax Chronicle Herald*, April 14, 1994, pA3). The weak results for Quebec are however somewhat surprising. Following province-wide deregulation in January 1993, the Quebec Alliance for Sunday Shopping, a lobby group made up of retailers across the province, attributed large gains in provincial retail sales to Sunday shopping deregulation (see "The average shopper in Quebec can rejoice in the latest retail statistics" *Montreal Gazette*, March 25, 1993, pC1). Weak estimates for Prince Edward Island and Newfoundland probably reflect a combination of little variation in the deregulation dummy variable and significant rural populations, and so lack of response to deregulation.

and experienced year-round deregulation. Hence, the labour demand analysis in this study uses only data from Ontario, Manitoba, and Alberta.

2.3. Differences-in-differences estimates

Before developing and estimating the structural labour demand model, it is worth considering whether a simple differences-in-differences estimator shows any evidence of an employment or hours effect of Sunday shopping deregulation. Ideally, retail labour market trends in Ontario, Manitoba, and Alberta could be compared to similar retail sector data from a province or set of provinces that have never deregulated Sunday shopping. Unfortunately, the Canadian context offers no such comparison. Instead, the wholesale trade industry in each of the three provinces is used as a control group. Arguably, the wholesale sector is an imperfect control in this context since increased retail labour demand driven by heightened sales following deregulation may also serve to increase wholesale labour demand. The wholesale sector will then be an imprecise indicator of what would have happened in the retail sector if deregulation had not occurred. However, this bias should, if anything, dampen the estimated employment and hours effects.

Table 3 presents differences-in-differences estimates of the labour demand effects of Sunday shopping using the retail and wholesale trades industry data. Specifications are estimated for the employment level of hourly paid workers, average weekly hours of hourly paid workers, the part-time share of employment,

and the female share of employment. Each specification represents a separate pooled OLS regression of the variable of interest (normalized by subtracting the January 1983 value) on a full set of year, month and province dummies. When the sample is restricted to the Ontario, Manitoba, and Alberta cross-sections the estimates suggest Sunday shopping resulted in significant increases of about 14 and 2 percent in the employment level and weekly hours respectively. These results are entirely consistent with the results of Burda and Weil (2001) although their estimated employment effects are slightly smaller. However, they do not limit their sample to jurisdictions where there is evidence of a significant increase in Sunday openings following deregulation. As expected, the estimated employment effect is considerably weaker when data from all the provinces are used. Although a slight increase in weekly hours does not rule out an increase in part-time employment rates (a bifurcation of retail jobs into high hour full-time jobs and low hour part-time jobs), stable or declining part-time rates seem more likely. The highly insignificant estimates in the third row of Table 3 suggests that the need for Sunday labour was not disproportionately satisfied by the hiring of part-time workers. There is also no evidence that these job vacancies were disproportionately filled by women.

A problem with the differences-in-differences methodology is it hinges entirely on the assumption that the treatment and control group would have experienced the same employment and hours patterns had deregulation not occurred (Angrist and Krueger 1999). Figure 1 plots the seasonally adjusted

employment and hours series used to obtain the Table 1 estimates. The vertical lines in each panel indicate the provincial deregulation date, while a tick on the top-axis indicates a return to restrictions. Clearly, there is considerable variation in the patterns of these series even before any deregulation occurs. The amount of variation is perhaps surprising given the strong interdependence of these industries. However, a comparison of the retail series *across* provinces reveals much stronger correlation. This confirms that a preferred comparison group is another province or set of provinces that have not experienced deregulation. Since there are no such comparisons, the approach taken here is to use the Ontario, Manitoba, and Alberta retail data and estimate a system of structural equations that, as in a fixed effects model, can account for unexplained contemporaneous correlations.

A final issue worth addressing before developing and estimating the theoretical model is the assumed exogeneity of the provincial deregulation indicator variables. It is entirely possible that deregulation is driven by underlying economic and cultural trends, such as structural shifts to service sector employment or changing attitudes to female labour force participation. To the extent that these trends are correlated with the outcomes of interest, the estimated policy effects will be contaminated. Both Ferris (1991) and Burda and Weil (2001) use IV estimators to identify deregulation effects, while Price and Yandle (1993) focus exclusively on the first-stage model determining the incidence of Sunday shopping restrictions. Common instruments in these studies include the

female labor force participation rate and measures of the Christian share of the population. In the context of a large number of political jurisdictions this first-stage estimation can, at least plausibly, generate some useful exogenous variation in legal regimes. However, the additional stage and the instruments involved are a stretch in the present context of variation in legal regimes between three large provinces. This is particularly the case because in two of the three provinces deregulation was motivated by the decisions of courts, who are arguably less influenced by public preferences and therefore underlying economic and social trends than are state legislators or city councils. The decision of the Ontario Court of Appeal to reverse a decision made nine months earlier by the province's Supreme Court is indicative of this randomness. Also, the influence of underlying economic or cultural trends should be gradual, whereas policy effects should produce discontinuous changes in the variables of interest. By including province-specific quadratic trends in the regression the "glacial" changes in underlying trends can be controlled for, leaving the deregulation indicator variables to capture policy effects. This identification strategy is what Angrist and Krueger (1999) refer to as a "regression-discontinuity design."

3. Model

3.1. Theoretical model

In order to get some idea of the estimated policy effects we might reasonably expect, a simple theoretical model that includes both employment and

hours as imperfect substitutes in production is examined. Consider a cost-minimizing optimization problem in which homogeneous retail firms within a province face costs of operation:

$$C_L = whN + qN \tag{3}$$

where C_L are total weekly labour costs, N is total employment by each firm, h is average weekly hours of those employed, w is the average wage within each firm which is assumed to be independent of average weekly hours, and q are quasi-fixed costs such as hiring, training and benefit costs. Real retail sales per representative firm are given by:

$$Q = H(u, r, Y, MONTH) \tag{4}$$

where u is the provincial unemployment rate, r is the consumer loan rate, and Y is real provincial labour income.

The model assumes that Q is exogenous to the optimization problem of the individual firm. This seems a particularly appropriate assumption for the retail industry where sales are by and large not a function of the labour employed to operate stores. Nonetheless, the total labour input employed, L, is constrained by the requirement that:

$$L \geq G(Q) \tag{5}$$

where $G' > 0$ and $G'' < 0$. If the production function for units of total labour input is given by:

$$L = F(h, N) \tag{6}$$

where F_h, $F_N > 0$ and F_{hh} , $F_{NN} < 0$, a retail firm's optimization problem can be expressed as:

$$\min_{h,N} \; C_L \;\; subject\ to \;\; F(h,N) \geq G(Q). \tag{7}$$

A solution to this problem must satisfy the first-order conditions:

$$\frac{wN}{wh+q} = \frac{F_h}{F_N} \tag{8}$$

$$F(h,N) = G(Q). \tag{9}$$

In order to derive closed-form solutions for N and h, it is convenient to assume the following specific functional forms for F and G:

$$F(h,N) = (h-s)^{\varepsilon} N^{1-\alpha} \;\; , \; 0 < \varepsilon, \alpha < 1 \, , \; h \geq s \tag{10}$$

$$G(Q) = Q^{\gamma} + A \;\; , \; \gamma > 1 \tag{11}$$

where, following Hart and Fitzroy (1985), (10) recognizes a minimal set-up time, s, per worker and, following Noteboom (1982), A in (11) is a threshold level of labour demand which is needed to oversee an open store regardless of the level of sales. Given these functional forms the long run factor demands are then:

$$\begin{aligned} h^* &= \left(\frac{1-\alpha}{1-\alpha-\varepsilon}\right) s + \left(\frac{\varepsilon}{1-\alpha-\varepsilon}\right)\left(\frac{q}{w}\right) \\ &= \beta s + \theta(q/w) \end{aligned} \tag{12}$$

$$N^* = \left[\theta(q/w) + (\beta-1)s\right]^{-\varepsilon/1-\alpha} (Q^{\gamma} + A)^{1/1-\alpha} \tag{13}$$

assuming that $(1-\alpha) > \epsilon$. This solution has the attractive result that only the employment level is a function of retail sales so that optimal average weekly

hours will be relatively constant over the long run as the data in Figure 1 suggest they are.[20]

Having derived a simple theoretical model of the retail firm, it is now possible to consider what impact Sunday shopping deregulation will have on employment and working time in the retail industry. In his examination of the economic impact of extending shop openings hours, Gradus (1996) distinguishes between three possible effects on employment. First, to the extent that Sunday shopping leads to an increase in Q there will be an increase in the long run employment level working through the production function or in the above model through Q^{γ} in equation (13). The size of this *sales effect* will depend not only on how much Sunday shopping increases total retail activity, but also on the output elasticity of the labour input, γ, and the marginal product of employment, α. However, over all reasonable values of γ and α the sales effect will be weak if deregulation has a small impact on Q.

Second, even if deregulation has absolutely no impact on Q, either because Sunday sales are nil or there is a one-for-one tradeoff with Monday to Saturday sales, increased opening hours implies a necessary increase in labour demand as there are more hours in which a store needs to be supervised. Thus, deregulation should lead to an unambiguous increase in A. Since A enters (13),

[20] This is a consequence of the production function for the total labour input. Indeed, any function of the form $L = g(h)\, N^{1-\alpha}$ where $g' > 0$ and $g'' < 0$ will produce this result. Clearly, there exists a wide range of production functions that are multiplicatively separable in h and N, including the Cobb-Douglas form. This particular form actually follows directly from the fact that the total labour input, L, is the product of the number of workers employed and the average weekly hours of these workers.

but not (12), this *threshold effect* will also serve to increase the total labour input entirely through the hiring of new workers, as opposed to increasing the hours of existing workers.

Finally Gradus follows Thurik (1984) and argues that extending retail business hours could have a *labour productivity effect* by flattening sales peaks during the day and thereby reducing the labour intensity at these times. This smoothing effect of Sunday shopping can be formally modeled by altering the basic model above so that there is a separate labour input constraint, as given by (5), for each day of the week. The minimum weekly labour input is then constrained to equal or exceed

$$G_B(Q_B) = \sum_{i=2}^{7} (Q_{Bi}^{\gamma} + A_i) \tag{14}$$

before deregulation and

$$G_A(Q_A) = \sum_{i=1}^{7} (Q_{Ai}^{\gamma} + A_i) \tag{15}$$

after deregulation, where i indexes the days of the week and both Q_B and Q_A are vectors of their respective daily values. The impact of deregulation on labour demand is then given by:

$$G_A(Q_{Ai}) - G_B(Q_{Bi}) = A_1 + \sum_{i=1}^{7} Q_{Ai}^{\gamma} - \sum_{i=2}^{7} Q_{Bi}^{\gamma} \tag{16}$$

where the first term is the threshold effect and the difference between the second and third term captures the sales and productivity effects. If $Q_{Ai} = Q_{Bi}$ for $i = 2, \ldots ,7$ the total effect is simply the sum of the threshold effect, A_1, and the sales effect

Q_{A1}^{γ}. In this case the productivity effect is nil because there is no smoothing of sales over the days of the week. However, to the extent that there is a redistribution of sales over the days of the week there will be a productivity effect that will serve to reduce the positive sales effect. In fact, even if $\sum Q_{Ai} > \sum Q_{Bi}$, with enough smoothing of sales and convexity in the labour input constraint, it is entirely possible that $\sum Q_{Ai}^{\gamma} < \sum Q_{Bi}^{\gamma}$ so that the productivity effect dominates the sales effect.

Therefore, when all three effects are taken together the theoretical model is unable to predict whether deregulation will increase or decrease the employment level in the long run. The only unambiguous prediction of the model is that average weekly hours will be unaffected by deregulation for a wide class of production functions. The possibility that Sunday shopping actually reduces the employment level is undoubtedly contrary to popular wisdom. However, to the extent that the productivity effect dominates the threshold and sales effects, it is a theoretical possibility. This implies that a properly specified empirical model should allow for this possibility by separately identifying all three potential effects of deregulation.

3.2. Empirical specification

The theoretical model suggests that deregulation will impact the optimal demand for workers, but not average weekly hours. Moreover, optimal employment can be affected by deregulation in three possible ways. This suggests

that an appropriate linear specification in the logarithms of all the continuous variables is given by:

$$N_{it}^{*} = \beta_0 + \beta_1 d_{it} + \beta_2 Q_{it} + \beta_3 (Q_{it} \cdot d_{it}) + \beta_4 w_{it} + \beta_{5i} T_t + \beta_{6i} T_t^2 + \mu_{it} \quad (17)$$

$$h_{it}^{*} = \gamma_0 + \gamma_1 d_{it} + \gamma_2 w_{it} + \gamma_{3i} T_t + \gamma_{4i} T_t^2 + \varepsilon_{it} \quad (18)$$

where t indexes the month, i the province, T_t is a linear time trend, and d_{it} is the provincial deregulation indicator variable defined in Table 1. Although the theory predicts that $\gamma_l = 0$, the parameter is included to provide a test of this theoretical prediction.

With reference to equation (17), it is possible to decompose the long run change in the employment level following deregulation into the threshold, sales, and productivity effects. If the long run predicted employment level prior to deregulation is given by:

$$N_{iB}^{*} = \beta_0 + \beta_2 Q_{iB} + \beta_4 w_{iB} + \beta_{5i} T_B + \beta_{6i} T_B^2 \quad (19)$$

and after deregulation by:

$$N_{iA}^{*} = \beta_0 + \beta_1 + (\beta_2 + \beta_3) Q_{iA} + \beta_4 w_{iA} + \beta_{5i} T_A + \beta_{6i} T_A^2 \quad (20)$$

the employment impact of deregulation is simply the difference

$$\begin{aligned} N_{iA}^{*} - N_{iB}^{*} &= \beta_1 + (\beta_2 + \beta_3) Q_{iA} - \beta_2 Q_{iB} \\ &= \beta_1 + \beta_2 (Q_{iA} - Q_{iB}) + \beta_3 Q_{iA} \end{aligned} \quad (21)$$

if w_{it} and T_t are held constant. The first term in (21) is the threshold effect, the second term is the sales effect, and the productivity effect is captured by the third

term.[21] For the estimates to be consistent with the theory we need $\beta_1 > 0$, $\beta_2 > 0$, and $\beta_3 < 0$.

The size of the sales effect will in part be determined by β_2, which captures how exogenous shocks to Q_{it} translate into adjustments in the employment level. However, it also depends on the difference ($Q_{iA} - Q_{iB}$). To estimate the sales effect it is then necessary to also obtain an estimate of the deregulation impact on Q_{it}. This is done by estimating a linear form of equation (4) which is given by:

$$Q_{it} = \alpha_0 + \alpha_1 d_{it} + \alpha_2 u_{it} + \alpha_3 Y_{it} + \alpha_4 r_t + \alpha_5 MONTH + \alpha_{6i} T_t + \alpha_{7i} T_t^2 + \omega_{it} \tag{22}$$

Although the theoretical model assumes Q_{it} is exogenous to the labour demand decision of the firm, this assumption was tested using an omitted variable version of the Hausman test. When the TDS sales data are used the test strongly rejects the exogeneity of Q_{it} in equation (17). The decision was therefore made to treat Q_{it} as an endogenous variable in the empirical model and to use (22) as its reduced form equation.[22]

In order to distinguish long run from short run firm responses to exogenous shocks it is necessary to add a dynamic element to this empirical model. Following Hart and Sharot (1978), the approach taken here is to assume

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[21] Notice that this is essentially the Oaxaca decomposition, due to Blinder (1973) and Oaxaca (1973), common to the discrimination literature where the gender wage gap is decomposed into a part due to differences in human capital acquisition between men and women and a part is due to unequal returns to that human capital.

[22] The Hausman test statistic is F(2,632) = 10.07 when the TDS data are used and F(2,632) = 0.59 with the DSTM data. It turns out that the general results are quite robust between specifications. This is true whether the TDS or DSTM data are used.

partial adjustment of workers and instantaneous adjustment of hours. The rationale is that there are quasi-fixed costs of adjusting the employment level, whereas temporary hours adjustments are relatively costless. The partial adjustment process is given by:

$$N_{it} = \lambda N_{it}^{*} + (1 - \lambda) N_{t-1} \tag{23}$$

where λ provides a measure of the degree of rigidity in the employment level. Since it is now possible that $N_{it}^{*} \neq N_{it}$, the firm may substitute towards h_t in the short run. Therefore, optimal short run average weekly hours is expected to be increasing in $(N_{it}^{*} - N_{it})$ which can be expressed as:

$$h_{it} = \gamma h_{it}^{*} + \pi (N_{it}^{*} - N_{it}), \quad \pi > 0 \tag{24}$$

where π measures to what extent hours are increased in the short run to accommodate shortfalls in the employment level.

A complication with estimating equation (17) and (18) is that the retail wage is likely to be endogenous. To identify exogenous retail wage fluctuations, monthly provincial data on minimum wages and the average manufacturing wage are used as instruments in a reduced form equation for w_{it}. When this is done and the dynamic structure in (23) and (24) is applied to (17) and (18), the complete empirical empirical model is given by the following equations:

$$\begin{aligned} N_{it} = {} & \lambda \beta_0 + \lambda \beta_1 d_{it} + \lambda \beta_2 Q_{it} + \lambda \beta_3 (Q_{it} \cdot d_{it}) + \lambda \beta_4 w_{it} + \\ & \lambda \beta_{5i} T_t + \lambda \beta_{6i} T_t^2 + (1-\lambda) N_{i,t-1} + \mu_{it} \end{aligned} \tag{25}$$

$$h_{it} = \gamma_0 + \gamma_1 d_{it} + \gamma_2 w_{it} + \gamma_{3i} T_t + \gamma_{4i} T_t^2 + \pi \left(\frac{1-\lambda}{\lambda} \right) (N_{it} - N_{i,t-1}) + \varepsilon_{it} \tag{26}$$

$$w_{it} = \delta_0 + \delta_1 d_{it} + \delta_2 Q_{it} + \delta_3 (Q_{it} \cdot d_{it}) + \delta_4 mw_{it} + \delta_5 manw_{it} + \delta_{6i} T_t + \delta_{7i} T_t^2 + \delta_8 N_{i,t-1} + \eta_{it} \quad (27)$$

and equation (22). The presence of the endogenous variable N_{it} in (26) does not present any problems due to the exclusion restrictions on Q_{it} and $(Q_{it} \cdot d_{it})$.

The estimation procedure involves estimating the parameters in (22), (25), (26), and (27) using data from the three provinces that experienced significant changes in Sunday retail activity following deregulation. In order to obtain a single set of parameter estimates and to identify exogenous fluctuations in Q_{it}, N_{it}, and w_{it}, the 12 equation system is estimated by 3SLS with cross-equation restrictions on all the parameters except the quadratic trends. This estimator allows us to gain efficiency from contemporaneous correlations as the estimated error-covariance matrix captures all the variances and covariances of the errors in (22), (25), (26), and (27). Thus, as in the fixed effects model, common unexplained movements in the sales, employment, hours, and wage data within and between cross-sections are accounted for.

3.3. Data

Instead of using the aggregate retail industry data, which includes many establishments not affected by Sunday shopping deregulation, the labour demand model is estimated separately using the department store type merchandise (DSTM) and total department store (TDS) sales data that were used in the trading-day regression analysis. Corresponding data by industrial classification on

employment, average weekly hours, and wages of hourly paid workers were constructed from data published by Statistics Canada in *Employment, Earnings and Hours*.[23] Unfortunately, these payroll data are not matched perfectly by industrial classification to either the DSTM or TDS sales data. The payroll data corresponding to the DSTM sales data exclude drug stores (SIC 641), which are in the DSTM sales data, and include a miscellaneous classification (SIC 659), which are not in the DSTM sales data. The payroll data corresponding to the TDS sales data include department stores as well as smaller, general merchandise stores (SIC 6412 and 6413). Monthly provincial aggregate data on the remainder of the variables in the empirical model were collected which produced two complete seasonally unadjusted, time-series data sets covering the period from January 1983 to December 2000. Descriptions and summary statistics of all the variables are provided in the Appendix. Ideally, data from the late 1970s should have been included in these series, but the payroll data from Statistics Canada only begin in 1983. This is unfortunate given that Alberta experienced deregulation in November 1984.

4. Empirical Results

The results from estimating equations (22), (25), (26), and (27) with the DSTM and TDS data are presented in the top and bottom panels of Table 4

[23] The payroll data published in *Employment, Earnings and Hours*, Statistics Canada, Catalogue no. 72-002 are compiled from monthly surveys of employers in each province at detailed industrial levels.

respectively. The sales, threshold, productivity, and hours effects estimates are shown in Table 5 with 95 percent confidence intervals. The results from both data sources suggest a modest, but significant, increase of about 2 to 3 percent in retail sales following deregulation. When these estimates of α_l are combined with the labour intensity estimates, β_2, the results suggest a sales effect on the employment level of about 1 to 2 percent.[24] These results challenge the position, taken by many opponents of Sunday shopping, that deregulation simply redistributes sales away from the rest of the week to Sundays. What explains this increase in total retail sales? The more obvious explanations are pent-up tourist or recreational demands for Sunday shopping or shifts in sales away from establishments that are not included in the sales data. The less obvious explanation is that deregulation serves to increase the price of retail *and* the CPI based on all consumer goods, which is used to deflate the nominal sales data, is an imperfect indicator of this price change for the retail sales series used. In this case, the estimated gain in sales volume may entirely reflect an increase in retail prices. It is therefore worth considering if and to what extent deregulation resulted in higher retail prices. Unfortunately, provincial data on the price of retail (i.e. retail margins) is not available, which explains its absence from equation (22). In addition, both the

[24] In fact, the sales effect is greater in the DSTM data. The reason is that the estimate of the labour intensity parameter, β_2 , is larger in the DSTM data. At least part of this difference probably reflects the superior sales capacity of the larger general merchandise stores relative to the smaller specialized merchandise stores included in the DSTM data.

theoretical and empirical literature on the price effect of retail trading hours deregulation is mixed and inconclusive.[25]

In order to obtain some evidence on whether the estimated sales increase reflects a volume or price change, I estimate simple differences-in-differences specifications using CPI data from the three selected provinces. The results from this analysis are presented in Table 6. It is assumed that price indices of goods that have always been available on Sundays (even-numbered goods) will reveal what would have happened to the price of goods affected by deregulation (odd-numbered goods) if deregulation had not occurred. Unfortunately, the analysis is restricted to published price indices so the goods chosen are more aggregated than is ideal and in all cases probably include some goods that do not satisfy the assumptions of the identification strategy. The food and alcohol comparisons are arguably the most convincing for the Canadian context. In all cases, except the pharmaceutical/personal care comparison, the estimates suggest significant positive price increases following deregulation. The estimates range from about 1 to 4 percent. As some assurance of the meaningfulness of these estimates it is worth noting that the estimated increase of about 4 percent using the food indices

[25] Theoretical arguments or models suggesting positive price effects of extending retail business hours include De Meza (1984), Ingene (1986), Kay and Morris (1987), Ferris (1991), and Burda and Weil (2001). In most of these papers this result is driven by increases in operating costs. In contrast, Clemenz (1990) presents a model in which consumers search for prices and trading hours liberalization leads to price reductions, while Tanguay et al. (1995) extent the Morrison and Newman (1983) model and predict, assuming a Cournot equilibrium, higher prices in large stores and lower prices in small stores. Empirical research suggesting positive price effects of deregulation include Burda and Weil (2001) using data on retail value-added by US state and Tanguay et al. (1995) using data from large grocery stores in Quebec, Canada. The empirical analysis in Kay and Morris (1987), on the other hand, suggests lower prices following deregulation.

is remarkably similar to the Tanguay et al. (1995) estimate of 5 percent using food price data before and after deregulation of food stores in Quebec and with the Burda and Weil (2001) estimate of 3 percent using retail value-added data for US states. This raises the possibility that sales volume may have been unaffected by Sunday shopping after all as deregulation instead served to raise the price of retail. The small estimated sales effect on employment of 1 to 2 percent should then be zero.[26] However, we are of course unable to rule out the possibility of *both* a 3 percent increase in prices and a 3 percent gain in sales volume.

In contrast to the modest estimated sales effect, the threshold effect estimates of 30.7 percent and 178.1 percent exceed any reasonable explanation.[27] Similarly, the productivity effect estimates, given by β_3, exceed expectation. From equation (21), the magnitude of the productivity effect depends on at what value of Q_{iA} it is evaluated. However, it is not obvious what the appropriate value should be. The approach taken here is to calculate for each province the annual average level of log retail sales in the twelve-month period in which deregulation occurs in the sixth month. The average of these values over the three provinces is

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[26] We should still expect a price effect on employment though as price changes signal more profitability. The price effect should then be captured by the threshold effect estimate of β_1.

[27] As argued by Noteboom (1982), the theoretical value of A in (13) should be opening time so the threshold effect is simply the increase in store hours. Typical retail hours in Canada are 10AM to 9PM from Monday to Friday, 10AM to 6PM on Saturday and 12PM to 5PM on Sunday. In this case, Sunday openings will increase total retail hours by about 8 percent. The threshold effect point estimates then imply that the threshold labour elasticity of employment exceeds 3 and 22 respectively. Is this theoretically plausible? From (13) this elasticity reduces to:

$$\varepsilon_{NA} = \frac{1}{(1-\alpha)(Q^{\gamma} / A + 1)} .$$

A necessary condition for this elasticity to exceed 22 is $\alpha > 0.95$. Since a solution to (7) requires that $(1 - \alpha) > \varepsilon$ the threshold effect point estimates imply strong decreasing returns to scale in the production of the labour input as given by (10) which is clearly a difficult result to explain.

20.3 in the DSTM data and 18.8 in the TDS data, which imply productivity effect estimates of 22.2 percent and 166.3 percent respectively. Although we have no strong predictions regarding the magnitude of the productivity effect, intuition suggests that the amount of sales smoothing needed to reduce the long run employment level by more than 100 percent could not have resulted from the addition of a single day of shopping. As the large standard errors on the estimates suggest, the problem is that both the threshold and productivity effects are estimated imprecisely. However, quite plausible estimates are obtained when the estimates of the two effects are combined. The DSTM and TDS data then imply 10 and 12.5 percent increases in the long run employment level respectively. These estimates are remarkably similar to the 14 percent differences-in-differences estimate in Table 3.

It appears that the empirical model is unable to separately identify both the threshold and productivity effect. The reason is simply that the threshold effect is calculated at $Q_{it} = 0$, but there are no data points close to 0. As a result of the linearity imposed on the data, any reduction in labour intensity following deregulation, reflected in a negative estimate of β_3, will tend to imply a corresponding increase in the threshold effect. Because there are no observations when Q_{it} is close to 0, the threshold effect is entirely determined by this change in the slope of the fitted regression line via this tradeoff. The point is that both estimates are identified by a single source of information, the change in labour

intensity following deregulation. It is therefore not surprising that when this information is limited to identifying a single parameter, by combining the two effects, the estimation implies an entirely plausible employment effect of deregulation.

In contradiction of the theoretical prediction of constant h_{it}^{*}, but again with remarkable similarity to the differences-in-difference estimate, the estimates of γ_l imply *permanent* increases in average weekly hours of 1.5 and 4.3 percent in the DSTM and TDS data respectively. How can these results be explained? One possibility is that the theoretical model incorrectly assumes N and h can be adjusted in any way to produce the required L. In reality, individual establishments must produce work schedules that divide total hours in a way that is acceptable to all new and existing employees. The resulting coordination problems, which will be particularly important with heterogeneity in workers' tastes, may make setting a unique level of L without some adjustment to h impossible. A second possibility is that deregulation has a direct effect on s in equation (10).[28] Since h is measured as weekly hours, s is the average hours in a week lost to set-up time. Intuition then suggests that s should depend positively on the average number of shifts worked in a week. This suggests that the positive γ_l estimates might be capturing an increase in average shifts worked in a week. In either case the positive hours effects, which are entirely consistent with the insignificant estimated effects on part-time employment rates in Table 3, suggests

[28] I would like to thank an anonymous referee for pointing out this theoretical possibility.

that obtaining the desired L following deregulation involved having either some existing employees work Sunday shifts in addition to their regular shifts or having some new employees work more than the pre-deregulation level of average weekly hours.[29]

Interestingly, comparison of the DSTM and TDS estimates suggests larger employment and hours gains among general merchandise stores than among more specialized retail establishments. To the extent that the TDS data are based on larger establishments on average, this result is consistent with the Morrison and Newman (1983) prediction that deregulation serves to redistribute sales from small to large stores. The complication is that the estimated sales effects do not suggest that such a redistribution of sales occurred. This is surprising given that the TDS sales data are based exclusively on department stores sales (SIC 6411). Of course it is still possible that department stores experienced relatively large price effects of deregulation, as the Tanguay et al. (1995) evidence suggests, or that the estimated sales volume effects are contaminated differently in the DSTM and TDS sales data making them incomparable. However, an alternative explanation of the contrasting labour demand effects is that our payroll data based exclusively on hourly-paid workers fails to capture increases in the working hours

[29] In either case, these results are in contrast to that of Upton (1986) who collected information from five large British retail firms that operated stores in Scotland, where there has never been formal regulation of Sunday shopping, to find out how their needs for Sunday employment were satisfied. He found that among these firms much of the labour was provided by "Sunday-only" part-timers who the firms claimed they had little difficulty in recruiting. Clearly, to the extent that this strategy is dominant in the industry, average weekly hours should fall, not rise, following deregulation.

of store managers and owners. The results then suggest that, unlike general merchandise stores, establishments with more specialized product lines were more likely to have store owners or managers work Sunday shifts than to hire new employees or raise the hours of existing employees.

Finally, the partial adjustment parameter, λ, estimates reveal some stickiness in the retail industry employment level. At first glance this rigidity of employment appears considerable, but it should be emphasized that it is estimated using monthly data so N_{it} will be more than half-way to reaching N_{it}^{*} three months following a shock. The point here is that the low estimate of λ leads to some short run dynamics. However, this in turn produces very mild fluctuations in average weekly hours due to the small, but insignificant, estimates of π. Specifically, weekly hours increase by 0.5 and 0.3 percent in the first month following deregulation in the DSTM and TDS data respectively. Thus, in the short run the total labour input employed, L, falls below its optimal long run level. Given that there were significant increases in Sunday opening hours, as the trading-day regression analysis suggests, how did stores overcome this temporary shortfall in the labour input? The lack of substitutability between workers and hours probably reflects the difficulty of adjusting workers' weekly hours of work. This rigidity is likely to be particularly important when retail firms are asking their existing workers, who in some jurisdictions have the legal right to refuse Sunday work, to temporarily work Sunday shifts until new workers can be hired. A possible firm response is for store owners or managers to work Sunday shifts themselves until

new employees with low preference for Sunday leisure are recruited. Again, since the hours of store owners and managers do not appear in the data there is no evidence of a short run tradeoff between worker and hours.

5. Summary

Using aggregate data on employment and hours of work at two levels of the retail trade industry classification, a simple dynamic labour demand model was estimated to examine retail firm responses to an exogenous increase in permitted opening hours. The results suggest that Sunday shopping deregulation led to long run increases in both the employment level of retail workers paid by the hour and in their average weekly hours of work. The large estimated employment gains of between 8 and 12 percent appear to have been driven by an increase in the level of threshold labour that dominated an offsetting gain in labour productivity, and not by an increase in sales volume. There is also some evidence that the labour demand increases were larger among general merchandise stores than among more specialized retail establishments. Although these estimated labour demand increases are large in comparison to similar estimates from previous research, it must be emphasized that they are obtained using data from selected provinces and industries where there is evidence or strong priors that provincial deregulation had a significant effect on Sunday opening hours. These sample selection decisions were motivated by the

importance in this type of natural experiment to first determine whether the actual treatment of interest was received.

There is also evidence that retail firms were unable to *temporarily* raise the weekly hours of their existing employees to overcome significant rigidities in the employment level. However, both data sources suggest that there was an instantaneous and *permanent* increase of about 2 to 4 percent in average weekly hours. Although these results imply that existing employees satisfied at least some of the need for Sunday labour immediately following deregulation, they do not provide any direct evidence on whether new or existing employees satisfied this need in the long run. In addition, even if the majority of the need for Sunday labour was satisfied through the hiring of new employees with low preferences for Sunday leisure, the results suggest that the new employees were no more likely to be hired on a part-time basis than their existing counterparts. More precise evidence on how retailers met their Sunday labour needs will require establishment level data with information on the hours of individual employees.

Table 1: Provincial regulation of Sunday shopping in Canada.

Province	Legal change date: general rule	Legislation	Indicator Variable
Newfoundland	January 1998: amendment to the *Shops Closing Act* passed to permit wide-open Sunday shopping throughout the province.	*Shops Closing Act* (1977)	January 1998 to December 2001.
Prince Edward Island	November 1992: legislation passed to permit business establishments to open on Sundays from the last Sunday in November to the Sunday preceding Christmas.	*Retail Businesses Holidays Act* (1992)	Each December from 1992 to 2001.
Nova Scotia	March 1990: temporary experiment which allowed stores less than 40,000 sq. feet to open on Sundays. October 1993: temporary experiment with deregulation by legislative amendment.	*Retail Business Uniform Closing Act* (1985) *An Act to Amend Chapter 402 of the Revised Statutes* (1993)	March 1990 to January 1991. October 1993 to December 1993.
New Brunswick	November 1991: temporary amendment to permit shopping in most retail establishments. September 1992: amendment to *Days of Rest Act* which allows Sunday shopping from first Sunday following Labour Day to the Sunday immediately preceding Christmas.	*Days of Rest Act* (1985) *Act to Amend the Days of Rest Act* (1992)	November 1991 to December 1991. September to December from 1992 to 1995. August to December from 1996 to 2001.

	August 1996: amendment to *Days of Rest Act* which allows Sunday shopping from first Sunday in August to the second Sunday after Christmas.		
Quebec	December 1992: move to wide-open Sunday shopping.	*Act respecting commercial establishments business hours* (1990) *Act to amend this law* (1992)	January 1993 to December 2001.
Ontario	June 1990: the *Retail Business Holidays Act* found to be unconstitutional by Ontario Supreme Court and in March 1991 the Ontario Court of Appeal reversed this decision. Result was 9 months of wide-open Sunday shopping. December 1991: legislation amended to permit Sunday shopping in the month of December. June 1992: Bill introduced to permit wide-open Sunday shopping.	*Retail Business Holidays Act* (1990) *Retail Establishments Statute Law Amendment Act* (1991) *Act to Amend the Retail Business Holidays Act in respect of Sunday Shopping* (1992)	July 1990 to March 1991. December 1991. June 1992 to December 2001.
Manitoba	December 1992 and April 1993: two separate amendments to *Retail Business Holiday Closing Act* which led to 10	*Retail Business Holiday Closing Act* (1987) *Bill 4, Retail*	December 1992 to December 2001.

	month experiment with wide-open Sunday shopping. October 1993: municipal autonomy.	*Businesses Sunday Shopping* (1992) *Bill 23, An Amendment to Retail Businesses Sunday Shopping* (1993)	
Saskatchewan	May 1988: Province passed legislation providing municipal autonomy.	*Urban Municipality Amendment Act* (1988)	May 1988 to December 2001.
Alberta	November 1983: Alberta Court of Appeal struck down *Lord's Day Act*, but wide-spread Sunday shopping began in major cities in November 1984 following joint-decision to open by three major department stores in province. In 1985, legislation passed officially providing municipal autonomy.	*Municipal Government Amendment Act* (1985)	November 1984 to December 2001.
British Columbia	1980: legislation passed providing municipal autonomy.	*Holiday Shopping Regulation Act* (1980)	None.

Sources: Human Resources Development Canada website at http://labour-travail.hrdc-drhc.gc.ca/psait_spila/lmnec_eslc/eslc/stand7-e1.html, APEC Newsletter 36(8), various newspaper articles, and personal government with various provincial government officials.

Table 2: Trading-day regressions.

	30 Days			31 Days		
	Before	After	Wald	Before	After	Wald
(1) DSTM sales data						
Newfoundland	-1.989	-2.072	0.00	-2.393	-2.671	0.01
	(1.882)	(3.009)		(1.711)	(2.813)	
Prince Edward Island	-3.679	-6.880	0.18	-4.048	-2.493	0.12
	(2.133)	(7.712)		(1.953)	(4.763)	
Nova Scotia	-3.041	-4.418	0.21	-3.472	-4.636	0.15
	(2.242)	(3.618)		(2.047)	(3.480)	
New Brunswick	-4.240	-0.671	4.47*	-5.036	-1.941	4.17*
	(1.935)	(2.339)		(1.769)	(2.115)	
Quebec	-3.893	-1.900	2.11	-4.115	-2.528	1.59
	(1.882)	(1.952)		(1.717)	(1.785)	
Ontario	-4.998	-2.088	4.53*	-3.813	-3.470	0.08
	(1.831)	(1.830)		(1.669)	(1.672)	
Manitoba	-3.456	-0.935	4.59*	-3.914	-2.408	1.98
	(1.132)	(1.215)		(1.033)	(1.109)	
Saskatchewan	-3.934	-2.664	0.48	-4.687	-3.212	0.78
	(1.901)	(1.599)		(1.730)	(1.460)	
Alberta	-7.008	-2.792	2.88**	-5.914	-3.297	1.43
	(2.554)	(1.545)		(2.250)	(1.417)	
(2) TDS sales data						
Nova Scotia	-0.497	-2.506	5.14*	-0.914	-1.626	0.67
	(0.541)	(0.989)		(0.493)	(0.956)	
New Brunswick	-0.728	-0.572	0.09	-1.194	-0.840	0.57
	(0.393)	(0.566)		(0.360)	(0.507)	
Quebec	-0.509	-0.306	0.52	-0.688	-0.724	0.02
	(0.387)	(0.402)		(0.353)	(0.367)	
Ontario	-1.250	-0.205	6.96*	-1.319	-0.844	1.73
	(0.423)	(0.423)		(0.385)	(0.387)	
Manitoba	-0.994	-0.551	0.65	-1.343	-1.207	0.07
	(0.635)	(0.668)		(0.580)	(0.610)	
Saskatchewan	-0.688	-0.417	0.26	-0.437	-0.781	0.51
	(0.601)	(0.522)		(0.547)	(0.477)	
Alberta	-2.148	-0.321	5.06*	-2.438	-0.755	5.48*
	(0.890)	(0.592)		(0.790)	(0.543)	

Note: Standard errors are in parentheses. * and ** indicate significance at the 5 and 10 percent levels respectively.

Table 3: Differences-in-differences estimates (OLS) of labour demand effects of Sunday-shopping deregulation, retail and wholesale industries.

	Ontario, Manitoba, Alberta			All provinces		
	Retail	Wholesale	Diff.	Retail	Wholesale	Diff.
Log employment	-0.023*	-0.163*	0.141*	-0.059*	-0.089*	0.030*
	(0.007)	(0.018)	(0.017)	(0.005)	(0.010)	(0.011)
Log average weekly hours	0.005	-0.015*	0.020*	0.004	-0.015*	0.019*
	(0.004)	(0.005)	(0.006)	(0.003)	(0.004)	(0.004)
Part-time share	0.002	-0.004	0.006	-0.002	0.003	-0.005*
	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)
Female share	-0.006	-0.004	-0.002	0.000	-0.002	0.003
	(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.004)

Note: Standard errors are shown in parentheses. * indicates significance at the 5 percent level.

Table 4: Labour demand model estimates (3SLS).

(1) DSTM data							
Q_{it}		N_{it}		h_{it}		w_{it}	
d_{it}	0.026	d_{it}	0.307	d_{it}	0.015	d_{it}	0.069
(α_1)	(0.006)	(β_1)	(0.465)	(γ_1)	(0.005)	$(*_1)$	(0.092)
u_{it}	-0.018	Q_{it}	0.615	w_{it}	-0.480	Q_{it} $(*_2)$	-0.026
(α_2)	(0.002)	(β_2)	(0.095)	(γ_2)	(0.059)		(0.008)
Y_{it}	1.007	$Q_{it} \cdot d_{it}$	-0.011	$N_t - N_{i,t-1}$	0.056	$Q_{it} \cdot d_{it}$	-0.002
(α_3)	(0.067)	(β_3)	(0.023)	(π)	(0.017)	$(*_3)$	(0.004)
r_{it}	-0.004	w_{it}	-0.565			mw_{it}	0.265
(α_4)	(0.001)	(β_4)	(0.339)			$(*_4)$	(0.033)
		$N_{i,t-1}$	0.176			$manw_{it}$	0.212
		(λ)	(0.026)			$(*_5)$	(0.067)
						$N_{i,t-1}$	0.110
						$(*_8)$	(0.021)

(2) TDS data							
Q_{it}		N_{it}		h_{it}		w_{it}	
d_{it}	0.027	d_{it}	1.781	d_{it}	0.043	d_{it}	-0.057
(α_1)	(0.007)	(β_1)	(0.500)	(γ_1)	(0.006)	$(*_1)$	(0.110)
u_{it}	-0.014	Q_{it}	0.252	w_{it}	-0.363	Q_{it} $(*_2)$	-0.030
(α_2)	(0.002)	(β_2)	(0.089)	(γ_2)	(0.049)		(0.010)
Y_{it}	0.873	$Q_{it} \cdot d_{it}$	-0.088	$N_{it} - N_{i,t-1}$	0.031	$Q_{it} \cdot d_{it}$	0.004
(α_3)	(0.091)	(β_3)	(0.026)	(π)	(0.015)	$(*_3)$	(0.006)
r_{it}	-0.7E-03	w_{it}	-0.336			mw_{it}	0.195
(α_4)	(0.002)	(β_4)	(0.328)			$(*_4)$	(0.039)
		$N_{i,t-1}$	0.187			$manw_{it}$	0.289
		(λ)	(0.026)			$(*_5)$	(0.087)
						$N_{i,t-1}$	0.135
						$(*_8)$	(0.019)

Note: Asymptotic standard errors are in parentheses.

Table 5: Combined employment and hours effects.

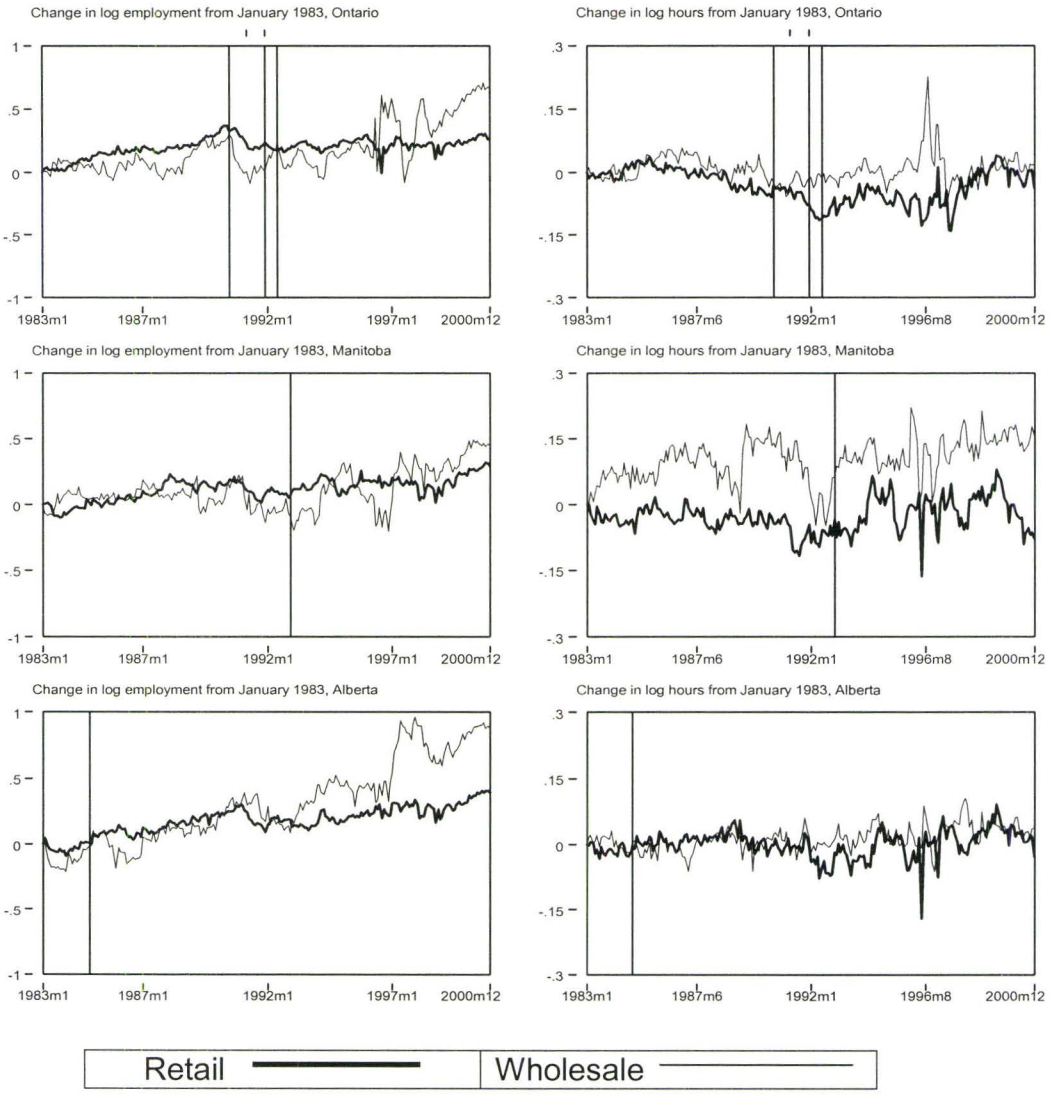
	(1) DSTM		(2) TDS	
	Estimate	95% CI	Estimate	95% CI
Sales effect	0.016	0.008 to 0.024	0.007	0.001 to 0.013
Threshold effect	0.307	-0.604 to 1.218	1.781	0.801 to 2.761
Productivity effect	-0.222	-1.126 to 0.682	-1.663	-2.627 to –0.699
Total employment effect	0.100	0.043 to 0.157	0.125	0.058 to 0.192
Hours effect	0.015	0.005 to 0.025	0.043	0.031 to 0.055

Table 6: Differences-in-differences estimates (OLS) of price effects of Sunday-shopping deregulation.

(1)	Goods	0.008	(0.002)
(2)	Services	-0.025	(0.004)
	Difference	0.033	(0.004)
(3)	Personal care supplies	0.011	(0.002)
(4)	Pharmaceutical products	0.041	(0.003)
	Difference	-0.030	(0.003)
(5)	Food from stores	0.038	(0.004)
(6)	Food from restaurants	0.3E-03	(0.003)
	Difference	0.037	(0.004)
(7)	Alcohol from stores	-0.038	(0.006)
(8)	Served alcohol	-0.049	(0.005)
	Difference	0.011	(0.003)

Note: Standard errors are shown in parentheses.
Source: *The consumer price index*, Statistics Canada, Catalogue no. 62-001.

Figure 1: Changes in employment and average weekly hours, retail and wholesale industries.



Note: Vertical line indicates date of Sunday shopping deregulation. Tick on top-axis indicates return to restriction on Sunday store openings.
Source: *Employment, Earnings and Hours*, Statistics Canada, Catalogue no. 72-002, 1983- 2000.

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Appendix: Variable descriptions and first and second moments.

Symbol	Description	Ontario	Manitoba	Alberta
(1) DSTM				
Q_{it}	Real retail sales	21.482	19.042	20.244
		(0.239)	(0.201)	(0.194)
N_{it}	Employment of hourly paid workers	12.071	9.700	10.686
		(0.072)	(0.072)	(0.085)
h_{it}	Average weekly hours of hourly paid workers	3.188	3.175	3.196
		(0.065)	(0.056)	(0.053)
w_{it}	Average real wage of hourly paid workers	2.220	2.120	2.197
		(0.036)	(0.061)	(0.069)
(2) TDS				
Q_{it}	Real retail sales	19.981	17.751	18.736
		(0.296)	(0.297)	(0.290)
N_{it}	Employment of hourly paid workers	11.176	9.005	9.864
		(0.130)	(0.169)	(0.303)
h_{it}	Average weekly hours of hourly paid workers	3.115	3.129	3.182
		(0.085)	(0.076)	(0.070)
w_{it}	Average real wage of hourly paid workers	2.269	2.161	2.274
		(0.060)	(0.067)	(0.083)
(3) Common				
d_{it}	Sunday shopping deregulation indicator	0.523	0.449	0.898
		(0.501)	(0.499)	(0.303)
Y_{it}	Real seasonally adjusted labour income	23.329	20.809	21.901
		(0.115)	(0.048)	(0.124)
r_t	National consumer loan rate (Bank of Canada index)	12.124	12.124	12.124
		(2.278)	(2.278)	(2.278)
u_{it}	Seasonally adjusted unemployment rate	7.926	7.581	8.128
		(1.926)	(1.378)	(1.933)
mw_{it}	Real minimum wage	1.760	1.639	1.582
		(0.087)	(0.062)	(0.045)
$manw_{it}$	Average real manufacturing wage	2.776	2.542	2.707
		(0.031)	(0.019)	(0.047)

Note: Standard deviations are in parentheses. Real retail sales are constructed using the provincial CPI (1992=100) based on consumer goods. All other real variables are constructed using the all-items provincial CPI (1992=100).

CONCLUSION

In the hope of providing some insights into the relative advantages and disadvantages of what have arguably become the three most common identification strategies found in the empirical labour economics literature, this thesis contains a collection of three essays employing each of these methods. The first essay uses *descriptive analysis* to document and explain a long-term upward trend in on-the-job search (OJS) in the U.S. and Canada between the mid-1970s and mid-1990s. Using a strategy of ruling out plausible explanations based on inconsistencies with the data, the essay reaches the conclusion that the observed trend in OJS is most consistent with a long-term reduction in search costs. Moving beyond the limitations of a descriptive analysis, the second essay estimates the effectiveness of internet job search in reducing unemployment durations by applying *instrumental variables* to the estimation of a duration model. Although descriptive statistics indicate shorter unemployment spells among internet job searchers, when observable differences between workers, such as education, are controlled for there is no evidence that using the internet significantly reduces durations. Correcting for endogeneity of the causal variable does nothing to change this result. Finally, the third essay employs a *differences-in-differences* strategy to infer the employment and hours of work effects of Sunday shopping deregulation. The results suggest that deregulation led to a long-

run increase in labour demand that was disproportionately satisfied through an increase in the employment level.

Taken together, these essays illustrate a clear distinction between the descriptive techniques of Chapter 1 and the more sophisticated identification strategies of Chapters 2 and 3. Angrist and Krueger (1999) refer to the former type of research as descriptive analysis and the latter as causal inference. However, it is clear from Chapter 1 that it is also possible to infer causation from what are essentially descriptive analyses. The typical identification strategy employed in this type of research, which includes important contributions to the economics literature such as Berman et. al. (1994) and Riddell (1993), is to rule out all but one plausible explanation based on inconsistencies with the data and infer causation based on what is essentially a "residual" explanation. The disadvantage of this approach is that the evidence is usually quite indirect in comparison to evidence based on instrumental variables or differences-in-differences techniques. As a result, the conclusions drawn are typically more ambiguous and less persuasive. The advantage is that a descriptive analysis does not depend on the critical identifying assumptions found in chapters 2 and 3. The results from the bivariate lognormal duration model in chapter 2 reveal how sensitive the conclusion is to the estimate of rho, which in turn depends critically on the assumed exogeneity of our instruments. Similarly, the differences-in-differences results in chapter 3 rest on the assumption that the wholesale sector in the same province or retail sector in other provinces are accurate indicators of

what would have happened in a province experiencing deregulation if a provincial legal change had not occurred. To the extent that these assumptions are unreasonable, the conclusions of chapters 2 and 3 will be suspect.

In reality, in the field of labour economics, where controlled experiments are rare, descriptive analysis is insufficient for most research questions of interest. In the context of explaining observed long-term trends a remarkable amount of information can be obtained from descriptive analyses using repeated cross-sections of data. This information can then serve to motivate and guide future research that uses more sophisticated empirical techniques. For example, future research can extend the findings of chapter 1 by developing theoretical models with predictions of the effect of declining search costs on both the incidence and duration of OJS. An additional example of the limitations of descriptive analysis is offered in chapter 2. Simple means indicate that internet searchers are significantly more likely to be employed 12 months into the future than unemployed workers who did not report using the internet for job search. However, when we extend this to a regression framework that controls for observable characteristics there is no evidence that using the internet reduces unemployment durations. Taken together, the three essays collected in this thesis emphasize the value of, wherever possible, building on descriptive analyses with more sophisticated empirical techniques that can more directly infer causation.