



*Canadian International  
Labour Network*

## **Labour Market Outcomes:**

**A Cross-National Study**

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McMaster University

DEPARTMENT OF ECONOMICS

**Employment Equity Programs  
and the Job Search Outcomes of Men and Women:  
Actual and Perceived Effects**

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## 1. Introduction

In December of 1993, Ontario's NDP government enacted one of the most comprehensive employment equity programs in the developed world. That policy was a major issue in the next election campaign, and within two years that program was dismantled by the Harris government. As the debate surrounding both those changes indicates, there is considerable disagreement in the Canadian policy community regarding the effects and desirability of such programs.

Opponents of employment equity generally claim it constitutes little more than thinly-veiled reverse discrimination, and primarily hurts young white men --who are not the beneficiaries of historical discrimination against designated groups. Further, by introducing considerations other than merit into hiring decisions, opponents argue that it taints all members of designated groups as potentially less qualified than their peers. Proponents of employment equity take issue with the notion that "free market" hiring decisions are based on pure merit, and argue that public policy is still needed to level the playing field. The Canadian public, while less and less tolerant of any kind of discrimination, still seems to have deep reservations regarding the use of active measures to promote employment of women and minorities.<sup>1</sup>

In view of these diverging viewpoints, what do we actually know about the effects of employment equity on the job market prospects of targeted and non-targeted groups? Existing

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<sup>1</sup>A Gallup poll in December 1993 asked Canadians: "As you may know, women and minority groups are often under-represented at the management level of government and the broader public service. Do you believe governments should actively attempt to hire more women and minority group members for management positions, or should governments take no action whatsoever and hire new employees solely on their qualifications?". Overall, 74 percent of Canadians chose the latter option (no action whatsoever, use qualifications only) including 69 percent of women. Twenty-one and 25 percent of all Canadians and women respectively supported active measures, with the rest venturing no opinion.

empirical studies, to our knowledge, are confined to the US (where these programs are called “affirmative action”), and tend to focus purely on the policy’s impact on the gender- and racial composition of covered firms (e.g. Heckman and Wolpin (1976), Goldstein and Smith (1976), Beller (1978), Leonard (1984, 1989) and Smith and Welch (1984)).<sup>2</sup> While this is of interest in assessing the policy’s impact *on firms*, and on its ability to change the face of the workforce *in targeted workplaces*, it sheds little light on a potentially more important question: does employment equity actually make it easier for designated groups, or harder for others, to find good jobs? Increases in employment of targeted groups at covered firms may show that firms are complying with employment equity policy, but do not, by themselves, imply that employment equity has made it easier for the average unemployed woman, aboriginal, disabled person, or member of a visible minority, to find a job, or a good job.

In this paper, we undertake what is, to our knowledge, the first attempt to measure the effect of employment equity programs on individual workers’ job search outcomes. Because women are by far the most numerous of the targeted groups and our data set is relatively small, our focus is on gender differences. We consider employment equity’s effects both on those it was intended to help (in this case women) and those who might be expected to lose (men). To our knowledge this is also the first paper to measure the effect of employment equity on *workers’ perceptions of discrimination*. In determining the level of political support for employment equity in the general population, perceptions may be even more relevant than actual outcomes.

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<sup>2</sup>Canadian studies of employment equity tend to be descriptive in nature and focus on how to design, implement and/or measure an effective employment equity program (See for example, Jain and Hackett (1989), Leck and Saunders (1992), Raskin (1994)).

Our analysis is conducted using a new survey of Canadian job seekers, the Canadian Out-of-Employment Panel (COEP), which includes information on a number of labour market outcomes in addition to men's and women's perceptions that they were affected by gender discrimination. All the individuals in this survey experienced a job separation in 1995; most were therefore engaged in active search for a new job. A key advantage of our focus on Canada is the fact that employment equity coverage varies considerably by province, municipality, industry, and firm size. Given costly geographical mobility and industry-specific skills, workers separating from covered jobs are more likely to search for new jobs in the covered sector, and thus are more likely to be affected by the law. The multidimensional variation in workers' (preseparation) employment equity coverage is thus, we argue, a reasonable way to identify the effects of employment equity on workers' job search outcomes.

Our main results are as follows. First, we find that employment equity coverage in a worker's preseparation job reduces the relative amount of time it takes unemployed women, versus men, to become re-employed, i.e. reduces the gender gap in re-employment rates. This effect is quite substantial in magnitude but imprecisely measured. Second, this differential can be explained by a simple and direct mechanism: employment equity increases the relative rate at which women, compared to men, are recalled to work for their old employer after a period of unemployment. Third, this increase in women's relative recall rate appears primarily to take the form of an increase in women's recall rates rather than a fall in men's; thus we cannot make a strong case that employment equity actually hurts men. Fourth, like its effect on actual unemployment durations, we find that employment equity reduces the gender gap in the extent to which workers feel harmed by gender discrimination. Interestingly, however, it appears to do so

not by reducing women's perceived discrimination levels, but by raising men's.

The remainder of this paper proceeds as follows. Section 2 describes employment equity legislation in Canada. Section 3 describes the data. Section 4 presents our main results on the effect of employment equity on job search outcomes; Section 5 presents its effects on perceived discrimination. Section 6 presents our concluding remarks.

## **2. Employment Equity Legislation in Canada**

Compared to the United States, employment equity legislation has a relatively short history in Canada. The United States introduced Equal Employment Opportunity (EEO) legislation under Title VII of the Civil Rights Act in 1964<sup>3</sup> and Affirmative Action (AA) legislation under the Federal contractors program in 1965.<sup>4</sup> In Canada, employment equity legislation was first introduced on the municipal and provincial levels starting in the late 1970's, with new laws introduced in Vancouver and Edmonton (1976), Toronto (1979), Saskatoon (1980), Winnipeg (1981), and the province of Quebec (1985). Federal legislation was implemented in 1986, in response to the 1984 Abella Commission Report on "Equality in

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<sup>3</sup>EEO legislation covers firms with 25 or more employees in the private sector. It stipulates that these firms can no longer discriminate on the basis of sex, race, color, religion or national origin in their employment practices such as hiring, promotions, training, etc. In 1972 the legislation was extended to include firms in the private sector with 15 or more employees, state and local governments, and educational institutions. Further, although the Equal Employment Opportunity Commission (EEOC) was put in place to oversee compliance at the onset of the legislation, it was not given substantial enforcement power until after the amendments in 1972.

<sup>4</sup>The Federal contractor program protects individuals from discriminatory employment practices on the basis of race, color, religion, and national origin. It did not cover individuals on the basis of sex until it was amended in 1967. The Federal contractor program covered all firms that held a contract with the Federal government worth \$50,000 or more and who employed more than 50 employees. Contracted firms were to use positive measures (ie. affirmative action) to ensure that employment practices were no longer discriminatory. To ensure that contracted firms were complying with the legislation the Office of the Federal Contract Compliance (OFCC) was created. However, Federal contractors were not required to submit an affirmative action plan to the OFCC until 1968.

Employment”. The province of Ontario did not pass employment equity legislation until December of 1993.<sup>5</sup>

Although the United States has been committed to employment equity for a greater length of time compared to Canada, employment equity legislation at the time of our study was much more comprehensive in Canada, for the following reasons. First, although the EEO legislation appears to have widespread coverage, it is largely based on individual complaints of discrimination. Second, the EEO legislation does not require an affirmative action plan. Third, the Federal contractor program in the United States was scaled back in the 1980s. In fact, Leonard (1994, p. 592) claims that “...affirmative action under the contract compliance program virtually ceased to exist in all but name after 1980.” Finally, our data come from the period when Ontario had one of the world’s most comprehensive employment equity laws, extending not just to government employees, or firms with government contracts, but all firms in the private sector with more than 50 employees. Our data thus provide a rare look at the effects of a very comprehensive program by world standards.

## **2.1 Federal Employment Equity Legislation**

The objective of Canada’s Federal employment equity legislation is to prohibit discriminatory employment practices on the basis of race, sex or personal disabilities. In particular there are four designated groups: Aboriginal peoples, persons with disabilities, women and visible or racial minorities; women are by far the most numerous of these. The Federal

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<sup>5</sup>The only employment equity legislation that existed in the province of Ontario prior to 1993 was the Ontario Police Service Act of 1990.

legislation covers the Federal public service as well as all Federally regulated industries (atomic energy, banking, communication, milling, resource extraction, and transportation) with at least 100 employees and Federal Crown Corporations. This only covers 5% of the national labour force. In order to ensure discriminatory employment practices are being rectified, employment equity plans, which use positive measures, must be created and implemented. Finally, compliance is monitored by the Canadian Human Rights Commission.<sup>6</sup>

There exists another Federal government employment equity initiative called the Federal Contractor's Program, which was also established in 1986. It covers firms with at least 100 employees, who have a contract with the Federal government for at least \$200,000. The contractor is required to develop and implement an employment equity plan.<sup>7</sup> Random compliance reviews by Employment and Immigration Canada (EIC) are undertaken in order to ensure that the contracted firms are complying with the legislation.

## **2.2 Provincial Employment Equity Legislation**

The most expansive employment equity legislation at the time of this study can be found in Ontario. The Ontario government modelled the objectives of their employment equity legislation and defined their designated groups to match those of the Federal government,

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<sup>6</sup>Firms covered under the Federal employment equity legislation must file an annual "employment equity report" to Employment and Immigration Canada (EIC). These reports must contain information on the number and proportion of designated group members employed in the firm. These reports are made available to the Canadian Human Rights Commission to ensure that firms are complying with the legislation. Although firms are required to complete and implement an employment equity plan, neither plans or proof of implementation have to be submitted to either the EIC or the Canadian Human Rights Commission.

<sup>7</sup>The contracted firm is not required to submit its employment equity plans or proof of implementation, nor is it required to report on the number and proportion of designated group members employed by the contractor. Further, subcontractors are not covered under the Federal contractors program.



however the coverage of the Ontario legislation is much broader. It covers all employees in the Ontario Public Service, the broader public sector with at least 10 employees, and firms in the private sector with 50 or more employees. This covers approximately 75% of the Ontario labour force. Like the Federal legislation, employers are required to design and implement an employment equity plan. Finally, the Employment Equity Commission and the Employment Equity Tribunal were created to ensure compliance with the legislation.<sup>8</sup>

The employment equity legislation in Ontario also established a contractor's program which covers all employers contracted out by the Ontario government. Unlike the Federal contractor program, neither the number of employees nor the dollar value of the contract is restricted. Further, the Ontario contractor program, unlike the Federal one, covers all subcontractors.

The province of Quebec has employment equity legislation found in the Quebec Charter of Human Rights and Freedoms. The legislation seeks to eliminate discrimination against "disadvantaged" groups in employment. The disadvantaged groups are the same as those found at the Federal level. The legislation covers all departments and agencies of the provincial government. Further, the Quebec Human Rights Commission may require that employment equity be implemented by private sector employers after the investigation of a complaint. Like the Federal legislation and the Ontario legislation, employers are required to develop and implement an employment equity plan. Compliance is monitored by the Quebec Human Rights

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<sup>8</sup>Employment equity plans do not have to be reported, however information on proof of implementation and the number and proportion of individuals in designated groups employed must be reported to the Employment Equity Commission.

Commission.<sup>9</sup>

Quebec also implemented a contractor's program in 1987 which covers all firms with at least 100 employees who are bidding for a government contract or grant valuing at least \$100,000 and all subcontractors with at least 100 employees who have a subcontract valuing at least \$100,000. Designated groups are the same as those found in the Ontario and Federal legislation, except that the disabled are not covered. Contracted employers are expected to design and administer an employment equity plan. The Quebec Human Rights Commission monitors compliance with the Quebec contractor program.<sup>10</sup>

Finally, three other provinces, British Columbia, Saskatchewan and Manitoba, have employment equity legislation which covers the province's public service and has the same "designated" groups and objectives as the Federal legislation.

### **2.3 Municipal Employment Equity Legislation**

There also exists employment equity legislation at the municipal level. Employment equity at the municipal level covers the municipal public service. The following municipalities have employment equity legislation: The cities of Vancouver, Winnipeg, Regina, Saskatoon,

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<sup>9</sup>An annual report, which includes information on proof of implementation of employment equity plans and future goals to further ensure the elimination of discriminatory employment practices, must be submitted to the Quebec Human Rights Commission.

<sup>10</sup>Contracted firms are required to report their employment equity plan, proof of implementation and the number and proportion of disadvantaged groups employed to the Quebec Human Rights Commission. After the first 13 months, where two reports are required, reports are to be made annually.

Edmonton, Calgary, Halifax, and all the municipalities in Ontario.<sup>11</sup>

### 3. Data

The data used in our analysis is the 1995 Canadian Out of Employment Panel (COEP), a survey designed by Human Resources Development Canada to track the experiences of individuals separating from jobs. Individuals separating from jobs in two window periods during 1995 were identified using administrative records of the Unemployment Insurance system, which requires employers to file a “Record of Employment” (ROE) form whenever a separation occurs. The data contains a rich set of measures about a worker’s pre-separation job, his or her first post-separation job, the (post-separation) job at the time of the interview, as well as on unemployment spells, and is designed to be a nationally-representative sample of all individuals separating from an employer. The focus of this data set on searchers is advantageous because employment equity directly affects job seekers.

Because we want to consider both actual and perceived effects, and because of a problem with how perceptions were measured in the second window of the survey, in this paper we only use the information from separations in the first window, which consists of 3898 individuals.<sup>12</sup> Eliminating individuals who were 65 years of age or over left us with a sample of 1586 women

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<sup>11</sup>Information on coverage by employment equity at the provincial and municipal level were obtained by contacting the municipal and/or provincial government and enquiring about whether there existed employment equity legislation for their jurisdiction. If employment equity legislation existed they were asked to send all relevant material to the authors. All this material is available from the authors upon request.

<sup>12</sup>In Cohort 2 the question on perceived discrimination was only asked of people who, at the survey date, were still searching for a job. To the extent that these individuals are still searching because they have had disappointing search outcomes, or because they can afford to search longer than others, they will be systematically different from the population of all job separations.

and 2280 men. The sample for job search outcomes is further restricted to individuals who had positive spells of unemployment and reported that they engaged in at least some search for a new job after the initial job separation.

The measure of perceived discrimination in our analysis is based on the following questions: “In any of the job search that you have done since [the separation date], do you feel that your gender has had an impact on your ability to find a good job?” To avoid framing the question in a way that might encourage responses in either direction, the allowed responses were (1) yes, hurt; (2) yes, helped; or (3) no impact. In cohort one, which forms the basis of our sample, the question was asked of all individuals, irrespective of gender, and irrespective of their employment status at the time of the interview.

Finally, we create a dummy variable for coverage by employment equity in the pre-separation job based on the legislative information presented in the preceding section. For instance, an individual is coded as one (i.e. being covered by employment equity) if they worked in a federally regulated industry, such as banking, in the pre-separation job with at least 100 employees. A dummy variable for coverage by employment equity in the post-separation job was created analogously.<sup>13</sup>

Descriptive statistics on the main variables used in our analysis are presented in Table 1. Inspection of Table 1 reveals that, on average, women in our sample are slightly older than men, but have about 10 weeks less tenure on their pre-separation job. Further, women are more likely to have higher levels of education (college and university). Women do not differ markedly from

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<sup>13</sup>It was not possible to fully capture all respondents who were employed in industries covered by employment equity as we were unable to determine if an industry was covered under either the Federal Contractors’ Program, the Quebec Contractors’ Program, or the Ontario Contractors’ Program.

men in their marital status distribution and presence of children.

Looking at the job search outcome variables, women had been unemployed about 2 weeks longer than men as of the survey date, which was usually about 22 weeks after the separation. Furthermore, the *ex ante* probability of recall is 8 percent greater for women than men, but the *ex post* probability of recall is equivalent for men and women at 27 percent. Turning to the reason for separation, men are more likely to be laid-off, less likely to quit, equally likely to be dismissed, and less likely to leave for other reasons, than women. Focusing on employment equity coverage, women are more likely than men to be covered by employment equity in both the pre-separation and the post-separation job, although the gap is substantially larger in the post-separation job. Finally, looking at perceived discrimination, about 14 percent of women, and 11 percent of men experiencing a job separation report that their gender had some effect on their ability to find a good new job, with the balance --a vast majority of both men and women-- indicating they felt their gender had no effect. Among those who said they were affected by discrimination, women were more likely to feel that they were hurt than helped, by a ratio of about 10 to 4, while men's reports were almost evenly split between those who were hurt or helped.

#### **4. Effects of Employment Equity Coverage on Job Search Outcomes**

##### **4.1 Estimation Approach**

In this section we attempt to estimate the effects of employment equity on the unemployment durations of men and women who are searching for a new job. To our knowledge this has not been attempted before; a main reason for this, we conjecture, is probably the

difficulty in determining which individuals, in a population of job searchers, are more or less likely to be affected by employment equity. In a certain sense, unemployed workers are not attached in any obvious way to a “covered” or “noncovered” sector, making it difficult to identify a source of cross-sectional variation in the extent to which one expects workers to be affected by employment equity policies.

In this paper, we confront this issue in three main ways. First, we use the fact that employment equity coverage varies across provinces, municipalities, firms, and industries in Canada, combined with an assumption of costly geographical and industrial mobility. Given such costly mobility, workers whose *preseparation* job was covered by employment equity are therefore more likely to be constrained (in the case of men) or aided (in the case of women) in their search for a new job.<sup>14</sup>

Of course, an issue in using cross-industry and across-province variation to identify the effects of employment equity is that interindustry and interprovincial labour market differences other than employment equity coverage may have important effects on job search outcomes. The second element of our approach is therefore, in addition to controlling for a variety of observed worker characteristics that vary across industries and provinces, to control for industry, province and firm size fixed effects. Once we do so, essentially all our identification is coming from interactions among these variables. We find it very hard to think of reasons, other than employment equity itself, why these very specific interactions might systematically influence workers’ job search outcomes.

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<sup>14</sup>Another reason why our analysis focuses on preseparation coverage is of course that postseparation coverage is endogenous --for example, men might be less likely to be covered in postseparation jobs if employment equity reduces the rate at which firms hire men.

Third, we use the fact that by design, employment equity's effects should be different for women and men. This allows us to estimate an effect of employment equity on the *relative* search outcomes of women and men even when there are unobserved differences between the job-finding rates of all workers separating from covered versus noncovered jobs (for example, due to unmeasured differences in industry mix-- a pure "sector" effect).

To see the latter point, we describe our estimation approach in somewhat more detail. Consider a labour market outcome variable,  $y$  (such as a re-employment hazard, measured in logs) and let the level of  $y$  for women whose pre-separation jobs are not covered by employment equity be  $\alpha$ . (In this discussion we shall abstract from the effects of other observed covariates ( $X$ 's) which might shift the hazard up or down linearly). Next, write the difference between  $\alpha$  and the hazard of women whose pre-separation jobs *are* covered by employment equity be given by the sum of two components:  $\theta + \delta^F$ . The first of these terms gives the effects of being in a covered industry that are common to men and women, and are not completely captured by the other observed covariates in the regression, such as differences in local labour market conditions and unmeasured industry characteristics. For example, it may be that, within the broad industry categories included in  $X$ , covered and noncovered firms tend to be in detailed industries facing quite different demand conditions, and thus have different re-employment rates overall. The second term is the policy effect we are interested in and gives the effect of (pre-separation) employment equity coverage on women's re-employment hazard. We expect this to be positive if employment equity opens up job prospects that would not otherwise be available to women.

Next, denote the difference between the re-employment rates of noncovered men and women as  $\gamma$ ; this captures differences that may be due to hiring discrimination as well as those

arising from other factors like the division of labour in the household. Finally, analogous to women, let the difference between the re-employment rates of covered and noncovered men be  $\theta + \delta^M$ , where  $\theta$  is the covered-sector effect that is common to women and men, and  $\delta^M$  is the causal effect of employment equity coverage on men's re-employment rates. If employment equity closes some job opportunities to men,  $\delta^M$  will be negative.

Combining all the above effects, we can write:

$$y = \alpha + \gamma M + (\theta + \delta^F)[EE * (1 - M)] + (\theta + \delta^M)[EE * M] \quad (1)$$

where  $M$  is a dummy for being male, and  $EE$  a dummy for employment equity coverage in the pre-separation job. Equation (1) can be estimated using standard techniques (for example a Cox partial likelihood model); it is important to note however that if there are unobserved characteristics of the covered sector that affect the re-employment hazards of both women and men (i.e.  $\theta \neq 0$ ), this does not identify the parameters of greatest interest,  $\delta^F$  and  $\delta^M$ . It does not, however, imply that we can learn nothing about the causal effect of employment equity on re-employment rates. To see this, rewrite (1) as:

$$y = \alpha + \gamma M + (\theta + \delta^F)EE + (\delta^M - \delta^F)[EE * M] \quad (2)$$

Equation (2) can also be estimated, and the coefficient on the interaction term between employment equity coverage and sex identifies the *differential* effect of employment equity coverage on men versus women ( $\delta^M - \delta^F$ ), even in the presence of unobserved differences



between the covered and noncovered sectors that might affect job-finding rates. Thus, even if there remain non-employment-equity induced intersectoral differences in job-finding rates that are not completely captured by our observed covariates, we can *still* determine whether the policy has an effect on the *relative* job-finding rates of women versus men: i.e. does it change the gender gap in re-employment rates?

Given the above discussion, when we discuss our results in the remainder of the paper, we shall proceed as follows. First, we discuss the estimated effects of employment equity on the gender *gap* in actual or perceived labour market outcomes, as measured by the coefficient on the interaction term in (2). This is the quantity that we believe is most precisely estimated in our data, as our estimate is robust even to industry and local labour market effects that are too fine to be captured by the industry and province dummies used in the regressions. Only then do we discuss our estimates of “absolute” effects of employment equity, i.e. whether, say, the outcome gap was closed by an improvement in women’s situation (as given by the coefficient on EE in (2)) or a deterioration in men’s (as given by the sum of the EE and EE\*M coefficients in (2)). These estimates, unlike the gap estimates, are not necessarily robust to an unobserved, sector-specific fixed effect that is not captured by our industry, province, and firm size dummies, but are, we feel, of considerable interest nonetheless.

## 4.2 Descriptive Statistics

To get an informal idea of the effect of employment equity legislation on job search outcomes, we first consider some simple descriptive measures. As previously stated, this

analysis is restricted to individuals who engaged in at least some job search after the separation.<sup>15</sup>

Table 2 shows the number of weeks of unemployment for men and women, respectively, by employment equity coverage in the pre-separation job. As in Table 1, these means exclude workers who did not experience any unemployment after their separation, and include right-censored spells that were still in progress at the survey date; thus they are rough guides to patterns in duration only. It can be seen that, as of the survey date, in the noncovered sector women had been unemployed about 2 weeks longer than men, about 22.5 weeks in comparison to 20.5 weeks. However, in the covered sector, unemployment durations among women and men are equal, at 21 weeks. While these differences do not yet control for differences in observable characteristics of separating men and women, they do provide a preliminary suggestion that coverage by employment equity in the pre-separation job reduces the relative unemployment durations of women. Further, it appears that this occurs primarily because of an increase in women's re-employment rates, rather than a reduction in men's.

Some other aspects of workers' postseparation experience in our sample are also shown in Table 2. For example, both men and women who worked in the covered sector in the pre-separation job are more likely to work in the covered sector in the post-separation job, confirming our expectation that sector-specific skills and geographical mobility costs are important. Table 2 also reports the probability of having an expected recall date ("recall") and actually returning to the same employer by employment equity coverage. Interestingly, women

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<sup>15</sup>We determined who was actively involved in search based on two questions in the COEP survey: "Did you look for work between the separation date and the first job [you held since the separation]?" (only asked of people who had a first job), and "Did you look for work between the separation date and the time of the interview?" (only asked of people who had no jobs since the separation). If the answer was no to either of these questions, the respondents were dropped from the sample.

who separated from jobs in the covered sector are much more likely to be recalled, both *ex ante* and *ex post*, than men who separated from jobs in the covered sector (e.g. the probability of *ex post* recall for women and men in the covered sector are 42 percent and 26 percent, respectively). As we shall see, this difference in recall plays an important role in explaining employment equity's effects on unemployment durations below.

### 4.3 Employment Equity Coverage and Unemployment Durations

In order to determine the effect of coverage by employment equity in the pre-separation job on unemployment durations we estimate a Cox proportional hazard model. To assess the robustness of our main result to model specification, we estimate several specifications. Specification (1) includes only the main variables of interest: gender, coverage by employment equity in the pre-separation job, and a cross term between coverage and gender. We add control variables to specifications (2) through (7), to see if, under any specification, the cross effect of gender and employment equity is substantially changed in magnitude or significance.

The results of the Cox proportional hazard regressions are presented in Table 3. Although the cross term between coverage by employment equity in the pre-separation job and gender does tend to vary in significance (in particular it falls in significance when reason-for-separation dummy variables, a seasonal dummy variable and a dummy variable for *ex ante* recall are introduced) it remains negative, and of roughly the same magnitude, across specifications. This is even the case when fixed effects for province, industry and firm size --essentially all the dimensions (except for the small number of cities with their own public sector employment equity plans) along which coverage varies in our data-- are included. The point estimates suggest

that employment equity reduces the gender gap in re-employment hazards by a substantial amount: women's relative job-finding hazards rise between 13 and 24 percent, depending on the specification. Unfortunately these effects are imprecisely measured; in most cases we cannot reject an effect of zero with 95% confidence.<sup>16</sup>

Does employment equity reduce the gender gap in re-employment rates by helping women or hurting men (or a combination of the two?). Regardless of the specification, Table 3 also shows that employment equity in a pre-separation job appears to raise women's re-employment hazard by about 20 percent (from the "EE" coefficient), but to have no effect on men's re-employment hazard (from the roughly offsetting coefficients on the "EE" dummy and the male/coverage interaction dummy). While --given our discussion of equation 2-- this could reflect, in part, unobserved differences between the covered and noncovered sectors that are common to women and men, it does however suggest that the primary effect is to help women and not to hurt men.

Finally, Table 3 also shows that quitters and laid off workers have higher re-employment hazards than workers separating for "other" reasons (the excluded category); relative to those two groups seasonal workers have a slightly lower re-employment rate. Unsurprisingly, dismissed workers have the lowest re-employment hazard of all. An *ex ante* expectation of recall to the former employer increases the re-employment hazard by 35 to 37 percent, most likely because those expectations are usually realized in fairly short order.

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<sup>16</sup>A discussant has suggested that our results might be an artifact of variation in union coverage in the pre-separation job. To check for this, we added a measure of union coverage to our most comprehensive specification, in column 7. Very little change occurred in the results.

#### 4.4 How Does Employment Equity Reduce Women's Unemployment Durations?

While the previous section indicated that employment equity coverage in a preseparation job appears to reduce women's postseparation unemployment durations relative to men's, it provided little information on the mechanisms via which this occurs. In this section we explore the importance of a direct mechanism that might produce this effect: conditional on the worker having separated from the preseparation firm, employment equity may change the relative rate at which men and women are recalled to that same firm after a period of unemployment. To see if this is the case, in Table 4 we estimate a competing Cox proportional hazard model for unemployment spells. There are two key points to note. First, the cross term between the male dummy and coverage by employment equity in the preseparation job indicates that preseparation coverage raises women's hazard into reemployment at the preseparation firm relative to men's (see column 1), but has no effect on women's relative hazard into reemployment at a different firm (see column 2). Thus, preseparation coverage by employment equity strongly increases the likelihood that a woman will return to her former employer after a period of unemployment, relative to a man. Second, bearing in mind the caveat about unobserved sectoral differences, it appears to do so primarily by raising women's hazard into reemployment at the preseparation firm (as suggested by the "EE" coefficient in column 1), and not very much by reducing men's hazard into reemployment at the preseparation firm (as suggested by the roughly offsetting "EE" and "male\*EE" coefficients in column 1).

In addition to the results on employment equity, Table 4 also shows the following about reemployment hazards. Unsurprisingly, given seniority layoff systems, high tenure workers have higher reemployment hazards at the preseparation firm than low-tenured workers, but have lower

relative reemployment hazards at a different firm. Perhaps more surprisingly, even after controlling for tenure, older workers have higher reemployment hazards at the pre-separation firm. Furthermore, having high levels of education reduces the probability of being recalled and raises the chances of finding a job elsewhere. Finally, among all reasons for separation, laid-off workers are the most likely to return to their former employer, followed by separations for “other” reasons (the omitted category), and then by quitters. Dismissed workers are by far the least likely to return.

Do the large and significant effects of employment equity on the relative recall rates of men and women found in Table 4 explain the shorter relative unemployment durations of covered women found in Table 3? To answer this question we simply added a dummy variable for *ex post* recall into the last specification of the Cox proportional hazard model in Table 3. When we did so, the coefficient on the gender/coverage interaction term essentially goes to zero (i.e. the coefficient becomes 0.04 with a z statistic of 0.28). This strongly suggests that the mechanism by which employment equity reduces the relative unemployment durations of women is through the higher relative probability --conditional on having separated and experienced some unemployment-- of actually being recalled to the pre-separation firm.

## **5. Employment Equity and Perceptions of Gender Discrimination**

### **5.1 Descriptive Statistics**

The preceding section suggests that employment equity reduces the relative unemployment durations of women who experience a job separation. It appears to do so by raising the rate at which women are recalled to their former employer, and does not appear to be

associated with a decline in men’s recall rates. How is this effect of employment equity perceived by men and women?

Table 5 reports perceptions of harmful and “helpful” discrimination by employment equity coverage for both men and women. Interestingly, in three of its four rows, perceptions of either hurtful or helpful discrimination are roughly the same in the covered versus noncovered sectors. The only exception is that men’s reports of suffering hurtful discrimination are substantially higher if their pre-separation job was covered by employment equity. Although Table 5 does not control for differences in observable characteristics of separating men and women, it provides tentative support for the notion that men feel that employment equity has hindered their ability to find a “good” job, while women are unwilling to acknowledge being helped by it. It should be noted however, that employment equity coverage only raises men’s perceptions of hurtful discrimination *to the same level as women’s*. In a sense, therefore employment equity eliminates the gender gap in perceptions of being unfairly treated; unfortunately it does so by raising men’s perceptions of unfairness rather than reducing women’s.

## 5.2 Probit Models

To get a more formal idea of the effect of employment equity on perceptions of gender discrimination we estimate two probit models of the propensity to report hurtful and “helpful” gender discrimination. We estimate the same specifications as in the job search outcome analysis, however, as previously stated, we use the full sample.<sup>17</sup> The dependent variable in the

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<sup>17</sup>Interestingly, quite a few of the men and women in this sample were prepared to report that their job search was affected by gender discrimination, even when they did not report doing any active job search. We take this as evidence that employment equity affects perceptions of fairness in some ways besides job search, and think it

regressions for “hurtful” discrimination equals one when the individual reported that his/her ability to find a “good” job was hurt by his/her gender, and zero otherwise. The “helpful” discrimination dummy was constructed analogously.<sup>18</sup>

Table 6 presents the probit estimates for hurtful gender discrimination. As suggested by the raw means, the cross term between the male dummy variable and coverage by employment equity in the pre-separation job is significant and positive: employment equity increases men’s probability of reporting hurtful gender discrimination, relative to women’s. Further, like our results on re-employment rates, the coefficient remains roughly the same in size across all specifications, even when controls for industry, province and firm size are included. Third, because the positive male\*EE coefficient substantially outweighs the insignificant, negative EE coefficient, the increase in men’s relative reports of hurtful discrimination largely takes the form of an increase in men’s, rather than a decrease in women’s reports of hurtful discrimination.<sup>19</sup> The only other variable that significantly affects reports of hurtful discrimination is university education. Individuals with a university education are more likely to report hurtful gender discrimination compared to individuals with below high school education, regardless of the specification. This may be a result of increased awareness of gender discrimination among the

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important to include these perceptions in our analysis. We did, however, also estimate the perceptions probits using the same sample as the labour market outcome variables. We get the same general results, although they are estimated less precisely.

<sup>18</sup>An alternative would be to combine reports of helpful and hurtful discrimination in an ordered probit. In another paper which focuses on workers’ perceptions in this survey in much more detail, (Antecol and Kuhn 1998) we find considerable evidence against this specification.

<sup>19</sup>As we cannot think of a convincing reason why *perceptions* of discrimination should be affected by aspects of industry mix that are too fine for us to control, we are somewhat more confident of our estimates of the effects of employment equity on levels of perceived discrimination than on the level of the re-employment hazard.



more educated population.

Table 7 presents the results for the “helpful” gender discrimination probit. Regardless of the specification, the cross effect of the male dummy variable with coverage by employment equity in the pre-separation job is now never significant and flips signs as more covariates are added to the base specification (1). Apparently, employment equity does not affect men’s or women’s reports of helpful gender discrimination. Perhaps unsurprisingly, few individuals of either sex are willing to state their gender actually helped them find a good job, even when --in the case of women and recall-- our estimates suggest it might have done so.

## **6. Conclusions**

As is reflected by its rapid imposition and subsequent withdrawal in Ontario this decade, employment equity has been one of the most controversial laws introduced in Canada. To shed some light on this issue, we have attempted to measure the effect of employment equity on the job search outcomes and on the perceptions of discriminatory treatment of both men and women using a new data set on Canadian job seekers. The data come from the period when employment equity coverage in Ontario was at levels that vastly exceed those in most other developed nations.

We find some evidence that employment equity coverage in a pre-separation job reduces the unemployment durations of women relative to men; an effect which is substantial in magnitude but imprecisely measured. Interestingly, this effect operates largely through highly significant differences in the rate at which women and men are recalled to the pre-separation employer, highlighting the (often ignored) fact that employment equity programs can change not only firms’ hiring policies, but the procedures governing employment reductions and layoffs as

well. Such effects were recently highlighted in a controversial New Jersey court case in which employment equity was used to justify race-based layoffs<sup>20</sup>, and appear to be an important, but ignored, area for future research on employment equity.

Finally, our results tentatively suggest that employment equity has lost an important public relations battle in Canada. While employment equity appears to raise unemployed women's re-employment rate, women seem unwilling to acknowledge this gain: we can detect no change in women's perceptions of discrimination. At the same time, while employment equity does *not* appear to have reduced men's re-employment rates, the policy clearly has increased the perception of reverse discrimination among men. Unless both of these perceptions change, it seems unlikely that public support for employment equity programs will increase in the foreseeable future.

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<sup>20</sup>See Pulley (1997). The Piscataway case involved two female schoolteachers, one black and one white, who were hired on the same day and had similar performance ratings. Forced to lay off one of the two, the school board chose the white woman purely for reasons of fostering diversity in the workplace. The prospects of a Supreme Court decision limiting affirmative action nationwide as a result of this case were judged so high that the NAACP provided funding to the school board to settle the suit out of court.

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**Table 1. Sample Mean Characteristics**

	Men		Women	
	Mean	Std. Error	Mean	Std. Error
Age	36.650	0.229	37.411	0.271
Preseparation Tenure (weeks)	230.528	7.786	219.786	7.193
<b>Education</b>				
Less than High School	0.336	0.010	0.251	0.011
High School	0.312	0.010	0.316	0.012
Some College or University	0.135	0.007	0.135	0.009
College	0.119	0.007	0.158	0.009
University	0.097	0.006	0.140	0.009
<b>Family Background</b>				
Married	0.611	0.010	0.619	0.012
Separated/Divorced/Widowed	0.076	0.006	0.123	0.008
Single	0.313	0.010	0.257	0.011
Children under 6	0.280	0.013	0.233	0.014
<b>Coverage by Employment Equity</b>				
Preseparation Job	0.235	0.009	0.278	0.011
Postseparation Job	0.240	0.011	0.314	0.014
<b>Reason for Separation</b>				
Laid-Off	0.670	0.010	0.548	0.013
Quit	0.145	0.007	0.171	0.009
Dismissed	0.032	0.004	0.032	0.004
Other	0.155	0.008	0.251	0.011
<b>Job Search Outcomes <sup>1</sup></b>				
Spell (weeks) <sup>2</sup>	20.623	0.376	22.159	0.501
Expected Recall	0.117	0.009	0.200	0.014
Returned to Same Employer	0.271	0.012	0.272	0.015
<b>Perceptions of Discrimination</b>				
Hurtful	0.057	0.005	0.103	0.008
Helpful	0.050	0.005	0.039	0.005
<b>Sample Size <sup>3</sup></b>	2280		1586	

Notes: 1. Individuals who did not search for work are excluded in the job search outcome analysis. Therefore, the number of observations for the job search outcome variables are 1427 and 860 for men and women, respectively. 2. Sample includes individuals with incomplete spells and excludes individuals with spell lengths of zero or less. 3. Due to missing data, the number of observations is lower for some variables. 4. Sample includes individuals between the ages 16 and 64.

**Table 2. Labour Market Outcomes, by Employment Equity in the Preseparation Job**

<b>Employment Equity (EE) in the Preseparation Job</b>		
	Covered	Noncovered
<b>Men</b>		
Unemployment Spells (weeks)	20.860	20.561
EE in First Job After Separation	0.744	0.076
Expected Recall	0.158	0.107
Same Employer	0.264	0.273
<b>Women</b>		
Unemployment Spells (weeks)	20.838	22.625
EE in First Job After Separation	0.810	0.104
Expected Recall	0.332	0.154
Same Employer	0.416	0.221

Notes: 1. Sample includes individuals between the ages of 16 and 64. 2. Sample excludes individuals who did not actively search for work between the separation date and the time they got their first job, or between the separation date and the date of the interview. 3. Sample includes individuals with incomplete spells and excludes individuals with spell lengths of zero or less.

**Table 3. Cox Proportional Hazard Coefficients for Unemployment Spells, Various Specifications**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male	0.191 (3.083)	0.184 (2.959)	0.181 (2.793)	0.181 (2.708)	0.166 (2.468)	0.085 (1.139)	0.095 (1.253)
Employment Equity (EE) 5	0.191 (2.011)	0.213 (2.230)	0.182 (1.882)	0.147 (1.494)	0.158 (1.485)	0.176 (1.605)	0.227 (1.814)
Male*EE	-0.231 (1.858)	-0.242 (1.941)	-0.233 (1.828)	-0.178 (1.368)	-0.161 (1.236)	-0.130 (0.975)	-0.147 (1.100)
Age		0.019 (1.150)	0.009 (0.513)	0.007 (0.353)	0.007 (0.377)	0.010 (0.503)	0.008 (0.408)
Age Squared		0.000 (1.847)	0.000 (1.400)	0.000 (1.191)	0.000 (1.234)	0.000 (1.321)	0.000 (1.243)
High School			0.107 (1.551)	0.126 (1.780)	0.131 (1.832)	0.117 (1.622)	0.096 (1.317)
Some College/University			0.152 (1.726)	0.152 (1.689)	0.168 (1.860)	0.151 (1.618)	0.147 (1.556)
College			0.043 (0.479)	0.080 (0.878)	0.086 (0.927)	0.090 (0.944)	0.070 (0.718)
University			0.069 (0.719)	0.080 (0.809)	0.079 (0.794)	0.109 (1.063)	0.099 (0.947)
Married			0.186 (2.548)	0.159 (2.140)	0.165 (2.215)	0.120 (1.580)	0.124 (1.615)
Separated/Divorced/Widowed			0.125 (1.144)	0.105 (0.942)	0.110 (0.991)	0.094 (0.831)	0.114 (0.993)
Children Under 6			-0.086 (1.724)	-0.087 (1.697)	-0.097 (1.880)	-0.090 (1.718)	-0.081 (1.532)
Tenure			0.000 (1.457)	0.000 (1.117)	0.000 (1.188)	0.000 (1.482)	0.000 (1.345)
Laid-off				0.156 (1.935)	0.158 (1.944)	0.155 (1.870)	0.147 (1.754)
Quit				0.217 (1.846)	0.202 (1.701)	0.252 (2.070)	0.234 (1.888)
Dismissed				-0.124 (0.777)	-0.142 (0.892)	-0.119 (0.739)	-0.138 (0.849)
Seasonal				0.122 (1.967)	0.132 (2.115)	0.091 (1.413)	0.084 (1.276)
Expected Recall				0.354 (4.742)	0.359 (4.801)	0.375 (4.857)	0.369 (4.744)
Province Dummies	No	No	No	No	Yes	Yes	Yes
Industry Dummies	No	No	No	No	No	Yes	Yes
Firm Size Dummies	No	No	No	No	No	No	Yes
Number of Observations	2269	2269	2096	2042	2041	1987	1952

Notes: 1. Absolute values of z-statistics in parentheses. 2. Sample includes individuals between the ages of 16 and 64. 3. Sample excludes individuals who did not actively search for work between the separation date and the time they got their first job, or between the separation date and the date of the interview. 4. Sample excludes individuals with spell lengths of zero or less. 5. Employment Equity (EE) is based on coverage in the pre-separation job.



**Table 4. Competing Cox Proportional Hazard Coefficients  
for Unemployment Spells**

	Individual's first job after the separation was with the same employer	Individual's first job after the separation was with a different employer
	(1)	(2)
Male	0.130 (1.039)	0.060 (0.645)
Employment Equity (EE) 6	0.446 (2.486)	0.015 (0.083)
Male*EE	-0.581 (2.919)	0.182 (0.992)
Age	0.073 (2.287)	-0.026 (1.070)
Age Squared	-0.001 (2.807)	0.000 (0.385)
High School	-0.106 (0.978)	0.223 (2.315)
Some College/University	-0.075 (0.494)	0.291 (2.424)
College	-0.252 (1.624)	0.246 (2.003)
University	0.049 (0.316)	0.200 (1.461)
Married	0.372 (2.906)	0.009 (0.097)
Separated/Divorced/Widowed	0.236 (1.257)	0.099 (0.686)
Children Under 6	-0.094 (1.222)	-0.056 (0.819)
Tenure	0.001 (5.873)	-0.001 (3.351)
Laid-off	0.753 (5.113)	-0.185 (1.808)
Quit	-0.486 (1.497)	0.209 (1.510)
Dismissed	-2.471 (2.444)	-0.044 (0.252)

Notes: 1. Absolute values of z-statistics in parentheses. 2. Sample includes individuals between the ages of 16 and 64. 3. Sample excludes individuals who did not actively search for work between the separation date and the time they got their first job, or between the separation date and the date of the interview. 4. Sample excludes individuals with spell lengths of zero or less. 5. Provincial, industry and firm size dummy variables were also included in the estimation procedure. 6. Employment Equity (EE) is based on coverage in the pre-separation job. 6. The number of observations is 1993.

**Table 5. Perceptions of Gender Discrimination, by Employment Equity in the Preseparation Job**

<b>Employment Equity (EE) in the Preseparation Job</b>		
	Covered	Noncovered
<b>Men</b>		
Hurt	0.085	0.048
Help	0.046	0.052
<b>Women</b>		
Hurt	0.095	0.106
Help	0.035	0.041

Notes: 1. Sample includes individuals between the ages of 16 and 64.

**Table 6. Probit Coefficients for Reported Hurtful  
Discrimination, Various Specifications**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male	-0.413 (5.738)	-0.420 (5.802)	-0.462 (5.955)	-0.465 (5.801)	-0.479 (5.895)	-0.511 (5.664)	-0.535 (5.808)
Employment Equity (EE) <sup>3</sup>	-0.060 (0.613)	-0.040 (0.411)	-0.076 (0.751)	-0.058 (0.556)	-0.074 (0.646)	-0.082 (0.667)	-0.184 (1.243)
Male*EE	0.351 (2.587)	0.351 (2.575)	0.388 (2.731)	0.355 (2.442)	0.364 (2.490)	0.331 (2.189)	0.377 (2.461)
Age		0.010 (0.507)	0.015 (0.685)	0.018 (0.767)	0.019 (0.842)	0.022 (0.910)	0.018 (0.726)
Age Squared		0.000 (1.060)	0.000 (0.995)	0.000 (1.037)	0.000 (1.108)	0.000 (1.185)	0.000 (1.025)
High School			0.140 (1.557)	0.110 (1.204)	0.084 (0.905)	0.105 (1.099)	0.120 (1.231)
Some College/University			0.184 (1.682)	0.158 (1.410)	0.149 (1.326)	0.212 (1.817)	0.245 (2.063)
College			0.142 (1.287)	0.091 (0.800)	0.080 (0.693)	0.151 (1.263)	0.180 (1.473)
University			0.363 (3.350)	0.365 (3.300)	0.347 (3.103)	0.429 (3.582)	0.453 (3.700)
Married			-0.195 (2.291)	-0.209 (2.411)	-0.208 (2.372)	-0.164 (1.804)	-0.121 (1.299)
Separated/Divorced/Widowed			0.077 (0.624)	0.085 (0.684)	0.077 (0.620)	0.105 (0.819)	0.158 (1.207)
Children Under 6			0.000 (0.005)	0.008 (0.124)	0.000 (0.002)	-0.011 (0.174)	-0.038 (0.574)
Tenure			0.000 (0.334)	0.000 (0.283)	0.000 (0.039)	0.000 (0.117)	0.000 (0.076)
Laid-off				-0.034 (0.397)	-0.035 (0.404)	-0.028 (0.317)	-0.046 (0.507)
Quit				-0.079 (0.716)	-0.115 (1.032)	-0.183 (1.564)	-0.209 (1.750)
Dismissed				-0.005 (0.028)	-0.023 (0.123)	-0.048 (0.252)	-0.109 (0.554)
Seasonal				-0.075 (0.909)	-0.076 (0.907)	-0.046 (0.521)	-0.045 (0.509)
Expected Recall				-0.011 (0.121)	-0.012 (0.140)	-0.007 (0.075)	0.007 (0.069)
Province Dummies	No	No	No	No	Yes	Yes	Yes
Industry Dummies	No	No	No	No	No	Yes	Yes
Firm Size Dummies	No	No	No	No	No	No	Yes
Number of Observations	3791	3791	3549	3462	3459	3376	3305

Notes: 1. Absolute values of z-statistics in parentheses. 2. Sample includes individuals between the ages of 16 and 64.  
3. Employment Equity (EE) is based on coverage in the pre-separation job.

**Table 7. Probit Coefficients for Reported Helpful  
Discrimination, Various Specifications**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male	0.108 (1.278)	0.106 (1.255)	0.120 (1.327)	0.096 (1.031)	0.112 (1.180)	0.059 (0.552)	0.061 (0.560)
Employment Equity (EE) 3	-0.077 (0.581)	-0.062 (0.466)	-0.034 (0.249)	0.002 (0.013)	-0.124 (0.819)	-0.089 (0.572)	-0.092 (0.504)
Male*EE	0.025 (0.148)	0.020 (0.118)	-0.001 (0.008)	-0.015 (0.082)	-0.035 (0.190)	0.009 (0.046)	0.009 (0.049)
Age		-0.001 (0.031)	0.005 (0.181)	0.011 (0.410)	0.014 (0.530)	0.012 (0.443)	0.008 (0.297)
Age Squared		0.000 (0.334)	0.000 (0.292)	0.000 (0.453)	0.000 (0.571)	0.000 (0.455)	0.000 (0.296)
High School			-0.176 (1.706)	-0.170 (1.600)	-0.192 (1.788)	-0.182 (1.647)	-0.161 (1.428)
Some College/University			0.113 (0.956)	0.155 (1.282)	0.151 (1.241)	0.179 (1.434)	0.208 (1.637)
College			0.114 (0.961)	0.146 (1.197)	0.119 (0.966)	0.137 (1.065)	0.163 (1.240)
University			0.059 (0.457)	0.079 (0.589)	0.064 (0.474)	0.109 (0.763)	0.118 (0.802)
Married			-0.064 (0.627)	-0.062 (0.607)	-0.079 (0.761)	-0.108 (1.019)	-0.113 (1.044)
Separated/Divorced/Widowed			-0.035 (0.228)	-0.105 (0.653)	-0.128 (0.785)	-0.130 (0.789)	-0.156 (0.919)
Children Under 6			0.023 (0.344)	0.027 (0.390)	0.027 (0.391)	0.021 (0.296)	0.031 (0.431)
Tenure			0.000 (1.220)	0.000 (0.789)	0.000 (0.708)	0.000 (0.434)	0.000 (0.525)
Laid-off				-0.034 (0.326)	-0.035 (0.332)	-0.024 (0.223)	-0.003 (0.024)
Quit				0.152 (1.190)	0.135 (1.046)	0.176 (1.321)	0.208 (1.530)
Dismissed				0.159 (0.759)	0.176 (0.831)	0.202 (0.940)	0.238 (1.094)
Seasonal				0.212 (2.383)	0.212 (2.347)	0.166 (1.736)	0.177 (1.820)
Expected Recall				-0.214 (1.886)	-0.232 (2.025)	-0.217 (1.842)	-0.204 (1.710)
Province Dummies	No	No	No	No	Yes	Yes	Yes
Industry Dummies	No	No	No	No	No	Yes	Yes
Firm Size Dummies	No	No	No	No	No	No	Yes
Number of Observations	3791	3791	3549	3462	3459	3376	3305

Notes: 1. Absolute values of z-statistics in parentheses. 2. Sample includes individuals between the ages of 16 and 64.  
3. Employment Equity (EE) is based on coverage in the pre-separation job.

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