GENDER DIFFERENTIALS IN LABOR MARKET OUTCOMES

By

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Abstract

In this dissertation, I study three aspects of gender differentials in labor market outcomes. The first essay examines why there is inter-ethnic variation in the gender wage among first generation, and among second-and-higher generation immigrants to the United States. Using the 1990 U.S. Census I contrast the role of human capital factors and "cultural" factors, i.e. differences in preferences regarding family structure and women's role in market versus home work. While human capital factors do play an important role, especially among second and higher generation immigrants, controlling for these factors does not eliminate inter-ethnic variation in the gender wage gap. In fact, for first generation immigrants, I find that even after controlling for all observable characteristics in the United States, a one percentage point increase in the home country gender wage gap is associated with a 0.9 percentage point increase in the gender wage gap across these ethnic origin groups in the United States. I argue that this positive correlation suggests the importance of cultural factors. Although I am unable to detect the effect of home country factors for second-and-higher generation immigrants, there appears to be a role for "tastes" regarding work and family, in addition to the more commonly-analyzed human capital and institutional factors, in explaining why some women earn more relative to men than others.

The second essay attempts to measure the effect of employment equity laws on job search outcomes, and on perceptions of discrimination by both men and women in a sample of Canadian job seekers. I find some evidence that employment equity coverage in a *preseparation* job reduces the relative amount of time it takes women, versus men, to become re-employed. This effect operates largely through highly significant differences in the rate at which women and men are recalled to the preseparation employer. Finally, I find that employment equity coverage reduces the gender gap in the extent to which workers feel harmed by gender discrimination. Perhaps unfortunately, this effect primarily occurs via an increase in men's perceptions of being harmed, rather than a reduction in women's.

The final chapter seeks to explain why, compared to older women, young female job seekers are more than three times as likely to report that their ability to find a good new job is compromised by the simple fact that they are female. Using the same sample of job seekers as in Chapter 2, I show, first, that young women's more frequent reports of gender-induced harm cannot be statistically attributed to any observed personal or job characteristics, or to any "objective" measure of discrimination computable in my data. Second, using new questions asked in the aforementioned survey, I note that women's reports of gender-induced *advantage*, as well as *men's* reports of gender-induced harm, are also more prevalent among the young. Using a formal model of the reporting decision, I conclude that the most likely cause of all these phenomena is a particular kind of age difference in reporting behavior: young people of both sexes are more likely than older people to interpret departures *in either direction* from gender-neutral treatment as causally affected by their gender. This may have important implications for future public support of anti-discrimination policies, and for the design of those policies.

In Memory of Patrick Dunn
A Dear Friend
Who Will Always Have a Special Place in My Heart

Preface

I wish to thank my thesis supervisor, Professor Peter J. Kuhn, for his invaluable assistance and encouragement during the preparation of this thesis and throughout my doctoral studies. My sincere thanks also goes to the other members of my thesis committee, Professors Martin Browning and Lonnie Magee, for their aid and direction.

I owe many thanks to my family and friends, for their love, support, and understanding throughout the course of my doctoral studies. Special thanks to my mother, Faigie, who provided me with a lifetime of encouragement. A special thank you also to my brother, Michael, who was both a mentor and a friend.

The second and third chapters of the thesis were prepared with the intention of publication jointly with my thesis supervisor, Professor Peter J. Kuhn. I had primary responsibility for data preparation and empirical analysis, and played a major role in the writing of the papers.

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I

Introduction

Labor economists have long been studying gender differentials in labor market outcomes. The study of gender differentials is important because it broadens our understanding of wage determination and it allows us to determine the continued relevance and success of anti-discrimination policies, such as pay equity and employment equity programs. This thesis is composed of three essays investigating gender differentials in the labor market. Ethnic differences in gender wage differentials is the first topic studied. In the second chapter, the effect of Employment Equity Legislation on gender differentials in unemployment is addressed. Finally, perceptions of gender differentials and how these perceptions are related to more standard "residual gap" measures of gender differentials in labor market outcomes are examined in chapter 3.

In the first chapter of the thesis, the determinants of ethnic differences in gender wage gaps are examined. Two streams of literature are related to this issue. The first focuses on international differences in gender wage gaps. Papers by Gregory, Daly and Ho (1986), Gregory and Daly (1991), Blau and Kahn (1996), and Kidd and Shannon (1996) investigate the role of two factors, human capital and wage setting institutions, in explaining cross country variation in the gender wage gap. These studies have found that wage setting institutions, which compress the wage structure in general, play a key role in explaining international differences in the gender wage gap. However, despite controls for both human capital factors and wage setting institutions, these studies are unable to fully account for the cross country variation in gender wage gaps.

Another determinant of ethnic differences in gender wage gaps, in addition to the more commonly analyzed human capital factors and wage setting institutions, is suggested by the second stream of literature, which examines why there are ethnic differences in married (immigrant) women's labor force participation rates within a single country (Long (1980), Reimers (1985), Duleep and Saunders(1993), and Baker and Benjamin (1994)). Of particular interest is the study by Reimers (1985) which examines inter-ethnic variation in married women's labor force participation rates in the United States. Reimers (1985) argues that cultural factors can affect married women's labor force participation rates either indirectly or directly. Cultural factors indirectly affect married women's labor force participation rates by acting through other factors, such as women's education, experience and fertility choices, while cultural factors play a direct role if ethnic differences in married women's labor force participation rates persist despite controls for observable characteristics. Despite the claims made by Reimers (1985), however, I argue that her results cannot be interpreted as "culture" unless the residual inter-ethnic variation in married women's labor force participation rates can be attributed to home country factors (i.e., labor force participation rates in the home country).

The goal of this thesis chapter is to merge the two existing literatures to assess the effect of human capital factors and "cultural" factors on ethnic differences in gender wage gaps using evidence on inter-ethnic variation in gender wage gaps within the United States. I focus on within country ethnic differences, as opposed to international differences, because within country differences allow for better observable controls of human capital factors, such as education and experience. Further, ethnic differences in gender wage gaps cannot be easily attributed to wage setting institutions since all United States residents operate under roughly the same overall wage setting institutions. Finally, gender wage gaps among immigrants and ethnic groups in the United States can be compared with the same gaps in those groups' countries of origin. As mentioned, if these gaps are related, evidence of a third determinant (such as culture) of the gender wage gap exists.

I find that while human capital factors do play an important role, especially among second-and-higher generation immigrants, controlling for these factors does not eliminate inter-ethnic variation in the gender wage gap. In fact, for first generation immigrants, I find that even after controlling for all observable characteristics in the United States, a one percentage point increase in the home country gender wage gap is associated with a 0.9 percentage point increase in the gender wage gap across these ethnic origin groups in the United States. I argue that this positive correlation suggests the importance of cultural factors. Although I am unable to detect the effect of home country factors for second-and-higher generation immigrants, there appears to be a role for "tastes" regarding work and family, in addition to the more commonly-analyzed human capital and institutional factors, in explaining why some women earn more relative to men than others.

In the second chapter of the thesis, the effect of Employment Equity (EE) legislation on labor market outcomes is explored. Employment Equity legislation is one of the most controversial laws in the United States and Canada (in the United States these programs are called Affirmative Action). Although the controversial nature of this legislation led many U.S. labor economists (e.g. Heckman and Wolpin (1976), Goldstein and Smith (1976), Beller (1978), Leonard (1984, 1989) and Smith and Welch (1984)) to examine the effect of EE on labor market outcomes, to my knowledge, no Canadian research on the effect of EE on labor market outcomes exists. The U.S. studies, in general, focus on the impact of EE on the gender and racial composition of covered firms. 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 women, or harder for men, to find good jobs? Increases in the fraction female 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 to find a job, or a good job.

The goal of this thesis chapter is to fill in the aforementioned gaps in the existing literature on the effectiveness of EE. In particular, using a new survey of Canadian job searchers, this paper attempts to measure the effect of employment equity on job search outcomes of both men and women. Furthermore, because perceptions may be even more relevant than actual outcomes in determining the level of political support for employment equity in the general population, I also examine the effect of employment equity on workers' perceptions of discrimination for both men and women.

I find some evidence that employment equity coverage in a *preseparation* job reduces the relative amount of time it takes women, versus men, to become re-employed. Although this effect is quite substantial in magnitude, it is imprecisely measured in the data. This effect operates largely through highly significant differences in the rate at which women and men are recalled to the preseparation employer. Finally, I find that employment equity coverage reduces the gender gap in the extent to which workers feel harmed by gender discrimination. Perhaps unfortunately, this effect primarily occurs via an increase in men's perceptions of being harmed, rather than a reduction in women's.

In the third chapter I examine women's perceptions of being harmed by their gender in the labor market and how these perceptions are related to the more standard "residual gap" measures of discrimination computed by economists. In a number of recent papers (Kuhn (1987, 1990); Barbezat and Hughes (1990); Heywood (1992); Hampton and Heywood (1993, 1996); and Laband and Lentz (1993, 1998); and Johnson and Neumark (1997)) labor economists have begun to analyze aspects of labor market discrimination that, unlike more standard "residual gap" measures, are directly derived from survey reports of discriminatory, or unfair treatment. One reason for this is simply as a check on the validity of residual gap measures. As is well known, these measures suffer from a number of problems, including measurement error (e.g. Kamalich and Polachek 1982, Solon 1983, Goldberger 1984), endogeneity of measured characteristics (e.g. Blau and Ferber 1986, England 1992), unmeasured taste differences (Filer 1983), and probably most importantly, the assumed identity between discrimination and any differences in labor market outcomes that cannot be explained by the characteristics typically measured in socioeconomic surveys. If patterns in reports of discriminatory treatment do not coincide with patterns in these standard measures of discrimination, then a re-examination of what is captured by both measures may be warranted. Another is a more fundamental suspicion that residual gap measures of discrimination are very different from what real people actually perceive as discriminatory. For many issues, including the degree of political support for anti-discrimination policies, these perceptions may be much more important than residual gap measures, and thus may warrant serious study in their own right.

In some previous studies of reported discrimination, authors have noted that there is a greater tendency for younger women to feel more harmed by discrimination than older women (see for example, Kuhn (1987) and Laband and Lentz (1998)). This seems somewhat puzzling given the casual notion that young women today face fewer discriminatory impediments than any of their forebears. It seems important to know how to interpret this phenomenon, because if newer cohorts of women are indeed facing problems not detected by standard measures, one would like to know why, and what these problems are.

The goal of this chapter is to shed some new light on why young women are much more likely to report being harmed by their gender in the labor market than older women, using information from a new survey of Canadian job seekers. This survey is the first, to my knowledge, to include detailed information on individuals' perceptions that their labor market success was *enhanced* by their gender, and on *men's* perceptions of gender effects as well as women's. This additional information is used to sharpen my hypotheses and eliminate certain simple explanations for why perceptions of harm are so much greater among young women.

I show, first, that young women's more frequent reports of gender-induced harm cannot be statistically attributed to any observed personal or job characteristics, or to any "objective" measure of discrimination computable in my data. Second, using the new questions asked in the aforementioned survey, I note that women's reports of gender-induced *advantage*, as well as *men's* reports of gender-induced harm, are also more prevalent among the young. Using a formal model of the reporting decision, I conclude that the most likely cause of all these phenomena is a particular kind of age difference in reporting behavior: young people of both sexes are more likely than older people to interpret departures *in either direction* from gender-neutral treatment as causally affected by their gender. This may have important implications for future public support of anti-discrimination policies, and for the design of those policies.

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II

Why is there Inter-Ethnic Variation in the Gender Wage Gap?

The Role of "Cultural" Factors

A number of recent studies (e.g., Gregory, Daly and Ho (1986); Gregory and Daly (1991); Blau and Kahn (1996); and Kidd and Shannon (1996)), have attempted to explain the sizeable variation in the gender wage gap across industrialized countries. A key question addressed by these studies has been the relative role of two factors --human capital versus wage-setting institutions-- in explaining the gender wage gap. Analysts have argued, for example, that the small gender wage gaps seen in many European countries are not due to smaller gender gaps in women's observable qualifications, but simply to institutions which compress the wage structure in general.

Even after accounting for human capital and institutional differences, however, there still remain substantial international differences in the gender wage gap. What explains these differences? Everyday conversations and casual empiricism often invoke "cultural" factors, such as differences in preferences regarding family structure and women's roles in market versus home work, yet economists have been reluctant to invoke such explanations due to difficulties in testing them.[1]

In this paper I try to assess the effect of cultural factors on gender wage gaps using evidence on inter-ethnic variation in the gender wage gap within the United States.[2] I argue that these differentials are informative about culture for the following reasons. First, in contrast to international differences, differences between ethnic groups in one country --the United States--cannot easily be attributed to institutional factors, since all United States residents operate under roughly the same overall wage-setting regime. Second, compared to international studies, within-country studies offer better observable controls for human capital factors, like education and experience. Third, gender wage gaps among immigrants and ethnic groups in the United States can be compared with the same gaps in those groups' countries of origin. If these gaps are related, evidence of a third determinant of gender wage gaps exists. This factor is transmitted between countries with different wage setting institutions, as one would expect to be the case for cultural attitudes to family and work.

I begin in Section 2 by describing the data used in the study. I then document the existence of inter-ethnic variation in the unadjusted gender wage gap for first generation,

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[1]There have been several studies which examine the role of culture in other contexts. For example, Reimers (1985) examines the role culture plays in explaining inter-ethnic variation in female labor force participation rates; Caroll, Rhee and Rhee (1994) examine the role culture plays in explaining cross country variation in the saving rates.

[2]While a number of authors have studied international differences in the gender wage gap, and immigrant-native or ethnic wage differences for both men and women, perhaps surprisingly, to my knowledge this is the first study to directly study inter-ethnic variation in the gender wage gap within a single country.

and for second-and-higher generation immigrants, in Section 3. Next, I explore the role differences in personal characteristics across ethnic origin groups play in explaining the inter-ethnic variation in the unadjusted gender wage gap. I examine two types of personal characteristics: those that are likely to affect wages but seem unlikely to be correlated with inter-ethnic cultural differences (such as age, year of arrival, and region) and those that influence wages, but might depend on inter-ethnic cultural differences (such as women's education, experience, and fertility choices). In Section 5, I examine the correlations between the gender wage gap among immigrants and ethnic origin groups in the United States with the same gaps in those groups' countries of origin.[3] Section 6 concludes.

2. Data

The data set used for the host country analysis is the 1990 United States Census five percent Public Use Microdata sample . The data contains indicators of ethnic origin (ancestry, race and place of birth), a rich set of labor market variables (employment status, hours worked in the previous year, weeks worked in the previous year, wages and salary in the previous year, industry and occupation), and personal characteristics (age, year of arrival (for first generation immigrants), education, marital status, fertility,

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[3]The role of home country factors, in different contexts, has been examined in several studies in the past. For example, Borjas (1987) examines whether home country factors explain native/immigrant wage differentials, all else being equal; and Fairlie and Meyer (1996) examine whether home country factors explain the residual inter-ethnic variation in self-employment rates.

English fluency, and region).

I restrict the sample to individuals between the ages of 25 and 54 who earned positive wages in 1989. Individuals earning less than $1 per hour or greater than $100 per hour arc excluded from the sample.[4] Further, individuals who earned self-employment income in 1989, and those attending school at the time of the survey are excluded from the sample. Additionally, first generation immigrants whose parents were born in the United States are excluded from the sample. Finally, I restrict second-and-higher generation immigrants to a 1 percent sample (as opposed to the full 5 percent sample) in order to obtain a manageable sample size, by randomly choosing 20 percent of the original sample.

Because I want to compare outcomes of immigrants and ethnic groups in the United States with the same outcomes in those groups' countries of origin, I need individuals in the United States to be linked as closely as possible with their country of origin or the country of origin of their ancestors. Therefore, two approaches are used to determine an individual's ethnic origin. Place of birth is used to determine the ethnic origin of first generation immigrants, i.e., individuals born outside of the United States. For second-and-higher generation immigrants, who by definition are born in the United States, ancestry is used. To facilitate the estimation of differences between ethnic origin groups, second-and-higher generation immigrants who reported multiple ancestries are

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[4]Hourly wages are calculated as wages and salary in 1989 divided by (weeks worked in 1989 times hours worked in 1989).

excluded from the sample.[5] Finally, because the population of the United States mainly consists of immigrants or their descendants, second-and-higher generation immigrants who identified themselves as "Americans" in the ancestry question are excluded from the analysis.

Based on the above criteria, I restrict the sample to 21 ethnic origin groups because these are the most detailed groups that I can make comparable across first generation, across second-and-higher generation immigrants, and across home countries, and have large enough sample sizes (See Figure 1 for a list of these countries). This leaves a first generation immigrant sample size of 85,996 males and 65,407 females, and a second-and-higher generation immigrant sample size of 129,415 males and 107,151 females. For the breakdown of sample size by ethnic origin group see Appendix I.

The wage data used for the home country analysis is from the ILO Yearbook of Labour Statistics, various years with the following exceptions: wage data for Austria and Italy is from Blau and Kahn (1996); wage data for Mexico, provided by the Commission for Labor Cooperation, is from STPS/INEGI, Encuesta Nacional de Empleo (ENE); and the wage data for Canada is from the 1990 Survey of Consumer Finance (SCF) for the 1989 income year. The home country wage data is based on 1989 hourly wages in the

[5]Note the following exception: individuals who reported multiple United Kingdom ancestries (i.e., British and Scottish) are included in the sample.

non-agricultural sector.[6]

There is probably substantial measurement error in the home country wage data because, as indicated above, the home country wage data is from a number of different sources. This variation in sources causes the home country wage data to be based on different industries, different units, and different years. For example, countries where the wage data is based on monthly wages implicitly assumes that men and women would have to work the same number of hours per month for the gender wage gap to be the same as it would have been if hourly wage data had been observed.

3. The Unadjusted Gender Wage Gap

Figure 1 presents the unadjusted gender wage gap for first generation, and for second-and-higher generation immigrants within the United States. The unadjusted gender wage gap within each ethnic origin group is measured as the difference in the mean log hourly wages of men minus the mean log hourly wages of women.

Figure 1 reveals the following patterns. First, there is considerable variation in the

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[6]Note the following exceptions: wage data for Czechoslovakia, Hungary, Japan and the Philippines is based on monthly wages; wage data for Mexico is based on weekly wages; wage data for Austria is based on monthly wages adjusted for hours worked; wage data for Italy is based on annual wages adjusted for hours worked; hourly wage data for Finland, Greece, Ireland, Norway and Sweden is based on the manufacturing industry; the wage data for Mexico and the United Kingdom include agricultural workers; wage data for the Philippines is based on 1993 wages; wage data for Hungary is based on 1992 wages; wage data for Mexico is based on 1991 wages; wage data for Austria is based on 1985-1989 pooled wages; and wage data for Italy is based on 1987 wages.

unadjusted gender wage gap across ethnic origin groups for both first generation, and for second-and-higher generation immigrants, but the variation is greater for first generation immigrants. For example, for first generation immigrants the unadjusted gender wage gap ranges from 12.4 percent for Filipinos to 70.5 percent for the Japanese whereas for second-and-higher generation immigrants it ranges from 17.9 percent for Filipinos to 48.9 percent for Belgians. Second, these differences are not confined to variation between "traditional" (i.e., European) and newer source countries for immigrants. For example, for first generation immigrants the unadjusted gender wage gap ranges from 34.8 percent for Greeks to 59.9 percent for the English, and for second-and-higher generation immigrants the unadjusted gender wage gap ranges from 30.4 percent for the Portugese to 48.9 percent for Belgians.[7] Finally, inspection of Figure 1 suggests that there is assimilation towards the United States mean gap of 32.9.[8] In particular, first generation ethnic origin groups with gender wage gaps that are substantially higher than the U.S. mean have gender wage gaps which are much closer to the U.S. mean after one generation away from the home country, while the reverse is true for first generation ethnic origin groups with gender wage gaps that are considerably lower than the U.S.

[7]Difference across European ethnic origin groups is not confined to differences in the unadjusted gender wage gaps. For instance, Fairlie and Meyer (1996) find substantial differences in self-employment rates across European ethnic origin groups in the United States.

[8]The overall United States unadjusted gender wage gap is based on the entire 1990 United States Census 5 percent sample for first generation immigrants, and a 1 percent sample for second-and-higher generation immigrants. The sample includes only individuals between the ages 25 and 54 who earned positive wages, were not enrolled in school, earned between $1 per hour and $100 per hour, and did not have self-employment income.

mean. For example, Norwegians have a gender wage gap of 56 percent in the first generation and a gender wage gap of 40 percent in the second-and-higher generation while Mexicans have a gender wage gap of 22 percent in the first generation and a gender wage gap of 27 percent in the second-and-higher generation.

4. The Role of Personal Characteristics

Although the results in the preceding section are suggestive, much of the inter-ethnic variation in the unadjusted gender wage gap may simply result from differences in personal characteristics across ethnic origin groups. For example, year of arrival may play a key role in explaining inter-ethnic variation in the gender wage gap among first generation immigrants. There exist large differences in the mean year of arrival across these ethnic origin groups, and year of arrival may proxy for investment in host country specific human capital. In particular, Long (1980) finds that earnings of immigrant women increase with year of arrival, i.e., the more recently immigrant women arrived, the more they earn, and earnings of immigrant men decrease with year of arrival.[9] Education may also play an important role in explaining inter-ethnic variation in the gender wage gap, as there are substantial differences in the amount women invest in education relative

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[9] Long attributes this to the family investment decision in host country specific human capital. Baker and Benjamin (1994) examine the family investment decision model. They find that first generation immigrant women married to first generation immigrant men are more likely to work upon arrival in the host country, i.e., Canada, in jobs which do not require host country specific human capital and have little room for future advancement in order to facilitate the host country specific human capital accumulation of their husbands.

to men across ethnic origin groups.

In this section I focus on two types of personal characteristics: "exogenous" personal characteristics (X) and "potentially endogenous" personal characteristics (Z). An exogenous personal characteristic is any characteristic that influences wages but seems unlikely to be correlated with "cultural" factors--i.e., differences in individuals' tastes regarding family structure and women's role in market versus home work. Exogenous personal characteristics include a quartic in age, 9 regional dummy variables, a dummy variable for metropolitan status, and 8 year of arrival dummy variables (for first generation immigrants). Analogously, a potentially endogenous characteristic is any characteristic that influences wages, but could also depend on cultural factors. Potentially endogenous characteristics include years of education, English fluency, number of children and marital status.

4.1 Regression Results

In order to predict an "unadjusted" gender wage gap, I first estimate the following pooled regression for men and women:

$$w_i = a + cM_i + \sum_{j=1}^{J-1} d_j^1 E_{ij} + \sum_{j=1}^{J-1} d_j^2 (E_{ij} M_i) + e_i \tag{1}$$

where w_i is the log wage of person i, M_i is a "male" dummy variable, E_{ij} are ethnic origin dummy variables, and j indexes the ethnic origin group. The d^2_j's from this regression reflect whether the unadjusted gender wage gap varies across ethnic origin groups.

Second, assigning the left out ethnic origin dummy a value of zero, I re-normalize the $\hat{d}_j^2$'s from equation (1) as deviations from the mean as follows:

$$\hat{d}_j^{2*} = \left(\hat{d}_j^2 - \left(\frac{\sum_{j=1}^{J} \hat{d}_j^2}{J} \right) \right) \tag{2}$$

This re-normalization is employed because it allows for easy comparison and it is used in the calculation of the weighted standard deviation (WSD) measure discussed below.

Now, to see the role of personal characteristics, I re-estimate equation (1) first adding controls for only exogenous personal characteristics, and then adding controls for both exogenous and potentially endogenous personal characteristics.[10] I will refer to the former specification as the X-adjusted gender wage gap and the latter specification as the X,Z-adjusted gender wage gap. The coefficient estimates of interest from these regressions are also re-normalized according to equation (2).

The estimates of the $\hat{d}_j^{2*}$'s for the unadjusted, for the X-adjusted, and for the X,Z-adjusted gender wage gaps across ethnic origin groups for first generation, and for second-and-higher generation immigrants are presented in Table 1. The results in Table 1 can be interpreted as follows: a negative coefficient implies that a given ethnic origin

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[10]All exogenous and potentially endogenous personal characteristics are also interacted with the male dummy variable. Because number of children is only observable for women, it is only included as a direct term.

group has a gender wage gap that is smaller than the average gender gap of all ethnic origin groups.

Inspection of Table 1 reveals the following patterns. First, for first generation immigrants, the ethnic origin variables frequently have a large and significant impact on the gender wage gap for the unadjusted, for the X-adjusted, and for the X,Z-adjusted gender wage gaps. For example, depending on the specification, Filipinos have a gender wage gap that is between 31 and 34 percentage points lower than the average gender wage gap of all ethnic origin groups while the English have a gender wage gap that is between 12 and 15 percentage points above the average gender wage gap of all ethnic origin groups. Furthermore, the magnitude of the ethnic origin coefficients, in general, remains stable across the three specifications for first generation immigrants.

For second-and-higher generation immigrants the impact of the ethnic origin variables on the gender wage gap are not as large as they are for first generation immigrants across all three specifications. For instance, depending on the specification, the gender wage gap of Filipinos is now only between 10 and 19 percentage points lower than the average gender wage gap of all ethnic origin groups while the gender wage gap of the English is now only between 1 and 3 percentage points higher than the average gender wage gap of all ethnic origin groups. Additionally, for second-and-higher generation immigrants the ethnic origin coefficients, in general, decline in magnitude as more control variables are added. For example, the gender wage gap of Belgians range from 12 percentage points higher than the average for the unadjusted gender wage gap, 7 percentage points higher than the average for the X-adjusted gender wage gap, and 3 percentage points higher than the average for the X,Z-adjusted gender wage gap. Finally, the F-test shows that the ethnic origin variables are jointly significant for both first generation, and for second-and-higher generation immigrants for the unadjusted, for the X-adjusted, and for the X,Z-adjusted gender wage gaps.

4.2 Weighted Standard Deviation Measure

As a simple summary measure of the importance of differences in personal characteristics across ethnic origin groups, I calculate a measure of total inter-ethnic variation in the gender wage gap, called the weighted standard deviation (WSD).[11] The WSD is measured as follows:[12]

$$SD(d^{2*}) = \sqrt{\frac{1}{J}\sum_{j=1}^{J}(\hat{d}_j^{2*})^2 - \frac{1}{J}tr(V^*)} \tag{3}$$

where $\hat{d}_j^{2*}$ is mean difference in the $\hat{d}_j^{2}$'s from equation (1) and tr(V^*) is the trace of V^*, which is the variance/covariance matrix of $\hat{d}_j^{2*}$.[13] The first term of SD(d^{2*})

[11]This measure can be attributed to Krueger and Summers (1988), who used this technique to explain industry wage differentials. Their original measure however is sensitive to which industry is used as the left out category. Haisken-DeNew and Schmidt (1997) correct this problem. Thus, I use the Haisken-DeNew and Schmidt measure.

[12]The underlying weights in the WSD measure I employ are equal weights for each ethnic origin group because the unit of observation I am interested in is the group not the individual. Equal weights for each ethnic origin group are implicitly assumed in the calculation of equation (2).

[13]For details on how the variance-covariance matrix is calculated see Haisken-DeNew and Schmidt (1997).

represents the sample variance and the second term represents a term which corrects for the least squares sampling errors.[14] Therefore, $SD(d^{2*})$ is a summary statistic of the total inter-ethnic variation in the unadjusted gender wage gap. I calculate the WSD for the X-adjusted, and the X,Z-adjusted gender wage gaps analogously. While inter-ethnic variation in the gender wage gap can be attributed to personal characteristics if the WSD for the adjusted gaps are substantially smaller than the WSD for the unadjusted gap, most of the inter-ethnic variation in the gender wage gap remains unexplained if the WSDs remain similar in magnitude.

The weighted standard deviations (WSDs) of the unadjusted, the X-adjusted, and the X,Z-adjusted gender wage gaps are presented in the bottom line of Table 1. The WSD for the unadjusted gender wage gap for first generation immigrants is 11.25 log points whereas for second-and-higher generation immigrants it is 5.80 log points. For both first generation, and for second-and-higher generation immigrants I partition the "unadjusted" WSD's into three components: explained by X, explained by adding Z, and unexplained.

For first generation immigrants the three components of the unadjusted WSD, which is 11.25 log points, are: -0.86 log points, 0.75 log points, and 11.36 log points,

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[14]See Krueger and Summers (1988) and Haisken-DeNew and Schmidt (1997) for a more detailed discussion of the correction term.

respectively.[15] For second-and-higher generation immigrants the three components of the unadjusted WSD, which is 5.80 log points, are: 2.75 log points, 0.63 log points, and 3.38 log points, respectively.[16] These results illustrate that even after controlling for both exogenous and potentially endogenous personal characteristics there remain substantial differences across ethnic origin groups for both first generation, and for second-and-higher generation immigrants.[17] In fact for first generation immigrants the unexplained component is larger than the original unadjusted WSD. It is not surprising that the unexplained component is much larger for first generation immigrants since this is the group of individuals where one would expect cultural differences to be greater. Further, potentially endogenous characteristics play a limited role in explaining the inter-ethnic variation in the gender wage gap for first generation immigrants while for second-and-higher generation immigrants these characteristics play an important role. Finally,

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[15]For first generation immigrants, as well as for second-and-higher generation immigrants, the variables that cause these changes in the WSD measures are highly jointly significant (i.e., a p-value of 0.0000).

[16]The fact that X explains a large portion of the unadjusted WSD for second-and-higher generation immigrants may be explained by the fact that the age distribution of this group is very dispersed. For instance, second-and-higher generation immigrants from European countries tend to be much older and have higher gender wage gaps on average than second-and-higher generation immigrants from Asian countries. Antecol and Kuhn (1998) find that the gender wage gap is smaller among young individuals than old individuals. Thus, once age is controlled for the gender wage gap among European countries and Asian countries move closer together, which causes the overall dispersion to be reduced.

[17]It may be argued that occupation and industry choices should be included in potentially endogenous personal characteristics. The inclusion of these additional variables, however, does not change the overall findings. In particular, inter-ethnic variation persists despite controls for potentially endogenous personal characteristics, including occupation and industry choices, for both first generation, and for second-and-higher generation immigrants, and the unexplained component remains substantially larger for first generation immigrants.

although potentially endogenous characteristics are important for second-and-higher generation immigrants, exogenous personal characteristics also play an important role in explaining inter-ethnic variation in the gender wage gap for this group.[18]

5. The Role of Home Country Factors

The previous section illustrated that, despite controls for personal characteristics, there continues to exist inter-ethnic variation in the gender wage gap. In this section I compare the gender wage gaps among first generation, and among second-and-higher generation immigrants in the United States with the gender wage gaps in those groups' countries of origin. If these gaps are related, evidence of a determinant, other than human capital factors and wage setting institutions, of the gender wage gap exists. This factor is transmitted between countries with different wage setting institutions, as one would expect cultural attitudes to family and work to be transmitted via socialization.

5.1 Descriptive Statistics

Table 2 presents the unadjusted gender wage gap for the home country. For comparison, it also reproduces the unadjusted, the X-adjusted, and the X,Z-adjusted

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[18]Although I am more concerned with the unexplained component of the inter-ethnic variation in the gender wage gap, the order in which I introduce X and Z into the regression will of course influence how much of the inter-ethnic variation in the gender wage gap can be attributed to X and Z. To see the effects of this I re-estimate the model adding Z first and then adding X. I find for first generation immigrants that 0.44 log points is now explained by Z and -0.55 is now explained by adding the X's. For second-and-higher generation immigrants I find that 2.16 is now explained by Z and 1.22 is now explained by adding X's.

gender wage gaps for the host country from previous sections.[19] There are several key points to note. First, there is large variation in the gender wage gap across home countries. The gender wage gap ranges from 9.6 percent for the Philippines to 68.6 percent for Japan. Second, as for inter-ethnic gaps within the U.S., there is large variation in the gender wage gap across European countries. For instance, the gender wage gap in Sweden is 11.1 percent while the gender wage gap in Switzerland is 38.9. Finally, the gender wage gap in the home country is, in general, smaller than the gender wage gap in the United States for both first generation, and for second-and-higher generation immigrants. This pattern might result from cross country differences in wage setting institutions. In particular, Blau and Kahn (1996) find that the gender wage gap in the United States is higher than other developed countries because the United States has highly decentralized wage setting institutions compared to other developed countries.[20] Therefore, it is not surprising that the gender wage gaps in the home country are smaller than the gender wage gaps in the host country because the host country in this analysis is the United States and many of the home countries are the same as those in Blau and Kahn

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[19]In general, the unadjusted gender wage gap within a home country is measured as ln(average male wage) minus ln(average female wage). It should be noted that while the home country data is based on logs of means, with the exception of Canada, Austria, and Italy, whose gender wage gaps are based on means of logs, the host country data is based on means of logs.

[20]Centralized wage setting institutions are characterized by greater wage equality and smaller gender wage gaps than are decentralized wage setting institutions because wages in centralized wage setting institutions-- particularly at the bottom of the distribution--are kept up due to minimum wages and unions and women are more likely to be at the bottom of the wage distribution. For more detailed information on the role of wage setting institutions see Blau and Kahn (1996) and Kidd and Shannon (1996).

(1996).

5.2 Estimation Approach

In order to estimate the importance of "portable" cultural factors, I estimate equations of the following form:

$$\hat{d}_j^{2*}(X) = \beta_1 h_j + \varepsilon_j \tag{4}$$

$$\hat{d}_j^{2*}(X,Z) = \beta_2 h_j + \varepsilon_j \tag{5}$$

where $\hat{d}_j^{2*}(X)$ and $\hat{d}_j^{2*}(X,Z)$ are the normalized regression coefficients of the X-adjusted and X,Z-adjusted gender wage gaps from the previous section, respectively, h_j are the mean differences in the home country gender wage gaps, and j indexes the ethnic origin group.[21] β_1 and β_2 are the parameters of interest. If $\beta_1>0$, then a 1 percentage point increase in the home country gender wage gap is associated with a β_1 percentage point increase in the host country X-adjusted gender wage gap. I argue that this reflects portable cultural factors, including personal characteristics that may themselves be affected by culture, such as the presence of children, marriage and education. Further, if after controlling for potentially endogenous personal characteristics (i.e., Z) $0<\beta_2<\beta_1$, then a 1 percentage point increase in the home country gender wage gap is still associated

[21]The two stage estimation approach for the linear random effects model was proposed by Amemiya (1978), and was adapted by Borjas and Sueyoshi (1994) for probit models with structural group effects. Fairlie and Meyer (1996) use the Borjas and Sueyoshi approach to determine the role home country factors play in explaining inter-ethnic variation in self-employment rates.

with a β_2 percentage point increase in the host country X,Z-adjusted gender wage gap. I argue that this must reflect portable cultural factors that are not captured by measures of childbearing, education, and marriage. Alternatively, it can be argued that the above correlations reflect unobserved differences in human capital factors across ethnic origin groups. This could be, but the main reason unobserved human capital factors differ across ethnic origin groups could itself be driven by cultural factors.[22]

I estimate equations (4) and (5) by ordinary least squares (OLS) and generalized least squares (GLS). I employ GLS to take into account the fact that the dependent variable is estimated (Borjas and Sueyoshi, 1994). For illustration purposes, I focus on equation (4) although an analogous argument can be made for equation (5). The underlying model of equation (4) is:

$$d_j^{2*}(X) = \beta_1 h_j + u_j \qquad \text{and} \qquad \hat{d}_j^{2*}(X) = d_j^{2*}(X) + v_j \tag{6}$$

Therefore, equation (4) can be rewritten as:

$$\hat{d}_j^{2*}(X) = \beta_1 h_j + u_j + v_j \tag{7}$$

Assuming u_j and v_j are independent, the weighting matrix for the GLS estimation is equal to:

$$\Omega = \mathrm{Var}(u) + \mathrm{Var}(v) = \sigma_u^2 I + \hat{V}^{2*} \tag{8}$$

where $\hat{\sigma}_u^2 I$ is a scalar variance-covariance matrix assumed for u, and $\hat{V}^{2*}$ is the

[22]It should also be noted that unobserved human capital factors must differ for men and women. This need for differences across men and women strengthens the cultural argument.

estimated variance-covariance matrix of the coefficient estimates of the mean difference in the gender wage gap across ethnic origin groups.[23, 24]

5.3 Results

Panels 1 and 3 of Table 3 present the regression results for equations (4) and (5) for first generation, and for second-and-higher generation immigrants, respectively. The following results are noteworthy. First, in general the OLS and GLS estimates are similar. Therefore, the remaining discussion focusses on the GLS estimates only. Second, for first generation immigrants the coefficient estimates on the home country gender wage gaps are positive. In particular, a 1 percentage point increase in the home country gender wage gap is associated with a 0.69 percentage point increase in the X-adjusted host country gender wage gap, while controlling for potentially endogenous personal characteristics (Z) still implies a 1 percentage point increase in the home country gap is associated with a 0.63 percentage point increase in the X,Z-adjusted host country gender wage gap. Third, the coefficient estimates on the home country gender wage gap are significant at less than the one percent level. This significant relationship persists despite the large measurement error associated with the home country wage data. Therefore, for first generation immigrants, portable cultural factors play a key role in

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[23]See Appendix II for details on how the matrix Ω is estimated.

[24]Alternatively, I could have weighted equation (4) by ethnic origin group size; however, this either implies that the Var(u) is zero, or that the Var(u) is also related to ethnic origin group size in the same way (proportionally) the Var(v) is.

explaining inter-ethnic variation in the gender wage gap, with unobservable portable cultural factors being of the utmost importance.

Fourth, for second-and-higher generation immigrants portable cultural factors do not appear to play a role in explaining inter-ethnic variation in the gender wage gap. In fact, the coefficient estimate for the X-adjusted gender wage gap does not go in the expected direction. One of the reasons home country factors play a role for first generation immigrants but not for second-and-higher generation immigrants has to do with the labor market outcomes of Japanese immigrants. In particular, Table 2 illustrates that first generation Japanese immigrants have high gender wage gaps, ranging from 70.5 to 75.9 percent depending on the specification, which is consistent with the gender wage gap of 68.6 percent in Japan. However, after one generation away from the home country, the gender wage gap of Japanese immigrants, which ranges from 27.0 to 31.1 percent, is much smaller than the gender wage gap in Japan. Furthermore, after one generation away from the home country the role of the Japanese woman in home versus market work changed dramatically. Japanese second-and-higher generation women are less likely to be married, have smaller families and invest heavily in human capital factors (i.e., education). These results provide preliminary evidence that there appears to be a complete reversal in the cultural attitudes towards and of Japanese women after one generation away from the home country. Further investigation is needed to add to our understanding of how cultural factors affect the labor market outcomes of Japanese women.

Finally, for both first generation, and for second-and-higher generation immigrants, there remains an unexplained component of the inter-ethnic variation in the gender wage gap. This is based on the fact that the estimates of the R-squared presented in Panels 1 and 3 of Table 3 range from 0.02 to 0.35. Therefore, portable cultural factors explain as little as 2 percent and as much as 35 percent of the variation in the gender wage gap across ethnic origin groups. I propose that some of the remaining unexplained component may be attributed to labor market discrimination-i.e., equally qualified individuals are being paid differently based solely on ethnic background. It should be noted however, that, as for unobserved human capital factors, in order for labor market discrimination to explain some of the remaining inter-ethnic variation in the gender wage gap, discrimination would have to affect men and women of a given ethnic origin group differently.

5.4 Robustness Checks

One limitation of the above analysis is that the home country gender wage gaps are based on 1989 wage data while the year of arrival of immigrants into the United States date as far back as pre-1950s. This may be important, because, as Figure 2 illustrates, the gender wage gap in the home country has changed dramatically over the 1946 to 1989 period. In all countries, with the exception of Japan and France, the gender wage gap has declined over time. Furthermore, in general, the gender wage gaps in all home countries stabilized in the mid-1970's. One way to overcome this measurement error bias is to re-estimate equation (4) and (5) with home country data from the mid-1970's. However, home country data dating as far back as the mid-1970's is only available for a subset of the home countries.[25] Therefore, I choose to re-estimate equation (4) and (5) for first generation immigrants who immigrated to the United States after 1975 using home country data from 1989. Because, for those countries for which we have historical data home country gender wage gaps tend to be stable after the mid-1970's, this approach is an alternative method to overcome the aforementioned measurement error bias.

Panel (2) of Table 3 presents the results for first generation immigrants who arrived in the United States after 1975. It can be seen that once again, portable cultural factors play a key role in explaining inter-ethnic variation in the gender wage gap, with unobservable portable cultural factors being of the utmost importance. Furthermore, the magnitude of the coefficients are larger than for the full sample first generation estimates. In particular, a 1 percentage point increase in the home country gender wage gap is associated with a 0.93 percentage point increase in the X-adjusted host country gender wage gap, while controlling for potentially endogenous personal characteristics (Z) still

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[25]Because the home country data is available for only a sub-set of home countries (Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Japan, Netherlands, Norway, Sweden, Switzerland, and the United Kingdom), the precision with which equation (4) and (5) can be estimated is greatly reduced. For example, I re-estimated equation (4) by OLS for the full sample first generation immigrants using home country data from 1975 for these 13 home countries. Although the coefficient on the home country gender wage gap has the expected positive sign, it is not significant at conventional levels. Further, the magnitude of the coefficient is also greatly reduced (i.e., the coefficient estimate is now 0.3445).

implies a 1 percentage point increase in the home country gap is associated with a 0.89 percentage point increase in the X,Z-adjusted host country gender wage gap. Therefore, it can be argued that if the use of 1989 home country numbers are biasing the full sample results, they are biasing them downwards.[26]

An additional concern with these results is sample selection bias: of necessity my wage regressions only include individuals who earn positive wages. This is not so much a problem for men because their probability of employment is similar across ethnic origin groups, but it might be problematic for women. For example, Reimers (1985) illustrates that female labor force participation rates vary substantially across ethnic origin groups within the United States. Typically, the selection problem that researchers are most concerned about is that only the most "able" women participate in the labor market. As a result, women who participate in ethnic origin groups with low participation rates are disproportionately highly able women who receive high wages relative to men's wages in that group.

In order to control for this type of sample selection bias I use an estimation procedure proposed by Card and Payne (1997). Once again I focus on equation (4) for illustration purposes, although an analogous approach can be applied to equation (5). Thus, I re-estimate equation (4) by GLS as before, except this time I add a regressor which controls for the fraction of women who worked across ethnic origin groups.

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[26]It should be noted that because I control for the length of time an individual has been in the United States, the higher coefficients in Panel 2 of Table 3 are not driven by the fact that individuals have not been in the United States as long as individuals in Panel 1 of Table 3.

Following Card and Payne (1997), I use two functional forms for this new regressor: the inverse mills ratio and a log functional form.[27] If the sample selection correction coefficient is positive and significant, then a sample selection problem of the usual type described above exists: in ethnic groups with low participation rates, only the most able women participate. Table 4 presents the estimation results when a sample selection correction term is included in the analysis. There are several key points to note. First, and most importantly, controlling for sample selection does not alter the main results–i.e., the home country gender wage gap coefficients are roughly the same in terms of magnitude and significance as without the selection correction term. Second, the sign and significance of the selection coefficient is not sensitive to functional form.[28] Finally, I find no evidence of selection bias of the expected type for either first generation or second-and-higher generation immigrants because the coefficient estimate on the selection correction term is never significant. In fact, for both first generation, and for second-and-higher generation immigrants the coefficient estimate on the selection term is negative. Interestingly, this suggests that there are unobservable differences across ethnic origin groups such that certain ethnic origin groups have a high fraction of women employed and high female relative wages (i.e., small gender wage gaps). This pattern is

[27]The inverse mills ratio is calculated as $\phi(\Phi^{-1}(\pi))/\pi$ where π is the fraction of women employed. This is a decreasing function in π.

[28]The selection correction coefficient reported for the inverse mills ratio functional form is multiplied by minus 1 in order for it to have the same interpretation as the log functional form, i.e., an increasing function in the fraction of women employed.

more consistent with unobserved cultural factors than with selection.

6. Conclusions

On average women earn less than men in virtually all developed countries, but the gender wage gap varies in size from country to country. Recent studies for why the gender wage gap varies across countries have traditionally focussed on two factors: human capital and wage setting institutions. Even after controlling for these two factors, however, there still is significant cross country variation in the gender wage gap. According to everyday conversations and casual empiricism, this variation may be explained by "cultural" factors, such as differences in "tastes" regarding family structure and women's roles in home and market work.

In this paper I attempt to examine the effect of cultural factors on the gender wage gap using evidence on inter-ethnic variation in the gender wage gap among first generation, and among second-and-higher generation immigrants to the United States, in the 1990 Census. I show that there is sizable variation in the gender wage gaps across different ethnic origin groups in the United States. Although human capital factors, especially for second-and-higher generation immigrants, are important determinants of inter-ethnic variation in the gender wage gap, controlling for these factors does not eliminate inter-ethnic variation in the gender wage gap. In fact, for first generation immigrants, I find that even after controlling for all observable characteristics in the United States, a one percentage point increase in the home country gender wage gap is associated with a 0.9 percentage point increase in the gender wage gap across ethnic origin groups in the United States. This positive correlation exists despite the huge measurement error associated with the home country gender wage gap. I argue that this strong positive correlation suggests the importance of cultural factors. Interestingly, I am unable to detect an effect of home country factors among second-and-higher generation immigrants, a finding suggestive of the presence of cultural assimilation as well.

Although it is unclear how large of a role culture plays after one generation away from the home country, there appears to be a role for "tastes" regarding work and family, in addition to the more commonly-analyzed human capital and institutional factors, in explaining why some groups of women earn more relative to men than others.

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Appendix I: Sample Size by Ethnic Origin

	1st Generation	2nd + Generation
Austria	816	547
Belgium	411	385
Canada	10034	4271
Czechoslovakia	804	1901
Denmark	442	1379
Finland	279	864
France	1785	7431
Germany	10838	76239
Greece	2628	1155
Hungary	1242	1444
Ireland	2208	40744
Italy	6773	21097
Japan	4084	1537
Mexico	72343	13761
Netherlands	1414	4515
Norway	367	4530
Philippines	19860	449
Portugal	4072	1191
Sweden	625	4419
Switzerland	519	629
United Kingdom	9859	48078
Total	151403	236566

Appendix II

The second stage GLS estimation requires an estimate of the weighting matrix, Ω. In order to estimate Ω, estimates of $\hat{\sigma}_u^2$ and $\hat{V}^{2*}$ are needed. As previously stated, $\hat{V}^{2*}$ is the variance of the coefficient estimates of the mean difference in the gender wage gap across ethnic origin groups and can be estimated consistently from the first stage log wage equation. $\hat{\sigma}_u^2$ is obtained by equating the trace of $\hat{\Omega}$ to the sum of the squared OLS residuals from the second stage regression (equation (4)).[29] Specifically,

$$e = \hat{d}^{2*} - z\hat{\gamma}_{ols} \tag{A2.1}$$

and

$$tr(\hat{\Omega}) = e^T e \tag{A2.2}$$

which gives

$$n\hat{\sigma}_u^2 + tr(\hat{V}^{2*}) = e^T e \tag{A2.3}$$

where n is the number of observations in the second stage regression. Equation (A2.3) can be rewritten as:

$$\hat{\sigma}_u^2 = \{e^T e - tr(\hat{V}^{2*})\} / n \tag{A2.4}$$

The weighting matrix, $\hat{\Omega}$ can now be formed and GLS can be performed.

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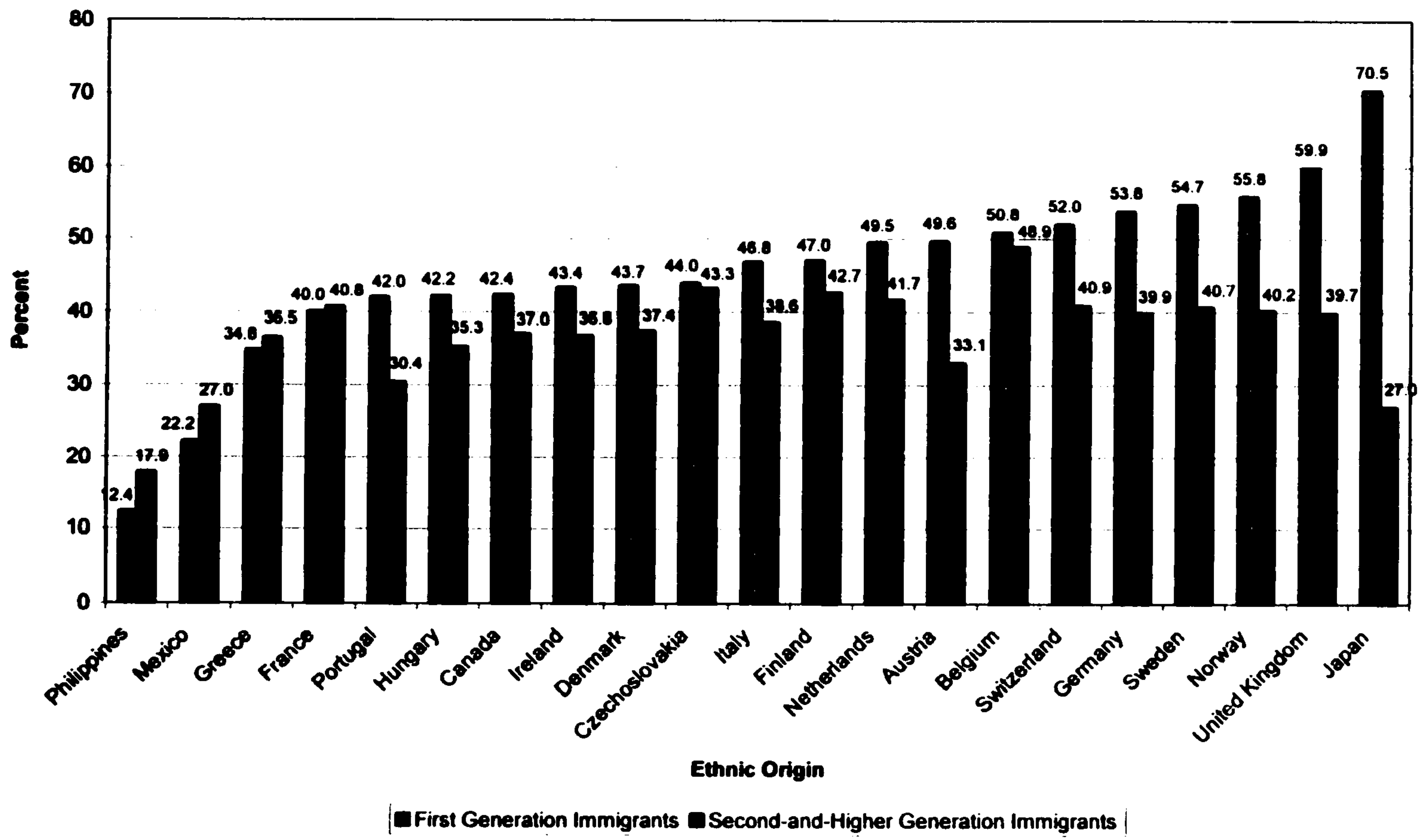
[29] See Borjas and Sueyoshi (1994) for a similar approach and an alternative estimation approach of $\hat{\sigma}_u^2$.

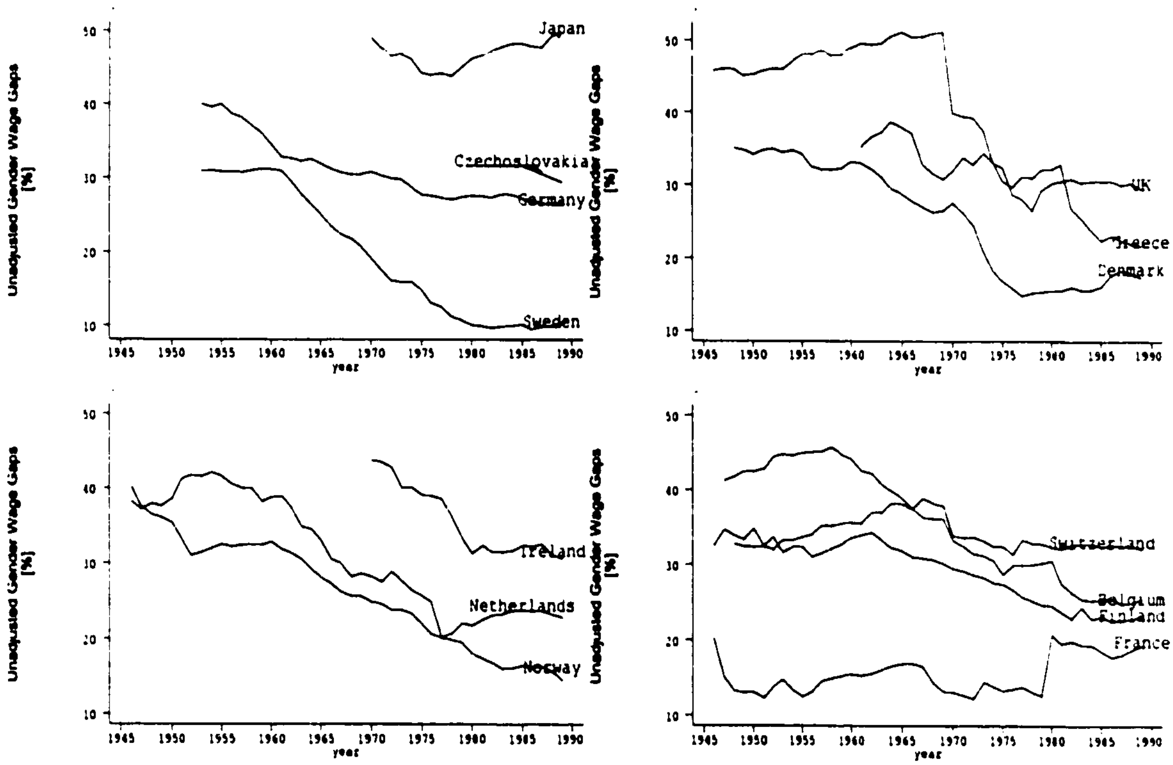
Figure 1:
Unadjusted Gender Wage Gaps



Source: 1990 U.S. Census

Figure 2

Home Country Unadjusted Gender Wage Gaps (1946-1989)



Source: ILO Yearbook of Labour Statistics, Various Years.

Notes:
All wage data is based on non-agricultural hourly earnings with the following exceptions: wage data for Finland, Greece, Ireland, Norway, and Sweden is based on the manufacturing industry; wage data for Belgium is based on daily earnings up until 1969, after which the wage data is based on hourly earnings; wage data for the U.K. is based on weekly earnings up until 1969, after which the wage data is based on hourly earnings; wage data for the Netherlands is based on the manufacturing industry up until 1972, after which the wage data is based on non-agricultural industries; wage data for Czechoslovakia and Japan is based on monthly earnings.

Table 1: Estimates of the Mean Differences in the Gender Wage Gap Across Ethnic Origin Groups

	Unadjusted Gap		X-adjusted Gap		X,Z-adjusted Gap	
	1st Gen (1)	2nd+Gen (2)	1st Gen (3)	2nd+Gen (3)	1st Gen (5)	2nd+Gen (6)
Austria	.0404	-.0389	.0096	-.0414	.0170	.0130
Belgium	.0515	.1193**	.0609	.0650	.0614	.0286
Canada	-.0322**	.0001	-.0288*	.0288	-.0100	.0124
Czechoslovakia	-.0162	.0636**	-.0624	.0336	-.0515	.0390
Denmark	-.0188	.0050	-.0055	-.0138	-.0015	-.0437
Finland	.0140	.0579	-.0098	.0400	.0451	-.0018
France	-.0558	.0383**	-.0262	.0522***	-.0182	.0281*
Germany	.0822	.0296***	.0351**	.0125	.0383***	.0074
Greece	-.1080***	-.0042	-.1310***	.0224	-.1453***	.0550
Hungary	-.0336***	-.0168	-.0913**	-.0422	-.0688*	-.0152
Ireland	-.0215	-.0011	.0002	.0023	.0141	.0061
Italy	.0116	.0169	-.0020	.0297***	-.0309*	.0280***
Japan	.2489***	-.0995***	.3070***	-.0806***	.2871***	-.0084
Mexico	-.2336***	-.1001***	-.1857***	-.0607***	-.1962***	-.0985***
Netherlands	.0393	.0476**	.0022	.0340*	.0193	.0097
Norway	.1022	.0327*	.1179*	.0164	.1018	.0061
Philippines	-.3317***	-.1901***	-.3435***	-.1098**	-.3119***	-.0956*
Portugal	-.0361	-.0656**	-.0305	-.0297	-.0571***	-.0312
Sweden	.0908*	.0371*	.1194**	.0293	.1145**	.0228
Switzerland	.0638	.0400	.0965	.0034	.0704	.0267
United Kingdom	.1427***	.0279***	.1478***	.0088	.1222***	.0106

Table 1: Estimates of the Mean Differences in the Gender Wage Gap Across Ethnic Origin Groups (Continued)

	Unadjusted Gap		X-adjusted Gap		X,Z-adjusted Gap	
	1st Gen (1)	2nd+Gen (2)	1st Gen (3)	2nd+Gen (3)	1st Gen (5)	2nd+Gen (6)
F-test	106.11***	8.74***	75.42***	3.74***	70.53***	5.58***
N	151392	236555	151392	236555	151392	236555
WSDs	11.25	5.80	12.11	3.05	11.36	2.42

Notes: 1. Data is from the 1990 U.S. Census. The sample includes individuals between the ages 25 and 54 who earned positive hourly wages and were not currently enrolled in school. Sample excludes the following groups: first generation immigrants born abroad of U.S. born parents, second-and-higher generation immigrants with multiple ancestries (except multiple U.K. ancestries), individuals earning less than $1/hour and greater than $100/hour, and individuals earning self-employment income. Sampling weights were used. 2. Other explanatory variables are: Specification 1–a male dummy variable, and 20 ethnic origin dummy variables. Specification 2 includes Specification 1 plus a quartic in age, an urban/rural dummy variable, 9 region dummy variables, and 8 year of arrival dummy variables for first generation immigrants and cross terms between gender and all additional variables. Specification 3 includes Specification 2 plus education, marital status, number of children, and English Fluency and cross terms between gender and all additional variables (with the exception of number of children).
3. *** significant at less than 1%, ** significant at less than 5%, * significant at less than 10%.
4. The F-test shows that the ethnic origin variables are jointly significant for both first generation, and for second-and-higher generation immigrants for the unadjusted, for the X-adjusted, and for the X,Z-adjusted gender wage gaps.

Table 2: Gender Wage Gaps

	Host Country						Home Country
	Unadjusted Gap		X-adjusted Gap		X,Z-adjusted Gap		Unadjusted Gap
	1st Gen	2nd Gen	1st Gen	2nd Gen	1st Gen	2nd Gen	
Austria	49.6	33.1	46.1	33.0	43.9	33.2	31.0
Belgium	50.8	48.9	51.3	43.7	48.3	34.8	29.4
Canada	42.4	37.0	42.3	40.1	41.2	33.1	31.9
Czechoslovakia	44.0	43.3	38.9	40.5	37.1	35.8	34.9
Denmark	43.7	37.4	44.6	35.8	42.1	27.5	19.0
Finland	47.0	42.7	42.6	42.4	40.4	31.8	26.4
France	40.0	40.8	44.2	41.2	46.7	34.7	21.4
Germany	53.8	39.9	48.7	38.4	46.0	32.6	30.8
Greece	34.8	36.5	32.1	39.4	27.7	37.4	24.5
Hungary	42.2	35.3	36.1	33.0	35.3	30.4	21.3
Ireland	43.4	36.8	45.2	37.4	43.6	32.5	36.6
Italy	46.8	38.6	45.0	40.2	39.1	34.7	32.5
Japan	70.5	27.0	75.9	29.1	70.9	31.1	68.6
Mexico	22.2	27.0	26.6	31.1	22.6	22.0	24.9
Netherlands	49.5	41.7	47.4	40.6	44.1	32.9	25.9
Norway	55.8	40.2	57.0	38.8	52.4	32.5	15.7
Philippines	12.4	17.9	10.8	26.2	11.0	22.3	9.6
Portugal	42.0	30.4	42.1	34.2	36.5	28.8	36.2
Sweden	54.7	40.7	57.1	40.1	53.7	34.2	11.1

Table 2: Gender Wage Gaps (Continued)

	Host Country						Home Country
	Unadjusted Gap		X-adjusted Gap		X,Z-adjusted Gap		Unadjusted Gap
	1st Gen	2nd Gen	1st Gen	2nd Gen	1st Gen	2nd Gen	
Switzerland	52.0	40.9	54.8	37.5	49.3	34.6	38.9
United Kingdom	59.9	39.7	60.0	38.1	54.4	33.0	35.0

Notes: 1. Host Country data is from the 1990 U.S. Census. The sample includes individuals between the ages 25 and 54 who earned positive hourly wages and were not currently enrolled in school. Sample excludes the following groups: first generation immigrants born abroad of U.S. parents, second-and-higher generation immigrants with multiple ancestries (except multiple U.K. ancestries), individuals earning less than $1/hour and greater than $100/hour, and individual earning self-employment income. Sampling weights were used. 2. The predicted gender wage gaps in the host country are based on log wage regressions, which are pooled for men and women. The variables included in the log wage regressions are the same as those listed in Table 1. 3. Home country wage data is from the ILO Yearbook of Labour Statistics, Various Years with the following exceptions: wage data for Austria and Italy is from Blau and Kahn (1996); wage data for Mexico, provided by the Commission for Labor Cooperation, is from STPS/INEGI, Encuesta Nacional de Empleo (ENE) for 1991; and wage data for Canada is from the 1990 Survey of Consumer Finance (SCF) for the 1989 income year. 4. All home country wage data is based on 1989 hourly earnings in the non-agricultural sector with the following exceptions: wage data for Czechoslovakia, Hungary, Japan, and the Philippines is based on monthly wages; wage data for Finland, Greece, Ireland, Norway, and Sweden is based on the manufacturing industry; wage data for Mexico is based on weekly earnings; wage data for the Philippines is based on 1993 earnings; wage data for Hungary is based on 1992 earnings; wage data for Mexico is based on 1991 earnings; wage data for Mexico and the United Kingdom include agricultural workers; wage data for Austria is based on monthly wages adjusted for hours worked; the wage data for Italy is based on annual wages adjusted for hours worked; the wage data for Austria is based on 1985-1989 pooled wages; and the wage data for Italy is based on 1987 wages. 5. In general, the home country gender wage gap is measured as ln(average male wages) minus ln(average female wages).

Table 3: Second Stage Regression Results Explaining the Estimated Coefficients of the Mean Differences in the Gender Wage Gaps Across Ethnic Origin Groups from the First Stage Regression

	X-adjusted		X,Z-adjusted	
	OLS	GLS	OLS	GLS
Panel 1: Full Sample First Generation Immigrants				
Home Country Gender Wage Gap in 1989	.6182*** (.1994)	.6943*** (.1843)	.5566*** (.1910)	.6276*** (.1769)
R-Squared	.3360	.3494	.3088	.3201
Panel 2: First Generation Immigrants who Immigrated between 1975-1990				
Home Country Gender Wage Gap in 1989	.7925*** (.2139)	.9343*** (.1686)	.7624*** (.2090)	.8910*** (.1677)
R-Squared	.4195	.4346	.4118	.4317
Panel 3: Full Sample Second-and-Higher Generation Immigrants				
Home Country Gender Wage Gap in 1989	-.0518 (.0840)	-.0948 (.0750)	.0684 (.0716)	.0335 (.0623)
R-Squared	.0196	.0245	.0457	.0423

Notes: 1. Host Country data is from the 1990 U.S. Census. The first stage sample includes individuals between the ages 25 and 54 who earned positive hourly wages and were not currently enrolled in school. Sample excludes the following groups: first generation immigrants born abroad of U.S. parents, second-and-higher generation immigrants with multiple ancestries (except multiple U.K. ancestries), individuals earning less than $1/hour and greater than $100/hour, and individual earning self-employment income. Sampling weighted were used. 2. The variables included in the log wage equation are the same as those listed in Table 1. 3. Home country wage data is from the ILO Yearbook of Labour Statistics, Various Years with the following exceptions: wage data for Austria and Italy is from Blau and Kahn (1996); wage data for Mexico, provided by the Commission for Labor Cooperation, is from STPS/INEGI, Encuesta Nacional de Empleo (ENE) for 1991; and wage data for Canada is from the 1990 Survey of Consumer Finance (SCF) for the 1989 income year. 4. All home country wage data is based on 1989 hourly earnings in the non-agricultural sector with the following exceptions: wage data for Czechoslovakia, Hungary, Japan, and the Philippines is based on monthly wages; wage data for Finland, Greece, Ireland, Norway, and Sweden is based on the manufacturing industry; wage data for Mexico is based on weekly earnings; wage data for the Philippines is based on 1993 earnings; wage data for Hungary is based on 1992 earnings; wage data for Mexico is based on 1991 earnings; wage data for Mexico and the United Kingdom include agricultural workers; wage data for Austria is based on monthly wages adjusted for hours worked; the wage data for Italy is based on annual wages adjusted for hours worked; the wage data for Austria is based on 1985-1989 pooled wages; and the wage data for Italy is based on 1987 wages. 5. The independent variable is the mean differences in the gender wage gaps across home countries. 6. Sample size in the second stage regression is 21. 7. Standard errors in parentheses. 8. *** significant at less than 1%, ** significant at less than 5%, * significant at less than 10%.

Table 4: Second Stage Regression Explaining the Estimated Coefficients of the Mean Differences in the Gender Wage Gap Across Ethnic Origin Groups from the First Stage Regression Controlling for Sample Selection Bias

	First Generation Immigrants	
	Inverse Mills Ratio	Log Functional Form
Home Country Gender Wage Gap in 1989	.5580*** (.1932)	.5682*** (.1991)
Selection Correction	-.3087 (.1923)	-.2607 (.1906)
R-Squared	.4502	.4295
	Second-and-Higher Generation Immigrants	
Home Country Gender Wage Gap in 1989	-.0879 (.0661)	-.0899 (.0674)
Selection Correction	-.0736 (.1468)	-.0656 (.1681)
R-Squared	.1963	.1833

Notes: 1. See notes in Table 3 for sample criteria. 2. Results are based on the X-adjusted gap estimated by GLS. 3. The coefficient on the selection term for the inverse mills ratio functional form is multiplied by minus 1 in order for it to have the same interpretation as the log functional form, i.e., an increasing function in the number of women employed. 4. *** significant at less than 1%, ** significant at less than 5%, * significant at less than 10%.

III

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

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.[1]

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 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)).[2] 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.

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[1]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.

[2]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)).

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.

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[3] and Affirmative Action (AA) legislation under the Federal contractors program in 1965.[4] 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 Employment". The province of Ontario did not pass employment equity legislation until December of 1993.[5]

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

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[3]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.

[4]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.

[5]The only employment equity legislation that existed in the province of Ontario prior to 1993 was the Ontario Police Service Act of 1990.

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 a 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 legislation covers all Federally regulated industries (atomic energy, banking, communication, milling, resource extraction, and transportation) with at least 100 employees and Federal Crown Corporations.[6] This only covers 5% of the national labour force. In order to ensure discriminatory employment practices are being rectified,

[6]Although the Federal legislation does not cover the Federal public service, the Federal government as an employer, has committed itself to employment equity (Weiner, 1993).

employment equity plans, which use positive measures, must be created and implemented. Finally, compliance is monitored by the Canadian Human Rights Commission.[7]

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.[8] 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, however the coverage of the Ontario legislation is much broader. It covers

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[7]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.

[8]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.

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.[9]

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

[9]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.

equity plan. Compliance is monitored by the Quebec Human Rights Commission.[10]

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.[11]

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, Edmonton, Calgary, Halifax, and all the municipalities in

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[10] 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.

[11] 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.

Ontario.[12]

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

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[12]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.

3898 individuals.[13] Eliminating individuals who were 65 years of age or over left us with a sample of 1586 women 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 preseparation 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 preseparation job with at least 100 employees. A dummy variable for coverage by employment equity in

[13]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.

the postseperation job was created analogously.[14]

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 preseparation job. Further, women are more likely to have higher levels of education (college and university). Women do not differ markedly from 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 preseparation and the postseperation job, although the gap is substantially larger in the postseparation 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--

[14]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.

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.[15]

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.

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 preseparation jobs are not

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[15]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.

covered by employment equity be α. (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 α and the hazard of women whose preseparation 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 (preseparation) 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 γ; 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 θ is the covered-sector effect that is common to women and men, and δ^M is the causal effect of employment equity coverage on men's re-employment rates. If employment equity closes some job opportunities to men, δ^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 preseparation 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, δ^F and δ^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.[16] 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 preseparation 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 preseparation job are more likely to work in the covered sector in the postseparation job, confirming our expectation that sector-specific skills and geographical

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[16]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.

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 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 preseparation 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 preseparation 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 preseparation 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.[17]

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 preseparation 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.

[17]A discussant has suggested that our results might be an artifact of variation in union coverage in the preseparation 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.

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.

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 "EE*male" 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 preseparation 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 preseparation 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 preseparation 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.[18] The dependent variable in the 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.[19]

[18]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 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.

[19]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.

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 preseparation 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 EE*male 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.[20] 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 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 preseparation job is now never significant and

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[20]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.

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 preseparation 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 preseparation 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[21], 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 rates, 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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[21]See Pulley (1997). The Piscattaway 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 propects 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	36.65	0.229	37.411	0.271
Preseparation Tenure (wks)	230.528	7.786	219.786	7.193
Education				
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
Family Background				
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
Coverage by EE				
Preseparation Job	0.235	0.009	0.278	0.011
Postseparation Job	0.240	0.011	0.314	0.014
Reason for Separation				
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

Table 1: Sample Mean Characteristics (Continued)

	Men		Women	
	Mean	Std. Error	Mean	Std. Error
Job Search Outcomes [1]				
Spell (wks) [2]	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
Perceptions of Discrimination				
Hurtful	0.057	0.005	0.103	0.008
Helpful	0.050	0.005	0.039	0.005
Sample Size [3]	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

	Employment Equity (EE) in the Preseparation Job	
	Covered	Noncovered
Men		
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
Women		
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)
EE*Male	-0.231 (1.858)	-0.242 (1.941)	-0.233 (1.828)	-.0178 (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)

Table 3: Cox Proportional Hazard Coefficients for Unemployment Spells, Various Specifications (Continued)

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
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 value of z-statistics in parentheses. 2. Sample includes individuals between the ages 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 preseparation 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 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)
EE*Male	-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)

Table 4: Competing Cox Proportional Hazard Coefficients for Unemployment Spells (Continued)

	Individual's first job after the separation was with the same employer	**Individual's job after the separation was with a different employer**
	[1]	[2]
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 value of z-statistics in parentheses. 2. Sample includes individuals between the ages 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 preseparation job. 7. The number of observations is 1993.

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

	Employment Equity (EE) in the Preseparation Job	
	Covered	Noncovered
Men		
Hurt	0.085	0.048
Help	0.046	0.052
Women		
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) [3]	-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)
EE*Male	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/ Divorce/ 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)

Table 6: Probit Coefficients for Reported Hurtful Discrimination, Various Specifications (Continued)

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
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 preseparation 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)
EE*Male	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/ Divorce/ 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)

Table 7: Probit Coefficients for Reported Helpful Discrimination, Various Specifications (Continued)

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
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 preseparation job.

IV

Gender as an Impediment to Labor Market Success:

Why do Young Women Report Greater Harm?

In a number of recent papers (Kuhn (1987, 1990); Barbezat and Hughes (1990); Heywood (1992); Hampton and Heywood (1993, 1996); Laband and Lentz (1993, 1998); Johnson and Neumark (1997)) labor economists have begun to analyze aspects of labor market discrimination that, unlike more standard "residual wage gap" measures (Oaxaca 1973), are directly derived from survey reports of discriminatory treatment. Survey reports of discrimination can be of interest to economists for a number of reasons, including as a check on the validity of these traditional "wage gap" measures, or as objects of interest in their own right. For a number of issues, including public support for antidiscrimination policies, and the prevalence of the kind of discriminatory treatment that may be remediable in courts of law[1], survey reports of discrimination might be more directly relevant than wage gap measures.

A common, but often overlooked finding in the literature on survey reports of discrimination is a greater tendency for younger women to feel harmed by discrimination

[1] While econometric evidence of wage gaps is becoming increasingly accepted in many Courts, evidence of specific incidents of discriminatory treatment –which may or may not be reflected in wage measures-- is still seen as stronger support for a claim of discrimination in almost all cases. See Kuhn (1987) for a discussion.

than older women (see for example Kuhn (1987) and Laband and Lentz (1998)). Does this mean that young women face particular gender-related problems that old women do not? Perhaps more importantly, might it indicate an increasing level of discrimination, of a kind not usually measured, against new cohorts of women? If so, this age pattern might be a cause for some concern. The goal of this paper is to shed some light on this phenomenon, using a new survey of Canadian job seekers with extensive information on both labor market outcomes and perceptions of gender-related harm.

We begin our analysis by establishing that women's reports of experiencing gender-induced harm in the labor market are indeed more frequent among the young: In our data, female job seekers aged 15-24 are *more than three times as likely* to say they have been hurt by their gender in the labor market than those aged 55-64. We then assess various possible explanations of this phenomenon in turn. We show, first, that higher reports of harm among young women cannot be explained by any measured personal or job characteristics in our sample. Second, these reports cannot be explained by any standard measures of "objective" discrimination that have been used by economists: in our sample measured discrimination on all dimensions considered is *lower* among young women.

Third, using the extra information in the current survey on women's perceptions of gender-induced *advantage,* and on men's perceptions of gender-induced harm, we also rule out a higher mean level of *unmeasured* discrimination against young women as a possible explanation. The reason for this is, perhaps surprisingly, that women's reports of gender-induced advantage, and men's reports of perceived harm, are also higher among younger than among older job seekers. Clearly, these patterns cannot be explained by a higher overall level of discrimination against young women, *even of a form that is not captured by standard discrimination measures.*

Finally, we estimate a formal model of the decision to report gender-induced advantage or harm that allows us to test two further explanations of the above patterns. One of these --greater dispersion in the labor market experiences of young workers-- receives mixed support. The other --a particular difference in reporting behavior-- receives more consistent support, and is our preferred explanation for the patterns observed. According to this interpretation, young workers *of both sexes* are simply more willing to label departures from gender-neutral treatment *in either direction* as being caused by their gender. In this sense, young workers are less "tolerant" of both traditional *and* reverse discrimination, a phenomenon which might pose a challenge for the design of future antidiscrimination policy.

Section II describes the data used in this study. Section III presents our main results concerning women's reports of gender-induced harm. Section IV broadens the analysis to include women's reports of gender-induced advantage, and men's reports of harm, and estimates our formal model of reporting. Section V concludes.

II. Data.

The data used in our analysis is a nationally representative sample of Canadians who have recently experienced a job separation: the 1995 Canadian Out of Employment Panel (COEP). 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 contain a rich set of measures about a worker's preseparation job, his or her first postseparation job, the (postseparation) job at the time of the interview, as well as on unemployment spells; our analysis focuses on the actual and perceived outcomes of these workers' search for a new job. Unlike some other studies of discrimination, the current study thus focuses particularly on actual and perceived discrimination in *access to jobs*, rather than, for example, discrimination within jobs. This is useful, in our view, because it is widely accepted that the former issue of access to jobs, rather than the latter, plays the central role in explaining male-female wage gaps today (see for example Johnson and Solon 1986).[2]

Because of a problem with how reports of gender-induced harm were measured, in this paper we only use the information from separations in the first window of the 1995 COEP survey, which consists of 3898 individuals.[3] Eliminating individuals who were 65

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[2]The focus on job seekers also serves to eliminate a potential explanation of the age difference in reports suggested in Kuhn (1987): the notion that young women might have had less of a chance to locate a nondiscriminatory employer than older women. In the current data, women of all ages are engaged in the search for a new job, yet we continue to see the patterns found in nationally-representative surveys of reported discrimination.

[3]In Cohort 2 the question on perceived harm 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.

years of age or over left us with a sample of 1586 women and 2280 men. 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 preseparation jobs. Further, women are more likely to have education at the higher levels (college and university) compared to the men in our sample. Not surprisingly, women are much more likely to be in highly feminized occupations, though they do not differ markedly from men in their marital status distribution and presence of children. Turning to labor market "outcome" variables, Table 1 shows that the mean hourly preseparation wages of men and women are $15.72 and $10.09, respectively. Thus, women earn 64% of what men do before the separation. A slightly smaller earnings ratio (61%) is found for postseparation wages. In our sample of unemployment durations, women had been unemployed about 3 weeks longer than men as of the survey date, which was usually about 30 weeks after the separation.[4]

Finally, the measure of gender-induced harm in the current paper is based on the following question: "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

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[4]In some of what follows we analyse the unemployment durations of the individuals in our sample. When we do so our sample is further restricted to individuals who lost their job due to a layoff, and who reported that they engaged in at least some search for a new job after the layoff. See Section 3, footnote 11.

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. For brevity in what follows, we occasionally refer to this as the "reported discrimination" question; to individuals choosing the "yes, hurt" response as experiencing "hurtful discrimination"; and to those indicating "yes, helped" as experiencing "helpful discrimination", i.e. being a beneficiary of labor market discrimination.[5]

According to Table 1, 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 reported advantage or harm, 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. Thus, while hurtful discrimination against women is the most common way in which gender is perceived to affect search outcomes, other forms of perceived gender-induced harm or advantage also play substantial roles in our sample.

III. Women's Reports of Gender-Induced Harm.

Table 2 shows the fraction of women reporting gender-induced harm in five age

[5]It may be objected that this question does not include the word discrimination. Although this is true, the age patterns of reported "discrimination" in our study are similar to a number of other studies (e.g. Laband and Lentz 1998), increasing our confidence that they capture the same phenomenon.

categories. As in Kuhn (1987), it is clear that women's reports of hurtful discrimination are highest among the young: in the present case they are more than three times as frequent among women under 25, at 13.2 percent of the sample, than among women aged 55-64, at 4.3 percent. This difference is the main stylized "fact" we analyze in this paper, and echoes earlier findings in Kuhn (1987) and others.

Can young women's more frequent reports of gender-induced harm be explained by differences in observed characteristics between them and older women? To answer this question we first estimate a probit model of hurtful discrimination on age only. We then add observed covariates, to see if, under any specification, the direct effect of age is substantially reduced in magnitude. Before discussing our results, it is worth commenting on the role of one particular variable included in these regressions: the gender composition of one's occupation.[6] We included this variable to capture what we initially considered a very appealing explanation for the pattern observed in our data: Perhaps young women are more likely to be "pioneers": i.e. to be among the first women to enter high-paying, formerly all-male occupations like law, science, medicine and management. High levels of reported discrimination could occur because these workplaces had not yet adapted themselves to women's presence and because of lingering

[6]Percent female was calculated for the 19 occupations coded in the COEP survey from a nationally representative sample of workers in the 1994 Survey of Consumer Finances (SCF). Like all the right-hand-side variables used here, this refers to the preseparation job to avoid endogeneity. The correct interpretation, is, we believe, that women with previous jobs in highly-feminized occupations are likely to have skills that are specific to such occupations. Their search activity is therefore more likely to focus on those kinds of jobs.

sexist attitudes; this could be consistent, as noted below, with low levels of measured discrimination because these are high-paying occupations for women.[7]

Table 3 presents estimates of various specifications of a probit for reported gender-induced harm. The following results are of note. Although the age coefficient does tend to decline in significance as controls are added, it remains negative and of roughly the same magnitude no matter what the specification. The only other variables that affect reports of hurtful discrimination are the percent female in one's occupation, and perhaps marital status. Percent female does have the effect anticipated on reports: women who are a minority in their occupations (low values of percent female) are much more likely to report encountering hurtful discrimination in their search for a new job, perhaps simply because they are more likely to be interacting with, and competing with men in the job search process. Importantly, however, this does not help explain the age pattern in reports in our data: adding the percent female variable has essentially no effect on the estimated age effect on reported harm. The reason is that, in our data, age and percent female are hardly related: while "pioneers" do run into more barriers, apparently women of all age groups have their share of pioneers.

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[7]A formal version of the "pioneers" hypothesis can be found in Kuhn (1993). In that model, binding entry restrictions against women seeking access to jobs "designed" for men become more likely as women's labor force attachment becomes more *similar* to men's, and as the gender-wage gap consequently narrows. (The intuition is that women will not want those jobs until they become sufficiently committed to the work force). Thus, to the extent that wanting a traditionally "male" job but being denied entry to it is a kind of discrimination, this kind of discrimination will (a) not be captured by standard wage-gap measures, and (b) be most common among those groups of women (e.g. younger cohorts) who are most committed to the labor market and face the smallest measured wage gaps.

We conclude from Table 3 that, while some of the observable characteristics of women in our sample have interesting and potentially important effects on women's reports of gender-induced harm, none of these other characteristics is able to account for the observed effect of age on reported harm. Therefore, we examine an alternative explanation for young women's more frequent reports of hurtful discrimination: higher levels of measured labor market discrimination against them. To examine this hypothesis, we first compute individual-specific measures of discrimination for three labor market outcomes --postseparation wages, preseparation wages, and unemployment durations, as follows.[8] We compute three alternative measures because, especially in this sample of job searchers, there may be a number of plausible ways in which women are affected by discrimination, and we want to be sure that no plausible channel by which measured discrimination patterns might explain the age pattern in reports is overlooked.[9]

For the case of wage discrimination, we compute our measure of "objective"

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[8] Most plausibly, we would probably expect reported discrimination during the job search interval to be related to the quality of the job that was ultimately found (i.e. the postseparation wage) and the length of time required to find it (unemployment duration). However it is also possible that the frequency of discriminatory treatment encountered by a population subgroup (e.g. young women) can have equilibrium effects on the wages or unemployment durations of all members of the group (for example via a lower reservation wage the search for the preseparation as well as the postseparation job). If it does, then the preseparation wage --which has the advantage of being observed for substantially more individuals in our data-- should also give an indication of the distribution of discrimination across broad population subgroups.

[9] In our analysis we also looked at two other outcomes: wage *differences* between the pre- and post-separation jobs, and reported reservation wages. Consistent with our prior that the amount of discrimination is a relatively permanent characteristic of the market for workers of a given age, wage differences were unrelated to age, while reservation wages followed the same pattern as pre- and post-separation wage levels. Thus neither of these are able to explain the age pattern in reports either.

discrimination by first estimating the following log wage regressions:

$$w_i^F = \sum_{j=0}^{J} X_{ij}^F \beta_j^F + \mu_i^F \qquad (1)$$

$$w_i^M = \sum_{j=0}^{J} X_{ij}^M \beta_j^M + \mu_i^M \qquad (2)$$

where F stands for female, M stands for male, w_i^F and w_i^M are individual log wages, and X_{ij} is a vector of ones for j=0. We then use the estimated coefficients from equations (1) and (2) to compute two alternative individual-specific measures of discrimination against women, as follows[10]:

$$\hat{D}_i^1 = \sum_{j=0}^{J} X_{ij}^F \hat{\beta}_j^M - w_i^F \qquad (3)$$

$$\hat{D}_i^2 = \sum_{j=0}^{J} X_{ij}^F \hat{\beta}_j^M - \sum_{j=0}^{J} X_{ij}^F \hat{\beta}_j^F \qquad (4)$$

[10] More commonly, discrimination measures have been computed as a predicted log wage difference for an "average" woman in the sample (Oaxaca, 1973). The measures used here simply apply Oaxaca's decomposition to each individual woman in the sample (see Kuhn 1987).

Conceptually, both the above definitions of discrimination can be thought of the (log) difference between what a specific woman actually earns and what she would earn "if she were a man". The difference between the two concerns the definition of the counterfactual: $\hat{D}_i^2$ implicitly compares a woman to a man with both her observed characteristics *and* unobserved ability (thus the earnings function residual drops out of the expression); $\hat{D}_i^1$ compares a woman's actual earnings to a man with her observed characteristics of "average" unobserved ability. Because, even controlling for measurable characteristics, men tend to earn more than women, we expect the majority of individual $\hat{D}_i$'s , according to either measure, in our sample to be positive.

Our measure of discrimination in unemployment durations is computed as similarly to the wage measures as possible, given the nature of our data. We first estimate male and female log duration regressions in a manner strictly parallel to (1) and (2), using a censored-normal model to account for incomplete spells. Unlike the wage regressions, however, this regression was restricted to individuals who lost their job due to a layoff and who engaged in at least some job search after the separation.[11] Because longer durations are worse labor market outcomes than shorter ones, our estimates of discrimination are then the negatives of (3) and (4). The measure, $\hat{D}_i^1$, that relies on actual individual durations is treated as missing for individuals whose durations are

[11] 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 survey date]?" (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.

censored.

In Table 4, we present unadjusted means of the three different labor market outcomes, and of individual-specific discrimination measures based on each outcome, for women in different age categories. In all the regressions underlying Table 4, the following variables are included in the X_{ij}'s: age, education, region, marital status, number of children less than six years of age, and tenure on the preseparation job.[12] Two immediately apparent features of Table 4 are the similarity of the patterns in unadjusted versus regression-adjusted wage gaps, and the similarity of patterns for pre- and post-separation wages. In one sense, neither of these should be surprising: measured discrimination is a relatively permanent characteristic of the labor market for workers of a given age and education level, and (because men's and women's characteristics differ little) is largely driven by gender differences in the unadjusted wage gap. Further, unlike survey reports of discrimination, all three estimates of "objective" discrimination against women are *lower* among young workers, though the patterns are stronger and more consistent for wages than unemployment spells. (There is also an exception for the youngest age category when wage discrimination is calculated using $\hat{D}_i^1$). Although we explore these issues further in the multivariate analysis below, we draw two main conclusions from these patterns here. First, given these raw correlations, patterns of measured discrimination are not likely to be able to explain patterns of reported gender-

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[12]Percent female is not included in the calculation of measured discrimination because we adopt the view that women's concentration in highly-feminized occupations is an outcome of discrimination. That said, the results do not differ markedly when it is included.

induced harm across age groups. Second, the much weaker age pattern in unemployment than wage gaps suggests the following interpretation of our evidence: compared to men of their own age, young women face a more favorable wage distribution than older women, and take advantage of this by raising their reservation wages in searching for both their pre- and post-separation jobs. This is further justification for our treatment, in this paper, of both post- *and* pre-separation wage levels as outcomes of the general level of demand, supply and discrimination conditions in labor markets for workers in a given age group.

To get a more formal idea of the ability of measured discrimination to explain the age pattern of reported discrimination, we estimate probit models analogous to those presented in Table 3 adding controls for "observed" discrimination. Panels A and B of Table 5 present the estimates of various specifications of the probits of hurtful discrimination when discrimination is calculated for postseparation wages using the measures $\hat{D}_i^1$ and $\hat{D}_i^2$, respectively. The following results are of note. As in Table 3, the age coefficient again fluctuates in significance but remains negative, and of roughly the same magnitude no matter what the specification. Additionally, percent female in one's occupation continues to be a negative and significant determinant of reports. Finally, it is worth noting the absence of a robust relationship between measured discrimination and women's survey reports of discrimination. The $\hat{D}$ coefficients in Panels A and B in Table 5 are often negative, and in all cases but one are highly insignificant, further supporting the notion that measured discrimination is of little help in explaining patterns of reported discrimination across age groups, or across individuals.

Table 6 assesses the robustness of the results in Panel A and B in Table 5 by presenting age coefficients from probits analogous to those in Table 5, but when "objective" discrimination is measured, alternatively, using preseparation wages and unemployment durations.[13] As can be seen, the age coefficients found in Table 6 display the same general patterns as those found in Panels A and B in Table 5, although the patterns are weaker for unemployment spells. Further, percent female in occupation once again remains a negative and significant influence on reports. (There is an exception when unemployment duration discrimination is calculated using $\hat{D}_i^2$.) Thus, the results found in Panels A and B in Table 5 seem to be robust.[14]

One final, potential concern with our result in this section is "omitted experience bias". In particular, because our measures of labor market experience are limited to age, education and tenure on the preseparation job, unobserved differences in labor market experience between men and women are likely to exist. More importantly, they are likely to be greater among older than younger individuals, suggesting that our estimates of measured discrimination may be biased upward for older women.

While "omitted experience bias" may be an important issue in the precise

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[13] Similar results are also found when discrimination in reported reservation wages, or wage changes are used in these regressions

[14]The one exception noted above likely results from collinearity between personal characteristics and measured discrimination, which becomes substantial when the list of covariates comes close to exhausting the list of X variables used to calculate the $\hat{D}$ measures.

quantification of gender-wage gap measures, it can explain the higher levels of reported gender-induced harm among young women in our sample only if it is severe enough to *reverse* the observed positive correlation between measured discrimination and age. We feel this is highly unlikely for the following reasons: First, because labor market experience *cannot* differ greatly among young workers, measured discrimination among the youngest cohort in our sample is unlikely to be affected by omitted experience bias. This is the smallest amount of discrimination for any age group in our sample (see Table 4), and "true" discrimination faced by older women would have to be lower than this measure if omitted experience bias explains our results. Second, when we include a measure of "partial" labor market experience as an additional underlying determinant of measured discrimination, the magnitude and significance levels of the age coefficients presented in Tables 5 and 6 are virtually unchanged.[15]

Third, unlike our data, the 1985 Panel Study of Income Dynamics (PSID) does collect information on total labor market experience. When we analysed age patterns of measured wage discrimination in this data set, we found that controlling for actual labor market experience does not reverse the positive correlation between measured discrimination and age. In particular, the PSID asked the following questions of household "heads" and their "wives": "How many years altogether have you worked for

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[15] The COEP does contain information on whether a respondent had income from wages in any of the five years preceding the survey. Using this information, we constructed a "partial" measure of actual labor market experience equal to the number of years in the past five in which they had income from wages. All "partial" labor market experience results are available from the authors upon request.

money since you were 18?" and "How many of these years did you work full-time for most or all of the year?" Using this information we are able to construct a measure of full-time experience, part-experience, and time out of the labor force.[16] We then re-estimate the log wage regressions given by equations (1) and (2), where the following variables are included in the X_{ij}'s: years of education, full time experience, full time experience squared, part time experience, part time experience squared, years out of the labor force, marital status, number of children, racial dummy variables, and state dummy variables.[17] As before, we use the estimated coefficients from equations (1) and (2) to compute two alternative individual-specific measures of discrimination against women given by equation (3) and (4).

In Appendix I, we present unadjusted means of log wages, and of individual-specific discrimination measures based on log wages, for women in different age categories using the 1985 PSID. As in Table 4, there is a similarity of the patterns in the unadjusted versus the regression-adjusted wage gaps. Further, measured discrimination is *lower* among younger workers than among older workers despite the inclusion of actual

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[16]Full-time experience is equal to the number of years the respondent reported working full-time since the age 18. Part-time experience is equal to the total number of years the respondent reported working since the age 18 minus the number of years of full-time experience. Finally, time out of the labor force is equal to the number of years the individual was out of the labor force since they graduated from school.

[17]Interestingly, we find that time out of the labor force has a negative and significant effect on the wages of women, but not of men. Further, men receive a higher return to each additional year of both full-time and part-time experience than women. In fact, women do not receive a wage premium for part-time work experience. All the remaining coefficients have the expected sign and are available from the authors upon request.

labor market experience as an additional determinant of measured discrimination.

The final and most important reason why omitted experience bias cannot explain our results is the evidence we present in the next section, that like gender-induced harm, survey reports of gender-induced *advantage* are also more common among young women. This is very hard to explain with a story based only on higher unmeasured discrimination faced by young women; clearly a more complex explanation than omitted experience alone is required.

We conclude the following from the analysis in this section. First, observable characteristics of the women in our sample are unable to account for the observed effect of age on reported discrimination. Second, no standard measure of "objective" discrimination can explain the more frequent reports of hurtful discrimination made by younger women in our sample either.

IV. Broadening the analysis: Gender-induced advantage, and men's perceptions.

In this section we broaden our analysis by examining women's reports of gender-induced advantage in the labor market, as well as men's reports of gender-induced advantage and harm. The main goal is to ask whether this additional information can shed any more light on young women's much greater propensity to report gender-induced harm. We begin our analysis in Table 7, which presents means of these additional indicators by age. These means reveal what were to us some very surprising patterns: First, like women, young men are *also* more likely to feel that they have been hurt by gender discrimination than older men, though the relationship is certainly not as strong as it is for women.[18] Further, young women, while more likely to report gender-induced harm, are also more likely than other women to report that they *benefitted* from being female. Thus, especially for women, reports of both harm and advantage seem to move in tandem across age categories. This is illustrated in columns 2 and 5 of Table 7, which simply add together all those individuals who reported either harm or advantage. In all cases, these fractions fall with age, generally more strongly than reports of harm or advantage alone.

In order to interpret these patterns, in this section we develop a framework of the decision to report both gender-induced harm and advantage in a confidential survey. An interesting challenge for this framework arises from the fact that, while our dependent variable is clearly ordered (one's gender either hurt, had no impact, or helped), a worker's age appears (at least in our raw data) to have the effect of increasing the frequency of *both* "extreme" outcomes ("hurt" and "helped"). As most existing techniques for dealing with ordered data cannot fit such patterns well we develop some simple models which are better suited to this purpose.

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[18]Interestingly, men's reporting patterns across *education* groups are however much stronger than women's, but of a similar nature to women's reporting patterns across age groups: Highly-educated men are much more likely to say they were hurt by discrimination, and more likely to say they were helped by discrimination, than less-educated men. We view this as a useful topic for further research.

(a) Conceptual Framework.

Assume that the net amount of favorable treatment, relative to the opposite sex, faced by individual i in his or her job search is given by a scalar, Δ_i.

$$\Delta_i = \sum_{j=0}^{J} \theta_j Z_{ij} + \phi_i; \tag{5}$$

where ϕ_i is a normal error term with mean zero and variance σ_ϕ^2. What we have in mind as entering into Δ_i includes longer or shorter unemployment spells relative to the opposite sex, differential treatment in job interviews, outright sexual harassment, and any other labor market outcome differentials encountered by the individual (whether observed or unobserved by the econometrician) relative to a similar person of the opposite sex. To allow for reports of both gender-induced harm and advantage, suppose that a woman reports that her job search was unaffected by her gender iff:

$$-K < \Delta_i < K \tag{6}$$

where K is the threshold amount of differential treatment that induces reports of either harm (in the lower tail of the distribution of Δ_i) or advantage (in the upper tail).[19]

[19]For now, we assume a constant reporting threshold across individuals; in what follows we shall explicitly model the dependence of this threshold on age. Note also that, because Δ_i has an intercept (see (8)), (6) does not constrain reports of harm and advantage to be equally frequent; instead the symmetry of (6) is meant to capture the notion of individuals implicitly conducting two-tailed hypothesis tests about whether discrimination exists, which by construction are symmetric.

Combining (5) and (6), an individual reports neither harm or advantage iff:

$$\frac{-K - \sum_{j=0}^{J} \theta Z_i}{\sigma_\phi} < \mu_i < \frac{K - \sum_{j=0}^{J} \theta Z_i}{\sigma_\phi} \tag{7}$$

or

$$\frac{K_1 - \sum_{j=1}^{J} \theta_j Z_{ij}}{\sigma_\phi} < \mu_i < \frac{K_2 - \sum_{j=1}^{J} \theta_j Z_{ij}}{\sigma_\phi} \tag{8}$$

where μ_i is a standard normal variate. In (8), $K_1 = \theta_0 - K$, and $K_2 = \theta_0 + K$. Reports of hurtful and helpful discrimination are given by the obvious complementary expressions.

The model in (8), as written, is a standard ordered probit equation with three ordered responses. As is well known, σ_ϕ, which in our model represents the standard deviation of individuals' experiences that is not associated with observed characteristics, is not identified; the standard practice is to normalize it to one and interpret the coefficient estimates as relative to this term, i.e. as estimates of θ/σ_ϕ. Note that this model also generates estimates of standardized "cutoffs", K_1/σ_ϕ and K_2/σ_ϕ.

(b) Two Hypotheses; Three Models.

We now use (8) as a framework for testing two broad hypotheses that might explain the greater propensity of both young men and women to report helpful *and* hurtful discrimination. The first of these is greater *heterogeneity* in the gender-related treatment experienced by the young: large amounts of both hurtful *and* helpful discrimination may be more common among the young, for example if the young find themselves in a much greater variety of jobs and work situations than their parents.

There are two main ways that this idea of "greater heterogeneity" might enter the model in (8). One is that young women could be endowed with a distribution of observed characteristics (Z's) that generates more reports in both tails; to assess whether this kind of heterogeneity can explain the pattern of reports in our data, we simply estimate (8) as it stands, then ask whether (given the actual distribution of observed characteristics in our data) it can successfully mimic the pattern of reports across age groups in the data. In what follows, we shall refer to this ordinary ordered probit as "Model 1".

Maximum likelihood coefficient estimates for Model 1 are shown in column 1of Table 8 for women, and column 1 of Table 9 for men.[20] They show the following. First, as in the previous section, only a few observed characteristics (Z's) have significant direct effects (θ's) on the total amount of net favorable treatment faced by individual women or men. These include the percent female in one's occupation, marital status and education; they do *not* include age. As expected, high values of percent female in one's occupation significantly increase (decrease) the probability of reporting helpful (hurtful) discrimination for women. Although not significant, the reverse is true for men: thus

[20] A key method of assessing our models' performance in this section involves comparing the predicted pattern of reports across age groups with the unadjusted means in Tables 2 and 7. For that reason we represent age by a set of four dummy variables, instead of a continuous variable, throughout this section.

both women and men are more likely to feel harmed when they find themselves in a "minority" in their occupation. Further, marital status also has opposite effects on the underlying "net favorable treatment" index, Δ, for men and women, with the effect being significant for women. Compared to less than high school –the omitted category--, men's probability of reporting hurtful (helpful) discrimination is significantly higher (lower), at the two highest levels of education (college and university). The same is not true for women.

Can Model 1 successfully explain the pattern of reported gender-induced harm and advantage in our data? To address this issue, the predicted pattern of reported discrimination from the model across age groups is shown in part (a) of Table 10. These predictions give the mean predicted fraction of individuals in each age category reporting discrimination, with each age category evaluated at its *own* mean characteristics, Z. Thus they are directly comparable with the sample means for these variables in Tables 2 and 7. Clearly, Model 1 cannot successfully mimic the pattern of lower reports of both harm and advantage among younger women in our data.

A second way in which greater heterogeneity for the young could enter our model is through *unobserved* factors rather than observed ones, i.e. through a higher level of σ_ϕ. To explore this possibility, let "Model 2" be a variation of (8) in which:

$$\sigma_\phi = \sigma_0 + \sum_{2}^{5} b_k A_k \qquad (9)$$

where the A_k are dummies for each of the age groups in the sample. It is then easy to see how age, working through the σ_ϕ, can increase reports in both tails of the distribution. Again, while the relative variance terms for different age groups, i.e. the b_k, are identified, the overall "baseline" variance is not; we thus set σ_0 equal to one and interpret the b_k accordingly. To the extent the estimated variance of unobserved differential treatment declines with age, and that this model successfully mimics the pattern of reported discrimination across age groups in the raw data, we can conclude that a possible cause of the greater reports of hurtful discrimination among young women is greater *unobserved* heterogeneity in the labor market experiences of such women.

Coefficient estimates for Model 2 are shown in column 2 of Tables 8 and 9; for the most part all the observed covariates (Z) have coefficients of similar size and significance compared to column 1 of Tables 8 and 9. Concerning the variance parameters (b), we find that the estimated variance of unobserved evidence decreases as we move "up" the age categories, beginning with the omitted category (age 15-24). The standard deviation is significantly higher among the youngest age group, compared to the oldest. Further, as Table 10 shows, Model 2 is also much better at mimicking the patterns of reported discrimination across age groups for both men and women in our data. Overall, we take our estimates of Models 1 and 2 together as mixed support for the "heterogeneity" hypothesis, because if the experiences of the young really are more heterogeneous, we would expect at least some of this to be captured by their observed characteristics in Model 1. Given this mixed message, we now turn to a final hypothesis.

The final hypothesis we consider in this paper involves not a difference in the "actual" labor market experiences of the young and old, but a difference in how their experiences are translated into survey reports of harm or advantage. In particular, we now ask what happens when we hold σ_ϕ constant, but let the amount of departure from gender neutrality, K, which induces reports of hurtful or helpful discrimination, vary across age groups. Noting from (7) and (8) that an increase in K has the effect of reducing K_1 and increasing K_2 by equal amounts, this is equivalent to specifying:

$$K_1 = \overline{K_1} + \sum_{2}^{5} d_k A_k \qquad (10)$$

$$K_2 = \overline{K_2} - \sum_{2}^{5} d_k A_k \qquad (11)$$

The idea is, in a sense, that younger women (and perhaps younger men) are less "tolerant" of (or more sensitive to) departures from gender neutrality, in the sense that these are more likely to induce reports of harm or advantage, than older people. It is worth noting that this notion is, at least in principle, empirically distinguishable from the previous version of the model where unobserved heterogeneity, σ_ϕ varied across age groups;[21]

[21]To see that these "unobserved heterogeneity" and "differences in thresholds" models have different empirical implications, consider the effect of σ_ϕ and K_1 on the left hand side of (8), which determines the fraction of individuals reporting hurtful discrimination. An age-related change in

thus we estimate Model 3 as well, and ask how well it can mimic actual patterns of reported discrimination in the data.

Coefficient estimates from Model 3 are shown in column 3 of Tables 8 and 9. As we might expect, the estimated cutoffs for reporting harm or advantage move farther apart as we move up the age ladder.[22] And, as part (c) of Table 10 shows, Model 3 –like Model 2-- does a successful job of reproducing the pattern of increasing reports in both tails found among young women and men in the raw data. Indeed, while there are some subtle conceptual differences between Models 2 and 3, both yield essentially the same maximized value of the log likelihood function, and (because they have the same number of parameters as well) are essentially indistinguishable statistically. On the other hand, testing Model 1, which has four fewer parameters, against Model 3 yields a LR test statistic for women of 15.15 with a p-value of 0.004; for men of 8.93 with a p-value of 0.063. Similar test results are obtained when Model 2 is used as the unrestricted model; thus both models are preferred to the simple ordered probit specification of Model 1.

Conceptually, both greater heterogeneity in the experiences of young people and less "tolerance" of departures from gender-neutrality among the young could of course explain the greater tendency of young men and young women to report both hurtful and

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σ_ϕ changes the sensitivity of the left hand side to the observed covariates, Z. An age-related change in K_l does not.

[22]A negative d_1, for example, reduces the lower threshold (thus reducing reports of hurtful discrimination) while at the same time increasing the upper threshold and reducing reports of "helpful" discrimination.

helpful discrimination in our data. In this section, however, we have shown two things. First, if it is differences in the amount of heterogeneity that matters, it must be heterogeneity that is not captured by any of the independent variables in our data set, i.e. what we call heterogeneity on unobserved dimensions. Second, a model which allows for differentials in *either* unobserved heterogeneity, or in reporting thresholds, across age groups is statistically preferred for both women and men to one which does not. Overall, since we expect the broad idea of "heterogeneity" to be at least partially captured by observable characteristics, and because we rejected a number of other possible competing explanations in Section III of the paper, we are led to conclude that a difference in reporting behavior --of the very particular kind formalized in Model 3-- offers the most parsimonious and effective explanation of the pattern of reported harm and advantage across age groups in our data.

V. Conclusions.

Virtually all the standard "objective" measures of discrimination computed by economists, including unadjusted or residual wage gaps, and unadjusted or residual gaps in unemployment durations, tend to be higher among older than younger women. Whether this represents true age effects or a difference across cohorts remains an open question, but it is clear that, among women in the labor market today, discrimination as we usually measure it is much more prevalent among older women. At the same time, and perhaps paradoxically, younger women are much more likely than older women to report, in confidential surveys, that they have suffered from sex discrimination, or experienced gender-induced harm in the labor market. This lack of correspondence between the two measures, in our view, should raise serious questions about whether "objective" measures really capture what most women really see as discriminatory treatment in labor markets today.

This paper has attempted to resolve the apparent inconsistency between age patterns of reported and measured discrimination using a new data set on Canadian job seekers. In addition to information on a variety of "objective" labor market outcomes and on women's perceptions of gender-induced harm, this data contains information on women's perceptions of gender-induced advantage, and on men's perceptions of harm, which to our knowledge have not been examined before. We find, first of all, that young women's more frequent reports of gender-induced harm cannot be statistically attributed to any observable differences between them and older women, including the presence of children and degree of occupational segregation. Interestingly, we do find that minority status in an occupation increases reports of gender-induced harm among both women and men. However, as minority status is not correlated with age in our data, this does not help explain the age pattern of reports.

Second, as expected, young women's more frequent reports of harm also cannot be attributed to a higher level of "objectively" measured discrimination: such measures are uniformly lower for young women in our data. More generally, even a higher level of *unmeasured* discrimination cannot explain this phenomenon because it is inconsistent with the more frequent reports of gender-induced harm among young *men*, or *advantage* among young women, that we also see in our data.

In the paper we identify two further hypotheses that are potentially consistent with what we observe. One of these --greater dispersion in the labor market experiences of young workers-- receives mixed support. The other --a particular kind of change in reporting behavior-- is more immediately consistent with our data, and is our preferred explanation for the patterns observed. According to this interpretation, young workers *of both sexes* are simply more willing than older workers to interpret departures *in either direction* from gender-neutral treatment as causally related to their gender.

Overall, our conclusion that the principal cause of more frequent reports of gender-induced harm among young women is a difference in reporting is an optimistic one: it is not young women's "objective" circumstances that are worse, but simply the standards by which these circumstances are judged that are different. Whether young peoples' standards, in any sense, are more or less "correct" is a question to which we have no answer, but in either case these different standards may have interesting policy implications. For example, if lower "tolerance" of non-gender-neutral treatment remains a permanent attribute of today's cohort of young workers, designers of future antidiscrimination policies face a dilemma: while the young are less tolerant of discrimination, they also appear to be less tolerant of *reverse* discrimination. Creating policies that address the former problem without creating perceptions of the latter may thus become increasingly difficult in coming decades.

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Appendix I: "Objective" Wage Discrimination, by age group: Estimates from the 1985 Panel Survey of Income Dynamics

	Measured Discrimination		
	Log Wages		
	unadjusted gap	adjusted gap ($\hat{D}^1$)	adjusted gap ($\hat{D}^2$)
Age:	[1]	[2]	[3]
55-64	0.519	0.465	0.499
45-54	0.594	0.455	0.420
35-44	0.465	0.340	0.334
25-34	0.326	0.233	0.258
18-24	0.219	0.258	0.202

Table 1: Sample Mean Characteristics

	Men		Women	
	Mean	(Std. Error)	Mean	(Std. Error)
Age	36.65	(0.23)	37.41	(0.27)
Education Variables: Less than High School	0.34	(0.01)	0.25	(0.01)
High School	0.31	(0.01)	0.32	(0.01)
Some College or University	0.14	(0.01)	0.13	(0.01)
College	0.12	(0.01)	0.16	(0.01)
University	0.10	(0.01)	0.14	(0.01)
Marital Status: Married	0.61	(0.01)	0.62	(0.01)
Separated/ Divorced/ Widowed	0.08	(0.01)	0.12	(0.01)
Single	0.31	(0.01)	0.26	(0.01)
Children less than 6	0.28	(0.01)	0.23	(0.01)
percent female in occupation	0.25	(0.00)	0.55	(0.01)
tenure (weeks)	230.53	(7.79)	219.79	(7.19)
pre-separation hourly wage	15.72	(0.16)	10.09	(0.13)
post-separation hourly wage	16.37	(0.19)	10.06	(0.16)
Unemployment Spell (weeks)[1]	20.70	(0.44)	23.84	(0.68)
Discrimination Measures: Hurt	0.06	(0.00)	0.10	(0.01)
Help	0.05	(0.00)	0.04	(0.00)

Notes: 1. Includes incomplete spells. 2. Number of observations is 2280 and 1586 for males and females, respectively. Due to missing data, the number of observations is lower for some variables.

Table 2: Women's Reports of Gender-Induced Harm, by age group

	Fraction Reporting Gender-Induced Harm	
Age:		
55-64	0.043	(0.005)
45-54	0.088	(0.007)
35-44	0.093	(0.007)
25-34	0.123	(0.008)
15-24	0.132	(0.009)

Note: Exact binomial standard errors in parentheses.

Table 3: Probit Coefficients for Gender-Induced Harm, Various Specifications

	[1]	[2]	[3]	[4]	[5]
Age	-.0134 (3.20)	-.0138 (3.15)	-.0115 (2.05)	-.0112 (1.99)	-.0106 (1.85)
High School		-.0142 (0.12)	.0098 (0.08)	-.0171 (0.14)	.0151 (0.12)
Some College/ University		-.1490 (0.95)	-.1758 (1.09)	-.1807 (1.10)	-.1173 (0.71)
College Degree		-.1387 (0.93)	-.1442 (0.95)	-.1251 (0.81)	-.0445 (0.28)
University Degree		.1932 (1.38)	.1953 (1.36)	.1747 (1.19)	.2472 (1.64)
Married			-.2465 (2.17)	-.2186 (1.89)	-.2270 (1.96)
Separated, Div-orced, Widowed			.1366 (0.85)	.1494 (0.92)	.1224 (0.75)
Children under 6			-.0396 (0.45)	-.0472 (0.52)	-.0409 (0.45)
Tenure			-.0001 (0.37)	.0000 (0.20)	.0000 (0.05)
Percent female in occupation					-.5660 (2.72)
Province dummies	no	no	no	yes	yes
Number of obs.	1548	1519	1473	1471	1466

Note: Absolute value of t-statistics in parentheses.

Table 4: Selected Measures of "Objective" Discrimination, by age group

	Measured Discrimination: Various Outcome Measures								
	Preseparation Wages			Postseparation Wages			Unemployment Durations		
	unadjusted gap	adjusted gap ($\hat{D}^1$)	adjusted gap ($\hat{D}^2$)	unadjusted gap	adjusted gap ($\hat{D}^1$)	adjusted gap ($\hat{D}^2$)	unadjusted gap[1]	adjusted gap ($\hat{D}^1$)	adjusted gap ($\hat{D}^2$)
Age:	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
55-64	0.568	0.738	0.602	0.66	0.831	0.678	0.358	0.976	0.377
45-54	0.575	0.564	0.556	0.609	0.633	0.615	0.392	0.551	0.292
35-44	0.504	0.438	0.494	0.546	0.493	0.553	0.468	0.608	0.317
25-34	0.372	0.34	0.377	0.423	0.403	0.434	0.171	0.476	0.28
15-24	0.259	0.45	0.321	0.373	0.504	0.405	0.071	0.791	0.031

[1]Female unemployment duration minus male. Censoring adjusted for using a censored normal regression model of log spell duration on a gender dummy, separately by age category.

Table 5: Probit Coefficients for Gender-Induced Harm, Controlling for "Objective" Discrimination on the Postseparation Job

Panel A					
	[1]	[2]	[3]	[4]	[5]
Age	-.0182 (3.12)	-.0202 (3.33)	-.0155 (2.09)	-.0144 (1.92)	-.0146 (1.92)
$\hat{D}^1$	-.0031 (0.02)	.0100 (0.08)	.0397 (0.32)	-.0024 (0.02)	-.0086 (0.07)
percent female in occupation					-.5808 (2.11)
education	no	yes	yes	yes	yes
demographics	no	no	yes	yes	yes
province	no	no	no	yes	yes
Number of obs.	927	927	927	908	907

Table 5: Probit Coefficients for Gender-Induced Harm, Controlling for "Objective" Discrimination on the Postseparation Job (Continued)

Panel B					
	[1]	[2]	[3]	[4]	[5]
Age	-.0116 (2.37)	-.0157 (2.78)	-.0216 (2.99)	-.0112 (1.99)	-.0106 (1.85)
$\hat{D}^2$	-.1326 (0.48)	.3342 (0.65)	1.507 (2.23)	dropped[2]	dropped[2]
percent female in occupation					-.5660 (2.72)
education	no	yes	yes	yes	yes
demographics	no	no	yes	yes	yes
province	no	no	no	yes	yes
Number of obs.	1471	1471	1471	1471	1466

Notes: 1. In these tables $\hat{D}^2$ was calculated for all workers --even those without postseparation jobs-- from the postseparation log wage regressions. Very similar results are obtained if the sample is restricted to those with postseparation jobs. 2. We exclude $\hat{D}_i^2$ from the estimating equation because $\hat{D}_i^2$ is a linear combination of the other included variables. 3. Absolute value of t-statistics in parentheses.

Table 6: Probit Coefficients of Gender-Induced Harm on Age, Controlling for Discrimination in Other Dimensions

Observed characteristics controlled for:	Preseparation Wage		Unemployment Spell	
	[1]	[2]	[3]	[4]
	$\hat{D}^1$	$\hat{D}^2$	$\hat{D}^1$	$\hat{D}^2$
$\hat{D}$ only	-.0123 (2.65)	-.0104 (2.13)	-.0317 (2.60)	-.0136 (1.70)
education	-.0135 (2.83)	-.0118 (2.26)	-.0366 (2.85)	-.0138 (1.69)
education, demographics	-.0122 (2.05)	-.0182 (2.44)	-.0287 (1.86)	-.0113 (1.05)
education, demographics, province	-.0116 (1.93)	-.0112 (1.99)	-.0372 (2.25)	-.0181 (1.55)
education, demographics, province, percent female in occupation	-.0107 (1.77)	-.0106 (1.86)	-.0421 (2.38)	-.0184 (1.58)
"femocc" coefficient in above	-.7286 (3.33)	-.5550 (2.65)	-1.450 (2.24)	-.5863 (1.48)

Note: Absolute value of t-statistics in parentheses.

Table 7: Frequency of other aspects of reported discrimination, by age group

	Women		Men		
Age	Help	Hurt+Help	Hurt	Help	Hurt+Help
	[1]	[2]	[3]	[4]	[5]
55-64	0.011	0.053	0.045	0.038	0.083
45-54	0.034	0.122	0.042	0.030	0.071
35-44	0.027	0.120	0.058	0.063	0.122
25-34	0.051	0.174	0.061	0.050	0.111
< 25	0.064	0.195	0.067	0.058	0.125

Table 8: Ordered Probit Models of Gender-Induced Harm and Advantage: Women

	Model 1	Model 2	Model 3
Age 25-34	-.0982 (0.78)	-.0956 (0.82)	-.1236 (0.99)
Age 35-44	-.0945 (0.72)	-.1162 (0.97)	-.1859 (1.38)
Age 45-54	-.0360 (0.24)	-.0508 (0.38)	-.1009 (0.67)
Age 55-64	.0010 (0.01)	-.0780 (0.41)	-.1983 (0.74)
Education : high school	-.0789 (0.76)	-.0832 (0.92)	-.0833 (0.79)
some college or university	.0821 (0.62)	.0557 (0.48)	.0777 (0.58)
college degree	.1377 (1.09)	.1169 (1.05)	.1349 (1.05)
university degree	-.0842 (0.65)	-.1149 (1.02)	-.0992 (0.76)
Married	.1970 (1.99)	.1725 (1.97)	.1934 (1.97)
Separated/divorced/ widowed	.0463 (0.33)	.0083 (0.07)	.0206 (0.15)
Children less than 6	.0118 (0.16)	.0215 (0.33)	.0141 (0.19)
Tenure (ROE job)	.0000 (0.14)	.0000 (0.28)	.0000 (0.16)
Percent female in occupation	.4385 (2.57)	.4087 (2.73)	.4612 (2.66)
c_1	-1.10 (6.87)	-.9704 (6.32)	-.9680 (5.70)
c_2	1.99 (11.72)	1.71 (9.22)	1.75 (9.09)
d_2	n/a	n/a	-.0849 (0.79)
d_3	n/a	n/a	-.2988 (2.66)
d_4	n/a	n/a	-.2428 (2.02)
d_5	n/a	n/a	-.6352 (2.66)

Table 8: Ordered Probit Models of Gender-Induced Harm and Advantage: Women (Continued)

b_2	n/a	-.0649 (0.87)	n/a
b_3	n/a	-.2013 (3.17)	n/a
b_4	n/a	-.1747 (2.48)	n/a
b_5	n/a	-.3219 (3.61)	n/a
Log likelihood	-705.71	-697.44	-698.13

Notes: 1. 1466 observations in all three models. 2. Province dummies were included in all three models. 3. Absolute value of t-statistics in parentheses.

Table 9: Ordered Probit Models of Gender-Induced Harm and Advantage: Men

	Model 1	Model 2	Model 3
Age 25-34	.0636 (0.56)	.0575 (0.53)	.0588 (0.53)
Age 35-44	.0940 (0.75)	.0819 (0.68)	.0852 (0.70)
Age 45-54	-.0172 (0.12)	-.0322 (0.24)	-.0420 (0.29)
Age 55-64	.0389 (0.22)	.0320 (0.19)	.0349 (0.19)
Education : high school	-.1799 (2.04)	-.1815 (2.13)	-.1875 (2.11)
some college or university	-.1286 (1.13)	-.1367 (1.25)	-.1375 (1.21)
college degree	-.2341 (1.96)	-.2254 (1.96)	-.2353 (1.97)
university degree	-.2741 (2.12)	-.2940 (2.35)	-.2908 (2.23)
Married	-.0320 (0.34)	-.0215 (0.24)	-.0283 (0.30)
Separated/divorced/ widowed	-.1038 (0.71)	-.1081 (0.77)	-.1078 (0.72)
Children less than 6	.0000 (0.00)	-.0042 (0.07)	-.0016 (0.03)
Tenure (ROE job)	.0000 (0.60)	-.0001 (0.70)	-.0001 (0.65)
Percent female in occupation	-.2672 (1.69)	-.2617 (1.73)	-.2702 (1.70)
c_1	-1.76 (13.14)	-1.70 (11.51)	-1.70 (11.51)
c_2	1.57 (11.89)	1.47 (10.04)	1.48 (9.76)
d_2	n/a	n/a	-.0515 (0.54)
d_3	n/a	n/a	-.0117 (0.12)
d_4	n/a	n/a	-.2860 (2.39)
d_5	n/a	n/a	-.1862 (1.21)

Table 9: Ordered Probit Models of Gender-Induced Harm and Advantage: Men (Continued)

b_2	n/a	-.0358 (0.61)	n/a
b_3	n/a	-.0051 (0.08)	n/a
b_4	n/a	-.1578 (2.65)	n/a
b_5	n/a	-.1173 (1.43)	n/a
Log likelihood	-805.14	-800.45	-800.68

Notes: 1. 2057 observations in all three models. 2. Province dummies were included in all three models. 3. Absolute value of t-statistics in parentheses.

Table 10: Predictions of Gender-Induced Harm and Advantage by Age Category: Alternative Models

(a) Model 1: Fixed Cutoffs and Variance				
Gender	Age Category	Hurt	No Impact	Help
Females	55-64	0.085	0.867	0.047
	45-54	0.095	0.862	0.043
	35-44	0.106	0.855	0.039
	25-34	0.108	0.854	0.038
	<25	0.112	0.852	0.035
Males	55-64	0.051	0.897	0.052
	45-54	0.056	0.899	0.045
	35-44	0.047	0.899	0.055
	25-34	0.050	0.899	0.050
	<25	0.055	0.900	0.046
(b) Model 2: Variance varies with Age				
Gender	Age Category	Hurt	No Impact	Help
Females	55-64	0.051	0.935	0.014
	45-54	0.087	0.878	0.036
	35-44	0.093	0.880	0.027
	25-34	0.122	0.826	0.051
	<25	0.140	0.799	0.061
Males	55-64	0.039	0.921	0.041
	45-54	0.038	0.935	0.028
	35-44	0.054	0.884	0.062
	25-34	0.053	0.895	0.052
	<25	0.063	0.883	0.054

Table 10: Predictions of Gender-Induced Harm and Advantage by Age Category: Alternative Models (Continued)

(c) Model 3: Cutoffs vary symmetrically with Age				
Gender	Age Category	Hurt	No Impact	Help
Females	55-64	0.048	0.940	0.012
	45-54	0.089	0.874	0.037
	35-44	0.094	0.878	0.028
	25-34	0.121	0.829	0.050
	<25	0.138	0.803	0.059
Males	55-64	0.040	0.918	0.042
	45-54	0.038	0.935	0.027
	35-44	0.054	0.885	0.062
	25-34	0.053	0.894	0.053
	<25	0.063	0.883	0.054

V

Conclusion

Three issues on gender differentials in the labor market are explored in this thesis. In the first chapter, I examine why there exists inter-ethnic variation in the gender wage gap, contrasting the role of human capital factors and cultural factors, such as differences in preferences regarding family structure and women's role in market versus home work. While human capital factors do play an important role, controlling for these factors does not eliminate inter-ethnic variation in the gender wage gap. In fact, for first generation immigrants, I find that even after controlling for all observable characteristics in the United States, a one percentage point increase in the home country gender wage gap is associated with a 0.9 percentage point increase in the gender wage gap across ethnic origin groups in the United States. I argue that this strong positive correlation suggests the importance of cultural factors. Although I am unable to detect the effect of home country factors for second-and-higher generation immigrants, there appears to be a role for "tastes" regarding work and family, in addition to the more commonly-analyzed human capital and institutional factors, in explaining why some women earn more relative to men than others.

The second chapter of my thesis assesses the effect of Employment Equity programs on the job search outcomes and on the perceptions of discriminatory treatment of both men and women. I find some evidence that employment equity coverage in a preseparation job reduces the unemployment durations of women relative to men. Although this effect is substantial in magnitude, it is imprecisely measured. Interestingly, this effect operates largely through highly significant differences in the rate at which women and men are recalled to the preseparation 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.

Finally, my 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 rates, women seem unwilling to acknowledge this gain: I 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.

The final chapter of my thesis attempts to shed some new light on why young women are more likely to report being harmed by their gender in confidential surveys than older women. Using a recent sample of Canadian job seekers, I show, first, that young women's more frequent reports of gender-induced harm cannot be statistically attributed to any observed personal or job characteristics, or to any "objective" measure of discrimination computable in my data. Second, using new questions asked in the aforementioned survey, I note that women's reports of gender-induced *advantage*, as well as *men's* reports of gender-induced harm, are also more prevalent among the young. Using a formal model of the reporting decision, I conclude that the most likely cause of all these phenomena is a particular kind of age difference in reporting behavior: young people of both sexes are more likely than older people to interpret departures *in either direction* from gender-neutral treatment as causally affected by their gender.

Overall, my conclusion that the principal cause of more frequent reports of gender-induced harm among young women is a difference in reporting is an optimistic one: it is not young women's "objective" circumstances that are worse but simply the standards by which these circumstances are being judged that are different. Gender, per se, appears to have a greater salience among the young in that they are more willing to attribute adverse *or* advantageous outcomes to it. Whether they are right is another question, but the greater salience of gender issues among the young is an important fact to bear in mind when interpreting evidence on perceived discrimination or inequities in labor markets.