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SEX DISCRIMINATION AND WOMEN'S LABOR MARKET INTERRUPTIONS

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ABSTRACT

The human capital explanation of sex differences in wages is that women intend to work in the labor market more intermittently than men, and therefore invest less. This lower investment leads to lower wages and wage growth. The alternative "feedback" hypothesis consistent with the same facts is that women experience labor market discrimination and respond with career interruptions and specialization in household production. This paper explores the relationship between self-reported discrimination and subsequent labor market interruptions to test this alternative hypothesis, attempting to remove biases associated with using data on self-reported discrimination.

The paper provides evidence consistent with the feedback hypothesis. Working women who report experiencing discrimination are significantly more likely subsequently to change employers, and to have additional children (or a first child). On the other hand, women who report experiencing discrimination, and who consequently have a greater tendency for career interruptions of these types, do not subsequently have lower wage growth.

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I. Introduction

The central tenet of the human capital explanation of sex differences in wages is that women intend to work in the labor market more intermittently than men, and therefore invest less. Because much human capital investment is unobserved, it is argued that this lower investment leads to lower wages, even after controlling for observable variables that affect wages (Mincer and Polachek, 1974).[1] That women intend to work less is attributed to the "traditional" sexual division of labor in households, sometimes interpreted as reflecting efficient behavior by households (Mincer, 1962; Becker, 1985). This is reflected in numerous studies showing that women accumulate less actual experience and job tenure than men (*e.g.*, O'Neill, 1985). The evidence originally presented by Mincer and Polachek (1974) is consistent with the human capital explanation. In log wage regressions the coefficients on experience tend to be higher for never married women and women without children than for women with children. Other researchers have presented similar supporting evidence (*e.g.*, Sandell and Shapiro, 1980), as well as evidence partially consistent with the human capital model's hypotheses regarding the effects of household specialization on men's and women's wages (Korenman and Neumark, 1991 and 1992). On the other hand, empirical evidence against the human capital explanation has also been garnered, focusing on estimation of the returns to experience (*e.g.*, Sandell and Shapiro, 1978; Blau and Ferber, 1986; Goldin, 1991), and on the role of depreciation of human capital during labor market withdrawals (*e.g.*, Corcoran, 1979).

An alternative "feedback" hypothesis that is consistent with these findings is that women experience labor market discrimination--partly reflected in lower wage levels and wage growth --and respond with career interruptions and specialization in household production (Blau and Jusenius, 1976). These interruptions may in turn lower subsequent wage growth. This paper

[1]This model has been extended to explain occupational choices of men and women, and the pronounced degree of occupational segregation by sex (Polachek, 1981).

tests the feedback hypothesis, by asking whether the experience of sex discrimination increases the likelihood of future interruptions. "Interruptions" are broadly defined to include behavior that might adversely affect women's wages or their human capital investment, including breaks in the accumulation of labor market experience, employer changes, and childbearing and marriage. The paper assesses the effects of sex discrimination by looking at the relationships between women's self-reports of sex discrimination on the job and subsequent labor market interruptions (and subsequent wage growth). A variety of approaches to studying these relationships are considered, to account for numerous possible biases in estimating the effects of discrimination from self-reported discrimination data, including: systematic differences in women's propensities to report discrimination; the influence of adverse labor market outcomes unrelated to discrimination on self-reports of discrimination; and selection bias related to experiencing discrimination and continuing to work.

The paper provides evidence that sex discrimination in labor markets is partly responsible for women's labor market interruptions. Working women who report experiencing discrimination subsequently accumulate less labor market experience, although this effect is small and is not statistically significant. There is, however, a statistically significant effect of discrimination on employee turnover; women who experience discrimination are subsequently more likely to change employers. There are also statistically significant effects of experiencing sex discrimination on births of additional children, or of first children. However, women who experience discrimination, and who consequently have a greater tendency for these career interruptions, do not subsequently have lower wage growth.

II. Relationship to Previous Research

To test the human capital and feedback hypotheses, Gronau (1988) and Blau and Ferber (1991) address the question of the joint causality running from women's labor market intermittency to lower wages, and from lower wages (presumably attributable in part to

discrimination) to intermittency. Both papers do this by invoking sufficient identifying assumptions to estimate simultaneous equations models with wages and future labor market separations as the jointly dependent variables. Gronau's results tend to reject the human capital explanation, and to support the feedback hypothesis, finding no evidence that future separations lower current wages, but that lower wages lead to labor market separations. Blau and Ferber tend to find no evidence supporting either the human capital model or the feedback hypothesis, using data on expected earnings growth and planned years of work,[2] with no effect of expected years of full-time work on expected earnings growth, nor of expected earnings growth on planned years of work.

As these authors acknowledge, the validity of these findings hinge on the identifying assumptions. Gronau excludes the following variables from the wage equation: presence of children; age of youngest child; children born in the period between the wage observation and the end of the period over which separations occur; changes in marital status and residence over that same period; and family income excluding the respondent's earnings. He excludes from the separation equation tenure on the job and required training. Blau and Ferber exclude the mother's and father's occupation and the proportion of years worked by one's mother from the expected wage growth equation, and exclude preferred occupation and the respondent's job priorities from the equation for expected years of work. These exclusion restrictions are clearly somewhat arbitrary, and arguments can be made against them.

The point of this paper is not, however, to take issue with the identifying assumptions used by Gronau and by Blau and Ferber in estimating their simultaneous equations models, instead proposing different instruments. Rather, the paper seeks to shed complementary

[2]They actually run separate regressions for expected starting salaries, and expected salaries 10 and 20 years hence. But because expected starting salaries are nearly identical for men and women in their sample, we can think of expected future salaries as effectively measuring expected earnings growth.

evidence on the feedback hypothesis. Instead of studying the joint determination of wages and labor market interruptions, this paper looks at the relationship between women's self-reports of sex discrimination on the job and future interruptions.

This approach differs in two ways from the existing research. First, instead of attempting to unravel the joint determination of wages and labor market attachment, this paper attempts to study the effect of discrimination practiced by others on labor market interruptions. Since this discrimination is not a choice variable of the individual (in contrast to wage levels or growth rates, which, according to the human capital hypothesis, are choice variables), there is not a simultaneity problem. That is, self-reported discrimination may provide a more exogenous measure of discrimination experienced by women. Second, in contrast to previous research, this approach focuses on the effects of discrimination *per se*, in contrast to the effects of wages paid. Thus, it is a more direct test of the hypothesis that sex discrimination contributes to women's labor market interruptions. This is not to say that the data provide something close to a natural experiment; the following section discusses possible sources of correlation between self-reported discrimination and the residuals in equations for accumulated experience, employer changes, *etc.*, and procedures to minimize the biases from these correlations.

III. The Data and the Empirical Approach

III.A. The Data

This paper utilizes self-reported data on sex discrimination in the National Longitudinal Survey of Young Women. In 1972, 1978, 1980, 1982, 1983 and 1988 women were asked whether they had experienced sex discrimination on the job. In particular, in all of these years but one women were asked, for a period ranging over the past two to five years, whether "so far as work is concerned, you have been in any way discriminated against because of race,

religion, sex, age, nationality, or for any other reason?" Respondents could then indicate all applicable reasons (sex, race, *etc.*). In the 1972 survey, respondents were asked explicitly about sex discrimination. Variables measuring actual labor market experience and tenure were constructed using information on work histories and current employment in each year. The other variables used are quite standard.

III.B. Basic Empirical Approach

The paper focuses on three broad categories of dependent variables. The first is labor market attachment, measured by the accumulation of actual labor market experience, and by employer changes.[3] The second is demographic changes, including changes in the number of children, incidence of first births, and marriage, all of which may be associated with direct breaks in labor market attachment, less effort on the job, or lower human capital investment. Finally, because the question regarding the link between sex discrimination and labor market interruptions is motivated by the presumed effects of these interruptions on women's wages, the consequences of sex discrimination for wage growth are also examined.

The question regarding sex discrimination was asked of all respondents. However, for non-working women a response of no discrimination may simply reflect the fact that they were not working. Thus, only the data collected from working women are useful. Attention is focused on the relationship between discrimination reported by currently working women, and future labor market behavior. In general, equations are estimated of the form:

$$(1) \quad (y_{it''} - y_{it'}) = (x_{it''} - x_{it'})\beta + D_{it}\delta + \epsilon_{it''} \ , \ t'' > t \ , \ t' \geq t \ , \ t' < t'' \ .$$

D_{it} is a dummy variable indicating that respondent i reported sex discrimination in period t, and x represents a (row) vector of control variables. Changes in various dependent variables

[3]Looking at changes in tenure generates asymmetric distributions because tenure can only increase by the number of years elapsed between surveys, whereas it can decrease by the whole amount of accumulated tenure. Consequently, the discrete outcome of whether or not a woman changes employers is used instead.

are studied from some time beginning with or following the self-report on discrimination (which occurs at time t), to some future date t". For example, in the first set of specifications reported, $(y_{it''} - y_{it'})$ is the change in actual experience measured from period t--the time of the discrimination report--to the last available observation. As a shorthand, equation (1) will be written

$$\Delta y_{it''} = \Delta x_{it''}\beta + D_{it}\delta + \epsilon_{it''} \ .^4$$

The key parameter of interest in equation (1) is δ. For example, a negative estimate of δ would suggest that discrimination reduces the future accumulation of labor market experience. This would provide support for the hypothesis that discrimination causes, at least to some extent, women's labor market interruptions.

Note that in contrast to the research by Gronau (1988) and Blau and Ferber (1991), there is not an inherent simultaneity problem in equation (1), if the self-reported discrimination data accurately reflect discrimination experienced by women that is exogenous to their own decisions. In contrast, this past research substitutes the wage level (or wage growth) for D_{it} in equation (1), a variable that is clearly jointly determined under the human capital hypothesis. Nonetheless, while there is no inherent simultaneity problem, there are a number of influences that may cast doubt on the assumption that D_{it} is uncorrelated with the error term in equation (1).

III.C. Potential Biases

One feature of the data that may create a correlation between D_{it} and the error term is that there is no detail on the nature of the sex discrimination experienced. It is possible that women who experience negative labor market outcomes (such as wage declines)--of whom there are likely to be some even in the absence of discrimination--may report sex

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[4] $\Delta y_{it''}$ will sometimes be a dichotomous variable, in which case logit estimates of equation (1) are reported.

discrimination. This suggests that when variants of equation (1) are estimated with wage growth as the dependent variable, there is a potential bias introduced from a negative correlation between contemporaneous wages and reports of discrimination.[5] That is, if

(2) $\Delta y_{it'} = w_{it''} - w_{it}$,

where w_{it} is the wage, and $Corr(w_{it},D_{it}) < 0$ because of the relationship between low wages and reported discrimination, then the estimate of δ may be biased upward, against evidence for the feedback hypothesis. Because of this potential problem, when wage changes are the dependent variable in equation (1) they are defined from the first observation following the report of discrimination to the last available observation.[6] However, if the negative outcomes experienced by women consist of shifts to less steep wage profiles, then looking at future wage growth does not necessarily eliminate this type of bias. In this case there may be a bias towards finding that discrimination is associated with lower wage growth.[7,8]

A correlation between D_{it} and the error term in equation (1) could also arise because of heterogeneity of two broad types. The first is related to the problem that self-reported discrimination may reflect negative labor market outcomes. Women may have patterns of

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[5]In contrast, with information on the nature of sex discrimination, it might be possible to separate out perceived wage discrimination from other forms of discrimination, such as sexual harassment, and to look at the effects of the latter on wage growth.

[6]Paralleling this approach, when working with "attitudinal" data akin to self-reported discrimination, Freeman (1978) focuses on future job mobility when studying the effects of responses to job satisfaction questions, because in a cross-section job satisfaction might be spuriously high for workers who have just changed jobs.

[7]It turns out that there is no evidence of lower wage growth associated with reports of sex discrimination, so that this bias is not important in assessing the results.

[8]It is similarly possible that women with adverse labor market outcomes unrelated to discrimination respond by reporting discrimination, and in response to these outcomes change employers or stop working. Thus, with respect to the other dependent variables considered in this paper (labor force attachment, demographic changes), it is possible that relationships with self-reported discrimination do not reflect true effects of sex discrimination. Unlike for wages, there is no obvious solution to this problem. However, among the adverse outcomes--potentially unrelated to discrimination--to which women might respond by, *e.g.*, changing employers, are low wages or scant opportunities for wage growth. Thus, the findings (reported below) that self-reported discrimination is not associated with lower contemporaneous wages or lower future wage growth suggest that this problem is not severe.

labor market behavior that are persistent over time, and that influence employer decisions and thus self-reported discrimination. For example, again focusing on the specifications in which Δy_{it}, is the change in experience, consider a woman who has a job history of short spells of employment interrupted frequently by spells out of the labor market. An employer familiar with this record may tend to pay this woman lower wages, and to grant fewer promotions. This may be (incorrectly) perceived as sex discrimination by the woman, and, because this intermittent employment pattern persists into the future, generate a spurious negative correlation between self-reported discrimination and future accumulation of labor market experience, and hence spurious evidence in favor of the feedback hypothesis.

Biases of this sort are addressed in two ways. First, period t is defined as the first observation on a woman at which she is working for a wage and at which the self-report of discrimination is available. This minimizes as much as possible the influence of past labor market behavior on employer decisions that might, in turn, be reflected in self-reported discrimination. Second, a subset of the dependent variables can be constructed in a way such that there is no possibility of a "track record" influencing employer decisions, and results can be contrasted with those for dependent variables for which a track record can arise. Specifically, results are contrasted in which Δy_{it}, is the change in number of children, and in which alternatively it is the incidence of a first birth (for which there can obviously be no track record). Similarly, results are contrasted for the incidence of marriage (among single women) and the incidence of first marriage (among never married women).[9]

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[9]An alternative approach would be to study "change in changes," estimating an equation of the form

$$\Delta y_{it'} - \Delta y_{is} = (\Delta x_{it'} - \Delta x_{is})\beta + D_{it}\delta + \epsilon_{it'} ,$$

where Δy_{is} and Δx_{is} are changes defined over some period prior to the self-report of discrimination. In this type of equation the estimate of δ would detect the relationship between reported discrimination and the change in, for example, the rate of accumulation of labor market experience.

In the present context, however, there are three problems with this approach. First, it is desirable to focus on the earliest available data for each woman on self-reported discrimination. If a report later in a woman's career

A second type of heterogeneity bias may arise if women differ in their propensity to report sex discrimination. There is evidence of this possible source of bias in research by Kuhn (1987). "Statistical" estimates of wage discrimination--*i.e.*, the gap between actual wages paid to women and the wage that would be paid to them based on their characteristics and the male wage structure--are negatively related to self-reported discrimination, although the evidence is only weakly significant. That is, women who are least underpaid based on conventional wage equation decompositions are most likely to report sex discrimination. One explanation consistent with this evidence is that women who report discrimination experience relatively little wage discrimination, and relatively more non-wage discrimination (such as sexual harassment, lower benefits, *etc.*). Kuhn assumes that all variation in reported discrimination reflects variation in actual discrimination (wage and non-wage), and hence is led to this latter interpretation of the results. An alternative hypothesis is that those women who are in jobs experiencing relatively small amounts of wage discrimination happen to be the same women who are more likely to report sex discrimination. Evidence potentially consistent with heterogeneity in the propensity to report discrimination is that relatively young, more educated women in Kuhn's samples are more likely to report sex discrimination, controlling for "statistical" discrimination. It does not seem implausible that these women are more aware or attuned to sex discrimination, and hence are more likely to report it independently of measured wage discrimination, or even when measured wage discrimination is relatively low.

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is used, it is possible that behavior has already responded to the earlier experience (or lack thereof) of discrimination. Second, by introducing a correlation between past labor market behavior (Δy_{it}) and present self-reported discrimination (D_{it}), the problem of the influence of labor market outcomes on self-reported discrimination is re-introduced. Third, this approach would demand considerably more data, since it would require two observations on labor market behavior both before and after the discrimination self-report, and hence would result in even smaller cell sizes for those reporting discrimination than those available in the paper.

This hypothesis was put forth more forcefully in Kuhn (1990).[10]

Bias from heterogeneity in the propensity to report discrimination is addressed by studying the empirical relationship between the various dependent variables and changes in self-reports of discrimination. The simplest way to explain this experiment is to consider the subsample of women reporting no sex discrimination at the time of their first report. Then define D_{it} in equation (1) to be one for those women among this subset who report sex discrimination at the time of their second report (and define the changes in dependent and control variables as changes subsequent to the second report). The estimated coefficient of this new dummy variable should be more free of bias from heterogeneity, because it is identified solely from women who initially reported no sex discrimination. Thus, the dummy variable seems more likely to reflect actual changes in sex discrimination experienced by these women, and its coefficient is more likely therefore to reflect a causal effect.

The actual heterogeneity experiment used in the paper, which parallels this example, is to define a set of dummy variables indicating the possible responses in the two reports of sex discrimination. In particular, the following specification is estimated:

$$(3) \qquad \Delta y_{it'} = \Delta x_{it'}\beta + N1D2_{it}\delta_1 + D1_{it}\delta_2 + D1N2_{it}\delta_3 + \epsilon_{it'} \ .$$

In this equation N1D2 is a dummy variable equal to one for women reporting no discrimination in the first period (at the time of the first report), and discrimination in the second. D1 is a dummy variable equal to one for women reporting discrimination in the first period. And D1N2 is a dummy variable equal to one for women reporting discrimination in the first period, and no discrimination in the second. With this parameterization, δ_1 measures the effect of a change from no discrimination to discrimination, relative to those experiencing

[10]Yet another hypothesis is advanced in Barbezat and Hughes (1990). They argue that the quality of information about wage discrimination also influences individuals' self-reports (and actual sex-discrimination claims). Employers may exploit this tradeoff, leading to a negative correlation between measured wage discrimination and reported wage discrimination. However, the evidence presented in Barbezat and Hughes is not consistent with this alternative hypothesis.

no discrimination in either period. Conversely, δ_3 measures the effect of a change from discrimination to no discrimination, relative to those experiencing discrimination in both periods.[11] One might argue that the effects of N1D2 and D1N2 should be symmetric but opposite signed ($\delta_1 = -\delta_3$). But this is too strong a restriction, since the initial experience of discrimination for the group with D1N2 equal to one may affect their subsequent behavior. Thus, the coefficients of N1D2--the dummy variable indicating changes from reporting no discrimination to reporting discrimination--probably provide the best statistical experiments for testing the feedback hypothesis. Nonetheless, the estimates of δ_3 can be used to provide corroborating evidence, since this coefficient compares those who no longer report experiencing discrimination at work to those who continue to do so.[12]

A final source of bias that receives attention is sample selection related to self-reported discrimination. For the equations in which the dependent variable is the change in wages, the availability of the dependent variable requires that women be working for a wage (in two periods) following the self-report on discrimination. Assuming that discrimination decreases the relative utility derived from market work, it seems plausible that women who experience discrimination but choose to continue working are particularly high wage earners (net of observables). In the present context, because the dependent variable is wage growth following the self-reports of discrimination, the selection mechanism may be that women who experience

[11]An alternative, more standard parameterization (that provides the identical statistical fit) is to define dummy variables for the following categories: no discrimination-discrimination; discrimination-no discrimination; and discrimination-discrimination. The advantage of the parameterization in equation (3) is that the statistical significance of the effect of the "removal" of discrimination in the second period can be read off of the t-test for the estimated coefficient of the discrimination-no discrimination dummy variable D1N2.

[12]To check on the robustness of the conclusions, the equations were also estimated for the simpler equation restricting the sample to women reporting no discrimination in the first period, and including a dummy variable for second period discrimination. In every case, the estimated coefficients and standard errors of the second period dummy variable were nearly identical to those of N1D2 in equation (3). The only reason to expect results to differ for the alternative formulations is if the coefficients of the control variables differ among the groups with different discrimination reports. Apparently, this does not occur to any significant degree.

discrimination and continue to work are those who also enjoy relatively rapid wage growth. This can generate a positive correlation between the error term for the wage change equation and the dummy variable indicating self-reported sex discrimination (*i.e.*, a switch to discrimination in equation (3)). Admittedly, the selection problem seems most likely to be severe with respect to wage levels, and not wage growth, but the issue still merits investigation. Sample selection bias is addressed in the standard manner (Heckman, 1979) by jointly modeling wage growth and employment.

IV. Empirical Results

IV.A. Characteristics Associated with Self-Reported Discrimination

Table 1 provides descriptive statistics classified by discrimination self-reports. The first two columns classify the observations by whether or not women reported sex discrimination at the first labor market observation at which a self-report was provided. The last four columns classify observations by changes (or lack thereof) in self-reported discrimination from the first to the second observation. In all cases, means and standard errors are reported for the variables at the time of the first observation, in order to focus on characteristics associated with reporting discrimination, rather than outcomes that might ensue from experiencing discrimination. The latter type of information is provided in the tables that follow. Looking first at columns (1) and (2), quite large differences are apparent between those who do and do not report discrimination at the first report. The wages of those who report discrimination are 15 percent higher than those who do not, and those reporting discrimination have a schooling advantage of more than one year, and have slightly higher (although not significantly so) experience and tenure. They are also less likely to be married, and have fewer children on average. These results may indicate that women in higher paying jobs are more likely to experience sex discrimination. An alternative interpretation, however, is that these women are

more likely to report discrimination, independently of actual experiences of discrimination; this is the heterogeneity problem referred to above.

The proposed solution to this heterogeneity problem is to identify the effects of discrimination from women who change from reporting no discrimination to reporting discrimination. This approach will be successful to the extent that changes in reported discrimination are exogenous with respect to unobserved characteristics. Columns (3)-(6) attempt to shed some light on the plausibility of this assumption, by asking whether changes in discrimination self-reports are less strongly related to observable characteristics (at the time of the self-report) than are the first self-reports in columns (1) and (2). Along many dimensions--wages, schooling, experience, tenure, marital status, and number of children--the differences between columns (3) and (4) are in fact considerably smaller than those between columns (1) and (2), and sometimes become statistically insignificant. Since changes in reported discrimination are less strongly related with the observables at the time of the first report than are first reports of discrimination, it seems likely that the heterogeneity bias is in fact lessened by identifying the effects of discrimination from these changes.

IV.B. The Effects of Reported Discrimination on Labor Market Interruptions and Wage Growth

Next, estimates of the equations measuring the relationships between reported discrimination or changes in discrimination and subsequent labor market behavior are reported. The results are presented for three categories of dependent variables: direct measures of labor market attachment; demographic changes; and wage growth. For each set of variables, two types of analyses are carried out. First, ignoring the possibility of heterogeneity bias from a correlation between the dependent variables and women's propensities to report discrimination, estimates of equation (1) are reported. These estimates measure the association between self-reported discrimination at an early labor market observation and subsequent behavior. For each dependent variable, the maximum number of

observations with available data is used.

In each case, estimates of this association are reported first with no controls, then adding exogenous factors such as the time elapsed between observations (and sometimes age), and finally adding as additional controls variables that may themselves be influenced by the experience of discrimination. These latter estimates are of some interest because there may be simultaneity between the various dependent variables; for example, discrimination may lead to a greater tendency to have children, which may decrease labor market attachment. But discrimination may also have direct effects on labor market attachment. There do not appear to be valid identifying assumptions to untangle the potential simultaneity biases. As such, the "reduced form" specifications (without the potentially endogenous controls) are the most interesting. But the specifications with the additional controls also provide useful information on the multivariate relationships.

Second, results are reported for the equations using the changes in self-reported discrimination, again with various sets of control variables. One potentially confounding influence in these estimates is the sample selection criterion imposed because there have to be two observations at which women are working for a wage and provide self-reports on discrimination, as well as data on subsequent behavior. Another confounding influence is that the dependent variable now measures changes following the second self-report. Consequently, results of the first type of analysis, using only the initial report of discrimination as an independent variable, but with the dependent variable (the change following the second self-report) and smaller sample used in the heterogeneity experiment, are reported. This provides a means to gauge the separate influences of the sampling rule (and variable construction), on the one hand, and the heterogeneity experiment, on the other.

Labor Market Attachment

Column (1) of Table 2 reports results from regressions for changes in experience, and

logits for employer changes. The changes in experience or changes of employer are measured from the time of the discrimination report to the last available observation. In each case, a dummy variable indicating whether the woman reported sex discrimination is included as an independent variable. Estimates of the coefficient of this variable, for the various dependent variables and samples described in panels A-C, are reported in the table; each row reports a separate regression or logit.

Panel A reports results for the change in experience. The sample consists of all women working at the time of the discrimination report; the dependent variable then captures changes in experience both for women who do and do not work at a later date. The first row reports results with no controls. The estimated coefficient of .17 in column (1) indicates that women who initially report discrimination subsequently accumulate slightly more actual labor market experience, in contrast to the feedback hypothesis. The second row reports the results once years elapsed is added to the regression. The estimate (-.01) indicates no difference in accumulated experience between women who do and do not report discrimination. Adding controls for changes in marital status[13] and number of children does not alter the result.

Panel B carries out a similar analysis, restricting the sample to women working for a wage at the last observation, and measuring the change in experience to that point. If one response to discrimination is to drop out of the labor market, then smaller responses might be expected in panel B than in panel A. On the other hand, discrimination may be relatively more important for women who are more committed to the labor market. The results in panel B are potentially of greatest interest, because by focusing on women who remain at work, they speak to the question of wage differences among employed women (and between employed women and men). The estimated coefficients are the same as in panel A, once the controls

[13]This includes changes into and out of two states: married, spouse present; and divorced, widowed, or separated.

are added, reflecting no association between self-reported discrimination and the accumulation of labor market experience.

Panel C focuses on an alternative dependent variable--whether a woman who reports sex discrimination is subsequently more likely to change employers. This analysis also restricts attention to women who are working at some later date, since only for such women can an employer change be meaningfully defined.[14] The estimates of .44 to .48 reported in column (1), corresponding to a partial derivative of .10, indicate that women who report discrimination are more likely to change employers. This result holds with and without the alternative control variables, and is statistically significant.

The next step is to ask whether the results in column (1) of Table 2 are biased because of heterogeneity such that, for example, women who are likely to accumulate more experience (*i.e.*, women who are more committed to the labor market) are also more likely to report discrimination, which would bias the results against the feedback hypothesis. As a preliminary to this heterogeneity experiment, column (2) of Table 2 reports estimates of the same specifications in column (1), but for the subsample of women for whom the heterogeneity experiment can be conducted, and with the dependent variable defined as changes following the second discrimination report. In panels A and B the estimates in column (2) are very similar to those in column (1), while in panel C the evidence of an association between reported discrimination and turnover is somewhat weaker.

Table 3 reports results of the heterogeneity experiment for the changes in experience and changes of employer. The estimates of most interest are those in column (1), which measure the differentials between women who report no discrimination in the first period, and

[14]The sample is considerably larger than in panels A and B because the construction of the experience measure used in those panels requires job history and employment data that are frequently missing for some year(s), while the construction of the tenure variable is less demanding.

discrimination in the second, and women who report no discrimination in either period. These estimates more plausibly measure the causal effect of discrimination than do the estimates in Table 2, because by conditioning on a report of no discrimination in the first period the heterogeneity bias should be reduced. The results for changes in experience, reported in panels A and B, provide little evidence that discrimination leads to lower subsequent accumulation of labor market experience. This is especially true in panel A, where once account is taken of the years elapsed between observations the estimated coefficient of the change in discrimination variable in column (1) is close to zero (-.02).

The results for the subsample of women working at a later date, in panel B, are somewhat more supportive of the feedback hypothesis. The coefficients in column (1) indicate that women who change to reporting discrimination subsequently accumulate less experience, although the effects are not statistically significant. These negative estimates contrast with those in column (2) of Table 2, suggesting that the propensity to report discrimination is positively related to subsequent accumulation of experience, although the differences between the alternative estimates are small relative to the standard errors of the estimates. Including changes in the number of children reduces the absolute value of the coefficient in column (1) because, as reported below, women who switch to reporting discrimination are also more likely to have children, and children tend to reduce experience. However, the change in the estimated effect of discrimination is small. Thus the effects of discrimination on changes in the number of children and on turnover (or accumulated experience) are largely independent.

Panel C turns to the effects of self-reported discrimination on employer changes. The estimates in column (1) indicate statistically significant effects of discrimination on the probability that women change employers. The estimates in column (1) are larger than those in column (2) of Table 2, suggesting that there is heterogeneity bias against the feedback hypothesis in the cross-section estimates; the bias, again, is consistent with those more attached

to the labor market having a greater propensity to report discrimination.

Finally, Table 3 shows that the estimates of δ_3, the differential between those who no longer report discrimination, and those who continue to do so, are nearly always the opposite sign from the estimates of δ_1 in column (1) (although they are not significant). The signs of these estimates provide corroborating evidence that sex discrimination reduces labor market attachment; women who no longer report discrimination accumulate more experience and change employers less frequently than those who continue to report discrimination.

Demographic Changes

Another type of career "interruption" is demographic changes, including marriage and childbearing. Existing work on labor force participation indicates that marriage and childbearing reduce participation (Killingsworth and Heckman, 1986). The effects of demographic changes via participation ought to be reflected in the regressions for experience reported in Tables 2 and 3. However, the independent effects of marriage and childbearing are of interest for two reasons. First, the construction of actual experience is rather data intensive, and may result in a somewhat noisy measure. Second, marriage and childbearing may have independent effects on human capital investment, and therefore wages, independently of their effects on experience and tenure (Becker, 1985). Thus, negative effects of marriage and childbearing on wages can be interpreted as consistent with the human capital model.[15] But a similar feedback hypothesis can be entertained with respect to such effects. For example, if children lower wages, but childbearing is partly "encouraged" by sex discrimination in labor markets, then the negative association between children and wages may not solely reflect effects of childbearing that are exogenous with respect to labor market experiences, in which case these negative effects would overstate the causal effect of

[15]Generally, only negative effects of children appear in wage regressions for women, and these estimates are prone to heterogeneity and endogeneity bias (Korenman and Neumark, 1992).

childbearing on wages via lower human capital investment.

Table 4, column (1), reports results for specifications paralleling those in Table 2, except that changes in number of children and marital status are now the dependent variables. Panel A reports results for the change in the number of children following the first self-report on discrimination. The estimates are negative, indicating that women who initially report discrimination subsequently have fewer children; but these estimates are not statistically significant. Panel B instead focuses on the probability of having a first birth, for the subset of women who are childless at the time of the first self-report on discrimination. In this case the coefficient estimates are positive, but again they are not significant. Panels C and D focus on marital status transitions. In Panel C, the dependent variable is whether previously never married women marry for the first time, while in Panel D it is whether unmarried women (some of whom may be divorced, widowed or separated) marry subsequently. In both cases, the estimated coefficient of the discrimination dummy variable is not statistically significant, with or without controls.

Before looking at results for changes in discrimination reports, column (2) reports estimates for the subsample of women for whom the experiment using these changes can be conducted, again measuring changes in labor market behavior from the second discrimination self-report. In this case, the sampling rule and redefinition of the dependent variable result in some changes compared with the estimates in column (1). The estimated coefficients of the first-period discrimination variable increase and are now all positive. The non-robustness of these results to changes in the sample and variable definition suggests some caution in drawing conclusions for these dependent variables.

Table 5 turns to results using changes in discrimination self-reports. For the change in the number of children or probability of a first birth, reported in panels A and B, the results suggest that discrimination leads to increased childbearing. In panel A, in which the

dependent variable is the change in the number of children, the coefficient estimates in column (1) are statistically significant, with or without controls. In panel B, in which the dependent variable is a first birth, there is a positive effect that is statistically significant with no controls, and that remains significant at the ten-percent level once changes in marital status are added as controls. With respect to the results for the change in the number of children, the estimated coefficients in column (3) are negative although not significant, suggesting that in this case the "reverse" experiment yields consistent results; among women who experience discrimination in the first period, those who do not experience it in the second period have somewhat lower subsequent childbearing. The column (3) estimates in panel B are not negative, but are considerably closer to zero than the column (1) estimates. Together, these findings reinforce the evidence of a positive effect of discrimination on childbearing.[16] In addition, the results once again suggest a similar pattern of heterogeneity bias to that seen for labor market attachment; in this instance women less likely to have children (and consequently more likely to be attached to the labor market) are more likely to report sex discrimination.

Panels C and D report results for marriage. In both cases, the evidence suggests that discrimination also increases the tendency to marry; the coefficient estimates in column (1) are positive and statistically significant. However, this evidence should be regarded as less compelling because the reverse experiment in column (3) gives such strikingly similar results;

[16]It is possible that pregnant women experience (or perceive) sex discrimination related to their pregnancy, and that the estimated effects of reported discrimination on childbearing in Table 5 reflect this, rather than effects of discrimination on subsequent childbearing. To examine this possibility, the models in Table 5 were reestimated defining changes in number of children (or incidence of a first birth) beginning one year (or one survey) after the second discrimination report, to exclude births by women who may have been pregnant at the time of the report. The effects are qualitatively similar, although the estimates are less precise because some observations get lost from requiring data for an additional year (the survey year following the second report). For the estimates corresponding to panel A, for the change in the number of children, the estimates corresponding to column (1) range from .24 to .27, and remain statistically significant, and the estimates corresponding to column (3) are more negative, and statistically significant in two out of three cases. For the estimates corresponding to panel B, for first births, the column (1) estimates remain positive, although their statistical significance declines somewhat. But the column (3) estimates become negative, strengthening evidence of feedback effects.

the positive coefficient estimates in column (3) imply that women who switch from discrimination to no discrimination are also more prone to marry, relative to those who continue to report discrimination.

Wage Growth

Finally, Tables 6 and 7 report results on the relationship between self-reported discrimination and wage growth. If discrimination causes career interruptions, and career interruptions lead to lower human capital investment, then there should be a negative relationship between self-reported discrimination and subsequent wage growth. In principle, this should be strongest if account is not taken of these interruptions, since then differences in wage growth reflect both the interruptions and differences in returns to time in the labor market; once account is taken of these interruptions, only the latter effect should remain. However, as the preceding results indicate, the two types of interruptions that follow self-reports of discrimination are employer changes and childbearing, while there is little evidence of lower accumulation of experience. Because wage changes are measured beginning after the discrimination report, if most employer changes have already occurred, the only "interruption" that should lower wage growth is childbearing. But the effects of childbearing on wage growth are relatively small; in the equations estimated in this section, an additional child reduces annual wage growth by less than one percentage point. Furthermore, reported discrimination is a highly imperfect predictor of future childbearing. Consequently, the coefficient estimates of the discrimination dummy variables in the wage growth regressions that follow should be interpreted largely as testing for differences in returns to time in the labor market, whether or not account is taken of the other interruptions associated with reported discrimination.

As a preliminary, panel A of Table 6 reports results in which the dependent variable is the log wage at the time of the first discrimination self-report. These results parallel those in Kuhn (1987), with women reporting discrimination having significantly higher

contemporaneous wages. This result may reflect heterogeneity bias. But it could also reflect sample-selection bias, if women who experience discrimination and remain at work have particularly high wages. However, experimentation with standard employment-selectivity corrections (not reported) did not alter the results in panel A. Nonetheless, there may be selection bias for which this technique fails to correct.

Panel B uses subsequent wage growth as the dependent variable, where the change is measured from the first observation following the discrimination report, to a later date, in order to avoid a possible negative relationship between contemporaneous wages and self-reported discrimination. In contrast to what might be expected if discrimination reduces human capital investment, women reporting sex discrimination subsequently have higher wage growth, with statistically significant effects whether or not controls are included. Reflecting the associations between reported discrimination, experience, and employer changes, documented in Table 2, the coefficient on the discrimination dummy variable rises slightly when changes in experience and tenure are added to the equation. In panel C, where the sample is restricted to women who did not change employers, there is no association between reported discrimination and subsequent wage growth. The question remains, however, whether these results are influenced by heterogeneity bias, such that women with higher wage growth are more likely to report sex discrimination, obscuring evidence for the feedback hypothesis. As a preliminary to the heterogeneity experiment, column (2) of panels B and C report results from the same equation, for the sample and dependent variable for which the heterogeneity experiment is carried out. The results are qualitatively similar to those in column (1).

Table 7 reports results for wage growth after attempting to control for heterogeneity bias by identifying the estimated effects from those who change to reporting discrimination. As a preliminary, panel A reports results for wage changes measured from the first to the second discrimination report. If using changes in discrimination reports "solves" the

heterogeneity problem, there should be less evidence of a positive association between reported discrimination and wage growth, in contrast to the results for wage levels in panel A of Table 6. In fact, a negative association might be expected if women who experience wage declines report discrimination, irrespective of whether discrimination actually occurred. However, there is still a positive association, with those who switch to reporting discrimination experiencing annual wage growth that is higher by 2.6 to 2.9 percentage points, and, in column (3), those who switch from reporting discrimination experience lower wage growth than those who report discrimination in both periods.

The results in panel A of both Tables 6 and 7 do not provide evidence that self-reported discrimination is associated with lower wages. This raises some questions regarding the nature of the discrimination self-reports, and regarding the "first step" in the feedback hypothesis--that low wages or wage growth are perceived as discrimination. There are three possible interpretations of these findings. First, the self-reports may in fact have nothing to do with discrimination. Second, the self-reports may reflect discrimination, including wage discrimination, but other biases remain; the potential importance of heterogeneity bias was illustrated in the preceding tables, and there is no reason to believe that using changes in reported discrimination eliminates all of this bias. Finally, the self-reports may reflect discrimination that does not affect women's wages. This could take the form of non-wage discrimination, such as sexual harassment, or it could take the form of advantages accruing to male co-workers that are perceived as discriminatory.[17] If the self-reported discrimination data were not associated with any subsequent labor market behavior, there would be a strong temptation to conclude that the data have nothing to do with discrimination. However, given that these reports are associated with employer changes and with greater childbearing, it seems

[17]This might be consistent with the association between high wages and reported discrimination, if high-wage women are more likely to be working alongside males.

likely that these data do in fact reflect negative events experienced by women.

Panel B returns to the estimates looking at wage growth subsequent to the discrimination reports. The estimated "effects" of discrimination on wage growth (in column (1)) are lower that those in column (2) of Table 6, consistent with heterogeneity bias in the cross-section estimates. However, the estimated effects are still positive (and are not statistically significant).[18] As expected, adding the change in the number of children as a control increases the estimated effect; changes in the number of children are positively associated with switches to reporting discrimination, and negatively associated with wage changes. Furthermore, the reverse experiment captured in the column (3) estimates suggests the opposite results; among women who initially report discrimination, those who subsequently report no discrimination have lower, rather than higher, future wage growth than women who continue to report discrimination. Thus, in general there is not compelling evidence that discrimination leads to lower wage growth.

There are two reasons why the estimates in panel B of Table 7 may be biased against finding lower subsequent wage growth for women who switch to reporting discrimination. First, women who report discrimination are more likely to change employers. This may entail a one-time reduction in wages as accumulated tenure is lost, but these women may then, at their new employers, invest at rates higher than other women. To examine this possibility, panel C focuses on observations on women who do not subsequently change employers. Consistent with this source of bias, the estimates in column (1) of panel C provide evidence that discrimination lowers wage growth, although the estimated coefficients are not statistically significant. Furthermore, the estimated coefficients in column (3) are more negative.

[18]A comparison of the estimates in column 1 of panels A and B gives the suggestion that wage growth slows subsequent to the discrimination report for those who switch to reporting discrimination. However, a more compelling "changes-in-changes" analysis would probably require measuring wage growth prior to either of the reports of discrimination (as well as using the same sample before and after the reports). The reasons for not carrying out such an analysis are given in footnote 9.

Second, there may be selectivity bias with respect to which women continue to work after experiencing discrimination. In particular, it is plausible that among those who experience discrimination, only women with particularly high wage growth continue to work in subsequent years. The result of this selection may be that the coefficient estimates in column (1) are biased upward. The plausibility of this sort of bias is suggested by the results in Table 3 indicating that, to some extent, women who report discrimination accumulate less experience. To study this bias explicitly, the standard selection correction is used. The present context, however, requires an unusual treatment of the data. First, the "employment" equation is a single probit indicating whether a woman worked for a wage in two periods subsequent to the second discrimination report. This is the only feasible approach, given that any available observations at which women were employed for a wage were used (maximizing the length of the period between them), in order to maximize the sample size. Second, it is difficult to define the control variables such as changes in age, union status, *etc.*, for the "non-employed," partly because it is not apparent over what period these changes should be calculated. Consequently, only the specification with no controls is considered. Variables included in the probit, but not the wage change equation, are husband's income and dummy variables for marital status. The discrimination dummy variables are included in both equations.

Results are reported in panel D of Table 7. The OLS and selectivity-corrected estimates are very similar, implying no selectivity bias in the estimated coefficients of the discrimination dummy variables. These results, like the preceding ones, provide no evidence of lower future wage growth for women who experience sex discrimination.

IV. Conclusion

This paper provides evidence that sex discrimination in labor markets plays a role in leading to women's labor market interruptions. There is a statistically significant effect of

reported discrimination on the probability that women change employers. This is consistent with Gronau's (1988) finding that low wages (presumed to reflect discrimination) encourage separations. In addition, women who report sex discrimination at work are significantly more likely subsequently to have additional children, or to have a first child if they have not already done so. However, the evidence for the feedback hypothesis is not one-sided. While working women who report discrimination subsequently accumulate somewhat less labor market experience than otherwise similar women, this effect is small and is not statistically significant.

The potentially more troubling result for the feedback hypothesis is that there does not appear to be a negative relationship between self-reported discrimination and contemporaneous or subsequent wage growth. There may be biases that obscure such a negative relationship, for which the procedures used in the paper fail to correct. It is also possible that the discrimination reported by women is unrelated to wages. Finally, as discussed in the Introduction, the empirical findings in the literature on sex differences in wages tend to be ambiguous when it comes to assessing evidence on differences in returns to experience among women with different time-paths of employment. In contrast, the evidence that women tend to have different time-paths compared with men is not ambiguous. Thus, the results for wage growth in this paper may reflect the difficulties of pinning down the effects of interruptions on the returns to time in the labor market.

On the other hand, there may be coherent explanations of the findings that discrimination leads to labor market interruptions, but not to lower returns to time in the labor market. First, there may be a statistical inference problem on the part of employers. They may know that women, on average, are more prone to career interruptions than men, but cannot discern which women are more likely to experience interruptions; hence, they shy away from specific human capital investments in all female workers. This could lead to low returns to experience for women generally, but no differences in these returns among women who do

and do not experience sex discrimination. This may seem paradoxical, since employers ought to realize that by reducing discrimination they can reduce the likelihood of interruptions among their female employees. But if discrimination emanates from employees rather than employers, this form of statistical discrimination can still persist.

An alternative explanation is that women hold similar *ex ante* probabilities of experiencing sex discrimination and the ensuing labor market interruptions. Since human capital investments are largely based on expected interruptions, then because women hold similar expectations regarding discrimination and interruptions, there may be small differences, *ex post*, in the human capital investment of those women who do and do not experience discrimination. At the same time, there would be large *ex post* differences regarding labor market interruptions, as those who experience discrimination respond by, for example, changing employers.

If either of these explanations is correct, then the evidence that sex discrimination is partly responsible for women's labor market interruptions implies that sex discrimination may also be part of the reason for women's lower wages compared with men, even if sex discrimination explains little of the variation in wages among women.

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Table 1
Descriptive Statistics at First Observation on Self-Report of Discrimination[1]

	First Self-Reports		First and Second Self-Reports			
	No Disc. (1)	Disc. (2)	No Disc.-No Disc. (3)	No Disc.-Disc. (4)	Disc.-No Disc. (5)	Disc.-Disc. (6)
Log wage (nominal)	1.85	2.00	1.86	1.92	2.00	2.01
	(.01)	(.03)	(.01)	(.04)	(.06)	(.05)
Schooling	12.93	14.14	13.10	13.61	13.98	14.44
	(.06)	(.20)	(.08)	(.26)	(.32)	(.30)
Experience	4.93	5.24	5.06	4.93	4.78	5.72
	(.07)	(.20)	(.08)	(.33)	(.30)	(.36)
Tenure	2.87	2.93	3.00	2.53	2.88	3.35
	(.07)	(.22)	(.09)	(.32)	(.32)	(.42)
Age	25.92	26.80	26.32	26.06	27.22	27.10
	(.09)	(.25)	(.11)	(.37)	(.43)	(.39)
Year	1975.25	1976.50	1975.78	1976.29	1976.20	1976.80
	(.08)	(.25)	(.09)	(.34)	(.36)	(.31)
Married, spouse present	.60	.47	.60	.52	.45	.54
	(.01)	(.04)	(.02)	(.06)	(.06)	(.07)
Divorced, widowed, or separated	.13	.17	.13	.15	.27	.07
	(.01)	(.03)	(.01)	(.05)	(.06)	(.03)
Number of children	.99	.63	1.06	.78	.90	.52
	(.03)	(.07)	(.04)	(.12)	(.13)	(.12)
Union	.24	.21	.24	.25	.26	.17
	(.01)	(.03)	(.01)	(.06)	(.06)	(.05)
Urban	.74	.81	.72	.81	.78	.81
	(.01)	(.03)	(.01)	(.05)	(.05)	(.05)
South	.42	.34	.42	.33	.37	.38
	(.01)	(.04)	(.02)	(.06)	(.06)	(.06)
Maximum sample size	1517	156	951	63	60	60

1. Standard errors are reported in parentheses. In columns (3)-(6) the sample size for some variables is sometimes less than the maximum because good data for the first observation were not required for the regressions using changes in self-reports of discrimination.

Table 2
Change in Experience and Employer Regression (Logit) Coefficients, Based on First Self-Report of Discrimination[1]

	Full sample, changes measured from first observation on discrimination report, to last available observation	Sample with two discrimination reports, changes measured from second observation on discrimination report, to last available observation
	Discrimination first period (1)	Discrimination first period (2)
A. Change in actual experience, no controls	.17	-.07
	(.32)	(.20)
Add years between observations	-.01	.03
	(.15)	(.13)
Add changes in marital status and number of children	-.02	.05
	(.15)	(.13)
Sample size	1776	1532
B. Change in actual experience, for women working for a wage at last available observation, no controls	.43	.08
	(.37)	(.22)
Add years between observations	.02	.03
	(.15)	(.13)
Add changes in marital status and number of children	-.01	.03
	(.15)	(.13)
Sample size	1437	1239
C. Proportion changing employer, no controls	.44	.28
	(.18)	(.16)
	[.10]	[.07]
Add years between observations	.47	.28
	(.18)	(.16)
	[.10]	[.07]
Add changes in marital status and number of children	.48	.28
	(.18)	(.16)
	[.10]	[.07]
Sample size	2027	1791

1. Dependent variables are first entries in each panel. Employer change equations are estimated as logits. Standard errors are reported in parentheses. For the logits, partial derivatives of the probability of an employer change with respect to the discrimination dummy variable are reported in square brackets. Each entry reports the coefficient estimate of the discrimination dummy variable for one regression or logit. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question.

Table 3
Changes in Experience and Employer Regression (Logit) Coefficients, Based on Changes in Self-Reports of Discrimination[1]

	No discrimination first period-discrimination second period (1)	Discrimination first period (2)	Discrimination first period-no discrimination second period (3)
A. Change in actual experience, no controls	-.22	-.14	.09
	(.27)	(.28)	(.39)
Add years between observations	-.02	-.09	.24
	(.18)	(.19)	(.26)
Add changes in marital status and number of children	.09	-.02	.16
	(.18)	(.18)	(.25)
Cell size	88	162	83
Sample size		1532	
B. Change in actual experience, for women working for a wage at last available observation, no controls	-.53	-.07	.22
	(.29)	(.31)	(.42)
Add years between observations	-.13	-.04	.12
	(.17)	(.18)	(.25)
Add changes in marital status and number of children	-.07	-.03	.11
	(.17)	(.18)	(.24)
Cell size	67	126	65
Sample size		1239	
C. Proportion changing employer, no controls	.53	.47	-.29
	(.21)	(.24)	(.31)
	[.13]	[.11]	[-.07]
Add years between observations	.54	.48	-.30
	(.21)	(.24)	(.31)
	[.13]	[.12]	[-.07]
Add changes in marital status and number of children	.52	.47	-.29
	(.21)	(.24)	(.31)
	[.13]	[.11]	[-.07]
Cell size	109	180	97
Sample size		1791	

1. Dependent variables are first entries in each panel. Changes are measured as in column (2), Table 2. Employer change equations are estimated as logits. Standard errors are reported in parentheses. For the logits, partial derivatives of the probability of an employer change with respect to the discrimination dummy variables are reported in square brackets. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question. For each row, columns (1)-(3) report coefficient estimates for one regression or logit. The omitted category is no discrimination in either period. Thus column (1) compares those who report discrimination in the second period, but not in the first, to those who report discrimination in neither period (the coefficient of N1D2 in equation (3)), and column (3) compares those who report discrimination in the first period, but not the second, to those who report discrimination in both periods (the coefficient of D1N2 in equation (3)).

Table 4
Change in Number of Children, First Birth, and Marital Status Regression (Logit) Coefficients, Based on First Self-Report of Discrimination[1]

	Full sample, changes measured from first observation on discrimination report, to last available observation	Sample with two discrimination reports, changes measured from second observation on discrimination report, to last available observation
	Discrimination first period (1)	Discrimination first period (2)
A. Change in number of children, no controls	-.06	.04
	(.07)	(.06)
Add years between observations and age at time of self-report	-.05	.03
	(.07)	(.06)
Add changes in marital status	-.06	.01
	(.07)	(.06)
Sample size	1629	1582
B. Proportion having first birth, no controls	.03	.35
	(.22)	(.23)
	[.01]	[.08]
Add years between observations and age at time of self-report	.10	.43
	(.22)	(.24)
	[.02]	[.09]
Add changes in marital status	.10	.37
	(.22)	(.25)
	[.03]	[.08]
Sample size	712	568
C. Proportion marrying for first time, no controls	-.10	.20
	(.28)	(.33)
	[-.02]	[.04]
Add years between observations and age at time of self-report	.17	.39
	(.29)	(.35)
	[.04]	[.07]
Sample size	549	365
D. Proportion marrying, no controls	.06	.45
	(.22)	(.22)
	[.02]	[.10]
Add years between observations and age at time of self-report	.22	.53
	(.23)	(.23)
	[.05]	[.12]
Sample size	823	727

1. Dependent variables are first entries in each panel. First birth and marriage equations are estimated as logits. Standard errors are reported in parentheses. For the logits, partial derivatives of the probability of the events with respect to the discrimination dummy variable are reported in square brackets. Each entry reports the coefficient estimate of the discrimination dummy variable for one regression or logit. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question.

Table 5

Change in Number of Children, First Birth, and Marital Status Regression (Logit) Coefficients, Based on Changes in Self-Reports of Discrimination[1]

	No discrimination first period-discrimination second period (1)	Discrimination first period (2)	Discrimination first period-no discrimination second period (3)
A. Change in number of children, no controls	.31	.15	-.16
	(.08)	(.08)	(.11)
Add years between observations and age at time of second self-report	.27	.11	-.12
	(.07)	(.08)	(.11)
Add changes in marital status	.26	.10	-.14
	(.07)	(.08)	(.11)
Cell size	92	166	85
Sample size		1582	
B. Proportion having first birth, no controls	.66	.34	.18
	(.32)	(.30)	(.43)
	[.15]	[.08]	[.04]
Add years between observations and age at time of second self-report	.61	.41	.19
	(.33)	(.31)	(.45)
	[.13]	[.09]	[.04]
Add changes in marital status	.59	.40	.06
	(.34)	(.31)	(.46)
	[.13]	[.09]	[.13]
Cell size	43	91	37
Sample size		568	
C. Proportion marrying for first time, no controls	.81	-.20	.98
	(.44)	(.48)	(.63)
	[.15]	[-.04]	[.19]
Add years between observations and age at time of second self-report	.64	.02	.86
	(.46)	(.50)	(.66)
	[.11]	[.003]	[.15]
Cell size	24	51	22
Sample size		365	
D. Proportion marrying, no controls	.71	.13	.69
	(.29)	(.34)	(.43)
	[.16]	[.03]	[.15]
Add years between observations and age at time of second self-report	.72	.23	.64
	(.29)	(.34)	(.43)
	[.16]	[.05]	[.14]
Cell size	54	96	53
Sample size		727	

1. Dependent variables are first entries in each panel. Changes are measured as in column (2), Table 4. First birth and marriage equations are estimated as logits. Standard errors are reported in parentheses. For the logits, partial derivatives of the probability of the event with respect to the discrimination dummy variables are reported in square brackets. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question. For each row, columns (1)-(3) report coefficient estimates for one regression or logit. The omitted category is no discrimination in either period. Thus column (1) compares those who report discrimination in the second period, but not in the first, to those who report discrimination in neither period (the coefficient of N1D2 in equation (3)), and column (3) compares those who report discrimination in the first period, but not the second, to those who report discrimination in both periods (the coefficient of D1N2 in equation (3)).

Table 6
Wage Level and Change Regression Coefficients, Based on First Self-Report of Discrimination[1]

	Full sample, wage changes measured from first observation following first discrimination report, to last available wage	Sample with two discrimination reports, wage changes measured from first observation following second discrimination report, to last available wage
	Discrimination first period (1)	Discrimination first period (2)
A. Log wage, first observation, no controls	.15 (.03)	...
Add age, union, urban, South, and schooling, and year dummies	.07 (.03)	...
Add experience and tenure	.07 (.03)	...
Add marital status and number of children	.06 (.03)	...
Sample size	1673	...
B. Annual change in log wage, no controls	.012 (.005)	.011 (.008)
Add changes in union, urban, South, and schooling	.012 (.005)	.010 (.008)
Add changes in experience and tenure	.013 (.005)	.012 (.008)
Add changes in marital status and number of children	.013 (.005)	.013 (.008)
Sample size	1211	1134
C. Annual change in log wage, working at same employer, no controls	.001 (.007)	.002 (.009)
Add changes in union, urban, South, and schooling	.001 (.007)	.001 (.009)
Add changes in experience and tenure	.001 (.007)	.002 (.009)
Add changes in marital status and number of children	.002 (.007)	.002 (.009)
Sample size	443	549

1. Dependent variables are first entries in each panel. Standard errors are reported in parentheses. Each entry reports the coefficient estimate of the discrimination dummy variable for one regression. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question.

Table 7
Wage Level and Change Regression Coefficients, Based on Changes in Self-Reports of Discrimination[1]

	No discrimination first period-discrimination second period (1)	Discrimination first period (2)	Discrimination first period-no discrimination second period (3)
A. Annual change in log wage, first to second report, no controls	.026	.031	-.038
	(.014)	(.015)	(.020)
Add changes in union, urban, South, and schooling	.026	.028	-.033
	(.014)	(.015)	(.020)
Add changes in experience and tenure	.029	.036	-.036
	(.013)	(.015)	(.020)
Add changes in marital status and number of children	.027	.036	-.036
	(.013)	(.015)	(.020)
Cell size	79	139	72
Sample size		1321	
B. Annual change in log wage, no controls	.006	.023	-.023
	(.011)	(.011)	(.015)
Add changes in union, urban, South, and schooling	.004	.020	-.018
	(.011)	(.011)	(.015)
Add changes in experience and tenure	.004	.022	-.018
	(.011)	(.011)	(.015)
Add changes in marital status and number of children	.006	.023	-.019
	(.011)	(.011)	(.015)
Cell size	63	120	60
Sample size		1134	
C. Annual change in log wage, working at same employer, no controls	-.018	.014	-.028
	(.013)	(.013)	(.018)
Add changes in union, urban, South, and schooling	-.018	.014	-.028
	(.013)	(.013)	(.018)
Add changes in experience and tenure	-.018	.014	-.028
	(.013)	(.013)	(.018)
Add changes in marital status and number of children	-.018	.014	-.027
	(.014)	(.013)	(.018)
Cell size	25	52	25
Sample size		549	

Table 7 (continued)

	No discrimination first period-discrimination second period (1)	Discrimination first period (2)	Discrimination first period-no discrimination second period (3)
D. Selectivity-corrected estimates, annual change in log wage, no controls[2]			
OLS	.010	.024	-.023
	(.011)	(.011)	(.016)
Selectivity corrected	.015	.014	-.011
	(.011)	(.013)	(.019)
Cell size	59	116	57
Sample size		1088	

1. Dependent variables are first entries in each panel. Changes are measured as in column (2), Table 6, except in panel A where they are measured from the first to the second report. Standard errors are reported in parentheses. Sample is restricted to women working for a wage at the time of the first observation on the self-reported discrimination question. For each row, columns (1)-(3) report coefficient estimates for one regression. The omitted category is no discrimination in either period. Thus column (1) compares those who report discrimination in the second period, but not in the first, to those who report discrimination in neither period (the coefficient of N1D2 in equation (3)), and column (3) compares those who report discrimination in the first period, but not the second, to those who report discrimination in both periods (the coefficient of D1N2 in equation (3)).

2. The two-stage selection correction was used. Variables included in the employment probit are the discrimination dummy (dummies), husband's income, and dummy variables for marital status, defined at the time of the second self-report on discrimination. Sample size is smaller than in panel B because of missing data on husband's income.