Analyzing Employment Discrimination:  
From the Seminar Room to the Courtroom  

By Mark R. Killingsworth*

In scholarly work, most economists investigate empirical questions about employment discrimination by estimating the parameters of a suitable econometric model—for example, the parameters $b$ and $d$ of the simple prototype equation

$$P_i = \sum_j b_j X_{ji} + \sum_k d_k D_{ki} + u_i$$

where $i$ subscripts refer to individuals; $P$ is some employment outcome (e.g., pay); the $X_j$'s are individuals' characteristics (e.g., education); the $D_k$'s are demographic indicator variables for individuals' sex, race, ethnicity, and the like; and $u$ denotes unobservables. If the coefficient $d_k$ on variable $D_k$ denoting group $k$ is positive (or negative), then persons in group $k$ receive higher (or lower) $P$, on average, than persons in a reference group who are the same in terms of all other factors (both $X$ and $u$). In this case, and subject to the usual caveats, economists would generally say that group $k$ benefits from (or suffers) employment discrimination, relative to otherwise similar persons in the reference group, with respect to the outcome $P$.

I. Problems with the Economist's Methodology

The main caveats concern the characteristics ($X$) taken into account when (1) is estimated. The first is the omitted-variables problem: data on some variables relevant to the outcome $P$ may not be available. If so, these variables cannot be included among the $X$'s; rather, they must be subsumed into the error term $u$. If persons in different demographic groups who are similar in terms of the included variables ($X$) are nevertheless systematically different in terms of the omitted variables, then the error term $u$ will be correlated with the demographic variables ($D$), even when the included variables are held constant. In this case, conventional regression estimates of the $d$ parameters will be statistically biased: they will not measure the true difference in the outcome $P$ for otherwise similar persons in different demographic groups.

The second caveat about the variables used in estimating (1) is the included-variable problem: some of the included variables may themselves be affected by discrimination. In this case, conventional regression estimates of the $d$ parameters face two kinds of objections (David E. Bloom and Killingsworth, 1982). The first is conceptual: if the $X$'s are affected by discrimination, then differences in the outcome $P$ for persons in different demographic groups who have the same characteristics ($X$) measure only the extent of “incremental” discrimination (i.e., only those effects of discrimination not already embodied in discrimination-induced differences in these characteristics). The second objection is statistical: if the $X$’s are affected by discrimination—i.e., if they are determined along with the employment outcome of direct interest, $P$—then they are endogenous; and conventional regression estimates of the parameters of a model with endogenous right-hand-side variables may suffer from statistical bias. Thus, conventional regression methods may not even provide unbiased estimates of the extent of “incremental” discrimination.

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II. The Legal Relevance of the Economist's Methodology

Broadly speaking, in evaluating claims of employment discrimination the legal system has been remarkably receptive to studies using the economic methodology just described. Moreover, in considering how the above-noted caveats might apply to empirical studies in specific cases, the legal system has at times shown a degree of sophistication that compares not unfavorably with that found in the typical economics seminar room (see e.g., Bloom and Killingsworth, 1982; Orley Ashenfelter and Ronald Oaxaca, 1987 [especially p. 324]). However, like Oliver Twist and Samuel Gompers, the legal system not infrequently asks economists for "more"—for studies that tackle substantive questions not usually considered in the literature, and which may take the researcher into terrain that is not well marked on conventional methodological maps.

One such issue has to do with identifying the specific employment practices that may have led to discriminatory outcomes. For example, suppose an economist analyzes this week's payroll at the XYZ Company using equation (1) and obtains a negative and statistically significant coefficient on the indicator variable for female sex. In a scholarly setting, this would be quite sufficient to establish that XYZ's current salary payments discriminate against women relative to men (subject, of course, to the caveats noted previously). For legal purposes, however, it can be desirable or even essential to go beyond this simple finding to ask how it came about: Does XYZ pay different wages to equally qualified men and women doing the same job ("unequal pay for equal work")? Are women less likely than equally qualified men to be in better-paid jobs ("unequal access to better-paid work")? Is there discrimination in starting salaries? in salary increases? in promotions?

A paper by Burton G. Malkiel and Judith Malkiel (1973), one of the first contributions to the scholarly literature in this area, is noteworthy both for its insightful discussion of some of these issues and for its complete silence regarding others. Using an expression similar to (1), the Malkiels found strong evidence of sex discrimination in current salary at a particular employer. Rather than stop there, however, they went on to consider what factors might have produced this result. In effect, they generalized (1) by introducing a two-equation system of which the following is a simple prototype:

\[ \begin{align*}
(2a) \quad P_{1i} &= \sum_{j} b_{1j} X_{ji} + \sum_{k} d_{1k} D_{ki} + g_1 P_{2i} + u_{1i} \\
(2b) \quad P_{2i} &= \sum_{j} b_{2j} X_{ji} + \sum_{k} d_{2k} D_{ki} + u_{2i}
\end{align*} \]

where there are now two employment outcomes, \( P_1 \) and \( P_2 \), and thus two sets of parameters (\( b_1 \), \( d_1 \) for \( P_1 \); and \( b_2 \) and \( d_2 \) for \( P_2 \)) and two terms (\( u_1 \) and \( u_2 \)) denoting unobservables. In terms of the Malkiels' analysis, \( P_1 \) refers to current salary and \( P_2 \) refers to job level. Note that (2a) implies that \( P_2 \) is a structural determinant of \( P_1 \) via the coefficient \( g_1 \).

Equation (2a) raises the included-variables issue, for \( P_2 \) ("job level") may itself be affected by discrimination (i.e., the \( d_2 \)'s may be nonzero). Indeed, the Malkiels' estimate of the \( d_1 \) coefficient for female sex in (2a) was essentially zero. Far from concluding that this meant that there was no "discrimination," however, they noted that this indicated only that there was no unequal pay for equal work (i.e., no sex difference in pay for otherwise similar persons in the same job level \( P_2 \)). They also obtained a large negative estimate of the \( d_2 \) coefficient for female sex in (2b) and noted that this measured unequal access to better-paid work (i.e., a sex difference in job level for otherwise similar persons). Finally, the bottom-line discriminatory difference in pay is the sum of these two components—for example, \( \frac{dP_1}{dD_k} = d_{1k} + g_1 \frac{dP_2}{dD_k} = d_{1k} + g_1 d_{2k} \) for group \( k \). Thus, the Malkiels not only showed how specific employment practices contributed to a bottom-line discriminatory pay differential; they also showed how to resolve the conceptual issues.
raised by the included-variables problem—
by properly interpreting the results.

Unfortunately, the Malkiels had much less
to say about the statistical issues, even
though these may be as serious as the con-
ceptual issues. Using ordinary least-squares
regression (OLS) to estimate (2a) as the
Malkiels did (and as many economists would
do) may yield biased estimates of \( g_1 \) (i.e.,
the effect of “job level” on pay) and even of
the \( d_1 \)'s (i.e., the extent of “unequal pay for
equal work”). For example, suppose a com-
pany discriminates against women by sys-
tematically putting them in lower job levels
than men, other things being equal. It fol-
lows that, for persons with the same ob-
served characteristics \( X \), at the same job
level, the average woman will be better-
qualified than the average man in terms of
the unobserved factors \( u_2 \) that affect job
level. If the unobservables \( u_2 \) are positively
correlated with the unobservables \( u_1 \) that
affect pay (e.g., if unobserved “motivation”
affects both job level and pay within job
level), then OLS estimation of (2a) may
tend to imply that women are paid more
than otherwise similar men at the same job
level. Of course, such a result would not
mean that men actually suffer discrimina-
tion in the form of “unequal pay for equal
work”; it would merely measure the extent
of statistical bias in OLS estimation.

As any good econometrician will attest,
estimation with endogenous right-hand-side
variables—the essential statistical issue
raised by expressions such as (2a)—is not
exactly a novel problem. Might it not be
possible to use a standard technique already
available in the literature—instrumental
variables, say—to derive consistent esti-
mates of the parameters of equations such
as (2a)? In principle, yes. In practice, how-
ever, the legal system’s interest in investi-
gating the variety of distinct decisions that im-
pinge on current salary and the complexities
of each may require new techniques. For
example, in the Malkiels’ work, the “job
level” variable \( P_2 \) is categorical rather than
continuous (as in the usual instrumental-
variables setup) and has ordinal but not
cardinal meaning; and, in many companies,
pay given one’s “job level” is subject to both
a minimum and a maximum. Perhaps, then,
one might tackle this problem by extending
the two-limit Tobit model (Richard N.
Rosett and Forrest D. Nelson, 1975) to the
case of a simultaneous-equation system with
an ordered categorical variable. Unfortu-
nately, this is likely to lead into some unfa-
miliar territory, as no such model exists,
insofar as I know.

III. An Example: Analyzing Hiring
Discrimination

In contrast with other employment prac-
tices, it might seem that analyzing race or
sex differences in an employer’s hiring deci-
sions would be quite straightforward. Since
such an analysis would be concerned with
job applicants, who by definition are not
now working for the employer, none of their
characteristics \( X \) can have been influenced
by the employer. Apparently then, there are
no endogenous variables and no included-
variables problems.

The reality can often be more compli-
cated, however. For example, hiring may
not be a simple yea-or-nay affair but rather
a multistage process in which interviews,
tests, and the like precede the final hiring
decision. Here, as with pay, the law may
either advise or require analysis of each
stage as well as (or even instead of) the
ultimate hiring decision. For analyses of
each stage other than the first, a form of the
included-variables problem may arise: the
applicants considered at stage \( n \) are limited
to those who survived stage \( n - 1 \), whose
outcome, like that of stage \( n \), was ultimately
determined by the employer. Thus, esti-
mates of race and sex effects at any stage
may be statistically biased due to nonran-
dom selection at the previous stage.

As an example, consider the following
analyses of alleged race discrimination in
hiring for low-level jobs based on actual
litigation involving a nondurable-goods
manufacturer. Each applicant was first in-
terviewed by one of several plant officials;
the survivors of this stage were then given
further consideration. As implied earlier,
analyzing the first or “interview” stage of
this process is straightforward; in column (i)
of Table 1, I present the estimated coefficient on the indicator variable for black race in a probit analysis of an equation similar to (1) in which the dependent variable, \( P \), denotes passing the interview stage. Even for this stage, legal considerations might suggest (or require) consideration of the effect of each interviewer. Accordingly, column (ii) of Table 1 repeats the analysis of column (i) with one difference: the analysis of column (ii) adds indicator variables to identify the plant official who interviewed each candidate. The difference in log likelihoods between columns (i) and (ii) suggests that the plant officials differed significantly in their propensity to pass applicants. However, as shown in column (iii) these differences were race-neutral: interacting the indicator for “black race” with the indicators for “interviewer” does not appreciably change the log likelihood. In other words, although some of the interviewers evaluated applicants more stringently than did others, all of them evaluated blacks relative to whites in essentially the same way.

The firm then gave further consideration to the survivors of the interview stage, hiring some and rejecting the rest. To evaluate the hiring decision per se, one might simply perform another probit analysis, using only the data for the survivors, like the one summarized in column (iv) of Table 1. Of course, this ignores the possibly nonrandom nature of the final selection induced by the first or interview stage (and, in particular, by the very strong racial differential at that first stage). For a more general approach that reflects the two-stage nature of this process, consider the model

\[
\begin{align*}
(3a) \quad P_{1i} &= \sum_j b_{1j}X_{ji} + \sum_k d_{1k}D_{ki} + u_{1i} \\
(3b) \quad P_{2i} &= \sum_j b_{2j}X_{ji} + \sum_k d_{2k}D_{ki} + u_{2i}
\end{align*}
\]

where \( Y_{1i} = 1 \) if and only if \( P_{1i} > 0 \), and \( Y_{1i} = 0 \) otherwise; \( Y_{2i} = 1 \) if and only if \( P_{1i} > 0 \) and \( P_{2i} > 0 \), and \( Y_{2i} = 0 \) otherwise. \( Y_{1i} \) is an indicator for “passes the first interview,” and \( Y_{2i} \) is an indicator for “is hired.” (Equivalently, one might say that \( Y_{2i} \) is observed if and only if \( P_{1i} > 0 \); i.e., \( i \) passes the interview.) In the spirit of the univariate probit models in Table 1, let the joint distribution of the unobservables in (3) be bivariate normal with unit variances and correlation \( \rho \). Then the log-likelihood func-
tion for this model becomes

\[
L = \sum_i (1 - Y_{1i}) \log(\Phi(-a_{1i})) + \sum_i Y_{1i}(1 - Y_{2i}) \\
\times \log[F(a_{1i}, -a_{2i}, -\rho)] + \sum_i Y_{1i} Y_{2i} \log[F(a_{1i}, a_{2i}, \rho)]
\]

where

\[
a_{1i} = \sum_j b_{1j} x_{ji} + \sum_k d_{1k} D_{ki}
\]

\[
a_{2i} = \sum_j b_{2j} x_{ji} + \sum_k d_{2k} D_{ki}
\]

and where \(\Phi\) is the univariate standard normal cumulative density function and \(F\) is the bivariate standard normal cumulative density function.

Columns (v) and (vi) present maximum-likelihood estimates of the parameters for black race in the "interview" and "hiring" equations, (3a) and (3b), respectively. The cross-equation correlation, \(\rho\), is positive, large in absolute value, and significantly different from zero at reasonable test levels. The black coefficient for hiring is about 50-percent larger in the two-equation model than it is in the single-equation model [compare the results in columns (vi) and (iv) in Table 1]. At least in this example, then, some purely statistical issues, arising from the interest in (necessity of?) looking at each stage of the selection process for legal purposes, turn out to be empirically important.

IV. Concluding Comments

Both in the news media and among members of the bar, there is considerable cynicism about so-called experts of all kinds, including economists. "Joked a Chicago trial lawyer at a recent conference, there are three kinds of liars: 'The common liar, the didactic liar, and the scientific expert'" (Wall Street Journal, 1989). Yet outright misconduct by economists in (or outside) the courtroom is so rare that it cannot explain the degree of skepticism about economists among the legal profession. A much more serious problem (and a more plausible explanation for such skepticism) is that the so-called expert analyses provided by economists to the courts may well produce confusion instead of enlightenment. In employment-discrimination cases as in other kinds of litigation, rival economists present analyses that lawyers (and even other economists) regard as dauntingly technical and esoteric. Predictably, the rival economists' results are usually different, but the reasons for the difference may not be obvious. Each side's lawyers then praise their own economist's analyses and cite what they view as fatal flaws in the other's work. Only rarely do they attempt to show that the alleged flaws actually explain why the results diverge, however. For example, each side may well attempt to identify coding errors in the data used by the other side's economist. However, it is less common for either side to take the logical next step: to do new analyses, with corrected data, in order to determine whether the supposed errors are in fact empirically important. More often than not, the difference in results remains largely unexplained. From the point of view of the judge in the case, this may not be a happy situation.

In fairness to the legal profession, it should be noted that lawyers are not particularly well equipped for such work, and they do not always get much help from economists, for economists are sometimes guilty of precisely the same failings in their everyday work as scholars. As a case in point, consider the widely divergent estimates of the female labor supply elasticity that have appeared in the literature: one economist may fit a Tobit model to 1980 Census data for women aged 25–44; another, using a different set of variables, may fit a selection bias-corrected regression to 1985 Current Population Survey data for women aged 35–54; and so on. Differences in the elasticity estimates may be due to differences in data, variables, statistical
technique, or something else (e.g., definitional differences or even programming errors). Possible reasons such as these for differences in elasticity estimates have been much discussed, but one rarely sees an attempt to determine which of them is actually material in a given setting.

In sum, the legal system’s questions about employment discrimination may sometimes raise some relatively unfamiliar conceptual and statistical issues for economists. However, as implied in the previous section, courtroom confrontations about different empirical results represent one version of an old story whose familiar but oft forgotten moral is perhaps the most important one economists can teach to lawyers and judges (and relearn for themselves). When different analyses generate different answers, one should try to determine which aspects of those analyses really matter empirically and which do not. That will make judging easier for judges; and it is excellent exercise for scholars.

REFERENCES