MEASURING THE EFFECT OF

AN ANTIDISCRIMINATION PROGRAM

Orley Ashenfelter*
James Heckman*

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*Orley Ashenfelter, Princeton University
 James Heckman, The University of Chicago and NBER

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Since 1941, six Executive Orders have been issued forbidding

Federal government contractors from discriminating against minority

workers.[1] The latest and strongest Order, 11246, issued in 1965,

enjoins firms to take affirmative action in recruiting and promoting

minority workers. Contractor firms are required to abide by the Order

in all operations including those unrelated to the performance of

the contract. In principle, all prospective contractors are required

to demonstrate compliance with the law before a contract is let. The

potential penalties are severe: failure to comply with the law may

result in revocation of current contracts and suspension of the right

to bid on future contracts.

Despite these provisions, doubts have been raised about the

effectiveness of the Orders. In part, these doubts are based on the

small size of the enforcement staff relative to the population of

potential violators.[2] The division of responsibility between the

Office of Federal Contract Compliance (OFCC) and the staffs of the

various Federal departments weakens the enforcement effort. The Order

is imprecise as to the definition of "discrimination" and "affirmative

action." To heighten doubts, before 1971 no firm had its contract

revoked for noncompliance.

Defenders of the Orders cite cases in which contract award

dates have been postponed until firms have taken steps toward compliance

with the law. Competition among contractors has led firms to adopt

affirmative action programs to improve their chances of winning contracts.

In this paper, we investigate these competing claims using data

from 40,445 establishments sampled in 1966 and 1970. For this period,

we find that the employment of black males relative to white males increased 3.3 percent more in firms with government contracts than in firms without contracts. This comparison controls for the effect of employment expansion, variation in the size of firms, and geographical variation in the supply of labor. Using similar controls, we find little evidence to support the view that the Orders have improved the relative occupational distribution of black workers. We find strong evidence that among segregated firms, contractors are more likely to integrate than noncontractors.

In the first section of this paper, we distinguish what can be measured from what cannot. We develop a framework to measure and interpret program effects. In the second section we discuss the design of our sample and present results of an analysis of the randomness of this sample. In the third and concluding section, we present the estimates and discuss their plausibility.

I. A Framework of Analysis

a. What Is and Is Not Measured

In this study, we attempt to measure the effect of Executive Order 11246 on the employment and occupational distribution of black males relative to white males in Federal contractor firms compared to identical relative measures for blacks in noncontractor firms. Before discussing specific indices, it is clarifying to consider a problem that plagues this, and other studies that seek to measure the effect of a program on its target population.[3] Since we have a (two point) time series, a natural method for assessing the effectiveness of a program is to measure the change in a suitable index for target firms relative to the change in the index for the remaining firms. The basic problem is the absence of a control group in the presence of a program with economy-wide impact. Although a program may directly affect one group of firms, it also indirectly affects the remaining group of firms as well. Accordingly, comparisons of changes in the relative status of blacks in target and nontarget firms cannot be measured relative to what might have been in the absence of the program since that state is not observed. Thus, we cannot measure the contribution of the Executive Order to improvements in the aggregate relative status of blacks. Nonetheless, we can measure whether or not the contract compliance program has had any differential impact on the two types of firms.

Consider an index of black male relative (to white male) status measured at time t, S_t. This index will be defined more precisely

below, but for the purposes of the present discussion, it can be viewed

as an acceptable index of relative black male welfare. The proportionate

change in this index through time

$$(1) \qquad I = \frac{S_t - S_{t-1}}{S_{t-1}}$$

affords a measure of trend in black economic status. The actual

behavior of S_t is to be distinguished from the hypothetical behavior

S_t^h that would have occurred in the absence of the program. Proportionate

movements in the hypothetical index

$$(2) \qquad I_h = \frac{S_t^h - S_{t-1}^h}{S_{t-1}^h}$$

measure trends in relative black economic welfare in the absence of the

program.

Index I can be fruitfully decomposed into three components.

Denoting the relative status of blacks in contractor firms at time t by

S_t^g, one measure of the change in relative black status in contractor

firms compared to the hypothetical change in the absence of a contract is

$$(3) \qquad I_g = \frac{S_t^g}{S_{t-1}^g} - \frac{S_t^h}{S_{t-1}^h} .$$

Assuming small changes, we may write

$$(4) \qquad \ln(1 + I_g) \overset{\sim}{=} I_g = \Delta \ln S_t^g - \Delta \ln S_t^h .$$

Using obvious notation, a similar index for blacks in noncontractor firms may be written

$$(5) \qquad \ln(1 + I_{ng}) \doteq I_{ng} = \Delta \ln S_t^{ng} - \Delta \ln S_t^h .$$

If fraction G of black employment is in contractor firms, the observed total change in the relative position of blacks may be written as

$$(6) \qquad \Delta \ln S_t = \Delta \ln S_t^h + G(\Delta \ln S_t^g - \Delta \ln S_t^h) + (1-G)(\Delta \ln S_t^{ng} - \Delta \ln S_t^h)$$

$$= \Delta \ln S_t^h + GI_g + (1-G)I_{ng} .$$

Equivalently,

$$(7) \qquad \Delta \ln S_t = \Delta \ln S_t^{ng} + G(\Delta \ln S_t^g - \Delta \ln S_t^{ng}) .$$

Note that I_{ng} and I_g are meaningful only if black status would be the same in both contractor and noncontractor firms in the absence of a compliance program.

The first component of equation (6) is the change that would occur in the absence of the program. We cannot measure this component, nor can we measure I_G or I_{ng}. We can measure the components of equation (7). The first component of this equation is the change in relative black status in noncontractor firms. The second component measures the difference in the change in black status between contractor and noncontractor firms weighted by the proportion of black employment in contractor firms.

If contracts were randomly assigned to firms, a simple calculation of mean changes in contractor and noncontractor firms would allow us to estimate the two components of total change in equation (7). Since random selection is unlikely, an alternative approach which is pursued in this paper is to utilize regression techniques to standardize for nonrandom changes in the indices for the two types of firms.

We postulate the following relationship for firm k:

$$(8) \qquad \Delta \ln S_{tk} = BX_{tk} + \gamma d_k + \varepsilon_{tk}$$

where d_k is a dummy variable with a value of unity for firms holding a government contract in the second period of the data, X_{tk} is a vector of control variables, and ε_{tk} is a disturbance with the classical properties. Estimates of "γ", weighted by G, yield an estimate of the second component of equation (7).

The available data put substantial restrictions on the way in which the quantity S may be measured. Since we do not have wage data, we must use more approximate measures of black relative economic status. First, consider the index of occupational position for black workers whose logarithm is defined by

$$(9) \qquad \ln O_b = \Sigma (B_j/B) \ln P_j,$$

where B_j/B is the fraction of all black workers in the jth occupation and $\ln P_j$ is the logarithm of the mean salary of <u>all</u> workers in the jth occupation. O_b is nothing more than the (geometric) mean salary

that would be received by black workers if they were paid the average

wage a white worker within the occupation is paid. O_b therefore

reflects only variation in the occupational distribution of black

workers and is nothing more than a method for summarizing the information

available on that distribution. If we define O_w as the occupational

distribution index for white workers, one possible measure of the

quantity S is the ratio O_b/O_w. The disadvantage in using this measure

is that it does not reflect the relative number of black workers in an

establishment. An alternative measure of S is $(B/W)(O_b/O_w)$, which

corrects for this deficiency. $(B/W)(O_b/O_w)$ is the ratio of the total

compensation of black workers to the total compensation of white workers

that would be received if black and white workers were paid the mean

salary that is paid to all workers in each occupation. Defining

$S = (B/W)(O_b/O_w)$ it follows that ln S may be decomposed according to

$$(10) \quad \ln S = \ln(O_b/O_w) + \ln(B/W)$$

$$= [\Sigma(B_j/B) \ln P_j - \Sigma(W_j/W) \ln P_j] + \ln (B/W)$$

$$= \Sigma[(B_j/B) - (W_j/W)] \ln P_j + \ln B/W,$$

so that for $\Delta \ln S$ we have

$$(11) \quad \Delta \ln S = \Sigma[\Delta B_j/B - \Delta W_j/W] \ln P_j + \Delta(\ln B - \ln W).$$

Thus, we decompose the relative wage share into components of

interest in their own right, and perform a separate regression analysis

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for each component. We estimate a separate coefficient for the dummy "presence of government contract" for each occupation, and for the relative employment equation. Assuming J occupations, and letting γ_j be the coefficient of the dummy for occupation j, and letting γ_E be the coefficient of the dummy in the relative employment equation,

$$(12) \quad \sum_{j=1}^{J} \gamma_j \ln P_j + \gamma_E$$

measures the estimated average difference in the ratio of the payroll of blacks to the payroll of whites between firms with a government contract and those without, holding other factors constant.

If γ_E is positive and statistically significant, government contracts induce greater relative employment of black males. If γ_j is positive and significant, the presence of a government contract alters the occupational distribution of black workers relative to white workers towards greater relative concentration of blacks in occupation j in contractor firms. The measured occupational shift may arise for several reasons: internal promotion within existing labor forces, lateral occupational moves between contractor and noncontractor sectors, and nonlateral moves between sectors.

If internal promotion is the only factor at work the expression

$$I_J = \sum_{j=1}^{J} \gamma_j \ln P_j$$

measures the differential shift in contractor firms, as compared to noncontractor firms, in the difference between the geometric mean of the

black male wage distribution and the geometric mean of the white male wage distribution. Thus I_J is a measure of differential promotion rates of blacks relative to whites between contractor and noncontractor firms.

Reshuffling of workers between contractor and noncontractor sectors obscures this measure. A lateral occupational move of a black from the noncontractor sector to the contractor sector does not contribute to the improvement of the black workforce that existed before the contract was let. Accordingly, on this score we cannot strictly interpret I_J as a relative measure of advancement of black workers. Nonetheless, since in a competitive labor market sectoral transfers occur only if wages are raised in contractor firms, a statistically significant γ_j gives indirect evidence of a rise in black wages relative to white wages in occupation j for contractor firms.

Nonlateral reshuffling between contractor and noncontractor firms has the same unmeasured wage effect. In addition, there is a promotion (or retardation) effect if the transfer of blacks relative to the transfer of whites to contractor firms alters pre-transfer differences between black and white occupational distributions within contractor firms. If the regression equations are properly specified, this source of movement in the index I_J measures the effect of occupational advancement (or retardation) through transfer.

Of these three sources, only lateral shuffling creates ambiguity in interpretation. Since we cannot measure the magnitude of the changes due to each source, we cannot measure the importance of this component in contributing to the measured total movement of the index. However, if it is small, the index I_J weighted by the proportion of

post program black employment in government contractor firms measures
the direct contribution of the contract compliance program to relative
black occupational advancement.

b. Specification of the Regression Relationships

The literature on the employment dynamics of the firm suggests that
fixity in factors causes firms to gradually adjust current levels and composi-
tion of employment to long-run target levels. We incorporate this idea into
the regression equations and make the further assumption of separability
between the dynamic system generating firm racial occupational
differentials and the equation generating relative employment.

To simplify the notation, we supress subscripts for individual
firms. Let O_t be a J x 1 vector where the jth row is the difference
in the proportions of the black and white forces in occupation j for
year t, i.e.,

$$O_{tj} = \frac{B_{tj}}{B_t} - \frac{W_{tj}}{W_t} .$$

A firm's adjustment system may be written as

(13) $O_t - O_{t-1} = \Lambda(O_t^* - O_t)$

where Λ is a J x J matrix of partial adjustment coefficients and O_t^*
is the long-run level of occupational differentials.

This relationship is exact if firm cost functions for movement
to equilibrium, and for persistence in disequilibrium are quadratic and

if the target level, O_t^*, does not depend on the costs of adjustment.
Otherwise, the adjustment function may be viewed as an approximation
to the true process of adjustment. Allowing Λ to be a general matrix,
we explicitly incorporate the notion of interrelated adjustment
processes introduced into the literature by Nadiri and Rosen.[4]

The jth component of O_t^* is given by

$$(14) \qquad O_{tj}^* \;=\; \beta_j X + \gamma_j d$$

where, as before, d is one if a firm has a government contract and is
zero otherwise. A common set of K variables, "X", is assumed to
determine all occupational differentials. With this specification, we
may rewrite equation (13) as

$$(15) \qquad O_t - O_{t-1} \;=\; \Lambda\, \beta \bar{X} - \Lambda\, \Gamma d - \Lambda\, O_{t-1} + \varepsilon_t$$

where ε_t is a $J \times 1$ vector of disturbances, $\bar{X}$ is a matrix with J identical
columns of the $K \times 1$ X vector, and β is a $J \times K$ matrix whose rows are the
β_j vectors for each of the J relationships, j=1,...,J. "Γ" is a vector
whose jth element is the coefficient of the dummy variable for the jth
long-run relationship. "$\Lambda\beta$" and "$\Lambda\Gamma$" are the short-run coefficients.

Since O_t is a column vector of differences in proportions, the
sum of the elements of O_t must be zero (i.e., if i is a $J \times 1$ vector of
unit elements, $i'O_t = 0$ for all t). This arithmetic relationship
induces an exact linear dependency among the O_{t-1} regressors. By
omitting one row of O_{t-1}, we can avoid exact multicollinearity among

the regressors. Less obviously, we can delete any one of the J

equations and estimate both short-run coefficients ($\Lambda\beta$ and $\Lambda\Gamma$) and

long-run coefficients (β and Γ).[5]

The equation for relative employment is specified in the same

fashion. Only one lagged dependent variable is involved, and

complications from proportionate dependent variables are avoided. The

regression specification becomes

$$(16) \quad \left(\frac{B}{W}\right)_t - \left(\frac{B}{W}\right)_{t-1} = \lambda \, X \, \beta_E + \lambda \, \gamma_E d - \lambda \left(\frac{B}{W}\right)_{t-1} + u_t \, .$$

The same control variables are used as those in equation (15). "u_t" is

a disturbance with the classical properties. "d" is a dummy variable

indicating presence of a government contract.

To control for variations in local labor market conditions,

regional measures of firm location are included in the X vector. In

addition, firm size variables are introduced to control the effect of

variation in technology and the efficiency of management. Measures

of firm growth and decline are used to separate the expansionary

effect of government contracts from the compositional effect which is

the focus of this study.

In our data, there are many firms with no black workers. This

forces us to confine our estimation of equations (15) and (16) to firms

with at least one black worker in both sample years, and is an important

limitation of our proposed decomposition.

However, we can utilize the remaining data to investigate the

effect of government contract compliance programs on firm integration.

Since the complete absence of blacks is easily detected, it is likely

that among a group of firms initially segregated in 1966, more government

contractor firms will be integrated in 1970 than noncontractor firms.

By estimating equations which determine the probability of integration

in 1970, we test this hypothesis.

II. The Data

Since 1966, all firms with one hundred employees or more, and all firms with a government contract, are required to file a report on the status of their employment by race and sex for each of the nine broad Census occupational categories. These reports are collected by the Equal Employment Opportunity Commission and placed on magnetic tapes (the EEO-1 tapes). From these annual samples, it is possible to utilize firm identification numbers and geographical codes to match establishments in successive years. This paper utilizes a matched sample of 40,445 such establishments which filed reports in 1966 and 1970.

The basic dilemma in matching EEO-1 reports for two different years for a given economic unit is that as we take greater care to ensure that the two economic units we match are similar in the two years, we are less likely to complete a successful match. There is thus a basic trade-off between accuracy of the match and the size of the sample. On the one hand, we are interested in careful matching so that the characteristics of firms that are difficult to control in a single cross-section can be controlled by examining the changes from one period to another for firms that maintain the same values of the uncontrolled characteristics. On the other hand, some characteristics of virtually all firms change between one period and another, and we do not want to reduce the size of the matched sample because of minor changes that are probably of little relevance to the analysis.

The most basic characteristics on which it is hypothetically possible to match EEO-1 reports are the employer identification number (EI), assigned by the Social Security Administration, and zip coded address. As it turned out, the 1966 data did not, in fact, contain zip codes, although the 1970 data did. It was thus not possible to use a matching device based on this characteristic.

The procedure we adopted was based on the existence of the appropriate data, although it can be argued on some grounds that it was a good procedure on its own. Our first step was to aggregate the reports for 1966 and 1970 by E.I. number and county. This resulted in the 117,579 reports filed in 1966 being reduced to 98,655 aggregated units, and the 126,686 reports filed in 1970 being reduced to 96,244 aggregated units (hereafter, "aggregates"). Of these two sets of aggregaged units we were able to find 40,445 aggregated units that were common to both years.

The matched sample of aggregated units was thus about forty percent of all units in the two years. In some ways, however, this is an underestimate of the extent of success of the matching process because the successfully matched units tended to be significantly larger than the unmatched units in the two years. Thus, in 1966 the 98,655 aggregated units represented 25.6 million employees and in 1970 the 96,244 aggregated units represented 28.5 million employees. The 40,445 matched units represented approximately 18.6 million employees, however, which is 65 percent of all employees covered. Thus, when the standard of comparison is the extent of employment in the matched units the success of the matching system is somewhat more impressive.

Still, it is of substantial importance to know the reasons for the failure to match more of the aggregated units than the number indicated. First, if there is some systematic factor at work, knowledge concerning it might be helpful in designing a method to secure a more complete match in future work. Second, it is important to understand the consequences of any systematic factor at work so as to be able to analyze its effects on the sample's bias.

The ideal method for examining the causes of mismatching would be to take a random sample of the units that were not matched in each year and determine the cause for a mismatch in each case. From these data it would be possible to estimate the fraction of mismatched cases due to the variety of factors that may cause a mismatch.[6] In practice this ideal procedure was unavailable because of constraints of both time and resources. Given the importance of the problem, however, it seemed essential to obtain at least some idea of the causes of non-response, and so we pursued the problem in the following crude manner. We first chose a single county to examine in more detail using the criterion that it be inexpensive to query the unmatched firms in the county. Mercer County in New Jersey was chosen for this purpose because of its representative nature regarding industry and minority groups.

There were approximately 180 aggregate units in Mercer County subject to analysis in 1966 and 1970. Of these, 103, or 57 percent of the aggregate units were mismatched, which is significantly higher than the 40 percent matched nationwide. Tables 1 and 2 list the largest of the unmatched units in 1960 and 1970 respectively, along with their total employment.

Table 1. Largest unmatched aggregates (1966) in Mercer County.

Name of firm	No. of employees
American Cigar Corporation	399
Kayes Tex Manufacturing Corporation	221
First Trenton Bank	422
Mercer Hosipital	727
Rider College	331
Capital Transit	212
Yellow Cab Company	125
Princeton Inn Company	166
Smooth Surface Floor	451
New Jersey Bell Telephone Company Trenton	4,617
H. K. Porter	948
H. D. Lee Company, Inc.	183
State New Jersey Department of Health	730
Automatic Retailer	242
American Biltrite Rubber	785
National Sponge Cushion	148
Mathematics	58
Electro Mechanical Research	145

Table 2. Largest unmatched aggregates (1970) in Mercer County.

Name of firm	Number of employees	Government contract
American Biltrite Rubber	579	Yes
Hill Refrigeration	1,127	Yes
Stauffer Chemical	196	No
Atlantic Thrift Stores	361	No
Dow Jones & Company, Inc.	344	No
Trenton Office	421	Yes
Blakely Laundry Company	174	No
Goodall Rubber Company	786	Yes
Needham's Motor Service, Inc.	177	Yes
AAA Trucking Company	209	No
Oxzyn Company	370	No
Princeton Applied Research Corporation	202	Yes
Applied Logic Corporation	167	Yes
National Sponge Cushion	166	No
Coca Cola	111	Yes
Shoprite	139	No
F.M.C. Chemical Corporation	391	Yes
DeLeval Holroyd	149	Yes
Ginos of New Jersey	119	No

We contacted some of the larger firms on each list to see if
we could determine why they had not been matched. As it turned out,
the causes could be grouped under three categories: (a) natural
causes, (b) errors, and (c) identification problems.

The three major natural factors causing an inability to match
were: closing or opening of a new establishment, a decline or increase
in size between the two years so that the obligation to file an EEO-1
form changed, and mergers. For example, Princeton Inn with 166
employees in 1966 closed its doors between 1966 and 1970, so that it
did not appear in the data for 1970; Yellow Cab Co. with 125 employees
in 1966 declined in size to fewer than 100 employees in 1970 so that
it no longer had an obligation to file in 1970. Likewise, Gino's of
New Jersey with 119 employees in 1970 had grown to over 100 employees
between 1966 and 1970 so that it had an obligation to file in 1970 but
not in 1966. Finally, First Trenton Bank with 422 employees in 1966
merged with another bank between 1966 and 1970 so that it no longer
existed as a separate entity. Clearly, each of these cases represents
a situation where the normal growth, decline, and consolidation of firms
results in some firms appearing in one year but not in another. Aside
from changes in reporting requirements, not much could have been done
to match records for firms affected by these factors.

Errors in reporting also accounted for some of the failure to
match firms. In some cases errors were due to the firms themselves,
in other cases the errors were a result of changes in reporting systems
from one year to another, and in still other cases it is not possible
to determine the source of the error. For example, Mercer Hospital

has apparently reported every year except 1970 when its EEO-1 form was never filed due to a new computerized data processing system that was installed that year and did not function correctly. The most disturbing unmatched item in Tables 1 and 2 is surely New Jersey Bell Telephone Co. - Trenton, with 4,617 employees in 1966. This unmatched item is apparently due to a limitation in the matching program that resulted from the fact that the Bell System reported on a statewide basis in 1966, but on a county basis in 1970. It was thus not possible to match the same units in the two years. Only those reporting units using a consistent system from year to year would be susceptible of a match using the scheme outlined above. Finally, Dow Jones Co. and New Jersey Department of Health, Trenton, apparently reported in both years with the same name and EI number, although their 1970 reports did not appear on our data tapes. We can only conclude that these reports were either lost or miscoded at some point.

A final cause of mismatching was the problem of identifying firms with a unique EI number. The Social Security Administration, which is responsible for administering the EI numbering system, has assigned 10,000 to 12,000 more EI numbers than there are firms. Thus, a single firm may have more than one EI number and may report using different EI numbers in different years. It is for precisely this reason that American Biltrite Rubber and National Sponge Cushion appear in both Tables 1 and 2, even though we report them as being unmatched. Each of these companies has more than one EI number and each reported under different EI numbers in the two years 1966 and 1970. Since the EI number is one of the criteria for matching, this made it impossible to match these particular firms.

Such a limited sampling is not adequate, of course, for making detailed estimates of the proportion of our unmatched firms that are accounted for by natural causes as against the proportion that are accounted for by errors and identification problems. From our limited experience, however, we would guess that perhaps 30 to 60 percent of the unmatched aggregate units are due to errors or improper identification, while the remainder are due to the natural changes in establishments over any four-year period.

In order to explore more systematically the results of the matching process we computed the multiple regression listed in Table 3. The purpose of this regression is to provide a systematic summary of the differences in the industrial, regional and other characteristics of aggregates as between those that were and were not matched. We pooled all of the 96,244 aggregates for 1966 and 1970 and created a dummy dependent variable that took on the value unity if the aggregate fell in the matched sample of firms and zero if it did not. The independent variables are sets of industrial, regional, and other dummy variables. The industrial variables are the set of nine listed in Table 3 plus a deleted variable for agriculture, fisheries, and forestry. The regression coefficients are thus interpreted as the difference between the industry listed in the table and the deleted industry in the probability that an aggregate from that industry was matched, holding the other characteristics in the table constant. For example, Table 3 indicates that holding other things constant, an aggregate in durable manufacturing was .403 more likely to be matched than an aggregate in agriculture. Likewise, an aggregate in durable

Table 3. Estimates of the effect of various factors on the probability
that an aggregate was matched (dependent variable = 1 if
matched, 0 otherwise). 96,244 aggregates for 1966 and 1970.

Industry	Mean of independent variable	Regression coefficient	Absolute T-value
1. Mining	.049	.306	21.1
2. Durable manufacturing	.187	.403	30.5
3. Nondurable manufacturing	.191	.341	25.8
4. Chemicals and allied products	.091	.333	24.2
5. Transportation, communication, other public utilities	.240	.272	20.5
6. Wholesale and retail trade	.091	.336	24.2
7. Finance, insurance, real estate	.058	.227	15.8
8. Business and other services, entertainment	.078	.298	21.5
9. Public administration	.014	.125	2.95
Region (by first digit of zip code; see key below)			
0	.095	.041	5.84
1	.141	.063	9.65
2	.098	-.031	4.48
3	.102	.081	11.8
4	.138	.063	9.64
6	.098	.032	4.58
7	.092	.082	11.6
8	.034	.078	8.15
9	.110	.047	6.94
Inside SMSA	.732	.049	13.7
Government contract	.425	.065	20.7
Firm size under 100 employees	.422	-.205	61.1
Firm size over 500 employees	.117	.170	34.1
Intercept		.061	

F-Ratio 518.6 (22,96221)

Table 3 (concluded)

Key to regions

    0   Puerto Rico, Vermont, New Hampshire, Maine, Massachusetts,
          Virgin Islands, New Jersey, Connecticut, Rhode Island

    1   Pennsylvania, New York, Delaware

    2   North Carolina, Virginia, West Virginia, South Carolina,
          Maryland, District of Columbia

    3   Florida, Georgia, Tennessee, Alabama, Mississippi

    4   Kentucky, Michigan, Indiana, Ohio

    5   Montana, South Dakota, Wisconsin, Minnesota, Iowa, North Dakota

    6   Nebraska, Illinois, Kansas, Missouri

    7   Texas, Louisiana, Arkansas, Oklahoma

    8   Arizona, Colorado, Utah, Wyoming, Idaho, New Mexico, Nevada

    9   California, Alaska, Washington, Hawaii, Oregon

Key to industries

    0   Agriculture, forestries, fisheries

    1   Mining

    2   Durable manufacturing

    3   Non-durable manufacturing

    4   Chemicals and allied products

    5   Transportation, communication and other public utilities

    6   Wholesale and retail trade

    7   Finance, insurance and real estate

    8   Business and repair service, personal services, entertainment
          and recreation

    9   Public administration

manufacturing was .062 (= .403 - .341) more likely of being matched than an aggregate in nondurable manufacturing.[7] There are also ten regional variables, with the fifth deleted. The precise lists of states in each region are presented in the key following Table 3 and is based on the first digit of the zip codes for the aggregates. As before, a regression coefficient is interpreted as the estimated difference between the region examined and the deleted region in the probability that an aggregate was matched, holding other things constant. The remaining variables are dummy variables for whether the firm was inside an SMSA, for whether it had a government contract, and for its size class.

As can be seen from Table 3, the most important determinant of whether an aggregate was matched seems to be its size. Firms with fewer than 100 employees were .21 less likely to be matched than firms with 100 to 500 employees. Likewise, firms with over 500 employees were .17 more likely to be matched than firms with 100 to 500 employees. This result is probably not very surprising given that employer size is an important determinant of whether a firm must report in any year. It suggests that the natural growth and decline of the smaller firms may be an important determinant of the extent of success of the matching process, a result previously discussed. Firms inside SMSA's are also more likely to be matched than firms outside SMSA's, but the size of this effect is not very large. Perhaps more important, firms with government contracts are .065 more likely of being matched than firms without them. It would be interesting to know the extent to which this difference reflects the fact that government contractors have more

stringent reporting requirements and the extent to which it reflects
greater care by government contractors in meeting given reporting
requirements. The regional differences do not tend to be very large,
and their direction may be surprising to some readers. For one thing,
there is no obvious tendency for the matching to be less successful in
Southern than in Northern states. Although in simple tabulations there
is substantially less matching in the South than the North, this
apparently results from the failure to hold constant industry, size of
firm, etc. Second, the midwestern states tend to be those where the
matching was poorest, although the differences involved are not dramatic.
Finally, as we have already noted there are some fairly large differences
in the extent of match by industry. Both agriculture and public
administration were industry groups where the matching was especially
poor, but the finance, insurance, and real estate industry group was
also notably less successful than other industry groups. The most
successful matching was in durable and nondurable manufacturing.
Given the few sizable differentials we have noted, however, the remainder
of the industry groups do not differ very much in the success of the
match.

It is clear from these results that the aggregates in our
matched sample are not a random sample of the aggregates we created
from the 1966 and 1970 data. If they were, there would be no significant
differences between the matched and unmatched samples of firms associated
with region, industry, etc. That is, region, industry, and the other
variables in Table 3 would not have a significant effect on the probability
of a match. In fact, these variables have a statistically significant

effect. At the same time, it would be easy to exaggerate both the size of the effect that these variables have on the probability of matching and their importance in demonstrating bias. As we have seen, most of the effects of the variables in Table 3 on the probability of a match can be explained by the variable effect of the reporting requirements on whether a firm filed a report in both 1966 and 1970. Thus, the natural consequence of firms growing and declining about the 100 employee reporting requirement is to induce substantial variability in the extent to which some employers file reports in both years, and thus in the probability of a match. On the other hand, the differences in probability of matching by region and industry that we found, and which would be much more difficult to explain by reference to the reporting requirements, are relatively small. In the absence of any further evidence, therefore, it seems very likely that the most important systematic determinant of the non-random character of the matching is the basic reporting requirements. If this is the case, inferences drawn from our matched sample may not be too different from inferences that would have been drawn from the whole population of EEO-1 reports.

III. Empirical Results

a. Estimates of the Components of the Relative Wage Bill

In this section, we report results from estimating equations
(15) and (16). Before presenting the estimates, it is useful to extend
the discussion in Section II to consider potential sources of bias
that may affect our estimates.

One source is due to improper measurement of the government
contract variables. Since the time the first draft of this paper was
written, further study has shown that many firms listed as noncontractors
in fact were contractors.[8] Establishments associated with firms
doing government contract work in other establishments frequently did
not report themselves as contractors. Yet, as noted in the introduction,
all plants of a government contractor firm are liable to the provisions
of Executive Order 11246. Since geographically disparate units of the
same firm are treated as separate data points in our analysis, this
source of bias is severe if central managements impose uniform policies
in all plants.

However, it is unlikely that uniform policies are applied to
all establishments. Indeed, it is this presumption which led us to
treat separate geographical units as distinct data points. Nonetheless,
if government contractors advance blacks at a greater rate than
noncontractors, estimates of the government contract effort are biased
downwards, and degrees of freedom are overstated to the extent that
uniform policies apply to some firms. Given the size of the sample
and the number of separate firms, the potential problem with degrees
of freedom is unlikely to be important.

Similarly, potential contractors and recent contractors not listed as contractors in 1970 are expected to be like measured government contractors in all but name. Our inability to identify such firms leads to a downward bias if these firms have taken affirmative action.

There are a host of other biases about which we have much less to say. Since reports are filed by the firm, contractor firms are more likely to exaggerate their compliance effort in order to avoid embarrassing investigations. If this is the case, an upward bias is imparted to our estimates of contract effects. However, it is by no means obvious that this effect applies only to contractor firms. Since noncontractor firms with employment less than one hundred are not required to report, it is likely that the sample of noncontractors includes a biased selection of firms which have taken steps toward affirmative action, or at least are willing to say they have.

Another potential source of bias comes from restricting our sample to establishments with at least one black worker. This restriction is dictated by our choice of the decomposition of the relative wage bill. If contractor firms initially without black workers contribute to decisive gains in the aggregate relative occupational distribution through faster promotion rates, our estimate of the effect of the compliance progress understates the true effect of the program on the mean difference in occupational distributions of blacks and whites between contractor and noncontractor firms. Unfortunately, it is not possible to determine the importance of this effect. Moreover, it is equally possible to imagine that the unmeasured firms are more sluggish in their affirmative action programs than are measured firms.

Our estimates of equations (15) and (16) are reported in
Table 4. The first column reports the relative employment equation.
The second column is the (log earnings) weighted sum of the detailed
occupational results recorded in columns 3-11. Long-run values,
estimated from the scheme of Appendix A, are presented below the
standard errors for the estimated coefficients. A convenient summary
of these results is reported in Table 5.

In this table we record both long-run and short-run coefficients
for the dummy variables indicating presence of a government contract.
Line 1 contains the estimated difference between the percentage change
in the employment of black male workers and the percentage change in the
employment of white male workers as between firms with and without a
government contract, holding other factors constant. According to
this estimate, black male employment increased by 3.3 percent more
relative to white male employment in firms with government contracts
than in firms without them, and this difference is statistically
significant. The long-run estimate is 12.9 percent. Lines 2 through 10
contain the percentage point differences between firms with and without
government contracts in the change in the percentage of black workers
minus the percentage of white workers in each of the nine occupational
categories available in the EEO-1 reporting system data. By definition
these nine percentage point changes must sum to zero, since an increase
in the percentage of black (or white) workers in one occupation implies
a decrease in the percentage of black (or white) workers in some other
occupation. In effect, the estimated coefficients in lines 2-10 of
column 2 of Table 5 tell us how the incremental employment of black

Table 4. Regression equations determining the change in the relative employment and occupational distribution of black male workers, 1966-70 (standard errors below short-run coefficients; long-run coefficients below standard errors).

Independent variables	Relative employment (1)	Relative occupational index (2)	Officials, managers (3)	Professionals (4)	Technicians (5)	Sales workers (6)	Office clericals (7)	Craftsmen (8)	Operatives (9)	Laborers (10)	Service workers (11)
Firm:											
has government contract	.0331 (3.87)	.0020 (1.33)	-.0028 (2.18) -.0069	-.0056 (4.72) -.0106	.0020 (1.69) .0033	.0005 (.400) .0003	.0031 (1.81) .0078	-.0013 (.670) .0020	.0218 (8.15) .0325	.0001 (.025) .0002	-.01 (7.6) -.0286
in Northeast	.0179 (1.51)	.0079 (3.74)	-.0003 (.180) -.0003	0.0051 (3.09) -.0099	.0006 (.353) .0023	-.0010 (.542) -.0025	.0184 (7.72) .0330	.0016 (.597) .0056	.0114 (3.08) .0147	-.0100 (2.94) -.0157	-.01 (4.8) -.0272
in Northcentral	.2000 (17.4)	-.0137 (6.97)	-.0004 (2.41) -.0028	-.0030 (1.95) -.0053	-.0034 (2.19) -.0053	-.0075 (4.45) -.0170	.0001 (.043) .0010	-.0121 (4.82) -.0179	.0136 (3.92) .0243	.0131 (4.22) .0220	.003 (1.0) -.0010
in West	-.0660 (4.46)	.0051 (1.96)	.0028 (1.26) .0080	-.0017 (.807) -.0024	.0041 (2.01) .0063	.0015 (.671) .0045	.0056 (1.89) .0081	.0009 (.276) .0004	-.0049 (1.06) -.0068	.0029 (.700) .0036	-.01 (2.8) -.0217
in SMSA	.1134 (1.00)	-.0091 (4.49)	-.0010 (5.79) -.0055	-.0088 (5.54) -.0160	.0032 (2.01) -.0053	-.0126 (7.30) -.0296	.0056 (2.46) .0144	.0102 (3.96) -.0230	.0047 (1.33) .0113	.0069 (3.00) .0125	-.00 (.64) .0154
Size of firm is:											
100-500 employees	-.0055 (.350)	-.0034 (1.20)	-.0045 (1.90) -.0094	-.0012 (.558) -.0022	.0051 (2.33) .0073	.0032 (1.35) .0094	.0042 (1.34) .0092	-.0032 (.901) -.0068	-.0134 (2.71) -.0202	.0070 (1.58) .0084	.0028 (.64) -.0043
greater than 500	.0033 (1.22)	-.0087 (1.80)	-.0010 (2.47) -.0012	-.0025 (.664) -.0043	.0057 (1.53) .0081	.0023 (.568) .0081	.0068 (1.26) .0125	-.0025 (.402) -.0069	-.0201 (2.38) -.0296	.0085 (1.12) .0104	.0118 (1.60) -.0029

Table 4 (continued)

Independent variables	Relative employment (1)	Relative occupational index (2)	Officials, managers (3)	Professionals (4)	Technicians (5)	Sales workers (6)	Office clericals (7)	Craftsmen (8)	Operatives (9)	Laborers (10)	Service workers (11)
Logarithm of total employment	.0095 (1.22)	.0018 (1.25)	.0078 (6.57) .0181	-.0058 (5.27) -.0109	-.0009 (.544) -.0022	.0050 (4.17) .0124	-.0001 (.093) .0027	-.0068 (3.82) -.0134	.0063 (2.54) .0077	-.0039 (1.75) -.0031	-.001 (.703) -.0113
Change in employment (in 00's) of:											
Officials, managers	–	-.0054 (4.40)	-.0200 (19.2) -.0463	.0030 (3.08) .0041	.0023 (2.39) .0045	.0018 (1.68) .0033	.0042 (3.03) .0154	.0034 (2.14) .0105	.0050 (2.29) .0064	.0019 (.970) -.0009	-.0015 (.775) -.0030
Professionals	–	-.0022 (3.91)	.0035 (7.27) .0061	-.0081 (18.1) -.0156	.0009 (1.98) .0007	.0000 (.082) .0006	-.0003 (.456) .0013	.0018 (2.49) .0032	-.0022 (2.18) -.0043	-.0015 (1.68) .0066	.0060 (6.80) -.0057
Technicians	–	-.0003 (.295)	-.0002 (.245) .0005	.0036 (4.38) .0067	-.0040 (4.85) -.0051	-.0008 (.847) -.0021	.0019 (1.61) .0014	-.0014 (1.07) -.0026	.0028 (1.52) -.0055	.0033 (1.97) -.0003	-.0052 (3.24) .0040
Sales workers	–	.0017 (3.87)	.0038 (10.3) .0100	.0004 (1.08) .0011	.0002 (.535) .0004	.0031 (8.36) .0073	-.0006 (1.32) -.0036	.0007 (1.28) -.0003	.0002 (.275) .0002	-.0004 (.606) -.0003	.0007 (1.07) .0142
Office clericals	–	.0010 (1.82)	.0021 (4.67) .0049	.0002 (.439) .0006	.0000 (.039) -.0001	-.0006 (1.25) -.0012	.0002 (.389) -.0005	.0005 (.792) .0001	-.0030 (3.29) .0644	-.0008 (.957) .0018	.0014 (1.74) .0007
Craftsmen	–	.0017 (3.61)	.0032 (7.83) .0081	.0021 (5.58) .0044	-.0009 (2.43) -.0015	.0001 (.309) .0001	-.0020 (3.71) -.0060	-.0028 (4.64) -.0057	.0027 (3.15) .0046	.0002 (.228) .0018	-.0025 (3.39) .0058
Operatives	–	.0018 (6.20)	.0019 (7.67) .0047	.0007 (2.88) .0016	.0003 (1.14) .0003	.0004 (1.50) .0008	-.0011 (3.46) -.0031	0.0001 (.402) -.0007	.0002 (.416) .0003	-.0011 (2.50) -.0011	-.0010 (2.14) .0028

Continued

Table 4 (continued)

Independent variables	Relative employment (1)	Relative occupational index (2)	Officials, managers (3)	Professionals (4)	Technicians (5)	Sales workers (6)	Sales clericals (7)	Crafts-men (8)	Opera-tives (9)	Labor-ers (10)	Service workers (11)
Laborers	—	-.0017 (2.58)	.0033 (6.03) .0077	.0010 (2.05) .0021	-.0005 (.971) -.0011	.0003 (.530) .0012	-.0024 (3.30) -.0057	.0000 (.031) -.0021	-.0089 (7.83) -.0133	.0001 (1.05) .0004	-.0034 (3.49) -.0108
Service workers	—	.0003 (.448)	.0030 (6.31) .0065	-.0010 (2.35) -.0019	-.0006 (1.26) -.0012	-.0002 (.313) -.0004	-.0007 (1.14) -.0019	0.0009 (1.28) -.0023	-.0014 (1.42) -.0028	-.0032 (3.61) -.0042	.0050 (5.79) .0082
Lagged (1966 value of $B_i/B - W_i/W$ for: Officials, managers	—	-.3270 (48.8)	-.4066 (71.8)	.0229 (4.36)	.0069 (1.32)	.0053 (.931)	-.1196 (15.8)	-.0503 (5.91)	.0239 (2.02)	.0384 (3.64)	.4791 (4.67)
Professionals	—	-.3371 (48.3)	.0677 (11.5)	-.5104 (93.4)	.0341 (6.28)	-.0145 (2.44)	-.1063 (13.5)	-.0306 (3.45)	-.329 (2.68)	.0381 (3.47)	.4889 (45.7)
Technicians	—	-.4069 (55.3)	.0285 (4.58)	.0576 (9.99)	-.7123 (124.3)	.0084 (1.34)	.0236 (2.84)	.0353 (3.78)	-.0004 (.028)	.0200 (1.72)	.5394 (47.8)
Sales workers	—	-.1779 (28.9)	.0448 (8.62)	-.0126 (2.61)	.0182 (3.79)	-.4241 (80.7)	-.0322 (4.64)	-.0494 (6.33)	-.0383 (3.53)	-.0486 (5.00)	.5422 (57.5)
Office clericals	—	-.1622 (33.3)	-.0304 (7.37)	-.0018 (.483)	.0411 (10.8)	.0122 (2.94)	-.5998 (109.0)	.0045 (.731)	.0043 (.499)	-.0256 (3.33)	.5348 (71.6)
Craftsmen	—	-.2253 (46.2)	.0096 (2.32)	-.0042 (1.10)	.0329 (8.64)	-.0173 (4.15)	.0019 (.346)	-.5690 (91.8)	.0319 (3.71)	-.0659 (8.56)	.5802 (77.6)
Operatives	—	-.1460 (39.6)	.0103 (3.30)	.0055 (1.91)	.0115 (4.02)	-.0182 (5.79)	-.0128 (3.08)	.0493 (10.5)	-.6574 (101.1)	.0297 (5.10)	.5821 (103.1)
Laborers	—	.1215 (31.8)	.0191 (5.91)	.0101 (3.37)	.0164 (5.51)	.0179 (5.51)	-.0221 (5.13)	-.0111 (2.29)	.1142 (17.0)	-.7005 (1.16)	.5919 (101.2)

Table 4 (concluded)

Independent variables	Relative employment (1)	Relative occupational index (2)	Officials, managers (3)	Professionals (4)	Technicians (5)	Sales workers (6)	Office clericals (7)	Craftsmen (8)	Operatives (9)	Laborers (10)	Service workers (11)
Change in logarithm of total employment	-.0386 (4.77)	—	—	—	—	—	—	—	—	—	—
Logarithm of black employment minus white employment, 1966	-.2645 (77.5)	—	—	—	—	—	—	—	—	—	—
Constant	-.7112	-.0659	-.0746	.0260	-.0072	-.0331	-.0218	-.0006	.0088	.0623	.0401
R^2	.2131	.3524	.2118	.2998	.4178	.2375	.3642	.3269	.4301	.4572	.4088

Characteristic roots of adjustment system: .2323, .5883 ± .00525i, .2657, .4810, .4503, .4069 ± .0266i; all moduli less than one, greater than zero. Thus, the system is stable.

Table 5. Estimates of the percent effect of the presence of a government
contract on the change in the employment and occupational distribu-
tion of black male workers relative to white male workers, 1966-70.

Line no.	Occupation	Estimated short-run coefficient (t-value)	Estimated long-run coefficient
1	Total employment	3.307 (2.84)	12.89
2	Officials and managers	-.279 (-2.18)	-.693
3	Professionals	-.560 (-4.71)	-1.061
4	Technicians	.199 (1.68)	.331
5	Salesmen	.052 (.40)	.028
6	Clerical workers	.304 (1.80)	.785
7	Craftsmen	-.129 (-.67)	.196
8	Operatives	2.18 (8.15)	3.25
9	Laborers	.006 (.025)	.170
10	Service workers	-1.77 (-7.65)	-2.86

workers over white workers in firms with government contracts reflected in line 1 was distributed by occupation. As can be seen from the table, the largest positive difference between those firms with and without government contracts fell in the operative category. To offset this positive difference, the change in the relative percentage of black workers in the service worker category was smaller among government contractors. The other positive coefficients in the table are for technicians, clerical workers, salesmen, and laborers, although the former are marginally insignificant statistically and the latter are essentially zero. The other negative coefficients in the table are for the managerial, professional, and craft occupations, although only the former two are statistically significant. Except for a reversal of sign in the case of craftsmen, the long-run effects preserve the same pattern as the short-run effects, but are algebraically larger.

In sum, the basic results of Table 5 indicate that the presence of a government contract in a firm was associated with a statistically significant and larger increase in the employment of black workers relative to white workers than was the case in the firms without government contracts. The increase in the relative employment of black workers increased their representation compared to white workers primarily in the operative occupation, and decreased their relative representation primarily in the service and professional occupations.

Table 6 summarizes the estimated overall quantitative impact of the pressure of governmental efforts to increase the portion of black male workers relative to white male workers. According to these results, in the short run the governmental effort increased the employment of

Table 6. Estimated partial effect on relative wage bill.

Line no.	Occupation	Estimated short-run coefficient	Estimated long-run coefficient
1	Total employment	1.654	6.424
2	Officials and managers	-.264	-.659
3	Professionals	-.529	-1.002
4	Technicians	.188	.312
5	Salesmen	.042	.0238
6	Clerical workers	.238	.614
7	Craftsmen	-.107	-.163
8	Operatives	1.59	2.37
9	Laborers	.003	.009
10	Service workers	-1.059	-1.710
	Total employment effect	1.65	6.42
	Total occupational distribution effect	.100	-.21
	Total wage share	1.75	6.21

black workers relative to white workers by 1.7 percent while in the

long run the increase is 6.4 percent. In contractor firms, the occupa-

tional position of black workers relative to white workers increased

by .1 percent while it is expected to decrease by .2 percent in the

long run. Thus, the governmental effort increased the wage share by

1.8 percent over the 1966 to 1970 period, while this increase is

expected to rise to 6.4 percent in the long run.

It is possible to briefly summarize the effects of the other

variables we examined on the employment and occupational position of

black male workers relative to white male workers. The sets of

variables we examined were a set of regional dummy variables, a variable

equal to unity if the firm was located inside a Standard Metropolitan

Statistical Area, a set of variables representing the size in total

employment (in 1970) of the firm, a set of variables representing the

change in total male employment in each of the nine occupational

categories, and the 1966 (lagged) relative position of black workers

in the firm. We first describe the estimated effects of these variables

on the employment of black male workers relative to white male workers

and then the estimated effects of these variables on the relative

occupational position of black workers. As we have observed previously,

the sum of these two effects is the estimated effect of the variable

on the relative share of black wages.

Region appears to be the most important factor associated with

relative employment increases for black workers. In particular, the

relative employment of black workers increased by 20 percent more in

the North Central region of the U.S. than in the South, and by about

18 and 26 percent more than in the Northeast or West. Second, there is no indication that black relative employment changes were significantly related to size of firm. Third, the rate of growth of employment of the firm was negatively related to changes in the relative employment of black workers, although the size of this effect was very small. Finally, the growth over 1966 to 1970 in the relative employment of black workers was smaller in firms where the 1966 ratio of black to white employment was highest.

Just as region of location was an important determinant of the change in the relative employment of black workers, it was also an important determinant of the change in the relative occupational position of black workers. In particular, the relative occupational position of black workers increased by 1.4 percent less in the North Central region than in the South, and by 2.2 and 1.9 percent less than in the Northeast or West. This suggests that the relative employment increases of black workers in the North Central region were a result of the migration of black workers from the South. If these migrants were from the lower paying occupational categories in the South, then their migration would by definition increase the relative occupational distribution of black workers in the South and decrease their average relative position in the North Central region so long as the in-migrants were in lower paying occupations than the workers already in the North Central region. Second, the relative occupational position of black workers increased significantly less in firms inside of SMSA's than in firms outside SMSA's. Third, there was little relationship between firm size and changes in the relative occupational position of black

workers. Fourth, the change in the relative occupational position of black workers was greater the greater the growth of employment in the middle level occupations, i.e., sales, clerical, craft, and operative workers. On the other hand, the growth in the relative occupational position of black workers was smaller the greater was the growth in employment of workers in the highest or lowest paying occupations, i.e., professionals, managers, or laborers. Finally, the increase in the relative occupational position of black workers was smaller the greater the relative occupational position of black workers in the firm in 1966.

b. Integration Effects

The analysis to this point has been performed for only those firms that contained at least one black and one white employee in both 1966 and 1970. Since black workers make up only one-eighth of the population, many firms would not be expected to be "integrated" in this sense. In fact, of the 40,445 firms in the sample, only 24,535 contained at least one black and one white male worker. It is of some interest, therefore, to know what effect the overall government effort to increase the relative position of black workers has had on these firms. In order to examine this question we have estimated linear probability functions using data for those firms that had no black male worker in 1966 and in which the dependent variable equals unity if the firm contained at least one male black worker in 1970, and is zero otherwise. Table 7 reports the results of this analysis for total establishment integration as well as detailed

Table 7. Estimated regression equations determining the probability that a firm with no black workers in 1966 employed at least one black worker in 1970. Dependent variable is one if a firm integrated in the given occupation reported in the column, zero otherwise.

Independent variables	Total employment	Officials, managers	Profes- sionals	Techni- cians	Sales workers	Office clericals	Craftsmen	Operatives	Laborers	Service workers
Constant	-.438	-.430	-.297	-.324	-.139	-.426	-.356	-.281	-.140	-.284
In employment increase	.115 (17.0)	.034 (9.71)	.027 (9.66)	.028 (8.81)	.035 (12.8)	.035 (9.10)	.046 (10.5)	.062 (12.2)	.064 (14.0)	.034 (8.47)
In employment decrease	.029 (2.07)	-.030 (6.48)	-.009 (2.32)	.009 (2.61)	-.017 (4.74)	-.020 (3.69)	-.020 (3.10)	-.013 (1.62)	.004 (.508)	-.012 (2.00)
Size 101-500	.005 (4.05)	-.059 (10.4)	-.053 (11.9)	-.045 (8.76)	-.027 (6.27)	-.050 (7.93)	.010 (1.39)	.019 (2.03)	.036 (4.63)	-.011 (1.50)
Size 7500	.035 (1.26)	.009 (.816)	-.011 (1.36)	.024 (2.51)	-.022 (2.82)	-.005 (.414)	.037 (2.62)	.058 (3.32)	.058 (3.91)	.035 (2.57)
North	.001 (.053)	.009 (2.06)	.015 (4.69)	.001 (.335)	-.002 (.684)	.021 (4.57)	-.018 (3.35)	-.020 (3.09)	-.022 (3.98)	-.015 (3.00)
Central	.141 (13.1)	.031 (8.00)	-.006 (2.05)	.003 (.928)	.016 (5.21)	.052 (11.9)	.099 (19.1)	.109 (16.4)	.061 (10.6)	.086 (16.6)
West	.005 (.456)	.000 (.009)	.004 (.927)	-.001 (.335)	-.010 (2.74)	-.001 (.125)	-.013 (2.04)	-.022 (3.13)	-.016 (2.51)	-.017 (3.01)
Government contract	.089 (12.04)	.019 (6.11)	.024 (9.77)	.034 (12.2)	-.009 (3.97)	.041 (12.1)	.051 (12.6)	.059 (12.0)	.018 (4.19)	-.011 (2.76)

Table 7 (concluded)

Independent variables	Total employment	Officials managers	Profes- sionals	Techni- cians	Sales workers	Office clericals	Craftsmen	Operatives	Laborers	Service workers
In SMSA	.224 (27.8)	.079 (22.3)	.054 (19.1)	.063 (19.4)	.053 (19.0)	.100 (25.7)	.068 (14.9)	.060 (10.6)	.043 (8.66)	.068 (15.4)
Total employment (thousands)	.106 (2.07)	.093 (29.7)	.064 (26.1)	.070 (24.7)	.032 (13.7)	.089 (24.9)	.079 (18.9)	.073 (14.0)	.040 (9.24)	.068 (16.7)
R^2	.393	.360	.314	.331	.176	.320	.305	.289	.225	.271

occupational integration. Since the linear probability model is known
to suffer from important statistical limitations, we computed estimates
of these equations using the logit model on a subsample of firms. These
results, reported in Appendix C, demonstrate that if the true integration
probability process is governed by a logistic disturbance, the linear
probability model provides a reliable guide to the true effects of a
government program.

Table 8 lists the estimated effect of the presence of a government
contract on the probability that a firm that was not integrated in 1966
was integrated in 1970. Integration may be defined for the firm as a
whole, and this result is reported in line 1 of the table, or for
each of the separate occupations, and these results are reported in
lines 2-10. As can be seen from the table, the probability was .09
larger that a firm that was not integrated in 1966 would be integrated
in 1970 if the firm had a government contract. The results for the
separate occupations are similar, and in all cases statistically
significant. These results imply that the presence of the overall
government effort significantly increased the extent of integration in
American industry. Even if the increase in the employment of black
workers is relatively small in each firm so affected, there is a
substantial number of firms involved so that this effect may not appear
so small when taken in the aggregate.

Table 8. Estimated effects of the presence of a government contract on
the probability that a firm with no black male worker in 1966
employed at least one black male worker in 1970.

Line no.	Occupation	Estimated coefficient (t-value)
1	Total employment	.089 (12.0)
2	Officials and managers	.019 (6.11)
3	Professionals	.024 (9.77)
4	Technicians	.034 (12.2)
5	Sales workers	-.009 (3.97)
6	Clerical workers	.041 (12.1)
7	Craftsmen	.051 (12.6)
8	Operatives	.059 (12.0)
9	Laborers	.018 (4.19)
10	Service workers	-.011 (2.76)

Summary

We have used detailed records from the Equal Employment Oppor-
tunity tapes for a matched sample of firms in 1966 and 1970 to estimate
the extent to which the change in the position of black male workers
relative to white male workers was greater among firms with contracts
with the Federal government than among firms without such contracts.
Since all of the Office of Federal Contract Compliance efforts to
improve the relative position of black workers operate on firms with
Federal contracts, and since the EEOC's efforts are presumably more
successful with such firms, we concentrate on this issue because it
presumably gives us some indication of the overall impact of government
efforts on changes in the relative position of black workers.

First, we find that the relative employment of black male workers
increased by 3.3 percent more over the period 1966 to 1970 in firms with
government contracts than in firms without government contracts, and
that this difference is statistically significant. The long-run effect
is estimated to be 12.9 percent. Second, we find that in the short run
the relative occupational position of black male workers increased by
.2 percent more in firms with government contracts than in firms without
contracts, but that this difference is not statistically significant.
In the long run, a .4 percent decrease is expected.

The most important factor affecting the change in the relative
position of black male workers in these data appears to be the migration
of black workers to the North Central region of the U.S. The employment
of black workers relative to white workers increased by approximately

20 percent more in the North Central region than in any other region of the country. This migration also resulted in a smaller rate of increase in the relative occupational position of black male workers in the North Central region than in any other region, and undoubtedly resulted because the migration of workers was from the lowest paying occupational categories. This presumably increased the growth in the relative occupational position of black male workers outside the North Central region and decreased this growth inside that region.

Finally, we have investigated the factors that affected the probability that a firm with no black male worker in 1966 would have hired at least one such worker by 1970. We find that the probability that a firm that was not "integrated" in this sense in 1966 was integrated in 1970 was nearly .1 percent higher in 1970 among firms with government contracts than in firms without them.

APPENDIX A

Using the notation of Section I.a, in the text,

$$i'(0_t - 0_{t-1}) = 0 = i' \Lambda \beta \bar{X} + i' \Lambda \Gamma d - i' \Lambda 0_{t-1} + i' \varepsilon_t$$

where i is a J x 1 vector with unit elements. Thus, for general $\bar{X}$, d, 0_{t-1},

$$i' \Lambda \beta = 0$$

$$i' \Lambda \Gamma = 0$$

and

$$i' \Lambda = 0.$$

This implies that knowledge of the coefficients of any J − 1 equations automatically gives the coefficients of the omitted equation. Note further that since

$$i' 0_{t-1} = 0$$

we may replace the regressors 0_{t-1} with 0_{t-1}^{Δ}, a J − 1 x 1 vector. For convenience suppose the final element of the vector, $(0_{t-1,J})$, is removed. The choice of the element to be removed is inessential. Letting i^{Δ} be a J − 1 x 1 vector of unit elements, $(i^{\Delta})' 0_{t-1}^{\Delta} = -0_{t-1,J}$.

Rewriting the original equation, we see that

$$(A-1) \quad 0_t - 0_{t-1} = \Lambda \beta \bar{X} + \Lambda \Gamma d - \Lambda \binom{0_{t-1}^{\Delta}}{-(i^{\Delta})' 0_{t-1}^{\Delta}} + \varepsilon_t.$$

Partition Λ into $[\tilde{\Lambda} \ \tilde{C}]$ where $\tilde{\Lambda}$ is a $J \times J-1$ matrix of coefficients and $\tilde{C}$ is a column vector. The new equation system is equivalent to

$$(A-2) \qquad 0_t - 0_{t-1} \ = \ \Lambda \ \beta \ \bar{X} + \Lambda \ \Gamma \ d - \tilde{\Lambda} \ 0^{\Delta}_{t-1} + \tilde{C} \ i' \ 0^{\Delta}_{t-1} + \varepsilon_t$$

$$= \ \Lambda \ \beta \ \bar{X} + \Lambda \ \Gamma \ d - [\tilde{\Lambda} - \tilde{C} \ i']0^{\Delta}_{t-1} + \varepsilon_t.$$

Note further that in long-run equilibrium ($0_t = 0_\infty$) the expected value of long-run values is

$$0_\infty \ = \ \beta \ \bar{X} + \Gamma \ d.$$

Since $i'0_\infty = 0$, it is clear that

$$(A-3) \qquad i'\beta \ = \ 0 \qquad \text{and} \qquad i'\Gamma \ = \ 0,$$

where is is a $J \times 1$ unity vector. This implies that we may use precisely the same reasoning as in the previous paragraph to write

$$\Lambda\beta \ = \ [\tilde{\Lambda} - \tilde{C}i']\beta^{\Delta}$$

$$\Lambda\Gamma \ = \ [\tilde{\Lambda} - \tilde{C}i']\Gamma^{\Delta}$$

where "β^{Δ}" is "β" with the last row removed, and "Γ^{Δ}" is "Γ" with its final element removed.

Drop the final equation and make the appropriate change in $[\tilde{\Lambda} - \tilde{C}i']$ (i.e., delete its final row) to reach

$$[\Lambda^\Delta - (C^\Delta)i^{\Delta'}]$$

where Λ^Δ is a J-1 x J-1 matrix, and C^Δ is the final column of Λ with its last element deleted, and i^Δ is a (J-1) x 1 vector of unity elements. The reduced equation system is

$$(A\text{-}4) \quad 0_t^\Delta - 0_{t-1}^\Delta = [\Lambda^\Delta - (C^\Delta)(i^\Delta)']\beta^\Delta \bar{X} + [\Lambda^\Delta - (C^\Delta)(i^\Delta)']\Gamma^\Delta$$

$$- [\Lambda^\Delta - C^\Delta(i^\Delta)']0_{t-1}^\Delta + \varepsilon_t^\Delta$$

where 0_t^Δ, 0_{t-1}^Δ, and ε_t^Δ are the original vectors with the final element removed.

In general, the matrix in brackets is nonsingular, and estimable. Accordingly, we can estimate β^Δ and Γ^Δ. Using equations (A-3), we estimate β and Γ. Note, however, that Λ is not estimable without further information. Note further that while the variance-covariance matrix of the original system is singular, in the reduced system this complication is avoided. Thus we have shown that knowledge of any J-1 short-run equations is sufficient to determine both short-run and long-run coefficients for the control variables and the government contract variable. Stability of the reduced system of equations implies stability of the original system. Since the characteristic roots of the adjustment matrix presented in Table 4 are all less than one in modulus, the complete system is stable.

APPENDIX B

The previous appendix shows that estimates of Λ, the adjustment matrix, are not needed to estimate long-run coeeficients. Nonetheless, if estimates are available, they provide an additional check on the model. Implausibly low rates of adjustment, or implausible orderings or rates of adjustment will cast doubt on our estimates. Specificity of training, and length of the training period should cause slower rates of adjustment in the more skilled occupations.

Without further information, it is impossible to estimate Λ. One restriction, overly strong, that will provide identification is the assumption of symmetry for Λ. A weaker restriction, consistent with symmetry, is the assumption that

$$(A-5) \quad \Lambda i = 0,$$

where i is a $J \times 1$ vector of ones, with J being the number of occupations. Since we know that the dependent variable in the J equations must be zero,

$$(A-6) \quad i'\Lambda = 0.$$

It is easy to verify that if Λ is symmetric, and the last equation applies, then the previous equation applies as well. It is also easy to verify that restriction (A-5) allows estimation of Λ from $[\tilde{\Lambda} - \tilde{C}i']$.

The empirical results are presented in Table A-1. Reading down the rows for a given column gives the coefficient on the lagged difference in proportion for the dependent variable designated in the row. Casual inspection of the table shows that the matrix is remarkably "close" to symmetry despite the fact that symmetry is not an implication of assumption (A-5). Letting λ_{ij} be the element of Λ in the ith row and jth column, one index of the departure of the matrix from full symmetry is

$$\sqrt{\frac{1}{N} \sum_{\substack{i < j \\ i \neq j}} \sum (\lambda_{ij} - \lambda_{ji})^2}$$

where N is the number of distinct off diagonal elements in the Λ matrix if Λ is symmetric. The value of the index is .0342 which indicates a small average deviation from symmetry.

Reading down the diagonal, the own adjustment coefficient tends to increase as the skill level decreases, suggesting that adjustments to final equilibrium are slower for more highly skilled occupations. This result is consistent with prior notions on the adjustment rates of fixed factors. Nonetheless, the implied rates of adjustment seem slow, especially when a four year time period separates the dependent variable and its lagged value. First order serial correlation in the disturbances of equations (15) will tend to depress the estimated rates of adjustment. Moreover, serial correlation will tend to impart symmetry in the estimated Λ matrix. Since we have only two points in time, we cannot test for serial correlation in a distributed lag model.

Table A-1. Estimated matrix of adjustment coefficients (estimated under assumption (A-5)).

Coefficient/Equation	Officials and Managers	Professionals	Technicians	Sales Workers	Office Clericals	Craftsmen	Operatives	Laborers	Service Workers
Officials & Managers	.3848	-.0710	-.0681	-.0531	.0232	-.0187	-.0782	-.1178	.0027
Professionals	-.0895	.4623	-.0953	-.0333	.0099	-.0384	-.0872	-.1175	-.0071
Technicians	-.0503	-.1057	.6511	-.0562	-.1200	-.1043	-.0539	-.0994	-.0576
Sales Workers	-.0666	-.0355	-.0791	.3763	-.0642	-.0196	-.0160	-.0308	-.0604
Office Clericals	-.0522	-.0463	-.1023	-.0600	.5034	-.0735	-.0586	-.0538	-.0530
Craftsmen	-.0314	-.0439	-.0941	-.0305	-.0983	.5000	-.0862	-.0135	-.0982
Operatives	-.0321	-.0536	-.0727	-.0296	-.0836	-.1183	.6027	-.1084	-.0982
Laborers	-.0409	-.0582	-.0776	-.0657	-.0743	-.0579	-.1683	.6206	-.1102
Service Workers	-.02180	-.0481	-.0612	-.0478	-.0964	-.0690	-.0543	-.0794	+.4818

Nonetheless, we conjecture that a lag model is appropriate, although serial correlation may be present as well. It would be an unlikely coincidence to find that the ordering of own adjustment coefficients is so closely in accord with theory if serial correlation were the only factor leading to significant coefficients on the lagged variables.

APPENDIX C

In the text, we present empirical results on the effect of contractor status on the racial integration of firms. The analysis is performed using the linear probability model which is known to suffer from important defects.[9]

Accordingly, it is of interest to compare results from this conceptually defective but computationally effective method with results from a more attractive statistical model. The logistic model is known to possess a likelihood function which possesses sufficient statistics. Thus, it is possible to compute efficient and consistent estimates of the probability of integration.

Since the logistic technique is computationally more expensive, we randomly selected a 10 percent sample of the firms used to compute the results from the linear probability model. Those results are reported in Table A-2. The rows in this table correspond to the columns of Table 7 in the text.

For our purposes, the most appropriate comparison is between the coefficient on the variable "government contract" in the linear probability model, presented on line 4 within each group, and the implied probability from the logistic model, presented on line 3. Both results are computed from the identical subsample of 1420 firms. The estimates from the two models are quite close, indicating that the results reported in the text may be taken as good approximations to a more theoretically desirable econometric model.

Table A-2. A comparison of the logistic results with the linear probability model for a subsample of 1,420 firms selected randomly from the tape.[*]

Group	Intercept	Natural Log of Employment Increase	Natural Log of Employment Decrease	Size of Firm 100-500	Size of Firm 500-750	Size of Firm 750+	Region North	Region Central	Region West	Government Contract	Inside SMSA?	Total Employment
All	-2.78	.221	.0982	1.036	1.447	1.554	-.165	.677	-.179	.662	1.01	-.00055
	(-10)	(3.7)	(1.4)	(6.1)	(3.5)	(3.1)	(-.9)	(3.3)	(-.8)	(4.6)	(6.1)	(-.0313)
	-.641	.051	-.022	.238	.333	.358	-.038	.156	-.0413	.152	.234	-.128x10^-5
	-.0623	.0458	.0184	.207	.298	.314	-.0314	.136	-.0314	(.135)	.195	-4.75x10^-7
		(3.93)	(1.39)	(6.28)	(3.62)	(3.12)	(-.859)	(3.34)	(-.790)	(4.69)	(6.26)	(-.0127)
Officials Managers	-8.55	1.046	.789	.240	-.3747	.298	.0366	.4872	-1.356	.5849	.71	-.289x10^-3
	(-7.2)	(4.02)	(2.88)	(.3245)	(-.323)	(.255)	(.0661)	(.833)	(-1.653)	(1.27)	(1.36)	(-1.056)
	-.0595	.0073	.0054	.00167	-.00261	.0021	.00025	.00339	.0094	.0041	.00495	-.2x10^-5
	-.0433	.0175	-.00992	.00269	.00200	.0825	(-1.34x10^-4)	.0126	.021	.0150	.0145	4.80x10^-6
		(4.58)	(2.29)	(.248)	(.0745)	(2.5)	(-1.11)	(.941)	(-1.60)	(1.58)	(1.42)	(.393)
Professionals	7.137	.3898	-.246	.763	.9986	2.096	.0665	.8532	-.0093	1.276	1.295	-.217x10^-3
	(-8.29)	(2.29)	(1.17)	(1.52)	(1.14)	(2.23)	(.134)	(1.69)	(-.01798)	(3.15)	(2.59)	(-.499)
	-.122	.00668	.0042	(.0131)	.0171	.0359	.00114	.0146	-.00015	.0219	.02219	-.372x10^-5
	-.0595	.0131	.0058	.0209	.0451	.161	.00206	.0273	.00138	.0405	.0346	-1.82x10^-5
		(2.82)	(1.10)	(1.59)	(1.37)	(4.02)	(.141)	(1.67)	(.0872)	(3.51)	(2.78)	(-1.22)
Technical Workers	-5.16	.142	-.021	.787	1.54	-2.49	-2.07	-.074	1.635	.6039	.936	-.124x10^-3
	(-7.98)	(1.03)	(-.114)	(1.78)	(2.07)	(3.13)	(-.47)	(-.145)	(.388)	(1.81)	(2.25)	(.368)
	-.147	.004	-.00059	.0225	.0440	.0713	-.0059	-.0021	.0046	.0172	.0267	-.35x10^-5
	.0265	.00748	1.91x10^-4	.0251	.0801	.199	-.00695	-.00234	.0101	.0240	.0336	-1.75x10^-5
		(1.46)	(.0331)	(1.74)	(2.22)	(4.51)	(-4.33)	(-.130)	(.582)	(1.90)	(2.46)	(-1.07)
Sales Workers	-5.54	.281	.207	.1089	.1614	-.71	.101	.333	.474	-.1936	1.41	.55x10^-4
	(-7.14)	(1.76)	(1.14)	(.252)	(.177)	(-.52)	(.203)	(.61)	(.99)	(-.53)	(2.53)	(.149)
	-.135	.0068	.0050	.0027	.0039	-.0173	.00247	.0081	.01156	-.00472	.0843	.00136
	-.0181	.00841	.00581	.00431	.0116	-.0188	.00178	.00812	.0170	-.00592	.0332	7.04x10^-7
		(1.83)	(1.12)	(.332)	(.359)	(-.476)	(.123)	(.505)	(1.09)	(-.521)	(2.71)	(.0481)
Clerical	-6.22	.347	-.338	.505	1.336	1.40	.667	.705	.250	.276	1.877	-.213x10^-3
	(-9.77)	(2.94)	(2.54)	(1.51)	(2.18)	(1.79)	(1.91)	(1.74)	(.644)	(1.038)	(4.34)	(-5.19)
	-.2496	.0139	.0136	.0202	.0536	.0561	.0267	.0283	.0100	.0111	.0753	-.85x10^-5
	-.0899	.0196	(.0163)	.0278	.124	.137	.0379	.0318	(.0122)	.0176	.0817	-2.25x10^-5
		(3.11)	(2.29)	(1.56)	(2.79)	(2.52)	(1.91)	(1.43)	(.566)	(1.12)	(4.85)	(-1.11)
Craftsmen	-4.91	.2712	.6112	.0539	.9077	1.715	-.582	.511	-.044	.914	.9009	-.24x10^-3
	(9.56)	(2.43)	(.81)	(1.634)	(1.51)	(2.62)	(-1.57)	(1.5)	(-.129)	(3.47)	(2.83)	(-.11)
	-.237	.013	.0054	.026	.043	.082	-.028	.0247	-.0021	.044	.0435	-.11x10^-5
	-.0564	.0190	-.00604	.0306	.0765	.208	-.0297	.0349	9.37x10^-4	.0595	.0512	-8.76x10^-6
		(2.91)	(.81)	(1.65)	(1.65)	(3.69)	(-1.44)	(1.52)	(.0419)	(3.66)	(2.92)	(-.419)
Operatives	-.321	.1846	.122	.74	1.47	1.456	-.36	.329	-.375	.6212	.2764	.37x10^-4
	(9.71)	(2.43)	(1.38)	(3.21)	(3.46)	(2.84)	(-1.49)	(1.34)	(-1.43)	(3.39)	(1.36)	(.217)
	-.37	.0215	.0142	.086	.17	.1695	-.042	.0383	-.0436	.072	.032	.43x10^-5
	-.0100	.0240	.0135	.0781	.233	.235	-.0406	.0438	.0427	.0778	.0312	1.07x10^-5
		(2.63)	(1.31)	(3.03)	(3.63)	(2.99)	(-1.41)	(1.37)	(-1.37)	(3.44)	(1.28)	(.37)
Laborers	-3.7	.29	.1346	.5848	1.18	.838	-.569	.103	-.229	.676	-.0391	.808x10^-4
	(-9.157)	(3.02)	(1.17)	(2.00)	(2.41)	(1.39)	-(1.86)	(.347)	(-.76)	(3.03)	(-.1671)	(.459)
	-.266	.0209	.0097	.0422	.0854	.0604	-.041	.0075	-.0165	.0488	-.0028	.58x10^-5
	-.00886	.0254	.00960	.0373	.146	.098	-.0421	.0119	-.0191	.0579	-.00224	2.68x10^-5
		(3.37)	(1.12)	(1.75)	(2.75)	(1.50)	(-1.78)	(.45)	(-.741)	(3.09)	(-.11)	(1.1)
Service Workers	-4.69	.2256	.085	1.154	1.615	1.61	-.437	1.01	-.421	-.79	1.06	.1295x10^-4
	(-8.81)	(2.02)	(.605)	(3.14)	(2.54)	(2.17)	(1.16)	(3.08)	(-1.07)	(-2.74)	(3.37)	(.06)
	-.1985	.0095	.0036	.0488	.0633	.0676	-.985	.0429	-.0178	-.0334	.045	.54x10^-6
	-.0237	.0151	.00601	.0499	.106	.0987	-.0232	.0671	-.0222	-.0415	.0548	-2.02x10^-7
		(2.39)	(.837)	(2.77)	(2.38)	(1.79)	(-1.16)	(3.00)	(-1.02)	(-2.63)	(3.22)	(-9.9x10^-3)

[*]The first line in each group records the logit coefficient, the second is the associated normal statistic, the third line records the partial derivative of the probability of integration with respect to the associated variable. This is to be compared with the coefficient of the linear probability model recorded in the fourth line. The fifth line shows the t statistic for the linear probability model.

FOOTNOTES

1. The material in the introduction draws heavily from Chapter 4 of R. P. Nathan's excellent Jobs and Civil Rights, prepared for the U.S. Commission on Civil Rights. Clearinghouse Publication No. 16, April, 1969.

2. It has been estimated that in March 1967 there were 228 fulltime professional contract compliance personnel in all government agencies except the OFCC to investigate 225,000 contractor facilities. (R. Nathan, op. cit., p. 113.)

3. Readers familiar with the work of H. G. Lewis reported in Unionism and Relative Wages, Chicago, 1963, will recognize the similarity in Lewis' methodology and our own.

4. See M. Nadiri and S. Rosen, Interrelated Factor Demand, National Bureau of Economic Research, New York, 1974.

5. A discussion of this point is given in Appendix A. With a fairly mild assumption, it is possible to estimate Λ as well. This provides a further check on the model. These results are reported in Appendix B.

6. This would also be a desirable project to undertake for the entire universe of firms, since it has been estimated that perhaps only three-fourths of the firms obligated to submit an EEO-1 form actually do so. A periodic audit of a random sample of firms whose EEO-1 forms have not been tabulated would produce estimates of the causes for non-response in the overall universe of obligated firms and might have the additional benefit of producing a higher response rate in the longer run.

7. We are thus treating the results in Table 3 as a linear probability function. There are well known statistical problems that arise from using this procedure. As is also well known, the same computational results may be interpreted as a linear discriminant function. Under this interpretation a simple transformation of the coefficients in Table 3 would make them the weights in a function that would allow us to predict which of two populations an item had come from, the matched or unmatched groups. In some ways the linear discriminant function would be a better way to interpret the computations in Table 3 for the problem we are examining here.

8. This information appears in a letter dated December 5, 1973, from George Travers, Associate Director for Plans, Policies and Programs, Employment Standards Administration, Office of Federal Contract Compliance, U.S. Department of Labor, to Orley Ashenfelter. Travers estimates that in 1971, 28 percent of government contractors were listed as noncontractors. Since similar data procedures were employed in 1966 and 1970, as in 1971, it is likely that this estimate applies to our data as well.

9. See, e.g., A. S. Goldberger, Econometric Theory, Wiley, 1964.