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THE FEDERAL ATTACK ON LABOR MARKET DISCRIMINATION: THE MOUSE THAT ROARED?

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ABSTRACT

The purpose of this paper is to review available evidence on the impact of federal equal employment opportunity programs. While Title VII of the Civil Rights Act of 1964 and Executive Order 11246 have been in effect for over 15 years, the lag in data collection and evaluation means that little can be said regarding the last few years' experience. In particular, evidence on the impact of recent administrative changes in the agencies responsible for enforcement is unavailable.

In general, time series studies find significant improvements in the relative labor market position of blacks compared with whites since 1965. While several arguments have been advanced that these gains are illusory, the most plausible interpretation is that much of the apparent progress is real.

Cross-sectional studies of the impacts of the Office of Federal Contract Compliance Programs (which enforces the nondiscrimination and affirmative action requirements of the Executive Order) and the Equal Employment Opportunity Commission (which enforces Title VII) have been much less conclusive. Half of the major studies of the OFCCP find that the program had the intended effects on the relative position of blacks -- or at least black males. Unfortunately, variations in conclusions among studies are not readily explained, even after a careful look at the competing data and methods. Equally disturbing is the inability of studies producing positive results to associate such impacts with the "levers" by which OFCCP might exert influence. Studies of EEOC impacts are more vulnerable to problems of identifying the appropriate control group, since Title VII covers contractor and noncontractor firms. Apart from evidence that relative black employment grew considerably faster in firms which must report to EEOC (firms with over 15 employees are subject to Title VII, but only those with 100 or more must report to EEOC), available studies have not produced consistent evidence of EEOC impact.

Besides the lack of strong cross-sectional support for the time series conclusions, three puzzles emerge: (1) What caused the decline in black male labor force participation which began about the same time as the federal antidiscrimination effort? (2) Why did black females advance more rapidly than black males since the federal effort began? (3) Why did advantaged blacks advance more rapidly than less advantaged blacks?

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While limited efforts were made earlier by state fair-employment commissions and federal executive orders, the major components of government efforts to reduce labor market discrimination were created fifteen years ago. Title VII of the Civil Rights Act of 1964 outlawed discrimination based on race, sex, or national origin by private employers[1] and created the Equal Employment Opportunity Commission (EEOC) to investigate complaints. In 1972, the non-discrimination provisions were extended to government employment, and the EEOC was given the power to initiate suits on behalf of those injured by employment discrimination. Executive Order 11246, issued in 1965, required that federal contractors refrain from discrimination. The Office of Federal Contract Compliance (OFCC, later Office of Federal Contract Compliance Programs) was created to oversee the compliance activities of federal agencies. Discrimination on the basis of sex was not a part of the contract-compliance program until 1967, and affirmative action to eliminate sex discrimination was not required until 1971.

The definition of discrimination, the boundary between "non-discrimination" and "affirmative action", and the limits of permissible affirmative action have been sources of substantial public controversy. Apart from occasional accounts of organization difficulties, the impact of EEOC and OFCC on discriminatory employment practices has received less public discussion.

The impact of the agencies charged with carrying out the federal equal opportunity effort has, however, received a good deal of attention from economists. Indeed, the literature has grown large enough to be the subject of several reviews, usually in the form of extended prologues to the authors' own work.

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[1] Unions and employment agencies were also covered. Firms with fewer than 100 employees (later reduced to 25, and then 15) were not covered.

This paper is not intended as a review of the reviews. It attempts to provide a more detailed view of the literature than previous efforts, in order to resolve or at least clarify some of the controversies. Where these controversies remain unresolved and only slightly clarified, potential resolutions are suggested. Finally, puzzles which are suggested by the literature, but have received little direct attention, are noted.

Postwar and post-1964 trends in relative earnings are discussed in section 1. The records of EEOC and OFCC, viewed from an administrative or procedural perspective, are considered in section 2. Time series evidence on the effectiveness of the federal EEO effort is reviewed in section 3. Sections 4 and 5 are devoted to cross-section studies of OFCC and EEOC, respectively. Concluding observations are presented in section 6.

1. Postwar Trends in Black/White and Male/Female Earnings Ratios

Median earnings of wage and salary workers have been published annually by race (white-nonwhite[2]) and sex since 1947 by the Current Population Survey (CPS). Accepting this widely studied series as a measure of labor market status, the simplest way of summarizing the postwar and post-1964 trends in relative position is to fit the equation

$$(1) \quad \ln (E_{1,t}/E_{2,t}) = b_0 + b_1 t + b_2 t^* + e_t$$

where $E_{i,t}$ equals median earnings of race/sex group i in year t and t^* equals max(t-1964,0). Thus b_1 measures the postwar trend and b_2 captures any acceleration in the post-1964 period.

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[2]Because the vast majority of nonwhites are black, the two terms will be used interchangeably.

Ordinary least squares estimates of b_1 and b_2 are presented in Table 1. Relative to whites of the same sex, both black males and black females have experienced increasing earnings, especially in the post-1964 period. By 1977, black male earnings were still considerably below those of white males, while black and white females has essentially equal earnings. The white female/white male comparison tells a different story: the positive post-1964 effect roguhly offsets the negative postwar trend, and females' earnings are only one half of male earnings.[3]

While the black female-white female comparison in line 2 is the most-often encountered approach to measuring the relative earnings gains of black females, it has been criticized as implicitly accepting white female earnings as the "appropriate" base toward which black female earnings might be expected to converge (Anderson and Wallace, 1975, p. 50). Line 4 presents the alternative black female/white male comparison. As can be inferred from lines 2 and 3, this comparison shows a smaller postwar trend, a larger post-1964 effect, and much lower 1977 relative earnings for black females.

Clearly, Table 1 can be read to emphasize either the considerable improvement in the relative position of black males and females, or the substantial gap in earnings between white males and other groups. Both emphases are relevant for analyzing the impact of antidiscrimination programs.

The evidence of post-1964 progress for blacks in Table 1 has been challenged from a number of perspectives. Perhaps the most frequently cited contrary evidence is the behavior of black/white family income ratios, which

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[3]Female <u>hourly</u> wages were only 67% of male wages in 1978 (unpublished CPS tabulations).

Table 1. Trends in Relative Median Wage and Salary Earnings

Earnings Ratio	Constant	Time Trend (1947=1)	Post-1964 Time Trend	R^2	1977 Ratio
BM/WM	-.567*	.002	.017*	.80	.71
	(.021)	(.002)	(.004)		
BF/WF	-1.022*	.026*	.025*	.97	1.01
	(.030)	(.002)	(.005)		
WF/WM	-.530*	-.012*	.009*	.92	.47
	(.011)	(.001)	(.002)		
BF/WM	-1.551*	.014*	.034*	.93	.47
	(.034)	(.003)	(.006)		

Sample Period: 1947-77.

Notes:

Standard errors in parentheses below coefficients.

*Significant at the .05 level (t>1.96)

how smaller, less regular gains, especially in the 1970's (Brimmer, 1976, p. 5; Coleman, 1977, p. 6). In part, this is due to the slower growth of relative individual incomes than relative individual earnings (Smith and Welch, 1979, p. 5). Moreover, the increases in single-parent families among blacks and in two-earner families among whites have both lowered family income ratios in the 1970's (Munnell, 1978, pp. 13-14). While relative family incomes are a more appropriate focus in many contexts, they are clearly less appropriate than relative individual earnings for studying relative labor market position.

A more subtle criticism of the evidence of improving black/white earnings ratios is Lazear's (1979a) challenge to the use of wages (or earnings) to measure the returns from working. In addition to earnings, workers' investments in human capital are part of the compensation from working, and differences in such investments would show up in later earnings differentials. Lazear estimates the value of this investment per hour worked from wage growth, and this is added to the observed wage to obtain an estimate of the true wage (i.e., true wage ≡ observed wage + value of human capital acquired). He finds a large increase in the black-white (male) disparity in the human capital component (from $.55 to $2.70 per hour) between 1966 and 1974 (Lazear 1979a) but a large reduction (virtually and elimination) of the (white) male-female human capital disparity (Lazear, 1979b). This would suggest that trends in observed wage ratios greatly overstate black male progress and greatly understate while female progress.

Lazear's theoretical point -- that one would want to investigate the consequences of present jobs for future wages, as well as current wages, in evaluating the relative position of minorities and women -- is an important

one. Moreover, even if one doubts that the impact of experience on earnings is due to "skill" accumulation, Lazer's point remains. However, his evidence that the narrowing of black/white wage differentials is "illusory" is not convincing. Statistically, one could not reject the hypothesis that the difference in human capital investments (between black and white males, or white males and females) had remained constant at any reasonable level of significance.[4]

Even granting Lazear's contention that, when correctly measured, relative wages of black men have not increased -- because the relative impact of current experience on future wages is declining -- it does not follow that the improvement in their relative labor market position is "illusory." The reason is that what is relevant in Lazear's wage re-calculation is "that wage growth which occurs as a result of job experience per se ... Residual wage growth that occurs as one 'ages' even in the absence of job experience, is not part of work compensation and as such should not be counted ..." However, in judging the relative labor market position of blacks, the "residual" wage growth may be quite important, especially if this residual growth is changing to blacks' relative advantage -- which is what one observes in Lazear's equations.[5] Consequently, further evidence on the behavior of relative black earnings as individual cohorts age would be important.

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[4]The black-white difference is $.55 (st. error = .67) in 1968 and $2.70 (2.28) in 1974, so the change in this difference corrected for inflation is .55 - (.67) 2.70 = -1.26 with a standard error of 1.41. For white males and females, the analogous figure is 1.98, with a standard error of 1.45.

[5]Blacks' "passage of time" effect was 2.8 percent per year larger than whites' in his 1966-69 equation, compared with 7.6 percent per year in 1972-74.

Available evidence does not suggest an erosion of black gains as cohorts age. Smith and Welch (1979, p. 52) present weekly wage ratios (predicted from equations utilizing 1967-74 CPS data) which allow one to follow various male cohorts, defined according to years of schooling and 1967 experience,[6] over the 1967-74 period. Relative black earnings rise in all experience -- education cohorts (except the schooling = 8 years group) from 1967-72, but are lower for all but the schooling = 16 years group in 1974 than in 1967 (presumably, a reflection of cyclical effects). Smith and Welch conclude that black relative earnings do not seem to fall as individual cohorts grow older (p. 53). Raisian and Donovan (1980) report that, among male household heads, relative wages increased for those with up to five years' experience, and remained roughly constant for others, after controlling for cyclical factors.

A third challenge to the evidence of improving relative black earnings is Butler and Heckman's (1977) observation that published median earnings rise when low earners withdraw from the labor market, because published medians (and means) relate to those with earnings. Regressing black-white worker/population ratios in each year on the postwar and post-1964 time trends reveals a declining post-1964 trend for males (due to black male declines) and females (due to white female increases and black female declines). If the "marginal" participants in each group are low earners, one would expect a rise in relative median earnings ratios for both males and females, even if the distribution of offered wages or earnings in each group were unchanged. Attempts to assess the magnitude of this effect, by "replacing"

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[6]The groups were: schooling = 16, 12, and 8 years; 1967 experience = 1, 5, 10, 15 years.

labor market dropouts in the lower tail of the distribution and estimating "corrected" median suggest that the Butler-Heckman argument could explain part of the post-1964 trend (Levy, 1981).

A very different "correction" suggested by Darity and Myers (1980) leads to very different conclusions. They calculated average earnings in the conventional way (i.e., for those with earnings), and then by dividing total earnings by total labor-force age populations. Over the 1968-78 period, ratios of conventional means show substantial upward trends for both males and females, but the corrected means show no trend for males and a greatly reduced trend for females. This approach would be appropriate if it were known that declining black participation was due to inability to find work at prevailing wages (see Vroman, 1975, p. 297); the zeros which they assigns to nonearners would then reflect the "offered" wage. While much too little is known about the decline in relative black participation, inability to find work does not appear to be the major reason for not participating in the labor market at all during the year.[7] Inability to find work undoubtedly contributes significantly to part-year employment, but part-year employment is reflected in the official, uncorrected series.

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[7] 10.3 percent of nonwhite men who did not work in 1978 looked for work during that year, and only 5.9 percent reported inability to find work was the major reason for not working. For nonwhite females, the corresponding proportions were 9.5 and 6.2 percent.

2. Procedural Analyses

If the post-1964 improvements in relative black earnings suggest that federal equal opportunity programs were effective, analyses of the scale and effectiveness of the agencies charged with enforcing federal regulations seems to point in the opposite direction. Based on "procedural" analyses (which adopt an administrative perspective) Butler and Heckman (1977, p. 247) conclude that "first-hand observations suggest that the EEOC is unlikely to have had a major impact" and that similar observations "foster suspicion of [OFCC's] contribution to eliminating measured black/white wage differentials."

Both EEOC and OFCC were hampered by lack of personnel, and by questionable allocation of available resources. In fiscal year 1973, for example, EEOC received charges of discrimination from nearly 50,000 individuals, and had an authorized professional staff of 1293. Actual staff levels were considerably lower (U.S. Civil Rights Commission, 1975, pp. 496, 499, and 512). These professionals must investigate complaints and attempt conciliation when there is reasonable cause to believe the charge is true, before the litigation stage is reached. Not surprisingly the "backlog" of charges grew steadily, reaching 130,000 in 1977 (U.S. Civil Rights Commission, 1977, p. 211). Meanwhile, OFCC faced similar resource limitations -- less than one contractor in five was the subject of a compliance review in the three-year period 1970-1972 (Goldstein and Smith, 1976, p. 524).

The allocation of the resources of both agencies has been subjected to frequent criticism. For EEOC, the recurring question has involved the relative emphasis given to large- vs. small-payoff complaints. In general, EEOC has been criticized for devoting too many resources to individual complaints which are unlikely to have large impacts, and too few resources to

attacking "systemmic" discrimination (e.g., Bergmann, 1976, p. 133). Meanwhile, OFCC's activities received vastly differing levels of support in different federal agencies, so that (unless gains per compliance review differed greatly across agencies) the total of compliance personnel were allocated inefficiently.[8] Moreover, review-per-month standards sometimes encouraged compliance personnel to focus on small contractors, which required less time per review, in order to meet the standard (Ahart, 1976, p. 570).

The quality of EEOC and OFCC enforcement activities -- e.g., investigations of complaints and conciliation efforts, and the adequacy of negotiated settlements by EEOC, and the adequacy of pre-award compliance reviews and approved affirmative action plans by OFCC -- have also been challenged (U.S. Civil Rights Commission, 1975; Wolkinson, 1973, pp. 59-97; U.S. Comptroller General, 1976; Ahart, 1976).

The analysis of the penalties for those found not in compliance is more complicated. EEOC does not have the power to fine a firm which discriminates, or issue a legally binding order that it cease discriminatory policies. Indeed, before 1972 it could not file lawsuits to enforce Title VII, though it could refer cases to the Justice Department or file briefs as "friend of the court." From 1972 to 1976, it filed roughly 15 suits per month (U.S. Civil Rights Commission, 1977, p. 200), a relatively small number compared with the number of charges which are not successfuly resolved by conciliation (U.S. Comptroller General, 1976, pp. 30-31). Those who believe they have been discriminated

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[9] In fiscal year 1973, NASA, EPA, and the Department of Commerce reviewed at least half of their contractors, while the Departments of Treasury and Agriculture reviewed only 2% of their contractors (U.S. Civil Rights Commission, 1975, p. 292).

against may file suit on their own behalf, sometimes with the assistance of public advocacy groups, if EEOC chooses not to do so.

OFCC's ultimate enforcement power is "debarment" -- excluding the firm from government contract eligibility. However, this power was very rarely used. No contractors were debarred from 1965 to 1970, and an average of about two per year was maintained in 1971-77 (U.S. Civil Rights Commission, 1975, p. 298 and 1977, p. 139).

Consequently, the penalties for noncomplicance consist primarily of back pay awards and other remedial actions under pre-litigation agreements and "consent decrees" negotiated by EEOC and OFCC with the firms in question, and some court-awarded payments. Surprisingly, there appears to be no published tabulation of back pay wards and workers affected. A very rough guess for 1977 would be that EEOC litigation led to settlements of perhaps $65 million in back pay and other immediate relief,[9] plus an unspecified gain from future consequences of these settlements, which is probably considerable.

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[9]This rough guess is based on these statistics (from U.S. Civil Rights Commission, 1977, pp. 204-209); (1) In 1977, "section 706" litigation produced $13 million in back pay and other specific relief; (2) "section 707" litigation has produced $4.8 million in 8 settlements with 45 other cases pending settlement negotiations, but these totals do not relate to 1977 alone, (3) litigation of individual complaints had produced more than $100 million through January 1, 1977, and 70% of these settlements had been achieved in the preceeding 18 months.

Whether such sums -- or the more widely publicized settlements with AT&T and major steel companies -- have a substantial deterent effect is hard to assess. Often, the sums involved are simultaneously "large" and "small". The steel settlement, for example, provided for $31 million in back pay. However, the maximum payment was $1000 per worker (U.S. Civil Rights Commission, 1975, pp. 556-7). Thus, a full-time worker adversely affected by company policies for the entire 9-year period (1965 to settlement in 1974) would receive less than $120 per year, or less than 6¢ per hour (before discounting). This is less than any plausible estimate of the harm suffered.[10] Back pay awards which seem "large" in total dollars may be quite small when compared with the number of workers and worker-hours involved.

On the other hand, the firm's gain from discriminating (it any) may[11] be considerable less than the wages lost by the victims of discrimination. Thus, even back pay awards which inadequately compensate victims may appreciably

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[10] Ichniowski reports that, in 1973, the seniority-adjusted minority/non-minority earnings ratio was 0.92. If we take this as a rough indication of discrimination, weekly wage loss due to discrimination would be 18.40 per week (8 percentof average weekly earnings of $230) or over $950 per year. An alternative perspective is that the $660 average payment would reflect only 8 months discriminatory wage loss.

[11] The <u>firm's</u> gain from discriminating is likely to take the form of "saving" the resources necessary to monitor and enforce nondiscriminatory behavior by its employees and the wage premia which might be required to secure cooperative white workers. This "gain" must be reduced by any increases in labor costs due to its discriminating.

alter the firm's incentives for non-discriminatory behavior. An additional consideration is that potential back-pay awards cumulate, and EEOC's "batting average" in court in cases it does choose to litigate is impressive[12] -- so that a firm which "stonewalls" a charge of discriminatory hiring and promotion does so at some risk. Finally, as King and Marshall (1974; p. 470) have argued, the Civil Rights Act may "[make] it possible for employers who have economic motives for doing so to hire blacks and shift the blame for doing so to "legal requirements," whether or not there is a serious threat of losses from prosecution under the law.

Thus, a review of the procedural analyses leave one with the clear impression that the federal enforcement effort could have been larger and more efficient. But the conclusion that the effort was so inept that it could not have had an appreciable effect on earnings ratios seems to strong. This seems particularly true of EEOC, where the fear that "sooner or later, we're gonna get you" would tend to counteract the low probability of enforcement action against the firm in any one year.

Nothing has been said thus far about the more recent reorganization of the federal antidiscrimination effort. EEOC has adopted an expedited procedure for dealing with individuals complaints, in order to reduce the complaint backlog and to devote more attention to systemic discrimination. The contract

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[12]For early successes in establishing legal precedents, see Adams, 1972, pp. 24-31. More recent evidence of litigation showed that "favorable settlements" out numbered cases "dismissed .. no appeal" by a 3-to-1 ratio (U.S. Comptroller General, 1976, p. 31).

compliance program has also been reorganized, with more centralized authority. Early indications are that these efforts have been successful, The EEOC charge backlog has been greatly reduced,[13] and allocation of compliance resources has been significantly improved. Moreover, back pay awards per worker seem to have grown considerably.

The neglect of these activities does not reflect a judgement that they are unimportant, or predestined to fail. Rather, they are too recent to show up in the trends discussed in section 1, or in the studies discussed in the next three sections. In evaluating these studies, the difficulties of the enforcing agencies must be kept in mind.

3 Time Series Analyses

Time-series analyses of the impact of federal EEO activities seek to determine whether the post-1964 improvement in relative black earnings can be explained by other factors (which can be measured with annual data over the postwar period). Implicitly, the "survival" of the post-1964 trend as additional control variables are added is taken as favoring the causal role of EEO activities in improving relative earnings.

The most influential time-series studies were those of Freeman (1973; 1977, Chapter 5) and Vroman (1974, 1975). Using 1947-71 data, Freeman controlled for business-cycle influences by including the proportional deviation

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[13] By early 1981, the back log had been reduced to 32,000 cases, and the resolution period for new cases was under four months (Washington Post, February 20, 1981).

of real GNP from its trend as an independent variable. The statistically significant post-1964 trends were one and two percentage points (i.e., two and four per cent), per year for males and females, respectively.[14] Adding a measure of relative education -- to control for the improvement in black educational attainment which was seen by others as a major contributor to improving relative earnings[15] -- left the estimated post-1964 trends almost unaffected.[16]

An updated version of Freeman's equations appears in Table 2. Despite the considerable change in sample period (1950-1977), the post-1964 trend estimates are quite close to Freeman's estimates.

Freeman also tested the sensitivity of his results to alternative measures of relative labor-market position. Replacing relative median wage and salary earnings with relative mean income or relative occupational position, or restricing the sample to year-around full-time workers produced similar, though smaller, estimates.

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[14]Freeman's actual equations used cumulated real per capita EEOC expenditures rather than a simple post-1964 trend. The estimates in the text are based on Freeman (1977, p. 130).

[15]Freeman agrees that improving education has contributed to rising black earnings, but places greater emphasis on education as a response to improved opportunities.

[16]These trends are statistically significant. A typographical error created the erroneous impression that the male post-1964 coefficient was less than its standard error.

Table 2. Relative Median Wage and Salary Earnings Equations

Earnings Ratio	BM/WM	BF/WF
Constant	-.205 (.160)	- 0.497 (.324)
Time Trend (1950=1)	-.011* (.005)	.004 (.016)
Post-1964 Time Trend	.017* (.004)	.037* (.010)
Deviation of ln (GNP) from Trend	.060 (.249)	.677 (.360)
ln (Relative Median Education).	.689* (.345)	.990 (.686)
R^2	.90	.98

Notes:

See Table 1.

In a subsequent paper, Freeman (1978b, pp. 39-41) argued that attributing these gains to federal equal-opportunity pressures is reasonable in light of evidence that firms have changed their personnel practices in response to such pressures. Citing a Bureau of National Affairs (1976) study, he notes that 86 percent of surveyed firms have formal EEO programs, that 60 percent had changed their selection procedures for EEO reasons, and one third include EEO achievements in managers' performance appraisals. (There is a half full, half empty -- or more accurately, a two thirds empty -- issue here; two thirds of the firms did not provide this incentive.)

Vroman (1974) reported similar results -- post-1964 trends of 1.8 and 3.7 percentage points for males and females, respectively -- from his analysis of CPS earnings ratios which included postwar trend and cyclical variables as controls. Analysis of Social Security earnings data produced similar estimates for females, but (statistically insignificant) estimates of 0.5 percentage points for males. No explanation for this difference was suggested. When changing age distributions were held constant (by using a fixed-age-weighted dependent variable, rather than adding relative age as a right-hand side variable) the male post-1964 trend rose to 1.2 percentage points. This suggests that not controlling for changing age distributions may have biased his (and Freeman's) estimates of post-1965 male trend from CPS data.

Vroman reported results of several further experiments. First, he used the Social Security data to estimate separate equations for Northern and Southern regions. Post-1964 gains in relative earnings were larger in the South for females, and nearly absent outside the South for males (Vroman,

1974, pp. 183-186).[17] Second, Vroman (1975, pp. 298-300) introduced (mean) education and a simple industrial composition variable (instead of postwar trend and cycle) as controls. Estimates of post-1964 trend[18] were essentially the same as those in the earlier paper. Third, Vroman noted falling employment-and labor force-population ratios for black males,[19] and raised the sample truncation problem emphasized by Butler-Heckman. However, this appeared base on then-available data to be a post-war rather than post-1964 phenomenon (Vroman, 1975, pp. 297-298). Fourth, Ginsberg and Vroman (1976) used pooled time series - cross section Social Security earnings data to estimate models with a richer set of controls (education, industry mix, region, and black share of employment). They found relative black earnings had increased approximately 1.3 percent (or about .7 percentage points) per year[20] in the post-1964 period. While larger gains were found in the South, statistically significant gains outside the South of about 1 percent per year were reported.

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[17]Vroman's finding of a post-1965 acceleration for males in the South contradicts Knapp and Marshall's (1974, pp. 464-465) finding of no acceleration, based on 1959-65 and 1965-69 comparisons for craftsmen in husband-wife families.

[18]For males; female equations were not reported.

[19]These ratios fell for both black and white males, but fell more rapidly for blacks.

[20]The percentage point figure is based on a 1964 earnings ratio of .528, taken from Vroman, 1974, p. 186. Strictly speaking, this value applies to the whole U.S. rather than the Gingberg-Vroman sample.

Freeman's (and, with more qualifications, Vroman's) finding of accelerated post-1964 progress in relative earnings has been challenged in three different ways. First, the post-1964 acceleration was attributed to the unusually tight labor markets[21] of 1965-69, due to the Vietnam war (e.g., discussion following Freeman, 1973; Flanagan, 1976, pp. 494-495). This objection no longer seems tenable. As Freeman (1973, p. 128) noted in response to his critics, "The experiment that tests my interpretation ... versus the cyclical explanation will come in the next recession." Relative earnings continued to rise in (the sharp recession) 1974-75. Thus, as Table 2 demonstrates, the post-1964 trend did not depend on limiting the post-1964 observations to the tight labor markets of 1965-71 (see also Butler and Heckman, 1977, p. 255).

A second objection focuses on regional patterns in relative black improvement. Butler-Heckman (1977, p. 256) argue that relative incomes[22] show no post-1964 trend for Northeast or West, and only a "weak" effect (for males) in the North Central region. Thus, like Vroman, they find that the post-1964 effect is confined to the South. While greater progress in the South would be expected, given Federal enforcement patterns, the absence of effects outside the South is troubling.[23] Moreover, they find that relative incomes began a regular trend increase in the South in the late 1950's -- long before federal EEO pressures became operative.

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[21]The unusually is important -- one must argue that there is something about these tight labor markets not captured by the included cyclical variables.

[22]CPS wage and salary earnings series by region are not published.

[23]This is particularly true in light of the Supreme Court's holding that employment selection policies which are apparently neutral but operate to

More recent Social Security data suggest different conclusions. A comparison of recently published 1974 earnings data with previously published 1969 data shows increases relative earnings for both regions:[24]

Region	Male Relative Earnings		Female Relative Earnings	
	1969	1974	1969	1974
U.S.	.587	.624	.804	.882
South	.540	.579	.709	.806
Non-South	.630	.666	.882	.955

While these are simple averages rather than regression-standardized means, the fact that unemployment rates were higher in 1974 than 1969 at least suggests that these gains do not reflect cyclical factors.

A final challenge to the time series evidence is Butler-Heckman's (1977) argument that rising relative earnings reflect reductions in relative supply, due to expansion of transfer programs and perhaps other factors rather than increasing relative demand. Actually, the Butler-Heckman position involves two related but separate effects. First, reductions in relative supplies raise the ratio of offered wages or earnings. Second, changes in relative

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the disadvantage of minorities (e.g., educational requirements) are illegal unless they can be shown to be job related. Whatever the geographic incidence of "explicit" discrimination, the broader notion of discrimination surely had nationwide implications.

[24]The data are for whites and nowhites, from U.S. Social Security Administration (1975 and 1980).

supply are likely to be concentrated among low earners; withdrawl of low earners raises published earnings ratios because these are based on those with earnings (a "censoring" effect).[25]

The empirical importance of these observations has been the subject of a series of papers by Butler-Heckman (1977, 1978) and Freeman (1978a, 1978b). Thus far, four conclusions appear to be warranted. First, black labor force participation rates have declined relative to white rates in the postwar, and especially the post-1964 period; black male rates falling faster than while males', and black female rates not increasing as rapidly as while females'. If the marginal participants are drawn from the lower tail of the earnings distributions, the truncation effect would work to produce post-1964 trends similar to those observed. Second, black population shares have increased to almost exactly offset declining relative participation. Black/white labor force and employment ratios have changed very little, casting doubt on the notion that backward shifts in the relative suuply curve ar responsible for observed trends. Third, separating anti-discrimination (demand-curve), supply-curve, and truncation effects places extreme demands on time series data, due to collinearity among the variables and the shortage

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[25]Actually, the published median wage and salary earnings series are based on those with positive wage and salary earnings and are employed as wage and salary workers in March at the following year. The latter restriction is the more important one (Brown, 1981).

of "strong" variables affecting only relative supplies to serve as instruments.[26] Fourth, if truncation effects are estimated "independently" from earnings distributions, they are found to explain part of the post-1964 trend for relative earnings -- up to half for males, and 20-50 percent for females (Brown, 1981).

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[26]The transfer payment variables suggested by Butler-Heckman do not do very well on this score (Brown, 1981). Smith and Welch (1979, p. 23) note that "the increase (in fraction of men receiving either welfare or unemployment benefits) appears as large among whites as that observed for blacks" over the 1967-74 period they studied. This contradicts the Butler-Heckman premise that an increase in those programs would disproportionately affect blacks.

4. Cross-Sectional Studies of OFCC Impact

Attempts to estimate the impact of Executive Order 11246 on the relative labor market position of minorities and women have generally relied on comparisons between firms or establishments which are government contractors and others which are not contractors. The basic source of data is the "EEO-1" file (which contains reports made by firms with government contracts and non-contractors with 100 or more employees), augmented by information on whether the firm is a contractor. The EEO-1 form allows one to determine the number of workers in each of nine broad occupational categories by race and sex, but wages rates paid by race and sex are not available. Consequently, studies using these data typically use measures of relative employment and earnings-weighted[27] occupational distributions as dependent variables. The latter index may understate minority gains by failing to reflect within-category advances (Goldstein-Smith, 1976, p. 525), or overstate them if promotions to a new category tend to be concentrated in its lower rungs. (The EEO-1 reporting format would seem to encourage the latter).

Four studies have used the EEO-1 file to estimate the impact of OFCC on the relative position of minorities (Burman, 1973; Ashenfelter-Heckman, 1976; Goldstein-Smith, 1976; Heckman-Wolpin, 1977[28]). Because OFCC concern about sex discrimination was minimal in the periods studies in these papers, the emphasis has been on race rather than sex comparisons.

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[27]The earnings weights are taken from other sources.

[28]This is an expanded version of Heckman-Wolpin (1976).

Each paper attempts to estimate the impact of OFCC activities from the partial-adjustment model:

$$Y_t = \alpha X_t + \beta G_t + \gamma Y_{t-1} + e_t$$

where Y is an index of the relative position of minorities,[29] G is a dummy variable for government contractors.[30] X represents other variables, e is a disturbance, and t indexes the year. The unit of observation, however, is the establishment, and t is the same for all observations. The short-run (one period) effect of G is β, while the long-run effect (if the establishment were to remain a contractor indefinitely) is $\beta/(1-\gamma)$.

Short-run and long-run effects of OFCC on relative employment and occupational position are presented in Table 3. All except Goldstein-Smith find substantial effects on relative black male employment. Results for black female relative employment are quite varied. Except for Burman, none of the studies finds a substantial effect on relative occupational position.

Burman, Ashenfelter-Heckman, and Goldstein-Smith used similar estimation strategies, so the differences among them do not have obvious explanations. Indeed, Goldstein-Smith replicated the Ashenfelter-Heckman equations with their own (1970-72) data and report small negative gains for male relative employment using their specification.

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[29]Some studies use multiple-equation specifications, one equation for each race-sex group, but this complication is largely notational: β becomes a vector, and γ a matrix (lagged Y's of all groups are included on the right-hand of each equation).

[30]Goldstein-Smith use G_{t-1}, but indicate that using G_t produced very similar results.

Table 3

Estimates of OFCC Impact on Relative Employment and Occupational Position

Study Period Analyzed		Burman 1967-70	Ashenfelter-Heckman 1966-70	Goldstein-Smith b,c 1970-72	Heckman-Wolpin 1972-73
% change in employment:					
BM/WM	Short run [a]	} 5.6	0.8	0.04	8.8
	Long run		12.9		10.8
BF/WF	Short run[a]	}12.1		2.1	-1.6
	Long run				-1.5
WF/WM	Short run [a]			-2.3	-2.7
	Long run				-2.9
% change in occupation index:					
BM/WM	Short run[a]	} 2.5	0.2	0.05	
	Long run		-0.4		
BF/WF	Short run[a]	} 9.2		0.32	
	Long run				
WF/WM	Short run[a]			0.14	
	Long run				

Notes:

a. Short run impact is in per cent per year: estimated short run effects are divided by number of years covered.

b. Goldstein-Smith include both a government-contract variable G and a dummy variable for establishments undergoing compliance reviews, CG. OFCC effects calculated as β_G + (mean of CG among contractors) $\cdot \beta_{CG}$

c. Long-run impacts not presented. Inversion of lagged-adjustments matrix γ revealed that implied long run levels of dependent variables were negative for some race-sex groups.

d. Burman constraints the lagged dependent variable's coefficient to equal 1.0; Butler-Heckman (1977, p. 264) suggest his results "should be interpreted as an average of short-and-long-run effects."

Goldstein-Smith suggested two possible explanations for their smaller estimated impacts. First, their sample period (1970-72) was a time of higher unemployment than the late-sixties periods studied by Burman and Ashenfelter-Heckman. Second, the effect of OFCC may have declined as contractors learned how to "show 'good-faith efforts' without really making significant changes in their personnel policies" (1976, p. 542).

While relative employment of blacks is known to be higher in tight labor markets than in periods of high unemployment, it does not follow that the effect of OFCC policies must be appreciably smaller in loose labor markets. Even in loose labor markets, new hires provide leeway for appreciable changes in work force composition. In manufacturing, new hires per 100 workers per month averaged 2.9 in 1970-72, compared with 3.8 in 1966-69. Moreover, OFCC regulations govern layoffs as well as new hires. In any case, whether expanding establishments provide appreciably more leeway for OFCC influence can be tested by interacting the government-contract dummy with employment growth. The only study to do so found no support for this hypothesis (Heckman-Wolpin, 1977, Table 2, p. 87).[31] Thus, the looser labor markets in the period studied by Goldstein-Smith do not provide a wholly satisfactory explanation for their results.

The alternative explanation -- that the OFCC impact declined for reasons unrelated to the labor market -- is difficult to verify or disprove directly. Rather, it becomes the most plausible conclusion onece other explanations are

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[31]While employment expansion (among non-contractors) increased employment shares of all other groups at the expense of white males, the interaction of employment growth and contractor status favored white males.

discarded. Unfortunately, even if we accept this view, its implications are unclear. The declining OFCC impact could reflect relatively permanent factors (contractors learning how to beat the system) or temporary, reversible considerations (declining urgency of enforcement efforts, or contractors' realization that probabilities of sanctions are very small).

Another possible explanation lies in differences in the treatment of establishments which do not have government contracts, but are part of firms which do. Though legally bound by the same rules as establishments actually performing the government-paid work, these establishments were often reported as noncontractors. Ashenfelter-Heckman (1976, p. 63) found that 28 percent of all government contractors were reported as noncontractors. They used the data as originally reported. Goldstein-Smith, however, treat all establishments of contractor firms as "contractors." It seems reasonable to assume that these "hard to classify" establishments experience some OFCC impacts but less than "unambiguous" contractors. While this might lead to lower estimated impacts by following Goldstein-Smith's approach, it would explain only a fraction[32] of the difference in results.[33]

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[32]Let P_1 = proportion of establishments who are "unambiguous" contractors, P_2 = proportion who are "hard to classify", and let β_1 and β_2 be the effect of these two statuses relative to noncontractors. We expect $0 \leq \beta_2 \leq \beta_1$. Define $R = \beta_2/\beta_1$. Then Ashenfelter-Heckman estimate $\beta_1 - P_2\beta_2/(P_2+(1-P_1-P_2)) = \beta_1 - \beta_2(P_2/(1-P_1))$, the latter term being a weighted average of "hard to classify" and non-contractor establishements. Goldstein-Smith estimate $(P_1\beta_1+P_2\beta_2)/(P_1+P_2)$. One can then solve for the ratio of the two estimates,

In general, estimated impacts on relative occupational position are quite small. However, the finding that relative employment grows faster in contractor establishments might lead one to anticipate a decline in relative occupational position, since new entrants to the firm might be expected to start in the lower occupational groups. From this perspective, the rough constancy of relative occupational position would be interpreted as evidence of a positive effect of OFCC on this index as well. (Lacking an estimate of how much a given change in relative employment would affect relative occupational position, there is no way to determine whether this "positive OFCC impact" is quantitatively important.) None of the studies "control for" changes in relative employment in assessing OFCC impacts on occupational position.

Heckman-Wolpin (1977) give more careful consideration to the econometric problems of estimating the stock-adjustment model used in earlier papers. In particular, they note that if e is serially correlated, e_t and Y_{t-1} will be correlated, and estimates of β and γ will be biased. If the serial correlation is positive, γ will be over-estimated (so that the speed of adjustment is underestimated) and β (the short-run OFCC impact) will be

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as a function P_1, P_2, and R. If $P_1 = .5$ and $P_2 = .14$ (Ashenfelter-Heckman's proportions) and $0 \leq R \leq 1$, then the G-S estimate would be between 0.77 and 1.53 times the A-H estimate. Clearly, other factors are at work.

[33]Butler-Heckman (1977, p. 204, n. 18) suggest that the small estimated impact of being a contractor in Goldstein-Smith's paper is due to their inclusion of a compliance-review variable. However, the "impacts" in Table 3 reflect both variables (see note b).

underestimated.[34] (The sign of the bias in estimating the long run effect $\beta/(1-\gamma)$ is indeterminate a priori). A further problem arises if previous minority hiring (Y_{t-1}) is rewarded with contracts (G_t=1). If the latter relationship is in fact positive (and serial correlation is positive), G_t and e_t are positively related, and β is <u>overestimated</u> on this account. However, if $e_t = \rho e_{t-1} + V_t$, we can ρ-th difference the stock-adjustment equation to obtain.[35]

$$Y_t - \rho Y_{t-1} = \alpha(X_t - \rho X_{t-1}) + \beta(G_t - \rho G_{t-1}) + \gamma(Y_{t-1} - \rho Y_{t-1}) + V_t,$$

which can be estimated straightforwardly.* Heckman-Wolpin estimate this differenced form using data for establishments in the Chicago metropolitan area.

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[34]Heckman-Wolpin (1976, p. 553; 1977, p. 84). For more general discussions, see Griliches (1967) and Maddala (1977, pp. 371 - 73).

[35]Equation (6) of Heckman-Wolpin (1976, p. 554) contains a typographical error: "G_{t-1}" should be "$G_t - \rho G_{t-1}$."

*The statement in the text is valid even if lagged Y's determine G_t, so long as V_t and the disturbance in the equation determining G_t are contemporaneously uncorrelated. For a critical discussion of this aspect of the model, See Cain (1976, p. 575). It is unclear why Heckman-Wolpin use instruments for Y_{t-1} and Y_{t-2}, since both are uncorrelated with V_t if the recursive model for Y and G is correct. (If the recursive structure is relaxed, instruments for G would be needed.)

Their estimate of ρ is quite large (.90), suggesting that the dangers of ignoring serial correlation are real, and their estimates of γ indicate rapid adjustment. Their estimates of short-run OFCC impacts are larger for black males, relative to white males, than those in earlier papers, but are smaller (indeed negative) for black females. Since firms hiring larger proportions of black females are not rewarded with government contracts (Heckman-Wolpin, 1977, p. 94), there was no reason to expect their method of estimation to produce smaller gains for black females.

Unfortunately, Heckman-Wolgin did not present comparable estimates based on their data but the estimation strategy of the earlier papers. Consequently, it is not clear whether the differences between their estimates and those of the other studies in Table 3 reflect their treatment of serial correlation or other differences (Ashenfelter, 1976, p. 579). Four such differences are relevant.

First, after finding residual variances were inversely related to total employment, Heckman-Wolpin weighted by employment. None of the other studies did so. If OFCC activities are more effective in larger firms, such weighting would tend to increase the "contractor" effect. Adams (1973, p. 348) reports (non-statistical) evidence of greater EEOC impact in larger firms; similar differences in OFCC impacts are thus plausible (but conjectural).

Second, only Heckman-Wolpin include dummy variables for (one-digit) industries among the control variables. Government contracts are concentrated in industries with low levels of minority employment (Ashenfelter, 1976, p. 578) but it is not clear whether contractors are concentrated in industries whose minority employment shares would otherwise have grown more slowly (or more rapidly).

Third, the Heckman-Wolpin sample was limited to the Chicago area. This focus on a single labor market could remove a potential bias in the other studies' estimates, if areas with disproportionate contract representation are usually rich (or poor) in some unmeasured factors which determine minority employment growth (Flanagan, 1976, pp. 499 - 500). It is not clear, however, whether this would be expected to lead to higher or lower estimates of OFCC effects. Moreover, it is also possible that OFCC efforts are more (or less) effective in Chicago than elsewhere.

A final potential difference is the period studied -- 1972 - 73. Heckman-Wolpin do not estimate their equations for earlier years, but they do provide sample mean employment proportions for contractors and nonconctractors for 1970 - 73. This means that we can compare contractor-noncontractor differences in relative minority employment growth for 1970 - 72 (Goldstein-Smith's sample period) with those for 1972 - 73. While such differences are a very crude indicator of OFCC impact,[36] the results of the comparison are striking. The contractor-noncontractor differences in percent change in relative employment were:

BM/WM	-1.2% (1970-72)	vs.	7.0% (1972-73)
BF/WF	4.9%	vs.	-9.1%
WF/WM	-6.0%	vs.	3.1%

These differences suggest that year to year fluctuations in contractor-noncontractor relative employment are substantial. Moreover, they show more

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[36]Not only are such comparisons not corrected for differences in other variables, but the contractors in adjoining years are not (excactly) the same firms.

positive effects for black/white males in 1972 - 73 than in 1970 - 72, and the reverse for black/white females. Since this is the same pattern which is observed in the more careful estimates for 1970 - 72 (Goldstein-Smith) and 1972 - 73 (Heckman-Wolpin), there are at least hints that "year" influences are a factor in explaining the differing results. The white female/white male results do not fit this pattern, however.

While considerable effort and ingenuity have been devoted to estimating "the" difference between contractors and noncontractors in improving minority labor-market status, this difference need not reflect the impact of OFCC programs. Four such difficulties have been discussed in the literature. Each argument challenges, in one way or another, the appropriateness of noncontractor firms as a "control group" for the contractors.

One possibility is that successful EEO programs in contractor firms have favorable demonstration effects on noncontractor firms, as "erroneous employer prejudgments" are proven wrong by contractors' experience with minorities (Flanagan, 1976, p. 502). This would mean that contractor-noncontrator differences would understate OFCC impact. While such a tendency would hopefully be at work in the long run, it seems unlikely that such erroneous judgements would be quickly overcome. Thus, evaluations of the early OFCC experience (all studies in Table 3 deal with the first eight years of the program) are unlikely to be seriously affected.

A second possible source of error is the possibility that some non-contractor firms would have improved their minority hiring in order to increase their chances of becoming contractors. This "improvement" would legitimately be regarded as a positive OFCC impact on minority position. However, because it shows up as an improvement in noncontractor firms, it would reduce the estimated impact of

OFCC. Heckman-Wolpin (1977, p. 95) find some evidence that firms with higher or growing black male employment are more likely to receive contracts, but those with higher or growing black and white female and other (non-white, non-black) employment are less likely to become contractors. However, none of these coefficients is statistically significant. Whether firms believe that such improvements will increase their prospects for contracts (strongly enough to act on those beliefs) is even more uncertain. The magnitude of this bias would depend on how often non-contractors become contractors: the smaller is such a turnover, the fewer noncontractors who are likely to be improving for this reason. Heckman-Wolpin (1977, p. 94) report than in their Chicago data, 3.6% of those who did not have any contracts from 1970 - 72 became contractors in 1973.[37]

The interaction of OFCC and EEOC impacts represents a third area of concern. If EEOC activities are equally directed to contractors and non-contractors, differences between the two groups would reflect OFCC (rather than EEOC) efforts. However, EEOC activities could be weighted toward contractors (if EEOC tended to become involved once inadequate EEO performance was identified, or if "independent" enforcement happened to target certain industries) or toward noncontractors (if attempts to avoid duplication of effort led EEOC to focus on noncontractors). Flanagan (1976, p. 501)

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[37]OFCC efforts may also have impacts on non-contractors who were recent contractors.

suggested the former possibility was more likely, but the evidence on this point is quite weak.[38]

Finally, it has been suggested that contractor-noncontractor differences in minority employment could reflect a reshuffling of minority workers from noncontractor to contractor firms (Heckman-Wolpin, 1977, p. 74). The limiting case -- in which such transfers occur with no gain to minority workers -- seems implausible, since there must be some incentive for minority workers to move to the contractor sector. The more general issue -- the relationship between the contractor-noncontractor difference in minority employment and the shift in the contractor-sector relative demand curve (and the wage increases resulting from this shift) -- deserves careful attention. Discussion of this issue has been obscured by a lack of formal models of the impact of OFCC activities in a general equilibrium framework.

The most fully worked-out approach is Freeman's (1978b, pp. 20 - 22) two sector model. Freeman makes the standard assumption that inputs (in this case, black and white labor) will move to the sector which pays the highest price for that input. Thus, in equilibrium, black workers receive the same wage in each sector; so do white workers. However, the wage paid black workers need not equal the wage paid to white workers. Instead, the relative wage W is determined by the interaction of supply and demand.

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[38]Flannagan reports most efforts to "coordinate" the activities of the two agencies were unsuccessful, leading to duplication of effort. But some duplication would be observed if EEOC activities were uncorrelated with those of OFCC -- the condition for getting unbiased OFCC impacts. Goldstein-Smith (1976, p. 537) argue that EEOC efforts were in fact unrelated.

Freeman adopts three simplifying assumptions: in the absence of OFCC pressures, relative employment in the two sectors would be equal; the elasticity of relative employment, E, with respect to W is equal to the same value, η, in both sectors; total supplies of blacks and whites are fixed. Let c and n index contractor and noncontractor sectors, respectively. Then the relative demand curves, in the absence of OFCC pressure are

$$\ln(E_c) = \ln(E_c^o) - \eta \ln(W)$$

$$\ln(E_n) = \ln(E_n^o) - \eta \ln(W)$$

The purpose of the model is to determine the impact, on E_c, E_n, and W of a proportional outward shift on $\dot{X}$ in the relative demand curve in the contractor[39] sector; i.e., the contractor demand curve becomes

$$\ln(E_c) = \ln(E_c^o) + \dot{X} - \eta \ln(W).$$

Letting a dot above a variable stand for the proportionate change in that variable, and α be the contractor sector's initial share of total employment, the change in relative employment, summed over contractor and non-contractor sectors, is

$$\alpha\dot{E}_c + (1-\alpha)\dot{E}_n = \alpha(\dot{X}-\eta\dot{W}) - (1-\alpha)\eta\dot{W} = \alpha\dot{X} - \eta\dot{W}$$

If relative supplies are fixed, W must rise to exactly offset $\dot{X}$, so that the above expression is zero. Solving for $\dot{W}$ in terms of $\dot{X}$ gives

$$\dot{W} = \alpha\dot{X}/\eta$$

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[39]Thus, it is assumed that OFCC has no effect on the relative demand curve of the non-contractor sector.

Finally, the contractor-noncontractor difference in the change in relative employment exactly measures the proportionate effect of OFCC on relative demand in the contractor sector:

$$\dot{E}_c - \dot{E}_n = \dot{X}$$

The intuition behind this result is that, once OFCC activities have shifted contractors' relative demands, relative wages rise. The rise in relative wages leads both contractors and noncontractors to move back along their relative demand curves.

Thus, the model produces two conclusions: the OFCC "impact" on employment as conventionally measured corresponds to the shift in contractors' relative demand, and illustrative parameter values ($\alpha = 0.6$, $\eta = 1.0$, $\dot{X} = .10$) suggest a nontrivial effect on relative wages ($\dot{W} = .06$). However, if these illustrative parameters are close to correct, long-run OFCC impact remain small compared with the post-1964 increases reported in the time series studies.

Obviously, the model makes a number of strong assumptions. Relaxing the most basic assumption -- the long-run, market-clearing approach -- is difficult, because doing so would alter the whole character of the model.[40] The three empirical, "simplifying" assumptions mentioned above are more easily relaxed, at the cost of slightly more complicated notation.

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[40]One simple alternative is to assume that relative wages in each sector are fixed, by custom, minimum wage laws, or EEOC enforcement of equal-pay provisions. In this case, "excess" supplies of black labor would be available, and, so long as they were not exhausted, $\dot{E}_c = \dot{X}$, $\dot{E}_n = 0$, and $\dot{W}_c = \dot{W}_n = 0$.

Let η_c and η_n be the demand elasticities in the two sectors, α' be the fraction of whites initially employed in the contractor sector, and σ be the elasticity of relative supply with respect to W. The two key results then become

$$\dot{W} = \alpha'\dot{X}/(\eta'+\sigma)$$

$$\dot{E}_c - \dot{E}_n = \dot{X}[1-\alpha'(\eta_c-\eta_n)/(\eta'+\sigma)]$$

where $\eta' = \alpha'\eta_c + (1-\alpha)\eta_n$, the "average" demand elasticity. The conclusion that $\dot{E}_c - \dot{E}_n$ measures $\dot{X}$ can be seen to depend only on the assumption that $\eta_c = \eta_n$, and is only moderately sensitive to "moderately" unequal η's.[41] $\dot{E}_c - \dot{E}_n$ overstates $\dot{X}$ if $\eta_c < \eta_n$. Positive supply elasticities reduce the implied $\dot{W}$ (and moderate any disparity between $\dot{E}_c - \dot{E}_n$ and $\dot{X}$).[42]

A review of the OFCC studies thus seems to point to a positive, though hardly revolutionary effect (probably no more than 10% in the "long run") of OFCC activities on relative black male employment in contractor firms, and very little effect on black females. Three of the four studies find such effects (for males), and they survive Heckman-Wolpin's more careful estimation procedures. Using noncontractor firms as a control group, to estimate what would have happened to contractors in the absence of the OFCC, is open to challenge. However, there are potential biases in both directions, and none seems uniquely persuasive.

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[41] If $\alpha' = 0.6$, $\sigma = 0$, and $(\eta_c-\eta_n)/\eta' = 0.5$, $\dot{E}_c - \dot{E}_n = 1.3\ \dot{X}$.

[42] Unequal initial relative employment ratios have no effect, beyond reinterpretation of α, only to a first-order approximation.

However, this positive reading of the OFCC studies is open to challenge for another reason: the "lever" by which OFCC achieved these gains is not apparent. Two such "levers" seem plausible a priori: either firms with higher relative employment are rewarded with higher probabilities of receiving government contracts, or contractors with lagging relative employment are significantly affected by compliance reviews. To data, little evidence has produced suggesting that either lever has been used successfully.

As noted above, Heckman-Wolpin find very weak evidence that firms with above-average shares of black male employment are more likely to receive contracts. For example, a firm in which 10% of all employees were black males (substantially above the average among noncontractors of 7.6%) would find its probability of receiving a contract increased from .036 to .03604.[43] This is hardly surprising, given that "pre-award" reviews are not required for contracts below $1 million (and requirements for large contracts are sometimes not fulfilled (Ahart, 1976, p. 569)).

Compliance reviews have not been associated with significant improvements in relative black male employment in any of the studies which considered them -- Burman (1973), Goldstein-Smith (1976), or Heckman-Wolpin (1977), who once again corrected for potential biases from serially correlated disturbances.

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[43]The change in the probability of receiving a contract is equal to the coefficient of the black male employment share in their logit regression (= .052) times $\bar{P}$ $(1-\bar{P})$, times the .024 change in the black male share. Thus, $\Delta P = .052\ (.036)\ (.964)\ (.024) = .00004$, assuming the .024 change comes at the expense of white males.

A rather different approach to estimating OFCC impacts, taken by Smith and Welch (1977 and 1979), strengthens one's reservations about OFCC effects. They estimate an equation of the form

$$\ln Y_{it} = \sum_j X_{jit}[b_{j0} + b_{j1}(\text{race} = \text{black}) + b_{j2}(\text{year} = 0) + b_{j12}(\text{race} = \text{black and year} = 0)]$$

and observe that ΔR, the change in the logarithm of the black/white earnings ratio between year zero and year 1 can be decomposed into

$$\Delta R = \sum_j \{[(\bar{X}_{bj1} - \bar{X}_{wj1}) - \bar{X}_{bj0} - \bar{X}_{wj0})]b_{jo} + (\bar{X}_{jb1} - \bar{X}_{jb0})b_{j1} - (\bar{X}_{jb0} - \bar{X}_{jw0})b_{j2} - \bar{X}_{jb0}b_{j12}\}$$

$$= \text{"main effect"} + \text{"race effect"} + \text{"year effect"} + \text{"interaction effect"}$$

where the subscripts b and w refer to blacks and whites, and 0 and 1 the base and final years, respectively. $\bar{X}$ denotes the mean of the independent variables. The independent variable for our purposes is the fraction of industry product purchased by the federal government -- a measure of the leverage which OFCC can exert over private-sector hiring practices in different industries. Other variables held constant include schooling, "experience," and region.

In their earlier paper, Smith and Welch (1977) consider the 1960 - 70 period. They report that b_0 and b_1 are positive, and imply that the mean differences associated with the "main" and "race" effects are positive as well. The year coefficient was constrained to zero, after unconstrained estimates were insignificant. The interaction term, however is negative, and very large (b_{12} equaling 3 to 6 percent per year depending on the experience group), and the overall effect of "indirect" government employment is negative, though small.

In a later paper (Smith and Welch, 1979) covering the 1967 - 74 period, the effect of the federal purchase variable is allowed to differ by region rather than by experience group. Their results for the North are similar to their earlier findings. For the South, however, main and race effects are positive, and year and interaction effects are constrained to zero, based on results with unconstrained equations. Thus, the overall effects of indirect federal employment is now positive, but tiny -- less than three tenths of a percent over the period.

In both studies, Smith and Welch included other government variables -- direct federal employment and employment in federally regulated industries[44] -- and found no evidence that these made important contributions to increasing black males' relative earnings. They conclude that government pressure reflected in the contract compliance program is an unlikely explanation of the reduction of earnings differences between black and white males. Smith (1978) and Beller (1980) reach similar conclusions for black/white female and male/female earnings ratios, respectively.

Before analyzing the Smith-Welch results in detail, it is useful to consider what one might expect if the earlier-reported evidence of positive effects of OFCC activities on relative employment (among males), and Freeman's interpretation of those findings, was correct. In the simplest version of Freeman's model, wages paid to members of each race, and hence relative wages, would be no different among contractors than among noncontractors either before or after OFCC activities, so all four b's would be zero.

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[44] In the more recent paper, they include analogous variables for direct, indirect, and regulated state employment.

Thus, a finding of "no effect" in the Smith-Welch framework might be consistent with a positive "real" effect, if Freeman's model is correct.

However, this attempt at reconciliation fails for two reasons. First, not only would be the "overall" effect as measured by Smith-Welch be zero, but each of the components would be zero. In fact, as noted above, Smith-Welch consistently find a negative interaction effect reducing or reversing the other terms. Second, Freeman assumes that the supply of labor of either race to the contractor sector is infinitely elastic. While that may be a plausible long run assumption, some wage inducement must be postulated to attract blacks to the contractor sector in the short run; that inducement should show up as a negative value of b_{12} (relative wages rising in the contractor sector compared with the noncontractor sector) and hence a positive interaction effect. Again, Smith-Welch find precisely the reverse.

It is, however, difficult to be sure how one should interpret the Smith-Welch findings. The basic problem is their finding of immense premia to blacks in the contractor sector, relative to blacks in the noncontractor sector, prior to OFCC activities. Controlling for schooling, experience, and region, Smith-Welch's equations say that in 1960, relative black earnings were between 42 and 159 percent higher earnings in the contractor sector than in the contractor sector.[45] Lacking a clear understanding of what led to this difference in the first place, one doesn't know at what rate it would have been reduced absent OFCC presures, or whether the implicit Smith-Welch assumption -- that it would have been maintained -- is correct.

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[45]Based on Table A2, Smith-Welch, 1975 (a longer version of Smith-Welch, 1977). The range of estimates reflects differences in government contractor coefficients by experience.

Cross-Section Studies of EEOC Impact

Because of the near-universal coverage of Title VII (all firms with 15 or more employees are now covered), cross-section studies of EEOC impact are hampered by the lack of an obvious control group. Apart from very small firms, there are no firms not affected by Title VII which would allow us to estimate what would have happened to covered firms in the absence of Title VII.

One potential basis for inference lies in the fact that only firms with 100 or more employees are required to report the race/sex composition of their workforce to EEOC on a regular basis. One might plausibly expect EEOC pressures to be concentrated on these reporting firms. Brimmer (1976, pp. 26-28) observed that while blacks' share of total employment rose by 0.9 percentage point (from 9.6 to 10.5 percent) from 1966-74, their share of EEOC-reporting employment rose by 3.4 percentage points (8.2 to 11.6 percent) in the same period.[46] However, Brimmer stops short of labelling the differential growth and EEOC "effect." Not only are many non-reporting firms still covered by EEOC enforcement activities; the lack of controls for other factors (industry, or firm size per se) makes the comparison suggestive at best. The difference is, however, striking.

An alternative approach is to compare firms or establishments which were directly involved in EEOC enforcement procedures with other, otherwise similar units. Adams (1972)[47] compared 65 metropolitan firms which had reached

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[46]He notes, however, that these greater gains are concentrated in the lower occupational groups.

[47]See also Adams (1973) and U.S. Comptroller General (1976).

successful conciliation agreements[48] with a comparison group (matched by industry and metropolitan area) not involved in EEOC proceedings. Using changes in black employment shares and relative occupational position as dependent variables, he found that, on average, black employment shares (and, less consistently, relative occupation position) increased faster in the firms with conciliation agreements, but the differences were small and never statistically significant (Adams, 1972, p. 118).

There are two problems with using these results as an indication of EEOC activities. First, firms which are the subject of complaints to EEOC are likely to be less "progressive" than those which are not involved at all with EEOC. If less "progressive" is taken to mean low initial levels of minority employment, Adams's focus on changes in minority employment situation would control for this tendency. However, if firms with lagging rates of improvement in minority employment are more likely to be the object of complaints, then Adams's comparisons would underestimate the true EEOC impact.[49]

Second, the sample size and other characteristics of the experimental design led to standard errors which were large relative to plausible EEOC effects. For example, black males' (females') share of total male (female) employment

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[48] In the periods covered by Adams's data (1966-69), EEOC lacked the legal power to initiate court action.

[49] It is possible that Adams's comparison overstates EEOC impact, if the incidence of complaints is fairly random, and willingness to settle indicated "progressive" attitudes of the firm. Using firms with unsettled complaints as control group would have been likely to produce overstated EEOC impacts.

problem, and uses a two-state estimation procedure which treats enforcement as endogenous. However, the two stage results are uniformly weaker than those using ordinary least squares. The instruments for complaint are presence of an EEOC regional office, the proprotion of nonwhite males with at least a high school education, the proportion of nonwhite males who live in the state's urban areas, and the unemployment rate for black males. Apparently, opening a regional office did increase complaint volume (U.S. Equal Employment Opportunity Commission, 1966, p. 58), but Beller's argument that the location of regional (as opposed to district offices) was not sensitive to relative employment and wages is unpersuasive.[53] No attempt is made to justify the assumption that the other instruments do not themselves "belong" in equations determining relative employment and wage gains. Finally, use of the state as the unit of observation is questionable. The deterrent effect of EEOC's activities seem more likely to be felt by other firms in the same industry or industry group (whose employment practices may mirror those of the challenged firm) than by the firm's geographic neighbors in unrelated industries (Killingsworth, 1980 p.)

For example, Ichniowski (1980) noted that historically, aluminum industry contracts follow the pattern set in basic steel. As a result, after the steel industry agreed to a consent decree which revised the seniority system, "the terms of the basic steel consent decree were incorporated practically verbatim

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[53]"We can take the locations of [regional] offices as exogeneous in modelling the original compliance system; since they were chosen to enable EEOC to carry out its field operations, they would be independent of Title VII's enforcement. But the location of district offices would be endogenous in modelling the current system; because they are chosen on the basis of experience they would depend upon Title VII's enforcement." (Beller, 1980, p. 367).

within the framework of the aluminum industry's collective bargaining agreement" without direct government or court involvement. None of these objections, however, would lead one to expect the differing impacts of employment and wage complaints which Beller reports.

In two other papers, Beller extends her work to female earnings. Beller (1977) relates female earnings in 1967 and 1974 to a set of personal characteristics and measures of EEOC enforcement. The enforcement variables were investigations of female complaints completed per employed woman, and the ratio of successful to attempted settlements. Pre-and post-1972 values of these variables are distinguished, as are charges of race discrimination (by black females) and sex discrimination, making a total of eight enforcement variables. (Only the sex discrimination variables are entered into the white female wage equations.) Separate equations are estimated for each year, and differences in coefficients between 1967 and 1974 for the enforcement variables are used to measure their impact.[54] The overall effect of the sex-discrimination enforcement variables was to increase female earnings by 4.7 percent, and race-discrimination enforcement on behalf of black females increased their earnings by an additional 1.2 percent. These differences are smaller, as one might expect, when industry and occupation are held constant.

Beller (1979) looks at the effect of EEOC sex-discrimination enforcement on male and female earnings from 1967 to 1974. Her two basic enforcement variables are distinguished by year (before/after 1972) and sector (government/private) to account for changes in EEOC enforcement powers and coverage of

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[54]The coefficients of the enforcement variables in 1967 are interpreted as capturing the relationship between enforcement and pre-existing wage differences.

government employment in 1972. She finds EEOC efforts reduced the female/male earnings differential by seven percentage points. However, the difference is not statistically significant, and six of the seven points come from lower male wages.

The two more recent papers are based on much more plausible enforcement variables -- investigations and settlement ratios rather than complaints. The simultaneity and unit-of-observation problems in the earlier paper remain. Coefficients are often imprecisely estimated, and raise several unanswered questions: Why should EEOC efforts impair black male relative position, but help females? Why does enforcement of sex-discrimination complaints close the male-female wage gap by reducing male wages rather than improving female wages? Why do successful settlements of sex discrimination before 1972 "significantly" reduce female wages, but post-72 settlements "significantly" increase them?

6. Conclusion

The majority of the evidence seems to point to the conclusion that the labor market position of minorities improved more rapidly in the past 15 years than might have been expected on the basis of prior trends, general business conditions, or the relative educational attainment of minorities. Females' earnings have not increased appreciably relative to males; whether such stability in the face of rising female labor force participation would have been predicted from pre-1964 experience has received little attention.[55]

This reading of the time series evidence is not without its critics, and further work could be most helpful in resolving remaining doubts. The claim that the post-1964 acceleration of black relative position was confined to the south (and there predates Federal anti-discrimination efforts) is based on regional income data[56] and early studies using Social Security earnings data. Because CPS time series of regional earnings data by race and sex stretching back before 1964 are unavailable, an updated study using Social Security data by region and race-sex would be most helpful. The very simple 1969-74 changes in average Social Security earnings presented in section 3 does not support the position that gains were confined to the south.

The censoring argument -- that rising relative median earnings reflect greater labor-market withdrawal of (otherwise) low earning blacks is also of

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[55]The large body of work "explaining" male/female differentials is not very helpful in attempting to detect trends.

[56]In some contexts, earnings and income data are thought to be relatively interchangeable. However, national income data show appreciably slower post-1964 relative black improvement, compared with median wage-and salary earnings.

considerable concern. Further work to isolate its importance is needed. Understanding the causes of the dramatic reduction in labor force participation of black males is important, not only to help estimate the importance of censoring but also, more directly, to get a complete picture of changes in the labor-market situation of blacks. To date, neither side of the censoring debate has been successful in isolating these causes. The impact of censoring on the regional trends discussed above would also be of interest, but is currently unexplored.

The distribution of the post-1964 gains is also in need of explanation. Virtually every study finds greater relative gains for black females than black males. However, this is not the pattern one would expect on the basis of the government-pressure hypothesis. Beller (1975, p. 28) notes that roughly 60 percent of all EEOC complaints were filed by black males. Studies of OFCC impact surveyed in section 4 point to smaller effects for black females than black males. Four possible explanations for the greater relative progress of black females have been suggested in the literature, two "real" and two "statistical". Anderson (1976, p. 197) suggests that since female turnover rates are higher than male rates, the continuing impact of prior disadvantages in firm-specific skills and seniority would be smaller for females than males. (This leaves the smaller OFCC impacts for females as a continuing puzzle). Smith (1978) has suggested that black females benefited from employer perceptions that they fulfill two "quotas", race and sex, at once. Smith (1978) has also noted that the departure of black females from domestic service to other work may have overstated their true earnings gains, if earnings of domestics are underreported.[57]

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[57]Under-reporting would be beneficial for tax purposes, and easier for domestics than for those in other employment due to lack of effective withholding. Earnings not reported to IRS may also not be reported to Census interviewers.

Finally, the censoring issue may be more important for females than males, though in the case of females the significant trend would be the increasing participation of white females. Brown (1981) reports larger post-1964 tends for females than males after correcting for censoring however.

A further puzzle that emerges from the study of post-1964 trends is the concentration of the gains among the most educated and, at least for young men, those from the most advantaged backgrounds (Freeman, 1978b). Johnson and Welch's (1976, p. 520) theoretical model predicts the greatest gains from their "economy-wide affirmative action plan" would accrue to minority workers in the middle of the skill distribution. Employee selection guidelines issued by EEOC and OFCC prohibit use of educational requirements or standardized tests if they disproportionately exclude minorities but cannot be shown to be "job-related," and in practice employers have had considerable difficulty in establishing job-relatedness of such hiring criteria. In this sense, the greater gains of more advantaged blacks run counter to such guidelines. There are several possible explanations. First, federal enforcement efforts may have been concentrated at the upper end of the skill distribution. Second, if discrimination were initially greater against the most educated blacks -- as studies of the early 1960's often found--then reducing all discrimination by, say, one half would lead to greater gains in relative black earnings at the higher educational levels. A third possibility is that observed trends represent a combination of federal efforts to help blacks in general and other forces which most severely penalize disadvantaged blacks (the movement of business activity from central cities comes to mind here). Finally, it is possible that employers respond to the selection guidelines by establishing different hiring standards for minorities, in order to keep the old, familiar

selection criteria but avoid disproportionately excluding minorities. This hypothesis would explain the persistent business charges -- and persistent agency denials -- that quotas are being imposed: firms impose quotas on themselves, to avoid abandoning selection criteria they cannot validate to the agencies' satisfaction.[58] While this conjecture explains why disadvantaged blacks haven't gained the most (the premise that the guidelines should have done so is wrong) it doesn't explain why they have gained the least.

Studies of the impact of OFCC and EEOC activities, based on cross-sectional data, are not very conclusive for either agency. Most studies of OFCC show substantial employment gains for black males, but the exceptions raise serious doubts. None of the studies provides a "mechanism" by which OFCC secured these gains, since there is little evidence that minority hiring significantly increases the probability of receiving a contract, or that compliance reviews actually encouraged the hiring of minorities. Finally, the almost total reluctances to debar firms not in compliance must contribute to skepticism about the OFCC impact. One possibility is that contractors' employment practices were affected by OFCC initially, before the weak sanction posture was appreciated, but has had less impact subsequently. If this scenario is regarded as realistic, the relatively recent (1970-72) negative findings of Goldstein-Smith (1976) would suggest that the program had lost its punch by then. More recent evidence would, therefore, have greater value than most simple replications.

Results for EEOC have also been relatively inconclusive. In large part, this stems from difficulties in finding the appropriate control group, and probably from aggregating up to the state as unit of observation.

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[58]There is a crucial issue here which receives almost no attention even in "informed" public discussions -- are failures to validate selection criteria an indication of inept selection or unreasonable application of standards on validation?

What is needed in "second-generation" studies of OFCC and EEOC impact is more careful specification of the "treatment" which "experimental" establishments or firms receive. Existing studies look only at numbers of complaints, investigations, and settlements by EEOC, and contractor status and completion of a compliance review by OFCC. In addition to the possibility that the "overall" impact of the agencies are not well measured by such crude measures of agency efforts, the overall impacts are of very little use to policy makers.

Differentiating among OFCC activities and impacts could include these issues: Is the size of the contract relative to the firm's total sales an important aspect of OFCC leverage? As noted in section 2, the frequency of compliance review varies widely among federal departments. Do contractors of frequent-review departments have better minority employment records, even in the absence of a review, because the threat of review is greater? Does the minority hiring record of firms not initially found in compliance improve faster than that of firms found in compliance, or than that of firms not reviewed at all?

In studying EEOC, greater detail in specifying what EEOC "does" would also be desirable. For example, does the position of minorities improve during the period between complaint and investigation, or during the period during which the firm is being investigated? Do minority employment gains (if any) following a conciliation agreement or court decree vary with the details of the settlement? (For example, do settlements specifying goals and timetables have a greater impact on minority position?) Does settlement by one firm lead to to improvement by other firms in the same industry? The endogeneity of almost any EEOC "treatment" appreciably complicates the analysis.

The above menu is restricted, in the sense that it is limited to refinements that might, possibly, with considerable effort, be coded from

existing records and matched to the relevant firm's EEO-1 records. Whether such refinements are in fact feasible is a much harder question. Ashenfelter-Heckman (1976) reported considerable problems in simply matching establishments from successive EEO-1 files and determining contractor status. Beller (1979) noted the difficulties of even computing EEOC investigation and settlement frequencies by industry. The OFCC and EEOC efforts were not lavishly funded or staffed, and keeping records of the sort which would permit investigating these issues with a large-scale statistical approach may have been an unaffordable luxury.

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