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THE GOVERNMENT'S IMPACT ON THE LABOR
MARKET STATUS OF BLACK AMERICANS:
A CRITICAL REVIEW

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This paper surveys recent evidence on the impact of government programs on the measured labor market status of black Americans. The mid-sixties witnessed an outpouring of civil rights legislation unprecedented since the time of the Civil War. In view of popular concern with civil rights, it is remarkable that so little analysis of these programs is available. Even more remarkable is the diversity of opinions offered by the few professional economists who have analyzed these programs. Some claim that governmental activity has been instrumental in reducing observed black-white income differentials, although they do not specify particular channels of influence. Other economists who investigate the effects of particular programs document only miniscule effects of government policy.

These conflicting claims can be reconciled in part by examining differences in data and methodology used by the dissenting analysts and in part by taking a broader view of government policy than previous analysts have considered. In this paper, we argue that previous studies neglect the impact of recent government policy on the supply side of the labor market, and that the supply side effects of recent policy play an important role in explaining the recent measured increase in the ratio of the wages and incomes of blacks to the wages and incomes of whites.

Most studies of black-white differences in earnings and wage rates are conducted within a demand-oriented framework suitable to the textbook economic analysis of discrimination. Within this framework, the role of governmental civil rights policy is to tax discriminatory firms and to subsidize nondiscriminatory behavior in order to favorably shift the demand curve for black labor relative to that for white labor. This framework is

not inappropriate. Indeed, specific laws such as those implemented in Title VII of the 1964 Civil Rights Act forbid both employment and wage discrimination and hence act directly on the demand for black labor albeit in potentially conflicting ways. Affirmative action programs instituted by the Office of Federal Contract Compliance (OFCC) are designed to affect the demand for black labor.

Even though a demand-oriented framework of analysis is not inappropriate, it is incomplete. Virtually coincident with recent civil rights activity there has been an expansion of government welfare and transfer programs. While these programs have not been explicitly oriented towards specific racial groups, their criteria for participation have differentially favored blacks by virtue of their lower position in the income distribution.

Income transfer programs raise the minimum wage that potential labor market participants will voluntarily accept for market work and hence tend to reduce the labor force activity of groups with low market wages, a phenomenon observed in the past decade. Since blacks are over-represented in the population potentially eligible for these programs, the induced decline in their labor force participation and employment serves to raise measured wage rates and earnings for market participants for two distinct reasons.

First, even if all blacks are homogeneous in their ability to perform market work, a reduction in the supply of blacks relative to whites tends to raise the wage of working blacks relative to working whites as long as blacks and whites are not perfect substitutes in the demand for labor by firms. Second, by removing the lowest wage blacks from the labor force, social transfer programs can manufacture the illusion of relative wage growth by simply subtracting the least productive blacks from the population base used to measure wages and earnings.

These supply side effects of government activity are not the whole story of the recent convergence in black-white wage ratios but they are an important and neglected component that deserves more stress than previous studies have accorded it. These supply effects serve to explain the apparent anomaly that prime age black male participation rates have declined in the face of narrowing racial wage differentials, a fact that cannot be explained by analysts who argue that the government has improved the market position of black labor by reducing discrimination.

Despite our conviction that the scope of research on this question should be broadened, we do not go far in performing that service here although we do so in a companion paper. Rather, we confine our analysis to a detailed review of previous evidence without restricting ourselves to the view of the labor market maintained in previous studies.

The paper proceeds in the following way. We first review the basic set of facts that any study of government impact should be able to explain. We then review key features of recent antidiscrimination legislation. Then we discuss problems in measuring the impact of such legislation and review the findings of previous work. In an Appendix, we offer some evidence that casts a new light on some of the previous evidence.

I. The Facts to be Explained

Before turning to an analysis of the impact of antidiscrimination programs, it is useful to review the basic set of facts on the recent black labor market experience with which any study of the impact of government policy must contend. These facts are extracted from two principle sources of data: aggregate time series data from the Current Population Surveys (CPS) and information from cross sections removed in time, such as the 1960

and 1970 U.S. Census. The latter data have been extensively analyzed and are rich in information about personal characteristics. The aggregate time series data, while crude, are especially useful for examining broad trends in black economic progress, and for determining the timing of the changes in trends. For these reasons we turn to this data first.

A. The Time Series Data

Some graphs from a more extensive analysis (Butler and Heckman, 1976) are reproduced below. The time series data display the following features.

(1) Chart 1 reveals that the ratio of the median earnings of black males to the median earnings of white males was roughly constant until the mid-sixties at which point in time it began to grow at a rate which only recently decelerated. The same ratio for women shows a steady upward growth which accelerates in the mid-sixties--at about the same time that black male relative earnings began to grow. For both sex groups, the growth in relative earnings occurred at the same time that real earnings grew for all groups. In the 1974 business cycle, all demographic groups suffered a decline in real income but black relative status continued to grow.

Essentially the same story can be told for alternative measures of income. The median earnings of full time workers (not shown here) display the same pattern as that of the median earnings of all workers just discussed. The rate of growth of relative income for full time workers is lower, suggesting that part of the growth in relative black status recorded in Chart 1 is due to a growth in relative hours worked by blacks.

Chart 2 presents relative median income, a measure that is defined for the entire civilian population, and not just for wage earners, as is the case for median earnings. This measure includes capital and transfer

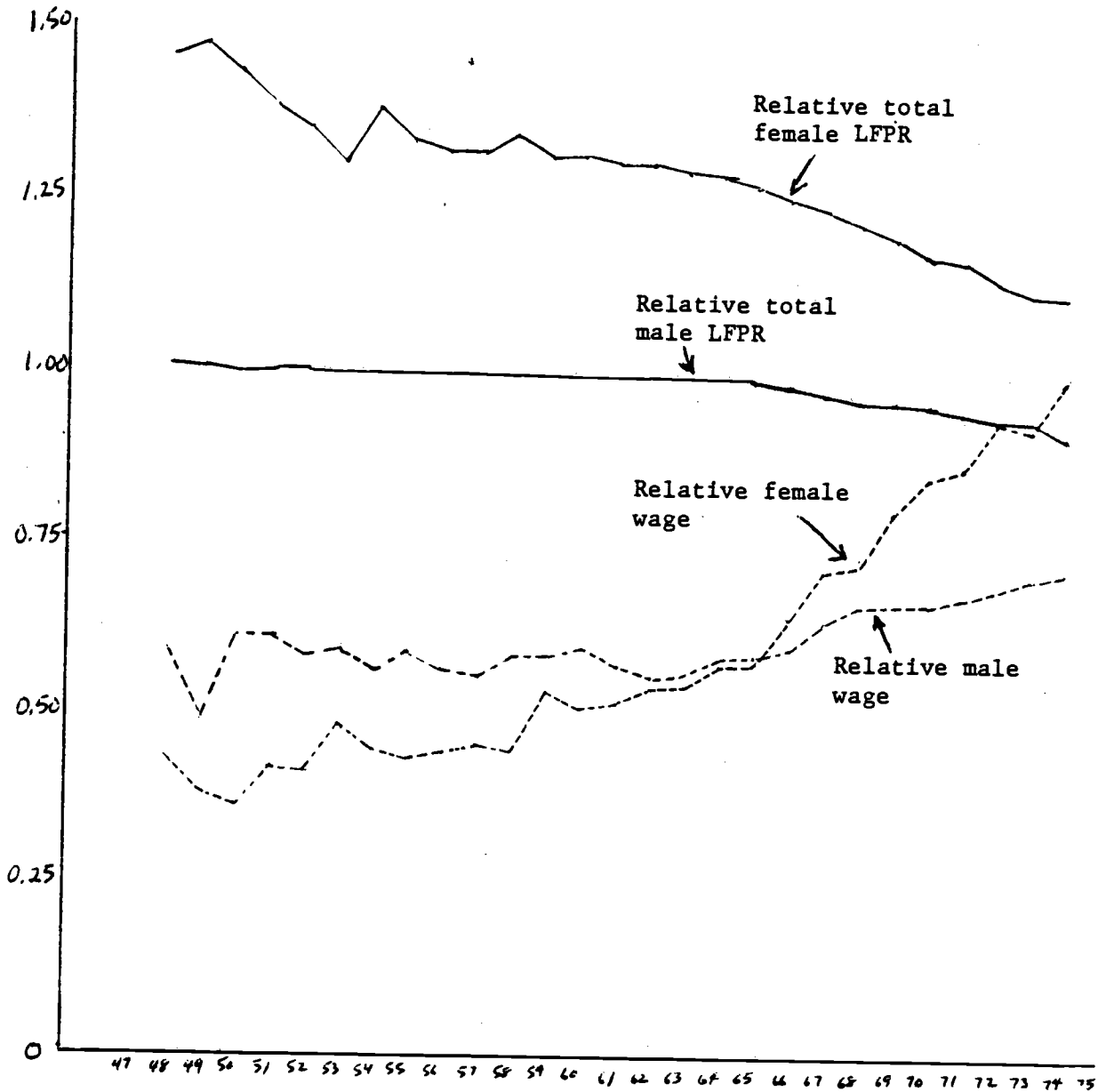


Chart 1 -- Relative nonwhite to white median wage/salary income (dotted lines) and relative total labor force participation rates; by sex.

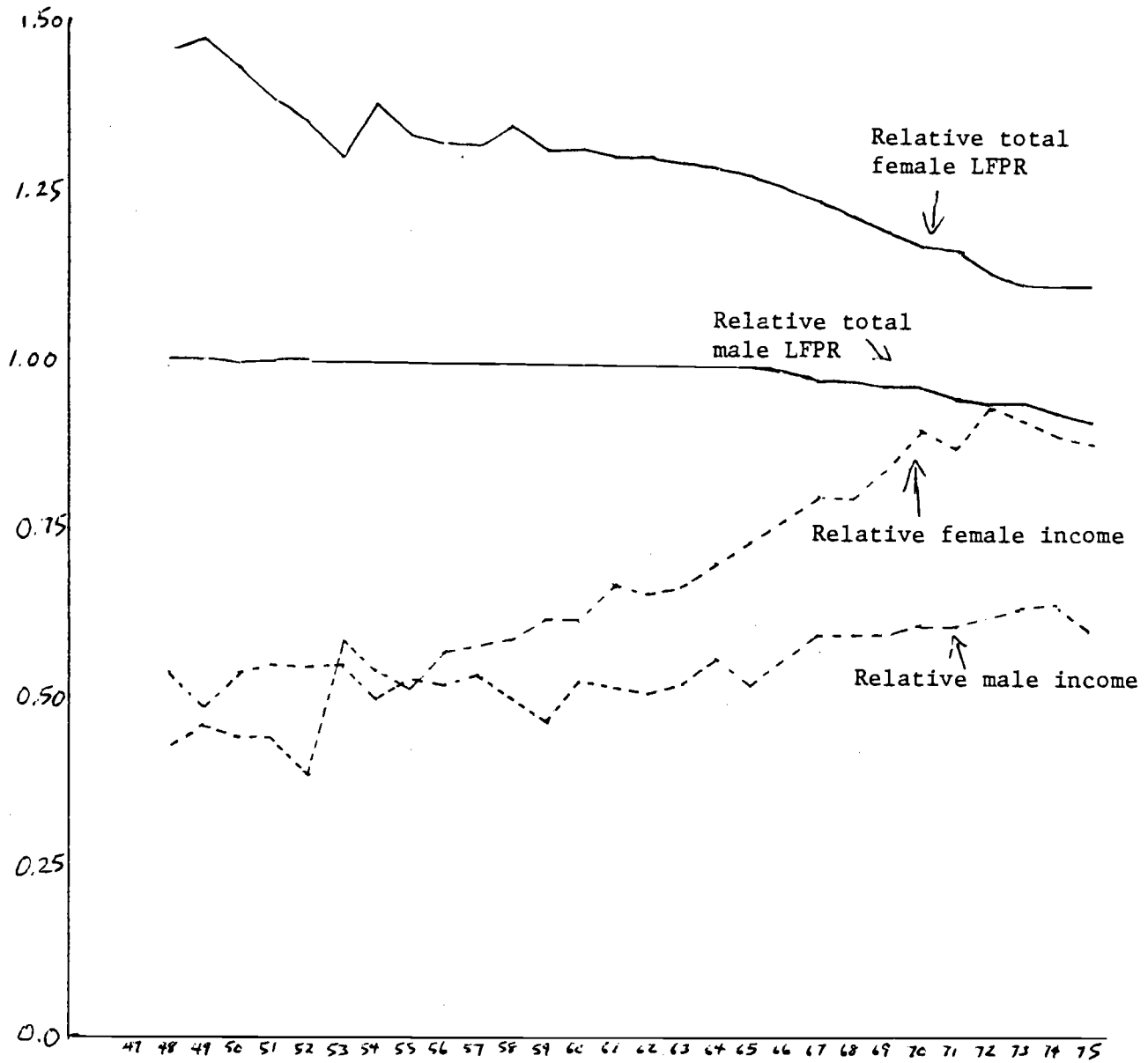


Chart 2 -- Relative median income and relative total labor force participation rates; by sex .

income in addition to earnings, and so provides a clearer picture of the relative monetary welfare of the entire black population. The relative median income data display the same pattern shown by the median earnings data. The two measures differ only in that the relative income data exhibit a lower rate of growth. To the extent that black economic welfare has been raised by income transfer programs that reduce work effort, it is expected that measured income defined for the entire black population would exhibit a slower growth rate than the earnings of workers since it is possible for a transfer program recipient to have lower money income and fewer hours of work and still be better off than if he did not participate in such a program.

(2) Chart 1 also reveals that the ratio of black male labor force participation to white male participation exhibits a near stability until the mid-sixties at which point it begins a rather steady decline. (From ".91" in 1965 to ".81" in 1975.) This decline is a result of a greater decline in the aggregate black male participation rate than in the white rate, which also fell. By no means is this decline confined to older males. As Chart 3 reveals, the labor force participation rate for prime age blacks has declined relative to the rate for prime age whites which exhibits only a slight tendency to drop over the post-war period. Indeed, as Chart 4 reveals, the recent decline in the aggregate relative male rate is not due to a decline in the relative participation rates of older workers.

From these charts, it is clear that there has been a narrowing in racial wage and income differentials which began in the mid-sixties--coincident with the emergence of federal antidiscrimination programs. It is precisely this coincidence that has led Freeman (1973) and others to infer a significant

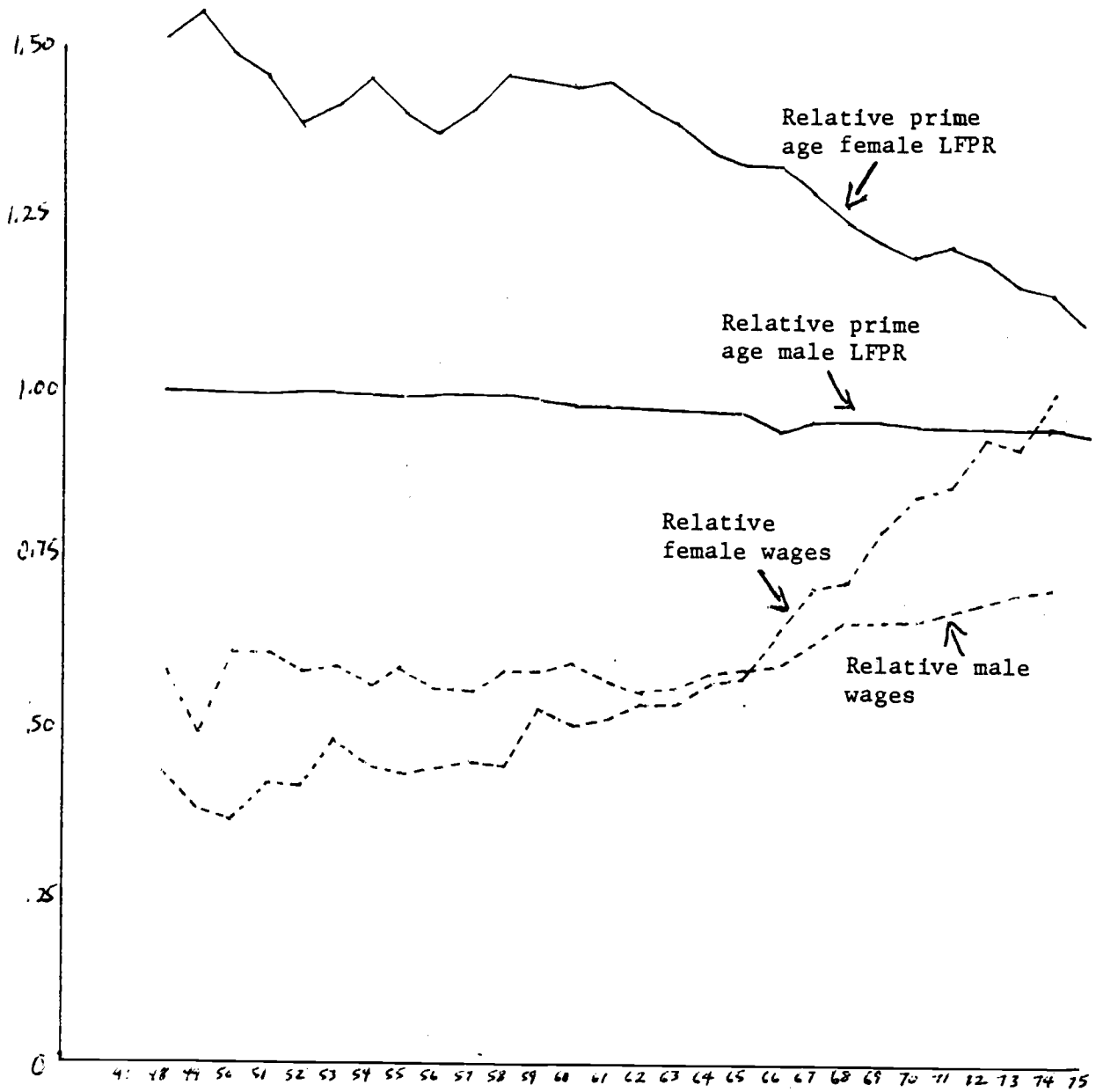


Chart 3 -- Relative nonwhite to white median wage/salary income and relative prime-age (35-44 years old) labor force participation; by sex.

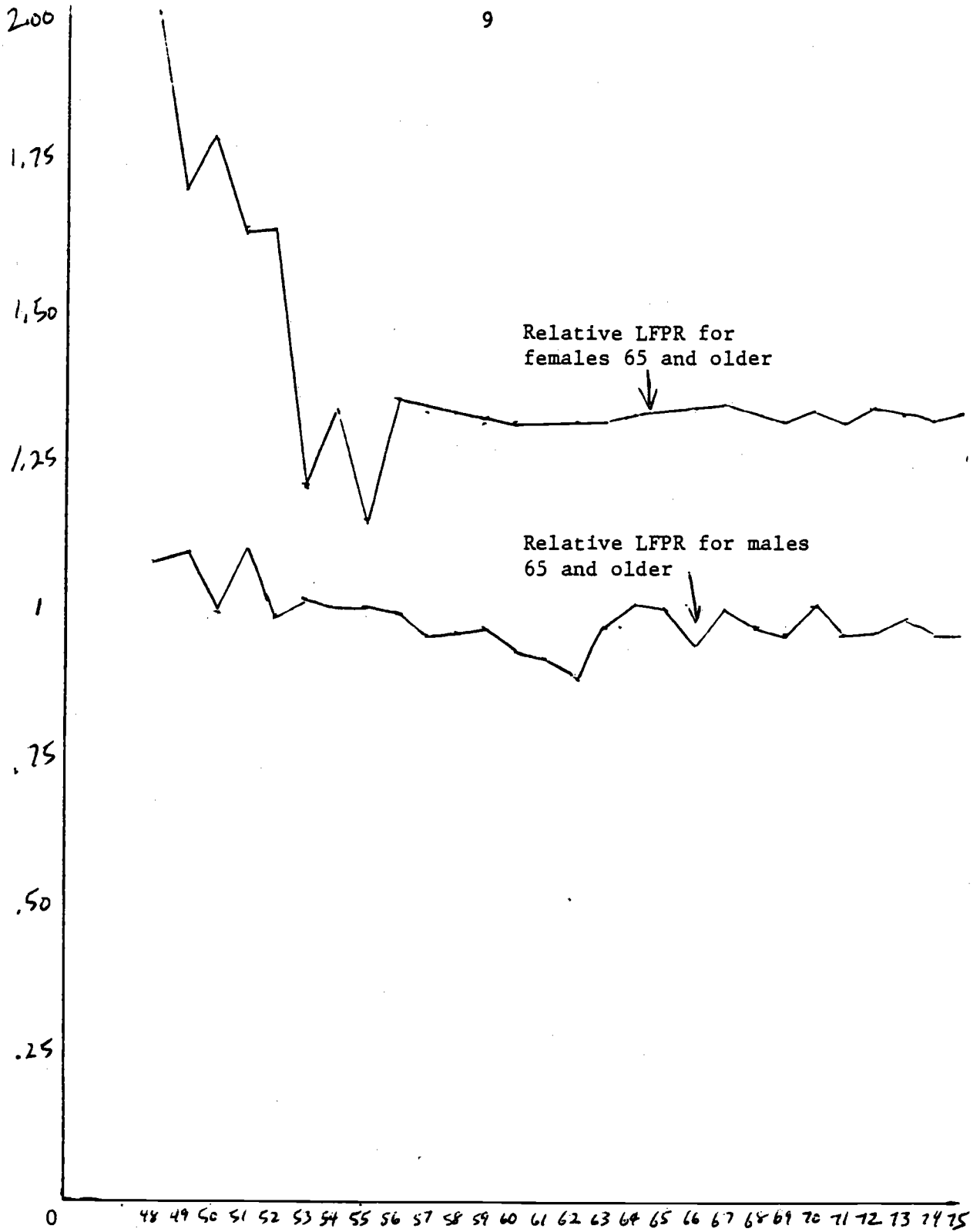


Chart 4 -- Relative nonwhite to white labor force participation rates for the civilian labor force 65 years and older; by sex.

role for government antidiscrimination policy. However, it is reasonable to seek some explanation for the relative reduction of black workers in the labor force that has been particularly pronounced among prime-age males. A simple demand induced / ^{increase} in black wages and income should increase black labor force participation relative to white labor force participation give market opportunities for blacks have expanded. Yet, a decrease is observed.

The story for women is different. Although the relative female participation rates (displayed in Chart 1) decline over the post-war period, the relative decline accelerates in the mid-sixties. The decline in the black female participation rate relative to the white rate reflects a slower growth rate in the black rate and not a downturn. In fact, the black female labor force participation rate has remained constant in the past eight years while the white rate has continued to grow.

(3) Data on family income are displayed in Chart 5. The growth in relative family income occurs in a brief period in the mid-sixties, and there is little evidence of relative growth since then. This lack of growth in relative family income is largely due to the growth in female headed households among blacks recorded in Chart 6. One possible reason for the growth in female headship rates among black families is the AFDC program which provides incentives for the formation of female-headed households. The average real payment per AFDC recipient rose in the mid-sixties at the same time female headship rose among blacks.¹ (See Chart 11, p. 56.) Even though

¹ Some caution is required in interpreting this result, since greater AFDC participation by unwed mothers may mechanically raise the measured payment per recipient. A cross sectional study by Honig (in Lerman, 1974) documents a real effect of AFDC payments on headship rates so that the causal correlation apparent in the time series has some support from independent evidence. But, as pointed out to us by Glen Cain, Honig's AFDC inducement variable--AFDC payments per family--potentially suffers from the same bias as the AFDC per recipient variable, and may be as much a consequence as a cause of increasing female headship rates.

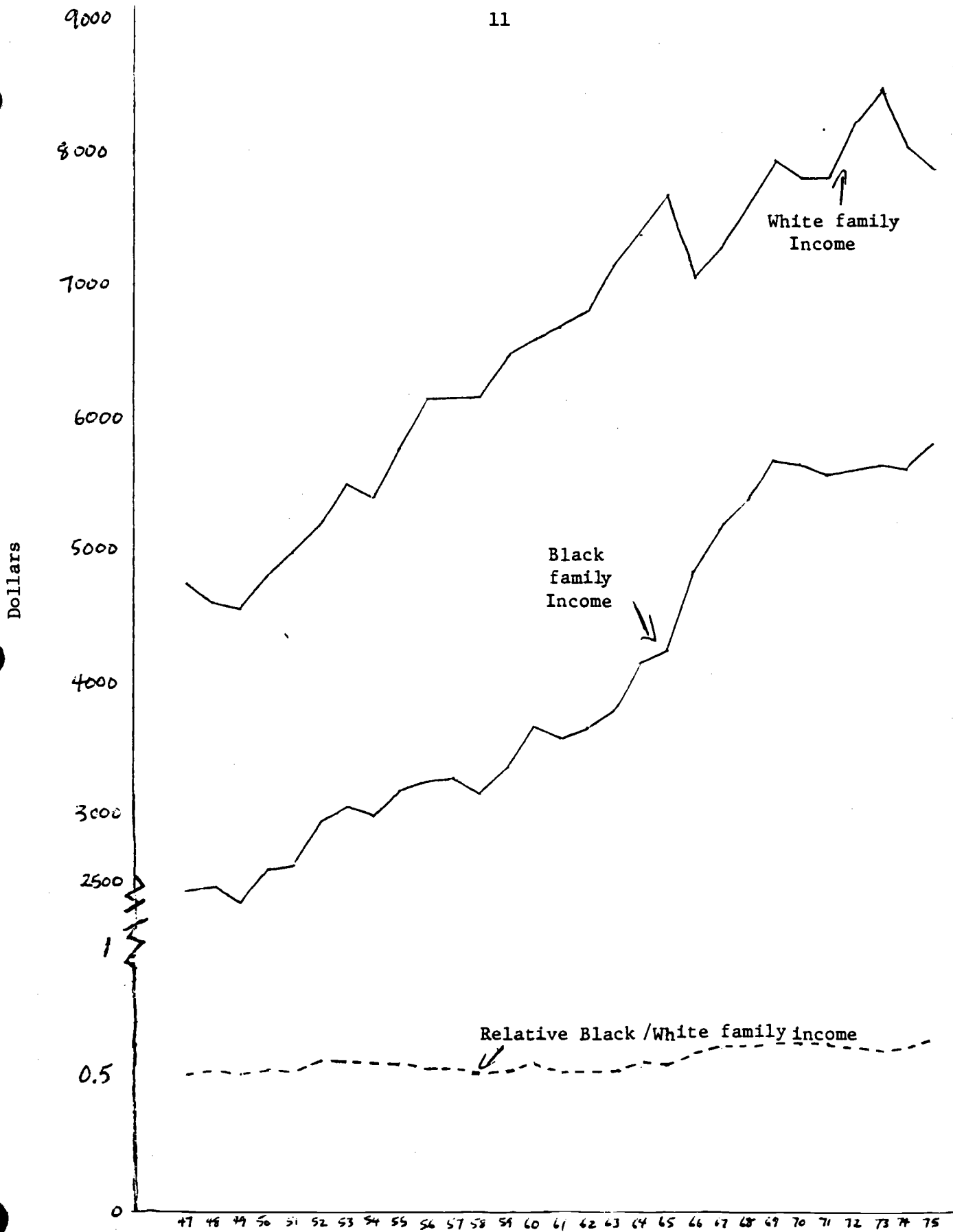


Chart 5 -- Median real income for black and white families and relative black/white family income (dotted line).

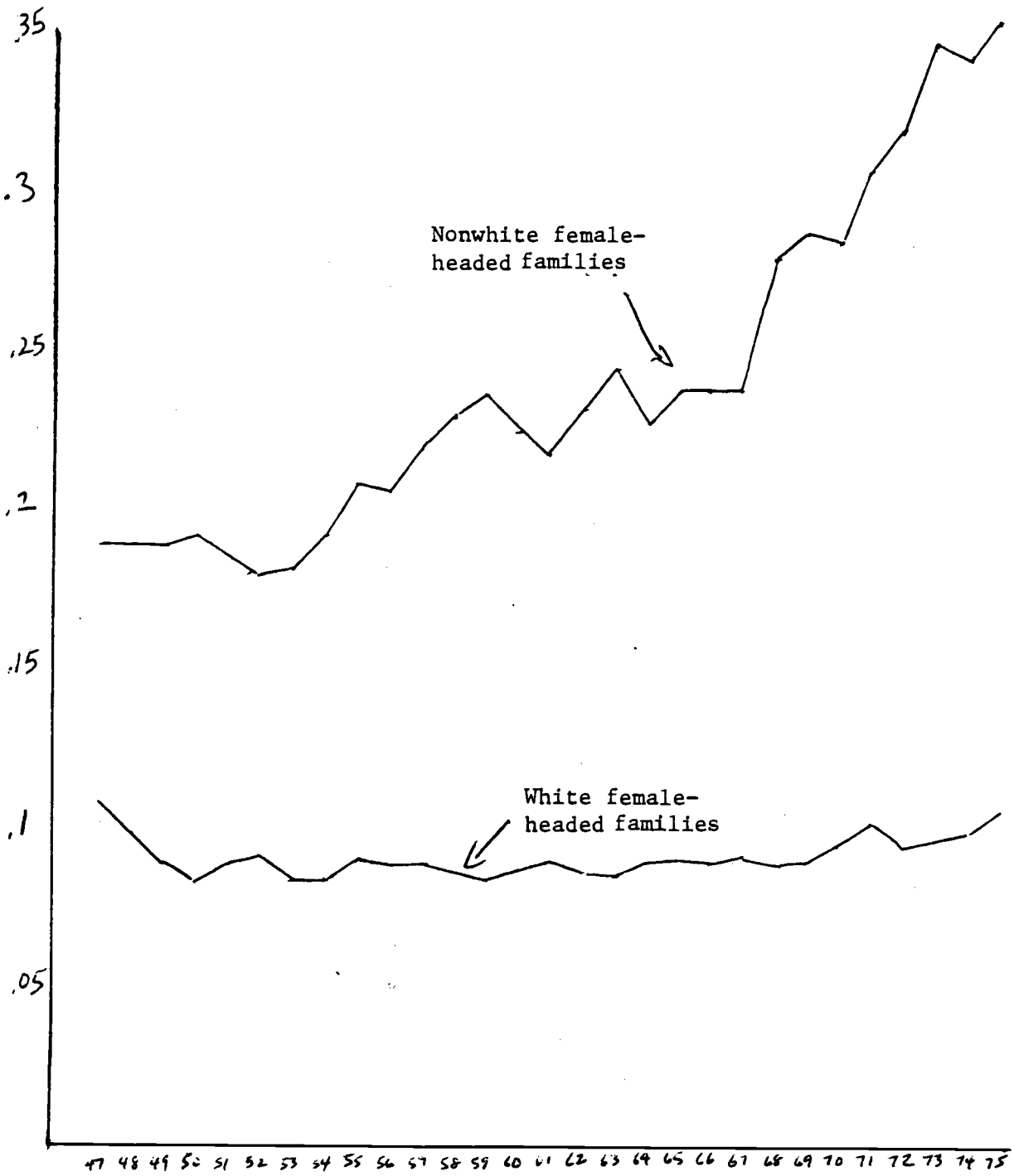


Chart 6 -- Proportions of female-headed families; by race..

black female relative income has been increasing, the level of female income is lower than the level of male income, and given the increasing fraction of female headed black families, this compositional effect serves to retard the relative growth of black family incomes.

(4) The U.S. aggregate data confound distinct regional patterns displayed in Charts 7, 8, 9 and 10. The key feature of these charts is that the growth in relative median income that occurred for both males and females in the aggregate data in the mid-sixties did not occur in the West or the Northeast. The data for the South reveal that the growth in the relative incomes of blacks began long before any of the recent civil rights legislation was passed. Only the graph for the North Central region displays any tendency for a jump in relative median income in the mid-sixties, and this occurs only for males, and virtually disappears by 1975. The acceleration in the aggregate male ratio that appears in the mid-sixties stems from the jump in the North Central ratio reinforced by a preexisting steady upward Southern trend.

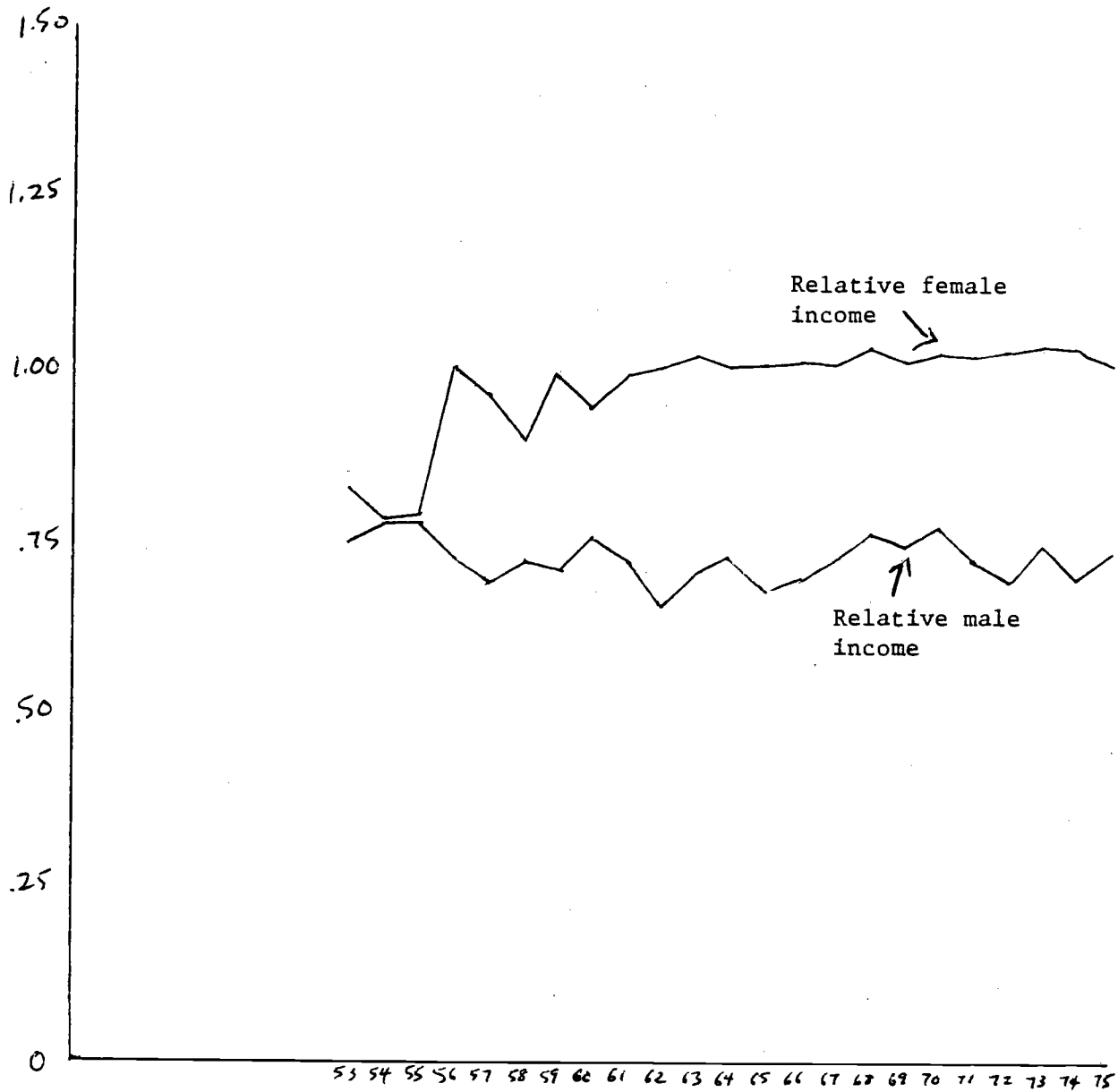


Chart 7 - Northeast relative nonwhite/white median incomes by sex.

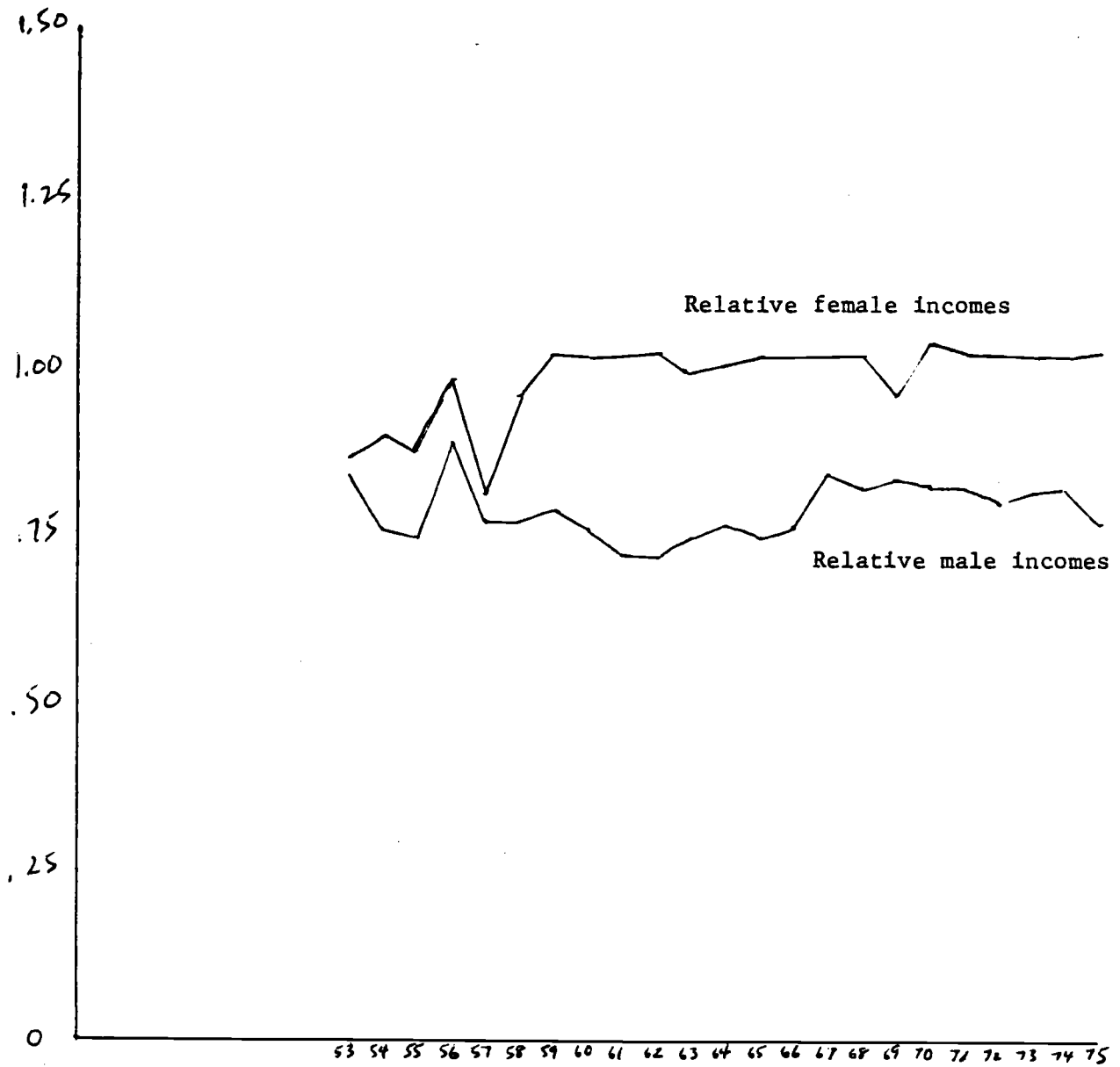


Chart 8 -- North Central relative nonwhite/white incomes by sex.

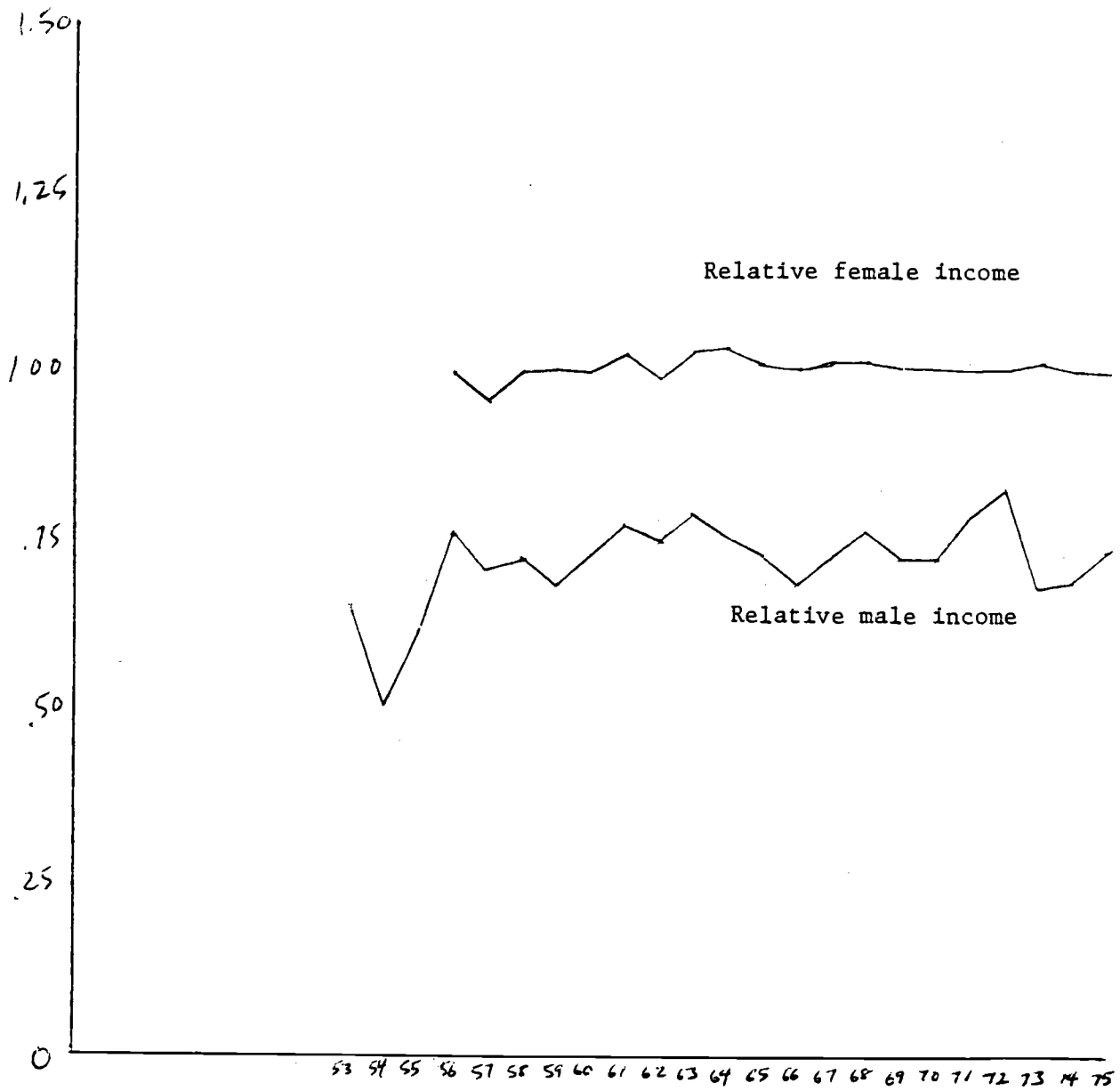


Chart 9 -- Western relative nonwhite/white incomes by sex.

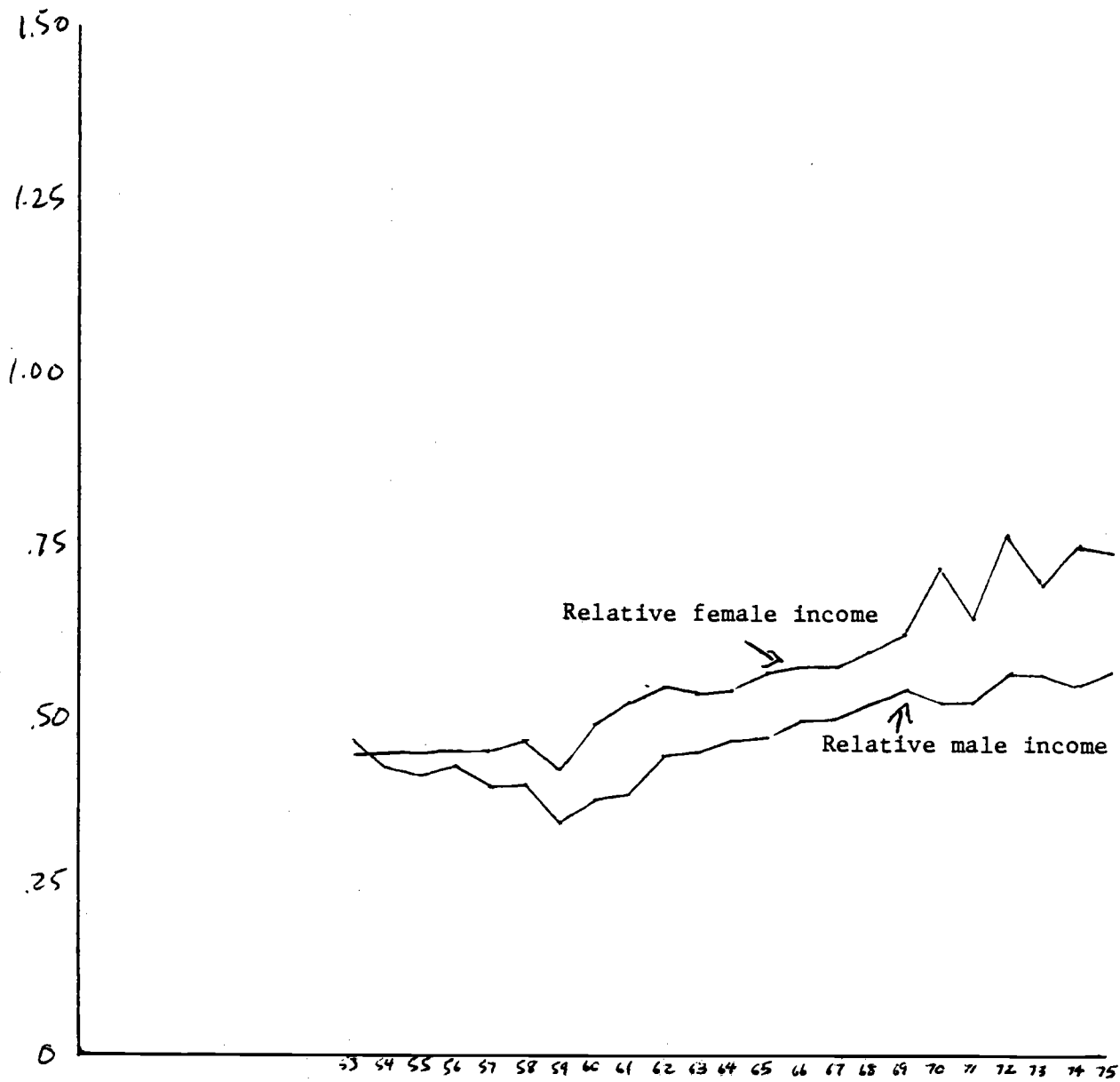


Chart 10 -- Southern Region: relative nonwhite/white incomes; by sex.

B. Findings from the Disaggregated Studies

A detailed review of all the disaggregated evidence on the determinants of racial wage differentials is beyond the scope of this paper. Yet, a discussion of the broad conclusions that emerge from these studies helps to clarify the interpretation of time series evidence and affords a more complete understanding of the aggregate trends.

The best available studies compare male earnings by race from large cross sections of data calculated in different time periods. Welch (1973) utilizes data from the 1960 Census and data from the 1967 Survey of Economic Opportunity. Smith and Welch (1976) and Haworth, Gwartney and Haworth (1975) use 1960 and 1970 Census data. The advantage of these studies is that they present a richer analysis of the determinants of earnings than is possible using aggregate data.

In his 1973 paper, Welch finds that black-white earnings differentials have been narrowed primarily through a reduction in wage differentials among younger workers. Not only are younger blacks more educated than older blacks, but the reward to their education is greater. And, racial differences in schooling levels and market payment to schooling have narrowed for younger workers. He utilizes this finding, and other evidence on increases in school expenditure for blacks, to conclude that increasing quality of schooling by cohort explains the reduction in aggregate racial wage differentials. By no means does this evidence exclude competing hypotheses. To the extent that affirmative action programs create a premium for blacks in the labor market, the effect of such programs would be greatest for the youngest blacks because firms are more likely to invest in younger workers (to harvest returns from workers with longer expected working lives) and because young workers, with long lifetime horizons, are more likely to invest in themselves and respond to increased labor market incentives.

In a later paper, Smith and Welch (1976) find less evidence for a young-cohort effect and more evidence for across-cohort improvements in earnings in explaining 1960-1970 changes in earnings. This contrast with Welch's previous findings may be due, in part, to differences in treatment of nonworking blacks in the two studies. Smith and Welch eliminate nonworking blacks from their 1970 sample while Welch (1973) did not eliminate this group in 1967. As noted in Chart 3, the labor force participation rate of prime age black males has been declining in the decade 1960-1970. It is plausible to assume that it is lowest wage blacks who became the non-participants. If this is so, Smith and Welch manufacture an apparent relative wage growth for older black workers which may not, in fact, exist. This bias induced by sample selection procedures accounts for the discrepancy between the findings of these two studies, and leads us to conclude that most of the measured relative wage gains have accrued to younger, more educated black workers. Evidence by Freeman (1977) supports this view.

A study by Haworth, Gwartney and Haworth (1975) corroborates the Welch (1973) view. These authors find that an important component of measured black wage gain is the retirement of older black workers from the labor force. This effect arises not because the black retirement rate exceeds the white rate, but because of the relatively lower quality of older workers relative to younger workers in the black population than is the case in the white population.

There is broad agreement on the other findings.

(1) Relative wage gains have been greatest in the South.

(2) Migration contributes little to measured relative wage growth in the sense that while regional racial wage differentials exist, the volume

of migration weighted by the regional differentials is a negligible component of aggregate racial relative wage growth.

(3) The contribution of federal government employment to the reduction in black-white wage differences is minor. While the black-white wage ratio is higher in the government than in the private sector, federal government employment has been relatively stable over the past decade. In addition, black-white wage ratios inside and outside of government have come closer together because of a greater narrowing in private sector racial differentials.

(4) The conventional wisdom that black-white income ratios decrease for more educated workers no longer applies and there is some evidence that ratios increase by education class.

(5) Growth in the average level of schooling among blacks is an important source of the observed decline in racial earnings differentials.

(6) Controlling for industry, age, education, and location variables, and for shifts in coefficients on these variables in earnings equations (interpreted as shifts in payments for productive attributes), more than half of the observed reduction in the black-white earnings differential remains unexplained. Haworth, Gwartney and Haworth allocate the residual to government activity and the like, but it is important to note that no direct evidence on this question exists in any of these micro studies, nor is there any information as to the timing of the observed changes within the period 1960-1970.

II. Key Features of Principal Antidiscrimination Programs

The main pillars of federal antidiscrimination policy in the private sector have been the Office of Federal Contract Compliance (OFCC) and Title VII of the 1964 Civil Rights Act which created the Equal Employment Opportunity Commission (EEOC). Many interesting institutional facts about these programs are recorded more fully elsewhere in this volume and need not be repeated here.

Until recently (1972), the EEOC has had limited enforcement powers and has acted primarily as a fact finding agency, investigating employee-initiated reports of wage and employment discrimination; and acting as a conciliation agency, seeking to reach out-of-court settlements between aggrieved workers and firms. With the exception of firms with fewer than 25 employees, firms not engaged in interstate commerce, the self-employed, and state and local governments, most employers were covered by the EEOC. By law, the EEOC was required to refer cases to state fair-employment practices commissions (where they existed), although the record shows that most cases so referred returned to the EEOC (Beller, 1974, p. 23). In the event of unsuccessful arbitration, workers could use the courts to bring individual suits against firms, with the EEOC serving as a friend of the court. In principle, the EEOC could invite the Justice Department to present cases, although no information is available as to how often this avenue was used. Since 1972, the EEOC has had the power to initiate litigation and has met with some success. However, official reports suggest that delays in litigation are rather lengthy.

The view of informed observers of the EEOC, best represented in the paper by Phyllis Wallace (1973), suggests that the agency is understaffed, limited in political status and legal power, and slow to process claims. These first-hand observations suggest that the EEOC is unlikely to have had a major impact—a view widely held in policy circles.

The OFCC was established in 1965 to administer an executive order forbidding discrimination by government contractors in any of their operations. This agency is responsible for the affirmative action time tables required of many firms and unions. Unlike the EEOC, the OFCC has

always had enforcement powers, although it has rarely used them. These powers include the right to cancel government contracts and to prevent firms from bidding on future contracts. Unlike the coverage of EEOC, the coverage of OFCC is restricted to firms that voluntarily agree to sell goods to the government; so, in principle, OFCC is not a compulsory program. Knowledgeable observers such as J. Jones (1976) and G. Ahert (1976) report on the ineffectiveness of the OFCC compliance machinery and foster suspicion of its contribution to eliminating measured black-white wage differentials even before any data are considered.

The principal vehicles for state antidiscrimination efforts have been the state fair-employment practices commissions. (For a complete description, see Landes, 1966.) These commissions have worked in cooperation with the EEOC and deserve consideration in their own right, if only because they have been studied more carefully than most federal programs. These analyses have served as prototypes for the analysis of federal programs.

The anecdotal evidence available on all of these programs and agencies leads one to suspect that they have had negligible effects on improving minority status. This sort of evidence has led some to dismiss the possibility that these agencies have had any important effect on minority status.

III. Measures of Program Impact and Their Interpretation

Before turning to the detailed evidence on the programs just discussed, it is helpful to consider what might be expected from them, the merits of various measures of their impact, and the limitations of the data and empirical techniques used to measure this impact.

First consider the expected consequences of these programs. As Merton (1936) noted long ago, the consequences of legislation often differ

from the stated intentions of legislators. Antidiscrimination programs may provide an example of this principle. Some economists have argued that affirmative action programs lead to an inefficient use of economic resources because they interfere with a preexisting optimum allocation of resources. If this is so, the real wages of all workers--black and white--may be reduced even though the relative wages of blacks increase.

As another example of Merton's principle, Landes (1966) and Beller (1974) both note that antidiscrimination programs such as EEOC and state fair-employment laws often contain conflicting provisions. Laws that lead to equality in wage rates by race may cause firms to substitute white labor for black labor, working counter to the employment provisions of laws that enjoin firms to hire proportionately more minorities in occupations in which they are under-represented. To insist that wage and employment provisions be enforced may be to insist on an economically inefficient use of resources.

There are several possible consequences of this inefficiency. Affected entrepreneurs may shut down their operations, or curtail investment in their plants or relocate to areas not covered by the regulations. The higher costs of operation necessarily imply higher prices for goods in affected industries and a smaller quantity of output demanded. This leads to lower total employment in sectors covered by the legislation although not necessarily lower minority employment. If a smaller minority employment occurs, the legislation enhances the status of blacks in surviving firms but may cause an exodus of other blacks from the covered sector. If all firms in the economy are covered, the black exodus is from the mainstream economy into a variety of nonmarket pursuits. Micro data on surviving firms which record improvement in the relative position of blacks is not inconsistent with macro data which record a reduction in black status relative to white status.

There are other examples of possible unintended effects of anti-discrimination legislation. Occupational racial quotas imposed in an attempt to move the black job distribution closer to the white job distribution may lead to a reduction in the employment of low skill blacks. Having too many blacks in low-skill occupations is just as much a violation of affirmative action as having too few blacks in high skill occupations. If quotas are met by firing unskilled blacks or adopting a preferential hiring pattern for low-skilled whites, the effect of such quotas will be to reduce both the wages and employment of unskilled black labor.

If, as Johnson and Welch (1976) suggest, firms meet the requirement for skilled black labor by promoting unskilled (and unqualified) blacks to skilled jobs, productive inefficiency may result. Moreover, if unskilled blacks are hired merely to "meet the quota", the long term consequence of such a policy may be to retard black incentives to acquire skills. Thus, such programs may result in a long run widening in the average level of acquired skills held by each race group.

The upshot of this discussion is that the evaluation of the consequences of a program should be separated from the evaluation of program effectiveness. The most effective program may have the worst consequences from the point of view of elevating the real income of blacks. Moreover, this discussion suggests that a variety of measures of program impact may be useful in order to assess both good and bad consequences of legislation.

Closely related to the analysis of program impact is the question of devising appropriate measures for evaluating programs. Most analysts who proceed within the demand-oriented framework of the economics of discrimination assume that relative wage rates or relative incomes are key policy goals. While wage ratios are important, they measure only one aspect of

the effect of laws on black status. Presumably the goal of any antidiscrimination program is to improve black economic welfare in both the short run and the long run. For either time frame, wage ratios may be a poor measure of black welfare.

This is an empirically important problem because the data from the last decade show that racial differences in real wage rates have widened while ratios have declined. A widening in the level of wage rates may lead to a widening in racial schooling and training differentials, and hence to a widening in long run earnings differentials. If the real return to schooling has increased for blacks, it has increased more for whites if the direct costs of schooling are the same for both race groups.

A fixation on the wage rates or the incomes of full time workers neglects the impact of legislation on employment, labor force participation and stability of employment for blacks. Surely one goal of policy is to increase black incomes and to integrate blacks into the main stream economy. Measurements of higher wage rates paid to working blacks confound intra-occupational narrowing of racial differentials with interoccupational advances. Measurements of both components are required in order to determine the sources of black improvement and the probability of permanence of the improvement. Patterns of unemployment differentials, labor force participation, school attendance and the like all yield information about the degree to which blacks are integrated into the economy.

Most studies surveyed below focus on the relative wage rates of blacks and exclude many important nonwage dimensions. Accordingly, these studies yield an incomplete picture of total government impact. In particular, all focus on short run effects, so that estimates of long run consequences of antidiscrimination legislation are not available.

In addition to the problem of the limited number of measures of black status that have been studied, there is also an important problem that arises from the fact that most of the antidiscrimination programs considered below do not cover all firms in the economy. The existence of a residual uncovered sector has important implications for the measurement of program impacts. Laws elevating black wages in a covered sector may depress wages in the uncovered sector because covered-sector firms tend to hire fewer blacks at the higher wages and release blacks to the uncovered sector, driving down wage rates there. Comparisons of the relative wages of blacks in covered and uncovered sectors at a given point in time of the sort utilized in many studies reported below lead to an overstatement of the economy-wide impact of such a program. One must resort to aggregate time series data to estimate true program impacts. On the other hand, if legislation causes the employment and wages of blacks to rise in the covered sector, wages also rise in the uncovered sector. In this case, cross sectional comparisons understate the contribution of legislation to relative minority status. Again, resort to time series evidence is required in order to assess the impact of legislation. Cross sectional evidence need not be consistent with the time series evidence.

Finally, it is important to note that the presence of laws, contract compliance reviews or government contract awards is not independent of the environment in which they occur. Yet most studies surveyed below ignore this point. Accordingly, estimated program impacts from these studies confound a pure policy effect with preexisting conditions that cause the policy to be effected.

OFCC provides a case in point. In this program, the government seeks to impose antidiscrimination provisions on its contractors. If the government also seeks to purchase its goods at the lowest price (say through the process of competitive bidding) it tends to select the least discriminating firms with higher proportions of black workers. Studies comparing "covered"

and "uncovered" firms may record a successful selection process by the government but convey no information on the real impact of the program on black status. In this case, measured OFCC effects overstate actual effects (Heckman and Wolpin, 1976).

The direction of this bias may be reversed. Suppose that all firms are alike and that the government runs a lottery to determine which firms are to be its contractors. All contractors must comply with the provisions of OFCC and are fully compensated for their increased costs. If, as much theory and evidence suggest, it is costly to hire and fire labor, all firms would have^{roughly}/the same racial composition in their work force. A cross sectional comparison of contractors with noncontractors, or a time series study of firms classified by contract status would reveal little or no program effect. Only an aggregate time series analysis would record shifts in black status induced by the contract compliance program.

This analysis also applies to macro studies. Antidiscrimination programs like EEOC and OFCC may well be the consequence and not the cause of reduced discrimination against blacks. If this is so, aggregate evidence on the effect of policy overstates the true impact of the policy.

The upshot of this discussion is that any analysis of policy impact is incomplete and purely descriptive until some account is made of how the policy came into existence. Virtually none of the estimates of program impact reported below can be used to estimate the expected impact of programs initiated without regard to the factors that cause policy to be effected.

IV. A Review and Reconsideration of the Evidence From
Aggregate Time Series on the Impact of Government Policy

A. Introduction

The work of Freeman (1973) is the most influential analysis of the impact of government policy on the status of blacks in the aggregate. In his paper, Freeman utilizes a time series of annual observations on relative black status to conclude that cumulative EEOC expenditure, interpreted as a proxy measure for the entire package of Federal antidiscrimination programs passed in the mid-sixties, raised the relative wages and earnings of blacks (as well as raising other indices that record relative racial occupational position). The shift in economic status due to government activity is proportionately greater for black women relative to white women than it is for black men relative to white men (Freeman, 1973, Table 6). Freeman's study has been widely cited as evidence for the success of recent government policy.

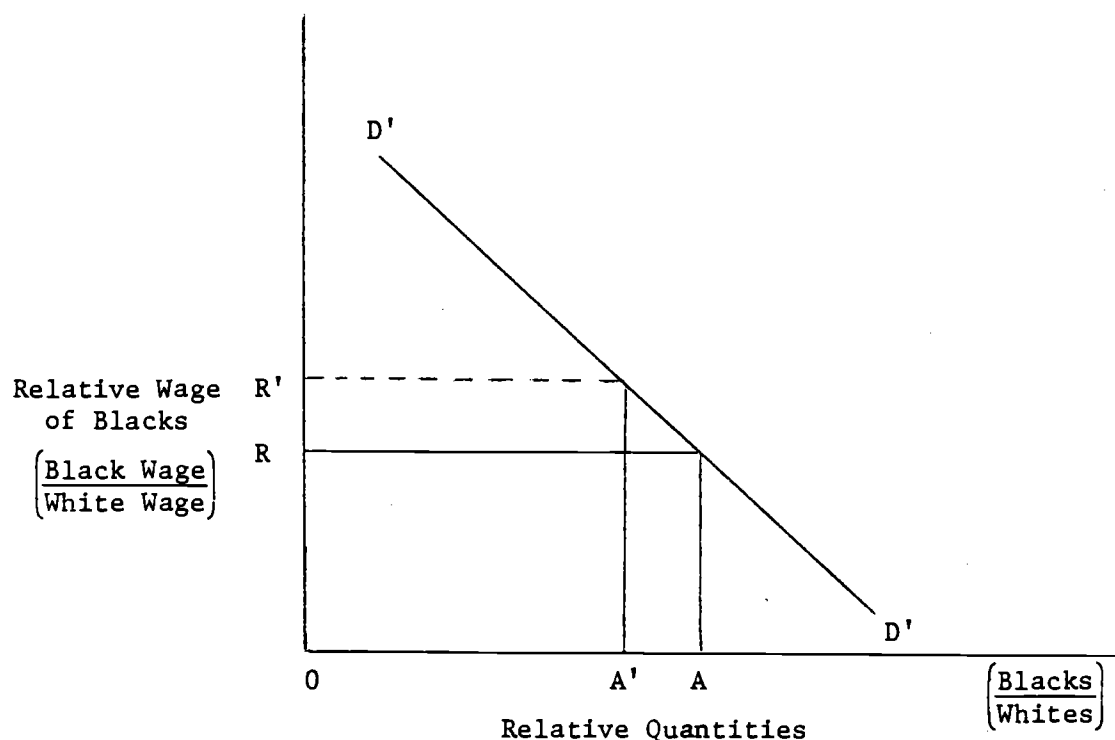
In this section, we examine Freeman's findings and argue that his conclusions are refuted when a properly specified model is estimated. Our analysis rests on the observation that if the demand for labor has shifted favorably toward blacks one would expect that the market participation of blacks relative to the market participation of whites would not have decreased. But, Chart 1 of Section I shows that it did decrease. And, as noted in Chart 3 of that section, the decrease is quite pronounced among prime age males, so that the aggregate decline is not solely a phenomenon of declines among peripheral workers.

There are important consequences of this decrease in participation. A general reduction in the relative quantity of black labor relative to white

labor should raise black relative wage rates. This is so as long as the two types of labor are not perfect substitutes in demand. There are several reasons advanced in the economics literature why blacks and whites need not be perfect substitutes in demand. The first reason, proposed by Gary Becker (1957) arises from employer tastes for discrimination. The greater the proportion of blacks in a population, the more likely it is that blacks must deal with ^{the} more bigoted employers and accordingly must settle for lower wages. The second reason, advanced by Finis Welch (1967) is that blacks and whites are imperfect substitutes because they possess different average skill levels and because workers of different skill are not perfect substitutes (Griliches, 1968). The third reason, due to Kenneth Wolpin (1974), arises from a model of firm uncertainty about worker quality. Even if the average black had the same quality at work as the average white, if the dispersion of quality in the black population differs from the dispersion of quality in the white population, blacks and whites are imperfect substitutes in demand.

The situation of less than perfect substitutability in demand is depicted in the Figure (next page) which displays the relative wage of blacks as a function of their relative quantities. A reduction in the relative quantity of blacks from A to A' should raise their relative wage from R to R'.

The cause of the reduction in the relative quantities of black labor may have additional consequences for the measured relative wage of working blacks. If, as is surely the case, there is some distribution of skills in the black population, it is important to know which blacks exit the labor force. In a companion paper, we present evidence that the reduction in the black work force came from the low wage black population which is a larger fraction of the total black population of workers than the low wage white



Figure

population is of the white population. Given the expansion of social welfare programs in the mid-sixties, coincident with EEOC and OFCC activity, proportionately more blacks participated in these programs than did whites--a result which is entirely consistent with the view that blacks and whites of identical demographic characteristics have the same propensity to participate in work reducing income transfer programs but that blacks occupy a lower position in the income distribution.

These transfer program induced changes in the composition of the black work force tend to raise measured average (and median) wage rates for working blacks relative to comparable measures for working whites. Such changes create the illusion of black progress as measured. Further, the increases in real transfer program benefits that occurred in the mid-sixties, help to explain the relative decrease in labor force participation for blacks.

B. Freeman's Analysis

Using regression analysis, Freeman estimates relative demand curve DD' under the assumption that it is horizontal, i.e., that blacks and whites are perfect substitutes in demand.¹ This is a strong assumption which turns out to be counterfactual. The determinants of relative wage rates for both men and women that are considered by Freeman are (1) a time trend, reflecting productivity growth and a shift in attitudes towards blacks (2) deviations from trend real GNP--a variable designed to capture the idea that tight labor markets are favorable for minorities (3) cumulative EEOC to the date of the observation on relative wage rates--a variable designed to capture the cumulative effect of all federal antidiscrimination activity on relative wages, /and (4) relative mean education levels--a variable designed to capture the effect of improvements in the quality of the black labor force relative to the white labor force. His empirical results show (1) that there has been a smooth upward trend in relative status/^{for both sexes} that is more pronounced for black women (2) that there is evidence that black relative status is procyclical (3) that federal activity has been instrumental in eliminating black-white wage differences and (4) that relative education has an important positive effect on relative minority status.

Flanagan (1976) was the first to systematically criticize the Freeman analysis. He noted that when relative education is introduced into Freeman's equation for males, the measured impact of federal activity vanishes although it does not vanish in the female equation. He conjectures that Freeman's

¹An alternative interpretation of Freeman's procedure is that he follows Ashenfelter (1970) in assuming that relative quantities are fixed for males. While this assumption is appropriate for Ashenfelter's sample period (1948-1966), it is not appropriate for a post-1966 analysis. Yet another interpretation of Freeman's equation is possible. Under this view, Freeman's relative wage equation is a function of the determinants of both the supply curve and the demand curve. If this interpretation is adopted, Freeman omits determinants of supply behavior. For a more extensive discussion of this issue, see Butler and Heckman (1977) or Appendix A.

finding that federal impact is weaker on the relative wages of full-time workers, as compared with its much stronger effect on relative median earnings of all workers, may reflect the fact that EEOC expenditure grew rapidly during a period of sustained low aggregate unemployment rates so that Freeman's measure of federal activity is nothing more than a proxy for unusually favorable macroeconomic conditions. To support this conjecture, Flanagan notes that in Freeman's work there is no evidence of a federal impact on relative full-time female earnings.

In an effort to examine these and other claims, we reworked the time series data, updating Freeman's analysis through 1974, experimenting with alternative models, and disaggregating by region. Because the last few years are ones of high unemployment, they provide a test of Flanagan's conjecture that Freeman's measure of federal activity is a proxy variable for the unusually low unemployment rates of the late sixties. The results of our empirical analysis, reported more fully elsewhere (Butler and Heckman, 1976) and briefly summarized in Appendix A to this paper, are as follows.

(1) Expansion of the data base to include years of high unemployment does not alter the conclusions to be drawn from Freeman's specification. Experimentation with the specification of unemployment variables results in little change in Freeman's original conclusions. From our analysis, we conclude that Freeman's federal activity variable does not proxy labor market tightness.

(2) When relative education variables are included in equations with relative median earnings as the dependent variable, measured EEOC effects remain for both sex groups, so that Flanagan's criticism is inappropriate.

(3) When a relative labor force participation variable is added to the basic Freeman model augmented to include a relative education variable,

and appropriate statistical methods are used to estimate the relative demand equation, there is no impact of measured federal antidiscrimination policy on relative wage rates for men or for women.¹ Relative labor force participation levels are sensitive to demographic variables, education, and benefit levels per recipient in federal transfer programs.

(4) An analysis of regional data on relative median incomes utilizing Freeman's own specification of the relative demand curve (i.e., assuming that this curve is perfectly elastic) reveals no impact of his measure of federal activity in any region but the South, although there is a weak effect on North Central region male relative median income. Given differential enforcement of antidiscrimination laws in the South in the mid-sixties, this finding appears to be prima facie evidence in support of Freeman's analysis. Yet an examination of Chart 10 reveals that the upward trend in relative black status in the South began in the late fifties--long before any federal antidiscrimination legislation was passed.² Indeed, when only the most recent 14 years of Southern data are used to fit the Freeman model, there is no measured impact of federal activity on the relative incomes of blacks.

The last two findings lead us to conclude that there has been some government effect on the status of blacks. But it is not the effect that Freeman sought to measure. In our view, the expansion of income transfer programs that occurred during the War on Poverty removed blacks from the labor force at a greater rate than it removed whites. This transfer program induced withdrawal of blacks lead to growth in the measured relative wage of

¹By "appropriate statistical methods" we mean a standard two stage least squares procedure in which the quantity of black labor relative to the quantity of white labor is regressed on appropriate instrumental variables. For a list of instrumental variables see Butler and Heckman (1977) or Appendix A.

²King and Marshall (1974), using different data also note this phenomenon.

working blacks relative to working whites, and accounts for the measured decline in relative labor force participation rates that is quite pronounced among prime age black males. This hypothesis also accounts for the observed slower growth in relative full-time wage rates since full-time workers are likely to be high-wage workers, and hence their measured wage rates are less likely to be affected by the compositional effect of transfer programs in eliminating low wage workers from the statistics.

C. A Brief Discussion of Vroman's Analysis

The work of Vroman (1973) has been interpreted as evidence in support of Freeman's work. Vroman analyzes a time series of cross sectional observations on workers (with positive earnings) registered with the Social Security Administration. His empirical results show a shift in the earnings of black earners relative to white earners for both sexes. The most pronounced shift occurs among southern workers although there is some evidence of a shift in relative male earnings in the North Central region as well. He does not trace the source of the shift to any particular agency. These findings appear to complement Freeman's analysis since the South was the locus of the greatest antidiscrimination efforts.

Before this conclusion is drawn, there are several points to note in Vroman's study. (1) His analysis is conducted only for workers, is not a panel study, and is thus subject to the same wage growth inducing selection bias arising from labor force withdrawal that plagues the Freeman (1973) and Smith and Welch (1975) studies. (2) While there is evidence of structural shift in southern earnings (based on a significant coefficient/^{in an earnings regression} on a dummy variable that is "one" for blacks after 1965), Vroman does not test the hypothesis that the shift occurred before 1965, as Chart 10 strongly suggests is the

case.¹ Indeed, if there has been steady improvement in black status in the South, one would expect Vroman's empirical result precisely because later earnings are higher than earlier earnings, and not because of any dramatic change in the condition of blacks in the South in 1965.

V. A Review of the Disaggregated Studies

A. Evidence from the Analysis of State Fair-Employment Practice (FEP) Commissions

In its original charter, the EEOC was instructed to cooperate with state fair-employment practices commissions and to refer cases to them when appropriate. Accordingly, it is of some interest to document the impact of these commissions on black labor market status. Studies of state fair employment practices commissions provide examples of some of the best available work that measures the impact of government programs.

The best work on this topic is a study by Landes (1968), who examined the impact of the wage and employment provisions of fair-employment practices laws. His analysis is conducted within the framework of the economics of discrimination developed by Becker (1957). State fair-employment laws are assumed to shift the demand curve for black labor relative to the demand for white labor (DD' in Figure 1 of Section IV). A crucial assumption in his empirical work is that both black and white workers are immobile across states, so that supplies of each type of labor are inelastically supplied to the market within each state.² A regression model is developed in which

¹1965 was the year that the EEOC commenced operation.

²Landes follows Becker (1957) in this assumption. It is as crucial to Becker's theoretical argument as it is to Landes' empirical analysis. If there is free migration across state lines, black-white wage differentials will be the same in each state if racial differences in psychic income are negligible. And, in the presence of migration there could be no measured effect of fair-employment legislation on the status of blacks measured in a cross section, even if fair-employment laws as a group raise black status in the aggregate.

alternative indices of black status are regressed on a dummy variable (which assumes the value of 1 when a state has a fair-employment law, and is zero otherwise) and on other control variables. The regression coefficient on the dummy variable is interpreted as a measure of the effect of the law on black status in states with such laws relative to black status in states without such laws.

Utilizing 1960 aggregates for the 48 contiguous states, Landes finds a quantitatively small (3 to 4 percent), weakly statistically significant improvement in the relative earnings of black males due to the enforcement of equal pay provisions of fair-employment laws. This effect is partly offset by the increased unemployment of blacks relative to whites which results from the enforcement of wage provisions of the laws. In regressions utilizing income and occupational position as dependent variables, Landes finds no effect of the legislation. This work provides evidence for conflicting effects of fair-employment practices legislation which cancel out in the aggregate and yield no measured effect.

In a later study, Ligget (1969) claims to find a strong effect of fair-employment practice commissions on black status. In our judgment, his evidence is too crude to be taken seriously. His analysis consists of a single rank correlation between an index of change in black occupational distributions relative to white occupational distributions (measured from the 1950 and 1960 Censuses) and an index of the strength of fair-employment commissions governing each location / ^{in his sample.} The data base is a scattered sample of cities and states. Both of his indices are suspect. The first index is defined to include only selected skilled occupations (those with average education exceeding 12 years) and thus excludes many black employees. The second index is derived from an ad hoc weighting of enforcement authority and budgets derived from the work of Norgren and Hill (1964).

Heckman (1976) reanalyzes the Landes data to determine whether or not the incidence of fair-employment practices laws by state can be related to economic variables, and if so, whether a more careful treatment of laws as socially determined events affects Landes' estimate of the impact of fair-employment laws on black status. He presents evidence that higher levels of education and a greater percent of unionism/ in a given state favor passage of fair-employment legislation. Using a new statistical technique, he doubles Landes' estimate of the effect of fair-employment laws on earnings (to 7.5 percent) and finds that the measured effect/ becomes strongly statistically significant. Unfortunately, Heckman does not present estimates for the other indices of black status employed by Landes, so that it is not possible to determine whether or not Landes' conclusions on the total effect of the law would be sustained in a more careful econometric analysis. Nonetheless, these new findings suggest that there may have been a greater impact of FEP legislation than previous analysts have measured.

B. Micro Studies of EEOC

Because the coverage of EEOC is so extensive, all micro studies of the impact of EEOC rely on a comparison between firms or states that are subject to different / amounts of "treatment" of EEOC activity. To the best of our knowledge, there is no study that compares the progress of blacks in EEOC-covered firms with black progress in uncovered firms.

Adams (1972) presents one of the first attempts to measure the direct impact of the EEOC on the relative employment of minorities in the period 1966 to 1971. During this period, the EEOC did not have the power to initiate any direct civil or criminal action in the courts--and so was limited to seeking compliance with Title VII through its conciliation efforts in cases

in which racial discrimination was alleged. In both the case studies and statistical analysis he presents, Adams confirms popular folk wisdom about the inefficacy of the EEOC's conciliation process. He tests for a direct effect of conciliation by pairing 26 "respondent" firms (those who signed a "successful conciliation" involving race as an issue in 1967 and 1968) with peer firms in the same two-digit industry and SMSA (Standard Metropolitan Statistical Area). He finds no significant improvement in the respondent group (relative to the peer group) in measures of relative employment or occupational position. In a regression analysis, Adams finds that the intensity of conciliation activity in an SMSA is an insignificant determinant of either change in SMSA minority share of employment or SMSA change in relative occupational position.

Adams' tests may not be conclusive for two reasons. First, he implicitly assumes that during the period of his study the EEOC focused on the enforcement of the employment provisions of Title VII. If, instead, the EEOC concentrated on enforcing the wage provision of the legislation and succeeded in improving the relative wages of blacks while not lowering relative black employment, (a question not examined by Adams), its impact would, in fact, have been substantial. Second, even if the EEOC were more concerned with discrimination in employment than with discrimination in wage rates during the period of his analysis, it is still possible that firms with a "successful conciliation" were initially the least discriminatory firms. Even though the conciliation process may induce recalcitrant firms to hire more blacks, if the EEOC chose to focus its conciliation efforts on firms already upgrading minority employees, the EEOC procedure for selecting firms would serve to explain why in 16 of the 26 pairs, "respondent" firms had a

higher proportion of minority employees in 1966 than did their peers and also why (in 11 of the 26 pairs) they had superior 1966 relative occupational indices. Thus, without a clearer notion of how respondent firms are chosen, it is not possible to make any inference about the impact of EEOC from Adams' analysis.

Utilizing the case study method, Wolkinson (1973) and Marshall (1976) reinforce Adams' pessimistic / conclusions regarding the impact of EEOC's conciliation and minority litigation efforts. Examining 75 instances of alleged union discrimination, Wolkinson finds that after settlements are reached, they often provide inadequate relief to the workers involved, or are not adhered to by the unions involved. These cases arose prior to the 1972 law permitting the EEOC to initiate litigation proceedings, and the author attributes the lack of measured impact to the absence of any EEOC enforcement power. For the construction industry, Marshall (1976) finds that Title VII suits are always long and costly in terms of lawyer years, and that the effects of the court proceedings are empirically difficult to distinguish from the effects of concurrent civil rights activity. While litigation appears to have had a minimal direct impact, Marshall asserts that the court cases had a substantial impact in furthering the "hometown" plans in areas involved in litigation.

Kidder (1972) analyzes data from the textile industry in North and South Carolina in an attempt to determine whether it was federal antidiscrimination programs or a tightening of the labor market that was responsible for the observed increase in black employment in that industry in the 1960's. Like Adams, Kidder assumes that the EEOC concentrated on enforcement of the employment provision of Title VII. She finds a negative correlation between

EEOC charges and subsequent increases in black employment, and estimates that the presence of a government contract in a firm insignificantly affects black employment.

The principle thrust of her analysis is directed toward disproving the tight labor market hypothesis. She finds, for instance, that the major black employment breakthrough in the textile industry came in 1963-65, before the industry's overall unemployment rate dropped in 1966 (and before EEOC was established). However, as Waisglass (1972) notes, the relevant unemployment rate is not the textile industry's rate, but that of the labor markets in the Carolinas, which started falling in 1962.¹

The most sophisticated of all of the micro EEOC studies is the unpublished Ph.D. thesis of Andrea Beller (1974). This work explores the effect of charges filed on both the relative employment and relative wages of black males. Beller's study is the first to disentangle conflicting wage and employment effects of Title VII legislation and is a clear improvement on previous work. Using state data drawn from the 1970 Census matched with EEO-1 data assembled by Ashenfelter and Heckman (1976), she finds a net negative impact of charges filed with the EEOC on the employment of blacks

1

OVERALL UNEMPLOYMENT RATES

	59	60	61	62	63	64	65	66	67	68	69	70
North Carolina	4.1	4.4	5.5	3.7	3.6	3.3	2.5	1.8	2.1	1.7	1.5	2.4
South Carolina	3.4	3.5	4.5	3.1	3.2	2.8	2.2	1.6	2.3	1.8	1.6	2.7

SOURCE: Statistical Abstract (various issues).

relative to whites. A strong negative effect on employment that results from enforcement of Title VII wage equality provisions (measured by the number of charges filed with wage inequality as an issue) swamps a weak positive employment effect that results from enforcement of Title VII employment provisions (measured by the number of charges filed with employment discrimination as the principle issue).

Beller also estimates the impact of charges filed on black relative wage rates. Her estimates display the same kind of offsetting behavior found in the relative employment regressions; a greater number of wage charges filed results in an increase in measured relative wages while a greater number of employment charges filed results in a measured decrease in wages. The two effects cancel. In both studies, she explicitly recognizes the problem of mutual causation between charges and outcomes and presents methods for removing the bias that results from reverse causation.¹ The conclusion from both analyses is that total charges filed with the EEOC, interpreted as proxies for the enforcement of the wage and employment provisions of Title VII, had a slight negative impact on relative black employment and a negligible (and possibly perverse) impact on black relative wages. Her work thus provides some support for ^{the} notion that the cure of antidiscrimination legislation may be worse than the disease, and that the consequences of conflicting provisions in legislation may be quite perverse.

C. Micro Studies of OFCC

In contrast with the diversity of approaches and data used to measure the impact of EEOC, most micro studies of the impact of OFCC use the same

¹ However, her solution to this problem is based on the somewhat questionable assumption that the presence of a regional EEOC office in a state affects the volume of charges filed in the state.

methodology and the same source of data: the annual EEO-1 tapes. These tapes contain information on employment and occupational distributions for all major race-sex groups in firms covered by EEOC. No information is available on wages or salaries, but whether the firm has a government contract (and hence is covered by OFCC) is given, although the size of the contract and its contribution to a firm's total sales is not. Due to the absence of any measure of wages, analysts of OFCC have focused on the impact of the presence of a government contract (measured by a dummy variable) on the employment and occupational position of minorities within firms.

Table 1 presents a summary of the principal empirical results from the four principle studies of OFCC. The authors and sample period for each study are given in Column 1.¹ Control variables used in the regression analysis performed in these studies are given in Column 2, and estimates for the index of minority status indicated at the top of each column are recorded in the final three columns. The key to the right of the table defines the variables.

All authors claim to estimate demand equations for firms, although Burman supplements his model with supply variables that contribute nothing to the fit of his equations. Ashenfelter and Heckman, and Heckman and Wolpin interpret their estimates within a dynamic adjustment framework and claim to estimate both long-run and short-run effects.²

¹With the exception of the sample used in the Heckman-Wolpin (1976) study, all samples are national. (Heckman and Wolpin use data from the Chicago consolidated SMSA.) The Ashenfelter-Heckman (1976) paper and the Burman (1973) paper use samples from the late sixties when OFCC was newly established, while the Goldstein-Smith (1976) and Heckman-Wolpin studies cover a later period.

²Long-run effects are measured under the implicit assumption that current prices and wages prevail in the future, and hence are inaccurate to the extent that the stationarity hypothesis is invalid.

The first three papers treat the presence of a government contract as an exogenous event, beyond the control of the firm (or the government); the Heckman-Wolpin paper presents evidence that contract status is determined by firm (and government) policy, and methods are devised to circumvent the bias that results from mutual causation between receipt of a contract and a firm's policy toward minorities.¹

As recorded in the Table, Ashenfelter and Heckman find that in the short run government contractors raise the employment of black males relative to white males 3.3 percent more than nongovernment contractors do, while in the long run this effect is estimated to be 12.9 percent. These numbers bracket Burman's estimate (which should be interpreted as an average of short-run and long-run effects), and are in rough agreement with the estimates of Heckman and Wolpin for a later period. The only discordant estimate in this table is that of Goldstein and Smith, who find evidence for a statistically insignificant decline in black status among government contractors.²

Unlike the authors of the other papers, Ashenfelter and Heckman attempt to measure the economy-wide impact of OFCC on the relative employment of blacks. (See the second pair of numbers in Row 1, Columns 3-5.) These estimates are obtained from the first pair of numbers by weighting each number by the proportion of workers covered by OFCC (50 percent in their sample). These estimates have meaning only if the measured relative impact is directly related to the absolute (economy-wide) impact, and it clearly need not be.

¹The Heckman-Wolpin paper is also the only paper in this group to account for serial correlation in the presence of lags in firms' adjustment to long-run equilibrium.

²This may be due to their inclusion of a compliance review variable along with a government contract variable. Even if the former variable belongs in the equation, the estimated impact of contract status, holding compliance reviews constant, would be expected to be less than if a compliance review variable were excluded.

TABLE

Study	Independent Variable	Relative Employment	Relative Occupational Position	Key
<u>Ashenfelter-Heckman (1976)</u> 40,445 firms from EEOC-1 Data, 1966-1970 males only	Presence of contract plus region dummy, size of firm, log employment changes, SMSA, total firm employment, lagged value of dependent variable	Black males only 3.3% ↑ SR 12.9% ↑ LR	.2% ↑ SR .4% ↑ LR .1% ↑ SR .2% ↑ LR	P_j = wage in occupation j B_j = blacks in occupation j W_j = whites in occupation j
<u>Burman (1973)</u> 7,154 firms from EEOC-1 Data, 1967-1970	Presence of gov't. contract plus compliance contract, change SMSA empl., change SMSA popula., change establ. empl., change establ. skill-mix, change relative black education, change SMSA prop. black change in percent union	5.6% ↑ black males 12.9% ↑ black females $\Delta \ln B/W$	2.5% ↑ black males 9.2% ↑ black females $\frac{\Sigma P_j (B_j/B)}{\Sigma P_j (W_j/W)}$	B = blacks W = All whites
<u>Goldstein-Smith (1976)</u> 74,563 firms in 1970-72 from EEOC-1 data	Presence of gov't. contract plus region dummy, size of firm, log employment changes, SMSA, total firm empl., compliance review	.2% ↑ males 3.6% ↑ females $\Delta \ln B/W$.42% ↑ males 3.9% ↑ females $\frac{\Sigma P_j B}{\Sigma P_j W}$	
<u>Heckman-Wolpin (1976)</u> 3,677 Chicago area firms from EEOC-1 data for 1970-73	Gov't. contract plus white collar, increase in firm empl., SIC one-digit code, previous year empl., by occupa. group, interaction of contract: with white collar, with inc. in empl.	$\frac{LR}{SR}$ 10.4% ↑ 8.9% ↑ black males 1.7% ↑ 1.7% ↑ black females $\Delta \ln B/W$		

Cross sectional comparisons between contractor and noncontractor firms are intrinsically uninformative on the question of OFCC's economy-wide impact on the position of blacks. The best one can hope to measure is the differential between contractor and noncontractor firms. Without more information on the structure of labor markets than is available from any of these studies, these estimates yield no information on the true impact of OFCC on the relative position of blacks in the economy. A 3.3 percent differential can arise either because blacks are reshuffled among firms or because of an influx of blacks into the labor market. In presenting their calculation of economy-wide impacts, Ashenfelter and Heckman implicitly assume the latter interpretation, although their estimated relative impact is consistent with either interpretation.

The results for black female employment relative to white female employment, and the results for relative occupational position for both sexes suggest either small favorable or negligible negative impacts.¹ The same criticism levied against the black/^{male} employment indices also applies to measurements of these indices. Cross sectional estimates are intrinsically ambiguous and are neither evidence for nor evidence against the role of OFCC in promoting the status of blacks. From these studies, we can conclude that OFCC has altered the behavior of firms or at least has sorted out firms by their treatment of minorities. We cannot conclude from these studies that the OFCC has served to elevate black status, nor can we (as do Smith and Welch, 1976) conclude that it has not.

Other pieces of evidence suggest that the OFCC may have had some effect on the behavior of firms. Ashenfelter and Heckman note that segregated

¹When a coefficient is negative, it is usually statistically insignificant.

white contractors were more likely to integrate than were segregated non-white contractors. Heckman and Wolpin note that government contracts were awarded to less discriminatory firms. But none of those studies provides a reliable quantitative measurement of the direct impact of OFCC on black status.

Another shred of evidence on this issue is contributed by Smith and Welch (1976) in their analysis of 1960 and 1970 Census data. In their excellent analysis of male wages, they find evidence of a differentially greater increase in black-white wage differentials in industries more dependent on the government, and hence more liable to intervention by OFCC.¹ This is so because both black and white wages are higher in the covered sector than in the noncovered sector, but white wages are proportionately higher than black wages.

An increase in black real wages in the covered sector may boost black real wages in the uncovered sector more than white real wages if the mobility of blacks to the covered sector is greater than the mobility of whites to the covered sector, and hence (perversely) understated.²

Summary and Conclusions

In this paper, we review recent evidence on the impact of anti-discrimination legislation on the labor market status of black Americans. Two types of evidence are considered: analyses of aggregate time series of relative black status and microeconomic analyses of the impact of particular programs.

Freeman's influential time series evidence on governmental impact on black status is shown to be less convincing than is generally assumed in popular discussions. Macro relative wage and income data are quite sensitive

¹The measure of dependency is the fraction of industry sales to the federal government.

²This conclusion assumes that minority employment in covered sectors increases. If it decreases, cross sectional estimates overstate the true policy impact.

to the relative number of blacks in the labor force, and the composition of the black work force. As the relative number of blacks in the work force declines, and as low wage blacks are siphoned out of the labor force by transfer programs, measured relative wages of blacks tend to rise. Such growth in relative black status has nothing to do with a lessening of discrimination against blacks. We present some evidence (in Appendix A) that part of the observed decline in black labor force participation is due to increased benefit levels of government transfer programs. This analysis suggests that Freeman's measured impact of government programs on black status confuses supply shifts related to federal transfer program activity with demand shifts related to federal antidiscrimination activity. Government impact on the supply of labor explains the rise in the relative wage rates of blacks that occurred at precisely the same time that relative labor force participation rates for prime age black males declined, and labor force participation rates for black women became stationary, after a long period of growth.

The best available micro studies document considerable evidence of governmental antidiscrimination activity at the firm level but are silent on the question of governmental impact on black status. Some studies show weekly perverse effects of legislation. For example, enforcement of both wage equality and employment provisions of Title VII legislation appears to result in lower relative employment for blacks (Beller, 1974). Other studies that appear to show favorable impacts of government activity are shown, on closer scrutiny, to be inherently ambiguous as to their true interpretation.

Having stated essentially negative conclusions, it is important to note that there is evidence that the labor market for young black Americans

has dramatically improved in recent years (Freeman (1977), Smith and Welch (1976)). If antidiscrimination programs are effective at all, they are likely to have their largest impact on young workers. This is so because firms prefer to train younger workers--with longer expected working lives--and younger workers are able to more fully adjust to revised expectations of labor market opportunity than are older workers, who would find retraining less profitable. The analysis of Freeman and Smith and Welch, as well as the sharp rise in black school enrollment rates in the mid-sixties, suggests that antidiscrimination legislation may have had a real impact.

But the data are too crude to support this conclusion, especially for the aggregate of black workers. At our current level of understanding, the most honest summary of existing knowledge of government impact on the status of blacks is to say that there is no evidence that government antidiscrimination policy has had any impact on elevating black-white wage differentials.

APPENDIX A

A Brief Discussion of the Time Series Evidence

In this appendix, we present a brief account of an extensive analysis of the time series of black-white income differences reported more fully elsewhere (Butler and Heckman, 1976). This work does not claim to offer a complete explanation of the aggregate data but does present some new results, and causes us to discard some old results, due to Freeman, that have gained widespread currency in popular discussions.

The data discussed here are a sample of annual observations from the Current Population Survey for the period 1948-1974. We thus use three more observations than Freeman (for the years 1972-1974). The expansion of the data base does not alter the conclusions to be drawn from his model. Rather, the specification of the relative demand curve is the important point. In order to focus on essential issues, we are faithful to Freeman's functional forms and definitions of variables except for some inessential changes noted in Appendix B. That appendix also gives a complete description of data sources and methods used to construct variables.

Table A-1 reports our regression results for males while Table A-2 reports regression results for women. The key before these tables defines the variables used in the regression analysis. The numbers reported in each column are the regression estimates of the effect of the variable listed at the top of the column on the dependent variable listed in the appropriate row. "t" statistics are recorded below each regression coefficient.

To understand these results, consider the first row of Table A-1 which reports empirical estimates of one of Freeman's models (1973). Reading

across the columns, the effect of an additional year is to raise relative income by $1/2$ percent; a one percent deviation from the trend of real GNP raises relative income by .72 percent, while an additional 10 percent of accumulated real EEOC expenditures per nonwhite worker (this represents about 21.5 million dollars in 1975) increases relative median income by .34 percent.

Key to Tables A-1 and A-2

(For a more precise definition of variables, see Appendix B.)

Relative Median Income	- the logarithm of the ratio of the median income of all black males to the median income of all white males. This measure includes earnings and transfer payments.
Relative Median Earnings	- a similar measure for the earnings of <u>workers</u>
Relative Median Wage/Salary Income for Year Round Full-Time Workers	- self explanatory
C	- an intercept term
TIME	- is a linear time variable initialized at 1 in 1947
DGNP	- the log of deviation of/real GNP from a fitted trend line (measured in 100 billion dollar units)
RLFMT	- the ratio of the labor force participation rate of blacks to the labor force participation rate for whites (defined separately for each sex)
ED	- the ratio of the median education of nonwhites to the median education of whites (defined separately for each sex)
AID	- the average real monthly payment <u>per recipient</u> to individuals participating in the aid to families with dependent children (AFDC)
UNCOMP	- the average real monthly unemployment compensation per beneficiary (units: dollars)

TABLE A-1

RELATIVE WAGE/INCOME REGRESSIONS FOR MALES
(t-statistics in parentheses)

Dependent Variable	Independent Variable										R ²	D.W.
	C	TIME	DGNP	EEOC	RLFMT	ED	AID	UNCOMP				
1. Relative Median Income, 1948-74	-.689 (30.6)	.005 (3.07)	.726 (2.63)	.034 (2.76)694	2.13
2. Relative Median Income, 1948-74	-.629 (15.95)	-.003 (-.689)	.614 (2.30)	-.018 (-.577)	-4.12 (-1.75)	2SLS	2.05
3. Relative Median Income, 1948-74	-.380 (1.44)	-.003 (-.494)	.563 (1.94)	.032 (.506)	.924 (.160)	.662 (.954)	2SLS	2.51
4. Relative Median Income, 1948-74	2.49 (1.05)	.016 (2.17)	.243 (.934)	-.003 (-.247)522 (2.73)	-.858 (-2.86)820	2.60
5. Relative Median Wage/Salary Income, 1948-74	-.585 (-27.26)	.004 (2.84)	.438 (1.66)	.044 (3.81)734	2.37
6. Relative Median Wage/Salary Income, 1948-74	-.539 (-13.2)	-.002 (-.451)	.348 (1.26)	.003 (.088)	-3.28 (1.35)	2SLS	1.94
7. Relative Median Wage/Salary Income, 1948-74	-.594 (-2.17)	-.002 (-.447)	.359 (1.20)	-.008 (-.128)	-4.41 (-.728)	-.148 (-.206)	2SLS	1.77
8. Relative Median Wage/Salary Income, 1948-74	4.16 (1.66)	.019 (2.51)	.038 (.139)	.020 (1.33)218 (1.07)	-.833 (-2.61)804	2.52
9. Relative Median Wage/Salary Income, Year round, full time, 1955-74	-.527 (-21.69)	.006 (4.22)	-.257 (1.57)	.015 (2.06)823	2.27
10. Relative Median Wage/Salary Income, Year round, full time, 1955-74	-.498 (-10.5)	.003 (.681)	-.274 (-1.57)	.002 (.133)	-1.18 (-.748)	2SLS	2.15
11. Relative Median Wage/Salary Income, Year round, full time, 1955-74	-.168 (.288)	-.012 (-1.17)	-.483 (-2.24)	.034 (1.28)	2.03 (.81)	1.16 (1.63)	2SLS	2.69
12. Relative Median Wage/Salary Income, Year round, full time, 1955-74	-.101 (-.051)	.006 (.920)	-.392 (-2.12)	.006 (.605)289 (1.40)	-.285 (-1.05)850	2.59

TABLE A-2

RELATIVE WAGE/INCOME REGRESSIONS FOR FEMALES
(t-statistics in parentheses)

Dependent Variable	C	TIME	DGNP	Independent Variable			AID	UNCOMP	R ²	D.W.
				EEOC	RLFFT	ED				
1. Relative Median Income, 1948-74	-9.21 (-28.70)	.032 (14.65)	-2.78 (-7.05)	-.002 (-.100)				.941	2.05	
2. Relative Median Income, 1948-74	-1.47 (-3.29)	.044 (4.51)	.015 (.031)	.028 (.911)	1.43 (1.22)			2SLS	2.23	
3. Relative Median Income, 1948-74	-3.75 (-1.27)	.114 (1.27)	.673 (.633)	.052 (1.02)	4.43 (1.08)	-2.29 (-.790)		2SLS	2.40	
4. Relative Median Income, 1948-74	-4.37 (-2.25)	.010 (.799)	.058 (.128)	.009 (.393)			.159 (.481)	.899 (1.74)	2.41	
5. Relative Median Wage/Salary Income, 1948-74	-1.05 (-32.99)	.030 (13.57)	.532 (1.36)	.073 (4.25)				.953	1.67	
6. Relative Median Wage/Salary Income, 1948-74	-1.13 (2.72)	.031 (3.46)	.574 (1.25)	.077 (2.67)	.206 (.189)			2SLS	1.73	
7. Relative Median Wage/Salary Income, 1948-74	3.19 (1.40)	-1.00 (-1.46)	-.668 (-8.16)	.032 (.815)	-5.46 (-1.73)	4.32 (1.94)		2SLS	1.92	
8. Relative Median Wage/Salary Income, 1948-74	1.04 (.352)	.038 (4.24)	-.164 (-.505)	.013 (.742)			1.00 (4.20)	-1.09 (-2.91)	2.01	
9. Relative Median Wage/Salary Income, 1955-74 (full time, 1955-74)	-.775 (-19.63)	.023 (10.06)	-.478 (-1.79)	.022 (1.87)				.947	1.64	
10. Relative Median Wage/Salary Income, 1955-74 (full time, 1955-74)	-.448 (-1.21)	.017 (2.10)	-.536 (-1.87)	.002 (.073)	-.876 (-.887)			2SLS	1.39	
11. Relative Median Wage/Salary Income, 1955-74 (full time, 1955-74)	1.08 (.709)	-.029 (-.656)	-.783 (-1.92)	-.016 (-.458)	-2.97 (-1.29)	1.42 (1.05)		2SLS	1.36	
12. Relative Median Wage/Salary Income, 1955-74 (full time, 1955-74)	-2.79 (-9.97)	.013 (1.57)	-.767 (-2.98)	.001 (.055)			.824 (2.86)	-.368 (-.968)	2.22	

The models that correspond to those reported in Freeman's original analysis are reported in rows 1, 5 and 9 in each table. With the exception of the equation for the relative median income of females (row 1 of Table A-2), our empirical results support Freeman's contention that cumulated EEOC expenditure, interpreted as a proxy for federal antidiscrimination activity, elevated the labor market status of blacks.

Suppose, however, that the assumption that blacks and whites are perfect substitutes in demand is relaxed. This can be done by adding a relative quantity variable (the ratio of blacks to whites) to the list of variables in the relative demand curve. This modification permits the demand equation to resemble DD' in Figure 1. If relative quantities are added to the model, certain technical difficulties in estimation arise. These are resolved by use of standard statistical methods.¹

There are dramatic empirical consequences of relaxing Freeman's assumption about factor substitution. To see this, look at rows 2, 6 and 10 in each table. In all but one case, in the modified relative demand equations the measured impact of federal activity becomes quantitatively negligible and statistically insignificant. Moreover, in the one aberrant case (row 6 of Table A-2), the measured impact of EEOC become statistically insignificant when relative education level (ED) is added to the equation (row 7 of Table A-2). There are strong prior reasons for including this variable in the relative demand function to adjust for racial changes in skill endowment.

The empirical results for the male equations show that there is reasonably strong evidence that the relative male demand curve is downward

¹The technique is two stage least squares. The instrumental variables are EEOC, DGNP, AID, UNEMP and ED.

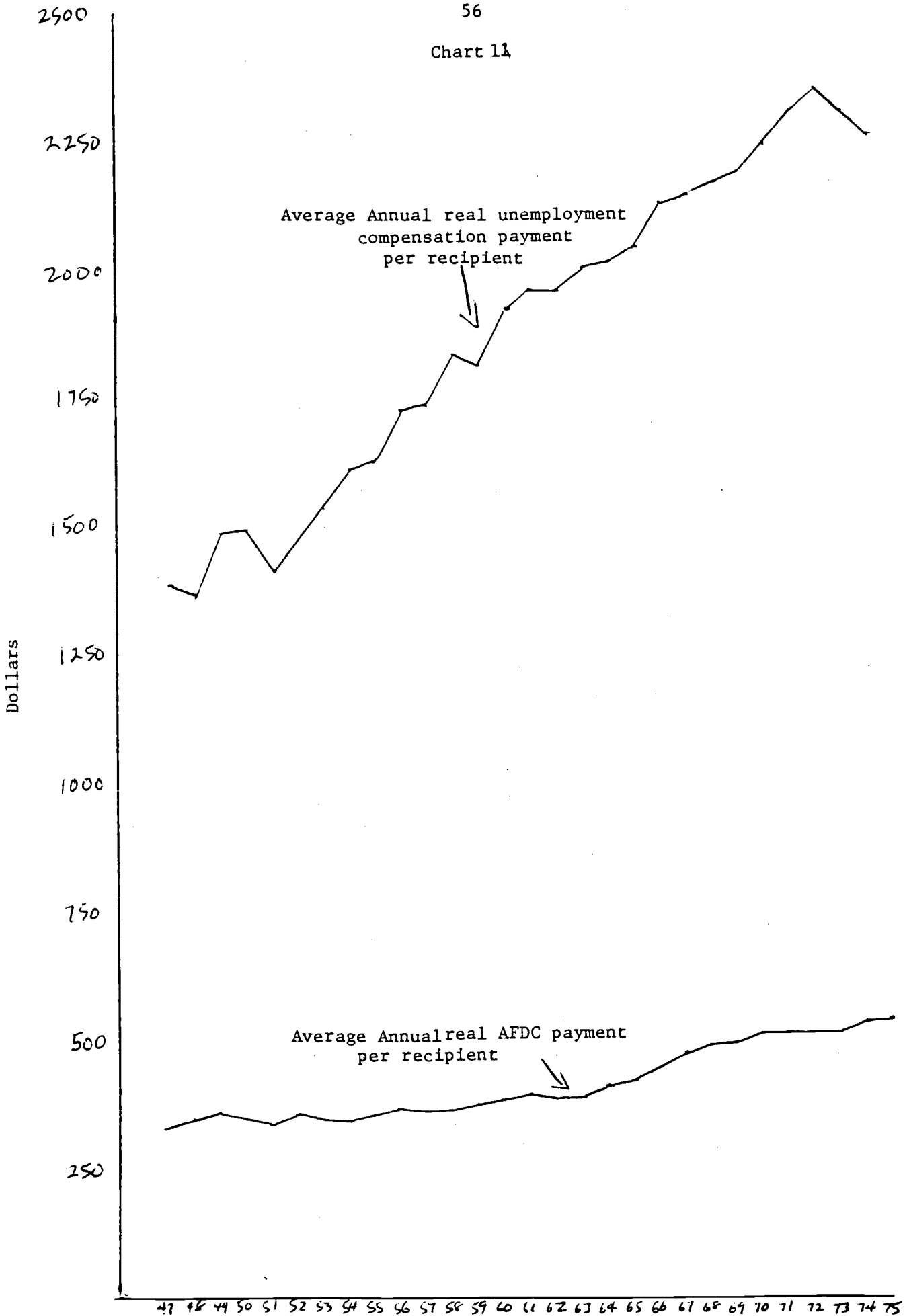
sloping . When relative education is added to the female equation (rows 3, 7 and 11 of Table A-2), the same conclusion carries over to the female results.¹

An alternative way of telling the same story is given in lines 4, 8 and 12 of each table. The empirical results reported there are estimates of the dependence of measured relative wages and incomes on all the factors that determine both relative demand and relative supply in the system (i.e., in the econometric jargon, these are "reduced form" estimates). When account is taken of the real value of transfer payments per recipient, there is no measured effect of federal antidiscrimination activity on relative wages and incomes.

In results not reported here (see Butler and Heckman 1976), we discuss the relationship between relative labor force participation rates and transfer program benefits. Rising levels of AFDC payments help to explain the observed decline in black male headship rates (see Chart 6, p. 12 and Chart 11, p. 56). As black male participation in family life declines, so does their labor market participation--married males of any race are more likely to participate in the market than single or unattached males. Black female participation in AFDC programs serves to retard growth in their labor force participation. More generally, the AID variable used in our analysis may be viewed as a proxy for the entire package of social welfare programs, including AFDC, that favored low income workers, and hence blacks, and resulted in declines (or decelerations) in black labor force participation rates. Even without a marital disruption effect, income transfer programs raise the minimum wage that recipients will voluntarily accept in order to work, and hence an expansion of the benefit levels in social programs leads to a reduction in work activity, and an increase in relative black wage rates

¹Precisely the same results arise if relative employment, relative

Chart 11



if blacks and whites are imperfect substitutes in demand. To the extent that social programs selectively eliminate low income blacks from the labor force, and hence the measured statistics, they accentuate the effect of movement up the relative demand curve induced by a reduction in the number of blacks relative to the number of whites.

In contrast with the effect of most income transfer programs, increases in the real level of unemployment compensation benefits (UNCOMP) are expected to raise the minimum acceptable wage required to perform market work more for whites as a group than for blacks. This is so because proportionately more whites work in sectors covered by unemployment insurance than do blacks. Tables A-3, A-4 and A-5 provide evidence in support of this argument.

Table A-3 records the importance of income, by type, as a fraction of the total income received by persons 14 years and older. For both sexes, the proportion of total income due to welfare (WEL) is at least five times larger for nonwhites than it is for whites. Table A-4 shows the same pattern with respect to the distribution of aid in 1968. For example, only 1.5 percent of white males over 14 received welfare while 5.6 percent of black males did. The interesting fact that emerges from this table is that a greater proportion of white women receive unemployment compensation than do nonwhite women (the COMPEN variable includes unemployment and workmen's compensation, government employee pensions, and veterans payments), and unemployment compensation represents a larger fraction of their income. While unemployment compensation is roughly the same proportion of income for males of both races, a higher proportion of white males receive unemployment compensation than do black males. Finally, note the overall pattern revealed in the income level regressions of Table A-5. AID has a generally positive employment rates or relative labor force are used as measures of the relative quantity of blacks in the market rather than the measure employed here.

TABLE A-3

	MALES						FEMALES					
	WAGE		WEL		COMPEN		WAGE		WEL		COMPEN	
	W	NW	W	NW	W	NW	W	NW	W	NW	W	NW
1968	.786	.866	.002	.013	.016	.020	.754	.772	.015	.101	.020	.015
1969	.792	.868	.002	.013	.018	.021	.754	.787	.016	.096	.018	.018
1970	.791	.864	.002	.013	.022	.025	.754	.782	.019	.104	.022	.016
1971	.784	.839	.002	.014	.025	.026	.746	.758	.019	.124	.023	.017
1972	.781	.857	.002	.012	.025	.026	.736	.761	.019	.114	.022	.016
1973	.776	.861	.002	.009	.024	.023	.733	.754	.019	.117	.022	.015
1974	.778	.841	.003	.014	.029	.032	.720	.749	.022	.110	.026	.017

Proportions of income coming from wages and salaries (WAGE); public assistance and welfare (WEL); and unemployment and workers compensation, government employee pensions, and veterans payments (COMPEN); by race and sex.

TABLE A-4

	MALES						FEMALES					
	WAGE		WEL		COMPEN		WAGE		WEL		COMPEN	
	W	NW	W	NW	W	NW	W	NW	W	NW	W	NW
1968	.799	.843	.015	.056	.094	.082	.716	.748	.028	.176	.054	.037
1969	.804	.843	.017	.057	.102	.084	.717	.758	.041	.178	.056	.046
1970	.799	.836	.019	.062	.124	.103	.708	.727	.047	.203	.063	.051
1971	.798	.812	.019	.067	.131	.105	.703	.702	.048	.233	.066	.054
1972	.793	.812	.018	.062	.126	.099	.695	.688	.049	.236	.065	.053
1973	.796	.824	.016	.052	.115	.095	.701	.699	.046	.221	.060	.046
1974	.789	.804	.022	.071	.140	.118	.692	.694	.057	.238	.073	.054

Number of people receiving wage, WEL, and COMPEN (as defined in Table A-3) as a proportion of all people receiving income; by race and sex.

TABLE A-5
(t-statistics in parentheses)

	C	TIME	DGNP	EEOC	AID	UNCOMP	R ²	D.W.
Black Males								
1. Income 1948-74	6.29 (4.34)	.026 (6.21)	1.17 (3.94)	.002 (.099)	.198 (.793)	.009 (1.07)	.965	2.35
2. Wage/Salary 1948-74	10.31 (3.10)	.041 (4.07)	.505 (1.38)	.011 (.550)	-.174 (-.645)	-.236 (-.550)	.969	2.04
3. Wage/Salary of Yr-round full- time 1955-74	5.06 (2.17)	.021 (2.98)	-.192 (-.891)	.005 (.489)	.288 (1.19)	.164 (.518)	.983	2.15
Black Females								
4. Income 1948-74	1.81 (.920)	.029 (5.17)	.637 (1.57)	.018 (.818)	.780 (2.29)	.007 (.598)	.973	1.85
5. Wage/Salary 1948-74	5.94 (1.84)	.048 (4.88)	.229 (.644)	-.002 (-.131)	1.02 (3.92)	-.719 (-1.76)	.987	1.76
6. Wage/Salary of Yr-round full- time 1955-74	2.51 (.801)	.022 (2.31)	-.574 (-1.98)	-.002 (.138)	1.04 (3.22)	-.162 (-.380)	.985	2.05
White Males								
7. Income 1948-74	9.72 (11.38)	.029 (11.57)	.554 (3.16)	-.013 (-1.32)	-.288 (-1.94)	.011 (2.32)	.978	1.28
8. Wage/Salary 1948-74	6.15 (4.42)	.022 (5.19)	.467 (3.04)	-.009 (-1.10)	-.391 (-3.48)	.600 (3.40)	.991	1.56
9. Wage/Salary of Yr-round full- time 1955-74	5.17 (3.99)	.015 (3.95)	.199 (1.67)	-.0003 (.052)	-.002 (-.012)	.449 (2.55)	.991	2.47
White Females								
10. Income 1948-74	3.98 (4.55)	.001 (.324)	.961 (5.34)	.027 (2.74)	.584 (3.84)	-.010 (-2.19)	.954	1.74
11. Wage/Salary 1948-74	4.90 (4.90)	.010 (3.20)	.393 (3.57)	-.015 (-2.62)	.019 (.236)	.372 (2.93)	.988	1.54
12. Wage/Salary of Yr-round full- time 1955-74	5.29 (4.52)	.009 (2.46)	.193 (1.79)	.001 (.241)	.220 (1.81)	.205 (1.29)	.987	1.55

impact on the level of female income with a much stronger effect for black females than for whites. Unemployment compensation payments clearly result in larger increases in white male income than in black male income.

The upshot of this discussion is that the aggregate data do not support the argument that measured growth in relative black wages and income is due to federal antidiscrimination efforts as measured in the literature. Part of the measured growth is more properly attributed to increasing social expenditure in the sixties which led to a withdrawal of black workers--especially low wage black workers--from the labor force.

However, we do not shift to the other extreme and conclude that all of the measured relative wage growth of blacks is due to the reduction of blacks from the work force. The excellent studies of Freeman (1977) and Smith and Welch (1976) refute any such conclusion. Nor do we deny that antidiscrimination activity may have had some effect on raising black status. A noteworthy finding in both the Freeman and Smith and Welch analyses is that young black workers have made spectacular gains in relative status in recent years. Moreover, the data on school enrollment rates for blacks shows a sharp increase in the mid-sixties--precisely what would be expected if their long term prospects in the labor market had been improved by antidiscrimination legislation. Since it is likely that antidiscrimination programs have their greatest impact on young workers who can still alter their career plans, this evidence tends to support the view that antidiscrimination legislation may have had a beneficial impact.

The important point to extract from our analysis is that for the group of blacks as a whole, there is little evidence from the time series that antidiscrimination legislation has altered relative black status. And

for this group, there is considerable evidence that social transfer programs have played an important, albeit not monolithic, role in elevating black wage rates relative to white wage rates.

APPENDIX B

DATA SOURCES

Income

Median income includes all income for persons 14 years of age and older receiving income. The median wage and salary income group consists of all wage and salary income for persons 14 years of age and over receiving such income. Year round, full-time income refers to all wage and salary income of year round, full time workers. Due to the lack of published data, the regional figures refer only to median income. All these figures (except for the 1949 regional income figures, which came from the U.S. Census of Population, 1950, Detailed Characteristics) come from the following issues of the U. S. Bureau of the Census, Current Population Reports, Series P-60: 5 (1947), 6 (1948), 7 (1949), 9 (1950), 11 (1951), 14 (1952), 16 (1953), 19 (1954), 23 (1955), 27 (1956), 30 (1957), 33 (1958), 35 (1959), 37 (1960), 39 (1961), 41 (1962), 43 (1963), 47 (1964), 51 (1965), 53 (1966), 60 (1967), 66 (1968), 75 (1969), 80 (1970), 85 (1971), 90 (1972), 97 (1973) 101 (1974), and 103 (an advanced report on 1975). The last eight issues also provided the information for Tables A and B in the Appendix.

DGNP, TIME, and EEOCYR

Consistent with the original Freeman study (1973) our time variable takes a value of 1 beginning in 1947, and our deviation from gross national product (DGNP) is computed using Freeman's computed trend (see his footnote 8)--
 that
 though it should be noted / his data goes up to 1972 and our data extends through
 based on a
 1975. The reported EEOC budget is / fiscal year from July 1 to June 30,
 and it appears to us that Freeman used this fiscal year measure in the

construction of his EEOC variable. The corrected calendar year variable (the log of accumulated real EEOC calendar year expenditures per nonwhite worker) is of course highly correlated with Freeman's variable, shows only a marginally smaller effect, and is the variable we employ in the above regressions.

Education, Labor Force Participation Rates

These data come from the 1976 Employment and Training Report of the President and the 1975 Handbook of Labor Statistics. The labor force participation rates are for the civilian labor force 16 years and over, and are only available to 1948. Education refers to the median years of school completed by the civilian labor force 18 years and older, and is available only for selected years before 1964. We employed Freeman's interpolated values for the missing years.

AID and UNCOMP

Average monthly payment per recipient in the Aid to Families with Dependent Children program came from the 1974 and 1975 Annual Statistical Supplement to the Social Security Bulletin. The 1975 Statistical Abstract of the United States and ^{the} 1970 edition of the Bureau of the Census' Historical Statistics of the United States contain data on the average monthly unemployment compensation per beneficiary.

Additional Data Used in the Tables

The proportion of nonwhite female headed families are from the following issues of the U. S. Bureau of the Census, Current Population Reports, Series P-20: 16 and 17 (1947), 26 (1949), 33 (1950), 44 (1952), 53 (1953), 67 (1954 and 1955), 75 (1956), 83 (1957), 88 (1958), 100 (1959), 106 (1960),

116 (1961), 125 (1962), 139 (1963 and 1964), 153 (1965), 164 (1966),
173 (1967), 191 (1968), 200 (1969), 218 (1970), 233 (1971), 251 (1972),
258 (1973), 276 (1974), and 291 (1975). We interpolated for the missing
1949 and 1951 data.

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