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ABSTRACT

Researchers propsed a new method for measuring the mismatch between the level of schooling and occupation on a per-worker basis. The method is basically a prevalence measure that relies on census data pertaining to schooling, occupation, and the match between them on a per-worker basis. After applying their method to data derived from the March Current Population Survey for each of the 12 years from 1969 through 1980, researchers determined that there has been at least a 64 percent increase in occupational mismatch over the 12-year interval. Also emerging from the analysis was the fact that the 20 through 34 age group experienced the most alarming increase in mismatch. In addition, researchers found that the broad category of managerial occupations registered over a 9 percent increase in mismatch risk, indicating the types of occupations into which over-educated workers are typically being placed. Feeling that their measure of occupational mismatch is more comprehensive than other similar previously-developed measures, the researchers called for further research on how the new method should be used in demographic research on labor force or labor market problems. They also recommended using the mismatch measure on the educational and employment policy fronts. (MN)

TRENDS IN THE MISMATCH BETWEEN OCCUPATION AND SCHOOLING: 1969-1980*

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ABSTRACT

A relatively simple method is proposed to measure the mismatch between level of schooling and occupation on a per-worker basis. This technique produces direct "prevalence" measures of one important form of underemployment or under-utilization. Results for U.S. labor force data over the 1969-1980 interval show that there has been a substantial increase in the prevalence of mismatch, at least 6.4 percent over the interval. Descriptive statistics indicate that the 20-34 age group experienced the most alarming increase in mismatch, but non-trivial increases were also recorded for other demographic groups. The broad category of "managerial" occupations registered over 9 percent increase in mismatch risk, indicating the types of occupations where "over-educated" workers are typically being placed. It is argued that mismatch increases for such diverse demographic groups, of the magnitude estimated here, cannot be easily explained as statistical artifacts associated with operational definitions. Therefore the basic fact of increasing mismatch cannot be questioned for this interval of recent labor force history. The rise in occupational mismatch might very well be one of the most significant developments in the U.S. labor force in recent history.

TRENDS IN THE MISMATCH BETWEEN OCCUPATION AND SCHOOLING: 1969-1980

This paper is concerned with the mismatch between occupation and educational attainment, the imbalance between the demand for and the supply of educated labor. A relatively simple technique is proposed for measuring occupational mismatch on a per-worker basis, one which produces a "prevalence" measure that can be easily applied to existing labor force and other survey data. This measure can be analyzed without recourse to the "returns-to-education" measures associated with human capital economics or status attainment sociology, and yet we believe that it could be used to further specify models that are typically used in studying attainments over the life course. Trends over the recent period of U.S. history from 1969 to 1980 will be examined using this new mismatch measure, and a preliminary attempt will be made to characterize the way that mismatch "risks" are distributed by age, sex, color, occupation, and other key demographic and structural variables.

The topic of occupational mismatch, while implicit in several sociological and economic studies of recent trends in earnings or occupational returns to education, does not appear to have been considered much on its own terms in recent literature. Surely there is as yet no "theory" of occupational mismatch which is entirely satisfactory, and the research that currently exists seems to suffer from an inability to formulate a direct measure of the concept. The concern of this study is not on testing theories of how mismatch is determined or how it is causally related to micro or macro social factors. Rather, it is oriented to a logically prior task. The objective is to propose a method for measuring the concept of mismatch. Then the measure's characteristic relationships with demographic and other variables will be demonstrated. It is hoped

that this will encourage use of the concept and the proposed measure of it in demographic research on the functioning of modern labor markets.

The foundations of this work derive from disparate intellectual sources. The principal one is the field of labor force statistics, a significant part of which has recently been concerned with devising measures of underemployment or underutilization. It has generally been observed that the usual labor force statistics on labor force participation and unemployment need to be supplemented with measures of underemployment (Levitan and Taggart, 1974; International Labour Organisation, 1976; Sullivan, 1978; Clogg, 1979; National Commission on Employment and Unemployment Statistics, 1979). One of the frameworks recently proposed for the measurement of underemployment, the Hauser-Sullivan Labor Utilization Framework, includes a component aimed at measuring occupational mismatch, and it has already been calculated and analyzed for a variety of national labor forces (Hauser, 1977; Sullivan, 1978; Clogg, 1979). It is essentially the mismatch component of the Hauser-Sullivan framework that is used herein, and the results should provide further evidence concerning the utility of the mismatch component of this general framework.

While the field of labor force statistics provides the port of entry for the specific mismatch measure considered here, there are other areas of research where the concept of mismatch is recognized. Only specific measurement strategies are different in the other types of research. Berg's (1970) influential work on the mismatch between education and occupation constitutes one example. His work, essentially based on data of the 1960s, relied on rather informal methods of assessing the fit between educational attainment and occupational placement. Indeed, he does not present comprehensive measures of the prevalence of mismatch, and he gives no specific empirical results showing how rapidly mismatch has grown. By focusing on only selected occupations, he was unable to estimate the prevalence

of mismatch for the aggregate labor force or its time trend. Berg's work stimulated our own research on mismatch definition and measurement, and we believe that many of the criticisms of his work are dealt with satisfactorily in our general approach.

Yet another field of research where the concept of mismatch is used is exemplified by Freeman's (1975, 1976, 1980) assessment of the declining earnings returns of college education. Freeman used data derived essentially from the early 1970s, and he focused for the most part on rather specific types of college graduates and rather specific types of occupations. His analysis compared earnings of recent college graduates with high school graduates, using models that are now routine parts of economic analysis. There were no estimates in Freeman's work of the actual prevalence of occupational mismatch in the labor force as a whole, and there was no special consideration of persons who attend college but do not complete it, but who are still "at risk" to mismatch. Similarly, there was no special significance placed on the fact that many persons who obtain post-college education have been at least temporarily placed in occupations that do not typically require post-college education, which also represents an important form of mismatch. We believe that the human capital orientation of his work, which focused on returns to education viewed from the regression models of attainment research, was chiefly responsible for his inability to come up with an altogether convincing measure of mismatch prevalence. Mismatch, when viewed through the models of status or income attainment studies, can only be inferred by indirect means. These consist essentially in comparing the regression coefficient linking schooling to earnings (or occupational status), after suitable controls have been included in the equation, over time or across cohorts (also see Featherman and Hauser, 1978). There is no easy way that a

prevalence measure can be extracted from the regression models of attainment research, even though the attainment approach represents the method most often used to infer the trend in mismatch. (The general strategy is not questioned by critics such as Rumberger (1980), Witmer (1980), and Schwartz and Thompson (1980), although Rumberger does present an alternative to it.) One goal of the present paper is to show how a simple prevalence measure, as compared to a returns-to-education measure, can be constructed without utilizing attainment models.

The common theme in all of these studies is that the U.S. occupational structure has not changed nearly as rapidly in recent times as the proportion of the work force possessing post-high school education. For example, the proportion of the work force in professional or managerial occupations increased by only 1.4 percent (24.4 percent to 25.8 percent) between 1970 and 1980, whereas the proportion of the labor force which had completed college increased by 5.8 percent (12.8 percent to 18.1 percent) over the same interval of time.[1] In the field of labor force statistics, such trends necessarily imply rising underemployment or underutilization. The underutilization of worker skills represents a waste of individual resources as well as inefficient allocation of social resources among the various types of public good investments that are available. In Berg's institutional critique of the so-called "training robbery," high mismatch prevalence is assumed to present basic challenges to the entire system of higher education. While the system of higher education ostensibly exists to promote social mobility and equality of opportunity (Boudon, 1974), it actually functions more and more as a socialization agent without apparent success in promoting the egalitarian goals which legitimate the institution (Suda, 1979). Freeman's programmatic implications are couched in both a micro- and a macro-level cost-benefit analysis. At the individual level, college education has

become, on average, a poor investment; at the macro-level, the very high subsidization of higher education is judged to be inefficient.[2]

Deriving an acceptable method of measuring mismatch prevalence would seem to have the highest priority for research on the labor force. A good measure would enable more precise estimation of the underemployment burden than we currently have at our disposal in official publications like Employment and Earnings. It would enable policy makers to assess the extent of the problem and then propose programmatic response. Similarly, a good measure of mismatch, coupled with extensive time-series information on its growth and trend, would seem to provide a launching pad for the sociological and economic analyses typified by Berg or Freeman. And it is not inconceivable that a good mismatch measure would enlighten the general area of status attainment research (Featherman and Hauser, 1978), or its competitor in segmented labor market research (Beck et al., 1978).

We first describe the data source and then discuss the specific procedures used for measuring mismatch prevalence. Results for 12 recent years in U.S. history are examined, including descriptive statistics on aggregate trend, demographic differentials, and occupational differentials. An attempt is made to link up mismatch changes to changing occupational distributions and to changing educational distributions. Virtually all of the statistical analyses should be regarded as exploratory and descriptive, as opposed to testing structural models of mismatch. But at least two sections contain material that is likely to constitute first steps in a more "explanatory" mode of analysis (e.g., material on cohort tendencies, material on sex-color group differentials).

Data

The data derive from the March Current Population Survey (Annual Demographic File) for each of the 12 years 1969-1980. The CPS is a representative national

sample containing from 50,000 to 70,000 labor force members in a given year. These data are described fully in a variety of sources (e.g., U.S. Bureau of Census, 1978), so it will be assumed that readers are sufficiently familiar with their makeup.

Rather complicated clustering, stratification, and weighting procedures are used to acquire CPS samples. Except for reweighting the data to produce sample sizes, for each year, that are approximately equal to the actual sample sizes, no special accounting for the sample design effects has been incorporated into our analysis. Another important aspect of the data that deserves comment is the rotation scheme of sample selection. Approximately one-half of the individuals sampled in year t are resampled in year t+1, a feature that actually allows for the development of "matched subfiles," partially longitudinal in character (Bureau of Labor Statistics, 1980). It should be noted that the matched subfiles were not used herein, owing partly to the difficulty in creating data that would span the entire decade of recent experience. But the rotation aspect of the data means that the samples are not entirely independent of each other. For this reason, some of the statistics (e.g., chi-square statistics) reported in this paper should be regarded solely as descriptive indexes of fit.

A definite advantage of the data source is its repeated cross-sectional format (Duncan, 1969). Virtually identical variable definitions were employed throughout the series, the major exception being the change in occupation codes between 1970 and 1971. Annual fluctuation in labor force indicators (such as mismatch measures) can therefore be studied over time without difficulties in reconciling different variable definitions. The repeated cross-sectional format of our data will be exploited quite heavily in the following analyses.

Measuring Mismatch Prevalence

There are two general strategies available to measure mismatch. One consists of an indirect approach built upon status or earnings attainment models, and the other is based on a more direct approach of measuring mismatch prevalence. This paper is based on a certain form of prevalence measure; the immediate goal is to construct a dichotomous variable which classifies workers as either mismatched or not mismatched. A variety of methods might be used to construct prevalence measures, however, and these can be divided into two general types.

First, procedures might be devised which are based on external criteria which effectively define the schooling requirements for jobs. Persons with greater educational attainment than that "typically" required for job performance would be classified as mismatched in this type of approach. An early example of this approach is Eckaus (1964), some parts of which were used by Berg in his fundamental work. Another strategy can be found in applications of the Dictionary of Occupational Titles (DOT). The DOT lists job characteristics for specific occupations, including "General Educational Development" necessary for proper job performance (Miller et al., 1980). These GED scores, which range from 1 to 6 in an ordinal scale, have been translated into "equivalent years of schooling" by several researchers (Eckaus, 1973; Rumberger, 1980). A problem with the DOT variable on General Educational Development is that is has only two codes that could plausibly be considered as values concordant with post-high school education. (Rumberger, using a conservative strategy, considers GED score 5 to be equal to 16 years of schooling, GED score 6 to be equal to 18 years of schooling.) There is simply not enough variability in the GED scores, at the upper range, to derive sensitive prevalence measures of occupational mismatch or over-education. Use of the DOT represents only one possible set of external

criteria that can be used in measuring mismatch; other kinds of external criteria, some based on a blending of the "returns-to-education" concept and DOT-based criteria, have apparently been used in preparing the rather gloomy forecasts of the occupational outlook for college graduates (U.S. Department of Labor, 1978).

A second type of prevalence measure, the one employed here, is based on internal criteria, i.e., on evidence contained within survey or census data on schooling, occupation, and the match between them on a per-worker basis. There are certain advantages to a strategy based exclusively on internal criteria. The data that now exist, for the recent or the more distant past, can be directly used to analyze mismatch trends. Occupation and schooling information that are universally included on labor force and other surveys can be utilized to measure mismatch without extreme expenditure of resources, implying that mismatch could be studied as an "independent" or a "dependent" variable in attitudinal or other kinds of sociological analyses. The vexing problem of discounting earnings, of modeling earnings foregone by those who enroll in college, is sidestepped. The approach yields mismatch measures that are valid for workers of all ages. It could be more easily generalized to cross-societal analysis. Finally, this approach might be just as efficacious as others in measuring demographic and other types of differentials or across-time change.

Mismatch prevalence is then conceived in reference to the comparative "fit" between two variables: years of schooling and occupation (Sullivan, 1978; Clogg, 1979). Years of schooling is an obvious candidate, but here completed years of schooling will be used, partly to render the procedure as conservative as possible. The occupation variable employed is based on a regrouping of the 3-digit census classifications of occupation. The regrouping of these 3-digit codes was done in order to build broad categories which were essentially internally homogeneous with respect to educational attainment.[3] This task was

carried out by Sullivan using the 1970 Census Public Use Sample, and her classification scheme was essentially the one used here. For each "condensed" occupational category, the mean and the standard deviation of completed years of schooling were computed for the benchmark year 1970. Any worker who had more schooling than the mean plus one standard deviation (rounded up to a whole year) was classified as mismatched.[4] The criteria developed for the 1970 Census were then directly applied to all CPS files which used the 1970 Census classification of occupations, and they were applied to the CPS files using the 1960 Census codes after making the 1960 codes comparable to the 1970 codes. Since the same benchmark criteria were applied to all years of the study, the trend in mismatch should be estimated with negligible bias, even though the absolute levels of prevalence must be regarded at least partly as statistical artifacts of the "mean-plus-one-standard-deviation" rule. It must be emphasized that such a strategy, yielding nearly 8 percent mismatch in 1970, could not in itself hope to pinpoint the actual level of mismatch, but all indications are that this approach yields conservative estimates.

Regrouping occupations into educationally homogeneous groups produces education distributions within categories that are quite concentrated about the mean, at least for the benchmark year. This implies that those having educational attainment greater than the mean plus one standard deviation are actually quite "deviant" from the average or norm, again at least for the benchmark year. (It can be noted that the normal distribution is not an acceptable description of the education distrubutions for many occupational groups; the distributions tended to be skewed rightward and to have "fat" centers.) While mismatch as a concept can be defined for nearly all members of the labor force (except for the 10 percent to 15 percent of the unemployed who do not have occupations to report, and, in later years, for some inexperienced labor force members), further sorting

criteria were actually applied which effectively reduce the "universe" ostensibly exposed to the risk of mismatch. Unemployed and part-time (voluntary and involuntary) workers were excluded from the sorting for mismatch. Excluded as well were those persons who had very low earnings during the previous year (below 1.25 times the Poverty Threshold for individuals, adjusted for weeks worked). One justification for this latter exclusion is that mismatch for low-earnings workers is probably "frictional" in character. That is, greater than average educational attainment within an occupation should be associated with greater income returns, not lesser income returns (Mincer, 1974). Those who are nominally mismatched but also currently receive very low earnings are probably only in a transitory state of mismatch, or a frictional state of mismatch. These several exclusions from the universe "exposed" to mismatch sorting together imply, once again, that the technique is probably quite conservative in measuring mismatch prevalence. Indeed, we have as yet learned of no criticisms of the approach which make the contrary claim (see National Commission on Employment and Unemployment Statistica, 1979).

The complete recoding scheme, together with the mismatch criteria, is available from the authors.

Results

Mismatch Trends for the Aggregate Labor Force

Table 1 presents mismatch rates for each of the 12 years, 1969-1980, calculated by using the procedures discussed in the previous section. The 1969 year registered 7.8 percent mismatch in the labor force as a whole.[5] Care must be exercised in interpreting this figure. While every effort was made to conservatively measure the prevalence of mismatch, there is certainly room for the

objection, from both sides, that this figure is either too low or too high as a result of arbitrariness of definition. The essential points in our subsequent argument would not change, however, if a more liberal method of imputing mismatch prevalence were used. Note that each of the inter-period changes recorded in Table 1 show an increase in mismatch, and the same criteria were used for all years of the study. Thus, while the absolute level of mismatch prevalence might in any given year be treated as somewhat artifactual, there is good reason to believe that the relative change over time is being recorded rather well. The 1980 mismatch rate stood at 14.2 percent of the labor force, for an absolute increase over the interval of 6.4 percent, or a proportionate increase of 82 percent. The average inter-period change is .6 percent, implying that with a total labor force of approximately 100 million, there is a net flow of around 600 thousand workers into the mismatch status with each successive year.

It was noted earlier that the 1960 Census occupation codes were used for 1969-1970, while the 1970 Census occupation codes were used for 1971-1980. The change from 1970 to 1971 (8.7 percent - 7.9 percent = .8 percent) is rather large, perhaps indicating some slippage in our reconciliation of the two different occupational coding systems. However, four other inter-period changes are as great in absolute magnitude (72-73, 73-74, 75-76, 78-79), and thus there does not appear to be good reason to question our reconciliation of 1960 and 1970 occupation codes in the measurement of mismatch prevalence.

Table 1 also presents mismatch prevalence defined in relation to only the full-time, adequate-income labor force. (See note a to Table 1 for definitions.) These percentages show even greater temporal change as well as higher prevalence. Restricting the denominator in the mismatch rates in this way works to refine the prevalence concept by reducing the universe of those exposed. It

could be argued that such restrictions more effectively define the risk of "structural" mismatch, since persons in the aggregate labor force are also exposed to "frictional" mismatch, as well as the other types of underemployment. In many respects, the unemployed, the part-time unemployed, the low-income workers, and the like are not yet fully integrated into the labor market system, implying that nominal mismatch risk for these workers should be regarded as frictional in character. Of course, the exclusions made in forming the second set of rates in Table 1 come much closer to pinpointing those persons who are at risk to non-frictional, or structural, mismatch, although we hasten to add that this is only a very rough proxy. Using these prevalence measures, it can be seen that the absolute increase in mismatch over the interval was 8.2 percent, the proportionate increase was 89 percent, and the average inter-period change was .7 percent.

Mismatch by Age and Period

Much of the literature on the phenomenon of over-education deals with highly specific population groups, usually the "young" group aged 20-34. In most cases, the focus is only on the "returns" to college education, where the group studied is necessarily college graduates. There does not appear to be a recognition that the supply of highly educated labor in the young age groups can have the effect of stifling demand for educated labor in the older age groups.[6] One advantage of the prevalence measure advocated herein is that it is defined for all age groups. Since the data available span a rather long interval of time, it is possible to study age variability in mismatch prevalence over time, thereby inferring the way cohorts change in their mismatch risk over time. The data and models applied here show that mismatch

prevalence increased for every five-year age group over time, and for every five-year cohort over time. That is, no age group and no cohort was immune to the general rise in mismatch risk over this interval.

Table 2 presents mismatch prevalence for five-year age groups in the age interval 20-64, for each of the years 1970, 1975, and 1980. As before, two sets of prevalence rates are presented: prevalence as a percent of the labor force, and prevalence as a percent of the full-time adequate-income labor force. Regardless of which rate is used, mismatch is found to be lower in the 20-24 age group than in the immediately older age groups. (For example, the 1970 rate is 7.4 percent for the 20-24 group, 10.3 percent for the 25-29 group.) Such a finding is to be expected, since many persons in the labor force in the 20-24 age group have not yet completed much post-high school education. Mismatch is highest in the 25-29 age group (10.3 percent in 1970, 20.3 percent in 1980), and it declines regularly thereafter. Such mismatch differentials by age are surely to be expected, but what is remarkable about these age differentials is that they are themselves very different across time. There was an increase in the age differential over time, as the following elementary calculations will show.

A simple measure of the age differential in mismatch is the ratio of the 25-29 age group's rate to the rate of the 45-49 group. The first group has the highest rate for every period, and the second group can be taken as a compromise between the age group for which mismatch is at a minimum and the age group which typically experiences the greatest advantage in securing the main labor market regards. Perhaps this ratio--a ratio of relative risks--could serve as a rough proxy for relative over-education in a given period. This argument could be made because the 25-29 age group has largely completed their schooling, and the relative experience of this group is a legitimate

summary of current over-education. This ratio is 1.18 for 1970, 1.57 in 1975, and 1.51 in 1980 when the first prevalence measure is used. It is 1.18, 1.66, and 1.56 when the more restrictive prevalence measure is used. There was thus a sharp increase in age variability over time with the greatest increase occurring in 1970-1975, the period most studied by Freeman and his critics. Relative to older workers, young persons in the labor force had much greater difficulty in finding jobs commensurate with their schooling, and the difficulty became more severe over time.

Also presented in Table 2 are the average absolute increases in mismatch prevalence for age groups and for age cohorts. In every comparison there is a nontrivial increase in mismatch, regardless of the type of prevalence measure used. The age groups 25-29 and 30-34 experienced 10.8 percent and 10.9 percent increases in mismatch over the interval; in relative terms mismatch nearly doubled. For age cohorts, there were also increases for every comparison possible to make. The cohort aged 25-29 in 1970, e.g., increased its mismatch prevalence from 10.3 percent to 16.6 percent in 1980, when it was age group 35-39. We will postpone for the moment the question of whether there are "cohort effects" in the data, but it must be emphasized that every comparison of cohort experiences over time registers an increase in mismatch prevalence. Accumulated labor force experience does not seem to be enough of a prophylactic to ward off increased mismatch risks. This basic fact strengthens the general conclusion of this paper: there is unquestionably a general (as opposed to an age or cohort-specific) increase in mismatch prevalence over time.

Another telling feature of the data in Table 2 bolsters the general argument. Mismatch rates for every age group in 1980 are higher than the maximum mismatch rate in 1970, even though identical definitions were applied for all age groups in both periods. For example, those aged 50-54 in 1980 had higher

mismatch (13.2 percent) than the 25-29 age group in 1970 (10.3 percent). This indicates that application of current age-specific profiles of mismatch prevalence, say for the older groups, cannot be justified in modeling lifetime mismatch risks. Of course, much of the work cited thus far rests on the assumption that currently observed age-specific risks of mismatch for at least older age groups would remain unchanged over time, accommodating gradually to the entry of younger, more educated workers over time (see Smith and Welch, 1981).

Age, Period, and Cohort Effects

Repeated cross-sectional surveys can be used to perform cohort analysis. As noted in several contributions to a recent volume on cohort analysis (Duncan and Winsborough, 1982), there are distinct advantages of the age-period-cohort accounting scheme for socio-demographic inquiry. This section presents a modest beginning of a cohort analysis of mismatch prevalence, taking as data the second set of age- and period-specific prevalence rates in Table 2. This analysis is quite tentative owing to three considerations. (1) The data used here do not disaggregate by sex, or by color, and so we know that there is considerable heterogeneity in the population universe being studied. Nevertheless, it appears useful to aggregate in this way to make general inferences about life cycle (age) effects, business cycle "demand" (period) effects, and cohort effects. (2) The data do not really allow us to say that the same cohort is sampled at successive intervals. For example, the mismatch rate for 25-29 year olds in 1970 pertains to only the full-time, adequate-income labor force; in 1975 the mismatch rates for the 30-34 age group is also based on currently full-time, adequate-income labor force participants. The pool of individuals sampled at the later year is not the same as the pool sampled in the earlier year. This pool changed somewhat by labor force increments and decrements, by

shifting unemployment and part-time work risks, etc. The assumption that we will make is that the age-period specific rates in Table 2 are representative of the "true" risks that attach to age-period, and hence cohort, combinations. (3) As is well known (Fienberg and Mason, 1978), there is a nontrivial identification problem in cohort analysis that must be squarely faced. Here a form of "sensitivity analysis" proposed in Clogg (1982) is employed to qualify the inferences to be drawn. It will be assumed that the last two cohorts in the data (cohort aged 55-59 in 1970, cohort aged 60-64 in 1970) have identical effects on mismatch. The possible error in this assumption will be examined by seeing how the other estimated effects in the model could be "biased" by an error in imposing the identifying restriction.

It was noted earlier that age variability in mismatch increased over time. This denotes a kind of "age-period" interaction, and one way to explain it is to introduce cohort effects. As is universally appreciated by cohort analysts, cohort effects show up in data as a special kind of age-period interaction, since the cohort variable is defined in relation to the values of the age and period variables. Does the concept of "cohort effects"--intrinsic cohort tendencies that are separate from age and period effects--make substantive sense in the present context? We believe it does. Cohorts differ by virtue of educational attainment, as well as by other factors, and rising educational attainment is associated with higher mismatch risks. If succeeding cohorts possess greater and greater proportions of highly educated workers, then it seems likely that there should be logically distinct cohort effects on mismatch, effects which are ostensibly different from age and period effects.

Table 3 presents the degrees of freedom and likelihood-ratio chi-square statistics for several cohort models. The log-odds, or logit, of mismatch was "regressed" on dummy variables marking age, period, and/or cohort categories

(Fienberg and Mason, 1978; Clogg, 1982). The model with age and period effects (line 3) accounts for 94.8 percent of the baseline chi-square. Neither the age nor the period effects can be adequately described as linear effects, as the fit of the model in line 4 shows. The model with age, period, and cohort effects gives $L^2 = 47.3$ on 7 df and accounts for 98.5 percent of the baseline chi-square. With a sample size of over 150,000 such a level of fit is quite acceptable: trivial departures of the data from the model would be "significant" with such a large effective n. The cohort variable does indeed add somewhat to explanatory power, and the full model accounts for the age-period interaction that is apparent in Table 2.

Table 4 presents the parameter estimates for the full model. The estimated period effects are -.40 for 1970, -.11 for 1980. Thus, the period effects--business cycle swings--were more marked from 1970 to 1975 than in the latter five-year interval. The age effects, net of period and cohort, are rather flat over the age range 30-64. They are lower for ages 20-29, even negative in comparison with the "left-out" 60-64 category. There is a dramatic increase in the age effect from 20-24 to 25-29 to 30-34, reflecting the ages where mismatch risk becomes more and more salient. That is, as cohorts pass through the young age categories they experience great increases in mismatch risk, a life cycle relationship that is certainly to be expected. The cohort effects, which serve in part as a proxy for increased educational attainment as a cohort-specific factor, are in the expected direction. It indeed appears as though the cohort effects would correlate very highly with cohort-specific variables measuring post-high school educational attainment.

The estimated effects depend on the equality restriction used to identify parameters. If cohort 10 in fact differs by $\pm$ .01 in its effect on the logit of mismatch, in comparison to cohort 11, the parameter estimates would change by an amount reported in the "bias" columns of Table 4. With an error of this

magnitude in the identifying restriction, the 1970 period effect would be biased by ± .02, the 20-24 age effect would be biased by ± .08, and the first cohort effect would be biased by ± .10. It seems likely that the cohorts 10 and 11 are quite close to each other in their "intrinsic" mismatch risks, so this analysis of possible bias probably suffices to outline the probable range of the true parameter values. If the error made in imposing the equality restriction were of magnitude ± .05, the bias terms in Table 4 would each be mutiplied by five, and it can be seen that much the same general inferences about age, period, and cohort effects would be drawn. We believe that cohort analysis methods of demography could be brought to bear on some of the issues raised concerning the long-run picture. If educational attainment continues to increase on a cohort-specific basis, then with the pattern of cohort effects and age effects observed here it would necessarily be the case that mismatch would not disappear in the next decade as some have suggested (c.f., Smith and Welch, 1981).

Mismatch by Sex-Color Group

Table 5 presents mismatch prevalence rates for the sex-color groups, for broad age categories and for the total labor force. Three years of CPS data are examined once again: 1970, 1975, and 1980. The color dichotomy is defined as black versus nonblack, and the prevalence rates are calculated with the labor force as base. Much of the literature on over-education deals only with males, and sometimes even only with white males, so the additional detail in these tables presents a more comprehensive view of mismatch than that concontained in other accounts. We see that nonblack males in the 20-34 age group increased their mismatch rate from 11.5 percent in 1970 to 21.4 percent in 1980. That is, practically one-quarter of the young nonblack male labor force

in 1980 was mismatched by our definition. In terms of absolute prevalence, it can be said that the other three sex-color groups were roughly equal to each other. What is noteworthy is not the absolute rates themselves, but rather the inferences that can be drawn about relative or absolute changes over time. Table 6 provides the requisite information about trend. It is apparent that black males registered the largest proportionate increase in mismatch, although the two female groups came very close to the black male figures. Absolute increases in mismatch were the greatest for the 20-34 male groups (both black and nonblack), but as a whole the nonblack male group showed the greatest absolute increases. From these data it is apparent that rising mismatch is a characteristic of all sex-color groups, at all ages, and the magnitude of either the absolute or relative changes suggests that purely artifactual "defects" of the mismatch definitions could not alter the basic contention of this work. The general conclusion about rising mismatch risk is thus sustained by demographic disaggregation.

Mismatch by Major Occupation

While mismatch is defined in relation to occupation, the procedures used to measure it were actually based on examination of detailed occupation codes. A natural question to ask is how mismatch is distributed across major occupational groups, and how the increasing overall prevalence of mismatch is accounted for by changing occupation-specific risks of mismatch. A related question is how changes in occupation distribution could be a partial determinant of mismatch prevalence. Even though occupational distribution did not change dramatically over the interval considered here, there might conceivably be some modest year-to-year changes that could partially account for rising mismatch.

Table 7 presents mismatch prevalence rates by major occupational group for three selected years. The 1960 census classification of broad occupational groups was used throughout the series.[7] It should be noted that in defining mismatch for occupational groups there is a "ceiling effect" that works to produce some artifactual results. Occupations that require advanced graduate degrees (20 or more years of schooling) cannot be expected to register much mismatch. Indeed, the years-of-schooling codes end with "20 or more," implying that there is little point in trying to use internal criteria to arrive at a mismatch prevalence rate, for, say, college professors or doctors. Mismatch as a concept does not apply to incumbents of occupations which require the most advanced education. It is the diminished demand in precisely these occupations, with respect to the supply of persons ostensibly trained for them, which forces increasing mismatch in the other occupations. Such a fact must be taken into account in interpreting mismatch prevalence among professionals, which increased from 10.6 percent to 13.9 percent over the interval, for an increase of only 3.3 percent. Clearly this category yields estimates of mismatch which are contaminated with the "ceiling effect" just noted.

The results in Table 7 show that the greatest absolute increase in mismatch prevalence was in the managerial category. This group's mismatch prevalence stood at the alarmingly high rate of 27.2 percent in 1980, and the increase was 9.2 percent absolutely, .8 percent per annum change. A criticism often raised about this measure of mismatch, as well as others, has to do with the concept of "educational upgrading" of jobs over time. This criticism is based on the supposition that nominally equivalent occupations change over time by virtue of the technological skill--educational attainment--required for job performance. While this is certainly plausible in the abstract, or even in specific instances, it appears to us that this charge is limited to long-term trends. We are not

aware of any research that has attempted to estimate whether in fact this change occurs over the medium or short-term, and we are likewise unaware of any estimates of how fast this "educational upgrading" occurs. It would be incredible to claim that within occupation upgrading of jobs has been so rapid, in the interval from 1969 to 1980, that it could account for the mismatch figures reported here for the managerial category. It must be concluded that the principal reason for the increase in mismatch over the interval for managerial jobs has been the dramatic decline in demand for educated labor, relative to supply, in the traditionally highly educated occupations, pushing highly educated persons into the managerial jobs.

Other occupations that recorded substantial increases in mismatch prevalence are, in rank order, crafts (6.2 percent), "other service" (4.9 percent), and clerical and sales (both with 4.8 percent). Absolute or relative increases in mismatch prevalence for the other occupational groups are relatively modest by comparison. College-educated workers are still rarities in these other groups of occupations.

Can Occupational Distribution Changes Account for Rising Mismatch?

Let us now proceed as if changes in occupational composition could be partly responsible for producing the dramatic increases in mismatch recorded over the interval. That is, since mismatch risks are quite different across occupations, change in the occupational distribution per se would result in higher overall mismatch, appealing to straightforward demographic logic. The changes in occupational distribution were indeed in the direction that would favor an increased crude rate of mismatch (managerial categories, e.g., increased slightly in their proportionate representation among all occupations).

To examine this question, standardization or adjustment methods are useful. Conventional methods are somewhat unwieldy to apply to a set of data that has 11 "composition" categories (occupations) and 12 groups (years), but the method of "purging" proposed by Clogg (1978) can be used. This method is based on calculating the interactions in the data determined by the log-linear parameterization, and then adjusting the frequencies for the "composition-group" (occupation-period) interaction. Then the adjusted frequencies are converted into group (period) rates, and inter-group comparisons of these rates are, under the model, free of composition-group interaction. The occupation-by-period-by-mismatch (11x12x2) contingency table was used in its full detail for this analysis, and the results are summarized in Table 8.

The difference in crude rates, 1969 to 1980, is 5.1 percent when the calculations are based only on labor force members who have occupations to report. How different would the increase be if we adjust for the changes in occupational distribution? Four different adjustment strategies reported in Table 8 show that there is little difference between the crude difference and the difference after adjustment. Lines 2 and 4 show what happens if the occupation-period interaction is purged from the data: the adjusted difference is virtually identical to the crude difference. Lines 3 and 5 report differences based on purging the data of both the occupation-period and the 3-factor interaction. Briefly, this latter type of adjustment removes the confounding influence of 3-factor interaction, which is known to complicate usual adjustment procedures (Bishop, Fienberg, and Holland, 1975). With this other kind of adjustment, the increase in mismatch is recorded as being <u>greater</u> than the observed change in crude rates. What is responsible for this finding is almost surely the fact that transitory recession effects are removed from the data with this other method of adjustment,

a point that will not be further elaborated here (see Clogg, 1979, ch. 4). But note that all adjustments indicate that the increase in mismatch prevalence is real, or even understated by the crude rates. The increase in mismatch cannot in any way be accounted for by changing occupational distribution.

Mismatch and Schooling Attainment

For practical purposes, both the concept and the measure of mismatch pertain to the part of the labor force with some post-high school education. It is therefore worthwhile to examine how mismatch prevalence varies with schooling for the labor force with at least some post-high school education. Part of the justification for this strategy is the attempt to further refine the "exposed" population used in calculating mismatch prevalence. Table 9 presents crude mismatch rates for three selected years. The mismatch rate for those with 13-15 years of schooling was 20.3 percent in 1970, and by 1980 it had increased to 28.7 percent, for an absolute increase of 8.4 percent. For those with 16 years of schooling, the mismatch rate was a large 38.9 percent in 1970, and an even larger 46.8 percent in 1980, for an absolute increase of 7.9 percent. Finally, those with 17 or more years of schooling registered 38.1 percent mismatch in 1970 and 45.3 percent in 1980, quantities which are nearly consistent with those for the 16 years-of-schooling category. On the basis of these figures, it must be concluded that graduate education (17 or more years) was just as likely to be associated with mismatch as college education alone (16 years). Increasing mismatch is characteristic of all three major categories of post-high school educational attainment; it is not just restricted to "college graduates," as some studies would suggest.

It is well known that the educational composition of the labor force changed dramatically over the interval of time studied here, and the natural question to ask is how the changing educational distribution of the labor force could partly account for rising mismatch. This question is answered best with the kind of data summarized in the top half of Table 10, which shows both the mismatch rates and the changing proportions in the various educational attainment categories.[8] It is rather obvious that the changing educational distribution of the labor force operates to increase mismatch (the proportion of the labor force with 13 or more years of schooling increased by 10 percent over the decade. But it will now be shown that the entirety of the increase cannot be attributed to changing educational composition.[9]

The summary rates at the bottom of Table 10 are useful to see how compositional changes and changes in the schooling-specific mismatch rates operate together to produce the change observed. The actual increase in mismatch (the difference in crude rates) was 6.3 percent over 10 years. But when the 1980 rates are applied to the 1970 schooling distribution (line 2), we see that there would have been an increase of 2.0 percent in mismatch even if there had not been general educational upgrading of the labor force. In other words, at least 32 percent of the increase recorded was due to increases in the schooling-category-specific rates of mismatch. Line 3 applies the 1980 rates to the 1970 educational distribution, reversing the standardization logic employed, and calculations show that a 3.2 percent increase in overall mismatch would attributed to "true" change in the schedule of rates. Or, in other words, 51 percent of the increase overall would be attributed to increases in the schooling-category-specific rates. The other adjusted rates in Table 10 are based on Clogg's (1978) method of purging, and they also indicate that the general rise in mismatch is

very much a result of increasing schooling-category-specific mismatch rates. The results from this other method of adjustment are somewhat different from those obtained from the more usual methods, and this is to be expected in cases where there are dramatic differences in both the schedules of rates and the compositions involved in the comparison. But it can be noted that from 32 percent (line 2) to a full 70 percent (line 8) of the crude difference in mismatch rates, 1970-1980, is due to increases in the schooling-category-specific mismatch rates.

These figures indicate that it is unfair to attribute the general rise in mismatch to increasing schooling attainment per se. Rising mismatch rates within categories of schooling attained by labor force members must also be reckoned with, and they must be explained in something other than a demographic way. It would surely be inappropriate to view the "solution" to the problem solely in terms of a narrowly conceived policy response aimed at slowing the growth in educational attainment, as the summary rates in Table 10 show.

Mismatch and Schooling Attainment: Sex-Color Groups

Table 11 presents mismatch rates by schooling attainment level for each of the four sex-color groups. As elsewhere in this paper, such data are useful to examine the way that inferences hold up under demographic disaggregation. Particularly salient in these data are the following: (1) absolute increases were the smallest for nonblack males, indicating that this group has been somewhat less susceptible to increases in mismatch; (2) absolute increases for the female and/or black groups were the greatest for those with 16 years of schooling, with blacks registering over 20 percent increase; (3) for many of the groups and many of the schooling categories, nearly one-half of the labor force was mismatched by the end of the decade; and (4) every comparison shows an increase in mismatch, again demonstrating the ubiquity of the trend. It would

appear that the mismatch figures for college graduates (those with 16 years of schooling) are especially relevant to examining the topic of discrimination. The inference that would have to be drawn here is that discriminatory patterns of job placement could not be ruled out for the female or black groups; the dramatic rise in mismatch for these groups seems to argue that, if anything, there has been an increase in discriminatory job placement for the highly educated members of these groups. Of course, such an analysis is very different from those discrimination studies focusing on earnings or occupational attainments, but we believe there would be some value in pursuing the question using the mismatch-by-educational attainment figures reported on here.

To examine the sex-color mismatch relationships more precisely, a series of logit models (Goodman, 1979) were applied to a contingency table cross-classifying schooling, age, time, and mismatch. The age variable used had categories 25-29, 30-34, ..., 60-64, for eight categories; the restriction to the age groups 25-64 focuses attention on the population that is most likely to have completed schooling. The universe considered in the contingency was the labor force employed full-time, with adequate income, with schooling attainment greater than 12 years. Thus, the contingencies being studied all pertain to the "educated" labor force exclusively. The likelihood-ratio chi-square statistics for these logit models, applied to each of the four sex-color groups, appear in Table 12. In order to summarize results in a compact way, the percentage of the baseline chi-square accounted for by the various models will be appealed to. It can be noted that the most comprehensive model applied to these data yielded acceptable levels of fit for three groups; only for black males is the most comprehensive model called into question.

Consider first the case of nonblack males. Introducing main schooling effects and main age effects accounts for 71 percent of the baseline; adding

the interaction of these two predictors increases this to a full 83 percent. Thus, a large fraction of the variability in mismatch logits can be accounted for without introducing the time variable at all. This docusments the relatively modest increase in mismatch over time for this group. Adding main effects of time increases the percent accounted for to 93 percent, which is very close to that amount accounted for by the most comprehensive model. Mismatch risk is quite dependent on age and schooling for nonblack males, the relationship does not change dramatically over time, and relatively simple time effects can be posited to explain the change observed.

Consider next the case of nonblack females. While the most comprehensive model fits almost as well for this group as for nonblack males, there are dramatic differences in the factors that are associated with mismatch. The model with main effects of age, schooling, and their interaction accounts for only 61 percent of the baseline chi-square (as compared to 83 percent for nonblack males). Adding main effects of period--to account for the overall increase in mismatch--increases the percent accounted for to 80 percent. And there are nontrivial interactions of the time variable with both age and schooling, the latter of which would be anticipated from results in Table 11. It can be concluded that age (life cycle) effects are less important for nonblack females than for nonblack males, that schooling level is less important for females than for males, and that the time change in mismatch logits for nonblack females is much more dramatic than for nonblack males.

Results for the black groups are noteworthy mainly because the simple logit models applied to explain the data do not perform well for either group. Indeed, the baseline model, even when taking into account the differing sample sizes, does better for the black groups than for the nonblack groups. Mismatch risk is thus more homogeneous across age groups, and the variability in mismatch

logits that exists seems to imply rather complicated forms of interaction in the data. Partly because the data on which these calculations were based is rather sparse, we do not here go into more elaborate explanations of the results.

A comparison that is interesting to make is summarized in Table 13, where some of the parameter estimates from the most comprehensive model are compared for nonblack males and nonblack females. The model considered allows for an age-time interaction effect on the mismatch logits, and inspection of the parameters for this type of interaction can help to understand the life cycle dynamic for these groups. The quantities are to be interpreted as "net" age effects on mismatch, and since there is an interaction of age with time, there is a different set of effects for each year. Note that for nonblack males aged 25-34 there is a general increase in mismatch over time. For age groups 45-64, there is on the other hand a general decline in mismatch over time. And the intermediate age groups (35-44) show rather mixed tendencies. It appears as though there is some "compensation" between the older and the younger age groups over time, with the mismatch burden being most cumbersome for the young age groups over time.

For nonblack females, rather striking results are obtained, results which contrast sharply with those for nonblack males. There is a general increase in mismatch associated with ages 25-39 over time, but for at least the young groups (25-34), the increase is no different than that for nonblack males. (Compare parameter estimates for 1980 and 1970 for these two age groups, across the sexes; the sex difference in the magnitude of the change in age effects is about .02.) Thus, it appears that the age-mismatch relationship is very similar for young males and females (nonblacks), although it is quite different for the

older groups. To account for the differences between nonblack males and nonblack females more rigorously it is necessary to consider the other effects in the model, in particular the schooling-age and schooling-time interactions. It would take us far beyond the scope of the present paper to do this here, so we content ourselves with the rather descriptive results thus far advanced.

Discussion

This paper has presented a new measure of occupational mismatch, one based on an examination of the "fit" between a worker's schooling attainment and occupation. It is quite different from other measures of mismatch that have been constructed in the past, but it is at the same time more comprehensive. Using this measure it was shown that mismatch increased dramatically over the interval 1969-1980. Every comparison made showed that mismatch increase was characteristic of the period; there was no comparison for which mismatch decrease was observed. We have attempted to defend the measure of mismatch in two ways, first by elaborating its internal makeup, and second by examining the way that this measure is associated with demographic, occupational, educational, and other key variables. We speculated on how this new variable should be analyzed and interpreted, but clearly this is just a modest beginning to rigorous research on mismatch.

If it will be granted that there is validity to the measure, then the next question to ask is how this measure should be used in demographic research on labor force or labor market problems. In some other work (Clogg, 1979; Clogg and Sullivan, 1981), the mismatch measure has been considered in conjunction with other measures of underemployment or underutilization, and the general conclusion is that mismatch may well be the most important single characteristic of the labor force in recent history. Other underemployment measures fail to

demonstrate the time-trend, or overall variability, that the mismatch measure exhibits. This can be said at least for demographic comparisons, across-time comparisons, and for many other types of comparisons as well (e.g., by occupation). An increase in unemployment of .6 percent over one year is often grounds for claiming that the economy is gripped by a recession. But our results show that there was an average increase of .6 percent in mismatch per year over the 12-year period under study. When a measure is found to exhibit such an alarming trend component, registering a steady increase almost regardless of business cycle swings, the problem must surely evolve into a more systematic examination of the determinants of mismatch, and the specific consequences of mismatch for labor force members. On the policy front, we can think of many possible uses of the mismatch measure. Some of the data presented in the last sections of this paper link mismatch directly to schooling attainment, and policy response should be made partly on the basis of this data. We believe that meeting the challenges posed by a grossly over-educated work force will be one of the chief policy priorities of the next decade.

NOTES

1. Calculations based on 1970 and 1980 CPS data used in this paper.
2. Why so many individuals continue to enroll in college in the face of declining returns to college presents an enigma to Freeman's analysis. What other public investments would be more "economical" than the investment in higher education likewise receives little consideration in Freeman's analysis.
3. The existing 2-digit codes do not suffice for this purpose, since they exhibit unacceptably high within-category educational heterogeneity. For example, professional dancers and college professors are lumped into the same category in the usual 2-digit codes, and dancers typically have much lower educational attainment than college professors. Such problems imply that it would be difficult to come up with a standard that could be applied to determine mismatch prevalence for the given category as a whole. The 3-digit codes existing in the CPS or the Public Use Samples of the censuses are too numerous to be of much use with sample data, making it nearly impossible to apply criteria that would establish mismatch cut-offs for them.
4. Actually, about 1 percent of the labor force with 12 or less years of schooling were mismatched by this definition (.7 percent in 1970, 1.2 percent in 1980).
5. Throughout this paper, the definition of the labor force includes a proxy for "discouraged workers" in addition to the usual labor force (U.S. Bureau of Census). Our proxy for discouraged workers represents about 1-2 percent of the total labor force. See Clogg (1979) for further discussion.
6. We are not arguing that young educated workers are strictly "substitutable" for old educated workers with more experience, but merely assuming that there is some substitutability.

7. The 1970 classification divided "operatives" into "transportation" and "other" operatives; these two categories were combined for the relevant years.

8. Note that these figures pertain to members of the labor force; published data on schooling attainment of the population as a whole are not especially relevant.

9. For somewhat different methods of analyzing the relationship between schooling distribution and occupational distribution, see Folger and Nam (1964) and Rodriguez (1978). These researchers use demographic standardization methods to estimate "educational upgrading," which contrasts with our use of standardization or adjustment to estimate "true" mismatch increase.

TABLE 1

Mismatch Rates for the Aggregate Labor Force, 1969-1980

Year	% of Labor Force[a]	% of Full-Time, Adequate Income Labor Force[b]
1969	7.8%	9.2%
1970	7.9	9.4
1971	8.7	10.5
1972	9.3	11.3
1973	10.1	12.0
1974	10.9	13.1
1975	11.0	14.0
1976	11.7	14.7
1977	12.5	15.5
1978	13.1	16.1
1979	13.9	16.8
1980	14.2	17.4
Change 1980-1969	6.4%	8.2%
Average annual increase	.6%	.7%

a--Labor force 16 years and older, including a proxy for discouraged workers.

b--Denominator excludes discouraged workers, unemployed, part-time workers (voluntary and involuntary), low income workers. Low income workers determined as those whose average weekly wage is below 1.25 time Poverty Threshold for individual workers.

TABLE 2

Mismatch Prevalence by Five-Year Age Groups: 1970, 1975, 1980

Age Group	Mismatch Prevalence[a] 1970	1975	1980	Average Increase Per 5-Year Period, for Age Group	Average Increase for Cohort
20-24	7.4	10.9	11.5	2.1	7.3
	9.3	16.0	15.7	3.2	9.1
25-29	10.3	17.3	20.3	5.0	3.2
	11.8	21.6	24.4	6.3	3.8
30-34	10.2	15.1	21.9	5.9	2.9
	11.5	18.2	25.5	7.0	3.5
35-39	9.4	13.0	16.6	3.6	2.0
	10.7	15.7	19.3	4.3	2.5
40-44	9.0	12.3	15.9	3.5	2.1
	10.2	14.5	18.4	4.1	2.6
45-49	8.7	11.0	13.4	2.4	1.7
	10.0	13.0	15.6	2.8	2.0
50-54	8.4	9.6	13.2	2.4	1.0
	9.5	11.4	15.3	2.9	1.4
55-59	7.3	7.6	12.0	2.4	--[b]
	8.5	9.2	14.0	2.8	--
60-64	6.8	7.8	10.4	1.8	--
	8.0	9.7	12.3	2.2	--

a-- Top figure is prevalence in labor force; lower figure is prevalence in full-time, "adequate income" labor force.

b-- Averages not calculated for cohorts aged 55-64 in 1970.

TABLE 3

Some Age-Period-Cohort Logit Models
Applied to the Data in Table 2[a]

Model	Degrees of Freedom	Likelihood-Ratio Chi-Square	% of Chi-Square Explained
1. No effects	26	3070.2	--
2. Age effects	18	1694.2	44.8%
3. Age and Period effects	16	160.1	94.8
4. Linear Age and Period effects	24	790.9	74.2
5. Age, Period, and Cohort effects	7	47.3[b]	98.5

a--Models were applied to data producing the second set of prevalence rates in Table 2, i.e., the universe "exposed" to mismatch was the full-time, adequate-income labor force in each age-period combination.

b--The index of dissimiliarity between observed and estimated expected frequencies was .4 percent, indicating a very acceptable level of fit. The sample size was 155,139.

TABLE 4

Parameter Estimates for Age, Period, and Cohort Effects on Mismatch Logits

Period	Estimated Effect	Possible[a] Bias, + or -	Age	Estimated Effect	Possible Bias, + or -	Cohort	Estimates Effect	Possible Bias, + or -
1970	-.40	.02	20-24	-.74	.08	1	1.10	.10
1975	-.11	.01	25-29	-.21	.07	2	1.15	.09
1980	.00[b]	--	30-34	-.04	.06	3	1.01	.08
			35-39	-.03	.05	4	.65	.07
			40-44	.06	.04	5	.48	.06
			45-49	.05	.03	6	.32	.05
			50-54	.07	.02	7	.22	.04
			55-59	.01	.01	8	.15	.03
			60-64	.00[b]	--	9	.03	.02
						10	.00[b]	.01
						11	.00[b]	--

a--Sensitivity analysis, to determine possible bias, was carried out by assuming that the equality constraint on cohorts 10 and 11 could be in error by .01 in either direction.

b--Denotes restriction. The parameter estimates can be regarded as "effects" of dummy variables for the age, period, or cohort categories with these categories excluded.

TABLE 5

Mismatch Prevalence for Sex-Color-Age Groups[a]

Age	Nonblack Males 1970	1975	1980	Black Males 1970	1975	1980
20-34	11.5%	17.6	21.4	5.9	12.5	15.9
35-49	11.7	15.5	19.6	7.1	10.8	14.0
50-64	8.3	10.0	14.6	5.6	6.2	8.5
Total labor force	9.8	13.6	17.4	5.6	9.5	12.4
	Nonblack Females			Black Females		
20-34	6.6%	10.6	13.7	6.4	7.9	14.0
35-49	5.0	7.4	10.4	7.3	7.2	11.5
60-64	6.8	6.7	9.2	7.0	6.6	9.1
Total labor force	5.5	7.7	10.4	6.2	6.7	11.1

a--Mismatch prevalence as percent of labor force.

TABLE 6

Absolute and Relative Increases in Mismatch
1970-1980, for the Age-Sex-Color Groups

Age	Nonblack Males		Black Males	
	Absolute	Relative	Absolute	Relative
20-34	9.9%	86%	10.0%	169%
35-49	7.9	68	6.9	97
50-64	6.3	76	2.9	52
Total labor force	7.6	78	6.8	121

	Nonblack Males		Black Males	
	Absolute	Relative	Absolute	Relative
20-34	7.1%	108%	7.6%	119%
35-49	5.4	198	4.2	58
50-64	2.4	35	2.1	30
Total labor force	4.9	89	4.9	79

TABLE 7

Mismatch Prevalence by Major Occupation, Selected Years

Occupation Group[a]	Mismatch as % of Labor Force in Occupations 1969	1975	1980	Difference, 1980-1969	Average Annual Rate of Change
Professional	10.6	10.8	13.9	3.3	.3%
Managers	18.0	24.1	27.2	9.2	.8
Clerical	8.1	12.3	12.9	4.8	.4
Sales	8.8	12.7	13.6	4.8	.4
Crafts	7.4	10.2	13.6	6.2	.6
Operatives	3.9	6.2	7.6	3.7	.3
Laborers	3.8	6.4	6.4	2.6	.2
Private housework	4.0	6.4	2.1	-1.9	-.1
Other service	6.4	10.6	11.3	4.9	.4
Farmer/Manager	.1	.2	1.2	1.1	.1
Farm Laborer	6.2	14.7	10.4	4.2	.4

a--1960 Census major occupation groups.

TABLE 8

Mismatch Prevalence Adjusted for Occupational Distribution Changes

Type of Adjustment	1980 Rate[a]	1969 Rate	Difference
1. None (crude rates)	13.0	7.9	5.1
2. 2-factor occ x period	12.9	7.8	5.1
3. 2-factor occ x period and 3-factor occ x period x mismatch	13.5	7.5	6.0
4. 2-factor, with 1969 as "standard"	12.9	7.9[b]	4.9
5. 2-factor and 3-factor, with 1969 as "standard"	14.2	7.9[b]	6.3

a--Mismatch prevalence rates defined for labor force members with occupations. Data in other previous tables were based on total labor force.

b--With 1969 singled out as a base or standard, the adjusted rate for 1969 equals the crude rate.

TABLE 9

Mismatch Prevalence for Labor Force Members with at Least Some Post-High School Schooling

Completed Years of Schooling	Period[a] 1970	1975	1980
13-15	20.3%	25.5	28.7
16	38.9	40.0	46.8
17+	38.1	41.2	45.3

a--Rates in each period calculated as percent of labor force with schooling level indicated.

TABLE 10

Educational Composition Changes and Mismatch:
Crude and Adjusted Rates

Completed Years of Schooling	1970 Mismatch Rate	1970 Percent in Category	1980 Mismatch Rate	1980 Percent in Category
12 or less	.7%	74.8%	1.2%	64.2%
13-15	20.3	12.9	28.7	17.7
16	38.9	7.4	46.8	10.4
17+	38.1	4.9	45.3	7.7

	Summary Rates[a] 1970	1980	Difference 1970-1980
1. Crude rate	7.9%	14.2%	6.3
2. Standardized rate (1970 standard)	7.9	9.9	2.0
3. Standardized rate (1980 standard)	11.0	14.2	3.2
4. Purged of ed x period	8.8	12.9	4.1
5. Purged of ed x period and 3-factor	9.0	12.7	3.7
6. Line 4, 1970 standard	7.9	11.6	3.7
7. Line 5, 1970 standard	7.9	11.3	3.4
8. Line 4, 1980 standard	9.8	14.2	4.4
9. Line 5, 1980 standard	10.1	14.2	4.1

a--Summary rates in lines 2 and 3 based on conventional standardization. Summary rates in lines 3-9 based on log-linear model. See Clogg (1978).

TABLE 11

Mismatch Prevalence for Sex-Color Groups
in Labor Force with at Least Some Post-High School Schooling

Sex-Color Group	Completed Years of Schooling	Period 1970	1980	Absolute Increase
Nonblack Males	13-15	20.6%	32.5%	11.9
	16	48.5	53.9	5.4
	17+	39.7	47.1	7.4
Nonblack Females	13-15	17.9	21.3	3.4
	16	23.0	35.7	12.7
	17+	32.5	41.2	8.7
Black Males	13-15	39.1	42.5	3.4
	16	34.9	55.6	20.7
	17+	40.4	46.5	6.1
Black Females	13-15	23.3	32.6	9.3
	16	18.0	41.9	23.9
	17+	45.9	48.8	2.9

TABLE 12

Logit Models Applied to the Schooling x Age x Time x Mismatch Table[a]

Effects on Logits	Degrees of Freedom	Likelihood-Ratio Chi-Square: Nonblack Males	Nonblack Females	Black Males	Black Females
None	71	1972.5	487.3	133.0	123.5
S,A	62	575.1	198.6	120.1	99.5
SA	48	341.9	189.2	104.4	88.4
SA,T	46	131.9	98.5	97.5	59.9
SA,TA	32	70.0	72.2	70.5	42.5
SA,TS	42	89.6	62.4	88.7	54.8
SA,TA,TS	28	30.0	35.8	62.6	37.6
		Percentage of Baseline Chi-Square Explained			
S,A		71%	59%	10%	19%
SA		83	61	22	28
SA,T		93	80	27	51
SA,TA		96	85	47	66
SA,TS		95	87	33	56
SA,TA,TS		98	93	53	70

a--The schooling variable (S) had three categories (13-15, 16, 17+); the age variable (A) had eight categories (25-29, ..., 60-64); the time variable (T) had three categories (1970, 1975, 1980). The mismatch dichotomy is based on classification of full-time, adequate-income labor force members.

TABLE 13

Net Age Effects on Mismatch Logits

Age Effects	1970	1975	1980
	Nonblack Males (Constant = -.284)		
25-29	-.178	.142	.196
30-34	.006	.068	.222
35-39	.006	.094	-.022
40-44	.016	-.002	.082
45-49	.126	.072	.014
50-54	.124	-.170	-.008
55-59	.026	-.110	-.144
60-64	-.128	-.096	-.340
	Nonblack Females (Constant = -.712)		
25-29	-.260	.150	.008
30-34	-.346	-.112	-.098
35-39	-.278	-.122	-.206
40-44	-.016	.068	-.040
45-49	.214	-.090	-.038
50-54	.200	.098	.128
55-59	.310	-.078	.098
60-64	.176	.086	.146

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