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MARKET AND INSTITUTIONAL SOURCES OF EDUCATIONAL GROWTH

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ABSTRACT

This paper synthesizes research from several traditions on causes of change in the level of formal educational attainment in the United States for cohorts born during the first half of the twentieth century. It considers two general sources of educational growth: (1) changing population composition on family background factors that affect how far individuals go in school, and (2) changing characteristics of the labor market and educational organization that are experienced uniformly within cohorts but vary over time such as to alter incentives to stay in school. Using the 1973 Occupational Changes in a Generation Survey and published economic and schooling data, we simultaneously consider individual and societal level causes of change in school attainment. Net of intercohort change in social background composition, we isolate the effects of persistent economic incentives to acquire schooling on grade progression rates. But these effects fail to dominate the data. Instead, cohorts are also highly responsive to short-run fluctuations in schooling costs and to the favorability of institutional conditions under which schooling takes place. With regard to the latter, in contrast to previous cross-sectional findings, school quality indicators strongly affect educational growth. Levels of educational expenditures, teacher salaries, within-year attendance, and school consolidation all vary directly with grade progression and, in places, mediate the effects of economic incentives.

MARKET AND INSTITUTIONAL SOURCES OF ECONOMIC GROWTH[1]

The increasing level of formal educational attainment in America is one of the most important social trends in the twentieth century. Cohorts born during the first five years of the century achieved a median of 8.6 grades of schooling, while cohorts born at mid-century had a median of 12.8 grades (U. S. Bureau of the Census 1960, p. 216; 1976a). In 1910 62.5 percent of Americans aged 5 to 19 years were enrolled in school; in 1974, 89.4 percent were enrolled (U. S. Bureau of the Census 1960, p. 214; 1976b). The educational composition of the population has implications for many dimensions of social life, including the economic and social standing of individuals and occupations, political attitudes and behavior, consumption patterns, social participation, and family formation. Yet our understanding of the causes of change in average educational levels is incomplete. A number of obviously relevant social trends accompany the rise in attainment: Real family incomes have increased substantially throughout the century; the farm population has dwindled and the urban population and non-farm segments of industry have grown; skills and formal credentials required in the labor market have also increased; the economic advantage to comparatively well-educated workers has persisted; laws requiring students to remain in school for larger fractions of their adolescence have been passed; and at all levels, school systems have been bureaucratized, extended, and enriched. Citing these and other obvious social changes is a first step to understanding the changing educational composition of the American population. But it remains to specify which, if any, of these changes have caused successive generations of students to remain in

school for lengthier portions of their lives, and which are merely collateral developments. To answer this question requires formulating a number of arguments that may explain educational growth and empirically unravelling the influences of numerous collinear social trends. As a contribution to accounting for the increase in the formal educational status of the American population, this paper presents an analysis of the effects of market and institutional factors on change in levels of formal schooling for white American males born during the first half of the twentieth century.

To account for educational growth is to synthesize two research traditions, which may be termed the social demographic and macroeconomic perspectives. The social demographic sources of change in formal school attainment are intercohort changes in population composition on social background factors that, within cohorts, affect how far indiviuals go in school. Individuals' formal school attainments are affected by their parents' schooling levels, their numbers of siblings, the income and occupational statuses of their parents, and their places of origin (Blau and Duncan 1967; Duncan 1965; Duncan, Featherman, and Duncan 1972; Jencks 1972; Sewell and Hauser 1975; Mare 1977b). Thus, intercohort changes in average levels of these background factors induce changes in average cohort attainment levels. Increases in the levels of schooling, occupational status, and income and decreases in average numbers of siblings and the fractions of cohorts raised on farms induce intercohort increases in average grades of school completed and grade progression rates over cohorts born during the twentieth century (Hauser and Featherman 1976; Winsborough and Sweet 1976; Mare 1977a, 1977c).

The macroeconomic sources of educational change are aggregate market incentives, such as the costs or the monetary returns to formal schooling. There is evidence that individuals respond to changes in the direct and opportunity costs of formal schooling as well as the perceived long-run economic benefit to continued schooling in deciding whether to remain in school (Duncan 1965a; Freeman 1975, 1976). Over cohorts, therefore, levels of formal schooling reflect changes in the costs and returns of schooling which are experienced approximately uniformly within cohorts but vary over time.

While each of these perspectives partially accounts for change in schooling levels, neither, by itself, explains educational change in twentieth-century America. Inasmuch as family background composition changes slowly over cohorts, it cannot account for short-run fluctuations in levels of attendance or attainment. More important, only between one-third and one-half of intercohort change in highest grade of schooling completed or grade progression rates during the twentieth century (for males) is due to measured background compositional change (Hauser and Featherman 1976; Mare 1977c). On the other hand, aggregate econometric models of market effects on attendance are seriously misspecified insofar as they fail to take account of intercohort change on family background. The apparent effect of persistent economic advantages to persons with higher levels of schooling on aggregate school attendance decisions may be confounded with the effects of secularly rising parental socioeconomic levels.

This suggests that much can be learned from considering within a common analytic framework the impact of changing population composition on factors affecting school attainment at the individual level and changing market incentives to continue with schooling. Such an analysis has a number of benefits.

First, it contributes to our understanding of the sources of educational growth. Recent debate focuses on the relative emphasis to be accorded "consumption" and "investment" motives for changes in the aggregate accumulation of schooling. The dominant source of educational growth may be accelerated competition for relative economic status through the acquisition of formal schooling (Boudon 1974; 1976). Alternatively, a broader set of cultural and economic influences may determine cohort attainment levels (Hauser 1976). Despite the importance of this issue for understanding change in advanced societies, little evidence has informed the arguments. Although the issue is ill-suited to precise comparisons of competing hypotheses, assessing market effects on school accumulation, after taking account of changes in family factors, casts light on the broader debate. In particular, it shows whether the perceived market value of school credentials can indeed account for intercohort changes in average levels of educational attainment not due to changes in the desire for schooling engendered by changes in the social backgrounds of students.

Second, simultaneous analysis of market and family effects on changes in schooling levels should lead to improved methods for forecasting enrollments generally, college enrollments in particular. Dresch (1975)

criticizes forecasts which estimate future enrollments mainly as fixed fractions of "college-age" cohort size. Arguing that students make attendance decisions in response to anticipated payoffs to college education, and that this is a period of oversupply of college graduates, he concludes that the relative earnings of graduates will fall and enrollments will plunge further than age structure changes imply. On the other hand, Hauser and Featherman (1976) and Winsborough and Sweet (1976) emphasize the potential for future educational growth. Continued intercohort increases in parental socioeconomic status and declines in sibship size augur increases in average levels of schooling. By themselves, neither of these perspectives yields forecasts upon which social policy should rest. It is more satisfactory to forecast the outcomes of both population compositional change and change in the market for college-educated labor. The present paper does not go so far as to develop forecasting models, but it reports the effects of a number of historical determinants of attendance which can be used in a comprehensive forecasting scheme.

A third benefit to this approach to studying educational change is that it permits observation of effects of other organizational and political factors on changes in school attainment levels. The character of school systems; political support for education; and legal statutes regulating the school attendance, military service, and labor force participation of youth change over time and may affect cohorts' propensities to drop out of school. Such hypotheses can be fruitfully examined within a design which takes account of changes in cohort composition on family

background factors and macroeconomic determinants of school attendance. The analysis described herein considers a number of orgainzational sources of educational change. These are discussed more fully below.

Finally, the analysis has didactic value. Recent critics of research on socioeconomic attainment argue for more focus on institutional and labor market effects on individuals' achievements (Spilerman 1977). This should certainly be true for change in educational attainment levels, where social background composition accounts for only a fraction of observed intercohort increases during the twentieth century. The present study, therefore, embodies a strategy for the analysis of both individual-level and market and institutional influences on an important socioeconomic outcome, namely formal school attainment. It illustrates not only methods for societal-level analysis based on limited data but also the difficulties of such analyses.

We have emphasized the value of simultaneously examining the effects of compositional and macro-level determinants of educational growth. This report, however, focuses on the latter. Since the validity of the analysis turns upon proper adjustment for composition on individual-level variables, we discuss this adjustment. But we present no empirical results on background compositional effects on average educational attainment. The latter are treated by Hauser and Featherman (1976) and Mare (1977c). Concentrating on market and institutional effects, we address more problematic issues. [For a strictly macro-level analysis of educational trends since World War II, see Felson and Land (1978).]

The paper is organized as follows: The first section enumerates market and institutional influences on educational change that have been considered in previous research and are amenable to study with available data, and outlines mechanisms through which they may affect school attendance. The second section discusses the measurement of formal schooling and describes our data on social background and schooling. The third section focuses on measurement of market and institutional conditions and presents specific hypotheses. Section four outlines methods for exploratory time-series analysis of school attainment. Section five presents our empirical results. The paper concludes with a summary of our findings and future research needs.

MARKET AND INSTITUTIONAL INFLUENCES

We consider three structural sources of change in levels of formal school attainment. These include the economic returns to formal schooling, the costs of schooling experienced by households, and characteristics of the organization of schooling that determine access to or quality of schooling. Although we interpret these influences broadly, they do not exhaust factors that may affect school attainment. Political factors, such as compulsory school attendance or child labor legislation may induce increases in attendance rates. Whether this is true or, alternatively, whether legislative developments merely rationalize social trends engendered by other social, economic, and demographic processes is hard to determine with extant data. Data limitations restrict our analysis to accounting for educational changes at the national

level; to wit, we do not take account of regional differences in the rate or the sources of educational change. At the national level, however, legislative conditions regulating school attendance have changed little during the twentieth century. Variation in legislation bearing on attendance, for example, is mainly cross-sectional (interstate) rather than longitudinal.[2] At the national level, therefore, a legal variable which, prima facie, may affect school attendance is nearly constant when aggregated. Such factors are better studied at the state level where their interstate variation can be exploited and qualitative interstate differences in statutes can be taken into account. Thus we focus on structural determinants of schooling which change significantly when monitored for the nation as a whole.

Economic Returns to Schooling

Persistent labor market advantages to persons with high educational attainment levels may induce successive cohorts to accumulate progressively higher levels of schooling. During the twentieth century, the most rapidly growing occupations and industries have work forces with higher than average educational attainment (Dresch 1975; Gordon 1974). This apparently keeps the demand for relatively well-educated workers high enough to maintain their earnings advantage. But even ignoring changes in occupational and industrial structures, there may be a continued incentive for successive cohorts to demand larger amounts of schooling. Boudon, for example, argues:

> When each demands more education than a similar individual would have demanded some time before, this has the effect

> of increasing the price in terms of years of education that all have to pay to get a given social status,...(1976, p. 1185).

Thus, increases in formal school attainment due to such other causes as rising parental socioeconomic levels cause further increases in the demand for schooling as individuals strive to attain the highest relative socioeconomic position they can.

This implies that persons make school continuation decisions by taking stock of the anticipated economic benefit of staying in school. A key source of these anticipations is the relative fortunes of persons already in the labor force who have acquired different amounts of schooling. That is, significant reference groups for students are labor force groups differentiated by their educational standing. Workers with more schooling typically receive higher economic rewards than those with less. When the relative advantage to additional schooling experienced by workers in previous cohorts increases, cohorts currently in school are, ceteris paribus, more likely to stay there. Conversely, when the relative advantage declines for previous cohorts, persons in school are more likely to drop out.

Although this argument is plausible, there has been little attempt to examine it empirically. While there has been some aggregate time-series analysis showing a relationship between education-specific earnings levels and attendance, this has been restricted to the post-World War II era and to college attendance rather than the full range of

educational experience (Freeman 1975; 1976). Data on the returns to schooling are limited, thereby restricting analysis of the effects of relative economic benefits of schooling on attendance, but it is possible to progress beyond past attempts to isolate the effects of market incentives.

Costs of Schooling

Costs of schooling include both direct and opportunity costs. The former include transportation, tuition, supplies, and books; the latter include employment opportunities foregone while attending school. What little research has examined direct costs effects on school attendance has been restricted to cross-sectional analysis of college attendance decisions (Radner and Miller 1975; Kohn et al. 1974; Corazzini et al. 1972). There has been little attempt to assess whether fluctuations in the capacities of households to meet the costs of keeping children in grade and high school affect cohort attendance rates. Nor do we know whether college attendance rates over the twentieth century are affected by the costs of college experienced by the household. Our analysis permits tentative inferences about these relationships.

The effects of foregone employment opportunities on school retention rates are better documented. When unemployment is high there is relatively little incentive for students to drop out and seek work. When unemployment is low, work opportunities are more plentiful and the appeal of further schooling is diminished. Thus, unemployment and school attendance should vary directly. Evidence supporting this

hypothesis is reported by Duncan (1965a), Corazzini et al. (1972), Crean (1973), and Duncan (1974). Nonetheless, it remains unclear that fluctuations in employment opportunities affect attendance. There are other possible explanations for the historical correlation between unemployment and attendance. During the depression, for example, high school continuation rates exceeded their long-run upward trend, presumably due to the lack of work opportunities for youth. At the same time, however, low depression fertility levels made families typically smaller than at earlier or later periods. Since intercohort declines in average sibship size raise average school attainment, higher depression continuation rates may result from favorable family background composition. Alternatively, they may have been due to the favorable labor market position of well-educated workers, inasmuch as the latter typically held jobs relatively immune to unemployment. These alternative explanations for the attendance-unemployment correlation suggest that the effects of schooling costs should be examined in a multivariate context taking account of change in family background composition.

Institutional Characteristics

The third set of macro-level factors which may facilitate educational growth are characteristics of educational institutions. Accompanying secular increases in average school attainment levels are trends in indicators of school quality and accessibility. School facilities and personnel expand, school curricula become more extensive, schools consolidate, children spend increasing fractions of their time

in school, and the qualifications of teachers increase. In part these changes are institutional responses to increases in school attendance induced by other demographic and institutional changes. But changes in the formal educational system may themselves affect attendance. Improvements in instructional quality resulting from more and better teachers and increased resources allocated to learning facilities and materials may increase students' awareness of the nature and rewards of further schooling and their ability to go on with it. A formal age-graded system of schooling, with a diversified curriculum tailored to the diversity of student aptitudes, diffuses through the nation. While schools consolidate, their organization and curriculum become favorable to higher attendance. With the decline in the decentralized rural schools comes a corresponding institutionalization of norms of regular, uninterrupted attendance and an age-graded class organization. At the post-secondary level institutions proliferate, making higher education available to more high school graduates. All of these changes make it both physically and intellectually easier to acquire formal schooling and may, therefore, affect school attainment levels.

Research to date provides scant support for these conjectures. Cross-sectional studies indicate little effect of school characteristics on academic performance once school social background composition of students is taken into account (Coleman et al. 1966; Hauser 1971; Jencks et al. 1972; Alwin and Otto 1977). Important exceptions, however, are analyses showing that the quantity of schooling obtained during a

year varies directly with achievement (Wiley 1976; Heyns 1976). There is also little apparent independent effect of the physical accessibility of higher educational institutions on college attendance (Anderson, Bowman, and Tinto 1972). But negative cross-sectional findings do not preclude long-term effects of change in aggregate features of the American educational system on attendance, given the substantial expansion of resources allocated to schooling during the twentieth century. Still, schooling trends are collinear with other determinants of educational growth such as family socioeconomic levels. On balance, therefore, it is useful to examine the effect of school characteristics on attainment taking account of the other factors already discussed.

The economic returns, costs, and organization of schooling are possible sources of educational growth that can best be studied by first taking account of intercohort changes in composition on social background factors affecting schooling at the individual level. At the same time, however, these factors present hard conceptual and measurement problems. We discuss these after discussing the measurement of schooling per se.

SCHOOLING AND SOCIAL BACKGROUND

We regard formal schooling as a sequence of age-grade transitions during a cohort's early life cycle. For the individual, schooling is a set of dichotomous events conditional upon previous events--to wit, whether he attains one schooling level given that he attains the previous level. Correspondingly, for the cohort, school attainment is

denoted by grade progression rates. Examining schooling as a sequence of events enables closer study of schooling by showing how family influences change over the schooling years (Mare 1977b). More important, by this approach, each step in the schooling process for each cohort can be dated with respect to societal events occurring at that step. For a cohort, schooling occurs over many years. Thus, different values of a macro-level determinant of continuation rates may affect the odds of continuing at different school transitions. Moreover, several cohorts at different stages of their schooling may be affected by the same social conditions. Finally, different macro-level variables may affect continuation rates at different attainment levels. In short, educational attainment is the outcome of a series of period-specific influences. To specify an interpretable relationship between average completed grades of schooling and the sequences of aggregate-level independent variables is difficult if not impossible. On the other hand, piece-wise analysis of the schooling process allows precise specification of period influences and thus affords more interpretable results.

We focus on four school transitions: Whether the individual (1) completes elementary school (completes 8th grade); (2) attends high school given that he completes elementary school (attends 9th grade given 8th grade completion); (3) graduates from high school given that he attends high school (completes 12th grade given 9th grade attendance); (4) attends college given that he graduates from high school (attends 13th grade given 12th grade completion). These

transitions are the points in the American schooling process where the greatest attrition has historically occurred (Duncan 1968, p. 640), and they denote movement between and across the major divisions of the educational system.

Since most of each cohort finishes school before age 25, cross-sectional observations on highest grade of school completed reveal the grade progression decisions of cohorts observed in the cross section. Our data source is white males in the 1973 Occupational Changes in a Generation Survey (OCG) of the civilian noninstitutional male population born 1907-52 (Featherman and Hauser 1975). The OCG data include measures on a number of social background factors that affect school continuation decisions. To adjust cohort grade progression rates for cohort differences in social background composition we use the following: father's grades of school completed, mother's grades of school completed, annual family income when the respondent was 16 years old in constant (1967) dollars, father's occupational status in units of the Duncan socioeconomic index when the respondent was 16 years old, number of ever-living siblings, a dichotomy equalling one if the respondent did not live with both parents most of the time up to age 16 and zero otherwise, a dichotomy equalling one if the respondent was born in the South census region and zero otherwise, and a dichotomy equalling one if the respondent lived on a farm at age 16 and zero otherwise. For substantive discussion of how these variables' effects vary over school transitions, see Mare (1977a; 1977b).

Our analysis relies on retrospective reports of social background and educational attainment. This raises two problems -- sample

unreliability or unrepresentativeness and intracohort variability in the timing of schooling. Relying on retrospective reports is a potential source of bias because our data are not a representative sample of persons in each cohort who attended American schools during this century. Cross-sectional observations, then, are affected by differential mortality, international migration, and age-related faculty recall of experience. Analysis reported elsewhere considers the first two of these sources of bias and determines that they have no important effects on estimated grade progression rates (Mare 1977a, Ch. 4). Fienberg and Mason (1977) present evidence that nonrandom faculty recall of schooling has negligible effects on grade progression estimates from retrospective reports.

Intracohort variability in the timing of schooling poses serious conceptual problems for examining macro-level effects on schooling. Many persons delay or interrupt their schooling because of late starting, failures, or leaves of absence. Since not all persons in a birth cohort take each phase of their schooling in the same year, cohorts respond to heterogeneous economic and institutional conditions in making their school continuation decisions. Cohort grade progression rates, then, may poorly represent grade progression rates in each period and grade progression rates adjusted for social background are not meaningful functions of period-specific macro-level phenomena for each school transition. Instead, they are weighted averages of macro-level variables where the weights change with the nature and extent of age-grade abnormality. To take account of this problem, we

estimate the years when persons take each year of their schooling, using OCG information on the incidence and timing of school interruptions. From this information we construct period grade progression rates; that is, ratios of persons at, say, the (i+1)st level of schooling in year t+1 to persons at the ith level of schooling in year t. The time-series of these ratios can then be meaningfully related to series of macro-level independent variables. When suitably adjusted for social background composition, these period rates, rather than the directly observed cohort rates, are used in our analysis.[3]

To examine change in grade progression net of cohort differences in social background composition, we examine effects of social background and period on each school transition. We use a logit specification to estimate the effects because school continuation decisions are dichotomous outcomes. If p_{ijt} is the probability that the ith individual in the tth cohort makes the jth school transition, and X_{ik} is the value on the kth social background characteristic for the ith individual, the model is

$$\log_e \frac{p_{ijt}}{1-p_{ijt}} = \alpha_{jo} + \sum_k \alpha_{jk} X_{ik} + \lambda_{jt} \qquad (\sum_t \lambda_{jt}=0) \tag{1}$$

where α_{jk} and λ_{jt} are parameters. Constrast among the λ_{jt} denote differences in the log odds of school continuation among persons making the jth school transition in different years adjusted for differences in composition on the X_{ik}. Using (1) we estimate the adjusted log odds of grade progression as

$$\lambda^*_{jt} = \hat{\alpha}_{jo} + \sum_k \hat{\alpha}_{jk} X_{ik} + \hat{\lambda}_{jt} \tag{2}$$

where the $\hat{\alpha}_{jk}$ and $\hat{\lambda}_{jt}$ are the maximum likelihood estimates of the α_{jk} and λ_{jt} respectively.

With estimates of adjusted log odds of grade progression in hand, we assess the effect of societal-level causes of schooling changes via time-series analysis. Then, if Z_{jmt} is the value of the mth macro-level variable affecting the continuation rate for groups making the jth school transition in the tth year, we estimate macro-level models of the form

$$\lambda^{*}_{jt} = \delta_{jo} + \sum_{m} \delta_{jm} Z_{jmt} + \varepsilon_{jt} \qquad (3)$$

where δ_{jm} are parameters and ε_{jt} is a stochastic disturbance. Methods of estimating (3) depend upon the dynamic properties of the ε_{jt}, which may differ over equations. The next section discusses the aggregate independent variables Z_{jmt}.[4]

MARKET AND INSTITUTIONAL FACTORS: MEASURES AND HYPOTHESES

Following the earlier discussion, we consider three classes of variables affecting grade progression rates: the economic returns to schooling, the costs of schooling, and characteristics of educational organization.

Economic Returns to Schooling

To assess the argument that persons take account of the economic value of schooling, as indicated by the experience of previous cohorts, is difficult given limitations in both theory and data on how persons

perceive and interpret the value of schooling. Determining how persons use the experiences of earlier cohorts to evaluate schooling presents the classical difficulties of reference-group theory (Merton 1968, Chs. 10-11). On the one hand, it is plausible that individuals make schooling decisions using information about the fortunes of groups who have made the decisions in the recent past. On the other hand, as in all reference group formulations, it is unclear which groups are in fact the reference groups used by individuals.[5] We must, therefore, face the problem of specifying the intergroup comparisons made by persons deciding whether to continue in school.

In the one previous attempt to examine the effects of economic returns to college on attendance, Freeman (1975; 1976) shows that college enrollments vary directly with the earnings of college graduates relative to those of all workers since World War II. Unfortunately, earnings data specific to education level are unavailable for most of the period to which our dependent variables pertain, making it unfeasible to extend straightforwardly Freeman's approach. An alternative way to measure economic incentives is to eschew direct earnings measurement and, instead, examine the time-series of occupational opportunities of labor force entrants with varying amounts of schooling. This information was obtained in the OCG survey which asked respondents to report their first full-time civilian job after school completion. To each individual we assign the median earnings for his detailed first occupational title based on the 1970 Census (U. S. Bureau of the Census 1973, pp. 368-381). Then we use the joint distribution of first job,

earnings, schooling level, and cohort to model the average starting earnings for each cohort at each level of schooling.[6] The relationships between (imputed) earnings of first occupation and educational attainment for recently preceding cohorts, by the arguments above, affect the grade progression decisions of persons still in school.

We can express this formally by letting Y^1_{it} be the earnings (that is, the average of the occupational median earnings scores) of those members of the ith cohort who make the tth school transition; Y^2_{it} be the earnings of those members of the ith cohort who make the (t-1)st transition but do not make the tth transition; and λ_{it} be the background adjusted log odds of making the tth school transition for the ith cohort. Then

$$\lambda_{it} = F(Y^1_{(i-p)t}, Y^1_{(1-p-1)t}, \ldots, Y^1_{(i-p-j)t}, Y^2_{(i-q)t}, \ldots, Y^2_{(i-q-k)t}) \quad (4)$$

where p, j, q, and k define the cohorts whose experiences affect the decisions of the ith cohort. That is, the adjusted log odds of grade progression is a function of the average first occupation earnings for members of previous cohorts who made and who did not make the tth school transition. In practice, we consider ratios of the Y^1_{it} to the Y^2_{it} for a limited number of cohorts. For a single ratio, then, we have

$$\lambda_{it} = F(Y^1_{mt}/Y^2_{nt}) = F(Y^1_{n+s}/Y^2_n)_t \quad (5)$$

where $s = n - m$. We shall have occasion to refer to (5) in discussing our findings.

The first occupation-based measures approximate the initial returns to schooling for the cohorts we consider in our analysis. The measures, however, present several conceptual problems.[7] These include, first, using first-occupation-based measures, when measures of the rewards to schooling later in life are more appropriate; second, specifying the cohorts that persons compare when evaluating the returns to grade progression; and third, specifying the educational groups that they compare.

First job earnings are not the only signal which individuals use to determine the economic rewards of schooling. They may also examine the earnings of workers in their prime working ages, job mobility patterns, or the relative chances of being unemployed of workers with different educational statuses. Since each of these possibilities may hold for some individuals in a cohort, grade progression rates depend on a distribution of perceived rewards of schooling, only an element of which is captured in our first-job measure.

This has implications for our analysis. We would like to specify the maximum lag at which the previous cohorts can affect decisions of persons in school. If first jobs are the most important basis for perceptions of the relative value of schooling, then the cohorts that an individual observes in making decisions are probably only three or four years older than he since the experiences of earlier cohorts poorly represent the future. In terms of (4), j and k would be at most three or four and the earliest cohorts who affect the decisions of the ith cohort are the (i-p-3)rd or (i-p-4)th for persons

facing the tth school transition. But if first jobs are not an important basis for perceptions, then it is hard to rule out much larger values of j and k. We may find no effects of Y_t^1 and Y_t^2 on λ_{it} because they are the wrong variables, but more likely if rewards after first job are more important, their effects will appear through their association with first job earnings. Thus, even though individuals rely upon their view of the labor market during the three or four years before their decisions, the cohorts whom they look at may be workers who entered the labor force 10 to 15 years in the past. Then if first job earnings is correlated with later earnings, empirical versions of (4) will show that a cohort is influenced by the experience of cohorts born, say, 10 to 15 years earlier. If the range is in fact 10 to 15 years, then p = 10, j = 5, q is a number slightly less than or equal to 10, and k is approximately five. We may then detect effects of returns on grade progression even when the first job earnings of previous cohorts do not directly affect perceptions of the returns to schooling. But for such effects to emerge it is necessary to estimate (4) for values of p, q, j, and k which are too large to denote the effects of relative first job earnings. Because earnings at first and subsequent jobs are correlated, the large lags may reflect the effects of returns to schooling realized later in life.

A second issue is determining the cohorts that the individual uses to ascertain the relative value of grade progression. We have discussed identifying the lag at which previous cohort experience affects the decisions of an individual in mentioning the weakness of first-job-

based measures when later labor force experiences are more relevant. It is a matter to be resolved empirically for each school transition. For a given lag between the experience of a previous cohort and the current cohort's continuation decision, we must also determine the pairs of groups who make and do not make the transition in question who are compared by cohorts. Individuals may compare members of the same cohort who complete different amounts of schooling; they may make intercohort comparisons between persons with different amounts of schooling but who enter the labor force at the same time; or, they may make comparisons less precise than strictly within-schooling cohorts or within-labor-force-entry cohorts. Again, we do not know which comparisons are the most prevalent component of cohorts' aggregate perceptions. We use an exploratory strategy to determine the lags at which previous cohorts' experiences affect the decisions of the current cohort by considering alternative formulations and reporting those that are the best evidence for economic returns effects on grade progression. This procedure is discussed in the next section.

A third problem is that we do not know which education groups in preceding cohorts are compared by individuals in making their grade progression decisions. Equation (4) indicates that at each transition persons compare two sets of groups--previous cohort members who made the transition and previous cohort members who dropped out at the transition. But other comparisons are possible, such as between persons who make the transition but make no subsequent transitions.

and persons who drop out at the transition. In short, cohorts have an unknown distribution of aspirations and perceptions of the fortunes of previous cohorts. Our analysis assumes the typical individual compares persons in previous cohorts who make the transition and persons who do not and ignores intracohort differences in foresight, ambition, and sophistication. We may thereby overlook some effects of perceived returns to schooling on grade progression, but given our ignorance about how cohorts use available information, this is the best that we can do.

To summarize, we assess the argument that individuals appraise the economic reward to schooling by observing the first jobs obtained by persons in previous cohorts who faced the school transition in question. When previous cohort members who make the transition take jobs that are well paying relative to jobs entered by dropouts, then the progression rates of cohorts still in school are relatively high. When, by contrast, the relative first-job earnings of previous cohort members who make the transition are lower, grade progression rates of cohorts in school are lower. There are limitations of the first-job-based measure of the returns to schooling. It fallibly measures experiences of older workers (which provide better information on the value of schooling) and does not take account of intracohort differences in perceptions and aspirations.

Since our understanding of the aggregate perceptions of a cohort's value of schooling is limited, we approach the data analysis exploratively enhancing the possibility of mistakenly identifying

random fluctuations in the data as relative earnings effects. On balance, however, if, as some have claimed, the source of educational growth in western industrial societies is the perceived need to acquire increased schooling to maintain one's relative socioeconomic position, then the effects of the returns to schooling should show up despite data limitations.

Costs of Schooling

For the cohorts for which we estimate adjusted grade progression rates data on schooling costs are limited. Below college, there are no recorded series of direct schooling costs. For college, there is a biennial series on aggregate income of institutions of higher education from student fees (U. S. Bureau of the Census 1975, p. 384). When evaluated relative to higher educational enrollment, this estimates the costs born by students.[8] If cohorts respond to the direct costs of schooling, fees assessed per student should negatively affect the adjusted log odds of progression from high school graduation to college attendance.

Though we cannot directly measure schooling costs below college, we can measure the aggregate capacity of families to meet the costs. An indicator of fluctuations in aggregate welfare is the national unemployment rate. When unemployment rates are high, more principal wage earners are out of work and families have a harder time meeting the costs of sending their children to school. When low, families, on average, can more readily bear schooling costs, and if costs affect school continuation decisions progression rates should increase.[9]

The opportunity costs of schooling are potential earnings opportunities available to dropouts. When opportunities are considerable, the costs of remaining in school are relatively high and transition rates should be depressed. When opportunities are scarce, the costs of schooling are minimized and rates should increase. Ideally, we would index the opportunity costs of making a school transition with unemployment rates for white male youths the same age as persons making the transition. For the full period experienced by the cohorts in our analysis, however, there are only unemployment estimates for the entire civilian labor force. Thus, we use the unemployment rate for the labor force as a whole. This implies contradictory hypotheses regarding the net effect of unemployment on grade progression. Since high unemployment enhances the aggregate burden of meeting the direct costs of schooling, it may depress grade progression. But when unemployment is high in the total labor force it is high for school-age youths making persons in school more inclined to stay there. These contradictory implications make it impossible to isolate the partial positive effect of opportunity costs and the negative effect of direct costs on grade progression [though previous research shows positive zero-order correlations between fluctuations in unemployment and in high-school-level progression rates (Duncan 1965a)]. In appraising the opportunity costs of schooling, however, individuals may not merely note the stock of available employment opportunities. More likely, they expect the future employment situation to resemble recent labor market trends. If changes in the unemployment rate are for the worse, students are more likely to remain in school.

Conversely, net of the unemployment level, decreases in the unemployment rate portend an improved labor market and may reduce grade progression rates. Thus, while it is hard to specify the direction of the effect of unemployment levels on grade progression, changes in unemployment rates should positively affect progression rates inasmuch as they signal work possibilites for dropouts.

Institutional Characteristics

We consider several indicators of school organization including elementary and secondary school expenditures per pupil enrolled, average annual salary of instructional staff in elementary and secondary schools, pupil-to-teacher ratio in elementary schools, pupil-to-teacher ratio in secondary schools, average number of days attended per year per pupil enrolled in elementary and secondary schools, number of one-teacher public schools, and number of four-year colleges (U. S. Bureau of the Census 1975, pp. 369, 373-375, 382-383).

These measures permit us to examine several elementary--yet to date untested--hypotheses. Per pupil expenditures index resources allocated to schooling. High resource levels imply, on average, more diversified curricula and improved physical facilities. These make school attendance easier and more attractive, thereby increasing grade progression rates. Teacher salaries should be correlated with the quality of instruction since higher remuneration should recruit more able persons into the teaching profession. On average, the better the instruction the more students will be able to continue their schooling, and thus the higher the rates of grade progression.

Similarly, the pupil-teacher ratio should affect progression rates negatively since smaller classes will, other things being equal, imply more intensive instruction and enhanced student capability and interest in further schooling.

Average days attended per pupil should positively affect grade progression for two reasons: First, it measures the intensiveness of formal schooling and, thus, longer school terms imply increased student ability to go on with schooling. Second, it indexes the extent of the traditional schooling pattern of seasonally interrupted attendance in favor of farm or other child labor. This traditional pattern socialized young persons early into labor force participation and facilitated early permanent school withdrawal. Conversely, lengthy school terms imply a break from tradition, and a decline in the salience of competing work opportunities.

The number of one-teacher schools again indicates the strength of traditional rural schooling patterns. With the consolidation of schools come uniform yet diversified curricula, reduced child labor force participation, and a stronger age-graded system of school organization. All of these factors should increase progression rates and thus the number of one-teacher schools itself should have a negative effect on grade progression. Finally, the number of four-year colleges indexes the physical availability of higher education and should, other things being equal, have a net positive effect on the progression rate from high school to college.

Although suited to exploration of determinants of educational growth, the school indicators have an important limitation. They are only available biennially and thus for one-half of the cohorts for which adjusted grade progression rates are available. Therefore, including these variables reduces observations to at most 23 and thereby restricts the complexity of the models we consider. But they are the best source of aggregate historical information on school characteristics making it worthwhile to learn what we can from the limited number of observations.

METHODS OF TIME-SERIES ANALYSIS

Since we consider a number of plausible yet previously unexamined hypotheses, our strategy is exploratory. We have searched the data for support for the hypotheses outlined above, to find the lag(s) at which macro-level events affect grade progression, and, in examining the effects of the perceived rewards to schooling on grade progression, to find the comparisons between previous cohort members with different amounts of schooling made by a cohort deciding whether to continue in school.

This section outlines the methods that we use to determine the regression equations presented in the next section. (Further discussion of the procedures is in the Appendix.) These methods do not ensure plausible specifications or unique, best-fitting models. Whether we accept the results of the data search procedures depends upon their plausibility. Several searches of the data may yield

different models which fit the data well and are reasonable. In these instances we report the several models and, where suitable, their interpretations.

All variables in our analysis, except the level and change in unemployment rates, are strongly correlated with a linear or exponential trend. Thus, we express variables as deviations from linear or exponential trends.[10] Parameter estimates are equivalent to those obtainable by using the observed values of the variables and by including a linear time trend as a variable in the equations. The coefficients, therefore, denote the effects of fluctuations about trends in the independent variables on corresponding fluctuations in the adjusted log odds of grade progression.

Our principal tool is the cross-correlation function (for example, Box and Jenkins 1970, Ch. 11). That is, we examine the zero-order correlations between the adjusted log odds of grade progression and independent variables over plausible ranges of lagged values of the variables. For example, we expect that the unemployment rate affects progression from 9th grade attendance to high school graduation, but it is unclear at what year of high school the effect emerges. We use the cross-correlation function to estimate the correlation between the adjusted log odds of grade progression and the unemployment rate when the cohort was 17, 16, 15, and 14 years old. This suggests the ages when unemployment has a zero-order effect on graduation and the appropriate lagged value(s) of the unemployment rate to be used in an equation including additional independent variables.

We also use the cross-correlation function to identify multivariate relationships between grade progression and its determinants, proceeding from bivariate relationships to more complex multiple regression models. In addition, we explore alternative forms of economic returns that may affect grade progression. We experiment with different lags at which relative earnings affects grade progression and with different pairs of groups who make and do not make a given transition that may be compared by cohorts facing the transition. The details of these procedures are in the Appendix. These methods are not a rigorous search algorithm; rather, we combine a search strategy with substantive reasoning to specify models more precisely than theory alone permits.

FINDINGS

This section reports the effects of structural variables on the log odds of grade progression at four levels of schooling adjusted for intercohort changes in social background composition.[11] We describe in sequence the results for the transitions to completion of 8th, from 8th to 9th, from 9th to 12th, and from 12th to 13th grades. For each transition we report first, the relative earnings measure suggested by the cross-correlation function; second, the effects of labor market factors--that is, the economic returns and costs of schooling--; and third, comprehensive equations that incorporate the effects of both labor market factors and school characteristics. We present fuller discussion of the transition to 8th grade completion than of the subsequent transitions to provide a point of reference

for discussing the latter. We also present additional detail for the transition from high school attendance to graduation in the attempt to interpret unanticipated findings.

Eighth Grade Completion

The cross-correlation function indicates that the strongest effects of the first job benefits of grade progression on decisions whether to complete elementary school occur through intercohort comparisons of completing and noncompleting groups in previous cohorts born two years apart. Men completing 8th grade are compared to men two years older who failed to complete 8th grade. The cross-correlation function also indicates that these effects occur at a substantial lag. The strongest returns effect on the adjusted log odds of 8th grade completion is for the returns of men completing 8th grade 11 years older than the cohort facing the transition relative to the returns of men not completing 8th grade born nine years before the cohort facing the transition. These groups are depicted at the bottom of Figure 1. [In terms of (5), n is nine and s is two.] This is the returns to schooling measure used in equations reported below.[12]

This formulation is not unreasonable in light of our earlier discussion. The effect is at a long lag because persons who complete 8th grade may stay in school a number of years after the transition, typically, until high school graduation. That the comparison of graduates and dropouts is between cohorts born only two years apart indicates that, for 8th grade completion, students (or, more realistically, their parents) do not compare groups entering the labor force in the

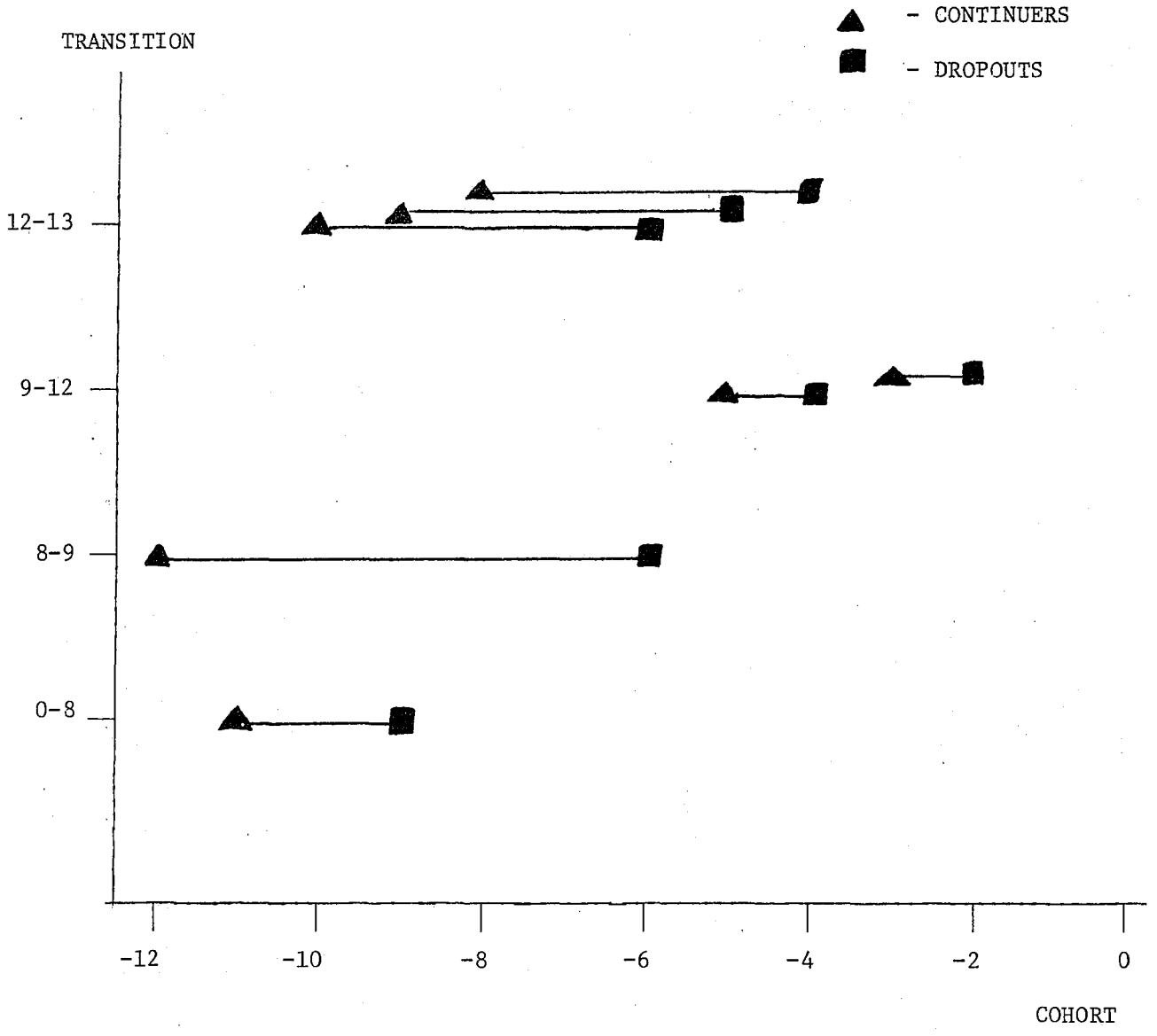

Figure 1. - Reference groups for earnings comparisons at selected school transitions made by cohort 0.

same year. Dropouts entering the labor force without completing 8th grade are more than two years younger than the typical labor force entrant who has completed 8th grade. A two-year separation between the ages of continuers and dropouts implies, therefore, that the former tend to enter the labor market after the latter. This suggests that the decision to complete 8th grade relies on perceptions of returns to schooling of workers who have been in the labor force for several years and that youths (and their parents) compare persons similar in age not allowing for differences in duration of labor force participation. Thus, the effects of returns to 8th grade completion reported below might be stronger were they measured for mature workers rather than labor force entrants.

Table 1 reports regression results that include this measure in equations showing the effects of the costs and organization of formal schooling. Equation 1 includes the effects of levels and changes in the total labor force unemployment rate as well as relative earnings. The estimated effects support the arguments presented above. There is a significant effect of fluctuations in relative earnings on 8th grade completion, implying that a 10 percent change in the earnings of previous cohort members who complete 8th grade, relative to those who do not, induces a 5.7 percent change in the adjusted odds of grade progression.[13] Increases in unemployment rates, implying worsening economic conditions for labor force entrants, enhance the chances of 8th grade completion. One percent changes in the unemployment rates when cohorts were aged 6 to 7 and 12 to 13 imply a 1.6 percent change in the

TABLE 1

REGRESSION ANALYSIS OF THE LOG ODDS OF EIGHTH GRADE COMPLETION ADJUSTED FOR SOCIAL BACKGROUND COMPOSITION

	1	2	3	4	5	6	7
UR_{12}	-0.0056 (-1.16)	-0.0126 (-4.68)	-0.0184 (-4.93)	-0.0157 (-3.88)	-0.0294 (-3.92)	-0.0170 (-3.78)	-0.0269 (-4.46)
DUR_6	0.0165 (2.26)	0.0097 (2.59)	0.0101 (2.21)	0.0082 (1.83)	0.0082 (1.52)		
DUR_{12}	0.0162 (2.05)	0.0230 (6.38)	0.0240 (5.50)	0.0237 (5.00)	0.0218 (3.90)	0.0236 (4.12)	0.0208 (3.29)
$LSAL_6$		0.6773 (8.21)		0.7709 (5.81)		0.8826 (6.27)	
$LEXPP_6$			0.5631 (6.49)		0.6319 (4.59)		0.7406 (5.70)
$ONERM_6$		-0.0087 (-5.21)	-0.0042 (-1.97)	-0.0095 (-4.45)	-0.0023 (-0.87)	-0.0102 (-3.97)	
$DAPP_{10}$		0.0191 (3.63)	0.0324 (5.11)	0.0237 (3.27)	0.0286 (3.14)	0.0244 (3.19)	0.0260 (3.58)
PTR_8		-1.0297 (-0.94)	-0.1621 (-0.12)	-0.6240 (-0.46)	-0.4526 (-0.28)		
$RETURNS_{-9}$	0.5717 (2.33)			0.0785 (0.33)	0.4636 (1.61)	0.1125 (0.42)	0.6246 (2.35)
Constant	0.0424 (1.11)	0.0369 (3.97)	0.0441 (3.92)	0.0510 (2.67)	0.0439 (3.37)	0.1146 (2.54)	0.2285 (4.19)
N	35	20	20	17	17	17	17
R^2	0.454	0.941	0.913	0.933	0.905	0.869	0.816
S.E.E.	0.112	0.018	0.019	0.023	0.013	0.059	0.066
D.W.	1.71	1.82	2.23	2.23	2.23	2.38	2.23

NOTE: Ratios of coefficients to their estimated standard errors are in parentheses. All variables except UR and DUR are deviations from a linear time trend. Equations 2-6 are estimated by a two-stage procedure to adjust for serial correlation. For explanation see text. Independent variables are: UR_{12}: unemployment rate when cohort was age 12; DUR_6 and DUR_{12}: change in unemployment rate between years when cohort was 6 and 7 and was 12 and 13 respectively; $LSAL_6$: natural logarithm of public elementary and secondary school teachers' average annual salaries in constant (1967) dollars when cohort was age 6; $LEXPP_6$: natural logarithm of annual public elementary and secondary school expenditures per pupil enrolled in constant (1967) dollars when cohort was age 6; $ONERM_6$: number of one-teacher public schools (in thousands) when cohort was age 6; $DAPP_{10}$: annual average days attended per pupil enrolled in public elementary and secondary schools when cohort was age 10; PTR_8: pupil-teacher ratio in elementary schools when cohort was age 8; RETURNS: natural logarithm of the ratio of estimated first job earnings of persons completing 8th grade to earnings of persons two years younger not completing 8th grade.

adjusted odds of grade progression. Unemployment rate change effects at the end of elementary school are reasonable inasmuch as these are the ages when employment opportunities are first available. That there is an effect when cohorts begin schooling is less reasonable since child labor is vestigial during the childhoods of the cohorts represented in our data.[14] The effect of the unemployment level is negative, suggesting that economic hardship prevents families from meeting the costs of keeping their children in school. But this effect is small and the coefficient barely exceeds its standard error.

The remaining equations in Table 1 summarize the effects of school organization indicators on grade progression, first ignoring and then taking account of the effects of relative earnings. Since the equations are subject to (negative) first-order serial correlation in their disturbances, they are estimated by Durbin's two-stage procedure.[15] Note the lag structure of the schooling effects. Teacher salaries, expenditures, and one-teacher schools affect 8th grade completion through their levels when cohorts are six; days attended per pupil through its level when cohorts are ten; and the pupil-teacher ratio through its level when cohorts are eight. The lags can be interpreted as follows: School expenditures are, in principle, investments in educational facilities and personnel. High expenditure levels when a cohort enters school benefit the cohort throughout its schooling. Similarly, teachers' salaries regulate the number and quality of new teachers. High salaries when a cohort starts school need not imply exceptional teachers in that year, but rather recruitment of better

teachers over the years as more persons respond to the salary levels. Persons entering school when there are many one-teacher public schools (relative to their long-run downward trend) spend their entire elementary school career in such schools more often than persons entering when there are relatively few. School entry, therefore, may determine the fraction of a cohort that attends small rural schools and experiences their disadvantages. Days attended per pupil and the pupil-teacher ratio affect 8th grade completion later. Since these are school quality dimensions which should have relatively immediate impact on attendance, their impact should be greatest where attrition is greatest. During the elementary years, attrition is greatest between 5th and 7th grade for most cohorts born during the first half of the twentieth century (Duncan 1968, p. 640). Days attended per pupil and the pupil-teacher ratio should exert their effects in response to their values when a cohort is about to enter 5th grade. The lags reflect these effects.

Columns 2 and 3 report the effects of schooling and the level and change in unemployment in the absence of relative earnings effects.[16] The school effects support our conjectures and, despite the small number of observations, most parameters are larger than twice their standard errors. There are positive effects of teacher salaries and expenditures per pupil: a 10 percent increase in these variables raise the odds of grade progression by 6.8 and 5.6 percent respectively. There are also significant effects of days attended per pupil (a one day

increase induces a 2 to 3 percent increase in the odds of progression) and one-teacher public schools, although the latter is marginally significant in the equation including expenditures. Although the effect of the pupil-teacher ratio is in the hypothesized direction, its coefficient estimates are insignificant. That the one-teacher schools and pupil-teacher effects are much smaller in the equation including general expenditures than in the equation including teacher salaries is consistent with the meaning of the expenditures measure. Expenditures per pupil is a general source of school quality operating through numerous channels that include consolidation, the number of teachers available to students, and teacher salaries. To a degree, therefore, the other school quality indicators are redundant with general expenditures. The effects of the level and change in unemployment are much stronger when fluctuations in school characteristics are taken into account. The level of unemployment has a strong negative effect, while the positive effects of unemployment changes persist, particularly at the end of elementary school.

Now consider schooling, unemployment, and returns effects simultaneously. We continue to present separate equations for expenditure and teacher-salary effects. These variables bear different relationships to other schooling indicators but, more important, their associations with <u>returns</u> to schooling may also differ. Since teachers are among better-educated workers, fluctuations in their earnings, other things being equal, mirror changes in the relative earnings of 8th grade graduates relative to elementary school

dropouts. General expenditures, by contrast, has no obvious direct link with returns to grade progression.

Equations 4 and 5 augment 2 and 3, respectively, with the relative economic value of 8th grade completion. In neither instance is the earnings effect significant. Its coefficient, however, is nearly zero in the equation including teacher salaries, while in the expenditures equation it is almost as large as when schooling variables are excluded. This indicates, then, that the correlation between relative earnings and teacher salaries is stronger than between relative earnings and general expenditures.[17] Equations 4 and 5 are re-estimates of equations 2 and 3, excluding variables (other than relative earnings) with coefficients less than twice their respective standard errors. These estimates sustain our inference from the latter two equations.

To summarize our results for 8th grade completion, we find effects of most of the factors hypothesized to affect grade progression. Deterioration of labor market conditions induces positive fluctuations in background adjusted grade progression, whereas the unemployment level negatively affects grade progression. There are also effects of educational expenditures--either general or teacher salaries--on progression rates, part of which may operate via the consolidation of schools, and of school attendance levels within the school year.

As for the returns to schooling, the evidence is mixed. There is a positive effect at a substantial lag but only in the absence of controls for teacher salaries. This may validate the relative earnings measure inasmuch as teacher salaries are a component of the earnings of

relatively well-educated workers. On the other hand, it suggests considerable unreliability in the earnings measure implying that if a better estimate of the latter were available, it would show an effect independent of teacher salaries. On balance, there may be an effect of the relative economic value of 8th grade completion on the progression rates of later cohorts, but its importance relative to that of transitory labor market fluctuations and change in characteristics of the educational system is not so overwhelming as to be easily detectable with crude measurement.

High School Attendance Given Elementary Completion

We consider next market and institutional effects on the adjusted log odds of 9th grade attendance given 8th grade completion. The cross-correlation function indicates that the strongest effects of the relative first job benefits of high school attendance on continuation decisions is through intercohort comparisons between persons dropping out after 8th grade and persons attending at least 9th grade who are born six years apart. Men attending high school are compared to men six years younger who complete elementary school but do not attend high school. The cross-correlation function also indicates that persons make this comparison at a lag of six years. That is, men attending high school who are 12 years older than the cohort facing the transition from 8th to 9th grade are compared to 8th grade dropouts who are six years older than the cohort facing the transition. These groups are depicted in Figure 1. (In terms of (5), n is six and s is six.) We use this measure of returns to high school attendance in the analysis

reported below. Although other specifications of the relative first job earnings benefits of grade progression are, a priori, equally plausible to this measure, the latter is not unreasonable. The six year separation between the continuing and dropout groups in the comparison suggests that cohorts (or their parents) respond to the experiences of groups who enter the labor force at about the same time. That the comparison is made at a six year lag makes it impossible that cohorts base their continuation decisions on their own perceptions of labor force entrants. Rather, either parents remember the experiences of past labor force entrants or sons and/or parents rely on the experiences of a labor-force-entry cohort several years after entry. The latter suggests that relative first job earnings is correlated with subsequent cohort grade progression because it is also correlated with relative earnings of older workers. The relative earnings measure may then fallibly indicate the fortunes of men who have been working for several years.

Table 2 reports estimates of market and institutional effects on fluctuations in the adjusted log odds of progression from 8th grade completion to 9th grade attendance. In the first equation, except for unemployment level, the effects are similar in pattern to those for the corresponding equation for 8th grade completion. One percent changes in unemployment rates at ages 12-13, the year preceding the modal age of transition to 9th grade, induce approximately 1.6 percent increases in the odds of grade progression. The effect of relative earnings is significant and positive though the coefficient is less

TABLE 2

REGRESSION ANALYSIS OF THE LOG ODDS OF NINTH GRADE ATTENDANCE GIVEN EIGHTH GRADE COMPLETION ADJUSTED FOR SOCIAL BACKGROUND COMPOSITION

	1	2	3	4	5
UR_{12}	0.0062 (1.35)	0.0005 (0.13)	0.0034 (0.95)		
DUR_6	0.0069 (1.91)	0.0034 (0.65)	0.0047 (0.88)		
DUR_{12}	0.0160 (3.94)	0.0155 (2.82)	0.0131 (2.42)	0.0149 (3.75)	0.0168 (3.73)
$LSAL_6$			0.4253 (2.16)		0.5330 (4.41)
$LEXPP_6$		0.3951 (2.41)		0.3973 (5.30)	
$ONERM_6$		0.0027 (0.83)	-0.0020 (-0.72)		
$DAPP_8$		-0.0013 (0.83)	-0.0027 (-0.32)		
PTR_6		-1.067 (-0.81)	-2.193 (-1.74)	-1.2611 (-1.35)	-1.1549 (-1.10)
$RETURNS_{-6}$	0.1651 (2.73)	0.1980 (1.62)	0.1770 (1.41)	0.1842 (2.03)	0.2209 (4.41)
Constant	0.0052 (0.74)	0.0225 (2.41)	-0.0042 (-0.13)	0.0240 (2.13)	0.0150 (1.18)
N	32	17	17	17	17
R^2	0.523	0.838	0.824	0.809	0.756
SEE	0.038	0.051	0.054	0.046	0.051
D.W.	2.04	2.68	2.43	2.50	1.75

NOTE: Ratios of coefficients to their estimated standard errors are in parentheses. All variables except UR and DUR are deviations from a linear time trend. Independent variables are: UR_{12}: unemployment rate when cohort was age 12; DUR_6 and DUR_{12}: change in unemployment rate between years when cohort was 6 and 7 and was 12 and 13 respectively; $LSAL_6$: natural logarithm of public elementary and secondary school teachers' average annual salaries in constant (1967) dollars when cohort was age 6; $LEXPP_6$: natural logarithm of annual public elementary and seconday school expenditures per pupil enrolled in constant (1967) dollars when cohort was age 6; $ONERM_6$: number of one-teacher public schools (in thousands) when cohort was age 6; $DAPP_8$: annual average days attended per pupil enrolled in public elementary and secondary schools when cohort was age 8; PTR_6: pupil-teacher ratio in elementary schools when cohort was age 6; RETURNS: natural logarithm of the ratio of estimated first job earnings of persons attending 9th grade to earnings of 8th grade dropouts who are six years younger.

than one-third of its size in the equation for 8th grade completion. The small unemployment rate effect suggests no effect of aggregate economic fluctuations on families' willingness to meet the costs of enabling their children to make this transition.

The remaining equations estimated from biennial observations take account of opportunity cost, returns, and educational organizational effects on grade progression. The lag structure of the schooling effects is similar to that for the transition to 8th grade completion implying that the same organizational mechanisms regulate the 8th to 9th grade transition rate and the 8th grade completion rate. Educational expenditures and teacher salaries are indexed by their values when the cohort facing the transition to 9th grade is early in its elementary school career, reflecting that expenditure and salary levels determine school conditions over a period of years. Experimentation with alternative specifications indicates that the strongest effects of days attended per pupil and the pupil-teacher ratio are through the values of these variables when the cohort is aged eight and six years respectively. We suggested that these variables should affect grade progression with relatively short lags and, given that the transition to 9th grade occurs at about age 13, we would not expect them to have strong effects at ages so early as six or eight. As the equations show, the effects are negligible for days attended and marginally significant for the pupil-teacher ratio. Equations 2 through 5 show strong effects of teacher salaries and educational expenditures. The effects of both of these variables indicate that a 10 percent increase in salaries

or expenditures implies a 4 percent increase in the odds of grade progression.[18] Other school variables, however, have negligible effects.

To summarize, we find effects of changes in the opportunity costs of schooling, as indicated by unemployment rate changes, of the relative earnings of previous cohorts of high school attenders and 8th grade dropouts, and of some school quality indicators on the adjusted log odds of progression from 8th to 9th grade. In contrast to 8th grade completion, however, there is no negative unemployment rate effect, suggesting that transitory fluctuations in the ability to support children's school attendance have little aggregate effect at this transition. We also find no one-teacher public school effect, suggesting that while the consolidation of elementary schools contributed to near universal elementary school graduation, it had little effect on high school attendance rates of elementary graduates.

High School Graduation Given Ninth Grade Attendance

The cross-correlation function indicates that insofar as persons facing the transition from high school attendance to graduation take account of the relative earnings of graduates and dropouts in previous cohorts, they do so through an intercohort comparison of groups born one year apart. High school graduates are compared to dropouts born one year later. The cross-correlation function also shows that cohorts make these comparisons using the experience of cohorts who were in high school only a few years before them. The strongest relative earnings effect on high school graduation is through comparison between the earnings of high school graduates five years older and high school

dropouts four years older than persons making the comparison. These groups are depicted in Figure 1. (In terms of (5), n = 4 and s = 1.) In addition, there is a smaller effect of a comparison between the earnings of high school graduates three years older than the cohort making the comparison and dropouts two years older than the cohort making the comparison. (That is, n =2 and s = 1.) We use these two ratios in the analysis reported below.

That the comparisons between dropouts and graduates are for recent cohorts suggests that for this transition students are more inclined to evaluate the first job experiences of recent labor force entrants than at earlier transitions where the data suggest that they compare groups of mature workers. That the comparisons are between groups born only a year apart, however, makes it unlikely that cohorts make comparisons within cohorts of labor force entrants. Since many dropouts leave before 11th grade completion and many high school graduates go to college, the comparisons implied by the relative earnings measure suggest that the dropouts used in a given comparison enter the labor force before the high school graduates to whom they are compared.

Table 3 reports market and institutional effects on fluctuations in progression to high school graduation. Compared to the earlier transitions, the relative earnings effects are much larger. Equation 1 shows that a 10 percent change in the earnings of high school graduates relative to dropouts for groups born five and four years,

TABLE 3

REGRESSION ANALYSIS OF THE LOG ODDS OF HIGH SCHOOL GRADUATION GIVEN NINTH GRADE ATTENDANCE ADJUSTED FOR SOCIAL BACKGROUND COMPOSITION

	1	2	3	4	5
UR_{16}	0.0001 (0.05)	-0.0100 (-1.94)	-0.0072 (-1.69)	0.0027 (0.66)	-0.0082 (-1.83)
DUR_{16}	0.0021 (0.37)	-0.0106 (-1.14)	-0.0052 (-0.63)		
$LEXPP_{10}$		0.2874 (1.62)		-0.0967 (-0.65)	0.1985 (1.39)
$LSAL_{10}$			0.4036 (1.69)		
$ONERM_{16}$		-0.0002 (-0.06)	-0.0029 (-0.74)		
$DAPP_{16}$		0.0441 (3.08)	0.0371 (2.86)		0.0439 (3.38)
PTR_{16}		-0.5247 (-0.23)	0.4012 (0.19)		
$RETURNS_{-2}$	0.3550 (1.60)	0.2933 (0.99)	0.2802 (0.96)	0.4980 (1.66)	0.2648 (1.10)
$RETURNS_{-4}$	0.7300 (3.15)	0.1818 (0.48)	0.2571 (0.70)	0.7633 (2.27)	0.2003 (0.65)
Constant	0.0128 (0.55)	0.0357 (1.07)	0.0240 (0.81)	-0.0579 (-0.162)	0.0241 (0.84)
N	41	20	20	20	20
R^2	0.277	0.691	0.696	0.362	0.649
S.E.E.	0.082	0.061	0.061	0.075	0.058
D.W.	1.78	1.71	1.71	2.14	1.62

NOTE: Ratios of coefficients to their estimated standard errors are in parentheses. All variables except UR and DUR are deviations from a linear time trend. Independent variables are: UR_{16}: unemployment rate when cohort was 16; DUR_{16}: change in unemployment rate between years when cohort was 16 and 17; $LSAL_{10}$: natural logarithm of public elementary and secondary school teacher's average annual salaries in constant (1967) dollars when cohort was age 10; $LEXPP_{10}$: natural logarithm of annual public elementary and secondary school expenditure per pupil enrolled in constant (1967) dollars when cohort was age 10; $ONERM_{16}$: number of one-teacher public schools (in thousands) when cohort was age 16; $DAPP_{16}$: annual average days attended per pupil enrolled in public elementary and secondary schools when cohort was age 16; PTR_{16}: pupil-teacher ratio in secondary schools when cohort was age 16; RETURNS: natural logarithm of the ratio of estimated first job earnings of persons completing 12th grade to the earnings of high school dropouts who are one year younger.

respectively, before persons facing the transition implies a 7 percent change in the odds of graduation. The relative earnings coefficient for groups born two and three years prior to persons facing the transition is smaller but still substantial.

Surprisingly, Equation 1 shows no effect of either the level or change in unemployment on high school completion. This contrasts with previous findings that fluctuations in grade progression vary directly with fluctuations in unemployment (for example, Duncan 1965a). We have no explanation for this discrepancy. Differences in statistical design between this study and others may account for the differences between our findings and those of others, but we have been unable (in analysis not reported here) to determine which, if any, of our methods is responsible for the findings.[19] On the other hand, that we find clear positive effects of change in unemployment rates on progression to 8th grade completion, from 8th to 9th grade, and from high school to college (see below) suggests that were there similar effects on high school graduation, we would identify them.

Equations 2 and 3 report market and schooling effects on graduation. The lags at which the several schooling variables are specified to affect grade progression are largely justifiable by arguments presented for the earlier school transitions. (We date the number of one-teacher public schools with respect to when the cohort was 16 years old, but it has no detectable effect on graduation irrespective of the lag at which it is indexed.) Equations 2 and 3 summarize the net effects of all of the independent variables. They show that fluctuations in the

log odds of high school graduation depend on a small subset of these variables. There is a strong effect of days attended per pupil on grade progression: a 10 day increase in average days attended implies about a 4 percent increase in the adjusted odds of high school graduation. Notably, the level of unemployment has a negative effect not present in Equation 1. Salaries and expenditures have marginally significant coefficients similar in size to those observed for the transition from 8th to 9th grade. Beyond these variables--the unemployment rate, days attended per pupil, and salaries (or expenditures)--all others have negligible effects.

These findings raise two questions: first, why does a negative unemployment effect emerge in the presence of schooling indicators; and second, why do the relative earnings effects disappear? Equations 2 and 3 may imply that Equation 1 is misspecified. Alternatively, they may suggest mechanisms through which unemployment and relative earnings affect grade progression. That days attended per pupil strongly affects grade progression suggests that high school attendance levels within a year may be means through which other more remote causes of grade progression are transmitted.

Both the level of unemployment and relative earnings may positively affect average attendance within a school year. When unemployment is high, a source of absence from school--namely part-time employment--is scarce. In addition, students will be less likely to take time off from school to look for employment. Thus, the opportunity costs

of schooling within a school year will be low just as the opportunity costs of a school year as a whole will be low. The unemployment rate, therefore, will positively affect annual school attendance. A similar process may regulate the effects of returns to schooling on grade progression. The higher the perceived value of high school graduation, the greater the incentive to attend regularly within a year. Regular attendance is required for academic success. In addition, part-time work appeals less to persons aspiring to at least a high school degree. But whatever the mechanism, there may be positive effects of the perceived relative earnings of high school graduation on attendance within as well as across school years.

To investigate this we examine first the effects of unemployment and relative earnings in equations both taking and not taking account of the average attendance effect, and then the effects of unemployment and relative earnings on average daily attendance.[20] Equations 4 and 5 of Table 3 are estimates of expenditures, unemployment and relative earnings effects on graduation, with and without control for days attended. When days attended is excluded from the equation, the unemployment effects are negligible as they were in Equation 1, while relative earnings effects are large. When days attended is included, relative earnings effects are reduced and a negative unemployment effect emerges. This pair of equations suggests that unemployment and relative earnings affect days attended per pupil. To provide better evidence that they determine attendance levels, however, it is useful to consider other determinants of average daily attendance per pupil.

Attendance responds not only to economic incentives to stay in school, but also to legal attendance requirements. Fluctuations in daily attendance, then, depend on fluctuations in the average official school term. To assess the effects of relative earnings and unemployment on days attended, we also consider the effect of the average length of school term as reported in U. S. Bureau of the Census (1975, p. 375). The estimated equation based on biennial observations is as follows:

$$\widehat{DAPP} = -0.7630 + 0.9196L + 0.2083U + 8.8452R_{-4} \qquad (6)$$

$$(-1.38) \quad (2.91) \quad (3.63) \quad (1.57)$$

$$(N = 20;\ R^2 = 0.600;\ S.E.E. = 1.18;\ D.W. = 1.90)$$

where DAPP denotes days attended per pupil enrolled, L the average length of school term, U the unemployment rate, and R_{-4} the log of earnings of high school graduates born five years before relative to high school dropouts born four years before; all variables are deviations from linear trends; and the ratios of coefficients to their standard errors are in parentheses. Equation (6) shows positive effects of both official average school year length and unemployment on attendance and a marginally significant relative earnings effect on attendance. That effects of unemployment and relative earnings on attendance are detectable even when the length of school year is taken into account suggests the business cycle and the returns to schooling may determine attendance levels within a school year. The regression results in Table 3, therefore, suggest that unemployment and relative earnings

effects on high school graduation are transmitted through attendance within the school year.

To summarize, the effects of relative earnings in recent cohorts on high school graduation are larger than for the earlier school transitions. Reduced form effects of levels and changes in unemployment on grade progression, however, are negligible. We find modest effects of educational expenditures and teacher salaries, large effects of school days attended per pupil, and little effect for the remaining schooling variables. Taking account of the effect of average daily attendance attenuates the relative earnings effect and reveals a negative unemployment effect. The relationships among days attended per pupil, relative earnings, and unemployment suggest that attendance within a school year responds to both short-run employment changes and to the anticipated long run value of schooling.

College Attendance Given High School Graduation

The cross-correlation function shows that individuals compare the earnings of college students to those of high school graduates not attending college who were born four years later.[21] It also indicates that cohorts compare high school graduates (who do not attend college) born between four and six years earlier to college students born between eight and 10 years earlier. This is graphed in Figure 1. (In terms of (5), s is four and n ranges from four to six.) This specification is highly reasonable. The four-year separation between the college attending and non-attending groups implies that high school graduates make comparisons within cohorts of labor force entrants; that is, they

compare the earnings of college graduates to those of persons with only a high school degree. This seems a rational comparison upon which to evaluate college attendance. That persons compare groups four to 10 years older than them, however, suggests that they do not focus on the first job returns to college, but rather the experiences of men who have been working several years. Once again, then, relative first-job earnings fallibly measure comparisons between groups of experienced workers, that are more pertinent to school continuation decisions.

Equations 1 and 2 of Table 4 report the effects of the same set of variables estimated by alternative methods. Equation 1 is estimated by OLS and Equation 2 by Durbin's two-step procedure. The Durbin-Watson statistic for Equation 1 indicates positive serial correlation suggesting that Equation 2 is possibly the preferable specification.[22] Both equations show effects of the relative earnings of previous cohorts on the decision to attend college. The unemployment effects in the two equations, however, differ considerably. Equation 1 suggests that fewer persons can afford college during recessions, but labor market deterioration reduces alternatives to further schooling for high school graduates and drives up continuation rates. In Equation 2, however, both unemployment effects are negligible. Although the t-statistics for Equation 1 are invalid, we are more inclined to believe the parameter estimates of that equation than those in Equation 2. Inclusion of additional variables does not materially alter the coefficient estimates for the level and change in unemployment given in Equation 1 while it eliminates most of the serial correlation, suggesting that the unemployment

TABLE 4

REGRESSION ANALYSIS OF THE LOG ODDS OF COLLEGE ATTENDANCE GIVEN HIGH SCHOOL GRADUATION ADJUSTED FOR SOCIAL BACKGROUND COMPOSITION

	1	2	3	4	5	6
UR_{18}	-0.0049 (-2.42)	-0.0008 (-0.23)	-0.0071 (-1.47)	-0.0074 (-1.38)	-0.0064 (-3.55)	-0.0053 (-3.81)
DUR_{17}	0.0107 (2.58)	0.0042 (1.18)	0.0117 (1.74)	0.0122 (1.63	0.0228 (5.06)	0.0221 (3.56)
$LEXPP_{12}$				0.1583 (1.37)		0.1305 (1.20)
$LSAL_{12}$			0.3828 (1.87)		0.3318 (3.34)	
$ONERM_{16}$			0.0017 (0.41)	0.0059 (1.77)		
$DAPP_{16}$			0.0034 (0.44)	0.0016 (0.19)	0.0151 (3.35)	0.0140 (1.86)
PTR_{16}			-1.9812 (-1.09)	-3.1534 (-1.70)		
$LCOSTPP_{18}$			-0.1270 (-1.01)	-0.0429 (-0.37)	-0.1890 (-4.66)	-0.1533 (-2.46)
$COLLEGES_{18}$			-0.4113 (-0.59)	-0.7088 (-0.98)		
$RETURNS_{-4}$	0.1162 (1.35)	0.1304 (2.09)	0.2497 (1.27)	0.3472 (1.59)		
$RETURNS_{-5}$	0.3285 (3.37)	0.2766 (3.93)	0.4907 (3.47)	0.4149 (2.82)	0.6144 (5.72)	0.5382 (3.81)
$RETURNS_{-6}$	0.1036 (1.31)	0.1441 (2.48)	0.1760 (1.25)	0.1397 (0.89)		
Constant	0.0421 (3.20)	0.0090 (1.13)	0.0540 (1.70)	0.0608 (1.75)	0.0386 (3.03)	0.0318 (1.86)
N	34	33	17	17	18	18
R^2	0.399	0.421	0.908	0.886	0.863	0.756
S.E.E.	0.032	0.024	0.020	0.022	0.022	0.030
D.W.	0.75	1.28	1.64	1.90	2.62	1.91

NOTE: Ratios of coefficients to their estimated standard errors are in parentheses. All variables except UR_{18} and DUR_{17} are deviations from a linear time trend. Equation 2 is estimated by a two-stage procedure to adjust for serial correlation. For explanation, see text. Independent variables are: UR_{18}: unemployment rate when cohort was age 18; DUR_{17}: change in unemployment rate between years cohort was 17 and 18; $LSAL_{12}$: natural logarithm of public elementary and secondary school teachers' average annual salaries in constant (1967) dollars when cohort was age 12; $LEXPP_{12}$: natural logarithm of annual public elementary and secondary school expenditures in constant (1967) dollars when cohort was age 12; $ONERM_{16}$: number of one-teacher public schools (in thousands) when cohort was age 16; $DAPP_{16}$: annual average days attended per pupil enrolled in public elementary and secondary schools when cohort was age 16. PTR_{16}: pupil-teacher ratio in secondary schools when cohort was age 16. $LCOSTPP_{18}$: natural logarithm of student fees collected per resident student in constant (1967) dollars for institutions of higher education when cohort was age 18; RETURNS: natural logarithm of the ratio of estimated first job earnings of persons attending 13th grade to earnings of persons completing exactly 12 grades who are four years younger.

coefficient estimates in Equation 2 are an artifact of the Durbin procedure.

The remaining equations show schooling as well as unemployment and relative earnings effects on college attendance. They include secondary school characteristics which may affect the ability and desire of high school graduates to attend college, as well as the number of four-year colleges and the income from student fees per student in residence. Of these latter two measures we use the first to see if the physical accessibility of college affects attendance rates, and the second to see if high school graduates respond to the direct costs of college in deciding whether to attend. These measures are indexed by their values when the cohort facing the transition between high school and college was approximately 18 years old.

Equations 3 and 4 contain all of the independent variables. The small t-statistics combined with large coefficients of determination indicate that these specifications overfit the data. Nonetheless, the equations highlight strong effects. A 10 percent increase in the earnings of cohorts born nine years prior to the cohort facing the transition, relative to high school graduates not attending college born five years prior to the cohort facing the transition, implies between a 4 and 5 percent increase in the odds of making the transition. Equations 3 and 4 also show negative effects of the unemployment level and positive effects of unemployment change, though the coefficients are insignificant by conventional criteria. Finally, while teacher salaries and expenditures per pupil effects are both large enough to merit interpretation, the

teacher salary coefficient exceeds the expenditures coefficient by more than for any earlier school transition. This suggests that teacher quality may be more important than the general benefits of expenditure levels in facilitating the transition to college. It is necessary, however, to compare these effects in more parsimonious specifications.

Exploration of the data suggested that the variables included in Equations 5 and 6 have persistent effects on grade progression over a broad range of specifications while the excluded variables did not. We found no significant effects of one-teacher public schools, the pupil-teacher ratio, the number of colleges, or the relative earnings measure at lags of four and six years (Mare 1977a). These variables are excluded from the remaining equations.

Equations 5 and 6 reaffirm the positive effects of changes in unemployment, returns to college attendance, and teacher salaries on the adjusted log odds of progression to college shown in previous equations. They also reveal an effect of fluctuations in the cost of higher education. The equation including teacher salaries indicates that a 10 percent increment in fees per pupil induces approximately a 2 percent decrement in the odds of continuation. There is also a positive effect of average daily attendance per pupil within the high school year, though the effect is less than one half that observed for the transition from high school attendance to graduation. Finally, Equations 5 and 6 taken together show that teacher salaries have a much stronger effect on college attendance than expenditures per pupil. The former variable explains 10 percent more of the variance in the adjusted

log odds of attendance than the latter.

To summarize, earnings of college attenders relative to non-attenders in previous cohorts affect continuation decisions. In addition, high school graduates respond to both the direct and opportunity costs of college. Our results also support the conjecture that school quality positively affects progression to college. Cohorts spending more days in school during high school and attending high school following periods of high teacher salaries have significantly higher transition rates. On the other hand, as for the transition from 8th to 9th and 9th to 12th grades, there is little effect of the number of rural one-teacher schools on grade progression rates, most likely reflecting that such schools are mainly elementary schools for most of the era represented by our data. Finally, we find no impact of the number of four-year post-secondary institutions on college attendance. It is beyond this analysis to investigate the proliferation of institutions, but the concomitant growth of four-year institutions and school completion rates and the negative finding in our analysis suggest that institutional growth responded to rather than fostered increased college attendance.

CONCLUSION

This analysis is exploratory and based on limited data. Nonetheless, we have isolated the effects of most of the market and institutional factors suggested as possible determinants of educational growth. Grade progression rates appear to respond to change in the labor market value

of schooling. This holds, moreover, for not only the college level where we are accustomed to thinking of school attendance as partially an economic decision, but also earlier school transitions. As intuition leads one to expect, the perceived benefit to schooling has weaker and less pervasive effects at pre-college transitions, but they are detectable nonetheless. The analysis, therefore, broadly supports the view that educational growth in twentieth-century America is a function of persistent market advantages to better educated workers.

The relative earnings of persons with different amounts of schooling is, however, only one among many structural sources of change in attainment levels. At all schooling levels students respond to short-term fluctuations in the direct and/or opportunity costs of schooling. Cost-responsiveness generally increases with schooling level, again suggesting that monetary considerations become more salient as work opportunities become available in the teenage years. There are also strong effects of several dimensions of school quality on grade progression. School expenditures, teacher salaries, and within-year school attendance all affect progression at most schooling levels, suggesting that growth in the resource levels and the intensity of schooling have determined educational growth. The pervasiveness and strength of these institutional effects generally decline over levels of schooling, showing that the influence of school characteristics is later supplanted by influences external to the schooling process.

That we detect the effects of school conditions on grade progression is perhaps puzzling given prior research which shows no school effects on

achievement. Differences in design between this study and others preclude clear explanation of these differences in results. A possible interpretation, however, is that we examine schooling over a period during which there are enormous changes in resources allocated to schools. Variation over the first half of the twentieth century in resource levels of schools is much greater than typically observed in previous cross-sectional studies. Given sufficient variability in school environments, school characteristics may induce variation in educational outcomes; whereas given limited cross-sectional variation in schools, their impact may appear negligible. But further analysis, taking account of the methodological differences between the present and previous studies, should critically examine this conjecture.

In sum, then, we can make several broad conclusions. First, the pattern of institutional and market effects accords with our intuition about the differential impact of educational and economic institutions at various life cycle stages: the effects of educational organization decline and the effects of the returns and costs of schooling increase from the early to the later stages of schooling. Second, while there is support for Boudon's argument that persistent economic advantages to well-educated workers have driven educational growth, these effects are not so crucial as to dominate our data. So far as we can tell, students (and their families) respond to their perceptions of the labor market, but this is only one among many factors in their decision-making. Finally, the changing characteristics of school systems induce changes in students' attendance in contrast to what most cross-sectional evidence on schooling

would lead one to expect. From the standpoints of both an historical understanding of educational growth and forecasting future attendance levels, therefore, not only the demographic profiles of students and the labor markets they face but also the structural characteristics of the institutions they attend need to be taken into account.

Future research on these problems is in order. Analyses parallel to the present one for the black and female populations naturally are desirable, though they are limited by the lack of large data sets on the schooling and the family backgrounds of these groups. With the present data one can consider more parsimonious specifications by pooling the time series across school transitions and constraining some market and institutional effects to be equal across transitions. The resultant increased degrees of freedom can be exploited to search for possible temporal variation in the processes elucidated here. The present analysis assumes constant effects of the independent variables over all cohorts, but the effects may differ between periods when grade progression rates are low and when they are high. For example when high school graduation rates are lower than they are for more recent cohorts, the economic value of schooling may have a stronger effect than in recent periods when graduation is the norm. Similar changes in effects as a result of changing attainment norms may also occur for other market and institutional factors.

Market and institutional effects can be further studied by spatially disaggregating schooling trends to the state or city level using census school attainment data and Office of Education data on school systems. It is possible, therefore, to further exploit the richness of extant

schooling data to corroborate the findings reported here and to examine the effects of legal statutes relating to school attendance and child labor.

The study of educational change can benefit from several theoretical developments. The analysis of economic returns effects on grade progression is hampered by the absence of theory about how individuals use information on experiences of previous cohorts to make their school continuation decisions. Formal models of how perceptions of relevant labor force reference groups are determined and distributed within cohorts would mitigate the need for data exploration. Moreover, they would benefit the study of other phenomena such as relative income effects on aggregate fertility fluctuations, political sentiments, or subjective well-being, inasmuch as they have isomorphic formal problems of identifying reference groups for relative welfare measurement.

The study of educational growth could also benefit from abstract models of the mechanisms responsible for shifts in grade progression. The present analysis unearths a congeries of determinants of change in continuation rates. But save for general arguments, we lack theory to explain the findings. The process of school attendance decision-making is complex. Individuals respond to relatively long-run returns as well as the short-run costs of schooling; and they are sensitive to the constraints of the resource levels of the institutions they attend. How attendance is determined by the combination of incentive and constraint, and how these influences are aggregated into observed continuation rates are unsolved problems.

APPENDIX: USING THE CROSS-CORRELATION FUNCTION

We use the cross-correlation function (1) to identify the lags at which independent variables affect grade progression rates; (2) to identify multivariate relationships between grade progression and its aggregate determinants; and (3) to identify the form of the relative economic benefits to grade progression that affect grade progression. The first of these problems is discussed in the text. In this appendix we discuss our methods for the second and third of these problems. Additional discussion is given in Mare (1977a).

To identify multivariate equations relating grade progression and its determinants, we examine in sequence the cross-correlations between the adjusted log odds of grade progression and potential independent variables over the range of plausible lags of those variables. Then we estimate the zero order regression of grade progression on one of the independent variables shown by the cross-correlation function to have a particularly strong effect. We take the residuals from this equation and examine <u>their</u> cross-correlations with the independent variables. Since the residuals are othogonal to the first independent variable included in the regression equations, large cross-correlations between the residuals and certain independent variables indicate that some of those variables will improve the fit of the original regression equation if included along with the initial independent variable. We proceed sequentially, estimating successively more complex regression equations, taking the residuals and examining their cross-correlations with the independent variables. Eventually, the cross-correlations between the

residuals of the estimated equations and the independent variables are small, indicating that the effects of additional independent variables would, in the present equation, be minor and that their potential contribution to the overall fit of the equation is trivial.

We also use the cross-correlation function to search for the appropriate specification of the economic returns to schooling measure. In estimating (4), it is necessary to identify which previous cohorts affect the school continuation decisions of persons still in school. In comparing persons making the transition in question in previous years to persons not making the transition, persons may make intracohort comparisons between groups who enter the labor market at different times, intercohort comparisons between groups entering the labor market at the same time, or a combination of intercohort and intracohort comparisons. To explore these possibilities, for the t<u>th</u> school transition we formed ratios of the form

$$(Y^1_{n+p}/Y^2_n)_t$$

where Y^1_{n+p} denotes the average first job earnings score of members in the (n+p)<u>th</u> cohort who make the transition, Y^2_n denotes the score of members of the n<u>th</u> cohort who did not make the transition, and $p \geq 0$.

Then we examine the cross-correlation function between the adjusted log odds of grade progression and the natural logarithm of these ratios for selected values of p. That is, we estimate the cross-correlation function for each value of p where p = 0, 1, ..., P, and P denotes the maximum difference in ages between cohorts making the transition and

those not making the transition. For example, in the transition between high school graduation and college attendance, a realistic value of p is four since in labor-force-entry cohorts college graduates are typically four-years older than persons who completed high school but did not attend college.

When we use the cross-correlation function to examine the relationship between grade progression and a variety of estimates of the relative returns to grade progression in previous cohorts, the search procedure is two-dimensional. First, it searches for the age difference between the cohorts making and failing to make a transition which maximizes the correlation between the log odds of grade progression and relative returns measure. And second, for a given age difference, it searches for the appropriate lag(s) at which the relative returns measure, defined by the age difference, affects the grade progression rate of persons still in school. With the first-job returns to school measure in hand, it can be incorporated into the sequential search procedure for developing regression equations predicting grade progression rates.

FOOTNOTES

[1]A version of this paper will be presented to the 1978 meetings of the American Sociological Association. This research was supported by the National Science Foundation (NSF SOC76-80450) and by funds granted to the Institute for Research on Poverty, University of Wisconsin--Madison, by the U. S. Department of Health, Education, and Welfare pursuant to the provisions of the Economic Opportunity Act of 1964. The Occupational Changes in a Generation Survey data used in the analysis were gathered through National Science Foundation Grant NSF GI-31604. The author is grateful to William Mason, Hal Winsborough, and Christopher Winship for helpful suggestions; David Featherman and Robert Hauser for making available the OCG data; and Frances Simkus for clerical assistance. Opinions expressed herein are those of the author.

[2]Both the median and the 75th percentile of the state minimum compulsory school attendance age was 16 years for the period when persons born during the first half of the twentieth century attended school. The 25th percentile was 15 years up to 1918 and 16 years thereafter (U.S. Office of Education 1951, p. 14; 1966, p. 3).

[3]These period rates compare favorably to period rates independently derived from Office of Education school enrollment data, validating our methods of adjusting the data for age-grade abnormality. For full discussion of age-grade abnormality, our adjustments, and analysis validating the adjustments, see Mare (1977a, Ch. 4). The period grade progression estimates are not without conceptual ambiguity in that they are not affected solely by attrition. They are also affected by persons

interrupting and returning to school. The estimates, therefore, are not of progression rates per se, but changes in grade-specific enrollments due to changes in rates and timing of grade progression. We must assume that causes of progression rates uniformly affect rates of attrition, temporary withdrawal, and return from temporary withdrawal.

[4]A more parsimonious model of grade progression would include all schooling levels simultaneously. This would be a time series of cross sections models, where the school transitions were the cross sections and the periods the time series. It is hard, however, to construct such a model given limitations of theory and data on macro-level independent variables. Different processes, moreover, may govern educational growth at various levels of schooling. In any event, the analysis is exploratory and thus uses models less parsimonious than the ideal.

A more efficient method of estimating macro-level effects on grade progression net of change in cohort social background composition is to replace λ_{jt} in (1) with the Z_{jmt} and simultaneously estimate individual and macro-level effects. However, the size of the OCG sample, the expense of the maximum likelihood estimation of (1), and our interest in exploring many alternative specifications of macro-level effects made this method impractical.

[5]For discussion of analogous problems in specifying perceived relative income effects on fertility decisions, see Liebenstein (1976).

[6]The 1970 Census earnings by occupation tabulations give the 1969 earnings of persons by their 1970 occupation. Thus, occupational mobility

between 1969 and 1970 renders the tabulations fallible measures of the 1970 occupational earnings distribution. To obtain earnings scores, we must assume that the census tabulations reliably indicate the 1970 earnings distribution within occupations, an assumption made by most researchers working with occupation-income relationships in Census or Current Population Survey data. See Duncan, Featherman, and Duncan (1972, p. 39) for further discussion. In preliminary analyses we considered alternative scales, including the Duncan socioeconomic index and median earnings taken from tabulations for the 18-24 and 25-34 year old men. The alternative earnings scales did not affect the results of these analyses. The Duncan socioeconomic index yielded smaller and less stable estimates of returns effects on grade progression than the median earnings scales. All of these scales assume that the relative socio-economic positions of occupations are stable over time.

As argued above, it is necessary to consider not birth cohorts but persons who make grade progression decisions at the same time. Unless stated otherwise, therefore, cohorts denote schooling-level cohorts rather than birth cohorts. Similarly, calculations of the average first job earnings of cohorts are for men who terminate or continue schooling in the same year rather than for birth cohorts.

[7]Some OCG respondents report first jobs that they took before finishing school. Since these are often jobs taken to earn money to acquire further schooling, they do not indicate the "return" that the individual derives from his final educational attainment. Men who reported starting their job prior to finishing school, therefore, are

excluded from our calculations of earnings. When average earnings estimates are computed for one-year cohorts by schooling-level, they are in places based on small numbers of observations. As a result, the relative earnings series are subject to considerable sampling variability.

[8]The series on student fees denotes fees paid by students or their private benefactors for all educational services except room and board. Federal payments for veterans are excluded. The series is taken from U. S. Bureau of the Census (1975, p. 384) and adjusted to constant (1967) dollars. The series and its original sources are given in U. S. Bureau of the Census (1975, p. 383) and (1975, p. 366) respectively.

[9]The unemployment series is given in U. S. Bureau of the Census (1975, p. 135) for 1910-1930 and 1944-1970. For 1931-1943 we use Darby's (1976) "Corrected BLS" series.

[10]That is, we use the residuals from the regression of the variable or its natural logarithm on a linear time trend. Which variables are expressed in logarithmic scales is made clear in the presentation of our results.

[11]Unless stated otherwise, equations presented in this section are estimated by ordinary least squares (OLS). When Durbin-Watson statistics indicate first order serial correlation, we use other methods as noted in the text. Because some series are biennial and others annual and equations differ in the lags at which the independent variables have effects, regressions are run over different numbers of observations. The reported equations are always estimated over the maximum available observations. In analysis not reported here, we have compared equations estimated with equal numbers of observations and verified that differences

among reported equations are not artifacts of the unequal numbers of observations upon which they are estimated.

[12]At all stages of the analysis we considered alternative ratios at other lags. With no important exceptions, however, the same returns variable had the strongest impact on 8th grade completion.

[13]Strictly speaking, it is wrong to regard the coefficients as proportionate responses in the dependent variable. Were the final dependent variable in logarithmic form, this would be correct. But our dependent variables are adjusted log odds of grade progression rather than log adjusted odds. Since these are not generally identical, the coefficients are not measured in proportionate changes in adjusted odds. Nonetheless, since they are approximately equal, we discuss our results as if the proportionate change interpretation were correct.

[14]The effect of the change in unemployment at this age suggests that it is correlated with other factors, such as fluctuation in the relative earnings of persons of different education levels, not captured by the earnings measure included in the equation. If, for example, individuals' decisions are affected by the relative chances of unemployment of men in earlier cohorts with different amounts of schooling, then there could be a positive effect of unemployment changes a number of years before the given change could influence the cohorts' own employment possibilities. When unemployment increases, the relative chances of unemployment for well-educated workers decrease and induce higher progression rates for cohorts still in school.

[15]The first stage is OLS. The residuals from this stage are used to estimate ρ where

$$\varepsilon_t = \rho\varepsilon_{t-1} + v_t,$$

ε_t is the disturbance in the first-stage equation, and v_t is a disturbance assumed to be serially independent. Then we transform each variable X_{it} as follows:

$$X'_{it} = X_{it} - X_{i(t-1)}.$$

The second stage is to estimate the equation by OLS using the X'_{it}. Second-stage estimates are reported in Table 1. For further discussion see, for example, Johnston (1973, pp. 263-264).

[16]Since the correlation between fluctuations in expenditures and teachers' salaries is 0.85, we report the effects of these variables in separate equations.

[17]In the presence of the returns measure, the effect of change in unemployment when the cohort enters school is negligible, suggesting that the observed early effect of change in unemployment rates is due to a positive correlation between unemployment and the economic benefits to formal schooling, But this does not explain the effect of DUR_6 in Equation 1. The latter evidently requires consideration of other relative benefits to grade progression, such as those discussed in footnote 14.

[18]That the relative earnings effect is the same in the equations including teacher salaries and educational expenditures suggests that teacher salaries and relative earnings are not redundant measures in

contrast to our earlier argument. Alternatively, however, the relative earnings measure for this transition is based on more individual-level observations than the one we used for 8th grade completion making it more reliable and thus enabling us to isolate the relative earnings effect and the school quality effect of teacher salaries simultaneously.

[19]We examined the effects of the level and change in unemployment on the series of fluctuations in the log odds of grade progression not adjusted for social background composition, the 16 year old unemployment rate and its changes on high school graduation for cohorts attending high school in the post-war era, and interactions between period (post-war versus pre-war) and unemployment levels and changes on high school graduation. In no instance did we detect unemployment effects large enough to warrant interpretation.

[20]Additional analysis not reported here shows that the pupil-teacher ratio, the number of one-teacher public schools, and changes in the unemployment rate have no effect on high school graduation irrespective of what subset of the independent variables are included in an equation. In addition, we do not present further equations including teacher salaries and excluding expenditures per pupil. Results for equations including teacher salaries parallel those obtained from equations including expenditures reported here.

[21]Our data provide reliable enough estimates of progression rates from high school to college to permit time-series analysis. Evidence from other data, however, suggests that our progression rates from high school to college are less reliable than for earlier school

transitions inasmuch as they are based on fewer observations at the individual level (Mare 1977a, Ch. 4). This renders parameter estimates sensitive to the inclusion or exclusion of one or two observations in the series. To handle this problem, we smoothed the adjusted grade progression rates by taking a simple three-term moving average of the observations. This both stabilizes and makes more interpretable the parameter estimates. Since this reduces high-frequency variation in the dependent series, it is easier to obtain a close fit between the independent variables and the grade progression series than would otherwise be possible. It would be unwarranted, therefore, to conclude that the large coefficients of determination we report indicate great success in explaining variation in grade progression rates from high school to college. (Freeman (1975) explains 95 percent of the variance in college attendance rates with a relative earnings measure and an index of the demand for college educated labor. In that analysis, however, variables are measured at their observed levels rather than as deviations from the trends that they follow. Although Freeman's model accounts for the recent downturn in attendance, its close fit is partly due to the common linear trend in the variables.)

[22]The Durbin-Watson statistic for Equation 2 is in the interval where the test for first order autocorrelation is ambiguous, but the serial correlation parameter ρ estimated from the residuals is large enough (0.352) that it is unlikely that Equation 2 is free from autocorrelation. We attempted to take account of this by reestimating the equation via the Cochrane-Orcutt iterative method (Cochrane and Orcutt 1949), using the $\hat{\rho}$ estimated from the residuals of the first round of

estimation as the start value for ρ. This procedure, however, converged to estimates similar to those of Equation 2 but with a smaller Durbin-Watson statistic. Under the assumption that this is a local solution to the iterative procedure, we estimated the equation for several other values of ρ. To obtain a Durbin-Watson statistic close to 2.0, it is necessary to assume that ρ is approximately 0.9. When the equation is estimated under this assumption, parameter and standard error estimates are similar to those of Equation 2.

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