CHAPTER 5

The Process of Stratification

Stratification systems may be characterized in various ways. Surely one of the most important has to do with the processes by which individuals become located, or locate themselves, in positions in the hierarchy comprising the system. At one extreme we can imagine that the circumstances of a person's birth—including the person's sex and the perfectly predictable sequence of age levels through which he is destined to pass—suffice to assign him unequivocally to a ranked status in a hierarchical system. At the opposite extreme his prospective adult status would be wholly problematic and contingent at the time of birth. Such status would become entirely determinate only as adulthood was reached, and solely as a consequence of his own actions taken freely—that is, in the absence of any constraint deriving from the circumstances of his birth or rearing. Such a pure achievement system is, of course, hypothetical, in much the same way that motion without friction is a purely hypothetical possibility in the physical world. Whenever the stratification system of any moderately large and complex society is described, it is seen to involve both ascriptive and achievement principles.

In a liberal democratic society we think of the more basic principle as being that of achievement. Some ascriptive features of the system may be regarded as vestiges of an earlier epoch, to be extirpated as rapidly as possible. Public policy may emphasize measures designed to enhance or to equalize opportunity—hopefully, to overcome ascriptive obstacles to the full exercise of the achievement principle.

The question of how far a society may realistically aspire to go in this direction is hotly debated, not only in the ideological arena but in the academic forum as well. Our contribution, if any, to the debate will consist largely in submitting measurements and estimates of the

strength of ascriptive forces and of the scope of opportunities in a large contemporary society. The problem of the relative importance of the two principles in a given system is ultimately a quantitative one. We have pushed our ingenuity to its limit in seeking to contrive relevant quantifications.

The governing conceptual scheme in the analysis is quite a commonplace one. We think of the individual's life cycle as a sequence in time that can be described, however partially and crudely, by a set of classificatory or quantitative measurements taken at successive stages. Ideally we should like to have under observation a cohort of births, following the individuals who make up the cohort as they pass through life. As a practical matter we resorted to retrospective questions put to a representative sample of several adjacent cohorts so as to ascertain those facts about their life histories that we assumed were both relevant to our problem and accessible by this means of observation.

Given this scheme, the questions we are continually raising in one form or another are: how and to what degree do the circumstances of birth condition subsequent status? and, how does status attained (whether by ascription or achievement) at one stage of the life cycle affect the prospects for a subsequent stage? The questions are neither idle nor idiosyncratic ones. Current policy discussion and action come to a focus in a vaguely explicated notion of the "inheritance of poverty." Thus a spokesman for the Social Security Administration writes:

> It would be one thing if poverty hit at random and no one group were singled out. It is another thing to realize that some seem destined to poverty almost from birth—by their color or by the economic status or occupation of their parents.[1]

Another officially sanctioned concept is that of the "dropout," the person who fails to graduate from high school. Here the emphasis is not so much on circumstances operative at birth but on the presumed effect of early achievement on subsequent opportunities. Thus the "dropout" is seen as facing "a lifetime of uncertain employment,"[2] probable assignment to jobs of inferior status, reduced earning power, and vulnerability to various forms of social pathology.

[1] Mollie Orshansky, "Children of the Poor," *Social Security Bulletin*, 26(July 1963).

[2] Forrest A. Bogan, "Employment of High School Graduates and Dropouts in 1964," *Special Labor Force Report*, No. 54 (U. S. Bureau of Labor Statistics, June 1965), p. 643.

In this study we do not have measurements on all the factors implicit in a full-blown conception of the "cycle of poverty" nor all those variables conceivably responding unfavorably to the achievement of "dropout" status. For practical reasons, as explained in Chapter 1, we were severely limited in the amount of information to be collected. For theoretical reasons—also spelled out more fully in Chapter 1—and in conformity with the tradition of studies in social mobility, we chose to emphasize occupation as a measure both of origin status and of status achievement. The present chapter is even more strictly limited to variables we think can be treated meaningfully as quantitative and therefore are suited to analysis by the regression technique described in Chapter 4. This limitation, however, is not merely an analytical convenience. We think of the selected quantitative variables as being sufficient to describe the major outlines of status changes in the life cycle of a cohort. Thus a study of the relationships among these variables leads to a formulation of a basic model of the process of stratification. In this chapter we consider also certain extensions of this model. Subsequent chapters provide, in effect, a number of additional detailed extensions, although these are secured only by giving up some of the elegance and convenience of the particular analytical procedures employed here.

A BASIC MODEL

To begin with, we examine only five variables. For expository convenience, when it is necessary to resort to symbols, we shall designate them by arbitrary letters but try to remind the reader from time to time of what the letters stand for. These variables are:

V: Father's educational attainment
X: Father's occupational status
U: Respondent's educational attainment
W: Status of respondent's first job
Y: Status of respondent's occupation in 1962

Each of the three occupational statuses is scaled by the index described in Chapter 4, ranging from 0 to 96. The two education variables are scored on the following arbitrary scale of values ("rungs" on the "educational ladder") corresponding to specified numbers of years of formal schooling completed:

0: No school
1: Elementary, one to four years
2: Elementary, five to seven years

3: Elementary, eight years
4: High school, one to three years
5: High school, four years
6: College, one to three years
7: College, four years
8: College, five years or more (i.e., one or more years of postgraduate study)

Actually, this scoring system hardly differs from a simple linear transformation, or "coding," of the exact number of years of school completed. In retrospect, for reasons given in Chapter 4, we feel that the score implies too great a distance between intervals at the lower end of the scale; but the resultant distortion is minor in view of the very small proportions scored 0 or 1.

A basic assumption in our interpretation of regression statistics—though not in their calculation as such—has to do with the causal or temporal ordering of these variables. In terms of the father's career we should naturally assume precedence of V (education) with respect to X (occupation when his son was 16 years old). We are not concerned with the father's career, however, but only with his statuses that comprised a configuration of background circumstances or origin conditions for the cohorts of sons who were respondents in the OCG study. Hence we generally make no assumption as to the priority of V with respect to X; in effect, we assume the measurements on these variables to be contemporaneous from the son's viewpoint. The respondent's education, U, is supposed to follow in time—and thus to be susceptible to causal influence from—the two measures of father's status. Because we ascertained X as of respondent's age 16, it is true that some respondents may have completed school before the age to which X pertains. Such cases were doubtlessly a small minority and in only a minor proportion of them could the father (or other family head) have changed status radically in the two or three years before the respondent reached 16.

The next step in the sequence is more problematic. We assume that W (first job status) follows U (education). The assumption conforms to the wording of the questionnaire (see Appendix B), which stipulated "the first full-time job you had after you left school." In the years since the OCG study was designed we have been made aware of a fact that should have been considered more carefully in the design. Many students leave school more or less definitively, only to return, perhaps to a different school, some years later, whereupon they often

finish a degree program.[3] The OCG questionnaire contained information relevant to this problem, namely the item on age at first job. Through an oversight no tabulations of this item were made for the present study. Tables prepared for another study[4] using the OCG data, however, suggest that approximately one-eighth of the respondents report a combination of age at first job and education that would be very improbable unless (a) they violated instructions by reporting a part-time or school-vacation job as the first job, or (b) they did, in fact, interrupt their schooling to enter regular employment. (These "inconsistent" responses include men giving 19 as their age at first job and college graduation or more as their education; 17 or 18 with some college or more; 14, 15, or 16 with high-school graduation or more; and under 14 with some high school or more.) When the two variables are studied in combination with occupation of first job, a very clear effect is evident. Men with a given amount of education beginning their first jobs early held lower occupational statuses than those beginning at a normal or advanced age for the specified amount of education.

Despite the strong probability that the U-W sequence is reversed for an appreciable minority of respondents, we have hardly any alternative to the assumption made here. If the bulk of the men who interrupted schooling to take their first jobs were among those ultimately securing relatively advanced education, then our variable W is downwardly biased, no doubt, as a measure of their occupational status immediately after they finally left school for good. In this sense, the correlations between U and W and between W and Y are probably attenuated. Thus, if we had really measured "job after completing education" instead of "first job," the former would in all likelihood have loomed somewhat larger as a variable intervening between education and 1962 occupational status. We do not wish to argue that our respondents erred in their reports on first job. We are inclined to conclude that their reports were realistic enough, and that it was our assumption about the meaning of the responses that proved to be fallible.

The fundamental difficulty here is conceptual. If we insist on *any* uniform sequence of the events involved in accomplishing the transi-

[3] Bruce K. Eckland, "College Dropouts Who Came Back," *Harvard Educational Review*, 34(1964), 402-420.

[4] Beverly Duncan, *Family Factors and School Dropout: 1920–1960*, U. S. Office of Education, Cooperative Research Project No. 2258, Ann Arbor: Univers. of Michigan, 1965.

tion to independent adult status, we do violence to reality. Completion of schooling, departure from the parental home, entry into the labor market, and contracting of a first marriage are crucial steps in this transition, which all normally occur within a few short years. Yet they occur at no fixed ages nor in any fixed order. As soon as we aggregate individual data for analytical purposes we are forced into the use of simplifying assumptions. Our assumption here is, in effect, that "first job" has a uniform significance for all men in terms of its temporal relationship to educational preparation and subsequent work experience. If this assumption is not strictly correct, we doubt that it could be improved by substituting any other *single* measure of initial occupational status. (In designing the OCG questionnaire, the alternative of "job at the time of first marriage" was entertained briefly but dropped for the reason, among others, that unmarried men would be excluded thereby.)

One other problem with the *U-W* transition should be mentioned. Among the younger men in the study, 20 to 24 years old, are many who have yet to finish their schooling or to take up their first jobs or both —not to mention the men in this age group missed by the survey on account of their military service (see Appendix C). Unfortunately, an early decision on tabulation plans resulted in the inclusion of the 20 to 24 group with the older men in aggregate tables for men 20 to 64 years old. We have ascertained that this results in only minor distortions by comparing a variety of data for men 20 to 64 and for those 25 to 64 years of age. Once over the *U-W* hurdle, we see no serious objection to our assumption that both *U* and *W* precede *Y*, except in regard to some fraction of the very young men just mentioned.

In summary, then, we take the somewhat idealized assumption of temporal order to represent an order of priority in a causal or processual sequence, which may be stated diagrammatically as follows:

$$(V, X) - (U) - (W) - (Y).$$

In proposing this sequence we do not overlook the possibility of what Carlsson calls "delayed effects,"[5] meaning that an early variable may affect a later one not only via intervening variables but also directly (or perhaps through variables not measured in the study).

In translating this conceptual framework into quantitative estimates the first task is to establish the pattern of associations between the variables in the sequence. This is accomplished with the correlation coefficient, as explained in Chapter 4. Table 5.1 supplies the correla-

[5] Gösta Carlsson, *Social Mobility and Class Structure,* Lund: CWK Gleerup, 1958, p. 124.

TABLE 5.1. SIMPLE CORRELATIONS FOR FIVE STATUS VARIABLES

Variable	Variable Y	W	U	X	V
Y: 1962 occ. status	...	.541	.596	.405	.322
W: First-job status		...	.538	.417	.332
U: Education			...	.438	.453
X: Father's occ. status				...	.516
V: Father's education					...

tion matrix on which much of the subsequent analysis is based. In discussing causal interpretations of these correlations, we shall have to be clear about the distinction between two points of view. On the one hand, the simple correlation—given our assumption as to direction of causation—measures the gross magnitude of the effect of the antecedent upon the consequent variable. Thus, if $r_{YW} = .541$, we can say that an increment of one standard deviation in first job status produces (whether directly or indirectly) an increment of just over half of one standard deviation in 1962 occupational status. From another point of view we are more concerned with net effects. If both first job and 1962 status have a common antecedent cause—say, father's occupation—we may want to state what part of the effect of *W* on *Y* consists in a transmission of the prior influence of *X*. Or, thinking of *X* as the initial cause, we may focus on the extent to which its influence on *Y* is transmitted by way of its prior influence on *W*.

We may, then, devote a few remarks to the pattern of gross effects before presenting the apparatus that yields estimates of net direct and indirect effects. Since we do not require a causal ordering of father's education with respect to his occupation, we may be content simply to note that $r_{XV} = .516$ is somewhat lower than the corresponding correlation, $r_{YU} = .596$, observed for the respondents themselves. The difference suggests a heightening of the effect of education on occupational status between the fathers' and the sons' generations. Before stressing this interpretation, however, we must remember that the measurements of *V* and *X* do not pertain to some actual cohort of men, here designated "fathers." Each "father" is represented in the data in proportion to the number of his sons who were 20 to 64 years old in March 1962.

The first recorded status of the son himself is education (*U*). We note that r_{UV} is just slightly greater than r_{UX}. Apparently both measures on the father represent factors that may influence the son's education.

In terms of gross effects there is a clear ordering of influences on first job. Thus $r_{WU} > r_{WX} > r_{WV}$. Education is most strongly corre-

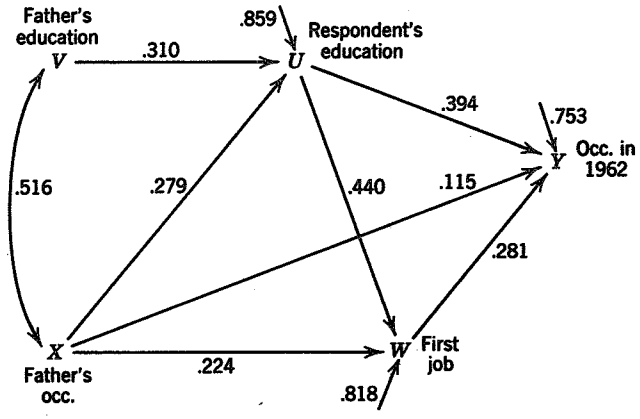

Figure 5.1. Path coefficients in basic model of the process of stratification.

lated with first job, followed by father's occupation, and then by father's education.

Occupational status in 1962 (Y) apparently is influenced more strongly by education than by first job; but our earlier discussion of the first-job measure suggests we should not overemphasize the difference between r_{YW} and r_{YU}. Each, however, is substantially greater than r_{YX}, which in turn is rather more impressive than r_{YV}.

Figure 5.1 is a graphic representation of the system of relationships among the five variables that we propose as our basic model. The numbers entered on the diagram, with the exception of r_{XV}, are path coefficients, the estimation of which will be explained shortly. First we must become familiar with the conventions followed in constructing this kind of diagram. The link between V and X is shown as a curved line with an arrowhead at both ends. This is to distinguish it from the other lines, which are taken to be paths of influence. In the case of V and X we may suspect an influence running from the former to the latter. But if the diagram is logical for the respondent's generation, we should have to assume that for the fathers, likewise, education and occupation are correlated not only because one affects the other but also because common causes lie behind both, which we have not measured. The bidirectional arrow merely serves to sum up all sources of correlation between V and X and to indicate that the explanation thereof is not part of the problem at hand.

The straight lines running from one measured variable to another represent *direct* (or net) influences. The symbol for the path coefficient, such as p_{YW}, carries a double subscript. The first subscript is the variable at the head of the path, or the effect; the second is the causal variable. (This resembles the convention for regression coefficients, where the first subscript refers to the "dependent" variable, the second to the "independent" variable.)

Finally, we see lines with no source indicated carrying arrows to each of the effect variables. These represent the residual paths, standing for all other influences on the variable in question, including causes not recognized or measured, errors of measurement, and departures of the true relationships from additivity and linearity, properties that are assumed throughout the analysis (as explained in the section on regression in Chapter 4).

An important feature of this kind of causal scheme is that variables recognized as effects of certain antecedent factors may, in turn, serve as causes for subsequent variables. For example, U is caused by V and X, but it in turn influences W and Y. The algebraic representation of the scheme is a system of equations, rather than the single equation more often employed in multiple regression analysis. This feature permits a flexible conceptualization of the *modus operandi* of the causal network. Note that Y is shown here as being influenced directly by W, U, and X, but not by V (an assumption that will be justified shortly). But this does not imply that V has no influence on Y. V affects U, which does affect Y both directly and indirectly (via W). Moreover, V is correlated with X, and thus shares in the gross effect of X on Y, which is partly direct and partly indirect. Hence the gross effect of V on Y, previously described in terms of the correlation r_{YV}, is here interpreted as being entirely indirect, in consequence of V's effect on intervening variables and its correlation with another cause of Y.

PATH COEFFICIENTS

Whether a path diagram, or the causal scheme it represents, is adequate depends on both theoretical and empirical considerations. At a minimum, before constructing the diagram we must know, or be willing to assume, a causal ordering of the observed variables (hence the lengthy discussion of this matter earlier in this chapter). This information is external or *a priori* with respect to the data, which merely describe associations or correlations. Moreover, the causal scheme must be complete, in the sense that all causes are accounted for. Here, as in most problems involving analysis of observational data, we achieve a formal completeness of the scheme by representing unmeasured causes as a residual factor, presumed to be uncorrelated with the remaining factors lying behind the variable in question. If

any factor is known or presumed to operate in some other way it must be represented in the diagram in accordance with its causal role, even though it is not measured. Sometimes it is possible to deduce interesting implications from the inclusion of such a variable and to secure useful estimates of certain paths in the absence of measurements on it, but this is not always so. A partial exception to the rule that all causes must be explicitly represented in the diagram is the unmeasured variable that can be assumed to operate strictly as an intervening variable. Its inclusion would enrich our understanding of a causal system without invalidating the causal scheme that omits it. Sociologists have only recently begun to appreciate how stringent are the logical requirements that must be met if discussion of causal processes is to go beyond mere impressionism and vague verbal formulations.[6] We are a long way from being able to make causal inferences with confidence, and schemes of the kind presented here had best be regarded as crude first approximations to adequate causal models.

On the empirical side, a minimum test of the adequacy of a causal diagram is whether it satisfactorily accounts for the observed correlations among the measured variables. In making such a test we employ the fundamental theorem in path analysis, which shows how to obtain the correlation between any two variables in the system, given the path coefficients and correlations entered on the diagram.[7] Without stating this theorem in general form we may illustrate its application here. For example,

$$r_{YX} = p_{YX} + p_{YU}r_{UX} + p_{YW}r_{WX};$$

and

$$r_{WX} = p_{WX} + p_{WU}r_{UX}.$$

We make use of each path leading to a given variable (such as Y in the first example) and the correlations of each of its causes with all other variables in the system. The latter correlations, in turn, may be analyzed; for example, r_{WX}, which appeared as such in the first equation, is broken down into two parts in the second. A complete expansion along these lines is required to trace out all the indirect connections between variables; thus,

$$r_{YX} = p_{YX} + p_{YU}p_{UX} + p_{YU}p_{UV}r_{VX} + p_{YW}p_{WX} + p_{YW}p_{WU}p_{UX} + p_{YW}p_{WU}p_{UV}r_{VX}.$$

[6] H. M. Blalock, Jr., *Causal Inferences in Nonexperimental Research*, Chapel Hill: Univer. of North Carolina Press, 1964.

[7] Sewall Wright, "Path Coefficients and Path Regressions," *Biometrics*, **16** (1960), 189–202; Otis Dudley Duncan, "Path Analysis," *American Journal of Sociology*, **72**(1966), 1-16.

Now, if the path coefficients are properly estimated, and if there is no inconsistency in the diagram, the correlations calculated by a formula like the foregoing must equal the observed correlations. Let us compare the values computed from such a formula with the corresponding observed correlations:

$$\begin{aligned} r_{WV} &= p_{WX}r_{XV} + p_{WU}r_{UV} \\ &= (.224)(.516) + (.440)(.453) \\ &= .116 + .199 = .315 \end{aligned}$$

which compares with the observed value of .332; and

$$\begin{aligned} r_{YV} &= p_{YU}r_{UV} + p_{YX}r_{XV} + p_{YW}r_{WV} \\ &= (.394)(.453) + (.115)(.516) + (.281)(.315) = .326 \end{aligned}$$

(using here the calculated rather than the observed value of r_{WV}), which resembles the actual value, .322. Other such comparisons—for r_{YX}, for example—reveal, at most, trivial discrepancies (no larger than .001).

We arrive, by this roundabout journey, at the problem of getting numerical values for the path coefficients in the first place. This involves using equations of the foregoing type inversely. We have illustrated how to obtain correlations if the path coefficients are known, but in the typical empirical problem we know the correlations (or at least some of them) and have to estimate the paths. For a diagram of the type of Figure 5.1 the solution involves equations of the same form as those of linear multiple regression, except that we work with a recursive system of regression equations[8] rather than a single regression equation.

Table 5.2 records the results of the regression calculations. It can be seen that some alternative combinations of independent variables were studied. It turned out that the net regressions of both W and Y on V were so small as to be negligible. Hence V could be disregarded as a direct influence on these variables without loss of information. The net regression of Y on X was likewise small but, as it appears, not entirely negligible. Curiously, this net regression is of the same order of magnitude as the proportion of occupational inheritance in this population—about 10 per cent, as discussed in Chapter 4. We might speculate that the direct effect of father's occupation on the occupational status of a mature man consists of this modest amount of strict occupational inheritance. The remainder of the effect of X on Y is indirect, inasmuch as X has previously influenced U and W, the son's education and the occupational level at which he got his start. For reasons noted in Chapter 3 we do not assume that the full impact of

[8] Blalock, *op. cit.*, pp. 54ff.

TABLE 5.2. PARTIAL REGRESSION COEFFICIENTS IN STANDARD FORM (BETA COEFFICIENTS) AND COEFFICIENTS OF DETERMINATION, FOR SPECIFIED COMBINATIONS OF VARIABLES

Dependent Variable[a]	Independent Variables[a]				Coefficient of Determination
	W	U	X	V	(R^2)
U[b]	...	...	.279	.310	.26
W	...	.433	.214	.026	.33
W[b]	...	.440	.224	...	.33
Y	.282	.397	.120	-.014	.43
Y[b]	.281	.394	.115	...	.43
Y	.311	.428	...	...	.42

[a]V: Father's education.
X: Father's occ. status.
U: Respondent's education.
W: First-job status.
Y: 1962 occ. status.
[b]Beta coefficients in these sets taken as estimates of path coefficients for Figure 5.1.

the tendency to take up the father's occupation is registered in the choice of first job.

With the formal properties of the model in mind we may turn to some general problems confronting this kind of interpretation of our results. One of the first impressions gained from Figure 5.1 is that the largest path coefficients in the diagram are those for residual factors, that is, variables not measured. The residual path is merely a convenient representation of the extent to which measured causes in the system fail to account for the variation in the effect variables. (The residual is obtained from the coefficient of determination; if $R^2_{Y(WUX)}$ is the squared multiple correlation of Y on the three independent variables, then the residual for Y is $\sqrt{1 - R^2_{Y(WUX)}}$.) Sociologists are often disappointed in the size of the residual, assuming that this is a measure of their success in "explaining" the phenomenon under study. They seldom reflect on what it would mean to live in a society where nearly perfect explanation of the dependent variable could be secured by studying causal variables like father's occupation or respondent's education. In such a society it would indeed be true that some are "destined to poverty almost from birth . . . by the economic status or occupation of their parents" (in the words of the reference cited in footnote 1). Others, of course, would be "destined" to affluence or to modest circumstances. By no effort of their own could they materially alter the course of destiny, nor could any stroke of fortune, good or ill, lead to an outcome not already in the cards.

Thinking of the residual as an index of the adequacy of an explanation gives rise to a serious misconception. It is thought that a high multiple correlation is presumptive evidence that an explanation is correct or nearly so, whereas a low percentage of determination means

that a causal interpretation is almost certainly wrong. The fact is that the size of the residual (or, if one prefers, the proportion of variation "explained") is *no* guide whatever to the validity of a causal interpretation. The best-known cases of "spurious correlation"—a correlation leading to an egregiously wrong interpretation—are those in which the coefficient of determination is quite high.

The relevant question about the residual is not really its size at all, but whether the unobserved factors it stands for are properly represented as being uncorrelated with the measured antecedent variables. We shall entertain subsequently some conjectures about unmeasured variables that clearly are not uncorrelated with the causes depicted in Figure 5.1. It turns out that these require us to acknowledge certain possible modifications of the diagram, whereas other features of it remain more or less intact. A delicate question in this regard is that of the burden of proof. It is all too easy to make a formidable list of unmeasured variables that someone has alleged to be crucial to the process under study. But the mere existence of such variables is already acknowledged by the very presence of the residual. It would seem to be part of the task of the critic to *show,* if only hypothetically, but *specifically,* how the modification of the causal scheme to include a new variable would disrupt or alter the relationships in the original diagram. His argument to this effect could then be examined for plausibility and his evidence, if any, studied in terms of the empirical possibilities it suggests.

Our supposition is that the scheme in Figure 5.1 is most easily subject to modification by introducing additional measures of the same kind as those used here. If indexes relating to socioeconomic background other than V and X are inserted we will almost certainly estimate differently the direct effects of these particular variables. If occupational statuses of the respondent intervening between W and Y were known we should have to modify more or less radically the right-hand portion of the diagram, as will be shown in the next section. Yet we should argue that such modifications may amount to an enrichment or extension of the basic model rather than an invalidation of it. The same may be said of other variables that function as intervening causes. In theory, it should be possible to specify these in some detail, and a major part of the research worker's task is properly defined as an attempt at such specification. In the course of such work, to be sure, there is always the possibility of a discovery that would require a fundamental reformulation, making the present model obsolete. Discarding the model would be a cost gladly paid for the prize of such a discovery.

Postponing the confrontation with an altered model, the one at hand is not lacking in interest. An instructive exercise is to compare the magnitudes of gross and net relationships. Here we make use of the fact that the correlation coefficient and the path coefficient have the same dimensionality. The correlation $r_{YX} = .405$ (Table 5.1) means that a unit change (one standard deviation) in X produces a change of 0.4 unit in Y, in gross terms. The path coefficient, $p_{YX} = .115$ (Figure 5.1), tells us that about one-fourth of this gross effect is a result of the direct influence of X on Y. (We speculated above on the role of occupational inheritance in this connection.) The remainder ($.405 - .115 = .29$) is indirect, via U and W. The sum of all indirect effects, therefore, is given by the difference between the simple correlation and the path coefficient connecting two variables. We note that the indirect effects on Y are generally substantial, relative to the direct. Even the variable temporally closest (we assume) to Y has "indirect effects"—actually, common antecedent causes—nearly as large as the direct. Thus $r_{YW} = .541$ and $p_{YW} = .281$, so that the aggregate of "indirect effects" is .26, which in this case are common determinants of Y and W that spuriously inflate the correlation between them.

To ascertain the indirect effects along a given chain of causation we must multiply the path coefficients along the chain. The procedure is to locate on the diagram the dependent variable of interest, and then trace back along the paths linking it to its immediate and remote causes. In such a tracing we may reverse direction once but only once, following the rule "first back, then forward." Any bidirectional correlation may be traced in either direction. If the diagram contains more than one such correlation, however, only one may be used in a given compound path. In tracing the indirect connections no variable may be intersected more than once in one compound path. Having traced all such possible compound paths, we obtain the entirety of indirect effects as their sum.

Let us consider the example of effects of education on first job, U on W. The gross or total effect is $r_{WU} = .538$. The direct path is $p_{WU} = .440$. There are two indirect connections or compound paths: from W back to X then forward to U; and from W back to X, then back to V, and then forward to U. Hence we have:

$$\underset{\text{(gross)}}{r_{WU}} = \underset{\text{(direct)}}{p_{WU}} + \underbrace{p_{WX}p_{UX} + p_{WX}r_{XV}p_{UV}}_{\text{(indirect)}}$$

or, numerically,

$$\begin{aligned} .538 &= .440 + (.224)(.279) + (.224)(.516)(.310) \\ &= .440 + .062 + .036 \\ &= .440 + .098. \end{aligned}$$

In this case all the indirect effect of U on W derives from the fact that both U and W have X (plus V) as a common cause. In other instances, when more than one common cause is involved and these causes are themselves interrelated, the complexity is too great to permit a succinct verbal summary.

A final stipulation about the scheme had best be stated, though it is implicit in all the previous discussion. The form of the model itself, but most particularly the numerical estimates accompanying it, are submitted as valid only for the population under study. No claim is made that an equally cogent account of the process of stratification in another society could be rendered in terms of this scheme. For other populations, or even for subpopulations within the United States, the magnitudes would almost certainly be different, although we have some basis for supposing them to have been fairly constant over the last few decades in this country. The technique of path analysis is not a method for discovering causal laws but a procedure for giving a quantitative interpretation to the manifestations of a known or assumed causal system as it operates in a particular population. When the same interpretive structure is appropriate for two or more populations there is something to be learned by comparing their respective path coefficients and correlation patterns. We have not yet reached the stage at which such comparative study of stratification systems is feasible.

AGE GROUPS: THE LIFE CYCLE OF A SYNTHETIC COHORT

For simplicity, the preceding analysis has ignored differences among age groups. Our present task is to venture some interpretation of such differences. The raw material for the analysis is presented in Table 5.3 in the form of simple correlations between pairs of the five status variables under study. For the reasons mentioned in Chapter 3, this analysis is confined to men with nonfarm background.

We must consider immediately what kinds of inferences or interpretations are allowed by comparisons among the four cohorts. Three of the variables are specified as of a more or less uniform stage of the respondent's life cycle: father's occupation (X), respondent's education (U), and first job (W). Father's education (V), on the other hand, was presumably determinate in the father's youth; the time interval between V and any of the former variables would be determined in large part by father's age at respondent's birth. This interval is variable in length. We might, however, assume that the time interval from V to X, though highly variable within each cohort of respondents, has a similar average and dispersion from one cohort to another. If father's education is taken as a fixed status once the father has completed his

TABLE 5.3. SIMPLE CORRELATIONS BETWEEN STATUS VARIABLES, FOR FOUR AGE GROUPS OF MEN WITH NONFARM BACKGROUND

Age Group and Variable	Variable W	U	X	V
25 to 34 (age 16 in 1943 to 1952)				
Y: 1962 occ. status	.584	.657	.366	.350
W: Status of first job	...	.574	.380	...[a]
U: Education		...	.411	.416
X: Father's occ. status			...	.488
V: Father's education				...
35 to 44 (age 16 in 1933 to 1942)				
Y: 1962 occ. status	.492	.637	.400	.336
W: Status of first job	...	.532	.377	...[a]
U: Education		...	.440	.424
X: Father's occ. status			...	.535
45 to 54 (age 16 in 1923 to 1932)				
Y: 1962 occ. status	.514	.593	.383	.261
W: Status of first job	...	.554	.388	...[a]
U: Education		...	.428	.373
X: Father's occ. status			...	.481
55 to 64 (age 16 in 1913 to 1922)				
Y: 1962 occ. status	.513	.576	.340	.311
W: Status of first job	...	.557	.384	...[a]
U: Education		...	.392	.409
X: Father's occ. status			...	.530

[a]Not computed because requisite tabulation was not available.

schooling, then the temporal proximity of V to respondent's education (U) and first job (W) is about the same from one cohort to another.

Tentatively, therefore, we might assume that intercohort comparisons with respect to V, X, U, and W, and their interrelations, are tantamount to a historical time series, such as might have been observed had we surveyed men 25 to 34 years old not only in 1962 but also in 1952, 1942, and 1932. This assumption, of course, entails some corollary premises: most particularly, the reliability of retrospective data and the representativeness of the survivors to 1962 of the cohort membership at earlier dates. If these assumptions are accepted, we may inspect Table 5.3 in a straightforward manner for historical trends. The correlation between W and X was studied in just this way in Chapter 3.

The correlation between father's education and his occupation, r_{XV}, fluctuates between cohorts without showing a unidirectional trend. We are somewhat reluctant to give an interpretation to these fluctuations, in view of the fact that both variables place a heavy requirement on the respondent's knowledge and memory. The proportion of NA's for this combination of variables is relatively high.

The correlation of respondent's with father's education, r_{UV}, shows one cohort out of line with what is otherwise a nearly constant value. No plausible interpretation of this fluctuation comes to mind. There was an apparent, if slight, increase in r_{UX}—respondent's education with father's occupation—up to 1933 to 1942, dating the cohort by the years in which its members reached age 16. This was followed by a drop to the most recent cohort. It may be sheer coincidence that both r_{UX} and r_{UV} show the highest value for the 1933 to 1942 cohort. This cohort happens to be the one with by far the largest proportion (roughly three-quarters) of its members veterans of World War II. Sociologists have sometimes speculated that the availability of educational benefits in the "G.I. Bill" may have equalized opportunities for men coming from different socioeconomic backgrounds. The present data contain no hint of such an equalization effect, which would reduce r_{UV}, not enhance it.

We have already noted in Chapter 3 that there is hardly a trend worth discussing in r_{WX}, first job with father's occupation. Somewhat greater fluctuations, though no monotonic trend, are observed for r_{WU}, first job with education. The lowest value is for the 1933 to 1942 cohort, many of whom entered the labor market in the depression years. Perhaps the circumstances of that period made education a somewhat less important advantage than in the subsequent period of more nearly full employment.

It is difficult, in summary, to detect any bona fide trends in the correlations just reviewed. There are some intercohort fluctuations possibly too large to attribute to sampling variation alone. Attributing these to particular historical circumstances of the several cohorts involves a large element of conjecture. Indeed, despite the occurrence of some puzzling fluctuations, we get the strong impression of an essentially stable pattern of interrelationships.

When we turn to correlations involving respondent's occupational status in 1962 (Y), the interpretation of intercohort differences as a historical time series is no longer legitimate. The cohorts, observed as a cross-section of age groups in 1962, differed in length of working experience and in time elapsed since leaving their families of orientation. Effects of these differences are inextricably mixed with any differences due to the periods at which the cohorts initiated their careers.

Consider r_{YU}, the correlation of 1962 occupational status with education of respondent. There is a monotonic increase in the magnitude

of this correlation, from .576 for the oldest cohort to .657 for the youngest. This could mean either (1) that education has been becoming a more important factor in occupational achievement in recent decades, or (2) that education is most important at the stage of one's career just following the completion of schooling. Whereas it is not possible to distinguish between these two interpretations unequivocally, some data permit us to make plausible inferences in this case. The second interpretation would imply that the correlation between education and first job, r_{WU}, is larger than that between education and 1962 occupation, r_{YU}. In fact, however, r_{WU} is smaller than r_{YU} for all four age cohorts. The probable inference, therefore, is that the first of the two alternatives is the correct interpretation, though the questionable reliability of the data on first jobs would make us reluctant to rest the case on evidence provided by these data alone. But the tentative conclusion is that the influence of education on ultimate occupational achievement, though not on career beginnings, has increased in recent decades. The correlation between education and occupational status is considerably higher for respondents (r_{YU}) than for their fathers (r_{XV}) in all four age groups, and the difference between son's and father's correlation has become more pronounced for the youngest age cohort. Any one of these findings might be explained differently, but all of them together constitute fairly convincing evidence that the influence of education on careers has become more pronounced over time, the most reliable evidence in support of this contention being the difference between fathers and sons.

None of the other three correlations involving Y shows a similar monotonic relationship with age. Making use of the model developed earlier in this chapter, we examine in Table 5.4 the dependence of each of the respondent's achieved statuses on a combination of antecedent statuses. For the moment, each of the four cohorts is regarded as a distinct population, and we shall consider whether the time series interpretation of intercohort differences is informative.

The regression of respondent's education on father's education and occupation (first line in each of the four panels of Table 5.4) shows some variation over cohorts. Father's occupation appears to have the greater relative importance for the two middle cohorts, father's education for the two extreme age groups. It is difficult to suggest an interpretation for this variation, if it is, indeed, a genuine phenomenon. The combined effects of the two background variables, as registered in the coefficients of determination, are just slightly greater for the two most recent cohorts than for the two earlier ones.

In the set of regressions for first job (second line of each panel) there

TABLE 5.4. PARTIAL REGRESSION COEFFICIENTS IN STANDARD FORM (BETA COEFFICIENTS) AND COEFFICIENTS OF DETERMINATION FOR SPECIFIED COMBINATIONS OF VARIABLES, FOR FOUR AGE GROUPS OF MEN WITH NONFARM BACKGROUND

Age Group and Dependent Variable	Independent Variable: Respondent's First Job (W)	Respondent's Education (U)	Father's Occ. (X)	Father's Education (V)	Coefficient of determination (R^2)
25 TO 34 (AGE 16 IN 1943 TO 1952)					
Respondent's education (U)	...	...	.273	.283	.23
Respondent's first job (W)	...	.503	.174	...	.36
1962 occ. (Y)	.294	.462	.065	...	.50
35 TO 44 (AGE 16 IN 1933 TO 1942)					
Respondent's education (U)	...	...	.299	.264	.24
Respondent's first job (W)	...	.455	.177	...	.31
1962 occ. (Y)	.191	.485	.115	...	.45
45 TO 54 (AGE 16 IN 1923 TO 1932)					
Respondent's education (U)	...	...	.323	.218	.22
Respondent's first job (W)	...	.474	.186	...	.33
1962 occ. (Y)	.243	.410	.114	...	.41
55 TO 64 (AGE 16 IN 1913 TO 1922)					
Respondent's education (U)	...	...	.244	.280	.21
Respondent's first job (W)	...	.481	.195	...	.34
1962 occ. (Y)	.258	.399	.084	...	.39

is again fluctuation, albeit of modest magnitude, in the size of the net regression coefficients. There is no ambiguity about the relative importance of the two independent variables: education is a much more important influence on first job than is father's occupation. The only noteworthy fluctuation in the coefficients of determination is the relatively low value for the 1933 to 1942 cohort. We have already noted that this cohort may have been especially subject to depression influences. If these are indeed the relevant influences here the finding suggests that the depression lessened the significance of education and background for first jobs. With its heavy quota of World War II veterans, moreover, this cohort may have deviated more widely than others from our idealized assumption about the temporal sequence of the status variables. Despite the fluctuation noted we are inclined to emphasize the intercohort stability of the regression pattern.

With 1962 occupational status as the dependent variable (third line in each panel), we are back in the situation in which intercohort comparisons must involve an inescapable ambiguity. There is, in any case, no monotonic relationship with age for any of the three net regression coefficients. The 1933 to 1942 cohort is distinctive in that the coefficient for first job is the lowest among the four cohorts, whereas the coefficients for education and father's occupation are the highest. It seems that first jobs in the depression were out of line, but that education and social origins made up for their lesser influence on first jobs by influencing later careers more. In addition to the possibly relevant special historical circumstances of this depression cohort, there is another consideration of a different kind. At age 35 to 44 in 1962, this cohort had attained the age probably most typical of fathers of 16-year-old boys. We might suppose that at this age the effect of father's occupation (when the respondent was 16 years old) via occupational "inheritance" would be at a maximum. This interpretation gains no support from a tabulation of the proportions of men in the four cohorts having occupational status scores identical with those of their fathers: 7.3 per cent for men 25 to 34; 7.1 per cent at 35 to 44; 7.0 at 45 to 54; and 7.6 at 55 to 64. (Recall that the data in this section omit men whose fathers were in farm occupations.)

To find a striking monotonic relationship with age we need only inspect the coefficients of determination, $R^2_{Y(WUX)}$. These range from .39 for the oldest cohort to .50 for the youngest. If we were to make the time-series interpretation of the intercohort comparisons we should have to conclude that occupational achievement has been becoming much more closely dependent on antecedent statuses. At this point, however, the completely confounded factor of length of time in the working force presents itself for a rival interpretation. At age 55 to 64 the oldest men are 30 years or more removed from the experiences indexed by variables W, U, and X. Over this span of time many influences on occupational status that are unrelated to background and early experience have had a chance to operate. The youngest men, conversely, are still fairly near the time when their working life actually got under way, and the many contingencies yet to come can be expected to attenuate the initially established relationship of achievement to antecedent statuses.

The final topic of this discussion is the development of the latter interpretation, which rests on the assumption that the cohort differences in Y are due to the individual's age and not to a secular trend, an assumption that cannot be tested with our data. As a vehicle for the interpretation, we treat the observations on the four cohorts as four sets of observations on a single *synthetic* cohort. As will become evident, it is difficult to maintain this fiction with complete consistency, as demographers have found in connection with the synthetic-cohort approach to fertility analysis. Nevertheless, the artifice has considerable didactic value and, at the least, formulates hypotheses that one might hope to check later with more complete data on real cohorts.

As a first step we assume that the intercohort fluctuations in the three intercorrelations among W, U, and X are mere sampling variations. We eliminate these fluctuations by averaging the four sets of correlations. Then we assume that the correlations involving Y (1962 occupational status) represent a time series of observations on a single cohort observed at decade intervals. For notational convenience, let Y_1 stand for occupational status at age 25 to 34, Y_2 at 35 to 44, Y_3 at 45 to 54, and Y_4 at 55 to 64. The variable Y, by virtue of this mental experiment, is thus to be regarded as four different variables, depending on the age at which occupational status is measured. One further simplification is easily justified. We disregard altogether variable V (father's education) in view of the earlier evidence that it affects occupational status almost exclusively via X and U. This allows us to represent the relationship between U and X as merely a bidirectional correlation.

The model suggested for the synthetic cohort interpretation is portrayed in the form of a path diagram in Figure 5.2. This diagram suggests that each achieved occupational status is affected directly by the immediately preceding occupational status (that is, by first job in the case of the men aged 25 to 34, and by status 10 years ago for men at

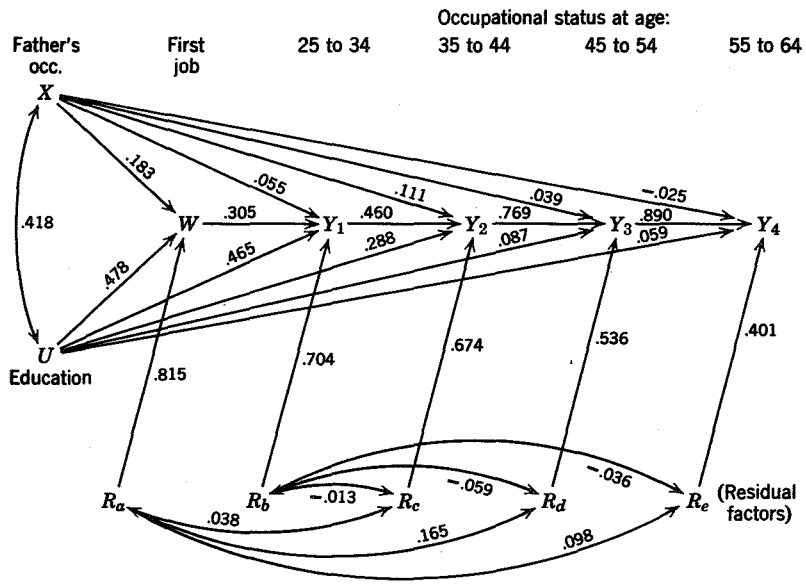

Figure 5.2. Synthetic cohort interpretation of the achievement of occupational status, for men with nonfarm background (numerical values from "Set 4," Appendix Table J5.1).

the more advanced ages). Moreover, each such status is assumed to be subject to direct influence by educational attainment and by father's occupational status.

To obtain a solution for this model we must rely on partial information. Although we have distinguished four occupational statuses subsequent to first job (Y_1, Y_2, Y_3, Y_4) we have no observations in the OCG data from which to estimate the six intercorrelations among these four variables. Nonetheless, if the model were literally correct and if we assumed no intercorrelations among residual factors, we could write just exactly the number of equations required to solve for each path in the diagram. The reason is that the unknown correlations can be expressed as a function of known correlations in the particular causal structure portrayed by this diagram. A first solution was obtained in this way (set 1, Appendix Table J5.1). Unfortunately, it turned out to be an unacceptable solution, for two of the implied values of unknown correlations were required to be above unity, which is algebraically impossible.

To overcome this difficulty, external information was brought to bear on the problem. Two studies in the literature report certain correlations that are lacking in the OCG data: present occupation

with occupation 10 years earlier. Both sets of correlations pertain to the 1940 to 1950 decade. Data for a Chicago sample[9] supply the values $r_{21} = .55$, $r_{32} = .77$, and $r_{43} = .87$. Correlations for a Minneapolis sample[10] run appreciably higher: $r_{21} = .83$, $r_{32} = .91$, $r_{43} = .96$. Discounting the likelihood of so great a difference between the two cities, there are at least two reasons why the discrepancy may have occurred. First, the measure of occupational status was not the same. The Chicago study used the same index of occupational status as that employed in the OCG research, whereas the Minneapolis investigators used an "occupational rating" that is not fully described. Second, the Chicago results derive from a detailed investigation of labor mobility in which respondents gave complete work histories for the period 1940 to 1951. The Minneapolis study apparently asked respondents only to report current occupation and occupation 10 years earlier. The approach taken in Chicago may well have elicited a more complete report of actual changes in status during the decade. The Chicago data are presumably, therefore, the more reliable as well as the more nearly comparable, in terms of the concept of occupational status, to the OCG data. Yet there is one respect in which the Minneapolis data may actually be preferable. The OCG questionnaire, like the Minneapolis interview (we assume), asked for only one antecedent occupational status: first job in the case of OCG and occupation ten years ago in the Minneapolis study. If there is a tendency for respondents to err in making retrospective information more compatible with current status than may actually have been the case, then the two studies must have shared a common source of spurious correlation.

Without offering a dogmatic resolution to this dilemma, we simply computed alternative solutions for the diagram in Figure 5.2 using the correlations for Chicago, for Minneapolis, and the average of the two sets in turn (respectively, set 3, set 4, and set 5 in Appendix Table J5.1). The last expedient, in a sense, worked best, and it is the one used in Figure 5.2. It gave results not too dissimilar from still another alternative (set 2). Here we borrowed from the Chicago data not the correlations but the path coefficients, p_{21}, p_{32}, and p_{43}, which had been obtained from a calculation with the Chicago data for a causal diagram much like Figure 5.2.[11]

[9] Otis Dudley Duncan and Robert W. Hodge, "Education and Occupational Mobility," *American Journal of Sociology*, 68(1963), 629-644. (The correlations appear on p. 641.)

[10] Godfrey Hochbaum, John G. Darley, E. D. Monachesi, and Charles Bird, "Socioeconomic Variables in a Large City," *American Journal of Sociology*, 61 (1955), 31-38. (The correlations are in Table 7.)

[11] Duncan, "Path Analysis," *loc. cit.*

All four alternatives yield results that are not only permissible algebraically but also plausible in a crude quantitative sense. All require that we acknowledge certain intercorrelations among residual factors. No substantive interpretation can be given to these correlations which, fortunately, are almost negligible in size, especially in the set shown in Figure 5.2. The presence of such correlations can suggest three conclusions: (1) The model is not entirely correct; unmeasured variables disturb the relationships portrayed in it in a systematic rather than random fashion. (2) There are real differences in the experience of the four cohorts such that the heuristic fiction of a synthetic cohort recapitulating the pattern of each does not yield a self-consistent set of assumptions. (3) There are correlated errors in the data, as suggested above in regard to the possible distortion of retrospective information.

In all likelihood there is an element of truth in each explanation. Yet we must not exaggerate the possible defects in our interpretation. The intercorrelation of residuals arises from the fact that the model omitting them does not fully account for the observed correlations of Y with W in the three older age groups. We can compute values of r_{YW} assuming the path coefficients shown in Figure 5.2 and neglecting the correlations among the residuals. Here are the computed values (with actual values in parentheses): $r_{Y_2W} = .471$ (.492); $r_{Y_3W} = .442$ (.514); $r_{Y_4W} = .481$ (.513). This is quite a close agreement. Hence the intercorrelations of residuals, though they are required for the sake of consistency, may have little substantive importance.

Despite the extended discussion of technicalities, Figure 5.2 is offered as something more than a methodological *tour de force*. It is a compact representation of our causal interpretation of a vast body of data, an interpretation contrived to take account of and thus help explain the patterns of association revealed by those data. Let us dwell, in conclusion, on some substantive implications of the results.

By showing that we can come close to forcing the data into conformity with the synthetic cohort model, we suggest strongly that there has been a quite stable—though not completely invariant—pattern of occupational status achievement in this country over the past four decades. This suggestion is at least not seriously compromised by our earlier results on trends in occupational mobility. For direct evidence one may compare the average path coefficients p_{WX} and p_{WU} in Figure 5.2 with the corresponding statistics for individual cohorts in Table 5.4. No single set of these coefficients differs from the average by more than a trivial amount.

The model suggests that factors salient at an early stage of a man's career may continue to play a *direct* role as he grows older. But the direct effects of education and father's status are attenuated drastically with the passage of time. A compensatory effect is the increasing relevance of the accumulation of occupational experience as time passes. A striking result is the diminution in importance of unspecified residual factors with aging of a cohort. This is directly opposite to the finding of higher coefficients of determination for the younger cohorts observed in Table 5.4. (The implied coefficients of determination in the model are obtained by subtracting from unity the squared values of the appropriate residual paths. Hence decreasing values of the residual path imply increasing coefficients of determination.) The explanation is, of course, that the synthetic-cohort model takes into account the occupational experience intervening between first job and a given age, allowing such experience to have a cumulative effect as the cohort grows older. The calculations for individual age groups in Table 5.4 do not take this factor of work experience into account in any direct way.

One may properly be skeptical of the precise numerical values in Figure 5.2: they are, in any case, values for an unobservable entity, the synthetic cohort. We could possibly make a case for the realism of the estimate that $p_{Y_2X} > p_{Y_1X}$ in terms of the previously noted delayed impact of background on achievement for the depression cohort, though it seems unwise to press the point. We doubt that the negative value of p_{Y_4X} corresponds to any true effect; the safe conclusion is that this path is essentially zero. There is every reason to suppose that education is, at every stage, a more important influence, both direct and indirect, on occupational achievement than father's occupation.

As a by-product of the solution, we secure values for correlations between occupational statuses held two or three decades ago. Since we know of no published values of such coefficients, there is no way to check the plausibility of these results. The solution shown in Figure 5.2 implies that $r_{Y_3Y_1} = .602$, $r_{Y_4Y_2} = .775$, and $r_{Y_4Y_1} = .565$. These correlations imply a considerable persistence of status over long intervals of time. Yet they do allow some significant amount of status mobility after age 25 to 34 or even 35 to 44, by which time the principal effects of background already have been registered. Although the literature has stressed intergenerational transmission of status and, by implication, the younger ages during which career lines are established, there is room for more careful study of intragenerational transmission from the middle to the later years of the working life cycle.

When and if complete data become available for a real cohort, we shall expect the quantitative relationships to differ somewhat

from those estimated here. In the meantime we have a description of the "typical" life cycle of a cohort that is more detailed, precise, and explicit as to causal or sequential relationships than any hitherto available.

CONJECTURES AND ANTICIPATIONS

In an earlier section of this chapter we suggested that the critic might share part of the burden of proof for the proposition that our results are distorted by the omission of important variables. There is, however, evidence at hand, supplemented by judicious conjecture, to show that at least some obvious candidates for crucial omitted variables are not as formidable as might be supposed.

One kind of question has to do with the temporal relevance of our measure of father's status. The OCG questionnaire asked for father's occupation at the time the respondent was about 16 years old. Might we not suppose that father's occupation at an earlier date would have been a better choice, on the theory that occupational ambitions are developed in late childhood and early adolescence, being more or less fixed by the time a boy reaches high school age? Moreover, if the father were mobile during the respondent's youth, the sharing of the experience of mobility may have induced distinctive orientations in the respondent.

A different issue is whether we have overlooked a crucial factor in failing to procure some information about the respondent's mother. Several sociologists have recently emphasized the mother's role in the formation of achievement orientation and have called attention to her educational attainment as an indicator of her possible influence.

We shall discuss these two possibilities together because our approach in both cases is to present hypothetical calculations based on data that are largely conjectural but include a key item of information for which reasonably firm estimates are available.

Suppose the OCG survey had ascertained not only father's occupation at respondent's age 16 (variable X) but also at respondent's age 6 (variable X'). We must make two sorts of assumption. The first assumption is that X' has the same correlation with the other variables, V, U, W, and Y, as that observed for X. There is some support for this assumption. In the son's generation, as shown by the OCG data, r_{UW} is not strikingly different from r_{UY}. This suggests that in the father's generation X and X' might have similar correlations with V. As for the father-son correlations, we assume that the earlier occupation is as highly correlated with son's educational attainment and occupational achievement as is the later occupation of the father; that

TABLE 5.5. HYPOTHETICAL REGRESSION COEFFICIENTS IN STANDARD FORM (BETA COEFFICIENTS), FOR SPECIFIED COMBINATIONS OF VARIABLES, FOR MEN WITH NONFARM BACKGROUND, BASED ON PARTLY CONJECTURAL DATA

Dependent Variable[a]	Independent Variables[a]						Coefficient of Determination (R^2)
	W	U	X'	X	V'	V	
SET 1							
U	...	...	...	.265	...	.285	.23
U	...	...	.183	.183	...	.233	.25
W	...	.450	...	.170	...	.037	.32
W	...	.434	.120	.120	...	.008	.33
Y	.279	.411	...	.103	...	-.019	.43
Y	.271	.405	.074	.074	...	-.037	.43
SET 2							
U	...	...	...	.265	...	.285	.23
U	...	...	...	.209	.196	.196	.25
W	...	.450	...	.170	...	.037	.32
W	...	.446	...	.163	.027	.027	.32
Y	.279	.411	...	.103	...	-.019	.43
Y	.279	.413	...	.107	-.014	-.014	.43

[a]V: Father's education.
V': Mother's education (conjectured).
X: Father's occ. status at respondent's age 16.
X': Father's occ. status at respondent's age 6 (conjectured).
U: Respondent's education.
W: Respondent's first job status.
Y: Respondent's occ. status in 1962.

is, that the correlations of X and X' with U, W, and Y are the same. The second assumption—and this is the crucial one—concerns the correlation of X with X'. Here we can draw on the data given earlier as well as on an OCG finding. The latter, which may be less relevant, is that for men 35 to 44 years old r_{YW} is .492. It will be recalled that there are two sources giving correlations between current occupation and occupation ten years earlier. For men 35 to 44 years old the Chicago data showed this to be .55; in the Minneapolis study it was .83. Our argument will only be weakened if we estimate $r_{XX'}$ on the low side; accordingly, we assign it the low compromise value of .60.

With these assumptions we have enough actual and hypothetical data to enter X' into a regression equation alongside X. Set 1 of Table 5.5 shows the results, in each case the previously calculated regression followed by the new hypothetical calculation in which X' is included as an independent variable. For each dependent variable the two measures of father's occupation split into equal shares the net influence formerly attributed to X alone. This particular result is without interest, as it merely reflects the assumption of equality of the respective correlations, which we assumed. The more important results—those we take to be indicative of what actual data might well

show—concern the coefficients of the other variables in the equations and the over-all change in proportion of variation determined. The most substantial change, and it is small enough, is noted with U as the dependent variable. With both occupational variables in the equation, the net influence of father's education is slightly diminished, and R^2 is two percentage points higher than with only X and V in the equation. At the other extreme, with Y as the dependent variable, we find no change in the other coefficients worth reporting and no detectable increase in R^2 due to the addition of X' to the other four variables.

Altogether, these results suggest that having much more detailed information on the father's occupational career would change very little our estimate of the relative importance of this factor as a determinant of the son's occupational achievement. The results leave open, of course, the question of the age at which the influence of father's occupation is most directly relevant to the course of the son's career, as well as the question of the particular influence a rare but extreme change in the father's career may have on that of the son.

In set 2 of Table 5.5 we have carried out the analogous exercise, considering hypothetical variable V' (mother's education) alongside measured variable V (father's education). Again we assume that their respective correlations with other variables in the system are the same. Unpublished data we have seen on educational plans and occupational aspirations of high-school youth suggest that mother's education is, at most, no more highly correlated with such variables than is father's education. Again, the crucial assumption has to do with the intercorrelation of the two key independent variables, V and V'. From the OCG data we can ascertain that there is substantial assortative mating by education in the respondent's generation. For men 45 to 54 years of age, the correlation between husband's and wife's education is .580, and for men 55 to 64 years old it is no less than .632. In 1940 Census tables on fertility we find a tabulation of education of husband by education of wife for parents of children under five years old; this correlation, computed somewhat approximately owing to broad class intervals, is .637. There should, of course, be little difference between this correlation and one computed for parents of boys 16 years old. Evidently we shall not greatly overestimate $r_{VV'}$ in setting it equal to .60.

The reader who has grasped the principle at work here will not be surprised to see in set 2 results much like those obtained in set 1. Mother's education divides with father's education the influence initially attributed to the latter, as a consequence of the assumptions

made. With U (respondent's education) as the dependent variable, inclusion of V' results in an appreciable diminution of the net influence attributed to father's occupation and a measurable increase in the proportion of variation in the dependent variable accounted for. For dependent variables W and Y, however, the additional variable contributes no additional information, since the education of neither parent has an appreciable direct effect on respondent's occupational status. It should be reiterated that these calculations do not answer the question of whether mother's or father's education exerts more influence on sons.

It is hardly conjectural to generalize from these two experiments in a certain respect. If we think of additional socioeconomic indicators applying to the respondent's family background it is fairly certain that each of them will correlate moderately highly with the two that we have measured here. We do not know for sure, but it seems rather unlikely that any of them will have a much higher simple correlation with our measures on the respondent than X or V. In this event inclusion of other family background socioeconomic variables may lead to some reinterpretation of how the effect of such variables is transmitted, or of what is their relative importance, but it will not alter greatly our over-all estimate of the importance of variables of this kind. He who thinks differently, of course, has the option of trying to support his opinion with evidence. As far as we can see there is every reason to suppose that we have not appreciably underestimated the role of the socioeconomic status of the family of orientation as an influence upon the respondent's occupational achievement.

Concerning several other omitted variables, we need not resort to conjecture but merely to anticipate a little of the content of subsequent chapters in this volume. These chapters are mainly concerned with qualitative or classificatory factors as possible influences on occupational achievement. This kind of factor is not readily introduced into the kind of causal diagram we have been working with in this chapter. We can, however, inquire whether neglect of such factors may have seriously misled us in regard to the nature of the causal relationships we have assumed. If, for example, a qualitative factor H operates as a determinant of both one (or more) of the independent variables and one (or more) of the dependent variables in our causal model, then the link between the two that we postulate is, in greater or lesser degree, spurious. In the event of this kind of spuriousness, holding the qualitative factor constant should markedly reduce, if not eliminate entirely, the apparent correlation between the two variables.

In Table 5.6 we report the amount of change in the correlation

TABLE 5.6. EXCESS OF SIMPLE CORRELATION OVER PARTIAL CORRELATION WITH DESIGNATED FACTOR HELD CONSTANT, FOR SELECTED PAIRS OF STATUS VARIABLES, BY FARM BACKGROUND

Background and Factor[a] Held Constant	Pair of Variables[b] Correlated			
	Y and X	W and X	Y and W	U and V
All men				
A	.039	.031	.026	.016
B	.029	.022	.022	.033
C	.002	.001	.002	-.001
D	.066	.071	.045	.037
E	.043	.044	.029	.056
F	.026	.019	.020	.029
G	.000	.002	-.003	.002
Nonfarm background				
A	.010	.008	.010	.007
B	.025	.017	.019	.022
C	.003	.002	.005	-.001
D	.025	.024	.025	.019
E	.034	.034	.025	.048
F	.023	.014	.017	.019
G	.001	.002	-.003	.002
Farm background				
A	...	...	.024	.003
B	...	...	.018	.061
C	...	...	.001	.003
D	...	...	.024	.002
E	...	...	.008	.026
F	...	...	.014	.044
G	...	...	.001	.001

[a]A: Size of place (community of residence in 1962).
B: Race, nativity, and migration from region of birth.
C: Presence of parents in family in which respondent grew up.
D: Geographic mobility since age 16.
E: Number of siblings and sibling position.
F: Region by color.
G: Marital status in 1962.

[b]Y: Respondent's occ. status in 1962.
W: Respondent's first job status.
U: Respondent's education.
X: Father's occ. status.
V: Father's education.

between two quantitative variables when each of seven qualitative factors is held constant. That is, we compare the simple correlation between, for example, Y and X with the average within-class correlation, holding constant, say, factor A, as derived from covariance statistics. In general, Table 5.6 suggests that any element of spuriousness in the correlations we have been using is rather minor. When there is an ap-

preciable difference between the respective simple and partial correlations, moreover, each of the correlations r_{YX}, r_{WX}, r_{YW}, and r_{UV} is affected in much the same way. Hence the *pattern* of correlations tends to remain intact. If the effects suggested by Table 5.6 are taken as evidence of spuriousness the main conclusion we should draw is that the path coefficients in our causal diagram may all be slightly overestimated, although their relative magnitudes are probably not greatly distorted.

Even this qualification is not unequivocally indicated. It is not clear that all the factors in Table 5.6 may logically be regarded as sources of spurious correlation. We do not wish to enter here upon the question of the correct causal interpretation of each of these factors, since this matter is considered in detail in subsequent chapters. One element of factor E (number of siblings and sibling position), for example, is probably best conceived as an intervening variable, accounting for part of the relationship of X and V to U. As such, its introduction into a causal scheme provides a useful extension or elaboration of the interpretation but does not require us to think of the original relationship as spurious.

We note that the discrepancies between simple and partial correlations are generally reduced when attention is focused on the nonfarm-background population. Several of the factors in Table 5.6 have to do with residence or change of residence—size of place, interregional migration, geographic mobility, and region of residence. Such factors tend to pick up the correlated effect of farm origin. When we eliminate this influence by confining the analysis to men with nonfarm background, the disturbance issuing from these factors is minimized.

We should observe, finally, that the disturbances suggested in Table 5.6 are not additive over the seven factors there listed. These factors, as defined, are in several instances logically redundant. As just noted, residential location is an aspect of four of the classifications; race or color appears in two. Hence simultaneous control of several factors would probably not produce much greater discrepancies between simple and partial correlations than appear in the table.

We must likewise be clear about what is *not* established by this analysis. First, it does not purport to estimate the effects or relative importance of the several classificatory variables; that task is reserved for subsequent chapters. It only shows that, whatever their effects, taking them into account will not require us to modify drastically our previous estimate of relationships among the quantitative variables. Second, this summary does not confront the issue of possible interaction effects. The statistic used here is the average within-class correla-

tion. If there are wide differences between classes in the magnitude of correlations like r_{YX} or r_{UV} we would, indeed, be in serious difficulty. This would mean that the causal relationships hitherto described actually differ from one subpopulation to another. (See the discussion of interaction in Chapter 4.) To anticipate the findings of later chapters, there are in fact some interactions that are sizable enough to be interesting. For most of them, however, it appears that we have not done too great violence to the data in averaging the within-class correlations. A possible exception is the factor of color. Many relationships are different among nonwhites than among whites. This important finding, which merits considerable emphasis, is dealt with at length in Chapter 6. Yet its importance should not be allowed to cloud the issue at hand—whether our analysis to this point is vitiated by the action of color as a disturbing factor. The fact is that nonwhites are a small proportion of the whole population; hence results for the total sample approximate closely results for the white subpopulation.

These observations suggest the appropriate qualifications for the analyses reported in this chapter. The findings are probably most valid for the white population, and particularly for the segment of the white population with nonfarm origins. Extended to persons of farm origin or to nonwhites, the results may require more or less drastic revision to render them applicable, in consequence of disturbances our model has not taken into account. The error to avoid, then, is that of overgeneralization. For particular subpopulations, defined in terms of variables studied here or other variables that might be suggested, our estimates of causal relationships may be more or less wide of the mark. For the bulk of the U. S. population considered in the aggregate, we have no strong evidence that they need major revision.

ISSUES POSED BY MOBILITY VARIABLES

Again, methodology rears its ugly head. We did not begin with the intention of writing a treatise on methodology. Appearances to the contrary notwithstanding, we have tried to limit the presentation of methodological problems to the very minimum necessary for the critical reader to grasp the rationale of our procedures. The truth of the matter is, however, that many an issue ordinarily considered to fall exclusively within the province of theory turns out to hinge on principles of methodology as soon as we consider how the issue could conceivably be resolved by empirical inquiry. We are, therefore, contending for a much more intimate relationship between theory and method than ordinarily has been contemplated, even by writers preoccupied with this particular interface between segments of the scien-

tific quest. Our causal diagram, for example, is not to be regarded as merely a convenient device for summarizing data, although it is at least that. It purports to be a theoretical model—even if the theory is quite tentative and rudimentary and as yet on a rather low level of generality and abstraction—about how a given process works in a particular society.[12] The stance on method taken here has other implications for theory that might go unnoticed unless made explicit. In particular, it has implications for some issues that loom large in the literature on the subject under study.

In most studies and discourses on social mobility it seems to be taken for granted that the phenomenon to be explained is, indeed, "mobility"—either actual movement between positions or intentions, aspirations, and orientations concerning mobility. We have acknowledged the significance of this interest in mobility by describing patterns of movement between occupations in Chapters 2 and 3. Once we go beyond description, however, and seek a conceptual framework with potential explanatory value, the focus on mobility—so we shall argue—becomes a liability. For this reason the present chapter, concerned as it is with the causal interpretation of relationships involved in the process of stratification, has avoided more than incidental reference to the concept of mobility. In effect, the process of stratification has been analyzed by decomposing the concept of occupational mobility into its major components.

An initial simplification will permit us to avoid some cumbersome notation. Assume that all status variables are measured in standard form, and designate such standardized variables by lower-case letters, such as $y = (Y - \bar{Y})/\sigma(Y)$. This implies that mobility has reference to a change in position in a distribution, abstracting from the mean difference between the two status variables. Thus $(y - x)$ could in some cases be negative when $(Y - X)$ is positive. But this does not affect the principles to be stated below.

Let us consider some distinct types of correlation involving mobility variables, thus defined. A Type-I correlation is a correlation between two mobility variables, involving four distinct status variables in their definition. An example is the correlation between "occupational mobility" and "educational mobility," that is, between $(y - x)$ and $(u - v)$. Without indicating the derivation of the formula, we simply state that

$$r_{(y-x)(u-v)} = \frac{r_{yu} - r_{xu} - r_{yv} + r_{xv}}{2\sqrt{1 - r_{yx}}\sqrt{1 - r_{uv}}}.$$

[12] Herbert L. Costner and Robert K. Leik, "Deductions from 'Axiomatic Theory'," *American Sociological Review*, 29(1964), 819-835.

From this mathematical identity it is immediately evident that the correlation of mobility variables is nothing more than a tautological rearrangement of the information contained in the six possible correlations of status variables. Such a tautology could, of course, be interesting insofar as it enabled the investigator to perceive a property of the system not otherwise evident to him (see the discussion in the next paragraph). For men 25 to 64 years of age having nonfarm background (taking this population for purposes of illustration), we have the following simple correlations (the correlation between two standardized variables is, of course, the same as the correlation between their raw-score forms):

$$r_{yu} = .611$$
$$r_{xu} = .414$$
$$r_{yv} = .317$$
$$r_{xv} = .505$$
$$r_{yx} = .377$$
$$r_{uv} = .418$$

Substitution in the formula yields $r_{(y-x)(u-v)} = .320$. We conclude that occupational mobility is not strongly related to educational mobility, in conformity with the conclusion reached by the author of "A Skeptical Note on the Relation of Vertical Mobility to Education,"[13] after elaborate manipulations of two-way and three-way tables, presented *in extenso*. His conclusion could have been obtained simply by observing that education and occupation are far from perfectly correlated, either within or between generations.

The finding that the correlation between occupational and educational intergenerational mobility is not very high—lower than most of the correlations between statuses that underlie it—serves to focus attention on the elements contributing to the process of mobility. To simplify the discussion let us look at upward movements from low positions of fathers to higher positions of sons; the principle illustrated here applies to other movements as well. If upward mobility would usually be due to the fact that the fathers are low on both education and occupational status and the sons are high on both, the correlation between educational and occupational mobility would be high. But the facts underlying upward mobility may well be different. Thus an uneducated father may have improved his occupational position, permitting him to provide his sons with a better education, which raises their occupational chances; this would be reflected in a low

[13] C. Arnold Anderson, "A Skeptical Note on the Relation of Vertical Mobility to Education," *American Journal of Sociology*, 66(1961).

correlation between the mobility measures. Or the sons of an uneducated father with low occupational status may themselves receive little education but nevertheless rise above their father in occupational status; this also would be reflected in a low correlation between educational and occupational mobility. These possibilities are by no means purely hypothetical, given the correlations between status variables. The finding that the correlation between educational and occupational mobility is low calls attention to the fact that the process of upward mobility does not necessarily or typically involve a jump from fathers inferior on all dimensions to sons superior on all. *Intergenerational* mobility may result from a variety of combinations of *intra*generational and intergenerational movements, and most of these combinations depress the correlation between different aspects of intergenerational mobility, such as that between educational and occupational mobility.

A Type-2 correlation likewise involves two mobility variables, but the initial status in the definition of one mobility variable is also the terminal status in the definition of the other. This arises, for example, in correlating intergenerational mobility from father's occupation to first job with intragenerational mobility from first job to subsequent occupation. The formula can again be written as an identity in terms of simple correlations among status variables:

$$r_{(y-w)(w-x)} = \frac{r_{yw} - r_{yx} + r_{wx} - 1}{2\sqrt{1 - r_{yw}}\sqrt{1 - r_{wx}}}.$$

To evaluate this correlation in the same population as used for the previous example, we need the additional simple correlations

$$r_{yw} = .529$$
$$r_{wx} = .382.$$

Before peeking at the answer, the reader might make a guess as to how it comes out. It could be reasoned that a man who demonstrated his mobility drive by achieving upward mobility from his origin level to his first job will further express that drive by strong intragenerational mobility. Conversely, a man who has already started to "skid" when he takes his first job may persist in the habit, undergoing still further downward mobility. On this argument, early mobility should be prognostic of—that is, positively correlated with—later mobility.

This fine example of deductive reasoning comes to grief when we look at the actual value of $r_{(y-w)(w-x)}$, which turns out to be $-.432$, modest enough in size but negative in sign. What went wrong? Our

point is that the intuition behind such reasoning is sound but leads to a sound conclusion only if the steps in the argument are carried through in terms of status variables, not mobility variables. We see from $r_{yw} = .529$ that a good start on the first job is indeed a favorable sign for later occupational status, in that a man initially high is likely to be high later on. When we try to express the matter in terms of mobility variables, what happens is this. The scale interval from x to y, whatever it may turn out to be, is a distance. If movement from x to w covers most of that interval there is only a short distance left to go from w to y. But if x to w covers only a little of the interval there is a long distance left to go from w to y. For this reason the lengths of the two mobility steps, x to w and w to y, *tend* to be inversely related. Once we have found that r_{yw}, r_{yx}, and r_{wx} all are positive and of a similar order of magnitude, the negative sign for the correlation between mobility variables, $r_{(y-w)(w-x)}$, is a tautological necessity, and not a very illuminating tautology at that. A Type-2 correlation, in fact, is perilously close to being simply a spurious correlation, in the classical sense of that term.

In a Type-3 correlation a mobility variable is correlated with a status variable other than one of the two whose difference is the measure of mobility. Is educational mobility affected by a person's level of origin? Let us consider $r_{(u-v)x}$. It will occasion no surprise to learn that it, too, can be written as a function of simple correlations between status variables:

$$r_{(u-v)x} = \frac{r_{ux} - r_{vx}}{\sqrt{2(1 - r_{uv})}}.$$

With data already given, we obtain —.085. But what has this told us? We could certainly have anticipated that a man's occupation will be more closely related to his own education than to the education of his son, and this information is summarized in straightforward fashion by the two coefficients r_{ux} and r_{xv}. The negative sign for $r_{(u-v)x}$ is then guaranteed. Once we reflect on the matter the more or less mechanical explanation of the negative sign is evident: the higher the father's occupational level, the higher his educational level is likely to be and hence the harder it will be for his son to exceed it. Type-3 correlations are well designed to demonstrate such truisms. Yet they do not, of themselves, give any useful indication of the interesting associations whose magnitudes cannot be foretold. The exercise of computing Type-3 correlations is harmless enough. But if we had *only* such correlations involving mobility variables our interpretation would have

to involve exceedingly devious circumlocution to avoid erroneous inferences. At the same time, such correlations would have concealed useful information.

One might be tempted, finally, to consider a Type-4 correlation, relating intergenerational mobility to the level of the origin status. The verbal rationale seems straightforward. We would like to know if "lower-class" people have the same "chance for upward mobility" as "middle-class" people. It is easily shown, however, that

$$r_{(y-x)x} = \frac{r_{yx} - 1}{\sqrt{2(1 - r_{yx})}} = -\frac{\sqrt{1 - r_{yx}}}{\sqrt{2}}.$$

Hence $r_{(y-x)x}$ is merely a simple transformation of r_{yx}. Its algebraically necessary negative sign only serves to express what is obvious from the fact that $r_{yx} < 1$; there is an inescapable "regression toward the mean."[14] Substantively, this says that the higher a man's status, the less are his son's chances of upward mobility.

We have illustrated pitfalls in the study of mobility variables as they are encountered in correlation analysis, but the same logical problems are involved even in such simple procedures as the classification of persons into categories like "upward mobile," "stable," and "downward mobile." Unless we take extraordinary precautions, using such a classification as a dependent variable incurs a serious risk of rediscovering "regression toward the mean" in a variety of disguised forms. How elaborate the precautions must be has been indicated in Chapter 4 (section entitled "Analyzing Mobility Distributions").

THE CONCEPT OF A VICIOUS CIRCLE

The problem just considered is basically one in which there is grave danger of circular reasoning. The other issue on which we have some comments concerns reasoning about circles, specifically the "vicious circle" that is sometimes identified as a crucial feature of stratification processes.

Although the concept of a "cycle of poverty" has a quasi-official sanction in U. S. public policy discussion, it is difficult to locate a systematic explication of the concept. As clear a formulation as any that may be found in academic writing is perhaps the following:[15]

> Occupational and social status are to an important extent self-perpetuating. They are associated with many factors which make it difficult for individuals

[14] Duncan and Hodge, *op. cit.*, esp. p. 639.

[15] Seymour M. Lipset and Reinhard Bendix, *Social Mobility in Industrial Society*, Berkeley: Univer. of California Press, 1959, pp. 198-199.

to modify their status. Position in the social structure is usually associated with a certain level of income, education, family structure, community reputation, and so forth. These become part of a vicious circle in which each factor acts on the other in such a way as to preserve the social structure in its present form, as well as the individual family's position in that structure. . . . The cumulation of disadvantages (or of advantages) affects the individual's entry into the labor market as well as his later opportunities for social mobility.

The suspicion arises that the authors in preparing this summary statement were partly captured by their own rhetoric. Only a few pages earlier they had observed that the "widespread variation of educational attainment within classes suggests that one's family background plays an enabling and motivating rather than a determining role."[16] But is an "enabling and motivating role" logically adequate to the function of maintaining a "vicious circle"? In focusing closely on the precise wording of the earlier quotation we are not interested in splitting hairs or in generating a polemic. It merely serves as a convenient point of departure for raising the questions of what is specifically meant by "vicious circle," what are the operational criteria for this concept, and what are the limits of its usefulness.

To begin with, there is the question of fact—or, rather, of how the quantitative facts are to be evaluated. How "difficult" is it, in actuality, "for individuals to modify their status" (presumably reference is to the status of the family of orientation)? We have found that the father-son correlation for occupational status is of the order of .4. (Assuming attenuation by errors of measurement, this should perhaps be revised slightly upward.) Approaching the measurement problem in an entirely different way, we find that the amount of intergenerational mobility between census major occupation groups is no less than seven-eighths as much as would occur if there were no statistical association between the two statuses whatsoever, or five-sixths as much as the difference between the "minimum" mobility involved in the intergenerational shift in occupation distributions and the amount required for "perfect" mobility.[17] Evidently a very considerable amount of "status modification" or occupational mobility does occur. (There is nothing in the data exhibited by Lipset and Bendix to indicate the contrary.) If the existing amount of modification of status is insufficient in terms of some functional or normative criterion implicitly employed,

[16] *Ibid.*, p. 190.

[17] U. S. Bureau of the Census, "Lifetime Occupational Mobility of Adult Males: March 1962," *Current Population Reports*, Series P-23, No. 11 (May 12, 1964), Table B.

the precise criterion should be made explicit: *How much mobility must occur to contradict the diagnosis of a "vicious circle"?*

Next, take the postulate that occupational status (of origin) is "associated with many factors" and that "each factor acts on the other" so as "to preserve . . . the individual family's position." Here the exposition virtually cries out for an explicit *quantitative* causal model; if not one of the type set forth in the first section of this chapter, then some other model that also takes into account the way in which several variables combine their effects. Taking our own earlier model, for want of a better alternative, as representative of the situation, what do we learn about the "associated factors"? Family "position" is, indeed, "associated with . . . education," and education in turn makes a sizable difference in early and subsequent occupational achievement. Yet of the total or gross effect of education (U) on Y, occupational status in 1962 ($r_{YU} = .596$), only a minor part consists in a transmission of the prior influence of "family position," at least as this is indicated by measured variables V (father's education) and X (father's occupation)—and this statement requires little modification on behalf of our conjectured variables V' (mother's education) and X' (father's earlier occupation). A relevant calculation concerns the compound paths through V and X linking Y to U. Using data for men 20 to 64 years old with nonfarm background, we find:

$$\begin{aligned} p_{YX}p_{UX} &= .025 \\ p_{YX}r_{XV}p_{UV} &= .014 \\ p_{YX}p_{WX}p_{UX} &= .014 \\ p_{YW}p_{WX}r_{XV}p_{UV} &= .008 \\ \hline \text{Sum} &= .061 \end{aligned}$$

This is the *entire* part of the effect of education that has to do with "perpetuating" the "family's position." By contrast, the direct effect is $p_{YU} = .407$ and the effect via W (exclusive of prior influence of father's education and occupation on respondent's first job) is $p_{YW}p_{WU} = .128$, for a total of .535. Far from serving in the main as a factor perpetuating initial status, education operates *primarily* to induce variation in occupational status that is independent of initial status. The simple reason is that the large residual factor for U is an indirect cause of Y. But by definition it is quite uncorrelated with X and V. This is not to gainsay the equally cogent point that the degree of "perpetuation" (as measured by r_{YX}) that does occur is mediated in large part by education.

This conclusion is so important that we should not allow it to rest

on a single calculation. The reader accustomed to a calculus of "explained variation" may prefer the following. For men 35 to 44 years of age with nonfarm background (a convenient and not unrepresentative illustration), we have these pertinent results: $r_{YX} = .400$; $R_{Y(XV)} = .425$; $R_{Y(UXV)} = .651$. Note that adding the "associated factor" of father's education to father's occupation increases very slightly our estimate of the influence of "family position" on occupational achievement. Including respondent's education, however, makes quite a striking difference. Squaring these coefficients to yield an accounting of the total variation in respondent's 1962 occupational status (Y), we obtain these percentages:

(*i*) Gross (or total) effect of father's education and occupation	18.06
(*ii*) Education of respondent, independent of (*i*)	24.32
(*iii*) All other factors, independent of (*i*) and (*ii*)	57.62
Total	100.00

An analogous calculation, derived from multiple-classification rather than linear-regression statistics, was offered in Chapter 4. The results are rather similar. Here we have imputed to the measures of "family position," X and V, their *total* influence, including such part of this as works through education; the 24 per cent contribution of respondent's education refers only to the part of the effect of education that is net of the background factors. Still, education has a greater influence, *independent of these factors,* than they have themselves, operating both directly and indirectly. Overshadowing both these components, of course, is the unexplained variation of nearly 58 per cent, which can have nothing to do with "perpetuating status."

Whatever the merit of these observations, they should at least make clear that statistical results do not speak for themselves. Rather, the findings of a statistical analysis must be controlled by an interpretation—one that specifies the form the analysis will take—and be supplemented by further interpretations that (ideally) make explicit the assumptions on which the analyst is proceeding. The form in which our results are presented is dictated by a conception of status achievement as a temporal process in which later statuses depend, in part, on earlier statuses, intervening achievements, and other contingent factors. In such a framework it may not be a meaningful task to evaluate the relative importance of different causal factors. Instead, attention is focused on how the causes combine to produce the end result. From this point of view we can indicate, first, the gross effect of the measured background factors or origin statuses of a cohort of men on their adult

achievement. We can then show how and to what extent this effect is transmitted via measured intervening variables and, finally, to what extent such intervening variables contribute to the outcome, independently of their role in transmission of prior statuses. In a balanced interpretation all these questions should be dealt with explicitly.

Our treatment seems to indicate the advisability of keeping in perspective the magnitude of the gross relationship of background factors and status of origin to subsequent achievement. The relationship is not trivial, nor is it, on the other hand, great enough in itself to justify the conception of a system that insures the "inheritance of poverty" or otherwise renders wholly ineffectual the operation of institutions supposedly based on universalistic principles.

Our model also indicates where the "vicious circle" interpretation is vulnerable. In the passage on the vicious circle quoted there seems to be an assumption that because of the substantial intercorrelations between a number of background factors, each of which has a significant relationship to subsequent achievement, the total effect of origin on achievement is materially enhanced. Here, in other words, the concept of "cumulation" appears to refer to the intercorrelations of a collection of independent variables. But the effect of such intercorrelations is quite opposite to what the writers appear to suppose. They are not alone in arguing from a fallacious assumption that was caustically analyzed by Karl Pearson half a century ago.[18] The crucial point is that if the several determinants are indeed substantially intercorrelated with each other, then their combined effect will consist largely in redundancy, not in "cumulation." This circumstance does not relieve us from the necessity of trying to understand better *how* the effects come about (a point also illustrated in a less fortunate way in Pearson's work). It does imply that a refined estimate of how much effect results from a combination of "associated factors" will not differ greatly from a fairly crude estimate based on the two or three most important ones. Sociologists have too long followed the mirage of "increasing the explained variance."

Let us not fall into the trap of supposing that, had we measured more of the "real" background factors, the outcome would have been greatly different. (Had it occurred to the reader, perchance, that background determines the kind of marriage contracted and the latter then plays a crucial role in the subsequent career? Then let him consult Chapter 10, wherein we evaluate the importance of "making a good match.") Either the "real" factors would be associated with the

[18] Karl Pearson, "On Certain Errors with Regard to Multiple Correlation Occasionally Made by Those Who Have Not Adequately Studied This Subject," *Biometrika,* 10(1914), 181-187.

measured ones, or they would not. If the former, they would add little to the "explained variation"—as we illustrated, quite cogently though conjecturally, with two "omitted variables." If, on the other hand, the "real" factors are not associated with our measures of "family position," then they would operate independently thereof and *not* to "perpetuate" family position.

We do not wish to imply that the idea of cumulation of influences, or even the particular form of cumulation describable as a "vicious circle," is without merit. Our aim is to call attention to the necessity of specifying the actual mechanism that is only vaguely suggested by such terms. One legitimate meaning of cumulation is illustrated by the model of a synthetic cohort presented earlier in this chapter. In this case what is cumulative is the experience of an individual or a cohort of individuals over the life cycle, so that in the latter part of the life cycle achieved status depends heavily on prior achievements, whatever the factors determining those achievements may have been. The cumulation here consists in large measure of the effects of contingent factors not related to social origins or measured background factors.

The situation of the Negro American, which is analyzed in Chapter 6, exemplifies mechanisms inviting the label of a vicious circle. What is crucial in this case is not merely that Negroes begin life at a disadvantage and that this initial disadvantage, transmitted by intervening conditions, has adverse effects on later careers. Rather, what happens is that, in addition to the initial handicap, the Negro experiences further handicaps at each stage of the life cycle. When Negroes and whites are equated with respect to socioeconomic circumstances of origin and rearing, Negroes secure inferior education. But if we allow for this educational disadvantage as well as the disadvantage of low social origins, Negroes find their way into first jobs of lower status than whites. Again, allowing for the handicap of inferior career beginnings, the handicap of lower education, and the residual effect of low socioeconomic origins—even with all these allowances—Negroes do not enjoy comparable occupational success in adulthood. Indeed, even though we have not carried our own analysis this far, there is good evidence that Negroes and whites do not have equal incomes even after making allowance for the occupational status difference and the educational handicap of Negroes.[19] Thus there surely are disadvantaged minorities in the United States who suffer from a "vicious circle" that is produced by discrimination. But not all background factors that create occupational handicaps are necessarily indicative

[19] See Herman P. Miller, *Rich Man, Poor Man,* New York: Crowell, 1964, pp. 90-96.

of such a vicious circle of *cumulative* disadvantages; the handicaps of the Southern whites, for example, are not cumulative in the same sense, as Chapter 6 will reveal. A vicious circle of cumulative impediments is a distinctive phenomenon that should not be confused with any and all forms of differential occupational achievement.

As noted earlier, the issue of equalitarianism is one that has generally been more productive of debate than of cogent reasoning from systematized experience. Without becoming fully involved in such a debate here, we must at least attempt to avoid having our position misunderstood. We have *not* vouchsafed a "functional interpretation" that asserts that somehow American society has just the right amount of stratification and just the appropriate degree of intergenerational status transmission. We *have* indicated that it is easy to exaggerate the latter and, in particular, that it is possible seriously to misconstrue the nature of the causal relationships in the process that characterizes status transmission between generations.

In conclusion, one question of policy may be briefly mentioned, which pertains to the distinction between the plight of the minorities who do suffer disadvantages due to their ascribed status and the influence of ascribed factors on occupational life in general. To help such minorities to break out of the vicious circle resulting from discrimination and poverty is a challenge a democratic society must face, in our opinion. To advocate this policy, however, is not the same as claiming that *all* ascriptive constraints on opportunities and achievements could or should be eliminated. To eliminate all *dis*advantages that flow from membership in a family of orientation—with its particular structure of interpersonal relationships, socioeconomic level, community and regional location, and so on—would by the same token entail eliminating any *advantages* the family can confer or provide. If parents, having achieved a desirable status, can *ipso facto* do nothing to make comparable achievement easier for their offspring, we may have "equal opportunity." But we will no longer have a family system—at least not in the present understanding of the term. (This point has not been misunderstood in radical, particularly Marxist, ideologies.)

We do not contemplate an effortless equilibrium at some optimum condition where the claims of egalitarian values and the forces of family attachment are neatly balanced to the satisfaction of all. A continuing tension between these ultimately incompatible tendencies may, indeed, be a requisite for social progress. We do contend that both equity and effectiveness in the policy realm call for a deeper understanding of the process of stratification than social science and politics yet can claim.