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PEER INFLUENCE VS. PEER SELECTION: AN ATTEMPTED SEPARATION

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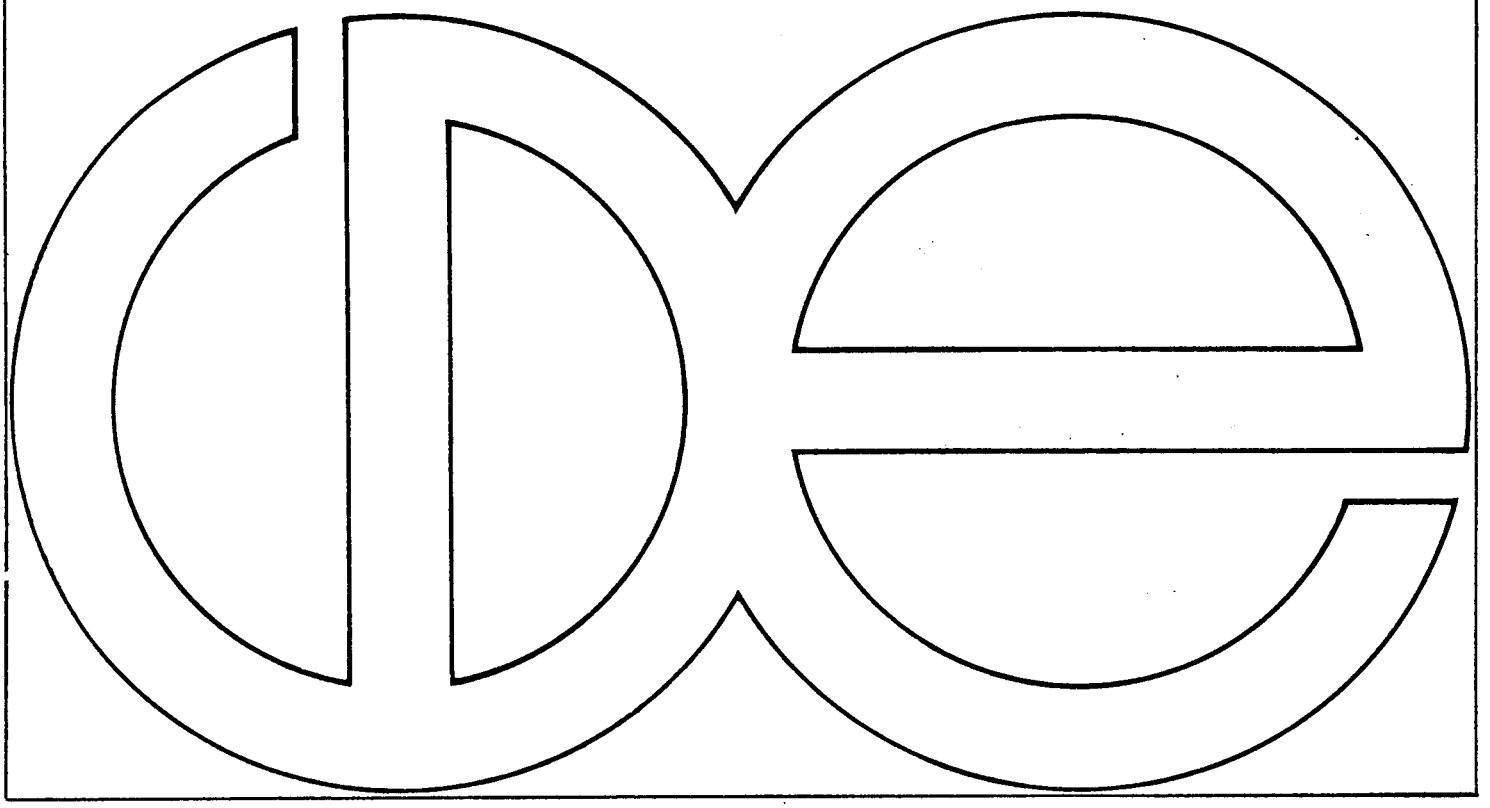

PEER INFLUENCE VS. PEER SELECTION:

AN ATTEMPTED SEPARATION

BY

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ABSTRACT

A number of studies have attempted to measure the extent to which peers influence the status attainment of an individual. Previous research has been flawed, however, by the failure to separate peer influence from peer selection. These studies have assumed that the similarity between peers resulted from their friendship, when in fact these similarities may be what caused the individuals to become peers in the first place. Using the Sewell-Hauser Wisconsin data, the author replicates work done by Alexander and Campbell and Duncan, Haller, and Portes, and provides extensive controls for peer selection. The author concludes that peer influence has, in fact, been generally overestimated, and that some studies which attempted to control for peer selection did not in fact do so. However, although results are not clearcut, the bulk of the evidence seems to suggest that peer influence does have a significant effect on the individual.

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INTRODUCTION

Studies on the characteristics and importance of significant others have long been a major part of Sociology. Recently, those interested in social stratification have attempted to expand the study of peer influence into the field of status attainment. The status attainment processes involve those sets of events by which individuals come to occupy their positions in the social hierarchies of wealth, power, and prestige (Haller and Portes, 1973). Besides clarifying how those processes work, some researchers have expressed the hope that an understanding of significant others will suggest ways by which attainment behavior might be altered (Sewell, Haller, and Portes, 1969). For instance, the negative impact that low parental status has on childrens' educational and occupational attainment might be overcome if counterbalancing influences could be created in schools and peer groups (Haller and Portes, 1973).

There are many significant others in an individual's life, and among the most prominent are the individual's friends. As we will see, numerous studies, using radically different methodologies and data, have claimed to show that peer influence is a major factor in the status attainment

process. There has been growing controversy, however, as to just how valid some of that research has been. The critics contend that, rather than measuring peer influence, these studies have really only measured peer selection. Individuals are not similar because of their friendship; rather, because they are similar, they are friends. What has been labeled as peer influence, then, is really just peer selection; actual influence is either exaggerated or nonexistant.

This thesis, then, will attempt to do three things. First, the literature relevant to the peer influence/ peer selection controversy will be discussed. Second, we will explain how one of the major data bases in the status attainment field has been modified to deal with this controversy. Finally, we will attempt to develop models which actually separate peer influence from peer selection, and provide estimates of the relative importance of each.

PEER SELECTION AND PEER INFLUENCE: A REVIEW

Before we can adequately examine peer influence, we must first have some idea as to what causes individuals to become friends in the first place. Not surprisingly, there are several different theories as to how the friendship

formation process works. Two of the more interesting have been dealt with at length in an article by Hallinan (1976a). One viewpoint holds that people find each other mutually attractive and simultaneously develop positive feelings towards each other. An alternative theory contends that friendship is a status based, initiation-response phenomenon; one individual makes an offer of friendship to another individual who may or may not respond.

To test these theories, Hallinan used a sample of elementary and junior high school students who were asked several times a year to name their best friends. She created matrices of all dyadic relationships. Dyads were classified as being in one of three states: (1) null (meaning no friendship existed) (2) asymmetric (meaning one individual named the other as a friend, but the choice was not reciprocated), and (3) mutual (both members named each other as friends). The change in dyads over time was of primary interest. Hallinan reasoned that, if simultaneous mutual attraction best described the friendship process, most shifts would be from the null to the mutual state, or vice-versa. Alternatively, the initiation-response theory would be supported if most of the shifts were to or from

the asymmetric state.

Analysis of the data revealed that shifts to the asymmetric state were by far the most likely. In addition, asymmetric dyads were much less stable than mutual dyads. This evidence, she concluded, strongly implied that persons would extend an offer of friendship, and then wait for that friendship to be reciprocated. When it was not reciprocated, the dyad would frequently shift into the null state. Thus, initiation-response appeared to be the more plausible of the two theories.

While Hallinan does not discuss at length what implications these theories might have for peer influence, a few inferences might be drawn. To the extent that mutual attraction characterizes the friendship process, estimates of influence will probably be too high. Initiation-response, however, might imply that friends do indeed influence each other, as one individual tries to convince another to be friends. While further research would be needed, Hallinan's findings seem to provide some support for the importance of friends in status attainment.

A second area of interest concerning friendship formation is the mediating impact of schools. Much of the research on friendship in schools has noted a positive skew in the distribution of choices. Students have been classified as stars, above average, below average, neglectees, or isolates (Coleman, 1961; Hallinan, 1976b). This research has been criticized, however, on the grounds that it ignores the structural characteristics of the classroom and does not recognize the fact that friendship formation is a dynamic process (Hallinan, 1976b). One characteristic frequently cited is the openness of the classroom. In open classrooms, students tend to have more opportunity to interact, and work on self-selected activities. Traditional classrooms tend to be more teacher oriented with all students working on the same task (Hallinan and Tuma, 1978).

Working with a longitudinal sample of over 4000 grade and high school students, Epstein (1978) has illustrated how important these structural characteristics can be. More students in open than traditional schools tend to be selected as friends. More friendships are reciprocated in open schools. In traditional schools, a more limited pool of students are singled out as sociometric stars.

Stability of choices seems to vary little between the two types of structures, however.

Hallinan (1976b) finds remarkably similar results, except that her data indicate that friendships tend to last longer in the more open schools. She also notes that, surprisingly, children in traditional classrooms tend to have more friends, thus casting some doubt as to which approach is superior for friendship facilitation. Also, in work with Tuma (1978) she notes that task homogeneity tends to foster friendship (because it provides common bases for discussions and interests) and that traditional schools are superior in this respect.

The implications of such findings for peer influence are somewhat ambiguous (Epstein, in fact, found little difference between open and traditional schools when it came to influence). However, a more detailed analysis might be able to distinguish whether the causes of similarity differed between the two settings, and might therefore be of interest.

One last point on friendship formation, which also has clear relevance for peer influence, concerns group cohesiveness. Cohen (1977) has attempted to analyze the

process by which groups become homogenous (i.e., similar in their beliefs and attitudes). He suggests three possibilities: (1) groups exert pressures towards conformity, (2) deviates are selectively eliminated from the group, and (3) homophilic selection causes the group to overchoose as cliquemates others who are similar to themselves.

Cohen uses the Newlawn High data from The Adolescent Society study (Coleman, 1961) to examine each of these. The results of his analysis indicate that conformity effects, though measurable, are not large and are confined to a minority of items. Group leaving by deviates has virtually no effect on group homogeneity, while homophilic selection falls inbetween the other two. His most crucial finding, however, is that homophilic selection that occurred when the group was formed is the most crucial factor in group homogeneity. Cohen feels that such results cast severe doubts on the importance of peer influence.

Now that we have a clearer idea of how the friendship formation process works, we can now look more directly at peer influence and its relationship to status attainment. Much of the present research on status attainment has been heavily influenced by the work of Blau and Duncan (1967;

also see Haller and Portes, 1973). Using a 1962 nationwide sample of the adult male population as their data base, Blau and Duncan tried to develop a causal theory of status attainment (or, perhaps more appropriately, status transmission). They focused upon the extent to which inherited status determines the social fate of individuals, and the extent to which earlier positions in status hierarchies affect later levels of attainment. Figure 1 illustrates their model. According to the model, father's education directly affects son's education, and father's occupation directly affects son's education and son's first job. The primary influence of father's characteristics on son's current job is indirect via their impact on son's educational level, however. Education affects both early and late occupational attainment, and first job has a significant impact on current job.

While Blau and Duncan's work was highly praised, it was also felt that the failure to include social-psychological variables in the model was a severe weakness. Sewell, Haller, and Portes (1969) noted that a number of studies had suggested the importance of such variables as reference groups, significant others, self concept, behavior expectations, and levels of educational

and occupational aspiration. They also felt that the Blau and Duncan model failed to indicate why any connection at all would be expected between the input variables (father's education and father's occupation) and the three subsequent factors (son's education, son's first job, and son's 1962 job). As noted earlier, they thought a good social-psychological model might suggest ways by which attainment behavior could be altered.

As a result of these concerns, the Wisconsin Model of Educational and Early Occupational Attainment was developed, and this model is presented in figure 2 (Sewell, Haller, and Portes, 1969; and Sewell, Haller, and Ohlendorf, 1970). The data set used consisted of a one-third sample of 1957 Wisconsin high school seniors. In 1957, respondents completed a questionnaire which included such information as parental status, residence, post high school plans, plans of friends, etc. In 1964, a followup questionnaire sent to the respondents' parents determined educational and occupational attainments since leaving high school (another, more elaborate followup was also done in 1975). Socioeconomic status and significant other influence were actually measured by taking weighted linear composites; in addition, it should be noted that

significant others' influence is based upon the respondents' perceptions of teacher encouragement, parental encouragement, and friends' plans regarding college.

The model has a number of interesting implications, not all of which need be discussed here. Basically, the model provides a causal argument linking social origins and ability with educational and early occupational status attainments by means of intervening behavioral mechanisms (Sewell, Haller, and Ohlendorf, 1970). Perhaps its most crucial finding is that virtually all the effect that family SES has on a child's educational and occupational attainment is due to its impact on the influences of significant others (Haller and Portes, 1973); the effects of social background on later attainment are largely explained by social-psychological experiences during the high school years (Sewell and Hauser, 1972). The data seem to offer little support for the argument that the achievement process is dominated by any single aspect of social inequality (Hauser, 1972).

While illustrating the impact of significant others and other social-psychological factors on the process of status attainment, the model also provided strong support for the original work of Blau and Duncan. Haller and

Portes (1973) have noted the remarkable comparability in the two studies in the relationships between similar variables. They note that the main contribution of the Wisconsin model is not that it challenges the conclusions of Blau and Duncan, but rather that it clarifies their outline of the processes through which the influence of earlier status variables affects later ones.

The Wisconsin model has itself been clarified over time. Hauser (1972) has demonstrated that many interesting relationships have been needlessly lost by relying on composite measures of socio-economic status and significant others' influence. By disaggregating these variables, a number of insights have been gained. Among the more relevant of these for this paper is the fact that parental encouragement and friends' plans depend heavily on the son's SES origin, while teacher's encouragement is more heavily dependent upon the student's academic ability and performance (Sewell and Hauser, 1972). Unfortunately, while teacher's encouragement is more egalitarian, it also has less impact than the other two factors.

Another major model, which deals even more directly with the question of peer influence, has been developed by Duncan, Haller, and Portes (1968). Their data comes from a sample of all seventeen year old boys in Lenawee County, Michigan, during the spring of 1957. For 329 of these boys, data were included in the same sample for at least one person listed as a best friend. The authors use five variables in their model, which is presented in figure 3. Level of occupational aspiration is the score on Haller and Miller's occupational scale. Level of educational aspiration is a composite score based on several questions about educational plans. Socioeconomic status (SES) is measured by Sewell's socioeconomic scale. Intelligence refers to scores on Cattell's test of G Culture Free. Parental aspirations are indexed by a composite score from the answers to questions asked about the degree to which parents "encouraged" the respondent to have high levels of achievement. Using these variables, the authors argue that educational and occupational aspirations are actually indicators of an unobserved variable which they label as "ambition". They justify this by arguing that many boys do not make a neat conceptual separation of educational and occupational aspirations, nor do they make plans for schooling and job choices in any fixed order. The model

further argues that the correlation between a boy's educational and occupational aspiration is entirely accounted for by his "ambition". Most crucially for this paper, the model contends that, while much of the similarity between aspirations and peers is due to the way in which peers come to associate (assortatively with respect to background variables), a significant part of the association is due to mutual influence, as is indicated by the reciprocal paths between the two ambition variables.

McDill and Coleman (1965) provide another interesting example. Students in six schools who were freshmen at the time of The Adolescent Society study (Coleman, 1961) were reinterviewed their senior year. A measure of high school status was created for each individual, based on the number of times he/she was mentioned by others as being in the leading crowd; this was interpreted as being a measure of peer influence. Parental background was operationalized by dichotomizing father's education, and parental aspirations for their children was determined from a freshmen year questionnaire that asked parents if they wanted their child to go to college.

While these operationalizations of concepts are somewhat different from those used in the Wisconsin model, the results are remarkably similar. In the freshman year, parental aspirations were more than twice as important as family background and high school status in determining college aspirations. By the end of the senior year, however, high school status was only slightly less important than parents' aspirations, while the impact of parental background remained about the same. The Adolescent Society had argued that the elites of a school accentuated the dominant characteristics of the student bodies they represent. Such results strongly suggest the type of relationship hypothesized in the Wisconsin model; significant others are influenced by SES and ability (characteristics which also reflect the dominant attributes of the student body) which in turn affect the aspirations of the individual.

While the above studies would seem to provide a compelling argument for the importance of peer influence, they have not gone unchallenged. Critics contend that it is incorrect to conclude that similarities between friends are proof of peer influence. For instance, if aspiration levels are alike at the time of friendship choice,

similarity between friends cannot be attributed solely to peer influence (Cohen, 1977). Neighborhood boundaries, grouping or tracking procedures, student demographic characteristics, etc., may force the student to select friends similar to himself (Epstein, 1978). Even when individuals are not constrained to pick similar friends, balance theory would lead us to expect them to choose as friends people with whom they already agree (Alexander and Campbell, 1964). While attempts might be made to control for the factors which affect friendship selection, they are limited by the fact that the behavior being measured may itself be a basis for friendship (Epstein, 1978). (In other words, when examining college aspirations, you might try controlling for SES, neighborhood, etc.; but similarity of college aspirations may be what caused the individuals to become friends in the first place). As a result of such problems, "we are unable to estimate either the extent to which an individual selects as friends those who are similar to him or the extent to which friends, once selected, influence him to become more similar to them..." (Alexander and Campbell, 1964). Based on such arguments and the results of his own analysis, Cohen (1977) feels that peer influence has been consistently overestimated in the status attainment literature, since in each case the

joint effects of peer influence and homophilic selection have been taken to be the effect of peer influence alone.

While these concerns are valid, some authors have contended that certain research designs can provide a means of getting around this seemingly hopeless problem. Longitudinal analysis especially seems to offer hope. For example, Alexander and Cambpell (1964) conducted a panel study of 1410 male seniors in North Carolina. Controlling for status level, they found that an individual is more likely to expect to attend college, to have a strong desire to go when he expects to go, to want to go when he does not expect to, and to actually attend, when his best friend does rather than does not plan to go to college. Even more importantly, when both the individual and his peer plan to go to college, the individual is more likely to attend if his friend does. The authors therefore concluded that the individuals were influencing each other, and not merely choosing each other as friends because of their similarities.

In the same research cited earlier, Epstein (1978) has attempted to provide a strong argument for peer influence. On each of several measures (among them college aspirations and high school grades) students were divided into four

groups, labeled as LL, LH, HL, and HH. The first letter represented the student's initial score on a measure (low or high) while the second letter represented the average of the initial scores of the student's friends. When reinterviews were conducted one year later, it was discovered that, on most measures, students who were initially low or high with high-scoring friends had significantly higher scores on the same outcomes one year later. More specifically, students initially in LH groups had significantly higher scores than students initially in LL groups, while students initially in HH groups also had significantly higher scores than students in the HL groups. Based on such results, she makes the important observation that selection and influence do not have to be viewed as competing explanations for peer similarity. Friends selected for one reason are not similar on all attitudes and behaviors. Peers probably influence each other by supporting and maintaining some behaviors and attitudes, while modifying and changing others.

A few general comments on the above research are now in order. First, it is clear that the educational and occupational aspirations of peers are strongly associated, and a strong case can be made that at least part of this

association is due to peer influence. Nevertheless, it still seems reasonable to assume that estimates of peer influence will tend to be overly high, since selection factors and influence factors are not totally separated.

Second, most of the studies cited above have information from the peer himself, and this has obvious benefits for measuring peer influence. The Wisconsin data has been an exception; the only measure of peer influence used to date has been the respondent's self-report of what he perceived his friends' college plans to be.

Third, longitudinal data, while not perfect, appear to offer one of the strongest means by which peer influence can be evaluated. One of the primary limitations of the Wisconsin data has been that, while the study is longitudinal, the data on peers is not; the only measure of peer influence comes from the 1957 data, or from retrospective reports of what the friends' 1957 plans were. As was noted earlier, comparisons of such things as aspiration levels and actual outcomes (i.e., college attendance) can provide means of assessing how much peer influence is actually taking place.

Fourth, even without longitudinal data or data directly from peers, estimates of peer influence can still be improved by controlling for other variables which affect the status attainment of an individual and the way in which he selects his friends. For instance, few of the studies cited above controlled for such factors as the high school rank of the individual, the type of academic program he was in (i.e., college prep vs. noncollege prep), or for influence from teachers. Not only do such variables have clear relevance for the status attainment process, they also have relevance for the peer selection process as well. For instance, it may be that high-ranking students, or students who are in the same academic program, choose each other as friends. If we do not control for this fact, we may erroneously attribute an association between friends to peer influence when it is actually just peer selection. Indeed, in the Alexander and Campbell study, which seemed to provide strong support for the existence of peer influence, only socio-economic status was controlled for, leaving open the possibility that other factors which affected peer selection and college attendance were actually at work. Thus, models which do include these and other relevant variables should be able to minimize the extent to which we erroneously label peer selection as peer

influence.

Finally, the most difficult problem in measuring peer influence is the fact that the behavior being measured may itself be the basis for friendship. For instance, as noted before, similarity of college aspirations may itself be what caused the individuals to become friends. We should keep this point in perspective, however. Taken to its logical extreme, this argument suggests that high school students belong to such organizations as "Future college students of America", and that is where they meet all their friends. Such a possibility seems a bit far-fetched. As noted above, however, it is not so unreasonable to believe that students of similar academic ability or who are in the same high school program may come to choose each other as friends. Thus, if we provide a more extensive set of controls for factors which affect both status attainment and peer selection, and still find that the association between friends remains significant, it seems hard to believe that this association is due simply to unmeasured selection factors. Nevertheless, the theoretical possibility is always there, and in our later analysis we will attempt to provide controls for this type of selection.

In the above analysis, we have given several reasons for the inability of the Wisconsin data to accurately measure peer influence and its impact on the status attainment process. In the next section, we will discuss ways in which the Wisconsin data might be used more effectively.

THE DATA

Of the 10,317 people in the original Wisconsin sample, over 9000 were reinterviewed by telephone in 1975. At that time, each respondent was asked to name three best same-sex friends from his/her high school who also graduated in 1957. Respondents were told to select those individuals who were their best friends in high school, not the ones they see now. There is approximately a one-third chance that any peer named will also be a member of the sample. About 6275 respondents could be matched up with at least one of their peers, either because they named a friend who was in the sample, or a friend who was in the sample named them, or both (i.e., each named the other - a reciprocated friendship).

At this point, a number of comments on the data base are in order. First, the retrospective determination of friendship eighteen years later obviously leaves much to be desired. Respondents might tend to name people they kept in contact with, rather than the people who were their strongest friends in 1957. For example, they might name people they went to college with. This might lead to peer influence being overestimated. Conversely, they might not remember who their strongest friends were and then name people they were not really that close to; in that case, peer influence might be underestimated. It seems reasonable to assume, however, that respondents would not name peers who were total strangers to them in 1957, and we can hope that any biases would tend to offset each other. A related problem is the fact that, when respondents were asked about their friends' plans in 1957, they may not have had in mind the particular friend(s) we were able to match them up with. For instance, most of a respondent's friends may have been planning to go to college, but the particular friend who happened to be in the sample may have been planning to join the marines. The author would feel far more comfortable with the results that follow if (1) respondents were asked in 1957 to name their friends, and (2) they were then asked questions about each specific

friend (i.e., is this friend planning to go to college?).

A second thing to note is that there is no requirement in the question that friends be named in any particular order with regard to strength of relationship. However, Epstein (1978), using a similar question, has found that individuals do seem to name friends in order, and there is some indication that this may also be true in the Wisconsin data. A more subtle problem in the friendship measure lies in its fixed-choice character (i.e., the requirement that three friends be named). Holland and Leinhardt (1975) have suggested a number of problems in such measures. An individual may want to make more than three choices, but is unable to do so. Alternatively, he may want to name less, but goes ahead and names three anyway. These problems might help account for the fact that 72% of the friendship choices are not reciprocated. Some individuals might be naming borderline acquaintances who do not reciprocate the choice. Others may want to reciprocate, but are prevented from doing so because they have named three friends already. Of course, anyone who was not contacted in the 1975 followup cannot reciprocate at all. More important, of course, are the implications that the inclusion of unimportant relationships and the exclusion or mislabeling

of important ones can have for the validity of the results.

Third, there are a wide number of alternatives by which individuals can be linked with data from their peers. Some studies have chosen to include data from only one of the respondent's peers (Alexander and Campbell, 1964; Rhodes, Reiss, and Duncan, 1965). Others have computed average scores from all peers named (Epstein, 1978; Woelfel and Haller, 1971; Haller and Woelfel, 1972). Still others have chosen to examine multiple friendship dyads, pairing each individual with each of his friends (Haller and Butterworth, 1960; Duncan, Haller and Portes, 1968; Hallinan, 1976a). Much work has been devoted to studying more elaborate friendship structures, such as triads (Hallinan, 1976b) and cliques (Coleman, 1961; Cohen, 1977). Another seemingly reasonable approach (although no study has been found yet which uses it) would be to include in a single record data from all peers named without averaging; thus, you would have variables like SES1, SES2, SES3, etc. Implicit in such a construction would be the assumption that the ordering of friends was not just arbitrary and that the order the friend was named in was important in determining the impact that friend would have.

While each of these approaches has merits, unique characteristics of the Wisconsin data make some alternatives more desirable than others. Most of the studies cited above included (or tried to include) an entire population of students in their data base. In other cases, friends named by a respondent were themselves located and interviewed. As a result, the coverage of peers named has been generally very high.

In the Wisconsin data, however, the inclusion of information on a peer is strictly a matter of chance; about one-third of the peers named will be in the sample, but the other two-thirds will not. In addition, the peers included will be randomly distributed between the first, second, and third choices of the respondents. This makes the last alternative cited above (including data from all peers in a single record) extremely unattractive, since about one-third of the sample would have data on SES1, another third on SES2, another third on SES3, but only a very few would have data on all three. Also, examination of complicated friendship structures, such as cliques, could be extremely misleading. For instance, it is not at all unreasonable to think that, with a sample of this size, a very popular person would have only one of his peers

included, while a less popular person would have complete coverage of his peers. Averaging could get around the missing data problem somewhat, but could not be used for non-interval level variables that may be of interest (such as occupation, religion, etc.).

Creating records which consist of friendship dyads seems to be the strongest course to follow, then. Even here, however, we have a great deal of flexibility over which dyads to include. For instance, we could only use dyads in which friendship choices were reciprocated. Or, when a respondent was matched up with more than one peer, we could include only the dyad involving his first or highest choice. Or, we could include all the dyads in which a respondent named a peer, while excluding dyads in which a peer named a respondent and the respondent did not reciprocate. Any approach which does not use all friendship dyads, however, requires that we develop some criteria for selection, and this criteria will not necessarily be valid. As noted before, we do not know if the order in which friends are named has any importance. Also, 72% of the friendship choices are not reciprocated; it seems difficult to believe that mutual friendship bonds existed in only about a fourth of the dyads. Therefore,

while we may be reasonably certain that some sort of friendship bond exists between two persons, we must be careful about making any sorts of assumptions about the strength of their friendship. The most appropriate course, then, seems to be to include all dyads in the analysis, at least initially. If we wish, we can then check to see whether any of the factors mentioned above (reciprocated-nonreciprocated, order named, etc.) are important.

By including all friendship dyads in this manner, 9638 records can be created. There are several interesting points about this approach that should be kept in mind. First, data from the same individual can appear multiple times on the left (ego) and right (peer) hand sides of the data records. Second, because each peer is also a respondent, what is "peer" in one record is "respondent" in another. In other words, if there is a record in which John Doe is Jim Doe's friend, there will be another "mirror-image" record in which Jim Doe is John Doe's friend. Thus, the number of unique dyads is only 4819. One implication of this is that correlations between comparable variables will be identical (for example, the correlation between peer's SES and ego's occupational

aspiration will be the same as the correlation between ego's SES and peer's occupational aspiration). Likewise, a crosstabulation of comparable variables will produce a symmetric table (i.e., the 1-2 cell will have the same count as the 2-1 cell, etc.). While such symmetries are true by construction, they are also fairly reasonable (for example, why should a peer's SES affect the peer differently than the respondent's SES affects the respondent?). These symmetries are less reasonable if one believes that strength of friendship is important and can be accurately measured with these data. For instance, if respondent names peer and peer does not reciprocate, and reciprocation is a meaningful indicator of strength of friendship, then we might reasonably expect the peer to have more effect on the respondent than the respondent has on the peer.

One last point to keep in mind about the data concerns the matter of weighting and sample size. Since each record has a mirror image, the author has deemed it reasonable to weight each case as a half-record. It could be argued, however, that it would be more appropriate to weight each record according to the number of times that an individual is matched with a peer. For instance, if an individual is

matched with three friends, each of his records would be weighted as one-third. This approach would be valid if one viewed the individual as the unit of analysis; however, since the friendship dyad is the unit of analysis, the approach chosen seems more appropriate to the author.

The last problem to consider, and the one that is by far the most difficult to deal with, is that we cannot be sure that we are actually dealing with a random sample of friendship dyads. We know that we have a random sample of individuals, but this does not guarantee that each friendship dyad has an equal chance for inclusion within the sample. Points raised earlier about reciprocation apply here. There also appears to be some sort of sex bias in the data; females tended to name more friends than males did, and they also tended to provide better quality information (in other words, more of the friends they named could be found and their id numbers coded). Any non-randomness, of course, raises severe questions about results, and we can only hope that these problems are not too great. Incidentally, this same concern could be raised about a great deal of the research in this area, but it has been generally ignored.

Keeping in mind the limitations mentioned above, this reconstruction of the Wisconsin data does offer us two major qualities we have been seeking. First, we now have data directly from peers themselves; we do not have to rely on just the respondent's report of his peers' plans. Second, the data on peers is longitudinal. We can therefore look at such factors as actual college attendance, and see whether or not peer influence seems to be operating. With those thoughts in mind, we can now turn to the analysis section of this paper.

THE ANALYSIS

Two statistical approaches will be presented in this paper. The first technique, standardization of compositions, is relatively simple, and has several advantages. First, results are quite easy to interpret. Second, it is easy to draw comparisons with some of the research cited earlier, such as the Alexander and Campbell study and Epstein's work. The main drawback of this approach is that it does not enable us to easily develop multivariate models of peer influence. The second approach, which involves structural equation models with latent variables, enables us to examine more complicated relationships and develop more precise estimates of peer

influence.

THE STANDARDIZATION APPROACH

In the standardization approach, respondents are divided into two groups; those whose friends would presumably be influencing them to go to college, and those whose friends would presumably influence them not to go to college. After comparing the college attendance rates of the two groups, we then see what the attendance rates would be if the second group (those whose friends influence them not to go to college) had the same demographic composition as the first group, while still maintaining their own level-specific rates of attendance. By so doing, effects of demographic differences are controlled, and any remaining differences between the two groups can be attributed to peer influence, or to some other unmeasured variables. Using Dickinson's (1973) method of decomposition, it is also possible to determine how much of the difference between groups is due to differences in demographic composition, how much is due to differences in the level-specific college attendance rates of the groups, and how much is due to the interaction of rates and composition. Because of missing data, only 8590 of the 9638 friendship dyads are used in this analysis.

Let us turn now to specifics. In Table 1, perceived college plans of friends as reported by the respondent in 1957 is used for grouping. Respondents who said most of their friends were planning to go to college are in the first group, while respondents who said most of their friends were not planning to go to college are in the second group. As the table shows, about 66% of the people in the first group ended up attending college themselves, as opposed to only 21.3% of the people in the second group.

It seems reasonable to assume, however, that much of this difference is probably due to differences in the SES composition of the two groups. Those at higher SES levels will be more likely to have friends who are planning to attend college, and will also be more likely to attend college themselves. Sex composition might also be a factor; respondents were limited to naming same-sex friends, and females were less likely than males to attend college. Therefore, it seemed reasonable to standardize for the SES and sex composition of the two groups. A factor-created combination of several variables is used to measure the SES of the respondent's 1957 family (this is the same measure used in the early Wisconsin research). The original scale, which runs from 1 to 99, was collapsed

into six intervals. The first five intervals are each ten units long, while the last interval (because of the declining number of cases) includes all respondents who had a score of 51 or higher on the scale. A computer program developed by the author determined what the college attendance rate was for each level of SES and sex, and then determined what the college attendance rate would be for individuals whose friends did not plan to go to college if they had the same SES and sex composition of the other group while maintaining their own level-specific rates. As Table 1 shows, the college attendance rate jumps from 21.3% to 30.7%, but is still 35.2% lower than the attendance rate of individuals whose friends planned to go to college. As a sidelight, it is interesting to note that standardizing for SES and sex did little more than standardizing for SES alone; sex apparently is not as important as intuition might lead one to expect. Nor was sex particularly important in any of the other standardizations about to be reported.

The next two columns of Table 1 give us some idea of how important the strength of friendship is. Presumably, when friendship choices are reciprocated, the association between friend's plans and the actual actions of the

respondent will be stronger than when the friendship choice is not reciprocated. There is some very weak evidence that this is in fact the case. For reciprocated friendships, respondents whose friends plan to go to college are slightly more likely to attend than when the friendship choice is not reciprocated (66.3% vs. 65.8%). Likewise, when friends do not plan to go to college, those with nonreciprocated friendships are more likely to attend college than those with reciprocated friendships (21.9% vs. 19.9%). In both cases, standardization reduces but does not eliminate the differences between groups. All of these differences are quite small, however, and we should not attach too much importance to them.

While these first three columns have clearly shown that there is an association between friend's plans and actual college attendance over and above that which can be accounted for by SES and sex composition, they have not been able to show that this association is due to peer influence rather than peer selection; individuals could be choosing each other as friends because of the similarity of their college aspirations. Columns four and five of table 1 give us strong reason to believe that this is not the case. In column four, the sample is limited to those

respondents who planned to go to college (according to their 1957 self-report) while column five gives the results when the sample is limited to those who did not plan to go to college. As the table shows, when respondents plan to go to college but their friends do not plan to go, their attendance rate is 15% lower than when both respondent and friend are planning to go to college. Even after standardization, the gap is still 12.6%. Conversely, when respondents do not plan to go to college but their friends do, their attendance rate is 12.9% higher than when both the respondent and friends do not plan to go; after standardization, the gap is still 12.5%. Thus, regardless of what a respondent's college attendance plans are, he is about twelve and a half percent more likely to attend college if his friends are planning to do so. This similarity in the value of having college-oriented friends is even more impressive when one considers that people who plan to go to college are almost seven times as likely to actually attend than those who do not plan to go (85.4% vs. 12.9% - these figures are not reported in the table).

Still, there are at least two problems with the above analysis. First, it is possible that individuals who are on the borderline about going to college, but lean slightly

against, choose as friends individuals who are also on the borderline, but who lean slightly towards college. If they both independently make the same decision, it will appear that one has influenced the other, when no such influence has taken place. Thus, the apparent association between plans of friends and actual college attendance of the respondent may be due entirely to peer selection and not peer influence. This seems unlikely, but it is not impossible, and if selection factors are present at all the amount of peer influence will be overestimated.

The second problem with the above analysis is that it does not take full advantage of the way in which the data have been reconstructed. Perceived plans of friends has always been available for use, as have the other variables mentioned. We will now see how this modification of the data base makes the analysis of peer influence clearer.

As was noted earlier, Alexander and Campbell (1964) found that whether or not an individual's friend attended college could be a useful indicator of peer influence. In table 2, then, we divide respondents into two groups; those whose friends actually went to college, and those whose friends did not go to college. Again, SES is standardized for, but since sex had so little impact in

Table 1 it is dropped from further study. Instead, a much more powerful variable is standardized for; namely the perceived plans of friends. Recall that the strongest argument against peer influence was that individuals may choose friends because they perceive them as being similar to themselves with regards to their aspirations and plans. If, in fact, similarity in aspirations is a basis for friendship rather than a result of it, we control for that selection by letting both groups have the same composition on this variable. Rather than overestimating peer influence, we will probably underestimate it, since some of the association between peers which is actually due to peer influence will be attributed to peer selection instead. In other words, this standardization assumes that individuals have a perception of the college plans of some other person, and choose that person as a friend because of this perception. Furthermore, the actual attendance or nonattendance of a friend cannot be a basis of peer selection, since individuals do not know in 1957 what their friend will actually do (of course, they may know by 1975, which is when they actually named their 1957 friends, which again leads us back to our hope that the retrospective naming of friends does not affect the validity of the results).

Let us put the argument another way. By controlling for perceived plans of friends, we assume that all similarity in college aspirations that existed in Spring of 1957 was due to selection factors, and not peer influence. If, after having made their selection, friends do not influence each other, then whether or not one member of the dyad attends college should have absolutely no effect on the other member of the dyad. However, it may be that, even if similarity of college aspirations is what caused the individuals to become friends, they may still have influenced each other afterwards. For instance, they might have mutually reinforced each other, therefore increasing the probability that each would stick with his original decision. Or, they might have chosen each other as friends because they had different aspirations (i.e., an "opposites attract" form of friendship) and then one convinced the other to change his mind. Or, one half of the dyad could change his mind, and, as a result, the other half changed their mind as well. If any of these things happened, then, even after controlling for SES and peer selection, differences between the groups would remain. Thus, by controlling for perceived plans of friends, we are theoretically controlling for all similarity between friends that is due to selection on the basis of the

behavior being measured (i.e., similarity of college aspirations), plus an unknown amount of similarity that is actually due to peer influence. Whatever association remains, then, is presumably due to peer influence (or other unmeasured variables), and can be viewed as being a lower bound for peer influence (since we have erroneously attributed some peer influence to peer selection).

As Table 2 shows, respondents are much more likely to attend college when their friend does rather than does not go (60.5% vs. 25.8%). Even after standardizing for SES and perceived plans of friends, over half the difference between the groups remains. Columns two and three again seem to show that strength of friendship (as measured by reciprocation) is slightly important in determining how great the relationship is between friends, although again the results are hardly devastating.

Columns four and five provide the strongest evidence for peer influence. Even after standardization, individuals who plan to go to college are 9.4% less likely to do so if their friend does not go to college. Likewise after standardization, individuals who do not plan to attend college are 9.8% more likely to wind up going to college if their friend does.

Based on this analysis, it would seem that peer influence does have a major impact on the status attainment of an individual, even after rigid controls are applied for peer selection. At the same time, this influence is probably not as great as previous research has indicated, because this research has not attempted to control for peer selection.

Nevertheless, we must keep in mind that only a few variables have been taken into account. Like Alexander and Campbell, we have not considered such factors as high school rank, high school program, parental influence, etc. In the next section, we will attempt to develop more complete models of the peer influence, peer selection, and status attainment processes.

MODELING PEER INFLUENCE: THE STRUCTURAL EQUATIONS APPROACH

Earlier in this paper, we discussed the Duncan, Haller, and Portes model of peer influence, and the Wisconsin model of status attainment. In this section, we will try to combine features of both of these models, and again control for peer selection. In order to make results as comparable as possible with these other studies, we will limit the sample to 1555 male friendship dyads who had complete data on all variables of interest. Tables 3A and 3B give the means and standard deviations and the correlation matrix for the variables used in this part of the analysis.

In Table 4A, model I, we present the Wisconsin replication of the Duncan, Haller, and Portes model. When comparing these and other results with figure 3, a few key differences should be kept in mind.

First, operationalization of concepts differs between the two data bases. For instance, in the Wisconsin data, parental influence and educational aspirations are measured as dichotomies (go to college versus not go) while Duncan used composite scores based on a series of questions. The Wisconsin data uses Duncan SEI scores to measure

occupational aspirations, while Duncan uses the score on Haller and Miller's occupational scale. Duncan measures SES by using Sewell's socioeconomic scale, while Sewell's Wisconsin data uses a factor created scale based on several variables. Finally, different types of IQ tests are used in the two studies.

Second, there are some slight differences in the structure of the two models. The Wisconsin data uses an additional indicator of ambition, namely, the 1975 retrospective report of 1957 educational plans. Deletion of this variable produces only minor differences in the rest of the model. Besides improving our measurement of this latent variable, having an additional indicator lets us allow for correlated measurement error between the 1957 reports of educational and occupational aspirations; this use of within-occasion correlated error will be expanded upon later. This is the only correlated measurement error we allow for, however (the other possible correlated measurement errors, i.e., between friends, are statistically insignificant when added to the model, and this was also true in the original Duncan model; Duncan may have included these paths simply because, in 1968, he did not know how to exclude them). Also, the paths from

respondent's SES to friend's ambition, and from friend's SES to respondent's ambition, have been deleted because they were statistically insignificant (these paths were also just barely significant in the original Duncan model).

Third, as with the other analysis that has been done, the Wisconsin model is perfectly symmetric, while the Duncan model did have a few asymmetries. For this reason, in this and following tables we will present only the top halves of our models.

Finally, in tables 4 through 7, in addition to presenting the structural equation models we will also present the correlations between our latent variables and their indicators. While we will not go into detail on any of these subtables, we will provide a general discussion or the use of our latent variables later on.

Given these differences, it is not surprising that the two models produce somewhat different path coefficients; the most noteworthy difference is that the parental influence coefficient is twice as big in the Wisconsin replication. However, the paths indicating peer influence are almost identical in the two models. Also, while the Wisconsin replication does not fit the data well, it should

be kept in mind that there are almost five times as many cases in the Wisconsin than the Duncan data. The author feels that it is more appropriate, when evaluating this and other models, to look at how well the model is able to reproduce the original correlation matrix, than to just look at statistical fit. For this model, none of the reproduced correlations differs from the original by more than .05. Furthermore, all of the paths that are in the model are statistically significant.

In model II of Table 4A, we add three additional variables to the model; High school program (college prep versus non-college prep), a standardized measure of high school rank, and a measure of teacher influence (did the teacher encourage the respondent to go to college, or not?). All three measures are from the 1957 data. All three of these paths have a significant effect on ambition, and their inclusion also causes the coefficients of the other exogenous variables to change. The parental influence path is reduced by about 25% (from .42 to .31), the IQ path drops dramatically (from .25 to .07), while SES stays about the same (about .16). Most importantly, the paths indicating peer influence change from .21 to .16, but remain statistically significant.

A few comments are in order. First, the large drop in the IQ coefficient is not particularly surprising, given that IQ is strongly associated with the added variables. Furthermore, as we saw earlier, Sewell's research has shown that IQ tends to affect aspiration levels indirectly, by influencing significant others and high school performance. This expanded version of the Duncan, Haller, and Portes model ignores these complexities, a problem we will attempt to remedy later. Second, the inclusion of other variables reduces the apparent impact of peer influence, but it in no way eliminates it. Finally, while the model again does not fit, it does do a good job of reproducing the correlation matrix, with discrepancies again being no bigger than about .05.

Up to this point, however, we have still not controlled for the possibility that individuals may be choosing each other as friends because of the perceived similarity of their ambition level. As we did in the standardization section of this paper, we again try to control for that selection by adding perceived plans of friends to the model. If selection on the basis of ambition is all that is taking place, then inclusion of this variable should theoretically cause the reciprocal

paths between respondents and friends ambition levels to shrink to zero. As model III of Table 4A indicates, however, this is clearly not the case. Adding perceived friends' plans as an exogenous variable does cause the peer influence coefficient to decline (from .157 to .112) but it is still statistically significant. Interestingly, this new value for peer influence is about half as large as it was in the original replication, before we added other exogenous variables and controlled for selection, suggesting the convenient possibility that previous research has overestimated peer influence by a factor of two (although again, we may be overcontrolling by saying that all of the association between perceived friends' plans and ambition levels is due to selection factors).

There are, however, at least two arguments that suggest that the model does not really control for peer selection. First, only perceived educational plans of friends is controlled for. Ambition, however, consists of both educational and occupational aspirations. It might be that, if we had a measure of the perceived occupational aspirations of friends, all of the effect that we are attributing to peer influence would disappear.

Unfortunately, no such measure exists. It is possible, however, to simply delete occupational aspirations from the model; ambition is then measured solely by the educational aspirations of the respondent. As Table 5A shows, when this is done, the model remains basically the same, and the peer influence effects are still significant. Therefore, this argument does not seem to present a major problem.

The second argument, and the more difficult to deal with, concerns the temporal ordering of the variables. Before, when we controlled for perceived plans of friends, our dependent variable was college attendance. By controlling for perceived plans of friends, we were able to attribute any similarity between friends in the Spring of 1957 to selection factors, and any additional similarity in college attendance had to be due to peer influence (or other unincluded variables). Thus, we were able to claim that we had controlled for 100% of selection, plus some unknown amount of influence which we were attributing to selection. In the present model, however, we do not have that neat temporal ordering of events. Perceived plans of friends, actual plans of friends, respondent's own plans, etc., are all measured at the same time; we cannot be

sure, then, that controlling for perceived plans actually provides a complete control for selection. Things are further complicated by the fact that, while we are providing a partial control for selection, we are also providing a partial control for influence. In other words, by controlling for perceived plans, we might be controlling for 80% of the similarity between friends that was due to selection, and 50% of the similarity due to peer influence; or, the figures may be radically different. Thus, in our earlier analysis, we at least knew we had a lower bound for peer influence; but in the present analysis, our estimates may be either too high or too low. In actuality, we would be quite surprised to find that our estimates of peer influence were too high, since it still seems reasonable to believe that, overall, controlling for perceived plans results in giving selection factors too much credit for similarities which are due in part to peer influence. Nevertheless, the possibility that peer influence is overestimated cannot be ruled out entirely, and the argument for peer influence will be strengthened if some sort of influence after Spring of 1957 can be demonstrated.

In order to try and deal with this problem, we need to temporally extend our model by including college attendance as another endogenous variable. If we then find that either college attendance of peer or peer's ambition have a significant effect on the college attendance of the respondent, even after controlling for perceived plans of friends, we will have reasonably clearcut proof of peer influence.

However, that proof is not forthcoming from the data. As Table 6A indicates, even without controlling for perceived plans of friends, none of the paths which would indicate peer influence after Spring of 1957 are statistically significant. This does not, of course, mean that peer influence never took place. It does challenge our earlier conclusion, however, that influence took place after the Spring of 1957. It may be that the association in college attendance rates of friends that existed even after controls for SES and selection was due to other variables which were not controlled. Indeed, it should be kept in mind that other longitudinal studies, such as Alexander and Campbell's, provided very few controls for other variables that might affect peer similarity. The fact that they found an added association between the

college attendance rates of friends may be due to their failure to control for these other relevant variables. In our own analysis, we have found that simply including such variables as teacher influence, high school program, and high school rank substantially reduces the apparent peer influence effect. Again, this does not mean that friends did not influence each other, but it does suggest that most of that influence took place prior to their college attendance decision.

At this point, a few additional comments are in order. First, critics may contend that the temporal ordering problem, along with the failure to find an association between college attendance rates of friends, means that the analysis so far has failed to provide proof of peer influence. Several points should be kept in mind, however. First, we have included a number of relevant variables, ignored by other studies, and we have still found that the paths we are labeling as peer influence remain significant. As noted before, these variables also provide controls for selection factors that other studies have not. Even without using the perceived plans of friends variable, then, we would still be providing a strong control for selection.

Second, it must nevertheless be conceded that the earlier analysis using standardization, the similar work done by Alexander and Campbell, and possibly other longitudinal studies as well, may be flawed by their failure to take into account enough of the relevant variables which affect the status attainment and peer selection processes. Table 6A clearly seems to suggest that whether or not a peer goes to college, and the level of peer's ambition, do not directly affect an individual's own college attendance, once other variables are taken into account. However, while it cannot be proven beyond a shadow of a doubt, the evidence so far does seem to indicate that peer influence does occur while the students are still in high school, and that that influence is perhaps about half as large as other studies have suggested.

All of the modeling done so far, however, can be improved upon in at least three ways. First, with the exception of ambition, only single indicators have been used for each of the variables. Obviously, such complicated factors as parental influence and teacher encouragement cannot be completely captured by a single measure. If we had other reliable indicators of these

variables, the precision of our estimates would be increased.

Second, for the most part we have ignored the possibility of correlated error in our measurements (the exception being the correlation between educational and occupational plans in our ambition measure). Since our measures of parental influence, teacher encouragement, and friends' plans are all based on reports from the respondent in 1957, it is not unreasonable to believe that our errors in measuring these variables will be correlated. Again, if we can devise a means to allow for this correlated error, the quality of our estimates should be improved.

Finally, we have used a rather simple causal structuring in the development of our models, and we have ignored the complexity of the relationships that exist between our exogenous variables. The Wisconsin model, which did postulate what some of these relationships should be, can serve as a guide towards developing a more meaningful model.

In a forthcoming work, Hauser and Tsai (1981) suggest means by which the first two problems can be overcome. In the 1975 Wisconsin followup, respondents were asked

questions about their parents, teachers, and friends. Since these are retrospective measures, there is naturally some doubt about how valid it is to use these questions as indicators of 1957 significant other influence. Hauser and Tsai demonstrate, however, that creating latent variables involving the 1975 retrospective measures with the original 1957 questions results in improved measurement for each of the three variables. Furthermore, when multiple indicators are used, it is possible to allow for within-occasion correlated error. In other words, we can allow all the errors of the 1957 measures to be correlated with each other, and let all of the 1975 measures be correlated as well. In addition to the reasons given above, this allowance for within-occasion correlated error is justified on the grounds that responses given at one point in time tend to be more highly correlated with themselves than responses given at another point in time. Since we only have one measure of IQ, SES, high school rank, and high school program, we cannot allow for correlated error with these variables. Fortunately, these variables do not involve as much subjective answering by the respondent, and the quality of their measurement is probably already quite high. Also, with one exception, we do not allow for correlated error between friends. Since the two

individuals provide their responses independently of each other (especially in 1975, where there is not even the possibility that they could be in the same classroom) most allowances for correlated error would have little theoretical justification. After all, respondents do not have the same parents. They may have the same teachers, but test runs not reported show that the error correlations here are not significant. The respondent and his peer do have some of the same friends, however, so it does seem reasonable to allow for correlated error in the perceived plans of friends.

Because of these findings of Hauser and Tsai, we will assume that it is proper and valid to use the 1975 retrospective measures of significant other influence to improve our measurement of these concepts. The reader should keep in mind, however, that the present study is based on a different construction of the Wisconsin data, and that Hauser and Tsai based their findings on models that more closely paralleled the original Wisconsin model. It is possible, then, that their results are not valid for the peer data, although it is difficult to think of a good reason this would be the case.

What we now wish to do, then, is to reestimate the last two models we presented in Table 4A, this time using multiple indicators of the significant other variables and allowing for correlated error. The results are presented in Table 7A. We will first look at model I, which does not include the perceived plans of friends variables but which does have all the other variables we have added to the Duncan model.

As model I indicates, these revisions in the model result in major changes in the parameter estimates. The effects of IQ, SES, high school program, and high school rank are all cut by about 50 to 75% from what they were in model II of Table 4A; in fact, only high school rank remains statistically significant at the .05 level. The effect of teachers, however, increases by about 45%, while parental influence jumps by about 76%. Finally, the peer influence path, which had been .1573, declines to .1093, but remains statistically significant.

The fact that the multiple indicator variables all increase in importance, while the single indicator variables all decline, does make one wonder whether or not this use of the retrospective measures is actually valid. It should be kept in mind, however, that these results are

quite consistent with the original Wisconsin model, which argued that background variables like SES and IQ did not have significant direct effects on the educational and occupational aspirations of an individual. Furthermore, as was argued above, the single-indicator variables are already measured fairly well. Since the other variables are not measured as well, it should not be surprising to find that their influence gets underestimated when we only use a single indicator of them, while the effects of the other variables get overestimated. In other words, one might contend that the inclusion of the retrospective measures has distorted the results, perhaps because people have altered their perceptions to make them conform more closely to what actually happened. However, the results we have gotten are consistent with what we would expect with any type of multiple indicator. For instance, if we had had another measure of parental influence from the 1957 questionnaire and we used it to construct our latent variable, we would not be surprised to find that the effect of parental influence on ambition had increased (since our new measure was more precise, and also because of the theory developed in the Wisconsin model). While this paper does not wish to go into an elaborate defense of the retrospective measures, it should be noted that the changes

they cause are not that surprising and that, in fact, those changes are consistent with sociological theory.

In any event, even if one still has qualms about the variables used, the key point is that peer influence remains significant even after providing these more stringent controls.

In model II of Table 7A, however, we have quite another story. When a latent variable for perceived plans of friends is added to the model (to control for selection, as we have done before) there are small declines in most of the paths, but the path for peer influence drops to almost zero, and is no longer statistically significant. Thus, if we really believe this model, we can finally claim to have disproven peer influence.

Before making such a conclusion, however, we should keep a few points in mind. Model I already provided some very strong controls for selection, and it showed that the peer influence effect was still significant. Furthermore, we have always known that controlling for perceived plans of friends could only provide us with a lower bound for peer influence, since this approach probably overcontrols for selection. Of course, being able to conclude that peer

influence may or may not exist is not particularly informative, and, from an esthetic standpoint, we would have preferred to have our results come out in a more consistent manner.

Overall, though, the bulk of the evidence does seem to suggest that peer influence does exist, and that it is about half as large as other studies have indicated. Because we have not been able to come up with a measure that controls only for selection and not influence, we have not been able to rule out the possibility that peer influence is nonexistant, but we have been able to suggest that that is unlikely.

Even though it is not essential to our estimation of peer influence, the theoretical usefulness of our last two models would be enhanced if we could develop the causal ordering among our exogenous variables. Fortunately, the insights we have gained from the Wisconsin model make this rather easy to do. In the models presented in Tables 8 and 9, we postulate that IQ and SES are exogenous variables which affect high school rank and high school program. All four of these are then allowed to have direct effects on the significant other variables of Parents, Teachers, and (in Table 9) perceived plans of friends. All of these

variables are then allowed to have direct effects on ambition, and, as before, the two ambition variables have direct effects on each other. Before proceeding, a few methodological points are in order.

First, the fact that the fit reported in Table 8 is the same as that for model I, Table 7A, and the fit in Table 9 is identical to model II of Table 7A, and the coefficients for the direct paths to ambition are also the same, is not simply an odd coincidence. In Table 7A we allowed the exogenous variables to freely intercorrelate with each other. In the models presented in Tables 8 and 9, whenever we do not have a direct path between two variables, we allow their equation errors to be correlated, thereby producing the same fit and parameter estimates as before. In effect, these correlated errors between equations represent unanalyzed association, just as they did in the original models.

Second, as the above result would indicate, any rearranging of the exogenous variables would produce an identical fit, so long as we also included all of the correlated equation errors. It is only the theoretical rationale provided by the Wisconsin model which makes this structure more valid than any other; it is not a matter of

having empirically "proven" that this structure is more correct.

Finally, we do not really believe that all of the direct effects that are allowed for will be statistically significant; for instance, we already know that the direct effects from IQ, SES, and high school program to Ambition are insignificant. We do little harm, however, by estimating these paths, and it does make our theory construction simpler.

An examination of Tables 8 and 9 yields several interesting insights. IQ has much larger effects on the academic program and rank of an individual than SES does. This may reflect the fact that students in the same school tend to be more homogeneous with respect to SES than to IQ. SES has a slightly larger effect than IQ on teacher encouragement, while type of program has a somewhat larger effect. By far the most crucial factor affecting teacher encouragement, however, is the high school rank of the individual. This suggests that teachers are much more influenced by the achievements of the individual than they are by his background. For parental encouragement, however, just the opposite is true. Here the most crucial variable is SES, followed by high school program, with high

school rank and IQ of somewhat less importance. With perceived plans of friends, we see basically a similar pattern. SES clearly has the largest impact, followed by high school program, rank, and IQ. Finally, as we have already seen, IQ, SES, high school program, and (in Table 9) Peer's ambition do not have signficant effects on respondent ambition. Parental encouragement has by far the largest impact on ambition, followed by perceived plans of friends and teacher encouragement.

Thus, we see basically the same patterns that other researchers with the Wisconsin data have found. The effects of background variables like SES and IQ on ambition levels are largely mediated by intervening social-psychological variables. While parental encouragement and friends plans depend heavily upon the student's SES origins, teacher's encouragement is more heavily dependent upon the student's actual academic performance. However, while teacher's encouragement is thus more egalitarian, it also has less impact than the other two.

SUMMARY, CONCLUSIONS, AND RECOMMENDATIONS

What, then, are the main contributions that this study has made?

First, we have successfully replicated the work of Duncan, Haller, and Portes. Although many of our parameter estimates were quite different (due in part to different operationalizations of concepts) the overall structure of relationships was quite similar, and the estimates of peer influence were virtually identical. We were further able to extend their model to include other variables relevant to the status attainment process. Finally, we found that the Duncan, Haller and Portes model was quite compatible with the Wisconsin model; indeed, in the last section we were able to virtually synthesize the two (although we did not look at later events in the life cycle the way the Wisconsin model does).

Second, we were able to provide the strongest controls yet for peer selection. We found that, even when variables which affect both status attainment and peer selection (such as high school program, high school rank, and teacher encouragement) were included, the peer influence effect remained significant. The bulk of our evidence seemed to

suggest that the peer influence effect was perhaps about half as large as Duncan had originally estimated.

Third, we were also able to point out the flaws in other studies which had claimed to find proof of peer influence. In particular, Alexander and Campbell had thought that, by using longitudinal data on actual college attendance, they had proven the existence of peer influence. Our own analysis, using standardization techniques, seemed to confirm their findings. Both approaches, however, failed to provide controls for other relevant variables affecting college attendance. When we subsequently added those variables to our analysis, the apparent impact of having a friend attend college disappeared.

Our main failure, of course, has been our inability to come up with a measure which could perfectly assess peer selection without causing peer influence to be underestimated. Our use of perceived plans of friends originally offered much hope towards this end, and did seem to work quite well in our early standardization analysis. When we tried to apply this same control in our replication of the Duncan model, we found that we had less success. We noted that the control had logical weaknesses which could

not be clearly overcome unless we were able to extend our model temporally to include the college attendance effect, which we were not able to find. Nevertheless, it still seemed reasonable to believe that inclusion of this variable resulted in overcontrolling for peer selection, and that any remaining association between friends could be viewed as representing a lower bound for our estimate of peer influence. Our last models, however, set this lower bound at zero, leading to the somewhat uninformative conclusion that peer influence may or may not exist. As noted above, it is the author's opinion that the bulk of the evidence indicates that peer influence is about half as large as the original Duncan model estimated, but those wishing to disagree do have evidence to the contrary.

While this paper has attempted to provide an in-depth analysis of peer influence and the status attainment process, it has hardly been exhaustive. The following might therefore be considered for future research.

First, one of the problems in measuring peer influence has been that no one data base has all the necessary information. Studies which are not longitudinal are severely handicapped in their ability to separate peer influence from peer selection. The studies which have been

longitudinal, on the other hand, have often lacked information which the present analysis has found to be quite important. Even the Wisconsin data would be much more useful if a few more questions about friends had been asked in 1957. Future researchers, then, would do well to consider a longitudinal design which gathers information on the variables we have found to be important.

Second, the present study has been rather casual in its sample selection. We have assumed that all dyads should be treated equally, and that our sample was representative. As we noted earlier, however, these assumptions simply may not be true. The effect of such factors as reciprocity, order named, etc., need to be examined more carefully.

Third, in order to enhance comparability with other research, the last part of our analysis has not included women. Examining how the models for the two groups differ would, of course, be of considerable interest.

Fourth, we have not considered what effect school characteristics might have on the status attainment and peer selection processes. Our earlier review of the literature suggested that these school effects might be

quite important. Such characteristics as the mean SES of a school, mean IQ, whether private or nonprivate, etc., can be easily determined, and other information may be available as well.

In conclusion, this thesis has provided strong evidence for the existence of peer influence, and has tried to better estimate how strong that influence is. Ironically, however, while other studies have suffered because they did not control for peer selection, our analysis was weakened because we provided too much of a control. While this thesis may have done a better job than most of separating peer influence from peer selection, it remains for future analysts to separate the two completely.

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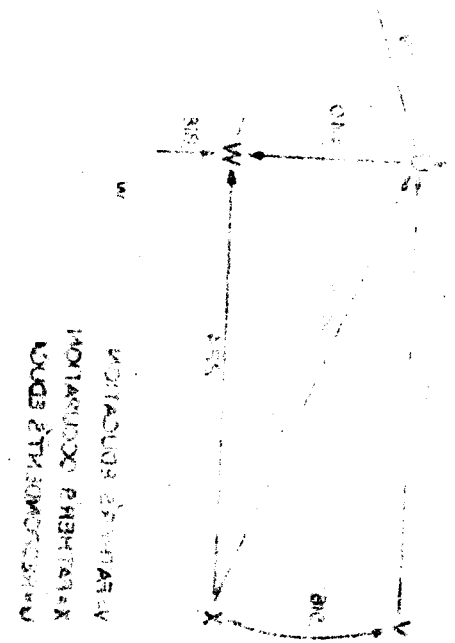
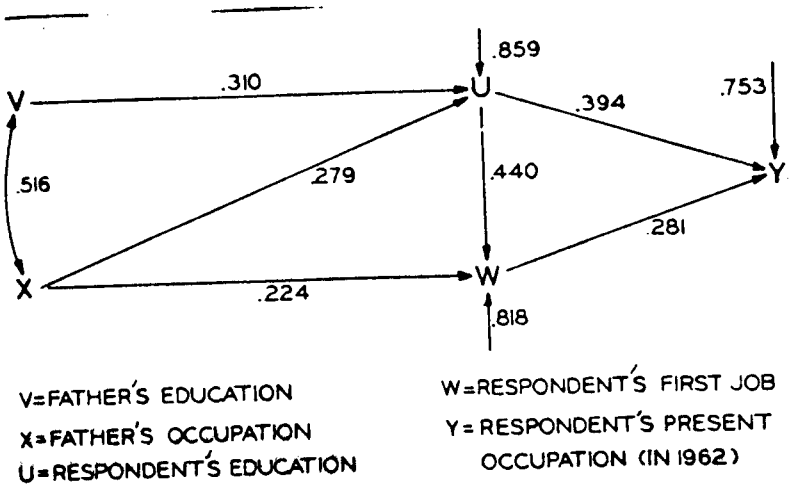


FIGURE 1

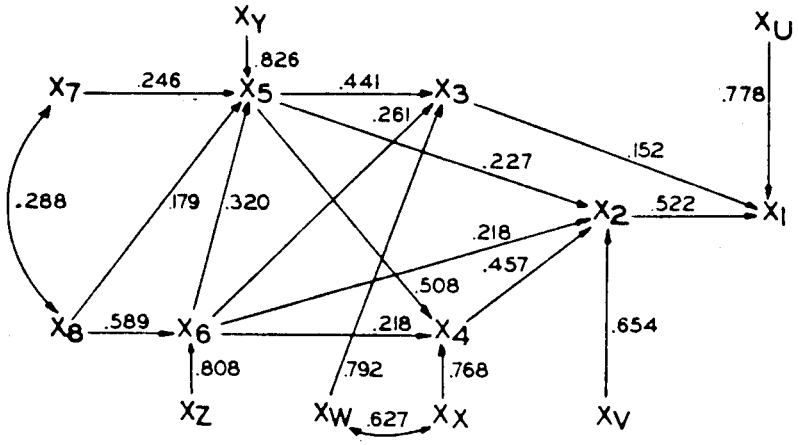
The Blau-Duncan Model of Status Attainment



SOURCE: Archibald O. Haller and Alejandro Portes, "Status Attainment Processes", Sociology of Education 1973, Vol. 46 (Winter), p. 57.

FIGURE 2

The Sewell-Haller-Ohlendorf Model of Educational and Early Occupational Attainment

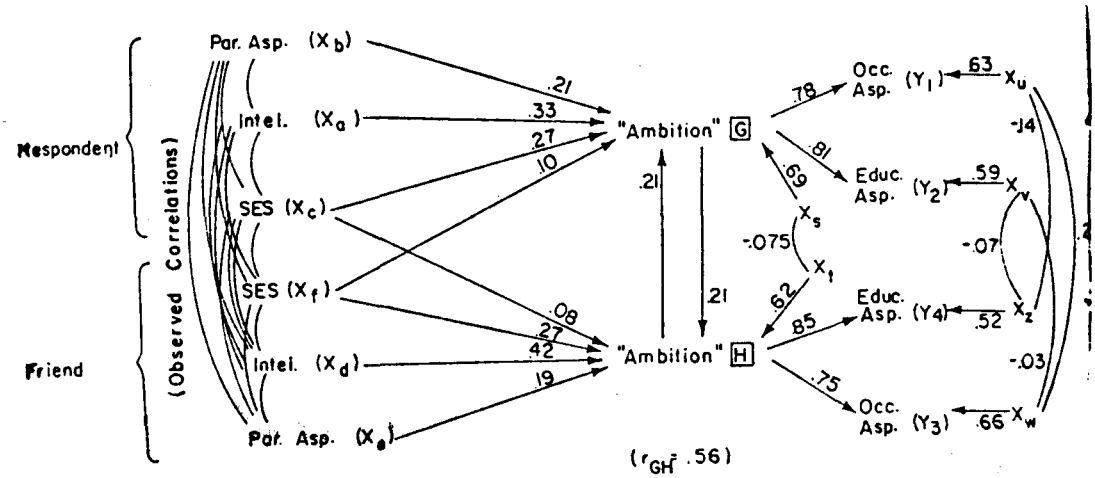


X_1 - OCCUPATIONAL ATTAINMENT
X_2 - EDUCATIONAL ATTAINMENT
X_3 - LEVEL OF OCCUPATIONAL ASPIRATION
X_4 - LEVEL OF EDUCATIONAL ASPIRATION
X_5 - SIGNIFICANT OTHERS' INFLUENCE
X_6 - ACADEMIC PERFORMANCE
X_7 - SOCIOECONOMIC STATUS
X_8 - MENTAL ABILITY

SOURCE: Archibald O. Haller and Alejandro Portes, "Status Attainment Processes", Sociology of Education 1973, Vol. 46 (Winter), p. 59.

FIGURE 3

The Duncan-Haller-Portes Model of Peer Influence



SOURCE: Otis Dudley Duncan, Archibald O. Haller, and Alejandro Portes, "Peer Influences on Aspiration: A Reinterpretation", American Journal of Sociology, Sept. 1968, Vol. 74, p. 130.

TABLE 1

College Attendance Rate By Perceived Plans of Friends, Standardizing for SES and Sex

	(1) Full sample	(2) Reciprocated choices	(3) Nonreciprocated choices	(4) College planners only	(5) Noncoll. planners only
(A) Attendance rate of resp. whose frds. planned to go to college	65.95%	66.33%	65.78%	89.63%	22.77%
(B) Attendance rate of resp. whose frds. did not plan to go to college	21.28	19.89	21.91	74.58	9.91
(C) Difference between the groups (A - B)	44.67	46.44	43.87	15.05	12.86
(D) Attendance rate of group B after standardization	30.71	29.33	31.83	77.04	10.24
(E) Amount of difference after standardization (A - D)	35.24	37.00	33.95	12.59	12.53
(F) Amount of difference due to rates	34.09	36.33	32.95	12.19	12.01
(G) Amount of difference due to composition	9.43	9.45	9.92	2.46	.33
(H) Amount of difference due to interaction	1.15	.67	1.01	.41	.52

TABLE 2

College Attendance Rate By Educational Attainment of Friend, Standardizing for SES and Perceived Plans of Friends

	(1) Full sample	(2) Reciprocated choices	(3) Nonreciprocated choices	(4) College planners only	(5) Noncoll. planners only
(A) Attendance rate of resp. whose frds. went to college	60.54%	61.98%	59.91%	90.69%	21.75%
(B) Attendance rate of resp. whose frds. did not go to college	25.80	24.19	26.54	77.26	9.51
(C) Difference between the groups (A - B)	34.74	37.79	33.37	13.43	12.24
(D) Attendance rate of group B after standardization	43.03	43.80	42.74	82.29	11.97
(E) Amount of difference after standardization (A - D)	17.51	18.18	17.17	8.40	9.78
(F) Amount of difference due to rates	16.48	17.87	15.87	9.70	8.16
(G) Amount of difference due to composition	17.23	19.61	16.21	5.03	2.46
(H) Amount of difference due to interaction	1.03	.31	1.29	-1.30	1.63

TABLE 3A

Means and Standard Deviations of Variables Used in Structural Equation Models

Variable	Mean	S.D.
(1) I.Q. score	103.057	14.668
(2) Family SES (1957)	17.564	11.980
(3) High school program	.701	.458
(4) High school rank	99.064	14.260
(5) Parental aspirations (1957)	.641	.480
(6) Parental aspirations (1975)	.563	.496
(7) Teacher encouragement (1957)	.511	.500
(8) Teacher encouragement (1975)	.494	.500
(9) Perceived plans of friends(1957)	.420	.494
(10) Perceived plans of friends (1975)	.389	.488
(11) Educational aspirations (1957)	.446	.497
(12) Educational aspirations (1975)	.521	.500
(13) Occupational aspirations (1957)	50.116	27.391
(14) College attendance	.497	.500

* Because of the symmetry of the data, means and standard deviations on these variables are the same for both respondent and peer.

TABLE 3B

Correlation matrix of variables used in structural equation models

		(1)	(2)	(3)	(4)	(5)
(1)	I.Q. score	1.000				
(2)	SES (1957)	0.316	1.000			
(3)	H.S. program	0.456	0.285	1.000		
(4)	H.S. rank	0.620	0.216	0.380	1.000	
(5)	Par. aspir.(57)	0.419	0.374	0.441	0.390	1.000
(6)	Par. aspir.(75)	0.354	0.404	0.383	0.331	0.550
(7)	Tch. encour.(57)	0.354	0.210	0.316	0.436	0.445
(8)	Tch. encour.(75)	0.283	0.161	0.254	0.342	0.299
(9)	Frd. plans(57)	0.351	0.377	0.344	0.340	0.422
(10)	Frd. plans(75)	0.307	0.364	0.319	0.260	0.344
(11)	Educ. asp.(57)	0.457	0.400	0.439	0.503	0.570
(12)	Educ. asp.(75)	0.434	0.372	0.437	0.451	0.533
(13)	Occ. asp.(57)	0.491	0.396	0.458	0.483	0.545
(14)	College att.	0.489	0.425	0.464	0.474	0.538
(15)	Frd. coll. att.	0.259	0.294	0.277	0.208	0.274
(16)	Frd.occ.asp(57)	0.278	0.297	0.278	0.227	0.280
(17)	Frd.ed.asp.(75)	0.232	0.269	0.261	0.190	0.272
(18)	Frd.ed.asp.(57)	0.261	0.285	0.269	0.213	0.278
(19)	Frd frd plns(75)	0.267	0.322	0.260	0.218	0.294
(20)	Frd frd plns(57)	0.276	0.325	0.269	0.240	0.306
(21)	Frd tch enc(75)	0.132	0.111	0.120	0.124	0.101
(22)	Frd tch enc(57)	0.174	0.122	0.153	0.174	0.138
(23)	Frd par asp(75)	0.207	0.234	0.202	0.153	0.226
(24)	Frd par asp(57)	0.221	0.251	0.223	0.171	0.227
(25)	Frd H.S. rank	0.219	0.141	0.176	0.291	0.171
(26)	Frd H.S. prog	0.230	0.229	0.318	0.176	0.223
(27)	Frd SES(1957)	0.229	0.391	0.229	0.141	0.251
(28)	Frd IQ score	0.300	0.229	0.230	0.219	0.221

(continued)

TABLE 3B (Continued)

		(6)	(7)	(8)	(9)	(10)
(6)	Par. aspir.(75)	1.000				
(7)	Tch. encour.(57)	0.368	1.000			
(8)	Tch. encour.(75)	0.336	0.395	1.000		
(9)	Frd. plans(57)	0.388	0.336	0.251	1.000	
(10)	Frd. plans(75)	0.373	0.264	0.216	0.483	1.000
(11)	Educ. asp.(57)	0.518	0.451	0.362	0.506	0.421
(12)	Educ. asp.(75)	0.520	0.432	0.382	0.477	0.403
(13)	Occ. asp.(57)	0.482	0.401	0.329	0.499	0.380
(14)	College att.	0.509	0.393	0.317	0.471	0.399
(15)	Frd. coll. att.	0.263	0.191	0.149	0.356	0.352
(16)	Frd.occ.asp(57)	0.261	0.199	0.166	0.370	0.337
(17)	Frd.ed.asp.(75)	0.252	0.194	0.144	0.331	0.316
(18)	Frd.ed.asp.(57)	0.266	0.195	0.151	0.372	0.356
(19)	Frd frd plns(75)	0.290	0.176	0.150	0.367	0.382
(20)	Frd frd plns(57)	0.289	0.191	0.140	0.456	0.367
(21)	Frd tch enc(75)	0.101	0.123	0.127	0.140	0.150
(22)	Frd tch enc(57)	0.137	0.129	0.123	0.191	0.176
(23)	Frd par asp(75)	0.221	0.137	0.101	0.289	0.290
(24)	Frd par asp(57)	0.226	0.138	0.101	0.306	0.294
(25)	Frd H.S. rank	0.153	0.174	0.124	0.240	0.218
(26)	Frd H.S. prog	0.202	0.153	0.120	0.269	0.260
(27)	Frd SES(1957)	0.234	0.122	0.111	0.325	0.322
(28)	Frd IQ score	0.207	0.174	0.132	0.276	0.267

(continued)

TABLE 3B (Continued)

	(11)	(12)	(13)	(14)	(15)
(11) Educ. asp.(57)	1.000				
(12) Educ. asp.(75)	0.719	1.000			
(13) Occ. asp.(57)	0.759	0.637	1.000		
(14) College att.	0.672	0.646	0.603	1.000	
(15) Frd. coll. att.	0.341	0.310	0.336	0.350	1.000
(16) Frd.occ.asp(57)	0.339	0.321	0.356	0.336	0.603
(17) Frd.ed.asp.(75)	0.315	0.300	0.321	0.310	0.646
(18) Frd.ed.asp.(57)	0.349	0.315	0.339	0.341	0.672
(19) Frd frd plns(75)	0.356	0.316	0.337	0.352	0.399
(20) Frd frd plns(57)	0.372	0.331	0.370	0.356	0.471
(21) Frd tch enc(75)	0.151	0.144	0.166	0.149	0.317
(22) Frd tch enc(57)	0.195	0.194	0.199	0.191	0.393
(23) Frd par asp(75)	0.266	0.252	0.261	0.263	0.509
(24) Frd par asp(57)	0.278	0.272	0.280	0.274	0.538
(25) Frd H.S. rank	0.213	0.190	0.227	0.208	0.474
(26) Frd H.S. prog	0.269	0.261	0.278	0.277	0.464
(27) Frd SES(1957)	0.285	0.269	0.297	0.294	0.425
(28) Frd IQ score	0.261	0.232	0.278	0.259	0.489

(continued)

TABLE 3B (Continued)

	(16)	(17)	(18)	(19)	(20)
(16) Frd.occ.asp(57)	1.000				
(17) Frd.ed.asp.(75)	0.637	1.000			
(18) Frd.ed.asp.(57)	0.759	0.719	1.000		
(19) Frd frd plns(75)	0.380	0.403	0.421	1.000	
(20) Frd frd plns(57)	0.499	0.477	0.506	0.483	1.000
(21) Frd tch enc(75)	0.329	0.382	0.362	0.216	0.251
(22) Frd tch enc(57)	0.401	0.432	0.451	0.264	0.336
(23) Frd par asp(75)	0.482	0.520	0.518	0.373	0.388
(24) Frd par asp(57)	0.545	0.533	0.570	0.344	0.422
(25) Frd H.S. rank	0.483	0.451	0.503	0.260	0.340
(26) Frd H.S. prog	0.458	0.437	0.439	0.319	0.344
(27) Frd SES(1957)	0.396	0.372	0.400	0.364	0.377
(28) Frd IQ score	0.491	0.434	0.457	0.307	0.351

	(21)	(22)	(23)	(24)	(25)
(21) Frd tch enc(75)	1.000				
(22) Frd tch enc(57)	0.395	1.000			
(23) Frd par asp(75)	0.336	0.368	1.000		
(24) Frd par asp(57)	0.299	0.445	0.550	1.000	
(25) Frd H.S. rank	0.342	0.436	0.331	0.390	1.000
(26) Frd H.S. prog	0.254	0.316	0.383	0.441	0.380
(27) Frd SES(1957)	0.161	0.210	0.404	0.374	0.216
(28) Frd IQ score	0.283	0.354	0.354	0.419	0.620

	(26)	(27)	(28)
(26) Frd H.S. prog	1.000		
(27) Frd SES(1957)	0.285	1.000	
(28) Frd IQ score	0.456	0.316	1.000

TABLE 4A

Structural Coefficients, Respondent's "Ambition" Dependent, with 1957 and 1975 Educational Aspirations and 1957 Occupational Aspirations Used as Indicators of "Ambition" (Z Values in Parentheses)

Variable	Model I	Model II	Model III
Parental aspirations (1957)	.4243 (18.12)	.3098 (13.53)	.2752 (12.27)
IQ score	.2506 (11.39)	.0682 (2.82)	.0599 (2.54)
Family SES (1957)	.1616 (7.55)	.1644 (8.17)	.1251 (6.30)
High school program	- (-)	.1385 (6.49)	.1228 (5.90)
High school rank	- (-)	.2112 (8.80)	.1957 (8.35)
Teacher encouragement (1957)	- (-)	.1467 (7.02)	.1250 (6.10)
Perceived friends' plans (1957)	- (-)	- (-)	.2108 (9.79)
Friend's "ambition"	.2055 (6.60)	.1573 (5.91)	.1121 (4.30)
Likelihood ratio chi-square	88.00	146.02	153.00
D.F.	18	33	38

TABLE 4B

Correlations Between "Ambition" and Its Indicators (Z Values in Parentheses)

Variable	Model I	Model II	Model III
Educational aspirations (1957)	.8783 (41.85)	.8755 (42.45)	.8742 (42.82)
Retrospective educational aspirations (1975)	.8044 (30.50)	.8076 (32.12)	.8073 (32.83)
Occupational aspirations (1957)	.8187 (Fixed)	.8145 (Fixed)	.8167 (Fixed)

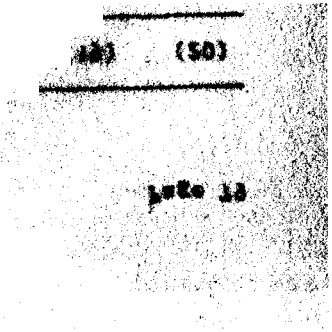


Table 5A

Structural Coefficients, Respondent's "Ambition" Dependent, With 1957 and 1975 Educational Aspirations as the Only Indicators of "Ambition" (Z Values in Parentheses)

Variable	Coefficient
Parental aspirations (1957)	.2768 (11.70)
IQ score	.0372 (1.49)
Family SES (1957)	.1241 (5.92)
High school program	.1154 (5.25)
High school rank	.1991 (8.04)
Teacher encouragement (1957)	.1410 (6.50)
Perceived friends' plans (1957)	.2066 (9.11)
Friend's "ambition"	.1060 (3.81)
Likelihood ratio chi-square	52.597
D.F.	21

Table 5B

Correlations Between "Ambition" and Its Indicators (Z Values in Parentheses)

Variable	Correlation
Educational aspirations (1957)	.8756 (36.28)
Retrospective educational aspirations (1975)	.8213 (Fixed)

TABLE 6A

Structural Coefficients, Respondent's "Ambition" and College Attendance Dependent (Z Values in Parentheses)

Variable	Ambition	College Attendance
Parental aspirations (1957)	.3172 (13.97)	- (-)
IQ score	.0661 (2.72)	.0696 (2.93)
Family SES (1957)	.1642 (8.11)	.0695 (3.39)
High school program	.1390 (6.48)	.0493 (2.30)
High school rank	.2125 (8.80)	.0118 (.48)
Teacher encouragement (1957)	.1438 (7.00)	- (-)
Friend's "ambition"	.1552 (5.86)	.0027 (.03)
Respondent's "ambition"	- -	.6632 (18.97)
Friend's college attendance	- (-)	.0270 (.22)

Likelihood ratio chi-square with 45 degrees of freedom = 181.856

TABLE 6B

Correlations Between "Ambition" and Its Indicators (Z Values in Parentheses)

Variable	Correlation
Educational aspirations (1957)	.8692 (42.92)
Educational aspirations (1975)	.8175 (33.70)
Occupational aspirations (1957)	.7973 (Fixed)

TABLE 7A

Structural Coefficients, Respondent's "Ambition" Dependent, With Multiple Indicators of "Ambition", "Parental Aspirations", "Teacher Encouragement", and "Perceived Friends' Plans" (Z Values in Parentheses)

Variable	Model I	Model II
"Parental Aspirations"	.5465 (8.48)	.4448 (6.86)
IQ score	.0216 (.84)	.0139 (.56)
Family SES (1957)	.0447 (1.74)	-.0038 (-.15)
High school program	.0310 (1.23)	.0171 (.71)
High school rank	.1128 (3.56)	.1070 (3.47)
"Teacher encouragement"	.2067 (3.38)	.1762 (2.95)
"Perceived friends' plans"	- (-)	.2823 (4.86)
Friend's "ambition"	.1093 (4.02)	.0128 (.37)
Likelihood ratio chi-square	168.56	208.16
D.F.	61	84

TABLE 7B

Correlation Between "Ambition" and Its Indicators (Z Values in Parentheses)

Variable	Model I	Model II
Educational aspirations (1957)	.8779 (42.08)	.8766 (42.18)
Retrospective educational aspirations (1975)	.8065 (30.90)	.8088 (31.32)
Occupational aspirations (1957)	.8170 (Fixed)	.8125 (Fixed)

TABLE 7C

Correlations Between "Parental Aspirations" and "Teacher Encouragement" With Their Indicators for Model I (Z Values in Parentheses)

Variable	1957 Measure	1975 Measure
"Parental aspirations"	.7702 (24.47)	.7096 (Fixed)
"Teacher encouragement"	.7128 (16.37)	.5551 (Fixed)

TABLE 7D

Correlations Between "Parental Aspirations", "Teacher Encouragement", and "Perceived Friends' Plans" With Their Indicators for Model II (Z Values in Parentheses)

Variable	1957 Measure	1975 Measure
"Parental aspirations"	.7694 (24.70)	.7144 (Fixed)
"Teacher encouragement"	.7085 (16.49)	.5563 (Fixed)
"Perceived friends' plans"	.7385 (21.55)	.6532 (Fixed)

TABLE 8

Structural Coefficients for Hypothesized Causal Ordering, With No Control for "Perceived Friends' Plans" (Z Values in Parentheses)

Dependent Variables	Independent Variables: IQ	SES	H.S. prog	H.S. rank	Parent aspir.	Teacher encour.	Friend's ambition
H.S. prog	.4059 (17.30)	.1569 (6.69)					
H.S. rank	.6131 (29.23)	.0218 (1.04)					
"Parental aspirations"	.1455 (4.4)	.3388 (12.72)	.3165 (11.18)	.2070 (6.70)			
"Teacher encouragement"	.0949 (2.48)	.1104 (3.64)	.1992 (5.98)	.4574 (11.15)			
"Ambition"	.0216 (.84)	.0447 (1.74)	.0310 (1.23)	.1128 (3.56)	.5465 (8.48)	.2067 (3.37)	.1093 (4.02)

Likelihood ratio chi-square with 61 d.f. = 168.5649

TABLE 9

Structural Coefficients for Hypothesized Causal Ordering, Controlling for "Perceived Friends' Plans" (Z Values in Parentheses)

Dependent Variables	Independent Variables: IQ	SES	H.S. prog	H.S. rank	Parent aspir.	Teacher encour.	Friends' plans	Friend's Ambition
H.S. prog	.4059 (17.30)	.1569 (6.69)						
H.S. rank	.6131 (29.23)	.0218 (1.04)						
"Parental aspirations"	.1444 (4.46)	.3382 (12.73)	.3154 (11.17)	.2075 (6.72)				
"Teacher encouragement"	.0950 (2.47)	.1115 (3.67)	.2004 (6.00)	.4585 (11.19)				
"Perceived friends' plans"	.1303 (3.65)	.3786 (12.64)	.2370 (7.69)	.1833 (5.39)				
"Ambition"	.0139 (.56)	-.0038 (-.15)	.0172 (.71)	.1070 (3.47)	.4448 (6.86)	.1761 (2.95)	.2824 (4.86)	.0128 (.37)

Likelihood ratio chi-square with 84 d.f. = 208.16