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ORGANIZATIONAL CAREER ATTAINMENT:

THE SALARIES OF MALE MANAGERS

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ABSTRACT

This paper examines the effects of certain achievement-related personal characteristics on firm-specific variation in the salary attainment of white male managers of a utility firm. Special emphasis is given to assessing the degree to which the effects of personal attributes on current salary are transmitted via starting salary and/or via the rate of increase in salary. Characteristics found to have a significant direct effect on starting salary include education, starting age, and previous organizational experience; characteristics having a significant direct effect on the rate of increase in salary include education, religious group membership, and, ambiguously, previous organizational experience. The analysis also turned up evidence of significant temporal fluctuation in the effect of education and previous experience -- but not of religious group membership -- on the level in the company hierarchy at which a manager begins his organizational career. On the negative side, neither family origins nor nativity had an effect on the salary attainment of these managers.

INTRODUCTION

Sociological research on organizational career attainment processes has had a checkered history. In an early and well-known paper on "informal factors in career achievement," Dalton observed that "the means by which individuals rise to higher positions in the organizations in which they work out their careers has long been a matter of dispute" (1951:407), but neither this long-standing dispute nor Dalton's paper itself generated anything like a sustained line of research on the subject. Although the subject attracted a number of qualitative analyses (Coates and Pellegrin, 1956), a full decade was to pass before Janowitz (1960) and Marsh (1961) brought quantitative evidence to bear on factors affecting career attainment in the American military and the nineteenth century Chinese imperial bureaucracy, respectively. Marsh concluded his paper with a call for more research on organizational attainment processes, but his plea fell apparently on deaf ears. The ensuing decade witnessed a host of qualitative accounts of organizational attainment processes (Glaser, 1968), but again no rigorous multivariate treatments. In fact, only recently has there been forthcoming a stream of research that promises to produce a solid body of evidence on the processes governing attainment in organizational careers (Beattie and Spencer, 1971; Wise, 1975; Marsh and Mannari, 1976; Petersen, 1976).

That sociologists' strong interest in socioeconomic careers has not promoted greater interest in organizational attainment is curious in view of the close connections, formal and otherwise, between the two processes. Socioeconomic careers are age (chronological) variations in earnings and prestige; organizational careers are age (length of service) variations in earnings and hierarchical rank. What this formal isomorphism does not reflect and what is of course more important is the degree to which these two attainment processes increasingly coincide *in fact*. After all, organizational careers are a subset of all socioeconomic careers. As large-scale organizations increasingly dominate the labor markets of advanced industrial societies and individual work-life histories increasingly unfold within an organizational context, the area of overlap in the phenomena addressed by models of these two attainment processes naturally expands. Carried to an extreme, the complete bureaucratization of individual work-life histories would render socioeconomic career trajectories reducible to their constituent intra- and interorganizational position shifts.

This extreme need never be realized in practice for the study of organizationally-based careers to yield valuable results pertaining to the general theory of achievement processes. For some time now models of the attainment process have been predominantly global in character, lacking detailed attention to the form the attainment process takes as it unfolds in different sectors of the social and occupational structure. This significant gap in our knowledge is gradually being closed with the accumulation of research that is more sensitive to the structural context of attainment processes. Work on occupation-specific models of attainment (Perucci and Perucci, 1970; Stolzenberg, 1975), as well as efforts to take account of community and industry effects on status attainment (Lane, 1968; Spilerman and Miller, 1976, Mueller, 1974;

Hauser and Featherman, 1977), indicate incipient movement in this direction. Similarly, greater attention to the organizational nature of attainment, be it through construction of system-level models of organizational mobility (White, 1971; Stewman, 1975) or through the analysis of individual-level processes or organizational career advancement, promises to contribute importantly to the theory of achievement.

It is against this background that this paper reports an analysis of the nature and determinants of career attainment in an organizational system. Attention is confined to the earnings dimension of organizational careers; we seek to indentify the manner in which certain personal attributes an individual brings with him (our sample consists of males only) to the organization enter into the earnings function. The data, about which more will be said shortly, pertain to a large California-based firm in the utility industry.

While we recognize that it is impossible to generalize to <u>the</u> bureaucratic career on the basis of data from one organization, a number of considerations suggest that the marginal contribution of our analysis to an understanding of general organizational earnings attainment processes may be relatively high. First, it is high because, as already noted, evidence on this subject is very meager to begin with. Secondly, because our data pertain to an organization of a type different from those examined by previous studies,[1] our results should help to extend the external validity of research findings in this area. Finally, not only do we examine the effects of personal characteristics omitted from previous studies (e.g., religion), but we formulate models of attainment that depart from past sociological treatments of the subject. The special character of these models inheres in the fact they generate evidence bearing on whether personal attributes affect current salary through their effect on starting salary or on the rate of salary increase. Previous research has either completely ignored this important issue, or treated it in ways not wholly attentive to the problems of inference that hinder its resolution on the basis of cross-sectional evidence (Weisbrod and Karpoff, 1968; Wise, 1975). Before formally introducing and discussing these models we briefly survey the relevant results of earlier research and describe the data.

PREVIOUS RESEARCH

Much of the work on organizational career attainment has been couched in terms of the familiar contrast between the influence of achieved, performance-related attributes or resources as compared to ascribed or "informal" individual characteristics. Dalton, for example, compared the effects of what he termed functional criteria of selection -- encompassing such attributes as education and seniority -- to the effects of nonfunctional or informal criteria such as religion, ethnicity, participation in prescribed social activities, etc. Marsh and Mannari draw on Weber to develop the distinction between bureaucratic (e.g., achievement and seniority) and extra-bureaucratic (e.g., family origins) determinants of advancement; a similar distinction underlies the analysis of Beattie and Spencer. In much the same spirit but without attention to nomenclature we distinguish between family origins, religious group membership, and nativity on the one hand, and age, education, previous work experience and length of service on the other.[2]

The selection of these variables reflects not only the conventions of past research and the limits of the available data, but other considerations as well. The most important of these had to do with my unwillingness to make excessively strong assumptions regarding causal order, or to unnecessarily complicate matters by carrying out the analysis within a simultaneous equation framework. Hence, with the exception of length of service, all these variables may plausibly be viewed as fixed in value at the time an individual enters the organization, and therefore predetermined with respect to salary. Numerous attitudinal variables, including indicators of an individual's commitment to and identification with both the company and his superiors, are excluded from this analysis because of the plausible suspicion that affective orientation and earnings are jointly dependent.[3]

As for the predetermined variables that are encompassed by our analysis, the literature pertaining to their influence on organizational attainment may be briefly summarized.

Family Origins. Researchers who have looked at the effects of family origins on attainment report results similar to those obtained by studies of the socioeconomic career (Duncan, Featherman, and Duncan, 1972): small but significant positive direct effects of father's occupation and/or father's education. Petersen (1976) found that father's occupation had such an effect on the ecclesiastical status achieved by American Catholic priests. Marsh and Mannari (1976:375-377) report significant (small) direct effects of father's occupation on the monthly pay of employees of two different Japanese firms. Wise, however, failed to find a direct effect of family SES on the monthly salary of a sample of managers of an American firm, but this may be explained by the fact that his sample was confined to college graduates and he controlled for college academic performance. Because our data is not limited in this way our analysis will provide a better basis of comparison to Marsh's and Mannari's results, allowing for an admittedly rough assessment of the relative influence of family origins on earnings in an American and Japanese firm. Assuming that ascriptive criteria are indeed more characteristic of the employment relations of Japanese than of American industries, we may expect to find smaller direct effects of family origins on earnings than those reported by Marsh and Mannari.

Religious Group Membership. Studies of socioeconomic achievement have consistently found significant gross differences in the occupational prestige and earnings attainment of major American religious groups, but such differences are entirely accounted for by disparities in family origins and education, leaving only a negligible net direct effect of religious group membership per se (see Featherman, 1971). But these results, based as they are on societal level treatments of attainment, do not neatly translate into inferences regarding the effect of the religious factor at the organizational level. Indeed, in the absence of organizational-level research the nature of the effect of religious group membership on organizational career attainment remains an open question.

Of course, grounds for informed speculation are not entirely lacking. In the firm he studied Dalton observed that the majority Protestants held -- to the disadvantage of the minority Catholics -- a disproportionately large number of upper-echelon management positions. He interpreted this evidence as showing that religious group membership was operating as a criterion of advancement, thereby enabling members of the dominant majority group to achieve an attainment advantage over minority group members (see Beattie and Spencer, 1971). However, Dalton did not control for other determinants of rank known to be associated with religious affiliation, the most prominent being education and, in this instance, membership in the Masons. But even ignoring these limitations, the fact of the matter is that Dalton's findings speak to only one organizational context; it is only reasonable to expect that the capacity of a religious majority to engineer its status dominance vis-a-vis a minority will vary across organizations as well as over time. The problem, of course, is that at this stage in our knowledge of organizational attainment processes it is impossible to be very specific about the nature of this variation. It is perhaps enough to examine the nature of the effect of the religious factor in yet another enterprise and a different time period, taking care to control for other relevant determinants of attainment.

Nativity. The term nativity is used here to capture a pair of contrasts of interests. First, the earnings differential distinguishing native and foreign-born men pursuing careers in the same organization deserves attention. Research at the societal level indicates that the overall gross prestige advantage enjoyed by native-born men is explained by their more favorable family origins and greater education (Blau and Duncan, 1967), but again evidence at the organizational level is lacking. Second, native-born men of local origin can be distinguished from those whose community of origin is more or less remote from their organization of employment. Marsh and Mannari (1976) report that distance of the community of origin from the company location had a significant positive direct (i.e., net of other factors) effect on pay in their Japanese electrical factory but not the shipbuilding firm. They do not explain why the local-extralocal distinction should be relevant to attainment; perhaps it is the cosmopolitan outlook that comes with (or generates) geographic mobility that results in the status advantage of extralocal men (Stinchcombe, 1974:53-58). But we are at a loss to explain why this mechanism should operate in the one enterprise but not the other. Nonetheless, the positive results obtained for the Japanese electrical factory alone suffices to warrant a look at the relative earnings of locals and extralocals in another enterprise.

Age. Research consistently reports a strong positive effect of age on salary (Beattie and Spencer, 1971:482; Marsh and Mannari, 1976:375,377). In some instances the effect of age is even substantially stronger than the effect of length of service (Bowles and Ginnis, 1976:100). Because age is usually highly correlated with length of service as well as education and previous experience, unbiased estimation of the parameters associated with these variables demands that age be controlled.

Education. The important part played by education in the socioeconomic career hardly needs documenting. Not surprisingly, findings of research in a diversity of organizational settings consistently show that education is one of the most powerful determinants of both rank and earnings attainment. I fully expect the results of my analysis to conform to this well established pattern.

Previous Experience. Only two prior studies have examined the effect of experience gained prior to entering a firm on earnings levels attained thereafter. Wise's data on managers of an American manufacturing firm and Marsh's and Mannari's data on managers of a Japanese electrical machinery company and a shipbuilding company both indicate that prior experience, measured in years, has a significant positive effect on earnings. Again, in the absence of evidence suggesting otherwise, I expect my analysis will replicate these findings.

Length of Service. All studies indicate that length of service has a sizeable positive effect on organizational rank as well as earnings attainment. This by itself is hardly surprising; what is curious is the reluctance of sociologists to go beyond this finding to explore in greater detail the nature of the length of service-achievement curve. While it is obviously necessary to establish the magnitude and sign of the slope of this function, the more interesting questions concern the second derivative of the function and the role personal attributes play in conditioning the slope itself. With regard to the first issue, most analyses have implicitly assumed that achievement is linear with respect to length of service, increasing at a constant rate as the organizational career unfolds; the most compelling alternatives -- one holding that achievement increases at a decreasing rate as a career equilibrium is approached, the other holding that achievement increases at a constant percentage rate -- receive no attention, even when suggested by the data itself (Beattie and Spencer, 1971:480, Table 2). We will want to examine and compare the fit of a number of different functional forms of the relation between salary and length of service.

By the same token, sociological analyses have failed to explore the manner in which the slope of the curve relating length of service to achievement is affected by individual personal attributes, the prevailing assumption being that prestige and earnings returns to length of service are constant. This, as shall be seen, is tantamount to assuming that the personal characteristics and resources a person brings to the organization affect his starting salary (or prestige level) but not his rate of salary (or prestige) increase thereafter. Wise (1975), however, has shown that this assumption is untenable for many personal attributes, at least for his data. It is of some consequence to the theory of organizational achievement to determine if career attainment differentials induced by personal characteristics are created at the time of employment or during the course of the organizational career proper. It is this issue around which we organize our analysis of the effects on salary of family origins, religious group membership, nativity, age, education and prior experience.

DATA AND MEASUREMENT

The data upon which this analysis is based were collected in 1960 by Oscar Grusky. The data pertain to managers of a California-based utility firm, at the time of the study the largest single enterprise of a major public utility holding company in the United States. Questionnaires were distributed to all 2,198 managers of the firm; 1649 (75%) usable, signed questionnaires were returned. The analysis that follows bears only on the attainment of the (all white) male subsample (N=1238):[4] females were excluded because of evidence indicating that the processes governing their attainment differ from that of males (Grusky, 1966).

Social origins are represented by father's occupation and father's education. Father's occupation, originally measured in terms of Edward's seven-point scale, was translated into the Duncan occupational prestige metric in accordance with a transformation suggested by Featherman, Jones and Hauser (1975).[5] Father's as well as respondent's education is a six-point scale corresponding to grouped years of schooling (see Blau and Duncan, 1967:165).

From detailed information on religious group membership the following set of dummy-variable categories was created for analysis: high Protestant (N = 520), low Protestant (N = 467) and Catholic or Orthodox (N = 211).[6] Although data was also obtained on ethnicity, an excessively large number of missing observations precluded the use of this variable. From detailed information on location of birth a set of three dummy-variable nativity categories was created: local (California; N = 371), extralocal (United States, excluding California; N = 800) and foreign-born (N = 60).

The data on length of service came to me coded in four four-year intervals. Efforts to obtain the raw values of length of service were unsuccessful. For the analysis that follows respondents were assigned the midpoint (2, 7, 12 or 17 years) of the interval into which they fell. Later we shall present evidence indicating that, all things considered, this linear coding is preferable to a dummy-variable treatment of length of service.

The data on current age also came coded into four intervals, and respondents were assigned the midpoint (26, 39, 49 or 59 years) of the interval into which they fell. For the purpose of analysis this coding of current age was replaced by an estimate of respondent's age at time of employment, namely (starting age) = (current age -- length of service).[7]

This transformation was employed in order to clarify the interpretation of the coefficient of length of service in regressions of salary on length of service and "age." In such a regression the coefficient of "age" does not depend on whether current age or starting age is used; each coding yields the identical coefficient. However, the coefficient of length of service is affected, as is the interpretation of tests of significance on this coefficient. To see this consider the following simple but sensible model:

$$(1) \quad S_1 = a_1 \ (Age_1) + u$$

$$(2) \quad S_2 = b_1 \ (LS) + b_2 \ (S_1) + e,$$

where S_1 is starting salary, S_2 is current salary, Age_1 is starting age, LS is length of service, and u and e are disturbances. Now for all reported analyses of organizational earnings attainment, including our own, data on starting salary is unavailable. In order to estimate equation 2 we can substitute equation 1 for S_1, yielding

$$(3) \quad S_2 = b_1 \ (LS) + b_2 a_1 \ (Age_1) + e'$$

where $e' = e + b_2 u$. Now, in a regression of current salary on length of service and starting age the coefficient of LS will be an estimate of the structural parameter b_1, and the coefficient of Age_1 will be an estimate of the product of the structural parameters a_1 and b_2, i.e, an estimate of the total effect of starting age on current salary. Now all of this would hardly be worth mentioning if it were not the case that many investigators (Beattie and Spencer, 1972; Marsh and Mannari, 1976) use current age (Age_2, say) in place of starting age in equation 3. To see the implications of this we substitute $Age_1 = (Age_2 - LS)$ in 3 and gather terms:

$$(4) \quad S_2 = (b_1 - b_2a_1)\ LS + b_2a_1\ (Age_2) + e'$$

The coefficient of current age in this equation is exactly the same as the coefficient of starting age in equation 3; in both cases the coefficient represents an estimate of the product b_2a_1. However, the coefficient of LS estimates not the structural parameter b_1, but rather the difference $(b_1 - b_2a_1)$.[8] Because no special significance attaches to this difference, and because in any case an estimate of b_1 is what we want, we should proceed by using starting age and an equation like (3) rather than current age and equation like (4).

Data on the number of years work experience each respondent brought with him to the utility firm is not available. In place of such a measure the total number of positions held in other companies prior to coming to the utility firm will be used as an indicator of previous experience. On its face this variable appears to tap breadth of organizational experience, but it most certainly captures length of work experience as well. In any case, previous organizational experience may itself be an important factor in determining salary, quite apart from any association with years of work experience.

Finally the dependent variable is annual salary. Data on salary came precoded into nine intervals corresponding to the actual salary-bracket structure of the firm; respondents were assigned the dollar value of the midpoint of the interval into which they fell.[9]

THE MODEL

We want to specify a model that captures a range of alternative representations of the manner in which current salary is affected by individual personal attributes and resources. The development that follows is a more general treatment of a formulation used by Wise. After presenting the model we will discuss certain problems of inference and interpretation not treated by Wise. To facilitate this presentation we will ignore the identity of the actual background and resource variables of interest.

Let the relation between current salary and length of service take the form

$$(1) \quad S = e^{a} e^{bt} e^{u}$$

where S is salary, t is length of service, a and b are parameters, and u is a random disturbance. For the time being we assume that this exponential function adequately fits the data. The interpretation of the parameters is straightforward: a and b control starting salary and the rate (with respect to t) of salary increase, respectively. Now a person comes to the organization with a set of attributes, some of which affect his salary. Of these attributes that affect salary, some remain more or less constant over the individuals career in the organization, while others may change. Here we assume that the attributes under consideration remain constant and are organized in a vector X.

An individual's current salary depends on his vector of personal attributes. The influence of X on salary may be registered via the parameter a, or b, or both. That is, some attributes affect salary through their effect on starting salary; some affect salary through their effect on the rate of increase in salary over the course of the individual's career in the organization; and some attributes may influence starting salary as well as the rate of increase in salary thereafter. We may therefore write

$$(2) \quad a = f_m(X_m)$$
$$b = f_p(X_p) \ ,$$

where X_m and X_p are subsets of X and f_m and f_p are nonstochastic. Taking logarithms of (1), substituting (2) and assuming f_m and f_p are linear yields

$$(3) \quad \ln S = a_o + \sum_{i=1}^{m} a_i X_i + (b_o + \sum_{i=1}^{p} b_i X_i)t + u,$$

where the first two terms represent $f_m(X_m)$ and the third term $f_p(X_p)$. In this equation all the 'a' parameters refer to starting salary (which is an implicit rather than observed variable) and all the 'b' parameters refer to the rate of increase in salary.

It is of some interest to consider the different forms this model can take and to examine their implications. The most general form of (3) is achieved when $X_m = X_p = X$ and all parameters of f_m and f_p are nonzero. In this case all personal attributes affecting current salary would register their impact via both starting salary and rate of salary increase. For example, if education is the first element of X_m and X_p, then (3) asserts that education has an effect of a_1 on starting salary and b_1 on the rate of increase in salary.

Somewhat less general models can be obtained by varying the elements of X_m and X_p (varying the nonzero parameters of f_m and f_p). Two cases of special interest occur when personal attributes transmit their effect on current salary via either starting salary or rate of increase in salary, but not both. If personal characteristics affect rate of salary increase but not starting salary (i.e., $a_1 = \ldots = a_m = 0$), then $f_m(X_m) = a_o$ and (3) becomes

$$(4) \quad \ln S = a_o + (b_o + \sum_{i=1}^{p} b_i X_i)t = u.$$

If personal attributes affect starting salary but not rate of salary increase (i.e., $b_i = \ldots = b_p = 0$), then $f_p(X_p) = b_o$ and (3) becomes

$$(5) \quad \ln S = a_o + \sum_{i=a}^{m} a_i X_i + b_o t + u.$$

Except for the logarithmic transformation of salary, equation (5) is the model informing sociological treatments of organizational earnings attainment (Beattie and Spencer, 1971; Marsh and Mannari, 1976). Estimates of the parameters are obtained by a simple OLS regression of earnings on personal attributes and length of service. What is lacking from previous uses of (5) but what is made explicit here is the special interpretation of the regression coefficients. The intercept may be viewed as an estimate of average starting salary; the coefficients of the X_i indicate the effect of each personal attribute on starting salary; the coefficient of length of service is an estimate of the average rate of salary increase. What the present formulation also makes plain is that equation (5) assumes that rate of salary increase is not only constant through time for each individual, but is constant across individuals as well. In other words, the assumption has been that rate of salary increase is not contingent on personal characteristics, or at least not on those fixed characteristics that influence starting salary. Concretely, equation 5 states that current individual differences in salary are the result of the application of a constant rate of increase to different starting salaries, a very different process from that of equation 4, say, which attributes current salary differentials to differential rates of increase "operating" on constant (with respect to personal attributes) starting salaries.

These remarks are not to be construed as implying that equation 5 constitutes a misspecification of the organizational attainment process. Whether equation 5 or equation 4 gives a better representation is not a logical question that can be treated abstractly. Indeed, it is not even the question one should ask. Rather, attention should be directed toward identifying and distinguishing personal attributes according to whether, for a particular organization, they affect starting salary, affect rate of salary increase, or affect both. Not unrelated to this issue is the problem of ascertaining how this partitioning of personal attributes according to the nature of their effects is itself a function of the type of organization as well as the historical time period. Because this latter question demands data on many organizations at varying points in time, our analysis can speak only to the first problem.

Our approach is baldly empirical. Lacking grounds for assigning one or the other type of salary effect to particular personal characteristics, we will begin by assuming that a general model of the form of equation 3 captures the manner in which background and resource variables affect current salary. In order to facilitate the discussion of our results it will help if we display the actual equation around which the analysis is organized. This equation is:

$$(6) \quad \ln S = a_o + a_{s,focc}FOCC + a_{s,fed}FED + a_{s,age}AGE + \sum_{i=1}^{3} a_{s,r_i}R_i + \sum_{i=1}^{3} a_{s,n_i}N_i + a_{s,ed}ED + a_{s,p}P + (b_o + b_{s,focc}FOCC + b_{s,fed}FED + b_{s,age}AGE + \sum_{i=1}^{3} b_{s,r_i}R_i + \sum_{i=1}^{3} b_{s,n_i}N_i + b_{s,ed}ED + b_{s,p}P)\cdot LS + u$$

and[10]

$$\sum_{i=1}^{3} a_{s,r_i} = \sum_{i=1}^{3} a_{s,n_i} = \sum_{i=1}^{3} b_{s,r_i} = \sum_{i=1}^{3} b_{s,n_i} = 0,$$

where the 'a' parameters refer to starting salary, the 'b' parameters refer to the rate of increase in salary, and the variables are: FOCC-father's occupation; FED-father's education; AGE-starting age; R_i-religious group categories; N_i-nativity categories; ED-education; P-number of previous organizational positions; LS-length of service; u-random disturbance.

Before turning to the results we should consider the limitations entailed by the assumptions, frequently unstated (Wise, 1975; Weisbrod and Karpoff, 1968), underlying this type of analysis. The most pressing problem of inference concerns the special interpretation we have imposed on the coefficients of terms representing the interaction of length of service with some personal attribute (e.g., terms like $(X_i t)$). By assuming that these coefficients represent the impact of a particular attribute on the rate of increase in salary over the course of the organizational career, we are, in effect, committing ourselves to what has been called the synthetic cohort interpretation of cross-sectional data (Blau and Duncan, 1967:183). This approach amounts to treating what are in fact intercohort comparisons at one point in time as if they were intracohort comparisons over time. In the present context cohorts are represented by the different length of service groups (newcomers and oldtimers, say) because these groups happen also to define the historical period during which managers entered the firm. In pressing a synthetic cohort interpretation we are assuming that, with respect to starting salary, current oldtimers "looked" like current newcomers when they were newcomers themselves (i.e., no period effects), and that, with respect to future salary, current newcomers will look like current oldtimers when they are oldtimers themselves (i.e., no cohort effects). More formally, we have to assume that the "a" parameters controlling starting salary -- and possibly subject to period effects, such that the effect of a personal attribute on starting salary depends on the period during which entry occurred -- and the "b" parameters controlling the rate of increase in salary -- and possibly subject to cohort effects, such that the effect of a personal attribute on the rate of salary increase varies by cohort -- are constant with respect to historical time.

The assumption concerning the absence of period effects on the "a" parameters is of critical importance, for without this assumption we can make no assertions whatsoever regarding the impact of personal characteristics on the rate of salary increase. To see how this assumption informs our inferences consider the simple model

$$\ln S = a_o + a_1X + b_ot + g(X \cdot t) + e.$$

Because t represents the time elapsed since entry into the firm as well as the period during which entry occurred, the interpretation of g is necessarily equivocal. Concretely, g merely indicates the difference between oldtimers and newcomers in the current salary differences (which for newcomers are equivalent to differences in starting salary) induced by variation in attribute X. If g is positive, say, it means that the current salary differences induced by X are greater among oldtimers than among newcomers. This much is beyond dispute; what is at issue is the nature of the mechanism producing the observed g. On the one hand, g may be interpreted as an "a" parameter, in which case it reflects interperiod differences in the effect of X on starting salary; that is, X created greater differences in starting salary in the period oldtimers entered the firm than when newcomers entered the firm, and the persistence of this difference is reflected in positive g. On the other hand, g may be viewed as a b parameter, in which case it reflects the effect of X on the rate of salary increase. The assumption here, of course, is that starting salary differentials induced by X when oldtimers began their careers resemble the current salary differentials observed among current newcomers (whose current salary is equivalent to their starting salary).[11]

It is this latter interpretation of the coefficient of interaction terms (like X·t) that informs the model we have presented. We assume the absence of period effects on the "a" parameters, so that these coefficients express the impact of personal attributes on the rate of salary increase. The first part of our analysis will be carried out as if there were no question regarding the truth of this assumption and the validity of our interpretation. Once we have established that there are statistically significant "g" coefficients whose interpretation is worth arguing about, we take up the issue of period effects on starting salary as an alternative explanation.

PRELIMINARY ANALYSIS

Salary and Length of Service. The model developed in the previous section assumes that the relation between salary and length of service can be represented by an exponential function. Evidence bearing on this assumption and on the fit of competing functional forms is displayed in Table 1.

The upper-half of the table gives the parameter estimates and R^2's corresponding to the linear, semi-log and step-function representations of the relation between salary and length of service. These forms differ with respect to their assumptions concerning the rate at which salary increases with length of service. The linear model assumes that salary increases at a constant dollar rate; the semi-log model assumes that salary increases at a decreasing rate; the step-function imposes no constraints on the rate of increase. The R^2's in Table 1 indicate that the linear function performs better than the semi-log function by a fair margin (.038), and is only slightly worse (.020) than the step-function. Between the step-function and linear models, the greater convenience of and savings in degrees of freedom achieved by the linear function more than make up for what it lacks by the R^2 criterion, thereby making it the preferred choice.

The lower-half of Table 1 gives the results pertaining to the fit of the exponential, double-log and step-function (in the logarithms of salary) forms. The exponential model assumes that salary increases at a constant percentage rate; the double-log (with $o < b < 1$) assumes that salary increases at a decreasing percentage rate (or, equivalently, constant (<1) elasticity); the step-function imposes no constraints on the rate of increase. All things considered, the exponential appears to perform the best of the three. The fact that the exponential falls a shade (.013) short of the step-function by the R^2 criterion is more than compensated by its greater convenience and the savings it achieves in degrees of freedom.

These comparisons leave us with the linear and exponential models from which to choose. While it is clear that the R^2 associated with the exponential (.150) is higher than that associated with the linear function (.122), strictly speaking the R^2's are not comparable because the dependent variables are not identical. However, certain relevant and legitimate comparisons of residual sums of squares are possible (Theil, 1971:544). First, using the exponential model as a baseline, we can take the logarithms of the predicted salaries from the linear regression and compute the sum of their squared deviations from the observed logarithms of salary, comparing this to the actual sum of squared residuals from the exponential regression. This yields residual sums of squares of 40.212 for the linear model as against 39.45 for the exponential baseline, which means that the error of the linear function exceeds that of the exponential by about 2%. Reversing the comparison yields a residual sum of squares of $394(10)^7$ for the exponential as against $392(10)^7$ for the linear

Table 1. Regression coefficients and R^2 for selected functional forms of the regression of current salary on length of service, male managers of a utility firm, 1962.

Relation[a]	a	b	R^2
(1) $S = a + b(LS)$	6673	164	.122
(2) $S = a + b \ln(LS)$	6314	958	.084
(3) $S = a + \Sigma_i b_i (LS\ )$	7470	150 962 2411	.142
(4) $\ln S = a + b(LS)$	8.81	.019	.150
(5) $\ln S = a + b \ln(LS)$	8.76	.113	.112
(6) $\ln S = a + \Sigma_i b_i (LS\ )$	8.89	.035 .131 .273	.163

[a]The functional forms are: (1) linear; (2) semi-logarithmic; (3) step-function; (4) exponential; (5) double-logarithimic; and (6) step-function. For both step-functions length of service is represented by three dummy variables; the fourth and lowest length of service category is excluded.

baseline, so that the error of the exponential exceeds that of the linear model by only .5%. While these comparisons are not entirely unequivocal, and the difference in the error differences (2% compared to .5%) is very small, all the evidence taken together suggests that we are justified in going forward with the exponential form.

Substantively, it would be unwise to make too much of these results. Recall that both the salary and length of service data are grouped, and that the salary groups are modeled after the official salary brackets of the firm. For these reasons our findings probably say more about the way this firm has organized its salary structure than about the "true" relation between salary and length of service that would be observed with raw data. In fact, this may partly account for the extremely low coefficients of determination obtained for all functional forms. Wise (1975), for example, obtained an R^2 of .49 by fitting an exponential function to raw salary and length of service data. The disparity between his fit and ours may reflect the fact, not without interest in its own right, that a substantial proportion of the variation in salary accounted for by length of service occurs within rather than between official salary brackets. It is just this "within" variation that is not captured by out data.

Current Salary and Personal Attributes. Before exploring in detail the manner in which each personal attribute enters into the determination of current salary -- be it via starting salary or the rate of increase in salary -- we consider the overall net contribution each personal characteristic makes to the explained variance. Table 2 shows the increment to R^2 resulting from the addition of all terms involving a particular

Table 2. Total net contributions (ΔR^2) of selected personal attributes to the variance explained in the current salaries of male managers of a utility firm, 1962 (N = 988).

Attribute[a]	ΔR^2	df	F	P
Family Origins[b]	.0004	4	0.11	n.s.
Religion	.0119	4	3.85	<.01
Nativity	.0027	4	0.87	n.s.
Age	.0116	1	11.56	<.01
Education	.0508	2	32.83	<.01
Previous Positions	.0059	2	3.78	<.05
Length of Service	.1906	9	27.36	<.01

[a]Each attribute is represented by all terms involving the relevant variable. For example, education refers to both the main effect of education as well as its interaction with length of service. This way of assessing net contributions of particular variables in equations involving interaction terms is standard practice (Kmenta, 1971:456-457).

[b]Family origins represents all terms involving father's education and father's occupation.

characteristic to a model containing terms in all other variables. Thus, the row labelled 'Family Origins' gives the statistics bearing on the explanatory power achieved by adding all terms involving father's occupation and father's education, i.e., achieved by fitting the parameters $a_{s,focc}$, $a_{s,fed}$, $b_{s,focc}$ and $b_{s,fed}$. Similarly, the row labelled 'Religion' refers to the improvement obtained by fitting the six parameters (only four of which are 'free') $\sum_{i=1}^{3} a_{s,r_i} + \sum_{i=1}^{3} b_{s,r_i}$; and so on.

As indicated by the last column of this table, four of the six (ignoring length of service) characteristics register a statistically significant net impact on current salary. The only variables failing to have an impact are family origins and nativity. The absence of an effect for family origins is consistent with Wise's finding for an American firm but, on its face, inconsistent with the significant positive effect Marsh and Mannari report for two Japanese firms. Whether this indicates the greater significance of social background in Japanese than in American employment relations is hard to say with certainty. Similarly, without a broader and firmer basis for comparison, not too much significance should be assigned to the fact that the absence of a direct effect for the nativity categories fails to conform to Marsh's and Mannari's finding that distance of the community of origin from the firm had a positive impact on pay in their Japanese electrical factory. What we can say is that our results indicate that whatever advantages geographic mobility yields in terms of cosmopolitanism, the consequences for salary are ultimately very small.

Of the remaining variables length of service is clearly the most powerful explanatory factor, with an incremental R^2 of 19%. Even judging by the F-statistics, which take account of disparities in degrees of freedom, length of service is very powerful. If we consider only those characteristics with which a manager comes to the firm, education is by far the most important salary determinant, followed by starting age, religious group membership and previous organizational experience.

The significant net effects of religious group membership and prior experience deserve special attention, since both are novel additions to research on organizational attainment. The impact of religious group membership, while not entirely unanticipated, does stand in sharp contrast to results generated by societal-level treatments of attainment. The influence of prior organizational experience on salary is consistent with results obtained when previous experience is measured in years, but also suggests as a possibility that experience in other organizations may itself be an important causal variable.

<u>Starting Salary and Rate of Increase in Salary</u>. The results just reviewed indicate neither the direction nor nature of the effects of personal characteristics on current salary. Of primary thematical interest is whether these effects on current salary are transmitted via starting salary, and thus represent a once-and-for-all phenomenon occurring at the time of employment, or are transmitted via the rate of increase in salary, and thus have a continuing impact on the course of the organizational career.

Table 3 gives the parameter estimates and test statistics bearing on these issues. The upper and lower panels of Table 3 pertain to starting salary and the rate of increase in salary, respectively. The first column (b') gives the coefficients obtained on the assumption that the effects of personal characteristics are transmitted exclusively via starting salary. This 'partial' model constrains to zero the effects of personal characteristics on the rate of salary increase. This constraint is relaxed by our 'full' model, the coefficients of which are given in the second column (b) of Table 3. As can be seen by a comparison of the coefficients of determination given at the bottom of the table, relaxing the constraint imposed by the partial model results in an admittedly small (.017) but statistically significant ($p < .01$) increase in explained variance. In terms of the parameters of our structural equation (equation 6), this means we can reject the global null hypothesis $b_{s,focc} = b_{s,fed} = b_{s,r_i} = b_{s,n_i} = b_{s,ed} = b_{s,np} = 0$. This justifies our treating the full model as 'true' and using the partial model as a secondary point of comparison.

The first entries in the upper and lower panels of column b give the estimates of average starting salary and average rate of salary increase, respectively. Average starting (ln) salary is 8.53 ($5064), with salary increasing at an average annual rate of 1.84%. A comparison of these figures with the parameter estimates obtained without considering personal characteristics (Table 1, line 4) shows a substantial difference with respect to starting salary (8.53 vs. 8.81), but virtually no difference with respect to rate of salary increase (1.84 vs. 1.9). This

Table 3. Regression coefficients for the regression of current salary on selected personal attributes of male managers of a utility firm, 1962.

Variables	b'[1]	b	beta	t	F
		Starting Salary			
Average	8.4967	8.5299			
Age	.0028 (.0008)	.0027 (.0008)	.108	3.40**	
Father Occ	.0001 (.0004)	.0001 (.0009)	.011	0.13	
Father Educ	.0006 (.0050)	.0035 (.0118)	.022	0.29	
Religion[2]					0.81
Hi Prot	.0213 (.0086)	-.0236 (.0226)			
Lo Prot	-.0154 (.0088)	-.0090 (.0233)			
Catholic	-.0059	.0326			
Nativity					1.14
Foreign	.0306 (.0188)	.0353 (.0399)			
Extralocal	-.0036 (.0111)	.0072 (.0254)			
Local	-.0271	-.0425			
Education	.0520 (.0067)	.0338 (.0153)	.160	2.21*	
Previous Positions	.0039 (.0043)	.0293 (.0109)	.208	2.70**	

Table 3 (Continued)

Variables	b[1]	b	beta	t	F
		Rate of Salary Increase			
Average	.0222 (.0015)	.0184 (.0063)	.384	2.92**	
Father Occ		.0000 (.0001)	.000	0.00	
Father Educ		-.0002 (.0011)	-.013	0.15	
Religion					2.76**
Hi Prot		.0045 (.0019)			
Lo Prot		-.0005 (.0020)			
Catholic		-.0040			
Nativity					0.46
Foreign		-.0016 (.0036)			
Extralocal		-.0006 (.0022)			
Local		.0022			
Education		.0017 (.0013)	.150	1.38	
Previous Positions		-.0024 (.0009)	-.222	2.69**	
Number of Cases	988	988			
R^2	.233	.250			17.95**
Increment to R^2 due to Interaction	.017				2.78**

[1]For comparison this column (b') gives the estimates of the coefficients obtained in the absence of the rate of salary increase interaction terms. The other columns (b, beta, t, F) refer to the full model. Appearing in parentheses below the regression coefficients are the standard errors.

[2]The coefficients of the religious group categories and of the nativity categories are expressed as deviations from the grand mean; this is why they sum to zero for each classification. Also, the computer program I used did not give the standard error of the bottom category (Catholic and local, respectively) of each classification.

*$p < .05$

**$p < .01$

indicates what we might have guessed, namely, that the personal characteristics a man brings to the hiring process have a greater impact on the salary with which he starts his organizational career than on his rate of increase in salary thereafter.

From Table 2 we know that starting age has a direct net effect on current salary. The coefficient of age given in column b (.0027) indicates, as expected, that this effect is positive, with older men enjoying a salary advantage over their younger counterparts at the time of employment. The estimates of the age coefficient obtained from the partial and full models are virtually identical. Two other points are worth noting. First, as mentioned earlier, we have assumed that the effect of starting age is transmitted exclusively by way of starting salary. Calculations not presented here show that the addition of a term representing the effect of age on the rate of salary increase serves to substantially increase the standard errors of our estimates without contributing anything to the explained variance. Secondly, the substantive interpretation of the coefficient of starting age is somewhat ambiguous. We cannot say for sure whether age per se is being rewarded (Bowles and Gintis, 1976:100), or whether starting age is acting as a surrogate for years of prior work experience. This distinction coincides, of course, with the question of the relative role of ascriptive and achievement criteria in the reward allocation process. Our suspicion is that the coefficient of starting age is probably inflated owing to the omission of a measure of years of experience, but would not be negligible in any case. For this reason we tentatively suggest that some of the salary advantage enjoyed by older men is indeed ascriptive in nature.

To this point we have observed no gross disparities between the conclusions one would draw from the partial and full models. The first instance of a notable disparity is occasioned by an examination of the nature of the effect of religious group membership. In this case it pays to consider first the estimates of the effect coefficients served up by the partial model (column b' of Table 3). The religious group coefficients, expressed as deviations from the grand mean of the dependent variable, indicate that High Protestants (.0213) enjoy a (ln) salary advantage of .0367 over Low Protestants and .0272 over Catholics, differences that are statistically significant (test statistics not shown). The direction of these overall salary differentials do, of course, conform to the general idea that a numerically predominant high status group (High Protestants) may be capable of engineering a reward advantage vis-a-vis lower status and numerically smaller groups.

What is of greater moment is the mechanism by which this advantage is achieved. According to the logic of the partial model, the reward advantage enjoyed by High Protestants is attributable to differences in starting salary created at the time of employment, with religious differences having little affect on the salary advances occurring during the course of the organizational career. On the basis of the partial model, then, we might conclude that salary practices (discriminatory?) which systematically favor High Protestants at the expense of Low Protestants and Catholics are structurally localized and confined to the hiring process, rather than being a property of the broader reward-allocation process of the firm.

The problem with this reading of the evidence is that it is based on a naive and unrealistic representation of the process by which ascriptively-based and normatively proscribed inequalities become institutionalized. Because the creation of gross salary differences among religious (or other kinds of) groups at the start of organizational careers cannot be disguised or rationalized in terms of the vagaries of on-the-job performance differences, this interpretation virtually forces one to assume the operation of a conscious policy of discrimination. Are we willing to accept an interpretation that <u>automatically</u> assigns ascriptively-based salary differences to discrimination? But even if we do accept the arguable premise that religious group salary differences are due to discrimination, implementing discrimination through the manipulation of starting salaries makes little sense, not only because its visibility may threaten worker morale, but because the same end may be achieved gradually through differentials in salary advances that may more plausibly be attributed to variations in performance. Furthermore, the idea that religious inequalities are the result of the gradual accumulation over time of incremental salary differences at least leaves open the possibility, not to be <u>automatically</u> dismissed, that overall salary differences are associated with real or 'honestly' imagined performance variations. Hence, whether discrimination or performance differences is the operative cause of religious inequalities, an explanation based on differential rates of salary increase rather than on differential starting salaries has greater appeal.

The above reasoning is confirmed by the estimates of the full model given in column b of Table 3. The effect coefficients of the religious group categories given in the upper and lower panels together indicate that the overall salary advantage enjoyed by High Protestants is due to favorable disparities in the rates of salary increase rather than in starting salary. Indeed, the coefficients for starting salary suggest that High Protestants are at a slight disadvantage in this respect, though these main effects are not statistically significant ($F = 0.81$). The statistically significant religious-group differences detected by the partial model are actually a reflection of differences not in starting salary but in the rate of increase in salary. The relevant coefficients are .0045, -.0005 and -.0040 for High Protestants, Low Protestants and Catholics, respectively. This means that, with respect to the annual rate of salary increase, High Protestants have an advantage of .5% over Low Protestants and .85% over Catholics, not trivial amounts when we recall that the average annual rate of increase is only 1.84%.

The results pertaining to the effect of education on salary lend themselves to a more straightforward interpretation. The coefficients for the full model indicate, as we would expect, that the more educated males enjoy a significant advantage over their less educated counterparts in starting salary (.0338), as well as a slight (though statistically nonsignificant) advantage (.0017) in the rate of salary increase. We conclude tentatively that the effect of education on current salary is transmitted mainly via starting salary and registered at the time of employment.

Finally, we consider the impact on current salary of having held previous positions in other companies. Here again a comparision of the partial and full models turns out to be especially revealing. For the partial model (column b', Table 3) the coefficient of previous positions is a mere .0039, slightly less than its standard error (.0043). On the basis of this result one might conclude that previous experience is not a factor in the determination of salary. However, the relevant coefficients of the full model indicate that such a conclusion would be extremely misleading. In fact, previous positions held in other companies yield a significant advantage in starting salary (.0293) and an equally significant disadvantage in the rate of salary increase (-.0024). This means that men entering the firm from other companies enjoy a higher starting salary at the time of employment, but a lower rate of increase in salary thereafter. Over time these positive and negative effects of previous positions tend to cancel out, so that -- as a more accurate reading of the coefficient of the partial model indicates -- variation in experience has no overall net impact on current salary.[12]

Why the direction of the effects of previous positions on starting salary and rate of salary increase should differ is a little hard to explain. One possibility, admittedly speculative, is that the firm gradually adjusts the salary advances of experienced employees downward to make up for starting salaries in excess of 'value' that were offered in order to successfully lure men from other companies. A firm that suddenly finds itself in need of experienced men would be willing to take the short-term loss entailed by higher-than-average starting salaries if it knows that the loss can be recouped in the long run. Another possibility, not inconsistent with this reasoning, is that the firm slows down the rate of increase in the salaries of experienced men in order to allow similar men who started with company to catch-up, thereby eliminating any potentially disruptive inequities that may have developed.

In addition to these substantive interpretations there is, of course, a third possible explanation: the negative coefficient of the interaction involving previous positions and length of service actually represents a period effect, namely, that the effect of experience on starting salary is greater for younger cohorts than for older cohorts. Obviously, an analogous interpretation may be imposed on the interaction terms involving religious group membership and education. The issues entailed by this alternative forces us to go beyond the interpretative framework employed to this point.

DETAILED ANALYSIS

Up to this point our analysis has relied exclusively on an interpretation which treats the data as if they represented a historical time series. To the coefficients of the interaction terms involving length of service and personal attributes we have brought a synthetic cohort interpretation. That is, differences between newcomers and oldtimers in the effect of a particular personal characteristic on salary have been attributed to changes occurring during the course of the organizational career rather than to differences in effects between the period in which oldtimers started and the period in which newcomers started.[13] Hence, the interaction coefficients have been interpreted in terms of the rate of increase in salary rather than in terms of interperiod variation in the effect of personal attributes on starting salary. We could be wrong. While the available data preclude distinguishing unequivocally between these competing interpretations, grounds for plausible inference are not entirely lacking.

The problem of inference stems from the absence of data on actual starting salary. In our analysis starting salary and the effects of personal attributes on it are an implication of the model rather than observed phenomena. But with data on actual starting salary we could readily assess the magnitude of period effects on starting salary. For example, we would be able to determine whether current salary differences among oldtimers that are induced by variation in some personal characteristic and that exceed similar differences among newcomers -- thereby giving rise to a positive interaction coefficient -- were present when oldtimers started their careers (period effects) or developed thereafter. Equivalently, we could assess directly the magnitude and direction of period effects on the differences in starting salary induced by personal characteristics.

Now data on starting salary are not available, but we do have information on the level in the firm hierarchy at which each respondent began his career.[14] Theoretically, entry-level position should be an excellent surrogate for starting salary, since salaries are often directly tied to positions and based on the hierarchical location of them. Evidence indicating that this is true in the present case is provided by the fact that <u>current</u> hierarchical position and current salary are correlated .78. We conclude that the level in the hierarchy at which a man started in the organization (hereafter referred to as his entry level) is a valid indicator of his starting salary.[15] This variable can now be used to assess the competing interpretations of the interactions of religious group membership, education and previous positions with length of service, taking each in turn.

<u>Religious Group Membership</u>. We want to explain the pattern of coefficients associated with the interactions involving the religious group

categories and length of service in the full model (Table 3). Empirically, the coefficients indicate that the magnitude and direction of religious group salary differences vary considerably between newcomers and oldtimers. The nature of this difference is well-illustrated by a comparison of High Protestants and Catholics. In the youngest length of service group the actual figures show that High Protestants earn 4.25% less than Catholics; in the oldest length of service group they earn 15.88% more than Catholics.

Our synthetic cohort interpretation assumes that religious differences in starting salary at the time oldtimers began their careers resemble the comparable differences in current salary among newcomers, so that the advantage in current salary enjoyed by High Protestant oldtimers reflects an over-time shift resulting from their higher rate of increase in salary. In contrast, the competing explanation in terms of period effects attributes the higher current salaries of High Protestant compared to Low Protestant and Catholic oldtimers not to disparities in the rate of salary increase, but to the persistence of a pattern of religious differences in starting salary which existed during the period oldtimers started their careers but which was no longer operative during the period newcomers entered the firm.

A test that distinguishes between these two interpretations may be fashioned by examining how religious differences in entry level depend on the period during which a man was hired. This test may be carried out by regressing entry level on length of service, the religious group categories and a set of interaction terms. In this context length of service represents not the time elapsed since entry to the firm but rather the historical time period during which entry occurred. Hence, high values of length of service indicate that entry occurred in the 'distant' past, while low values indicate that entry occurred in the more recent past. The coefficients of interaction terms involving length of service and some personal characteristic -- in this case religious group membership -- reveal the manner in which the effect of the personal attribute on entry level depends on <u>when</u> a man was hired. The synthetic cohort interpretation predicts that the coefficients of the interactions involving the religious group categories will not significantly depart from zero, thereby indicating the absence of a historical trend in the effect of religion on entry level (and, by assumption, on starting salary). The period-effects hypothesis predicts that these same coefficients will significantly depart from zero <u>and</u> that the coefficient for High Protestants will exceed the coefficients for Low Protestants and Catholics.

Table 4 gives the pertinent results, showing the coefficients obtained by regressing entry level on all terms in the full model of current salary excepting those involving family origins and nativity. The relevant terms are, of course, those involving religious group membership. The first thing we notice is that the coefficients corresponding to the main effect of religion do not significantly depart from zero (F = 0.99). This is consistent with our earlier observation that religious group membership does not affect starting salary. Although we are dealing with very small differences, we do note that these coefficients indicate that High Protestants have an advantage over Low Protestants and Catholics (.140 compared to .023 and -.163, respectively) in entry level, while the starting salary

Table 4. Regression coefficients for the regression of entry level on length of service, starting age, religious group membership, education, previous positions and interactions involving length of service, male managers of a utility firm, 1962.

Variables	b[1]	beta	t	F
Length of Service	.148 (.029)	.624	5.17*	
Starting Age	.017 (.004)	.138	4.42*	
Religion				0.99
Hi Prot	.140 (.111)			
Lo Prot	.023 (.117)			
Catholic	-.163			
Education	.669 (.072)	.638	9.31*	
Previous Positions	.217 (.053)	.312	4.08*	
Length of Service X				
Religion				0.58
Hi Prot	-.009 (.009)			
Lo Prot	-.002 (.010)			
Catholic	.011			
Education	-.043 (.006)	-.739	7.15*	
Previous Positions	-.016 (.004)	-.308	3.75*	
Number of Cases	988			
R^2	.221			27.65

[1]Appearing below the coefficients are the standard errors.

*$p < .01$

coefficients from the full model (Table 3) indicate High Protestants are at a disadvantage. Secondly, and more to the heart of the issue, the coefficients of the religion interaction terms reveal no significant (F = 0.58) differences between periods in the effect of religious group membership on entry level. From this we infer that High Protestants hired in the distant past (current oldtimers) did not, as the period-effects hypothesis would have it, enjoy an advantage in starting salary that has been withheld from more recent High Protestant recruits. In other words, the actual religious group differences in starting salary among those men who are now oldtimers probably closely resemble the religious differences in current salary among current newcomers, thus lending support to our synthetic cohort interpretation of the full model of current salary.

But the results in Table 4 suggest something else as well. Not only do they support the inference of substantial religious disparities in the rate of salary increase, but they indicate that the full model actually underestimates these disparities. Note that the pattern of coefficients for the religious interaction terms indicate that the slight overall advantage in entry level enjoyed by High Protestants was <u>less</u> for those entering in the distant past than for those entering more recently. Assuming that, within a comfortable range, what holds for entry level holds for starting salary, introducing controls for entry level to our model of current salary should increase religious differences in the rate of increase in salary. Table 5, which gives the estimates of a revised model (family origins and nativity have been dropped) of current salary, shows that this is just what happens.[16] Comparison of the coefficients of

Table 5. Revised estimates of the coefficients of the regression of salary on selected personal attributes, without and with controls for entry level; male managers of a utility firm, 1962.

	Without Entry Level				With Entry Level			
Variables	b[1]	beta	t	F	b	beta	t	F
	Starting Salary				Starting Salary			
Average	8.542				8.988			
Age	.0030	.118	3.84**		.0022	.086	2.86**	
	(.0008)				(.0008)			
Religion				1.14				1.77
Hi Prot	-.0274				-.0339			
	(.0220)				(.0214)			
Lo Prot	-.0100				-.0111			
	(.0232)				(.0226)			
Catholic	.0374				.0450			
Education	.0304	.143	2.13*		-.0006	-.003	0.04	
	(.0143)				(.0145)			
Previous Positions	.0323	.230	3.05**		.0222	.158	2.14*	
	(.0106)				(.0104)			
Entry Level					.0463	.228	7.49	
					(.0062)			
	Rate of Increase				Rate of Increase			
Average	.0171	.357	3.01**		.0103	.214	1.82	
	(.0057)				(.0056)			
Religion				3.30*				4.18*
Hi Prot	.0048				.0052			
	(.0019)				(.0018)			
Lo Prot	-.0004				-.0003			
	(.0020)				(.0019)			
Catholic	-.0044				-.0049			
Education	.0020	.172	1.69		.0040	.341	3.36**	
	(.0012)				(.0012)			
Previous Positions	-.0026	-.241	2.98**		-.0018	-.170	2.15*	
	(.0009)				(.0008)			
Number of Cases	988				988			
R^2	.245			31.59	.286			35.44
Incremental R^2 Due to Length of Service Interactions[2]	.018			5.81**	.023			7.90**

[1] Appearing in parentheses below the coefficients are the standard errors.

[2] This gives the total net contribution of those terms representing the effect of religious group membership, education and previous positions on the rate of salary increase, i.e., the terms for the interaction of each of these characteristics with length of service.

*p < .05

**p < .01

the religion interactions in the lower panel shows that the effect of religion on the rate of salary increase remains significant and is larger with controls for entry level, the difference between High Protestants and Catholics going from .92 to 1.01.

All of this evidence lends strong support to our synthetic cohort interpretation of the effects of religious group membership on current salary. Our original model of current salary attributed the higher earnings of High Protestants relative to Low Protestants and Catholics to inequalities in the rate of salary increase rather than to disparities in starting salary; we have seen nothing that would lead us to retreat from this conclusion. It appears that High Protestants achieve their advantage through mechanisms built into the process of salary advancement itself, rather than through inequities in starting salaries set at the time of employment. The fact that the salary advantage of High Protestants is significantly greater among oldtimers cannot be explained by period differences in the effect of religion on starting salary, for such differences do not exist. There is, then, no evidence for a pattern of localized discrimination confined to the hiring process. Rather, if discriminatory salary practices are the root cause of religious inequalities in this firm, they appear to operate during the organizational career proper, and thus in a context in which they may be easily disguised as performance judgements.

Previous Positions. A method of analysis analogous to that used in the previous section may be employed to explore the meaning of the negative coefficient associated with the interaction term involving previous positions and length of service in the full model of salary (Table 3). Empirically, this negative coefficient merely records the fact that the salary advantage of men with previous experience is significantly less among oldtimers than among newcomers. The synthetic cohort interpretation of this fact assumes that experienced oldtimers began their careers with an advantage in starting salary comparable to the advantage in current salary enjoyed by newcomers, so that the absence of an advantage in current salary for experienced oldtimers is attributable to an over-time shift resulting from their lower (compared to their less experienced counterparts) rate of increase in salary. The competing interpretation of the same fact argues in terms of period effects on starting salary, namely, that previous organizational experience counted less in determining starting salary when current oldtimers began their careers than when newcomers began theirs.

To distinguish between these interpretations we can consider how the effect of experience on entry level -- and thus on starting salary -- depends on the period during which a man was hired. As before, this entails including an interaction term involving previous positions and length of service in an equation predicting entry level. The synthetic cohort explanation predicts that the coefficient of this term will be equal to or less than zero, thereby indicating the absence of an historical trend in the direction of increasing effects of experience on starting salary. The period-effect hypothesis predicts a coefficient greater than zero, thereby indicating the presence of the hypothesized historical trend.

The pertinent results appear in Table 5. We note first that the coefficient representing the main effect of previous positions (.217) is positive, indicating that experience yields a higher entry level position and thus a higher starting salary, a conclusion consistent with that arrived at on the basis of the full model of salary. Secondly, the coefficient of the interaction between length of service and previous positions is a statistically significant -.016, indicating that in fact previous experience did yield less of an advantage in entry level (and thus in starting salary) at the time current oldtimers took employment than in more recent periods. This is, of course, strong evidence in favor of the period-effects hypothesis, but is not enough to warrant outright rejection of the synthetic cohort interpretation. Though it is now clear that part of the difference between newcomers and oldtimers in the effect of previous positions on current salary can be accounted for by inter-period variation in the effect of experience on starting salary, it does not follow that <u>all</u> of the difference can be accounted for in this manner. There may still be room left for the operation of the negative effect of experience on the rate of salary increase hypothesized by the synthetic-cohort model. To be sure, by failing to take account of period effects we necessarily overestimate the magnitude of this negative effect, but it may still be nonnegligible. If this issue is to be resolved we must take the extra step of introducing entry level as a control in our model of current salary; nearly clinching evidence in favor of the period-effects hypothesis would entail not merely the reduction in magnitude (which we now know must occur) but the disappearance altogether of the coefficient of the interaction between length of service and previous positions (i.e., $b_{s,p} = 0$).

The results in Table 5 show that this does not occur. On the left hand side of the lower half of this table we see that without controls for entry level the coefficient of the interaction of length of service and previous positions is -.0026; controls for entry level reduces this to -.0018, a value which is statistically significant. Hence, controlling for entry level explains part but not all of the difference between oldtimers and newcomers in the variation in current salary attributable to previous positions. Unfortunately, what remains (-.0018) cannot unambiguously be assigned to the synthetic cohort model, that is, to the negative effect of previous organizational experience on the rate of increase in salary. This is because the entry level variable is, with respect to assessing the effects of experience, a less than perfect surrogate for starting salary. This is revealed by the fact that the coefficient indicating the effect of previous positions on starting salary (.0222) is still significant even with controls for entry level. This means that previous experience induces variation in starting salary above and beyond that induced via entry level. It is here, in this residual variation in starting salary, that the bulk of the period effects we seek may reside. Without information on starting salary per se it is impossible to say with a reasonable degree of certainty.

We are, then, at an impasse. We have pursued two possible interpretations of a single coefficient as far (and by some lights perhaps too far) as the data allow. To some degree our failure to obtain conclusive results almost seems appropriate, since the contending interpretations are based on equally plausible hypotheses. In fact, one could argue that the hypotheses are logically linked, and stand or fall together. Recall that the synthetic-cohort explanation claims that experience has a negative effect on the rate of salary increase, the reason being that firms which need experienced men and are forced to pay starting salaries in excess of "true" value in order to lure recruits from other firms must thereafter adjust rates of increase downward in order to restore equilibrium. The period effects hypothesis claims, essentially, that the positive effect of experience on starting salary is subject to fluctuations as a firm passes through time. These hypotheses are rendered consistent by the plausible conjecture that those periods during which experience has a high positive effect on starting salary occur at the very time that firms most demand experienced personnel and are most willing to pay excessive starting salaries. It is this unusual form of collinearity that may explain our inability to convincingly distinguish the two interpretations.

<u>Education</u>. Recall that the results pertaining to the original model of current salary (Table 3) showed a positive (though statistically nonsignificant) coefficient for the interaction of length of service and education. Empirically, this positive coefficient merely records the fact that differences in current salary induced by education are slightly greater among oldtimers than among newcomers. The synthetic cohort interpretation takes this to indicate that educated managers increase their salaries at a slightly faster rate then their less educated counterparts. The period-effects hypothesis takes this same fact to indicate that education counted slightly more in determining starting salary when oldtimers began their careers than when newcomers began theirs. One

possible rationale for this latter hypothesis might be that the supply of educated personnel was lower in the 'distant' past when oldtimers began, thereby enhancing the value of education.

The problem with the period-effects hypothesis -- and the thing that makes the analysis of this section different from that of the previous sections -- is that it is unconvincing on its face. If anything, technological changes and the rise of credentialism in the last few decades have made education a more important determinant of starting salary than it was in the past. If this is the case -- that is, if education has a greater effect on the current salary of newcomers than it had on the starting salaries of oldtimers when they began their careers -- then it suggests not then only that the synthetic cohort interpretation is correct, but that the full model of salary actually underestimates the positive effect of education on the rate of increase in salary.[17]

Evidence bearing on these issues is presented in Table 4, where again interest centers on the coefficient of the interaction of length of service and education in the equation predicting entry level. We see that this coefficient is a statistically significant -.043. This means that, contrary to the period-effect hypothesis, education actually had less of an effect on entry level, and thus on starting salary, during the period oldtimers took employment than during more recent periods. In other words, the trend appears to be in the direction of increasing effects of education on starting salary. This means that the synthetic-cohort interpretation of the full model of salary may be maintained, and that education may be regarded as having a positive effect on the rate of increase in salary. Furthermore, this evidence implies that we have underestimated the true magnitude of this education effect, so that controlling for entry level should result in an increase in the coefficient representing this effect.

Table 5 shows the expected pattern. Without controls for entry level the effect of education on the rate of salary increase is .0020; with controls it doubles to .0040, and is now statistically significant. Moreover, the right-hand-side of the upper panel of Table 5 shows that the main effect coefficient of education is virtually zero, which means that all of the (implied) effect of education on starting salary is transmitted via entry level. This suggests that, at least with respect to assessing the effect of education, entry level is a very good proxy for starting salary. Substantively this means that, in contrast to what we observed for previous organizational experience, education does not yield any advantage in starting salary beyond that associated with entering the firm at a higher hierarchical level.

In summary, the findings of this section point to the operation of a two-part process by which educated managers achieve an overall net advantage in current salary. Initially, educated men begin their careers at higher hierarchical levels and earn concomitantly higher starting salaries than their less educated counterparts. Secondly, whether they begin at higher levels or not, educated managers tend to increase their salaries at a faster rate. Unfortunately, none of the evidence examined here can tell us why more educated managers tend to earn a higher rate of increase in salary. Whether this effect of education is transmitted via actual performance, via the affective traits education imparts (Gintis, 1971), or via certification requirements built into the job structure of the firm, is an unsettled issue.

CONCLUSIONS AND DISCUSSION

In this paper I have examined the effect of certain achievement-related personal characteristics on firm-specific variation in the salary attainment of white male managers of a utility company. Although the analysis was based on cross-sectional data, the statistical methods employed treat the data as if they were a historical time series. With suitably strong assumptions, these methods allow for an assessment of the degree to which the effects of personal characteristics on current salary are transmitted via starting salary and/or via the rate of salary increase. Before considering the more problematic aspects of this type of analysis, we may briefly describe the findings regarding current salary and about which there is little question.

On the negative side, neither family background (father's occupation and education) nor nativity (local, extralocal or foreign born) had a significant net direct effect on current salary. Calculations not reported here show, however, that both variables have an indirect effect. As one would expect, a positive indirect effect of family background is transmitted through education. Similarly, there are significant gross earnings differences among the nativity groups, with local men earning less than extralocal and foreign men, but this is attributable mainly to the younger starting age of local men.

Education, age, and, of course, length of service have positive net effects on current salary. Previous organizational experience also has significant effects on salary processes, but because of the special nature of these effects (positive and negative effects cancel out) there is no overall net difference in the current salaries of managers who start their careers with more or less prior experience. Finally, the significant salary differences associated with religious group membership point to a system of allocation of economic rewards that favors High Protestants at the expense of Low Protestants and Catholics.

These conclusions are based on a straightforward reading of the empirical evidence. Somewhat less straightforward and more problematic are our inferences regarding the manner in which the effects on current salary are registered. For religious group membership, previous organizational experience and education, we tried to distinguish between effects on starting salary and effects on the rate of salary increase. In the absence of data on starting salary, making this distinction requires a synthetic cohort interpretation of cross-sectional differences among length of service groups. Specifically, this means that significant differences between newcomers and oldtimers in the effect of a given personal characteristic on current salary are interpreted in terms of effects on the rate of increase in salary. The difficulty involved in making this interpretation arises because these same differences between oldtimers and newcomers may be used to support the hypothesis of interperiod or temporal differences in the effect of personal characteristics on starting salary. To distinguish between these contending interpretations we tested for interperiod differences in the effect of religious group membership, previous experience and education on the level in the firm hierarchy at which a manager started his career. We reasoned that any explanation based on period effects on the determinants of starting salary must be consistent with observed period effects for entry level, since the latter is one of the primary determinants of starting salary.

The first conclusion to emerge from the application of this two-step mode of analysis concerned the nature of religious inequalities in salary. The results suggest that religious inequalities are rooted in the process of salary attainment itself rather than confined to the hiring process. The observed overall economic advantage of High Protestants appears to be due exclusively to their higher rate of salary increase rather than to an advantage in starting salary bestowed at the time of employment. We could find no support for an explanation couched in terms of temporal variation in the effect of religion on starting salary. Such an explanation -- which calls for a historical trend in the direction of decreasing religious discrimination in starting salary -- is inconsistent with the fact that no significant temporal differences in the effect of religion on entry level were found.

The results pertaining to previous organizational experience were more mixed than this. On the one hand, we can be fairly confident about the findings which concern starting salary. First, prior experience in other companies yields a positive advantage in starting salary, not least of all because experienced men tend to begin at higher hierarchical levels than their less experienced counterparts. But only part of the economic advantage in starting salary enjoyed by experienced men is due to their higher entry level; even taking account of their hierarchical advantage, men coming from other companies still appear to get higher starting salaries than less experienced men. Secondly, the effect of experience on entry level and presumably starting salary is subject to temporal fluctuation. In the firm studied there has been a significant trend in the direction of increasing returns to experience, with the affect of prior experience on entry level being greater for younger than for older cohorts.

Much less conclusive are the results bearing on the rate of increase in salary. Because not all of the effect of experience on starting salary is accounted for by entry level, and because there is evidence for temporal differences in the effect of experience on entry level, we cannot unequivocally attribute the difference between newcomers and oldtimers in the effect of experience on current salary to the negative impact of experience on the rate of increase in salary. To be sure, such a negative effect is theoretically plausible, and evidence favoring it appeared even when entry level was controlled, but matters remain clouded by the slippage in the relationship between entry level and starting salary. With an exact measure of starting salary, we might find that all of the difference between length of service groups in the effect of experience on current salary might be explained by the period effects hypothesis. In any event, this entire issue may be moot. As observed earlier, certain theoretical considerations suggest that temporal fluctuation in the effect of experience on starting salary may coincide, in an empirical sense, with a negative effect of experience on the rate of increase in salary.

Finally, the results point to the operation of a two-part process by which educated men achieve an overall economic advantage over their less educated counterparts. First, education has a positive impact on starting salary, an impact that appears to be due exclusively to the higher hierarchical level at which educated men start their organizational careers. Secondly, the more educated managers increase their salary at a faster rate than less educated managers. That this was only weakly evident in our initial set of results was due to the absence of controls for the trend toward increasing effects of education on entry level and starting salary. When differences in entry level are controlled, the positive impact of education on the rate of increase in salary clearly emerges.

Any overall assessment of these findings must be tempered by a good deal of caution, especially as regards their external validity. Virtually every conclusion is subject to the proviso "at least in this utility firm during this span of time." While we seriously doubt that our results reflect the unique effects of firm, industry or region -- either singly or in combination -- only future research will tell. Similarly, many of our inferences are limited to the period prior to 1946 up to 1960. On this count there is the strong suspicion -- though difficult to document -- that our findings, especially as concerns the effects of religion and perhaps education, are historically bounded. One of the benefits derived from using data that is somewhat dated is that this suspicion is subject to test as data from more recent periods begins to accumulate.

Finally, we cannot overlook the problems of internal validity that hinder all analyses of this type. Obviously, internal validity is always an issue in synthetic cohort analyses, although in our case the problems are mitigated by our ability to marshall retrospective data on entry level. Somewhat less tractable -- and, perhaps for this reason, never mentioned in analyses of organizational attainment -- are the problems engendered by the lack of closure of the study population. By this we mean, simply, that inferences drawn from studies of organizational attainment are necessarily confined to persons employed by the organization at the time of the study; statements about the attainment process governing the careers of those who have left the organization are precluded, since data on the characteristics of these persons is never obtained. If the parameters governing the attainment of persons who remain and have left are similar, then the results of the kind of analysis presented here may be validly interpreted in terms of the <u>organizational system</u> of allocation of rewards. However, sharp differences in the two processes would mean that statements about the attainment of those persons currently employed are one step removed from -- or biased approximations of -- statements about the organizational system of allocation per se. Short of major and expensive improvements in research design, there is no way of finessing this problem.

FOOTNOTES

1. Models of organizational career attainment have been constructed for departments of the Canadian Federal administration (Beattie and Spencer, 1971), an automotive manufacturing firm (Wise, 1975), the American Catholic Church (Peterson, 1976), a Japanese shipbuilding firm and Japanese electrical company (Marsh and Mannari, 1976).

2. We intentionally shy away from hard and fast distinctions which automatically classify attributes as achieved or ascribed. We do so because we wish to avoid the convention which views achieved but not ascribed characteristics as related to actual performance. Questions concerning the mechanisms by which personal attributes get translated into rewards raise issues that are more unsettled than this convention suggests.

3. The exclusion of these commitment variables should not seriously bias parameter estimates obtained for the predetermined variables, since these two sets of factors are only weakly correlated.

4. Listwise deletion was used to handle missing observations; this reduced the sample size for all regressions here reported to 988. Results obtained using pairwise deletion were comparable in every way to those reported here, but I prefer listwise deletion because it facilitates comparisons between equations.

5. The status scores for father's occupation are: professional and semi-professional -- 75; managers, officials and proprietors -- 57; clerical and sales -- 47; skilled craftsmen and foremen -- 31; operatives -- 18; service -- 17; farmers -- 12. A dummy variable for farm origins was tried but eventually dropped becouse it was inconsequential.

6. The High Protestant category includes Episcopalian, Congregationalist, Methodist, Presbyterian and Mormon; Low Protestant includes Baptist, Lutheran, Fundamentalist and "other" Protestant. The ordinal designation of these categories conforms to scores on Laumann's ethnoreligious status scale (Laumann and Segal, 1971: Table 1).

7. The slippage introduced by the fact that current age and length of service are based on grouped data necessarily makes the measure of starting age a rough estimate.

8. This means that investigators employing equation (4) must be careful about their conclusions regarding the relative effect of age and length of service. For example, if the coefficient of current age exceeds the coefficient of length of service, one may be tempted to conclude that age is more important. In fact, just the reverse is usually the case. Since the coefficients of equation (4) are always positive in practice, b_1 must exceed b_2a_1.

9. For the purpose of analysis we must assume that the salary <u>structure</u> of the firm has remained fairly stable in the decades prior to the survey.

10. These constraints serve to express the coefficients of the categorical variables as deviations from the grand mean of the dependent variable rather than from the omitted category.

11. This statement in parentheses is not quite accurate. Because the data on length of service are grouped, we are speaking loosely when we say that current salary is equal to starting salary for the lowest length of service group. Indeed, because zero is not within the range of observed values of length of service, our statements about starting salary are doubly tentative.

12. Obviously, no general conclusions can be drawn from this cancelling out of effects. This merely reflects the particular composition of this firm with respect to the joint distribution of experience and length of service.

13. The historical periods to which we refer in this section are: prior to 1946, 1946-50, 1951-55, 1956-60. These dates are based on a simple transformation of the length of service intervals, taking 1962=0. However, for the analysis that follows historical time is treated linearly rather than as a set of discrete periods. We do this because the so-called period effects we seek must obey a linear trend in order to account for the interaction terms, all of which are linear in length of service.

14. Entry level is measured as a seven-point scale indicating the hierarchical position of a respondent's first job in the corporation. Positions were assigned to hierarchical levels according to the authority and responsibility associated with them. Scale values were assigned to hierarchical levels in ascending order, with the lowest level assigned 1. For details see Grusky(1966:491).

15. We are assuming, of course, that the relation between hierarchical level and salary has not changed dramatically in the decades preceding the survey.

16. This regression was also run with a term for the interaction of length of service and entry level. The coefficient of this term was zero, and the coefficients of other terms were similar in all relevant ways to those reported here.

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