APPENDIX The stacked data-set.

The following data-sets have been used in the analysis:

Name of study - year of data collection (principal investigators, number in Steinmetz Archives)	number of cases in educational cohorts		number of cases in labour market cohorts	
Nationaal Election Study 1970 (Stouthard a.o., P0136)	558	6.9%	732	7.7%
Seven Nations Study 1971 (Irving and Molleman, not in Steinmetz Archives)	294	3.6%	380	4.0%
Income Satisfaction 1976 (Hermkens and Van Wijngaarden, P0653)	581	7.2%	641	6.7%
Situation of Living Survey 1977 (Central Bureau of Statistics, P0328)	1201	14.8%	1517	15.9%
National Election Study 1977 (Working Group NES, P0354)	464	5.7%	629	6.6%
Political Action, second survey 1979 (Barnes and Kaase, P0823)	271	3.4%	352	3.7%
National Election Study 1981 (Working Group NES, P0350)	621	7.7%	762	8.0%
National Election Stuy 1982 (Working Group NES, P0633)	481	5.9%	551	5.8%
Mobility Study 1982 (Ultee and Sixma, P0839)	398	4.9%	427	4.5%
NPAO Labour Market Survey 1982 (Heinen and Maas, P0748)	690	8.5%	739	7.7%
OSA Labour Market Survey 1985 (OSA, not in Steinmetz Archives)	1455	18.0%	1596	16.7%
SOCON project 1985 (SOCON, not in Steinmetz Archives)	313	3.9%	350	3.7%
National Election Study 1986 (Working Group NES, P0866)	488	6.0%	554	5.8%
Income Satisfaction 1987 (Hermkens and Van Wijngaarden, not in Steinmetz Archives)	281	3.5%	318	3.3%
Total	8096	100.0%	9548	100.0%

[a] Selection criterium: men, 12 year old between 1929 and 1970, older than 25 years at the date of survey, complete information on father's occupational status and educational attainment.

[b] Selection criterium: men, 12 year old between 1929 and 1970, older than 25 years at the date of survey, complete information on father's occupational status and educational attainment.

Chapter 17

PRELIMINARY RESULTS ON EDUCATIONAL EXPANSION AND EDUCATIONAL ACHIEVEMENT IN COMPARATIVE PERSPECTIVE[1]

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17.1. Introduction

Ever since the introduction of the basic status attainment model by Blau and Duncan (1967), it has been clear that educational attainment is the main force that drives the process of stratification. Education's role is two-fold: on the one hand, it is the main way by which a person qualifies for status positions in modern society. On the other hand, it is the main vehicle by which family status is transferred from generation to generation.

Students of comparative intergenerational stratification patterns are therefore well advised to make the analysis of educational opportunity (i.e. the relationship between educational attainment and family background) one of their main concerns. In this chapter we present an analysis of a large body of empirical data on the determinants of educational attainment around the world. This chapter follows from our previous analysis of intergenerational class mobility (Ganzeboom, Luijkx, and Treiman, 1989). The main finding of that paper was unequivocal evidence of significant between-country variation in class mobility patterns and a virtually ubiquitous increase in class mobility over time. The present chapter addresses a possible explanation for that result--the expansion of education in virtually all countries. Here we restrict ourselves to a regression analysis of educational attainment. In future work, we expect to supplement the analysis reported here with an analysis of continuation ratios for educational careers, as well as multivariate analyses of occupational attainment.

17.1.1. Theory

In modern research, two modes of analysis have been used to analyze the relationship between social origins and educational attainment. The older approach is to regress a metric measure of educational attainment on a set of background variables via OLS regression. This approach was pioneered by Blau and Duncan (1967) and replaced tabular approaches. The metric regression model assumes that the dependent variable, educational attainment, can be represented adequately by a metric variable (usually, but not necessarily, years of education) and that the relationship between social origin variables and successive levels of educational attainment is linear (or smoothly curvilinear). A second approach was introduced by Mare (1980, 1981)--partly in response to and as a critique of Boudon's (1974) simulation approach (see also Hauser, 1976; Boudon, 1976). Mare separated the educational career into a set of successive transitions, e.g. from primary school to secondary school. The relative odds of making each transition, given that one had successfully made the previous transition, were then assessed for people from different social origins, using logistic regression procedures. In Mare's model, there is no assumption that the associations between social origins and the odds of making a transition are the same for different transitions, and indeed, in general, they are not.

The two models give different but reconcilable accounts of the structure of educational opportunity and of historical trends therein. Mare's basic result, which has been widely replicated (Shavit and Blossfeld, 1993), is that the association between social origins and the likelihood of making a transition decreases for successive transitions; that is, the relative odds of making the transition from primary to secondary schooling are more strongly dependent upon social origins than are the relative odds of making the transition from secondary to tertiary schooling. This result helps to explain the historical decline in the dependence of educational attainment on social origins observed in many (but not all) countries. If the effect of social origins on the odds of making each transition remain constant over time, it will under certain circumstances (specifically, the absence of an increase in the variance of education)[2] be the case that as the average level of education increases the overall dependence of educational attainment on social origins decreases. Since education has expanded in virtually all countries in the world, this result leads us to expect, *ceteris paribus*, a decline in the dependence of education on social origins.

However, other things need to be equal, else other patterns may occur. For example, Mare (1981) found in the U.S. an increase over time in the effect of social background on each of the transitions and, consequently, historically stable metric regressions (since the two trends just offset each other). Others have found stable social background effects on each of the transitions and hence declining regressions (e.g., Smith and Cheung, 1986). Still others have found historically declining social background effects at some transitions and stable and increasing social background effects on others (Simkus and Andorka, 1982; De Graaf and Ganzeboom, 1990), with the secular decrease strongest at the earliest transitions and absent or reversed for the later transitions. Of the 13 countries included in the Shavit-Blossfeld volume (1993), the effect of father's education declined in about half and remained unchanged in the remainder (except for Czechoslovakia, where the effect first declined then increased); and the effect of father's occupational status remained unchanged in nine of the 13 countries, declining in three and increasing in one (Italy) (Blossfeld and Shavit, 1993:16). In sum, depending upon the specific combination of the distribution of educational attainment and the relative sizes of the social background effects at each of the transitions, the outcomes have been (with the partial exception of Italy[3] and Czechoslovakia) either historically decreasing or stable metric effects of social background.

The introduction of the model of the educational career as a sequence of educational transitions has led to some confusion among analysts about the validity of the metric regression model as a predictor of educational attainment, and some have no longer bothered to present results from the metric regression model (e.g. Shavit and Kraus, 1990). We disagree strongly with this position. In our opinion, Mare's model yields important insights about the mechanics of educational opportunity, and for that matter about the most important mechanism of social mobility and social reproduction. Specifically, Mare's model explains why, other things being equal, a general increase of educational attainment promotes social mobility, as was anticipated some time ago (Treiman, 1970). This should not obscure the fact that the parameters of the metric regression model (or some other measure of the association of educational attainment with social origins), and changes in these parameters over time, are of fundamental importance, because these are what directly measure the degree of educational inequality in a society. For this reason, we will focus in this chapter entirely on the metric relationships between educational attainment and social background. As we have noted, a future chapter will consider models of educational transition.

The main research problem of this chapter is to estimate the quantitative relationship between the average years of education completed in a society at a particular point in time and the effect of social background indicators on the level of education achieved by individuals in that society. While Mare's model implies that such a macro-level relationship exists, the direction and magnitude of the relationship depends upon empirical contingencies: the county- and cohort-specific educational distributions and the levels of social inequality at each transition. We will estimate the exact quantitative relationship across cohorts and societies from our data. Our expectation is that when the variance in educational attainment remains constant, an increase in the average level of education reduces the effect of social origins on educational attainment; however, when the average level of education remains constant, an increase in the variance in education increases the effect of social origins on educational attainment.

17.2. Data

The data we use in this chapter are from 115 data files obtained from surveys conducted in 29 countries throughout the world.[4] Appendix 1 gives an overview of the surveys included in the analysis: the country, the year the survey was conducted, and the number of men (and women) included. These surveys are drawn from the International Stratification and Mobility File (ISMF), which we are continuously updating (see Ganzeboom, Luijkx, and Treiman, 1989; Ganzeboom, De Graaf, and Treiman, 1992). Two criteria govern inclusion of files in the ISMF: they must be based on a probability samples of a national (or regional)[5] population (or labor force) and they must include information on father's (and respondent's) occupation.

Although industrialized Western nations are--not surprisingly--over-represented among the 29 countries included here, we do have data from one industrialized non-Western nation (Japan), from three Eastern European countries (Czechoslovakia, Hungary, and Poland), and from six developing nations (Brazil, India, Malaysia, the Philippines, Taiwan, and Turkey). Hence, our conclusions can reasonably be taken as representing universal patterns and need not be regarded as holding only for industrially developed Euro-American capitalist nations. Most of our data sets are from the 1970s, but they range from the 1955 Japanese National Mobility Survey to the 1988 U.S. NORC General Social Survey. Since all of our analysis is cohort specific, we need not be concerned that country differences in the date at which surveys were conducted will distort our results.

We restrict our analysis to men age 25-64 for whom we have complete information on educational attainment and on father's education *and/or* father's

occupation. The restriction to men was made because some of the main data sets in our collection exclude women by design. To avoid problems of noncomparability, we decided to exclude women from the analysis.[6] The lower age cutoff was chosen on the assumption that by age 25 nearly all persons will have completed their education, even in highly industrialized countries.[7] The upper age restriction was imposed to avoid noncomparabilities between samples (some are restricted to those age 64 or less, others have no age restriction, and still others fall in between), as well as to minimize the possibility of selection bias due to differential mortality.

17.3. A cohort design with multiple surveys

The analysis presented here constitutes by far the largest cross-national comparison of the educational attainment process yet undertaken. Previous comparative research typically has dealt with only two or three countries, Müller et al. (1990) being the main exception. Müller and his colleagues analyzed nine countries, each of which was represented by a single data set. The 1993 volume edited by Shavit and Blossfeld reports on 13 countries, but is not strictly comparative, both because each country was analyzed separately and because the research design used in each study conformed only approximately to a uniform standard. The analysis reported here constitutes a considerable advance over these previous reports, at least in scale. First, we have more than twice as many countries as any previous researcher. The distinctive characteristic of our approach, however, is that we use multiple surveys for most of our countries. For about a third of the countries one data set was available, but for the others we have a number of data sets (as many as 27, for the United States). More often than not, these surveys were conducted in different years, which extends our historical horizon.

As is appropriate in the case of educational attainment, we will analyze our data with a cohort design; that is, we assume that educational attainment is finalized early in the life-cycle (before age 25) and can therefore be located at a given point in time. This allows us not only to infer historical trends from cohort comparisons, but considerably increases the available degrees of freedom. Although we include data from only 29 countries, we have nearly 300 data points for our macro-level analysis since each five year cohort within a country can be treated as an independent observation--a sample of the population born in a particular country within a particular five year interval.[8]

A multiple survey design has a number of advantages over a single survey design, but it also has some disadvantages. The first advantage is the increase of statistical power that is obtained by amassing data. Having a massive database is quite crucial to permit the detection of substantive historical trends and systematic variation that might otherwise go unnoticed. Second, multiple surveys amount to a multiple measurement perspective at the macro level and this is an appealing way to deal with measurement and comparability problems. Having single surveys for each country makes it impossible to distinguish between country-specific effects and survey-specific effects. Multiple surveys allow the researcher to average out idiosyncrasies that are produced by question format, sampling procedures, coding procedures, and other ingredients that determine the quality and comparability of data sets, to arrive at a more reliable estimate of true country/cohort effects. Third, the use of multiple surveys allows one to explicitly model survey-specific effects (either, as we do in the present analysis, by introducing dummy variables for each survey, or, when we have specific hypotheses about effects associated with particular types of surveys, by introducing explicit measures of these effects). A multiple survey design is particularly powerful when used in conjunction with a cohort design, where survey effects and historical effects are not confounded.

The basic disadvantage of a multiple survey design is the sheer amount of work involved. Some economies of scale are possible and we have exploited them wherever we could. For example, sometimes surveys share similar occupational codes and we have developed a system to process these in a standardized way (Ganzeboom, Luijkx, and Treiman, 1989). However, there are also dis-economies of scale, in particular because noncomparabilities within countries are more troublesome than non-comparabilities between countries--because in the former case they must be regarded as mainly reflecting methods effects whereas in the latter case they may be attributable either to methods effects or to true substantive differences. The surveys we have used differ in virtually every conceivable way. We have been as liberal as possible regarding the inclusion of surveys and have suppressed all impulses (mainly on the part of the second author) to discard data for their reputed or observed low quality. We have included surveys with different levels of measurement precision and detail and have found ways to deal with the possible deficiencies (see below). In general, we have favored more over better data, and explicit acknowledgement and modelling of measurement problems and noncomparabilities over discarding data. This strategy derives from our conviction that a major limitation in cross-national research is the lack of degrees of freedom, and that it is better to err on the comparability side than on the statistical power side.[9]

17.4. Converting educational classification categories into a common metric

Many different educational measures have been used in the surveys we analyze. There are two reasons for this. First, educational systems differ, mostly between countries, but often also between historical periods and as a consequence between cohorts and between generations. Second, even apart from institutional differences, the educational measures differ because the original investigators and subsequent data producers have adopted different question formats, coding conventions, and assorted practices. In order to carry out a meaningful comparative analysis, the variety of measures used in the original data sets has to be converted into a common metric or brought under control in another way. The difficulties encountered in achieving comparability with respect to the measurement of educational levels can hardly be overestimated: our experience suggests that the standardization of educational attainment is far more difficult than the creation of standard occupational classifications or occupational status scales. This is the case not only because institutional differences in educational systems are far larger than those for job classifications, but also because national and cross-national standardization procedures are much less well developed for educational than for occupational categories.

17.4.1. Variations in educational systems

Educational systems differ from country to country and, within countries, over time. The most important institutional contrast is probably between comprehensive systems such as that in the United States, where educational attainment can be measured adequately by years of education completed and the type of the curriculum matters little (see Treiman and Terrell, 1975:580-581), and divided educational systems such as in The Netherlands, where from a certain point in the educational career (in this case age 12) students follow entirely different tracks (usually in different schools). One of the implications of dealing with data from divided systems is that, in principle, years of schooling is an inadequate measure of the level of education attained. For example, in The Netherlands several students may leave school at age 18 with 12 years of schooling, yet have entirely different qualifications. One may have received basic elementary training plus lower vocational training, with no possibility of continuing further; another may have a *gymnasium* diploma, which qualifies a student for virtually every university curriculum; still another may have received general training that does not qualify him for university entry; and another may have middle vocational training of a kind that permits entrance to a vocational college. Situations of similar and even larger complexity arise in other countries.

17.4.2. Non-standardized coding conventions

Educational variables are very often cast in local terms and abbreviations with little or no effort by the original investigators to relate these to a generally applicable set of categories or metric. For instance, while there exists an International Classification of Education (ISCED) (UNESCO, 1976), we have not encountered a single data set in which this classification was employed. By contrast, original investigators or subsequent data producers sometimes have translated original designations into internationally accessible, but non-standard, terms, and by so doing have sometimes obscured rather than illuminated the nature of the original educational classification. To the comparative researcher who wishes to utilize a large number of foreign data files, the diversity of educational measures poses a problem that requires formidable mastery of local information.

Fortunately, educational status variables generally prove to be very robust, which suggests that there is a strong underlying dimension that is resistant to the corruptions that measurement practices and conversions to a common metric introduce. That is, the strong association between education and other variables is usually well preserved under alterations of the coding schemes. We have exploited this fact to use the association with various criterion variables, together with other information, to convert local educational classifications into a common metric.

17.4.3. Approaches to comparative educational measurement

There are basically three possible approaches to the comparative measurement of education: (a) mapping the educational categories from each study into a common *classification*; (b) mapping the educational categories from each study into a common *metric*, such as years of education; and (c) optimally scaling the educational categories from each study with respect to some extrinsic criterion. We discuss these methods in turn.

A manual by UNESCO (1976) provides the relationship between national educational classifications and these categories. Another example is the system used by Müller et al. (1990), who distinguish seven categories but do not show the relation between their categories and the original codes used in each study.

Mapping into a common classification

An example of an attempt to match educational categories cross-nationally is given by the ISCED classification (1976), which distinguishes the following broad levels of educational attainment:

- Pre-primary
- Primary
- Secondary
  - lower level
  - higher level
- Tertiary
  - lower level
  - higher level

Unlike the ISCED, however, Müller et al. explicitly acknowledge in their system the importance of tracking (vocational vs. academic).

The major disadvantage of the category matching approach is that it results in a high level of aggregation and ignores distinctions that are made in the original classification schemes. Given the very small fractions of the populations of industrialized nations that end their education at the pre-primary level and, for the younger cohorts, even at the primary level, the ISCED scheme in effect distinguishes only four, or at most five, categories, fewer than are typically employed in local classification schemes. To make things worse, the ISCED categories make no provision for divided tracks which, as we have noted, can result in persons with the same *amount* of schooling attaining very different *qualifications*. For example, in the case of The Netherlands the ISCED classification glosses over the fundamental differences between vocational and academic tracks that exist at both the secondary and tertiary level. In sum, ISCED achieves comparability by using an unfortunately high level of aggregation, and it seems to miss its target by essentially adopting an 'American' (unidimensional) point of view.

Müller's categorical scheme is somewhat more detailed and explicitly takes vocational tracks into account. However, it is not useful for our current purposes since it does not result in a clearly rank-ordered variable, let alone a variable with an interpretable metric. It also employs aggregation as its main tool to achieve comparability. In our opinion, approaches such as these will be most useful when one wants to study the relationship between education and occupational attainment, in particular when this latter variable is measured in discrete categories. For our present purposes, the category matching approach is not as useful as other methods available.

Mapping into a common metric

The remaining methods (mapping into a common metric and optimal scaling) can preserve at least some of the distinctions peculiar to particular educational systems. They do this by assuming that a single dimension underlies the educational categories and that scores on the underlying dimension can be assigned to each category. One approach is to assign scores to educational categories on the basis of intrinsic knowledge of the educational system involved.[10] Typically, those categories known to correspond to a specific number of years of education are initially assigned scores, and the remaining categories are then interpolated, with adjustments of the scores (including those initially assigned) to accommodate differences in the qualifications resulting from particular *types* of schooling among those who obtain the same *amount* of schooling. The advantage of years of education over a simple rank order of the categories is that the constructed variable is (in principle) completely comparable across studies and has an interpretable metric. The major disadvantage is that adjustments to accommodate 'slow' and 'fast' (academic and vocational) tracks violate the strict interpretability of the resulting scale as measuring years of schooling; what the scale measures is something more like 'virtual' years of schooling. We discuss this issue further below.

Optimal scaling

The alternative method for establishing a common metric is to scale each set of local education categories with respect to some extrinsic criterion, such as occupational status. This approach was utilized by Treiman and Terrell (1975) in their early comparison of status attainment models for England and the United States. They scaled the educational categories in each country proportionally to the mean level of occupational prestige attained by those with each level of educational achievement. Since a single occupational prestige scale (Treiman's [1977] international scale) was used as the criterion in both countries, the result was a comparable scaling of educational categories with respect to their average occupational return. As Treiman and Terrell observe, this technique is a case of optimal scaling, where the linear correlation between education and occupational attainment is optimized.

The advantages of optimal scaling procedures for deriving an educational status measure are several. First, the procedure reveals whatever (linear) association between the educational categories and the criterion variable is contained in the data, by finding the scale scores that maximize that association. Second, an optimal scaling procedure can be used without reference to the meaning of categories. For this reason it is of invaluable assistance when in-depth knowledge of educational categories is absent, for example when the documentation on a data set is not sufficiently clear.

However, optimal scaling procedures have their disadvantages as well. First, there is some ambiguity regarding which criterion variable is conceptually preferable. Treiman and Terrell (1975) use occupation as their criterion, but they could as well have chosen income, the other dependent variable in their analysis, or father's occupation, their main independent variable. Father's education was not present in the Treiman-Terrell comparison, but would have been another obvious choice. As a compromise, one could choose several criteria at the same time, as is done in generalized optimal scaling approaches (Gifi, 1990), but then the meaning of the scale scores becomes less clear. Second, optimal scaling procedures confound random and systematic variance and hence tend to overestimate the relationship that is being modelled; that is, optimal scaling procedures treat chance aspects of association as if they are systematic. This is particularly problematic when the level of detail of measurement differs between the data sets to be compared: data coded in less detail are more prone to aggregation error but less prone to overestimation error, but it is difficult to assess the extent to which different data sets are subject to the various sources of error. Finally, it might be objected that optimization procedures are atheoretical and blind, because they do not take into account the specific contents of educational categories and their relationship to other components of the stratification process. We would disagree with this argument, because these methods correspond well to queuing and relative status concepts of education (Thurow, 1975; Ganzeboom, De Graaf, and Treiman, 1992). A related objection must be taken more seriously, however: the statistical properties of the resulting distributions do not depend simply upon the distribution of the sample over educational categories but on the nature of the relation between the educational categories and the criterion variable(s); hence they are difficult to compare in a meaningful way across countries or cohorts. Moreover, as we have noted and will discuss in detail below, the distribution of educational attainment has important consequences for the structure of educational opportunity.

17.4.4. Scaling education attainment

Our strategy for scaling educational categories combines elements of the second and third approaches. In general, we have preferred to use *a priori* information for the scaling of categories, but have often checked our preliminary scale scores against the results of optimal scaling exercises and have corrected our information where appropriate. Specifically, we have carried out the following procedures for each data set.

First, we settled upon a metric of educational attainment: 'virtual' years of education. For countries with a comprehensive, unidimensional (U.S.-type) educational system (such as the US, Brazil, Japan [new system], and the Philippines), this measure is identical to the years of education claimed by the respondent or known to correspond to specific levels of educational attainment, e.g. completion of a bachelor's degree. In these cases, categories that span more than one year of education were recoded to their assumed modal value. For multi-dimensional systems, we began by coding the years of education associated with 'anchor' categories for which the relation to years of school completed was known, and then interpolated the remaining categories, modifying the scores for the anchor categories as necessary to preserve a monotonic relationship between any rank ordering found in the original data and our 'virtual' years of school completed measure. For example, in The Netherlands both *gymnasium* and middle vocational school (*MBO*) typically are completed at age 18; but in our 'virtual' years of school completed scale, the former is scored at 12 years of education while the latter is scored at 11 years to accommodate the fact that the qualification obtained from *MBO* is lower than that obtained from *gymnasium*.

We then validated our preliminary code assignments by assessing the linearity of the relationship between the recoded education scale scores and various criterion variables (father's occupational status, recoded spouse's education, and respondent's occupational status) via the visual inspection of scatter plots. We also studied the relationship between recoded father's education and father's occupation.[11] We took nonlinearities as evidence of the possibility of error, checked the interpretation of the original education categories where possible, and adjusted the scale scores as necessary. The result is a scale of educational categories for each country that we think is cross-nationally valid, not only with respect to the ordering of the categories but with respect to its distributional properties as well; that is, we think it legitimate to compare the mean and standard deviation of scale scores across cohorts and countries.

17.5. Cohorts

Figures 1a and 1b introduce the main units of our analysis: five-year birth cohorts in the 29 countries. In the figures, the cohorts are centered around a year ending in 0 or 5 and are numbered with 1898-1902 as the 0 point. They range from -2 (born 1888-1892) to 12 (born 1958-1962). For no country are all cohorts present in the data; in each country the cohorts are censored on one or both sides. The cohorts are plotted in Figure 1a by the square root of their absolute size and in Figure 1b by relative size within countries. From Figure 1a it is clear that our cases are very unevenly distributed over countries. At the extreme, the sample size for the U.S. (43,422) is more than 100 times as large as that for Belgium (404). The data sets for Hungary (29,627) and Poland (30,463) are also extremely large, which means that if we based our analysis on the observed frequencies, the results would be substantially driven by contrasts between Hungary, Poland, and the U.S. To avoid this, we have rescaled the data from each country to a uniform frequency.

Figure 1b shows the results of this standardization, displaying the relative distribution of cases over cohorts, weights that are preserved throughout the analysis.[12] In general, these histograms show most cases falling in the middle cohorts, and many of them have longer left than right tails. This pattern is produced by stacking surveys conducted in different years, as well as the under-representation of the elderly due to mortality. Some specific forms need additional comments: the gaps between cohorts for Belgium are due to the fact that age was measured in broad categories, and hence not all cohorts could be filled. The same is true of some older surveys in Sweden and The Netherlands, but this is not visible in these countries because the surveys with large age ranges are merged with other surveys.

Figure 2a shows the trend in average educational attainment in each country. The plot symbols reflect the relative cohort sizes as displayed in Figure 1b. Figure 2a is striking for its massive regularity: educational expansion has been nearly universal, with India the only possible exception; and the upward trends are nearly always linear. This regularity should not, however, distract us from two additional observations. First, the average level of education not only varies widely across cohorts, but differs also between countries, even industrialized countries. For example, for the 1930 cohort the average years of education for industrialized nations ranged from 6.8 for Italy to 11.5 for the United States, and for non-industrialized nations ranged from 1.3 for India to 5.3 for the Philippines. These inter-country differences are very large, as is evident when we consider that the standard deviation of educational attainment within countries and cohorts is generally about 2.5. Second, countries differ widely in their tempo of educational expansion, ranging from almost flat curves (Austria, Czechoslovakia, India, Northern Ireland, and Switzerland) to steep slopes (Canada, Malaysia, Spain, and the United States). As we will see below, such variation has important consequences.

Figure 2b shows another statistic on the educational distributions of the cohorts within countries: the standard deviations. The pattern of these coefficients is somewhat irregular (which is consistent with the observation that central tendency measures generally are much more stable than are dispersion measures), but the important thing here is that we observe even more widely diverging trends than for the means. In some countries (most notably in Canada and the United States) the trend is towards less dispersion; in others (Brazil, Malaysia, and Norway) there is a steep increase in dispersion. These differences are most likely related to the effect of compulsory schooling policies on the distribution of educational attainment. In countries where the minimum school leaving age has stayed the same throughout most of the period covered by our cohorts, the standard deviation is likely to grow with educational expansion, whereas the reverse is true for countries where expansion mainly took the form of pushing up the bottom of the educational distribution.

For each cohort we have centered the independent variables, father's education and father's occupation, within each 5-year cohort. That is, each variable is expressed relative to its within-cohort mean. This transformation leaves the units of measurement unchanged, but removes the correlations with interaction terms in the subsequent regression models. In the analysis, cohorts in each of the countries are centered around 1930 as the 0-point. The convenience of this scaling is that the intercept and main effects will refer to the same cohort in each of the countries. The 1930 cohort was chosen because it is as close we can get to a 'mean cohort' in every country.

We estimate the following three models, separately for each country:

$$\begin{aligned} \text{EDUCYR} = {} & \text{b0} + \text{Bcoh*COHORT} \\ & + \text{Bfed*FEDUCYR} \\ & + \text{Btrend*COHORT*FEDUCYR} \\ & + \text{control variables} \end{aligned} \tag{1}$$

$$\begin{aligned} \text{EDUCYR} = {} & \text{b0} + \text{Bcoh*COHORT} \\ & + \text{Bfed*FISEI} \\ & + \text{Btrend*COHORT*FISEI} \\ & + \text{control variables} \end{aligned} \tag{2}$$

$$\begin{aligned} \text{EDUCYR} = {} & \text{b0} + \text{Bcoh*COHORT} \\ & + \text{Bfed*FEDUCYR} \\ & + \text{Bfis*FISEI} \\ & + \text{Btrend*COHORT*FEDUCYR} \\ & + \text{Btrend*COHORT*FISEI} \\ & + \text{control variables} \end{aligned} \tag{3}$$

Given the pattern of missing values, these three equations refer to different subsets of the data. Since all our files have father's occupation, Equation (1) refers to our broadest database, 290 cohorts in 29 countries (N = 193,395). Equation (2) refers to the cohort-country combinations for which we have a valid measure of father's education. This removes Belgium, France and Turkey from our database, as well as a number of surveys from other countries, which reduces the number of cohort-country combinations to 252 and the number of individual cases to 168,963. Equation (3) refers to cohort-country combinations for which we have data on both father's occupation *and* father' education; hence, Equation (2) and Equation (3) are based on the same data. These three equations are then estimated for two different versions of the cohort variables:

(A) Cohorts are expressed as dummy variables. This is equivalent to estimating the equation cohort-by-cohort, except that the effect of the control variables is modelled identically across all cohorts. The estimated coefficients for these equations are the input for the pooled time-series/cross-section analysis discussed below.

(B) Cohorts are expressed as dummy variables for the main effects, but as a linear variable ranging from -6 to +8 for the interaction effects. Comparing Model (B) with Model (A) provides a one-degree-of-freedom test for linear trends in the effect of each father's social status characteristic on respondent's education.

Figures 3a-3d display the estimated coefficients for equations 1.A, 2.A and 3.A, by cohort within countries. Note the rather large dispersion in these parameters, as well as the absence of a universal decline in the effects of father's status on educational attainment. Consider first the effect of father's education (Figure 3a). In 14 of the 29 countries the trend is clearly downward; in two counties it is clearly upward; and in the remaining countries the trend line is either flat or incoherent. Similar results hold for the effect of father's occupation considered alone and the effect of each social origin variable controlling for the other. On the basis of this visual inspection, we tentatively conclude that the dominant, but by no means universal, trend is toward a decline in the influence of social origins on educational attainment.

A formal test of these trends is provided by assessment of the significance of the linear trend coefficients in Model (B). The results for each country are found in Table 1. For father's occupation, the simple effect is declining in 19 (73 per cent) of the 26 countries for which we have data but the trend is significant in only 11 (42 per cent) of the countries. For father's occupation, the simple effect is declining in 14 (48 per cent) of the 29 countries for which we have data, but the trend is significant in only seven (24 per cent). The coefficients for the multivariate models tell more or less the same story. We are thus led to the same conclusion from the formal analysis as from the visual inspection of the figures--there is a dominant but hardly universal pattern of decline in the influence of social origins on educational attainment.

17.6. Pooled cross-cohort cross-section contextual analysis

The results we have so far, showing that the effects of social origins on educational attainment decline in most countries, but remain constant or even increase in others, require explanation. We thus attempt to model variation in the observed coefficients across countries and across cohorts. As noted above, we expect that educational expansion (an increase in the average level of schooling) will tend to reduce the effect of social origins on educational attainment and that educational inequality (an increase in the variance of completed schooling) will tend to strengthen the effect of social origins on educational attainment. Our independent variables for the macro analysis are measured in the following way:

- Educational Expansion: the average years of education completed by each cohort within each country.

- Educational Inequality: the standard deviation of the years of education completed by each cohort within each country.

There is no reason to expect the variability across cohorts to be similar in magnitude to, nor explained in the same way as, variability across countries. In particular, there should be more contextual variation between countries than between cohorts. This implies that we cannot simply pool cross-cohort and cross-country variation and estimate the relationships in one step. Instead, we must take into account the differences in residual variation that occur between the two types of context. Formal models that deal with this problem are error-components or random-coefficient models (Sayrs, 1989).

While such models are readily available for balanced designs with unweighed data, they are not available for situations like ours, where we do not observe all cohorts over all countries and the observed cohorts have widely different frequencies. We have therefore taken recourse to the Least Squares Dummy Variable (LSDV) model also discussed by Sayrs. In the LSDV model the variation is not simply pooled; rather, the main differences that exist between contexts of one type are controlled by a set of dummy variables. In our application, we have two type of contexts, and so we have two LSDV models: one that controls the main effect differences between countries, and therefore models cross-cohort variation; and another that controls the main effect differences between cohorts, and therefore models cross-national variation. In both models, we start with the same number of contexts (cohort-country combinations), but different numbers of degrees of freedom are consumed by the dummy variables. LSDV models are inefficient because they estimate a large set of dummies in addition to the structural effects of interest. However, they are very easy to estimate.

Table 2 reports the LSDV model for the effect of the characteristics of the educational distributions within contexts (country-cohort combinations) on the metric regression coefficients retrieved from the Model (A) in the previous section. All models are weighted by the associated within-cohort relative frequencies (where the country frequencies have been equalized) and therefore represent weighted least squares estimates. The results are as hypothesized: the effect of social origins on educational attainment increases as educational inequality increases and decreases as educational opportunities expand. These results hold both for the coefficients estimated from the simple regressions (on a larger database) and (with minor exceptions) for the coefficients from the multivariate regression (estimated on a smaller database); the exceptions are the non-significant net effect of Educational Expansion on the father's occupational status coefficients in the cross-sectional analysis and the pooled analysis, and the non-significant net effect of Educational Inequality on the father's education coefficients in the cross-temporal analysis. All three non-significant coefficients are, however, in the hypothesized direction. Moreover, the gross results for the cross-cohort and the cross-country analysis are strikingly similar, except for the explained variance: as expected, the explained variance is much higher for cross-cohort variation, which indicates that there are no important residual effects given this partition of the data. Put differently, changes over time in the average level of educational attainment and the extent of educational inequality more or less completely explain cross-temporal variability in the equality of educational opportunity (measured by the independence of educational attainment from social origins); but this is not true for cross-national variations in the degree of equality of educational opportunity--other factors besides the level and variation of educational achievement also play an important role. This result is not particularly surprising, since cross-national differences in the way educational systems are organized tend to be much larger than cross-temporal variations in educational organization within countries.[13]

Now let us consider the magnitude of the effects. The coefficients in Table 2 show the expected *change* in the slope coefficients for the individual level equations as a function of changes in the level and variability of education. Consider first the gross effects (the top panel). The coefficient in the upper left hand corner, -.037, tells us that within any cohort two countries that differ by one year in their average level of schooling would be expected to differ by -.037 in the effect of father's on son's level of education. This is a large effect, which can be seen in comparison with the mean effect of father's education on son's education over all cohorts and countries (.49). For example, the expected difference in the effect of father's on son's education between India (where men average of 2.54 years of schooling) and Canada (where men average 11.90 years of schooling) is -.35 (= -.037(11.90 - 2.54)), or more than two-thirds the magnitude of mean effect (computed over countries and cohorts). The effect of Educational Inequality on the magnitude of the father's education-son's education link is equally large (.087). Thus, for example, net of the effect of Educational Expansion, we would expect the coefficient relating father's to son's education to be .19 (= .087(4.71 - 2.48)) larger in the Philippines (the country with the largest variance) than in England (the country with the smallest variance). The cross-temporal and pooled comparisons yield similar results, as do the corresponding comparisons of the net regression coefficients in the bottom panel of the table.

The results for the effect of father's occupational status are generally similar, but not as robust. Although the gross effect of father's occupational status declines with Educational Expansion, the net effect does not, except for the cross-temporal analysis. However, both the gross and net effects of father's occupational status increase with Educational Inequality, as expected, albeit the increases are more modest for the net than for the gross coefficients.

17.7. Conclusions

We believe that the quantification of these trend coefficients provides important insights about the mechanics of educational expansion. First, it quantifies the size of the expected effect: roughly speaking, increasing average educational attainment by one year decreases the effect of father's on son's education by about five per cent. Second, and probably more importantly, it qualifies the expectation derived from the progression rates model: educational expansion will only drive down the effect of family background in sofar as it does not increase the level of educational inequality. Our data suggest that educational expansion sometimes, but not always, does increase educational inequality. This is typically the case for countries that have had a stable compulsory school leaving age (or no compulsory leaving age at all) for many years. An increase in educational inequality may occur at all levels of economic development, but it tends to be most conspicuous in developing nations lacking compulsory education. In such nations an increase in the availability of schooling tends to increase the amount of education obtained by the most advantaged members of society while leaving the least advantaged with little or no education. By contrast, educational inequality, and hence the dependence of educational attainment on social origins, tends to decline in countries where a concerted effort is made to increase the minimum level of schooling available to all children.

NOTES

1. Earlier versions of this paper were presented at the Meetings of the Research Committee on Social Stratification and Social Mobility of the International Sociological Association, Ohio State University (Columbus), August 1991; the Department of Sociology, University of California-Los Angeles, August 1992; and the SYSWO Working Group: Social Stratification, in November 1992. This paper was prepared while the first author was a visiting scholar at the University of California at Los Angeles. Important archival assistance was provided by Elizabeth Stephenson of the UCLA Social Science Data Archive and helpful research assistance was provided by Yu-Sheng Peng. We are indebted to a number of colleagues for sharing their knowledge about the peculiarities of national educational systems with us: Margaret and John Heritage (England), Jonathan Kelley (Australia), David Radick (Germany), Philip Smith (England), and Ken'ichi Tominaga (Japan). We also thank Karl Ulrich Mayer for sharing his scolecoid insights regarding our paper at the Solidarity of Generations Conference.

2. Although in some of his work (1981:75-76), Mare seemed to suggest that the relationship between the increasing level of education and the decline in the dependence of education on social origins holds in general (assuming the relative odds of transitions remain constant over time), this is in fact not so, as he recognizes in his discussion of the algebraic equation decomposing the OLS regression coefficient into a portion associated with the relative odds of transition and a portion associated with the distribution of persons over levels of education (1980, 1981). It turns out that whether the regression coefficient increases or decreases as the average level of education increases depends also on the variance of the educational distribution; as the variance increases, the regression coefficient increases as well.

3. Cobalti and Schizzerotto (1993:165) found an increase in the effect of father's occupational status for both men and women whereas Ganzeboom and Treiman (1993:15), using a larger data set that encompassed the data used by Cobalti and Schizzerotto and other data as well, found such an effect only for women.

4. In seven cases, we created a separate file from information on the spouses of married persons. Also, three of the British files are from a single partially overlapping panel design. Hence, these data are actually derived from 106 sample surveys, not 115.

5. In principle, we are willing to include surveys of identifiable "nations" within "states," e.g., Quebec within Canada; Scotland and Northern Ireland within Great Britain; etc. However, with the exception of Northern Ireland, no such data are included in the present analysis. We exclude from the ISMF data surveys conducted in single metropolitan areas, e.g., Beijing, on the ground that metropolitan samples cannot be considered as comparable to national or regional samples on any principled ground.

6. However, we expect to supplement our analysis of men with an analysis of the educational attainment of women in a future paper.

7. Those remaining in school at age 25 are virtually all engaged in post-graduate education, which is the highest category in the educational classification of nearly every study we utilize; so we do not truncate the educational distribution by including men age 25 and older.

8. This claim must be qualified by the observation that our data refer only to the surviving members of each cohort. But we do not think that differential survival rates will substantially influence any of our results, especially since we have excluded all those age 65 or older, precisely to minimize the effect of differential mortality. Furthermore, we have defined our cohorts according to year of birth, and not to year of educational completion, in order to avoid any distortion resulting from the secular trend toward increased education.

9. This position is quite foreign to the stance of most current comparative macro-sociological research, which tends to involve comparisons of two or at most a handful of countries. We find it a bit odd that comparisons of very limited numbers of countries are quite acceptable; certainly, no one would believe the results of similar designs at the individual level. Moreover, while the standard approach at the macro-level is to go for the 'best data', and often the higher quality of one dataset relative to another is used to justify a substitution, this would be a quite unacceptable strategy at the individual level. Nevertheless, the situation for macro- and micro-level analysis is structurally the same: one tries to assess the relative influence of a number of different variables on one or more outcomes. The difference between the micro- and macro-situation is only that usually there are data for many individuals available to model micro-processes, but for only a few societies to model macro-processes. It is therefore particularly ill advised to restrict the data available for comparative analysis.

10. We have consulted with local experts wherever possible (they are listed in the acknowledgements), and plan to have our tentative scoring of educational categories reviewed by other experts.

11. To measure father's occupational status we applied the recently developed International Socio-Economic Index (ISEI) of occupational status (Ganzeboom, De Graaf, and Treiman, 1992). The ISEI scale ranges between 10-90, but for reasons of convenience, we have scaled this back in the 1.0-9.0 range, so that one unit of ISEI in our present analysis refers to 10 points of ISEI in the original scale.

12. It is to be noted that relative cohort sizes are used to weight the data, but that the total degrees of freedom in each analysis are equal to the number of cohorts. Most of the analysis was conducted in Stata, where this is the default procedure. In SPSS the same can be accomplished by reweighing the total N appropriately.

13. It must be noted that in general variations in the *net* regression coefficients are much less well predicted by country-cohort variations in Educational Expansion and Educational Inequality than is true of the gross regression coefficients. Our suspicion is that this is due to the sensitivity of net regression coefficients to minor sampling variations.

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APPENDIX 1.

Figure 1a. **Raw sqrt (N) of cases per cohort.**

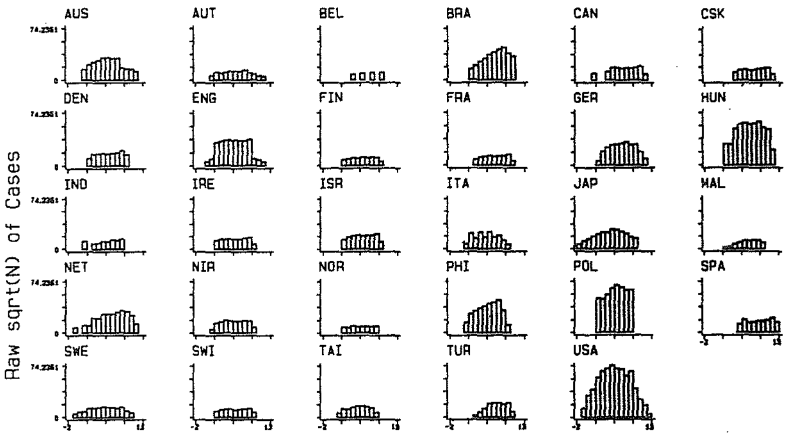

Figure 1b. **Reweighed N of cases per cohort.**

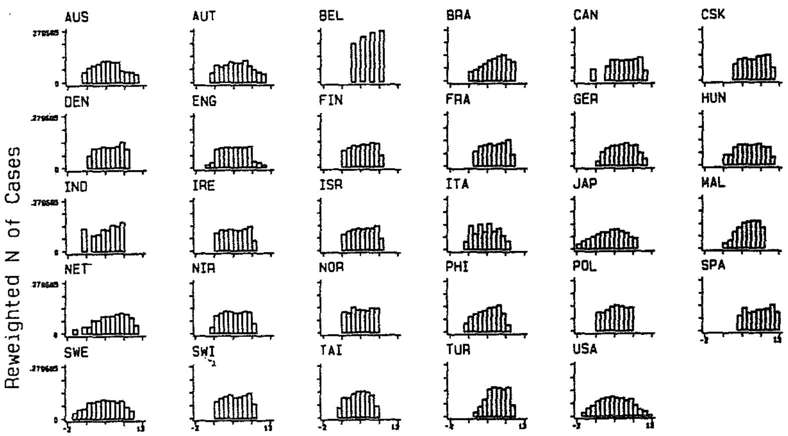

Figure 2a. **Educational expansion expansion by cohort.**

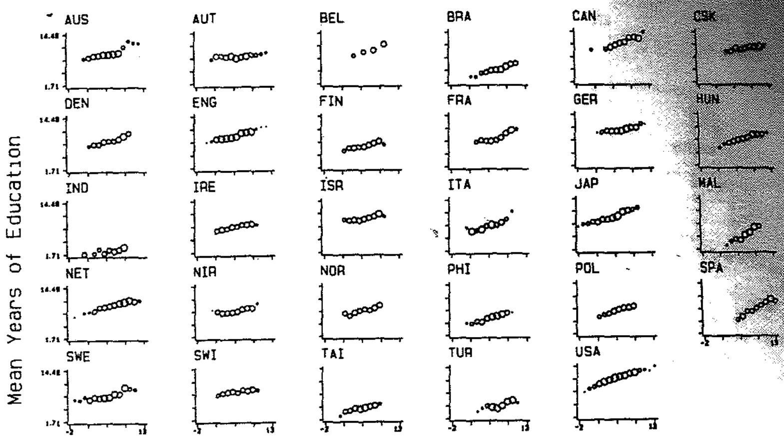

Figure 2b. **Educational inequality by cohort.**

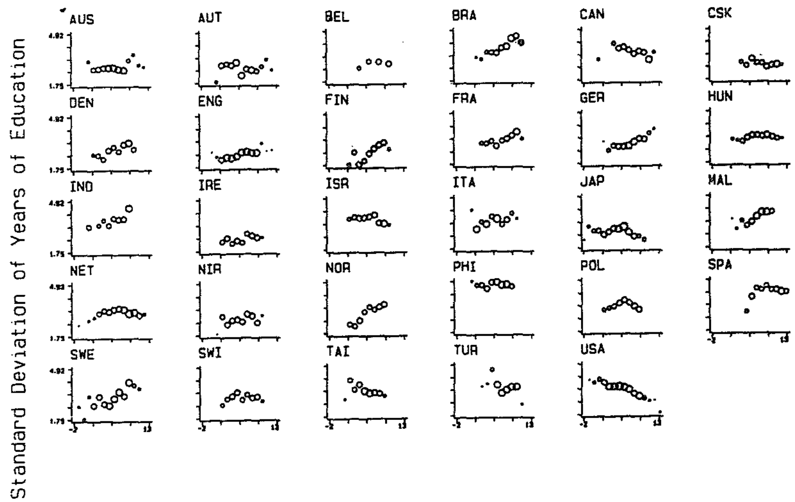

Figure 3a. **Gross effect of father's education by cohort.**

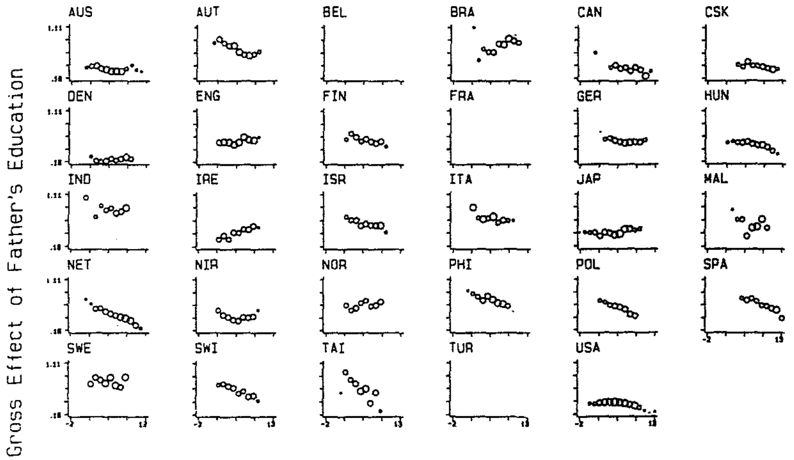

Figure 3b. **Gross effect of father's occupation by cohort.**

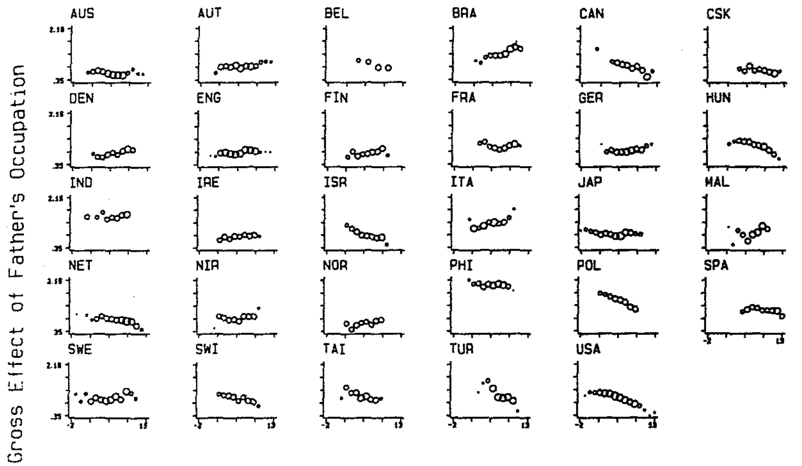

Figure 3c. **Net effect of father's education by cohort.**

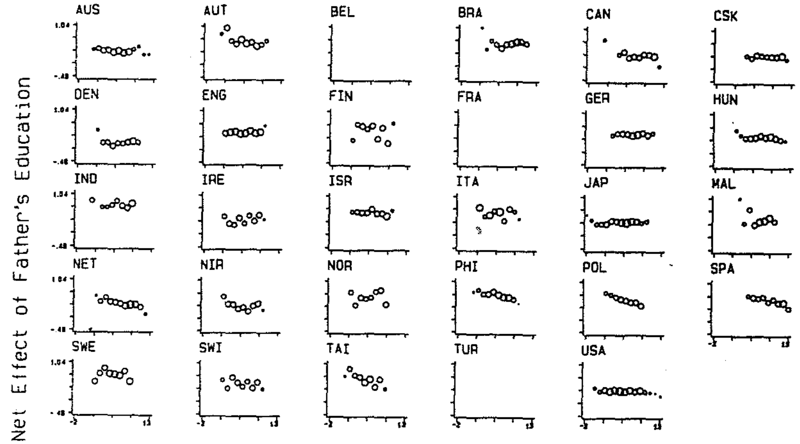

Figure 3d. **Net effect of father's occupation by cohort.**

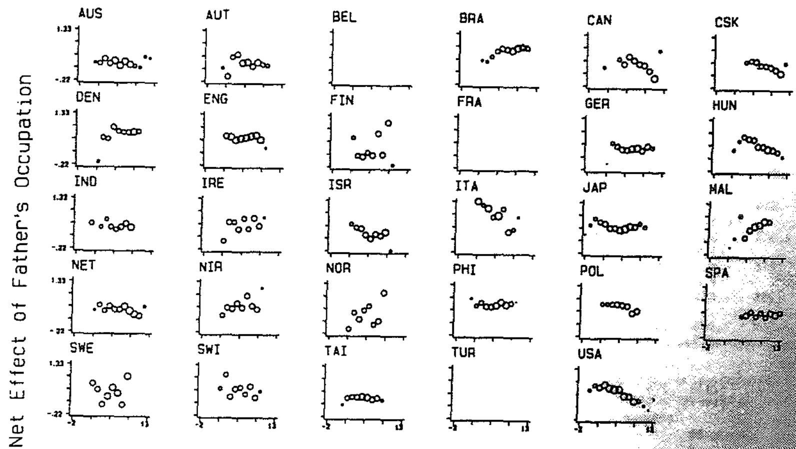

Table 1. **Linear trend regression models for effects of (A) Father's Education, (B) Father's Occupation, and (C) Both, on Educational Attainment in 5-year cohorts, 29 countries (t-ratios in parentheses).**

		SIMPLE REGRESSIONS A				B				MULTIPLE REGRESSION C					
	number of studies	Father's Education				Father's Occupation				Fr's Educ.		Fr's Occ.			
		B-1930	B*Cohort	R^2	N	B-1930	B*Cohort	R^2	N	B-1930	B*Chrt	B-1930	B*Chrt	R^2	N
AUS	6	.352	-.002 (.4)	.291	4953	.632	-.007 (.9)	.228	7205	.284	-.003 (.6)	.319	.000 (.0)	.313	4848
AUT	2	.931	-.033 (2.8)	.317	1041	.724	.016 (.9)	.190	1306	.832	-.044 (2.5)	.233	.016 (.6)	.317	977
BEL	1					1.193	-.044 (1.0)	.249	403						
BRA	3	.572	.028 (4.3)	.308	7569	.951	.060 (6.7)	.295	12192	.425	.013 (1.4)	.656	.023 (1.4)	.388	6315
CAN	3	.531	-.023 (2.9)	.226	1790	1.342	-.068 (4.0)	.194	2536	.417	-.020 (2.1)	.913	-.038 (1.6)	.245	1708
CSK	1	.545	-.019 (2.0)	.156	2049	.939	-.029 (1.7)	.148	2004	.296	-.004 (.4)	.859	-.053 (2.5)	.189	2003
DEN	3	.178	.007 (1.1)	.173	2318	.533	.037 (2.6)	.281	2648	.073	.002 (.3)	.547	.027 (1.5)	.267	2200
ENG	7	.499	.010 (1.7)	.211	9731	.690	.017 (2.3)	.239	10334	.383	.002 (.2)	.647	-.006 (.7)	.292	8917
FIN	3	.725	-.022 (1.2)	.233	696	.351	.035 (1.2)	.176	1018	.732	-.045 (2.0)	-.124	.086 (2.2)	.266	688
FRA	1					.976	.055 (.2)	.287	1365						
GER	10	.542	.001 (.1)	.228	6049	.652	.026 (2.4)	.179	6826	.374	.006 (.8)	.439	-.001 (.1)	.254	5752
HUN	4	.634	-.026 (12.6)	.287	29377	1.473	-.065 (11.8)	.259	2764	.508	-.021 (8.0)	.896	-.049 (2.2)	.317	27387
IND	1	.907	-.011 (.9)	.461	1091	1.446	.008 (.3)	.264	1098	.745	-.004 (.2)	.588	-.020 (.6)	.473	1059
IRE	1	.228	.034 (3.2)	.197	1830	.639	.019 (1.0)	.182	1888	.159	.024 (2.2)	.385	.023 (1.1)	.250	1,817
ISR	1	.695	-.019 (2.3)	.243	3054	1.216	-.068 (3,1)	.097	3199	.578	-.009 (.9)	.620	-.053 (2.3)	.249	2,905
ITA	4	.737	-.012 (.8)	.421	559	.984	.045 (2.9)	.310	3342	.520	-.004 (.2)	1.443	-.092 (1.5)	.449	522
JAP	4	.354	.014 (3.4)	.308	5217	.831	.001 (.2)	.306	5831	.241	.011 (2.6)	.624	-.009 (.9)	.377	4,891
MAL	1	.598	-.007 (.3)	.284	652	.402	.068 (1.6)	.221	906	.581	-.021 (.8)	.222	.057 (1.2)	.329	652
NET	7	.747	-.041 (7.3)	.209	5581	.997	-.036 (4.3)	.204	7201	.553	-.032 (4.8)	.637	-.028 (2.2)	.238	5,332
NIR	2	.448	-.001 (.1)	.177	1913	.709	.021 (1.3)	.284	2387	.384	-.015 (1.4)	.406	.029 (1.6)	.251	1,888
NOR	2	.561	.014 (.5)	.271	376	.439	.033 (1.0)	.166	699	.537	-.001 (0)	.007	.075 (1.3)	.303	365
PHI	2	.915	-.032 (7.6)	.362	12406	2.076	-.011 (.6)	.221	11,777	.813	-.032 (6.1)	.700	.003 (.1)	.389	10,965
POL	1	.836	-.044 (13.3)	.233	30463	1.980	-.094 (10.9)	.224	28,585	.811	-.052 (11.5)	.912	-.039 (3.7)	.294	28,585
SPA	1	1.045	-.044 (5.9)	.403	2664	1.343	-.026 (1.3)	.289	2,566	.893	-.040 (4.6)	.459	.005 (.2)	.427	2,483
SWE	3	.816	-.009 (.4)	.381	394	.836	.030 (1.9)	.245	1,834	.722	-.013 (.5)	.305	.029 (.6)	.401	392
SWI	2	.835	-.037	.261	1073	1.224	-.043	.214	1,070	.568	-.021	.766	-.038	.299	1,030
TAI	1	1.009	-.074 (4.7)	.330	1010	1.316	-.054 (1.9)	.204	1,592	.937	-.075 (3.9)	.248	.021 (.5)	.349	950
TUR	1					1.920	-.108 (3.9)	.198	2,342						
USA	26	.454	-.016 (11.1)	.276	36120	1.350	-.068 (18.4)	.257	41,584	.314	-.009 (5.4)	.971	-.065 (13.8)	.322	34,286
Decreases			19				14				20		13		
Increases			7				15				6		13		
Significant (.05, 2-tailed)															
Decreases			11				7				9		5		
Increases			3				5				2		1		

Notes:
a. Model A is defined as: EDUCYR=f(FEDUCYR, BYR_FED, SEX, SEX_BYR + study control variables).
b. Model B is defined as: EDUCYR=f(FISEI, BYR_FIS, SEX, SEX_BYR + study control variables).
c. Variables are scaled as follows: EDUCYR: years of education; FEDUCYR, years of education, centered within cohorts, FISEI: International Socio-Economic Index of occupation status, range 1..9.

Table 2. **Least Squares Dummy Variables Models for the contextual analysis (t-ratios in parantheses).**

	Cross-sections (Cohort controlled)		Cross-time (countries controlled)		Pooled	
	Bfed	Bfis	Bfed	Bfis	Bfed	Bfis
Gross Effects						
Educational Expansion	-.037 (8.7)	-.032 (5.9)	-.028 (5.0)	-.039 (5.6)	-.040 (10.7)	-.042 (8.4)
Educational Inequality	.088 (5.8)	.375 (18.4)	.090 (5.0)	.271 (13.0)	.076 (5.4)	.343 (17.8)
adj R^2	.425	.642	.801	.898	.427	.629
N of Cohorts	252	290	252	290	252	290
NDF	17	17	27	30	2	2
Net Effects						
Educational Expansion	-.035 (7.4)	-.001 (.2)	-.034 (4.3)	-.031 (2.4)	-.039 (9.4)	-.007 (1.1)
Educational Inequality	.085 (5.0)	.125 (4.9)	.043 (1.7)	.205 (5.0)	.068 (4.3)	.111 (4.7)
adj R^2	.355	.086	.652	.417	.351	.094
N of Cohorts	252	252	252	252	252	252
NDF	17	17	27	27	2	2

Note: "Net effects" means that the dependent variables are effects from a micro-level equation, when both predictors (Father's Education, Father's Occupation) are entered. "Gross Effects" refers to effects in a micro-level regression equation without controlling for the other predictive variable. All models are estimated using relative cohort sizes within countries as weights.

APPENDIX 2. Data sources and sample characteristics.

(a)	(b)	(c)	(d)
Australia			
AUS65	1756	Broom, Jones & Zubrzycki	
AUS67	723	Aitken, Kahan & Stokes	
AUS67l	574	/Aitken, Kahan & Stokes	In-law information
AUS73	2674	Broom et al.	Only active labor force.
AUS84	973	Kelley, Cushing & Heady	
AUS87	612	McAllister & Mughan	
Austria			
AUT69	637	Verba, Nie & Kim	
AUT74p	443	Barnes & Kaase	
AUT86	306	International Social Survey Program	
Belgium			
BEL75	404	Bernard & Delruelle	
Brazil			
BRA72	410	Converse, McDonough et al.	
BRA73	5377	IBGE	10% sample, women are omitted
BRA82	7505	IBGE	10% sample
Canada			
CAN65	434	Porter & Pineo	
CAN82w	1065	Wright et al.	
CAN84	1109	Lambert et al.	
Czechoslovakia			
CSK84	2049	Czechoslovak Academy of Sciences	
Denmark			
DEN72	403	Allardt & Uusitalo	
DEN72l	315	Allardt & Uusitalo	In-law information
DEN76	2017	Hansen	
England			
ENG63	202	Butler & Stokes	Panel start, weighted by .333
ENG64	182	Butler & Stokes	Panel continuation, weighted by .333
ENG66	199	Butler & Stokes	Panel continuation, weighted by .333
ENG69	328	Butler & Stokes	
ENG72	8867	Halsey et al.	
ENG74p	425	Barnes & Kaase	
ENG84w	664	Wright et al.	
Finland			
FIN72	344	Allardt & Uusitalo	
FIN72l	279	Allardt & Uusitalo	In-law information
FIN74p	403	Barnes & Kaase	
France			
FRA78	1365	Capdevielle et al.	
Germany			
GER75p	685	Barnes & Kaase	
GER76z	648	ZUMA - Zumabus 1	
GER77z	588	ZUMA - Zumabus 2	

(a)	(b)	(c)	(d)
GER78c	589	ZUMA - Capitalstudie	
GER78x	543	ZUMA - Wohlfahrtstudie	
GER79x	575	ZUMA - Zumabus 4	
GER79z	576	ZUMA - Zumabus 3	
GER80a	803	ZUMA - Allbus 1	
GER80c	557	ZUMA - Capitalstudie	
GER80z	620	ZUMA - Zumabus 5	
GER85w	664	Wright et al.	
GER87	275	International Social Survey Program	
Hungary			
HUN73	12065	Andorka	
HUN82	5198	Kolosi et al.	
HUN83	10415	Kulszar & Harcsa	
HUN86	1949	Kolosi	
India			
IND71	1130	Verba, Nie & Kim	
Ireland			
IRE73	1901	Jackson, Iutaka & Hutchinson	
Israel			
ISR74	3347	Matras, Weintraub & Kraus	
Italy			
ITA63	1211	Lopreato	
ITA68	919	Barnes	
ITA72	654	Barnes & Sani	
ITA75p	595	Barnes & Kaase	
Japan			
JAP55	1685	Odaka & Fukutake	
JAP65	1779	Yasuda	
JAP67	463	Ward & Kubota	
JAP75	2230	Tominaga	
Malaysia			
MAL76	906	Fain & Kheong	
MAL76l	1017	Fain & Kheong	In-law information
The Netherlands			
NET58	457	Gadourek	
NET70	759	Heunks, Jennings et al.	
NET71	370	Verba, Nie & Kim	
NET74p	364	Barnes & Kaase	
NET77	1418	CBS	
NET77e	590	Werkgroep National Verkiezingsonderzoek	
NET82	411	Heinen & Maas	
NET82u	1068	Ultee & Sixma	
NET85	1715	OSA	
NET87	300	Hermkens & Van Wijngaarden	
Northern Ireland			
NIR68	436	Rose	
NIR73	1976	Jackson, Iutaka & Hutchinson	
Norway			
NOR72	384	Allardt & Uusitalo	
NOR72l	316	Allardt & Uusitalo	In-law information

(a)	(b)	(c)	(d)
Philippines			
PHI68	6834	Population Institute	
PHI73	6370	Population Institute	
Poland			
POL72	30463	Zagorski	
Spain			
SPA9091	2565	CIRES	
Sweden			
SWE60	564	Saerlvik	
SWE72	404	Allardt & Uusitalo	
SWE72l	337	Allardt & Uusitalo	In-law information
SWE80w	531	Wright et al.	
Switzerland			
SWI72	657	Kerr, Sidjanski & Smidtchen	
SWI76p	456	Barnes & Kaase	
Taiwan			
TAI70	1031	Grichting	
TAI70l	621	Grichting	In-law information
Turkey			
TUR78l	2342	Institute of Population Studies (?)	In-law information
United States			
USA56	637	Survey Research Center	
USA58	545	Survey Research Center	
USA60	421	Survey Research Center	
USA62	10852	Featherman & Hauser	
USA64	613	Political Behavior Program	
USA66	378	Political Behavior Program	
USA67	815	Verba & Nie	
USA68	515	Political Behavior Program	
USA70	493	Center for Political Studies	
USA72g	494	Davis & Smith	
USA73o	20676	Featherman & Hauser	
USA73g	435	Davis & Smith	
USA74g	395	Davis & Smith	
USA74p	486	Davis & Smith	
USA75g	419	Davis & Smith	
USA76g	393	Davis & Smith	
USA77g	449	Davis & Smith	
USA78g	414	Davis & Smith	
USA80w	569	Wright et al.	
USA80g	390	Davis & Smith	
USA82g	480	Davis & Smith	
USA83g	435	Davis & Smith	
USA84g	361	Davis & Smith	
USA85g	440	Davis & Smith	
USA86g	406	Davis & Smith	
USA87g	514	Davis & Smith	
USA88g	397	Davis & Smith	

Notes:
(a) Study acronym; (b) Number of men between 25 and 64 years of age with complete information; (c) Number of women between 25 and 64 years of age with complete information; (d) Data reference (see Data Reference Section); (e) Sample characteristics.

'In-law information' means that the information was obtained through the wife.

Chapter 18

CAREER AND ORGANIZATION STRUCTURE. CAREER MOBILITY OF THREE GENERATIONS IN A CLOSED NAVAL OFFICER CORPS

Gertjan Oosterhuis

18.1. Introduction

One of the interesting questions in the research of generations is the relation between opportunity and achievement. It may be assumed in general that generations with poor opportunities, are less capable of reaching desirable positions. Nevertheless, each generation plays its part in holding the top positions of their time. It is therefore an interesting question which individual capacities are needed to reach the top under less favorable conditions.

Are there special capacities needed in times of scarcity? Or is it so that under less favorable conditions only those individuals reach the top who excel in the qualities that are also needed under normal conditions?

One of the difficulties in analyzing these questions is the availability of sufficient data. One should have the disposal of the personal data of one or more generations as well as data describing the opportunity structure in which they have lived. For the society as a whole these data are very difficult to collect. In a relatively closed organization with an internal labor market however, these problems are much easier to manage. Personnel departments collect and file a lot of personal data (age, education, fitness-reports), and data about the organization structure (numbers of personnel, job vacancies, turnover, promotions) are collected as well.

In this chapter the careers of three generations of naval officers are analyzed. The first generation entered the Royal Netherlands Navy at the beginning of the twentieth century, the second enlisted in the thirties and the third in the fifties. The naval officer corps is a relatively closed internal labor

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