

# Factors Related to Level of Educational Attainment in Ireland\*

VINCENT GREANEY

and

THOMAS KELLAGHAN

*St Patrick's College, Dublin*

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## I INTRODUCTION

In this paper we comment on a number of aspects of the three preceding papers which were written in response to our study, *Equality of Opportunity in Irish Schools* (Greaney and Kellaghan, 1984). We are pleased that equality in education, about which there has been relatively little debate in Ireland, was taken up by the authors of the papers as a topic in need of analysis and discussion and we welcome the consideration and time they have given to a discussion of our work.

One of the papers (Lynch) examines two of the concepts used in our study, meritocracy and ability, and also considers relationships between ability and social class. The other two papers are based on statistical re-analyses of our data. In one case the authors (Raftery and Hout) requested our data, which were made available to them, and so were in a position to engage in extensive reformatting and analyses. In their analyses, they used a useful statistical model which shed further light on the relative contributions of socio-economic status and ability to retention in the educational system. The effect of reformatting their data, however, was such that some of their analyses are based on slightly different categorisations of students than the ones we used. Thus, their treatment of the small number of students who

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transferred from one type of post-primary school to another differs from ours. (Their assumption that we treated transfer students as dropouts, however, is incorrect.) More important from the point of view of comparing findings is the fact that they employed a categorisation of parental occupations that differed from ours. Their categorisation had the advantage that it distinguished between manual and non-manual occupations, but the disadvantage that all farmers (other than farm labourers) appear to have been assigned to a single category.

The analyses in the second paper (Whelan and Whelan) are limited to information which could be obtained from our book. The paper is a shortened version of an earlier report (Whelan and Whelan, 1984) and a number of tables presented in the original report are omitted in the paper. However, it will be necessary for us to refer to some of the omitted tables since they are used as sources of evidence in the paper.

Two general points may be made about differences between the original study and the re-analyses. Firstly, the re-analyses were carried out on a limited number of the independent variables which were used in the original study. Both re-analyses include socio-economic status (based on parental occupation), verbal reasoning ability, and gender, while Raftery and Hout's analyses also include the type of post-primary school which students had attended. Our study, on the other hand, included many other variables relating to the students' homes, their educational history, the type of primary school they had attended, and their personal characteristics (cf. Greaney and Kellaghan, 1984, Table 2.2). A second point of difference between our study and the re-analyses is that the latter limit themselves to the use of one dependent variable based on "attainment" (i.e., the last level of the system reached by a student). Our analyses, on the other hand, included students' "achievement", as exemplified by performance on public examinations, as well as students' attainment. Obviously, our data provide a more comprehensive picture of students' scholastic progress than does either of the re-analyses. The significance of this point becomes clear when we consider the correlates of retention in the system after the end of junior cycle; at this stage, neither ability nor socio-economic status was as important as school achievement (as measured by performance on State examinations). This was so for retention in Secondary school after the Intermediate Certificate examination and, in particular, in the decision after the Leaving Certificate examination to terminate one's formal education or to proceed to third-level education.

Despite differences in analyses between the studies, the overall conclusions to the analyses seem substantially similar. Whelan and Whelan conclude that "educational participation rates are heavily influenced by social class. Equally clearly ability is a major factor in determining progress through the system," while Raftery and Hout conclude that "both father's occupation and verbal

ability have substantial and significant effects on the probability of making each transition up to and including the completion of senior cycle. For the transition to post-primary school, the effect of father's occupation is about half that of verbal ability . . ." These conclusions may be compared with ours that "of all the primary-school factors which were related to persistence in or withdrawal from the educational system, the most important was the verbal reasoning ability of a student" (p. 248). Further, we conclude that relationships between socio-economic status and participation strongly indicate that the educational system is not meritocratic.

Not only is the system not meritocratic in the sense that some more able students are lost from it at relatively early stages, it is also non-meritocratic in the sense that the loss of such students is proportionally greater among students from non-professional backgrounds (skilled, semi-skilled, and unskilled) than among students from professional backgrounds (p. 261).

There are a number of further points of agreement between our conclusions and those of Raftery and Hout. These relate to the diminishing effect of socio-economic status throughout the educational career of students, the absence of the influence of socio-economic class at entry to third level, the fact that, in general, gender was not related to persistence in the system, and the greater incidence of dropout among Vocational-school students than among Secondary-school students.

While these conclusions might point to substantial agreement between the findings of the three sets of analyses, it is also obvious that there is a perception of differences between the reports in their emphases, particularly relating to the relative importance for success in the system which is assigned to ability and socio-economic status. A difference is certainly perceived by the authors of both sets of re-analyses, who at times employ what to us appears to be an unnecessary level of rhetoric to underline those differences. There are also more obvious differences in detail in the findings of the analyses. These are most obvious in Whelan and Whelan's paper which presents findings that differ not only from ours but also from those of Raftery and Hout. In this paper we will address the differences which arise from a consideration of the analytic papers, as well as considering some of the criticisms of our book which appeared in all three papers.

## II ATTRIBUTIONS, MISCONCEPTIONS AND ERRORS

First of all, we wish to comment on a number of positions and statements which are attributed to us by the authors of the three papers. Lynch asserts that our study assumes that meritocracy is a "legitimate" educational goal

and that we “implicitly place a value on it”. Raftery and Hout also refer to our “meritocratic ideal”. We neither accept nor reject in our book a meritocratic interpretation of equality of educational opportunity. The reasons for using the meritocratic interpretation were set out (clearly we believe) in our book from which we quote the following (to which we have added emphases):

In our study, . . . we shall carry out analyses which examine a model of opportunity *based on a meritocratic assumption*. We feel justified in doing this, not only because such analyses will provide data which will be similar, if not directly comparable, to those obtained in other countries, but also because some government policy statements on equality of opportunity, as we have seen, *seemed to assume the operation of meritocratic principles* (p. 27).

Thus, our choice of the meritocratic model, while recognising its problematic nature (p. 34), was based on our reading of what the system was trying to achieve, particularly in the period of reform of the 1960s. If ministerial statements had led us to believe that the system was attempting to achieve equality of outputs, irrespective of student origins and characteristics, we would have adopted a different design and strategy.

We also had difficulty in reconciling other positions attributed to us with what we had actually written. Raftery and Hout say that we claim “that the effect of socio-economic background is small relative to the dominant effect of ability.” We did conclude that the effect of background was smaller than that of ability, but we regarded both as substantial. Several of Lynch’s statements, of which we can only provide a few examples, created similar difficulty for us. She says that we believe talent to be limited (we have no reason to believe that this is so, though opportunities do appear to be limited), that ability is “a fixed entity”, that we regard intelligence as a “quantity” which people “possess” in varying and lesser degrees, and that some pupils are “essentially” lacking in some mental ability. We have epistemological problems with all these terms.

Apart from these general points, there are specific errors in Raftery and Hout’s comments on our study. They say that “most” of our discriminant analyses do not include socio-economic class and ability simultaneously. This is incorrect. All such analyses contain both variables and a number of them consider the relative importance of class and ability for survival in the system (Chapter 6). It would appear that the authors over-looked some of these analyses. They may also have been misled by the fact that in some of the analyses we employed reduced models, which contain fewer variables than full models. This may have led Raftery and Hout to believe that the results of two of our analyses are contradictory since ability had a stronger effect than socio-economic background in one (Table 6.11) while the con-

verse was true in the other (Table 3.11). Even if their interpretation of the data in terms of the variable with the stronger effect was correct, the results could not have been regarded as contradictory, since the data in the two tables refer to different samples of students at different time periods. Actually, Raftery and Hout are not even correct in identifying the more powerful variable. They appear to have limited their attention to the first of two functions. While socio-economic status had a higher standardised coefficient on the first function (Table 3.11), verbal reasoning ability entered the step-wise discriminant analysis prior to socio-economic status, as is clear in the note on p. 70.

Raftery and Hout raise a number of problems regarding the non-appearance of ability in a number of our discriminant analyses. They do recognise, however, in an accompanying footnote that this might be a function of the relationship between ability and other variables. This indeed is the case. For example, in Table 6.9, the inclusion of a number of ability-related measures, including satisfactory classroom behaviour (which correlated .49 with verbal reasoning ability) obviously led to the exclusion of ability. A similar explanation applies to Table 6.12 except that in this case the most powerful cognitive variable was performance on the Intermediate Certificate examination (which correlated .52 with verbal reasoning ability).

Whelan and Whelan also make a factual error in reading data when they state that "20 per cent of those (i.e., less able females) from professional and managerial backgrounds survive the transition to third level while the corresponding figure for those with unskilled origins is 9 per cent." The correct percentages, which can be calculated from Table 9.6 of our book, are 15.2 per cent and 7.3 per cent. It is obvious, in this instance, that they have interpreted the two transitional probabilities (.20 and .09) in Table 7.3 (Whelan and Whelan, 1984) as percentages.

Finally, Raftery and Hout say that our analyses could not distinguish between the effect of type of school attended and the effect of student characteristics. We addressed this issue in our chapter on examinations.

### III ANALYTIC PROCEDURES AND MODELS

We are criticised by Raftery and Hout for an over-reliance on bivariate analyses and, in particular, for the way in which part/whole comparisons were made. We deliberately selected a system of comparing mean scores for individual samples with the corresponding mean score of the original larger sample since it seemed to summarise in a simple manner the nature of change over time. Since the book was written with a broad readership in mind, we attempted to keep the level of analyses as simple as possible without, we hope, underestimating the complexity of the phenomena under investigation.

The relegation of much information to footnotes and the provision of a glossary of terms reflected our concern to communicate with a broad readership. This does not mean that we do not appreciate the value of multivariate analyses for the reasons which Raftery and Hout advance. However, neither are we unaware of their limitations, apart from those involving problems of communication.

We are also criticised by Raftery and Hout, as well as by Whelan and Whelan, for our failure to use an adequate statistical model. However, as we shall show, the use of a model does not solve all analytic problems. Major problems can arise when there is bias in the selection of a model and when inferences are made on the basis of a model which, when examined, are found to be inconsistent with aspects of the original data. This is something that occurs especially in the Whelan and Whelan paper.

An example of how bias in the selection of a model leads to inadequate inference is to be found in Whelan and Whelan's examination of the goodness of fit of two models. Their first model specified that a person's educational destination was independent of his or her social origin (independence model). The second specified that a person's social origin and educational destination were equally spaced (the uniform association model). Not surprisingly, they find that the independence model fits the data significantly less well than the uniform association model. Thus, it can be concluded, one's educational destination is not independent of one's social origin; the system is not *totally* meritocratic. What cannot be concluded, however, is the *extent* to which one's destination is dependent on one's background or on other factors.

It is possible to construct an alternative to Whelan and Whelan's model, which admittedly provides no more information than theirs, but is worth examining to illustrate the operation of bias in the selection of a model. In the alternative, independence and uniform association models are again specified. In the independence model this time, educational destination is posited as being independent of a student's ability while the uniform association model specifies that a person's ability and educational destination are equally spaced. We used the same procedure as Whelan and Whelan, as outlined by Breen (1984), to test the goodness of fit of the models. To do this, students in the total sample were divided into four levels of ability on the basis of percentile rank. The number in each category corresponded as closely as possible to the numbers in Whelan and Whelan's four categories. The  $\chi^2_{LR}$  value for the independence model was 154.5 (df = 12), that for the uniform association model 26.34 (df = 11). Again, the uniform association model provides a much better fit than the independence model. So, it can be concluded one's educational destination is not independent of one's ability. Taken together, the two approaches show that persistence in the

system is related to both ability and socio-economic background.

A further problem with models is that one may attempt to make inferences on the basis of the model without checking whether or not these inferences are compatible with the original raw data. For example, Whelan and Whelan conclude that less able males from the professional/intermediate group enjoy advantages over the unskilled group of 20:1 in their probability of entering third-level education. The actual figures suggest otherwise. Of 42 less able males from professional/intermediate professional homes, 5 entered third-level (probability = .12) while of 38 less able males from unskilled backgrounds, one entered third level (probability = .03).

Again they note that

more able females from professional and managerial origins enjoy relative advantages over other females in relation to the probabilities of making the transition from second level to third level. These advantages are substantially greater than those found in the corresponding male group.

How great are these relative advantages which the upper-class more able females enjoy? Reverting to the original data we find that out of a total of 27 more able females from professional and managerial backgrounds who completed senior cycle, 10 enrol in third-level education ( $p = .37$ ). The corresponding figures for other females are 89 completing senior cycle and 18 entering third level ( $p = .20$ ). Are the advantages substantially greater than those found in the corresponding male group? The original data clearly suggest not. More able boys from professional and managerial backgrounds have a better chance than similar girls of transferring to third level. Out of a total of 30 more able boys from professional and managerial backgrounds who completed senior cycle, 15 entered third level ( $p = .5$ ), whereas of the 76 remaining boys, 21 advanced to third level ( $p = .28$ ). We fail to see how any comparison of these two groups leads to the conclusion of a substantial advantage in favour of more able females.

In a similar lack of attention to the original data, Raftery and Hout conclude, on the basis of the application of their model, that both socio-economic status and ability were substantial and significant for transition during senior cycle (it should be emphasised that the number of dropouts at this juncture was very small). When one examines the actual data, one finds that dropouts during senior cycle scored lower (though not significantly) on the ability measure and higher (again not significantly) on the socio-economic measure. Does one accept an inference based on a poorly fitting model or one based on the actual data which the model purports to represent?

The problem of not paying adequate attention to the raw data becomes acute when the numbers involved are small, which is the case in our data

at later stages of students' educational careers. It arises in Whelan and Whelan's analysis of outflow patterns in which they compute disparity ratios for entry to third level which are based on their best-fitting association model (Table 1). The ratios are calculated by dividing the figure for students from professional backgrounds by the corresponding figure for students from unskilled backgrounds. The disparity ratios equal 20.2 for less able males, approximately 7 for less able females, approximately 2 for high ability males, and 6.2 for high ability females. If they had looked at the actual numbers on which these ratios are based, they would have found that the numbers of students from unskilled backgrounds for the four groups were 1, 0, 3 and 0 respectively.

In many instances, Whelan and Whelan's use of extreme groups to highlight particular points leads them to make strong inferences on the basis of very small numbers. For example, they conclude that "less able male students from unskilled origins are ten times more likely to fail to enter the post-primary sector than those from professional and managerial origins". Their emphasis on those "more likely to fail to enter" tends to divert the reader from the fact that over 70 per cent of less able pupils (27 of 35) from unskilled origins enrolled in post-primary school. The corresponding figure for less able males from professional and managerial backgrounds was 97.5 per cent (41 of 42). Thus, the ten to one ratio highlighted by Whelan and Whelan was based on *one* male from a professional and managerial background. Had one further male from this background failed to enter post-primary the ratio would have been reduced substantially.

#### IV VERBAL ABILITY

Verbal ability is one of the concepts which Lynch addresses in her paper. She begins by asserting that "the only place one finds anything approaching a definition of verbal reasoning ability is on page 206". This assertion is incorrect. A formal definition is provided on page 271.

However, our concern in this work was not with the nature of ability, about which a voluminous literature exists, but with its role in the educational system. It is obvious that to define ability in this context as "whatever it is on the basis of which the system distributes its benefits" has the logical fault of circularity. But in terms of the operation of the educational system, it would appear, as Green (1980) has suggested and as our data confirm, that ability and benefit are correlative. The circularity in fact describes the behaviour of the system: the distribution of ability (whatever its nature or origin) to some extent determines the distribution of educational benefits by the system (Green, 1980). Thus,



The logical circularity . . . reflects a social reality. Understanding it is absolutely vital to understanding the system. It explains, for example, why no system is in serious trouble as long as only one of these terms is opened for redefinition. The system can always provide an operational definition in terms of the other. It also explains why no policy proposal for the system would be truly radical if it seeks redefinitions of either ability or benefit as though they were independent terms (Green, 1980, p. 52).

If ability is an important variable in our present educational system, it does not follow of course that tests of ability (including tests of intelligence) play an important role in the system, as Lynch seems to accept, apparently on the basis of Simon's (1978) statement that intelligence tests are "the very keystone of the present educational system". A little reflection would indicate that a great many countries in the world (including our own) make little use of formal tests of ability. If the keystone does not exist, how do the structures not only stand but continue to expand in a variety of environments? Perhaps it would have been fairer to readers if Lynch had made clear that Simon's conclusion was based on his experience of the British educational system and, secondly, that he formed his conclusion on the basis of historical evidence which, it is now clear, was inadequate (Sutherland, 1984).

Lynch's discussion of the history of mental testing raises a number of issues, only a few of which we can attend to here. In tracing the origins of mental testing, she fails to refer to the work of Binet and his collaborators (e.g., Binet, 1911; Binet and Simon, 1905), although it is on this work that the tradition of testing is based. She seems to regard Terman as the key person in this context though he actually did little to develop or extend the work of Binet, his contribution being largely limited to the popularisation of testing in both individual and group form. Though he devoted considerable time to the construction of tests, we are not aware, as Lynch claims, that he ever selected items for these tests on the basis of the items' ability to discriminate between people from different socio-economic backgrounds. If Lynch has found a work by Terman which substantiates this claim, then it seems extraordinary that she should fail to provide readers with a reference to the work.

It is obvious from Lynch's comments that she has been less than successful in tracking down attempts to develop "class-fair" or "culture-fair" tests. Why, she asks, are tests not made "class-fair" or "race-fair", since she believes "there is no logical reason why they should not be". Information on attempts to build tests which would not discriminate between people from different cultural backgrounds and on the problems inherent in this task is in fact freely available in a variety of sources (e.g., Anastasi, 1966; Brislin, Lonner,

and Thorndike, 1974; Cronback and Drenth, 1972; Eells *et al.*, 1951; Gregory, 1981; Jensen, 1980; Thorndike, 1971).

## V SOCIO-ECONOMIC STATUS

All three papers raise issues about our conceptualisation and measurement of socio-economic status. We must admit, however, that we are surprised that none of the papers undertook an analysis of the concept of social class, which we regard as extremely problematic. Our use of parental occupation as a measure of students' home backgrounds was largely because this has been done in most studies of equality of educational opportunity which have been carried out in Ireland over the past two decades.

Lynch raises doubts about the accuracy of our information on parental occupation, questioning the accuracy of teachers' knowledge in large classes and schools in urban areas. All primary schools are required to record information on the parental occupation of students in the school register. We had no reason to believe that teachers in any type of school or area are less diligent in obtaining this information than are teachers in other types of school or area.

The question raised about the effect of our categorisation of parental occupations on the relationship between parental occupation and students' ability can be answered by examining the effect of a recoding of the occupations on the relationship. It was possible for us to reclassify 487 of the 500 occupations according to the Hope-Goldthorpe schema, which is a seven point classification and distinguishes between manual and non-manual occupations. We found the correlation between the recoded socio-economic measure and scores on the verbal reasoning ability test to be .32 (compared to .30 between the original measure and ability). Thus, Whelan and Whelan's suggestion that we may have underestimated the socio-economic status-ability relationship finds little support. Similarly, their expectation that a more appropriate categorisation would have increased the correlation between socio-economic status and level of educational attainment (persistence in the system) is not met; the value of the correlation increased from -.38 in the original analysis to -.42 in the analysis using the revised system of classification.

## VI SOCIO-ECONOMIC STATUS AND ABILITY

In one place, Lynch states that we claim that socio-economic status and verbal ability are not related, though elsewhere our claim is presented as being that ability is only *relatively* independent of social class. We in fact report a statistically significant correlation of .3 between the two variables and the relationship is obvious in many of our analyses. Lynch claims that

this correlation "between ability and social class, may be more significant than it appears." While we were unable to interpret this statement with any degree of confidence, it would appear that Lynch is arguing that measures of verbal ability are heavily and intentionally biased in favour of some social classes and against others. In her argument, she introduces the concept of statistical significance without apparently taking into account the fact that very low magnitudes of correlation can be "statistically significant" (which only means that an observed relationship was not due to chance at a specified level of probability). Whether or not the amount of variance shared by two variables which are related is so great that one suspects the variables may be measuring the same underlying construct is an entirely different matter. If the amount of shared variance is less than 10 per cent (which is the case for our measures of ability and socio-economic status), one could not possibly conclude that one measure could be used as a surrogate or substitute for the other. This conclusion is confirmed by Raftery and Hout's findings.

In our book, we referred to a paper by White (1982) which the reader could consult to assess the magnitude of our correlation against the findings of other studies. White's paper contains a large number and variety of statistics and we did not specifically refer to any of them. We left it to the reader to judge for himself or herself whether our figure seemed reasonable in the light of those presented in White's paper. However, Lynch selected certain figures from White's paper with which to compare ours and this selection requires some comment.

First, the correlation data she selects for published and unpublished work refer to correlations between socio-economic status and verbal *achievement* (not ability) (White, 1982, Table 5). Despite the fact that in her paper she equates our measure of ability with an IQ measure, she ignores White's data in the same table on the relationship between socio-economic status and IQ. Second, Lynch does not tell the reader that the data in this table on type of publication include aggregated, confounded and individual student data. However, it is well known that aggregated data provide higher correlations than individual data (Knapp, 1977) and indeed Figures 1 and 2 in White's paper make this point very clear. The inclusion of aggregated data obviously has the effect of raising the overall levels of the correlations and also means that the figures are not comparable with ours. Third, the data in the table are based on non-status measures of home background (e.g., home atmosphere) as well as status measures (based on parental occupation, education and income). Obviously, our correlation should be compared with status measures. Indeed, Lynch seems to accept the idea, supported by White's data, that measures of scholastic ability and performance are more closely related to measures of family circumstances than to measures of socio-economic status based on parental occupation, income, or education. What

she apparently fails to appreciate is that family characteristics, such as home atmosphere, are not synonymous with socio-economic status, though, as White points out they are "sometimes incorrectly referred to as SES". Many studies (cf. Laosa and Sigel, 1982; Marjoribanks, 1979), including ones in Ireland (Greaney and Hegarty, 1983; Kellaghan, 1977a, 1977b), have clearly demonstrated that variance on home-environment measures exists *within* socio-economic groupings. Perhaps it is in advantageous home conditions that one acquires "cultural capital", a term, which as Lynch points out, we do not use. If "cultural capital" is simply as Lynch describes it a label to describe "a highly relevant educational ability or series of abilities" it would appear to have no greater explanatory value than Terman's (1906) concept of intellectual ability as a "bank account". We do not find either concept particularly illuminating.

Our study may have given the impression that we do not have an interest in the determinants of scholastic ability or achievement. Whelan and Whelan are critical of the inadequate attention which we paid to the reasons for the substantial variations by social class in ability at age eleven and, in particular, of our failure to acknowledge the importance of "family climate" factors. However, our study was focused on the operation of the educational system and, as we pointed out above, we selected parental occupation as an index of home environment not for any theoretical reason but because it has been used in most studies of equality of educational opportunity which have been carried out in this country. The identification of the family climate factors related to the development of children's abilities, achievements and personalities is obviously an important area of research which has received considerable attention in recent years.

## VII ABILITY, SOCIO-ECONOMIC CLASS AND PARTICIPATION IN EDUCATION

A central issue arising from the empirical analyses is whether ability or socio-economic background is the more important variable for retention in the educational system. Indeed, it would appear that it was our emphasis on the importance of ability that instigated the re-analyses which form the basis of the papers which appear in this journal. It was also claimed that we underestimated the effect of socio-economic class. However, our data clearly indicate that at the end of primary schooling, ability and socio-economic status were the two variables among over 30 variables which best discriminated between those who enrolled in a Secondary school, those who enrolled in a Vocational school and terminal leavers. The relationships between socio-economic status and persistence in Secondary school during junior cycle (p. 116) and at the end of junior cycle (p. 130) were highlighted. We also

identified the increased selectivity of the system in terms of socio-economic status, pointing to the tendency over time among second-level students for the representation of students from higher socio-economic backgrounds to increase, while the representation of students from low socio-economic backgrounds decreased (pp. 87, 99, 108, 216, 217). The selective effect of socio-economic status was particularly strong between the commencement of post-primary schooling and the beginning of senior cycle; 62.7 per cent of students from partly skilled and unskilled backgrounds, but only 27.5 per cent of students from professional and intermediate professional backgrounds, dropped out during this period (Tables 4.5 and 5.4).

The main difference between our study and the other papers in assessing the effects of socio-economic status relates to drop-out following junior cycle. For example, Raftery and Hout say that "it is never clear whether the underrepresentation of students from lower class backgrounds among those sitting the Leaving Certificate examination reflects a prior selection effect or additional social class effects operating during the senior cycle." We believe it is clear that the former is the position. As we have shown above, reliance by Raftery and Hout on their model led them to a conclusion on this issue that conflicts with the actual data.

We believe Whelan and Whelan misrepresented the effects of socio-economic status during senior cycle for a different reason. Their third transition period (completion of junior cycle to completion of senior cycle) covers two transitions used in most of our analyses (but not those in Chapter 9): end of junior cycle to commencement of senior cycle and commencement of senior cycle to end of senior cycle. In the first of the transitions (and the most important from the point of view of the numbers involved), we found that remaining on at school was related to social class. Among the relatively small number who dropped out during senior cycle, however, we were not able to detect a class effect. The class effect at the senior-cycle level found by Whelan and Whelan appears to reflect the relatively large drop-out rate of students from lower socio-economic backgrounds which occurred after the completion of junior cycle but prior to the commencement of senior cycle.

Raftery and Hout are in agreement with us that socio-economic status does not operate at the point of transfer from second to third level. Whelan and Whelan, however, reach a different conclusion partly on the basis of the application of their model and partly on the basis of data presented in a table in their original report (Whelan and Whelan, 1984, p. 162). However, the data in the table are incorrect. First, though the text indicates that the data refer to those completing second cycle, they are in fact confined to Secondary school students and omit Vocational school students. Second, the percentages under the heading "Leaving Cert Terminal" do not correctly represent terminal leavers in Secondary schools but would appear to refer

to all students in Secondary schools who took the examination, not just those who left the system at that point. Third, when the socio-economic distributions of terminal leavers and entrants to third level are compared, no statistically significant difference is found ( $\chi^2 = 4.43$ ;  $df = 3$ ). This supports our conclusion, based on analyses elsewhere, that "higher-education students and Leaving Certificate terminal leavers were not differentiated in terms of socio-economic status" (p. 202).

The best overall picture of the relative effects of socio-economic status and ability is to be found in Raftery and Hout's analysis. The weights for entry to post-primary school in Table 1 of their paper indicate that both variables have significant effects at this stage. Throughout second level, both variables again have significant effects. At the point of transfer to third level, ability has a significant effect, while socio-economic status does not. These findings are in agreement with ours with one exception relating to dropout during senior cycle.

To compare the relative size of the effects of the two variables, Raftery and Hout decompose deviance at each transition point and devise a ratio in which the numerator represents variance attributable to socio-economic status uniquely as well as variance shared by socio-economic status and ability and the denominator represents variance explained by ability independently of socio-economic status. Inverting the index provides an estimate of the increment in variance which can be attributed to ability over and above that which can be attributed to socio-economic status and the joint effect of that variable and ability. The inverted ratio yields indices of 2.46 for entry to post-primary school, 1.16 for transitions within second-level schools and 9.31 for entry to third-level education. Thus, in their model, ability has more than twice the explanatory power of socio-economic status at entry to post-primary school, slightly more throughout the post-primary period, and over nine times as much explanatory power at the point of transition to third level.

### VIII CONCLUSION

An examination of the re-analysis of our data indicates that Raftery and Hout tend to support the main conclusions in our book, while Whelan and Whelan's findings are consistent with ours up to the end of junior cycle. We attributed differences beyond this point to alternative methods of grouping of data and to the application of an inappropriate model in the re-analyses to explain the transition from post-primary to third-level education.

The reviewers of our study appear to have reacted more to the last sentence of our book than to its overall findings. A reading of the book and in particular of the last chapter could scarcely lead to the conclusion that we

regard the system as meritocratic. We recognised that students of high ability are lost from the system and that the loss was not equally spread across students from all socio-economic backgrounds. However, we did conclude that ability was a more important consideration than socio-economic class for retention in the system and reading the findings of the re-analyses of our data did not cause us to change that conclusion. We feel that in interpretations of the re-analyses, the authors of the papers in this symposium have placed undue emphasis on the role of socio-economic status. For example, Raftery and Hout conclude that "students with the same ability but higher social class backgrounds have a better chance of success in Irish schools." Their own analyses also indicate that students of the same socio-economic status but of higher ability have an even better chance of success.

In the concentration on socio-economic status and ability in these papers, there is a danger of narrowing the debate on the educational system to these two factors. An examination of a number of the analyses in our book and also in Raftery and Hout's paper, however, indicates that neither ability nor socio-economic status, uniquely or jointly, explain considerable portions of variance in such factors as level of educational attainment, first occupation, or performance on State examinations. Obviously, there are many factors which influence a student's progress through the educational system. Some of these were included in our study but obviously many others were not.

At the same time, we do have to recognise that ability and socio-economic background are related to students' attainment and achievement and this has to be taken into account in planning educational provision. We do not agree with Raftery and Hout, however, that our data lead to the conclusion that "to be effective, future efforts to equalise the educational opportunities of all social classes in Ireland must focus on getting disadvantaged students into *secondary* schools and keeping them there." Such a conclusion might be warranted if the data on which it had been based had been obtained in an experimental study in which students had been randomly assigned to different kinds of school. It cannot be made on the basis of survey data. If the analyses and re-analyses in this review demonstrate anything, it is that the diagnosis of educational problems is not a simple matter. Neither, unfortunately, is their solution.

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