

We blame the parents! A response to “*Cultural capital as an explanation of variation in participation in higher education*” by John Noble and Peter Davies in the British Journal of Sociology of Education (volume 30 Issue 5)

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Abstract : This paper offers a response to a recent article where the authors argue cultural capital is the only determinant of the propensity of young people to seek to enter higher education, dismissing other indicators such as attainment and social class. This response questions the support the original authors draw from other literature and offers criticism of the sample used in their study, the survey tool employed, the analysis undertaken and their conclusions relating to the agenda of 'widening participation'. In reality, the wider literature suggests that social class, as represented by parental occupation, continues to have a role in explaining educational trajectories, even once other factors are controlled for.

Keywords : social class, higher education, widening participation, cultural capital, methodology

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At the recent annual conference of the British Educational Research Association (September 2009), one of the authors of this paper chaired a workshop session that included a presentation of the paper cited in the title; the second author was also present. Discussion was animated and we were surprised by the reaction of colleagues attending the workshop. As a consequence, we have collated some of the insightful comments made in response to the presentation, and our subsequent thoughts into a critique of the work presented in the authors' full paper.

The principal thesis presented by Noble and Davies (2009) is that traditional conceptualisations of social class have no impact on young people's decisions regarding whether to apply for higher education once attainment at 16 is taken into account statistically. To support this view, they present findings from three recent studies, as well as their own data collected from three schools. They conclude that parental occupation and educational levels are not useful predictors for which 16 year-olds will progress to higher education. The paper then presents a measure of cultural capital combining an interest in current affairs and classical music as a useful predictor, concluding that the current focus of widening participation efforts are misguided as a result.

We argue that the supporting evidence from previous quantitative studies is not as reported and, in fact, speaks for an identifiable impact of parental occupation and education on young people's intentions, albeit reduced significantly by attainment at GCSE. We also argue that the conceptualisation of cultural capital is not well-developed by the authors and that this has important ramifications for the reliability and usefulness of their analysis. Finally, we argue Noble and Davies have missed the point about widening participation, specifically in terms of the age groups targeted and the role of parental occupation in targeting individuals.

Evidence from the literature

Noble and Davies draw the inspiration for their study and subsequent analysis from three other recent papers: Chowdry *et al* (2008), Davies, Mangan and Hughes (2009) and Gorard (2008). They suggest all three provide compelling evidence that parental occupation and education levels are irrelevant to demand for higher education once attainment at 18 is factored into the equation; *“[T]hese studies find that after taking account of students’ school attainments, there is no observable effect of parents’ occupation and education on the likelihood of participation in higher education”* (Noble and Davies 2009, 593). However, on closer inspection, these papers provide far a more complex and nuanced picture, especially given the slightly different datasets with which they work.

Chowdry *et al* (2008) reports a significant and impressive study undertaken by the Institute of Fiscal Studies, forging a powerful connection between schools-based and HE-based datasets. This innovative linkage enabled young people to be tracked through school (including information on attainment at Key Stages (KS) 2 to 5) and into higher education. The dataset used provided composite variables representing (a) household deprivation, and (b) average educational levels in the young person’s neighbourhood. Neither parental occupation nor parental education levels were measured directly, with the variables relying primarily on area-based statistics. Numerous recent studies (e.g. Batey and Brown 2007; Rees, Power and Taylor 2007; Harrison and Hatt 2010) have challenged the idea that ‘we are where we live’, especially in the context of educational deprivation.

One of the principal findings of Chowdry *et al* (2008) was that educational attainment at 18 does indeed explain the vast majority of variation in higher education progression rates. However, and contrary to the presentation in Noble and Davies' paper, they do still find a significant effect both for household deprivation and local average educational level. Whilst admittedly small, there is nevertheless a social class difference in who progresses once attainment at KS5 (and other background variables) are held constant.

It is important to unpick this further. In reality, initial decisions about higher education are taken either consciously or by default before young people enter post-compulsory education (e.g. Maras *et al* 2007). Many young people from lower socio-economic groups do not progress to Level 3 study, effectively barring themselves from entry to higher education. Many others make their choice about higher education between KS4 and KS5; i.e. in Year 12 or early Year 13. If we are concerned with how demand for higher education is constructed, we need to focus on attainment up to the point when decisions are being taken. Chowdry *et al* (2008) focus their analysis on the explanatory power of KS5 results, but it could be argued attainment at KS4 is more salient; GCSE results are the temporally closest indication to young people of whether they have the capacity to contemplate higher education.

Returning to Chowdry *et al's* (2008) figures, we find KS4 attainment does indeed predict most of the variation in university progression rates. However, household deprivation and local average educational level do once again come out as significant predictors, and this time the effect is larger. For example, there is a 5% difference in progression rates between girls from the highest and lowest household deprivation groups, with 3.2% for boys; we return to this shortly. Under closer scrutiny, Chowdry *et al* in fact suggest a measurable and significant effect of deprivation

and education on whether young people with the same GCSE results go on to higher education. That some of this effect disappears by KS5 is likely to be due to the act of applying for a university place and the motivational aspiration it creates; young people working towards getting their required grades are likely to try harder than those without specified targets (and related additional staff assistance) not intending to remain in education. Reversing Chowdry *et al's* analysis to provide a different insight, young people going on to higher education did better at KS5, even after KS4 attainment was controlled for.

Davies, Mangan and Hughes (2009) report a study into 'marginal' young people in post-16 education and their intentions towards higher education. They focused particularly on a group who stated that they were 'unsure' about whether they wanted to progress and what predictors could be found for membership of this group. They found household income and parental education levels were not significant predictors for this uncertainty; it is not reported whether they were predictors for those with greater certainty about their plans, either for or against progression. However, one of the significant negative predictors for being 'unsure' was attendance at an independent school; they had a higher level of certainty about their plans. This almost certainly acts as a proxy for both parental occupation and education, given the fee-paying nature of independent schools. Once again we find that there is evidence for a social class effect on decision-making about progression to university, albeit operating through a proxy. Certainly there is no evidence presented in this paper speaking against the possibility of a social class effect for students who are not 'unsure' at the age of 16 or 17.

Gorard (2008) focuses primarily on the difficulties in measuring and analysing figures for progression to higher education, especially in the context of social class. He demonstrates that

the data available for comparison purposes is of dubious reliability due to missing information, changing definitions and populations, inadequate sampling and a failure to examine other explanatory variables. Whilst he examines the relevance of prior qualifications, he presents secondary data (Connor and Dewson 2001) to suggest that, in fact, a social class difference in progression does continue to exist once results at 18 are held constant; 3% for high achievers and 4% for lower achievers. He presents no new data on this topic.

Further examination of the literature used by Noble and Davies to support their premise that traditional measures of social class do not impact on post-16 decisions to demand higher education, reveals that the foundation is distinctly shaky. While all three papers do demonstrate the primary role of attainment in influencing higher education decision-making, they are equivocal at best about the role of parental occupation or education. In fact, they do all seem to allow for a small, but significant, social class effect exerting an influence over whether suitably qualified young people demand higher education or not. Furthermore, none specifically addresses the important question of whether the desire to progress works to enhance motivation and performance during Level 3 study.

Methodology and analysis

Turning to the specific study reported by Noble and Davies, we find a number of significant problems with the methodology and analysis, calling into question the reliability of their findings.

A key observation is that the sample chosen by the authors is a highly rarefied subset of the post-compulsory education sector. It does not include young people studying in further education colleges, who comprise 37% of entrants to higher education and who are disproportionately drawn from the lower socio-economic groups (UCAS 2009). Furthermore, by using only young people studying for A Levels, they are missing the 20% of young entrants who qualify for higher education with Level 3 vocational or other non-traditional qualifications; again these are most often pursued by those from lower socio-economic groups. Although the authors acknowledge this omission, it raises serious concerns about the sample's representativeness, as it only comprises young people following the most traditional qualifications in the most traditional post-compulsory settings. It is particularly questionable whether there was sufficient social class diversity to reach valid conclusions about its effect on decision-making. It is notable the largest school with the lowest proportion of young people from professional or managerial households also had the lowest response rate at 56%, raising additional questions about which young people did not provide data and whether this provided a further source of sampling bias. Surprisingly, this is not discussed in the paper.

The choice of the Registrar-General's classification system also needs to be challenged. This was abandoned from official use in the run up to the 2001 Census in favour of the eight-point NS-SEC system (ONS 2009). The rationale for this change was to recognise the changing occupational profile in the United Kingdom, especially the growth of unskilled and semi-skilled non-manual occupations. While it is noted that the variable used in the regression models was whether the young person came from a professional or managerial household, these definitions have also

changed significantly over time. No justification is provided as to why the older, more simplistic and obsolete system was preferred in this study.

It was surprising to also find a very cursory treatment of gender in the study. Noble and Davies state that a significant percentage of the data on gender was missing from that collected, nevertheless concluding this had no bearing on the question at hand. This has to be viewed as a major weakness of the study given recent literature (e.g. Burke 2009; Evans 2009; David 2009) highlighting the vital intersection between class and gender in understanding educational decision-making. For example, Evans (2009, 351) concludes that *“a dynamic model of cultural capital must be one that integrates that gendered aspirations that individuals have and the various values which they place on certain forms of achievement”* and that *“cultural capital cannot be understood without including within that term the value to individuals of the social relations within which they live.”* In addition, a recent policy focus has settled on this issue with the emergence of ‘white working class boys’ as a specific under-represented group (NAO 2008). It is further noted that no data was recorded about ethnicity and the same criticisms apply; this is returned to below.

Survey instrument

Perhaps the most problematic aspect of the study is the conceptualisation of cultural capital used. The authors’ desire to devise a short questionnaire offering *“a valid and reliable measure of cultural capital”* (Noble and Davies 2009, 593) is noted, but the approach taken over-emphasises traditional and outdated artefacts of ‘highbrow’ culture to the exclusion of more contemporary

forms, and the questionnaire they design is not fit for this purpose. A number of specific criticisms are presented:

- There is no reference to following current affairs on the internet, which is now doubtlessly at least as important for young people than newspapers or radio; Helsper and Eynon (in press) report that eighty-five percent of the 14-17 age group use the internet for this purpose.
- The provision of cultural 'credit' for watching current affairs and other 'highbrow' television programmes, but not other similarly classed content, as such quality drama shows.
- The provision of cultural 'credit' for listening to the radio, regardless of the content or style of the station or the programmes; the audience for BBC Radio 4 is quite different than, say, for Kiss FM. The same criticism applies to the reading and owning different types of books.
- A highly subjective and ethnocentric judgement about the 'quality' of different musical forms and their relevance to cultural capital, with, for example, the various forms of 'world music' being excluded.
- No reference to other forms of classed cultural behaviour that could relate to demand for higher education, such as travel or use of information technology.
- The highly subjective nature of the declared frequency of undertaking cultural activities; "often", "sometimes" and "hardly ever" may mean very different things to different respondents. Does "often" with respect to "watching popular entertainment on television" necessarily mean as frequent as "often" in relation to "going to art galleries or museums"? The impact on someone's cultural capital 'score' is high.

Of the 48 cultural capital ‘points’ available within the construct, 15 (31%) were available for very traditional forms of engagement with current affairs and 12 (25%) for involvement with classical music to the exclusion of other forms. From this rarefied view of culture, it is hardly surprising the two key components statistically isolated were around these areas! The overall impression of the questions asked is of the authors seeking a measure of cultural capital appropriate to the 1930s, not for young people in twenty-first century multi-ethnic Britain. No discussion is presented for how the survey instrument articulated with recent theories of class and cultural capital.

A major recent study into cultural capital and social exclusion (Bennett *et al* 2009) demonstrates how Bourdieu’s notions of cultural capital still offer a powerful insight into cultural practices and social inequalities. The significance of nominally private and ‘innocent’ leisure activities remain great; cultural preferences distinguish lines of demarcation between not just class groupings and fractions thereof as Bourdieu (1984) himself analysed, but, not unsurprisingly, its intersections with gender, ethnicity and age. Noble and Davies briefly mention a few studies of cultural capital’s role in shaping student choice and subsequent university experience (e.g. Ball *et al* 2002), but overlook the extensive recent work of others. Notable omissions include the broader *Degrees of Choice* (see Reay, David and Ball 2005) project, one early paper from which is mentioned by the article. This bigger project also explored the role of *habitus* in higher education choice, something Noble and Davies do not address. Lehmann’s recent work (e.g. 2007, 2009) in Canada demonstrates how the importance of *habitus* is not restricted to the UK’s higher education system, but is prevalent in an arguably less class-bound society. Other gaps in the discussion of cultural capital include more recent work emanating from the TLRP-funded projects examining class and higher education participation (e.g. Reay, Crozier and Clayton 2009, 2010).

Analysis of results

The authors acknowledge some of the challenges presented by the dataset in moving to the analysis stage of their work, like, for example, the reliability of data on parental education and occupation when provided by young people (also see Harrison and Hatt 2009). However, they gloss over the most important feature of the dataset; namely, the size and statistical importance of the constant in the regression model. Eighty-two per cent of the dataset intended to progress into higher education¹, leaving relatively little variability for the model to explain. Given the overwhelming influence of the constant and attainment levels, that any other variables were significant predictors for progression is perhaps surprising. However, there was only statistical 'room' for a limited number of factors; a larger or more diverse sample (as discussed above) may have given scope for parental education, at least, to gain significance alongside the authors' flawed cultural capital constructs.

Furthermore, it would have been helpful to see a regression model without the school dummy variables as it is possible that these were proxies for social class through the well-known phenomenon of middle class families relocating in order to gain access to certain schools (Ball 2003). Given the broad homogeneity of the schools in the sample, it is unsurprising no effect on intentions towards higher education was identified.

Relevance to widening participation

¹ In fact, the proportion might have been higher, but the questionnaire asked specifically only about progression directly from school, thus excluding those planning a gap year; an increasingly common phenomenon.

The main purpose of Noble and Davies' paper appears to be to add to the critique of the 'widening participation' agenda. This has been a key plank of the government's policy intervention in higher education since the late 1990s, when the Dearing Report identified certain social and ethnic groups that were under-represented in the student body (NCIHE 1997). Widening participation has grown to a significant industry within higher education, costing £392 million a year through a variety of funding streams (NAO 2008). The overarching aim of widening participation is to make the university population more representative of the population as a whole and one strand of this is to encourage participation from the lower socio-economic groups. This is a particular focus of the government's Aimhigher initiative (HEFCE 2007).

Noble and Davies attempt to establish in the discussion of their findings that work aimed at encouraging 16 to 19 year olds from lower socio-economic groups (as defined by parental education or occupation) to apply for university is misdirected. They assert that *"recent quantitative studies have challenged the aims of [Aimhigher] in so far as they relate to interventions with students in 16–19 education"* (Noble and Davies 2009, 599).

However, this is misunderstanding of the mission and approach of Aimhigher. HEFCE (2007), which lays out the process for targeting what it describes as 'disadvantaged learners', makes clear that the focus of widening participation should be a wider group, taking in 13 to 19 year olds, but also arguing that *"work with younger learners from underrepresented groups, for example in primary schools, will be important in changing attitudes and sowing the seeds of raised ambition"* (HEFCE 2007, 8). This document makes clear that the specific activities undertaken by Aimhigher should be defined at the local partnership level.

In the main, Aimhigher partnerships have pursued a more nuanced understanding of widening participation than Noble and Davies give them credit for. Taking a typical example, the current Aimhigher West strategic plan (Aimhigher West 2008) places most emphasis on getting academically-able young people into 16 to 19 education, raising their attainment at GCSE and supporting their transition into pathways that can lead to higher education. The learner progression framework (Aimhigher West 2009) focuses work with the 16 to 19 age group on the provision of guidance to enable young people to make the best choices of higher education, in terms of course and institution. This is a component of decision-making that Noble and Davies do not address. The widening participation agenda is not simply about getting young people from lower socio-economic groups into higher education, but also to ensure that they make informed choices about the options available. A more significant study would have also looked at the types and status of higher education demanded by young people from various class backgrounds (Reay, Crozier and Clayton 2009, 2010; Zimdars, Sullivan and Heath 2009). Chowdry *et al* (2008) found both course and status of institution chosen are influenced to a degree by deprivation and education, as do several other studies Noble and Davies cites, but whose significance they do not discuss sufficiently (e.g. Ball *et al* 2002).

Aside from the fallacy of assuming that widening participation for the 16 to 19 age group is simply about 'getting them in', Noble and Davies (2009) make an additional error of understanding. They assert that as there are no identifiable social class differences by parental occupation or education among the post-16 cohort, cultural capital should instead be the focus of efforts and propose the use of questionnaires for targeting individuals. Leaving aside the criticisms of the questionnaire raised above, this recommendation simply misses the point. Getting more young people from lower socio-economic groups into higher education is not an end in itself. The widening

participation agenda is about removing structural and institutional barriers to progression (Gorard *et al* 2007) that operate by dint of birth and upbringing. There is ample evidence that cultural capital can form one such barrier, along with various forms of social and economic capital (Raphael Reed, Gates and Last, 2007).

Parental occupation (through classification systems like NS-SEC) is employed to measure social class because it has some degree of objective reality and an established methodology permitting its use for target-setting, monitoring and evaluation. While Gorard (2008) and Harrison and Hatt (2009) question its reliability, there are few obvious alternatives. Noble and Davies (2008) themselves find that young people's cultural capital significantly correlates with their parents' occupation. The reality is that parental occupation (and, by extension, education levels) are simply proxies for locating barriers. This is a point made in guidance to Aimhigher practitioners: *"the first stage in targeting often involves 'proxies' for social class or broad measures of social disadvantage that do not directly identify learners from the target group"* (HEFCE 2007, 9). This document further recommends the use of area deprivation and higher education participation statistics as proxies for finding the right sorts of young people in the right sorts of schools.

Were an objective and easy-to-operationalise measure of cultural capital (and other forms of capital) available, this would indeed be invaluable to the widening participation agenda. Bennett *et al's* (2009) approach is certainly more thorough, but it is more time-consuming as a result, though this would come far closer to what policy makers and committed practitioners are seeking to achieve. In the meantime, parental occupation remains the most widely accepted and understood proxy available, albeit a flawed one.

Conclusion

Noble and Davies do present a very useful finding, but it is underplayed in the paper. They identify a link between progression to higher education and an active interest in current affairs; though whether the latter is a meaningful measure of cultural capital is challenged in the discussion above. However, that an interest in the wider world has an impact on educational aspirations is itself a useful finding for sociologists, educationalists and policy makers. There is a clear linkage to Hodkinson *et al*'s (1996) concept of 'horizons for action', with those young people with a greater awareness of the world around them and the opportunities and challenges it holds having a stronger demand for extended education at a high level. This could act as a touchstone for both further research and practical action and, as such, is a more fruitful avenue than attempting to debunk the role of parental occupations and education as predictors for higher education progression, using a flawed sample, survey tool and understanding of widening participation.

The arena of widening participation has long been a contested space in sociology and social policy (e.g. Archer 2007; Kettley 2007; Gorard *et al* 2007) and this looks set to continue given the new financial pressures on the higher education sector recently announced by Secretary of State, Lord Mandelson. Questions will continue to be asked about the value-for-money offered by Aimhigher and other widening participation activity (PAC 2009), its targeting (Harrison and Hatt 2010) and its efficacy in challenging traditional patterns of participation (Chilosi *et al* 2010). The latest official figures (HEFCE 2010) speak for a considerable improvement in higher education progression from areas with traditionally very low participation rates, while progression from lower socio-economic groups appears largely static (DBIS 2009). This provides *prima facie* support for the view (e.g.

Raphael Reed, Gates and Last 2007) that situated social capital lies at the heart of the widening participation issue rather than the limited account of cultural capital Noble and Davies advance.

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