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Professor Tony Brown's Inaugural Address
Special Section on Theorising Pedagogy

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SECOND REPLY TO NASH

MARTIN THRUPP

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INTRODUCTION

This is my second reply to Nash concerning my doctoral study of 'school mix' in Wellington schools. The first was in the *New Zealand Journal of Educational Studies* some five years ago (Thrupp, 1998). Nash's latest critique is a little less dismissive than his earlier one; for instance he now seems to accept the plausibility of many of the reported differences between the Wellington schools. For my part I think Nash makes some useful points. For example, those arguments about within-class selection add useful complexity to the debate around compositional effects (although they also raise questions about the categories in use; for example, are those in 'the skilled working class' with high and low educational resources really similar in class terms?). Yet, generally, I still find Nash's critique unconvincing. One reason for this is that Nash does not discuss the inadequacies of existing statistical studies of compositional effects. Second, and partly following from this, he is not generous enough about the contribution of the Wellington study. Third, his 'triangulation exercise' is simply unsustainable. Fourth, Nash often misrepresents the findings of the Wellington study. Fifth, Nash's policy agenda is too conservative to bring about much change towards social justice. I shall discuss these problems in turn.

THE LIMITATIONS OF STATISTICAL MODELLING

The central difficulty I have with Nash's critique is that he does not acknowledge that, to date, statistical methodologies have been quite inadequate for studying compositional effects. Reading his critique you would think studies like *Progress at School* with their "larger and more competently analysed data sets" have a reasonable grasp on compositional effects but this is far from the case. The extent of the shortcomings of existing studies can be illustrated by considering some adequacy criteria for quantitative studies of compositional effects that Hugh Lauder, Tony Robinson and I (2002) have recently developed. These are that:

1. samples should include schools from *both ends* of the socio-economic spectrum rather than from the middle as well-mixed schools are unlikely to show compositional effects;
2. a *full set of entry-level variables*, including prior achievement variables, need to be included to establish whether compositional variables are acting as proxies for other variables, especially prior achievement;
3. there should be measures that can capture the possible correlations between the *three dimensions* of the school composition model (peer group, instructional, and school organisational and management processes);
4. a *combination* of compositional variables (e.g., prior achievement mix or socio-economic composition) should be constructed and the

- relationships between them identified. These would include mean socio-economic status measures and measures of parental education;
5. where possible, a mix of school *types* would be included in samples;
 6. where possible, studies should be *longitudinal*;
 7. different techniques for *measuring* composition should be used;
 8. *robust* measures of social class or other compositional variables should be used;
 9. *additional data* should be collected to narrow down the possible interpretations of compositional effects; and
 10. in addition to the above, studies should conduct their analyses according to *multi-level modelling* techniques.

Some of these criteria are demanding, particularly no. 3, which requires statistical modelling to match the likely processes actually underlying the effect. It is, therefore, not surprising that when we went on to survey key studies from the UK, USA, New Zealand and Belgium over the last decade for the *International Journal of Educational Research* we found that none of them were ideal against these criteria and many were far from it (see Table 1). Only two studies had tried to include the processes likely to be involved (Bryk, Lee & Holland, 1993; Opdenakker & Van Damme, 2001 – see ‘Measures 3 dimensions’ column in Table 1) but in both cases the weak measures involved were only a tentative step towards what is really required.

THE RELATED CONTRIBUTION OF THE WELLINGTON STUDY

Given that Nash generally ignores the limitations of the existing quantitative literature on compositional effects, it is not surprising that he does not acknowledge the way these were the starting point of the Wellington study – to him my critique of the quantitative literature is little more than “its failure to agree with ‘qualitative’ research”. Furthermore, Nash seems to hold that since student achievement levels were not analysed, the Wellington study is incapable of making any contribution to the question of compositional effects. Nevertheless, one obvious response to the limitations of statistical studies in this area was to ask whether there was any kind of less direct evidence that could be brought to bear on the subject. Hence I embarked on a study which looked at whether there were school processes at work which could plausibly create a compositional effect. The findings have proved very useful but I have always been careful not to make unsustainable causal claims around them. Thus, it is quite incorrect to describe my observation that “it is hard to see how a school mix effect would not occur” as plaintive. On the one hand, this is as much as my study could responsibly say because of the causal issues involved but, on the other, there was no need to be apologetic since it was apparent that quantitative studies could not reliably establish the presence or otherwise of compositional effects either.

Table 1. Summary of reviewed studies

Study	Shows mix Effect	Longitudinal	Prior achievement	Measures 3 dimensions	Covers range of social spectrum	School type	Robust measur social class	Different techniques for measuring composition
Smith & Tomlinson (1989)	No	No	Yes	No	yes	no	no	no
Gray et al. (1990)	No	No	Yes	No	yes	ns ¹	no	no
Thomas & Mortimore (1994)	No	No	Yes	No	yes	yes	no	no
Sammons et al. (1997)	Yes	Yes	Yes	No	yes	yes	no	no
Robertson & Symons (1996)	Yes	No	Yes	No	yes	ns ²	no	no
Strand (1998)	No	No	Yes ³	No	yes	ns ⁴	no	no
Chubb & Moe (1990)	Yes	No	Yes	No	yes	yes	no	no
Bryk, Lee & Holland (1993)	Yes	No	Yes	Yes	yes	yes	no	no
Ho & Willms (1996)	Yes	No	No	No	yes	no	no	no
Pong (1998)	Yes	No	Yes	No	yes	yes	no	no
Zimmer & Toma (in press)	Yes	No	Yes	No	yes	yes	no	no
Harker & Nash (1996)	no (but yes in Harker 2001)	No	Yes	No	no (but yes in Harker 2001)	no (but yes in Harker 2001)	no	no
Lauder et al. (1999)	Yes	No	Yes	No	yes	no	Yes/no ⁵	no ⁶
Opdenakker & Van Damme (2001)	Yes	No	Yes	Yes	yes	yes	No	no

¹ ns= not stated.

² ns= not stated.

³ This study measure of prior achievement was shortly before the dependent variable was measured.

⁴ ns= not stated.

⁵ This study does try to capture both the family dynamics involved in SES and the work history of partners but it is still limited.

⁶ There is work in progress in this area.

It is also important to note that school processes in the Wellington study were studied *as they impacted on a set of working class students matched across the schools*. This gave the study a greater likelihood of identifying the salient processes at work in creating compositional effects than one which just considered the impact of social class on school processes in a general way. Put another way, whereas Harker (2003) notes that to demonstrate a compositional effect “students with similar background characteristics *need to be shown to be* doing much better (or worse) at other schools with similar background characteristics where the mix is more favourable (unfavourable)” (Harker, 2003, p. 11, my emphasis), what my study was able to do was to illustrate that students with similar background characteristics *were exposed to processes conducive to* doing much better (or worse) at other schools with similar background characteristics where the mix is more favourable (unfavourable).

It also hasn't been the case that the “basic theory of a school composition effects and its mechanism of generation are widely held” as Nash suggests – or at least this is not reflected in academic work. Certainly before the Wellington study there was little previous empirical evidence about the processes which might be involved. There were only a handful of ethnographic studies which could be used to compare processes across schools of different compositions – Metz (1990) was arguably the best. As Nash notes, there were some theoretical discussions from Bourdieu and some explanations from quantitative researchers of their statistical findings. But the latter were hardly substantial. For instance Willms (1992) provided about as much detail as any when he commented:

Schools with high social class or high ability intakes have some advantages associated with their context: on average they are more likely to have greater support from parents, fewer disciplinary problems, and an atmosphere conducive to learning. They are more likely to attract and retain talented and motivated teachers. Also there are peer effects that occur when bright and motivated pupils work together. (p. 41)

The Wellington study has, therefore, filled an important gap in the literature and over the last few years has proved useful to a range of researchers. Of most relevance, it is being used to help develop more sophisticated statistical models of compositional effects by providing an indication of the processes which need to be taken into account (e.g., Opdenakker & Van Damme, 2001). It is also helping researchers into peer effects to better understand the significance of the ambient peer environment (Parr & Townsend, 2002). More generally it provides a useful model of how to bring multiple data sources together to create a rich account of school processes.

NASH'S 'TRIANGULATION' OF TUI COLLEGE

Nash's use of *Progress at School* data to triangulate the findings of the Wellington study is unsustainable in several respects. First, Nash's 'Kotuku' School could not have been a very similar school, as at the time of the study there were no other schools in the same conurbation which had a similar SES and ethnic intake to Tui College. All the schools in the area had quite

different student compositions, leading them to occupy different positions in the local educational market (as discussed in Lauder et al., 1999). Second, and this is a point I have made previously in response to Nash (Thrupp, 1998), there is a world of difference between looking at students and school processes at Year 9 level (as I did) and at senior secondary level (as Nash did). The students who reach the latter are a narrower self-selecting group and school processes will reflect this. Third, and moving now to Nash's data on Tui College itself, we seem to be comparing different year groups as my study was conducted with students who were in year 9 in 1994 whereas Nash gives figures for students who were in year 9 in 1991. This is more important than it may at first seem since, as a result of market processes, Tui College became a lower SES school over these years. Thus, again, the comparison of data on student aspirations is invalid. Finally it should be noted that, in addition to interviews with the matched students and their friends, my findings in this area were drawn from responses to student questionnaires administered to two year 9 classes in each of the schools (about 220 students in total).

MISREPRESENTING THE FINDINGS OF THE STUDY

I have already noted that Nash fails to acknowledge the background of the study and hence what it was trying to achieve. He also frequently underplays the complexity of the study's methodology, scope and findings. In particular he focuses too much on the 'reference group' hypothesis at the expense of instructional and management and organisational hypotheses. Here, he rather patronisingly attributes the argument that a critical mass of alienated working class students will produce "a spectacular explosive celebration of cultural resistance". However, these are certainly not my words and the study is not a repeat of *Learning to Labour* (Willis, 1977). Rather, following Brown (1987), the study was premised on the understanding that most working class students are 'ordinary kids', those who do not overtly resist school but go along with it to working class ends.

NASH'S POLICY AGENDA

Nash takes the moral high ground when he complains about my "unprincipled acceptance and reproduction of the common sense realism that underpins the decision making of parents". In part this stance is research-based – his belief that my rejection of statistical studies is out of order and my study invalid as evidence for compositional effects. It is also clearly more overtly political, in that he sees my arguments as damaging because they undermine working class schools by confirming the view that higher SES schools are advantageous for students.

The research side has already been dealt with inasmuch as I have revisited the rationale for the Wellington study and its contribution. Nash may still disagree but it's hardly a question of principle. Indeed as Dick Harker, Nash's long-term collaborator, has recently written:

Clearly the compositional effect is of considerable importance for policy and yet it is just one of those issues on which honest researchers find it difficult to agree. The issues are so complex and

the definitive findings of randomised controlled trials are notable by their absence. (Harker & Tymms, 2002, p. 20)

What might be unacceptable is not being open to investigating the matter further but I do accept that the findings of the Wellington study need replicating in other settings and with larger samples. I also accept that (improved) statistical analysis has a role to play in finding more definitive answers in this area. At present I'm actively pursuing both of these directions by leading an ESRC bid for a quantitative and qualitative study of the impact of school composition across all primary schools in Hampshire, England (Harvey Goldstein, Hugh Lauder and Tony Robinson are co-applicants). This will test the findings of the Wellington study in a different national context and school sector (primary) and against a background of newly-sophisticated statistical analysis.

Nash's other reason for branding my research 'unprincipled' seems to be the concern that it supports the 'commonsense' view of the public that higher SES schools are superior, thus undermining working class schools even further. But here it is important to emphasise that I am not arguing that compositional effects are acceptable but seeking to challenge the inequitable conditions which give rise to such effects. Most middle class parents can and do already send their children to relatively high SES schools, either state or private. Yet while Rena was 'upfront' about what she saw as the importance of school composition, the class wisdom which creates a preference for high SES schooling is more often left unspoken. For instance, research on school choice shows us that most middle parents refer to other reasons for selecting schools – location, their child's wishes, academic quality and so on. Thus as families pursue positional advantage and social mobility or reproduction through higher SES schooling there may be more often an ideological avoidance of school compositional issues than recognition of them.

Whether or not school composition is overtly acknowledged as a factor in school choice, we do need more acknowledgement amongst the middle classes that school choice is not value-free: that enrolling one's own child in a high SES school has implications for the schooling and subsequent life-chances experienced by children who attend the low SES schools which this action creates. This is because sustainable interventions to reduce segregation will only become politically feasible through public debate which accepts that parents want to advantage their children but weighs this against the costs of a schooling system which is highly segregated. In contrast, Nash's policy agenda is less concerned with addressing segregation but involves providing working class schools with additional resources and making sure they all have the characteristics of 'good schools' so that they serve their students well. I agree with both of these proposals. However, they won't have much impact on social justice because they leave segregated schooling untouched and unquestioned.

FINALLY...

Looking back on what I have written here, I see that some of it has ended up reiterating points made in my first response to Nash (Thrupp, 1998). That reply also covered a number of other points Nash raises such as my use of the terms 'quantitative' and 'qualitative'. There is a growing international

consensus around the need to revisit compositional effects with better studies and I'm pleased that the Wellington study is at least partly responsible for this development. We shall see what results in the next generation of studies. In the meantime I'm thankful to Nash for helping to keep the Wellington study 'alive' in New Zealand with his various critiques.

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