

NEW DIRECTIONS FOR SCHOOL EFFECTIVENESS RESEARCH: TOWARDS SCHOOL EFFECTIVENESS WITHOUT SCHOOLS

ABSTRACT. School effectiveness research has made a number of valuable contributions to educational research over the past three decades. However, its validity is threatened by a number of evolutions that question the continuing centrality of its basic research object, the public school. Moves towards more flexible school organization such as networks of schools, a broader role for schools reconceptualized as community centers, the emergence of new providers outwith the public sector, the increasingly internationalised nature of research and moves towards greater use of distance learning and home schooling all mean that this focus may rapidly become outdated, potentially making school effectiveness research irrelevant.

In this paper we will discuss the consequences of these evolutions for school effectiveness research and argue that, rather than lessening the need for effectiveness research, they increase the imperative for this type of research, as long as it is broadened to educational effectiveness in its broadest sense whether it takes place in the traditional public school or not, and is conducted in an empirical and open-minded way.

KEY WORDS: education policy, educational change, educational research, school effectiveness

1. THE ORIGINS AND IMPACT OF SCHOOL EFFECTIVENESS RESEARCH

School effectiveness research started of in the early 1970's as a radical movement to go beyond prevailing pessimism about the impact of schools and education on students' educational performance (e.g. Jencks et al., 1972) and move towards studying those within-school factors that may lead to better performance for students, regardless of their social background (Reynolds et al., 1994). This was a key move, resulting in methodological advances (such as the advent of multilevel modeling to study hierarchical systems), the identification of a reasonably consistent set of organizational characteristics of schools that contribute to enhanced educational outcomes (Teddlie & Reynolds, 2000), and informing a significant proportion of school improvement work as well (Reynolds, Sammons, Stoll, Barber & Hillman, 1996).

This enterprise has been highly successful on a number of levels. Firstly, the prevailing pessimism of the 1960's has well and truly been swept away, with the view that schools can make a difference even to students in socio-economically disadvantaged circumstances being almost universally accepted (Mortimore, 1991; Thrupp, 2001), even to the extent that in many cases school effectiveness researchers have taken to issuing warnings as to the real extent of influence that schools can have (which, let us not forget, is usually only around 10–20% of variance) (e.g. Mortimore & Whitty, 1997).

Key factors identified in school effectiveness research have influenced theory and practice of school reform both at the policy and school level, even if this influence is not always acknowledged or understood as such. An example of this is the emphasis on such factors as academic leadership, which in recent decades has come to be seen as central to efforts to improve schools and education systems, as attested by the increasing number of leadership development initiatives in education (such as the National College for School Leadership in the UK) and the concomitant increase in research interest in "leadership for school improvement" (Bennett, Wise, Woods & Harvey, 2003). This focus does represent progress compared to a situation in previous decades where research on school leadership often took the form of descriptive studies of what managers did, or normative prescriptions on what they should be doing rather than focus on relationships to organizational effectiveness (Muijs & Harris, 2003). Academic leadership was rarely mentioned in official or research discourse before school effectiveness research found this to be a crucial factor in improving and effective schools from the 1970's onwards, and even though it would be wrong to attribute the increasing emphasis on leadership entirely to school effectiveness in view of the influence of research and theory on private sector leadership in this evolution as well as the structural changes in education that have led to greater autonomy and responsibility for school managers, it is undoubtedly the case that school effectiveness and school improvement research have played a key role in this development (Reynolds, 2001). Other examples also exist. Research findings on the importance of shared vision and culture and concentration on a limited number of goals have undoubtedly contributed to the growth in activity in terms of developing school visions and missions over the past decades, alongside, once again, related findings from the private sector (Muijs, Harris, Chapman, Stoll & Russ, 2004).

Of course, school effectiveness research also suffers from a number of well-rehearsed weaknesses, such as undertheorization, susceptibility to misappropriation of findings for political purposes, the creation of oversimplified lists of factors, insufficient empirical research on certain findings and a somewhat myopic focus on test results as the measure of outcomes (Slee & Weiner, 1998; Thrupp, 2001; Teddlie & Reynolds, 2000). These weaknesses have been widely discussed in different fora (see, amongst others, the special issue of *School Effectiveness and School Improvement*, vol. 12, no 1 on the debate on the validity of school effectiveness research), so I will not rehearse them again here. An issue I do wish to address, however, is one that I believe will be becoming ever more important in future. This weakness is central to the origins of school effectiveness research, but may conversely provide great opportunities for school effectiveness researchers in the future. As mentioned above school effectiveness research originated in part to help show that schools can make a difference to the educational performance of students. This has meant that researchers in this area have concerned themselves primarily with schools as organizations. Moreover, because school effectiveness research originated in a context (US, and UK in the 1970's) in which the traditional public school was the only form of provision for students from disadvantaged backgrounds (always a prime target of interest for school effectiveness researchers), the overwhelming focus of interest has been this type of school. In previous decades this focus was defensible, as the traditional public school was, and largely still is, the very heart of the education system. However, I would argue that this will become increasingly problematic for a number of reasons discussed below.

2. THE TRADITIONAL PUBLIC SCHOOL, THE MODEL OF THE FUTURE?

The traditional model of the school, which is the one school research has studied, is one of an organization which is publicly funded and can be described as a concrete physical and organizational structure within which all of formal education occurs. For children from non-wealthy areas in the Anglo-Saxon countries, which have, in contrast to what is often believed, come late in terms of providing school choice compared to other nations such as Belgium and Sweden, it has traditionally been the monopoly provider of formal education (Wolf,

Macedo, Ferrero & Venegoni, 2004). This of course provides researchers with a convenient arena for research, thanks to the self-contained nature and strong focus of schools as organizations. However, the question must be asked to what extent to this model will in future retain this unique position in the educational landscape. A number of evolutionary changes ongoing at present suggest that this might not be the case. As well as potentially altering the education system in more or less radical ways, they will require a fundamental rethink for school effectiveness research.

2.1. Flexible organization

A first evolution in many educational systems is towards a partial dissolution of the traditional single school model towards more flexible modes of organizational link-up and organization. This is taking the form of both the dissolution of large schools into smaller entities (the so-called small schools movement) and of increased linkages between schools.

The small schools movement is based on the premise that smaller units allow for the development of a stronger community culture and shared values, as well as closer and more personal connections between staff and students, which are said to lead to enhanced student outcomes in both the cognitive and affective domains (Lee & Smith, 1995; Raywid, 2001). This has led to calls and actions to dissolve the large schools that had grown up in many countries, for example by creating subunits within existing schools (such as the 'house system' employed in many UK and US schools), or even through the actual dissolution of large schools into smaller units. In some cases these exist in the same shared site as the old school, but in some cases entirely new small schools have been set up. There is some support for a link between smaller school size and higher academic performance of students (Lee & Smith, 1997; Raywid, 2001), though questions remain with regards to the cost effectiveness of the strategy, the relative importance of school size compared to other school effectiveness characteristics, and possible intake effects in many studies, and one recent systematic review of the evidence found ambiguous results for secondary schools (Garrett et al., 2004). What is clear is that in some of its forms this evolution, if sustained, presents some challenge to the school as traditional unit of analysis in school effectiveness research. The more schools are dissolved into smaller organizations on one site, the more the question of the

relative effectiveness of these smaller units with respect to one another will arise, which is likely to raise some interesting questions regarding organizational culture and composition effects. Previous research on departmental effectiveness suggests both that departments within a school are differentially effective, and that the effect of the department is greater than that of the school (FitzGibbon, 1992; Sammons, Thomas & Mortimore, 1997), which leads us to hypothesize that the same will be true of other subdivisions within schools.

The second important change to the organization of schools as autonomous but unified single units takes the form of an increase in networking between schools. This movement is premised on the principles of improving capacity by sharing best practice and increasing levels of creativity through confrontation with other practices and views. Creating “communities of practice” in which practitioners work together for school improvement is therefore a key aim of these forms of networking (Day & Hadfield, 2004). Networks themselves are taking on different forms and levels of complexity. In their simplest form they consist of groups of, usually geographically proximal, schools coming together for specific school improvement purposes, often under the auspices or encouragement of the Local Education Authority. A step up from this are more permanent consortium arrangements, where schools work together across a range of issues. Arrangements in which schools link up that are geographically non-proximal are increasingly possible in the light of evolutions in communication technology, and it is likely that groupings of schools that have shared philosophies or pedagogical approaches may increasingly work closely together in this way, as is already happening in a number of instances (e.g. the Basic Schools Network), albeit more often under the auspices of an external body than through school-initiated action. An interesting evolution and a further step is the formation of so-called Federations of schools in the UK. In their so-called “hard” form, schools set up joint governing bodies, management structures or principalships (Department for Education and Skills, 2005). In some cases schools offering different forms of provision (such as mainstream and special schools) are forming a Federation to offer a more integrated approach, in others we are seeing successful schools joining up with (or some would say, taking over) failing schools in order to improve provision therein.

Again, for researchers in school effectiveness the key questions of both the unit of research (is this the individual school or a network or

Federation?) and the effectiveness of the different models arises, as well as the question of how one can make these new arrangements work to the benefit of students. Overall effectiveness of this way of working is likewise in need of further research, as there is currently still a surfeit of advocacy over empiricism in this area. An important question here is the relative effectiveness of these different models – are these alternatives actually more, equally or possibly even less effective than traditional models of schooling? This is another aspect that requires research attention with some urgency as policy-makers and practitioners need to be able to distinguish advocacy from evidence.

2.2. *Extended schools*

A further change that is taking place in many countries is one that is redefining the role of schools. Traditionally, schools have been largely single purpose institutions, devoted to educating children of a particular age and stage of learning. Depending on the educational culture of the particular country additional pastoral goals may be more or less developed and important, and in different contexts factors such as well-being, attitudes to learning and self-esteem have been deemed important outcomes of education (e.g. Van Landeghem, Van Damme, Opdenakker, De Frairie & Onghena, 2002). However, essentially schools have been occupied with the cognitive and to a lesser extent social development of youngsters up to age 18. Recently, though, an increased emphasis on schools as centers for their communities has started to emerge, both in the rhetoric of education reformers and in practical policy initiatives in a number of countries such as the UK and parts of the US. This is seen as especially beneficial where schools are serving disadvantaged areas (Department for Education and Skills, 2003).

Two key premises underlie this movement: the potential power of schools as organizations to reach out to their community, and the importance of stronger linkages with the community to improving parental involvement and, as a result of this, student performance (Hiatt-Michael, 2003). The vehicle through which this will happen is increasingly seen to be the creation of “extended schools,” offering child care, social services, adult education and other forms of provision to the community. However, while the vehicle is the same, the two underlying premises are based on essentially contrasting views of the role and esteem of schools in the community. The “outreach”

proponents assume that schools are well-embedded organizations, which will enhance attitudes of community members to other services held within them. The “improving parental involvement” view supposes that parents may have negative views or experiences of school, and that by getting them to interact with schools in a new context these problems may be overcome. This latter view has received some support from research evidence in school improvement, where schools in disadvantaged communities have been found to be quite successful in increasing parental involvement through adult education and service provision initiatives (Maden & Hillman, 1993; Muijs et al., 2004). Whether incorporating social services (as opposed to say, adult education classes) into schools is likely to have this effect is a moot point, however, as these services are not always themselves popular with recipients, and can be seen as alienating in their own right. However, this movement has received considerable support in some ‘futures scenarios’ (e.g. Organization for Economic Cooperation and Development, 2001) and therefore more experiments in this area are likely.

This movement raises a few key questions for school effectiveness research: is the concentration on educational outcomes as the main or, in most studies only goal of schooling still suitable in view of the many goals that integrated schools may have? If that *is* still the case, the question will have to be asked as to whether and to what extent extending schools in this way leads to a dissipation of energies possibly impacting on the “academic focus” found to be important in school effectiveness studies (Teddle & Reynolds, 2000)? Other key questions include: do extended schools require different forms of management, different cultures or structures, and how does this impact on staff relationships, workload and professional development needs?

2.3. Different providers

As mentioned above, school effectiveness research originated in a context, in both the US and UK, where for the vast majority of students the state school was the sole monopoly provider of education. The only exceptions to this have traditionally been faith-based schools, for example the large Catholic education sector in many countries, but these too have operated largely within the constraints of the state system, and have in most countries been funded through state subsidy mechanisms in similar ways to non-faith schools (Wolf,

Macedo, Ferrero & Venegoni, 2004). This picture is changing, however, and in some cases quite rapidly. In the UK, the new Academies program is designed to introduce private sector involvement in state schooling, through combined private-public funding (although the extra government and private sector funding for these schools compared to other state schools does not seem to have led to the expected improvements in performance (Price Waterhouse Coopers, 2005). Furthermore, while private schooling has traditionally aimed at a very wealthy market, with fees far out of reach of ordinary parents, tentative moves towards for-profit schools aimed at a less affluent market are afoot, such as evidenced by the Edison project in the US and the GEMS and Cognita projects in the UK.

Supporters of more involvement of the private sector in schooling claim that this will improve school effectiveness for three main reasons:

- Increased competition between providers is likely to compel schools to improve the effectiveness of their practice, or go out of business, as in the private sector;
- The introduction of private sector management practices is supposed to lead to decreased inefficiencies and better forms of incentivization than is common, according to privatization proponents, in the public sector;
- The introduction of a profit motive may lead to higher investment and better incentivization of staff than is currently present in the system (Tooley, 2002a).

Opponents doubt these advantages, questioning the extent of inefficiency currently in the system, and pointing to the likelihood of inequalities arising from differential access to information and the system depending on the social class of parents (Hatcher, 2001). Others point to the lack of impact of structural changes on effectiveness more generally (Barber, 1997).

For school effectiveness researchers this increased involvement of private sector providers has a number of consequences. Firstly, it is necessary for us to go beyond an exclusive concentration on state schools, and attempt to study the effectiveness of these new providers. This will inevitably be challenging where commercial interests are at stake. However, this is imperative as what research now exists, is marred by the partisan nature of most of the writing in this area (Hatcher 2001; Tooley, 2002a are good examples of this).

Outside the Western context, that has so dominated educational research, there is the interesting phenomenon of private school provision in some the poorest areas of a number of countries (as diverse as India, China and Nigeria). These are profit-making organizations, that cater for those families that have limited means to pay tuition fees, while often providing bursaries for those who cannot afford these. While state and non-governmental organizations have often criticized these schools, some evidence is starting to emerge from work by Tooley (2002b) that these schools may in fact be more effective than most State schools in these countries, and certainly that they are often chosen by parents over state schools.

2.4. Internationalization and multiculturalism

Another challenge for school effectiveness research and practice, that has actually existed for quite some time without being fully addressed, is the need to deal with issues of internationalization and increasingly multicultural classrooms and schools. These are related but distinct issues.

Internationalization is a necessity for all educational research, the vast majority of which has taken place in a limited number of Western countries. This is therefore not an issue that is unique to school effectiveness research, but is one that is especially important to it, as we wish to influence and inform practice, and improve schooling across countries. Dissemination of school effectiveness research internationally is quite well advanced, through amongst others the work of the World Bank and OECD (e.g. Heneveld & Craig, 1996; Scheerens, 1999), and clearly there is a strong interest in the principles and philosophy of school effectiveness research worldwide. This does, however, lead us to the question of the extent to which our findings translate well to non-Western contexts. Our experience within these contexts would certainly suggest that this will only partially be the case (Scheerens, 1999). We know that, while many school effectiveness findings do appear to hold across countries, there are clear differences even between relatively similar Western contexts. The size of the school effect itself varies, from almost 50% in some countries, such as Flanders, to less than 10% in others, such as Singapore (Teddlie & Reynolds, 2000). There is therefore a need for more research in developing contexts, and not least for the development of research capacity in these countries.

Related to this is the need to look more broadly at system-level effects. To what extent do differences in the effectiveness of schools depend on differences in the effectiveness of education systems at the local and national levels? This is a key question for policymakers at the national level, but one that has not properly been addressed in research. International comparative studies such as TIMSS and PISA have compared performance of different countries in science, math and English and have looked at comparisons at the systemic level (see e.g. Haahr, Nielsen, Hansen & Jackobsson, 2005), but typically conclude that the system level has limited impact compared to the within-country variation. While this is true at a statistical level, and is predicted by proximity models that find that those factors closest to the student (student personal characteristics and classroom practice) most strongly affect outcomes while those that are not directly experienced by students affect them less, it is nevertheless clear both that systemic reform can affect macro-level effectiveness and that the impact of systems may be greater than that found in simple statistical models. For instance, educational reforms in Poland and Latvia have been linked to marked improvements in PISA scores (Haahr et al., 2005), while the Numeracy strategy in England has been credited with a large improvement in math scores on international tests by English students (Sammons, 2006), though causality is always hard to determine in this kind of study. The second point, that the impact of systems may be greater than the measured impact is due to the reliance of most research in this area on relatively simple multilevel modeling designs. In essence, these designs only model direct effects of predictors on outcome variables, resulting in the well-known findings that proximal variables have a (relatively) strong impact, while variables that are distant from the student tend to have no or a very weak effect on student achievement (Muijs & Reynolds, 2000). This may, however, be an underestimate of these distal variables that is down to not measuring the indirect impact they have on outcomes through their impact on more proximal variables. That this statistical modeling practice leads to underestimation of the impact of distal variables is shown by De Maeyer and Rymenans (2006) for the impact of school leadership on student achievement. In the same way that leadership creates the conditions under which teachers can be optimally effective in schools, it is likely that government policies can create conditions under which schools and teachers can be more or less effective, and it is time for research that addresses this in a more detailed fashion than the international studies have been able to do.

The increasingly multicultural nature of schools is another issue that urgently deserves more attention from school effectiveness research. When school effectiveness research started, this was often in still relatively homogeneous contexts, in which schools experienced a strong dominance by one ethnic or social class group, be it the majority group in the country or a minority group concentrated in a particular (usually urban) area served by the school (though obviously this did not necessarily mean that this dominant culture was in tune with the largely middle class values of teachers, see e.g. Willis, 1977). Similarly, school staffing was predominantly confined to members of one community. However, both conditions are rapidly changing. In the large cities, such as London, and even in more provincial settings, the demographic and social trends are now creating very different contexts, where monocultural assumptions may not apply (Lindsay & Muijs, 2004). This obviously has heightened the need to take account of this diversity in school organization, teaching and learning (Lumby et al., 2005) and poses some awkward questions for school effectiveness researchers, who have traditionally attempted to find universal recipes and generalize these across contexts. As Lumby and Muijs (2004) point out, this may be problematic in multicultural contexts, where values such as a shared culture, found to be so important to effectiveness and improvement, may conflict with the need to value and support diversity, both among students and staff. This challenge is therefore another one that needs to be addressed with some urgency.

Clearly, this makes the issue of curriculum ever more important in school. Disaffection with the curriculum has a long history in education, especially among working class students (e.g. Alhassan, 1990; Willis, 1977,). However, mismatch between curriculum and students is likely to have increased since then, both due to the greater diversity of students and the increased imposition of central curricula in many countries and States of the US (Carnell, 2004; Raffo, 2003). The issue of curriculum has traditionally been neglected in school effectiveness research, largely due to a traditional reluctance to discuss values (Reynolds & Teddlie, 2000). It should be clear that this is no longer a tenable position for effectiveness researchers. If we are looking at ways of reaching educational goals, the curriculum forms and constrains what is possible, has a strong impact on student motivation and should therefore become far more central in effectiveness research than it has been to date.

2.5. *Distributed schooling?*

So far, the challenges we have discussed are relatively limited in the sense that they leave the basic organizational foundation of school effectiveness untouched. The school, whatever form it may take, is the basic arena for education. Some other evolutions are pointing in a different direction, however. Will, in future, education still predominantly take place in detached physical settings? Or will the school become a networked entity, where education may in part take place online, in part in a variety of buildings specialized to accommodate different subjects, and in part at home? It is, at present, obviously not possible to accurately predict the future direction of schooling, though moves towards personalization of learning and adaptive learning suggest that more diffuse forms of schooling are likely in future, as, if schools are to tailor their offerings to the needs of individual schooling (Leadbeater, 2004) it is unlikely that they will be able to provide all student needs within their own school building and with their own school staff. What is clear, though, is that the technical possibilities for distributed schooling are rapidly increasing. Initiatives are already underway that are leading to at least part of pupils' education taking part in more than one institution or setting. There is also some evidence that dissatisfaction with the school system is leading to an increased search for alternatives to school based education.

The home schooling movement has grown rapidly in countries where the legal framework allows this, such as the US and the UK (Stevens, 2001). The extent to which this challenges the school system is an interesting one. On the one hand, this increase leads to questions over the perceived effectiveness of schooling, and the extent to which it is catering for the diverse needs of different constituencies. On the other hand, it is hard to see this ever becoming more than a minority pursuit, in the light of the demands made on families. It is hard to imagine home schooling being sustainable in two parent families where both parents work, or in single parent families where the need to provide is pressing. As, in most cases, it is the mother who provides the bulk of schooling, this could also be seen as a potentially very unwelcome setback to the cause of gender equality in society (Stevens, 2001). Strong claims have been made as to the effectiveness of home schooling compared to regular school experience by both proponents and opponents. Proponents point to evidence that home schooled students are as likely to perform well in state examinations and to continue on to further education as regularly schooled

children (Meehan & Stephenson, 1994; Rudner, 1999). As effectiveness researchers, however, we would have to point to the overwhelmingly middle class composition of this cohort as one explanation for this finding (Rudner, 1999; Stevens, 2001). Opponents claim home schooled students are missing out on key social development, but again evidence for this is limited (Meehan & Stephenson, 1994). While the discussion on home schooling may seem somewhat divorced from mainstream concerns, not least due to the rarefied nature of the participants, it gains more importance when looked at as one of the avenues that future forms of schooling may take (OECD, 2001). It would certainly appear imperative that effectiveness research informs and becomes part of futures thinking in education, and an overly conservative concentration on the traditional school may hinder this.

Increasingly, initiatives are ongoing that are leading to at least parts of students' education no longer taking place in their schools. Some schools in the UK are collaborating in the provision of A-level courses (post-compulsory qualification necessary for university entrance, usually completed over two years following successful completion of the GCSE exams at the end of compulsory schooling at age 16). In these cases, students will follow some courses in the 6th form center of their own school, and some in the 6th form centers of neighboring schools. In some schools in the US, students are simultaneously completing college and high school, by enrolling students in college courses which act as their high school courses. In these cases, and again this is an increasing trend, the question that arises for school effectiveness researchers is that of attribution of effects. It would no longer be accurate to attribute these to the school. However, a simple 'carve up' of subjects between institutions would ignore the interactions that occur in these hybrid models. For example, if university courses are taught in school, to what extent is this a school or university program? Likewise, when an A-level course is taught in a school other than the one the student attends it would be methodologically incorrect to attribute achievement in that subject to this other college, as the culture and learning in the students' original school will affect achievement in this other subject. Complex, cross-classified structural analysis is needed to look at this type of relationship.

Similarly, there is a need to get to grips better with the emerging issues of distance and online learning. While still much more limited than some overenthusiastic proponents predicted several years ago

(e.g. Clark, 1989), this is undoubtedly a growing area in education as a result of the evolution of information technology which has allowed the development of cheaper and more flexible modes of delivery. Furthermore, the move towards personalized forms of learning may well result in greater use of distance learning in order to be able to provide the student with a personal learning package. Discussion of this has so far, however, mainly taken place in the Educational Technology community, with little contact with the world of effectiveness research, and there is therefore a severe limit of research studying the relative effectiveness of different modes of delivery, with what research exists on effectiveness being mainly concerned with higher education. Again, while in itself in view of the still limited impact of this type of education at the school age level, this lack of interest from effectiveness researchers is problematic in the light of steering an informed debate on the futures of education.

At a broader level, if the education system does move towards a more diffuse model, and there are some moves in all of the above to suggest that this is certainly one likely educational futures scenario, the whole basis of 'school effectiveness' is called into question. What, in these circumstances, would be a 'school'? What is the entity we would be researching? Once again the question arises as to whether the school will remain the proper unit of analysis and research in future in terms of gaining the best understanding of how we can maximize the effectiveness of learning for our students.

3. IMPLICATIONS FOR SCHOOL EFFECTIVENESS RESEARCH

School effectiveness research has strongly enriched our understanding of schools and education, and in many cases led to genuine improvements in the way schools are run and organized. However, as mentioned above, the focus of research and development has been very much on the state run public school charged with providing education for all. As we have seen above, evolutions in education policy and practice are eroding the centrality of this type of school, even though at present it remains central in most education systems. This poses a serious challenge to school effectiveness research: if these trends continue, as is likely in the case of at least some of them, school effectiveness may find itself becoming increasingly sidelined. So does this mean that we need to gracefully retire, leaving the field of educational research to others?

On the contrary. These changes require the attention of school effectiveness researchers more than ever. However, a reconceptualization of the field will be needed.

What is needed is both a revisit of the basic premises of effectiveness research and a broadening of interest. The basis we need to revisit lies in a focus on looking at “what works,” using an input-process-output research model, which is basically value free in terms of its willingness to take an empirical stance towards a range of possible educational outcomes and the means to achieve them (Scheerens & Bosker, 1997). This methodology and epistemology is the major strength of the effectiveness approach, and this approach becomes even more important to educational policy and practice in times of change, during which typically many solutions are touted as the way forward or equally strongly opposed by actors influenced by ideology or even self-interest rather than empirical fact. This is not in itself a bad thing, as innovation and change will often originate among precisely these “true believers,” and innovations tend to have little chance of success if they are not espoused strongly and passionately (Kotter, 2002). However, there is an obvious danger here of allowing education policy and practice to be determined on the basis of enthusiasms which have not been empirically tested. Especially where new models are being created, there is an overriding need to test the effectiveness (and cost effectiveness) of these models in delivering the goals they are claiming. Furthermore, there is a need to compare the effectiveness of these models to alternatives, and to ascertain that they do not lead to a surfeit of negative unintended consequences, such as extended schooling leading to a loss of focus on teaching and learning, or private schooling increasing inequity, for example. Where new models are created, we also need to find out what the optimal circumstances, policies, cultures and structures are in which they can be effective. Does an extended school require a different style of leadership? Does online learning require a specific pedagogy? Effectiveness research can provide this crucial perspective, and therein lies its importance whichever way the education system evolves (for evolve it surely will) over the next decades.

Obviously, in reaffirming the importance of the effectiveness perspective, we do need to reorient ourselves to some extent. An exclusive focus on the traditional school as an organization may no longer be tenable. We need to look at the effectiveness of education, rather than schools, and education needs to be defined in the broadest possible terms.

There is also clearly a need for more evaluative research on the effectiveness of the different initiatives that are occurring at present – are small schools better? Do Federations benefit performance? Effectiveness research, with its more hard-edged approach to outcomes than a lot of extant evaluation, could provide a strong steer.

International involvement needs to be strengthened. We need to develop our international networks, and where possible, work with research institutions in the developing world to build research capacity where it is currently weak. This will benefit the international knowledge base of school effectiveness, and may make a valuable contribution to strengthening the potential of education as a tool for development, especially in those countries where basic levels of education are being reached, and the next steps, towards more effective education, need to be taken.

Finally, it is of key importance that effectiveness research does not fall into the trap of so much educational research, in allowing ideology and rhetoric to come before empirical research and findings. An open-minded focus on empirical results is necessary, even where they may conflict with previously held views and challenge previously held findings, such as may be the case where diversity in schools challenges views on culture that are strongly held and supported in the school effectiveness community. School effectiveness research is challenged by diversity, but again the need for effectiveness research is strengthened rather than weakened by this challenge. What this means is that we may need to explore different models of effectiveness, and different outcomes that take more account of diversity, but the need to ascertain what does make for effective education in these contexts is as strong as ever. Are there models where diversity and shared cultures do not conflict, and what do they look like? How do we create educational environments that effectively address the needs of students and parents with very different values, religious or otherwise? These factors lead us to question some current findings, but they certainly do not lessen the need for effectiveness research.

An open-minded approach is needed in particular where innovations or changes are highly controversial. In these cases, such as that of the involvement of the private sector in schooling, the force of argument and rhetoric is in danger of overwhelming evidential scholarship more than ever, and it is imperative that the various arguments are tested empirically within an effectiveness framework,

even if this may challenge our own beliefs and values. At the end of the day, our approach should be to determine what leads to the best outcomes for students, regardless of whether this fits into any prior belief, or fits into the interests of governments, business, teachers, administrators, religious groups or unions. Similarly, an affection for the schools we have grown up with, researched and seen achieve must not blind us to the possible effectiveness of other models. This does not mean that we can do research free from presuppositions or ideology. What we see as “best outcomes” is determined by our personal and institutional ideologies, as well as by the values of the societies in which we work and live, and in that sense effectiveness can never be uncontested. This debate over goals will obviously affect the debate over means, as what may be the most effective way of reaching one goal (such as high achievement) may not be the most effective way of reaching another goal (such as student well-being). However, what distinguishes an effectiveness approach is a focus on the best ways to achieve goals, rather than discussion of the goals itself, which have in the past been taken somewhat for granted in effectiveness research. This is unlikely to be a tenable position for effectiveness researchers in the future, however, as the increasing scope of education in terms of expected outcomes, as well as the increasingly multicultural nature of societies and the foregrounding of educational matters in political debates will mean that goal discussions will have to form a larger part of effectiveness research than has heretofore been the case.

Overall then, changes are afoot that challenge traditional school effectiveness research. Rather than weakening the need for us as school effectiveness researchers, I have argued that these, by contrast, strengthen that need. However, to make this happen we need to conceptualize ourselves as researchers into “educational effectiveness” in its broadest sense, looking at what works in an empirical way, using, as we always have, a variety of research methods both quantitative and qualitative, but always with the bottom line of improving education for all in mind.

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