Cultures of Excellence in Educational Research

I would like to say that I am delighted to be here today. I felt really honoured when the organising committee asked me if I would be happy to cross the great divide from Western Australia and share some ideas with you all. This campus means a lot to me. As a young boy growing up in Waterford – and I don’t mean the suburb in Perth - we not only heard a lot about it, but were also treated to photographs of it when they appeared in the Christian Brothers Annual Educational Record. Accordingly, when I visited here for the first time it all felt quite familiar to me.

Nevertheless, it has been a long time since I spoke to a group of people at this campus. That was about 10 years ago and the group was way smaller than this one. However, I have a very nice memory of that occasion. Even though Patrick Duignan was present, I was a bit concerned that my accent would present some problems. Therefore, I opened my address by asking everybody to be patient with me and to tune in, and stating that after about 10mns they would probably get used to me and understand what I was saying. To my pleasant surprise, one member of the group quickly interjected and told me to relax, saying that I had nothing to worry about. ‘Tis all right’ she said, ‘sure you sound just like every parish priest I ever had’.

And so – to cultures of excellence in educational research.

I want to start by saying that while I will put before you a range of ideas today, I do not want you to assume I am doing so out of a conviction that I have the answers. I do have views. They are my views. I think they may be important, but while I might present them in a manner which might indicate certainty on my part, they are offered to you only for cogitation, reaction and possibly even rejection.
By way of further introduction, let me say that while the topic is entitled ‘cultures of excellence in educational research’ I am not going to subject you to yet again another analysis of culture. What a contested term that is !!! I am not as paranoid as Goebbels was about it, who said: “every time I hear that word culture I reach for my gun”. Let’s just say that I wish to address some matters related to perspectives on research and ways of acting in the light of those perspective, which I believe could help you in improving, achieving and maintaining excellence in educational research.

In this regard, then, there are really four things I wish to talk to you about which I think are related to each other logically and which may be of help to you in pursuing excellence in educational research. Certainly, I believe they have been of great help to me in keeping my thinking grounded and focused, especially when I have felt overwhelmed - asking myself ‘What in the name of God am I doing?” and desperately looking for a place, or places, to come home to – psychologically I mean – for guidance. The four things I wish to talk about can be stated as follows:

**One needs to:**

- **Have a clear idea as to the nature of Educational Studies**
- **Adopt a clear and realistic approach to the research process**
- **Be brave and contest educational ideas held out as almost ‘revealed knowledge’**
- **Be brave when it comes to disseminating the results of your research**
SO, TO THE FIRST MATTER

THE NEED TO HAVE A CLEAR IDEA AS TO THE NATURE OF EDUCATIONAL STUDIES

This may be unfair of me to say, but I strongly suspect that a great many faculties and schools of education operate in a disjointed manner. Certainly they address market needs, such as the need for newly-prepared teachers, the need to provide professional development for already-practicing teachers, the need to respond to problems of literacy and numeracy, and the need to provide research preparation in the form of research masters and doctorate degrees. However, I suggest that while it is important to be alert to, and to respond to, market forces, if that is the central notion binding together all of the people involved in educational studies in a school or faculty then excellence in research is likely to suffer.

Let me put the issue like this. The great majority of you here are involved in educational studies either as lecturers, or students. You are all doing your own thing in terms of your teaching and research and, in most cases, doing it well. But what view of Education Studies unifies you all. Now in asking this question I am NOT asking what your view of the nature and purpose of education itself should be. Rather – I repeat – I am asking what view of Education Studies, as a discipline of the university so to speak, binds you together? Until we are clear about what this is, or more importantly, what it could, or should be, we are in danger of occupying a highly fractured field and thus not in a position to set out our wares amongst politicians, academics from other faculties, and the general public, in a manner that shows that Educational Studies has an autonomous existence, and a highly valuable one at that, and thus merits research funding and produces research outcomes that should be taken seriously.
The fact that there is disjointedness in many quarters should not surprise us, of course, since the study of Education as we know it today extends back only about one hundred years. When, by the end of the nineteenth century, the training of teachers began to move away from the pupil-teacher system in favour of the teacher-training college model, the main emphasis remained on practical work, with school management and methods of teaching basic subjects dominating the syllabus. Slowly, however, student teachers also began to be exposed in their courses to some history of education and educational psychology. Both subjects were offered to place teaching on a more professional footing, history of education being aimed at locating teaching within a great tradition and psychology of education being aimed at giving it a scientific basis. Little by little various other theoretical strands were added to courses, including the study of the progressive education movement, child development, and the antecedents of what we now term philosophy of education and sociology of education, while all of the time a practical focus was also maintained. In a few odd quarters, especially in a small number of US universities, specialist research centres on educational studies also existed, but they were few and far between and had nothing to do with teacher preparation, which dominated the work of the great majority. Also, one or two universities, including in Australia, existed where Education (usually in the form of such specialisms as comparative education, child study, and so on) could be studied as ‘interesting in its own right’, again totally divorced from any notion of practice, and usually in an Arts Faculty.

By the late 1960s and early 1970s, however, the syllabus in Education Studies, especially in terms of what went on in regard to teacher preparation, was such a smorgasbord (as some
might argue it is, once again, today) that educationalists were forced to address themselves seriously to asking what is the nature of education studies? I am not here to give you a history lecture. Also, for those of you my age and older who are familiar with what I am going to say, please be patient, especially as I think it needs restating today, if only to make clear the problem I am highlighting, whatever about the answer.

In Britain the educational philosopher Paul Hirst made a major contribution to the debate in the 1970s in his distinction between ‘**Forms of Knowledge**’, ‘**Fields of Knowledge**’, and ‘**Practical Theories**’.


Regarding ‘**forms of knowledge**’:
Put simply, Hirst contended that, over time, human beings mutually constructed specific modes of thought, or ways of knowing, namely, philosophy, mathematics, physical sciences, social sciences, morals, religion, literature, fine arts. These modes of thought, or ways of knowing, are complex ways of understanding experience, which are publicly specifiable and require justification. Each deals with different concepts, possesses a different logical structure, each contains distinctive expressions which are testable against experience, each utilises different techniques and skills for exploring experiences, and each defines its own criteria for distinguishing true from false and good from bad.

Regarding ‘**fields of knowledge**’:
Related to ‘**forms of knowledge**’ are ‘**fields of knowledge**’, Hirst held. ‘**Fields of knowledge**’ are akin to what we call interdisciplinary studies – geography is probably one of the best examples of this – where certain forms of knowledge, especially, history, economics,
anthropology and geomorphology are brought to bear to illuminate our understanding of the central Geographical concept of place, or space.

Regarding ‘practical theories’:
Related to ‘forms of knowledge’ and ‘fields of knowledge’ are what Hirst termed ‘Practical theories’. These ‘practical theories’ are defined as relevant knowledge from the various ‘forms’ organised around some central practical problems in order to help us to:

a. Understand the problems better
b. Come up with possible solutions to be tried out and tested

Medicine fits into this latter category since it consists of knowledge organised around a series of medical problems. It is a practical theory since it draws upon such disciplines as anatomy, physiology, biology and so on to help us to understand medical problems and come up with possible solutions.

We can conceptualise law, architecture, veterinary science and Education Studies in similar manner.

Around the same time as Hirst was clarifying his thinking on this matter, the famous educationalist, Harry Broudy and colleagues at the University of Illinois (Urbana-Champaign) in the USA (and later people like Bill Connell at the University of Sydney and Brian Hill at Murdoch University) were designing a model for the study of education as a university area of study which was consistent with Hirst’s view of the study of Education as a practical theory.

(see, for example, Broudy, H. S., Parsons, M. J., Snook, L. A., Szcze, R. D. Philosophy of Education: An Organization of Topics and Selected Sources, Chicago, University of Illinois, 1967.)
Instead of using the term ‘practical theory’ they spoke of areas of study such as medicine, the law, engineering, architecture, veterinary science and Education as ‘professional areas of study’.

Their starting point in providing a possible justifications for the autonomous existence of such professional areas of study can be summarised as follows:

- For a professional area of study like Education to justify an autonomous existence it must have a set of special problems that direct and focus its enquiries.

- For a professional area of study to be professionalised it must use and organise facts and principles taken from relevant diverse disciplines, e.g., chemistry, physics, sociology psychology, philosophy and history around the demands of its own problems.

- If a field of study is to be professional, it has to utilise practice in order to illuminate theory and to use theory as a guide to practice.

Their argument was that a profession is engaged in a practical enterprise; it is goal oriented. Again, this position can be summarised as follows:

- Law, medicine, agriculture, engineering, and Education have distinctive social functions, and their raison d'être in every case is the rendering of a service to clients.

- Hence a professional field of study generates rules of practice as well as principles, or generalizations, that guide practice.

From this position, they developed the following schema for education studies:
In identifying major educational 'problem' areas as indicated above, Broudy was equating the study of education with the study of medicine which defines itself by drawing upon such disciplines as anatomy, physiology and psychology and organising relevant bodies of knowledge around a series of medical problems to give us such areas of study and research as psychiatry, paediatrics, gynaecology and so on.

Engineering can be similarly viewed. It defines itself by drawing upon such disciplines as physics, chemistry and mathematics and organising relevant bodies of knowledge around a series of engineering problems to give us such areas of study and research as chemical engineering, structural engineering, nautical engineering, electrical engineering and nuclear engineering.

If we place Education within such a scheme, it is clear that one can engage in academic study and research either on the vertical axis or on the horizontal axis.

<table>
<thead>
<tr>
<th>Educational Problem Areas</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aims and Policy</td>
</tr>
<tr>
<td>Philosophy</td>
</tr>
<tr>
<td>History</td>
</tr>
<tr>
<td>Social Sciences</td>
</tr>
</tbody>
</table>
If, for example, one is interested in working in philosophy of education, one is concerned with drawing from existing knowledge within philosophy and philosophy of education to help one understand better and come up with suggestions for improvement in the areas of aims and policy, curriculum, teaching and learning, and management, administration and leadership studies, and teacher preparation.

On the other hand, one can start with a contemporary issue within one of these major educational problem areas and investigate it from the position of its philosophical, historical, psychological or societal aspects, or a combination of these aspects. It is when viewed from this perspective that it makes sense to talk of such areas of study as curriculum studies, teaching and learning studies, or educational management, administration and leadership studies.

This identification of the five major educational problem areas is also extremely helpful when we consider the manner in which they are organised from left to right. There is nothing haphazard about this. The implication is that in any educational system, from the national level right down to the classroom level, we should initially be clear about our aims and policy. It is only in the light of this that we should make pronouncements about curriculum. Furthermore, it is only when we are clear about the curriculum that we should pronounce on matters of teaching and learning. Once we are clear on all of these areas we can pronounce on Management, Administration and Leadership. And, finally, when we are clear on all of these matters we can pronounce on Teacher Preparation, both pre-service and on-going.
This might all seem very obvious. However, I suggest to you that it is not so. Take for example that fact that so much of our research in the last 10-15 years has been on Teaching and Learning. That emphasis is excellent. Nevertheless, so much of it has taken place divorced from considerations like those I am now trying to promote. To put it another way, research on teaching and learning has taken place as if teaching and learning are neutral activities, that all we need to do is investigate the efficacy of as wide a variety of approaches as possible and then that we can pick and choose from them on the basis of the evidence of ‘what works’ and ‘what is best practice’. Yet, cognisance of the importance of making clear our aims and policy and from there making clear our curriculum should surely suggest that teaching, and hence drawing on the results of research on teaching and learning, is not a neutral activity. We do not need to look very far, or to look back very far, to have this made clear to us. Off the top of my head I thought of the furore, especially in Queensland back in the 1970s when Jerome Bruner’s widely acclaimed MACOS social studies programme led to great unrest when educational policy makers failed to take note of the fact that ‘values clarification’ pedagogical practices can be very confronting for parents who believe that our values are already totally clarified and the purpose of the teacher is to teach those values, not engage in questioning and clarifying them.

To finish on this first theme - in terms of developing our culture of excellence in educational research, the notion of the five great problem areas in educational research is also helpful in three ways I have not mentioned so far. First, it is helpful in terms of conceptualising our research, By this I am suggesting it is important to locate one’s research issues, problems, or
questions not only in relation to the major one of the five problem areas to which it belongs, but also in terms of how it relates to each of the other four areas.

The second way in which the framework I have presented is helpful is that it gives us a meaningful model for having debates about one of the current flavours of the month – interdisciplinary research, including interdisciplinary research in education. I suggest that without such a structure such debate cannot take place in a meaningful way.

There is also a third way in which the framework I have presented is helpful. When coming to laying out implications for practice and further investigations, it is important that one does this not only in terms of the specific problem areas to which it belongs, but also in terms of how it relates to each of the other four areas. In other words, if one has completed a piece of research on teacher preparation, it is important that the results speak not only specifically to improving practice in teacher preparation and indicating areas of further research in the field, but one should also tease out implications for educational aims and policy, curriculum, teaching and learning, and organisation, management and leadership.

Finally, and hopefully not contradicting myself, I would be the first to caution against the essential nature of these structures I have been presenting. Rather, they should be seen as provisional, as a way of beginning to think intelligently about our studies in the first instance while accepting that our frames of reference may change as our research proceeds. In other words, it needs to be recognised that this is not the only way to view educational studies. Also, it is a perspective which can be subjected to all kinds of criticism. However, it can be
very useful as an anchor point if you get into a state of confusion or information overload. It can be a helpful place ‘to come home to’ when you begin to ask yourself how the many diverse issues, areas of study and research perspectives which you will come up against relate to each other.

THIS THEN BRINGS ME TO THE SECOND MATTER I WISH TO ADDRESS, Namely

**THE IMPORTANCE OF ADOPTING A CLEAR AND REALISTIC APPROACH TO THE RESEARCH PROCESS IN ORDER TO FOSTER A CULTURE OF EXCELLENCE**

This is aimed primarily at the postgraduate students. I think it may also be somewhat contentious. I hope I do not upset any of you who are thesis supervisors. It is not my intention to do so and by all means take the initiative and tell the students why I am wrong (if you think I am) when next you speak. Again, also, I would ask you to be patient with me while I rehearse some very basic points regarding the origins of research. I know that for most of you this will be ‘old hat’, something you have long mastered. However, I have to make these very basic points in opening the discussion so that the central matter I wish to address can be contextualized.

Research, experiencing and reasoning, are the principal ways in which we attempt to understand our environments. Many of us have been socialized into assuming that engagement in research is the most sophisticated of the three activities. Emery, however, has argued that it is as much a natural human function as breathing, stating:
Research...is an ancient and ubiquitous activity. Curiosity about other and the worlds in which they live has always been displayed through conversation, asking questions, working together to see what happen after different kinds of actions are performed, talking or gossiping about others to tease out intentions and other reasons for behaviour, clarifying and understanding circumstances; all are fundamental research functions.


He concluded by arguing that it is upon such slender foundations that ‘the whole massive superstructure of “research”’ is based.

The challenge for anyone contemplating engaging in research, however, is to try to find a pathway from one’s ‘natural’ research inclination into this ‘massive superstructure’. One way of proceeding is to view the initial stage in the research process as consisting of two major steps. The first step has its origins in an observation one makes. This observation may arise from viewing something on TV, reading something in the newspaper, hearing something on the radio, becoming cognisant of a particular policy issue, or being faced with some difficult decision to make. What may quickly follow is some curiosity, perplexity, confusion or doubt on one’s part. This curiosity, perplexity, confusion or doubt, in turn, prompts one to want to know something. The result is that one begins to engage in research.

The second major step in the initial stage of the research process often follows quite rapidly. It involves ‘moving from an ordinary everyday question to a researchable question by focusing on one aspect of the issue arousing your interest’. The goal in this step is to produce a clear statement of the problem to be studied. Such a statement of a problem must explicitly identify
the issues on which the researcher has chosen to focus. This requires that one ‘clarify the issues’ and narrow the ‘focus of concern’.

And this is where I come to the central idea I want to promote here, namely, that it is important to adopt a clear and realistic approach to the research process in order to foster a culture of excellence. What, you may well ask, is the problem that prompts me to be emphasising this. I want to spell it out for you. I believe that over the past 20 years great confusion has manifested itself in the work of many postgraduate students, and much more so in that of those engaging in research using what we loosely term ‘qualitative research approaches’ that in that of those using what we equally loosely term ‘quantitative research approaches.

As an aside, it think it may be even worse now in the work of many of those who are embracing the new religion of mixed methods research; or is it mixed methodology; or does the difference in terminology between ‘method’ and ‘methodology’ not matter any more? And then there is that word ‘mixed’, again used soo loosely without clarity. We know what it is to mix something – I have mixed sand, water and cement to make concrete. As a son of a baker, I have mixed flour, milk, yeast, eggs and cut fruit to make lovely fruit loaves. So what is being ‘mixed’ in mixed methodology? In standard mode, one does a quantitative study – probably a survey, using an instrument that has to be tested and validated. It uses concepts, or constructs that have been operationally defined. A set meaning is attached to each of them. Then it is usually followed up by some in-depth interviewing, often on the argument this may give more in-depth understanding of the phenomenon under investigation. But in-depth
interviews, or even open-ended questions, are designed to elicit participants' own definitions and understandings of situations, and while they may use the same language as that in the survey or questionnaire, we have no way of knowing that the attribute the same meanings to these terms. So can we really be justified then somehow mixing, blending, the results of both approaches into some kind of synthesis. On this I had better stop or I will really head off on a rant about confusion.

I have digressed – I was about to illustrate what I meant by confusion I have witnessed in the work of many students engaging in research using what we loosely term ‘qualitative research approaches’. So disturbed was I by this that a number of years ago, 2007 in fact, I was driven to deal with it in a book published by Routledge and entitled Planning Your Qualitative Research Project: A Beginner’s Guide to Research in Education. I would like to quote at length from Chapter One, to illustrate my concern:

There is a great enthusiasm now for qualitative research One of the difficulties faced by beginning researchers is that many of the available texts stress that qualitative research is interdisciplinary, drawing from academic disciplines as wide ranging as psychology, sociology, anthropology and linguistics. The position adopted here is that while some approaches to qualitative research can be interdisciplinary, qualitative research does not always have to be so. Also, to seek to plan research projects within an interdisciplinary framework necessitates an academic training of both great depth and breadth since to achieve competence in the central concepts of any one of the academic disciplines mentioned above takes years of sustained and concentrated study. Yet, many
conscientious students approaching a qualitative research project for the first time, particularly in order to produce a thesis as part of the requirements of a university degree, often do not have this position made clear to them. Consequently, they regularly embark on an unsystematic dredge through a wealth of material, driven by a belief that they have to somehow come to master not only the central concepts and methods of investigation of a number of disciplines, but also those of the sub-schools within them, some of which are diametrically opposed to one another. Also, because of the emphasis on the interdisciplinary nature of qualitative research, students often seem to think that their task is to reconcile competing positions, develop their own eclectic stance and then defend it as being suitable to undergird their chosen research questions.


What I find amazing about this is that I often come on theoretical frameworks designed by students which, I am told, constitute an eclectic framework integrating, for example, the central notions of the German critical theorist, Jurgen Habermas with those of the French Poststructuralist, Michele Foucault. And it does not seem to be a bother that these two giants of the intellectual world could never find any agreement themselves, could find no touchstones, could find no way forward together. And yet here I have before me the work of someone who thinks it can be done, and has done it. And, worse still, felt he, or she needed to do it.

Let me go on to continue with what I said about this in the preface to my methodology book (and it is a book about methodology rather than about method)
This is not to argue that it is a waste of time to engage in the latter exercise; indeed, cognitive dissonance has long been considered to be a valuable learning experience. On the other hand, the enormity of the task of constructing an eclectic theoretical framework for one’s qualitative research project is often not appreciated by beginning students. Consequently, they dig deeper and deeper, often only to find that they have simply wandered into a swamp from which, without expert guidance, they find it extremely difficult to extricate themselves. I regularly have students come to me after engaging in various introductory programmes desperately trying to disentangle, compartmentalise and connect together such strange bedfellows as discourse analysis, positivism, case study research, telephone interviewing techniques, dilemma analysis, poststructuralism, autoethnography, and non-participant observation.


So, what I am asking for here is some realism, some structure, some road maps, to borrow the latest Americanism., to avoid this situation. My view is that a culture of research excellence is more likely to thrive if such scaffolding is regularly available, its need regularly discussed, and one is not afraid to contest those who like to ‘posture’ and, in the interest of promoting a notion of their own intellectual superiority, justified on the grounds of promoting rigour and excellence, only succeed in generating confusion. I am not going to now engage in a seminar on research design. Just to ram home my point, however, it is useful to point to two such pieces of scaffolding. One of these is that outlined by Crotty, which has become populat in a number of education and social science faculties, where he impresses the need to consider one’s chosen area of research in terms of how it connects to:
a. an underlying research paradigm;
b. a specific theoretical position within the paradigm;
c. a specific methodology consistent with the paradigm and the theoretical position;
d. a set of methods for data gathering and analysis consistent with the specific methodology.


Harry Wolcott, the famous educational anthropologist, suggested a slightly different, though equally useful way of thinking to help one engage in the related processes of clarifying one’s research issue and narrowing the focus. His position is that as inquiry proceeds, the idea that prompted it should become both better formed and better informed. To this end he suggested three categories which ‘form a modest typology’ of the ideas that guide inquiry:

 reform-driven ideas,

 concept-driven ideas, and

 ‘big’ theory-driven ideas.


reform-driven ideas,
It is arguable that most educational research has its origin in reform-driven ideas. What underlies all research questions articulated within the category is an ‘assumption on the part
of the researcher that things are not right as they are or, most certainly, are not as good as they might be’. Examples of research questions which could be posed in this vein include the following:

- What is the most effective leadership style to adopt in schools?
- Does smoking affect academic performance?
- What is the extent of truancy in inner-city high schools?
- Does a concurrent programme of teacher education where students engage in teaching practice throughout their years of undergraduate study produce better-skilled teachers than a consecutive programme where students first of all study for a degree in a substantive subject area and then undertake a one-year teacher preparation programme?

The researcher who poses questions such as these usually feels that all is not well with current practice and is prompted to bring about change directed at trying to improve the situation.

**concept-driven ideas,** Wolcott’s second category of ideas that guide inquiry are those identified as ‘concepts’. These come in all shapes and sizes. Working at this level with, for example, a concept like ‘culture’ (I am not reaching for my gun), ‘can provide a focus without allowing the seeming absence of theoretical structure to become overbearing’. The main concern of those adopting such an approach is with attempting to describe aspects of culture, usually by producing an ethnography – although I know there are not people who believe it can be measured, and try to do so.

A number of other examples illustrate how research can be concept-guided:

- Sindell developed research questions in his study of the socialization of Mistassini Cree children from factors identified as important in Bandura’s social learning theory.
Spindler examined the impact of the school on attitudes towards urbanization among inhabitants of a rural German village drew explicitly from assimilation theory.

The tendency in much contemporary research on adolescent alienation from school to draw heavily on the concepts of hegemony, symbolic violence and the political context of knowledge and meaning.

'big' theory-driven ideas.
Wolcott’s third category of ideas that guide inquiry encompass those ideas that relate to an overall ‘grand’ or ‘big theory’. He quoted the sociologist Robert Merton in this regard, where Merton spoke of ‘the all inclusive systematic effort’ to develop a unified theory that explains ‘all the observed uniformities of social behaviour, social organization and social change’. Wolcott argued that those who ‘think theory’ in this ‘grand’ sense are attempting to ‘link up in someone’s – perhaps even their own – Big Theory, everything that matters to everyone’.

Educational researchers have, in fact, put forward various typologies that can be drawn upon to decide which ‘big theory’ should underpin one’s research project – one such typology arguing that there are four such ‘big’ theories, namely, positivism, interpretivism, critical theory, and postmodernism. Also, it is held that each of these ‘big theories’ can, in turn, be broken down into a number of related theoretical perspectives. For example, the interpretivist ‘big theory’ embraces such theoretical positions as hermeneutics, ethnomethodology, symbolic interactionism and phenomenology.

To conclude, I am not advocating any particular structured approach, whete it be Crottly’s, Wolcott’s, a combination of both, or any others. What I am strongly advocating, however, is that many students, from the outset need to be quickly placed on the path of adopting a
particular structured approach. In this way, they have some basis to help them to move forward. This does not mean that one promotes it as a strait jacket and as ‘the one correct way’. Indeed, one can ask students not to dismiss the possibility that, after they have worked through this approach a number of times they may eventually, and quite legitimately, reject it and embark, this time with confidence, on developing once again their own eclectic conceptual frameworks. Nevertheless, I hold that without a structured approach as initial scaffolding when they embark on planning their initial research projects, many are unlikely to ever come out of the swamp of confusion.

AND NOW TO THE THIRD MATTER I WISH TO ADDRESS, NAMELY,

THE IMPORTANCE OF BEING BRAVE AND CONTESTING EDUCATIONAL IDEAS HELD OUT AS ALMOST ‘REVEALED KNOWLEDGE’

It is important within Education Faculties that we serve the profession. Certainly we should spend certain amounts of time ‘out in schools’ working with practitioners. This is important, to demonstrate that we are not ‘ivory tower’ academics, out of touch with those doing the work in the field. It is also important so that we can have our own research agendas informed by the issues, both immediate and in the medium and long term, that are of concern to practitioners. Further, it is important that we engage in research both for the profession and with the profession. That research can be of various kinds, including survey work, case study work and action research. However, I feel strongly that it is vital that we do not conceptualise our research only in this way - as a sophisticated form of professional development - not to mention that educational research should be not just about schools, but about adult education, further education, higher education and alternative and non-formal education.
The danger in conceptualising our research only in this way - as a sophisticated form of professional development - is that in doing so we will be part of an exercise in maintaining the status quo, which in the view of the late Laurence Stenhouse, could possibly contribute to rigor mortis in educational research.


Let me put it another way; we have to be ever alert to the danger of just becoming the professional development arm of educational bureaucracies, government departments of education and their private sector equivalents. Again, I repeat, - research for and with schools and such organisations is vital, but it should not be our only research. The danger of it being our only research is that we may develop practices such that our expertise is only sought out to provide evidence to legitimate already pre-ordained policy positions. In order to promote a culture of excellence in educational research we need to be ever alert to the possibility of being drawn into such practice and be brave from time to time to say no if we suspect that our objectivity may be compromised in order to provide organisations with the answers they want. I have been at briefing sessions by state departments of education where those conducting these sessions have openly stated things like “now we don’t want University X doing this research because of their findings in previously conducted work of a similar nature”.

So, what sorts of contestation am I talking about. I want to give you some examples. In the interest of maintaining coherence of some sort in this presentation, I want to give them to you
in a manner which relates to my very early considerations. In other words, I want to do so by referring back to the framework I gave you for Education Studies as a professional area of study. Within that I noted - you will recall - the five great problem areas around which educational studies can be organised. In my own case I have tended to approach my own research by focusing in particular on one of those problem areas, namely, curriculum. To put it another way, I have constructed my research as essentially coming under the banner of Curriculum Studies.

Now I know that this term Curriculum Studies itself can be confusing since within many university ‘education’ faculties the term curriculum is used to refer simply to the subject matter that is transmitted to students by teachers and lecturers in schools and colleges. In similar vein, the term ‘curriculum studies’ is used to denote the teaching approaches that need to be studied, understood and practiced in order to facilitate the effective and efficient transmission of that content. In this view, as Wilf Carr has put it:

**Curriculum debate is a narrow technical debate about the instrumental effectiveness of different pedagogical traditions” and one which policy makers do not wish to see extending beyond these parameters (Carr, 1993, p. 6).**

(see Carr, W. Reconstructing the curriculum debate, *Curriculum Studies*, 1, 1, 1993, pp. 5-9.)
The consequence for teachers was summed up as follows by Sutherland:

There seems little impetus to serious consideration of central and general aims. Fashions succeed each other, and teachers – theirs not to reason why – are expected to change content and methods of their work in due conformity, following and climbing on each band-wagon as it comes along (Sutherland, 1985, p. 223).


And so, it seems to me, various educational bureaucracies expect members of education faculties to serve in like manner.

To develop this argument a little more, most public debate on education, it seems to me continues to be about how to achieve in the most cost-effective manner, pre-ordained outcomes, but very little debate is about the value of pursuing such outcomes in the first instance. And yet there is a tradition – one with a rich academic lineage, which promotes a more enlightened approach – and it needs to be resurrected, promoted and practiced. This tradition is based on a definition of ‘curriculum’ that is not just a description of subject matter, but also sees it as a set of proposals indicating how this subject matter is to be organized, the educational purposes it serves, the learning outcomes it is intended to achieve, and the methods by which such outcomes are to be evaluated. When the meaning of curriculum is conceptualised in this way, ‘curriculum studies’ becomes a wide-ranging intellectual interrogation of the educational aims and values guiding the selection of curriculum content,
the epistemological principles underlying the way that content is organized, and the pedagogical principles underlying the way it is taught, learned and assessed. Research in curriculum studies thus becomes an interdisciplinary study concerned with theoretically examining the grounds on which any curriculum proposals have been erected and empirically evaluating the consequences – for both teachers and learners – of efforts to translate these proposals into educational policy and practice. It thus feeds an open discussion about the validity of the educational principles on which the curriculum is based and the feasibility of their practical implementation.

Such an approach is badly needed at the present time, when we are bombarded with a range of curriculum initiatives, most of which have been taken up in various quarters in some form or another without being subjected to interrogation and reflection, and where the focus of associated research is simply about finding the most efficient means to achieve the ends.

The uncritical embracement of outcomes-based education immediately comes to mind. This approach varies little from its precursor, the ‘behavioural objectives’ approach, particularly in terms of stating educational outcomes in behavioural terms (what it is the student should know and be able to do at the end of a lesson and at the end of a programme of study). Some solid critiques have appeared, revealing the absence of a theoretically robust research base to support arranging the school curriculum and assessment systems sequentially in terms of measurable outcome statements and achievement objectives. Rarely, however, are such critiques articulated publicly and in a manner that might pause key stakeholders to pause and think about what the ‘real’ agenda might be. Could it be, for example, that politicians,
policymakers and educational administrators are wedded to a competencies and student outcomes approach to the curriculum, not because they are intrinsically worthwhile educationally, but because they facilitate the exercising of surveillance and control over the professional work and lives of teachers. The research agenda this opens up is very large, spanning not just the historical and policy fields, but also philosophy of education, sociology of education, and comparative education, along with cognitive psychology. Rarely, for example, do we hear pleas for more research to follow up the contention of Keddie that the simple hierarchical sequencing of content knowledge can promote a notion that teachers’ and lecturers’ presentations are to be taken on trust and can result in students becoming less autonomous, of those who have contested Gagne and Merrill’s notion that higher-order reasoning should not be attempted until lower-order skills and knowledge are well established, and of those like John who held that assembling the elements in a bottom-up sequence does not create a flexible cognitive skill and is “self-defeating”,

To take a second example to make my point that there is a need to be brave and contest educational ideas held out as almost ‘revealed knowledge’, how often do we hear calls for research that might cause us to have second thoughts about Howard Gardner’s notion of multiple intelligences. This constitutes another construct which has been largely uncritically adopted within the curriculum of many schools and educational systems. Indeed, some have taken to it with almost religious zeal. This is not to argue that the notion does not contain insights that can be harnessed to improve pedagogical practice. Yet, few are aware of the rather devastating critique to which it has been subjected by John Whyte at the London Institute of Education, who argues convincingly that the credentials of Gardner’s theory of
multiple intelligences lack justification. He also points to changes in the theory since its inception in 1983, highlights problems with its application to education, and pleads for more research in the field, both of an empirical and philosophical nature.

Other examples to make my point that there is a need to be brave and contest educational ideas held out as almost ‘revealed knowledge’ if we are to promote a culture of excellence in educational research, also present themselves. For example, A curriculum approach that has come into vogue in primary school sectors is that of Reggio Emelia, named after the city of the same name in Italy. Clearly, it is a very popular and successful curriculum approach in its home setting. However, issues of context are very important when it comes to considering what works elsewhere. In this particular case, the fact that the approach thrives in a sector of Italy that is communally based, and which has had a communist government for over 50 years, raises questions about the extent to which we can transplant what are clearly successful curriculum innovations from one context to another. Institutions that have and promote a vibrant research culture would be keen to interrogate such phenomena through their research agenda.

My final example relates to the uncritical embrace of constructivism by curriculum designers. Once more, this approach clearly has much to recommend it, particularly in terms of sensitizing us to the importance of trying to harness where the student is ‘at’ cognitively when deciding on our pedagogical practices. However, those who advocate a constructivist approach to the exclusion of all others fail to take heed of those curriculum theorists who caution against the neglect of other pedagogical approaches, including more traditional
approaches. Geddis, for example, pointed to the folly of ignoring direct instruction when he highlighted the

incongruity of leaving children on their own to devise scientific perspectives that have taken the human race centuries to articulate (Geddis, 1996, p. 254).

To finish then on this third theme of mine, namely, THE IMPORTANCE OF BEING BRAVE AND CONTESTING EDUCATIONAL IDEAS HELD OUT AS ALMOST ‘REVEALED KNOWLEDGE’, I am acutely aware that in advocating this position I am making a case that conceptual research is every bit as important, and perhaps even more important, than empirical research. My argument is that if we are to have a culture of excellence in educational research then conceptual research must not be neglected. Indeed, it must be fostered and we must make sure we recruit suitably qualified people academically to teach and practice it.

THIS BRINGS ME TO THE FOURTH, AND FINAL MATTER I WISH TO ADDRESS, NAMELY,

BE BRAVE WHEN IT COMES TO DISSEMINATING THE RESULTS OF YOUR RESEARCH

I don’t want to say too much about this, partly because I have kept you here long enough, but more importantly because I don’t want to end by giving you stress. If you are a staff member I would imagine you are feeling the pressure to publish. We all are, although
some of us are better able to deal with it than others. I find it a shame that we are coming under pressure to publish because there are dollars attached to it, because each publication is allocated points and that these go towards the research quantum and the funding of our respective schools and faculties. That, however, is the world in which we live and there is little we can do to change it. Daily I am bombarded by statements about where my university is located within this world index and that world index. And, to tell the truth, the way the indexes are set up even if all of us in education faculties were all publishing prolifically every year in Thomson ISI journals it would make very little difference to our respective universities rankings anyway, since it is all loaded primarily towards the physical and biological sciences and related fields, where what counts is citations and impact factors.

In terms of promoting a culture of educational excellence I think we would be so much better off to promote a culture of wanting to publish rather than one of having to. Fora like this should be catalysts for such activity. And also there are the tried and tested research seminars which are held at regular intervals in most institutions throughout the year for staff and student presentations, as well as presenting at conferences. And yet, taken on their own, even these have an element of compulsion about them.

The answer, I think, is to find a way to promote TALKING about research, about one’s own research, about the research of others, about research questions that come to one in the dead of night and in the middle of the day, as a natural part of our daily work in the university. It should become part of what is ‘normal’ conversation. We need to promote a
notion of stopping each other in the corridors to share our thoughts and plans, and to try to find time to head off for a coffee amidst our busy days and talk to each other about our latest bright idea. There is no magic solution to achieving this, but I believe that all it takes is one, or two (or three) to be of such a disposition and it will enthuse many.

And in fields like education, the humanities and the ‘soft’ social sciences, I believe that this is a more appropriate culture to foster than that of dedicated research teams. Such dedicated teams are all the go in the sciences – indeed I was struck by the extent of this lately on reading Lee Smolin’s fascinating book, *The Problem With Physics*, where he indicates you haven’t a hope nowadays of getting a research grant as an individual scholar in physics. Not that I am saying we should not promote dedicated research teams in Education. They are a good idea. Rather, what I am saying is that when it comes to prioritising one approach over another I think that within Education Studies we should prioritise this culture of talking about one’s research as a natural part of one’s daily work, and with as many colleagues as possible from as many sub-fields and disciplines as possible. In that way, research teams will evolve as natural alliances, will disband, regroup, take new shape, and so on, rather than be set up as contrived structures. Maybe this is too much to hope for, but I think it is worth striving for and I also believe that it will yield results and outcomes every bit as quickly as the ‘pressure to publish’ model, which can be a bit scary – and trying to do research and get it published when you are scared will mean no research and publications for some.
I do, however, encourage you to publish and to cultivate a culture of writing up your research as part of your daily work. If you are a research student there is nothing wrong with trying to disseminate your ideas en route to completion. Research proposals themselves, suitably reworked, are of great interest to journal publishers – not everything published has to be ‘results’ And when you are finished that thesis, then work with your supervisor (or on your own if he or she has not got the time) to write it up in a paper, or papers, so that the rest of us can know about it. Too many theses lie in cupboards gathering dust, or now on data bases which nobody is desperately trying to hack into and provide leaks.

To you as students, and to you as academics, I say, do not underestimate the importance of your work and the importance of disseminating it. While you would not want to underestimate what it takes to get into the journals, you should also not overestimate it and, at the same time, underestimate your own ability. Believe me, as a member of too many editorial boards of journals, you have no idea how bad many of the papers are that reviewers have to read. And be aware what a wonderful free service journals provide. You send off your paper, reviewers give lots of their precious time reading it, reviewing it, criticising it – really, this is an amazing free service. Don’t be put off by the humbling experience that it can often result from getting copious pages of feedback telling you what is wrong with your work. Believe me, we all get these kinds rejections, and more often than many of you might imagine. But after a day or two you get over it, pick up the pieces and give it another go. It never gets easy.
Finally, do not forget that the thesis you are doing, or the one you are supervising, might well be a book. If you are savy enough you may indeed be able to craft it from the outset so that once it is examined it may lend itself to being published almost ‘as is’ apart from a quick search to make sure that everywhere you have used that word thesis (as in the sense of a dissertation, or product, rather than argument), and simply replace it with the word ‘book’. Many of the theses written using the model of separate chapters devoted to an Introduction, Literature Review, Methodology, Presentation of Results, Discussion, and Conclusion do not lend themselves to such quick transformation into a book. But there are other models, depending on the topic and research approach and it is worth considering them at the thesis stage just in case a book could be a possible additional prize for all of your effort.

So let me just finish by saying that once you get into writing and publishing, are brave enough to send out the work for review and are prepared to take the feedback on the chin, it actually becomes very satisfying. Better still, it becomes fun, And wouldn’t it be wonderful to come to the university every day to have fun.

Thank you very much.