

## **Increasing Impact? An analysis of issues raised by the impact agenda in educational research**

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### **ABSTRACT**

The notion of 'impact' is interrogated in the context of academic education research. A moral agenda for increasing the impact of academic education research is distinguished from the 'impact agenda' reflected in UK research assessment practices. It is argued that, especially in the English case, academic educational research currently has relatively scant impact on policy and practice, and that this comprises a major concern, both with regard to evidence-informed good practice within the education system, and the often retrogressive practices that are undertaken instead. Various explanations for this lack of impact, and impediments to academic engagement with policy and practice, are identified and discussed. It is concluded that there is a moral imperative for education researchers, and the educational research associations that support them, to develop ways to better ensure impact of our vital research.

### **BACKGROUND**

The role of academic research in public life has recently come under increasing scrutiny in the UK and some other parts of the world. Arguably, the ensuing debates have a particular salience for the Education discipline, given that education is primarily a field of practice. The word 'impact' has recently developed specific and perhaps somewhat distorted connotations in British academic circles, due to policy debates suggesting a need to measure the impact of academic research on society at large (and hence an implied critique that some academic research has been insufficiently concerned with impact, or has had insufficient impact). The application of notions of utility and 'value for money' in these debates has put academics on the defensive, both with regard to the insinuations that their research does not currently offer value for money, but also, more profoundly, in a philosophical rejection of the notion of utility (and its narrow conception) as the driving principle for the validity and funding of academic research.

Conversely, it is asserted in this paper that *educational* research has become far too removed from policy and practice, at least in the case of England, but arguably also – albeit to a lesser extent – in other parts of the UK and the world. And that as educationalists, we should be concerned to increase our research 'impact'. The paper begins by briefly looking at what we mean by impact. It then questions whether educational research is having impact at present, arguing that (especially in the case of England) evidence points to the contrary. This argument begs a further question as to whether this matters, and it is argued here that there is a

moral imperative for academics to engage the impact agenda *beyond* the narrow drivers of research assessment measurements. There follows discussion of key impediments to impact, and some pragmatic approaches via which we might secure better impact for academic educational research.

## THE NOTION OF RESEARCH IMPACT

Research 'impact' can be understood as making a difference. 'Impact' is generally seen as distinct from 'influence' – one might communicate with, and even impress, a teacher or politician, but unless this influence results in tangible outcomes of change in practice or policy production, impact has not been achieved. Immediately, one can see the potential blurriness of boundaries likely to beset any attempt to measure impact.

Nevertheless, this measurement is precisely what is being attempted in the Research Excellence Framework<sup>1</sup>, the mechanism by which UK academic research is subjected to quality assessment. Building on policy discussions concerning research utility and value for money alluded to above, it was suggested that 'impact' become a key criterion within research assessment, generating heated debate among, and between, academics and policymakers. Following consultation and piloting, it has been set down that - while a secondary criterion to 'excellence', and comprising a lesser proportion of score value than had been originally mooted - 'impact' will feature as a measure of research value in the forthcoming 2014 Research Excellence Framework exercise. Specifically, while research entered for assessment must demonstrate 'excellence', the *impact* of this excellent research will generate up to 20% of the available score.

As a consequence, the notion of 'impact' has been tainted in academic circles, both by the utilitarian (and even political) discursive drivers of this focus, and by its application as a criterion for measurement in the Research Excellence Framework (REF). This latter reduces 'impact' to credentials; an exercise whereby the recording of impact of academic work beyond the ivory tower generates money for one's institution. Albeit, it is instructive to observe the 'impact' that the impact agenda is having on academics as they prepare for their REF submissions: suddenly, the retrospective construction of narratives around the impact of one's research has become a swift priority, as the issue of impact dominates departmental research agendas. A mini-market has emerged in consultancy and academic seminars wherein key figures 'in the know' are invited to speak to university departments and conferences. British academics have become adept game-players over the years of the RAE/REF, and tend to be pragmatic in adjusting to new rules, whatever they may think of the premise.

As such, it is arguable that the REF adopts a narrow view of impact, and distorts motivation for it. This being said, it is interesting that the debates generated by inclusion of the 'impact' category in the REF have provoked theoretical analysis, as well as the afore-mentioned 'mini market', in Higher Education. This analysis tends to be more concerned with the philosophical questions generated by the concept of , and demand for, 'impact'. In educational research contexts, such analysis frequently includes reflection on long-standing philosophical debates

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<sup>1</sup> Formerly the 'RAE' – Research Assessment Exercise

about the purposes, validity and limits of research and 'evidence' – for example, the validity of findings that underpin potential recommendations for 'impact', and the extent to which research can and should make claims. This immediately gets us into the territory visited by every sociology 'A' level student concerning positivism v interpretivism, along with recent related contestations concerning the notion of 'evidence-based practice' (for discussion in the case of education, see for examples Oakley, 2001; Hammersley, 2001; 2008).

There is also debate, both conceptual and technical, about how impact can be recorded and assessed (see e.g. Reiss et al, 2010). However, my concern in this article is more related to calls by researchers such as Sara Delamont (2010) for us to focus on what we consider to be *our own* impact priorities. And here I am not thinking simply in relation to the REF, but rather beyond such instrumental pressures to more profound concerns about impact in education.

Might it be argued that there is a moral imperative for academics to be concerned with the impact of their work? And might such an argument have particular traction in relation to the Education discipline? Clearly not all research in education can or should have direct relevance or utility for educational practice – 'blue skies' research remains valuable, here as in other disciplines. As does historical and philosophical analyses (albeit such analyses may well in any case be directly relevant and/or 'impact' contemporary educational work). Geoff Whitty (2005) has also drawn out the distinction between 'education research' (broadly addressing all aspects of the education terrain) and 'educational research' (which he characterises as research that is geared towards improving education policy and practice). However, it may be argued that most (all?) research in education, whether focused on analysing teaching methods or investigating teachers' beliefs, considering the assumptions embedded in the contemporary curriculum or elucidating assumptions of the past, exploring the impact of class sizes or the arrangements for school buildings programmes, may all be relevant to outcomes for learners (and/or practitioners), and to that end, to good practice and improvement.

Education has been called a field of practice, and the various contents and subjects of education - pedagogy, learning, systems, institutional contexts, practitioners and students – constitute the subjects of education research. Education quality, including quality of pedagogy and quality of student experience, must be a key point of investigation for the field; whether the specific research project explores the best way to teach maths, what a quality education should contain, or why certain groups of young people remain excluded from quality education. Of course, such a focus also demands reflection on what *comprises* high quality pedagogy and learning experience. Such questions in turn cannot be disconnected from the socio-economic context (and drivers) in which discursive constructions of the purpose of education are manufactured, and from which policies concerning the content of education flow; or from broader philosophical/historic debates about the purpose of education.

As this indicates, education is a highly politically and philosophically contested field. However, in a British society in which access to high status education and outcomes has always been, and remains, extremely unequal (OECD, 2007), and quality and experience of education are often overlooked in an overwhelmingly

instrumentalist policy model (Griffiths, forthcoming 2011), it is especially important that research evidence informs debate. Many education academics came to higher education from teaching careers. As Skelton (2009) observes, most teachers join the teaching profession because they wish to 'make a difference' to children's lives. I would posit that the majority of education academics (myself included) embarked on education research because we held strong opinions and commitments to informing the content and direction of state education. And the research we have produced has provided hugely important analysis. Not just of the most effective ways to teach and learn, but also critique of the existing education system - its systematic perpetuation of social inequality, the lack of fitness for purpose of the National Curriculum in England, and the tyranny of current assessment practices, to list but a few areas.

It would be difficult to argue that these concerns are no longer pertinent. Across the UK and beyond, governments are riven with anxiety about educational performance. This is partly due to the increasing measurement of global educational attainment (e.g. OECD PISA). The increase in such audits, evaluations and comparisons is a product of neoliberal premises that education is the key mechanism for ensuring adequate human capital in the competitive global economic market. The hegemony of such understandings, and the resulting anxiety and competitiveness that such mechanisms fuel in national governments, has in turn fed the drive towards instrumental understandings of the purpose of education as exclusively economic, and of education credentials as exclusive indicators of 'quality'. Actually, the various recent crises in the neoliberal model, and resulting factors such as the high levels of youth unemployment in the UK and elsewhere, starkly reveal the 'brokenness' of such neoliberal perspectives on education. In a climate of austerity and what Ainley (1999) has referred to as 'education without jobs', with (connected) massive structural changes such as the semi-privatisation of Higher Education in England and elsewhere, questions about purpose, content and quality of education have never been so relevant. For example, as I write, in England we have just experienced the most significant social unrest since the 1980s, with youths rioting and looting in London and many other urban centres. The period of clean-up and reflection has just begun. I predict that it will not be long before attention turns to the role of education, both as a point of blame for 'failures', and as a potential solution for multiple perceived ills and challenges. A question I might posit, and which will likely be pursued at least by some commentators, is: what is the role of education in securing well-being, understanding, citizenship, happiness and resilience in a period of austerity in which jobs are not guaranteed? This may be an opportunity for the re-emergence of more liberal conceptions of education, and a valuing of the intrinsic in education besides the extrinsic.

Moreover, the neoliberal model, with its emphasis on self-entrepreneurship, meritocracy, and credentials to delineate and reward ability and hard work, both illuminates and exacerbates educational inequalities. Its focus on education as *the* ticket to prosperity and consumption, and its reliance on markets to drive 'excellence', further embeds educational inequalities (especially according to social class), which have always been entrenched in the English education system.

A raft of research records the ways in which white middle class parents are able to 'play the market' in education to ensure the reproduction of high achievement for their children, while working class families are disadvantaged by lack of financial and cultural capital. Neoliberal methods of audit and measurement themselves identified the scale of the various gaps – for ethnicity, gender, poverty, SEN – resulting in anxious tinkering by policymakers. Now that resources are likely to be increasingly restricted, and state provision increasingly turned over to private suppliers, such inequalities – in the quality of experience of education, as well as in outcomes – are at risk of increasing even further.

Given these motivations, then, it is surprising that we do not devote more time and energy to trying to achieve impact. It may be the case that the impact agenda as pursued by policy-makers and pursued through the mechanism of the REF is a further neoliberal expression of the demand for 'value for money'. Nevertheless, the need for policy and practice to be informed by evidence, and for our revelations of poor practice and injustice to be attended and acted upon, has never been greater.

### **IMPACT DILEMMAS**

Having indicated what I consider to be the moral imperative of working to further the impact of our work, it is important to acknowledge that this is far from straightforward, and identify some of the tensions and philosophical issues arising.

One of the issues particularly challenging for social constructionists and those of us who have pursued postmodern and poststructural theories in our work, is policymakers' apparent need for 'definitive answers'. Many of us have rejected notions of single 'truth'. The second wave feminist work of the 1970s and '80s, on which my own work builds, mounted a devastating and well-evidenced critique against 'objective' social science, illustrating how the masculinist and sexist assumptions among many researchers had led to skewed foci, skewed samples and skewed findings. This critique coincided with broader postmodernist challenges to scientific 'truth claims', which has been highly influential in the social sciences. Clearly this postmodernist relativism simply comprises a particularly radical expression of scepticism within centuries-old debates between positivism and interpretivism, universalism and individualism and so on. However, it is arguable that this relativist turn has impeded education researchers from offering recommendations and policy solutions, focusing instead on critique. Sally Power (2010) argues that education research is dominated by approaches reflecting the 'cultural turn', and that the focus has been on deconstructing current policies and the assumptions underpinning them, rather than offering anything in their stead. This argument partially convinces: however, it is observable that many education researchers (even those espousing social constructionist/poststructuralist perspectives) *do* offer recommendations for practice (perhaps more so in books, rather than journal articles); but that these tend to be somewhat vague, and perhaps not directly connected to present policy.

This lack of direct engagement with policy, and a lack of bringing government, policymakers and practitioners 'solutions rather than problems', may reflect a range of factors, including:

- The relativism of the cultural turn (with two associated features: a] encouragement of deconstruction rather than construction, and b] rejection of 'truth claims' impeding confident recommendations).
- Hierarchies of research status within Higher Education, fed by the various research assessment exercises, in which relatively esoteric, specialised knowledge for academic audiences (as produced in international peer-reviewed journals) is given higher value than that directed at practitioners and other audiences.
- Cynicism at the likelihood of impact (especially in the English case), given policymakers historic lack of reference to – and even direct rejection of – educational research.
- A wish to avoid dirtying hands. Especially in research critical of government policies, there is a tendency to critique rather than closely engage and become associated with existing regimes and practices.

I might argue that each of these reasons is spurious, although lack of space prevents me from doing so fully. For my critiques of poststructuralist relativism (from a feminist perspective), see e.g. Francis, 1999; Francis, 2007. I also argued recently that our position in an increasingly marketised, competitive, and (in the English case) now largely privatised, competitive Higher Education system means we should be mindful of our glass house before criticising the 5-18 education sectors for apparent acquiescence to marketised, credentialised systems and so on.

### **HOW MUCH IMPACT ARE WE MAKING?**

At least in the English case, it seems academic educational research is making relatively *little* impact. Whether this is true elsewhere in the world demands exploration: impressionistically, it appears that educational research has far stronger and more direct impact on policy than in England. Scotland and Australia are obvious examples of countries where research appears to have greater impact. A range of factors may explain this, including the devolution of education decision-making to states in the Australian case; the smaller size of Scotland and of Australian states in comparison to England; and the recent tradition of marginalisation of educational research from English policy-making (and as I shall argue, a connected withdrawal by education researchers). In the Scottish case, 'Assessment is for Learning', and the 'Curriculum for Excellence', comprise significant examples wherein research has impacted policy development (albeit products inevitably look somewhat different to the specific visions of individual researchers; Mansell, 2010). It may also be argued that *non-academic* education research is achieving far greater impact, not just in England but more globally, as private sector corporations such as McKinsey and Price Waterhouse Coopers expand their research offers. A mapping of the extent and influence of educational research (and the sectors from which this research derives) on policy in different countries would provide important information.

However, certainly in England the ongoing crisis concerning (limited) influence of educational research on policy has continued for many years. Margaret Thatcher famously rejected social theory outright, claiming there 'is no such thing as society', with a resulting rupture in influence of (especially sociological) social research

on government policy that left sociological work largely confined to critique from the margins. While this critique resonated with some institutions still strong in the 1980s, the rise of neoliberalism and the declining power of the public sector unions meant that – arguably at least – such critique gradually lost its purchase on public debates and policy-making. In the case of educational research, a particularly damaging infliction came in the form of a range of reports commissioned on the quality of educational research, which provided critical reports suggesting that educational research was insufficiently robust to impart useful findings to inform policy. The most damning of these was the infamous Tooley Report (1998; 2000).

With the arrival of a somewhat more receptive New Labour government in 1997, ‘education, education, education’ being one of its central priorities, these reports provided a context for policy-makers to attempt to shape educational research, ostensibly to ensure its better usefulness for policymaking. Some academics might question whether this simply meant ‘more useful for government’, especially those who found their government-commissioned research findings embargoed, and/or demanded to be re-written. However, there was at this time a genuine and funded effort by government to encourage what it saw as greater rigour into educational research, to support its vision of ‘evidence-based practice’.

There was a significant resistance to the ‘evidence based practice’ movement in education, especially in response to its positivism and its perceived elevation as bearing greater validity to alternative (qualitative) research modes (e.g., as Hammersley [2001] notes, the suggestion that qualitative research does not comprise ‘evidence’). These debates are too extensive to be pursued here, but a flavour can be found in e.g. Oakley, 2001; Hammersley, 2001, Archer, 2003; Davies, 2003.<sup>2</sup>

Whatever one’s view of such initiatives (and indeed the ongoing ‘paradigm wars’ underpinning them), there is no doubt that they reflect a frustrated perception among policymakers in England that educational research cannot or will not provide answers to key policy questions. Especially since moving more in policy circles (and especially among former advisers to the New Labour government!) at The Royal Society for the encouragement of Arts, Manufactures and Commerce (RSA), I have frequently heard frustration expressed that education research seems unable to give policymakers decisive answers. And indeed such assertions sometimes include a suspicion that educational research remains politically motivated. Many academics would in turn reproach policymakers for not wanting to listen to decisive findings when they do not fit government agendas, and argue that education is a hugely (and perhaps inevitably) political terrain. Of course, as I observed above, such quests for ‘definitive answers’ can precipitate us back into familiar debates around positivism and the limitations/benefits of associated methods such as randomised controlled trials. However, as Scottish curriculum and assessment initiatives may demonstrate, actually a pragmatic approach, and the provision of ‘strong

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2 However, the then-government’s paternalistic and directive attempt to develop ‘evidence based practice and policymaking’ in educational research was perhaps a harbinger to their later endeavour to ensure ‘value for money’ in university research more broadly, as embodied in the ‘impact agenda’ for the Research Excellence Framework. This agenda has been maintained by the Coalition government.

evidence' (whether qualitative or quantitative) can reap rewards in impacting education policy.

So what has been the problem in the English case? In his 2010 presidential speech to the British Educational Research Association Conference, John Gardner controversially asked whether educational research has become too theoretical for access and engagement by policy-makers. In my experience this is not necessarily the case: many policymakers, and those working in think tanks and third sector organisations that mediate research to policymakers, are often highly educated and enjoy both access and recourse to 'catchy' and 'innovative' theoretical ideas, though these may lose nuance in translation. Albeit, clearly such research 'users' need to be able to see the relevance of such conceptual work.

And there are also strong cultural differences in how things are done. English policymaking has a propensity to move very quickly, as the recent Coalition policies around Higher Education, compulsory sector curriculum, Free Schools, and – especially – Academies, demonstrate<sup>3</sup>. This can leave educational research on the back foot, simply documenting and analysing changes that are taking place (and even this often somewhat 'after the event'), rather than shaping policy direction. Moreover, it also appears the case that academic educational research interests are often not strongly in tune with government interests and policymaking. The relative scarcity of research on curriculum content and assessment in England provides an example. Arguably, the higher education research assessment practices (RAE and REF) have exacerbated such dissonance by valuing highly specialist, often esoteric research and publication as published in ('gold standard') international academic journals, over more pragmatic and accessible, practice-relevant work. There is also the aforementioned challenge that too much academic educational research focuses primarily on critique and identification of problems, rather than researching best practice or using findings to develop solutions.

As such, there can be a feeling that as academic researchers we are talking to ourselves. Clearly, this does not apply to all education research, and there have been significant attempts to address some of the dilemmas identified above – for example via the Teaching and Learning Research Programme (TLRP), and Strategic Forum for Research in Education (SFRE). But in conclusion I would argue that the problem is not theory in educational research, but rather the removal of theory from practical work and recommendations.

Filling the lacuna left by academic research in the English case is the monopolisation of the policy arena by voluntary sector, think tank and philanthropic organisations. This is very notable anecdotally: audience members at the launch event for Stephen Ball and Carolina Junemann's research findings (2010) noted a scarcity of academic representation at key policy events, an observation supported by my own impressions. Ball and Junemann's research provides the evidence to support such impressions. Their ESRC-funded project "New Philanthropy, Education Policy and the State" examined the 'new philanthropy' in state education. As part

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3 Free Schools are schools which can be set up by groups such as parents, charities and religious groups to receive state funding, but outwith local authority control; Academies are existing state-funded schools which can achieve a high degree of autonomy, again outwith local authority control

of their analysis Ball and Junemann present a series of 'network maps' which depict the riot of charities, private companies, think tanks, educational bodies and key individuals involved in 'new' educational philanthropy, and the ways in which they are connected via networks, partnerships, memberships and associations. What these vividly illustrate is the dominance of key corporations and individuals across these maps. The maps also depict a relatively small community, in which particular names and organisations frequently recur, and are directly networked to government and other powerful agencies. But crucially, the maps also illustrate a clear set of absences, notably including the relative lack of Higher Education Institutions (HEIs) and academic figures represented on these network maps.

Philanthropy and third and private sector provision of services previously provided by the state are at the heart of English Coalition policies (and were to a lesser, but still strong, extent espoused by the previous New Labour government). Of course, the private sector has long been involved in education via for example the sale of educational materials, facilities, and qualifications to schools. However, it is certainly the case that under the previous and current governments this involvement has been actively prioritised and facilitated in government policy. That 'new philanthropic' organisations are often backed by multi-national corporations, and that their representatives often have immediate access to ministerial ears, clearly raises important questions relating both to the boundaries between philanthropy and profit, and to accountability and the democratic process (see e.g. Ball, 2007). Yet as Ball and Junemann argued at their dissemination event (2010), no one can doubt the genuine commitment of many of the key players involved in 'new philanthropy', nor the innovative practice that some of their interventions have generated. My concern in this article is not to analyse the quality, motivation or consequences of these practices, but rather to highlight the contrast in effectiveness between the new philanthropists and education researchers/HEIs in impacting educational policy and practice, illuminated by Ball and Junemann's research.

## **DOES IT MATTER?**

I assert that as educationalists we *should* be concerned with impact (in its meaningful guise), and with the relatively scant impact our work currently appears to be making on educational policy and practice, because young people's educational needs are at stake.

Hence my argument leaves aside the motivations of the 'academic impact agenda' driven by the REF, and returns rather to more profound arguments about the role of evidence in informing educational policy. Current policies and practices, and the relative lack of involvement of academics in the development of research-informed products and materials for schools, leaves the terrain dominated by charitable and commercial organisations providing schools with often un-evidenced educational interventions. As Sebba (2010) has maintained, there are social/economic imperatives for academic educational work to make better impact, and also a imperative to ensure that service provision is informed by the best possible evidence (see also William, 2010). Of course discussions about what counts as evidence are important, but the whole notion of academic research (as distinct from lay impression or belief) is based on the concept of rigour – and it is this rigour that

is lacking in many current education initiatives, discourses and policies to which young people are subject.

### **INCREASING IMPACT**

So I see Ball and Junemann's evidence a timely prompt to academics to think harder about how to 'get their ideas out there' (Francis, 2010). Admittedly, a host of factors mitigate against such engagement. I will address some of the most obvious here.

#### **1) Policy imperviousness to academic research**

As observed above, there have been on-going complaints from academics that policymakers do not listen to them or take their work seriously, and indeed have a propensity to reject their findings outright if they do not fit with current policy. In England especially we do not have the culture of policy-academic interface that we see in some other countries (Mansell, 2010). Key pieces of high quality research and analysis that were rejected by government are often held up as exemplars of this tendency (the 2009 Cambridge Primary Review on the condition and future of primary education, and the 2004 Tomlinson Report on 14-19 Curriculum and Qualifications Reform, being notable examples). However, these very exemplars illustrate that influence and impact can be wider than simple take-up in government policy. These pieces of work have influenced thinking throughout the sector, driven debates and influenced the development of specific interventions, and indeed pedagogic and organisational practices within many education institutions.

It is important to recognise (if not appreciate) issues of timing and 'political necessity' in policymaking and related take-up or otherwise of research findings – and the serendipity and non-linearity of these processes. As Warwick Mansell observed in his talk to the Society for Educational Studies (2010), politicians cannot be seen to be content with the status quo (which is never perceived good enough by the electorate); hence they have to maintain a presentation of themselves as 'reformers', and inflict this endlessly on the education system. Hence what research does influence policy is influenced and constrained by political, economic, social and timing issues.

#### **2) Capacity issues**

A further potential impediment is the intensification of labour in HEIs (including preparation for the REF), which restricts the amount of time academics may dedicate to dissemination methods, and negatively impacts the consequent quality of such methods. The introduction of the impact measure as an aspect of the REF may provide incentives for engagement with policy and practice; however the priority remains focused on high quality research for academic consumption. Moreover, it is arguable that academics cannot be expected to have all the skills which enable production of high quality research *and* interface with multiple user groups and production of materials, events and products most likely to ensure impact. It may be that academics will need to be more pragmatic and creative in working with partners (e.g. think tanks, designers, events managers, publishers, headteachers and so on) in order to draw in these skills from other sectors (and of course, such

activities need to be costed into research funding bids). However, it is certainly the case that there are effective methods developed in the third and private sectors that aid the transfer of complex ideas to lay audiences, from which academics could learn. An example in my own organisation is RSA Animates (e.g. RSA 2010), which present edited keynote speeches given at the RSA by key thinkers and social commentators, engagingly illustrated by a comic animation: these have been immensely popular online, with the most popular having been downloaded over 10 million times.

### **3) Wariness over distortion**

Potential distortion of research findings may be a further impediment to academic engagement: many academics remain wary of the tendency for their research to be 'dumbed down' and even misrepresented by the media and other 'brokers' of ideas (such as think tanks). This is clearly a risk, and many of us that are used to engaging the media have had negative experiences in this regard. Moreover, the clash of agendas – in the case of the media, the imperative to sell newspapers, and in the case of think tanks, the imperative to sell ideas to funders and policymakers – can mean that ideas become 'lost in translation'. The media, particularly, is notoriously fixated on the salacious and controversial, meaning that often, when presenting a research project, key findings (as perceived by the researcher) are ignored in favour of minor points that meet newspaper agendas. Nevertheless, it is arguable that such coverage draws attention to research for members of the public and users of research, and enables such stakeholders to access a researcher's work for more in-depth engagement. Moreover, if we do not engage, we leave the space to be dominated by others, often those with agendas of which we may not approve. It is notable that, especially in the English education media, particular names circulate as quoted commentators, and very few academics stand among these.

Think tanks comprise another opportunity for brokerage and engagement. The stereotype of these organisations as populated by young people who have limited experience or scholarship in the policy areas in which they work, and formulating knowledge without engagement from academic community (Power, 2010), may not be entirely without foundation. However, most credible organisations do contain genuine experts, and moreover there is often a respect and hunger for academic research – but the latter can prove difficult for those in think tanks to access. It can be easy to forget that academic library collections are not easily accessible for think tank staff and policy-makers, and individual articles and so on are expensive to access. Without being embedded in the existing academic literature, these workers are often reliant on individual academics making contact and feeding in their work – and in my experience welcome such engagement.

Looking at the roles of think tanks and other 'brokering' organisations, Judy Sebba (2010) distinguishes helpfully between 'knowledge transfer and dissemination', and 'knowledge translation and research mediation'; the former implying a direct passing on, the latter implying some intervention such as summarising, interpreting, and so on. Both are useful, although arguably we as academics ought to increase our ability to undertake the 'research translation' element ourselves – which would also reduce the risk of distortion. However, I do not wish to underplay the complexity

in these relationships, or to suggest that the role of think tanks in policymaking (in England, and increasingly in the other UK nations) is unproblematic<sup>4</sup>. Sebba (2010) emphasizes the importance of social networks, and of building this element into research design in order to facilitate research impact.

In my speech to the Scottish Educational Research Association (SERA) I even went so far as to suggest some 'tips' for academics wishing to increase the impact of their findings, drawn from my recent experience in working at the RSA. While this does not seem appropriate for a journal paper, it is worth reiterating my argument that we need to be open to drawing on methods used in other sectors, and to think about ways to make our research findings more accessible to non-academic audiences. Moreover, while I have focused much of this paper on the issue of influencing education policy, we must not forget about influencing educational practice. It may be that influencing practice is both potentially easier (certainly on a local level), and more direct – a 'bottom-up' approach to impact, rather than 'top-down' via policy. This is where the engagement of practitioners becomes vital. Again, a focus has to be on provision of accessible messages and materials to facilitate practitioners to take up and apply our research.

## CONCLUSIONS

Having recently begun work in a think tank environment and observing their practices, it becomes evident in contrast how little work is being done by academics to effectively engage the media and policymakers. As Ball and Junemann's work in the English context illustrates, it is think tanks, private and voluntary sector organisations that are owning the policy-practice space in educational researchers' stead. This is without even counting the myriad, and often bizarre practice proffered to schools by individual consultants and companies. I have analysed some of the key factors, both historic and contemporary, that have worked against academic impact (and their pursuit of impact) on education policy and practice, and argued that we have a moral imperative to seek better impact for our important research.

I believe this to be a significant issue for educational research associations, which perhaps could do more in presenting excellent academic research to the outside world, and/or to support the development of their members' skills in this regard. Far more needs to be done in advocating educational research, both by research associations and individual researchers, if we are to repair the link between research and practice and re-engage the educational policy arena.

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4 As Ball and Exley (2010) identify, think tanks and their staff are highly inter-connected (sitting on one another's boards, speaking at each others' events etc), and hence they promulgate exclusivity and closedness as specific ideas and authors proliferate, and a building of ideas in particular directions. This is of course rather like academia! However, leaders of think tanks often have the more direct ear of politicians and policymakers than do academics. I remain interested in the ways in which think tanks are highly reliant on external funding, and hence, where they are supposed to present innovative ideas and 'policy solutions', in fact what is 'sayable' is highly circumscribed by the 'fit' with funder agendas and the existing political climate. This raises a number of issues in relation to democratic representation and accountability.

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