

Phil Hodkinson

**Abstract**

A common perception is that scientific principles should govern the value of social research. This paper argues that this is a dangerous oversimplification. Rather, decisions about whether research is sound are made through inherently political processes. Normally these processes are micro-political, as groups of academics struggle for the acceptance of their work and/or for the exclusion of work that they deem to be unacceptable. In recent years, in the USA and the UK, an additional macro-political dimension has been added, as Governments strive to control the types of research which they believe can be legitimately funded, within the field of education. Micro and macro political perspectives became intertwined, as academics whose views coincided with those of government supported the imposed approaches, whilst those who did not agree opposed them. The paper describes and explains some of the ways in which political processes work to determine research quality, and argues that the extent to which such processes are themselves legitimate are value judgements which are contested within the very political process of which they are part.

**Keywords**

Research quality • Micropolitics • Macropolitics • Politics of research • Legitimation

**Introduction**

As the twenty-first century progresses, the relationship between research and politics has been increasingly foregrounded, especially in the social sciences. At one level this is unsurprising. Much of the funding for such research comes from national governments, either directly or indirectly. In England, for example, there are three sources of government research funding. Firstly, the government provides a large part of the basic resources needed to run Higher Education, money that is distributed through the Higher Education Funding Council for England (HEFCE). Part of this funding is explicitly linked to research, and is allocated through a complex Research

Assessment Exercise (RAE), now Research Excellence Framework (REF). Secondly, the government pays for the largest and most prestigious research project funding scheme, through the Economic and Social Research Council (ESRC). Thirdly, the government directly commissions research. When such a government is laying out millions of pounds for research expenditure, it wants value for money. This demand brings in its wake a range of political pressures on all aspects of the conduct of research, including its legitimization.

Yet for many researchers, it is a basic tenet that research should be above politics. Hammersley (1995) argues that research can and should be value-neutral. Researchers should tell it like it is, regardless of external pressures or favours. Without this academic independence, the argument goes, research loses its central defining principle of rigorous objectivity. I share with Hammersley and others a concern about recent government interventions into the research process. However it is a fundamental mistake to pose the

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P. Hodkinson (✉)  
School of Education, University of Leeds, Leeds, UK  
e-mail: [p.m.hodkinson@leeds.ac.uk](mailto:p.m.hodkinson@leeds.ac.uk)

problem as a dichotomy between research on the one hand and politics on the other, for the legitimisation of research is an inherently political process.

In order to understand this claim it is helpful to think of two different types of politics. The first is macro-politics. By this I mean the politics of policymaking and government, at national, regional local and increasingly international scales of action. In Western democracies, this is the politics of the party systems, of elections, etc. In all states, it includes the processes of policy determination, policy enforcement, policy enactment and policy resistance. The second is micro-politics. This term was coined by Hoyle, when analysing the work of schoolteachers. He defined it as ‘the strategies by which individuals and groups in organizational contexts seek to use their resources of authority and influence to further their interests’ (Hoyle 1982, p. 88). Though he focused explicitly on micro-politics within organisations, I use the term more generally to include the day-to-day politics at the heart of the workings of academic research communities. Like macro-politics, this operates at different scales: within institutions and departments (Hoyle’s concern), but also within national and international research groupings and organisations. This micro-political activity is overlooked in calls to take politics out of research. Yet, as I will argue later, macro and micro politics are interwoven. Indeed, the very calls for the research community to mobilise in resistance to government interference are themselves both micro and macro political acts.

The dominant view of research legitimacy within academe focuses on procedural objectivity. Drawing from the physical sciences, this view is that social science research is legitimate if it is valid, reliable and generalisable. These qualities can be judged through a careful examination of the methodology used, with a particular focus on objectivity and the minimisation of bias. In addition legitimate research tells us something interesting or useful, which adds to what is already known. Given the rise of qualitative and case study research in the social sciences, many researchers modify and loosen the scope of these procedural requirements. Indeed, it is often argued that qualitative case study research can never be very reliable, because the methods cannot be repeated to demonstrate identical findings, or generalisable, because no case study can fully represent the wider population of which it is part (Gomm et al. 2000; Stake 1995). Despite these ameliorations, the prime focus in most methods texts remains on the significance of methodology in determining legitimacy, and on objectivity as central to this process. What follows is a need for agreed criteria against which a piece of research can be judged, to determine its legitimacy. Sometimes, this is referred to as the warrant for the research.

For reasons which have been widely articulated in the research methods literature, this broad stance towards

research legitimacy has been under prolonged and repeated attack. Many of the issues in this attack have been dealt with elsewhere in this volume, particularly in Chap. 50 by John K. Smith. My own position is set out in Smith and Hodkinson (2005). The core argument of these attacks is that research objectivity is impossible, because researchers can never completely separate themselves from the objects of their study. It follows that if objectivity is unachievable, then criteria for judging research legitimacy based upon methodological objectivity are not appropriate. As Schwandt (1996) argues, we need to move beyond criteriology. Of course, the position adopted by Smith, Schwandt and others is far from universally accepted. Hammersley (1990, 2009a) for example, argues that we can and should use the same broad criteria to judge the legitimacy and worth of any research piece (See Hammersley 2009a, b; Smith and Hodkinson 2009, for a debate between these two different positions). Also there have been recent calls for social science educational research to be judged more rigidly against ‘scientific’ or positivist research procedures, with an explicit reference to generalisability (Feuer et al. 2002; National Research Council 2002).

Much of this debate about research legitimacy consists of carefully argued assertions about how research should or should not be judged. These arguments are important to the on-going strength and well being of social science research, yet there is no sign of a generally accepted resolution, despite frequent calls from some participants in the debate that such a resolution is necessary (Feuer et al. 2002). It is this failure that draws attention to the micro-political nature of research legitimisation.

Two points can be dealt with quickly. Firstly, even those who argue that we should have a broadly agreed set of criteria to judge social research each produce different lists of what those actual criteria should be. This is partly because, as Smith (1990) points out, no list of criteria can ever be definitive. That is, new criteria can always be added, and others taken away. This leads Sparkes (2002), for example, to describe and justify numerous different ways in which qualitative research in Sports Science can be legitimately judged. Given this proliferation of possible criteria and lists of criteria there are no agreed procedures to use in order to determine which of these many alternative lists of criteria should be adopted – either universally or in particular cases (Garratt and Hodkinson 1998).

The second point is that many of the lists produced favour one type of research over another. Thus, if criteria drawn up on the assumption that randomised controlled trials are the best and most robust research method (Oakley 2000), then much qualitative research will largely fail. Similarly, if some of the ways of judging qualitative research advanced by Sparkes (2002) were applied to such experiments, the reverse would happen, and the research based upon

randomised controlled trials could be seen as reductionist, in what many claim is an inherently complex and relational social world (Flyvbjerg 2001).

These two issues illustrate a more fundamental issue, which is that different researchers and groups of researchers hold very different views about the nature of research legitimacy, and the ways in which research can and should be judged. At least until such differences can be mutually resolved, judgements about social science research remain contested within the field, and the playing out of those judgement contests will be inherently micro-political, as different individuals and groups strive to advance their own positions. One of the results of such political disagreements is a fragmentation of the research field, where, for example, journals use very different criteria to judge the legitimacy of published research.

For researchers of a more positivist or post-positivist bent, this fragmentation and the failure to agree and adopt criteria based upon research objectivity are symptoms of the immaturity of educational and social science research. Feuer et al. (2002) argue that educational researchers in the United States must learn to pull together behind an agreed list of broad procedural principles for all research. Interestingly, they offer no explanation for the failure of the educational research community to do this already, implying that those who do not share their position and accept these principles are misguided and even self-indulgent. In calling for all educational researchers to pull together as what they term a community of practice, Feuer et al. (2002) clearly believe that it is purely a matter of choice that many do not already do so.

One of the many ironical paradoxes that are found within research legitimization debates centres upon this claim. Feuer et al. (2002) see research as establishing facts upon which educational policy and practice should be based. Yet in relation to their own argument, they seem unable to either accept or deal with the fact that very many serious and thoughtful researchers take divergently differing approaches to the issue of research legitimization. That is, not only are very many eminent educational researchers unwilling to unite behind Feuer et al.'s principles, but also they are unable to do so without fundamentally changing their deeply held beliefs about research. Even relatively slight differences of view on these matters can be impossible to resolve. Martyn Hammersley and John K. Smith have been arguing for years (see earlier references), without either being able to convince the other.

The on-going failure to resolve these sorts of argument originate in the complexities of the legitimization debate in relation to social science, as outlined by Smith elsewhere in this book. There are many different positions taken and defended, and the protagonists each believe that their own position is superior to the others. The nature of the debate, which normally takes place within the academic discourse of

logical argument, conceals an important underlying truth. When researchers adopt particular positions in relation to research and research legitimization, those positions often become a key part of their professional identity as researchers. One way to understand this claim is through Bourdieu's concept of habitus.

For Bourdieu (Bourdieu and Wacquant 1992), a person's habitus is a battery of dispositions, which orientate us towards all aspects of life and the world in which we live. These dispositions are partly discursive and cognitive, but are also embodied and partly tacit. That is, they also have physical, practical, affective and emotional dimensions. The dispositions which make up the habitus are developed throughout our lives, arising from our social and historical positions in the world, and our ongoing experiences living in the world. Amongst other things, these dispositions show the ways in which social structures act within and through the person. These dispositions are enduring, but can and do change – often gradually and imperceptibly, occasionally rapidly and dramatically, for example through what Denzin (1989) terms an epiphany. A person's dispositions will strongly influence their feelings, thoughts and actions in any situation they find themselves in. Those dispositions make some thoughts and actions easy, others difficult, others impossible. Followers of Bourdieu often examine the significance of the habitus in the lives of their research subjects. The argument can be equally applied to the researchers themselves, and the ways they think about and conduct their research.

For example, my own relativist position in relation to research legitimization has developed over many years. Whenever I read something on research methodology, I can only do so from within previously established dispositions. These dispositions may be strong and well formed, or weak and provisional. When I engage with that literature it may cause me to rethink aspects of my prior beliefs and ideas, and my dispositions towards research legitimization may change. Alternatively, the engagement may reinforce and strengthen established dispositions. When I read something that challenges my existing beliefs, a range of reactions is possible, including the need to rethink my position more carefully, in order to clarify why I do not agree with the challenge. As my own research career has progressed, my thinking about research legitimization has modified and gradually firmed up and strengthened. My views may continue to change, but it becomes less likely that I will encounter arguments that I have not met before.

This process of dispositional development does not take place simply through reading, but also through the conduct of research, through formal and informal conversations and through writing and publication. As my beliefs strengthened, they became a central part of my researcher identity. My allegiance to them became more than intellectual. Attacks on

my beliefs, even when not directed at me personally, have an emotional impact. When reading such an attack, my research identity feels under threat.

Other researchers can be as deeply committed to their positions as I am to mine. The commitment centres upon a rational belief in the logic of the arguments within a deeply held position, but always involve more than that. This is why Feuer et al. (2002) were being naïve in calling for all researchers to unite around an agreed position. Deeply held dispositions do not change easily. One might as well ask all committed socialists to become right wing free market thinkers, overnight.

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## Research and Micro-Politics

Thus far, I have considered issues of research legitimation from two perspectives. I have challenged the common assertion that such issues are simply a matter of research technicality, and advanced the alternative view that individual researcher dispositions and identities are important. Now it is necessary to consider the social dimension, for individual researchers are part of wider professional and academic groups and communities. As researchers, we seek out others whose interests are similar to our own. Often, this centres around those who research the same issues or topics as us. We need to ground our work in theirs, and we want them to recognise and value the contributions we are making. Such groupings often develop formalised structures, such as specialist journals and conferences, or special interest groups within larger research associations. Sometimes, researchers group together around more macro-political purposes, such as the advancement of a feminist agenda, or a drive to achieve better treatment for minority ethnic groups, or those defined as having ‘special educational needs’. There are similar associations around particular methodologies. For example, groups of expert researchers regularly share their expertise and interests in using the British Household Panel Survey (BHPS). This includes sharing new statistical techniques for analysing the data generated by the survey. Also, there are journals and research groups focussed on life history and auto-ethnography as methodological approaches, and several international journals are aimed explicitly at qualitative research.

Thus, debates about research legitimation involve groups and organisations, not just individuals. The result has been an on-going struggle around this issue for at least 20 years, since the first paradigm wars (Gage 1989). Individually and collectively, researchers strive for influence in the research field to which they belong. This micro-political striving is a normal and essential part of academic life. If we believe what we write, we should want to convince others of our arguments, for the benefit of the field as a whole. However,

the struggles involve more than the exchange of knowledge and ideas, and have a direct bearing on access to scarce resources, such as research project funding and space for publication in prestige outlets. As Bourdieu (1988) argued, academics are striving for distinction on the fields within which they work. That distinction is partly a matter of personal pride, but also of job security (getting tenure, avoiding redundancy), promotion and pay. Academe is a highly hierarchical and competitive field, where the players of the game develop detailed knowledge of the numerous and often subtle signifiers of status and influence, for which they strive. Of course, not all academics strive for all forms of academic distinction. Some are more ambitious and competitive than others, and may value one form of distinction whilst others value a different form. Some strive to become professors, some strive for senior administrative positions, some strive for prestigious publications, for journal editorships, or for research income. Some strive to be good teachers and colleagues, some to use research and teaching to help others.

As Bourdieu’s writing makes clear, within any field, including academe, these competitive strivings for distinction are much more than individual struggles and are inherently unequal (Bourdieu and Wacquant 1992). Groups and individuals strive for success from very different and unequal positions, and with very different and unequal resources – what Bourdieu termed economic, social, cultural and symbolic capital. These unequal micro-political struggles concern the quest for distinction, but also the achievement of influence over what counts as distinction itself. In practice, these two aspects of the struggle are interwoven. Thus, one way of achieving distinction is through publication in a prestigious journal. One way of succeeding in this ambition is to gain influence over the criteria used by such journals to determine what is published and what is not – for example through joining the editorial board or becoming editor. At a more mundane level, academics search for prestigious journals that already like the sorts of things that they write, and either directly or indirectly strive to maintain or raise the status of the journals they use. Sometimes we change the ways in which we write in order to get things accepted in journals whose criteria and conventions differ from those we centrally support and believe in. The other side to this striving for publication, research money or promotion is the equally important striving to exclude from funding, publication and promotion work or, in the last case, people whose work we believe is sub-standard. Such refereeing advice and editorial, funding committee or promotion committee decisions are centrally concerned with struggles to establish and/or maintain particular views of distinction. The supervision and examination of research degree work entails the same struggle and purposes. I have often come across examples of research students being forbidden by the supervisors from doing things

which I would regard as appropriate, and the choice of an external examiner for a thesis legitimately entails identifying someone sympathetic to the approach taken by the student.

All these and other normal academic activities are essential to the functioning of the academic research field, yet they are fundamentally micro-political. These micro-political processes only become 'improper' when someone uses their influence illegitimately to gain personal advancement, the unfair advancement of a friend or protégé, or the unfair blocking of the legitimate work or advancement of another. However, the decision about what forms of activity are illegitimate itself depends upon the rules of the academic game, and these rules are, in turn, determined through the struggles within the field.

In these micro-political struggles, research legitimation plays a fundamental role, because it underpins most of the currently accepted facets of and criteria about distinction. This may be why debates about research legitimation so often generate as much heat as light, provoking very strong feelings from participants. For struggles over the principles and criteria that govern research legitimation concern not just our individual sense of professional identity, but also our individual and collective ability to succeed in the field within which we work. This being the case, arguments that such issues only concern the achievement of technical standards about which all can easily agree are at best a form of what Bourdieu termed *misrecognition*: a failure to appreciate the real nature of the micro-political processes which lie behind such technical claims. At worst, they are an ideological smokescreen, through which the powerful protect and legitimate their preferred positions. This misrecognition also works to enhance the superior status of academic knowledge in wider public and policy debates, to which I turn next.

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## Research and Macro-Politics

Pressure for a greater macro-political research engagement comes from two different directions, with opposite emphases. The first direction comes from within the research community. This is because many social science researchers wish their research to make a difference to the world that they research. In fields as diverse as health care, social services and my own specialism, education, many of us want our research findings to improve provision. This improvement is often centred upon a deeply held desire to improve the lives of less advantaged members of society, but in other cases focuses more on improving the technical effectiveness of provision. The difference between these two approaches to improvement is closely related to differences in approach to methodology and research legitimation, which I turn to later. This drive to make a difference sometimes results in direct attempts to influence policy, and/

or to bemoan the lack of attention paid to research by policy makers. It is this type of political engagement that Hammersley (1995) warns against. For him, by adopting what I term a macro-political stance, researchers surrender key aspects of their claim to expert status and, much more seriously, open the research community up to political interference from outside. The second pressure for a macro-political dimension for research comes from governments and politicians. This pressure has been evident in both England and the United States, where there has been a strongly expressed government insistence for research to make a direct contribution to improve things. Of course, improving things normally means helping those in power do better what ever it is they want to do. That is, the emphasis is on technical improvement, not on improving the lives of the disadvantaged or oppressed. Recently, there have been pressures for educational research to help the U.S. and English governments achieve their policy objectives. In both countries, this was explicitly related to a call for 'evidence-based practice' and for research to identify 'what works'.

In 2000, the Secretary of State for Education in England addressed the ESRC with a demand that:

Social science should be at the heart of policy-making. We need a revolution in relations between government and the social research community – we need social scientists to help to determine *what works and why*, and what types of policy initiatives are likely to be most effective (Blunkett 2000, cited in Evans et al. 2000, p. 1, emphasis added by them).

As I have argued elsewhere (Hodkinson 2008; see also Hammersley 2002), this government pressure for research to determine what works coincided with a drive from within the academic research community for a scientific approach to educational research, with the explicit purpose of providing robust evidence to improve policy and practice. As Thomas (2004) points out, central to this approach is a need to produce robust syntheses of the findings of many research projects, in order to produce safe generalisable guides to action. These combined pressures resulted in much political debate. Many comments by national political figures, and any linked press coverage, adopted the general stance that British educational research was too often of poor quality and/or largely irrelevant (Hammersley 2002). Within the educational research community, there were mixed reactions. There were many supporters of this new evidence-based policy and practice approach. There were also many opponents.

In 2001, the United States federal government followed a similar path to that in Britain, but in a much more draconian form. The No Child Left Behind Act legally required all researchers using government funding for educational research to adopt the methodological principles of evidence-based practice. This was an unprecedented direct

macro-political intervention into the existing struggles over research legitimisation, backing positivist (Oakley 2000) or post-positivist (Phillips and Burbules 2000) research principles. This government intervention went much further than exhortation, involving a direct legal control over one of the major sources of research funding. This intervention provided a direct and substantial threat to the careers of American educational researchers and groups of researchers who were committed to work in ways that lie outside these legal principles.

In the UK, government pressures were more subtle but important, none the less. Arguably the most significant intervention was indirect. The government has put increasing and sustained pressure on the ESRC and on the HEFCE to place increased emphasis on the value of any research they fund to what are termed 'users'. This means that user value is now a routine criterion against which all funding applications to ESRC have to be judged. This user interest was also applied to the means of judging the amounts of general research income should be awarded to social science disciplines through the Research Assessment Exercise (RAE). For example, the 2008 Panel which determines research grades in education was made up of 20 experts including four members chosen from user organisations, including some government funded and regulated educational quangos, but perhaps unsurprisingly not including teachers' Trades Unions. The outcomes of the RAE directly influence University funding and the prestige of the University and of its constituent departments. League tables are constructed by the specialist press based upon the panel evaluations, and most of not all British universities are increasingly concerned to improve their standing in those league tables.

In my terms, the English government engaged in deliberate macro-political interventions intended to influence what counts as legitimate social and educational research, and who is entitled to make those value judgments. The workings of the ESRC committee and referees who decide which project applications will be funded and which will not, and of the HEFCE subject panels which will determine the research gradings and therefore the funding and status of departments, will themselves work micro-politically. There will be strategies and struggles, alliances and conflicts.

The second British government approach was to substantially refocus research funding in education, through initiatives centred upon evidence-based approaches, but without the enforcement of legislation. They established and paid for a major research centre, the Evidence for Policy and Practice Information and Coordinating Centre (EPPI), in 2000. The centre's job was to carry out systematic reviews of educational research findings, to establish valid, reliable and generalisable findings that could then be safely used by policy makers and practitioners. As MacLure (2005) shows, the procedures adopted by EPPI in conducting these

reviews were broadly positivist, in line with the thinking of the Centre's director (Oakley 2000, 2003). MacLure also argues that the procedures are deeply flawed and the outcomes risible. Despite her attack, as the EPPI reports were progressively published, the effect was to label research that did not fit their criteria as of no value, whilst promoting and valuing studies that did fit their pattern.

Another major government-led innovation was the Teaching and Learning Research Programme (TLRP). The Programme has been allocated an unprecedentedly large sum of money to fund research projects (£43 m between 2000 and 2007). Most of that funding was not new, but came from a top-slice of the existing HEFCE grant to universities for educational research. The TLRP remit was to produce robust educational research that would directly contribute to the improvement of teaching and learning. In order to overcome perceived weaknesses in the educational research field, projects were to be large (often between £400 k and £1 m). There was an initial unwritten presumption that most would be experimental, quantitative or at least involve mixed qualitative and quantitative methods. Project funding was allocated through normal ESRC competitive bidding procedures. In line with the new conventions, the programme steering group, which made the final decisions about which projects to fund, contained significant research users, alongside eminent researchers.

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### The Interrelations Between Macro and Micro Politics

In both Britain and the USA, the macro-political interventions by government are working alongside strong micro-political efforts to reintroduce and/or reinforce positivist views on research legitimisation. I have argued elsewhere (Hodkinson 2008) that both positivism within the research community and the 'what works' policy initiatives are underpinned by the same forms of technically rational thought. In both cases, the assumption is that good social research will produce findings that are unarguably valid, reliable and generalisable; and that such research can directly lead to better social provision. The parallel is often explicitly medical – educational research should produce the equivalent of a successful treatment for asthma, and governments and educational providers can all use this treatment in their work. For many protagonists in both groups there is a further political ambition to be achieved, because the focus on the technical, in teaching or in research, brackets off social problems of deep-seated inequality or cultural diversity. Schools are 'poor' (they get low measured achievement grades) because of poor teaching, not because their intakes are deeply disadvantaged. Torrance (2008) argues that the policies currently promulgated under this

'what works' agenda invoke the need to address educational disadvantage as a key legitimating purpose. However, 'disadvantage is conceptualised (theorised) as a social problem that can be addressed by education, rather than an economic problem which might be addressed by higher wages and/or stronger employment legislation' (Torrance 2008, p. 8). What follows is that if people fail despite scientifically proven educational provision, it is their own fault.

In advancing this shared agenda, there is a valuable political alliance for the politicians and the positivists. For governments like those in Britain and the United States, the arguments of the positivists (National Research Council 2002, 2004; Oakley 2000, 2003) provide reassurance that what they desire can be achieved, and purport to show how it can be done. For the positivist researchers, the external macro-political intervention of governments enormously strengthens their micro-political position and resources in the research field. For those who do not share their views, it is as if the positivists have welcomed the use of governmental power to force through values and procedures in the field, having failed to win the rational argument.

If the introduction of government enforced views on research legitimization was both macro and micro-political, so were the reactions to it. The introduction of the TLRP was macro-political, but from the start, the micro-politics of legitimization was inherent in its operation. Though there are no published records of the workings of the steering committee, it is safe to assume that meetings entailed arguments and struggles over how competing research projects should be judged, and over which would be eventually funded. From within the educational research field, there was a micro-political dimension to the responses to the programme. As had no doubt been intended, the size of the funding on offer provided a major incentive for researchers to bid for it, and very many did. Such research funding not only gave successful applicants the resources to do research that they wanted to do, but also brought with it prestige and status. Many researchers whose research identity and research principles were at odds with the espoused stance of the TLRP, still worked to win the funding. This resulted in strategic compliance, as such researchers strove to construct proposals that would allow them to work in ways they preferred, but were worded to maximise their chances of getting funding. Surveys were added to predominantly qualitative proposals, and sometimes claims were made about impact on practice that were unlikely to be achieved.

Occasionally this micro-politics became confrontational. My first successful TLRP bid was as part of a research network looking and improving learning in the workplace. Initially, the funding for this network was conditional, because the TLRP steering group felt that we had only partly met their rigorous methodological standards. The research team responded to the conditions, and then had an informal

meeting with the original Programme Director, Charles Desforges. After this, my project was instructed to do some more methodological work, to establish ways to measure learning outcomes in the workplace, in order to establish the extent to which learning at work could be improved. This challenge resulted in a later publication about the dangers of focusing research on learning at work on measured outcomes (Hodkinson and Hodkinson 2004). Long before that happened, I had a meeting with Professor Desforges, and was informed that unless I toed the required line, he would fund the rest of the network, but without my project. There followed further micro-political activity before the network was allowed to proceed in its entirety.

Though I have my own personal views about the appropriateness of some of this pressure, my argument here does not concern the legitimacy or otherwise of the micro-political processes around the TLRP. My point is to establish that the TLRP was directly concerned with the nature of research legitimization, and that its influence and eventual effects upon the thinking and practices of research legitimization were both micro- and macro-political. This political activity was directly concerned with establishing access to resources and prestige in the research field. The eventual and on-going influences on research legitimization were the result of this micro-political activity, which in turn was influenced by the unequal positions and capital of the political players. My own position was supported by the fact that I was already a research professor of some standing, as part of a research network that contained other eminent researchers who stood together to deal with the threat to our work. I also used some of my social capital to raise the issue informally and indirectly with some members of the TLRP steering group.

Another good example of the interactions between the macro and micro politics of research legitimization concerns the recent growth of very powerful research ethics committees. Ethics Committee is the British name. In the USA they are called Institutional Review Boards (IRBs) and in Canada Research Ethics Boards (REBs). These are University organisations with the remit to prevent any unethical research operating in the institution's name. They vet all research applications made within or on behalf of the institution. At least in the UK, ethics committees are also run by other organisations – notably the regional health trusts that manage state-funded hospitals.

These ethics monitoring organisations have wide powers. If they refuse to sanction a research proposal, including a proposal by a new doctoral student, it cannot take place. They routinely require detailed changes to proposals, which sometimes radically change the original intention (Johnson 2008). As Boser (2007) points out, they exercise power as domination over researchers, and work on the assumption that researchers exercise power as domination over their research subjects. The exercise of this power by these bodies places

them right at the centre of political struggles over research legitimization. The macro-political dimension to their work arises through their place within current moves to reassert positivist research principles in social science research. In the USA, for example, Lincoln (2004) and Johnson (2008) demonstrate direct links between the ways IRBs are now working and the National Research Council (2002) report, which provided the academic underpinning to the earlier No Child Left Behind Act. When Lincoln attacks what she calls ‘the stark politicization of research and its methods’ (2004, p. 10), she means what I term macro-politics.

As ethics committees enforce positivist research approaches predominantly drawn from medical science (Boser 2007), the result is to place additional difficulties on research with people, i.e. the social sciences, and within the social sciences, upon those doing qualitative research rather than ‘safer’ large-scale surveys (Boser 2007; Johnson 2008; Patterson 2008). In the name of protecting research subjects from harm, such committees can prevent much research which qualitative researchers view as unproblematic from ever taking place. They can thus significantly harm the developing careers of qualitative researchers.

There will always be micro-politics in the workings of the committees themselves, as Johnson (2008) suggests. There is also micro-politics in the ways that researchers operate within the remits of those ethics committees. The journal *Qualitative Inquiry* has recently published several confessional tales from researchers, describing the difficulties they have faced in dealing with IRBs, and the various strategies they have had to resort to in responding to the power of the Boards. Johnson (2008) describes the need to adopt a docile role, being subservient and expressing gratitude for help, rather than confronting. She also describes how, in her case, getting ‘help’ from a Board member helped smooth the resubmission pathway, despite the fact that Johnson had not made all the changes that had been originally required. Her and other stories demonstrate that getting IRB approval is far from a technical process, though the assertion that the process is technical, with formal criteria and procedures, is a major political weapon regularly used by the Boards.

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## Conclusion

In this chapter my prime objective has been to establish that the research legitimization question is inherently political. It concerns changing balance of power in on-going struggles about the ontological and epistemological ideas that underpin research practice. The fact that debates about research legitimization are normally conducted in philosophical language using logical argument should not delude us into believing that this is all there is. I have argued that

underlying these abstract debates lie power struggles that directly and indirectly influence success and even survival within the academic community. I have further argued that, at the present time, there is a significant macro-political dimension to these struggles, which interacts with and significantly changes the power relations within the ‘normal’ ongoing micro-politics. Without in any way trying to belittle the significance of these macro-political interventions, it is a mistake to characterise the problem as the need to somehow separate research from politics. Rather, we need to find ways to fight micro and macro politically for the research approaches we value.

The micro-political processes of research legitimization are normal and essential for the healthy conduct of the field. Furthermore, given that much of the funding for social science research comes from government sources, and that governments have an interest in getting value for money, it is probably naive to argue that there is no place for macro-politics within research debates either. What is unusual and dangerous in the current situation, especially in the United States, is the direct involvement of government in the struggle over what counts as legitimate research.

For about the last 10 years, there has been a concerted and powerful positivist movement, operating at both macro and micro political levels. The purpose of this movement is to shift social science and especially educational research away from qualitative research, the place of which is assumed to be subservient at best to other more scientific forms of research (National Research Council 2002). This is happening despite a huge literature arguing that positivist social science does not work well and that qualitative case study research may be the best way forward (Flyvbjerg 2001). As a committed qualitative researcher myself, I find these positivist attacks deeply worrying and threatening, and they must be continually resisted.

I am also deeply concerned by the role played by national governments in pushing this partisan approach to research legitimization, which poses a major threat to academic freedom. In resisting these interventions, the struggle is to achieve the spaces needed to approach social science research in diverse ways. Feuer et al. (2002) call for the educational research community to combine behind a uniformly agreed and operated approach, in order to fend off government intervention. This amounts to doing what government wants voluntarily, in order to prevent compulsion. My argument here suggests the opposite. Until such time as all social science researchers freely agree to adopt a single position on research legitimacy, the survival of academic freedom depends upon sustaining diversity of approach. Only in this way can academics fend off unified and authoritarian political control. Continued robust arguments within academe about research legitimization are healthy, for



example by undermining complacency and intellectual stagnation. However, some parts of the social science research community would do well to remember the clichéd defence of democracy – ‘I fundamentally disagree with your arguments, but I will defend your right to express them’. Of course, in making this plea I am making my own micro-political intervention.

**Acknowledgement** I am grateful to John Smith and Deborah Gallagher, for their helpful comments on an earlier draft of this chapter.

### Note on Contributor

Phil Hodkinson is Emeritus Professor of Lifelong Learning at the University of Leeds, in England. He has extensive experience of conducting research, using a range of qualitative methods within an interpretive conceptual framework. He has written widely on research methodology, with a central interest in making sense of what counts as legitimate qualitative research.

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