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ORGANISATION STUDIES STREAM

A CRITICAL RESEARCH AGENDA FOR ORGANISATIONAL PERFORMANCE MEASUREMENT

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DRAFT, JULY 1999 – IN THE UNLIKELY EVENT OF QUOTING, PLEASE SPECIFY DRAFT STATUS! EXPRESSIONS OF INTEREST IN RESEARCH IN PERFORMANCE MEASUREMENT ARE WELCOMED BY THE PERFORMANCE MANAGEMENT RESEARCH UNIT.

Abstract

Managers are continually under pressure to measure the performance of their organisation, but there is little empirical evidence about the impact of measurement on performance. Furthermore, many measurement tools and techniques lack clear theoretical foundations.

This paper proposes investigations into neglected aspects of organisational performance measurement, reflecting the concern of managers and illustrating the potential impact of theoretical developments on practice. Issues about theory, epistemology and methodologies that accompany such a research agenda, are also raised.

1.   Preamble

This paper may be a bit unusual for a formal conference discussion as it concentrates neither on presenting empirical results, nor on developing theoretical explanations, but simply seeks to make the case for more of both of these. As such, it is 'work in the early stages of progress' on which feedback on all aspects of the paper will be welcome.

And as befits a primarily qualitative and subjectivist-inclined researcher, I felt it important to put the development of the proposed research agenda in context. Its origins lie as far back as the completion of my doctoral research (Holloway 1990), which had examined the operation and impact of formal approaches to performance measurement in vogue in the British National Health Service (NHS), my employer in the 1980s. My focus was the extent to which systems approaches (such as Peter Checkland's soft systems methodology and Stafford Beer's viable systems model) could assist in the design of 'better' performance measurement systems for operational control, strategic planning, and organisational change. I ended my thesis on an up-beat note, having demonstrated to my own satisfaction (and, fortunately, that of my examiners!) that careful choice of an appropriate systems methodology could contribute to at least less-dysfunctional performance measurement structures and processes, while noting that performance measurement and control were inherently problematic in terms, for example, of the stakeholder interests served.

Because it was not obvious at that time how one 'did' a PhD in 'systems', it had been necessary to develop a methodology that linked systems approaches to analysis and organisational design, with problem situations and their (often highly political) contexts. Having a natural preference for interdisciplinarity, a dislike of the functionalist and unitary assumptions present to some extent in most systems approaches, and the tail end of a Marxist streak, the aforementioned methodology included what may now be seen as a 'get out' clause. This took the form of the explicit and deliberate incorporation of

> 'ideas from outside the systems analytical approach … where these would have some important and useful parts to play. Rather than blend a systems methodology with some other analytical perspectives, opportunities were built in for explicit departures from systems thinking or practice where necessary. … Several 'free standing' devices [such as Hopwood's 'uncertainty matrix', 1980] brought additional insights and perspectives to bear on the analysis without posing problems of conflicting assumptions'. (ibid., p.428)

A similar approach today would need to include some fairly explicit discussion of issues of paradigm (in)commensurability, but in 1990 it seemed sufficient to recognise that each systems approach was underpinned by values and assumptions, and to acknowledge that mixing methodologies, bodies of theory and social science disciplines needed careful handling.

From my empirical work I noted (following Jackson, 1988, and Oliga, 1988) that 'systemic coercive' problem contexts and those dominated by major power imbalances were not well served by the emerging critical systems approaches (such as the work of Ulrich, 1987). I concluded:

'… systems approaches did not come up with serious challenges to dominant groups or interests; nor did they produce ideas for revolutionary change. In part this was because of the pragmatic requirement [for the thesis] that changes should be politically and culturally feasible and acceptable. This is not a concession to the powers that be; changes which would be emancipatory, which could increase the involvement of lower status staff, patients and the community, were actively sought, and in a number of cases they were found. There were instances where radical changes to power relations could have resolved the problems of [some of the research topics] at a stroke, but when the more critical systems approaches were considered they did not seem to provide the design capacity of the more conventional hard and soft approaches. … The limited capacity of current systems approaches to tackle political problems, conflicts of value and wider social inequalities should not be regarded as a problem so long as these issues can be tackled with political tools in the political domain.' (Holloway 1990, p.430-1)

As well as my preferences and values, I started my doctoral research with a rudimentary knowledge of organisation and social theory gained as a sociology undergraduate in the '70s, an introductory understanding of systems approaches acquired via the Open University in the early '80s, and experience of performance measurement gained as an NHS administrator during a particularly turbulent period. I ended that initial intensive phase of research with a pretty solid grounding in the systems thinking of the time, located within a growing awareness of theories about management and public policy, and a strong appreciation of the impact of politics (with both a large and small 'p') on organisational performance. Plus a number of nagging doubts about unfinished (research) business concerning the role of performance measurement within unequal power structures and the limitations of systems thinking; and a desire to explore the contribution of a wider range of 'critical' approaches some day.

This paper indicates that that day has at last arrived, having spent ten years in academia mainly teaching (and learning from) managers about performance measurement and undertaking relatively ad hoc research into particular performance improvement practices. New research projects for myself and colleagues need to be designed now, and an input from fellow researchers as well as the users of such research will be welcome. To a considerable extent taking up where the PhD left off, I accept that organisational performance measurement, and the broader field of performance management (which is too wide a focus for this paper!) are part of everyday management life and here to stay. Experience has shown that they continue to be problematic in terms of diagnosis of performance measurement 'problems', and the design and implementation of robust, user-friendly and equitable – even possibly emancipatory – performance measurement systems. The current quest therefore is to resume the critical approach to research in this area, taking 'critical' to be wider than, but including, critical theory; 'critical' in the sense used by Spaull (1997) to mean:

'… being able to make a holistic judgement on the probity of [working or management practices]; a judgement which considers consumers and shareholders, and the wider social fabric to which the work in question contributes.' (p.343)

Thus I'm back to the 'rudimentary knowledge' stage, as the agenda I am developing includes a number of bodies of theory which are relatively new to me, together with some old friends such as a bit of systems and contingency theory. Issues such as the extent to which a pluralist agenda can be defended, and the role of theories (competing, complementary, or developmental) need early attention. The main purpose of this paper is therefore to elicit support, critique, pointers and warnings, to help make the agenda worthwhile.

2.   Introduction

The measurement of organisational performance appears to have been undergoing a welcome broadening during the 1990s, away from the traditional dominance of financial performance measures criticised by writers such as Eccles (1991) and Grasso et al (1995), and towards the use of 'portfolio' approaches such as the Balanced Scorecard (Kaplan and Norton 1992, 1996) and European Model for total quality management or Business Excellence Model (EFQM 1993). Activities such as

benchmarking and environmental audit concentrate attention on causal relationships and can reveal unintended effects of processes which may be damaging to organisational effectiveness or efficiency.

Potentially at least, such approaches address the interests of a number of stakeholders in the performance of organisations, beyond the owners or controllers (see for example Wheeler and Sillanpaa, 1997, and Kanter and Summers, 1987). These multidimensional approaches are also readily adapted for use by not-for-profit organisations. They should (in theory) assist organisational decision makers to take a longer term and more holistic view of the impact of the organisation within its economic, social and physical environment, and help to reveal interdependencies affecting managers' abilities to control and improve organisational performance. Information systems architectures are also both more powerful and potentially accessible to non-specialist managers. Thus the scene is set to achieve at least parts of Eccles' 'performance measurement manifesto' (op. cit.), one could reasonably assume.

In practice, while managers are bombarded with literature about 'successful' applications of such approaches, gaining the promised benefits is not guaranteed simply by following their promoters' prescriptions. Choosing appropriate approaches to performance measurement for the needs of the organisation, implementing them systematically, and evaluating their impacts internally and externally, are three complex sets of processes which managers have to grapple with while being under increasing pressure to actually deliver 'optimum performance' now. There are also tensions implicit in the drive for 'better' performance measurement: does the aim to empower staff, and provide greater autonomy and discretion, enable knowledge and creativity to be managed more effectively? Or is the main aim still effective command and control, adapting information and communication systems to reflect new flatter organisational structures and more complex supply chains? (See for example the critiques by Ezzamel et al. 1994, and Willmott 1993.)

Even within the same management discipline this sort of dichotomy frequently surfaces. Take for instance operational research (OR): for some, the quest for 'complete control' is still apparently a driving force for research, while for others the goal is emancipation. This diversity is neatly encapsulated in the first issue of Journal of the Operational Research Society for 1999, which opens with Cooper's 1997 Blackett Memorial Lecture entitled 'Operational research / management science: where it's been. Where it should be going?' (Cooper 1999) in which (when expounding the potential for data envelopment analysis to have a real impact on health care practice) he writes approvingly about Fetter's work on the use of diagnosis related groups:

> 'Once one has a means of measuring performance, one can develop a system for understanding, predicting and ultimately controlling the process of production (of even so complex a phenomenon as medical services)'. (p.9).

An unholy alliance of OR with 'disciplines like psychology and sociology, economics and accounting' is proposed by Cooper in order to enrich, expand and promote the use of DEA. In the same issue of JORS we see the opposite end of the OR spectrum, with two (quite distinctively different) calls for interdisciplinarity and pluralism within management science from Mike Jackson and Ion Georgiou and responses (to the latter) from Taket, Flood and Valverde (the usual suspects…) debating in complex (or possibly even convoluted) ways the emancipatory potential of OR and MS. What is the practising manager to make of these options when perhaps considering how operational research might be of practical use in improving performance?

Perhaps as a consequence of these confusing messages, in Western economies at least, every few years 'new' approaches to performance measurement and improvement which on the face of it have at least some sensible and desirable features, are abandoned before they could reasonably have been expected to bear fruit, in favour of the latest 'fad' or panacea. Total quality management (TQM), Business process re-engineering (BPR, Hammer and Champy 1993), Management by objectives (MBO), '70s style corporate planning all spring to mind. Considerable human and financial resources are committed to such approaches, and the cumulative impact of adopting the 'flavour of the month' tends to be a growing cynicism among managers as well as staff about any new approach as initiative fatigue sets in. Yet it is managers themselves who lead or collude with such fickle behaviour, creating in some organisations a vicious circle where no new approach could be expected to succeed in improving performance, simply because it is new or 'not invented here'.

Perhaps it is unfair to blame the victims. After all, they are not responsible for creating the 'guru' industry, some shortcomings of which are summed up by Huczynski (1992) – particularly the lack of critique and examination of failures, and dismissal of theoretical ideas. Indeed academia is at least partly responsible for the creation of 'guru-dom', playing a significant role in the creation of management 'fashions' (Abrahamson 1996, Kieser 1997) while neglecting to subject the fashion-setting process to scrutiny either for the purposes of critique or development. Indeed the temptation to respond to the market and provide more blueprints or prescriptions seems particularly strong in the performance measurement area. A more critical management education has been advocated by a number or writers recently but will take a long time to be realised.

The aim of this paper is to redress the balance some way in favour of clarity, critique, rigour and theory-development. It seeks to identify some priorities for improving our understanding of 'what is going on in the field of performance measurement' (with its likely impact on performance *management*), to explore to what extent and in what ways this is problematic (and for whom), and to suggest ways to inject a touch of theory into a field where fellow academics are justified in crying 'atheoretical!' in a disapproving tone! It would also be desirable for the research outcomes to be accessible to practitioners of many kinds.

The term 'research agenda' has been used as shorthand for 'a collection of research topics which may usefully be undertaken in these and related areas, which has been agreed to be of concern and perhaps can be placed in some sort of priority order'. ('Any other business' may catch ideas and issues that arise during the pre-planned discussions.) The assumptions made about the nature of such a research agenda are that it will:

- be multidisciplinary and interdisciplinary (or even 'transdisciplinary', to use Starkey and Tranfield's term, once this has been satisfactorily defined).

- be pluralist and inclusive (notwithstanding the risk of 'theoretical promiscuity' of which Sotirin and Tyrell accuse Alvesson and Willmott)

- involve multiple methods and methodologies (although not necessarily always as self-consciously as in Holloway (1990) or Mingers and Brocklesby's 'multimethodology' (1997))

- ultimately contribute to advancing ways in which emancipation and performance measurement / control might coexist constructively (probably unduly idealistic!); or to a better-informed assessment of the extent and nature of their incompatibility.

All of these characteristics are up for critique and logically may not be able to co-exist in individual projects (and see a selection of pertinent articles in British Journal of Management Vol. 8, no. 1). Nonetheless the overriding aim is to find ways to subject performance measurement to far more critical scrutiny in constructive ways.

That there is a market for such research is clear. Writers such as Otley et al. (1995) and Taylor (1998) stress the need for greater breadth and depth in research related to management control and TQM implementation respectively. And alongside the annual parade of consultant hiring-fairs masquerading as 'strategic performance measurement conferences', there have now been several more academic fora seeking to problematise performance measurement. For example, a workshop run by Robert Dyson and Jonathan Rosenhead (professors of operational research) in April 1997 brought some quite diverse researchers together. The group coped with multidisciplinarity well enough to arrive at a consensus on: the need for better understanding (in public and private sector contexts) of where performance measurement was required; the impact of methods of measuring; the relationship between performance measurement and process improvement; and the nature and needs of stakeholders.

Somewhat ironically, even as the Critical Management Studies conference is taking place, a symposium on 'The theory of measurement' is being run in Cambridge. It is likely to have an operations management slant, and aims to 'explore and review the academic theory underpinning the topic of performance measurement; establish and understand the current state of industrial practice with regard to performance measurement; and to debate the research agenda in the field of performance measurement'. It is probably safe to assume that after mid-July 1999 there will still be some under-explored theories and unknown practices! But it will be interesting to see how their proposed agenda compares to that in this paper, and to the outcomes of Dyson and Rosenhead's event. At the same time it is to be hoped that the current agenda will complement and be improved by the questions posed in

the initial call for papers for this Organization Studies stream such as 'do critical organisation studies have anything to say to people in their everyday lives?'.

Of course one might rightly ask 'do we need another performance measurement research agenda then?', and the answer may well be 'not really' or 'perhaps, but it has proved too difficult to implement up till now'. But without a review and synthesis of issues in the field from time to time one would not be able to judge! The next section provides a few examples from managers of the need for greater attention to be given to 'what, why, how, who for and to what effect' questions about performance measurement, together with a brief overview of some recent efforts to take a more rigorous, impartial and (dare one say) academic approach to such questions. Then in section four the need for theory in this field is discussed together with suggestions about the potential impact of theoretical developments on practice. Examples of practical ways of operationalising research in this field are given in section five, and in the final part some agenda items are proposed and issues about theory, epistemology and methodologies are raised.

3.   The 'problem' of performance measurement

What problems do managers *really* have to grapple with in the name of 'performance measurement'? A quick run down of the challenges being faced by two groups of MBA students, at residential school for the OUBS elective on performance measurement and evaluation in 1998, produced the following concerns, among others:

> Individual efforts which worked against team goals
>
> Negative handling of a difficult performance issue by senior managers
>
> How to develop a 'customer focus' in a military service
>
> How to set measurable standards for 'customer focus'
>
> What to do with qualitative data from service users about their experiences
>
> How to develop systems in a charity for acting on the (competing) concerns of the members
>
> How to find out why a magazine publisher's consumers don't buy titles or renew subscriptions
>
> How to improve turnaround times in responding to enquiries and orders
>
> How to get the right level of accuracy in measurement to compare with specifications
>
> How to meet legislative as well as (potentially conflicting) customer requirements.

All of these problems relate directly to organisational performance and its measurement; and as can readily be seen, they are just as likely to be 'soft' messy problems as ones to do with techniques and procedures. When they meet on such occasions, managers from very diverse organisations can frequently relate to issues in common (and often help their peers to make headway with them). These unplanned discussions probably generate as many useful reflections as formal participation in benchmarking clubs can! Is performance measurement particularly problematic, then? Or is it just another part of managerial work that can be accomplished sooner or later?

Performance measurement activities have always been part of the operation of management control, but the extension of their impact from the shop floor to all levels and types of organisation has been most marked from the 1970s in the West. Most managers are now self-conscious about their own performance and how it is associated with corporate level successes and failures; and approaches such as TQM, BPR and to some extent environmental performance management have ensured that employees at all levels are made aware of targets to be met beyond their own immediate achievements. Public services have been at least as caught up in this 'wave of performance measurement' as the private sector, (see for example McKevitt and Lawton 1996, Brignall and Modell 1999, Pollitt 1986 and Smith 1995), and the voluntary sector are now catching up (Paton and Payne 1997). The main untapped home market for the performance measurement proselytisers seems to be SMEs, whose managers 'lag' in terms of awareness and use of approaches like benchmarking (Holloway *et al.* 1999)

and quality assurance systems (Small Business Research Trust 1992). However it is highly likely that they will be sufficiently resource-constrained to remain relatively immune from this and other management 'fads' for some time yet.

We opened this section with specific examples of 'real world' performance measurement problems owned by individual managers studying the MBA course B889 *Performance measurement and evaluation* (studied by about 3000 practising managers since 1994). To generalise further from the experiences of such managers, we find that:

- the priority areas of strategic importance to the organisation to target for performance measurement systems may be contested

- selecting relevant and valid approaches which are also culturally and politically acceptable to the organisation can be problematic

- the provision of resources for systematic implementation can be resisted from above and below

- what works well in some organisations may fail to deliver in apparently similar ones

- the evaluation of performance measurement activities is often limited in scope because the causal links between performance measurement and performance improvement may be unclear – indeed causal relationships across the whole of the organisation's operations are rarely fully understood!

Individual managers encountered through the above course – who include managers from the former command economies of Eastern Europe and from the Horn of Africa – are increasingly sensitive to such issues, and have to grapple with both technical and 'softer' performance measurement problems as part of their day jobs. They appear to have a strong appreciation of the complexities of performance indicator design and use, and the challenges of measuring strategic performance (as discussed by Smith, 1995 and Chakravarthy, 1986 respectively). How far such appreciation is translated into action affecting whole organisations or sub-organisational units is, however, something of an unknown quantity although managers' reports of their efforts at the systematic design and implementation of effective performance measurement systems can provide some important insights. This is potentially a very fruitful area for future research.

Where it exists, much of the critical focus has been on the narrower area of management control (see Chua, Lowe and Puxty 1989, Vagneur *et al.* 1995, Berry *et al*. 1995, each of which are very useful texts in their own right). Another popular target is performance indicators (PIs) especially in the public sector, starting with the work of Pollitt and others in the early 1980s (inspiring the present author's doctoral research) and continuing to the present day with critiques of PIs as used in hospital waiting lists, airports and school and HE league tables (e.g. Lane *et al.* 1998, and the THES headline of 2 July 1999: 'Flawed targets damage access'). There has also tended to be a greater emphasis on the shortcomings of performance measurement in public services than in the commercial sector (such as McKevitt and Lawton 1996) partly because the impact of government policies on public service activities is relatively transparent and a wider range stakeholders have been prepared to voice concerns.

As each 'new' approach is promoted by consultants or (occasionally) academics, and adopted by more or less willing managers, there are usually some murmurings of dissent. Most often they relate to the 'faddish' motivation for the change, or the resource requirements with uncertain returns, or 'initiative fatigue'. Popular and accessible studies still appear to be aimed primarily at promoting performance improvement fashions, although perhaps in a style appealing to a new, more sceptical, audience (recent examples being Porter et al. 1998; Oakland 1999, Ghobadian et al. 1998, all of which draw on some empirical work and occasionally point out problems with performance management but could not be said to contain a critique).

Occasionally managers (perhaps especially those on Masters programmes!) as well as academics voice suspicions about the atheoretical nature of TQM, BPR, the Balanced Scorecard and the like – what are their underpinning assumptions, and why should they 'work' here? More challenging questions about the interests served by performance measurement, and the potential for unintended impacts on behaviour, surface relatively rarely even among the academic community. The examples which follow of recent empirical work (a somewhat randomly-acquired selection, it must be admitted), do mostly have a critical side to them and/or attempt theory development or testing, but even with these works the primary objective was rarely a fundamental critique.

- On quality assurance standard ISO 9000: Seddon 1997

- On TQM: Choi and Behling 1997, Neergaard 1995, Wilkinson and Willmott 1995, Taylor 1998

- On quality circles: Fabi 1992

- On the balanced scorecard: Dinesh and Palmer 1998, Broadbent and Cullen 1999

- On self-assessment models for quality management: Van der Wiele *et al*. 1996

- On environmental management systems: Wycherley 1995

- On benchmarking: Holloway *et al*. 1999

- On service industry performance: Fitzgerald and Moon 1996, Wright 1998

- On BPR: Davenport and Stoddard 1994, Allender 1994, Hammer 1995, Ackermann *et al*. 1999

- On shareholder value, Francis and Spencer (forthcoming, references available from the author)

Of course details of truly critical or theoretically-rich works will always be welcome by the author! Sadly, not all efforts to inject theoretical content into research on performance improvement techniques such as benchmarking are very effective. For example in their efforts to promote the potential use of Cox's 'critical asset theory' (which seems to be premised on assumptions of optimisation and perfect information), Cox and Thomson (1998) rightly point out the 'dearth of theoretical rigour' in the benchmarking literature and the need for awareness of contingency variables. However they then proceed to ignore a number of key features of what one might term 'ideal type' benchmarking (for example the essential focus on learning from comparable processes rather than necessarily similar firms, making it suitable for not-for-profit organisations as well as those seeking market dominance). They also pay little or no attention to current empirical evidence about its use, and condemn academics and consultants for promoting inappropriate fads while presenting a very misleading picture and universal prescription for 'success' themselves (and interestingly do not mention resource-based theories of the firm which would appear to address a number of their concerns).

Patently, many organisations do gain a great deal from performance measurement and improvement approaches, not only in terms of increased shareholder value or its equivalent, but also often in terms of staff and customer satisfaction and impact on the wider environment. Yet others do not, and may experience negative rather than neutral effects. Examples given in Holloway (1990) have many equivalents today. What are in short supply are thorough empirical studies of these varying experiences, particularly in terms of organisational processes and relationships beyond the mere technicalities of implementation; and theoretical explanations for such experiences. Is there something special about the approaches adopted by 'successful' users, or the way they implement the chosen approaches, or their organisational contexts or structures? What are the relationships between performance measurement and performance management? Is it possible to empower or emancipate workers (and managers?) while developing increasingly sophisticated ways of measuring the impact of their actions? Indeed, how can 'performance measurement' and 'performance management' be fully understood within the complex social context of organisational life?

It would seem that to develop a more systematic critique, a major part of the research agenda will involve looking for theoretical insights indirectly, from applications to other aspects of management and from original sources. Some candidate theories and theory-based works are identified in the next section, after which an attempt at sketching out a methodological framework is also proposed.

4. Understanding performance measurement experiences: potentially useful theories

4.1 Context

At this point it is worth reiterating that we are interested in performance measurement not just in its own right, but because it is a key part of performance *management* which is about enacting control on a

more comprehensive scale, as such involving and impacting on multiple stakeholders. As indicated at the start of this paper, an early concern of the author was to focus particularly on critical theories (with a large 'c') and associated methodologies through which the coercive potential of 'command and control' performance measurement could be counterbalanced or at least exposed. Over time the perceived role for theory has broadened in recognition of the potential contribution which a range of theoretical perspectives might bring to a (small 'c') critical examination of this area of management activity. It is not possible to address methodological developments in this paper but some links will be made where appropriate.

One of the major problems with conventional performance measurement is the ease with which organisational wholes are carved up, and their interactions with their environments cease to be of interest as management functions or academic disciplines devise measures (and associated targets) for their own territory. Improving our understanding of performance measurement by applying (for example) accounting, marketing or human resource theories more rigorously and actively may well have some merits, but risks substituting 'reductionism by theory' for reductionist practice. Stepping back and viewing organisational performance through more general lenses such as contingency, systems and critical theories – explanations which seem largely free of any functional origins / connections – would seem desirable. This does not preclude applying theory to functional spheres, or any other sub-systems or elements (as with Power and Laughlin 1992) of course.

There are also bodies of theory which, while relevant to performance measurement, can probably contribute little to emancipatory change or critical appreciation; and some overly-eclectic frameworks which harbour conflicting theoretical assumptions that are hard to reconcile (see for example Jackson's critique of pluralist models in management science, 1999, and Probert 1995). Suffice it to say that topics for inclusion in the proposed research agenda will need to be carefully chosen if it is to be distinctive, creative and coherent.

Time does not permit an extensive review of existing efforts or potential opportunities to bring theory to bear on performance measurement practice (and in any event this paper is not based upon a particularly systematic literature search!). Instead a small number of examples of theoretically-informed work that is directly pertinent to performance measurement are introduced, together with some suggestions for theories that deserve consideration in future work. This approach is consistent with that of Pfeffer (1997) although because he appears to focus primarily on survival or competitive success as the main definitions of 'good' performance his choice of major theories is necessarily different (apart from structural contingency). As the aim of the paper is to identify agenda items for research, the next section provides suggestions for a framework (analogous to 'standing orders' perhaps) so that if such research is undertaken as part of an organised programme of work, there will be some common elements of methodological rigour and theoretical relevance as well as a logical relationship to the purpose of the research.

4.2 Focal theories

Three bodies of theory which, it is recommended, a research agenda will encompass, are discussed briefly next: systems theory, contingency theory and critical theory. As readers are no doubt aware, each of these have somewhat indeterminate boundaries but a flavour of their potential contributions (and limitations) may be gained from the references below.

Systems theories

Systems theories have at their core a concern with 'organised wholes', with boundaries that separate them from their environment and internal divisions into sub-systems and elements. The nature and behaviour of the system are unlikely to be predictable from a reductionist examination of the parts, as the system and its parts are changed if parts are added or taken away (the whole being greater than the sum of the parts). Over time systems will exhibit emergent properties which again may not be possible to predict. We can talk of systems 'existing' in both society and nature, but defining them and drawing boundaries generally requires human intellect and interest.

It is probably safe to say that beyond these generalities, the various 'schools of thought' sometimes have little in common. For example there are subjectivist approaches (systems being regarded purely as social / human constructs that are defined to suit current purposes, as in soft systems methodology) and objectivist views (systems being 'out there', having a purpose of their own, or perhaps determined by environmental or natural forces whether or not they involve humans; see for example Probert 1997). Early use of systems ideas can be found in biology, engineering and organisation studies (general systems theory, cybernetics, socio-technical systems theory).

Since the '70s in the UK at least, more emphasis seems to have been placed on methodologies than theories (e.g. Checkland's soft systems methodology, the Open University's hard systems methodology and systems intervention strategy, Beer's viable systems model, Flood and Jackson's total systems intervention), at least in part reflecting the growth of consultancy work and problem-solving. Nonetheless there is anecdotal evidence that recent relatively 'popular' works by writers such as Luhmann and Senge may be more familiar to managers than the products of systems departments in British universities, suggesting problems of accessibility, perceived relevance or both.

Gradually critical systems approaches have been developed to assist practitioners with emancipatory interventions or to aid understanding of coercive situations, mostly based around the work of Habermas. Midgley (1995) provides a helpful overview; and see for example, the critical systems heuristics of Ulrich (1997), Flood and Jackson (1991) and Romm (1995) who identifies tensions between the theory and practice of critical systems heuristics. Habermas is not the sole critical source, however, as Thomas and Lockett (1991) provide a rare reference to the contribution of Marxism to systems action research.

Sadly a lot of the theoretical material in this field is far less accessible particularly to lay people than 'off-the-peg problem solving methodologies' promoted increasingly in (aspiring) popular management texts. Closely-related disciplines such as operational research and management science, which may be familiar to more managers, now also contain 'soft' and 'critical' wings (accepted somewhat grudgingly by traditionalists!) but again their potential role in management practices such as performance measurement are relatively under-exploited. To put this relationship in context, a thoughtful article by Midgley (1998) in the Journal of the Operational Research Society provides some useful views on the relationship between theory and practice in OR (with reference to critical systems thinking and critical theory).

Systems thinking has long been associated with functionalism, unsurprisingly given the emphasis in much of the earlier literature on systems as entities operating to achieve a common purpose. (See for example Scott 1974, Burrell and Morgan 1979). More critically-oriented systems writers would argue that this is an illogical conjunction, perhaps owing too much to biological analogies; or that even if those involved in a system such as an organisation that can be described in systems terms happen to have some common interests, these may involve oppression / power imbalances / conflict. Systemic interconnectedness need not equate to harmony! Jackson (1999) has helpfully teased out emancipatory, interpretive and emancipatory categories of systems methodologies and made some links to their theoretical origins.

Accessible descriptions of the basics of systems theory can be found in Hatch (1997), Open University texts for undergraduate courses in systems, and the work of authors such as Wilson (1984), and Checkland (1981, and with Scholes, 1990). Dent and Ezzamel (1987) provide a discussion of cybernetic systems theories particularly pertinent to management control and management accounting, and hence to performance measurement. And a flavour of trends in systems thinking can be gained from the proceedings of bi-ennial conferences of the UK Systems Society (e.g. Ellis et al. 1995, Stowell et al. 1997).

Viewing organisations as systems can be a very logical and productive starting point for understanding the nature and origins of, and constraints on, organisational performance, as Holloway (1990) demonstrated. The continuing toll of performance measurement horror stories however reminds us that notions of holism, emergence, boundaries and synergy may not be part of the everyday experience of designers of performance measurement 'systems'! It is suggested that systemic explorations of performance measurement systems (especially successes and failures as viewed by diverse stakeholders), and collaborative efforts to develop critical systems approaches which are accessible to

managers and policy-makers when designing such systems, could usefully be included on the research agenda.

Contingency theories

Systems and contingency 'theories' have an intrinsic affinity with each other, it could be argued. As with systems, it is common to find references to a contingency 'approach' or 'view' rather than 'theory', and it is far easier to recognise a 'contingency approach' than to find the body of 'contingency theory' in one place on a library shelf. (It is not a concern of the present author to include 'development of a single contingency theory' on the research agenda, partly because this could appear to be a contradiction in terms and partly because it has already proved beyond the powers of far better scholars since Lawrence and Lorsch!) Burrell and Morgan (*op. cit.*) view contingency approaches as 'a loosely organised set of propositions which in principle are committed to an open systems view of organisation, which are committed to some form of multivariate analysis of the relationship between key organisational variables as a basis of organisational analysis, and which endorse the view that there are no universally valid rules of organisation and management' (p.167).

The contingency view can be described as a modernist view of the organisation-environment relationship. There are numerous examples of contingency approaches especially in the core studies of organisational structures of the 1950s and '60s by Lawrence and Lorsch, the Aston Group and the Tavistock Institute. Burrell and Morgan (*op. cit.*) and Morgan (1986) provide sound and constructive overviews; Lewis and Holloway (1996) introduce MBA students to contingency approaches to management control; and Dent and Ezzamel (*op. cit.*) usefully review the contribution of such approaches to management accounting. Euske *et al.* (1993) report on an international study of environmental influences on performance management, and Sitkin *et al.* (1994) adopt a contingency perspective on benchmarking.

For research into organisational performance and its measurement and management, the view that there are no 'universally valid rules of organisation and management' enhances the attractiveness of contingency explanations at least for the time being, given the state of the art in this field. (As an ontological stance it happens also to appeal intuitively to the author.) Empirically there may be an implicit assumption that contingency explanations depend on quantitative methodologies, but as Derek Pugh (a core member of the Aston group and self-confessed 'unreconstructed positivist') has explained (Greenwood and Devine 1997), and collegial discussions], in order to identify variables to study through the surveys and quantitative analysis for which the Aston studies are noted, qualitative insights had first to be gained. Thus a mix of methodologies is to be expected, although Pugh would reject studies which did not ultimately aspire to generalisable findings. More contentious perhaps is the extent to which a thoroughly contingent explanation depends logically on realist or positivist epistemologies; and whether contingency explanations for organisational forms and cultures are inevitably deterministic. These are potential issues for the proposed research agenda.

A largely-unmet challenge to the contingency approach to date is to apply it to explanations of organisational *processes* (see for example Greenwood and Devine *op. cit.*). It was a key contention in Holloway 1990 that the processes of developing and operating performance measurement systems were influenced by contextual and environmental variables. To a small extent the impact of contingency variables on processes has been addressed by the studies of benchmarking by Holloway *et al* (1999) but other examples appear hard to find. However, structural and other influences on processes such as the use of Kaplan and Norton's Balanced Scorecard (Dinesh and Palmer, and to some extent Broadbent and Cullen cited above), innovation (Mone *et al.* 1998), and quality circles (Fabi *op cit.*) should be noted. A more thorough literature review may reveal that either deliberate or emergent contingency explanations for relevant processes are now coming into vogue; another item for the research agenda!

It is difficult to envisage the development of contingency *theories* per se which do not have an empirical basis. However, obtaining the resources to undertake research programmes equivalent to the Aston Studies today is difficult to envisage. Whether this would be a worthwhile quest is in any event open to question. Pfeffer for example (1997, pp.162-3) views the decline in popularity of 'structural contingency theory' as inevitable, because 'as it came to be operationalized and elaborated, [it] was neither readily comprehensible nor useful'. Citing the critiques of Schoonhaven, Argyris, Starbuck and

others, he concludes: 'The theory was too disconnected from decision variables actually controlled in organizations, employed concepts that were too abstract, and did not deal in robust, parsimonious ideas.'

In the 'demise' of contingency theory Pfeffer sees cautionary messages for much organisation theory. In the context of a critical performance measurement research agenda, it may be more constructive to regard the death of useful contingency approaches as somewhat exaggerated, but to be mindful of the duty for all research to be rigorously designed and conducted and the need for management research in particular to be accessible to a wide range of audiences.

Critical theories

In terms of the present author's research, this is where experience is replaced by aspiration! Neither contingency nor systems approaches have a very good track record in explaining the sorts of problems with performance measurement that appear to have their origins in differences in stakeholders' (material) interests or disparities of power. In addition we have noted that contingency approaches have tended to focus on structure at the expense of process, and critical systems thinking in particular (ironically perhaps) suffers from inaccessible language and unduly complicated methodologies. However, just as there are natural linkages between contingency and systems approaches, critical theory has links to systems as it seeks holistic rather than reductionistic explanations of society (embracing both the objective and subjective worlds) through the concept of 'totality'.

Nonetheless research which includes in its aims a desire to influence emancipatory change in practices as well as structures has thus been poorly served by the approaches discussed so far. Logically (but on the basis of limited reading), one could hope that the critical theories of the Frankfurt School (whose aims are both to understand and change the world), and Marxists / neo-Marxists, might have useful contributions to make to these neglected areas.

Burrell and Morgan (op. cit.) again provide a helpful contextualised discussion, locating critical theory as a 'objective idealist' body of theory within their 'radical humanist' sociological paradigm (albeit a discussion penned too long ago to reflect more recent developments particularly postmodernism). They characterise critical theory (p.284) as seeking 'to operate simultaneously at a philosophical, a theoretical and a practical level' (although critics have suggested that the achievement of these ideals may be somewhat uneven). These aspirations suggest that a useful contribution to our research agenda may be provided by some if not all critical theorists.

We have noted in section 2 that understanding 'management' is essentially an interdisciplinary quest, and therefore useful theoretical insights into management are likely to have diverse origins. Habermasian critical theory reflects this by drawing on hermeneutics and psychoanalysis as well as functionalist systems theorists. A sound starting point for understanding the contribution to management of the Frankfurt School and in particular Habermas, is Alvesson and Willmott (1996). Their allegiance is not solely with Habermas however, as they seek to problematise the role of management in modern society and counter the tendency in 'mainstream' management theory to '*reduce the political to the technical*' (p.37, emphasis in original). In so doing they make constructive links with labour process analysis and elements of postmodernism.

For the purposes of our performance measurement research agenda, Alvesson and Willmott's 'critical assessments of management specialisms' are particularly useful, as are the works of Mingers and Power and Laughlin in Alvesson and Willmott (1992). Recent empirical research applying Habermasian critical theory in a performance measurement context includes Broadbent and Laughlin (1997) and Townley and Cooper (1998) and no doubt further examples can be found.

Oddly, in spite of having roots in Marx's work, if one relied on Pfeffer's (1997) account of critical theory one would not find any references at all to the Frankfurt School or Habermas! The emphasis instead is on explicitly Marxist analyses of the labour process. (Fortunately he does at least cite Alvesson and Willmott (1992), but only in order to provide a definition of critical theory!). In any event it would be inappropriate to omit a reference to labour process analysis (Braverman 1974, Knights and Willmott 1990; and see a critique by Littler and Salaman 1982) in this discussion of

critical theories, not least because for a long time this work was at the forefront of the quest for critique in the context of manager:worker relations. This is therefore another potential source of ideas for the critical performance measurement agenda.

Three useful approaches to understanding organisational structures and (to a lesser extent) processes have been identified above, together with some examples of ways in which they have already been applied in a performance measurement context. Before closing this section, it is perhaps worth mentioning a few more potentially-useful contributory theories which may be used to critique at least some of the more prescriptive approaches to performance measurement and management. For example:

Structuration theory (Giddens 1984), in its own right and because it can provide insights into power (Rowlinson and Procter 1997), can fruitfully be linked with critical theory (Forester 1983), can be complemented by traditional systems concepts to aid empirical utility (Garnsey and Kelly 1995) and provide critical insights into soft systems methodology (Kawalek, undated).

Resource dependence theory (Pfeffer and Salancik 1978, Pfeffer 1997 chap.3), a 'social model' of organisational behaviour enhancing understanding of interorganisational networks of power / dependence relations within the organisation's environment; relevant to aspects of performance such as the supply of materials, knowledge and staff.

Institutional theory (Powell and Di Maggio 1991), described by Hatch (1997) as a symbolic-interpretive variant of theories about organisational environments such as contingency theory and effectively applied by Brignall and Modell in the context of public sector performance measurement.

Resource-based views of the firm (Prahalad and Hamel 1990, Grant 1991 for example), because they relate strategic success to contingency factors including (in the form of 'capabilities') perhaps some elements of processes and behaviours. 'Flavour of the month' in strategy circles (see for instance Bromiley (1999) but intuitively appealing to managers, a resource-based perspective has been usefully applied to studies of environmental performance (Russo and Fouts 1997) and product development (Verona), and linked through the work of Penrose to critical systems thinking (Garnsey 1995).

Stakeholder theory – is this another 'approach' masquerading as a coherent body of theory (Mitchell *et al*. 1997)? Certainly no critical approach to performance measurement worth its salt would neglect the issue of which stakeholders are served by such activities, and how their interests differ. Stakeholder theories such as the work of Freeman (1984) have become firmly linked to ethical and environmental performance management practices in certain 'exemplar' organisations (see for example Wheeler and Sillanpaa, 1997). North American researchers are taking stakeholder theory in a variety of directions (see for example Rowley, 1997, and a collection of articles in AMR April 1999). Stakeholder approaches are often linked to environmental performance, in which regard Power and Laughlin (1992 op. cit.) rightly point out that to have a critical impact, organisational practices have to change and not just the accounting mechanisms. And finally the 'social accountability' aspect of performance measurement is addressed by Woodward et al (1996) who link stakeholder theory to **agency theory** (see Eisenhardt 1989, Donaldson 1990) in the context of providing theoretical underpinnings for corporate social reporting.

Post-modernism – last but not least, a more leisurely development of a performance measurement research agenda ought to consider what contributions might come from post-modernist writers, although they may consist primarily of a rejection of the endeavour! However they are also informing the work of critical and systems theorists so cannot be completely ignored (and see Mingers 1997, Mingers and Brocklesby 1997).

5.   Bringing theory to bear on practice – and vice versa

Practical problems in applying systems, contingency and critical approaches and theories empirically have been alluded to above, quite apart from the dearth of 'good theorising' in the performance measurement field. One factor could be the piecemeal nature of the research, and disparate routes to dissemination, which militate against coherence and critical development and increase the risk of poor quality work and reinventing the wheel. Another simpler factor may be a relative lack of 'received wisdom' about how to do research into organisational performance, particularly of an interdisciplinary nature.

If a number of research projects or questions were to follow the articulation of this agenda, it ought to be possible for them to be very diverse in terms of epistemology and methodology, while providing inputs to a 'bank' of findings to inform related research. If so, some basic common framework might help (while striving to avoid yet another prescriptive model!). A candidate model and three examples are given below which illustrate how a common framework might shape the organisation of research regardless of the theoretical underpinnings, paradigm, or methodology. (It should be noted at the outset that there will be ontological and epistemological reasons why some forms of research cannot be described in such a way.)

Toulmin's (1958) model of the 'components of argumentation' is suggested here as a framework for research questions because it ensures that the claims being made, and their relationships to data and theory, are clearly articulated. It has been used effectively in teaching and research at the OUBS, but there are plenty of other contenders (e.g. Allison 1971, and see also Zey 1998 for her critique of rational choice theory, and Forester's (*op. cit.*). practical methodology linking phenomenology and structuralism) – or one can devise one's own approach. To reiterate, the aim here is <u>not</u> to invent another prescriptive model, but simply to illustrate how one might proceed to the substantive elements of research and be able to identify links between 'projects' more readily if an organising framework was already in place.

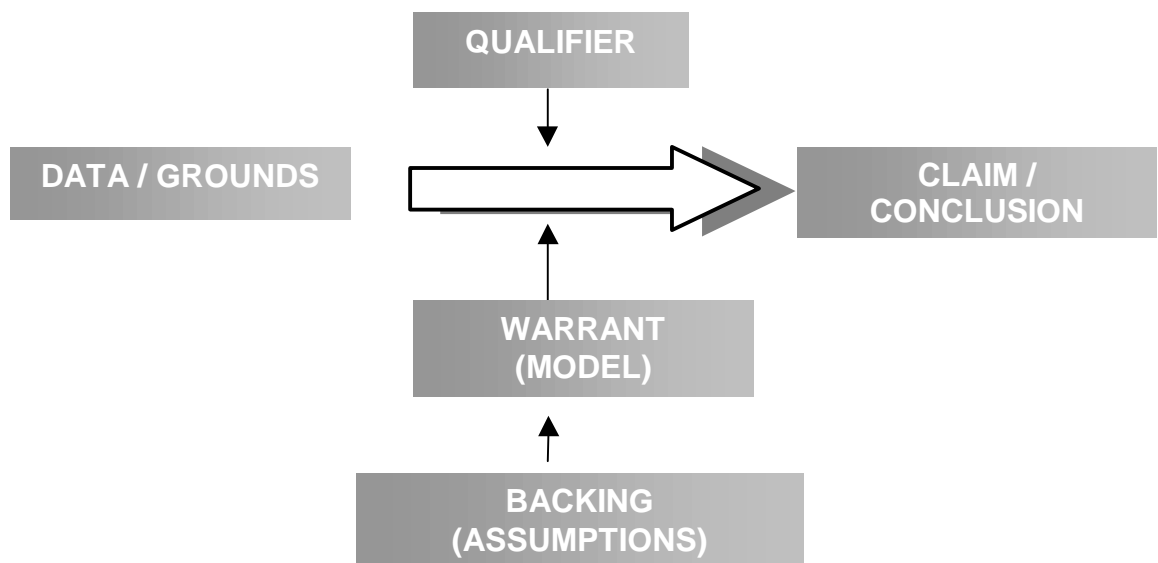**Figure 5.1   The components of argumentation**

Figure 5.1 presents Toulmin's underlying model. The 'warrant' or model equates to theory which explains how the data and claim are linked. The 'qualifier' is any condition which limits the claim.

Figure 5.2 provides an example from the author's doctoral research (Holloway, 1990) mapped on to the framework, retrospectively! Recall that this research aimed to examine the systemic factors influencing the effectiveness of NHS performance measurement systems. In this example the 'data and grounds' for the argument are contained in the literature and empirical data collected. The 'qualifier' is that the claims only apply to formal performance measurement systems (so the conclusions cannot logically be expected to apply to informal arrangements). The theories used to explain the relationship of the data to the conclusions came in the main from systems thinking, underpinned by assumptions such as holism and emergence. The conclusions (as mentioned in the 'Preamble' to this paper) were, crudely, that the nature of the performance measurement issue and its context (e.g. political and technical) limited the extent to which 'good' performance measurement systems be designed and implemented; and that they could not be designed for all NHS issues and contexts, especially conflictual ones, by using systems theories alone.

Figure 5.2   The framework applied to critique NHS performance measurement systems
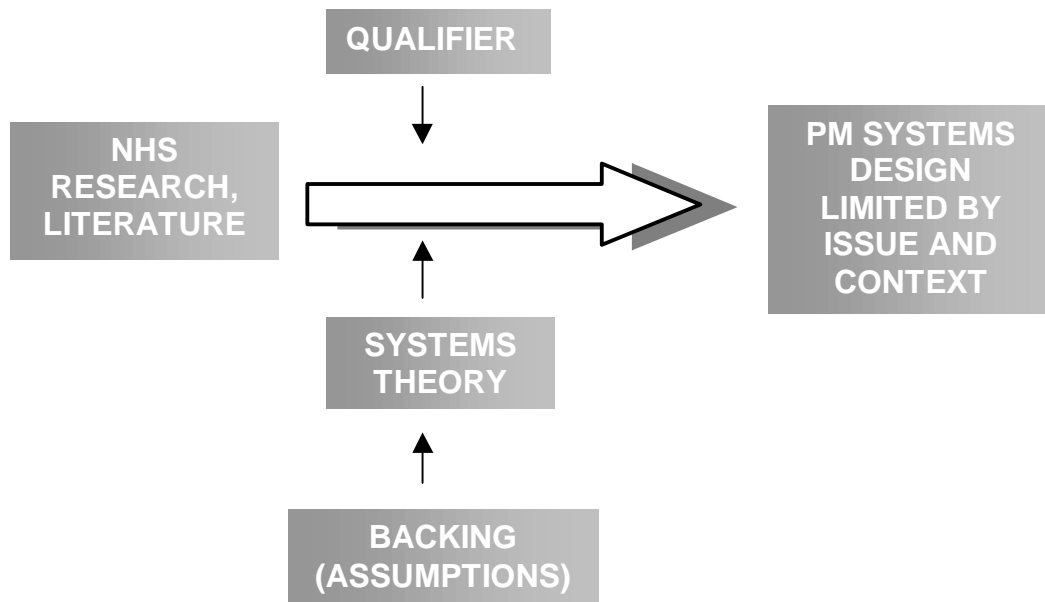
A further illustration from existing research is provided in Figure 5.3, mapping part of the research into benchmarking undertaken by Holloway et al (1999) mentioned above. One of the drivers for this project was to build on Holloway (1990), looking at a specific approach to performance improvement in a wide range of contexts and informed by different theories. Here the data are from the ad hoc reported experiences of MBA students (practising managers), surveys and case studies involving MBA students and management accountants (CIMA members), and a large amount of literature. Among the claims made from this research was that in the case of benchmarking (a qualifier), both context and process characteristics influenced performance measurement effectiveness. ('Effectiveness' here was defined as attainment of the objectives which participants held for benchmarking.) Existing contingency explanations helped to identify potential variables and explain some observed results, while the findings also suggested that contingency explanations for the effectiveness of processes could usefully be more widely sought. In addition the findings suggested that in the case of benchmarking, performance measurement systems design had not generally been informed by theories from organisational behaviour or information systems.

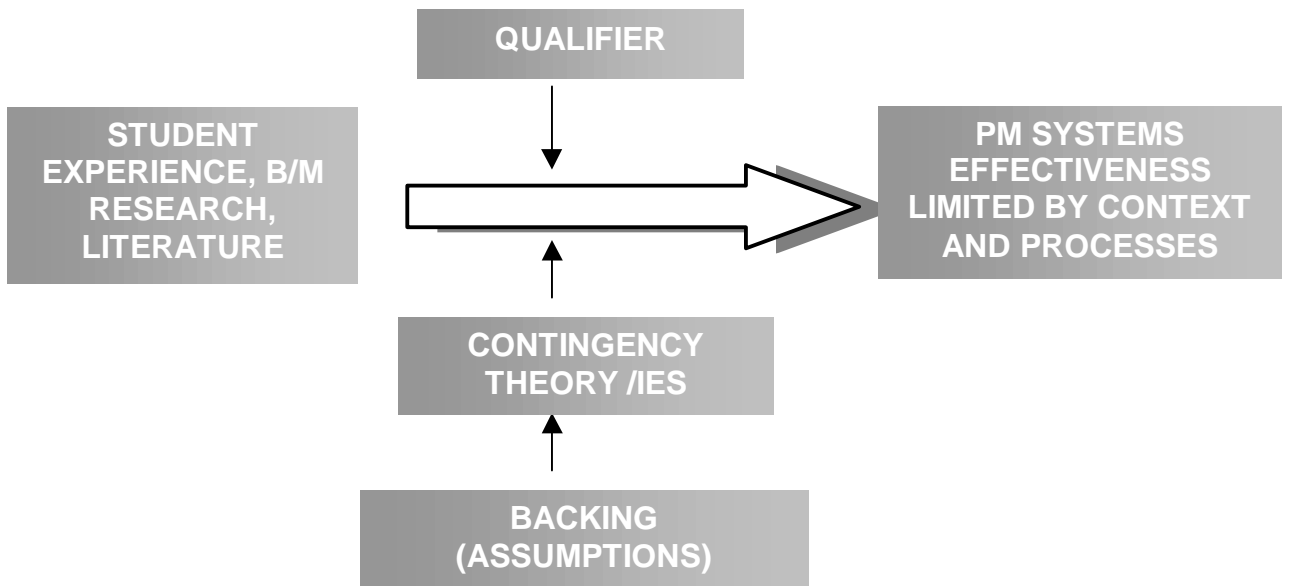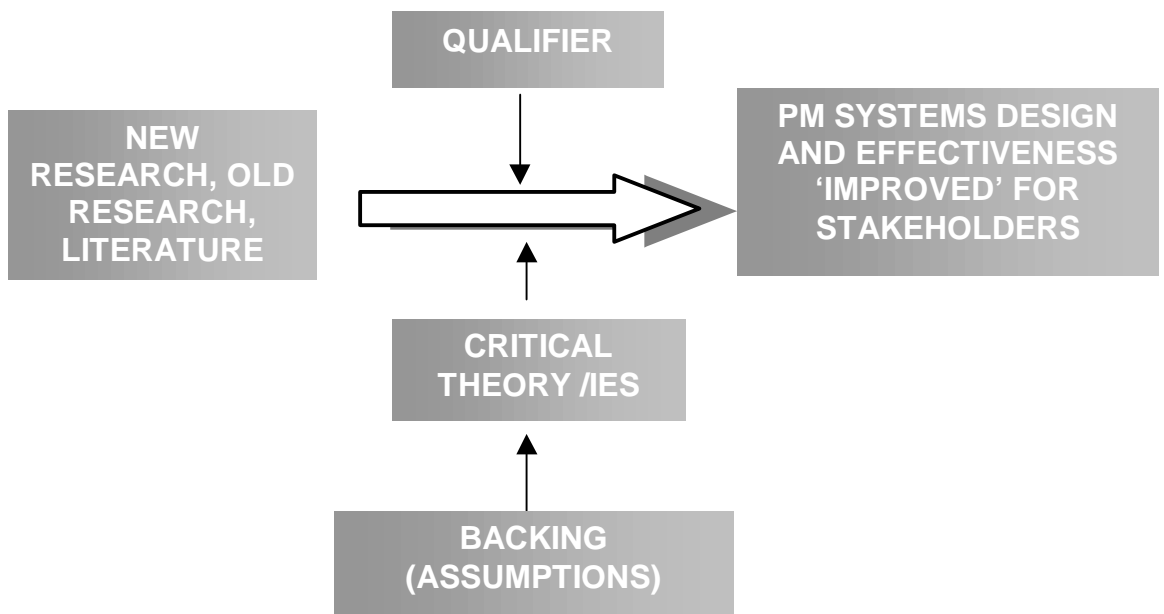**Figure 5.3   An 'argument' drawing on contingency theories**

QUALIFIER

STUDENT EXPERIENCE, B/M RESEARCH, LITERATURE

PM SYSTEMS EFFECTIVENESS LIMITED BY CONTEXT AND PROCESSES

CONTINGENCY THEORY /IES

BACKING (ASSUMPTIONS)

Figure 5.4   What an 'argument' based on critical theory might lead to

QUALIFIER

NEW RESEARCH, OLD RESEARCH, LITERATURE

PM SYSTEMS DESIGN AND EFFECTIVENESS 'IMPROVED' FOR STAKEHOLDERS

CRITICAL THEORY /IES

BACKING (ASSUMPTIONS)

With Fig. 5.4 we start to suggest routes into uncharted territory, which those with direct experience of empirical critical research are best placed to start mapping.

As the preceding examples have implied, the research represented by the 'arguments' has not fully addressed the issue of whether performance measurement systems can be designed and implemented that do not harm some stakeholders and privilege others whose interests differ or are more powerful. Carrying out research which links data about such situations to explanations for how an emancipatory performance measurement system might work could well draw on critical theory of the Habermasian variety.

The simple examples presented in this section illustrate one way of organising linked projects in a way that could aid communication as well as research design. There are many others, as well as the normal relatively ad hoc way in which research is carried out! Indeed by using more than one framework, benefits akin to methodological triangulation could be obtained (with equivalent cautions).

Perhaps a subconscious motivator for seeking to apply an organising framework is to mirror the approach of funding bodies, who increasingly prescribe programmes of research which they are prepared to fund rather than responding to proposals based on academic concerns. At the least a shared research agenda could provide a starting point for an interdisciplinary programme of work for a network of researchers, an approach currently favoured by research councils in the UK. It would be appropriate for such a programme to be informed too by the expressed needs of practitioners.

6.   Conclusions: taking the 'critical agenda' forward

To conclude, an eclectic mix of potential items for a critical research agenda are pulled together from the foregoing text, followed by some issues that need to be debated before such an agenda could inform a programme of research.

For the purposes of this paper, it is suggested that research needs which have been identified as 'agenda items' could usefully continue to be linked or inform each other. The agenda should acknowledge and seek to address: academics' criticisms of much of the performance measurement literature for its atheoretical and / or anecdotal nature; the perceptions of practitioners (who may be both funders and potential informants) that current management research lacks clarity and relevance; and the intrinsic desirability of rigour (particularly in non-traditional methodologies, to protect them from unnecessary criticism). Dissemination of results should strive to be accessible to a wide range of audiences, and thus employ an innovative range of media. Beyond these 'requirements', the agenda can be diverse and may well lead to incompatible or competing insights and explanations. However, it is to be hoped that some sort of synergistic benefits may arise as well as an increase in the scale and scope of performance measurement research.

So far, then, the following more specific agenda items have been suggested above, or proposed elsewhere:

1.   A systematic literature search related to applications of the most promising theories and / or explanations for performance measurement experiences.

2.   A more thorough and better-informed review of bodies of theory which have so far been under-exploited in the performance measurement and performance management arena, some of which are identified above.

3. In-depth empirical research into the relationships between organisational processes for performance measurement, the impact of such measurement (and associated performance management activities) and internal and external contextual variables

4. Research which will move forward our understanding of whether effective performance measurement is a cause or an effect of good organisational performance (or whether both have a common cause)

5. Research which looks at performance measurement from the perspective of the measured rather than the measurers, and seeks to understand the limits or extent to which common interests can be pursued through careful performance measurement design or other enabling mechanisms.

We have also noted a number of related 'agendas' arising from events, review articles and work with managers; the above points are just a collection assembled at this point in time.

There could also be a parallel agenda in terms of methodology development, for example:

- triangulation of methods and data sources (for example a wider range of ethnographic studies to complement existing and future survey or short case study data sets)

- greater involvement of practising managers as partners in collaborative research (not just as survey or case study fodder). Their use of qualitative research as part of their normal work can also be fruitful in the performance measurement field – see for example Skinner, Tagg and Holloway (2000).

- explorations of the role (implicit and explicit) of evaluation in performance measurement contexts, and of how evaluation could be encouraged by the development of simple but rigorous methodologies.

Turning to the issues which will need to be addressed if the idea of a shared agenda is to be accepted, the first few relate primarily to 'theory' and the rest to epistemology and methodology.

It may be necessary to defend the emphasis given above to theory – is it just an academic comfort zone? Delanty (1997), advancing the cause of critical constructivism, stresses that

> '… the problem of social science today is one of practical relevance of both theory and research. The tendency in recent years has been for a divorce between theory and research, to the extent that theory itself is becoming a new professional discourse as irrelevant to social communication as traditional positivistic empirical research. One of the challenges for the future will be the incorporation of theory into empirical social research.' (pp. 139-40)

This sentiment certainly applies to management in general (Tranfield and Starkey 1998), to much work on performance measurement, and (according to Pfeffer) to contingency theory. (It is fair to note that similar concerns exist in medicine, for example – see Alderson 1998.) It will also be important to explore how to make our theorising reflexive (Hardy and Clegg 1997), and to avoid falling into a 'metatheory trap' (in the spirit of Willmott, 1996).

Finally there is a question about whether there are logical, ontological or other types of limits to the ingredients of the 'theoretical soup' implied by the approach outlined above (thanks to Richard Laughlin for this metaphor). Obviously theories need to be of demonstrable relevance, but is theoretical and methodological pluralism inherently OK? Debates around paradigm (in)comensurability are still taking place, for example in management science (for which read systems and OR, see Jackson 1999, Mingers 1997, Mingers and Brocklesby 1997). There is a risk of accepting intrinsically incompatible ontologies which one researcher can't really hold unless they are schizophrenic! Claims to do so may simply reflect a lack of detailed understanding, or inadequate reference to the 'original sources'.

Nonetheless one could still envisage research projects where team members and – if participative – clients / users held different ontological beliefs and could somehow turn this to advantage by approaching a research project from different intellectual as well as interest viewpoints. An approach fraught with potential dangers, but perhaps a worthwhile endeavour to explore further. To close with a paraphrased quote from Spaull (1997), I invite

> '… the trading of neat scientific frameworks for the deployment of expertise for a public process of reflection, interpretation and education' (p.343.

and (from Sotirin and Tyrell), wonder if

> 'as critical analysts … we can take critical management studies as an exploration into the creative paradoxes of an oxymoron'. (p.326)

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