Overcoming Barriers to Academic/Practitioner Engagement in Management Research

Abstract

Social entrepreneurship and non profit management as emerging academic disciplines are generating a rapidly expanding literature on a growing range of topics. While this literature includes themes and research directed towards policy makers, social entrepreneurs, public service managers little of the research output is produced by practitioners themselves and its content tends to reflect the priorities of academics. This paper considers the way the disciplinary system and its reward structure create barriers to academic/practitioner engagement, influencing the questions we ask, and examines how non-profit management and social entrepreneurship are beginning to address this tension.

Keywords: Nonprofit Organizations; Collaboration & Conflict among Public, Nonprofit & Private Organizations; Theoretical Development

1. Executive Summary

While research on public and non-profit organisations, and in particular the literature on the challenges of developing theories of management and innovation have a long history and an impressive literature, I will instead focus on an area of research which though covering much of the non-profit terrain, is enclosed by different boundaries, that of social entrepreneurship. While almost all the arguments discussed in this paper apply directly to public and non-profit research, focussing on the social entrepreneurship literature will enable a more comparable disciplinary case to be developed, especially the contrast between the analysis of the social and the (exclusively) commercial aspects of business practices.

Social entrepreneurship is no longer just a topic within business studies or economics but is in many ways an academic discipline in its own right, with university courses, academic journals and specialist conferences used as evidence to support this claim. Academic research in social entrepreneurship has attracted a wide range of researchers, coming from a range of other disciplines and bringing with them a variety of models, methods and theories with which to investigate and explain entrepreneurship phenomena. Indeed, academic who specialise in social entrepreneurship research come from a wide variety of perspectives and backgrounds, some of whom have been entrepreneurs, policy makers, advisors or having been engaged in other forms of entrepreneurship practice. The way social entrepreneurship is investigated by academics, while reflecting some of this diversity, has, though, been limited in its scope by a range of factors related to the status of social entrepreneurship as an emerging discipline.

The article addresses some of the reasons likely to explain this phenomenon. Firstly as a small but quickly growing field of research, a relatively small number of

influential individuals within the academic field of social entrepreneurship research are able to establish the research agenda. Secondly, the success of such rapid growth has generally been attributed to the strategy of discipline development through meeting high academic standards, as determined by publishing articles in established journals, developing new journals of a high reputation and impact value, establishing courses and centres or departments, which meet high assessment criteria, establishing international academic conferences with a small number of keynote speakers, directed primarily towards academics, and winning funding, particularly from prestigious research councils, often piggybacking on the successes of "commercial" entrepreneurship research. Such factors in themselves have greatly contributed to the growing status of social entrepreneurship research; however, the combination of narrow performance targets, powerful academic networks, strict policing of funding, recruitment and dissemination, have meant that academics are often directed towards research questions likely to lead to publishable articles which address the existing debate. In addition to addressing often minor problems in isolation from each other and in isolation from other relevant issues, the objectives and measures which have been developed to assess research, emphasise research outputs which are often inappropriate to the needs of practitioners, while the views and knowledge of practitioners and non academics are rarely disseminated in journals which have a high impact on academics, except though the interpretation of academics and in the context of debates generated by academics.

This article argues that the consequences of decisions made by key social entrepreneurship academics concerning how to make entrepreneurship a credible academic discipline coupled with the type of assessment made to rate academic research in general have widened the gap between academics and practitioners, while

narrowing the scope of what constitutes research worthy of funding and dissemination. The consequence is that while excellent literature is being produced, many issues identified by practitioners are not being addressed in ways that are enlightening or helpful either to academics or non academics looking for insightful and innovative ideas.

To examine possible alternatives, the article analyses some of the research findings from three different areas relevant to the analysis of field building for social entrepreneurship research. The first of these areas is the theory and analysis of the evolution of academic disciplines. The second area is the literature produced by social movement research explaining the process of collective action and the competition for resources amongst groups or networks. These two sets of arguments are contrasted in order to explain the key considerations and possible alternative approaches to field building that can be applied to entrepreneurship. These findings are then compared with some of the recent ideas generated by a third area of research, that of non-profit management and social entrepreneurship itself.

Social entrepreneurship was chosen ahead of non-profit management because it poses a variety of questions for management scholarship due to its complex relationship with other forms of entrepreneurship. Indeed some academics argue that it is a branch of entrepreneurship, which should use the models, theory and techniques developed by mainstream commercial entrepreneurship research, and imitate its field building approach by directing its attention to achieving research outputs of the type research assessments rate highly. The article instead argues that among the strengths of recent social entrepreneurship research has been that it has made effective use of its position as a new area of research to develop an open debate concerning new approaches to research. It has also been very reflexive about its social prefix and its

potential for a pronounced social impact, in ways that other fields of research have undertaken. The success of practitioners in disseminating their ideas directly to academics on their own terms and the perceived mutual benefits of such engagement, is also striking, compared to some other areas of entrepreneurship. Some of the challenges in applying some of these practices to entrepreneurship research as a whole are also discussed.

The result of the comparison suggests that inclusiveness and practitioner engagement is an essential part of the development of social entrepreneurship and, combined with some of the lessons from social movement research and recent non-profit literature, can provide a model for developing research that is more innovative, insightful and relevant, i.e. building insight and breaking boundaries. Conceptualising field building and discipline development in terms of developing an academic movement, which resonates with core research values, is suggested as a way of developing a more pluralist and inclusive approach to developing entrepreneurship scholarship which is more relevant to practitioners and academics in general.

The article concludes with a suggestion concerning how the benefits from the two approaches to discipline development outlined in the article can be retained as entrepreneurship scholarship evolves, using innovations from social entrepreneurship and non-profit management as its model. Social entrepreneurship has developed measures of the social and environmental impact of a firm, known as the double and triple bottom line, which are used along with the bottom line of profitability to assess the performance of a social venture. A similar type of double bottom line of practitioner relevance, and a triple bottom line of research innovation, could be used alongside the more tradition, but vital, research assessment based on academic journals, publications, university courses and specialist conferences.

2. Introduction

The emergence of entrepreneurship as an academic discipline has been analysed by a growing number of commentators (see, for example Gartner 1985; Low and MacMillan 1988; Shane and Venkataraman 2000; Phan 2004; Murphy, Liao and Welsch 2006; Cornelius, Landstrom, and Persson 2006, Reader and Watkins 2006; Zahra 2007). The majority of such evaluations focus primarily, if not exclusively, on the commercial, wealth creating aspects of entrepreneurship, and the producers of the research most frequently cited tend to be university-based academics. The reason for this tendency is very clear: wealth creation has accumulated a larger literature, greater discussion, more academic analysis and this material is therefore the best place to begin when examining entrepreneurship as a concept. Likewise, academics have a great deal of expertise in different methods, theories and perspectives with which to enable them to analyse data or problems or develop models that generalise beyond a case in ways that practitioners, even with greater expertise and knowledge, may not have. In this way, the research literature on entrepreneurship and the issue of discipline development, generally begins with a discussion of the emergence and growth of entrepreneurship in the commercial sector, the importance of innovation in exploring opportunities in commercial markets and a discussion of the way academics and/or business managers as different classes of analysis, have tried to make sense of these practices (see, though Stevenson and Jarillo 1990: 22-23). This implies that to understand entrepreneurship as a series of practices or as an academic field, requires an understanding of commercial entrepreneurship theories and practices, even if entrepreneurship is also directed towards non-commercial or social ends. This approach is somewhat vindicated by illustrating that the social sector can indeed learn from the commercial sector and that social entrepreneurs are learning from their

counterparts in the commercial sector (see Austin, Stevenson and Wei-Skillern 2006); however, this relationship is also of reciprocal benefit. One area in which commercial entrepreneurship research can learn from social entrepreneurship is to develop a more inclusive approach, one which enables policy makers, entrepreneurs and innovators not merely to be the subject of research, but to participate more in the emerging objectives of entrepreneurship research. This process involves rethinking both discipline development and the metrics used to determine academic success. This paper will examine ways in which discipline development can direct research towards problems and issues that practitioners do not identify as among the most important research needs. It will examine the way social entrepreneurship is beginning to address this tension, though under academic pressures which can act as barriers to greater inclusiveness, innovative practice and knowledge production. The paper will conclude by examining how such inclusiveness can be further enhanced, suggesting an approach to field building able to strengthen entrepreneurship as an academic discipline.

3. The Entrepreneurship Disciplines

Entrepreneurship has a long history but has emerged as an academic discipline in the last few decades. The factors contributing to the development of entrepreneurship research have been widely discussed (see for example Low and MacMillan 1988; Stevenson and Jarillo 1990; Shane and Venkataraman 2000) but in brief, in the late 1970s and 1980s many concepts and assumptions developed within research on the economics of innovation and the business impact of entrepreneurs became central to the stated agenda of political parties worldwide. This growing interest in innovative business ventures and enterprise was itself a response to turbulent economic times, the

perceived failure of Keynesian economics, the decline of large scale mass production as a proportion of economic activity in developed economies, the expansion of new technology and the increased number of driving forces to explore new opportunities by both firms and individuals. These factors, coupled with an expansion in academic research and academic journals directed towards an evaluation of this new socio-economic reality, and a growing popular literature (for example Casson 1982; Drucker 1985), gave more prominence to the idea that entrepreneurship was more than a topic, and could, with an appropriate conceptual framework, evolve into an academic discipline (see Gartner 1985).

At around the same time, and perhaps in response to the same uncertainty, coupled with the expansion of other opportunities, this period saw the expansion of a wide range of NGOs, new cooperative groups, community development corporations, non-profit organisations and micro-credit organisations. This expansion has continued and, if anything has accelerated during the past ten years, both in the number and variety of organisations whose existence is primarily to serve the social good. While such activity is examined by researchers specifically interested in non commercial organisations, many such organisations are commercial organisations and business ventures, even if their social function takes precedence in their mission. In this way, individuals and organisations in this sector, in that they share characteristics with those primarily driven by economic goals, are analysed by entrepreneurship scholars; however, a distinction between commercial entrepreneurship and social entrepreneurship can be used to divide up activity according to the *primary* goal of individuals and organisations.

The type of problems that confront commercial entrepreneurs, the means by which individuals and organisations address such problems and the general measures of their success, can be very quickly comprehended. For these and other reasons "entrepreneurship" or "business venture" have often been studied in the narrower sense of achieving market success, as measured by turnover and profit. Social entrepreneurship, which addresses a much wider problematic, a rich diversity of missions, agendas and criteria for success, reflecting a more normative metric, with a greater intangibility of an organisation's second, and sometimes third, bottom line, has made it much less attractive to those interested in distilling a simple view of entrepreneurship or business venturing. The irony is that the clarity with which commercial entrepreneurship can be conceptualised has contributed to a situation where the great majority of research on entrepreneurship has been confined to research in the commercial sector (see Cornelius, Landstom and Persson 2006: 382-395), while the social sector, which is in many ways much richer, broader, more striking and thus in many ways more appropriate to disciplinary status, has been marginalised in research terms. Indeed, a reasonable case could be made to show that the more mature field of commercial entrepreneurship is merely a well documented special case of social entrepreneurship, confined to the area of business organisations. I do not propose such an argument, but instead attempt to show that the core themes of commercial entrepreneurship and that of social entrepreneurship represent entrepreneurship in different ways requiring different methods directed towards different outcomes. This is positive for entrepreneurship as a discipline because the successes of entrepreneurship field building have so far been through conventional academic means, a method which has key limitations. The challenge to develop a more integrated and coherent view of entrepreneurship, with a more systematic and

reflexive approach to research, will highlight some of these limitations and suggest the need for a modification of existing research goals, one which can benefit greatly from a more inclusive approach to field building. The benefits of having two complementary approaches aimed at different, but related, problematics, are likely to enrich entrepreneurship scholarship through cross fertilisation, while at the same time clarifying the importance of practitioner involvement in the research process. What is more important, however, is that research of the highest quality is directed towards issues and problems that policy makers and entrepreneurs themselves deem to be crucial. The metrics and reward structure used in evaluating academic departments can direct research – and discipline development – away from practitioner relevance, the issue to which the paper will now address. Barriers to such plurality are, though, often the unintended consequences of academic conventions which determine the priorities of research. It is to such conventions that the paper will now turn.

4. Normal Science and Academic Research

> An etymological consideration of the word "discipline" suggests it to be a character of ontology and characteristic of modernity's faith in foundations. "Discipline" is derived from the Latin *disciplina:* instruction of disciples. Disciples, in turn, are instructed in a doctrine (and by "doctors") - they are "indoctrinated" (Shepherd 1993: 83)

The word "discipline" therefore originally referred to authority, specialisation and restraint. Far from suggesting a process of continual creativity and perpetually developing innovative and transformative ideas, the term "discipline" initially conceptualised areas of knowledge in terms of setting the limits to that which is to be

investigated or, as the above quote suggests, instructed. The term "academic field" seems, instead to imply a landscape to be explored, cultivated or enclosed. On closer inspection, however, the literature seems to pay little attention to this subtlety and discussions of field building seems to be the same issue as discipline development. The paper will follow this tradition by describing field building and discipline development as intertwining functions of the same project, though where these two dimensions of regulation and geographical separation resonate subtly within the literature, the paper will attempt to reflect this difference.

The literature on the development and evolution of academic disciplines is relatively small, partly because discipline development is not a key problematic of any specific academic discipline. Nevertheless there are a number of key texts that engage with the process of field-building and discipline development as their central theme (see for example Whitley 1984; Abbott 2001; Becher and Trowler 2001). A selective examination of this literature will be made to evaluate the way disciplinary status both contribute to and limit the productive potential of a body of practices and their analysis, and exemplify this in detail with the specific case of entrepreneurship. To begin with, it is helpful to engage with and challenge the assumptions of Thomas Kuhn. This is not because Kuhn provides a coherent model of paradigm shifts to explain this dynamic. Instead it is Kuhn's account of *stability* that is of importance to the issue of academic disciplines. Kuhn explains the crucial stabilisation process, which he terms "normal science," around which factors such as communities of academics, international conferences, research funding opportunities and academic journals, converge. Kuhn's approach offers a heuristic model of change and stabilisation against which other descriptive accounts can be measured. For example,

in other discussions on field-building and discipline development, in areas as diverse as organisational studies, communications, education studies and marketing (see for example Metzger 1987; Craig 1999; Bridges 2006) the same features identified by Kuhn in describing normal science – the proliferation of publications, journals, conferences, funding opportunities and the growth of the field's academic community – have been considered central to the credibility of a claim to disciplinary status (see in particular Becher and Trowler 2001: 75-95). Indeed, these assumptions are often uncritically invoked in support of the credibility, stability and importance of a research area being worthy of disciplinary status, as in the following quote on the example of Management Information Systems, taken from an article which specifically states its intention to critique the Kuhnian model as inadequate:

> We first have to ask if MIS qualifies as a scientific field. The following facts support a positive answer to this question. Many universities offer MIS programs. These programs are managed by MIS departments that receive and use MIS research funds. Specific publication outlets have emerged…while papers published in other prestigious journals are identified as MIS papers. Prestigious conferences are held on a regular basis…and MIS sections are created in more general conferences. Based on these characteristics, we feel justified to assume that MIS is a field (Banville and Landry 1989: 55)

Turning to Kuhn's work is helpful because he sets out a clear and thorough argument detailing how certain disciplines have developed in the past and various metrics used to illustrate this, an argument which is alluded to uncritically and unreflectively throughout the literature, as shown in the quote above. Additionally, Kuhn draws

attention to some of the assumptions and consequences related to academic communities, particularly when applied to describe the process of field-building. Kuhn describes the negative cost of this method, costs which are rarely discussed (though, see Daft and Lewin 1990) when considering the shaping of academic disciplines.

Kuhn uses the term “normal science” to describe an approach to undertaking research. Such an approach is based on past scientific achievements that the appropriate academic community acknowledges as a foundation for its practice. Kuhn describes these achievements, or “paradigms” as both sufficiently unprecedented to attract a group of adherents away from competing modes of academic research, but, at the same time, sufficiently open-ended to leave various problems for the community of research practitioners to address. Paradigms, in this way, thus help academic communities to demarcate their discipline. They do so, Kuhn argues, by creating avenues of inquiry, helping to formulate research questions, directing the selection of methods appropriate to these questions, defining areas of relevance, structuring the fact gathering process and identifying acceptable technologies appropriate for research. A paradigm also acts to draw in individuals to act as advocates. These advocates and followers are then transformed into a research community, a profession or a discipline as the paradigm becomes accepted and gains credibility. This occurs, Kuhn argues, through the formation of journals, societies or specialist groups, which develop the discipline through articles that are directed to their colleagues who accept the paradigm, rather than needing to justify the concepts, questions, and methods from first principles. This professionalism is supported by the community using its

expertise to claim, both for themselves and their paradigm, a place in the academic establishment.

According to Kuhn, normal science makes no effort to develop new theory or explore new types of phenomena, nor does it set out to discover anomalies to the paradigm and when such anomalies occur is not primarily concerning with exploring them (see Kuhn 1970: 24). Indeed through the course of undertaking normal science practices, anomalies are *less* likely to be noticed as research is directed towards ideas and phenomena supplied by the paradigm itself. Those who question the methods and theories of the paradigm or investigate anomalies tend to seem less productive both to their research community and to academia in general, and it is the productivity of researchers, particularly in solving problems that is the strength of normal science in field-building. This assumption creates an obvious tension for those arguments which simultaneous invoke community building in the traditional normal science way of convergence, as outlined by Kuhn, and creative, free thinking, inclusive innovation in methods or theories which implies increasing divergence. It may be true that, as Philip Thomas argues: "boundaries of disciplines are constantly moving and subject to renegotiation" (Thomas 1997: 12); however, this does not support the assumption that divergence is tolerated, as who and what is included or excluded from the negotiation process is no accident and the process of assessing boundaries, territory and methods is policed very tightly and generally "intolerant of [new methods and theories] invented by others" (Kuhn 1970: 24).

This model suggests that for a discipline to take shape, gain purchase and grow in credibility, it must demonstrate the ability to produce resources, filter information,

explore knowledge opportunities and, perhaps most of all, solve problems that other approaches or other fields have not been able to solve. It is for this reason that Kuhn describes the process of research as essentially puzzle-solving, with the paradigm acting as a criterion for choosing problems and setting the rules that outline the type of solutions acceptable and admissible.

Problem solving is therefore an important feature in the complex process of field-building as outlined by Kuhn. Its primary function is to quickly build up a reservoir of acceptable problem-solutions, of which the field takes ownership and transmits to members of the community (and those to be initiated into the field) as model solutions, which are clear illustrations of the success of the paradigm and its discourse, and evidence of academic progress. Solving problems, testing and refining theory and methods, the manufacture of data sets or other bodies of evidence are thus, according to this view, the key factors required for the maturity of a discipline. The argument concludes that this is particularly important for quickly increasing the volume of research that a *community as a whole* recognises as relevant, and that this community can *collectively* modify and improve, in order to then disseminate tangible and useful outputs, which can then be further developed as (relatively uncontested and reliable) building blocks to identify and solve further problems.

> The man who is striving to solve a problem defined by existing knowledge and technique is not just looking around. He knows what he wants to achieve, and he designs his instruments and directs his thoughts accordingly (Kuhn 1970: 96)

Once obtained, the systematic nature of the data, and its reliability, further add to the credibility of the discipline. Such an approach requires communicating its ideas not challenging or systematically testing the underlying assumptions. This is entirely incompatible with the model that characterises discipline development in terms of searching for novelty, developing ideas through continually questioning the foundations, methods and theories that the emerging discipline is in the process of developing, or by testing the underlying assumptions of that theory. Creativity and speculation play their part in conventional research, but a focus on philosophical or practical aspects of a problem without sufficient data analysis or an exemplification through empirical sources is unlikely to produce the outputs that academic metrics deem most important. This is an important factor in assessing the successes and failures of entrepreneurship research. It is clear that entrepreneurship has progressed as an academic discipline when measured in terms of these normal science indicators, but this does not imply that research that generates outputs meeting these indicators is where innovative entrepreneurship ideas are being produced. Indeed, the danger is that such an approach to undertaking research influenced by normal science metrics puts an undue emphasis on solving minor problems in isolation from each other and in isolation from other relevant issues, at the expense of developing and engaging with concepts and concerns of much greater importance to practitioners.

Emphasising the need to achieve these metrics also has a range of limitations associated with constant reference to “the current debate” or a small number of canonical texts. These factors can impose a structure or series of pigeonholes on research, which can hamper the intellectual development of a discipline, particularly if adopted too early in the development of a field. Indeed, according to Richard Daft

and Arie Lewin, before research becomes normal science, researchers and academics have a degree of flexibility, but once a paradigm is in position, researchers are trained to rigidly conform to its conventions:

> The boundaries of a paradigm can put the field in an intellectual straitjacket. Research may be generated at a fast pace, but contributions will typically defend the extant point of view, and are unlikely to lead to fundamental new insight. (Daft and Lewin 1990: 2)

The danger, then, is that in pursuit of rapid growth of a field or discipline, particularly in a very early stage of the development of that field, research can be directed towards inappropriate or secondary issues. This occurs either because they are easier to identify or there is more consensus about their meaning and how to address them, or, alternatively concepts used to structure or analyse the research has limitations as a consequences of the immaturity of the research topic. The rewards process for academics, outlined earlier, which encourage journal article-length "slight modification on existing work" outputs, conference "positioning" papers, near-repetition of outputs by researchers, framing projects in accordance with existing research council priorities, overspecialising and excluding communities, hierarchical (and politically invested) setting of priorities and discourses, and compliance with established conceptual paradigms, are not easy to oppose or reverse. They are particularly difficult to challenge without the credibility that a track record in research invested in the existing paradigm provides. Credibility to challenge the paradigm will therefore often only be granted to those who have most to lose by challenging it.

Perhaps as a consequence of such pressures, recent literature examining entrepreneurship scholarship, seem to suggest that it is quickly developing a normal science approach to research, by stabilising its topic areas and excluding others, developing into a more exclusive community, dominated by a core group of leading authors, and demonstrating a greater specialisation of research (see Cornelelius, Landstom and Persson 2006: 395 and Reader and Watkins 2006: 426-427). While these features have contributed to success, as measured by the normal science metrics as outlined above, such success does indeed seem to coincide with fragmentation and clustering that excludes potential intellectual allies (see Reader and Watkins 2006: 430-432). This would suggest less engagement with novel research approaches, greater methodological conformity, and an intellectual distance from non-academic practitioners. This would also seem to imply a reduced potential to be innovate in conceptualising the research problematic (Welsch and Maltarich 2004: 60), less willingness to engage with the complex realities from which entrepreneurship emerges (Steyaert and Hjorth 2006: 1-3), and greater readiness to imitate research patterns with perceived successful outcomes (Zahra 2007: 446), in a domain which does not fit the pattern of a mature discipline (see Sarasvathy 2004: 707-708) at least as described by Kuhn. The success in meeting targets for normal science metrics must be judged alongside other types of research targets and objectives. In social entrepreneurship, an intellectual landscape where ideas, creativity and innovation are most prized by practitioners and are the very subject of research by academics, it would seem a sad irony for researchers to merely imitate their academic commercial entrepreneurship cousins, a strategy which has been suggested in recent academic papers (see, for example Thompson, Alvy and Lees 2000; Austin, Stevenson and Wei-Skillern 2006). Indeed the pluralism of methods, theories and approaches that

social entrepreneurship can afford in the context of a more inclusive research group would seem better equipped to produce innovative ways of understanding the process and practice of entrepreneurship than those approaches that score well when measured by normal science metrics. Daft and Lewin suggested that one way of being more reflexive in the production of new knowledge, but within the framework of conventional field-building, is to acknowledge the limits of normal science, and try to extend the conceptual framework with which research is developed. They prescribe a number of strategies, such as focussing on equivocal problems and following heretical research methods or outlier research:

> Building theory on the basis of in-depth understanding of a few cases is different from the traditional theory-testing goal of statistical rigor, parsimony and generalizability. However, this type of research can provide the genesis for new theory that may spawn further research that uses traditional methods (Daft and Lewin 1990: 6)

Their claim in putting forward such a research agenda is that conventional research is concerned with factors such as rigour, planning, control of variables as well as validity, both internally and externally. These factors are likely to give incremental knowledge gains, with much of the research merely confirming the conventional wisdom. The real breakthroughs that genuinely contribute to a better understanding of the issues researchers examine, Daft and Lewin argue, emerge from exploring issues without knowing in advance what the outcome is likely to be such that "the greater the surprise, the more interesting the result, and the greater the new knowledge" (Daft and Lewin 1990: 7). The problem, of course, is how criteria for good research can be developed that produce interesting outcomes and new

knowledge without excluding the most worthy, useful and innovative research, nor including research that is doomed before it has even begun. It thus becomes more difficult to make a clear assessment of a proposal for research. Perhaps mistakes and heroic failures should be tolerated more in academic research and maybe such failure is the price for more innovative methods. These challenges, though, need not be obstacles to originality or to developing applied research:

> The good news is that these often very legitimate challenges can be mitigated through precise language and thoughtful research design: careful justification of theory building, theoretical sampling of cases, interviews that limit informant bias, rich presentation of evidence in tables and appendixes, and clear statement of theoretical arguments. (Eisenhardt and Graebner 2007: 30)

The requirements which serve to generate credibility within the research community might also be made to conform to a more tolerant model. This is, though, difficult, particularly where training to master specific categories and methods alters individual and collective vision of what constitutes the very fabric of subject. This is illustrated by Reuben Hersh in his reflection of the practices in his own field of partial differential equations:

> I had my field [which] used categories and evaluative modes that I had absorbed years before, in my training as a graduate student. They were part of the way I saw the world not part of the world I was looking at. My advancement was dependant on my research and publication in my field. There were rewards for mastering the outlook and ways of thought sharing by those whose training was similar to

> mine, the other workers in the field. Their judgement would decide the value of what I did. (Davis and Hersh 1990: 1)

However, this poses even greater problems for smaller emerging fields because the broadness and generality of a tolerant and innovative approach to research, weakens the bonds between researchers, and these bonds are often a key factor in such fields existing in the first place. Unless it is explicitly clear that such a field is not committed to a normal science pattern of field building, the assumption will be that the research process of this field is a failure. This is because it will be judged in terms of normal science metrics. While it is important that the outputs they measure continue to be produced, a second bottom line of metrics can be produced to measure other non normal science factors which help to build a more diverse, inclusive and robust research agenda. Indeed, as with the bottom line of profit, the additional double and triple bottom line of social and environmental measures are used to ensure that the mission are addressed, so too the bottom line of research assessment exercise criteria can be supplemented with the second bottom line of practitioner-directed outputs and a third bottom line of outlier research. While such formal measures have not been finalised, centres of social entrepreneurship are indeed emphasising their production of practitioner outputs as part of their mission (see Nicholls 2006).

5. The Emergence of New Research Practices

The importance of developing research beyond disciplinary boundaries is evident and an interdisciplinary approach can contribute to strengthening pluralism, circumventing disciplinary elites within the academy and can address disciplinary crises or support the early development of new areas of research. However, interdisciplinary approaches to research are not independent of the existing

disciplinary structure but supervene on the existing disciplinary categorical relationships, or, as Michael Moran describes this relationship: "Interdisciplinarity…only makes sense in a disciplinary world" (Moran 2006: 73). Moran continues: "in a world where almost everyone speaks approvingly of interdisciplinarity, disciplinary identities are if anything strengthening their hold over the academic mind" (Moran 2006: 73). Interdisciplinary research implies, therefore, the firming up of disciplines rather than supporting a strategy for replacing existing disciplines. Consequently, even though entrepreneurship research might productively develop its academic profile through developing strategic relationships with other disciplines, this is not a route to developing an independent research agenda or creating new concepts directed towards the type of practices such research is primarily interested in. This would still require some type of field-building strategy. The duel requirement for interdependence between the themes and topics likely to advance entrepreneurship research, coupled with the need for a distinctive and bounded landscape, is a difficult tension to manage. The requirement to manage such a tension might, though, actually become a strength of entrepreneurship. This is because such an approach requires a field to engage beyond the confines of a small community, while at the same time developing a base from which to ensure that such engagement is deemed credible, value adding and productive. The robust series of research issues at the disposal of entrepreneurship make such engagement much easier and network building much more likely.

A normal science approach, as outlined in the previous section, would suggest that a tightly managed focus on a few core problems is the only way to sustain a group focussed in a topic area. Developing a sustainable group by appealing only to those

with a range of interests in the topic is thus an unrealistic strategy for a new discipline. However, there is a body of literature which empirically examines many such cases of the identification, investigation and resolution of multiple problems through collective means, coupled, at the same time, with engagement with a wider network, i.e. social movement theory. Some of the models developed by social movement theory might therefore be useful tools for entrepreneurship field building strategy. This is not to say that discipline building is the same as mobilising people around a single issue. Clearly they are very different in factors such as focus, objective, complexity, political means, impact, commitment, demographics, for example, but it is in the key consideration of forming functioning network relationships based on a perceived sense of common interest or values that is important. I will therefore turn to some of the themes developed within this literature in order to show why such an approach to field building is both attractive and feasible.

A number of academics have examined academic disciplines, including social entrepreneurship, in relation to some features of social movement theory (see for example Harty and Shove 2004; Rojas 2006; Johnston 2006; Steyaert and Hjiorth 2006) and some disciplines, notably women's studies, have close ties with specific social movements; however, my intention is to identify considerations for field building strategy developed in social movements scholarship. I do so in order to outline the essential factors in field building strategy which must be met in order to develop an alternative to the normal science approach, appropriate for social entrepreneurship. That is to say, to outline a feasible way to achieve an academic research agenda, through the type of collective ownership conferred by a field or discipline.

Examining the social movement literature, one of the most elementary issues is that of sustainable mobilisation, i.e. to bring together a sufficient number of people for a sufficiently long time period to address a specific issue or complex of issues. This generally requires an issue or topic with a high degree of resonance with the interests and values of a section of the population. Entrepreneurship in general, and social entrepreneurship in particular, has much potential in this regard. For example social entrepreneurship, as a category of practices is rapidly expanding in each of its forms, from non-profit organisations, philanthropic investors, social-purpose commercial ventures and ethical businesses, to various hybrid types of venture. It is also expanding in the number of different models of venture that can be included under its banner (see, for example, Austin, Stevenson and Wei-Skillern 2006: 1-3). The richness of this territory for new academic research is not in question, nor the need for grounded theory, analytic models, normative and prescriptive guidance derived from empirical research, heuristic models, tools and methods to conceive and achieve the vision of practitioners and for academic analysis (see Nicholls 2006: 407-411). Social entrepreneurship therefore satisfies the requirements and expectations that an academic movement might need to address, but this is not sufficient to mobilise the quantity and quality of researchers and resources required to do so, as there is tough competition between the vast assortment of research agendas. In the social movement literature, Stephen Hilgartner and Charles L. Bosk address a similar scarcity for attention among social problems:

> Given the vast universe of possibilities, how do social forces select particular problem definitions? (Hilgartner and Bosk 1988: 53)

Hilgartner and Bosk argue that social problems are projections of collective sentiments and in the competition between problems, the "carrying capacities" of public arenas limits the number of problems gaining attention, while principals of selection, including a range of institutional, political, and cultural factors, influence the survival chances of each problem (see Hilgartner and Bosk 1988: 56). By analogy, the ideas, topics and research themes in the academic arena are involved in the same type of competition for resources, researchers and attention, both within and outside the university. Clarifying the institutional, political, and cultural context influencing the arena appropriate to the growth of a field such as social entrepreneurship is therefore the first part of developing a sustainable academic movement determining strategies for maintaining a continual presence appropriate for an academic discipline or field is the second task for such an academic movement. To provide such information by examining the relationship between academic cultures and disciplinary knowledge, I will turn to the work of Tony Becher and Paul Trowler, who identity a means with which to effectively categorise academic communities.

Becher and Trowler, using an extensive amount of empirical data gathered in twelve disciplines, argue that social and institutional characteristics of knowledge communities, which they characterise as tribes, impact on the epistemological properties of the knowledge they produce (Becher and Trowler 2001). They describe the relationship between disciplinary tribes using a range of geographical metaphors: disciplines are domains, specialities are fields, new ideas are frontiers or territories, which are separated along cognitive (hard/soft and pure/applied) and social (convergent/divergent and urban/rural) axes, with (moveable) boundaries separating these domains. In analysing university culture, they argue that disciplinary status

identity can be influenced by the way universities are funded, how faculties and departments are structured or how the budgets of funding councils are planned. However, in addition to these factors, they also state that the community active in contributing to an area of research also contributes to setting the disciplinary boundaries. Enrolling new practitioners and developing a cycle of research reproduction, reinforce these boundaries and help in constituting the discipline as a credible body of knowledge. Becher and Trowler's analysis therefore indicates that academic movements, in the form of knowledge communities, have indeed made an impact on the disciplinary structures. In addition, they suggest that the nature of the community determines the field building strategy. Close-knit communities, clustered in close communication, with demarcated problems centred on few topics and quick solutions, which Becher and Trowler term "urban" (Becher and Trowler 2001: 106-108) tend to field build through limited means. The most significant of these is by developing a profile based on disseminated research findings in a small number of journals, concentrated into specialist articles with shared terminology, models, methods and conceptions, which the community applies to their problem solving agenda. Alternatively, those in dispersed communities, with multiple topics, comprised of problems which are less delineated (i.e. "rural scenarios") tend to set aside time for discussing definitions, justifying the research as relevant and explaining the key concepts and assumptions to those beyond an immediately identifiable research community, disseminating information in inclusive ways, which also reflect its wider scope. The strategies adopted under urban scenarios seem to fit very closely with Kuhn's notion of normal science. This would make sense for exactly the type of disciplines which Kuhn was exclusively interested, i.e. in Becher and Trowler's terms, pure and applied hard science, centred on the converging and urban typology.

Social entrepreneurship, though, fits the softer side of the disciplinary landscape and thus the strategies outlined under rural scenarios seem entirely appropriate, in terms of Becher and Trowler's analysis. Therefore developing a strategy based on a "rural" conception of field building, centred on the "soft" side of the academic landscape gives more coherence to an academic movement based on inclusion and directed towards analysing types of practice. However, as Kuhn correctly observed, if such research is merely descriptive, not problem-oriented and concerned with first principles, there is no growth in consensus or increased confidence in the approach adopted by such research. Clearly, then, a field will have difficulty developing if it is a research free-for-all, and, indeed, social movement research supports this view (see Bob 2005). However, in social entrepreneurship, grounded theory and comparative case studies using comparable methods, are being used as the basis for bringing together practitioners and academics, providing some of the bridging for building an inclusive academic movement (see, for example O'Connor 2006; Young 2006). This does, though, clarify a second tension for such an approach to field building: that of the degree to which creativity, originality and the continual creation of new patterns is emphasised, at the expense of the need to test or build on existing ideas, methods and practices. To explore this tension and its implications for field building, this paper will consider the work of Richard Whitley on the organisation of disciplines.

Whitley presents a number of arguments concerning the relationships between specific academic disciplines. One key argument is that converting resources available to academics into new knowledge, as mediated by the organisational structures that the specific academic community relies upon, ensure that research outputs are fed into further research as inputs. Whitley argues that reputation,

recommendation and mutual dependence are therefore key factors in bridging the originality/continuity tension because a reputation is maintained by developing research that is considered relevant to those engaged with extending existing debates within a discipline (Whitley 1984: 95-97). The need to attain approval and support from colleagues and to direct the research of others along one's own lines, can actually be greatly inclusive. The reward structure associated with successfully developing and controlling a reputation is therefore essential in maintaining the integrity of a field under the pressure of fragmentation that the emergence of novelty might otherwise imply (see Whitley 1984: 42-80). According to Whitley, a discipline evolves according to the role of consensus within a discipline's community and the influence of administrative structures available to that community. Similarly, the social movement literature, which has examined for many years the issue of how reputation functions within a community to facilitate consensus, seems to concur that it is an important factor in the growth and sustainability of individual movements (see for example Knoke 1983). Recent research on social movements and social networks suggests that there are indeed alternatives to normal science policing, in addition to reputation, that enable a productive convergence of priority setting by collectives of individuals (see Frickel 2004; Gamson 2005). Concepts such as collective action frames, i.e. systems of shared beliefs around which a movement resonates (see, for example Benford and Snow 2000), agenda setting, i.e. the ability to make an issue seem more salient than other issues (see, for example Gamson 1990) and priming, i.e. the relation between factors such as knowledge context, exposure and collocation on the formation of opinion (see, for example Weiss and Tschirhart 1994) are of considerable value in explaining the basis on which movement have successfully developed mobilisation strategies. Developing a detailed strategy based on such

concepts, while beyond the scope of this paper, is not therefore trivial, but an important stage in the research agenda of social entrepreneurship. Indeed, Steyaert and Hjorth (2003; 2006) outline a wide range of social change movements which combine academic research and entrepreneurship practice, while events such as the Skoll World Forum, which attract policy makers, academics and entrepreneurs in their hundreds and almost in equal measure, suggest that social entrepreneurship has already attained something of a social movement ethos that could feasibly be harnessed in field building, by a more strategic approach to developing an academic movement. Using such networks not merely for disseminating ideas and discussing cases, but as a tool for identifying research needs, and more importantly, enabling those developing such research to be rewarded, would be measure of an academic movement, in both senses of the word.

6. Conclusion

Justification for claims over research territory and, by implication, claims to disciplinary status, are typically measured by their exponents, in journal articles, conferences, research council funding, processing graduate students quickly and departments or centres, as repeatedly mentioned. Such measures are easy to quantify as research outputs and/or demonstrate the strength of a research community and, through peer review, they preserve minimum standards. As indicators or measures, they give, *some* indication of the investment in specific ideas, methods and topics, equated with the values of a group of academics and related bodies. Researchers and academics should certainly continue to strive to disseminate ideas in academic journals, teach specialist courses, bid for research funding, organise conferences, but not at the expense of reducing the scope and significance of the research. Indeed, as a

basis for a strategy of field building, it is likely to be counterproductive, concentrating on the *epiphenomena* of good research, producing identikit answers to the most accommodating problems rather than on more worthy issues that might be too complex to yield publishable article-length outputs. In addition, a departmental emphasis upon publication in academic journals as the measure of value is likely to reduce the amount of time academics spend engaging with practitioners and reduce the relevance of research outputs to a practitioner audience, with interaction tending to be more instrumental or strategic. The pressure of research assessment exercises, such as the RAE in the UK have been criticised for limiting opportunities for practitioner-oriented, and inter disciplinary-oriented, researchers, the flouting equal opportunity policies, lack of long term strategic planning, the likelihood of creative researchers opting out of academia all together, among other criticisms (see Elton 2000: 280-281). The consequence is that with fewer intermediaries and with younger researchers guided away from such practitioner engagement, the gap between academics and practitioners is in many cases widened, when the benefits of narrowing the gap are obvious to those engaged with entrepreneurship. As Elton concludes: "academic traditionalism in research, often in the very areas where it ought to be lessened, have discouraged new developments and interdisciplinary research, and have isolated researchers from practitioners" (Elton 2000: 279).

A normal science strategy may help in producing solid agreement on how to get an abundance of answers, acquire support to do so, and avenues for disseminating them, but at a tremendous cost in exactly the areas that academic research should be contributing to knowledge, i.e. in identifying key problems and developing applications in practice, while developing and investigating new concepts and

theories, which enable the complex landscape to be more effectively explained. As Daft and Lewin observed:

> Research may be generated at a fast pace, but contributions will typically defend the extant point of view, and are less likely to lead to fundamental new insight. (Daft and Lewin 1990: 2)

Such a divorce from practitioners and overemphasis on normal science measures can have serious consequences in developing appropriate outputs for practitioners or developing useful, creative and stimulating debates, as Gareth Morgan argues:

> The control systems developed by journals and university departments alike exert a confining if well-meaning hold on the jugular of scholarship, which threatens to strangle the development of new possibilities. (Morgan 1990: 29)

The alternative to this strong grip is not to internalise the stranglehold, but to learn how to build an academic movement capable of innovating through engagement with the experience and knowledge of practitioners, as seems to be a fundamental strategy in some of the key social entrepreneurship research networks, as outlined in recent collections of articles and case studies by researchers and practitioners (see Nicholls 2006; Steyaert and Hjorth 2006). There is no simple recipe or "how to" model, and extensive research by Whitley and Becher and Trowler, among others, demonstrates that organisational factors such as academic culture and administrative structures are as important as the research agenda and discipline problematic, in determining the prospects of a new field. Mastery of such organisational factors might be easier for social entrepreneurship than areas of entrepreneurship research, due to its considerable support from a wide range of well connected foundations and an

emerging leadership vision from powerful ambassadors, as well the organisational appeal of its ethical dimension. Nevertheless, what is absent is the type of mobilisation strategy able to draw together the expertise and credibility to provide the academic coherence to the agglomeration of specific cases. The competition with other claims over parts of the same research territory requires this, both as a measure of success, i.e. as an alternative to normal science measures, and as a way of enabling its standing, contacts, knowledge, resources, leadership etc to gain critical mass within the academy. This is, of course, a difficult challenge, and clarifies why a normal science approach seems the easy option to a young field, such as entrepreneurship. Additionally, however, social entrepreneurship has one important advantage over other fields in that it can also feed into and be nourished from the research in different and identifiable phases, such as exists between the commercial oriented and social oriented research patterns. Building a strong and inclusive academic movement, one which can thrive on developing and implementing a pluralist, and no less rigorous, research agenda, strengthens its potential for collaboration in which both orientation types can benefit.

An academic movement approach, then, in contrast with the normal science approach, is a collective, inclusive and emergent process, unfolding an agenda that resonates with the group's interests and concerns. Such research need have no fear of developing exciting, bold theories and imaginative testing methods, able to tolerate academic failure while rewarding vision, engagement and change-making tools through prominence in an active and vibrant movement. Understanding the phenomenon of management in its myriad forms requires asking research questions which policy makers and entrepreneurs benefit from asking and which academics

benefit from answering, but without greater engagement are neither asked nor answered. The questions we ask (the theme of this conference) should be directed towards these ends, rather than an anticipation of how a set of data might fit with the values of a peer reviewed journal.

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