The Managerial (Ir)relevance of IMP

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Abstract

The practical relevance of research to the needs of managers matters to academics, business people, and policy makers. The paper asks whether IMP research has achieved such relevance. If not, was relevance never achieved, was it once achieved but has now been lost, or are inadequate technology transfer processes at fault?

Introduction

Has the research conducted by the IMP Group been relevant to practical business decision-making? Is it still? Should it be relevant? Have the marketing technologies developed by IMP researchers been successfully transferred to business practice? Should they have been? The relevance of IMP research to management practice is explored in this paper by investigating three arguments: that IMP work was once of practical relevance but is no longer so; that IMP research was never of practical relevance; and, that IMP work was and remains of potential practical relevance but has not been effectively transferred into management practice.

The theme of the 15th annual IMP Group conference is “Interaction, Relationships and Networks: Towards the New Millennium” A time for stocktaking and reflection, as well as gazing into the future. This paper is explicitly and unashamedly polemical, taking an extreme point of view in order to stimulate debate, with the aim of exposing and undermining any sense of complacency that may have infiltrated the IMP research tradition. In essence, the thesis of the paper amounts to this: that the IMP approach to understanding business marketing and purchasing was a development of paradigmatic significance, but that, two decades later, it has failed to make any significant contribution to management practice and, therefore, may be accused of being largely sophistic and symbolic. Astley (1984) has argued that it is of the nature of scientific research in management to be subjective, sophistic, and symbolic. In this paper, Astley’s general proposition is tested in the context of one particular school of thought, namely the IMP approach. A number of different perspectives are used to explore the underlying question, “is IMP research essentially sophistic and symbolic?” The starting point is some recent internal reflections on the state of development of IMP thinking. Attention then turns to the notion of IMP research as sophistry, following some discussion of just what sophistry is, and whether it must always be reckoned a term of academic abuse. Subsequently, two further analogies are explored to see if light can be cast on the presumed failure of IMP research to light up the world of the management practitioner. First, the

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analogy is drawn with the rise of activity-based costing in management accounting, which was contemporaneous with, and precipitated by, the crisis of confidence in management accounting termed by Johnson and Kaplan (1987) “Relevance Lost.” Second, it is asked whether the limited managerial impact of IMP can be attributed to an inadequate process of technology transfer.

So, in exploring the thesis that IMP research is sophistic and symbolic, the authors use wisdom from IMP’s own luminaries, have recourse to the sophists themselves, engage with the processes of technology transfer, and assess the parallel with an earlier crisis in management accounting research. The lessons from these adventures are drawn together in the conclusion, in which some tentative suggestions are made about how to make IMP more relevant to managers without sacrifice of scientific rigour.

Internal critiques

As the century comes to an end doubtless many social institutions are indulging in some self-examination. Over the last few years such introspection has become manifest among the senior members of the IMP Group (Turnbull, Ford and Cunningham 1996, Valla and Salle 1997, Gemunden 1997, Ford 1998). Turnbull et al (1996, p 58) provided a rather optimistic perspective: “It is not easy to write a conclusion to a report of the historical evolution of a very wide-ranging research process; the work continues! ... Our continuing work is to try to understand the patterns of meanings and the beliefs which guide managers in their interactions with others in the increasingly complex networks in which they operate.” Yet the subsequent contributions to this emerging debate have been far from universally optimistic. Gemunden (1997) conducted a detailed examination of IMP research conference proceedings over the period 1984 to 1996, and found that nearly half of the papers did not use any data analysis to derive or test their conclusions. Furthermore, in few papers was there any thought given to what Gemunden calls “performance analysis”, which is to say the examination of the hypothesis that relationships and networks actually influence the performance of the firm. Subsequently, Valla and Salle (1997, p6) argued that “key issues for marketing managers ... (list of issues) ... have not yet been given the adequate importance in our general approach”. Valla and Salle went on to point to organisational change and changes in the business environment, which might tend to invalidate prior IMP research findings. For example, the rapid growth in the importance of the quality function, the increasing sophistication of logistics management, drastic reductions in staff personnel in numerous large firms, and the rapid globalisation of markets leading Valla and Salle to ask whether stable long-term business relationships exist any more, and to advocate a new empirical research project. This suggestion was taken up with a vengeance by Ford (1998), advocating a new IMP research project, necessary because IMP research had become out of touch with business reality, was descriptive not prescriptive, and because changes in industry structures and in the internal organisation of firms may have rendered prior research invalid.

Ford’s *mea culpa* approach is strangely beguiling. We are not in touch with business realities, we describe too much and prescribe too little, we have failed to keep pace with the rapidly changing business environment so that our models no longer make much sense. Indeed, Gemunden’s (1997) conscientious analysis of the IMP conference proceedings lends support

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at least to the first two of those propositions. But let us consider further the implications of the third. The world has moved on. There has been substantial change in patterns of industrial structure, and within firms old patterns of organisation are giving way to new. Therefore it is to be expected that models of reality developed two decades ago will be obsolete. Once, they were useful, meaningful representations of reality. Reality has changed, so they no longer work.

While at first sight a relatively uncontroversial statement of the semi-obvious, this admission is surely at odds with the entire IMP purpose "to try to understand the patterns of meanings and the beliefs which guide managers in their interactions with others" as Turnbull et al (1996) so eloquently put it. Part of that purpose, certainly, is to paint a picture of reality. But a larger part of the purpose is to investigate the structures underlying that reality. The words coined to refer to those structures are very familiar - power, trust, adaptation, closeness and so on. Surely the greater purpose of the IMP _oeuvre_ is to define the durable structures which underlie inter-firm behaviour, exactly so that it is unnecessary constantly to redraw the map of business-to-business reality in response to surface change (change in the "domain of empirical" as Bhaskar put it [Bhaskar 1997, p.13]). To extend the geographical analogy, does the IMP Group aspire regularly to draw new maps of continually changing terrain, or does it aspire to explain (and even predict) how and why the terrain is changing? There is little doubt from early IMP works (Hakansson 1982, Turnbull and Valla 1986, Ford 1990) that these researchers believed that they were identifying the causal mechanisms, not just painting pictures.

**IMP as sophistry**

The arguments put forward by Ford, Valla and Salle, partially supported by Gemunden's evidence, imply rather too much clever description and empty reasoning, and rather too little concern for managerial realities based on thorough scientific investigation. This is very close to the charge levelled by Astley (1984) against the management sciences in general, that they are intrinsically sophistic and symbolic. Management academics no doubt pride themselves on playing a range of different roles. They repackage existing knowledge and transmit it both to students, and to practitioners in pursuit of lifelong learning. They establish the best of current business practice and synthesise it for interested parties. They conduct scientific studies of the organisational domain in order to fathom the underlying forces that drive business systems within and between firms. Astley (1984) argued, however, that underlying the role of the management scientist was a less obvious and perhaps less honourable purpose, namely, to construct a symbolic world of management. In its most extreme form, the thesis would be that management scientists are apologists on behalf of capitalism. Their job is to construct symbols and clever arguments to dress up the grubby pursuit of profit, exploitation of low-paid workers, despoliation of the environment and so on, as some kind of higher endeavour. This is essentially a rhetorical project, and today's use of the word "sophistic" is in exactly this sense: in placing greater emphasis on the rhetorical and presentational aspects of an argument rather than its substance. Some further investigation of sophism is appropriate.

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An appropriate starting point is with the sophists themselves (Barrett 1987, Kerferd 1981). According to the Concise Oxford Dictionary a sophist is “a person who reasons with clever but fallacious arguments”, and Astley (1984) recounts Plato’s contention that the sophists employed eristic methods in order to achieve victory in debate regardless of the fundamental merit of their argument. Therefore, for Astley, the sophistic manipulation of ideas is central to management science. It matters not a great deal if we are ultimately right or wrong, what matters is that we win the debate. The label "sophistic", therefore, would probably be regarded as undesirable by most management academics, who pride themselves on the clarity and relevance of their work, who aspire to contribute to management practice through their research. However, beyond the narrow stereotype of the sophistic movement which is the legacy of Plato’s antipathy towards this group of scholars there exist, in the sophistic movement, ideas that would find sympathy among many management researchers. Barrett contends that “Their (the sophists’) epistemology was functional: knowledge was for use in meeting needs as they arose and coming to good decisions ... Sophists were leaders in advocacy of verbal precision ... From the sophists ... people learned that one person’s ‘truth’ might not be another’s, that positions are debatable, that the only way to ‘truth’ is through examination of both sides ...” (Barrett 1987, pp 38-39). Paradoxically, management scientists might well aspire to these sophistic virtues, while shying away from the dread label itself.

Plato considered that the sophists were purveyors of fine rhetoric in support of whatever argument suited them at the time, an essentially mercenaries group. It was the winning of the argument that mattered, not the truth. A modern reinterpretation is that the sophists may well have been early supporters of “subjective truth”, although this is not entirely clear, and that they certainly believed that a failure of rhetoric was a serious matter in public debate. It was not that they placed rhetoric above truth, rather, they held that no matter how true one’s argument it would fail to hold sway unless presented with the maximum rhetorical conviction.

What is to be learned from this foray to ancient Greece? The starting point was the contention that IMP research is sophistic, to be translated as excessively descriptive and full of clever reasoning designed to impress with rhetorical bravura. The authors have argued that this is a reasonable paraphrase of what Ford (1998), Valla and Salle (1997) and Gemunden (1997) have proposed. However, it is worth reflecting that the key lesson to be drawn from this discussion may be the exact opposite of that expected. Consider the proposition that IMP researchers have been insufficiently sophistic. There has been too great an emphasis on the pursuit of abstract truth, and insufficient attention has been paid to the important rhetorical job of demonstrating the rightness of that truth. Mattsson (1997) evaluated the "markets-as-networks tradition" and the "relationship marketing" school, concluding that the former (substantially overlapping with the IMP tradition) was based on scientific investigation of social phenomena, whereas the latter was managerially driven and scientifically ungrounded. Yet there is no doubt which of these two research streams holds greater attention for managers, journalists and industry commentators. A triumph for presentation over substance? Perhaps. And, if so, arguably also a triumph for sophistry. In which case, the message for IMP researchers would be to improve their rhetoric. There may be a modicum of satisfaction to be had in knowing that you are right and the other guy, whom everyone believes, is wrong. Not much, though.

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IMP as relevance lost

Integral to the charge of sophistry against the IMP research tradition is the contention that little contribution has been made to management practice. If a series of resounding successes could be cited, practical tools developed from IMP research papers, case studies of firms which have adopted such tools and thrived as a consequence, surveys of firms which have used IMP methods and whose performance exceeds a carefully matched control sample who have not, then this contention would be manifestly invalid. Such studies are notable by their absence. Indeed, such tools are notable by their absence. Certain recent contributions to the IMP oeuvre suggest that this may be a matter of concern to prominent interaction and networks researchers. Gemunden (1997) concluded that the most common category of IMP research paper included no empirical data, and that “IMP researchers have rarely analysed explicitly how, when, why, and to what extent relationships and networks really do influence the performance of a firm” (Gemunden 1997, p.9). Valla and Salle (1997) suggested that simply as a result of the passing of time and radical changes observed in the business world during the 1980s and 1990s, the IMP view of interaction and networks may be of declining relevance. Ford (1998) reinforced Valla’s call for a review of research priorities in the IMP tradition, and a revisiting of former ‘truths’, while drawing attention to a gap between IMP research and the wider reality of business markets or management practice.

In essence, the contention here is that IMP research was once “true”, but is increasingly less so. If, as seems plausible, truth and managerial relevance are positively related to each other, then the decay of truth has also led to a loss of managerial relevance. There may be a parallel here with the notion of ‘relevance lost’ in the field of cost and management accounting. Johnson and Kaplan (1987) argued that management accounting was developed in the early part of the twentieth century to deal with the information and control problems presented by large, multi-divisional companies. However, this clear relevance to the needs of business was lost, changes in business practice and in the business environment were not matched by changes in management accountancy theory, rendering much accounting data irrelevant to the decision-making needs of the firm. Is it plausible to argue that something similar has happened in the field of interaction and networks research, that the relevance, the closeness to industrial practice, achieved in the 1980s, has been lost as researchers have failed to notice the world leaving them behind?

IMP as (absence of) technology transfer

In conducting scientific research into inter-organisational relationships and networks over a period of two decades, the IMP research tradition has laid the groundwork for a managerial technology of relationship and network management. The fundamental purpose of a recent textbook written in the IMP tradition (Ford, Gadde, Hakansson, Lundgren, Snehota, Turnbull and Wilson 1998) seems to be to provide a synthesis of this managerial technology. However, it could be argued that the problems associated with (managerial) technology transfer have been under-estimated by IMP researchers. Consider the views expressed by Williams and Gibson (1990, p10): “To make technology transfer successful requires overcoming the many barriers to communication encountered when individuals use different vocabularies, have different motives, represent organizations of widely differing cultures, and
when the referents of the transactions may vary from highly abstract concepts to concrete products."

Scientists in virtually all fields face the same challenges in making the results of their research useful. For example, there is a major and continuing debate in healthcare about the need to, and the process of, improving evidence-based medicine. The contention in this paper is that insufficient attention has been paid to the transfer of IMP ‘technology’ from the academic community to the business community. There is a certain irony associated with the discussion of the transfer of IMP technology, since the management of technological development in industrial networks has, itself, been an important component of IMP research (e.g. Hakansson 1989, Gemunden and Ritter 1997). In this context consider the words of Hakansson and Snehota (1995, p343): “An issue that warrants special attention in the context of intensive industry-university co-operation is the transfer of the original design from the university to the industrial firm. Due to mutual misunderstanding and overrating/underrating, this transfer may be a major source of friction between both parties ... These problems may be significantly reduced if industrial firms and universities establish open communication and cooperate more closely during product development.” Just so. And might not these same observations be equally valid where the technology transfer concerns ideas on the management of relationships and networks, as when it is ‘hard’ systems which are the object of transfer?

The issue of the transfer of new managerial technologies, with a focus on systems for monitoring and evaluating success, was addressed by Mandell (1985). He pointed to a number of differences between managerial and production technologies which make the processes of technology transfer different. First, managerial technologies are more reversible and more prone to “partialization” than production technologies. It is relatively easy to re-impose the status quo ante when adopting a new managerial technology, and relatively easy to implement a new managerial technology bit-by-bit, implying a series of recurrent management decisions rather than one big adoption decision. Second, Mandell holds that managerial technologies are more “squishy” (sic) than production technologies. By this he means that managerial technologies often comprise a “loose bundled” collection of mix and match components, and that these components are flexible, are subject to modification during the implementation process. It follows that, compared to “hard technologies”, the adoption process for new managerial technologies is subject to a process of reinvention, and that an important role in applied research is to facilitate appropriate reinvention of the technology in the specific application context. On the basis of these arguments, Mandell concludes that “the primary way in which applied research can contribute to the adoption of managerial technologies is through monitoring and evaluation, rather than ex ante analysis” (Mandell 1985, p266). When seen in this light, Gemunden’s (1997) finding that “performance analysis” is conducted infrequently by researchers working in the IMP tradition assumes greater significance. Mandell concludes that monitoring and evaluation is central to the processes of applied research directed at facilitating the adoption of new managerial technologies: Gemunden concludes that IMP researchers seldom conduct performance analysis. Put these two conclusions together and you have a recipe for research with low managerial impact.

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In order to enhance the technology transfer process, it is necessary first to try to understand what are the barriers to effective university-industry transfer of managerial technologies. By drawing on the wider literature dealing with university-industry linkages, much of which is concerned with the transfer of production technologies, some insight can be gained into the barriers that have to be overcome. Moebus (1991) refers to the different aspirations and goals of the university and industry communities, an argument with which Wigand (1990) concurs, pointing to differences in purpose and philosophy between universities and firms. Even though Wigand sees little remnant of the "ivory tower" attitudes that once bedevilled university-industry relations, he argues that universities are still concerned more for public gain, while firms are concerned first for their own private gain. This is what Godkin (1988) summarises as "source-recipient incompatibility" in the technology transfer process. Dorf (1988) points to legal barriers (concerning licensing, contract matters, and exclusivity issues), and to a lack of incentives for individuals to become involved in university-industry personnel exchanges - the remuneration and promotions structures of the academic and business communities are incompatible. Stewart and Gibson (1990) provide a list of 72 linkage methods between universities and industry, which they classify into four headings - classroom linkages (teaching, lifelong learning); research linkages; publication linkages; and financial linkages. However, Stewart and Gibson counsel that the development of such linkages is inhibited by different attitudes towards the intrinsic worth of research, by difficulties involved in the bureaucracy associated with university-industry linkages, and by the incompatibility of university and industry reward systems. They argue that a dedicated linkage champion is needed if university-industry collaboration is to be successful, but observe that the job of overseeing such linkages (from either the university or the industrial end) is usually simply added on as an extra duty to the job of an already busy individual. To summarise, the key barriers to effective university-industry technology transfer appear to be:

- Different attitudes towards the intrinsic worth of research;
- Different preferences towards the public dissemination of research findings;
- Incompatibility of reward and promotions systems, which makes it difficult for individuals to move easily between the academic and industrial communities;
- Fundamentally different aspirations and goals of the two communities (what Godkin (1988) calls "source-recipient incompatibility");
- Bureaucracy involved in managing joint initiatives, which is reflected in a slower rate of progress than one or both partners would prefer (often, the industrial partner finds the academic bureaucracy frustrating);
- Legal barriers, notably different attitudes towards the exclusivity of research findings (the industrial partner wants exclusivity, the academic partner wants to disseminate the findings publicly and quickly);
- While there is a need for a dedicated linkage manager, neither party wants to resource such a position, preferring to add the responsibility to the portfolio of an existing employee.

To augment this list, one can add Wigand's suggestion that there may exist intrinsic mistrust between university and industry researchers: "The university-based researcher brings to industry the university’s assets - a long-term, more theoretically oriented perspective. The industry-based researchers brings (sic) to the university industry’s assets state-of-the-art..."
applications and knowledge of future trends. Often the assets of the other group are perceived as detriments; therefore, it becomes the job of the researchers to convince their colleagues in a language that they can understand that these qualities are valuable.” (Wigand 1990, p150, emphasis added).

Given that such a daunting list of potential barriers can be identified when a single university aims to develop collaborative relationships with industry, it is to be expected that the problems will be greatly magnified where a loose network of informally affiliated researchers, such as the IMP Group, pursues the same end.

**Conclusion and suggestions for further research**

It is hoped that the intrinsic irony of this paper is obvious. Certainly it contains no empirical data, no evaluation of performance indicators, and no managerial implications. Perhaps, therefore, it is quintessentially sophistic. Nevertheless, the issues addressed are of quite widespread concern. In the introduction the purpose of the paper was stated to be an evaluation of the thesis that IMP research has been sophistic and symbolic. To recast this in more straightforward language, the question is whether this substantial body of empirical and conceptual research, conducted by hundreds of highly skilled researchers in many different universities and many different countries, has had as much impact on management practice as it should. There has not been a great deal of concrete evidence to bring to bear on this central question. The work of Gemunden (1997) is fascinating, and pertinent, but addresses issues which are tangential to that of managerial relevance. The absence of empirical data, a predilection for conceptual model-building, the absence of evaluative studies, suggest a research tradition which does not put the development and testing of management technologies high on its list of priorities. Authoritative support for this point of view is provided by researchers such as Valla, Salle and Ford, but their assertions are more *cris de coeur* than propositions supported by tangible evidence. Nevertheless, it seems that there is a *prima facie* case for the argument that the body of IMP research is of limited relevance to management practitioners. There is scope for this *prima facie* case to be further investigated, and interesting problems remain to be overcome in coming up with an operational definition of “managerial relevance”, and in developing instrumentation to measure this kind of management technology. It is interesting that Sheth and Sisodia, discussing the achievements of marketing science as a whole, commented upon: “... the surprising paucity of instances in which academic research in marketing in the past two decades has resulted in widespread change in business practice” (Sheth and Sisodia 1999, p84). According to this proposition, the managerial irrelevance of IMP is but a microcosm of the greater managerial irrelevance of scholarly research in marketing.

Accepting for the moment that “irrelevance” is a fair charge, it is interesting to reflect for a moment on why this might be the case. To simplify, the charge is that the IMP body of scientific knowledge lacks application, or that the rate of application is far less than should have been expected from such a substantial weight of scientific endeavour. Explanations for this state of affairs can readily be split in two; first, explanations which contend that there is nothing there to apply, and, second, explanations which contend that there is something there to apply but that application has simply not happened yet. The first set of explanations (that

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there is nothing there to apply) is the basis for the thesis that IMP research is essentially
toxic and symbolic – who on earth would expect it ever to work? Studies in this genre
may be highly accurate, descriptive pictures of reality, but they contain no information on
underlying causal mechanisms and never could be used as anything other than representations
of a fast-receding reality (cf. Ford 1998). The parallel with “relevance lost” in management
accounting is strong here. The second set of explanations (that there is something to apply but
it has not happened) points to some breakdown in the technology transfer process. It would
not be enough simply to argue, as one might with an immature body of knowledge (which
IMP clearly is not), that application must follow the full development of the scientific ideas.
Rather, it is barriers to technology transfer such as lack of drive or lack of know-how on the
part of researchers, and to mutual ignorance (fear, mistrust?) between the technology
developers and the potential beneficiaries, which may be plausible explanations here.

Clearly, the solution, the means by which greater transfer of IMP technology into
management practice can be achieved, depends on the correct diagnosis of the problem. If the
problem lies in the nature of the body of knowledge – that it fails to illuminate the underlying
causal mechanisms within business networks and relationships – then an accelerated
technology transfer program would be a major waste of time. Nevertheless, there would still
be scope for an honest, sophisticated endeavour, to popularise IMP research (an “applied papers”
series). If the problem lies in “relevance lost” – IMP research was once germane to
management practice but has lost its way then in addition to simply going all out to regain
that relevance, the adoption of action research as a method can be advocated as one which
keeps the researcher’s eye on both corporate and scientific goals (Brown, Brennan and Frame
1999). However, probably the most likely diagnosis is that IMP research has generated much
that is applicable, but that application has seldom been high up the researcher’s list of
priorities. This would be neither surprising nor shameful. It is precisely the conundrum of
university-industry technology transfer, which is well-understood in the context of hard
science, but less well-understood in management science. The presumption may well be that
researchers in the management sciences can look after themselves a lot better than white-
coated laboratory scientists, when it comes to the interface with industry. After all, are not the
subjects of their research also, in many cases, the potential users of their work? Do they not,
therefore, have a deep and intimate understanding of the needs of the industrial community
that they putatively serve? But, of course, nothing is quite as simple as that. It is a reasonable
hypothesis that the cultural gap between management researchers and commercial
organisations is at least as great as that between hard science researchers in publicly funded
laboratories and firms’ R&D departments (scope for further research there). The technology
transfer process will take place no more “naturally” in the management sciences than in the
hard sciences. Furthermore, while the scientific status of the hard sciences is undisputed, and
the output of public laboratories legally protected to prevent ideas from being stolen, these
conditions do not apply in the management sciences. So, first there is the hurdle of getting
prospective beneficiaries of technology transfer to accept the scientific status of the work.
Second, there is a cadre of management consultants all too ready to pick up any likely piece
of management research, strip away the scientific basis, and peddle it as a “common sense”,
proprietary technology.

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In the case of IMP, there are certain other obvious hurdles to be overcome in the technology transfer process. These are the multi-culturalism of the research group, implying a multiplicity of views on appropriate methods (or fundamental appropriateness) of technology transfer, and the absence of a single, controlling institution. The latter makes it very difficult to conceive how "ownership" of IMP management technologies could be asserted (whether in legal or any other terms), which in turn renders it difficult to grant researchers the degree of protection that they require for their intellectual property.

However, this paper has not been written as a counsel of despair. It has been written in the knowledge that the IMP Group contains a large number of able and creative thinkers who, should they choose, will be able to develop solutions to the problems of technology transfer outlined here. That is the challenge the authors offer!

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