FIFTY YEARS OF IRRELEVANCE: THE WILD GOOSE CHASE OF MANAGEMENT SCIENCE

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ABSTRACT
Modern management science has existed since 1959 when two reports (by Pierson and Gordon & Howell) on the future of business education were published in the US. At least since 1980, there has been a practically continuous, but somewhat fragmented discussion on the relevance of management research. Although many different proposals have been made to rectify the situation, the mainstream of management research seems to be relatively untroubled and unaffected by this widely sensed irrelevance. The paper aims at initial understanding of the reasons for this spectacular failure of (general) management research to reach relevant results in the period of 1960-2010. Two related questions are considered in more detail. How was the social science turn of management science in 1959 justified and achieved? Which correctives have been proposed for management research, up to now?

KEY WORDS

Management science, irrelevance.

INTRODUCTION
Modern management science has existed since 1959 when two reports (Pierson 1959, Gordon & Howell 1959) on the future of business education were published in the US. At least since 1980, there has been a practically continuous, but somewhat fragmented discussion on the relevance of management research. Surprisingly, it seems that no synthesis has been made on this discussion that occurs in all major branches of the field. Although many different proposals have been made to rectify the situation, the mainstream of management research seems to be relatively untroubled and unaffected by this situation.

The paper aims at initial understanding of the reasons for this spectacular failure of management science to reach relevant results in the period of 1960-2010. This issue is important both for general management research and more specialized areas that draw from that, such as construction management, project management and operations management.

The paper is structured as follows. First, the situation of management science before 1959 is outlined. Then the suggestions in the 1959 reports are described. Next, the outcomes of implementing these suggestions are evaluated. Subsequently, reasons for the wide failure of management science to provide relevant knowledge are sought for. The paper ends with conclusions.

MANAGEMENT SCIENCE BEFORE 1959

In the beginning of the 20th century, management was essentially factory management. Only through the extension of productive activities and along with the

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enlarged firm sizes, general management as an activity emerged in the first decades of that century. Through its genesis, classical management science evolved as a technical discipline; it was intimately connected to production (design included) in three senses:

- The science of organization and (general) management was developed as an extension of production and industrial management (Wren 1994).
- The interest was to organizational engineering and design: prescriptive principles (for example, Fayol) and best practice descriptions
- Management was studied by engineers or managers of productive operations, by persons involved in the phenomena studied (Shenhav 1999). This is exemplified by Taylor and Fayol.

Surely, classical management science had its serious weaknesses. There was no solid methodology in use, and hardly any systematic empirical evidence. The disciplinary structure of organization and management studies was nascent, at best confused.

THE 1959 REPORTS

It is well known that the current understanding on management science and research has been strongly influenced by two reports from 1959, funded by the Carnegie Foundation and the Ford Foundation (Gordon & Howell 1959, Pierson 1959). In their suggestions, the reports blazed a trail for a social science understanding of management science. In making these suggestions, the reports distanced from and discredited the classical management and organization science that had evolved from the beginning of the 20th century.

WHAT DID THE REPORTS SUGGEST?

In the prescription of these reports, management was to be approached through three root stems: behavioural science, economics and quantitative modelling. These stems already existed. The behavioural stem had been promoted by Simon, March and others. In economics, the neoclassical doctrine had just been consolidated and seemed to provide a firm foundation for understanding decision-making. Quantitative modelling was in good currency after the successes of operations research in the World War II and also through the prospect of using computers to facilitate modelling.

In addition, teaching and research was to be organized in so called functional fields, such as production, marketing, finance, human relations, etc. These were understood as application areas for the (general) management theories and methods.

All in all, in comparison to classical management science, the 1959 reports suggested a radically different direction:

- Management and organization science was seen as falling into social sciences.
- Research had to result in empirical generalizations about behaviour.
- Research was to be done by scientists external to the phenomena studied.

IMPLEMENTATION OF THE SUGGESTIONS AND ITS OUTCOMES

The mainstream research work on management in business schools started to follow the guidelines presented in these reports. The behavioural stem gathered especially around Academy of Management Journal, whereas Management Science, which had been established in 1954, acted as the flagship for quantitative modelling. In contrast to the two other stems, the economics stem did not create any new scholarly area with a clear identity. Rather, topics of interest for management were studied in the framework of general economics, perhaps reflecting the view that issues pertaining to management and organization are inseparable ingredients of the economic doctrine.

SOCIAL SCIENCE ORIENTED MANAGEMENT RESEARCH

Assume that we have accounts from two exploration parties, each visiting an unmapped island, the location of which is not precisely known. Assume further, that these accounts are coherent, topic by topic. We are justified to think, first, that it is the same island that is being described, and secondly that the agreement of the two independent accounts adds to their trustworthiness. As oddly as it may sound, we have a somewhat similar situation regarding the mainstream management science. In two Harvard Business Reviews articles separated by 21 years (Behrman & Levin 1984, Bennis & O'Toole 2005), knowledgeable insiders of academic management science come up with a surprisingly similar diagnosis on management research in business schools; hardly anything has changed. Table 1 gives a self-explanatory overview on the similarities in these two papers.

These two articles are by no means outliers. One of the first overviews on critical views on relevance of management science was the paper by Thomas and Tymon (1982), which referred to several earlier criticisms from 1972 onwards. Also, the discussion on irrelevance is not only an American phenomenon; rather similar discussion has been carried out in the UK (Starkey & Madan 2001, Tranfeld 2002). Cogently, Tranfield found that there was a strong view that much management research was unreliable for use by both the academic community and particularly practising managers in providing a basis for justifying their decision-making and actions.

QUANTITATIVE MODELLING

Operations research had its heyday in the 1960s and 1970s. However, in 1979, Ackoff bitterly attacked the developments in operations research:

> The meetings and journals of the relevant professional societies, like classrooms, were filled with abstractions from an imagined reality. As a result OR came to be identified with the use of mathematical models and algorithms rather than the ability to formulate management problems, solve them, and implement and maintain their solutions in turbulent environments.

Ackoff's attacks initiated a fierce debate. Checkland (1983) commented some years later that in that debate the divorce of theory from practice is no longer taken as requiring proof; it is taken as a given. It has been presented that after the 1980's, operations research has been on the decline.

ECONOMICS

In 1985, Kuttner wrote an article in the Atlantic Monthly that strongly criticized the discipline of economics: "...departments of economics are graduating a generation of *idiots savants*, brilliant at esoteric mathematics yet innocent of actual economic life." However, wider discussion on irrelevance of economics was ignited only a decade later, in 1996, again on a forum external to economics: the magazine New Yorker Cassidy's (1996) article had a simple message: "...that a good deal of modern economic theory, even the kind that wins Nobel Prizes, simply doesn't matter much." The article succeeded in stimulating debate both among economists and laymen.

Table 1. Textual comparison of (Behrman & Levin 1984) and (Bennis & O'Toole 2005) regarding irrelevance of management research.

Topic	*Behrman & Levin 1984*	*Bennis & O'Toole 2005*
Sources of criticisms	The current criticisms of business schools (which come from the business press, corporate officers, the deans themselves, journalists, and other professional observers)[...]	These criticisms come not just from students, employers, and the media but also from deans of some of America's most prestigious business schools, [....]
Scientific approach as a root cause	The numbers orientation: By the early 1960s business school curricula showed a large increase in the number of quantitative courses such as management science and operations research on the one hand and behavioural science courses on the other hand.	During the past several decades, many leading B schools have quietly adopted an inappropriate - and ultimately self-defeating - model of academic excellence. Instead of measuring themselves in terms of the competence of their graduates, or by how well their faculties understand important drivers of business performance, they measure themselves almost solely by the rigor of their scientific research.
Incompatibility between problems and methods	Since real problems have an annoying habit of being difficult to solve, legions of the "new scholars and their undergraduate and graduate disciples promptly set about applying their new sciences to unreal problems, that is, to all those that would yield to these new models [...]	When applied to business-essentially a human activity in which judgments are made with messy, incomplete, and incoherent data-statistical and methodological wizardry can blind rather than illuminate.
Irrelevance of research done and published	In fairness, some research breakthroughs have been useful in managerial contexts, [...]. But, for the most part, given the thousands of faculty members doing it, the research in business administration during the past 20 years would fail any reasonable test of applicability or relevance to consequential management problems or policy issues concerning the role of business nationally or internationally.	To be fair, some of what is published in A-list journals is excellent, imaginative, and valuable. But much is not. A renowned CEO doubtless speaks for many when he labels academic publishing a "vast wasteland" from the point of view of business practitioners. In fact, relevance is often systematically expunged from these journals.
Professors are evaluated based on their publications	Any good and rising young professor had only to prove that he could communicate with those who *were* interested – his colleagues.	Another consequence of the scientific model is that professor's evaluations are influenced by the number of articles they publish in A-list business research journals.
Journals become solely academic	Most academic business journals have consequently become inhouse (within discipline) organs rather than a means of communicating with those involved in management procedures and business leadership.	[...] the system creates pressure on scholars to publish articles on narrow subjects chiefly of interest to other academics, not practitioners.
Lack of relevance of journals; management must get help from elsewhere	The serious policy issues management faces tend not to be addressed in "academic" journals. Managers must get help from other quarters.	In fact, relevance is often systematically expunged from these journals. Practitioners who have to make real decisions, however, must meanwhile look elsewhere for guidance, notably to the business press and to the bestseller list-now home to fewer and fewer books by faculty members.

The kernel of the criticism is aptly summarized by Blaug (1997):

> Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigour is everything and practical relevance is nothing.

The economic crash in 2008 added further weight to such calls for a renewal (Hodgson 2009).

PRODUCTION/OPERATIONS MANAGEMENT

It is of course of special interest how production management coped with the re-orientation of management science away from production in 1959. The starting points were indeed not good. Buffa (1980), who wrote one of the first post 1959 text books on production management, comments[2]:

> Being left with what we knew about production systems at that time was to be left with a nearly empty basket of techniques: time and motion study, plant layout, Gantt's production control boards, the simple EOQ model, and simplistic descriptions of how production systems worked.

In this situation, the majority of production management scholars turned to quantitative methods. However, the problem of fragmentation plagued the field (Buffa 1980):

> ...looking at research in the field before and after the MS/OR revolution, it appears that we have learned a great deal about inventories, scheduling, aggregate planning, quality control, capacity planning, and so on, in the sense of models of those isolated subsystems. We have not learned very much about the relationship between these subsystems; we view the field as a collection of seemingly unrelated subsystems rather than as whole systems (there are exceptions).

Later, Portougal and Robb (2000) commented that scheduling research undertaken for more than 40 years has done little to improve production planning practice. Thus, not even this field seems to have avoided the problem of irrelevance; perhaps with some understatement, Slack & al. (2004) state:

> Yet despite the apparently overwhelming practical focus of academic OM, it also appears to have a history that demonstrates anxiety about how "helpful" to operations practice it is really being [...]

DISCUSSION ON OUTCOMES OF IMPLEMENTATION

In connection to the 50 year anniversary of the business education reports of 1959, they have been commented in a largely positive tone (Anon. 2009), although pinpointing that Gordon & Howell (1959) called for better research, and that in this regard, there is still much room for improvement. In other words, there is a slight problem of implementation of the 1959 recommendations.

It is argued here that such an assessment is misinformed: the poverty of current management research has been directly caused by the very recommendations of the two reports. All the three stems of management science have miserably failed; the functional fields, spearheaded by production/operations management, do not seem to have fared any better.

Indeed, with the benefit of 50 years hindsight, it can now be convincingly argued that the direction proposed in 1959, and closely followed by the management scholar

[2] We can argue that Buffa (and his colleagues) failed to see many conceptual and methodical gains existing, for example the quality methodologies and their underlying concepts.

community, has been utterly wrong. It has led to a massive, discipline wide idling of management science.

Another striking feature is the helplessness and inertia of the scholarly community in rectifying the situation, as illustrated through the above mentioned two almost identical diagnoses, separated by 21 years. This has not been a period of the Kuhnian normal science, focusing on remaining pieces of the puzzle and waiting to be replaced by a new paradigm when exhausted. Rather, would this be more aptly characterized as cargo cult science (Feynman 1974), where just the external forms of research are followed, without reaching to the essence of the phenomena in focus?

These observations and judgements raise many serious and difficult questions. We briefly consider two questions arising. First: how was the social science turn of management science in 1959 justified and achieved? Second: which correctives have been proposed for management research, up to now?

SOCIAL SCIENCE TURN IN MANAGEMENT SCIENCE AS A PARADIGM SHIFT

The reports of 1959 achieved a social science turn in management and organization theory, which up to that point had been largely been developed as a technical field oriented around production. How did this social science turn happen?

Cutting the connection of management science to production

In practice, the suggestions in the 1959 reports meant that the connection of management to production, which earlier had been the conceptual starting point, was to be cut off. This was realized by reconceptualising organizations around decision-making, and around the interplay between individual and organization. These ideas did not emerge in an intellectual vacuum. Rather, a sense of general hostility to the production centric paradigm was clearly visible. Gordon and Howell (1959), two economists, repeatedly make negative comments on all things related to production – by way of example:

> Production management courses are often repository of some of the most inappropriate and intellectually stultifying materials to be found in the business curriculum. Not only do many faculty members have little respect for such courses, but students in a number of schools complained.

It is not difficult to find the probable inspiration to this attitude. Production had been purged out of economics somewhat earlier (Koskela 2011), with comparable attitudes and arguments. One of the leading proponents of this purge, Robbins (1935), wrote about the old paradigm in economics:

> It should not be necessary at this stage to dwell upon the inappropriateness of the various technical elements which almost inevitably intrude into a system arranged on this principle. We have all felt, with Professor Schumpeter, a sense almost of shame at the incredible banalities of much of the so-called theory of production…

A parallel trend existed in organizational science. In his seminal book on administrative behaviour (first edition in 1947), Simon (1976) states:

> In the post-industrial society, the central problem is not how to organize to produce efficiently (although this will always remain an important consideration), but how to organize to make decisions – that is, to process information.

In March' and Simon's (1958) book "Organizations", the contempt of the technical understanding went even further: the importance of organizations is derived from the fact that people spend so much time in them - rather than from the production purpose, which is not even mentioned.

Rejecting production as an independent scholarly field

Moreover, production as an independent scholarly field was to be rejected; rather production was to be seen as a functional field, best approached through the underlying disciplines. Say Gordon & Howell (1959):

> In the world of business, the so called functional fields (e.g., marketing and production) provide the major problem areas, short of general management, for the exercise of decision-making and tool-using abilities.

Pierson (1959) writes:

> If the functional business subjects are cut off from their underlying disciplines, as often tends to be the case, they are likely to become pedestrian and narrow, but if they are studied as integral parts of broader fields, they can become both challenging and meaningful. [...] Thus, the study of production should keep particularly close ties with mathematics, engineering and the sciences;...

More specifically, the division of work should be as follows (Pierson 1959):

> Putting the components together, we may generalize the complete decision process in production problems as follows: (1) the development of physically feasible alternatives, (2) identification of the more economical of these alternatives, (3) final choice of one alternative based on the human aspect involved. The first step is essentially engineering (applied physical sciences); the second step is essentially applied micro-economic theory; the third step is an application of the behavioural sciences, usually through judgement.

Thus, the consideration of production was divided among engineering, economics and behavioural sciences, and no space was left for any independent production theory or discipline.

Positive knowledge

Research leading to “positive” knowledge (generalization on behaviour) as well as methods and tools for decision making was encouraged. Instead, research oriented towards “principles” of classical management science, that is prescriptive knowledge, was discouraged. Similarly, practice-oriented R&D was implicitly discouraged.

Fate of the old paradigm

All in all, practically all major characteristics of the old management paradigm were thus discredited, and it soon fell into oblivion. Only a few defenders of classical management science, such as Koontz (1980), tried to mobilize for a counterattack, but it came to nothing.

CORRECTIVES SUGGESTED

During the long period of discussion on the relevance problem, of course a large variety of correctives (as well as defences) have been presented. However, a surprisingly high number of such correctives go counter-current, towards the things rejected in 1959.

Connecting organization theory back to production

Since 1959, production has been almost a taboo in organization science – it has simply not been discussed. In alignment with this, organizational theory has avoided the phenomena of work or materiality, both issues belonging to production. However leading organizational theorists are ready to criticize this situation. In a paper titled “Taking work back in”, Barley & Kunda (2001) argue:

> …we argue that organization theory's effort to make sense of post-bureaucratic organizing is hampered by a dearth of detailed studies of work. We review the history of organization theory to show that in the past, studies of work provided an empirical foundation for theories of bureaucracy, and explain how such research became marginalized or ignored."

Orlikowski (2007) writes:

> Over the years, the field of organization studies has generated important and valuable insights into the cultural, institutional, and situated aspects of organizing. However, I want to argue that these insights are limited in large part because the field has traditionally overlooked the ways in which organizing is bound up with the material forms and spaces through which humans act and interact.

It can be argued that these calls provide strong circumstantial evidence for the neglect of production and the need to rectify the situation. Regarding Barley's and Kunda's call, of course it has to be noted that work does not exhaust the phenomenon of production. Work is about what people do to objects of work. Production is also about what happens to objects of work in production and about what happens to the cause of production: customer voice. Regarding Orlikowski's call, these "material forms and spaces through which humans act and interact" are often, if not mostly, embodied in the respective production system.

Reviving production as a discipline and theory

One of the original promoters of the social science turn of management science, Simon, soon came to other thoughts. In (Simon 1969), he wrote:

> Natural science is knowledge about natural objects and phenomena. We ask whether there cannot also be "artificial science" - knowledge about artificial objects and phenomena.

Simon continued by explaining that a science of the artificial will be closely akin to a science of engineering: it is concerned how things ought to be, in order to attain goals, and to function. He remarkably presented business as one example of professional fields where this science applies.

Another approach to revive production as a theoretical field is that of the author (Koskela 2000). He argued that there are three mostly implicit theories on production in use: transformation, flow and value generation theory of production. In this presentation, for the first time, it is possible to pinpoint probable causes for this lack of explicit scholarly treatment of theories of production: the 1959 reports which denied production as an independent topic for theorizing.

Alternative ways to knowledge

Already in 1978, Susman and Evered suggested action research as a suitable type of research in organizational science. Somewhat later, often influenced by Simon's arguments for the science of the artifial, calls for constructive or design science research in accounting (Kasanen & Lukka 1993), information systems (March & Smith 1995, Hevner & al. 2004) and management research in general (van Aken 2004, Boland & Colloby (2004) were presented. The common feature in these calls was that the end result of research is seen to be a new artefact or technological rules on how a certain goal can be achieved. Thus, the goal is not to describe the world but to change it. Of course, these technological rules are near the "principles" of classical management science, poured scorn on by Simon (1976).

Another related corrective is "type 2 research", essentially co-production of knowledge (Starkey & Madan 2001). The central idea is close collaboration between the researcher and the manager, whose essential role is to pinpoint relevant problems.

Conceptual research is one more corrective forwarded. In another remarkable turnaround (besides Simon), March (Reed & al. 2000) belittles the sacred topics of the 1959 reports, and stresses the importance of conceptual gains:

> The key role of the university is not in trying to identify factors affecting organizational performance, or in trying to develop managerial technology. It is raising fundamental issues, and advancing knowledge about fundamental processes affecting management.

Conclusion

There has been a wide interest in correctives that factually equate to the production centric features of pre 1959 approach to management, which were pushed aside in the social science turn.

CONCLUSIONS

There are three major conclusions from this broad brush examination. First, the 1959 reports on business education have failed, throughout, to give appropriate direction for management research; the outcomes have not passed the test of relevance. Second, in spite of extensive (although somewhat myopic) discussion on irrelevance in the management scholar community from circa 1980 onwards, not much movement towards rectifying the situation can be seen. Thirdly, judging by the way the social science turn in management science happened, and at the correctives suggested, it is plausible that the ousting of production from management science in 1959 has been one major contributing factor to irrelevance across managerial sub-disciplines.

Management is important as a phenomenon and management science is an important scholarly field, which has a considerable influence on more specific managerial fields, like construction management and project management. Unfortunately, the self-complacent acceptance of irrelevance that currently radiates from management as a scholarly field is a dangerous disease. The situation seems to invite urgent volunteer efforts from all directions to find a cure.

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