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Why is management research irrelevant?

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Abstract

At least since 1980, there has been a practically continuous, but somewhat fragmented discussion on the relevance of management research. This discussion has addressed practically all fields of management; here, besides general management, operations management, project management and construction management are examined in more detail. Although many different proposals have been made to rectify the situation, no definitive resolution has been found. In this paper, it is argued that prior analyses have not reached the root causes of the irrelevance problem. By an analysis of the recent history of management research, the following novel findings are reached. First, the root cause of the irrelevance is argued to lie in the 1959 reports on American business education, written by Pierson and Gordon & Howell. Second, while the proposed direction in the 1959 reports was deficient in several ways, the rejection of production as an integral part of organizations and management has been perhaps the most damaging feature of those reports. Third, current research on management suffers from a variety of immediate causes for irrelevance, insufficiently recognized by the scholarly community. It is suggested that reaching the root causes for irrelevance will facilitate finding suitable cures.

Keywords Irrelevance, management research, organizational theory, quantitative methods, economics, operations management, project management, construction management

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Introduction

"But there is a choice to be made: sit in my office and make up problems or go out and find real ones." When the audience saw this on the slide of the admired Professor Spearman, speaking at the POMS 27th Annual Conference 2016 in Orlando[i] on "Relevance in the age of analytics", many expected to hear a strong call for finding real problems to work on. However, against all expectations, he instructed that associate professors and PhD students should stay in their offices and make up problems to solve, for publications; addressing real life problems should be taken care of by tenured professors.

The problem of irrelevance of management research has been discussed extensively and over many years; this resignation in front of the irrelevance problem, as shown by Spearman, is both puzzling and understandable. How can it be that thousands of management scholars, gifted and capable, should have gone so badly astray? On the other hand, the sharpest minds of the field have addressed the irrelevance problem but without a resolution. What then can one academic do?

The ambitious aim of this paper[ii] is to present such new understanding on the irrelevance problem that can be hoped to stimulate new solutions. Against the backdrop presented, the question immediately arises: what is the new approach to the irrelevance problem that would lead to novel understanding? I contend that the previous discussions have been myopic (looking at one managerial discipline only) and shallow (not determined to find the root causes, often because of prematurely jumping to a solution). Thus, in this investigation, the disciplines across the field of management will be covered, and through a critical historical analysis, the root causes will be pursued.

The issue of irrelevance is clearly important both for general management research and more specialized areas drawing from that, such as operations management, project management and construction management. However, it is also an inflammatory and controversial topic. The often sweeping statements made on the alleged irrelevance of management research may sound strange, unreasonable and implausible for a person who encounters them for the first time. In view of this, the reasons for which we should believe our sources, those who have critiqued management research, are emphasized more than usually. Some analyses of cases of irrelevance are presented; these will help to understand why irrelevance has emerged – why the thinking leading to irrelevance has seemed fully logical.

The paper is structured as follows. First, the long-standing discussion on irrelevance in management research is examined, by way of introduction to the topic. That discussion points, as a possible source of irrelevance, to the direction taken in 1959 regarding business education. The background, the contents and the overall impacts of these 1959 policies are analyzed next. Then, the evolution of management research from that point to the present time is examined, focusing on the factors leading to irrelevance, as well as the ways irrelevance gets embodied. A further analysis is made of the correctives proposed to the direction pinpointed by the 1959 reports. Finally, the 1959 reports on business education are compared to corresponding reports in medicine and engineering – areas where similar discussion on the irrelevance of research has not broken out. The paper ends with conclusions regarding the reasons for the wide-ranging failure of management science to provide relevant knowledge.

The long-standing discussion on the irrelevance of management research

Let us 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 between 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 Review* articles, separated by 21 years (Behrman & Levin 1984, Bennis & O'Toole 2005), knowledgeable insiders within academic management research (at the time of publication, the authors of the former article were associate deans and those of the latter, professors at a business school) come up with

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surprisingly similar diagnoses on management research in business schools. Table 1 gives an overview on the similarities in these two articles.

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	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.

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	test of applicability or relevance to consequential management problems or policy issues concerning the role of business nationally or internationally.	
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 professors' 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 two articles identify almost identical sources of criticism: media, employers and deans of business schools, one article also mentioning other professional observers and the other students. As the root cause of the problem, one article pinpoints the numbers orientation as shown in management science and behavioural sciences, while the other mentions the rigour in scientific research – arguably the same issue is being meant. Both articles then discuss the incompatibility between the methods used on one hand and the real business problems on the other.

The consequential irrelevance of research done and published is funnily enough discussed in almost the same format, "to be fair…., but…", with emphasis on what comes after "but": a strong statement on the irrelevance of "most" or "much" of research. How this irrelevance is continually reproduced is similarly discussed: for academics to progress on their academic career, they have to publish in leading

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journals, which have become purely academic. Managers have to find knowledge from other quarters for their business problems.

Two conclusions can be made based on these two almost identical diagnoses, separated by 21 years. First, the stability of the anomalous situation is striking; hardly anything has changed. Another prominent feature is the helplessness and inertia of the scholarly community to rectify the situation. A root cause is identified – quantitatively oriented science – and the pattern through which the situation is reproduced, but the analysis hardly goes any deeper or provides plausible remedies.

It is worth mentioning that these two articles are by no means outliers. One of the first overviews on critical views on the 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 this irrelevance is not only an American phenomenon; rather similar discussion has been carried out in the UK (Starkey & Madan 2001, Tranfeld 2002). In alignment with the American observations, Tranfield found there was a strong view that much management research was unreliable for both the academic community, and particularly practising managers, in providing a basis for justifying their decision-making and actions.

All in all, it can thus be suspected that the numbers-oriented, rigorous science is, for its part, to be blamed for the irrelevance of management research. But when was this ideal for doing research adopted in management research, and why?

The emergence of modern management research

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 Corporation and the Ford Foundation (Gordon & Howell 1959, Pierson 1959). However, to fully understand the direction shown in the reports and its implications, the prior evolution of management thinking and the general intellectual climate have to be examined.

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Evolution of management thinking and related intellectual trends up to 1959

In the beginning of the 20th century, management meant essentially factory management. Only through the expansion of productive activities and along with the growing firm sizes, general management as an activity emerged in the first decades of the century. Through its genesis, classical management science evolved as a technical discipline [iii]; it was intimately connected to production (design included) in the following three senses:

- The science of organization and (general) management was developed as an extension of production and industrial management (Wren 1994).
- The interest was in organizational engineering and design: prescriptive principles (for example, by 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.

Classical management science had its serious weaknesses. There was no solid methodology in use, instead the approach was rather experiential, and thus there was hardly any systematic empirical evidence. The disciplinary structure of organization and management studies was nascent, at best confused.

On the other hand, it cannot be denied that the outcomes of economical development, with improvement of technology, management and organization as one input, were impressive. Especially in the US, productivity had considerably risen in the years 1928-50, an exceptional phenomenon dubbed the “one big wave” (Gordon 2010). Mass manufacturing and electrification of manufacturing contributed to this phenomenon. An unprecedented affluence thus prevailed in the 1950’s, leading the Harvard economist J.K. Galbraith (1958, p. 146) to declare that the preoccupation in economics on production and productivity was obsolete:

> The effect of increasing affluence is to minimize the importance of economic goals. Production and productivity become less and less important.

Thus (Galbraith 1958, p. 138):

> Our preoccupation with production, in other words, may be a preoccupation with a problem of a rather low urgency.

These ideas of Galbraith came to be widely discussed, and for their part influenced the intellectual trends of the late 1950's.

In fact, the prior predominance of production in not only managerial and organizational but also economic thinking had started to gather criticism already somewhat earlier. Especially economics was active in this endeavor, and a purge of production out of the economic theory was in full swing. One of the leading proponents of this purge, Robbins (1935, p. 65), 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), the young Simon (1976, p. 292) 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.

Simon's logic is fallacious as production remains the topic of a major part of decisions (Koskela & Ballard 2012). Even worse, Simon's early attack on the previous production-centric "organizational principles" school, represented by Gulick, shows in critical analysis more zeal than logic[iv] (Georgiou 2013, p. 1015):

> … Simon's critique suffers from flawed and misleading argumentation, semantic incoherence, naïve simplicity, disproportionate emphasis, implied imputation,

> misdirected logic, historical misinterpretation, contextual overshooting, methodological incommensurability, false reproaches, misguiding charges, and an etiological approach unequipped to deal with complex webs of interrelationships.

In March and Simon's book, *Organizations* (1958), the contempt of production 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 function[v], which is not even mentioned.

The reports of 1959

In 1959, two reports (Pierson 1959, Gordon & Howell 1959) on the future of business education were published in the US. The reports were motivated by a wide dissatisfaction regarding the state of business schools. They had been funded, respectively, by Carnegie Corporation and Ford Foundation. In their remarkably similar suggestions, the reports blazed a trail for an understanding of managerial sciences based on social 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.

The similarity of the contents in these reports is explained by the fact that there was a network of sponsors and academics, with the agenda of boosting behavioural science research in business schools. Thus, the program officer for behavioural science at the Ford Foundation, Berelson, had written already in 1951 (Crowther-Heyck 2006, p. 323):

> "…the critical problems which obstruct advancement in human welfare and progress toward democratic goals are today social rather than physical in character"

The staff at the Graduate School of Industrial Administration at Carnegie-Mellon, including its dean, G. L. Bach, as well as Herbert Simon and James March, were actively influencing the report work in the background (Crowther-Heyck 2006, p. 321):

> Indeed, one could say without exaggeration that the GSIA staff did more than anyone

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> to create not only the present model of the MBA but also the idea that a business school should be a research institution. Bach played a particularly important missionary role in this endeavor, as he was intimately involved in both the Ford Foundation and Carnegie Corporation reports on business education that were published in 1959 (Gordon & Howell, 1959; Pierson, 1959). These "Flexner Reports for Business Education" went along with a $35 million program of grants to business schools by the Ford Foundation.

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

In addition, teaching and research were to be organized in so called functional fields, such as production, marketing, finance, human relations, etc. These were seen 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.
- Research had to be done preferably through quantitative/mathematical methods, either analytically or statistically.

In the following, these new directions are considered in more detail.

Cutting the connection of management science with production

In practice, the suggestions in the 1959 reports meant that the connection of management with 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 the individual and the organization. As discussed above, these ideas did not emerge in an intellectual vacuum. A sense of general hostility to the production-centred paradigm is transparent in the reports. Gordon and Howell (1959, p. 190), two economists, repeatedly make negative comments on all things related to production – for 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.

Moreover, production as an independent scholarly field was to be rejected; instead, production was to be seen as a functional field, best approached as an application area for management or through the underlying disciplines. Says Pierson (1959, p. 311), also an economist:

> 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.

Further, Pierson (1959, p. 215) wants to see production in relation to the underlying disciplines:

> 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, p. 492):

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> 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 created by scientists external to the phenomena in focus

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

Similarly to established fields of scientific research, it was required that scientists (business school academics) should be external to the phenomena studied, for ensuring objectivity.

Quantitative/mathematical methods

The reports emphasized statistical methods, especially in the context of behavioural research, and “analytical tools” in the meaning of algebraic formulas. For Gordon and Howell (1959), the role of mathematics and statistics equalled to that of substantial underlying disciplines: “This in turn requires that the business schools turn for help to the underlying disciplines such as the behavioral sciences and mathematics and statistics, as well as economics”.

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Evolution of management disciplines after 1959

The recommendation of the 1959 reports started to be implemented in a surprisingly active manner. One reason for this was the funding by the Ford Foundation for renewal initiatives in business schools. Another, institutional reason will be considered in a later section.

Koontz (1980, p. 176), a representative of more traditional views on management research, sourly comments:

> (…) the famous Ford Foundation (Gordon and Howell) and Carnegie Foundation (Pearson) reports in 1959 on our business school programs in American colleges and universities, authored and researched by scholars who were not trained in management, indicted the quality of business education in the United States and urged schools, including those that were already doing everything the researchers recommended, to adopt a broader and more social science approach to their curricula and faculty. As a result, many deans and other administrators went with great speed and vigor to recruit specialists in such fields as economics, mathematics, psychology, sociology, social psychology, and anthropology.

The analysis of Goodrick (2002) shows that the share of mathematically framed papers in *Academy of Management Journal* increased from roughly one quarter in the period 1959–1966 to almost 100 % in the period 1972–1978. Goodrick (2002, p. 649) interprets this so that "the shift from a management as a vocation model to one that is scientifically based" had thus occurred in the timespan addressed. Of course it must also be noted that the irrelevance discussion started just as this transformation had reached its completion.

However, we have to look at the different streams of management research to fully understand the influence of the 1959 reports. In the following, the three root stems are first examined. Then one functional area [vi], namely production management, is selected for scrutiny, as it is a special case (to be discussed below). Finally, the evolution of two specialized management disciplines, project management and construction management, is addressed to gauge the influence of the 1959 reports on

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areas not directly treated in them.

The three root stems

Social science oriented management research

The behavioural stem had started to gather especially around *Administrative Science Quarterly*, established in 1956, and *Journal of the Academy of Management*[vii], created in 1958. The social science oriented management research covers a wide domain. In the following, the major organizational theories, a prominent focus area in the field, are addressed.

The current editor of *Administrative Science Quarterly*, Gerald Davis, has in several recent papers (2010, 2015a, 2015b) presented devastating critique of the current status of organizational and management theory. He contends that by 1970, the field had produced six major theories[viii]: contingency theory, transaction cost economics, agency theory, resource dependence, population ecology, and new institutional theory. Unfortunately, since their creation, there has hardly been theoretical progress or accumulation of knowledge in terms of these theories. He finds three major reasons for this state of affairs. First, a lack of experimental control – however, this important issue related to research methodology cannot be discussed further in this investigation.

Second, he contends that as organizations are human-designed tools rather than objects occurring in nature, there is little reason to expect law-like statements to hold up across situations.

Third, empirical generalizations may change over time (Davis 2015a, p. 311): "…statistical relationships discovered in one era were prone to disappearing in the next era." Davis (2010, p. 703) has an admirably evocative metaphor for this: "Like a cadaver that keeps jumping up from the autopsy table, the empirical generalizations derived from the study of organizations often get away from us as time moves on."

A further problem is that such theories cannot necessarily be validated or falsified. For example, regarding the institutional theory, Davis states: "For all purposes, this theory cannot be tested or corroborated as it is written." He ends up with the view that "…seeking increasingly "precise" or "general" theories about organizations is probably a pointless endeavor".

Davis suggests, on one hand (2010), more modest aims for organizational theorizing in view of these intrinsic limitations to general, predictive or precise theories, and on the other hand (2015a), a focus on problem-driven (rather than theory-driven) research.

Davis fails to mention one more generic difficulty, observed by Davies (2006). Empirical research is based on data on existing organizations, and this does not allow the generation of novel insights into organizing. Says Davies (2006, p. 2):

> The vast majority of academic research in management is concerned to explain extant phenomena, not to provide solutions to problems. As the emphasis is on the evaluation of existing practices, such work is necessarily backward-looking and can tell us nothing about the construction of hitherto unknown solutions to problems.

Quantitative methods

The stem of quantitative methods focused especially on operations (or operational) research, which successfully expanded both in industrial practice and as an academic discipline in the 1960s. The first journal in the field, *Journal of the Operations Research Society of America*, had been established in 1952 (and renamed *Operations Research* in 1955), followed by the launch of *Management Science* in 1954.

Rosenhead (2009, pp. S7-S8) describes the evolution of operational research (OR) in the after-war period ("groups" refer to OR groups; ORQ refers to *Operational Research Quarterly*):

> In the start of this period, techniques were little known or practiced in groups. The predominant philosophy was one of problem—rather than technique—orientation.

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> However, there began during this period the gradual and continuing elevation of techniques as the rationale for the existence of OR, and as material for the ORQ.

This shift from the original problem-orientation to technique-orientation had more or less completed by the early 1960's, as witnessed by a letter (Hypher 1963) to the editor of *Operations Research*, complaining about the small proportion of case histories in journals and books, and about the perceived problem of matching the publicized models to actual problems. Mathematical formalisms came to be a methodological guideline for this type of research, as characterized by Bertrand and Fransoo (2002, p. 250):

> In fact the researchers look at the operational process or the operational decision problem through the looking glass of the mathematical models that can be analyzed.

This approach led to looking at idealized problems, amenable for mathematical representation. This is what Spearman disparagingly calls making up problems as mentioned above. How can this be defended? Bertrand and Fransoo (2002, p. 243) comment on this as follows (OM refers to operations management, a major application area for operations research):

> ...idealized OM problems were not intended as scientific models of real-life managerial problems, in the sense that the models could be used to explain or predict the behavior or performance of real-life operational processes. They were just partial models of problems that operations managers may encounter. The models were partial because all aspects of the problem that were not related to the method or technique used were left out, the implicit assumption being that these aspects would not affect the effectiveness of the problem solutions based on these models. It was left to the practitioner to include these aspects into the solution based on his knowledge of reality and of the partial model of the problem.

Thus, the central argument for focusing on idealized problems is based on the assumption that practitioners can "fill in" what has been left out from the problem definition. But can they? Bertrand and Fransoo do not forward any supporting evidence and indeed that evidence is difficult to find. That Ackoff (1979, p. 94), a pioneer in the field, bitterly attacked the developments in operations research shows that the gap between the ideal and the real was growing intolerable:

> 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, which triggered some new approaches (especially "soft OR") but failed to change the mainstream of the field. 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.

That the problems pinpointed by Ackoff have remained unsolved is revealed in the overview on methodologies by Bertrand and Fransoo (2002, p. 257), where they usefully characterize the missing type of research for validating quantitative models:

> Quantitative model-based empirical research is concerned with either testing the (construct) validity of the scientific models used in quantitative theoretical research, or with testing the usability and performance of the problem solutions obtained from quantitative theoretical research, in real-life operational processes. [...] these core processes are identified as implementation and validation. Quantitative empirical research is still in its infancy and there therefore exists much less consensus about what is good quantitative empirical research than about what is good quantitative axiomatic research.

It is of course easy to see a connection between the focus on starkly idealized problems and the missing validation of model results, on the one hand, and the long-standing relevance problem of quantitative methods, on the other. In resonance with this, operations research seems to have stagnated since the 1980s, both regarding its industrial and academic application (Rosenhead 2009, Grossman 2001).

Economics

In contrast to the two other stems, the economics stem did not create any new scholarly area for management and organization 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.

General economics

Classical economics focused on wealth, created through production, and its distribution. However, from the 1870's onwards, a new idea concerning the task of economics started to be developed. The difference between old and new has been characterized as that between an economics focusing on production and one focusing on exchange (Vaggi and Groenewegen 2003). The new economics, also called marginalism, borrowed its approach from physics, which had axiomatic starting points and a mathematical approach (Toulmin 2009). Moreover, the new economic theory deliberately treated production as a black box (Koskela 2011b). The paradigm shift can be seen as completed in 1948, when Samuelson published the first new synthesis of economic theory. While economic theory started to be expanded and refined after that, ingredients for the irrelevance discussion were accumulating as well.

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, a wider discussion on the irrelevance of economics was ignited only a decade later, in 1996, again on a forum external to economics: the magazine *The 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 among both economists and laymen.

The kernel of the criticism is aptly summarized by Blaug (1997, p. 3):

> 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 of 2008, not predicted by mainstream economists, added further weight to such calls for a renewal (Hodgson 2009). In his book titled *Seven Bad Ideas* (2014), Madrick continues the critical discussion by pinpointing that many of the most fundamental results of economics, such as the principle of the optimal efficiency of the free market, has never been empirically validated. The Nobel laureate Krugman (2014) is ready to characterize mainstream economists as follows:

> They claim that their doctrine is a deep insight derived from first principles, but dismiss as irrelevant the overwhelming evidence that these assumed principles don't hold in practice.

Intriguingly, this methodological discussion is not new; already in 1963 Albert attacked neoclassical economic theory for its construction of fundamentally non-testable, hence Platonic-like, theories (Albert et al. 2012, p. 315):

> There is no set of problems in the empirical sciences, not even in the social science disciplines, for which it makes sense to immunize theory formation a priori to possible objections that emerge on the basis of relationships to the facts.

Transaction cost economics

Transaction cost economics was one of the six major organizational theories discussed by Davis. Developed by Williamson (1979), it posits that in firms, there are production costs but also transaction costs, related to purchasing of inputs. The central point is that economic organization aims at the minimization of transaction costs through selection of governance and contractual mechanisms suitable for each particular situation. In particular, this would determine the boundaries of a firm.

Transaction cost economics has been criticized for a variety of reasons (Ghoshal & Moran 1996), including the narrowness and stylized nature of its assumptions. Here we refer to another serious shortcoming in those very assumptions, which becomes glaringly visible when looking at the theory from a production viewpoint.

Williamson (1991) equates transaction costs to waste, as it was discussed by classical economists. In this way, transaction cost economics would be a response to their calls for attention to waste minimization in economics. However, as discussed in (Koskela & Ballard 2012), this conclusion is fallacious. There is waste also in production, and economic organization should aim at the minimization of both production and transaction costs through elimination of waste. Elimination of waste in production costs also sets various requirements to economic organization – it is a mistake to focus only on transaction costs. Unfortunately, this finding, for its part, casts a dark shadow on the validity of transaction cost economics. As argued in (Koskela and Ballard 2012), the root cause of this weakness of transaction cost economics is the exclusion of production from the economic discourse.

An example of management related empirical research in economics

One question that has recently intrigued economists is why certain factories are more productive than others. The studies by Bloom et al. (2010) have been pioneering. In a remarkable and widely-cited study, a consultancy company was hired to implement mostly lean methods into textile factories in India. The implementation led to significantly higher efficiency and quality, and lower inventory levels, the average plant's productivity increasing by about 11 %. The authors concluded that for the first time, it had been shown that management, as an input, counts.

Unfortunately, this study is irrelevant on three counts. First, that lean methods provide performance improvement generally and in textile industry especially has been proven in numerous prior papers in operations management: "We have long known, and empirically proven, that Lean practices, for example, lead to superior performance" (Boer et al. 2015, p. 1240).

Secondly, the "best practices" introduced in the factory, interpreted as representing management, actually derive from a non-conventional view on production. Among other things, these management practices address the elimination of waste – the very notion of waste does not have any place in economic theory (Stigler 1976). Thus it is the impact of a certain theory of production, rather than management as such, that was observed.

Thirdly, that management counts belongs to the field of practical knowledge; how many of us have witnessed the difference a good manager or a bad manager can make in an organization? The response to the research question cannot interest anybody outside the community of interested economists – in their ivory towers.

It has to be acknowledged that the study has also merits, brought by unintentional findings. However, it provides an example of research, which, while justified in the framework of theory, fails to fulfill wider requirements of relevance.

Conclusions on the three stems

The account on the evolution of the three stem disciplines of business education is sad and alarming. All three stems have encountered a slow scientific progress and are now in an impasse. The reasons for the slow progress and the ensuing irrelevance seem to be varied. Most reasons are directly related to the recommendations of 1959, while some derive from decisions made during their implementation:

- Pushing production out of consideration, especially regarding economics
- Uncritical rejection of the accumulated knowledge on organizations prior to 1959
- Unfounded consideration of organizations as natural science objects regarding which knowledge can be created through fundamental research
- Uncritical adoption of extreme positions in philosophy of science; especially model Platonism in quantitative methods and economics
- Parochialism; failure to take advantage of knowledge in neighboring fields or in methodological fields.

Production/operations management

The turn in 1959 meant that production management was defined as a vassal discipline to management, from which major conceptual and theoretical breakthroughs were expected, to be applied by the vassal (Koskela & Rooke 2012). It is then of special interest to see how production management coped with this reorientation of management science away from production in 1959.

The starting points were indeed not good. Buffa (1980, p. 1), who wrote one of the first post-1959 text-books on production management, comments:

> 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.

According to Buffa, in this situation the majority of production management scholars turned to quantitative methods through which different decision problems in operations management were approached. In this regard, the discipline indeed took a vassal role.

How did the underlying assumption in the 1959 reports on functional fields being application areas of (general) management realize otherwise? Chase (1980, p. 12) commented: "OM research does not draw upon management theory to any noticeable degree". Similarly Slack et al. (2004, p. 372) state that, in comparison to "[…] strategy, marketing or finance", which "[…] are more-or-less directly connected to base theoretical disciplines such as economics, sociology, psychology and mathematics, OM's underpinnings are more fragmented" .

Thus, as the management discipline did not provide worthwhile theory for operations management, its research focused on isolated problems, with fragmentation as the result (Buffa 1980, p. 2):

> 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).

In 1989, Meredith et al. found that operations management research suffered from three shortcomings: narrow instead of broad scope; technique instead of knowledge orientation; abstract instead of reality perspective. They continue (p. 300):

> In sum, it appears that OM research has failed to be integrative, is less sophisticated in its research methodologies than the other functional fields of business, and is, by and large, not very useful to operations managers and practitioners.

Later, Saaty (1998, p. 12) commented in a similar vein:

> After more than a half century of tinkering with and solving problems, we need to characterize the system underlying our activity, classify, and generalize its problems.

But perhaps progress has been made in solving individual problems? Probably in some instances, but regarding the central topic of scheduling, Portougal and Robb (2000) commented that research undertaken for more than 40 years has done little to improve production planning practice.

However, it is fair to say that there has been definite progress in taking more integrative views on production. The book *Factory Physics*, by Hopp and Spearman (1996), gave for the first time a mathematically based description of comprehensive production processes, thus integrating many prior narrower models. This approach is based on the queueing theory that addresses the flows of entities through a network.

Not even this field seems to have avoided the problem of irrelevance; perhaps with some understatement, Slack & al. (2004, p. 372) 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 [...]”. In turn, Voss (2010, p. 1) writes: “However, I see symptoms that cause me to worry. The first is the separation of much research from practice.”

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In view of what has been presented, it will not be a surprise that the academic discipline of operations management can hardly boast with major innovations influencing practice (however, Factory Physics, mentioned above, may provide an exception). A great industrial shift has been occurring through the progressive adoption of lean production, originated as Toyota Production System, but its origin and diffusion lies overwhelmingly with practitioners.

It is also interesting to note that more than fifty years after the 1959 reports, the idea of operations management as a vassal to (general) management is keeping and tightening its hold. Yet, in 1998 Schmenner and Swink stated (p. 99): “Operations management can arguably be viewed as a mongrel mixture of natural and behavioral science.” However, in an article by leading academics of the field (Schmenner included) (Boer et al. 2015), operations management is now discussed in the context of social sciences.
The 1959 reports did not see production as a phenomenon worth theorizing. As a consequence, discussion on the conceptual nature of production has been almost totally missing from operations management. This has been damaging. Already during the period of scientific management, three powerful concepts of production were developed (Koskela 2000), namely to see production either as transformation, flow or value generation. Each concept has influenced production management in a major way: the mainstream production management doctrine is based on the transformation view, lean production has been engendered by the flow view, and the quality movement has its origin in the value generation view. Nevertheless, Buffa (and his colleagues later) failed to see these conceptual gains.

All in all, the 1959 turn in management research hit hard the discipline of production management. This discipline was relegated to being a vassal of general management, but this master has not had much to offer. That production management has been struggling to achieve any relevant outcomes seems to be directly related to this situation.

Specialized managerial disciplines

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Around the main fields of management, there are numerous narrower specialties, such as project management, construction management, hospitality management, healthcare management, sports management, design management, innovation management, to mention a few. In such fields, the contextual characteristics of management are accentuated, while also trends in mainstream management fields are followed. In the following, the focus is on two such disciplines, project management and construction management.

Project management

The discipline of project management has its origin in the methods, especially Critical Path Network and PERT, developed in the 1950's for mastering construction and military product development projects. These were quantitative models of development or production activities developed in the framework of operations research. These models raised considerable interest and were rapidly diffused.

Project management started to develop as a professional field rather than as an academic discipline. Professional associations began to codify project management principles and procedures (Morris et al. 2006), not from theoretical starting points but inducing from the practice of project management.

In spite of its importance as a form of organizing, project management originally attracted very little interest on the part of management scholars. The reason is simple: the doctrine of project management had started in the wrong place, namely from models of production, and mainstream management scholars could not relate to it.

However, after a few decades academic communities started to gather around the existing practice of project management as well. *Project Management Quarterly* was launched in 1970 in the US (it was later renamed to *Project Management Journal*) and *International Journal of Project Management* was launched in 1983 in Europe. Betts and Lansley (1995, p. 207) describe the first ten years of the latter journal as follows:

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> Its papers predominantly review practical experience and literature. Some case studies have been published, but relatively few published papers have been based on empirical data. Most of the papers contribute interesting insights and describe new techniques, but few have contributed to the more formal aspects of the development of the discipline of project management by building and testing models and theories.

Incidentally, at the same time an offensive especially by mainstream management researchers was started to integrate project management into the field of general management. One of the champions of this offensive was Packendorff (1995), who pinpointed three main shortcomings in the conventional research and theory on project management:

- Project management was seen as a general theory and a theoretical field in its own right; the differences between projects were not acknowledged.
- Research on project management was not sufficiently empirical. Instead, normative advice was emphasized.
- Projects were seen as “tools”, as means for attaining ends at higher levels in the system, rather than as organizations.

It is easy to see that these alleged shortcomings resonated with the topics emphasized in the 1959 paradigm change and its aftermath. Indeed, projects were reframed as temporary organizations in this new understanding. Generally, this social science offensive meant, as Morris (2012) describes, that “(t)he unit of analysis moves from delivery management to the project as an organizational entity that has to be managed successfully”. However, in the case of project management, the production oriented view was already well developed, and did not vanish from the scene (as in the case of general management), resulting in the emergence of two views on the subject, one execution and delivery oriented, the other focusing on “managing projects” (Morris 2012). This duality has engendered initial discussion and healthy debate between the two views (for example, between Koskela and Ballard (2006) and Winch (2010)).

In terms of academic research, since the early 1990s there has been a substantial improvement in the quality and rigour of research, reflected in a wider range of topics and more use of sound methodologies (Turner et al. 2012). Several theoretical approaches to project management have emerged, to some extent combining production and (more holistic) social views on projects (Söderlund 2012). As this field has been in the periphery of mainstream management, the pressure to a purely social science approach has been less accentuated.

In the professional field of project management, there have been several developments and innovations especially in the last decades (Morris 2012): agile project management, Critical chain, programme management, etc. However, these have mostly originated from practitioners; it is difficult to pinpoint innovations flowing from academic research.

Construction management

Construction (and civil engineering) is the only industry not covered by the discipline of production/operations management but has its own academic discipline: construction management. The origin of construction management as a discipline is also related to the diffusion of CPM – which has been seen as the greatest innovation in the field. The major European journal of this field, *Construction Management and Economics* (CME), was launched in 1983.

Looking at the first ten years of CME, Betts and Lansley (1993, p. 241) found a situation similar to general management before 1959:

> Thus, despite the variety in the papers to be found in CME, it has narrow focus which is largely concerned with project level issues related very broadly to production aspects of construction.

However, the drift towards the social sciences was also starting to be visible (Betts & Lansley 1993, p. 243):

> Rather, there is a discipline which is in its early stages of evolution, whereby

> theoretical traditions of research drawn from the social sciences are becoming integrated with empirical engineering work.

Indeed, the application of social science[ix] in construction management had started in the 1980s. However, as late as around the beginning of the 1990s the role of social science was not yet established, judging by the fact that at that time Winch (1990, p. 212) endeavoured to convince his readers about the usability of social science for construction management:

> Where I make no apologies for being partisan is in my belief that the social science disciplines identified above all have major contribution to the study of management in construction.

The emerging view of construction management as a social science was usefully exposed, in an email debate among construction management scholars, by Bon (2002):

> Construction management falls in the domain of social sciences. The emphasis is on management, a sui generis discipline. The other two disciplines that contribute to construction management are economics and law. Engineering is more or less incidental to what we do, just as film development is incidental to what a film director does.

Increasingly, the view of construction management falling into social science was adopted, as revealed in a remark by Murray (2009) on the social scientist role of construction management researchers. The interest moved away from production. When addressing the papers published in CME in the period 1983–2007, Pietroforte et al. (2008, p. 1531) found a shift towards firm and industry level topics:

> The initial emphasis on managing projects has been losing momentum and has been replaced by contributions that are concerned with the operations of firms and matters pertaining to the construction industry at large, both domestically and internationally.

How relevant has published research been in construction management? Seymour (2008) analyzed the papers in one issue of CME and concluded that the research reported is primarily read by other researchers. He made suggestions as to ways of ensuring that research topics address practitioner interests and concerns; and that they are addressed are made an intrinsic part of the research process. Arguably his observation and suggestions indicate the existence of relevance problems.

Has academic construction management research been able to influence the practice? The original big production-oriented achievement, CPM, emerged from industrial research, and it dominated the scene for decades and provided for a central ingredient in teaching. However, its ineffectiveness was widely sensed in practice. It was only the Last Planner System of production control (Ballard 2000), developed through industry-based action research, that started to challenge the place of CPM. Interestingly, this method covers technical, production related issues and social and psychological phenomena alike. However, neither CPM nor the Last Planner can be said to have emerged from purely academic research. Also in the wider issue of how projects should be organized, initiatives like partnering, alliance models, public-private partnerships and integrated project delivery all have their origins in industrial practice. It is indeed difficult to pinpoint major innovations in construction management flowing from academic research. However, surely has applied research in construction management, say in relation to safety, construction codes and many other topics, played a useful role, often at a national level.

Conclusions regarding specialized managerial disciplines

The long-term pattern of the development of the selected specialized disciplines is more or less identical. At the starting point, these disciplines have been pragmatic and production oriented. However, over time, the example provided by mainstream organization and management research has started to influence disciplinary identity and the type of research towards the ideal of social science. However, especially in project management, the strong position of production-based understanding has allowed some steps towards a better integration of production and social science based views.

Both fields, project management and construction management, have a history of low

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performance against expectations. However, progress in these fields has mostly been triggered by innovation engendered in practice; it is difficult to pinpoint results from academic research with major relevance and impact. Also, it is fair to say that there has been much less discussion on the irrelevance of research than in the mainstream managerial fields.

Correctives suggested

Not all management research has been realizing the recommendations of the 1959 reports. A wide variety of suggestions deviating from those recommendations have been made, some triggered directly by the perceived relevance problem, some perhaps more reflecting internal dynamics of the field. For brevity, such suggestions are called here correctives. It is thus interesting to analyze whether such correctives can, implicitly or explicitly, pinpoint problems in the 1959 recommendations, and possibly also root causes of the relevance problem.

Connecting organization theory back to production

Since 1959, production has been almost a taboo in organization science – it has simply not been discussed[x]. However, leading organizational theorists have readily found aspects of organizational life factually falling into production, the neglect of which has hampered progress. Thus, the phenomena of work, materiality and practice have been discussed.

In a paper titled “Taking work back in”, Barley & Kunda (2001, p. 76) 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.”

Regarding Barley 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.

Orlikowski (2007, p. 1435) 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.

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.

Another related novelty in organization theory is the practice turn (Schatzki & al. 2001). Practice theory takes it for granted that social reality is fundamentally made up by practices (Feldman & Orlikowski 2011, p. 1240): "…social life is an ongoing production and thus emerges through people's recurrent actions". What are practices then, precisely? Feldman and Orlikowski (2011) describe a case study on an organization providing student housing, where routines of budgeting, hiring and training of staff as well as opening and closing of residence halls are considered as practices. It is difficult to avoid the impression that the question is about operational activities in functional areas (finance, human resources) and in the production of the main output of the organization – in other words, about production! Thus, practice theory seems to claim that people's recurrent and improvised productive actions make up social reality, produce social life (at least in business organizations). This is a remarkable and important insight: the social and the technical cannot be separated.

It can be argued that these calls – all bizarrely avoiding to use the term "production" (in its conventional meaning) in their main vocabulary - provide strong evidence for the neglect of production in managerial and organizational theory and for the need to rectify the situation.

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Reviving production as a discipline

The 1959 reports relegated production to a humble application area for theories and tools developed in general management research. One of the original promoters of the social science turn of management science, Simon, soon came to other thoughts. In (Simon 1969, p. 3), 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 about 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. However, it must be added that although totally deviating from the mainstream doctrine, this initiative of Simon lacks novelty; it closely follows Aristotle’s call for a science of production (Koskela 2008).

In his prior publications, which informed the 1959 reports, Simon had subscribed to the idea of organizations being similar to biological organisms, i.e., falling into the domain of natural science. Simon left it for others to address the implications of seeing business falling into the artificial science (to be discussed below).

Alternative ways to knowledge

The 1959 reports suggested following the model of natural science: researchers external to the phenomenon, using quantitative methods, and pursuing fundamental research.

Morgan and Smircich (1980) strongly attacked the dominance of quantitative methods in social science, with the argument that any methodological approach is connected to interrelated assumptions regarding ontology, human nature and epistemology. Methodological choices are not ends in themselves – rather they should be compatible with other assumptions and choices made in research. Morgan and Smircich promote qualitative research as a methodological alternative, and state that its appropriateness derives from the nature of social phenomena to be studied.

Susman and Evered (1978) suggested action research as a suitable type of research in organizational science. The conclusions by Pfeffer and Sutton (1999, pp. 5-6) resonate with the idea of action research although they refer to managers as creators of knowledge:

> …one of the most important insights from our research is that knowledge that is actually implemented is much more likely to be acquired from learning by doing than from learning by reading, listening, or even thinking… Taking action will generate experience from which you can learn.

Somewhat later, often influenced by Simon’s arguments for the science of the artificial, calls were presented 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). The common feature in these calls was that the end result of research was 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 closely related to the “principles” of classical management science, poured scorn on by Simon (1976).

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

Conceptual research is one further corrective. In another remarkable turnaround (besides Simon’s), March (Reed & al. 2000, p. 55) 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

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raising fundamental issues, and advancing knowledge about fundamental processes affecting management.

Conclusions on correctives

There has been a wide interest in correctives that in many cases factually resonate with the production-centric features of the pre-1959 approach to management, which were pushed aside in the social science turn, while in other cases they represent new developments. In both situations, they signal shortcomings in the 1959 recommendations.

Comparison to medicine and engineering

The 1959 reports on business education are not isolated occurrences in the history of higher education in the US. Rather, there have been other influential and celebrated reports, of which Gordon and Howell (1959) even mention one, the Flexner report in medicine from 1910. In engineering, the Grinter (1955) report was published only a few years before the business education reports. It is an institutional peculiarity of the US higher education that changes are achieved through reports initiated by foundations or professional societies. For its part, this explains the considerable influence of the 1959 reports on the business schools in the US – and through their leading position, also elsewhere in the world.

However, this institutional peculiarity allows comparative analyses. As research in medicine and engineering has arguably been more successful than in business – there is no irrelevance discussion – it is interesting to compare these four reports (Flexner 1910, Grinter 1955, Gordon & Howell 1959, Pierson 1959). An overview is given in Table 2.

Table 2. Overview on major educational reports in medicine, engineering and business.

Field	Medicine	Engineering	Business

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Report	Flexner, 1910	Grinter, 1955	Gordon & Howell, Pierson, 1959
Funder	Carnegie Foundation for the Advancement of Teaching	American Society of Engineering Education	Ford Foundation; Carnegie Corporation of New York
Organization of preparation	Flexner as investigator under the direction of the Foundation	Committee of 46 men, chaired by Grinter	Gordon and Howell as investigators and a review group; Pierson as investigator and a review committee
Identified basic sciences (in the case of business education, general education subjects)	Biology, chemistry, physics	Mathematics, physics, chemistry	Humanities (including English language and literature) and fine arts, natural sciences and mathematics, behavioral-social sciences
Identified underlying sciences	Anatomy, physiology, pathology, pharmacology	Mechanics of solids, fluid mechanics, thermodynamics, transfer and rate mechanisms, electrical theory, nature and properties of materials	Organizational behavior, quantitative methods, economics
Professional divisions	Medicine, surgery, obstetrics, specialties (diseases of eye, ear, skin, etc.)	Not treated.	General management and functional areas of management: marketing, production management, financial management, human resources
Teaching of practice	Clinical training	Engineering analysis and design; cases	Cases, role-playing.

The comparison of interest here is in regard to the nature of underlying sciences, especially regarding their maturity, coverage and further progress.

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In medicine and engineering, the majority of the underlying sciences had (at the respective time of each report) a long history. In the case of medicine, anatomy and physiology had begun in Antiquity, and, although there had been periods of standstill, they had showed continual progress and impact on clinical medicine. In engineering, say, structural mechanics (which underlies especially structural engineering) had been consolidated already in 1863 by Rankine (1872). Thermodynamics had similarly evolved in the 19th century and started to provide scientific fundamentals to respective tasks in engineering.

Instead, the underlying sciences for business education were all either young or nascent at the time, as discussed above. This meant that they hardly had any track record of a successful application in business and management. Organizational behavior only started to be a recognized field in the 1950s – as admitted by Gordon and Howell (1959, p. 382): "Research on organizational problems is still in its infancy". Quantitative methods had evolved through military applications during the Second World War, and the transfer of these methods to business applications had started only recently. As discussed above, economics, although having been a well-established science already in the 19th century, had undergone a major paradigm shift, from which a comprehensive new synthesis had emerged only in the 1940s. Although all these three fields seemed promising, none had proven a lasting significance for management, and the decision to base business education and research on them was inherently risky.

Another interesting aspect for comparison is the coverage of the underlying sciences. The various medical problems had directed research to all salient aspects of the health and lack of it. In a similar way, engineering problems and the progress of technology had propelled different engineering sciences. In both cases, the underlying sciences had evolved organically.

The situation of business education and research provides yet another picture. Here, the determination of underlying sciences is rather based on a deliberate choice and plan, as discussed above. Of course, organizational behaviour is clearly a relevant field, addressing significant phenomena in organizations – however, productive activities are excluded from the purview. In contrast, quantitative methods are not addressing any specific field, the question is rather about a methodical approach or tool. Economics, in its newly developed form, was about optimal decisions, allocation of scarce resources – this is a narrowly defined aspect of human activities. One can easily list other corresponding and related cognitive activities, like design (of alternatives among which the optimal decision is to be made) and improvement (after all, decisions have to be implemented and the invariantly emerging deviations from the optimal need to be reduced). Thus, it seems that the three stems were mutually disparate and only patchily covering the phenomena significant for management.

What kinds of progress have the underlying sciences made after each report? As it is commonly known, the underlying sciences for medicine have vastly developed and increased the effectiveness of medical interventions. The same situation applies to engineering sciences, exemplified by electronics and material science. However, regarding business and management, it is here that we encounter the widely felt lack of relevance – the progress in these sciences has been slow, and their impact on management has been modest, while innovative managerial methods and organizational forms have been developed in practice.

All in all, a comparison to other fields highlights the fact that the 1959 reports on business education chose to base business education on unproven scientific fields that did not completely cover the phenomena in business management. The lacking progress in these fields up to now, as well as the holes in their coverage, arguably have a strong connection to the irrelevance of management research.

Conclusions

As irrelevance in management research, in its many forms, has been discussed at length, it is opportune to start by noting that management research has certainly produced relevant, useful and influential outputs – only the focus of this analysis has been on the irrelevant outputs, which form the majority, according to so many observers.

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Why, then, is management research irrelevant? While the immediate causes for irrelevance vary widely, there are also common root causes. The analysis made, although brief and operating on the basis of samples and examples, pinpoints to three important findings not discussed in prior literature.

First, it seems that the role of the 1959 reports needs a critical reassessment. In connection to the 50th anniversary of the business education reports of 1959, they have been commented in a largely positive tone (Anon. 2009), while pinpointing that Gordon & Howell (1959) called for better research, and that in this regard, there is still much room for improvement. In other words, the 1959 recommendations were assessed to be correct and sound, just their implementation could have been better.

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 failed miserably; the functional fields, spearheaded by production/operations management, do not seem to have fared any better.

Indeed, with the benefit of more than 50 years' hindsight, it can now be convincingly argued that the direction proposed in 1959, and closely followed by the management scholar community, has been utterly wrong. It has led to a massive, discipline-wide idling of management research. This has not been a period of the Kuhnian normal science, focusing on the remaining pieces of the puzzle before the current paradigm will exhaust itself and eventually be replaced by a new paradigm. Rather, this would be more aptly characterized as cargo cult science (Feynman 1974), where just the external forms of research are followed, without an understanding of the essence of the undertaking.

These problems in the outcomes of the 1959 reports can be related to the shortcomings in the preparation of those reports. In critical analysis, the 1959 turn in management research was based on fragile justification, disciplinary lobbying and contemporaneous intellectual fashion rather than a reasoned, balanced and mature examination of the situation and future requirements. The example and experiences of

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GSIA at Carnegie Mellon in the 1950s were generalized into a general ideal model, to be followed in all business schools. In so doing, the unique success factors of GSIA, such as a strong interdisciplinary approach ensured by an effective leadership, and staffing with extraordinarily capable scholars (several were later awarded the Nobel prize), were not taken into consideration (Khurana & Spender 2012). The restlessly creative minds of Simon and March, chief figures at GSIA, later came to radically new ideas on the nature of business and research in business schools. However, the army of management researchers is still marching into the direction commanded in the 1950s, even if the generals have changed their minds long since.

The second novel insight is related to the suppression of production from managerial research. Looking from the angle of organizational science, the repositioning of production as an application area of general management has been extremely damaging as production plays two important roles in organizations. First, as even textbooks are ready to admit, organizations transform inputs into outputs, that is, produce (Scott 1990, p. 20):

> …we will insist that every organization does work and possesses a technology for doing that work. Some organizations process material inputs and fabricate new equipment and hardware. Others "process" people, their products consisting of more knowledgeable individuals, in the case of effective school systems, or healthier individuals, in the case of effective medical clinics.

Simply, production of outputs for the external world is the *raison d'être* of any organization, and the effectiveness and efficiency in production continued through the 1950's up to this moment and will continue in the future to be paramount goals for organizations.

Second, production actually prevails everywhere in an organization, also outside the so-called production function[xi]. Let's look at the other functions of an organization: management, marketing, finance, human resources. Everywhere there are tasks with specified outputs; there are information and material flows; there are customers for whom outputs are produced. Perhaps it would be clearer to call these operations, but nevertheless all the hallmarks of production are there.

Thus, in an organization, there is a production function, which is primary among the different functions, but at the same time production is a ubiquitous aspect throughout the organization. The question how to organize to produce efficiently has lasting relevance – but to respond to that question we need to understand production and the way it is interfaced with management. A denial to conceptually and theoretically address production cannot be justified. Regarding the interface between production and management, the attitude of the “absent presence” of production in management discourses has prevailed, when the proper view rather should be that there is such an entanglement[xii] of management and production that they are best to be addressed as one entity.

The third novel insight is about the great variety of immediate sources or irrelevance of a piece of research. A multitude of causes of irrelevance, with causal chains leading mostly to the 1959 reports, were observed:

- research topic not relevant (in operations research)
- partial conceptualization of the phenomenon addressed (production left out from consideration of organizing)
- unhelpful conceptual and ontological assumptions regarding the phenomenon (focus on quantitative methods in social science)
- failure to embrace the topic conceptually (failure to conceptualize production in production management)
- unhelpful epistemological choices (axiomatic approach in economics and operations research)
- missing or deficient validation of results (in operations research)
- deficient historical awareness of the evolution of the field (generally)
- deficient awareness of methods and methodological discussion outside own research community (generally).

In the irrelevance discussion hitherto, some immediate reasons for irrelevance have been discussed while many others have not been addressed.

What should be done? Although it would be tempting to delve into that discussion, it must suffice to present just a few pointers, given that the focus of this investigation is on reasons, i.e. on diagnosis, as a preparatory step for prescription.

The diagnosis made suggests, first, that a fundamental rethinking of management research (and the business education based on it) is requisite. The 1959 reports were not adequately prepared, and their outcomes have not passed the test of relevance. Such rethinking should be done from a clean slate. Second, production needs to be reintegrated, conceptually, theoretically and practically, into management. Third, the different immediate causes of irrelevance need to be classified, characterized and exemplified, and management students and scholars need to be sensitized to them.

Management is important as a phenomenon and deserves a flourishing scholarly field, with a positive impact both directly on practice and indirectly through education and training. The self-complacent acceptance of irrelevance that has radiated from management as a scholarly field is a dangerous disease. The situation seems to invite urgent efforts from all disciplines and fields of management to find and deliver a cure. The general management fields (the three stems), from which the irrelevance problem has diffused to functional and specialized fields, have been more or less incapable of taking action. However, this idea of there being a centre and a periphery in management scholarship can be challenged; fundamental changes are needed, and it may be that the functional and specialized fields of management, being nearer practice and often less indoctrinated by the 1959 legacy, are more capable to lead towards these changes.

Given it that this paper is published in an issue about new directions in construction management research, it is appropriate to offer an analysis on what could and should be done in this specialized field. First, the general need for a fundamental rethinking of management research implies that also in this field a thorough discussion is launched on the role of research, methods to be used and the criteria to be set for it. The idea of construction merely providing a context for the application of (general) management ideas and methods has to be rejected. Especially, this means that such construction management research, where topics and approaches from general management arenas are applied, does not automatically inherit its justification and relevance from general management disciplines - which are in a deep crisis.

Innovations in management practice and/or theory can emerge in any industrial context, and thus scholars in construction management should be encouraged to publish also in general management journals, in order to contribute to the needed renewal across managerial fields. Second, the damaging idea of management falling into social science alone has to be rejected. In construction management, which so much focuses on designing and making, this idea is especially counterproductive. Construction management scholars should confidently approach the key phenomena of designing and making as well as related organizing, and conceptualize and theorize them as needed, without prejudices. It is perfectly acceptable to focus on a relevant aspect, be it social or technical or something else, in one given research study. However, a systematic avoidance of one aspect cannot be justified. Third, the many immediate threats to relevance need to be addressed: published research needs to be made more relevant. Here the main responsibility lies with the scholarly community. The above mentioned discussion may be needed for changing attitudes and clarifying the direction. In the reviewing guidelines of the journals, there must be more attention to the justification of the research problem, the assessment of the value of the findings or the evaluation of the proposed method. The burden of proof must be with the author(s) to claim a submitted paper relevant. However, also the processes feeding to publications, such as PhD research arrangements and the selection and promotion criteria of academics, need to be improved for the sake of added relevance.

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[i] The presentation by Spearman is available at (POM Society 2016). POMS stands for Production and Operations Management Society.

[ii] This journal article is a considerably expanded and improved version of the conference paper (Koskela 2011a).

[iii] There were notable exceptions, such as Elton Mayo who approached management from the point of view of applied social science (Smith 1998).

[iv] Hammond (1990, p. 143), who similarly has critically evaluated Simon's arguments against Gulick, says: "Simon is generally considered to have 'won' the debate in the 1940s and 1950s, and there is good reason to think that this 'victory' turned the field of public administration in a direction very different from where it had been headed previously." Further: "…had Gulick's approach been pursued in the ways Gulick suggested, there is reason to think we would know considerably more about the design of organizational structures than we currently do."

[v] The role of production in organizations was a well-known topic. For example Parsons, a leading sociologist of the time, had somewhat earlier (1956) stated that business firms are organizations oriented to economic production, i.e. production is their goal or function. Also, in his analysis of organizations, he identified three contexts within them, whereby the first concerns the factors of production and how they are combined for attaining the goal (in other words, production), the second consists essentially of decision-making, and the third refers to the institutional structure that integrates the organization with others.

[vi] To avoid doubts that this selection of the functional area has been biased towards one area showing more irrelevance than others, the following literature is suggested: regarding accounting (Johnson & Kaplan 1987), strategy (Abraham & Allio 2006) and marketing (Reibstein et al. 2009).

[vii] The title was later changed to *Academy of Management Journal*. In 2016, a new journal with the title *Academy of Management Discoveries* (AMD) was launched. It is described as follows (*Academy of Management Discoveries* 2015): "AMD is a member of the family of journals from the Academy of Management (AOM). As such, we view the AMD mission as distinct from, but complementary with other AOM publications. AMD focuses on reporting novel findings or unusual empirical patterns that are not adequately explained with current theories. This in turn inspires future theory-building and

testing." Of course, this characterization of the contents raises an embarrassing question: what was the mission of the other journals if a new one is needed for novel findings?

[viii] Two of these, namely transaction cost economics and agency theory, have been developed in economics, others stem from work on organizational behavior.

[ix] In the UK, Science and Engineering Research Council's Specially Promoted Programme in Construction Management started to fund social science based research into construction management in the 1980s (Fellows 2008).

[x] There are certainly exceptions, such as (Thompson 1967).

[xi] A discussion on this aspect can also be found in textbooks, for example, in (Krajewski et al. 2013).

[xii] The notions of ”absent presence” and ”entanglement” have been used by Orlikowski (2009) to describe the relation of organization to technology – here the notions are used in a slightly wider setting.